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RESEARCH ARTICLE

# 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 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 and the comparison group definition. 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 the way the entire suite of policy instruments operates on earnings and liveable income for affected low wage workers.

## INTRODUCTION

The policy paper that sets out the National Living Wage (NLW) by the UK national government conceptualized the national living wage as an essential part of ensuring that low-wage workers take a greater share of the gains from the growth and expansion experienced in the economy relative to similar developed economies after the 2008 Global Financial Crisis (GFC). The NLW policy aimed at ensuring that work pays by reducing reliance on the government supplementing earnings through the benefits system (Department for Business Energy and Industrial Strategy, 2016). 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 labor market and employment outcomes. For example, the Low Pay Commission, the independent body responsible for advising the UK national government about the national minimum and living wage rates, did an impact review of the National Living wage between 2015 and 2020. The report considers outcomes including employment, impact on 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 behaviors. Past empirical evidence on the health effects of minimum wage policy is largely concentrated in the U.S. (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), and OECD and developing countries (Lenhart, 2017b; Ponce et al., 2018). Various health outcomes and health behaviors have also been considered, including physical and mental health, smoking behavior, 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, the same data sources, and comparable health outcomes.

In addition to the NLW introduction, the UK government also implemented a 4-year freeze on several working-age welfare benefits and tax credits during the 2015 national budget announcement. The 4-year benefits freeze was part of the national government's series of welfare reforms aimed at supporting efforts to increase employment, reward hard work, and increase fairness to working households by reducing workers' dependence on state benefits (Kennedy, 2015). The Social Security legislation in the UK requires an annual review of certain welfare benefits to ensure that they retain their real values relative to prices. However, the 2016 Welfare Benefits freeze policy introduced a freeze to the annual increase in income-related welfare supports and tax credits. By implication, the value of support received by affected individuals was maintained at the 2015 rate, rather than receiving the annual uprating with inflation. As a result, while the value of benefits received remains the same in nominal terms, the value reduced in real terms, provided the claimants continued to meet the eligibility conditions. The benefits freeze impacted a range of different welfare benefits, including Income Support, Housing Benefit, Jobseeker's Allowance, Employment and Support Allowance, Child and Working Tax Credits, and the Universal Credit. The simultaneous implementation of the NLW with the freeze on working-age benefits largely led to a decline in the gross earnings of affected individuals (Barnard, 2019). Additionally, the negative effects of the freeze could choke off the positive benefits of the NLW.

We make two main contributions to the literature on economic policies and health nexus. First, we consider the mental health effects of the UK's NLW and the benefits freeze policies. Literature in the past has often considered wage policy to be unrelated to the expansion or contraction of other safety net programs (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). Also, the availability and generosity of other safety net programs work together with a minimum wage increase to enhance income and reduce deaths of despair (Dow et al., 2020). Our study adds to the small number of studies that consider the health effects of the 2016 NLW and subsequent unrating (see Maxwell et al., 2022).[1] Our choice of mental health outcome is premised on its immediacy, and the relative ease with which one can attribute mental health outcomes to income

[1] The 1999 National Minimum Wage was a flagship UK national government wage policy reform which set the minimum hourly wage rate at £3.60 for adult workers over the age of 22 and £3.00 for those aged 18 to 22 years. On the contrary, the NLW coverage is restricted to workers aged at least 25 years.

and poverty related policy actions. Mental health symptoms can be assessed with or without taking physical measurements; in this study, they have been reasonably and reliably measured through various survey instruments. Further, poor mental health 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). The costs associated with mental health disorders in the UK—including only broad mental conditions that meet diagnosable thresholds of certain mental conditions and 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.

Secondly, we contribute to existing evidence by employing an estimation approach that accommodates the dynamics of the NLW, which has a changing annual minimum wage thresholds and new workers becoming eligible annually. We follow recent developments in the difference in differences (DID) setup, which allows the identification and estimation of policy-relevant disaggregated and interpretable causal parameters (see Callaway & Sant'Anna, 2021). Recent literature that investigated the health effects of the UK NLW employed the canonical DID method by assuming that each wage uprating followed separate parallel paths over time (see Maxwell et al., 2022). Additionally, previous studies that explored the heterogeneous effects of wage policies mostly focused on labor market outcomes, mostly in the U.S. (Cengiz et al., 2019; Dube et al., 2016). Specifically, 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 by only examining yearly increases in wage rates (Borusyak et al., 2021; Redmond & McGuinness, 2022).

Our findings show that the introduction of and subsequent annual upratings in the national living wage positively affect mental health outcome. However, the significance of the average treatment effect is dependent on the choice of mental health measure and methodology. 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 work-related well-being 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. The rest of the paper is structured as follows: the next section discusses some of the socioeconomic and welfare reforms in the UK preceding the introduction of the NLW in 2016. The section "Literature Review – Wage Policy and Health" provides a review of literature on wage policy and health, while "Data and Method" describes the data and empirical methods. "Empirical Results" presents the results, and "Discussion and Conclusion" concludes the paper.

## 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 these households. These challenges may be connected to 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 away from reliance on benefits and public support (Alvarez-Vilanova, 2018). For example, the government's main objective in introducing the 4-year freeze on working-age benefits between 2016 and 2020 was to ensure that growth in earnings overtakes growth in benefits and, therefore, make it financially better

for people to work rather than claiming benefits (Kitara, 2016). Additionally, the government intended 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 deteriorating impacts on low-wage workers and poor households. For example, the cumulative effects of major welfare reforms before 2017, including the benefits caps, localization 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, Davis et al. (2021) evaluated the extent to which the NLW and the Universal Credit (UC) could facilitate achieving a minimum living standard for the UK population. They showed that the costs of living increased at a higher rate than the increase in the UC. The authors also found that 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 well-being largely suggests that these policies and programs 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 Universal Credit Policy. Katikireddi et al. (2018) investigated 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.

Moreover, these reforms do not have equal effects on all groups. For example, the cuts to local government budgets implemented in 2010 hit the poorest parts of the country hardest (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). In addition, because beneficiaries are usually not well organized 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 characterized by austerity and large-scale cuts in government funding. Additionally, the NLW was estimated to facilitate a direct wage boost for about 2.7 million low-wage workers aged 25 and above, and up to 6 million people receiving pay rise as a result of the NLW (Office for Budget Responsibility, 2016). 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.

## LITERATURE REVIEW – WAGE POLICY AND HEALTH

Empirical research has consistently demonstrated that income affects health and health behaviors through various channels. These channels can be broadly organized into three categories. The first is through countries' national income, 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 well-being (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 behaviors 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 labor market outcomes. Issues widely considered include employment, work hours, poverty, income inequality, job automation, and job quality, both in commissioned studies and impact assessment reports (see Dube, 2019; Low Pay Commission, 2022). However, 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) documented 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, whose 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, all of which are 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 Leigh, 2021a).[2] 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 U.S., and Vietnam (among others with variegated and spatial clustering of minimum wage policies allowing different states or regions to set their own minimum wage), previous studies evaluating the effects of minimum wage policies have largely explored variations in implementation across and within different 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 with wage policies that are centrally determined and binding nationally, have delineated 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.

[2] Studies have also deployed randomized control trials to study other aspects of income interventions on health outcomes such as the universal basic income (Gibson et al., 2020). The other quasi-experimental approach employed in previous literature is the regression discontinuity approach (Dickens et al., 2014).

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. In contrast, Reeves et al. (2017) and Lenhart (2017a) evaluated similar NMW policy experiments and their findings showed 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 assumed 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 whose gross earnings include other components of wages such as overtime premiums and bonuses (Stewart & Swaffield, 2002). Both Kronenberg et al. (2017) and Lenhart (2017a) exploited the question in the BHPS that asked participants whether they received increased wages to comply with the UK's 1999 NMW policy, allowing a cleaner identification of workers who were actually treated and those in the control group.

Recently, Maxwell et al. (2022) found that the effects of the 2016 to 2018 increase in UK NMW on self-reported health outcomes are insignificant. By assuming that each wage uprating followed a separate parallel path over time, the authors estimated multiple difference-in-differences regressions. However, while this approach is simple and provides the instantaneous health effects of the wage policy (Stewart, 2012), it does not provide effects in successive periods. Also, there could be variations in the treatment effects for individuals treated in different years and over different lengths of time. Overall, adopting an identification approach that follows the canonical DID setup in estimating the treatment effect 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. (2023) concluded 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, 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. According to the authors, "eliminating the undesirable comparisons seems to be a better approach than diagnosing the extent of the issue" (Roth et al., 2023, p. 18).

# DATA AND METHOD

## Data source

We collected data from different 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 followed over time. The applicability of the UKHLS in policy research has 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 harmonizing it into corresponding financial years between 2013 and 2019 to ensure the sample is nationally representative (Kaminska & Lynn, 2019). The data also provide information on the actual interview dates.

Given that the introduction and subsequent uprating of the NLW are effective in April every year, we harmonize the data to define a financial year lasting from April to March of each successive year. More importantly, the survey collects detailed data on respondents' age and basic hourly wages. We use this information to identify individuals eligible for receiving the treatment as well as those who do not

and thus form the comparison group in our analysis. We restrict our analysis to workers aged between 25 and 65 in each treatment period to include only those individuals who met the NLW eligibility condition of age 25, while excluding workers eligible for state pension benefits, which started at age 66 during the study periods.

## The difference in differences with heterogeneous treatments

The NLW policy has multiperiod and multigroup dynamics, given that it was introduced in 2016 for workers above age 24 with a new rate introduced in April of every subsequent year, as well as additional eligible individuals who reached the minimum age threshold and earning below the basic wage rate. For example, a worker aged 24 years and above who earned below £7.20 pounds (the NLW introduction rate) in 2015 but earns at or above £7.20 in 2016 is delineated as treated in the 2016 cohort. In the same vein, when a worker aged 24 and above earns below £7.50 in 2016, but their reported earnings increase to £7.50 or above in 2017, they are classified among the 2017 treatment cohort. By implication, new workers become eligible for treatment every successive period. As such, our choice of estimation approach deviates from the commonly used methods to evaluate policy interventions involving two periods and two groups, which is usually the canonical difference in difference method or two-way fixed effects (TWFE) estimation (de Chaisemartin & D'Haultfœuille, 2022b). The typical TWFE specification employed to estimate the average treatment effect could be specified as:

$$Y_{i,g,t} = \alpha_g + \lambda_t + \beta_{twfe} D_{g,t} + \varepsilon_{g,t} \tag{1}$$

where $Y_{i,g,t}$ is the outcome for individual $i$ in group $g$ at period $t$. $\alpha_g$ is the vector of group fixed effects, $\lambda_t$ is the period fixed effects, and $D_{g,t}$ is the treatment in group $g$ at period $t$. $\beta_{twfe}$ denotes the treatment in group $g$ at period $t$. However, recent literature has shown that treatment estimates using $\beta_{twfe}$ may provide biased estimates when treatment varies across groups and time. Hence, to estimate the heterogeneous treatment effects of the NLW on mental health, we followed the estimation procedure of the treatment effects with identification conditions involving multiple treatments cohorts and variations in the timing of their treatments. Various estimators have been proposed in methodological literature that are capable of handling treatment estimates when the design is staggered (see Borusyak et al., 2021; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2022a; Sun & Abraham, 2021).[3] In our analysis, we employ the estimator proposed by Callaway and Sant'Anna (2021). The estimator allows for the evaluation of heterogeneous treatment effects of the NLW policy, providing its disaggregated and cumulative mental health impacts across treatment cohorts and 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 $\{Y_{i1}, Y_{i2}, \ldots, Y_{iT}, X_i, D_{i1}, D_{i2}, \ldots, D_{iT}\}_{i=1}^{n}$ 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 1 indicating an individual in the treatment category and 0 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 Callaway and Sant'Anna (2021), 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 becomes 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 $\infty$. The treatment group, $G_g \in \{0, 1\}$, is a binary variable and equals 1

[3] For a survey of recent literature on difference in differences estimators with heterogenous treatments effects, see de Chaisemartin and D'Haultfœuille (2022b).

for an individual belonging to a group that becomes treated in period $g$ (i.e., $G_{ig} = 1\,[G_i = g]$), 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., $C_i = 1\,\{G_i = \infty\} = 1 - D_{iT}$).

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):

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

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 ($ATT(g, t)$), which accordingly is the "natural eneralization" of the average treatment effect on the treated (ATT) in the canonical DID setup with two time periods, before and after treatment.[4] This is denoted as:

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

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 different aggregated causal parameters including (i) the *simple average treatment effects*, which show 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; (iii) the average treatment effects by the length of exposure within which a group become exposed to treatment; and (iv) the event study dynamic effects.

## Identification strategy – Treatment and comparison groups

We begin with 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 labor market and on population-level mental health. Our definition of the NLW 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 et al., 2019; Vadean & Allan, 2021), and general and mental health as well as health behaviors (Kronenberg et al., 2017; Lenhart, 2017a; Maxwell et al., 2022; Reeves et al., 2017).[5] 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, was comprised of workers with basic hourly earnings below £7.20, aged between 25 and 65 years, from April 1, 2016, to March 31, 2017. Subsequent treatment cohorts comprise eligible workers

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

[5] Unlike the minimum wage policies in the U.S. and other countries that have minimum wage laws decentralised across states and regions, the NMW and NLW rates are centrally determined by the UK government. Also, the stipulated basic wage rate covers all employees who meet the eligible age condition and is also legally binding on all employers with no sector or industry exceptions. The no exception condition is also different from the U.S. minimum wage laws, which have some coverage exceptions including agricultural employees.

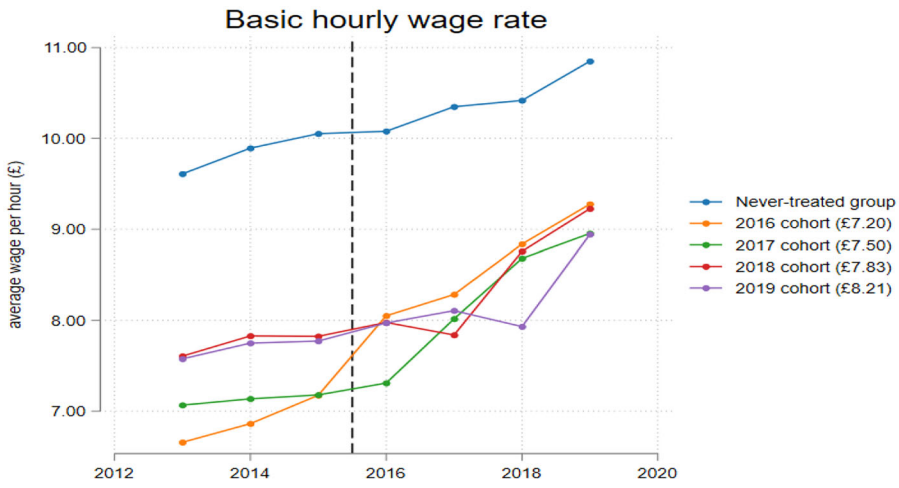


**FIGURE 1** Dynamics of the hourly wage for the treatment cohorts and never-treated group.
[Color figure can be viewed at wileyonlinelibrary.com]
*Notes*: The figure shows the average basic hourly wage for the treatment cohorts and the comparison (never-treated) group over the years under consideration. The values in the parentheses show the basic NLW wage rates cut-off used to define each treatment cohort.

earning below the uprated rates in periods before the upratings but increased after the NLW policy changes (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 worth noting that there could be instances of spillover effects of the wage changes for some category of workers earning at or above the NLW thresholds. These spillovers could occur for several reasons, including an increase in the reservation wages of all workers as more workers become aware of what constitutes fair pay (Falk et al., 2006). Employers may also wish to maintain pay differentials across their workforce to maintain workers' morale, while some may simply choose to pay above the NLW or to avoid inadvertently underpaying (Harkness & Avram, 2019). Nonetheless, the main aim of the policy is to increase earnings for workers in the lowest wage band, and it directly targets 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 spillover effects. However, the trade-off is such a comparison group might have dissimilar features from the treatment group (Stewart, 2012). Accordingly, the comparison group is comprised of workers whose hourly wage rate is equal to or above the basic rate in 2019 but not more than the annual median hourly wage in 2019, given at £13.28 (Office for National Statistics, 2022).

## 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 a shorter adaptation and 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 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

**TABLE 1** Summary statistics.

| | Treatment cohorts | | | | |
|---|---|---|---|---|---|
| | **2016** | **2017** | **2018** | **2019** | **Comparison** |
| Income | 1188 | 1131 | 1199 | 1222 | 1560 |
| Age (average) | 42.3 | 43.5 | 43.0 | 45.2 | 46.3 |
| *Below 25* | 4.6% | 4.3% | 5.7% | 4.2% | 4.8% |
| *25–29* | 15.6% | 14.3% | 15.5% | 14.5% | 8.8% |
| *30–39* | 25.1% | 23.0% | 23.3% | 17.3% | 17.4% |
| *40–49* | 24.4% | 23.3% | 22.2% | 22.0% | 24.4% |
| *50 and above* | 30.4% | 35.1% | 33.3% | 41.9% | 44.7% |
| Gender (female) | 65.1% | 68.7% | 65.7% | 66.3% | 51.9% |
| Marital status | | | | | |
| *Never married* | 23.5% | 23.1% | 25.0% | 23.4% | 17.6% |
| *Married or cohabiting* | 65.1% | 64.1% | 63.9% | 65.5% | 71.8% |
| *Not married* | 11.4% | 12.9% | 11.0% | 11.1% | 10.6% |
| Education | | | | | |
| *GCSE & A-level* | 60.8% | 53.3% | 56.4% | 55.9% | 54.8% |
| *Degree & higher* | 17.5% | 24.0% | 24.8% | 21.9% | 30.9% |
| *Other qualification* | 12.6% | 12.8% | 12.6% | 12.7% | 10.9% |
| Receiving benefits | 52.7% | 51.1% | 45.4% | 44.1% | 29.3% |
| Number of observations | 772 | 758 | 599 | 893 | 1406 |

*Notes*: The treatment columns show the averages for people that received treatment in each period in the pre-treatment years. 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. Income is monthly personal income after tax; the row "Receiving benefits" indicates the percentage of individuals across each cohort that were receiving at least one of the in-work frozen benefits. The "Number 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.

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). The 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, and role-emotional and emotional wellbeing. The MCS scores range from 0 to 100, with higher scores indicating better mental health.

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-invariant confounding. The covariates considered include age, age-squared, gender, marital status, and educational qualification.

## EMPIRICAL RESULTS

We begin this section by discussing the descriptive statistics between the treatment cohorts and the comparison group. Table 1 provides summary statistics showing the average values across each treatment cohort and the comparison group in the pre-treatment 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

**TABLE 2** Mental health effects of the NLW using TWFE estimator.

| | Without covariates | With covariates |
|---|---|---|
| Treatment effect | 0.2824 | 0.2834 |
| | (0.2464) | (0.2474) |
| Time fixed-effects | Y | Y |
| Group fixed-effects | Y | Y |
| No. of observations | 15723 | 15367 |

*Notes*: The table summarizes the average treatment effects estimates of the NLW 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. Clustered robust standard errors are presented in parentheses, while ** and *** indicate statistical significance at 5% and 1% levels, respectively.

the 2019 cohorts at £1,222, is less than the average income for the comparison group at £1,560. The average age appears very close across all the treatment cohorts but is slightly higher for the comparison group. The summary statistics also show that most individuals in the treatment cohorts were women. However, the comparison group had proportionally fewer females than males as observed in the treatment groups, which is consistent with the findings that female workers are more likely than men to be paid the minimum wage (Dickens et al., 2015; Lenhart, 2017a). 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. The treatment cohorts have a higher fraction of individuals who reportedly received at least one of the affected frozen work-related benefits. Lastly, the number of workers treated in the 2016 cohort is large and twice the size of workers that received NLW in subsequent cohorts. This is expected given that the NLW was first introduced in 2016 and the age eligibility of 25 years was also first implemented in the same year. Additionally, the 2016 basic NLW rate was the largest rise in UK's minimum wage's history, and it has a higher coverage rate than the previous NMW and in subsequent NLW upratings considered in this study between 2017 and 2019 (Low Pay Commission, 2022). Also, the Low Pay Commission (2019) estimates of hourly wage underpayment as a proportion of coverage for eligible NLW workers were lower in 2016 than subsequent years (Low Pay Commission, 2019).

## The average treatment effects results

In this section, we present the results of the estimated average treatment effects of receiving the national living wage on mental health. We begin by considering the static difference-in-differences results using the canonical two-way fixed effects estimator. The results are summarized in Table 2, which shows that the average treatment effects of the NLW on mental health is positive but not significant. However, recent studies in the minimum wage literature have reported the potential for bias in the TWFE estimator when different units receive treatment at different time (Goodman-Bacon, 2021). There is also conclusive evidence in emerging methodological literature that applying alternative DID estimators when treatment is staggered addresses the theoretical problems with the standard TWFE DID estimator (Baker et al., 2022). Hence, the estimated results summarized in Table 2 show the "static" treatment effect estimate, and can be defined as the "weighted average of all the possible two-group two-period estimates of the NLW effects" (Baker et al., 2022, p. 373).

The estimated average treatment effects of the NLW policy on mental health using the Callaway and Sant'Anna (2021) estimator are summarized in Table 3. We considered the treatment effects estimates under the unconditional parallel trend assumptions (Panel A) and conditional on the covariates (Panel B). The pre-trend test column summarizes the estimates of the Chi-squared statistics of

**TABLE 3** Treatment effects estimates of the NLW policy using CSDID.

| Outcome: MCS | 2016 | 2017 | 2018 | 2019 | Simple average | Pre-trend test |
|---|---|---|---|---|---|---|
| **Panel A: Unconditional** | | | | | | |
| Weighted average | | | | | 0.5810** | 6.3003 |
| | | | | | (0.2933) | [0.3904] |
| Cohort | 1.0656* | 0.3486 | 0.1076 | 0.3913 | 0.4720 | |
| | (0.5311) | (0.5214) | (0.6561) | (0.5639) | (0.2973) | |
| Time | 0.9172 | 0.2130 | 0.4709 | 0.7803* | 0.5954* | |
| | (0.6684) | (0.5001) | (0.4936) | (0.4176) | (0.3072) | |
| **Panel B: Conditional** | | | | | | |
| Weighted average | | | | | 0.8824*** | 7.3603 |
| | | | | | (0.3029) | [0.2888] |
| Cohort | 1.5906*** | 0.3513 | 0.4854 | 0.6730 | 0.7571** | |
| | (0.5651) | (0.5737) | (0.6927) | (0.5867) | (0.3130) | |
| Time | 1.1412 | 0.4355 | 0.8110 | 1.1284** | 0.8790*** | |
| | (0.7180) | (0.5535) | (0.5300) | (0.4391) | (0.3272) | |

*Notes*: The table summarizes 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 using their "csdid" package in Stata. The "Weighted average" row reports the weighted average using the cohort size for all the group-time average treatment effects. The "Cohort" row summarizes 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 parentheses, and ***, **, and * indicate that the simultaneous 95% confidence band of the estimate does not cover 0 and is thus statistically significant at the 0.01, 0.05, and 0.1 levels in a 2-tailed test, respectively. The "Pre-trend test" column provides the Wald test of parallel trend assumption, and the corresponding $p$-values are provided in the squared brackets.

the parallel trends assumption with the null hypothesis indicating that all the pre-treatment group-time average treatment effects are equal to zero. The estimated results indicate that the parallel trend assumption holds with and without including the covariates in the treatment effects estimation. The corresponding $p$-value estimates show 0.3904 and 0.2888, respectively, and both are larger than the 0.05 significance threshold, suggesting that the parallel trends assumption holds in the pre-treatment 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 summarizes 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 summarizes 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 4** Mental health effects of the in-work welfare benefits freeze.

| | Without covariates | | | With covariates | | |
|---|---|---|---|---|---|---|
| | **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) |
| Time fixed-effects | Y | Y | Y | Y | Y | Y |
| Group fixed-effects | Y | Y | Y | Y | Y | Y |
| Number of obs. | 17672 | 13272 | 4400 | 17342 | 13027 | 4315 |

*Notes*: The Table summarizes 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 parentheses, while ** and *** indicate statistical significance at 5% and 1% levels, respectively.

## The impacts of the welfare benefits freeze policy

The introduction of the NLW policy in 2016 coincided with the UK government's commencement of a 4-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. Therefore, we evaluate the impacts of the benefits freeze program 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. For robustness, we begin with estimating the average treatment effects of the welfare benefit freeze policy on mental health. The results show negative and significant effects of the benefits freeze on the mental health of the affected workers, particularly those eligible for the NLW (see Table 4). Previous studies and reports have also documented the negative effects of the benefits freeze. For example, Barnard (2019) found that 3 years of the benefits freeze eroded average household spending by about the 8 weeks' equivalent cost of food shopping (see also Kumar et al., 2017).

The estimated average group-time treatment effects of the NLW are summarized in Table 5. Panel A shows the average treatment effects across treatment cohorts and calendar years for the workers that reportedly received at least one of the welfare benefits affected by the government freeze in 2016. 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 summarized 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.

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

**TABLE 5** NLW treatment effects – Receiving vs. not receiving work-related benefits.

| Outcome: MCS | 2016 | 2017 | 2018 | 2019 | Simple average | Pre-trend test |
|---|---|---|---|---|---|---|
| **Panel A: Receiving benefits** | | | | | | |
| Weighted average | | | | | 0.1569 | 2.5773 |
| | | | | | (0.4700) | [0.8597] |
| Cohort | 0.2323 | 0.1798 | −0.1986 | 0.2383 | 0.1493 | |
| | (0.7918) | (0.8384) | (1.1173) | (0.9228) | (0.4641) | |
| Time | 0.6132 | −0.3311 | −0.2076 | 0.5931 | 0.1670 | |
| | (1.0322) | (0.7929) | (0.8153) | (0.6606) | (0.4856) | |
| **Panel B: Not receiving benefits** | | | | | | |
| Weighted average | | | | | 1.0550*** | 5.2219 |
| | | | | | (0.3733) | [0.5157] |
| Cohort | 2.0629*** | 0.7169 | 1.1182* | 1.0886** | 0.7883** | |
| | (0.7300) | (0.5737) | (0.6927) | (0.5867) | (0.3894) | |
| Time | 1.3194 | 1.0696* | 1.1375* | 0.9214* | 1.1120*** | |
| | (0.8502) | (0.6468) | (0.6053) | (0.5394) | (0.3915) | |

*Notes*: The table summarizes 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 using their "csdid" package in Stata. 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 summarizes 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 parentheses, and ***, **, and * indicate that the simultaneous 95% confidence band of the estimate does not cover 0 and is thus statistically significant at the 0.01, 0.05, and 0.1 levels in a 2-tailed test, respectively. The "Pre-trend test" column provides the Chi-squared value of parallel trend assumption, and the corresponding *p*-values are provided in the squared brackets.

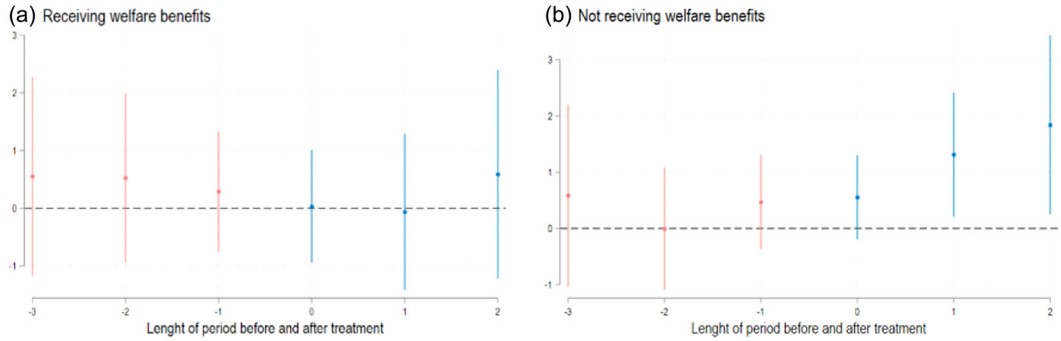


**FIGURE 2** Average treatment effects by the length of exposure to treatment.
[Color figure can be viewed at wileyonlinelibrary.com]
*Notes*: The figure shows the dynamic average treatment effects aggregated by event time for the two groups: those affected and unaffected by the benefit freeze policy. The red lines present the point estimates and the 95% confidence bands for the pre-treatment periods. Blue lines are the point estimates of the NLW on mental health, and the lines represent their 95% confidence bands.

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 pre-treatment 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 pre-treatment 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 2(b) 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). The estimated results for the group of workers affected by the freeze to working-age welfare benefits suggest that positive effects of increasing the basic wage is affected by the freeze. More importantly, changes to the prevailing social welfare structure largely affect low-wage earners who rely on the welfare benefits system to subsidize 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).

## Additional results

### Mental health measured using GHQ-12

In a robustness exercise, we employ an additional measure of mental health, the GHQ-12, which has been widely used in literature to measure mental health. Unlike the MCS, the GHQ-12 has 12 components, each asking participants about their conditions. Each question has a four-point Likert scale in descending order from 1 to 4, with 1 indicating better mental health status. While most previous studies aggregate the GHQ score by summing across responses to each individual component, this could create measurement error. Instead, we employ factor analysis to construct a continuous score for mental health. The factor provides a latent variable for mental health using combined information from each of the GHQ 12 scores (Brewer et al., 2019). The factor analysis result is summarized and discussed in Online Appendix B.[6]

The estimated average group-time treatment effects using the GHQ-12 mental health measure are summarized in Table 6. The results show a positive (negative) aggregate average treatment effect under conditional (unconditional) parallel trends assumptions, that is before (after) including the covariates. However, unlike the statistically significant estimated results when mental health is measured using the MCS score reported in Table 3, the estimated average treatment effect results using GHQ-12 are not statistically significant. The results suggest that the significance of the effects or otherwise of NLW on mental health is dependent on the choice of mental health measure.

### The NLW policy effect on the labor market and wellbeing outcomes

In this section, we evaluate the NLW policy effect on some selected labor 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

[6] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

**TABLE 6** Treatment effects estimates of the NLW using GHQ-12.

| Outcome: GHQ | 2016 | 2017 | 2018 | 2019 | Simple average | Pre-trend test |
|---|---|---|---|---|---|---|
| **Panel A: Unconditional** | | | | | | |
| Weighted average | | | | | 0.0057 | 11.4033 |
| | | | | | (0.0290) | [0.0767] |
| Cohort | 0.0005 | −0.0232 | 0.1261 | −0.0401 | 0.0014 | |
| | (0.0530) | (0.0515) | (0.0642) | (0.0574) | (0.0300) | |
| Time | 0.0344 | −0.0234 | 0.0255 | 0.0025 | 0.0097 | |
| | (0.0647) | (0.0484) | (0.0494) | (0.0415) | (0.0305) | |
| **Panel B: Conditional** | | | | | | |
| Weighted average | | | | | −0.0064 | 7.3474 |
| | | | | | (0.0307) | [0.2899] |
| Cohort | −0.0198 | −0.0087 | 0.1098 | 0.0562 | −0.0172 | |
| | (0.0581) | (0.0562) | (0.0667) | (0.0597) | (0.0312) | |
| Time | 0.0361 | −0.0289 | 0.0170 | −0.0204 | 0.0010 | |
| | (0.0737) | (0.0518) | (0.0540) | (0.0429) | (0.0329) | |

*Notes*: The table summarizes 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 using their "csdid" package in Stata. The "Weighted average" row reports the weighted average using the cohort size for all the group-time average treatment effects. The "Cohort" row summarizes 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 parentheses, and ***, **, and * indicate that the simultaneous 95% confidence band of the estimate does not cover 0 and is thus statistically significant at the 0.01, 0.05, and 0.1 levels in a 2-tailed test, respectively. The "Pre-trend test" column provides the Chi-squared value of parallel trend assumption, and the corresponding $p$-values are provided in the squared brackets.

wage policies to health outcomes. Consistent with previous literature, we considered the effects of NLW policy on two labor 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 summarized in Table 7.

Panel A in Table 7 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 summarized in panel B of Table 7 show that the cumulative effect of the NLW policy is positive and significant on reported work hours by workers that remained in employment. Previous studies have widely documented negative or no effects of minimum wage policies on employment and work hours of the general working population, especially for most vulnerable workers including those on contractual hours and female workers (Dickens et al., 2015). Our result however shows that NLW effect is positive for workers that received the pay rise from the NLW and remained in employment after the policy.

The policy effects on job satisfaction and satisfaction with leisure time for the affected workers are summarized respectively in panels C and D of Table 7. Following a large body of literature that has employed self-reported measures as a construct of wellbeing (e.g., Akanni et al., 2022; 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 standardized values using 0 mean and 1 standard deviation for ease of interpretation. The treatment effects estimates show significant positive effects on job

**TABLE 7** NLW policy effects on labour market and wellbeing outcomes.

| | 2016 | 2017 | 2018 | 2019 | Simple average | Pre-trend test |
|---|---|---|---|---|---|---|
| **Panel A: Earned income** | | | | | | |
| Weighted average | | | | | 0.0404*** | 7.6798 |
| | | | | | (0.0127) | [0.2625] |
| Cohort | 0.0433** | 0.0380 | 0.0470* | 0.0326 | 0.0386*** | |
| | (0.0212) | (0.0248) | (0.0250) | (0.0248) | (0.0131) | |
| Time | 0.0423** | 0.0487** | 0.0397** | 0.0339* | 0.0422*** | |
| | (0.0230) | (0.0197) | (0.0196) | (0.0194) | (0.0126) | |
| **Panel B: Work hours** | | | | | | |
| Weighted average | | | | | 0.8290*** | 2.0697 |
| | | | | | (0.2438) | [0.9132] |
| Cohort | 0.6523 | 1.2724*** | 0.6171 | 0.5861 | 0.7672*** | |
| | (0.4326) | (04278) | (0.4954) | (0.4257) | (0.2407) | |
| Time | −0.1270 | 1.2092*** | 1.0263*** | 0.7601** | 0.7172*** | |
| | (0.6481) | (0.5784) | (0.6115) | (0.5359) | (0.0.2424) | |
| **Panel C: Job satisfaction** | | | | | | |
| Weighted average | | | | | 0.1509*** | 8.9078 |
| | | | | | (0.0453) | [0.1307] |
| Cohort | 0.1771** | 0.2424*** | 0.0255 | 0.1377* | 0.1509*** | |
| | (0.0838) | (0.0828) | (0.0979) | (0.0826) | (0.0453) | |
| Time | 0.0686 | 0.2193*** | 0.1996*** | 0.1447** | 0.1580*** | |
| | (0.1002) | (0.0756) | (0.0740) | (0.0649) | (0.0475) | |
| **Panel C: Leisure satisfaction** | | | | | | |
| Weighted average | | | | | 0.0076 | 3.2998 |
| | | | | | (0.0310) | [0.7704] |
| Cohort | 0.0402 | 0.0232 | 0.0074 | −0.0928 | −0.0201 | |
| | (0.0561) | (0.0549) | (0.0698) | (0.0583) | (0.0312) | |
| Time | −0.0099 | 0.0210 | 0.0276 | −0.0085 | 0.0076 | |
| | (0.0710) | (0.0550) | (0.0509) | (0.0444) | (0.0327) | |

*Notes*: The table summarizes the group-time treatment effect of NLW on earned income, work-hours, and 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, and educational qualification. Standard errors are in parentheses, and ** indicates that the simultaneous 95% confidence band of the estimate does not cover 0 and is thus statistically significant at the 0.05 level in a 2-tailed test. The *p*-value denotes the probability values for the Chi-squared value of parallel trend assumption as reported by the '*att_gt*' function from the '*did*' package (see Callaway & Sant'Anna, 2021).

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.

Figure 3 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.,

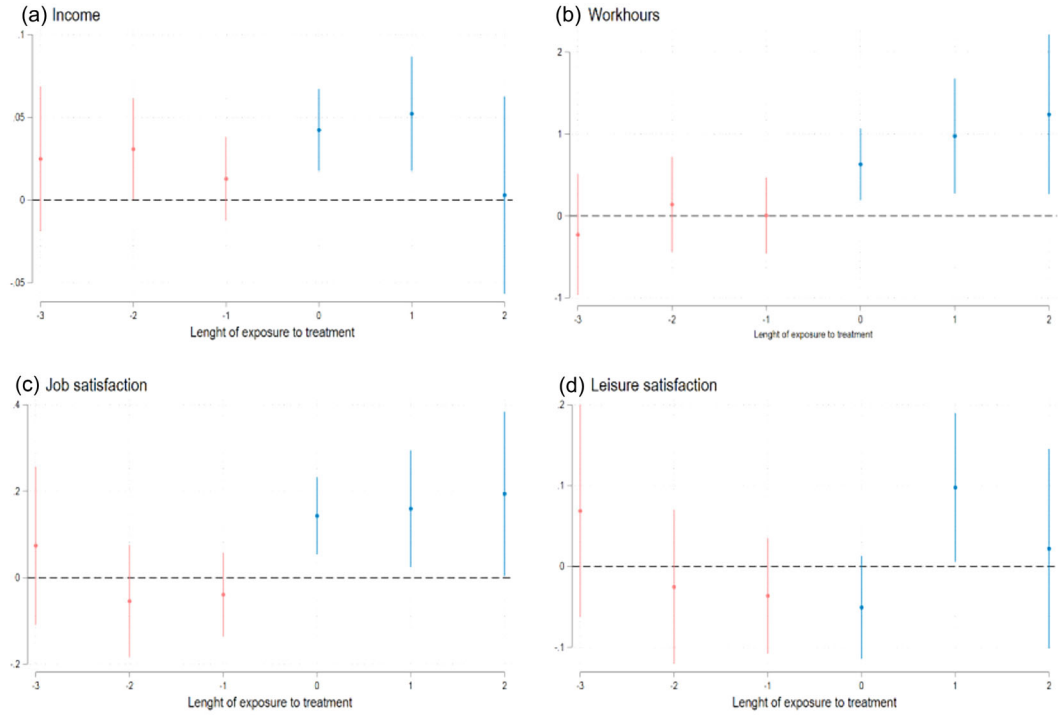


**FIGURE 3** Aggregate treatment effects on labor market and wellbeing outcomes.
[Color figure can be viewed at wileyonlinelibrary.com]
*Notes*: The figure shows the dynamic average effects of the NLW policy aggregated by event time on the selected labor market and work-related wellbeing outcomes. The red points and the lines present the point estimates and the 95% confidence bands for the pre-treatment periods, respectively. Blue lines are the point estimates of the NLW on mental health, and the lines represent their 95% confidence bands.

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). The results also reflect Leigh (2021b)'s findings that job-satisfaction, mental health, and wellbeing are among the few outcomes that consistently show positive effects from minimum wages.

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' trade-off 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).

## 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, 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. When using GHQ-12 as an alternative mental health outcome, we also find evidence for improvements in mental health, although these effects are imprecisely estimated, given that the average treatment effects estimates are not significant across the different aggregations.

Our finding is similar to previous minimum wage studies in the UK. For example, Reeves et al. (2017) documented a significant positive effect of the UK 1999 NMW on mental health, while Kronenberg et al. (2017) found that the estimated mental health effect using GHQ-12 is negative, although their estimate is not statistically significant. Similarly, Maxwell et al. (2022) found that the estimated impacts of introducing the NLW in 2016 and subsequent increases on mental are insignificant. However, contrary to the conclusion in Maxwell et al. (2022) that the cost-benefit analyses of wage policy should not include the health effects, Kronenberg et al. (2017) suggested that a larger increase in the minimum wage could lead to improvements in mental health. Besides, the non-significant average treatment effects estimates do not suggest a harmful effect of the NLW policy on health. Overall, our results show a mixed effects of the NLW on mental health and the significance of the effects or otherwise is dependent on the choice of mental health measure and methodological approach including the treatment design, identification strategy and estimation approach (see Leigh et al., 2019).

Furthermore, we find that the positive effects of the NLW policy on mental health are constricted by the counteracting working-age welfare benefits freeze program, which stagnate or reduce the affected workers' income. The contractionary impacts of the working-age benefit freeze resulted in a decline or zero net additions to income. Thus, they cancelled out the positive benefits of wage policies, especially on the mental health outcome. This is also consistent with findings that changes to social welfare systems largely affect low-income individuals who augment their low earnings through social welfare supports such as tax credits and other in-work benefits (Garfinkel et al., 2006; Marchal et al., 2018). 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 UK national by shifting costs to employers through increased wages while also preventing the precarious situation of low-income workers from further degradation.

The improvements in mental health due to increased wages could be 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) found that the minimum wage policy in the U.S., which provide the lowest guaranteed wage floor for workers across different U.S. 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.

One of the challenges of this study is the problem of attrition associated with most longitudinal surveys, which results from a range of unavoidable factors including survey participants' non- willingness to continue in subsequent survey rounds, deaths, immigration, and residential relocation. Findings by Lynn and Borkowska (2018) showed that attrition in the Understanding Society study is greater amongst younger age groups, men, Black people, and people with lower incomes. Also notably, low wages are highly associated with low income, and low-income people are more likely to have other serious health problems (Fertig & Reingold, 2007), and this might keep some of them from participating in subsequent waves of the survey. Nonetheless, excluding individuals with incomplete data to maintain a balanced panel to address the attrition challenge will further reduce the size of the sample, imposes other bias on the estimated results, and compromises the statistical power. It also leads to underestimating the treatment effects by dropping people from the treatment and control sample. Besides, the 25-year-old age criteria to receive the NLW also restricts the workers sample considered in our study to older adults, while excluding young workers between 18 and 24 years of age, who are more likely to be new entrants into the labor market, in their early careers, earning around the

minimum wage, and forming parts of the cohorts considered in most minimum wage and health literature (Leigh, 2021b).

Additionally, our analyses did not capture the employment effects of the NLW given that our sample composition is made up of workers who remain employed, and it does not account for individuals that lost their jobs during the periods considered. Hence, the results might underestimate the mental health effects of the NLW by not considering self-employed and people who lost their jobs, and employers. Although low-wage workers were the central target of the NLW policy, not considering the effects on self-employed and employers might bias the mental effects of NLW on the society at large. For example, increased in minimum wage might reduce employers' income, which could also impact their mental health. Nonetheless, previous empirical investigations of the labor market effects of the UK NLW policy indicate no significant decline in employment or work hours (see Aitken et al., 2019; Brewer et al., 2019; Dube, 2019). However, these findings also do not rule out the fact that minimum wage policy might have other employment-related effects such as the reduction in employer-provided fringe benefits (see Clemens et al., 2018; Simon & Kaestner, 2004).

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 wage increases and welfare benefits as alternatives, they complementarily have the prospects of reducing poverty and generating livable income for families. Above all, future research should consider the health impacts of the interaction between wage policies and other complementary or conflicting income-related programs and policies using data from the UK.

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## SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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