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Does Homeownership Reduce Crime?
A Radical Housing Reform from the UK

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Abstract

"Right to Buy" (RTB), a large-scale natural experiment by which incumbent tenants in public housing could buy properties at heavily-subsidised prices, increased the UK homeownership rate by over 10 percentage points between 1980 and the late 1990s. This paper studies its impact on crime, showing that RTB generated significant reductions in property and violent crime that persist up to today. The behavioural changes of incumbent tenants and the renovation of public properties were the main drivers of the crime reduction. This is evidence of a novel means by which subsidised homeownership and housing policy may contribute to reduce criminality.

1.   Introduction

In many countries, crime is spatially concentrated in areas characterised by low incomes and low rates of homeownership. Localities with high rates of tenancy in public housing commonly exhibit high crime rates, and the UK is no exception. 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 have lower incentives to maintain the security and upkeep of their property and to invest in neighbourhood monitoring.

Boosting homeownership is often viewed as a means of delivering benefits to communities, one of which is reduced crime. 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 capitalize into house prices, and therefore household wealth. Generating an understanding of the effects of homeownership on crime is therefore a first order research question in the economics of crime.

This paper provides a novel study of the impact of homeownership on crime by examining a large-scale nationwide program of subsidised public housing sales to incumbent tenants in the United Kingdom (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 could purchase their rented accommodation at heavily-subsidised prices.[1] 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 Conservative 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.[2]

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[1] The terms "council housing" and "social housing" indicate public housing in the UK. Public housing in the United Kingdom 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".

[2] 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

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% in 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 and large scale policy, RTB has until recently been little analysed (but see Disney and Luo, 2017). Analysis of RTB is useful to derive policy prescriptions that extend beyond the UK, as the RTB scheme shares many of the same characteristics as reforms implemented in Israel (e.g., Hausman, Ramot-Nyska and Zussman, 2020), Sweden (Sodini, Van Nieuwerburgh, Vestman and Von Lilienfeld-Toal, 2017) 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 to the late 2010s in order to utilise the RTB policy as a nationwide policy experiment.[3] The bulk of public house sales occurred in the 1980s and area-level sales of public housing were matched to crime data over several decades to estimate both the short-run and long run effects of homeownership on crime. The analysis shows that the large movements in housing tenures induced by the RTB led to lasting falls in crime rates for certain types of crime.

An important contribution of this study is to document the mechanism behind the crime-reducing effect of sales of public housing in local areas. It 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 many recent studies of the crime-reducing effect of gentrification (e.g., Autor, Palmer and Pathak, 2019), whereby changes in the composition of households induced by inward and outward migration are thought to explain local trends in crime rates. Our study thereby reveals a novel means, not documented to date, by which subsidised homeownership and housing policy may have contributed to the decline

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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 upon 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).

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

in crime in the United States and other Western economies in the 1990s and early 2000s (see 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.[4] 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.[5]

The study of the causal impact of homeownership on crime exploits the unfolding of the RTB policy and it is investigated separately in the short-run and in the long-run. To estimate the short-run impact of the RTB policy on crime, difference-in-differences specifications exploit the differential intention-to-treat (ITT) effect of the RTB policy across localities. RTB was introduced as a national policy, but the intensity of treatment across localities varied with the predetermined length of tenure duration of incumbent tenants in public housing. We explain how these differences in average tenures across localities arose in the later part of Section 2. Tenure duration determined access to the policy, with a minimum tenure duration requirement of three years, together with larger purchase price discounts for those with longer tenures. Variations in the length of tenure duration in public housing at the start of the RTB 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 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 roughly -0.3, so

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[4] 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, and Bell, Blundell and Machin, 2018).
[5] See, e.g., Katz, Kling and Liebman (2001); Ludwig, Duncan and Hirschfield (2001); Kling, Ludwig and Katz (2005); Ludwig and Kling (2007); Kling, Liebman and Katz (2007); Sanbonmatsu, Ludwig, Katz, Gennetian, Duncan, Kessler, Adam, McDade and Lindau (2011); Ludwig, Duncan, Gennetian, Katz, Kessler, Kling and Sanbonmatsu (2013); Sciandra, Sanbonmatsu, Duncan, Gennetian, Katz, Kessler, Kling and Ludwig (2013); Damm and Dustmann (2014); Bernasco, de Graaf, Rouwendal and Steenbeek (2017); and Rotger and Galster (2019).

that a 10 percent increase in incidence of RTB public housing sales reduces crime by around 3 percent. This conclusion is robust to a battery of additional tests, including variations in the specification of the econometric model. We explain in later sections how we deal with the obvious issue of feedback insofar as local crime rates may affect the willingness of public tenants to exercise their right to purchase their public house through the RTB policy.

The paper examines other 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 are very unlikely to be 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. We show that 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. On the

other hand, 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 led to significant reductions in burglaries and robberies, while theft and handling of stolen goods offences as well as other violent crime offences remained unchanged.

The paper next moves on to study the long-run persistence of the impact of the RTB policy on crime. To estimate the long-run crime effects of the RTB policy, extended crime data from 2003 to 2017 are used. Instrumental variable regressions are used to quantify the effects of public housing sales from 1980 to 1992 on the change in the crime rate from 2003 to 2017. Public housing sales are instrumented using variation in the initial public housing stock, which is a valid instrumental variable once the initial level of local crime and contemporaneous community characteristics are controlled for. In line with the short-run estimates, long-run estimates show that localities where the RTB policy triggered larger increases in homeownership also experienced faster reductions in crime throughout the later period.

Additional tests reveal that, by 2017, these localities also experienced slower increases in house prices. In the standard definition, gentrification arises as middle-class salary earners displace lower income or unwaged households as a result of rising house prices. In the case of tenure changes induced by RTB sales, the long time horizon of this research is required to identify displacement since it takes time before RTB purchasers who are ex-tenants move on, sell or rent out their properties, allowing other households to move into the neighbourhood. However, the *reduced* growth in house prices documented here in areas of high RTB sales is inconsistent with the standard gentrification story, and suggests that the supply-side shock of low-quality properties entering the housing market induced by the RTB policy outweighed any displacement of original tenants. There is also a political response as, by 2017, Conservative votes increased more rapidly in localities most affected by the RTB scheme. This further points towards RTB resulting in changing attitudes and behaviour of local communities.

The rest of the paper is structured as follows. Section 2 outlines the relevant literature and 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 United States) 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 3, and the empirical analysis is presented in

Section 4. Finally, section 5 provides a concluding discussion, including an important word of caution in assessing the overall merits of the RTB policy.

2. Housing and Crime

i. Previous literature

A large spatial crime literature shows that crime rates, especially crimes against property, are higher in areas with high concentrations of public housing, even when controlling for other salient characteristics of the resident population. This has been a major driver of policy in the United States towards the replacement of inner city public housing projects by rent subsidies and by shifting tenants to mixed-tenure low housing density urban neighbourhoods (see, for example, Schill, 1993, Olsen, 2003, and Kling, Ludwig and Katz, 2005). A similar association between crime and public housing has been observed in the UK (Murie, 1997). More broadly, studies show that increased local homeownership rates are associated with a range of spillover effects on the locality, such as lower crime rates, greater civic involvement and improved child development (see, for example, DiPasquale and Glaeser, 1999, and Haurin, Dietz and Weinberg, 2003).

A well-known difficulty in the large literature on housing tenure type, household composition and crime is the range of feedback effects which suggest a number of channels exist by which crime affects the composition of neighbourhoods and *vice versa.* 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. Much of this 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. 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 the findings of McDonald, 1986, with Ellen, Horn and Reed, 2017).

In addition, there is robust evidence that higher crime is associated with lower property values (see Gibbons, 2004, for an illustration using data for London) and this feedback effect will induce spatial equilibria of neighbourhood composition when account is taken of not just crime rates but other neighbourhood (dis)amenities, transport costs, and so on. For example, Owens, Mioduszewski and Bates (2019) 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

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willingness-to-pay for the civil liberties affected by the injunction.[6] Morales-Mosquera (2019) finds that police station openings generate localized crime reductions and housing value increases in three major cities in Colombia.

A large literature has addressed the basic identification problem of assessing neighbourhood effects on crime by exploiting the variation in residential locations generated by the Moving To Opportunity (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 victimization in distressed areas were in fact key reasons for low-income families to participate in the MTO experiment. Katz, Kling and Liebman (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 victimization. Similar findings emerge in Baltimore in the study of Ludwig, Duncan and Hirschfield (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.

Kling, Ludwig and Katz (2005) document important gender differences in the impact of the MTO experiment on crime. While both property and violent crime decreased among young women, young men experienced a decrease in violent crime but also an increase in property crime a few years after relocating to the new neighbourhood.[7] Ludwig and Kling (2007) show the importance of racial segregation among other neighbourhood characteristics in predicting youth violence, while Kling, Liebman and Katz (2007) document the beneficial impact of the MTO experiment on the education, risky behaviour and physical health of young women. As the opposite effects are found for young men, their findings further suggest that young men and women respond differently to similar new neighbourhood circumstances.

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

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[6] Grogger (2002) and Ridgeway, Grogger, Moyer and MacDonald (2018) are two earlier studies of the effects of gang injunctions on crime.

[7] The comparative advantage of juveniles relocated from disadvantaged backgrounds in exploiting the available property-crime opportunities in their new neighbourhoods and the different ways in which male and female juveniles respond to similar new neighbourhood circumstances help the authors rationalize these findings.

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.[8] To understand why low-income families remain segregated into high-poverty areas, Bergman et al, (2020) 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.

Our findings on homeownership also relate to Engelhardt et al, (2010), who analyse an experiment in Oklahoma that 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, since 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 status.

Recent studies have sought to estimate the effects of homeownership and gentrification on crime, focusing on 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, Palmer and Pathak (2019) examine the impact of the deregulation of rents in Cambridge, Massachusetts, on local crime rates. They hypothesize 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

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[8] 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, Hendren and Katz, 2016). Substantial exposure effects of neighbourhoods are also presented in Damm and Dustmann (2014), Bernasco, de Graaf, Rouwendal and Steenbeek (2017), Altonji and Mansfield (2018), Chetty and Hendren (2018a), Chetty and Hendren (2018b), Rotger and Galster (2019), and Aliprantis and Richter (forthcoming).

capitalised into higher property values.[9] In a related study, Diamond and McQuade (2019) document the crime-reducing effect of the Low Income Housing Tax Credit.

Most of the studies that have examined the effects of *gentrification* on crime exploit policy changes such as the lifting of rent controls and removal of public housing that led low-income neighbourhoods to increasingly become middle-income neighbourhoods through outward migration of low-income households and inward migration of higher-income households. 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 results are obtained from relatively short time periods after implementation of the policy.[10]

The main contribution of this study, by contrast, is to estimate the effect on local crime rates of increasing homeownership of indigenous communities and to examine the extent to which this induced behavioural change in these communities. This is a novel means, which has not been documented to date and which complements the existing literature, by which homeownership and housing policy may contribute to reduce crime. The RTB policy constitutes an ideal policy experiment to study this, and it is therefore exploited here using large-scale data over multiple decades.

ii. Public Housing in England and Wales

The stock of public housing in England and Wales grew rapidly throughout the twentieth century over the period from the end of World War I in 1918 until the start of the Right-to-Buy policy in 1980. By 1980, public housing in England and Wales was more extensive, more heterogeneous and more spatially dispersed than in the United States.[11] These spatial outcomes arose from the decisions to rebuild outside as well as within inner city areas, in turn stemming from the partially random distribution of war destruction and from the differential application of planning restrictions across the UK.[12]

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[9] A recent paper on the UK (Alonso, Andrews and Jorda Gil, 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.

[10] The reviewed literature examining the MTO experiment constitutes an exception.

[11] For examples of the heterogeneous nature of public housing across towns, see Table 1 in Disney and Luo (2017).

[12] For evidence on the continued important role of planning restrictions on private housing supply in England, see Hilber and Vermeulen (2014).

Figure 1 illustrates housing tenure shares, by tenure types, in England and Wales between 1918 and 2011. As illustrated in the Figure, in 1918 more than three-quarters of residential housing in England and Wales was privately rented, with public housing accounting for only 1% of the housing stock. As a result of a public sector building program and the development of the mortgage market, both private homeownership and public renting (primarily from local public authorities) rose steadily until 1980, by which time private renting had shrunk to a little over one tenth of housing tenure. While the increase in homeownership prior to 1980 is mostly explained by the development of a formal mortgage market in the deepening retail financial sector, two main factors explain the increase in public housing prior to 1980.

First, municipal government investment programs developed after the World War I. Much of the stock of privately-owned housing which was rented out was in very poor conditions post-1918. Replacement of the housing stock was hindered by high re-building costs coupled with borrowing constraints for many small landlords (Disney and Luo, 2017). Under the Housing and Town Planning Act 1919, much of the responsibility for replacement housing and slum clearance was taken over by local authorities. However, replacement public housing was not simply built in inner-city areas where slum clearance had taken place, but also in so-called suburban 'garden' estates and even 'garden towns' located on greenfield sites. Some of these sites were developed as large-scale public housing projects, often with low-density low-rise properties, others in mixed estates with a mixture of public and private housing. Later in the interwar period, as economic conditions improved, local authorities reverted to replacing inner-city slums with local high-rise buildings in the same neighbourhood, while private developers tended to build in suburbs and rural areas.[13] However planning (zoning) restrictions, which tend to be much tighter in the UK than in the United States, limited the growth of private housing in some areas.

Second, a further impetus to public housebuilding occurred after 1945. Between 1939 and 1945 around four million homes in the UK were destroyed or seriously damaged by bombing. Although bombing campaigns were initially targeted at military targets during 1939-45, they gradually evolved into area bombing strategies, focusing on cities. Until near the end of the World War II, bombing (by all sides) was notoriously inaccurate, lacking precision instruments for target identification. Target visibility relied on key geographical features such

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[13] For further discussion, see https://fet.uwe.ac.uk/conweb/house_ages/council_housing/print.htm and Jones and Murie (2006).

as coasts and large rivers and towns and cities in such localities were bombed heavily. Other areas remained relatively unscathed because of weather conditions, distance, counter-jamming measures, 'fake' fires lit to divert bombers to relatively uninhabited areas and so on.[14] This scattering of war destruction implies that there was a random element to where replacement housing was required once the hostilities had ended. As a result of war damage, many areas saw the rapid construction of publicly-owned housing estates to replace damaged and destroyed homes. This link at the local level between bombing and public housebuilding is illustrated in Figure A1 in Appendix A which uses official Bomb Census data from http://bombsight.org/data/sources to show that the location of bombings across boroughs in London during the World War II is a significant predictor of where publicly-owned housing estates were built in the post-war period in London.

The somewhat random nature of bombing is illustrated by the fact that whilst areas that were targeted such as the London Docks already contained high-density low income housing, bombs also fell heavily in the more affluent boroughs of south and south east London, which thereby constructed more public housing after 1945 than equally affluent suburbs to the north of the capital. Other areas where major rebuilding took place were also mixed-income communities. Nevertheless, after the post-war reconstruction period that lasted well into the 1950s, new public properties tended to be built to replace slums in low-income neighbourhoods, often as high-rise developments, with private housebuilding being the dominant feature of more affluent areas. By 1980, close to one-third of the residential housing stock in England and Wales was public housing.

For the analysis that follows, in particular to understand the identification strategy that is taken, it is very important to understand how a household accessed a public house and how the length of tenure within public housing was determined. Policies varied between local authorities, but priority for access to public housing would initially be given to local residents who required rehousing as a result of slum clearance or war destruction. Thereafter, most local authorities operated a queuing system by which eligibility depended on evidence that the person or household would otherwise be homeless, on family size and on means of household income support if any. Local residents were prioritised. The extent of rationing (waiting time) for public housing, if any, in a given area would depend on the supply of public housing that had been constructed relative to demand. Incoming applicants from outside the local authority

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[14] There is a vast literature on this. An early volume that revealed efforts to thwart the accuracy of the German bombing campaign is Jones (1978).

area would typically be at the back of the queue. In turn, residents already within public housing could apply to move from one public property to another – typically by requesting 'upgrading' as a nicer property became available – for example, a house with a yard or garden rather than an apartment. Tenure within any given property would therefore depend on the availability and heterogeneity of the public housing stock within an area.

iii. The "Right to Buy" Policy

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 3 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' 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 rents on public tenancies towards 'market' levels.[15] Figure 2 indicates 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 but did tighten eligibility conditions, limit access to publicly-provided mortgages and impose caps on the maximum discounts in some areas where

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

sales had diminished the public housing stock quickly (since local authorities still had a statutory responsibility to house homeless families). On the other hand, the 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 led to a brief upsurge in sale volumes, as illustrated in Figure 2, although sales continued in any event 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 mid-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 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, Machin and Manning, 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 4.

3.   Data

Our empirical analysis combines multiple data sources at annual frequency. Housing data are provided by the Chartered Institute of Public Finance and Accounting (CIPFA). The data are provided at the Local Authority (LA) level for the 314 LAs in England and Wales in 1980.[16] 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 Right-to-Buy (RTB) scheme in each year. LAs had a statutory requirement to report RTB sales to CIPFA and provide details

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[16] 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 RTB 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).

Two sources of crime data complement the housing data. The short-run analysis, which covers the period 1975 to 1992, uses administrative crime records collected by the police and published annually by the UK Home Office in *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. These data are provided at the Police Force Area (PFA) geography, a geographic unit that sits above and nests LAs.[17] 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 that exist in England and Wales.[18]

The long-run analysis, which covers the period 2003 to 2017, draws upon administrative crime records at the LA level recorded by the police and published online by the UK Office for National Statistics (ONS). The long-run analysis starts in 2003 because there was a very substantial change in reporting rules in the late 1990s and early 2000s in the UK that had different effects across PFAs and crime types. Thus, it is not possible to use police recorded crime data before and after 2002 in the same analysis. The City of London PFA is also excluded from the long-run analysis. For both the short-run and long-run analyses, 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 combined with the crime data to create two data sets for the analysis. For the short-run analysis, running from 1975 to 1992, the LA-level crime data were aggregated at the PFA level using the 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

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[17] For example, London LAs sit within the Metropolitan Police PFA.

[18] 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, hardly anybody lives there and consequently few RTB sales took place there.

panel of 42 PFAs spanning 18 years was obtained, with the PFA-year being the unit of analysis. Data from the GHS on tenure were matched to PFA from regional data subdivided into 'rural', 'urban' and 'mixed urban-rural' areas.

For the long-run analysis (2003 to 2017), which uses a long-difference model, observations of the change in crime measured at the LA level for the period 2003-2017 were joined with observations of total RTB sales, also at the LA level, for the period 1980-1992. Not all observations could be joined due to changes in LA geographical boundaries that preclude a one-to-one mapping between LAs in the earlier and later period. This analysis is therefore based on 293 observations that could be joined between the earlier and latter periods.

These data sources were complemented with administrative records of local area conditions. In the short run analysis, 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. In the long run analysis, local area conditions at the LA level from the Annual Population Survey (APS) complement the housing and crime data.[19]

i. Summary Statistics

Summary data for the composition of the housing stock in 1980, just prior to the RTB policy start, are provided in the first five 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. More than 1% of all residential properties were sold under the RTB scheme by 1981, i.e., in the first year of the policy.[20] 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 1980 had spent 11 years in their public properties, with some areas featuring much larger average years of tenure duration than others. The distribution of the public housing rate, as well as of public housing sales and tenure duration 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 (the standard deviation, shown in Table 1, is 8.6%) as well as some outlier LAs – in 10% of LAs, public housing accounted for more than one third of the residential housing stock.

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[19] Additional details of the data used in the empirical analysis and instructions for data access are provided in the Appendix.

[20] Our 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.

Figure A2 Panel A in 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 Appendix A illustrates the distribution of public housing sales across LAs in the 1980s-90s (expressed as a percentage of the residential housing stock in 1980). Panel B illustrates the distribution of public housing sales in the 1980s-90s in absolute terms and Panel C shows the distribution of public housing sales across LAs in the 1980s-90s (expressed as a percentage of the public housing stock in 1980). All distributions in 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.

The remaining rows of Table 1 provide summary data for crime rates and other covariates in 1980. Crime rates are measured as 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.[21] 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 and 50th percentiles of the distribution, the local log unemployment rate and the local share of 15-24 years olds in the population in the PFA. Since 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, while the 50th percentile of log wages is also included to depict the distribution of wages. Finally, Table 1 also suggests that approximately 16% of the overall population is aged 15 to 24. Since the likelihood to commit crime is observed almost

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[21] 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.

universally to be highest in the late teens and then decrease later in life (Quetelet, 1831, and Landersø, Nielsen and Simonsen, 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.

4. Empirical Analysis

i. Public housing and crime in the 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 a natural starting point to the empirical approach. Figure 3 illustrates 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. A clear positive association appears between the concentrations 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 OLS regression of the following form was also estimated:

$$C_i = \alpha + \beta_1 H_i + \beta_2 X_i + \epsilon_i \tag{1}$$

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.

Estimates of Equation (1) are shown in Table 2. The Table shows estimates of crime as a function of the public housing stock (expressed as a proportion of the residential stock). Columns (2), (4) and (6) additionally control for the log real hourly wages at the 25th and 50th percentiles of the distribution within the PFA, the log unemployment rate in the PFA and the share of 15-24 year olds in the population in the PFA. Public housing is positively and precisely correlated with all crime outcomes. The coefficient on total crime in column (2) of 0.064 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 0.0064 units, equating to approximately half of one standard deviation increase in crime. The positive correlation between public housing and criminality is not affected by the inclusion of local area variables in Equation (1).

ii. Identification issues

The primary object of this study is to quantify the causal impact of homeownership on crime rates. In our empirical setting, the Right-to-Buy (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 very 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 Appendix A, further suggesting that RTB sales were not orthogonal to crime rates at the onset of the RTB policy.

iii. Short-run estimates

a. Empirical strategy

Our empirical strategy exploits two approaches to estimating the effect of sales of public housing on crime rates that isolate 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 short-run 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 Intention-To-Treat (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.

In particular, the Housing Act of 1980 introduced a statutory right to buy for public tenants with at least three years' tenure duration in their public house. Moreover, discounts on the sale price relative to the market value of the property linearly increased with public housing tenure duration, ranging from 33 percent for public housing tenants with three years' residence through to a maximum of 50 percent after twenty 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 predetermined source of differential exposure to the RTB scheme that could not be gamed in 1980.

In Columns (7) and (8) in Table 2, equation (1) is re-estimated and the fraction of public housing in the PFA, the log real hourly wages at the 25th and 50th percentiles of the distribution within the PFA, the log unemployment rate in the PFA and the share of 15-24 year olds in the population in the PFA are used to predict log average years of tenure duration in public housing in the PFA in 1980. As Table 2 shows, none of these variables predicts public housing tenure duration in 1980, suggesting that its distribution across regions and thus RTB eligibility in 1980 is orthogonal to local area circumstances and the average socio-economic status of individuals. 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.

Eligibility to the RTB scheme, and in turn intensity of adoption, varied across regions according to the composition of the local housing stock, which was determined by historical factors. 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.

Since 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 short-run 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. When the analysis is restricted to the first five years of the RTB scheme, years of tenure duration in public housing of incumbent tenants in 1980 are also used as an instrumental variable (IV) for the actual sales of public housing under the RTB scheme in the first year of the RTB scheme. The same IV approach is not used for 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*.

Figure 4 illustrates a strong positive correlation between the log average years of tenure duration in public housing in the PFA in 1980 (on the x-axis) and the log 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 Figure 5 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 a well-known problem of reverse causation in our estimates, the public housing tenure duration in 1980 constitute 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 estimating difference-in-differences specification can be expressed as follows:

$$C_{i,t} = \alpha_i + \alpha_t + \beta_1 * RTB_{i,80} + \beta_2 * Post_t + \beta_3 * \left(RTB_{i,80} * Post_t\right) + \beta_4 \boldsymbol{X_{it}} + \epsilon_i, \qquad (2)$$

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. $RTB_{i,80}$ is log average years of tenure duration in public housing, i.e., a measure of RTB eligibility in 1980, defined both as a continuous variable and as a binary variable 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. *Post* is a dummy variable that takes up value 1 starting from 1980.

The main estimand of interest is β_3, the coefficient associated with the interaction between $RTB_{i,80}$ and *Post* which measures the impact of the initial RTB eligibility-induced shock to homeownership on crime. A negative coefficient associated with β_3 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 policy maker wishing to modify homeownership rights for the general population, as tenure duration in public housing was an explicit, predetermined 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 causal interpretation of β_3 crucially 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_3 can

be interpreted as the ITT 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* variable, which includes the fraction of public housing in the PFA, the log real hourly wages at the 25th and 50th percentiles of the distribution within the PFA, the log unemployment rate in the PFA and the share of 15-24 year olds in the population in the PFA, while ϵ_i is the error term. Due to the small number of clusters (42 PFAs) in the analysis, p-values were derived for inference from Wild Cluster Bootstrap estimation with standard errors clustered at the PFA level (see Cameron, Gelbach and Miller, 2008).

b. Short run: results

Table 3 presents unconditional difference-in-differences estimates based upon the dichotomous treatment version of Equation (2) (calculated absent any of the labour market controls or fixed effects stated in the equation). Panel A shows results for the 1975-85 period, when property resales should have been extremely rare since the discount on the sale price would be repayable, and Panel B shows results for the 1975-92 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 percent terms (the unconditional percent 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.

Figure 6 plots the raw data underlying these statistically significant difference-in-differences estimates. The Figure shows average crime rates (weighted by population at the PFA level) for every year from 1975 to 1992 separately for ITT treatment and ITT control PFAs. The visual inspection of Figure 6 reveals that, while crime rates appear on similar trends prior to 1980 in ITT treatment and ITT control PFAs, the gap widened after 1980 with ITT

control PFAs experiencing a greater increase in crime rates starting from the early 1980s. Compared with the 1970s, a wider gap in crime rates is also observed in the early 1990s, when the short run analysis terminates. These unconditional estimates and those in Table 3 suggest that sales of public housing under the RTB scheme may have helped tackle the nationwide rising trend in criminality experienced in England and Wales in the 1980s.

Table 4 subjects these results to a set of more robust econometric specifications. Columns (1)-(3) show ITT results for the 1975-85 period, columns (4) and (5) show IV results for the 1975-85 period, and Columns (6)-(9) show ITT results for the 1975-92 period. All estimated specifications include PFA fixed effects and year fixed effects, and the models in Columns (2)-(5) and (7)-(9) additionally include the interaction between the *Post* variable in equation (2) and local area variables measured in 1980 (i.e., fraction of public housing, log of the unemployment rate, log of real hourly earnings at the 25th and 50th percentiles of the distribution, and the fraction of 15-24 year olds in the population). Columns (1) to (8) report the ITT and IV estimates of β_3, the coefficient associated with the interaction between $RTB_{i,80}$ and *Post* in equation (2), whereas the model in Column (9) shows the results separately for the first eight years of the RTB policy, 1980-87, and for the subsequent years that followed the 1987 UK General Elections when Margaret Thatcher was re-elected as UK Prime Minister, 1987-92. In all specifications, standard errors were clustered at the PFA level and Wild Cluster Bootstrap techniques were used for inference due to the small number of clusters.

Column (1) shows results based upon the dichotomous treatment version of $RTB_{i,80}$ in Equation (2), indicating that ITT treatment group PFAs experienced a 10 percent reduction in crime by 1985. Column (2) shows results based upon the continuous treatment version of $RTB_{i,80}$ and when the interactions between local area circumstances measured in 1980 and the *Post* variable are also added to the estimated specification. Column (2) also shows 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 equation specification. In particular, estimates in Column (2) uncover an elasticity of crime with respect to RTB eligibility of approximately -0.19, implying that a 10 percent increase in eligibility to the RTB scheme reduced crime by around 1.9 percent within the first five years of the scheme.

The causal interpretation of these estimates crucially relies on the absence of differential pre-treatment crime trends between PFAs that were exposed to different degrees of RTB eligibility in 1980. Column (3) shows event-study estimates, where the log average years

of tenure duration in public housing in 1980 was interacted with three pre-reform years. Two key facts emerge from Column (3). First, Column (3) shows there to be no differential pre-treatment trends between PFAs that received different exposures to the RTB scheme. In other words, RTB eligibility in 1980 does not predict crime trends across PFAs prior to 1980. This implies that any post-1980 deviation from the existing trend that is predicted by the eligibility to the RTB scheme in 1980 can be interpreted as the ITT impact of the RTB scheme on crime. Second, estimates in Column (3) show a reduction in crime rates among PFAs that experienced greater RTB eligibility in 1980, uncovering an elasticity of crime with respect to RTB eligibility of approximately -0.28.

Columns (4) and (5) show the IV estimates for the period 1975-85, and they retrieve similar, slightly larger negative elasticities. In both columns, the actual public housing sales under the RTB scheme in the first year of the policy, i.e., in 1980-81, are instrumented using the log average years of tenure duration of incumbent tenants in public housing in 1980. Both columns show negative and statistically significant estimates, with Column (5) also showing there to be no differential pre-treatment trends between PFAs that received different exposures to the RTB scheme. Similarly to the ITT estimates in Column (3), IV estimates in columns (4) and (5) uncover an elasticity of crime with respect to RTB sales of approximately -0.28 to -0.42, implying that a 10 percent increase in the RTB public housing sales reduced crime by around 3 to 4 percent by 1985.

In the remaining Columns (6)-(9), the analysis is extended up until 1992, the year when the Thatcher era ended, but very similar conclusions emerge. Column (6) shows results based upon the dichotomous treatment version of $RTB_{i,80}$ in Equation (2), and it indicates that ITT treatment group PFAs experienced a 12.8 percent reduction in crime by 1992. Columns (7) and (8) show results based upon the continuous treatment version of $RTB_{i,80}$ and when the interactions between local area circumstances measured in 1980 and the *Post* variable are also added to the estimated specification. Columns (7) and (8) also show negative and statistically significant estimates, uncovering an elasticity of crime with respect to RTB eligibility of roughly -0.28 to -0.36, implying that a 10 percent increase in eligibility to the RTB scheme reduced crime by around 2.8 to 3.6 percent by 1992. When broken down between the years prior to 1987 and later years, Column (9) shows that a sizeable and statistically significant crime-reducing effect of RTB eligibility appears in both the 1980s and early 1990s, with little variation appearing in the estimated crime-reducing effects of RTB across time and model specifications.

Throughout these estimates, the magnitude of the crime reduction caused by the RTB scheme by 1992 appears very similar to the magnitude observed by 1985. In the early years of the RTB scheme, no one who purchased a property under the RTB scheme could have possibly 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 1980-85, as well as 1980-87, 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 rather consistent with behavioural changes within the local communities that were more exposed to the provisions of the scheme.

Estimates for property crime in Table 5 tell a similar story, while no detectable effects of RTB eligibility and RTB sales appear for violent crime in Table 6. Both Tables are organised in the same way as Table 4. In both Tables, all estimated specifications include PFA fixed effects and year fixed effects, with the models in Columns (2)-(5) and Columns (7)-(9) additionally including the interactions between the *Post* variable in equation (2) and local area variables measured in 1980 (i.e., fraction of public housing, log of the unemployment rate, log of real hourly earnings at the 25th and 50th percentiles of the distribution, and the fraction of 15-24 year olds in the population). In all specifications, standard errors were clustered at the PFA level and Wild Cluster Bootstrap techniques were used again for inference due to the small number of clusters.

Regardless of whether estimates are derived from the 1975-85 period or the 1975-92 period, and regardless of whether ITT or IV estimates are considered, results in Table 5 indicate that the RTB scheme had a negative and significant impact on property crime, uncovering an elasticity of property crime with respect to the RTB scheme of approximately -0.3 to -0.4, implying that a 10 percent increase in eligibility to the RTB public housing sales reduces property crime by around 3 to 4 percent. Also in Table 5, Columns (3), (5), (8) and (9) show there to be no differential pre-treatment trends between PFAs that received different exposures to the RTB scheme, as none of the estimated pre-treatment coefficients appears statistically significant. Columns (1) to (5) show that a RTB-induced property crime reduction was already visible by 1985, and Column (9) also shows that a sizeable and statistically significant property

crime-reducing effect of RTB eligibility appears both in the 1980s and early 1990s, consistent with the notion that RTB eligibility led to behavioural changes within the local communities rather than gentrification induced by in-migration.

Table 6 studies the effect of RTB eligibility on violent crime, and no significance appears in the estimates. Also in this case, ITT and IV estimates from the 1975-85 period reach the same conclusion as ITT estimates from the 1975-92 period. Estimates Columns (3), (5), (8) and (9) show there to be no differential pre-treatment trends between PFAs that received different exposures to the RTB scheme, suggesting that RTB eligibility in 1980 does not predict violent crime trends either across PFAs prior to 1980, and further supporting the causal interpretation of the estimates presented in Tables 4 and 5 and discussed in this section.

c. *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 3-6 and Figure 6 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. 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. Although RTB sales were only used in the IV estimates and not in the ITT reduced-form estimates of equation (2) presented in Tables 4-6, one may worry that the *de facto* supply-side availability of public housing for RTB sales might have differed by local political control. If, for example, pro-Thatcher Conservative-controlled LAs were more willing to fulfil their statutory responsibility to accelerate sales through faster processing (or, potentially, be more likely to advertise and encourage the possibility locally), then this omitted confound might weaken the predictive power of public housing tenure duration and co-determine RTB sales and crime. However, a scatter plot in Figure A5 in Appendix A suggests no correlation between the conservative vote share within the PFA and RTB sales from 1980-92 as a percentage of the residential stock in 1980. 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.[22] Results are unchanged with the inclusion of this additional variable, which suggests that our results are not caused by Labour controlled LAs opposing

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

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.

One 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 experienced reduced criminality thanks to the improved labour market circumstances, then the reduction in crime observed in Tables 3-6 and Figure 6 would be erroneously attributed to the RTB scheme. Models to test for the effects of RTB eligibility on local labour market conditions were therefore estimated and results are displayed in Table 7. 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 eligibility to the RTB scheme in 1980 predicted the evolution of local labour market conditions in the 1980s and early 1990s. These results mitigate the concern that our estimates might be picking up the effect of other local area circumstances that interacted with public housing sales over time in the 1980s.

Eligibility to the RTB scheme 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. 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 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 socioeconomic 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 the fact that we are 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 *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 (5) of Table 7. When crime is regressed on the average (log) value of RTB transactions in the first year of the RTB scheme, the estimated coefficient appears small in magnitude, as well as in percent terms, and very statistically insignificant. Thus, unlike RTB eligibility, the value of RTB sales does not predict the evolution of crime. This conclusion is robust to the set of controls added to the estimating equation (fixed effects only, or fixed effects and local area variables). In sum, the initial eligibility to RTB sales, not their average value, predicts the reduction in crime since the 1980s.

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, our 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 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 log number of police officers deployed by the PFA. Results are displayed in column (6) of Table 7, 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 our main results reveal the genuine reduction in criminality generated by the RTB scheme.

d. *Homeownership and Behavioural Change*

Did the RTB policy induce indigenous 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) 1988 contains a rich set of variables concerning homeownership and crime.[23] 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, 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. Table A1 in the Appendix shows these treatment group individuals and control group individuals to be 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 victimization. For each of these individual and household characteristics, Table A1 presents a balancing test that retrieves an insignificant estimate, thus showing the suitability of the comparison group.

Table 8 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 Appendix Table A1, i.e., gender, ethnicity, a quadratic of age, past experiences of burglary victimization 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 is driving our results here. 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 8 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

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[23] The same detail of information is not available from any other BCS survey from 1982 to 1992, when the short run analysis ends.

and have 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. 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 Appendix Table A1 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.

Additional analysis using the BCS data corroborates our main findings for the effects of the RTB policy on crime. Table A2 in the Appendix uses the BCS data to compare crime victimization rates among treated versus control group individuals. The Table shows that, in 1988, treatment group homeowners reported between 20% and 25% less cases of crime victimisation since the beginning of 1987 than control group council 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.[24] 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 3-6 and Figure 6 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.

Figure 7 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.

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

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 in Table 8 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 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 8. In contrast, since homeowners were no more likely to join a neighbourhood watch scheme, no evidence appears here that the RTB induced greater investment in social capital in local communities where RTB unfolded more intensively.

Table A2 in the Appendix suggested that no crime displacement occurred from public houses sold under the RTB scheme to neighbouring public housing not yet sold under the RTB scheme. However, one may also worry that the property crime reduction in Table 5 is entirely driven by reductions in burglary, with potential thieves shifting, albeit to a lesser extent, to other thefts in the street. Table 9 shows the results of our analyses in Tables 4-6 broken down by crime categories. Panel A shows results for the 1975-85 period and Panel B shows results for the 1975-92 period. Results are displayed separately for the crime categories that constitute our measure of property crime, namely burglary and theft and handling of stolen goods, as well as for the crime categories that constitute our measure of 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 the 1975-85 period, i.e., in Panel A. The reduction in theft and handling of stolen goods does not appear statistically significant by 1992, but certainly 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 and sexual offences remain unchanged. Interestingly, both by 1985 and by 1992, a significant negative effect of the RTB scheme is found on robberies, which are

categorised as violent crimes here because they involve a violent act, but they are also often economically motivated criminal offences.

iv. Long-Run Estimates

a. Empirical strategy

But what of the long-run consequences of homeownership on crime? This section presents estimates of the long-run effects of homeownership induced by the Right-to-Buy (RTB) policy on local crime rates. To do so, an instrumental variable (IV) long-differenced model specification is used, which exploits variation in the size of the public housing stock in 1980 across localities. As argued above, exposure to the policy was bounded by the size of the public housing stock in 1980. Localities with less public housing would not experience large increases in the homeownership rate due to the more limited supply. As discussed in Section 2, variation in the size of the public housing stock across localities arose due to historical events around the period of the two World Wars, events which are unrelated to recent dynamics of crime. In formal terms, the set of estimating equations can be expressed as follows:

$$\left(C_{i,2017} - C_{i,2003}\right) = a + \beta_1 * \widehat{H_{i,92}} + \beta_2 * C_{i,80} + \beta_3 * X_i + \epsilon_i \tag{3}$$

$$H_{i,92} = a + \beta_1 * H_{i,80} + \epsilon_i, \tag{4}$$

where $\left(C_{i,2017} - C_{i,2003}\right)$ is the change in the crime rate in locality i from 2003 to 2017 and $H_{i,92}$ is the endogenous change in the size of the public housing stock from 1980 to 1992 expressed as a proportion of the residential stock in 1980. $H_{i,92}$ is therefore instrumented with $H_{i,80}$, the 1980 level of the public housing stock; $C_{i,80}$ is the crime rate in locality i in 1980, X_i is a vector of contemporaneous local area variables (i.e., the local log real hourly wages at the 25th and 50th percentiles of the distribution, the local log unemployment rate and the local share of 15-24 years olds in the population) and ϵ_i is the error term. As crime data is available from 2003 at the Local Authority (LA) level, this model can be estimated at the LA level.

The identifying assumption in the model described in Equations (3) and (4) is that, conditional on the level of crime in 1980 and the set of contemporaneous local area variables captured in the vector X_i, the size of the public housing stock in 1980 in the LA is exogenous to the dynamics of crime within the LA in the 2000s. The exclusion restriction is that, conditional on the level of crime in 1980 and the set of contemporaneous local area variables in the vector X_i, the size of the public housing stock in 1980 affects the dynamics of crime in the 2000s only through public housing sales over the subsequent period.

b. Results

Summary data for the dependent variable in Equation 3 and the set of local area variables in the vector X_i are shown in Table 10. The crime rate fell over the period in more than 90% of LAs. The 25th percentile log real hourly wage fell too over the period, while the share of 15-24 year olds in the population remained unchanged and the log unemployment rate rose (at a national level, the unemployment rate rose from 5% in March 2003 to 7.6% by March 2013, during the period following the financial crisis).

The instrumental variable regression requires a strong first-stage relationship between the public housing stock in 1980 and subsequent RTB sales over the period 1980-92. Figure A6 in Appendix A illustrates that there is a strong correlation between the size of the public housing stock in the LA in 1980 and subsequent sales under the RTB policy. The scatterplot in the Figure shows a strong positive relationship. The coefficient (standard error) on the instrument in the first-stage regression is 0.242 (0.017) with a F-statistic from the regression of 195.55.

Estimates of Equation 3 are shown in Table 11. Estimates are grouped by the three measures of crime examined above: the overall crime rate in columns (1)-(3), property crime rate in columns (4)-(6) and violent crime rate in columns (7)-(9). The leftmost specification for each outcome includes only the independent variable of interest, $H_{i,92}$, instrumented by $H_{i,80}$. For each outcome, the coefficient on $H_{i,92}$ is negative and precisely defined. The middle column of each sub-panel adds local area variables to the model, with the rightmost column of each sub-panel also adding a control for the crime rate in 1980. The coefficient on $H_{i,92}$ remains negative and precisely defined in each specification.

The coefficient estimates imply that LAs experiencing larger increases in homeownership experienced more rapid crime reductions. The coefficient value of 0.244 in column (3) of Table 11 implies that a five percentage point higher increase in the homeownership rate due to RTB sales caused an approximately fifty percent faster decline in crime rate over the period 2003-2017, an effect approximately equal to a one standard deviation higher speed of reduction in crime. Among the covariates, in all of the regressions the controls for the level of crime in 1980 return negative and precisely defined coefficients. These imply a degree of convergence in LA level crime rates: LAs with higher crime rates in 1980 experienced a faster decline in crime in the early 2000s, conditional on the other covariates in the regression.

c. Additional Long-Run Estimates

Did the RTB scheme leave any detectable mark on house prices and political preferences of indigenous communities? In this final sub-section, the long-run effects of increases in the homeownership rate brought about by RTB sales on house prices and on voting behaviour are examined. The increase in the homeownership rate via RTB sales increased the potential housing supply to the private resale market. Hence, all else being equal, RTB sales would be expected to decrease house prices over time. On the other hand, the RTB scheme may have also potentially increased the demand for housing in the private resale market. Moreover, in the presence of outward migration of low-income households and inward migration of higher-income households, RTB sales may have had a positive impact on house prices over time.

To explore the impact of RTB sales on house prices, in column (1) of Table 12 the dependent variable was replaced with the change in the log of the average house price in the LA over the period 2003 to 2014. House price data is obtained from the Land Registry house price index. The set of control variables is the same as in the models shown in Column (2) of Table 11. The coefficient on RTB sales is negative and precisely defined, and it implies that a five percentage point increase in the homeownership rate due to RTB sales decreased house prices over the period by approximately 6.5%. This is an economically modest effect in the context of an approximate doubling of house prices on average in LAs over the sample period. However, this result highlights the general equilibrium effects of RTB sales on the housing market.

Second, the effects of RTB sales on voting behaviour in LA elections were also estimated. To do so, LA-level data on vote shares in the local election were matched to the housing and crime data used so far and the changes in the Conservative vote share within the LA over the period 1995 – 2015 were constructed and modelled as the dependent variable in the regression. The sample size in these estimates is smaller, owing to the fact that the timing of LA election cycles varies across LAs. In Table 12, the coefficient on the RTB sales variable is positive and precisely defined. Hence, these estimates suggest that the RTB policy was effective in increasing the Conservative vote share in localities that saw larger increases in the homeownership rate under the policy. This result echoes the conclusion in Di Tella, Galiani and Schargrodsky (2007) that show that lucky squatters who received legal titles in Buenos Aires in the 1990s report beliefs closer to those that favour the workings of a free market.

5.   Conclusion

This paper studies local crime reduction connected to the increase in homeownership rates induced by the UK Government's Right To Buy (RTB) scheme since 1980. One of the key goals of the RTB scheme was, as it were, to 'gentrify' households endogenously. Specifically, it aimed to ease access to an owned property for (broadly) working class families and, in doing so, to alter their behaviour and induce them to take greater care of their property, as well as to alter their economic position by giving them a collateral asset in financial markets. The RTB scheme offered a means of upgrading the economic position of households in neighbourhoods previously dominated by public housing.

The RTB scheme led to a reduction in crime. Estimates from the Thatcher era uncover an elasticity of crime with respect to eligibility to sales of public housing of roughly -0.3, implying that a 10 percent increase in eligibility to public housing sales reduced crime by around 3 percent. The RTB scheme led to a reduction in both property crime and robberies, and both short run and longer run evidence of crime reduction is presented. However, 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-come-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 continuing to benefit from the discounts offered under this scheme. In line with this conclusion, and contrary to what a migration-based model of gentrification would predict, house prices decreased in the long run and Conservative votes increased in areas most affected by the RTB scheme. These results therefore vindicate the Thatcher rationale for the policy. More broadly, they suggest that increasing homeownership reduces local crime as public housing tenants become owners of their own homes *separately* 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. These results complement the existing literature and inform policy by showing how granting homeownership to indigenous communities can also act to reduce crime.

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Before concluding, some words of caution are necessary. First, although the results conform to the Thatcher rationale for the policy, they certainly do not vindicate it overall; to show that would require a more general welfare analysis. Indeed, an analysis of this sort presented in Disney and Guo (2017) suggests that the RTB policy generated complex inter- and intra-generational welfare effects, and that welfare-improving adjustments to the policy should have been considered. 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 US, also requires further analysis. Nonetheless, the change in behaviour and beliefs in indigenous 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 subsidized homeownership have scope to act as potentially important features of the sizable crime drops observed in the United States and several other Western economies since the 1990s.

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Figure 1. Housing Tenure Shares by Tenure Type: England and Wales 1918-2011
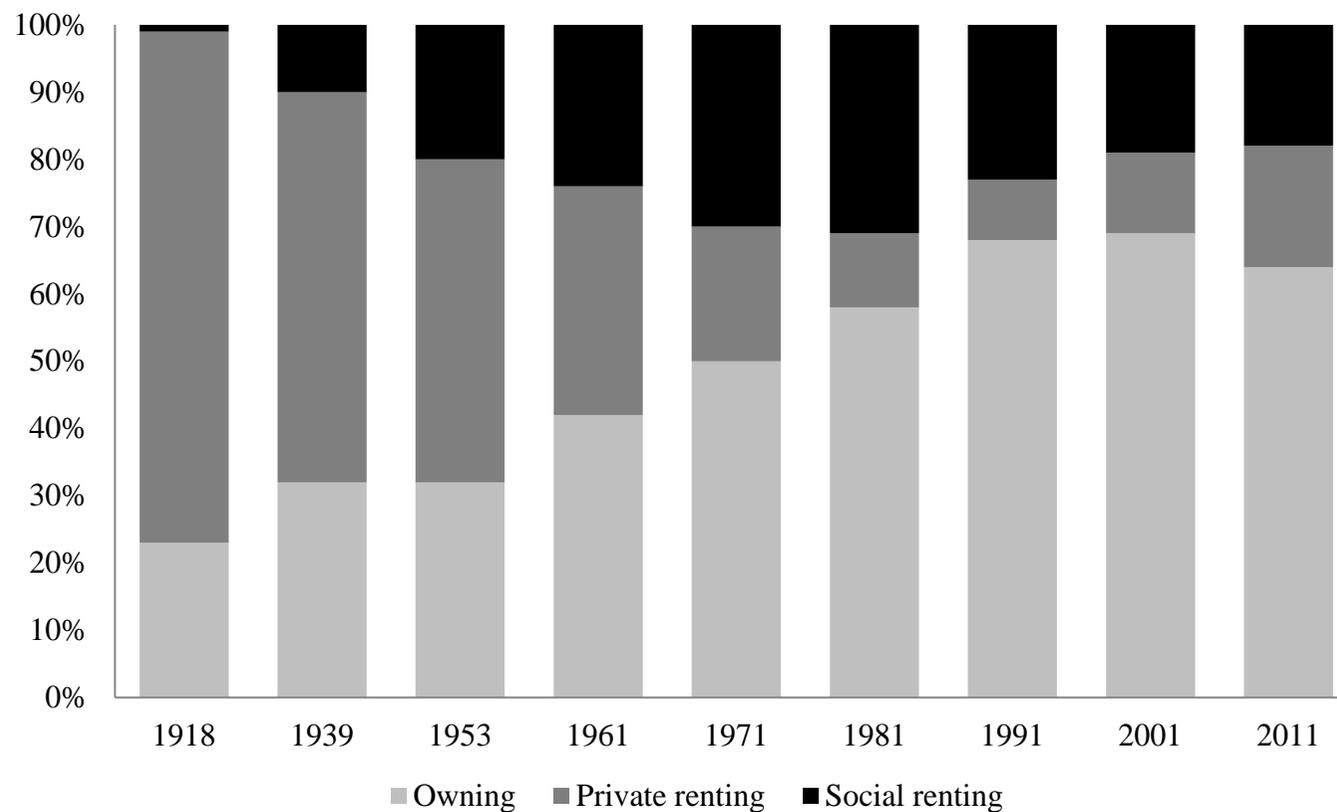
Source: Reproduced from Disney and Luo (2017) Chart 1, calculated from Office for National Statistics (2013).

Figure 2. Right to-Buy and Other Sales of Public Housing in England, 1980-81 to 2013-14
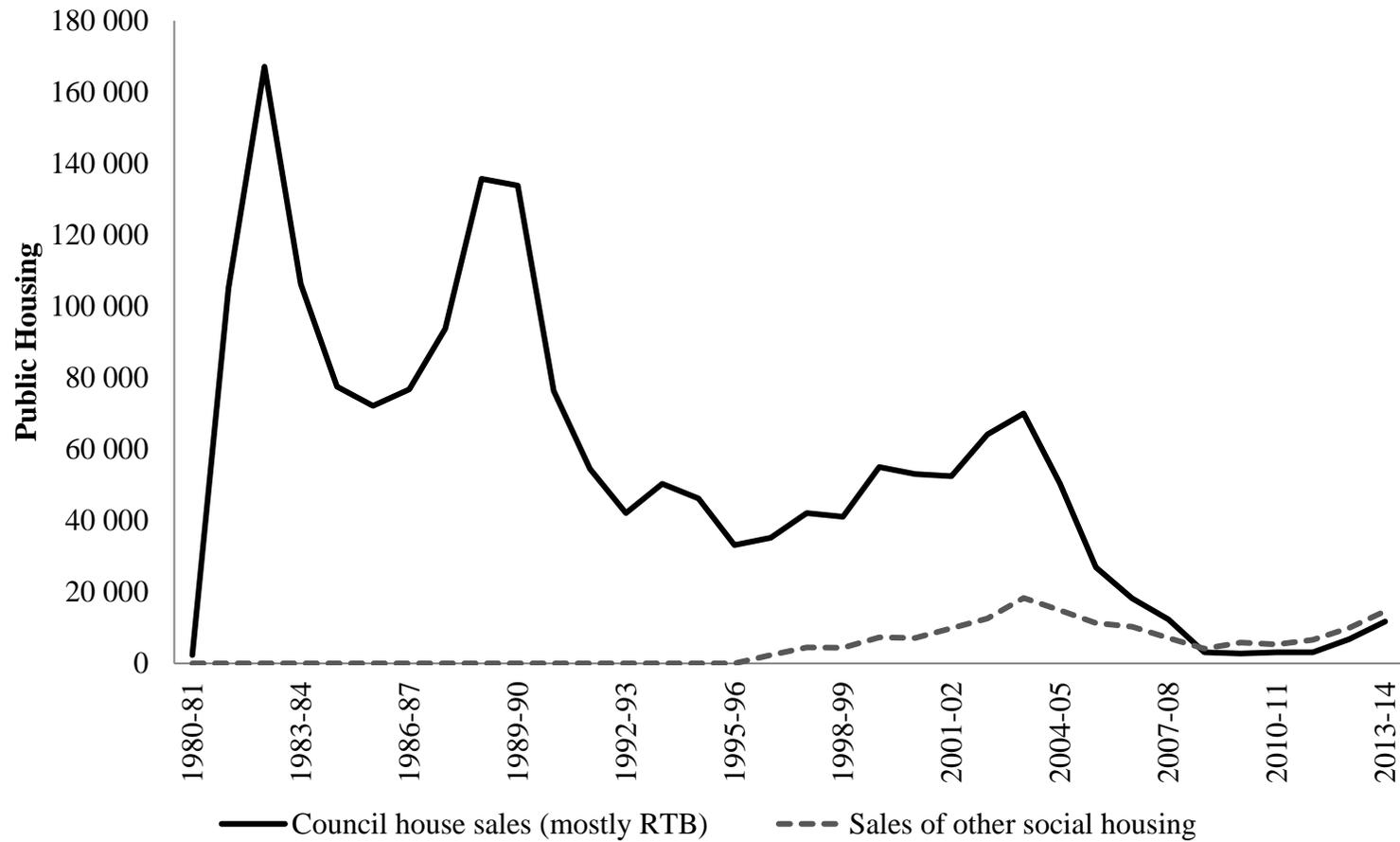
Source: Department of Communities and Local Government Housing Statistics, Table 678.

Figure 3. Correlation Between Crime and Public Housing in 1980, Police Force Area (PFA)
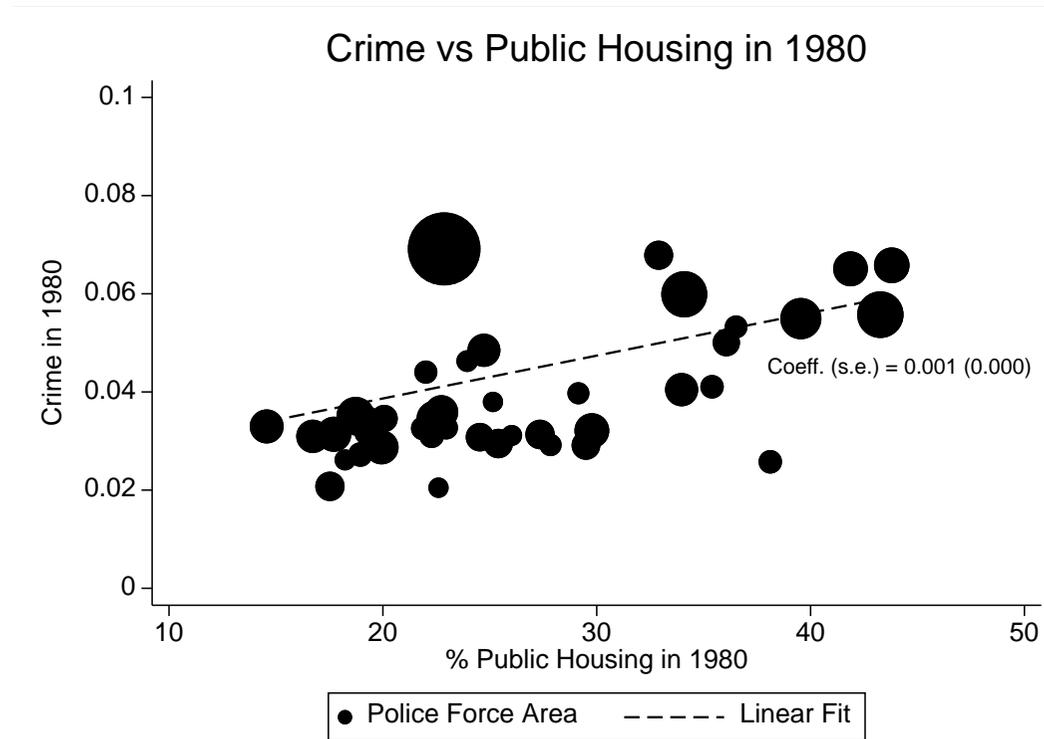
Notes: Figure shows for each Police Force Area (PFA) the crime rate in 1980 plotted against the fraction of public housing as a percent of the total residential stock in 1980. Crime is defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime 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.

Figure 4. Correlation Between Right-To-Buy (RTB) Public Housing Sales by 1981 and Tenure Duration in Public Housing in 1980.
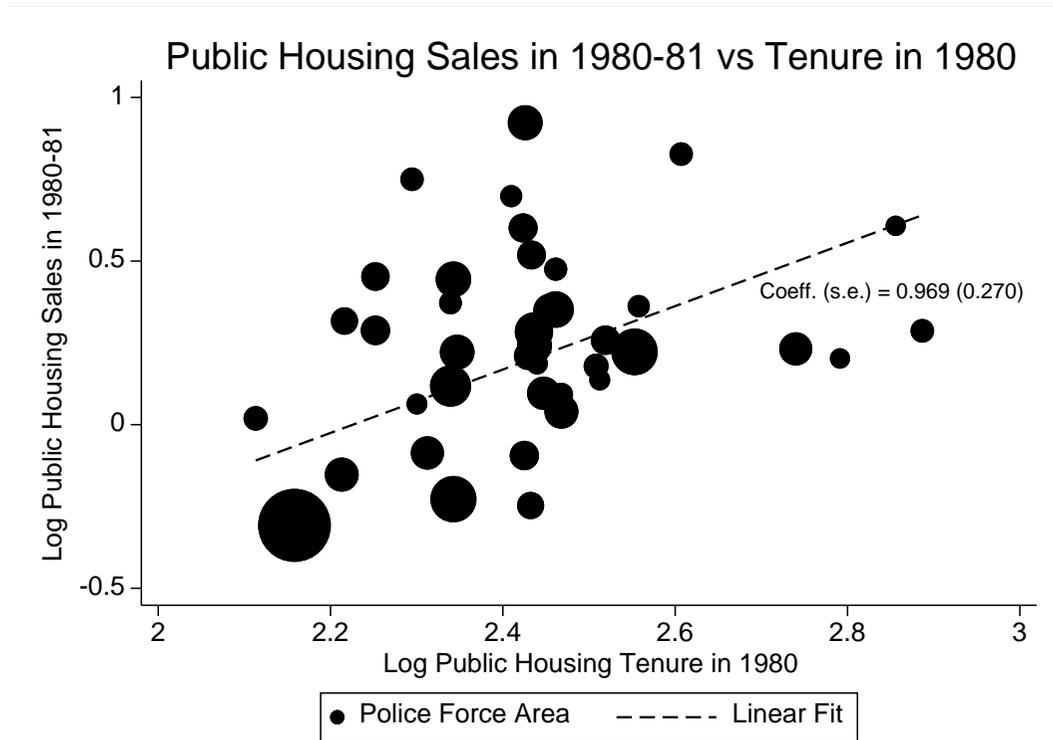
Notes: Figure shows for each Police Force Area (PFA) the log sales of public housing under the Right-To-Buy (RTB) scheme by 1981 plotted against PFA-specific log average years of tenure duration in public housing in 1980. A linear function fitting the distribution of PFA-specific log sales and log tenure duration weighted by PFA-specific population size in 1980 is also shown. For each PFA in the analysis, the horizontal axis shows the log value of average years of tenure duration in public housing in 1980, whereas the vertical axis shows the log count of sales of public housing under the RTB scheme by 1981 as a percent of the total residential stock in 1980.

Figure 5. Correlation Between Right-To-Buy (RTB) Public Housing Sales in 1980-92 and RTB Public Housing Sales by 1981.
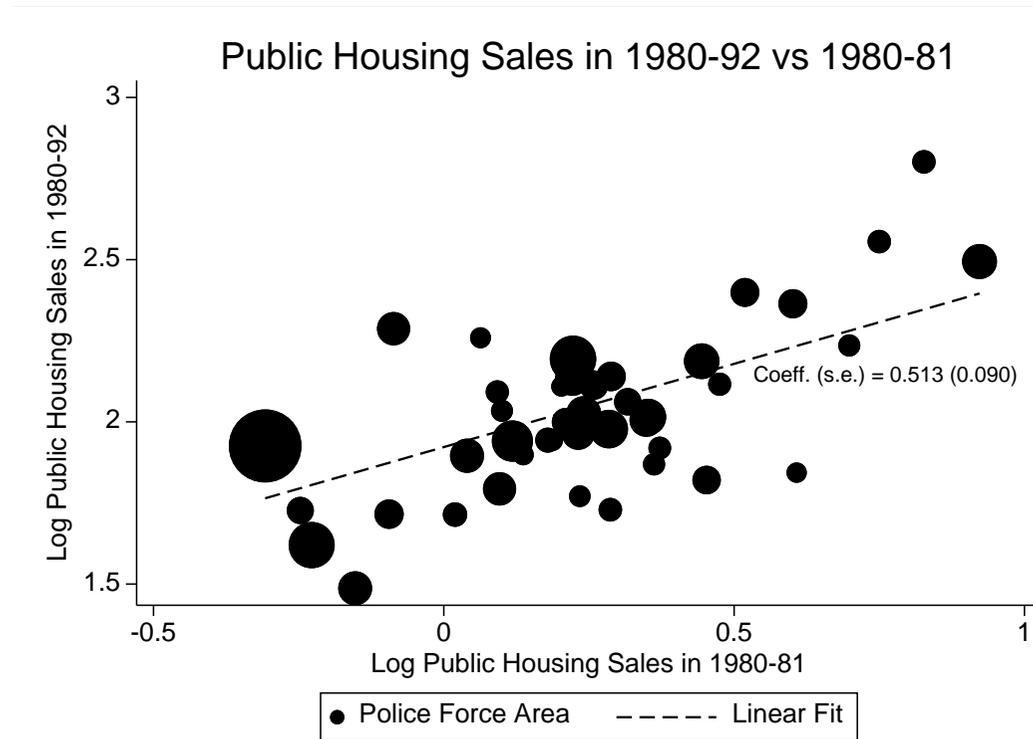
Notes: Figure shows for each Police Force Area (PFA) the log sales of public housing under the Right-To-Buy (RTB) scheme by 1992 plotted against PFA-specific log sales of public housing under the RTB scheme by 1981. A linear function fitting the distribution of PFA-specific log sales weighted by PFA-specific population size in 1980 is also shown. For each PFA in the analysis, the horizontal axis shows the log count of sales of public housing under the RTB scheme by 1981 as a percent of the total residential stock in 1980, whereas the vertical axis shows the log count of sales of public housing under the RTB scheme by 1992 as a percent of the total residential stock in 1980.

Figure 6. Crime Rates in Intention-To-Treat (ITT) Treatment Police Force Areas (PFAs) and in ITT Control PFAs, 1975-1992
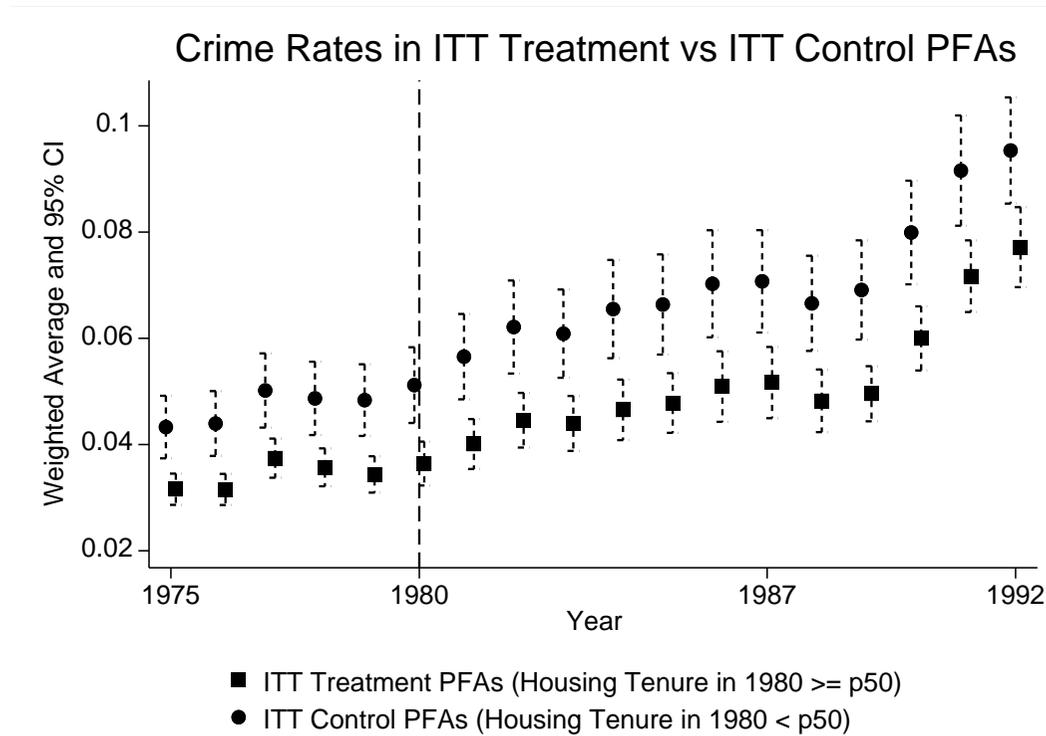
Notes: Figure shows crime rates separately for ITT Treatment PFAs (Housing Tenure duration in 1980 >= p50) and ITT Control PFAs (Housing Tenure duration in 1980 < p50). Crime defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined 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. Reported dots are averages weighted by population at the PFA level and capped bars are 95% confidence intervals.

Figure 7. County-Specific Estimates of Correlation between Right-To-Buy (RTB) Public Housing Sales and Crime Victimisation against County-Specific Estimates of Correlation between RTB Public Housing Sales and Home Improvement, 1988
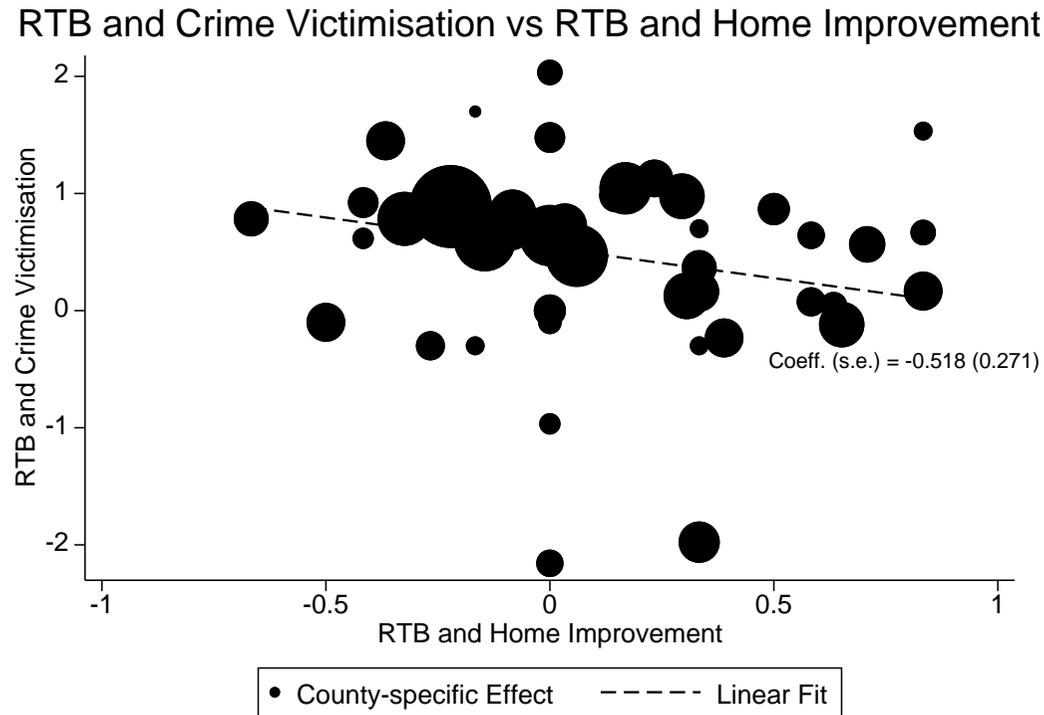
Notes: The figure shows county-specific estimates of the correlation between Right-To-Buy Scheme (RTB) public housing sales and crime victimisation plotted against county-specific estimates of the correlation between RTB public housing sales and home improvement. A linear function fitting the distribution of county-specific estimates is also shown. For each county in the analysis, the horizontal axis shows the estimate of the correlation between RTB public housing sales and home improvement, whereas the vertical axis shows county-specific estimates of the correlation between RTB public housing sales and crime victimisation. 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.

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

	Mean	Standard Deviation	Min	Max	Percentiles			
					p25	p50	p75	p90
Housing Stock								
Public Housing Stock (% Residential Stock)	27.202	8.574	14.568	43.798	21.865	22.966	34.096	41.869
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 Units	191721	177523	27612	563046	71116	98794	246040	563046
Residential Stock Units	725288	738428	128410	2462295	301632	427384	639665	2462295
Crime Variables								
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
Covariates								
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
Log 50th Percentile Real Hourly Earnings	1.613	0.069	1.488	1.751	1.565	1.606	1.635	1.751
Fraction 15-24 Year Olds in Population	0.157	0.008	0.138	0.173	0.152	0.158	0.162	0.165
Observations				42				

Notes: Crime variables are measured as crime rates at the Police Force Area (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 are the log of the unemployment rate, the 25th percentile log real hourly wage and the share of 15-24 year olds in the population, each measured at the PFA geography.

Table 2. Crime Rate, Public Housing Stock and Tenure Duration in 1980 OLS Estimates, Police Force Area (PFA)

	All Crime		Property Crime		Violent Crime		Log Housing Tenure Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public Housing (Prop. Res Stock)	0.087***	0.064***	0.082***	0.059***	0.005***	0.004***	0.082	-0.023
	(0.028)	(0.017)	(0.026)	(0.017)	(0.001)	(0.001)	(0.317)	(0.447)
Log Unemployment Rate		0.016***		0.015***		0.001**		0.015
		(0.005)		(0.005)		(0.000)		(0.104)
Log 25th Percentile Real Hourly Earnings		0.398***		0.378***		0.020***		-2.358
		(0.080)		(0.076)		(0.006)		(1.425)
Log 50th Percentile Real Hourly Earnings		-0.252***		-0.238***		-0.014**		1.400
		(0.078)		(0.074)		(0.007)		(1.440)
Fraction 15-24 Year Olds in Population		0.574**		0.546**		0.028*		-2.914
		(0.219)		(0.206)		(0.016)		(2.764)
Observations	42	42	42	42	42	42	42	42

Notes: Table reports estimates from Ordinary Least Squares regression models. Dependent variables are crime rates. Crime defined as total yearly counts of property and violent crime offences per population at the Police Force Area (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. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

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

	Intention-To-Treat (ITT) Treatment PFAs (Log Public Housing Tenure Duration in 1980 >= p50)	Intention-To-Treat (ITT) Control PFAs (Log Public Housing Tenure Duration in 1980 < p50)	Difference ((1) - (2))	Unconditional DiD	Unconditional DiD % Effect ((4) / Pre (2))
	(1)	(2)	(3)	(4)	(5)
Panel A. All Crime, 1975-85					
Pre-	0.034	0.047	-0.013		
Post-	0.043	0.060	-0.017		
Post – Pre	0.009	0.013		-0.004*	-8.5%
				(p = 0.085)	
Number of Police Force Areas	21	21			
Number of Observations	231	231			
Panel B. All Crime, 1975-92					
Pre-	0.035	0.047	-0.012		
Post-	0.053	0.070	-0.017		
Post – Pre	0.018	0.023		-0.005*	-10.6%
				(p = 0.072)	
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 Right to Buy (RTB) Policy in England and Wales. Panel A shows results for the 1975-85 period and Panel B shows results for the 1975-92 period. Crime defined as total yearly counts of property and violent crime offences per population at the Police Force Area (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 parentheses. % Effect calculated as estimated coefficient divided by mean of dependent variable prior to 1980 in the ITT control group PFAs (Public Housing Tenure duration in 1980 < p50).

Table 4. Estimates of Impact of Right-To-Buy Scheme on Crime

	ITT, 1975-85			IV, 1975-85			ITT, 1975-92		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(Log RTB Sales in 1980-81) x Post				-0.013** (p = 0.012)	-0.020** (p = 0.026)				
(Log Tenure >= p50) x Post	-0.005* (p = 0.085)					-0.006* (p = 0.073)			
Log Tenure x Post		-0.009* (p = 0.055)	-0.013* (p = 0.087)				-0.013** (p = 0.038)	-0.017** (p = 0.049)	
Log Tenure x 1988-92									-0.018** (p = 0.047)
Log Tenure x 1980-87									-0.017* (p = 0.090)
Log Tenure x 1979			-0.009 (p = 0.183)		-0.009 (p = 0.183)			-0.009 (p = 0.189)	-0.009 (p = 0.189)
Log Tenure x 1978			-0.007 (p = 0.514)		-0.007 (p = 0.541)			-0.007 (p = 0.525)	-0.007 (p = 0.525)
Log Tenure x 1977			-0.005 (p = 0.611)		-0.005 (p = 0.623)			-0.005 (p = 0.628)	-0.005 (p = 0.628)
% Effect	-10.638%	-0.191%	-0.277%	-0.277%	-0.425%	-12.766%	-0.277%	-0.362%	
% Effect x 1988-92									-0.383%
% Effect x 1980-87									-0.362%
Log Unemployment Rate in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log 25th Percentile Earnings in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log 50th Percentile Earnings in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Fraction 15-24 Years Old in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Fraction of Public Housing in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
PFA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage Coefficient on ITT				0.647** (p = 0.015)	0.647** (p = 0.019)				
Sample Size	462	462	462	462	462	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 Right to Buy (RTB) Policy in England and Wales. Columns (1)-(5) show ITT and IV results for the 1975-85 period and Columns (6)-(9) show ITT results for the 1975-92 period. Tenure duration in public housing measured in years and calculated in 1980. Sales of public housing calculated in 1980-81. Crime defined as total yearly counts of property and violent crime offences per population at the Police Force Area (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 parentheses. % Effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable in ITT control group (Public Housing Sales by 1981 < p50). *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 5. Estimates of Impact of Right-To-Buy Scheme on Property Crime

	ITT, 1975-85			IV, 1975-85		ITT, 1975-92			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(Log RTB Sales in 1980-81) x Post				-0.013** (p = 0.012)	-0.018** (p = 0.021)				
(Log Tenure >= p50) x Post	-0.004* (p = 0.089)					-0.005 (p = 0.102)			
Log Tenure x Post		-0.008* (p = 0.052)	-0.012* (p = 0.085)				-0.012** (p = 0.038)	-0.016* (p = 0.052)	
Log Tenure x 1988-92									-0.015* (p = 0.095)
Log Tenure x 1980-87									-0.017* (p = 0.099)
Log Tenure x 1979			-0.008 (p = 0.168)		-0.008 (p = 0.173)			-0.008 (p = 0.177)	-0.008 (p = 0.177)
Log Tenure x 1978			-0.006 (p = 0.518)		-0.006 (p = 0.546)			-0.006 (p = 0.535)	-0.006 (p = 0.535)
Log Tenure x 1977			-0.004 (p = 0.609)		-0.004 (p = 0.619)			-0.004 (p = 0.624)	-0.004 (p = 0.623)
% Effect	-9.091%	-0.182%	-0.273%	-0.295%	-0.409%	-11.364%	-0.273%	-0.364%	
% Effect x 1988-92									-0.341%
% Effect x 1980-87									-0.386%
Log Unemployment Rate in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log 25th Percentile Earnings in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log 50th Percentile Earnings in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Fraction 15-24 Years Old in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Fraction of Public Housing in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
PFA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage Coefficient on ITT				0.647** (p = 0.015)	0.647** (p = 0.019)				
Sample Size	462	462	462	462	462	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 Right to Buy (RTB) Policy in England and Wales. Columns (1)-(5) show ITT and IV results for the 1975-85 period and Columns (6)-(9) show ITT results for the 1975-92 period. Tenure duration in public housing measured in years and calculated in 1980. Sales of public housing calculated in 1980-81. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the Police Force Area (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 parentheses. % Effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable in ITT control group (Public Housing Sales by 1981 < p50). *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 6. Estimates of Impact of Right-To-Buy Scheme on Violent Crime

	ITT, 1975-85			IV, 1975-85		ITT, 1975-92			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(Log RTB Sales in 1980-81) x Post				-0.001 (p = 0.166)	-0.001 (p = 0.104)				
(Log Tenure >= p50) x Post	-0.000 (p = 0.186)					-0.001 (p = 0.124)			
Log Tenure x Post		-0.000 (p = 0.228)	-0.001 (p = 0.202)				-0.001 (p = 0.204)	-0.001 (p = 0.212)	
Log Tenure x 1988-92									-0.003 (p = 0.383)
Log Tenure x 1980-87									-0.000 (p = 0.697)
Log Tenure x 1979			-0.001 (p = 0.480)		-0.001 (p = 0.493)			-0.001 (p = 0.480)	-0.001 (p = 0.481)
Log Tenure x 1978			-0.001 (p = 0.472)		-0.001 (p = 0.480)			-0.001 (p = 0.474)	-0.001 (p = 0.475)
Log Tenure x 1977			-0.000 (p = 0.653)		-0.000 (p = 0.611)			-0.000 (p = 0.654)	-0.000 (p = 0.655)
% Effect	-0%	-0%	-0.333%	-0.333%	-0.333%	-33.333%	-0.333%	-0.333%	
% Effect x 1988-92									-1%
% Effect x 1980-87									-0%
Log Unemployment Rate in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log 25th Percentile Earnings in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Log 50th Percentile Earnings in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Fraction 15-24 Years Old in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Fraction of Public Housing in 1980 x Post	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
PFA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage Coefficient on ITT				0.647** (p = 0.015)	0.647** (p = 0.019)				
Sample Size	462	462	462	462	462	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 Right to Buy (RTB) Policy in England and Wales. Columns (1)-(5) show ITT and IV results for the 1975-85 period and Columns (6)-(9) show ITT results for the 1975-92 period. Tenure duration in public housing measured in years and calculated in 1980. Sales of public housing calculated in 1980-81. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the Police Force Area (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 parentheses. % Effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable in ITT control group (Public Housing Sales by 1981 < p50). *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 7. Estimates of Impact of Right-To-Buy (RTB) Public Housing Tenure Duration on Local Area Outcomes and (Log) Value of RTB Public Housing Sales on Crime, 1975 – 1992

	Log Unemployment Rate	Log 25th Percentile Real Hourly Earnings	Log 50th Percentile Real Hourly Earnings	Fraction 15-24 Years Old in Population	Total Crime	Log Count of Police Officers
	(1)	(2)	(3)	(4)	(5)	(6)
Log Tenure x Post	-0.168	-0.079	-0.076	0.002		-0.024
	(p = 0.313)	(p = 0.269)	(p = 0.346)	(p = 0.746)		(p = 0.594)
Log Value Sales x Post					0.002	
					(p = 0.280)	
% Effect				0.014%	0.043%	
Log Unemployment Rate in 1980 x Post	No	Yes	Yes	Yes	Yes	Yes
Log 25th Percentile Earnings in 1980 x Post	Yes	No	No	Yes	Yes	Yes
Log 50th Percentile Earnings in 1980 x Post	Yes	No	No	Yes	Yes	Yes
Fraction 15-24 Years Old in 1980 x Post	Yes	Yes	Yes	No	Yes	Yes
Fraction of Public Housing in 1980 x Post	Yes	Yes	Yes	Yes	Yes	Yes
PFA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	756	756	756	756	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 Right to Buy (RTB) Policy in England and Wales. 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 Police Force Area (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 parentheses. % Effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable in ITT control group (Public Housing Tenure duration in 1980 < p50), except in columns (1), (2) and (5) where the dependent variable is measured in log values.

Table 8. Correlation between Right-To-Buy Public Housing Sales and Home Improvement, Insurance and Social Behaviour, 1988.

	Installed Double Locks on Outside Doors	Installed Locks on Windows	Theft Insurance of Home Contents	Installed Burglar Alarm	Neighbourhood Watch Scheme
	(1)	(2)	(3)	(4)	(5)
Panel A. RTB Scheme	0.217***	0.193***	0.251***	0.043**	0.005
	(p = 0.000)	(p = 0.000)	(p = 0.000)	(p = 0.037)	(p = 0.828)
% Effect	60.6%	130.4%	38.4%	226.3%	8.1%
Panel B. RTB Scheme	0.232***	0.168***	0.239***	0.048**	0.018
	(p = 0.000)	(p = 0.003)	(p = 0.000)	(p = 0.046)	(p = 0.505)
% 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 Victimization	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 parentheses. % 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 victimization, household type, household rooms, household income, employment status and county fixed effects were included in the estimating equation. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 9. ITT Estimates of Impact of Right-To-Buy Scheme on Crime Separately by Crime Category

	Property Crime		Violent Crime		
	Burglary	**Theft and Handling of Stolen Goods**	**Violence Against the Person**	**Sexual Offences**	**Robbery**
	(1)	(2)	(3)	(4)	(5)
Panel A. Crime Category, 1975 – 1985					
Log Tenure x Post	-0.004*	-0.004*	-0.0002	0.000	-0.0003*
	(p = 0.083)	(p = 0.070)	(p = 0.531)	(p = 0.894)	(p = 0.082)
% Effect	-0.309%	-0.132%	-0.104%	0.021%	-87.392%
Log Unemployment Rate in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Log 25th Percentile Earnings in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Log 50th Percentile Earnings in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Fraction 15-24 Years Old in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Fraction of Public Housing in 1980 x Post	Yes	Yes	Yes	Yes	Yes
PFA Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Sample Size	462	462	462	462	462
Number of PFAs	42	42	42	42	42
Panel B. Crime Category, 1975 – 1992					
Log Tenure x Post	-0.006**	-0.006	-0.0004	0.000	-0.0004*
	(p = 0.016)	(p = 0.130)	(p = 0.447)	(p = 0.987)	(p = 0.090)
% Effect	-0.489%	-0.191%	-0.191%	0.002%	-127%
Log Unemployment Rate in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Log 25th Percentile Earnings in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Log 50th Percentile Earnings in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Fraction 15-24 Years Old in 1980 x Post	Yes	Yes	Yes	Yes	Yes
Fraction of Public Housing in 1980 x Post	Yes	Yes	Yes	Yes	Yes
PFA Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Sample Size	756	756	756	756	756
Number of PFAs	42	42	42	42	42

Notes: Post-period defined as starting from 1980, the date of adoption of the Right to Buy (RTB) Policy in England and Wales. Panel A shows results for the 1975-85 period and Panel B shows results for the 1975-92 period. Tenure duration in public housing measured in years and calculated in 1980. Yearly counts of crime were measured at the Police Force Area (PFA) level for all crime categories. 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 parentheses. % Effect calculated as estimated coefficient divided by pre-1980 mean of dependent variable in ITT control group (Public Housing Tenure duration in 1980 < p50). *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 10. Summary Data for Long-Difference Analysis

	Mean	Standard Deviation	Min	Max	Percentiles			
					p25	p50	p75	p90
Housing Variables								
1.Public House Sales 1980-1992	3718	3429	151	33386	1843	2643	4223	7857
2.Public House Stock 1980	15032	16757	2180	148394	6358	8912	18018	32832
1./2.	29	13	0	116	22	27	35	42
Crime Variables								
All Crime Rate	-0.020	0.011	-0.075	0.002	-0.026	-0.019	-0.013	-0.008
Property Crime Rate	-0.015	0.009	-0.064	0.001	-0.019	-0.013	-0.009	-0.006
Violent Crime Rate	-0.005	0.004	-0.018	0.001	-0.007	-0.005	-0.003	-0.001
Covariates								
Log Unemployment Rate	0.466	0.462	-0.816	1.901	0.183	0.476	0.768	1.004
Log 25th Percentile Real Hourly Earnings	-0.084	0.058	-0.278	0.139	-0.117	-0.080	-0.047	-0.018
Log 50th Percentile Real Hourly Earnings	-0.080	0.064	-0.288	0.096	-0.114	-0.083	-0.036	-0.002
Fraction of 15-24 Year Olds in Population	-0.000	0.009	-0.035	0.034	-0.004	0.001	0.004	0.009
Observations				293				

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

Table 11. IV Estimates of Impact of Change in Public Housing Stock 1980-1992 on Change in Crime Rate 2003-2017

	Δ All Crime Rate			Δ Property Crime Rate			Δ Violent Crime Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
RTB Sales 1980-1992 / 1980 Residential Stock	-0.339***	-0.310***	-0.245***	-0.236***	-0.218***	-0.179***	-0.103***	-0.092***	-0.073***
	(0.053)	(0.050)	(0.042)	(0.038)	(0.037)	(0.032)	(0.017)	(0.016)	(0.014)
Changes in Covariates									
Log Unemployment Rate		0.003	0.002		0.002	0.001		0.001	0.001
		(0.002)	(0.002)		(0.002)	(0.002)		(0.001)	(0.001)
Log 25th Percentile Earnings		-0.048***	-0.055***		-0.034***	-0.039***		-0.014***	-0.013***
		(0.023)	(0.020)		(0.017)	(0.016)		(0.008)	(0.006)
Log 50th Percentile Earnings		0.009	0.011		0.009	0.001		-0.001	-0.001
		(0.022)	(0.018)		(0.016)	(0.001)		(0.007)	(0.001)
Fraction 15-24 Year Olds		0.428***	0.250**		0.278***	0.169**		0.149***	0.119***
		(0.104)	(0.095)		(0.077)	(0.074)		(0.034)	(0.029)
Crime Rate in 1980									
All Crime Rate in 1980			-0.319***						
			(0.053)						
Property Crime Rate in 1980						-0.203***			
						(0.043)			
Violent Crime Rate in 1980									-1.718***
									(0.287)
Observations	293	293	293	293	293	293	293	293	293

Notes: Table reports Instrumental Variable estimates. Crime defined as total yearly counts of property and violent crime offences per population at the LA level. Property crime defined as total yearly counts of burglary and theft or handling of stolen goods offences per population at the LA level. Violent crime defined as total yearly counts of robbery, violent and sexual offences per population at the LA level. Dependent variable is the change in the Local Authority (LA) crime rate between 2003 and 2017 for each crime type denoted by column headings. Independent variables are i) total Right-to-Buy sales within the LA for the period 1980 to 1992 (divided by 1980 residential stock), ii) changes in the log unemployment rate, in the 25th percentile log real hourly wage and in the share of 15-24 year olds in the population, each measured at the LA level, iii) crime rates in 1980 for each crime type denoted by column headings, measured at the Police Force Area (PFA) level. The instrument for i) is the stock of public housing in the local authority in 1980 (divided by 1980 residential stock). *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table 12. IV Estimates of Impact of Change in Public Housing Stock 1980-1992 on Changes in (Log) Average House Price and Conservative Vote Share

	Δ (Log) Average House Price	Δ Con Vote Share in LA
	(1)	(2)
RTB Sales 1980-1992 / 1980 Residential Stock	-1.327***	1.757***
	(0.475)	(0.352)
Changes in Covariates		
Log Unemployment Rate	-0.070***	-0.014
	(0.021)	(0.017)
Log 25th Percentile Earnings	-0.740***	0.379**
	(0.213)	(0.177)
Log 50th Percentile Earnings	-0.069	-0.111
	(0.198)	(0.158)
Fraction 15-24 Year Olds	-8.575***	-1.188
	(1.005)	(0.891)
Observations	290	235

Notes: Table reports Instrumental Variable estimates. In Column (1) the dependent variable is the change in the log of the average house price in the Local Authority (LA) between 2003 and 2017. In Column (2) the dependent variable is the change in the Conservative vote share in Local Authority elections between 1995/6 and 2014/15. Independent variables are i) total Right-to-Buy sales within the LA for the period 1980 to 1992 (divided by 1980 residential stock), ii) changes in the log unemployment rate, in the 25th percentile log real hourly wage and in the share of 15-24 year olds in the population, each measured at the LA level. The instrument for i) is the stock of public housing in the local authority in 1980 (divided by 1980 residential stock). *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

A. Data Sources

    a. Public Housing Stock and Residential Stock in 1980

    Public housing stock records for 1980 are sourced from the Chartered Institute of Public Finance and Accountancy (CIPFA) Rate Collection Statistics 1980-81 Actuals, SIS ref: 42.82. This publication is based on data returned by authorities from DoE forms VO7140. The data covers 33 London Boroughs, 34 Metropolitan District Councils, 286 English Non-Metropolitan District Councils and 37 Welsh District Councils, which we refer to in the main paper as Local Authorities. The data cover 97.3% of total rateable hereditaments. The number of residential properties in the Local Authority is stated at Column 2 'Domestic', with the number of public house properties in the Local Authority stated at Column 22 'Council Dwellings'. These data were extracted from hard copy held by UK libraries.

    b. Right-to-Buy Public Housing Sales

    Data on the number of public housing properties sold each year through the Right-to-Buy scheme by each Local Authority for the period 1980 - 2001 are sourced from the CIPFA Housing Revenue Account (HRA). The HRA records expenditure and income on running a council's own housing stock and closely related services or facilities, which are provided primarily for the benefit of the council's own tenants. Under the Housing Act 1985 Local Authorities are obliged to report revenues from public house sales via their HRA. These data are accessed under license from CIPFA. For further details see https://www.cipfa.org/policy-and-guidance/publications (link last accessed on 28 March 2020). We use these data in our main analysis. For Figures 1 and 2 we also source data on sales under the Right-to-Buy scheme after 2001. These data are provided by the Department for Communities and Local Government Table 685. For further details see https://www.gov.uk/government/statistical-data-sets/live-tables-on-social-housing-sales (link last accessed on 28 March 2020).

    c. Crime

    Area-level crime data compiled by the Home Office and published annually in Criminal Statistics for the period between 1975 and 1992 were sourced from the archives of the LSE Library, where both hard and soft copies of the data can be accessed. Yearly crime records of all Local Police Forces in England and Wales were available at the Police Force Area (PFA) level and, with the sole exception of the City of London, they were used in this analysis. Area-level crime data compiled by the Office for National Statistics (ONS) and published annually on the ONS website for the period between 2003 and 2017 were available at the Local Authority (LA) level. Due to the redrawing of LA boundaries over time, it was not possible to include all LAs in the analysis. Our analysis for the period between 2003 and 2017 therefore includes 293 LAs. These data can be freely accessed here: https://www.ons.gov.uk/peoplepopulationandcommunity/crimeandjustice/datasets/recordedcrimedataatcommunitysafetypartnershiplocalauthoritylevel/current (link last accessed on 28 March 2020).

    d. Local Area Circumstances and Housing Tenure

    Local area circumstances were measured using the New Earnings Survey (NES) provided by the Department for Employment for the period 1975-1992, and using the version of the Annual Population Survey (APS) that is publicly available on the Nomis website for the period 2003-2017. Access to data on local unemployment rates during the period 1975-1992 was obtained from the Department of Employment. Permission to access the NES data must

be obtained via the UK Data Service and are downloadable at the following link: https://beta.ukdataservice.ac.uk/datacatalogue/studies/#!?Search=new%20earnings%20survey&Rows=10&Sort=1&DateFrom=440&DateTo=2020&Page=1 (link last accessed on 28 March 2020). The Nomis website can be freely accessed here: https://www.nomisweb.co.uk/ (link last accessed on 28 March 2020). Local area population data compiled by the Home Office and published annually in Criminal Statistics for the period between 1975 and 1992 were sourced from the archives of the LSE Library, where both hard and soft copies of the data can be accessed. Area-level population data compiled by the Office for National Statistics (ONS) and published annually on the ONS website for the period between 2003 and 2017 were available at the Local Authority (LA) level. These data can be freely accessed here: https://www.ons.gov.uk/peoplepopulationandcommunity/populationandmigration/populationestimates/datasets/populationestimatesforukenglandandwalesscotlandandnorthernireland (link last accessed on 28 March 2020). Data on local housing tenure duration were obtained from the 1980 General Household Survey, which can be accessed via the UK Data Service and are downloadable at this link (last accessed on 09 July 2020): https://beta.ukdataservice.ac.uk/datacatalogue/series/series?id=200019.

e. Home Improvement, Insurance and Social Behaviour (Table 8, Appendix Tables A1 and A2)

Individual level data on homeownership, expenditure, individual and property characteristics, income, employment status and locality of residence used in Table 8 were obtained from the British Crime Survey (BCS) 1988. Access to the BCS 1988 data must be obtained via the UK Data Service and are downloadable at the following link: https://beta.ukdataservice.ac.uk/datacatalogue/studies/study?id=2706&type=Data%20catalogue (link last accessed on 28 March 2020).

f. Election Results

Data on the Conservative vote share in Local Elections for the period 1995/6 to 2014/15 are sourced from the British Local Elections Database maintained by the University of Plymouth, UK. The Conservative vote share is calculated as the proportion of all votes cast in the election for Conservative candidates. Ware, L., Rallings, C., Thrasher, M. (2006). British Local Election Database, 1889-2003. [data collection]. UK Data Service. SN: 5319, http://doi.org/10.5255/UKDA-SN-5319-1 (link last accessed on 28 March 2020).

g. House Prices

House price data is obtained from the UK Land Registry House Price Index (HPI), the UK's official house price index. The HPI uses house sales data from HM Land Registry, Registers of Scotland, and Land and Property Services Northern Ireland and is calculated and maintained by the Office for National Statistics. Local Authority house price index values were extracted from the HPI Full File. For further details see https://landregistry.data.gov.uk/app/doc/ukhpi (link last accessed on 28 March 2020).

Figure A1. Correlation between Public House building in 1945-64 and Bombs per Dwelling dropped during World War II by Borough in London.
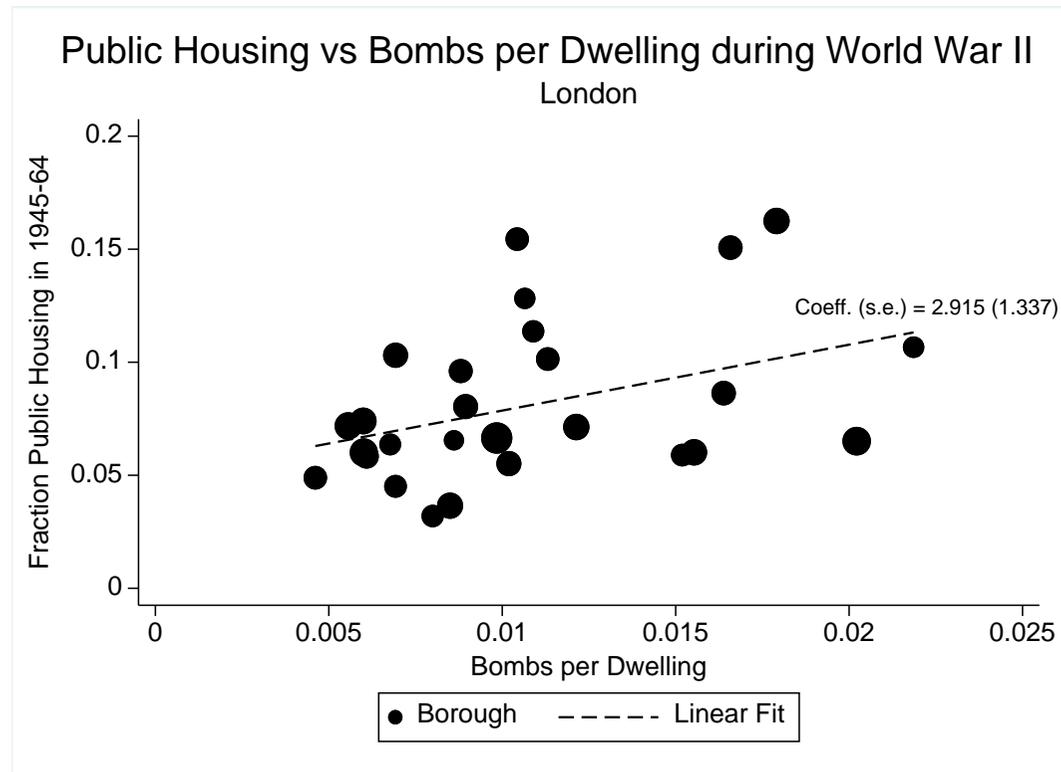

Notes: The figure shows for each Borough in London the public houses built from 1945-64 as a fraction of the residential stock plotted against the average number of bombs per dwelling. A linear function fitting the correlation between the Borough-specific average number of bombs per dwelling and the public housing built from 1945-64 as a fraction of the residential stock is also shown in the figure.

Figure A2. Residential Housing Stock in 1980, by Local Authority (LA)

a) Public Housing Stock (% Residential Stock)
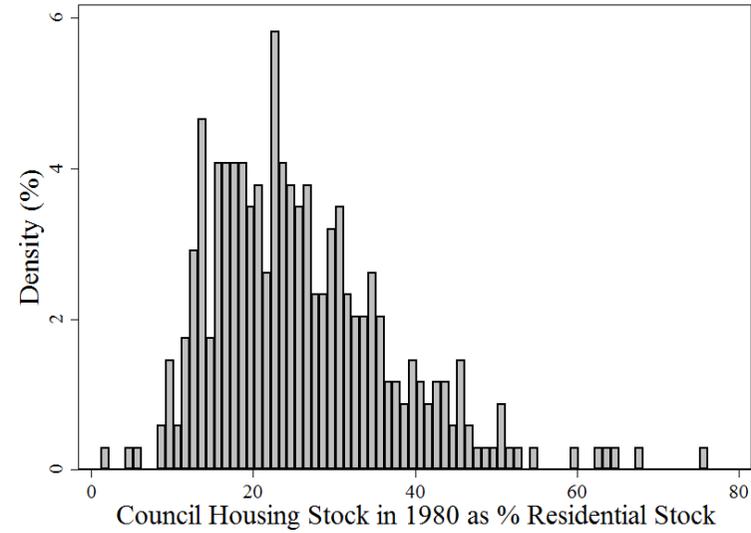
**b) Public Housing Stock (N)**
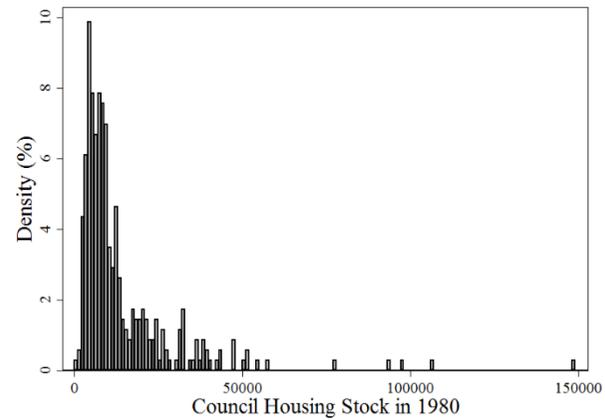


c) Residential Stock (N)
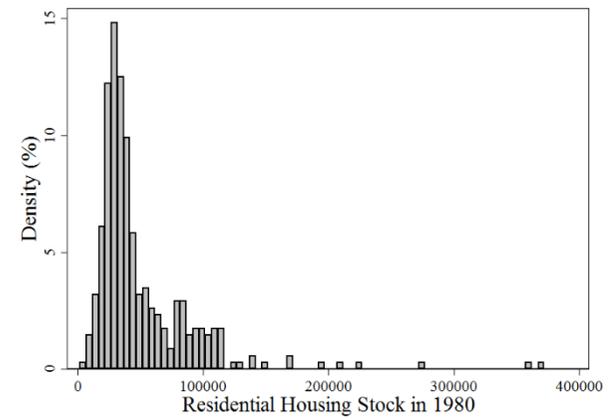
Notes: Data from 344 Local Authority (LA) Districts in 1980. Panel a shows public housing stock in 1980 within the LA (shown in Panel b) divided by the total residential housing stock in 1980 within the LA (shown in Panel c).

Figure A3. Right-to-Buy Public Housing Sales 1980-1992, by Local Authority (LA)

a) Right-to-Buy Public Housing Sales (% 1980 Residential Stock)
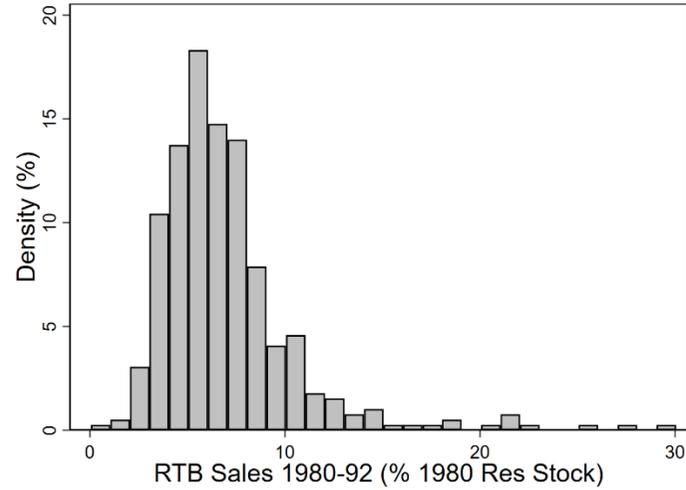
**b) Right-to-Buy Public Housing Sales (N)**
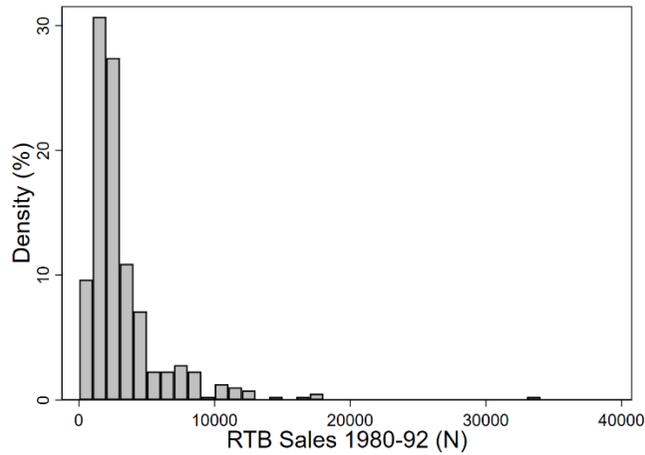
**c) Right-to-Buy Public Housing Sales (% 1980 Public Housing Stock)**
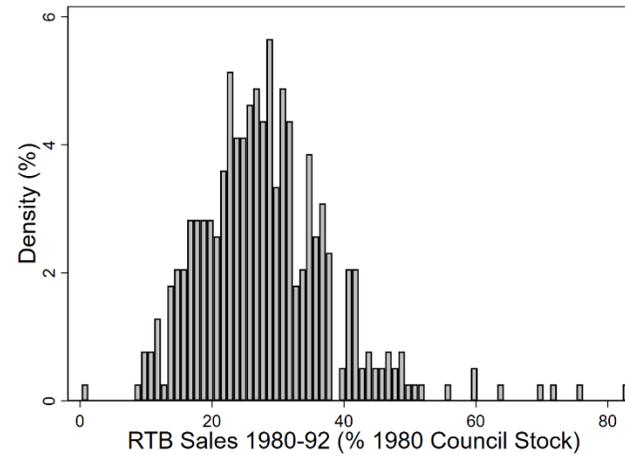
Notes: Data from 344 Local Authority (LA) Districts. Panel A shows total RTB public housing sales in the period 1980-1992 as a percentage of the 1980 residential stock in the LA. Panel B shows a count of total RTB public housing sales over the period 1980-1992 in the LA. Panel C shows total RTB public housing sales in the period 1980-1992 as a percentage of the 1980 public housing stock in the LA.

Figure A4. Correlation between Crime in 1980 and Right-To-Buy Public Housing Sales by 1981.
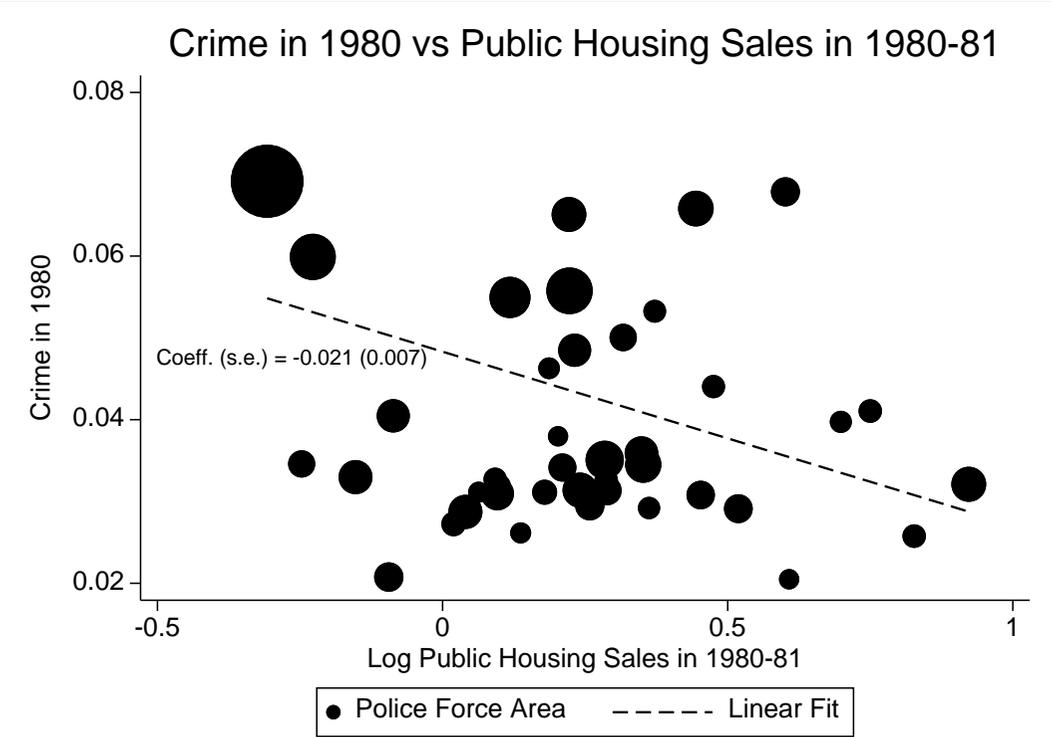

Notes: Figure shows for each Police Force Area (PFA) the crime rate in 1980 plotted against the log sales of public housing under the Right-To-Buy (RTB) scheme by 1981. Crime is defined as total yearly counts of property and violent crime offences per population at the PFA level. Property crime defined 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 distribution of PFA-specific crime and RTB sales weighted by PFA-specific population size in 1980 is also shown. For each PFA in the analysis, the horizontal axis shows the log count of sales of public housing under the RTB scheme by 1981 as a percent of the total residential stock in 1980, whereas the vertical axis shows the crime rate in 1980.

Figure A5. Correlation between Right-To-Buy Public Housing Sales in 1980-92 and Conservative Share in 1977 Local Elections.

Public Housing Sales in 1980-92 vs Conservative Share

Coeff. (s.e.) = -0.000 (0.007)
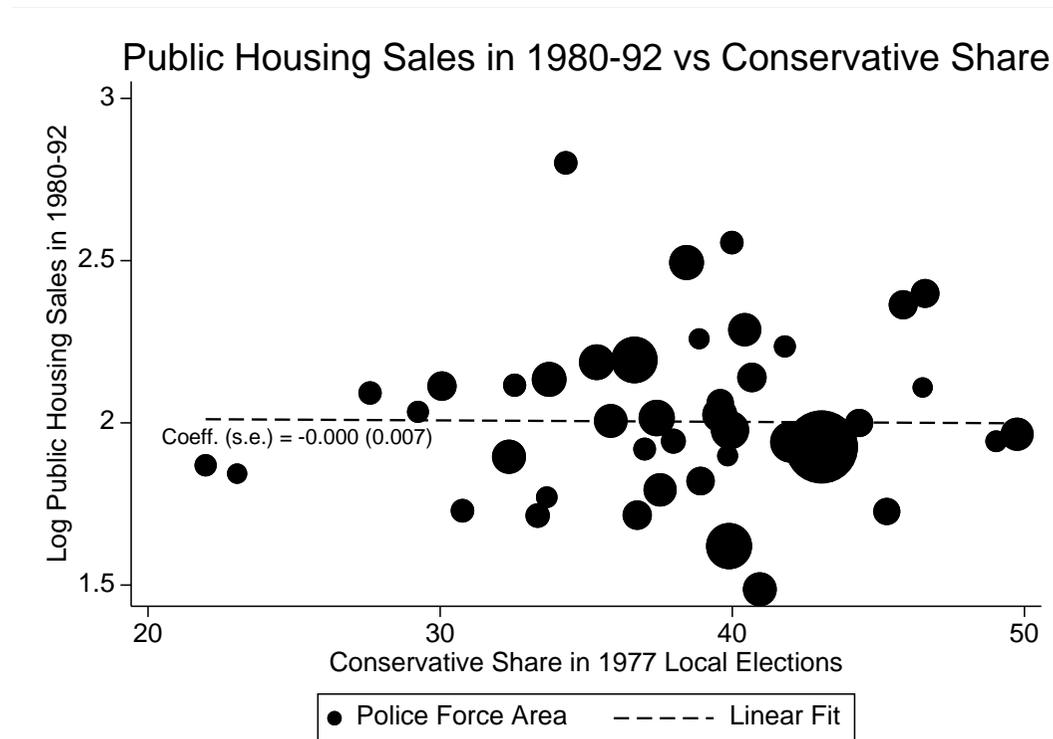
Notes: Figure shows for each Police Force Area (PFA) the sales of public housing under the Right-To-Buy (RTB) scheme by 1992 plotted against the share of conservative votes in the local elections of 1977. A linear function fitting the distribution of PFA-specific sales and conservative votes weighted by PFA-specific population size in 1980 is also shown. For each PFA in the analysis, the horizontal axis shows the share of conservative votes in the local elections of 1977, whereas the vertical axis shows the log count of sales of public housing under the RTB scheme by 1992 as a percent of the total residential stock in 1980.

Figure A6. Right-to-Buy Public Housing Sales in 1980-92 and Public Housing Stock in 1980

Public Housing Sales in 1980-92 vs Public Housing in 1980

Coeff. (s.e.) = 0.180 (0.018)
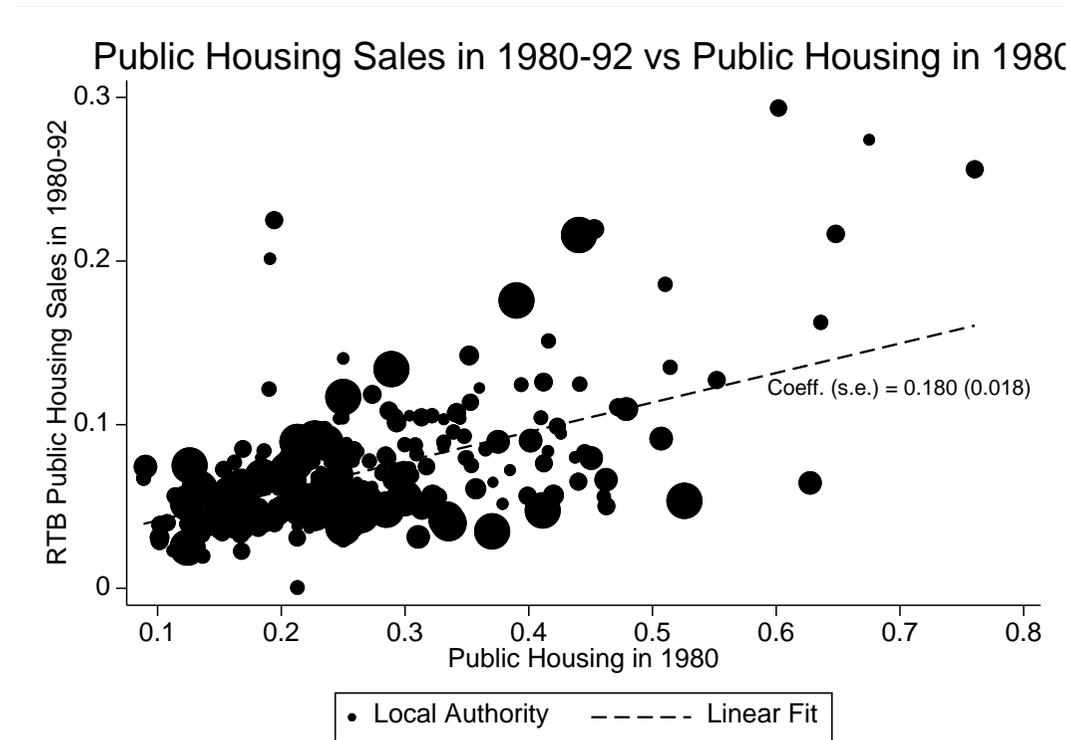
Notes: Figure shows scatter plot of public housing in 1980 (as a proportion of the Local Authority resident stock) on the x-axis and subsequent Right-to-Buy public housing sales between 1980 and 1992 (as a proportion of the Local Authority residential stock) on the y-axis. Binned scatterplot of 344 observations.

Table A1. Correlation between Right-To-Buy Public Housing Sales and Characteristics of Individuals and Households, 1988.

	(Log) Age	Male	White British	Income Group	No. Rooms	Housing Type	Past Burglary Victimization
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RTB Scheme	0.055	0.015	0.018	-0.031	0.017	-0.078	0.005
	(p = 0.230)	(p = 0.713)	(p = 0.366)	(p = 0.489)	(p = 0.802)	(p = 0.105)	(p = 0.886)
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
% Effect	5.5%	3%	1.9%	-3.4%	0.4%	-6.9%	3.8%
Mean Dep. Var. Control Group	3.614	0.5	0.938	0.920	4.715	1.123	0.130
Sample Size	520	520	520	520	520	520	520
Number of Counties	52	52	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 parentheses. % 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. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

Table A2. Correlation between Right-To-Buy Public Housing Sales and Crime Victimisation, 1988.

	Crime Victimisation					
	(1)	(2)	(3)	(4)	(5)	(6)
		% Effect		% Effect		% Effect
RTB Scheme	-0.202***	-24.9%	-0.171*	-21.1%	-0.193***	-23.8%
	(p = 0.002)		(p = 0.059)		(p = 0.005)	
RTB Share					-0.013	-1.6%
					(p = 0.430)	
County Fixed Effects	No		Yes		No	
Mean Dep. Var. Control Group	0.810		0.810		0.810	
Sample Size	1058		1058		1058	
Number of Counties	53		53		53	

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. For each respondent, RTB Share calculated as share of other treatment group respondents at the county level (leaving out the respondent herself/himself). Standard errors were clustered at the county level and p-values from Wild Cluster Bootstrap are reported in parentheses. % 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. *** indicates significance at 1%. ** indicates significance at 5%. * indicates significance at 10%.

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