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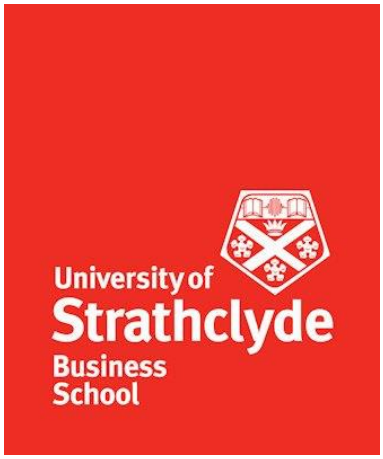

Conflicting economic policies and mental health: evidence from the UK national living wage and benefits freeze

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Abstract

This study evaluates the mental health effects of two simultaneously implemented but conflicting policies in the UK: the National Living Wage and the benefits freeze policy. We employed the Callaway and Sant'Anna (2021) DID estimator to evaluate the heterogeneous policy effects, and we found that NLW leads to positive improvements in mental health. Also, we find that the negative impact of the benefits freeze policy constricts the NLW effects. Our result is robust to the sensitivity analysis of the parallel trend assumption. Additional results support the psychosocial hypothesis that increased job satisfaction is strongly correlated with improvements in mental health. Also, we found evidence of substitution effects between work hours and leisure. Overall, our findings suggest that the effects of the NLW cannot be understood in isolation from how the entire suite of policy instruments operates on earnings and liveable income for affected low-wage workers.

Keywords: National living wage, Benefits freeze, Mental health, Income policy, Difference in Differences, Treatment effect heterogeneity

JEL Classification: C14, C21, C23, I18, J38

1. Introduction

The policy paper that sets out the NLW by the UK government conceptualised the national living wage as an essential part of ensuring that low-wage workers take a greater share of the gains from the post-GFC growth and expansion experienced in the economy relative to similar developed economies. The NLW policy aimed at ensuring that work pays by reducing reliance on government supplementing earnings through the benefits system.[1] In other words, it is an attempt by the government to shift the associated costs and burdens of augmenting low income through welfare benefits to employers in the form of higher wages while also preventing the degradation of the existing precarity of low-income workers. Previous impact evaluations and policy reports of the NLW seem to provide evidential support that the government's objectives have been met. However, the evidence is mostly restricted to the labour market and employment outcomes. For example, the 2015-2020 impact review of the National Living wage by the Low Pay Commission, the independent body responsible for advising the UK government about the national minimum and living wage rates, considers outcomes, including its impact on employment, hours worked, and employer responses through price adjustments, profits, productivity and underpayment, among others (Low Pay Commission, 2022).

Attempts have also been made to evaluate the indirect effects and the unintended consequences of wage policies, especially the impacts on health and health behaviours. Past empirical evidence on the health effects of minimum wage policy is largely concentrated in the US (Leigh et al., 2019). Attention has also extended to other countries and regions, including the UK (Kronenberg et al., 2017; Lenhart, 2017a; Maxwell et al., 2022; Reeves et al., 2017), Germany (Hafner & Lochner, 2021), China (Chen, 2020), OECD and developing countries (Lenhart, 2017b; Ponce et al., 2018). Various health outcomes and health behaviours have also been considered, including physical and mental health, smoking behaviour, fertility, access to health insurance, etc. However, the empirical findings did not provide consensus on wage policies' impact on health, perhaps because of the various theoretical pathways linking minimum wage policy to health. For example, the findings and inferences drawn in previous studies evaluating the health effects of the UK's national minimum wage (NMW) are mixed and inconclusive, even though some of these studies explored similar methodology, same data sources and comparable health outcomes.

[1] See https://www.gov.uk/government/publications/national-living-wage-nlw/national-living-wage-nlw

We contribute to the literature on economic policies and health nexus by evaluating the mental health effects of the UK's National Living Wage and working-age benefits freeze policies. In line with the NLW objective of reducing workers' dependence on state benefits to augment low income, the benefits freeze policy suspended the usual annual uprating of income-related benefits using the prevailing Consumer Prices Index.[2] The simultaneous implementation of the NLW with the freeze on working-age benefits, which largely led to a decline in the real value of the affected welfare benefits and overall gross earning of affected individuals, could choke off the positive benefits of the NLW. Moreover, previous literature often considers wage policy to be unrelated to the expansion or contraction of other safety net programmes (Rothstein & Zipperer, 2020). However, low-wage workers are highly susceptible to changes in temporary income, and they often rely on welfare benefits to augment their spending (Mosley, 2021). Besides, the availability and generosity of other safety net programmes work together with a minimum wage increase to enhance income and reduce the death of despair (Dow et al., 2020).

The choice of mental health outcome is premised on its immediacy, and as such, it is relatively easy to attribute to policy action. Also, mental health symptoms can be assessed with or without taking physical measurements; hence, it has been reasonably and reliably measured through various survey instruments. Besides, it is predictive of poor physical health and predisposes to other poor health outcomes, with no other health condition close to its persistence and breadth (Kousoulis, 2019; Ohrnberger et al., 2017). Also, the costs associated with mental health disorders in the UK, including only broad mental conditions that meet diagnosable thresholds of certain mental conditions, excluding dementia, intellectual disabilities, alcohol or substance misuse, and deliberate self-harm, are estimated at approximately 5% of the country's GDP (McDaid et al., 2022). More importantly, given the societal burden and high economic costs associated with mental disorders, understating the mental health effects of the NLW policy could provide an economic case for preventative and proactive measures to promote better mental health.

Furthermore, we employed an estimation approach that accommodates the NLW dynamics of different annual basic rates and additional eligible workers over time. Previous studies that explored the heterogeneous effects of wage policies mostly focused on the labour market outcomes and mostly in the US (Cengiz et al., 2019; Dube et al., 2016). We follow the recent

[2] The affected benefits include the universal credit (UC) and other legacy benefits that it replaced, such as the income-based job seeker's allowance, income-related employment and support allowance, income support, housing benefit, child and working tax credits, and child benefits.

development in the difference-in-differences (DID) setup, allowing us to identify and estimate policy-relevant disaggregated and interpretable causal parameters (see Callaway & Sant'Anna, 2021). Specifically, the strength of the staggered-adoption DID design allows for cumulative impact assessment of the introduction and upratings in the NLW policy, as well as comparing the effects trajectories across units treated at different times, which the canonical two-periods and two-groups framework could have missed just by examining the yearly increases in wage rates (Borusyak et al., 2021; Redmond & McGuinness, 2022).

Our findings show that the introduction and subsequent annual upratings in the national living wage positively affect mental health. We also find that the mental health effect of the policy is stronger for workers reportedly not affected by the benefits freeze policy between 2016 and 2020. Additional estimations of the NLW effects on the selected labour market and work-related wellbeing outcomes, including earned income, work hours, job satisfaction and satisfaction with leisure time, confirm the positive and significant policy effects on outcomes that potentially link wage policy to mental health. Finally, the sensitivity analyses confirmed the validity of our results to post-treatment violations of the parallel trend assumptions.

2. Background on economic policies and welfare reforms in the UK

Despite the annual increase in the NMW since its introduction in 1999 and the subsequent introduction of the NLW in 2016, low-income workers and poor households in the UK still grapple with meeting basic life necessities (Goulden, 2016), creating an atmosphere of precarity and distress in such households. These challenges may not be unconnected with the impacts of the series of welfare reforms and austerity policies, particularly those introduced after the 2008 GFC. For most of these reforms, the central objectives are to reduce welfare spending and encourage people to move into work and employment from reliance on benefits and public support (Alvarez-Vilanova, 2018). For example, the government's main objective for introducing the four-year freeze on working-age benefits between 2016 and 2020 was to ensure that the growth in earnings overtakes the growth in benefits and, therefore, make it financially better for people to be in work rather than claiming benefits (Kitara, 2016). Additionally, the government intend to reduce the overall spending on welfare by a projected £4 billion saving each year of the benefits freeze.

Empirical findings on the impacts of these reforms are mixed, but they mostly point to the deteriorating impacts on low-wage workers and poor households. For example, the cumulative effects of major welfare reforms before 2017, including the benefits caps, localisation of

council tax support administration, local housing allowance shortfall, and the bedroom tax, also known as the under-occupancy charge, reveal a decline in average income for working-age households (Policy in Pratice, 2017). Also, the evaluation of the extent to which the NLW and the Universal Credit (UC) could facilitate achieving a minimum living standard for the UK populace finds that the rising costs of living increased at a higher rate than the increase in the UC and full-time workers earning a living wage fall short of the acceptable income needed for a stable and secure life even when they are on universal credit (Davis et al., 2021).

Evaluation of the impacts of these welfare reforms on health and wellbeing largely suggests that these policies and programmes culminated in increasing health issues, particularly mental health disorders, and widening health inequalities (Reeves et al., 2013). Other studies found an increased association of these reforms with rising trends in health problems. For example, Wickham et al. (2020) found increasing psychological distress among the people affected by the introduction of the UC policy. Also, Katikireddi et al. (2018) investigate the effects of the changes to the Lone Parent Obligation (LPO) policy, which requires lone parents to seek work as an eligibility condition to continue to receive welfare benefits once their youngest child attains a certain age. They found that the continuous reductions in the LPO lower age thresholds since 2008 led to a decline in the mental health of affected lone mothers.

More importantly, these reforms do not have equal effects on all groups. For example, the cuts to local government budgets implemented in 2010 had the hardest hit on the poorest parts of the country (Crawford & Phillips, 2012), while the tax and benefit reforms in 2012 which reduced the adequacy of some benefits through capping, disproportionately affected low-income households of working age (De Agostini et al., 2014). Moreover, because beneficiaries are usually not well organised and sometimes weakly represented in the policy-making process, social assistance benefits form an easy target by policymakers when dealing with budgetary pressure (van Vliet & Wang, 2017).

Hence, this study focuses on evaluating the mental health effects of the NLW policy, given that the NLW was introduced during a period characterised by austerity and large-scale cuts in government funding. Additionally, NLW was estimated to facilitate a wage boost for about six million people by increasing earnings for low-wage workers aged 25 and above. More importantly, our focus on evaluating the mental health effects of the policy provides empirical evidence of whether the policy has facilitated low-income working individuals to meet the level of material sufficiency adequate to live securely and without worry, which is also the implicit

intention of most wage policies. The next section reviews some literature on wage policies and health outcomes.

3. Literature review – wage policy and health

Empirical research has consistently demonstrated that income affects health and health behaviours through various channels. These channels can be broadly categorised into three. The first is through countries' gross national product, individual incomes, and income inequalities, all of which have been separately found to influence public health (Marmot, 2002). The second dimension that has also received attention in the empirical literature is income dynamics, which evaluates the effects of short-run and long-run measures of income on health outcomes. Income stability, volatility and income trajectories over time significantly predict health outcomes and wellbeing (Akanni et al., 2022; Davillas et al., 2019). The third dimension of the income-health nexus is the role of socioeconomic policies. Empirical studies have shown that health and health behaviours are among the important indirect consequences of social and economic policy interventions to improve the earnings of low-income workers (Osypuk et al., 2014).

There has been particularly growing attention in the literature on the health effects of wages and other socioeconomic and safety-net policies. The amount of empirical evidence is limited compared to the attention devoted to evaluating the effects of these policies on labour market outcomes. Popular issues widely considered include employment, work hours, poverty, income inequality, job automation and job quality both in commissioned studies and impact assessments reports (seeDube, 2019; Low Pay Commission, 2022). However, the public health effects, particularly on mental health, are rarely considered during policy discussions and debates regarding the determination of minimum wages (Leigh et al., 2019).

Theoretically, the pathways and transmission mechanisms between wage policies and health are considerably interconnected. Leigh et al. (2019) document three theoretical pathways in the literature linking wage policies to health, including (1) consumption or affordability, (2) psychosocial and (3) decision-making pathways. Another pathway is the intergenerational pathway that links minimum wage to children's health (Hill & Romich, 2018). The first and dominant pathway identified and widely evaluated in the empirical literature is the consumption pathway, and the theoretical explanation is premised on Grossman's (1972) model for health demand. The desirability of good health depends on health-enhancing consumption activities subject to the constraints imposed by limited resources at every

individual's disposal (Wagstaff, 1986). Hence, low-income individuals tend to exhibit poorer health status than individuals earning a higher income.

Secondly, the psychosocial hypotheses propose that individuals with less income often have worse health than individuals with higher income due to negative upward social comparisons, resulting in frustration, shame, stress, and subsequently ill health" (Hounkpatin et al., 2016, p. 76). The material disadvantage brought by low earnings is a precursor to psychosocial adversities that include greater stress, depression, and less satisfaction with job and life, associated with poorer health (Macleod & Davey Smith, 2003; Marmot & Wilkinson, 2001). Higher income and satisfaction with compensation can significantly boost employees' work motivation, job satisfaction, and perceived quality of life and thus improve psychosocial factors such as control over one's life, anxiety, financial insecurity, depression, and social affiliations (Che Ahmat et al., 2019).

A third pathway considered in the literature is the workers' and firms' decision-making, which considers firms' investment motives and workers' opportunity costs between work hours and leisure time following an increase in wages (Leigh et al., 2019). Finally, the intergenerational pathway links parents' socioeconomic status to children's health through improved household provisions and consumption activities following increased family income, as well as through changes in parenting time and routine and changes in parental stress and parenting practices (see Averett et al., 2021; Hill & Romich, 2018).

In terms of empirical methodology, the difference-in-differences technique remains the most popular quasi-experimental strategy widely employed to estimate the health effects of wage policies. (see Dube, 2019).[3] The usual approach is to designate treatment and control units using appropriate and applicable criteria relevant to the study and policy context. In certain countries such as Brazil, the USA, and Vietnam, among others that have variegated and spatial clustering of minimum wage policies which allow different states or regions to set their minimum wage, previous studies that evaluate the effects of minimum wage policies in this context have largely explored the variations in the implementation across and within different

[3] Studies have also deployed the randomised control trials to study other aspects of income interventions on health outcomes such as the universal basic income Gibson, M., Hearty, W., & Craig, P. (2020). The public health effects of interventions similar to basic income: a scoping review. *Lancet Public Health*, *5*(3), e165-e176. https://doi.org/10.1016/S2468-2667(20)30005-0 . The other quasi-experimental approach employed in previous literature is the regression discontinuity approach Dickens, R., Riley, R., & Wilkinson, D. (2014). The UK minimum wage at 22 years of age: a regression discontinuity approach. *Journal of the Royal Statistical Society Series a-Statistics in Society*, *177*(1), 95-114. https://doi.org/10.1111/rssa.12003

states and regions in identifying the treatment and comparison units (Dube, 2019). On the other hand, past studies in the UK and other similar countries that have wage policies that are centrally determined and binding nationally delineate treatment and controls using different approaches, premised mainly on available data on workers' hourly wages and other characteristics that make participants eligible to receive the pay rise. However, despite the similarities in the data sources and outcomes considered across these studies, findings on the health effects of the UK's minimum wage policy are not conclusive.

The differences in findings are connected to their delineation of treatment and control groups. For example, Kronenberg et al. (2017) did not find statistically significant effects of the NMW introduction on mental health improvements. On the contrary, Reeves et al. (2017) and Lenhart (2017a) evaluate similar NMW policy experiments. Their findings show significant improvements in mental health and other self-reported health outcomes. Arulampalam et al. (2004) used the information about earnings and usual work hours in the British Household Population Survey (BHPS) to derive individuals' basic hourly wages by dividing usual gross pay by work hours (see also Kronenberg et al., 2017; Reeves et al., 2017). These studies assume the absence of measurement errors in their adopted measure of basic hourly wage. However, the inadequacy of this approach is the possible inclusion (or exclusion) of individuals in the treatment or control groups who have their gross earnings, including other components of wages such as overtime premium received and bonuses (Stewart & Swaffield, 2002). Both Kronenberg et al. (2017) and Lenhart (2017a) exploit the question in the BHPS, which asked participants whether they received increased wages to comply with the UK's 1999 NMW policy, further allowing a cleaner identification of workers that were actually treated and those in the control group (see Kronenberg et al., 2017; Lenhart, 2017a).

Recently, Maxwell et al. (2022) find that the effects of the 2016 to 2018 increase in UK NMW on self-reported health outcomes are insignificant. Assuming each wage upratings followed separate parallel paths over time, the authors estimated multiple difference-in-differences regressions, considering one period before and after treatment in the estimation. However, while this approach is simple and provides the instantaneous health effects of the wage policy (Stewart, 2012), it does not provide the effects in successive periods. Also, there could be variations in the treatment effects for individuals treated in the different years and the length of time they became treated. Overall, adopting an identification approach that follows the Canonical DID setup in estimating the treatment effects dynamics of such a heterogenous wage policy could lead to poor estimates and inferred conclusions (Borusyak et al., 2021).

Furthermore, in evaluating the strengths and limitations of the recent approaches and advances in the DID literature, Roth et al. (2022) conclude that the most direct remedy for the identification and estimation problem is to use the methods that allow one to estimate a well-defined causal parameter under parallel trends, with transparent weights and transparent comparison groups. While diagnostics provide information on the extent to which conventional TWFE specifications make bad comparisons, the approaches that estimate the disaggregated and aggregate heterogeneous treatment effects parameter provide a complete solution to the problem. These methods also explicitly specify the comparisons to be made between treatment and control groups, as well as the desired weights in the target parameter. Besides, eliminating the undesirable comparisons seems to be a better approach than diagnosing the extent of the issue" (Roth et al., 2022, p. 18).

4. Data and method

4.1. Data source

We collected data from twelve waves of the Understanding Society UK Household Longitudinal Study (UKHLS). The UKHLS provides a large-scale individual-level dataset across a longitudinal spectrum. Individuals are selected from households across all geographical areas of the UK and repeatedly followed over time. The applicability of the UKHLS in policy research has also been demonstrated in previous empirical research on the nexus between income and wellbeing (Akanni et al., 2022; Davillas et al., 2019; Platt et al., 2021). We accommodate the complexity in the longitudinal design by pooling individual data from intersecting waves and harmonising it into their corresponding financial years to ensure the sample is nationally representative (Kaminska and Lynn, 2019). The data also provide information on the actual interview dates.

Given that the introduction and subsequent upratings of the NLW are effective in April every year, we harmonise the data to define a financial year commencing from April to March of each successive year. More importantly, the survey collects detailed data on respondents' age and basic hourly wages that we required to identify individuals eligible for treatment and the comparison groups. We restrict the analysis to workers between the age of 25 and limited to those below 65 in each interview year, limiting the estimation to individuals that met the minimum eligible age entitled to the NLW and excluding those eligible for pension benefits which commence at age 65.

4.2. The difference-in-differences with multiple time periods

The NLW policy has a multiperiod and multigroup dynamics, given that it was introduced in 2016 for workers above age 24 with a new basic rate introduced every year subsequently as well as additional eligible treatment groups, that is, individuals that reached the minimum eligible age threshold and earning below the basic wage rate in the successive periods. By implication, new workers become eligible for treatment every successive period. As such, our estimation approach deviates from the methods used to evaluate policy interventions with two periods and two groups. We followed the identification condition and estimation procedure of the treatment effects proposed by (Callaway & Sant'Anna, 2021)[hereafter CS]. The estimator allows for evaluating the heterogeneous treatment effects of the NLW policy, providing its disaggregated and cumulative mental health impacts over the periods under consideration.

We begin the DID model setup specification by defining certain parameters and assumptions. Following the notation in CS, we denote $\{Y_{i1}, Y_{i2}, \ldots, Y_{i\mathcal{T}}, X_i, D_{i1}, D_{i2}, \ldots, D_{iT}\}_{i=1}^{n}$ as an independent and identically distributed random sample, with Y_i representing the mental health outcomes for individual $i \in \{1, \ldots, n\}$, while X_i indicates a vector of covariates. The treatment condition is denoted by $D_i \in \{0,1\}$, with D_i Equal to one indicating an individual in the treatment category and zero otherwise. We consider a case of multiple treatment periods (denoted as T), with each period of treatment indexed by $t = 1, \ldots, T$, where $T > 2$.

In line with the approach by CS, we follow the *treatment irreversibility* assumption, which implies that no one is treated in the first period where $t = 1$, and that treatment is absorbing such that once an individual is treated, they remain treated in subsequent periods. Hence, we define the group when an individual first become treated as G, with g denoting each group that eventually participated in the treatment. If an individual never participated throughout the treatment cycle, G is arbitrarily set at ∞. The treatment group, $G_g \in \{0,1\}$ is a binary variable and equals one for an individual belonging to a group that becomes treated in period g (i. e. , $G_{ig} = \mathbf{1}\,[G_i = g]$), and $C \in \{0,1\}$ is also a binary variable for the individuals that never participated in the treatment in the time period considered (i. e. , $C_i = \mathbf{1}\,\{G_i = \infty\} = 1 - D_{iT}$).

Finally, the observed and potential outcomes for each individual in the treatment and comparison group are related through the following framework (Callaway & Sant'Anna, 2021):

$$Y_{it} = Y_{it}(0) + \sum_{g=2}^{T} \big(Y_{it}(g) - Y_{it}(0)\big) \cdot G_{ig}$$

where $Y_{it}(0)$ denotes individual i untreated potential mental health status at time t provided they do not participate in the treatment across the entire periods considered and remain untreated throughout the period T. On the other hand, $Y_{it}(g)$ denotes the potential mental health outcome that the individual i would experience at time t when they first participate in the treatment in period g.

Similar to the approach in CS, our main estimand of interest is the family of the "group-time average treatment effect" parameter $\big(ATT(g,t)\big)$, which accordingly is the "natural generalisation" of the average treatment effect on the treated (ATT) in the canonical DID setup with two time periods, before and after treatment (see Callaway & Sant'Anna, 2021, pp. 4 - 5). [4] This is denoted as:

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

The $ATT(g,t)$ enable us to consider how the average treatment effects vary across different dimensions of the individual according to when they participate in the treatment and the varying length of time they have participated. Finally, we estimate and present three main aggregated causal parameters, including (i) the simple average, which shows the average treatment effects for all participating groups that received treatment irrespective of when they become treated, (ii) the cohorts' average treatment effects, which provide the varying average treatment effects across the different treatment groups, and (iii) the average treatment effects by the length of exposure within which a group become exposed to treatment.

4.3. Identification strategy – treatment and comparison groups

We focused on the NLW introduction in 2016 and the subsequent upratings in 2017, 2018 and 2019, restricting the empirical analysis to periods before the emergence of the COVID-19 pandemic, which had its own major impacts both on the operation of the labour market and on population-level mental health. Our definition of the treatment and comparison groups follows previous studies that have evaluated the effects of the UK's wage policy on various health and non-health outcomes, including studies on employment, earnings, and hours worked (Aitken

[4] See Callaway and Sant'Anna (2021) for a detailed discussion on the specifications and other assumptions of the model.

et al., 2019; Vadean & Allan, 2021), and general and mental health as well as health behaviours (Kronenberg et al., 2017; Lenhart, 2017a; Maxwell et al., 2022; Reeves et al., 2017). Accordingly, an individual worker is eligible for treatment if they are at least 25 and their current basic hourly wage is below the NLW rate. Hence, the treatment group comprises workers directly affected or most likely affected by the NLW policy based on their reported hourly wages. For example, the first treatment cohort in 2016, when the NLW was introduced at £7.20, comprised workers with basic hourly earnings below £7.20, aged between 25 and 64 years, from April 1, 2016, to March 31, 2017. Subsequent treatment cohorts comprise eligible workers earning below the uprated rates of £7.50 in 2017, £7.83 in 2018, and £8.21in 2019 (see Figure 1).

Both the treatment and comparison groups are expected to be similar in many ways, and the untreated group should not suddenly change around the time of treatment (Huntington-Klein, 2021). However, it is worthy to note that there could be instances where there are wage adjustments for the higher-earning category of workers who do not directly benefit from the wage policy but to ensure wage differentials for different skill requirements and job responsibilities. Nonetheless, the main aim of the policy is to increase earnings for workers in the lowest wage band, which directly affects only those earning below the defined wage threshold. Therefore, we designate the comparison group such that they are not directly affected by the NLW policy and were "never-treated" between 2016 and 2019. Also, choosing a comparison group that is further away from the treated group and higher up in the wage distribution reduces the risks posed by the spillover effects. However, the tradeoff is such a comparison group might have dissimilar features from the treatment group (Stewart, 2012). Accordingly, the comparison group comprises workers whose hourly wage rate is equal to or above the basic rate in 2019 but not more than an upper band limit of 50% higher than the NLW rate.

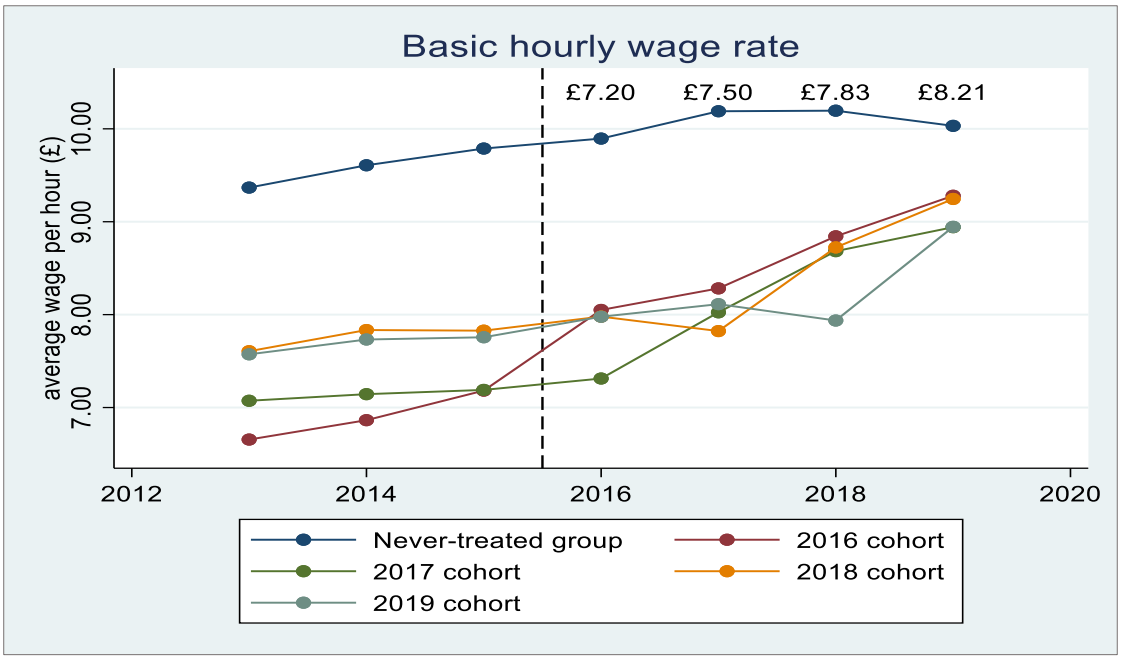

Figure 1: Dynamics of the hourly wage for the treatment cohorts and never-treated group
Note: The figure shows the average basic hourly wage for the treatment cohorts and the comparison (never-treated) group over the years under consideration. The values in the top row show the cut-off wage rates for each treatment cohort.

4.3.1. Variables' measurement

We measure mental health using the Mental Component Summary (MCS) of the 12-item Short-Form Health Survey (SF-12). The SF-12 is well-validated as the shorter adaptation and an efficient alternative to the 36-item generic quality of life instrument (SF-36) (Wee et al., 2008). The MCS is one of two global components, and it converts valid responses to the SF-12 questions into a single mental functioning score with a continuous scale. Ware et al. (1998) proposed the item weights to produce the two components, MCS and the Physical Component Summary (PCS) scales, from the eight domains of the SF-36 using orthogonal factor rotation. The SF-36 has been found to yield acceptable results for detecting recent and active depressive disorders. It has been successfully used as a screening tool to monitor the presence and severity of physical and mental disorders in clinically defined groups in addition to targeting treatment and prevention (Gill et al., 2007; Vilagut et al., 2013). Besides, construct validity of the SF-36 is premised on its successful use to define distinct aspects of physical and mental health (Ware et al., 1998) with the four scales in the summary measure for MCS including vitality, social function, role-emotional and emotional wellbeing. The MCS scores range from 0 to 100, with higher scores indicating better mental health conditions.

Additionally, the DID setup requires accounting for time-invariant confounders. Hence, we follow extant literature that has evaluated the health effects of minimum wage policies by considering certain prespecified covariates to reduce the risk of time-varying confounding. The

covariates considered include age, age-squared, gender, marital status, educational qualification, and region of residence.

5. Empirical Results

We begin this section by discussing the descriptive statistics between the treatment cohorts and the comparison group. Table 1 provides the summary statistics showing the average values across each treatment cohort and the comparison group in the pretreatment periods before the NLW policy was first introduced in 2016. The results show differences in some of their attributes and demographic features. For example, the average monthly after-tax income across each treatment cohort, with the highest for the 2018 cohorts at £1208, is less than the average income for the comparison group at £1537. The average age appears very close across all the treatment cohorts but slightly higher for the comparison group. The summary statistics also show that most individuals in the different treatment cohorts were women. But the comparison group had proportionally fewer females than the treatment groups. There are also differences in marital status, with most workers in both the treatment and comparison groups either married or cohabiting. Most of the workers in the comparison group lived in areas designated as urban. Lastly, the treatment cohorts have a higher fraction of individuals who reportedly received at least one of the affected frozen work-related benefits.

Table 1: Summary statistics

Variables	**Treatment cohorts**					**Comparison**
	2016	2017	2018	2019	Average	
Income	1192.66	1110.63	1208.41	1164.72	1172.53	1537.56
Age	42.126	42.609	42.140	43.568	42.556	45.527
Gender (f*emale*)	0.652	0.724	0.613	0.644	0.659	0.482
Marital status						
Never married	0.224	0.231	0.231	0.240	0.230	0.191
Married or cohabiting	0.660	0.644	0.646	0.654	0.654	0.701
Not married	0.115	0.126	0.123	0.106	0.116	0.108
Education						
GCSE & A-Level	0.600	0.569	0.540	0.553	0.575	0.618
Degree & Higher	0.190	0.203	0.252	0.224	0.209	0.238
Other qualification	0.117	0.123	0.138	0.137	0.126	0.107
Residence region (*Rural*)	0.212	0.247	0.204	0.247	0.226	0.262
Receiving benefits	0.283	0.414	0.288	0.306	0.314	0.271
No of observations	1,066	473	341	571	2,451	568

Notes: The average column shows the average values for all the treatment cohorts, including those that first received treatment in 2016, 2017, 2018 and 2019. On the other hand, the comparison column provides the average values for the group of workers in the comparison (never-treated) group as defined in the identification strategy section. ***, **, and * indicate significance at 1%, 5% and 10% levels, respectively; Income is monthly personal income after tax; the row 'Receiving benefits' indicates the proportion of individuals in both groups that were receiving at least one of the work-related frozen benefits. The 'No of observations' row reports the baseline observation for the entire treatment cohorts and the comparison (never-treated) group before the first treatment occurred in 2016.

5.1. The group–time average treatment effects

The estimated treatment effects of the NLW policy on mental health are summarised in Table 2. We considered the treatment effects estimates under the unconditional parallel trend assumptions (Panel A) and conditional on the covariates (Panel B). The *'p-value'* shows the Wald pre-test of the parallel trends assumption, and the results indicate that the parallel trend assumption holds with and without including the covariates in the treatment effects estimation. The estimated p-value show 0.1122 and 0.1035, respectively and both are larger than the 0.05 significance threshold, suggesting that the parallel trends assumption holds in the pretreatment periods.

The aggregate group-time average treatment effects show positive coefficients suggesting that the cumulative mental health effects of the NLW policy are positive. In metrics terms, the simple weighted summary parameter of the average treatment effect suggests that the MCS score by about 0.79 index points for those in the treatment cohorts compared to the comparison group following the NLW policy between 2016 and 2019. Also, the *'Cohort'* row in Table 2 summarises the effect of the NLW based on all individuals that received treatment during each treatment period. For example, the 2016 cohort is defined as the group of eligible workers when the NLW policy was first introduced in 2016. The estimates show supportive evidence of the positive mental health effects of the NLW policy for each treatment cohort.

The *'Time'* row summarises the treatment effects by the length of time the NLW policy has been in place. The estimated results show that the cumulative effects of the NLW policy on mental health are positive. In summary, the disaggregated treatment effects estimate by cohort and time show consistency in the positive mental health effects of the NLW policy across the different treatment cohorts and period they became treated. The estimates also suggest a dynamic effect of the NLW policy on mental health, with an estimated magnitude of the impact across the intervention groups cumulatively increasing with the length of the period each cohort received treatment.

Table 2: Treatment effects estimates of the NLW policy

Outcome: MCS	***2016***	***2017***	***2018***	***2019***	***Single parameter***
Panel A: Unconditional					
weighted average					0.7888**
					(0.3578)
Cohort	0.6119	1.2721	0.5924	1.1760	0.8687**
	(0.5240)	(0.6029)	(0.7232)	(0.6490)	(0.3158)
Time	0.2560	0.4548	0.7402	1.2251**	0.6690
	(0.6113)	(0.5234)	(0.5132)	(0.4942)	(0.3504)
P-value			0.1122		
Panel B: Conditional					
weighted average					0.9705**
					(0.3788)
Cohort	0.8015	1.4524	0.8792	1.2082	1.0292**
	(0.5802)	(0.6682)	(0.7983)	(0.6911)	(0.3348)
Time	03090	0.7549	0.8985	1.4134**	0.8440**
	(0.7050)	(0.6547)	(05569)	(0.5476)	(04076)
P-value			0.1035		

Note: The Table summarises the group-time treatment effect parameters under conditional and unconditional parallel trends assumptions, that is, with and without including the covariates, using the estimation method from Callaway and Sant'Anna (2021) and implemented by their '*did'* R package. The *'weighted average'* row reports the weighted average using the cohort size for all the group-time average treatment effects. The '*cohort'* row summarises average treatment effects by the timing of each NLW upratings, with *g* indexing the year each cohort first becomes treated. The '*Time*' row reports average treatment effects by calendar year and *t* indexes the year. The 'single parameter' column provides the aggregation based on each type of parameter, cohort and time. Standard errors are in parenthesis, and ** indicates that the simultaneous 95% confidence band of the estimate does not cover 0 and is thus statistically significant. The p-value denotes the probability values for the Wald test of parallel trend assumption as reported by the `*att_gt*` function from the '*did*' package (see Callaway & Sant'Anna, 2021).

5.2. The impacts of the working-age benefits freeze policy

The introduction of the NLW policy in 2016 coincided with the UK government's commencement of a four-year freeze on working-age benefits. Although the NLW aimed to increase income, its simultaneous introduction and implementation with the benefits freeze program could disproportionately affect low-paid workers. Besides, the government's attempt to reduce reliance on benefits and shift the costs burden to employers through higher wages could worsen the precarious conditions of low-income workers. Hence, we evaluate the impacts of the benefits freeze programme on the mental health effects of the NLW policy. To achieve this, we re-estimate the group-time average treatment effects separately for the group that were receiving any of the frozen working-age benefits and, as a result, were affected by the benefits freeze policy and the other group that were not on any of the frozen benefits.[5] The estimated average group-time treatment effects are summarised in Table 3. Panel A shows the average treatment effects across treatment cohorts and calendar years for the workers that

[5] For robustness purposes, we estimate the average treatment effects of the working-age benefits freeze policy on mental health. The results show negative and significant effects of the policy on the mental health of the affected workers, particularly those eligible for the NLW (see Appendix 1).

reportedly received at least one of the welfare benefits affected by the freeze policy. The results show mixed signs of the treatment effects across the treatment cohorts. However, none of the estimated single parameters, which aggregate overall treatment effect parameter across cohorts and periods of exposure to treatment, is significant. Thus, suggesting that the mental health effects of the NLW are not significant for the group of workers receiving any of the affected benefits.

Similarly, the estimated average group-time treatment effects for workers that reportedly did not receive any of the affected frozen benefits are summarised in Panel B. The results show positive estimates for the single parameter across all treatment cohorts and the periods they became treated. The simple weighted average and summary parameters estimates across the cohorts and time are significant, and they show supportive evidence that the mental health effects of the NLW policy are positive and significant for the group of workers that did not receive any of the frozen benefits.

Table 3: NLW treatment effects – receiving vs not-receiving work-related benefits

Outcome: MCS	***2016***	***2017***	***2018***	***2019***	***single parameter***
Panel A: Receiving benefits					
weighted average					0.1901
					(0.5187)
Cohort	0.0780	0.3937	-0.9983	1.5798	0.4099
	(0.8545)	(0.9013)	(1.2427)	(1.1247)	(0.5346)
Time	0.3663	-0.4927	-0.3200	0.8542	0.1019
	(0.9972)	(0.7674)	(0.8508)	(0.7622)	(0.5352)
P-value			0.3023		
Panel B: Not receiving benefits					
weighted average					1.5287**
					(0.4578)
Cohort	1.5046	1.5514	1.6205	1.6022	1.5477**
	(0.5979)	(0.8135)	(0.9975)	(0.8485)	(0.3976)
Time	1.3841	1.4161	0.9761	2.1113**	1.4719**
	(0.6937)	(0.6704)	(0.6330)	(0.6397)	(0.4647)
P-value			0.8398		

Note: The Table summarises the group-time treatment effect parameters for individuals affected by the benefits freeze and those that do not, using the estimation method from Callaway and Sant'Anna (2021) and implemented by their '*did'* R package. The treatment effects are estimated under the unconditional parallel trend assumptions without including the covariates. The *'weighted average'* row reports the weighted average using the cohort size for all the group-time average treatment effects. The '*cohort'* row summarises average treatment effects by the timing of each NLW uprating with *g* indexing the year each cohort first becomes treated. The '*Time*' row reports average treatment effects by calendar year and t indexes the year. The 'single parameter' column provides the aggregation based on each type of parameter, cohort and time. Standard errors are in parenthesis, and ** indicates that the simultaneous 95% confidence band of the estimate does not cover 0 and is thus statistically significant. The p-value denotes the probability values for the Wald test of parallel trend assumption as reported by the `*att_gt*` function from the '*did*' package (see Callaway & Sant'Anna, 2021).

Figure 2 depicts the event-study aggregation of the treatment effects estimates based on the time each cohort was treated for the two groups. The event time is expressed as the time elapsed

since the NLW was first introduced in 2016. The estimated effect at period 0 provides the instantaneous treatment effect, that is, the average effect of the NLW across all the treatment cohorts when they first got treated. Similarly, the length of periods equal to -1 and 1 respectively correspond to the one period immediately before and after when the treatment cohorts first participated in the treatment. The plot shows that the simultaneous confidence band for the estimated coefficients in the pretreatment periods include 0, which suggests that the null hypothesis that the parallel trend assumption holds in all the periods before treatment cannot be rejected. Hence, the pretreatment trends in mental health outcomes in the treatment cohorts and the comparison groups are similar. This also suggests that the comparison group is a suitable control for the units in the treatment cohorts.

Furthermore, Figure 2b confirms that the mental health effects of the NLW policy are positive and increase in magnitude in the post-treatment periods for the group unaffected by the benefit freeze policy. The post-treatment average effect shows positive and significant impacts in periods after treatment, suggesting positive and increasing effects of the NLW policy on the mental health of the affected workers. Overall, the results suggest that the net positive effects of NLW on mental health could have been eroded by the contractionary fiscal and austerity policies that affected and reduced the social benefits components of people's income. Although the separate analyses of the estimated treatment effects for the two categories of workers separated by their benefits statuses do not directly provide the mechanisms through which the working-age freeze policy affects the mental health effects of the NLW policy, our finding is consistent with earlier reports indicating that low-income workers are disproportionately affected by the benefits freeze policy (Barnard, 2019). Besides, the non-significant estimates for the groups affected by the benefit freeze show the systemic perspective that the mental health effects of the annual incremental additions to basic wage are just as limited or as enabled by the prevailing wider socio-economic and existing welfare policy structures. More importantly, these policy structures largely affect low-wage earners who rely on the welfare benefits system to subsidise their low income (Carr et al., 2016). Our findings also align with past studies that found the austerity and contractionary policies as the choice of the UK's government economic response to the GFC crises as questionable and at high risk to health and wellbeing (see Reeves et al., 2013).

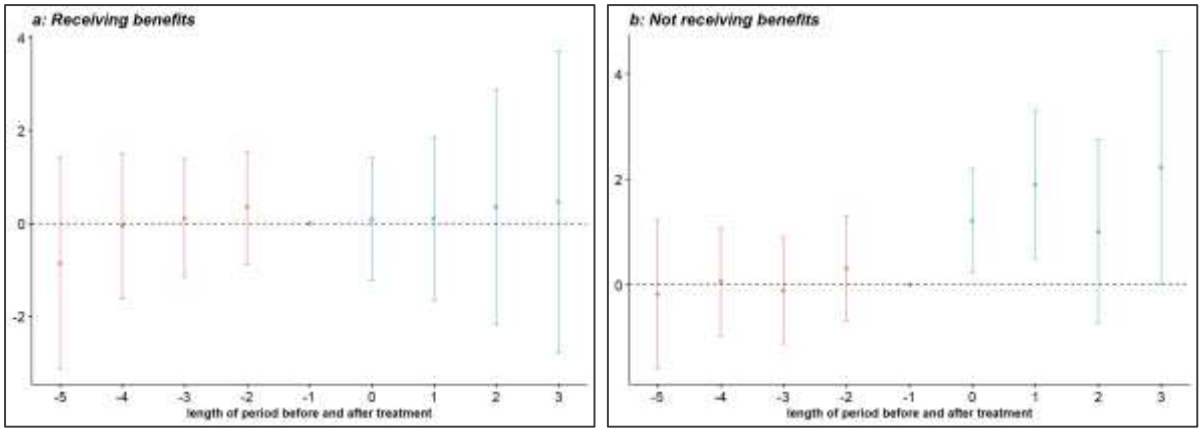

Figure 2: Average treatment effects by the length of exposure to treatment.
Note: The figure shows the dynamic average treatment effects aggregated by event time for the two groups: those affected and unaffected by the benefit freeze policy. The red lines present the point estimates and the 95% confidence bands for the pretreatment periods. Blue lines are the point estimates of the NLW on mental health, and the lines represent their 95% confidence bands.

5.3. Robustness checks and additional results

5.3.1. Sensitivity analysis of the parallel trend assumption

Next, we conduct the sensitivity analysis of our results to violations of the parallel trend assumption. We follow the standard practice in the literature by conducting some robustness checks to evaluate the validity and robustness of the estimated results. First, we evaluate the sensitivity of our DID designs to possible violations of the parallel trend assumption. The estimated Wald test statistics presented and discussed in the main results provide a statistical check on whether the parallel trends assumption holds in the periods before the treatment cohorts become treated. The event-study plots also confirm whether the assumption of a parallel trend between the treatment cohorts and comparison group holds before treatment. However, the Wald tests and event study plots do not provide information on whether the parallel trends actually hold in the post-treatment periods (Callaway & Sant'Anna, 2021; Rambachan & Roth, 2022). Therefore, we evaluate the sensitivity of our estimated group-time average treatment effects to possible violations of the parallel trends in the post-treatment periods. Specifically, we considered the sensitivity of the event-study estimates to violations of parallel trends.

We followed the approach Rambachan and Roth (2022) proposed, given its strengths in addressing issues related to making inferences without relying on the exact pretreatment parallel trends assumption. The method incorporates statistical uncertainty about the estimated coefficients and the strength of causal conclusions inferred from the estimations. We also assume a relative magnitude bound that bounds the maximum post-treatment violations of the parallel trends based on the observed violations in the pretreatment periods (see Rambachan &

Roth, 2022). Additionally, the aggregate group-time event-study estimates may be biased by other socioeconomic and welfare policies and programmes that occurred during the periods under investigation and may confound the estimated mental health effects of the NLW policy. These policies include the benefits freeze and other welfare reforms implemented between 2016 and 2019. Hence, we further imposed a negative restriction in the sensitivity analysis to account for the additional bias restriction in the sensitivity analysis.

Figure 3 shows sensitivity plots of the estimated robust confidence set in the periods after treatment using the significant post-treatment estimates from Figure 2. The plots show that the robust confidence intervals allow for up to 50% of the maximal pretreatment violation in parallel trends in the post-treatment periods (see Figure 3). Overall, the sensitivity plots suggest that the estimated average treatment effects of the NLW policy on mental health for the group not affected by the benefit freeze policy are valid and robust to parallel trend assumptions, provided the relative magnitudes of post-treatment violation of parallel trends are below the pretreatment violations by up to 50%.

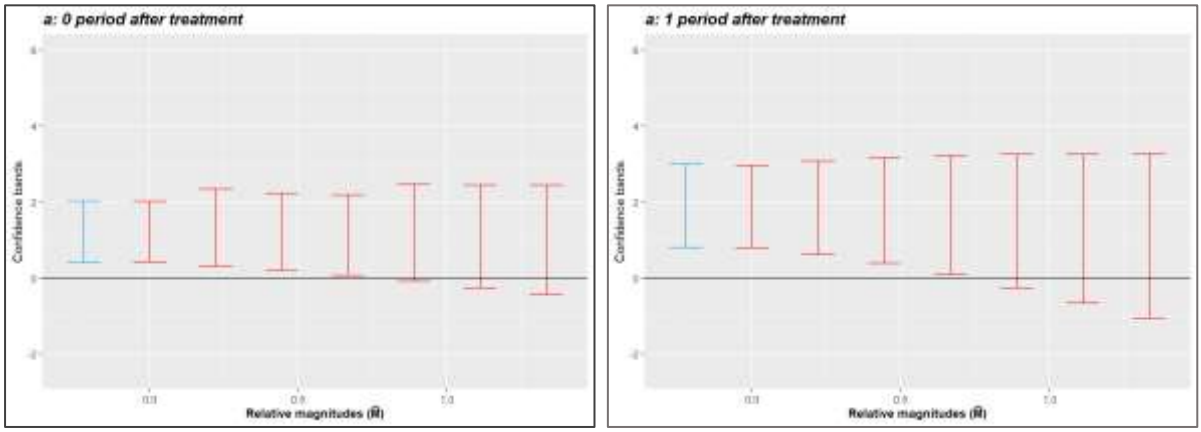

Figure 3: Sensitivity analysis plot

Note: The figure shows the sensitivity analysis plot using the event study from the estimated aggregate group-time average treatment effects. The plot is based on the "relative magnitude" restrictions. The blue line indicates the estimated event-study confidence intervals one period after treatment, as reported in Figure 2. The red lines show the confidence intervals for different consecutive values of the relative magnitudes. The point where the red line crosses 0 indicates the maximum allowed relative magnitudes of the post-treatment violations in parallel trends based on observed violations in the pretreatment periods.

5.3.2. The NLW policy effect on the labour market and wellbeing outcomes

In this section, we evaluate the NLW policy effect on some selected labour market and wellbeing outcomes, particularly those that could serve as potential mechanisms linking wage policy to mental health. As discussed in the review section, there are interconnections in the pathways linking wage policies to health outcomes. Consistent with previous literature, we considered the effects of NLW policy on two labour market outcomes, self-reported earned income and work hours, and two aspects of work-related wellbeing: job satisfaction and satisfaction with leisure time. The estimated heterogeneous treatment effects are summarised in

Table 4.

Panel A in

Table 4 shows the estimated average treatment effects on monthly personal earned income disaggregated across the treated cohorts and the period they became treated. Consistent with the findings by Aitken et al. (2019) that the NLW introduction is associated with growth in real wages of affected workers, our estimated results show that the introduction and upratings in the NLW lead to significant positive effects on the monthly personal income of the affected workers. Similarly, the estimated results on report hours worked by the affected treatment units summarised in panel B of

Table 4 show that the cumulative effect of the NLW policy is positive and significant on their reported work hours. The policy effects on job satisfaction and satisfaction with leisure time for the affected workers are summarised respectively in panels C and D of

Table 4. Following a large body of literature that has employed self-reported measures as a construct of wellbeing (e.g., Akanni et al., 2022; Flint et al., 2014; Gülal & Ayaita, 2020; Kuroki, 2018), we collect data on the job and leisure time satisfaction from the UKHLS using the Likert scale from 1 to 7 ranging from “completely dissatisfied” to “completely satisfied”. The two variables are then rescaled to standardised values using zero mean and one standard deviation for ease of interpretation. The treatment effects estimates show significant positive effects on job satisfaction for the affected workers following the introduction and subsequent upratings in the NLW. This finding is also consistent with previous literature that the minimum wage policy positively affects job satisfaction and other dimensions of wellbeing (Gülal &

Ayaita, 2020). However, the estimated results show insignificant NLW effects on leisure time satisfaction.

Table 4: NLW policy effects on labour market and wellbeing outcomes

	2016	*2017*	*2018*	*2019*	*single parameter*
Panel A: Earned income					
Weighted average					0.1062**
					(0.0169)
Cohort	0.0985**	0.0797**	0.1715**	0.1183**	0.1131**
	(0.0244)	(0.0266)	(0.0296)	(0.0301)	(0.0157)
Time	0.0526	0. 0379	0.1070**	0.1658**	0.0908**
	(0.0278)	(0.0244)	(0.0255)	(0.0234)	(0.0169)
P-value			0.1101		
Panel B: workhours					
Weighted average					2.6108**
					(0.3972)
Cohort	2.8397**	2.368**	2.0579**	2.3655	2.5061**
	(0.5735)	(0.6670)	(0.7575)	(0.6705)	(0.3544)
Time	1.6091	2.2468**	2.7714**	3.1066**	2.4335**
	(0.6481)	(0.5784)	(0.6115)	(0.5359)	(0.4074)
P-value			0.0278		
Panel C: Job satisfaction					
Weighted average					0.0958**
					(0.0360)
Cohort	0.0667	0.2305*	0.0819	0.0327	0.0879**
	(0.0521)	(0.0637)	(0.0679)	(0.0577)	(0.0318)
Time	-0.0121	0.1296*	0.1323*	0.0928	0.0857**
	(0.0623)	(0.0496)	(0.0500)	(0.0479)	(0.0374)
P-value			0.1089		
Panel C: Leisure satisfaction					
Weighted average					0.0202
					(0.0347)
Cohort	-0.0089	0.0639	0.0854	0.0256	0.0287
	(0.0545)	(0.0573)	(0.0679)	(0.0602)	(0.0341)
Time	-0.0552	-0.0078	0.0356	0.0545	0.0068
	(0.0631)	(0.0555)	(0.0500)	(0.0453)	(0.0373)

P-value	0.8404

Note: The Table summarises the group-time treatment effect of NLW on earned income, work-hours job and leisure satisfaction. The treatment effects are estimated under the conditional parallel trend assumptions with the covariates, including age, age-squared, gender, marital status, educational qualification, and region of residence. Standard errors are in parenthesis, and ** indicates that the simultaneous 95% confidence band of the estimate does not cover 0 and is thus statistically significant. The p-value denotes the probability values for the Wald test of parallel trend assumption as reported by the `*att_gt*` function from the '*did*' package (see Callaway & Sant'Anna, 2021).

Figure 4 depicts the event study aggregates and the simultaneous confidence bands for the estimated coefficients for each outcome. The positive and significant policy effects on income, work hours and job satisfaction lend support to our main findings that the NLW introduction and upratings lead to a cumulatively positive effect on mental health. The findings relate to the psychosocial and workers' decision-making pathway linking minimum wage policy to health and wellbeing (see Leigh et al., 2019). First, the significant effect estimates for earned income and affected workers' job satisfaction corroborates our main results. They reflect the psychosocial hypothesis that increased job satisfaction is strongly correlated with improvements in mental health, depression and other psychological health problems (Faragher et al., 2005). Secondly, the results suggest a substitution effect between work hours and leisure. The positive and significant effect on work hours and the non-significant policy effects on leisure satisfaction reflect workers' tradeoff between work hours and the amount of time devoted to leisure following the NLW policy. Finally, contrary to the hypothesis that increased wages lead to a reduction in available working hours, empirical evidence from the UK shows no evidence that the UK minimum and living wage policies negatively affect work hours (see Capuano et al., 2019; Connolly & Gregory, 2002).

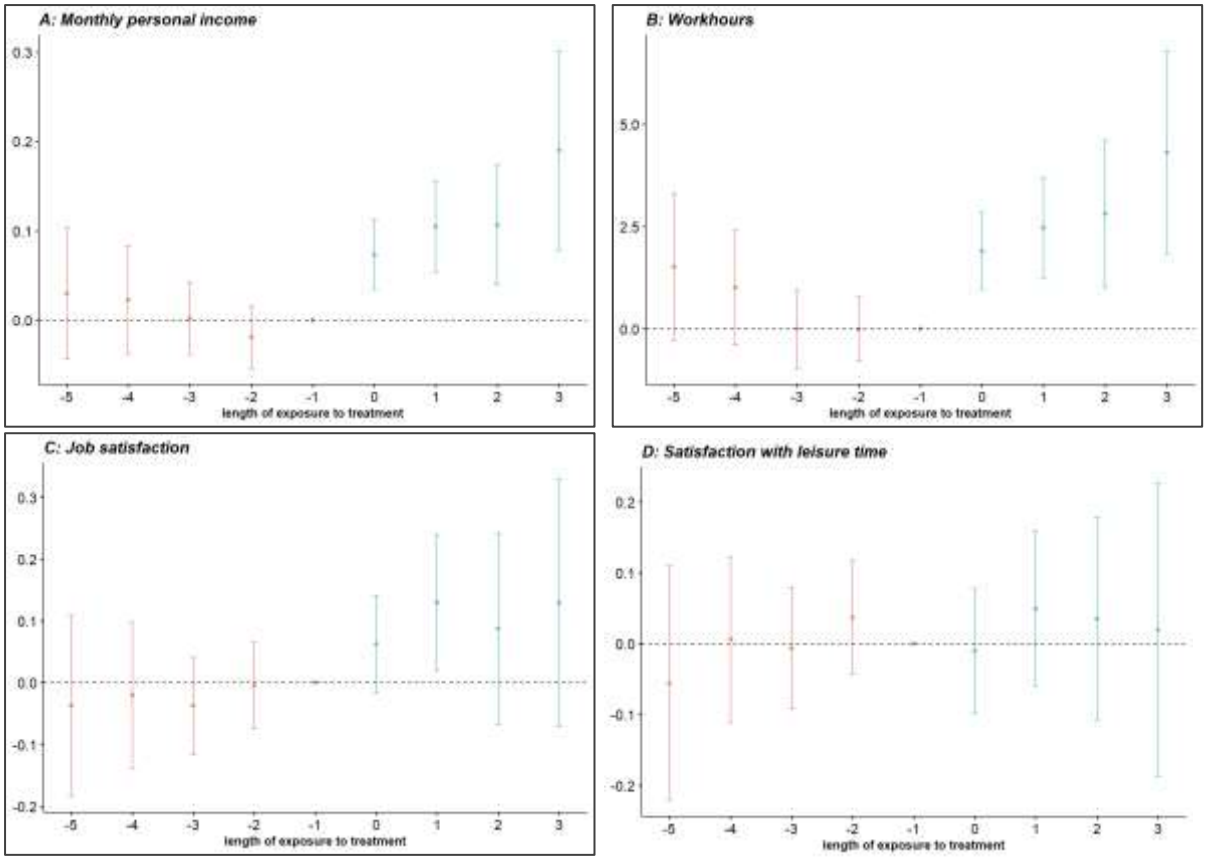

Figure 4: Aggregate treatment effects on labour market and wellbeing outcomes
Note: The figure shows the dynamic average effects of the NLW policy aggregated by event time on the selected labour market and work-related wellbeing outcomes. The red points and the lines present the point estimates and the 95% confidence bands for the pretreatment periods, respectively. Blue lines are the point estimates of the NLW on mental health, and the lines represent their 95% confidence bands.

6. Discussion and conclusion

This paper investigates the impact of the UK's national living wage policy between 2016 and 2019 on mental health using the heterogeneous difference-in-differences setting that estimates the disaggregated and interpretable impact of the policy. The estimated group-time aggregate treatment effect results show positive effects of the wage policy on mental health. The event-study aggregate also shows a positive instantaneous effect of the NLW policy on mental health. The positive effect is significant and cumulatively increasing over the considered length of treatment exposure. These findings suggest that a sustained increase in the marginal additions to wage floors could lead to significant changes and improvements in mental health outcomes. Our finding is similar to Reeves et al. (2017), who also document a significant effect of the UK 1999 NMW on mental health. Also, Kronenberg et al. (2017) conclude that a larger increase in wages could lead to improvements in mental health. On the contrary, our findings differ from Maxwell et al. (2022) conclusion that the cost-benefit analysis of the wage policy should not include the health effects.

Additionally, we find that the positive effects of the NLW policy on mental health are constricted by the counteracting benefits freeze policies, which stagnate or reduce the affected workers' income. The contractionary impacts of the working-age benefit freeze policies resulted in a decline or zero net additions to income. Thus, they might have cancelled out the positive benefits of wage policies, especially on the mental health outcome. Nonetheless, the NLW seems to have achieved its intended objective of reducing the reliance of low-income workers on benefits. Although admittedly restrictive, the NLW objective as set out by the government was primarily to cut the size of welfare benefits by shifting costs to employers through increased wages while also preventing the precarious situation of low-income workers from further degradation. More importantly, the increase in minimum wage floor over the years is plausibly not high enough to provide the minimum living standard and help low-income families to build better lives without the additional supplementation (see also Davis et al., 2021).

Overall, our findings suggest that positive improvements in mental health due to increased wages are better achieved when accompanied by other interventions that lead to income gains and increased earnings (or at least prevent compensating income losses) for affected workers. For example, Rothstein and Zipperer (2020) find that the minimum wage policy in the US, which provide the lowest guaranteed wage floor for workers across different US states and regions, can be augmented by the Earned Income Tax Credit (EITC) policy, which also provides a refundable tax credit to low-income working individuals and households. These socioeconomic and welfare policies toward income expansions worked together with wage policies to improve the low-wage workers' situations.

Finally, our results support living wage campaigns that wage floor determination should encompass a broader consideration of the prevailing welfare systems and policies that could effectively undermine or augment low earnings. Rather than considering wages increase and welfare benefits as alternatives, the two are complementary. Besides, their prospects of reducing poverty and generating liveable income for families may be more effective in combination rather than reducing one for the other. Such tradeoffs risk diluting the effectiveness of wage policies. Overall, our study contributes to the importance of evaluating the health impacts of wage floor changes amid other counteracting welfare policies. Future research should consider the health impacts of the interaction between wage policies and other complementary or conflicting income-related programmes and policies using data from the UK.

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Appendix 1 The effects of the working-age benefits freeze policy

Table A 1: Mental health effects of the working-age benefits freeze

	Full sample	**With NLW**	**Without NLW**	**Full sample**	**With NLW**	**Without NLW**
Treatment	-0.6112**	-0.7059***	-0.6113	-0.5549*	-0.6278*	-0.6452
	(0.2912)	(0.3290)	(0.6394)	(0.2911)	(0.3295)	(0.6408)
Covariates				Y	Y	Y
Time fixed-effects	Y	Y	Y	Y	Y	Y
Group fixed-effects	Y	Y	Y	Y	Y	Y
No of Obs.	17672	13272	4400	17342	13027	4315

Note: The Table summarised the average treatment effects estimates of the benefits freeze policy using the two-way fixed effects estimator and implemented using the heteroskedasticity robust estimator, '*reghdfe'* Stata package developed by Correia (2019). The dependent variable is the Mental Component Summary of the 12-item Short-Form Health Survey. The treatment effects are estimated with and without including the covariates. The *'Full sample'* columns present the estimated treatment effects results for the full observations used in the main analysis, while the *'With NLW'* columns restrict the samples to workers in the NLW treatment groups and vice versa for the *'Without NLW'* columns. Clustered robust standard errors are presented in parenthesis, while ** and *** indicate statistical significance at 5% and 1% levels, respectively.