THE ECONOMIC AND SOCIAL CONSEQUENCES OF HOUSING TENURE

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Declaration

This thesis is submitted according to the requirements of the Degree Committee of Land Economy. It does not exceed the regulation length of 80,000 words including footnotes, references and appendices. It is the result of my own work and includes nothing which is the outcome of work done in collaboration with others, except where specifically indicated in the text and Acknowledgements. I further state that no substantial part of my thesis has already been submitted, or, is being concurrently submitted for any such degree, diploma or other qualification at the University of Cambridge or any other University or similar institution.

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Abstract

This thesis explores the consequences of housing tenure in three separate yet interlinked domains that entail economic and social dimensions: portfolio choice, mental health, and voting behaviour. It does so by crystallising the fundamental features of housing tenure and by characterising conceptually tenure transitions as salient events, drawing tight links to recent advances in the statistics of event studies. Crucially, an individual's housing tenure stems from an endogenous decision that makes it hard to disentangle causal effects. I approach such an identification problem by leveraging insights in housing theory from different disciplines, in combination with the bleeding edge of the realm of statistics that focuses on events affecting different groups at different points in time. I argue that housing, because of its dual nature as both a weighty consumption good and a large asset in portfolios, can be seen as the connecting point between seemingly disparate areas of the lived experience of individuals and households. While housing tenure can take many forms, I focus mostly on a tripartite categorization in renting, mortgaged homeownership, and outright homeownership. Consequently, the events of interest are two: the transition from renting to mortgaged homeownership and the transition from mortgaged homeownership to outright homeownership. Using large surveys from the United Kingdom, I find both transitions to have consequences but, in fact, the transition to outright homeownership to be more conspicuous. Such findings have implications for policy: while I do not analyse housing policy changes per se, tracing out the consequences of tenure transitions offers insights on how changes in tenure composition (possibly propelled by policy decisions) may affect aggregate outcomes. The discussion of the fundamental features of housing tenure, as well as of the tenure transition as an event and its statistical implications, constitute the first chapter of the thesis. The remaining chapters delve into the three realms of portfolio choice, mental health, and voting behaviour, delineating the connections between them.

To those who believe in freedom and in solidarity,
and stand by them

Acknowledgements

As my time at Cambridge comes to a conclusion, acknowledgements to those near and far who shared this journey are in order. I would like to thank Franz Fuerst for his constant presence and support, to navigate both the complex but overt subtleties of research as well as the less obvious intricacies of the *hidden curriculum*; Franz also worked with me on the paper that constitutes Chapter 2 of the thesis. I would like to thank Sónia Alves for the advice and for the jolly conversations, especially in the early phases of my PhD, when we were both in Cambridge. I would like to thank my examiners, Rebecca Tunstall and Li Wan, for their thorough and engaged comments. The Viva was an insightful, challenging and above all constructive experience that has greatly improved the thesis and provided me with new perspectives on housing. My thanks go also to those who, at different stages, provided comments on my thesis work. In alphabetical order: Julia Braun, Christopher Buckle, Gemma Burgess, Olga Goldfayn-Frank, Francesco Mazzola, Christina Li, Thies Lindenthal, Tobias Schmidt, Elisabete Silva, Pawel Smietanka and participants at the European Network for Housing Research (2019) and European Real Estate Society (2019, 2021, 2022) Annual Conferences, the Health Studies User Conference (2022), the Cambridge ESRC DTP Graduate Student Conference (2019), the Cambridge Land Economy Early Career Researchers' Conference (2019, 2021), and a 2020 Savills internal seminar.

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whom I hold close to my heart.

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CHAPTER 1

The fundamental features of housing tenure and tenure transitions as events

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DOES being a renter have economic and social implications which differ from other types of housing tenure, such as outright homeownership or repaying a mortgage loan? While the question of what determines the choice of housing tenure has been investigated more conclusively, the effects of this choice are wide-ranging and leave more scope for exploration. It is of particular interest to study how outcomes change around tenure transitions, as these events provide longitudinal variation in housing tenure that can help identify its effects in a more robust manner as compared to cross-sectional analyses of differences across housing tenures. Nevertheless, longitudinal analysis are not in themselves a guarantee of correct identification of effects, because the choice of housing tenure is an endogenous one. In other words, individuals or households do not, in most cases, change their housing status because of sudden and unexpected occurrences, but rather as a result of a decision taken after, in the language of economics, some form of optimisation. Because of this, simply observing the trajectory of an outcome variable before and after a tenure transition cannot convincingly tell us whether the changes we observe can be attributed to the tenure transition. In fact, neither the absence of a change can be taken as evidence of the absence of an effect of the tenure transition. Statistics attempts to obviate to these problems of identification of an effect by either design, that is by producing or mimicking a random assignment of housing tenure, or by producing a fully specified model of how housing tenure causes outcomes. In this thesis I attempt to find a compromise between these two approaches, by

discussing theoretically how housing tenure could affect a set of outcome variables, and then using a design-based approach, event studies, to learn how tenure affects such outcomes. The event nature of the tenure transition implies that there exist a number of periods before the event (leads), a number of periods following the event (lags) and a discontinuity at the time of the event. In a potential outcomes framework, the effect of an event (or of a treatment, in the experimental jargon), can be estimated only after modelling a *counterfactual* outcome, that is the value of the dependent variable one would have observed had the event not occurred. If one can credibly build a counterfactual outcome, event studies provide a neat, visual way to observe how the distance between the counterfactual and the observed outcome evolves during leads and lags, to assess whether a discontinuity occurs starting from the time of event. Identifying an effect of the tenure transition in an event study framework also requires a precise set of assumptions (discussed in more detail in Section 1.5.3). Such assumptions involve, for instance, that once individuals enter a tenure, they remain in it throughout the sample. This excludes, or does not account properly for, *churners* (Ong ViforJ et al., 2021), who move in and out of homeownership.

Moreover, the focus of the thesis is on a sharp classification of tenure into renting (differentiating between private and social renting in some cases), mortgaged homeownership and outright homeownership. The decision to combine, in most cases, social renting and private renting stems primarily from the infrastructural underpinning of the thesis, relying on fundamental features of housing tenure and how these determine outcomes (See Section 1.3). I argue that this conceptual infrastructure can capture well important drivers of differences between renting, mortgaged homeownership and outright homeownership. It is nevertheless not well-equipped to address the drivers of the differences across social renting and private renting. This is because much of the difference in these fundamental features lies in the extent of one's ownership of property, and in this respect social and private renters are largely equivalent. Section 1.3 introduces as a fundamental feature of housing tenure the right entitling to the proceeds from the sale of the property. Such proceeds amount to one's home equity, ranging from 0 for renters (be they social or private) to

the full value of the property for outright homeowners, and values in between for mortgagors, depending on how far in the repayment they are. A second fundamental feature is represented by the right granting use and benefits from the use of the property, with respect to which the two rental categories are also largely equivalent. For a third fundamental feature, related to the ability to exclude others from the property, the two rental categories are similarly placed. Finally, adjustment or transaction costs related to changing property, though they might vary across social and private renters, still leave the two groups substantially closer to each other than to homeowners. What could be the implications on the estimation of separating the two rental categories? The primary consequence may be added nuance in the results. As it is hinted by Section 3.5 and by the existing literature, private renters appear to fare better in mental health terms than social renters, yet both tenures fare worse than mortgaged or outright homeownership. Whether a similar pattern may hold when it comes to tenure transitions, and whether it would also apply to the other two outcome variables studied in this thesis, namely portfolio[1] choice and voting behaviour, is an open question for further research. Furthermore, the two main events of interest are limited to the transition from renting to mortgaged homeownership, and to the transition from mortgaged homeownership to outright homeownership, narrowing the investigation to a linear path from renting to outright homeownership. One of the implications is that intermediate tenures (Monk and Whitehead, 2011), such as shared ownership or co-ownership, are also not accounted for. These are important limitations of my research that are further explored in Chapter 5.

Recent and less recent literature, including Rosen (1979), Henderson and Ioannides (1983), Kan (2000), Hubert (2007) and Lisi (2016), have looked at the mechanisms behind the renting versus owning decision. One of the drivers is the demographic profile of the individual: the probability of owning is found, for instance, to increase in income, age, number of children and previous experience of being an owner.

[1] In the financial context, *portfolio* is meant as a collection of investments. *Portfolio choice*, when it comes to households or individuals, takes the meaning of how households or individuals allocate their money across different investments.

Another important aspect is the tax treatment: tax advantages in the form of local property tax and mortgage interest deductions, or the exclusion of the imputed rent from taxable income, increase the expected share of homeowners. The contractual form of housing exchange also plays a role, in that incomplete contracts and transaction related incentives give rise to complex interactions with other variables determining tenure choice. Finally, the dynamics of the search and matching process can affect the housing tenure decision, most interestingly as homeowners are possibly repeat buyers and therefore active as both buyers and sellers. Once the tenure decision is made however, many, even seemingly disparate outcome variables are affected, with housing possibly being their connecting point. Coulson and Fisher (2009) look at the interaction between housing tenure and the labour market, comparing the predictions of five theoretical models with empirical evidence from the estimation of micro and macro regressions through instrumental variables; they find the outcomes to depend on firm behaviour, but there is evidence that homeowners enjoy more employment stability and lower wages, while higher homeownership rates can result in overall positive outcomes for society, such as increased job creation and production. Monk et al. (2010) review instead existing research spanning several topics: on the labour market outcomes, for instance, they recognise mechanisms complementary to those reported by Coulson and Fisher (2009), in that private renting, as compared to owner-occupation or also social renting, spurs labour mobility; they also describe an ample consensus around the role of poor housing conditions in affecting negatively physical and mental health. The latter relationship is explored more in depth in works such as Popham et al. (2015), which test the hypothesis that the channel through which housing affects mental health could materialise in the stress induced by social comparison; using survey data to observe the effects around the decision to buy under the UK's Right to Buy scheme, they nevertheless come to the conclusion that changes in tenure did not have an impact on psychological distress. Waldegrave and Urbanová (2016) compile a wide overview of the social and economic impacts of housing tenure, covering health, employment, crime, welfare,

wealth and education; the evidence generally goes in the direction of homeownership having a positive effect on all variables, with the exception of employment. More specifically, homeownership appears to be related to better physical and mental health, both clinically measured and self-assessed; it is also associated with lower welfare dependency and crime rates, higher economic mobility and better educational outcomes for children growing up in a homeowner household; finally, the effects on employment are mixed because, while several studies find a positive impact of homeownership on employment stability and faster exits from unemployment spells, some find a negative association between the two variables, possibly through the mobility mechanism found also in Coulson and Fisher (2009) and Monk et al. (2010). A further stream of research is that speaking of what could be called the "political economy of housing", linking housing tenure to the way people interact with institutions and among themselves, measured as political engagement or through alternative measures of social capital. As an example, DiPasquale and Glaeser (1999), look at how homeownership influences engagement in the local community in the United States and Germany. They argue that homeowners have stronger incentives to invest in local amenities and to benefit the community more in general, largely, once again, for the mechanism of lower mobility as compared to renters, highlighted already as a reason for differences in labour market outcomes for the two categories. Leviten-Reid and Matthew (2018) come to related conclusions using Canadian data: homeownership is positively associated to trust at a neighbourhood level as well as, to a lesser extent, to voting in municipal elections, while there does not seem to be an association when it comes to participation in local organisations.

Among these many domains, I decide to focus on three notable ones: portfolio choice, mental health and voting behaviour. I motivate this choice further in Section 1.4 of this introductory chapter, while providing a framework of how households and individuals might think about tenure transitions and what the consequences could be. The remaining sections of this introductory chapter focus on contextualising housing tenure in the United Kingdom, on defining what an event is, on what features of housing tenure transitions can determine jointly different outcomes such as portfolio

choice, mental health and voting behaviour, and on the statistical perspective on event studies. The following chapters will each deal with one of the three realms, first reconnecting with the first chapter and discussing how the theoretical framework can yield predictions relevant to the variable of interest, and then verifying empirically such predictions.

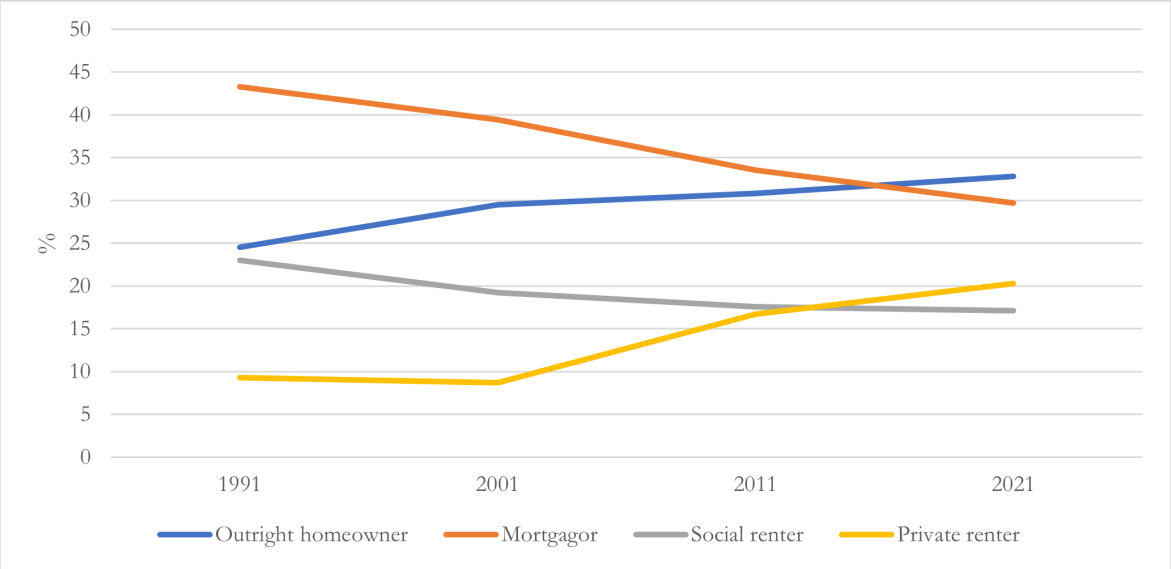
1.1 Contextualising housing tenure in the United Kingdom

Empirically, I focus on the case of the United Kingdom, in the period spanning 1991 to the pandemic period (up to March 2021). Recent results for the 2021 Census for England and Wales (ONS, 2023b) tell us that 15.5 million households (corresponding to 62.5% of the total) owner-occupy, while 9.3 million households (37.3% of the total) rent. The number of households owner-occupying were less in absolute value (15.0 million) at the time of the previous Census in 2011, but their share in the total was higher (64.3%). Conversely, the share of households renting was lower in 2011 (34.3%), as was the absolute number of households (8.0 million). In the 2001 Census (ONS, 2023a), the number of households owner-occupying were 14.9 million, or 68.9% of the total. Households renting were instead 6.0 million, corresponding to 27.9% of the total. Finally, in the 1991 Census (ONS, 2023a), owner-occupier households were 13.4 million or 67.8% of the total, while renter households were 6.4 million or 32.2% of the total.

Zooming in further on the type of ownership, in 2021 8.1 million households (32.8%) owned outright, an increase in both absolute and percentage terms from 2011 (7.2 million and 30.8% respectively). 7.4 million households (29.7%) owned with a mortgage (or shared ownership), a decline in both absolute and relative terms as compared to 2011 (7.8 million and 33.5% respectively). 5.0 million households were private renters (20.3%), an increase in both absolute and percentage figures as compared to 2011 (3.9 million and 16.7% respectively). Finally, 4.2 million households were social

renters (17.1%), while in 2011 the absolute figure was slightly lower (4.1 million), and the relative one higher (17.6%). In 2001, 6.4 million households (29.5%) owned outright, while 8.5 million (39.4%) were mortgagors (or in shared ownership). On the rental side, only 1.9 million households were private renters (8.7%), while 4.2 million (19.2%) were social renters. In 1991, 4.8 million (24.5%) were outright homeowners and 8.6 million (43.3%) mortgagors. At the same time, 1.8 million (9.3%) rented privately and 4.5 million (23.0%) were social renters. These patterns are summarised visually in Figure 1.1.

FIGURE 1.1. Evolution of tenure shares for England and Wales, 1991-2021

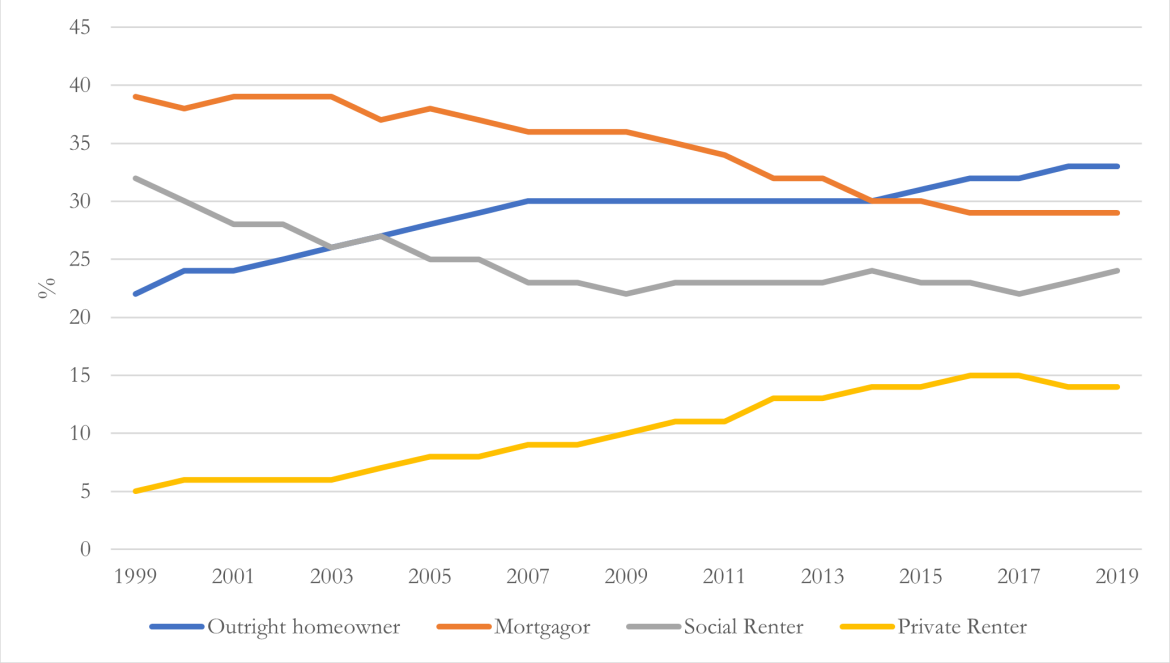


Note: ONS (2023a) and ONS (2023b).

The data for Scotland[2] (SHS, 2020) and Northern Ireland (NISRA, 2023) come from different sources. Their respective evolution in tenure shares over time are reported in Figures 1.2 and 1.3. The patterns for Scotland and Northern Ireland are broadly similar to those for England and Wales. The share of mortgagor households is highest in 1999 (39%) for Scotland and 2001 (39%) for Northern Ireland, but on a descending trajectory. In both cases, mortgagors are overtaken by the share of outright homeowners during the second decade of the 21st century, so that the last

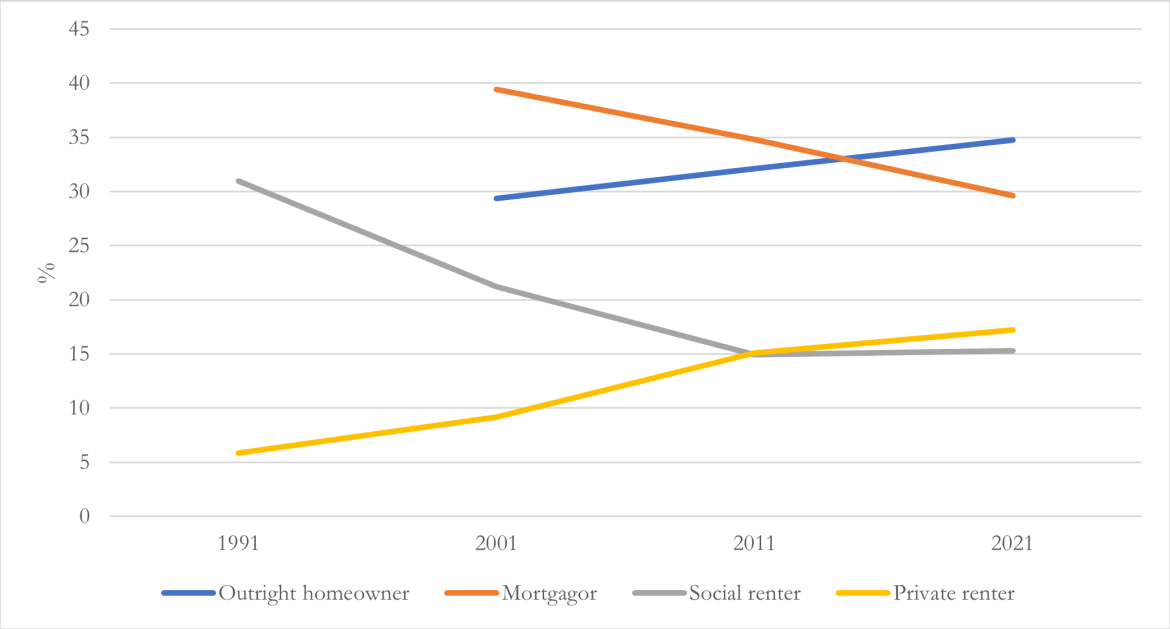
[2] I have not used Census data for Scotland as the 2021 Census was moved to 2022 because of the pandemic, and results are not yet available.

FIGURE 1.2. Evolution of tenure shares for Scotland, 1999-2019



Note: SHS (2020).

FIGURE 1.3. Evolution of tenure shares for Northern Ireland, 1991-2021



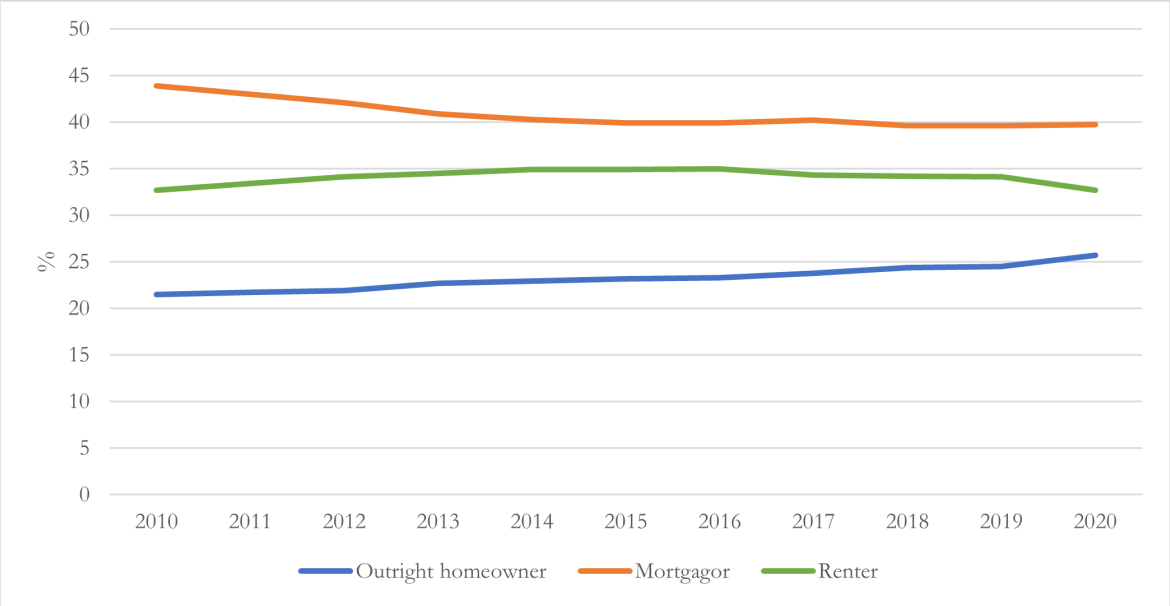
Note: NISRA (2023). Separate statistics for outright homeowners and mortgagors were not available for the 1991 Census.

recorded value for the share of mortgagors is 29% for Scotland (2019) and 30% for Northern Ireland (2021), and for the share of outright homeowners 33% for Scotland and 35% for Northern Ireland. While similarly to England and Wales, the share of social renters decreases over time while that of private renters increases, in the case of Scotland the share of social renters (32% in 1999 and 24% in 2019) remains always higher than that of private renters (5% in 1999 and 14% in 2019), while in the case of Northern Ireland the share of private renters (6% in 1991 and 17% in 2021) overtakes that of social renters (31% in 1991 and 15% in 2021) around 2011.

It is useful to compare these statistics to those for the United States and for Australia, which much of the empirical literature I will discuss throughout the thesis focuses on. For the United States (OECD, 2023), I report tenure data for the period 2010-2020, in which social renters are lumped together with private renters (Figure 1.4). We see how the share of outright homeowners in the US in 2010 and 2011 (about 21.5%) is substantially lower than that of England/Wales for 2011 (30.8%). The latest figure, for 2020 (25.7%) is also lower than the equivalent one for England/Wales in 2021 (32.8%), though in both cases there has been an increasing trend over the second decade of the 21st century. In the same period, the share of mortgagors has been declining in both the United States and England/Wales. In the former, mortgagors went from a share of around 43% in 2010/2011 to a share of 39.7% in 2020. In the latter the share was lower to start with (33.5% in 2011) and reached 29.7% in 2021. While the past decade has seen the share of private renters rising above that of social renters in England/Wales, their combined share went from 34.3% in 2011 to 37.4% in 2021. In the same period, the combined renter share in the US went from about 33% in 2010/2011 to 32.7% in 2020. In this case the US and England/Wales see diverging trend in that the former saw a stagnation in the proportion of renters, while the latter an increase.

Figure 1.5 shows the patterns in the share of housing tenures in Australia between 1999 and 2020 (ABS, 2022). The share of outright homeowners in these earlier years was about 38.5%, and steadily declined to 30.9% in 2011/2012 and to 29.5% in

FIGURE 1.4. Evolution of tenure shares for the United States, 2010-2020

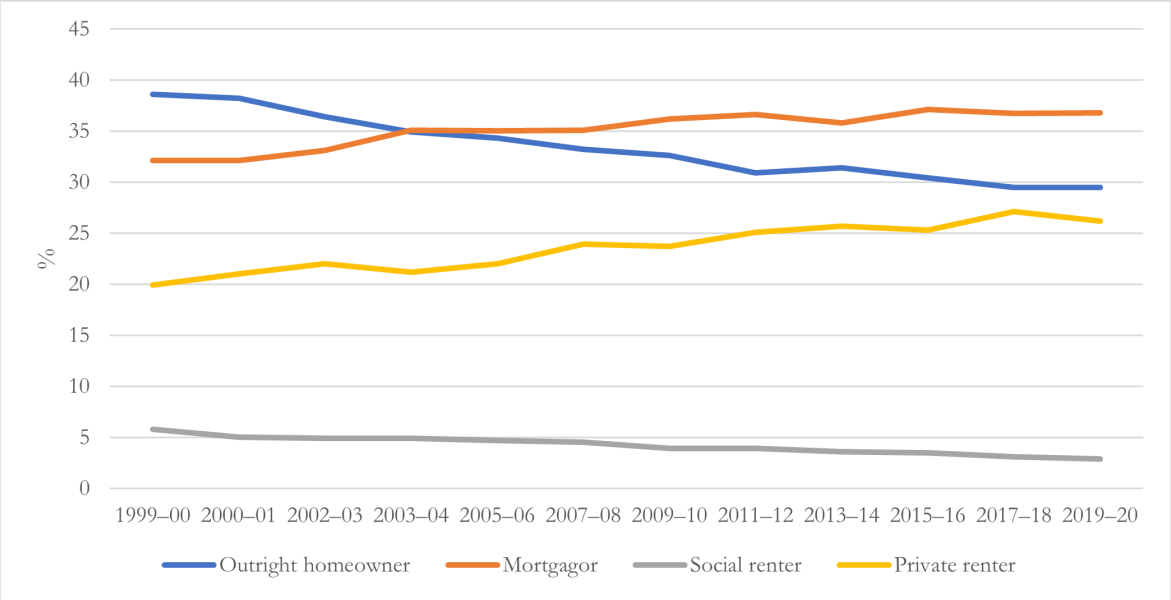


Note: OECD (2023). The data source lumps social and private renters together.

2019/2020. This follows an opposite trend as compared to both England/Wales and the US, which both witnessed an increase in the share of outright homeowners: from 29.5% in 2001 to 32.8% in 2021 for England/Wales, and from 21.5% in 2010 to 25.7% in 2020 for the US. In the case of mortgagors, their share in Australia went from 32.1% in 1999/2000 to 36.6% in 2011/2012, to reach 36.8% in 2019/2020. In this case too, the pattern is opposite as compared to England/Wales and also to the US: in the former the share of mortgagors decreased from 39.4% in 2001 to 29.7% in 2021, while in the latter from 43.9% in 2010 to 39.7% in 2020. The share of private renters steadily increased in Australia between 1999/2000 (19.9%) and 2019/2020 (26.2%), a pattern also found, albeit in a more pronounced fashion, in England/Wales, where the share went from 8.7% in 2001 to 20.3% in 2021. Finally, the share of social renters in Australia went from 5.8% in 1999/2000 to 2.9% in 2019/2020. While the decline occurred in England/Wales too, it started from a much higher share: 19.2% in 2001, reaching 17.1% in 2021.

For what concerns patterns of transition between tenures, most of the available information is focused on England and relies on the English Housing Survey (DLUHC,

FIGURE 1.5. Evolution of tenure shares for Australia, 1999-2020



Note: ABS (2022).

2022). Its most recent wave, referring to 2021-2022 reports total household moves of about 1,827,000, including moves within the same tenure, as well as new households forming and entering one of the tenures. Out of the total, 11% (192,000) of the moves were from the private rental sector to the owner occupier one, while those from the social rental sector to the owner-occupier one were negligible in number (the related survey observations were too few to be reported, due to confidentiality issues). Conversely, the moves from the owner-occupier sector to the private rental one were about 5% (91,000) of the total, and the moves from the owner-occupier sector to the social rental sector 0.4% (8,000) of the total. Most moves actually happened within a same housing tenure. 34% (620,000) within the private rental sector, 20% (361000) within the owner-occupier sector, 6% (115,000) within the social rental sector. On average, at the time of survey homeowners lived at the declared address for 17.6 years (24.5 years for outright homeowners and 9.4 years for mortgagors). For private renters, the figure is only 4.4 years, while for social renters 12.7 years. Looking more closely at recent first-time buyers (referring to purchases up to three years before the survey), suggests a number of about 852,000, with a mean age of 34 and 63% of

the total belonging to the top 40% of the income distribution. Only 5% of the total bought outright.

1.2 The definition of event and the mapping onto tenure transitions

Pearlin (1999) defines an event as such "by virtue of having an identifiable point in time at which it occurred". Can housing tenure transitions fit this definition? Surely, there exists a precise point in time at which a renter becomes a mortgagor and at which a mortgagor becomes an outright homeowner. This definition does not imply that the event should be exogenous, or in other words that it should come as a surprise to the individual. Indeed, this would rarely be the case for tenure transitions as these are endogenous decisions, taken by the individual for a host of reasons. Of course, were tenure transitions exogenous and therefore coming unexpected to the individual, it would be much easier to attribute an effect to them. In light of the complex sociological, economic and psychological reasons that lead someone to change their tenure, the investigation of the consequences of housing tenure transitions must face endogeneity and resort to alternative ways to identify an effect attributable to tenure transitions.

The thesis focuses on two tenure transitions in particular: that from renting to mortgage homeownership, and that from mortgaged homeownership to outright homeownership. The decision to limit the scope of this work to these two transitions spurs from the observation that homeownership is a highly prized achievement in a number of societies, including the United Kingdom (Marshall and Smith, 2016). As a consequence, many of those who hold this aspiration and do not have substantial financial means available to them as they start into adult life, will need to rent for a period before being able to afford a mortgage, and will pay back their mortgage over many years. This is of course not the only tenure trajectory, not even for those who would like it to be. Some may be able to become mortgagors without

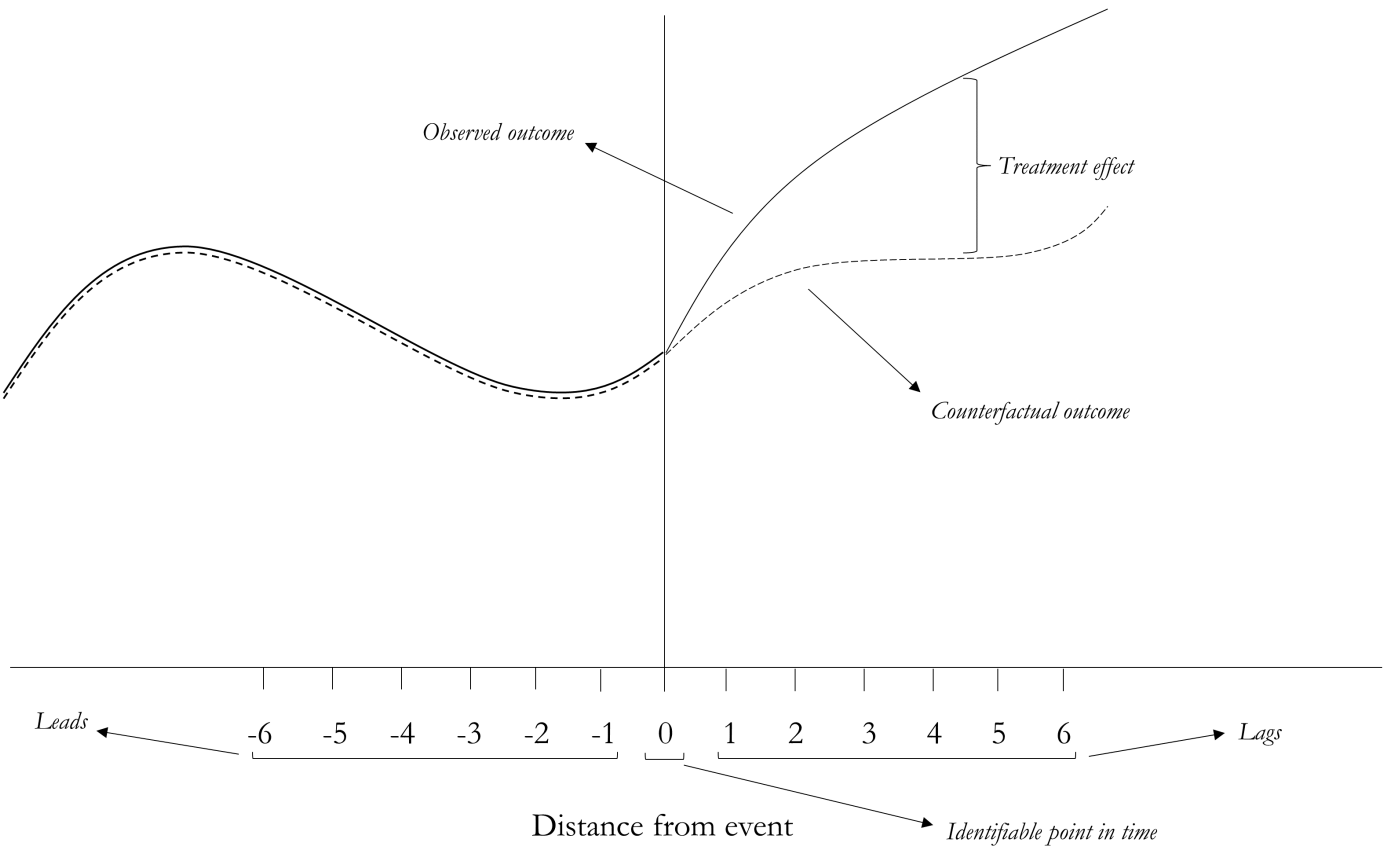
being renters first, while others may even afford outright homeownership as they form a new household. Many others, because of their own decision or because of external circumstances, may transition from homeownership back to renting. The transitions from renting to mortgaged homeownership and then in turn to outright homeownership are therefore two of a series of possible ones, yet they are of interest in themselves and concern a considerable part of the population. For instance, as described in Section 1.1, the most recent data for England suggest that out of all household moves (this includes tenure transitions as well as within-tenure movements), transitions from renting to owning (without distinguishing between mortgage and outright) represent about 11% of all moves, while transitions from owning to renting about half as much (around 5.4%).

The transition from renting to mortgaged homeownership has been studied extensively, though it is not as common to frame it as a transition to mortgaged homeownership, rather than simply to homeownership. In other words, mortgaged and outright homeownership tend to be lumped together. I argue nevertheless, as it will be also illustrated below when discussing the transition to outright homeownership, that there is merit in separating the two categories and transitions. Chiefly, the transition to mortgaged homeownership involves a down payment as well as the beginning of a large and long-lasting committed expenditure. This contrasts sharply with the end of this committed expenditure, namely the transition to outright homeownership. Additionally, many aspects of individuals' and households' lives are likely to change around the time of transition from renting to mortgaged homeownership, making it more difficult to determine whether changes in an outcome variable can be attributed to the transition itself: these could be changes in dwelling, broader changes in the place of residence or in employment, as well as the formation of a new household, for example.

The transition to outright homeownership has not been given as much attention in previous research, but I argue that it is important to focus on it because it represents a fundamental discontinuity in people's lives, one that would appear, based on

the results of this thesis, to have even larger consequences than the transition to mortgaged homeownership, at least for some outcome variables. In the case of this transition, the identification of an effect is made easier by the fact that, while the dwelling most likely changes when transitioning from renting to mortgaged ownership, the dwelling remains the same when transitioning from mortgaged to outright homeownership. This rules out confounding that has to do with housing quality, unless major renovations are carried out together with the tenure transition, as well as confounding related to a relocation of the household.

FIGURE 1.6. Conceptual illustration of an event



Note: Diagram showing the concept of event with connections to the statistical perspective.

The issue of identification of an effect is indeed central to the empirical contribution of this thesis. One way to identify an effect attributable to tenure transitions is that of building a counterfactual outcome, that is an estimate of what the observed outcome variable would have looked like had the event not occurred. Figure 1.6 illustrates how the concepts of event, leads, lags, observed outcome and counterfactual outcome relate to each other. The horizontal axis represents the distance, in terms of periods, from the event of interest (for instance, the transition to mortgaged homeownership). The "identifiable point in time" when the event occurs corresponds to 0. The negative periods preceding the event are termed *leads*, while the periods following the event are termed *lags*. The vertical axis measures instead the outcome of interest (for example, an indicator of mental health). Ideally, one is able to credibly model *counterfactual* outcomes (the dashed line) to match the *observed* outcomes in the lead periods. Section 1.5 will approach this more formally, introducing concepts such as "pre-trends", or "anticipation". The counterfactual outcome tracking the observed outcome up to the point of the event lends plausibility to the belief that such counterfactual trend is indeed able to represent how the outcome variable would have evolved in the lag periods in the absence of the treatment. If this is the case, the rift opening between the observed and counterfactual outcomes after the event in Figure 1.6 corresponds to the treatment effect of the event on the outcome variable of interest. The next section explores the features of housing tenure that could be responsible for the presence of treatment effects following tenure transitions.

1.3 How fundamental features of housing tenure can determine jointly many outcomes

Dietz and Haurin (2003) argue that there are two fundamental attributes of homeownership that can be considered responsible for driving outcomes differently across renters and homeowners. The first is the set of property rights that come with homeownership. At a conceptual level and without reference to any specific legal

system, they entail three classes: *possession*, or the ability to exclude others from the property, *use*, to determine use and benefit from the proceeds of such use, and *disposition*, the claim on the residual value of the property (Colwell and Trefzger, 1994). Importantly, *possession* and, under certain circumstances, *use* (for instance in the case of a main tenant who sublets), are rights enjoyed by renters too. The last class of rights instead, *disposition*, is exclusive to homeowners and determines a large set of incentives and other consequences related to the reselling value of the property. The right of *disposition*, once the property is owned, will induce homeowners to invest in the property not to let it depreciate (with possible implications for the remaining asset portfolio, see Chapter 2), to oppose policies that would lower the reselling price of the house (think about the Not In My Backyard phenomenon) and to vote for those parties more likely to support house prices (Chapter 4), but also relaxes the financial security concerns with positive consequences for mental health, because it constitutes an economic buffer (Chapter 3). At the same time, the degree of *disposition* implies a different composition of the exposure to rent risk, as compared to house price risk (Sinai and Souleles, 2005), possibly leading to different effects on portfolio choice as well as on stress levels and consequently on mental health. Even the *possession* and *use* rights, which are shared across tenures, differ in their intensity and therefore in their implications. The intensity is always maximum with outright homeownership. Indeed, for renters *possession* is always temporary, for mortgagors is *conditionally* (upon successful repayment of the mortgage) permanent, and only for outright homeowners is unconditionally permanent. Chapter 3 will introduce the concept of ontological security, that is a stability and continuity of environment in one's lived experience, which has immediate connections to the intensity of the right of *possession*. A similar reasoning is true for the right of *use* too. Agency over the lived environment can be considered an important component of ontological security, and the degree of agency over the physical changes to the property, reflected in the right of *use*, follows a similar degree of intensity in terms of tenure as for the right of *possession*, with outright homeowners enjoying the full extents of the right, followed by mortgagors and then by renters.

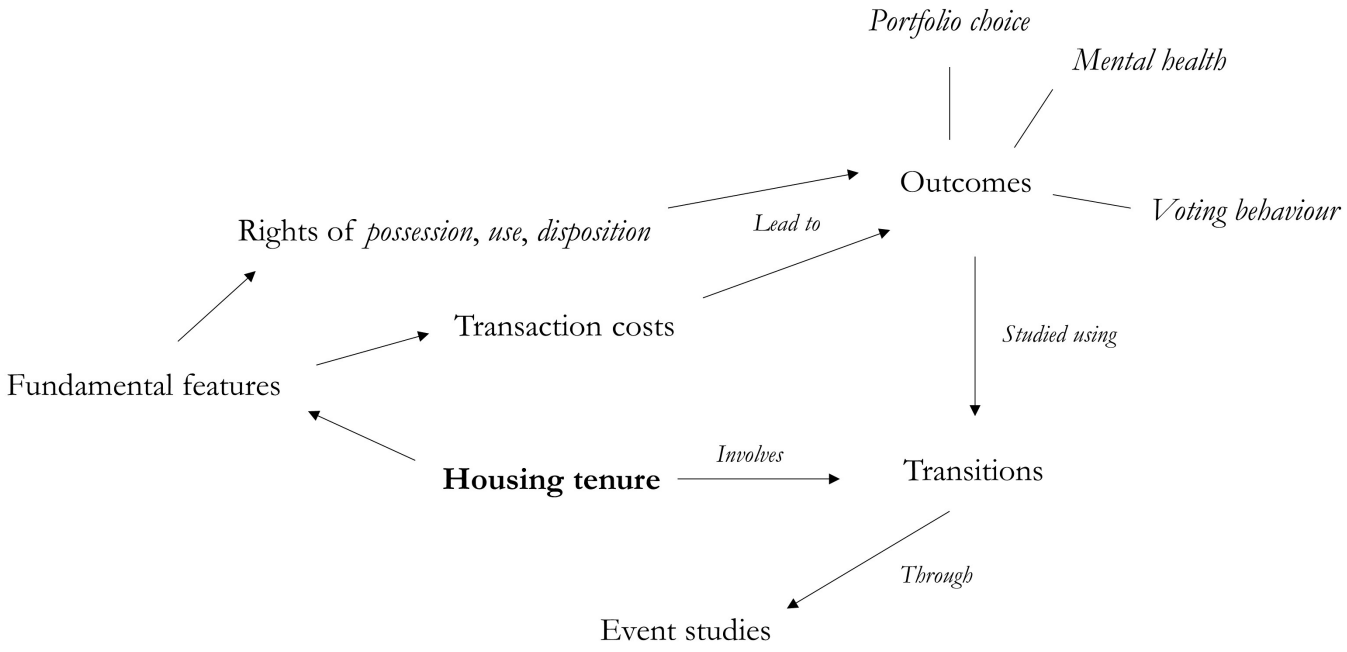
The second fundamental attribute of homeownership is represented by the transaction or adjustment costs associated with changing dwelling, which are pronouncedly higher for homeowners. Chiefly, this has implications for mobility and residential stability. But as it will be discussed in Chapter 2, transaction costs can be seen as the link between property holdings and risk aversion. When there exist substantial transaction costs, a change in property value should affect risk aversion and in turn holdings of risky assets. At the same time, the increased residential stability that comes with transaction costs could indirectly contribute to ontological security by "forcing" more continuity of environment (Chapter 3). Residential stability makes it also more likely for homeowners to engage civically and politically in local affairs, possibly with spillovers at the non-local level too (Chapter 4), with resemblances to the incentives tied to the reselling value of the property.

1.4 A conceptual framework of tenure choice and of its interconnected consequences

Figure 1.7 summarises the overall framework used in this thesis to study the economic and social consequences of housing tenure. As discussed in Section 1.3, the fundamental features of housing tenure, that is the different degrees of the right of *possession*, *use*, *disposition*, as well as the presence or absence of transaction costs, lead to a set of outcomes, or consequences.

The three outcomes that I focus on in this thesis are portfolio choice, mental health and voting behaviour. My choice is guided by their salience for households' and individuals' life, by the possibility of drawing theoretical and empirical parallels between them, and of course by the natural limit in scope that the thesis requires. For portfolio choice, the precise measure used is the share of stockholdings in liquid financial assets. Such a ratio (or close variants of it) is a popular measure used in the literature to measure the risk exposure in households' portfolios (see for instance Vestman (2019), Chetty et al. (2017), Cocco (2005), Yao and Zhang (2005)). Of course,

FIGURE 1.7. Thesis framework



Note: Diagram summarising the overall framework used in the thesis to study the consequences of housing tenure.

it is a simplification as stockholdings are not necessarily the only risky asset in the portfolio, and the risk level varies across stockholdings. Yet, their relative proportion within the financial portfolio is a useful proxy to gauge information on movements in the riskiness of households' portfolios. For mental health, the main measure used is the answer to the question "Have you been feeling recently unhappy or depressed?" and, as a second measure, the 12-items Generalised Health Questionnaire (GHQ, described in more details in Appendix B). I align with the literature in using the GHQ (Romppel et al. (2013), Hystad and Johnsen (2020)), while I depart from it in using the individual survey question on unhappiness and depression. The reason behind this is that using the direct survey question offers the advantage of tying more closely the measurement to the concept of mental health, while the GHQ, being a composite indicator of many survey questions, does not have a 1-to-1 mapping to any specific concept. Yet, the advantage of the GHQ is that it lends itself better to quantitative analysis, since it ranges from 0 to 36, as compared to a single survey question, which

having only four answers can at most take four values for quantitative purposes. More information about these measures, as well as the assumptions behind using them for quantitative analysis, are found in Section 3.1.1. For voting behaviour, the measures used are the "Yes/No" answer to the question on whether individuals participated or not in general elections, as well as whether or not they voted for the Conservative Party (derived from a more general question on what party an individual voted for if they participated in a general election). Both are widely used measures in the literature on voting behaviour (see for instance Hall and Yoder (2022), McCartney (2021), Ansell and Adler (2019a), Johnston et al. (2001)), and rely on less stringent assumptions as compared to those used to map the concepts of portfolio choice and mental health to the respective quantitative measures that I use. Because of the relative infrequency of general elections, I augment these two measures of voting behaviour by imputation, as described in detail in Section 4.2.

How are these three realms connected conceptually? The set of property rights and the adjustment costs discussed in Section 1.3 constitute the backbone of these connections. The right of *disposition*, which guarantees the proceeds from the sale of the property, connects all three measurements. By giving origin to a market for property and in turn to prices, it opens a channel to voting behaviour because of the ability to influence the price of one's own property through political action. Variations in one's property price also change the relative weight of property in one's portfolio, and this, in interaction with the presence or absence of adjustment costs, may influence households' risk aversion and their holdings of stocks relative to their liquid financial assets. The value of mortgages follows from prevailing house prices, with the consequence that the amount of total debt, as well as the time taken to pay it back, follow too, at least to an extent, from the right of disposition. The presence and characteristic of a mortgage may in turn affect financial security and therefore mental health.

The right of *use*, which entitles to use the property as well as to benefits from the proceeds of such use, connects two of the three realms. There exist clear channels

to mental health: one is of an intangible nature, as the right of use grants agency over the lived environment, while another is of a material nature as the right of use is connected to enjoying the physical characteristics of the dwelling. Increased agency over one's lived environment and better dwelling characteristics are both conducive to improved mental health. Because it governs use and benefits from use of the property, this right is also at the base of the landlord-tenant relationship. The stakes are high for both parties involved and this suggests an interest in influencing the political process to obtain more favourable conditions. This is exemplified by contentious issues such as caps on rents. On the tenant side, rent payments are also connected to mental health through their effect on financial security.

The right of *possession*, which entitles to exclude others from the property, is connected solely to mental health. Indeed, through the exclusive right to the property, it increases the stability and continuity in one's life, leading to improved mental health. Adjustment costs, finally, which increase the incentives to residential stability, connect all three realms. The presence of adjustment costs affects risk aversion and in turn portfolio choice. At the same time, through residential stability, adjustment costs alter the gains from political participation because of the potential effects on the amenities of one's local area. Once more through residential stability, adjustment costs play a role also in the stability and continuity in one's life discussed in terms of the right of possession, which in turn affects mental health.

In addition to these conceptual interconnections, there are practical aspects linking the three realms in this thesis. The first is that, in all cases, the main estimation model will be of the event study, or difference-in-differences type, described more in detail in section 1.5. The second aspect is that the main variable of interest will be housing tenure, together with concepts that are strictly related to it, such as the amount of housing (for instance, Chapter 2 uses also the share of housing in total assets). The third aspect concerns the data sets used. While Chapter 2 uses the Wealth and Assets Survey (WAS), Chapters 3 and 4 both use the British Household Panel Survey/Understanding Society (BHPS/US). The parallels between Chapters

3 and 4 are obvious in this respect, but it should be underscored how the WAS and the BHPS/US are also comparable: they are both large surveys, representative of the whole of the UK; they overlap in terms of time span (the WAS covers 2006 to 2018, while the BHPS/US goes from 1991 to the pandemic period); they contain commensurable variables concerning demographic and socio-economic characteristics; they both provide data at the household and individual level. The decision to use a different dataset for Chapter 2 stems from the additional information on portfolio structures that the WAS contains, as compared to the BHPS/US. Because portfolios tend to be determined at the household level and are therefore less individual than mental health or voting behaviour, for Chapter 2 the unit of analysis is the household, while for Chapters 3 and 4 is the individual.

To study such outcomes, I resort to the variation generated by events connected to housing tenure, namely tenure transitions. In practice, I do this by employing the branch of statistics concerned with event studies. To further appreciate how the fundamental features of housing tenure may lead to economic and social outcomes, it is useful to work through conceptual examples.

Let us consider a household making the decision to rent or buy a house. Many aspects play a role in this choice and depend more or less directly on the fundamental features of housing tenure. First of all, preferences. Households will have preferences about where to live (which in turn includes both the location and the type of neighbourhood and amenities desired, see Büchel et al. (2020), Diamond (2016)), or about the specific dwelling characteristics (say, the presence or not of an elevator (Andersson et al., 2019)). Yet these preferences can be thought of in even more primitive terms: consider for instance the ranking of preference between renting and owning. What underlies it? It could be preferences about saving (those that choose to become homeowners have a stronger saving motive (Vestman, 2019)), about security and stability (homeowners prefer to have a stable place they can call their own (Watson and Webb, 2009), or they associate renting with more precarity (Adu-Gyamfi et al., 2020)) or even status (homeownership as a signal of achievement or self-congruity (Sirgy et al.,

2005), or as an aspiration as in the case of the UK (Marshall and Smith, 2016)). The connection between these preferences and the tenure decision are mediated by the expected degree of the rights of *possession*, *use*, *disposition* and by the expected extent of transaction or adjustment costs that households associate with the different tenures. Once preferences are established, we shall consider the personal circumstances. A household whose preference is homeownership will be able to transit to it only if it has enough savings to put down a deposit, as well as a stable enough future stream of earnings that can guarantee the repayment of a mortgage (Dieleman and Everaers (1994); Lersch and Dewilde (2015)). These enabling conditions can be characterised through wealth, employment and income.

Consider further an illustrative example of how the events of housing tenure transition may shape outcomes for households and individuals and how portfolio choice, mental health and voting behaviour may be connected. Take a household that prefers stability over instability and higher status over lower status, as well as standard assumptions about positive but decreasing marginal utility from consumption of housing. Let us also assume that this household starts out the working life with little inherited savings but its members have a job that guarantees a stable income flow. In the first periods of life, when the household is young (two people in their mid 20s moving in together, for instance), it saves towards a deposit and meanwhile it rents. In this period, stability and status are a possibility not yet realised, but the household has flexibility over what to invest in and in fact tries to maximise its returns to finance the purchase of the house. As soon as the household has saved up enough for a deposit, say when the components reach their mid 30s, it becomes a homeowner. This represents a discontinuity, an identifiable point in time after which stability and status are substantially higher, but the combination of the illiquidity of housing and of the committed expenditure from the mortgage lead to a portfolio that has shifted towards a composition that is less risky. Moreover, having such a large part of their wealth in the house, the household members, by virtue of the right of *disposition*, may be more inclined to protect and in fact try to increase their wealth by participating in elections and supporting those who are believed to enact

policies that facilitate increases in house prices or reduce taxation on property. Once the household finishes repaying the mortgage, finally, all the rights associated with tenure reach their maximum degree, and so does security and status. Moreover, the end of the committed expenditure represents a sizeable income increase. This added financial and ontological security may mean the household can afford becoming less risk-averse. It also increases the probability of improving mental health. Because the right of *disposition* has only risen in intensity, the transition to outright homeownership should affect positively, though only marginally, also the motive to vote and to vote for those who favour property-owning.

While this conceptual example helps to imagine the linkages between the features of housing tenure and its consequences, and possibly formulate hypotheses about what could happen in practice, testing such claims involves the use of data. The next section illustrates the statistical considerations underpinning event studies, which will be used to analyse tenure transitions.

1.5 Event studies from a statistical perspective

The event study (or difference-in-differences) literature has witnessed a small revolution in the past few years. In particular, this stream of econometric research concerned difference-in-differences models with staggered adoption, that is when different units are "treated" at different points in time. The main object of investigation has been a problem arising in the classical two-way fixed effects (TWFE) estimator, that is an Ordinary Least Squares (OLS) regression where both individual and time fixed effects are included (see equation 1.1). The coefficient on a binary treatment from the TWFE estimator can be misleading if the assumption of homogeneous treatment effect is violated: this assumption implies that the effect of a treatment, for instance the transition to mortgaged homeownership, would be the same for all units and over time. This assumption is generally unlikely to hold, and presumably also in the case of the effect of tenure transitions. This strand of research, summarised in

de Chaisemartin and D'Haultfœuille (2022) and Roth et al. (2022), has also provided a number of alternative estimators that allow to estimate Average Treatment Effects on the Treated (ATTs, a more formal version of the *treatment effect* of Figure 1.6) even when the assumption of homogeneous treatment effects does not hold. I use two of these alternative estimators: the imputation estimator of Borusyak et al. (2022) (implemented by Borusyak (2021)) and the doubly-robust estimator of Callaway and Sant'Anna (2021) (in the implementation of Rios-Avila et al. (2021)), and I compare them to the results from a standard event study with dynamic treatment. A classic TWFE estimator with dynamic treatment effects takes the form:

$$O_{it} = \gamma_i + \lambda_t + \sum_{h=-q}^{-1} \delta_h \text{HO}_{ih} + \sum_{h=0}^{m} \tau_h \text{HO}_{ih} + X'_{it}\Gamma + \epsilon_{it} \tag{1.1}$$

where O_{it} is the outcome of interest, γ_i are individual fixed effects, λ_t are time fixed effects, δ are lead coefficients on the homeownership dummy (HO, in turn mortgaged homeownership and outright homeownership), τ are lag coefficients on the homeownership dummy, Γ are the coefficients on the possible additional controls X and ϵ is the error term. Goodman-Bacon (2021) shows how such an estimator is a weighted average of all the two-periods, two-units comparisons in the sample and does not correspond to an ATT, unless, among others, a homogeneous treatment effects assumption holds. Crucially, this is an often implausible assumption as the treatment effect may vary across units and over time. Recovering the ATT requires to use estimators that can correctly aggregate the two-periods, two-units comparisons. The first that I consider is that of Borusyak et al. (2022), henceforth BJS. It is based on explicitly imputing counterfactual outcomes modelled using only never-treated and not-yet-treated observations[3]:

$$O_{it}(0) = \gamma_i + \lambda_t + \epsilon_{it} \tag{1.2}$$

[3]Equation 1.2 refers to the case without covariates, but it can be expanded to accomodate them too

where $O_{it}(0)$ are counterfactual outcomes, that is the outcomes that would be observed in the absence of treatment. In this thesis case, counterfactual outcomes would be those outcomes that would occur for those who undergo a tenure transition, if the tenure transition had not occurred. The predictions $\widehat{O}_{it}(0)$ are then subtracted from observed outcomes:

$$\hat{\tau}_{it} = O_{it} - \widehat{O}_{it}(0) \tag{1.3}$$

Finally, individual $\hat{\tau}_{it}$ are aggregated into target estimands, in my case horizon-specific ATTs:

$$\widehat{ATT}_h = \sum_{i=1}^{N_h} w_{it}\hat{\tau}_{ih} \tag{1.4}$$

Where w_{it} are individual weights set at $\frac{1}{N_h}$ in this paper, N_h being the number of observations for each horizon h, a specific value of t expressed in terms of h periods from the beginning of homeownership. The second estimator is that of Callaway and Sant'Anna (2021), henceforth CS. While the main building block of BJS was the individual level treatment effect τ_{it}, for CS it is the group-time ATT, where the group refers to a cohort of individuals who start to be treated at the same time (call that specific time g and the set of all times of initial treatment $\mathcal{G}$). In this thesis' case, this means a group of people that transition to homeownership in the same survey wave. Under a set of assumptions specified later and in the absence of covariates, the group-time ATT can then be recovered as the $\beta_{g,t}$ coefficient from the following regression on the subset of observations from time t and from time $g-1$:

$$O_{i,g,t} = \alpha_{1,g,t} + \alpha_{2,g,t} \cdot G_g + \alpha_{3,g,t} \cdot 1\{T = t\} + \beta_{g,t} \cdot (G_g \cdot 1\{T = t\}) + \epsilon_{i,g,t} \tag{1.5}$$

Where G_g is a dummy variable equal to 1 if the unit belongs to the group that starts being treated at time g. $1\{T = t\}$ is instead an indicator function that takes value 1 if the time period is equal to t. The group-time ATTs can then be aggregated and for the scopes of this thesis it will be done at the event-study coefficients level, so to obtain estimates of the ATT in terms of periods from the transition to homeownership. If $h = t - g$, that is the time elapsed since treatment, each horizon-specific ATT will be obtained as:

$$ATT_h = \sum_{g \in \mathcal{G}} \mathbf{1}\{g + h \leq \tau\} P(G = g | G + h \leq \tau) ATT(g, g + h) \tag{1.6}$$

Where τ is the maximum time-period observed and $P(G = g | G + h \leq \tau)$ are weights based on group size.

As a general remark not strictly related to event studies, survey weights will not, with the exception of a robustness check in Appendix A, be used throughout the analysis. Although their role can be important in ensuring the representativeness of descriptive results drawn from surveys, the focus of this thesis is on estimating relationships between variables, and in such a case the usage of survey weights is unlikely to help above and beyond what is already contained in the empirical strategy. A more thorough discussion of these issues can be found in Solon et al. (2015).

1.5.1 Identifying assumptions

There is a set of common identifying assumptions to both BJS and CS:

- *Irreversibility of treatment*: once a unit is treated, it remains treated. In this setting, this means that once someone becomes a homeowner in the panel, I consider them homeowners in subsequent periods even if they revert to renting. For the average case, this is probably not as stringent an assumption both because people tend to stick to homeownership once they transition,

and because even in those cases in which people transition back to renting, it is not obvious that the effects of previous homeownership should disappear. In fact, the assumption that the effects of previous treatment status do not affect the current status for groups whose treatment is reversible, is necessary for estimation in the framework of Borusyak et al. (2022).

- *No or limited treatment anticipation*: units are expected to anticipate treatment by a fixed number of periods δ and δ can equal 0 (the no-anticipation case) but needs to be non-negative. Formally, CS define this assumption as:

$$\mathbb{E}[O_t(g)|G_g = 1] = \mathbb{E}[O_t(0)|G_g = 1] \; a.s. \quad \forall g \in \mathcal{G}, t \in \{1, ..., \mathcal{T}\} \;\; s.t. \;\; t < g - \delta \tag{1.7}$$

and BJS as:

$$O_{it} = O_{it}(0) \qquad \forall i, t \in \Omega_0 \tag{1.8}$$

Ω_0 being the set of observations for never-treated or not-yet-treated individuals. Anticipation effects can be handled by redefining event dates based on δ.

- *Parallel trends*: in the absence of treatment, the outcome evolution in the treated group follows the same trend as that of the non-treated group. This assumption takes two different flavours:

$$\mathbb{E}[O_{it}(0)] = \gamma_i + \lambda_t \qquad \forall i, t \in \Omega \tag{1.9}$$

imposed at the individual level for BJS, and

$$\begin{aligned} &\mathbb{E}[O_t(0) - O_{t-1}(0)|G_g = 1] = \mathbb{E}[O_t(0) - O_{t-1}(0)|HO_s = 0, G_g = 0] \; a.s. \\ &\forall g \in \mathcal{G}, (s,t) \in \{2, ..., \mathcal{T}\} \times \{2, ..., \mathcal{T}\} \;\; s.t. \;\; t \geq g - \delta \; and \; t + \delta \leq s < \bar{g} \end{aligned} \tag{1.10}$$

imposed at the group level for CS when using not-yet-treated units as comparison (using the never-treated group as comparison can be specified too), with $\bar{g}$ the maximum G in the data.

BJS in addition requires:

- *Model of causal effects*: an explicit parametric characterization of treatment effects. Its general form is $\tau = \Gamma\theta$, with θ an $(N_1 - M) \times 1$ vector of unknown parameters and Γ an $N_1 \times (N_1 - M)$ known matrix of full column rank. N_1 is the number of treated observations and M the number of restrictions. In this thesis, I adopt the "conservative default" of the null model, that leaves treatment effects unrestricted, namely $\tau_{it} \equiv \theta$, with $M = 0$ and $\Gamma = \mathbb{I}_{N_1}$.

While CS:

- *Random sampling*: having access to panel data, formally

$$\{O_{i,1}, ..., O_{i,\tau}, HO_{i,1}, ..., HO_{i,\tau}\}_{i=1}^{n} \quad is \quad i.i.d. \tag{1.11}$$

1.5.2 Inference

For the BJS and CS estimator, I use standard errors clustered at the level of the individual (except for Chapter 2, where they are clustered at the household level). The standard errors are nevertheless computed differently in the two cases. BJS uses an analytical variance estimate clustered at the individual level of the form:

$$\hat{\sigma}_w^2 = \sum_i \left(\sum_{t;D_{it}=0} v_{it}\hat{\varepsilon}_{it} + \sum_{t;D_{it}=1} v_{it}\tilde{\varepsilon}_{it} \right)^2 \tag{1.12}$$

where v_{it} are weights corresponding to the weights w_{it} of equation 1.4 for treated observations (call these w_1), and to a function of w_1 and of the unit and time fixed effects for the untreated observations. $\hat{\varepsilon}_{it}$ are the residuals from the model $O_{it} =$

$\gamma_i + \lambda_t + \varepsilon_{it}$, while $\tilde{\varepsilon}_{it} = \hat{\tau}_{it} - \hat{\bar{\tau}}_{it}$, with $\hat{\tau}_{it}$ recovered in equation 1.3 and $\hat{\bar{\tau}}_{it}$ an average of $\hat{\tau}_{it}$ at the cohort-period level.

CS produce instead simultaneous confidence intervals using a bootstrap procedure, also clustered at the level of the individual. To do so, they use draws at the level of the individual from a Bernoulli distribution with Mammen parameters, to perturb the influence function of the group-time ATTs. This procedure adjusts for multiple-testing arising from considering the whole path of confidence intervals together in an event-study type estimation. Finally, for the TWFE reference estimator I use Huber-White standard errors also clustered at the level of the individual. BJS discusses how their estimator is more efficient (both theoretically and in simulations) as compared to alternative estimators recently appeared in the difference-in-differences literature with staggered adoption, even in the case of heteroscedasticity or serial correlation. Although they do not compare directly their estimator to that of CS, they do compare it to that of Sun and Abraham (2021), which is equivalent to CS in the absence of covariates.

1.5.3 Testing assumptions

Two key identifying assumptions, namely parallel trends and no anticipation, can undergo formal testing to an extent. For parallel trends, one can test if parallel behaviour is observed up to treatment ("pre-trends"), while the no anticipation assumption can be directly tested by observing whether or not there exists any sign of a response to treatment before this starts. Referring back to Figure 1.6, testing pre-trends would amount to assessing whether the curves of the observed and counterfactual outcomes move together before the event occurs (and are in this sense "parallel"), while testing anticipation effects would involve verifying whether the rift between the observed and counterfactual outcomes opens before the event occurs. BJS observe that in a difference-in-differences set-up, testing of identifying assumptions is generally conflated with treatment effect estimation: using the δ_h coefficients from equation 1.1 to test pre-trends or no anticipation and, at the same

time, using the τ_h coefficients to estimate treatment effects. This is a problem because such testing of the identifying assumptions is implicitly assuming a model of causal effects and it is creating a correlation between the coefficients used for testing and those used for the estimation of treatment effects, via the use of a same reference period. To avoid this, BJS suggest using model:

$$O_{it} = \gamma_i + \lambda_t + W_{it}^{'}\Gamma + \widetilde{\epsilon}_{it} \tag{1.13}$$

only on never-treated or not-yet-treated observations, testing $\Gamma = 0$ for pre-trends and individual coefficients in Γ for anticipation effects, with $W_{it}^{'}$ indicators of pre-treatment periods. If one adds a further assumption of homoscedasticity, this approach also obviates the critique of Roth (2022), who documents distortions to estimation and inference when treatment effects are estimated conditional on pre-trends being not statistically different from 0.

CHAPTER 2

The consequences of housing tenure for portfolio choice

THE dual nature of housing, as both a consumption and an investment good, produces non-trivial effects on households' investment choices[1]. Most renters not only have relatively less property assets in their portfolios as compared to homeowners and property holders in general, but possibly a different composition of their liquid assets (meant as non-property assets in a portfolio), especially in terms of risk. This could be generated by preferences (individuals own property based on, say, saving preferences), covariates (such as income or age) or by characteristics specific to property, such as its relative illiquidity.[2] These questions are important for policy: if property holdings affect the risk profile of households portfolios, there will be aggregate implications. Several of the papers in the literature make, in fact, explicit reference to the connection between household behaviour with respect to property and aggregate effects such as those on asset prices and financial wealth inequality (see for instance Piazzesi et al. (2007) or Favilukis et al. (2017)). Generally, the problem of the household faced with both a consumption and an investment decision related to property (notably, but not solely, for owner-occupied property), is framed as the consumption motive constraining the investment motive (Henderson and Ioannides, 1983). The reason for this is imputed primarily to the illiquid and indivisible nature of property, which makes adjustment more difficult (Grossman and Laroque, 1990) and demand for property overdetermined (Flavin and Yamashita, 2002). While adjustment or transaction costs (one of the fundamental

[1] The work within this chapter is based on a collaboration with Prof. Franz Fuerst. To maintain narrative consistency with the rest of the thesis, the first person is used in this chapter too.

[2] For a recent overview of the literature on household portfolios and on household finance more in general, see Gomes et al. (2021).

features of housing tenure discussed in Section 1.3), are more closely associated with the consumption motive of housing, the other fundamental feature, property rights, are closely tied to both consumption and investment. The right of *possession*, because it pertains to the ability to exclude others from the property, characterises consumption. On the other hand, the investment nature of housing is really determined by the right of *disposition*, in that the claim over the residual value of property and a market for it generate prices and in turn portfolio choices. The right of *use* sits somewhere in between, because determining and benefiting from the use of housing has surely consumption implications, but it is also what governs the relationship between a renter and a landlord, befitting a strongly investment nature. The effects of the peculiar nature of property on household portfolios may, nevertheless, extend to financial holdings. A rational household that maximises its expected utility will consider its whole portfolio, maximising returns while accounting for the risk correlations among the different assets, including property and stocks. Several works are set within this framework and stress different implications. One is that renters' portfolios should not be affected by property because there is no consumption constraint for them (Brueckner, 1997). For homeowners instead, their holdings of risky financial assets will be affected by adjustment costs associated with moving (Grossman and Laroque (1990), Chetty and Szeidl (2007), Stokey (2009)) and possibly by their degree of indebtedness, that is by the (relative) size of the mortgage they took out (Flavin and Yamashita, 2002), because of committed expenditure risk (Fratantoni (2001), Flavin and Nakagawa (2008), Chetty et al. (2017))[3]. If the expected returns between property and the other assets are correlated, and because property cannot be adjusted in the short-term and therefore is effectively a state variable in the household problem, financial assets may be used to hedge against housing risk depending on the correlation structure between property and the financial assets (Pelizzon and Weber, 2008). There is therefore a consensus that at least owner-occupied property should have an effect on the financial portfolio and specifically on the share of stocks, although the direction of this effect depends on the channels through which it acts: if it is

[3]See also Cocco (2005), Hu (2005).

linear through adjustment costs or committed expenditure risk, the effect should be negative. If it is through correlation between the expected returns from housing and the expected returns from the other assets, it could go in either direction depending on the variance-covariance matrix. As an empirical question, there appears to be more evidence in favour of the former, as most estimates of the effect of property on stockholdings are negative (Yao and Zhang (2005), Chetty et al. (2017), Vestman (2019)).

Two important aspects are either absent or overlooked: the different relationship of owner-occupied and investment property with stockholdings, as well as the possible heterogeneity of these relationships across households. With respect to the first, most studies focus on owner-occupied property, while others make explicit that property should not concern renters' portfolio choice (Brueckner, 1997) and very few consider investment property explicitly (for either renters or homeowners), such as Yao and Zhang (2005). Concerning the second aspect, Yao and Zhang (2005) is an example in which non-linear terms are included in the estimation, allowing for a differential effect of property (relative to wealth) depending on its size. The analysis of these two aspects is the central focus and the main contribution of this chapter, zooming in on the theoretical implications and the empirical evidence on how investment property (in the broad acception of non owner-occupied, as well as for the specific subset of Buy-to-Let property) is related to stockholdings differently from owner-occupied property, and whether these relationships are heterogeneous in nature. While the main specification uses the event study setting to observe portfolio changes around tenure transitions, effectively looking at the *extensive margin* of the relationship between housing and stockholdings, secondary analysis looks at the *intensive margin*, that is at the marginal change in stockholdings when property shares in portfolios vary by a small amount. I start by laying down the hypotheses based primarily on the stochastic control model with fixed adjustment costs by Stokey (2009), which shows how the presence of transaction or adjustment costs for owner-occupied property can translate to state-dependent risk aversion (where the state is the relative size of property to wealth) and, in turn, to heterogeneous holdings of the risky asset. Since

this is a behaviour linked directly to the consumption nature of owner-occupied property, it should not hold for investment property instead. The predictions are therefore that the relationship between the risky asset and owner-occupied property changes based on the relative size of the owner-occupied property, as well as that there is no relationship between the risky asset and investment property.

While most of the empirical literature reports an average negative effect of property on stockholdings, when turning to the data I find different results at the extensive and the intensive margin. The transition to mortgaged homeownership does not alter the share of stocks in the financial portfolio. At the same time, after the transition to outright homeownership, the share of stocks increases linearly, reaching an increase of almost 4% after four waves from the transition. For both the broad definition of investment property and for Buy-to-Let property, there appears to be an increase in the stock share following the purchase, that even reaches peaks of 5% and 6%. The intensive margin may hide instead a pronouncedly heterogeneous distribution: when owner-occupied property is a relatively small part of the entire portfolio (for instance 10 or 20%), a 1% increase in the share of owner-occupied property in total assets is associated positively to changes in the share of stocks, but with large standard errors that make it indistinguishable from 0. As the share of property in the portfolio increases, nevertheless, this association decreases first towards zero and then ever more negative, so that for a household with a portfolio composed almost entirely by property, additional property is markedly associated with lesser stockholdings: when owner-occupied property is 90% of total assets, an increase in it of 1% corresponds to a decrease of 0.14% in the share of stocks. These results are therefore consistent with a model of portfolio choice where property affects stockholdings non-linearly through the risk-aversion of the value function, when adjustment costs for property exist. The adjustment costs should matter only for owner-occupied property and not for investment property. This is confirmed in the data only for a strict definition of investment property: it holds when one considers Buy-to-Let property, which is more clearly exempt from a consumption motive, but not as much for investment property defined more broadly as anything that is not owner-occupied. A 1% increase in

the share of Buy-to-Let property in total assets is never associated to a significant movement in the share of stocks: the coefficients are both smaller in magnitude and with larger standard errors than for the owner-occupied share. For the intensive margin and a strict definition of investment property, these results provide then justification for the literature to focus on owner-occupied property when assessing the effect of property on household portfolios. If investment property is defined instead more broadly, as anything that is not owner-occupied, the patterns in the data are still weakly consistent with a consumption motive on property, which might be justified by the fact that second homes are part of such definition. For instance, when such a definition of investment property is 90% of total assets, an increase in it of 1% corresponds to a decrease of 0.18% in the share of stocks, though standard errors are more than proportionally larger than in the case of the owner-occupied share. Finally, while there are studies on the topic that look at other countries beyond the US (such as Vestman (2019) for Sweden), there seems to be scant evidence on the United Kingdom specifically and this work adds to this aspect too. The remainder of the chapter proceeds as follows. Section 2.1 provides insights from theory. Section 2.2 describes the Wealth and Assets Survey and relevant patterns in the data. Section 2.3 sets out the empirical strategy to characterise the relationship between property and stockholdings in addition to the main discussion in Section 1.5. Section 2.4 presents and discusses the results, showing that investment property purchases have more sizeable consequences for the holdings of risky assets as compared to tenure transitions, while owner-occupied property matters the most for marginal changes in property shares, changes that have a heterogeneous relationship with stockholdings. Section 2.5 concludes.

2.1 Insights from theory and hypotheses

In this section I lay the groundwork to understand the channels through which housing may be connected to portfolio choice. After defining important concepts, I discuss a model that connects adjustment costs to risk aversion, thereby establishing a link between the consumption nature of housing and the holdings of risky assets; these considerations are then summarised in a set of hypotheses.

2.1.1 Definitions

I first provide two key definitions:

DEFINITION 2.1 (Illiquid Share). The *Illiquid Share* is the share of property in **total** assets.

DEFINITION 2.2 (Risky Share). The *Risky Share* is the share of stocks in **liquid** assets.

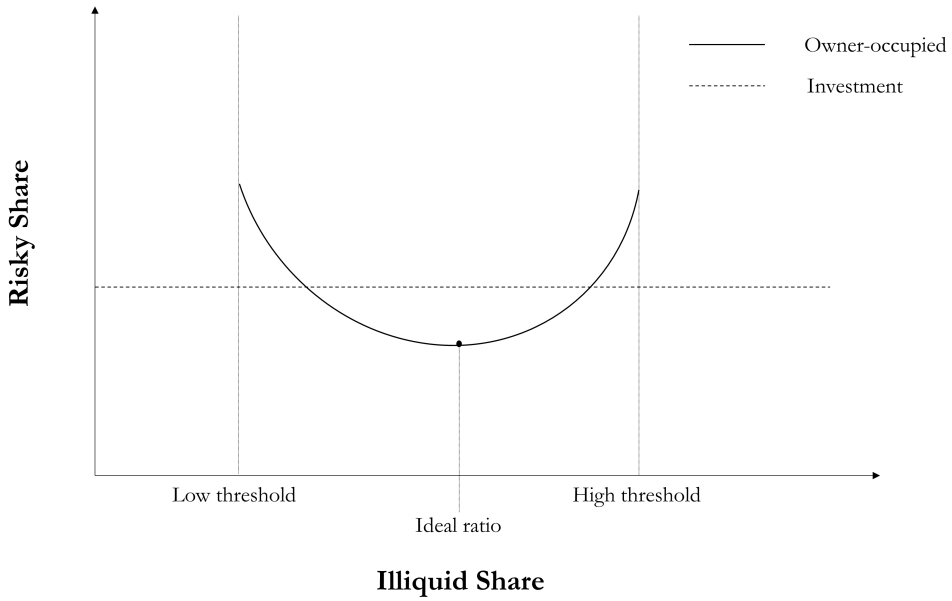
Where liquid assets are all non-property assets. The *Illiquid Share* can in turn be subdivided into its owner-occupied and investment components. The choice of these definitions is motivated by the attempt to separate the co-movements in the two quantities one would observe if, for instance, the total value of assets were to be used as the denominator for both property and stocks.

2.1.2 The Stokey model

The theoretical importance of this definition of *Illiquid Share* is underscored in Stokey (2009), a stochastic control model that I borrow and discuss qualitatively in this section to flag the essential theoretical features that justify the empirical approach. The key takeaway of this model, a more elaborate version of Grossman and Laroque (1990), is that, in the presence of adjustment costs when the amount of property is changed (so essentially for owner-occupied property), the relationship between

holdings of risky assets and property, relative to wealth, is not constant and depends instead on the size of property as compared to wealth (while it is constant or 0 for owner-occupiers if there are no adjustment costs, and 0 for renters). This stems from the fact that, with adjustment costs, risk aversion depends non-linearly on the relative size of property, while in their absence such relationship is either constant or zero.

FIGURE 2.1. The theoretical relationship between the *Illiquid Share* and the *Risky Share*, by type



Note: Diagram showing the main features of the theoretical model, after Figure 1b in Stokey (2009).

Imagine that a household has an ideal ratio of owner-occupied property to wealth, and wealth varies stochastically based on holdings of the risky asset. Since changing the main residence is costly in terms of time and money spent in the process of looking for an alternative place and moving, it occurs infrequently and only when convenient (or when triggered forcefully by sudden events). It is assumed that the household finds convenient to move only when their ratio of owner-occupied property to total wealth is far away enough from their ideal ratio, in either direction, so that there is a high and a low threshold that trigger a move. This implies that there exists an inaction region between these two thresholds, in which the ratio changes but

no move is triggered, as well as an action region, below the low threshold or above the high threshold, that triggers a move and a return to the ideal ratio. Changes in the ratio within the inaction region mean that the distance between the actual ratio and the thresholds changes and so does the risk aversion, which the holdings of stocks in turn depend on. For a range of numerical simulations, Stokey (2009) finds that the model implies a U-shaped portfolio policy when adjustment costs bind: the consumer is less risk-averse when the *Illiquid Share* is close to the thresholds and most risk-averse towards the centre of the inaction region (this was suggested by Grossman and Laroque (1990) too), while it is constant for the "frictionless consumer", which is framed in this study as the case of the investment *Illiquid Share*. A graphical illustration of the main features of this framework is reported in Figure 2.1. I decided to opt for this model to build my hypotheses because its central feature, the presence of adjustment costs, reflects directly the consumption motive as the crucial distinction between owner-occupied and investment property and is therefore the most credible driver of the difference in their effect on the financial portfolio. Moreover, the model allows to study the second dimension of interest too: the non-linear association between the *Risky Share* and the *Illiquid Share*, conditional on adjustment costs binding. Adjustment costs are present but less central to alternative existing models, which are not concerned with the difference between owner-occupied and investment property, and include for instance price risk effects of housing (Chetty et al., 2017) or life-cycle considerations (Vestman, 2019).

2.1.3 Hypotheses

Based on the framework, I formulate four hypotheses. The first two concern the extensive margin, or event study setting:

HYPOTHESIS 2.1. The transition to mortgaged homeownership is followed by a drop in the *Risky Share*, while that to outright homeownership is not consequential for the *Risky Share*.

HYPOTHESIS 2.2. The purchase of investment property is not followed by any movement in the *Risky Share*.

While the second two focus on the intensive margin. For marginal changes in owner-occupied property, we should expect a relationship between property and stockholdings to exist:

HYPOTHESIS 2.3. The relationship between the *Risky Share* and the owner-occupied *Illiquid Share* changes based on the size of the *Illiquid Share*.

At the same time, when the transaction costs are less or not relevant (that is, the underlying motive concerns investment only), there should not be a relationship. In fact, property that is not owner-occupied loses its consumption nature. Stokey (2009) interprets this case as that of a renter, while we extend it to capture all investment property, owned by either renters or homeowners. The assumption that adjustments costs would be negligible for property that is not owner-occupied does not imply that these costs do not exist for investment property. All types of property involve transactions costs including search costs, broker fees and forms of due diligence. Yet, I argue that such transaction costs are not as relevant to households' decisions concerning their stockholdings as compared to those involved in the case that the property is owner-occupied. In this scenario, adjustment costs do not apply and there are no action and inaction region. The second hypothesis is then:

HYPOTHESIS 2.4. There is no relationship between the *Risky Share* and the investment *Illiquid Share*.

Second homes, which we do not separate from investment property for the purpose of this paper, can be a partial exception as they retain a consumption motive, but not nearly to the extent of owner-occupied property and are thus not subject to the same adjustment costs.

2.2 Patterns and data description

The main data source for this work are the first six waves (2006-2018) of the Wealth and Assets Survey (WAS) provided by the Office for National Statistics (2020). The WAS is a repeated cross-section of British households and contains a panel component. It offers insight specifically on the distribution of assets within a household, as well as containing information on a number of demographic variables. Since wealth is not distributed evenly, to increase the efficiency of the sample, those addresses deemed more likely to correspond to wealthier households are sampled at a higher rate. The entire sample used for estimation contains about 66,000 observations, while the number of unique households amounts to about 18,500. Table 2.1 below provides summary statistics for the main variables of interest, looking separately at renters, mortgagors and outright homeowners; for the scope of the table all waves and households are pooled together (therefore a same household in two different waves is counted as two separate observations).

TABLE 2.1. Overview of summary statistics

	(1) Outright homeownership		(2) Mortgage		(3) Rent	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Overall *Illiquid Share* (%)	76.078	19.865	89.102	13.046	3.725	17.529
Illiquid Share for investment property (%)	5.118	13.494	5.865	14.978	3.725	17.529
Illiquid Share for Buy-to-Let property (%)	2.319	9.223	3.363	11.734	1.143	9.772
Risky Share (%)	6.036	15.731	6.373	17.487	1.786	10.320
Total property value (£100,000)	3.662	5.382	3.223	3.572	0.092	0.898
Investment property value (£100,000)	0.591	3.984	0.497	1.943	0.087	0.819
Buy-to-Let property value (£100,000)	0.256	1.492	0.318	1.555	0.036	0.456
Liquid assets (£100,000)	1.822	13.087	0.550	2.030	0.150	0.806
Mortgage debt (£100,000)	0.061	0.598	1.161	1.398	0.019	0.197
Home equity (£100,000)	3.600	5.275	2.062	2.843	0.073	0.827
Income (£100,000)	0.325	0.494	0.435	0.356	0.208	0.172
Number of children	0.112	0.456	0.881	1.044	0.630	1.051
Below age 35 (%)	0.456	6.734	12.527	33.103	19.173	39.367
In employment (%)	34.284	47.467	91.378	28.070	47.525	49.940
Education at degree level or above (%)	30.103	45.871	40.024	48.996	13.701	34.387
Observations	30293		21155		14364	

Note: Summary statistics for the six waves (2006-2018) of the Wealth and Assets Survey, pooling all cross-sections together. Nominal values have July 2015 as reference period.

The table highlights how ownership of property has a strong correlation with demographic and socio-economic variables. The *Illiquid Share* including owner-occupied property is, necessarily, much higher for homeowners (and so is the absolute property value), while this difference decreases substantially when looking at the investment property *Illiquid Share* (where the numerator includes all property excluding what is owner-occupied) and at the Buy-to-Let property *Illiquid Share*. The *Risky Share* too is markedly higher for homeowners, who also appear to have more investment property and Buy-to-Let property in absolute value. In all these respects, mortgagors and outright homeowners are indeed similar and can be considered together and contrasted with renters. On the other hand, sharper differences exist across the two categories of homeownership when it comes to another set of financial and demographic variables. In terms of liquid assets, the distance between outright homeowners and mortgagors is higher than between mortgagors and renters. For age and number of children, mortgagors are closer to renters than to outright homeowners. At the same time, mortgagors have (intuitively) by far the highest value of mortgage debt, but also of share in employment, as well as the highest share with education at degree level or above.

To fully appreciate the meaning of these patterns, it is worth describing in more detail how the key financial variables are calculated. The *Risky Share* is computed as the absolute value of stocks in £, divided by the sum of the absolute value of all liquid assets, that is stocks, bonds, accounts, insurance and a residual component comprising additional financial assets outside these categories, in £. In turn, stocks are the sum of UK, overseas and employees' stocks; bonds are the sum of UK, overseas bonds/gilts and fixed term investment bonds; accounts include current and savings accounts, Individual Savings Accounts and National Savings Products; insurance is the sum of insurance products which build cash value; the residual component includes a variety of formal and informal financial assets, for instance trust funds, endowments or amounts to be repaid loaned to others privately. The *Illiquid Share* is computed instead as property, that is as the sum of the value of the main residence, other houses and buildings (including Buy-to-Let houses), UK and overseas land

TABLE 2.2. Frequency of tenure transitions and salient variables

Number of transitions	Absolute frequency	Relative frequency (%)	Risky Share (%)	Share below age 35 (%)	Share with higher education (%)
0	11,974	64.693	4.835	11.739	28.964
1	5,574	30.115	5.202	8.497	29.103
2	853	4.609	5.433	8.048	36.004
3	97	0.524	4.931	14.708	39.708
4	11	0.059	4.125	16.667	46.364
Total	18,509	100			

Note: The table lists the absolute and relative frequency of the number of households in the sample by number of tenure transitions, as well as the mean of salient variables.

and any residual property, in £, divided by the sum of property and liquid assets, as defined above, in £. Throughout the chapter I use variations of this share and specifically: owner-occupied *Illiquid Share*, when the numerator includes only the main residence; investment property *Illiquid Share*, when the numerator includes all property except for the main residence; Buy-to-Let *Illiquid Share*, when the numerator includes only Buy-to-Let houses.

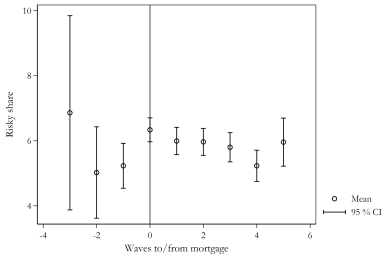
Shifting the focus to tenure transitions and looking at Table 2.2, one can appreciate how the majority of households in the sample did not in fact experience a tenure transition within the sample period (about 65%). A considerable amount experienced one transition (about 30%), while a non-negligible amount experienced two or more (about 5% cumulatively). The mean *Risky Share* is higher for those experiencing one tenure transition than for those experiencing none, and even higher for those experiencing two transitions, while it is again lower for those experiencing more than two transitions (though their frequency is small and hence the estimate less reliable). Those with no experience of tenure transition (or with more than two) appear also relatively younger than those experiencing one or two transitions. Finally, those experiencing two or more transitions appear to have relatively higher educational attainment as compared to those with experience of one or no transitions.

My first two hypotheses suggest a relationship between the transition to mortgaged homeownership and the *Risky Share*, but no relationship for the transition to outright

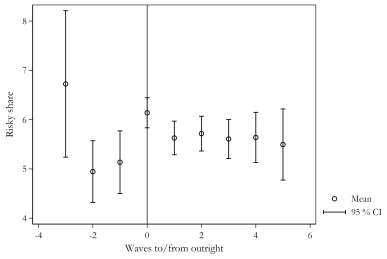
homeownership, nor for the purchase of investment property. From an event study perspective, we can also think about the evolution of the *Risky Share* as a function of the distance, in time, from the event of tenure transition or purchase of property. Panels *a* to *d* of Figure 2.2 show this for all property types, further subsetting owner-occupied property into two separate events: that of taking out a mortgage and that of concluding the mortgage, that is of becoming outright homeowners. The average *Risky Share* increases upon taking out a mortgage and then slowly decreases but largely remains above pre-mortgage averages. The pattern is similar but more pronounced for outright homeownership. In fact, this suggests that before taking out a mortgage, the average *Risky Share* is at about 5%, then following the beginning of the mortgage it increases to about 6% but, by the periods immediately before reaching outright homeownership, it has converged back to 5% and again jumps to around 6% following outright homeownership. The model from Stokey (2009) suggested that upon purchase of property (corresponding to when the mortgage is taken out), the owner-occupied *Illiquid Share* should match the ideal ratio, and in fact that when the *Illiquid Share* is in the neighbourhood of the ideal ratio, risk aversion should be relatively higher. In this sense, the fact that upon purchase the *Risky Share* increases goes against the predictions of the model. Panel *c* suggests an increase in the *Risky Share* following the purchase of investment property that is substantially higher than that for owner-occupied property. Based on the theoretical framework discussed in Section 2.1, one would have expected no relationship. For Buy-to-Let property (panel *d*) the figures are even higher but so is the starting point, suggesting, as expected, that this is a very selected group of individuals; a time trend is noticeable too. If only a time trend is responsible for this pattern (that is, if the *Risky Share* increases linearly independently from the event of purchase of Buy-to-Let property), the pattern could fit with the hypothesis of no relationship between the *Risky Share* and the *Illiquid Share* in the case of investment property. These descriptive patterns do not, nevertheless, take into account the counterfactual trend of the *Risky Share*, had these events not occurred.

FIGURE 2.2. Evolution of the *Risky Share* as a function of time distance from an event

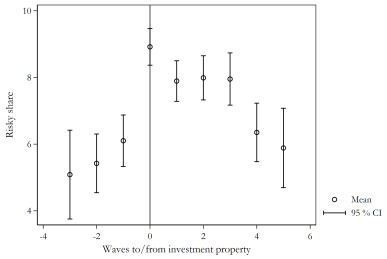
a. Taking out a mortgage



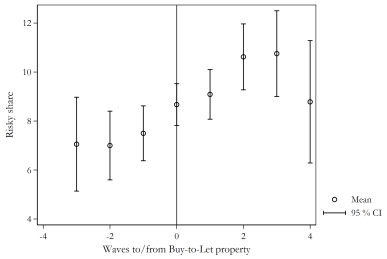
b. Becoming an outright homeowner



c. Purchase of investment property



d. Purchase of Buy-to-Let property



Note: Panels *a* to *d* in Figures 2.2 show the evolution of the *Risky Share* as a function of salient housing events: the beginning of a mortgage, outright homeownership, the purchase of investment property broadly defined and the purchase of Buy-to-Let property.

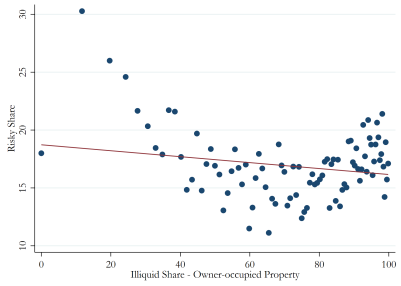
The second two hypotheses suggest a non-linear relationship between the *Risky Share* and the *Illiquid Share* for owner-occupied property and no relationship for investment property. Panels *a* to *c* of Figure 2.3 below show three binned scatterplots[4], where the two variables are plotted against each other for the subsample of those participating in the stock market, across types of *Illiquid Share*. Consistently with the Hypothesis 4.1, the relationship pictured in Panel *a* of Figure 2.3 shows a non-linear pattern, while those in Panels *b* and *c* show a rather positive, linear one, which would go against hypothesis 2.4.

The first aspect that is left out in this scatterplots is the selection due to participation in the stock market. Including non-participants in the stock market would skew the

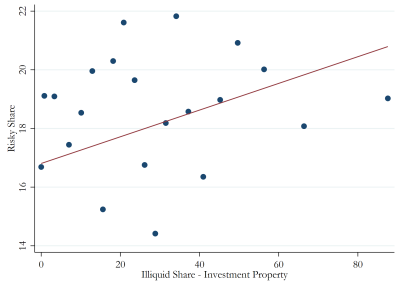
[4]Binned scatterplots divide each variable in a number of equally sized bins. The averages within each bin are then plotted against each other (Stepner, 2013).

FIGURE 2.3. *Risky Share* against *Illiquid Share* by property type

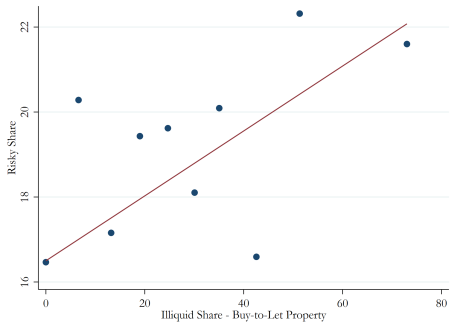
a. Owner-occupied property



b. Investment property



c. Buy-to-Let property



Note: Panels *a* to *c* of Figure 2.3 are binned scatterplots showing in turn the relationship between the *Risky Share* and the three types of *Illiquid Share*, for participants in the stock market. The binned scatterplots are built by dividing each variable in equally sized beans, computing the average within each bin and then plot those against each other. The smaller number of bins for panels *b* and *c* are due to the distribution of the *Illiquid Share* for investment and Buy-to-Let properties being concentrated at 0.

results by introducing many 0*s* for the *Risky Share*, but focusing only on stock market participants does not provide the full picture either, because it does not account for systematic differences between participants and non-participants. The second aspect is of course that we are not controlling for a host of factors that could drive both shares. The empirical design will aim at reducing these sources of bias.

2.3 Additional empirical considerations

While Section 1.5 discusses the empirical framework for event studies that I use as the main model throughout the thesis, effectively looking at the extensive margin of how housing affects stockholdings, in this chapter I study also the intensive margin of this relationship. The insights from the Stokey (2009) model suggest that we test for the presence and linearity of the relationship between the *Risky Share* and the *Illiquid Share* across owner-occupied and investment property. The empirical strategy consists of first accounting for selection into the stock market and then using variations of a fixed effects model for the different types of *Illiquid Share*.

2.3.1 Accounting for selection into the stock market

As a classical problem of selection (Heckman, 1979), we can observe stockholdings only for stock market participants. Estimating our model only on stock market participants would therefore lead to a bias, specifically:

$$\mathbb{E}(s_{i,w}|X_{i,w}, D_{i,w} \geq 0) = \beta_x' X_{i,w} + \mathbb{E}(v_{i,w} > -\beta_h' H_{i,w}) \tag{2.1}$$

where $s_{i,w}$ is the *Risky Share* for household i in wave w, $X_{i,w}$ is the vector of explanatory variables for stock market participants, $D_{i,w}$ is the latent variable such that $s_{i,w}$ is observed if $D_{i,w} \geq 0$ (0 is used as a threshold without loss of generality), $v_{i,w}$ is the error term in the selection equation and $H_{i,w}$ the vector of explanatory variables in the selection equation. The bias term exists as long as $Cov(\varepsilon_{i,w}, v_{i,w}) \neq 0 \Leftrightarrow \mathbb{E}(v_{i,w} > -\beta' H_{i,w}) \neq 0$ (with $\varepsilon_{i,w}$ being the error term in the selected model), that is, if stock market participants are different from non participants, which is the empirical consensus (Campbell (2006), Gomes et al. (2021)).

To account for selection, we first model this as a probit describing the probability of participating in the stock market as a function of educational attainment, age and

whether the household has positive or negative net worth (these characteristics are represented by $H_{i,w}$):

$$\mathbb{P}(S_{i,w} = 1|H_{i,w}) = \Phi(\beta_h' H_{i,w}) \tag{2.2}$$

where $S_{i,w}$ indicates participation in the stock market and $\Phi(\bullet)$ is the standard normal cumulative distribution function. Participation is defined for those households and periods in which the household held stocks, or a period in which they held no stocks but following a period in which they held stocks (so that 0, in such a case, does not represent non-selection but an actual value of 0 for stockholdings). Notice that this is a pooled regression, so a same household is treated as a different observation in each time period, relying on the assumption that selection into the stock-market can be predicted without relying on time-invariant characteristics; a fixed effects panel probit regression would also suffer from the incidental parameters problem and lead to biased estimation (Greene, 2004). Our selection variables are three that find strong support in the literature. Higher educational attainment, as well as correlated concepts such as financial literacy, have been associated to a higher likelihood of stock market participation (Black et al. (2018); van Rooij et al. (2011); Calvet et al. (2007)). Age is also associated to participation, though the direction of association is not established in a conclusive manner (Ameriks and Zeldes (2004); Fagereng et al. (2017)), while net worth is associated with higher participation and captures both wealth effects (wealth is intimately connected to stock market participation, supporting a financial cost barrier explanation, see Campbell (2006), Guiso and Sodini (2013)) and effects related to being in debt, such as committed expenditure risk (Fratantoni (2001); Cocco (2005); Yao and Zhang (2005)). As a next step, we recover the inverse Mills' ratio as:

$$\lambda_{i,w} = \frac{\phi(\beta_h' H_{i,w})}{\Phi(\beta_h' H_{i,w})} \tag{2.3}$$

with $\phi(\bullet)$ the standard normal density function. The inverse Mills' ratio will be used to correct the selection bias in the main estimation equation.

2.3.2 Testing the relationship between the *Risky Share* and the *Illiquid Share*

The baseline specification for the intensive margin aims at testing the linearity of the relationship between the *Risky Share* and the *Illiquid Share*:

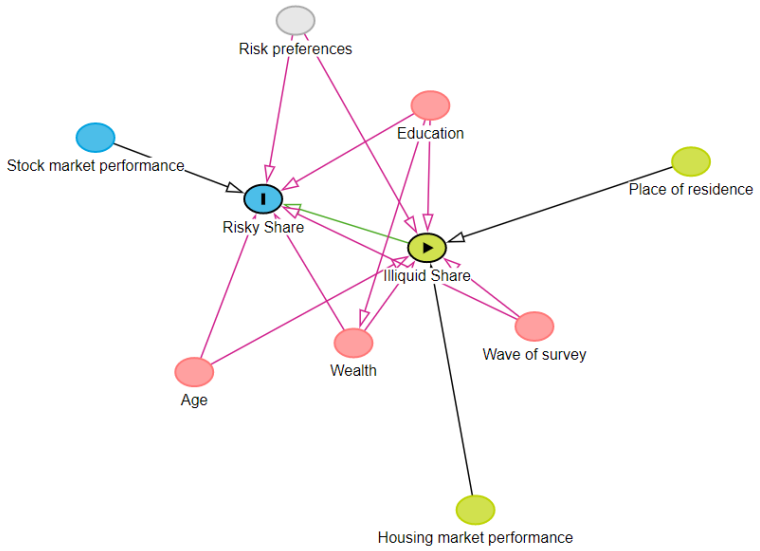
$$s_{i,w} = \beta_p p_{i,w} + \beta_{p^2} p_{i,w}^2 + \underline{\beta}_p P_{i,w} + \beta_{FTSE} FTSE_t + w_i + f_i + \lambda_{i,w} + \varepsilon_{i,w} \tag{2.4}$$

That is, each combination of household i in wave w constitutes an observation, and the *Risky Share* for the household in a specific wave ($s_{i,w}$) is a function of a second order polynomial for the *Illiquid Share* ($p_{i,w}$), the vector of household characteristics ($P_{i,w}$), the close value of the FTSE100 on the first day of the month of survey ($FTSE_t$), the wave fixed effects (w_i) and the household fixed effects (f_i). $\lambda_{i,w}$ is the inverse Mills' ratio computed in Section 2.3.1 and $\varepsilon_{i,w}$ is the error term. The vector of household characteristics $P_{i,w}$ includes households' net worth, as well as educational level and age band of the Household Representative Person (HRP). The educational level is a dummy indicating whether the HRP has a degree or not, while there are seven household age bands: 16-24, 25-34, 35-44, 45-54, 55-64, 65-74, 75+.

Including household fixed effects allows to control for all those time-invariant characteristics of households that we cannot observe directly, such as risk preferences when these are assumed constant. Non time-invariant observables that could affect both the *Risky Share* and the *Illiquid Share* are instead, as in the case of participation in Section 2.3.1, age, education and net worth (these three variables are therefore assumed to influence both the extensive and the intensive margin of stockholdings). In this case though, net worth is expressed in July 2015 £, rather than as a dummy indicating positive or negative net worth (as in the selection equation 2.2), to capture

more closely wealth effects on the intensive margin of stockholdings. The wave of the survey that the response was in is also included, to capture generic time-specific events. Stock market performance is included because it affects directly the *Risky Share*, and although it is assumed not to affect the *Illiquid Share*, it can improve the precision of the estimate. Variables measuring the performance of the housing market, or the place of residence of the household, were excluded because they are assumed to affect the *Illiquid Share* but not the *Risky Share*. This considerations can be summarised in the Directed Acyclical Graph (DAG)[5] in Figure 2.4.

FIGURE 2.4. Directed Acyclical Graph for the baseline model



Note: Directed Acyclical Graph describing the model in equation 2.4 (Textor et al., 2011). The green arrow identifies the relationship of interest. Purple arrows represent confounding effects, while black arrows refer to effects that do not affect our relationship of interest. Pink nodes are observed confounders, while grey nodes unobserved ones. Finally, blue nodes and yellow nodes are parent variables of the *Risky Share* and of the *Illiquid Share* respectively that do not affect the relationship between the two.

As outlined in Section 2.1, there are theoretical reasons to believe that if property does affect portfolio choice, it should do so differently whether the household lives or not in it. This is because not living in it eliminates (or at least relaxes, in case it is a holiday home), the consumption motive. I explore whether such difference

[5]For an introduction to DAGs see Cunningham (2021), while see Pearl (2009) for an extensive discussion.

exists by decoupling the share of owner-occupied and investment property, as well as by additionally focusing on the subset of the non owner-occupied property that is more strictly exempt from the consumption motive, namely Buy-to-Let property. In equation 2.4, the terms $\beta_p p_{i,w}$ and $\beta_{p^2} p_{i,w}^2$ represent then in turn the *Owner-occupied Share*, the *Investment Property Share* and the *Buy-to-Let Share*.

In all cases, the presence of selection would lead to inconsistent standard errors (Heckman, 1979), so we resort to bootstrapping, clustered at the household level (200 repetitions). To exploit as much information as possible, the panel used is unbalanced (18,509 households with on average 3.6 observations per household in the full sample and 7,536 households, with on average 2.8 observations per household, in the sample of stock market participants). To ensure that the unbalance does not affect estimation, I test for the randomness of household attrition (Verbeek and Nijman, 1992), as described in Wooldridge (2010): by adding a lagged or leading selection indicator to model 2.4 estimated on the full sample, therefore without the inverse Mills' ratio.

I decided not to include survey weights when estimating our main models, although they are used in robustness checks[6]. This is motivated by the fact that the weighting aimed primarily at accounting for oversampling of wealthy households, but such a bias is taken care of already by controlling for net worth (Solon et al., 2015).

2.3.3 Reformulating the model in terms of homeowners and renters

Another way to formulate the model is to interact the overall *Illiquid Share* with an indicator variable that distinguishes three categories: homeowners without investment property, homeowners with investment property and renters with investment property (of course, renters without investment property need to be excluded). This can be formulated as:

[6]Two different robustness checks are run, to ensure that weighting does not affect the main results. Since a stable weight is needed throughout the waves for each household, the first check uses the weight assigned to the household in its first wave, while the second check uses the weight assigned to the household in its last wave. Result tables and charts are reported in Appendix A.

$$s_{i,w} = \beta_p p_{i,w} + \beta_{p^2} p_{i,w}^2 + (\beta_{pI} p_{i,w} + \beta_{p^2 I} p_{i,w}^2) I_c + \underline{\beta}_P P_{i,w} + \beta_{FTSE} FTSE_t + w_i + f_i + \lambda_{i,w} + \varepsilon_{i,w} \tag{2.5}$$

where $p_{i,w}$ in this case represents the overall *Illiquid Share* and I_c is the indicator variable for the category of *Illiquid Share*. Framing the model in this fashion allows to adopt a different angle: rather than looking at how much the *Risky Share* increases or decreases for a 1% increase in the share for owner-occupied or investment property across their distribution (as in Section 2.3.2), one looks at the variation in *Risky Share* for a 1% increase in the *Illiquid Share*, across its distribution, for those who rent but still have property, for those who owner-occupy without owning additional property and for those who owner-occupy and own investment property. In other words, the perspective shifts from the type of investment to the type of investor. Homeowners are further distinguished between outright and mortgagors, to check if the presence of debt has any implications. The same econometric considerations made in Section 2.3.2 apply in this case too.

2.4 Results and discussion

This Section addresses first the results from the event study model presented in Section 1.5, to then move on to that of Section 2.3.2, which looks at the different types of *Illiquid Share*, and finally to an alternative specification that looks at the type of investor instead and matches the model in Section 2.3.3. Results for Section 2.3.1, which accounts for selection into the stock market, are not reported and I include only the coefficient for $\lambda_{i,w}$, the inverse Mills' ratio in the tables. Robustness checks for the intensive margin, using different weighting specifications, are reported in Appendix A and referred to in the text.

2.4.1 The event study perspective

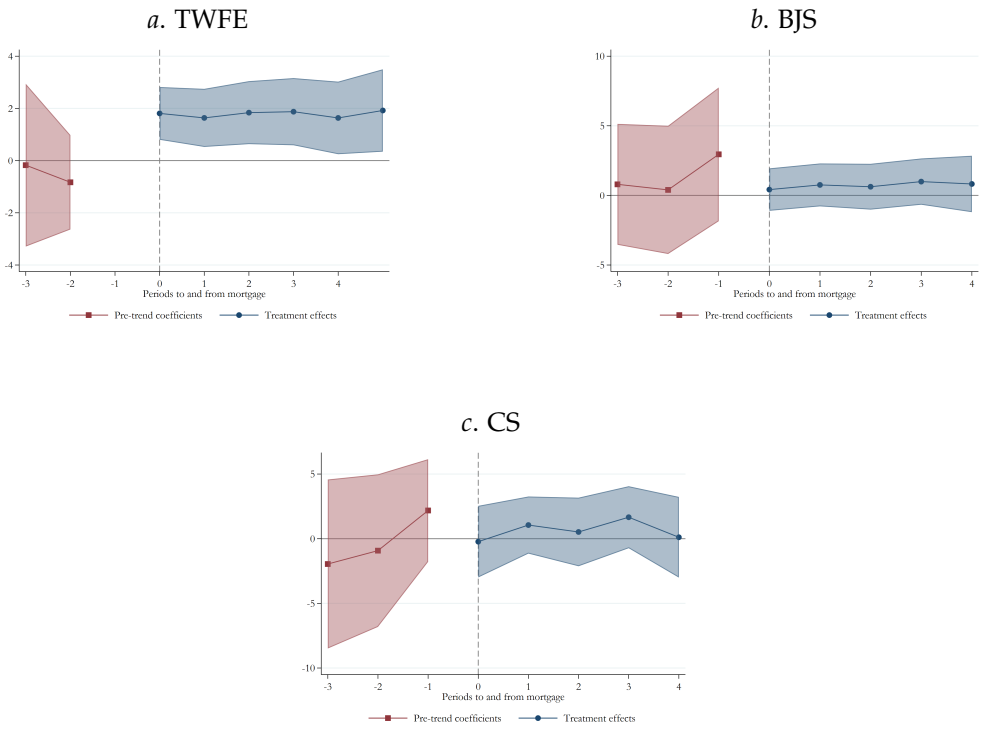
If we see either the tenure transition or the purchase of property as an event, we can estimate a counterfactual outcome for the *Risky Share* both before and after the event, and see if the relative trends between observed and counterfactual differ following the event. The discussion is divided in owner-occupied property and investment property.

Owner-occupied property

The event studies for owner-occupied property convey a different picture across tenure transitions. The transition to mortgaged homeownership (Figure 2.5 and Table 2.3) suggests that in fact no adjustment is made to the *Risky Share* around the tenure transition. This is in contrast with the descriptive evidence in Panel *a* of Figure 2.2 showing an increase in the share following the transition, revealing that simply looking at the evolution of the *Risky Share* as a function of the event of tenure transition, without a suitable counterfactual, can give a misleading picture. Moreover, one can appreciate the difference between the TWFE estimator, which is known to be biased when the assumption of homogeneous treatment effects does not hold, and the BJS and CS estimators. The BJS/CS estimators do not make use of the

so-called "forbidden comparisons", that is those using as a comparison group one that is already treated. In this case, it would mean using households that are already mortgagors. This is not a valid comparison, but one that is mechanically made by the standard OLS estimator. The fact that the initial increase appears in the TWFE estimation in Figure 2.5 and in the descriptives of Figure 2.2, but not in the BJS/CS estimation, suggests that this is driven by forbidden comparisons in the case of the TWFE estimation, and by selection bias in the case of the descriptives of Figure 2.2 (since there is no comparison group).

FIGURE 2.5. Event studies of portfolio choice for the transition to mortgaged homeownership



Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*). The units on the y-axis correspond to percentage point changes in the *Risky Share*.

These results are less in contrast than the descriptive statistics with the theoretical framework, whose predictions led to the expectation that the *Risky Share* should

TABLE 2.3. Event study of portfolio choice for the transition to mortgaged homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 3	-0.174	0.797	-1.956
	(1.591)	(2.215)	(3.333)
Pre-trend 2	-0.830	0.398	-0.922
	(0.922)	(2.349)	(3.009)
Pre-trend 1		2.951	2.181
		(2.450)	(2.024)
ATT 0	1.808***	0.418	-0.229
	(0.515)	(0.777)	(1.412)
ATT 1	1.636***	0.755	1.056
	(0.567)	(0.786)	(1.126)
ATT 2	1.838***	0.624	0.521
	(0.615)	(0.838)	(1.352)
ATT 3	1.874***	0.992	1.661
	(0.656)	(0.847)	(1.222)
ATT 4	1.635**	0.823	0.112
	(0.709)	(1.033)	(1.592)
N	45957	22533	19484

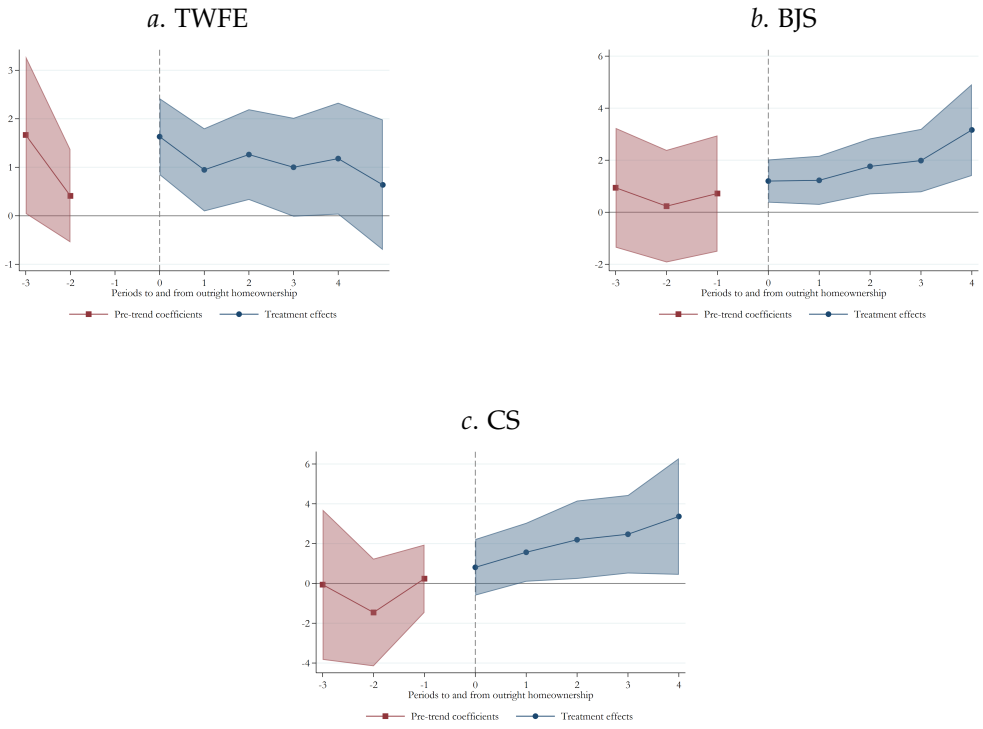
Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

have decreased following the transition, because once the ideal ratio is reached by the household, risk aversion should be at its maximum. On the other hand, the transition to outright homeownership (Figure 2.6 and Table 2.4) involves a steady increase in the *Risky Share*, more in accord with Panel *b* of Figure 2.2, which reported a level increase following outright homeownership. The Stokey (2009) model does not offer much of an insight into what should happen with the transition to outright homeownership. As this transition does not involve in itself a change in the *Illiquid Share*, one could expect that no effect should be there. Yet, it could be driven by some other characteristic of the tenure transition. The most likely reason connected to the transition to outright homeownership that could imply an increase in the *Risky Share* is the increased availability of income because of the termination of

mortgage payments. This increased security could imply a decrease in risk aversion and therefore an increase in the *Risky Share*.

FIGURE 2.6. Event studies of portfolio choice for the transition to outright homeownership



Note: Event studies in terms of becoming an outright homeowner, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*). The units on the y-axis correspond to percentage point changes in the *Risky Share*.

Investment and Buy-to-Let property

The purchase of investment property, broadly defined to include, for instance, holiday homes, is accompanied by a steady increase in the *Risky Share* (Figure 2.7 and Table 2.5), while in the case of the more narrow definition of Buy-to-Let property (Figure 2.8 and Table 2.6), there is a similarly steady increase but with a steeper slope, as well as a decline in the last period. Both have similarities to the corresponding descriptive patterns in Panels *c* and *d* of Figure 2.2, in that they show an increase of the *Risky Share*

TABLE 2.4. Event study of portfolio choice for the transition to outright homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 3	1.666**	0.942	-0.0612
	(0.829)	(1.174)	(1.926)
Pre-trend 2	0.410	0.233	-1.459
	(0.490)	(1.102)	(1.379)
Pre-trend 1		0.721	0.240
		(1.139)	(0.872)
ATT 0	1.631***	1.198***	0.809
	(0.404)	(0.422)	(0.723)
ATT 1	0.947**	1.227**	1.565**
	(0.437)	(0.480)	(0.754)
ATT 2	1.262***	1.762***	2.194**
	(0.477)	(0.548)	(1.003)
ATT 3	1.000*	1.985***	2.471**
	(0.520)	(0.620)	(1.005)
ATT 4	1.179**	3.163***	3.365**
	(0.588)	(0.900)	(1.495)
N	66737	41494	33430

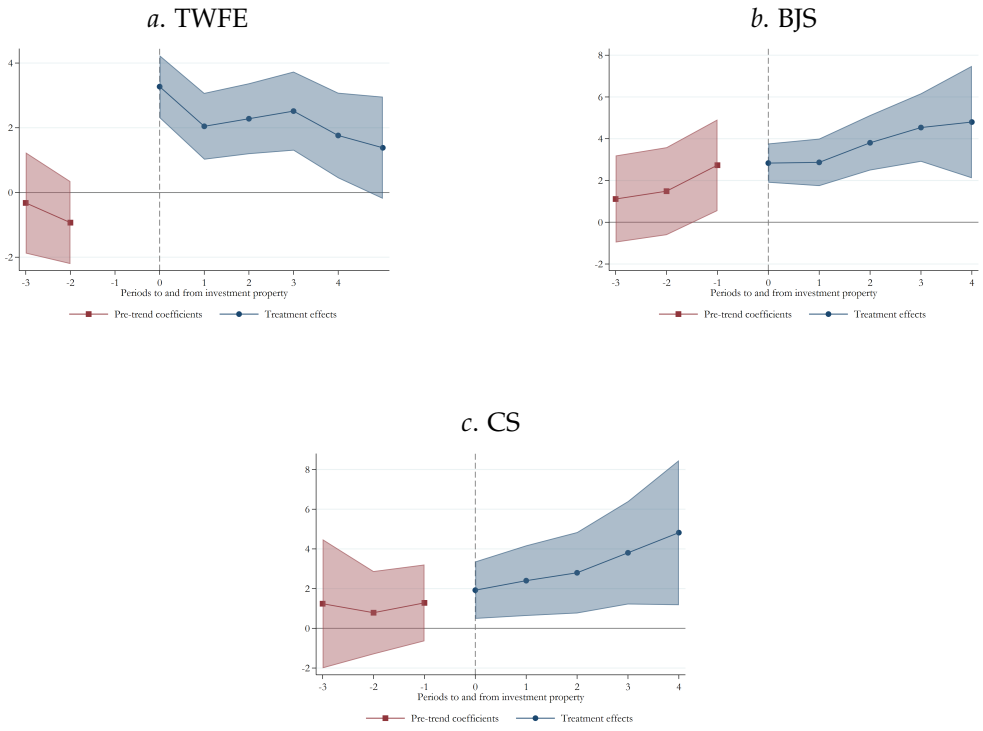
Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of becoming an outright homeowner, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

post event. While the Stokey (2009) model would have predicted no relationship between the *Illiquid Share* for investment property and the *Risky Share*, these results suggest that the two variables are connected at the extensive margin. There seem to exist then a change in risk attitudes towards decreased risk aversion following the purchase of investment property that is disconnected with the channel of adjustment costs. The linear trend observed in Panel *d* of Figure 2.2, which could suggest age was responsible for the steady increase in the *Risky Share*, is absent for the event study of the purchase of Buy-to-Let property. What could be causing the decline in the last period for the *Risky Share* in the case of Buy-to-Let property? The last period is expressed relative to when the purchase of the Buy-to-Let property occurred. Therefore, it does not occur for all households at a same time. Yet, since the data cover the period 2006 to 2018, the last period for most households will fall in the latter

part of the sample time range. In the 2015 Summer Budget, a restriction on tax relief on financing costs for landlords was announced[7]. The measure would be phased in gradually starting from April 2017. The announcement and initial implementation of the restriction falls indeed within the last period of the sample, and since owners of Buy-to-Let properties might have started adjusting their portfolio behaviour even in advance of the implementation, such a measure might indeed be responsible for the decline observed in the last period on Figure 2.8.

FIGURE 2.7. Event studies of portfolio choice for the purchase of investment property



Note: Event studies in terms of purchasing investment property, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*). The units on the y-axis correspond to percentage point changes in the *Risky Share*.

[7] See https://www.gov.uk/government/publications/restricting-finance-cost-relief-for-individual-landlords/restricting-finance-cost-relief-for-individual-landlords, accessed 29/03/2023.

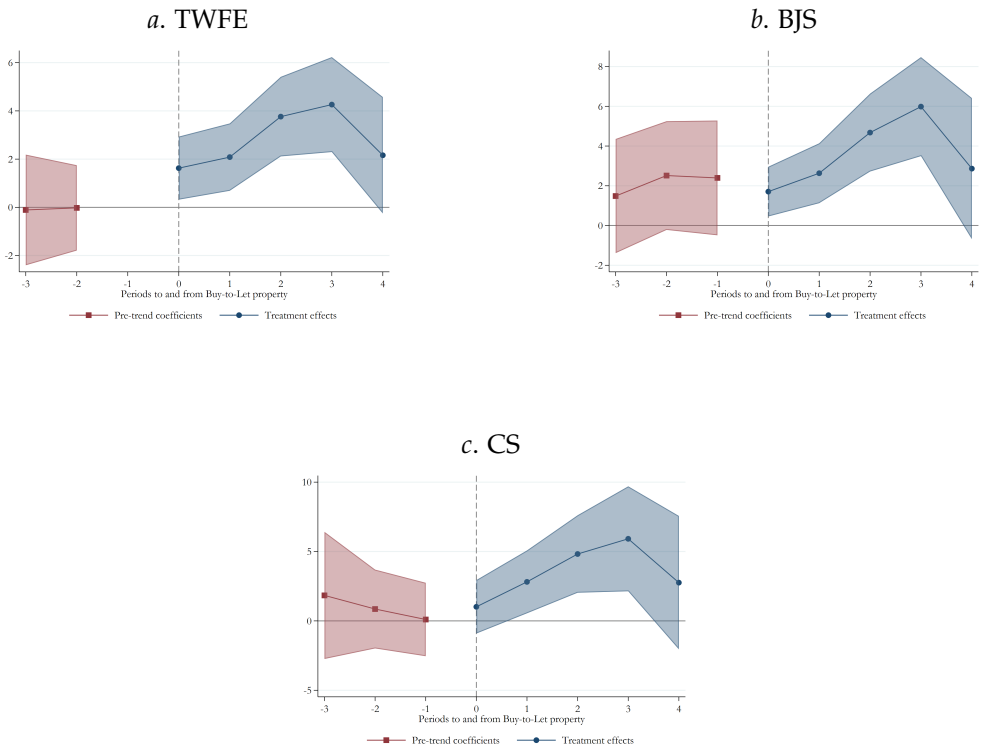
TABLE 2.5. Event study for the purchase of investment property

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 3	-0.291	1.122	1.239
	(0.793)	(1.066)	(1.659)
Pre-trend 2	-0.906	1.465	0.787
	(0.647)	(1.074)	(1.069)
Pre-trend 1		2.628**	1.283
		(1.118)	(0.985)
ATT 0	3.291***	2.853***	1.922***
	(0.493)	(0.475)	(0.737)
ATT 1	2.033***	2.817***	2.401***
	(0.522)	(0.575)	(0.906)
ATT 2	2.256***	3.760***	2.798***
	(0.554)	(0.669)	(1.044)
ATT 3	2.530***	4.498***	3.804***
	(0.621)	(0.836)	(1.326)
ATT 4	1.761***	4.789***	4.819***
	(0.670)	(1.364)	(1.863)
N	66979	57578	53806

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of purchasing investment property, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

FIGURE 2.8. Event studies of portfolio choice for the purchase of Buy-to-Let property



Note: Event studies in terms of purchasing Buy-to-Let property, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*). The units on the y-axis correspond to percentage point changes in the *Risky Share*.

2.4.2 The heterogeneous relationship of property and stockholdings

Table 2.7 reports the results for model 2.4, where the *Illiquid Share* is in turn the one for owner-occupied (1), investment (2) and Buy-to-Let (3) property. We see that the first order term can hardly be distinguished from 0, for all three types of property. The square term, on the other hand, is negative and well-identified for owner-occupied property, substantially less so for investment property and again not distinguishable from 0 in the Buy-to-Let case. Therefore, there is evidence of a concave shape in

TABLE 2.6. Event study for the purchase of Buy-to-Let property

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 3	0.0493	1.551	1.838
	(1.157)	(1.470)	(2.337)
Pre-trend 2	0.0505	2.424*	0.856
	(0.892)	(1.401)	(1.449)
Pre-trend 1		2.111	0.101
		(1.467)	(1.350)
ATT 0	1.719***	1.771***	1.015
	(0.659)	(0.634)	(0.982)
ATT 1	2.110***	2.641***	2.812**
	(0.701)	(0.763)	(1.156)
ATT 2	3.816***	4.623***	4.820***
	(0.834)	(0.993)	(1.424)
ATT 3	4.335***	5.984***	5.916***
	(0.996)	(1.266)	(1.930)
ATT 4	2.231*	2.869	2.756
	(1.231)	(1.800)	(2.454)
N	66979	65047	61956

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of purchasing Buy-to-Let property, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

the relationship between the level of the *Illiquid Share* and the *Risky Share* for owner-occupied property and, to a lesser extent, for investment property, but not for the specific case of Buy-to-Let property. This is in line with our hypothesis 2.3 and, to an extent, with our hypothesis 2.4. In particular, a 1% increase in the *Illiquid Share* for owner-occupied property is indeed not constant across the distribution of the *Illiquid Share*, determining heterogeneities in the marginal effect (Panel *a* of Figure 2.9) and an *n*-shape trajectory for the *Risky Share* (Panel *b* of Figure 2.9).

Panel *a* of Figure 2.9 and Table 2.8 tell us that the marginal effect is estimated to be around 0 for low levels of the owner-occupied share, to then turn to ever more negative territories as we move towards higher shares. This is consistent with the risk aversion of the value function being dependent on the ratio of owner-occupied property to wealth. Once calibrated, the model in Stokey (2009) as well as in the earlier Grossman and Laroque (1990) suggested that the *Risky Share* as a function of

TABLE 2.7. *Owner-occupied*, *Investment* and *Buy-to-Let Share*

	(1)	(2)	(3)
Owner-occupied Share	0.0641		
	(0.0462)		
Investment Share		0.0820*	
		(0.0484)	
Buy-to-Let Share			-0.0351
			(0.0678)
Owner-occupied Share Sq.	-0.00112***		
	(0.000429)		
Investment Share Sq.		-0.00144*	
		(0.000740)	
Buy-to-Let Share Sq.			0.000523
			(0.00112)
Inverse Mills' Ratio	-4.174	-2.485	0.335
	(3.533)	(3.487)	(5.824)
Observations	21041	21041	19075
Adj. R-squared	0.3612	0.3594	0.3862
Household FE	YES	YES	YES
Wave FE	YES	YES	YES

Note: All three models refer to specification 2.4, varying the type of *Illiquid Share*. Additional controls include household net worth, HRP's age band and educational attainment, and the FTSE 100 monthly performance. Standard errors (in parentheses) are bootstrapped, clustered at the household level (200 repetitions). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the *Illiquid Share* would be higher when the ratio of property to wealth was closer to the thresholds that trigger a moving and lowest when it was away from them (that is, right after adjusting property and therefore close to the household's ideal ratio). Both the thresholds and the ideal ratio are endogenously determined in the model depending, among other things, on the coefficient of relative risk aversion and on the size of adjustment costs, which are presumably heterogeneous in the cross-section of households. Therefore thresholds and ideal ratio should vary across households and while one cannot observe the exact pattern described theoretically in Stokey (2009) and Grossman and Laroque (1990), I find all the same a non-linear relationship. If

one assumes that the results of those works hold, that is that risk aversion is highest when the ratio of property to wealth is close to the ideal and lower when the ratio moves towards the thresholds, then it could be concluded that the ideal ratio in the data is to be found above 40%, and that the higher above 40%, the higher the share of households who have that as an ideal ratio, or alternatively that the higher the ratio, the closer we get to the ideal ratio on average, and therefore the higher is the risk aversion. While the expectation from the theoretical framework was of no relationship of investment property with stockholdings, the answer from the model is more complex. When one takes a broad definition of investment property, that is everything outside owner-occupied property, one sees a similar relationship as for owner-occupied property (Panels *a* and *c* of Figure 2.9), but the marginal effect cannot be as precisely estimated and might in fact be 0 all throughout.

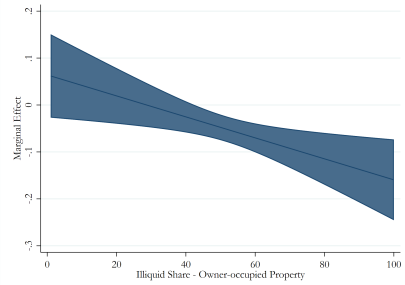
The picture changes though when one limits the definition of investment property to Buy-to-Let (Panel *d* of Figure 2.9): the marginal effect carries added uncertainty and one cannot pin-down a relationship. It is unlikely that this is due to lack of power as one can still count on 19,075 observations, 4,089 of which have a non-zero value for the Buy-to-Let share. While I theorised no relationship between the *Risky Share* and the *Illiquid Share* for investment property, and an examination of the joint distribution of the data in Panels *b* and *c* of Figure 2.3 provided indication of a linear relationship, there appears to be indeed no detectable relationship. There could be at least two explanations for the fact that the looser definition of investment property is somewhat related to stockholdings and appears to behave in a similar, albeit less well-identified, fashion as compared to owner-occupied property. The first one is that in its looser definition, investment property still includes property, such as second homes, that has a consumption nature as well as an investment one and could therefore show intermediate properties between the two, while Buy-to-Let has a pure investment nature. A second explanation could be that agents adapt stocks to their investment property holdings based on the covariance structure of their expected returns, as suggested by Pelizzon and Weber (2008). The latter seems less likely nevertheless, both because the pattern is similar to that of owner-occupied property

(suggesting that it has to do with consumption) and because no relationship can be observed in the Buy-to-Let case, for which the investment nature should be more pronounced. Moreover, most households are probably not sophisticated enough financially to consider covariance structures in expected returns, as it is known from the financial literacy literature that the lack of sound financial knowledge is a generalised phenomenon around the world, including in the UK (Gomes et al. (2021), Hastings et al. (2013), Nicolini et al. (2013)). The robustness checks in Appendix A, estimating the same models as those of Table 2.7 but using different weighting strategies, confirm the general patterns of the baseline results. They also suggest the possibility that the owner-occupied *Illiquid Share* could have a positive relationship with the *Risky Share* at low levels of the *Illiquid Share*.

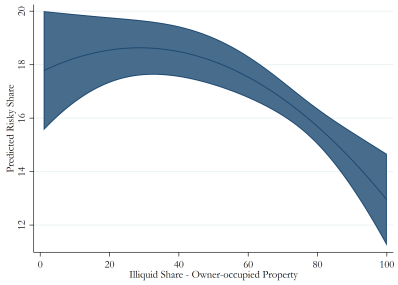
These findings are consistent with the earlier empirical literature but add flavour to it by showing that there is merit in investigating the heterogeneity of the relationship between property and stockholdings. On the one hand, I confirm that the focus of the literature on owner-occupied property is justified at the intensive margin, as this is the most relevant for stockholdings decisions. On the other, it is shown that while the average result (see the first row of Table 2.8) broadly agrees with those works finding a negative relationship between property and stockholdings (Yao and Zhang (2005), Chetty et al. (2017), Vestman (2019)), this average hides a richer distribution that possibly goes from positive to negative. The coefficient on the inverse Mills' ratio cannot be distinguished from 0 in any of the models, indicating that the covariates can already account for selection into the stock market (most notably, net worth). To rule out the fact that the unbalancedness of the panel could lead to bias in our estimates, model 2.4 was estimated on the full sample, without the inverse Mills' ratio, and including in turn a lagged and leading selection indicator, as per Wooldridge (2010). For all variations of model 2.4 using the different specifications of *Illiquid Share*, I cannot reject the hypothesis that the coefficients on the lagged or leading indicator are equal to 0. For instance, the χ^2 statistics for the lagged selection coefficient in the case of the owner-occupied share is 2.03, with a corresponding p-value of 0.1538. Unbalance in the sample is therefore not likely to be a cause for bias in the estimates.

FIGURE 2.9. Marginal effect of the *Illiquid Share* on the *Risky Share* and predicted *Risky Share*, with 95% CI

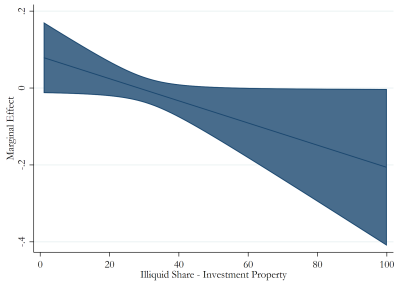
a. Marginal effect for owner-occupied case



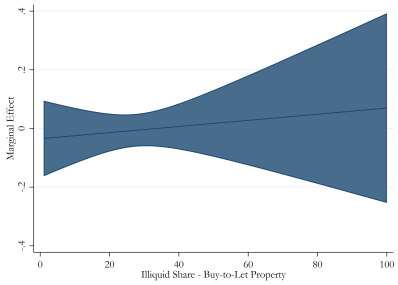
b. Predicted *Risky Share* for owner-occupied case



c. Marginal effect for investment case



d. Marginal effect for Buy-to-Let case



Note: Panels *a*, *c* and *d* plot the marginal effects and 95% confidence intervals at each percentage level of the three types of *Illiquid Share*, based on model specification 2.4. The units on the y-axis correspond to percentage point changes in the *Risky Share*. Panel *b* plots instead the fitted values and 95% confidence intervals for the *Risky Share* at each percentage level of the owner-occupied *Illiquid Share*. In this case, the units on the y-axis correspond to the level (in %) of the *Risky Share*.

TABLE 2.8. Marginal effects by type of shares

	(1) Owner-occupied Share	(2) Investment Share	(3) Buy-to-Let Share
Overall	-0.0739***	0.0609	-0.0314
	(0.0159)	(0.0394)	(0.0601)
10%	0.0418	0.0531	-0.0247
	(0.0382)	(0.0353)	(0.0480)
20%	0.0194	0.0243	-0.0142
	(0.0303)	(0.0250)	(0.0338)
30%	-0.00295	-0.00452	-0.00374
	(0.0230)	(0.0195)	(0.0285)
40%	-0.0253	-0.0334	0.00672
	(0.0168)	(0.0224)	(0.0362)
50%	-0.0477***	-0.0622**	0.0172
	(0.0134)	(0.0315)	(0.0513)
60%	-0.0700***	-0.0910**	0.0276
	(0.0149)	(0.0431)	(0.0692)
70%	-0.0924***	-0.120**	0.0381
	(0.0202)	(0.0556)	(0.0881)
80%	-0.115***	-0.149**	0.0486
	(0.0272)	(0.0685)	(0.108)
90%	-0.137***	-0.178**	0.0590
	(0.0348)	(0.0817)	(0.127)
100%	-0.159***	-0.206**	0.0695
	(0.0428)	(0.0949)	(0.147)

Note: All three columns report overall marginal effects as well as the marginal effects at 10% intervals along the distribution of the *Illiquid Share*, by type. All models refer to specification 2.4, whose estimation is reported in Table 2.7. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.4.3 Does investor type make a difference?

Another way of looking at the same problem is by analysing the marginal effect of the overall *Illiquid Share* for different investors' types. Instead of dividing the share into its owner-occupied, investment or Buy-to-Let components, I focus on how homeowners who hold only the main residence, homeowners who hold their main residence as well as investment property and renters who hold investment property respond to an increase in the *Illiquid Share*. To better distinguish the three groups, I classify a household as belonging to the renters group if it was always renting throughout the sample period and held non owner-occupied property in at least one period; as belonging to the homeowner without property investment category if the household owner-occupied for at least one period but never held non owner-occupied property; finally, as homeowner with investment property if it owner-occupied for at least one period and held non owner-occupied property for at least one period. Figure 2.10 and the first three columns of Table 2.9 show the marginal effects for model 2.5 across these three groups. While the estimate for renters is difficult to pin-down (this might be in part a matter of statistical power, since the renter group includes only 613 observations, 324 of which with a non-zero value for the *Illiquid Share*) those of the two types of homeowners are similar between them (with the main residence only homeowners showing positive marginal effects at low levels of the *Illiquid Share*) and comparable to that of the owner-occupied share in Panel *a* of Figure 2.9. This lends further support to the hypothesis that the consumption motive drives the relationship between property and stockholdings.

I also further divide homeowners in those who hold and those who do not hold a mortgage (in this case allowing a same household to change category between waves), to see if the relationship depends on the fact of being leveraged, above and beyond what is accounted for by net worth (which is included as a covariate). The *Risky Share* has a comparable relationship with the *Illiquid Share* whether or not the household has a mortgage on its main residence (Figure 2.11 and the last two columns of Table 2.9). In line with the theoretical framework of Section 2.1, this supports an

FIGURE 2.10. Marginal effect of the *Illiquid Share* on the *Risky Share* by investor type, with 95% CI

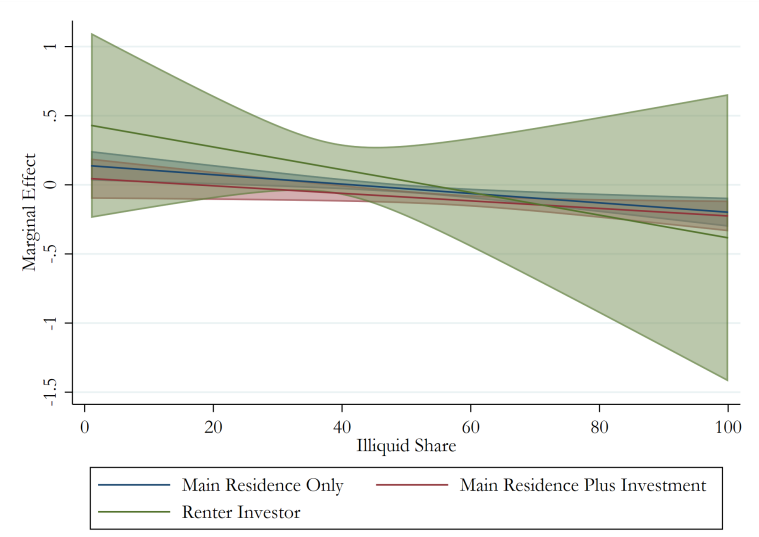
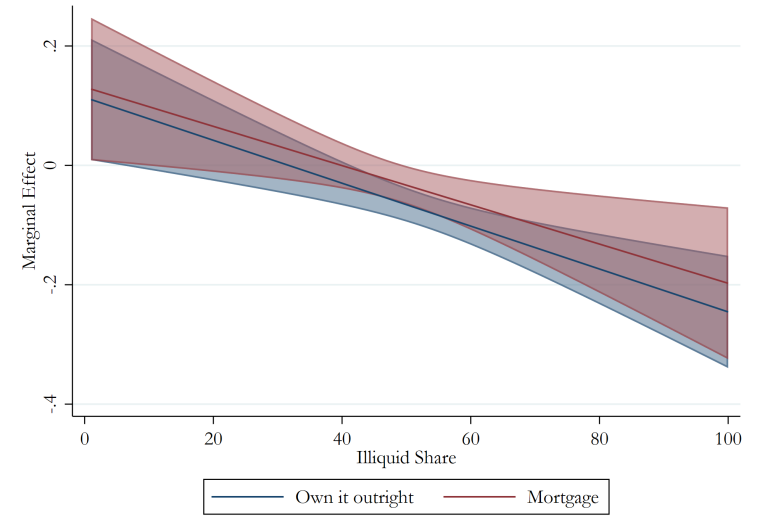


FIGURE 2.11. Marginal effect of the *Illiquid Share* on the *Risky Share* by homeownership type, with 95% CI



Note: Figures 2.10 and 2.11 plot the marginal effects and 95% confidence intervals at each percentage level of the *Illiquid Share*, by investor type, based on model specification 2.5. The units on the y-axis correspond to percentage point changes in the *Risky Share*.

adjustment cost explanation for the relationship between the *Risky Share* and the

Illiquid Share which is independent of the committed expenditure implied by holding a mortgage.

TABLE 2.9. Marginal effect by type of investor

	(1) Main residence	(2) Main residence + investment	(3) Renter	(4) Outright homeowner	(5) Mortgagor
Overall	-0.0944***	-0.150***	0.0928	-0.131***	-0.145***
	(0.0248)	(0.0281)	(0.0962)	(0.0208)	(0.0460)
10%	0.107**	0.0203	0.356	0.0780**	0.0982**
	(0.0426)	(0.0685)	(0.286)	(0.0393)	(0.0485)
20%	0.0731**	-0.00702	0.274	0.0420	0.0654*
	(0.0340)	(0.0531)	(0.198)	(0.0310)	(0.0368)
30%	0.0392	-0.0343	0.192	0.00609	0.0325
	(0.0262)	(0.0442)	(0.119)	(0.0233)	(0.0258)
40%	0.00530	-0.0616*	0.110	-0.0299*	-0.000352
	(0.0199)	(0.0335)	(0.0851)	(0.0169)	(0.0169)
50%	-0.0286*	-0.0888***	0.0274	-0.0658***	-0.0332**
	(0.0171)	(0.0256)	(0.134)	(0.0138)	(0.0148)
60%	-0.0625***	-0.116***	-0.0548	-0.102***	-0.0661***
	(0.0191)	(0.0235)	(0.215)	(0.0160)	(0.0214)
70%	-0.0965***	-0.143***	-0.137	-0.138***	-0.0989***
	(0.0250)	(0.0284)	(0.304)	(0.0219)	(0.0318)
80%	-0.130***	-0.171***	-0.219	-0.132***	-0.135***
	(0.0326)	(0.0377)	(0.396)	(0.0295)	(0.0433)
90%	-0.164***	-0.198***	-0.301	-0.210***	-0.165***
	(0.0410)	(0.0489)	(0.488)	(0.0376)	(0.0552)
100%	-0.198***	-0.225***	-0.383	-0.245***	-0.198***
	(0.0499)	(0.0611)	(0.581)	(0.0461)	(0.0673)

Note: All columns report overall marginal effects as well as the marginal effects at 10% intervals along the distribution of the *Illiquid Share*, by investor type. All models refer to specification 2.5. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.5 Conclusion

The model of portfolio choice with adjustment costs by Stokey (2009) predicts a non-linear relationship between the *Risky Share* and the *Illiquid Share* when the adjustment costs for housing bind, and no relationship otherwise. Within this framework, my hypotheses are that the adjustment costs should bind only for owner-occupied property, so that the relationship between its share and stockholdings should be heterogeneous and dependent on the level of the share. On the other hand, when the adjustment costs do not matter as much, as in the case of investment property, I hypothesise there to be no relationship. I test these hypotheses at the extensive and at the intensive margin, that is for tenure transitions and for purchases of property, as well as for marginal variations in the value of the *Illiquid Share*. The extensive margin is tested as an event study, while the intensive margin by means of linear fixed effects models at the household level, with a squared term to capture potential curvatures in the relationship between the *Risky Share* and the *Illiquid Share*, and accounting for selection into the stock market. The models are fit to the Wealth and Assets Survey for the UK, which follows households over time and details their asset distribution, including of property.

At the extensive margin, the results depart from what could be expected based on the theoretical framework. For owner-occupied property, the transition to mortgaged homeownership does not alter the *Risky Share*, so that the presence of adjustment costs does not appear to affect risk aversion. On the other hand, the *Risky Share* starts an upward trajectory of increase upon the transition to outright homeownership, suggesting a role for the termination of the committed expenditure represented by the mortgage. For both the broad definition of investment property and for Buy-to-Let property, there appears to be an increase in the *Risky Share* following the purchase, indicating that the investment *Illiquid Share* and the *Risky Share* are connected by more than the presence of adjustment costs. At the intensive margin instead, I find indeed that the relationship between the *Risky Share* and the *Illiquid Share* for owner-occupied property is not constant and depends on the level of the *Illiquid Share*. The

relationship is 0 or slightly positive at low levels of the *Illiquid Share*, to turn ever more negative at high levels of it. This is confirmed when one looks at homeowners, both those holding only the main residence and those owning non-main residence property. Moreover, the relationship holds similarly for both outright homeowners and mortgagors. At the same time, there appears to be no relationship between the *Risky Share* and the *Illiquid Share* for a strict definition of investment property, namely Buy-to-Let property, and a less identifiable albeit similar relationship to the owner-occupied property, for the broad definition of investment property (property excluding what is owner-occupied).

The findings of this chapter add to the existing literature in three ways. First, they show how the relationship between property and stockholdings is not the same across owner-occupied and investment property; rarely had the literature made this distinction, especially in treating investment property separately. Second, my results add complexity in that they show how, even within the categories of owner-occupied or investment property, the relationship between property and stockholdings is significantly heterogeneous. Third, my work provides evidence on the United Kingdom, while most previous works on property and stockholdings focused on the US and occasionally on other countries.

One limitation of this study concerns the estimate of the value of property. Being self-reported (and being property difficult to price in the first place), the value used might not correspond to the true value of the property. Still, this should not be a problem as long as the household decision is driven by the household's perception of the house value rather than the actual selling price it would have in the market. Another limitation concerns the measurement of the *Risky Share* on the one hand and of the *Illiquid Share* on the other. Both are in line with the literature on the topic, but may suffer from measurement error depending on whether specific assumptions on what quantities are relevant to households' decisions hold. For the *Risky Share*, it is assumed that the quantity relevant to households' decisions and reflecting their risk propensities is the absolute value of stocks in £, divided by the sum of the absolute

value of all liquid assets in £. This implies that a decrease in current or savings accounts (for instance after mortgage payments) would lead to an increase in the *Risky Share*, even if the absolute value of stocks remains the same. Indeed, if the *Risky Share* reflects households' risk aversion, such a situation would still imply an increase in risk aversion. Conversely, if this assumption is not warranted, then the change in the *Risky Share* would be simply a mechanical change in the portfolio composition, and therefore a measurement error with respect to the quantity relevant to capture households' risk aversion. For the *Illiquid Share*, the use of the total value of the property rather than its net equity, which is the difference between the value of the property and that of the outstanding mortgage, also relies on the assumption that the quantity relevant to households' decisions concerning stockholdings is the relative amount of their total assets held in property, independent of the value of the mortgage (this does not exclude that the presence of a mortgage is taken into account in other ways outside the *Illiquid Share* per se). In this case too, if the relevant quantity is the relative amount of net equity, one would face measurement error.

This study is also cautious as to causal claims. In the event study setting, identification is sharper but the endogenous nature of property purchases and tenure transitions, coupled with a short time-series of pre-trends, do not ensure that the patterns observed are causal. At the extensive margin, causal claims would rely on assuming that the model specification captures all confounding effects and isolate only exogenous variations in the *Illiquid Share*, which is unlikely. Especially for the intensive margin, the results are rather descriptive but they still provide valuable evidence, contrasted with theory, on the heterogeneous nature of the relationship and on the relative importance of owner-occupied as compared to investment property.

CHAPTER 3

The consequences of housing tenure for mental health

THERE exists a fundamental disagreement in evidence for and against an effect of housing tenure on mental health. On the one hand, cross-sectional results, even recent ones such as those offered by Park and Seo (2020), show that homeowners and renters (or at least some subpopulations of homeowners and renters) differ in their mental health in virtue of their tenure. Cross-sectional evidence is of course particularly prone to selection and concerns of endogeneity, as individuals choose, for a host of reasons, their tenure. Indeed, the cross-sectional results are in contrast with the longitudinal evidence, for instance that offered by Pierse et al. (2016), which suggests that once we look at the same individuals before and after they transition to homeownership, we cannot detect meaningful changes in mental health. The question is therefore if the difference we observe across tenure types is causal or compositional (Baker et al., 2013), because the correlation between housing tenure and mental health arises spuriously as individuals with higher mental health systematically select into homeownership. This chapter adds nuance to the debate around these two strands of the literature by showing that, given the nature of the housing tenure transition, a longitudinal approach helps accounting for omitted variable bias, namely in terms of unobserved and time-invariant heterogeneity, but focusing on a short panel around the change in tenure may hide longer term dynamics. Moreover, the literature tends to overlook the role of the type of homeownership, neglecting especially the transition to outright homeownership. Finally, recent advances in the event studies literature, or difference-in-differences with staggered adoption, warn about the risks of estimating event study models with two-way fixed effects (TWFE). As described in Section 1.5, I use

two different estimators developed to remedy such problems and compare them to the results of a standard event study.

From a theoretical perspective, discussed in Section 3.1, there is reason to believe that housing tenure may affect mental health. This should occur primarily through two channels. The increased security experienced by (outright) homeowners in terms of probability of eviction, the financial relief connected to concluding mortgage payments, as well as physical characteristics of the dwelling that tend to correlate positively with homeownership, constitute the material channel. The sense of stability and continuity, as well as agency over the house that an owner-occupied dwelling provides, together with its social meaning as a status symbol, constitute instead the intangible channel. Both channels are shaped by the fundamental features of housing discussed in Section 1.3. Transaction or adjustment costs increase the probability of residential stability and therefore affect the intangible channel, as does the right of *possession* through the ability to clearly attribute to oneself the status of homeowner. The right of *use* influences one's agency over the living environment, thereby also shaping the physical characteristics of the dwelling, but it also moves together with the probability of eviction, therefore conditioning both channels. The right of *disposition* bears a direct connection to mortgage payments, as these are calibrated to the residual value of the property, and thus to the material channel; at the same time, the financial value of a property can be argued to be also strictly related to the intangible channel, through the social status conferred upon the owner by the property. This theoretical framework bears a direct link with the identification strategy. One of the assumptions to identify an effect in difference-in-differences is that the unit treated does not anticipate treatment. In principle, an endogenous decision such as that of transitioning from one housing tenure to another should imply violation of the assumption of no anticipation because the individual is fully aware of what is coming. Yet, the material and intangible channels linking housing tenure to mental health may manifest their effects only once the tenure transition has occurred. They may in this sense be exogenous to the tenure decision, or at least less

endogenous than the effects on portfolio choice. The assumption of no anticipation can be tested in the data as described in Section 1.5.3.

The empirical literature on the existence of an effect of housing tenure on mental health is contested. While this may be due in part to differences in the definitions of mental health or in the context of investigation, the main cleavage exists between cross-sectional and longitudinal studies. A recent review of the effects of housing disadvantage on mental health is found in Singh et al. (2019), which lists tenure as one possible driver of mental health through the works of Kang et al. (2016) and Rumbold et al. (2012). Kang et al. (2016) establish a positive association between rented housing and the prevalence of anxiety in the elderly in South Korea, while Rumbold et al. (2012) find a positive association between continuous rental occupancy and externalising behaviour[1] in children in Australia. Additional recent cross-sectional contributions that report a role for tenure include Park and Seo (2020) and Ellaway et al. (2016). Park and Seo (2020) focus on low-income households in South Korea and find that, for those with adequate housing conditions[2], there is indeed higher incidence of depressive symptoms among renters, even after controlling for housing affordability, age, gender, marital status, educational attainment, log income and depressive symptoms at baseline. They also put forward the hypothesis that this is due to psychological comfort and ontological security linked to homeownership, if homeownership is seen as a positional good. Ellaway et al. (2016) use Scottish data to evaluate the effect of tenure on anxiety and depression, reporting a positive effect of renting even after controlling for age, gender, social class and income. Cairney and Boyle (2004) document with Canadian data how outright homeowners fare better than mortgagors, who in turn fare better than renters in terms of the Bradburn positive-negative affect balance scale. Li et al. (2022) take a cross-sectional approach over a long time span, using data from Australia. Comparing low-income renters and homeowners, they find that the difference in mental health between the two

[1] Aggressive/destructive behaviour on the part of the child.

[2] Reaching a minimum housing standard in terms of floor space, sanitary facilities, ventilation and heating, among others.

tenures shrinks as a function of stability, converging to 0 after five to six years of continuous rental occupancy.

On the other hand, a series of studies claim that no effect of tenure can be detected after accounting for appropriate confounding, chiefly by accounting for unobserved heterogeneity in fixed effects models. Baker et al. (2013) find no effect of tenure per se on the Mental Component Summary (MCS, a composite measure) in Australian data, after controlling not only for demographic and socio-economic differences between renters and homeowners, but exploiting the longitudinal data to account for time-invariant unobservables. Pierse et al. (2016) and Popham et al. (2015) confirm these results with a similar fixed effects model, the first using data from New Zealand and a psychological distress measure based on the Kessler-10 scale as outcome variable, the latter using the British Household Panel Survey (BHPS, which I use too) and the General Health Questionnaire (GHQ, also a composite index) as outcome variable. An exception is Courtin et al. (2018), whose study finds a negative association between the transition to homeownership and the Center for Epidemiologic Studies Depression Scale using a TWFE estimator, but limited to a sample of adults older than 50 in the United States.

While the TWFE estimator accounts for individual level time-invariant confounding, it might suffer from two problems in relation to estimating the effect on mental health of the transition to homeownership. The first one is that, if the effect is dynamic, looking only at the periods immediately before and after the transition, or looking only at the average, might hide an evolution over time; indeed, most of the studies use a relatively short time-series. By leveraging the combined British Household Panel Survey and Understanding Society datasets, described in Section 3.2, I can instead follow the joint evolution of tenure and mental health for individuals up to 27 periods. A second problem, which is the object of research of the recent literature on difference-in-differences designs with staggered adoption, arises because the coefficient on a binary treatment estimated as TWFE can be misleading if the assumption of homogeneous treatment effect is violated, which is presumably the

case when thinking about the effect of the transition to homeownership. This strand of research, summarised in de Chaisemartin and D'Haultfœuille (2022) and Roth et al. (2022), has also provided a number of alternative estimators that allow to estimate Average Treatment Effects (ATE) even when the assumption of homogeneous treatment effects does not hold. I use two of these alternative estimators (discussed in more details in Section 1.5 of Chapter 1): the imputation estimator of Borusyak et al. (2022) (BJS, implemented by Borusyak (2021)) and the doubly-robust estimator of Callaway and Sant'Anna (2021) (CS, in the implementation of Rios-Avila et al. (2021)), and I compare them to the results from a standard event study with dynamic treatment. My main result is that the transition to mortgaged homeownership is not associated with a meaningful change in reported mental health when looking at a direct question on unhappiness and depression, while there is some evidence of a worsening when using a composite measure of mental health. Conversely, the transition to outright homeownership corresponds to the beginning of a significant downward trajectory in mental distress (Section 3.5.3). These results suggest that the difference in mental health observed between mortgagors and renters is more likely to be compositional, but that the one between mortgagors and outright homeowners has reason to be considered (at least partly) causal. To uncover these relationships, it is important to take a long view on housing tenure and mental health. I also show that the results for the transition to mortgaged homeownership are consistent across degrees of indebtedness, while those for the transition to outright homeownership are more pronounced for those with a Loan-to-Value at origination between 0.6 and 0.8. Section 3.4 provides concluding remarks.

3.1 Insights from theory and hypotheses

I first lay out the foundations of the paper by defining and discussing key concepts such as mental health, as well as giving structure to the framework through which to think about how the transition to mortgaged and outright homeownership could affect mental health.

3.1.1 Definitions and operationalisations

Mental health is commonly defined in its positive acceptation:

> [...] a state of well-being in which the individual realizes his or her own abilities, can cope with the normal stresses of life, can work productively and fruitfully, and is able to make a contribution to his or her community (WHO, 2005).

I focus instead on the the lack of mental health, which may lead instead to being outside such a state of well-being, potentially falling into depression, defined as:

> [...] a common mental disorder, involving persistent sadness or loss of interest or pleasure accompanied by several of the following symptoms: disturbed sleep or appetite, feelings of guilt or low self-worth, feelings of tiredness, poor concentration, difficulties making decisions, agitation or physical restlessness, talking or moving more slowly than normal, hopelessness, and suicidal thoughts or acts (WHO, 2016).

In this study, the main survey question used gauges directly in the respondent their own assessment of the extent to which they have or lack mental health, asking *Have you recently been feeling unhappy or depressed?*, with answer options:

- *Much more than usual*
- *Rather more than usual*
- *No more than usual*
- *Not at all*

The validity of such a question to successfully elicit the true mental health of the respondent is unobvious and ultimately unverifiable, yet one can believe it to convey some relevant information, under a set of assumptions. The first is that *feeling unhappy or depressed* has a similar meaning across respondents and for the same respondent over time and that this meaning is not substantially different from the definition

of depression above. Analogously, the second is that the four categories used in the answer option represent a scale that remains consistent across individuals and over time for a same individual. In operationalising the question for quantitative estimation, I assume this scale to be ordinal and going from a score of 1 (*Not at all*) to 4 (*Much more than usual*), therefore making the further assumption that the distance between one category and the next is fixed and normalised to 1. The third assumption is that the concepts of *recently* and *usual* elicit the true, underlying psychological distress of the respondent and not their emotional state when answering. That is, the *evaluation* component of their answer prevails over the *affect* component, in a subjective well-being framework (OECD, 2013). Evaluation involves a cognitive component, as the respondent needs to think about a standard for unhappy or depressed and compare their experience to it. At the same time, affect is the component that refers to the emotional state the respondent is in at the moment of survey, and that will possibly have an impact itself on their answer. While the evaluation component should give information on an average of how the respondent felt in the period before the survey (since the survey question is phrased in terms of how one felt *recently* and *recently* is stated to correspond to "the last few weeks"), the affect component could be unrepresentative of the average of the respondent's feelings in the period before the survey. Nevertheless, both of them should give an ordinal indication of the feeling of unhappy or depressed that goes in the same direction (more or less unhappy/depressed) and can therefore be used for estimation. Even if respondents' answer were to contain a substantial affect component, this would still convey non-contradictory information on the mental health of the respondent, as long as the affect component is not dramatically distant from the evaluation component (for instance, if the respondent has recently felt much more unhappy or depressed than usual, but because at the moment of answering they feel happy, their answer is *Not at all*). Systematic and sizeable deviations from these assumptions could in fact generate results that are not driven by actual changes in mental health, but rather by different meanings, or changes in the meaning, of our key concepts. While the testing of such assumptions is outside the scope of this work, Section 3.5, exploring housing

tenure and mental health during the pandemic period, shows how an indicator based on this survey question tracks plausible trends in psychological distress during the pandemic period, worsening as the pandemic became more severe and improving as the contagion (and the restrictions) diminished. Though not as directly linked to the concept of depression, a common measure of mental health used in the psychiatric literature, either as a single or as multiple-dimension indicator, is the 12-items General Health Questionnaire(Romppel et al. (2013), Hystad and Johnsen (2020)), henceforth GHQ. This is composed of 12 questions, including *Have you recently been feeling unhappy or depressed?*, that gauge psychological distress. In Appendix B I describe the 12-items GHQ and use it as a secondary measure of mental health, to increase the robustness of the analysis. Next, I explore each of the two channels through which housing tenure may affect mental health. Housing can be thought of as taking on two main meanings for an individual and, as a consequence, there exist two main channels through which housing can affect mental health: a material one, and an intangible one.

3.1.2 Material channel

Housing can be thought of as a condition that has material consequences in terms of security of tenure and, at the extremes, of fear of eviction or foreclosure. Moreover, dwelling characteristics may intuitively affect mental health, as well as correlate with tenure because homeowners generally enjoy higher quality housing. Housing is a special kind of financial commitment, because, be it a rent or mortgage payment, it represents a large part of households' monthly expenses and a payment that cannot be flexibly reduced at will, as it more easily happens for leisure, food or energy consumption; in fact, housing payments' inflexibility may in turn spillover and cause attempts to save on consumption items that can be adjusted more easily. In this respect, mortgage payments are especially constraining as they imply a long-term commitment to one specific property and exposure to a substantial debt burden. These financial commitments can become a source of stress in different

ways. In a stress process model (Pearlin et al. (1981); Pearlin (1999)), debt may differ from stress related to economic hardship (for instance, being behind with rent payments) because it is found more among individuals of higher socio-economic status (Drentea and Reynolds, 2015). With a similar logic, mortgagors (but also outright homeowners) are exposed to different market phenomena as compared to renters, because owning a house shields from rent risk but implies asset price risk (Sinai and Souleles, 2005). Both types of financial stress related to housing payments are found to be associated to worsened mental health. Taylor et al. (2007), using data from the British Household Panel Survey, reports how arrears in housing payments are negatively associated with mental health, measured by the 12-items GHQ. Communities more heavily affected by foreclosures in the United States see more emergency visits for (among others) mental health conditions (Currie and Tekin, 2015) and individuals exposed to foreclosures have increased symptoms of major depression and generalised anxiety disorder (McLaughlin et al., 2012). Conversely, the end of such a financial commitment through the achievement of outright homeownership should intuitively break the connection between housing payments stress and mental health.

The physical quality of the dwelling is also plausibly affecting mental health. Evans et al. (2000) suggest that strained interpersonal relationships and diminished motivation, as well as decreased self-esteem, could mediate the relationship between housing quality and mental health. Insecurity stemming from physical features of the dwelling that cannot be easily dealt with (e.g. an infestation) could be an additional mediator (Evans et al., 2003). In fact, poorer quality housing is associated with worse mental health outcomes. Curl et al. (2015) finds that the Mental Component Summary scale rises for residents of deprived communities in Scotland after physical housing improvements to their homes, while Pevalin et al. (2017), using previous waves of the same data that I use for the UK, show how physical housing problems in the present as well as in the past are associated with worse mental health, assessed on the 12-items GHQ. In addition, homeowners tend to live in higher quality housing. Park and Seo (2020), using Korean data for low-income households, is a recent example

that reports how the proportion of homeowners living in substandard housing is lower than that living in adequate housing (in terms of floor space, sanitary facilities and building services) and that homeowners fare better than renters in terms of depressive symptoms. Combined with the fact that homeowners also have more agency concerning modifications to the physical features of the house, this makes dwelling conditions an important part of the material channel connecting housing tenure and mental health.

3.1.3 Intangible channel

Housing and especially housing tenure take nevertheless also less material meanings for individuals and therefore a second channel leading to mental health is intangible. Shaw (2004) suggests that there exist *soft* ways in which housing tenure can affect mental health. Such an intangible channel has to do with the meaning that a house takes for the individual. Saunders (1986) introduces the concept of *ontological security* developed earlier by Laing (1965) and Giddens (1984) to the realm of housing. Ontological security can be thought of as a need for continuity and stability in one's existence that rests on routine and habit. The house appears as a central locus of ontological security because of the time spent in it but even more because it is a place of rest and shelter from the outside world. The stronger the agency one has over the house, the easier it will be to feel ontologically secure. It is natural then to consider homeownership, and moreso outright homeownership, as a status that facilitates ontological security as compared to renting. Dupuis and Thorns (1998) note that ontological security is not to be conceived in absolute terms but rather as a context-specific concept that can also vary across socio-demographic axes. Hiscock et al. (2001), Padgett (2007) and Colic-Peisker et al. (2015)) are empirical examples of the importance of ontological security with respect to housing, across different contexts. Moreover, recent studies focusing on tenure stability suggest that a higher degree of security of rental occupancy might shrink or close the gap with homeowners, in

terms of mental health as well as of a wider range of outcomes (Acolin (2022); Li et al. (2022)).

At the same time, the home could be seen as a marker for social status. Anderson et al. (2015) argue that the desire for status is a fundamental human motive. Its fundamental characteristic would make it a driver of mental health, so that there exists a "[...] danger of failing to conform to the ideals of success laid down by our society and that we may as a result be stripped of dignity and respect [...]" (de Botton, 2004). I posit that reaching homeownership is a key status symbol in the UK context (Hiscock et al. (2001); Marshall and Smith (2016)) and that, if the desire for status is indeed a fundamental human motive, homeownership should affect mental health through this channel too.

3.1.4 Hypotheses

Based on this framework, I put forward the following hypotheses:

HYPOTHESIS 3.1. Because of the intangible channel (increased social status and ontological security), the transition to mortgaged homeownership corresponds to a decrease in mental distress.

HYPOTHESIS 3.2. Because of the material channel (the conclusion of mortgage payments and the probability of eviction becoming negligible), the transition to outright homeownership corresponds to a decrease in mental distress.

The theoretical framework does not rule out a role for the material channel in the transition to mortgaged homeownership or of the intangible channel in outright homeownership, but rather implies that the primary channel differs across the two types of transition.

3.2 Patterns and data description

The dataset used is a rich longitudinal survey composed of the British Household Panel Survey between 1991 and 2009 and then continued as Understanding Society, with the most recent survey I use (wave 10) collected between 2018 and 2020 (University of Essex, Institute for Social and Economic Research, 2021b). There exists a COVID-19 module too, with increased frequency, that is described and used in Section 3.5. The total observations used in the analysis vary based on the model and range between around 180,000 and 602,000, depending on the relevant sample. For the descriptive statistics reported in this section, I use the widest sample of 602,000 observations that reduces to about 595,000 observations for the presence of covariates that are not available for all observations.

Table 3.1 reports mean and standard deviation across the different tenure groups for a set of variables. The first one is the share of individuals reporting to be more unhappy or depressed than usual, a binary variable created from the four item response that is used as the primary outcome in this study. Outright homeowners have the lowest share, indicating the best reported mental health, followed by mortgagors and renters. Outright homeowners are also markedly older, at a mean age of 60, as compared to mortgagors and renters whose mean age is around 40. Mortgagors have also the largest share of individuals with higher education, followed by outright homeowners and then by renters.

As this work is focused on the trajectory of mental health before and after tenure transitions, it is useful to look at how frequent tenure transitions are in the sample. Table 3.2 reports the frequency of individuals in the sample by number of tenure transitions experienced. We see that the majority of individuals (during the sample period) does not experience a tenure transition (79.6%). A substantial share experiences one tenure transition (14.2%), while a non-negligible share experiences more than one transition (6.2% cumulatively). The share of individuals reporting to be more unhappy or depressed than usual is rather similar across the different groups,

TABLE 3.1. Overview of summary statistics

	(1) Outright homeownership		(2) Mortgage		(3) Rent	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Share more unhappy or depressed than usual	0.155	0.362	0.197	0.398	0.258	0.438
Age	60.172	17.054	40.076	13.369	42.396	18.771
Share with higher education	0.299	0.458	0.374	0.484	0.216	0.411
Observations	180680		248106		165950	

Note: Mean and standard deviation for a set of salient variables, by housing tenure. Variables reported as shares are binary recoding of variables with wider ranges.

TABLE 3.2. Frequency of tenure transitions and salient variables

Number of transitions	Absolute frequency	Relative frequency	Share more unhappy and depressed than usual	Age	Share with higher education
0	77,451	0.796	0.209	43.501	0.277
1	13,833	0.142	0.200	43.077	0.347
2	3,607	0.037	0.211	38.831	0.352
3	1,421	0.015	0.213	41.247	0.333
4	557	0.006	0.235	39.189	0.322
>4	393	0.004	0.214	41.579	0.325
Total	97,262	1			

Note: The table lists the absolute and relative frequency of the number of individuals in the sample by number of tenure transitions, as well as the mean of salient variables.

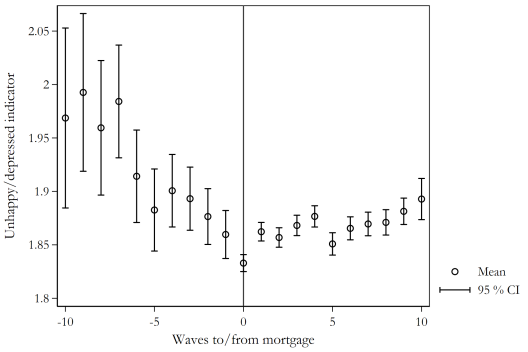
with the exception of those experiencing four transitions, who represent nevertheless only 0.6% of the total. Mean age shows more variations across the groups, but without a clear pattern. In terms of share with higher education, those experiencing no transition appear to have a relatively lower figure as compared to all the other groups.

How does the mean level of the unhappy/depressed score evolve in relation to the distance from the event of transition, for those who transition? Panel *a* of Figure 3.1 shows how for those transitioning into a mortgage, the unhappy/depressed

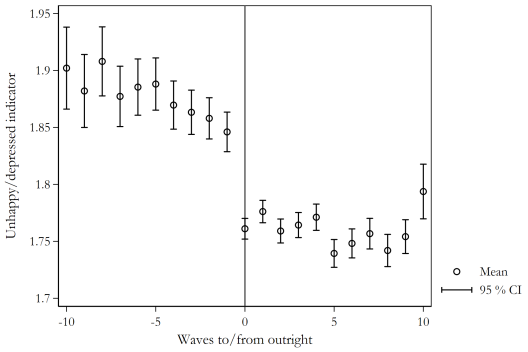
indicator linearly decreases up to the point of the transition, to then flatten and possibly slightly increase. The pattern for the 12-items GHQ shows a much more pronounced increase in distress following the transition (Panel *a* of Figure B.1 in the Appendix). Conversely, for those transitioning to outright homeownership, there appears to be a clean discontinuity around the tenure transition, with their reported mental distress jumping down by about 0.1 points at the wave when they become outright owners, and then remaining largely stable thereafter. While the discontinuity is still there when using the 12-items GHQ (Panel *b* of Figure B.1 in the Appendix), following the transition, the score tends to converge back to pre-transition values. Of course, this descriptive patterns are ignoring counterfactual outcomes, which will be the concern of the rest of the paper.

FIGURE 3.1. Evolution of reported mental health as a function of time distance from an event

a. Taking out a mortgage



b. Becoming an outright homeowner



Note: Evolution of the mean unhappy/depressed score as a function of distance from obtaining a mortgage (Panel *a*) and from outright homeownership (Panel *b*).

3.3 Results and discussion

I start the illustration and discussion of the results with the transition to mortgaged homeownership and then I move on to the transition to outright homeownership. In both cases, as detailed in Section 1.5, I use three different difference-in-differences set ups: a TWFE estimator, the imputation estimator of BJS and the approach of CS. In addition, when studying the transition to outright homeownership, I distinguish between the inclusion and exclusion from the never-treated comparison group of those who first appear in the sample as renters. A last section describes how the results vary across individuals based on their Loan-to-Value (LTV) ratio.

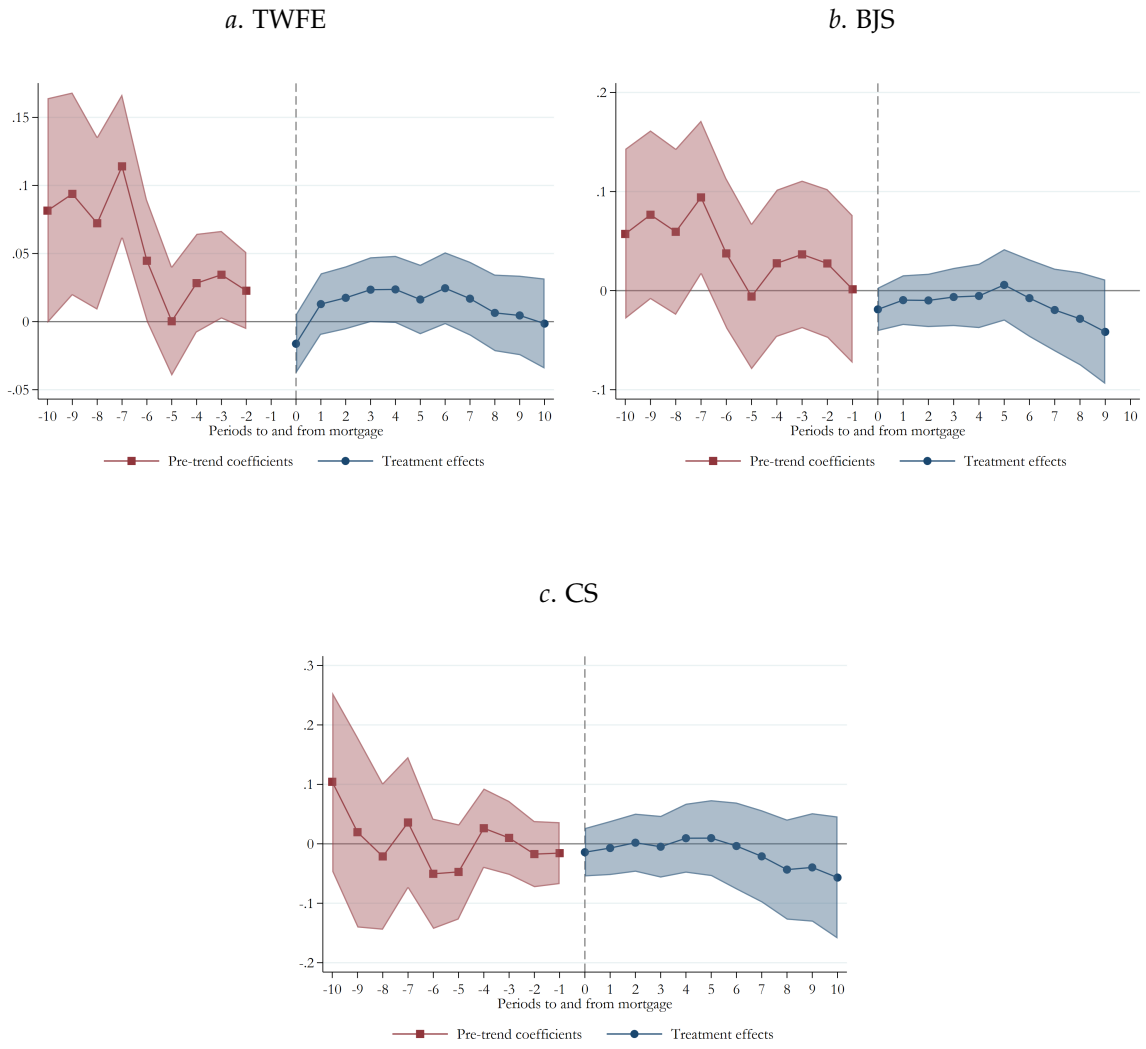
3.3.1 The transition to mortgaged homeownership

Figure 3.1, as a descriptive illustration of the trajectory of the unhappy/depressed indicator, had shown that on average the indicator decreases in value as the beginning of the mortgage approaches. In a difference-in-differences setting where we model a counterfactual outcome, we would expect there to be negative anticipation effects if the same pattern were to hold. The results tell nevertheless a different story. The pre-trend and treatment effect coefficients from a two-way fixed effects estimator, as reported in Panel *a* of Figure 3.2, show large positive pre-trend coefficients up to 6 periods before the event, and smaller coefficients, in part not distinguishable from 0, right before the event. Although in the absence of a treatment effect homogeneity assumption such coefficients are biased, the BJS method (Panel *b* of Figure 3.2), which is robust to treatment effect heterogeneity and separates the testing of pre-trends from the treatment effect estimation, yields similar results. The CS estimator (Panel *c* of Figure 3.2) shows instead pre-trend coefficients that are much tighter around 0. All coefficients are reported in Table 3.3. In all cases, looking at the evolution of mental health among prospective mortgagors before the transition, when modelling their counterfactual mental health score, shows no sign of the negative anticipation effects that one might expect based on Figure 3.1. In fact,

anticipation effects are either absent or positive. Moreover, treatment effects in the first ten periods after becoming a mortgagor are close to 0. After about five periods from the transition, we observe a declining trend that nevertheless remains in the neighbourhood of 0. Treatment effects at long horizon are anyway less reliable and have increased uncertainty around their estimate. This result goes against Hypothesis 3.1, as the transition to mortgage homeownership does not appear to bring about a reduction in mental distress, not in anticipation of the event, nor after the event occurs. Coupled with the fact that cross-sectionally we still observe a better reported mental health in mortgagors, it strengthens the evidence that the cross-sectional difference is compositional: those who take out a mortgage tend to have systematically better mental health *before* taking out the mortgage. An alternative explanation is that the positive effect from the intangible channel is offset by a negative effect from the material channel, if transitioning to mortgaged homeownership implies a more sizeable financial commitment than in renting. When repeating the analysis using the 12-items GHQ instead of the unhappy/depressed indicator (Figure B.2), there is mixed evidence whereby the BJS estimator detects large anticipation effects, with a drop at the moment of transition followed by a steady rise in distress, while the CS and the TWFE estimators show less evidence of anticipation effects and an increase in distress following the tenure transition. If the transition to mortgaged homeownership were to increase mental distress, that would point even more strongly to the fact that the cross-sectional difference in mental health between renters and mortgagors is compositional, because even as becoming a mortgagor worsens mental health, average mental health is higher for mortgagors than for renters.

The null result for the unhappy/depressed indicator is in line with previous longitudinal evidence. Baker et al. (2013) using a different indicator of mental health from mine (MCS) and data from Australia, employ a TWFE estimator where they additionally control for age, income, occupation, education, mobility and housing affordability. They look at the average treatment effect, without focusing on dynamics over-time nor testing for pre-treatment trends. They do not find significant

FIGURE 3.2. Event studies of mental health for the transition to mortgaged homeownership



Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

differences between those who go from renting to owning, nor from owning to renting, as compared to those who remain in the same tenure group. Importantly, they follow individuals for a maximum of 7 periods and therefore, although this is not stated explicitly, they mostly refer to transitions to mortgaged homeownership. They also note how these results, together with the cross-sectional differences between renters and homeowners, point in the direction of the difference in mental health

between homeowners (with a mortgage) and renters to be compositional and not due to tenure. Popham et al. (2015) use the same data of this paper on a restricted sample, up to 2009, of social renters who opted to buy the house they already lived in (using *Right to Buy*, a UK government policy), and using the 12-items GHQ as the mental health indicator (used also in the Appendix to this chapter). They follow for either three or four periods social renters who transition to homeownership but remain in the same house, and compare them to those who transition to homeownership changing house and to those who remain renters, adjusting additionally for age, marital status, employment status, financial status and highest educational qualification. They mention that the individuals in the sample may move to either mortgaged or outright homeownership. They perform testing of pre-trends and notice evidence of anticipation effects in the direction of increased distress in the period before moving for those who moved to owned or rented accommodation. Apart from this, they do not find that becoming homeowners under *Right to Buy* affected mental health and they note how those who bought a house were different from the outset, including in terms of lower reported psychological distress. Pierse et al. (2016) use data from New Zealand and focus on the Kessler-10 scale as the mental health measure. They use a one-way fixed effects estimator and report the overall coefficient on the housing tenure dummy, also looking separately at homeowners with a sizeable mortgage, and do not find an association between mental health and housing tenure. In this case too, the authors report that there existed a pronounced cross-sectional gradient instead. Courtin et al. (2018) is an example using a TWFE estimator (with additional controls for lagged reported mental health, age, marital status, size of the household, number of children, labor-force participation, income, non-housing wealth, self-reported health, health behaviours, daily activities) and reporting an association between tenure and mental health. They focus on individuals older than 50 in the United States and use as their outcome variable the 8-item CES-D scale. They do not test for pre-trends but they look at dynamic effects, finding a decrease in psychological distress in the first wave of homeownership as well as two years after, with the effect converging back to 0 thereafter. They do not distinguish between transitions to

mortgaged and outright homeownership. Interestingly, this study investigates also the reasons behind the transition from renting to owning in old age, with the most important category of reasons associated to a decline in psychological distress being that of pull factors, for instance neighbourhood level amenities.

TABLE 3.3. Event study of mental health for the transition to mortgaged homeownership

	(1) TWFE	(2) BJS	(3) CS
Pretrend 10	0.0815*	0.0572	0.104
	(0.0421)	(0.0438)	(0.0771)
Pretrend 9	0.0938**	0.0766*	0.0196
	(0.0380)	(0.0435)	(0.0819)
Pretrend 8	0.0722**	0.0594	-0.0213
	(0.0324)	(0.0428)	(0.0629)
Pretrend 7	0.114***	0.0941**	0.0358
	(0.0271)	(0.0396)	(0.0565)
Pretrend 6	0.0447*	0.0376	-0.0505
	(0.0230)	(0.0387)	(0.0474)
Pretrend 5	0.000310	-0.00591	-0.0472
	(0.0205)	(0.0376)	(0.0409)
Pretrend 4	0.0282	0.0276	0.0262
	(0.0185)	(0.0379)	(0.0342)
Pretrend 3	0.0344**	0.0366	0.00991
	(0.0164)	(0.0380)	(0.0318)
Pretrend 2	0.0227	0.0274	-0.0173
	(0.0144)	(0.0383)	(0.0284)
Pretrend 1		0.00132	-0.0157
		(0.0381)	(0.0266)
ATT 0	-0.0163	-0.0189*	-0.0142
	(0.0112)	(0.0112)	(0.0208)
ATT 1	0.0129	-0.00959	-0.00710
	(0.0115)	(0.0128)	(0.0232)
ATT 2	0.0175	-0.00988	0.00187
	(0.0118)	(0.0138)	(0.0250)
ATT 3	0.0235*	-0.00644	-0.00492
	(0.0122)	(0.0150)	(0.0265)
ATT 4	0.0237*	-0.00537	0.00944
	(0.0126)	(0.0166)	(0.0296)
ATT 5	0.0162	0.00578	0.00958
	(0.0130)	(0.0184)	(0.0326)
ATT 6	0.0245*	-0.00758	-0.00368
	(0.0135)	(0.0200)	(0.0373)
ATT 7	0.0169	-0.0195	-0.0211
	(0.0138)	(0.0213)	(0.0396)
ATT 8	0.00642	-0.0284	-0.0434
	(0.0144)	(0.0240)	(0.0430)
ATT 9	0.00455	-0.0416	-0.0397
	(0.0149)	(0.0269)	(0.0466)
ATT 10	-0.00145	-0.0583**	-0.0568
	(0.0169)	(0.0281)	(0.0525)
N	477705	187876	180298

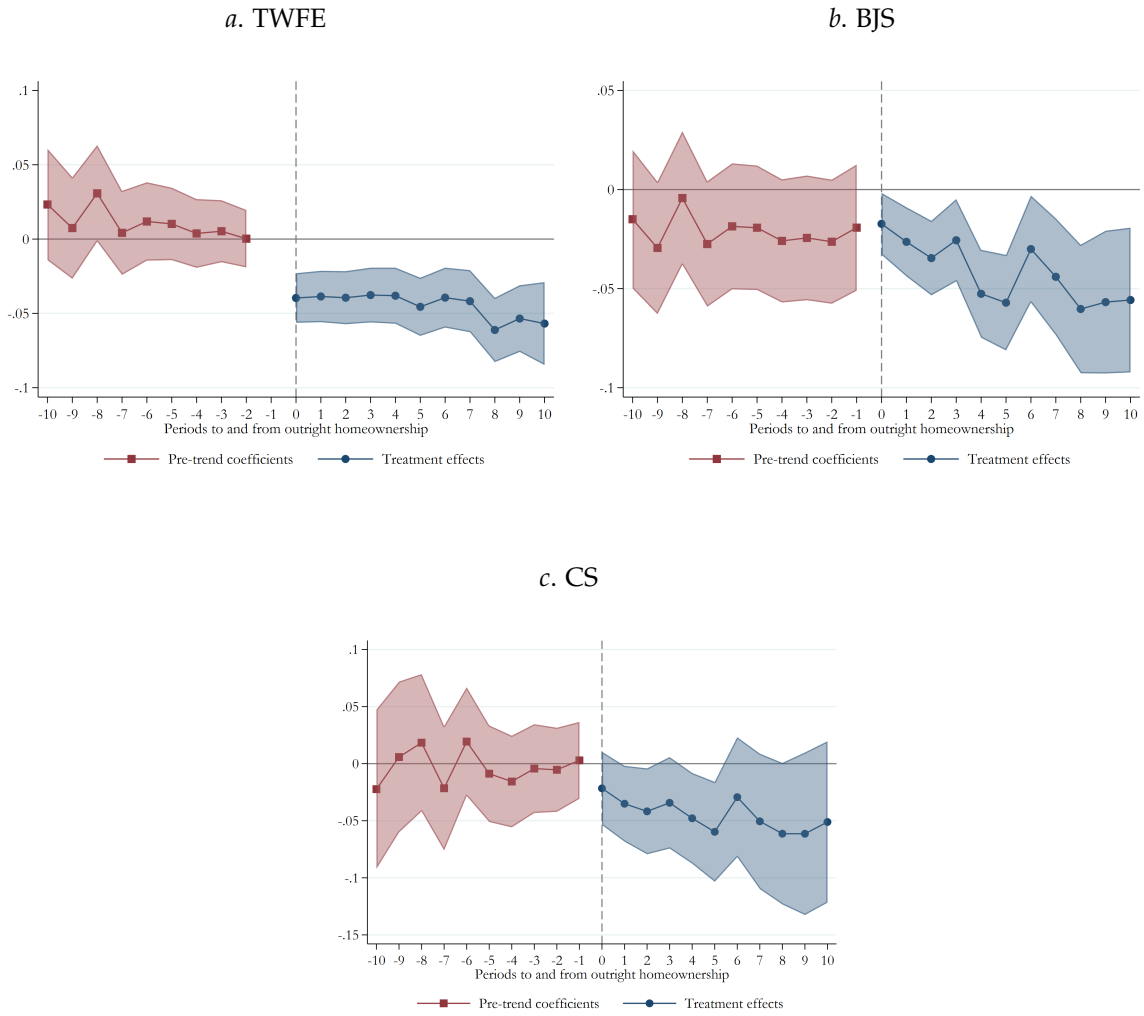
Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

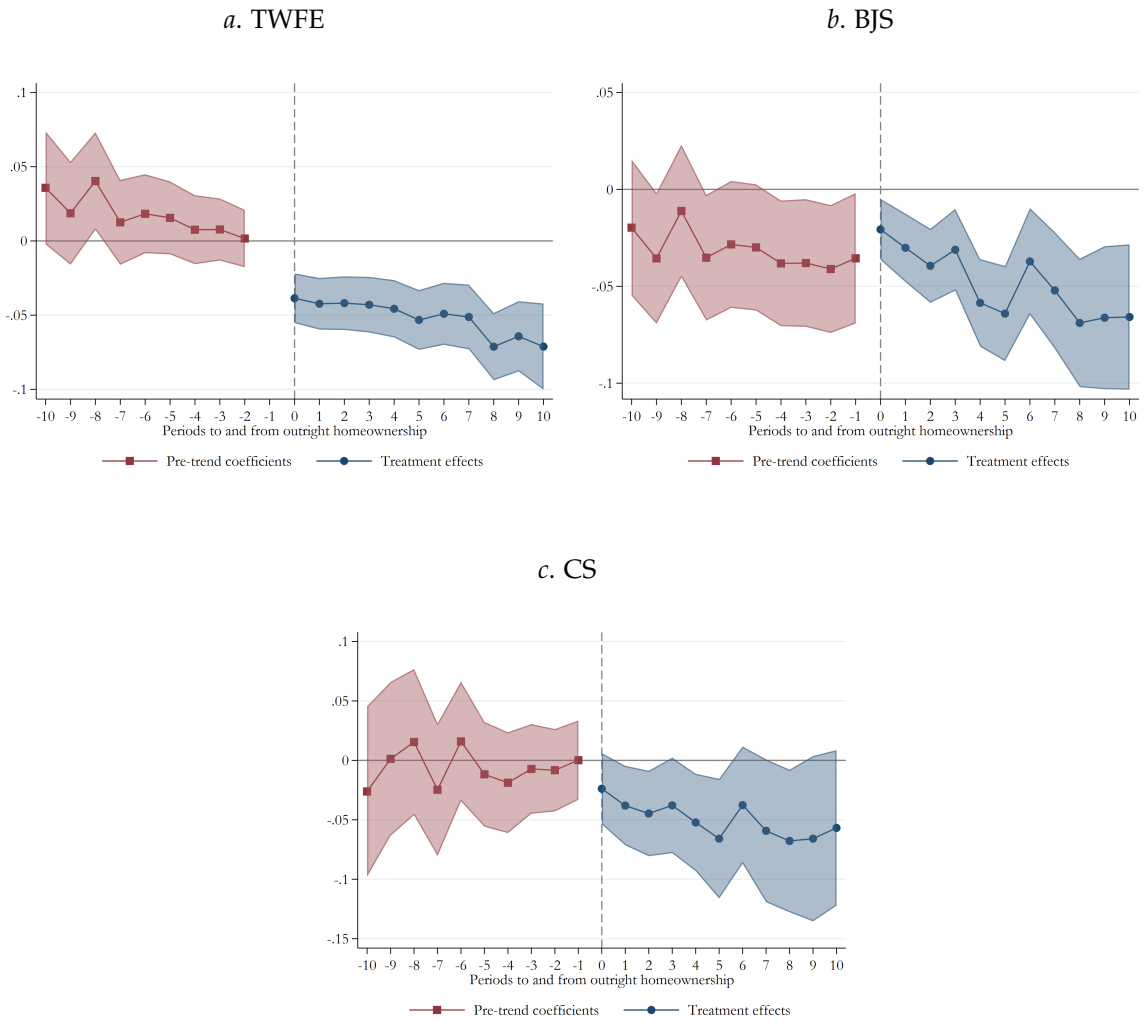
3.3.2 The transition to outright homeownership

FIGURE 3.3. Event studies of mental health for the transition to outright homeownership



Note: Event studies in terms of becoming an outright homeowner, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE 3.4. Event studies of mental health for the transition to outright homeownership, excluding renters



Note: Event studies in terms of becoming an outright homeowner and excluding renters as counterfactuals for the never-treated group, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

The transition to outright homeownership reveals different dynamics from the transition to mortgaged homeownership. Looking only at those who transition to outright homeownership (Panel *b* of Figure 3.1), without modelling a counterfactual, shows a big discontinuity in reported mental health around the time of becoming an outright homeowner, with the unhappy/depressed score jumping down by about 0.1. This is in contrast with the linearly declining mental health in the years before the

transition to mortgaged homeownership, for those who transitioned (Figure C.1). The difference-in-differences model of a counterfactual is carried out both including (Figure 3.3) and excluding (Figure 3.4) from the sample renters who never became outright homeowners. The corresponding coefficients are reported in Tables 3.4 and 3.5. As compared to the results in Section 3.3.1, pre-trends are tighter around 0, indicating that before the transition there is less of a difference in mental health dynamics between those who transition to outright homeownership and the sample used as a counterfactual. After the transition to outright homeownership, all models shows a significant, negative ATT that increases in magnitude over time, with an average of about 0.05 points. This is in contrast with the transition to mortgaged homeownership, for which the ATTs for each horizon could not be distinguished from 0. The estimates from the TWFE (Panel *a* of Figure 3.3 when renters are included and of Figure 3.4 when excluded) differ from those of the BJS and CS methods primarily because of the discontinuity at the first period of outright homeownership, where the coefficients jumps down by almost 0.05 points, in a fashion similar to that observed in the descriptive Panel *b* of Figure 3.1. As discussed in Section 1.5 nevertheless, this model is not correctly identifying the ATTs unless more stringent assumptions are made. Figures B.3 and B.4 in the Appendix repeat the same exercise using the 12-items GHQ instead of the unhappy/depressed indicator. The results are qualitatively similar, indicating a decline in the GHQ indicator following the transition to outright homeownership, but their dynamics differ for the BJS estimator, for which there is a discrete jump down of the indicator around the transition, instead of a gradual decline, and for the TWFE estimator, where the opposite is true.

Therefore, while there does not appear to be an effect of the transition to mortgaged homeownership on mental health for the unhappy/depressed indicator and possibly a worsening when looking at the 12-items GHQ, the transition to outright homeownership sees a decrease in psychological distress that might grow in magnitude over time. While this result disagrees with much of the literature discussed in Section 3.3.1, I should note that given the short time span considered by most previous studies and their focus on the transition from renting to homeownership (sometimes

not distinguishing between mortgaged and outright homeownership), such results are likely to refer to the transition to mortgaged homeownership and in this sense cannot be compared with my results on outright homeownership. There could be of course residual confounding responsible for the effect of outright homeownership observed, yet such confounding should be something with an onset that, on average, coincides with the transition to outright homeownership. Age is unlikely to be a confounder because its effect would be linear rather than discrete at a point of discontinuity. Socio-economic variables such as educational attainment do not have a clear theoretical connection to a discontinuity in reported mental health around the time of the tenure transition. Retirement is possibly the main event that could be intentionally timed to occur together with the conclusion of mortgage payments, though it is unclear whether this would happen systematically. While the dwelling is likely to change for the transition to mortgaged homeownership, the transition to outright homeownership does not, in most cases, involve a change in dwelling. This rules out that part of the material channel connecting tenure to mental health that has to do with housing quality, unless this happens to coincide with major renovations. What is left of the material channel discussed in Section 3.1 is therefore security in terms of risk of eviction and the financial relief linked to the end of the mortgage payments. There could be a case for the feelings of ontological security and the sense of status to be strengthened following the transition, yet the material channel of financial relief is intuitively the one that is most closely connected to a change in the trend of mental health right after the transition to outright homeownership. These results are therefore in line with Hypothesis 3.2.

TABLE 3.4. Event study of mental health for the transition to outright homeownership

	(1) TWFE	(2) BJS	(3) CS
Pretrend 10	0.0233	-0.0150	-0.0224
	(0.0191)	(0.0178)	(0.0356)
Pretrend 9	0.00741	-0.0295*	0.00579
	(0.0174)	(0.0171)	(0.0337)
Pretrend 8	0.0308*	-0.00429	0.0184
	(0.0166)	(0.0172)	(0.0307)
Pretrend 7	0.00419	-0.0275*	-0.0215
	(0.0144)	(0.0162)	(0.0278)
Pretrend 6	0.0119	-0.0186	0.0193
	(0.0135)	(0.0162)	(0.0243)
Pretrend 5	0.0102	-0.0193	-0.00881
	(0.0124)	(0.0160)	(0.0216)
Pretrend 4	0.00380	-0.0259	-0.0157
	(0.0118)	(0.0159)	(0.0205)
Pretrend 3	0.00530	-0.0244	-0.00433
	(0.0106)	(0.0161)	(0.0198)
Pretrend 2	0.000268	-0.0263*	-0.00537
	(0.00987)	(0.0160)	(0.0188)
Pretrend 1		-0.0192	0.00290
		(0.0162)	(0.0172)
ATT 0	-0.0396***	-0.0173**	-0.0216
	(0.00852)	(0.00797)	(0.0164)
ATT 1	-0.0386***	-0.0264***	-0.0351**
	(0.00882)	(0.00891)	(0.0169)
ATT 2	-0.0394***	-0.0346***	-0.0418**
	(0.00911)	(0.00961)	(0.0192)
ATT 3	-0.0377***	-0.0256**	-0.0343*
	(0.00940)	(0.0106)	(0.0204)
ATT 4	-0.0381***	-0.0526***	-0.0478**
	(0.00963)	(0.0113)	(0.0203)
ATT 5	-0.0456***	-0.0571***	-0.0597***
	(0.00999)	(0.0123)	(0.0223)
ATT 6	-0.0394***	-0.0300**	-0.0293
	(0.0103)	(0.0138)	(0.0268)
ATT 7	-0.0418***	-0.0440***	-0.0505*
	(0.0107)	(0.0151)	(0.0303)
ATT 8	-0.0611***	-0.0603***	-0.0614*
	(0.0110)	(0.0166)	(0.0316)
ATT 9	-0.0535***	-0.0568***	-0.0614*
	(0.0115)	(0.0184)	(0.0363)
ATT 10	-0.0568***	-0.0558***	-0.0511
	(0.0142)	(0.0186)	(0.0361)
N	601739	444742	432049

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

TABLE 3.5. Event study of mental health for the transition to outright homeownership, excluding renters

	(1) TWFE	(2) BJS	(3) CS
Pretrend 10	0.0358*	-0.0197	-0.0262
	(0.0194)	(0.0179)	(0.0366)
Pretrend 9	0.0187	-0.0356**	0.00127
	(0.0177)	(0.0172)	(0.0330)
Pretrend 8	0.0404**	-0.0112	0.0154
	(0.0168)	(0.0174)	(0.0314)
Pretrend 7	0.0125	-0.0353**	-0.0247
	(0.0146)	(0.0165)	(0.0284)
Pretrend 6	0.0183	-0.0284*	0.0160
	(0.0136)	(0.0167)	(0.0257)
Pretrend 5	0.0155	-0.0300*	-0.0118
	(0.0125)	(0.0166)	(0.0225)
Pretrend 4	0.00755	-0.0382**	-0.0188
	(0.0119)	(0.0165)	(0.0216)
Pretrend 3	0.00771	-0.0380**	-0.00719
	(0.0107)	(0.0168)	(0.0192)
Pretrend 2	0.00160	-0.0411**	-0.00830
	(0.00988)	(0.0168)	(0.0177)
Pretrend 1		-0.0356**	0.000128
		(0.0172)	(0.0170)
ATT 0	-0.0386***	-0.0207**	-0.0239
	(0.00855)	(0.00803)	(0.0153)
ATT 1	-0.0423***	-0.0301***	-0.0380**
	(0.00888)	(0.00901)	(0.0171)
ATT 2	-0.0419***	-0.0395***	-0.0447**
	(0.00922)	(0.00975)	(0.0183)
ATT 3	-0.0430***	-0.0312***	-0.0379*
	(0.00956)	(0.0108)	(0.0205)
ATT 4	-0.0457***	-0.0586***	-0.0522**
	(0.00986)	(0.0115)	(0.0209)
ATT 5	-0.0532***	-0.0641***	-0.0658**
	(0.0103)	(0.0125)	(0.0257)
ATT 6	-0.0491***	-0.0372***	-0.0376
	(0.0107)	(0.0140)	(0.0251)
ATT 7	-0.0512***	-0.0522***	-0.0592*
	(0.0111)	(0.0154)	(0.0307)
ATT 8	-0.0712***	-0.0689***	-0.0678**
	(0.0116)	(0.0169)	(0.0306)
ATT 9	-0.0643***	-0.0662***	-0.0659*
	(0.0121)	(0.0188)	(0.0355)
ATT 10	-0.0712***	-0.0659***	-0.0568*
	(0.0148)	(0.0191)	(0.0334)
N	450571	293574	288453

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

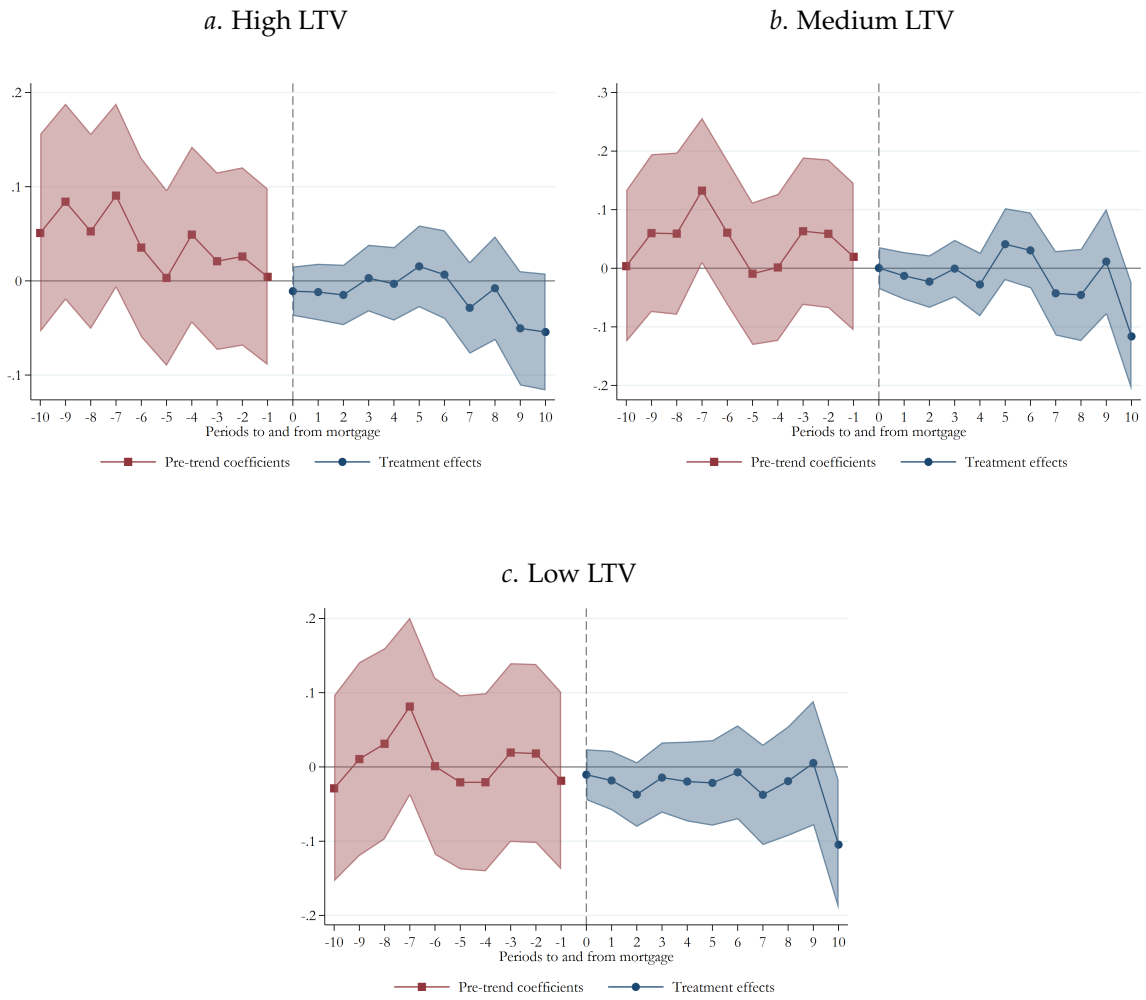
Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

3.3.3 Heterogeneity across degrees of indebtedness

The dynamic effect of tenure transition analysed in the previous sections is an average effect that is likely heterogeneous across individuals based on their characteristics. A salient one that varies based on tenure is the degree of indebtedness. To study this, I compute for each individual who held a mortgage at some point during the survey period their original Loan-to-Value ratio, as the total initial value of the mortgage divided by the total original value of the house, excluding as unreliable those individuals with a LTV greater than 1. I then divide these individuals in three categories: *high* LTV if the ratio is greater than 0.8, *medium* if the ratio is greater than 0.6 but lower or equal to 0.8, and *low* if the ratio is lower than or equal to 0.6. I repeat the event study, using only the BJS estimator, for each of these categories but leveraging the same group of never-treated individuals as counterfactual. Figure 3.5 shows the results for the transition to mortgaged homeownership. The results are broadly similar across degrees of indebtedness and resemble closely the average case shown in Panel *b* of Figure 3.2. The group whose pre-trends are closest to the counterfactual, as well as with the least pronounced post-event dynamics, is that of those with low LTV. These results bring further evidence that the transition to mortgaged homeownership does not substantially alter mental health and that this phenomenon is generalised in the sample across individuals with differing degrees of initial indebtedness. Interestingly, there is evidence in the literature that short-term debt or debt relating to consumer credit may be more relevant than mortgage debt for mental health because the latter is characterised by more sustainable conditions and by more agency (Hojman et al. (2016); Berger et al. (2016)), which may explain why mortgagors with different degrees of indebtedness show rather similar mental health trajectories. Figure 3.6 repeats the exercise for the transition to outright homeownership (the case where renters are excluded from the counterfactual is not reported for lack of convergence of the estimation due to the decreased sample size). As compared to the transition to a mortgage, in this case one can notice more heterogeneity across the LTV categories. While those with either high or low LTV

have systematically lower pre-trends as compared to the counterfactual, those with medium LTV have pre-trends closer to 0 and the clearest jump down in mental distress following the event of transition, though in contrast to the average event study in Panel *b* of Figure 3.3, the negative effect is reabsorbed about 6 waves post transition.

FIGURE 3.5. Event studies of mental health for the transition to mortgaged homeownership, by LTV

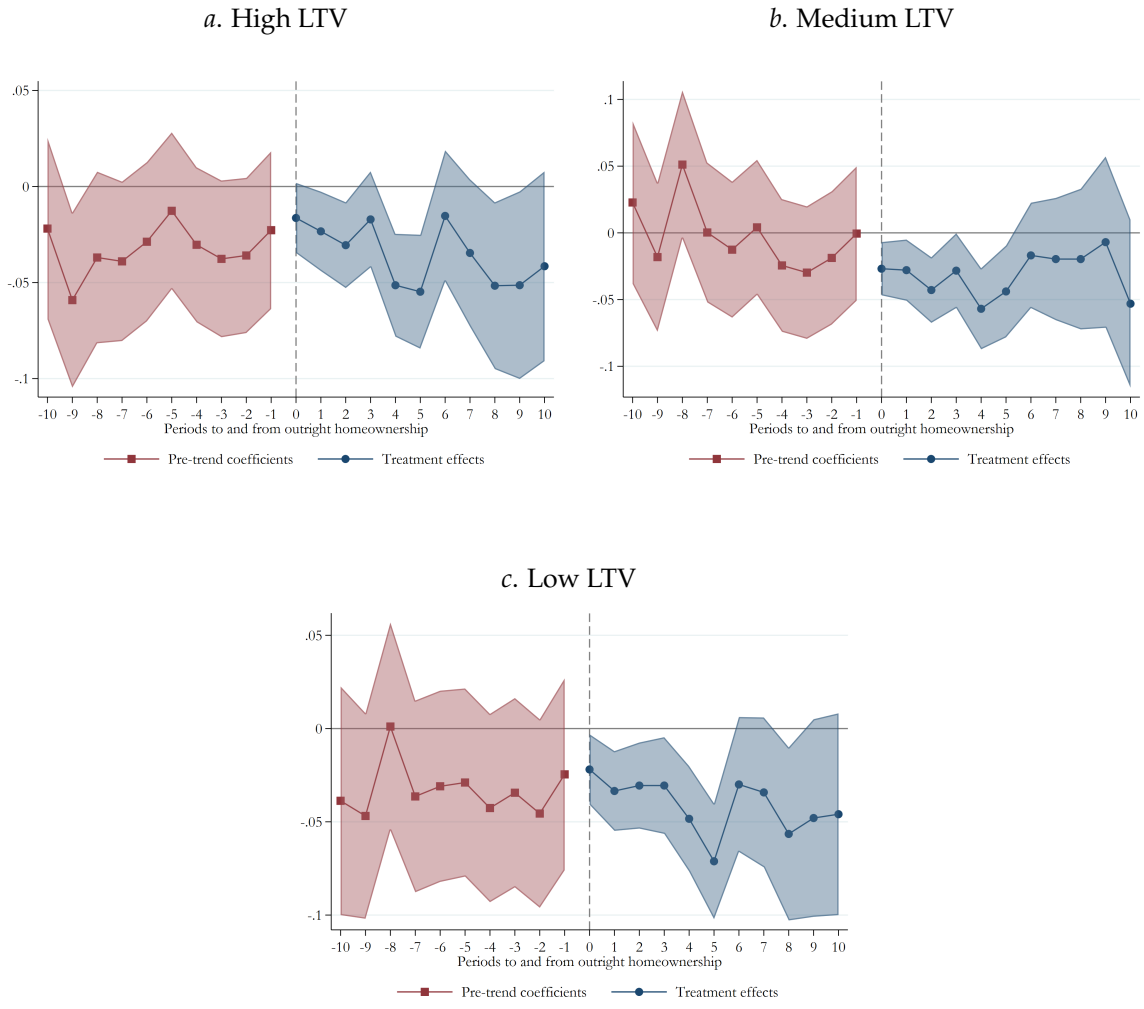


Note: Event studies in terms of obtaining a mortgage for individuals with high (Panel *a*), medium (Panel *b*) and low (Panel *c*) LTV ratios. All estimations follow the approach of Borusyak et al. (2022).

This suggests that those individuals with medium LTV are those driving the level decrease in mental distress that follows the transition to outright homeownership,

while for those with high and low LTV the change seems to happen in terms of the trend of treatment effects. Therefore, while the degree of indebtedness does not appear to be relevant when entering a state of debt (the transition to mortgaged homeownership), it affects mental health when exiting it (the transition to outright homeownership), as the end of the mortgage commitment is particularly felt among those whose LTV values at origination lie between 0.6 and 0.8.

FIGURE 3.6. Event studies of mental health for the transition to outright homeownership, by LTV



Note: Event studies in terms of becoming an outright homeowner for individuals with high (Panel *a*), medium (Panel *b*) and low (Panel *c*) LTV ratios. All estimations follow the approach of Borusyak et al. (2022).

3.4 Conclusion

This chapter took a long view on the relationship between housing tenure and mental health. While the existing literature largely focused on either cross-sectional or longitudinal results over short panels, I show that it is important to tease out how this relationship evolves dynamically over time, especially to appreciate the role for mental health of the transition to outright homeownership. To do this, I frame the tenure transition as an event study and leverage recent advances in the difference-in-differences with staggered adoption literature to identify the ATT of the tenure transition. My results, which estimate no change or possibly a worsening in mental health following the transition to mortgaged homeownership, are in line with the existing literature arguing that the fact that mortgagors tend to have better mental health than renters in the cross-section is likely compositional rather than causal. Conversely, my result that mental health improves following the transition to outright homeownership departs from the literature, primarily because the difference between mortgaged and outright homeownership was often overlooked in previous studies. Moreover, I illustrate how the results for the transition to mortgaged homeownership are largely the same across degrees of indebtedness, but how those with medium LTV ratios have a clearer decrease in mental distress upon transitioning to outright homeownership.

The main limitation of the empirical strategy is that the identification of the effect of the transition to homeownership does not rely on plausibly exogenous variation in the probability of transition to homeownership. This means that while it identifies the ATT for a large sample under the assumptions laid out in Section 1.5, it does not rule out the possibility that there exists something unknown arising with the same timing as the homeownership transition, which provokes a discontinuity in reported mental health *instead of* the tenure transition. The results of this paper would be strengthened if they were to be confirmed in a sample for which one can claim exogenous transition to homeownership. At the same time, such exogenous variation is both unlikely to occur and, if occurring, likely to affect a limited set of

individuals, so that a Local Average Treatment Effect (LATE) is estimated. In this context, a LATE could claim clearer attribution of the effect to the tenure transition, but at the expense of generalisability. An additional limitation concerns the frequency of the data. Since for each individual observations are collected on a yearly basis, one cannot detect potentially important fluctuations between waves, for instance if the effect of the tenure transition were to be very short lived and concentrated in the first few months after the transition. Those who change tenure may also have different histories of previous tenure transitions, and this may mean that the mental health of those that had multiple transitions may be affected differently from those for which there were no previous ones. Moreover, differences in the results across measures of mental health, namely the unhappy/depressed indicator and the 12-items GHQ, are a warning of the importance of both the definition of a concept and its measurement, and of the sensitivity of conclusions. The solutions to these limitations, that is estimation based on exogenous variation in housing tenure, the study of higher frequency data and of how histories of tenure transition may affect the results, as well as the testing of a wider range of definitions of mental health, are promising avenues for future research.

3.5 Housing, financial conditions and mental health during a pandemic

This additional section of the chapter zooms in on the pandemic period, analysing housing tenure, mental health and the channels connecting them. The COVID-19 pandemic and the policy responses connected to it have had a prominent role in people's lives since the first quarter of 2020. Predictably, this has had major impacts on mental health across the world (Le and Nguyen (2021); O'Connor et al. (2021); Kola et al. (2021); Farkhad and Albarracín (2021)). Lockdowns and social-distancing, in their different forms and implications, have as a consequence that individuals spend much more time than before at home. While the importance

for mental health of one's living space was significant in pre-pandemic times (see Krieger and Higgins (2002), Schulz and Northridge (2004), Bambra et al. (2010), Marmot et al. (2010), Braubach (2011)), it is likely that such importance has grown during the pandemic (Tunstall, 2021b). Moreover, the increased financial stress that some individuals have experienced due to negative shocks connected to the pandemic, such as employment loss or reduced income, potentially compounds the toll on mental health. Housing is a prominent feature in this respect too, since rent payments or mortgage loan repayments generally represent a sizeable part of household expenses and one that cannot easily be adjusted in times of decreased resources. In fact, the inclusion of housing costs can sizeably increase estimates of individuals in poverty (Tunstall et al., 2013). One's housing situation is also connected to the degree of digital exclusion, an additional axis of inequality made more apparent by the pandemic (Holmes and Burgess, 2022). In the first instance, this section focuses on documenting the differences in mental health outcomes over the pandemic period across four tenure types (outright homeowners, mortgagors, private renters and social housing renters) in the United Kingdom. As compared to the main section of this chapter, I further divide renters in private and social housing because, given the cross-sectional character of the analysis and the nature of the pandemic shock, such a split can provide supplemental variation for the identification of an effect. Additionally, this section zooms in on two further and related dimensions: the condition of falling behind with housing payments (as a renter or as a mortgagor) and the ability to access outdoor areas in general as well as specific ones (such as a balcony, a private garden or a shared garden). As discussed in Section 3.1, there are two main theoretical channels through which housing (including its financial aspects) may affect mental health: a material one, related to security of tenure and dwelling characteristics and an intangible one, related to ontological security and social status. This theoretical foundation allows to formulate testable predictions, for which I use the COVID-19 Study of Understanding Society, together with the whole time-series of Understanding Society, described already in Section 3.2. Using a series of difference-in-differences designs that differ partly from those discussed in

Section 1.5 and are therefore discussed more in detail in Section 3.5.2, Section 3.5.3 reports a number of insights. The headline result is that pre-pandemic gradients in mental health, measured as the indicator of unhappiness/depression, stayed largely unchanged during the pandemic. Apart from sizeable yet short-lived impacts, average mental health across all dimensions rose and fell in a synchronised manner, as the severity of the infection waves rose, fell and rose again. In Appendix B, using a wider definition of mental health spanning 11 additional dimensions (the already cited 12-items GHQ), I find qualitatively similar results as those estimated for the indicator of unhappiness/depression, with some patterns being more pronounced. While, in terms of ability to keep up with housing payments, government subsidies and moratoriums on evictions were probably responsible for preventing wedges in mental health to widen, the results on housing tenure and on outdoor spaces in particular suggest that pre-existing gradients in mental health are persistent and resistant to shocks affecting the material channel connecting housing tenure and mental health. One explanation behind this is that the intangible channel could be the main driver behind the structural differences in mental health across tenure groups. An alternative explanation is of course that there exists self-selection across tenure groups and that this correlates with mental health.

The material and the intangible dimensions of housing, discussed in Section 3.1, would lead us to believe that outright homeowners should do structurally better than mortgagors and renters, in mental health terms. Does this structural difference holds unchanged during the pandemic? Since this period has put additional pressure on the financial security of households because of loss of employment and income, my first hypothesis is that:

HYPOTHESIS 3.3. The gap in reported mental health between outright homeowners and the remaining tenure categories has increased as a result of the pandemic through increased financial pressure on mortgagors and renters.

An effect that would therefore occur through the material channel that connects housing to mental health. Tenure is also correlated with housing characteristics.

An important housing characteristic during the pandemic, arguably overlooked in pre-pandemic times, has been access to outdoor spaces. A second hypothesis is therefore:

HYPOTHESIS 3.4. The gap in reported mental health between those with and without access to outdoor spaces has increased as a result of the pandemic.

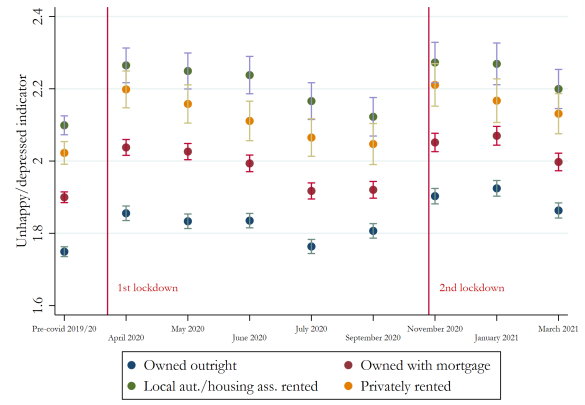
Conversely, the intangible channel is not as clearly affected by the pandemic. Ontological security and concerns over status in society should retain their structural characteristics and be largely unaffected in the short-term.

3.5.1 Understanding Society's COVID-19 Study

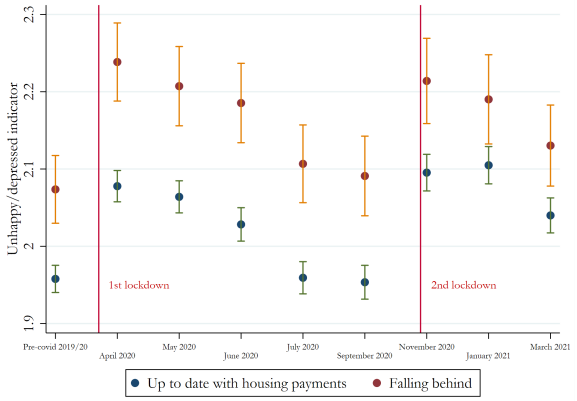
Understanding Society's COVID-19 Study is a longitudinal survey that started in April 2020 (University of Essex, Institute for Social and Economic Research, 2021a), following the beginning of the pandemic, as a continuation of the main Understanding Society survey (University of Essex, Institute for Social and Economic Research, 2021b), discussed in Section 3.2. The COVID-19 Study was collected monthly until July 2020 and then bimonthly, with the latest wave used in this study being March 2021. Responses were collected mostly through an online survey and only residually through a telephone survey; to maintain consistency in the mode of response during the pandemic period, I use the web-based survey only. The respondents across the different waves ranged from about 12,000 to about 18,000, depending on the wave. It is possible to link the COVID-19 Study to the main Understanding Society survey. Because of the covariates and the technique used in the estimation, the actual sample used ranges between about 128,000 and 338,000 observations, depending on the model, for about 32,500 unique individuals. The survey contains information on many individual and household characteristics, most notably providing information on reported mental health, housing tenure, ability to keep up with housing payments and access to outdoor space.

FIGURE 3.7. Evolution of reported mental health during the pandemic

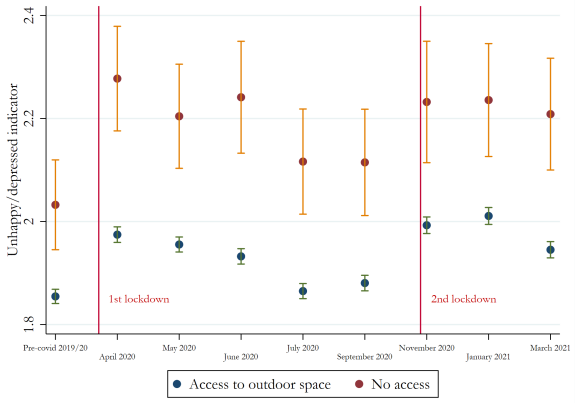
a. By housing tenure



b. By ability to keep up with housing payments



c. By access to outdoor space



Note: Trend in mean unhappy/depressed score by housing tenure (Panel *a*), ability to keep up with housing payments (Panel *b*) and access to outdoor space (Panel *c*). Pre-pandemic averages for 2019 and part of 2020 (until March) are included as a benchmark. The bars around the point estimates represent 95 % confidence intervals.

Figure 3.7 shows the average by survey period during the pandemic for the unhappy/depressed measure of mental health, separating in turn the four tenure groups, those with and without the ability to keep up with housing payments, and those with and without access to outdoor spaces. To match the sample in the estimation model, individuals are assigned, for each dimension, to only one category for the entirety of the analysis period. To assign the housing tenure category, I use

information from the last wave before the pandemic and, if that is missing, from the first period closest to it that is available. This means that if someone is an outright homeowner in the wave before the pandemic, they will be categorised as homeowner for the whole time-series. To assign a category for the ability to keep up with housing payments, I divide individuals into those who did not experience problems being up to date with housing payments at any point during the pandemic period, and those that experienced them at least once. Therefore, if an individual replies that is not able to keep up with housing payments in May 2020 only, they will be categorised as such also in the rest of the time-series. Finally, I use April 2020 or the first next pandemic period with available data to assign an individual to their category of access to outdoor space, since this information is available in a consistent manner only for the pandemic survey modules. This kind of assignment is useful for estimation but of course loses some of the dynamics as individuals transition from one category to the other during the pandemic period, chiefly in terms of ability to keep up with housing payments. This is less of an issue for housing tenure and access to outdoor space, since they are categories that tend to be persistent over short periods of time. The first period in the charts, termed "Pre-covid 2019/20", is the average in the last round of survey of the pre-covid Understanding Society. There are then eight more periods from the pandemic module. These descriptive patterns suggest, in the first instance, that there existed an ordering in reported mental health across the different dimensions before the pandemic period, and that such an ordering was qualitatively unchanged in the pandemic period. For instance, outright homeowners reported the lowest mental distress before the pandemic as well as throughout it, and those who remained up to date with housing payments throughout the pandemic reported in fact a lower mental distress, as compared to those who would have fallen behind with housing payments at some point between April 2020 and March 2021, even before the pandemic started. Moreover, there is a high degree of synchronisation in how the mental health of the different groups was affected during the pandemic. April 2020 saw an increase in distress as compared to pre-pandemic levels, which then decreased over the following months for all groups, to then pick up again only

in November 2020. The observed pattern is very similar also when employing the 12-items GHQ as a measure of mental health (see Figure B.5 in Appendix B). Earlier studies on mental health and subjective well-being during the pandemic have found a similar overall pattern starting from April 2020. Foa et al. (2022) build a Negative Affect Index from YouGov polling data and a Negative Affect Search Index from Google Trends for the United Kingdom, highlighting a spike in both around April 2020 as compared to the pre-pandemic period, and then a fall until the indices start growing again in Autumn 2020. Daly and Robinson (2021), using the same Understanding Society COVID module used in this study, also document an increase in psychological distress measured using the 12-items GHQ following the onset of the pandemic, that declines after April 2020 to reach pre-pandemic levels by September 2020. They remark how this pattern is also largely the same across a set of demographic dimensions (age, gender, ethnicity and income). Akay (2022) documents instead how at the intensive margin, pandemic severity measured as local (UK) and global reported cases affected more, in terms of the 12-items GHQ, those who do not own their home as well as those without access to a private garden. These results are also broadly consistent with the cross-sectional ordering in reported mental health across tenure and access to outdoor space that I document here, although I study the extensive margin (before and after the pandemic) rather than the intensive margin (pandemic severity).

Table 3.6 provides summary statistics for the unhappy/depressed indicator and for relevant covariates, by, in turn, housing tenure, ability to keep up with housing payments and access to outdoor spaces. Within each category, the mean for the unhappy/depressed indicator follows the same ordering as in Figure 3.7, both for the pre-pandemic and for the pandemic period. Private renters, mortgagors, those without access to an outdoor space and both those who can and cannot keep up with housing payments have a similar mean age between 40 and 43 years, then come social renters and those with access to outdoor space, between 47 and 52 years and finally outright homeowners at about 60. The share of female individuals is highest among social renters (63%), between 59% and 60% among private renters,

among those up to date and not up to date with housing payments and among those who have access to outdoor spaces, while between 54% and 56% for those without access to outdoor spaces, for outright homeowners and for mortgagors. The share of individuals with degree level education is highest among mortgagors and those up to date with housing payments (between 53% and 54%), followed by both those with and without access to an outdoor space at about 50%, then private renters, outright homeowners and those not up to date with housing payments, between 41% and 45%, followed by social renters at 20%. Next, I outline the identification strategy to verify whether the patterns described in this section hold also in a difference-in-differences design.

TABLE 3.6. Overview of summary statistics, by category

	Outright homeowners		Mortgagors		Public renters		Private renters	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Pre-pandemic unhappy/depressed score	1.71	0.76	1.83	0.79	2.06	0.91	1.97	0.85
Pandemic unhappy/depressed score	1.85	0.80	2.00	0.82	2.23	0.90	2.14	0.86
Age	60.50	14.67	41.98	12.71	47.41	17.33	40.55	16.17
Share female (%)	55.51	49.70	56.45	49.58	63.06	48.26	59.64	49.06
Share with a degree (%)	42.19	49.39	53.79	49.86	20.44	40.33	40.81	49.15
Observations	149110		120707		41485		26369	

	Up to date with housing payments		Not up to date	
	Mean	S.D.	Mean	S.D.
Pre-pandemic unhappy/depressed score	1.89	0.82	1.99	0.86
Pandemic unhappy/depressed score	2.04	0.84	2.17	0.87
Age	43.17	13.54	41.86	12.79
Share female (%)	60.60	48.86	59.50	49.09
Share with a degree (%)	52.73	49.93	45.32	49.78
Observations	109326		19069	

	Access to outdoor space		No access	
	Mean	S.D.	Mean	S.D.
Pre-pandemic unhappy/depressed score	1.80	0.79	2.01	0.87
Pandemic unhappy/depressed score	1.94	0.82	2.21	0.91
Age	52.08	15.58	41.73	15.89
Share female (%)	58.88	49.21	53.60	49.88
Share with a degree (%)	50.55	50.00	50.44	50.00
Observations	195157		4754	

Note: Summary statistics for the unhappy/depressed indicator and additional relevant variables by housing tenure, ability to keep up with housing payments and access to outdoor space.

3.5.2 Additional empirical considerations

I exploit the interaction between the onset of the pandemic and a set of housing conditions to identify effects on reported mental health. The first housing condition is tenure. Being a homeowner for instance, as compared to a renter, is of course an endogenous assignment and one that by design remains static, yet the interaction with the onset of the pandemic can be used as a source of variation to study whether the cross-sectional difference across tenures changed during the pandemic period, as compared to the pre-pandemic one, under the assumption of parallel trends across tenure types in the absence of the pandemic. Another housing condition is falling behind with housing payments after the onset of the pandemic, which can occur to both renters and mortgagors. There is then a series of conditions relative to access to outdoor spaces during the pandemic: whether there exists an outdoor space at all, whether the person can access a balcony, whether the person can access a private garden and whether the person can access a shared garden. For each of these, I recover the dynamic Average Treatment effect on the Treated (ATT), that is the ATT by period after the onset of the pandemic. Since the housing conditions an individual is assigned to are static indicators, and longitudinal variation comes only from the interaction with a time dummy representing the onset of the pandemic, using the term "treatment" is conceptually questionable. Nevertheless, I retain it in the case of the ATT for ease of exposition and to make apparent the connections to the mechanics of estimations. Specifically, I use the imputation representation of the efficient estimator as described in Borusyak et al. (2022), implemented in Borusyak (2021) and discussed for the main setting of staggered adoption in Section 1.5. While this estimator is designed to remedy the problems arising precisely in difference-in-differences set ups with staggered adoption (which is not the case here), I chose to use it in this case too for its intuitive estimation and aggregation of individual effects. Moreover, Borusyak et al. (2022) provide a framework to separate the testing of assumptions from model estimation, which I will use later. Given this setup, units cannot exit and then re-enter their housing condition (e.g. changing tenure) during the pandemic period unless one additionally assumes that changing housing

condition does not have an effect, which is a rather restrictive assumption. Therefore, for each housing condition, each individual is assigned to a single category for the whole time-series of observations, as discussed more in detail in Section 4.2. In this case too, the estimation is composed of three steps, that I will discuss again because of some variations as compared to the general model in Section 1.5. First, the estimation of a model of counterfactual outcomes excluding the specific category of interest (e.g. outright homeowners when looking at housing tenure) after the onset of the pandemic:

$$UD_{it}(0) = \gamma_g + \lambda_t + \beta_a Age_{it} + \beta_g Gender_{it} + \beta_h HousingTenure_{it} + \beta_q Qualification_{it} + \epsilon_{it} \tag{3.1}$$

Where UD_{it} is the unhappy/depressed indicator at the individual level ($UD_{it}(0)$ is the counterfactual in the absence of the pandemic), γ_g is the dummy indicating whether the observation belongs or not to the category of interest (e.g. outright homeowners when looking at housing tenure), λ_t time fixed effects, β_a the coefficient on the interval variable Age_{it}, β_g is the coefficient on the binary variable $Gender_{it}$ (which in the survey takes values "Female" and "Male"), β_h is the coefficient on the nominal variable $HousingTenure_{it}$ (composed of four groups), β_q is the coefficient on the highest qualification achieved (a nominal variable with six categories) and ϵ_{it} is the error term. Age and gender are included as the two main exogenous, demographic variables that may affect mental health. Age has been recognised to bear a close relationship with major depression (Kessler et al., 2019) and with subjective well-being in general (Steptoe et al. (2015); Blanchflower and Graham (2020)), while gender plays a role in the kind of mental health issues experienced (Rosenfield and Mouzon, 2013). Moreover, policy responses to the pandemic may have impacted mental health based on age and gender (García-Prado et al., 2022). Education is instead something acquired early in life that can affect heavily career trajectories, income and geographical mobility and therefore likely playing a role for

mental health too, not necessarily in a positive way (Avendano et al., 2020). Finally, housing tenure is included as a control in addition to the indicator of assignment to the housing condition because the latter is binary (e.g. outright homeowners as compared to the rest) and, by design, static for the whole time-series, while the tenure variable is allowed to vary over time and can take all four values (outright homeowners, mortgagors, social renters and private renters). Including the time-varying tenure variable therefore conveys additional useful information to build the counterfactual. To give an example, someone who became an outright homeowner in 2019 and was still an outright homeowner in April 2020 (the first month of the COVID module), will be considered, for the purpose of the indicator of assignment to the housing condition, as belonging to the outright homeowner group for the whole time-series. At the same time though, including the four-categories tenure variable as a control provides extra information about the person being a mortgagor until 2019 in the estimation of the counterfactual in equation 3.1. The second step in the procedure recovers estimates of individual effects τ_{it} as the difference between the observed outcome for UD_{it} and the imputed counterfactual from 3.1:

$$\hat{\tau}_{it} = UD_{it} - \widehat{UD}_{it}(0) \tag{3.2}$$

Finally, the third step aggregates the invididual $\hat{\tau}_{it}$ into horizon-specific ATTs:

$$\widehat{ATT}_h = \sum_{i=1}^{N_h} \frac{1}{N_h} \hat{\tau}_{ih} \tag{3.3}$$

Where N_h is the number of observations for each horizon h, a specific value of t expressed in terms of h periods from the onset of the pandemic (April 2020 in this sample). It is important to remark that t has different frequencies before and after the start of the pandemic period. Before April 2020, each period t is a wave overlapping more than one calendar year but, from the perspective of the individual, having a yearly cadence. As an example, wave 9 of Understanding Society was

collected between 2017 and 2019 while wave 10 between 2018 and 2020, yet for a same individual the gap in their response between wave 9 and wave 10 will still be about 12 months. Starting from April 2020 and until July 2020 though, each period t is a month, while from July 2020 until March 2021 each period t corresponds to two months. The ability to recover the ATTs rests on two assumptions:

(1) *Parallel trends*

It is assumed that in the absence of the pandemic, the trends in the unhappy/depressed indicator would have been the same between the category of interest (e.g. outright homeowners when looking at housing tenure) and the remaining categories. This assumption is equivalent to equation 3.1. Although this cannot be tested directly, I will test whether pre-trends are parallel through the placebo test suggested in Borusyak et al. (2022), which mitigates the problems highlighted in Roth (2022). This is done as a placebo test on the same observations used in equation 3.1 with the model:

$$\begin{aligned} UD_{it} = \gamma_g + \lambda_t + \widetilde{\beta}_a Age_{it} + \widetilde{\beta}_g Gender_{it} + \widetilde{\beta}_h HousingTenure_{it} \\ + \widetilde{\beta}_q Qualification_{it} + W_{it}^{'}\Gamma + \widetilde{\epsilon}_{it} \end{aligned} \tag{3.4}$$

and testing $\Gamma = 0$. $W_{it}^{'}$ are indicators for pre-pandemic periods.

(2) *No anticipation*

It is further assumed that individuals do not react to the pandemic before the onset of the pandemic, or:

$$UD_{it} = UD_{it}(0) \tag{3.5}$$

For all individuals before the onset of the pandemic. Although news of the spread of the Coronavirus were available before April 2020, it is unlikely that individuals could gauge the implications of the pandemic and of the connected policy responses. I include March 2020 as the last month of survey for the pre-pandemic period. The assumption of no anticipation can

equally be tested by relying on 3.4 and looking at individual pre-pandemic coefficients.

Since it is expected there to be serial correlation in the responses of a same individual over time, standard errors are clustered throughout at the individual level.

3.5.3 Results and discussion

I discuss in turn the results of the difference-in-differences models starting from housing tenure, to then move on to being up to date with housing payments and finally to access to outdoor spaces.

Housing tenure

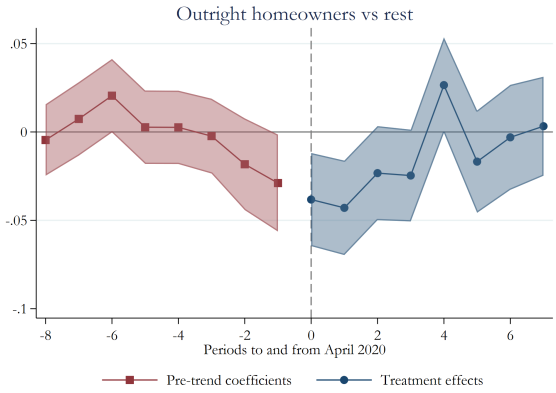
Panel *a* of Figure 3.8 shows both the tests on pre-trends (in red) and the dynamic coefficient estimates (in blue) for the model where the category of interest is that of outright homeowners, as compared to the aggregate of all other three tenures, during the pandemic. The pre-trends coefficients show signs of anticipation effects as the pre-trends for the two periods before the beginning of the pandemic time have a declining trend in line with the negative coefficients during the first months of the pandemic period (see also column 1 of Table 3.7). Indeed, the joint test of significance for the pre-trends has a p-value of 0.089, providing evidence against the parallel trends assumption too. Starting from April 2020, there appears to be a decrease of about 0.04 points in the unhappy/depressed indicator for outright homeowners as compared to the counterfactual. The effect persists in May 2020 to then converge back to 0 by June 2020 and remains so thereafter. Because there is evidence against the no-anticipation and the parallel trends assumptions, the effect observed seems to pre-date the onset of the pandemic. Panel *b* of Figure 3.8 repeats the exercise for the mortgagors group as compared to the other three tenure groups. In this case too, the pre-trends show signs of anticipation two periods before the event, with a coefficient between 0.04 and 0.05, though the p-value of the joint test of significance of all pre-trends is 0.152. In the pandemic period, the unhappy/depressed indicator for mortgagors as compared to the other tenure groups mirrors (in the opposite direction) somewhat the result for outright homeowners, as there exists a positive effect for the first two periods of the pandemic between 0.05 and 0.06. The actual estimates for this group are found in column 2 of Table 3.7. Panel *c* of Figure 3.8 compares social renters (from local authority or housing associations) to the remaining tenure categories. In

this case, the pre-trends have a systematically negative coefficient, which is clearly identified though for pre-trend 6 and 4 only. The joint test for significance has a p-value of 0.343. The coefficients in the pandemic period rise above pre-pandemic levels between April and July 2020 but with standard errors large enough to make them undistinguishable from 0 (see also column 3 of Table 3.7). The absence of a detectable effect on the mental health of social renters can be thought of also as a placebo test on whether possible effects that we see for outright homeowners and mortgagors are driven by reasons unrelated to housing but that correlate with it, such as a higher probability of non-housing related financial distress. In fact, if mortgagors were to experience non housing-related financial distress, it is likely that social renters should experience this to an even greater extent, being substantially more financially fragile than mortgagors. Finally, Panel *d* of Figure 3.8 shows that for private renters the unhappy/depressed indicator decreased by a substantial amount, between 0.07 and 0.12 in April and May 2020, to then converge back to pre-pandemic levels. To give a sense of magnitude, 0.12 is just short of the difference between the pre-pandemic mean unhappy/depressed score between mortgagors and private renters (0.14). There is also strong evidence in favour of the parallel trends and no anticipation assumptions since all pre-trends are individually very close to 0 and their joint test has a p-value of 0.942 (see also column 4 of Table 3.7). This is therefore the clearest effect identified in the sample, whereby upon the onset of the pandemic, private renters experienced a substantial decrease in mental distress as compared to the other tenure groups, or in other words, their mental health worsened less than proportionally as compared to the other tenure groups, though the effect is short-lived. When repeating the analysis on the 12-items GHQ (see Appendix B), I find more pronounced effects for the outright homeowners group and for the mortgagors group, but also more pronounced violation of the parallel trends and no anticipation assumptions. These estimates could indicate a structural increase in the gap between outright homeowners and mortgagors, but if that is the case it pre-dates the pandemic period. The results for the social and private renters groups are instead very similar to those obtained for the simple unhappy/depressed indicator. Using

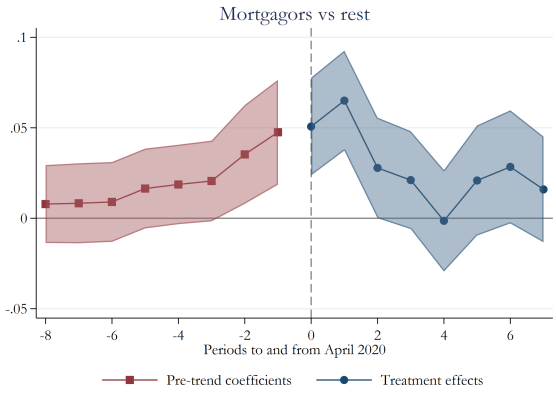
US data between April and November 2020 for a composite measure of mental health derived from survey questions on feeling down, experiencing axiety or worrying and losing interest, Ghimire et al. (2021) find also that outright homeowners fared better than mortgagors and renters in mental health terms during the pandemic, though they do not use a difference-in-differences approach and do not compare to pre-pandemic levels. As discussed earlier in the chapter, the pre-pandemic literature had longed highlighted the existence of a cross-sectional gradient in mental health based on housing tenure. Park and Seo (2020) find, using Korean data for low-income households, that renters tend to be about 1.5 times more likely to experience depressive symptoms than homeowners among a sample that lived in dwellings deemed adequate in terms of floor space, sanitary facilities and building services. Ellaway et al. (2016), using Scottish data, report that being a social renter as compared to a homeowner is associated to about 0.05 η^2 (a measure of effect size) more on their anxiety scale and 0.03 η^2 more on their depression scale. Other studies, which have highlighted how the relationship between mental health and housing tenure generally disappears longitudinally, also provide evidence that the cross-sectional relationship exists: Pierse et al. (2016), using data from New Zealand, show that renters have psychological distress (measured on the Kessler-10 scale) about 0.9 points higher than homeowners. Using Australian data instead, and the Mental Component Summary (MCS) as the outcome variable, Baker et al. (2013) find that social renters have on average a worse MCS by about 3.5 points as compared to homeowners, and private renters have a worse MCS by about 1 point as compared to homeowners. My results show that the cross-sectional gradient identified in these studies holds qualitatively true in terms of relative ordering (outright homeowners faring best and renters worst) throughout the pandemic in the UK and, apart from short-lived exceptions, it remains quantitatively unchanged too.

FIGURE 3.8. Event studies of mental health for the onset of the pandemic, by housing tenure

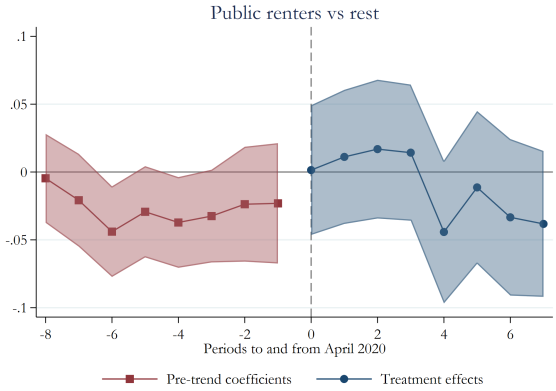
a. Outright homeowners VS rest



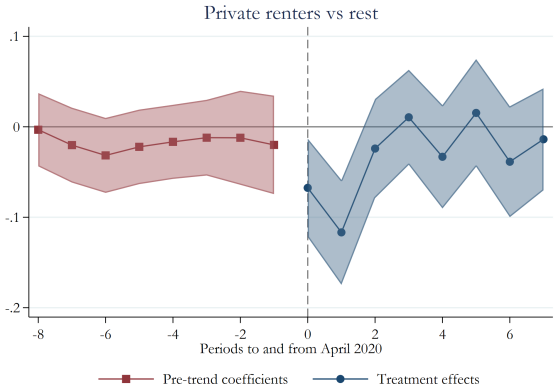
b. Mortgagors VS rest



c. Social renters VS rest



d. Private renters VS rest



Note: Event study comparing each housing tenure category during the pandemic to the aggregate of the other tenure groups, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Shaded areas around the coefficients represent 95% confidence intervals. The p-value for the joint test of no pre-trends is equal to 0.089 for outright homeowners, to 0.152 for mortgagors, to 0.343 for social renters and to 0.942 for private renters.

TABLE 3.7. Event studies by housing tenure

	(1) Outright homeowners	(2) Mortgagors	(3) Public renters	(4) Private renters
April 2020	-0.0382***	0.0507***	0.00143	-0.0675**
	(0.0134)	(0.0138)	(0.0244)	(0.0278)
May 2020	-0.0429***	0.0650***	0.0111	-0.117***
	(0.0136)	(0.0141)	(0.0252)	(0.0296)
June 2020	-0.0233*	0.0278**	0.0169	-0.0240
	(0.0136)	(0.0142)	(0.0261)	(0.0280)
July 2020	-0.0247*	0.0211	0.0143	0.0105
	(0.0133)	(0.0138)	(0.0256)	(0.0268)
September 2020	0.0265*	-0.00141	-0.0442*	-0.0332
	(0.0138)	(0.0143)	(0.0269)	(0.0292)
November 2020	-0.0168	0.0209	-0.0113	0.0153
	(0.0148)	(0.0155)	(0.0287)	(0.0303)
January 2021	-0.00302	0.0284*	-0.0334	-0.0386
	(0.0152)	(0.0160)	(0.0294)	(0.0313)
March 2021	0.00326	0.0159	-0.0382	-0.0138
	(0.0143)	(0.0149)	(0.0274)	(0.0288)
Pre-trend 1	-0.0289**	0.0475***	-0.0231	-0.0200
	(0.0140)	(0.0148)	(0.0226)	(0.0278)
Pre-trend 2	-0.0183	0.0353**	-0.0237	-0.0121
	(0.0132)	(0.0140)	(0.0216)	(0.0265)
Pre-trend 3	-0.00234	0.0206*	-0.0325*	-0.0120
	(0.0108)	(0.0114)	(0.0174)	(0.0214)
Pre-trend 4	0.00261	0.0187*	-0.0372**	-0.0166
	(0.0106)	(0.0112)	(0.0171)	(0.0209)
Pre-trend 5	0.00270	0.0164	-0.0293*	-0.0222
	(0.0106)	(0.0113)	(0.0172)	(0.0210)
Pre-trend 6	0.0205*	0.00902	-0.0440***	-0.0316
	(0.0106)	(0.0113)	(0.0170)	(0.0212)
Pre-trend 7	0.00739	0.00827	-0.0208	-0.0203
	(0.0106)	(0.0113)	(0.0175)	(0.0211)
Pre-trend 8	-0.00455	0.00785	-0.00463	-0.00322
	(0.0103)	(0.0110)	(0.0167)	(0.0207)
Observations	337671	337671	337671	337671

Note: Event studies comparing each housing tenure category to the aggregate of the other three groups, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Standard errors in parentheses, clustered at the individual level over time. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Up to date with housing payments

Next, I compare individuals who declare to be behind in housing payments (rent or mortgage payments) to those who do not. Arrears in housing payments spurred by

the pandemic were substantial in the UK. Judge (2021) estimates that they reached 9% for social renters, 6% for private renters and 2% for mortgagors in January 2021, about twice as much as the corresponding estimates of pre-pandemic levels, and previous estimates are even larger for mortgagors and social renters (MHCLG (2020); Baxter-Clow et al. (2020)). Given such financial distress imposed on households by the pandemic, one could expect the difference in reported mental health between those running into arrears and those who do not to increase. In fact, studies on these same data that look at the effect of financial distress on mental health during the pandemic have generally found that higher financial distress is associated with worse mental health in the pandemic period (Chandola et al. (2020); Pierce et al. (2021); Ellwardt and Präg (2021)). Figure 3.9 shows the event study where the category of interest is that falling behind with housing payments in the pandemic period. One can notice how pre-trends are not significantly different from 0, with a p-value on their joint test equal to 0.569. Although the standard errors are large and most coefficients cannot be clearly distinguished from 0, those among mortgagors and renters who could not keep up with housing payments in at least one of the survey periods had a higher unhappy/depressed score in the first months of the pandemic as compared to the counterfactual outcome, but this difference converged slowly closer to 0 as time passed. The estimates are comparable to those observed for outright homeowners and mortgagors, but smaller than those for private renters (see Table 3.8): about 0.05 in April and June 2020. The results go qualitatively in the same direction but are substantially more pronounced when using the 12-items GHQ as the measure of mental health (see Figure B.7 in Appendix B). It appears therefore that the additional financial distress provoked by the pandemic had a substantial yet also short-lived effect in widening the gap between those with and without housing payment arrears, as compared to pre-pandemic levels. Below, I summarise relevant government policy aimed at alleviating the financial burden since the onset of the pandemic.

Relevant UK government response. The UK government took a number of policy measures that had consequences for the ability to sustain housing payments[3]. For instance, in England starting 26 March 2020 until 28 August 2020, eviction notices to social and private renters (with some exceptions) were extended to 3 months (normally, they vary between 2 weeks and 1 month). From 29 August 2020 until 31 May 2021 this was further extended to 6 months. It was then decreased to 4 months starting 1 June 2021 and 2 months from 1 August 2021. From 17 November 2020 to 31 May 2021 lender repossession actions for mortgages were suspended and mortgagors could defer up to six monthly payments for payments due no later than July 2021[4]. There is then indirect support with housing costs through the Coronavirus Job Retention Scheme, financial support for the self-employed and Universal Credit, which started already in March 2020. The combination of all these measures had likely a role in preventing the wedge in mental health between those with and without housing payments problems to widen further, even though it cannot remove completely the financial burden that households experienced. In fact, a recent study on the same data I use suggests that furloughed individuals were still 30% more likely to be late on housing payments as compared to those who did not experience similar income reductions (Görtz et al., 2021).

[3]These can be found for England at the link: https://www.gov.uk/government/publications/covid-19-and-renting-guidance-for-landlords-tenants-and-local-authorities/coronavirus-covid-19-guidance-for-landlords-and-tenants

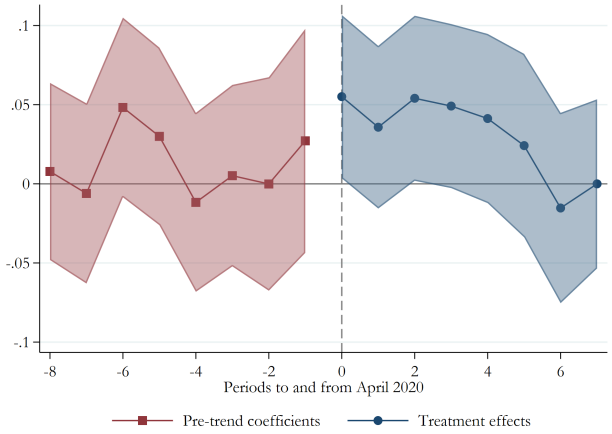
[4]More information at: https://www.fca.org.uk/publication/finalised-guidance/mortgages-coronavirus-tailored-support-guidance.pdf

TABLE 3.8. Event studies by ability to meet housing payments

	(1) Not up to date with housing payments
April 2020	0.0551**
	(0.0263)
May 2020	0.0358
	(0.0262)
June 2020	0.0540**
	(0.0266)
July 2020	0.0491*
	(0.0264)
September 2020	0.0412
	(0.0273)
November 2020	0.0242
	(0.0296)
January 2021	-0.0153
	(0.0307)
March 2021	-0.0000226
	(0.0273)
Pre-trend 1	0.0271
	(0.0361)
Pre-trend 2	-0.0000931
	(0.0344)
Pre-trend 3	0.00515
	(0.0293)
Pre-trend 4	-0.0117
	(0.0288)
Pre-trend 5	0.0300
	(0.0286)
Pre-trend 6	0.0483*
	(0.0290)
Pre-trend 7	-0.00609
	(0.0290)
Pre-trend 8	0.00782
	(0.0286)
Observations	128395

Note: Event studies comparing those who cannot keep up with housing payments to those who can, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence. Standard errors in parentheses, clustered at the individual level over time. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 3.9. Event study of mental health for the onset of the pandemic, by ability to keep up with housing payments



Note: Event study comparing those who cannot keep up with housing payments to those who can during the pandemic, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence. Shaded areas around the coefficients represent 95 % confidence intervals. The p-value for the joint test of no pre-trends is equal to 0.569.

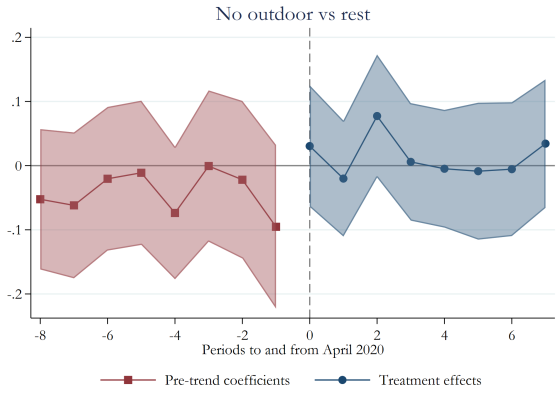
Access to outdoor space

A last aspect of housing that I investigate is access to outdoor spaces. This feature has been, intuitively, especially important in pandemic times as individuals were spending much more time than usual at home. I therefore compare those with general access to outdoor space to those without, as well as zooming in on specific outdoor spaces: a balcony, a private garden and a shared garden. Panel *a* of Figure 3.10 shows the event study set up where the category of interest is that without access to an outdoor space in pandemic times, as compared to having access to some form of it. The test on pre-trends provides evidence in favour of the parallel trends and no anticipation assumptions, but the effects' coefficients, despite being sizeable in terms of point estimates (around 0.08 by June 2020), have large standard errors and cannot be distinguished from 0. A clearly identified effect of absence of outdoor spaces in the first months of the pandemic is found instead using the 12-items GHQ in Panel *a* of Figure B.8 of Appendix B. Other studies have found linkages between mental health and access to outdoor spaces during the pandemic. Amerio et al. (2020), using a cross-sectional sample of undergraduate students from Italy surveyed in April 2020, find that absence of a livable balcony or garden is found for a larger share among those reporting moderate–severe or severe depressive symptoms (operationalised as a 9-item Patient Health Questionnaire score equal or above 15), with the association weakening in significance once accounting for age and gender. Hubbard et al. (2021), using a cross-sectional sample for Scotland, detect that psychological distress (measured using the 4-item Patient Health Questionnaire) is worse for individuals with a shared outside space or with no outside space, as compared to individuals with private residential outside space (such as a garden, a balcony or a patio). Panel *b* of Figure 3.10 refers to those with access to a balcony as category of interest. Although the unhappy/depressed indicator appears to decline at the very beginning of the pandemic period, the standard error around the decline makes it indistinguishable from 0. A similar pattern is found using the 12-items GHQ in Panel *b* of Figure B.8, except that by November 2020 there appears to be an increase in the score for those with access to a balcony. Panel *c* of Figure 3.10

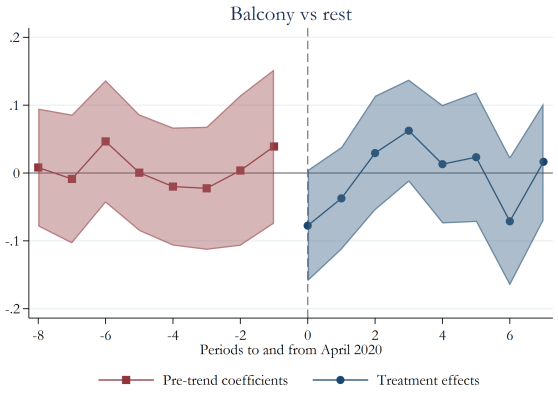
shows how the presence of a private garden in pandemic times (as compared to its absence, including the presence of alternative outdoor spaces) is estimated to have increased the unhappy/depressed indicator by about 0.05 in April 2020, though the rest of the coefficients cannot be distinguished from 0 and the pre-trends, despite not being individually and collectively different from 0, show sign of anticipation in their point estimates. Finally, Panel *d* of Figure 3.10 shows that the presence of a shared garden seems to have decreased the unhappy/depressed indicator in April 2020, as compared to pre-pandemic times, by a sizeable amount (0.12 points) but the dynamic effect is soon back to being statistically indistinguishable from 0 in May 2020. The latter two show both no effect when estimated on the 12-items GHQ (see Panels *c* and *d* of Figure B.8 in Appendix B). These results are in contrast with Hubbard et al. (2021), which found shared residential outdoor spaces to be associated with worse psychological distress, as compared to private ones. A possible reason for a positive effect of shared spaces, in contrast to private ones, could be that they allow individuals to interact and this could be beneficial to mental health in a period of otherwise isolation. A mechanism with the opposite effect would instead be the additional anxiety that a shared space could generate with respect to an increased probability of contagion.

FIGURE 3.10. Event studies of mental health for the onset of the pandemic, by access to outdoor space

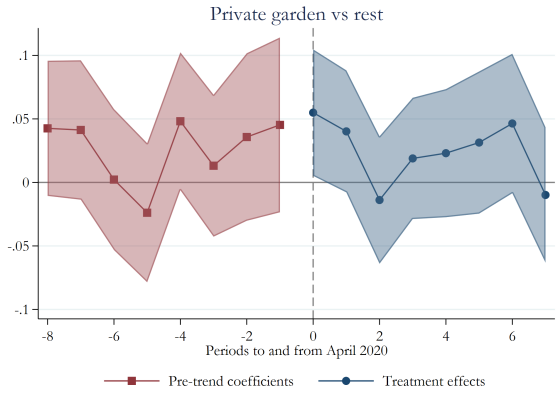
a. No outdoor VS rest



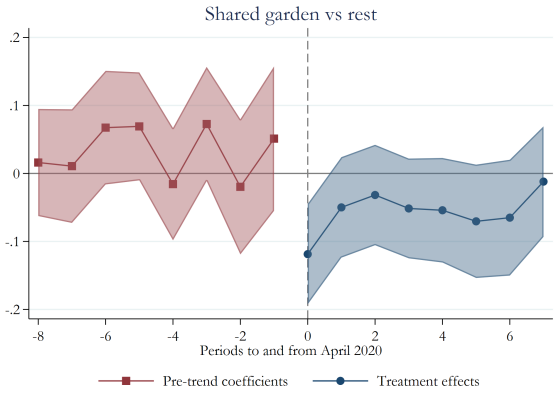
b. Balcony VS rest



c. Private garden VS rest



d. Shared garden VS rest



Note: Event study comparing each outdoor space category during the pandemic to the aggregate of the others, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Shaded areas around the coefficients represent 95 % confidence intervals. The p-value for the joint test of no pre-trends is equal to 0.555 for those without outdoor space, to 0.906 for those with a balcony, to 0.145 for those with a private garden and to 0.220 for those with a shared garden.

TABLE 3.9. Event studies by access to outdoor space

	(1) No outdoor space	(2) Balcony	(3) Private garden	(4) Shared garden
April 2020	0.0305	-0.0776*	0.0549**	-0.119***
	(0.0484)	(0.0418)	(0.0254)	(0.0377)
May 2020	-0.0201	-0.0374	0.0402	-0.0499
	(0.0461)	(0.0387)	(0.0245)	(0.0377)
June 2020	0.0774	0.0294	-0.0138	-0.0317
	(0.0489)	(0.0431)	(0.0256)	(0.0377)
July 2020	0.00588	0.0624	0.0189	-0.0515
	(0.0467)	(0.0385)	(0.0244)	(0.0374)
September 2020	-0.00484	0.0131	0.0230	-0.0541
	(0.0468)	(0.0446)	(0.0257)	(0.0392)
November 2020	-0.00863	0.0233	0.0314	-0.0704*
	(0.0545)	(0.0487)	(0.0285)	(0.0425)
January 2021	-0.00547	-0.0711	0.0464*	-0.0650
	(0.0532)	(0.0484)	(0.0280)	(0.0435)
March 2021	0.0343	0.0165	-0.00987	-0.0120
	(0.0511)	(0.0442)	(0.0272)	(0.0415)
Pre-trend 1	-0.0953	0.0391	0.0453	0.0512
	(0.0651)	(0.0580)	(0.0351)	(0.0542)
Pre-trend 2	-0.0219	0.00370	0.0357	-0.0196
	(0.0627)	(0.0566)	(0.0336)	(0.0507)
Pre-trend 3	-0.000627	-0.0225	0.0132	0.0727*
	(0.0602)	(0.0463)	(0.0285)	(0.0429)
Pre-trend 4	-0.0738	-0.0200	0.0482*	-0.0157
	(0.0528)	(0.0444)	(0.0277)	(0.0421)
Pre-trend 5	-0.0111	0.000541	-0.0239	0.0692*
	(0.0574)	(0.0439)	(0.0279)	(0.0405)
Pre-trend 6	-0.0204	0.0466	0.00225	0.0674
	(0.0571)	(0.0461)	(0.0283)	(0.0426)
Pre-trend 7	-0.0619	-0.00879	0.0413	0.0108
	(0.0580)	(0.0485)	(0.0280)	(0.0426)
Pre-trend 8	-0.0524	0.00813	0.0426	0.0161
	(0.0558)	(0.0443)	(0.0271)	(0.0402)
Observations	199911	199911	199911	199911

Note: Event studies comparing those who have access to outdoor space to those who have not, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Standard errors in parentheses, clustered at the individual level over time. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.5.4 Conclusion

This section of the thesis has tested whether the COVID-19 pandemic has affected the relationship between housing, financial conditions and mental health in the United Kingdom. There exists a well-documented gradient in reported mental health (across different measures of mental health and across geographical and temporal contexts) depending on housing tenure, with outright homeowners faring the best and renters the worst. Using a large panel that follows individuals on a yearly basis before the pandemic and then on a monthly and bimonthly basis through it, I document that the gradient observed in pre-pandemic times is largely unaltered during the pandemic. For instance, although the gap in mental health between renters and the remaining tenure categories decreases in the first two months of the pandemic, it returns to pre-pandemic levels by the third month of survey and remains there in the subsequent period. The persistence of the gradient despite a shock such as that of the pandemic underscores how deep-seated it is, especially given the nature of the pandemic and the consequent policy responses, which gave stronger importance to one's living environment. In fact, the presence of outdoor spaces had also short-lived effects on mental health, as compared to pre-pandemic times. Moreover, another channel related to housing through which the pandemic could affect mental health, namely financial distress related to housing payments, did not have an effect outside the early months of pandemic. The latter though is the one most easily affected by government policy, aimed precisely at sustaining incomes and employment.

These results suggest that pre-existing gradients in mental health, in terms of housing tenure, ability to keep up with housing payments and access to outdoor spaces are persistent and resistant to multi-faceted shocks such as that represented by the COVID-19 pandemic. They could be explained by competing mechanisms: it could be that self-selection is primary responsible for the correlation between housing tenure and mental health, that the intangible channel connecting housing tenure to mental health is more important than the material one, or a combination of the two. Material security and the physical characteristics of the lived environment surely matter for

mental health, yet if they were its main drivers one would have expected a stronger change in the gradient during the pandemic. Ontological security, agency over one's home or social status conferred to specific tenures, plausibly unaffected during the pandemic, could have instead a stronger role in the structural differences in reported mental health that can be observed before and during the pandemic. This result is not immediately reconcilable with the main analysis of this chapter, suggesting that self-selection plays a role, but also that the most consequential channel is likely to be the material one, following the transition to outright homeownership. Such puzzle remains for future research, together with the question on whether these same patterns can be found in countries where homeownership has a less important meaning than in the UK, such as Germany.

The main novelty of the findings of this section lies in confirming the existence of a structural gradient in mental health across housing tenure categories, extensively studied with UK and non-UK data in pre-pandemic times, also during the pandemic period. While other works explored the evolution of mental health and of subjective well-being during the pandemic (showing patterns that are consistent with my results) and others looked at differences across housing tenures abstracting from pre-pandemic trends or at the intensive margin (in terms of pandemic severity), my work contributes to the understanding of the change (or the absence thereof) in the mental health gradient across housing tenure categories during the pandemic *as compared to* pre-pandemic times.

In terms of limitations, the study follows a long time-series at the individual level that is very informative but, due to the irregular frequency of the survey that has yearly cadence in normal times and monthly or bimonthly during the pandemic, could be sensitive to seasonal patterns in the pandemic periods that are not captured in the pre-pandemic survey. At the same time, both the pre-pandemic and pandemic modules do not capture more granular variations in measures of mental health or subjective well-being that occur during the day (such as those detailed in time use surveys), and that may reveal relevant dynamics during the pandemic period (Chen

and Wan, 2022). Also, the estimation assumes that once individuals are assigned to a housing category, they remain in it for the entirety of the sample, therefore I do not capture changes in status during the pandemic. Moreover, the results rely on self-reported measures of mental health of an ordinal nature that could display the same problems as life satisfaction scales when it comes to, among others, scale norming (Fabian, 2022). Finally, the analysis cannot gauge the impact on mental health of homeownership per se, or of having access to an outdoor space. Rather, it estimates if this effect was affected at all by the pandemic. The result of little or no effect of access to a private garden, for instance, would likely be very different if one were to estimate the effect of accessing a private garden during a pandemic for those who did not have access to one in pre-pandemic times. Despite the relevance of such a question for policy, this study can only shed light on the effect of variations in exposure for those who were already exposed to an existing condition.

CHAPTER 4

The consequences of housing tenure for voting behaviour

HOUSING, because of its salience in both social and economic terms, has political implications and there may be political causes as well as consequences of housing tenure (Ansell, 2019). The literature has had so far little focus on studying tenure transition in the context of voting behaviour as an event (Hall and Yoder (2022) is an example at the local level), and especially on distinguishing the transition to mortgaged homeownership and that to outright homeownership as two distinct moments, with potentially different implications. Theoretically, as discussed in more detail in Section 4.1, there are two main channels connecting housing tenure to voting behaviour: the *local* channel, which will not be investigated directly in this paper but which remains useful conceptually, and the *general*, or national channel. Works such as those of DiPasquale and Glaeser (1999) or Leviten-Reid and Matthew (2018) established quite strongly the link between housing tenure and local elections: a homeowner is more likely to stay longer in the community and therefore invests more in it, both by voting and by participating more in the local society in general; moreover, local affairs may influence house prices (if the house is seen as an investment). Additional evidence in this direction includes Gin and Sandy (1994) and Malpezzi et al. (1998), as well as some going in the opposite direction (Engelhardt et al., 2010). The fundamental features of housing tenure discussed in Section 1.3 play a role in this context too. The presence of transaction or adjustment costs increases the probability of residential stability in the community, while the right of *disposition* drives the concerns around how local affairs intertwine with house prices.

A less intuitive connection, but one that still finds evidence in support of and that this chapter will be particularly concerned with, is that among housing tenure and general elections, specifically in the British context. A strand of this literature observes a tenure effect in determining the party voted for. For the UK, most works, such as Johnston et al. (2001) or Pattie et al. (1995), date back to about two decades ago, while recent discussions are scarce (Ansell and Adler, 2019b). These accounts tend to agree on associating homeownership to more conservative beliefs, chiefly because of the economically liberal motive of independent control of the home, underlying the "property-owning democracy" ideal of the Conservative Party in Britain. Importantly, political attitudes may be affected by the process through which one becomes a homeowner (Bueno et al., 2022). Another smaller strand, which remains agnostic as to political beliefs, is that linking homeownership to increased participation in general elections. Recent contributions which argue in favour of the existence of an effect are those of André et al. (2017) for a set of 19 countries including the UK, and of Gius (2017) and Hall and Yoder (2022) for the US, the latter recognising a correlation between participation in local elections and that in national ones, possibly through a mechanism of habit formation. Moreover, at all levels of government in the US, there is a strong overrepresentation of homeowners in public office (Einstein et al., 2019). The motive of independent control over the home relates closely with the rights of *possession* and of *use*, including for issues related to legislation on property taxes or on landlord-tenant relationships; the latter represents also an immediate link between the right of *disposition* and the vote in general elections, for instance if a party is in support or against rent caps. More in general, the right of *disposition* is salient whenever an election is expected to be consequential for house prices.

Employing the same data source as for Chapter 3, described in the context of this chapter in Section 4.2, and the event study approach outlined in Section 1.5, with some additional considerations contained in Section 4.3, my results are discussed in Section 4.4. I find some evidence that the transition to mortgaged homeownership affects participation in national elections, specifically by raising the probability of voting, depending on the horizon, by up to 8% (an effect identified most clearly

among those with medium Loan-to-Value ratios), while leaving the probability of voting the Conservative Party unaffected. On the other hand, the transition to outright homeownership decreases, transitorily, the probability to vote by up to 2% among those with high Loan-to-Value ratios, while increasing the probability to vote for the Conservative Party by more than 2% (up to 4% among those with high Loan-to-Value ratios). Section 4.5 concludes.

4.1 Insights from theory and hypotheses

I discuss two theoretical channels through which housing tenure may affect voting behaviour. Although the first channel, the *local* one, is not treated in this study, it remains relevant because of its interactions with the second channel, the *general* one; for this same reason, I discuss the local channel first. The general channel is in turn divided between participation in the elections and direction of the vote.

4.1.1 The *local* channel

The *local* channel concerns the fact that becoming a homeowner attaches you more strongly to the place where you live. This implies that stakes at local elections become higher. Moreover, homeowners will tend to vote for candidates who pledge to increase the flow of benefits that *place* has towards them: anything that raises either local services or house prices (raising local services should indirectly also raise house prices). The latter is the direct link between households' portfolios and their voting behaviour, and may as well go in the opposite direction of provoking protest voting where homeowners lost housing wealth (Adler and Ansell, 2020). In the United States, there exists recent evidence that becoming a homeowner makes individuals more likely to engage in local political activity (Yoder, 2020), as well as that support for local housing development follows housing tenure rather than political alignment: liberal homeowners are closer in their views to conservative homeowners rather than to liberal renters (Marble and Nall, 2021).

4.1.2 The *general* channel

On a *general* level, there are in turn two channels. The first concerns the likelihood of participating in general elections. Homeowners may be participating more in general elections as a by-product of voting in local elections: because both local and national elections occur together, because of habit formation in voting, or because of a permanent shift in information acquisition (Hall and Yoder (2022); Prato (2018)). This is unless local and national elections are substitutes and compete for a voter's attention, with one crowding out the other (Hall and Yoder, 2022). Homeowners may be participating more also because it tends to be easier for them to overcome the cost of voting (André et al., 2017): voting is costly in terms of time and information, especially in countries with active voter registration (Braconnier et al., 2017), and homeownership tends to correlate with material resources that ease the cost. Finally, homeowners may participate more because they have a higher stake in the elections. André et al. (2017) frame such a relationship within instrumental motivation theory (Franklin, 2001). Individuals who have a desire to influence policy will have higher electoral participation. For homeowners, this instrumental motive tends to be stronger, in many Western countries at least, because institutional set-ups favour and promote homeownership both in social and economic terms. The asset nature of one's home makes the dimension of the stake apparent and indeed variations in house prices are found to be connected to participation in general elections in McCartney (2021) and in Hall and Yoder (2022).

This brings us to the second channel connecting tenure to voting, the direction of the national vote. Institutional set-ups, for instance subsidies to ownership of property, may also spillover to specific political orientations. Prato (2018) argues that public policy favouring homeowners, as deductions on mortgage interest or, as in the case of the Right to Buy scheme in the UK, discounted sales of public housing, increase support for less redistribution and thereby also (fiscal) conservatism. Homeownership is indeed generally associated to a more conservative vote (Ansell, 2014), even though it is a topic not solely appropriated by conservative parties (Kohl, 2020). Already

in a 1872 series of articles, Engels presented homeownership as, *inter alia*, a device to make individuals more reactionary (Engels, 1976). This could be true because of legacy (the Conservative Party has historically promoted homeownership) or expectations (the Conservative Party will protect/favour homeownership). In Britain, homeownership was part of the discussion on the process of embourgeoisement of the working class already in the late fifties and early sixties, following repeated electoral defeats of the Labour Party (Crosland (1960); Goldthorpe and Lockwood (1963)). Yet such drivers appear to be in action in contemporary Britain too, the theme of homeownership intimately linked to the Conservative Party: a classic example being Margaret Thatcher's "property-owning democracy" (Pattie et al., 1995) and a recent one Boris Johnson's pledges at the 2020 Conservative Party Conference (SkyNews, 2020).

> *We believe that this policy could create two million more owner-occupiers - the biggest expansion of home ownership since the 1980s. [...] We will help turn generation rent into generation buy.*

4.1.3 Hypotheses

These theoretical considerations strongly suggest that homeowners, especially in the UK, should be more likely to participate in national votes, as well as to favour conservative agendas. My first hypothesis is therefore:

HYPOTHESIS 4.1. The transition to mortgaged homeownership increases participation in general elections and increases the likelihood to vote for the Conservative Party.

On the other hand, once the first transition to homeownership has occurred, there do not appear to be as clear reasons why the transition to outright homeownership should provoke a discontinuity in terms of participation or direction of the vote. In other words, the observed and counterfactual outcomes should follow the same trends before and after the transition. My second hypothesis is therefore:

HYPOTHESIS 4.2. The transition to outright homeownership does not alter participation in general elections or the likelihood to vote for the Conservative Party.

4.2 Patterns and data description

As in Chapter 3, I use 28 waves of the combined British Household Panel Survey (1991 to 2009) and Understanding Society (2009 to 2020) (University of Essex, Institute for Social and Economic Research, 2021b), to obtain a large longitudinal survey containing a wide range of information on households and individuals in the UK, including on their housing tenure and on their voting behaviour. Depending on the model, observations range between around 82,000 to around 519,000.

The first outcome variable of interest, participation in general elections, is obtained from the answer to the question *Did you vote in this (past) year's general election?* and considering the binary answer *Yes* or *No*. The second variable of interest is whether the person voted for the Conservative Party. This is obtained as a binary recoding of the wider question *Which political party did you vote for?*, for the subset of individuals who answered *Yes* to the question on voting in the general election. Since elections occur less frequently than the number of waves, many datapoints on participation and party voted for are missing. I imput missing values, for a same individual, as follows:

- For *participation in the last general elections*, a missing value is replaced by the closest past observation for a same individual. In Appendix C, I run a robustness check where a missing value is replaced by the closest future observation for a same individual. This also implies a partly different set of observations.
- For *party voted for at the last general election*, a missing value is replaced in the first place with the answer to the question on which *party one feels closest to*, and then, as for the participation question, remaining missing values are replaced by the closest past observation for a same individual. In Appendix C, I run a robustness check where a missing value is replaced by the closest future observation for a same individual.

As shown in Table 4.1, outright homeowners tend to have both the highest average share of individuals participating in general elections (around 86%) and voting for the Conservative Party (36%). Mortgagors follow with 75% and 29% respectively, and renters are last with 63% and 18%. Outright homeowners are also oldest (mean age of around 62 years), but in this case renters come second at 46 years and mortgagors at 41. Mortgagors have the highest share of individuals with a university degree (about 36%), as compared to homeowners (28%) and renters (19%). The patterns are qualitatively similar if one adopts the alternative imputation of Appendix C, as shown in Table C.1. André et al. (2017) report an unconditional estimate of the cross-sectional difference in the likelihood to participate in general elections between homeowners and renters, for the US and a set of 18 European countries including the UK, to be 13% for the average across countries and 20% for the UK, in the ballpark of the descriptives in Table 4.1. Gius (2017) reports a cross-sectional difference in participation for the US also around 20%.

TABLE 4.1. Overview of summary statistics

	(1) Outright homeownership		(2) Mortgage		(3) Rent	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Share participating in general elections	0.855	0.352	0.753	0.431	0.632	0.482
Share voting for the Conservative Party	0.362	0.481	0.287	0.453	0.176	0.380
Age	61.679	15.984	41.471	12.386	45.605	19.107
Share with higher education	0.278	0.448	0.359	0.480	0.185	0.388
Observations	127297		166116		102330	

Note: Mean and standard deviation for a set of variables, by housing tenure. Variables reported as shares are binary recoding of variables with wider ranges.

Table 4.2 presents additionally descriptive statistics on tenure transitions. The majority of individuals in the sample experiences no tenure transitions (72.4%), while a sizeable amount experiences one tenure transition (18.5%) and about half as much

TABLE 4.2. Frequency of tenure transitions and salient variables

Number of transitions	Absolute frequency	Relative frequency	Share participating in general elections	Share voting for the Conservative Party	Age	Share with higher education
0	44,836	0.724	0.726	0.275	46.523	0.280
1	11,462	0.185	0.762	0.285	44.163	0.348
2	3,296	0.053	0.719	0.283	39.190	0.348
3	1,361	0.022	0.713	0.288	41.643	0.330
4	549	0.009	0.687	0.230	39.232	0.319
>4	392	0.006	0.694	0.360	41.582	0.326
Total	61,896	1				

Note: The table lists the absolute and relative frequency of the number of individuals in the sample by number of tenure transitions, as well as the mean of salient variables.

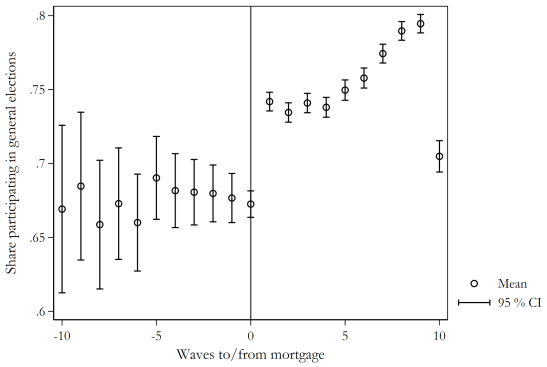
(9%) experiences two or more transitions. The share of those participating in general elections among those experiencing one tenure transition is higher than the figure for both those experiencing no tenure transition and for those experiencing two or three, while the figures for those experiencing more than three transitions are even lower (though it should be kept in mind that these have rather low frequency). On the other hand, the share voting for the Conservative Party is rather similar across all groups, except for those experiencing four transitions, for which it is substantially lower, and for those experiencing more than four, for which it is substantially higher. Mean age varies across the groups but it tends to be higher for those experiencing one or no transitions, while the share with higher education is lowest among those experiencing no transitions, followed by those experiencing three or more, and finally by those experiencing one or two transitions.

Figure 4.1 shows how the share of individuals participating in general elections and voting for the Conservative Party changes as a function of the distance (in terms of waves) from the tenure transition to mortgaged and outright homeownership. Based on these descriptive trends, there appears to be a discontinuity for both variables and for both tenure transitions around the event time, with the share participating in general elections and that of voting for the Conservative Party sizeably increasing following the event. The share participating in general elections increases by about 8%

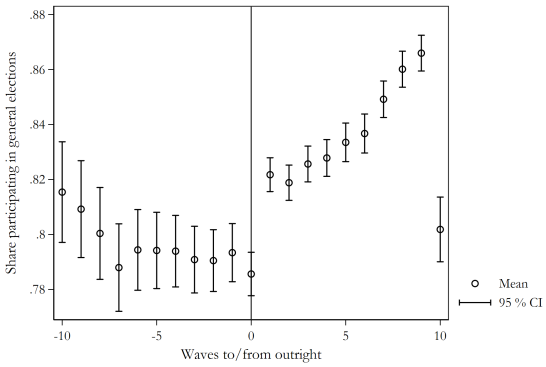
following the transition to mortgaged homeownership and then keeps increasing as the distance from the event increases. For the transition to outright homeownership, the immediate increase following the event is of about 3% and also keeps increasing as the distance increases. The share voting for the Conservative Party increases instead by about 10% following the transition to a mortgage, and by about 5% following the transition to outright homeownership; in both cases, the share remains rather stable after the initial jump following the event. The charts show similar patterns when using the alternative imputation of Appendix C, as shown in Figure C.1. The results section will employ event-study type regressions to leverage suitable counterfactuals and identify the treatment effect from tenure transition.

FIGURE 4.1. Evolution of voting shares as a function of time distance from an event

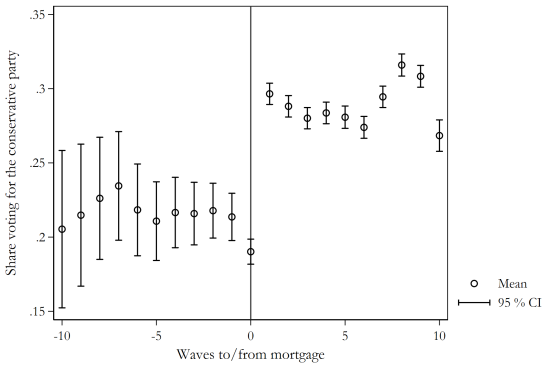
a. Share participating for the event of taking out a mortgage



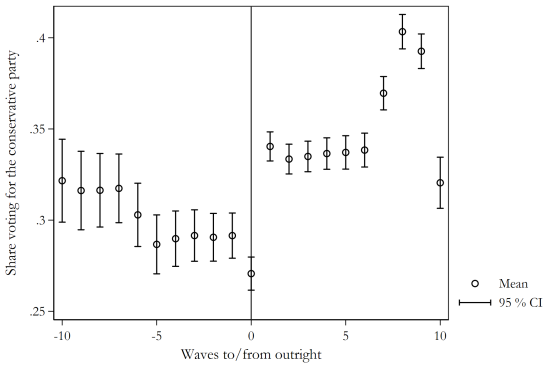
b. Share participating for the event of becoming an outright homeowner



c. Share voting Conservative for the event of taking out a mortgage



b. Share voting Conservative for the event of becoming an outright homeowner



Note: Evolution of the share participating in general elections (Panels *a* and *b*) and of the share voting the Conservative Party (Panels *c* and *d*) as a function of distance from obtaining a mortgage and from outright homeownership.

4.3 Additional empirical considerations

To estimate the change in the probability of participating in general elections and of voting for the Conservative Party I use three difference-in-difference methods, as described in Chapter 1. The first is a dynamic two-way fixed effects (TWFE) estimator, that is ordinary least squares with individual and time fixed effects and with time interactions capturing the distance from the event of transition. Since the TWFE estimator has been proven not to identify the Average Treatment Effect on the Treated unless very strict and likely unrealistic assumptions are imposed (Goodman-Bacon, 2021), I also use two methods that are robust to the violations of such assumptions, specifically those of Borusyak et al. (2022) and of Callaway and Sant'Anna (2021). In all cases, I impute missing data as described in Section 4.2 and employ linear probability models, which are in all respects the same as the general models described in Section 1.5, but with the outcome variable taking only values 1 and 0. In the case of the TWFE estimator, this means:

$$V_{it} = \gamma_i + \lambda_t + \sum_{h=-q}^{-1} \delta_h \mathrm{HO}_{ih} + \sum_{h=0}^{m} \tau_h \mathrm{HO}_{ih} + X'_{it}\Gamma + \epsilon_{it} \tag{4.1}$$

Where all elements have the same meaning as in Section 1.5, except for V_{it}. When looking at participation in general elections, V_{it} takes value 1 if individual i participated in general elections at time t and 0 otherwise. Similarly, when looking at the party voted for, V_{it} takes value 1 if individual i voted for the Conservative Party at time t, and 0 otherwise. Analogously, for the BJS estimator the linear probability model takes the form (in the counterfactual outcome equation):

$$V_{it}(0) = \gamma_i + \lambda_t + \epsilon_{it} \tag{4.2}$$

With $V_{it}(0)$ representing counterfactual outcomes for the voting variable, that is the voting outcomes that would be observed in the absence of treatment. Finally, for the CS estimator the main regression would take the form:

$$V_{i,g,t} = \alpha_{1,g,t} + \alpha_{2,g,t} \cdot G_g + \alpha_{3,g,t} \cdot 1\{T = t\} + \beta_{g,t} \cdot (G_g \cdot 1\{T = t\}) + \epsilon_{i,g,t} \quad (4.3)$$

Where $V_{i,g,t}$ is the voting outcome for individual i at time t, belonging to the cohort of individuals who start to be treated at time g. The linear probability model estimates the variation in probability for a binary outcome based on a linear specification, as compared to non-linear ones such as logit or probit. The main drawback of this model as compared to logit or probit is that it is not bounded on the 0-1 space, and is also known to be biased and inconsistent under certain circumstances (Horrace and Oaxaca, 2006). Its appeal in this context is that it allows the application of the recent advances in event studies with staggered adoption, such as those described in BJS and CS, adopting a linear model.

4.4 Results and discussion

To test the hypotheses, I first analyse the results from the estimation of the event study around the transition to mortgaged homeownership, for both participation in general elections and vote for the Conservative Party, and then that for the transition to outright homeownership. A final part of this section is dedicated to discussing heterogeneities of results based on Loan-to-Value (LTV) ratios.

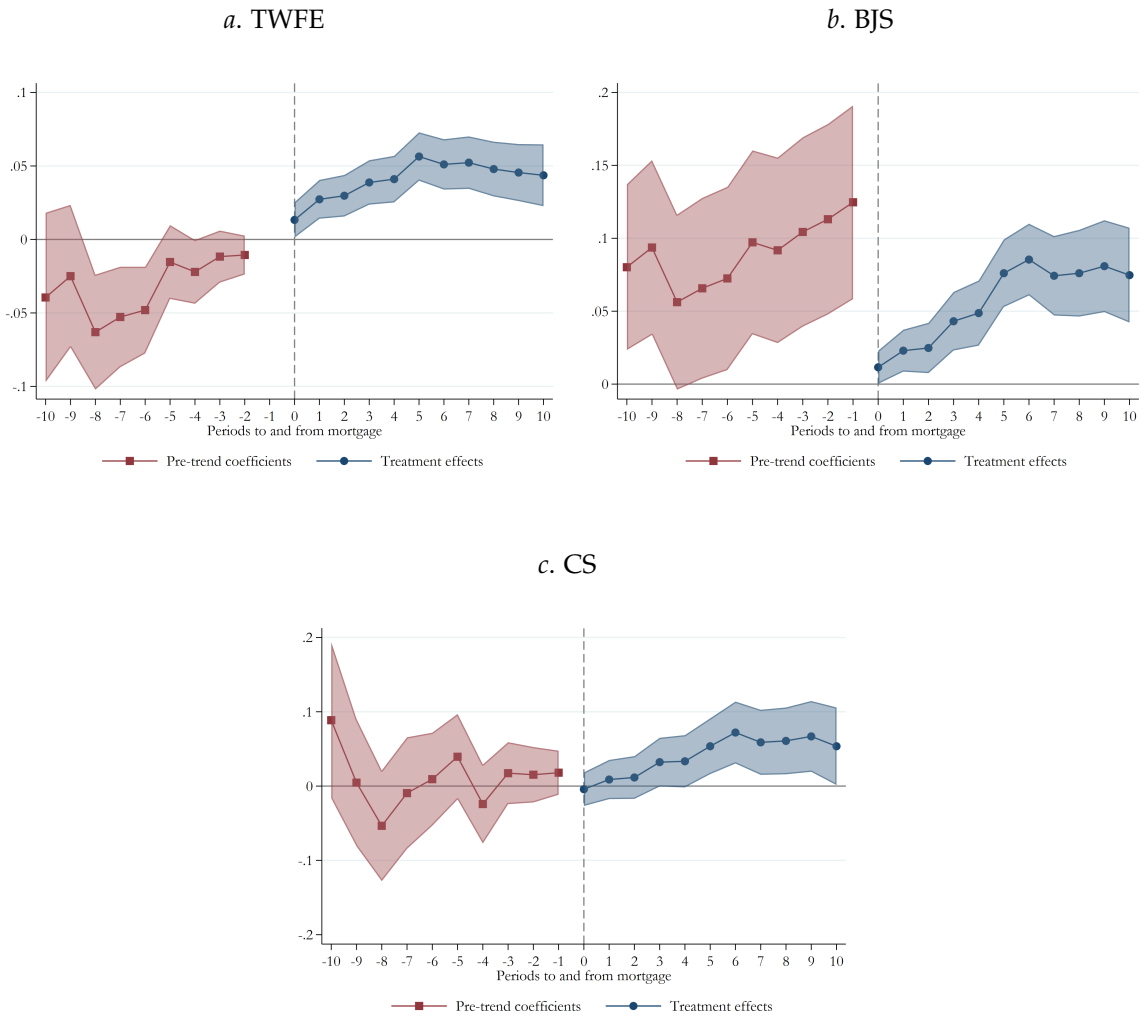
4.4.1 The transition to mortgaged homeownership

The descriptive patterns of Panel *a* of Figure 4.1 suggested that the share of people participating in general elections, following the transition to mortgaged homeownership, increases substantially. Yet in the absence of a valid counterfactual it is not possible to attribute such an increase to the effect of the tenure transition. Figure 4.2 and Table 4.3 offer an estimation of the ATT of the transition to mortgaged homeownership by treatment horizon, as well as tests on pre-trends. Panel *a* is the TWFE estimation, which I include as a reference although it is known to yield incorrect estimates of our target parameter under plausible assumptions. It shows a linear increase in the coefficient associated with the tenure transition, including in the pre-trends. The BJS estimator (Panel *b*), which is robust to treatment effect heterogeneity and separates the pre-trend testing from estimation, shows also increasing coefficients for the pre-trends, indicating anticipation effects as well as the likely absence of parallel trends between those who transition to mortgaged homeownership and those who serve as counterfactual, if no tenure transition occurred. On the other hand, the CS estimator (Panel *c*) shows evidence of parallel pre-trends and no-anticipation, while treatment effects post transition are positive and increasing. The largest treatment effects, occurring for all estimators after five waves from the tenure transition, range between a 5% and 8% increase in the share participating in general elections following the transition to mortgaged homeownership. These results offer some evidence in line with Hypothesis 4.1, suggesting that the transition to mortgaged

homeownership might affect positively participation in general elections. Results are qualitatively similar when employing the alternative imputation of Appendix C, as shown in Figure C.2 and Table C.2. Johnston et al. (2001), analysing the 1997 general election in the UK, find that owner-occupiers were the tenure least likely to abstain. André et al. (2017) provide conditional estimates on cross-sectional data for the UK showing homeowners to be 8% more likely than renters to vote in general elections, while Gius (2017) reports a figure between 13% and 18% for the US. Hall and Yoder (2022) use a very large administrative dataset and a difference-in-differences set up to estimate the longitudinal effect of homeownership on participation in national elections in the US, finding it to be between 4% and 8%. They find evidence that the probability of participation is higher following local elections and if the individual has used a federal housing program to acquire their home.

When we turn to Figure 4.3 and Table 4.4, showing the event study for the same transition to mortgaged homeownership but where the outcome is the share of individuals voting for the Conservative Party, the patterns are rather different. While the descriptive share shown on Panel *c* of Figure 4.1 suggested a substantial jump in the share following the transition, all estimators show a rather flat profile for both pre-trends and horizon-specific ATTs. In this case too, results are consistent when using the alternative imputation of Appendix C, as shown in Figure C.3 and Table C.3. This suggests that the transition to mortgaged homeownership actually does not have an effect on the Conservative vote, counter to Hypothesis 4.1. Johnston et al. (2001), just cited with reference to abstention, find also that owner-occupiers were the tenure most likely to vote for the Conservative Party. This is still the case 20 years later (at the 2017 general election): while both young homeowners and renters are as likely to vote Conservative, the gap emerges as individuals grow older (Ansell and Adler, 2019b).

FIGURE 4.2. Event studies of vote participation for the transition to mortgaged homeownership



Note: Event studies for participation in general elections in terms of obtaining a mortgage, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

TABLE 4.3. Event study of vote participation for the transition to mortgaged homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	-0.0395	0.0802***	0.0886*
	(0.0294)	(0.0290)	(0.0536)
Pre-trend 9	-0.0250	0.0937***	0.00461
	(0.0248)	(0.0306)	(0.0437)
Pre-trend 8	-0.0631***	0.0562*	-0.0535
	(0.0200)	(0.0306)	(0.0380)
Pre-trend 7	-0.0528***	0.0657**	-0.00943
	(0.0175)	(0.0316)	(0.0383)
Pre-trend 6	-0.0481***	0.0725**	0.00928
	(0.0151)	(0.0321)	(0.0319)
Pre-trend 5	-0.0154	0.0972***	0.0395
	(0.0128)	(0.0322)	(0.0293)
Pre-trend 4	-0.0221**	0.0917***	-0.0242
	(0.0111)	(0.0325)	(0.0271)
Pre-trend 3	-0.0117	0.104***	0.0174
	(0.00904)	(0.0331)	(0.0213)
Pre-trend 2	-0.0106	0.113***	0.0153
	(0.00675)	(0.0333)	(0.0190)
Pre-trend 1		0.125***	0.0180
		(0.0339)	(0.0151)
ATT 0	0.0133**	0.0115**	-0.00421
	(0.00615)	(0.00581)	(0.0115)
ATT 1	0.0273***	0.0229***	0.00882
	(0.00674)	(0.00734)	(0.0135)
ATT 2	0.0297***	0.0248***	0.0116
	(0.00722)	(0.00880)	(0.0147)
ATT 3	0.0388***	0.0431***	0.0322*
	(0.00771)	(0.0102)	(0.0167)
ATT 4	0.0410***	0.0487***	0.0334*
	(0.00809)	(0.0114)	(0.0180)
ATT 5	0.0564***	0.0760***	0.0536***
	(0.00841)	(0.0118)	(0.0191)
ATT 6	0.0511***	0.0854***	0.0721***
	(0.00875)	(0.0126)	(0.0213)
ATT 7	0.0523***	0.0743***	0.0589***
	(0.00911)	(0.0139)	(0.0224)
ATT 8	0.0479***	0.0760***	0.0608***
	(0.00951)	(0.0152)	(0.0229)
ATT 9	0.0455***	0.0809***	0.0669***
	(0.00990)	(0.0161)	(0.0243)
ATT 10	0.0437***	0.0747***	0.0536**
	(0.0108)	(0.0166)	(0.0267)
N	324980	112707	112252

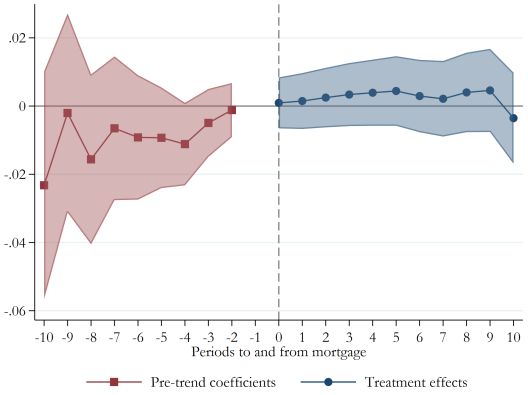
Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

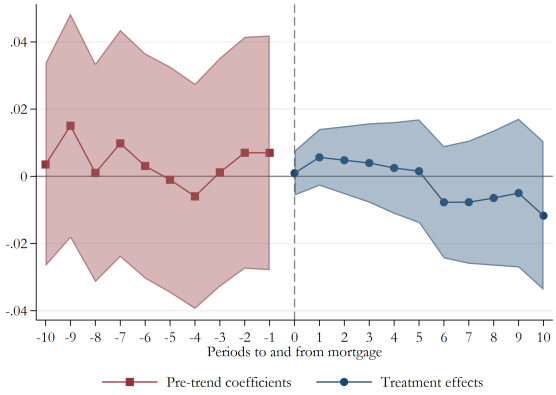
Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

FIGURE 4.3. Event studies of Conservative vote for the transition to mortgaged homeownership

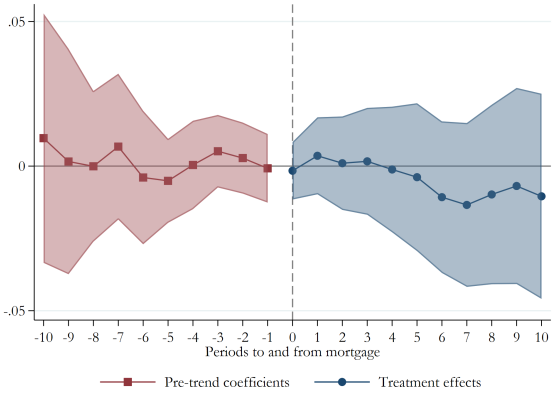
a. TWFE



b. BJS



c. CS



Note: Event studies for Conservative Party vote in terms of obtaining a mortgage, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

TABLE 4.4. Event study of Conservative vote for the transition to mortgaged homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	-0.0232	0.00353	0.00966
	(0.0170)	(0.0155)	(0.0220)
Pre-trend 9	-0.00203	0.0151	0.00160
	(0.0149)	(0.0171)	(0.0199)
Pre-trend 8	-0.0156	0.00102	-0.0000757
	(0.0127)	(0.0166)	(0.0133)
Pre-trend 7	-0.00652	0.00979	0.00673
	(0.0108)	(0.0173)	(0.0129)
Pre-trend 6	-0.00918	0.00307	-0.00395
	(0.00931)	(0.0171)	(0.0118)
Pre-trend 5	-0.00930	-0.00101	-0.00509
	(0.00753)	(0.0172)	(0.00742)
Pre-trend 4	-0.0112*	-0.00596	0.000395
	(0.00617)	(0.0171)	(0.00780)
Pre-trend 3	-0.00495	0.00119	0.00515
	(0.00509)	(0.0174)	(0.00640)
Pre-trend 2	-0.00117	0.00702	0.00278
	(0.00405)	(0.0176)	(0.00627)
Pre-trend 1		0.00700	-0.000764
		(0.0178)	(0.00606)
ATT 0	0.000935	0.000940	-0.00161
	(0.00382)	(0.00342)	(0.00508)
ATT 1	0.00147	0.00565	0.00357
	(0.00418)	(0.00432)	(0.00679)
ATT 2	0.00248	0.00479	0.00101
	(0.00445)	(0.00516)	(0.00824)
ATT 3	0.00337	0.00397	0.00165
	(0.00471)	(0.00603)	(0.00943)
ATT 4	0.00391	0.00249	-0.00117
	(0.00495)	(0.00697)	(0.0111)
ATT 5	0.00442	0.00153	-0.00383
	(0.00521)	(0.00788)	(0.0130)
ATT 6	0.00293	-0.00771	-0.0107
	(0.00541)	(0.00854)	(0.0134)
ATT 7	0.00214	-0.00770	-0.0134
	(0.00565)	(0.00936)	(0.0145)
ATT 8	0.00400	-0.00646	-0.00981
	(0.00594)	(0.0103)	(0.0158)
ATT 9	0.00458	-0.00498	-0.00687
	(0.00622)	(0.0113)	(0.0173)
ATT 10	-0.00355	-0.0117	-0.0104
	(0.00681)	(0.0113)	(0.0181)
N	401474	143701	142062

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

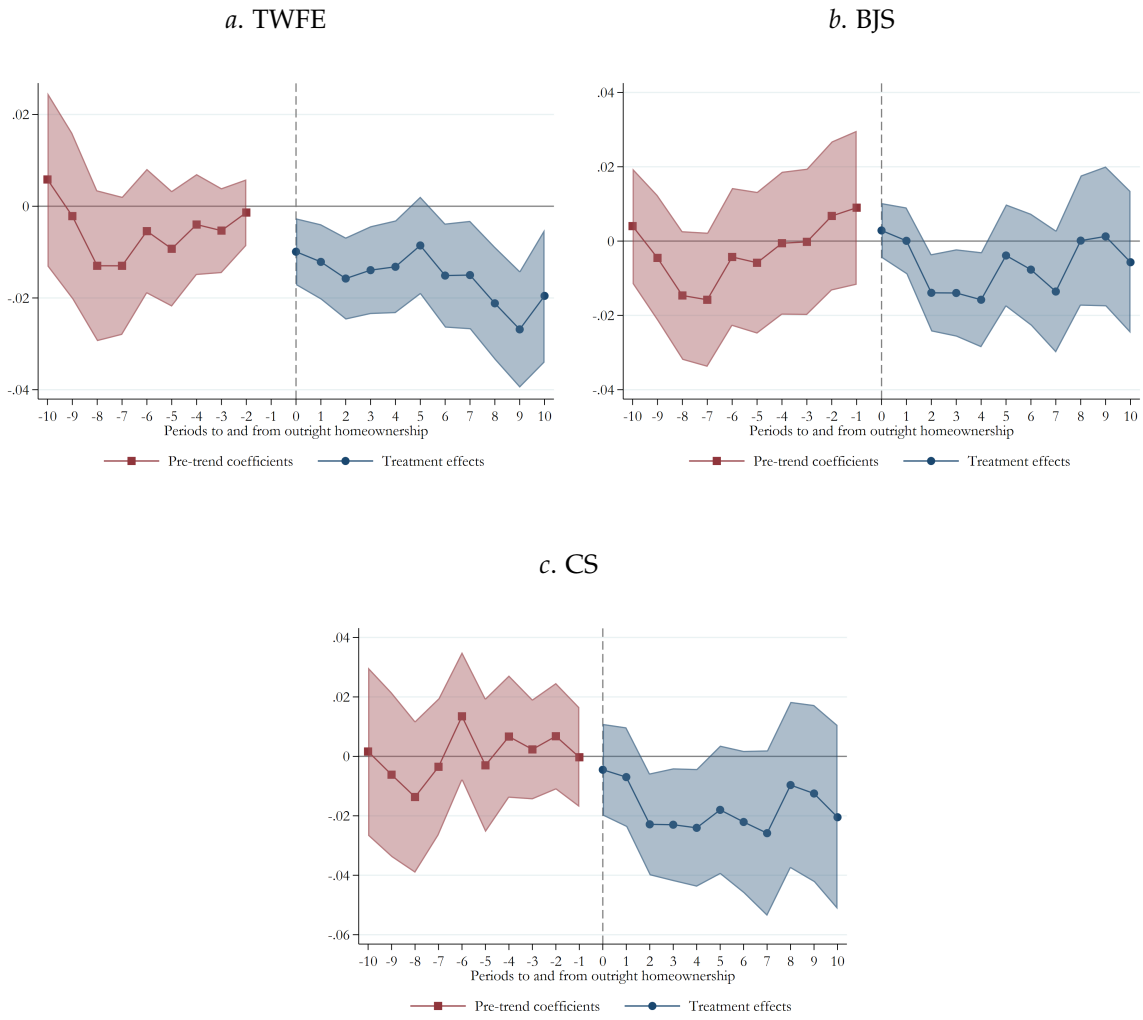
Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

4.4.2 The transition to outright homeownership

Figure 4.4 and Table 4.5 show the event study estimations for the effect of the transition to outright homeownership on the share participating in general elections. While all estimators show pre-trend dynamics, these are relatively close to 0, especially for the CS estimator. The ATT estimations vary across the estimators, but they all agree on a negative and significant ATT between horizons 2 and 4. The magnitude of the ATT at these horizon is close to a 2% decrease in the share of people participating in general elections following outright homeownership. The patterns are similar but more pronounced when excluding renters from the group of never-treated counterfactuals (Figure 4.5 and Table 4.6). When adopting the alternative imputation approach of Appendix C, as shown in Figures C.4 and C.5 and Tables C.4 and C.5, the negative effect on participation following the event of transition is not observed, except in the TWFE case and anyway to a smaller extent. The results for the first imputation method are therefore in contrast with Hypothesis 4.2, since I expected the transition to outright homeownership to have no effect on participation in general elections.

The estimation of the effect of the transition to outright homeownership on the Conservative vote is shown in Figure 4.6 and Table 4.7. All estimators suggest a significant increase in the share of individuals voting for the Conservative Party following the transition to outright homeownership. The ATTs increase with distance from the event, ranging between slightly below and slightly above 2%. Pre-trends coefficients are tight around 0, suggesting no anticipation and the presence of parallel trends, except for the TWFE estimator. When repeating this exercise excluding renters from the never-treated counterfactual group (Figure 4.7 and Table 4.8), the patterns are similar but the evidence in favour of parallel trends and no anticipation is weaker, and the ATTs are lower in magnitude. The alternative imputation of Appendix C, as shown in Figures C.6 and C.7 and Tables C.6 and C.7, shows similar patterns. These results too suggest that Hypothesis 4.2 should be rejected, since no effect of the transition to outright homeownership on the Conservative vote was expected.

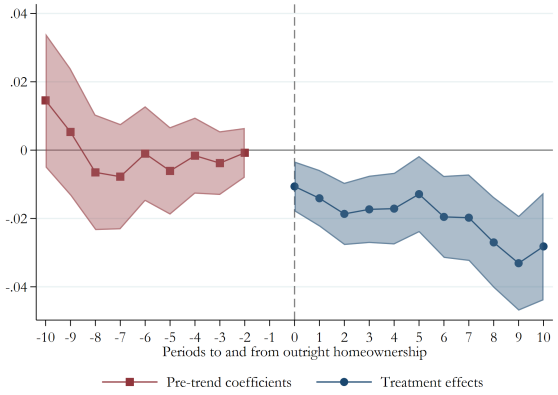
FIGURE 4.4. Event studies of vote participation for the transition to outright homeownership



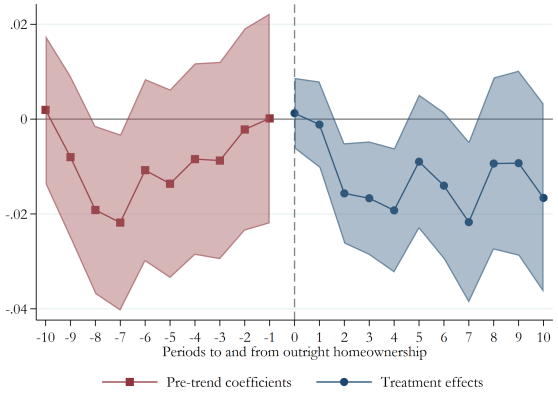
Note: Event studies for participation in general elections in terms of becoming an outright homeowner, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE 4.5. Event studies of vote participation for the transition to outright homeownership, excluding renters

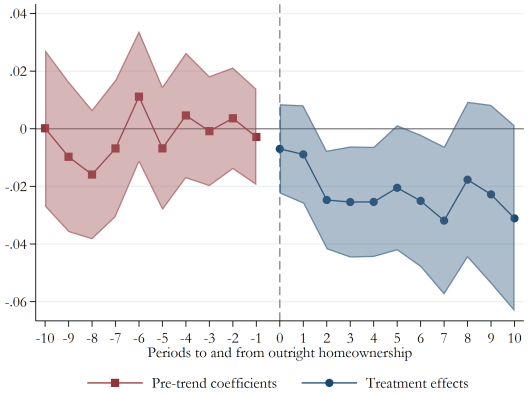
a. TWFE



b. BJS

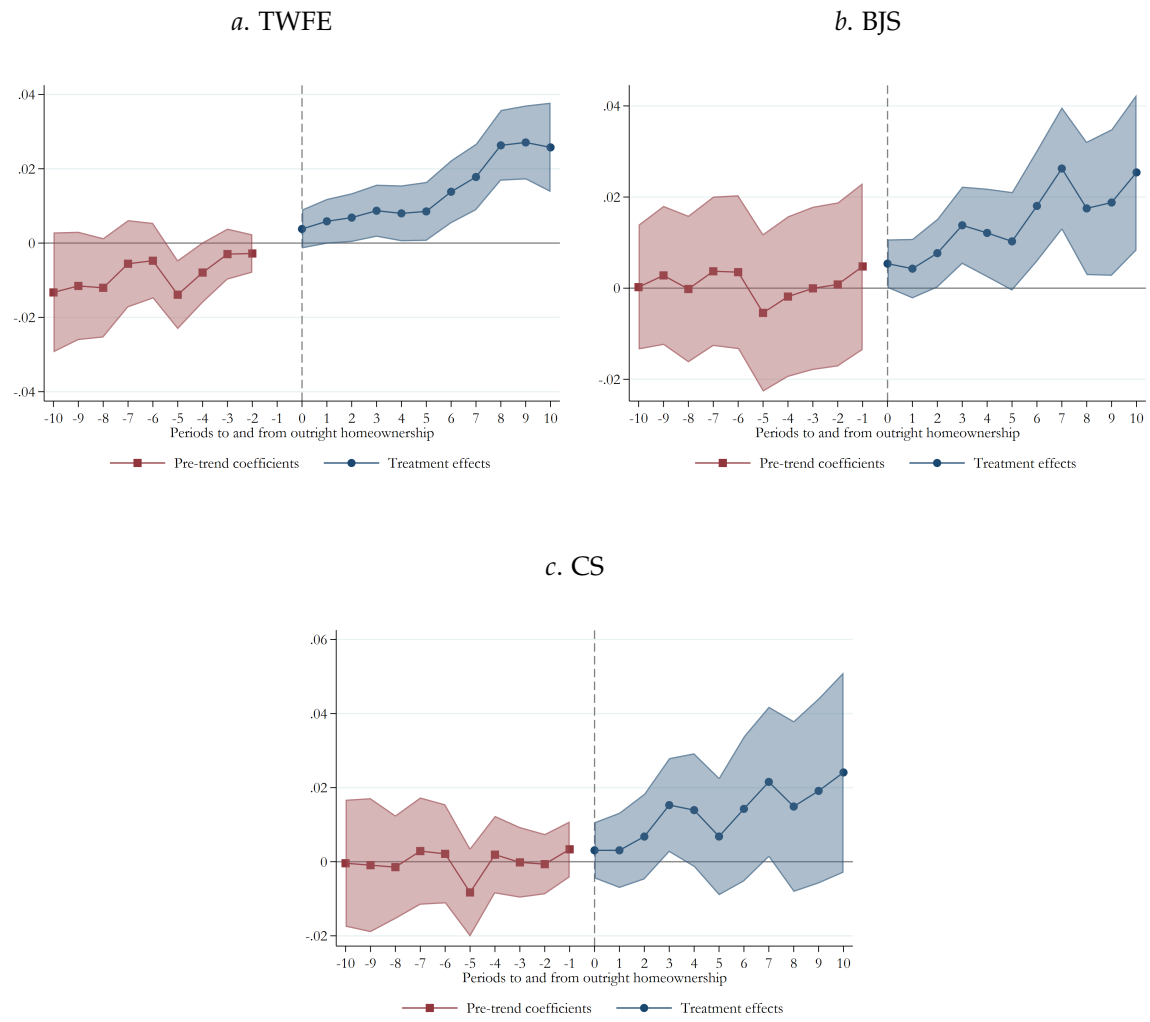


c. CS



Note: Event studies for participation in general elections in terms of becoming an outright homeowner and excluding renters as counterfactuals for the never-treated group, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE 4.6. Event studies of Conservative vote for the transition to outright homeownership



Note: Event studies for Conservative Party vote in terms of becoming an outright homeowner, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

TABLE 4.5. Event study of vote participation for the transition to outright homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	0.00584	0.00404	0.00164
	(0.00967)	(0.00793)	(0.0145)
Pre-trend 9	-0.00215	-0.00453	-0.00617
	(0.00924)	(0.00864)	(0.0141)
Pre-trend 8	-0.0130	-0.0146*	-0.0137
	(0.00839)	(0.00884)	(0.0130)
Pre-trend 7	-0.0130*	-0.0158*	-0.00352
	(0.00768)	(0.00922)	(0.0118)
Pre-trend 6	-0.00543	-0.00427	0.0135
	(0.00694)	(0.00948)	(0.0111)
Pre-trend 5	-0.00927	-0.00583	-0.00301
	(0.00644)	(0.00974)	(0.0115)
Pre-trend 4	-0.00399	-0.000577	0.00666
	(0.00562)	(0.00982)	(0.0105)
Pre-trend 3	-0.00532	-0.000204	0.00235
	(0.00473)	(0.0101)	(0.00859)
Pre-trend 2	-0.00137	0.00676	0.00678
	(0.00370)	(0.0102)	(0.00915)
Pre-trend 1		0.00898	-0.000297
		(0.0106)	(0.00858)
ATT 0	-0.00991***	0.00286	-0.00450
	(0.00372)	(0.00377)	(0.00788)
ATT 1	-0.0121***	0.0000782	-0.00698
	(0.00419)	(0.00459)	(0.00855)
ATT 2	-0.0158***	-0.0139***	-0.0229***
	(0.00457)	(0.00531)	(0.00875)
ATT 3	-0.0139***	-0.0140**	-0.0230**
	(0.00489)	(0.00600)	(0.00969)
ATT 4	-0.0132**	-0.0158**	-0.0240**
	(0.00516)	(0.00654)	(0.0101)
ATT 5	-0.00855	-0.00388	-0.0180
	(0.00543)	(0.00703)	(0.0110)
ATT 6	-0.0151***	-0.00768	-0.0221*
	(0.00579)	(0.00768)	(0.0122)
ATT 7	-0.0150**	-0.0136	-0.0258*
	(0.00603)	(0.00840)	(0.0142)
ATT 8	-0.0212***	0.000127	-0.00965
	(0.00629)	(0.00894)	(0.0143)
ATT 9	-0.0268***	0.00126	-0.0125
	(0.00649)	(0.00961)	(0.0152)
ATT 10	-0.0195***	-0.00569	-0.0205
	(0.00741)	(0.00977)	(0.0158)
N	402754	287364	287186

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

TABLE 4.6. Event study of vote participation for the transition to outright homeownership, excluding renters

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	0.0146	0.00196	0.000171
	(0.0100)	(0.00800)	(0.0139)
Pre-trend 9	0.00531	-0.00801	-0.00972
	(0.00952)	(0.00876)	(0.0133)
Pre-trend 8	-0.00650	-0.0192**	-0.0159
	(0.00863)	(0.00904)	(0.0115)
Pre-trend 7	-0.00776	-0.0218**	-0.00682
	(0.00787)	(0.00948)	(0.0122)
Pre-trend 6	-0.00102	-0.0108	0.0112
	(0.00708)	(0.00981)	(0.0116)
Pre-trend 5	-0.00608	-0.0136	-0.00678
	(0.00654)	(0.0102)	(0.0109)
Pre-trend 4	-0.00161	-0.00843	0.00463
	(0.00569)	(0.0103)	(0.0111)
Pre-trend 3	-0.00380	-0.00873	-0.000830
	(0.00476)	(0.0106)	(0.00974)
Pre-trend 2	-0.000771	-0.00218	0.00364
	(0.00371)	(0.0109)	(0.00901)
Pre-trend 1		0.000160	-0.00285
		(0.0113)	(0.00854)
ATT 0	-0.0106***	0.00125	-0.00698
	(0.00373)	(0.00380)	(0.00791)
ATT 1	-0.0141***	-0.00114	-0.00890
	(0.00423)	(0.00464)	(0.00871)
ATT 2	-0.0187***	-0.0157***	-0.0247***
	(0.00465)	(0.00540)	(0.00874)
ATT 3	-0.0173***	-0.0167***	-0.0254***
	(0.00503)	(0.00611)	(0.00984)
ATT 4	-0.0171***	-0.0192***	-0.0254***
	(0.00535)	(0.00669)	(0.00976)
ATT 5	-0.0129**	-0.00897	-0.0205*
	(0.00570)	(0.00722)	(0.0111)
ATT 6	-0.0195***	-0.0140*	-0.0250**
	(0.00613)	(0.00791)	(0.0117)
ATT 7	-0.0198***	-0.0217**	-0.0319**
	(0.00646)	(0.00868)	(0.0131)
ATT 8	-0.0270***	-0.00936	-0.0177
	(0.00680)	(0.00926)	(0.0138)
ATT 9	-0.0331***	-0.00928	-0.0228
	(0.00709)	(0.00996)	(0.0159)
ATT 10	-0.0282***	-0.0166	-0.0311*
	(0.00804)	(0.0102)	(0.0165)
N	312349	196959	199767

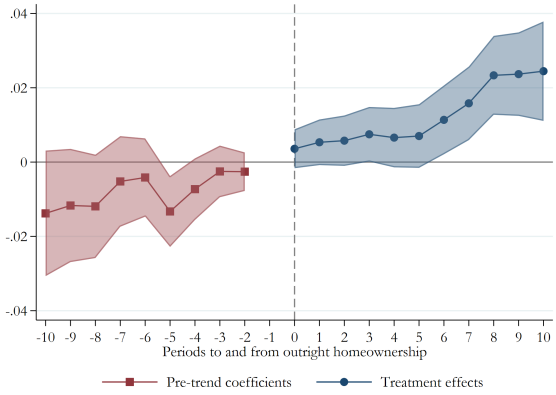
Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

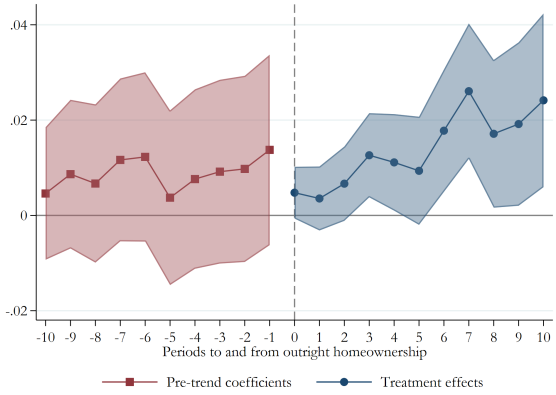
Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

FIGURE 4.7. Event studies of Conservative vote for the transition to outright homeownership, excluding renters

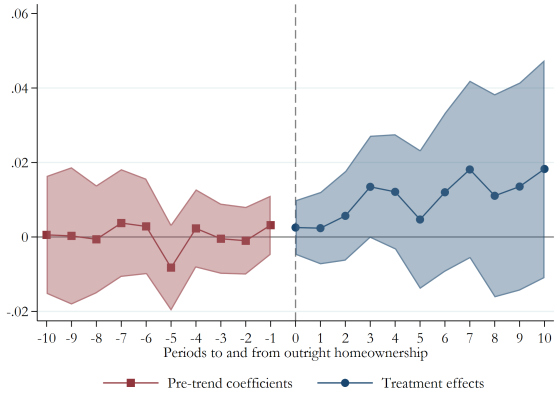
a. TWFE



b. BJS



c. CS



Note: Event studies for Conservative Party vote in terms of becoming an outright homeowner and excluding renters as counterfactuals for the never-treated group, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

TABLE 4.7. Event study of Conservative vote for the transition to outright homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	-0.0133	0.000237	-0.000403
	(0.00824)	(0.00699)	(0.00875)
Pre-trend 9	-0.0115	0.00280	-0.000925
	(0.00744)	(0.00779)	(0.00923)
Pre-trend 8	-0.0120*	-0.000188	-0.00147
	(0.00683)	(0.00822)	(0.00714)
Pre-trend 7	-0.00556	0.00371	0.00288
	(0.00600)	(0.00837)	(0.00740)
Pre-trend 6	-0.00474	0.00352	0.00213
	(0.00520)	(0.00862)	(0.00683)
Pre-trend 5	-0.0139***	-0.00542	-0.00829
	(0.00475)	(0.00883)	(0.00610)
Pre-trend 4	-0.00794*	-0.00185	0.00191
	(0.00416)	(0.00899)	(0.00535)
Pre-trend 3	-0.00297	-0.0000387	-0.000160
	(0.00351)	(0.00914)	(0.00487)
Pre-trend 2	-0.00280	0.000821	-0.000657
	(0.00263)	(0.00919)	(0.00416)
Pre-trend 1		0.00477	0.00335
		(0.00934)	(0.00386)
ATT 0	0.00380	0.00538**	0.00308
	(0.00267)	(0.00272)	(0.00388)
ATT 1	0.00586*	0.00429	0.00308
	(0.00308)	(0.00333)	(0.00519)
ATT 2	0.00686**	0.00768**	0.00679
	(0.00335)	(0.00385)	(0.00591)
ATT 3	0.00871**	0.0138***	0.0153**
	(0.00358)	(0.00433)	(0.00647)
ATT 4	0.00800**	0.0121**	0.0139*
	(0.00384)	(0.00496)	(0.00784)
ATT 5	0.00853**	0.0103*	0.00681
	(0.00405)	(0.00553)	(0.00810)
ATT 6	0.0138***	0.0181***	0.0143
	(0.00433)	(0.00625)	(0.0100)
ATT 7	0.0178***	0.0263***	0.0216**
	(0.00457)	(0.00687)	(0.0104)
ATT 8	0.0263***	0.0175**	0.0149
	(0.00486)	(0.00747)	(0.0118)
ATT 9	0.0271***	0.0188**	0.0191
	(0.00508)	(0.00821)	(0.0128)
ATT 10	0.0258***	0.0254***	0.0241*
	(0.00615)	(0.00873)	(0.0138)
N	518717	365335	362664

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

TABLE 4.8. Event study of Conservative vote for the transition to outright homeownership, excluding renters

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	-0.0138	0.00460	0.000563
	(0.00862)	(0.00710)	(0.00808)
Pre-trend 9	-0.0117	0.00866	0.000291
	(0.00778)	(0.00796)	(0.00941)
Pre-trend 8	-0.0119*	0.00670	-0.000621
	(0.00710)	(0.00848)	(0.00740)
Pre-trend 7	-0.00520	0.0116	0.00374
	(0.00622)	(0.00872)	(0.00739)
Pre-trend 6	-0.00415	0.0123	0.00284
	(0.00538)	(0.00907)	(0.00655)
Pre-trend 5	-0.0133***	0.00372	-0.00823
	(0.00487)	(0.00936)	(0.00592)
Pre-trend 4	-0.00729*	0.00763	0.00230
	(0.00423)	(0.00961)	(0.00537)
Pre-trend 3	-0.00251	0.00918	-0.000469
	(0.00355)	(0.00984)	(0.00481)
Pre-trend 2	-0.00258	0.00976	-0.00101
	(0.00265)	(0.00997)	(0.00464)
Pre-trend 1		0.0137	0.00318
		(0.0102)	(0.00406)
ATT 0	0.00359	0.00477*	0.00254
	(0.00267)	(0.00277)	(0.00374)
ATT 1	0.00533*	0.00355	0.00237
	(0.00313)	(0.00343)	(0.00496)
ATT 2	0.00577*	0.00666*	0.00567
	(0.00346)	(0.00398)	(0.00615)
ATT 3	0.00749**	0.0126***	0.0135*
	(0.00375)	(0.00451)	(0.00700)
ATT 4	0.00659	0.0111**	0.0121
	(0.00408)	(0.00517)	(0.00790)
ATT 5	0.00701	0.00936	0.00469
	(0.00437)	(0.00579)	(0.00952)
ATT 6	0.0114**	0.0178***	0.0120
	(0.00473)	(0.00655)	(0.0109)
ATT 7	0.0158***	0.0261***	0.0181
	(0.00505)	(0.00725)	(0.0122)
ATT 8	0.0234***	0.0171**	0.0111
	(0.00542)	(0.00791)	(0.0139)
ATT 9	0.0237***	0.0192**	0.0135
	(0.00573)	(0.00875)	(0.0143)
ATT 10	0.0245***	0.0242***	0.0183
	(0.00683)	(0.00930)	(0.0149)
N	405007	251625	252337

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

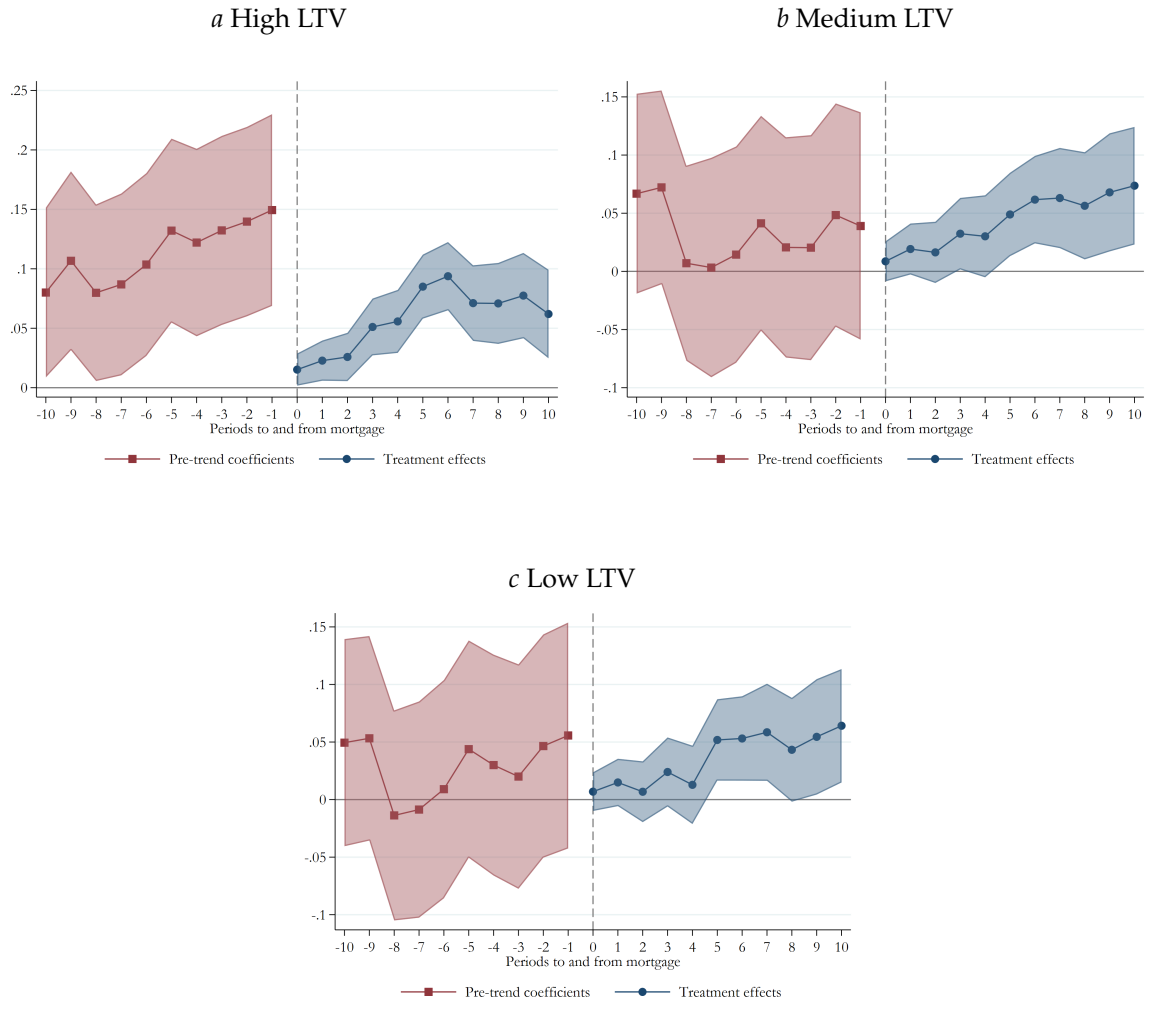
4.4.3 Heterogeneity across degrees of indebtedness

As for Chapter 3, the average results reported in the previous sections can be analysed along relevant dimensions of heterogeneity. One salient dimension that distinguishes between types of mortgagor is the level of indebtedness. To gauge this, I compute the LTV ratio at mortgage origination for all those individuals with a mortgage. I then divide them in three groups: *high* LTV for a ratio greater than 0.8, *medium* for a ratio greater than 0.6 but lower or equal to 0.8, and *low* for a ratio lower than or equal to 0.6. For this exercise I report only the results from the BJS estimator, but using the same group of never-treated individuals as counterfactual for all three groups. The event study is not repeated for outright homeowners when excluding renters from the counterfactual, as the estimation did not converge for some of the categories (due to the low sample size). Figure 4.8 contains the event studies for participation in general elections following the transition to mortgaged homeownership. Panel *a*, for those with high LTV, is the closest to the average result shown in Panel *b* of Figure 4.2, showing large positive anticipation effects as well as large(r) treatment effects. Those with medium and low LTV ratios have instead much less pronounced anticipation effects, as well as lower treatment effects. The high LTV individuals seem to be the ones most dissimilar from the counterfactual group and thus with the least reliable estimates of treatment effects. McCartney (2021) finds that in times of house price decline, high LTV households tend to decline most their electoral participation. While this result is not directly comparable to mine, it is interesting to not that in my results such a group is the most dissimilar in terms of trends before the event of tenure transition.

Figure 4.9 studies instead the effect of taking on a mortgage on the probability of voting the Conservative Party. The results are all rather close to the average in Panel *b* of Figure 4.3, indicating no treatment effect, with the low LTV group the one with pre-trends tightest around 0. Turning to heterogeneity of the transition to outright homeownership, Figure 4.10 suggests rather different outcomes based on the LTV ratio. Those with high LTV are the closest to the average results of Panel *b*

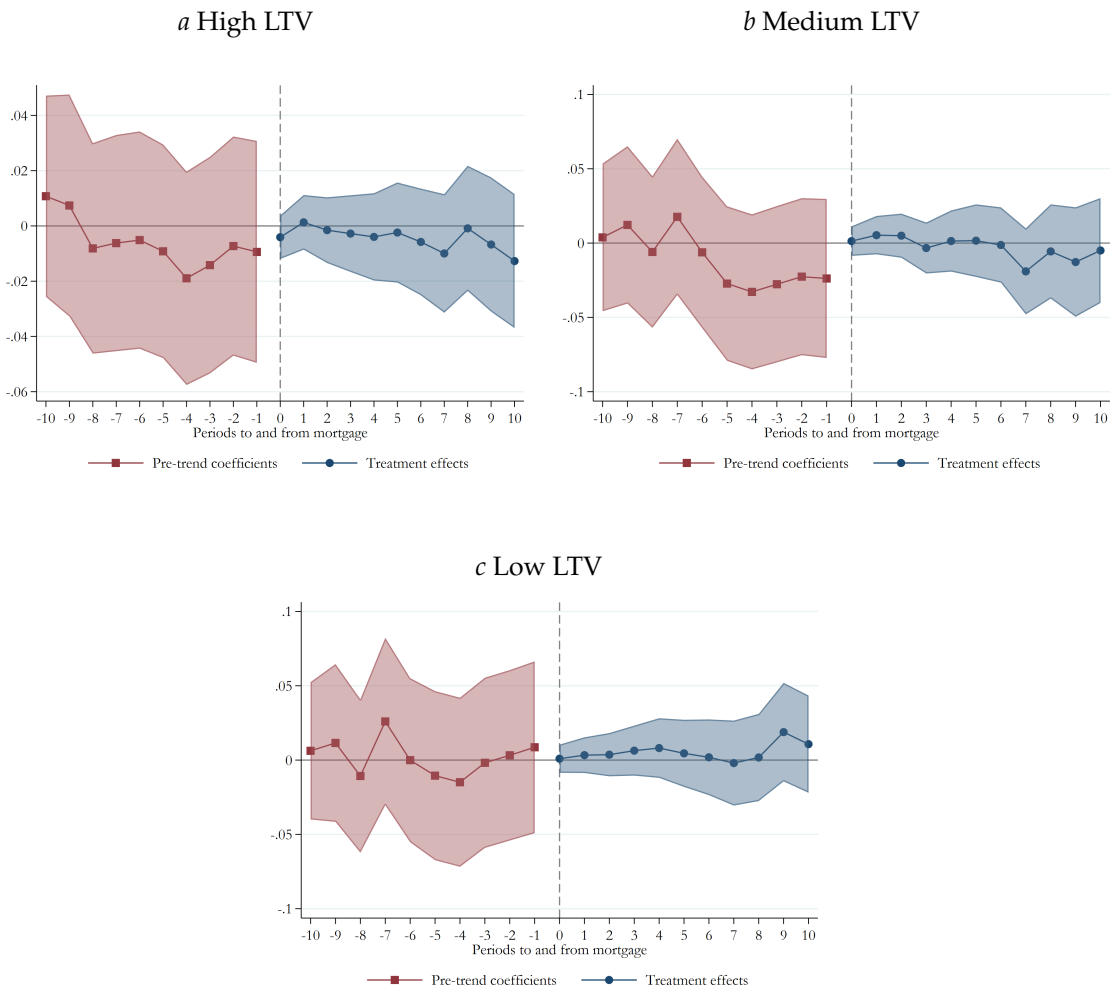
of Figure 4.4, showing a temporary drop in participation following the transition, while those with medium and especially low LTV show no drop and an eventual increase, although with sizeable anticipation effects that cast doubt on the treatment effect estimate. Finally, Figure 4.11 confirms the general pattern of Panel *b* of Figure 4.6, but highlighting how this is most pronounced among those with high LTV ratios and least pronounced among those with medium LTV ratios.

FIGURE 4.8. Event studies of vote participation for the transition to mortgaged homeownership, by LTV



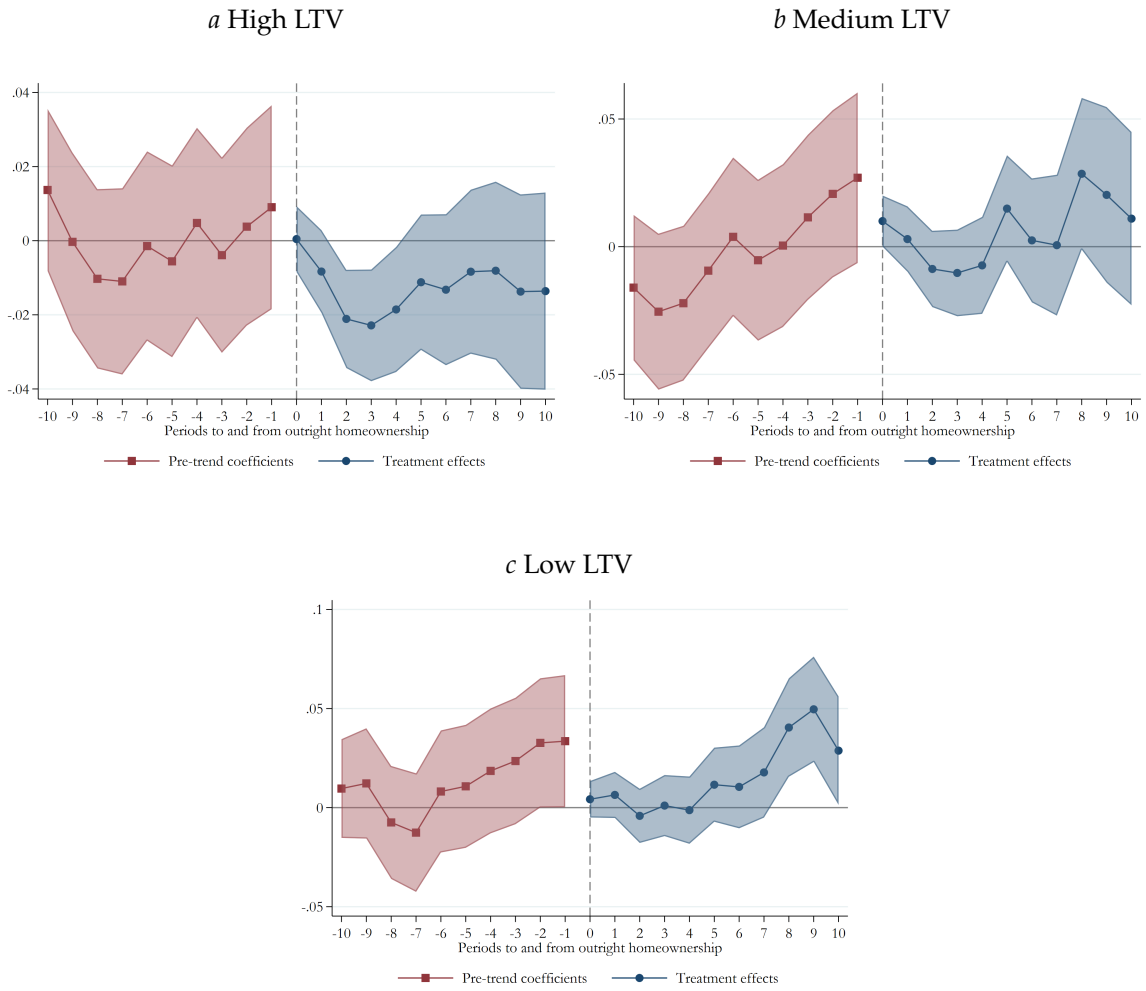
Note: Event studies in terms of obtaining a mortgage for individuals with high (Panel *a*), medium (Panel *b*) and low (Panel *c*) LTV ratios. All estimations follow the approach of Borusyak et al. (2022).

FIGURE 4.9. Event studies of Conservative vote for the transition to mortgaged homeownership, by LTV



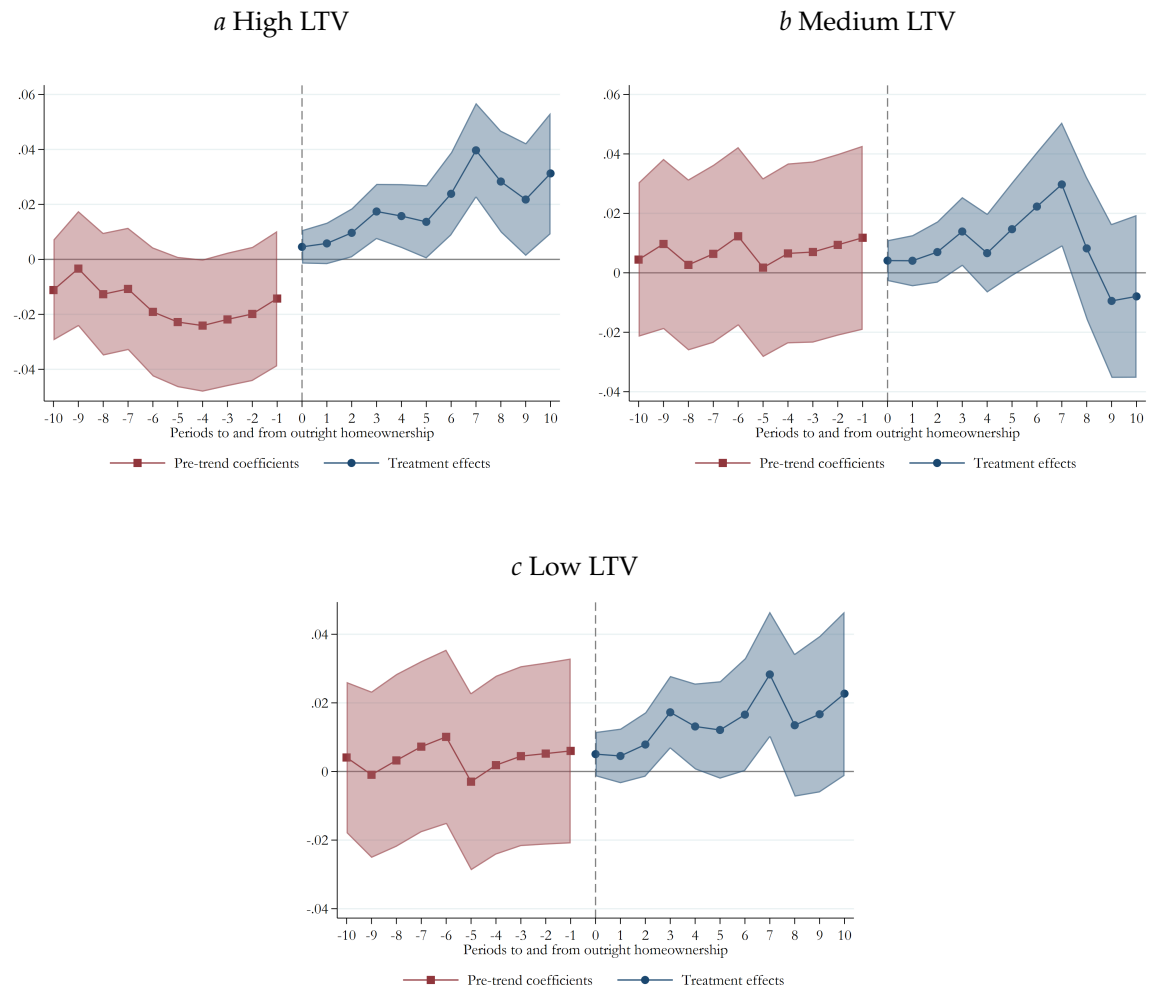
Note: Event studies in terms of obtaining a mortgage for individuals with high (Panel *a*), medium (Panel *b*) and low (Panel *c*) LTV ratios. All estimations follow the approach of Borusyak et al. (2022).

FIGURE 4.10. Event studies of vote participation for the transition to outright homeownership, by LTV



Note: Event studies in terms of becoming an outright homeowner for individuals with high (Panel *a*), medium (Panel *b*) and low (Panel *c*) LTV ratios. All estimations follow the approach of Borusyak et al. (2022).

FIGURE 4.11. Event studies of Conservative vote for the transition to outright homeownership, by LTV



Note: Event studies in terms of becoming an outright homeowner for individuals with high (Panel *a*), medium (Panel *b*) and low (Panel *c*) LTV ratios. All estimations follow the approach of Borusyak et al. (2022).

4.5 Conclusion

This chapter adds to the literature on housing tenure and voting behaviour in general elections by framing the tenure transition as an event, exploiting recent econometric advances in the event study literature, and differentiating between the transition to mortgaged homeownership and that to outright homeownership. In particular, while the topic of housing tenure and voting behaviour has featured extensively in the literature, there are very few examples of the application of an event study methodology and they are limited to the US and to local elections. Moreover, even those works that study housing tenure outside an event study framework, tend not to distinguish mortgagors from outright homeowners. My results, suggesting that the consequences of the two transitions may be meaningfully different, provide novel inputs to the scholarly debate in this respect too.

Theoretically, the connection between housing tenure and elections is sharper at the local level: local ballots are immediately consequential to one's living environment and this has widely different implications based on tenure. Yet, the focus of this chapter was placed on national elections. At this level, housing tenure may be connected to both participation in national elections and to the direction of the vote. For a host of reasons, potentially linked also to local elections, homeowners are expected to be more likely to participate in general elections, as well as to vote for the Conservative Party.

I provide evidence that the transition to mortgaged homeownership may affect participation positively, an effect best identified among those with medium LTV ratios, while it appears not to affect the Conservative vote. On the other hand, the transition to outright homeownership appears to lower participation in those with high LTV ratios and to increase the Conservative vote, especially among those with high LTV ratios. Some of the results fit well with the theoretical predictions, chiefly the increase in participation following the transition to mortgaged homeownership. It is less apparent why, instead, a positive effect on the Conservative vote appears only following the transition to outright homeownership and not the one to mortgaged

homeownership. Moreover, the reason why outright homeowners should decrease their participation following the transition is unclear. These two results, taken together, may be explained by a selection effect among those who reduce their participation in elections: if such a reduction is concentrated among those who did not vote Conservative, then the positive effect of the transition to outright homeownership on the Conservative vote would be compositional. These open questions are offered as a contribution to spur further research.

The main limitation of this chapter lies in the frequency of the data. While individuals in the panel are surveyed approximately on a yearly basis, general elections in the UK occur at a variable frequency but no more than 5 years apart[1]. This implies that for many individual-wave combinations there is a constant answer on both participation in general elections and party voted for between any two elections. Depending on the timing of tenure transition, such data on voting behaviour may not actually capture effects from the tenure transition. Moreover, while pre-trends, especially for the CS estimation, are clustered around 0, there may still be concerns of confounders affecting both housing tenure and voting behaviour. This is a further limitation that could be addressed by introducing covariates through, for instance, propensity scores in the inverse probability weighted estimator within the CS framework.

[1] See https://www.parliament.uk/about/how/elections-and-voting/general/, accessed on 24/03/2023.

CHAPTER 5

General conclusions and outlook

TENURE is a crucial aspect of the lived experience of individuals and households. It encompasses material as well as intangible aspects and it produces far-reaching and multi-faceted consequences. This thesis studied the economic and social consequences of housing tenure by drawing theoretical and empirical insights from the literature and building a conceptual framework within which to investigate the question empirically, using large surveys from the United Kingdom. To identify such effects of housing tenure, I mostly relied on the variation generated by tenure transitions, which are salient events in households' and individuals' lives, and delineating close connections with the statistical features of event studies, including with recent advances in the relevant literature. Among the consequences of housing tenure, the thesis is restricted to three separate but interlinked domains: portfolio choice, mental health and voting behaviour.

Chapter 1 introduced the fundamental features of housing tenure that are consequential for many different outcomes, as well as discussing housing tenure transitions as events and their statistical implications. Here the foundations were laid to understand why three seemingly disparate realms, such as those of portfolio choice, mental health and voting behaviour, are indeed connected by housing tenure. Chapter 2 looked at the implications of tenure transitions for the share of stockholdings in a portfolio, while also expanding on other events (the purchase of investment property) and on the *intensive margin* of housing, that is variations in the share of housing value in a portfolio. Chapter 3 delved into the differential paths of mental health following the transition to mortgaged homeownership and to outright homeownership, and

also focused a large section on exploring whether the cross-sectional gradient in mental health across tenures was affected by the COVID-19 pandemic. Chapter 4 was concerned with the event study of tenure transitions for two dimensions of voting: participation in general elections and vote for the Conservative Party.

What are, therefore, the consequences of mortgaged homeownership? A household transitioning from renting to mortgaged homeownership does not appear to alter its share of risky assets (stockholdings) in the financial portfolio, since the share after the transition follows the same counterfactual trend as before it. This indicates that the transition does not alter the household's risk aversion and runs counter to the theoretical insight that, because of the adjustment costs involved in becoming a homeowner, risk aversion should be maximal right after the transition. In fact, the additional results around portfolio choice, both at the extensive and at the intensive margin, suggest contrasting roles for adjustment costs: the fact that the share of risky assets is sensitive to the purchase of investment property downplays the importance of adjustment costs, while the apparent lack of a relationship between variations in the share of property and in the share of risky assets for Buy-to-Let property, and the presence thereof for owner-occupied property, lends credibility to the significance of adjustment costs. At the same time, an individual who transitions from renting to mortgaged homeownership is estimated to have no change, or possibly a worsening, in mental health following the transition, without much heterogeneity across degrees of indebtedness. This result too goes against the hypothesis positing that the transition to mortgaged homeownership should imply a decrease in mental distress because of increased social status and ontological security (the intangible channel). One explanation for this could be that the negative effect of the material channel of committed expenditure that comes with a mortgage, offsets completely or even exceeds the positive effect of increased social status and ontological security that accompanies homeownership. If this was the case, it would mean that the relative changes in the degree of enjoyment of the rights of *disposition* and of *possession* that come with the transition to mortgaged homeownership are not, cumulatively, particularly consequential for mental health. In fact, if the increase in committed

expenditure experienced with this transition were important for mental health, one could have expected greater variation across degrees of indebtedness, which instead does not occur. Another explanation is that in fact both channels are muted and the transition just does not affect mental health much, as was also found in a large part of the previous literature. Transitioning to mortgaged homeownership also involves an increased likelihood of participation in general elections, especially for those with medium LTV ratios, but it does not affect the probability of voting the Conservative Party. This result is partly in line with the hypothesis of both higher probability of participating in general elections and of voting for the Conservative Party, born out of the insights from theory. It stresses how the bundle of characteristics that come with becoming a mortgagor pushes individuals to become more involved in voting, possibly because of the higher stakes in property assets (driven by the right of *disposition*), but also because of residential stability (driven by higher transaction or adjustment costs). Yet, the fact that the Conservative Party generally represents more strongly the interests of homeowners does not push mortgagors to an increased likelihood of switching their allegiance after the transition.

Consequences become more pronounced once mortgagors transition to outright homeownership. When households transition to outright homeownership, their share of risky assets starts to increase. Most likely, this is an *income effect* following the termination of the committed expenditure represented by the mortgage. By having more income at their disposal every month, households not only increase the absolute amount put in risky assets, but in fact increase their share in the overall portfolio. In other words, it is as if households become less risk-averse upon becoming outright homeowners. Interestingly, the end of the committed expenditure represented by mortgage payments would appear to be also the cause of the start of a downward trajectory in mental distress, of a decrease in participation in general elections and of an increase in the probability of voting for the Conservative Party. The right of *disposition*, arguably at the source of mortgage payments, appears therefore as critical for the transition to outright homeownership. While in terms of mental health this can be easily rationalised as the end of the committed expenditure from the

material channel weighting on households' budgets, it less clear what mechanism should drive the voting behaviour results. Potentially, one could argue that the increased security connected to the end of the committed expenditure, also plausibly responsible for the decrease in mental distress and the increase in the investment in risky assets, could induce individuals to be less concerned with what politics can do for them, and therefore to participate less in voting. If the decrease in voting participation is especially concentrated among those who did not vote Conservative, then, because of the change in composition, the probability to vote Conservative would increase following the transition, even though individuals, on average, do not in fact become more likely to vote Conservative once they become outright homeowners.

The fact that the transition to outright homeownership appears so much more consequential than that to mortgaged homeownership can be traced back to the debate over the causal versus compositional effects of tenure, especially lively for what concerns mental health. The contemporary relevance of such a debate is underscored by the results of Section 3.5, showing how the gradient in mental health across tenures is entrenched and resistant to large, multi-faceted shocks such as the COVID-19 pandemic. It would appear as if, in general, the transition to mortgaged homeownership occurs for a selected group of individuals. That is, those who remain renters indefinitely and those who become mortgagors are rather different groups of individuals. Because of this, those that in the cross-section appear as effects of being a mortgagor are in fact largely a product of selection into the mortgagor category. On the other hand, the changes observed after the transition to outright homeownership are more convincingly causal. Indeed, most mortgagors should become outright homeowners at some point in the future (the very fact that they obtained a mortgage in the first place should be an indication of their likelihood of repaying back said mortgage), so there should be substantially less selection into outright homeownership to start with. This does not mean that selection cannot occur at this stage: Forrest and Hirayama (2018) discuss, in an intergenerational perspective, how homeowners in what they call the era of *late homeownership* can be separated in those who accumulate and in

those who dissipate housing wealth. The latter category includes aging homeowners that, even after entering retirement, have not paid off their mortgage.

In fact, a first contribution of this thesis to the literature is empirical and consists of the insights on the differences between mortgaged and outright homeownership with respect to portfolio choice, mental health and voting behaviour. While the degree to which the respective literatures had explored this issue varies, the two types of homeownership tend to be lumped together across the board. The broader conceptual framework through which this emerges is the second contribution of the thesis: I offer a novel lens through which to look at tenure transitions as events. In this respect too, the existing literature counted some instances of studying tenure transitions as events, but without fully exploring the implications of such a framework and not in a systematic manner. A strictly related, third contribution is that of approaching the empirical application of this framework by using recent advances in the event study literature, which has identified major flaws with established methods. Since the progress in this area of statistics has been fast-paced and concentrated in the past few years, there is still only a relatively small number of studies which apply these new methodologies.

Many questions remain unanswered and are connected to the limitations of my research method. These are also promising avenues for future research. One such question concerns how these results change for those who enter, exit and enter again a tenure, or that transition in an opposite direction as those I considered, for instance becoming a renter after mortgaged homeownership. Borusyak et al. (2022) discuss an extension of their imputation estimator for the case of "treatment switching on and off". Crucially, this setting requires an additional assumption, namely that there is no contamination between the periods of treatment and those of no treatment. When applied to this thesis case, this means that one should assume that for those who, for instance, become homeowners but then go back to renting, there is no effect of having been homeowners once they become renters again. Such an assumption is unlikely to hold in practice. Wood et al. (2017) show how those who drop out of ownership

are a very selected group, and so are those who then return (Ong ViforJ et al., 2021), strongly suggesting that periods of homeownership and periods of renting, for *churners*, are not independent of each other. A related question is about those whose tenure does not fall in the three main categories I consider, or intermediate tenures (Monk and Whitehead, 2011), such as shared equity, low-cost ownership, discounted market rental housing, cost renting. Since these tenures are aimed at filling the gap between social renting and owner-occupation, one could guess, in light of the framework of this thesis, that also their consequences could lie between those of the main tenure categories. Yet this is far from obvious, not least because the group falling in intermediate tenures is also highly selected. The thesis does not investigate either notable events that may occur together with the tenure transition, and that may therefore act as a confounder of the main estimated effect. While it is unlikely for most events to occur systematically at the same time as a tenure transition, it would be important to study how, for instance, employment changes around the beginning of a mortgage may affect the relationship between the tenure transition and an outcome variable, or how retirement may confound the effect of the transition from mortgaged to outright homeownership. Further, it would be worthwhile to investigate, as an additional axis of heterogeneity, how the effect of a tenure transition may vary between households or individuals that had already experienced multiple transitions in their past, and household or individuals that did not.

Of course, another extension of this work could go in the direction of applying the event study methodology to, and connect theoretically, more outcomes. Prominent ones include labour market outcomes (Coulson et al., 2022), various forms of social capital beyond general elections (Leviten-Reid and Matthew, 2018), such as involvement in the local community, and of course physical health outcomes (Pledger et al., 2019). Moreover, I did not study the geographical dimension of the phenomenon, which may matter substantively (Ramond and Oberti, 2022): belonging to the same housing tenure in rural Scotland or in central London could lead to different outcomes. Another potential axis of heterogeneity, apart from space, could be time. While I focus on the average effect of tenure over long periods (for the BHPS/US, the

data span around thirty years from 1991 to the pandemic period), the consequences of being a mortgagor were likely different at the end of the 90s and during the global financial crisis, for instance. In fact, marked evolutions in the conditions in which housing tenure occurs are argued to give rise to separate homeownership *eras* (Forrest and Hirayama, 2018).

The findings of this thesis can provide valuable insights for policy. The UK has a long tradition of consequential housing policy, with common traits across the country but also a degree of geographical fragmentation (McKee et al., 2017). While discretionary sales of council houses had been steadily happening even before the Housing Act of 1980, its introduction of the *Right to Buy* scheme brought about an unprecedented rise in homeownership (Mullins and Murie, 2006). The Right to Buy was amended in subsequent years but continued to be in place. In 2013, a similarly sounding but different programme, the *Help to Buy*, was introduced to facilitate first-time buyers in the purchase of their homes. The scheme was considered generally successful in providing stimulus to the British housing market (Finlay et al., 2016), and such large swings in tenure composition would suggest, based on the results of this thesis, major aggregate consequences in relation to portfolio choice, mental health, and voting behaviour. While the verification of these hypotheses is outside the scope of the thesis, there are some apparent ways in which my work can speak to overarching policy questions on housing: the results on mental health, for instance, can help identifying ways in which housing policy and public health policy can be integrated towards a "tenure neutral" approach (Stewart, 2005). Less extensive policy interventions have a tenure angle to them too, such as the "bedroom tax", also of 2013, removing a spare room subsidy for social renters to align the sector more to the private rental one (Nowicki, 2018). In this respect too, one can trace a connection to some of the central themes of my thesis: the implications of housing tenure for material and ontological security, since the bedroom tax increased the probability of falling behind with housing payments and of eviction for social tenants (Nowicki, 2017), but also for voting behaviour, most notably with reference to support for UKIP and to the success of the *Leave* campaign in the referendum on Brexit (Fetzer, 2019).

What does the future hold for housing tenure and its consequences? Part of recent discussions have focused on what shifting tenure compositions will mean for the elderly population of the future. In the UK the share of mortgagors in their mid-30s to mid-40s has decreased substantially in the past two decades, while the share renting has increased (ONS, 2020). This raises questions around what it could mean to have a substantially larger share of the elderly population in rental accommodation or with outstanding mortgages. Outright owner-occupation in old age served as a shield from regular housing payments and as a store of wealth for possible smoothing of consumption (and possibly as a way to increase housing supply through "downsizing", though the term does carry less nuance than it would be warranted by the lived experience of the elderly (Burgess and Quinio, 2021)). Discussions about the shrinking of such a "double-dividend" in old age are not unique to the UK (Slaymaker et al. (2022); Forrest and Hirayama (2018)). As argued in this thesis, this could have sizeable repercussions. As an example, based on the steady decrease in mental distress observed following the reaching of outright homeownership, one could speculate that mental health for the elderly might worsen substantially as the share of outright homeowners in this age group falls. One avenue of intervention is that of providing more information to elderly people both in terms of housing options and of personal finance, especially to those who are more vulnerable and "reactive" when it comes to housing (Burgess and Morrison, 2016). Yet these discussions assume that the main tenure types and their characteristics will remain largely unaltered (while allowing for fluctuations in socio-tenurial polarisation (Tunstall, 2021a)). Another area of debate explores instead ways to converge towards a state in which tenure matters less. As recently as in the wake of the COVID-19 pandemic, there have been calls for renewed effort towards housing affordability and tenure neutrality (Delclós and Vidal, 2021). Intuitively, a state of tenure neutrality should involve a convergence in the fundamental features of housing tenure, closing the gap in the presence of transaction or adjustment costs, and in the enjoyment of the rights of *possession*, *use* and *disposition*. Can this be achieved by changes in housing finance alone? Ball (1983) argued already that housing finance reforms alone cannot in themselves be of much

effect because they represent changes in accounting conventions and not in the social relations underpinning them. A more radical restructuring of housing provision is called for, such as breaking the link between one's need and ability to pay, which could have a greater chance of actually achieving tenure neutrality. Contemporary discourse, nevertheless, tends to indeed focus on housing finance. Two examples for the UK include Smith et al. (2013), discussing the potential of equity finance in transforming homeownership, and Baxter-Clow et al. (2022), proposing ways to shrink the private rental sector, including by extending access to homeownership through shared or cooperative ownership models. As uncertain as their incisiveness may be, the policy outcomes of current debates will determine to what extent the gap in the fundamental features of tenure can be closed, shaping in turn what the economic and social consequences of housing tenure will be in the time to come.

Statement on collaboration

The work within Chapter 2 is based on a collaboration with Prof. Franz Fuerst. Prof. Fuerst worked together with me on the conceptualisation and research design of the part of the chapter concerned with studying the intensive margin of housing, primarily Section 2.3, as well as parts of the introduction and of Section 2.1 and 2.2, participating also in the interpretation of the analyses' results (primarily Sections 2.4.2 and 2.4.3) and in the revision of the paper draft.

The idea for these sections of Chapter 2 started in October 2018 in parallel with the beginning of my PhD thesis, as a possible first chapter. In November 2018, Prof. Fuerst and I decided to develop the idea together by applying for funding within the Cambridge Humanities Research Grant Scheme, with Prof. Fuerst as the Principal Investigator, and me as a research assistant. Our project obtained funding that was used throughout 2019. In 2019, 2020 and the first half of 2021, alongside our meetings for the supervision of the thesis, we continued working on the project that culminated in a working paper available at SSRN (Felici and Fuerst, 2022), whose first version dates back to July 2021. I remain responsible for leading the research, including in terms of the literature review, theoretical framework, analysis, interpretation and writing. The work has been presented by me at the Department of Land Economy's Early Career Researcher Conference (May 2019), at the European Network for Housing Research Conference (August 2019), at a Savills internal seminar (February 2020) and at the European Real Estate Society Annual Conference (June 2021), while it was presented by Prof. Fuerst at the European Real Estate Society Annual Conference of July 2019.

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Appendices

Appendix A: Robustness check on survey weights

TABLE A.1. *Owner-occupied*, *Investment* and *Buy-to-Let Share*, first weight specification

	(1)	(2)	(3)
Owner-occupied Share	0.180**		
	(0.0704)		
Investment Share		0.0599	
		(0.0637)	
Buy-to-Let Share			-0.00923
			(0.0815)
Owner-occupied Share Sq.	-0.00199***		
	(0.000647)		
Investment Share Sq.		-0.000929	
		(0.000857)	
Buy-to-Let Share Sq.			0.000639
			(0.00117)
Inverse Mills' Ratio	-0.768	-1.042	0.182
	(5.130)	(4.784)	(8.145)
Observations	21041	21041	19075
Adj. R-squared	0.3764	0.3744	0.3935
Household FE	YES	YES	YES
Wave FE	YES	YES	YES

Note: All three models refer to specification 2.4, varying the type of *Illiquid Share*, when the first definition of survey weights is used. Additional controls include household net worth, HRP's age band and educational attainment, and the FTSE 100 monthly performance. Standard errors (in parentheses) are bootstrapped, clustered at the household level (200 repetitions). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

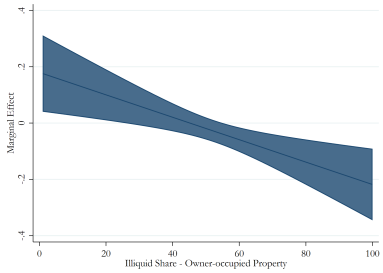
TABLE A.2. *Owner-occupied*, *Investment* and *Buy-to-Let Share*, second weight specification

	(1)	(2)	(3)
Owner-occupied Share	0.116**		
	(0.0513)		
Investment Share		0.0984*	
		(0.0562)	
Buy-to-Let Share			-0.0212
			(0.0760)
Owner-occupied Share Sq.	-0.00155***		
	(0.000481)		
Investment Share Sq.		-0.00167**	
		(0.000784)	
Buy-to-Let Share Sq.			0.000187
			(0.00118)
Inverse Mills' Ratio	-1.340	-0.549	3.620
	(4.205)	(4.441)	(6.693)
Observations	21041	21041	19075
Adj. R-squared	0.4063	0.4046	0.4235
Household FE	YES	YES	YES
Wave FE	YES	YES	YES

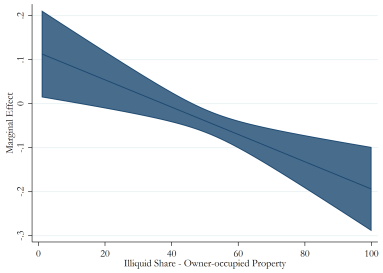
Note: All three models refer to specification 2.4, varying the type of *Illiquid Share*, when the second definition of survey weights is used. Additional controls include household net worth, HRP's age band and educational attainment, and the FTSE 100 monthly performance. Standard errors (in parentheses) are bootstrapped, clustered at the household level (200 repetitions). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE A.1. Marginal effect of the *Illiquid Share* on the *Risky Share*, with 95% CI

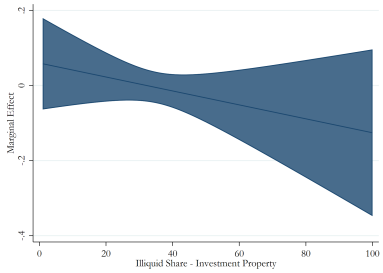
a. Marginal effect for owner-occupied case, first weight specification



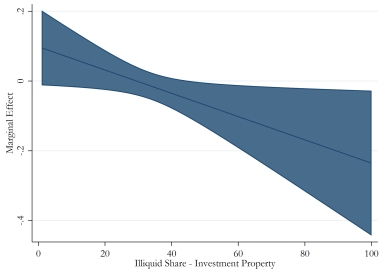
b. Marginal effect for owner-occupied case, second weight specification



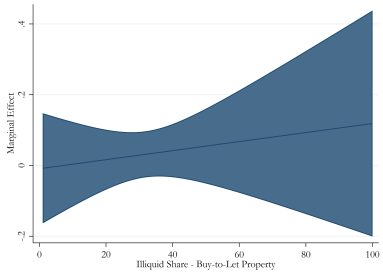
c. Marginal effect for investment case, first weight specification



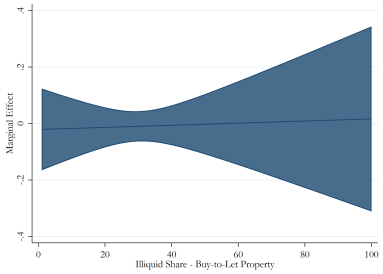
d. Marginal effect for investment case, second weight specification



e. Marginal effect for Buy-to-Let case, first weight specification



f. Marginal effect for Buy-to-Let case, second weight specification



Note: Panels *a*, *c* and *e* plot the marginal effects and 95% confidence intervals at each percentage level of the three types of *Illiquid Share*, based on model specification 2.4 when using the first definition of survey weights. Panels *b*, *d* and *f* do the same but using the second definition of survey weights. The units on the y-axis correspond to percentage point changes in the *Risky Share*.

Appendix B: Robustness check on the 12-items General Health Questionnaire

The contents of the 12-items General Health Questionnaire

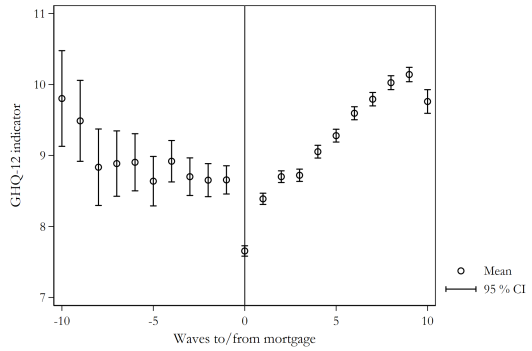
The 12-items General Health Questionnaire is composed of 12 questions with ordinal answers that vary slightly depending on the question, but in all cases assess how the respondent felt recently (specified at the beginning of the set of questions to refer to "the last few weeks"), as compared to usual. For each question, the four answers are coded 0 to 3 in order of increasing distress, and then summed together yielding an indicator ranging from 0 to 36. The 12 questions are:

- Have you recently been able to concentrate on whatever you're doing?
- Have you recently lost much sleep over worry?
- Have you recently felt that you were playing a useful part in things?
- Have you recently felt constantly under strain?
- Have you recently felt you couldn't overcome your difficulties?
- Have you recently been feeling unhappy or depressed?
- Have you recently been losing confidence in yourself?
- Have you recently been thinking of yourself as a worthless person?
- Have you recently been feeling reasonably happy, all things considered?
- Have you recently felt capable of making decisions about things?
- Have you recently been able to face up to problems?
- Have you recently been able to enjoy your normal day-to-day activities?

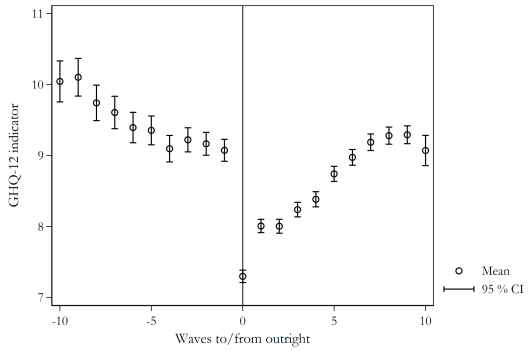
Results

FIGURE B.1. Evolution of reported mental health (GHQ) as a function of time distance from an event

a. Taking out a mortgage

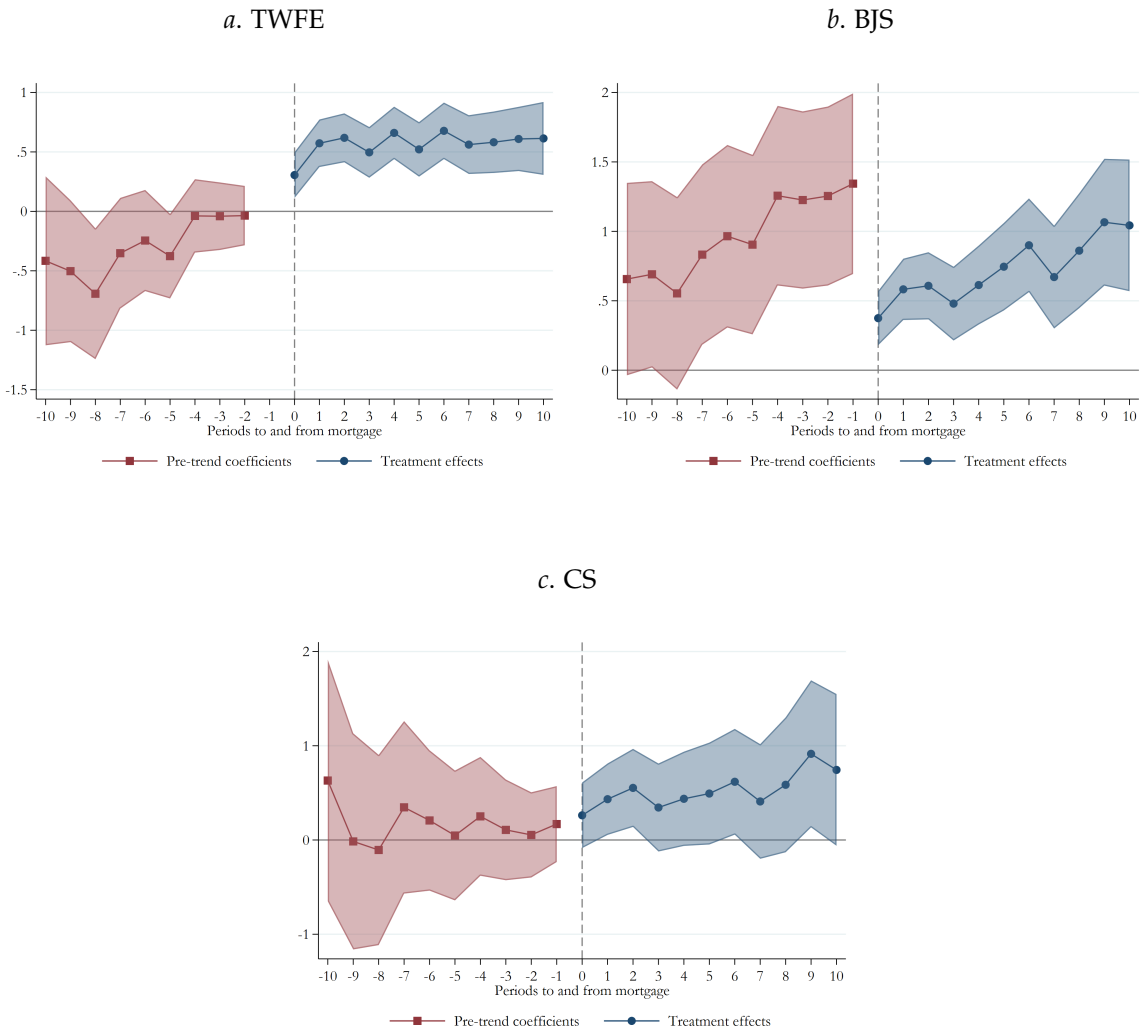


b. Becoming an outright homeowner



Note: Evolution of the mean 12-items GHQ score as a function of distance from obtaining a mortgage (Panel *a*) and from outright homeownership (Panel *b*).

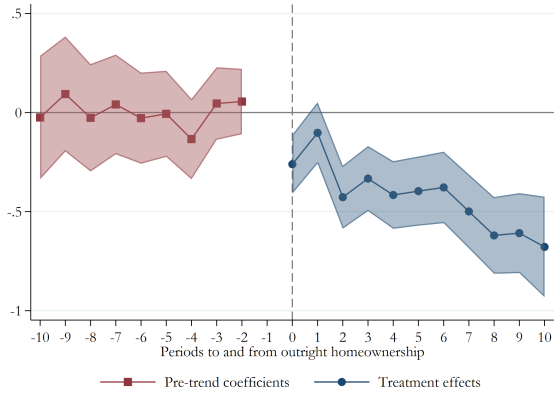
FIGURE B.2. Event studies of mental health (GHQ) for the transition to mortgaged homeownership



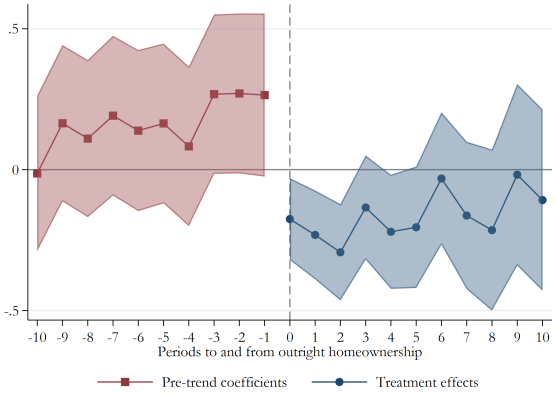
Note: Event studies for the 12-items GHQ in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE B.3. Event studies of mental health (GHQ) for the transition to outright homeownership

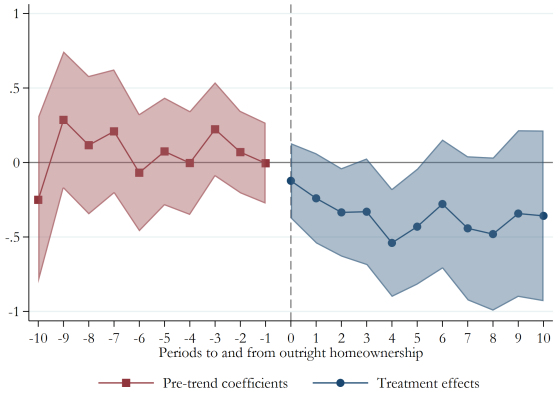
a. TWFE



b. BJS



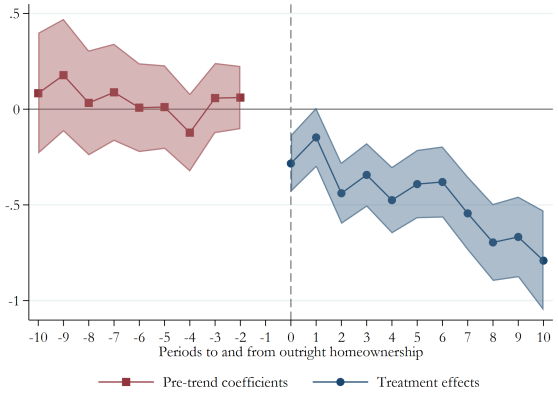
c. CS



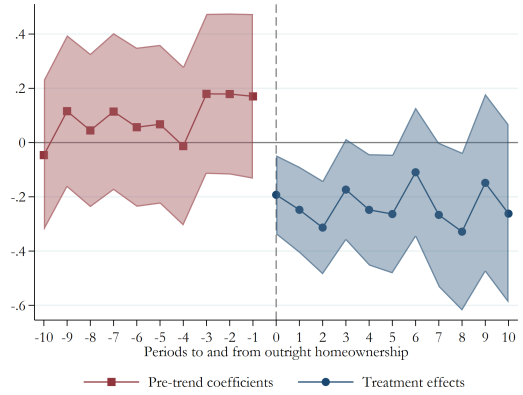
Note: Event studies for the 12-items GHQ in terms of becoming an outright homeowner, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE B.4. Event studies of mental health (GHQ) for the transition to outright homeownership, excluding renters

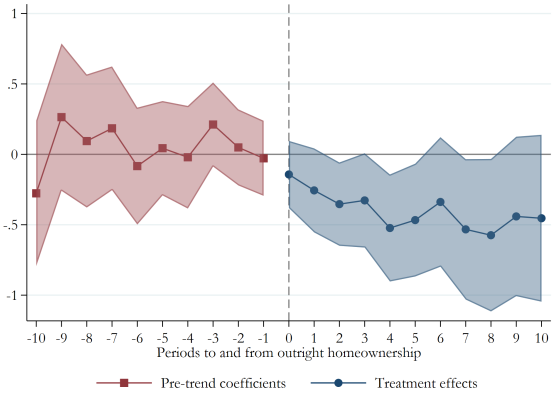
a. TWFE



b. BJS



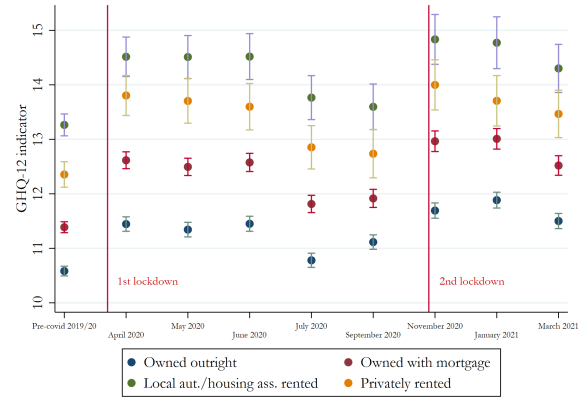
c. CS



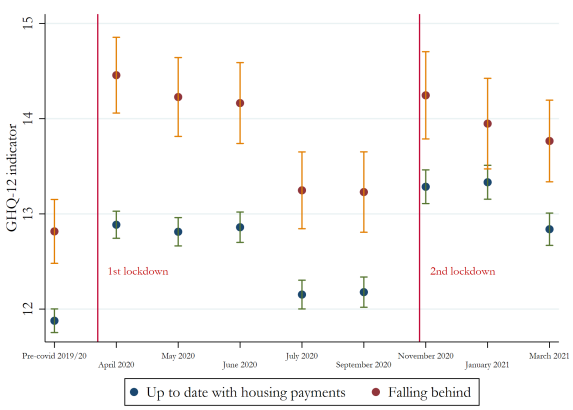
Note: Event studies for the 12-items GHQ in terms of becoming an outright homeowner and excluding renters as counterfactuals for the never-treated group, using a classic two-way fixed effects estimator (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE B.5. Evolution of reported mental health (GHQ) during the pandemic

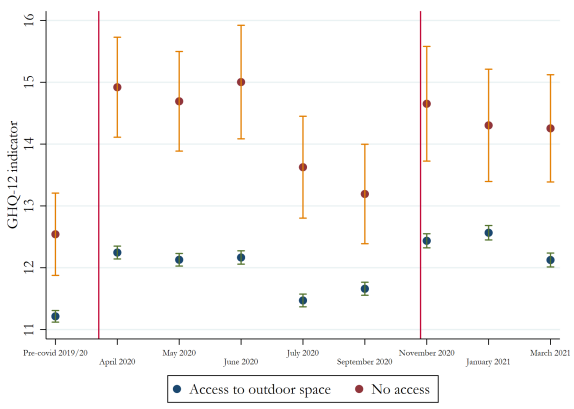
a. By housing tenure



b. By ability to keep up with housing payments



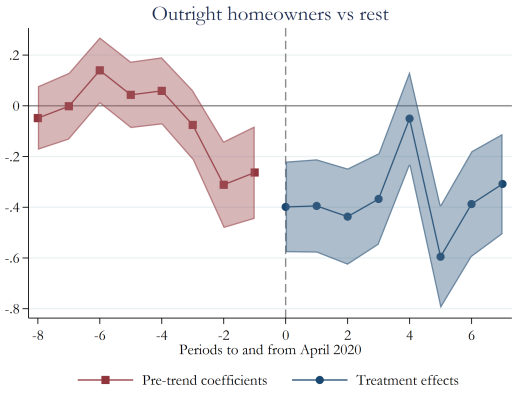
c. By access to outdoor space



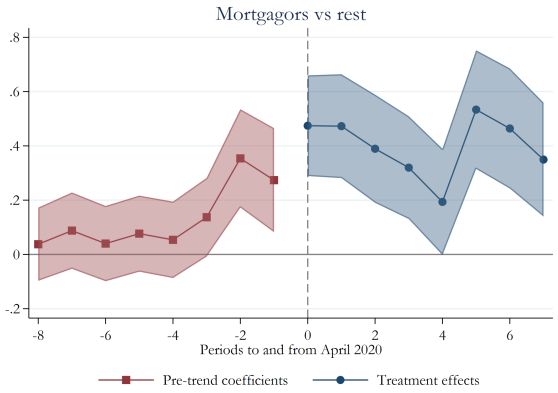
Note: Trend in mean 12-items GHQ score by housing tenure (Panel *a*), ability to keep up with housing payments (Panel *b*) and access to outdoor space (Panel *c*). Pre-pandemic averages for 2019 and part of 2020 (until March) are included as a benchmark. The bars around the point estimates represent 95 % confidence intervals.

FIGURE B.6. Event studies of mental health (GHQ) for the onset of the pandemic, by housing tenure

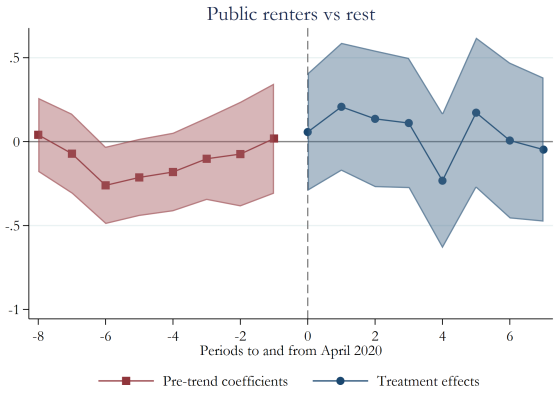
a. Outright homeowners VS rest



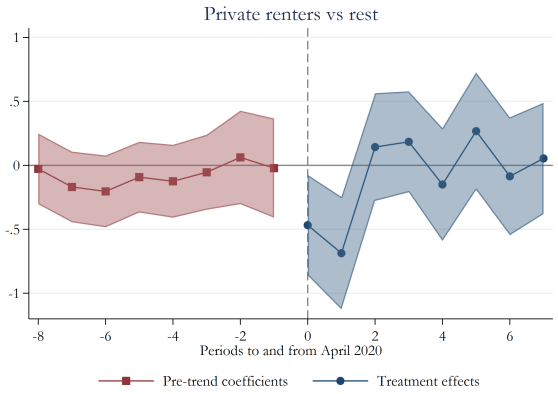
b. Mortgagors VS rest



c. Public renters VS rest



a. Private renters VS rest



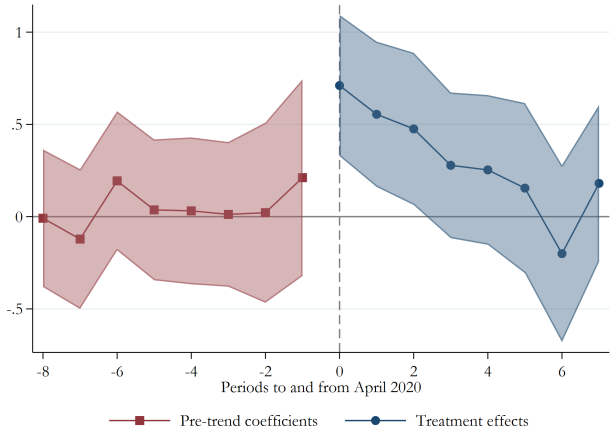
Note: Event study comparing each housing tenure category during the pandemic to the aggregate of the other tenure groups, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Shaded areas around the coefficients represent 95 % confidence intervals. The p-value for the joint test of no pre-trends is equal to 0.00009 for outright homeowners, to 0.027 for mortgagors, to 0.347 for public renters and to 0.871 for private renters.

TABLE B.1. Event studies by housing tenure

	(1) Outright homeowners	(2) Mortgagors	(3) Public renters	(4) Private renters
April 2020	-0.399***	0.474***	0.0572	-0.468**
	(0.0912)	(0.0947)	(0.179)	(0.201)
May 2020	-0.395***	0.473***	0.208	-0.687***
	(0.0939)	(0.0977)	(0.195)	(0.225)
June 2020	-0.437***	0.389***	0.136	0.143
	(0.0969)	(0.102)	(0.208)	(0.215)
July 2020	-0.367***	0.320***	0.111	0.184
	(0.0919)	(0.0967)	(0.198)	(0.201)
September 2020	-0.0505	0.194*	-0.233	-0.150
	(0.0952)	(0.0999)	(0.206)	(0.225)
November 2020	-0.595***	0.534***	0.173	0.268
	(0.104)	(0.112)	(0.229)	(0.234)
January 2021	-0.387***	0.464***	0.00671	-0.0857
	(0.106)	(0.113)	(0.237)	(0.235)
March 2021	-0.308***	0.349***	-0.0475	0.0539
	(0.101)	(0.107)	(0.219)	(0.222)
Pre-trend 1	-0.263***	0.274***	0.0185	-0.0216
	(0.0931)	(0.0978)	(0.168)	(0.198)
Pre-trend 2	-0.311***	0.354***	-0.0742	0.0619
	(0.0871)	(0.0925)	(0.159)	(0.186)
Pre-trend 3	-0.0754	0.137*	-0.102	-0.0541
	(0.0702)	(0.0740)	(0.125)	(0.150)
Pre-trend 4	0.0588	0.0538	-0.181	-0.125
	(0.0676)	(0.0719)	(0.120)	(0.145)
Pre-trend 5	0.0432	0.0766	-0.213*	-0.0923
	(0.0668)	(0.0715)	(0.117)	(0.141)
Pre-trend 6	0.140**	0.0400	-0.260**	-0.204
	(0.0665)	(0.0708)	(0.118)	(0.143)
Pre-trend 7	-0.00173	0.0877	-0.0717	-0.169
	(0.0671)	(0.0718)	(0.122)	(0.141)
Pre-trend 8	-0.0483	0.0376	0.0407	-0.0275
	(0.0639)	(0.0688)	(0.113)	(0.141)
Observations	336274	336274	336274	336274

Note: Event studies comparing each housing tenure category to the aggregate of the other three groups, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Standard errors in parentheses, clustered at the individual level over time. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE B.7. Event study of mental health (GHQ) for the onset of the pandemic, by ability to keep up with housing payments



Note: Event study comparing those who cannot keep up with housing payments to those who can during the pandemic, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence. Shaded areas around the coefficients represent 95 % confidence intervals. The p-value for the joint test of no pre-trends is equal to 0.921.

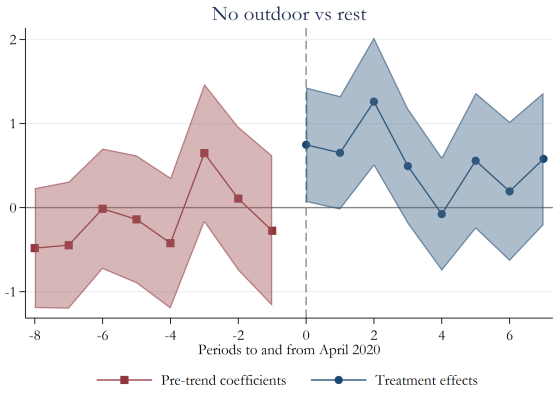
TABLE B.2. Event studies by ability to meet housing payments

	(1) Not up to date with housing payments
April 2020	0.711***
	(0.195)
May 2020	0.555***
	(0.201)
June 2020	0.476**
	(0.210)
July 2020	0.278
	(0.201)
September 2020	0.253
	(0.207)
November 2020	0.155
	(0.235)
January 2021	-0.200
	(0.245)
March 2021	0.180
	(0.217)
Pre-trend 1	0.211
	(0.272)
Pre-trend 2	0.0215
	(0.249)
Pre-trend 3	0.0123
	(0.200)
Pre-trend 4	0.0314
	(0.203)
Pre-trend 5	0.0367
	(0.195)
Pre-trend 6	0.194
	(0.192)
Pre-trend 7	-0.122
	(0.193)
Pre-trend 8	-0.00846
	(0.190)
Observations	127921

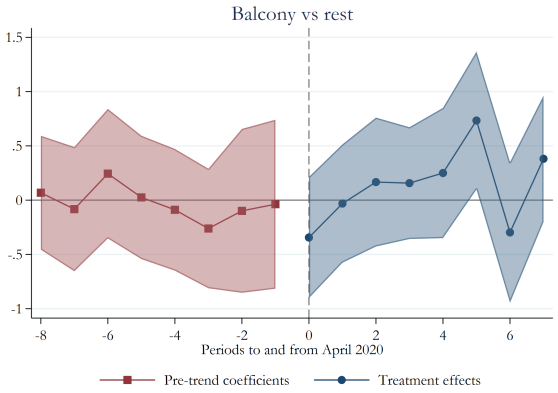
Note: Event studies comparing those who cannot keep up with housing payments to those who can during the pandemic, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence. Standard errors in parentheses, clustered at the individual level over time. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE B.8. Event studies of mental health (GHQ) for the onset of the pandemic, by access to outdoor space

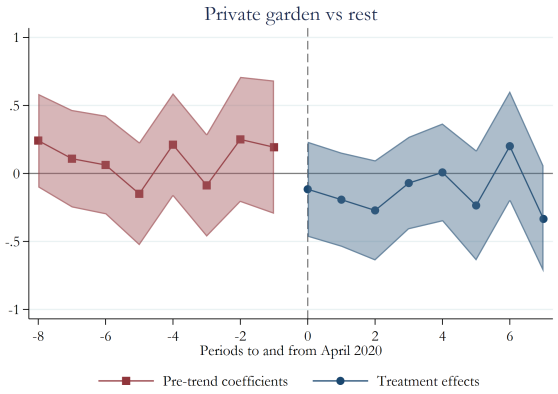
a. No outdoor VS rest



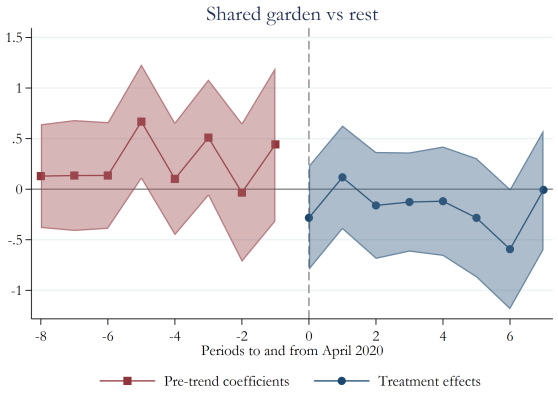
b. Balcony VS rest



c. Private garden VS rest



d. Shared garden VS rest



Note: Event study comparing each outdoor space category during the pandemic to the aggregate of the others, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Shaded areas around the coefficients represent 95 % confidence intervals. The p-value for the joint test of no pre-trends is equal to 0.105 for those without outdoor space, to 0.942 for those with a balcony, to 0.360 for those with a private garden and to 0.238 for those with a shared garden.

TABLE B.3. Event studies by access to outdoor space

	(1) No outdoor space	(2) Balcony	(3) Private garden	(4) Shared garden
April 2020	0.748**	-0.344	-0.116	-0.283
	(0.348)	(0.285)	(0.178)	(0.264)
May 2020	0.652*	-0.0312	-0.193	0.117
	(0.345)	(0.278)	(0.177)	(0.262)
June 2020	1.261***	0.167	-0.272	-0.161
	(0.390)	(0.303)	(0.188)	(0.270)
July 2020	0.494	0.157	-0.0706	-0.127
	(0.352)	(0.263)	(0.174)	(0.250)
September 2020	-0.0767	0.250	0.00768	-0.119
	(0.345)	(0.306)	(0.184)	(0.276)
November 2020	0.558	0.734**	-0.235	-0.283
	(0.413)	(0.324)	(0.207)	(0.301)
January 2021	0.194	-0.297	0.201	-0.593*
	(0.423)	(0.330)	(0.207)	(0.304)
March 2021	0.580	0.381	-0.334*	-0.00714
	(0.402)	(0.297)	(0.200)	(0.303)
Pre-trend 1	-0.275	-0.0374	0.193	0.443
	(0.457)	(0.397)	(0.250)	(0.389)
Pre-trend 2	0.107	-0.0985	0.250	-0.0333
	(0.437)	(0.385)	(0.235)	(0.351)
Pre-trend 3	0.648	-0.261	-0.0878	0.509*
	(0.421)	(0.281)	(0.193)	(0.294)
Pre-trend 4	-0.423	-0.0889	0.211	0.102
	(0.398)	(0.286)	(0.194)	(0.285)
Pre-trend 5	-0.139	0.0257	-0.150	0.668**
	(0.388)	(0.290)	(0.194)	(0.290)
Pre-trend 6	-0.0136	0.244	0.0624	0.135
	(0.366)	(0.304)	(0.185)	(0.269)
Pre-trend 7	-0.446	-0.0826	0.109	0.135
	(0.386)	(0.292)	(0.183)	(0.280)
Pre-trend 8	-0.481	0.0684	0.242	0.129
	(0.364)	(0.268)	(0.176)	(0.262)
Observations	199363	199363	199363	199363

Note: Event studies comparing those who have access to outdoor space to those who have not, controlling additionally for age, tenure group, gender and highest qualification. Periods before April 2020 are waves with approximate yearly cadence, while starting from April 2020 they have monthly cadence and finally bimonthly from July 2020. Standard errors in parentheses, clustered at the individual level over time. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C: Robustness check on the imputation method for voting behaviour

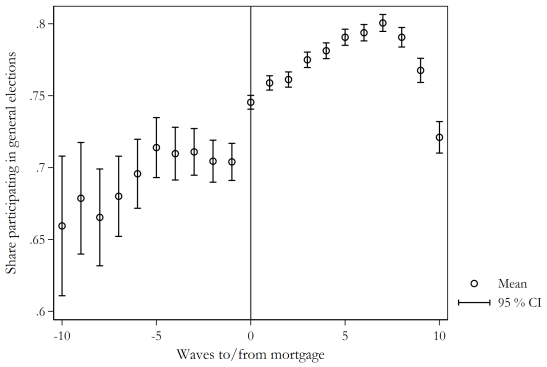
TABLE C.1. Summary statistics of salient variables by housing tenure

	(1) Outright homeownership		(2) Mortgage		(3) Rent	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Share participating in general elections	0.870	0.337	0.775	0.417	0.654	0.476
Share voting for the conservative party	0.381	0.486	0.302	0.459	0.181	0.385
Age	60.137	16.482	40.429	13.039	43.564	18.801
Share with higher education	0.292	0.455	0.367	0.482	0.198	0.399
Observations	150942		209848		127391	

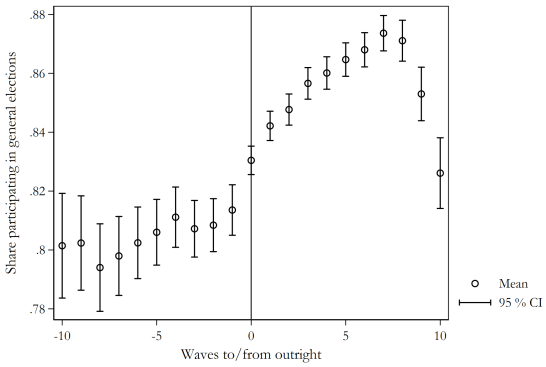
Note: Mean and standard deviation for a set of variables, by housing tenure. Variables reported as shares are binary recoding of variables with wider ranges.

FIGURE C.1. Evolution of voting shares as a function of time distance from an event

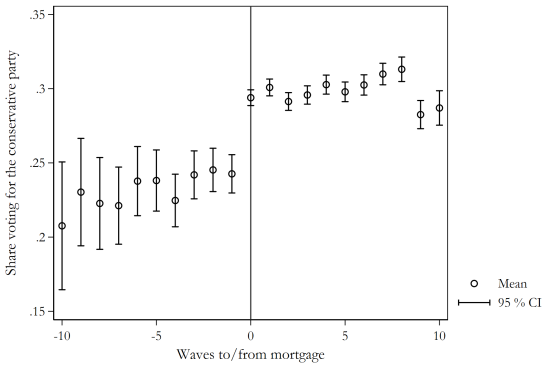
a. Share participating for the event of taking out a mortgage



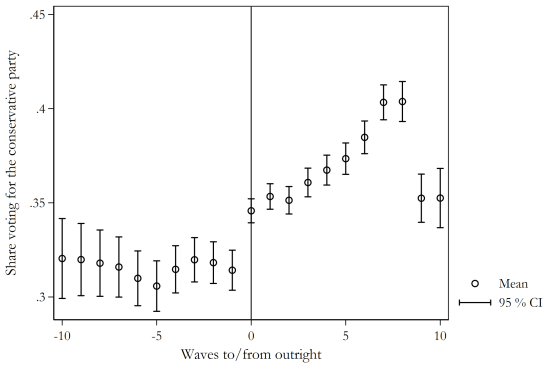
b. Share participating for the event of becoming an outright homeowner



c. Share voting Conservative for the event of taking out a mortgage



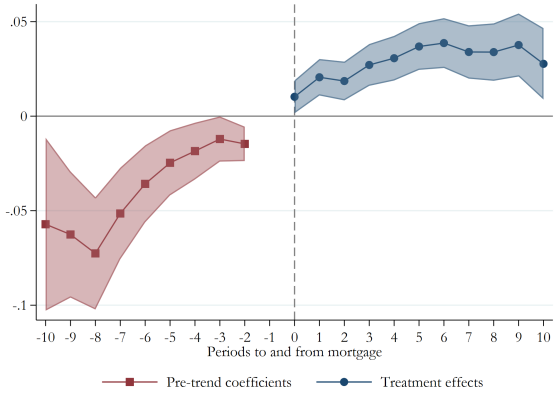
b. Share voting Conservative for the event of becoming an outright homeowner



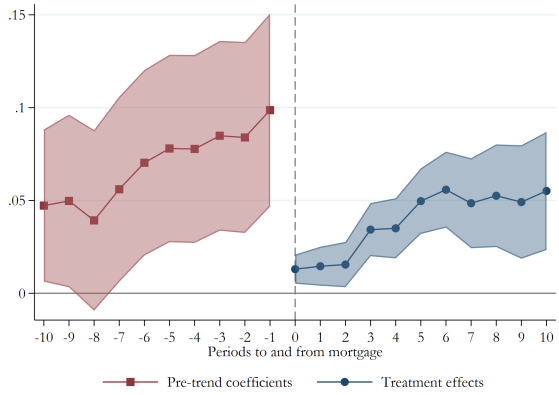
Note: Evolution of the share participating in general elections (Panels *a* and *b*) and of the share voting the Conservative Party (Panels *c* and *d*) as a function of distance from obtaining a mortgage and from outright homeownership.

FIGURE C.2. Event studies of vote participation for the transition to mortgaged homeownership

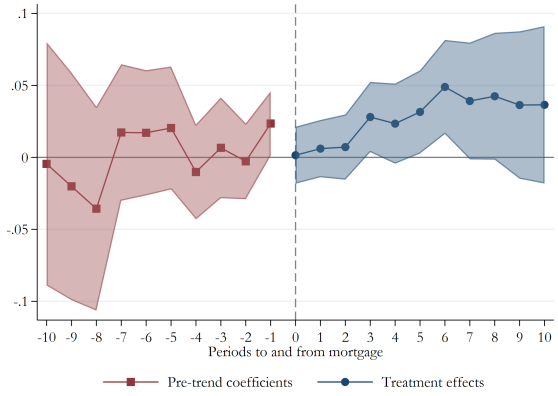
a. TWFE



b. BJS



c. CS



Note: Event studies for participation in general elections in terms of obtaining a mortgage, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

TABLE C.2. Event study of vote participation for the transition to mortgaged homeownership

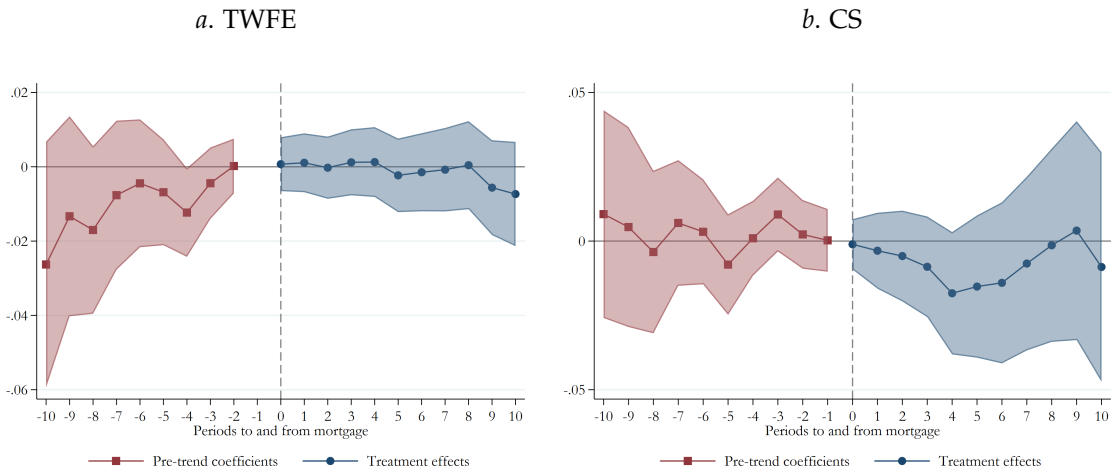
	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	-0.0572**	0.0472**	-0.00448
	(0.0233)	(0.0209)	(0.0431)
Pre-trend 9	-0.0626***	0.0497**	-0.0201
	(0.0170)	(0.0237)	(0.0403)
Pre-trend 8	-0.0726***	0.0393	-0.0356
	(0.0152)	(0.0248)	(0.0362)
Pre-trend 7	-0.0515***	0.0560**	0.0173
	(0.0124)	(0.0254)	(0.0242)
Pre-trend 6	-0.0358***	0.0703***	0.0171
	(0.0104)	(0.0255)	(0.0222)
Pre-trend 5	-0.0247***	0.0780***	0.0204
	(0.00879)	(0.0258)	(0.0218)
Pre-trend 4	-0.0185**	0.0777***	-0.0101
	(0.00766)	(0.0258)	(0.0168)
Pre-trend 3	-0.0121**	0.0848***	0.00664
	(0.00610)	(0.0261)	(0.0179)
Pre-trend 2	-0.0147***	0.0839***	-0.00274
	(0.00465)	(0.0263)	(0.0134)
Pre-trend 1		0.0987***	0.0235**
		(0.0266)	(0.0114)
ATT 0	0.0103**	0.0130***	0.00151
	(0.00446)	(0.00400)	(0.0101)
ATT 1	0.0206***	0.0146***	0.00609
	(0.00493)	(0.00535)	(0.0101)
ATT 2	0.0186***	0.0155**	0.00717
	(0.00524)	(0.00623)	(0.0116)
ATT 3	0.0271***	0.0343***	0.0281**
	(0.00565)	(0.00731)	(0.0124)
ATT 4	0.0307***	0.0350***	0.0235*
	(0.00602)	(0.00827)	(0.0142)
ATT 5	0.0369***	0.0496***	0.0316**
	(0.00631)	(0.00902)	(0.0147)
ATT 6	0.0387***	0.0558***	0.0489***
	(0.00673)	(0.0105)	(0.0167)
ATT 7	0.0340***	0.0485***	0.0392*
	(0.00720)	(0.0124)	(0.0207)
ATT 8	0.0339***	0.0525***	0.0425*
	(0.00775)	(0.0141)	(0.0225)
ATT 9	0.0376***	0.0491***	0.0364
	(0.00851)	(0.0156)	(0.0261)
ATT 10	0.0277***	0.0551***	0.0365
	(0.00964)	(0.0162)	(0.0279)
N	394255	148917	150434

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

FIGURE C.3. Event studies of Conservative vote for the transition to mortgaged homeownership



Note: Event studies for Conservative Party vote in terms of obtaining a mortgage, using a classic two-way fixed effects model (Panel *a*) and the approach of Callaway and Sant'Anna (2021) (Panel *b*). The computation for the approach of Borusyak et al. (2022) did not converge and could not be reported.

TABLE C.3. Event study of Conservative vote for the transition to mortgaged homeownership

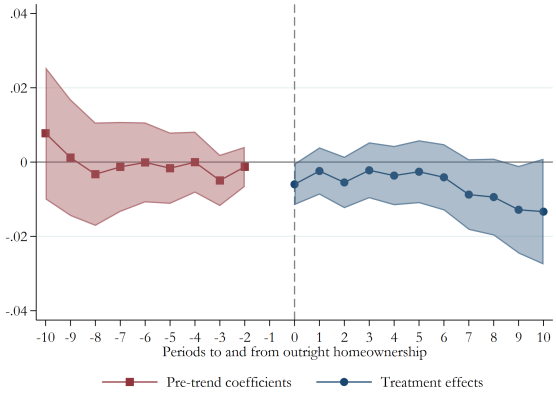
	(1) TWFE	(2) CS
Pre-trend 10	-0.0263	0.00908
	(0.0169)	(0.0178)
Pre-trend 9	-0.0133	0.00472
	(0.0137)	(0.0172)
Pre-trend 8	-0.0170	-0.00368
	(0.0115)	(0.0139)
Pre-trend 7	-0.00765	0.00610
	(0.0102)	(0.0108)
Pre-trend 6	-0.00445	0.00313
	(0.00878)	(0.00902)
Pre-trend 5	-0.00685	-0.00790
	(0.00727)	(0.00864)
Pre-trend 4	-0.0123**	0.000945
	(0.00609)	(0.00643)
Pre-trend 3	-0.00442	0.00894
	(0.00491)	(0.00635)
Pre-trend 2	0.000203	0.00224
	(0.00376)	(0.00590)
Pre-trend 1		0.000245
		(0.00538)
ATT 0	0.000705	-0.00108
	(0.00370)	(0.00432)
ATT 1	0.00108	-0.00324
	(0.00404)	(0.00651)
ATT 2	-0.000249	-0.00503
	(0.00427)	(0.00779)
ATT 3	0.00120	-0.00864
	(0.00453)	(0.00864)
ATT 4	0.00128	-0.0175*
	(0.00479)	(0.0105)
ATT 5	-0.00230	-0.0153
	(0.00505)	(0.0122)
ATT 6	-0.00147	-0.0140
	(0.00536)	(0.0138)
ATT 7	-0.000787	-0.00756
	(0.00573)	(0.0149)
ATT 8	0.000429	-0.00140
	(0.00605)	(0.0166)
ATT 9	-0.00562	0.00353
	(0.00651)	(0.0188)
ATT 10	-0.00734	-0.00871
	(0.00716)	(0.0197)
N	401665	148677

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

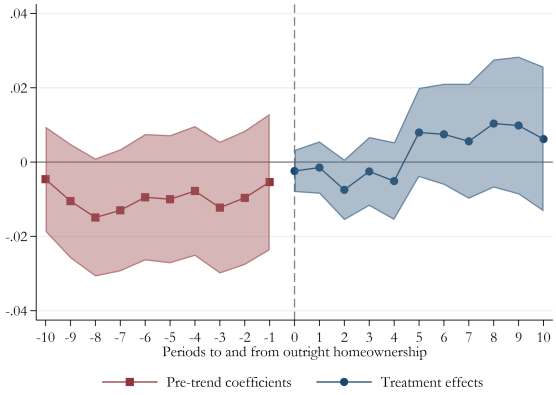
Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1) and the approach of Callaway and Sant'Anna (2021) (column 2). The computation for the approach of Borusyak et al. (2022) did not converge and could not be reported.

FIGURE C.4. Event studies of vote participation for the transition to outright homeownership

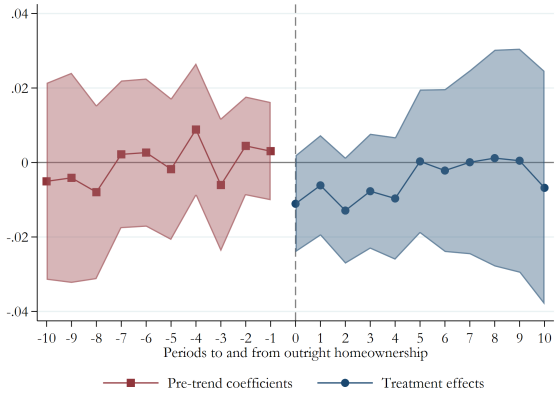
a. TWFE



b. BJS



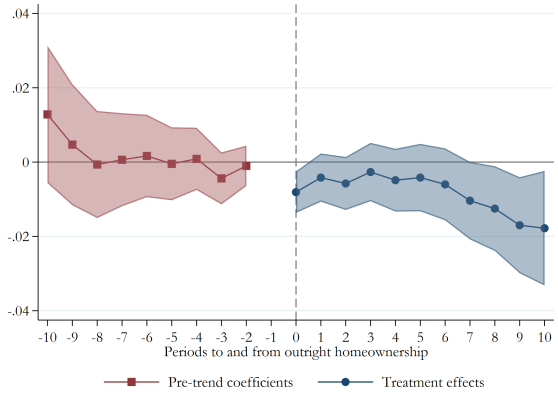
c. CS



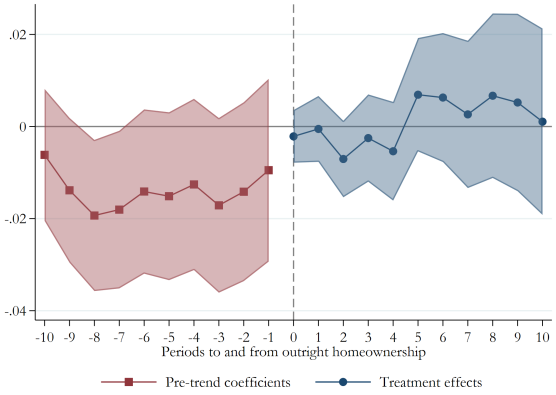
Note: Event studies for participation in general elections in terms of becoming an outright homeowner, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE C.5. Event studies of vote participation for the transition to outright homeownership, excluding renters

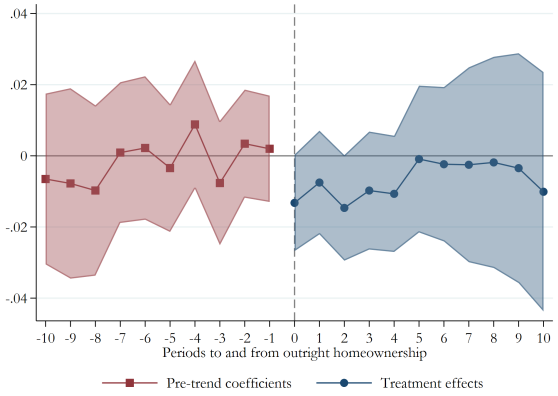
a. TWFE



b. BJS



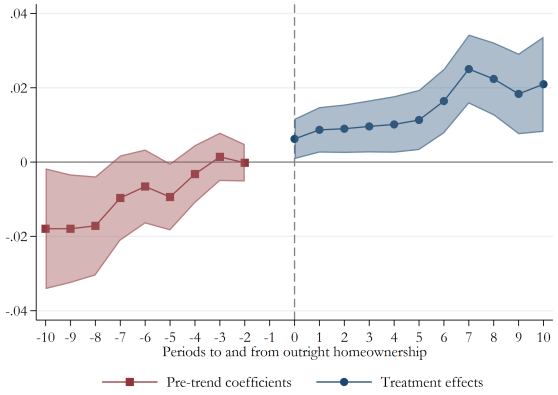
c. CS



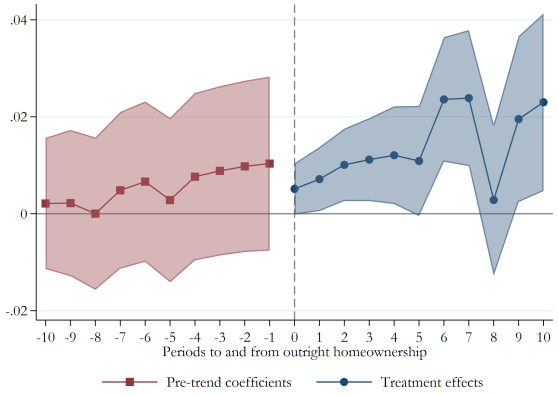
Note: Event studies for participation in general elections in terms of becoming an outright homeowner and excluding renters as counterfactuals for the never-treated group, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

FIGURE C.6. Event studies of Conservative vote for the transition to outright homeownership

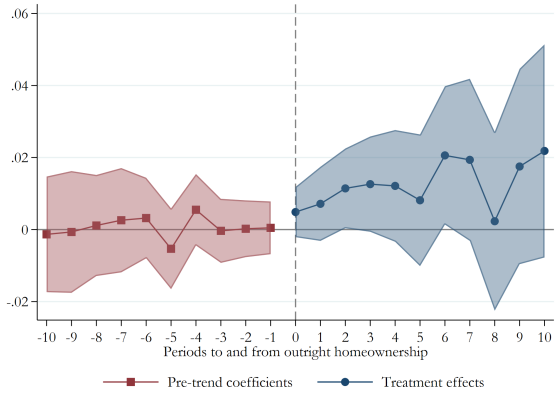
a. TWFE



b. BJS



c. CS



Note: Event studies for Conservative Party vote in terms of becoming an outright homeowner, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

TABLE C.4. Event study of vote participation for the transition to outright homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	0.00776	-0.00460	-0.00505
	(0.00909)	(0.00723)	(0.0135)
Pre-trend 9	0.00121	-0.0105	-0.00412
	(0.00803)	(0.00786)	(0.0144)
Pre-trend 8	-0.00327	-0.0149*	-0.00796
	(0.00710)	(0.00811)	(0.0119)
Pre-trend 7	-0.00130	-0.0130	0.00219
	(0.00618)	(0.00838)	(0.0101)
Pre-trend 6	-0.0000830	-0.00946	0.00266
	(0.00549)	(0.00868)	(0.0102)
Pre-trend 5	-0.00164	-0.01000	-0.00179
	(0.00490)	(0.00880)	(0.00971)
Pre-trend 4	-0.0000266	-0.00778	0.00886
	(0.00420)	(0.00892)	(0.00910)
Pre-trend 3	-0.00495	-0.0123	-0.00605
	(0.00353)	(0.00905)	(0.00913)
Pre-trend 2	-0.00128	-0.00962	0.00445
	(0.00276)	(0.00922)	(0.00677)
Pre-trend 1		-0.00537	0.00304
		(0.00937)	(0.00674)
ATT 0	-0.00598**	-0.00240	-0.0111*
	(0.00286)	(0.00289)	(0.00664)
ATT 1	-0.00240	-0.00147	-0.00613
	(0.00325)	(0.00360)	(0.00689)
ATT 2	-0.00548	-0.00746*	-0.0129*
	(0.00354)	(0.00417)	(0.00728)
ATT 3	-0.00221	-0.00253	-0.00771
	(0.00385)	(0.00474)	(0.00788)
ATT 4	-0.00364	-0.00511	-0.00966
	(0.00408)	(0.00534)	(0.00840)
ATT 5	-0.00259	0.00797	0.000295
	(0.00432)	(0.00612)	(0.00985)
ATT 6	-0.00409	0.00748	-0.00216
	(0.00455)	(0.00695)	(0.0112)
ATT 7	-0.00874*	0.00560	0.0000659
	(0.00486)	(0.00790)	(0.0126)
ATT 8	-0.00942*	0.0104	0.00118
	(0.00528)	(0.00880)	(0.0148)
ATT 9	-0.0128**	0.00984	0.000481
	(0.00602)	(0.00946)	(0.0154)
ATT 10	-0.0133*	0.00620	-0.00680
	(0.00728)	(0.00994)	(0.0160)
N	494478	365818	367462

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

TABLE C.5. Event study of vote participation for the transition to outright homeownership, excluding renters

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	0.0128	-0.00616	-0.00651
	(0.00943)	(0.00731)	(0.0122)
Pre-trend 9	0.00470	-0.0138*	-0.00776
	(0.00832)	(0.00802)	(0.0137)
Pre-trend 8	-0.000662	-0.0193**	-0.00974
	(0.00735)	(0.00838)	(0.0122)
Pre-trend 7	0.000636	-0.0180**	0.000906
	(0.00639)	(0.00873)	(0.0101)
Pre-trend 6	0.00164	-0.0141	0.00220
	(0.00566)	(0.00910)	(0.0103)
Pre-trend 5	-0.000461	-0.0151	-0.00343
	(0.00501)	(0.00930)	(0.00917)
Pre-trend 4	0.000877	-0.0126	0.00882
	(0.00427)	(0.00949)	(0.00923)
Pre-trend 3	-0.00439	-0.0171*	-0.00764
	(0.00357)	(0.00968)	(0.00892)
Pre-trend 2	-0.00101	-0.0142	0.00342
	(0.00277)	(0.00991)	(0.00776)
Pre-trend 1		-0.00949	0.00197
		(0.0101)	(0.00763)
ATT 0	-0.00810***	-0.00214	-0.0132*
	(0.00285)	(0.00292)	(0.00692)
ATT 1	-0.00418	-0.000522	-0.00751
	(0.00331)	(0.00365)	(0.00743)
ATT 2	-0.00576	-0.00706*	-0.0146*
	(0.00364)	(0.00424)	(0.00757)
ATT 3	-0.00268	-0.00251	-0.00976
	(0.00400)	(0.00484)	(0.00846)
ATT 4	-0.00488	-0.00537	-0.0107
	(0.00431)	(0.00547)	(0.00834)
ATT 5	-0.00417	0.00691	-0.000910
	(0.00463)	(0.00628)	(0.0105)
ATT 6	-0.00600	0.00630	-0.00237
	(0.00494)	(0.00715)	(0.0111)
ATT 7	-0.0104*	0.00264	-0.00251
	(0.00533)	(0.00817)	(0.0140)
ATT 8	-0.0125**	0.00669	-0.00185
	(0.00582)	(0.00911)	(0.0151)
ATT 9	-0.0170***	0.00522	-0.00347
	(0.00660)	(0.00983)	(0.0165)
ATT 10	-0.0178**	0.00105	-0.0101
	(0.00787)	(0.0103)	(0.0172)
N	380508	251848	254091

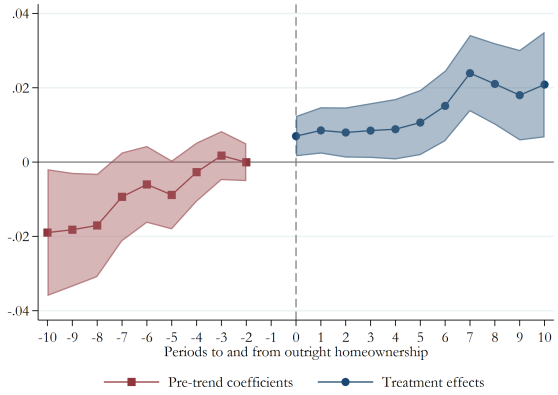
Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

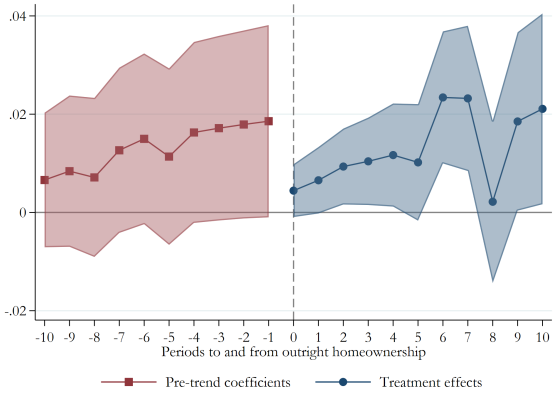
Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

FIGURE C.7. Event studies of Conservative vote for the transition to outright homeownership, excluding renters

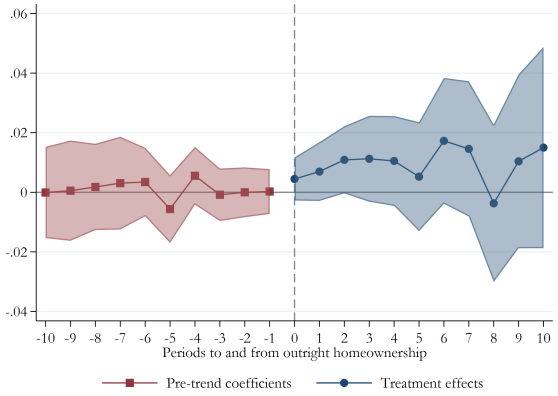
a. TWFE



b. BJS



c. CS



Note: Event studies for Conservative Party vote in terms of becoming an outright homeowner and excluding renters as counterfactuals for the never-treated group, using a classic two-way fixed effects model (Panel *a*), the approach of Borusyak et al. (2022) (Panel *b*) and the approach of Callaway and Sant'Anna (2021) (Panel *c*).

TABLE C.6. Event study of Conservative vote for the transition to outright homeownership

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	-0.0179**	0.00213	-0.00133
	(0.00829)	(0.00691)	(0.00819)
Pre-trend 9	-0.0179**	0.00220	-0.000666
	(0.00746)	(0.00770)	(0.00862)
Pre-trend 8	-0.0172**	0.0000247	0.00113
	(0.00680)	(0.00802)	(0.00716)
Pre-trend 7	-0.00967*	0.00483	0.00260
	(0.00585)	(0.00825)	(0.00739)
Pre-trend 6	-0.00657	0.00662	0.00319
	(0.00508)	(0.00844)	(0.00571)
Pre-trend 5	-0.00939**	0.00281	-0.00534
	(0.00460)	(0.00865)	(0.00572)
Pre-trend 4	-0.00322	0.00764	0.00550
	(0.00399)	(0.00881)	(0.00506)
Pre-trend 3	0.00141	0.00885	-0.000355
	(0.00333)	(0.00891)	(0.00454)
Pre-trend 2	-0.000188	0.00977	0.000227
	(0.00257)	(0.00901)	(0.00402)
Pre-trend 1		0.0104	0.000484
		(0.00915)	(0.00374)
ATT 0	0.00624**	0.00512*	0.00486
	(0.00277)	(0.00270)	(0.00356)
ATT 1	0.00868***	0.00714**	0.00715
	(0.00313)	(0.00337)	(0.00525)
ATT 2	0.00898***	0.0101***	0.0114**
	(0.00333)	(0.00382)	(0.00565)
ATT 3	0.00960***	0.0112**	0.0126*
	(0.00358)	(0.00437)	(0.00675)
ATT 4	0.0101***	0.0121**	0.0121
	(0.00389)	(0.00514)	(0.00792)
ATT 5	0.0113***	0.0109*	0.00813
	(0.00413)	(0.00580)	(0.00933)
ATT 6	0.0164***	0.0236***	0.0206**
	(0.00444)	(0.00656)	(0.00980)
ATT 7	0.0250***	0.0239***	0.0194*
	(0.00475)	(0.00716)	(0.0115)
ATT 8	0.0224***	0.00284	0.00231
	(0.00502)	(0.00798)	(0.0127)
ATT 9	0.0184***	0.0195**	0.0175
	(0.00555)	(0.00873)	(0.0138)
ATT 10	0.0210***	0.0230**	0.0218
	(0.00654)	(0.00935)	(0.0151)
N	517285	373284	370163

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).

TABLE C.7. Event study of Conservative vote for the transition to outright homeownership, excluding renters

	(1) TWFE	(2) BJS	(3) CS
Pre-trend 10	-0.0190**	0.00661	-0.0000645
	(0.00871)	(0.00699)	(0.00782)
Pre-trend 9	-0.0182**	0.00841	0.000544
	(0.00782)	(0.00786)	(0.00859)
Pre-trend 8	-0.0170**	0.00713	0.00179
	(0.00710)	(0.00826)	(0.00738)
Pre-trend 7	-0.00936	0.0127	0.00307
	(0.00611)	(0.00858)	(0.00794)
Pre-trend 6	-0.00601	0.0150*	0.00344
	(0.00528)	(0.00887)	(0.00588)
Pre-trend 5	-0.00884*	0.0114	-0.00561
	(0.00474)	(0.00916)	(0.00581)
Pre-trend 4	-0.00272	0.0163*	0.00555
	(0.00407)	(0.00939)	(0.00494)
Pre-trend 3	0.00175	0.0172*	-0.000830
	(0.00337)	(0.00958)	(0.00449)
Pre-trend 2	-0.0000511	0.0179*	-0.00000583
	(0.00259)	(0.00976)	(0.00427)
Pre-trend 1		0.0186*	0.000257
		(0.00998)	(0.00384)
ATT 0	0.00699**	0.00444	0.00447
	(0.00277)	(0.00274)	(0.00369)
ATT 1	0.00853***	0.00655*	0.00696
	(0.00319)	(0.00345)	(0.00504)
ATT 2	0.00796**	0.00934**	0.0109*
	(0.00345)	(0.00394)	(0.00575)
ATT 3	0.00848**	0.0104**	0.0112
	(0.00376)	(0.00454)	(0.00736)
ATT 4	0.00885**	0.0117**	0.0105
	(0.00415)	(0.00535)	(0.00769)
ATT 5	0.0106**	0.0102*	0.00524
	(0.00448)	(0.00605)	(0.00934)
ATT 6	0.0151***	0.0234***	0.0173
	(0.00486)	(0.00686)	(0.0108)
ATT 7	0.0239***	0.0232***	0.0146
	(0.00525)	(0.00754)	(0.0116)
ATT 8	0.0210***	0.00218	-0.00371
	(0.00560)	(0.00844)	(0.0135)
ATT 9	0.0180***	0.0185**	0.0104
	(0.00622)	(0.00926)	(0.0149)
ATT 10	0.0209***	0.0211**	0.0150
	(0.00725)	(0.00990)	(0.0172)
N	401966	257965	257848

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Event studies in terms of obtaining a mortgage, using a classic two-way fixed effects estimator (column 1), the approach of Borusyak et al. (2022) (column 2) and the approach of Callaway and Sant'Anna (2021) (column 3).