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The health and economic costs of welfare reform: A quasi-experimental evaluation of the roll-out of Universal Credit in England

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Abstract

We provide new evidence on the mental health and healthcare utilisation impacts of the roll-out of Universal Credit, one of the most significant welfare reforms in Europe in recent decades. Designed to simplify the benefit system and strengthen work incentives by merging six means-tested programmes into a single payment, Universal Credit also introduced features that may adversely affect mental health. Exploiting the plausibly exogenous, staggered geographical introduction of Universal Credit across England between 2013 and 2018, we implement a staggered difference-in-differences design using comprehensive small-area administrative data on clinically recorded mental health and related healthcare utilisation. We find substantial adverse effects of Universal Credit exposure: a 0.10 standard-deviation increase in clinically diagnosed depression prevalence, a 0.03 standard-deviation increase in mental health-related hospital admissions and attendances, and a 0.06 standard-deviation rise in antidepressant prescribing. In natural units, these correspond to approximately 113,742 additional cases of diagnosed depression, 29,993 extra hospital admissions and attendances, and 1.29 million additional antidepressant prescriptions annually by 2018. Results are robust across specifications, sensitivity analyses, spatial aggregation, and alternative estimators. Because outcomes are measured at the small-area level, our estimates capture population-wide effects that combine direct impacts on Universal Credit recipients with spillovers to non-recipients. Indicative valuations imply sizeable associated quality-of-life losses and direct healthcare costs of approximately £2.84 billion per year. These findings highlight that welfare reforms can generate substantial mental health externalities and underscore the importance of incorporating health and healthcare system consequences into social policy design and evaluation.

Keywords: *Welfare Policy; Living Standards; Universal Credit; Mental health; Population health*

JEL Classification: *I10, I31, I38.*

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1 Introduction

Introduced in 2013 by the Welfare Reform Act (2012), Universal Credit fundamentally restructured the United Kingdom's welfare system by consolidating six means-tested benefits into a single monthly payment for low-income, working-age individuals. Widely described as one of the most consequential welfare reforms in over six decades (Dwyer and Wright, 2014), Universal Credit was intended to simplify administration, reduce complexity, and strengthen incentives for employment and increased working hours (Department for Work and Pensions, 2010). By January 2025, around 7.5 million individuals—approximately 18.3% of the working-age population—were receiving Universal Credit across Great Britain (Department for Work and Pensions, 2025a).

While the reform's stated objectives include improving labour-market attachment and, indirectly, health outcomes, the theoretical and empirical literature highlights competing mechanisms. A substantial body of research links employment and re-employment to improvements in mental health and wellbeing (e.g., Hoare and Machin, 2010; Schuring et al., 2017), consistent with the latent- and manifest-benefits-of-work hypothesis. At the same time, Universal Credit embeds design features that plausibly generate psychological strain: a digital-first application process, heightened and formalised conditionality with sanctions risk, a mandated minimum five-week wait for the first payment (often accompanied by payment delays and deductions), and reductions in benefit generosity for some groups. These features have been linked to financial insecurity, uncertainty, and stress, particularly among low-income households already facing economic and health vulnerabilities (Brewer et al., 2024; Cheetham et al., 2019; Dwyer et al., 2020; Edmiston, 2025; Thornton and Iacoella, 2024; Wickham et al., 2020; Wright et al., 2022). As Universal Credit disproportionately affects disadvantaged populations, these mechanisms risk exacerbating existing health inequalities and increasing demand for health and care services.

International evidence shows that restrictive or destabilising social protection reforms can adversely affect mental health (Barr et al., 2015, 2016; Lundberg et al., 2008; Simpson et al., 2021; Stewart, 2023; The Lancet, 2024). In England, mental ill-health is already pervasive: around one in six adults experiences a common mental disorder in any given week, with similar prevalence among children and adolescents (Newlove-Delgado et al., 2023; Song et al., 2024). The associated economic and social burden is substantial, with recent estimates placing total annual costs at approximately £300 billion—around twice the NHS England budget in 2022—reflecting not only healthcare expenditure but also productivity losses and reductions in quality of life (Cardoso and McHayle, 2024).

A growing literature examines the mental-health consequences of Universal Credit. Notably, Wickham et al. (2020), Brewer et al. (2024), and Marimpi et al. (2025) estimate causal effects on self-reported mental health using survey data. These studies provide compelling evidence of adverse impacts on claimants directly exposed to the reform. Survey-based approaches have important strengths, particularly in identifying individuals known to be affected by Universal Credit. However, they are less well suited to capturing impacts on clinically recorded morbidity, healthcare utilisation, or broader population-level consequences. Moreover, they are inherently limited in their ability to detect spillover effects operating through households, neighbourhoods, and local service systems.

This paper complements and extends the existing literature along four dimensions. First, we move beyond self-reported mental health to study clinically recorded outcomes and healthcare utilisation, including diagnosed depression, mental health-related hospital admissions and attendances, and antidepressant prescribing, using national administrative data. Second, we provide population-wide estimates rather than claimant-only effects, enabling assessment of how welfare reform translates into system-level pressures on health services. Third, by working at a fine geographic scale, we capture the combined influence of direct effects on Universal Credit recipients and indirect spillovers to non-recipients, such as stress transmission within households, informal caregiving burdens, and local service congestion. While the data do not permit a clean decomposition of these channels, comparing our population-level estimates with prior claimant-focused estimates allows us to assess the plausibility and scale of spillovers. Fourth, we translate estimated impacts into natural units and monetary terms, providing indicative valuations of quality-of-life losses and direct healthcare costs that are directly relevant for policy appraisal.

Methodologically, we exploit the plausibly exogenous, staggered geographical roll-out of Universal Credit across England between 2013 and 2018, implementing a difference-in-differences framework designed for staggered adoption and heterogeneous treatment effects. Our analysis uses comprehensive small-area administrative data at Lower-layer Super Output Area (LSOA) level, enabling national coverage while retaining sensitivity to local exposure intensity.[1] Focusing on the pre-pandemic period avoids confounding from COVID-19-related disruptions to labour markets, welfare administration, and healthcare delivery. Extensive robustness checks—including alternative estimators, anticipation effects, treatment intensity specifications, and aggregation to the Local Authority District level—support the credibility of the identification strategy.

Across specifications, we find that earlier exposure to Universal Credit significantly worsened

[1] Lower-layer Super Output Areas (LSOAs) are small statistical geographies used in England and Wales, designed to improve the reporting of small-area statistics. Each LSOA is built from groups of contiguous Output Areas–the lowest level of geographical area for census statistics—typically comprising four or five Output Areas. LSOAs generally contain between 400 and 1,200 households and have a usually resident population between 1,000 to 3,000 persons. They serve as stable, consistent 'building blocks' for the production of official statistics and for nesting within larger administrative areas such as wards and Local Authority Districts (LADs). LSOAs typically change following each Census due to population flows and changing boundaries. For this study, we adopt the 2011 Census-based LSOA geography, which defined 32,844 LSOAs in England. For more details, see: https://www.ons.gov.uk/methodology/geography/ukgeographies/statisticalgeographies.

population mental health and increased healthcare utilisation. In baseline estimates, areas exposed earlier experienced a 0.10 standard-deviation increase in clinically diagnosed depression prevalence, a 0.03 standard-deviation increase in mental health-related hospital admissions and attendances, and a 0.06 standard-deviation increase in antidepressant prescribing relative to later-exposed areas. Effects emerge immediately following exposure, persist over time, are larger in areas exposed earlier, and increase with local uptake. In natural units, these effects correspond to approximately 113,742 additional depression cases, 29,993 extra hospital admissions and attendances, and more than 1.29 million additional antidepressant prescriptions annually by the end of 2018. The implied combined annual cost, accounting for quality-of-life losses and direct healthcare expenditures, is around £2.84 billion.

By situating these estimates alongside prior claimant-level studies, our findings suggest that the total population impact of Universal Credit is substantially larger than the direct effects previously documented, consistent with meaningful spillovers beyond recipients themselves. More broadly, the results highlight how welfare reforms can generate unintended health externalities that propagate through communities and public service systems. This work contributes to a growing economic literature on unintended consequences of welfare reforms, including impacts on human capital (Bailey et al., 2024; Kalil et al., 2023), subjective wellbeing (Herbst, 2013), political outcomes (Fetzer, 2019), and crime (d'Este and Harvey, 2024; Giulietti and McConnell, 2021; Tuttle, 2019; Watson et al., 2020).

The remainder of the paper proceeds as follows. Section 2 outlines the institutional background on Universal Credit and mental healthcare provision in England. Section 3 describes the data and variable construction. Section 4 details the empirical strategy. Section 5 presents the results, and Section 6 concludes with policy implications.

2 Institutional Background

To evaluate the impact of the Universal Credit roll-out on mental health and healthcare utilisation, it is essential to understand both the institutional design of Universal Credit and the organisation of mental healthcare provision in England. This section summarises the key features of Universal Credit and the pathways through which it may plausibly influence mental health, before briefly outlining the structure of mental healthcare services relevant for interpreting our outcomes.

2.1 Universal Credit: Transforming the UK welfare system

Introduced by the Welfare Reform Act (2012), Universal Credit represents one of the most significant reforms to the UK welfare system since the Beveridge Report (Cameron, 2012; Dwyer and Wright, 2014). Universal Credit consolidated six legacy benefits—income-based Jobseeker's Allowance, income-related Employment and Support Allowance, Income Support, Working Tax Credit, Child Tax Credit, and Housing Benefit—into a single monthly payment for low-income, working-age individuals. The stated objectives included simplifying the benefit system and reducing administrative complexity, strengthening incentives for employment and increased working hours, reducing poverty, and improving health outcomes (Department for Work and Pensions, 2010; Hobson, 2021).

It was anticipated that merging fragmented benefits into a unified payment, and moving to a digitalised claims platform, would streamline procedures for claimants and administrators by simplifying access and case management. In practice, however, the reform introduced material design changes—including a digital-first application process, a mandated five-week wait for the first payment, stricter conditionality, and reductions in benefit generosity—that have the potential to affect financial security and wellbeing. These features form the basis for the mechanisms linking Universal Credit to mental health outcomes, discussed below.

2.1.1 Phased implementation of Universal Credit

A defining feature of Universal Credit has been its phased roll-out, intended to facilitate an orderly transition from legacy benefits. Three partly overlapping pathways were used: natural migration, voluntary migration, and managed migration (Department for Work and Pensions, 2022).

Natural migration began in April 2013. Under this pathway, new claimants and existing legacy-benefit recipients experiencing a change in circumstances that required a new claim (e.g. changes in employment or housing) were moved onto Universal Credit.[2] Voluntary migration operated alongside natural migration, allowing eligible claimants in roll-out areas to switch proactively if they expected to be better off under Universal Credit relative to the legacy system. Managed migration, launched later in 2019, was designed to move the remaining legacy-benefit claimants—with transitional protections—towards comprehensive coverage (Department for Work and Pensions, 2022). The timetable for managed migration has been revised several times, partly due to the COVID-19 pandemic, with subsequent policy decisions delaying the movement of some groups (notably Em-

[2]Two systems operated during the early roll-out: live service and full service. The system in place locally determined who could claim and how. Under live service (from April 2013), claims were limited to relatively simple cases—typically single, unemployed adults without children or housing costs—subject to 'gateway' restrictions. Under full service (later rolled out universally), these restrictions were removed and all eligible claimants, regardless of circumstances, were able to claim Universal Credit.

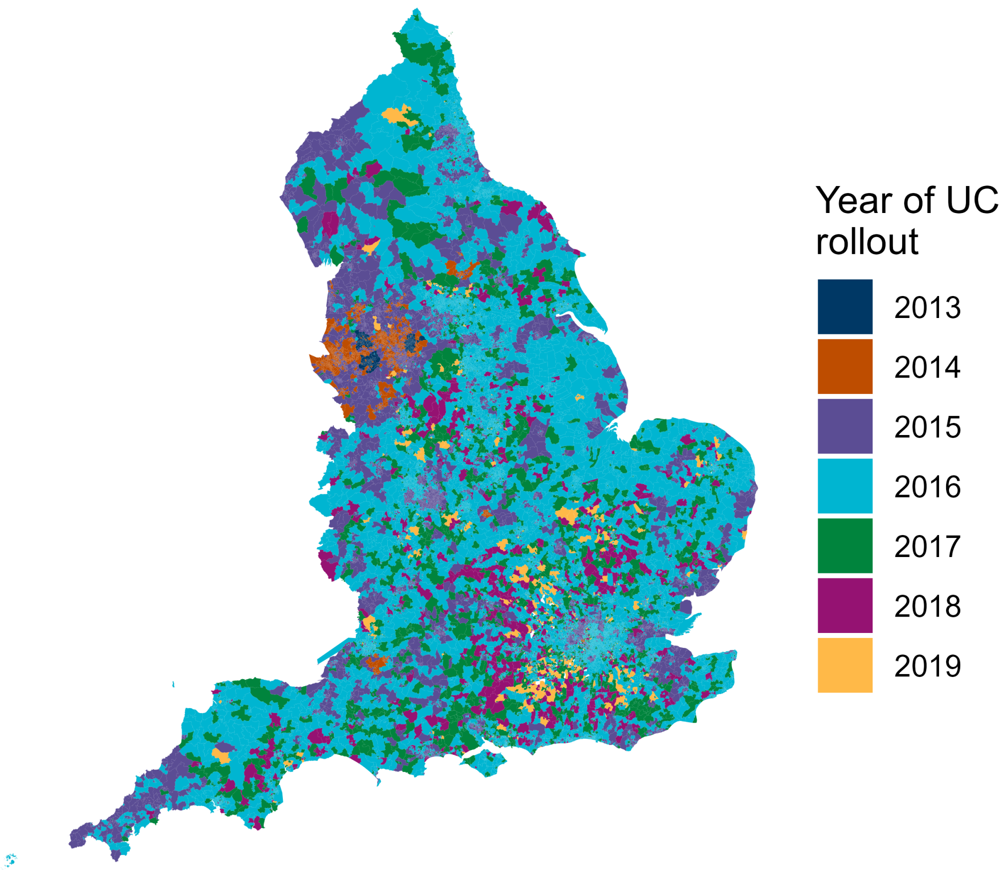
ployment and Support Allowance claimants) until 2028. Details of the implementation phases and timing are presented in Figures A1-A3 of the Supplementary Material.

Natural and concurrent voluntary migration proceeded incrementally across Local Authority Districts (LADs), reflecting the staggered introduction of Universal Credit at Jobcentre Plus (JCP) offices between April 2013 and December 2018. The Department for Work and Pensions (DWP) published a roll-out schedule mapping JCP offices to LADs; while a small number of LADs without an in-area office were served by neighbouring JCPs, the mapping shows strong geographical alignment (Department for Work and Pensions, 2018). Previous work finds that observable local characteristics did not predict the order of roll-out (Brewer et al., 2024). Consistent with this, in our data we find no systematic relationship between pre-roll-out mental health trends or baseline area characteristics and the timing of Universal Credit exposure, supporting the plausibility of exogenous variation in roll-out timing. Even within LADs, LSOAs became exposed at different times following the local JCP's Universal Credit launch, with the final group of LSOAs exposed in 2019. Figure 1 documents this spatial and temporal variation.

The combined natural and voluntary migration phase forms the basis of our empirical identification for two reasons. First, over the period 2013-2018 it effectively transitioned areas from an 'unexposed' to an 'exposed' state, generating quasi-experimental variation in exposure timing across LSOAs at national scale. Second, unlike managed migration, this phase is fully contained within our study period, avoiding policy-induced discontinuities that could confound identification.

At the same time, the pathways into Universal Credit during this phase raise potential selection concerns. Individuals entering via natural migration are exposed because of a change in life circumstances—such as job loss or housing changes—that may itself be associated with heightened vulnerability to financial stress and mental health shocks. In contrast, individuals entering via voluntary migration do so by choice and may be better positioned to manage the new system. Administrative data do not allow us to distinguish reliably between claimants entering via natural versus voluntary migration at the small-area level, nor to quantify their respective shares within exposed LSOAs. As a result, our estimates should be interpreted as capturing the average area-level effect of Universal Credit exposure during the roll-out phase, reflecting the combined influence of these pathways rather than isolating effects for specific claimant subgroups.

Figure 1: **Realised roll-out of Universal Credit for LSOAs in England based on DWP's People on Universal Credit data**



Notes: Figure covers all 32,844 LSOAs in England and shows the date when Universal Credit was first introduced locally, defined as the year in which at least one household in the LSOA successfully completed a Universal Credit claim. The data source is DWP's People on Universal Credit dataset from Stat-Xplore, which records counts of claimants at the LSOA level.

2.1.2 Core features of Universal Credit

Although Universal Credit aimed to streamline welfare administration and support smoother transitions into employment, its design and implementation introduced several features that plausibly affect mental health. Notably, stricter eligibility criteria, a mandated minimum five-week wait for the first payment, intensified work-search conditionality, and reductions in benefit generosity had the potential to increase financial insecurity and psychological strain (Brewer et al., 2024; Cheetham et al., 2019). Key policy changes are summarised in Figure A4 of the Supplementary Material.

The digital-first claims process simplified procedures for some but created barriers for others, particularly individuals with limited digital literacy or inadequate access to technology (Clegg, 2024; Hobson, 2021). Such barriers may delay or deter benefit receipt, exacerbating short-term financial stress. The minimum five-week wait—considerably longer than the one-to-two weeks typical under legacy benefits—was explicitly intended to mimic monthly salary cycles and encourage 'work-like' financial routines. While advance loans were introduced from April 2014 to mitigate immediate hardship, repayments through subsequent deductions can prolong cash-flow difficulties. Windle et al. (2019) report that around three-quarters of Universal Credit claimants experienced rent arrears due to these delays. In addition, Universal Credit shifted housing costs from direct landlord transfers (common under legacy benefits) to claimant-managed payments, increasing budgeting and financial management burdens. Consistent with these stressors and heightened vulnerability, Reeves and Loopstra (2021) show that Universal Credit roll-out was associated with increased food bank use.

A further mechanism operates through conditionality. A core instrument unique to Universal Credit is the claimant commitment, a formalised and legally binding agreement that specifies individualised work-related requirements monitored continuously by work coaches. Unlike legacy benefits—which imposed conditionality through fragmented, benefit-specific rules with more limited monitoring and discretion—Universal Credit consolidates conditionality into a single, comprehensive contract that explicitly links ongoing benefit receipt to compliance with prescribed job-search and work-preparation activities. Claimants can be required to undertake up to 35 hours per week of job-search and related activities, subject to exemptions (e.g., caring responsibilities, ill-health, or limited capability for work), with requirements dynamically adjusted over time.[3] Non-compliance may result in sanctions ranging from temporary suspensions to prolonged benefit reductions or termination (Williams, 2021). While intended to promote labour-market participation, the increased intensity and enforcement of conditionality under Universal Credit can generate heightened uncertainty and anxiety, particularly for individuals already experiencing economic or health vulnerabilities (Cheetham et al., 2019; Wright et al., 2022).

The impacts of Universal Credit are therefore unlikely to be uniform across the population. Although a streamlined structure could, in principle, ease access and raise entitlements for some households, distributional effects vary markedly across groups. Existing evidence indicates that

[3]Several Universal Credit claimants with specific conditions are exempted from the full work-related requirements if they have one of the following conditions: no sufficient ability for work or work-related activities, eligible for pension credit, pregnant and within 11 weeks of the due date, caring responsibility for a severely disabled individual or an under-one year old child, students who are aged under 21 without parental support and have a student loan or grant which will be deducted from the benefit payment, students who are in a couple and have a student loan or grant which will be deducted from the benefit payment, or a victim of domestic violence (would be given a 13-week duration of work-related requirement exemption) (Brewer et al., 2024; Department for Work and Pensions, 2025b).

employed couples often experience smaller losses or modest gains, whereas single parents and other economically vulnerable groups can face substantial reductions in income and greater exposure to conditionality and sanctions (Brewer and Hoynes, 2019). Such heterogeneity implies that the mental-health consequences of Universal Credit may differ systematically across areas depending on local population composition, baseline deprivation, and the prevalence of groups more exposed to financial strain or intensive conditionality.

Beyond individual claimants, there are community-level pathways that motivate our small-area approach. Income shocks and deductions may propagate within households—via debt accumulation, intra-family transfers, or borrowing from relatives and friends—raising stress and potential conflict. Time diverted to conditional job-search can reduce availability for unpaid or informal care within families and neighbourhoods. Arrears and food insecurity can create local service pressures (e.g., advice agencies and food banks), while uncertainty may strain landlord-tenant relationships with wider neighbourhood effects. Recognising these externalities helps avoid an atomistic fallacy: Universal Credit could plausibly generate area-level mental-health consequences through household spillovers and community resource pressures. These mechanisms underpin our empirical focus on small-area outcomes in mental health and care utilisation.

2.2 Mental healthcare services in England

The English NHS provides universally accessible, publicly funded healthcare, free at the point of delivery. Mental healthcare follows a tiered service model with general practitioners (GPs) acting as primary gatekeepers. GPs typically diagnose common mental health conditions, such as depression, using clinical criteria recorded in the Quality and Outcomes Framework (QOF). Diagnoses are entered into primary care electronic health records and aggregated centrally for monitoring and performance management.

Treatment in primary care commonly involves antidepressant prescribing by GPs, comprehensively captured in NHS Digital's national prescribing datasets. Prescriptions are dispensed by community pharmacists. While GP consultations are free, medicines attract standard prescription charges unless the patient qualifies for an exemption (e.g. low-income groups, recipients of means-tested benefits, children, or older adults).

More complex or severe cases—such as recurrent or treatment-resistant depression or substantial co-morbidities—are referred to secondary or specialist services, including community mental health teams, psychiatric outpatient clinics, crisis services, and acute inpatient care.[4] Hospital-based care for mental health conditions (including emergency presentations such as self-harm,

[4] See the NHS page, https://www.nhs.uk/nhs-services/mental-health-services for further details.

elective admissions for specialist therapies, and non-elective psychiatric care) is provided by NHS mental health trusts and funded through NHS commissioning arrangements. All hospital admissions and attendances, including mental health-related episodes, are recorded in Hospital Episode Statistics.

3 Data, Treatment Status, and Summary Statistics

3.1 Data

Our primary data source is Version 5.00 of the Small Area Mental Health Index (SAMHI), accessed via the Place-Based Longitudinal Data Resource (Daras and Barr, 2021), which we link to administrative data from DWP on the roll-out of Universal Credit. SAMHI provides a balanced panel covering all 32,844 LSOAs—small statistical areas containing approximately 1,000 to 3,000 residents—in England. We focus on the period 2011-2018, which spans the pre-roll-out years and the natural and voluntary migration phases of Universal Credit while avoiding later policy changes and pandemic-related disruptions.

SAMHI integrates three administrative sources relevant for population mental health and care utilisation: (i) GP-recorded depression prevalence from QOF; (ii) NHS Digital prescribing records for antidepressants; and (iii) Hospital Episode Statistics for mental health-related hospital admissions and Accident & Emergency attendances. We augment SAMHI with socioeconomic and demographic characteristics from the Office for National Statistics (ONS) to account for time-varying area characteristics.

Our analytical sample comprises 262,752 LSOA-year observations (32,844 LSOAs observed annually from 2011 to 2018), forming a balanced panel for all analyses.

3.2 Outcomes: Mental health measures

We analyse three SAMHI indicators measured at the LSOA level, capturing complementary dimensions of population mental health and related healthcare utilisation.

First, diagnosed depression, defined as the proportion of adult residents aged 18 or older with an active GP-recorded diagnosis of depression in the QOF. GP-level prevalence rates are allocated to LSOAs annually using NHS Digital patient registration data.

Second, antidepressant prescribing, measured as average daily quantities (ADQs) per capita using NHS Digital prescribing data. ADQs combine information on prescription counts and dosage strength to reflect typical daily use. Although antidepressants can be prescribed for non-psychiatric indications (e.g. chronic pain), interpreting this outcome alongside diagnosed depression and hospital utilisation helps mitigate concerns about indication-specific prescribing.

Third, mental health-related hospital admissions and attendances, defined as per-capita rates of emergency, elective, and non-elective hospital activity for self-harm, alcohol- and drug-related presentations, and common psychiatric disorders recorded in HES.[5]

These indicators have been externally validated and are widely used in spatial and population health analyses (e.g., Fahy et al., 2023; Rose et al., 2023).

For comparability and ease of interpretation, we standardise all outcomes to have mean zero and unit standard deviation across all LSOA-years, with higher values indicating worse outcomes. Regression coefficients can therefore be interpreted in standard-deviation units.

3.3 Area-level covariates

We include time-varying covariates at both LSOA and LAD levels. At the LSOA level, controls include population size, the share of residents of working age (16-64), and the share of female residents. At the LAD level, controls include job density (jobs per working-age resident), the unemployment rate, ethnic composition (percentage White British), and per-capita gross service expenditure on education, social care, and cultural and related services, drawn from the Place-Based Longitudinal Data Resource (Alexiou and Barr, 2019a,b,c).

These covariates are included in 'conditional' specifications to improve precision and absorb residual variation; our identification strategy does not rely on them. Consistent with the assumption that the timing of Universal Credit roll-out is plausibly exogenous to underlying mental health trends, unconditional and conditional estimates are very similar in magnitude, and event-study analyses show no evidence of differential pre-trends in either case. We therefore interpret covariate adjustment as a robustness check rather than a source of identification.

3.4 Defining area-level Universal Credit exposure

We measure Universal Credit exposure using LSOA-level claimant counts from DWP's Stat-Xplore database (Department for Work and Pensions, 2025c), restricting attention to the roll-out window relevant for our analysis. An LSOA is coded as treated in year t if (i) its parent LAD has Universal Credit in operation and (ii) at least one household in the LSOA claims Universal Credit in year t. This definition captures exposure arising from the natural and voluntary migration phases between

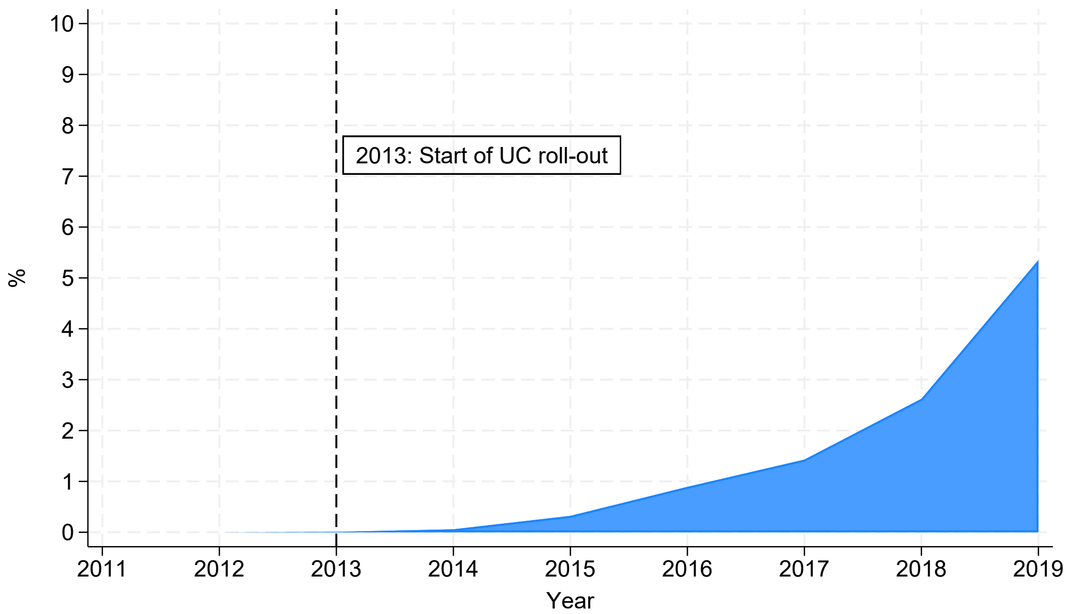
[5] Included are all emergency and elective admissions for ICD-10 codes: X60*-X84*, Y10*-Y34*, F00-F99, E244, G312, G621, G721, I426, K292, K70, K852, K860, Q860, R780, T510, T511, T519, X45, X65, Y90, Y91, as constructed by the World Health Organization, but excluding Y33.9* and Y87*. Plus, all A&E attendances for self-harm – where AEPATGROUP==30 and DIAG_NN: 141-144, 35, 37.

2013 and 2018, as discussed in Section 2, and deliberately excludes managed migration, which began in 2019.

Treatment is modelled as an absorbing state: once an LSOA is exposed to Universal Credit, it remains exposed in all subsequent years. In robustness analyses reported in Section 5.2, we replace the binary exposure indicator with a continuous measure of treatment intensity, defined as the proportion of residents who claim Universal Credit in each LSOA-year. We also assess robustness to spatial aggregation by collapsing outcomes and treatment to the LAD level and re-estimating the model at that level.

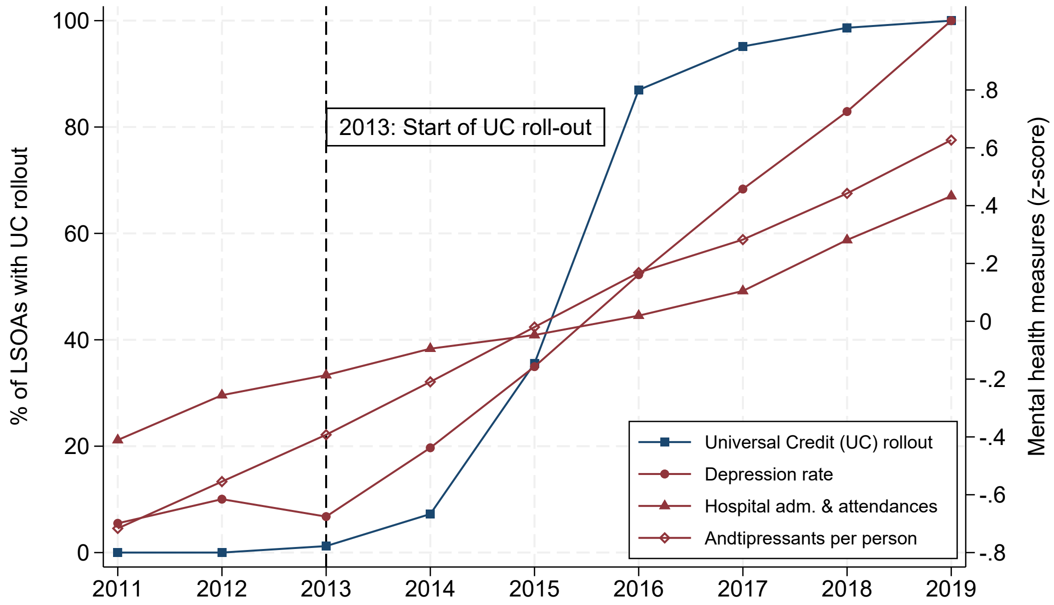
Figure 2 shows the population-weighted share of the working-age population receiving Universal Credit by year, illustrating the rapid increase in coverage following the start of the roll-out in 2013. Figure 3 plots the evolution of Universal Credit exposure alongside the three mental health outcomes at the LSOA level. While mental health burden rises gradually over the period, Universal Credit exposure increases sharply, particularly from 2014 onwards, which highlights the staggered but relatively abrupt nature of the policy rollout exploited in our empirical strategy.

Figure 2: **Weighted share of LSOA working age population on Universal Credit by year, England**



Notes: Observations: 32,844 LSOAs in England. Proportions are weighted by LSOA population. Source: People on Universal Credit, DWP Stat-Xplore.

Figure 3: **The evolution of mental health burden and Universal Credit exposure, LSOA level**



Notes: Observations: 32,844 LSOAs in England. Proportions and scores are weighted by LSOA population.

3.5 Summary statistics

Table 1 reports summary statistics for the analysis sample over the study period 2011-2018, covering 32,844 LSOAs and 262,752 LSOA-years observations. The average LSOA has a population of 1,661 residents, of whom approximately 63% are of working age (16-64) and 51% are female. At the LAD level, job density averages 0.83 jobs per working-age resident, and the mean unemployment rate over the study period is 5.9%. Ethnic composition varies substantially across areas, with an average of 86% of residents identifying as White British.

Local authority service spending also exhibits considerable cross-area variation. On average, per-capita expenditure amounts to £774.45 on education (SD £365.28), £576.80 on social care (SD £253.15), and £75.68 on cultural and related services (SD £136.77), reflecting differences in local needs, funding allocations, and fiscal capacity.

Mental health outcomes are analysed primarily in standardised form. By construction, each outcome has mean zero and unit standard deviation across all LSOA-years, with higher values indicating worse mental health or greater healthcare utilisation. For interpretability, we also report outcomes in natural units where available. In these units, the average prevalence of clinically diagnosed depression is 7.4% of the adult population, and antidepressant prescribing averages 31.5 average daily quantities per capita.

Table 1: **Summary statistics, 2011-2018**

	Obs.	Mean	SD	Min	5th percentile	Median	95th percentile	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Area-level characteristics:								
LSOA-level characteristics								
Population size (#)	262,752	1,661	363	362	1,234	1,592	2,306	14,696
Share of working age population 16-64 (%)	262,752	63.24	6.78	33.16	53.64	62.67	75.12	99.43
Share of female population (%)	262,752	50.77	2.38	15.04	47.13	50.98	53.87	67.07
LAD-level characteristics								
Job density (#)	262,752	0.83	1.52	0.39	0.54	0.78	1.13	124.78
Unemployment rate (%)	262,752	5.87	2.50	1.77	2.80	5.35	10.60	15.52
Share of White British population (%)	262,752	85.84	15.16	28.97	53.53	92.17	98.23	98.93
Per capita spending on culture (£)	262,752	75.68	136.77	2.51	34.07	67.14	132.01	10858.88
Per capita spending on education (£)	262,752	774.45	365.28	272.68	452.47	701.80	1255.92	3352.41
Per capita spending on social care (£)	262,752	576.80	253.15	338.83	422.21	521.40	779.93	2520.47
Mental health measures								
Z-score								
Diagnosed depression	262,752	0	1	-2.68	-1.44	-0.12	1.84	6.30
ADQ of antidepressants per person	262,752	0	1	-1.71	-1.14	-0.24	1.92	11.36
Hospital admissions and attendances	262,752	0	1	-3.01	-1.57	-0.04	1.74	5.21
(Available) Natural units								
Diagnosed depression (%)	262,752	7.44	2.66	0.31	3.59	7.13	12.35	24.20
ADQ of antidepressants per person (#)	262,752	31.45	10.19	0.83	15.44	31.05	49.17	84.58

Notes: This table presents summary statistics for main variables used for our analysis. Columns (1)-(2) shows the mean and standard deviation for the total population. The panel is balanced and contains data on all LSOAs in England (32,844) for the period from 2011 to 2018. Higher values of mental health measures indicate worse mental health outcomes.

4 Empirical Strategy

4.1 Identification and estimation framework

We estimate the effects of the Universal Credit roll-out on mental health and healthcare utilisation using a difference-in-differences (DiD) design that exploits staggered adoption across LADs between 2013 and 2018. Our preferred estimator follows Callaway and Sant'Anna (2021), henceforth CS-DiD, which constructs valid comparisons between each cohort of first-treated units and contemporaneously untreated ('not-yet-treated') units and identifies cohort- and time-specific treatment effects. This approach explicitly accommodates treatment-effect heterogeneity across cohorts and over time, rather than implicitly pooling effects as in conventional two-way fixed-effects models.

For context, two-way fixed effects (TWFE) specifications commonly used in policy evaluations take the static and dynamic forms:

$$Y_{i,t} = \alpha_i + \phi_t + \beta D_{i,t} + X'_{i,t}\gamma + \epsilon_{i,t} \tag{1}$$

and

$$Y_{i,t} = \alpha_i + \phi_t + \sum_{e=-K}^{-2} \beta^e D^e_{i,t} + \sum_{e=0}^{L} \beta^e D^e_{i,t} + X'_{i,t}\gamma + \nu_{i,t} \tag{2}$$

where $Y_{i,t}$ denotes the outcome for LSOA i in year t; α_i and ϕ_t are LSOA and year fixed effects, respectively; $X_{i,t}$ is a vector of time-varying covariates; $D_{i,t}$ indicates Universal Credit exposure; and $D^e_{i,t} = 1\{t - G_i = e\}$ denotes denotes the event-time indicator relative to first exposure (i.e. whether an LSOA is e periods from initial exposure at time t), with G_i denoting the first exposure year for LSOA i. Parameters β and β^e (for $e \geq 0$) are, respectively, the static and dynamic average treatment effects on the treated (ATT).

Recent methodological work shows that in staggered-adoption settings TWFE estimators combine multiple two-group, two-period comparisons and may assign negative weights to comparisons involving already-treated units. When treatment effects are heterogeneous across cohorts or over time, this can yield biased or even sign-reversed estimates (Borusyak et al., 2024; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Roth et al., 2023; Sun and Abraham, 2021). These issues are particularly salient in our context, as local responses to Universal Credit plausibly differ across areas due to variation in deprivation, digital access and literacy, legacy-benefit reliance, labour-market conditions, and local administrative capacity. Such heterogeneity motivates the use of an estimator that isolates cohort-specific effects without contamination from already-treated units.

We therefore implement the CS-DiD estimator.[6] Within this framework, the group-time-specific ATT is defined as:

$$\mathrm{ATT}(g, t) = \mathbb{E}[\,Y_t(g) - Y_t(0)\,|\,G_g = 1] \tag{3}$$

where $g \in \{2013, \ldots, 2018\}$ indexes the year in which a cohort of LSOAs is first exposed to Universal Credit. Each cohort (G_g) comprises all LSOAs whose residents first claim Universal Credit in year g, and Y_t denotes the outcome in year t. Estimating $\mathrm{ATT}(g, t)$ allows us to trace dynamic

[6] While there are different estimators available for estimating causal effects in staggered DiD setups (e.g., de Chaisemartin and D'Haultfœuille, 2020; Gardner et al., 2024; Roth and Sant'Anna, 2023; Sun and Abraham, 2021), we choose the one proposed by Callaway and Sant'Anna (2021) because it allows matching on baseline characteristics, achieving the double robustness property, and using not-yet-treated units as the comparison group.

responses within cohorts and to compare cohorts with different adoption timings. Our analysis distinguishes six cohorts corresponding to initial exposure between 2013 and 2018.

To summarise cumulative effects for each cohort g, we aggregate the group–time ATTs across post-treatment years:

$$\theta_s(g) = \frac{1}{T - g + 1} \sum_{t=g}^{T} \mathrm{ATT}(g, t) \tag{4}$$

This cohort-level aggregation permits comparison of early- and late-exposed areas. Aggregating further across cohorts using cohort shares as weights yields an overall ATT:

$$\theta_s^O = \sum_{g \in G} \theta_s(g) \ \Pr(G = g \mid G \leq T) \tag{5}$$

where $\Pr(G = g \mid G \leq T)$ denotes the probability that an LSOA is first exposed in year g. This overall estimand has a natural interpretation analogous to the standard two-period DiD ATT, capturing the average effect of Universal Credit exposure across all treated areas over their post-treatment periods.[7]

Estimation uses the doubly robust procedure of Sant'Anna and Zhao (2020). For statistical inference, we implement a bootstrap procedure with 1,000 replications and cluster standard errors at the LAD level—the administrative unit at which Universal Credit roll-out was assigned. This clustering strategy allows for arbitrary serial correlation over time and spatial correlation across LSOAs within the same LAD, thereby accommodating correlated shocks within local labour markets and administrative areas. We also include LSOA-specific and period-specific fixed effects to control for time-invariant unobserved heterogeneity and common temporal shocks affecting mental health outcomes and healthcare utilisation.

To assess dynamic responses and evaluate the plausibility of parallel trends, we report CS-DiD event-study estimates. A potential concern in recent DiD methods is that default event-study implementations may treat pre- and post-treatment periods asymmetrically, mechanically flattening pre-treatment coefficients (Roth , 2024). To address this concern, we follow Roth (2024)'s recommendation and estimate event-study coefficients using a common 'universal' base period, implemented via the *base-period = "universal"* option in the CS-DiD estimation. This ensures that pre-treatment coefficients are constructed using long-difference comparisons in the same manner as post-treatment coefficients.

Finally, for completeness, Section B of the Supplementary Material reports estimates from al-

[7]For a detailed overview of the CS-DiD method and its assumptions we refer to Callaway and Sant'Anna (2021) and Roth et al. (2023).

ternative estimators designed for staggered adoption, including Borusyak et al. (2024); Roth and Sant'Anna (2023), and Sun and Abraham (2021). We also present TWFE estimates based on Equations (1) and (2) in Supplementary Table B2. Consistent with heterogeneous treatment effects and the presence of negative weighting, TWFE estimates are attenuated relative to CS-DiD estimates in our data, as early-treated areas experienced larger adverse impacts and comparisons that mix already-treated with later-treated units bias pooled estimates downward (Goodman-Bacon, 2021).

4.2 Identifying assumptions

Our empirical strategy rests on three key identifying assumptions required for a causal interpretation of the estimated effects.

First, we assume that the staggered geographic roll-out of Universal Credit across LADs is plausibly exogenous to underlying trends in mental health and healthcare utilisation. This assumption rules out systematic prioritisation of earlier roll-out areas based on factors that would independently affect mental health outcomes. It is supported by existing evidence showing that observable local characteristics did not predict exposure to Universal Credit (Brewer et al., 2024), a pattern that is also borne out in our data.

Second, our baseline specification adopts a no-anticipation assumption: households and local institutions are not assumed to substantially change behaviour prior to the local introduction of Universal Credit. This assumption is credible given that the roll-out schedule was centrally determined by the DWP, with limited scope for influence by local health services or authorities. We assess the sensitivity of our findings to potential anticipatory responses by allowing for one year of anticipation in Section 5.2. The resulting estimates remain positive, precisely estimated, and of similar or larger magnitude, indicating that our conclusions are robust to plausible pre-implementation behavioural adjustments.

Third, our design relies on a parallel-trends assumption, namely that in the absence of Universal Credit, outcomes in earlier-treated areas would have evolved similarly to those in not-yet-treated areas. We assess this assumption using event-study estimates of pre-treatment effects. As discussed in Section 4.1, these event-study coefficients are constructed using a common base period so that pre- and post-treatment estimates are derived symmetrically, avoiding mechanical flattening of pre-treatment coefficients highlighted in recent methodological work (Roth , 2024). Under this specification, pre-treatment estimates are close to zero and mostly statistically indistinguishable from zero, providing no evidence of differential pre-trends. We report both unconditional specifications and models conditioning on time-varying area covariates.

For some group–time comparison—particularly for early-treated cohorts (e.g. those first exposed in 2013) evaluated many years after treatment (e.g. in 2018)—diagnostic checks indicate limited common support between treated and comparison units in the conditional specifications. In these cases, propensity-score distributions and covariate-balance diagnostics reveal insufficient overlap, which prevent reliable construction of counterfactual outcomes using the doubly robust estimator and resulting in missing $\mathrm{ATT}(g, t)$ estimates for those cells. Consequently, conditional models report post-treatment dynamics for up to two years following exposure, whereas unconditional models support a full five-year event-study window. We interpret these patterns as reflecting data limitations in high-dimensional conditioning rather than violations of the identifying assumptions, and we therefore place primary emphasis on cohort-aggregated estimates and robustness across specifications.

5 Results

5.1 Baseline results

We begin by estimating the average effects of the Universal Credit roll-out on mental health and related care utilisation at the small-area level. Figure 4 presents dynamic event-study estimates under both unconditional and conditional parallel-trends assumptions, allowing an assessment of pre-treatment trends and post-treatment dynamics.[8] Group-specific treatment effects aggregated across post-treatment periods, as defined in Equation (4), are shown in Figure 5. Overall average treatment effects across all cohorts, aggregated according to Equation (5), are reported in Table 2. Supplementary Figure B1 reports the full set of group-time ATT estimates from Equation (3).

Event-study estimates in Figure 4 provide no evidence against the parallel-trends assumption. Under both unconditional (Panel A) and conditional (Panel B) specifications, pre-treatment coefficients are close to zero and mostly statistically indistinguishable from zero across outcomes, indicating comparable pre-policy trajectories between treated and not-yet-treated areas. Following exposure, coefficients increase sharply and persistently for all three outcomes—diagnosed depression, mental health-related hospital admissions and attendances, and antidepressant prescribing—indicating immediate and sustained adverse effects associated with Universal Credit roll-out..

Figure 5 complements these dynamics by reporting cohort-specific treatment effects aggregated over post-treatment years. A clear gradient emerges across cohorts: areas first exposed earlier (2013-2015) experience larger deteriorations in mental health outcomes and greater increases in care utilisation than later-exposed cohorts. One interpretation is cumulative or 'scarring' effects

[8] Overall ATT aggregation based on event-study analysis is reported in Supplementary Table B1. Following Callaway and Sant'Anna (2021), we caution that group composition changes across periods complicate interpretation of aggregated dynamic ATT estimates, so we exclude these aggregated estimates from the main results presented here.

associated with longer exposure. However, this gradient may also reflect differences in treatment intensity, as earlier-exposed areas had more time for Universal Credit caseloads to build up. Consistent with this interpretation, the intensity specification in Section 5.2.3 reveals a dose-response relationship, whereby higher local uptake is associated with larger area-level effects. Taken together, the cohort gradient and intensity results suggest that both duration of exposure and the share of residents transitioning onto Universal Credit contribute to the observed effects. We therefore interpret larger effects in early cohorts as reflecting the joint influence of accumulated exposure and greater penetration within local areas, rather than purely time-since-treatment effects.

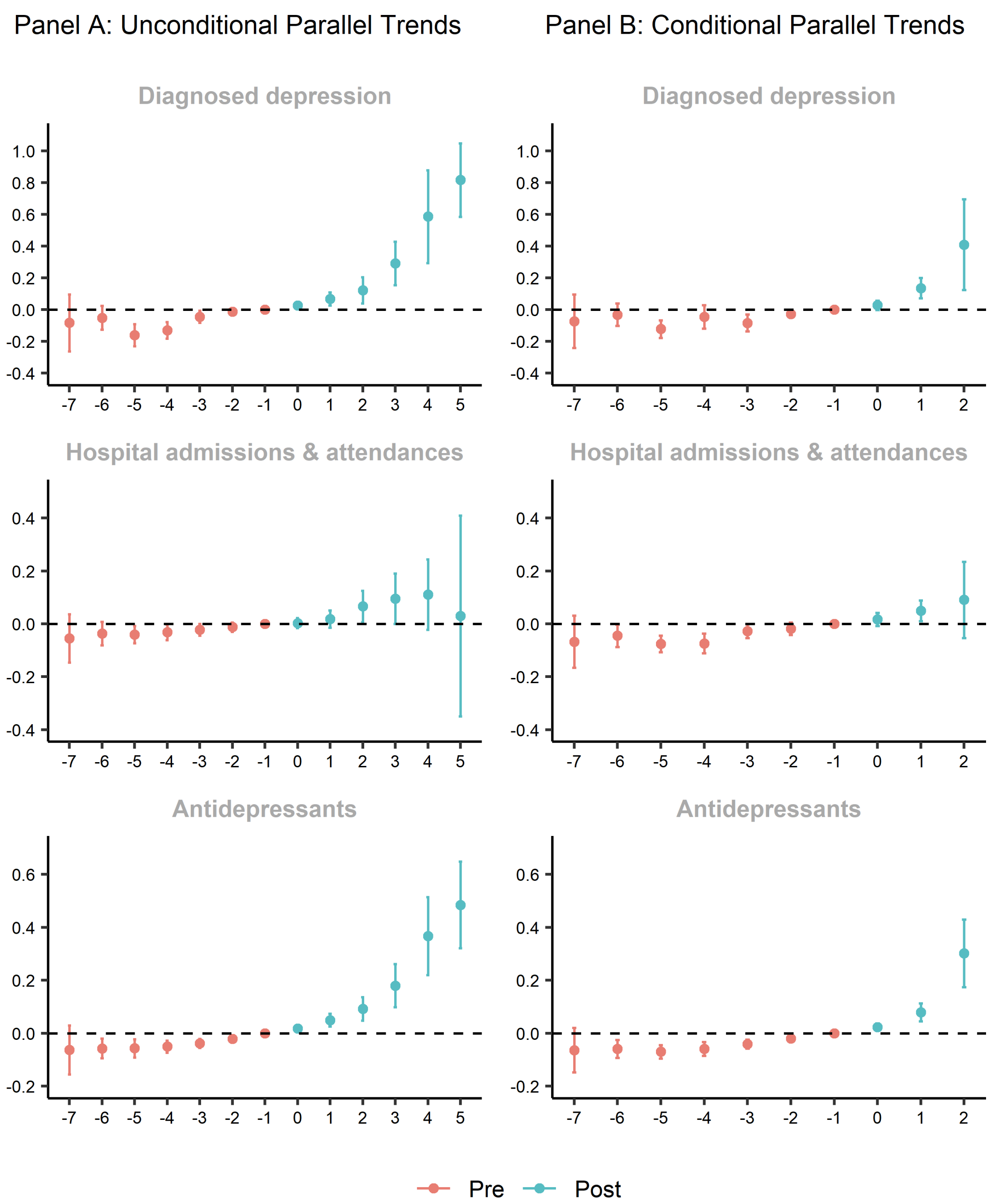
Table 2 reports the aggregated overall treatment effects for each outcome. Panel A presents unconditional estimates, while Panel B adds time-varying area covariates. Across outcomes and specifications, Universal Credit exposure is associated with statistically significant increases in mental health burden and related healthcare utilisation. In the covariate-adjusted specification, exposure increases diagnosed depression by 0.096 standard deviations, mental health-related hospital admissions and attendances by 0.034 standard deviations, and antidepressant prescribing by 0.059 standard deviations. These magnitudes are economically meaningful. In Section 5.3, we translate these effects into natural units and national aggregates, including implied numbers of additional depression cases, hospital attendances, and antidepressant prescriptions attributable to the roll-out.

A central contribution of this paper is that our estimates capture area-level effects of Universal Credit exposure, which may reflect both direct effects on Universal Credit claimants and spillover effects on non-claimants residing in exposed areas. Our administrative data do not allow us to separately identify these components, as outcomes are measured at the LSOA level rather than at the individual claimant level. As a result, we cannot directly decompose the estimated effects into those operating through claimants versus non-claimants. However, we can benchmark the magnitude of our estimates against prior studies that focus on direct effects only, typically using individual-level survey data. Translating our baseline estimates into natural units implies approximately 113,742 additional cases of diagnosed depression per year attributable to Universal Credit roll-out when both direct and spillover effects are included (Table 5). By contrast, Wickham et al. (2020) estimate around 21,760 additional cases of depression meeting clinical diagnostic thresholds attributable to Universal Credit using survey-based methods that identify effects among directly exposed individuals. Similarly, Marimpi et al. (2025) estimate approximately 27,115 additional cases meeting clinical diagnostic thresholds for common mental disorders associated with Universal Credit exposure.

The substantially larger magnitude of our area-level estimates is consistent with the presence of spillover effects operating beyond direct claimants, such as through household stress, intra-family financial strain, reduced informal care capacity, and pressures on local services. While this comparison is not a formal decomposition, it provides suggestive evidence that spillovers may be quantitatively important and that analyses restricted to direct effects may substantially understate the population-level mental health consequences of welfare reform.

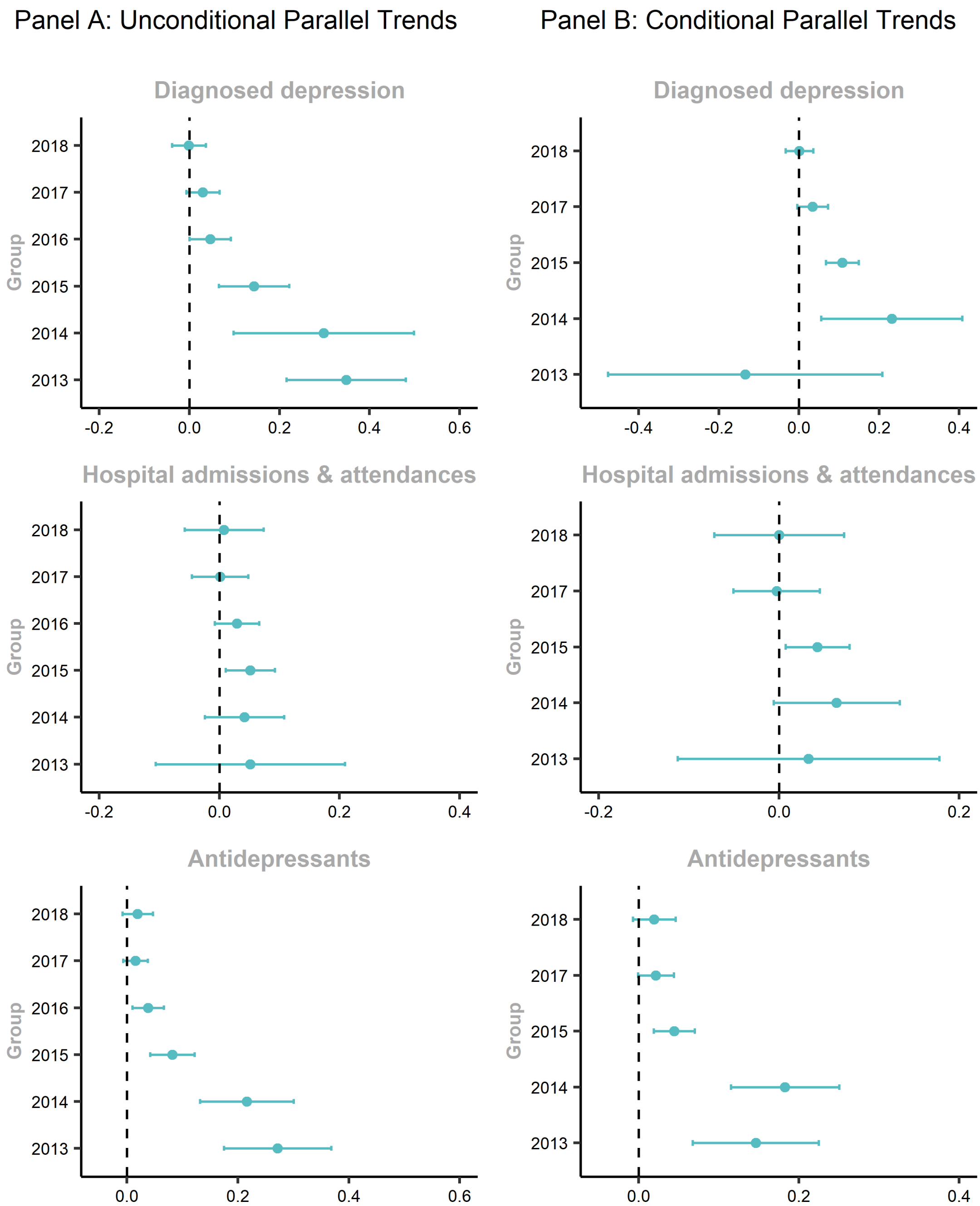
To benchmark against conventional TWFE estimation, Supplementary Table B2 reports results from Equations (1) and (2). As expected in a staggered-adoption setting with heterogeneous treatment effects, TWFE estimates are attenuated relative to our preferred CS-DiD estimates. This attenuation reflects negative weighting and contamination from comparisons involving already-treated units, reinforcing the value of cohort-specific aggregation for estimating the effects of Universal Credit roll-out.

Figure 4: **Event study of Universal Credit roll-out effect on mental health**



Notes: This figure presents event-study estimates of the Universal Credit roll-out effect on mental health outcomes, displaying the average treatment effect by length of Universal Credit exposure (measured in years from exposure). Panel A shows estimates under the unconditional parallel trends assumption (without covariates), while Panel B reports estimates conditional on local area covariates. Red dots and lines represent point estimates and simultaneous 95% confidence intervals for pre-treatment periods, estimated using the Callaway and Sant'Anna (2021) difference-in-differences (CS-DiD) estimator with the doubly robust estimation method, without anticipation of treatment effects. Blue dots and lines represent point estimates and simultaneous 95% confidence intervals for post-treatment periods, capturing dynamic effects of Universal Credit roll-out. We follow Roth (2024)'s recommendation and estimate event-study coefficients using a common 'universal' base period, implemented via the *base-period = "universal"* option in the CS-DiD estimation, so that pre- and post-treatment estimates are derived symmetrically and avoid mechanical flattening of pre-treatment coefficients. Standard errors and confidence bands are clustered at the Local Authority District (LAD) level and computed using a bootstrap procedure with 1,000 iterations. The comparison group comprises 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). The conditional model (Panel B) includes the following covariates: population size, share of working-age population (16-64), share of female population, job density, unemployment rate, share of White British population, and local authority per capita service spending on culture, education, and social care. All models include LSOA and year fixed effects. Post-treatment effects under conditional parallel trends (Panel B) are available for only two years following Universal Credit exposure due to limitations in covariate overlap in later years, as discussed in the main text. The overall summary average treatment effects (ATT) based on dynamic event-study aggregation are reported in Supplementary Table B1.

Figure 5: **Group average effect of Universal Credit roll-out on mental health**



Notes: This figure reports group-specific average effects of the Universal Credit roll-out on mental health outcomes, as described by Equation (4), estimated under the unconditional parallel trends assumption (Panel A) and conditional parallel trends assumption (Panel B). Each group on the y-axis represents LSOAs categorised by the year in which their residents first claimed Universal Credit benefits. Point estimates (dots) and simultaneous 95% confidence intervals (lines) for each group's treatment effects are computed using the CS-DiD estimator with the doubly robust estimation method, without anticipation effects. Confidence intervals are constructed using a bootstrap procedure with 1,000 iterations and are clustered at the LAD level. The comparison group consists of 'not-yet-treated' LSOAs. The conditional parallel trends model (Panel B) includes local area covariates: population size, share of working-age population (16-64), share of female population, job density, unemployment rate, share of White British population, and per capita local spending on culture, education, and social care. All models control for LSOA and year fixed effects.

Table 2: **Universal Credit roll-out overall aggregate effect on mental health: Estimates based on cohort aggregation**

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Panel A: No covariates			
ATT	0.0903***	0.0332**	0.0621***
	(0.017)	(0.012)	(0.010)
Panel B: With covariates			
ATT	0.0960***	0.0337***	0.0590***
	(0.017)	(0.010)	(0.009)
Treated cohorts	6	6	6
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
Universal Credit roll-out anticipation	NO	NO	NO
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports overall aggregate average treatment effects (ATT) of Universal Credit roll-out on mental health outcomes based on cohort aggregation, estimated using the Callaway and Sant'Anna (2021) difference-in-differences (CS-DiD) estimator. Panel A presents estimates under the unconditional parallel trends assumption (no covariates), while Panel B presents estimates conditional on local area covariates. The ATT parameters correspond to the aggregation described by Equation (5) and Figure 5, with no anticipation of Universal Credit roll-out effects. The comparison group consists of 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). Estimates are obtained using the doubly robust estimation method, and standard errors (in parentheses) are clustered at the LAD level and computed using a bootstrap procedure with 1,000 iterations. Covariates included in Panel B are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LSOA and year fixed effects. *** $p<0.01$, ** $p<0.05$, and * $p<0.1$.

5.2 Robustness checks and sensitivity analysis

We conduct four sets of robustness checks. First, we assess sensitivity to spatial aggregation by collapsing outcomes and treatment to the LAD level and re-estimating the model using only variation from the LAD roll-out schedule. Second, we relax the no-anticipation assumption to allow for pre-implementation behavioural adjustments. Third, we replace the binary exposure indicator with a continuous measure of treatment intensity based on local uptake. Fourth, we re-estimate treatment effects using alternative estimators designed for staggered adoption. Across all checks, results remain stable in sign and magnitude, supporting the internal validity of the baseline findings.

5.2.1 Estimation at the LAD level

To assess whether our results are driven by within-LAD variation or by the timing of the roll-out across LADs, we collapse outcomes and treatment to the LAD level and re-estimate the CS-DiD model using cohort aggregation. Table 3 reports the resulting estimates.

The LAD-level results closely mirror the baseline LSOA-level findings. Under the covariate-adjusted specification, Universal Credit exposure increases diagnosed depression by 0.128 SD, mental health-related hospital admissions and attendances by 0.093 SD, and antidepressant prescribing by 0.086 SD. These magnitudes are comparable to, and in some cases slightly larger than, the corresponding LSOA-level estimates, and all effects are statistically significant at conventional levels.

The persistence of sizable effects when collapsing to the LAD level indicates that the estimated impacts are not driven by fine-grained spatial variation within LADs, but instead reflect policy-relevant differences in exposure timing across LADs. This finding directly addresses concerns about reliance on sub-LAD variation and confirms that the main conclusions are robust when identification relies solely on the administrative roll-out schedule.

5.2.2 Allowing for one year anticipation of Universal Credit roll-out

Households may adjust behaviour in advance of transitioning to Universal Credit, for example in response to information about upcoming policy changes. We therefore re-estimate our model allowing for one year of anticipation, following Callaway and Sant'Anna (2021). Results of this anticipation-adjusted specification are reported in Table 4, with Panel A showing unconditional and Panel B showing covariate-adjusted estimates.

Coefficients remain positive, statistically significant, and generally similar to—or larger than—those in the baseline specification. Under conditional parallel trends, exposure increases diagnosed depression by 0.132 SD, antidepressant prescribing by 0.088 SD, and mental health-related hospital admissions and attendances by 0.051 SD. These patterns suggest that anticipatory responses, such as heightened financial strain or uncertainty prior to benefit transition, may form part of the overall effect of Universal Credit roll-out. Supplementary Figures B2-B4 and Table B3 report the corresponding event-study dynamics and group-time ATT estimates, which are consistent with these conclusions.

5.2.3 Treatment intensity

To examine how effects scale with the degree of local exposure, we replace the binary treatment indicator with a continuous measure equal to the proportion of residents in each LSOA claiming

Universal Credit in each year. We estimate this dose-response relationship using OLS with LSOA and year fixed effects, including the full set of time-varying covariates from the baseline specification and clustering standard errors at the LAD level. Table 5 summarises the results.

Estimated coefficients on treatment intensity are positive and statistically significant across all outcomes. A one-percentage-point increase in Universal Credit uptake raises diagnosed depression by 0.057 SD, hospital admissions and attendances by 0.032 SD, and antidepressant prescribing by 0.037 SD. These findings confirm a clear dose-response relationship at the small-area level, consistent with the cohort gradient documented in Section 5.1. Together, the intensity and cohort analyses indicate that both the duration of exposure and the extent of local penetration contribute to the overall impact of Universal Credit on mental health and healthcare utilisation.

5.2.4 Alternative staggered DiD specifications

Finally, we assess robustness to alternative estimators. Re-estimation using the approaches proposed by Borusyak et al. (2024), Sun and Abraham (2021), and Roth and Sant'Anna (2023) yield effects that are positive, statistically significant, and closely aligned in magnitude with our preferred CS-DiD estimates. Full results are reported in Supplementary Material Section B and show strong concordance across methods.

Table 3: **Universal Credit roll-out overall aggregate effect on mental health: Estimates at the LAD level and based on cohort aggregation**

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Panel A: No covariates			
ATT	0.1089**	0.0828***	0.0663***
	(0.052)	(0.028)	(0.024)
Panel B: With covariates			
ATT	0.1282***	0.0933***	0.086***
	(0.050)	(0.023)	(0.019)
Treated cohorts	6	6	6
Number of LADs	309	309	309
Observations	2,472	2,472	2,472
Universal Credit roll-out anticipation	NO	NO	NO
LAD FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table re-estimates the models in Table 2 by by collapsing outcomes and treatment to the LAD level. The table reports overall aggregate ATT of Universal Credit roll-out on mental health outcomes based on cohort aggregation, estimated using the CS-DiD estimator. Panel A presents estimates under the unconditional parallel trends assumption (no covariates), while Panel B presents estimates conditional on local area covariates. The ATT parameters correspond to the aggregation described by Equation (5) and Figure 5. The comparison group consists of 'not-yet-treated' LADs. Estimates are obtained using the doubly robust estimation method, and standard errors (in parentheses) are clustered at the LAD level and computed using a bootstrap procedure with 1,000 iterations. Covariates included in Panel B are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LAD and year fixed effects. *** p<0.01, ** p<0.05, and * p<0.1.

Table 4: **Universal Credit roll-out overall aggregate effect on mental health with one year anticipation: Estimates based on cohort aggregation**

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Panel A: No covariates			
ATT	0.1338***	0.0553***	0.1075***
	(0.032)	(0.018)	(0.018)
Panel B: With covariates			
ATT	0.1315***	0.0505***	0.0878***
	(0.028)	(0.015)	(0.014)
Treated cohorts	6	6	6
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
Universal Credit roll-out anticipation	YES	YES	YES
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports overall aggregate ATT of Universal Credit roll-out on mental health outcomes based on cohort aggregation, estimated using the CS-DiD estimator. Panel A presents estimates under the unconditional parallel trends assumption (no covariates), while Panel B presents estimates conditional on local area covariates. The ATT parameters correspond to the aggregation described by Equation (5) and Figure 5, with one year anticipation of Universal Credit roll-out. The comparison group consists of 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). Estimates are obtained using the doubly robust estimation method, and standard errors (in parentheses) are clustered at the LAD level and computed using a bootstrap procedure with 1,000 iterations. Covariates included in Panel B are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LSOA and year fixed effects. *** p<0.01, ** p<0.05, and * p<0.1.

Table 5: **Effect of Universal Credit roll-out on mental health with treatment intensity**

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Proportion on Universal Credit	0.0568***	0.0324***	0.0366***
	(0.013)	(0.009)	(0.010)
R2	0.8674	0.8647	0.9657
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports the effect of Universal Credit roll-out on mental health outcomes based on based on treatment intensity, using OLS. Robust standard errors in parentheses are clustered at the LAD level, the administrative unit of Universal Credit roll-out. *** p<0.01, ** p<0.05, and * p<0.1. We measure treatment intensity by the proportion of the LSOA population claiming Universal Credit each year. Covariates included in each model are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LSOA and year fixed effects.

5.3 Interpreting population mental health impacts of Universal Credit roll-out: Indicative cost estimates

The deterioration in mental health associated with Universal Credit roll-out has implications that extend beyond individual wellbeing, generating substantial pressures on publicly funded health services and broader social welfare. To contextualise the magnitude of these impacts, we provide indicative valuations of quality-of-life losses and direct healthcare costs, based on the baseline estimates in Table 2, Panel B. These valuations are intended to be illustrative rather than exhaustive; detailed methods and assumptions are described in Supplementary Section C.

Table 6 translates the estimated standard-deviation effects into natural units and monetary terms. Column (1) reproduces the baseline coefficients from Table 2; Column (2) expresses these effects in natural units; Column (3) aggregates the implied annual changes across England during the roll-out period (2013-2018), based on observed LSOA-level population changes; Columns (4) and (5) report unit costs and corresponding national cost estimates, all expressed in 2022 prices.

Following Cardoso and McHayle (2024), we value the human cost per additional case of diagnosed depression using a blended approach. This combines QALY-based valuations for younger

(18-19 years) and older adults ($\geq$65 years) with WELLBY-based valuations for the working-age population (20-64 years), yielding a population-weighted value of approximately £23,549 per additional case attributable to Universal Credit exposure. Applying this valuation to an implied 113,742 additional cases of diagnosed depression per year results in an estimated annual human cost of £2.68 billion.

In addition to quality-of-life losses, the roll-out is associated with increased healthcare utilisation. Antidepressant prescribing rises by an estimated 1.29 million additional prescriptions per year, implying direct NHS costs of approximately £8.76 million, using the 2022 average net ingredient cost per prescription (£6.77). Mental health-related hospital admissions and A&E attendances increase by around 29,993 episodes annually, corresponding to NHS costs of approximately £155.3 million, based on a weighted unit cost of £5,178 that reflects the observed mix of admissions and emergency presentations. Confidence intervals for these cost aggregates can be obtained by applying the same transformations to the corresponding effect-size intervals; we report point estimates for parsimony. Further details are presented in Supplementary Section C.

Summing quality-of-life losses and direct healthcare costs yields an indicative annual burden of approximately £2.84 billion attributable to Universal Credit roll-out. These figures should be interpreted as conservative lower-bound estimates, as they exclude productivity losses, informal care burdens, impacts on carers and households, and longer-term consequences for labour-market attachment and human capital. Moreover, because our empirical strategy captures area-level effects, these cost estimates reflect the combined influence of direct impacts on Universal Credit claimants and spillover effects on non-claimants residing in exposed areas. As discussed in Section 5.1, comparisons with prior studies that estimate only direct effects suggest that spillovers may account for a substantial share of the total population burden.

These indicative valuations underscore that welfare reforms designed to improve administrative efficiency and work incentives can entail sizable unintended mental-health costs at the population level. Accounting for such costs is essential for a comprehensive assessment of the social and fiscal consequences of large-scale welfare policy changes.

Table 6: **Implied cost of the impact of Universal Credit roll-out on adult population mental health, 2022 prices**

	(1)	(2)	(3)	(4)	(5)
Outcome	Baseline impact coefficient (in SD)	Impact coefficient in natural units	Implied total change in outcome (#)	Average monetary unit cost	Average national cost
Depression rate (% point change)	0.0960	0.2556	113,742	£23,549	£2,678,466,940
Antidepressants (# of items)	0.0590	0.0291	1,294,950	£6.77	£8,762,088
Hospital admission & attendance (#)	0.0337	0.000674	29,993	£5,178	£155,291,762
Total					**£2,842,520,789**

Notes: This table presents indicative estimates of the cost—in terms of human cost and healthcare cost—associated with the adverse mental health impacts of Universal Credit roll-out in England, based on our baseline estimates reported in Table 2, Panel B. Column (1) repeats the estimated treatment effects expressed in SD units. Column (2) converts these effects into natural units for each outcome. For diagnosed depression and antidepressant prescribing, we re-estimate the model using outcomes expressed in their original units (see Supplementary Table B4). For hospital admissions and attendances, where data in natural units were unavailable, we approximate the implied effect in terms of admission and attendance rate per capita using the standard deviation of the original per capita admission and attendance rate. Column (3) reports the implied aggregate annual increase in the relevant mental health outcome across the adult population, based on observed changes at the LSOA level and the national adult population from 2013 to 2018. Column (4) presents the monetary unit cost associated with each outcome. For depression, the unit cost reflects the wellbeing-adjusted monetary valuation of reduced quality of life, drawing on Cardoso and McHayle (2024). This combines QALY-based estimates for younger (<20) and older (65+) adults with WELLBY-based valuations for the working-age population. For antidepressant prescribing, we use the average net ingredient cost per prescription item in 2022 prices from NHS Digital. For hospital admissions and attendances, we apply a weighted-average NHS tariff combining admissions and A&E costs from 2017-2019 Trust-level reference data, uprated to 2022 prices using the HM Treasury GDP deflator. Column (5) reports the total implied annual cost to the health system and society, aggregating the product of the estimated change in each outcome and its corresponding unit value. Full methodological details are provided in Section C of the Supplementary Material.

6 Conclusion

This paper provides new evidence on the mental health and healthcare utilisation consequences of Universal Credit, one of the most far-reaching welfare reforms in recent decades. Exploiting the staggered and plausibly exogenous timing of roll-out across England, we identify causal effects on clinically recorded depression, mental health-related hospital admissions and attendances, and antidepressant prescribing using comprehensive administrative data at small-area level. This approach complements and extends prior survey-based work by capturing population-level impacts and potential spillovers within communities.

Across specifications and a wide range of sensitivity analyses—including allowing for anticipation, modelling treatment intensity, collapsing the data to the LAD level, and applying alternative estimators for staggered adoption—we consistently find that exposure to Universal Credit worsened population mental health and increased related healthcare utilisation. Effects emerge immediately following exposure and persist over time. They are larger in areas exposed earlier and in places where a greater share of residents transitioned onto Universal Credit. Taken together with the clear dose-response relationship, this pattern is best interpreted as the joint consequence of longer exposure duration and deeper within-area penetration, rather than a pure time-since-treatment effect.

Translating these impacts into natural units and monetary terms indicates that the population burden is substantial. Combining quality-of-life losses associated with additional cases of diagnosed depression and direct public healthcare costs implies an indicative annual cost of approximately £2.84 billion. Because these estimates exclude productivity losses, informal care burdens, and other indirect costs, they should be viewed as conservative lower-bound estimates.

These findings have clear policy implications. Welfare reforms designed to simplify administration and strengthen work incentives can generate sizable mental-health externalities for vulnerable households and the communities in which they live, with consequent pressures on public health systems. Policy design and implementation should therefore incorporate safeguards that mitigate predictable stressors, such as payment delays, deductions, and administrative frictions, and integrate targeted mental-health support in areas with earlier or more intensive exposure. Our results also underscore the value of monitoring impacts at the community level, where spillovers within households and neighbourhoods are likely to be most salient.

A natural question is whether these conclusions remain relevant given subsequent changes to Universal Credit delivery after our study period ends in 2018. We deliberately focus on the pre-pandemic period to avoid COVID-related confounding and to evaluate the natural and voluntary migration phases of the roll-out. While some operational features have evolved since then, the mechanisms we discuss—financial strain around the payment cycle, reduced benefit generosity, conditionality and sanctions risk, deductions, and digital access burdens—are structural and likely to persist unless directly addressed. The cohort gradient and intensity results suggest that adverse effects can accumulate and propagate within communities; consequently, even where later administrative adjustments improved delivery, there remains a risk of lasting mental-health scarring in areas with early or widespread exposure. The policy priority is therefore twofold: learning from the early roll-out to avoid repeating stress-inducing design features as managed migration progresses, and targeting remedial support to areas and groups with deeper or earlier exposure where scarring is most likely.

Our study has limitations. Effects are estimated at the small-area level and therefore capture the combined influence of direct impacts on Universal Credit claimants and spillover effects on non-claimants; the available data do not allow a decomposition of these components. Outcomes are observed through administrative records of diagnosis and treatment and may understate latent or untreated morbidity. The analysis cannot identify specific behavioural or institutional channels, nor examine heterogeneity by household-level socioeconomic characteristics. Finally, Universal

Credit is a bundled policy: while exposure is associated with adverse mental-health outcomes on average, we cannot disentangle the contributions of individual design features such as payment timing, reduced benefit generosity, deductions, conditionality, or digital requirements. Future research linking individual-level administrative records would enable identification of mechanisms, subgroup heterogeneity, persistence over the longer term, and the extent to which subsequent delivery changes attenuated these effects.

In sum, the roll-out of Universal Credit was associated with materially worse population mental health and increased public healthcare utilisation, with effects that scale with exposure duration and local caseload. By placing these impacts on a common economic footing, we show that the mental-health costs are large and policy-relevant. Evaluations of welfare reform should therefore extend beyond expected employment gains to incorporate health and public health system consequences, and current implementation should be adapted to avoid a recurrence of predictable administrative and financial stressors that have been linked to adverse mental-health outcomes.

Supplementary Material

Additional supporting material for this paper:

Appendix A: Universal Credit implementation phases and key policy changes.

Appendix B: Detailed additional results and robustness checks.

Appendix C: Details of cost estimation.

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Online Supplementary Material

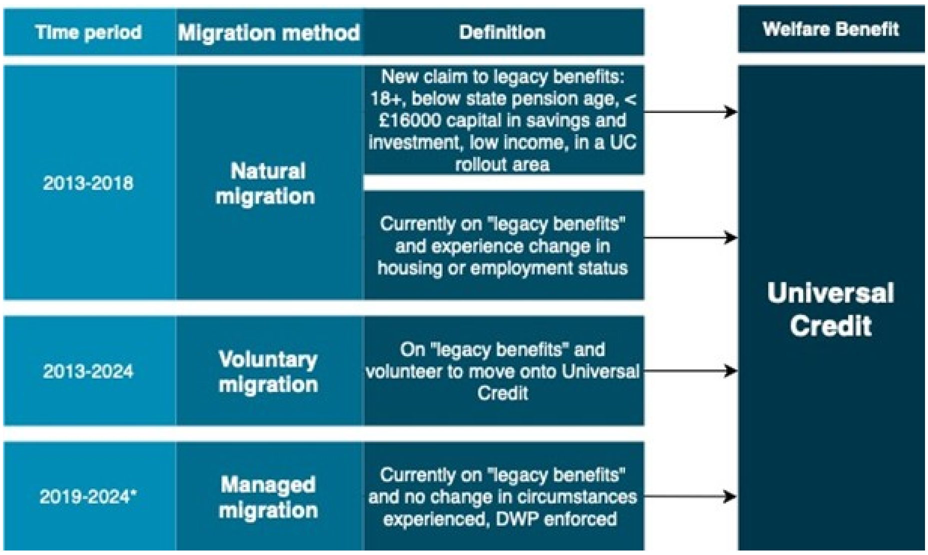
The health and economic costs of welfare reform: a quasi-experimental evaluation of the roll-out of Universal Credit in England

This Supplementary Material contains the following sections. Section A presents additional details of the phases of Universal Credit (UC) implementation and key policy changes over the roll-out period. Section B reports additional results and robustness checks. Section C provides details of the economic valuation, detailing the conversion from standard-deviation effects to natural units, aggregation to national totals, and the unit-cost used to construct indicative monetary impacts.

Unless stated otherwise, confidence intervals are 95% and standard errors are clustered at the Local Authority District (LAD) level, consistent with the main text. All outcomes are defined at Lower-layer Super Output Area (LSOA) level; any differences between numbers reported here and in the main article reflect rounding. Data sources, definitions, and citations correspond to those listed in the main article's References.

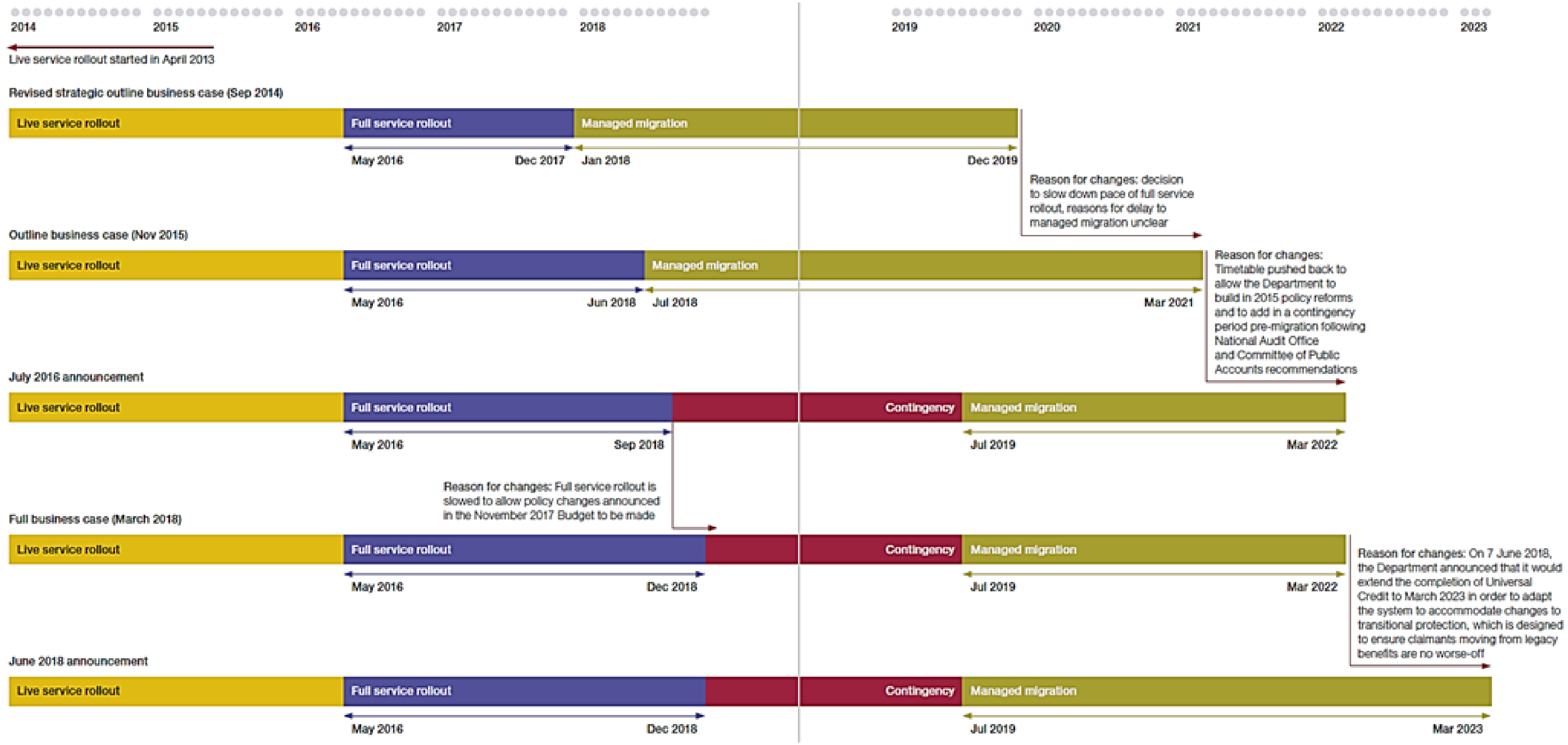
A Universal Credit implementation phases and key policy changes

Figure A1: Timeline of Implementation of Universal Credit between 2013 and 2024



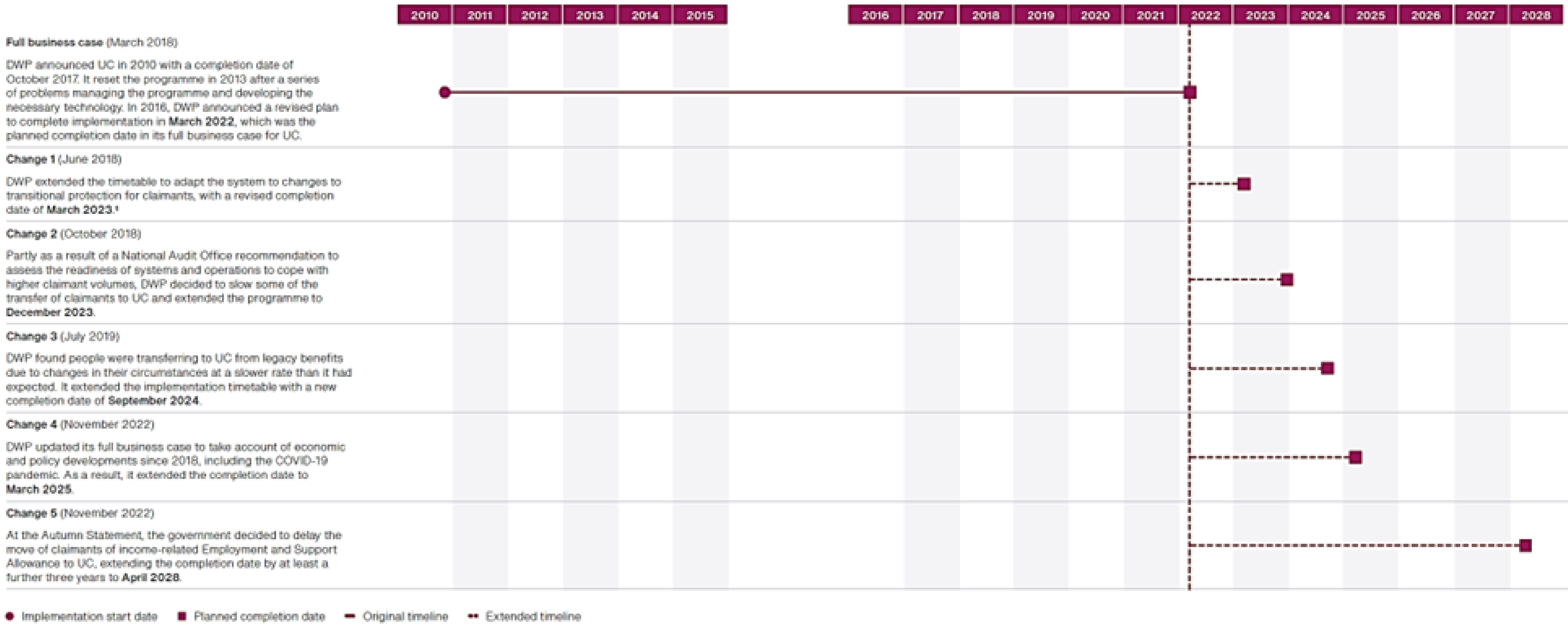
Note: *Implementation of managed migration paused in 2020 due to the COVID-19 pandemic. The timetable has since been revised several times; notably, the move of income-related Employment and Support Allowance claimants to UC has been deferred, extending completion to at least April 2028 (see Figure A3). Source: Marimpi et al. (2025).

Figure A2: The original timetable for Universal Credit with implementation phases



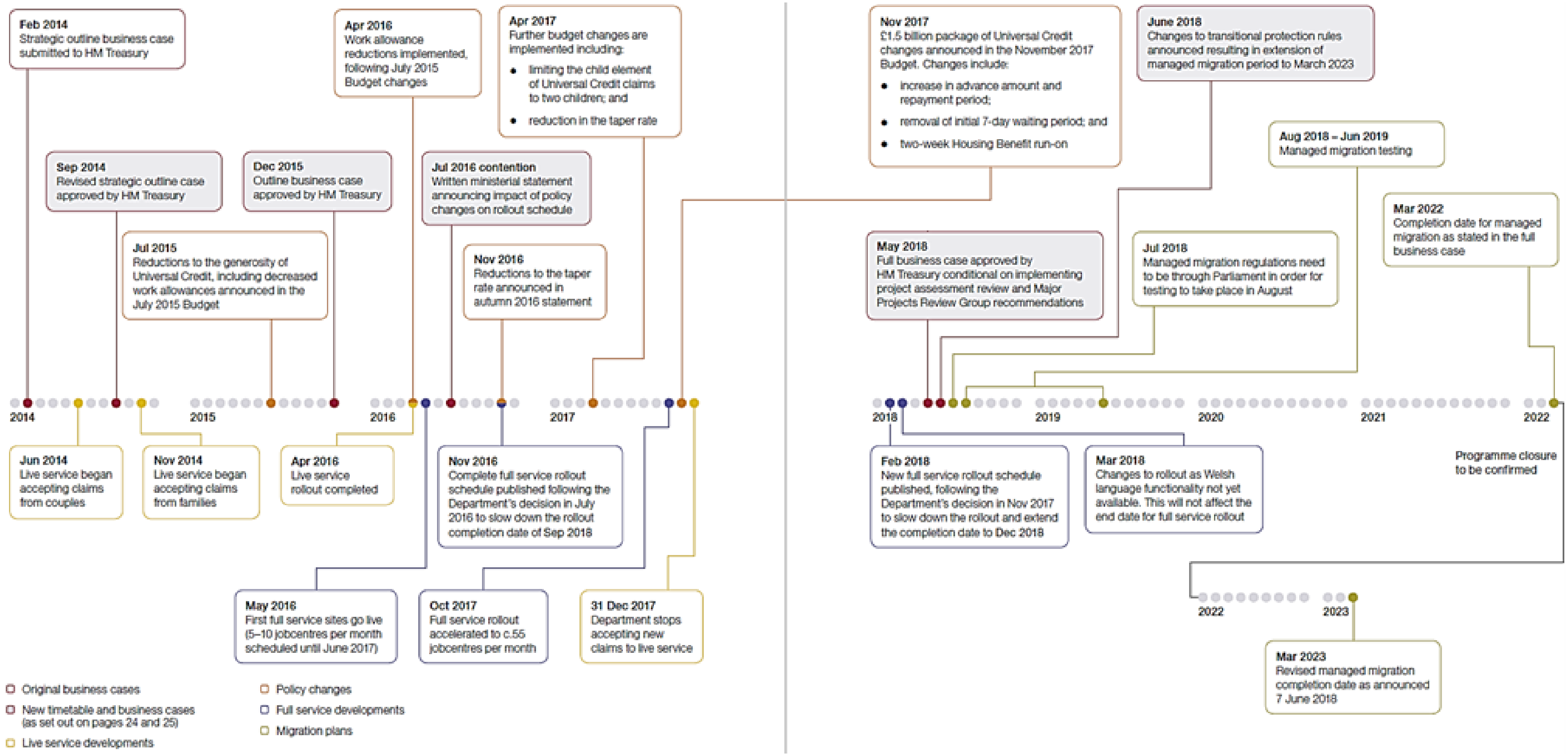
Note: Natural migration and voluntary migration cover the live service and full service roll-out as well as the contingency periods. Source: National Audit Office (2018).

Figure A3: Changes to the timetable for implementing Universal Credit since 2018



Note: [1] DWP provides financial support to claimants it moves under the managed migration process to make sure they are not worse off on UC at the point of moving. This is known as transitional protection. Source: National Audit Office (2024).

Figure A4: The 2018 timetable for Universal Credit with some key policy changes to its features



Source: National Audit Office (2018).

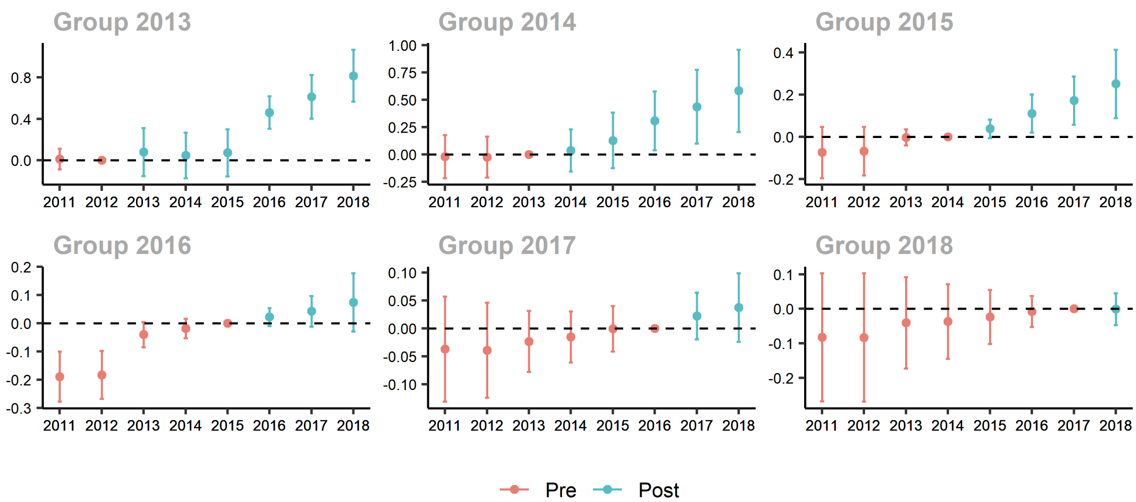
B Additional results and robustness checks

To address potential robustness concerns over the findings discussed in the main paper, we performed a large set of additional sensitivity analyses on the baseline results presented in Table 2. Figures B1-B4 and Tables B1-B6 report these additional results and robustness checks.

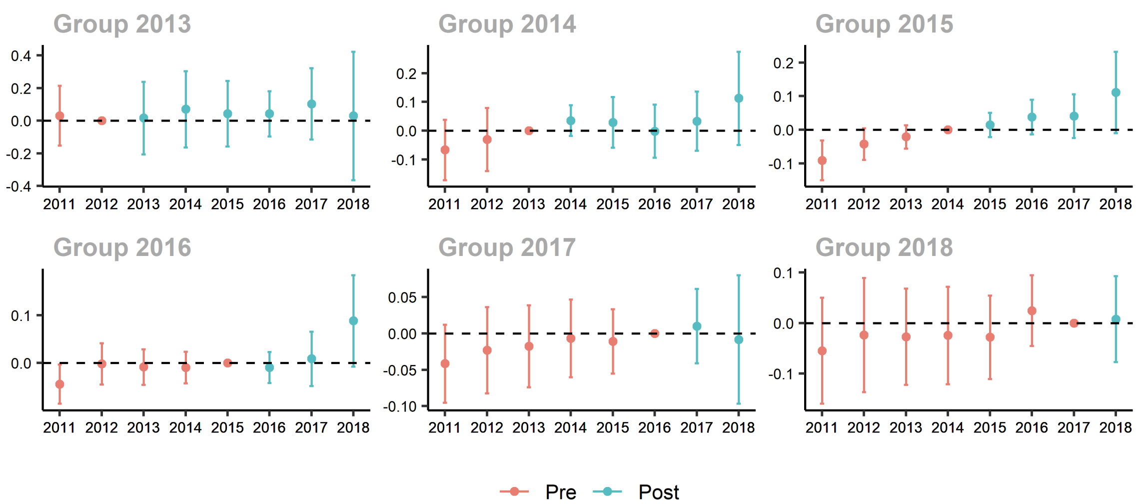
B.1 Baseline results

Figure B1: Group-time average treatment effects of Universal Credit roll-out on mental health

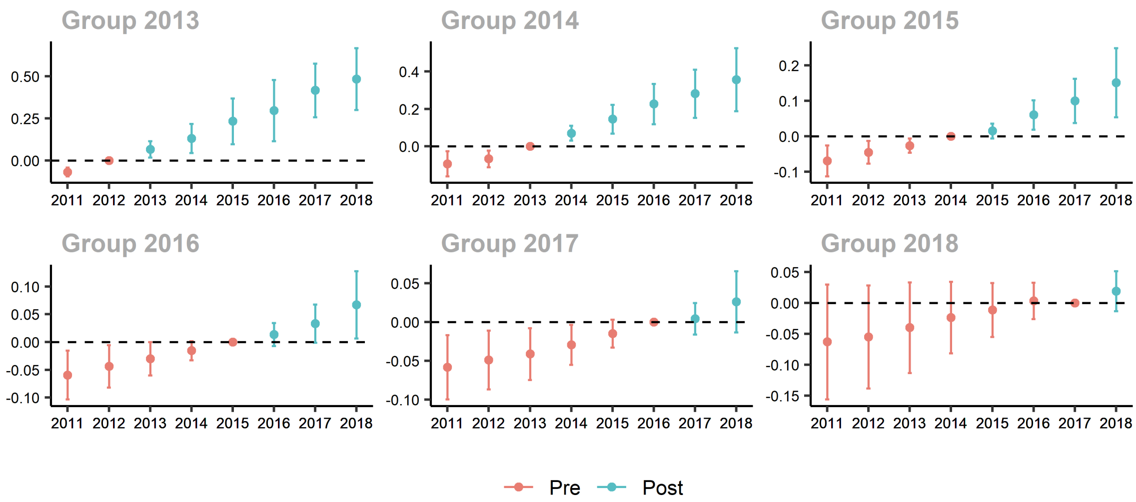
Panel A: Diagnosed depression



Panel B: Hospital admissions and attendances



Panel C: Antidepressants



Notes: This figure presents the group-time average treatment effects (ATT) of UC roll-out on mental health outcomes (based on Equation (3)), estimated under the unconditional parallel trends assumption with no anticipation effects. Red dots represent point estimates with simultaneous 95% confidence intervals for pre-treatment periods, computed using the Callaway and Sant'Anna (2021) difference-in-differences (CS-DiD) estimator with the doubly robust estimation method, and clustered at the Local Authority District (LAD) level. Blue dots represent point estimates and simultaneous 95% confidence intervals for post-treatment periods, capturing the dynamic effects of UC roll-out. We follow Roth (2024)'s recommendation and estimate event-study coefficients using a common 'universal' base period, implemented via the *base-period = "universal"* option in the CS-DiD estimation, so that pre- and post-treatment estimates are derived symmetrically and avoid mechanical flattening of pre-treatment coefficients. Confidence intervals are constructed using a bootstrap procedure with 1,000 iterations and clustered at the LAD level. For each outcome shown in Panels A-E, the top row reports estimates for LSOAs whose residents first claimed UC benefits in earlier roll-out years (2013, 2014, or 2015), while the bottom row reports estimates for LSOAs with later roll-out years (2016, 2017, or 2018). The comparison group comprises 'not-yet-treated' LSOAs. All models include LSOA and year fixed effects.

Table B1: Universal Credit roll-out overall aggregate effect on mental health: Estimates based on event-study/dynamic aggregation

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Panel A: No covariates			
ATT	0.3190***	0.0538*	0.1984***
	(0.040)	(0.029)	(0.023)
Panel B: With covariates			
ATT	0.1917***	0.0524***	0.1351***
	(0.042)	(0.020)	(0.020)
Treated cohorts	6	6	6
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
Universal Credit roll-out anticipation	NO	NO	NO
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports overall aggregate average treatment effects (ATT) of UC roll-out on mental health outcomes based on event-study/dynamic aggregation, estimated using the CS-DiD estimator. Panel A presents estimates under the unconditional parallel trends assumption (no covariates), while Panel B presents estimates conditional on local area covariates. The ATT parameters correspond to the aggregation of the event study estimates from Figure 4 of the main paper, with no anticipation of UC roll-out effects. The comparison group consists of 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). Estimates are obtained using the doubly robust estimation method, and standard errors (in parentheses) are clustered at the Local Authority District (LAD) level and computed using a bootstrap procedure with 1,000 iterations. Covariates included in Panel B are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LSOA and year fixed effects. *** $p<0.01$, ** $p<0.05$, and * $p<0.1$.

Table B2: Universal Credit roll-out overall aggregate effect on mental health: Estimates based on TWFE models

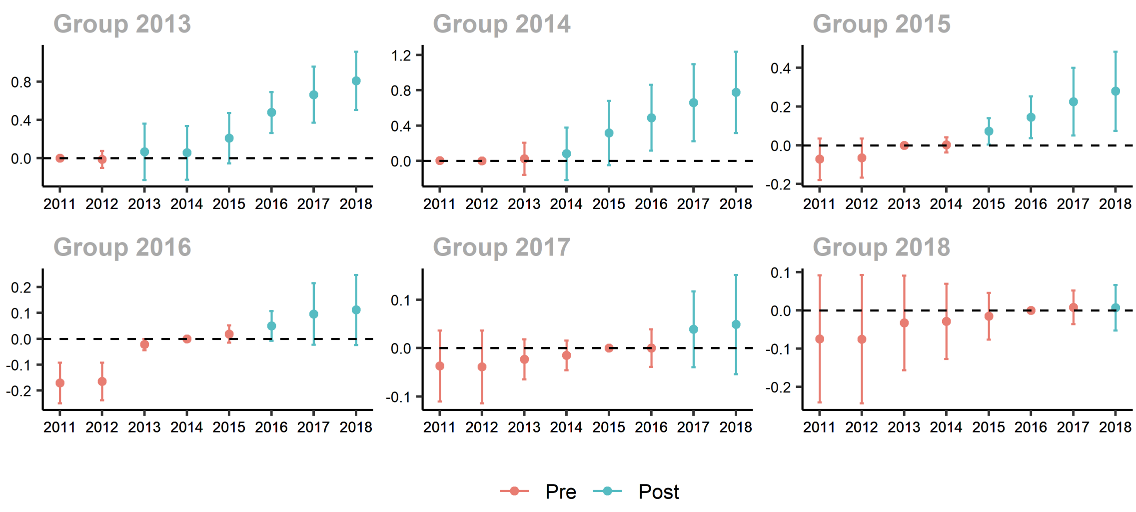
	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Panel A: No covariates			
ATT	0.0299	0.0303***	0.0374***
	(0.020)	(0.010)	(0.007)
R^2	0.8642	0.8631	0.9632
Panel B: With covariates			
ATT	0.0361*	0.0332***	0.0483***
	(0.018)	(0.010)	(0.008)
R^2	0.8660	0.8642	0.9652
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports overall ATT of UC roll-out on mental health outcomes, estimated using the two-way fixed effects (TWFE) estimator based on the model in Equation (1). Panel A presents estimates under the unconditional parallel trends assumption (no covariates), while Panel B presents estimates conditional on local area covariates. Robust standard errors in parentheses are clustered at the LAD level. *** p<0.01, ** p<0.05, and * p<0.1. Covariates included in Panel B are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LSOA and year fixed effects.

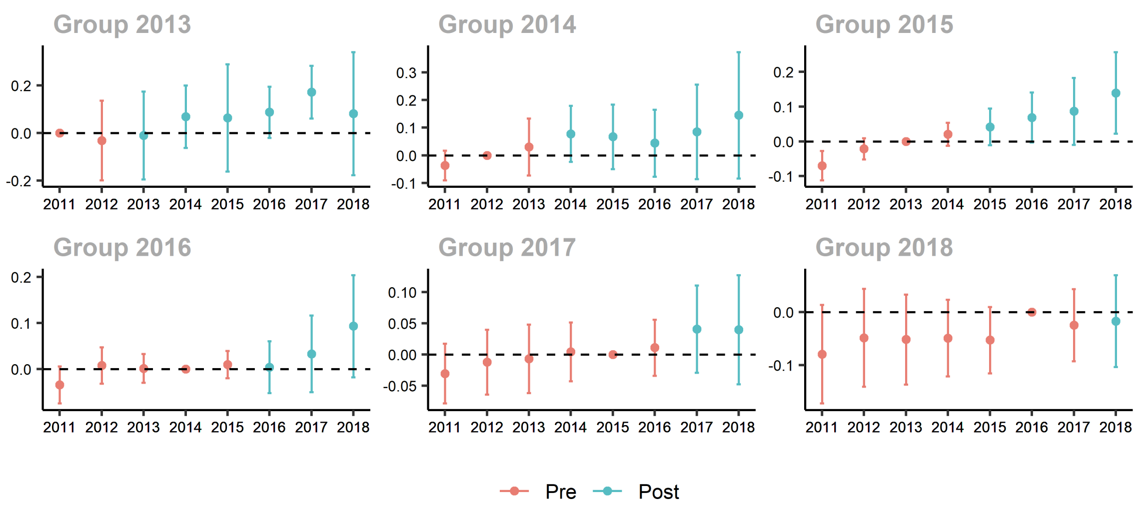
B.2 Robustness checks and sensitivity analysis

Figure B2: Group-time average treatment effects of Universal Credit roll-out on mental health, with one year UC anticipation

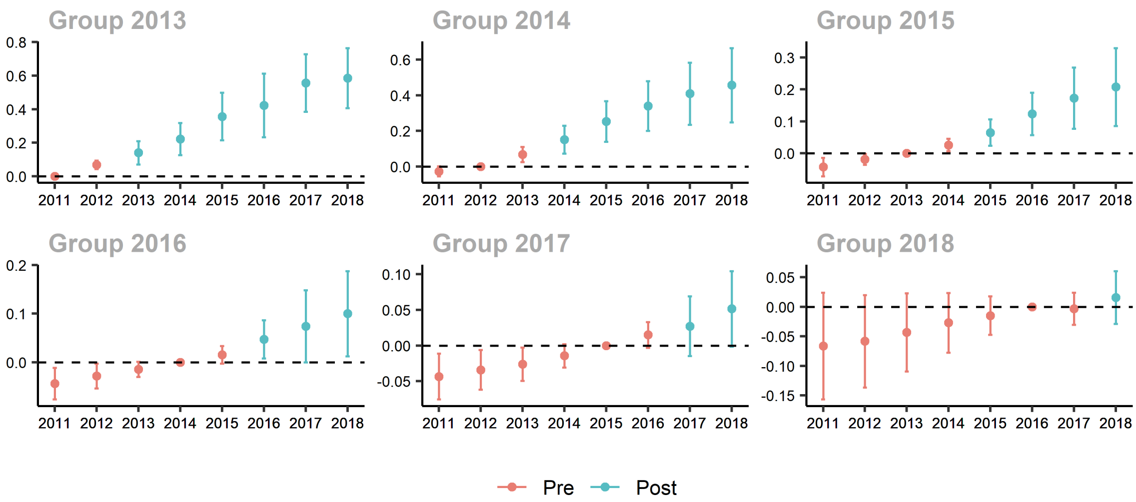
Panel A: Diagnosed depression



Panel B: Hospital admissions and attendances



Panel C: Antidepressants



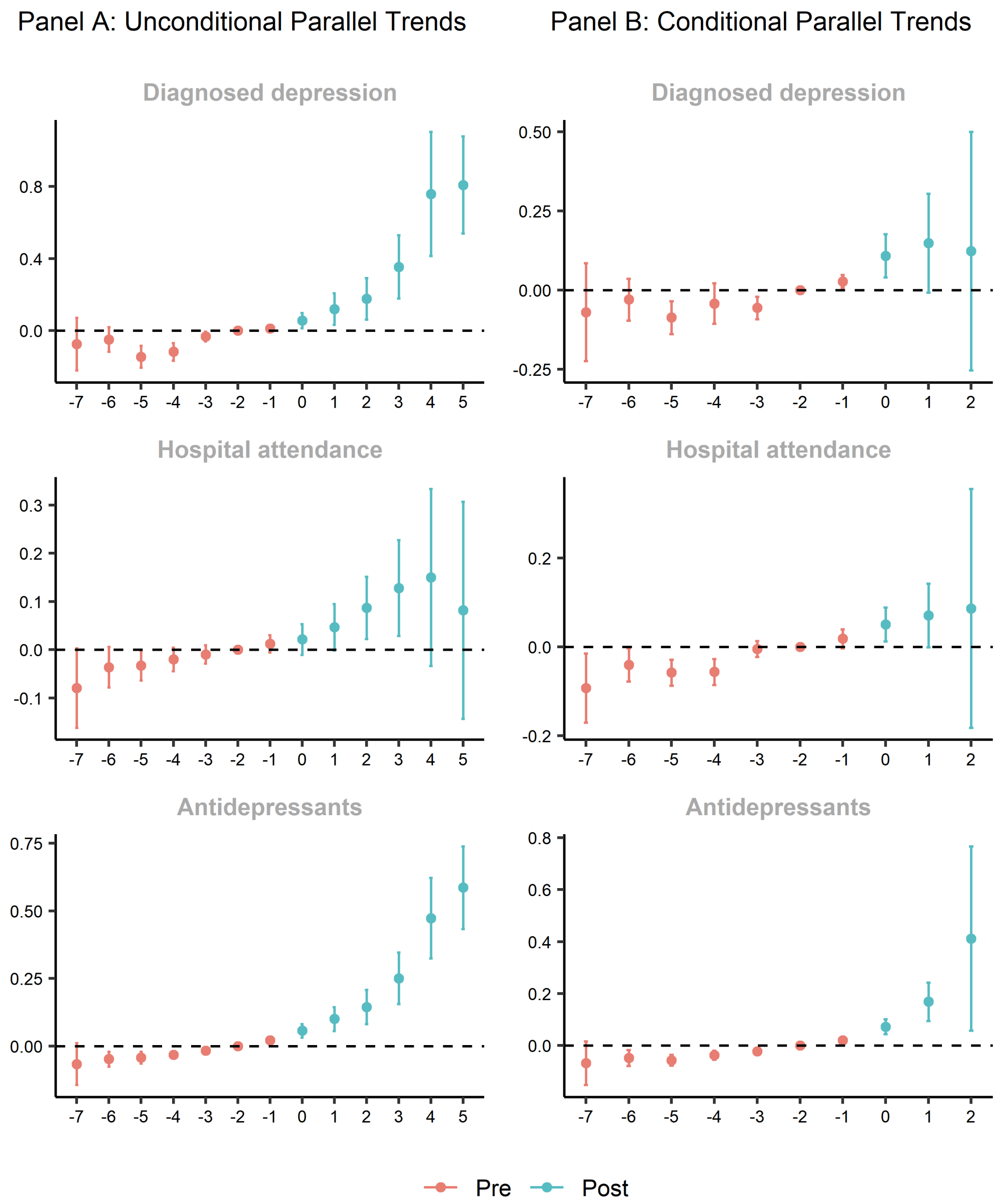
Notes: This figure presents the group-time average treatment effects (ATT) of UC roll-out on mental health outcomes (based on Equation (3)), estimated under the unconditional parallel trends assumption with one year anticipation. Red dots represent point estimates with simultaneous 95% confidence intervals for pre-treatment periods, computed using the CS-DiD estimator with the doubly robust estimation method, and clustered at the Local Authority District (LAD) level. Blue dots represent point estimates and simultaneous 95% confidence intervals for post-treatment periods, capturing the dynamic effects of UC roll-out. We follow Roth (2024)'s recommendation and estimate event-study coefficients using a common 'universal' base period, implemented via the *base-period* = *"universal"* option in the CS-DiD estimation, so that pre- and post-treatment estimates are derived symmetrically and avoid mechanical flattening of pre-treatment coefficients. Confidence intervals are constructed using a bootstrap procedure with 1,000 iterations and clustered at the LAD level. For each outcome shown in Panels A-E, the top row reports estimates for LSOAs whose residents first claimed UC benefits in earlier roll-out years (2013, 2014, or 2015), while the bottom row reports estimates for LSOAs with later roll-out years (2016, 2017, or 2018). The comparison group comprises 'not-yet-treated' LSOAs. All models include LSOA and year fixed effects.

Table B3: Universal Credit roll-out overall aggregate effect on mental health with one year anticipation: Estimates based on event-study/dynamic aggregation

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Panel A: No covariates			
ATT	0.3791***	0.0859***	0.2687***
	(0.050)	(0.028)	(0.030)
Panel B: With covariates			
ATT	0.1264**	0.0693**	0.2176***
	(0.058)	(0.036)	(0.036)
Treated cohorts	6	6	6
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
Universal Credit roll-out anticipation	YES	YES	YES
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

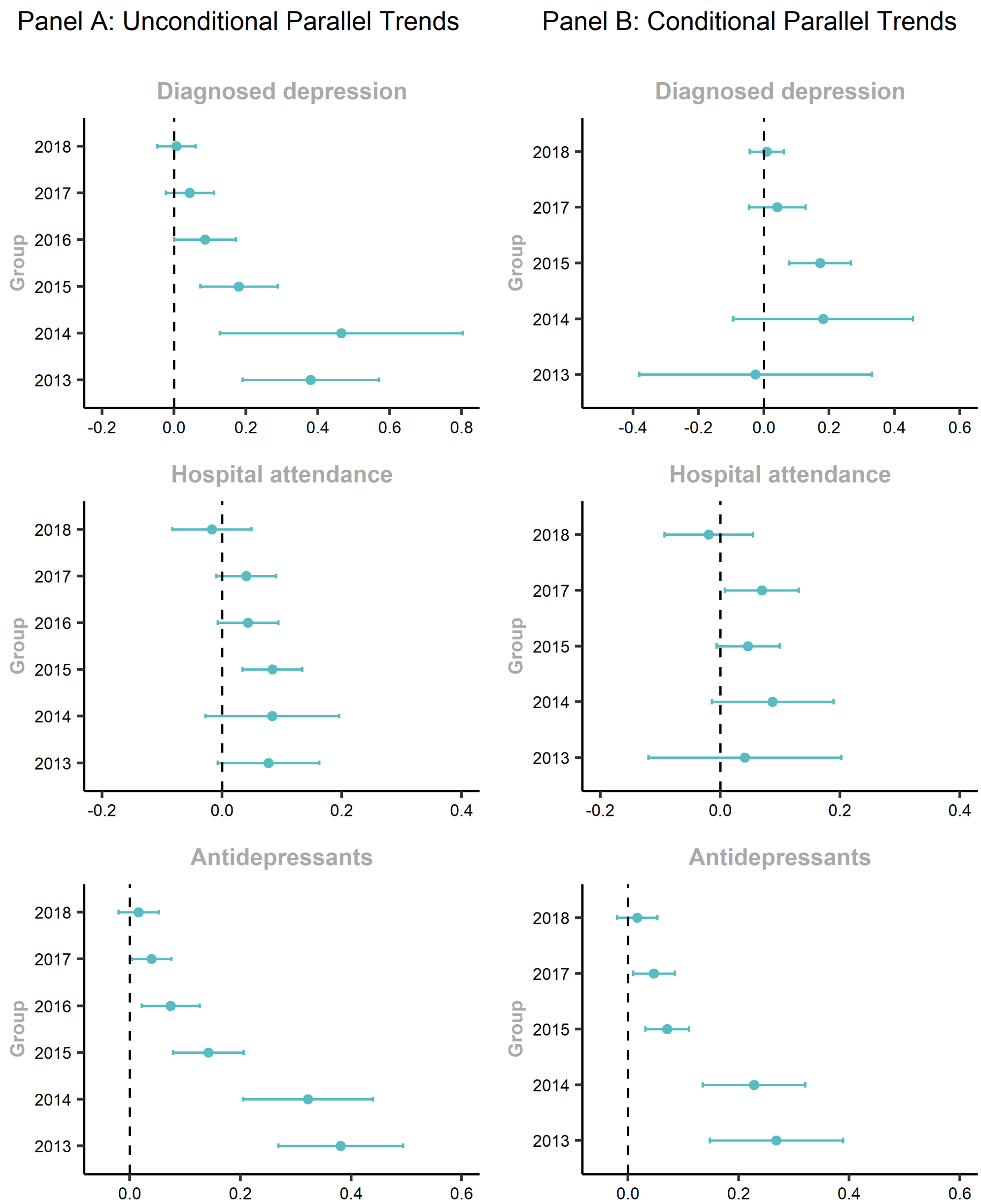
Notes: This table reports overall aggregate ATT of UC roll-out on mental health outcomes based on event-study/dynamic aggregation, estimated using the CS-DiD estimator. Panel A presents estimates under the unconditional parallel trends assumption (no covariates), while Panel B presents estimates conditional on local area covariates. The ATT parameters correspond to the aggregation of the event study estimates from Figure 5, with one year anticipation of UC roll-out. The comparison group consists of 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). Estimates are obtained using the doubly robust estimation method, and standard errors are clustered at the Local Authority District (LAD) level and computed using a bootstrap procedure with 1,000 iterations. Covariates included in Panel B are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LSOA and year fixed effects. *** p<0.01, ** p<0.05, and * p<0.1.

Figure B3: Event study of Universal Credit roll-out effect on mental health, with one year anticipation



Notes: This figure presents event-study estimates of the UC roll-out effect on mental health outcomes, displaying the average treatment effect by length of UC exposure (measured in years from exposure). Panel A shows estimates under the unconditional parallel trends assumption (without covariates), while Panel B reports estimates conditional on local area covariates. Red dots and lines represent point estimates and simultaneous 95% confidence intervals for pre-treatment periods, estimated using the CS-DiD estimator with the doubly robust estimation method, with one year anticipation of treatment effects. Blue dots and lines represent point estimates and simultaneous 95% confidence intervals for post-treatment periods, capturing dynamic effects of UC roll-out. We follow Roth (2024)'s recommendation and estimate event-study coefficients using a common 'universal' base period, implemented via the *base-period = "universal"* option in the CS-DiD estimation, so that pre- and post-treatment estimates are derived symmetrically and avoid mechanical flattening of pre-treatment coefficients. Standard errors and confidence bands are clustered at the Local Authority District (LAD) level and computed using a bootstrap procedure with 1,000 iterations. The comparison group comprises 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). The conditional model (Panel B) includes the following covariates: population size, share of working-age population (16-64), share of female population, job density, unemployment rate, share of White British population, and per capita local spending on culture, education, and social care. All models include LSOA and year fixed effects. Post-treatment effects under conditional parallel trends (Panel B) are available for only two years following UC exposure due to limitations in covariate overlap in later years, as discussed in the main text. The overall summary average treatment effects (ATT) based on dynamic event-study aggregation are reported in Table B3.

Figure B4: Group average effect of Universal Credit roll-out on mental health, with one year anticipation



Notes: This figure reports group-specific average effects of the UC roll-out on mental health outcomes, as described by Equation (4), estimated under the unconditional parallel trends assumption (Panel A) and conditional parallel trends assumption (Panel B). Each group on the y-axis represents LSOAs categorised by the year in which their residents first claimed UC benefits. Point estimates (dots) and simultaneous 95% confidence intervals (lines) for each group's treatment effects are computed using the CS-DiD estimator with the doubly robust estimation method, with one year anticipation. Confidence intervals are constructed using a bootstrap procedure with 1,000 iterations and are clustered at the LAD level. The comparison group consists of 'not-yet-treated' LSOAs. The conditional parallel trends model (Panel B) includes local area covariates: population size, share of working-age population (16-64), share of female population, job density, unemployment rate, share of White British population, and per capita local spending on culture, education, and social care. All models control for LSOA and year fixed effects.

B.3 Alternative staggered DiD specifications

Tables B4-B6 report overall aggregate treatment effects from three alternative estimators tailored to staggered adoption: Roth and Sant'Anna (2023) (Table B4), Sun and Abraham (2021) (Table B5), and Borusyak et al. (2024) (Table B6). For Roth and Sant'Anna and for Sun and Abraham, we present three aggregation schemes (simple-, cohort-, and calendar-weighted); for Borusyak et al., we report the imputation-based ATT. Across outcomes and aggregation schemes, point estimates are positive and statistically significant, and lie within the range implied by the main CS-DiD results. As expected, magnitudes vary with aggregation weights and comparison groups (e.g. 'not-yet-treated' vs. 'last-treated'), but the conclusion is consistent.

Table B4: Universal Credit roll-out overall aggregate effect on mental health: Estimates from Roth and Sant'Anna (2023)'s efficient estimator

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Aggregation:			
Simple-weighted	0.0976***	0.0523***	0.0206***
	(0.008)	(0.007)	(0.003)
Cohort-weighted	0.0787***	0.0432***	0.0180***
	(0.008)	(0.008)	(0.003)
Calendar-weighted	0.1145***	0.0767***	0.0218***
	(0.008)	(0.007)	(0.002)
Treated cohorts	6	6	6
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
Universal Credit roll-out anticipation	NO	NO	NO
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports overall unconditional, aggregate average treatment effects (ATT) of UC roll-out on mental health outcomes, using Roth and Sant'Anna (2023) plug-in efficient estimator. Aggregation is done with weighting across treatment cohort size and years. Three types of treatment effect aggregations are used: (i) the simple-weighted treatment effect (weighted by cohort size); (ii) weighted average of cohort effects (cohort-specific); and (iii) weighted average of calendar time effects (calendar-specific). The comparison group consists of 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). All specifications control for LSOA and year fixed effects. *** p<0.01, ** p<0.05, and * p<0.1.

Table B5: Universal Credit roll-out overall aggregate effect on mental health: Estimates from Sun and Abraham (2021)'s estimator

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
Aggregation:			
Simple-weighted	0.1274***	0.0533***	0.0905***
	(0.012)	(0.011)	(0.006)
Cohort-weighted	0.1049***	0.0488***	0.0755***
	(0.012)	(0.011)	(0.006)
Calendar-weighted	0.1388***	0.0448***	0.0964***
	(0.010)	(0.008)	(0.005)
Treated cohorts	6	6	6
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
Universal Credit roll-out anticipation	NO	NO	NO
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports overall unconditional, aggregate average treatment effects (ATT) of UC roll-out on mental health outcomes, using SSun and Abraham (2021)'s estimator. Aggregation is done with weighting across treatment cohort size and years. Three types of treatment effect aggregations are used: (i) the simple-weighted treatment effect (weighted by cohort size); (ii) weighted average of cohort effects (cohort-specific); and (iii) weighted average of calendar time effects (calendar-specific). The comparison group consists of 'last-treated' Lower-layer Super Output Areas (LSOAs). All specifications control for LSOA and year fixed effects. *** p<0.01, ** p<0.05, and * p<0.1.

Table B6: Universal Credit roll-out overall aggregate effect on mental health: Estimates from Borusyak et al. (2024)'s imputation estimator

	(1)	(2)	(3)
	Depression rate (z-score)	Hospital admissions & attendances (z-score)	ADQ of antidepressants (z-score)
ATT	0.1873***	0.0627***	0.1168***
	(0.023)	(0.014)	(0.017)
Treated cohorts	6	6	6
Number of LSOAs	32,844	32,844	32,844
Number of LADs	309	309	309
Observations	262,752	262,752	262,752
Universal Credit roll-out anticipation	NO	NO	NO
LSOA FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports overall unconditional, aggregate average treatment effects (ATT) of UC roll-out on mental health outcomes, estimated using Borusyak et al. (2024)'s imputation estimator. The comparison group consists of 'not-yet-treated' Lower-layer Super Output Areas (LSOAs). Standard errors (in parentheses) are clustered at the Local Authority District (LAD) level. All specifications control for LSOA and year fixed effects. *** p<0.01, ** p<0.05, and * p<0.1.

C Details of cost estimation

This section details the approach we used to construct the indicative human and healthcare cost estimates associated with the mental health impacts of UC, which we report in Section 5.3 of the main paper. We quantify: (i) human costs associated with depression; (ii) direct NHS costs from increased antidepressant prescribing; and (iii) direct NHS costs from additional hospital admissions and A&E attendances. Unless stated otherwise, monetary figures are expressed in 2022 prices and national aggregates reflect England's adult population during the study window. These valuations exclude productivity losses and therefore represent conservative estimates.

C.1 Human costs due to depression

We define the human cost due to depression as the monetary value of reduction in quality of life associated with depression attributable to UC exposure. Following Cardoso and McHayle (2024), we monetise wellbeing losses separately for three age groups: younger adults (18-19), working-age adults (20-64), and older adults ($\geq$65). For younger and older adults, we value quality-of-life reduction using a Quality-Adjusted Life Year (QALY) framework, using the EuroQol fivedimensional (EQ-5D-5L) measure, applying the UK Treasury Green Book value of £70,000 per QALY. For working-age adults, we adopt a Wellbeing-Adjusted Life Year (WELLBY) valuation approach in line with UK Treasury guidelines (GOV.UK, 2024), using £13,000 per WELLBY in 2019 prices uprated to £15,650 in 2022 prices.

Our baseline estimate from Table 2 (Panel B) shows a 0.096 SD increase in diagnosed depression. Re-estimating the model with depression expressed in natural units—percentage points of adult prevalence at the Lower-layer Super Output Area (LSOA) level—yields an effect of 0.256 percentage points (Table C1, Panel B), corresponding to approximately 113,742 additional depression cases nationally ,on average by 2018 (Table C2), consistent with prior estimates (Marimpi et al., 2025). For younger and older adults, we apply EQ-5D decrements of 0.422 and 0.257, respectively (Cardoso and McHayle, 2024), implying monetary values of about £29,540 and £17,960 per case at £70,000 per QALY. For working-age adults, we apply a life-satisfaction decrease of 1.521 points (0-10 scale) on average due to depression and the 2022 £15,650 WELLBY value, yielding approximately £23,804 per affected individual (Cardoso and McHayle, 2024).

Combining the three age-specific values using observed population shares across LSOAs gives a population-weighted average human cost of approximately £23,549 per additional depression case attributable to UC roll-out. Multiplying by the implied case increase of 113,742 yields an aggregate human cost of about £2.68 billion per year (£2,678,466,940), as reported in Table C2.

Table C1: Universal Credit roll-out overall aggregate effect on depression prevalence and antidepressants prescribing, using natural units

	(1)	(2)
	Depression rate (% adult residents)	Antidepressants prescribing (# of items per adult)
Panel A: No covariates		
ATT	0.2405***	0.0397***
	(0.048)	(0.005)
Panel B: With covariates		
ATT	0.2556***	0.0291***
	(0.044)	(0.003)
Treated groups	6	6
Number of LSOAs	32,844	32,844
Number of LADs	309	309
Observations	262,752	262,752
UC roll-out anticipation	NO	NO
LSOA FE	YES	YES
Year FE	YES	YES

Notes: This table reports overall aggregated ATT of UC roll-out on depression and antidepressants prescribing based on group/cohort aggregation, estimated using the CS-DiD estimator. Panel A presents estimates under the unconditional parallel trends assumption (no covariates), while Panel B presents estimates conditional on local area covariates. The ATT parameters correspond to the aggregation described by Equation (5), with no anticipation of UC roll-out effects. The comparison group consists of 'not-yet-treated' LSOAs. Estimates are obtained using the doubly robust estimation method, and standard errors are clustered at the LAD level computed using a bootstrap procedure with 1,000 iterations. Covariates included in Panel B are population size, share of working-age population (16-64), share of female population, job density, unemployment rate, proportion of White British residents, and per capita local spending on culture, education, and social care. All specifications control for LSOA and year fixed effects. *** $p<0.01$, ** $p<0.05$, and * $p<0.1$.

C.2 Healthcare costs: antidepressants prescribing

We estimate incremental prescribing costs using practice-level data on antidepressant items, average daily quantities (ADQs), and net ingredient costs (NIC) from the Antidepressants Prescribing Indicators dataset (Daras and Barr, 2021). The average NIC per antidepressant item, pooled over 2013-2018 and uprated to 2022 prices, is £6.77.

The baseline ATT (Table 2, Panel B) indicates a 0.059 SD rise in antidepressant prescribing. In natural units, this corresponds to an additional 0.0291 items per adult (Table C1, Panel B). Aggregated nationally, this implies approximately 1,294,950 additional items annually. Multiplying by the unit cost yields an incremental NHS prescribing cost of about £8.76 million per year (£8,762,088), shown in Table C4.

C.3 Healthcare costs: hospital admissions and A&E attendances

We value the additional healthcare costs associated with increased hospital admissions and A&E attendances stemming from UC-related mental health deterioration by combining admissions and A&E attendances into a single 'attendance' unit priced at a weighted average tariff. The calculation proceeds in three steps.

Step 1: Unit costs by care setting. Using NHS reference tariffs at Trust level for 2017/18 and 2018/19, uprated to 2022 prices, we obtain an average cost per admission (elective and non-elective combined) of £6,183 and an average cost per A&E attendance of £187 over the same two years.

Step 2: Weights from observed activity mix. Let w_{adm} and w_{AE} denote the shares of mental–health–related activity accounted for by admissions and A&E, respectively, where $w_{\mathrm{adm}}+w_{\mathrm{AE}} = 1$. We compute these weights from the total counts of mental–health–related admissions and A&E attendances used to construct SAMHI, summed over 2017/18–2018/19 across all Trusts. This anchors the price to the observed mix of episodes underlying our outcome measure.

Step 3: Weighted average tariff. The attendance-unit cost is

$$\bar{c} = (w_{\mathrm{adm}} \cdot \pounds 6{,}183) \ + \ (w_{\mathrm{AE}} \cdot \pounds 187) \tag{1}$$

Applying the observed activity shares yields a pooled attendance-unit cost of £5,178. In other words, £5,178 reflects the two-year, 2022-price weighted average of admissions and A&E tariffs, with weights proportional to the corresponding numbers of mental-health-related episodes.

The baseline ATT (Table 2, Panel B) implies a 0.0337 SD increase in the per-capita admission and attendance rate. Because natural-unit data are unavailable for this outcome, we convert the SD effect using the standard deviation of the original per-capita rate to obtain an increase of 0.000674 per capita, implying approximately 29,993 additional combined admissions and attendances nationally by 2018 (Table C3). Multiplying by the unit tariff of £5,178 gives an incremental NHS cost of approximately £155.3 million per year (£155,291,762).

Note on interpretation

These valuations are indicative. They reflect quality-of-life losses (human costs) and direct NHS expenditures only; they do not include productivity losses, informal care, or broader social costs. Population totals are constructed from observed LSOA-level changes and national adult population counts over 2013-2018; small differences from figures in the main text reflect rounding. Confidence intervals for cost aggregates can be derived by applying the same transformations to the corresponding effect-size intervals; we report point estimates for parsimony.

Table C2: Implied cost of the impact of UC roll-out on adult population mental health, 2022 prices – Depression

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline impact coefficient (in SD)	Impact coefficient in natural unit (% point change)	Implied total change in depression prevalence	EQ-5D decrease value (<20)	WELLBY decline value for for working-age population (20-64)	EQ-5D decrease value (65+)	Weighted monetary unit cost	Average national cost
0.0960	0.2556	113,742	£29,540	£23,804	£17,960	£23,549	**£2,678,466,940**

Notes: This table presents indicative estimates of the human cost associated with the adverse mental health impacts of UC roll-out in England, based on our baseline estimates reported in Table 2, Panel B. Column (1) repeats the estimated treatment effects on QOF-depression expressed in SD units. Column (2) converts the effect into natural units by re-estimate the model using diagnosed depression expressed as a percentage of the adult population in each LSOA (Table C1). Column (3) reports the implied aggregate increase in the depression prevalence across the adult population, based on observed changes at the small-area level and the national adult population from 2013 to 2018. Column (7) presents the monetary unit cost associated with depression, weighted by the share of each population group, which reflects the wellbeing-adjusted monetary valuation of reduced quality of life due to depression, drawing on Cardoso and McHayle (2024). This combines QALY-based estimates for younger (<20) [Column (4)] and older (65+) [Column (6)] adults with WELLBY-based valuations for the working-age population [Column (5)]. Column (8) reports the total implied cost to society, aggregating the product of the estimated increase in depression prevalence and its corresponding unit value.

Table C3: Implied cost of the impact of UC roll-out on adult population mental health, 2022 prices – Hospital admissions and A&E attendances

(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baseline impact coefficient (in SD)	Impact coefficient in terms of admission/ attendance per capita	Implied total change in hospital admissions/ attendances	Average elective/ non-elective admissions cost	Average A&E attendance cost	Weighted average cost per admission/ attendance	Average national cost
0.0337	0.000674	29,993	£6,183	£187	£5,178	**£155,291,762**

Notes: This table presents indicative estimates of the healthcare cost—in terms of hospital admissions and A&E attendances—associated with the adverse mental health impacts of UC roll-out in England, based on our baseline estimates reported in Table 2, Panel B. Column (1) reproduces the estimated treatment effects on hospital admissions and attendances expressed in SD units. Column (2) converts the effect into natural units, where we approximate the implied effect in terms of admission and attendance rate per capita using the standard deviation of the original per capita rate. Column (3) reports the implied aggregate increase in combined hospital admissions and A&E attendances across the adult population, based on observed changes at the small-area level and the national adult population from 2013 to 2018. Column (6) presents the monetary unit cost associated with hospital admissions and A&E attendances, using NHS tariff from 2017-2019 at the Trust level, weighted by the shares of hospital admissions and A&E attendances in the total number of admissions and attendances. This combines NHS tariffs for elective/non-elective admissions [Column (4)] with A&E attendance [Column (5)]. Column (7) reports the total implied cost to NHS, aggregating the product of the estimated increase in combined hospital admissions and A&E attendances and its corresponding unit cost.

Table C4: Implied cost of the impact of UC roll-out on adult population mental health, 2022 prices – Antidepressants prescribing

(1)	(2)	(3)	(4)	(5)
Baseline impact coefficient (in SD)	Impact coefficient in terms of prescribed items (per adult)	Implied total change in items prescribed	Average unit cost of item prescribed	Average national cost
0.0590	0.0291	1,294,950	£6.77	**£8,762,088**

Notes: This table presents indicative estimates of the antidepressants prescribing cost associated with the adverse mental health impacts of UC roll-out in England, based on our baseline estimates reported in Table 2, Panel B. Column (1) reproduces the estimated treatment effects on antidepressant prescribing expressed in SD units. Column (2) converts the effect into natural units by re-estimate the model using antidepressant prescribing items per adult person in each LSOA (Table C1). Column (3) reports the implied aggregate increase in the number of antidepressant items prescribed across the adult population, based on observed changes at the small-area level and the national adult population from 2013 to 2018.

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