

Cash Transfers for Care Leavers

A Randomised Controlled Trial



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Trial registration

The trial protocol and analysis plan for this trial are pre-registered here:

<https://osf.io/jaev7/files/xu8pv>

Funding and conflicts of interest

This trial was funded by the Centre for Homelessness Impact and the Cabinet Office Evaluation Task Force through the Evaluation Accelerator Fund. There are no other conflicts of interest.

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 Policy Institute at King's

The Policy Institute at King's College London works to solve society's challenges with evidence and expertise. The Institute's Experimental Government Team - which led the trial - uses innovation, experimentation, and data-driven measurement to help create a fairer society for all.

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In Brief

This is the first completed trial of unconditional cash transfers for any population affected by homelessness within the UK to reach a sample of sufficient size to draw quantitative conclusions. People with experience of the care system are vastly over-represented among those experiencing homelessness.

Care leavers who received the cash transfer were more likely to be in stable housing six months later and less likely to be sofa surfing, the equivalent to around eight more care leavers in every 100 having stable housing six months after receiving the payment

Young people who received the money reported better wellbeing over time, equivalent to between three and five more care leavers in every 100 saying their wellbeing had improved one year later

Young people given a cash sum on leaving care were six percentage points less likely to be arrested or have a criminal conviction, though base rates of criminal justice contact were low across both groups

Findings on health service use suggest participants may have shifted towards greater use of preventative primary care, with a reduction in overnight hospital stays at twelve months

Social workers were required to inform researchers of any adverse outcomes among trial participants, none were reported

Recommendations in Brief

The government should commission a larger randomised trial of unconditional cash transfers for care leavers. This trial provides the first experimental evidence in England on the effects of this intervention, and its findings are consistently encouraging. A larger study is needed to build a sufficiently robust evidence base and enable future meta-analyses

Policymakers should consider how one-off transfers can be complemented by ongoing support. The reduction in housing effects between six and twelve months suggests a single payment alone may not be sufficient for lasting change. Future trials should test whether higher amounts, staged payments, or integration with wider support packages can extend positive outcomes

Local authorities should ensure flexible delivery mechanisms are in place when implementing cash transfer programmes. The varying circumstances of care leavers mean that a one-size-fits-all payment model may not suit all participants. Programme design should allow for alternative arrangements where appropriate

Future evaluations should build in longer follow-up periods and pre-specified analyses of spending, income, and specific homelessness outcomes from the outset, to capture the full range of effects suggested by this trial

Executive Summary

This paper reports the results of a randomised controlled trial of unconditional cash transfers for care leavers.

Eligible individuals were care leavers aged 18–25, known to the local authority, without safeguarding concerns, and with self-reported savings under £4,000. Local authorities identified eligible individuals, who were then invited to participate and provided with detailed study information. After informed consent, participants completed a baseline online survey capturing demographics and measures of housing and financial wellbeing, social connectedness, subjective wellbeing and contact with services. Eligibility was subsequently confirmed in collaboration with local authorities to address potential mis-referrals.

302 participants from nine local authorities were recruited, with 100 randomised to the treatment condition and 202 to the control condition, at randomisation rate of 1:2.

99 Participants in the treatment condition received a £2,000 unconditional cash transfer, while post randomisation one participant was identified as ineligible and did not receive the transfer.

Participants were followed up at six and twelve months post randomisation, and surveyed on a range of outcomes including housing security, financial wellbeing, wellbeing, social connectedness, criminal justice contact and health contact. Approximately 65% of participants were retained on average between midline and endline. There is no evidence of imbalance or differential attrition between time points.

We find signs of positive effects on housing security among the treatment group compared to the control group, when controlling for baseline levels of the outcome measure and participant demographics in line with the trial protocol ($p < 0.1$). The effect on housing security is smaller at the twelve months point, but remains positive, if much less significant. We see a particular reduction of 6.6% points in the likelihood of sofa surfing at six months, though this declines in magnitude (and significance) to 3.3 percentage points at endline. Put another way, six months after receiving the cash transfer this is equivalent to eight more care leavers out of every 100 having stable housing, and six or seven fewer sofa surfing out of every 100, although this was less pronounced a year later.

We find a substantial drop in the likelihood that participants are arrested, of 6% points at both midline ($p < 0.1$) and endline ($p < 0.05$). At endline participants are also significantly less likely to have a criminal conviction at twelve months (6% points, $p < 0.05$). In other words, one year after getting the payment six fewer care leavers in every 100 were likely to be arrested and six fewer in every 100 had a criminal conviction

than those who did not receive this sum. These results are very encouraging, but need to be contextualised by low levels of the outcome among both treatment and control group.

The findings relating to the use of healthcare services find an increase at midline in the likelihood of using secondary care services such as a GP or drop-in clinic ($p < 0.2$), and a substantial (9%, $p = 0.23$) reduction in the number of hospital stays at endline. This is equivalent after six months to between eight and 10 more care leavers out of every 100 using a GP or walk-in health service compared to the group who did not receive the payment service, although this difference was not seen a year later; and to nine fewer overnight hospital stays per 100 compared with the control group. Taken together, this suggests that the financial support might facilitate greater use of preventative services.

There are consistently positive effects on participants' well-being, and on their relationships with others ($p < 0.1$). Effects on some outcomes, like financial stress and healthcare use, grow larger over the course of the study. This means that, for every 100 care leavers given the cash lump sum, between three and five more reported that their wellbeing was good after a year in comparison with those who did not receive a payment.

The sample for the study is substantial compared to some other, previous studies, but further studies of this intervention should be conducted to increase total sample size and allow for potential meta-analyses.

Robustness checks and sensitivity analyses find results with the same pattern as our main analysis.

Key limitations include the relatively small size of the study, differential attrition between treatment and control groups, staggered recruitment across sites, which may introduce temporal variation, and inability to blind participants or local authorities.

This trial provides the first experimental evidence in England on the effects of an unconditional cash transfer for care leavers. Overall, the findings from the trial are encouraging, with consistent findings across analyses of positive effects on the outcomes of interest, particularly in the most robust analyses. Effects on some housing outcomes appear to shrink over time, which suggests that a one-off transfer of this amount may be insufficient to have durable effects on housing outcomes.

On the strength of this evidence, we recommend that the intervention is scaled up to larger numbers of care leavers in a larger randomised trial.

Introduction

Background

The concept of unconditional cash transfers, as a means to combat poverty and improve outcomes, has a long history.¹ Advocated on both ethical and practical grounds, such transfers have been extensively studied in the context of developing countries.² More recently, there has been growing interest from both policymakers and researchers in exploring the potential of unconditional cash transfers in developed countries, across a range of policy areas. This interest has been accompanied by a small but increasing number of empirical studies investigating their impacts. Examples include a randomised trial of basic income in Finland, a study of one-off cash transfers aimed at alleviating homelessness in Canada, an ongoing quasi-experimental evaluation of a basic income programme for care leavers in Wales and most notably the first unconditional cash transfers trial in the UK aimed at people suffering from homelessness, funded by the Centre for Homelessness Impact.³

Despite this emerging body of research, the evidence base remains fragmented. This is partly due to the diverse groups hypothesised to benefit from unconditional cash transfers—ranging from care leavers and individuals experiencing homelessness to those being discharged from hospitals or prisons, and even the general population in the case of UBI (although this is quite a different policy to restricted, targeted, and time limited transfers). Additionally, there is significant variability in the design of these interventions, including differences in the amounts, frequency, and duration of payments, as well as the range of outcomes they aim to influence.

Objectives

This trial was designed to test the impact of the provision of an unconditional one-off lump sum to care leavers in the UK. This vulnerable group often faces significant challenges as they transition from care into adulthood, including housing insecurity, lower levels of well-being and barriers to education and employment.⁴ By offering direct support, the trial aims to empower care leavers to make choices that best suit their needs, fostering stability and long-term positive outcomes while mitigating the risk of adverse outcomes and thus reducing the future use of services and associated costs.

The structure of the remainder of this paper is as follows. First, we provide an overview of the trial's design followed by a summary of the recruited sample and the results of the trial. Finally, we discuss the implications of our findings and offer recommendations for future research.

Methods

Trial Design

This trial was conducted across nine local authorities in England, beginning in June 2023. The trial design was outlined in a published research protocol (Sanders and Vallis, 2023) and received ethical approval from the King's College London Social Sciences Research Ethics Committee (reference HR/DP-22/23-35202). The study aimed to evaluate the impact of a one-off £2,000 unconditional cash transfer provided to care leavers in England.

Local authorities were recruited to participate in the trial and began enrolling participants once they were ready, resulting in a staggered start to recruitment. This setup established the trial as a multi-site study (Venable, 2018).

The primary outcomes for this trial are housing security (measured through the Housing Security Scale developed by Frederick et al., (2014)) and financial wellbeing (measured through the InCharge Financial Distress/Well-being scale developed by Prawitz, et al., (2006)) at 12 months post-randomisation. Secondary outcomes include subjective wellbeing (WEMWBS), social connectedness (ENRICH social support instrument), and criminal justice involvement and health service use (MHCLG Rough Sleeping Questionnaire (MHCLG, 2020)) at twelve months, and all outcomes at the six month point.

Changes to the trial protocol

In addition to the analyses we proposed in the protocol, we also look at spending habits, other sources of income, and specific types of homelessness and use of temporary accommodation as exploratory analyses. These were not pre-specified but were considered of substantial interest and relevance to the evaluation.

Settings

Nine local authorities in England were recruited to this study: Birmingham, Bolton, Camden, Rochdale, Salford, Stockport, Walsall, Warrington, and Warwickshire. Research was conducted in partnership with case workers and officials in these local authorities.

Participant Recruitment and Eligibility

Participants were identified by local authorities based on pre-defined eligibility criteria. To be eligible, individuals had to:

- Be care leavers aged 18–25.
- Be known to the local authority and pose no safeguarding concerns.
- Have savings of less than £4,000 to avoid potential impacts on benefit eligibility (self reported during recruitment).

Once identified, case workers from the local authorities discussed the details and purpose of the trial with the potential participants. However, they were not informed of the cash transfer amount to minimise undue influence on their decision to participate. After providing informed consent, participants completed a baseline online survey. This survey collected demographic data (e.g., gender, date of birth, ethnicity) and measured key outcomes, including housing and financial wellbeing, social connectedness, well-being, access to public services and contact with the justice system..

Following baseline data collection, the research team worked with local authorities to confirm participants' care leaver status and ensure they met the risk-based eligibility criteria. This additional step was necessary to account for potential mis-referrals, such as individuals who were not care leavers or who did not meet the eligibility requirements.

Randomisation

Once eligibility was confirmed, participants were randomly assigned to either the treatment or control group. Randomisation was stratified by local authority, with participants having a one-third probability of being allocated to the treatment group and a two-thirds probability of being allocated to the control group. This allocation ratio was chosen to maximise statistical power while constrained to deliver 100 cash transfers. Randomisation was carried out in Stata 17 by the research team. Both participants and local authorities were informed of the allocation immediately after randomisation.

Participants in the control group were informed about the amount of the transfer during debriefing, in accordance with ethical guidelines for this project. All participants received a £20 shopping voucher as compensation for completion of surveys at each time point.

Intervention and comparator

Participants in the treatment group were asked to provide their payment details to receive a £2,000 cash transfer, which was transferred within two weeks of their being randomised.

In two cases, local authorities raised concerns about the potential risks of providing the full £2,000 transfer upfront. For these individuals, alternative arrangements were made:

- One participant received the payment in smaller installments over four months.
- For another participant, the local authority managed the funds on their behalf.

Participants in the comparator group received the same information as those in the treatment group, but did not provide their bank account details and did not receive any payment except for vouchers for research participation. All participants continued to receive support as usual from the local authority in which they had been in care.

Outcomes

Primary Outcomes

As mentioned under Trial Design, we are interested in two primary outcomes: Housing Security and Financial Wellbeing. The measures used are described below.

Housing Security

- Instrument: Housing Security Scale (HSS)⁵
- Description: A 12-item likert-type questionnaire rated from 1 (Strongly disagree) to 5 (Strongly agree). This instrument covers scales on housing type, recent housing history, current housing tenure, financial status, standing in the legal system, education and employment status, and subjective assessments of housing satisfaction and stability.

Financial Wellbeing

- Instrument: InCharge Financial Distress/Financial Wellbeing Scale⁶
- Description: An 8-item, likert-type questionnaire rated from 1 (Overwhelming financial distress) to 10 (No financial distress/complete financial wellbeing). This scale measures a participant's financial state through self-reported distress or wellbeing. It has been tested on both the general population and individuals experiencing financial distress.

Secondary Outcomes

Secondary outcomes and their measures are described below.

Social Connectedness

- Instrument: ENRICHD Social Support Instrument⁷
- Description: A 7-item, self-reported scale. Items are summed to create a continuous total score. This scale is used to measure social connectedness, it has been used in research with participants with experience of rough sleeping in previous studies.

Subjective Wellbeing

- Instrument: Warwick–Edinburgh Mental Wellbeing Scale (WEMWBS)⁸
- Description: A 14-item wellbeing questionnaire with likert-type questions rated from 1 (None of the time) to 5 (All of the time). This instrument was developed to enable the monitoring of mental wellbeing in the general population and the evaluation of projects, programmes and policies which aim to improve mental wellbeing.

Contact with Health Services

- Instrument: MHCLG Rough Sleeping Questionnaire, Section F with items relating to public service contact
- Description: Provides detailed information on patterns of contact with health services.

Contact with the Criminal Justice System

- Instrument: MHCLG Rough Sleeping Questionnaire, Section H of criminal justice-related items
- Description: Provides detailed information on patterns of contact with the criminal justice system.

Power calculations

Power calculations for various attrition scenarios are shown in the table below as defined in the protocol.

Table 1. Power Calculations

Unit of randomisation ⁹	Postcode		
Alpha	0.05		
Power	0.8		
Baseline-Endline Correlation (Housing Security)	0.5		
Treatment Arm Sample Size	100		
Total Sample Size	300		
Attrition	0%	20%	50%
Effect Size	0.298	0.334	0.423

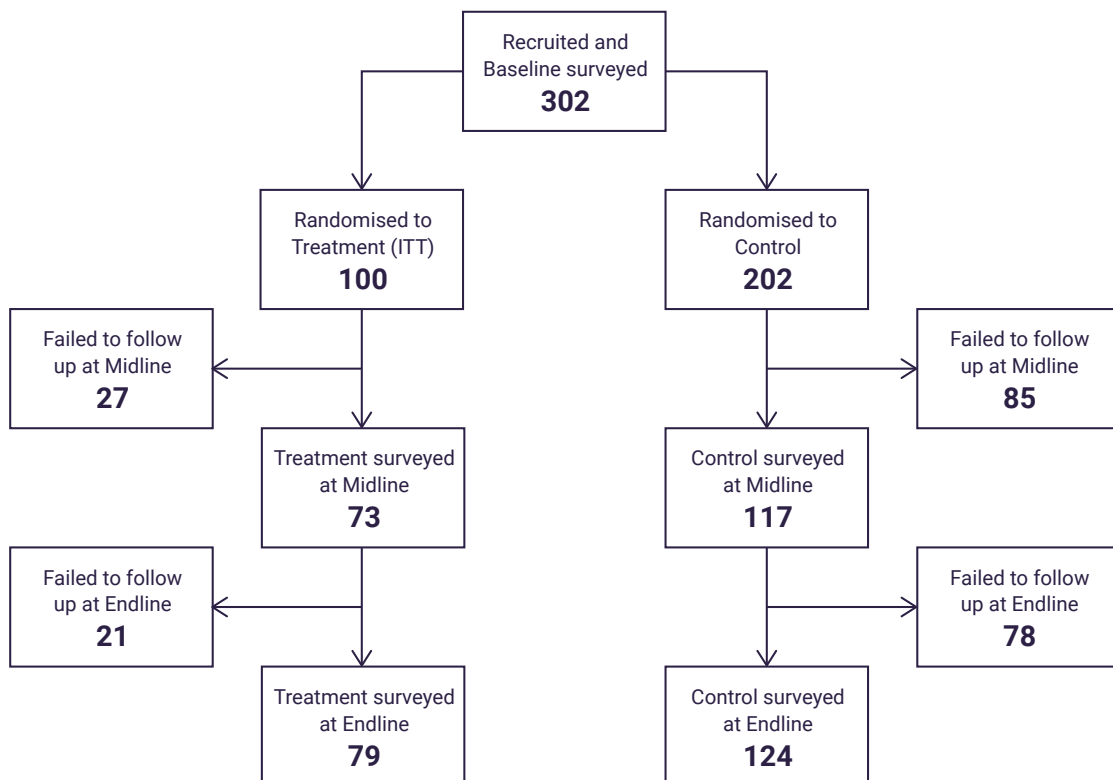
Results

Participant flow

Six months (midline) and twelve months (endline) after the intervention, participants completed a follow-up survey, which revisited the same outcome measures collected at baseline. The response rate was 62.3% (N=188) at midline and 67.2% (N=203) at endline. To maximise follow-up participation, the research team reached out via email, text messages, and phone calls. For participants who were repeatedly unresponsive, local authorities were contacted to assist with re-establishing contact. Particularly high levels of resource were devoted to ensuring endline responses due to this being the primary point of analysis.

The CONSORT diagram below provides an overview of participant flow through the trial.

Figure 1. Consort diagram



Recruitment

Recruitment started in May of 2023 and ran for thirteen months. The first cohort assignment to treatment and control groups took place in June of 2023, with the final cohort assigned in July of 2024. Recruitment was conducted in a total of 9 Local Authorities across England. Birmingham and Walsall contributed the largest number of recruited care leavers with sample sizes representing 33.1% and 18.2% of the total sample, followed by Stockport and Camden, each with 9.3%. Table 2, below shows recruitment numbers for each local authority involved in the trial.

Table 2. Recruitment by Local Authority

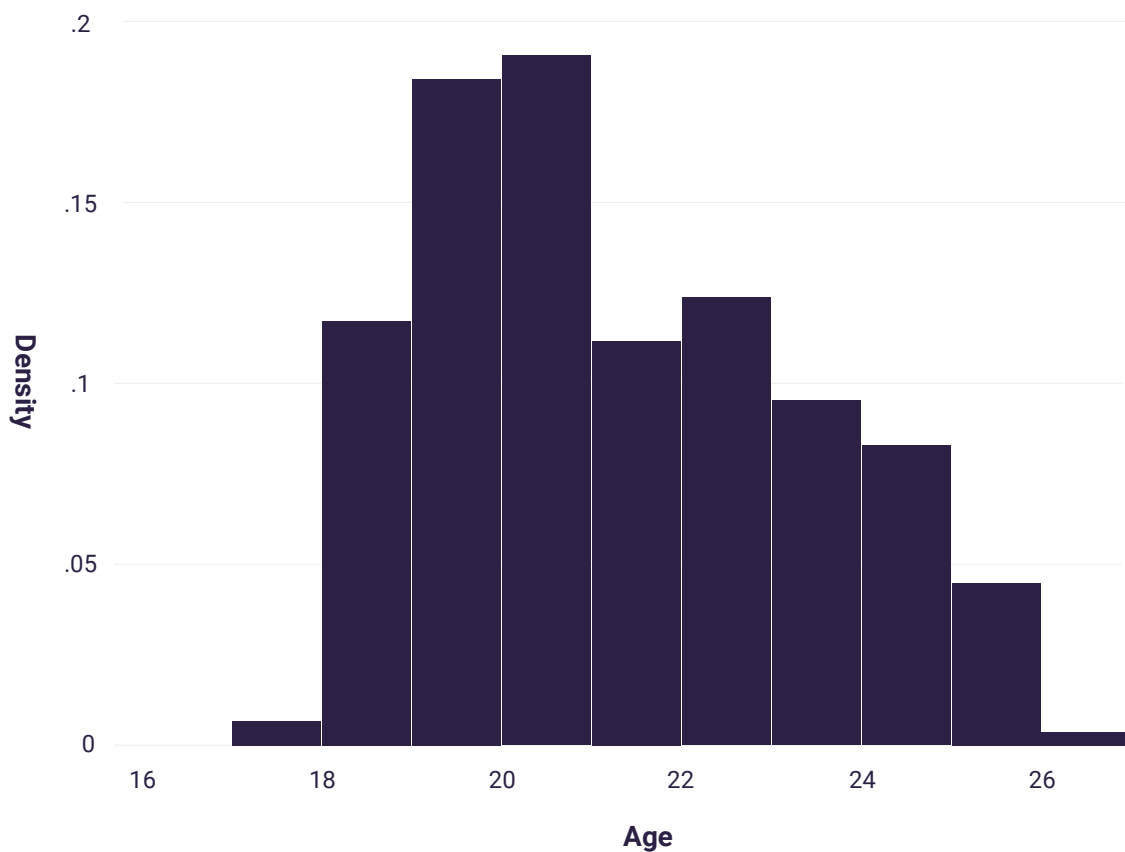
Local Authority	Baseline	(%)
Birmingham	100	33.11
Bolton	21	6.95
Camden	28	9.27
Rochdale	18	5.96
Salford	12	3.97
Stockport	28	9.27
Walsall	55	18.21
Warrington	14	4.64
Warwickshire	26	8.61
Total	302	100

Participant Demographics

The average age of participants was 21.3 at time of recruitment with a confidence interval of [21.0; 21.5]. Birmingham, Stockport and Walsall exhibited the widest distributions with the highest average age.

The distribution of birth year for all participants is shown below. The resulting distribution deviates slightly from normality due to the stratified nature of sampling.

Figure 3. Distribution of participants' Age



Ethnic background was largely representative of the care leaver population which tends to exhibit some deviation from the population as a whole, with white young people underrepresented among the care leaving cohort. Table 3, below, shows the distribution of ethnicities in both the sample and the overall population.

This finding aligns with previous work by Coram Voice which showed that these groups are typically over-represented among care populations (Briheim-Crookall et al., 2020). The resulting ethnicity breakdown is compared to the latest census data for ages 18-25 below (ONS, 2023). Statistical testing using the chi-squared test confirmed the frequencies were statistically significantly different.¹⁰

Table 3. Ethnic origin

	Sample Frequency	Sample (%)	Population (%)
Any other ethnic background	12	4.07	2.54
Asian/Asian British	22	7.46	11.74
Black British/African/Caribbean	45	15.25	5.25
Mixed/Multiple ethnic groups	24	8.14	4.03
White	192	65.08	76.20
Total	295	100	100

53% of participants were female and 44% male, with 3% identifying as other genders, as shown in table 4, below. This runs slightly counter to the overall population of care leavers, who are more likely to be male than female according to the most recent data published by the Department for Education.¹¹

Table 4. Gender

Gender	Sample frequency	Sample (%)
Female	159	52.82
Male	133	44.19
Other	9	2.99
Total	301	100

Balance Checks

Balance checks were carried out at each stage of the trial - recruitment, midline, and endline. Participants were well balanced between treatment and control on all characteristics at all stages, with the exception of a significant imbalance in the proportion identifying their gender as “other” at endline.

Attrition & Balance

The overall retention rate was 62.3% at midline and 67.2% at endline. Variation between response rates was not large with generally homogeneous retention between local authorities. The highest response rate was from Salford (83% at midline and 75% at endline) followed by Birmingham (67% at midline and 71% at endline), Stockport (68% at midline and endline) and Walsall (69% at midline and 65% at endline).

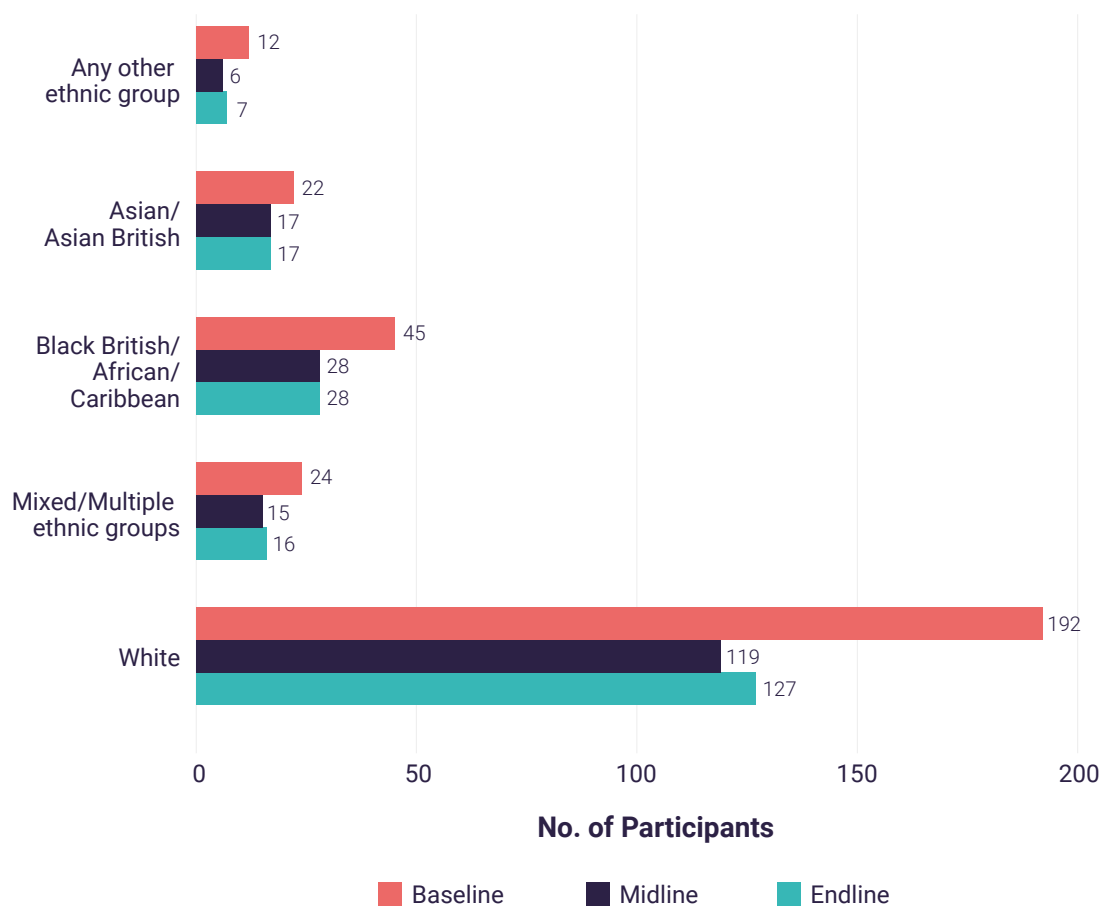
Table 5. Retention by Local Authority

	Baseline	Midline	Endline
Birmingham	100	67 (67%)	71 (71%)
Bolton	21	10 (48%)	10 (48%)
Camden	28	14 (50%)	17 (60.7%)
Rochdale	18	10 (56%)	14 (78%)
Salford	12	10 (83%)	9 (75%)
Stockport	28	19 (68%)	19 (68%)
Walsall	55	38 (69%)	36 (65%)
Warrington	14	7 (50%)	11 (79%)
Warwickshire	26	15 (58%)	16 (62%)
Total	302	190 (63%)	203 (67%)

Average age at baseline of the retained sample 0.21 years older at midline and 0.19 at endline. Among genders, the highest responders were females with a retained sample of 72.5% at midline and 78.1% at endline, compared to 51.1% and 54.9% for males at midline and endline, respectively. This is consistent with findings across other surveys of care leaver populations in England and Wales (Holland et al., 2024; Briheim-Crookall et al., 2020), which find that older, and female care leavers are more responsive.

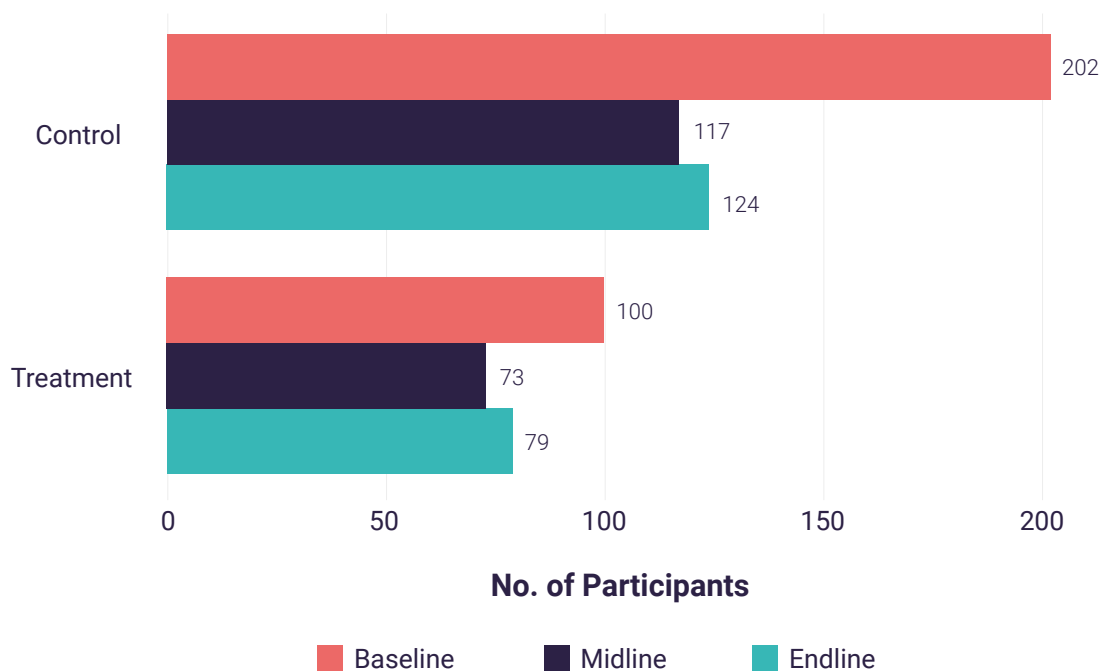
Retention among ethnicities was largely homogeneous, with most representing retainment close to 62%. At the higher end of retention were participants with an Asian/Asian British ethnic background with 72% for both time points, while at the lower end participants from Any other ethnic background with 50%; a chi-square test of independence revealed no statistically significant differences between time points.¹²

Figure 12. Retention by ethnic origin



Retention between treatment arms was heterogeneous with 58% retained for the control and 72% for treatment at midline and 61.4% for the control and 79% for the treatment at the endline; a chi-test of independence revealed no statistically significant differences between treatment and control groups in their rate of retention¹³, and as described above, both groups remained balanced at both follow up points. A breakdown of the retained number of participants by arm is shown below.

Figure 13. Retention by Treatment Arm



Balance

Balance checks were conducted at baseline, midline and endline, to test the statistical comparability of the treatment and control groups at each stage. We find that the sample was well balanced at baseline on all baseline characteristics that we measure, including baseline levels of the outcome measure.

At midline and endlines, despite some differential numerical attrition, the treatment and control groups remain balanced on most demographics except for gender, with males more likely to be retained. There was no imbalance detected in terms of baseline levels of any outcome measures. Our analytical model controls for gender, as well as for baseline characteristics, and so any imbalance is controlled for by design.

Findings

We now proceed to provide our analysis of the results of the trial on estimated effect sizes for all primary and secondary outcomes. Analysis was carried out in line with the pre-registered protocol, and full regression model estimates can be found in Appendix A. We describe each finding in turn, but the main analysis findings are summarised in table 6, below.

All analyses are carried out on an Intention to Treat basis, such that a participant's treatment status is assumed to be the same as their treatment assignment. Given the high compliance with the intervention (97%), this analysis is not meaningfully different to a treatment-effect-on-the-treated approach. Due to uneven random allocation, with one third of participants assigned to the treatment condition, analysis was not possible to conduct blind, but analytical code was written before data were received for analysis.

Table 6. Estimates for Primary and Secondary Outcomes

Outcome	Unadj. Mean (Control)	Unadj. Mean (Treat)	Effect Size (Glass's d)	ES CI (95%)	P-value	Time Point	Sample Size
Housing Security	3.791	3.831	0.261	[-0.038; 0.560]	0.087	Midline	184
	3.746	3.702	0.103	[-0.219; 0.425]	0.531	Endline	197
Financial Wellbeing	3.916	3.821	-0.081	[-0.347; 0.186]	0.553	Midline	185
	3.502	3.848	0.178	[-0.104; 0.460]	0.215	Endline	194

Outcome	Unadj. Mean (Control)	Unadj. Mean (Treat)	Effect Size (Glass's d)	ES CI (95%)	P-value	Time Point	Sample Size
Eviction (Antisocial behaviour)	0.027	0.015	-0.039	[-0.095; 0.016]	0.162	Midline	175
	0.018	0.027	-0.021	[-0.070; 0.029]	0.412	Endline	182
Caution	0.009	0.015	-0.007	[-0.063; 0.049]	0.799	Midline	174
	0.026	0.000	-0.038	[-0.089; 0.013]	0.146	Endline	182
Arrest	0.027	0.015	-0.058	[-0.126; 0.009]	0.089	Midline	175
	0.027	0.014	-0.061	[-0.118; -0.004]	0.035	Endline	182
Conviction	0.000	0.029	-0.015	[-0.073; 0.044]	0.622	Midline	177
	0.017	0.000	-0.063	[-0.114; -0.012]	0.015	Endline	185
GP/Nurse Walk-in	2.847	2.985	0.277	[-0.090; 0.642]	0.136	Midline	173
	2.909	2.865	0.112	[-0.247; 0.471]	0.541	Endline	179

Outcome	Unadj. Mean (Control)	Unadj. Mean (Treat)	Effect Size (Glass's d)	ES CI (95%)	P-value	Time Point	Sample Size
Hospital Stay	1.72	1.694	-0.054	[-0.371; 0.264]	0.739	Midline	166
	1.804	1.627	-0.157	[-0.419; 0.105]	0.239	Endline	170
Subjective Wellbeing	2.79	2.939	0.193	[-0.076; 0.463]	0.159	Midline	185
	2.73	2.91	0.239	[-0.040; 0.518]	0.092	Endline	196
ENRICHD Social Support	3.038	3.128	0.165	[-0.096; 0.425]	0.214	Midline	185
	2.961	3.055	0.148	[-0.106; 0.401]	0.253	Endline	194

Effect sizes are scaled, in line with the Centre for Homelessness Impact's statistical analysis guidance (CHI, 2024) using Glass's delta, which expresses the size of an effect relative to the standard deviation of the outcome in the control group.¹⁴ Also in line with CHI's guidance, we do not make use of a binary interpretation of statistical significance at the 0.05 level, and instead attempt to produce a nuanced interpretation of the findings.

Primary Outcomes

Housing Security

Results on the primary outcome of Housing Security Scale (HSS) are presented below. To construct the HSS we estimate the average of all non-missing housing security outcomes for each participant and then express them in the form of a combined value¹⁵ ranging from 1 to 5 (see Appendix C).

Figure 14. Effects on Housing Security Scale

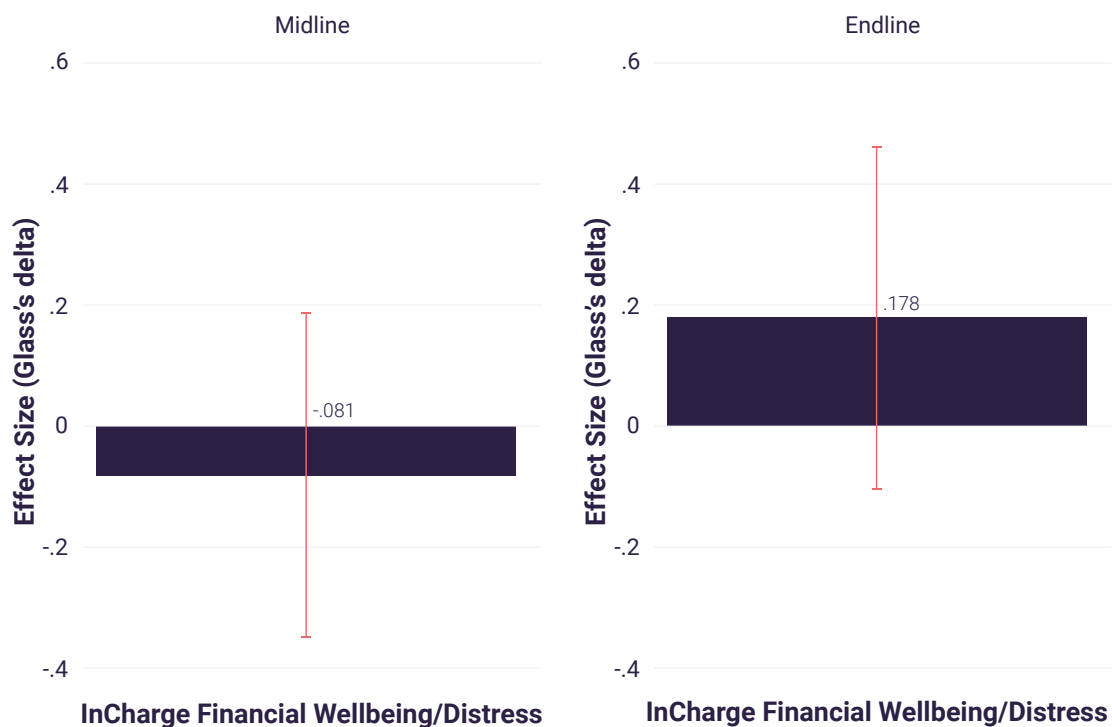


The resulting effect of the intervention on the HSS at midline is positive with a moderate effect size and a statistically significant effect at the 10% level with an absolute effect of 0.153 ($d=0.26$, $p=0.09$). The effect remains positive at endline but decreases in magnitude and becomes statistically insignificant at endline, with an absolute effect size of 0.060 ($d=0.10$, $p=0.53$). This suggests that any effect on housing security during the lifetime of the trial is fairly short in duration.

Financial well-being

Results on the effects of the intervention on Financial Distress/Well-being scale at midline were inconclusive. The estimated effect was negative, but small in size (0.173) and statistically insignificant ($d=-0.08$, $p=0.55$). Results at endline were positive (more financial wellbeing/less distress), with a moderate effect size of 0.346 ($d=0.18$, $p=0.21$).

Figure 15. Financial Distress/Financial Well-being Scale



Secondary Outcomes

We now turn to our secondary outcome measures. There are several secondary outcome measures for this trial, reflecting the number of potential outcomes for care leavers on which unconditional cash transfers might plausibly have an impact. These are; contact with the justice system; use of health services; subjective wellbeing, and social connectedness.

Contact with the Justice System

Analysis of contact with the justice system revealed a number of effects, particularly at endline. We find a reduction in the probability of arrest by 6 percentage points ($d=-6pp$, $p=0.088$); this effect is sustained at endline and becomes statistically significant at the 5% level ($d=-6pp$, $p=0.03$). We also see a statistically significant reduction in Convictions at endline ($-6pp$, $p=0.02$). These results should be contextualised by low overall levels of these outcomes in the control group.

Surprisingly, participants in the treatment group were more likely to receive an anti-social warning letter than those in the control group ($d=3pp$, $p=0.14$); a similar effect is also observed at endline albeit reduced and less significant ($d=1.8pp$, $p=0.24$). This observed effect could be the result of participants transitioning from more extreme to less extreme behaviour as a result of the intervention. Eviction as a result of anti-social behaviour also fell at midline ($d=-4pp$, $p=0.16$) but became statistically insignificant at endline ($d=-2.1pp$, $p=0.41$)

We also find other significant effects at endline, such as a reduction in the likelihood of a receipt of Parenting orders ($d=-3.5pp$, $p=0.09$) Penalty notices ($d=-2pp$, $p=0.18$) and Notices seeking possession of home (NOSP) ($d=-2.8pp$, $p=0.21$) as a result of anti-social behaviour (ASB).

Figure 18. Arrests and Cautions

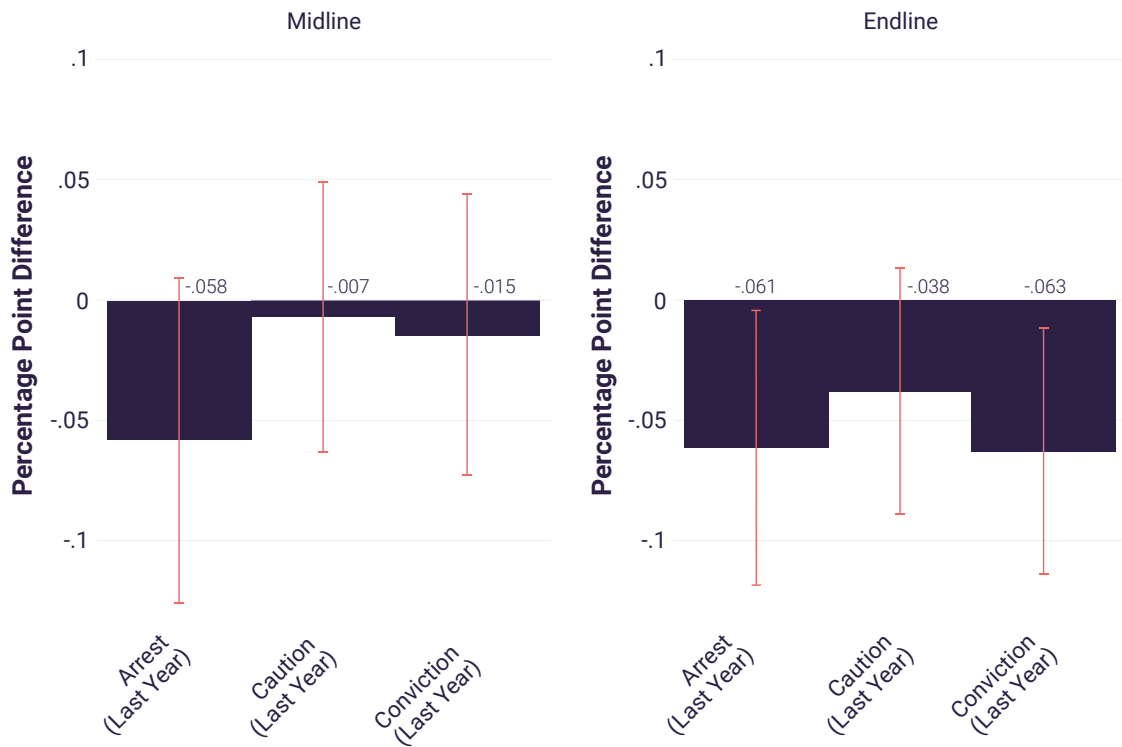
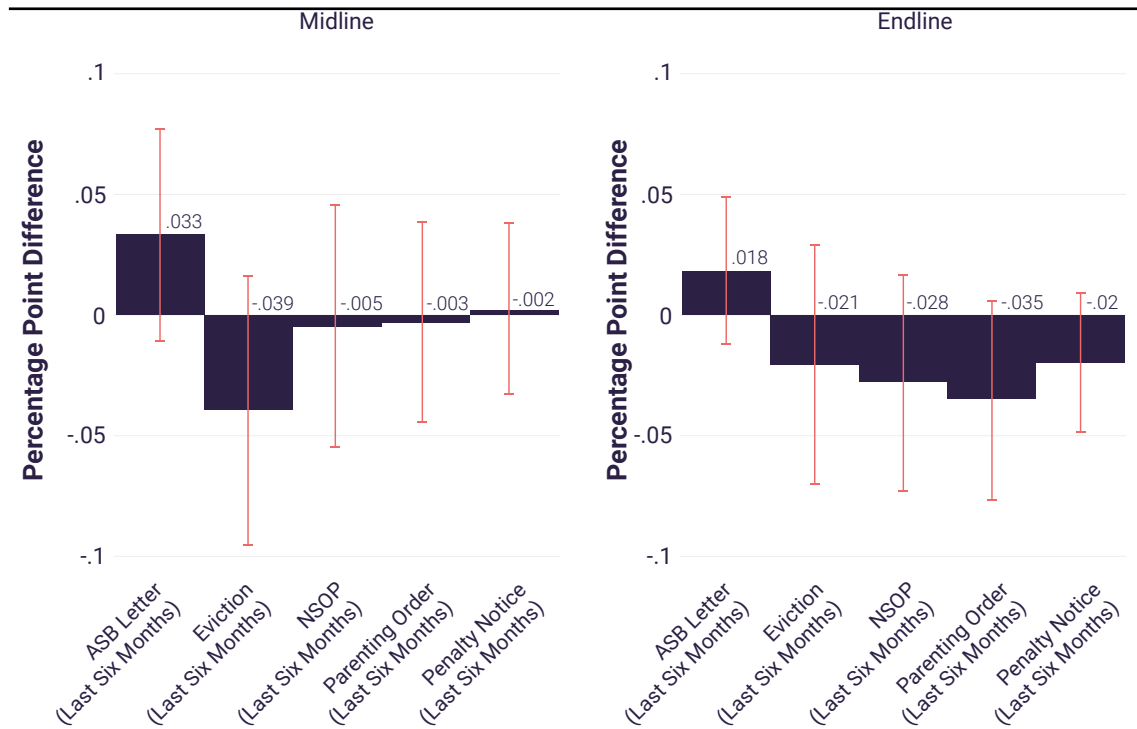


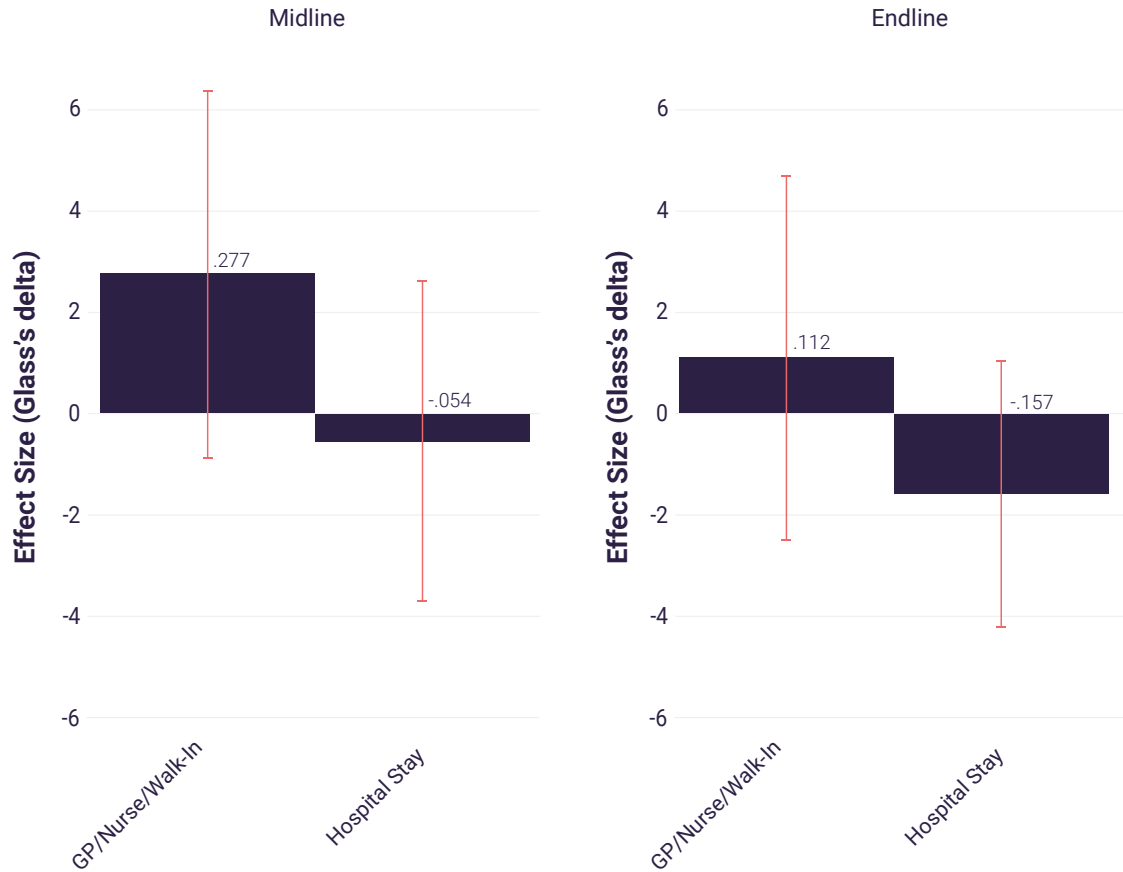
Figure 19. Anti-Social Behaviour



Contact with Health Services

For contact with health services we observed a moderately sized, positive effect on the use of GP/Nurse walk-in centres at midline, equivalent to 0.32 extra visits per participant ($d=0.28$, $p=0.14$), and small, but imprecisely estimated reduction in the number of hospital stays, equivalent to 0.05 fewer visits per participant ($d=-0.05$, $p=0.74$). At endline, the effect on GP/Nurse walk-in centres had reduced in magnitude, to 0.11 fewer visits per participant, ($d=0.11$, $p=0.54$), while the magnitude of the decrease in use of hospitals had increased by 0.15 visits per participant ($d=-0.16$, $p=0.24$). Although these findings are not conclusive, they suggest a pivot by participants towards short term use of primary care services, and a medium term shift away from the use of secondary care. Although our initial hypothesis was that health outcomes would improve, the pattern of service use was not thought about in detail.

Figure 20. Use of Health Services



These results are consistent with previous findings on cash transfers. For instance, a study by the World Health Organization reported strong evidence that cash transfers, particularly when conditional on health-related behaviours, can lead to increased healthcare service uptake, including preventive service attendance.¹⁶ Similarly, van Daalen et al. conducted a mixed-methods systematic review of conditional and unconditional cash transfers trials and their impact on health-related outcomes.¹⁷ Their analysis indicated that out of 34 trials included, 19 reported positive impacts on use of health services.

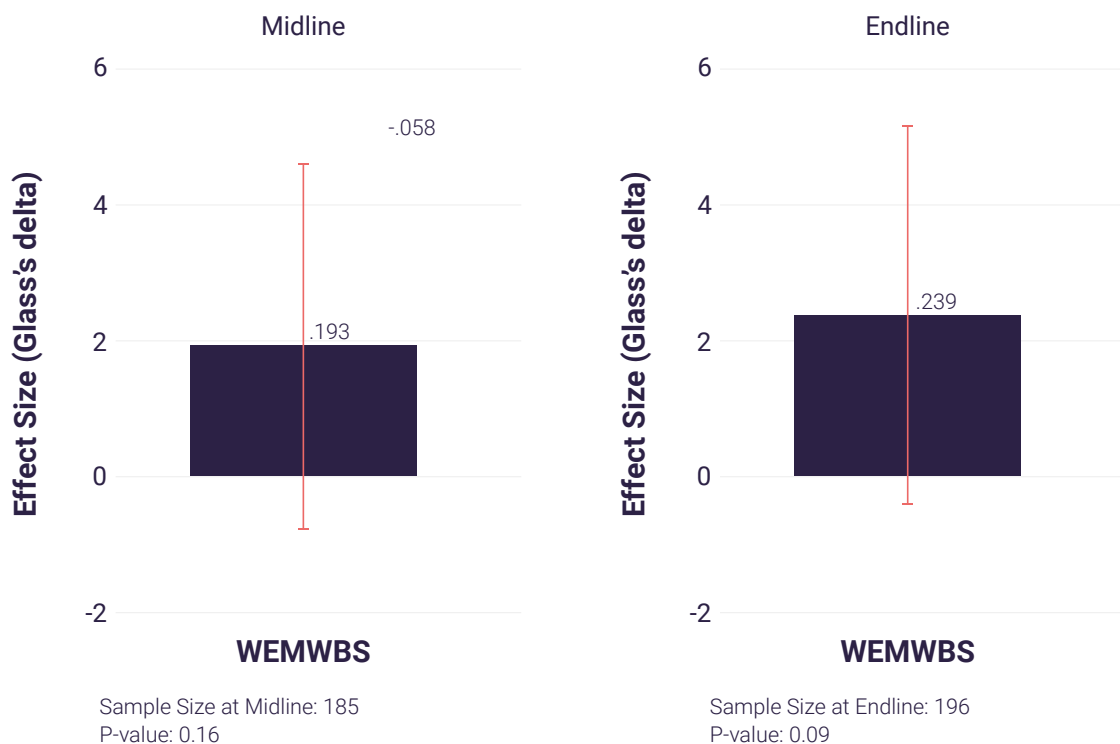
Subjective Well-being

Next, we consider the effect on wellbeing, measured using the Warwick-Edinburgh Mental Well-being Scale, which can be found in Appendix C.

The estimate of the effect size on well-being is positive at midline, increasing by 0.19 on the five point scale ($d=0.19$, $p=0.16$) and statistically significant at the 20% level. At endline, the effect size is slightly larger, at 0.20 becoming moderate in size and statistically significant at the 10% level ($d=0.24$, $p=0.09$).

These results are consistent with a large number of previous literature on cash transfers interventions. For instance, a meta-analysis by McGuire et al. of 37 studies, covering 100 outcomes and 112,245 individuals in low- and middle-income countries, found that cash transfers significantly improved mental health and subjective well-being.¹⁸ The magnitude of the effect was strongly associated with the amount of cash transferred, both in absolute terms (0.08 standard deviations (SD) per \$100 PPP) and relative to previous income (0.10 SDs for each doubling of income); unconditional cash transfers specifically, were found to have a slightly greater impact than conditional ones (+0.04 SDs).

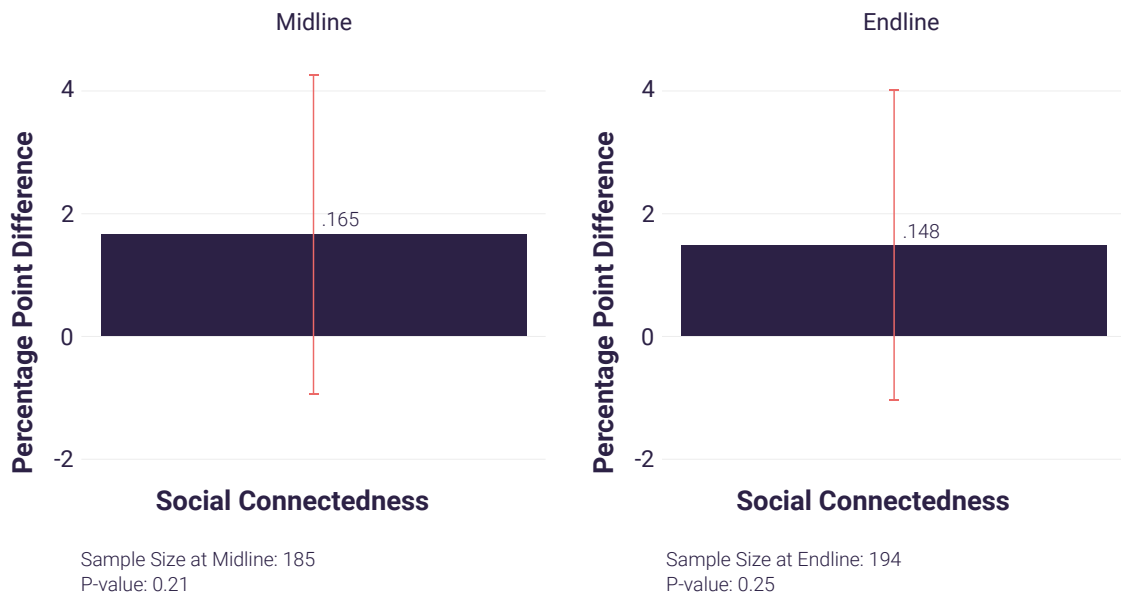
Figure 16. Subjective Well-being



Social Connectedness

We now look at participants' perceived social connectedness, measured through the ENRICHD Social Support instrument. Positive changes were also observed in the treatment cohort's personal relationships with a positive and small to moderate effect on the social connectedness scale at midline of 0.20 on a five point scale ($d=0.17$, $p=0.21$), sustained at endline with a similar effect size (0.17) and significance level ($d=0.15$, $p=0.25$).

Figure 17. Social Connectedness



Exploratory Analysis

We now conduct exploratory analysis, which were not pre-specified, but which aim to help us understand the effects of the intervention in more detail, or provide greater insight into the mechanisms. First, we consider the effects of the cash transfer on participants' use of temporary accommodation, and their likelihood of 'sofa surfing'.

Figure 22. Effects on Temporary Accommodation

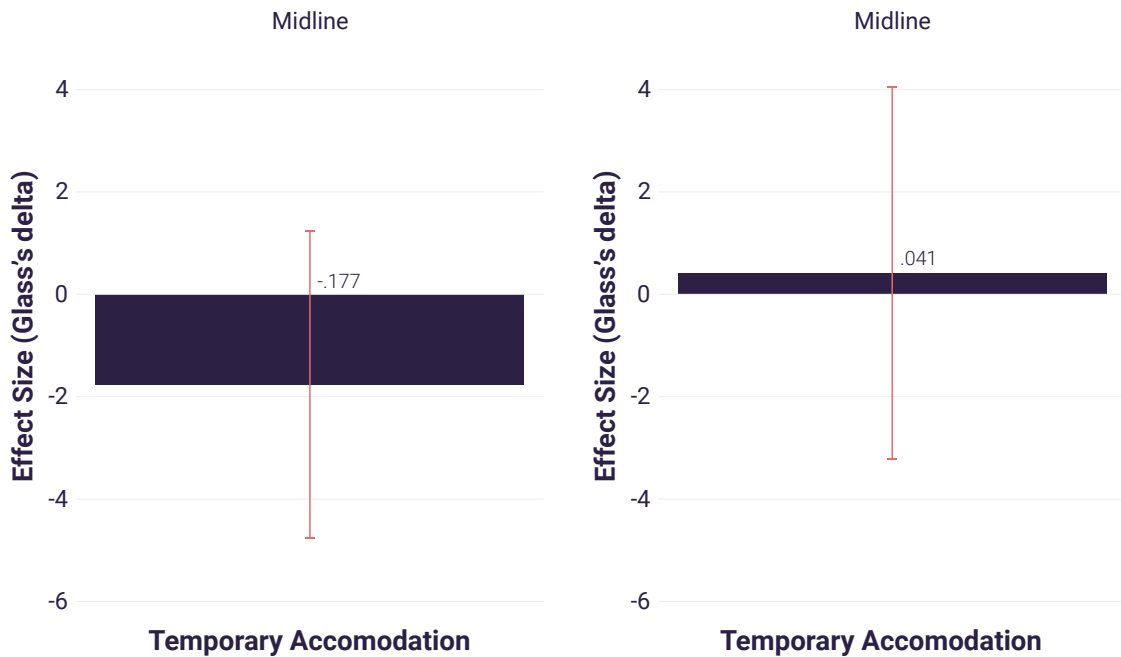
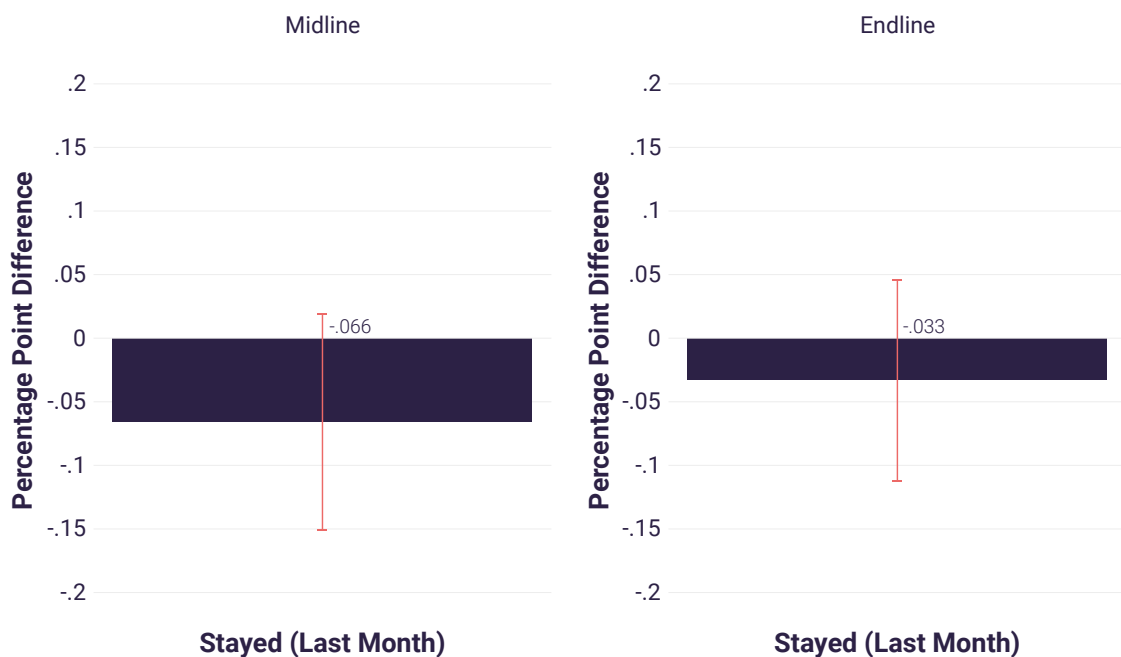


Figure 23. Effects on Homelessness

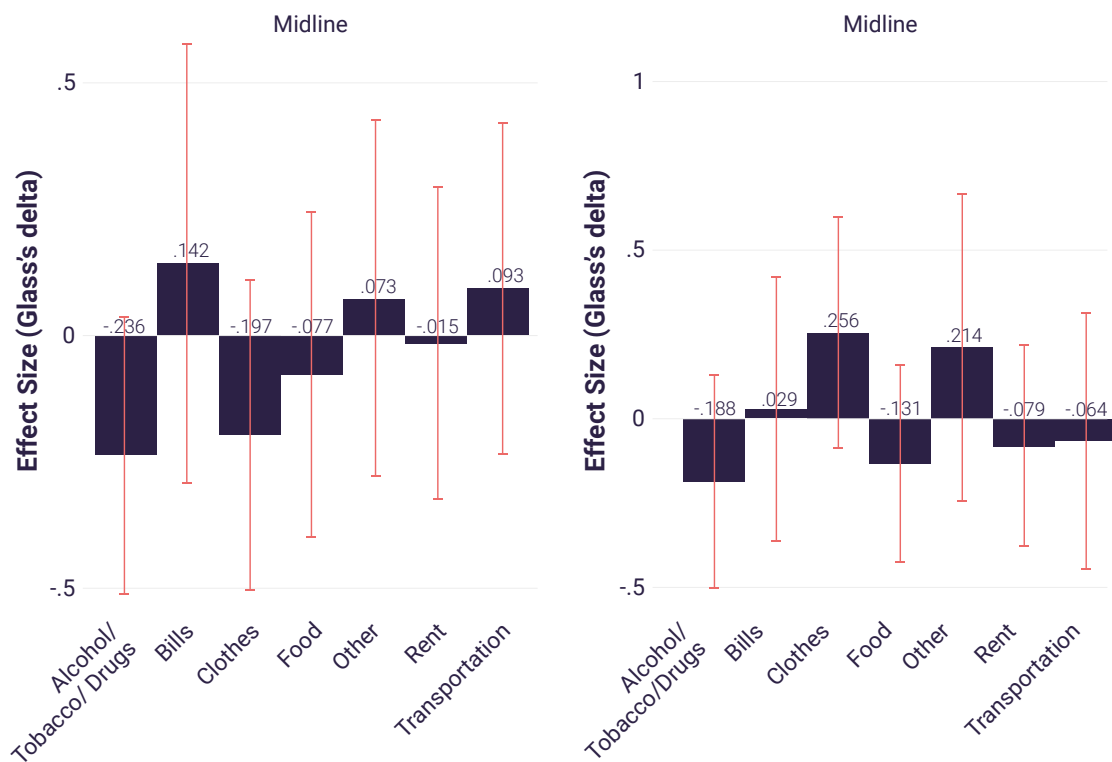


Exploratory analysis at midline suggests a reduction in the probability a young person rough slept or “sofa surfed” (variable Stayed) in the last month by 6 percentage points ($d=-6.6, p=0.13$), while the effect size on participants identifying their accommodation as temporary even if they wanted to stay was ($d=-0.18, p=0.25$). These effects, however, both became smaller and insignificant at endline, suggesting the effect of the one-off cash transfer was not sustained for 12 months.

Income and Spending

We now consider exploratory analysis of participants’ self reported spending patterns.

Figure 24. Spent income



Results show a moderate reduction in reported spending on drugs, alcohol and tobacco ($d=-0.24, p=0.087$) with a statistically significant effect at the 10% level at midline, this effect is largely sustained at endline in effect size magnitude however statistical significance is reduced ($d=-0.19, p=0.25$). We also find a moderate reduction in spending on clothes at midline ($d=-0.21, p=0.19$) which was significant at the 20% level; this effect is reversed at endline to a moderate positive effect ($d=0.26, p=0.14$). Given the number of elements of this question, which was not intended as an outcome measure of the trial in itself, we do not have high confidence around these findings.

Missing Data

We conducted analysis of the patterns of missingness in our data at midline and endline. These analyses can be found in appendix B. Overall, we find some evidence of differential missingness in our data. To investigate the impact of this on our findings, we conduct multiple imputation by chained equations (MICE), and we find that this does not meaningfully alter the pattern of our findings.

Harms

Most participants were in regular contact with children's services, and we maintained a safeguarding log through our interactions with participants. There was no evidence of harm to participants across either arm of the trial

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Discussion

This paper reports the results of a randomised trial of unconditional cash transfers of £2,000 given to young care leavers in local authorities around England. The trial randomised 302 participants to treatment and control in a 1:2 ratio.

Results are reported based on data provided at 6 months (midline) and 12 months (endline) after randomisation. Attrition at both midline and endline was approximately one third of the sample, with no significant differential responsiveness between treatment and control arms and good statistical balance on observable characteristics at both time points. Response rates at endline were higher than at midline, reflecting considerable efforts to maximise sample at endline. Balance on covariates was preserved throughout the study, giving us reasonable confidence in our findings.

We saw positive evidence of an effect on housing stability at six months, of modest size and statistically significant at the 10% level. However, this has become smaller, associated with less statistical confidence by twelve months post randomisation. Effects on our other primary outcome measure, financial wellbeing, become more positive and of higher statistical confidence over time.

Among our secondary outcome measures, we see statistically significant reductions in involvement in the criminal justice system at both six and twelve months, including a significant reduction in arrest, caution, and conviction at twelve months. These need to be contextualised by the low levels of these outcomes in both treatment and control group overall, suggesting the value of a larger and/or longer term study. We do not see meaningful effects on other anti-social behaviour.

Looking at healthcare outcomes, we see increases at midline in the number of visits to GPs/walk-in clinics, and significant decreases at endline in the number of hospital stays, of roughly 9%, or 0.16 stays per participant receiving the treatment. Neither GP visits at endline, nor hospital stays at midlines saw substantial changes.

There are positive and consistent effects of the intervention on participants' wellbeing and feelings of connectedness. Overall, we do not see meaningful changes in participants' consumption habits, in terms of the proportion of their income spent on different items. At midline there was a suggestion of less spending on drugs and alcohol, but we do not have much confidence in these findings.

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Limitations

There are a number of limitations to the study, which it is important to consider. First, although the study met its recruitment target, it remains a relatively small study, and a larger scale trial would allow us to have more confidence in our findings. Second, attrition is higher in the control group than the treatment group, and although the two groups remain balanced on baseline characteristics, we cannot rule out the possibility of some bias being introduced. These limitations will be substantially mitigated in future years when administrative data on employment and education become available for the full sample.

Third, recruitment was staggered across sites, introducing a degree of temporal variation to the implementation which may also influence the measured outcomes, although this is balanced across treatment and control groups.

A further limitation is the inability to blind participants and local authorities to treatment status due to the nature of the treatment, thus we cannot rule out that the treatment status itself can influence outcomes through the shaping of participants' expectations and beliefs.

In terms of fidelity, the intervention is a simple one and was delivered with fidelity for the vast majority of participants. However, one participant received payments in a staggered way due to safeguarding risks about that participant; one participant refused payment; and one participant's payment was received by the local authority, who purchased goods on their behalf.

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Conclusions

Our results are robust to different specifications of the analysis, and to the use of imputation of missing data. Given the scale of this trial, we have reasonable confidence in the existence of moderate effect on several outcomes. However, there are clear recommendations for the future, specifically;

- We have encouraging evidence on our primary outcomes - housing stability and financial wellbeing, suggesting that future research focusing on these outcomes with a large sample size is called for.
- A longer term trial could help to identify more meaningful effects on some outcomes, particularly healthcare use and criminal justice outcomes.
- These outcomes are in turn associated with the risk of experiencing homelessness, and so longer term effects on housing might emerge, meaning that housing should continue to be considered.
- A larger scale trial could help more precisely estimate effects.
- Although £2,000 as a one off transfer has had meaningful effects on some outcomes, larger transfers, or repeated transfers could have larger effects, particularly in the medium term.

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Appendix A: Regression Estimates

Table A1. Scale Outcomes – Midline

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Treatment	0.069 (0.258)	-0.080 (0.072)	-0.028 (0.105)	-0.045 (0.142)
Time=2	0.182 (0.192)	-0.076 (0.057)	-0.154* (0.086)	-0.077 (0.099)
Treatment * Time=2	-0.173 (0.291)	0.153* (0.089)	0.186 (0.132)	0.203 (0.163)
Male	0.407* (0.246)	-0.018 (0.063)	0.294*** (0.107)	0.215 (0.134)
Other	0.070 (0.839)	0.077 (0.180)	0.167 (0.231)	0.188 (0.360)

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Asian/Asian British	-0.313 (0.690)	-0.054 (0.198)	-0.237 (0.275)	0.686* (0.387)
Black British/African/Caribbean	-0.505 (0.612)	0.077 (0.180)	0.154 (0.264)	0.633* (0.376)
Mixed/Multiple ethnic groups	-0.176 (0.675)	0.296 (0.181)	0.223 (0.277)	0.860** (0.396)
White	-0.384 (0.580)	0.200 (0.161)	0.019 (0.242)	0.868** (0.343)
Assignmentwave=2	0.560 (0.780)	-0.059 (0.204)	0.074 (0.307)	-0.237 (0.375)
Assignmentwave=3	-0.034 (0.738)	-0.350* (0.211)	-0.299 (0.306)	0.027 (0.408)

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Assignmentwave=4	-0.072 (0.721)	-0.386* (0.205)	-0.169 (0.290)	-0.247 (0.383)
Assignmentwave=5	0.079 (0.653)	-0.285 (0.190)	-0.145 (0.271)	-0.367 (0.357)
Assignmentwave=6	-0.093 (0.634)	-0.294* (0.171)	-0.255 (0.248)	-0.657** (0.324)
Bolton	1.932*** (0.566)	0.363*** (0.132)	0.685*** (0.257)	0.544 (0.334)
Camden	0.192 (0.482)	-0.217 (0.139)	0.414** (0.166)	0.457** (0.230)
Rochdale	0.425 (0.617)	0.041 (0.147)	0.268 (0.337)	0.495 (0.344)

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Salford	0.566 (0.546)	0.394** (0.154)	0.640*** (0.202)	1.021*** (0.262)
Stockport	-0.103 (0.375)	0.178 (0.111)	0.185 (0.154)	0.413* (0.226)
Walsall	0.785* (0.444)	-0.023 (0.130)	0.232 (0.187)	0.621** (0.244)
Warrington	-0.133 (0.483)	-0.084 (0.143)	0.087 (0.219)	0.401* (0.231)
Warwickshire	0.551 (0.491)	0.102 (0.158)	0.178 (0.232)	0.236 (0.314)
Birth Year	-0.006 (0.051)	0.012 (0.015)	0.018 (0.022)	0.043 (0.030)

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Constant	15.468	-19.516	-34.044	-84.390
	(102.930)	(29.951)	(44.406)	(59.385)
N	479	477	479	479

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Table A2. Contact with Justice System – Midline

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Treatment	-0.026**	0.019	0.030	0.006	0.012	-0.004	0.012	0.049*	0.045**	-0.012
	(0.012)	(0.021)	(0.021)	(0.014)	(0.012)	(0.036)	(0.022)	(0.029)	(0.021)	(0.016)
Time=2	-0.018	-0.015	0.024	0.007	0.002	-0.015	-0.007	0.014	0.004	-0.016
	(0.017)	(0.010)	(0.016)	(0.011)	(0.002)	(0.033)	(0.015)	(0.018)	(0.003)	(0.011)
Treatment * Time=2	0.033	-0.005	-0.039	-0.003	0.002	-0.001	-0.007	-0.058*	-0.015	0.020
	(0.022)	(0.025)	(0.028)	(0.021)	(0.018)	(0.053)	(0.028)	(0.034)	(0.030)	(0.022)
Male	-0.014	-0.001	-0.003	-0.007	0.001	-0.012	0.027	0.044*	0.015	0.017
	(0.016)	(0.015)	(0.016)	(0.012)	(0.011)	(0.029)	(0.022)	(0.024)	(0.014)	(0.018)
Other	-0.015	-0.032**	-0.028	-0.020	-0.016	-0.030	-0.024	-0.030	-0.025	-0.014
	(0.018)	(0.015)	(0.018)	(0.013)	(0.012)	(0.069)	(0.015)	(0.021)	(0.016)	(0.018)
Asian/Asian British	0.017	0.092*	0.048	0.014	0.008	-0.007	-0.004	-0.007	-0.013	-0.134
	(0.019)	(0.049)	(0.036)	(0.010)	(0.007)	(0.038)	(0.067)	(0.065)	(0.069)	(0.118)

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Black British/African/ Caribbean	0.041	0.036**	0.027	0.031	0.013	0.039	-0.043	-0.007	-0.030	-0.122
	(0.027)	(0.018)	(0.021)	(0.021)	(0.011)	(0.042)	(0.060)	(0.067)	(0.061)	(0.111)
Mixed/Multiple ethnic groups	0.015	0.029*	0.044	0.010	0.009	-0.003	-0.028	-0.067	-0.034	-0.105
	(0.018)	(0.016)	(0.030)	(0.010)	(0.008)	(0.053)	(0.068)	(0.061)	(0.061)	(0.119)
White	0.026	0.036**	0.032	0.014	0.006	0.074**	-0.033	-0.025	-0.034	-0.129
	(0.018)	(0.018)	(0.020)	(0.011)	(0.006)	(0.031)	(0.060)	(0.060)	(0.060)	(0.118)
Assignmentwave=2	-0.079	-0.046	0.000	-0.009	-0.011	-0.104	0.071	0.025	0.059	-0.089
	(0.049)	(0.038)	(0.014)	(0.011)	(0.011)	(0.082)	(0.050)	(0.058)	(0.046)	(0.063)
Assignmentwave=3	-0.053	-0.020	0.011	0.008	0.011	-0.006	0.059	0.025	0.042	-0.042
	(0.050)	(0.039)	(0.013)	(0.013)	(0.009)	(0.096)	(0.039)	(0.052)	(0.027)	(0.058)
Assignmentwave=4	-0.056	-0.010	0.016	0.037	0.020	-0.051	0.053	-0.007	0.055**	-0.069
	(0.052)	(0.049)	(0.020)	(0.029)	(0.018)	(0.093)	(0.044)	(0.044)	(0.027)	(0.073)
Assignmentwave=5	-0.059	-0.018	0.016	0.010	0.007	-0.056	0.083**	0.017	0.037	-0.084
	(0.050)	(0.039)	(0.016)	(0.011)	(0.011)	(0.085)	(0.041)	(0.043)	(0.023)	(0.061)

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Assignmentwave=6	-0.074	-0.040	-0.002	-0.004	0.001	-0.092	0.018	-0.036	0.018	-0.051
	(0.048)	(0.037)	(0.005)	(0.006)	(0.003)	(0.071)	(0.018)	(0.038)	(0.014)	(0.056)
Bolton	-0.019	-0.011	0.028	-0.008	-0.002	-0.054	0.012	-0.044	-0.006	-0.032
	(0.017)	(0.014)	(0.033)	(0.012)	(0.009)	(0.074)	(0.037)	(0.045)	(0.015)	(0.021)
Camden	0.022	0.023	0.021	0.018	0.032	-0.030	0.000	-0.039	0.005	0.022
	(0.035)	(0.034)	(0.035)	(0.034)	(0.033)	(0.047)	(0.040)	(0.043)	(0.038)	(0.026)
Rochdale	-0.003	-0.004	0.030	-0.001	0.015	-0.025	-0.031	-0.059*	-0.012	0.019
	(0.017)	(0.017)	(0.033)	(0.016)	(0.012)	(0.060)	(0.029)	(0.033)	(0.022)	(0.016)
Salford	-0.014	0.027	-0.011	-0.033	-0.009	-0.066	0.020	-0.053**	-0.017	-0.012
	(0.023)	(0.065)	(0.017)	(0.028)	(0.015)	(0.063)	(0.063)	(0.023)	(0.018)	(0.024)
Stockport	0.004	-0.018	-0.013	-0.019	-0.003	-0.015	-0.013	-0.023	0.016	0.033
	(0.024)	(0.020)	(0.011)	(0.017)	(0.009)	(0.067)	(0.017)	(0.028)	(0.024)	(0.029)
Walsall	-0.000	0.006	0.004	0.002	0.012	-0.019	0.041	-0.008	0.038*	-0.010
	(0.016)	(0.015)	(0.014)	(0.013)	(0.011)	(0.060)	(0.041)	(0.026)	(0.019)	(0.016)

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Warrington	-0.020 (0.016)	0.032 (0.049)	0.038 (0.047)	0.028 (0.049)	0.047 (0.047)	0.026 (0.081)	0.019 (0.051)	0.003 (0.050)	0.050 (0.046)	0.106 (0.069)
Warwickshire	0.014 (0.033)	0.019 (0.032)	0.060 (0.047)	-0.013 (0.015)	-0.003 (0.010)	-0.098* (0.055)	-0.033 (0.026)	-0.055 (0.042)	0.011 (0.033)	-0.055** (0.027)
Birth Year	0.004 (0.003)	0.003 (0.003)	-0.001 (0.003)	0.004 (0.002)	0.002 (0.002)	0.006 (0.007)	0.004 (0.003)	0.008** (0.004)	-0.000 (0.003)	-0.000 (0.003)
Constant	-7.781 (6.086)	-6.687 (5.560)	1.974 (6.419)	-7.768 (4.719)	-3.338 (3.464)	-12.421 (13.421)	-8.741 (6.629)	-16.902** (8.526)	0.023 (5.275)	0.932 (5.640)
N	451	460	453	457	461	453	454	460	464	464

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Table A3. Contact with Health Services - Midline

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Treatment	0.009	0.023	0.001	-0.143	0.050	0.051	-0.071	-0.124	-0.092
	(0.097)	(0.049)	(0.043)	(0.153)	(0.113)	(0.126)	(0.113)	(0.142)	(0.141)
Time=2	0.041	-0.001	0.065	-0.095	-0.117	-0.122	-0.010	0.116	0.151
	(0.086)	(0.036)	(0.048)	(0.130)	(0.087)	(0.106)	(0.090)	(0.118)	(0.110)
Treatment * Time=2	-0.045	0.000	-0.043	0.321	0.029	-0.029	-0.113	0.113	-0.088
	(0.135)	(0.062)	(0.071)	(0.215)	(0.137)	(0.171)	(0.129)	(0.203)	(0.180)
Male	-0.176**	-0.011	-0.073**	-0.449***	-0.615***	-0.275**	-0.244**	-0.154	-0.322**
	(0.089)	(0.049)	(0.034)	(0.116)	(0.110)	(0.116)	(0.100)	(0.117)	(0.143)
Other	-0.332**	-0.055	-0.106**	-0.713**	0.162	-0.347	0.210	0.521**	0.745**
	(0.150)	(0.037)	(0.046)	(0.317)	(0.158)	(0.283)	(0.263)	(0.253)	(0.363)
Asian/Asian British	-0.149	0.072	-0.027	0.434	0.159	0.437	0.242	-0.305	0.592*
	(0.261)	(0.058)	(0.056)	(0.314)	(0.286)	(0.320)	(0.301)	(0.392)	(0.308)

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Black British/African/Caribbean	-0.102	0.137*	0.036	-0.093	0.015	0.406	0.217	-0.576	0.283
	(0.236)	(0.073)	(0.059)	(0.288)	(0.262)	(0.295)	(0.253)	(0.360)	(0.289)
Mixed/Multiple ethnic groups	-0.151	0.195*	-0.027	0.022	0.116	0.771**	0.219	-0.429	0.467
	(0.274)	(0.108)	(0.060)	(0.309)	(0.296)	(0.337)	(0.294)	(0.367)	(0.339)
White	-0.069	0.106**	0.030	-0.074	0.163	0.746***	0.355	-0.344	0.549**
	(0.215)	(0.051)	(0.052)	(0.243)	(0.226)	(0.249)	(0.222)	(0.305)	(0.237)
Assignmentwave=2	-0.161	0.048	-0.094	0.105	-0.259	-0.420	-0.307	-0.405	-0.209
	(0.222)	(0.056)	(0.085)	(0.305)	(0.268)	(0.315)	(0.308)	(0.283)	(0.390)
Assignmentwave=3	0.175	0.085	-0.030	0.265	0.005	-0.477	-0.135	-0.284	-0.084
	(0.283)	(0.075)	(0.101)	(0.326)	(0.290)	(0.317)	(0.332)	(0.353)	(0.415)
Assignmentwave=4	0.161	0.060	0.017	0.578*	-0.006	-0.376	0.048	-0.104	0.079
	(0.258)	(0.069)	(0.108)	(0.306)	(0.287)	(0.319)	(0.328)	(0.345)	(0.407)
Assignmentwave=5	0.026	0.017	-0.071	0.316	-0.046	-0.485*	-0.233	-0.184	0.113
	(0.228)	(0.040)	(0.086)	(0.256)	(0.255)	(0.270)	(0.296)	(0.288)	(0.350)

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Assignmentwave=6	0.009	-0.006	-0.044	0.185	-0.163	-0.285	-0.301	-0.028	-0.330
	(0.228)	(0.027)	(0.082)	(0.221)	(0.216)	(0.257)	(0.275)	(0.281)	(0.357)
Bolton	-0.376*	-0.067	-0.032	-0.024	-0.353	-0.093	-0.570**	-0.267	-0.430
	(0.206)	(0.077)	(0.061)	(0.287)	(0.259)	(0.301)	(0.232)	(0.264)	(0.328)
Camden	0.013	0.080	0.049	0.253	0.128	0.060	-0.254	0.159	-0.308
	(0.153)	(0.061)	(0.060)	(0.255)	(0.194)	(0.227)	(0.190)	(0.233)	(0.248)
Rochdale	0.004	-0.060	-0.036	0.259	0.193	-0.251	-0.207	0.149	-0.019
	(0.190)	(0.045)	(0.043)	(0.289)	(0.218)	(0.321)	(0.290)	(0.323)	(0.285)
Salford	0.202	0.293*	0.155	-0.030	0.261	0.137	-0.324	-0.325	-0.380
	(0.229)	(0.155)	(0.139)	(0.312)	(0.234)	(0.363)	(0.240)	(0.270)	(0.347)
Stockport	-0.074	0.134	-0.013	-0.047	-0.094	-0.018	-0.227	0.433*	0.090
	(0.193)	(0.101)	(0.066)	(0.223)	(0.170)	(0.209)	(0.190)	(0.254)	(0.240)
Walsall	-0.020	-0.000	-0.008	0.377*	0.041	-0.164	-0.160	-0.243	0.019
	(0.168)	(0.041)	(0.055)	(0.209)	(0.212)	(0.201)	(0.210)	(0.208)	(0.263)

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Warrington	0.442**	-0.008	0.287	0.080	0.238	0.186	-0.237	-0.030	-0.034
	(0.222)	(0.067)	(0.185)	(0.305)	(0.248)	(0.276)	(0.205)	(0.239)	(0.369)
Warwickshire	-0.385**	-0.025	-0.042	-0.178	-0.281	0.044	-0.237	-0.102	-0.317
	(0.183)	(0.076)	(0.058)	(0.264)	(0.271)	(0.278)	(0.253)	(0.331)	(0.294)
Birth Year	-0.020	0.021**	0.009	0.040	-0.014	-0.002	-0.013	-0.004	-0.005
	(0.025)	(0.008)	(0.009)	(0.027)	(0.022)	(0.028)	(0.024)	(0.028)	(0.032)
Constant	42.347	-40.211**	-17.099	-77.385	30.505	6.452	28.869	10.280	12.715
	(50.019)	(16.823)	(17.296)	(54.174)	(43.911)	(55.415)	(48.569)	(56.326)	(63.526)
N	428	450	454	431	443	440	454	422	430

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Table A4. Proportion Spent - Midline

	Rent	Food	Clothes	Transportation	Alcohol/ Tobacco/Drugs	Other	Bills
Treatment	-4.966*	2.848	0.990	-0.411	1.058	0.403	1.259
	(2.913)	(2.090)	(1.177)	(1.443)	(0.906)	(1.352)	(1.964)
Time=2	0.704	0.439	-0.366	-0.329	1.125	0.863	-0.901
	(2.489)	(1.888)	(0.806)	(1.188)	(0.761)	(1.250)	(1.414)
Treatment * Time=2	-0.370	-1.448	-1.608	1.076	-1.998*	0.836	1.611
	(3.827)	(3.064)	(1.273)	(1.903)	(1.175)	(2.048)	(2.506)
Male	0.214	-3.624*	1.141	-1.599	1.106	2.306	0.084
	(2.738)	(1.921)	(1.103)	(1.270)	(1.009)	(1.413)	(1.530)
Other	9.772	-7.117	-3.048	1.484	-2.136	2.036	0.575
	(8.397)	(4.378)	(2.072)	(4.368)	(1.415)	(2.326)	(3.498)
Asian/Asian British	4.673	-4.231	-1.978	3.065	1.865	-1.085	-3.915
	(6.308)	(6.460)	(3.241)	(3.151)	(2.204)	(1.898)	(4.849)

	Rent	Food	Clothes	Transportation	Alcohol/ Tobacco/Drugs	Other	Bills
Black British/African/Caribbean	13.322**	-9.111	-1.404	3.058	0.417	-0.931	-5.096
	(6.073)	(6.501)	(2.709)	(2.821)	(2.234)	(1.776)	(4.722)
Mixed/Multiple ethnic groups	8.552	-3.314	-4.031	1.510	2.603	-0.119	-5.508
	(6.434)	(6.646)	(2.688)	(2.999)	(2.627)	(2.346)	(5.540)
White	15.367***	-14.437**	-4.178*	1.666	1.534	2.163	-3.920
	(4.747)	(5.668)	(2.313)	(2.294)	(1.964)	(1.542)	(4.512)
Assignmentwave=2	1.876	3.046	3.844	2.322	0.055	-2.101	-1.881
	(7.327)	(5.181)	(3.130)	(3.496)	(2.702)	(2.956)	(2.619)
Assignmentwave=3	-10.261	2.791	3.965	-0.051	-0.181	-1.490	7.987*
	(7.160)	(5.300)	(3.004)	(3.442)	(2.584)	(3.779)	(4.740)
Assignmentwave=4	-1.746	0.657	4.953	0.733	-0.669	-3.309	2.042
	(7.054)	(4.869)	(3.145)	(3.304)	(2.495)	(3.208)	(4.033)
Assignmentwave=5	-4.235	2.332	2.697	2.479	-1.379	-3.944	5.495
	(6.191)	(4.318)	(2.492)	(2.952)	(2.332)	(2.958)	(3.771)

	Rent	Food	Clothes	Transportation	Alcohol/ Tobacco/Drugs	Other	Bills
Assignmentwave=6	1.968	3.423	0.894	0.331	-3.370*	-2.982	4.359
	(6.344)	(3.980)	(2.123)	(2.753)	(1.776)	(2.859)	(3.234)
Bolton	-2.840	2.591	3.021	4.250	2.201	0.864	-8.616**
	(6.272)	(4.286)	(2.234)	(3.853)	(2.179)	(3.773)	(3.644)
Camden	11.931**	-5.864	-1.926	-2.727	-0.326	-0.963	-1.791
	(5.725)	(4.176)	(2.482)	(2.247)	(1.299)	(2.089)	(2.560)
Rochdale	-0.783	-7.692*	2.671	-0.029	-0.230	-1.321	1.451
	(6.743)	(4.413)	(2.575)	(3.707)	(1.675)	(2.370)	(3.616)
Salford	5.897	1.214	-2.655	-0.685	-0.616	0.230	0.068
	(5.430)	(3.209)	(2.021)	(2.325)	(1.571)	(3.244)	(4.213)
Stockport	7.057	1.531	-1.737	-3.511*	3.700*	-2.325	-3.184
	(5.218)	(3.557)	(1.878)	(1.954)	(2.057)	(2.062)	(2.924)
Walsall	-3.231	-2.476	4.234**	2.055	1.751	-2.803	0.020
	(5.213)	(3.729)	(2.082)	(2.596)	(1.805)	(2.205)	(3.283)

	Rent	Food	Clothes	Transportation	Alcohol/ Tobacco/Drugs	Other	Bills
Warrington	2.180	1.393	2.210	0.411	3.198*	-5.354***	-2.105
	(5.591)	(3.955)	(1.796)	(4.242)	(1.785)	(1.517)	(3.927)
Warwickshire	7.314	1.503	0.573	0.562	-0.128	-2.176	-6.360*
	(5.482)	(5.026)	(2.183)	(2.847)	(1.905)	(2.786)	(3.558)
Birth Year	-1.459**	1.445***	0.313	0.099	0.058	-0.040	-0.367
	(0.618)	(0.438)	(0.234)	(0.327)	(0.189)	(0.261)	(0.383)
Constant	2942.812**	-2852.468***	-620.792	-188.214	-115.228	85.420	741.243
	(1237.664)	(877.756)	(467.892)	(654.992)	(378.697)	(523.404)	(767.564)
N	479	479	479	479	479	479	480

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Table A5. Scale Outcomes – Endline

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Treatment	0.068	-0.093	-0.033	-0.058
	(0.259)	(0.073)	(0.106)	(0.142)
Time=3	-0.270	-0.111*	-0.206***	-0.133
	(0.173)	(0.059)	(0.071)	(0.101)
Treatment * Time=3	0.346	0.060	0.197*	0.172
	(0.279)	(0.095)	(0.117)	(0.150)
Male	0.271	-0.017	0.226**	0.171
	(0.244)	(0.061)	(0.115)	(0.142)
Other	-0.114	0.051	0.209	0.300
	(0.842)	(0.184)	(0.253)	(0.363)
Asian/Asian British	-0.007	0.009	-0.028	0.468
	(0.731)	(0.173)	(0.264)	(0.367)

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Black British/African/Caribbean	-0.283	0.086	0.228	0.450
	(0.619)	(0.144)	(0.267)	(0.328)
Mixed/Multiple ethnic groups	0.105	0.207	0.308	0.614*
	(0.706)	(0.162)	(0.287)	(0.351)
White	-0.416	0.150	0.136	0.619**
	(0.560)	(0.126)	(0.233)	(0.284)
Assignmentwave=2	0.542	0.221	0.063	-0.026
	(0.639)	(0.154)	(0.272)	(0.321)
Assignmentwave=3	0.435	0.070	-0.184	0.060
	(0.637)	(0.185)	(0.287)	(0.374)
Assignmentwave=4	0.266	-0.085	-0.191	-0.250
	(0.607)	(0.167)	(0.274)	(0.359)
Assignmentwave=5	0.510	-0.013	-0.017	-0.180
	(0.531)	(0.156)	(0.245)	(0.309)

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Assignmentwave=6	0.076	-0.055	-0.170	-0.504*
	(0.505)	(0.145)	(0.223)	(0.284)
Bolton	1.773***	0.241*	0.621**	0.657*
	(0.532)	(0.134)	(0.276)	(0.344)
Camden	0.424	-0.095	0.481***	0.473**
	(0.539)	(0.114)	(0.172)	(0.226)
Rochdale	0.770	0.119	0.325	0.439
	(0.619)	(0.134)	(0.324)	(0.348)
Salford	0.455	0.342***	0.508**	0.684**
	(0.634)	(0.125)	(0.243)	(0.292)
Stockport	0.192	0.149	0.133	0.435*
	(0.377)	(0.118)	(0.168)	(0.234)
Walsall	1.009**	0.090	0.184	0.505**
	(0.432)	(0.118)	(0.183)	(0.233)

	Financial Wellbeing	Housing Security Scale	WEMWBS	ENRICHD Social Support
Warrington	-0.245	-0.084	0.004	0.299
	(0.422)	(0.091)	(0.214)	(0.264)
Warwickshire	0.641	0.005	0.211	0.213
	(0.490)	(0.166)	(0.249)	(0.360)
Birth Year	0.022	0.001	0.025	0.045
	(0.054)	(0.014)	(0.023)	(0.030)
Constant	-41.098	2.437	-47.757	-88.139
	(107.308)	(28.089)	(45.954)	(59.513)
N	488	490	490	488

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Table A6. Contact with Justice System – Endline

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Treatment	-0.024**	0.020	0.030	0.007	0.013	0.000	0.013	0.052*	0.045**	-0.013
	(0.012)	(0.020)	(0.020)	(0.014)	(0.012)	(0.036)	(0.022)	(0.029)	(0.021)	(0.016)
Time=3	-0.018	-0.006	0.011	0.025	0.010	-0.006	0.005	0.011	0.019	-0.019*
	(0.015)	(0.013)	(0.013)	(0.018)	(0.010)	(0.032)	(0.017)	(0.017)	(0.013)	(0.011)
Treatment * Time=3	0.018	-0.028	-0.021	-0.035*	-0.020	-0.013	-0.038	-0.061**	-0.063**	0.008
	(0.015)	(0.023)	(0.025)	(0.021)	(0.015)	(0.050)	(0.026)	(0.029)	(0.026)	(0.017)
Male	0.005	0.011	0.003	0.010	0.013	-0.025	0.039*	0.071***	0.027**	0.013
	(0.013)	(0.014)	(0.012)	(0.012)	(0.009)	(0.030)	(0.023)	(0.027)	(0.013)	(0.017)
Other	-0.002	-0.023*	-0.030	-0.006	-0.004	-0.104***	-0.011	-0.016	-0.008	-0.010
	(0.016)	(0.014)	(0.020)	(0.010)	(0.009)	(0.037)	(0.015)	(0.022)	(0.011)	(0.018)
Asian/Asian British	0.020	0.105**	0.070	-0.041	0.013	0.012	0.007	0.005	-0.009	-0.115
	(0.021)	(0.051)	(0.059)	(0.050)	(0.011)	(0.062)	(0.064)	(0.061)	(0.062)	(0.105)

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Black British/ African/Caribbean	0.019	0.034*	0.012	-0.032	0.010	0.004	-0.029	-0.008	-0.035	-0.116
	(0.019)	(0.018)	(0.018)	(0.054)	(0.010)	(0.063)	(0.054)	(0.057)	(0.054)	(0.100)
Mixed/Multiple ethnic groups	0.014	0.033*	0.011	-0.053	0.011	-0.028	-0.016	-0.050	-0.035	-0.092
	(0.019)	(0.018)	(0.019)	(0.053)	(0.010)	(0.068)	(0.063)	(0.056)	(0.055)	(0.107)
White	0.032	0.047**	0.035*	-0.037	0.017	0.051	-0.000	0.007	-0.020	-0.116
	(0.021)	(0.022)	(0.021)	(0.053)	(0.013)	(0.057)	(0.056)	(0.056)	(0.055)	(0.106)
Assignmentwave=2	-0.071	-0.039	0.010	0.017	0.002	-0.142	0.094*	0.038	0.037	-0.079
	(0.053)	(0.039)	(0.012)	(0.014)	(0.004)	(0.091)	(0.051)	(0.059)	(0.032)	(0.068)
Assignmentwave=3	-0.057	-0.028	0.015	0.029	0.008	-0.048	0.061	0.025	0.020	-0.042
	(0.055)	(0.040)	(0.014)	(0.029)	(0.007)	(0.101)	(0.040)	(0.053)	(0.020)	(0.067)
Assignmentwave=4	-0.082	-0.014	0.033	0.034	0.017	-0.065	0.059	0.004	0.032	-0.067
	(0.053)	(0.050)	(0.028)	(0.028)	(0.016)	(0.106)	(0.045)	(0.052)	(0.021)	(0.080)
Assignmentwave=5	-0.069	-0.028	0.006	0.033	-0.001	-0.096	0.085**	0.002	0.015	-0.085
	(0.053)	(0.039)	(0.015)	(0.027)	(0.004)	(0.091)	(0.040)	(0.043)	(0.014)	(0.071)

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Assignmentwave=6	-0.076	-0.046	-0.005	0.008	0.000	-0.164**	0.042	-0.035	0.007	-0.073
	(0.053)	(0.039)	(0.009)	(0.014)	(0.003)	(0.080)	(0.033)	(0.040)	(0.011)	(0.063)
Bolton	-0.028*	-0.018	0.005	-0.017	-0.012	-0.100	-0.034	-0.102***	-0.020	-0.033
	(0.017)	(0.013)	(0.034)	(0.011)	(0.008)	(0.069)	(0.026)	(0.039)	(0.016)	(0.022)
Camden	-0.004	-0.010	-0.023	-0.011	-0.004	-0.065	-0.042*	-0.079***	-0.026*	-0.005
	(0.010)	(0.009)	(0.014)	(0.012)	(0.005)	(0.040)	(0.025)	(0.030)	(0.015)	(0.011)
Rochdale	-0.009	-0.014	0.007	-0.008	0.001	-0.061	-0.048	-0.069**	-0.021	0.007
	(0.010)	(0.013)	(0.029)	(0.016)	(0.003)	(0.055)	(0.030)	(0.034)	(0.018)	(0.013)
Salford	-0.002	0.025	-0.043	-0.026	-0.016	-0.105	0.017	-0.073**	-0.029	-0.017
	(0.014)	(0.070)	(0.027)	(0.017)	(0.014)	(0.079)	(0.069)	(0.035)	(0.019)	(0.023)
Stockport	0.009	-0.023	-0.039**	-0.016	-0.009	-0.066	-0.024	-0.038	0.005	0.028
	(0.023)	(0.020)	(0.020)	(0.010)	(0.007)	(0.066)	(0.019)	(0.031)	(0.023)	(0.029)
Walsall	-0.009	-0.004	-0.014	0.010	-0.000	-0.026	0.035	-0.023	0.002	-0.010
	(0.011)	(0.010)	(0.010)	(0.023)	(0.002)	(0.058)	(0.044)	(0.028)	(0.014)	(0.016)

	ASB Letter	NSOP	Eviction	Parenting Order	Penalty	Police Called	Caution	Arrested	Convicted	Prison
Warrington	-0.011	0.023	0.014	0.021	0.037	-0.066	0.004	0.029	0.030	0.087
	(0.013)	(0.040)	(0.038)	(0.040)	(0.036)	(0.067)	(0.046)	(0.075)	(0.037)	(0.057)
Warwickshire	0.039	0.044	0.026	0.003	0.019	-0.105*	-0.005	-0.043	0.025	-0.053**
	(0.039)	(0.040)	(0.041)	(0.028)	(0.027)	(0.060)	(0.036)	(0.049)	(0.037)	(0.024)
Birth Year	0.001	0.002	-0.001	0.001	0.000	0.012*	0.001	0.004	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.007)	(0.003)	(0.003)	(0.002)	(0.003)
Constant	-0.998	-4.373	2.341	-2.860	-0.345	-23.846	-1.892	-8.615	1.272	2.670
	(3.616)	(4.776)	(4.240)	(3.768)	(1.085)	(14.494)	(5.819)	(6.939)	(3.777)	(5.526)
N	461	462	460	462	464	461	462	467	472	473

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Table A7. Contact with Health Services - Endline

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Treatment	0.026	0.023	0.009	-0.125	0.054	0.055	-0.039	-0.104	-0.094
	(0.097)	(0.049)	(0.042)	(0.152)	(0.113)	(0.128)	(0.111)	(0.145)	(0.140)
Time=3	0.106	-0.033	0.045	0.015	-0.150	-0.097	0.024	0.052	0.120
	(0.094)	(0.029)	(0.041)	(0.115)	(0.103)	(0.109)	(0.094)	(0.100)	(0.119)
Treatment * Time=3	-0.153	0.020	-0.025	0.118	-0.031	0.025	0.018	0.191	0.121
	(0.130)	(0.052)	(0.066)	(0.193)	(0.153)	(0.159)	(0.135)	(0.178)	(0.194)
Male	-0.188**	-0.021	-0.014	-0.378***	-0.626***	-0.268**	-0.261***	-0.277**	-0.402***
	(0.087)	(0.042)	(0.037)	(0.123)	(0.102)	(0.113)	(0.098)	(0.127)	(0.133)
Other	-0.378**	-0.066*	-0.078*	-0.709**	0.085	-0.419*	-0.111	0.160	0.667**
	(0.156)	(0.035)	(0.045)	(0.342)	(0.159)	(0.226)	(0.218)	(0.281)	(0.281)
Asian/Asian British	-0.225	0.088	0.075	0.212	0.190	0.307	0.192	-0.522	0.426
	(0.221)	(0.056)	(0.051)	(0.368)	(0.275)	(0.315)	(0.306)	(0.385)	(0.299)

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Black British/African/Caribbean	-0.199	0.157**	0.122**	-0.085	0.101	0.365	0.178	-0.635*	0.072
	(0.204)	(0.071)	(0.058)	(0.335)	(0.231)	(0.259)	(0.244)	(0.330)	(0.283)
Mixed/Multiple ethnic groups	-0.193	0.228**	0.084	0.041	0.228	0.681**	0.245	-0.488	0.180
	(0.230)	(0.107)	(0.060)	(0.337)	(0.238)	(0.285)	(0.292)	(0.347)	(0.343)
White	-0.111	0.113**	0.122***	0.003	0.198	0.671***	0.382*	-0.382	0.341
	(0.171)	(0.049)	(0.045)	(0.294)	(0.206)	(0.201)	(0.207)	(0.277)	(0.238)
Assignmentwave=2	-0.290	0.071	-0.060	0.049	-0.238	-0.037	-0.558***	-0.125	-0.450
	(0.215)	(0.057)	(0.093)	(0.254)	(0.231)	(0.262)	(0.194)	(0.287)	(0.295)
Assignmentwave=3	-0.064	0.001	-0.117	-0.017	0.048	-0.160	-0.232	0.119	-0.109
	(0.270)	(0.058)	(0.097)	(0.264)	(0.207)	(0.242)	(0.195)	(0.261)	(0.240)
Assignmentwave=4	-0.100	0.098	-0.064	0.181	0.047	-0.331	-0.199	0.198	0.109
	(0.256)	(0.069)	(0.111)	(0.277)	(0.216)	(0.319)	(0.298)	(0.344)	(0.278)

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Assignmentwave=5	-0.187	0.005	-0.131	-0.200	0.266	-0.094	-0.134	0.148	0.020
	(0.216)	(0.037)	(0.088)	(0.325)	(0.205)	(0.333)	(0.285)	(0.280)	(0.415)
Assignmentwave=6	-0.047	-0.003	-0.106	-0.110	-0.019	0.065	-0.113	0.541**	0.055
	(0.217)	(0.027)	(0.084)	(0.216)	(0.181)	(0.222)	(0.181)	(0.234)	(0.241)
Bolton	-0.477**	0.001	-0.045	-0.224	-0.190	-0.362*	-0.335	-0.257	-0.209
	(0.209)	(0.056)	(0.046)	(0.240)	(0.183)	(0.205)	(0.221)	(0.216)	(0.246)
Camden	-0.175	-0.006	0.012	-0.104	0.116	0.172	-0.017	0.132	-0.087
	(0.182)	(0.046)	(0.059)	(0.266)	(0.197)	(0.233)	(0.221)	(0.273)	(0.292)
Rochdale	0.056	-0.087**	-0.112**	-0.326	-0.295	-0.022	-0.167	0.037	-0.371
	(0.264)	(0.043)	(0.051)	(0.277)	(0.220)	(0.247)	(0.236)	(0.326)	(0.299)
Salford	0.078	0.197	0.132	-0.487	-0.392	-0.361	-0.209	-0.389	-0.573*
	(0.248)	(0.174)	(0.165)	(0.337)	(0.246)	(0.308)	(0.323)	(0.315)	(0.327)
Stockport	-0.104	0.099	-0.089**	-0.646*	-0.372	-0.616**	-0.346	-0.384	-0.328
	(0.194)	(0.074)	(0.040)	(0.364)	(0.280)	(0.303)	(0.312)	(0.385)	(0.374)

	Hospital Stay	Treat Drug Misuse	Treat Alcohol Misuse	GP/Nurse Walk In	Used GP	Used A&E	Used Ambulance	Hospital Appointment	Mental Health
Walsall	-0.345**	-0.013	-0.075	-0.235	-0.355	-0.456	-0.187	-0.236	-0.266
	(0.173)	(0.038)	(0.054)	(0.346)	(0.277)	(0.311)	(0.318)	(0.373)	(0.367)
Warrington	0.338	0.020	0.201	-0.428	-0.272	-0.528**	-0.302	-0.205	-0.203
	(0.222)	(0.071)	(0.151)	(0.315)	(0.251)	(0.254)	(0.278)	(0.324)	(0.302)
Warwickshire	-0.424**	0.019	0.010	-0.296	-0.335	-0.375	-0.216	-0.139	-0.436
	(0.183)	(0.061)	(0.065)	(0.292)	(0.224)	(0.239)	(0.256)	(0.317)	(0.327)
Birth Year	-0.003	0.017**	0.018**	0.027	-0.022	0.004	-0.007	-0.020	0.012
	(0.023)	(0.009)	(0.009)	(0.029)	(0.020)	(0.026)	(0.023)	(0.028)	(0.030)
Constant	7.677	-34.022**	-35.785**	-50.404	47.289	-6.255	16.004	43.482	-20.894
	(46.351)	(17.076)	(17.477)	(57.718)	(40.523)	(52.164)	(46.002)	(55.800)	(60.020)
N	432	454	460	437	444	447	460	429	437

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Table A8. Proportion Spent - Endline

	Rent	Food	Clothes	Transporation	Alcohol/Tobacco/ Drugs	Other	Bills
Treatment	-4.419	2.747	1.115	-0.470	0.936	0.472	0.859
	(2.925)	(2.082)	(1.191)	(1.441)	(0.911)	(1.372)	(1.960)
Time=3	2.067	0.877	-1.313	0.126	-0.000	0.847	-0.810
	(2.397)	(1.904)	(0.868)	(1.189)	(0.603)	(1.178)	(1.405)
Treatment * Time=3	-1.931	-2.479	2.322	-0.688	-1.164	2.272	0.345
	(3.696)	(2.787)	(1.577)	(2.078)	(1.004)	(2.456)	(2.353)
Male	3.228	-3.450*	0.888	-1.867	0.409	0.808	-0.119
	(2.768)	(1.929)	(1.116)	(1.294)	(0.853)	(1.270)	(1.499)
Other	5.900	-4.923	-5.418**	2.541	-1.773	0.022	5.190
	(9.394)	(5.009)	(2.097)	(5.291)	(1.579)	(1.656)	(4.224)
Asian/Asian British	6.118	-5.553	-0.171	2.520	2.641	-1.628	-5.726
	(6.545)	(5.869)	(3.963)	(3.661)	(2.326)	(1.912)	(4.506)

	Rent	Food	Clothes	Transportation	Alcohol/Tobacco/Drugs	Other	Bills
Black British/African/Caribbean	12.641**	-8.572	-2.277	2.722	0.924	-0.328	-5.124
	(5.981)	(5.992)	(2.795)	(3.583)	(2.205)	(1.985)	(4.615)
Mixed/Multiple ethnic groups	8.321	-3.243	-3.719	3.246	2.381	-1.644	-6.134
	(6.543)	(6.147)	(2.771)	(3.726)	(2.713)	(1.922)	(5.076)
White	15.809***	-14.766***	-4.468*	1.375	2.082	3.134**	-4.783
	(4.959)	(4.978)	(2.453)	(3.168)	(1.939)	(1.540)	(4.288)
Assignmentwave=2	1.258	2.758	2.375	-0.267	1.703	1.722	-2.893
	(8.469)	(5.039)	(3.882)	(3.556)	(2.768)	(2.290)	(2.924)
Assignmentwave=3	-12.298	2.650	3.152	-1.850	1.506	0.930	8.704*
	(8.260)	(4.971)	(3.270)	(3.245)	(2.453)	(3.122)	(4.702)
Assignmentwave=4	-5.769	0.644	3.918	-1.237	2.194	-1.472	3.698
	(7.986)	(4.537)	(3.416)	(3.157)	(2.548)	(2.651)	(4.321)
Assignmentwave=5	-5.600	4.242	1.310	-0.056	0.547	-1.180	4.330
	(7.416)	(3.979)	(2.820)	(2.797)	(2.215)	(2.316)	(3.849)

	Rent	Food	Clothes	Transportation	Alcohol/Tobacco/Drugs	Other	Bills
Assignmentwave=6	2.964	1.346	-2.099	-2.451	-1.967	1.235	5.614
	(7.398)	(3.631)	(2.790)	(2.739)	(1.658)	(2.297)	(3.640)
Bolton	0.582	0.763	2.441	4.990	1.585	-0.613	-8.838**
	(7.282)	(4.598)	(2.606)	(3.904)	(1.976)	(3.372)	(3.511)
Camden	10.805**	-3.971	-2.535	-1.458	-0.608	-2.237	-1.286
	(5.250)	(3.859)	(2.267)	(2.023)	(1.436)	(1.971)	(2.804)
Rochdale	-1.210	-8.173*	2.925	0.143	-0.518	-3.809*	5.201*
	(6.101)	(4.220)	(2.390)	(3.203)	(1.642)	(2.170)	(3.029)
Salford	10.453**	3.528	-2.958	1.082	-2.614*	-1.286	-3.371
	(4.489)	(3.800)	(2.396)	(2.649)	(1.492)	(3.034)	(4.389)
Stockport	9.980**	-0.448	-2.176	-1.001	0.765	-3.277	-2.206
	(4.386)	(3.061)	(1.861)	(1.888)	(1.803)	(2.128)	(3.208)
Walsall	-3.008	-2.839	3.804*	0.766	2.332	-4.648**	2.501
	(5.178)	(3.521)	(2.291)	(2.241)	(1.900)	(2.192)	(3.098)

	Rent	Food	Clothes	Transportation	Alcohol/Tobacco/Drugs	Other	Bills
Warrington	2.285	4.085	1.586	-0.292	1.770	-5.563***	-2.008
	(5.613)	(3.590)	(1.666)	(3.843)	(1.904)	(1.809)	(2.845)
Warwickshire	7.405	2.010	-1.347	1.331	-1.634	-0.424	-6.246*
	(5.706)	(5.168)	(1.986)	(2.793)	(1.488)	(2.742)	(3.523)
Birth Year	-1.598***	1.501***	0.457*	0.141	0.103	0.021	-0.527
	(0.563)	(0.422)	(0.233)	(0.303)	(0.183)	(0.257)	(0.420)
Constant	3221.298***	-2963.265***	-907.032*	-270.137	-205.557	-37.362	1063.507
	(1126.582)	(844.310)	(468.110)	(605.531)	(366.566)	(514.582)	(841.179)
N	488	488	488	488	488	488	492

Standard errors in parentheses. Significance indicated as: * p<0.10, ** p<0.05, *** p<0.01

Appendix B: Missing Data

Missingness Patterns

Variables that only exhibited missingness due to attrition at midline and endline have been excluded. Missingness here refers to lack of data for analysis, which includes lack of a response and/or opting out of a response (e.g., selecting “Don’t know”, “Don’t want to say” etc.)

The following patterns of missingness are considered:

- Pattern “100” (Non-missing at baseline; missing at midline, missing at endline)
- Pattern “010” (Missing at baseline; non-missing at midline, missing at endline)
- Pattern “001” (Missing at baseline; missing at midline, non-missing at endline)
- Pattern “110” (Non-missing at baseline; non-missing at midline, missing at endline)
- Pattern “101” (Non-missing at baseline; missing at midline, non-missing at endline)
- Pattern “011” (Missing at baseline; non-missing at midline, non-missing at endline)

Figure B1. Missingness Patterns (Non-response and Attrition)

We can infer from the resulting graph that those completely lost to attrition, i.e. those that did not respond to both midline and endline (Pattern “100”) represented approximately 24.5% of the sample, with around 12.5% not responding at midline but responding at endline (“101”) and 8.3% not responding at endline only (“110”).

To evaluate the underlying missingness mechanisms for each of the primary and secondary outcomes, we utilise logistic regression models. For each outcome variable, a binary indicator was created taking the value =1 if the observation was missing =0 otherwise. These missingness indicators were then regressed on a set of baseline covariates—including gender, ethnicity, local authority and birth year in order to assess whether the probability of missingness was systematically related to observable participant characteristics.

As per the protocol, this approach will allow us to evaluate whether data are likely to be missing at random (MAR) or display patterns of non-random missingness that could bias the main analyses.

The resulting model estimates shown in table B1 were indicative of significant differential missingness as a result of attrition at each time point. Missingness was negatively correlated with treatment assignment and positively correlated with being a Male. These results effectively suggest that those assigned to treatment were more likely to respond while, on the other hand, being a male implied a lower likelihood of responsiveness (i.e. missingness as a result of attrition or item-level non-response).

Table B1. Baseline Covariate Predictability of Missingness

Variable	Category	Midline		Endline	
		Estimate	P-value	Estimate	P-value
Allocation	Control	-	-	-	-
	Treatment	-0.12	0.05	-0.17	0.00
Gender	Female	-	-	-	-
	Male	0.20	0.00	0.21	0.00
	Other	0.10	0.53	0.29	0.06
Ethnicity	Other ethnic group	-	-	-	-
	Asian/Asian British	-0.13	0.44	-0.16	0.33
	Black British/African/Caribbean	-0.05	0.77	0.03	0.86
	Mixed/Multiple ethnic groups	-0.03	0.85	-0.06	0.72
	White	-0.03	0.83	0.00	0.99
Birth Year		0.02	0.09	0.02	0.25

Imputation

To address missing data for the primary outcome of HSS and ensure that all available information was used in the analysis, we employed two complementary imputation strategies. First, a Last Observation Carried Forward (LOCF) approach was used to retain participants' most recent observed values, allowing their previous responses to inform subsequent time points in cases of item non-response. We also implement Multiple Imputation by Chained Equations (MICE) to account for uncertainty in the imputation process and reduce potential bias arising from missingness that is not completely at random.

MICE uses observed data across variables to generate multiple plausible datasets, which are then combined to produce robust pooled estimates. We utilise MICE within each treatment group to avoid cross-group bias contaminating imputed values. Together these approaches maximise the analytical sample while ensuring that results remain reliable and reflective while assuming different underlying data patterns.

Finally, we also compare the Exploratory model estimation of the HSS which uses the full available sample by averaging the value of the scale only across all non-missing elements.

Table B2. Imputation Models vs. Main Model

HSS Model	Midline			Endline		
	Effect Size	P-value	Sample size	Effect Size	P-value	Sample size
Main	0.26	0.09	184	0.10	0.53	197
Exploratory	0.17	0.43	119	-0.20	0.40	143
LOCF	-0.02	0.87	294	-0.11	0.38	294
MICE	0.24	0.12	294	0.03	0.85	294

Imputation using LOCF produces the weakest and least significant results, which is consistent with LOCF’s known bias toward attenuation and overly conservative estimates (Streiner, 2008). Interestingly, MICE results are not that dissimilar to the main model approach indicating a consistent magnitude of effect at each time point, although significance level was higher for the main model approach. This agreement supports the robustness of findings when missing data are handled using MAR-based multiple imputation rather than fixed-value carry-forward assumptions.

Across all models, point estimates are positive and consistent in direction across analytic strategies, suggesting a potential improvement in housing stability associated with the intervention. The Main and MICE models which make fuller use of available data show larger effects and smaller p-values, implying that missing data in the complete-case analysis may have attenuated the true relationship.

Appendix C: Scale Instruments

Primary Outcomes

Table C1. Housing Security Scale

Questions	Response rating
My current accommodation is only temporary even if I wanted to stay.	1 to 5
I get along with the people I live with and they are reliable when it comes to obeying the landlord's rules and paying their rent on time.	1 to 5
Where I live has subsidies, workers, or specific policies that help me to maintain my housing.	1 to 5
In the last 6 months I have had a history of maintaining my accommodation and I have not been evicted.	1 to 5
I am settled in my place and know what to expect about living here.	1 to 5
I follow my landlord's rules.	1 to 5
I feel confident about my ability to pay my rent on time.	1 to 5
I am working or am enrolled in classes and I have been showing up on time and performing to a satisfactory level, which includes if you are on summer break or if your work is seasonal.	1 to 5
Drugs and alcohol are a source of conflict in my personal relationships or interfere with my ability to fulfill my responsibilities or to work towards personal goals.	1 to 5
I have legal troubles that may interfere with my ability to adequately maintain my housing or fulfill my personal responsibilities over the next 6 months.	1 to 5
I am satisfied with my current housing.	1 to 5
Overall my life feels stable to me.	1 to 5

Table C2. InCharge Financial Distress/Wellbeing

Questions	Response rating
<p>What do you feel is the level of your financial stress today on a scale of 1 to 10? 1 is overwhelming stress, 4 is high stress, 7 is low stress and 10 is no stress at all. - Select the level that best describes how you feel.+A2:A9</p>	1 to 10
<p>How satisfied are you with your present financial situation on a scale of 1 to 10? 1 represents complete dissatisfaction, and 10 represents complete satisfaction. - Select the level that best describes how you feel.</p>	1 to 10
<p>How do you feel about your current financial situation on a scale of 1 to 10? 1 is feeling overwhelmed, 4 is sometimes feeling worried, 7 is not worried, and 10 is completely comfortable. - Select the level that best describes how you feel.</p>	1 to 10
<p>How often do you worry about being able to meet normal monthly living expenses on a scale of 1 to 10? 1 is worry all the time, 4 is sometimes worry, 7 is rarely worry, and 10 is never worry. - Select the level that best describes how you feel.</p>	1 to 10
<p>How confident are you that you could find the money to pay for a financial emergency that costs about £700 on a scale of 1 to 10? 1 is no confidence, 4 is little confidence, 7 is some confidence, and 10 is high confidence. - Select the level that best describes how you feel.</p>	1 to 10
<p>How often do you want to go out to eat, go to a movie or do something else and don't go because you can't afford to, on a scale of 1 to 10? 1 is all the time, 4 is sometimes, 7 is rarely, and 10 is never. - Select the level that best describes how you feel.</p>	1 to 10
<p>How frequently do you find yourself just getting by financially and living paycheck to paycheck on a scale of 1 to 10? 1 is all the time, 4 is sometimes, 7 is rarely, and 10 is never. - Select the level that best describes how you feel.</p>	1 to 10
<p>How stressed do you feel about your personal finances in general on a scale of 1 to 10? 1 is overwhelming stress, 4 is high stress, 7 is low stress, and 10 is no stress at all. - Select the level that best describes how you feel.</p>	1 to 10

Secondary Outcomes

Table C3. Warwick-Edinburgh Mental Wellbeing Scale

Questions	Response rating
Please indicate to what extent each of the following statements is true for you. - I've been feeling optimistic about the future.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been feeling useful.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been feeling relaxed.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been feeling interested in other people.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've had energy to spare.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been dealing with problems well.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been thinking clearly.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been feeling good about myself.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been feeling close to other people.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been feeling confident.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been able to make up my own mind about things.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been feeling loved.	1 to 5
Please indicate to what extent each of the following statements is true for you. - I've been interested in new things.	1 to 5

Questions	Response rating
Please indicate to what extent each of the following statements is true for you. - I've been feeling cheerful.	1 to 5

Table C4. ENRICHD Social Support Instrument

Questions	Response rating
Please indicate to what extent the following statements are true for you. - Is there someone available to whom you can count on to listen to you when you need to talk?	1 to 5
Please indicate to what extent the following statements are true for you. - Is there someone available to you to give you good advice about a problem?	1 to 5
Please indicate to what extent the following statements are true for you. - Is there someone available to you who shows you love and affection?	1 to 5
Please indicate to what extent the following statements are true for you. - Is there someone available to help with daily chores?	1 to 5
Please indicate to what extent the following statements are true for you. - Can you count on anyone to provide you with emotional support like talking over problems or helping you make a difficult decision?	1 to 5
Please indicate to what extent the following statements are true for you. - Do you have as much contact as you would like with someone you feel close to, someone in whom you can trust and confide in?	1 to 5
Are you currently married or living with a partner? If yes or no, please specify below (e.g. married but not living with spouse)	Yes/No

Appendix D: CONSORT checklist

Section/topic	No	CONSORT 2025 checklist item description	Reported on page no.
Title and abstract			
Title and structured abstract	1a	Identification as a randomised trial	5
	1b	Structured summary of the trial design, methods, results, and conclusions	5
Open science			
Trial registration	2	Name of trial registry, identifying number (with URL) and date of registration	1
Protocol and statistical analysis plan	3	Where the trial protocol and statistical analysis plan can be accessed	1
Data sharing	4	Where and how the individual de-identified participant data (including data dictionary), statistical code and any other materials can be accessed	N/A
Funding and conflicts of interest	5a	Sources of funding and other support (eg, supply of drugs), and role of funders in the design, conduct, analysis and reporting of the trial	1
	5b	Financial and other conflicts of interest of the manuscript authors	1
Introduction			
Background and rationale	6	Scientific background and rationale	7
Objectives	7	Specific objectives related to benefits and harms	7

Section/topic	No	CONSORT 2025 checklist item description	Reported on page no.
Methods			
Patient and public involvement	8	Details of patient or public involvement in the design, conduct and reporting of the trial	N/A
Trial design	9	Description of trial design including type of trial (eg, parallel group, crossover), allocation ratio, and framework (eg, superiority, equivalence, non-inferiority, exploratory)	9-10
Changes to trial protocol	10	Important changes to the trial after it commenced including any outcomes or analyses that were not prespecified, with reason	9
Trial setting	11	Settings (eg, community, hospital) and locations (eg, countries, sites) where the trial was conducted	9
Eligibility criteria	12a	Eligibility criteria for participants	9
	12b	If applicable, eligibility criteria for sites and for individuals delivering the interventions (eg, surgeons, physiotherapists)	N/A
Intervention and comparator	13	Intervention and comparator with sufficient details to allow replication. If relevant, where additional materials describing the intervention and comparator (eg, intervention manual) can be accessed	10
Outcomes	14	Prespecified primary and secondary outcomes, including the specific measurement variable (eg, systolic blood pressure), analysis metric (eg, change from baseline, final value, time to event), method of aggregation (eg, median, proportion), and time point for each outcome	11-12
Harms	15	How harms were defined and assessed (eg, systematically, non-systematically)	33

Section/topic	No	CONSORT 2025 checklist item description	Reported on page no.
Sample size	16a	How sample size was determined, including all assumptions supporting the sample size calculation	12
	16b	Explanation of any interim analyses and stopping guidelines	N/A
Randomisation:			
Sequence generation	17a	Who generated the random allocation sequence and the method used	10
	17b	Type of randomisation and details of any restriction (eg, stratification, blocking and block size)	10
Allocation concealment mechanism	18	Mechanism used to implement the random allocation sequence (eg, central computer/telephone; sequentially numbered, opaque, sealed containers), describing any steps to conceal the sequence until interventions were assigned	10
Implementation	19	Whether the personnel who enrolled and those who assigned participants to the interventions had access to the random allocation sequence	10
Blinding	20a	Who was blinded after assignment to interventions (eg, participants, care providers, outcome assessors, data analysts)	37
	20b	If blinded, how blinding was achieved and description of the similarity of interventions	N/A
Statistical methods	21a	Statistical methods used to compare groups for primary and secondary outcomes, including harms	20
	21b	Definition of who is included in each analysis (eg, all randomised participants), and in which group	22
	21c	How missing data were handled in the analysis	32
	21d	Methods for any additional analyses (eg, subgroup and sensitivity analyses), distinguishing prespecified from post hoc	9,

Section/topic	No	CONSORT 2025 checklist item description	Reported on page no.
Results			
Participant flow, including flow diagram	22a	For each group, the numbers of participants who were randomly assigned, received intended intervention, and were analysed for the primary outcome	13
	22b	For each group, losses and exclusions after randomisation, together with reasons	13
Recruitment	23a	Dates defining the periods of recruitment and follow-up for outcomes of benefits and harms	13
	23b	If relevant, why the trial ended or was stopped	N/A
Intervention and comparator delivery	24a	Intervention and comparator as they were actually administered (eg, where appropriate, who delivered the intervention/comparator, how participants adhered, whether they were delivered as intended (fidelity))	13
	24b	Concomitant care received during the trial for each group	N/A
Baseline data	25	A table showing baseline demographic and clinical characteristics for each group	14-16
Numbers analysed, outcomes and estimation	26	For each primary and secondary outcome, by group: <input type="checkbox"/> the number of participants included in the analysis <input type="checkbox"/> the number of participants with available data at the outcome time point <input type="checkbox"/> result for each group, and the estimated effect size and its precision (such as 95% confidence interval) <input type="checkbox"/> for binary outcomes, presentation of both absolute and relative effect size	20-21
Harms	27	All harms or unintended events in each group	33

Section/topic	No	CONSORT 2025 checklist item description	Reported on page no.
Ancillary analyses	28	Any other analyses performed, including subgroup and sensitivity analyses, distinguishing pre-specified from post hoc	29-32
Discussion			
Interpretation	29	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	36
Limitations	30	Trial limitations, addressing sources of potential bias, imprecision, generalisability, and, if relevant, multiplicity of analyses	36-37

Citation: Hopewell S, Chan AW, Collins GS, Hróbjartsson A, Moher D, Schulz KF, et al. CONSORT 2025 Statement: updated guideline for reporting randomised trials. BMJ. 2025; 388:e081123. <https://dx.doi.org/10.1136/bmj-2024-081123>

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*We strongly recommend reading this statement in conjunction with the CONSORT 2025 Explanation and Elaboration and/or the CONSORT 2025 Expanded Checklist for important clarifications on all the items. We also recommend reading relevant CONSORT extensions. See www.consort-spirit.org.

Endnotes

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