Centre for Homelessness Impact

Encouraging landlords to let to people receiving benefits and at risk of homelessness: two online randomised controlled trials

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THE BEHAVIOURAL INSIGHTS TEAM

NRLA NATIONAL RESIDENTIAL LANDLORDS ASSOCIATION

Centre for
Homelessness Impact

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About the Centre for Homelessness Impact

The Centre for Homelessness Impact champions the creation and use of better evidence for a world without homelessness. Our mission is to improve the lives of those experiencing homelessness by ensuring that policy, practice and funding decisions are underpinned by reliable evidence.

About the Behavioural Insights Team

The Behavioural Insights Team (BIT) is one of the world's leading behavioural science organisations, working around the world to improve people's lives.

Through its teams in the UK, France, US, Canada, Australia, New Zealand and Singapore, BIT works in partnership with governments, local authorities, businesses and NGOs in over 30 countries, often using simple changes to tackle major policy problems and deliver improved public services and social outcomes.

BIT was established by the UK government in 2010 and in 2014 became an independent social purpose company, owned by the Cabinet Office, innovation charity Nesta, and BIT employees.

About the National Residential Landlords Association

The National Residential Landlords Association is the UK's largest membership organisation for private residential landlords, supporting and representing over 90,000 members. The association was created from the merger of the RLA and NLA in April 2020.

NRLA members range from full-time landlords running property portfolios to those letting single bedroom flats. Whatever their status, most landlords face the same regulatory and legal challenges, with over 100 Acts of Parliament and 400 sets of regulations governing the private residential lettings sector.

We help our members navigate these challenges and proudly offer some of the most comprehensive learning resources and market-leading intelligence available in the sector.

Foreword

As we work towards an evidence-led end to homelessness, we must consider all options for providing adequate, long term housing for those at risk of, or experiencing homelessness. That includes exploring all housing options, including the private rented sector (PRS).

Until now, it has been widely understood that there is some apprehension amongst private landlords to let properties to people in receipt of welfare benefits, including Universal Credit, but little research has been done into what can be done to change this. As the pandemic has led to a 100% increase in Universal Credit claimants, (rising from 3 million in March 2020 to more than 6 million in March 2021), it is more important than ever that we understand how to foster positive relationships between landlords and potential tenants.

Understandably, much research on homelessness focuses primarily on individuals experiencing homelessness, but if we are to truly improve their housing options we must also look to those who hold the keys to those homes: landlords. By undertaking this experiment, we have attempted to shift this paradigm by exploring ways to nudge landlords to let their properties to people who are at risk of homelessness.

Working alongside the Behavioral Insights Team (BIT) and the National Residential Landlords Association (NRLA) we undertook two online experiments to test whether disclosing additional information about a tenant increases the willingness of landlords to continue with the application of someone receiving Universal Credit and; which Local Authority incentives or support programmes are most effective at increasing the willingness of landlords to rent to someone receiving Universal Credit.

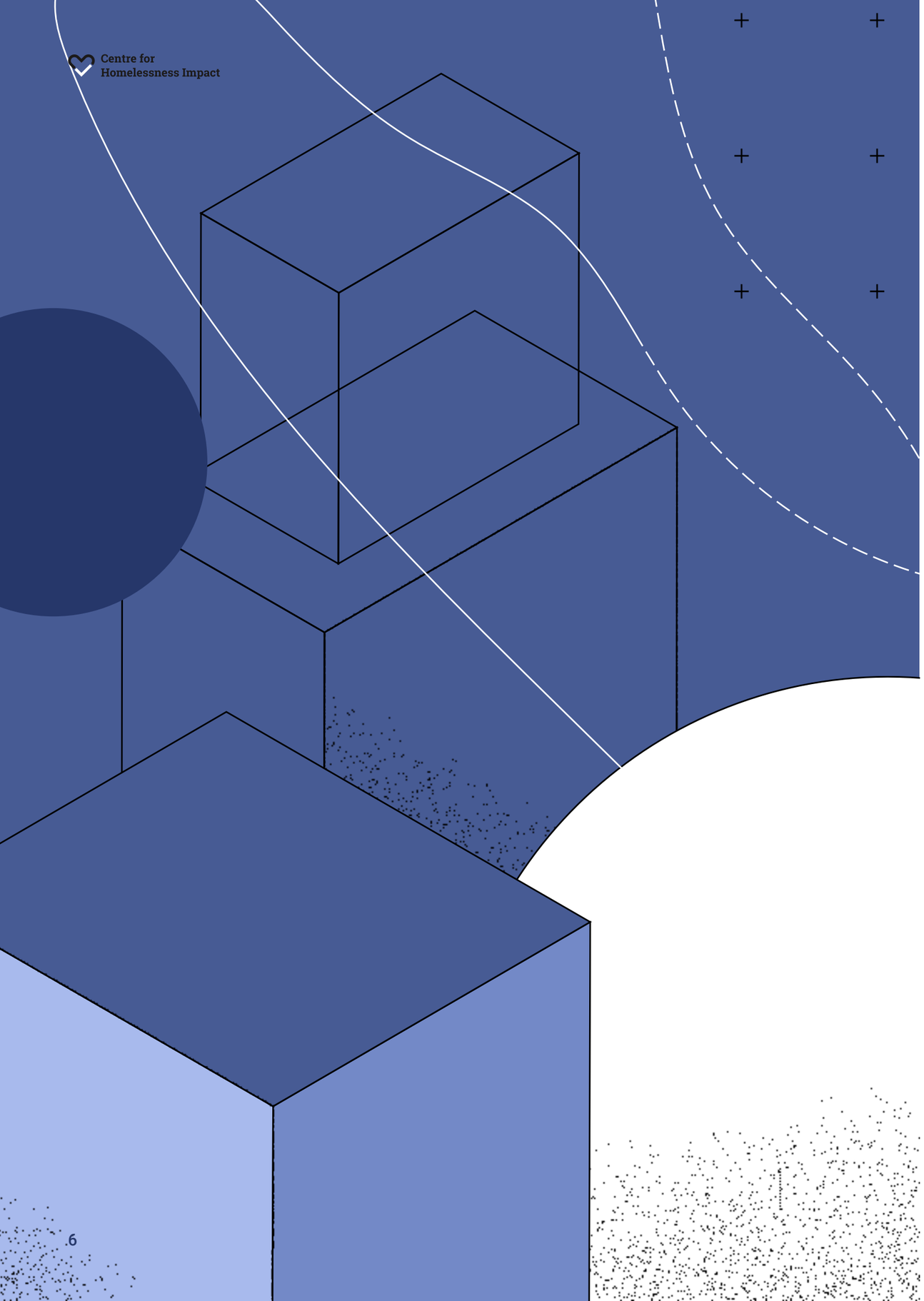
Most significantly, the study has shown that a rent guarantee and/or an upfront cash payment of £1,000 cash would significantly increase a landlord's willingness to rent to a potential tenant who was in receipt of Universal Credit.

However, the offer of budget planners, pre-tenancy training and alternative payment arrangements (APA) reported very similar willingness to rent to potential tenants as those who received no additional information. This suggests that these interventions are less effective at changing landlords' attitudes than previously expected.

The study also indicates that the willingness of landlords to rent to people at risk of homelessness remains low. When asked how likely they would be to accept a tenancy offer sent by a local council where a prospective tenant could only pay a lower rental price than advertised, landlords' mean response was around halfway between 'unlikely' and 'somewhat unlikely'. Even with the strongest interventions, landlords' reported willingness to let properties to people at risk of homelessness fell between 'somewhat unlikely' and 'neutral'.

Whilst it is clear there is no silver bullet, the report offers policy-makers, landlords and local authorities alike some promising insights into steps we can take to improve the private rented sector for those most in need of homes. We hope you find it useful.

Dr Lígia Teixeira
Chief Executive



Contents

1. Executive Summary

The Behavioural Insights Team (BIT) worked with the Centre for Homelessness Impact (CHI) and the National Residential Landlord Association (NRLA) to test interventions to encourage private landlords to rent to people receiving Universal Credit (UC).

The Private Rented Sector (PRS) plays a significant role in housing those who are experiencing homelessness. Housing people experiencing homelessness in the PRS is becoming increasingly important, as the social housing sector has been unable to keep up with demand. Nevertheless, according to the English Landlords Survey, half of PRS landlords say they are unwilling to rent to households that receive housing benefits or UC.

This report presents the results of two online randomised controlled trials. A total sample of 2,798 landlords who were registered with the NRLA participated. The landlords were randomised at the individual level in both trials. Each landlord went through both trials in succession. The order of the trials was randomly assigned upon entry, and treatment was randomly assigned for each trial independently of the other.

Trial 1, 'What to disclose' was a four-armed trial (n = 2798). The control arm was an email from a prospective tenant who received UC in a simulated rental scenario. The treatment email provided additional information about a prospective tenant. There were three treatment arms, each with additional information. The additional information included:

- A pre-tenancy training certificate;
- A budget planner; or
- A leaflet on alternative payment arrangements.

The primary outcome was landlords' self-reported likelihood of continuing with the tenant's application process, measured on a Likert scale. Secondary outcomes were: 1) landlords' perceived risk of late/missed rent payments, 2) landlords' perceived risk of damage to property and 3) landlords' perceived lack of communication, all measured on a Likert scale.

Trial 2, 'What council support is effective' was a four-armed trial (n = 2798). Landlords were presented with an email from a Local Authority (LA) housing officer asking landlords how likely they would be to accept a tenancy offer where the prospective tenant could only pay the Local Housing Allowance (LHA) rate, which was lower than the advertised rental price. The likelihood to rent at lower rent than advertised (without an incentive) acted as a baseline condition. This was followed by a treatment email that provided an updated offer with incentives (see Figure 2). There were four treatment arms. The updated offer with incentives included:

- £1000 cash upfront;
- Rent guarantee;
- Damage deposit bond; or
- Support from a landlord liaison officer.

The cash upfront treatment served as our comparison group. The primary outcome was the change in the landlords' self-reported willingness to accept the updated council's offer, compared to the willingness to accept the initial offer with no incentives. This was measured on a Likert scale. The secondary outcomes were: 1) landlords' perceived risk of late or missed rent payments, 2) landlords' expectation of responsiveness from the Council and 3) landlords' perceived risk of financial loss during tenancy, all measured on a Likert scale.

The results from Trial 1 showed that giving additional information about the tenant did not increase landlords' self-reported willingness to rent to a person receiving UC. While our treatment arms had substantial effects on landlord attitudes and expectations, this did not translate to higher willingness to proceed with the tenancy application.

The results from Trial 2 showed that providing cash upfront and providing a rent guarantee were the most effective at increasing the likelihood of accepting a tenant receiving UC at below-market rent. Offering cash upfront or a rent guarantee resulted in greater increases in landlords' likelihood to accept the offer (0.8 points on 1-7 scale) compared to offering a deposit bond or a landlord liaison officer (0.3 points). However, even with the two most effective incentives, landlords on the whole were not very inclined to accept the offers.

These trials are among the first online behavioural experiments with landlords in the UK. With the online approach, we were able to test interventions quickly and at a reduced cost to inform future field experiments and policy. It is important to note that the findings from these trials were based on hypothetical scenarios and a behavioural intention, rather than actual behaviour. This means there is a possibility that landlords may behave differently than our participants said they would if they encountered a similar situation in their real lives.

Taken together, our results showed that the willingness to rent to people receiving UC was low. The scenarios we presented in our trials were designed either to be affordable at the LHA rate or to compensate landlords for renting at a below-market rent. This demonstrates that a tenant's ability to afford rent is not the only barrier to overcome in encouraging landlords to rent to people who receive UC.

Both of our trials introduced landlords to a form of rent guarantee: either a leaflet with information on the circumstances under which a landlord can request that housing benefit is paid to them directly (alternative payment arrangements) or a written guarantee that the LA would cover for late or unpaid rent (rent guarantee). The alternative payment arrangements leaflet did not increase landlords' willingness to rent to someone receiving UC. However, the rent guarantee resulted in a relatively large increase. This suggests that securing on-time rent payments through rent guarantee schemes might be more effective than alternative payment arrangements.

We found that some commonly-used incentives (e.g. cash upfront and rent guarantee) may not be particularly strong selling points for encouraging landlords to rent to a person receiving UC. However, we found that incentives were more effective when the gap between market rent and the LHA rate was smaller (£50 compared to £200). This suggests that the closer LHA rates are to market rents, the more effective LA financial incentives are at encouraging landlords to rent to people receiving UC.

Based on these findings, we make the following recommendations:

- Offer UC recipients a choice of having the housing benefit portion of their UC payment transferred directly to their landlord;
- Combine financial incentives and behavioural interventions;
- Ensure LHA rates track market rents.

In addition, future research should collaborate with LAs to estimate 'cost per tenancy' for different programmes and incentives. Understanding cost effectiveness will help LAs decide which programmes are right for them as well as the benefits to scaling.

2. Background

Over the past few years, the number of homeless households (i.e. households owed a relief duty from their Local Authority) in England and Wales has remained relatively stable (note that legislative changes to homelessness duties mean it is hard to compare data over time).[1] However, the number of people placed into temporary accommodation in the UK has risen significantly over the past five years (driven mainly by increases in England).[2] People are placed into temporary accommodation when they have not been able to secure a permanent home. Living in temporary accommodation is suboptimal for everyone: conditions are often poor for residents and it is an expensive option for Local Authorities (LAs). Supporting households experiencing homelessnes into permanent housing is therefore a key priority.

The Private Rented Sector (PRS) plays a significant role in housing those who are experiencing homelessness.[3] Housing people experiencing homelessness in the PRS is becoming increasingly important, as the social housing sector has been unable to keep up with demand.[4] However, half of private landlords in England say they are unwilling to rent to people that receive housing benefits.[5] While there are structural reasons for this, such as the Local Housing Allowance (LHA) rate historically not covering the targeted 30th percentile of the PRS,[6] there are indications that some of the drivers for this may also be behavioural. For instance, research by the Bureau of Investigative Journalism found that half of landlords with a property affordable at the LHA rate refused to let to a single mother who received housing benefits.[7] This indicates that there may be beliefs and preconceptions among landlords about people who receive housing benefits that are not related to affordability alone. Having affordable housing options reduced due to discrimination can have severe consequences:

1 UK homelessness—Office for National Statistics. (2019, September 17). Retrieved 2 October 2019, from https://www.ons.gov.uk/peoplepopulationandcommunity/housing/articles/ukhomelessness/2005to2018#outcomes

2 ibid

3 While the PRS plays a larger role in housing homeless households in England and Wales than in Scotland, the PRS is increasingly important in Scotland's larger cities.

4 BBC (2020, February 26). Briefing Housing: BBC UK's. housing crisis (2020). Retrieved from: http://news.files.bbci.co.uk/include/newsspec/pdfs/bbc-briefing-housing-newsspec-26534.pdf

5 According to the English Private Landlord Survey 2018

6 The Chartered Institute of Housing (2019) found that, "in six out of every ten LHA rates, less than 15 per cent of accommodation is available and this is true for every LHA category and in every region and super-region except for the north of England." Note, however, that the LHA rate has been re-targeted to the 30th percentile of rents as part of the Government's COVID-19 support package.

7 The BIJ searched for 2 bedroom flats in the UK on 15.9.2019 using the Nestoria search engine. They captured 62,695 two-bed properties; of these, 3,497 properties would have been affordable at the LHA rate. They then contacted 180 landlords & agents associated with these properties posing as a single mother who is receiving housing benefits. (They do not describe how exactly they sampled the 180 properties). Half of the 180 contacted refused to consider the "single mother's" application.

Lack of housing options: In many parts of the country, there is a shortage of social housing and only a small share of the PRS is affordable at LHA rates.[8] This means that housing options are limited for households who receive housing benefits. If those options are further limited by discrimination, it means that households who receive housing benefits may be forced to move far from their place of work, far from a family member that they care for, or far from social networks.[9]

Longer search times: Facing discrimination from landlords who have affordable properties means that households who receive housing benefits need to spend relatively more time than other households looking for housing. A longer housing search can have both economic and emotional consequences: households are more likely to end up couch surfing or staying in temporary accommodation (rooms, often in hostels or bed & breakfasts, paid for by the local authority). Many people staying in temporary accommodation have reported substandard conditions and harrowing experiences.[10]

Mental health and wellbeing: Being at risk of or experiencing homeleness, facing rejections due to discrimination, or facing the prospect of having to leave your community and re-locate can contribute to increased stress, anxiety, and other negative mental health outcomes.[11]

8 The UK's Housing Crisis (BBC Briefing - Housing). (2020). BBC. http://news.files.bbci.co.uk/include/newsspec/pdfs/bbc-briefing-housing-newsspec-26534.pdf; Frozen out: The real value of the local housing allowance in the final year of the benefit freeze. (2019). Chartered Institute of Housing. http://www.cih.org/resources/Frozen%20Out.pdf

9 Lack of housing options for homeless households in London in particular often means they are asked to consider leaving London and moving to other parts of the country where housing is more affordable (such as Newcastle or Blackpool).

10 For more information on the experience of living in temporary accommodation, see Living in Limbo: London's temporary accommodation crisis. (2019). London Assembly Housing Committee. https://www.london.gov.uk/sites/default/files/temporary_accommodation_report_-_living_in_limbo_-_final.pdf and Williams, J. (2019, November 3). Broken people in a broken system, Manchester's forgotten families. Manchester Evening News. https://www.manchestereveningnews.co.uk/news/greater-manchester-news/broken-people-broken-system-manchesters-17192581

11 See Leng, G. (n.d.). THE IMPACT OF HOMELESSNESS ON HEALTH. Local Government Association. https://www.local.gov.uk/sites/default/files/documents/22.7%20HEALTH%20AND%20HOMELESSNESS_v08_WEB_0.PDF; Gompertz, S. (2020, February 23). Mind the gap? Scale of UK's housing shortfall revealed. BBC News. https://www.bbc.com/news/business-51605912

Many households experiencing homelessness receive some form of government benefit.[12] Given that landlords are hesitant to rent to people who receive benefits, this may be acting as a barrier to people experiencing homelessness accessing housing in the PRS. COVID-19 is likely to compound this issue further, as the number of people claiming benefits through universal credit (UC) rose by 2.7 million between March and October 2020, an increase of 90%.[13][14] Organisations such as Generation Rent, an advocacy organisation for PRS tenants, have warned that hundreds of thousands are falling behind on paying rent: tenants in the PRS were more likely than homeowners to suffer a loss of income during the pandemic, meaning many have fallen behind on rent payments.[15]

2.1 The role of local authorities in homelessness services

LAs have a responsibility (referred to as a duty) to help people find homes.[16] However, there are not enough properties in social housing to go around, even for those who qualify.[17] This means that LAs, particularly in England and Wales, must look to the PRS to help people find homes. LAs have two main routes in which they help people find homes in the PRS:

Supporting the search: helping the person experiencing homelessness to find a home in the PRS (if this is deemed an appropriate option).

Building relationships with landlords: getting contact information for landlords and agents who are willing to rent their properties to people that the LA is supporting.[18] For many LAs, this includes providing support or incentives to landlords who work with them.

12 Batty, E., Beatty, C., Rionach, C., Mike, F., McCarthy, L., & Kesia, R. (2015). Homeless people's experiences of welfare conditionality and benefit sanctions. Crisis & Sheffield Hallam University. https://shura.shu.ac.uk/14613/1/homeless-experiences-welfare-conditionality-benefit-sanctions.pdf

13 Universal Credit statistics: 29 April 2013 to 8 October 2020. (2020, November 10). GOV.UK. https://www.gov.uk/government/publications/universal-credit-statistics-29-april-2013-to-8-october-2020/universal-credit-statistics-29-april-2013-to-8-october-2020

14 Universal Credit (UC) is the benefits payment system for working age people in the UK. It has gradually been rolled out across the country since 2014. According to the House of Commons UC caseload tracker f 45% of people who receive housing benefits in Great Britain receive them through UC (as February 2020). The Government has committed to move all benefits claimants over to UC by 2024 (commitment as of June 2020). Because of this commitment, we are focusing this trial on receiving benefits through UC.

15 No home at risk: A recovery plan for private renters. (2020). Generation Rent. https://d3n8a8pro7vhmx.cloudfront.net/npto/pages/7372/attachments/original/1593549468/Gen_Rent_Recovery_plan_for_renters_July2020.pdf?1593549468

16 There are variations to this across the UK: in England, Wales & Scotland, LAs are responsible for homelessness services. In Northern Ireland, it is the Housing Executive.

17 The UK's Housing Crisis (BBC Briefing - Housing). (2020). BBC. http://news.files.bbci.co.uk/include/newsspec/pdfs/bbc-briefing-housing-newsspec-26534.pdf;

18 LAs often work with letting agents in the same way that they work with landlords. In this project, we focused on landlords rather than letting agents.

LAs want to find landlords who are willing to let their properties to people experiencing homelessness. By having these relationships, LAs can circumvent some of the search process and more quickly place people who are experiencing homelessness into homes in the PRS. Many LAs reach out to landlords to inform them about the opportunity to work with the LA to find tenants. LAs communicate the benefits of their offer (whether it is finding a tenant quickly, or additional incentives) through a variety of channels (see Table 1).

Table 1. Channels that LAs use to reach out to landlords.

Website	Most LAs have information about their landlord incentive and/or support programmes on website
Landlord forum	Some LAs have semi regular events with landlords, which could be organised by LA or jointly with a local landlord association
Newsletter	Some LAs send a newsletter to landlords. In places with a licensing scheme, this newsletter goes out to all landlords.
Social media	Some LAs used sites such as Facebook to communicate with landlords, however, it is difficult to target landlords specifically.
Traditional media	Some LAs place ads in local newspapers to reach landlords.
Word of mouth	Many LAs mentioned that they identify landlords through those they are already working with .

Many LAs offer landlords financial incentives and/or support programmes if they rent a property to someone through the LA. These vary in complexity, from paying a set amount of money to the landlord upon signing a tenancy contract, to providing in-depth support to the tenant throughout their tenancy. For example, the Dundee Homefinder service provides a key worker that meets with tenant at four points throughout the first year of their tenancy

Housing offices that work for LAs in and around London that we interviewed mentioned competing for affordable properties in the PRS, with some LAs offering increasingly higher financial incentives. Initiatives such as Capital Letters, which aims to bring LAs together in a shared service offerings, are working to ensure LAs collaborate on, rather than compete for, affordable housing (see Box 1).

Box 1: Case Study of Capital Letters

Capital Letters is a pan-London "accomodation procurement and management company" that is co-owned by 17 London Boroughs. Capital Letters aims to serve as a one-stop shop mediating between member boroughs and landlords/letting agents to house people experiencing homelessness in the PRS.

Capital Letters helps its member boroughs collaborate on a shared offering to landlords by agreeing on the incentives and services offered to landlords and agents. They also aim to find homes for people as locally as possible, helping, in their own words, "to reverse the trend in out-of-borough and out-of-London placements."

2.2 Behavioural barriers

Based on interviews with local authorities, landlords, and sector experts, we mapped the barriers to our target behaviour: encouraging private landlords to rent to households receiving UC. In our trials, we are testing different approaches to addressing the Capability, Opportunity and Motivation barriers shown in Table 2.

Table 2. Barriers to renting to households receiving Universal Credit addressed on our trials (following the COM-B model).[19]

Capability Knowledge & skills	Understanding of UC. Landlords may not be aware that they can, in certain circumstances, request that the housing benefit portion of UC is paid directly to them (for example, if the tenant is more than 2 months in rent arrears).
	Support from LA. Landlords may not feel that LAs provide adequate support if issues arise. This could be due to GDPR (landlords need tenant's consent to get help from LA), capacity issues at the LA, or poor coordination with job centres (who support on UC).
Motivation Reflective and automatic decisions	Previous bad experience. Landlords who have rented to a tenant receiving UC in the past may have had an unpleasant experience, either with the tenant themselves or when trying to get support from the LA or the job centre.
	Fear of a bad experience. Landlords may not have rented to a tenant receiving UC before, but they may have heard of (or from) other landlords who had bad experiences. Landlords were worried about missed or late payments from tenants not being able to manage finances on a tight budget.
Opportunity External factors in the environment	High demand for affordable lets. High demand and limited supply means landlords have multiple tenants to pick from for affordable properties. This means that, if landlords are concerned that tenants receiving HB won't be able to afford rent, there are many non-HB tenants to pick from instead.

19 Michie, Susan & van Stralen, Maartje & West, Robert. (2011). The Behaviour Change Wheel: a new method for characterising and designing behaviour change interventions. Implementation science : IS. 6. 42. 10.1186/1748-5908-6-42.

3. Intervention design and methodology

We partnered with the National Residential Landlords Association (NRLA) to conduct two online randomised controlled trials (RCTs) between July and October 2020.[20] An online trial is a trial that is conducted in a simulated environment as opposed to a field trial, which is conducted in real life. We chose to run online trials for the following reasons:

Quicker turnaround time: an online trial allows us to generate conclusions on effective interventions more quickly than a field trial. The timelines for this project were therefore more suited to an online trial than a full field trial.

Two trials instead of one: Running the trial in an online environment allows us to run more than one trial for a lower cost.

Simulated rental decisions: The aim of the project was to identify interventions to influence PRS landlord decisions on who they select as tenants. To measure this outcome in a field trial is not possible using routinely collected administrative data. An online trial allows us to collect data on this decision point in a simulated environment.

Proof of concept prior to running a field trial: Running an online trial allows us to test multiple trial arms to identify which intervention shows the most promise for testing in a field trial. In addition, an online trial helps us identify any potential backfire or unwanted effects before running a field trial (e.g. offering financial incentives might decrease intrinsic motivation and thus reduce the likelihood that the landlord is willing to rent to that tenant).[21]

Measure understanding and beliefs: On top of the main behavioural measure, an online experiment gives us an opportunity to measure understanding of the benefits system and subjective beliefs, which would be logistically more difficult to measure in a field trial.

20 The NRLA is a new organisation based on the merger of the National Landlords Association (NLA) and the Residential Landlords Association (RLA). Together, the NRLA has a mailing list of up to 70,000 landlords and letting agents.

21 Promberger M, Marteau TM. When do financial incentives reduce intrinsic motivation? comparing behaviors studied in psychological and economic literatures. Health Psychol. 2013 Sep;32(9):950-7. doi: 10.1037/a0032727. Erratum in: Health Psychol. 2013 Nov;32(11):1148. PMID: 24001245; PMCID: PMC3906839.

The experiments were conducted entirely online using Predictiv, BIT's platform for running online RCTs. We ran two trials jointly: Trial 1 What to disclose & Trial 2 What council support is effective. The order of the trials was randomly assigned upon entry, and treatment was randomly assigned for each trial independently of the other (see section 3.2 Experimental Design). Due to sample size considerations, participants completed both experiments in succession. Whilst we did not expect the order of experiments or trial arm combinations to influence outcomes in the other trial, we randomised the order of experiments and specified robustness checks and controls for linear contamination effects.

3.1 Sample

The NRLA provided access to their email list to recruit participants. The NRLA research team sent a link to the trial in an email to prospective participants inviting them to take part. When participants entered the trial, they were initially asked to identify whether they were a landlord, letting agent, or neither. Those who were not landlords were not allowed to continue with the survey. In addition, we excluded landlords whose primary property location was Scotland or Northern Ireland.[22] As an incentive for participating in the study, landlords were entered into a lottery to win one of two £250 vouchers to John Lewis.

We recruited 2,798 landlords who were registered with the NRLA (1,757 from RLA, 1021 from NLA, 20 unknown [who completed the experiment using an open survey link, after having met all screening criteria]). A total of 4,813 participants entered the experiment. After removing entries with incorrect referrer URLs, missing RIDs (which is a unique identifier piped through from the invitation URL), invalid or duplicate IP addresses, 3,841 entries remained. 3,000 landlords completed the survey. However, 202 of these did not have JavaScript enabled (meaning the materials didn't display for them) so we had to remove them, leaving 2,798 valid landlord responses.

3.2 Experimental design

3.2.1 Running two trials in a sequence

We ran two online RCTs: 1) 'What to disclose' and, 2) 'What council support is effective.' Each landlord went through both trials in succession. The order of the trials was randomly assigned upon entry, and treatment was randomly assigned for each trial independently of the other. Figure 1 shows the landlords' journey from entering the experiment. Landlords were randomly allocated to face either the Trial 1 'What do disclose' experiment or the Trial 2 'What council support is effective' experiment - going on to complete both. The landlord journey was as follows:

22 One of the interventions (the leaflet about alternative payment arrangements) is not applicable to tenancies in Scotland or Northern Ireland.

Landlords in Trial 1 'What to disclose' faced a fictional scenario and were randomly allocated to face an email from either a male (Mark) or female (Laura) prospective tenant, containing one of four possible interventions. In addition, participants were randomised to one of two possible UC contexts (standard or COVID-19). Landlords then were asked how likely they would be to continue Mark's (or Laura's) tenancy application.

Landlords in Trial 2 'What council support is effective' were randomly allocated into one of four intervention arms, and into one of two possible rental deficits subgroups. Each arm presented landlords the scenario of an initial offer regarding a UC tenant application (David) from a fictional council with no additional incentives. Landlords were asked how likely they would be to consider the council's offer. Then, landlords were presented with a follow-up email from the council with an updated offer including one of the four incentives. Landlords were then again asked how likely they would be to consider the council's offer, and thereby the tenant's application.

Landlords then faced several additional questions to capture characteristics that could explain their responses. Finally, they provided informed consent to submit their responses, before being thanked for taking part and the experiment ending.

Figure 1. Landlords' journey from entering the experiment.

3.2.2 Trial description

3.2.2.1 Trial 1: 'What to disclose'

In this trial, we tested different approaches to address the Capability and Motivation barriers we identified in Table 2, in particular: understanding of UC, previous bad experience and fear of bad experience. To understand how these barriers may be influencing participants' choices during the trial, we measured several secondary outcome measures that relate to these barriers.

Landlords were presented with a hypothetical rental scenario where they are advertising a one-bed property for £500 per month (see Annex 1, Fig. A1 for details on how we determined the rental scenario). They were shown an email exchange between them and a potential tenant who wanted to view the property. Landlords then read through the materials and decided if they wanted to continue with the prospective tenant's application process.

Landlords were randomly assigned to one of four trial arms: In the control condition, the prospective tenant writes that their main source of income is UC. In each of the treatment conditions, the tenant includes additional information in their email. These comparisons between the four types of conditions represent the main analyses for this trial.

An overview of the trial arms is provided in Table 3. To see intervention design materials see Annex 1, Fig. A2-A3.

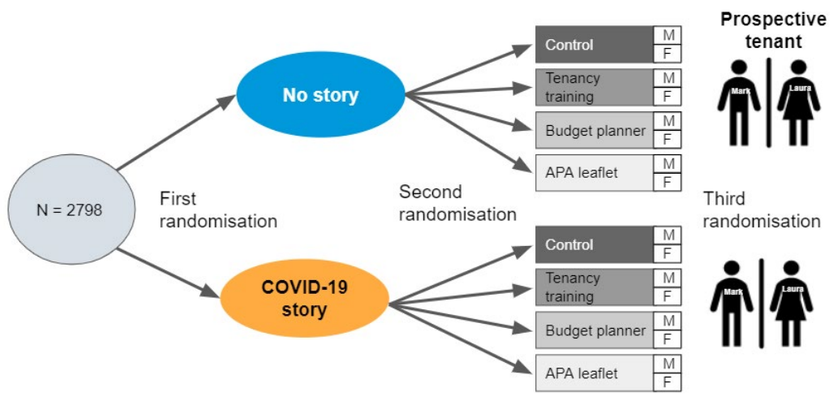
Table 3. Trial arms: 'What to disclose'

Condition	Operational definition23	Description
Control	UC status is disclosed with no additional information provided	N/A
Pre-tenancy training	UC status is disclosed and a pre-tenancy training certificate & course description is attached	A pre-tenancy course is a training programme offered by some local authorities (e.g. Manchester City Council)24 to teach people basic tenancy skills such as managing your money and taking care of a property.
Budget planner	UC status is disclosed and a budget planner is attached	Budget planners are designed to give users an accurate assessment of their finances to help them plan and make better financial decisions for the future. Currently, some councils offer tenants free courses on how to manage their finances (e.g. Dacorum Borough Council,25 Hull City Council).26
Alternative payment arrangements leaflet	UC status is disclosed and an information leaflet about alternative payment arrangements under UC is attached	Alternative payment arrange- ments are a programme available to tenants receiving UC in England and Wales.27 Under these arrangements, the Department for Work and Pensions transfers the housing benefit portion of the UC payment directly to the landlord, rather than sending the full UC payment to the recipient.

23 An operational definition is the specific way in which a variable is defined in a particular study.
24 Pre-tenancy training programme offered by Manchester City Council. Retrieved from https://secure.manchester.gov.uk/info/500319/care_leavers/7259/getting_your_own_home/2
25 Tenant Academy courses offered by Dacorum Borough Council. Retrieved from: http://www.dacorum.gov.uk/home/housing/current-tenant/tenant-academy
26 Training for tenants offered by Hull City Council. Retrieved from: http://www.hull.gov.uk/housing/council-tenants-and-leaseholders/training-tenants
27 In Scotland, people receiving UC have a choice as to whether they want the housing benefit portion of their UC payment to be transferred directly to their landlord (applies for both the private and social rented sectors).

For this trial, some landlords saw a woman's name as the tenant and others saw a man's name. The names were randomly assigned across all trial arms. We also introduced a 'COVID-19 story' as a moderating variable, as the COVID-19 crisis is changing the population of UC recipients.[28] Some landlords saw an email from a prospective tenant who does not reveal why he/she is receiving UC ('no COVID-19 story') while others saw an email from a person receiving UC because they lost their job in the wake of the COVID-19 lockdown ('COVID-19 story'). We randomised these conditions across trial arms. In our analysis, we then compared these two groups of scenarios. Note that these subgroup analyses were additional and likely to be underpowered.

Figure 2. Trial 1 'What to disclose' randomisation steps and trial arms.



The primary outcome measure was the likelihood to continue with the prospective tenant application process (Answer to the question: "What are the chances of you continuing with [tenant name] application process?") on a Likert scale of 1 (very unlikely) to 7 (very likely). To test the effect of our treatments on the stated likelihood of the landlord proceeding further with the tenant application, we used a linear regression model (for a list of covariates, see Annex 1: Outcome measures & covariates; for details on analytical strategy, see Annex 1: Analytical Strategy).

28 Universal Credit: 29 April 2013 to 9 April 2020. (2020, May 19). GOV.UK. Retrieved 21 May 2020, from https://www.gov.uk/government/publications/universal-credit-29-april-2013-to-9-april-2020/universal-credit-29-april-2013-to-9-april-2020

The secondary outcomes, using the same 1-7 scale, included:

- Perceived risk of late/missed rent payments: Answer to the question: "How likely do you think it is that [tenant name] would miss or be late on rent payments?"
- Perceived risk of damage to property: Answer to the question: "How likely is it that [tenant name] would take adequate care of the property?"
- Perceived lack of communication: Answer to the question: "How likely do you think it is that [tenant name] would get in touch with you if he/she struggles to pay the rent?"

In addition, we measured understanding of UC and alternative payment arrangements using a composite score of answers to 3 questions regarding UC & alternative payment arrangements (ranging from 0-100%):

- "Everyone receiving Universal Credit is unemployed." [True/False/Don't know]
- "Tenants who receive Universal Credit payments are expected to manage their own budget, including rent." [True, False, Don't know]
- "There are no circumstances under which landlords can request that the housing benefit portion of Universal Credit payments are paid directly to them." [True, False, Don't know]

3.2.2.2 Trial 2: 'What council support is effective'

In this trial, we tested different approaches to address the Capability and Opportunity barriers we identified in Table 2, in particular: support from LA and high demands for affordable lets. To understand how these barriers may be influencing landlords' choices during the trial, we measured several secondary outcome measures that relate to these barriers.

Landlords were presented with a hypothetical scenario where they are advertising a one-bed property. They were shown an email sent to them from a LA housing officer requesting that they consider renting their property to a person, but there is a gap between the local housing allowance (LHA) and rent asked. They read through the email and indicated how likely they would be to consider renting their property to that person. The likelihood to rent at a lower price than advertised (without an incentive) acted as our baseline condition.

The scenario then continued with a follow-up email from the housing officer, offering one of four possible incentives. Landlords were then asked for their updated likelihood of considering renting to this person. The incentives which served as our treatment conditions are summarised in Table 4. (The cash upfront treatment served as a comparison group in our analysis). We selected these four incentives based on interviews with LA housing options teams, interviews with landlords, and consultations with sector experts.[29] To see intervention materials, see Annex 2, Fig. A2.1-A2.2.

Table 4. Trial arms: 'What council support is effective'

Condition	Operational definition	Description
Cash upfront	Council offers landlord £1000 upfront cash payment in return for a 12 month assured shorthold tenancy (AST) with a monthly rent of [price below market rent].	In cash upfront incentive schemes, councils offer a sum of money for signing a tenancy agreement with a prospective tenant (e.g. Enfield Council).[30]
Rent guarantee	Council offers guaranteed rent paid directly to landlord on a monthly basis in return for a 12 month assured shorthold tenancy (AST) with a monthly rent of [price below market rent].	In rent guarantee incentive schemes, councils offer a written guarantee to the landlord that they will cover for late or unpaid rent. In our fictional scenario, the rent guarantee ensured that the landlord would be paid a below market rent that matched the LHA rate. In exchange, the landlord would accept the lower rate as rent. However, in the real world this can vary. For example, the rent guarantee offered by Oxford City Council can be up to £103 a month above the LHA rate.[31]

29 While there is no comprehensive information on the types of incentives (and combinations of incentives) that local authorities offer, our desk research suggests that cash upfront and rent guarantee are more common among London Boroughs, followed by deposit bonds. Many LAs offer support of some kind, but a dedicated support officer for landlords does not appear to be as common.

30 Cash upfront scheme offered by Enfield Council. Retrieved from https://centralhousinggroup.com/landlords-guaranteed-rent-scheme/let-to-the-council/let-to-enfield-council-area-guide/enfield-council-homefinder-scheme/

31 Rent guarantee scheme offered by Oxford City Council. Retrieved from: https://www.oxford.gov.uk/news/article/652/council_rent_guarantee_pilot_scheme_benefits_landlords_and_tenants

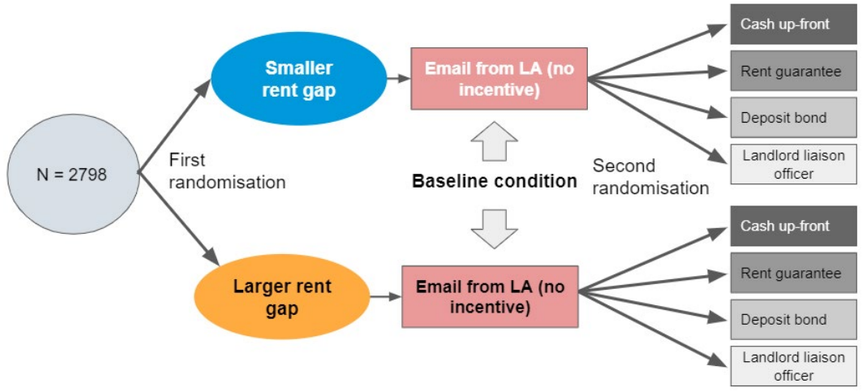
Condition	Operational definition	Description
Deposit bond	Council offers a £500 bond in return for a 12 month assured shorthold tenancy (AST) with a monthly rent of [price below market rent].	In the deposit bond incentive schemes, councils offer bonds to cover any costs a landlord may incur during the course of the tenancy. For example, Isle of Wight Council offers a damage bond of £2,000 per tenancy for any damage exceeding fair wear and tear.32
Landlords liaison officer	Council offers support through their landlord liaison officer in return for a 12 month assured shorthold tenancy (AST) with a monthly rent of [price below market rent].	A landlord liaison officer is a dedicated resource that acts as a single point of contact for private landlords who need support with a tenancy (e.g. West Berkshire Council).33

For this trial, we introduced 'rent gap size' as a moderating variable. The rental gap size is defined as the difference between the listed rent of the property and the maximum weekly rent payable under the LHA rate. We vary the size of the gap because people in different parts of the country are facing different gaps between what is available on the market and what is affordable when receiving UC. In this trial, we wanted to test if incentives and support have an impact both for smaller and larger rental gaps. The smaller rental gap was £50 while the larger rent gap was £200 (for an explanation on how we determined these amounts, see Annex 2: Determining rental gap sizes)

32 Deposit bond scheme offered by Isle of Wight Council. Retrieved from: https://www.iow.gov.uk/Residents/care-and-Support/Housing/Homelessness-Prevention-Landlord-Information/Find-a-Home-Scheme

33 Landlord liaison officer support offered by West Berkshire Council. Retrieved from: https://info.westberks.gov.uk/article/28817

Figure 3. Trial 2 'What council support is effective' randomisation steps and trial arms.



The primary outcome measure was the change in likelihood in accepting the council's updated offer, measured as the change in willingness to accept the updated council's offer, compared to an initial baseline council offer with no additional incentives (Answer to the questions: "How likely would you be to rent your property to [tenant name]?") with answers on a Likert scale ranging from 1 (very unlikely) to 7 (very likely). To test the effect of our treatments on our primary outcome, we used a linear regression model, supplemented with a robustness check using a non-parametric model (for a list of covariates, see Annex 2: Outcome measures & covariates; for details on analytical strategy, see Annex 2: Analytical Strategy).

The secondary outcomes, using the same 1-7 scale, included:

- Perception of risk of late or missed rent payments: Answer to the question: "How likely do you think you would be to experience late or missed rent payments from [tenant name]?"
- Expected responsiveness from the Council: Answer to the question: "How likely is it that the council would reply to your emails promptly?"
- In addition, we estimated the perceived risk of financial loss during tenancy (answer to the Question: "How much profit would you expect to make on this tenancy?") measured on a Likert scale ranging from 1 (far below average) to 7 (far above average).

4. Results

4.1 Demographic characteristics of the sample

Our participants were about equally male and female. The proportion of male and female landlords that participated in our experiments (50% male and 50% female) was different from the proportions that the NRLA typically receive for their surveys (male 60% and female 40%). The 60% male and 40% female split is also more representative of their membership population as a whole.[34] [35] Participants also had many years of experience with being a landlord (63.4% of the sample had 10+ years of experience) and had properties across all regions of England and Wales (though only a small minority had properties in Scotland or Northern Ireland). Only 18.7% of participants said that they owned a property under the LHA rate, though 32.6% didn't know. Moreover, 51.9% of our participants had experience with tenants on UC and 17.8% said they had worked with a Local Authority to find a tenant (for a full list of characteristics, see Annex 1).

4.2 Trial 1: 'What to disclose'

Headline findings

Primary outcome analysis

- Average likelihood to continue with the tenant's application was low across all treatment arms (halfway between 3 "Somewhat unlikely" and 4 "Neutral" on a 7-point Likert scale).
- Giving landlords additional information did not increase landlord willingness to rent to a person receiving UC (pre-tenancy training: M = 3.59, ES = 0.15 on 1-7 Likert scale, CI 95% [-0.04, 0.33]; budget planner: M = 3.43, ES = -0.04 on 1-7 Likert scale, CI 95% [-0.22, 0.15]; APA leaflet: M = 3.44, ES = -0.02 on 1-7 Likert Scale, CI 95% [-0.19, 0.16]).
- Landlords who had i) experience renting to tenants receiving UC, ii) properties at the LHA rate; and iii) fewer years of experience were more likely to continue with the application.

34 State of the PRS - 2019, Quarter 3: https://www.nrla.org.uk/research/quarterly-reports/2019/qtr-3.

35 It is possible that the opportunity to enter a lottery to win one of two £250 vouchers from John Lewis may have skewed the proportions in our experiment (the NRLA do not provide incentives for completing their surveys). Alternatively, female landlords might be more interested in renting to UC tenants, which may have contributed to fewer women withdrawing consent when the research aim was revealed. (We found that female landlords were more willing to rent to UC tenants; however, this result was correlational).

Secondary outcomes analysis

- Expectation of late or missed payment
  - The mean stated expectation of late or missed payment for the control group was 4.61, corresponding to halfway between "Neutral" and "Somewhat likely" on a 7-point Likert scale.
  - The APA leaflet decreased expectation of late or missed payment compared to the control group but differences were small (M = 4.45, ES = -0.16 on 1-7 Likert scale, CI 95% [-0.29, -0.03], P < 0.05). Neither the pre-tenancy training nor the budget planner made a substantial difference for this outcome.
- Perceived level of communication
  - The mean stated likelihood for perceived level of communication for the control group was 4.08, corresponding to "Neutral" on a 7-point Likert scale.
  - The pre-tenancy training and budget planner increased perceived level of communication by less than half a point between "Neutral" and "Somewhat likely" (pre-tenancy training: M = 4.50, ES = 0.43 on 1-7 Likert scale, CI 95% [0.28, 0.57], P < 0.01; budget planner: M = 4.43, ES = 0.35 on 1-7 Likert scale, CI 95% [0.20, 0.49], P < 0.01).
- Perceived level of care for the property
  - The mean stated likelihood for perceived level of care for the property for the control group was 4.47, corresponding to halfway between "Somewhat likely" and "Neutral" on a 7-point Likert scale.
  - The pre-tenancy training and budget planner increased perceived level of care but differences were small (pre-tenancy training: M = 4.68, ES = 0.22 on 1-7 Likert scale, CI 95% [0.09, 0.34], P < 0.01; budget planner: M = 4.66, ES = 0.19 on 1-7 Likert scale, CI 95% [0.07, 0.32], P < 0.01).
- Understanding of UC and APA
  - We observed no significant differences between the control and treatment arms.
  - Subgroup analysis (COVID-19 story)
  - Likelihood to continue with tenant's application
    - For those who saw the COVID-19 story, the pre-tenancy training substantially increased the likelihood they would continue with a tenant's application (M = 3.77, ES = 0.34 on 1-7 Likert scale, CI 95% [0.07, 0.62], P < 0.05).
- Exploratory analysis
  - The APA leaflet increased awareness about the possibility of directly receiving the housing benefit portion of Universal Credit payments (ES = 8.84 percentage points [log-OR = 0.32], CI 95% [0.09, 0.55], P < 0.01).
  - Landlords in the pre-tenancy training group were more likely to rent to a UC tenant if the tenant was female (ES = 0.345 on 1-7 Likert scale, CI 95% [0.02, 0.76], P < 0.05).
  - Landlords in the pre-tenancy training group that clicked through the attachments were more likely to continue with the tenant's application than those who did not click on the attachment (ES = 0.49 on 1-7 Likert scale, CI 95% [0.22, 0.75], P < 0.01).
  - We repeated the main analyses looking only at those landlords that said they had properties available at the LHA rate. As with the results for the whole sample, giving landlords additional information did not increase landlord willingness to rent to a person receiving UC (pre-tenancy training: ES = 0.05 on 1-7 Likert scale, CI 95% [-0.40, 0.50]; budget planner: ES = 0.01 on 1-7 Likert scale, CI 95% [-0.44, 0.46]; APA leaflet: ES = -0.09 on 1-7 Likert Scale, CI 95% [-0.51, 0.33]).

4.2.1 Primary outcome analysis[36]

Average willingness to rent to people receiving UC was low across all treatment arms

In the primary analysis, we asked landlords how likely they would be to continue with the prospective tenant's application. The answer was given on a 7-point scale from "Very unlikely" to "Very likely", which was analysed as a continuous variable. Table 5 shows the descriptive results: in all arms, the mean stated likelihood was around 3.50, i.e. halfway between the options "Somewhat unlikely" and "Neutral", indicating that participants were, on the whole, not very inclined to continue with the application.

36 Reported significance stars in primary and secondary analyses outputs have been separately adjusted for multiple comparisons in line with the Benjamini-Hochberg step-up procedure. In this trial there are 4 key statistical comparisons in the primary analysis, and 18 across the secondary analyses.

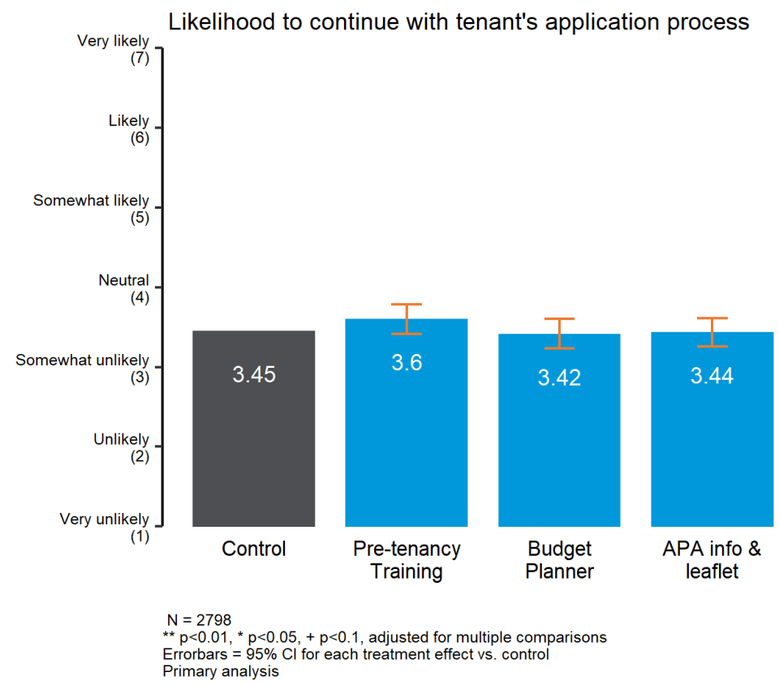
Table 6. Descriptive statistics of the primary outcome variable.

	Control (n = 674)	Pre-tenancy training (n = 664)	Budget planner (n = 679)	APA leaflet (n = 781)
Outcome	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**
Likelihood to continue tenant's application	3.45 (1.79)	3.59 (1.76)	3.43 (1.79)	3.44 (1.75)

Giving landlords additional information about a prospective tenant did not increase landlord willingness to rent to a person receiving UC

As shown in Figure 4, none of the intervention arms had a substantial effect on the stated likelihood of continuing with the application (i.e. the largest difference was between the control and the pre-tenancy training and this was less than 0.15 points in a 7-point scale). None of these differences were statistically significant (pre-tenancy training: M = 3.59, ES = 0.15 on 1-7 Likert scale, CI 95% [-0.04, 0.33]; budget planner: M = 3.43, ES = -0.04 on 1-7 Likert scale, CI 95% [-0.22, 0.15]; APA leaflet: M = 3.44, ES = -0.02 on 1-7 Likert Scale, CI 95% [-0.19, 0.16]). We also did not find a substantial effect of the inclusion of COVID-19 as the reason for the applicant receiving UC (M = 3.54, ES = 0.11 on 1-7 Likert scale, CI 95% [-0.02, 0.24]).

Figure 4. Effect of treatment on the likelihood to continue with the application.



Landlords who had i) experience renting to tenants receiving UC, ii) properties at the LHA rate; and iii) fewer years of experience were more likely to continue with the application

We observed an association between some covariates and the primary outcome that reached statistical significance at the 95 % level (i.e. we rejected the null hypothesis of no association). Note, however, that these results are correlational and not corrected for multiple comparisons so should only be taken as tentative.

- There were 1,461 landlords with experience renting to tenants on UC. These landlords were more likely to continue with the application (ES = 0.49 on 1-7 Likert scale, CI 95% [0.34, 0.64], P < 0.001).

- There were 533 landlords with properties at or below LHA rate. These landlords were more likely to continue with the application (ES = 0.31 on 1-7 Likert scale, CI 95% [0.10, 0.51], P < 0.01).

- Landlords with more years of experience (5-10 years: ES = 0.46 on 1-7 Likert scale, CI 95% [0.25, 0.67], P < 0.001; +10 years: ES = 0.59 on 1-7 Likert scale, CI 95% [0.40, 0.78], P < 0.001,) were less likely to continue with the application than those with less experience. Note that, since we didn't adjust for age, years of experience may be a proxy for it here.

- Seeing Trial 1 What to disclose after Trial 2 What council support is effective may have slightly increased landlords' stated likelihood of continuing with the application: all point estimates were positive (ranging from 0.10 to 0.26 points on 1-7 Likert scale), with the effect of the damage deposit bond arm being significant (E = 0.26 on 1-7 Likert scale, CI 95% [0.04, 0.46], P < 0.05).

- Female landlords stated they were more likely to continue with the application than male landlords (ES = 0.14 on 1-7 Likert scale, CI 95% [0.01, 0.26], P < 0.05).

Effects of treatment arms were broadly similar for landlords with properties at or below the LHA rate, compared to the full sample of landlords

We repeated the primary analysis for the subgroup of 533 landlords with properties at or below the LHA rate. This additional piece of analysis was exploratory, and was not pre-specified, but we felt that it was important to check for differences in treatment effects among this policy-relevant subgroup. We found no substantial differences between interventions, and coefficient estimates were similar to the main analysis (pre-tenancy training: ES = 0.05 on 1-7 Likert scale, CI 95% [-0.40, 0.50]; budget planner: ES = 0.01 on 1-7 Likert scale, CI 95% [-0.44, 0.46]; APA leaflet: ES = -0.09 on 1-7 Likert scale, CI 95% [-0.51, 0.33]).

4.2.2 Robustness check

We used a 1-7 point Likert scale to determine whether landlords would rent to UC tenants and to measure attitudes and beliefs about UC tenants. The table below shows the Likert scale ranging from 'Very likely' to 'Very unlikely' and its corresponding values.

Table 5. Seven point Likert scale cardinal values

1: Very unlikely	2: Unlikely	3: Somewhat unlikely	4: Neutral	5: Somewhat likely	6: Likely	7: Very likely

In our main analyses, we treated the rating-scale variables as if they were continuous variables. To explore a potentially skewed distribution of residuals, we ran the Mann-Whitney U test as a robustness check[37]. The results were qualitatively consistent with the main analysis: none of the treatments, nor the UC context, have a significant effect after correcting p-values for multiple comparisons (see Tables A1.7 and A1.16 in Annex 1).

4.2.3 Secondary outcome analysis

We asked landlords three additional questions about the presented tenancy application, all aimed at capturing potential mechanisms through which the treatments could have an effect. These questions were about:

- landlords' expectations of late or missed rent payments;
- their expectations about communication; and
- their expectations about the tenant taking adequate care of the property.

Additionally, we quizzed participants with three questions about universal credit (UC) and alternative payment arrangements (APA) legislation; their scores on these questions were combined into a single 'Understanding of UC and APA' variable.

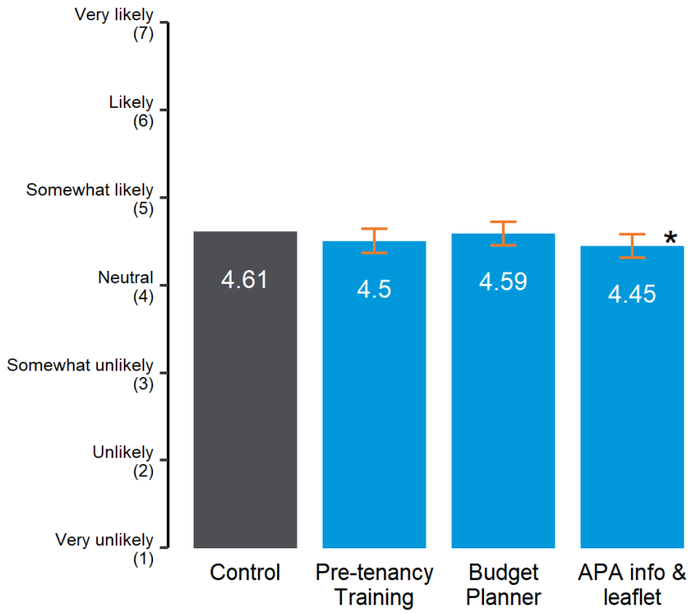
37 De Winter, J. F. C., & Dodou, D. (2010). Five-Point Likert Items: t test versus Mann-Whitney-Wilcoxon (Addendum added October 2012). Practical Assessment, Research, and Evaluation, 15(1), 11. This article focuses on the discussion of parametric and nonparametric testing methods for analysing 5-point Likert scale measures. Whilst this trial uses 7-point Likert scales, this is not necessarily at odds with the recommendations of the article; the authors simply acknowledge that 7-point scales can typically accommodate more "skewed and wider-tailed" distributions.

Expectation of late or missed payments was lower for landlords in the APA leaflet group, but differences were small

The mean stated expectation of late or missed payments for the control group was around 4.6 (i.e. halfway between "Somewhat likely" and "Neutral" on a 7-point Likert scale). The APA leaflet decreased the expectations of late or missed rent payments but differences were small (M = 4.45, ES = -0.16 on 1-7 Likert scale, CI 95% [-0.29, -0.03], P < 0.05). None of the other arms had a statistically significant effect, though all point estimates were negative. We had originally hypothesized that the APA leaflet would decrease the expectations for late payments. However, it was also possible it could increase these expectations because the leaflet draws attention to the limited circumstances under which APA applies: including when a tenant is in arrears for two months.

It seems landlords in the APA trial arm may have interpreted this in a more literal sense (i.e. awareness of APA decreased expectations of missed rent payments slightly compared to other treatment arms).

Figure 5. Effect of treatment on the perceived likelihood of missed or late rental payments by the prospective tenant.



N = 2,798
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95%% CI for each treatment effect vs. control
Secondary analysis

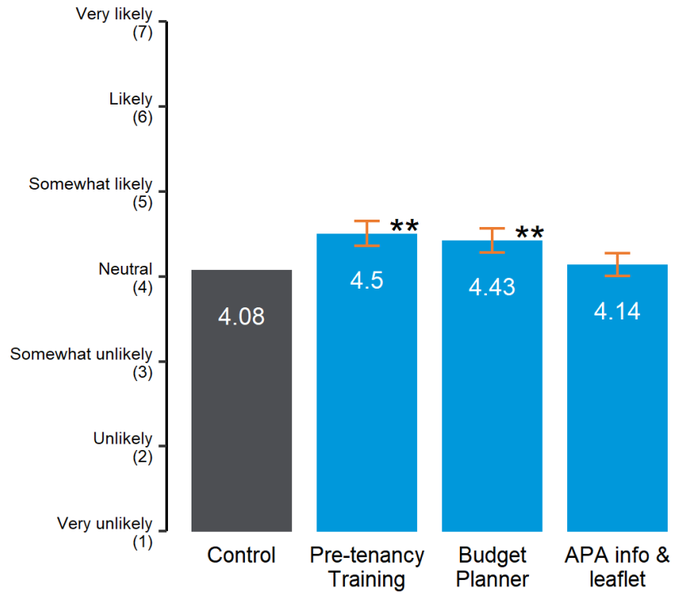
Perceived level of communication was substantially higher for landlords in the pre-tenancy training and budget planner groups

The mean stated likelihood for perceived level of communication for the control group was 4.08 corresponding to "Neutral" on a 7 point Likert scale. The pre-tenancy training and budget planner substantially increased this outcome by less than half a point between "Neutral" and "Somewhat likely" (pre-tenancy training: M = 4.50, ES = 0.43 on 1-7 Likert scale, CI 95% [0.28, 0.57], P < 0.01; budget planner: M = 4.43, ES = 0.35 on 1-7 Likert scale, CI 95% [0.20, 0.49], P < 0.01). Although we had hypothesised that the pre-tenancy training would have a positive impact on this outcome variable, we had not expected the budget planner to also have an impact.

The APA leaflet intervention had no significant effect.

In addition, landlords in the COVID-19 story subgroup reported that the tenant was more likely to communicate effectively but these differences are small (M = 4.20, ES = 0.12 on 1-7 Likert scale, CI 95 % [0.02, 0.22], P < 0.05).

Figure 6. Effect of treatment on the perceived likelihood of the prospective tenant communicating if he /she struggles to pay the rent.



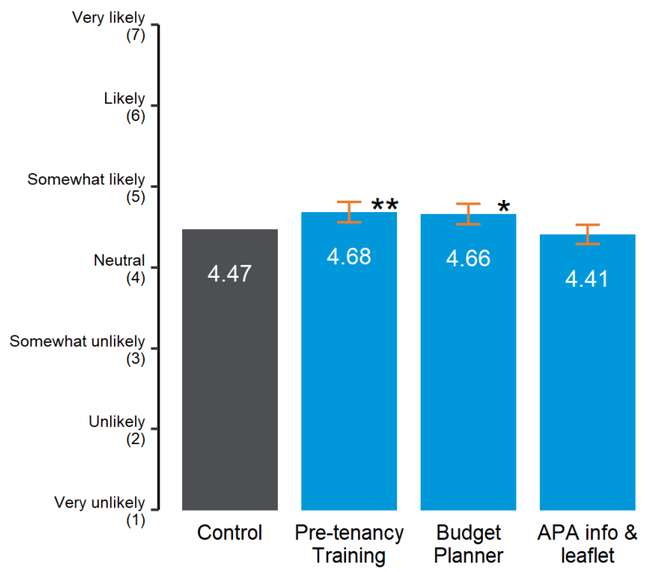
N = 2,798
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95%% CI for each treatment effect vs. control
Secondary analysis

Perceived level of care for the property was higher for landlords in the pre-tenancy training and budget planner groups

The mean stated likelihood for perceived level of care for the property for the control group was 4.47 corresponding to halfway between "Somewhat likely" and "Neutral" on a 7-point Likert scale. The pre-tenancy training and budget planner increased this outcome but differences were small (pre-tenancy training: M = 4.68, ES = 0.22 on 1-7 Likert scale, CI 95% [0.09, 0.34], P < 0.01; budget planner: M = 4.66, ES = 0.19 on 1-7 Likert scale, CI 95% [0.07, 0.32], P < 0.01). We expected the effect of the pre-tenancy training on this outcome, however we did not expect the budget planner to increase it too.

In addition, landlords in the COVID-19 story subgroup reported that the tenant was more likely to take care of the property but these differences are small (M = 4.60, ES = 0.10 on 1-7 Likert scale, CI 95% [0.02, 0.19], P < 0.05).

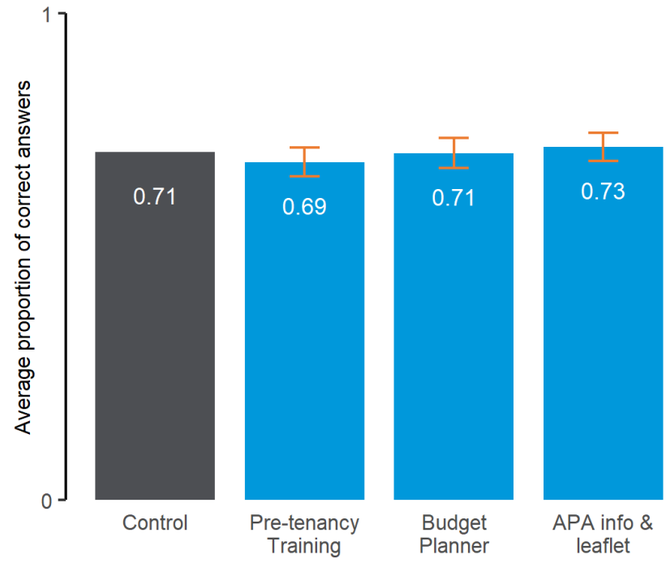
Figure 7. Effect of treatment on the perceived likelihood of the tenant applicant taking adequate care of the property



N = 2,798
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95%% CI for each treatment effect vs. control
Secondary analysis

No treatment had a significant effect on participants' understanding of UC and APA.

Figure 8. Effect of treatment on the understanding of UC and APA.



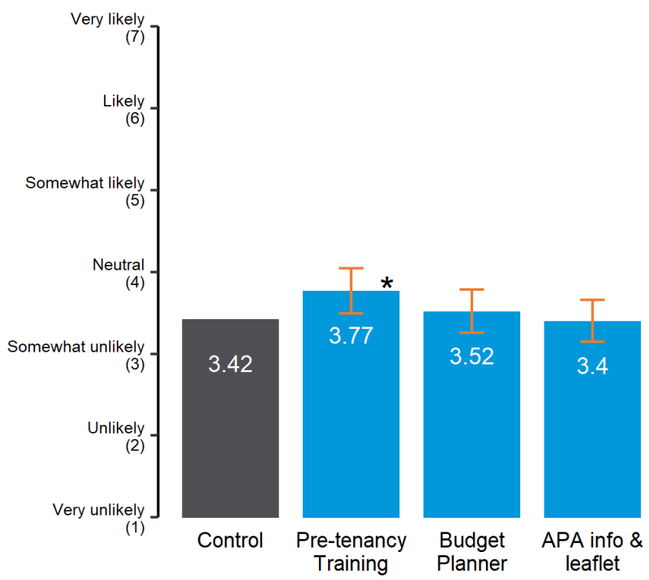
N = 2,798
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95%% CI for each treatment effect vs. control
Secondary analysis

4.2.4 Subgroup analysis of the primary outcome

Landlords were randomly split into two subgroups, one presented with an email providing no explanation for why the prospective tenant is on UC (n = 1446) and the other including an additional sentence stating that the reason is the COVID-19 pandemic (n = 1351; see Annex, Fig. A1.3). We tested whether our treatments had an impact on the primary outcome variable, separately within these two subgroups.

In the COVID-19 story subgroup, the mean likelihood to continue with the application was 3.42, corresponding to halfway between "Somewhat unlikely" and "Neutral" on a 7-point Likert scale. The pre-tenancy training increased this outcome (M = 3.77, ES = 0.34 on 1-7 Likert scale, CI 95% [0.07, 0.62], P < 0.05). Neither of the other two treatments had a significant effect.

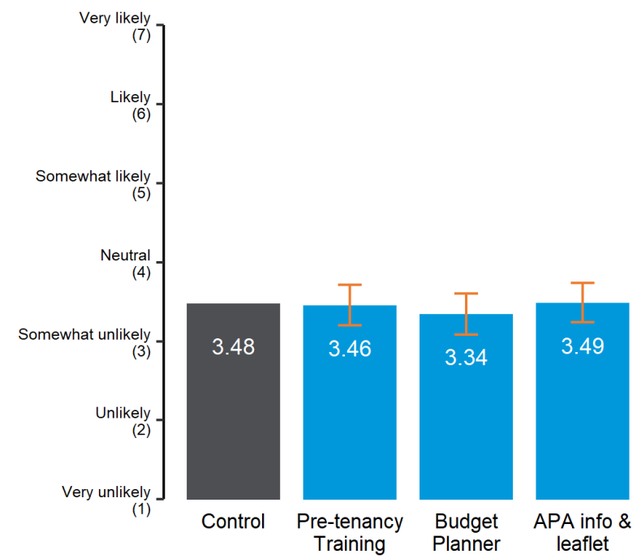
Figure 9. Effect of treatment on the likelihood to continue with the application, for the tenant who lost their job due to COVID-19.



N = 1,352
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95%% CI for each treatment effect vs. control
Secondary analysis

In the subgroup without the COVID-19 story, we observed no significant treatment effects.

Figure 10. Effect of treatment on the likelihood to continue with the application, for the tenant who did not reference COVID-19.



N = 1,446
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95%% CI for each treatment effect vs. control
Secondary analysis

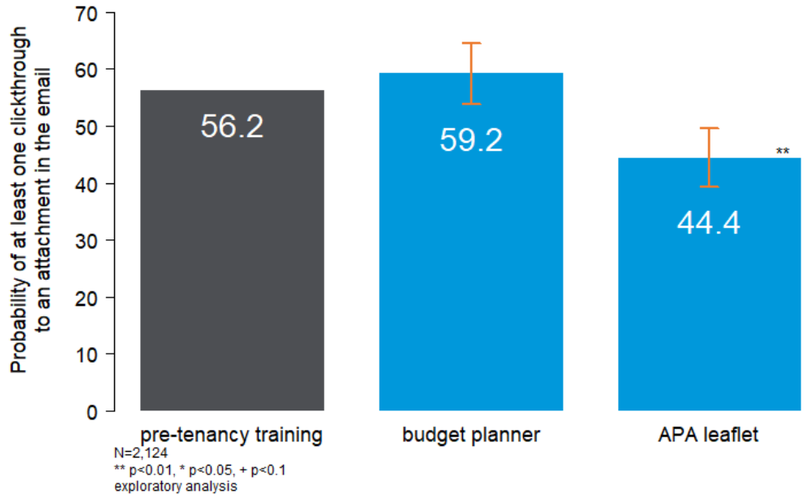
4.2.5 Exploratory analysis

Clickthroughs

Landlords in the APA leaflet group were less likely to click through to the attachment

In each of the intervention arms, the email from the hypothetical applicant contained attachments. In this analysis, we look at whether the rates at which participants clicked on these attachments varied across arms. As shown in Figure 11, there was no significant difference between the clickthrough to attachments in the pre-tenancy training and budget planner arms, both showing clickthrough rates of slightly under 60%. However, there was a substantially lower clickthrough rate in the APA leaflet arm (M = 44.4, log-OR = -0.47 , CI 95% [-0.68, -0.26], P < 0.01,).

Figure 11. Effect of treatment on the probability of clicking through to at least one email attachment.



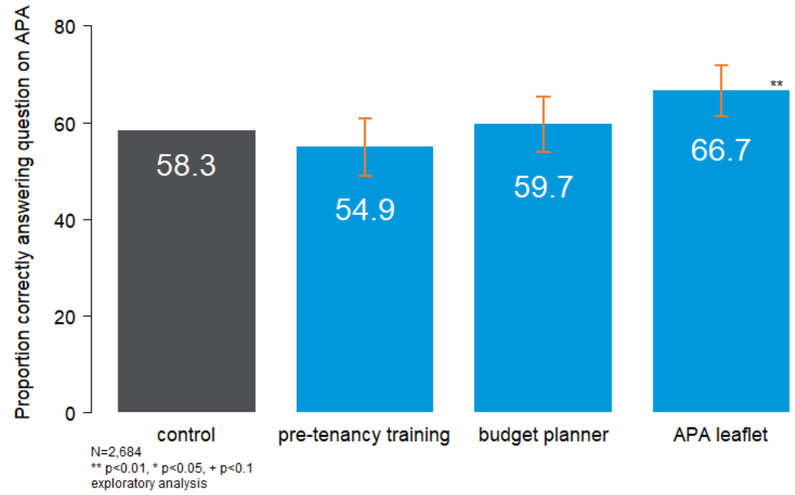
Understanding of alternative payment arrangements (APA)

Landlords in the APA leaflet arm were more likely to answer a question about APA correctly

In our secondary analysis, we observed no significant effects from any of the interventions on overall understanding of UC and APA, measured across 3 questions. However, only one of these questions was directly related to the content of an intervention: the APA comprehension question ("There are no circumstances under which landlords can request that the housing benefit portion of Universal Credit payments be paid directly to them." [True, False, Don't know]). Therefore, we have tested here whether there were also null effects on correctly answering this question specifically.

Using a logistic regression model, we found that the arm which referenced APA, and attached an APA leaflet, substantially increased awareness about the possibility of directly receiving the housing benefit portion of UC payments (ES = 8.84 percentage points [log-OR = 0.32], CI 95% [0.09, 0.55], P < 0.01). In the control group, around 58% of landlords were aware of this arrangement; in the APA arm, 67% were. The other arms did not differ significantly from the control.

Figure 12. Effect of treatment on correctly answering the APA comprehension question.



Estimating treatment effects for those who clicked through to the attachments

Landlords in the pre-tenancy training group that clicked through the attachments were more likely to continue with the tenant's application

In order to test whether effects on our primary outcome varied depending on opening treatment-specific email attachments, we ran an extended form of our primary-analysis model with additional terms for interactions between treatment indicators and clicking on an attachment. Compared to the control, the magnitude of the effect for pre-tenancy training strongly depended on whether the landlord viewed the attachment (ES = 0.49 on 1.7 Likert scale, CI 95% [0.22, 0.75], P < 0.01). Interestingly, the interaction effect for the APA arm was negative; however, this was not significant when evaluated against the control mean, suggesting there is no evidence of a backfire effect from the APA leaflet (i.e landlords opening the attachment being less likely to continue with the tenant's application; we hypothesised this could have happened by making more salient that the tenant needed to be in arrears to be eligible for APA). These results, however, must be interpreted tentatively, since estimates from such an interaction model are likely inhibited by low statistical power with our sample size.

Applicant profile-treatment interaction

The tenant's gender influenced landlord willingness to rent in the pre-tenancy training arm

We did not observe a significant main effect of the tenancy applicant's name (i.e. being called Mark instead of Lucy) on the stated likelihood of continuing the application process. However, we found a differential treatment effect of the pre-tenancy training intervention whereby the treatment effect was significantly lower for Mark than for Lucy (ES = -0.39 on 1-7 Likert scale, CI 95% [-0.76, -0.02], P < 0.05). As a result, the effect of pre-tenancy training intervention was positive on when the tenant was Lucy (ES = 0.35 on 1-7 Likert scale, CI 95% [0.07, 0.61], P < 0.05). The significant effect for Lucy is likely driven by the fact that it comprises two smaller, otherwise non-significant effects, namely the main effect of the pre-tenancy training intervention and of the female name.

4.2.6 Discussion

Overall, we found that giving landlords additional information about a prospective tenant did not increase landlord willingness to rent to a person receiving UC. While our treatment arms had a significant effect on landlord attitudes and expectations, this did not translate to higher willingness to proceed with the tenancy application. For example, neither the pre-tenancy training nor the budget planner increased landlord willingness to rent overall. This was despite a significant shift in landlord expectations about the tenant's communication skills and taking care of the property. This was surprising, as the landlords we spoke to during our fieldwork interviews emphasised that a tenant's ability to communicate was key to tenancy decisions, and LAs we spoke to emphasised that landlords feared tenants may not take care of the property. These findings could imply that communications skills and care are not that important for landlord decision making in comparison to paying on time, an outcome for which we saw no change. Another interpretation is that changing expectations about these aspects on their own are not sufficient to change landlords' decisions. Our interpretation of these results are that adding information such as a budget planner or tenancy training signals to the landlord that the tenant is proactive, but does not necessarily convince landlords that they've developed enough skills in budgeting and financial management to pay what should be an affordable rent on time.

Another interesting finding in our trial was that certain types of landlords were more likely to continue with the application. Note, however, that these results are correlational and not corrected for multiple comparisons so should only be taken as tentative:

Landlords with experience of renting to tenants who receive UC were more likely to continue with the application than those without experience

Landlords with 5-10 and 10+ years of experience were less likely to continue with the application than those with 0-5 years of experience.

It is important to note that these estimates are statistical associations and do not necessarily warrant a causal interpretation. For example, it may be that landlords with experience renting to tenants receiving UC are generally more open to low income tenants, rather than that the experience of renting to a tenant receiving UC causes a landlord to be more willing to do so again in the future. While measuring this causal relationship may be difficult, LAs may want to consider whether these insights can be included in targeting which landlords to reach out to for use as peer mentors.[38] Peer mentors could share their experiences renting to tenants receiving UC to encourage other landlords to do the same.

While we found no significant effect of our intervention on the primary outcome, the subgroup and exploratory analysis revealed that pre-tenancy training had a significant effect on willingness to rent for certain groups:

COVID-19 subgroup analysis: In half of our sample, the prospective tenant explained that the reason they received UC was because they lost their job during the COVID-19 pandemic. We found that the pre-tenancy training programme had a significant effect on likelihood of continuing with the application in this COVID-19 subgroup. This subgroup finding suggests that combining a signal of tenant commitment (through pre-tenancy training) with information about the context of the tenant's unemployment (a factor out of their own control) can influence a landlord's rental decision. It would be interesting to test the impact of pre-tenancy training with other reasons for receiving UC, to see if this effect is unique to the COVID-19 pandemic or whether it applies to other contexts.

Tenant profile exploratory analysis: Half of our sample received an email from a prospective tenant named Lucy, while the other half received an email from Mark. The pre-tenancy training programme had a significant effect on the likelihood of continuing with the application when the prospective tenant was named Lucy. This could be because landlords believe female tenants are more likely to benefit from pre-tenancy training (e.g. more likely to have retained what they learned) or because of a general preference for female tenants (e.g. females are tidier).

Attachment exploratory analysis: Landlords who clicked to view the pre-tenancy training attachment were more likely to continue with the application than those who did not. This association is not necessarily causal: for example, landlords who were more positive about pre-tenancy training programmes may have been more likely to click on the link.

38 Peer mentoring is a relationship between people who are at the same career stage, in which one person has more experience than the other in a particular domain and can provide support as well as knowledge and skills transfer (for example, see: Bryant SE. The Impact of Peer Mentoring on Organizational Knowledge Creation and Sharing: An Empirical Study in a Software Firm. Group & Organization Management. 2005;30(3):319-338. doi:10.1177/1059601103258439).

Taken on their own, each of these findings is not enough to recommend pre-tenancy training, given the overall null result. But looking at these findings together suggests that pre-tenancy training was the most promising of the interventions we tested. It may be worth exploring landlords' perceptions of pre-tenancy training (for example, through qualitative research) and using those findings to pilot a 'landlord-informed' pre-tenancy training programme. An important caveat is that while none of these interventions seemed to improve landlord's willingness to rent, it does not mean that tenants might not accrue other benefits from these types of training.

The findings from this trial were based on a hypothetical scenario and a behavioural intention, rather than actual behaviour. This means there is a possibility that landlords may behave differently than our participants said they would if they encountered a similar situation in their real lives. In general, we would expect to see larger effect sizes in online trials than we would in the real world. Therefore, the null result for this trial should be taken as an indication that light-touch information interventions on their own may not be enough to overcome the barriers to encourage landlords to rent to someone who receives UC.

4.3 Trial 2: 'What council support is effective'

Headline findings

Primary outcome analysis

- Landlords were, on the whole, not very inclined to accept offers of cash upfront, rent guarantee or landlord liaison officer support; across all groups, the mean change in stated likelihood of accepting the offer was around 0.57, which typically corresponded to a halfway movement from responses "Unlikely" to "Somewhat unlikely" on a 7-point Likert scale.

- The mean likelihood to accept the offer at baseline was 2.39, or around halfway between "Unlikely" and "Somewhat unlikely." All of the treatment arms increased this outcome.

- Our primary outcome was the average increase in the willingness to accept the offer (difference between willingness with incentive offer and with no offer). The cash upfront incentive offer resulted in an almost 1-point increase from the baseline (ES = 0.91 on 1-7 Likert scale, CI 95% [0.77, 1.04]).

- The change in willingness to accept the offer was substantially lower when offering deposit bond (ES = -0.43 on 1-7 Likert scale, CI 95% [-0.55, -0.31], P < 0.01) or landlord liaison officer support (ES = -0.51 on 1-7 Likert scale, CI 95% [-0.62, -0.40], P < 0.01) compared to cash upfront. There was no significant difference in the increase in willingness to accept the offer when comparing the rent guarantee to cash upfront (E = 0.03 on 1-7 Likert scale, CI 95% [-0.10, 0.16], P = 0.65).

Secondary outcomes analysis

- Expectation of missed or late payments over the tenancy

    - The mean stated expectation of late or missed payments for the cash upfront group was 4.62, corresponding to halfway between "Somewhat likely" and "Neutral" on a 7-point Likert scale.

    - The rent guarantee group substantially decreased this outcome compared to cash upfront (M = 4.07, ES = -0.56 points on a 1-7 Likert scale, CI 95% [-0.73, -0.39], P < 0.01).

- Expected level of profit from the proposed tenancy

    - Expected level of profit from the proposed tenancy was substantially higher in the cash upfront group compared to all the other groups (mean was 2.97, roughly corresponding to "A bit below average" on 1-7 Likert scale).

- All of the other treatment groups substantially decreased this outcome by roughly half a point between "A bit below average" and "Below average" (rent guarantee: M = 2.65, ES = -0.31 on 1-7 Likert scale, CI 95% [-0.42, -0.19], P < 0.001; damage deposit bond: M = 2.44, ES = -0.52 on 1-7 Likert scale, CI 95% [-0.64, -0.40], P < 0.001; landlord liaison officer: M = 2.53, ES = -0.47 on 1-7 Likert scale, CI 95% [-0.59, -0.35], P < 0.001).

- Expected level of responsiveness by the council
- We observed no significant differences between the cash upfront arm and the other trial arms.
- Subgroup analysis
- As expected, the change in the likelihood to accept the offer from baseline was greater in the smaller rent gap condition (£50) than in the larger rent gap condition (£200)*:
- Cash upfront: smaller rent gap = 0.91-points increase; larger rent gap = 0.66-points increase.
- Rent guarantee: smaller rent gap = 1.00-point increase; larger rent gap = 0.64-points increase.
- Damage deposit bond: smaller rent gap = 0.43-points increase; larger rent gap = 0.26-points increase.
- Landlord liaison officer: smaller rent gap = 0.34-points increase; larger rent gap = 0.21-points increase.
- We repeated the main analyses looking only at those landlords that said they had properties available at the LHA rate and also found that the offers did not increase landlord willingness to rent to a person receiving UC, compared to the cash up-front offer. These results were very similar to those of the main analyses for the whole sample (rent guarantee: ES = -0.06 on 1-7 Likert scale, CI 95% [-0.45, 0.33]; deposit bond: ES = -0.60 on 1-7 Likert scale, CI 95% [-0.93, -0.27]; landlord liaison officer: ES = -0.62 on 1-7 Likert Scale, CI 95% [-0.92, -0.31]).

*We didn't test this difference statistically

4.3.1 Primary outcome analysis[39]

Landlords were, on the whole, not very inclined to accept the offers

In the primary analysis, we asked landlords how likely they would be to accept a tenancy offer sent by the Council, where the prospective tenant could only afford to pay a lower rental price than advertised (amounting to the housing benefit portion of their UC income). The answer was given on a 7-point scale from "Very unlikely" to "Very likely". Mean likelihood was 2.39, or around halfway between "Unlikely" and "Somewhat unlikely." Then, we asked landlords how likely they would be to accept an updated tenancy offer, the details of which varied by treatment arm. This was measured on the same 7-point scale; the mean across all arms was 2.95, roughly corresponding to "Somewhat unlikely" (see Table 7 for mean values of each arm). For our primary analysis, we took the change in likelihood to accept the tenancy offers as our outcome.

Table 7. Descriptive statistics of the primary outcome variable

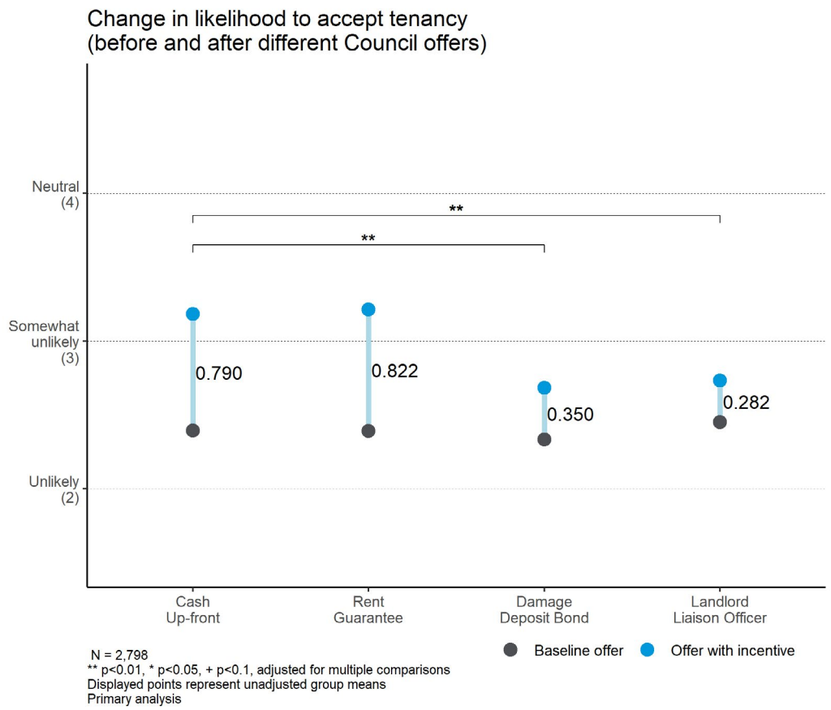
	Cash upfront **n=701**	**Rent guarantee** **n=692**	**Damage deposit bond** **n=678**	**Landlord liaison officer** **n=727**
Outcome	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**
Change in likelihood to accept council's offer	0.79 (1.31)	0.82 (1.26)	0.35 (0.95)	0.28 (0.83)

Change in the willingness to accept the offer from baseline was substantially larger in the cash-upfront and rent guarantee groups than in the damage deposit bond and landlord liaison officer groups

39 Reported significance stars in primary and secondary analysis outputs have been separately adjusted for multiple comparisons in line with the Benjamini-Hochberg step-up procedure. In this trial there are 4 key statistical comparisons in the primary analysis, and 15 across the secondary analyses.

All treatment arms increased the likelihood to accept the offer from baseline (see Mean and SD on Table 7). Cash upfront and rent guarantees both showed an improvement from the baseline of around a 0.8. Compared to cash upfront, the change in the likelihood to accept the offer was substantially lower for the damage deposit bond (ES = -0.43 on 1-7 Likert scale, CI 95% [-0.55, -0.31]) and landlord liaison officer groups (ES = -0.51 on 1-7 Likert scale, CI 95% [-0.62, -0.40]) The rent guarantee group did not differ significantly from the cash upfront group (ES = 0.03 on 1-7 Likert Scale, CI 95% [-0.10, 0.16], P = 0.65).

Figure 13. Change in the stated likelihood of accepting the offer, in each treatment arm.



Some covariates in the regression model also showed significant associations with the primary outcomes. For instance, seeing this trial after the What to disclose trial may have had an effect on the change in likelihood of accepting the council's offer (all point estimates were positive ranging from 0.07 to 0.17 points), with the effect of the pre-tenancy training arm becoming significant at the .05 level. This means that for half the sample that saw the other trial first, their likelihood to have a favourable response here was inflated. However, these would not qualitatively change the findings that cash upfront and rent guarantees were more effective than the deposit bonds and landlord liaison officer.

In addition, female landlords showed slight increases in the likelihood of accepting the offer by 0.08 points on 1-7 Likert scale (CI 95% [0, 0.16]). However, this result was not statistically significant (P = 0.07).

4.3.2 Robustness check

We used a 1-7 point Likert scale to determine whether landlords would rent to UC tenants and to measure attitudes and beliefs about UC tenants and LA support. In our main analyses (primary and secondary), we treated the rating-scale variables as if they were continuous variables. To explore a potentially skewed distribution of residuals, we ran a Mann-Whitney U test as a robustness check. The results were qualitatively consistent with the main analysis: the same treatments, as well as the larger rent deficit, have a significant effect after correcting p-values for multiple comparisons (see Table A2.7 in Annex 2).

4.3.3 Secondary outcomes analysis

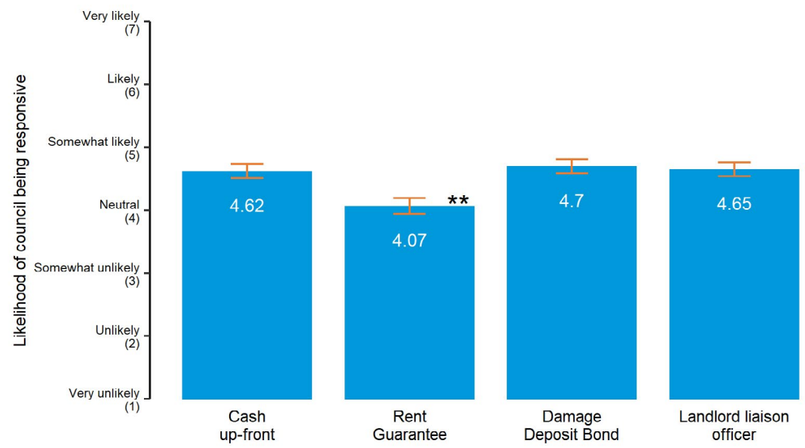
We asked participants three additional questions about the presented tenancy, all aimed at capturing potential mechanisms through which the treatments could have an effect. These questions were about:

- Participants' expectations of late or missed rent payments;
- their expectations about the council's responsiveness; and
- their expectations about the level of profit obtained from the tenancy.

Expectation of missed or late payments over the tenancy was substantially lower in the rent guarantee group than in the cash upfront group

The mean stated expectation of late or missed payments for the cash upfront group was 4.62, corresponding to halfway between "Somewhat likely" and "Neutral." The rent guarantee group substantially decreased this outcome (M = 4.07, ES = -0.56 on a 1-7 Likert scale, CI 95% [-0.72, -0.39], P < 0.01). None of the other groups differed significantly from the cash upfront group. However, the larger gap between the advertised rental price and the amount of housing benefit afforded by the tenant on UC (larger gap = £200) increased the perceived likelihood of late or missed payments (M = 4.58, ES = 0.14 on a 1-7 Likert scale, CI 95% [0.02, 0.26], P < 0.05). Overall, these results are not qualitatively surprising; the magnitudes of effect sizes for the rent guarantee arm versus the higher rent-UC gap suggests the guarantee is very effective at changing landlord perceptions on this dimension.

Figure 14. Effect of treatment on the perceived likelihood of late or missed rent payment by the prospective tenant.

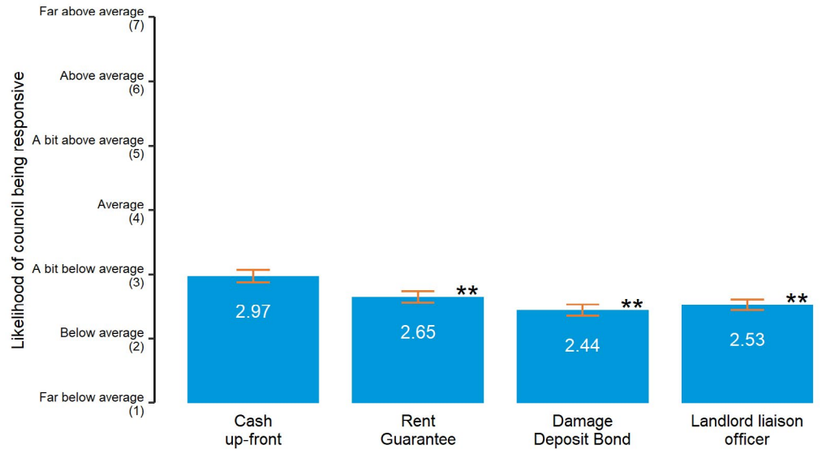


N = 2,798
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95% CI for treatment group means
Secondary analysis

Expected level of profit from the proposed tenancy was substantially higher in the cash upfront group compared to all the other groups

The mean expected level of profit for the cash upfront group was 2.97, roughly corresponding to "A bit below average" on 1-7 Likert Scale. All of the other treatment groups, substantially decreased this outcome by roughly half a point between "A bit below average" and "Below average" (rent guarantee: M = 2.65, ES = -0.31 on 1-7 Likert scale, CI 95 % [-0.42, -0.19], P < 0.001; damage deposit bond: M = 2.44, ES = -0.52 on 1-7 Likert scale, CI 95 % [-0.40, -0.64], P < 0.001; landlord liaison officer: M= 2.53, ES = -0.47 on 1-7 Likert scale, CI 95 % [-0.59, -0.35], P < 0.001). Interestingly, the larger rent-UC monthly deficit (£200) had the largest estimated effect, indicating that none of the three interventions could outweigh this perceived financial cost.

Figure 15. Effect of treatment on the expected level of profit from the tenancy.

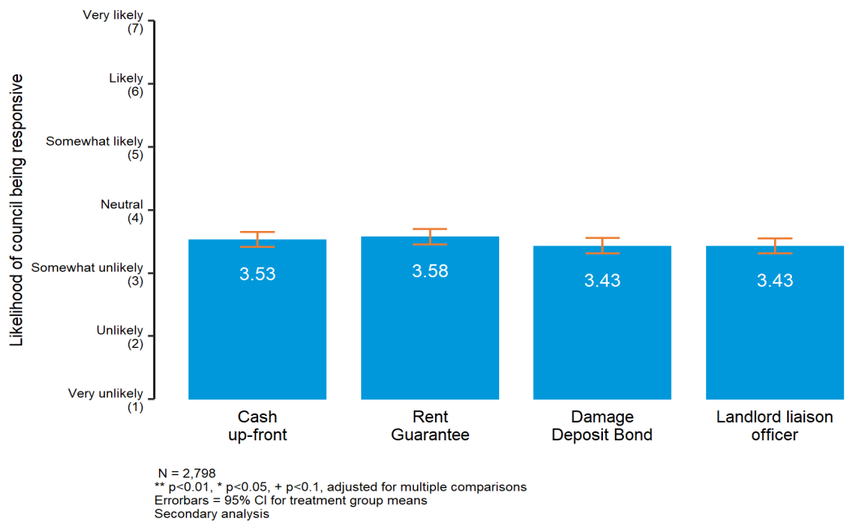


N = 2,798
** p<0.01, * p<0.05, + p<0.1, adjusted for multiple comparisons
Errorbars = 95% CI for treatment group means
Secondary analysis

None of the treatments made a difference for the expected level of responsiveness from the council

We observed no significant differences between the cash upfront arm and the other trial arms on the perceived likelihood of the council being responsive outcome. We had hypothesized that the landlord liaison officer would at least improve landlords' perceptions about dealing and communicating with the Council; however, this was not the case.

Figure 16. Effect of treatment on the perceived likelihood of the Council being responsive.



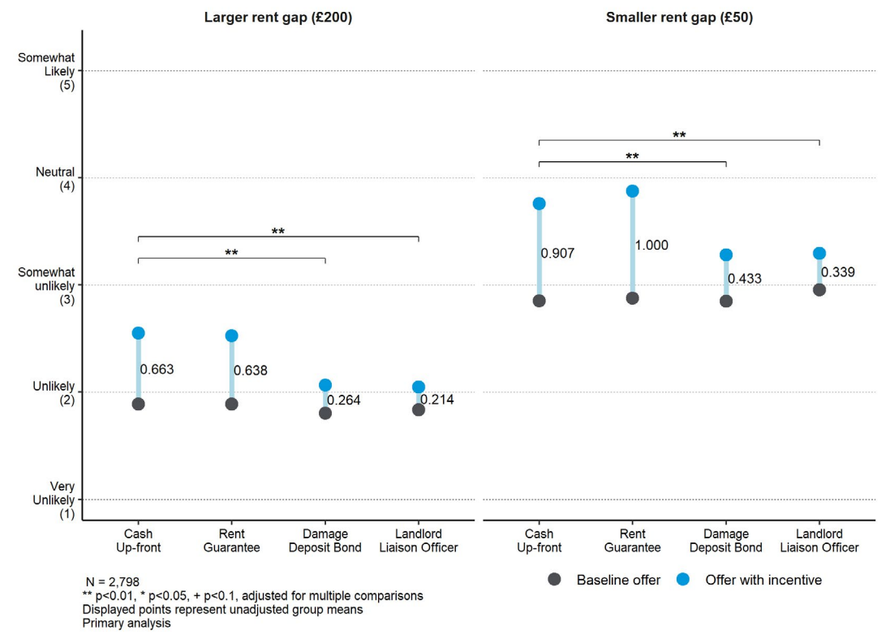
4.3.4 Subgroup analysis of the primary outcome

The change in the willingness to accept the offer from baseline was greater in the smaller rent gap condition (£50) than in the larger rent gap condition (£200)

Landlords were randomly split into two subgroups, one presented with a tenancy offer for a monthly rental price of £50 below the advertised amount, since the portion of housing benefits from UC was insufficient to cover the full rent; the other group faced a tenancy offer for a larger rental price deficit (£200 a month). In these analyses, we test whether our treatments had an impact on the primary outcome variable separately within these two subgroups. In both subgroups, the results showed the same pattern as in the whole-sample analysis: there was no significant difference between the cash upfront and the rent guarantee arms but the damage deposit bond and landlord liaison officer both showed significantly smaller increases in the likelihood to accept the offer than cash upfront.

The effects of the incentives, however, were all greater in the smaller deficit subgroup than in the larger deficit subgroup: for instance, cash upfront caused a 0.91-point increase (CI 95% [0.77, 1.04]) in the smaller deficit group but only 0.66-point increase (CI 95% [0.52, 0.80]) in the larger deficit group. This suggests that the treatments are more effective when the rent deficit is small (although we didn't test this difference statistically).

Figure 17. Effect of treatment on the change in likelihood to accept the tenancy, for the two rent gap conditions: larger rent gap (£200) in the left and smaller rent gap (£50) in the right. Stars represent significant differences to the respective cash upfront subgroup.



The effects of the incentives were similar for landlords with properties at or below LHA rate, compared to the full sample

We also ran the primary analysis for the subgroup of 515 landlords with properties at or below the LHA rate. This additional piece of analysis was exploratory, and was not pre-specified, but we felt that it was important to check for differences in the effects of the types of incentives among this policy-relevant subgroup. Again, we found significantly smaller increases in willingness to accept the offer in the damage deposit bond (ES = -0.60 on 1-7 Likert scale, CI 95% [-0.93, -0.27] and landlord liaison officer (ES = -0.62 on 1-7 Likert scale, CI 95% [-0.92, -0.31] incentives compared to cash upfront. We are cautious in making further interpretations from this analysis due to the very low sample size (n = 533), but there is descriptive evidence suggesting there are not strong differential incentive effects for this particular subgroup.

4.3.5 Discussion

In this trial, we found two incentives to be the best performing out of the four: providing £1,000 cash upfront and providing a rent guarantee were the most effective at increasing likelihood of accepting a tenant receiving UC at below-market rent. This was true for both the small and large rent gaps; however, the effects of the incentives were larger for the smaller rent gap. This finding – that incentives are more effective when the gap between market rent and the LHA rate is smaller – emphasises the importance of keeping the LHA rate in line with market rents. Failure to do so could result in LA incentive programmes being less effective.

Even with the two most effective incentives, landlords were still on average 'somewhat unlikely' to continue with the application. However, it is worth noting that these incentives impacted secondary outcomes: offering a rent guarantee significantly reduced landlords' expectations of missed or late rent payments, and offering cash upfront significantly increased expected profit. Overall, these results suggest that making profit and paying rent on time are important but not sufficient drivers to encourage landlords to rent to people receiving UC. This emphasises the need of addressing non-financial barriers such as the perceptions and beliefs about UC recipients.

The least effective incentives were the damage deposit bond and the landlord liaison officer. This suggests that these incentives are not as good selling points for landlords when signing new tenancy agreements. We also asked landlords about how responsive they thought the local authority would be in communication (by asking How likely is it that the council would reply to your emails promptly?). We had hypothesized that the offer of the landlord liaison officer would increase the perception that councils would be more likely to be responsive. However, we did not find this to be the case. While we do not know why, possible reasons include:

- Landlords did not believe the liaison officer would be responsive;
- The description we included in the trial was not clear enough; or
- Landlords assumed that an LA that offers financial incentives would be responsive to landlord needs in other areas as well (such as communication).

Several of the LAs we spoke to for this project had been investing in a single point of contact for landlords. It could be that LAs have invested in a single point of contact not to get more landlords on board, but rather to offer support to landlords if issues arise (i.e. for tenancy sustainment). In any case, our findings suggest that a single point of contact is not the most effective marketing tool for encouraging landlords to work with an LA.

Another consideration is that the incentives offered in this trial do not have the same cost: paying £1,000 upfront costs more per tenancy agreement than does a £500 deposit bond. Our two most effective incentives, cash upfront and a rent guarantee, have costs that are slightly more difficult to compare: paying cash upfront is a known cost requiring relatively little administration, while a rent guarantee is an unknown cost requiring more programme management (the landlord could claim against the rent guarantee for anything between 0-12 months rent). Therefore, when it comes to implementing these results in practice, LAs need to consider the cost effectiveness of different incentives. Determining 'cost per tenancy' estimates for different types of incentive programmes would involve working with LAs to understand the cost of these two options, but also how long the resulting tenancies are. This information could be used to understand benefits to scaling: for example, for smaller LAs and authorities that do not discharge many people into the PRS, providing cash upfront may be considerably cheaper than administering a guaranteed rent programme. Large LAs, on the other hand, may see benefits to scaling a guaranteed rent programme if the expected cost per tenancy falls to lower than £1,000.

The findings from this trial were based on a hypothetical scenario and a self-reported behavioural intention, rather than actual landlord behaviour. This means there is a possibility that landlords may behave differently than our landlord participants said they would if they encountered a similar situation in their real lives. For example, in the real world, landlords are able to compare offers from multiple LAs, and their relationship or experience with an LA (for example, through a licensing or enforcement scheme) may impact their decision on whether or not to accept a tenant through them. As a next step to take these findings forward, it would be interesting to work with LAs to understand the cost implications of different incentive offers and design ways to evaluate existing incentive offers in the field (for example, by estimating a 'cost per tenancy for different types of LA programmes).

5. Conclusions

Housing people experiencing homelessness in the PRS is becoming increasingly important, as the social housing supply has not kept up with the demand. There are, however, indications that PRS landlords are unwilling to rent to households that receive housing benefits or UC.

In our trials, we investigated interventions to encourage PRS landlords to rent to people receiving UC. We presented the results of two online randomised controlled trials with a total sample of 2,798 landlords who were registered with the NRLA.

Affordability is not the only barrier to overcome

In both of our trials, average willingness to rent to people receiving UC was low (on average 'somewhat unlikely' across interventions). The scenarios we presented in our trials were designed either to be affordable at the LHA rate or to compensate landlords for renting at a below-market rent. This demonstrates that a tenant's ability to afford rent is not the only barrier to overcome in encouraging landlords to rent to people who receive UC. While we did find that incentives were more effective when there was a smaller gap between the LHA rate and market rent, the overall low willingness implies that higher LHA rates on their own may not be enough to increase the supply of private lets accessible to people receiving UC.

Securing timely rent payments can be a powerful intervention to encourage landlords to rent to people receiving UC, but how the intervention is delivered matters

Both of our trials introduced landlords to a form of rent guarantee: in Trial 1, 'What to disclose', the APA leaflet included information on the circumstances under which a landlord can request that the housing benefit portion of a tenant's UC payment is transferred directly to the landlord. In Trial 2, 'What support is effective', landlords were offered a written guarantee that a LA would cover for late or unpaid rent. The APA leaflet only had a small impact on perceptions of missed or late rent payments and did not increase landlord willingness to rent to someone receiving UC.[40] The rent guarantee, on the other hand, had a relatively large impact on both perceptions of missed or late rent payments and willingness to rent to the tenant on UC. This finding suggests that APA are not an effective mechanism for reassuring landlords that they will get paid rent on time, and that alternative rent guarantees (such as those offered by LAs) are more effective. In Scotland, UC recipients have the option to have the housing benefit portion of their UC payment transferred directly to their landlord. Expanding this programme to UC recipients in England and Wales would provide a built-in rent guarantee for landlords, while preserving choice for tenants. This would have the added benefit of freeing up LA resources to focus on providing incentives over and above the LHA rate.

Commonly-used programmes and incentives may not be having their intended impact

We found that many of the programmes that LAs offer to tenants and landlords did not substantially increase landlord willingness to rent to a person receiving UC. For example, proof of a tenant's pre-tenancy training or budget planning did not change landlord decisions overall, despite shifting landlord perceptions about the tenant's communication skills (an aspect that landlords emphasised was important to them in our fieldwork interviews). Offering a damage deposit bond or a single point of contact for landlords only slightly increased willingness to rent compared to no incentive. Surprisingly, the single point of contact did not increase landlord expectations of LA responsiveness compared to other incentives, and the deposit bond did not change landlords' expected level of profit. Some of these programmes may have other benefits that we did not measure in this trial (for example, pre-tenancy training may increase tenancy sustainment). However, our findings suggest that some of the commonly-used programmes and incentives aimed at encouraging landlords to rent to people receiving UC may not be particularly strong selling points for encouraging landlords to sign new tenancy agreements.

Better evidence is needed on the cost effectiveness of PRS access schemes and incentive programmes

In our trials, we found that offering landlords £1,000 upfront or offering a rent guarantee were the most effective at increasing landlord willingness to rent at the LHA rate, while a single point of contact and a damage deposit bond were less effective. We also found that pre-tenancy training showed promise in specific circumstances. However, effectiveness only answers whether an intervention works; it does not consider the underlying cost of delivering these programmes or incentives. Therefore, the next step would be to understand the cost effectiveness, for example through developing estimates of 'cost per tenancy,' for different programmes and incentives. Understanding the cost base will also illuminate benefits to scaling: for example, for smaller local authorities and authorities that do not discharge many people into the private rented sector, providing cash upfront may be considerably cheaper than administering a guaranteed rent programme. Large local authorities, on the other hand, may see benefits to scaling a guaranteed rent programme if the expected cost per tenancy falls to lower than £1,000.

To our knowledge, these trials are among the first behavioural experiments with landlords in the UK. However, it is important to note that the findings from this trial were based on a hypothetical scenario and a self-reported behavioural intention, rather than actual landlord behaviour. This means there is a possibility that landlords may behave differently than our participants said they would if they encountered a similar situation in their real lives. For example, in the real world, landlords are able to compare offers from multiple LAs, and their

40 The comprehension question about APA demonstrated that landlords shown the APA leaflet were more likely to understand that they could request direct rent payments, so we do not think the low perceptions and willingness were due to lack of awareness of APA.

relationship or experience with an LA (for example, through a licensing or enforcement scheme) may impact their decision on whether or not to accept a tenant through them. As a next step to take these findings forward, it would be interesting to work with LAs to design ways to evaluate existing incentive offers in the field both for impact (do they increase landlord willingness to rent) and for cost effectiveness (estimating a 'cost per tenancy for different types of programmes).

Population validity

We ran our online trials with a population of landlords who were members of the NRLA. There is a question of how representative the NRLA landlords who participated in our trials are of the general PRS landlord population. Understanding whether the landlords in our trial were representative of the general landlord population will help us contextualise the generalisability of our trial findings.

There is little data on the characteristics of the PRS landlords population in England and Wales. The most comprehensive data available in England looks at landlord location and portfolio sizes based on registrations with a Tenancy Deposit Protection scheme.[41] Unfortunately, due to a technical issue, we were unable to collect data on the portfolio sizes of the landlords participating in our trials. However, a comparison of the geographical spread of our sample to the landlord population represented in the Tenancy Deposit Protection Scheme shows that there's an overlap between their data and ours (see Table 9).

Table 9. Geographical comparison of the primary location the landlords in our sample compared to the landlords registered a Tenancy Deposit Protection scheme in 2017

	English Private Landlord Survey[42]	Our trials*
London	24%	20%
South and East	40%	37%
Midlands	14%	17%
North	22%	20%

41 Please see the English Private Landlords Survey 2018 - Technical Report for an overview of how to use Tenancy Deposit Protection to map landlord portfolio sizes.
42 ibid

*This does not sum to 100 as 5% of landlords selected Wales as their primary locationThe next best overview on the landlord population in England is from responses to the English Private Landlord Survey. [43] [44] While this survey also samples from the landlord population, and therefore is not a census, it can provide some insights into how our sample may differ from the general landlord population in England. We found that, compared to the English Private Landlord Survey, our sample tended to have more years of experience as a landlord (see Table 10). In our trial, we found that landlords with more years of experience were less likely to continue with the application compared to less experienced landlords (those 0-5 years of experience). If our sample had an overrepresentation of experienced landlords, this could suggest that our results are more favourable than what you could expect compared to the general landlord population. In general, we would expect to see larger effect sizes in online trials than we would in the real world.

Table 10. Comparison of our sample to the English Private Landlord Survey sample.

	English Private Landlord Survey[45]	Our trials
Experience as a landlord	45% 10+ years experience as a landlord	63% 10+ years experience as a landlord

43 English Private Landlord Survey 2018
44 Note that this survey captured additional data; however we looked specifically at data that could be compared to our sample.
45 English Private Landlord Survey 2018

6. Recommendations

For policy makers

Ensure LHA rates track market rents

We found that LA incentives were more effective at encouraging landlords to rent to someone receiving UC when the gap between the LHA rate and market rent was lower (£50 vs £200). This suggests that the closer LHA rates are to market rents, the more effective LA financial incentives are at encouraging landlords to rent to people receiving UC. LHA rates were re-targeted to the 30th percentile of market rates at the start of the COVID-19 pandemic; our findings suggest that keeping rates pegged to market rents could be an important policy lever for housing people receiving UC in the PRS.

Offer UC recipients a choice of having the housing benefit portion of their UC payment transferred directly to their landlord

We found that an offer of below-market, guaranteed rent was among the two most effective incentives we tested. Giving UC recipients the option to have the housing benefit portion of their UC payment transferred directly to their landlord is a structural way to achieve guaranteed rent across England and Wales (the programme already exists in Scotland). This will allow LA incentive programmes to focus on providing additional incentives over and above LHA.

Combine financial incentives and behavioural interventions

Encouraging landlords to rent to people receiving UC is not only a question of bottom line and profits: landlords make judgements about the tenant based on the information they have, and these expectations may be difficult or expensive to overcome with incentive schemes alone. LA housing teams could test combining financial incentives with programmes that aim to reduce stigma against tenants receiving UC. For example, based on our finding that landlords who had rented to someone receiving UC were more likely to rent to the tenant in our trial, LAs may want to try using landlords they are already working with as peer ambassadors to recruit additional landlords to work with them.

7. Annex 1: What to disclose

OSF REGISTRY

Trial 1 'What to disclose'

7.1 Fig. A1.1. Fictional tenant and rent scenarios based on what a single person receiving UC could afford

Fictional tenant scenario presented to landlords as part of an email exchange in Trial 1

I'm 35 years old and I have worked in the catering industry for the last ten years. I am currently unemployed and make £950 per month from Universal Credit.

We wanted our tenant scenario to be representative of the following groups:

People experiencing homelessness	Single adult households make up 60% of prevention + relief duty cases; they are also more likely to need to find a home in the PRS. About 70% are men & the rest women.
People receiving UC	More than 65% of UC claimants are between 25-49 years old, and UC claimants are evenly split between men & women.

We then used the Turn2Us benefits calculator to calculate the UC payment that Mark/Lucy could expect

Fictional rental scenario presented to landlords at the start of Trial 1

*Imagine that **you are advertising a one-bed property for £500 per month** and receive an email from Mark/Lucy who wants to view your property. You respond to the email by requesting further information about him/her. You then receive an email in response.*

We presented landlords with this fictional rent scenario in which they were asked to imagine they were advertising a property at £500, as we had calculated that Mark/Lucy would receive £138.8 per week at current LHA rates for Manchester. We checked that it was possible to find a one-bed for this rent by entering Manchester postcodes into the OpenRent calculator.

7.2 Fig. A1.2. Landlords were randomly sorted into four groups and saw either a control email or a one of three intervention emails

7.3 Fig. A1.3. Landlords were also randomly allocated either a male or female tenant, and either no story or a COVID-19 story

- To understand the impact of gender, half of landlords across all groups (both control and intervention) received an email from Mark, while the other half received an email from Lucy
- To understand the impact of letting landlords know why the tenant is receiving UC, half of all landlords received a COVID-19 story, where the tenant explains that they lost their job due to the COVID-19 pandemic. This builds on previous research that found that highlighting how homelessness is often due to systemic issues **beyond an individual's control** expanded peoples' definition of homelessness and increased their willingness to engage in activities to address homelessness.

Annex 1: Outcome measures & covariates

Table A1.1 Outcome measures

PRIMARY		
Measure	**Definition / Question Wording**	**Coding**
Likelihood to continue with Mark's application process	Answer to the question: "What are the chances of you continuing with Mark's application process?" measured on a 7-point Likert scale ranging from 1 (very unlikely) to 7 (very likely)	Continuous variable*
SECONDARY		
Perceived risk of late/missed rent payments	Answer to the question: "How likely do you think it is that [tenant name] would miss or be late on rent payments?" measured on a 7-point Likert scale ranging from 1 (very unlikely) to 7 (very likely)	Continuous variable*
Perceived risk of damage to property	Answer to the question: "How likely is it that [tenant name] would take adequate care of the property?" on a 7-point Likert scale ranging from 1 (very unlikely) to 7 (very likely)	Continuous variable*
SECONDARY		
Perceived lack of communication	Answer to the question: "How likely do you think it is that [tenant name] would get in touch with you if he/she struggles to pay the rent?" on a 7-point Likert scale from 1 (very unlikely) to 7 (very likely)	Continuous variable*
Understanding of UC and APA	Composite score of answers to 3 questions regarding UC & APA (ranging from 0-100%) "Everyone receiving Universal Credit is unemployed." [True/False/Don't know] "Tenants who receive Universal Credit payments are expected to manage their own budget, including rent." [True, False, Don't know] "There are no circumstances under which landlords can request that the housing benefit portion of Universal Credit payments are paid directly to them." [True, False, Don't know]	Continuous variable*
EXPLORATORY		
Measure	**Definition / Question Wording**	**Coding**
Click-throughs to attached info	Whether the survey participant clicks the further information link in the tenant application email	Binary variable. Yes → 1 No → 0
Time spent on materials	Time spent on email page in seconds	Continuous variable
*See section 1.8 robustness check for alternative coding		

Table A1.2. Covariates

TREATMENT			
Measure	**Vector**	**Definition**	**Coding**
Treatment	T	Treatment assignment	Categorical variable: Control → C Pre-tenancy training → T1 Budget planner → T2 APA leaflet → T3
UC context	F	UC context subgroup assignment (standard or COVID-19 story supplement)	Binary variable: Standard → 0 COVID-19 story → 1
COVARIATES			
Measure	Vector	Definition	Coding
Gender	A	"Are you male or female?"	Binary variable: Male → 0 Female → 1
Location	A	"In which region do you live?"	Categorical variable†: London → 0 North East; North West; Yorkshire & Humber → 1 East of England; South East; South West → 2 East Midlands; West Midlands → 3 Wales; Scotland; N.Ireland → EXCLUDE
Years as a landlord	A	"How many years have you been a landlord?"	Categorical variable†: 0-5 → 0 5-10 → 1 10+ → 2
Size of portfolio46	A	"How many properties do you own?" [1; 2-4; 5-9; 10-49, 50-99, 100+]	Categorical variable†: 1 → 0 2-4 → 1 5-9 → 2 10-49 → 3 50-99 → 4 100+ → 5

46 We failed to collect data on this variable. This means we couldn't control for this in our analyses, though this did not affect the unbiased identification and estimation of treatment effects in this RCT.

Primary location of properties	A	"In what region is your property located? If you own properties in multiple regions, please select the one that you would consider your primary region."	Categorical variable (more than one option can be selected)†: London → London, 0 North East; North West; Yorkshire & Humber → North, 1 East of England; South East; South West → South & East, 2 East Midlands; West Midlands→ Midlands, 3 Wales → Wales, 4 Scotland; N.Ireland → EXCLUDE
Properties on LHA rate	Cov	"Do any of your properties fall at or under the Local Housing Allowance rate?"	Categorical variable†: Yes → 0 No → 1 Don't know → 2
UC/HB tenants	Cov	"Have you rented to Universal Credit or Housing Benefit claimants before?"	Categorical variable†: Yes → 0 No → 1 Don't know → 2
Work with a LA	Cov	"Have you ever worked with a Local Authority to find a tenant for one of your properties?"	Categorical variable†: Yes → 0 No → 1 Don't know → 2
† Each categorical variable will be coded as a series of dummy variables for the purposes of analysis.			

Annex 1: Analytical strategy

We determined to either use the full sample of completed responses, or restrict attention to the half of the sample which completed this experiment first. Although treatment assignment is independent in both trials, we couldn't exhaustively rule out contamination effects between interventions in this experiment and the 'What council support is effective' experiment. Since we faced this trade-off, we defined a sample size threshold of 4000 landlords, above which we were well powered and could afford to restrict analysis on this trial to landlords who completed this experiment first. As our end sample size was below this threshold, we proceeded with the sequential design approach, and controlled for treatment arm assignment from the parallel experiment (where that experiment is completed by a landlord first), in order to detect and control for linear, additive contamination.

This experiment was focussed on increasing landlords' willingness to let to applicants on Universal Credit, which was investigated in our primary analysis. Beyond that, we were also interested in the effect of treatments on a range of secondary outcomes. Before testing these hypotheses, we reported descriptive statistics of the end-sample composition; conducted balance tests for covariates across trial arms; and provided outcome summary statistics.

For each piece of analysis presented below, we specified three models, first including only the treatment variables, then additionally including a limited set of covariates including demographics and basic information about the landlord and finally including all covariates, including those about the landlords' experiences working with local authorities, with housing benefits, and with universal credit (those "Cov" in table A1.2). In our final reporting, we reported one model per outcome. We selected the model with the best fit to the data for our primary analysis, and kept models consistent between other outcomes for simplicity.

Primary Analysis

We specified a linear regression model to test the effect of our treatments on the stated likelihood of the landlord proceeding further with the tenant application. We included a 'UC context' subgroup dummy in the specifications in order to test whether the COVID-19 story supplement performs better than the standard email on average. As discussed in the summary above, we employed specifications i) or ii) depending on end sample size.

i) Full sample:

$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

where:

$Likelihood_i$ is a continuous variable representing the stated likelihood of continuing with the mock tenant's application process.

$T1_i$ is a binary indicator with a value of 1 if the *i*th participant was randomised into the pre-tenancy training reference arm and 0 otherwise.

$T2_i$ is a binary indicator with a value of 1 if the *i*th participant was randomised into the budget planner reference arm and 0 otherwise.

$T3_i$ is a binary indicator with a value of 1 if the *i*th participant was randomised into the APA leaflet reference arm and 0 otherwise.

F_i is a binary indicator holding a value of 1 if in the COVID-19 story subgroup, and 0 otherwise.

$PrevExpT_i$ is a vector of dummy variables which indicate the treatment arm seen by the participant in the 'What council support is effective' experiment, if completed before this experiment; there is an 'NA' identifier otherwise.

A_i is a vector of controls which indicate the gender, location, years as a landlord, size of portfolio, and primary location of properties of $participant_i$. These variables are coded as stated in the covariates table.

Cov_i is a vector of custom covariates added to the model, as specified in the covariates table in section 1.7.

ε_i is the error term (which we will estimate using a heteroskedasticity-robust method).

Secondary Analysis

As secondary analysis, we were interested in testing the impact of interventions on landlords' perceptions of the risk of late rent payments with this tenant applicant. We specified a set of linear regression models to compare treatment effects on this outcome.

i) Full sample:

$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

Furthermore, we wanted to investigate the effects on the perceived risk of damage to the property. We tested for differences between control and treatment arms by linear regression.

i) Full sample:

$$DamageRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$DamageRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$DamageRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$DamageRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \varepsilon_i$$
$$DamageRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \varepsilon_i$$
$$DamageRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

In addition, we will use linear regression to test the effect of interventions on the perceived lack of communication of the prospective tenant.

i) Full sample:

$$Communication_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$Communication_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$Communication_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$Communication_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \varepsilon_i$$
$$Communication_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \varepsilon_i$$
$$Communication_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

We will test the effect of interventions on understanding of UC and APA by linear regression.

i) Full sample:

$$UCunderstanding_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$UCunderstanding_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$UCunderstanding_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$UCunderstanding_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \varepsilon_i$$
$$UCunderstanding_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \varepsilon_i$$
$$UCunderstanding_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

Finally, we were interested in conducting a subgroup analysis of our primary outcome measure. Specifically, we looked to compare treatment effects within UC context subgroups. This was modelled with two sets of linear regressions, first on participants with the standard version, and then on those with the COVID-19 story supplement.

<u>Standard frame</u>

i) Full sample:

$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

<u>Non-prototypical frame (Fi=0):</u>

i) Full sample:

$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \varepsilon_i$$
$$Likelihood_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

where:

$PaymentRisk_i$ is a continuous variable representing the perceived frequency of late or missed payment issues, coded as stated in table A1.1.

$DamageRisk_i$ is a continuous variable representing the perceived risk of damages to the property by the prospective tenant.

$Communication_i$ is a continuous variable representing perceptions of how likely the tenant would be to notify the landlord of possible payment difficulties.

$UCunderstanding_i$ is a continuous variable representing the accuracy of responses to 3 questions about UC and APA, coded as stated in table A1.1.

Robustness check

In our main analyses (primary and secondary), we were treating the rating-scale variables as if they were continuous variables. However, they were actually ordinal variables and should thus arguably be analysed with either ordinal or non-parametric methods. Although analysis of 7-point scales with linear regression is common practice and simulation studies suggest that inferential conclusions reached with parametric tests match those from non-parametric methods the majority of the time,[47] linear regression can be less efficient than the Mann-Whitney U test if the data are skewed.

Therefore, we ran the Mann-Whitney U test as a robustness check for our primary and secondary analyses. In each analysis, we performed three pairwise comparisons, contrasting each of the treatment arms with the control arm, testing the null hypotheses that the data come from the same distributions.

Exploratory Analysis

We investigated possible interaction effects between UC contexts, tenant profiles and interventions, in order to descriptively evaluate the presence of heterogeneous treatment effects within and across these subgroups. We looked at these interaction effects separately, using the same covariate specification as in all prior analyses.

In addition, we were interested in comparing click-through rates to further information in each arm. Since this outcome is binary, we will estimate the effect of the treatments using a logistic regression model.

$$Clickthrough_i \sim bernoulli(p_i)\ ;\ logit(p_i) = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i$$
$$Clickthrough_i \sim bernoulli(p_i)\ ;\ logit(p_i) = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + A_i \Gamma$$
$$Clickthrough_i \sim bernoulli(p_i)\ ;\ logit(p_i) = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 F_i + A_i \Gamma + Cov_i \Pi$$

where Clickthrough is a binary indicator with a value of 1 if the participant clicks on the link in the simulated email to further information, and 0 otherwise.

Power Calculations

We ran power calculations for every trial to assess whether we could be sufficiently confident that we detected a difference between the intervention and the control material. This was based on the number of individuals participating in each of the test conditions, the variance in responses, and insights from academic literature and previous studies on the impact of the intervention tested.

47 De Winter, J. F. C., & Dodou, D. (2010). Five-Point Likert Items: t test versus Mann-Whitney-Wilcoxon (Addendum added October 2012). Practical Assessment, Research, and Evaluation, 15(1), 11.

In our power calculations, we aimed to have sufficient statistical power to detect an effect in the primary analysis, should it exist, with 80% confidence. We followed standard practice[48] by adopting a significance threshold for the p-value of our statistical tests of 5%. While we included demographic controls in our regressions, we did not adjust for this in our calculations, which makes our estimates conservative. Furthermore, we applied a simple Bonferroni correction to adjust for multiple comparisons separately in our primary (4 comparisons: 3 treatments vs control, and UC context coefficient) and secondary (18 comparisons: 3 treatments vs control for 6 outcomes) analyses. The table and graph below summarises our calculations.

Given uncertainty in levels of survey completion for this bespoke panel of landlords, we ran our calculations for sample sizes ranging from 1500 to 4000, which seem like blanket bounds given indications from the NRLA. In addition, because we neither had relevant prior information on the variance of our primary outcome nor ran a pilot, we compared substantive effect sizes across a reasonable set of standard deviations for a 7-point Likert scale measure.

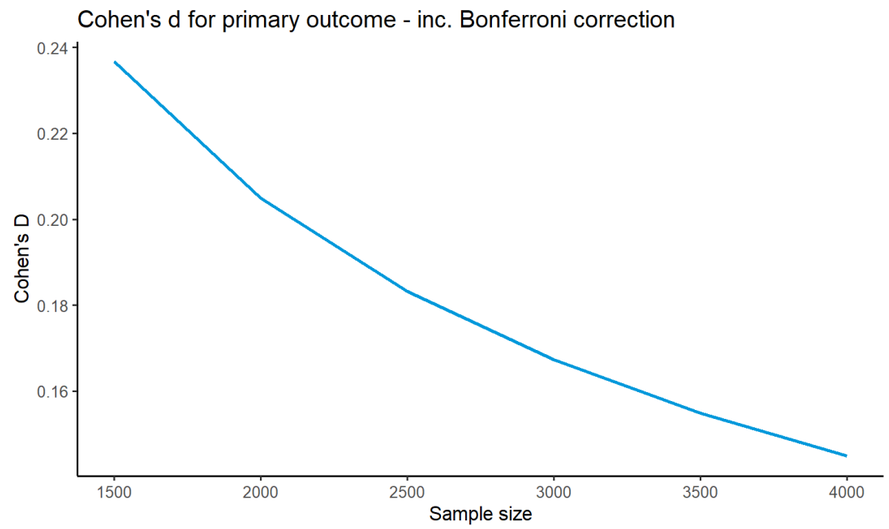
We anticipated a Cohen's D of 0.211 for our primary analysis, which corresponds to a minimum detectable substantive effect size of around 0.317 (assuming sd = 1.5). This means, for example, that if the control group had an average score (willingness to let) of 3.5 on the 7-point scale, we were able to detect a difference if a treatment group's average is at least 3.817.

48 List, J. A., Sadoff, S., & Wagner, M. (2011). So you want to run an experiment, now what? Some simple rules of thumb for optimal experimental design. Experimental Economics, 14(4), 439.

Table A1.3. Summary of Power Calculations

Analysis type	Total Sample Size	Number of arms	SD	Effect size (Cohen's D)	Substantive effect size (Difference on a 7-point Likert scale)
Primary (4 comparisons)	1000	4	1.5	0.300	0.449
	1000	4	2.5	0.300	0.749
	1500	4	1.5	0.244	0.366
	1500	4	2.5	0.244	0.611
	2000	4	1.5	0.211	0.317
	2000	4	2.5	0.211	0.529
	3000	4	1.5	0.173	0.259
	3000	4	2.5	0.173	0.432
	4000	4	1.5	0.149	0.224
	4000	4	2.5	0.149	0.374
Secondary (continuous outcomes - 18 comparisons)	1000	4	1.5	0.344	0.517
	1000	4	2.5	0.344	0.861
	1500	4	1.5	0.281	0.421
	1500	4	2.5	0.281	0.702
	2000	4	1.5	0.243	0.364
	2000	4	2.5	0.243	0.607
	3000	4	1.5	0.198	0.297
	3000	4	2.5	0.198	0.496
	4000	4	1.5	0.172	0.257
	4000	4	2.5	0.172	0.429

Figure A1.4. Visual summary of the power calculation



Differential attrition and balance checks

We tested for differential attrition across trial arms, among the subset of participants who were exposed to the intervention materials. There was some evidence of differential attrition: 5.4% of participants exposed the materials in the Control arm dropped out; this increased to 7.1% (p = .22) in the Pre-tenancy training arm, 7.7% (p = .09) in both the Budget planner and APA leaflet arms (see table A1 in Appendix 3 for a regression table). Note, however, that these differences are small and become non-significant if we adjust for multiple comparisons. Therefore, we did not further investigate this issue.

We also conducted balance checks on the final analysis sample using chi-squared tests for (categorical) covariates. Arms were balanced on all covariates (see Table A1.4).

Table A1.4. Balance check results for categorical covariates.

Covariate	Percentage per arm					
	Control	Pre-tenancy training	Budget planner	APA leaflet	p value	Balanced?
Gender						
Male	52.0	52.1	49.8	50.9	> .10	Yes
Female	48.0	47.9	50.2	49.1		
Landlord Experience						
0-5 years	16.0	15.0	14.5	15.6	> .10	Yes
5-10 years	20.6	21.2	24.3	21.4		
10+ years	63.4	63.8	61.1	63.0		
Location						
London	20.4	20.2	21.1	19.3	> .10	Yes
Midland	19.9	16.7	14.4	17.3		
North	19.3	20.9	20.4	20.4		
Scotland	0.5	0.5	0.6	0.3		
South and East	34.9	36.3	38.7	38.0		
Wales	5.0	5.2	4.7	4.6		
Northern Ireland	0.0	0.2	0.0	0.1		
Primary property locations						
East	5.6	5.7	5.5	8.2	> .10	Yes
East Midlands	11.1	10.6	8.9	9.3		
London	23.2	23.7	27.3	23.1		
North East	4.8	5.1	5.5	4.4		
North West	15.1	16.3	13.3	13.9		
Northern Ireland	0.0	0.0	0.0	0.1		
Wales	6.9	7.0	7.4	5.3		
Scotland	0.6	1.3	1.2	0.5		
Yorkshire	9.2	9.0	9.5	9.5		
South East	26.0	23.2	23.4	24.3		
South West	12.8	16.1	15.3	14.6		
Properties on LHA						
Yes	18.7	18.3	18.8	20.6	> .10	Yes
No	48.8	53.2	51.0	50.4		
Don't know	32.6	28.4	30.2	29.0		

Covariate	Percentage per arm					
	Control	Pre-tenancy training	Budget planner	APA leaflet	p value	Balanced?
Experience with UC tenants						
Yes	51.9	53.1	51.8	52.2	> .10	Yes
No	48.1	46.9	48.2	47.8		
Received help from a Local Authority						
Yes	17.8	15.2	21.1	18.0	> .10	Yes
No	82.2	84.8	78.9	82.0		
** p<0.01, * p<0.05, + p<0.1						

As shown in table A1.4, our participants were about equally male and female, had many years of experience with being a landlord (63.4% of the sample had 10+ years of experience), and had properties across all regions of England and Wales (though only a small minority have properties in Scotland or Northern Ireland). Only 18.7% of participants said that they owned a property under the Local Housing Allowance (LHA) rate, though 32.6% didn't know. 51.9% of our participants had experience with tenants on UC and 17.8% said they had worked with a Local Authority to find a tenant.

Primary analysis

As defined in the analytical strategy, since we didn't reach the threshold of 4000 participants, we estimate regression models for the full sample of landlords who completed both experiments. Therefore, in all following regressions, we control for treatment assignment from the parallel experiment, with an additional reference category for those who completed this experiment first.

We ran three regression models for the primary outcome: with no covariates, with a limited set of 'standard' covariates, and with a full set of covariates we collected. Table 11 shows the AIC of each model. Since model 3 has the lowest AIC – indicating best fit – we use the full set of covariates in the primary analysis as well as all secondary analyses.

Table A1.5. Model fit comparisons using the AIC statistic

Model	Model 1 (treatment assignments only)	Model 2 (treatments + standard covariates)	Model 3 (treatments + standard and landlord-related covariates
AIC	11,141.84	11,130.86	11,024.87

As shown in table A1.6 below, none of the intervention arms had a significant effect on the stated likelihood of continuing with the application. We also didn't find a significant effect of the inclusion of COVID-19 as the reason for the applicant being on universal credit.

Table A1.6. Regression table for the primary analysis

Outcome (likelihood to continue with application)	Coefficient (reference = control)				
	Constant	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
Coefficient	3.420**	0.147	-0.035	-0.015	0.112
(Standard error)	(0.165)	(0.095)	(0.095)	(0.091)	(0.065)
95% Confidence interval	[2.92, 3.56]	[-0.04,0.33]	[-0.22,0.15]	[-0.19,0.16]	[-0.02,0.24]
Standard covariates	Yes				
Custom covariates	Yes				
R squared	0.063				
Observations	2,798				

Primary analysis robustness check

Since we had a concern that the main analytical strategy may be statistically inefficient, due to a potentially skewed distribution of residuals, we have run the Mann-Whitney U test as a robustness check. As shown in table A1.7, the results are qualitatively consistent with the main analysis: none of the treatments, nor the UC context, have a significant effect (after correcting p-values for multiple comparisons).

Table A1.7. Results of the primary analysis robustness check

Outcome (likelihood to continue with application)	Coefficient (reference = control)			
	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
W-stat	213,598	209,573	239,944	1,019,349
adjusted P-value	0.296	0.967	0.967	0.200
Observations	1,338	1,353	1,455	2,798

Secondary analysis

We asked participants three additional questions about the presented tenancy application, all aimed at capturing potential mechanisms through which the treatments could have an effect. These questions were about: participants' expectations of late or missed rent payments; their expectations about communication; and their expectations about the tenant taking adequate care of the property. Additionally, we quizzed participants with three questions about universal credit (UC) and alternative payment arrangements (APA) legislation; their scores on these questions were combined into a single 'Understanding of UC and APA' variable. Table A1.8 shows the descriptive statistics for these variables and tables A1.9- A1.12 present the regression analysis results.

Table A1.8. Descriptive statistics for the secondary outcome variables

	Control (n = 674)	Pre-tenancy training (n = 664)	Budget planner (n = 679)	APA leaflet (n = 781)
Outcome	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Perceived risk of late/missed payment	4.61 (1.30)	4.51 (1.25)	4.58 (1.27)	4.44 (1.29)
Perceived care of the property taken by the applicant	4.47 (1.19)	4.68 (1.17)	4.66 (1.17)	4.41 (1.11)
Perceived communication score	4.08 (1.34)	4.50 (1.40)	4.44 (1.35)	4.13 (1.32)
Understanding of UC/APA	0.71 (0.30)	0.70 (0.30)	0.71 (0.31)	0.73 (0.30)

Table A1.9. Regression table for the secondary analysis (Perceived risk of late/missed rent payments)

Outcome (perceived risk of late/missed payments)	Coefficient (reference = control)				
	Constant	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
Coefficient (Standard error)	4.570** (0.123)	-0.105 (0.070)	-0.022 (0.070)	-0.161* (0.068)	-0.003 (0.048)
95% Confidence interval	[4.33,4.81]	[-0.24,0.03]	[-0.16,0.12]	[-0.29,-0.03]	[-0.10,0.09]
Standard covariates	Yes				
Custom covariates	Yes				
R squared	0.03				
Observations	2,798				

Table A1.10. Regression table for the secondary analysis (Perceived likelihood of necessary communications made by the tenant applicant)

Outcome (perceived level of comms)	Coefficient (reference = control)				
	Constant	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
Coefficient (Standard error)	4.020** (0.132)	0.425** (0.075)	0.347** (0.073)	0.060 (0.069)	0.123* (0.051)
95% Confidence interval	[3.76,4.28]	[0.28,0.57]	[0.20,0.49]	[-0.08,0.20]	[0.02,0.22]
Standard covariates	Yes				
Custom covariates	Yes				
R squared	0.042				
Observations	2,798				

Table A1.11. Regression table for the secondary analysis (Perceived level of care for the property by the tenant)

Outcome (perceived level of care for the property)	Coefficient (reference = control)				
	Constant	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
Coefficient (Standard error)	4.516** (0.117)	0.216** (0.065)	0.192** (0.064)	-0.061 (0.060)	0.103* (0.044)
95% Confidence interval	[4.29,4.75]	[0.09,0.34]	[0.07,0.32]	[-0.18,0.06]	[0.02,0.19]
Standard covariates	Yes				
Custom covariates	Yes				
R squared	0.036				
Observations	2,798				

Table A1.12. Regression table for the secondary analysis (Understanding of UC and APA)

Outcome (understanding of UC and APA)	Coefficient (reference = control)				
	Constant	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
Coefficient (Standard error)	0.537** (0.028)	-0.020 (0.015)	-0.002 (0.016)	0.011 (0.015)	-0.012 (0.011)
95% Confidence interval	[0.48,0.59]	[-0.05,0.01]	[-0.03,0.03]	[-0.02,0.04]	[-0.03,0.01]
Standard covariates	Yes				
Custom covariates	Yes				
R squared	0.135				
Observations	2,798				

Subgroup analysis of the primary outcome

Participants were randomly split into two subgroups, one presented with an email providing no explanation for why the prospective tenant is on UC and the other including an additional sentence stating that the reason is the COVID-19 pandemic. In this analysis, we test whether our treatments had an impact on the primary outcome variable separately within these two subgroups.

As shown in Table A1.13, in all intervention arms (though not the control arm), the average likelihood of continuing with the application was higher among participants shown the COVID-19 story.

Table A1.13. Descriptive statistics for the primary outcome variable, split by whether the COVID-19 story was included

	Control (n = 323)	Pre-tenancy training (n = 297)	Budget planner (n = 336)	APA leaflet (n = 395)
Outcome	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Likelihood to continue tenant's application - COVID-19 story	3.42 (1.79)	3.82 (1.74)	3.55 (1.78)	3.44 (1.77)
	(n = 351)	(n = 367)	(n = 342)	(n = 386)
Likelihood to continue tenant's application - no COVID-19 story	3.48 (1.78)	3.41 (1.77)	3.31 (1.79)	3.44 (1.72)

In the COVID-19-story subgroup, we observed a significantly higher stated likelihood to continue with the application in the pre-tenancy training group than in the control group, with an effect size of 0.344 points. Neither of the other two treatments had a significant effect.

Table A1.14. Subgroup analysis results – COVID-19 story

Outcome (likelihood to continue with tenant's application)	Coefficient (reference = control)			
	Constant	Pre-tenancy training	Budget planner	APA leaflet
Coefficient (Standard error)	3.393** (0.231)	0.344* (0.140)	0.096 (0.134)	-0.022 (0.131)
95% Confidence interval	[2.94,3.85]	[0.07,0.62]	[-0.17,0.36]	[-0.28,0.23]
Standard covariates	Yes			
Custom covariates	Yes			
R squared	0.07			
Observations	1,352			

In the subgroup without the COVID-19 story, we observed no significant treatment effects.

Table A1.15. Subgroup analysis results – No mention of COVID-19

Outcome (likelihood to continue with tenant's application)	Coefficient (reference = control)			
	Constant	Pre-tenancy training	Budget planner	APA leaflet
Coefficient (Standard error)	3.545** (0.235)	-0.022 (0.131)	-0.134 (0.134)	0.008 (0.126)
95% Confidence interval	[3.08,4.01]	[-0.28,0.23]	[-0.40,0.13]	[-0.24,0.25]
Standard covariates	Yes			
Custom covariates	Yes			
R squared	0.071			
Observations	1,446			

Secondary outcome robustness checks

Similar to the primary analysis, due to a potentially skewed distribution of residuals for Likert-scale outcomes, we have run the Mann-Whitney U test as a robustness check. As shown in table A1.16., the results are qualitatively consistent with the main analysis (after correcting p-values for multiple comparisons).

Table A1.16. Results of the secondary analysis robustness check

Outcomes	Coefficient (reference = control)			
Perceived risk of late/missed payment	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
W-stat	234,011	231,920	281,367	979,169
P-value	0.13	0.55	0.02	0.94
Perceived level of care for the property	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
W-stat	198,186	208,718	269,218	1,032,128
P-value	<0.001	0.003	0.42	0.01
Perceived likelihood of necessary communications by the tenant	Pre-tenancy training	Budget planner	APA leaflet	UC context (ref = no story)
W-stat	183,161	194,088	257,159	1,032,814
P-value	<0.001	<0.001	0.44	0.01

Exploratory analysis[49]

Clickthroughs

In each of the intervention arms, the email from the hypothetical applicant contained attachments. In this analysis, we look at whether the rates at which participants clicked on these attachments varied across arms.

As shown in table A1.16., there was no significant difference between the clickthrough to attachments in the pre-tenancy training and budget planner arms, both showing clickthrough rates of slightly under 60%. However, there was a significantly lower clickthrough rate in the APA leaflet arm.

Table A1.16. Effect of treatment on clicking through to the email attachment. Baseline category is the pre-tenancy training group. Effect sizes are given as log odds ratios.

Outcome (likelihood to continue with tenant's application)	Coefficient (reference = control)		
	Constant	Budget planner	APA leaflet
Coefficient (Standard error)	0.211 (0.227)	0.124 (0.112)	-0.473** (0.108)
95% Confidence interval	[-0.23,0.66]	[-0.10,0.34]	[-0.68,-0.26]
Standard covariates	Yes		
Custom covariates	Yes		
R squared	0.029		
Observations	2,124		

49 Note that p-values in this section are not corrected for multiple comparisons

Understanding of APA

In our secondary analysis, we observed no significant effects from any of the interventions on overall understanding of UC and APA, measured across 3 questions (refer to Interventions section). However, only one of these questions was directly related to the content of an intervention: the APA comprehension question ("There are no circumstances under which landlords can request that the housing benefit portion of Universal Credit payments be paid directly to them." [True, False, Don't know]). Therefore, we have tested here whether there were also null effects on correctly answering this question specifically.

Using a logistic regression, we found, somewhat reassuringly, that the arm which referenced APA, and attached an APA leaflet, significantly increased awareness about the possibility of directly receiving the housing benefit portion of Universal Credit payments (+8.4pp). In the control group, around 58% of landlords were aware of this arrangement; in the APA arm, 67% were. The other arms did not differ significantly from the control.

Table A1.17. Effect of treatment on correctly answering the APA comprehension question

Outcome (probability of answering the APA statement correctly)	Coefficient (reference = control)			
	Constant	Pre-tenancy training	Budget planner	APA leaflet
Coefficient (Standard error)	-0.751** (0.211)	-0.148 (0.120)	0.041 (0.120)	0.322** (0.117)
95% Confidence interval	[-1.16,-0.34]	[-0.38,0.09]	[-0.19,0.28]	[0.09,0.55]
Standard covariates	Yes			
Custom covariates	Yes			
R squared	0.109			
Observations	2,798			

Primary outcome: estimating treatment effects for those who clicked through to the attachments

Clickthrough-treatment interaction

Across intervention arms (excluding the control which had no attachments), we found that roughly half of participants clicked-through to at least one email attachment (i.e. the budget plan, the tenancy training programme, the tenancy training certificate, or the APA leaflet). In order to test whether effects on our primary outcome varied depending on opening treatment-specific email attachments, we ran our primary analysis with three interaction terms. Although this piece was not pre-specified in our analysis plan, we felt this could shed light on the underlying mechanisms of each treatment condition (see Table A1.18).

With this specification we found that, compared to the control, the magnitude of the effect for pre-tenancy training strongly depends on whether the landlord viewed the attachment, with those reading the certificate giving a likelihood score almost 0.5 points higher than those who did not click-through, on average. the results reported in table 25 must be interpreted tentatively, since estimates from such an interaction model are likely inhibited by low statistical power with our sample size.

Table A1.18. Interaction effects for those who clicked through to view an attachment

Outcome (likelihood of continuing with prospective tenant's application)	Coefficient (reference = control)						
	Constant	Pre-tenancy training	Budget planner	APA leaflet	Opened attachment x Pre-tenancy training	Opened attachment x Budget planner	Opened attachment x APA leaflet
Coefficient (Standard error)	3.42 (0.165)	-0.127 (0.123)	-0.109 (0.128)	0.031 (0.107)	0.488** (0.135)	0.126 (0.138)	-0.104 (0.123)
95% Confidence interval	[3.10, 3.74]	[-0.37, 0.11]	[-0.36 ,0.14]	[-0.18, 0.24]	[0.22, 0.75]	[-0.14, 0.40]	[-0.35, 0.14]
Standard covariates	Yes						
Custom covariates	Yes						
R squared	0.068						
Observations	2,798						

Primary outcome: exploring heterogeneous treatment effects

Applicant profile-treatment interaction:

We do not observe a significant main effect of the tenancy applicant's name (i.e. being called Mark instead of Lucy) on average stated likelihood of continuing the application process. However, we do find a differential treatment effect of the pre-tenancy training intervention for Lucy and Mark: The intervention effect when the applicant is called Lucy is estimated to be 0.345 points (significant at the .05 level), while the effect when the applicant's name is Mark is 0.345 + 0.033 - 0.392 = -0.014 (i.e. very close to zero), which is significantly less. The significant effect for Lucy is likely driven by the fact that the effect comprises two smaller, otherwise non-significant effects, namely the main effect of the pre-tenancy training intervention and of the female name.

Table A1.19. Interaction between treatment and applicant's name

Outcome (likelihood of continuing tenant's application process)	Coefficient (reference = control)							
	Constant	Pre-tenancy training	Budget planner	APA leaflet	Applicant name (ref=Lucy)	Pre-tenancy x Mark	Budget planner x Mark	APA leaflet x Mark
Coefficient (Standard error)	3.39** (0.181)	0.345* (0.136)	0.084 (0.135)	0.033 (0.131)	0.033 (0.133)	-0.392* (0.189)	-0.243 (0.188)	-0.095 (0.182)
95% Confidence interval	[3.04 ,3.74]	[0.08, 0.61]	[-0.18, 0.35]	[-0.22, 0.29]	[-0.23, 0.29]	[-0.76,-0.02]	[-0.61, 0.13]	[-0.45, 0.26]
Standard covariates	Yes							
Custom covariates	Yes							
R squared	0.066							
Observations	2,798							

COVID-19 framing-treatment interaction:

Similarly, we do not observe a significant effect of the COVID-19 story (i.e. referenced as the cause for the applicant's current unemployment spell) on stated likelihood of continuing the application process. We find some weak directional evidence that the budget planner intervention has a positive effect on the primary outcome when COVID-19 is so-mentioned; and no significant effects among other treatment-framing groups. However, it should again be noted that our power to detect interaction effects is impaired by the available sample size; table A1.20 reports key regression coefficients from a linear regression with the model 3 covariates plus treatment-applicant profile interaction terms, unadjusted for multiple comparisons.

Table A1.20. Interaction between COVID-19 story and primary outcome

Outcome (likelihood of continuing tenant's application process)	Coefficient (reference = control)							
	Constant	Pre-tenancy training	Budget planner	APA leaflet	COVID-19 framing (ref=no mention of COVID-19)	Pre-tenancy x COVID-19	Budget planner x Mark	APA leaflet x Mark
Coefficient (Standard error)	3.54** (0.177)	-0.058 (0.133)	-0.166 (0.136)	-0.005 (0.128)	-0.077 (0.136)	0.394* (0.194)	0.281 (0.193)	0.059 (0.185)
95% Confidence interval	[3.19, 3.89]	[-0.32, 0.20]	[-0.43, 0.10]	[-0.26, 0.25]	[-0.34, 0.19]	[0.01, 0.77]	[-0.10, 0.66]	[-0.30, 0.42]
Standard covariates	Yes							
Custom covariates	Yes							
R squared	0.069							
Observations	2,684							

Primary outcome: analysis for subgroup of landlords with properties at or below the LHA rate:

We do not observe any significant differences in the likelihood to continue with a tenancy application between interventions for the LHA subgroup. We are cautious in making further interpretations from this analysis due to the very low sample size (n=533), but the direction and magnitude of estimated coefficients are broadly consistent with the main analysis, suggesting there are not strong differential treatment effects for this particular subgroup.

Table A1.21. Subgroup analysis of primary outcome for landlords with properties at or below the LHA rate

Outcome (likelihood to continue with tenant's application)	Coefficient (reference = cash up-font)			
	Constant	Pre- tenancy training	Budget planner	APA leaflet
Coefficient (Standard error)	4.510** (0.419)	0.051 (0.230)	0.012 (0.229)	-0.091 (0.216)
95% Confidence interval	[3.69,5.33]	[-0.40,0.50]	[-0.44,0.46]	[-0.51,0.33]
Standard covariates	Yes			
Custom covariates	No			
R squared	0.10			
Observations	533			

8. Annex 2: What council support is effective

OSF REGISTRY

Trial 2 'What council support is effective'

Determining Rental Gap Sizes

To establish the rental gap sizes for this trial (smaller vs. larger), we used the following process.

1. We looked at the variation of the LHA rate around the country. We observed that smaller gaps occurred more frequently in areas with lower LHA rates and larger gaps were more common in areas with higher LHA rates (for example, the gaps between LHA rate and rents are larger in several London boroughs, even though these areas have among the highest LHA rates.)

2. We then explored the market rent price of one-bed flats in each area at the extremes of the LHA rate scale (£70 - £270 per week), excluding Central London. We used the OpenRent calculator to get an approximate rent price range.

3. Next, we searched for one-bed flat properties at OpenRent and estimated the gap between LHA rate and market rent.

For one-bed flats at the lower end of LHA rate, we observed a 'rent gap size' between £50-£100 (smaller gap). For one-bed flats at the higher end of LHA rate, we observed a 'rent gap size' between £200-£400 (larger gap). We decided to use the lower end of the gap size for each condition, given recent changes to LHA rates.

Fig. A2.1. Landlords were also randomly allocated either a large or small rent gap.

Our control email included a scenario in which the landlord was asked whether they would rent at below market rent to a tenant who received UC We varied the scenario based on the size of the rent gap, the difference between the rent that the landlord was asking for and the housing benefit portion of UC (LHA amount).

We varied the size of the rent gap to mimic the range of scenarios across England:

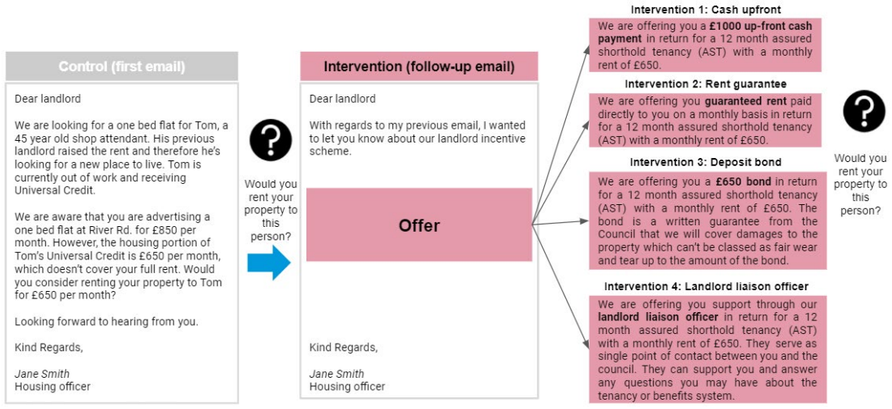
- The smaller gap (£50) was based on LHA and rental properties in Grimsby (area with among the lowest LHA rates).
- The larger gap (£200) was based on LHA rates and rental properties in London (area with among the highest LHA rates)

We used the OpenRent calculator to ensure our market rent amounts were realistic for these areas.

Control (first email)

Smaller gap = £50	Larger gap = £200
Dear landlord We are looking for a one bed flat for David, a 45 years shop attendant. His previous landlord raised the rent and therefore he's looking for a new place to live. Lucy is currently out of work and receiving Universal Credit. **We are aware that you are advertising a one bed flat at River Rd. for £550 per month. However, the housing portion of Tom's Universal Credit is £500 per month, which doesn't cover your full rent. Would you consider renting your property to Tom for £500 per month?** Looking forward to hearing from you. Kind Regards, *Jane Smith* Housing officer	Dear landlord We are looking for a one bed flat for David, a 45 years shop attendant. His previous landlord raised the rent and therefore he's looking for a new place to live. Tom is currently out of work and receiving Universal Credit. **We are aware that you are advertising a one bed flat at River Rd. for £850 per month. However, the housing portion of Tom's Universal Credit is £650 per month, which doesn't cover your full rent. Would you consider renting your property to Tom for £650 per month?** Looking forward to hearing from you. Kind Regards, *Jane Smith* Housing officer

Fig A2.2. Landlords were first shown an email from the LA (control); they were then shown on of four intervention email



Annex 2: Outcome measures & covariates

Table A2.1. Outcome measures

PRIMARY		
Measure	**Definition / Question Wording**	**Coding**
Change in likelihood to accept council's updated offer	Measured as the change in willingness to accept the updated council's offer, compared to an initial baseline council offer (with no additional incentives). Both questions are phrased: "How likely would you be to rent your property to David?" with answers on a 7-point Likert scale ranging from 1 (very unlikely) to 7 (very likely)	Difference in Likert scores treated as a continuous variable*
SECONDARY		
Perception of risk of late or missed rent payments	Answer to the question: "How likely do you think you would be to experience late or missed rent payments from David?" measured on a 7-point Likert scale ranging from 1 (very unlikely) to 7 (very likely)	Continuous variable*
Perceived risk of financial loss during tenancy	Answer to question: ""How much profit would you expect to make on this tenancy?" on a 7-point Likert scale ranging from 1 (far below average) to 7 (far above average)"	Continuous variable*
Expected responsiveness - Council	Answer to the question: "How likely is it that the council would reply to your emails promptly?" (very unlikely) to 7 (very likely)	Continuous variable*

EXPLORATORY		
Measure	**Definition / Question Wording**	**Coding**
Time spent on materials	Time spent on email page in seconds	Continuous variable
*See section 1.8 robustness check for alternative coding		

Table A2.2. Covariates

TREATMENT			
Measure	**Vector**	**Definition**	**Coding**
Treatment	T	Treatment assignment	Categorical variable†: Cash upfront (Control) → C Rent guarantee → T1 Deposit bond → T2 Landlord liaison officer → T3
Rent deficit	R	Rent-housing benefits gap size subgroup assignment (small (£50) or large (£200))	Binary variable: Small → 0 Large → 1
COVARIATES			
Measure	**Vector**	**Definition**	**Coding**
Gender	A	"Are you male or female?"	Binary variable: Male → 0 Female → 1

COVARIATES			
Measure	**Vector**	**Definition**	**Coding**
Location	A	"In which region do you live?"	Categorical variable†: London → 0 North East; North West; Yorkshire & Humber → North, 1 East of England; South East; South West → South and East, 2 East Midlands; West Midlands → Midlands, 3 Wales → Wales, 4 Scotland; N.Ireland → EXCLUDE
Years as a landlord	A	"How many years have you been a landlord?"	Categorical variable†: 0-5 → 1 5-10 → 2 15+ → 3

COVARIATES			
Measure	**Vector**	**Definition**	**Coding**
Primary location of properties	A	"In what region(s) are your properties located? Please check all that apply."	Categorical variable (more than one option can be selected)†: London → London, 0 North East; North West; Yorkshire & Humber → North, 1 East of England; South East; South West → South & East, 2 East Midlands; West Midlands→ Midlands, 3 Wales, → Wales, 4 Scotland, N. Ireland → Scotland & N. Ireland, 5
Size of portfolio	A	"How many properties do you own?" [1; 2-4; 5-9; 10-49, 50-99, 100+]	Categorical variable†: 1 → 0 2-4 → 1 5-9 → 2 10-49 → 3 50-99 → 4 100+ → 5

COVARIATES			
Measure	**Vector**	**Definition**	**Coding**
UC/HB tenants	Cov	"Have you rented to Universal Credit or Housing Benefit claimants before?"	Categorical variable†: Yes → 0 No → 1 Don't know → 2
Work with a LA	Cov	"Have you ever worked with a Local Authority to find a tenant for one of your properties?"	Categorical variable†: Yes → 0 No → 1 Don't know → 2
† Each categorical variable will be coded as a series of dummy variables for the purposes of analysis.			

Annex 2: Analytical Strategy

We determined to either use the full sample of completed responses, or restrict attention to the half of the sample which completed this experiment first. Although treatment assignment is independent in both trials, we couldn't exhaustively rule out contamination effects between interventions in this experiment and the 'What to disclose' experiment. Since we faced this trade-off, we defined a sample size threshold of 4000 landlords, above which we were well powered and could afford to restrict analysis on this trial to landlords who completed this experiment first. As our end sample size was below this threshold, we proceeded with the sequential design approach, and controlled for treatment arm assignment from the parallel experiment (where that experiment is completed by a landlord first), in order to detect and control for linear, additive contamination.

This experiment was focussed on increasing landlords' willingness to let to applicants on Universal Credit, which was investigated in our primary analysis. Beyond that, we were also interested in the effect of treatments on a range of secondary outcomes. Before testing these hypotheses, we reported descriptive statistics of the end-sample composition; conducted balance tests for covariates across trial arms; and provided outcome summary statistics.

For each piece of analysis presented below, we specified three models, first including only the treatment variables, then additionally including a limited set of covariates including demographics and basic information about the landlord and finally including all covariates, including those about the landlords' experiences working with local authorities, with housing benefits, and with universal credit (those "Cov" in table A 2.2). In our final reporting, we reported one model per outcome. We selected the model with the best fit to the data for our primary analysis, and kept models consistent between other outcomes for simplicity.

Primary Analysis

We specified a linear regression model to test the effect of our treatments on the change in stated likelihood to consider the council's offer from the no incentives baseline. We included a rent gap dummy variable in specifications to assess whether the larger rent gap is associated with a lower average change in likelihood to accept the offer. As discussed in the summary above, we employed specifications i) or ii) depending on end sample size.

i) Full sample:

$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

where:

$LikelihoodChange_i$ is a continuous variable representing the change in stated willingness to consider the council's offer from the no incentives baseline.

$T1_i$ is a binary indicator with a value of 1 if the *i*th participant saw the rent guarantee offer and 0 otherwise.

$T2_i$ is a binary indicator with a value of 1 if the *i*th participant saw the deposit bond offer and 0 otherwise.

$T3_i$ is a binary indicator with a value of 1 if the *i*th participant saw the offer of landlord liaison officer support and 0 otherwise.

R_i is a binary indicator holding a value of 1 if in the large rent deficit subgroup, and 0 otherwise.

$PrevExpT_i$ is a vector of dummy variables which indicate the treatment arm seen by the participant in the 'What to disclose' experiment, if completed before this experiment; there is an 'NA' identifier otherwise.

A_i is a vector of controls which indicate the gender and location of $participant_i$. These variables are coded as stated in the covariates table.

Cov_i is a vector of custom covariates added to the model, as specified in the covariates table in section 1.7.

ε_i is the error term (which we will estimate using a heteroskedasticity-robust method).

Secondary Analyses

As secondary analysis, we were interested in testing the impact of interventions on landlords' perceptions about the risk of late or missed rent payments in this tenancy scenario. We specified a set of linear regression models to compare treatment effects on this outcome.

i) Full sample:

$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \varepsilon_i$$
$$PaymentRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

Furthermore, we wanted to investigate the effects on the perceived risk of financial loss during tenancy. We tested for differences between control and treatment arms via linear regression.

i) Full sample:

$$FinancialLossRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$FinancialLossRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$FinancialLossRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$FinancialLossRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \varepsilon_i$$
$$FinancialLossRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \varepsilon_i$$
$$FinancialLossRisk_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

In addition, we intended to investigate effects on the expected responsiveness of the council to their needs in relation to this tenancy. Again, we tested for differences between control and treatment arms via linear regression.

i) Full sample:

$$Responsiveness_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$Responsiveness_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$Responsiveness_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$Responsiveness_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \varepsilon_i$$
$$Responsiveness_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \varepsilon_i$$
$$Responsiveness_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_1 R_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

Finally, we conducted a subgroup analysis on our primary outcome measure. Specifically, we estimated treatment effects within rental gap subgroups, in order to validate that effectiveness of interventions is consistent. This was tested with two sets of linear regressions, first on participants with the small rental gap, and then on those with the large gap.

Small deficit (Ri=0):

i) Full sample:

$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

b) Large deficit ($R_i = 1$):

i) Full sample:

$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \gamma_2 PrevExpT_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

ii) Restricted sample (this experiment completed first):

$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \varepsilon_i$$
$$LikelihoodChange_i = \alpha + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + \Gamma A_i + \Pi Cov_i + \varepsilon_i$$

where:

$PaymentRisk_i$ is a continuous variable representing the perceived risk of late or missed rent payment, coded as in table A2.1.

$FinancialLossRisk_i$ is a continuous variable representing the expected profit of the tenancy, coded as stated in table A2.1

$Responsiveness_i$ is a continuous variable representing the perceived responsiveness of the council to the landlord's needs in this tenancy, coded as stated in table A2.1

Robustness check

In our main analyses (primary and secondary), we treated the rating-scale variables as if they were continuous variables. However, they are actually ordinal variables and should thus arguably be analysed with either ordinal or non-parametric methods. Analysing 7-point scales with linear regression is common practice and simulation studies suggest that inferential conclusions reached with parametric tests match those from non-parametric methods the majority of the time.[50] However, since we were subtracting two values here (before and after exposure to treatment), there was a risk that the resulting distribution would significantly deviate from the normal distribution; for instance, it may be significantly leptokurtic.

In order to minimise the chance of false inference stemming from model misspecification, we supplemented our main analysis with a robustness check that uses the Wilcoxon signed-rank test, which is a non-parametric test used to determine whether two dependent samples were selected from the same distribution.

Exploratory Analysis

We investigated possible interaction effects between rental gaps, tenant profiles and interventions, in order to descriptively evaluate the presence of heterogeneous treatment effects within and across these subgroups. We looked at these interaction effects separately, using the same covariate specification as in all prior analyses.

50 De Winter, J. F. C., & Dodou, D. (2010). Five-Point Likert Items: t test versus Mann-Whitney-Wilcoxon (Addendum added October 2012). Practical Assessment, Research, and Evaluation, 15(1), 11.

Power Calculations

We ran power calculations for every trial to assess whether we were sufficiently confident that we could detect a difference between the intervention and the control material. This was based on the number of individuals participating in each of the test conditions, the variance in responses, and insights from academic literature and previous studies on the impact of the intervention tested.

In our power calculations, we aimed to have sufficient statistical power to detect an effect in the primary analysis, should it exist, with 80% confidence. We followed standard practice[51] by adopting a significance threshold for the p-value of our statistical tests of 5%. While we included demographic controls in our regressions, we did not adjust for this in our calculations, which makes our estimates conservative. Furthermore, we applied a simple Bonferroni correction to adjust for multiple comparisons separately in our primary (4 comparisons: treatments vs control, and rent gap dummy coefficient) and secondary (15 comparisons: treatments vs control x5) analyses. The table and graph below summarises our calculations.

Given uncertainty in levels of survey completion for this bespoke panel of landlords, we ran our calculations for sample sizes ranging from 1500 to 4000, which seemed like blanket bounds given indications from the NRLA. In addition, because we neither had relevant prior information on the variance of our primary outcome nor ran a pilot, we compared substantive effect sizes across a reasonable set of standard deviations for differences between two 7-point Likert scale measures.

We anticipated a Cohen's D of 0.211 for our primary analysis, which corresponds to a minimum detectable substantive effect size of around 0.317 (assuming sd = 1.5). This means, for example, that if the control group (cash upfront arm) had an average increase in willingness to accept of 2 points from the zero incentives offer, we were able to detect a difference if a treatment group's average increase is at least 2.317 (15.9% difference). We noted that such differences are considered plausible given the financial magnitude of the interventions.

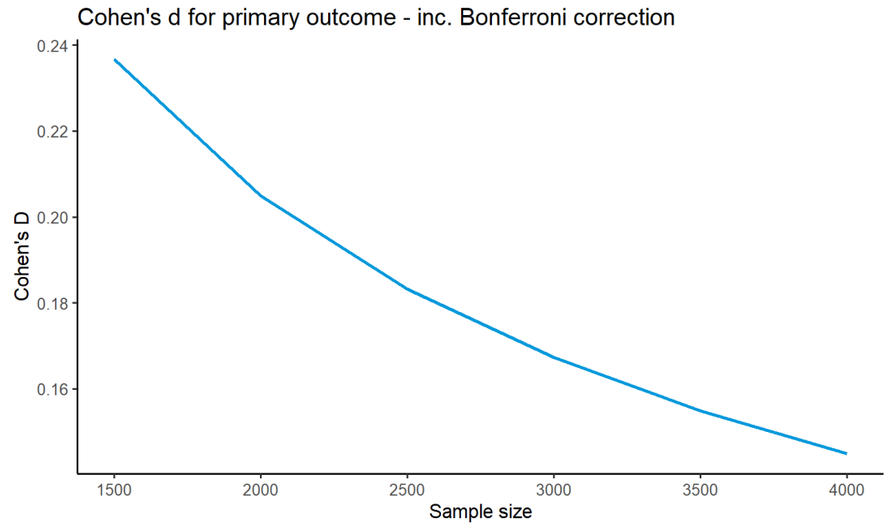
Overall, we were satisfied with these power implications, and expected to reach 2000 participants in total; equally, our predefined threshold of passing 4000 landlords in order to 'switch' analytical approach would ensure a Cohen's D of at most 0.211 for the primary analysis.

51 List, J. A., Sadoff, S., & Wagner, M. (2011). So you want to run an experiment, now what? Some simple rules of thumb for optimal experimental design. Experimental Economics, 14(4), 439.

Table A2.3. Summary of Power Calculations

Analysis type	Total Sample Size	Number of arms	SD	Effect size (Cohen's D)	Substantive effect size (Difference on a 7-point Likert scale)
Primary (4 comparisons)	1000	4	0.5	0.300	0.150
	1000	4	1.5	0.300	0.449
	1500	4	0.5	0.244	0.122
	1500	4	1.5	0.244	0.367
	2000	4	0.5	0.211	0.106
	2000	4	1.5	0.211	0.317
	3000	4	0.5	0.173	0.086
	3000	4	1.5	0.173	0.259
	4000	4	0.5	0.149	0.075
	4000	4	1.5	0.149	0.224
Secondary (continuous outcomes - 15 comparisons)	1000	4	0.5	0.339	0.170
	1000	4	1.5	0.339	0.509
	1500	4	0.5	0.277	0.138
	1500	4	1.5	0.277	0.415
	2000	4	0.5	0.239	0.119
	2000	4	1.5	0.239	0.359
	3000	4	0.5	0.195	0.098
	3000	4	1.5	0.195	0.293
	4000	4	0.5	0.169	0.085
	4000	4	1.5	0.169	0.254

Figure A2.3. Graphical summary of the power calculation



Annex 1: Results

Differential attrition and balance checks

We tested for differential attrition among participants who started but did not complete this experiment. From a linear regression there was no evidence for differential attrition ($p > 0.1$ for all treatment coefficients).

We also conducted balance checks on the final analysis sample using chi-squared tests for (categorical) covariates. Arms were balanced on all covariates (see Table A2.3[52]).

52 p-values were not adjusted for multiple comparisons.

Table A2.3. Balance check results for categorical covariates.

Covariate	Percentage per arm				p value	Balanced?
	Cash upfront	Rent guarantee	Damage deposit bond	Landlord liaison officer		
Gender						
Male	52.7	50.0	51.6	50.5	> .10	Yes
Female	47.3	50.0	48.4	49.5		
Landlord Experience						
0-5 years	15.9	14.9	13.7	16.7	> .10	Yes
5-10 years	20.7	22.7	23.6	20.6		
10+ years	63.4	62.4	62.7	62.7		
Location						
London	19.1	23.0	20.5	18.4	> .10	Yes
Midland	19.3	15.7	17.4	16.1		
North	18.8	20.1	20.5	21.4		
Scotland	0.3	0.3	0.5	0.7		
South and East	36.9	35.8	35.6	39.7		
Wales	5.5	5.1	5.6	3.5		
Northern Ireland	0.1	0	0	0.1		

Covariate	Percentage per arm				p value	Balanced?
	Cash upfront	Rent guarantee	Damage deposit bond	Landlord liaison officer		
Primary property locations						
East	6.8	7.0	6.1	5.5	> .10	Yes
East Midlands	11.1	10.1	9.0	9.4		
London	24.3	25.8	25.0	22.2		
North East	4.7	5.4	4.5	5.0		
North West	13.8	16.1	15.1	13.5		
Northern Ireland	0.1	0	0	0		
Wales	8.0	5.7	6.5	6.0		
Scotland	0.9	0.4	1.6	0.7		
Yorkshire	8.4	8.4	9.6	10.8		
South East	23.1	22.8	24.4	26.6		
South West	14.7	14.0	14.6	15.4		
Properties on LHA						
Yes	19.1	18.4	21.1	18.3	> .10	Yes
No	50.8	50.7	50.8	50.9		
Don't know	30.1	30.9	28.1	30.8		

Covariate	Percentage per arm				p value	Balanced?
	Cash upfront	Rent guarantee	Damage deposit bond	Landlord liaison officer		
Experience with UC tenants						
Yes	50.2	53.6	53.4	51.8	> .10	Yes
No	49.8	46.4	46.6	48.2		
Received help from a Local Authority						
Yes	17.8	17.5	18.9	18.0	> .10	Yes
No	82.2	82.5	81.1	82.0		

Primary analysis

As defined in the analytical strategy, since we didn't reach the threshold of 4,000 participants, we estimate regression models for the full sample of landlords who completed both experiments. Therefore, in all following regressions, we control for treatment assignment from the parallel experiment, with an additional reference category for those who completed this experiment first.

We ran three regression models for the primary outcome: with no covariates, with a limited set of 'standard' covariates, and with a full set of covariates we collected. Table 11 shows the AIC of each model. Since model 2 has the lowest AIC – indicating best fit – we use the limited set of covariates in the primary analysis as well as all secondary analyses.

Table A2.4. Model fit comparisons using the AIC statistic

Model	Model 1 (treatment assignments only)	Model 2 (treatments + standard covariates)	Model 3 (treatments + standard and landlord-related covariates
AIC	8,484.197	8,477.266	8,484.128

As shown in table A2.5 all of the treatment arms had a significant positive effect on the stated likelihood to accept the offer, based on 4 paired-sample t-tests comparing mean likelihoods before and after the updated offer, as shown before in table 10 .

Table A2.5. Tests of equivalence of baseline and updated offers, within respective intervention groups (p-values adjusted for 4 comparisons)

Paired-sample t-test comparing differences in mean reported likelihoods for each group	Cash upfront n=701	Rent guarantee n=692	Damage deposit bond n=678	Landlord liaison officer n=727
Difference in means	0.791	0.830	0.349	0.294
P-value	<0.001	<0.001	<0.001	<0.001

Some covariates in the regression model also showed significant associations with the primary outcomes.

Seeing this trial after the What to disclose trial may have had a small positive effect on the changes in likelihood of accepting the council's offer: all point estimates were positive (ranging from 0.07 to 0.17 points), with the effect of the Pre-tenancy training arm becoming significant at the .05 level

Female participants report slightly larger increases in the likelihood of accepting the offer (by 0.08 points)

Table A2.6. Regression table for the primary analysis

Outcome **(change in likelihood to accept tenant)**	**Coefficient** **(reference = cash upfront)**				
	Constant	Rent guarantee	Damage deposit bond	Landlord liaison officer	Rent-UC deficit (£200 vs £50)
Coefficient (Standard error)	0.889** (0.109)	0.031 (0.068)	-0.433** (0.061)	-0.510** (0.058)	-0.217** (0.041)
95% Confidence interval	[0.68,1.10]	[-0.10,0.16]	[-0.55,-0.31]	[-0.62,-0.40]	[-0.30,-0.14]
Standard covariates	Yes				
Custom covariates	No				
R squared	0.076				
Observations	2,798				

Primary analysis robustness check

Since we had a concern that the main analytical strategy may be statistically inefficient, due to a potentially skewed distribution of residuals, we have run the Mann-Whitney U test as a robustness check. Our expectation was correct: the distribution of primary model residuals was right-skewed (skewness = 0.914) and leptokurtic (kurtosis = 8.66). As shown in table A2.7 the results are qualitatively consistent with the main analysis: the same treatments, as well as the larger rent deficit, have a significant effect (after correcting p-values for multiple comparisons).

Table A2.7. Results of the primary analysis robustness check[53]

Outcome **(change in likelihood to accept tenancy offers)**	**Coefficient** **(reference = cash upfront)**			
	Rent guarantee	**Damage deposit bond**	**Landlord liaison officer**	**Rent-UC deficit (£200 vs £50)**
W-statistic	240,843	294,713	317,285	1,089,427
p-value	0.81	<0.001	<0.001	<0.001
Observations	1,393	1,379	1,428	2,798

Secondary analysis

Analysis of secondary outcomes

We asked participants three additional questions about the presented tenancy, all aimed at capturing potential mechanisms through which the treatments could have an effect. These questions were about: participants' expectations of late or missed rent payments; their expectations about the council's responsiveness; and their expectations about the level of profit obtained from the tenancy. Table A2.8 shows the descriptive statistics for these variables and tables A2.9-A2.11 present the regression estimates.

53 The number of observations in the trial arm column corresponds to the number of participant in that arm plus the number of participants in the cash upfront arm

Table A2.8. Descriptive table for the secondary outcome variables

	Cash upfront n=675	Rent guarantee n=670	Damage deposit bond n=644	Landlord liaison officer n=695
Outcome	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**
Perceived risk of late/missed rental payments	4.62 (1.50)	4.05 (1.71)	4.69 (1.53)	4.65 (1.50)
Expected profit from the tenancy	2.99 (1.26)	2.66 (1.17)	2.44 (1.14)	2.53 (1.14)
Expected level of responsiveness by the Council	3.53 (1.62)	3.57 (1.61)	3.41 (1.66)	3.43 (1.59)

Table A2.9. Regression table for the secondary outcome: perceived risk of late/missed rent payments

Outcome (perceived risk of late/ missed rental payments by the prospective tenant)	Coefficient (reference = cash upfront)				
	Constant	**Rent guarantee**	**Damage deposit bond**	**Landlord liaison officer**	**Rent-UC deficit (£200 vs £50)**
Coefficient (Standard error)	4.469** (0.143)	-0.563** (0.086)	0.073 (0.081)	0.038 (0.079)	0.140* (0.059)
95% Confidence interval	[4.19, 4.75]	[-0.73, -0.39]	[-0.09, 0.23]	[-0.12, 0.19]	[0.02, 0.26]
Standard covariates	Yes				
Custom covariates	No				
R squared	0.077				
Observations	2,798				

Table A2.10. Regression table for the secondary outcome: expected level of profit from the tenancy

Outcome (expected level of profit from the tenancy)	Coefficient (reference = cash upfront)				
	Constant	Rent guarantee	Damage deposit bond	Landlord liaison officer	Rent-UC deficit (£200 vs £50)
Coefficient (Standard error)	3.494** (0.111)	-0.305** (0.062)	-0.517** (0.061)	-0.470** (0.061)	-0.760** (0.043)
95% Confidence interval	[3.28, 3.71]	[-0.42, -0.19]	[-0.64, -0.40]	[-0.59, -0.35]	[-0.84, -0.68]
Standard covariates	Yes				
Custom covariates	No				
R squared	0.080				
Observations	2,798				

Table A2.11. Regression table for the secondary outcome: perceived likelihood of the council being suitably responsive

Outcome (perceived likelihood of the Council being suitably responsive)	Coefficient (reference = cash upfront)				
	Constant	Rent guarantee	Damage deposit bond	Landlord liaison officer	Rent-UC deficit (£200 vs £50)
Coefficient (Standard error)	3.748** (0.154)	0.040 (0.086)	-0.105 (0.088)	-0.112 (0.085)	-0.244** (0.062)
95% Confidence interval	[3.45, 4.05]	[-0.13, 0.21]	[-0.28, 0.07]	[-0.28, 0.05]	[-0.37, 0.12]
Standard covariates	Yes				
Custom covariates	No				
R squared	0.072				
Observations	2,798				

Subgroup analysis of the primary outcome

Participants were randomly split into two subgroups, one presented with a tenancy offer for a monthly rental price of £50 below the advertised amount, since the portion of housing benefits from UC was insufficient to cover the full rent; the other group faced a tenancy offer for a larger rental price deficit (£200 a month). In these analyses, we test whether our treatments had an impact on the primary outcome variable separately within these two subgroups.

In both subgroups, the results showed the same pattern as in the whole-sample analysis: there was no significant difference between the rent upfront and the rent guarantee arms but the damage deposit bond and landlord liaison officer both showed significantly smaller increases in the likelihood to accept the offer than cash upfront (see table A2.12-A2.13).

Table A2.12. Subgroup analysis results: small rent gap

Outcome (change in likelihood to accept tenant)	Coefficient (reference = cash upfront)			
	Constant	Rent guarantee	Damage deposit bond	Landlord liaison officer
Coefficient	0.916**	0.076	-0.481**	-0.586**
(Standard error)	(0.16)	(0.096)	(0.087)	(0.082)
95% Confidence interval	[0.60,1.23]	[-0.11,0.26]	[-0.65,-0.31]	[-0.75,-0.43]
Standard covariates	Yes			
Custom covariates	No			
R squared	0.051			
Observations	1,461			

Table A2.13. Subgroup analysis results: large rent gap

Outcome (change in likelihood to accept tenant)	Coefficient (reference = cash upfront)			
	Constant	Rent guarantee	Damage deposit bond	Landlord liaison officer
Coefficient	0.698**	-0.025	-0.380**	-0.434**
(Standard error)	(0.149)	(0.097)	(0.085)	(0.083)
95% Confidence interval	[0.41, 0.99]	[-0.22, 0.17]	[-0.55, -0.21]	[-0.60, -0.27]
Standard covariates	Yes			
Custom covariates	No			
R squared	0.047			
Observations	1,337			

Exploratory analysis

In the exploratory analysis, we compared time spent reading the offers across trial arms. As table A2.14, we didn't find any significant differences here.

Table A2.14. Exploratory outcome: time spent reading the Council's updated offer

Outcome (Time spent viewing updated offer)	Coefficient (reference = cash upfront)			
	Constant	Rent guarantee	Damage deposit bond	Landlord liaison officer
Coefficient	46.5**	1.06	0.35	-2.05
(Standard error)	(2.95)	(1.80)	(1.82)	(1.46)
95% Confidence interval	[40.72,52.28]	[-2.47,4.59]	[-3.22,3.92]	[-4.91,0.81]
Standard covariates	Yes			
Custom covariates	No			
R squared	0.019			
Observations	2,798			

Further exploratory analysis: Primary outcome analysis for subgroup of landlords with properties at or below the LHA rate:

For the subgroup of landlords with properties at or below LHA rate (n=515), we observed that cash-upfront significantly raised the change in stated likelihood of accepting the proposed tenancy arrangement, compared to the damage deposit bond and landlord liaison officer incentives. Similar to the main analysis, the rent guarantee offer did not differ significantly from cash upfront. We are cautious in making further interpretations from this analysis due to the very low sample size, but since the direction and magnitude of estimated coefficients are broadly consistent with the main analysis, this suggests that there are not strong differential treatment effects for this particular subgroup.

Table A2.15. Primary outcome: landlords with properties at or below LHA rate

Outcome (change likelihood to accept tenancy offer)	Coefficient (reference = cash up-font)			
	Constant	Rent guarantee	Damage deposit bond	Landlord liaison officer
Coefficient	0.606+	-0.056	-0.598**	-0.616**
(Standard error)	(0.363)	(0.200)	(0.167)	(0.155)
95% Confidence interval	[-0.11,1.32]	[-0.45,0.34]	[-0.93,-0.27]	[-0.92,-0.31]
Standard covariates	Yes			
Custom covariates	No			
R squared	0.10			
Observations	533			

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