The Impact of Universal Credit Rollout on Housing Insecurity in the UK

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Submitted in fulfilment of the requirements of the Degree of Doctor of Philosophy

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Abstract

A country's welfare system can have a profound impact on the housing security of its citizens. Welfare systems which include adequate provision of housing allowances can act as a 'buffer' that prevents an automatic association between persistent low-income, or a sudden loss of income, and loss of housing. In the UK, Universal Credit (UC) has been rolling out gradually since 2013 to replace six working-age means-tested benefits with a single payment. This represents a major change to the welfare system, and its design of long wait periods, increased conditionality and direct payments has led to concerns over housing security impacts.

Using quantitative research methods, this thesis exploits cross-area variation in the timing of UC rollout (arising from the fact that it was introduced in different areas at different times – a form of natural experiment) in order to measure its impacts on housing insecurity. This is carried out by linking data on the timing of UC rollout (at the local authority level) with panel data from administrative/survey datasets on housing insecurity indicators over time.

Housing insecurity for financial reasons can occur in four, increasingly severe, stages: (1) difficulties meeting rent payments, (2) build up of rent arrears, (3) legal repossession actions by landlord, and (4) threatened or actual homelessness. This thesis involves four separate empirical studies, with each making use of different data sources to examine UC's impact on different indicators and stages of insecurity. Empirical studies 1 and 2 are fixed effects panel designs examining the impact of UC rollout, respectively, on rates of landlord repossession actions, and advice sought from Citizens Advice on rent arrears/homelessness issues, within English local authorities. Empirical study 3, which is also a fixed effects panel design, examines the impact of UC rollout on rates of 'Housing Options' approaches and official homelessness claims within Scottish local authorities. Finally, empirical study 4 is a difference-in-differences analysis of data from the 'Understanding Society' survey, examining the impact of UC rollout on household financial problems.

Overall, the results suggest that, up to 2019, UC rollout was associated with increases in rates of household problems paying for housing/bills/council tax payments, rent arrears advice issues, landlord repossession actions and 'Housing Options' approaches. Taken together, these results provide a robust indication that UC rollout has weakened the UK welfare system's ability to provide housing security to low-income households.

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Acknowledgements

The research carried out in this thesis was funded by the Economic and Social Research Council (ESRC) (grant number: ES/P000681/1).

I would like to say a massive thank you to Nick Bailey and Ken Gibb for giving me the opportunity to undertake this PhD and for their excellent supervision and support throughout the course of the research. Their feedback and guidance have been invaluable, and I have been very fortunate to have them as my supervisors.

A big thanks also to those from the Citizens Advice data team, the Scottish Government homelessness analysis and statistics team, and the UK Data Service helpdesk who provided me with help and support in accessing the datasets that were used for the research in this thesis.

I am also very grateful to various others who have provided me with valuable feedback on my research at various stages. This includes Glasgow University staff/students who gave me feedback at various presentations/annual reviews, those who anonymously reviewed my research for publication in the Journal of Social Policy, and those who gave me feedback at the ESPAnet Conference 2019 in Stockholm and the WPEG Conference 2019 in Sheffield.

Finally, I would like to thank Emily, my parents, and the rest of my family/friends for all the moral support and encouragement throughout.

Author’s Declaration

I declare that, except where explicit reference is made to the contribution of others, that this dissertation is the result of my own work and has not been submitted for any other degree at the University of Glasgow or any other institution.

Name: Iain Hardie

Signature:

Abbreviations

ALMP	Active Labour Market Policy
APA	Alternative Payment Arrangement
CAB	Citizens Advice (aka Citizens Advice Bureau)
CSJ	Centre for Social Justice
DiD	Difference-in-Differences
DWP	Department for Work and Pensions
HB	Housing Benefit
HMRC	Her Majesty's Revenue and Customs
JSA	Jobseekers Allowance
LA	Local Authority
LHA	Local Housing Allowance
MHCLG	Ministry of Housing, Communities and Local Government
NRS	National Records of Scotland
ONS	Office for National Statistics
PRS	Private Rented Sector
RCT	Randomised Control Trial
RTI	Real Time Information
SRS	Social Rented Sector
UC	Universal Credit
UCFS	Universal Credit 'Full Service'
UKHLS	UK Household Longitudinal Study (aka Understanding Society)

Chapter 1. Introduction

1.1 Welfare Reform, Universal Credit and Housing Insecurity

The type of welfare system or welfare regime in place in a country plays an important role in the provision of housing security to its citizens. If a welfare system provides housing allowances that adequately reduce/cover housing costs – or alternatively, provides below-cost rents through social housing – then this will limit the extent to which sudden income shocks (e.g. job loss) or longer-term labour market precarity (e.g. low-paid, low hours or insecure employment) automatically result in loss of housing (Stephens and Fitzpatrick, 2007; Stephens, Fitzpatrick, Elsinga *et al.*, 2010).

In the UK specifically, both social housing and housing allowances via the welfare system are targeted at low-income households to help with the management of housing costs. This system is said to play a significant role in protecting households against eviction for financial reasons, and to provide a safety net for those whose housing security might otherwise be threatened because of their economic status (Pleace and Hunter, 2018, p. 336). However, since the 1980s access to social housing has become more restricted, with a marked shift away from 'producer subsidies' for "bricks and mortar" (i.e. central housing subsidies to local government) towards 'consumer subsidies' (i.e. rent allowances via Housing Benefit and Local Housing Allowance) (Lund, 2017). At the same time, welfare reforms in the UK in recent decades (for unemployment benefits in particular but also more broadly) have sought to reduce the value of social security benefits and make them more conditional on claimants meeting certain behavioural requirements, with a particular emphasis on 'active labour market policies' (ALMPs) (Kenway, 2009; Hamilton, 2014; Millar, 2018; Watts and Fitzpatrick, 2018).

The latest key development in UK welfare reform is the introduction of Universal Credit (UC). UC has been gradually rolling out to overhaul the UK's working-age means-tested benefit system by replacing six previous benefits (income-based Jobseekers Allowance, income-related Employment and Support Allowance, Income Support, Working Tax Credit, Child Tax Credit and Housing Benefit - now referred to as 'legacy' benefits) with a single UC payment. The key principles that motivated the UC reform were: (a) to simplify the welfare system, (b) to improve financial work incentives, (c) to increase conditionality, (d)

to make claiming welfare as 'like work' as possible (via a contract-like 'Claimant Commitment' and salary-like payment system), and (e) to better match up with the UK's flexible labour market (see Department for Work and Pensions [DWP] 2010a; Millar and Bennett, 2017). Through implementing these principles, UC sought to improve employment outcomes by tackling what was perceived to be a 'culture of worklessness' and 'welfare dependency', with the legacy benefits system said to 'trap' people in poverty and dependency by failing to incentivise work (see Brewer, Browne and Jin, 2011, pp. 49-50; Patrick, 2017, p. 36).

The rollout of UC to date has generated a great deal of controversy. In particular, the government have been criticised for having a single-minded focus on improving employment outcomes at all costs, for dismissing any evidence of claimant hardship, and for failing to work with others to establish an evidence base on UC's impacts (Alston, 2018; National Audit Office, 2018). Empirical literature on the impacts of UC has provided mixed evidence on UC's labour market impacts (Department for Work and Pensions, 2017a; Department for Work and Pensions, 2018b), but has tended to consistently suggest it has had negative wider impacts on claimant hardship. For example, quantitative research examining the impacts of UC so far have suggested that its rollout has been associated with increased debt (Drake, 2017), food bank usage (Reeves and Loopstra, 2020), psychological distress (Wickham, Bentley, Rose *et al.*, 2020), and crime linked to worsening financial conditions amongst claimants (d'Este and Harvey, 2020).

With regards to UC's impacts on housing insecurity, which is the focus of this thesis, the government initially set out how UC aimed to "simplify provision for rent support […] as much as possible, while protecting potentially vulnerable people from unintended consequences, such as getting into arrears or being made homeless" (Department for Work and Pensions, 2010a). However, in stark contrast to this, housing and homelessness charities have raised concerns throughout UC's rollout so far that it in fact puts claimants at greater risk of rent arrears, eviction and homelessness (e.g. see Crisis, 2017; Shelter, 2017). Such concerns have tended to be particularly linked to three of UC's key design features. Firstly, unlike the legacy system where benefits tended to be paid fortnightly, UC is paid monthly in arrears in an attempt to mirror a salary in work. This means that it entails a long wait period – typically around five weeks – between initially making a claim and receiving the first payment, and as a result claimants with limited savings to fall back on will have no income to meet housing costs during this period (Shelter, 2017). Secondly, UC significantly

extends and intensifies the use of benefit sanctions to reduce or stop benefit payments for those deemed to have failed to comply with their Claimant Commitment. This establishes a new "ubiquitous conditionality" (Dwyer and Wright, 2014), and there is concern that claimants struggle to meet housing costs when their benefit payments are reduced by sanctions (Beatty, Foden, McCarthy *et al.*, 2015, p. 35). Thirdly, UC involves direct payments, i.e. UC payments are paid directly into a claimant's own bank account. This can lead to difficulties meeting housing costs amongst those who prioritise other essential or unexpected costs over rent, or amongst those who have limited budgeting skills e.g. young people without prior experience of managing a household budget (Britain Thinks, 2018; Homeless Link, 2018).

To date, several empirical studies (e.g. Smith Institute, 2017; Batty, 2018; Cheetham, Moffatt and Addison, 2018; Wright, Dwyer, Jones *et al.*, 2018) have been conducted that can provide insight into UC's housing insecurity impacts, with evidence of some claimants struggling to meet rent payments, falling into arrears and facing repossession actions. However, these pieces of research have either been qualitative studies or small-scale quantitative studies limited to specific localities. As noted by National Audit Office (2018, p. 44), throughout the rollout of UC there has been a lack of robust, national-level quantitative analysis into its impacts on the ability of claimants to meet rent payments. This thesis seeks to address this, and contribute to existing literature, by providing a quantitative analysis into the impacts of UC rollout on housing insecurity.

Universal Credit has been introduced gradually, and its rollout has varied across time and space. Both UC 'Live Service' (the early version of UC only available to the simplest types of claim – mostly for single unemployed people) and UC 'Full Service' (full version of UC available to all claimant types) were introduced in different local authority areas of the UK at different times. This cross-area variation in the timing of UC rollout can be exploited in order to measure its impacts, as it means UC can be treated as a form of 'natural experiment' i.e. a policy intervention that is not under the control of the researcher but that is amenable to research which exploits variation in exposure to the policy to analyse its impact (Craig, Cooper, Gunnell *et al.*, 2010, p. 4). Consequently, the empirical analysis of this thesis, using quantitative research methods, exploits this cross-area variation in the timing of UC rollout in order to measure its impacts on housing insecurity. This is done by linking data on the timing of UC rollout (at the local authority level) with data on various indicators of housing insecurity over time.

Housing insecurity (at least, housing insecurity for financial reasons) tends to occur in four stages, with increasing severity of insecurity at each stage. First, if a household is experiencing financial hardship, they may struggle to afford their housing costs. Second, if this problem persists, this may lead to rent arrears building up. Next, if rent arrears cannot be paid off, the landlord will likely proceed with legal repossession actions to evict the tenant. Finally, if the landlord is successful in their attempts to repossess the property, then threat of or actual homelessness occurs.

1.2 Thesis Aims and Objectives

This thesis aims to provide insight into the impacts of Universal Credit rollout on each of the four stages of housing insecurity outlined in the above discussion. Specifically, the thesis aims to make use of panel data from administrative/survey datasets on various housing insecurity indicators (at various stages of insecurity) in order to examine whether there is evidence that UC rollout has led to an increase in household financial problems, rent arrears, repossession actions and threatened/actual homelessness in the UK. Based on this, the overarching objectives of this thesis are as follows:

1. To apply causal modelling to provide robust empirical analysis into the impacts of Universal Credit rollout on the different stages of housing insecurity in the UK.

2. To draw upon this empirical analysis in order to contribute to policy debates in the UK over the ongoing rollout of Universal Credit.

1.3 Structure of Thesis

This rest of this thesis is structured as follows. First, in the following chapter (Chapter 2), a review of Universal Credit as a policy is provided. In order to understand UC's impacts, it is first important to understand the context in which UC arrived. Consequently, Chapter 2 begins by setting out social security policy pre-Universal Credit, beginning with how the modern UK social security system was formed after the 'Beveridge Report' (1942) and then discussing how the system has changed subsequently, with particular emphasis on the pattern

of welfare reforms since the 1980s. Chapter 2 then provides an overview of Universal Credit, in terms of its structure, the motivating principles that led to the reform and shaped its design, and how the policy has rolled out since 2013.

Next, Chapter 3 provides a detailed review of existing literature on the impacts of Universal Credit rollout so far. It begins by discussing the DWP's focus on improving employment outcomes through the UC reform, and considers existing empirical evidence on this, which comes from the DWP's own research as well as research by others. The chapter then goes on to review literature on UC's wider impacts on forms of hardship that have not been considered by the DWP's own research but have been by qualitative and quantitative studies by others. This includes outcomes such as mental health and wellbeing, incomes and debt, and food bank usage. Chapter 3 ends by introducing, in detail, the concept of housing insecurity, providing theory on how UC is likely to effect insecurity and reviewing existing empirical evidence on its impacts.

Chapter 4 provides an overview of the data and methodological approach of the thesis's empirical analysis. This begins by justifying the use of quantitative over qualitative research methods, whilst also describing how the research questions addressed are informed by (and interpreted in the context of) existing qualitative studies on UC's housing insecurity impacts. The chapter then goes on to justify the use of secondary data for the analysis by making use of existing administrative/survey datasets. Next, it sets out how natural experimental studies can be a useful means of strengthening the causal inference of quantitative studies, and how this can be applied in the context of UC rollout. Chapter 4 ends by providing a summary of the thesis's four empirical chapters in terms of the stages of housing insecurity examined, data sources used, and methods used to reduce any sources of bias.

The empirical analysis chapters themselves are Chapters 5-8. Chapters 5 and 6 are both fixed effects panel designs using administrative data on housing insecurity indicators at the English local authority level. Specifically, Chapter 5 makes use of data from the Ministry of Justice on repossession actions to examine UC's impacts on rates of landlord repossession claims, orders, warrants and bailiff repossessions (i.e. actual eviction). Meanwhile, Chapter 6 makes use of advice trends data from Citizens Advice to examine UC's impacts on rates of advice sought on rent arrears and homelessness related issues. It also disaggregates the rent arrears advice data into the social versus private sector, in order to examine whether UC's impact varies by sector. Next, Chapter 7, also a fixed effects panel design but this time

at the Scottish local authority level, uses official homelessness data obtained from the Scottish Government to investigate UC's impact on rates of homelessness claims and 'Housing Options' approaches (i.e. approaches for the information and advice service used by local authorities in Scotland in attempt to prevent homelessness when a household approaches them with a housing problem). Finally, Chapter 8 is a difference-in-differences analysis of longitudinal data from the 'Understanding Society' survey, and examines UC's impact on household financial problems (i.e. subjective financial problems and household level self-reported difficulties paying for housing/bills/council tax).

The thesis ends with Chapter 9, which is the conclusion. It provides a concluding synthesis of the results of the empirical chapters, sets out how they contribute to ongoing debates over UC's housing security impacts, and discusses their implications for UC claimants, for landlords, and for UC as a policy going forward.

Chapter 2. Universal Credit Policy in Context

2.1 Introduction

This chapter is a review of Universal Credit (UC) as a policy, in terms of the context of its arrival, its structure, the motivating principles behind it and the way in which it has rolled out. The chapter begins by providing an overview of social security policy in the UK pre-UC (from the 'Beveridge Report' in the 1940s up to the policies of the Labour governments in the 2000s), as in order to understand the impacts of UC it is first important to understand the context in which it arrived. The chapter will then go on to provide a detailed account of the original motivating principles of the reform, and how the policy is designed to match these principles. Next, it will provide an overview of the structure of UC and how its payments are calculated. Finally, the chapter will conclude by setting out how UC has been rolled out, with specific reference to the different phases of rollout and the pace at which claimants have moved onto UC in each phase.

2.2 UK Social Security Policy Pre-Universal Credit

2.2.1 The Beveridge Report and the "Classic Welfare State" (1940s-1970s)

In the UK, the modern social security system and welfare state more broadly were founded following the work of William Beveridge in the 1940s, whose 'Beveridge Report' (1942) set out his plans for social security. This included three key strands: (a) '*national insurance benefits*', i.e. an insurance system in which contributions from workers, their employers and the government were used to replace any income lost through unemployment, ill-health, spousal bereavement or retirement; (b) '*national assistance benefits*' i.e. means-tested government support for non-working people; and (c) '*family allowances*' i.e. universal benefit for all families (Beveridge, 1942; Millar, 2018, pp. 39-40). This three-stranded system ran, with few significant changes, from 1945 to 1975. This is often viewed as the "classic welfare state" years (Macionis and Plummer, 2012, p. 339), which was based on the notion of community and collective caring, and embraced a Marshallian philosophy of

universal social rights, with social security being available for all citizens in times of need (Marshall, 1950; Daguerre and Etherington, 2014, pp. 10-11). Importantly, during this period there was a commitment to a mixed economy, and (outside of recessions and cyclical unemployment) 'full employment' was a key assumption. Unemployment was viewed as a short-term problem, with resultant income losses usually being replaced through the national insurance benefit system, although means-tested benefits were available for longer-term unemployed people and those without the required national insurance contributions (Macionis and Plummer, 2012, p. 339; Millar, 2018, p. 40). However, during this period the notion of 'full employment' had different implications than it does today. Whilst today's welfare system assumes employment for almost everyone (notably women, including those with major care obligations, and many disabled people), Beveridge's notion of 'full employment' has been criticised for its able-bodied male breadwinner model. It was based on the assumption that men would earn enough via employment to support families, with women being dependent on men (Maltby, Kennett and Rummery, 2008).

2.2.2 Three Decades of Welfare Reform: Retrenchment, Conditionality and Active Labour Market Policy (1980s-2000s)

Whilst the social security system outlined above had worked well in the 1950s and 1960s when there was near 'full employment' (at least based on the notion of 'full employment' at the time), it began to falter in the 1970s as the unemployment rate rose, increasing the role of the social security system in replacing lost income. In particular, the insurance-based system was not well-suited to rising unemployment as it excluded young people (who had not made national insurance contributions) and long-term unemployed people (who had used up all their contributions) (Millar, 2018, p. 40). Subsequently, the past 40 years have seen substantial changes to the UK's social security system, shifting away from the character of the post second world war 'social democratic' system of universal social rights and shared responsibility for managing risk towards a more individualised, 'liberal' system whereby individual responsibility for managing risk is emphasised (Esping-Andersen, 1990; Macionis and Plummer, 2012, p. 339; Hamilton, 2014, p. 453). This shift began in the late 1970s and 1980s, when rising unemployment became an important political issue. Politicians on the right began to increasingly use terms such as 'welfare dependency' and 'dependency culture' in order to frame social security as problematic, based on the notion that benefits 'trap' people into dependency by encouraging laziness and discouraging

employment (Patrick, 2017, pp. 35-39; Millar, 2018, p. 41). Subsequently, the welfare reforms that have followed in the decades since have attempted to tackle this perceived 'welfare dependency' through benefit retrenchment, conditionality and punitive 'active labour market policies' (ALMPs). Key reforms over time with regards to benefit retrenchment, conditionality and ALMPS are discussed in turn below.

Firstly, with regards to benefit retrenchment, successive UK governments have attempted to 'make work pay' by a combination of reducing the returns from social security benefits for unemployment and increasing the returns from work. According to Kenway (2009), relative to the average level of consumption, unemployment benefits steadily declined in the 1980s, 1990s and 2000s and in 2009 were worth only half of what they were worth in 1979, whereas in the previous 30 years before that (1948-1978) they maintained their value relative to average consumption. This decline began under the retrenchment initiatives of Margaret Thatcher's Conservative governments, whose strong anti-welfare narrative was backed up by significant reforms to unemployment benefits. In total, Atkinson and Mickelwright (1990, as cited in Pierson, 1994) identify at least seventeen reforms to unemployment benefits between 1979 and 1988, which in isolation were fairly modest changes but taken together had substantially unfavourable impacts on unemployment benefit claimants. Amongst these reforms, key changes included the taxation of unemployment benefits (enacted 1980), the end of the earnings-related component of unemployment benefit (enacted 1981), the abolition of child additions to unemployment benefit (enacted 1984) and the abolition of lower-rate benefits (enacted 1986) (see Pierson, 1994, p. 107; Bonoli, 2013, p. 106). However, perhaps the most important reason for the value of unemployment benefits declining, as noted by Kenway (2009, pp. 12-13) has been the switch in 1980 to uprating benefits in line with prices rather than earnings, as the decades that followed saw earnings grow faster than prices. This uprating policy was retained by the Conservative and New Labour governments of the 1990s and 2000s, effectively reducing the returns from social security benefits for unemployment, although, in terms of attempts to 'make work pay', New Labour did also enact some key policies to increase the returns from work and reduce barriers to employment, notably by introducing the National Minimum Wage, extending tax credits to increase the returns from low-paid employment, enhancing anti-discrimination legislation and extending childcare provision (Patrick, 2017, p. 44).

With regards to conditionality, welfare conditionality is defined as policy which "links eligibility for collectively provided welfare benefits and services to recipients' specified

compulsory responsibilities or particular patterns of behaviour […] with various sanctions for non-compliance" (Welfare Conditionality Project, 2018, p. 8). Conditionality is not a new feature of the UK welfare system, as benefits for unemployment have always been conditional on recipients looking for work and being available for work (Ibid, p. 9). However, as noted by Watts and Fitzpatrick (2018, p. 1), recent decades have seen a radical shift in the UK and other western welfare systems towards increasingly conditional forms of welfare. They characterise this as a shift away from a system that protects individuals from risk associated with market forces, and towards a system that uses the welfare system as a lever for changing behaviour. This shift arguably began under the successive Conservative governments of 1979-1997, whose social security reforms led to a "stricter benefit regime" from the late 1980s and ultimately to the introduction of Jobseekers Allowance (JSA) in 1996 (Watts, Fitzpatrick, Bramley *et al.*, 2014, p. 3). Importantly, JSA introduced a 'Jobseekers Agreement' setting out requirements for claimants and giving new powers to advisers, who could – under threat of benefit withdrawal for non-compliance – compel claimants to alter their behaviour if they felt it was damaging their chances of gaining employment (Dwyer, 2016). The Labour Governments that followed from 1997-2010 largely embraced the monitoring of claimants job search behaviour and use of sanctions, and expanded them further via mandatory 'Work-Focused Interviews' for JSA claimants, and by extending conditionality to previously exempt groups e.g. lone parents and some sick and disabled people (Watts, Fitzpatrick, Bramley *et al.*, 2014; Dwyer, 2016).

Finally, with regards to ALMPs, this label is used to describe a wide range of social policies, which can make discussions of ALMPs ambiguous and confusing. In order to address this, Bonoli (2010) provides a typology of four different types of ALMPs. These are set out in Table 2.1. Under this typology, many of the welfare reforms in the UK since the 1980s that have already been discussed – benefit retrenchment, conditionality, sanctions and tax credits – are considered as incentive reinforcement forms of ALMP. However, employment assistance has also been an important part of active labour market policy in the UK, especially since the 1990s. In particular, ALMP in the form of incentive reinforcement policy combined with employment assistance policy became a "high profile and highly publicised area of government policy" under the Labour governments of 1997-2010 via their New Deal programmes for young and long-term unemployed people (Bonoli, 2013). With a strong emphasis on both the right to real training/employment opportunities and the responsibility to take up these opportunities, the New Deal programmes involved a combination of work incentives, compulsory training, and compulsory work-related programmes, with

punishment through sanctions for non-compliance (Daguerre and Etherington, 2014, p. 20). For example, the New Deal for Young People entailed intensive work search assistance for those in the first four months of unemployment, followed by compulsory entering of either: (a) subsidised employment (in public or private sector), (b) education or training, or (c) self-employment (Ibid). This punitive approach, which combines intensive job searches and work placements with sanctions is considered an extreme form of ALMP, and is often referred to as the "workfare approach" (Bonoli, 2010, p. 439).

Table 2.1. Four Different Types of ALMPs.

ALMP Type	Objective	Examples
Incentive reinforcement	To strengthen work incentives for benefit claimants	• Benefit retrenchment • Conditionality • Sanctions • Tax Credits • Other in-work benefits
Employment Assistance	To encourage re-entry to the labour market by removing barriers to employment	• Placement services • Job subsidies • Job search programmes • Counselling • Childcare funding
Occupation	To prevent human capital from depleting by keeping jobseekers occupied	• Job creation (in public sector) • Non-employment related training
Upskilling	To provide opportunities for training to boost skills for employment	• Vocational training

Source: adapted from Bonoli (2010).

This type of ALMP and workfare approach was continued under the Coalition Government after they came into power in 2010, primarily through its flagship welfare to work programme 'The Work Programme' (WP). The WP was in place from 2011-2017 to replace the previous New Labour's New Deal, Flexible New Deal and Employment Zones. It aimed to get long-term unemployed people into work by matching benefit claimants with 'provider' employers, who were given financial incentives for achieving long-term job outcomes for claimants. Importantly, participation in the WP was mandatory for long-term Jobseekers Allowance claimants, meaning that it had an impact on sanctions as people were sanctioned for non-compliance. The WP, alongside other programmes include 'Mandatory Work Activity', 'Work Trials' and 'Sector Based Academies' expanded the proportion of claimants who were subject to welfare-to-work activation measures, and meant that for the first time, many lone parents and disabled people were mandated to participate if they had been (re)categorised as capable of waged work or preparation for waged work (Wiggan,

2015, p. 370). However, the WP was replaced by 'The Work and Health Programme' in 2017, which was a smaller programme and was available on a voluntary basis.

2.2.3 Housing Subsidies Within the Social Security System Pre-Universal Credit

Whilst sections 2.2.1. and 2.2.2. primarily focussed on social security benefits related to the labour market (unemployment benefits and in-work tax credits etc.), it is also important to outline UK social security policy pre-UC relating to housing subsidies, given that housing security is the outcome of interest in this thesis.

In the UK, under the initial years of the post-war welfare state – between 1945 and 1972 – rented housing was made more affordable via a combination of rent controls in the private rented sector and 'producer subsidies' (i.e. central subsidies to local government) for the social rented sector (Lund, 2017, pp. 136-137). During this period, where UK governments were committed to the welfare state, there was mass construction of social housing – it grew to represent nearly a third of the overall housing stock and was largely seen as providing for working families and ex-servicemen (Fitzpatrick and Pawson, 2014; Stephens, 2019).

However, into the 1970s social housing became more targeted at disadvantaged households (Fitzpatrick and Pawson, 2014), and following the 1972 Housing Finance Act there was a shift away from rent controls and 'producer subsidies' towards a 'consumer subsidies' system, whereby housing was subsidised through national rent rebates for tenants in the social rented sector and rent allowances for tenants in the private rented sector (Lund, 2011 pp. 131-132). Subsequently, these have been replaced by Housing Benefit (HB) from 1983 and then, for those in the private rented sector, Local Housing Allowance (LHA) from 2008. Importantly, in the late 1980s, the UK housing market was effectively deregulated via the Housing Act in 1988 and Local Government and Housing Act 1989, which allowed all rents to increase to market levels (by decontrolling private landlord lettings and reducing central government grants to housing associations), leaving HB to support low-income households and protect against eviction (see Lund, 2017, p. 137; Pleace and Hunter, 2018, pp. 334-336). The basic principle of both HB and LHA is that paying for housing costs should not reduce a household's income below set 'Income Support levels' (Lund, 2017, p. 137). Consequently, so called 'requirements' are set based on these levels, and HB/LHA pays all of the rent if

'requirements' match income, tapering off (e.g. at 63p since 2016) for each additional £1 increase in income (Ibid, p. 138).

2.2.4 Benefits Limits/Freeze (2013-2020)

A final important part of the social security context before and during UC rollout is policy to limit/freeze working-age benefit payments. Most working-age benefits in the UK had their annual increases limited to 1% for three years from 2013-14 to 2015-16, before then undergoing a four-year freeze from 2016/17 to 2019-20 (after which the usual system of uprating in line with inflation resumed) (McInnes, 2020, p. 6). The rationale for this was to "ensure that is always pays to work" (HM Treasury, 2015, p. 37), and it represented a major erosion to the social security safety net (Resolution Foundation, 2019). It has had implications for poverty and housing insecurity (Barnard, 2019), and as such is an important part of the context of housing insecurity during the period of UC rollout that is the focus of this thesis.

2.3 Motivating Principles of Universal Credit Reform

Universal Credit has been gradually rolling out since 2013 to overhaul the previous legacy welfare system, and whilst section 2.2 set out the general direction of welfare reform prior to UC rollout, this section discusses the specific origins of UC.

The motivation for, and roots of, the UC reform can be traced back to the work of the Centre for Social Justice (CSJ), a think tank set up in 2004 by Iain Duncan Smith who would later become the Secretary of State for Work and Pensions. Through their '*Breakthrough Britain*' report (see Centre for Social Justice, 2007), CSJ claimed to have identified the five primary "pathways to poverty" as: (1) economic dependency and worklessness, (2) family breakdown, (3) educational failure, (4) drug and alcohol addiction, and (5) serious personal debt. According to the CSJ, the UK's legacy benefit system played a key role in facilitating these pathways to poverty. They viewed welfare spending under the legacy system as excessive, poorly monitored and as facilitating poverty and intergenerational unemployment, setting out how "our benefit system is broken. Although it alleviates financial hardship, it […] traps millions in worklessness and dependency, often over several generations" (Centre

for Social Justice, 2009, p. 14). However, as noted by Gordon (2018) there was no evidence of any analytical methodology in CSJ's identification of poverty pathways, and there is a lack of scientific basis for their claims, with no other researchers having ever identified these same five factors as key to poverty causation. Moreover, subsequent research by Shildrick, MacDonald, Furlong *et al.* (2012) found little evidence of the existence of intergenerational cultures of worklessness in the UK. Despite this, the CSJ's work became highly influential to the social policy agenda of the Coalition Government (2010-2015). In particular, the notions of welfare dependency and intergenerational cultures of worklessness were key to the rationale for the UC reform. The 2010 white paper making the case for UC – entitled '*Universal Credit: Welfare that Works*' – sets out how:

> Successive governments have ignored the need for fundamental welfare reform, not because they didn't think that reform was needed but because they thought it too difficult to achieve. Instead of grasping the nettle, they watched as economic growth bypassed the worst off and welfare dependency took root in communities up and down the country, breeding hopelessness and intergenerational poverty. (Department for Work and Pensions, 2010a, p. 1).

Going on, it asserts:

> A life on benefits is a poor substitute for a working life but too much of our current system is geared towards maintaining people on benefits rather than helping them to flourish in work; we need reform that tackles the underlying problem of welfare dependency. That is why we are embarking on the most far-reaching programme of change that the welfare system has witnessed in generations. (Department for Work and Pensions, 2010a, p. 1).

This notion of the need to tackle welfare dependency, and a sharp emphasis on the distinction between those in work (seen as 'deserving') and those on benefits (seen as 'undeserving') is not new, but in many ways is an extension and intensification of the narratives of the Conservative and Labour Governments of the past three decades (as discussed in section 2.2) (see Patrick, 2012; Wiggan, 2012). In terms of the actual aims and principles of the UC reform, some of these (e.g. conditionality and 'making work pay') are similar to those of previous government welfare policies, whilst others (e.g. making the welfare system more simple and flexible) are more novel. A list of the five key aims/principles of the UC reform

(herein referred to as UC's 'motivating principles'), which all aim to promote transition off benefits and into work, is provided below:

1. Simplifying the Welfare System
2. Improving Financial Work Incentives (i.e. 'making work pay')
3. Extending and Intensifying Conditionality
4. Making Welfare as 'Like Work' as Possible
5. Modernising the Welfare System to match the UK's Flexible Labour Market

Sections 2.3.1-2.3.5 which follow discuss each of these five motivating principles in turn, in terms of why each has been important in the motivation for the reform, how the policy is designed to match the principle and any issues/problems that have arisen in reality to undermine them.

2.3.1 Simplifying the Welfare System

> [Universal Credit] will cut through the complexity of the existing benefit system to make it easier for people to get the help they need, when they need it. (Department for Work and Pensions, 2010a, p. 1).

The first UC motivating principle was to simplify the welfare system. Under the legacy system there were six different working-age means-tested benefits. These were paid in different intervals and withdrawn at different rates as earnings from work increased. They were also administered by three different government departments (out-of-work benefits by the DWP, in-work benefits by HMRC and Housing Benefit by local authorities), with many claimants being on different combinations of benefits at different times (Bennett, 2012).

The Coalition Government argued that this complexity in the system they inherited led to: (a) higher administrative costs, (b) confusion over how to navigate the system, and (c) the

onerous task of submitting the same details multiple times to different administrative bodies if claiming multiple benefits. This was said to reduce trust in the system, make it harder for claimants to see if they will be better off in work, and to stop people from focussing on getting back to work (Department for Work and Pensions, 2010a, p. 9).

Universal Credit attempts to overcome these problems by creating a simpler welfare system. As noted by Bennett (2012), there are two key simplifying elements to UC. Firstly, replacing six working-age means-tested benefits with a single UC payment means that claimants only make one benefit claim to a single department (the DWP) rather than multiple claims to multiple departments. Secondly, UC's single taper rate (as opposed to different benefits being withdrawn at different rates) as earnings rise was initially designed to make it easier for claimants to see how much financially better off they will be from taking on work. The combination of these two simplifying elements was projected by the government to reduce poverty by increasing benefit take up and allowing for a "smoother and simpler transition into work" (Department for Work and Pensions, 2010a; Department for Work and Pensions, 2017b). However, evidence to date suggests that UC has not reduced poverty, and evidence regarding its impact on improving transition into employment is mixed (Chapter 3 will provide a review of literature on the impacts of UC on various outcomes). However, as noted by Millar and Bennett (2017, pp. 169-170), the principle of simplifying the welfare system has been almost universally welcomed amongst service users and stakeholder organisations.

2.3.2 Improving Financial Work Incentives (i.e. 'making work pay')

> Today's Welfare Reform Bill will mean we move to just one core income-related benefit - a universal credit and one message - that it will always pay to work. Even if you just work a few hours at first, you'll see the benefits in the money you keep. Say for example you're on Jobseeker's allowance and you have the chance to do a few hours work. Today after the first £5 you earn, you lose a pound of benefits for every extra pound you take home. But with the universal credit, you would keep 35p of benefit for every extra pound you take home. And because this rate of benefit withdrawal is the same whatever you earn - it's easy to calculate just how much better off you will be. […] It's simple. You don't need a computer model to work it out any more. The more you work, the better off you will be. (Cameron, 2011).

The above quote comes from (Prime Minister at the time) David Cameron's speech to introduce UC via the Coalition Government's Welfare Reform Bill in 2011. It conveys –

albeit in a simplified way (the process of calculating UC payments is still fairly complicated, involving a three-stage calculation process which will be set out in Section 2.4) – the second motivating principle behind UC, 'making work pay'. As Cameron touches on, under the legacy system there was no incentive to take on 'mini-jobs' of just a few hours a week. This is because Jobseeker's Allowance claimants could lose a pound of benefits for every extra pound earned from work up to the 16 hours per week Working Tax Credit entitlement threshold (Bennett, 2012, p. 3; Royston, 2017, pp. 165-166). Moreover, there was little incentive to work more hours than this 16 hours per week threshold as, though qualifying for Working Tax Credit provided a big income boost, taking on extra hours beyond the 16 hours threshold did not provide much of a further income boost (Bennett, 2012, p. 3; Royston, 2017, pp. 165-166).

UC initially sought to overcome these problems through a combination of a single taper rate and the use of work allowances. The UC taper rate was initially 65% (hence Cameron's reference to keeping 35p of benefit), and is currently 63%, meaning that UC payments are reduced by 63p for every extra pound earned from work above work allowances. However, UC's work allowances allow claimants to earn up to a certain threshold before the taper rate kicks in. This means that 100% of UC payments are kept on top of extra earnings from work below the work allowance threshold.

The extent to which UC does improve work incentives compared to the legacy system depends on the claimant's number of hours worked and their household circumstances, with some groups gaining from the switch to UC and others losing out. UC has been effective in improving work incentives for 'mini-jobs' of less than 16 hours per week due to more generous withdrawal rates (Brewer, Finch and Tomlinson, 2017). However, UC continues to incentivise single parents to reduce working hours below 16 hours per week, and work allowances are only applied once per household, meaning that 'second earners' in a household have reduced work incentives (Finch and Gardiner, 2018). Given that both of these groups are more likely to be women, this has important gendered implications, and results in lower work incentives for women than men overall (see Bennett, 2012; MacDonald, 2018). This undermines the 'making work pay' principle of UC as, whilst work incentives are improved for some groups under UC they are weakened for others (for a detailed overview of the gainers and losers see Finch and Gardiner, 2018).

2.3.3 Extending and Intensifying Conditionality

> Mutual responsibility is the vital ingredient of a strong, successful, compassionate welfare system. We need responsibility on the part of those who contribute to the system - government and taxpayers. And responsibility on the part of those who receive from the system. (Cameron, 2011).

> Universal Credit will make sure that work pays for benefit recipients. […] In return, we expect recipients to do everything that can reasonably be expected of them to find work or prepare for work in the future as a condition of receiving support. (Department for Work and Pensions, 2010a, p. 25).

> We will introduce a 'claimant commitment' to clearly set out what is expected of each recipient. We will raise the requirements placed on some individuals and will introduce tougher sanctions to ensure recipients meet their responsibilities. (Department for Work and Pensions, 2010a, p. 24).

As set out in section 2.2., recent decades have seen a radical shift in the UK (and other western countries) towards more conditional forms of welfare. As indicated by the above quotes from the early years of Coalition Government, welfare conditionality has been extended and intensified further under UC, with a key motivating principle of UC being increasing the number of requirements placed on claimants and introducing harsher sanctions for non-compliance.

Specifically, UC extends conditionality (for the first time) to those who are in work via mandatory job search conditions for low-paid workers to promote 'progression' (which can include taking on extra hours/multiple jobs) (Wright and Dwyer, 2020). This blurs the binary distinction between 'deserving' workers ('hard working families') and 'undeserving' benefit claimants ('welfare dependents'), which has been used to frame many post 2010 welfare reforms, thus extending the stigmatising rhetoric of 'welfare dependency' to up to 1.2million workers (Bennett, 2012; Dwyer and Wright, 2014, p. 31; Patrick, 2017, p. 47).

In addition, conditionality is also extended to more lone parents under UC, increasing the number of requirements they face through 'Lone Parent Obligations' (LPOs). Reforms to increase LPOs have been ongoing since 2008, but UC takes this further by compelling lone parents with children over one year old to attend work-focussed interviews and by abolishing some of the legacy system's 'lone parent flexibilities' (Cain, 2015; Rafferty and Wiggan,

2017). Specifically, this includes: (a) no longer allowing lone parents to leave/refuse a job due to lack of affordable childcare, and (b) no longer allowing lone parents with children over 13 to restrict their work availability to school hours (Gingerbread, 2014 as cited in Rafferty and Wiggan, 2017, p. 516). As such, Rafferty and Wiggan (2017, p. 533) note that UC thoroughly incorporates lone parents into the supply of poor workers, removing the de-commodifying protections that previously existed via the Income Support system.

As well as extending conditionality to new groups, UC also intensifies its use in practice amongst groups for whom benefits were already conditional, through a greater number of work-related requirements and harsher sanctions for non-compliance. The specific rules regarding UC's system of conditionality and sanctions for different groups of claimants is provided in Table 2.2. Compared to the legacy benefits system, this new system is said to represent a major intensification of conditionality and sanctions, establishing a new "ubiquitous conditionality" within the UK welfare system (Dwyer and Wright, 2014). This has been reflected in the rates of sanctioning under UC in its rollout so far, with analysis of sanction statistics suggesting that UC has much higher sanction rates than legacy benefits (Webster, 2019), although it can be difficult to make accurate comparisons (see Keen, 2018).

Table 2.2 Conditionality and Sanctions Under Universal Credit.

Conditionality Group	Work Related Requirements	Sanctions for Non-Compliance
1) "Full Conditionality" Group: This is the default group for claimants, including lone parents and couples with older children	Those in this group are required to do all they can to find a job or a higher paid job. This includes being available for work straight away, actively looking for work, applying for jobs, and attending interviews. There may also be a requirement to participate in mandatory work related activity and in welfare-to-work programmes.	**High Level Sanctions:** Applied to claimants in this group who fail to participate in mandatory work activity, fail to apply for/accept paid work, leave work voluntarily, lose a job via misconduct, or lose pay without good reason. They consist of sanctions of: (a) 13 weeks for first failure to comply, or (b) 26 weeks if have already had a high level sanction in past year. **Medium Level Sanctions:** Applied to claimants in this group who fail to be available for work or take all actions expected to get work. They consist of a 4 week sanction for first failure to comply and a 13 week sanction for any further failures. **Low Level Sanctions**: Applied to those in this group who fail to attend a meeting or meet a work-search requirement. They are fixed escalating sanctions, with benefit payments withheld until behaviour is corrected. Penalty periods last 7

		days for first sanction, 14 days for the second and 28 days for the third.
2) "Work Preparation" Group: Claimants will be in this group if they are disabled or have a health condition which means they have limited capability to work at the current time.	Those in this group are required to 'take reasonable steps to prepare for work'. This includes CV writing, attendance of work experience or training, and attendance at work coach meetings. There is no requirement to actually search for or be available for work.	**Low Level Sanctions:** Outlined above – they can be applied to those in this group who fail to attend a meeting or comply with a work related requirement (e.g. failure to write a CV or attend CV writing workshop).
3) "Keeping in Touch with the Labour Market" Group: Claimants will be in this group if they are a lone parent or lead carer in a couple with a child aged 1-5.	Those in this group are required to attend regular interviews with their work coach to discuss preparations for future work. There is no requirement to look for or be available for work.	**Lowest Level Sanctions:** Applied to those who fail to attend jobcentre meetings without good cause. Consists of UC payments being reduced to a 40% rate until you attend an interview.
4) "No Conditionality" Group: Claimants will be in this group if they are in one of the following categories: (a) have a disability/condition that prevents them from working or preparing for work; (b) are a lone parent/lead carer of a child under one; (c) have intensive and regular caring responsibilities; or (d) earn above the conditionality threshold.	UC payments are unconditional (i.e. no work related requirements) for this group.	**No Sanctions**

Notes: high level sanctions could previously last up to three years, but this was phased out in 2019. Work searches and sanctions were temporarily suspended in response to the COVID-19 pandemic. Sources: Department for Work and Pensions (2010a); Citizens Advice (2018).

2.3.4 Making Welfare as 'Like Work' as Possible

Another motivating principle behind the UC reform was to make claiming welfare benefits as 'like work' as possible, with UC said to "mirror the world of work" (Department for Work

and Pensions and Gauke, 2017). As set out by Millar and Bennett (2017, pp. 171-172), UC being 'like work' means two things: (a) in the same way that those in work must sign and adhere to their employment contract, UC claimants must sign and adhere to their 'claimant commitment', and (b) to reflect how wages are paid, UC payments are paid monthly in arrears and directly into the claimants own bank account.

With regards to UC claimants signing a 'claimant commitment', this is nothing new as the contractualism of the 'claimant commitment' is markedly similar to that of the 'Jobseekers Agreement' under JSA. The only difference is that UC extends this contractualism to a greater number of groups (namely those in work, lone parents and disabled people), and compared to the legacy system has harsher sanctions for non-compliance (as discussed in section 2.3.3). In fact, benefit sanctions are harsher than any penalty you'd expect to receive for failing to meet requirements in an actual employment contract (Millar and Bennett, 2017, p. 171; Patrick, 2017, pp. 24-25).

Whilst UC's contractualism is similar to the JSA system, its reform to the benefit payment structure – i.e. the monthly direct payment in arrears system – does take the 'like work' principle further than before. Under the legacy system, benefits tended to be paid fortnightly, with Housing Benefit being paid to a claimant's landlord rather than directly to the claimant (UK Government, 2018).The rationale for this new monthly direct payment in arrears design is that it will "help low-income households develop a greater responsibility for managing their household budget" and "ensure that they will be better prepared and more in control of their money when they […] move into work" (Department for Work and Pensions, 2012, p. 38). Yet, it has been criticised for failing to fit with the pattern of how many low-income households manage their money (Bennett, 2012), and the assumption that monthly payments match pay from work is not true in reality given that only around half of people earning under £10,000 per year are paid monthly (Millar and Bennett, 2017). In addition, the switch to monthly arrears payments has created a long wait period (previously six weeks, now five) between households making a UC claim and receiving the first payment, whilst direct payments meant that help towards housing costs were no longer automatically paid to landlords. This has had considerable negative impacts, which are discussed in the review of UC's impacts in Chapter 3.

2.3.5 Modernising the Welfare System to match the UK's Flexible Labour Market

The final motivating principle for the UC reform is to modernise the welfare system to match the UK's flexible labour market. The UK labour market for much of the 20th century had seen firms (i.e. employers) seeking to attract and retain a high-quality loyal workforce whose skills improved over time – this maximised their productivity, but importantly also provided secure "jobs for life" for employees (Bender and Theodossiou, 2017, p. 2). However, flexibilization has occurred in recent decades, with technological advancements and globalisation resulting in what Bender and Theodossiou (2017, p. 1) describe as "a perception of a need for labour market flexibility enabling employers to respond to market pressures in order to respond to changing circumstances and to retain a competitive advantage in the face of global market pressures". The policy response to this has been to deregulate labour markets in order to provide increased flexibility to employers to adjust their staff levels in response to fluctuations in demand (see Deakin and Reed, 2000; Bailey, 2016, pp. 3-4). Consequently, the 21st century has seen a notable rise in the use of 'flexible' or 'atypical' forms of employment (see Taylor, 2017). This includes temporary work, zero-hour contracts and low-paid self-employment, which often involve more variable and/or anti-social hours and lower levels of employment security, rights and protections (Trade Union Congress, 2016; Bailey, 2018, p. 159).

On one hand, the UK's flexible labour market, and increasing usage of flexible forms of employment, is said to boost business efficiency, and in 2015 was hailed by then Prime Minister David Cameron as key to the UK's "jobs miracle", whereby (despite being in recovery from the 2008 financial crisis) unemployment rates were kept at record low levels in the 2010s (as cited in Rubery, Keizer and Grimshaw, 2016, p. 235). On the other hand, there are concerns that the insecure forms of work promoted by the flexible labour market, which pass financial risks from firms onto workers and their families (see Standing, 2011; Rubery, Keizer and Grimshaw, 2016), are associated with adverse mental health outcomes (Bender and Theodossiou, 2017) as well as persistent levels of underemployment (see Rafferty and Wiggan, 2017; Taylor, 2017, p. 20) and in-work poverty (see Hick and Lanau, 2017).

When introducing UC, the Coalition government set out how they wanted to "create a welfare system that provides people with the confidence and security to play a full part in

society through a flexible labour market within a modern competitive economy" (Department for Work and Pensions, 2010a, p. 12). In practice, as well as promoting the flexible labour market via conditionality – which supports flexibilisation by compelling UC claimants to actively search for and accept atypical and insecure forms of employment, thus increasing labour supply (Rafferty and Wiggan, 2017) – UC also promotes flexibilisation by implementing a new Real Time Information (RTI) system. RTI is designed to improve efficiency for those in atypical forms of employment by being more responsive to fluctuations in earnings. As such, payments are based on income in a specific monthly assessment period, with UC payments being automatically recalculated if earnings drop (Department for Work and Pensions, 2016). This overcomes issues within the legacy system whereby workers whose hours fluctuated above and below the 16 hours per week threshold struggled to access benefits. However, it has led to new problems, with evidence of some claimants losing out on payments if their work payment date varies from month to month or if their employer inaccurately record their information in the RTI system (Citizens Advice Scotland, 2016).

2.4 An Overview of Universal Credit's Structure and How Payments are Calculated

As previously intimated in section 2.3.1, unlike the legacy system it replaces, UC involves a single payment and is fully administered by the DWP. However, it is still made up of various different elements, with the UC child element, housing element and other additions potentially being added to the standard allowance depending on the claimant's circumstances. Based on this, Table 2.3 provides an overview of the structure of UC, and how this compares to the structure of the six different benefits of the legacy system that it replaces.

Table 2.3 An Overview of the Structure of Universal Credit and the Legacy System.

Legacy System	Universal Credit System
Six legacy benefits: • **Income-based Jobseeker's Allowance**: for unemployed people • **Income-related Employment and Support Allowance**: for out-of-work disabled people	One single benefit made up of the following elements: • **Standard Allowances**: standard amount for all UC recipients (amount varies depending on circumstances) • **Child Element**: additional amount for those with children

• **Income Support**: for out-of-work parents • **Working Tax Credit**: for topping up the incomes of those in low income employment (eligibility depends on number of hours worked, income and circumstances) • **Child Tax Credit**: for in-work and out-of-work families with children • **Housing Benefit**: to providing support towards housing costs for tenants who are out-of-work or in work on a low income	• **Housing Element**: additional amount to provide support towards housing costs (eligibility depends on age and circumstances) • **Other Additions**: other additional amounts are added for people with a disability/health condition or caring responsibilities
Entitlement as income from employment rises: • Different legacy benefits withdrawn at different rates as claimants increase their earnings from employment	Entitlement as income from employment rises: • UC withdrawn at taper rate of 63p for every £1 increase in earnings from employment. Taper rate only kicks in once claimants are earnings above a certain threshold (called their 'work allowance')

Source: adapted from Finch (2015, p. 26) and Brewer, Joyce, Waters *et al.* (2019).

As a result of having various different elements, the way in which a household's UC payment is calculated involves a three stage-process. Firstly, claimants are given a standard monthly amount that varies based on their age and whether they are in a couple, with single people and those aged under 25 receiving lower amounts. Secondly, additional monthly amounts are added on if claimants have a disability/health condition, caring responsibilities and for additional support towards housing costs, children (limited to two children) and childcare. Third and finally, monthly adjustments are made based on the claimant's income, earnings, capital, or if deductions or the benefit cap are applied. A detailed overview of this calculation process, and how much households will receive, is provided in Table 2.4.

Importantly, the housing costs amount under Universal Credit is broadly the same as those under the legacy system. This is because in the short and medium term it is still calculated based on the existing LHA and HB systems (Webb, 2012, p. 9). This means that cuts to HB and LHA since 2011 – such as reducing the level of LHA from 50th to 30th percentile of market rents, the national cap on LHA/HB, and size criteria in social rented housing (see Hamnett, 2014) – have all been carried over and continue to be in operation under UC (Wilson, 2019a).

Table 2.4 How a Household's Universal Credit Payment is Calculated, and How Much They Will Currently Receive.

1) Standard Monthly Amount	2) Additional Monthly Amounts	3) Monthly Adjustments
Single (aged under 25) • £256.05 (uplifted to £342.72 in response to COVID-19 pandemic) Single (aged 25+) • £323.22 (uplifted to £409.89 in response to COVID-19 pandemic) Couple (both under 25) • £401.92 (uplifted to £488.59 in response to COVID-19 pandemic) Couple (either or both aged 25+) • £507.37 (uplifted to £594.04 in response to COVID-19 pandemic)	Child Amounts • £281.25 (born before 6 April 2017) or £235.83 (born on/after 6 April 2017) for first child • £235.83 for second child • £128.25 or £400.29 for each disabled or severely disabled child Childcare Amounts • Up to £646.35 for one child • Up to £1,108.04 for 2 or more children Housing Costs Amount • Calculated broadly in same way as previous HB/LHA system Disability/Health Condition Amount • £128.25 if 'limited capability for work' (only applies if receiving benefit for the condition before 3 April 2017) • £341.92 if 'limited capability for work and work related activity' Carer's Amount • £162.92 for providing full-time care for a severely disabled person	Earnings • As set out in Table 2.3, UC is withdrawn as earnings from employment rise Capital • UC payments are adjusted based on capital (e.g. savings, investments or property) • Capital of <£6,000 ignored • Capital of £6,000-£16,000 reduces UC payments • Capital of >£16,000 disqualifies a UC claim Other Income • UC is also withdrawn for other income sources e.g. pension, student income or maintenance payments Deductions/Benefit Cap • Further deductions can be made for sanctions, repayments (for advances or overpayments) • The benefit cap caps the overall amount that a household can receive to £20,000 per year for couples/single parents (or £23,000 in Greater London), and £13,400 for single adults (or £15,410 in Greater London)

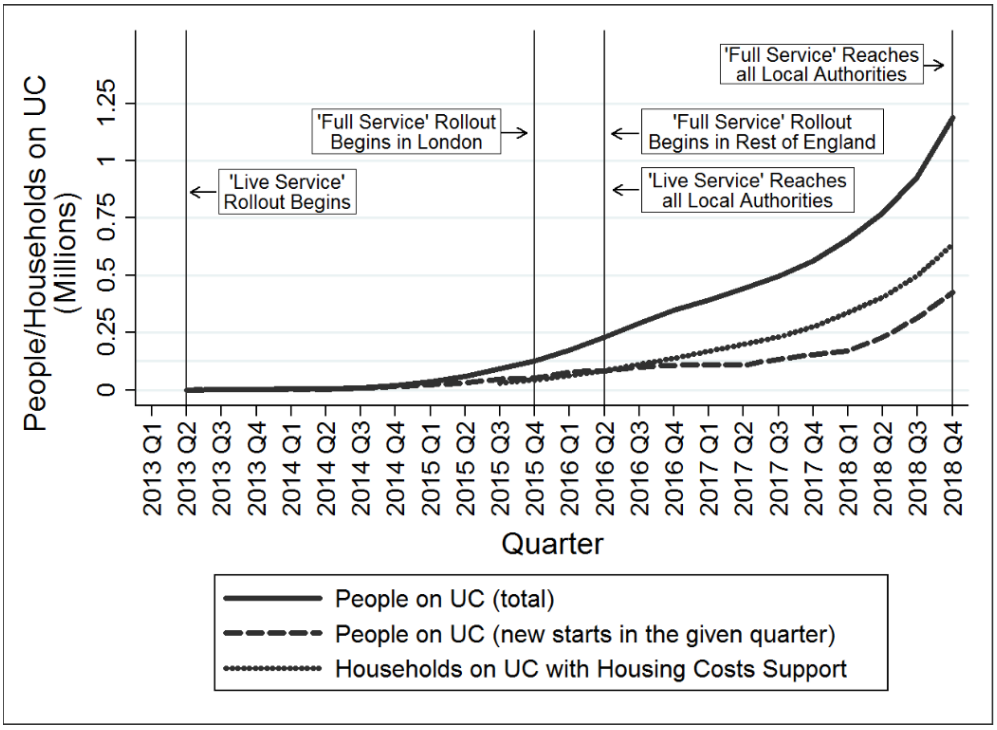
Notes: amounts are for 2020-2021. Standard monthly amounts were uplifted in March 2020 in response to the COVID pandemic – the amounts including the uplift are shown in brackets. At the time of writing, this uplift is due to end in September 2021. Sources: McInnes (2020); Department for Work and Pensions (2021a); UK Government (2021a); UK Government (2021b)

2.5 How Universal Credit Has Been Rolled Out

UC has been rolling out gradually since 2013, in a staggered way (being introduced in different areas at different times) and using a "twin track" approach (Kennedy and Keen, 2018, p. 8). This has been made up of the twin rollout of: (a) UC 'Live Service', which rolled out gradually between 2013 and 2016, becoming available to new benefit claims that were most simple to manage (typically claims from single, childless unemployed people), and (b) UC 'Full Service', which used an updated IT system and rolled out gradually between 2015 and 2018, becoming available to all new claimants (National Audit Office, 2018, pp. 14-15).

By the end of 2018, UC 'Full Service' had reached all Jobcentres in all UK local authorities, meaning that all new claims (and those on the legacy system with a change of circumstances) move onto UC. This process is known as "natural migration" (Kennedy and Keen, 2018, p. 15). The process of transferring those still on legacy benefits who have not had a change of circumstances onto UC (known as the "managed migration" process) had been due to take place between 2019 and 2023, but the COVID-19 pandemic led to this being paused in March 2020 whilst it was still being piloted (Work and Pension Committee, 2020, p. 20).

Figure 2.1. Quarterly Number of People on UC, New Starts to UC and Households on UC with Housing Costs Support (2013-2018).



Notes: the number of households on UC with housing costs support is measured on the second Thursday of the quarter's middle month. People on UC is a cumulative measure of the total number of people currently on UC in the given quarter who have accepted their claimant commitment.

Figure 2.1 outlines, for England only, the quarterly pace of UC rollout (in terms of number of people on UC, new starts per quarter and households on UC with housing costs support) and key dates in the rollout schedule over time. It highlights how initially the number of people on UC increased slowly during 'Live Service' rollout as this was targeted at the simplest claims to manage. However, claimants began to move onto UC more rapidly during 'Full Service' rollout, with over 1.2 million people in England being on UC by 2018 Q4.

2.6 Concluding Summary

This chapter has provided a review of Universal Credit as a policy, in terms of the context in which it arrived, its structure, its motivating principles, its design and its rollout. UC represents a major change to the welfare system. Whilst some of the key features of UC, such as increased conditionality, being 'like work' and 'making work pay' are not new and have been aims of previous welfare reforms, UC takes these further than before, and other features of UC like simplifying the welfare system and introducing the RTI system to promote the flexible labour market are novel.

As this chapter has set out, UC has been rolled out slowly since 2013. During this time, there has been much research and debate into the impacts that UC has had and is likely to have in the future. The following chapter (Chapter 3) focuses on this, providing a review of literature on the impacts on UC on various outcomes.

Chapter 3. Literature Review of Universal Credit's Impacts

3.1 Introduction

This chapter provides a review of literature examining the impact of Universal Credit on a variety of outcomes. As set out in Chapter 2, the notions of 'welfare dependency' and intergenerational 'cultures of worklessness' were key to the rationale for the UC reform, with all of UC's motivating principles being based on the aim of promoting transitions off benefits and into work. Given the strong emphasis placed on UC and employment outcomes by the government, this chapter will begin by reviewing literature on the impact of UC rollout on employment. However whilst the government has had a single-minded focus on employment outcomes when it comes to UC (see Alston, 2018), this has often come at the cost of evidence on UC's wider impacts being overlooked, and the DWP have been criticised for failing to work with others to establish an evidence base on hardship caused by UC on claimants (National Audit Office, 2018). In response to this, this chapter will also provide a review of evidence on these wider impacts of UC, with specific reference to existing research on claimant's mental health and wellbeing, incomes and debt, and food bank usage. The chapter will then end by introducing the concept of housing insecurity, and providing a detailed account of current evidence on the impact of UC on various indicators of housing insecurity.

3.2 Universal Credit's Impacts on Employment Outcomes

With regards to employment outcomes, the intention of UC is to increase both: (a) the number of people entering work, i.e., "work participation", and (b) the number of hours worked by those already in work, i.e., "work intensity" (Millar, 2018, p. 51). Increasing work intensity is often framed as promoting in-work "progression", e.g., by applying large scale job search requirements to low-paid workers in order to compel them to take on more hours or multiple jobs (Wright and Dwyer, 2020).

Throughout the rollout of UC, the government have repeatedly claimed that UC will get an additional 200,000 people into work through its increased financial incentives, additional

conditionality and its simpler/smoother system (e.g. see Department for Work and Pensions, 2018a). This has been key to the government's economic case for the UC reform, with improved employment outcomes being an important part of justifying the large upfront expenditure on the new system (Ibid). However, in reality UC's employment impact has been described as "highly uncertain" (Johnson, 2018), as there is a lack of precedents regarding some of UC's key features, particularly its integration of different benefits and its use of work allowances to attempt to incentivise work. Whilst there is some empirical evidence specifically on the labour market impacts of increased use of conditionality and sanctions, these have produced mixed results. For example, research by National Audit Office (2016), using an instrumental variables approach, suggests that under Jobseekers Allowance sanctions were associated with increased probability of claimants being in employment in later months, but had no impact on earnings. However, a fixed effects panel analysis of local authority level data by Loopstra, Reeves, Mckee *et al.* (2015) suggests that although Jobseekers Allowance sanctions were associated with claimants exiting benefits, this was more likely to be into non-work destinations (i.e. remaining out-of-work but not claiming benefits) than into employment. Similarly, a time-series analysis by Taulbut, Mackay and McCartney (2018) suggests that "intensifying the use of sanctions and introducing harsher penalties associated with being sanctioned has been largely ineffective at increasing flows from JSA into sustainable employment" (p. 1417).

Throughout the rollout of UC to date, the DWP have been conducting their own research into its labour market impacts, in terms of employment entry and progression, with mixed results. Their research into the early, short-term impacts on employment entry suggest a small positive increase under UC, with UC claimants (63%) being four percentage points more likely to have been in work at any point within six months of their claim than those in a matched group of JSA claimants (59%) (Department for Work and Pensions, 2017a). However, this analysis was limited to single unemployed claimants without children. To examine the ability of UC (via sanctions and support) to promote in-work progression, measured via increased earnings, the DWP conducted an in-work progression randomised control trial between 2015 and 2018. The trial had three treatment groups: "frequent support participants" (who had Jobcentre work search meetings every fortnight, with mandatory actions), "moderate support participants" (who had Jobcentre work search meetings every eight weeks, with mandatory actions), and "minimal support participants" (who had telephone calls with a work coach every eight weeks, with voluntary actions) (Department for Work and Pensions, 2018b, p. 13). The trial found no evidence that those in the more

intensive treatment group experienced "higher or more sustainable earnings growth" than those in the more light touch groups, and no evidence that sanctions helped motivate UC claimants to progress in work (Ibid, pp. 104-106).

Outside of the DWP's own research, results from research into UC's employment impacts are also mixed. Analysis by Vilanova and Ghelani (2018) investigates employment effects of UC using three alternative methodologies, with data from a small sample of three local authorities. Their findings suggest that UC had a "positive, albeit small" impact on employment outcomes (p. 12). Conversely, the results of a quasi-experimental study by Wickham, Bentley, Rose *et al.* (2020), "found no evidence that Universal Credit exposure was associated with moving into employment" (p. e158). Similarly, qualitative studies with UC claimants have suggested that employment outcomes under UC were relatively neutral (Wright, Dwyer, Jones *et al.*, 2018), that financial and housing insecurity under UC push claimants further away from the labour market (Cheetham M, Moffatt S, Addison M *et al.*, 2019), and that conditionality for in-work UC claimants has been largely counter-productive (Wright and Dwyer, 2020).

3.3 Universal Credit's Wider Impacts

Whilst the DWP have conducted some analysis into UC's employment impacts (as outlined in section 3.2), they have failed to work on establishing an evidence base on UC's other impacts, and there has been a tendency to dismiss any evidence provided by others on claimant hardship across a range of outcomes (see National Audit Office, 2018). Sections 3.1.-3.3. which follow provide a review of literature on UC's impacts on such outcomes, namely mental health and wellbeing, incomes and debt, and food bank usage. Section 3.4 will then provide a review of literature on housing security impacts, which are the focus of the empirical analysis of this thesis.

3.3.1 Impacts on Mental Health and Wellbeing

One area where the DWP have been criticised for failing to consider UC's impacts is mental health and wellbeing. As UC has been rolling out, concerns have been raised by mental health charities over its potentially detrimental impact on the mental health of claimants. In

particular, there have been concerns that financial problems associated with UC's long wait for the first payment cause worsening mental health, that UC's increased threat of sanctions cause emotional distress, and that some people with existing mental health problems will have difficulties coping with UC's 'digital by default' system and the increased budgeting responsibility of monthly direct payments (Mind, 2018; SAMH, 2019).

To date, empirical evidence on UC's mental health and wellbeing impacts has largely come from qualitative research studies. One such study, by Cheetham M, Moffatt S, Addison M *et al.* (2019), examined the health and social effects of UC rollout in North East England through semi-structured interviews with UC claimants with complex needs. Their results suggest that UC negatively impacts on mental health and wellbeing, due to: (a) the claims process, with the digital claims system being "complicated, disorientating, impersonal, hostile and demeaning" (p. 1), and (b) the consequences of managing on UC, with long wait periods and sanction threats leading to distress and deterioration of emotional wellbeing. This is backed up by further qualitative research by Britain Thinks (2018), whose findings suggest UC's long wait periods can push claimants into a spiral of depression/anxiety, and Wright, Dwyer, Jones *et al.* (2018), whose findings suggest that the constant threat of sanctions under UC leads to a great deal of stress and anxiety amongst claimants.

In terms of quantitative analysis into UC's mental health impacts, the only known study is Wickham, Bentley, Rose *et al.* (2020)'s quasi-experimental research, which uses longitudinal survey data to examine the effect of UC on psychological distress. Their findings are in line with the qualitative research – they suggest that UC's introduction increased the prevalence of distress by 6.57 percentage points amongst unemployed individuals. Whilst this is the only study on UC specifically, its findings are consistent with previous studies in highlighting the detrimental impact of increased sanctions and conditionality under Jobseekers Allowance on mental health (Williams, 2021a; Williams, 2021b), given that increased conditionality is a key feature of UC.

3.3.2 Impacts on Incomes and Debt

Given that Universal Credit radically reforms the structure of working-age means tested benefits (and how payments are calculated), it has a significant impact on the incomes of claimants. This is because, by combining six different legacy benefits together into a single

payment, UC, both naturally and as a results of policy choices, leads to changes in how much claimants are entitled to. 76% of those entitled to means-tested benefits will have their entitlements changed by (gains or losses of) at least £100 per annum as a result of the switch to UC (Brewer, Joyce, Waters *et al.*, 2019). Whether claimants income increase or decrease under UC depends on their circumstances. Brewer, Joyce, Waters *et al.* (2019)'s analysis, which modelled the impact of UC on claimant incomes over eight years, suggests that overall UC will likely result in income gains for around 4.2 million people and income losses for around 4.6 million people, and, importantly, the poorest 10% of adults lose the most. Similarly, a separate analysis by Finch and Gardiner (2018) suggests that 2.4 million families are likely to see income gains from the transition to UC and 3 million likely to see income losses, although more families could gain if UC manages to increase benefit take up.

Outside of the fact UC creates income gains/losses for different families, some of its key design features have been identified as problematic in potentially causing or exacerbating debt problems. In particular, UC's long wait period can leave claimants without income for rent and bills whilst waiting for the first payment, and thus push them towards borrowing money to get by, either from lenders or through UC advance payments (essentially an interest free loan from the DWP). This, combined with budgeting difficulties associated with UC's monthly direct payment system, can lead to personal debt.

Consequently, research by Drake (2017), which involved quantitative analysis of Citizens Advice's service data and qualitative interviews with their clients, suggests that those on UC are more likely to be struggling with debt problems than those on legacy benefits. The research found that UC claimants were particularly struggling with priority debts (i.e. debts which can lead to loss of home, essential goods/services or imprisonment) like council tax arrears, magistrates court fines, and rent arrears (UC's impacts on rent arrears are discussed in detail later in this chapter in section 3.4) (Ibid, pp. 8-9). The author notes that these could lead to further borrowing from high-cost lenders, making it more difficult to pay off debts in the long term (Ibid, p. 34). With regards to UC's detrimental impact on council tax arrears specifically, this is an important finding given that UC is not directly responsible for providing council tax support (following the abolishment of Council Tax Benefit in 2013, local authorities have been responsible for running their own council tax reduction schemes, which has increased the amount of council tax paid by low-income families in England (see Bushe, Kenway and Aldridge, 2013)). This implies that although UC does not directly affect council tax support, it is having an indirect impact in that financial

problems arising from UC are having a knock on impact on the ability of households to meet council tax payments.

In addition to Drake (2017)'s research, the negative impact of Universal Credit rollout on personal debt problems is also highlighted by research from the debt charity StepChange (2020). Their client survey suggests that inadequate support, long wait periods and unpredictable monthly payments meant that UC exacerbated debt problems amongst those with low-incomes by leading to increased borrowing from friends, family, lenders or illegal loan sharks. Other qualitative research by Robertson, Wright and Stewart (2020) and Britain Thinks (2018) have also had similar findings, highlighting several examples of UC claimants who have been forced to borrow money from friends and family to get by during UC's long wait for the first payment. Finally, quantitative research by d'Este and Harvey (2020), involving causal modelling strategies (including difference-in-differences and instrumental variables analysis), suggests that UC rollout has also had criminological effects via increased burglaries, which the authors link to benefit claimants worsening financial conditions under UC.

3.3.3 Impacts on Food Bank Usage

Another wider impact of UC rollout that has been identified in literature so far relates to food bank usage. In the UK, the past decade has seen rising demand for food parcels from foodbanks, and this has been linked to post 2010 austerity policies aimed at reducing central and local government spending on welfare (see Loopstra, Reeves, Taylor-Robinson *et al.*, 2015; Trussell Trust, 2020). This is because, in general, policies to reduce the financial returns from, and universality of, a country's welfare system also reduces their ability to provide food security (Loopstra, Reeves, McKee *et al.*, 2016), and in particular, the use of sanctions under the old Jobseekers Allowance system has been linked to rising food bank usage in the UK (Loopstra, Fledderjohann, Reeves *et al.*, 2018).

With regards to UC specifically, Trussell Trust (2019) have raised concerns that UC increases food bank use not only through its increased conditionality and sanctions, but also due to its long wait periods as a lack of income whilst waiting for the first payment means that people are forced to use food banks to feed their families. Their own research, which provides an analysis of their food bank parcel data covering 414 of their food bank centres,

suggests that usage had risen by 30% 12 months after UC rollout locally, rising to 40% after 18 months and 48% after 24 months (Thompson, Jitendra and Rabindrakumar, 2019). Qualitative case studies, conducted as part of the same research, identified the long wait period for the first UC payment as the key contributing factor to this (Ibid). This finding is complemented and backed up by analysis from Reeves and Loopstra (2020), who link data on food bank usage to data on the introduction of UC and, using a range of causal identification strategies (i.e. fixed-effects, granger causality tests and matching designs) consistently find that food parcel distribution has increased as UC has rolled out.

3.4 Universal Credit and Housing Insecurity

As set out in Chapter 1, and as will be discussed in more detailed in terms of the data and methodological approach in Chapter 5, the overarching aim of this thesis is to examine the impact of Universal Credit rollout on housing security. Consequently, it is important, here, to introduce the concept of housing insecurity, to set out how UC rollout may potentially lead to various stages of housing insecurity, and to provide a review of literature on existing evidence around UC's housing security impacts. This is done in sections 3.4.1-3.4.3, which follow.

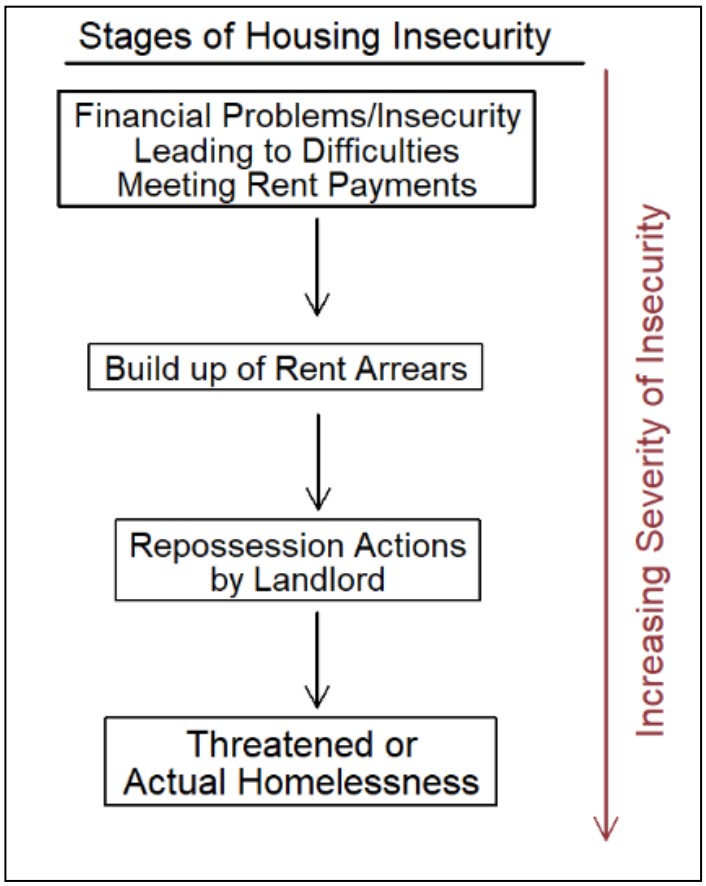
3.4.1 Housing Insecurity as a Concept

Housing is one of the major social, economic and environment conditions that influences the health and wellbeing of people and populations (Braubach, 2011). It has traditionally been conceptualised in terms of its physical dimensions, with reference to the role it plays in providing a physical place to dwell and prevent exposure to cold/damp conditions or dangerous toxins (Bentley, Pevalin, Baker *et al.*, 2016, p. 209). However, it also contributes to social wellbeing by providing a sense of identity, worth, security and constancy (Preece and Bimpson, 2019, p. 16).

Housing insecurity (sometimes instead referred to as housing instability, and part of the wider concept of housing need) is when personal or economic difficulties in a household threaten the sustainment of their housing. The most extreme form of housing insecurity, with the most obvious harms on population health (see Leng, 2017; Waugh, Knowles and Rowley,

2018), is homelessness. However, housing security does not always equate to homelessness as those facing housing insecurity may still have a place to live, but they are facing difficulties maintaining their residence (Rollins, Glass, Perrin *et al.*, 2012). As briefly introduced in Chapter 1, housing insecurity in the current context of the UC welfare reform can broadly be summarised as occurring in four, increasingly severe, stages – these are summarised in Figure 3.1. and are discussed in this context in more detail in section 3.4.2. Harms to health and wellbeing can arise not only from homelessness but also from the earlier stages of insecurity. Stress and anxiety can arise from fear of losing your home amongst those facing rent arrears and/or repossession actions (Bond, Evans and Holkar, 2018). More broadly, having a secure home provides a long-term base from which to engage in society through holding down work and building social networks/connections (Bailey, Besemer, Bramley *et al.*, 2015). This is threatened when housing becomes insecure, whether that is through rent arrears, repossession actions or actual eviction.

Figure 3.1. The Four Stages of Housing Insecurity.



Housing insecurity can arise for a variety of reasons, such as financial difficulties, relationship breakdown, domestic violence, harassment, or discharge from prison/care. One helpful way of conceptualising this comes from Preece and Bimpson (2019), who consider housing insecurity as being made up of three interacting dimensions: (a) financial insecurity, (b) spatial insecurity, and (c) relational insecurity. These are described in box 3.1.

Box 3.1. The Three Dimensions of Housing Insecurity.

1. **Financial Insecurity**: This relates to the affordability of housing and is typically measured by the relationship between income and housing costs, but, importantly, is also determined by other fixed costs and security of employment. Financial insecurity is indicated by housing-related debts and other financial stressors, which in the UK have been impacted by austerity and welfare reform in recent years.

2. **Spatial Insecurity**: This relates to the ability of a household to remain in their dwelling or neighbourhood. This can be determined by the security of their tenancy. In the UK's expanding private rented sector, tenancies have become less secure in recent years through short tenancy agreements and section 21 or 'no fault' evictions, which allow landlords to evict tenants without a reason, giving rise to 'revenge evictions'. Spatial insecurity is also caused by 'un-elective fixity', whereby individuals housing choices are limited, e.g.by refusal of some landlords to let to benefit recipients or people with mental health problems.

3. **Relational Insecurity**: This relates to how an individual's housing and their sense of home is bound to their relationship with other household members. This can include family members living together but also, as is increasingly the case amongst young people, strangers who live together. Insecurity can arise if these relationships break down or if other home sharers are unable to meet rent payments.

Source: adapted from Preece and Bimpson (2019).

3.4.2 Universal Credit and Housing Insecurity: Potential Causal Pathway to Insecurity

Using Preece and Bimpson (2019)'s conceptualisation of housing insecurity set out in Box 3.1, the rollout of UC directly impacts on financial insecurity (but also potentially indirectly on spatial and relational insecurity) as the UK's welfare system plays a key role in providing housing security to low-income households via provision of support towards housing costs. When UC was first announced, the DWP (2010a, p. 19) set out how their aim was "to simplify provision for rent support […] while protecting potentially vulnerable people from

unintended consequences, such as getting into arrears or being made homeless". However, instead UC has threatened housing security as not only is UC less generous than the legacy system for some poorer households (Brewer, Joyce, Waters *et al.*, 2019), but several of its design features are problematic when it comes to helping claimants meet housing costs.

Firstly, UC's long wait period can leave claimants with little or no income to meet housing costs whilst waiting for the first payment. Shelter (2017) have noted that many of their clients have few savings to fall back on during the wait period, and that this results in financial hardship and rent arrears. Whilst the DWP do offer advance payments to those in need of immediate financial support, these then have to be paid back through deductions on future UC payments. Therefore, advances are not a long-term solution because, as noted by Thompson, Jitendra and Rabindrakumar (2019), they effectively leave claimants with the choice of hardship now or hardship later.

Secondly, UC's extension and intensification of the use of conditionality and sanctions has implications for housing security as claimants may struggle to meet housing costs if their UC payments are reduced via sanctions (Beatty, Foden, McCarthy *et al.*, 2015, pp. 35-38). This is because, although UC sanctions do not directly reduce the amount claimants receive towards housing costs, by reducing the standard allowance they make it more likely that claimants will 'borrow' money from their housing costs amount to pay for other essential costs (e.g. food, bills etc.).

Thirdly, UC's novel use of a monthly direct payment system (i.e., monthly in arrears payments, all directly into claimants own bank account rather than paying housing costs to landlord's bank account) also has housing insecurity implications. This is because some UC claimants, particularly young people (see Homeless Link, 2018), may have limited budgeting skills and experience of managing a tenancy. In addition, UC claimants with less disposable income may be forced to 'borrow' money from their housing costs in order to pay for other essential or unexpected costs, which leads to rent arrears. In response to these concerns, 'Alternative Payment Arrangements' (APAs) and 'Scottish Choices' (in Scotland only) have now been put in place to provide the option of more frequent payments and managed payment of housing costs to landlords. However, it has been argued that there remains a lack of awareness of APAs (Hobson, Spoor and Kearton, 2019), and this is backed up by official statistics, which suggest that they are not widely taken up (Department for Work and Pensions, 2018c, p. 8).

The combination of long wait periods, increased conditionality and monthly direct payments under UC have led to widespread concerns amongst housing charities and homelessness charities over UC's housing security impacts (e.g. see Crisis, 2017). The four stages of housing insecurity previously set out in Figure 3.1 are the different stages that are likely to occur on the potential causal pathway from UC to housing insecurity. First, claimants may get into financial problems that lead to difficulties meeting rent payments. For example, it may be that the long wait period leads to a missed payment amongst a UC claimant without savings. Second, if the financial problems persist then more rent payments may be missed, leading to a build up of rent arrears. This can be exacerbated further by UC payments being reduced by sanctions, by claimants having limited budgeting skills to ensure rent payments are met, or if other essential or unexpected costs are prioritised over rent, which is permitted by UC's monthly direct payment system. Next, if claimants – who by this point are likely to also be in other forms of debt – still have no means to pay off arrears, and landlords have exhausted all other options of rent recovery, then formal repossession actions will be made. Finally, if other protective 'buffers' are not in play, e.g. social support networks (see Bramley and Fitzpatrick, 2018), this could result in threat of, or actual, homelessness.

3.4.3 Empirical Evidence on UC's Housing Insecurity Impacts

Literature examining the impacts of UC to date is made up of a mixture of: (a) qualitative studies with UC claimants, and (b) some quantitative studies that have tended to be small-scale and limited to specific localities. Overall, these have tended to consistently highlight the detrimental impact of UC's long wait periods, conditionality and monthly direct payments on housing security, particularly the early stages of insecurity set out in Figure 3.1.

In terms of qualitative evidence, one study by Robertson, Wright and Stewart (2020), which involved interviews, focus groups and 'deliberative' workshops with UC 'Full Service' claimants, found most of their participants to be struggling financially and finding it hard to stay afloat during the five-week wait for the first payment. They also found that many claimants were anxious about losing their home due to rent arrears, and some had already missed rent payments whilst waiting for the first payment, which had triggered "longer-term arrears and worsening debt" (p. 14). Further qualitative research in North East England by Cheetham M, Moffatt S, Addison M *et al.* (2019), involving interviews/focus groups with UC claimants with complex needs, and Britain Thinks (2018), involving

workshops/interviews with UC 'Full Service' claimants, have also highlighted the negative impact of UC's long wait period on housing security. In both studies, participants who did not have savings or the means to receive financial support from friends/family reported experiencing debt and unpaid bills/council tax/rent, with some experiencing threat of or actual eviction as a result. In another study by Bush, Templer, Allen *et al.* (2019), their focus groups with UC claimants in Rochdale and North Tyneside suggest that the combined costs of housing, bills, food, clothing and (where applicable) childcare often pushed claimants to the limit of their incomes, and this was exacerbated by the long wait period between UC payments and often led to debt and 'rotating' which household bills were paid each month. Other qualitative studies have highlighted the impact of UC sanctions on housing insecurity – interviews conducted by Batty (2018) and Wright, Dwyer, Jones *et al.* (2018) have suggested that UC's conditionality regime has led to rent arrears and repossession actions amongst some claimants.

The limited number of quantitative studies examining UC's housing insecurity impacts have tended to be small-scale localised studies focussing on rent arrears. The DWP have been criticised by the National Audit Office (2018, p. 44) for failing to conduct any national, representative analysis of whether UC is leading to increased rent arrears. The only piece of research the DWP have conducted on this has been an analysis limited to one single housing association, which found that, as tenants moved onto UC there was a stark rise in average rent arrears during the initial wait for the first payment, which then plateaued 10-12 weeks after the claim as they began to repay the arrears (see National Audit Office, 2018, pp. 44-45). Similarly, the Smith Institute (2017)'s rent account analysis of social housing tenants in two London Boroughs, using data from 2016, also found increased rent arrears for UC claimants compared to legacy benefit claimants. In a follow up study using data from 2018 (see Smith Institute, 2019), they also note that rent arrears have reduced over time between 2016 and 2018, due to better access to Alternative Payment Arrangements, but that levels of rent arrears still remained higher than under the legacy Housing Benefit system. In terms of quantitative analysis examining the specific impact of UC's monthly direct payment system, there is one study by Hickman, Kemp, Reeve *et al.* (2017) which examines a pilot programme of the direct payment system amongst social housing tenants. Their analysis suggests that the introduction of direct payments acted as a trigger that pushed tenants into debt, and the authors conclude that "although some tenants felt that direct payments enabled them to be better money managers [..] the harsh reality is that only a small minority managed to pay all their rent" (p. 19).

3.5 Concluding Summary

This chapter has provided a review of literature examining the impacts of Universal Credit. Throughout the rollout of UC the DWP have maintained a single-minded focus on improving employment outcomes, with an emphasis on the need to tackle 'welfare dependency' and a perceived 'culture of worklessness'. As set out throughout this chapter, evidence on the impact on UC on employment outcomes to date has been mixed, but research on its impacts on wider outcomes – namely mental health, debt and food bank usage – have consistently suggested that UC has had a detrimental impact. This chapter has also introduced the concept of housing insecurity, how UC's long wait periods, increased conditionality and monthly direct payments can threaten housing security, and existing evidence on this. Whilst existing research on UC's housing insecurity impacts have tended to be qualitative studies, or small-scale quantitative studies limited to specific localities, this thesis seeks to provide a more robust and nationwide analysis by making use of local authority and household level administrative/survey data. The specific data and methodology used in the empirical analysis of this thesis is set out in Chapter 4, which follows.

Chapter 4. Methodology

4.1 Introduction

This chapter sets out the research methods that the empirical chapters of this thesis employ to gain insight into the impact of Universal Credit rollout on housing security in the UK. The thesis includes four empirical chapters in total, using a range of quantitative research methods/designs in order to examine UC rollout's impact on the various stages of housing insecurity. This chapter begins by justifying the selection of quantitative over qualitative methods, whilst also acknowledging that the research questions addressed are informed by, and results interpreted in reference to, existing qualitative literature on UC's impacts. It then justifies the use of existing sources of data and secondary data analysis. Next, the chapter provides a broad overview of the use of natural experimental study designs and methods to improve causal inference within the social sciences, and sets out how these can be applied in the context of measuring the impact of UC rollout. Finally, the chapter ends by providing a summary of the four empirical chapters of the thesis, in terms of the stages of housing insecurity they examine and an overview of the data sources, level of analysis and methods they use. Some more specific details of the data, variables and modelling used in the empirical analysis are not included in this chapter, but instead are contained within the empirical chapters themselves.

4.2 Using Quantitative Research Methods

The analysis outlined in the empirical chapters of this thesis rely upon quantitative research methods to examine the impact of UC rollout on housing security, although the rationale for the research and interpretation of many its findings are partially informed by existing literature on UC from both qualitative and quantitative studies.

Traditionally, literature on social research methods has tended to make this distinction between quantitative and qualitative research because, as set out by Bryman (2015, pp. 31-32), it provides "a useful means of classifying different methods of social research" and "a helpful umbrella for a range of issues concerned with the practice of social research". In the

view of King, Keohane and Verba (1994), the difference between qualitative and quantitative research is not in the logic of inference that underlies the research, but rather is in the style of the research carried out. Quantitative research relies on numbers and statistical models, using numerical measurements to test causal hypotheses in relation to specific aspects of social phenomena (Ibid, p. 3). On the other hand, qualitative research does not rely on numerical measurements, instead tending to focus on a smaller number of cases and unearthing a larger amount of information from research participants e.g. via intensive interviews (Ibid, p. 4). This can be summarised as the difference between: (a) the 'reductionist' approach of quantitative research, whereby there is a narrow but sharp focus on breaking down theories or ideas to test a small discrete set of hypotheses/research questions, and (b) the 'holistic' approach of qualitative research, which aims to develop a more complex picture of the issue/problem of interest and identifying a broader range of factors involved in the situation to provide a more rounded understanding (Creswell and Creswell, 2018). With regards to sampling, quantitative research is often carried out by analysing data from a representative sample of a wider population so that research findings are generalisable (see Bryman, 2015, pp. 170-180), whereas in qualitative research sampling is often chosen based on the methodology and topic rather than by the need for generalisability, and usually is based on who has the best knowledge of the research topic e.g. via a purposive sampling (Elo, Kääriäinen, Kanste *et al.*, 2014; Smith, 2018).

In terms of research methods specifically for generating evidence on the impact of social policies (e.g. welfare reforms), there can be a variety of strengths and weaknesses to both qualitative studies and quantitative studies. These can also vary depending on which specific policy is being examined and what outcomes the research is interested in. For example, whilst medical research has traditionally favoured randomised control trials (RCTs) for generating evidence, this kind of approach can be difficult to undertake when researching the impact of social policies (Barr, Bambra and Smith, 2015) (this will be discussed in detail in section 4.4 of this chapter). Therefore, it has been argued that econometric techniques to examine 'natural experiments', whereby variation in the population's exposure to social policies can be exploited to measure their impacts, can often be the best way of addressing gaps in the evidence base, particularly when considered in combination with qualitative studies (Ibid). Considering the results of quantitative studies alongside the findings of qualitative studies can be particularly important when researching the impact of welfare reforms because, as noted by Patrick (2017, p. 2), the experiences of those directly impacted by welfare reforms are often neglected during political discussions regarding changes to the

benefits system. Importantly, qualitative research provides an opportunity to hear perspectives from those with a direct personal insight into the impacts of welfare reform, who are 'experts by experience' (Ibid). Therefore, quantitative research providing generalisable findings on the impact of welfare reform, considered alongside the findings of existing qualitative studies involving those with direct experience of reforms, can provide the most complete and robust evidence base.

Consequently, as set out in the remainder of this chapter, the empirical chapters of this thesis, whilst informed by existing qualitative studies, employ quantitative research methods to examine the impact of UC rollout on various indicators of housing insecurity. There are several reasons why this approach is particularly well suited here. Firstly, there are already multiple existing qualitative studies which provide some insight into the impact of UC on housing security. This was discussed in detail in Chapter 3. Secondly, the staggered nature of UC rollout means it can be treated as a form of natural experiment – this will be set out in detail in section 4.5 of this chapter. Despite this, there still remains a lack of robust quantitative research into UC's housing security impacts.

Overall, using quantitative research methods, whilst also being informed by existing qualitative studies, can provide a strong balance in terms of filling this gap in knowledge and providing insight into UC's impacts on various indicators of housing security. This is because: (a) indicators of housing insecurity can be tracked over time, before and after UC rollout, exploiting cross-area variation in the timing of rollout in order to measure its impact, and (b) existing qualitative studies can be drawn upon in order to shed light upon some of the key design features of UC (e.g. long wait periods, conditionality and monthly direct payments) that are likely to be contributing to this impact (if any impact is found).

4.3 Conducting Secondary Analysis of Existing Data Sources

In addition to solely employing quantitative research methods, the analysis in the empirical chapters of this thesis also solely employs secondary analysis of existing data sources. The majority of the existing data used in the analysis is local authority-level administrative data. That is, data derived from the operation of administrative systems in government and other organisations, which is usually collected for purposes such as operational monitoring and analysis rather than specifically for research purposes (see Office for National Statistics,

2015; Connelly, Playford, Gayle *et al.*, 2016). Specific local authority-level administrative data sources used include county courts repossessions data collected by the Ministry of Justice, 'advice trends' data on rent arrears/homelessness advice issues collected by Citizens Advice and homelessness data collected by the Scottish Government (this is outlined in section 4.6 of this chapter). In addition, one empirical study (Chapter 8) uses statistical survey data from a large longitudinal study, namely 'Understanding Society'.

In general, there are two approaches when it comes to secondary analysis of existing data sources. First, there is the 'research question-driven' approach, whereby the researcher identifies a hypothesis or research question and then searches for suitable datasets to address them afterwards (Cheng and Phillips, 2014, p. 373). Second, there is the 'data first' approach, whereby the researcher searches through the variables within datasets and only then decides what kind of hypothesis or research question can be addressed (Ibid). The empirical analysis of this thesis used a combination of these two approaches. Initially, a 'research question-driven' approach was used in that evaluating the impact of Universal Credit rollout on housing insecurity was broadly identified as the aim of the research – due to concerns raised by housing charities and early evidence from qualitative studies – before any data sources were identified. Subsequently, a more 'data-first' approach was used in order to identify longitudinal data on housing insecurity indicators and determine which specific research questions could be addressed. This involved using tools such as the UK Data Service and Shelter's housing databank (available from: Shelter, 2019) to search for data sources and identify suitable ones that capture different aspects of housing insecurity.

The key drawback of using existing data sources for analysis is the absence of control over how the data used is collected and what it measures, including how a survey question is worded or who/what is included in admin data (e.g. relating to demographic characteristics of who is included or geographical/temporal coverage of data) (Bryman, 2004). This means that many of the outcome variables used in the analysis, whilst useful indicators of various aspects of housing insecurity, by no means capture the outcomes of interest perfectly. Yet, this limitation is outweighed by the fact that secondary analysis of existing data provided routinely and regularly collected longitudinal data on various outcomes, in some cases covering the time period before UC rollout and throughout the different stages of its rollout. This created the potential for examining the rollout of UC as a form of natural experiment. This is discussed in the following sections of this chapter.

4.4. Causal Inference and the Use of Natural Experiments

In social sciences, research is commonly concerned with examining questions of cause and effect. If causal relationships between variables are known then this tells us what would happen in alternative or "counterfactual" worlds (Angrist and Pischke, 2009, p. 3). Importantly, if research can identify causal relationships, this can be used in order to gain insight into the impact of changing circumstances or of a policy intervention (Ibid).

Traditionally, when it comes to research interested in causal relationships, 'evidence-ranking schemes' or 'hierarchies of evidence' – which rank different research designs based on their perceived ability to infer causality, often in a medical context – have tended to place RCTs (or systematic reviews of RCTs) at the top and viewing them as gold standard evidence (for more in depth discussion on this see Cartwright and Hardie, 2012a; Barr, Bambra and Smith, 2015). In order to be considered as a 'true' experiment, RCTs aiming to establish the cause and effect of an intervention must meet three key principles. These are set out in Box 4.1.

Box 4.1. The Three Key Principles of Randomised Control Trials.

1. Individuals being researched are assigned into either (a) the 'treatment' group, who are exposed to the intervention, or (b) the 'control' group, who resemble the 'treatment' group but who are not exposed to the intervention and thus can be used to measure the counterfactual.

2. The assignment into the 'treatment' or 'control' group is done at random, with participants having an equal chance of being in either group.

3. The manipulation of the intervention is done under the control of an experimental researcher.

Sources: Dunning (2008, p282); Murnane and Willett (2011, p46)

By following these principles, RCTs can, in theory, measure the causal impact of the intervention being examined by comparing the outcomes of the 'treatment group' to the outcomes of the 'control group' to ascertain the "average treatment effect" (Murnane and Willett, 2011, p. 46). This is because when assignment to the treatment or control group is entirely random it is guaranteed to be exogenous to (i.e. independent of) all other characteristics of the recipients. This means that variation in outcomes must be due to the intervention rather than due to these confounding characteristics.

However, despite their high ranking in many 'evidence-ranking schemes' or 'hierarchies of evidence', the use of RCTs in the social policy context is more contentious than in a medical context where such rankings often originate (see Roberts, Petticrew , Macintyre *et al.*, 2008). There are a variety of reasons for this. First, RCTs may be viewed as too expensive or impractical to undertake (Roberts, Petticrew , Macintyre *et al.*, 2008, p. 1; Dunning, 2012, p. 7). Second, randomisation of policies may be politically and ethically difficult – for example, if theory suggests a given intervention is likely to have a positive impact it may not be considered ethical to provide it to some people but withhold it from others (Roberts, Petticrew , Macintyre *et al.*, 2008; Barr, Bambra and Smith, 2015). Third, RCTs may not be generalisable due to geographical heterogeneity. In other words, just because an RCT infers the causal impact of a policy in one population (e.g. an area, a city, or a school etc.) this does not necessarily mean it will also work for a different population, particularly if the second population has different characteristics to the first (see Cartwright and Hardie, 2012b). This is known as the 'transportation problem' (Deaton and Cartwright, 2018). Finally, many social policies of interest to researchers are implemented by policymakers and are therefore outwith the control of most researchers, and policymakers rarely allocate scarce resources in a way that involves true randomisation (Dunning, 2012, p. 7).

These limitations mean that there is a lack of RCTs in the social policy context, and in recent decades researchers have increasingly addressed this gap in the evidence base by examining 'natural experiments'. When it comes to defining the term 'natural experiment', there is no universal definition. Some authors (e.g. Dunning, 2012, pp. 15-16), define it narrowly as studies in which exposure to the given intervention is not manipulated by the researcher, but where exposure (or lack of exposure) to the intervention is still random, or 'as if' random (i.e. not truly random but still independent to any potential confounding variables). Under this definition, a natural experiment is close to a RCT in the sense that principles 1 and 2 of RCTs described in Box 4.1 are essentially met, with the only difference being that the intervention is not manipulated by the researcher. However, this can be rare in reality, and most definitions of 'natural experiment' are more broad than this. For example, one particularly clear definition comes from Craig, Cooper, Gunnell *et al.* (2010):

> By natural experiments, we mean events, interventions or policies which are not under the control of researchers, but which are amenable to research which uses the variation in exposure that they generate to analyse their impact. By natural experimental studies, we mean the methodological approaches to evaluating the impact on health or other outcomes of such events. The key features of these

> definitions are that (1) the intervention is not undertaken for the purposes of research, and (2) the variation in exposure and outcomes is analysed using methods that attempt to make causal inferences. (p. 4)

Under this broader definition, there are various scenarios in which researchers can exploit variation in exposure to policies in order to analyse their impact, provided that accurate data on outcomes of interest is available. For example, Barr, Bambra and Smith (2015), identify three common scenarios. These are: (a) exposure to a policy varies between groups *and* over time, e.g. a policy is introduced in one area (could be a state, local authority, pilot site etc.) but not a neighbouring area, and data on outcomes is available over time in both areas, (b) a policy is introduced in some areas but not others, and only cross-sectional data is available *after* the intervention, and (c) data on exposure to a policy and outcomes is available before and after the intervention, but there is no suitable unexposed comparison group, so the analysis examines change in outcomes only in those exposed (i.e. an interrupted time series).

Importantly, the major limitation of these kind of natural experimental studies is that, in the absence of randomisation (or at least 'as if' randomisation), outcomes in unexposed groups may not be a suitable means of judging the counterfactual in exposed groups. This is because differing outcomes may arise from underlying differences between groups as opposed to the policy intervention (Barr, Bambra and Smith, 2015). However, as will be discussed in regards to examining Universal Credit rollout later in the chapter, there are a range of analytical techniques that can be used in attempt to reduce this kind of bias, such as regression techniques to control for observed differences between groups, use of fixed-effects to account for unobserved differences between groups, robustness checks, falsification tests and/or sensitivity analysis to strengthen the researcher's confidence that observed relationships are causal (Barr, Bambra and Smith, 2015; Stock and Watson, 2015; Craig, Gibson, Campbell *et al.*, 2018).

4.5. Universal Credit Rollout as a Natural Experiment

Despite being hailed by UK policymakers as "the most radical overhaul of our welfare system since its inception" (see Department for Work and Pensions, 2010b), Universal Credit was introduced with little evaluation by the government into its impacts on certain outcomes. The UK Government have conducted research into the impact of UC on

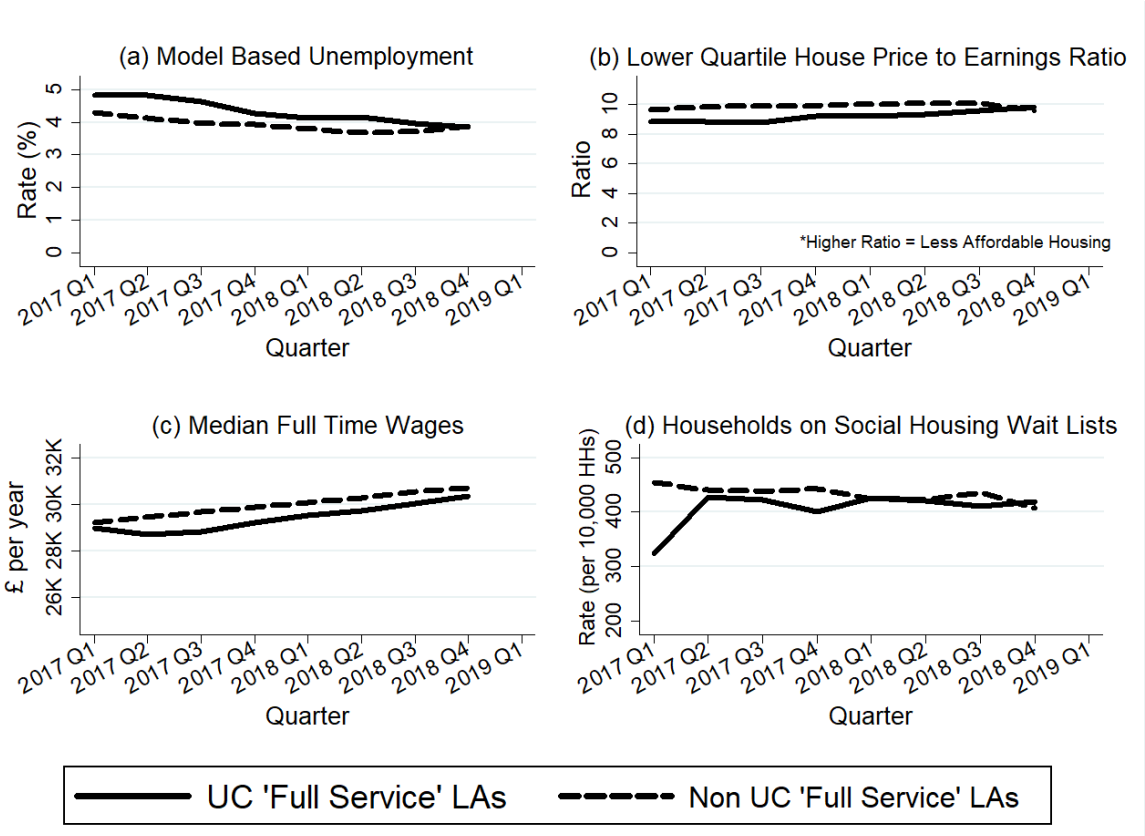
employment outcomes (Department for Work and Pensions, 2017a) and the impact of different levels of work coach support on in-work progression under UC (Department for Work and Pensions, 2018b). However, there has been a lack of analysis by the government into UC's impacts on other outcomes. As already discussed, the DWP have been criticised in particular for a failure to consider UC's impacts on health and wellbeing (Wickham, Bentley, Rose *et al.*, 2020, p. e158), for only undertaking very limited analysis of UC's impact on rent arrears (National Audit Office, 2018, p. 44), and for a general tendency to dismiss any evidence of claimant hardship rather than working with others to establish an evidence base (National Audit Office, 2018, p. 10).

Although the government have not carried out their own national, representative analysis into UC's housing security impacts, the staggered nature of the UC rollout schedule – i.e. the fact that it rolled out in different areas at different times between 2013 and 2018 – means that exposure to the policy varied across time and space. As such, it can be treated as a form of natural experiment in that variation in exposure to the policy over time makes UC amenable to research analysing its impact (Craig and Katikireddi, 2020). As set out in section 4.4, the ideal scenario in terms of confidently making causal claims in natural experimental studies is where variation in exposure to the intervention of interest is truly random (or at least 'as if' random whereby exposure is not truly random but is still independent to any potential confounding variables). Therefore, the ideal scenario to make causal claims on UC's impact would be if the variation in its rollout across time and space was truly random. This was not the case. The DWP have not formally stated the basis for which the order of rollout was determined, but their research has noted that rollout was not random, but rather was "designed, in part, to be deliverable" (Department for Work and Pensions, 2014). This can threaten the validity of making causal claims on UC's impact if there are confounding variables linked to both the timing of UC rollout and outcomes of interest relating to housing insecurity. This phenomenon is also known as 'omitted variable bias' (see Stock and Watson, 2020, p. 213). One way to examine potential cause for concern in this regard is to look at differences in labour and housing market characteristics between areas that became UC 'Full Service' areas earlier in the rollout schedule, and areas that became UC 'Full Service' areas later in the rollout schedule.

This is done using data for England in Figure 4.1, and the results suggest that there are minor differences. Areas where 'Full Service' rolled out earlier tended, on average, to have slightly higher unemployment rates, slightly lower wages and slightly less affordable housing than

areas where it rolled out later. However, whilst this may introduce some bias, methods such as a fixed-effects design, inclusion of control variables and falsification tests can be used to attempt to reduce the influence of any bias arising from non-randomness in the UC rollout schedule. In addition, analysis by Reeves and Loopstra (2020) in their paper examining UC's impact on food bank usage suggests that although UC was initially introduced in areas with slightly higher than average levels of deprivation, when they considered the speed at which new claimants enter UC by the period in which UC was implemented, they found similar trends in pace of rollout for areas with different levels of deprivation. Based on this, they conclude that bias arising from the timing of UC rollout was unlikely to influence the results of their analysis (Ibid, p. 6).

Figure 4.1. Non-Randomness in UC rollout in England: Quarterly Trends in Mean Housing and Labour Market Characteristics in UC 'Full Service' versus non UC 'Full Service' Local Authorities, 2017-2018.



Notes: The number of local authorities that were UCFS areas gradually increased over time as rollout progressed – 10% of local authorities were UCFS areas by 2017 Q1, increasing to 17% by 2017 Q3, 37% by 2018 Q1, 65% by 2018 Q3 and 85% by 2018 Q4. Data Sources: NOMIS official labour market statistics, Office for National Statistics 'Annual Survey of Hours and Earnings' and Ministry of Housing, Communities and Local Government Live Tables.

4.6. Summary of Empirical Chapters

The overall aim of this thesis is to examine the impact of UC rollout on housing security, and this is done via four empirical studies (Chapters 5-8). Each empirical study exploits cross-area variation in the rollout schedule – as outlined in section 4.5 – in order to examine its impact on the different indicators of housing insecurity arising for financial reasons in UK (i.e. the four stages of housing insecurity set out in Chapter 3). Box 4.2 provides details of various potential data sources indicating insecurity at each stage. A summary of the specific stages of housing insecurity examined in each empirical study, along with the data sources actually used to measure that insecurity, is provided in Table 4.1. These data sources were used over the other sources listed in Box 4.2 because: (a) these data sources were consistently available throughout the rollout of UC, (b) they were available at the local authority level so could be linked to data on the timing of UC rollout, and (c) with the exception of Understanding Society data, they were collected quarterly or monthly meaning they could be more accurately linked to data on the exact timing of UC rollout within local authorities.

The analysis in each of the empirical chapters employ a range of methods in attempt to reduce potential sources of bias, e.g. bias potentially arising from any non-randomness in UC's rollout schedule. Details of the methods used in each chapter are also summarised in Table 4.1.

Box 4.2. The Stages of Housing Insecurity and Data Sources Indicating Insecurity at Each Stage

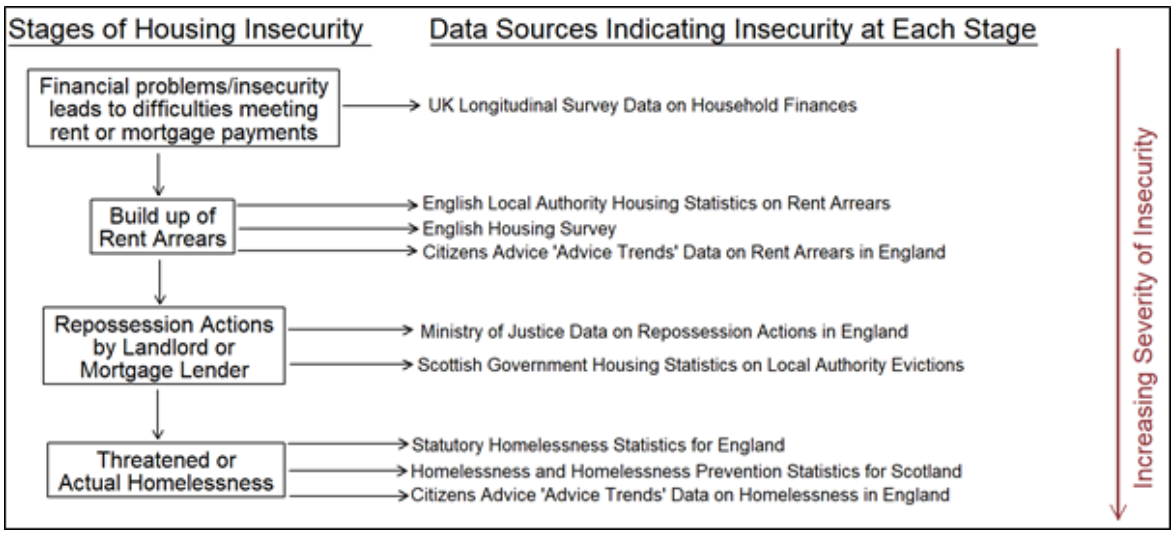


Table 4.1. Summary of the Four Empirical Chapters of this Thesis

Chapter	**Stage of Housing Insecurity Examined**	**Data Source Indicating Insecurity**	**Level of Analysis**	**Methods to Reduce Potential Sources of Bias**
Chapter 5	Repossession Actions	Quarterly Ministry of Justice Data on Repossession Actions	English Local Authority Level	Fixed Effects Panel Design & Falsification Test
Chapter 6	Rent Arrears & Threatened/Actual Homelessness	Quarterly Citizens Advice 'Advice Trends' Data on Rent Arrears & Homelessness	English Local Authority Level	Fixed Effects Panel Design & Falsification Test
Chapter 7	Threatened/Actual Homelessness	Monthly Scottish Homelessness & Prevention Data	Monthly Scottish Local Authority Level	Fixed Effects Panel Design
Chapter 8	Household Financial Problems	Longitudinal Survey Data	UK Individual & Household Level Data with Local Authority Identifiers	Difference-in-Differences

Firstly, Chapter 5 examines the impact of UC rollout on landlord repossessions rates within English local authorities. This was done using a fixed effects panel design, linking data on the timing of UC rollout in each local authority from its official rollout schedule (available from: UK Government, 2015a; UK Government, 2015b; UK Government, 2018 and listed in full in Appendix 1) with quarterly data on the rates of landlord repossession actions (including repossession claims, orders, warrants and actual bailiff repossessions) between 2012 Q1 and 2019 Q1. To improve casual inference, a falsification test (i.e. a test to assess the plausibility that the effect of the explanatory variable on the outcome variable is causal by checking the specificity of the effect (see Craig, Katikireddi, Leyland *et al.*, 2017, p. 51)) was also carried out by repeating the analysis using mortgage repossession rates as a non-equivalent outcome variable. A non-equivalent outcome variable is a new outcome variable that is "predicted not to change because of the treatment but […] expected to respond to some or all of the contextually important internal validity threats in the same way as the target outcome" (Shadish, Cook et al., (2002, p509). The falsification test is set out in full in Chapter 5 itself.

Secondly, Chapter 6 examines the impact of UC rollout on rates of advice given by Citizens Advice on issues relating to rent arrears and homelessness within English local authorities. In a similar research design to Chapter 5, a fixed effects panel design is used, linking data on the timing of UC rollout with quarterly data on rates of advice given on rent

arrears/homelessness issues between 2014 Q1 and 2019 Q1. As in Chapter 5, a falsification test was carried out to improve causal inference, using data on rates of advice given on mortgage and secured loan arrears as a non-equivalent outcome variable. In addition, data from DWP StatXplore on the pace of UC rollout in the social versus private rented sectors was used to examine whether there was any variation in the impact of UC rollout between the different sectors.

Thirdly, Chapter 7 examines the impact of UC rollout on rates of homelessness applications and 'Housing Options' approaches (i.e. approaches for the information and advice service used by local authorities in Scotland in attempt to prevent homelessness when a household approaches them with a housing problem) within Scottish local authorities. As in Chapter 5 and 6, a fixed effects panel design was used linking the claims/approaches data to data on the timing of UC rollout to create a monthly local authority level dataset covering the period April 2014 – March 2019. In addition, the homelessness claims data was disaggregated in order to assess whether there was any variation in the impact of UC rollout depending on what the reason for the claim was (e.g. asked to leave home, violent/non-violent household dispute, terminated tenancy) and what their prior circumstances were (e.g. staying in family home, with friends/partner, private rented sector, social rented sector or owner occupying). It was not possible to conduct a falsification test for this chapter's analysis due to lack of data on a suitable non-equivalent outcome variable.

Finally, Chapter 8 examines the impact of UC rollout on household financial problems such as subjective difficulties managing financially and self-reported difficulties meeting payments for housing, bills or council tax. Unlike in the other empirical chapters, the outcome variables here are individual level data, obtained from the 'Understanding Society' longitudinal survey, also known as the UK Household Longitudinal Study (UKHLS). This was linked to local authority level data on timing of UC rollout to enable difference-in-difference analysis to measure UC's impact. This analysis is split into two parts, with part 1 examining the initial impact of UC in the first 12 months of the 'Live Service' phase of rollout, and part 2 taking a more longitudinal approach using data from waves 1-9 of the survey. A more detailed description and explanation of all of the data, variables, methods and modelling used in each empirical chapter will be provided within the chapters themselves.

4.7. Concluding Summary

This chapter has outlined the broad methodological approach employed in this thesis, setting out how the empirical analysis uses quantitative research methods on existing data sources, and how cross-area variation in the UC rollout schedule is exploited in order to measure its impact. In addition, this chapter has provided a summary of the four empirical chapters of this thesis, in terms of the stages of housing insecurity they examine and an overview of the data sources, level of analysis and methods they use to reduce the potential influence of bias. The empirical chapters themselves – which follow this chapter – provide a more detailed account of the data, variables and methods used in each piece of analysis, as well as setting out the results and their policy implications.

Chapter 5. Empirical Study 1: Impact of Universal Credit Rollout on Landlord Repossession Rates

5.1 Introduction

This chapter examines the impact of Universal Credit rollout on landlord repossession rates within 323 English local authorities. As set out in Chapter 4, this is done by employing a fixed effects panel design, linking data on the timing of UC rollout (from its official rollout schedule – see Appendix 1) with aggregate Ministry of Justice data on legal repossession actions made by landlords (across both social and private rented sectors in England) between 2012 Q1 and 2019 Q1. The specific research questions addressed in this chapter are as follows:

1. Has UC rollout led to an increase in rates of landlord repossession actions (i.e. rates of legal actions by landlords to evict tenants) within 323 English local authorities up to 2019 Q1?

2. Does the impact of UC rollout on rates of landlord repossession actions (if any found) increase when it has been rolled out for longer and thus reached more claimants?

This chapter begins by providing an overview of the determinants of eviction. Whilst Chapter 3 has already introduced the concept of housing insecurity, the determinants of eviction are useful to set out here to provide some additional context on factors likely to shape the likelihood of households facing repossession actions, which is of specific relevance to this chapter. Following on from this, this chapter provides a detailed account of the data, variables, methods, and modelling used in the chapter's empirical analysis. The Chapter will then present the results of the empirical analysis, before ending with a conclusion which summarises the research findings, discusses the limitations of the analysis and considers what inferences can be drawn.

It is important to note that this chapter's analysis has been published in the following journal article in the Journal of Social Policy:

> Hardie, I. (2021). The Impact of Universal Credit Rollout on Housing Security: An Analysis of Landlord Repossession Rates in English Local Authorities. Journal of Social Policy, 50(2), 225-246. doi:10.1017/S0047279420000021.

5.2 Background: Determinants of Eviction

Where repossession of a property through eviction takes place, it often arises due to the same "trigger factors", which typically centre on income reduction due to unemployment, relationship breakdown or a combination of other factors (Pleace and Hunter, 2018). As set out by Chamberlain and Johnson (2013), experience of a "financial crisis" is a key pathway to eviction and homelessness, and this can occur for a variety of reasons. For example, financial crises may arise from: (a) persistent low-income, which can lead to hardship and an inability to afford housing and other essential costs (e.g. food, heating and bills), or (b) a sudden income loss, which may arise from a job loss or the collapse of a small business, resulting in the household no longer being able to meet housing costs (Ibid, p. 64). Qualitative research conducted with tenants in serious rent arrears (and thus at risk of eviction) suggests that, in the UK specifically, benefit complications/suspensions and insecure employment (e.g. temporary or zero-hour contracts) are also key triggers (Ambrose, Eadson, Hickman *et al.*, 2015).

Whilst eviction is often triggered by experiencing a financial crisis, it is also caused by a range of individual and structural factors which are key to determining the prevalence of eviction. With regards to individual factors, there are certain groups who face greater risk of eviction or tenancy breakdown than others. For example, analysis of tenancy sustainment amongst those aged under 35 by Ambrose, Eadson, Hickman *et al.* (2015) suggests that single adults, men and 21-25 year olds were the groups at most risk of eviction. The authors note that greater eviction risk amongst young people is partially related to the fact that they are more likely to be unemployed or on very low incomes during their tenancy, but that being aged 21-25 carries a risk that goes beyond any of these associated variables (Ibid, p. 3). Similarly, research by Pawson, Donohoe, Munro

et al. (2006) and Pawson and Munro (2010) also highlights the disproportionate risk for young people, single men, and childless couples, when it comes to tenancy breakdown. Finally, other research has highlighted the specific risks of eviction faced by former homeless people (Randall and Brown, 2002) and those who have previously served in the armed forces (Johnsen, Jones and Rugg, 2008).

With regards to structural determinants of eviction, these are linked to housing policy, the housing market (particularly housing affordability), poverty and landlord practices. The UK's private rented sector (PRS) has grown markedly over the past two decades, with people increasingly remaining in the sector into their 30s, and the sector increasingly housing young adults who are in low-income poverty (Bailey, 2020). This rise of the PRS has a significant impact on housing affordability, as 70% of the poorest fifth of households who live in the PRS face housing costs that account for over a third of their total income (Tinson, Ayrton, Barker *et al.*, 2016, p. 10). This unaffordability of housing is said to be one of the key drivers of forced moves and eviction in the UK (Clarke, Hamilton, Jones *et al.*, 2017, p. 17), with Pleace and Hunter (2018, pp. 334-335) noting that there is now a chronic shortage of affordable housing in the UK and a growing gap between income and housing costs in many areas. Another key structural determinant of eviction in recent years has been the use of section 21 or "no fault" evictions, whereby landlords can repossess a property without having to establish fault on the part of the tenant. The increasing use of section 21 drove a significant rise in overall evictions between 2000 and 2014, and was particularly prominent in London, where market rents were rising rapidly and landlords commonly evicted tenants in order to re-let the property at higher rates or as short-term lets (e.g. via Airbnb) (Clarke, Hamilton, Jones *et al.*, 2017). However, the use of section 21 has declined since 2014, and the government have now vowed to ban them (UK Government, 2019).

5.3 Data, Variables and Methods

5.3.1 Setting

As previously intimated in Chapter 4, the empirical analysis in this chapter involved the compiling of a quarterly local authority level dataset, which links data on the timing of

UC rollout with Ministry of Justice data on landlord repossessions. This dataset covers the period of 2012 Q1 – 2019 Q1, and the final analytical sample included 323 of the 326 lower tier local authorities in England. Isles of Scilly, West Somerset and City of London were the three local authorities that were excluded, and this was due to their small population sizes.

5.3.2 Repossessions Data (Outcome Variables)

The process for landlords (or mortgage lenders) repossessing a property in England is carried out via the county courts, and occurs in four stages (Ministry of Justice, 2019a). Firstly, the landlord must make a 'repossession claim' to the court in order to establish whether they have the right to repossess the property. Secondly, if the court agrees that the landlord has the right, a 'repossession order' will be granted. Next, in the third stage, the landlord can apply for a repossession warrant. If a warrant is issued this leads to the fourth and final stage of the process, which is a formal bailiff repossession (i.e. actual eviction carried out by a bailiff). The most common reason for repossession actions to occur is rent arrears (Ministry of Justice, 2015, p. 3). The Ministry of Justice publish data on each stage of this process, i.e. the quarterly number of landlord repossession claims, orders, warrants, and actual repossessions within each local authority. It is important to note that the latter stages are often not reached because tenants may pay off their arrears/leave the property voluntarily before latter stages are required, or a judge may decide not to make a repossession order. It has been estimated that approximately 72% of landlord repossession claims lead to repossession orders, 40% to repossession warrants and only 21% to actual bailiff repossessions (Clarke, Hamilton, Jones *et al.*, 2017).

For the purposes of analysis, this data on the number of landlord repossession claims, orders, warrants and bailiff repossessions was converted into rates per 10,000 rented dwellings in the given local authority (using Office for National Statistics 'Subnational Dwelling Stock by Tenure Estimates' and Ministry of Housing, Communities and Local Government (MHCLG) 'live tables on dwelling stock'). This led to the creation of four outcome variables for the analysis, with one variable for each of the four stages of the legal repossessions process. These are: (1) '*landlord repossession claim rate*', (2) '*landlord repossession order rate*', (3) '*landlord repossession warrant rate*', and (4)

‘*landlord bailiff repossession rate*’. It was not possible to disaggregate this data by tenure, so these variables indicate rates of actions carried out by both social landlords and private landlords. However, it should be stressed that the behaviour and motivations for social landlords cannot be viewed in the same way as private landlords, especially where the social landlord is a council who also have statutory duties around homelessness.

5.3.3 Universal Credit Data (Explanatory Variables)

Modelling within this chapter focusses on the timing of UC rollout within English local authorities. As noted in Chapter 4, data on the timing of UC rollout comes from the official UC rollout schedule, which is available from UK Government (2015a; 2015b; 2018) and is also listed in a single table in Appendix 1. This data was used to track each of the 323 English local authorities over time and create three explanatory variables for the analysis. These specify which stage of rollout they were at during each quarter between 2012 Q1 and 2019 Q1. The first explanatory variable is ‘*UC Live Service*’, which is a binary variable indicating whether UC ‘Live Service’ had rolled out yet in each local authority in each quarter of the analysis period. Second, ‘*UC Full Service*’, also a binary variable, indicates whether UC ‘Full Service’ had rolled out yet in each quarter of the analysis period. Third, ‘*UC Full Service (by length of rollout)*’ is a categorical variable indicating whether ‘Full Service’ had rolled out yet and if so for how long – this entail five categories: (1) ‘*pre rollout*’, (2) ‘*first quarter post rollout*’, (3) ‘*second quarter post rollout*’, (4) ‘*third quarter post rollout*’, and (5) ‘*fourth + (i.e. fourth or more) quarters post rollout*’.

For the purposes of these variables, for the quarter in which local authorities transitioned into ‘Live’ or ‘Full’ Service UC they were classed as a ‘Live’ or ‘Full’ Service area if the corresponding rollout date occurred in the quarter’s first half but not if it was in the quarter’s second half. In a minority of local authorities (29 out of the 323 included in the analysis), UC ‘Full Service’ rolled out in different Jobcentres in the area in different quarters. When this occurred, local authorities were classed as ‘Full Service’ areas from the first quarter in which ‘Full Service’ had rolled out in most Jobcentres for most of the quarter.

5.3.4 Control Variables

As set out in section 5.2, there are a range of individual and structural determinants of eviction, with labour market factors (e.g. unemployment and low-pay) and housing market factors (e.g. housing affordability) being particularly important. In attempt to account for this, three control variables were used in this chapter's analysis. These make use of the (limited) local authority level data available on housing and labour market eviction determinants. These are: (1) '*model based unemployment rate*', (2) '*median weekly wages*', and (3) '*mean weekly rents*'.

'*Model based unemployment rate*' comes from NOMIS official labour market statistics and is an estimate of the quarterly unemployment rate (%) in the local authority. It is based on the previous twelve months of data from the 'Annual Population Survey'. '*Median weekly wages*' comes from the 'Annual Survey of Hours and Earnings' (carried out by the Office for National Statistics). It is a quarterly estimate of median weekly wages from part/full time work in the local authority and is obtained through linear interpolation of the annual estimate from the survey. Finally, '*mean weekly rents*' is the mean weekly rents paid in the private rented sector (data from 'Valuations Office Agency Private Rental Statistics') and the social rented sector (covering tenancies through housing associations and, where applicable, local authorities – data from the MHCLG).

5.3.5 Statistical Analysis

Making use of the outcome variables and explanatory variables outlined above, the relationship between UC rollout and landlord repossession rates was formally investigated by using fixed effects panel models. Fixed effects modelling is a common approach for analysing panel data, and it measures change over time within local authorities. The key features of fixed effects panel modelling as a statistical technique are provided in Box 5.1 (adapted from Gayle and Lambert, 2018, p. 61). The key advantage of using fixed effects panel models here is that local authority fixed effects effectively control for unobserved baseline differences between local authorities. Meanwhile the inclusion of time fixed effects also effectively controls for unobserved

variables that vary over time but not between local authorities (for a full explanation of time fixed effects see Stock and Watson, 2015). In addition, fixed effects regression models can also include additional control variables, which control for potential confounders that are observed. This is done here through inclusion of the control variables set out above in section 5.3.4.

Box 5.1. The Key Features of Fixed Effects Panel Models.

The Fixed Effects Panel Model

One of the two most widely used models for analysing panel data (along with the 'random effects' panel model)

Measures change over time within an entity (e.g. an individual/school/local authority etc.) as opposed to between entities

Can include explanatory variables that change over time within the entity (e.g. if the entity you have data on is an individual this could include, for example, age/income/body mass index)

Cannot include variables that are time constant for the entity (e.g. if the entity you have data on is an individual this could be, for example, place of birth, birth weight or father's occupation when aged 14)

Has the attractive property of being able to control for unobserved differences between entities

Source: adapted from Gayle and Lambert (2018, p. 61)

The empirical analysis of this chapter is conducted in two parts. The first part examines the overall impact of UC rollout on average within local authorities, up to 2019 Q1, and makes use of the '*UC Live Service*' and '*UC Full Service*' explanatory variables. The specific modelling is as follows:

$$LR\ Rate_{it} = \beta_0 + \beta_1 UCLS_{it} + \beta_2 UCFS_{it} + \beta_3 Unemployment_{it} + \beta_4 Wages_{it} + \beta_5 Rents_{it} + \beta_6 Quarter_t + \alpha_i + u_{it} \quad (5.1)$$

In equation 5.1, i is the local authority and t is the quarter. $LR\ Rate$ is the landlord repossession rate, with a separate fixed effects regression models for each of the '*landlord repossession claim rate*', '*landlord repossession order rate*', '*landlord repossession warrant rate*', and '*landlord bailiff repossession rate*' outcome variables. $UCLS$ and

$UCFS$ are the '*UC Live Service*' and '*UC Full Service*' binary explanatory variables, whilst $Unemployment$ is the '*model-based unemployment rate*' variable, $Wages$ is the '*median full-time wages*' variable, and $Rents$ is the *'mean weekly rents'* variable. Finally, $Quarter$ denotes the time fixed effects, α_i the local authority fixed effects and u_{it} the error term.

The second part of this chapter's analysis examines whether the impact of UC rollout on rates of landlord repossession actions increases when it has been rolled out for longer and thus reached more claimants. This is done by making use of the '*UC Full Service (by length of rollout)*' explanatory variable, and the specific modelling for this part is as follows:

$$LR\ Rate_{it} = \beta_0 + \beta_1 UCFS\ By\ Length_{it} + \beta_2 Unemployment_{it} + \beta_3 Wages_{it} + \beta_4 Rents_{it} + \beta_5 Quarter_t + \alpha_i + u_{it} \quad (5.2)$$

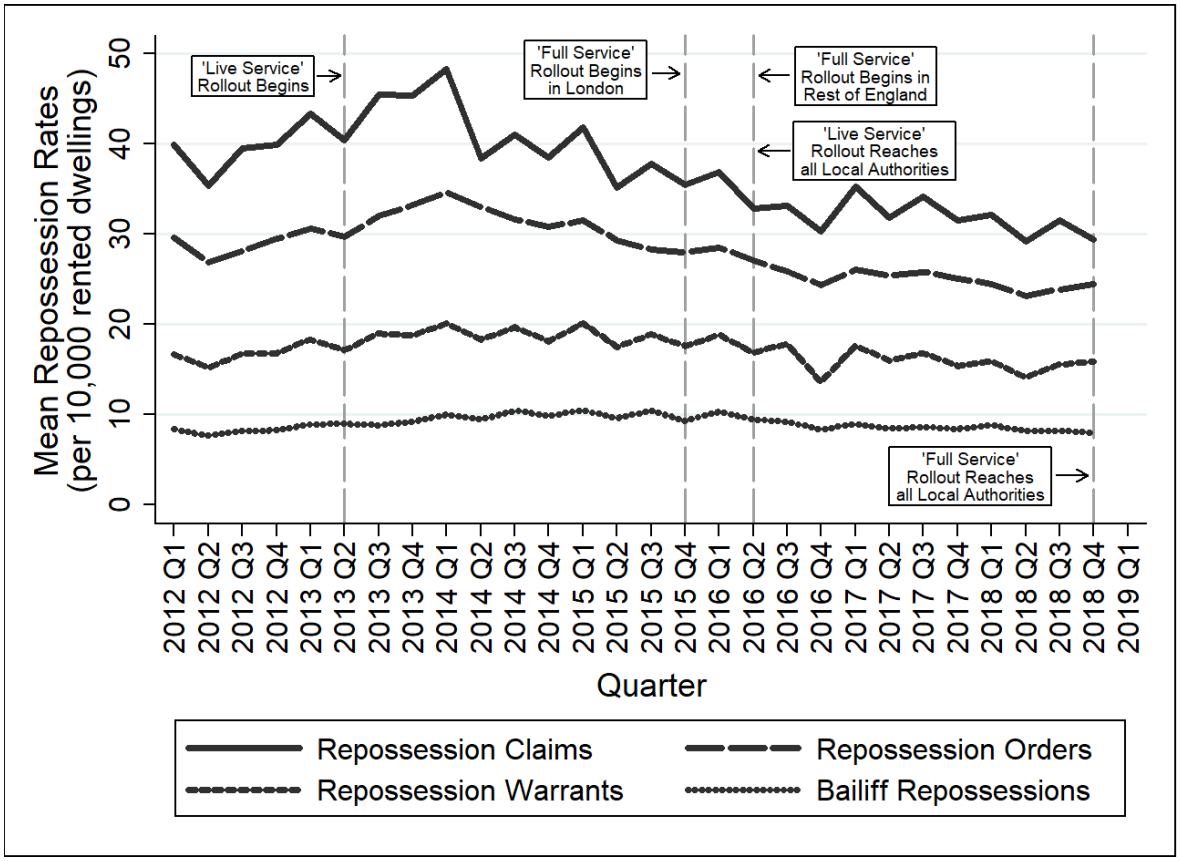
Here, $UCFS\ By\ Length$ is the '*UC Full Service (by length of rollout)*' outcome variable. All other variables are the same as those already included in equation 5.1 and set out above.

5.4 Results

5.4.1 Overall Trends in Landlord Repossession Rates (and their Relationship with UC Rollout)

Figure 5.1 provides the quarterly trends in mean landlord repossession rates during the analysis period. It highlights that, overall, there was (on average across local authorities) a downward trend in rates of repossession actions between 2014 and 2019. This can be attributed to the declining use of section 21 evictions since 2014 (as already discussed in section 5.2 of this chapter - see Wilson, 2019b, p. 8).

Figure 5.1. Quarterly Trends in Mean Landlord Repossession Rates Across English Local Authorities, 2012 Q1 – 2019 Q1.



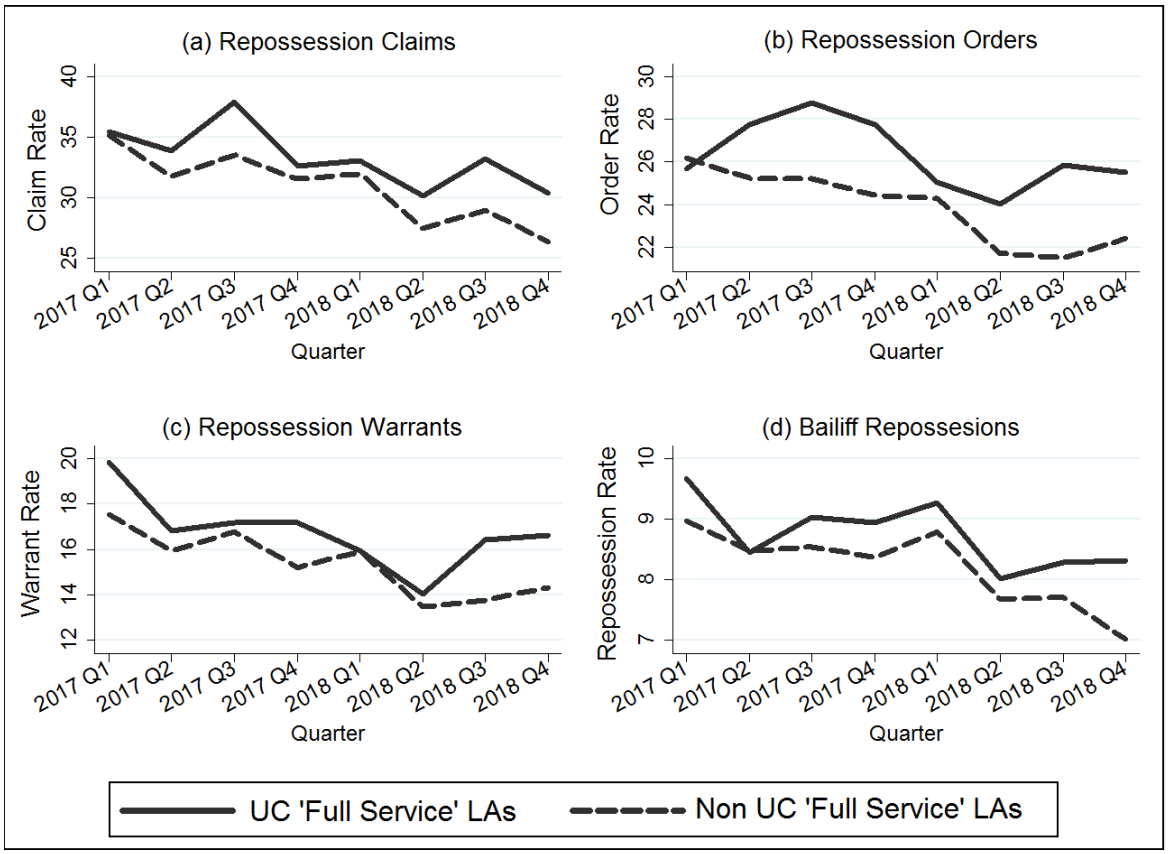
Notes: Data includes actions by both private and social landlords.

Mean rates of landlord repossession rates varied depending on whether or not a local authority had become a UC 'Full Service' (UCFS) area yet. Figure 5.2 again shows trends in mean landlord repossession rates, but this time disaggregates the data into UCFS and non-UCFS areas, i.e. local authorities where UC 'Full Service' had rolled out versus local authorities where it hadn't yet in the given quarter. It conveys that, on average, UCFS areas tended to have higher repossession rates than non-UCFS areas at any given point in time. Figure 5.3 similarly shows that in local authorities with a higher rate of households on UC receiving housing costs support (another indicator of UC rollout using DWP Stat-Xplore data) also tend to have higher repossession rates.

Taken together, Figures 5.1 and 5.2 suggest that areas whether UC had rolled out, and have a higher incidence of people claiming UC for help with housing costs, had higher landlord repossession rates. It is possible that some of this correlation may be due to the impact UC has had on housing security. However, it is also likely to be linked to the slight differences in the characteristics of local authorities that became UCFS areas

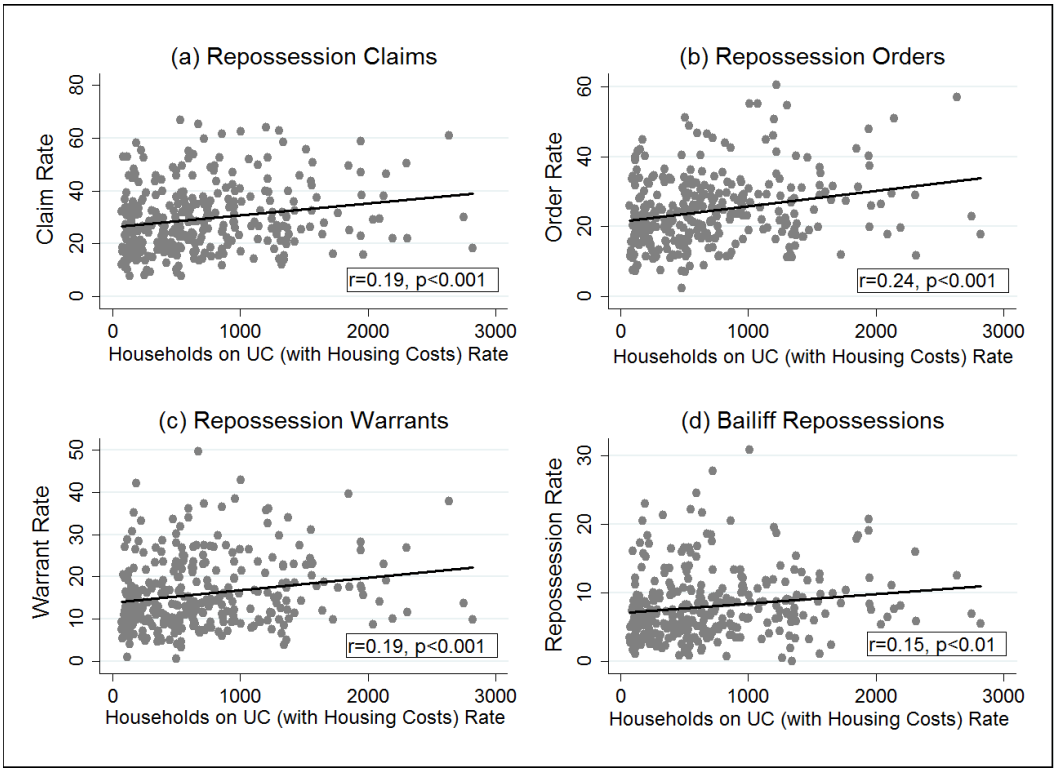
earlier compared to those that became UCFS areas later. This is because, as already set out in Chapter 4, areas that became UCFS earlier tended to have slightly higher unemployment rates, slightly lower wages and slightly less affordable housing than areas where it rolled out later. It is this uncertainty (relating to differences in characteristics between areas that became UCFS earlier versus later) which motivates the need for using fixed effects regression in order to examine variation in landlord repossession rates within local authorities over time rather than between local authorities.

Figure 5.2. Quarterly Trends in Mean Landlord Repossession Rates in UC 'Full Service' versus Non-UC 'Full Service' Local Authorities, 2017-2018.



Notes: The number of local authorities that were UCFS areas gradually increased over time as rollout progressed – 10% of local authorities were UCFS by 2017 Q1, rising to 17% in 2017 Q3, 37% by 2018 Q1 and 85% by 2018 Q4. Data includes actions by both social and private landlords. Y axes indicate mean repossession rates per 10,000 rented dwellings.

Figure 5.3. 2018 Q1 Snapshot of the Relationship Between the Rate of Households on UC (with Support for Housing Costs), and Landlord Repossession Rates.



Notes: All Rates are per 10,000 Rented Dwellings in the Local Authority. Source of UC data: DWP Stat-Xplore.

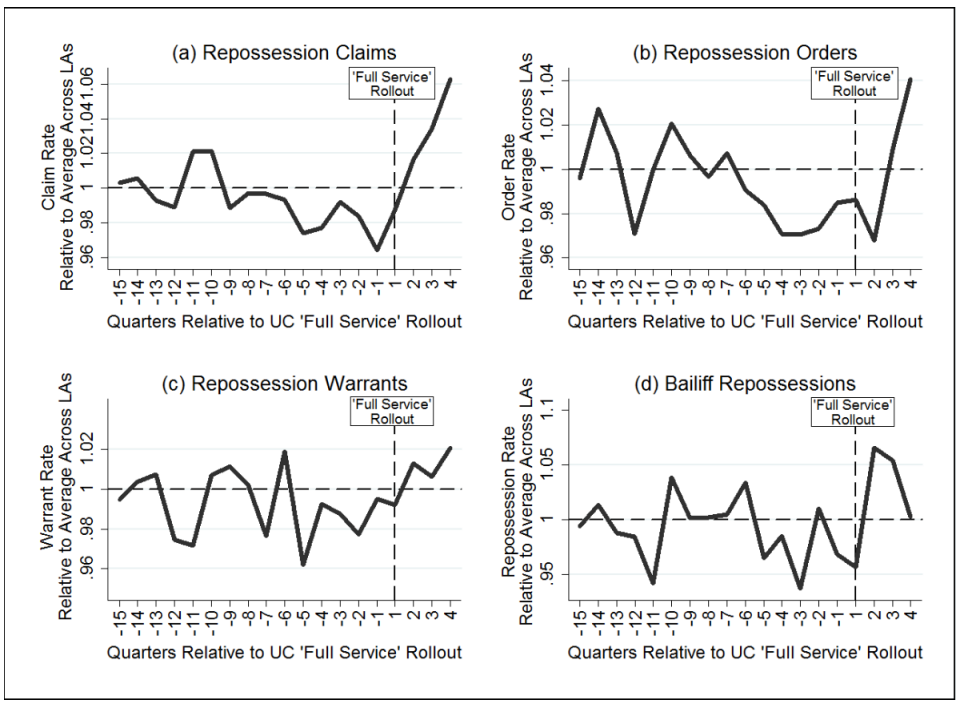
5.4.2 Overall Impact of Universal Credit Rollout

Trends in mean landlord repossession rates in the quarters before and after UC 'Full Service' rollout within local authorities are shown in Figure 5.4, with time being adjusted to be relative to the rollout date in the given area. Rates are shown as a ratio to the average across local authorities for the given quarter – this removes the influence of the secular downward trend since 2014 (as apparent in Figure 5.1 above). Panels (a) and (b) of Figure 5.4 suggest a clear rise in claims and orders following UC 'Full Service' rollout, with a smaller, and much less clear rise in warrants and bailiff repossessions shown in panels (c) and (d).

The relationships between UC rollout and the four separate landlord repossession measures are modelled formally in the fixed effects regression models in Table 5.1. This is the first part of the analysis set out in section 5.3.5, and it examines the overall impact of UC rollout, on average within local authorities, up to 2019 Q1. No significant relationship was found between UC 'Live Service' rollout and any of the four landlord

repossession rates. This is, most likely, because as set out in Chapter 2 'Live Service' rollout involved a relatively small number of people as it was targeted only at those whose claims were most simple to manage. This, combined with the fact that 'simple' claims may have been less likely to involve housing costs, makes it unsurprising that 'Live Service' would lead to a significant impact that could be picked up at the local authority level. However, a significant relationship was observed between UC 'Full Service' and landlord repossession rates. To be specific, after accounting for the unemployment rate, wages and mean rents, 'Full Service' rollout was associated with an increase of 1.74 landlord repossession claims, 1.42 landlord repossession orders and 0.70 landlord repossession warrants (all per 10,000 rented dwellings). To provide some context on what these figures mean in terms of the scale of the impact, the mean landlord repossession rates in the period immediately before 'Full Service' rollout within local authorities (i.e. 2015 Q1 – 2015 Q4) was 37.6 claims, 29.4 orders and 18.5 warrants (all per 10,000 rented dwellings – see Figure 5.1). Therefore, based on this, the observed increases associated with 'Full Service' rollout correspond to a 4.6% increase in claims, 4.8% increase in orders and 3.8% increase in warrants up to 2019 Q1.

Figure 5.4. Quarterly Trends in Mean Landlord Repossession Rates (Relative to the Average Across Local Authorities) Within English Local Authorities, Before and After UC 'Full Service Rollout.



Notes: only includes data on the 136 local authorities with repossessions data available to four or more quarters post rollout. Y axes give the mean of the ratio between landlord repossession rates and the average across the 136 local authorities in the given quarter.

Table 5.1. Relationship Between UC Rollout and Landlord Repossession Rates Within 323 English Local Authorities, 2012 Q1 – 2019 Q1.

	(1) Claim Rate	(2) Order Rate	(3) Warrant Rate	(4) Bailiff Repossession Rate
UC 'Live Service' Rolled Out:				
[No] *N=4617*				
Yes *N=4750*	-0.33 (0.53)	0.02 (0.48)	-0.96 (0.31)	-0.29 (0.23)
UC 'Full Service' Rolled Out:				
[No] *N=8065*				
Yes *N=1302*	1.74** (0.61)	1.42* (0.62)	0.70* (0.28)	0.42 (0.30)
Model Based Unemployment Rate	0.52 (0.32)	0.08 (0.29)	-0.09 (0.18)	-0.09 (0.15)
Per £100 increase in **Median Weekly Wages**	-0.77 (0.72)	-0.78 (0.56)	-0.53 (0.31)	-0.57** (0.19)
Per £10 increase in **Mean Weekly Rents**	-2.22*** (0.47)	-1.09* (0.44)	-0.74* (0.34)	-0.53* (0.22)
Local Authority Quarters (*N* total)	9367	9367	9367	9367
R^2	0.278	0.178	0.114	0.061

Notes: Driscoll-Kray standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. N refers to the number of local authority quarters. Landlord repossession rates are per 10,000 rented dwellings in the local authority. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

5.4.3 Impact by Length of Rollout

One drawback of the fixed effects regression models set out in Table 5.1 is that, because they use a simple binary measure of 'Full Service' rollout, they treat all local authority quarters post-rollout as being the same. This is over simplistic as in reality UC's impact is likely to be greater where it has been rolled out for longer. This is because it takes time for new UC claims to be made after 'Full Service' rolls out in an area, and more time for its effects on housing insecurity to become apparent through repossession actions. Consequently, the relationship between UC 'Full Service' rollout and landlord repossession rates, by length of rollout, is formally modelled in Table 5.2. This is the second part of the analysis set out in section 5.3.5, and – by splitting local authority

quarters post 'Full Service' rollout based on rollout length – examines whether UC's impact is greater when it has been rolled out for longer and thus reached more claimants.

Table 5.2. Relationship Between UC 'Full Service' Rollout and Landlord Repossession Rates Within 323 English Local Authorities, By Length of Rollout, 2012 Q1 – 2019 Q1

	(1) Claim Rate	(2) Order Rate	(3) Warrant Rate	(4) Bailiff Repossession Rate
UC 'Full Service' Rolled Out:				
[No] N=8065				
Yes [First Quarter Post] N=323	1.03* (0.50)	0.72 (0.58)	0.52** (0.14)	0.41* (0.19)
Yes [Second Quarter post] N=272	1.66* (0.71)	0.99* (0.47)	0.66 (0.53)	0.31 (0.48)
Yes [Third Quarter post] N=208	2.31* (1.10)	1.54 (0.94)	0.63 (0.40)	0.74+ (0.42)
Yes [Fourth+ Quarters post] N=499	2.60*** (0.55)	2.89** (0.82)	1.09*** (0.27)	0.41 (0.34)
Model Based Unemployment Rate	0.50 (0.32)	0.04 (0.28)	-0.10 (0.18)	-0.09 (0.15)
Per £100 increase in **Median Weekly Wages**	-0.73 (0.73)	-0.72 (0.57)	-0.51 (0.32)	-0.57** (0.19)
Per £10 increase in **Mean Weekly Rents**	-2.20*** (0.46)	-1.06* (0.04)	-0.73* (0.03)	0.05* (0.02)
Local Authority Quarters (*N* total)	9367	9367	9367	9367
R^2	0.278	0.180	0.114	0.061

Notes: Driscoll-Kray standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. N refers to the number of local authority quarters. Landlord repossession rates are per 10,000 rented dwellings in the local authority. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

The results of the modelling confirm that the impact does indeed tend to increase when UC has been rolled out for longer. There is a clear upward trend in repossession rates with each quarter that passes post rollout – this is strongest for claims and order but also evident for warrants. Whilst there is some variation in the significance/confidence intervals due to sample sizes (especially in the second and third quarters post rollout), the ordering of coefficients is largely consistent. Little impact was found on bailiff repossessions – this is likely to be because (as already explained in section 5.3.2) relatively few cases reach this stage, and those that do take longer for it to occur, so UC has little impact. Overall, by the fourth+ quarters (i.e. 12+ months) post rollout, UC

'Full Service' was associated with an increase of 2.60 landlord repossession claims, 2.89 orders and 1.09 warrants (again, all per 10,000 rented dwellings). This is markedly higher than the estimates of UC's overall impact in Table 5.1, and when compared to mean landlord repossession rates in the period immediately before 'Full Service' rollout, this corresponds to a 6.9% increase in claims, 9.8% increase in orders and 5.9% increase in warrants.

5.4.4 Falsification Test

As mentioned in Chapter 4, the empirical analysis of this chapter involves a falsification test in order to improve causal inference. A falsification test is a test which assesses whether the relationship observed between an explanatory variable and an outcome variable is causal by checking the specificity of the relationship (Craig, Katikireddi, Leyland *et al.*, 2017). Conducting a falsification test for the analysis here is important in order to assess whether the results may be spurious and somehow linked to the structure of UC's rollout (i.e. related to any non-randomness in UC's rollout schedule, as discussed in Chapter 4). This is done by repeating the analysis using data on mortgage repossession rates as 'non-equivalent outcome variables', i.e. new outcome variables which are "predicted not to change because of the treatment but […] expected to respond to some or all of the contextually important internal validity threats in the same way as the target outcome" (Shadish, Cook and Campbell, 2002, p. 509). Mortgage repossession rates make suitable non-equivalent outcome variables here because their data is collected by the Ministry of Justice in the same way as landlord repossessions data, and any confounders that may potentially effect landlord repossession rates (e.g. housing or labour market factors) are likely to also effect mortgage repossession rates in a similar way. However, mortgage repossession rates should, crucially, not be impacted by the rollout of UC as 99% of UC claimants who receive help for housing costs are in rented accommodation (Department for Work and Pensions, 2018c).

The results of the falsification test are provided in Appendix 4. No significant relationship was found between UC rollout and mortgage repossession rates within the 323 English local authorities in the dataset between 2012 Q1 and 2019 Q1, and the estimated direction of the effect was negative. This suggests that the results of the main analysis set out in this chapter are unlikely to be spurious and linked somehow to

confounding from any non-randomness in UC's rollout schedule. This boosts the internal validity of the analysis.

5.5 Conclusion

The results outlined in this chapter suggest that UC rollout has led to an increase in landlord repossession rates within English local authorities. The results from the first part of the analysis suggest that overall, UC 'Full Service' rollout was, on average, associated with an increase of 1.74 landlord repossession claims, 1.42 landlord repossession orders and 0.70 landlord repossession warrants within local authorities (per 10,000 rented dwellings) by 2019 Q1. When compared to mean rates in the period immediately before 'Full Service' rolled out (i.e. 2015 Q1 – 2015 Q4), this corresponds to a 4.6% increase in claims, 4.8% increase in orders and 3.8% increase in warrants.

In addition, the results from the second part of the analysis suggest that the impact of UC 'Full Service' tended to be greater when it had been rolled out for longer and thus reached more claimants. For example, where 'Full Service' had been rolled out for 12+ months it was associated with an increase of 2.60 claims, 2.89 orders and 1.09 warrants (again, per 10,000 rented dwellings). When this is, again, compared to mean rates in the period immediately before 'Full Service' rollout it corresponds to a 6.9% increase in claims, 9.8% increase in orders and 5.9% increase in warrants. There was no significant relationship between UC rollout and rates of bailiff repossessions, but this is likely to be because relatives few cases actually lead to this stage as cases tend to be resolved before the need for bailiffs.

The strength of this analysis is that it was able to exploit cross-area variation in the timing of UC rollout, linking data from the official UC rollout schedule (at the local authority level) with administrative panel data on landlord repossession rates. Whilst UC rollout was not truly random (as discussed in Chapter 4), the analysis used a fixed effects panel design to reduce the influence of any bias potentially arising from this (with local authority and time fixed effects to control for potential unobserved confounders as well as inclusion of additional control variables to control for observed potential confounders). In addition, the internal validity of the analysis was boosted by the fact that UC's impact tended to increase when it had been rolled out for longer (and

thus reached more claimants), and via the falsification test which suggests that it is unlikely the findings of the main analysis are spurious. It is therefore unlikely that the relationship observed between UC 'Full Service' rollout and landlord repossession rates is not causal.

However, the analysis did have some limitations, which are important to note here. Firstly, it was conducted at the local authority level as opposed to the individual level. This creates potential for ecological fallacy – there is no way of knowing whether the households facing repossession actions are the same households who have moved onto UC. Yet, it is hard to see why else there would be a rise in rates of repossession actions coinciding with UC rollout if it was not for those directly affected.

Secondly, the Ministry of Justice data on repossessions do not capture the full extent of households at risk of evictions/tenancy non-sustainment in England. This is because some households may be forced out without legal proceedings taking place, e.g. if they leave voluntarily following informal pressure or threats of being evicted. There is also some evidence of landlords paying tenants who are in arrears to leave, as this is a quicker than going through the legal process (Rugg, 2008). However, this phenomenon will not bias the results of this chapter's analysis unless its propensity to occur is somehow linked to the UC rollout schedule. There is no known reason why this may be the case.

Thirdly, whilst the outcome variables and explanatory variables used in the analysis are quarterly estimates, the control variables are annual estimates that were converted into quarterly estimates via linear interpolation (in case of '*median weekly wages*' and '*mean weekly rents*') or by taking the average of previous 12 months of data (in case of '*Model based unemployment rate*'). This limits their ability to pick up quarter to quarter variation, and introduces greater noise (although not bias) to the data.

Fourthly, as discussed in Chapter 2, alongside Universal Credit another important part of the social security context that has had implications for housing insecurity in recent years is benefits limits/freeze. These were in place between 2013-14 and 2019-20 and as such, at least at a national level, its timing broadly coincide with the rollout of UC. Therefore, a further limitation of the analysis was that, though it was able to exploit cross-area variation in UC rollout, it is not possible to fully disentangle the effects of UC with the national level effects of the benefits limits/freeze.

Finally, the Ministry of Justice repossessions data consists of actions by both social and private landlords and it was not possible to disaggregate the data into social rented sector versus the private rented sector. The impact of UC may vary between sectors because tenants in the social rented sector are likely to be more financially vulnerable than those in the private rented sector, whilst UC's design of direct payments is novel for the social rented sector but not the private rented sector (where direct payment to claimants has been in place since LHA was introduced in 2008) (Hickman, Kemp, Reeve *et al.*, 2017). This limitation is addressed in Chapter 6, which follows.

Despite these limitations, the analysis in this chapter has provided evidence of a clear link between UC rollout and landlord repossession rates. Going forward, Chapters 6-8 examine UC's impact on the stages of housing insecurity that occur both before (i.e. household financial problems and rent arrears) and after (i.e. homelessness) repossession actions.

Chapter 6. Empirical Study 2: Impact of Universal Credit Rollout on Rates of Advice Sought from Citizens Advice on Housing Insecurity Issues

6.1 Introduction

This chapter examines the impact of Universal Credit rollout on rates of advice sought from Citizens Advice on rent arrears and homelessness issues within 323 English local authorities. As set out in Chapter 4, and in a similar fixed effects panel design to that employed in Chapter 5, this involves linking data on the timing of UC rollout with 'advice trends' data obtained from Citizens Advice on the number of households seeking advice on rent arrears/homelessness issues between 2014 Q1 and 2019 Q1. The specific research questions addressed in this chapter are as follows:

1. Has UC rollout led to an increase in rates of advice sought from Citizens Advice on rent arrears and/or homelessness related issues within 323 English local authorities up to 2019 Q1?

2. Does the impact of UC rollout on advice rates (if any found) increase when it has been rolled out for longer and thus reached more claimants?

3. How does this impact vary between those in the private rented sector versus the social rented sector?

This can build on the analysis of Chapter 5 and address critical gaps in two ways. First, whilst chapter 5 focussed on legal landlord repossession actions, the analysis here can provide insight into UC's impacts on the stages of housing insecurity that occur both before (i.e., build-up of rent arrears) and after (i.e. threatened/actual homelessness) repossessions. Second, the Citizens Advice data on rent arrears can be disaggregated by tenure. This means that it is possible to examine how UC's impact varies between the private rented sector (PRS) and the social rented sector (SRS). This is important given

one of the key limitations of Chapter 5's analysis was that such disaggregation between sectors was not possible.

This chapter begins by providing some background that is specific to this chapter's empirical study. This includes background on the role of Citizens Advice and their data, and specific background on UC and the SRS versus PRS (which is included here as this is the only chapter in which data on outcomes is disaggregated by tenure). Next, the chapter will set out the data, variables, methods and modelling used for its empirical analysis. It will then present the results (including how this links with previous analysis in Chapter 5), before ending with a conclusion summarising the research findings and limitations.

6.2 Background

6.2.1 The Role of Citizens Advice and their Data

Citizens Advice (aka Citizens Advice Bureau or CAB) are a network of independent charities who offer free confidential advice online, over the phone, and in person to those facing problems e.g. relating to benefits, debt, housing, employment, consumer issues, family or immigration (Citizens Advice, 2019a). The main service aims of Citizens Advice are: 1) to provide the advice people need for the problems they face, and 2) to improve the policies and practices that affect people's lives (Citizens Advice, 2016, p. 3).

In order to identify the structural and policy issues contributing to problems for people in society, Citizens Advice collect data on how many clients they see, who they are and the main problems they face (Watson, 2018). Citizens Advice is often the first place people will go when faced with a problem (Ibid). They helped 1,273,000 people face-to-face, 867,000 people over the phone and 287,000 via e-mail or webchat in 2018/19 (Ibid). This, combined with their national coverage, means that their data can provide strong insight into the scale of different societal problems and how this varies across time and space.

6.2.2 Existing Evidence on the Impact of UC Rollout on Advice Given by Citizens Advice

There is some existing research, conducted by Citizens Advice themselves, on problems arising from UC rollout (some of which has already been briefly summarised in Chapter 3). Firstly, their report '*Delivering on Universal Credit*' set out how they had helped 47,000 people with UC issues by mid-2017, and how those claiming UC were more likely than those on 'legacy' benefits to need help with other issues (Foley, 2017, pp. 9-10). Secondly, their report '*Universal Credit and debt*' suggests that Citizens Advice clients advised on UC were more likely to also need advice specifically on debt issues than clients advised on 'legacy' benefits, particularly relating to priority debts such as rent arrears, council tax arrears, and magistrates court fines (Drake, 2017). More recently, their '*Managing Money on Universal Credit*' report suggests that around half of their clients helped with UC were in rent arrears, and around 60 percent were using advance payments during UC's long paid period (Hobson, Spoor and Kearton, 2019). Overall, these reports convey that Citizens Advice clients on UC were more likely to seek advice on housing insecurity issues compared to clients on legacy benefits. However, these studies are based on cross-sectional data. In order to more closely examine the causal impact of UC rollout, the analysis in this chapter tracks English local authorities over time in a fixed-effects panel design, measuring the relationship between UC rollout and rates of advice given on housing insecurity issues.

6.2.3 Universal Credit Housing Costs in the Social versus Private Rented Sectors, and the Broader Context of the Tenure Mix Amongst Low-Income Households in England

Historically, the SRS has played a key role in alleviating poverty in England (and the rest of the UK) – it is said to have provided a 'saving grace' that breaks the link between a low-income and poverty/material deprivation (Bradshaw, Chzhen and Stephens, 2008; Tunstall, Bevan, Bradshaw *et al.*, 2013; Clair, Fledderjohann, Lalor *et al.*, 2020). At its peak in the mid-1970s, the SRS made up nearly a third of the British housing stock, and given its provision was largely for families with children, this included considerably more than a third of the overall population (Ravetz, 2001). This meant that – in the second half of the 20th century – the PRS began to play only a very marginal role in housing those in poverty (Bailey, 2020).

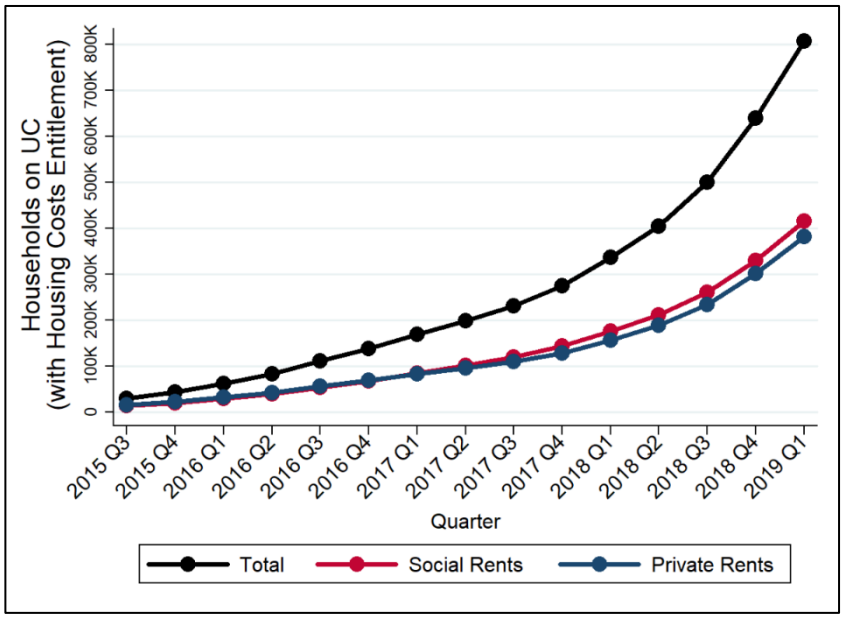
However, in recent decades the availability of social housing has declined, and the PRS has grown substantially (as touched upon in Chapter 5's section on determinants of eviction). This decline in social housing has largely been driven by sales through Right to Buy, reduced social housing grants and the implementation of building restrictions on Local Authorities (Clair, Fledderjohann, Lalor *et al.*, 2020, p. 56). Meanwhile, private renting has increased following a political consensus over its important role in providing short-term and flexible housing to young, mobile professionals (Bailey, 2020). According to national statistics, the PRS in England has doubled in size since 2002, and now accounts for around 4.5 million (or 19 percent) of the 23.3 million households living in England (MHCLG, 2019, p. 6). This makes it the second largest tenure group (behind owner occupation), having overtaken the SRS, which following a long downward trend has now stabilised at around 4 million households (17 percent) (Ibid). This rise in private renting has been particularly marked for poorer young adults and their children (Bailey, 2020).

Nevertheless, it is important to note that the SRS in England continues to play a key role in housing low-income households, and in alleviating poverty. The selective selling off of social rented accommodation to relatively better off households through Right to Buy has arguably led to the remaining social rented dwellings being even more closely targeted at those with the most social need (Bailey, 2020). Consequently, the SRS now plays more of a 'safety net' function rather than a 'wider affordability' function, and this is reinforced by the legal duties placed on local authorities to house unintentionally homeless households in priority need (Stephens and Leishman, 2017, p. 1040). In terms of the take up of Housing Benefit (HB) (targeted at low-income households), the majority of claimants continue to be housed in the SRS. In 2017/2018, around 60 percent (2.4 million households) of social renters received HB to help with rent payments, compared to just 20 percent (889,000 households) of private renters (MHCLG, 2019, p. 16).

With regard to housing costs under Universal Credit specifically, throughout its rollout so far there has tended to be more of an equal split between claimants in the social versus private rented sectors (perhaps because social tenants were more likely to already be claiming HB and thus do not transfer over to UC until they have a change of circumstances or in the 'managed migration' phase of rollout). This is highlighted in Figure 6.1, which shows that there was an almost identical number of households on UC (with housing costs) in the SRS and PRS up to the latter stages of 2017. Subsequently, there has been a marginally greater

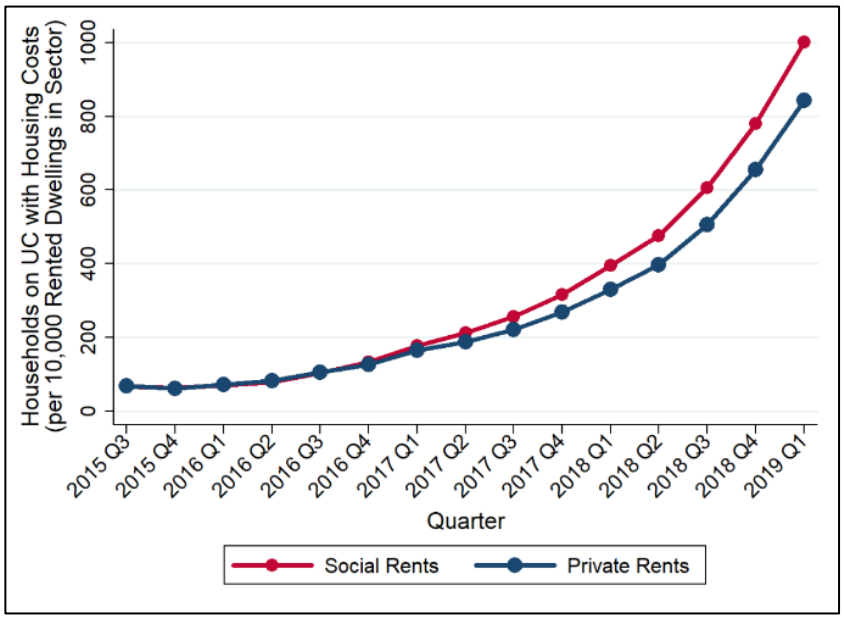
number of households in the SRS, with approximately 415,000 households on UC (with housing costs), compared to 380,000 in the PRS by 2019 Q1.

Figure 6.1. Quarterly Trends in the Number of households on UC (with housing costs) in England, SRS versus PRS, 2015 Q3 - 2019 Q1.



Data Source: DWP Stat Xplore.

Figure 6.2. Quarterly Trends in the Mean Rate of Households on UC with Housing Costs (per 10,000 rented dwellings in sector) Across 323 English local authorities, SRS versus PRS, 2015 Q3 – 2019 Q1.



Notes: rates for SRS are per 10,000 social rented dwellings in the local authority, whilst rates for PRS are per 10,000 private rented dwellings in the local authority. Data Sources: DWP Stat Xplore, ONS '*subnational dwelling stock by tenure estimates*' and MHCLG '*Live Tables on Dwelling Stock Estimates*'.

To provide insight into the proportion (rather than overall numbers) of households claiming UC with housing costs in the SRS/PRS, Figure 6.2 shows trends in the mean rate of households on UC with housing costs (per 10,000 rented dwellings in sector), across English local authorities. It shows a fairly similar trend to Figure 6.1, in that mean rates tend to be similar up to the middle of 2017, after which there has tended to be a marginally higher proportion of social renters on UC with housing costs than private renters. As of 2019 Q1, across local authorities there was a mean rate of around 1000 households on UC with housing costs in the SRS (per 10,000 social rented dwellings), i.e.10 percent. Meanwhile, there were around 844 households on UC with housing costs in the PRS (per 10,000 private rented dwellings), i.e. 8.4 percent. Both of these figures will rise substantially throughout the 'managed migration' phase of transferring remaining HB claimants over to UC.

6.2.4 Universal Credit's Impacts on the Social versus Private Rented Sectors

As set out above, UC housing costs are for tenants in both the social and private rented sectors. Importantly, this means that its rollout is likely to have wide-ranging implications, not just for social/private tenants claiming UC but also for landlords. The impact of UC rollout on the PRS specifically was debated in parliament in January 2018 following widespread concerns that it is exacerbating the reluctance of private landlords to let to those dependent on UC housing costs for rent payments (Wilson, 2018). Existing research suggests that this is a substantial issue. Landlord surveys suggests that the majority of private landlords are unwilling to let to those on UC, and that the majority of those who do have experienced the tenants who claim UC going into arrears (National Landlords Association, 2017; Simcock, 2018). Whilst unwillingness to let to benefit claimants was also an issue under the legacy Housing Benefit system, Simcock (2018) notes that part of the problem that is specific to UC is its ethos of promoting financial responsibility to claimants (reflected in its monthly direct payment system – this was discussed in Chapter 2), which means that landlords do not have confidence that they will receive their rent payments.

Regarding UC's impact on the SRS, Hickman *et al*. (2018) set out the extensive potential adverse impacts of UC rollout on housing associations. This consisted of concerns that UC rollout would: (a) reduce housing association income streams due to rent arrears building up, (b) require additional costs on resources (e.g. staff) for rent collection practices as well as

provision of personalised support to UC claiming tenants, and (c) lead to cash flow problems arising from these issues, potentially threatening the financial viability of some housing associations. Importantly, in the context of UC rollout and other welfare reforms, there is some evidence of increased use of pre-tenancy affordability assessments for prospective tenants, and potentially excluding those with poor financial histories (Preece, Hickman and Pattison, 2019).

Whilst UC rollout clearly impacts on both the PRS and SRS, there are two important ways in which the impact may differ between sectors, with social renters potentially being particularly vulnerable to rent arrears, evictions and homelessness. The first difference relates to the monthly direct payment system under UC. Unlike in the PRS – where direct payment to claimants have been in place since the introduction of Local Housing Allowance in 2008 – this is a completely novel approach within the SRS. Prior to UC, all HB claimants renting from local authorities and 92 percent of HB claimants renting from housing associations had the benefit paid to their landlord, not directly to themselves (Hickman et al., 2017, p. 4). This means that the new monthly direct payment system is likely to have a greater impact on the SRS than the PRS, which is by now used to the direct payment system.

Secondly, it has also been suggested that the impact of UC rollout on housing insecurity may be greater in the SRS than in the PRS because those in social housing are more likely to be vulnerable, and thus more likely to have difficulty paying their rent (Hickman et al., 2017). This is reflected in the fact that the majority of landlord repossession actions occur in the SRS (Ministry of Justice, 2019, p. 6), and that social renters are more likely to be in rent arrears than private renters despite spending a lower proportion of their income on rent (Ministry of Housing, 2018, p. 4). This means social renters may be particularly vulnerable to rent arrears during UC's long wait periods, when faced with an income reduction via a sanction, or as a result of the new monthly direct payment system.

Overall, these issues suggest that whilst UC rollout effects both the social and private rented sectors, its impact may be greater on the SRS. However, there is currently a lack of robust research into UC's varying impact on housing security between sectors. This chapter seeks to address this. After first examining the overall impact of UC on rates of advice sought from Citizens Advice on housing insecurity issues, the data is then disaggregated to measure if the impact varies between social and private rented sectors. The data and methodology used to achieve this is set out in detail in the following section.

6.3 Data, Variables and Methods

6.3.1 Setting

As with the analysis in Chapter 5, a quarterly local authority level dataset was compiled with a sample of 323 English local authorities (City of London, Isles of Scilly and West Somerset were again excluded due to small populations). Whilst Chapter 5's analysis covered 2012 Q1 – 2019 Q1, this time, given the availability of the Citizens Advice data, the dataset covers the period 2014 Q1 – 2019 Q1.

6.3.2 Citizens Advice Rent Arrears and Homelessness Advice Issues Data (Outcome Variables)

Data on the rates of advice given by Citizens Advice, at the local authority level, on rent arrears and homelessness issues were used to create three main outcome variables for this analysis. These are: (1) '*rent arrears advice rate*', (2) '*threatened homelessness advice rate*' and (3) '*actual homelessness advice rate*'. These indicate, for each local authority, the quarterly number of people advised by Citizens Advice on rent arrears issues, threatened homelessness issues and actual homelessness issues, as a rate per 10,000 rented dwellings (for rent arrears) or per 10,000 households (for threatened/actual homelessness). These include those who received advice face-to-face, over the phone, by e-mail or by webchat, with each issue being counted once for each client in each period so that those returning for help on the same ongoing issue are not duplicated.

All this data is available at the local authority level as each person who approaches Citizens Advice with an issue has the local authority they live in recorded, although for the '*actual homelessness advice rate*' this information was not always available due to some clients being unable to provide an address due to their homelessness. The '*threatened*' and '*actual*' homelessness advice rates were coded into rates per 10,000 households in the local authority using 2014-based annual household projections. The '*rent arrears advice rate*' was initially coded into rates per 10,000 total rented dwellings in the local authority, using Office for National Statistics' '*Subnational Dwelling Stock by Tenure Estimates*' and MHCLG's '*Live Tables on Dwelling Stock*'. It was then disaggregated by sector into: (1) '*SRS rent arrears*

advice rate', i.e. the number of social tenants advised on rent arrears issues (per 10,000 social rented dwellings), and (2)'*PRS rent arrears advice rate*', i.e. the number of private tenants advised on rent arrears issues (per 10,000 private rented dwellings).

Table 6.1. Details of subcategories making up Citizens Advice rent arrears, threatened homelessness and actual homelessness issues, 2018/2019.

Citizens Advice Issue	**Subcategories: % of Cases within Issue**
Rent Arrears	1. Liability for debt: **15.2%** 2. Creditor debt collection practices: **0.7%** 3. Impact on housing register: **0.1%** 4. Dealing with debt repayments: **48.6%** 5. Possession claim for arrears: **13.0%** 6. Eviction for arrears: **10.0%** 7. Direct deductions from benefit: **2.3%** 8. Former tenancy arrears recovery: **0.9%** 9. Not recorded or not applicable: **9.2%**
Threatened Homelessness	1. Relatives/friends unable/unwilling to house: **9.8%** 2. Relationship breakdown (excluding divorce): **5.8%** 3. Domestic Violence: **3.4%** 4. Harassment/illegal eviction: **1.9%** 5. Mortgage/secured loan possession: **3.0%** 6. LA possession action: **12.6%** 7. Housing association possession action: **15.4%** 8. Private landlord possession action: **21.4%** 9. Landlord's mortgage arrears: **0.4%** 10. Delays in HB claims: **0.7%** 11. LA won't re-house permanently: **1.7%** 12. Anti-social behaviour: **1.4%** 13. Not recorded/not applicable: **11.2%** 14. Benefit cuts (including cap): **0.8%** 15. Other: **11.3%**
Actual Homelessness	1. Relatives/friends unable/unwilling to house: **21.9%** 2. Relationship breakdown (excluding divorce): **12.2%** 3. Domestic Violence: **6.3%** 4. Harassment/illegal eviction: **2.2%** 5. Mortgage/secured loan possession: **0.8%** 6. LA possession action: **2.0%** 7. Housing association possession action: **2.3%** 8. Private landlord possession action: **4.9%** 9. Landlord's mortgage arrears: **0.1%** 10. Delays in HB claims: **0.2%** 11. LA won't re-house permanently: **5.6%** 12. Anti-social behaviour: **2.1%** 13. Not recorded/not applicable: **16.5%** 14. Benefit cuts (including cap): **0.4%** 15. Other: **22.5%**

Notes: percentages may not total 100 due to rounding. *Data Source*: Citizens Advice 'Advice Trends' Tableau Data.

Importantly, advice issues within each category are not homogenous, but rather are made up of various subcategories of client request reasons. When someone approaches Citizens Advice with a problem, this is first coded into a broad 'issue' category, and then subsequently sorted into a subcategory within that 'issue'. To provide more information on the most common reasons for Citizens Advice clients seeking advice within the rent arrears/homelessness advice issues, Table 6.1 details the different subcategories within Citizens Advice rent arrears, threatened homelessness and actual homelessness issues.

6.3.3 Universal Credit Data (Explanatory Variables)

Initially, the empirical analysis in this chapter uses the same three explanatory variables that were used in Chapter 5, namely: (1) '*UC Live Service*', (2) '*UC Full Service*' and (3) '*UC Full Service (by length of rollout)*'.

In addition, the analysis of this chapter also uses two new explanatory variables as part of examining UC's impact on the social vs. private rented sectors. Firstly, the '*SRS Households on UC with Housing Costs Rate'* indicates the quarterly number of social rented households claiming UC with entitlement to housing costs, as a rate per 10,000 social rented dwellings in the local authority. Secondly, the '*PRS Households on UC with Housing Costs Rate*' similarly indicates the quarterly number of privately rented households claiming UC with entitlement to housing costs, as a rate per 10,000 private rented dwellings. Data for both of these variables come from DWP Stat Xplore (Department for Work and Pensions, 2021b). They are not cumulative measures of the total number of households on UC with housing costs in the quarter, but rather provide a snapshot of the figure on the second Thursday of the quarter's third month. The data excludes households whose UC is not in payment, i.e. those not currently receiving UC payments, for example due to their earning being sufficiently high to remove their entitlement.

6.3.4 Control Variables

The need for advice on rent arrears and homelessness issues is likely impacted by similar labour and housing market factors as those which affect landlord repossession actions. As such the same three control variables used in Chapter 5's analysis are also used here,

namely: (1) '*model based unemployment rate*', (2) '*median weekly wages*', and (3) '*mean weekly rents*'. The only difference in the use of control variables here comes in the analysis involving disaggregation of outcomes by SRS versus PRS. In this part of the analysis, the '*mean weekly rents*' variable is disaggregated by sector into '*mean weekly SRS rents*' and '*mean monthly PRS rents*'.

6.3.5 Statistical Analysis

As in Chapter 5, the statistical analysis of this chapter involves fixed effects panel models. These were used to formally examine the relationship between UC rollout and rates of advice sought from Citizens Advice on rent arrears/homelessness issues, both overall and in the SRS versus PRS. As in Chapter 5, the models include both local authority and time fixed effects, which effectively control for unobserved baseline differences between local authorities and unobserved variables that vary over time but not between local authorities. Time fixed effects were particularly important to include here as rates of advice given tend to be lower in the second and fourth quarters of the year, which can largely be explained by their lower number of working days (Citizens Advice are closed during Christmas and Easter holidays).

In terms of the specific modelling used in this chapter's analysis, this was conducted in three distinct parts, to reflect the three research questions set out in this chapter's introduction. The first part of the analysis measures the overall impact of UC rollout on advice rates, as follows:

$$CAB\ Advice\ Rate_{it} = \beta_0 + \beta_1 UCLS_{it} + \beta_2 UCFS_{it} + \beta_3 Unemployment_{it} + \beta_4 Wages_{it} + \beta_5 Rents_{it} + \beta_6 Quarter_t + \alpha_i + u_{it} \quad (6.1)$$

Where i is the local authority and t is the quarterly time point. $CAB\ Advice\ Rate$ is the rate of advice given by Citizens Advice on rent arrears and homelessness issues, with separate models being run for '*rent arrears advice rate*', '*threatened homelessness advice rate*', and '*actual homelessness advice rate*'. $UCLS$ is the '*UC Live Service'* explanatory variable, $UCFS$ is the '*UC Full Service*' explanatory variable, $Unemployment$ is the '*model based unemployment rate'* variable, $Wages$ is the '*median weekly wages*' variable,

and $Rents$ is the '*mean weekly rents*' variable. Finally, $Quarter$ is the time fixed effects, α_i is the local authority fixed effects and u_{it} is the error term.

The second part of the analysis measures whether the impact of UC 'Full Service' rollout on advice rates (if any found) increases when it has been rolled out for longer and thus reached more claimants, as follows:

$$CAB\ Advice\ Rate_{it} = \beta_0 + \beta_1 UCFS\ Length_{it} + \beta_2 Unemployment_{it} + \beta_3 Wages_{it} + \beta_4 Rents_{it} + \beta_5 Quarter_t + \alpha_i + u_{it} \quad (6.2)$$

Where $UCFS\ Length$ is the '*UC Full Service (by length of rollout)*' categorical variable and all other variables are the same as those in Equation 6.1.

The third and final part of the analysis disaggregates the data in order to examine how the impact of UC rollout on rent arrears advice rates varies between the SRS and PRS. This is as follows:

$$CAB\ SRS/PRS\ RA\ Advice\ Rate_{it} = \beta_0 + \beta_1 UCFS_{it} + \beta_2 Unemployment_{it} + \beta_3 Wages_{it} + \beta_4 Rents_{it} + \beta_5 Quarter_t + \alpha_i + u_{it} \quad (6.3)$$

$$CAB\ SRS/PRS\ RA\ Advice\ Rate_{it} = \beta_0 + \beta_1 UCFS\ Length_{it} + \beta_2 Unemployment_{it} + \beta_3 Wages_{it} + \beta_4 Rents_{it} + \beta_5 Quarter_t + \alpha_i + u_{it} \quad (6.4)$$

$$CAB\ SRS/PRS\ RA\ Advice\ Rate_{it} = \beta_0 + \beta_1 SRS/PRS\ HHUCR_{it} + \beta_2 Unemployment_{it} + \beta_3 Wages_{it} + \beta_4 SRS/PRS\ Rents_{it} + \beta_5 Quarter_t + \alpha_i + u_{it} \quad (6.5)$$

Where '*CAB SRS/PRS RA Advice Rate*' is the rate of advice given on rent arrears, with separate models being run for rates in the SRS (per 10,000 social rented dwellings) and the PRS (per 10,000 private rented dwellings). '*SRS/PRS HHUCR*' is the rate of households on UC with housing costs, again with separate models being run for each sector. All other variables are the same as those set out in Equations 6.1 and 6.2, except $Rents$ which is disaggregated into SRS and PRS.

6.4 Results

6.4.1 Overall Trends in Advice Rates

Figures 6.3 and 6.4 provide an overview of trends in mean rates of advice given by Citizens Advice on rent arrears/homelessness issues during the analysis period. Specifically, Figure 6.3 shows trends in mean rates of advice given on rent arrears, as well as mortgage arrears for comparison. It highlights seasonal fluctuations in advice rates. Rates tend to be highest in Q1 (perhaps due to people delaying approaching Citizens Advice until after Christmas), and tend to be lowest in Q2 and Q4 of each year (which can largely be explained by closures for Easter/Christmas holidays reducing the number of working days in Citizens Advice offices during these quarters). This is particularly clear for advice given on rent arrears. Unrelated to this, there was a slight downward trend in advice given on both rent arrears and mortgage arrears advice rates between 2014 Q1 and 2017 Q4, since when rates (particularly for rent arrears) have begun to rise slightly. In 2019 Q1, the mean rates were 30.01 rent arrears issues (per 10,000 rented dwellings) and 2.44 mortgage/secured loan issues (per 10,000 owner occupied dwellings).

Figure 6.3. Quarterly Trends in Mean Rates of Housing Arrears Advice Given by Citizens Advice Across English local authorities, Q1 2014 – Q1 2019.

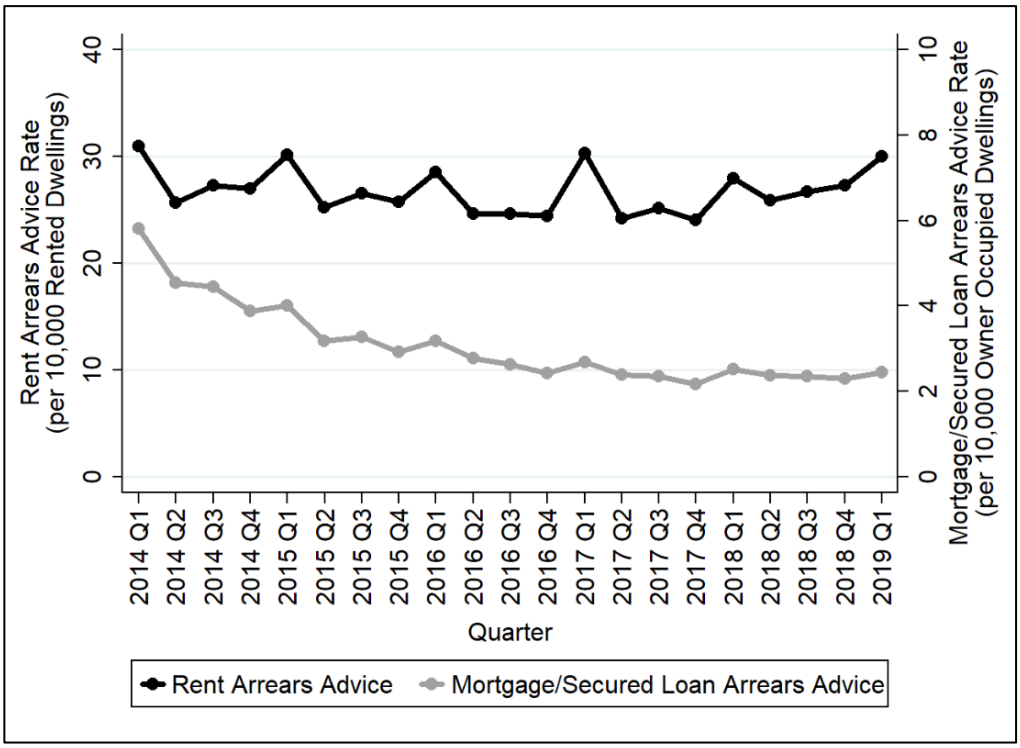


Figure 6.4. Quarterly Trends in Mean Rates of homelessness Advice Given by Citizens Advice Across English local authorities, Q1 2014 – Q1 2019.

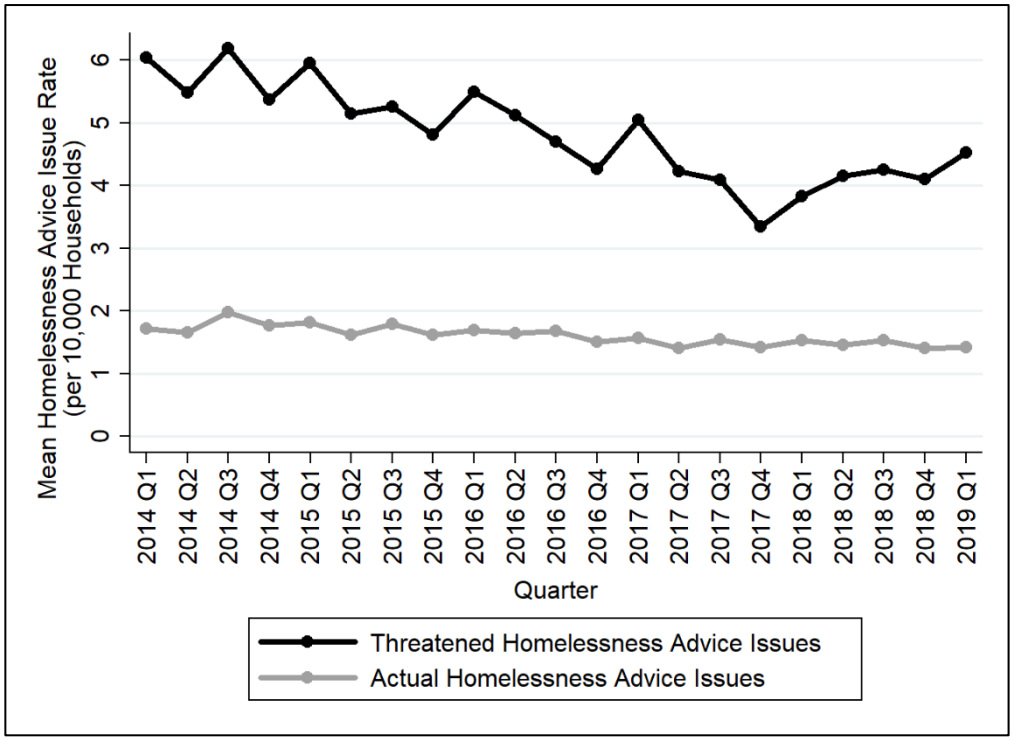


Figure 6.4 shows trends in mean rates of advice given by Citizens Advice on actual and threatened homelessness. It conveys that mean rates of actual homelessness advice has remained fairly constant, with just a slight downward trend from 1.98 in 2014 Q3 to 1.42 in 2019 Q1 (per 10,000 households). In a similar trend to rent arrears, mean advice rates for threatened homelessness were falling from 6.03 in 2014 Q1 to 3.34 in 2014 Q4, before subsequently rising to 4.52 in 2019 Q1 (per 10,000 households).

6.4.2 Overall Impact of Universal Credit Rollout

To examine the relationship between UC 'Full Service' rollout and rates of advice given on rent arrears/homelessness issues, Figures 6.5 and 6.6 plot trends in the quarters before and after its rollout. This is done by adjusting time to be relative to 'Full Service' rollout.

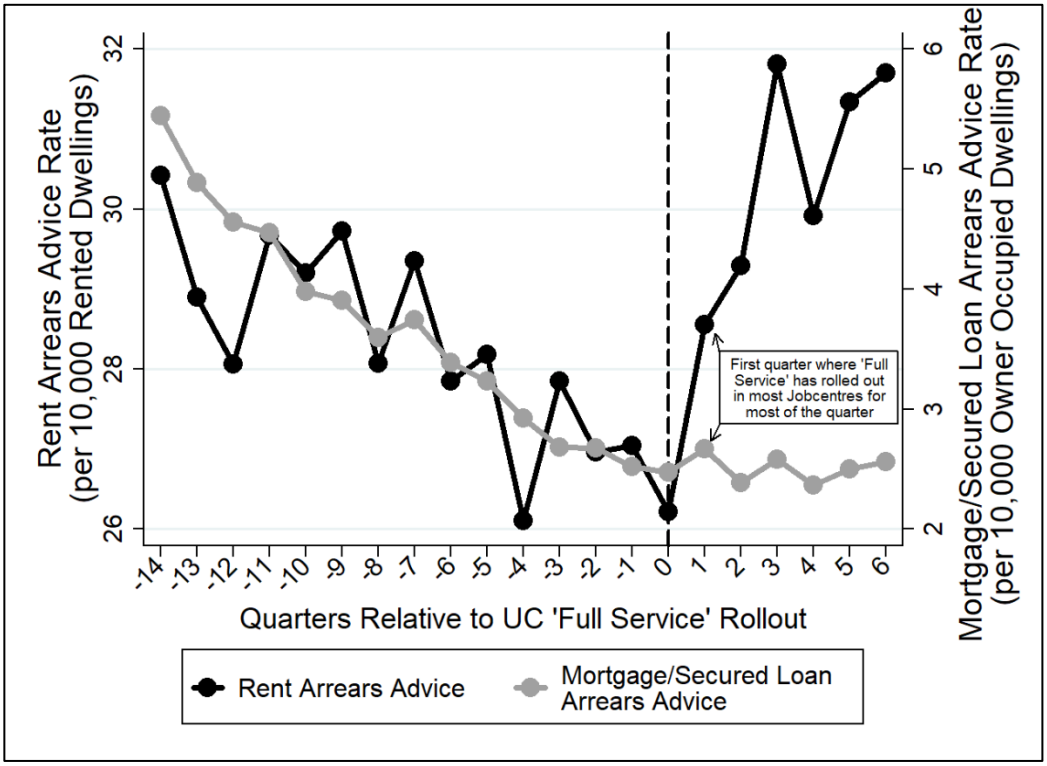
Figure 6.5 conveys that following a downward trend prior to rollout, there is a clear rise in the mean rate of advice given on rent arrears in the quarters immediately following the introduction of UC 'Full Service'. For comparison, it also shows the mean rates of advice given on mortgage/secured loan arrears, which are not expected to be affected by UC (as

discussed in Chapter 5). They follow a similar downward trend to the rent arrears advice rate, but importantly, this trend continues uninterrupted after 'Full Service' rollout.

However, whilst there is a clear rise in rent arrears advice following 'Full Service' rollout, the same was not found for homelessness advice rates. Figure 6.6 shows that trends in mean rates of advice given by Citizens Advice on threatened and actual homelessness appear to be unaffected by 'Full Service' rollout and continue on the same pattern seen before rollout.

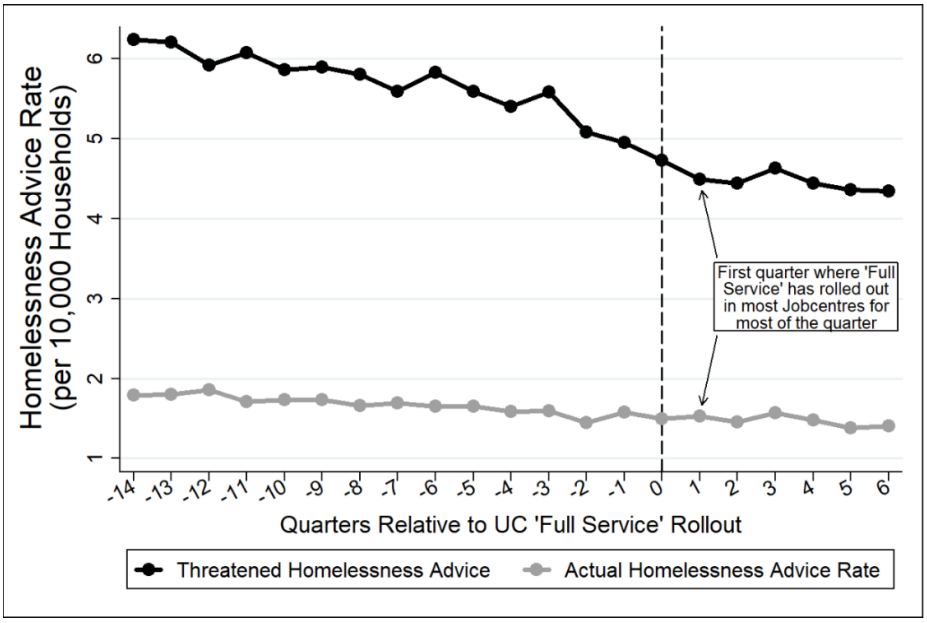
The relationship between UC rollout and rates of advice given by Citizens Advice is formally modelled in Table 6.2, which measures the overall impact of UC rollout within local authorities, on average, up to 2019 Q1. Consistent with the previous analysis of landlord repossession rates (in Chapter 5), no significant relationship was found between 'Live Service' rollout and rates of advice given by Citizens Advice on rent arrears/homelessness issues. Again, this is most likely because 'Live Service' rollout involved a relatively small number of people as it was targeted only at the simplest claims to manage.

Figure 6.5. Quarterly Trends in Mean Citizens Advice Housing Arrears advice rates in English Local Authorities, Before and After UC 'Full Service' rollout.



Notes: Due to UC's gradual rollout, the sample size of local authorities decreases in the quarters post rollout. Data was available for 323 local authorities in the first quarter post rollout, 272 in the second, 208 in the third, 136 in the fourth, 117 in the fifth and 82 in the sixth quarter post rollout.

Figure 6.6. Quarterly Trends in Mean Homelessness Advice Rates in English Local Authorities, Before and After UC 'Full Service' Rollout.



Notes: Due to UC's gradual rollout, the sample size of local authorities decreases in the quarters post rollout. Data was available for 323 local authorities in the first quarter post rollout, 272 in the second, 208 in the third, 136 in the fourth, 117 in the fifth and 82 in the sixth quarter post rollout.

Table 6.2. Relationship Between UC Rollout and Rates of Advice Given by Citizens Advice on Rent Arrears/Homelessness Within 323 English Local Authorities, 2014 Q1 - 2019 Q1.

	(1) Rent Arrears Advice Rate	(2) Threatened Homelessness Advice Rate	(3) Actual Homelessness Advice Rate
UC 'Live Service' Rolled Out:			
[No]			
Yes	-0.49	0.14	-0.10
	(0.28)	(0.09)	(0.06)
UC 'Full Service' Rolled Out:			
[No]			
Yes	2.97**	-0.08	0.04
	(0.81)	(0.13)	(0.02)
Model Based Unemployment Rate	-0.70+	0.01	-0.04**
	(0.36)	(0.03)	(0.01)
Per £100 increase in **Median Weekly Wages**	0.04	-0.17	-0.15**
	(0.73)	(0.15)	(0.05)
Per £10 increase in **Mean Weekly Rents**	-0.94***	-0.01*	-0.02*
	(0.20)	(0.06)	0.01)
Local Authority Quarters	6777	6684	6260
R^2	0.086	0.191	0.068

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. Rent arrears advice rate is per 10,000 rented dwellings in the local authority, whilst both homelessness advice rates are per 10,000 households in the local authority. Median weekly wages includes both part-time and full-time work. Mean weekly rents include private rents, housing association rents and, where applicable, local authority rents. The discrepancies in the number of local authority quarters occurs due a small amount of missing data, particularly for actual homelessness advice rates. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

However, the results suggest that UC 'Full Service' rollout was associated with an increase of 2.97 rent arrears advice issues within local authorities (per 10,000 rented dwellings). This is in line with the trend observed in Figure 6.5. To provide some context, the mean rate of rent arrears advice given by Citizens Advice in the period immediately prior to 'Full Service' rollout (i.e. 2015 Q3 – 2015 Q4) was 26.9 (per 10,000 rented dwellings). Therefore, the 2.97 (per 10,000 rented dwellings) figure in Table 6.2 corresponds to around an 11% increase on pre rollout rates. This suggests that significantly more people are having to seek help with dealing with rent arrears following UC 'Full Service' rollout.

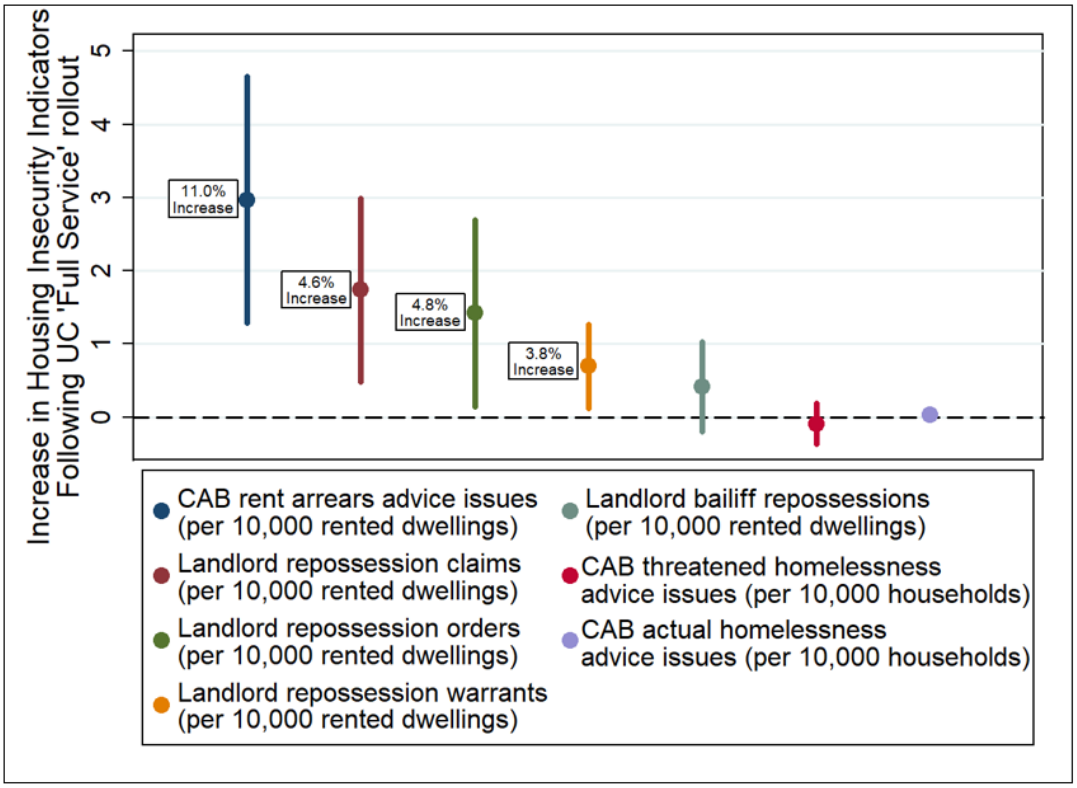
Whilst 'Full Service' was associated with an increase in rates of rent arrears advice, there was no significant relationship found with rates of advice given on threatened or actual homelessness. This suggests that whilst an increasing number of households may be struggling to maintain rent payments following UC rollout, this was not in turn leading to a significant increase in those requiring help from Citizens Advice with homelessness issues up to 2019 Q1.

It is important to note that some of the control variables in the regression models in Table 6.2 (and those in subsequent tables) provide counterintuitive results. Firstly, the results suggest a negative relationship between mean levels of rent within local authorities and rates of advice given on all three housing insecurity issues. This is counterintuitive given that rent increases can be important homelessness determinants (particularly in the PRS), although PRS affordability has been improving in England in recent years (Fitzpatrick, Pawson, Bramley *et al.*, 2019). Secondly, the results suggest a negative relationship between local authority unemployment rates and advice given on actual homelessness, which is counterintuitive given that unemployed individuals tend to be at greater risk of homelessness (Johnson, Scutella, Tseng *et al.*, 2015). These results may perhaps be explained by ecological fallacy, whereby relationships observed at an area level do not necessarily match those at the individual level. Indeed, previous local authority level analysis of homelessness in the UK have also observed a negative relationship between unemployment rates and homelessness (Loopstra, Reeves, Barr *et al.*, 2016; Bramley and Fitzpatrick, 2018).

In order to tie in the findings outlined in Table 6.2 with the findings of the previous analysis of UC's impacts on landlord repossession rates in Chapter 5, and thus provide a fuller picture of UC's housing security impacts (as well as showing corroboration between two independent data sources), Figure 6.7 displays the estimated 'Full Service' coefficients for

the various housing insecurity outcome variables from both pieces of analysis. These are ordered from left-to-right based on their position in the stages of housing insecurity (as set out in Chapter 3).

Figure 6.7. Impact of UC 'Full Service' Rollout on Housing Insecurity Indicators within English Local Authorities, Overall to 2019 Q1.



Notes: Citizens Advice (CAB) data is from 2014 Q1 - 2019 Q1 whilst repossessions data is from 2012 Q1 – 2019 Q1. Point estimates are derived from coefficients in the regression models already outlined. Vertical bars represent 95% confidence intervals. Where UC 'Full Service' impact is statistically significant (i.e. confidence intervals do not cross zero), text boxes next to coefficients show the percentage increase compared to mean rates in the pre UCFS period (2015 Q1 - 2015 Q4).

Figure 6.7 conveys a clear trend in that the impact of UC 'Full Service' does appear to be greater in the earlier stages of housing insecurity. Specifically, the biggest impact is found on Citizens Advice rent arrears advice rates, with an 11 percent increase on pre rollout rates. This is followed by landlord repossession claims (4.6 percent increase on pre rollout rates), landlord repossession orders (4.8 percent increase on pre rollout rates) and landlord repossession warrants (3.8 percent increase on pre rollout rates).

Conversely, no significant impact was found on landlord bailiff repossession rates or rates of advice given by Citizens Advice on threatened or actual homelessness. This suggests that

either: (a) there are some protective factors that are preventing increased rent arrears and repossession actions arising from UC leading to actual eviction/homelessness, (b) as UC rollout was still in its relatively early stages by 2019 Q1, impacts on the latter 'stages of housing insecurity' were not yet picked up by the data, and/or (c) there is no significant impact because relatively few households actually reach the stage of bailiff repossession as cases tend to be resolved (e.g. via paying off arrears or moving out voluntarily into new accommodation) before this occurs, thus preventing homelessness from arising.

6.4.3 Impact by Length of Rollout

In order to examine whether UC's impact increases over time post rollout, as more claimants move onto UC and its effects have more time to become apparent, Table 6.3 measures UC's impact by length of rollout.

The results suggest that, in general, the impact of UC 'Full Service' rollout on rent arrears advice rates does indeed tend to increase when it has been rolled out for longer. Specifically, it is associated with an increase of 1.65 rent arrears advice issues (per 10,000 rented dwellings) in the first quarter post rollout, rising to 2.81 in the second quarter post and 4.77 in the third quarter post. It then falls slightly to 4.37 in the fourth quarter post (although by this point data is only available for less than half of local authorities, as the majority hadn't reached this stage by 2019 Q1), before again rising to 4.40 in the fifth quarter post and 4.84 in the sixth quarter post.

These results are in line with the trend displayed in Figure 6.5, which also conveys the increasing impact of UC 'Full Service' in the quarters post rollout. Overall, this suggests that 'Full Service' rollout is associated with a significant negative impact on rent arrears, leading to an increasing number of households requiring help from Citizens Advice on how to deal with arrears. This impact appears to be almost immediate, as it is seen in the first quarter after rollout, and increases over time as more people move onto UC.

However, Table 6.3 also shows that there was no clear significant relationship between UC rollout and rates of advice sought from Citizens Advice on threatened or actual homelessness. This is consistent with the results shown in Table 6.2, as well as the trends outlined in Figure 6.6. It suggests that, up to 2019 Q1, UC rollout had not significantly impacted on the number of households requiring homelessness related advice from Citizens Advice.

Table 6.3. Relationship Between UC Rollout and Rates of Advice Given by Citizens Advice on Rent Arrears and Homelessness within 323 English Local Authorities, by Length of Rollout, 2014 Q1 - 2019 Q1.

	(1) Rent Arrears Advice Rate	(2) Threatened Homelessness Advice Rate	(3) Actual Homelessness Advice Rate
UC 'Full Service' Rolled Out:			
[No]			
Yes: 1st Quarter Post	1.65**	-0.07	0.05
[Data for 323 LAs]	(0.52)	(0.10)	(0.03)
Yes: 2nd Quarter post	2.81*	-0.10	0.04
[Data for 272 LAs]	(1.01)	(0.12)	(0.02)
Yes: 3rd Quarter post	4.77**	-0.13	0.09+
[Data for 208 LAs]	(1.17)	(0.12)	(0.05)
Yes: 4th Quarter post	4.37**	-0.26	0.02
[Data for 136 LAs]	(1.10)	(0.21)	(0.03)
Yes: 5th Quarter post	4.40***	-0.55*	-0.05
[Data for 117 LAs]	(0.89)	(0.20)	(0.07)
Yes: 6th Quarter post	4.84**	-0.37	0.02
[Data for 82 LAs]	(1.31)	(0.41)	(0.05)
Model Based Unemployment Rate	-0.76*	0.01	-0.04**
	(0.34)	(0.04)	(0.01)
Per £100 increase in **Median Weekly Wages**	-0.06	-0.31+	-0.17*
	(0.72)	(0.15)	(0.06)
Per £10 increase in **Mean Weekly Rents**	-0.78**	-0.14*	-0.02+
	(0.25)	(0.06)	(0.01)
Local Authority Quarters	6613	6527	6119
R^2	0.092	0.191	0.066

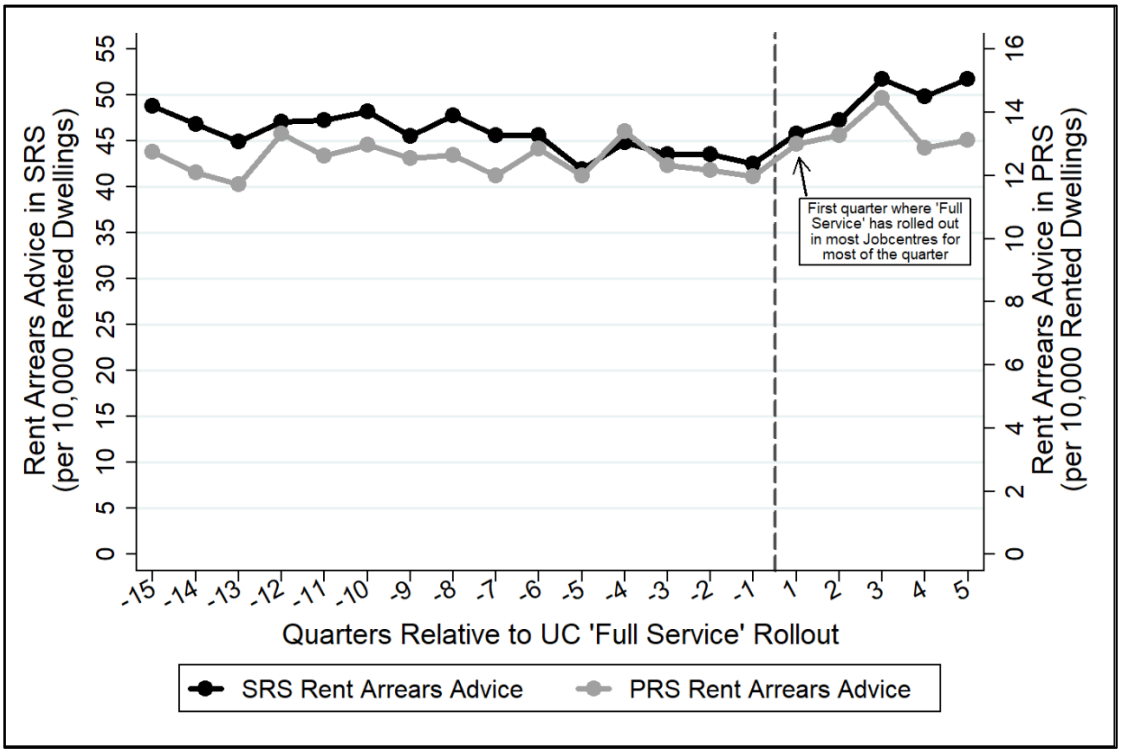
Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. Rent arrears advice rate is per 10,000 rented dwellings in the local authority, whilst both homelessness advice rates are per 10,000 households in the local authority. Median weekly wages includes both part-time and full-time work. Mean weekly rents include private rents, housing association rents and, where applicable, local authority rents. Discrepancies in the number of local authority quarters occurs due a small amount of missing data, particularly for actual homelessness advice rates. Models only include data up to the 6th quarter post rollout as the sample size of local authorities became too low in the 7th+ quarters post rollout. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

6.4.4 Impact on Arrears in the Social versus Private Rented Sector

In order to examine variation in the impact of UC rollout on rent arrears between the SRS and PRS, Figure 6.8 plots trends in rent arrears advice rates in each sector. Firstly, this shows that rent arrears advice rates are substantially higher in the SRS than in the PRS, i.e. those living in the SRS are more likely to seek advice on rent arrears from Citizens Advice than those living in the PRS. For example, in 2019 Q1 the mean rent arrears advice rate across

local authorities was 49.2 in the SRS (per 10,000 social rented dwellings) compared to 13.9 in the PRS (per 10,000 private rented dwellings). This backs up the point made in section 6.2.4 that those in the SRS tend to, in general, be more vulnerable to rent arrears.

Figure 6.8. Quarterly Trends in Mean Rent Arrears Advice Rates in the SRS versus PRS in English local authorities, Before and After UC 'Full Service' rollout.



Notes: Due to UC's gradual rollout, the sample size of local authorities decreases in the quarters post rollout. Data was available for 323 local authorities in the first quarter post rollout, 272 in the second, 208 in the third, 136 in the fourth, 117 in the fifth and 82 in the sixth quarter post rollout.

Moreover, Figure 6.8 also shows that the rise in rent arrears advice rates following UC 'Full Service' rollout (previously shown for the SRS and PRS combined in Figure 6.5) occurred in both the social and private rented sectors. However, the rise is more pronounced in the SRS, where mean rent arrears advice rates rose from 42.6 prior to rollout to 52.1 in the 6th quarter post rollout (per 10,000 social rented dwellings). This is compared to a rise from 11.9 to 14.0 in the PRS (per 10,000 private rented dwellings).

These relationships are formally modelled in Table 6.4. The results suggest that up to 2019 Q1, and after accounting for unemployment rates, wages and rents, UC 'Full Service' rollout was, on average, associated with an increase of 5.24 rent arrears advice issues in the SRS (per 10,000 social rented dwellings). Meanwhile, it was also associated with an increase of

0.96 issues in the PRS (per 10,000 private rented dwellings). Mean rent arrears advice rates in the pre UC 'Full Service' rollout period (2015 Q1 – 2015 Q4) were 44.05 in the SRS (per 10,00 social rented dwellings) and 12.17 in the PRS (per 10,000 private rented dwellings). Therefore, these coefficients correspond to around an 11.9% increase on pre rollout rates in the SRS, compared to a 7.9% increase on pre rollout rates in the PRS.

Table 6.4. Relationship Between UC Rollout and Rates of Advice Given by Citizens Advice on Rent Arrears in the Social versus Private Rented Sectors within 323 English Local Authorities, 2014 Q1 - 2019 Q1.

	(1) Social Rented Sector Rent Arrears Advice Rate	(2) Private Rented Sector Rent Arrears Advice Rate
UC 'Full Service' Rolled Out:		
[No]		
Yes	5.24*** (1.25)	0.96** (0.31)
Model Based Unemployment Rate	-0.88 (0.52)	-0.42* (0.18)
Per £100 increase in **Median Weekly Wages**	-0.54 (1.49)	-0.10 (0.54)
Per £10 increase in **Mean Weekly SRS Rents**	-2.96 (2.18)	
Per £10 increase in **Mean Monthly PRS Rents**		-0.04* (0.02)
Local Authority Quarters	6777	6559
R^2	0.066	0.051

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. SRS rent arrears advice rate is per 10,000 social rented dwellings in the local authority. PRS rent arrears advice rate is per 10,000 private rented dwellings in the local authority. Median weekly wages includes both part-time and full-time work. Mean weekly SRS rents include housing association rents and, where applicable, local authority rents. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

The impact of UC 'Full Service' on SRS and PRS rent arrears advice rates is further examined, by length of rollout, in Table 6.5. To enable visual interpretation, and comparison

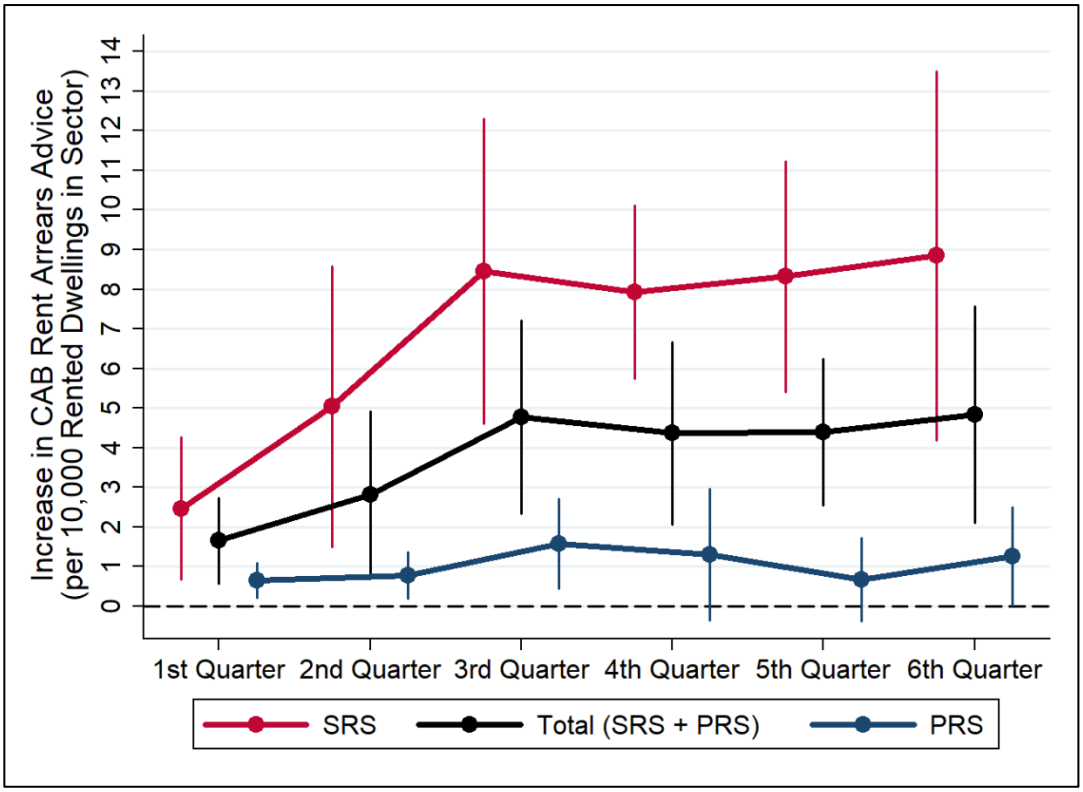
with the results for both sectors combined (as measured in Table 6.3), the coefficients are also plotted in Figure 6.9.

Table 6.5. Relationship Between UC Rollout and Rates of Advice Given on Rent Arrears in the Social versus Private Rented Sectors, by Length of Rollout, within 323 English local authorities, 2014 Q1 - 2019 Q1.

	(1) Social Rented Sector Rent Arrears Advice Rate	(2) Private Rented Sector Rent Arrears Advice Rate
UC 'Full Service' Rolled Out:		
[Reference: No - Pre Rollout]		
Yes: 1st Quarter Post *[Data for 323 LAs]*	2.47* (0.87)	0.65** (0.21)
Yes: 2nd Quarter post *[Data for 272 LAs]*	5.04** (1.04)	0.78* (0.28)
Yes: 3rd Quarter post *[Data for 208 LAs]*	8.45*** (1.84)	1.57** (0.55)
Yes: 4th Quarter post *[Data for 136 LAs]*	7.93*** (1.05)	1.30 (0.80)
Yes: 5th Quarter post *[Data for 117 LAs]*	8.32*** (1.40)	0.67 (0.50)
Yes: 6th Quarter post *[Data for 82 LAs]*	8.85** (2.24)	1.26* (0.59)
Model Based Unemployment Rate	-0.93+ (0.49)	-0.46* (0.18)
Per £100 increase in **Median Weekly Wages**	-0.51 (0.01)	-0.22 (0.60)
Per £10 increase in **Mean Weekly SRS Rents**	-2.77 (0.21)	
Per £10 increase in **Mean Monthly PRS Rents**		-0.03 (0.02)
Local Authority Quarters	6613	6413
R^2	0.073	0.053

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. Social rented sector rent arrears advice rate is per 10,000 social rented dwellings in the local authority. Private rented sector rent arrears advice rate is per 10,000 private rented dwellings in the local authority. Median weekly wages includes both part-time and full-time work. Mean weekly SRS rents include housing association rents and, where applicable, local authority rents. Models only include data up to the 6th quarter post rollout as the sample of local authorities became too low in the 7th+ quarters post rollout. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

Figure 6.9. Impact of UC 'Full Service' Rollout on Citizens Advice SRS and PRS Rent Arrears Advice Rates within English Local Authorities, by Rollout Length, to 2019 Q1.



Notes: Point estimates are derived from coefficients in regression models in Tables 6.3 and 6.5. Vertical bars represent 95% confidence intervals. Coefficients are less precise in quarters further post rollout (exemplified by wider confidence intervals) due to decreasing sample size of local authorities.

The increasing impact of UC 'Full Service' in the quarters post rollout is most pronounced in the SRS. Per 10,000 social rented dwellings, the impact rises from 2.47 additional rent arrears issues (i.e. a 5.6% increase on pre rollout rates) in the first quarter post rollout, up to 8.85 in the sixth quarter post rollout (i.e. a 20.1% increase on pre rollout rates).

This increasing impact is less pronounced, albeit still apparent, in the PRS. Per 10,000 private rented dwellings, the impact increases from 0.65 additional rent arrears issues (i.e. a 5.3% increase on pre rollout rates) in the first quarter post rollout, up to 1.26 in the sixth quarter post rollout (i.e. a 10.4% increase on pre rollout rates).

Overall, the regression models in Tables 6.4 and 6.5 (and the visualisation in Figure 6.9) suggest that whilst UC 'Full Service' rollout's impact on demand for Citizens Advice rent arrears advice is consistent across both the SRS and PRS, it has been greater and more detrimental in the SRS. This is seen both in terms of its overall impact up to 2019

Q1 and in the impact becoming more pronounced over time in the quarters post rollout as more households move onto UC.

However, one limitation of using the timing of UC 'Full Service' rollout in a local authority as the explanatory variable here is that this does not take into account the fact that the rate of households moving onto UC with housing costs is slightly higher in social rents than private rents (as previously set out in Figure 6.2 earlier in this chapter). Therefore, the greater impact observed in the SRS may be partially explained by their higher rate of households on UC with housing costs. In order to account for this, the analysis was repeated using the '*SRS Households on UC with Housing Costs Rate*' and the '*PRS Households on UC with Housing Costs Rate*' as explanatory variables and comparing the results. The results of the modelling are shown in Table 6.6.

Table 6.6. Relationship Between 'Households on UC with Housing Costs Rate' and Rates of Advice Given by Citizens Advice on Rent Arrears in the SRS and PRS Within 323 English Local Authorities, 2015 Q3 – 2019 Q1.

	(1) Social Rented Sector Rent Arrears Advice Rate	(2) Private Rented Sector Rent Arrears Advice Rate
Per 100 households increase in **Social Rented Sector Households on UC with Housing Costs Rate**	0.43** (0.11)	
Per 100 households increase in **Private Rented Sector Households on UC with Housing Costs Rate**		0.18*** (0.04)
Model Based Unemployment Rate	0.33 (0.24)	0.06 (0.10)
Per £100 increase in **Median Weekly Wages**	-0.76 (0.01)	-0.26 (0.65)
Per £10 increase in **Mean Weekly SRS Rents**	0.06 (0.18)	
Per £10 increase in **Mean Monthly PRS Rents**		0.001 (0.01)
Local Authority Quarters	5410	4393
R^2	0.074	0.061

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. The 'households on UC with housing costs rate' is per 10,000 SRS dwellings in model 1, and per 10,000 PRS dwellings in model 2. SRS rent arrears advice rate is per 10,000 social rented dwellings in the local authority, whilst PRS rent arrears advice rate is per 10,000 private rented dwellings in the local authority. Median weekly wages includes both part-time and full-time work. Mean weekly SRS rents include housing association rents and, where applicable, local authority rents. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

The results suggest that every 100 additional households (in sector) on UC with housing costs are associated with a 0.43 increase in rent arrears advice rate in the SRS (per 10,000 social rented dwellings) and a 0.18 increase in rent arrears advice rate in the PRS (per 10,000 private rented dwellings). To put these figures into context, as previously highlighted in Figure 6.2, the mean rates of households on UC with housing costs by 2019 Q1 were 1000.65 in the SRS (per 10,000 social rented dwellings) and 843.68 in the PRS. Therefore, the coefficients in Table 6.6, in an average local authority, would correspond to an increase of around 4.30 rent arrears advice issues in the SRS (per 10,000 social rented dwellings) and 1.52 in the PRS (per 10,000 private rented dwellings).

These estimates differ slightly from the estimates of UC's overall impact outlined in Table 6.4. However, in general it backs them up in the sense that: (a) it again suggests that UC's impact on demand for rent arrears advice has occurred in both the social and private rented sectors, and (b) that the impact has been greater in the SRS than in the PRS. Overall, this suggests that UC's impact has been greater on the SRS than in the PRS, even after accounting for variation between the sectors in the rates of households moving onto UC.

6.4.5 Falsification Test

As in Chapter 5's analysis, one way to test whether the results outlined in this chapter may be spurious, and linked somehow to the structure of UC rollout, is to conduct a falsification test. Similar to Chapter 5, this is done here by repeating the main analysis but using data on Citizens Advice mortgage arrears advice rates as a non-equivalent outcome variable. The results of the falsification test are provided in Appendix 5. No significant relationship was found between UC 'Full Service' rollout and rates of advice given by Citizens Advice on mortgage arrears. This suggests that the results of the main analysis are unlikely to be due to unobserved confounding, and thus improves causal inference.

6.5 Conclusion

This chapter has exploited data held by Citizens Advice in order to examine the impact of UC rollout on rates of advice they give on rent arrears/homelessness issues. This analysis can not only be used to confirm the results of Chapter 5's analysis of landlord repossession

rates, but also adds to it in some important ways. Namely, this is by: (a) providing insight into UC's impact on the stages of housing insecurity that occur before (rent arrears) and after (homelessness) landlord repossession actions, and (b) by examining how UC's impact on housing insecurity varies between the SRS and PRS. This has provided a number of important findings.

Firstly, the findings suggest that UC rollout has led to an increase in demand for advice from Citizens Advice on rent arrears issues. After accounting for unemployment rates, wages and rents, UC 'Full Service' rollout was associated with, on average, an increase of 2.97 rent arrears advice issues within local authorities (per 10,000 rented dwellings), overall to 2019 Q1. This corresponds to around an 11% increase on rates in the pre rollout period of 2015 Q3 – 2015 Q4.

Moreover, the findings suggest that whilst this impact on rent arrears advice rates tended to occur immediately in the first quarter post 'Full Service' rollout, it then also tended to increase over time when it had been rolled out for longer. Where 'Full Service' had been rolled out for longest (i.e. 6th quarter post), it was associated with an increase of 4.84 rent arrears advice issues (per 10,000 rented dwellings), i.e. an 18% increase on pre rollout rates. Therefore, a key concern is that this impact may continue to increase into the future, with the number of households on UC with housing costs expected to increase substantially during the remaining 'managed migration' phase of rollout.

However, the analysis found no significant relationship between UC 'Full Service' rollout and rates of Citizens Advice homelessness advice issues. This is in line with the finding of Chapter 5 that there was no impact of UC rollout on bailiff repossessions. It may be because it takes time for people to get to the stage of facing homelessness after rent arrears begin to build up – this could mean that UC's homelessness impact will only be seen in the longer term so is not picked up by the data, which was limited up to Q1 2019. However, it may also indicate that UC is leading to financial problems and arrears, but that these cases tend not reach the most extreme stage of actual repossession/homelessness. Cases may be resolved due to those affected managing to pay off their arrears or voluntarily moving out and into new accommodation. Protective barriers may also be preventing those with rent arrears from in turn becoming homeless – e.g. those affected may have good social support networks (Bramley and Fitzpatrick, 2018) or they may be supported out of their rent arrears as a direct

result of the advice given by Citizens Advice (or via support from the DWP's UC safeguards targeted at those in rent arrears, such as 'Alternative Payment Arrangements').

Finally, the analysis suggests that UC rollout has had a larger and more detrimental impact on rates of rent arrears advice in the SRS than in the PRS. Specifically, UC 'Full Service' was associated with an increase of 5.24 rent arrears advice issues in the SRS (per 10,000 social rented dwellings), i.e. a 11.9% increase on pre rollout rates. This is compared to an increase of 0.96 issues in the PRS (per 10,000 private rented dwellings), i.e. a 7.9% increase on pre rollout rates. Some of this divergence could be due to a higher proportion of social renters moving onto UC with housing costs. However, the divergence was still apparent after accounting for this by using the '*SRS Households on UC with Housing Costs Rate*' and the '*PRS Households on UC with Housing Costs Rate*' as explanatory variables and comparing the results. This means that the greater impact in the SRS is likely to be due to greater vulnerability in the sector and the fact that UC's direct payment system is novel in the SRS but not the PRS. However, one alternative explanation is that the divergence may arise due to differences in landlord behaviour between sectors – it may be the case that social landlords were more likely to signpost tenants who were facing arrears due to problems with UC towards advice services like Citizens Advice.

As in Chapter 5, the key strength of the analysis here is that it was able to exploit cross-area variation in the timing of UC rollout in order to measure its impact, by linking data from its official rollout schedule with administrative panel data from Citizens Advice. In addition, the analysis is further strengthened through its use of local authority and time fixed effects to control for unobserved confounding variables, and by its falsification test which boosts the internal validity of the analysis. This means that it is unlikely that the relationships observed were not causal.

Nonetheless, there were some limitations that should be noted. Firstly, it shares some limitations of Chapter 5 in that its use of local authority level data (as opposed to individual level data) creates the potential for ecological fallacy, and the accuracy of its control variables were limited by the fact that they are essentially annual estimates converted into quarterly estimates. One further limitation, which is specific to this chapter's analysis, is that the most common way of receiving advice from Citizens Advice during the analysis period was face-to-face. This means that those who have better access to a Citizens Advice Bureau (e.g. those who live near one) may be more likely to access advice services. However, the

use of local authority fixed effects should be able to control for this in the analysis, and this phenomenon should not bias the results unless there were new Citizens Advice Bureaux opened up in new areas as a direct result of UC rollout.

Chapter 7. Empirical Study 3: Impact of Universal Credit Rollout on Rates of Homelessness Assistance Need in Scotland

7.1 Introduction

This chapter investigates the impact of UC 'Full Service' rollout on homelessness assistance need within 29 Scottish local authorities. In Scotland, households can make a statutory homelessness claim to their local authority, and – provided they have a connection to the local area and are assessed as unintentionally homeless or threatened with homelessness in the next 90 days – are entitled to permanent accommodation (ScotPHO, 2019). In addition, since 2010 an approach called 'Housing Options' has also been adopted in Scotland, which is an information and advice process that attempts to prevent homelessness by identifying the options available to those approaching their local authority with a housing problem (ScotPHO, 2019; Scottish Government, 2020a).

The analysis in this chapter makes use of data obtained from the Scottish Government on the monthly number of statutory homelessness claims and approaches for Housing Options. As set out in Chapter 4, and in a similar fixed effects panel design to that employed in Chapters 5 and 6, data on the timing of UC rollout in Scottish local authorities is linked with the Scottish Government data on homelessness assistance need. The specific research questions addressed are as follows:

1. Has UC 'Full Service' rollout led to an increase in the rates of Housing Options approaches within Scottish local authorities up to March 2019?

2. Has UC 'Full Service' rollout led to an increase in the rates of statutory homelessness claims within Scottish local authorities up to March 2019?

3. Does the impact of UC 'Full Service' rollout (if any found) increase when it has been rolled out for longer and thus reached more claimants?

4. How does the impact of UC 'Full Service' rollout on homelessness claim rates (if any found) vary based on the main reason cited for the homelessness claim and the prior circumstances of the applicant?

This can build on Chapters 5 and 6 by providing further analysis into UC's impact on the last stage of housing insecurity (i.e. threat of or actual homelessness), and providing analysis in a different national context, Scotland. Despite its limitations (which are discussed throughout the course of this chapter), Scottish homelessness and Housing Options data arguably provides the best available data on local authority-level homelessness rates in recent years in the UK. This is because the way that official homelessness data in England is collected was changed in 2018 (see MHCLG, 2018), meaning it is not possible to accurately track homelessness claim rates over time in the period of UC rollout.

Moreover, whilst Chapters 5 and 6 only shed light on housing insecurity in terms of people facing rent arrears/eviction from their own property, the homelessness data also sheds light on those facing insecurity for other reasons and from other prior circumstances. In particular, relationship breakdown and family conflict forcing young people to leave their family home are key homelessness triggers in the UK, and these can occur (amongst other reasons) as a result of stresses associated with welfare reform (see Watts, Johnsen and Sosenko, 2015, p. 65). This is a particular issue in Scotland as, in contrast to England where the past decade has seen large increases in statutory homelessness claims due to ending of a private rented sector tenancy, Scotland's lower overall housing pressures mean this has not been the case (Fitzpatrick, Pawson, Bramley *et al.*, 2019; Scottish Government, 2020b). Instead, homelessness claims in Scotland most commonly come from: (a) those experiencing relationship breakdown, or (b) young people who are "asked to leave" their family home (Scottish Government, 2020b). This is why, in addition to examining the overall impact of UC rollout on homelessness claims, the homelessness data is also disaggregated by the main cited reason for the claim and the prior circumstances of applicant. This can provide insight into whether UC is linked to homelessness arising specifically from relationship breakdown, being "asked to leave" the family home, or from terminated tenancies.

This chapter is structured as follows. It begins by providing some background that is specific to this chapter. This includes an overview of the Scottish homelessness system and how its homelessness data is collected, as well as an overview of the determinants of homelessness (both broadly and in Scotland specifically). Next, the chapter will set out the data, variables

and methods used in its empirical analysis. It will then present the results of the analysis, before ending with a conclusion summarising the key findings and limitations of the research.

7.2 Background

7.2.1 The Scottish Homelessness System: Post Devolution Policy, Legislation and Practice

Scottish housing and homelessness policy has been largely devolved to the Scottish Parliament since 1999. Consequently, although the legal definition of homelessness remains unchanged since 1987 (see Box 7.1), Scotland has developed its own distinctive approach to homelessness, accentuating pre-existing differences in policy to the rest of the UK. This has occurred following the establishment of the Homelessness Task Force (1999-2002), which was commissioned by the Scottish Executive to comprehensively review legislation, policy and practice on homelessness in Scotland and to provide recommendations on how homelessness could be prevented and tackled (Homelessness Task Force, 2002).

Box 7.1. Legal Definition of Homelessness in Scotland.

1. A person is homeless if they have no accommodation in the United Kingdom or elsewhere.
2. A person is to be treated as having no accommodation if there is no accommodation which they, together with any other person who normally resides with them, are legally entitled to occupy. A person shall not be treated as having accommodation unless it is accommodation which it would be reasonable for them to continue to occupy, though regard may be had to the general housing circumstances prevailing in the local authority area.
3. A person is also homeless if:
    A. They have accommodation but cannot secure entry to it, or it is probable that occupation would lead to violence or threats of violence;
    B. Or the accommodation consists of a moveable structure, vehicle or vessel for human habitation and there is no place where they are entitled or permitted to place and reside in it;
    C. Or the accommodation is legally overcrowded and may endanger the health of occupants;
    D. Or it is not permanent accommodation and the local authority duty arose before occupation of the accommodation.
4. A person is threatened with homelessness if it is likely that they will become homeless within 2 months

Source: Amended from Housing (Scotland) Act 1987 and cited in Anderson and Serpa (2013, p. 16)

The work of the Homelessness Task Force has led to a legislative shift in Scotland away from the previous 'priority need' system. The 'priority need' system, which gave the right to settled accommodation to some homeless people (e.g. households with dependent children, pregnant women, care leavers or vulnerable people), but not others, has long being viewed as unfair and had been described as a "double edged sword" for excluding many households from rights (Drake, 1989, p. 126). Scotland has moved away from this system initially via the Housing (Scotland) Act 2001 and the Homelessness Etc. (Scotland) Act 2003, which strengthened the statutory safety net and extended councils' duties to non-priority homeless households in the build up to the ambitious target of fully abolishing the 'priority need' test by 2012, thus extending the right to settled accommodation to all those assessed as homeless (Anderson, 2009, pp. 107-109). Subsequently, this target has been successfully achieved in 2012 via the Homelessness (Abolition of Priority Need Test) (Scotland) Order 2012, which extended the right to settled accommodation (i.e. to a 'Scottish Secure Tenancy') to all unintentionally homeless households (although not those assessed as intentionally homeless).

Scotland's initial commitment to abolish 'priority need' and extend the right to housing was internationally lauded, with the Scottish homelessness system being interpreted as a unique 'rights based' approach (Anderson, 2009, p. 107), and referred to as "some of the most progressive homelessness legislation in Europe" (Scottish Executive, 2005, p. 10). However, implementing this new system has not been problem-free. As noted in one implementation review, "outcomes for those facing homelessness [have] varied somewhat from the highest aspirations of the radical 2002 policy review" (Anderson and Serpa, 2013, p. 13). Alongside the implications for homelessness from austerity measures and welfare reform (see Beatty, Foden, McCarthy *et al.*, 2015; Loopstra, Reeves, Barr *et al.*, 2016; Johnsen, Watts and Fitzpatrick, 2018), another issue has been that the extension of the right to settled accommodation has increased demand, which has led to a big challenge for local authorities to secure enough settled accommodation for everyone (Anderson and Serpa, 2013, p. 26; Lund, 2017, pp. 174-175). This led to increases in the numbers in temporary accommodation in Scotland, and ultimately has led to the adoption of a 'prevention' strategy from 2009/10, whereby information and advice is offered via the aforementioned Housing Options approach system, which is designed to reduce the number of people becoming officially homeless (Scottish Government, 2020a). This 'prevention' approach and the use of Housing Options has been positively reviewed (Ipsos Mori and Littlewood, 2012), but there has been controversy throughout over potential 'gatekeeping', with Housing Options sitting uncomfortably alongside the statutory homelessness system, and being used to discourage

people from making official homeless applications and accessing their statutory rights (Pawson, 2009; Fitzpatrick, Pawson, Bramley *et al.*, 2015). Consequently, it has been argued that Scotland should adopt homelessness prevention legislation similar to that in Wales and England, which place duties on local authorities to provide assistance to all eligible homeless households or households threatened with homelessness in the next 56 days (regardless of their 'priority need' status) (Fitzpatrick, Mackie and Wood, 2019).

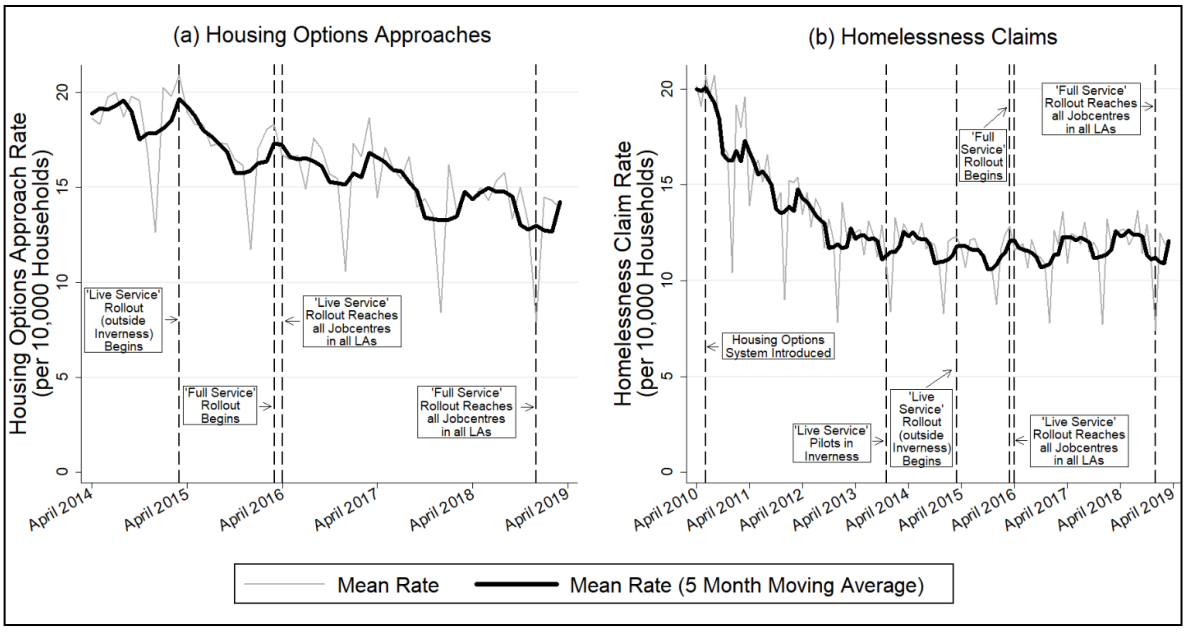
7.2.2 Scottish Homelessness Statistics: Data Collection, Key Trends and Challenges

The Scottish Government's National Statistics on homelessness are made up of two separate data collections. Firstly, homelessness data is collected from local authorities on statutory homelessness applications (including overall number of claims, main reasons for applications and characteristics/prior circumstance of applicants) and use of temporary accommodation, which are officially published bi-annually. Secondly, Housing Options data is collected from local authorities, which measures the number of Housing Options approaches and outcomes for those making the approach (e.g. whether they go on to make a homelessness claim or manage to remain in their accommodation/find alternative accommodation). Taken together, these statistics can provide insight into the scale of homelessness assistance need within local authorities over time in Scotland. However, trends in the data must be interpreted in the context of administrative and legislative changes ongoing at the time, as well as other data limitations (as discussed below).

The Scottish Government's own analysis of homelessness statistics suggests that following on from the extension of councils' duties to non-priority households, the number of homelessness applications made in Scotland rose from 52,217 (and 39,712 acceptances) in 2002/2003 to a peak of 60,542 applications (and 43,534 acceptances) in 2005/2006 (Scottish Government, 2018). More recent trends, covering the mean rates of both new Housing Options approaches and new homelessness claims across local authorities, are provided in Figure 7.1. There are monthly fluctuations (most notably via sharp reductions in rates in December each year), but the moving average highlights that, in general, there was a large drop in homelessness claim rates from 20.2 claims (per 10,000 households) in April 2010 down to 10.7 (per 10,000 households) in November 2014. This drop has largely been attributed to Scotland adopting their 'prevention' strategy and Housing Options system

(Scottish Government, 2018, p. 4). Indeed, analysis has shown that the annual level of 'homelessness presentations' – i.e. taking into account both homelessness applications and 'homelessness type' Housing Options approaches from 2009/10 – have in fact remained relatively steady at around 54,000 (Fitzpatrick, Pawson, Bramley *et al.*, 2015). In terms of trends in Housing Options approaches, Figure 7.1 highlights that since data collection began in April 2014, there has been a steady decrease in the rate of Housing Options approaches. One possible explanation for this is that an increasing number of households are making homelessness applications directly without first making a Housing Options approach (Scottish Government, 2019a).

Figure 7.1. Trends in the Mean Rates of Housing Options Approaches (April 2014 – March 2019) and Homelessness Applications (January 2010 – March 2019) Across 29 Scottish local authorities (per 10,000 households.



Data Source: Scottish Government Homelessness Statistics – authors analysis.

In addition to the issue of homelessness statistics being influenced by legislative and administrative changes, another issue is that people will not apply for homelessness assistance from their local authority (and thus will not be recorded in the statistics) if they think that they will not receive help. Importantly, this can reduce the reliability of official homelessness data as a means of gauging the scale of homelessness over time. For example, despite other evidence of acute housing and homelessness pressures in Edinburgh, there has been a sharp fall in both homelessness applications and Housing Options approaches in

recent years. This has been interpreted by some as arising from potential applicants feelings that Edinburgh provide poor offers to applicants (Fitzpatrick, Pawson, Bramley *et al.*, 2019).

Moreover, one final (and related) challenge with regards to official statistics is that they by no means fully capture homelessness. In general, governments tend to use 'minimalist' definitions of homelessness in order to reduce the size of the problem they have to deal with (Fitzpatrick, Kemp and Klinker, 2000), and whilst the Scottish Government have been praised for their wide-ranging definition of homelessness (as previously set out in Box 7.1), there are still many who could be classed as homeless but who are not picked up by the legal definition or in official homelessness statistics. This includes those involuntarily living with other households, squatters, 'sofa-surfers', those living in overcrowded/substandard conditions and those sleeping rough in hidden locations. The scale of 'hidden homelessness' is, by nature, difficult to measure. However, Scotland's 'Homelessness Monitor' in 2015 (Fitzpatrick, Pawson et al., 2015) did attempt to measure some forms of 'hidden homelessness' using official survey and census data. Their analysis suggests the following:

- About 9.3% of households in Scotland contain 'concealed households' (i.e. individuals/groups/families that are not able to form separate households so are obliged to live with others). This is made up of non-dependent children (6.7%), unrelated adults (2.3%) and concealed families (0.9%).

- 'Concealed household' trends in Scotland tend to parallel those in England.

- Scotland appears to have a higher rate of household sharing than England, which can be an indicator of 'hidden homelessness' including 'sofa-surfing', although this may partly reflect differences in housing regulation.

- Until 2010 overcrowding levels tended to be higher in England than Scotland but have since evened out due to a rise in overcrowding in Scotland.

- Overcrowding in Scotland appears to be strongly correlated with poverty, and most commonly occurs in Glasgow and other major cities, followed by the poorest urban areas (e.g. Inverclyde and West Dunbartonshire)

7.2.3 A Broad Overview of the Determinants of Homelessness

Traditionally, explanations of homelessness causation have tended to focus upon either 'individual' or 'structural' factors. 'Individual' explanations, which were dominant in the 1960s and 1970s, emphasise the personal vulnerabilities and behaviours of homeless people e.g. substance dependency, mental health problems, offending, family dysfunction and a lack of social support. These accounts often place an emphasis upon personal 'agency' and thus can have connotations of individual culpability and blameworthiness (Bramley and Fitzpatrick, 2017, p. 97). Conversely, 'structural' accounts, which became dominant in the 1980s, emphasise social and economic issues e.g., housing market conditions, poverty, unemployment and the welfare system. However, these accounts came under pressure in the 1990s when the growing number of single homeless people in the UK (particularly rough sleepers) tended to have high social support and health needs (Watts, Johnsen and Sosenko, 2015).

In the 21st century, academic literature has tended to suggest that explanations of homelessness are more nuanced than the old 'individual' versus 'structural' dichotomy, and that there is in fact a lot of interaction between different causes of homelessness. For example, Pleace (2000 cited in Fitzpatrick, Pleace, Stephens *et al.*, 2009, p. 4) described and critiqued the following 'new orthodoxy' of assertions on homelessness causes: (a) 'Structural' factors like housing shortages, poverty, and unemployment create the conditions for homelessness and impact upon its prevalence, (b) people with 'individual' personal problems are more vulnerable to homelessness under these conditions, and (c) high support needs among homeless people are explained by their susceptibility to structural forces rather than being the cause of their homelessness. However, 'hybrid' explanations such as this which incorporate both individual and structural factors, but emphasise the importance of structural factors, have been critiqued as "unsatisfying" for being too 'simplistic' and 'positivist' (Fitzpatrick, Pleace, Stephens *et al.*, 2009). It has instead been argued from a 'critical realist' perspective that, in reality, causation of homelessness is more complex than this and operates on four levels – economic, housing, interpersonal and individual – that interact in complicated and unpredictable ways, with economic and housing factors being most important for some homeless people, and individual factors being most important for others (Fitzpatrick, 2005).

Importantly, international comparative research also suggests that the relative importance of individual and structural factors in homelessness causation varies between countries. E.g. it has been posited that countries whose generous social democratic welfare regimes produce low levels of poverty and inequality (e.g. Sweden and Denmark) tend to have a lower prevalence of homelessness, and that those who are homeless are likely to be so due to complex personal problems like mental illness and/or addiction rather than structural factors (Stephens and Fitzpatrick, 2007). Conversely, countries with liberal welfare regimes that produce higher poverty and inequality (e.g. the US and the UK) tend to have greater homelessness prevalence which is more likely to occur structurally rather than through personal problems (Stephens and Fitzpatrick, 2007).

Much of the literature involving empirical modelling of homelessness determinants comes from the US. One such study used 1990 census data to assess variation in homelessness rates across 335 US metropolitan areas, and found housing affordability and demographic composition to be the key determinants of homelessness, with higher median rents and higher representation of single-person households being significantly associated with higher homelessness rates (Lee, Price-Spratlen and Kanan, 2003). It also found economic conditions, welfare safety net and climate to be further homelessness determinants (ibid). More recently, a study by Fargo, Munley et al. (2013) modelled homelessness determinants for four US subpopulations: families and single adults in metropolitan and non-metropolitan areas. It found economic factors like housing costs, unemployment and income to be the most important determinants of homelessness for all groups, whilst demographic and safety net factors were also important for all single-adults, and drug use was a uniquely important determinant for single adults in metropolitan areas (ibid). Gould and Williams (2010)'s study of the number of people in family units in emergency homelessness shelters further highlights unemployment as a key homelessness determinant, as well as a lack of social welfare support.

In terms of modelling in the UK, Loopstra, Reeves et al. (2016)'s cross-area analysis of English local authority data found that increasing rates of homelessness were "strongly linked" with budget cuts and reductions in welfare spending (particularly on housing services, discretionary housing payments and social care). Another important study found that structural factors such as higher house prices, fewer homelessness prevention programmes and lower household incomes are all significantly associated with increased statutory homelessness rates in England (Bramley, Pawson, White *et al.*, 2010). A more

recent study by Bramley and Fitzpatrick (2017) suggests that the odds of experiencing homelessness in the UK are "systematically structured" around a set of structural, social and individual factors outwith the control of those affected. It specifically highlighted the centrality of poverty (especially childhood poverty) to homelessness, as well as other structural factors like labour market and housing market contexts, and clearly discredits the myth that homelessness 'can happen to anyone'.

7.2.4 The Scottish Context: Reasons for Homelessness and Prior Circumstances of Homelessness Applicants

The main reasons for homelessness, and the prior circumstances of those making homelessness applications may differ somewhat in Scotland compared to other countries e.g. England or the US where much of the homelessness literature (discussed above) comes from.

As previously discussed, Scotland's official statistics on homelessness claims record the main reason why those submitting applications have become homeless, and (closely related) their prior circumstances. The main reasons cited have remained relatively unchanged over the past decade, with being "asked to leave" their prior accommodation – i.e. by the host, likely to be a parent (Fitzpatrick, Pawson, Bramley *et al.*, 2019, p. 72) – being the top one (25% of applicants in 2017/18) (Scottish Government, 2018, p. 13). Household disputes/relationship breakdown tends to be another key driver recorded – in 2017/18 13% and 18% of applicants became homeless following a violent or non-violent domestic household dispute respectively (Scottish Government, 2018, p. 13).

This prevalence of homelessness due to leaving a family home or shared home with a partner is also reflected in statistics on the prior circumstances of homelessness applicants in Scotland. Specifically, in 2018/19, homelessness applicants were more likely to have been living with friends, family or partners (42% of applicants) than to have been living in their own owned or rented accommodation (38% of applicants). This is in contrast with England, where fewer people tend to live with family/friends, and the main reason cited for homelessness is the ending of a private rented tenancy, followed by rent arrears – it has been noted that this is less of a problem in Scotland due to its "less challenging private rented sector, although unaffordability is a growing issue" (Shelter, 2016, p. 8).

Whilst the previous two empirical chapters suggest that UC rollout has increased demand for advice from Citizens Advice on rent arrears and on repossession actions made by landlords within English local authorities, the distinct reasons for homelessness in Scotland discussed above motivate the need for research on the impact of UC rollout on homelessness in Scotland, by reason and prior circumstances. However, it is important to note that data on the main cited reason for homelessness is unlikely to capture the full picture of why households become homeless. This is because, as set out in the previous section, becoming homeless is usually a complex process involving interaction between a number of variables, including 'structural' determinants and 'individual' triggers, rather than being down to one singular reason (Lund, 2017, pp. 157-159). For example, whilst being "asked to leave" and relationship breakdown/disputes are the main cited reasons for homelessness in Scotland, it is unlikely that this tells the full story, as it has been noted that:

> Family conflict can be the result of individual problems and support needs (e.g. substance misuse), interpersonal difficulties (e.g. in 'blended families', abusive relationships) or manifestations of childhood trauma. Further to this, stress within families may also be caused by 'structural' factors, for instance housing/labour market and welfare changes. (Watts, Johnsen and Sosenko, 2015).

This suggests that family conflict and relationship breakdown can be linked to economic factors, including welfare reform, with financial pressures putting a strain on relationships with family members and partners. Similarly, it has also been noted in Scotland's latest 'Homelessness Monitor' in 2019 (Fitzpatrick, Pawson, Bramley *et al.*, 2019, p. 3) that 'anchor' social relationships, which usually provide a homelessness 'buffer', can be strained by financial pressures, meaning that worsening economic conditions may increase 'interpersonal' vulnerability to homelessness over time in Scotland. Therefore, the negative financial impact of UC's long wait periods, increased conditionality and monthly direct payments may result in family conflict, and this may be a particular issue in Scotland due to its prevalence of homelessness arising from being "asked to leave" or relationship breakdown. Moreover, the impact of UC rollout on relationships may be exacerbated further by its default system of making payments into a single bank account (i.e. one partner of a couple), which has been criticised for reinforcing a 'male breadwinner' model and potentially leading to financial abuse (Howard, 2018; Women's Budget Group, 2018).

7.3 Data, Variables and Methods

7.3.1 Setting

For the purposes of this chapter's empirical analysis, a monthly Scottish local authority level dataset was compiled, with a final sample of 29 of Scotland's 32 local authorities. Na h-Eileanan Siar, Orkney Islands and Shetland Islands were excluded from the analysis due to their small population sizes.

Ideally, the final analytic sample would cover the entire UC rollout period. However, this was not possible here due to the need to minimise omitted variable bias, as well as data availability issues. The Scottish Government began to collect data on Housing Options approaches from April 2014, and thus the analytic sample for the analysis on Housing Options covered the period of April 2014 – March 2019. However, the analysis of homelessness claims data used a different timeframe of January 2015 – March 2019.

Importantly – as discussed/shown in Figure 7.1 – data on homelessness claims was affected by the introduction of Housing Options approaches in 2010, which meant that there was a large drop in claims. The inclusion of local authority and time fixed effects can control for baseline differences between local authorities and unobserved variables that vary over time but not between local authorities (Stock and Watson, 2015). However, their inclusion does not control for unobserved variables that vary both over time and between local authorities. Changes in homelessness claim rates due to introduction of Housing Options falls into this category as it is up to local authorities themselves to decide what constitutes an approach, which leads to large variation in rates of Housing Options approaches between them (Scottish Government, 2019a). Therefore, in order to reduce omitted variable bias, the analytic sample used to examine homelessness claims is restricted to the period of January 2015 – March 2019. Omitted variable bias should not be an issue from 2015 onwards as the effect of Housing Options on decreasing homelessness had reached a plateau and was said to be unlikely to be still having an impact on claim rates (Scottish Government, 2019b; Scottish Government, 2020b).

7.3.2 Homelessness and Housing Options Data (Outcome Variables)

The Scottish Government data on the number of Housing Options Approaches (April 2014 – March 2019) was used to create the first outcome variable: '*total Housing Options approach rate*'. This indicates, for each local authority, the total monthly number of Housing Options approaches, coded as a rate per 10,000 households in the local authority using National Records for Scotland (NRS) data on household estimates.

The data on the number of homelessness claims (January 2015 – March 2019) was then used to create a further ten outcome variables. Firstly, there is the '*working age homelessness claim rate*', which indicates the total number of homelessness claims made by households with a working age (16-64 year olds) main applicant. The variable is coded into a rate per 10,000 working age households (i.e. where head of household is aged 16-64 years) using NRS data on household projections.

Next, there are four outcome variables indicating the rate of homelessness claims by the main cited reason for the application. These are: (1) '*asked to leave rate*', (2) '*non-violent dispute rate*', (3) '*violent dispute rate*', which are all per 10,000 households in the local authority using NRS household estimates data, and (4) '*terminated tenancy rate*', which is coded into a rate 10,000 rented dwellings in the local authority using data obtained from the Scottish Government on dwelling stocks by tenure.

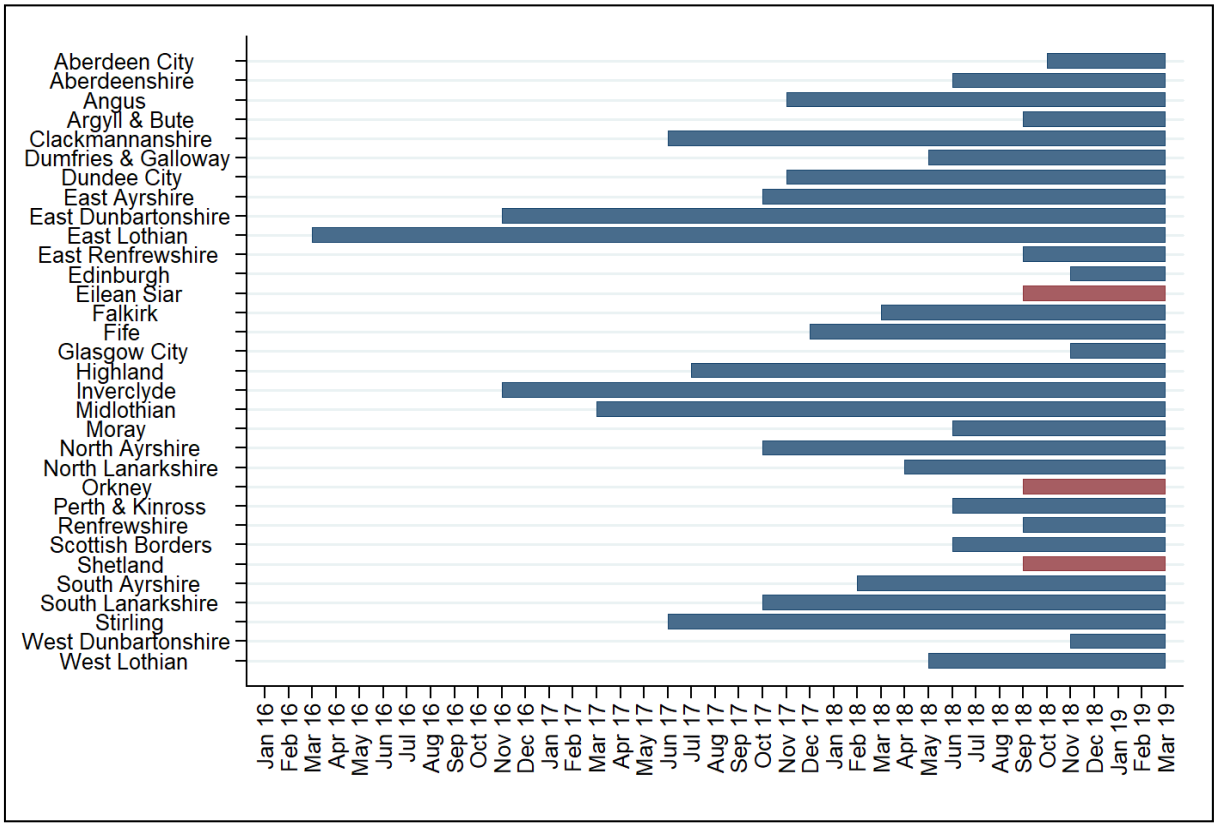
Finally, there are five outcome variables indicating the rate of homelessness claims by the prior circumstances of the applicant. These are: (1) '*staying in family home or with relatives rate*', (2) '*staying with friends/partners rate*', which are also coded as rates per 10,000 households in the local authority using NRS household estimates data, and (3) *'SRS property rate', (4) 'PRS property rate',* and (5) '*owner occupied property rate*', which are all coded per 10,000 dwellings in the given housing tenure using the aforementioned data obtained from the Scottish Government on dwelling stocks by tenure.

7.3.3 Universal Credit Data (Explanatory Variables)

The analysis uses two explanatory variables, which are similar to the main explanatory variables used in Chapters 5 and 6, but are applied to the monthly Scottish local authority

data as opposed to English quarterly data. These are: (1) '*UC Full Service'*, and (2) '*UC Full Service (by rollout length)*'. '*UC Full Service*' is created using data from the official UC rollout schedule on the timing of 'Full Service' rollout, i.e. it exploits variation in the timing of UC rollout across Scottish local authorities – this variation is summarised in Figure 7.2, and is also available in table form in Appendix 2. This is a binary variable and indicates whether 'Full Service' had rolled out yet in each month of the analysis period (coded 0 = '*No*' and 1 = '*Yes*'). Where rollout occurred in different Jobcentres within a local authority in different months it was coded as '*Yes*' only once rollout had reached the majority of Jobcentres. '*UC Full Service (by rollout length)*' is a categorical variable indicating if 'Full Service' had rolled out yet in each month of the analysis period, and if so for how long. It is coded as follows: 0 = '*No (pre rollout)*' 1 = '*Yes (1-3 months post)*', 2 = '*Yes (4-6 months post)*', 3 = '*Yes (7-9 months post)*', 4 = '*Yes 10-12 months post*', and 5 = '*Yes (13+ months post)*'.

Figure 7.2. Variation in Universal Credit 'Full Service' Rollout Across Scottish local authorities.



Notes: start point of bars represent month in which UC 'Full Service' rolled out within the local authority. Maroon bars represent the local authorities that were excluded from the analysis due to low population sizes. Data sources for rollout schedule: UK Government (2015a); UK Government (2015b); UK Government (2018)

Unlike in the analysis in previous chapters, the analysis here does not control for 'Live Service' rollout as the analysis period starts too late to accurately do so. However, this should not bias the results, as 'Live Service' involved relatively few cases and the analysis outlined in previous chapters have found no significant relationship between 'Live Service' rollout and housing insecurity indicators within English local authorities.

7.3.4 Control Variables

As discussed earlier in this chapter (in sections 7.2.3 and 7.2.4), homelessness causation is complex and is determined by various individual and structural factors. Without individual level data on those making Housing Options approaches or homelessness applications, it is not possible to control for all homelessness determinants in this analysis. However, the analysis does include four control variables in order to control for some key determinants.

As local labour market conditions are important homelessness determinants (Rossi and Wright, 1987; Lee, Price-Spratlen and Kanan, 2003) the first two control variables are: (1) '*model based unemployment rate*', which comes from NOMIS and provides a monthly estimate of local authority unemployment rates, based on the previous twelve months of 'Annual Population Survey' data, and (2) '10^{th} percentile weekly wages', which also comes from NOMIS and provides a monthly estimate of the lower ten percentile of (full-time and part-time) weekly wages in the local authority based on linear interpolated annual data. As recent research (Bramley and Fitzpatrick, 2017) has highlighted the particular vulnerability of single adult and single parent households to homelessness, the third/fourth control variables are: (3) '*rate of single adult households*' and (4) '*rate of single parent households*', which come from annual data from the Scottish Household Survey on the proportion of each household type, and is converted into rates per 10,000 households using NRS data on household estimates.

7.3.5 Statistical Analysis

As in Chapters 5 and 6, the empirical analysis here involves fixed effects panel models. These were used to formally examine the relationship between UC rollout and rates of Housing Options approaches and homelessness claims (with separate models for total

working age claims, claims by main cited reason, and claims by prior circumstances of applicant) within local authorities. As in Chapters 5 and 6, this exploits variation in the timing of UC rollout across local authorities to measure its impact. This is as follows:

$$HOA/HC\ Rate_{it} = \beta_0 + \beta_1 UCFS_{it} + \beta_2 Unemployment_{it} + \beta_3 SAHH\ Rate_{it} + \beta_4 SPHH\ Rate_{it} + \beta_6 Month_t + \alpha_i + u_{it} \quad (7.1)$$

Here, i is the local authority and t is the monthly time point. $HOA/HC\ Rate$ is the rate of Housing Options approaches/homelessness claims. Models in the first part of the analysis focus on the relationship between 'Full Service' rollout and the '*total Housing Options approach rate*' and '*working age homelessness claim rate*'. In subsequent models, homelessness claim rates are disaggregated by the main cited reason for the claim and the prior circumstances of the applicant. This is to examine whether UC rollout is straining relationships and thus increasing homelessness arising from leaving partners/friends/family home due to being "asked to leave"/relationship breakdown, and/or whether it is increasing homelessness arising from termination of tenancies in rented accommodation.

The final part of the analysis measures whether the impact of UC 'Full Service' rollout on Housing Options approach rates and homelessness claim rates (if any found) increases when it has been rolled out for longer and thus reached more claimants. This is as follows:

$$HOA/HC\ Rate_{it} = \beta_0 + \beta_1 UCFS\ Length_{it} + \beta_2 UCFS_{it} + \beta_2 Unemployment_{it} + \beta_3 SAHH\ Rate_{it} + \beta_4 SPHH\ Rate_{it} + \beta_6 Month_t + \alpha_i + u_{it} \quad (7.2)$$

Here, $UCFS\ Length$ is the '*UC Full Service (by length of rollout)*' categorical variable and all other variables are the same as those in Equation 7.1.

7.4 Results

7.4.1 Overall Impact of UC 'Full Service' Rollout

The relationship between UC 'Full Service' and the total Housing Options approach rate and working age homelessness claim rate within Scottish local authorities is set out in Table 7.1.

The results suggest that, on average, UC 'Full Service' rollout was associated with an increase of 1.32 Housing Options approaches (per 10,000 households) within local authorities up to March 2019. To put this figure into context, the mean Housing Options approach rate in the 6-month period prior to 'Full Service' rollout beginning (i.e. September 2015 – February 2016) was 16.2 approaches (per 10,000 households) – therefore this coefficient corresponds to approximately an 8.1% increase on the pre rollout period.

In addition, the results outlined in Table 7.1 also suggest that, on average, UC 'Full Service' was associated with an increase of 0.45 working age homelessness claims (per 10,000 working age households) within local authorities up to March 2019. This is a relatively small, albeit statistically significant, increase given that the mean homelessness claim rate in the 6 months prior to rollout was 15.2 claims (per 10,000 working age households) – this means that the coefficient corresponds approximately to a 3.0% increase on the pre rollout period.

Table 7.1. Relationship between UC 'Full Service' rollout and the Total Housing Options Approach Rate (April 2014 – March 2019) and Working Age Homelessness Claim Rate (January 2015 – March 2019) Across 29 Scottish Local Authorities.

	(1) Total Housing Options Approach Rate	(2) Working Age Homelessness Claim Rate
UC 'Full Service' Rolled Out		
[No]		
Yes	1.32*	0.45*
	(0.57)	(0.18)
Per 1% rise in	0.05	0.07
Model Based Unemployment Rate	(0.19)	(0.11)
Per £10 increase in	0.23	0.12
10th Percentile Weekly Wages	(0.34)	(0.10)
Per 100 households increase in	0.01	0.09**
Rate of Single Adult Households	(0.06)	(0.03)
Per 100 households increase in	0.25*	0.19**
Rate of Single Parent Households	(0.12)	(0.06)
Local Authority Months	1735	1479
R^2	0.235	0.352

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (monthly) time fixed effects. All rates are per 10,000 households in the local authority, except '*Working Age Homelessness Claim Rate*', which is per 10,000 working age households in the local authority. 10th percentile weekly wages includes both part-time and full-time work. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

The fact that a larger impact was observed upon Housing Options approaches than on homelessness claims is logical given that a Housing Options approach is likely to occur earlier than a homelessness claim as it is in itself designed to prevent actual homelessness. This is reflected in the fact that around 50% of Homelessness claims come after a Housing Options approach has already been made (Scottish Government, 2019a).

In terms of the control variables modelled in Table 7.1, the outcome variables tended to have positive associations with the rate of single adult and single parent households as expected (see section 7.3.4). However, there was no significant association with wages or unemployment rates – this may reflect the limitations of using linear interpolated annual data in the models, although the result is in line with previous local authority level analysis in England that also found no significant association between unemployment rates and homelessness claim rates (Loopstra, Reeves, Barr *et al.*, 2016).

7.4.2 Impact of UC 'Full Service' Rollout on Homelessness Claims by Main Reason for Claim and Prior Circumstances of Applicant

Given that homelessness can arise for a variety of – often interlinked – reasons and from a variety of prior circumstances, the regression models in this section disaggregate the data on homelessness claim rates by: (a) the main cited reason for the claim, and (b) the prior housing circumstances of the applicant. This can provide insight into whether UC rollout is linked to homelessness arising from relationship breakdown/being "asked to leave" a partner/friend/family's home, and/or homelessness arising from one's own property e.g. due to a terminated tenancy.

The relationship between UC 'Full Service' rollout and homelessness claim rates, by the main cited reason for the application, is modelled in Table 7.2. The results suggest that UC 'Full Service' rollout is associated with an increase of 0.22 homelessness claims (per 10,000 households) where the applicants cite "asked to leave" by the host as the main reason for their claim. However, no significant relationship was found between 'Full Service' rollout and the rate of homelessness claims citing household disputes or a terminated tenancy as the main reason. As mentioned in section 7.2.4., being "asked to leave" by the 'host' is the most common reason for homelessness claims in Scotland. The results outlined in Table 7.2 suggest that this issue may be being exacerbated by UC rollout, due to UC's long wait

periods, increased conditionality and monthly direct payments putting a strain on household finances and in turn straining relationships and the affordability of looking after all household members with payments. However, the results suggest that less common reasons for homelessness in Scotland e.g. terminated tenancies have not been impacted by UC up to March 2019.

Table 7.2. Relationship Between UC 'Full Service' Rollout and Homelessness Claim Rates Across 29 Scottish Local Authorities, by Main Cited Reason for Application, January 2015 – March 2019.

	(1) Asked to Leave Rate	(2) Non-Violent Household Dispute Rate	(3) Violent Household Dispute Rate	(4) Terminated Tenancy Rate
UC 'Full Service' Rolled Out				
[No]				
Yes	0.22**	0.08	-0.03	-0.33
	(0.08)	(0.08)	(0.06)	(0.28)
Per 1% rise in **Model Based Unemployment Rate**	0.04	0.06*	0.005	-0.03
	(0.02)	(0.03)	(0.01)	(0.10)
Per £10 increase in **10th Percentile Weekly Wages**	0.07*	-0.02	-0.02	0.01
	(0.03)	(0.03)	(0.02)	(0.01)
Per 100 households increase in **Rate of Single Adult Households**	0.03**	0.01	0.004	-0.02
	(0.01)	(0.01)	(0.007)	(0.02)
Per 100 households increase in **Rate of Single Parent Households**	-0.004	0.05*	0.04**	0.005
	(0.002)	(0.02)	(0.01)	(0.05)
Local Authority Months	1479	1479	1479	1479
$\mathbf{R^2}$	0.175	0.146	0.107	0.128

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (monthly) time fixed effects. All rates are per 10,000 households in the local authority, except the '*Terminated Tenancy Rate'*, which is per 10,000 rented dwellings. 10th percentile weekly wages includes both part-time and full-time work. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

The relationship between 'Full Service' rollout and homelessness claim rates, by prior circumstances of the applicant making the claim, is modelled in Table 7.3. No significant relationships were found between 'Full Service' rollout and any of the prior circumstance categories at the 5% level. However, the positive relationship between UC 'Full Service' rollout and claims from those previously staying in family home or with relatives was significant at the 10% level – this suggests that the increase in homelessness claims from those "asked to leave" by the host are likely to be coming from those in family homes or staying with relatives – this is unsurprising given that, as noted in section 7.2.4, the 'host' in these circumstances tends to be a parent (Fitzpatrick, Pawson, Bramley *et al.*, 2019, p. 72).

Table 7.3. Relationship Between UC 'Full Service' Rollout and Homelessness Claim Rates Across 29 Scottish Local Authorities, by Prior Circumstances of Applicant, January 2015 – March 2019.

	(1) Staying in Family Home or with Relatives Rate	(2) Staying with Friends/Partners Rate	(3) SRS Property Rate	(4) PRS Property Rate	(5) Owner Occupied Property Rate
UC 'Full Service' Rolled Out					
[No]					
Yes	0.17+	0.04	-0.03	0.15	-0.04
	(0.10)	(0.05)	(0.24)	(0.10)	(0.05)
Per 1% rise in	0.07**	-0.02	-0.11	0.02	0.02
Model Based Unemployment Rate	(0.03)	(0.02)	(0.09)	(0.06)	(0.02)
Per £10 increase in	0.003	0.02	-0.03	0.05	0.03
10th Percentile Weekly Wages	(0.04)	(0.02)	(0.02)	(0.05)	(0.02)
Per 100 households increase in	0.03**	0.01+	-0.01	0.01	0.000
Rate of Single Adult Households	(0.01)	(0.007)	(0.04)	(0.01)	(0.006)
Per 100 households increase in	-0.01	0.02	0.21***	0.000	0.01
Rate of Single Parent Households	(0.01)	(0.02)	(0.06)	(0.03)	(0.01)
Local Authority Months	1479	1479	1479	1479	1479
R^2	0.165	0.139	0.099	0.204	0.055

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (monthly) time fixed effects. All rates are per 10,000 households in the local authority, except the '*SRS Property Rate*', '*PRS Property Rate', and* '*Owner Occupied Property Rate*' which is per 10,000 rented dwellings in the given housing tenure. 10th percentile weekly wages includes both part-time and full-time work. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

7.4.3 Impact by Length of Rollout

The above analysis was repeated in Tables 7.4-7.6 using the '*UC Full Service (by rollout length)*' explanatory variable instead of the '*UC Full Service'* variable. As in the previous empirical chapters, this is carried out in order to examine whether the impact of UC 'Full Service' rollout increases when it has been rolled out for longer within local authorities, and therefore reached more claimants. To ease interpretation visually, the coefficients from the regression models in Tables 5-7 are also plotted in Figures 7.3-7.5.

The results in Table 7.4 (visualised in Figure 7.3) suggest that, in general, the impact of UC 'Full Service' rollout on Housing Options approaches does indeed tend to increase when it has been rolled out for longer. Specifically, it is associated with an increase of 0.65 approaches in the first 1-3 months post rollout (although not statistically significant), rising to 2.50 additional approaches 4-6 months post rollout and 3.65 additional approaches where 'Full Service' has been rolled out for 13+ months (all per 10,000 households). To put this into context, given that the mean Housing Options approach rate in the 6-months prior to

'Full Service' rollout beginning (i.e. September 2015 – February 2016) was 16.2 approaches (per 10,000 households), the 3.65 additional approaches (per 10,000 households) where it has been rolled out for 13+ months corresponds to a 22.5% increase on pre rollout rates.

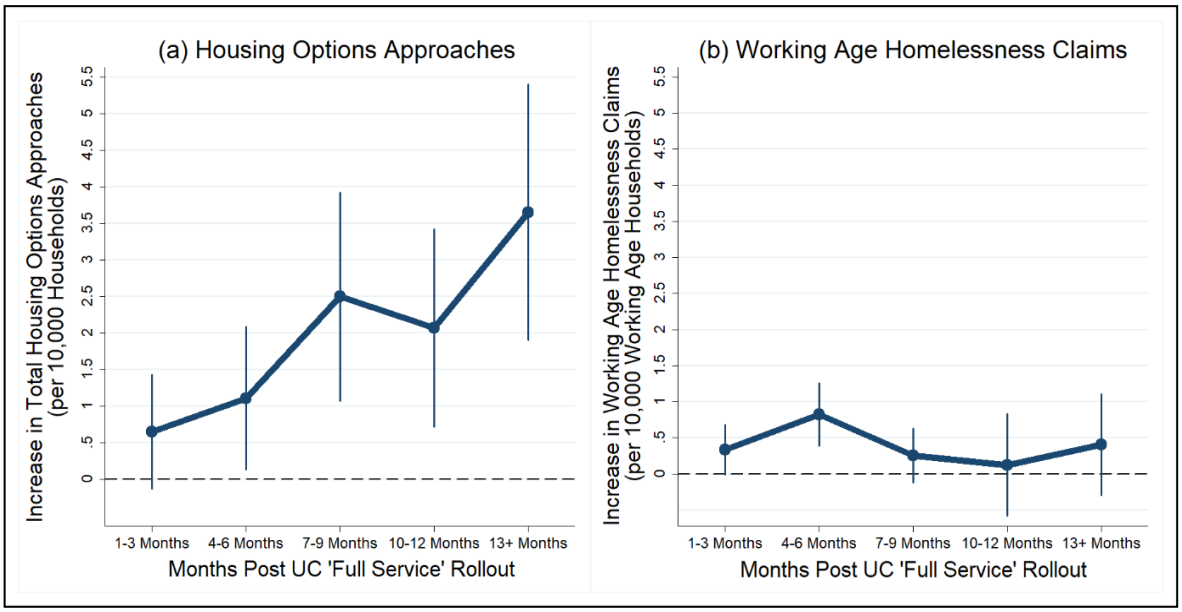
However, the impact of UC 'Full Service' rollout on working age homelessness claim rates is less clear. The results suggest that UC 'Full Service' rollout is associated with an increase of 0.34 working age homelessness claims (per 10,000 working age households) in the first 1-3 months post rollout. This rises to 0.83 claims (per 10,000 working age households) in the 4-6 months post rollout. However, the impact then appears to reduce. This suggests that although a positive relationship exists between 'Full Service' rollout and the working age homelessness claim rate, the impact of rollout does not appear to increase the longer it has been rolled out despite the fact that more people will have moved onto UC.

Table 7.4. Relationship Between UC 'Full Service' Rollout and the Total Housing Options Approach Rate (April 2014 – March 2019) and Working Age Homelessness Claim Rate (January 2015 – March 2019), by Rollout Length, Across 29 Scottish Local Authorities.

	(1) Total Housing Options Approach Rate	(2) Working Age Homelessness Claim Rate
UC 'Full Service' Rolled Out		
[No]		
Yes [1-3 Months post]	0.65	0.34+
	(0.39)	(0.17)
Yes [4-6 Months post]	1.11*	0.83***
	(0.49)	(0.22)
Yes [7-9 Months post]	2.50**	0.25
	(0.71)	(0.18)
Yes [10-12 Months post]	2.07**	0.12
	(0.67)	(0.35)
Yes [13+ Months Post]	3.65***	0.40
	(0.88)	(0.35)
Per 1% rise in **Model Based Unemployment Rate**	0.11	0.07
	(0.19)	(0.11)
Per £10 increase in **10th Percentile Weekly Wages**	0.17	0.12
	(0.03)	(0.10)
Per 100 households increase in **Rate of Single Adult Households**	-0.02	0.09**
	(0.06)	(0.03)
Per 100 households increase in **Rate of Single Parent Households**	0.28*	0.19**
	(0.12)	(0.06)
Local Authority Months	1735	1479
R^2	0.243	0.353

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (monthly) time fixed effects. All rates are per 10,000 households in the local authority, except '*Working Age Homelessness Claim Rate*', which is per 10,000 working age households in the local authority. 10th percentile weekly wages includes both part-time and full-time work. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

Figure 7.3. Impact of UC 'Full Service' Rollout on Housing Options Approach Rate and Working Age Homelessness Claim Rate within Scottish Local Authorities, by Rollout Length, to March 2019.



Notes: Point estimates are derived from the regression models in Table 7.4. Vertical bars represent 95% confidence intervals. Coefficients become slightly less precise in quarters further post rollout due to decreasing sample size of local authorities (this is conveyed by the widening confidence intervals).

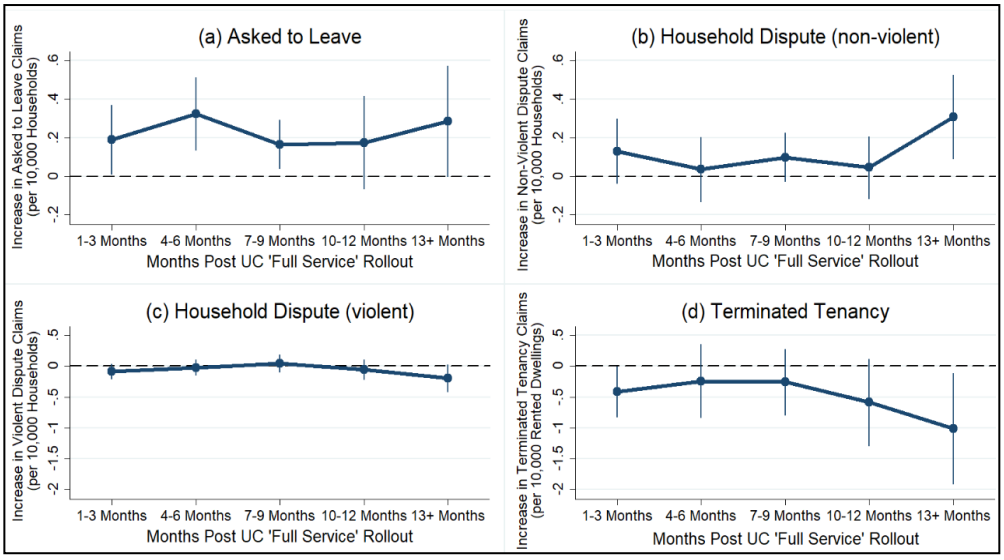
The regression models in Tables 7.5 and 7.6 examine any variation in the impact of UC 'Full Service' rollout (by rollout length) on homelessness claim rates between different reasons for the claim and different prior circumstances of applicants. The results in Table 7.5 (visualised in Figure 7.4) find no overall significant relationship between 'Full Service' rollout and claims arising from household disputes or terminated tenancies. The results do however further highlight a positive association between UC 'Full Service' rollout and the homelessness claim rate amongst those citing "asked to leave" as the main reason. Yet, similar to the relationship observed for the working age homelessness claim rate, the impact does not appear to increase where 'Full Service' has been rolled out for longer. Therefore, there is no clear increase in the impact of 'Full Service' rollout over time despite more people moving onto UC. Similarly, Table 7.6 (visualised in Figure 7.5) suggests that there was a small positive relationship between 'Full Service' and homelessness claims from those previously staying in family home or with relatives, but this did not clearly increase in the months post rollout. There was also no clear relationship between 'Full Service' rollout and homelessness claims from those from any other prior circumstances.

Table 7.5. Relationship between UC 'Full Service' rollout and Homelessness Claim Rates Across 29 Scottish Local Authorities, by rollout length and main cited reason for application, January 2015 – March 2019.

	(1) Asked to Leave Rate	(2) Non-Violent Household Dispute Rate	(3) Violent Household Dispute Rate	(4) Terminated Tenancy Rate
UC 'Full Service' Rolled Out *[No]*				
Yes [1-3 Months post]	0.19*	0.13	-0.08	-0.41+
	(0.09)	(0.08)	(0.05)	(0.21)
Yes [4-6 Months post]	0.32**	0.04	-0.02	-0.24
	(0.09)	(0.08	(0.06)	(0.30)
Yes [7-9 Months post]	0.17*	0.10	0.04	-0.26
	(0.12)	(0.06)	(0.07)	(0.27)
Yes [10-12 Months post]	0.18	0.05	-0.06	-0.58
	(0.12)	(0.08)	(0.08)	(0.35)
Yes [13+ Months Post]	0.28+	0.31**	-0.19+	-1.01*
	(0.14)	(0.10)	(0.11)	(0.45)
Per 1% rise in **Model Based Unemployment Rate**	0.04	0.06*	0.01	-0.05
	(0.03)	(0.02)	(0.01)	(0.10)
Per £10 increase in **10th Percentile Weekly Wages**	0.07*	-0.01	-0.02	0.06
	(0.03)	(0.02)	(0.02)	(0.01)
Per 100 households increase in **Rate of Single Adult Households**	0.03*	0.01	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Per 100 households increase in **Rate of Single Parent Households**	-0.003	0.06**	0.03	-0.01
	(0.02)	(0.02)	(0.01)	(0.05)
Local Authority Months	1479	1479	1479	1479
R^2	0.176	0.150	0.111	0.131

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (monthly) time fixed effects. All rates are per 10,000 households in the local authority, except the '*Terminated Tenancy Rate*', which is per 10,000 rented dwellings. 10th percentile weekly wages includes both part-time and full-time work. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

Figure 7.4. Impact of UC 'Full Service' Rollout on Homelessness Claim Rate within Scottish Local Authorities, by Main Cited Reason for Claims and Rollout Length (January 2015 – March 2019).



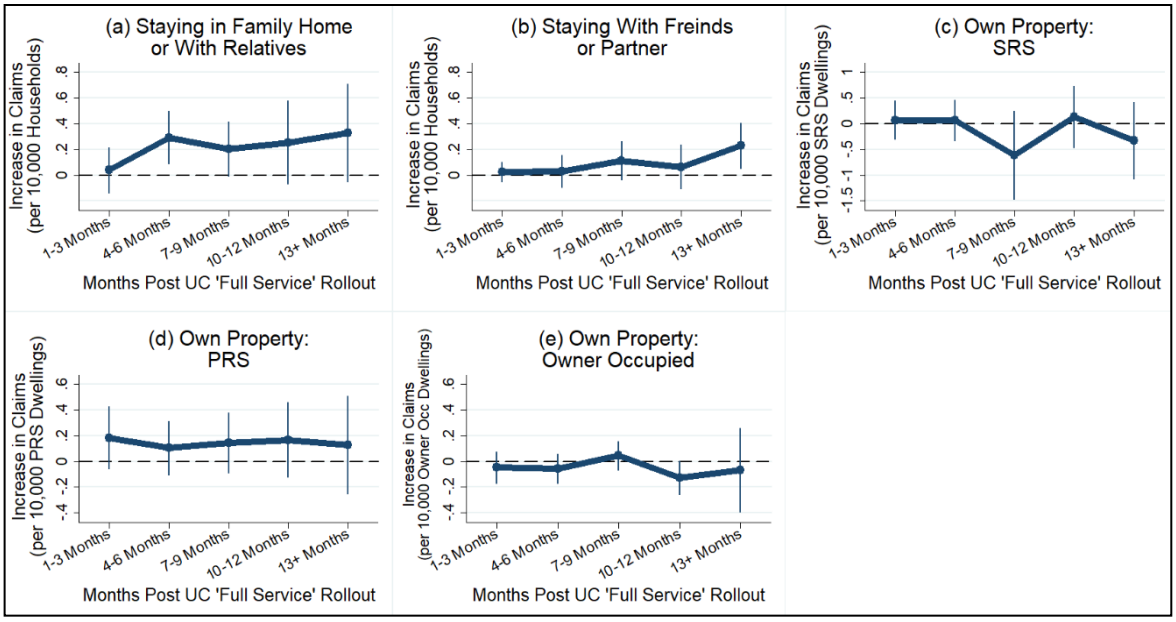
Notes: Point estimates are derived from the regression models in Table 6. Vertical bars represent 95% confidence intervals. Coefficients become slightly less precise (exemplified by widening confidence intervals) in quarters further post rollout due to decreasing sample size of local authorities.

Table 7.6. Relationship between UC 'Full Service' Rollout and Homelessness Claim Rates Across 29 Scottish Local Authorities, by Rollout Length and Prior Circumstances of Applicant, January 2015 – March 2019.

	(1) Staying in Family Home or with Relatives Rate	(2) Staying with Friends/Partners Rate	(3) SRS Property Rate	(4) PRS Property Rate	(5) Owner Occupied Property Rate
UC 'Full Service' Rolled Out					
[No]					
Yes [1-3 Months post]	0.04	0.02	0.07	0.18	-0.04
	(0.09)	(0.04)	(0.19)	(0.12)	(0.06)
Yes [4-6 Months post]	0.29**	0.03	0.06	0.10	-0.06
	(0.10)	(0.06)	(0.19)	(0.11)	(0.06)
Yes [7-9 Months post]	0.20+	0.11	-0.61	0.15	0.05
	(0.11)	(0.07)	(0.43)	(0.12)	(0.06)
Yes [10-12 Months post]	0.26	0.07	0.13	0.17	-0.13+
	(0.16)	(0.08)	(0.30)	(0.15)	(0.06)
Yes [13+ Months Post]	0.33+	0.23*	-0.32	0.12	-0.07
	(0.19)	(0.09)	(0.37)	(0.19)	(0.16)
Per 1% rise in **Model Based Unemployment Rate**	0.08**	-0.02	-0.13	0.02	0.02
	(0.03)	(0.02)	(0.09)	(0.06)	(0.02)
Per £10 increase in **10th Percentile Weekly Wages**	-0.0001	0.02	-0.01	-0.05	0.03
	(0.04)	(0.02)	(0.19)	(0.05)	(0.02)
Per 100 households increase in **Rate of Single Adult Households**	0.02*	0.01	-0.01	0.01	0.000
	(0.01)	(0.01)	(0.04)	(0.01)	(0.01)
Per 100 households increase in **Rate of Single Parent Households**	-0.002	0.02	0.21**	0.000	0.01
	(0.02)	(0.02)	(0.06	(0.03)	(0.01)
Local Authority Months	1479	1479	1479	1479	1479
R^2	0.168	0.142	0.101	0.204	0.056

Notes: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (monthly) time fixed effects. All rates are per 10,000 households in the local authority, except for the '*SRS Property Rate*', '*PRS Property Rate', and* '*Owner Occupied Property Rate*' which are per 10,000 rented dwellings in the given housing tenure. 10th percentile weekly wages includes both part-time and full-time work. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

Figure 7.5. Impact of UC 'Full Service' Rollout on Homelessness Claim Rate within Scottish Local Authorities, by Prior Circumstance of Applicant and Rollout Length (January 2015 – March 2019).



Notes: Point estimates are derived from the regression models in Table 7. Vertical bars represent 95% confidence intervals. Coefficients become slightly less precise (exemplified by widening confidence intervals) in quarters further post rollout due to decreasing sample size of local authorities.

7.5 Conclusion

This chapter has examined the impact of Universal Credit 'Full Service' rollout on Housing Options approach rates and homelessness claim rates (broken down by overall working age claims and claims for different reasons and from different prior circumstances) within Scottish local authorities. The findings suggest that a clear positive relationship exists between 'Full Service' rollout and Housing Options approach rates. Overall, after accounting for local authority and time fixed effects, unemployment rates, wages and rates of single adult and single parent households, 'Full Service' rollout was associated with an increase of 1.32 Housing Options approaches (per 10,000 households), on average within Scottish local authorities between April 2014 and March 2019. This corresponds, approximately, to an 8.1% increase on mean rates observed in the pre rollout period (i.e. September 2015 – February 2016). This impact of UC rollout on Housing Options approach rates tended to increase where it had been rolled out for longer and thus reached more claimants. Where 'Full Service' had been rolled out for 13+ months it was associated with an increase of 3.65 approaches (per 10,000 households) – this corresponds to a 22.5% increase on pre rollout rates.

The findings outlined in this chapter also suggest that UC 'Full Service' rollout is associated with a small increase in working age homelessness claim rates, but this impact was less clear than that seen for Housing Options approach rates. Overall, after accounting for local authority and time fixed effects, unemployment rates, wages and rates of single adult and single parent households, 'Full Service' rollout was associated with an additional 0.45 working age homelessness claims (per 10,000 working age households), on average within Scottish local authorities between January 2015 and March 2019. This increase was small, yet statistically significant, and corresponds approximately to a 3.0% increase on the mean rates observed in the pre rollout period. Unlike Housing Options, the relationship did not strengthen the longer 'Full Service' had been rolled out, meaning that the overall relationship was less clear, as claim rates did not increase over time despite more people moved onto UC. However, the fact that a larger and clearer impact was observed upon Housing Options approaches than on homelessness claims is logical given that a Housing Options approach is likely to occur prior to a homelessness claim (and thus more likely to be picked up in the data) as it is in itself designed to prevent an actual homelessness claim from having to be made.

The final finding outlined in this chapter is that disaggregation of the data on homelessness claims suggests that the small increase in claims associated with 'Full Service' rollout mostly arises from those previously staying in their family home/with relatives and becoming homeless due to being "asked to leave" by the 'host' of the household. Many of these 'hosts' will be the parent(s) of the homelessness applicant (Fitzpatrick, Pawson, Bramley *et al.*, 2019, p. 72), which suggests that UC rollout may be putting a strain on household finances and in turn straining relationships and the affordability of looking after all household members with payments. However, there was no significant relationship found between 'Full Service' rollout and other cited reasons for homelessness, i.e. household disputes or terminated tenancies. This may partially reflect some of the contrasting reasons for homelessness in Scotland compared to England – as noted in section 7.2.4., homelessness in Scotland most commonly occurs amongst those previously staying with family/friends/partners, whereas in England it most commonly occurs due to the ending of a private tenancy or rent arrears. Therefore, the tendency for UC rollout's small impact on homelessness claims to be "asked to leave" claims and not terminated tenancy claims may reflect Scotland's lower overall housing pressures than England – this has previously been highlighted as a contributing factor to welfare reform having less devastating homelessness impacts in Scotland than in England (Fitzpatrick, Pawson, Bramley *et al.*, 2019, p. 90).

As with the analysis in Chapters 5 and 6, the key strength of the analysis here is that it was able to exploit cross-area variation in the timing of UC rollout in order to examine its impact. However, unlike in Chapters 5 and 6, it was not possible to conduct a falsification test to improve causal inference by checking the specificity of the results. This is because no suitable non-equivalent outcome variable was identified for which data was available on. There are also several further limitations to the study.

Firstly, in the homelessness and Housing Options data used in the analysis all figures were round to the nearest five claims/approaches – this reduces the accuracy of detecting small changes month to month. Secondly, as the analysis uses local authority level data rather than individual level data, there is (as in Chapters 5 and 6) potential for ecological fallacy. Thirdly, whilst the UC variables and homelessness variables are based on monthly data, the control variables are based on annual data converted into monthly estimates using linear interpolation (or in case of unemployment rates by taking previous 12 months average). This means that they can control for long-term trends but are not able to accurately account for month-to-month variation and seasonal fluctuations. This reduces their reliability and means

their coefficients should be interpreted with caution. Furthermore, current data availability meant that it was not possible to control for any other homelessness determinants. However, for this to bias the results the omitted homelessness determinants would need to be correlated with both the timing of UC 'Full Service' rollout and monthly rates of homelessness claims/Housing Options approaches – there are no known reasons why this may be the case. Finally, whilst the inclusion of local authority and time fixed effects controls for unobserved factors that: (a) vary between local authorities but not over time, and (b) vary over time but not between local authorities, they cannot control for unobserved factors that vary both between local authorities and over time (Stock and Watson, 2015). In the analysis here, this means that the models are unable to control for any changes in procedures in the use of Housing Options over time within local authorities during the analysis period. This can be an issue given that it is up to local authorities themselves to decide what constitutes a recorded Housing Options approach. Fixed effects can control for the well documented variation in usage of Housing Options between different local authorities (see Fitzpatrick, Pawson, Bramley *et al.*, 2019, p. 67; Scottish Government, 2019a, p. 8), but not if certain local authorities reduced/increased their propensity to use Housing Options over time within the analysis period.

Chapter 8. Empirical Study 4: Impact of Universal Credit Rollout on Household Financial Problems

8.1 Introduction

This chapter examines the impact of Universal Credit rollout on household financial problems (i.e. the first stage of housing insecurity as set out in Chapter 3), including self-reported difficulties managing financially and self-reported problems paying for housing, bills and council tax. In contrast to the fixed effects panel design which was employed in Chapters 5-7 and which solely used local authority level data, the analysis here uses a difference-in-differences (DiD) research design, making use of individual and household level data from England, Scotland and Wales from the '*Understanding Society*' survey (aka the UK Household Longitudinal Study or UKHLS). The special licence version of UKHLS includes information on respondent's subjective financial situation, their ability to meet payments for housing/bills/council tax, their employment status, their local authority of residence and characteristics such as sex, age, marital status and education. The analysis here links this individual/household level data to local authority level data on the timing of UC rollout in UKHLS respondent's area of residence in order to address the following research questions:

1. Has UC rollout had an adverse impact on self-reported difficulties managing financially amongst unemployed individuals?

2. Has UC rollout had an adverse impact on the ability to meet housing payments amongst unemployed individuals?

3. Has UC rollout had an adverse impact on the ability to meet household bill payments amongst unemployed individuals?

4. Has UC rollout had an adverse impact on the ability to meet council tax payments amongst unemployed individuals?

To address these research questions, the DiD analysis of this chapter splits UKHLS respondents into two groups: (a) a 'treatment group', made up of unemployed individuals, and (b) a 'control group', made up of working-age individuals who were not unemployed. The treatment group focusses on unemployed individuals because they were the first group to become exposed to UC during the 'Live Service' phase of rollout, which typically affected those who would previously have claimed income related Jobseekers Allowance (Department for Work and Pensions, 2020). Therefore, this lends itself to a natural experiment research design as newly unemployed individuals between 2013 and 2018 either went onto UC or Jobseekers Allowance depending on where they made their claim (Craig and Katikireddi, 2020, p. E131). This type of analysis was not possible whilst using local authority level data in Chapters 5-7, as only individual-level data allows this type of disaggregation based on employment status.

The DiD analysis outlined in this chapter is split into two parts. In part 1 of the analysis, the immediate impact of UC 'Live Service' – i.e. in its first 12 months of rollout – is examined. Here, a design similar to the standard two-group and two-time period DiD design (see Wing, Simon and Bello-Gomez, 2018) is employed. Data from UKHLS waves in the 12 months immediately before and after UC 'Live Service' rollout in a given respondent's local authority of residence is used. Outcomes relating to subjective finances and problems paying for housing/bills/council tax are compared before and after the rollout date, measuring the difference for the treatment group (unemployed in both time periods and thus more likely to become exposed to UC) minus the difference for the control group (not unemployed in both time periods and thus unlikely to have become exposed to UC).

In part 2 of the analysis, the impact of UC rollout is examined using a more longitudinal approach, by employing a DiD research design similar to that used by Wickham, Bentley, Rose *et al.* (2020) in their study of the effects of UC on mental health. Here, data from waves 1-9 of UKHLS (i.e. 2009-2019) are used, and in each wave respondents are assigned to the treatment group if they are unemployed in the given wave, or the control group if they are not unemployed in the given wave. Again, the DiD analysis here compares changes in outcomes relating to subjective finances and problems paying for housing/bills/council tax before and after UC rollout in each respondent's local authority of residence.

This chapter begins by setting out the data, variables and difference-in-differences research design and methodology used in the chapter's empirical analysis. It will then go on to set out the results of part 1 and part 2 of the analysis, before ending with a conclusion which summarises the research findings and the limitations of the analysis.

8.2 Data, Variables and Methods

8.2.1 Data and Participants

UKHLS is a nationally representative longitudinal survey of around 40,000 households in the UK, and was created in order to build on and expand the longstanding British Household Panel Survey (BHPS), which ran from 1991-2009 with around 10,000 households (see Understanding Society, 2020). UKHLS began with its first wave of data collection in 2009-11, and the same households are visited each year in order to track changes in circumstances (Ibid). All data is collected through either a face-to-face interview in the participant's own home or a self-completed online survey. Up to the start of 2020, there had been nine waves of data collection, with Wave 9's data being collected in 2017-19. The survey includes data at both the household level, which is collected via the survey's 'household questionnaire', and individual level, which is collected via the survey's 'individual questionnaire' (Understanding Society, 2018).

Whilst part 1 of the analysis in this chapter uses data from UKHLS waves immediately before and after UC was first introduced in respondent's area of residence, part 2 of the analysis takes a more longitudinal approach, as discussed, and uses data from all nine waves of UKHLS. Both parts of the analysis include UKHLS respondents who were of working-age and who had data available on their employment status, their local authority of residence (available via the special licence version of UKHLS only), the various outcome variables of interest (outlined below in section 8.2.2.), and the various control variables (outlined below in section 8.2.4.). Information on each respondent's local authority of residence was used to link the UKHLS data with data from the UC rollout schedule in order to ascertain whether UC had rolled out yet in their area at the date of each interview. Those living in Northern Ireland were excluded from the analysis as data was not available on the UC rollout schedule here.

8.2.2 Household Financial Problems Data (Outcome Variables)

The data from UKHLS was recoded in order to create five separate binary outcome variables for the analysis, each of which indicates a household financial problem.

The first two outcome variables come from the UKHLS individual questionnaire, and provide a subjective measure of respondents' current financial situation and views about their financial future. They are: (1) '*Not Currently Managing Financially*' and (2) '*Think Will be Worse off Financially in Future*'. '*Not Currently Managing Financially*' uses the survey question "How well would you say you yourself are managing financially these days?" and classifies those who responded "Finding it quite difficult" or "Finding it very difficult" as not managing (coded 0 = managing or just about getting by, 1 = not managing). Similarly, '*Think Will be Worse off Financially in Future*' uses the survey question "Looking ahead, how do you think you will be financially a year from now?" and classifies those who responded as "Worse off than now" as thinking they will be worse off in future (coded 0 = better off/same and, 1 = worse off).

The remaining outcome variables come from the UKHLS household questionnaire. They are: (3) '*Problems Paying for Housing*', (4) '*Problems Paying Bills*', and (5) '*Problems Paying Council Tax*'. Those who responded "Yes" to the survey question "In the last twelve months, have you ever found yourself behind with your rent/mortgage?" are classified as having problems paying for housing (coded 0 = no, 1 = yes). Those who responded "Behind with all bills" or "Behind with some bills" to the survey question "Are you up to date with all your household bills such as electricity, gas, water rates, telephone and other bills or are you behind with any of them?" are classified as having problems paying bills (coded 0 = up to date with all bills, 1 = behind with some/all). Finally, those who responded "yes" to the survey question "In the last twelve months, have you ever found yourself behind with paying Council Tax?" are classified as having problems paying council tax (coded 0 = no, 1 = yes).

8.2.3 Universal Credit Rollout Variable and Assignment to Treatment and Control Groups

The UKHLS data on each respondent's local authority of residence (available via the special

licence version) was used to link the survey data with data from the official UC rollout schedule (available from: UK Government, 2015a; UK Government, 2015b and set out in Appendix 1 (for England), Appendix 2 (for Scotland) and Appendix 3 (for Wales)). A '*UC rollout by interview date*' variable was then created to indicate whether UC had rolled out yet by the date of each interview in the dataset (coded 0 = no, 1 = yes).

Part 1 of the analysis, as already explained, aims to examine the immediate impact of UC 'Live Service' rollout on household financial problems amongst unemployed individuals and their households, by using a DiD design similar to the standard two-group and two-time period approach (see Wing, Simon and Bello-Gomez, 2018). Here, time period one is the year (i.e. UKHLS wave) immediately before 'Live Service' rolled out in each respondents local authority of residence. This is based on respondents UKHLS interview date, and where more than one wave of data occurred in the year prior to rollout, the wave closest to rollout was taken. Similarly, time period two is the year (i.e. UKHLS wave) immediately after 'Live Service' rollout in each respondents local authority of residence, with this again being based on the interview date and the wave closer to rollout being taken if more than one wave of data occurred within a year post rollout. Respondents were assigned into either the treatment group or control group in each wave of the analysis based on their response to the survey question "Which of these best describes your current employment situation?". Respondents were included in the treatment group if they self-reported being unemployed in both time period one and time period two. Conversely, they were included in the control group if they were of working age and self-reported not being unemployed (including those who were employed, self-employed, on maternity leave, full-time students, doing an apprenticeship, doing an apprenticeship, or something else) in both time period one and time period two.

Part 2 of the analysis, as also already explained, takes a more longitudinal approach, using data from wave 1 (2009-11) to wave 9 (2017-19) of UKHLS. Here, in a similar to the DiD research design employed by Wickham, Bentley, Rose *et al.* (2020)'s paper examining UC's mental health impact, the analysis assigns respondents to the treatment group in a given wave if they self-reported being unemployed in that wave, or to the control group if they were of working age and self-reported not being unemployed in that wave. This means that – unlike in part 1 of the analysis – respondents could move from the treatment group to the control group or vice versa if their employment status were to change between waves. As in the aforementioned Wickham, Bentley, Rose *et al.* (2020) paper, the analysis here focuses on

unemployed people for the treatment group because they were the first group exposed to UC and more likely to have become exposed in the analysis period. Therefore, the analysis here again focusses on the timing of 'Live Service' rollout affecting mainly single unemployed people with no children. However, 'Full Service' would also have rolled out in the latter waves of the analysis, meaning that a wider range of groups would also have become exposed to UC, including unemployed people in couples with children and some people in work on low incomes.

8.2.4 Control Variables

In the UK, household difficulties managing financially can vary based on the demographics of the household. In particular, research into experiences of difficulties managing financially by the Financial Conduct Authority suggests that younger people are more likely to be in financial difficulties, as are women due to being more likely to be a single parent (see Financial Conduct Authority, 2017; Stack and Meredith, 2018; Gladstone, 2020). In addition, the link between educational attainment and labour market outcomes (and income) are well documented (e.g. see Furia, Castagna, Mattoscio *et al.*, 2010; Ionescu, 2012), and research into homelessness specifically indicates that the financial security that comes with having a partner or spouse (and thus income coming from multiple adults) is a key provider of housing security (Bramley and Fitzpatrick, 2018).

Consequently, in order to account for these potential confounders, four control variables were included in the analysis. These are: (1) '*Sex*' (coded 1 = male, 2 = female), (2) '*Age Band*' (coded 1 = 16-24, 2 = 25-34, 3 = 35-44, 4 = 45-54, 5 = 55-64), (3), '*Marital Status*' (coded 1 = married/civil partner, 2 = living as couple, 3 = widow/divorced/separated, 4 = never married), and (4) '*Education*', which shows respondents highest qualification (coded 1 = degree/equivalent, 2 = A level/GCSE/equivalent, 3 = other/no qualifications).

8.2.5 Difference-in-Differences Analysis

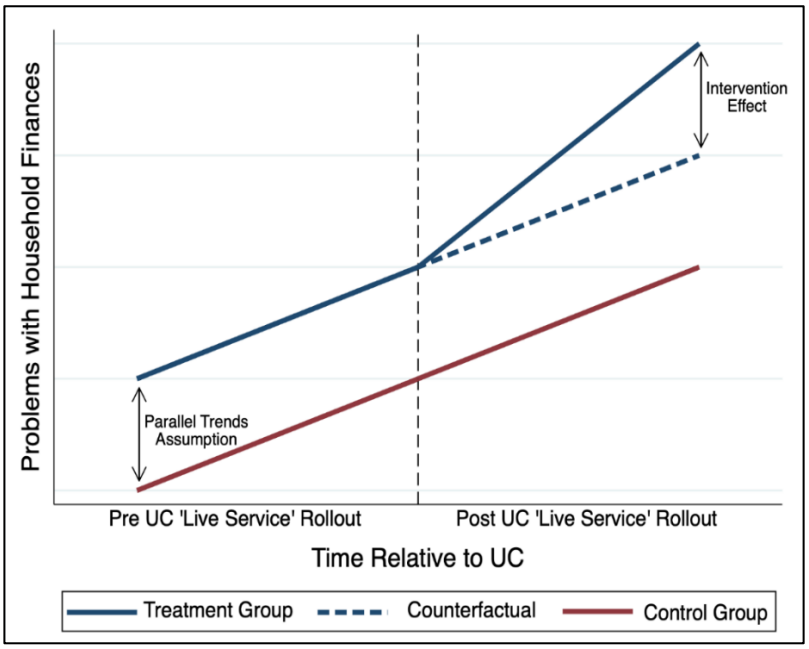
DiD analysis is a quasi-experimental econometric technique that uses longitudinal data on a treatment group and a control group as a means of judging the counterfactual and thus estimating a causal effect (Columbia Public Health, 2019). DiD is typically used to estimate

the effect of a policy intervention by assigning individuals into the treatment group if they are exposed to the intervention or into the control group if they are not exposed to the intervention. Changes in outcomes over time ('differences') are then compared between the treatment group (Difference 1 or D1) and the control group (Difference 2 or D2). This process is summarised – in the current context of estimating the intervention effect of UC rollout on household finances/ability to meet payments for housing, bills and council tax – via Equation 8.1 and Figure 8.1, which are below.

$$\begin{aligned} UC\ Intervention\ Effect &= \boldsymbol{D1} - \boldsymbol{D2} \\ &= \left(Financial\ Problems_{Post,Unemp} - \ Financial\ Problems_{Pre,Unemp}\right) \\ &- \left(Financial\ Problems_{Post,Non-Unemp}\right. \\ &- \ \left. Financial\ Problems_{Pre,Non-\ Unemp}\right) \end{aligned} \tag{8.1}$$

Where *Financial Problems* refers to the various outcome variables outlined in section 8.2.2 which indicate household financial problems. *Post* refers to interviews with respondents conducted after UC 'Live Service' had rolled out in the area in which they live. Conversely, *Pre* refers to respondents whose UKHLS interview date occurred before UC 'Live Service' had rolled out in the area in which they live. *Unemp* refers to those who self-reported being unemployed, whilst $Non - Unemp$ refers to those of working age who self-reported not being unemployed.

Figure 8.1. Graphical Explanation of Difference-in-Differences Research Design and Parallel Trends Assumption.



The key assumption of DiD analysis is the 'parallel trends' assumption. This assumption requires that, in the absence of the treatment, the difference in outcomes between the treatment group and the control group is constant over time (Murnane and Willett, 2011; Craig, Katikireddi, Leyland *et al.*, 2017; Columbia Public Health, 2019). In other words, this means that the treatment and control group would have moved in parallel through the time period being investigated had the intervention not occurred. Whilst, in reality, this cannot be fully proven, one way to support the assumption is to show that outcomes were moving in parallel before the intervention. This can be examined via a visual inspection of trends before and after the intervention, as conveyed in Figure 8.1. If the parallel trends assumption is violated, this will lead to a biased estimate of the intervention effect (Columbia Public Health, 2019).

As discussed by Craig and Katikireddi (2020), the staggered nature of the UC rollout schedule lends itself to natural experimental evaluation, as it meant that individuals that were newly unemployed between 2013 and 2018 either went onto UC or onto Jobseekers Allowance depending upon their area of residence. In general, there are a wide range of natural experimental approaches to evaluation other than DiD that can be used to analyse policy reforms – e.g. instrumental variables, regression discontinuity, and interrupted time-series – and decisions about which approach to use should be based on the specific features of policy intervention of interest (Craig, Katikireddi, Leyland *et al.*, 2017). For the analysis here, DiD was selected as the most appropriate design as: (a) a DiD design can take advantage of the fact that UC rolled out in different areas at different times in order to analyse its impact, and (b) DiD design can take advantage of the fact that UC rolled out to unemployed individuals first by using unemployed people (more likely to become exposed to UC) as the treatment group and not unemployed (less likely to become exposed to UC) as the control group.

The analysis here estimates the intervention effect of UC rollout by running multivariable logistic regression models using the UKHLS data outlined in section 8.2.1 and including an interaction term between the '*UC rollout by interview date*' set out in section 8.2.2 and the variable indicating whether respondents are in the treatment group or control group.

For part 1 of the analysis, which attempts to measure the immediate impact of UC 'Live Service' rollout, this is as follows:

$$
\begin{aligned}
&Financial\ Problems_{it} \\
&\quad = \beta_0 + \beta_1 Wave_t + \beta_2 Sex_{it} + \beta_3 Age\ Band_{it} + \beta_4 Mar\ Stat_{it} \\
&\quad + \beta_5 Education_{it} + \beta_6 Unemp_i + \beta_7 Post\ UC_t + \beta_8 Unemp_i * Post\ UC_t \\
&\quad + \varepsilon_{it}
\end{aligned} \tag{8.2}
$$

Where the data comes from the UKHLS waves immediately before and after UC 'Live Service' rolled out in each respondent's area of interest, and i is the individual, t is the year and ε is the error term. As in Equation 8.1, $Financial\ Problems$ refers to the various outcome variables outlined in section 8.2.2. Sex, $Age\ Band$, $Mar\ Stat$ and $Education$ represents the control variables outlined in section 8.2.4. $Unemp$ indicates whether respondents were unemployed in both waves (and thus in treatment group), or not unemployed in both waves (and thus in control group). This does not vary over time, as those who transitioned from unemployment to employment or vice versa were excluded from the analysis. $Post\ UC$ represents the '*UC rollout by interview date*' variable. By interacting $Unemp$ and $Post\ UC$ (i.e. $Unemp * Post\ UC$) this gives the difference-in-difference estimator of the immediate (i.e. in first 12 months of rollout) intervention effect of UC rollout.

For part 2 of the analysis, which attempts to measure the UC intervention effect between wave 1 (2009-11) and wave 9 (2017-19) of UKHLS, the DiD modelling is as follows:

$$
\begin{aligned}
&Financial\ Problems_{it} \\
&\quad = \beta_0 + \beta_1 Wave_t + \beta_2 Sex_{it} + \beta_3 Age\ Band_{it} + \beta_4 Mar\ Stat_{it} \\
&\quad + \beta_5 Education_{it} + \beta_6 Unemp_{it} + \beta_7 Post\ UC_{it} + \beta_8 Unemp_i * Post\ UC_t \\
&\quad + \varepsilon_{it}
\end{aligned} \tag{8.3}
$$

Where data comes from all nine UKHLS waves. Here, $Unemp$ indicates whether respondents are unemployed or not in a given wave whilst $Post\ UC$ indicates whether UC 'Live Service' has rolled out yet in a given wave, and hence can vary both over time and between individuals. All other variables are the same as those in Equation 8.2. By interacting $Unemp$ and $Post\ UC$ (i.e. $Unemp * Post\ UC$) this gives the difference-in-difference estimator of the intervention effect of UC rollout between wave 1 and wave 9 of UKHLS.

In order to account for attrition and non-response within UKHLS, the survey's longitudinal survey weight was applied when running the logistic regression models. This is in accordance with the weighting guidance from Understanding Society (2020b), which states

that the survey's longitudinal weight should be applied when using multiple waves of UKHLS data. The guidance also sets out that it is the longitudinal weight from the last wave of the analysis that should be applied (Understanding Society, 2021), meaning that it was the wave 9 longitudinal weight that was applied.

8.3 Results

8.3.1 Empirical Analysis Part 1: Two-Group and Two-Time Period Study on Immediate Impact of UC 'Live Service' Rollout

As already set out above, part 1 of the empirical analysis of this chapter examines the immediate impact of UC 'Live Service' rollout using data from the UKHLS waves that occur immediately before and after its rollout, employing a design similar to the standard two-group and two-time period DiD. Table 8.1 provides the unweighted baseline characteristics for the treatment group (i.e. those unemployed in both time periods) and the control group (i.e. those not unemployed in both time periods). It highlights that those who were in the treatment group were more likely to be male, young, unmarried and hold fewer formal qualifications when compared to those who were in the control group. This highlights the importance of controlling for these characteristics in the DiD analysis. In terms of the outcome variables indicating financial problems, those in the treatment group were more likely to not be managing financially and to be in a household with problems paying for housing, problems paying for bills, and problems paying for council tax. Yet, it is important to note that this divergence in itself does not introduce any bias to the analysis.

Table 8.2 shows the results of the logistic regression models for the five outcome variables indicating financial problems. Models report odds ratios and are run with and without control variables. With regards to control variables, the results suggest that – in line with the literature discussed in section 8.2.4. – financial problems tended to be more prevalent amongst those who were unmarried, female, held fewer formal qualifications. Problems paying for housing, bills and council tax (although not subjective financial difficulties) tended to be more prevalent amongst younger people. In terms of the DiD estimator (Unemployed*UC rollout), no significant UC intervention effect was found for any of the outcome variables. This suggests that there was no immediate impact (in first 12 months of

rollout) of UC 'Live Service' rollout on financial problems, although it is important to note that this finding is subject to several limitations (these will be discussed in section 8.4).

Table 8.1. Unweighted Baseline Characteristics of Treatment and Control Group in Analysis Part 1, pre UC 'Live Service' Rollout.

	Treatment Group	Control Group
Sex		
Male	312 (58.7%)	8,110 (49.3%)
Female	220 (41.3%)	8336 (50.7%)
Age Band		
16-24	126 (23.7%)	1,347 (8.2%)
25-34	109 (20.5%)	3,141 (19.1%)
35-44	109 (20.5%)	4,389 (26.7%)
45-54	110 (20.7%)	4,861 (29.6%)
55-64	78 (14.7%)	2,707 (16.5%)
Marital Status		
Married/Civil Partner	137 (25.8%)	9,396 (57.2%)
Living as Couple	70 (13.2%)	2,608 (15.9%)
Widow/Divorced/Separated	48 (9.0%)	1,301 (7.9%)
Never Married	276 (52.0%)	3,109 (18.9%)
Education		
Degree/Equivalent	104 (20.2%)	7,797 (48.2%)
A Level/GCSE/Equivalent	255 (49.4%)	6,870 (42.5%)
Other/No Qualifications	157 (30.4%)	1,516 (9.4%)
Not Currently Managing Financially		
Yes	169 (34.4%)	931 (6.0%)
No	322 (65.6%)	14,468 (94.0%)
Think Will be Worse off Financially in Future		
Yes	57 (12.5%)	1,332 (8.8%)
No	398 (87.5%%)	13,799 (91.2%)
Problems Paying for Housing		
Yes	116 (26.9%)	1,256 (9.7%)
No	315 (73.1%)	11,698 (90.3%)
Problems Paying Bills		
Yes	106 (20.6%)	601 (3.7%)
No	409 (79.4%)	15,614 (96.3)%
Problems Paying Council Tax		
Yes	119 (23.5%)	1,029 (6.5%)
No	388 (76.5%)	14,813 (93.5%)

Notes: Data is taken from the UKHLS wave immediately prior to 'Live Service' rollout in each given respondent's area of residence. The treatment group includes those who were unemployed in time period 1 and time period 2 of the analysis, whilst the control group includes those who were not unemployed in both time periods. Percentages do not account for any missing values and may not add up to 100 due to rounding.

Table 8.2. Logistic Regression Models (Analysis Part 1)

	(1) Not Currently Managing Financially		(2) Think Will be Worse Off In Future		(3) Household: Problems Paying for Housing		(4) Household: Problems Paying Bills		(5) Household: Problems Paying Council Tax	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Sex										
[Male]										
Female		1.19+ (0.11)		1.11 (0.08)		1.07 (0.09)		1.22+ (0.14)		1.27** (0.11)
Age Band										
[16-24]										
25-34		1.13 (0.29)		1.51+ (0.35)		1.09 (0.20)		1.34 (0.31)		1.21 (0.22)
35-44		2.07** (0.49)		1.40 (0.31)		0.90 (0.16)		0.90 (0.21)		0.84 (0.15)
45-54		1.83* (0.43)		1.66* (0.37)		0.81 (0.14)		0.85 (0.20)		0.72+ (0.13)
55-64		1.28 (0.32)		1.98** (0.45)		0.82 (0.16)		0.51* (0.14)		0.56** (0.11)
Marital Status										
[Married/Civ Ptr]										
Living as Couple		1.33* (0.19)		0.79* (0.09)		1.37** (0.15)		1.78*** (0.28)		1.50** (0.18)
Widow/Separated		2.42*** (0.29)		1.30** (0.13)		1.47** (0.20)		2.15*** (0.44)		1.64** (0.24)
Never Married		1.89*** (0.26)		0.90 (0.11)		1.51*** (0.18)		1.86*** (0.31)		1.49** (0.18)
Education										
[Degree]										
A Level/GCSE		1.56*** (0.16)		0.85* (0.06)		1.45*** (0.13)		1.95*** (0.27)		1.76*** (0.18)
Other/No Quals		2.30*** (0.31)		0.86 (0.18)		2.56*** (0.32)		3.47*** (0.67)		2.72*** (0.37)
Unemployed	10.36***(2.23)	8.24*** (1.86)	1.65+ (0.45)	1.89* (0.52)	4.21***	2.98*** (0.69)	11.39***(2.54)	7.19*** (0.18)	6.07*** (1.25)	4.40*** (0.95)
UC Rollout	0.85 (0.08)	0.81+ (0.09)	1.07 (0.08)	1.07 (0.09)	0.95 (0.07)	1.07 (0.11)	1.01 (0.12)	0.93 (0.14)	0.91 (0.08)	1.02 (0.11)
Unemployed* UC Rollout	1.02 (0.30)	1.03 (0.32)	1.04 (0.40)	1.01 (0.39)	0.82 (0.26)	0.81 (0.26)	1.10 (0.34)	1.15 (0.38)	0.78 (0.24)	0.76 (0.23)
Observations	13,154	13,130	12,946	12,922	11,196	11,174	14,411	14,293	14,233	14,208

Notes: Results show odds ratios followed by robust standard errors in brackets. Default values are shown in square brackets. Unemployed*UC Rollout is the odds ratio for the DiD estimator. All (b) models additionally control for the wave in which the data was collected in order to account for any secular time trends in outcome variables. UKHLS longitudinal weights were applied to all models, which automatically restrict the models to a balanced panel. +p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001.

8.3.2 Empirical Analysis Part 2: Longitudinal Study on Impact of UC Rollout

As already set out, part 2 of the analysis takes a more longitudinal approach than part 1. It examines the impact of UC rollout by using data from waves 1 to 9 of UKHLS and splitting respondents into a treatment group and control group based or their employment status in a given wave. Specifically, the treatment group was made up of those who were unemployed and thus more likely to be exposed to UC, whilst the control group was made up of those who were not unemployed and thus less likely to be exposed to UC. Table 8.3 provides the unweighted baseline characteristics of the treatment group and control group prior to UC rollout, with the data being taken from wave 3 of UKHLS (i.e. the first wave prior to 'Live Service' rollout commencing).

As in part 1 of the analysis, those who were in the treatment group here were more likely to be male, younger, unmarried and hold fewer formal qualifications when compared to those who were in the control group. With regards to the outcome variables indicating financial problems, those in the treatment group were again more likely to not currently managing financially and again more likely to be in a household that is facing problems paying for housing, problems paying for bills, and problems paying for council tax.

However, this in itself does not introduce bias to the analysis as long as the parallel trends assumption is not violated.

Table 8.3. Unweighted Baseline Characteristics of Treatment and Control Group pre UC 'Live Service' Rollout in Analysis Part 2.

	Treatment Group	Control Group
Sex		
Male	1,406 (56.2%)	14,079 (45.7%)
Female	1,097 (43.8)	16,694 (54.3%)
Age Band		
16-24	731 (29.2%)	5,561 (18.1%)
25-34	515 (20.6%)	6,211 (20.2%)
35-44	482 (19.3%)	7,641 (24.8%)
45-54	490 (19.6%)	7,184 (23.4%)
55-64	285 (11.4%)	4,176 (13.65)
Marital Status		
Married/Civil Partner	681 (27.2%)	15,819 (51.4%)
Living as Couple	392 (15.66%)	4,469 (14.5%)
Widow/Divorced/Separated	263 (10.5%)	2,297 (7.5%)
Never Married	1,167 (46.6%)	8,184 (26.6%)
Education		
Degree/Equivalent	563 (22.7%)	12,004 (39.2%)
A Level/GCSE/Equivalent	1,252 (50.4%)	14,616 (47.8%)
Other/No Qualifications	671 (27.0%)	3,979 (13.0%)
Not Currently Managing Financially		
Yes	842 (36.9%)	3,149 (11.3%)
No	1,438 (63.1%)	24,682 (88.7%)
Think Will be Worse off Financially in Future		
Yes	294 (13.3%)	4,734 (17.2%)
No	1,913 (86.7%)	22,763 (82.8%)
Problems Paying for Housing		
Yes	454 (21.8%)	2,864 (11.5%%)
No	1,630 (78.2%)	22,103 (88.5%)
Problems Paying Bills		
Yes	477 (19.2%)	1,826 (6.0%)
No	2,005 (80.8%)	28,799 (94.0%)
Problems Paying Council Tax		
Yes	386 (16.6%)	2,509 (8.2%)
No	2,083 (84.4%)	27,956 (91.8%)

Notes: Data is taken from UKHLS wave 3 (i.e. first wave before UC 'Live Service' Rollout Began). The treatment group includes all those who were unemployed in this wave, and the control group includes those of working age who were not unemployed in this wave. Percentages may not add up to 100 due to rounding.

Unlike in DiD analysis in part 1 involving just two time periods, longitudinal studies with multiple data points pre policy intervention (as is the case here) can gain some insight into whether the parallel trends assumption is met via a visual inspection of trends in outcomes before the intervention. This is done in Figures 8.2 and 8.3. Time – based on the date of the interview – is adjusted in the graphs in order to be relative to UC rollout in each respondents' area of residence. The graphs suggest that outcomes are broadly parallel prior to UC rollout, with the treatment group having a higher proportion of those not managing financially and struggling to pay for housing/bills/council tax pre-UC rollout (as already shown in Table 3). However, the gap between treatment and control group does appear to widen slightly over time pre rollout with regards to problems paying for housing and council tax, which may introduce some bias, although they do not diverge dramatically from the parallel trends assumption.

Figure 8.2. Subjective Financial Situation before and after UC rollout in Treatment and Control Groups.

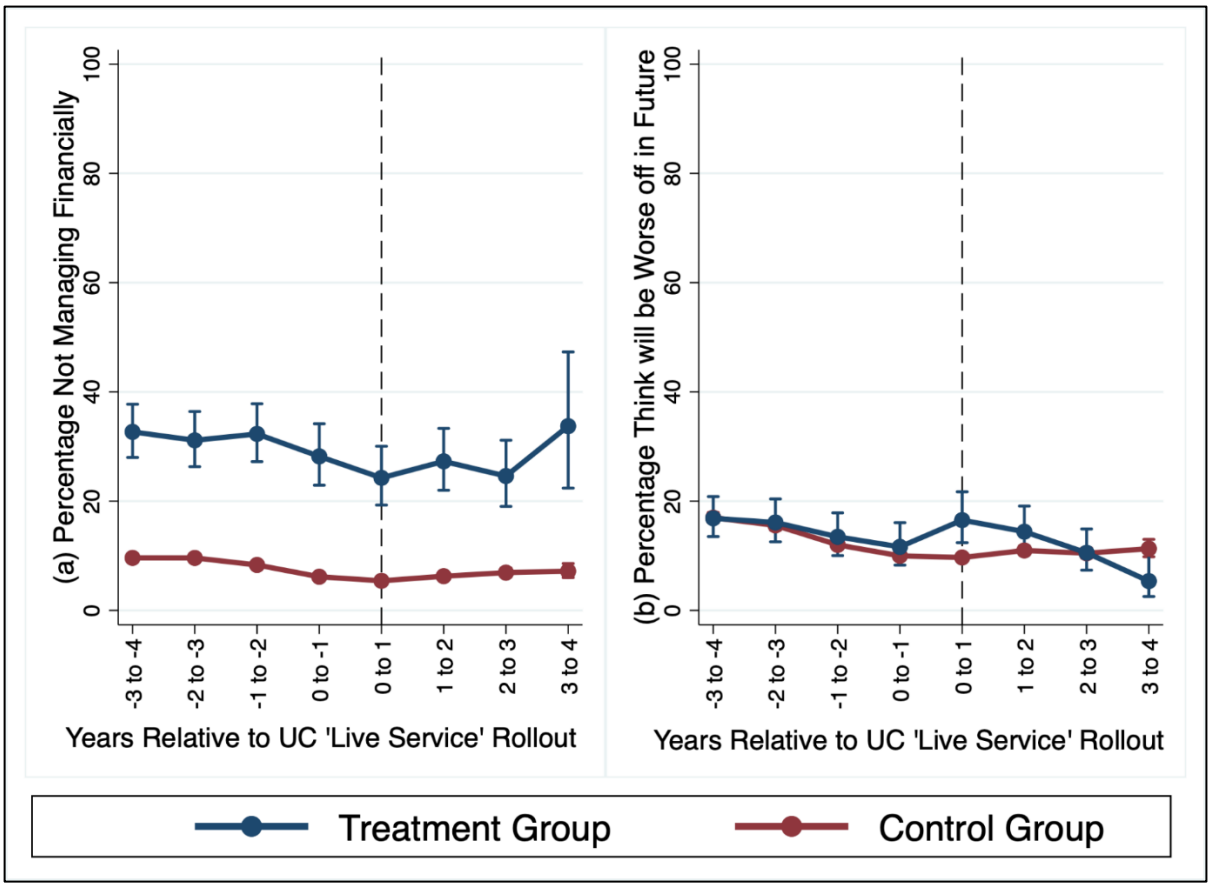


Figure 8.3. Household Problems Paying for Housing, Bills and Council Tax before and after UC rollout in Treatment and Control Groups.

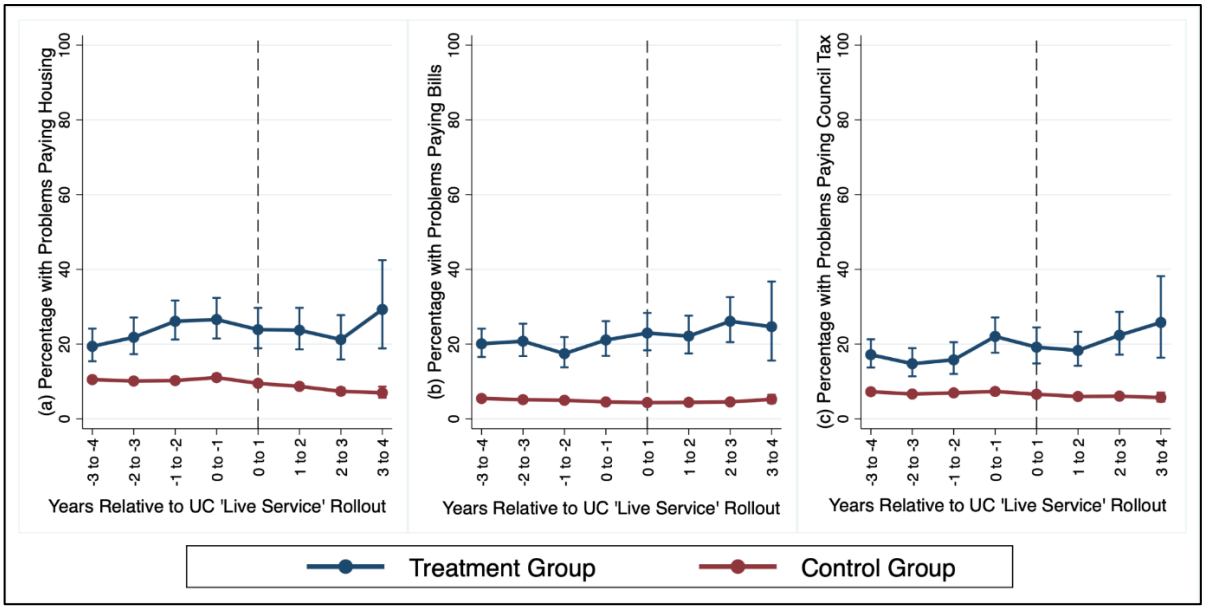


Table 8.4 shows the results of the logistic regression models. As in part 1 of the analysis, models are run with and without control variables. DiD estimates are shown in the form of an odds ratio in the main model, whilst the postestimation marginal test provides an estimate of the percentage point change in the prevalence of experiencing each financial problem following UC rollout. With regards to control variables, the results are broadly in line with the relationships expected based on discussion in section 8.3.4. – financial problems tended to be more prevalent amongst those who were female, unmarried, and who held fewer formal qualifications. In addition, household problems paying for housing, bills and council tax (although not subjective difficulty managing financially) were more common amongst young people.

In terms of the DiD estimates of the UC intervention effect, the results suggest that – after controlling for sex, age band, marital status and education – the prevalence of problems paying for housing, bills and council tax all increased (for the treatment group relative to the control group) following the introduction of UC in respondents' area of residence. Specifically, the prevalence of problems paying for housing increased by 3.9%, problems paying for bills by 3.1%, and problems paying council tax by 4.5% (again, this is prevalence in the treatment group relative to the control group). However, no significant (at 5% level) UC intervention effect was found with regards to subjective difficulties to manage financially. Therefore, the results suggest that UC rollout was associated with an increase in the prevalence of households being behind with payments for housing, bills and council tax,

but there was not a corresponding increase in the prevalence of unemployed individuals reporting that they were finding it difficult to manage financially. However, it should be noted that there are some important limitations associated with this analysis, which mean that causality cannot confidently be inferred – these limitations are discussed in section 8.4 below.

Table 8.4. Logistic Regression Models (Analysis Part 2)

	(1) Not Currently Managing Financially		(2) Think Will be Worse Off In Future		(3) Household: Problems Paying for Housing		(4) Household: Problems Paying Bills		(5) Household: Problems Paying Council Tax	
	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)	(a)	(b)
Sex										
[Male]										
Female		1.41*** (0.08)		1.15*** (0.04)		1.28*** (0.06)		1.56*** (0.11)		1.27*** (0.07)
Age Band										
[16-24]										
25-34		1.27+ (0.16)		1.15+ (0.10)		1.00 (0.10)		1.10 (0.13)		1.25+ (0.14)
35-44		1.91*** (0.24)		1.37*** (0.11)		0.98 (0.10)		1.01 (0.14)		1.16 (0.13)
45-54		1.62*** (0.20)		1.72*** (0.14)		0.93 (0.09)		0.74* (0.10)		0.89 (0.10)
55-64		1.05 (0.13)		2.19*** (0.19)		0.67*** (0.08)		0.35*** (0.05)		0.52*** (0.06)
Marital Status										
[Married/Civ Ptr]										
Living as Couple		1.35** (0.13)		0.80*** (0.05)		1.32** (0.11)		1.82*** (0.21)		1.57*** (0.14)
Widow/Separated		3.03*** (0.26)		1.24*** (0.07)		1.35** (0.12)		2.33*** (0.29)		1.61*** (0.15)
Never Married		1.61*** (0.16)		0.98 (0.06)		1.38*** (0.12)		1.81*** (0.21)		1.36** (0.13)
Education										
[Degree]										
A Level/GCSE		1.58*** (0.12)		0.94 (0.04)		1.69*** (0.11)		2.34*** (0.24)		1.82*** (0.13)
Other/No Quals		2.68*** (0.27)		0.96 (0.05)		2.68*** (0.23)		5.15*** (0.59)		2.85*** (0.26)
Unemployed	4.59*** (0.46)	4.20*** (0.42)	0.94 (0.07)	0.92 (0.08)	1.35** (0.13)	1.27* (0.12)	2.29*** (0.25)	2.18*** (0.23)	1.11 (0.12)	1.08 (0.11)
UC Rollout	0.54*** (0.03)	0.77* (0.08)	0.67*** (0.02)	1.01 (0.08)	0.72*** (0.03)	0.96 (0.09)	0.74*** (0.05)	0.79* (0.09)	0.74*** (0.04)	0.92 (0.09)
Unemployed* UC Rollout	1.06 (0.17)	1.12 (0.18)	1.37+ (0.23)	1.38+ (0.23)	1.56** (0.26)	1.58** (0.25)	1.78** (0.32)	1.93*** (0.34)	2.15*** (0.38)	2.15*** (0.38)
Observations	81,883	81,633	80,687	80,439	70,194	69,947	89,715	89,417	88,467	88,176
Individuals	11,228	11,202	11,233	11,206	10,155	10,131	12,329	12,300	12,322	12,293
Unemployed* UC Rollout Marginal Test	-0.021 (0.01)	0.00002 (0.01)	0.029+ (0.02)	0.031+ (0.02)	0.031* (0.01)	0.039** (0.01)	0.025* (0.01)	0.031** (0.01)	0.041*** (0.01)	0.045*** (0.01)

Notes: Results show odds ratios followed by robust standard errors in brackets. Default values are shown in square brackets. Unemployed*UC Rollout is the odds ratio for the DiD estimator. All (b) models additionally control for the wave in which the data was collected in order to account for any secular time trends in outcome variables. Marginal Test indicates the estimated overall percentage point change in the prevalence of outcomes and was calculated using Stata's 'margins' postestimation command. UKHLS longitudinal weights were applied to all models, which automatically restrict the models to a balanced panel. +p < 0.10, *p < 0.05, **p < 0.01, ***p < 0.001.

8.4 Conclusion

This chapter has used data from the UK Household Longitudinal Study (aka 'Understanding Society') in order to examine the impact of UC rollout in England, Scotland and Wales on various financial problems amongst unemployed individuals and their households. The empirical analysis was conducted in two parts, with part 1 investigating the immediate impact of UC 'Live Service' rollout via a difference-in-differences research design similar to the standard two-group and two-time period

approach, and part 2 examining the impact of UC rollout via a more longitudinal difference-in-differences approach.

The results from part 1 of the analysis suggest that there was no significant UC intervention effect for any of the five outcome variables indicating financial problems. This suggests that UC 'Live Service' rollout was not associated with an immediate increase in financial problems amongst unemployed people within the first 12 months of rollout. However, there are some key limitations that must be noted, and which could bias the results.

Firstly, two of the outcome variables used in the analysis – '*Problems Paying for Housing*' and '*Problems Paying Council Tax*' – ask respondents if they have experienced problems meeting their payments in the previous 12 months. Therefore, where UC had rolled out for less than 12 months in their area, respondents in the treatment group who report problems meeting housing/council tax payments may be referring to problems from before UC had rolled out.

Secondly, the treatment group includes those who self-report being unemployed in the waves immediately before and after UC rollout – this is not a perfect measure of exposure to UC. This is because even after UC had rolled out in their area, individuals would only become exposed to it if they had a change in circumstance or made a new claim (e.g. due to entering employment before becoming unemployed again). This means that although those in the treatment group were more likely to become exposed to UC than those in the control group, not all would have become exposed, which will lead to a conservative estimate of the UC intervention effect.

In terms of part 2 of the analysis, taking a more longitudinal approach, the results suggest that UC rollout was associated with an increase in the prevalence of unemployed individuals and their households experiencing some financial problems. Specifically, the results suggest that for the treatment group relative to the control group – after accounting for sex, age band, marital status and education – the prevalence of problems paying for housing increased by 3.9%, problems paying for bills by 3.1%, and problems paying council tax by 4.5%. However, no significant (at 5% level) UC intervention effect was found with regards to the prevalence of self-reported difficulties managing financially. This suggests that whilst there has been an increase in the prevalence of unemployed individuals' households having problems meeting basic costs, this has not corresponded to an increase in reporting of

difficulties managing financially. One possible explanation is that unemployed individuals may still feel that they are just about getting by financially despite falling behind with some payments for housing, bills or council tax – indeed, some qualitative research with UC claimants (Bush, Templer, Allen *et al.*, 2019, p. 5) described how claimants were pushed to the limit of their incomes but just about managed to get by via borrowing from family members or by employing the tactic of 'rotating' the payment of household bills.

As in part 1 of the analysis, part 2 has some important limitations that must be noted, and which could bias the results. Firstly, as in part 1 of the analysis, the issue discussed above of the outcome variables relating to housing and council tax measuring problems within the last 12 months also applies here. Secondly, as in part 1, the limitation of self-reported employment status not being a perfect measure of exposure to UC also applies here. However, using this more longitudinal approach means that this is less of an issue as over time a higher proportion of unemployed individuals in the analysis would have become exposed to UC due to having a change in circumstance or making a new claim. In Wickham, Bentley, Rose *et al.* (2020)'s paper on UC's mental health impacts, which employed a similar design using waves 1-8 of UKHLS, the authors estimated that around 73% of unemployed people were exposed to UC in this analysis period, with 27% remaining on legacy benefits. Thirdly, a limitation that is specific to part 2 of the analysis is that in the latter waves of the analysis period some of those in the control group are likely to have become exposed to UC. This is because as the 'Full Service' phase of UC rolled out it affected those in work on low incomes, and some individuals in this group could be in the control group. However, this is likely to be a very small proportion of the overall control group and would lead to an underestimate of the true UC intervention effect.

Fourthly, as outlined in the results section, graphical inspection of pre-UC rollout trends in the outcome variables used in the analysis suggest a small amount of divergence from the parallel trends assumption with regards to problems paying for housing and council tax. Parallel trends is the key assumption of difference-in-difference analysis, and its violation results in a biased estimate of the causal intervention effect (Columbia Public Health, 2019). Here, the graphs in Figure 8.3 suggest that the gap between treatment and control groups widened a small amount prior to UC rollout. Whilst this divergence from the parallel trends assumption was not dramatic, it may still introduce bias. Finally, as UKHLS respondents in part 2 of the analysis can move between treatment and control group in different waves, this may have a sorting effect on the treatment/control groups. Prior to UC rollout, unemployed

people would be composed of people with more/less severe financial problems. DiD estimators in the analysis will capture the causal effect of UC on prevalence of financial problems. But, as the aim of UC is to improve employment, it may also capture a sorting effect if those in treatment group with less severe financial problems are the ones who transition into employment – if this is the case, the DiD estimators in the analysis would overstate the UC intervention effect.

Overall, the DiD analysis in this chapter is a useful supplement to the previous empirical analysis in Chapters 5-7 of this thesis. Whilst the previous chapters have relied on local authority-level data to examine housing security, the analysis here was able to make use of individual level data from UKHLS linked to local authority level data on UC rollout in order to employ a difference-in-difference research design. Although the results of part 1 of the analysis suggest no immediate intervention effect of UC 'Live Service' rollout on financial problems, the part 2 longitudinal analysis suggests UC rollout is associated with an increase in problems paying for housing, bills and council tax, which is in line with the findings of existing qualitative studies with UC claimants (as set out in the literature review in Chapter 3, e.g. Britain Thinks, 2018; Cheetham, Moffatt and Addison, 2018; Bush, Templer, Allen *et al.*, 2019; Robertson, Wright and Stewart, 2020). However, there are numerous limitations to part 1 and part 2 of the analysis, and as a result, it is not possible to confidently infer causality from the findings of this chapter alone.

Chapter 9. Conclusion

9.1 Introduction

This chapter sets out how this thesis makes an original contribution to knowledge by going over the original aims and objectives, summarising the research findings, and explaining how they link in with existing empirical evidence on UC's housing insecurity impacts. The chapter ends by discussing what the implications of the research findings are for UC claimants, for landlords, and for UC as a policy going forwards.

9.2 Thesis Contribution to Knowledge

9.2.1 Thesis Aims and Objectives

As set out in Chapter 1, the aim of this thesis has been to exploit cross-area variation in the timing of UC rollout as a form of natural experiment in order to examine its impact on the four stages of housing insecurity. These four stages were set out in detail in Chapter 3. They are: (1) household financial problems, (2) build-up of rent arrears, (3) repossession actions, and (4) threatened/actual homelessness, and the empirical analysis of this thesis has sought to provide insight into the impact of UC rollout on all four stages. Based on this, the specific overarching objectives of this thesis (as outlined in Chapter 1) have been as follows:

1. To apply causal modelling to provide robust empirical analysis into the impacts of Universal Credit rollout on the different stages of housing insecurity in the UK.

2. To draw upon this empirical analysis in order to contribute to policy debates in the UK over the ongoing rollout of Universal Credit.

In order to address objective 1, this thesis has included four separate empirical chapters, with each one making use of different sources of administrative/survey data to examine

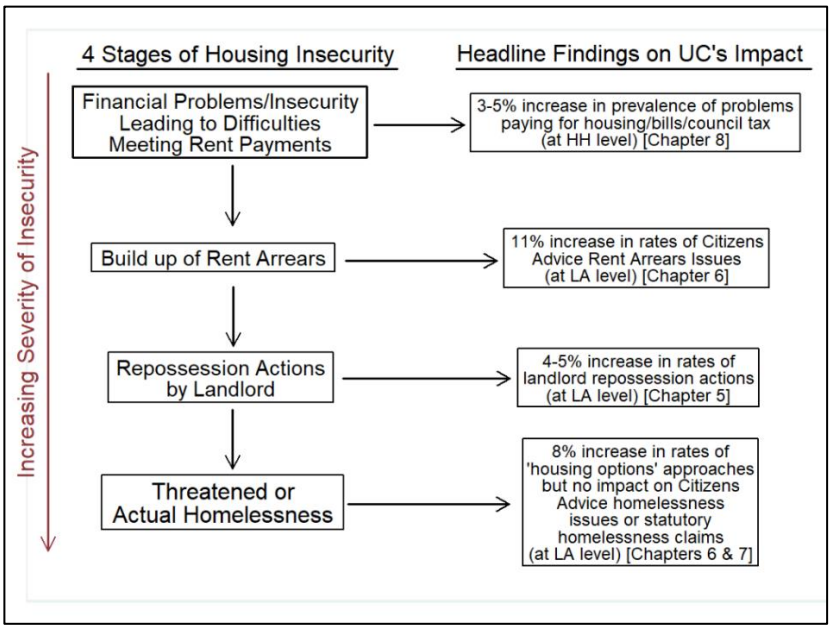
UC's impacts on the four stages of housing insecurity. Specifically, financial problems leading to difficulties meeting rent payments (first stage of insecurity) were examined in Chapter 8, build-up of rent arrears (second stage of insecurity) were examined in Chapter 6, landlord repossession actions (third stage of insecurity) were examined in Chapter 5, and homelessness (fourth and most extreme form of insecurity) was examined in Chapters 6 and 7.

In terms of objective 2, the policy review chapter (Chapter 2) and literature review chapter (Chapter 3) of this thesis introduced UC as a policy and have discussed existing empirical evidence and debates regarding its key design features and its impacts. The extent to which the empirical analysis of Chapters 5-8 contribute to these debates will be discussed throughout the course of this conclusion chapter.

9.2.2 Summary of Research Findings from Empirical Chapters

The four separate empirical studies in Chapters 5-8 have provided evidence of a clear link between the rollout of Universal Credit and housing insecurity in the UK, consistent across various indicators of insecurity from different data sources. The research findings for each of the four stages of housing insecurity are summarised below, with an overview of the headline findings also set out in Figure 9.1.

Figure 9.1. Headline Findings on UC's Impact on the Four Stages of Housing Insecurity



Firstly, with regards to the impact of UC rollout on financial problems leading to difficulties meeting rent payments (first stage of insecurity), this was examined in Chapter 8's difference-in-differences analysis. The results suggest that UC 'Live Service' rollout had no immediate (i.e. in first 12 months) intervention effect on financial problems such as self-reported difficulties managing financially, problems paying for housing, problems paying for bills and problems paying for council tax. However, when taking a more longitudinal approach using data from all nine waves of UKHLS, the results suggested that UC rollout was associated with a 3.9% increase in the prevalence of problems paying for housing, 3.1% increase in the prevalence of problems paying for bills and a 4.5% increase in the prevalence of problems paying for council tax (amongst unemployed individuals and their households, relative to those not unemployed). No significant UC intervention effect was found with regards to the prevalence of self-reported difficulties managing financially. This analysis had a range of limitations which mean that causality could not be confidently drawn from the findings of Chapter 8 alone.

Secondly, with regards to the impact of UC rollout on rent arrears (second stage of insecurity), this was examined in the fixed effects panel analysis of Chapter 6. The results suggest that UC rollout in England has led to an increase in demand for advice from Citizens Advice on rent arrears related issues. Specifically, the results suggest that 'Full Service' rollout, on average, led to an increase of 2.97 rent arrears advice issues within English local authorities (per 10,000 rented dwellings) by 2019 Q1. This corresponds to an 11% increase on rates in the pre 'Full Service' period. The impact tended to be greater when it had been rolled out for longer and thus reached more claimants. Where it had been rolled out for 6+ quarters, 'Full Service' led to an increase of 4.84 rent arrears advice issues (per 10,000 rented dwellings), i.e. an 18% increase on pre rollout rates. Importantly, disaggregating the advice data by housing tenure suggested that this detrimental impact of UC rollout on rent arrears advice rates was greater in the social rented sector than it was in the private rented sector. The internal validity of these research findings was strengthened by its use of a falsification test, whereby the main analysis was repeated using the mortgage arrears advice rate as a non-equivalent outcome variable.

Thirdly, in terms of UC's impact on landlord repossession actions (the third stage of insecurity), the results of the fixed effects panel analysis in Chapter 5 suggest that UC 'Full Service' rollout, on average, had no significant impact on bailiff repossessions but did lead to an increase of 1.74 landlord repossession claims, 1.42 landlord repossession orders and

0.70 landlord repossession warrants within English local authorities by 2019 Q1 (all figures are rates per 10,000 rented dwellings). To put these figures into context, this corresponds to a 4-5% increase on rates in the pre-UC 'Full Service' period. The results here also suggested that the impact of UC 'Full Service' on repossession rates tended to increase when it had been rolled out for longer and thus reached more claimants. Where it had been rolled out for 12+ months, 'Full Service' led to an increase of 2.60 landlord repossession claims, 2.89 landlord repossession orders and 1.09 landlord repossession warrants (again, all per 10,000 rented dwellings). This corresponds to a 6-10% increase on rates in the pre-UC 'Full Service' period. As with the above findings regarding rent arrears, the internal validity of these research findings was also strengthened through its use of a falsification test, whereby repeating the analysis using mortgage repossession rates as non-equivalent outcome variables.

Fourth and finally, the impact of UC rollout on threatened/actual homelessness (the fourth stage of insecurity) was investigated in both Chapter 6's fixed effects panel analysis of Citizens Advice data on homelessness issues in England and Chapter 7's fixed-effects panel analysis of 'Housing Options' approaches and homelessness claims in Scotland. Chapter 7's results suggest that UC rollout in Scotland was associated with an increase in rates of Housing Options approaches, but that its relationship with rates of statutory homelessness claims was less clear. To be specific, 'Full Service' rollout was associated with an increase of 1.32 Housing Options approaches (per 10,000 households) within Scottish local authorities by March 2019 (corresponding to an 8.1% increase on rates in the pre-'Full Service' period). This relationship was stronger where 'Full Service' had been rolled out for longer, with an increase of 6.65 approaches (again, per 10,000 households) where it had been rolled out for 13+ months (corresponding to a 22.5% increase on pre rollout rates). Whilst 'Full Service' was associated with a small increase in homelessness claim rates (specifically from claims where the applicant had been "asked to leave" their previous accommodation – most likely their family home), this relationship was less clear as it did not significantly strengthen the longer 'Full Service' had been rolled out. Chapter 6 found no significant relationship between UC rollout and rates of people approaching Citizens Advice with homelessness related issues in England.

When taken together, the empirical analysis of Chapters 5-8 provides strong insight into the detrimental impact of UC rollout on housing insecurity. Each separate piece of analysis (i.e. each empirical chapter) is individually robust. However, when they are considered together

the fact that they show consistently negative housing insecurity impacts across a range of data sources makes this thesis a form of "meta-analysis", which improves causal inference (see Weed, 2000). Moreover, the findings in general, also suggest that UC has adversely impacted more clearly on earlier stages of insecurity because (as discussed above) there was no evidence found of a clear link between UC rollout and actual homelessness. This is logical given that earlier stages of insecurity occur more quickly and there are fewer protective barriers against rent arrears/repossession actions than there are against the most extreme form of insecurity, homelessness.

9.2.3 Research in Context: How Findings Contribute to Existing Literature on Universal Credit's Housing Insecurity Impacts

Chapter 3 provided a full review of existing literature on UC's impacts on housing insecurity (and indeed its wider impacts on outcomes such as employment, mental health and wellbeing, incomes and debt, and food bank usage). Importantly, it set out how the DWP have tended to have a single-minded focus on employment outcomes, and have conducted their own research into UC's labour market impacts (Department for Work and Pensions, 2017a; Department for Work and Pensions, 2018b) but not on its wider impacts like housing insecurity. They have been criticised for this by the National Audit Office (2018) and by the United Nations special rapporteur on extreme poverty and human rights (Alston, 2018).

Outside of the government's own analysis, research that has been conducted on the housing insecurity impacts of UC rollout have tended to be qualitative studies or quantitative studies that were limited to specific localities. For example, qualitative studies have highlighted household financial problems during UC's long wait periods, leading to rent arrears (and in some instances repossession actions) amongst claimants who did not have adequate savings or the means to receive financial support from friends or family (Britain Thinks, 2018; Bush, Templer, Allen *et al.*, 2019; Cheetham M, Moffatt S, Addison M *et al.*, 2019; Robertson, Wright and Stewart, 2020). Meanwhile, other qualitative studies have highlighted how UC's conditionality regime has led to rent arrears and/or repossession actions for some claimants (Batty, 2018; Wright, Dwyer, Jones *et al.*, 2018). This is because claimants cut back on housing costs when their benefit payments are reduced by sanctions (see Beatty, Foden, McCarthy *et al.*, 2015,

pp. 35-36). In terms of quantitative research, small scale studies of social housing tenants in specific localities have found increased prevalence of rent arrears for UC claimants compared to Housing Benefit claimants (Smith Institute, 2017), and also that rent arrears are triggered by the introduction of the direct payment system (Hickman, Kemp, Reeve *et al.*, 2017).

The research conducted in this thesis makes an important contribution to this existing knowledge of UC's housing insecurity impacts. It backs up these findings of existing studies by providing strong evidence of the causal impacts of UC on housing insecurity. It also builds on some of the key limitations of the few existing quantitative studies on UC's housing insecurity impacts by using more national level data (from multiple administrative/survey data sources) and by applying causal modelling (fixed effects panel modelling, falsification tests, difference-in-differences analysis) to provide more robust empirical evidence that is qualitatively different to existing studies and is consistent across a diverse range of data sources. In particular, the key strength of the analysis was that it was able to exploit variation in exposure to UC – arising from cross-area variation in the timing of its rollout across local authorities – in order to examine its impacts. Whilst the analysis in each of the four empirical chapters had a set of limitations (discussed in detail at the end of each chapter) they overall, and taken together with existing empirical literature, provide evidence of a clear link between UC rollout and housing insecurity.

9.3 Implications of Research Findings

This thesis's research findings of increased housing insecurity arising from UC have wide-ranging implications. This includes implications for UC claimants, for landlords renting property to UC claimants, and for UC as a policy going forward.

9.3.1 Implications for Universal Credit Claimants

As set out in Chapter 3, housing is one of the key social, economic and environmental conditions that influences people's health and wellbeing (Braubach, 2011), contributing

to physical health by providing a physical place to dwell and social wellbeing by providing a sense of identity, worth, security and constancy (Preece and Bimpson, 2019). Therefore, this thesis's research finding of increased housing insecurity arising from UC has implications for UC claimants, with various harms potentially arising from housing insecurity, whether that is through being in financial difficulty/debt, threat of eviction or uncertainty about ability to meet rent payments.

Firstly, UC claimants who are facing rent arrears and/or repossession actions from their landlord may be forced to cut back spending on other essential costs, e.g. food or heating, in order to pay off the arrears and avoid losing their home through actual eviction. This may be a contributing factor to the rise in demand for food banks following UC rollout (Thompson, Jitendra and Rabindrakumar, 2019; Trussell Trust, 2019; Reeves and Loopstra, 2020), which was discussed in detail in Chapter 3.

Secondly, reduced security of housing for UC claimants has implications for their mental health, given that housing security is a known determinant of mental health. Previous research has highlighted how reduced security or affordability of housing is associated with adverse impacts on psychological wellbeing over and above that caused by the financial hardship alone (For example, see Nettleton and Burrows, 1998; Taylor, Pevalin and Todd, 2007), and this relationship has been shown to be causal in the context of previous welfare reforms to cut Housing Benefit under the UK's legacy benefits system (Reeves, Clair, McKee *et al.*, 2016). In the short-term, this arises most directly through the stress and anxiety caused by fear of losing your home, and this issue has been highlighted by qualitative studies with UC claimants (Wright, Dwyer, Jones *et al.*, 2018; Robertson, Wright and Stewart, 2020). More broadly, in the longer term, and as discussed in Chapter 3, having a secure home provides a base from which to engage in society through holding down work and building social networks and social connections (Bailey, Besemer, Bramley *et al.*, 2015). This is threatened by housing insecurity, and social connections will be disrupted (for both householders and their children) if evictions or unaffordability lead to frequent moves. Therefore, UC's adverse housing insecurity impacts can have wide-ranging implications for mental health, and as discussed in Chapter 3, research has already highlighted that the prevalence of psychological distress has increased as UC has rolled out (Wickham, Bentley, Rose *et al.*, 2020).

9.3.2 Implications for Landlords

In terms of implications for landlords, the build-up of rent arrears and subsequent repossession actions will likely negatively impact upon the incomes of landlords due to the income they lose through unpaid rent (and its associated other costs) along with the legal costs associated with evicting a tenant. This may also lead to landlords reducing services and cutting back on maintenance costs (thus leading to poorer standards of housing) in order to recoup their losses accrued from rent arrears or repossession actions.

For the social rented sector, it is clear that the shift from payment of housing costs to landlords under Housing Benefit to the direct payment system under UC increases the scope for rent arrears despite Alternative Payment Arrangements and UC 'Scottish Choices' being in place in attempt to mitigate this. One of the reasons for this is that there is economic logic in housing association tenants not paying some or all of their rent (and prioritising other costs) when experiencing a financial emergency because interest is not charged on rent arrears (Hickman, Pattison and Preece, 2018, p. 12). This is likely to reduce the income streams of housing associations, as not only do they lose income from unpaid rent, but they also need to invest more money in resources for rent collection and personalised tenant support (Ibid, pp. 33-39). Consequently, Hickman, Pattison and Preece (2018) note that the impact of UC is likely to have a significant adverse effect on the finances of housing associations, and ultimately may threaten the financial viability of some associations that are smaller and not well placed to manage UC's impacts on their cashflow.

UC's housing insecurity impacts also have implications for the incomes of private landlords. In particular, UC tenants going into rent arrears can place considerable financial risk onto private residential landlords. This issue has been highlighted by research by Simcock (2018), whose survey of 2,234 private landlords across the UK found that 61% of landlords who let to UC claimants had lost income due to tenants going into arrears, with these landlords on average being owed £2,390.19 and thus potentially putting both tenants and landlords in a dangerous financial situation.

As a result of all these problems with UC and housing insecurity, a broader implication is that landlords will become increasingly unwilling to let their property to tenants who are on UC due to fears they will be unable to pay their rent. In the social rented sector, there is evidence that social landlords are increasingly needing to use affordability assessments in

order to determine prospective tenant's ability to meet rental costs (Preece, Hickman and Pattison, 2019). UC and other welfare reforms have contributed to this, and potentially increase the potential for exclusion from affordable housing on the grounds of ability to pay (Ibid). Hickman, Pattison and Preece (2018, pp. 12-13) note that when housing associations do exclude households based on their ability to pay (e.g. households with poor 'credit' history), this raises concerns over who will house these households and whether alternative housing providers will have the capacity to do so. Moreover, in the private rented sector, there is evidence that, aware of all of UC's issues, some landlords are now discriminating against all UC claimants by refusing to let their property to anyone who is in receipt of UC. For example, the aforementioned survey of private landlords by Simcock (2018) found that 62% of landlords were unwilling to let to those who were on UC, and this is backed up by another survey by the National Landlords Association (2017).

9.3.3 Implications for Universal Credit as a Policy

The research findings of this thesis have several implications for UC as a policy. It is important to note that the empirical analysis of this thesis used data up to early 2019, meaning that it covers a time period that was still relatively early in the overall rollout of UC. Merging six different working-age and means-tested benefits – which were administered separately by three different government departments (DWP, HMRC and local authorities) – into a single benefit payment administered by the DWP was a significant administrative challenge. As such, 'teething problems' were to be expected. However, the research findings of this thesis suggest a significant impact on housing insecurity that it likely to be the result of systematic flaws with UC as a policy rather than merely due to teething issues and administrative errors.

Despite widespread criticism of UC over its potential impacts on the ability of claimants to meet rent payments, the government have tended to argue that safeguards such as Alternative Payment Arrangements and advance payments are in place to prevent such insecurity (e.g. see HC Debate 29 October 2018 cW), and that "the best way to help people pay their rent is to help them into work" (The Independent, 2018). The analysis of this thesis has focussed on the overall impact of UC rollout by examining the timing of its rollout within local authorities. This means that it isn't possible from the analysis to disentangle the impact of UC's different design features on housing insecurity, or the impact of safeguards such as advance payments or Alternative Payment Arrangements.

However, by considering the research findings of this thesis alongside the findings of existing qualitative studies which have examined the impacts of specific design features, it is possible to identify which aspects of UC have been most detrimental and should be reformed.

Firstly, it is likely that UC's long wait periods (typically 5 weeks) between a claimant initially making a claim and receiving their first payment has contributed to the observed impact on housing insecurity. This is because the wait period can leave claimants with little or no income with which to meet rent payments (as highlighted by qualitative studies such as Britain Thinks, 2018; Cheetham, Moffatt and Addison, 2018). The UC advance payments safeguard can mitigate this issue to some extent in the short-term. However, these are effectively just interest free loans and need to be paid back via deductions on future UC payments. Therefore, as discussed in Chapter 3, and pointed out by Thompson, Jitendra and Rabindrakumar (2019, p. 33), they effectively leave claimants with a choice between "hardship now or hardship later". Consequently, in order to protect claimants from housing insecurity there is clearly a need to end the 5-week wait period under UC entirely. This could be done by making UC advance payments non-repayable, or, as recommended by the House of Lords Economic Affairs Committee (2020), by introducing a non-repayable two-week initial grant for all UC claimants. This would provide some extra security for claimants and alleviate the problem of claimants falling into arrears whilst waiting for their first full UC payment.

Secondly, it is likely that UC's extended and intensified conditionality regime, which was set out in Chapter 2 and has been described as establishing a new "ubiquitous conditionality" (Dwyer and Wright, 2014), has also contributed to the observed impact on housing insecurity. This is because, by reducing claimants incomes, sanctions can lead to people cutting back on housing costs and thus falling into arrears (Beatty, Foden, McCarthy *et al.*, 2015), and qualitative research with UC claimants has highlighted how its sanctions regime has led to some claimants (including those were in work) being placed at risk of losing their homes due to rent arrears (Wright, Dwyer, Jones *et al.*, 2018, p. 10). Consequently, there is a need to reduce the severity of sanctions in a way that ensures claimants are not placed at risk of rent arrears and eviction (and that other essential costs e.g. food, heating etc. can also be met). Emerging evidence suggests that the temporary suspension of conditionality (amongst other forms of support) during the COVID-19 pandemic led to claimants – particularly those who were furthest away from

labour market and those with complex needs – finding it easier to manage financially under UC (Edmiston, Robertshaw, Gibbons *et al.*, 2021). This further motivates the need to reform UC's use of conditionality in the long-term.

Thirdly, UC's default system of monthly direct payments is also likely to have contributed to the observed impacts on housing insecurity. As discussed in Chapter 3, this system can have implications for claimants ability to meet rent payments as it can lead to missed payments amongst those who lack budgeting skills (particularly amongst young people - see Homeless Link, 2018) or those who experience a financial emergency and who prioritise other essential costs over rent (as discussed above in section 9.3.2 this can be particularly problematic in the social rented sector as interest is not paid on rent arrears and hence other costs that interest are paid on will be prioritised in the first instance). There is some evidence that Alternative Payment Arrangements (APAs) (e.g. managed payment of housing costs to landlords or more frequent UC payments) have helped mitigate this issue, at least in certain localities (Smith Institute, 2019). However, other research suggests that there is a lack of awareness of APAs amongst some claimants (Hobson, Spoor and Kearton, 2019). Therefore, going forward there is a need to ensure all claimants are aware of the option of APAs (as is already the case in Scotland where "Scottish Choices" are given to all claimants automatically). Greater take-up of APAs amongst those who need them is likely to help reduce rent arrears and repossession actions.

Finally, another important factor that will determine the housing security of UC claimants is the actual value of UC payments (not just in terms of the amount households receive specifically towards housing costs but also the amount of the standard allowance). As discussed in Chapter 3, the initial extent to which UC rollout effected the incomes of claimants compared to the legacy system depended on their household's circumstances, although importantly, analysis by Brewer, Joyce, Waters *et al.* (2019) had suggested that whilst there were many winners and losers it was the poorest households who were most likely to lose out from the switch to UC. However, in response to the COVID-19 pandemic, the UC standard allowance was uplifted by £20 per week, or £1,040 per year. This uplift effectively reversed three decades of benefit retrenchment (i.e. the three decades of benefit retrenchment outlined in Chapter 2) (Brewer and Gardiner, 2020). Early research by Summers, Scullion, Baumberg Geiger *et al.* (2021) suggests that the £20 UC uplift has helped claimants, but that even with

the uplift the value of UC was still inadequate, with many claimants still experiencing financial difficulties and falling behind with their housing costs. At the time of writing, the £20 UC uplift is due to end in September 2021. In order to protect the housing security of claimants, there is a need to not just make this uplift permanent, but also to increase it further.

9.4 Future Research

The empirical analysis of this thesis examined the impacts of Universal Credit rollout up to early 2019. The fixed effects panel analyses conducted in Chapters 5-7 all used data up to March 2019, whilst the difference-in-differences analysis of Chapter 8 used data up to wave 9 of UKHLS, which was collected from 2017-2019. By this point, UC was still in the relatively early stages of its rollout – there were around 1.8 million people on UC (UK-wide) by March 2019 (Department for Work and Pensions, 2021b), and this was initially expected to rise to 7 million by the time UC was fully rolled out, i.e. by the time the 'managed migration' process had transferred those still on legacy benefits over to UC. However, the COVID-19 pandemic has led to a huge surge in the number of UC claims (see Brewer and Handscomb, 2020, p. 18), and as of February 2021 there were already over 6 million claimants (Department for Work and Pensions, 2021b), whilst the 'managed migration' process had been delayed. As already touched on, the COVID-19 pandemic has also led to some important temporary changes to policy in relation to UC and housing insecurity, such as uplifting the UC standard allowance by £20 per week, increasing the UC housing element (at least in some areas) by re-linking Local Housing Allowance to 30% of market rents in the local area, suspending conditionality and suspending evictions. Going forwards, it is important that both qualitative and quantitative research continue to investigate the impacts of UC on housing insecurity as its rollout continues, and in particular to assess what the impacts have been/what lessons can be learned from changes to UC over time and policies enacted to provide security to claimants during the pandemic.

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Appendix 1: Timing of UC 'Live Service' and 'Full Service' Rollout in English Local Authorities

Local Authorities	'Live Service' rollout	'Full Service' rollout
North East		
1. County Durham	September 2015	October 2017 (2 JCPs) December 2017 (4 JCPs) June 2018 (4 JCPs)
2. North Tyneside	November 2015	May 2018
3. South Tyneside	February 2016	May 2018
4. Sunderland	November 2015	July 2018
5. Darlington	November 2015	June 2018
6. Hartlepool	February 2015	December 2016
7. Middlesbrough	February 2016	October 2018
8. Northumberland	February 2016	November-December 2018
9. Redcar and Cleveland	March 2016	November 2018
10. Stockton-on-Tees	December 2015	July 2018
11. Gateshead	June 2015	October-November 2017
12. Newcastle Upon Tyne	April 2015	May 2016 (1 JCP) February 2017 (1 JCP) March 2017 (1 JCP)
North West		
13. Blackburn with Darwen	December 2014	February 2018
14. Blackpool	December 2014	December 2018
15. Cheshire East	August 2014	July 2017 (1 JCP) July 2018 (2 JCPs)
16. Cheshire West and Chester	August 2014	July 2017 (1 JCP) November 2017 (1 JCP) December 2017 (3 JCPs)
17. Halton	December 2014	July 2016
18. Warrington	July 2013	February 2017
19. Allerdale	December 2014	November 2016
20. Barrow-in-Furness	December 2014	December 2018
21. Carlisle	December 2014	July 2018
22. Copeland	December 2014	November 2016
23. Eden	January 2015	July 2018
24. South Lakeland	December 2014	September 2018
25. Bolton	June 2014	November 2018
26. Bury	July 2014	July 2018

27. Manchester	October 2014	October 2017 November 2017 July 2018
28. Oldham	August 2013	April 2017
29. Rochdale	September 2014	May 2018
30. Salford	July 2014	September 2018
31. Stockport	December 2014	November 2018
32. Tameside	April 2013	March 2018
33. Trafford	July 2014	July 2017
34. Wigan	July 2013	April 2018
35. Burnley	October 2014	May 2017
36. Chorley	November 2014	July 2018
37. Fylde	December 2014	December 2018
38. Hyndburn	November 2014	February 2018
39. Lancaster	December 2014	July 2016
40. Pendle	October 2014	November 2018
41. Preston	August 2014	July 2018
42. Ribble Valley	December 2014	November 2018
43. Rossendale	November 2014	November 2018
44. South Ribble	August 2014	July 2018
45. West Lancashire	September 2014	December 2017
46. Wyre	November 2014	December 2018
47. Knowsley	August 2014	May 2018 (2 JCPs) September 2018 (2 JCPs)
48. Liverpool	October 2014	September 2018 (2 JCPs) November 2018 (2 JCPs) December 2018 (2 JCPs)
49. Sefton	July 2014	October 2017
50. St. Helens	July 2014	July 2018
51. Wirral	July 2014	November 2017
Yorkshire and the Humber		
52. East Riding of Yorkshire	January 2016	July 2018
53. Kingston upon Hull, City of	February 2016	December 2018
54. North East Lincolnshire	June 2015	December 2017
55. North Lincolnshire	June 2015	October 2017
56. York	February 2015	July 2017
57. Craven	February 2015	October 2016
58. Hambleton	February 2015	October 2016
59. Harrogate	February 2014	June 2016
60. Richmondshire	May 2015	June 2016
61. Ryedale	February 2015	June 2016
62. Scarborough	November 2015	May 2018
63. Selby	November 2015	May 2018
64. Barnsley	March 2015	July 2017

65. Doncaster	September 2015	October 2017
66. Rotherham	December 2015	July 2018
67. Sheffield	January 2016	November-December 2018
68. Bradford	November 2015	June 2018
69. Calderdale	April 2015	June 2017
70. Kirklees	June 2015	November 2017
71. Leeds	February 2016	October 2018
72. Wakefield	April 2016	November 2018
East Midlands		
73. Daventry	February 2015	November 2016
74. East Northamptonshire	October 2015	May 2018
75. Kettering	June 2015	October 2018
76. Northampton	November 2015	November 2018
77. South Northamptonshire	November 2015	November 2018
78. Wellingborough	September 2015	November 2018
79. Ashfield	June 2015	November 2018
80. Bassetlaw	February 2015	December 2017
81. Broxtowe	November 2015	November 2018
82. Gedling	June 2015	November 2018
83. Mansfield	October 2015	September 2018
84. Newark and Sherwood	October 2015	May 2018
85. Rushcliffe	February 2016	October 2018
86. Derby	January 2016	July 2018
87. Leicester	January 2016	June 2018
88. Nottingham	February 2016	October 2018
89. Rutland	October 2015	October 2017
90. Amber Valley	October 2015	June 2018
91. Bolsover	November 2015	November 2018
92. Chesterfield	March 2015	November 2017
93. Derbyshire Dales	November 2015	September 2018
94. Erewash	February 2015	May 2017
95. High Peak	June 2015	September 2018
96. North East Derbyshire	November 2015	July 2018
97. South Derbyshire	October 2015	November 2018
98. Blaby	January 2016	June 2018
99. Charnwood	June 2015	July 2018
100. Harborough	February 2015	November 2016
101. Hinckley and Bosworth	March 2015	March 2017
102. Melton	February 2015	November 2016
103. North West Leicestershire	September 2015	February 2018
104. Oadby and Wigston	January 2016	June 2018

105. Boston	April 2015	September 2018
106. East Lindsey	November 2015	June 2018 (1 JCP) September 2018 (1 JCP)
107. Lincoln	November 2015	March 2018
108. North Kesteven	November 2015	November 2018
109. South Holland	November 2015	July 2018
110. South Kesteven	March 2015	October 2017
111. West Lindsey	November 2015	September 2018
112. Corby	March 2015	February 2017
West Midlands		
113. Herefordshire, County of	March 2015	June 2018
114. Shropshire	April 2015	May 2018
115. Stoke-on-Trent	February 2016	June 2018
116. Telford and Wrekin	June 2015	November 2018
117. Cannock Chase	December 2015	November 2018
118. East Staffordshire	April 2015	November 2018
119. Lichfield	March 2015	November 2017
120. Newcastle-under-Lyme	April 2015	December 2018
121. South Staffordshire	December 2015	November 2018
122. Stafford	October 2015	November 2018
123. Staffordshire Moorlands	June 2015	September 2018
124. Tamworth	March 2015	November 2017
125. North Warwickshire	October 2015	September 2018
126. Nuneaton and Bedworth	June 2015	October 2017
127. Rugby	February 2015	May 2016
128. Stratford-on-Avon	February 2015	December 2016
129. Warwick	October 2015	October 2018
130. Birmingham	April 2015	November-December 2017
131. Coventry	December 2015	July 2017
132. Dudley	April 2015	July 2017
133. Sandwell	November 2015	November 2018
134. Solihull	May 2015	July 2017
135. Walsall	June 2015	July 2018 (2 JCPs) October 2018 (2 JCPs)
136. Wolverhampton	February 2016	December 2017
137. Bromsgrove	October 2015	September 2018
138. Malvern Hills	October 2015	September 2018

139. Redditch	February 2015	October 2017
140. Worcester	October 2015	October 2018
141. Wychavon	October 2015	November 2018
142. Wyre Forest	October 2015	November 2018
East of England		
143. Bedford	March 2015	May 2017
144. Central Bedfordshire	February 2016	November 2018
145. Luton	February 2016	October 2018
146. Peterborough	December 2015	November 2017
147. Southend-on-Sea	April 2015	July 2017
148. Thurrock	March 2015	October 2017
149. Cambridge	February 2016	September 2018 (1 JCP) October 2018 (1 JCP)
150. East Cambridgeshire	February 2016	September 2018
151. Fenland	March 2016	September 2018
152. Huntingdonshire	February 2016	October 2018
153. South Cambridgeshire	February 2016	October 2018
154. Basildon	March 2015	November 2017
155. Braintree	October 2015	October 2017
156. Brentwood	March 2015	November 2017
157. Castle Point	May 2015	July 2018
158. Chelmsford	September 2015	December 2018
159. Colchester	March 2015	July 2018
160. Epping Forest	February 2016	December 2018
161. Harlow	February 2016	July 2017
162. Maldon	September 2015	December 2018
163. Rochford	February 2016	July 2018
164. Tendring	April 2015	Tendrong 2
165. Uttlesford	October 2015	October 2017
166. Broxbourne	June 2015	November 2017
167. Dacorum	September 2015	December 2018
168. East Hertfordhsire	February 2016	October 2018
169. Hertsmere	September 2015	December 2018
170. North Hertfordshire	February 2016	October 2018
171. St Albans	May 2015	November 2017
172. Stevenage	February 2016	October 2018
173. Three Rivers	November 2015	December 2017
174. Watford	November 2015	December 2017
175. Welwyn Hatfield	September 2015	December 2017

176. Breckland	April 2015	June 2018 (1 JCP) September 2018 (1 JCP)
177. Broadland	December 2015	October 2018
178. Great Yarmouth	March 2016	April 2016
179. King's Lynn and West Norfolk	March 2016	November 2018
180. North Norfolk	September 2015	October 2018 (1 JCP) December 2018 (2 JCP
181. Norwich	December 2015	October 2018
182. South Norfolk	November 2015	May 2018
183. Babergh	September 2015	October 2017
184. Forest Heath	April 2015	December 2018
185. Ipswich	November 2015	April 2018
186. Mid Suffolk	September 2015	May 2018
187. St Edmundsbury	April 2015	October 2017
188. Suffolk Coastal	April 2015	October 2018
189. Waveney	March 2015	May 2016 (1 JCP) October 2017 (1 JCP)
London		
190. Camden	March 2016	December 2018
191. City of London	March 2015	March 2017
192. Hackney	March 2016	October 2018
193. Hammersmith and Fulham	February 2015	June 2016 (1 JCP) November 2016 (1 JCP) December 2016 (1 JCP)
194. Haringey	March 2016	October 2018
195. Islington	November 2015	June 2018
196. Kensington and Chelsea	November 2015	December 2018
197. Lambeth	February 2016	October 2016 (1 JCP) December 2017 (2 JCPs) February 2018 (2 JCPs)
198. Lewisham	February 2016	July 2018
199. Newham	February 2016	July 2018
200. Southwark	November 2015	November 2015 (1 JCP) April 2016 (1 JCP) October 2016 (1 JCP) November 2016 (1 JCP)
201. Tower Hamlets	March 2015	March 2017
202. Wandsworth	April 2015	September 2018
203. Westminster	November 2015	June 2018
204. Barking and Dagenham	July 2015	March 2018
205. Barnet	March 2015	May 2018
206. Bexley	February 2016	October 2018

207. Brent	March 2015	November-December 2018
208. Bromley	January 2016	July 2018
209. Croydon	July 2015	November 2015 (1 JCP) April 2016 (2 JCP)
210. Ealing	July 2015	March 2018
211. Enfield	July 2015	November 2017 (1 JCP) March 2018 (1 JCP) May 2018 (1 JCP)
212. Greenwich	January 2016	October 2018
213. Harrow	October 2015	July 2018
214. Havering	February 2016	June 2018
215. Hillingdon	November 2015	October 2018
216. Hounslow	April 2015	January 2016
217. Kingston upon Thames	November 2015	June 2018
218. Merton	January 2016	December 2017
219. Redbridge	February 2016	June 2018
220. Richmond upon Thames	November 2015	June 2018
221. Sutton	February 2015	December 2015
222. Waltham Forest	May 2015	May 2018
South East		
223. Bracknell Forest	September 2015	May 2018
224. Brighton and Hove	December 2015	October-November 2017
225. Isle of Wight	December 2015	June 2018 (1 JCP) October 2018 (1 JCP)
226. Medway	October 2015	May 2018
227. Milton Keynes	November 2015	December 2018
228. Portsmouth	March 2016	September 2018
229. Reading	September 2015	December 2017
230. Slough	September 2015	April 2018
231. Southampton	March 2015	February 2017
232. West Berkshire	July 2015	December 2017
233. Windsor and Maidenhead	September 2015	May 2018
234. Wokingham	September 2015	December 2017 (1 JCP) May 2018 (1 JCP)
235. Aylesbury Vale	November 2015	September 2018
236. Chiltern	November 2015	September 2018
237. South Bucks	November 2015	April 2018 (1 JCP) September 2018 (1 JCP) October 2018 (1 JCP)
238. Wycombe	November 2015	September 2018
239. Eastbourne	June 2015	October 2017

240. Hastings	April 2015	December 2016
241. Lewes	June 2015	September 2018
242. Rother	April 2015	July 2017
243. Wealden	June 2015	October 2017
244. Basingstoke and Deane	March 2016	May 2018
245. East Hampshire	February 2016	October 2018
246. Eastleigh	May 2015	July 2017
247. Fareham	March 2016	November 2018
248. Gosport	March 2016	November 2018
249. Hart	February 2016	October 2018
250. Havant	March 2016	September 2018 (2 JCPs) November 2018 (1 JCP)
251. New Forest	December 2015	June 2018 (1 JCP) September 2018 (2 JCPs)
252. Rushmoor	February 2016	October 2018
253. Test Valley	September 2015	July 2018
254. Winchester	September 2015	July 2018
255. Ashford	April 2015	June 2018
256. Canterbury	January 2016	July 2018
257. Dartford	January 2016	July 2018
258. Dover	January 2016	May 2017
259. Gravesham	May 2015	May 2018
260. Maidstone	March 2015	November 2018
261. Sevenoaks	October 2015	May 2018 (1 JCP) July 2018 (1 JCP)
262. Folkestone and Hythe	January 2016	May 2018 (1 JCP) June 2018 (1 JCP)
263. Swale	March 2015	December 2017
264. Thanet	October 2015	July 2017
265. Tonbridge and Malling	October 2015	November 2018
266. Tunbridge Wells	October 2015	November 2018
267. Cherwell	May 2015	November 2017
268. Oxford	April 2015	October 2017
269. South Oxfordshire	April 2015	October 2017 (1 JCP) December 2017 (1 JCP)
270. Vale of White Horse	April 2015	October 2017
271. West Oxfordshire	April 2015	November 2017
272. Elmbridge	February 2016	November 2018
273. Epsom and Ewell	February 2016	November 2018
274. Guildford	February 2016	October 2018
275. Mole Valley	February 2016	October 2018
276. Reigate and Banstead	February 2016	October 2018

277. Runnymede	February 2016	November 2018
278. Spelthorne	February 2016	November 2018
279. Surrey Heath	February 2016	November 2018
280. Tandridge	February 2016	October 2018
281. Waverley	February 2016	October 2018
282. Woking	February 2016	December 2017 (1 JCP) May 2018 (1 JCP)
283. Adur	October 2015	July 2018
284. Arun	October 2015	July 2018
285. Chichester	October 2015	July 2018
286. Crawley	September 2015	June 2018
287. Horsham	September 2015	June 2018
288. Mid Sussex	September 2015	June 2018
289. Worthing	October 2015	July 2018
South West		
290. Bath and North East Somerset	February 2014	May 2016
291. Bournemouth	June 2015	November 2017
292. Bristol, City of	November 2015	June 2018 (3 JCPs) September 2018 (2 JCPs) October 2018 (1 JCP)
293. Cornwall	May-July 2015	December 2017 (3 JCPs) May 2018 (4 JCPs) June 2018
294. Isles of Scilly	June 2015	June 2018
295. North Somerset	February 2015	June 2017 (1 JCP) July 2017 (1 JCP)
296. Plymouth	January 2016	October-November 2017
297. Poole	June 2015	October 2017
298. South Gloucestershire	January 2016	September 2018 (1 JCP) October 2018 (2 JCPs)
299. Swindon	February 2015	November-December 2016
300. Torbay	January 2016	September 2018
301. Wiltshire	March 2015	May 2017 (1 JCP) June 2017 (1 JCP) July 2017 (2 JCPs)
302. East Devon	November 2015	July 2018
303. Exeter	November 2015	September 2018
304. Mid Devon	November 2015	July 2018
305. North Devon	November 2015	July 2018
306. South Hams	November 2015	September 2018
307. Teignbridge	November 2015	September 2018
308. Torridge	November 2015	July 2018
309. West Devon	November 2015	September 2018 (1 JCP) October 2018 (1 JCP)

310.	Christchurch	June 2015	November 2017
311.	East Dorset	June 2015	October 2017
312.	North Dorset	June 2015	October 2017
313.	Purbeck	June 2015	October 2017
314.	West Dorset	October 2015	December 2017
315.	Weymouth and Portland	October 2015	December 2017
316.	Cheltenham	June 2015	December 2017
317.	Cotswold	May 2015	November 2017
318.	Forest of Dean	May 2015	November 2017
319.	Gloucester	June 2015	February 2018
320.	Stroud	June 2015	October 2017
321.	Tewkesbury	June 2015	December 2017
322.	Mendip	March 2015	July 2016
323.	Sedgemoor	March 2015	May 2016
324.	South Somerset	April 2015	April 2017
325.	Taunton Deane	April 2015	October 2016
326.	West Somerset	April 2015	October 2016

TABLE A1. Timing of Universal Credit Rollout in English Local Authorities. *Notes*: JCPs refer to Jobcentre Plus offices. Sources: (UK Government, 2015a; UK Government, 2015b; UK Government, 2018)

Appendix 2: Timing of UC 'Live Service' and 'Full Service' Rollout in Scottish Local Authorities

Local Authority	'Live Service' Go-Live Month	'Full Service' Go-Live Month
Aberdeen City	November 2015	October 2018
Aberdeenshire	May 2015	June 2018
Angus	April 2016	November 2017
Argyll and Bute	March 2016	September 2018
City of Edinburgh	March 2015	October-November 2018
Clackmannanshire	May 2015	June 2017
Dumfries and Galloway	April 2015	May 2018
Dundee City	November 2015	November 2017
East Ayrshire	February 2016	October 2017
East Dunbartonshire	December 2015	November 2016
East Lothian	April 2015	March 2016
East Renfrewshire	February 2016	September 2018
Falkirk	May 2015	March 2018
Fife	April 2016	December 2017
Glasgow City	June 2015	September-December 2018
Highland	November 2013 (Inverness) and February 2015 (Rest of Highland)	June 2016 (Inverness) and July 2017 (Rest of Highland)
Inverclyde	October 2015	November 2016
Midlothian	April 2015	March 2017
Moray	November 2015	June 2018
Na-h-Eileanan Siar	May 2015	September 2018
North Ayrshire	April 2015	October 2017
North Lanarkshire	March 2015	April 2018
Orkney	May 2015	September 2018
Perth and Kinross	April 2016	June 2018
Renfrewshire	June 2015	September 2018
Scottish Borders	April 2015	June 2018
Shetland Islands	May 2015	September 2018
South Ayrshire	October 2015	February 2018
South Lanarkshire	October 2015	October 2017
Stirling	May 2015	June 2017
West Dunbartonshire	March 2015	November 2018
West Lothian	November 2015	May 2018

TABLE A2. Timing of Universal Credit 'Live Service' and 'Full Service' Rollout in Scottish Local Authorities
Sources: (UK Government, 2015a; UK Government, 2015b; UK Government, 2018)

Appendix 3: Timing of UC 'Live Service' and 'Full Service' Rollout in Welsh Local Authorities

Local Authority	'Live Service' Go-Live Month	'Full Service' Go-Live Month
Blaenau Gwent	October 2015	April 2018
Bridgend	June 2015	February 2018
Caerphilly	May 2015	May 2018
Cardiff	November 2015	February 2018
Carmarthenshire	October 2015	March 2018
Ceredigion	November 2015	May 2018
Conwy	May 2015	February 2018
Denbighshire	July 2015	February 2018
Flintshire	February 2015	April 2017
Gwynedd	October 2015	April 2018
Isle of Anglesey	September 2015	March 2018
Merthyr Tydfil	September 2015	March 2018
Monmouthshire	September 2015	March 2018
Neath Port Talbot	April 2015	October 2017
Newport	June 2015	November 2017
Pembrokeshire	November 2015	May 2018
Powys	October 2015	June 2018
Rhondda Cynon Taf	March 2016	July 2018
Swansea	July 2015	December 2017
Torfaen	April 2015	July 2017
Vale of Glamorgan	February 2016	June 2018
Wrexham	March 2015	October 2017

TABLE A2. Timing of Universal Credit 'Live Service' and 'Full Service' Rollout in Welsh Local Authorities
Sources: (UK Government, 2015a; UK Government, 2015b; UK Government, 2018)

Appendix 4: Chapter 5 Falsification Test

	(1) **Claim Rate** (Mortgage Lender)	(2) **Order Rate** (Mortgage Lender)
UC 'Live Service' Rolled Out:		
[No] *N=4617*		
Yes *N=4750*	0.09 (0.16)	0.02 (0.14)
UC 'Full Service' Rolled Out:		
[No] *N=8065*		
Yes *N=1302*	-0.11 (0.08)	-0.14 (0.08)
Model Based Unemployment Rate	0.87*** (0.12)	0.75*** (0.11)
Per £100 increase in **Median Weekly Wages**	-0.29* (0.12)	-0.17 (0.11)
Per £10,000 increase in **Average Selling Price of a Home**	0.04*** (0.007)	0.08*** (0.008)
Local Authority Quarters (*N* total)	9367	9367
R^2	0.754	9.716

TABLE A4 Relationship Between UC Rollout and . *Notes*: Models examine the relationship between UC Rollout and Mortgage Repossession Rates Within 323 English Local Authorities, 2012 Q1 – 2019 Q1. *Notes*: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. *N* refers to the number of local authority quarters. Mortgage repossession warrants and bailiff repossessions are not considered as these take much longer to reach under the legal mortgage repossession process than under the landlord repossession process, meaning the two are not comparable. Mortgage repossession rates are per 10,000 owner occupied dwellings. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.

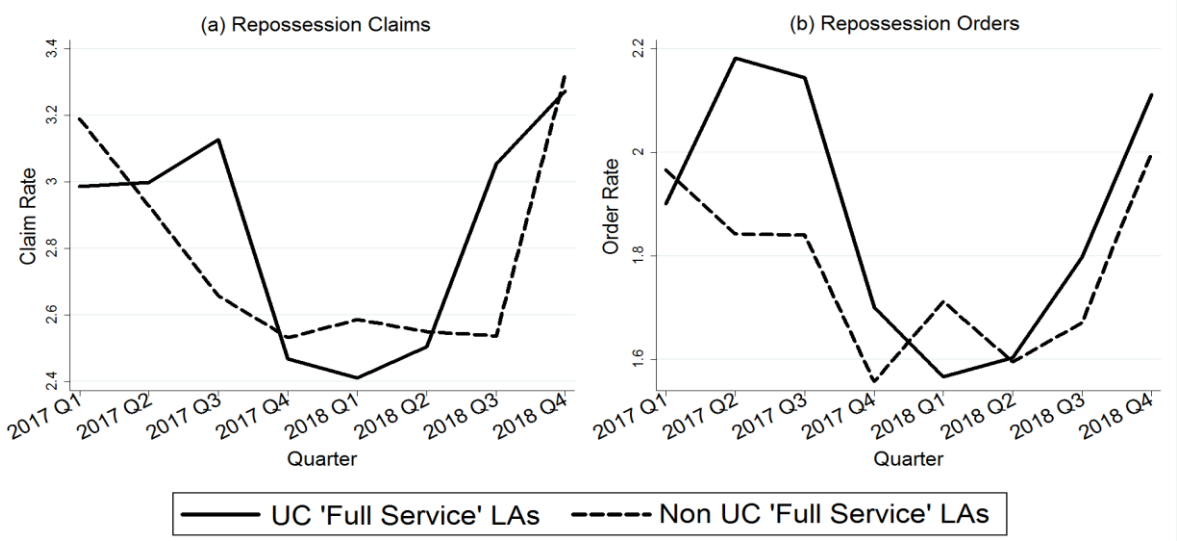

FIGURE A2.1. Quarterly trends in mean mortgage lender repossession rates in UC 'Full Service' versus non UC 'Full Service' local authorities, 2017-2018. *Notes*: The number of local authorities that were UCFS areas gradually increased over time as rollout progressed – 10 percent of local authorities were UCFS areas by 2017 Q1, increasing to 17 percent by 2017 Q3, 37 percent by 2018 Q1, 65 percent by 2018 Q3 and 85 percent by 2018 Q4.

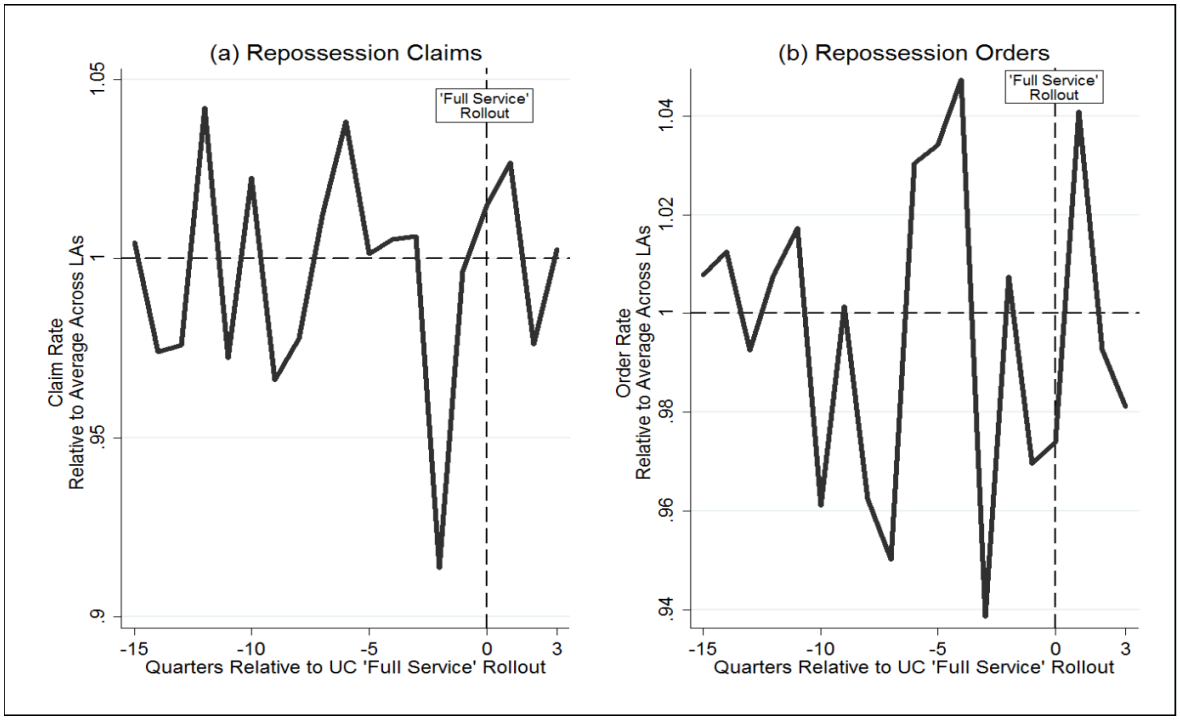

FIGURE A2.2. Quarterly trends in mean mortgage lender repossession rates (relative to the average across local authorities) in English local authorities, before and after UC 'Full Service' rollout. Notes: only includes data on the 136 local authorities with repossessions data available to the fourth quarter or more post 'Full Service' rollout. 'Full Service' rollout is the first quarter in which UC 'Full Service' was available in most Jobcentres in the local authority for most of the quarter. Y axes give the mean of the ratio between mortgage lender repossession rates and the average across the 136 local authorities in the given quarter.

Appendix 5: Chapter 6 Falsification Test

	Mortgage Arrears Advice Rate
UC 'Full Service' Rolled Out:	
[No]	
Yes	0.09
	(0.07)
Model Based Unemployment Rate	0.17***
	(0.03)
Per £100 increase in **Median Weekly Wages**	0.09
	(0.18)
Per £10,000 increase in **Average Selling Price of a Home**	0.11***
	(0.03)
Local Authority	
Quarters	6571
R^2	0.404

TABLE A5. Relationship between UC rollout and rates of CAB advice given on mortgage arrears within 323 English local authorities, 2014 Q1 - 2019 Q1. *Notes*: Driscoll-Kraay standard errors shown in brackets under coefficients. All models include local authority and (quarterly) time fixed effects. Mortgage arrears advice rate is per 10,000 owner occupied dwellings in the local authority. Median weekly wages includes both part-time and full-time work. +p<0.10, *p<0.05, **p<0.01, ***p<0.001.