

DOES HOMEOWNERSHIP REDUCE CRIME? A RADICAL HOUSING REFORM FROM THE UK*

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‘Right to Buy’, a large-scale natural experiment whereby incumbent tenants in public housing could buy properties at heavily subsidised prices, increased the United Kingdom homeownership rate by over 10 percentage points between its 1980 introduction and the 1990s. This paper studies the impact of this reform on crime by leveraging exogenous variation in eligibility for the policy. Results show that Right to Buy generated significant property crime reductions. Behavioural changes of incumbent tenants and renovation of public properties were the main drivers of this crime reduction. This is evidence of a novel means by which subsidised homeownership and housing policy can reduce criminality.

In many countries, crime is spatially concentrated in places with low incomes and low rates of homeownership. Localities with high rates of tenancy in public housing commonly exhibit high crime rates, even when controlling for other salient characteristics of the resident population, and the United Kingdom is no exception (see, for example, for the USA, Schill, 1993; Olsen, 2003; Kling *et al.*, 2005; for the UK, Murie, 1997). This association between crime rates and housing tenure arises not only because of differences in affluence between predominantly public and private housing areas, but also because residents in public housing may have lower incentives to maintain the security and upkeep of their property and to invest in neighbourhood monitoring.

Boosting homeownership has been viewed as a means of delivering benefits to communities, such as lower crime rates, greater civic involvement and improved child development (see, for example, Di Pasquale and Glaeser, 1999; Haurin *et al.*, 2003). Underlying this is the idea that, when public housing tenants take on ownership of their properties, their incentives adjust as they experience the positive private returns of crime-reducing investments (such as improving the security of the home), which capitalises into house prices and therefore household wealth.

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However, establishing causality between homeownership and crime is not straightforward.¹ Generating an understanding of the crime-reducing effects of homeownership is therefore a first order research question in the economics of crime.

The impact of homeownership on crime can be studied by examining a large-scale nationwide programme of subsidised public housing sales to incumbent tenants in the UK known as the ‘Right to Buy’ policy (hereafter RTB). This was established as one of the first legislative acts of the newly elected UK Conservative government led by Margaret Thatcher in 1979. Through the RTB policy, incumbent tenants in publicly owned ‘council housing’ (housing built and owned by local public authorities and rented to private tenants, henceforth called ‘public housing’) in the UK were permitted to purchase their rented accommodation at heavily subsidised prices.² The intention of the policy was to increase the long-run homeownership rate, with the underlying ethic of ‘an Englishman’s home is his castle’. It was heralded by the government as a means of improving local amenities and generating upward social mobility by giving citizens control over their housing and access to housing wealth.³

The RTB policy had a significant impact on housing tenure as it dramatically changed the ownership composition of housing in the country. It was largely responsible for an increase in the UK homeownership rate from around 60% in 1979 to over 70% by the early 2000s. Specifically, whereas around 32% of dwellings in the UK were publicly owned in 1979, totalling some 6.2 million properties, by the early 2000s around 2.8 million of these properties had been sold to their tenants (Jones and Murie, 2006). Although an innovative, large-scale and radical policy, RTB has until recently been little analysed in economics (see Disney and Luo, 2017, for a theoretical analysis of RTB policies). Analysis of RTB is useful to derive policy prescriptions that extend beyond the UK, as the RTB scheme shares some of the same characteristics as housing reforms implemented in Israel (e.g., Hausman *et al.*, 2022), Sweden (Sodini *et al.*, 2021) and multiple post-Communist countries.

This paper studies the causal impact of the RTB policy on local crime rates. The empirical analysis uses large-scale data from all regions in England and Wales from the early 1970s through the period of policy implementation in the early 1980s, viewing the RTB policy as a nationwide policy experiment.⁴ The bulk of the public house sales occurred in the 1980s and the analysis is based on matched area-level sales of public housing to crime data over several decades. It shows

¹ Much of the literature on the ‘flight to the suburbs’ took place against the backdrop of rising crime in inner cities in the United States in the 1990s. Cullen and Levitt (1999) is one of a number of studies that suggest that higher rates of crime, especially in inner cities, led to changes in neighbourhood composition by social class and economic status. It has been more challenging to show that falling crime in both earlier and later periods has been the primary driver of the so-called gentrification of inner-city areas (contrast, e.g., the findings of McDonald, 1986; Ellen *et al.*, 2017).

² The terms ‘council housing’ and ‘social housing’ indicate public housing in the UK. Public housing in the UK is managed in local jurisdictions by councils—hence the term for public housing is ‘council housing’ in the UK. There are also social housing tenancies provided by co-operatives and local housing associations rather than private homeowners. A limited form of tenancy purchase of such properties was introduced in the 2000s under the policy ‘Right to Acquire’.

³ In a speech delivered to the National Housebuilding Council in December 1984, Margaret Thatcher stated: ‘Spreading the ownership of property more widely is central to this government’s philosophy. It is central because where property is widely owned, freedom flourishes. Since we took office in 1979, 1.7 million more people have come to own their homes—1.7 million more sole kings on their own sole ground. That increase is one of our proudest achievements . . . But a house is more than this. It is a symbol of security, and a stake in the future. People who own houses do so not just for themselves, but for their children. They do so as members of a responsible society—proud of the heritage derived from the past, glad to care for it, and eager to give the next generation a bit of capital to give them a start’ (see <https://www.margaretthatcher.org/document/105815>).

⁴ The analysis covers only England and Wales owing to a need for comparable crime data, which is not available for Scotland and Northern Ireland.

that the large movements in housing tenures induced by the RTB led to falls in property crime rates that persisted for over a decade.

The mechanisms behind this crime-reducing effect of sales of public housing in local areas are studied. The analysis shows that the reduction in crime rates was driven primarily by behavioural changes *within* the local community rather than a ‘reshuffling’ of households between low and high crime areas, which might have had a smaller effect on overall crime. This is in contrast to the focus of some recent studies of the crime-reducing effect of ‘gentrification’ (e.g., Autor *et al.*, 2019), whereby changes in the composition of households induced by inward and outward migration are thought to explain local trends in crime rates. The study thereby reveals a novel means, not documented in detail to date, by which subsidised homeownership and housing policy may have contributed to the decline in crime in the United States and other Western economies in the 1990s and early 2000s (a point also noted by Van Dijk and Vollaard, 2012).

The RTB experiment provides a unique opportunity both to assess the effectiveness of selling public housing to grant homeownership rights to families in public housing and to measure the causal impact of sales of public housing on crime outcomes. From the perspective of a policy-maker designing a policy that would alter homeownership rights for the general population, the parameters estimated here are directly of interest.⁵ As such, this study complements the literature that has examined neighbourhood effects on crime by exploiting the variation in residential locations induced by the ‘Moving To Opportunity’ (MTO) experiment in the United States and by other housing policy initiatives in Western economies.⁶

To offer more detail on the adopted research design, the paper studies the causal impact of homeownership on crime by leveraging the unfolding of the RTB policy. Difference-in-differences (DiD) specifications exploit the differential intention-to-treat (ITT) effect of the RTB policy across localities. RTB was introduced as a national policy soon after the election of the new Conservative government in 1979, but the intensity of treatment across localities varied with the pre-determined length of tenure duration of incumbent tenants in public housing, which averaged more than 10 years. Specifically, tenants were required to have been resident in public housing for at least 3 years prior to the policy introduction, a discontinuity which is leveraged for identification. The identification strategy therefore exploits housing choices made by tenants many years prior to the introduction of the policy, ruling out any confounding anticipation effects. Variations in eligibility to the RTB policy in 1980, i.e., at the start of the policy, are shown to be unrelated with other local area socio-economic circumstances, such as unemployment, wages, fraction of juveniles and fraction of public housing; they arise from historical locality-specific factors, and this variation is a key part of the research design.

Estimates reveal sizeable negative short-run effects of increased incidence of RTB sales of public housing on property crime. The crime reduction appears sizeable since the early 1980s and it remains visible in the late 1980s and early 1990s until the end of the Thatcher era. These short-run estimates uncover an elasticity of crime with respect to RTB incidence of approximately -0.1 , so that a 10% increase in incidence of RTB public housing sales reduces crime by around 1%. This conclusion is robust to a battery of additional tests, including variations in the specification of the econometric model.

⁵ Homeownership is likely to have important implications also for intergenerational mobility, a link that until recently has been little analysed (see Blanden and Machin, 2017; Bell *et al.*, 2018).

⁶ See, e.g., Katz *et al.* (2001), Ludwig *et al.* (2001, 2013), Kling *et al.* (2005, 2007), Ludwig and Kling (2007), Sanbonmatsu *et al.* (2011), Sciandra *et al.* (2013), Damm and Dustmann (2014), Bernasco *et al.* (2017) and Rotger and Galster (2019).

The paper examines many potential mechanisms that could underlie the finding that RTB purchases reduce crime rates. These hypothesised channels include varying local area circumstances, local household compositional changes arising from the RTB policy and the potential role of feet-dragging practices in certain localities arising from the political affiliation of the local authorities responsible for administering the policy at the local level. The findings suggest that none of these factors explains the crime reduction generated by the RTB policy. The market value of the RTB sales at the onset of the scheme does not predict the evolution of crime either, suggesting that the crime reduction attributed to the RTB scheme is genuine and it is not the spurious result of better amenities in areas with more RTB sales.

By contrast, the findings are strongly indicative of the likelihood that local communities were induced to change their behaviour and attitudes towards criminal activity. Indeed, as suggested in the quote by Margaret Thatcher cited previously, this was one of the aims of the RTB policy. That is, to give (generally) working-class households access to an owned asset in order to change their behaviour, such as taking greater care of and improving the security of their property and to change their economic position by giving these households a collateral asset in financial markets. Hence, RTB was viewed as a mechanism for improving and upgrading the economic position of households in neighbourhoods that had been previously dominated by public housing.

The analysis reveals that immediate reductions in crime resulted from the RTB policy. These effects are not attributable to in-migration, as the rules of the RTB scheme effectively barred resale of the RTB-purchased property for a fixed period after purchase. Crime rates were reduced immediately after the introduction of the policy, and not once the restraint on resale was no longer binding. The analysis also shows that, after purchasing their properties from the local council, incumbent ex-tenants started to gentrify their properties by installing double locks on doors and windows, by installing burglar alarms and by purchasing insurance for their home contents. Locality-specific estimates show that incumbent tenants who bought under the RTB scheme experienced greater crime reduction in counties in which the RTB scheme was associated with greater home improvement. However, they did not become more likely to participate in a neighbourhood watch scheme, suggesting that the RTB policy did not generate detectable changes in social capital. The changing behaviour of incumbent ex-tenants resulting from RTB purchases did not result in increased victimisation of neighbouring householders, nor in a substitution of offenders away from burglary towards other crimes. Results indicate that the RTB policy caused sizeable reductions in burglaries and theft and handling of stolen goods offences, while robberies and other violent crime offences remained unchanged.

These findings complement the large literature that assesses the neighbourhood effects on crime by exploiting the variation in residential locations generated by the MTO experiment in the United States. Starting in 1994, the MTO experiment assigned housing vouchers via random lottery to thousands of public housing residents with children in five cities in the United States to relocate to less-distressed areas. Exposure to violence and crime victimisation in distressed areas were, in fact, key reasons for low-income families to participate in the MTO experiment. Katz *et al.* (2001) show that the MTO experiment improved children's behaviour, adult mental health and perceived safety in treatment group families in Boston, while also reducing exposure to violence and crime victimisation. Similar findings emerge in Baltimore in the study of Ludwig *et al.* (2001), who find that the MTO experiment led to a large reduction in juvenile arrests for violent crimes and to some increase in property crime arrests (see also Kling *et al.*, 2005).

Using more recent data, Sanbonmatsu *et al.* (2011) conclude that the MTO initiative enhanced safety in treatment group families, while Ludwig *et al.* (2013) find gender differences in the

impact of the MTO experiment on risky behaviour and health of juveniles to persist 10–15 years after the start of the experiment, while finding no evidence of persistent reductions in youth violence rates. However, Sciandra *et al.* (2013) show little evidence of crime reductions in the long run as neighbourhood conditions' effects of MTO dissipate, reflecting that crime is more affected by contemporaneous neighbourhood circumstances than by past neighbourhood circumstances.⁷ To understand why low-income families remain segregated into high-poverty areas, Bergman *et al.* (2019) randomly allocate housing vouchers to 430 recipient families with a child in the Seattle and King County areas, concluding that barriers in the housing search process are a critical source of residential segregation by income. Note that the MTO initiative involved the relocation of families, unlike the RTB policy which locked recipients into remaining in their neighbourhood for a minimum of five years.

The findings on homeownership relate more closely to the experiment in Oklahoma investigated by Engelhardt *et al.* (2010), which subsidised saving for down payments for homeownership among a group of low-income individuals using individual development accounts (IDA) with randomly assigned treatment status. In their setting, the treatment is the offer of matching funding to the IDA, given that only a fraction of those who were treated chose to take up the offer and to undertake a purchase, and, of course, these may not be a random group among the treated. They find only weak evidence that homeowners who benefited from the policy spent more money on 'community-facing' activities such as external improvements to their house or involvement in civic events in the 30 months after the take-up of homeownership.

There is also relevance to some recent studies of the effects of homeownership and gentrification on crime that focus on the neighbourhood composition effects of policy changes. Aliprantis and Hartley (2015) and Sandler (2017) examine the effect on local crime rates in Chicago when 20,000 units of concentrated high-rise public housing were demolished over the period 1999 to 2011. Both studies, albeit using slightly different methodologies, track relocated individuals to other neighbourhoods, and they conclude that these demolitions led to a net reduction in crime rates—primarily violent (gang-related) crime, but also theft, robbery and use of guns. Chyn (2018) shows the lasting beneficial effects of housing demolitions in Chicago on the schooling, professional and criminal trajectories of displaced individuals. Autor *et al.* (2019) examine the impact of the deregulation of rents in Cambridge, Massachusetts, on local crime rates. They hypothesise that rent deregulation raised property values and caused a differential mix of households to locate across local neighbourhoods, thereby disproportionately reducing the rate of property crime in neighbourhoods that had previously been subject to rent control. They find a significant reduction in crime overall, which was then capitalised into higher property values.⁸ In a related study, Diamond and McQuade (2019) document the crime-reducing effect of the low-income housing tax credit.

Most of these studies therefore examine the effects of *gentrification* on outcomes such as crime, exploiting policy changes such as the lifting of rent controls and removal of public housing that have led low-income neighbourhoods to increasingly become middle-income neighbourhoods

⁷ The MTO experiment, of course, affected also other dimensions of families' and children's lives. Most notably, it generated moderate improvements in school quality (Fryer and Katz, 2013), as well as educational and economic benefits for young children, including for young boys (Chetty *et al.* 2016). Substantial exposure effects of neighbourhoods are also presented in Damm and Dustmann (2014), Bernasco *et al.* (2017), Altonji and Mansfield (2018), Chetty and Hendren (2018a,b), Rotger and Galster (2019), and Aliprantis and Richter (2020).

⁸ A recent paper on the UK (Alonso *et al.* 2019) suggests that crime rates were reduced by expenditures from the Urban Renewal Fund; although these expenditures did not directly involve changes in housing tenure, their idea is closely related to the 'neighbourhood externalities' argument.

through outward migration of low-income households and inward migration of higher-income households. This study examines the impact on recipients of subsidy that remain in their neighbourhood. In addition, studies that have examined the impact on crime rates of moving tenants out of public housing into the private sector in the United States and experiments that gave households incentives to purchase properties are typically based on highly localised data and some results are obtained from relatively short time periods after implementation of the policy.⁹

Finally, there is a connection to work that documents the negative link between crime and property values (see Gibbons, 2004, for an illustration using data for London) and the spatial equilibria of neighbourhood composition induced by this feedback effect when account is taken of not just crime rates, but also other neighbourhood (dis)amenities, e.g., such as transport costs. For example, Owens *et al.* (2020) show that civil gang injunctions in Southern California, a common type of place-based crime control policy in the area, led to approximately a 3% decline of residential properties' values from 2002 to 2015, reflecting individual willingness to pay for the civil liberties affected by the injunction.¹⁰ Morales-Mosquera (2021) also finds that police station openings generate localised crime reductions and housing value increases in three major cities in Colombia.

The rest of the paper is structured as follows. Section 1 describes the RTB policy. In doing so, it seeks to emphasise the scale of public housing in the UK by 1980 (in comparison to, say, the USA) and to explain why the spatial distribution of public housing does not simply map into low-income areas at the start of the RTB policy. Data sources are described in Section 2 and the empirical analysis is presented in Section 3. Finally, Section 4 provides a concluding discussion, including a word of caution in assessing the overall merits of the RTB policy.

1. The Right to Buy Policy

Throughout the twentieth century, the stock of public housing in the UK grew from less than 1% of the housing stock in 1918 to approximately one-third of the housing stock by 1980. This growth was mainly due to post-war house building, as shown in Figure 1.¹¹ Shortly after Margaret Thatcher's election as UK prime minister, the Housing Act 1980 introduced a statutory right to buy (RTB) for public tenants with at least three years' tenure duration in their council house—'statutory' in the sense that the policy had to be implemented by all local councils (previously a few Conservative councils had allowed their tenants to buy their public property, generally at market price). The RTB policy allowed tenants to buy their properties at substantial discounts to market value ranging from 33% with three years' incumbent residence to a maximum of 50% after 20 years' residence. Local councils were also obliged to make mortgages available to would-be purchasers, although this feature became less pertinent as capital markets were liberalised during the 1980s. The discount on the sale price would be repayable if the property was resold within five years of an RTB purchase, although a purchased property could be rented out.

There were additional constraints in the 1980 legislation, particularly in relation to the sale of publicly owned apartments, but these too were relaxed in the mid-1980s. Purchase of public housing under RTB also became more attractive with later efforts to raise heavily subsidised

⁹ The reviewed literature examining the MTO experiment constitutes an exception.

¹⁰ Grogger (2002) and Ridgeway *et al.* (2019) are two earlier studies of the effects of gang injunctions on crime.

¹¹ Figure A1 in Online Appendix A uses official Bomb Census data from <http://bombsight.org/data/sources> to show that the location of bombings across boroughs in London during World War II is a significant predictor of where publicly owned housing estates were built in the post-war period in London.

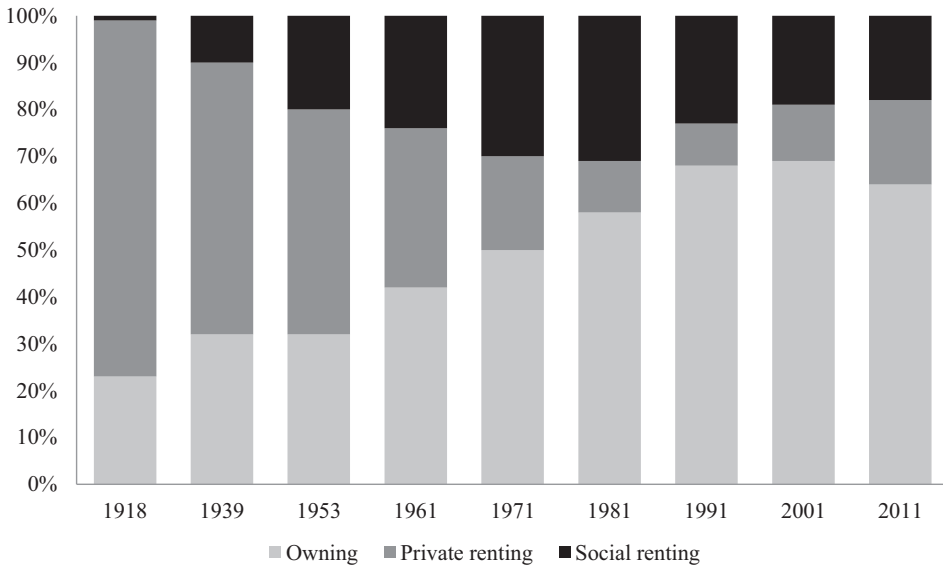


Fig. 1. *Housing Tenure Shares by Tenure Type: England and Wales 1918–2011.*

Source: Reproduced from Disney and Luo (2017, chart 1), calculated from Office for National Statistics (2013).

rents on public tenancies towards ‘market’ levels.¹² Figure 2 shows the pattern of sales under the RTB policy in England (the other UK nations had similar patterns—indeed the large stock of public houses in Scotland was sold even more rapidly, leading to a blanket ban on further sales by the Scottish government some decades later). The two peaks in sales in the 1980s are associated with the introduction of the RTB policy and its liberalisation in the mid-1980s; thereafter with the better-quality tenants (and public housing) having moved into the private sector, the rate of sales declines.

The Thatcher era ended in 1992 and the incoming Labour government in 1997 did not attempt to reverse the policy completely. However, it did tighten eligibility conditions, limit access to publicly provided mortgages and impose caps on the maximum discounts in some areas where sales had diminished the public housing stock quickly (given that local authorities still had a statutory responsibility to house homeless families). However, that government also introduced a similar, but less generous version of RTB known as ‘Right to Acquire’, which allowed tenants in some cases to purchase public housing (typically managed by ‘arms-length’ housing associations and charities rather than directly by local public authorities). This also led to a brief upsurge in sale volumes, as Figure 2 shows, although sales continued anyway as public tenants acquired sufficient years’ residency in their property to be eligible for the RTB scheme.

Not surprisingly, RTB purchases were selective, both by household type and by quality of property. For example, in Derby, a prosperous town in the East Midlands of England which has traditionally specialised in high-end manufacturing since at least the 1920s, over 80% of the large stock of public properties in 1980 were in the form of detached, semi-detached or terraced

¹² The economic incentives implied by these various policies are explored at some length in Disney and Luo (2017), but not considered in detail here.

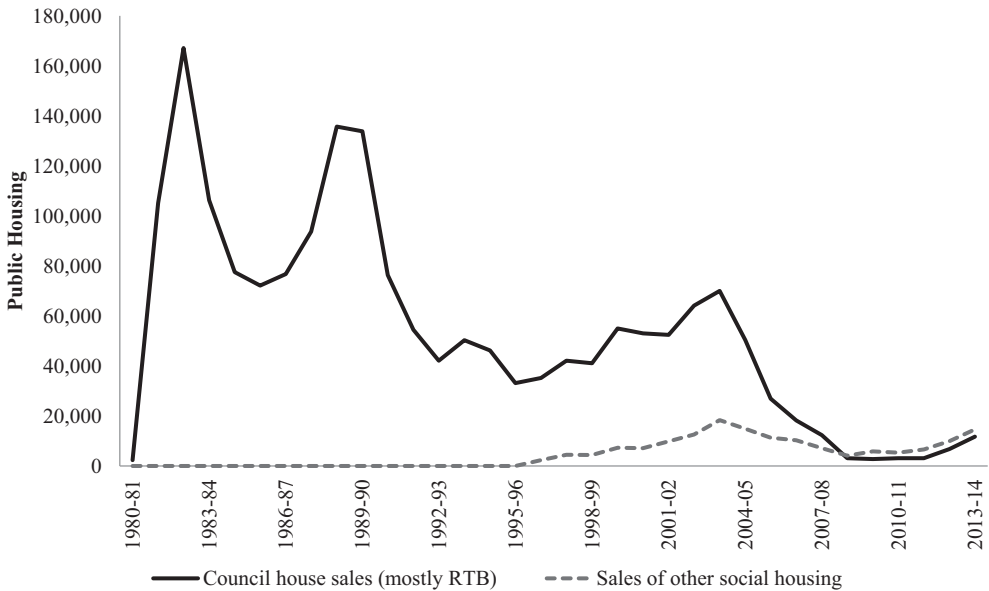


Fig. 2. Right to-Buy and Other Sales of Public Housing in England, 1980–1981 to 2013–2014.

Source: Department for Levelling Up, Housing and Communities and Ministry of Housing, Communities & Local Government (2012, Last Updated in 2023, table 678) and Home Office (2020).

housing in suburbs, and less than 20% in the form of (mostly) high-rise apartments in the inner city. By 1991, 27% of Derby's stock of public housing had been sold off, with apartments now constituting nearly 30% of the residual stock.

In contrast, in Hackney, an inner London borough, in 1980 around 80% of the public housing stock was in apartments, mostly in high-rise estates. By 1991, most of the non-apartment stock in Hackney had been sold off, but the overall stock of public housing had increased because the local council had constructed or purchased further apartments. RTB purchasers themselves were typically older, had higher incomes, and they were less likely to be unemployed (Gregg *et al.*, 2004). Hence, sales of public houses were evidently non-random and related to local crime rates, resulting in a well-known endogeneity issue in the regression analysis that is addressed empirically in Section 3.

2. Data

The empirical analysis combines multiple data sources at annual frequency. Housing data are provided by the Chartered Institute of Public Finance and Accounting (CIPFA) (1981, 1981–2001). The data are provided at the local authority (LA) level for the 314 LAs in England and Wales in 1980.¹³ The data include details of the composition of the residential housing stock (owned, privately rented and public housing) and the number of sales under the RTB scheme in each year. LAs had a statutory requirement to report RTB sales to CIPFA and provide details

¹³ The number of LAs changes over time due to some mergers and due to boundary redrawing. There were 314 LAs in England and Wales in 1980 at the onset of the Right to Buy policy.

of the revenue flow received from the sales. Data on average length of tenure duration in public housing by locality in 1980 are derived from the UK General Household Survey (GHS).

Crime data are sourced from administrative crime records collected by the police and published annually by the UK Home Office on criminal statistics. This period spans five years prior to the introduction of the RTB policy in 1980, through to the end of the period of Thatcher-led Conservative governments in 1992 (Home Office, 1975–1992). These data are provided at the police force area (PFA) geography, a geographic unit that sits above and nests LAs.¹⁴ With the sole exception of London's financial district, commonly known as 'the City of London' and which constitutes an independent PFA, data are used for all 42 PFAs in England and Wales.¹⁵

The measures of crime used in the empirical analysis are the numbers of recorded offences of different crime categories per population. Thus, the data capture incidents of crime recorded and classified by all UK local police forces. The data contain offence rates for five crime categories: property crime, defined as the sum of burglary and theft, and handling of stolen goods offences; and violent crime, defined as the sum of violence against the person, sexual offences, and robbery offences. The LA-level housing data were aggregated at the PFA level using the Office for National Statistics (ONS) Open Geography database, and then joined to the PFA-level crime data. The construction of the short-run panel is not affected by changes in LA geographical boundaries, as in all cases these occur within PFA units. Hence, a balanced panel of 42 PFAs spanning five years prior to the start of the RTB policy in 1980 through to 1992 was obtained, with the PFA-year being the unit of analysis.

Average tenure time for LA renters was calculated from the 1980 GHS, which provides approximately 10,700 observations of LA renters across the 42 PFAs (mean number of observations per PFA is 255). Across localities, average tenure ranges from 8.8 to 17.9 years. Using the same data, the proportion of public housing residents with tenure time of three years or more was also calculated. These GHS data on tenure were matched to the PFA-level data using a GHS cross-walk of regional data subdivided into 'rural', 'urban' and 'mixed urban-rural' areas.

These data sources were complemented with administrative records of local area conditions. In particular, data from the New Earnings Survey (NES) and the Department for Employment were used to calculate local area conditions from 1975 to 1992 in each PFA (Office for National Statistics, 2017a,b,c), while data from the UK Census 1981 were used to calculate local characteristics of the residential stock (e.g., fraction of flats) at the onset of the RTB scheme (Office of Population Censuses and Surveys, Social Survey Division, 1981).¹⁶

2.1. Summary Statistics

Summary data for the composition of the housing stock at the start of the RTB policy are provided in the first seven rows of Table 1. On average, 27.2% of all residential properties in a PFA were publicly owned, equating to approximately 14,000 individual properties per LA, and 7% of all residential properties in a PFA were publicly owned flats. More than 1% of all residential

¹⁴ For example, London LAs sit within the Metropolitan Police PFA.

¹⁵ The City of London constitutes an additional PFA that is independent from the rest of London. However, this PFA is excluded from this analysis because most property in the area is non-residential and consequently few RTB sales took place there.

¹⁶ Additional details of the data used in the empirical analysis and instructions for data access are provided in the Online Appendix.

Table 1. *Summary Data for 1980, Police Force Areas (PFA).*

	Mean	SD	Min.	Max.	Percentiles				
					p. 25	p. 50	p. 75	p. 90	
<i>Housing stock</i>									
Public housing stock (% residential stock)	27.202	8.574	14.568	43.798	21.865	22.966	34.096	41.869	
Public flats stock (% residential stock)	7.000	5.491	0	19.039	3.780	4.732	8.551	19.039	
Public housing sales by 1981 (% residential stock)	1.231	0.410	0.735	2.516	0.910	1.248	1.417	1.680	
Public housing years of tenure	11.052	1.875	8.274	17.936	9.977	11.302	11.721	12.843	
Public housing tenants eligible for RTB (% population)	18.985	6.186	8.903	31.839	14.928	16.672	24.005	28.414	
Public housing units	191,721	177,523	27,612	563,046	71,116	98,794	246,040	563,046	
Residential stock units	725,288	738,428	128,410	2,462,295	301,632	427,384	639,665	2,462,295	
<i>Crime variables</i>									
All crime rate	0.045	0.016	0.020	0.069	0.031	0.038	0.060	0.069	
Property crime rate	0.042	0.015	0.019	0.065	0.030	0.035	0.057	0.065	
Violent crime rate	0.003	0.001	0.001	0.006	0.002	0.003	0.003	0.004	
<i>Covariates</i>									
Log unemployment rate	-2.885	0.357	-3.730	-2.120	-3.219	-2.882	-2.688	-2.435	
Log 25th percentile real hourly earnings	1.345	0.070	1.228	1.497	1.305	1.327	1.360	1.497	
15-24-year-olds (% population)	15.681	0.779	13.780	17.254	15.173	15.792	16.171	16.488	
Employed in manufacturing (% population)	26.103	9.309	0	42.227	19.671	26.525	33.408	35.758	
Observations	42								

Notes: Crime variables are measured as crime rates at the PFA level. Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the PFA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the PFA level. Local area variables displayed are the log of the unemployment rate, the log 25th percentile real hourly earnings, the share of 15-24-year-olds in the population and the share of labour force employed in manufacturing, each measured at the PFA geography.

Source: Authors' calculation from CIPFA, Home Office, ONS and GHS data.

properties were sold under the RTB scheme by 1981, i.e., in the first year of the policy.¹⁷ A key criterion for eligibility to the RTB scheme and for the size of the discount on the market value of public properties was the length of tenure duration in public housing of incumbent tenants.

Table 1 shows that, on average, incumbent tenants in public housing in 1980 had spent 11 years in their properties, with some areas featuring much larger average years of tenure duration than others. The table also shows that, on average, nearly 19% of the resident population in a PFA was eligible for the RTB scheme in 1980. The distribution of the public housing rate, as well as of public housing sales, tenure duration and RTB eligibility rates in 1980, are heterogeneous across PFAs. The data show a quite high standard deviation of public housing stock as a proportion of the residential stock (8.6% SD shown in Table 1) as well as some outlier PFAs—in some PFAs, public housing accounted for more than 40% of the residential housing stock and more than 30% of the resident population was eligible for RTB in 1980, while in other PFAs public housing accounted for less than 15% of the housing stock and less than 10% of the resident population was eligible for RTB.¹⁸

The remaining rows of Table 1 provide summary data for crime rates and other covariates in 1980. Crime rates are measured as recorded cases per individuals. Thus, Table 1 shows that 4.5 criminal offences per 100 individuals were recorded on average in a PFA in 1980. Total crime is defined as the sum of property crime and violent crime, and these measures of crime are the key outcomes of interest here.¹⁹ Property crime, defined as the sum of burglary and theft and handling of stolen goods offences, is overwhelmingly the most common category of crime in 1980. Only the most serious types of violent offences, such as homicide, aggravated assault, sexual offences and robberies, were recorded and published by the Home Office since the 1970s in England and Wales. Minor violent offences only started to be recorded in the UK in the early 2000s. Thus, violent crime is defined here as the sum of serious violence against the person, sexual offences and robbery.

Table 1 also shows four additional variables which are used as covariates in regression models: the local log real hourly wages at the 25th percentile of the distribution, the local log unemployment rate, the local share of 15–24-year-olds and the local share of workers in manufacturing in the population in the PFA. Given that potential offenders are likely to earn low wages and have low levels of labour market attachment (Machin and Meghir, 2004), the 25th percentile of log wages and log unemployment are likely to be relevant features of the labour market in the determination of criminal activity. Finally, Table 1 also suggests that approximately 16% of the overall population is aged 15 to 24 and 26% of the working population is employed in manufacturing. Given that the likelihood to commit crime is observed almost universally to be

¹⁷ The data are for England and Wales. The higher proportion of the public housing stock at the start of RTB for the UK arises from the initial high levels of public houses in Scotland and Northern Ireland.

¹⁸ Figure A2 Panel A in Online Appendix A illustrates the distribution of public housing across LAs in 1980 (expressed as a percentage of the residential housing stock). Panel B illustrates the distribution of public housing in absolute terms and Panel C shows the distribution of residential housing in absolute terms. Similarly, Figure A3 Panel A in Online Appendix A illustrates the distribution of public housing sales across LAs in the 1980s–1990s (expressed as a percentage of the residential housing stock in 1980). Panel B illustrates the distribution of public housing sales in the 1980s–1990s in absolute terms and Panel C shows the distribution of public housing sales across LAs in the 1980s–1990s (expressed as a percentage of the public housing stock in 1980). All distributions in Online Appendix Figures A2 and A3 have a long right tail, further illustrating the uneven distribution of the public housing stock, public housing sales, the total residential stock and the ratios of these across LAs in 1980.

¹⁹ The analysis does not include drugs offences as no data on drugs offences in England and Wales was collected and published by the UK Home Office for the period of this study.

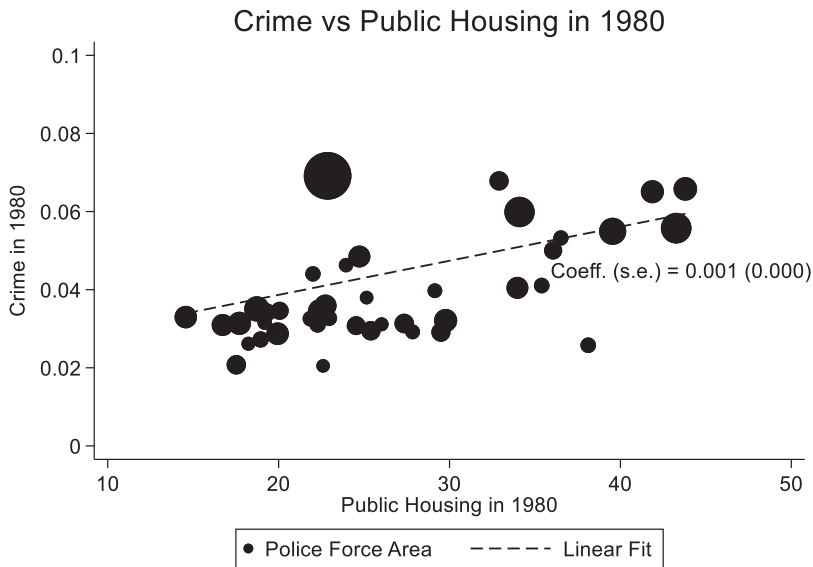


Fig. 3. *Correlation Between Crime and Public Housing in 1980, Police Force Area (PFA).*

Notes: Figure shows for each PFA the crime rate in 1980 plotted against the fraction of public housing as a percentage of the total residential stock in 1980. Crime is defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the PFA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the PFA level. A linear function fitting the correlation between the PFA-specific fraction of public housing and the crime rate weighted by PFA-specific population size in 1980 is also shown.

Source: Authors' calculation from CIPFA and Home Office data.

highest in the late teens and then decrease later in life (Quetelet, 1984; Landersø *et al.*, 2016), the fraction of individuals aged 15 to 24 in the population is also likely to be a relevant determinant of the local criminal activity, while the share in manufacturing is included in the analysis to capture the composition of the local workforce.

3. Empirical Analysis

3.1. *Public Housing and Crime—Initial Conditions*

The analysis of the relationship between density of public housing and crime rates prior to the introduction of the Right to Buy (RTB) policy is the natural starting point of the empirical analysis. Figure 3 shows the relationship between the public housing rate (the percentage share of public housing in the residential housing stock in the PFA) and the crime rate for the 42 PFAs included in the analysis in 1980. There is a clear positive association between the concentration of public housing and crime across PFAs in England and Wales, with the size of the dots on the scatter plot illustrating the PFA's population size in 1980. The positive relationship between the public housing rate and the crime rate in Figure 3 is statistically significant at all conventional levels. Using the same data, an ordinary least squares (OLS) regression of the following form was also estimated:

$$C_i = \alpha + \beta_1 H_i + \beta_2 X_i + \epsilon_i, \quad (1)$$

where C_i is the crime rate in PFA i ; H_i is the public housing stock as a proportion of the residential housing stock; X_i is a vector of PFA-level variables and ϵ_i is the error term. Equation (1) was estimated for total crime, as well as separately for property crime and violent crime.

The estimates are shown in Table 2. The base crime specifications in the table, in columns (1), (3) and (5), shows estimates of crime only as a function of the public housing stock (expressed as a proportion of the residential stock). In accordance with the scatterplot of Figure 3 for total crime, they show that public housing is positively and precisely correlated with total, property and violent crime rates.

Columns (2), (4) and (6) additionally include the stock of public flats (also expressed as a proportion of the residential stock), log unemployment rate in the PFA, log real hourly earnings at the 25th percentile of the distribution within the PFA, the share of 15–24-year-olds in the population and the share of working population employed in manufacturing in the PFA. The coefficient on total crime in column (2) of 0.038 implies that a 0.1 unit increase in the size of the public housing stock measured as a proportion of the residential stock in the PFA in 1980 (approximately a one standard deviation increase) is associated with an increase in the crime rate of 0.0038 units, equating to approximately a 25% of one standard deviation increase in total crime. Both the public housing stock and the stock of public flats are strongly positively correlated with the incidence of total crime, and this positive correlation is robust to the inclusion of additional local area variables in (1). With both housing variables included, the property and violent crime results become more nuanced. Given that public flats are disproportionately concentrated in urban centres, column (4) suggests that property crime is concentrated in areas characterised by high public housing rates both in urban and rural parts of the country, while column (6) indicates that violence is concentrated in urban areas characterised by high public housing rates.

3.2. *Research Design*

The primary object of this study is to quantify the causal impact of homeownership on crime rates. The RTB policy can be interpreted as a relaxation of a supply constraint on available property for homeownership, by allowing public housing tenants to purchase their homes at a subsidy. Of course, this policy does not directly generate a pure natural experiment in observed RTB sales for at least two reasons. First, there may be important time-varying omitted factors that drive both the decision by a tenant to purchase the house and the local crime rate. Second, RTB sales are a result of demand for public housing purchases together with the local supply of housing for sale. With demand being determined, at least in part, by local crime rates, OLS estimates of the relationship between crime rate and public housing sales will suffer from endogeneity bias. It is likely that the decision by the tenant to purchase the house may itself be partly determined by the level and dynamics of local crime. Indeed, unsurprisingly, a negative relationship appears between crime rates and public housing sales in 1980 in Figure A4 in Online Appendix A, further suggesting that RTB sales were not orthogonal to crime rates at the onset of the RTB policy.

3.3. *Empirical Strategy*

The empirical strategy utilises a feature of the eligibility rules governing whether tenants could buy their homes which isolates supply-side variation in exposure to the RTB policy. This variation arises from differences across localities in the potential exposure to RTB sales. The analysis of the

Table 2. *Crime Rate, Public Housing Tenure Length and RTB Eligibility in 1980 OLS Estimates, Police Force Area (PFA).*

	All crime		Property crime		Violent crime		Public housing tenure length		Share of public housing tenants eligible for RTB	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Public housing (prop. res. stock)	0.087 (0.028)	0.038 (0.021)	0.082 (0.026)	0.036 (0.020)	0.005 (0.001)	0.002 (0.002)	0.453 (3.503)	5.864 (3.163)	69.551 (4.336)	70.321 (4.489)
Public flats (prop. res. stock)		0.223 (0.051)		0.213 (0.048)		0.010 (0.005)		-7.841 (7.651)		-7.640 (10.857)
Log unemployment rate		0.012 (0.005)		0.011 (0.005)		0.001 (0.000)		-1.644 (0.722)		-0.542 (1.024)
Log 25th percentile real hourly earnings		-0.004 (0.040)		-0.004 (0.038)		0.000 (0.004)		-6.357 (6.110)		1.957 (8.671)
Fraction 15–24-year-olds in population		0.457 (0.168)		0.437 (0.159)		0.020 (0.015)		-23.903 (25.373)		48.725 (36.005)
Fraction in manufacturing in population		-0.009 (0.022)		-0.009 (0.020)		0.001 (0.002)		1.45 (3.247)		2.278 (4.607)
Observations	42	42	42	42	42	42	42	42	42	42

Notes: Table reports estimates from OLS regression models. Dependent variables are crime rates in columns (1)–(6), years of tenure duration in public housing in columns (7)–(8) and share of public housing tenants eligible for the RTB scheme in columns (9)–(10). Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the PFA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the PFA level. Data units are 42 PFAs in 1980. Standard errors shown in parenthesis.

Source: Authors' calculation from CIPFA, Home Office, ONS and GHS data.

impact of public housing sales on crime defines a series of difference-in-differences specifications whereby the years of tenure duration in public housing of incumbent tenants in 1980 are used to proxy the ITT intensity of the RTB treatment at the PFA level. The national level policy, albeit introduced uniformly across localities in 1980, was specified in such a way that eligibility to the RTB scheme and the size of the discounts varied across localities depending on the average number of years spent in public properties by the incumbent tenants prior to the reform.

The Housing Act of 1980 introduced a statutory right to buy for public tenants with at least three years' tenure duration in public housing. Incentives to RTB increased with tenure duration as discounts on the sale price relative to the market value of the property linearly increased with public housing tenure duration, ranging from 33% for public housing tenants with three years' residence through to a maximum of 50% after 20 years' residence. Thus, the discounts to purchase a public property generated by RTB were directly related with the years of tenure duration in public housing when the RTB scheme was introduced, and years of tenure duration in public housing in 1980 constitutes a pre-determined source of differential exposure to the RTB scheme that could not be gamed or anticipated in 1980.

To test the exogeneity of average tenure in the local authority, in columns (7) and (8) of Table 2, the dependent variable of (1) is switched to average years of tenure duration in public housing in the PFA in 1980. As column (7) shows, the fraction of public housing in the PFA does not predict tenure duration in public housing in 1980. When additional covariates are added in column (8), public housing tenure duration in 1980 appears negatively correlated with local unemployment, suggesting that greater length of tenure duration might be associated with better labour market circumstances. Length of tenure duration might also correlate with the average age of local residents, local turnover and moving rates, and in turn with the local social capital in the PFA. This, in turn, implies that average length of tenure duration per se might correlate with variables that affect crime rates over and above RTB sales.

Columns (9) and (10) focus on the discontinuity of three years of tenancy in public housing in the eligibility rules of the Housing Act of 1980. In columns (9) and (10), the dependent variable measures the share of public tenants with at least three years' tenure duration in public housing who were eligible for the RTB scheme in 1980 (as a percentage of the total population in the PFA). While this variable is positively correlated with the fraction of public housing in the PFA, its distribution across regions and thus RTB eligibility in 1980 is orthogonal to other local area circumstances and the average socio-economic status of individuals. While one might worry that length of tenure duration per se might carry a socio-economic component (as discussed above), there is no reason why the discontinuity at three years of tenancy in public housing should do so too. Combined with the fact that RTB came into force shortly after the 1979 election of the new Thatcher government and people in the 1970s could not freely move across public properties (especially between local authorities), but rather had to apply and join a potentially long queue prior to being able to move in and out of public properties, this makes the distribution of RTB eligibility at the onset of the policy quasi-random.

Variation in tenure time across localities implies that eligibility to the RTB scheme, and in turn intensity of adoption, varied across regions according to the composition of the local housing stock. Public housing sales under the RTB policy began in most LAs in October 1980 (the 1980 Housing Act was passed on 8 August, with most LAs starting to process applications for public housing purchases soon thereafter). Figure 2 shows that the RTB policy resulted in an initial peak in public housing sales from October 1980 to 1982, after which RTB sales continued at a slower

pace. The initial eligibility to the RTB scheme was mainly responsible for this and it is therefore exploited empirically in the econometric analysis.

Given that the discount on the sale price would be repayable if a property was resold within five years of an RTB purchase and the RTB scheme was extended to flats in 1986, the analysis is conducted separately on the first five years of the RTB scheme, i.e., up until 1985, and on the entire Thatcher era, i.e., up until 1992.²⁰ When the analysis is restricted to the first five years of the RTB scheme, both years of tenure duration in public housing of incumbent tenants in 1980 and the share of incumbent tenants eligible for RTB (i.e., the share of tenants with at least three years' tenure duration in public housing in 1980) are also used as instrumental variables (IV) for the actual sales of public housing under the RTB scheme in the first year of the scheme. This empirical approach is not extended to later years because, starting from 1986, the possibility that properties bought under the RTB scheme were resold in the private market cannot be ruled out *ex ante*.

The average tenure instrument correlates with RTB sales. Figure 4 illustrates a strong positive correlation between the average years of tenure duration in public housing in the PFA in 1980 (on the *x*-axis) and the percentage of the residential stock in the PFA which was sold off in the first year of the RTB scheme, i.e., by 1981 (on the *y*-axis). These initial sales of the residential stock in the first year of the RTB scheme are also shown in the Online Appendix Figure A5 to be positively correlated with the percentage sold off between 1980 and 1992 (on the *y*-axis). Therefore, while RTB public housing sales by 1992 may be the endogenous result of the evolution of crime post-1980, and thereby result in reverse causation, the public housing tenure duration in 1980 constitutes an ITT proxy that strongly predicts the actual intensity of the RTB scheme at the PFA level.

For this analysis, data on crime rates, public housing tenure duration and local area are used from 1975 to 1992. The difference-in-differences reduced-form specifications can be expressed as follows:

$$C_{i,t} = \alpha_i + \alpha_t + \beta_1 * (RTB_{i,80} * Post_t) + \beta_2 X_{it} + \epsilon_i, \quad (2)$$

and

$$C_{i,t} = \alpha_i + \alpha_t + \beta_1 * (ELIG_{i,80} * Post_t) + \beta_2 * (RTB_{i,80} * Post_t) + \beta_3 X_{it} + \epsilon_i, \quad (3)$$

where $C_{i,t}$ is the crime rate in PFA i in each year t from 1975 to 1992, α_i is a set of PFA fixed effects and α_t is a set of year fixed effects. $Post_t$ is a dummy variable that takes up value 1 starting from 1980. $RTB_{i,80}$ indicates RTB eligibility in 1980 and it is defined as a continuous variable measuring the average years of tenure duration in public housing in the PFA. $ELIG_{i,80}$ also indicates RTB eligibility in 1980 and, unlike $RTB_{i,80}$, it is defined as a continuous variable measuring the percentage share of tenants with at least three years' tenancy in public housing in 1980. Given that residents became eligible to purchase their properties only after three years of tenure time, the discontinuity in eligibility for the RTB scheme arising after three years of

²⁰ Flats comprise a small share of total RTB sales over the period of the analysis. The share of flats in total RTB sales in the first year the scheme was extended to flat (1986–1987) was 7%, rising to 24% by 1992, then falling to between 18% and 14% in each subsequent year up to the year 2000 (Source: Department for Levelling Up, Housing and Communities' Local Authority Housing Statistics (LAHS) and Pooling of Housing Capital Receipts data return, Table LT.681 last accessed: 27 January 2022.) It should also be noted that among the extensions of the RTB policy in 1985 was a reduction in initial tenure eligibility to two years rather than three (Jones and Murie, 2006). This does not affect the ITT strategy, but is a further reason why the interpretation of IV results beyond 1985 might be compromised. Hence IV estimates focus on the 1975–1985 period.

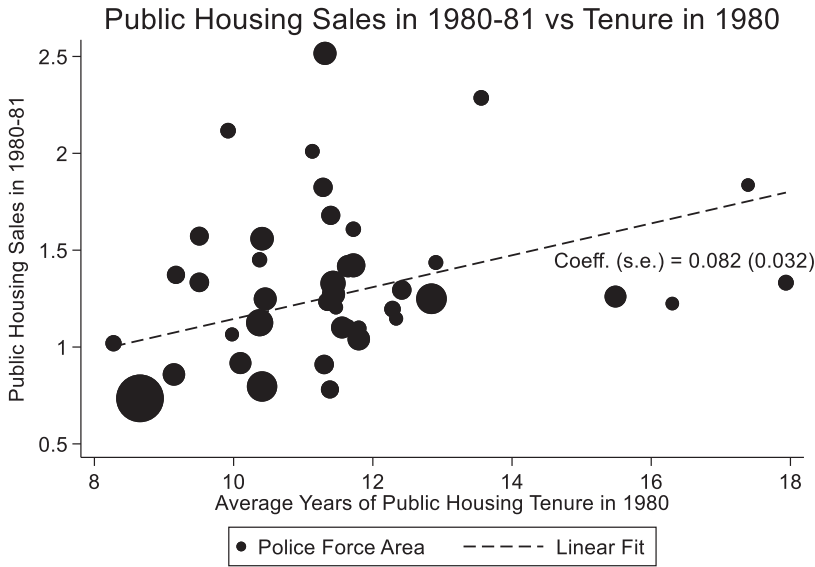


Fig. 4. Correlation Between Right to Buy (RTB) Public Housing Sales by 1981 and Length of Tenure Duration in Public Housing in 1980.

Notes: Figure shows for each PFA the sales of public housing under the RTB scheme by 1981 plotted against PFA-specific average years of tenure duration in public housing in 1980. A linear function fitting the distribution of PFA-specific sales and length of tenure duration weighted by PFA-specific population size in 1980 is also shown. For each PFA in the analysis, the horizontal axis shows the value of average years of tenure duration in public housing in 1980, whereas the vertical axis shows the count of sales of public housing under the RTB scheme by 1981 as a percentage of the total residential stock in 1980.

Source: Authors' calculation from CIPFA and GHS data.

tenure time is exploited in the regression analysis in (3). In this case, identification is obtained from the discontinuity at three years, which determines RTB eligibility but, as shown in Table 2, does not correlate with any other variable that would induce changes in crime in the locality. All regression estimates are weighted by population at the PFA level.

When the analysis is restricted to the first five years of the RTB scheme, a set of IV structural equations are also defined which use as instrumental variables either the interaction between $Post_t$ and $RTB_{i,80}$ in (2) or the interaction between $Post_t$ and $ELIG_{i,80}$ in (3). When the interaction between $Post_t$ and $ELIG_{i,80}$ in (3) is used as instrumental variable, the interaction between $Post_t$ and $RTB_{i,80}$ can be added as a control variable both in the first and second stage of the IV estimation. Thus, depending on the instrument that is used, the IV structural equations can be expressed as follows:

$$C_{i,t} = \alpha_i + \alpha_t + \beta_1 * \widehat{S}_{i,80} + \beta_2 X_{it} + \epsilon_i, \quad (4)$$

where

$$S_{i,80} = \alpha_i + \alpha_t + \theta_1 * (RTB_{i,80} * Post_t) + \theta_2 X_{it} + \epsilon_i, \quad (5)$$

and

$$C_{i,t} = \alpha_i + \alpha_t + \beta_1 * \widehat{S}_{i,80} + \beta_2 * (RTB_{i,80} * Post_t) + \beta_3 X_{it} + \epsilon_i, \quad (6)$$

where

$$S_{i,80} = \alpha_i + \alpha_t + \delta_1 * (ELIG_{i,80} * Post_t) + \delta_2 * (RTB_{i,80} * Post_t) + \delta_3 X_{it} + \epsilon_i, \quad (7)$$

where $S_{i,80}$ are the actual sales of public housing under the RTB scheme in the first year of the scheme in PFA i . In the structural (6) and first stage (7), the interaction between $Post_t$ and $ELIG_{i,80}$ is used as instrumental variable while the interaction between $Post_t$ and $RTB_{i,80}$ is added as a control variable both in the first and second stage of the IV estimation. When the percentage share of tenants with at least three years' tenancy in public housing in 1980 is used for identification, while the average tenure time variable interacted with the $post$ variable is also included as a control variable, identification is obtained from variation in the fraction of residents eligible for the RTB scheme, determined by the 3-year boundary, conditional on the average length of tenure duration of residents in the locality. This specification, while demanding of the data, further controls for variation in tenure duration which correlates with the fraction of residents eligible and, as shown in Table 2, might correlate too with other variables of socio-economic relevance.

Both in the reduced-form equations (2) and (3) and in the structural equations (4) and (6) the main estimand of interest is β_1 , which measures the impact of the initial RTB eligibility-induced shock to homeownership on crime. A negative coefficient associated with β_1 would imply that PFAs that experienced greater RTB eligibility in 1980 experienced lower relative crime rates by 1992 due to the RTB scheme. This parameter is directly of interest to a policymaker wishing to modify homeownership rights for the general population, as tenure duration in public housing was an explicit, pre-determined policy criterion in the Housing Act of 1980 that could not be gamed in the short run and that determined eligibility to the RTB scheme. The coefficient might potentially be biased by RTB affecting the incentives to report crime. However, this is likely to work against finding a crime-reducing effect of homeownership, as homeowners would be more likely to report crime to the police in protection of their owned property.

A causal interpretation of β_1 relies on the absence of differential pre-treatment trends between PFAs exposed to different degrees of RTB eligibility in 1980. Insofar as RTB eligibility in 1980 does not predict crime trends prior to the RTB policy, then β_1 can be interpreted as the causal impact of the RTB policy on crime. The potential presence of differential pre-treatment crime trends between PFAs that experienced different degrees of RTB eligibility is tested and results are presented below. Finally, X_i is a vector of local area controls measured in 1980 and interacted with the $Post_t$ variable, which includes the fraction of public flats in the PFA, a dummy for whether at least one-third of the housing stock was public housing in the PFA, the log unemployment rate in the PFA, the log real hourly earnings at the 25th percentile of the distribution within the PFA, the share of 15–24-year-olds in the population in the PFA and the share of workers in manufacturing in the PFA, while ϵ_i indicates the error terms.

Due to the number of clusters (42 PFAs), p -values were derived for inference from Wild Cluster Bootstrap estimation with standard errors clustered at the PFA level. With the Wild Cluster Bootstrap, the usual approach, followed here, is to report p -values (as opposed to standard errors). This is because the Wild Cluster Bootstrap allows for non-normality in the empirical distribution, and thus standard errors do not permit statistical inference from classical null hypothesis tests (see Cameron *et al.*, 2008, for further details).

3.4. Main Results

Table 3 presents unconditional difference-in-differences estimates based on a dichotomous vari-

Table 3. *Difference-in-Differences Before and After the Right to Buy Scheme.*

	Intention-to-treat (ITT) treatment PFAs (length of public housing tenure duration in 1980 > = p. 50) (1)	Intention-to-treat (ITT) control PFAs (length of public housing tenure duration in 1980 < p. 50) (2)	Difference ((1)-(2)) (3)	Unconditional DiD (4)	Unconditional DiD % effect ((4)/pre (2)) (5)
<i>Panel A. All crime, 1975-1985</i>					
Pre	0.034	0.047	-0.013		
Post	0.043	0.060	-0.017		
Post-pre	0.009	0.013		-0.004 (<i>p</i> = 0.085)	-8.5%
Number of police force areas	21	21			
Number of observations	231	231			
<i>Panel B. All crime, 1975-1992</i>					
Pre	0.034	0.047	-0.013		
Post	0.052	0.070	-0.018		
Post-pre	0.018	0.023		-0.005 (<i>p</i> = 0.072)	-10.6%
Number of police force areas	21	21			
Number of observations	378	378			

Notes: Post-period defined as starting from 1980, the date of adoption of the RTB scheme in England and Wales. Panel A shows results for the 1975-1985 period and Panel B shows results for the 1975-1992 period. Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the PFA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the PFA level. Averages weighted by population at the PFA level. Standard errors were clustered at the PFA level and *p*-values from wild cluster bootstrap are reported in parenthesis. Percentage effect calculated as estimated coefficient divided by mean of dependent variable prior to 1980 in the ITT control group PFAs (length of public housing tenure duration in 1980 < p. 50).

Source: Authors' calculation from Home Office, ONS and GHS data.

able indicating if average years of public housing tenure duration in PFA i were greater than or equal to the median value of average years of public housing tenure duration at the national level in 1980. This analysis is purely descriptive as it shows a non-causal difference-in-differences calculation of changes in headline crime rates. In particular, Panel A shows results for the 1975–1985 period, when property resales should have been extremely rare (given that the discount on the sale price would be repayable), and Panel B shows results for the 1975–1992 period. In the calculations, PFAs are split into two groups by the magnitude of $RTB_{i,80}$, in which the above-or-equal-to-median group is described as the ‘ITT treatment’ group and the below median group is described as the ‘ITT control’ group.

Columns (1) and (2) state the pre-1980 average crime rate in the PFA, the post-1980 average crime rate in the PFA and the within-PFA post–pre difference. Column (3) shows the pre-1980 and post-1980 differences between the ITT treatment and ITT control PFAs. Column (4) shows the unconditional difference-in-differences estimate and column (5) shows the unconditional difference-in-differences estimate deflated by the mean level of crime in the ITT control group prior to 1980 in percentage terms (the unconditional percentage effect). The unconditional difference-in-differences coefficient estimate is negative and precisely defined in both Panels A and B. On average, PFAs that experienced RTB eligibility greater or equal to the national median in 1980 experienced overall crime rates that were 8.5% lower from 1980 to 1985 and roughly 11% lower from 1980 to 1992.

Table 4 presents estimates based on (2), (4) and (5) where the ITT is defined by the average years of tenure duration in public housing in the PFA. Panel A shows results for total crime, while Panels B and C break down total crime by property crime and violent crime. For all outcomes, columns (1) and (2) show ITT results for the 1975–1985 period, with column (2) including a richer set of controls for local area circumstances in 1980. Columns (3) and (4) show IV results for the 1975–1985 period, and columns (5) and (6) show ITT results for the 1975–1992 period, with the latter column including the richer set of controls in each case.

All estimated specifications include PFA fixed effects, year fixed effects, and the interaction between the *post* variable and local area variables measured in 1980 (i.e., fraction of public flats, whether at least one-third of the residential stock is public housing, log of the unemployment rate and log of real hourly earnings at the 25th percentile of the distribution). In Table 4, columns (2), (4) and (6), the *post* variable is also interacted with the fraction of 15–24-year-olds in the population in 1980 and the fraction of workers in manufacturing in 1980.

In Table 4, columns (1) and (2), Panels A and B show negative and statistically significant estimates, suggesting that the conclusion from Table 3 that RTB eligibility led to a reduction in crime in the 1980s and early 1990s is robust to the equation specification. In particular, estimates in column (2) indicate that one additional year of average tenure length in public housing in 1980 led to a 0.48% reduction in crime and to a 0.56% reduction in property crime in the first five years of the RTB scheme.

As shown in Table 1, the average length of tenure in public housing in 1980 was 11.052 years and thus a one year increase corresponds to a 9.05% increase from the mean. Therefore, estimates in Panel A uncover an elasticity of crime with respect to years of tenure duration in public housing of approximately -0.053 , implying that a 10% increase in average years of tenure duration in public housing prior to 1980 coincided with a crime reduction by around 0.53% within the first five years of the RTB scheme. Similarly, estimates in Panel B uncover an elasticity of property crime with respect to years of tenure duration in public housing of approximately -0.062 , implying that a 10% increase in average years of tenure duration in public housing prior

Table 4. *Estimates of Impact of Right to Buy (RTB) Scheme on Crime using Public Housing Tenure Length as ITT.*

	ITT, 1975–1985		IV, 1975–1985		ITT, 1975–1992	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Total crime</i>						
(RTB sales in 1980–1981) × post (× 100)			–0.111 (<i>p</i> = 0.076)	–0.174 (<i>p</i> = 0.037)		
Tenure length × post (× 100)	–0.013 (<i>p</i> = 0.084)	–0.020 (<i>p</i> = 0.062)			–0.107 (<i>p</i> = 0.028)	–0.117 (<i>p</i> = 0.036)
Percentage effect		–0.481%	–2.671%	–4.187%	–2.575%	–2.815%
Mean dep. var. in 1975–1979 (× 100)	4.156	4.156	4.156	4.156	4.156	4.156
<i>Panel B. Property crime</i>						
(RTB sales in 1980–1981) × post (× 100)			–0.127 (<i>p</i> = 0.084)	–0.187 (<i>p</i> = 0.048)		
Tenure length × post (× 100)	–0.015 (<i>p</i> = 0.101)	–0.022 (<i>p</i> = 0.092)			–0.102 (<i>p</i> = 0.033)	–0.112 (<i>p</i> = 0.047)
Percentage effect		–0.562%	–3.241%	–4.773%	–2.603%	–2.859%
Mean dep. var. in 1975–1979 (× 100)	3.918	3.918	3.918	3.918	3.918	3.918
<i>Panel C. Violent crime</i>						
(RTB sales in 1980–1981) × post (× 100)			0.017 (<i>p</i> = 0.186)	0.013 (<i>p</i> = 0.296)		
Tenure length × post (× 100)	0.002 (<i>p</i> = 0.148)	0.002 (<i>p</i> = 0.214)			–0.005 (<i>p</i> = 0.113)	–0.005 (<i>p</i> = 0.150)
Percentage effect		0.840%	7.143%	5.462%	–2.101%	–2.101%
Mean dep. var. in 1975–1979 (× 100)	0.238	0.238	0.238	0.238	0.238	0.238
PFA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic local area circumstances in 1980 × post	Yes	Yes	Yes	Yes	Yes	Yes
Full local area circumstances in 1980 × post	No	Yes	No	Yes	No	Yes
First-stage coefficient on ITT			0.115 (<i>p</i> = 0.008)	0.116 (<i>p</i> = 0.004)		
Sample size	462	462	462	462	756	756
Number of PFAs	42	42	42	42	42	42

Notes: Post-period defined as starting from 1980, the date of adoption of the RTB scheme in England and Wales. Columns (1)–(4) show ITT and IV results for the 1975–1985 period and columns (5)–(6) show ITT results for the 1975–1992 period. Length of tenure duration in public housing measured in years and calculated in 1980. Sales of public housing calculated in 1980–1981. Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the PFA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the PFA level. Regressions weighted by population at the PFA level. Standard errors were clustered at the PFA level and *p*-values from wild cluster bootstrap are reported in parenthesis. Percentage effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable. Covariates for local area circumstances are measured in 1980 and they include: log unemployment rate, log 25th percentile earnings, fraction of 15–24-year-olds in population, fraction of public housing, fraction of flats and share of labour force in manufacturing.

Source: Authors' calculation from CIPFA, Home Office, ONS and GHS data.

to 1980 coincided with a property crime reduction by around 0.62% within the first five years of the RTB scheme.

Columns (3) and (4) show the IV estimates for the period 1975–1985, and Panels A and B display negative, significant and indeed similar crime–sales elasticities. In these specifications, the actual public housing sales under the RTB scheme in the first year of the policy, i.e., in 1980–1981, are instrumented using the average years of tenure duration of incumbent tenants in public housing in 1980, using sets of controls which mirror those in columns (1) and (2). The results show positive and precise first-stage estimates of the impact of average years of tenure duration in public housing prior to 1980 on RTB sales.

The resulting IV estimates are negative and statistically significant both in Panels A and B, indicating that a one percentage point increase in the RTB sales of public housing by 1981 led to a 2.7% to 4.2% reduction in crime and to a 3.2% to 4.8% reduction in property crime by 1985 (depending on specification). Given that, as shown in Table 1, roughly 1.2% of the residential stock was sold under the RTB scheme in 1980–1981, a one percentage point increase in RTB sales corresponds to an 81.2% increase from the mean. Thus, these IV estimates uncover an elasticity of crime with respect to RTB sales of approximately -0.052 and an elasticity of property crime with respect to RTB sales of approximately -0.059 , implying that a 10% increase in the RTB public housing sales reduced crime by around 0.52% and property crime by around 0.59% by 1985.

In the remaining columns (5) and (6), the analysis is extended up until 1992, the year when the Thatcher era ended. Columns (5) and (6) also show negative and statistically significant estimates in Panels A and B, uncovering an elasticity of crime with respect to years of tenure duration in public housing of roughly -0.311 and an elasticity of property crime with respect to years of tenure duration in public housing of roughly -0.316 , implying that a 10% increase in eligibility to the RTB scheme coincided with a crime reduction by around 3.11% and with a property crime reduction by nearly 3.16% by 1992. In Table 4, none of the ITT or IV estimates displayed in Panel C indicates that the RTB scheme affected violent crime.

Table 5 presents estimates of (3), (6) and (7) in which eligibility to RTB is expressed as the fraction of residents eligible to purchase their home as determined by the three-year eligibility rule and the average tenure length is added as a control variable. Table 5 is organised similarly to Table 4, as Panel A shows results for total crime while Panels B and C show results for property crime and violent crime separately. For all outcomes, columns (1)–(3) show ITT results for the 1975–1985 period, columns (4)–(6) show IV results for the 1975–1985 period and columns (7)–(9) show ITT results for the 1975–1992 period. All estimated specifications in columns (1) and (2), (4) and (5), (7) and (8) include the same varying sets of controls for local area circumstances in 1980 as in Table 4, with the last column including the richer set of controls in each case. Columns (3), (6) and (9) additionally control for the interaction between the *post* variable and the average years of tenure duration in public housing in the PFA, hence identifying the effect from the discontinuity in eligibility for the RTB scheme arising at three years conditional on average tenure time in the locality. In all specifications, standard errors were clustered again at the PFA level and Wild Cluster Bootstrap techniques were used again for inference due to the small number of clusters.

In columns (1)–(3), Panels A and B show again negative and statistically significant estimates, suggesting that the conclusion that RTB eligibility led to a reduction in crime in the 1980s and early 1990s is robust to the definition of the ITT status. In particular, estimates in column (3) indicate that a one percentage point increase in the share of tenants with at least three years'

Table 5. *Estimates of Impact of Right to Buy (RTB) Scheme on Crime Using Share of Public Housing Tenants Eligible for RTB as ITT.*

	ITT, 1975–1985			IV, 1975–1985			ITT, 1975–1992		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A. Total crime</i>									
(RTB sales in 1980–1981) × post (× 100)				-0.196 (<i>p</i> = 0.050)	-0.299 (<i>p</i> = 0.006)	-0.343 (<i>p</i> = 0.044)			
Fraction eligible × post (× 100)	-0.014 (<i>p</i> = 0.044)	-0.025 (<i>p</i> = 0.007)	-0.028 (<i>p</i> = 0.035)				-0.060 (<i>p</i> = 0.023)	-0.079 (<i>p</i> = 0.003)	-0.075 (<i>p</i> = 0.017)
Percentage effect	-0.337%	-0.602%	-0.674%	-4.716%	-7.194%	-8.253%	-1.444%	-1.901%	-1.805%
Mean dep. var. in 1975–1979 (× 100)	4.156	4.156	4.156	4.156	4.156	4.156	4.156	4.156	4.156
<i>Panel B. Property crime</i>									
(RTB sales in 1980–1981) × post (× 100)				-0.209 (<i>p</i> = 0.036)	-0.302 (<i>p</i> = 0.004)	-0.342 (<i>p</i> = 0.027)			
Fraction eligible × post (× 100)	-0.015 (<i>p</i> = 0.035)	-0.025 (<i>p</i> = 0.005)	-0.028 (<i>p</i> = 0.023)				-0.058 (<i>p</i> = 0.015)	-0.076 (<i>p</i> = 0.002)	-0.073 (<i>p</i> = 0.009)
Percentage effect	-0.383%	-0.638%	-0.715%	-5.334%	-7.708%	-8.729%	-1.48%	-1.94%	-1.863%
Mean dep. var. in 1975–1979 (× 100)	3.918	3.918	3.918	3.918	3.918	3.918	3.918	3.918	3.918
<i>Panel C. Violent crime</i>									
(RTB sales in 1980–1981) × post (× 100)				0.013 (<i>p</i> = 0.672)	0.003 (<i>p</i> = 0.467)	-0.001 (<i>p</i> = 0.793)			
Fraction eligible × post (× 100)	0.001 (<i>p</i> = 0.657)	0.000 (<i>p</i> = 0.448)	-0.000 (<i>p</i> = 0.788)				-0.002 (<i>p</i> = 0.399)	-0.003 (<i>p</i> = 0.301)	-0.003 (<i>p</i> = 0.614)
Percentage effect	0.42%	0%	0%	5.462%	1.261%	-0.42%	-0.84%	-1.261%	-1.261%
Mean dep. var. in 1975–1979 (× 100)	0.238	0.238	0.238	0.238	0.238	0.238	0.238	0.238	0.238
PFA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Basic local area circumstances in 1980 × post	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full local area circumstances in 1980 × post	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Tenure length × post	No	No	Yes	No	No	Yes	No	No	Yes
First-stage coefficient on ITT				0.074 (<i>p</i> = 0.003)	0.084 (<i>p</i> = 0.005)	0.082 (<i>p</i> = 0.007)			
Sample size	462	462	462	462	462	462	756	756	756
Number of PFAs	42	42	42	42	42	42	42	42	42

Notes: Post-period defined as starting from 1980, the date of adoption of the RTB scheme in England and Wales. Columns (1)–(4) show ITT and IV results for the 1975–1985 period and columns (5)–(6) show ITT results for the 1975–1992 period. Eligible tenants in public housing measured in 1980. Sales of public housing calculated in 1980–1981. Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the PFA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the PFA level. Regressions weighted by population at the PFA level. Standard errors were clustered at the PFA level and *p*-values from wild cluster bootstrap are reported in parenthesis. Percentage effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable. Covariates for local area circumstances are measured in 1980 and they include: log unemployment rate, log 25th percentile earnings, fraction of 15–24-year-olds in population, fraction of public housing, fraction of flats and share of labour force in manufacturing.

Source: Authors' calculation from CIPEA, Home Office, ONS and GHS data.

residency in public housing in 1980 led to a 0.67% reduction in crime and to a 0.71% reduction in property crime in the first five years of the RTB scheme. As shown in Table 1, on average 18.99% of the population in a PFA had at least three years' residency in public housing and, thus, was eligible for the RTB scheme in 1980. Therefore, a one percentage point increase in the share of eligible tenants for RTB corresponds to a 5.27% increase from the mean, and so estimates in Panel A uncover an elasticity of crime with respect to RTB eligibility of approximately -0.127 . This implies that a 10% increase in the share of eligible tenants for the RTB scheme in 1980 coincided with a crime reduction by around 1.27% within the first five years of the RTB scheme. Similarly, estimates in Panel B uncover an elasticity of property crime with respect to years of tenure duration in public housing of roughly -0.135 , implying that a 10% increase in the share of eligible tenants for the RTB scheme in 1980 coincided with a property crime reduction by around 1.35% by 1985.

Columns (4)–(6) show the IV estimates for the period 1975–1985, and Panels A and B display again negative, significant and coherent crime-sales elasticities. In these specifications, the actual public housing sales under the RTB scheme in the first year of the policy, i.e., in 1980–1981, are instrumented using the share of eligible tenants for the RTB scheme in 1980. The results show positive and precise first-stage estimates, indicating that the share of eligible tenants for the RTB scheme in 1980 was in fact a strong predictor of the actual RTB sales in 1980–1981.

Also in this case, the resulting IV estimates appear negative and statistically significant both in Panels A and B, indicating that a one percentage point increase in the RTB sales of public housing by 1981 led to a 4.7% to 8.2% reduction in crime and to a 5.3% to 8.7% reduction in property crime by 1985 (depending on specification). As shown in Table 1, roughly 1.2% of the residential stock was sold under the RTB scheme in 1980–1981, and so a one percentage point increase in RTB sales corresponds to an 81.2% increase from the mean. Thus, these IV estimates uncover an elasticity of crime with respect to RTB sales of approximately -0.101 and an elasticity of property crime with respect to RTB sales of approximately -0.107 , implying that a 10% increase in the RTB public housing sales reduced crime by around 1% and property crime by nearly 1.1% by 1985.

The remaining columns (7)–(9) show results when the analysis is extended up until 1992, the year when the Thatcher era ended. Columns (7)–(9) also show negative and statistically significant estimates in Panels A and B, uncovering an elasticity of crime with respect to RTB eligibility of roughly -0.342 and an elasticity of property crime with respect to RTB eligibility of roughly -0.353 , implying that a 10% increase in the share of eligible tenants to the RTB scheme coincided with a crime reduction by around 3.42% and with a property crime reduction by about 3.53% by 1992. Also in Table 5, none of the ITT or IV estimates displayed in Panel C indicate that the RTB scheme affected violent crime.

Figure 5 shows the event study analogue to the specification in column (8), while the Online Appendix Figure A6 does likewise for the specification in column (9) of Table 5. Both figures plot the estimated coefficients from the ITT model for all years for the period 1975–1992, and 95% confidence intervals were estimated again for inference using Wild Cluster Bootstrap techniques due to the small number of clusters. The visual inspection of the figures reveals that crime rates were on similar trends prior to 1980 in ITT treatment and ITT control PFAs, as evidenced by a set of numerically small and statistically insignificant estimated interactions between all the years prior to the RTB scheme and the share of eligible tenants for the RTB scheme. Figure 5 and the Online Appendix Figure A6 also show that the estimated interactions between the share of eligible tenants for the RTB scheme and the years following the introduction of RTB scheme

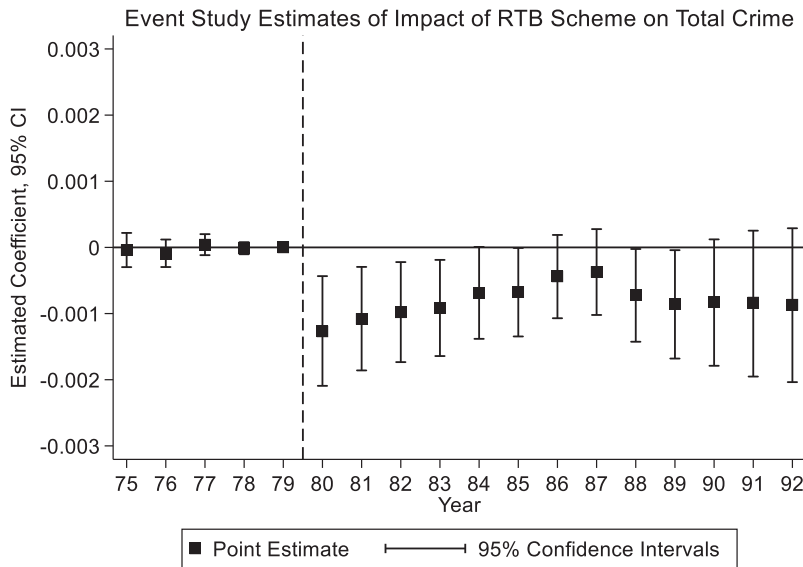


Fig. 5. Event Study ITT Estimates of Impact of Right to Buy (RTB) Scheme on Crime.

Notes: Post-period defined as starting from 1980, the date of adoption of the RTB policy in England and Wales. Figure shows ITT results for the 1975–1992 period. Fraction of eligible tenants in public housing measured in 1980. Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Estimated regressions include PFA fixed effects, year fixed effects and interactions between covariates for local area circumstances and the post-period respectively. Covariates for local area circumstances are measured in 1980 and they include: log unemployment rate, log 25th percentile earnings, fraction of 15–24-year-olds in population, fraction of public housing, fraction of flats and share of labour force in manufacturing. Regressions weighted by population at the PFA level. Standard errors were clustered at the PFA level and the 95% confidence intervals from wild cluster bootstrap were estimated for inference and are displayed in the figure.

Source: Authors' calculation from CIPFA, Home Office, ONS and GHS data.

in 1980 are negative and statistically significant. Therefore, Figure 5 and the Online Appendix Figure A6 corroborate the causal interpretation of the results in Table 5 and confirm that the RTB scheme caused a reduction in crime. Finally, Online Appendix Table A1 shows the log–log analogue to the specification in column (8) of Table 5, and a crime-reducing impact of the RTB scheme appears also in this case, with the effect driven by a reduction in property crime.

Throughout these estimates, the magnitude of the crime reduction caused by the RTB scheme by 1992 appears consistent across alternative specifications, alternative definitions of the ITT status and over time. In the early years of the RTB scheme, the IV estimates presented in Tables 4 and 5 imply that a 10% increase in the RTB public housing sales reduced crime by nearly 0.5–1% and property crime by around 0.6–1.1% by 1985.

Up until 1985, no one who purchased a property under the RTB scheme could have resold it while continuing to benefit from the large discounts offered by the Thatcher-led government under the RTB scheme, as explained above. Thus, crime reductions from 1980 to 1985 are very unlikely to result from the practice of reselling properties previously bought under the RTB scheme to incomers that are more affluent. If the 'migration-based' gentrification of certain areas of the country was the underlying mechanism driving these crime reductions, significance in the

estimates should not appear until the late 1980s, when the first properties bought under the RTB scheme in 1980 could be resold without having to renounce the RTB discounts. However, this is not what is found here. A crime reduction appears in the early years of the RTB scheme, a result that is consistent with behavioural changes within the local communities that were more exposed to the provisions of the scheme. Estimates up until 1992 imply that a 10% increase in the share of eligible tenants to the RTB scheme led to a crime reduction by 1992 by around 3.1–3.4% and to a property crime reduction by around 3.2–3.5% by 1992. Thus, greater eligibility to the RTB scheme led to a lasting crime reduction that was still visible at the end of the Thatcher era when the analysis terminates.

3.5. *Political Colour, Labour Markets, Local Amenities and Police Deployment*

Why did greater eligibility to the RTB scheme lead to a detectable reduction in crime? The mechanisms underlying the findings in Tables 3–5 and Figure 5 were investigated through a variety of statistical tests. First, one may worry that this analysis is spuriously picking up some other PFA-specific characteristics such as the political affiliation of the local administration. If, for example, pro-Thatcher Conservative-controlled LAs were more willing to fulfil their statutory responsibility to accelerate RTB sales through faster processing (or, potentially, be more likely to advertise and encourage the possibility locally) and also more enthusiastic in their methods to clamp down on property crime, then this omitted confounding variable might weaken the predictive power of public housing tenure duration and co-determine RTB sales and crime. Although RTB sales were only used in the IV estimates and not in the ITT reduced-form estimates of (2) and (3) presented in Tables 4 and 5, one may worry that the de facto supply-side availability of public housing for RTB sales might have differed by local political control.

To test this, data on the political ‘complexion’ of the PFA in the local elections in 1977, the latest local elections prior to the election of Margaret Thatcher as UK prime minister in 1979, were used. However, a scatter plot suggests no correlation between the Conservative vote share within the PFA and RTB sales from 1980 to 1992 as a percentage of the residential stock in 1980 (see Online Appendix A, Figure A7). Estimates of the impact of eligibility to the RTB scheme on crime were also produced when a set of interactions between the Conservative vote share within the PFA in the 1977 local elections and year fixed effects were added to the econometric specification.²¹ Results are unchanged with the inclusion of this additional variable, which suggests that the results are not caused by Labour controlled LAs opposing this policy for political reasons while facing greater local crime rates. This holds true regardless of whether local area variables are included in the analysis.

An additional concern may be that the reduction in crime rates observed in areas with greater RTB eligibility in 1980 may reflect some underlying trends in local labour markets. If, for example, incumbent tenants spent more years in public housing prior to 1980 in areas that faced more favourable labour market prospects, thus facing greater discounts under the RTB scheme, and these same areas then faced reduced criminality thanks to the improved labour market circumstances, then the reduction in crime observed in Tables 3–5 and Figure 5 would be erroneously attributed to the RTB scheme. More generally, as discussed above one may worry that the average length of tenure duration in public housing in 1980 may carry a socio-economic

²¹ The Conservative vote share is the share of Conservative votes in all votes cast in the parliamentary constituencies nested within the PFA.

component. Length of tenure duration might correlate with the average age of local residents, local turnover and moving rates, and in turn with the local social capital in the PFA.

Models to test for the effects of the average length of tenure duration in public housing in 1980 on local labour market conditions were therefore estimated and results are displayed in Table 6. Whether the unemployment rate, the 25th percentile real hourly earnings or the 50th percentile real hourly earnings are modelled as dependent variable, no evidence appears that average length of tenure duration in public housing in 1980 predicted the evolution of local labour market conditions in the 1980s and early 1990s. These results mitigate the concern that the exclusion restriction may not be tenable in Table 4 and that estimates might be picking up the effect of other local area circumstances that interacted with public housing sales over time in the 1980s.

Average length of tenure duration in public housing in 1980 does not even seem to predict the local fraction of 15–24-year-olds in the population in the 1980s and early 1990s, a relevant finding that further suggests that migration and a compositional change in the local population do not seem to be the key mechanisms at play here. The same conclusion appears if the dependent variable is modified to measure the local fraction of 15–29-year-olds in the population, the local fraction of 15–29-year-old males in the population or the local fraction of 15–29-year-old females in the population. If a ‘migration-based’ gentrification was behind the main results of this paper, RTB eligibility would predict the composition of the local population in the 1980s and 1990s. If, for example, all RTB purchasers had rented out their properties to students right after purchasing them (and this was the true driver of the crime reduction in Tables 4 and 5), then RTB eligibility should be a positive predictor of the fraction of 15–24-year-olds or 15–29-year-olds in the population. However, this is not what is found here.

One further concern may be that initial RTB eligibility positively correlates with the quality of local amenities across regions in England and Wales. The initial uptake of the RTB scheme may have been greater in areas with ‘better-quality’ public housing. Similarly, one may worry that in 1980 only the ‘well-off’ public tenants were able to exploit the RTB scheme and purchase their houses, while ‘the very poor’ were left behind. Both these scenarios would imply that, in the short-run analysis, initial RTB sales may be picking up other relevant socio-economic components of communities. If initial RTB sales were concentrated among better-off areas or individuals, the crime-reducing effect that is observed may not be due to the sales of public housing, but rather due to not comparing like with like.

To test for the possibility that RTB sales grew faster in areas with greater-quality public housing, the main model was re-estimated and the treatment variable replaced with the *average value* of RTB sales in the first year of the RTB scheme in place of the eligibility to the RTB scheme in 1980. The result of this exercise is shown in column (8) of Table 6. When crime is regressed on the average value of local RTB transactions in the first year of the RTB scheme, the estimated coefficient appears small in magnitude and very statistically insignificant. Thus, unlike RTB eligibility, the value of RTB sales does not predict the evolution of crime. The initial eligibility to RTB sales, not their average value, predicts the crime reduction since 1980.

Finally, one may worry that the RTB scheme may have coincided with differential policing strategies across regions and that this may have resulted in differential crime detection rates across regions. If, for example, fewer police officers were deployed in areas with greater RTB sales in 1980, the findings may reflect the lower crime detection rates of the police in these areas rather than a genuine reduction in crime. Availability of data on the number of officers employed in each PFA since 1975 allowed us to estimate the impact of RTB eligibility on police deployment. This is a further test of whether indeed the RTB scheme made some areas of the country safer, or

Table 6. *Estimates of Impact of Public Housing Tenure Length on Local Area Outcomes and Value of Right to Buy (RTB) Sales on Crime, 1975–1992.*

	Log unemployment rate (1)	Log 25th percentile real hourly earnings (2)	Log 50th percentile real hourly earnings (3)	Fraction 15–24-year-olds in population (4)	Fraction 15–29-year-olds in population (5)	Fraction 15–29-year-old males in population (6)	Fraction 15–29-year-old females in population (7)	Total crime (8)	Count of police officers (9)
Tenure length × post	-0.034 (<i>p</i> = 0.356)	-0.011 (<i>p</i> = 0.372)	-0.011 (<i>p</i> = 0.427)	-0.000 (<i>p</i> = 0.186)	-0.003 (<i>p</i> = 0.171)	-0.002 (<i>p</i> = 0.183)	-0.002 (<i>p</i> = 0.165)	-0.000 (<i>p</i> = 0.986)	-0.000 (<i>p</i> = 0.218)
Value RTB sales × post									
Percentage effect	Yes	Yes	Yes	-0%	-1.376%	-1.802%	-1.869%	-0%	-0%
PFA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	756	756	756	756	756	756	756	756	756
Number of PFAs	42	42	42	42	42	42	42	42	42

Notes: Post-period defined as starting from 1980, the date of adoption of the RTB scheme in England and Wales. Length of tenure duration in public housing measured in years and calculated in 1980. Value of sales of public housing calculated by 1981. Local area outcomes measured at the PFA level. Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the PFA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the PFA level. Regressions weighted by population at the PFA level. Standard errors were clustered at the PFA level and *p*-values from wild cluster bootstrap are reported in parenthesis. Percentage effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable, except in columns (1)–(3) where the dependent variable is measured in log values.

Source: Authors' calculation from CIPFA, Home Office, ONS and GHS data.

whether it coincided with a decrease in police deployment. To examine this idea, the dependent variable in the difference-in-differences specification was replaced with the number of police officers deployed by the PFA. Results are displayed in column (9) of Table 6, and the coefficient of interest is economically small and not statistically significant, suggesting that the greater exposure to the RTB scheme of some PFAs did not coincide with differential policing strategies. In turn, this mitigates the concern that differential policing strategies and crime detection rates across regions may have coincided with the RTB scheme, and further suggests that the main results reveal the genuine reduction in criminality generated by the RTB scheme.

3.6. Homeownership and Behavioural Change

Did the RTB policy induce local communities to refurbish their properties? One possibility is that home upgrading by purchasers under the RTB scheme explains the observed reduction in crime (absent any clear migration, labour market or amenity differential between areas that were exposed to differing degrees to the RTB policy). The British Crime Survey (BCS) (UK Data Service, 1988) contains a rich set of variables concerning homeownership and crime.²² Based on the information contained in the BCS 1988, it was possible to define a treatment group of incumbent tenants who had previously rented from the council and then subsequently purchased the property in which they currently reside, and a control group of council rental tenants who stated that they currently intend to buy the public property in which they reside within two years. This provides a natural comparison group against which to estimate the effects of purchase on behavioural change to build at least strongly suggestive evidence for the effects of the policy. Online Appendix A Table A2 shows that these treatment group individuals and control group individuals are observably indistinguishable in terms of age, gender, ethnicity, income, number of rooms in the household, household type (e.g., flat or house) and past experiences of burglary victimisation (Online Appendix A Table A2). For each of these individual and household characteristics, Table A2 presents a balancing test that retrieves an insignificant estimate, thus showing the suitability of the comparison group.

Table 7 shows a comparison of home improvement expenditure, insurance and social behaviour between these two groups of observably similar individuals. Panel A shows unconditional OLS estimates, thus providing a simple comparison of the means between these two groups. OLS estimates in Panel B were derived controlling for the set of variables for which balancing was tested in Online Appendix Table A2, i.e., gender, ethnicity, a quadratic of age, past experiences of burglary victimisation in the property of residence, type of property (e.g., flat or house), number of rooms in the property, household income band and county fixed effects. Thus, while self-selection into the treatment group is a possibility, controlling for this large set of observables and restricting the treatment and control groups to incumbent individuals that only seem to differ in their *timing* of willingness to purchase their property from the council within two years should mitigate the concern that self-selection drives the results. To be consistent with all other results at PFA level, standard errors were clustered at the county level, the closest geography to a PFA that was available in these data. As respondents are drawn from 52 counties, Wild Cluster Bootstrap *p*-values were estimated again as in all the analysis discussed above.

The results in Table 7 show that incumbent tenants who bought from the council were significantly more likely to install double locks to outside doors, to install locks in windows and have

²² No other BCS survey from 1982 to 1992, when the analysis terminates, contains the same detail of information.

Table 7. Correlation between Right to Buy (RTB) Public Housing Sales and Home Improvement, Insurance and Social Behaviour, 1988.

	Installed double locks on outside doors (1)	Installed locks on windows (2)	Theft insurance of home contents (3)	Installed burglar alarm (4)	Neighbourhood watch scheme (5)
<i>Panel A.</i>					
RTB Scheme	0.217 ($p = 0.000$)	0.193 ($p = 0.000$)	0.251 ($p = 0.000$)	0.043 ($p = 0.037$)	0.005 ($p = 0.828$)
Percentage effect	60.6%	130.4%	38.4%	226.3%	8.1%
<i>Panel B.</i>					
RTB scheme	0.232 ($p = 0.000$)	0.168 ($p = 0.002$)	0.239 ($p = 0.000$)	0.048 ($p = 0.045$)	0.018 ($p = 0.483$)
Percentage effect	64.8%	113.5%	36.5%	252.6%	29%
Gender	Yes	Yes	Yes	Yes	Yes
Ethnicity	Yes	Yes	Yes	Yes	Yes
Log age (quadratic)	Yes	Yes	Yes	Yes	Yes
Past burglary victimisation	Yes	Yes	Yes	Yes	Yes
Household type	Yes	Yes	Yes	Yes	Yes
Household rooms	Yes	Yes	Yes	Yes	Yes
Household income	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Mean dep. var. control group	0.358	0.148	0.654	0.019	0.062
Sample size	520	520	520	520	520
Number of counties	52	52	52	52	52

Notes: Treatment group defined as homeowners who rented from the council the property in which they currently reside prior to buying it. Control group defined as council tenants who currently intend to buy the public property in which they reside within two years. Standard errors were clustered at the county level and p -values from wild cluster bootstrap are reported in parenthesis. Percentage effect calculated as estimated coefficient divided by mean of dependent variable in the control group (council tenants who intend to buy the public property in which they reside within two years) reported here. Panel A reports unconditional OLS estimates and Panel B reports conditional OLS estimates where controls for gender, ethnicity, age, burglary victimisation, household type, household income, employment status and county fixed effects were included in the estimating equation.

Source: Authors' calculation from BCS data.

the contents of their home insured against theft. The same individuals were also significantly more likely to install burglar alarms, a relatively rare security device in England and Wales in 1988. Finally, these individuals were not more likely to join a neighbourhood watch scheme, a community-based crime prevention measure in which neighbours help each other in a form of informal guardianship. Arguably, this also mitigates the concern that the take-up of the RTB scheme may have been greater in areas characterised by greater social capital. The comparison group here is always council tenants who intended to buy the public property in which they resided in 1988 within two years. Consistent with the finding in Online Appendix Table A2 that observable characteristics are equally distributed between treatment group individuals and control group individuals, the comparison of Panel A and Panel B shows that these conclusions are unchanged whether the set of observable controls described above is included in the estimating equation or not.

The economic mechanism by which incumbent tenants who bought from the council were induced to invest in their homes might, of course, involve the transfer of wealth associated with the subsidised purchase of the home. Those eligible to purchase their homes were eligible to do so at a discount, and there is no availability of data that distinguishes between discounted and non-discounted sales that could be used to isolate the effects of transfer of home ownership independently.

Additional analyses using the BCS data corroborate the main findings for the effects of the RTB policy on crime. Online Appendix Table A3 shows results using the BCS data to compare crime victimisation rates among treated versus control group individuals. The table shows that, in 1988, treatment group homeowners reported between 21% and 25% less cases of crime victimisation since the beginning of 1987 than control group public tenants. Columns (1) and (3) show that this holds true regardless of whether county fixed effects are controlled for. Column (5) of the table also shows results when county fixed effects are replaced by the share of treatment group homeowners in the county.²³ The coefficient associated with homeownership under the RTB scheme remains negative and statistically significant. In contrast, the share of treatment group homeowners in the county itself does not predict crime victimisation. Thus, while reiterating that the crime reductions in Tables 3–5 and Figure 5 are indeed driven by RTB sales, these results also show that RTB sales did not lead to increased crime victimisation of *neighbouring* occupiers of public housing.

In additional analysis using the BCS data, Online Appendix Figure A8 shows county-specific estimates of the correlation between RTB sales and crime victimisation plotted against county-specific estimates of the correlation between RTB sales and home improvement. A linear function fitting the distribution of county-specific estimates is also shown. This appears negative and statistically significant, showing that, on average, incumbent tenants who bought under the RTB scheme experienced greater crime reduction in counties in which the RTB scheme was associated with greater home improvement.

The low-income homeownership experiment in Oklahoma investigated by Engelhardt *et al.* (2010) showed little or no significant evidence of home improvements of this sort subsequent to purchase, but that study covered a relatively short time interval post-purchase and take-up of the subsidy was relatively low. In contrast, the results for doors' and windows' locks and burglar alarms are consistent with the notion that homeownership led people to refurbish their properties. Homeownership may have given greater access to loans and financial markets in general, and

²³ For each respondent, this share was calculated at the county level leaving out the respondent herself/himself from the calculation.

the result for theft insurance plausibly reflects this. These results help rationalise the reduction in property crime. Controlling for past experiences of burglary victimisation also mitigates the concern that RTB purchasers may have experienced greater crime victimisation in the past and that this may drive the results in Table 7. In contrast, given that homeowners were no more likely to join a neighbourhood watch scheme, no evidence appears here that the RTB scheme induced greater investment in social capital in local communities where RTB unfolded more intensively. Similarly, this result provides no support to the notion that the take-up of the RTB scheme may have been positively linked with the local social capital.

Online Appendix A also suggests that no crime displacement occurred from public houses sold under the RTB scheme to neighbouring public housing not yet sold under the RTB scheme (see Online Appendix Table A3). However, one may also worry that the property crime reduction in Tables 4 and 5 is entirely driven by reductions in burglary, with potential thieves shifting, albeit to a lesser extent, to other thefts in the street. Online Appendix Table A4 shows the results of Tables 4 and 5 for the 1975–1992 period broken down by crime categories. Panel A shows results using the average public housing tenure length as ITT (as in Table 4) and Panel B shows results using the local share of public housing tenants eligible for RTB in 1980 as ITT (as in Table 5). Specifications in columns (1), (3), (5), (7) and (9) include the same set of controls as column (8) in Table 5, while specifications in columns (2), (4), (6), (8) and (10) include the same set of controls as column (9) in Table 5. Results are displayed separately for the crime categories that constitute property crime, namely burglary and theft and handling of stolen goods, as well as for the crime categories that constitute violent crime, namely violence against the person, sexual offences and robbery.

Breaking down property crime into burglary and theft and handling of stolen goods reveals that no evidence of such ‘displacement’ of crime appears. The reduction in property crime is driven by a reduction in burglaries, although a reduction in theft and handling of stolen goods appears in Panel B where the local share of public housing tenants eligible for RTB in 1980 is used as the ITT. In Panel B, this holds true regardless of whether controlling for the continuous average length of tenure duration in public housing and interacting it with the *post* variable. In Panel A, estimates for theft and handling of stolen goods appear numerically similar to those in Panel B, albeit less precisely estimated. Clearly, in both panels no increase in this crime category appears. Moreover, and similarly, the breakdown of the results for violent crime by crime category shows no evidence of crime displacement across categories, as violence against the person, sexual offences and robberies remain unchanged.

4. Conclusion

This paper studies the local crime reduction that occurred in direct response to the increased homeownership rates induced by the UK government’s Right To Buy (RTB) scheme. The RTB scheme offered a means of upgrading the economic position of households in neighbourhoods previously dominated by public housing. A key goal of the RTB scheme, therefore, was to offer access to owned property for (broadly) working-class families. In doing so, it intended to alter their economic position by giving them access to household wealth and a collateral asset in financial markets, and also to potentially alter their behaviour: for example, to induce them to take greater care of their property against crime. This was in sharp contrast to similar housing schemes elsewhere that intended to upgrade neighbourhoods through ‘gentrification’: that is, the in-migration of more affluent households and the displacement of lower-income groups.

The RTB scheme led to a reduction in crime over the decade following its introduction. Estimates from the Thatcher era uncover an elasticity of crime with respect to eligibility to sales of public housing of roughly -0.1 , implying that a 10% increase in the share of eligible tenants to the RTB public housing sales reduced crime by around 1%. The RTB scheme led to a reduction in property crime. Rather than being driven by changes in the composition of households through inward and outward migration, the key mechanisms underlying the reduction in crime rates appear to be the behavioural changes that the RTB scheme induced within the local community. The findings suggest that new renters becoming homeowners as a result of RTB altered their behaviour in response to the incentives arising from acquisition of housing wealth. They made their properties safer and gained greater access to the insurance markets.

While no evidence is found of compositional changes in the local population of different regions of the country, signs of this behavioural change appear from the early years of the policy, when no resales of properties bought under the RTB scheme could have taken place while the new owners continuing to benefit from the discounts offered under this scheme. These results therefore suggest that increasing homeownership reduces local crime as public housing tenants become owners of their own homes; a different mechanism from the process of gentrification whereby low-income neighbourhoods become middle-income neighbourhoods through outward migration of low-income households and inward migration of higher-income households. Hence, these results both complement the existing literature and inform policy by showing how granting homeownership to incumbent residents in neighbourhoods can also act to reduce crime.

Before concluding, some words of caution are necessary. First, although the results seem at face value to conform to the Thatcher rationale for the policy, they certainly do not vindicate it overall. To establish this would require a more general welfare analysis: for example, as to the welfare implications of the broad shift from direct provision of public housing to cash transfers for purchase of housing services in the UK implied by RTB and other policies, akin to the United States (Disney and Luo, 2017, discuss this in the context of RTB). Nonetheless, the change in behaviour in incumbent social housing communities documented here shows a novel means, not fully documented to date, that complements the existing literature based on different research designs and settings, by which homeownership and housing policy may contribute to reduce criminality. Thus, housing provision and subsidised homeownership have scope to act as potentially important features of some of the sizeable crime drops observed in the United States and several other Western economies seen since the 1990s.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix Replication Package

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