Impacts of the Universal Credit welfare reform on wellbeing: A natural experiment study using UK population survey data

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Abstract

Introduction: Universal Credit (UC) was a large-scale reform of the UK welfare system, replacing six existing benefits. UC aimed to simplify claims and encourage more claimants into work. Previous research has found evidence of harms to the mental health of recipients, potentially exacerbating existing health inequalities. We identify the effect of UC on self-reported measures of psychological well-being, treating the phased rollout from 2013-2018 as a natural experiment.

Methods: We estimated differences in psychological well-being outcomes associated with the staggered introduction of UC across Local Authorities, using areas where Universal Credit was not yet available as controls. We included working-age (18-64) respondents of the Annual Population Survey in Great Britain from 2012-2019 (n=245,658), living in low-income households. We used the four self-reported measures of psychological well-being recorded in the survey: Life Satisfaction, Happiness, Life Worthwhile and Anxiety. We tested for differential effects by disability, age, caring responsibilities, sex, country, ethnicity, education and household structure.

Results: UC was associated with per-claimant decreases in Life Satisfaction (-0.66; 95%CI -1.01 to -0.30), Happiness (-0.41; 95%CI -0.77 to -0.05) and Life Worthwhile (-0.73; 95%CI -1.03 to -0.42), and increases in Anxiety (+0.79; 95%CI 0.30 to 1.27). These changes were two to six times as large as the effects on wellbeing of the COVID-19 pandemic. Respondents in Wales and Scotland saw comparatively greater effects compared to those

in England across several outcomes. UC exposure saw greater comparative increases in anxiety amongst disabled people (+0.19; 95%CI 0.12 to 0.27), single people (+0.13; 95%CI 0.06 to 0.21) and people aged under 25 (+0.27; 95%CI 0.15 to 0.39).

Conclusions: The introduction of UC had adverse effects across all four measures of well-being. Vulnerable groups typically experienced greater harms, reinforcing calls for reforms to Universal Credit to reduce the health and wellbeing impacts of poverty and unemployment.

Keywords: Social security reform; UK Welfare Policy; mental well-being; natural experiment; difference-in-differences

Key Messages

What is already known on this topic:

- Universal Credit – a new benefits system – was introduced in the UK from 2013–2018
- Changes to welfare policies may affect claimants' mental health and well-being: Universal Credit has previously been shown to be detrimental to the mental health of claimants

What this study adds

- Incremental rollout of Universal Credit led to large negative effects on well-being compared to areas which hadn't yet undergone the change
- Effects were greater amongst several vulnerable groups

How this study might affect research, practice or policy

- Reforms to Universal credit should prioritise protection of the health and wellbeing of vulnerable households, addressing the potential harms of income insecurity and stringent conditions on benefit receipt

Introduction

Universal Credit (UC) was introduced in the UK under the 2012 Welfare Reform Act as a replacement for six existing working-age benefits and tax credits. The UC system was proposed as an innovation to simplify the benefit system and reduce spending [1,2]. A further stated aim of UC was to encourage more claimants into work by setting stricter eligibility criteria and changing payment structure [2–4]. Welfare policies are an established determinant of heath, and changes in social security systems are known to impact the mental health of benefits claimants [5,6]. A simplified claims system, improved access to employment and reduction in poverty have been proposed as routes to improving mental health and well-being via the UC system [7,8]. Health commentators and researchers have expressed concerns about the design and implementation of UC and have called for clearer evaluation of potential health effects [9–11].

Several studies on the implementation of UC suggest that it adversely affects the health and well-being of some recipients [4,12–15]. Such harms may be a combination of the effects of switching to and navigating an unfamiliar online system plus the lasting effects of a difference in award amounts or benefit administration. The minimum five-week assessment period at the beginning of an award plus administrative delays have resulted in waits of up to 12 weeks for first payments [1,16]. This waiting period has been shown to cause immediate distress in low-income households [16,17]. The subsequent struggle to repay loans and advance payments taken to cover the waiting period may also have prolonged this effect beyond the initial months [18]. Fluctuations in income due to unstable employment and self-employment can lead to volatility and unpredictability in UC payments in following months [16,19]. Under 25s receive a lower monthly allowance than claimants aged 25 or over; equivalent reductions were not applied to lone parents under previous benefits systems. More stringent work-search requirements combined with (the threat of) sanctions – reduced payments when conditions are not met – may also affect mental health and well-being throughout a spell of benefit receipt [16,20]. The switch to a digital system, reported to be perceived by some claimants as “complicated, disorientating, impersonal, hostile and demeaning” [16], may also contribute to poorer mental well-being. Conversely, tailored support in applying for jobs may improve mental well-being through increased employment [2,7]. Short-term effects of a new system may be reversed by such longer-term benefits. Effects may differ by family circumstances, reasons for claiming benefits and other

health conditions, and UC has been shown to mitigate the negative impacts of entering unemployment for some claimants [12,13].

The replacement of legacy benefits with UC has taken place in three phases. Each phase involved a staggered implementation across job centres [1,2,21]. The first phase, beginning in 2013, was a restricted rollout of the 'live service' to a limited subgroup, mainly single, unemployed claimants without dependent children. In the second 'natural migration' phase, from 2015 onwards, new claimants, existing recipients whose circumstances changed, and voluntary switchers moved onto UC. The concluding 'managed migration' phase, piloted in 2019–2020 but paused for the pandemic and restarted in 2022, involves a compulsory transition of all legacy claimants to UC. After repeated delays, this phase is currently planned for completion by 2028/29 [22]. Wickham *et al*. [4] previously used the 'restricted rollout' phase to create comparable exposed and unexposed populations within the limited at-risk population defined by their employment status. The natural migration phase, now complete, gives an opportunity to test effects across the broader scope of all eligible claimants over a longer period to test whether early observed harms persist or are ameliorated by the benefits of a maturing and adapting new system.

We aimed to estimate the effect of introducing UC on the well-being of working age individuals in low-income households over the full natural migration rollout period. Self-reported measures of psychological well-being are valuable for understanding the effects of welfare reform on mental health and how they vary among population sub-groups [23–26]. Evidence on changes in population well-being is increasingly recommended to inform economic policy [27]. Estimates of the changes in subjective well-being resulting from the UC rollout would provide a tangible measure of its overall impact on those affected by the benefit system reform.

We used the staggered rollout of the natural migration phases to create natural experimental comparisons between people living in areas exposed and areas not yet exposed to UC. To better understand impacts on inequalities, we investigated how effect sizes varied by characteristics which may determine eligibility and award amount, or which may make an individual more vulnerable to changes: family structure, sex, disability, ethnicity, age, education, student status, caring responsibilities and country. We hypothesised that exposure to UC would reduce well-being amongst claimants and that greater effects would be seen amongst vulnerable populations.

Methods

We followed a pre-published protocol [28] and analysis plan [29]. Deviations from the analysis plan are outlined in Supplementary Material F.

Study design

We conducted a difference-in-differences analysis to estimate the effects of the staggered rollout of UC on well-being among recipients and potential recipients. We examined changes over time in four well-being measures – Life Satisfaction, Happiness, Life Worthwhile and Anxiety – in local authorities as UC was introduced. We compared these with simultaneous changes in areas in which it had not yet been introduced to account for common trends. We took an intention-to-treat approach, using the planned natural migration dates as a proxy for whole-area exposure [21]. Previous studies found no association between tested demographic variables (ethnicity, labour market attachment, marital status and health) and rollout date [12]. Consultations with the study advisory committee and representatives from DWP indicated that decisions regarding order of rollout were determined by administrative reasons and prioritised geographic spread – these decisions were not based on performance indicators or relating to expected outcomes. Thus, order of rollout by Local Authority appears sufficiently random to treat this staggered pattern of exposure as a natural experiment (Figure 1).

Data

We used data from Annual Population Surveys (APS) collected from April 2012 to March 2020 (April-March pooled datasets, 921,139 observations) [30]. Previous and simultaneous research on the effects of UC have used the UK Household Longitudinal Survey and measures of mental health, quality of life and well-being [4,12,13,15,28]. We sought to complement this research using a large, cross-sectional dataset and further well-being measures. The Annual Population Survey is a collation of the Labour Force Survey responses, consecutively gathered across five quarters. Only respondent's quarter one and five responses are included in sequential annual APS datasets, which are weighted to be a representative cross-section of the UK population. The survey records details of employment and benefit receipt (by type), alongside demographic variables [31]. Questions recording personal well-being across four measures – Life Satisfaction, Happiness, Life

Worthwhile and Anxiety – were introduced from April 2012 [32]. We conducted a complete case analysis as, across all APS respondents in the 2012–2020 period, data across all selected variables were missing in only 1.6% of cases (14,745 observations removed).

We used the ONS personal well-being variables as measures of outcome and so included data from April 2012 onwards. We excluded observations from 2020 onwards as these would include COVID-19 affected responses and may have produced different patterns of employment and benefit claims. We used a secure access version of the dataset in which each respondent's area of residence was recorded, allowing for grouping of observations by Local Authority district. These districts were mapped onto the Department of Work and Pensions' Job Centre areas that were used in the 'natural migration' rollout of Universal Credit from 2015–2018 [21]. We excluded Local Authorities with low numbers of observations (fewer than 100) in any quarter (8,920 observations removed).

To control for area-level confounders, we calculated yearly proportional changes in economic productivity and local government spending from the 2012 baseline. We used data from the ONS 'Regional economic activity by gross domestic product' dataset to calculate gross value added to represent yearly economic productivity by UK Local Authority (LA) [33]. To represent local government spending, we used recorded spending per capita on social care, culture and education as these were deemed to be not directly affected by UC rollout. These were obtained for England from the 'Place-based Longitudinal Data Resource' [34–36], StatsWales open data for Welsh LAs [37], and from Scottish Local Government Finance Statistics (collated across years upon email request) [38].

Population

To identify an 'at risk' population comparable across all time points, we included residents of low-income households, aged 18 or over who were not retired and not working twenty or more hours per week. This would include most respondents who are eligible for benefits but may also include some who are not eligible. We use income rather than reported benefit receipt to identify the at-risk population for two reasons: (1) benefit receipt may have been under-reported in the survey; (2) the effects of the change from legacy benefits to UC may extend beyond benefit recipients, for example by prompting claimants to enter or increase employment and end their benefit claim. By including all low-income households, we aimed to make all observations comparable across time periods.

To identify low-income households, we calculated household equivalised income using the modified OECD equivalence scale [39]. We set a threshold of under £12,000 equivalised annual household income for inclusion. This was derived from the upper quartile of incomes amongst earners reporting benefit claims and the lower quartile of earners reporting no benefit claims (Supplementary Material Table A-3 and Figure A-1).

In sensitivity analyses we repeated these analyses for all respondents reporting a benefit claim (UC or at least one equivalent legacy benefit) and for higher and lower wage thresholds based on median and 90th income percentiles to test for similarities in effect direction and magnitude.

Exposure

We used the planned rollout dates to determine exposure to UC [21]. Dates were available by Job Centre Plus which in most cases were grouped by Local Authority. This level of geography allowed matching of observations to rollout dates and use of area-level covariates. We matched APS observations by Local authority and calendar quarter to assign a dummy variable coding UC exposure (‘1’ in the quarter of rollout and all quarters following; ‘0’ otherwise) and a count of quarterly leads/lags to UC rollout (centred at ‘0’ in the rollout quarter).

Outcomes

We used the ‘ONS4’ personal well-being measures to capture the impact of the UC rollout on individuals’ well-being and lived experiences across four key domains: Life Satisfaction, Happiness, Life Worthwhile, and Anxiety. Respondents were asked to rate how they were currently feeling in each domain from 0–10. For life satisfaction, happiness and life worthwhile, 0 represented the lowest level of wellbeing and 10 the highest. For anxiety the scale is reversed (see questions in Supplementary Material A.2). Unlike traditional economic and social metrics, subjective well-being reflects individuals' lived experiences, preferences, and personal values, capturing the net impact of policy changes on diverse groups. These measures offer a multidimensional view of well-being by assessing life satisfaction, emotional state, and a sense of meaning and purpose in life – critical for understanding the nuanced effects of welfare reform on individuals’ quality of life [25]. By treating these outcomes as continuous variables [32], we can quantify the specific ways the

UC rollout influenced well-being, providing valuable insights into the reform's overall impact on vulnerable populations.

Covariates

In confounder-adjusted models, estimates were adjusted for age, age squared, sex, ethnicity (combined into two categories: white and non-white), disability, whether has a work-limiting health condition, highest level of qualification, employment status (employed/inactive/seeking), housing tenure, whether has caring responsibilities (reporting not seeking work as looking after home/family), number of children (categorised 0, 1, or 2+), marital status (non-married or married/cohabiting), year of observation, area-level unemployment rate, area-level disability rate, area-level gross value added, area-level culture spending per capita (adjusted relative to 2013), and quarter of local authority migration to UC.

Statistical analysis

We used difference-in-differences methods to estimate the effects of the introduction of UC on well-being. We used person-weights as provided in the APS datasets to make the sample representative of the UK population by sex and age group [40].

Classic difference-in-differences models examining staggered exposure across multiple units use two-way fixed-effects (TWFE) models to estimate average effects of exposure. This method assumes that treatment effects do not change over time: if treatment leads to changes in trends, early-treated units become controls for later-treated units, thus potentially biasing results [41–43]. This assumption is likely to be violated in examining effects of UC due to incremental increases in numbers of claimants and thus an intensifying population-wide effect in the post-rollout period.

To study differences in effect across the post-rollout period robustly, we used several methods to account for this expected bias [42]. We used the two-stage difference-in-differences method and the `did2s` R package [43,44] as the most suitable for our (non-balanced) data. This method estimates a 'never-treated' potential outcome from a regression across not-yet-treated observations while accounting for period and time effects before estimating effects as observed differences from this imputed counterfactual outcome. Each model estimated dynamic event-study estimates for each outcome across

quarters following rollout and a static effect estimate across the whole exposed period. We recorded static estimates – average effects across the exposure period – as primary measurements of effect size for interpretability. We plotted event-study estimates to visually examine changes over time. In our main analysis we included all observations across the 2012–2019 period.

As a sensitivity analysis, we fitted a model to a 'truncated' dataset to exclude observations relatively distant from the rollout date. We excluded early observations before 2013 and late observations more than two years after UC rollout relative to respondents' local area.

We conducted TWFE sensitivity analyses to test whether the expected biases produced differing estimates, as outlined in our protocol [29]. We fitted unadjusted regressions and fully adjusted TWFE models, limiting post-rollout observations to the four quarters following the rollout date in each local authority. We fitted two further adjusted models with one post-rollout time point at 1 year and 2 years after the natural migration date – this prevents use of post-rollout observations as controls and estimates time-changing effects.

To give interpretable estimates of effects, population-wide differences in outcomes were scaled to a 'per claimant' indicator of effect sizes by dividing the estimated average change in outcome by the proportion of respondents reporting UC receipt in the exposed period. Per-claimant standardised measures of change from pre-rollout means were calculated by dividing scaled estimates by pre-rollout standard deviations. To place the effect estimates in context we used the April 2020–March 2021 APS dataset to estimate corresponding per-person effects of entering the COVID pandemic across all UK households.

To test for differences of effects on vulnerable populations, we estimated effects for the following subgroups: full-time students, young people (aged under 25, matching standard allowance threshold) [45], disabled people, people with dependent children, single people, people with caring responsibilities, lone parents, women, people of non-white ethnicity and people with lower education. We also compared Scotland and Wales with England to test for differences in effects across the three countries which may be produced by differences in benefit administration.

We tested for parallel pre-intervention trends across all Local Authority areas grouped by quarter of UC rollout (thirteen quarters). We plotted quarterly pre-rollout mean outcomes across all grouped observations with fitted trend lines and inspected these visually. We

further tested differences in trends relative to the trends of the latest quarter for unadjusted and fully adjusted models.

All statistical analyses were conducted in R (v4.2.2) [46]. Rendered documents containing analysis results were added to OSF at osf.io/knajb [29].

Ethics approval

We used a Special Licence version of the APS datasets, under the UK Data Service Secure Lab Project No. 116676. Survey respondents provided informed consent before being interviewed. All other data were openly published online. Further ethical approval for analysis was not required.

Role of the funding source

This study is part of an NIHR-funded study on evaluating the mental health impacts of Universal Credit, grant number NIHR131709 [28]. We also acknowledge funding from the Medical Research Council (MC_UU_00022/2), the Scottish Government Chief Scientist Office (SPHSU17) and the European Research Council (949582).

Results

We included 245,658 observations in our study sample (Table 1). Using survey weighting for representativeness, the mean age was 39.2 years in pre-local-rollout periods and 39.9 in post-rollout periods exposed to UC. 55.2% of pre-rollout and 56.4% of post-rollout respondents were women. Most respondents were white (80.9% pre and 78.6% post), living in England (85.2% and 86.5%), not disabled (65.5% and 61.9%) and living in rented accommodation (63.2% in both periods). Pre-rollout respondents had a mean Life Satisfaction score of 7.0 (SD = 2.1), mean Happiness score of 7.0 (SD = 2.4), mean Life Worthwhile score of 7.4 (SD = 2.1) and mean Anxiety score of 3.4 (SD = 3.0). Each outcome increased by ~0.1 points after rollout (not adjusting for pre-rollout trends).

An average of 6.3% of the sample reported receiving Universal Credit in the first year after rollout, increasing to 9.4% across all post-rollout time periods (37.8% of respondents reported still claiming legacy benefits in the post-rollout period). Small proportions (<2%) of respondents reported claiming UC in the period before the ‘natural migration’ rollout date

(see Supplementary Material Figure A-2), potentially having transitioned to UC during the 'restricted rollout' phase. A clear change in trend is seen at the intended transition period, with numbers of UC-claiming respondents rising rapidly in the first four quarters after exposure and continuing beyond this period. There is a rapid decrease in numbers of observations in later quarters, with only 67 of the 380 LAs being observed for nine or more quarters post-rollout.

Two-stage model estimates are presented in Table 2 and Figure 2. In adjusted models, rollout of UC was associated with an average -0.06 point drop in Life Satisfaction (-0.10 to -0.03), a -0.04 point drop in Happiness (-0.07 to -0.00), a -0.07 point drop in life rated as 'worthwhile' (-0.10 to -0.04), and a 0.07 point increase in Anxiety (0.03 to 0.12; Figure 2). Given that on average 9.4% of exposed people report claiming Universal Credit, this is equivalent to a -0.66 (-1.01 to -0.30) point per claimant change in Life Satisfaction (-0.31 standard deviations (SDs) from a pre-rollout mean of 7.0), a -0.41 (-0.77 to -0.05) point per claimant change in Happiness (-0.17 SDs from a mean of 7.0), a -0.73 (-1.03 to - 0.42) point per claimant change in feeling life is worthwhile (-0.36 SDs from a mean of 7.4), and a 0.79 (0.30 to 1.27) point per claimant change in Anxiety (0.26 SDs from a mean of 3.4; Table 2). To put these effects in context, the impact on psychological wellbeing across all survey respondents of entering the COVID pandemic was smaller in each of the four domains (Table 2).

In event-study plots produced from two-stage difference-in-differences models, across all four outcomes we see indicators of harm across the early period after the rollout dates (around the first two years), with diverging and uncertain effect estimates and effect directions in later periods (Figure 3). Over the first eight quarters, we observed small, mostly negative effects, with confidence intervals not including zeros in a few cases: for Life Satisfaction we saw significant effects in three periods (3rd, 4th and 7th quarters after rollout), for Happiness we saw effects in only one period (4th quarter), for Life Worthwhile we saw effects in three periods (3rd, 4th and 5th quarters) and for Anxiety in two periods (4th and 6th quarters).

In subgroup analyses we found evidence of greater effects across several markers of vulnerability (Supplementary Material C). Single people saw greater effects on Life Satisfaction and Life Worthwhile and greater Anxiety (Supplementary Material Figure C-1). Disabled people experienced greater effects across all four well-being domains

(Supplementary Material Figure C-2). We found larger effects of the UC rollout on Anxiety among people aged under 25, women, carers and full-time students, with no clear differences across other outcomes. Few clear differences in effect were seen across levels of educational attainment (Supplementary Material Figure C-10). We found no clear evidence of UC-related effects on well-being among people from minority ethnic groups whilst people of white ethnicity experienced negative effects across all four outcomes (Supplementary Material Figure C-9).

Across the three countries, there was some evidence of larger adverse effects among people in Wales on Life Satisfaction (-0.07 (-0.14 to -0.01)), Happiness (-0.11 (-0.22 to -0.01)), Life Worthwhile (-0.07 (-0.16 to 0.02)) and Anxiety (0.12 (-0.07 to 0.31)) than people in England. Scottish respondents saw greater harms to Life Satisfaction (-0.09 (-0.16 to -0.02)) compared to England (Supplementary Material Table C-1 and Figure C-7).

Across all observations, parents with caring responsibilities for 1+ children saw more positive outcomes in Life Satisfaction (0.12 (0.07 to 0.18)), Happiness (0.11 (0.04 to 0.18)) and Life Worthwhile (0.08 (0.03 to 0.13)) compared to non-parents. Lone parents experienced greater harms to Life Satisfaction (-0.06 (-0.14 to 0.02)) and Anxiety (0.20 (0.03 to 0.31)) compared to coupled parents, with the largest increases in anxiety across all groups, whilst seeing potentially positive differences in the other outcomes from single non-parents.

Across pre-intervention periods, grouped by quarter of rollout, most groups show stable trends in Anxiety and small increases across Happiness, Life Satisfaction and Life Worthwhile, with some diverging trends (Supplementary Material Figure A-3). Once adjusting for potential confounders, any differential trends across LAs were largely eliminated (Supplementary Material Figure A-5). We took these conditional parallel trends in the pre-rollout period as satisfying the parallel trends assumption. We hypothesised that anticipatory effects would largely be independent of rollout dates, as most claimants were likely unaware of UC coming to their own job centre next month. To test for anticipatory effects, we visually examined differences in outcomes in the quarters preceding rollout in event-study plots and found no consistent indicators of anticipatory effects.

Sensitivity analyses

Two way fixed-effect estimates are presented in Supplementary Material B. Across the first year following UC rollout, estimated effect directions and magnitudes are very similar to two-stage model estimates (Supplementary Material Figure B-9). Effects observed at the single time point one year after rollout were larger than period-average effects (two-to-three times larger than first year averages). At two years, effects were reduced across all four outcomes, with large confidence intervals likely driven by smaller numbers of observations (Supplementary Material Figure B-10).

In truncated models, restricting to observations from 2013 until two years after rollout dates, effect estimates were similar to main analyses for Life Satisfaction (-0.06 (-0.09 to -0.03)), Happiness (-0.04 (-0.07 to -0.00)) and Life Worthwhile (-0.06 (-0.09 to -0.03)), and smaller effects were seen for Anxiety (0.03 (-0.01 to 0.08); Supplementary Material Figure B-12).

Restricting analyses to 189,844 respondents reporting a benefit claim (Universal Credit or one of the six ‘legacy benefits’) produced larger effect estimates for Life Satisfaction (-0.10 (-0.14 to -0.06)), Happiness (-0.07 (-0.11 to -0.03)), Life Worthwhile (-0.10 (-0.13 to -0.07)) and Anxiety (0.10 (0.04 to 0.15); Supplementary Material D). Reported receipt of Universal Credit was greater in this population (18.1% compared with 9.8% of low-income population, which includes non-claimants). These estimates scale to a per-claimant effect on Life Satisfaction of -0.54 (-0.76 to -0.33), Happiness of -0.38 (-0.60 to -0.15), Life Worthwhile of -0.56 (-0.73 to -0.39) and Anxiety of 0.54 (0.25 to 0.82).

Setting equivalised income thresholds for inclusion to £8,040 per year (the median value of UC recipients’ incomes) decreased the number of observations to 222,492, of which 9.9% reported claiming UC in post-rollout years. Estimates of outcomes were similar in magnitude to main analysis outcomes (Supplement E; Figure E-2). Increasing the threshold to the 90th percentile value of £18,600 increased observations to 444,069, of which 6.2% reported claiming UC. Again, outcomes remained similar to main analyses (Supplement E; Figure E-4). These tests indicate that outcomes were not sensitive to the chosen threshold of £12,000.

Discussion

During the restricted rollout and natural migration phases from 2013-19, implementation of Universal Credit was associated with a reduction in each of the measured domains of well-being of adults in low-income households. These effects persisted across the first two years of rollout in each locality and were consistent across models with differing assumptions. We identified variation in the effects of UC among specific sub-populations, notably greater anxiety among young people, disabled people, women, full-time students, those with caring responsibilities and single people (with a stronger effect on lone parents). Conversely, people of non-white ethnicity experience less adverse effects than those of white ethnicity, and couples with children may have experienced improved wellbeing. People living in Scotland and Wales experienced poorer outcomes in some domains compared to people living in England. The potentially greater impact in Scotland is contrary to the expected effects of differences in administration of benefits, which aimed to reduce the impact of benefits system change – these estimates may indicate that early amendments were ineffective or insufficient [47]. Similar effects in Scotland and Wales may be indicative of populations which were more vulnerable to the UC rollout. The Scottish Child Payment, introduced in 2021 after the observation period of this study, awarded additional amounts to UC claimants with children to reduce child poverty [48], and may mitigate some of these effects in Scotland.

The effect sizes across the population represent an average effect in the magnitude of ~0.1 points in the 10-point scale but are potentially produced by a comparatively small subset of the observed population with fewer than 10% of respondents reporting UC receipt. When standardising to a per-person effect (under the assumption that the effects of benefits change were fully or substantially felt by those switching to the new benefit), these represent potentially substantial impacts on a person's well-being. Although small in absolute terms they are considerably larger (1.8 to over 6 times) than the effects of the COVID-19 pandemic (Table 2).

Well-being measures provide valuable insights into the complex effects of policy changes. Public consultations identified 'Life satisfaction' as an important indicator of national well-being, and it is increasingly used in economic analyses through the WELLBY (well-being adjusted life year) framework to quantify and monetise the well-being effects of policies [25,27]. In our baseline analysis, the estimated 0.66-point reduction in life satisfaction

across the UC rollout period translates into a significant well-being cost. Based on the recommended WELLBY values of £10,000 to £16,000 per person per year [27], this reduction corresponds to a monetary loss of between £6,600 and £10,560 per person per year (in 2019 prices). These figures highlight the substantial welfare costs of the rollout of the policy, which should be considered in further economic evaluations of the reform and in the costs of future changes to welfare.

Our findings are consistent with previous research which finds harms to mental health associated with the transition to UC, and additional harms for some groups of claimants [5,15,49,50]. Our recently published analysis of harms to mental, conducted using longitudinal data from Understanding Society, found similar estimates of harms [15]. In this analysis we were able to observe a larger population and estimate differential effects of subgroups whilst examining well-being outcomes as complementary measures. Our observations build on Wickham *et al*.'s [4] analysis of changes across the 'restricted rollout' period by assessing effects across all low income households – including both employed and unemployed potential UC claimants – during the subsequent ('natural migration') phase of UC implementation. Our findings on well-being follow a similar pattern to earlier reports of unequal increases in psychological distress. Exploring similar axes of inequality as examined by Brewer, Dang and Tominey [12], we find lower well-being among single people and lone parents. These observations are consistent with the finding of poorer mental health of such groups entering unemployment under UC relative to the legacy benefits. Similar to Thornton and Iacoella [13], we found smaller effects on claimants with children than those without. The exception to this pattern appears to be the increase in Anxiety amongst lone parents over all other family structures. Proposed positive aspects of UC – if indeed effective, e.g. simplifying the claims process, incentivising entry into work – may have produced stronger effects in some claimants or social circumstances may have buffered some of the negative effects felt more strongly by others.

We were unable to distinguish immediate effects of transitioning to or claiming UC from lasting effects of the difference in benefit administration or payment amount. Our data examined the population cross-sectionally. An observed increase of proportions of respondents reporting claiming UC – as expected from the structure of the 'natural migration' rollout – would produce greater average effects across time if the treatment effect were static. Some indication of intensifying effects can be seen across the initial quarters of

the exposed period, which is consistent with this. Alternatively, if a more intense ‘shock’ effect were seen by individuals upon first switching, this effect would reduce over time. As data did not record length of time on UC, we were unable to test for individual-level dynamics of effects.

Our analyses are limited to the period 2013–19, so we were not able to assess the effects of the two-child limit introduced in (2017). Likewise, we did not examine the effectiveness of pandemic-related changes to UC as these came after the natural migration rollout phase. Our analyses assumed an ‘as if’ random rollout of UC by local authority in the absence of a formal outline of how the order of rollout was decided [51]. Clustering of areas with similar factors affecting well-being earlier or later in the rollout schedule may have introduced bias if these corresponded with either other national events or changes in UC implementation. Testing of pre-UC trends across all LAs, grouped by rollout quarter, showed no evidence of diverging trends which strengthens the inference that subsequent differences are produced by the transition to UC. Further analyses using more granular markers of geographically determined exposure, where available, may allow more precise identification of exposed units.

Our outcomes of Life Satisfaction, Life Worthwhile, Happiness and Anxiety were selected as measures available in the dataset. These well-being outcomes, while validated measures used in other analyses of population-level mental health and well-being, do not translate directly into clinical outcomes or tangible life experiences. Our comparison to pandemic changes and monetising of WELLBY outcomes aim to give relatable contexts for these estimates.

Another limitation of our study is that our comparison across low-income households had low specificity in identifying claimants. Our use of an income marker would also have excluded higher-income households with circumstances eligible for higher extra award amounts [45]. The potential insensitivity of using observed benefit receipt and the threat to exchangeability of exposed and unexposed populations is discussed above. Using an equivalised income threshold determined from the data, our intention-to-treat approach aimed to include all households who were ‘at risk’ from changes in benefit systems, exchangeable across exposure states. Similarities in effect estimates from analyses restricted to those reporting benefit receipt suggest that our low-income household estimates are generalisable to the benefit-claiming population. Other approaches such as

propensity score matching could be applied to compare units reporting UC receipt with comparable units in unexposed areas to achieve specificity whilst maintaining exchangeability.

Implications

Our results add to existing evidence that the rollout of Universal Credit has had adverse effects on the well-being of claimants and potential claimants. Greater award amounts, a more accessible claims procedure and more flexible application of conditionality could mitigate these effects [5]. Further investigation should seek to identify which of the hypothesised mechanisms are most important, particularly for vulnerable households which have been shown to be more adversely affected and should therefore be the focus of efforts to improve the design and delivery of Universal Credit.

Statements

Author contributions

PC and CB were joint lead applicants on the NIHR grant and conceived the overall study aim and methods. AB led on data retrieval, development of methods, analysis and writing and revision of the manuscript. MT conducted second checking of analysis code and contributed to writing of manuscript. PC, SW, MM, SVK and DT-R contributed to selection of methods, review of results, drafting and revision of manuscript. HB, LM, MS, MR, MC and SA provided specialist advice on aspects of methods and interpretation and contributed to writing the paper. AB is the guarantor. All authors reviewed and approved the final manuscript.

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Transparency statement

The lead author (AB) affirms that the manuscript is an honest, accurate, and transparent account of the study being reported; that no important aspects of the study have been omitted; and that any discrepancies from the study as originally planned have been explained.

Competing interests

The authors declare none.

Patient and Public Involvement

The NIHR-funded study benefits from regular contributions from public partners, people with experience of claiming UC and staff supporting them [28]. In addition, one public partner is a member of the Study Advisory Group, where the findings were presented and implications discussed.

Our evolving approach to public involvement in this study is published at https://journals.sagepub.com/doi/full/10.1177/17579139221103178

Data sharing

Participant survey data are available through the Secure Access service at UK Data Service. The following datasets were used:

> Office for National Statistics, Social Survey Division. Annual Population Survey, 2004-2022: Secure Access. [data collection]. 29th Edition. UK Data Service, 2023 [Accessed 18 June 2024]. Available from: DOI: http://doi.org/10.5255/UKDA-SN-6721-28
>
> Office for National Statistics, Social Survey Division. Annual Population Survey Household, 2004-2021: Secure Access. [data collection]. 9th Edition. UK Data Service, 2023 [Accessed 18 June 2024]. Available from: DOI: http://doi.org/10.5255/UKDA-SN-6725-9

Figure captions

Figure 1 – Yearly Universal Credit rollout by Local Authority area. Dates for each jobcentre area are extracted from the Department for Work & Pensions schedule [21] and grouped by Local Authority.

Figure 2 – Well-being effects across all observed post-rollout periods, estimated using a 'two-stage difference-in-differences' model adjusted for all confounders

Figure 3 – Dynamic event-study plots of effects by quarter relative to the rollout of UC (at quarter ‘0’)

Tables

Demographic	Pre rollout	Post rollout
Observed Population		
Unweighted N	193,668	51,990
Weighted %	76.5%	23.5%
Well-being outcomes - Mean (SD)		
Life Satisfaction	7.0 (2.1)	7.1 (2.1)
Happiness	7.0 (2.4)	7.1 (2.4)
Life Worthwhile	7.4 (2.0)	7.5 (2.1)
Anxiety	3.4 (3.0)	3.5 (3.1)
Age		
Mean (SD)	39.2 (14.3)	39.9 (14.4)
Sex - N (%)		
Male	74,214 (44.8%)	19,477 (43.6%)
Female	119,454 (55.2%)	32,513 (56.4%)
Ethnicity - N (%)		
White	166,152 (80.9%)	44,171 (78.6%)
Non-white	27,516 (19.1%)	7,819 (21.4%)
Country - N (%)		
England	144,110 (85.2%)	38,982 (86.5%)
Wales	24,175 (5.7%)	5,447 (4.8%)
Scotland	25,383 (9.1%)	7,561 (8.7%)
Disabled - N (%)		
Not disabled	116,526 (65.5%)	29,256 (61.9%)
Disabled	77,142 (34.5%)	22,734 (38.1%)
Work-limiting health condition - N (%)		
No	126,539 (70.3%)	32,193 (67.5%)
Yes	67,129 (29.7%)	19,797 (32.5%)
Highest level of qualification - N (%)		
Degree or College	48,300 (25.7%)	14,805 (28.6%)
Upper secondary	43,053 (25.9%)	11,688 (26.8%)
Lower secondary	47,215 (23.1%)	12,283 (21.8%)
Tertiary	23,250 (11.6%)	5,427 (10.2%)
None	31,850 (13.7%)	7,787 (12.5%)
Employment status - N (%)		
In employment	82,480 (41.8%)	22,659 (43.4%)
ILO unemployed	21,963 (12.8%)	4,231 (9.1%)
Inactive	89,225 (45.5%)	25,100 (47.5%)
Housing tenure - N (%)		
Outright	40,252 (16.9%)	12,023 (18.4%)

Mortgaged	41,292 (18.8%)	10,012 (17.1%)
Rented	110,378 (63.2%)	29,413 (63.2%)
Other	1,746 (1.1%)	542 (1.3%)
Relationship status - N (%)		
Married/Cohabiting/Civil Partnership	93,404 (43.0%)	24,613 (42.1%)
Non married	100,264 (57.0%)	27,377 (57.9%)
Number of children - N (%)		
0	120,235 (61.5%)	33,617 (64.0%)
1	32,495 (17.4%)	7,736 (15.2%)
2+	40,938 (21.1%)	10,637 (20.8%)
Caring responsibilities - N (%)		
No	161,672 (84.7%)	43,670 (85.4%)
Yes	31,996 (15.3%)	8,320 (14.6%)

Ns represent unweighted counts of observations. Means, percentages and standard deviations (SDs) are weighted for representativeness. Total Unweighted N = 245,658

Table 1 – Population demographics, counted by observation and summarised by survey weighting.

Outcome	Two-stage Difference in Differences Estimates		Per-person effect of:	
	Unadjusted	Fully adjusted	Switching to UC	Entering COVID pandemic*
Life Satisfaction	-0.077 (-0.125 to -0.029)	-0.062 (-0.095 to -0.028)	-0.66 (-1.01 to -0.30)	-0.23 (-0.30 to -0.17)
Happiness	-0.059 (-0.109 to -0.010)	-0.039 (-0.072 to -0.005)	-0.41 (-0.77 to -0.05)	-0.23 (-0.36 to -0.11)
Life Worthwhile	-0.076 (-0.121 to -0.032)	-0.068 (-0.097 to -0.039)	-0.73 (-1.03 to -0.42)	-0.12 (-0.16 to -0.08)
Anxiety	0.099 (0.033 to 0.165)	0.074 (0.028 to 0.119)	+0.79 (0.30 to 1.27)	+0.43 (0.20 to 0.67)

*'COVID pandemic' effects estimated as weighted average, adjusting for year trends and month of observation; all survey participants. These estimates are not related to changes to UC claims during the pandemic.

Table 2 – estimated effects of Universal Credit from difference-in-differences models. Adjusted models are scaled up to per-person estimates and compared with the effect of the COVID-19 pandemic

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