Essays on the Impact of UK National Living Wage Policy

Insights into Health, Wellbeing, and Informal Caregiving

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Dedicated to all those who contributed in the making…

Declaration

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Abstract

Minimum wage remains a contentious policy issue with far-reaching implications for workers' health, quality of life and the overall economy. This thesis contributes to the literature on the effects of minimum wage policy on public health by exploring the 2016 UK National Living Wage policy (NLW) and its subsequent annual upratings. In three self-contained but related essays, we employ data from the Understanding Society longitudinal household survey, and applied econometrics and quasi-experimental methods, to objectively consider the effects of NLW on health and wellbeing, considering the policy interactions with in-work social security benefits freeze and unpaid caregiving.

The first essay begins with analyses of the association between income and different trajectories of income on self-reported health and wellbeing. We employed a fixed-effects ordered logit model which allows us to account for unobserved heterogeneity and time-invariant factors that may bias the estimated results. We show that income is a significant predictor of health and wellbeing. Also, our extended analyses show that stability and volatility in income are important determinants of self-reported health and wellbeing outcomes, while higher spells of low income increased the odds of reporting poor self-reported health and wellbeing outcomes. We also confirm a significant difference in the income and health nexus before and after the introduction of the NLW in 2016.

The second essay employs recent developments in the difference-in-differences methods literature to investigate the mental health effects of the NLW. We employ the longitudinal hourly wage and age data in the Understanding Society survey to identify individuals that are eligible

and received the NLW. We find that the NLW policy has a positive average treatment effect on affected workers' mental health. We additionally consider the effects of the simultaneous introduction of a four-year freeze to in-work welfare support benefits. We find no evidence of improvements in mental health for individuals affected by the welfare benefits freeze policy, indicating that the negative impact of the benefits freeze policy constricts the NLW impacts. These findings suggest that wages increase through minimum wage and social security supports are complementary and should not be treated as alternatives. The overall prospects of reducing poverty and generating liveable income for working individuals may be more effective with their combination rather than substituting one for the other.

The third essay extends the evaluation of the NLW to informal caregiving by considering the effects on unpaid carers' work hours and health. While minimum wage is a focal point in shaping labour markets, addressing income inequality and social welfare, little attention has been given to understanding their influence on informal carers workforce. The informal care sector has a dual influence on labour supply and overall health and long-term care. We begin the empirical analysis by estimating the effects of becoming an informal carer, and we note a significant decline in work hours with negative average treatment effects on physical health. However, while the overall average treatment effects of becoming an informal carer on work hours is negative, receiving the NLW makes a positive difference for informal carers' work hours but a null effect on health outcomes. The findings suggest differences in informal carers reactions to the NLW. While there is an overall decrease in work hours as a result of being unpaid carers, some carers increase their work hours following the NLW increase. By providing robust empirical evidence, this chapter underscores the potential role of the minimum wage as a tool to promote public health and well-being, especially for vulnerable populations in the labour market. Overall, the thesis contributes to the significance of wage policies to harmonise economic growth and advance public health and social equity, thereby contributing to a path of sustainable and inclusive development.

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Chapter 1

Introduction

1.1 Motivation

The conduct of the minimum wage policy in the UK is widely commended given its very formal connection to evidence. Since the inception of the National Minimum Wage (NMW) in 1999 and the subsequent introduction of the National Living Wage in 2016, annual changes and uprating in wage floors are usually asserted to be evidence-based and based on research findings (Brewer et al., 2019).[1] The Low Pay Commission (LPC), which is the independent body charged with the responsibility of advising the government on the wage rates, commissions and funds independent research annually to evaluate the impacts of the NMW, with the outcomes of these studies serving as a guide in providing wage recommendations to the government. However, this body of research and evidence is predominantly centred on labour market outcomes, such as the effects on employment, job retention, and hours worked.

Since the first introduction of the National Minimum Wage (NMW), the UK witnessed more than 16 upratings in the NMW between 2000 and 2015 before introducing the National Living Wage (NLW) in 2016. The introduction of the NLW was not just an extension of the NMW

[1] Although there are concerns that the two main political parties in the country are drifting away from evidence-based recommendations from the independent commission and experts in their laid-out proposals to increase future minimum wages to unprecedented high among developed countries (Cribb et al., 2019).

policy, which is statutorily binding on every employer; it additionally categorized affected workers to those aged 25 and above. Besides, it sets the new prevailing wage rate for workers in the affected age category relatively higher than previous annual upratings witnessed in the NMW.[2] In addition, the coverage rate, which is defined as the proportion of workers across all age groups that received the wage rise, increased by more percentage than previous upratings in NMW (Brewer et al., 2019; Low Pay Commission, 2019). Moreover, small upratings in nominal wages are unlikely to provide sufficient information for policy evaluations, and when such evaluations are conducted, the results may "yield more noise than signal" of the effects of such wage increase (Dube, 2019, p. 53).

There is vast empirical evidence on the labour market implications of the wage policy. The conclusion in most of these studies is that the UK minimum wage policy (both the NMW and NLW) has had no detrimental effects on labour market outcomes (Brewer et al., 2019). Rather, it has increased low-paid workers' wages with little adverse effects on job retention (Aitken et al., 2019). However, the absence of or little employment effects of increasing the NLW can imply that firms adjust the costs of higher wage bills through other channels. The evidence of the trade-offs between complying with the statutory requirement of higher wages and other trade-off margin effects is scanty and still developing in the literature. Dube (2019) summarised some of the evident trade-offs that absorb minimum wage changes as possible reasons for its limited employment effects. Prominent among these "margins of adjustment" is the transfer of the increased cost of labour to the consumers through higher price responses (Dube, 2019, p. 50). There is a body of evidence that found that increasing minimum wage is followed by higher prices in some countries and selected sectors (see Allegretto and Reich, 2018; Harasztosi and Lindner, 2019; MaCurdy, 2015; Renkin et al., 2020).

Additionally, other possible trade-offs include the decline in firms' value and profitability (Bell and Machin, 2018). Others are the possibility of reducing non-wage benefits received by

[2]The two exceptions in previous annual upratings than the NLW increase by 7.46% were in 2001 (10.81%) and in 2004 (7.78%).

workers and the increased incidence of delivered hours of work that are unpaid. Remarkably, these trade-offs and adjustments can have far-reaching effects on the general aspects of workers' health and well-being. For example, the UK's minimum wage policy interacts with tax and other social programs like in-work credits. An increase in wages may be associated with an increase in taxes and a decrease in in-work credits. By extension, increased wages may not necessarily translate into increased purchasing power but a loss in real income and consumption level (Dube, 2019). Increased wages followed by a decline in in-work benefits and an increase in unpaid work can lead to increased work-related stress and a decline in job satisfaction.

Moreover, based on the wide-reaching consensus that poverty is a major cause of poor health, the introduction and increase in wage floors could translate into increased consumption activities that can lead to improvements in the health of workers who are predominantly in the low-income category and mostly from households in the lower-income quintile (Marmot and Bell, 2012). There is also evidence that increasing wages are associated with improved psychosocial well-being among workers (see Flint et al., 2014; Wills and Linneker, 2012).[3].

Empirical evidence on the impacts of wage policies on non-labour market outcomes is still growing and receiving attention. Compared to the amount of emphasis placed on its implications on labour market outcomes, there is less emphasis on the non-labour market effects of wage policies. This may not be unconnected with the multiplicity of channels through which raising wage floors can affect non-labour market outcomes. Income is arguably the foremost determinant of population health (see Darin-Mattsson et al., 2017; Hahn and Truman, 2015; Marmot, 2002), and as such any policy that affects or leads to changes in income is expected to impact public health. However, based on the available empirical evidence, there is a lack of consensus on the magnitude of its true causal effects (Thomson et al., 2022).

The number of studies that have considered the health-related effects of minimum wage policy in the UK is minuscule compared to available studies on the labour market implica-

[3] Studies by Lenhart (2017b) and Kronenberg et al. (2017) both failed to reject the hypothesis that increasing national minimum wage in the UK do not affect self-reported general and mental health as well as smoking behaviour (see also Leigh et al., 2019)

tions, and studies from other countries like the US. The existing UK-based studies mainly focused on the health impacts of the introduction of the NMW in 1999 (see Kronenberg et al., 2017; Lenhart, 2017b; Reeves et al., 2017). To the best of our knowledge, only the study by Maxwell et al. (2022) considered the effects of the 2016 NLW on physical and mental health. However, while Maxwell et al. (2022) estimated separate two-way fixed effects regressions for each year following the policy introduction, we employed an approach that simultaneously accounts for the timing and duration in which an individual received the wage rise. Additionally, we consider the separate treatment effects for workers affected by the benefits freeze policy. The evaluation of the health effects of the NLW policy is of particular importance, given the numerous pathways and transmission mechanisms that connect public health and well-being. Besides, focusing on physical and mental health, alongside other dimensions of well-being including job and life satisfaction, and long-term care, is important for public policy. For example, mental health disorders, mostly depression, alcohol- and substance-use disorders and psychoses, have been attributable to about 14% of disease burden globally, while the same is said to account for about 28% of the total burden of diseases in the UK (Allwood and Bell, 2019).[4] The World Health Organisation's declaration that "there can be no physical health without mental health" (See Kolappa et al., 2013, p. 3).

Furthermore, the importance of health and well-being aspects of the living wage is captured in its comprehensive definition by the Global Living Wage Coalition (GLWC).[5] GLWC on its website defines a living wage as "the remuneration received for a standard workweek by a worker in a particular place sufficient to afford a decent standard of living for *the worker and her or his family*. Elements of a decent standard of living include food, water, housing, education, *health care*, transportation, clothing, and other essential needs, including provision for unexpected events." [italics added for emphasis] (Global Living Wage Coalition, 2018). This

[4]The importance of mental health is evidently important on the political agenda in the UK given the commitment of all UK political parties with their inclusion of mental health goals in manifestos prior to the 2015 election.

[5]The definition incorporates the main ideas of over 60 definitions and descriptions of living wage by different institutions including human rights declarations, national constitutions, non-governmental organisations, multinationals, and International Labour Organisation (ILO) documents (Anker, 2011).

definition provides an extensive implication of wages by decommodifying labour and embracing the fulfilment of workers' needs and their dependants, and their meaningful participation in the society while also insulating them from unforeseen financial hardships (Arrowsmith et al., 2020). The dignity component of a living wage is also succinctly captured in President Franklin Roosevelt's declaration in 1938, five years before introducing the first national minimum wage in the US. He proclaimed that "no business which depends for existence on paying less than living wages to its workers has any right to continue in this country. By living wages, I mean more than a bare subsistence level — I mean the "*wages of a decent living*" [italics added for emphasis] (see Dube, 2019, p. 6).

1.2 Historical Background of the UK National Living Wage Policy

Before the introduction of the National Living Wage in 2016, and the National Minimum Wage introduced in 1999, Britain has had regulation of wages in different forms for many centuries. For example, the Statute of Labourers enacted in 1351 has three key provisions: (i) it placed a maximum cap on higher wages demand by labourers, (ii) it restricted the movement of labour in search for higher pay or better working conditions, and (iii) to avoid deterrence to law and maintained the wage structure, the Statute also established penalties for employers and labourers that respectively offered or demanded wages above the legally fixed rates. Much later in 1891, the Fair Wage Resolutions were introduced as attempts to eliminate the unfair competition for public-sector contracts through undercutting pay rates, and these resolutions were in place for over ninety years (Metcalf, 1999).

More recently, the Equal Pay Act 1970 and the Employment Protection Act 1975 both provided some form of wage legislation and support in the UK labour market. The Equal Pay Act of 1970 was the first piece of legislation that attempted to close the gender pay gap by granting women the right to equal pay in the workplace. While the main implication of the Employment Protection Act 1975 was to protect female employees from being terminated from their jobs due to pregnancy, its Schedule 11 which came into operation in January 1977 was introduced with the objective of removing remaining areas of low pay in the labour market. It allowed employees through their union to institute claims where their employers fall short of the established or recognised terms and conditions of pay in the industry or section of the industry they operate (Harris, 1979). However, reviews of this legislation and analyses of the labour markets reveal inadequate protection for employees and the persistence of low pay (Grimshaw and Rubery, 2013; Metcalf, 1999).

The unique feature of the 1999 NMW and subsequent wage legislation, mainly the introduction of the NLW in 2016, was the establishment of the Low Pay Commission (LPC) in 1997 prior to the commencement of the NMW. Brown (2009) described the LPC as a form of 'social partnership' comprising representatives of employers, workers, and independent members. The main remit of the LPC was to recommend the initial level at which the NMW should be introduced and to monitor and evaluate the NMW introduction and its impact, among other terms of reference. The LPC was not the first establishment regarding institutional determination of wages in the UK. For example, the establishment of the Wages Council system in 1909 successfully provided surrogate collective bargaining for many low-paid workers until it was abolished in 1993. However, the LPC is independent and has a tripartite structure involving representatives of employers, trade unions, and academics/experts, to ensure a balanced approach in their decisions. The decisions and recommendations of the LPC to the government concerning wages are also evidenced-based through research, evidence gathering, and analysis of economic data to determine the appropriate wage floors. Nonetheless, the LPC remit does not include official wage-setting powers. Rather, they make recommendations to the government. However, subject to government reviews, the LPC's recommendations have continued to influence and shape the direction of wage policies in the UK.

More importantly, the 2016 NLW ushered in a different focus of wage legislation in the UK by setting a wage level targeted towards a higher living standard compared to the previous NMW. While previous wage legislation was mainly aimed at protecting workers by ensuring adequate remuneration and preventing unfair competition through undercutting wage rates, the introduction of the national living wage additionally seeks to ensure that 'work pays' by encouraging people to get into employment. According to the policy paper setting out the national living wage policy, the government aims to "move from a low wage, high tax, high welfare society to a higher wage, lower tax, lower welfare society" (Department for Business, Energy, and Industrial Strategy, 2016). The NLW seeks to address the increasing costs associated with the continuous expansion of state welfare and social security support. Essentially, the NLW was an integral means of reducing reliance on the state social security supports (Department for Business, Energy, and Industrial Strategy, 2016). However, the introduction of the NLW during a period when several changes were made to the various social security supports available to low-paid workers could pose challenges to its effectiveness. For example, the freeze to in-work welfare benefits such as Working and Child Tax Credits in 2016 was projected to result in a significant loss for low-paid workers (Hood and Waters, 2017).

While the *"living wage"* has been defined using '*ethical*' or '*human rights based*' approach to earnings by the GLWC such that it guarantees that every wage earner earns enough to sustain their household (Bronkhorst, 2020). The idea of the word *"living"* in the UK national living wage policy context is a 'superficially attractive' concept given the impression that it will provide a basic wage that has some relationship to working people's subsistence needs (Grover, 2016, p. 8). D'Arcy and Kelly (2015) describes the labelling as a "misnomer" given that the NLW did not reflect the rising cost of living. Besides, the approach taken to arrive at the wage threshold was not particularly sensitive to household needs but related to a low pay threshold approach, by setting an arbitrary target of 60 per cent of the median hourly earnings (Resolution foundation, 2014). The stipulated legal wage rate and adopting the low pay threshold approach in its wage determination have also been criticised as not meeting the guaranteed living standard ensured in the living wage definition. For example, six months after the introduction of the NLW, the Living Wage Commission review of the NLW policy concluded that the basic hourly rate fails to provide the basic needs for the lowest paid in Britain (Living Wage Commission, 2016). Moreover, the decision to raise wages for adult workers aged 25 years and above was described as a political smokescreen for the government's simultaneous decision to cut tax credits, which was initially suspended when evidence emerged that most low-income working families would suffer severe net losses (Grover, 2016).

In spite of the fact that national wage legislation and statutory wage floor are supported and enforced in many countries, both advanced and developing economies alike, some countries adopt multiple minimum wage levels agreed on by social partners and stakeholders through sector-based collective agreements. Also, for countries with statutory wage minimum, the motives for legislating wages and the policy goals are mixed, and these goals shape the successes and consequences of setting minimum wage in the first place (Grimshaw, 2013). For example, the central government through political coalition is responsible for wage setting in Germany, despite several resistance it attracted from employers, business owners and some workers' unions (Mabbett, 2016). On the other hand, the implementation of national wage-setting policies in the UK is accepted by employers and unions as providing "protection against damaging the cost-led competition" in industry and market segments where joint and mutual wage determination and regulation has diminished over the years (Grimshaw, 2013, p. 19).

Furthermore, national wage legislation in the UK, and the approach to setting the wage minimums, have had significant implications on the economy and more importantly, on the lives and livelihoods of the workers. Also, the design and implementation of the wage policies was not an emergency measure, but aimed at addressing deepening problems of increasing income inequality, with growing numbers of families and children in poverty, and escalating costs of addressing these problems through social security support (Brown, 2009). However, the direct consequences, which have been the main focus of impact evaluation of wage policies, have two issues dominating their discourse: (i) the impact on pay including nominal and real wage levels, wage inflation, pay gap and wage inequality, and (ii) the impact on employment outcomes (Metcalf, 2008). Increasing attention has also been devoted to understanding the indirect and unanticipated consequences of minimum wage legislation, such as the impacts on public health (Leigh et al., 2019), criminal behaviour (Fone et al., 2023), long-term care provision (Jutkowitz et al., 2022).

1.3 Contributions

From the foregoing discussions of the importance of wage legislation and the effects of current and past income experience, this thesis encompasses three essays evaluating related aspects of earnings on different dimensions of health and well-being. Each essay delves into specific aspects providing a focused analysis of the relevant connections between income, wage policies, health and well-being, and informal care. The thesis particularly highlights a common thread on the introduction of UK's national living wage policy in 2016.

Chapter 2 is titled *"Income Trajectories and Self-rated Health and Well-being Outcomes: Evidence from the UK"* and it considers the impact of income dynamics on health and well-being outcomes using data extracted from the UK longitudinal household survey. The cross-sectional regression model is the prominent technique used in previous studies. Few studies and extensions that have employed longitudinal data followed estimation approaches that assume random effects implying that the regression errors are normally distributed and independent of the regressors (Cameron and Trivedi, 2005). However, the presence of unobserved heterogeneity and time-invariant factors may bias results and lead to spurious non-causal relationships. Fixed effects models relax the distribution and independence assumptions on the error terms by leaving them completely unrestricted. Few studies on health well-being that applied the fixed-effects panel estimators considered binary health and well-being outcomes by creating binary variables for health outcomes from the usual multi-item response scale. However, we argued that altering the data distribution affects the measures of central tendency and variance of the data and can diminish the reliability and validity of such results.

We proposed a longitudinal model that accommodates health outcomes in their multi-item ordered measures. Thus, ensuring that we captured sensitive changes in people's health and well-being conditions over time as well as providing the complete profile of different health dimensions. The estimation approach that we employed, the panel fixed-effects ordered logit model (Frijters et al., 2004), relaxes the restricted distributional and independence characteristics of the regression errors leaving them completely unrestricted within a panel-data framework (see also Baetschmann et al., 2015; Carman, 2013; Ferrer-I-Carbonell and Frijters, 2004; Frijters et al., 2005a,b; Khanam et al., 2014). To our knowledge, we provided the first empirical evidence investigating the effects of income on health and well-being outcomes employing a panel-ordered logistic estimator for UK longitudinal data. The only two previous studies that applied a similar estimator to investigate the effects of household income on ordinal measured health outcomes employed German and US data. Additionally, we investigate the effects of income stability and volatility, and income spells duration on health and well-being. The importance of income dynamics and measures over extended periods are better indicators of economic status and predictors of inequalities in health and well-being outcomes. We contribute to the literature by providing information on health risks, especially those likely suffered by individuals with volatile incomes and largely from low-income households. Finally, given that exogenous policy income shocks influence health and well-being, we isolate the possible effects of NLW using a before and after analysis to gain insight into the likely impact of the increased wage floor on the income trajectory and health nexus.

Chapter 3 is titled "*Conflicting Economic Policies and Mental Health: Evidence from the UK National Living Wage and Benefits Freeze Policies*". Empirical research on the causal effects of minimum wage changes on health and well-being outcomes is growing. However, we provide new insights into the effects of increasing wage floors on mental health by exploring the introduction of the national living wage in the UK in 2016 and subsequent annual increments. Additionally, we considered the counteracting effects of the welfare benefits freeze policy also implemented in 2016, which suspended the annual increase in all work-related welfare benefits. The simultaneous implementation of the two policies disproportionately affects low-paid workers, who are more likely to rely on income supplements from welfare benefits to augment their low wages. Besides, the attempt by the government to reduce the costs of welfare benefits could worsen the precarious conditions of low-income workers. Also, understanding the mental health effects of both policies that affect income could provide an economic case for preventative and proactive measures to promote better overall health. This is particularly important given the high societal burden and economic costs associated with mental disorders.

Chapter 4 is titled "*The effects of the National Living Wage on Informal Carers Work Hours and Health*". The impact of wage policy has been evaluated on the health of the general working population and for specific demography within the society including youths, teenagers, children, and workers in certain sectors, especially those in sectors that are labour intensive and low pay sector. Empirical evidence abounds that part-time workers are also more exposed than full-time workers (Dube, 2019). Another strand of empirical studies has considered the wage penalties and labour market effects of becoming an informal caregiver. An important aspect that is lacking in empirical literature evaluating the effects of wage policies on health is the impact on the working population engaged in unpaid care responsibilities. Labour market policies and reforms like the increasing wage floors could have implications on informal caring decisions. Unpaid carers are more likely to be in low-paying and part-time jobs in order to continue fulfilling their care responsibilities. Investigating the health effects of wage policies on informal carers provides insights into designing a sustainable health and long-term care system. The United Nations Sustainable Development Goals (SDG) identified the importance of adequate compensation for informal carers, and policies supporting the reconciliation of employment with care responsibilities, as among the focal policies of reducing the health, well-being and social risks challenges facing unpaid carers. We begin the empirical analysis in the chapter with the evaluation of the impact of becoming an informal caregiver on work hours, to capture the labour market trade-off, as well as on physical and mental health outcomes. Thereafter, we estimate the impacts receiving the NLW had on the interaction between providing unpaid care and health.

There are several underlying themes that connect the three empirical essays. We considered the importance of income experience on health and well-being outcomes, by investigating the effects of past income status and exogenous shocks to income emanating from changes in policies that affect earnings and their effects on health and well-being. We accommodate recent developments in the methodological literature by accounting for specific features relevant to each of the empirical analysis chapters. The fixed-effects ordered logistic regression employed in chapter 2 highlights the importance of accounting for potential endogeneity from time-invariant characteristics. By relaxing the distributional and independence assumptions between the error term and the regressors, the estimator provides more accurate estimates of the impacts of income changes on health outcomes. In chapters 3 and 4, we employed the heterogeneous difference-in-differences method that allows for estimating the disaggregated causal effects of the national living wage policy on the various outcomes considered. Also, the choice of coarsened exact matching technique employed in chapter 4 to match informal carers to non-informal carers is premised on addressing some of the limitations in the popular matching approach used in previous literature. Finally, the three essays collectively contribute to different aspects of the Sustainable Development Goals (SDGs), including Goal 1 - *"No Poverty"*; Goal 3 - *"Good Health and well-being"*; Goal 5, Target 5.4 - *"Recognize and Value Unpaid Care"*; Goal 8 - *"Decent Work and Economic Growth"*; and Goal 10 - *"Reduced Inequalities"*.

As of January 2022, a version of Chapter 2 was published under the title "Income Trajectories and Self Rated Health: Evidence from the UK" and co-authored with Otto Lenhart and Alec Morton in the *Social Science and Medicine - Population Health*, Volume 17, No 101035. Also in October 2022, a working paper of Chapter 3 was deposited in the *Strathclyde Discussion papers in Economics*, No 22 - 10. Another version has also been submitted for publication consideration in a Journal, and it currently has a revise and resubmit status. Chapter 4 was also presented at the 12th American Society for Health Economists Conference in St. Louis, Missouri, USA in June 2023. The conceptualisation and all the empirical work including the original draft, writing, and editing in the joint publications and research were undertaken by myself.

Chapter 2

Income trajectories and self-rated health and well-being outcomes: Evidence from the UK

2.1 Introduction

Anecdotal evidence suggests that poor socioeconomic status and poverty are connected to higher morbidity and mortality rates. However, there continue to be recurring issues when analyzing the nexus between income and health. First, the empirical evidence is not conclusive on the causal relationship between income and health. The plausibility of socioeconomic conditions and health linked through a bi-directional relationship has been well discussed in the literature. For example, Blázquez et al. (2014) hypothesized that income enhances health through the acquisition of 'health-enhancing' goods and services, and good health also affects productivity and labour participation rates leading to higher wages and earnings. However, while studies based on the cross-sectional analysis of income and health nexus are popular in the literature, their results are likely biased by confounding (Gunasekara et al., 2012). On the contrary, analysis of longitudinal data on income and health outcomes, which has also grown in popularity in the literature, accounts for time-invariant confounding and provides better estimates of the health effects of income changes. Likewise, reverse causality and health selection can be reduced more efficiently using longitudinal approaches (Miething and Aberg Yngwe, 2014).

The second issue relates to the importance of income dynamics and its lagged effects on health and well-being. Although current income level and distribution are well-established causes of inequalities in the distribution of health and well-being status, there is supporting evidence that income measures based on more extended periods are better indicators of economic status (Benzeval and Judge, 2001). Moreover, both downward and upward trends in individual or household income are better predictors of health outcomes (Frech and Damaske, 2019; Schollgen et al., 2019). In addition, health and well-being are sometimes less sensitive to temporary fluctuations, including short periods of hardships, unemployment spells, and small changes in nominal incomes (Davillas et al., 2019).

We contribute to the growing literature on income dynamics and health by revisiting the relationship between income and health using the 2009 to 2019 waves of the Understanding Society UK Household Longitudinal Survey (USoc). Given the ordinal nature of the health and well-being measures, we employ the fixed-effects ordered logit model. Developed by Ferrer-I-Carbonell and Frijters (2004), the fixed effects estimator for ordinal outcomes has been widely employed in economics literature including health economics and empirical research of life satisfaction (see Carman, 2013; Frijters et al., 2005a; Khanam et al., 2014). However, studies that employ fixed-effect models using UK data mostly split health and wellbeing outcomes into dichotomous variables to enable estimation using binary fixed-effect models (see Benzeval and Judge, 2001; Collin et al., 2020). Altering data distribution may affect central tendency and variance measures, which could also diminish the reliability and validity of the estimated results (Dell-Kuster et al., 2014). Additionally, accommodating the multi-item scale of health and well-being measures captures the sensitive changes in people's health conditions over time, as well as provides a complete profile of their health dimensions (Bowling, 2005). Secondly, another strand of studies employs ordinal outcomes following the random-effect model assumptions that the error term is normally distributed and independent of the regressors. The fixed-effect estimator controls for the presence of unobserved heterogeneity and time-invariant factors which might determine both income and health. Failure to account for these confounding factors could lead to biased results and spurious relationships (Contoyannis et al., 2004; Frijters et al., 2011; Frijters and Ulker, 2008).[1]

Furthermore, we evaluate the effects of stability and volatility in household income position and duration of low-income and high-income spells on health and well-being. Using the 18 Waves of the British Household Panel Survey (BHPS), Davillas et al. (2019) evaluate the long-run average measure of household income on self-rated health (SRH) and biomarkers. However, this study further considers the effects of income volatility, which reflects ongoing shifts in economic risks and threatens health and well-being (see Prause et al., 2009). We employed a novel measure of income volatility by dividing the change in income by the mean of current and immediate past income. The volatility measure ensures that the size of income change is not dependent on the ordering of incomes in either year. Our approach also adjusts for outliers and the inclusion of observations where income is zero in both successive periods (Avram et al., 2019). Besides, the larger sample size and comprehensive geographical coverage of the USoc data used in this study provide a more recent and broader scope of the income-health dynamics across the full-age range, ethnic groups, and socioeconomic diversity (Platt et al., 2021).

We further consider how household position on the overall income distribution affects health and other well-being outcomes. We conduct separate analyses of the impact of income trajectories on health outcomes for low-income and high-income households. Thus, we contribute to the literature by providing information on health risks, especially those likely suffered by individuals from low-income households. Lastly, we partition the empirical analyses

[1]However, Jones and Schurer (2011) concludes that the fixed-effects ordered logit model do not sufficiently account for the heterogeneity in longitudinal health and income data, and as such could lead to misleading conclusions on the potential effects of income on health.

into two sub-sample periods, before and after 2016, coinciding with the introduction of the UK's National Living Wage (NLW) policy. Since income is known to causally determine health, after controlling for possible confounders (Gunasekara et al., 2011), it is essential to isolate the effects of such policies that could affect the income-health nexus. Hence, we conduct a pre-post analysis to gain insight into the likely impact of the increased wage floor on the income trajectory and health nexus.

In line with related literature, we consider the different array of health measures, including self-reported health (SRH), mental health and subjective well-being outcomes, including satisfaction with leisure and life. Combining physical and mental health with well-being indicators is more relevant to policy (see Apouey and Clark, 2015). The World Health Organisation declared that "there can be no physical health without mental health" (Kolappa et al., 2013, p. 3). Besides, the report of the 2019 global burden of disease showed that mental health disorders, including depression, anxiety and conduct disorders, bipolar, schizophrenia, and autism spectrum disorders, eating disorders, and a host of other residual category of mental disorders, remained one of the top-ten causes of disease burden globally, with no evidence of reduction in its burden since 1990 (GBD 2019 Mental Disorders Collaborators, 2022). In the UK, the importance of mental health is evidently a major policy agenda, with all major political parties having mental health goals included as part of their manifestos (Allwood and Bell, 2019).

Our findings show that current income positively affects self-reported general health, mental health, and life satisfaction, while the results show a negative effect on leisure satisfaction. The results of the income gradients - health nexus also show that stability in income position is strongly associated with improved health and well-being, while income volatility increases the odds of reporting poor health outcomes, particularly for individuals in low-income households. We also find that more years spent in a lower-income quartile group reduces the odds of reporting improved self-rated health. Finally, we find significant differences in the estimated effects before and after 2016. This highlights significant shifts in the effects of income trajectories on self-reported health and well-being following the National Living Wage policy implementation.

The rest of this chapter is structured as follows. The review of empirical literature on the nexus between income dynamics and health outcomes is provided in Section 2.2. Section 2.3 discusses the model and variables measurement, while Section 2.4 describes the data. Section 2.5 presents the results and discussions, and finally, Section 2.7 concludes the chapter.

2.2 Literature review

There is a long-standing debate concerning the proxies and data sources for health and well-being outcomes in evaluating the effects of income on population health and well-being. Broad health measures have been considered in the literature, including pathological and clinical measures extracted mostly from individual medical records and administrative data and individual SRH collected through surveys. The choice of health measures is determined by collection costs, ethical considerations, and external validity. As a result, SRH measures are prominently used in empirical studies. One of its features is the combination of different aspects of health. However, SRH measures are also widely criticized as subjective measures, and they may not give adequate and efficient assessments of the objective state of public health (Johnston et al., 2009).

Nonetheless, subjective measures can provide accurate and efficient assessments of objective states of health (Cleary, 1997; Nielsen, 2016). For example, through the interviewer's interaction with the respondents, they can objectively assess a self-rated measure of physical function by asking whether an individual has sight and hearing difficulties. The accuracy can further be attributed to the experience of ill health and health problems (Simon et al., 2005). Moreover, the potential measurement problems and reporting errors in using self-rated health status could be addressed by combining various health measures collected from similar individuals over time. The increased availability and popularity of longitudinal surveys have led to considerable growth in the empirical application of panel data approaches to investigate the relationship between income and health. The application of longitudinal data which asks the same health-related questions from the same informants over time further reduces the potential measurement errors since "the health status of every individual is compared to its prior assessment, [thus] every individual is assigned to its own scales of health ranking" (Lenhart, 2017b, pp. 832). Besides, self-rated health outcomes applied in a longitudinal framework provide a combination of different aspects of health, and they provide significant predictions of morbidity and mortality (Frijters et al., 2011; Van Doorslaer and Gerdtham, 2003).

Contrary to the approach by early literature, which largely provided cross-sectional analyses of people's static income and health experience, studies based on longitudinal data capture the variation and dynamics of income over time. The use of panel data methods which allows for correlation between the unobserved heterogeneity and the regressors could also address the reporting problems with SRH and omitted variable bias (Jones and Schurer, 2011). Another method deployed in empirical literature to systematically address the measurement error and reporting bias in SRH measures involves some form of latent analysis (Bound, 1991; Jones et al., 2010). Also regarded as an 'instrumental variable approach', the approach involves extracting a latent health stock variable from regressing self-reported health on objective health measures using various regression models such as the standard and generalized ordered probit model which allows for different thresholds. The health stock variable is then used as a health proxy in subsequent analysis (see Jones et al., 2010, p.869). Nonetheless, using repeated measures of income is less prone to error than using income information for a particular year (Miething and Aberg Yngwe, 2014).

Empirical studies have also combined longitudinal income data with cross-sectional health dimensions to investigate how income affects health and well-being outcomes over time. Most of these studies' findings suggest that a longer-term income position is more relevant in predicting health and well-being than current income. This finding is relatable to Friedman's

(1957) Permanent Income Hypotheses. Long-term income and socioeconomic position are more relevant to health and may reflect cumulative disadvantage than transitory status (Davillas et al., 2019). Benzeval and Judge (2001) found that family income averaged over five years better predicts SRH in the subsequent year than current income in any given year. Miething and Aberg Yngwe (2014) evaluate the role of stability and variability in individual income position over time using Swedish survey data. Their findings confirm that changes in income status and the time dimensions of income are important for health status.

Vanzella-Yang and Veenstra (2021) using ten years of average income of Canadian households between 2002 and 2011, finds that stable income is strongly associated with SRH. They also reported that spending more years at the bottom quintile of income distribution corresponds to increased odds of reporting poor or fair health by men and vice versa for women. Also, Davillas et al. (2019) evaluate income and health gradients in the UK using the combination of the BPHS and USoc survey data. Their findings also support the long-term relationship between income and health, which is larger than short-term cross-sectional measures.

Studies on income and health nexus based on longitudinal analyses are also prone to different measurement problems. Gunasekara et al. (2012) evaluate the limitations in using SRH, mostly ignored as bias sources in longitudinal analyses. The first limitation is regarded as "longitudinal validity or responsiveness" (Gunasekara et al., 2012, p. 1118), and it relates to SRH skewness and its accuracy in measuring health changes over time. One example is the SRH ceiling effect, which implies individuals who had previously rated their general health as excellent cannot provide a higher response category when they feel additional improvements in their health. Second, the corresponding changes in SRH and its underlying health status measurement accuracy. The third limitation is the reference group effects, where respondents adjust their SRH health responses to their perceived reference group. The authors further find that using the 36-Item Short-Form Survey (SF-36), defined as a more detailed measure of self-assessed current health status, is less affected by the longitudinal validity bias (Gunasekara et al., 2012). Overall, these limitations point to shortcomings in using SRH as the only index to capture all aspects of health, especially in longitudinal analyses. Moreover, studies like Au and Johnston (2014) found that the weak or non-significant effects of income on health using SRH co-exist significantly with other health domains, such as mental health and well-being (Mavaddat et al., 2011). Overall, no single measure captures all the dimensions of health and well-being; hence, it is essential to be clear and detailed in specific aspects of health being evaluated as different health dimensions may move in the opposite direction in response to income changes (Apouey and Clark, 2015).

Furthermore, changes in income that can be ascribed to changes in wage-related policies are significant predictors of health and well-being outcomes (Leigh et al., 2019; Lenhart, 2019; Reeves et al., 2017). For example, introducing the National Minimum Wage (NMW) and the National Living Wage (NLW) policies in the UK and the subsequent annual uprating in wage floors has led to a continuous increase in nominal wage rates. The standard approach employed in empirical literature to evaluate such policy effects includes methods similar to natural experiments, particularly the Randomised Control Trials (Craig et al., 2017). However, limited or no studies evaluate minimum wage policy using RCTs, given the costs and other ethical considerations for conducting such experiments. Instead, previous studies follow quasi-experimental approaches such as those that compare the health outcomes of individuals who received increased wages with a homogenous comparison group who do not receive the increase (Leigh, 2021).

Empirical studies have also adopted various estimation methods to investigate both intended and unintended consequences of wage policy interventions by considering the effects of shifts in the population's income distribution. These approaches may include individuals who are not "directly" affected by the wage policy and might produce inexact policy estimates (Renson et al., 2020). However, limiting the evaluation to only the individuals who receive the minimum wage increase could also underestimate the policy's exact effects. Besides, policymakers are usually interested in the results of policy evaluations that produce encompassing estimates that quantify the changes after minimum wage policies are introduced or changed. Additionally, individuals' perception about an increase in income received by others due to a change in wage policy can serve as another pathway that connects minimum wage to health and well-being (Kronenberg et al., 2017; Leigh et al., 2019; Lenhart, 2017b; Reeves et al., 2017). The group of individuals who are indirectly affected might include: (i) the self-employed who do not receive wages; (ii) the unemployed, who might be affected by the consequent reduction in job availability, and (iii) other workers who are earning at or above the minimum wage but might experience wage compression (Buszkiewicz et al., 2021b).

2.3 Models and variables measurement

2.3.1 Income and self-rated health and well-being

The first aspect of our empirical analysis involves evaluating the relationship between income and the different aspects of health and well-being using longitudinal data. Specifically, we begin by estimating the impact of income on health and well-being measures using longitudinal data on household income and selected health and well-being outcomes. Our choice of using the fixed effects ordinal logit model is to account for any potential endogeneity stemming from time-invariant characteristics. Besides, the estimator is useful for estimating causal effects between income and health (Baetschmann et al., 2020; Ferrer-I-Carbonell and Frijters, 2004). The model is an extension of the Chamberlain (1980) fixed-effects logit model employed for binary outcomes, and hence, for dichotomous health outcomes, the ordinal outcome model approximates the Chamberlain model. Also, to the best of our knowledge, very few studies have applied panel data fixed effects models on income-health nexus using ordinal health outcomes (see Carman, 2013; Frijters et al., 2011; Frijters and Ulker, 2008; Gunasekara et al., 2012).

The model is specified as:

$$h_{it}^{*} = \alpha_i + y_{it}^{'}\beta + X_{it}^{'}\delta + e_{it} \tag{2.1}$$

$$h_{it} = k \Leftrightarrow h_{it}^{*} \in [\phi_{ik}, \phi_{ik+1}] \tag{2.2}$$

where h_{it}^{*} is the latent health variable corresponding to the health outcome for individual i at time t, while h is the observed health and well-being measure. y indicates income measured as a natural log of household after-tax and inflation-adjusted equivalised income. X is a set of observable time-varying control variables. The considered covariates include age, age-squared, age-cubed, education attainment, marital status, number of people employed in the household, and region of residence.

The parameters, β and δ are the coefficients of the main explanatory and control variables, respectively. Both indicate the direction in which an increase in the regressors impacts the cumulative distribution of the health outcome. An estimated β that is positive and statistically significant indicates that an increase in household income will cause an increase in the probability of the highest health outcome category $[\Pr(h_{it} \geq K|y_{it}, \alpha_i)]$ and a decrease in the probability of the lowest category $[\Pr(h_{it} \geq 1|y_{it}, \alpha_i)]$. α_i indicates time-invariant, individual-specific fixed effects, and e_{it} is the time-varying logit-distributed, orthogonal error term.

Equation 2.2 ties the latent outcome variable h^{*} to the observed ordered health outcome variable, h through the threshold, ϕ_{ik}, where ϕ_{ik} is the cut-off point, increasing in k, with k indicating each response category for the health and wellbeing outcomes. The estimated standard errors are clustered at the individual level given that the replications of the dichotomized outcome variable into $k-1$ copies are not independent of each other (see Baetschmann et al., 2015). The probability of observing the health category k for individual i in period t, which also depends on household income (y) and parameter (β) is given as:

$$\Pr\left(h_{it} = k | y_{it}, \alpha_i\right) = \Psi\left(\phi_{ik+1} - y_{it}^{'}\beta - \alpha_i\right) - \Psi\left(\phi_{ik} - y_{it}^{'}\beta - \alpha_i\right) \tag{2.3}$$

By extension, equation 2.3 indicates that the probability also depends on the individual-specific fixed effects (α_i) and the cut-off point (ϕ). In addition to the direction of effects (β), we evaluate the size effect of household income on health outcomes using the odds ratio (OR). OR is the ratio between the probability of a certain outcome and the complementary probability (Baetschmann et al., 2020). Consequently, the odds of an individual i in period t having a health outcome above category k relative to a lower or equal outcome to K is defined as:

$$odds\left(k, y_{it}\right) \equiv \frac{\Pr\left(h_{it} > k | y_{it}\right)}{\Pr\left(h_{it} \leq k | y_{it}\right)} \equiv \exp\left(y_{it}^{'}\beta - \phi_{ik}\right) \tag{2.4}$$

On the other hand, the changes in odds due to changes in the regressor, which depends both on the β and the regressor shift is given as:

$$OR\left(k, \Delta y_{it}\right) = \frac{odds\left(k, y_{it} + \Delta y_{it}\right)}{odds\left(k, y_{it}\right)} = \exp\left(\Delta y_{it}^{'}\beta\right) \tag{2.5}$$

where OR is the odds ratio. It implies that a unit increase in household income increases the odds ratio by about $(\exp(\beta) - 1) \times 100\%$ for all categories of health outcomes except the first.

2.3.2 Income stability, volatility, and trajectory

In addition to evaluating the impact of current household income on health and well-being using longitudinal data, we also assess the effects of income dynamics on health by pooling income experience over time on current health and well-being outcomes. Specifically, we consider various aspects of income trajectories on health outcomes including, *stability in income position*, calculated as the average of equivalised and inflation-adjusted household after-tax income. A similar approach has been employed in extant literature to assess the effects of income stability on both subjective and objective health outcomes (see Benzeval and Judge, 2001; Davillas et al., 2019; Frech and Damaske, 2019; Miething and Aberg Yngwe, 2014; Schollgen et al., 2019).

Secondly, we evaluate the impact of *income volatility* on health and well-being outcomes. There is no established consensus on measuring income volatility. The prominent approach used in literature is to measure income instability or volatility as the deviations of an individual or household's income from the average over a defined period (Prause et al., 2009; The Aspen Institute, 2016). We define income volatility as the standard deviation of the arc-percentage change in income:

$$volatility_{it} = \sqrt{Variance\left[\left(\frac{y_{it} - y_{it-1}}{\frac{(y_{it}+y_{it-1})}{2}}\right) \times 100\right]} \tag{2.6}$$

where y_{it} indicates household real disposable income for individual i in period t. The division of the change in income by the mean of current and immediate past income ensures that the size of income change is not dependent on the ordering of incomes in either year. It also adjusts for outliers and the inclusion of observations where household income is zero in both successive periods. The measure has also been shown to be closely related to the variance of transitory shocks in income using more complex models (Avram et al., 2019). We rescale both the income stability and volatility measures to standardized logged values using zero mean and a standard deviation of one to facilitate results interpretation.

Furthermore, we filter out the health effects of changes in household income by separately evaluating the extent to which the health effects of income differ for those in low and high-income groups. The increase in "deaths of despair" arising mainly from drug overdoses, alcohol, and suicide deaths have been ascribed to the stagnant and falling income levels and decreasing labour market opportunities (Allik et al., 2020; Case and Deaton, 2017). Consequently, the third aspect of our analysis involves evaluating the health effects of enduring low-income or high-income spells. We create separate variables that consider the number of years household real disposable income was below the median or above the median income. Lastly, we evaluate the differential effects of income trajectories on health and well-being before and after 2016. The pre-post analysis provides insights into the intersection of income trajectories and health outcomes coinciding with the NLW implementation.

The covariates considered include age, age-squared, age-cubed, marital status, education attainment, number of people employed in the household, and region of residence. We also include gender and ethnic group as additional covariates in the income trajectory models. The choice of covariates considered in the models follows the standard approach in most public health studies on the determinants of SRH and SWB (Blanchflower and Oswald, 2004). Age is arguably considered the foremost determinant and correlate considered in empirical wellbeing literature, with studies employing linear, U-shaped, or cubic-shaped relationships (Das et al., 2020). Gender difference is also widely acknowledged as a key determinant of the inequalities in health and well-being outcomes. In addition, the inclusion of ethnicity as a control variable is motivated by the strong association between race and reporting poorer self-reported health status (Wolff et al., 2010).

2.3.3 Health and well-being outcomes measures

As discussed in the introduction, we consider different dimensions of health and well-being outcomes, including general and mental health, satisfaction with leisure, and life satisfaction. Health measures that are based on self-reports are shown to be reliable, valid, and comparable over multiple periods by different studies including Contoyannis et al. (2004), Vaillant and Wolff (2012), and Dasgupta (2018), among others. Also, Simon et al. (2005) show that assessing general health status through SRH can encompass health dimensions beyond physical health, including mental and other health-related behaviours. However, SRH remains a health measure in the general population that remains poorly understood (Gunasekara et al., 2012). Therefore, using SRH as the only index to capture all aspects of health in the analysis of income and health relationships can lead to incorrect inference. For example, while some studies found income to have little or no effect on SRH, the non-significant effects of income on health outcomes using SRH are found to co-exist significantly with other dimensions of health (see Au and Johnston, 2014). Hence, a separate evaluation of income effects on mental health outcomes will further provide a detailed analysis of the income-health nexus specific to cognitive health, including anxiety and depression, social dysfunction, and loss of confidence.

The prevalence of mental health problems and their measurement could be daunting due to the hidden nature of mental health issues and variations in diagnostic practices across countries. There are also diverse mental health measures across different countries, making it difficult and sometimes impossible to comparatively determine the prevalence of mental health problems across countries because of differences in the methodological approach used in measurements. However, longitudinal data may provide reliable evaluation and statistically comparable estimates over time since similar methods and techniques are repeatedly adopted in mental health measurements (Garcia and Marder, 2017).

Beyond health, the constructs and proxies employed in empirical research to measure subjective wellbeing are also vast and sometimes intertwined with different physical and mental health dimensions. For example, the items of the General Health Questionnaire 12-item version, the underlying construct mental health widely employed in empirical research, have questions including *"enjoying day-to-day activities and problem overcoming difficulties"* which could be indicative of poor satisfaction with life, and a major proxy for SWB. Nonetheless, there are some salient differences between the different constructs for self-reported health and wellbeing measures. Additionally, the corresponding estimates emanating from these measures should be differently interpreted given the attached different importance and weights people assign to the different health and wellbeing domains when reporting in surveys (see Powdthavee and van den Berg, 2011). The measurement indicators for the health and well-being outcomes considered in this chapter are discussed as follows:

General health

The health indicator used in empirical studies is usually based on how survey questions and responses are constructed. However, most surveys treat health outcomes using ordinal measures, with good health being better than poor health (Frijters and Ulker, 2008). The USoc collects information about participants' health status. Specifically, it asked respondents to rate their general health condition on a five-scale from 1 indicating excellent health to 5, poor health status. However, we inverted the responses by recoding 1 to indicate poor health and 5 to indicate excellent health to facilitate consistency in interpreting the estimated results.

Mental health

The General Health Questionnaire (GHQ-12) has been widely adopted in mental and health research as the validated screening tool for psychiatric illness (Griffith and Jones, 2019; Kronenberg et al., 2017). Hu et al. (2007) performed exploratory and confirmatory factor analyses of data extracted separately from both the BHPS and Health Survey for England (HSE). They confirmed that GHQ-12 consistently measures different dimensions of mental health in population-based research (Hu et al., 2007). The GHQ-12 questions comprise six positively worded and six negatively worded questions that describe respondents' mood states over a few weeks before the interview (Brown et al., 2021a). The sub-components of the GHQ-12 questions are recoded and summed to a single scale from 0 to 12. However, we reversed the caseness score to increase from 1 (most distressed) to 13 (least distressed) to measure mental health and easier results interpretation.

Leisure and life satisfaction

Leisure satisfaction has also been widely used in health studies as a core mediating or outcome variable (Biswas-Diener, 2008). Moreover, leisure satisfaction may unfold in various degrees during a person's life course, and consequently, may relate more or less strongly to health and overall life satisfaction (Gelissen, 2019). We measure leisure and life satisfaction using the respective indicators from the USoc. The leisure satisfaction ranks respondents' satisfaction with the amount of leisure time. Similarly, life satisfaction is the raking of their overall life over a seven-scale from completely unsatisfied to completely satisfied. The extent to which people are satisfied with their leisure time and activities has been employed in the empirical literature, and it's an important predictor of their overall well-being. For example, Gelissen (2019) found satisfaction with leisure time a consistent indicator of overall leisure satisfaction.

2.4 Data and descriptive statistics

The data used for the empirical analysis are drawn from waves 1 to 11 of the USoc covering periods between 2011 and 2019. The USoc is an annual survey of members of approximately 40000 households selected across the four countries comprising the United Kingdom, including England, Scotland, Wales, and Northern Ireland. The selected households from the first wave were repeatedly followed over time, making the survey one of the largest longitudinal surveys of its kind. The study is built on the BHPS, which ran between 1991 and 2009, by incorporating the approximately 8,000 previous households captured in the original BPHS. Also, it covers all age groups, and it is multi-topic, covering a range of social, economic, and behavioural factors. Also, it is designed with an Immigrant and Ethnic Minority Boost sample by allowing for increased sample sizes for different ethnic minority and immigrant groups. Buck and McFall (2011) and McFall et al. (2017) provided a complete discussion on the survey design overview and sample structure.[2]

The quality and applicability of the USoc data in empirical and policy research is well-rooted and demonstrated in earlier research, including studies on income, health, and well-being (Avram et al., 2019; Davillas et al., 2019; Knies, 2017). Besides, *USoc* has been described as "the data source for many research papers where income plays a central role, even if it is not the main outcome variable of the analysis" (Fisher et al., 2019, p. 3). Also, using USoc as the single data source allows the study of different health outcomes for the same individual over time, using the same controls. Besides, it eliminates the difference between estimates found in studies that use other data sources.

Using the USoc data also ensures a balanced cross-section of individuals over time. Data collection for each survey wave usually takes over 24 months, with the collection period for different successive waves overlapping, thus giving a complex data design. However, we collect data for all the variables of interest by pooling them correspondingly across different intersecting waves and harmonising them into uniquely identified calendar years to ensure that our analysis sample is nationally representative (see Kaminska and Lynn, 2019). Therefore, the calendar periods for empirical analysis after the harmonisation start and end in 2011 and 2019.

Following our main objective, we restrict the analysis to a sample of respondents with valid household income data throughout the survey periods under consideration. Lastly, as earlier discussed, we partitioned the analysis into periods before and after 2016. The summary of means and standard deviations for household income and the considered health outcomes

[2] A complete user guide of the USoc survey is provided by (University of Essex, Institute for Social and Economic Research, 2022).

are summarised in Table 2.1. The descriptive statistics show that the average real household disposable income increased with less deviation from the average for the 2016-2019 sub-sample, increasing real disposable income across households over time. However, the health and well-being indicators are comparably similar across the three sub-samples.

Table 2.1 Descriptive statistics: income and health outcomes

	Full sample		***2011 - 2015***		***2016 - 2019***	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Household disposable income	1879.82	1950.72	1866.94	2074.77	1895.92	1783.42
Health & well-being outcomes						
Self-rated general health	3.36	0.06	3.45	1.07	3.25	1.04
Mental health (GHQ-12)	11.37	2.93	11.38	2.89	11.35	2.98
Leisure satisfaction	4.88	1.66	4.79	1.69	4.99	1.62
Life satisfaction	5.23	1.43	5.22	1.45	5.24	1.42

Note: The Table summarises the mean and standard deviation of the main variables of interest: household income and health and well-being measures. Household disposable income is the equivalised and inflation-adjusted household net income. The full summary statistics for other variables including the covariates are presented in Table A.1 in Appendix A.

2.5 Results and discussions

2.5.1 The longitudinal fixed-effects model for health and well-being outcomes

Table 2.2 summarises the coefficients of the estimated fixed-effects model for the health and well-being indicators. The main explanatory variable is the log of household disposable income, inflation-adjusted and equivalised using the OECD-modified equivalence scale. The use of equivalised after-tax income is mainly to adjust for family size and composition (Miething and Aberg Yngwe, 2014). In addition, the log transformation of income accounts for the skewness in the income distribution. We considered different covariates, including age, age-squared, age-cubed, marital status, number of household members in employment, educational attainment,

Table 2.2 Fixed effects ordered logit model of health and well-being outcomes

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Household disposable income (log)	0.056***	0.150***	-0.067***	0.102***
	(0.016)	(0.019)	(0.016)	(0.016)
Age	-0.151***	-0.260***	-0.311***	-0.257***
	(0.029)	(0.031)	(0.026)	(0.029)
Age-squared	0.000	0.005***	0.007***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Age-cubed	-0.000	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Marital status				
(Ref: *Never married*)				
Married or Cohabiting	-0.104*	0.221***	-0.032	0.359***
	(0.058)	(0.060)	(0.050)	(0.055)
Unmarried	0.020	-0.053	0.131**	0.111*
	(0.067)	(0.069)	(0.059)	(0.063)
Education Attainment				
(Ref: *No qualification*)				
Other qualification	0.285	-0.367*	-0.319**	-0.203
	(0.173)	(0.206)	(0.152)	(0.161)
GCSE; A-level; etc	0.316	-0.064	-0.059	0.045
	(0.195)	(0.230)	(0.176)	(0.185)
Degree and other higher degrees	0.354*	0.032	-0.222	0.047
	(0.204)	(0.241)	(0.186)	(0.199)
Number employed in the household	0.028**	0.057***	-0.160***	0.020
	(0.014)	(0.016)	(0.013)	(0.014)
Region dummies	Yes	Yes	Yes	Yes

Note: ***, ** and * denotes statistical significance at 1%, 5% and 10% levels respectively. The standard errors are clustered at the individual level and are presented in parentheses. Household disposable income (log) is the log of the equivalised and inflation-adjusted after-tax household income SRH denotes self-rated health, while GHQ-12 is the mental health indicator. The models are estimated using the fixed effects ordered logit model.

and region of residence. It is worthy to note that the magnitude of the coefficients for each of the health and wellbeing measures cannot be directly compared because of the different constructs and dimensions of health and wellbeing that each represents (see Powdthavee and van den Berg, 2011). Also, the outcomes have different scales of measurement denoting different dimensions of health and wellbeing they measure.

Household real disposable income positively affects general health, mental health, and life satisfaction, while the effect is negative for satisfaction with leisure time. The significant positive coefficients indicate that an increase in household disposable real income increases the probability of reporting excellent SRH, the least distress in mental health outcome, and complete satisfaction with overall life. At the same time, it decreases the likelihood of reporting the lowest category for the three outcomes, respectively. An increase in household disposable income increases the odds ratio of reporting improved SRH for all categories from fair to excellent health status, by about 5.76% $[(\exp(0.056)-1)]$ (see Column I in Table 2.2).

Additionally, the odds ratio for GHQ-12 is about 16.18% $[(\exp(0.150)-1)]$ to report less distress in mental health outcomes. Life satisfaction has an odds ratio of about 10.74% $[(\exp(0.102)-1)]$ (Table 2.2 column II). This indicates an increased likelihood of reporting improved satisfaction with overall life as household income increases. On the contrary, the coefficient of the log of household disposable income for the leisure satisfaction estimation is negative and significant, indicating a reduction in the likelihood of reporting improvement in leisure satisfaction as income increases.We evaluate the potential moderating effects of the changes in household labour supply on the negative association between household disposable real income and leisure satisfaction. We achieve this by including the interaction of household disposable income and a binary indicator of the number of people employed in the household. The estimated results are summarised in Table A.2 in Appendix A. The result shows that household labour supply has a significant moderating negative effect on the relationship between household income and leisure satisfaction. Thus, indicating that the declining leisure satisfaction with increasing household income is driven by the increase in the household members' labour supply. The plot of the marginal effects of the interaction term between household disposable income and labour supply on leisure satisfaction depicted in Figure 2.1 further shows an inverted U-shaped relationship between the interaction of household labour supply with disposable income and leisure satisfaction. Thus, this indicates that as the household supply of labour increases, leisure satisfaction begins to increase with household income but at a declining rate (see also Rätzel, 2012).

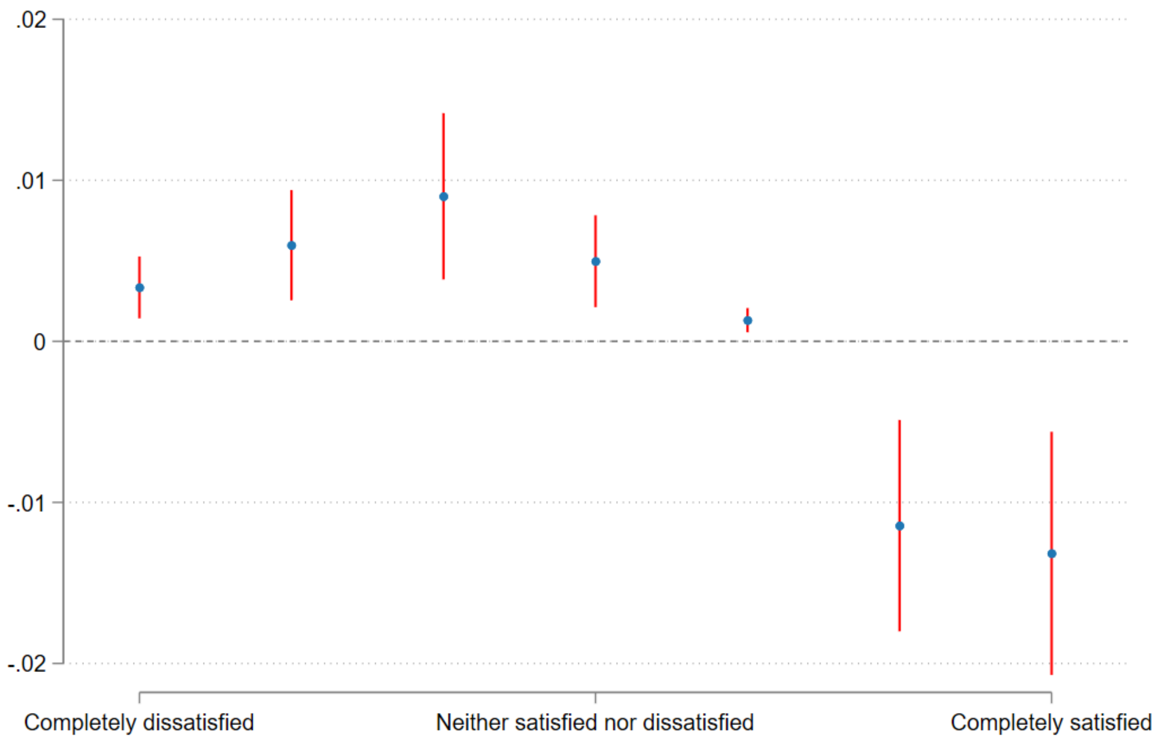


Fig. 2.1 Marginal effects of labour supply and household disposable income on leisure satisfaction

The estimated results in Table 2.2 further show that age and its cubed term both have negative and statistically significant coefficients for all the health and well-being outcomes, while age-squared is positive and significant, except for SRH. Thus, the results confirm the declining likelihood of reporting improved health and well-being as age increases. Besides, the results support findings in the literature on the non-linear relationship between age and health status (Lorem et al., 2017; Moret et al., 2007). The estimated coefficients of marital status for the SRH are not statistically significant, indicating no significant difference in the likelihood of reporting changes in health and well-being status as an individual's marital status changes. Similarly, the significant coefficients for education attainment indicate an increased likelihood of reporting improved SRH as education attainment improves. On the other hand, the estimated results for the three other outcomes; mental health, leisure, and life satisfaction, show mixed signs and significance. Specifically, the non-significant coefficients indicate no significant difference in the likelihood of reporting improved health and well-being status as an individual's marital status or education attainment changes.

Finally, the number of household members in employment increases the likelihood of reporting improvements in general and mental health as well as life satisfaction but a decrease in the probability of reporting improved satisfaction with leisure. For improvement in SRH from poor to excellent, the compensating variation[3] between having more members of the household that are working and an increase in household income is about 0.50 $\left(\frac{0.028}{0.056}\right)$. The value implies that the log of household disposable income must increase by about 35.91% $[\exp(0.50) - 1]$ to compensate for every unit reduction in the number employed in households for an individual's general health status to change from poor to excellent. On the contrary, the results show that a higher number of household members in employment has a negative and significant coefficient with leisure satisfaction.

2.5.2 Income trajectories and health outcomes

Next, we estimate cross-sectional regressions using different constructs of income gradients and spell duration and their effects on health and well-being outcomes. The summary of the

[3] The compensating variation is computed as the ratio of the corresponding coefficients of the regressors (Baetschmann et al., 2020)

estimated coefficients and odds ratios for the different income gradient models across the health and well-being indicators are summarised in Table 2.3. The estimated models also control for age, including its squared and cubed terms, gender, marital status, education status, number of people employed in the household, ethnicity, and region of residence. The results summarised as Model I in Table 2.3 show that stability in household real disposable income position over the period under consideration is positive and significantly impacts all the health and well-being outcomes. The estimated coefficients and odds ratios show that average household income increases the likelihood of reporting better and improved general and mental health outcomes. Stability in income position also increases the odds of reporting improved satisfaction with leisure time and overall life.

Model II summarises the estimated results for the second income gradient model, which evaluates the volatility in household income position on health and well-being. The estimated odds ratios show that the estimated coefficients of household income volatility are negative across the health and well-being outcomes but statistically significant for mental health and life satisfaction. The results suggest that increased volatility in household disposable real income decreases the likelihood of reporting improvements in health and well-being.

Models III and IV in Table 2.3 explored the length of time individuals endure low-income and high-income spells. Each variable comprises nine categories between never below (or above) the median income up to a maximum of eight periods below (or above) the median income. [4] An increase in the number of years that household real disposable income is below the median income significantly reduces the odds ratio of reporting improved health and well-being. On the contrary, the increase in the length of time when the family's real disposable income is above the median income increases the odds ratio for reporting excellent SRH, less distress in mental health, and complete satisfaction with leisure and life.

[4]Annual median income is computed using the income information in the USoc data and calculated separately for each calendar year between 2011 and 2019 (see also Miething and Aberg Yngwe, 2014).

Table 2.3 Income gradients and health & well-being outcomes

		SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Model I	*Income stability*	0.282***	0.122***	0.201***	0.280***
		(0.020)	(0.023)	(0.020)	(0.020)
Model II	*Income volatility*	-0.007	-0.057***	-0.030*	-0.083***
		(0.017)	(0.018)	(0.016)	(0.017)
Model III	*Below median income*	-0.066***	-0.032***	-0.047***	-0.050***
		(0.007)	(0.007)	(0.006)	(0.007)
Model IV	*Above median income*	0.066***	0.032***	0.047***	0.050***
		(0.007)	(0.007)	(0.006)	(0.007)

Note: ***, ** and * denotes statistical significance at 1%, 5% and 10% levels respectively. The standard errors are clustered at the individual level and are presented in parentheses. Household disposable income (log) is the log of the equivalised and inflation-adjusted after-tax household income SRH denotes self-rated health, while GHQ-12 is the mental health indicator. All models are estimated using the ordered logit model and the full estimation results are provided in Appendix A. The covariates considered include age, age-squared, age-cubed, gender, ethnicity, marital status, number of household members in employment, and region of residence.

Overall, the results indicate that long-term income and stability in income position over time increases the likelihood of reporting improved physical and mental health and increased satisfaction with leisure and life. On the contrary, volatility in household income and household position on the general income distribution are significant predictors of health and well-being.

2.5.3 Partitioned analyses: low- and high-income households

We evaluate the variations in income gradients on health outcomes by separately estimating the health effects of income stability and volatility for individuals in low- and high-income households. The estimated results in the preceding section provide estimates of the expected variations in household income position and other covariates on the expected level of health outcomes across individuals irrespective of their income position in the distribution. However, the effect of income on health and well-being is not uniform throughout the distribution of income (Marmot, 2002). Besides, recent studies that extended the conventional regression approaches in evaluating the health effects of income using distributional regression techniques found that households with poor income are particularly faced with more significant health risks. Also, they are at the lower end of the health distribution (Kessels et al., 2020; Silbersdorff et al., 2018). Using the health and income distribution data in 2019, Fig. 2.2 *(A - D)* depicts distributions of health and well-being indicators for households in the bottom and top 20 percent of the equivalised net income distribution. The figures show substantial variations in health outcomes between households in the lower and upper part of the income distribution. For example, low-income households are skewed to have more risks of poor general health and most distressed mental health. Similar variations are noted for satisfaction with leisure time and overall life (see Fig. 2.2).

Consequently, we compare the health effects of stable and volatile income for individuals in low-income (bottom 20%) and high-income (top 20%) households. The classification of households into low and high income is based on the distribution of income using 2019 equivalised household monthly disposable income.[5]. The results summarised in Table 2.4 show that the estimated coefficients and odds ratios for average household disposable income are positive across all the health and well-being outcomes for individuals in low-income households. However, it is statistically significant for general health and life satisfaction. In a similar vein, the estimated coefficients and odds ratios are positive for individuals in the high-income quartile. Again, it is statistically significant across all the health and well-being outcomes.

On the other hand, income volatility is positive but not statistically significant for low-income households, while it is negative across all the other health and well-being outcomes but statistically significant for mental health and life satisfaction. For the high-income quartile, the estimated results are positive for all outcomes, except for life satisfaction, which is not statistically significant. Overall, the results suggest that income stability is vital for health and well-being irrespective of the household's income or position on the income distribution ladder. In contrast, volatility in household disposable income significantly affects low-income

[5]2019 income was selected to correspond to the current year used for the regression estimations. It is also the last data period considered in the study for completeness and as discussed in Section 2.4

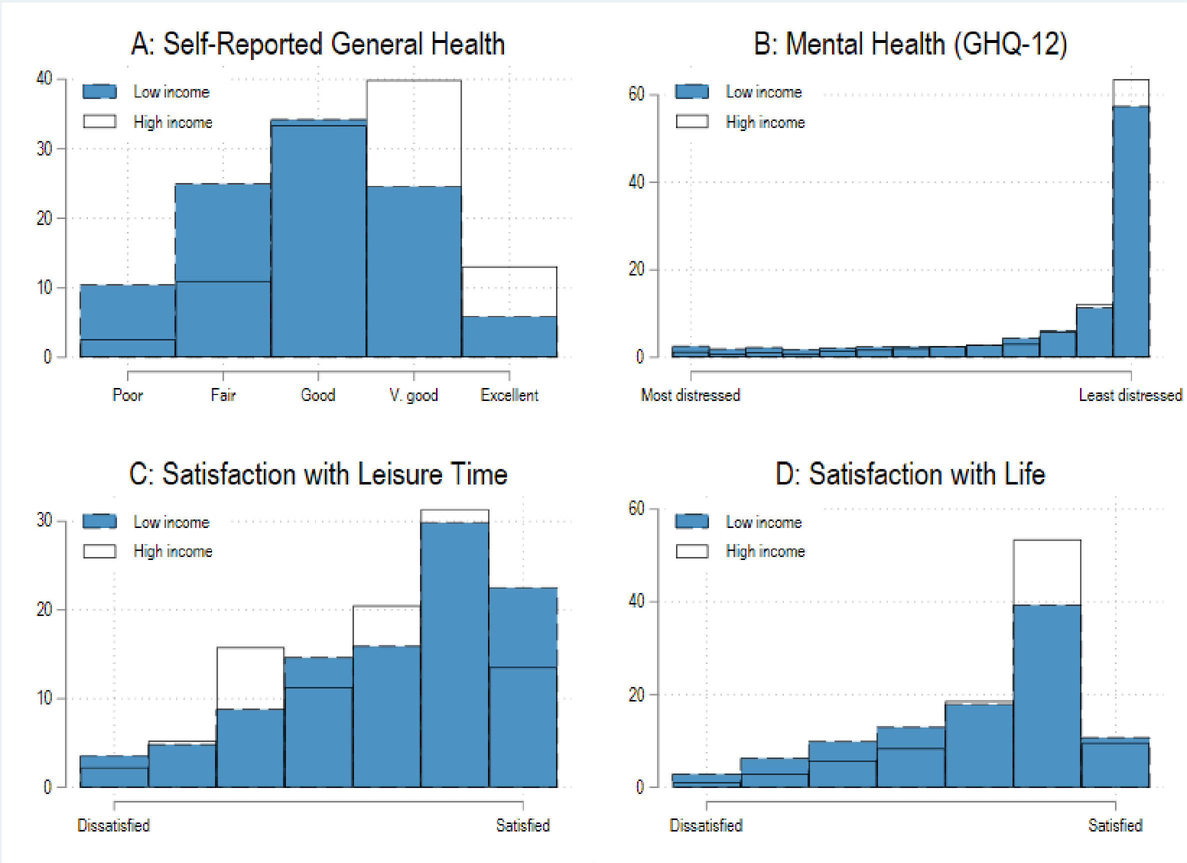


Fig. 2.2 Distribution of health and well-being outcomes

households. Volatile income is associated with the increasing likelihood of reporting poor health and well-being outcomes.

2.5.4 Sub-sample analysis: before and after 2016

In this section, we discuss the results of the sub-sample estimation before and after implementing the NLW policy. While the results do not provide any causal estimates of the policy, they provide insights into the likely changes in size and magnitude of income effects on health and well-being outcomes. Therefore, we focus the discussion mainly on the estimates of the log of household disposable income and the various income trajectory indicators considered in the previous analysis, before and after 2016. The results are summarised in Table 2.5.

Table 2.4 income gradients and health & well-being outcomes: low and high income households

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Panel A - Bottom 20% household				
Income stability	0.104**	0.059	0.136***	0.170***
	(0.050)	(0.052)	(0.049)	(0.049)
Income volatility	0.002	-0.083**	-0.025	-0.085**
	(0.032)	(0.036)	(0.034)	(0.034)
Panel A - Top 20% household				
Income stability	0.360***	0.107*	0.178***	0.313***
	(0.049)	(0.055)	(0.051)	(0.050)
Income volatility	0.070*	0.037	0.075**	-0.016
	(0.040)	(0.041)	(0.037)	(0.040)

Note: ***, ** and * denotes statistical significance at 1%, 5% and 10% levels respectively. The standard errors are clustered at the individual level and are presented in parentheses. Household disposable income (log) is the log of the equivalised and inflation-adjusted after-tax household income SRH denotes self-rated health, while GHQ-12 is the mental health indicator. All models are estimated using the ordered logit model. The covariates considered include age, age-squared, age-cubed, gender, ethnicity, marital status, the number employed in the household, and region of residence.

The estimated results using the fixed-effect models are summarised as the baseline model in Table 2.5. Current household disposable income is significantly associated with all the health and well-being outcomes considered, except for SRH and life satisfaction in the pre-2016 and post-2016 results respectively. Summarily, the non-significance for SRH estimation using the pre-2016 sample suggests that household income exerts a stronger effect on self-reported health in the periods following the NLW policy. On the other hand, the pre-2016 estimate shows that household income has a strong positive effect on life satisfaction, while the post-2016 estimate is not statistically significant. This could further speak to the dampening effects of the various fiscal and welfare austerity policies which Brown et al. (2021b) found to primarily affect individual life satisfaction through different ranges of the economic expectations channels.

Furthermore, Models I to IV show the estimated coefficients of the different income trajectories on health and well-being outcomes. Similar to the estimated results using the entire data sample 2.3, the estimated coefficients for average income are significant across all the health and well-being indicators, both before and after NLW. In contrast, the estimated coefficients of income volatility in Model II are significant in the post-2016 period (see Table 2.5).

To statistically evaluate the differences in the income effects on the health and well-being outcomes between the sub-sample periods, we compare the odds ratios of income trajectories on the various health and well-being outcomes before and after 2016.[6] The plausibility of comparing estimates between logistic regression models for different sub-samples using some of the popular methods available for linear regression models has been well discussed in the literature (Allison, 1999; Kuha and Mills, 2020; Mood, 2010; Williams, 2009). Therefore, we followed the procedure proposed in Buis (2015) to evaluate differences in the odds ratios in the pre- and post-2016 estimation for each income trajectory variable: income stability, income volatility, and length of time above and below the median income. The computed Wald tests are summarised in Table 2.6. The results show that the predicted differential in the odds ratios is statistically significant across the models. By implication, the results suggest that there are statistically significant differences in the estimated odds ratios of the income trajectory indicators before and after 2016. Overall, the results suggest significant shifts in the income trajectories effects on health and well-being outcomes, before and after implementing the NLW policy. Also, the result is suggestive that the NLW policy has direct effects on health and well-being outcomes.

[6]The procedure followed in computing the Wald tests is only compatible with cross-sectional, non-clustered standard errors. However, the fixed-effects ordinal logit model employed in the baseline model (Table 2.5) requires fitting the model with clustered standard errors. More importantly, using the estimator without clusters can be misleading (see Baetschmann et al., 2020, p. 270). Hence, we report the Wald test results for Models 1 – IV.

Table 2.5 Fixed effects ordered logit model - Before and After 2016

	Before 2016				After 2016			
	SRH	GHQ-12	Leisure satisfaction	Life satisfaction	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Household disposable income (log)	0.025	0.115***	-0.072***	0.106***	0.058**	0.128***	-0.068***	0.038
	(0.025)	(0.030)	(0.024)	(0.026)	(0.027)	(0.029)	(0.025)	(0.026)
Age	0.003	-0.212***	-0.328***	-0.230***	-0.305***	-0.399***	-0.389***	-0.363***
	(0.053)	(0.063)	(0.051)	(0.057)	(0.092)	(0.098)	(0.083)	(0.089)
Age-squared	-0.001	0.004***	0.008***	0.005***	0.001	0.006***	0.007***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Age-cubed	0.000	-0.000***	-0.000***	-0.000***	-0.000	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Marital status								
Ref: *Never married*								
Married or Cohabiting	-0.114	0.148	-0.058	0.229***	0.088	0.341***	0.054	0.242**
	(0.083)	(0.091)	(0.077)	(0.086)	(0.112)	(0.120)	(0.098)	(0.110)
Unmarried	0.017	-0.222**	0.071	-0.047	0.199*	-0.016	0.233**	0.125
	(0.106)	(0.112)	(0.091)	(0.101)	(0.112)	(0.119)	(0.099)	(0.104)
Education Attainment								
Ref: *No qualification*								
Other qualification	0.297	-0.290	-0.226	-0.057	-0.102	-1.639***	-0.606	-0.530
	(0.229)	(0.238)	(0.202)	(0.231)	(0.376)	(0.506)	(0.387)	(0.415)
GCSE; A-level; etc	0.325	0.089	-0.145	-0.312	-0.150	-1.779**	0.145	-0.269
	(0.237)	(0.309)	(0.221)	(0.269)	(0.597)	(0.696)	(0.579)	(0.556)
Degree and other higher degrees	0.384	-0.107	-0.277	-0.346	0.174	-1.168*	-0.168	-0.151
	(0.259)	(0.324)	(0.246)	(0.294)	(0.611)	(0.708)	(0.593)	(0.577)
Number employed in the household	0.026	0.117***	-0.125***	0.042**	0.004	0.079***	-0.151***	0.055**
	(0.020)	(0.023)	(0.019)	(0.021)	(0.026)	(0.030)	(0.025)	(0.026)
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: ***, ** and * denotes statistical significance at 1%, 5% and 10% levels respectively. The standard errors are clustered at the individual level and are presented in parentheses. Household disposable income (log) is the log of the equivalised and inflation-adjusted after-tax household income SRH denotes self-rated health, while GHQ-12 is the mental health indicator. The models are estimated using the fixed effects ordered logit model.

Table 2.6 Wald tests of coefficients, before and after 2016

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Model 1 - Average household income	0.791***	0.923***	0.836***	0.780***
	(0.016)	(0.019)	(0.017)	(0.016)
Model II - Income volatility	1.018*	1.044***	1.032***	1.066***
	(0.011)	(0.011)	(0.011)	(0.011)
Model III - Below median income	1.114***	1.048***	1.088***	1.081***
	(0.015)	(0.015)	(0.015)	(0.015)
Model IV - Above median income	0.898***	0.954***	0.919***	0.925***
	(0.012)	(0.014)	(0.013)	(0.013)

Note: ***, ** and * denotes statistical significance at 1%, 5% and 10% levels respectively. Cluster robust standard errors are reported in parenthesis. SRH denotes self-rated health, while GHQ-12 is the mental health indicator. Leisure is satisfaction with leisure time, and life denotes satisfaction with overall life.

2.6 Robustness check - testing for attrition

Attrition in longitudinal analyses is a major concern in panel analyses as it poses a threat to the quality of data and could result in biased estimates (Frijters et al., 2005a). For example, if specific types of individuals or demographics within the population have a higher tendency to attrite are more likely to attrite, it implies that the non-attriting samples are different from the population and the estimated results might not be generalizable to the larger population context.

Even with the use of state-of-the-art survey designs in world-leading longitudinal household surveys including the use of mixed data collection methods, multiple visitations, longer fieldwork duration, and the use of sampling boosts, substantial rates of non-responses are still recorded. Testing for attrition using any of the prominent methods including the attrition probits regression method (Fitzgerald et al., 1998) and the pooling test (Becketti et al., 1988) among other methods, requires comparing the samples that remained in the study with those that dropped out. Hence, it is expected that there would be larger samples at the start period than in the last period observed. However, this is not the case with the USoc data as new samples were included in subsequent waves. To ensure that the study accurately reflects the wider UK population and to account for inevitable attrition, the Understanding Society study from its outset usually includes boosted samples that mainly include ethnic minority households. Hence, additional respondents joined the sample over time. Considering attrition between the start and end periods would imply that I drop these samples who did not have information at the starting period and this is also a limitation.

Nonetheless, we formally test for attrition using the "Attrition Probits" approach. We estimate a probit model in which the dependent variable is one for individuals who dropped out of the sample after the first wave and zero otherwise. The explanatory variables include the lagged values of the outcome variable and the baseline values for all variables that could affect the outcomes of interest and other variables that characterize the survey quality all of which may be correlated with attrition. The considered variables include age, sex, ethnicity, income quintiles, marital status, educational qualification, employment status, whether in a full-time or part-time job, number of kids in the household, and region of residence.

The attrition probit results are summarised in Table A.8 in Appendix A. The pseudo R-squared suggests that the considered baseline variables explain only about 4% and 6% of panel attrition between 2011, the base period, and the two end periods considered in the partitioned analysis - 2016 and 2019 respectively. Thus, indicating the attrition is random. The probit estimates further show differences in the variables that are significant predictors of attrition in the two periods (see Table A.8). Similar to the findings in Cabrera-Álvarez et al. (2023) on the evolution of panel attrition in the USoc data focusing on the drop-in response rates from wave to wave. They considered several variables including health and income variables for the general population sample as well as the Immigrant and Ethnic Minority Boost of the survey. They find the cumulative attrition in the USoc data is about 60% between the 2nd and 11th waves with the subgroups more likely to attrite including young people, ethnic minorities, participants with poor health, those on lower incomes, full-time students or unemployed, singles, participants with no qualifications, renting their houses and lone parents, and those that are unemployed (see also Lynn and Borkowska, 2018). However, their findings show that including the survey weights in estimations using the USoc data is able to mitigate the impact of attrition.

2.7 Conclusion

This chapter contributes to the growing literature on the income and health relationship by evaluating how income dynamics affect various subjective health and well-being outcomes. We explore different aspects of income experience on health and well-being using data from the Understanding Society's UK Household Longitudinal Study between 2009 and 2019. In addition to fully exploiting the longitudinal dimension of the data, our empirical approach accounts for the time-invariant endogeneity in income-health relationships. We also explore the health implications of changing income trajectories, including stability and variability in income position and duration of spells in low and high income.

The estimated fixed effects model results show that increased household income is associated with an increased likelihood of reporting excellent general health outcomes, the least distress in mental health outcomes, and more satisfaction with life. These results are comparable to Davillas et al. (2019) who found that individuals who reported excellent health had higher household incomes than those likely to report lower SRH category. Analogous to the findings for life satisfaction, Knies (2017) previously found that UK households with higher family income have members who are more satisfied with their lives than those in less-income households. However, the negative coefficients for leisure satisfaction corroborate the labour supply decisions theory which assumes a trade-off between income and leisure (Chadi and Hetschko, 2017).

Additional results from the cross-sectional regressions show that income trajectories and time dimension matter for self-reported health and well-being. We find that stability in household disposable income is positive and significantly associated with the considered health and well-being outcomes. We also find that the length of time individuals endure low (high) income reduces (increases) their odds ratio of reporting improved self-rated health and subjective well-being. On the other hand, the negative coefficients for income volatility coupled with their position on the income distribution may suggest that unstable household income is associated with a declining trend in health and well-being. This could further be related to the submission in Wilson (2020) that observed increased wages in the UK had not reflected in comparable changes in price levels with income post-tax and national insurance contribution making people feel poorer at the end of every month (Office of National Statistics, 2018). Concurrently, there is a continuous increase in "people's concerns about the general economic outlook and decreased real household spending per person" (Office of National Statistics, 2020).

Lastly, the sub-sample analyses using the sample partitioned into periods before and after the NLW policy implementation show significant results. Thus, it suggests a significant shift in the effects of income trajectories on health and well-being following the NLW policy implementation. Nonetheless, these findings do not provide the actual causal effects of the NLW policy on the relationship between income dynamics and health outcomes. One of the implications of these findings is that policies designed to address health and well-being problems must consider income volatility as an important source of risks, along with existing socioeconomic factors. The significance of volatile incomes especially for low-income households further suggests that existing social policies and safety net programmes designed targeting low-income individuals must also be designed with strengthened measures that deal with rising income volatility.

Chapter 3

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

3.1 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 improving the situation of low-wage workers. The government seeks to increase the benefits accrued to low-wage earners from the growth and expansion experienced in the economy relative to similar developed economies after the 2008 Global Financial Crisis (GFC). The NLW policy is aimed at ensuring that work pays by reducing reliance on government supplementing earnings through the benefits system. 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 national 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 (see Averett et al., 2017; Buszkiewicz et al., 2021a; Horn et al., 2017; Leigh et al., 2019, among others). Attention has also extended to other countries and regions, including Canada (Bai and Veall, 2023), United Kingdom (Kronenberg et al., 2017; Lenhart, 2017a; Maxwell et al., 2022; Reeves et al., 2017), Germany (Hafner and Lochner, 2021), China (Chen, 2020), the Organisation for Economic Co-operation and Development (OECD) countries 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, healthy diet, nutrition, and suicide, etc (see Andreyeva and Ukert, 2018; Kaufman et al., 2020). 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 during the 2015 national budget announcement also introduced a four-year freeze on several working-age welfare benefits and tax credits. The four-year benefits freeze was part of the national government's series of welfare reforms aimed at supporting efforts to increase employment and support the policy of rewarding hard work and increasing fairness to working households by reducing workers' dependence on state benefits. 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. The monetary value of welfare support received by affected individuals was maintained at the 2015 rate, rather than the annual uprating with the prevailing inflation rates every year. However, the value of benefits received remains the same in nominal terms, provided the claimants continue to meet the eligibility conditions. The implication of the benefits freeze policy is that there is a reduction in affected benefits rates in real terms. The affected benefits include the main rates of Income Support, Housing Benefit, Job-seekers Allowance, Employment and Support Allowance, Child and Working Tax Credits, and the Universal Credit and other legacy benefits that it replaced. We hypothesize that the negative effects of the freeze could choke off the positive benefits of the 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 the benefits freeze policies. Literature in the past often considers wage policy to be unrelated to the expansion or contraction of other safety net programs (Rothstein and 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 the deaths of despair (Dow et al., 2020). The simultaneous implementation of the NLW policy with the freeze on working-age benefits makes the setting of the minimum wage policy in the UK unique to study, further providing the main contribution of this chapter. Besides, both policies led to a decline in the gross earnings of affected individuals (Barnard, 2019).

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. 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). 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 huge societal burden and high economic costs associated with mental disorders, understanding 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, following recent developments in the difference-in-differences (DID) setup, allowing us to identify and estimate policy-relevant disaggregated and interpretable causal parameters (see Callaway and Sant'Anna, 2021). Recent literature that investigated the health effects of the UK NLW employed the canonical DID method by assuming that each wage upratings 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 the labour market outcomes and mostly in the US (Cengiz et al., 2019; Dube et al., 2016). 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-group framework could have missed just by examining the yearly increases in wage rates (Borusyak et al., 2021; Redmond and 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 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. Finally, the sensitivity analyses confirmed the validity of our results to post-treatment violations of the parallel trend assumptions.

3.2 Background on austerity and welfare benefits 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 be connected 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 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 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, Davis et al.'s (2021) evaluation of the extent to which the NLW and the Universal Credit (UC) could facilitate achieving a minimum living standard for the UK populace shows that the rising costs of living increased at a higher rate than the increase in the UC. They 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. 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.

Moreover, 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 and 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). Additionally, 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 and Wang, 2017).

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, 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. 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.

3.3 Literature review: wage policy and health outcomes

Empirical research has consistently demonstrated that income affects health and health behaviours through various channels. These channels can be broadly categorized into three. 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., 2022b; 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 (see Dube, 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).

The transmission mechanisms through which wage policies affect health outcomes and health inequalities are considerably interconnected but with no unique theoretical hypothesis or empirical consensus. In the past, public policies on healthcare improvements have mostly focused on allocating healthcare resources to address the consequences rather than the pre-disposing factors that cause poor health (McGinnis and Foege, 1993). Also, past literature succeeded in explaining the extent and existence of health disparities rather than the reason for the persistence of these health inequalities (Feinstein, 1993). To address some of the mismatches, studies shifted focus to identifying the pathways and mechanisms linking health inequalities to various socioeconomic factors, including income (see Adler and Newman, 2002; Hoffmann et al., 2018; Lahelma et al., 2004; Lenhart, 2019). Evaluating these pathways and designing appropriate policy actions can be appreciated by identifying the sources of public health inequality and their connection with income. However, while public health practitioners "tend to interpret the income gradient in health as a symptom of inequity in the distribution of health", economists consider the income gradient as reflecting the constraints imposed by ill health to generate earnings (O'Donnell et al., 2015, pp.1421).

Over the last two decades, significant progress has been recorded with an increasing number of empirical studies that evaluate the pathways linking changes in income and health. Leigh et al. (2019, pp.122) acknowledged that there has been “a surge of interest in income and health literature studies". They also identified three potential pathways linking increased minimum wages and population health, including affordability or consumption pathway, psychosocial effects, and worker and firm decision-making pathways (Leigh et al., 2019, pp.123). These categorizations aligned with O’Donnell et al.’s 2015 broad mechanisms that connect income distribution to population health including (i) the level of individual’s (or parent’s) income, (ii) economic inequality within the society, and (iii) psychological stress attached to the stigma of being poor (O’Donnell et al., 2015).

The consumption pathway, according to both propositions by O’Donnell et al. (2015) and Leigh et al. (2019), is the primary mechanism linking minimum wage to health, and it is also closely related to "*the materialist hypothesis*" of Feinstein (1993). The theoretical explanation is premised on "the Grossman’s model for health demand" (Grossman, 1972). The desirability of having good health is a function of health-enhancing consumption activities and the constraint imposed by limited resources at an individual’s disposal. Therefore, individuals with limited resources because of low income may exhibit poor health status compared to individuals earning a higher income (Wagstaff, 1986). Hence, an increase in income accompanied by healthy consumption activities is expected to improve health outcomes. By extension, if an increase in income is followed by increased demand for unhealthy goods and activities, such as smoking, alcohol, and drug use, the increased income can adversely affect health.

Secondly, the psychosocial pathway is theoretically linked to the *"psychosocial hypotheses"* (Lynch et al., 2000). The psychosocial hypotheses propose that individuals with less income often have worse health than individuals with higher income due to negative upward social comparisons which can result in frustration, shame, stress and subsequently ill health (Hounkpatin et al., 2016, pp.76). Besides, low income brings about material disadvantage, which is a precursor to psychosocial adversities associated with poorer health including greater stress, depression, and less satisfaction with job and life, (Macleod and Davey Smith, 2003; Marmot and Wilkinson, 2001). Past studies have recorded success in linking psychosocial factors such as control over one's life, anxiety, financial insecurity, depression, and social affiliations to poor health (Marmot and Wilkinson, 2001). Satisfaction with compensation can significantly influence employees' work motivation, job satisfaction, and perceived quality of life (Che Ahmat et al., 2019).

Another 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). There is also the intergenerational pathway that 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 and Romich, 2018). Nevertheless, there is no consensus on the pathways linking income to health. Also, the empirical findings on the categorization of the effects of minimum wage on the different array of health and well-being outcomes (Leigh et al., 2019). For example, Bossler and Broszeit (2017) reports a modest increase in job satisfaction driven by pay satisfaction following an increase in Germany's minimum wage. A positive relationship between pay increase and job satisfaction was also observed by Smith (2015) using British data, but they also find a "step-reduction" in satisfaction with below-median growth in wages. Simon and Kaestner (2016) employ the United States Current Population Survey data to evaluate the employers' response to minimum wage changes through adjustments in non-wage incentives including fringe benefits, job safety, and training access but they find no discernible effect on the generosity of non-wage benefits for low-skill workers. In contrast, Marks (2011) using the same data, finds that minimum wage changes lead to a disproportionate reduction in the non-wage fringe benefits experienced by low-skilled workers. This lack of consensus in previous research indicates that evidence on the pathways and mechanisms linking income to health and well-being is inconclusive.

Furthermore, the other aspect of consideration relates to the methodological approach and estimation strategies employed in empirical research to analyze the income and health nexus. The strategy in early literature relates to determining the direction of the relationship between income and health. Studies employed various regression techniques involving time series, cross-sectional, or panel data mostly based on ordinary least squares (OLS) assumptions and have generated different findings and conclusions, including unidirectional and bidirectional relationships between income and health. However, regression estimates of minimum wage nexus on observational outcomes such as health and well-being, are sometimes fraught with methodological limitations, including but not limited to issues of reverse causation and confounding effects of other covariates (Reeves et al., 2017). Besides, these regression estimates do not provide true assessments of the intervention effects in evaluating whether the policy to increase wage floors achieved their desired objectives or otherwise (White and Sabarwal, 2014). The standard approach is to deploy randomized controlled trials (RCTs) like the natural experiments in life sciences. However, the costs, ethical consideration, and external validity are among other concerns for not implementing RCTs in social and economic policy research (Reeves et al., 2017).

In terms of empirical methodology to estimate causation, the difference-in-differences technique remains the most popular quasi-experimental strategy widely employed to estimate the health effects of wage policies (Leigh, 2021). 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 that 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 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.

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 concern with 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 and 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.

Recent findings by Maxwell et al. (2022) show 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. 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 different years and the length of time they were treated. Overall, adopting an identification approach that follows the Canonical DID setup in estimating the treatment effects dynamics of such a heterogeneous 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) 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., 2023).

3.4 Data and Method

3.4.1 Data source

We collected data from twelve waves of the Understanding Society UK Household Longitudinal Study (USoc). USoc 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 USoc in policy research has also been demonstrated in previous empirical research on the nexus between income and well-being (Akanni et al., 2022b; 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 them into their corresponding financial years to ensure the sample is nationally representative (Kaminska and Lynn, 2019). The data also provides information on the actual interview dates.

Given that the introduction and subsequent uprating of the NLW are effective on the first day of April every year (Low Pay Commission, 2022), we harmonize the annual data into a financial year period commencing from 1st April to the 31st March of each successive year. The survey also collects detailed data on respondents' age and basic hourly wages that we require 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 that commence at above age 65. The empirical analyses cover periods from 2013 to 2019. The choice of 2019 is to consider periods before the emergence of the Covid-19 pandemic. On the other hand, 2013 was considered as the start period for our analysis in order to exclude the periods immediately following the Global Financial Crisis and the series of austerity policies that followed which were mostly characterized by changes to UK welfare benefits systems (Tucker, 2017).

3.4.2 The difference-in-differences with heterogeneous and several treatments

The NLW policy has multi-period and multi-group 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 that reached the minimum age threshold and earned below the basic wage rate. 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. The typical TWFE specification employed to estimate the average treatment effect could be specified as:

$$Y_{igt} = \alpha_g + \lambda_t + \beta_{twfe} D_{g,t} + \varepsilon_{g,t} \tag{3.1}$$

where $Y_{i,g,t}$ is the outcome for individual i in group g at period t. α_g is the vector of group fixed effects, λ_t is the period fixed effects, and $D_{g,t}$ is the treatment in group g at period t. β_{twfe} denotes the treatment in group g at period t. $\varepsilon_{g,t}$ is the standard error and is clustered at the primary sampling unit level following the approach in previous studies (see Abadie et al., 2023; Costi et al., 2023).

Recent literature has shown that the treatment estimates using β_{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 treatment 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 and Sant'Anna, 2021; Sun and Abraham, 2021). However, we employed the estimator proposed by Callaway and Sant'Anna (2021)[hereafter CS]. 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 $\{Y_{i1}, Y_{i2}, ..., Y_{iT}, X_i, D_{i1}, D_i2, ..., D_{iT}\}_{i=1}^{n}$ as an independent and identically distributed random sample, with Y_i representing the mental health outcomes for individual $i \in 1, ..., n$, 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 $t = 1, ..., T$, 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 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 ∞. 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., $G_{ig} = 1[G_i = g]$) , 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 and Sant'Anna, 2021):

$$Y_{it} = Y_{it}(0) + \sum_{g=2}^{T} (Y_{it}(g) - Y_{it}(0))\dot{G}_{ig} \tag{3.2}$$

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 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 generalization" of the average treatment effect on the treated (ATT) in the canonical DID setup with two time periods, before and after treatment (see Callaway and Sant'Anna, 2021). This is denoted as:

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G_g = 1] \tag{3.3}$$

The $ATT(g,t)$ enables 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 two main aggregated causal parameters, which include (i) the simple average treatment effects, 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.[1]

3.4.3 Identification strategy: treatment and comparison groups

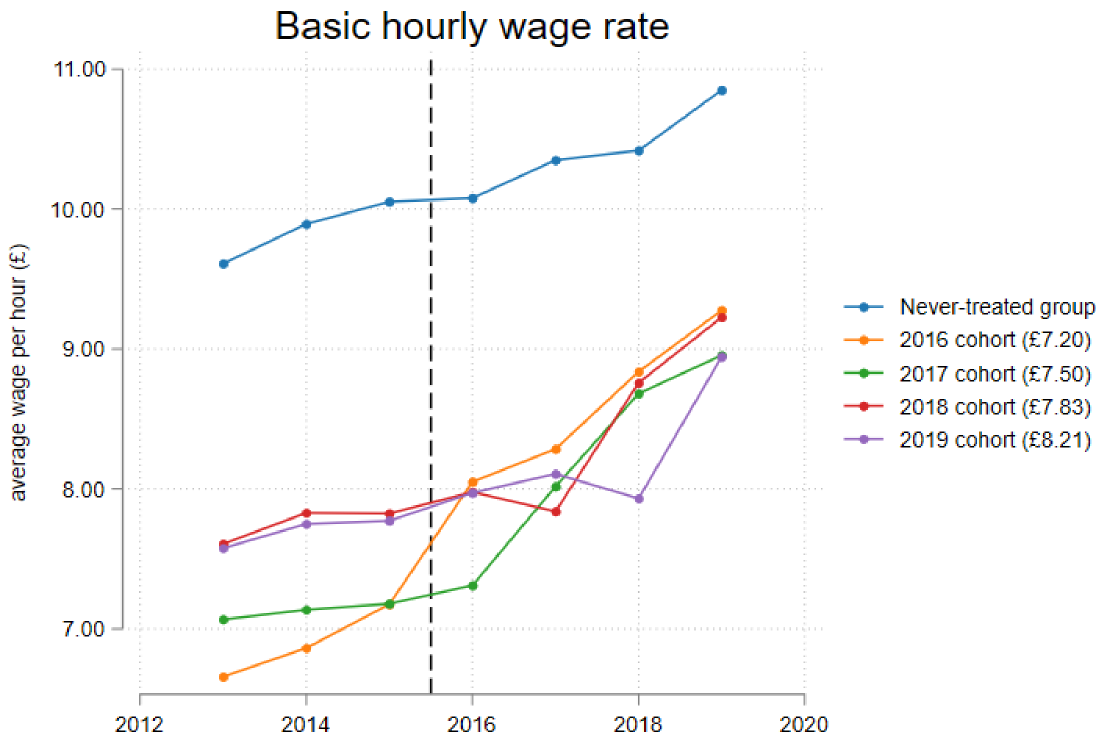
We begin with the NLW introduction in 2016 and the subsequent upratings in 2017, 2018, and 2019, restricting the empirical analysis to periods before the emergence of the COVID-19 pandemic, which had its own major impacts both on the operation of the labour market and on population-level mental health. 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 and 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). Accordingly, an individual worker is eligible for treatment if they are at least 25 years of age and their current basic hourly wage is below the NLW rate.

We defined the treatment group as comprising workers directly affected or most likely affected by the NLW policy based on their reported hourly wages. For example, the first

[1]The average treatment effects are also aggregated by the period that the NLW policy has been in place, denoting the length of time each group became exposed to NLW treatments.

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 £8.21 in 2019 (see Figure 3.1). While the NLW policy has a mandatory compliance requirement with the new annual stipulated wage rate, there are instances where employers did not comply with the rate in the current year (see Bargain et al., 2019; Low Pay Commission, 2017; Ram et al., 2017). Notably, some employers complied in subsequent years increasing workers' hourly wages to or above the previous stipulated rates. We adjust for these peculiarities by including all those whose earnings increased in future periods using the annual wage cutoffs. For example, an individual earning below £7.20 in 2017 but whose hourly wage increased above £7.20 in 2018 was assigned to a treatment group based on the margin of increase. If the new hourly rate is below the 2018 rate, they are assigned as a 2017 treated group, but if more than 2018 but below the 2019 rate, they are classified among the 2018 treated cohorts, and ditto for all other treatment periods and groups.

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 where there are spillover effects of the wage changes for some categories 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. 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



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 parenthesis show the basic NLW wage rates cut-off used to define each treatment cohort

Fig. 3.1 Dynamics of the hourly wage for the treatment cohorts and never-treated group

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 trade-off 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 the annual median hourly wage. given at £13.28 (see https://www.ons.gov.uk/).

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

Our choice for using MCS as the main proxy for mental health is also based on some of the assumptions of the estimation method as discussed in Section 3.5. One of the underlying assumptions of the CS DID framework requires that the outcome variable is independent and identically distributed. Also, discussions in Wooldridge (2005) and Roth and Sant'Anna (2023) show that the parallel trend assumption holds when the treatment and control groups have the same distribution for the outcome in the pretreatment period which is not the case for nonlinear (discrete) outcomes.

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 pre-specified 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. Our choice of covariates followed past studies on the mental health effects of minimum wage policies by considering covariates that are relevant to mental health (see Kronenberg et al., 2017).[2] Additionally, following the CS estimator, we selected these pre-treatment covariates that are potentially associated with the evolution of mental health over time. Also, we considered time-invariant covariates and those that are not anticipated to be affected by the treatment in line with the CS assumption given that "post-treatment covariates could be potentially affected by the treatment" (see Callaway and Sant'Anna, 2021, pp. 8).

3.5 Empirical Results

We begin this section by discussing the descriptive statistics between the treatment cohorts and the comparison group. Table 3.1 provides the 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 the 2019 cohorts at £1223, is less than the average income for the comparison group at £1588. 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, which is consistent with the findings that female workers are more likely than men to be paid the NLW (Dube, 2019). However, the comparison group had proportionally fewer females than males as observed in 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. 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 larger than

[2] Kronenberg et al. (2017) additionally considers other covariates in their DID specification including whether in part-time or full-time jobs, contract type, occupation classification, and length of employment among others. However, we did not include these variables in our specification for reasons including the possibility that these outcomes may be affected by the NLW and to avoid controlling for too many factors (see Callaway and Sant'Anna, 2021; Wooldridge, 2005).

Table 3.1 Summary statitics

	Treatment cohorts				Comparison
	2016	2017	2018	2019	
Income	1203	1139	1208	1223	1588
Age (average)	42.9	43.2	42.8	45.2	45.1
16 – 24	3.2%	4.4%	6.2%	4.3%	6.5%
25 – 29	15.8%	14.6%	14.9%	14.6%	8.3%
30 – 39	24.0%	23.3%	24.0%	17.7%	19.0%
40 – 49	23.7%	24.1%	22.1%	20.6%	26.3%
50 and above	33.2%	33.5%	32.8%	42.8%	39.9%
Gender (female)	64.5%	67.3%	64.3%	66.0%	46.0%
Marital status					
Never married	21.8%	22.9%	26.2%	23.0%	17.1%
Married or cohabiting	66.5%	64.3%	63.3%	65.5%	72.7%
Not married	11.7%	12.7%	10.5%	11.6%	10.2%
Education					
GCSE & A-Level	59.5%	54.5%	55.9%	54.3%	57.9%
Degree & Higher	19.1%	23.6%	25.0%	24.5%	26.3%
Other qualification	11.9%	12.4%	13.4%	12.5%	11.6%
Receiving benefits	49.9%	50.7%	44.2%	42.6%	30.9%
No of observations	1098	750	531	873	1027

Note: The treatment columns show the averages for people who 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. ***, **, and * indicate significance at 1%, 5%, and 10% levels, respectively; Income is monthly personal income after tax; the row ‘Receiving benefits’ indicates the percentage of individuals across the cohorts that were receiving at least one of the in-work 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.

Table 3.2 Treatment effects estimates of the NLW policy

	Outcome: MCS	2016	2017	2018	2019	Weighted average	P-value
Panel A	***Without covariates***	0.6119	1.2721	0.5924	1.1760	0.7888**	0.1122
		(0.5240)	(0.6029)	(0.7232)	(0.6490)	(0.3578)	
Panel B	***With covariates***	0.8015	1.4524	0.8792	1.2082	0.9705**	0.1035
		(0.5802)	(0.6682)	(0.7983)	(0.6911)	(0.3788)	

Note: The Table summarises the group-time average treatment effect parameters under the unconditional and conditional parallel trends assumptions, that is, without (Panel A) and with (Panel B) the inclusion of the covariates, using the estimation method from Callaway and Sant'Anna (2021) and implemented by their 'did' R package. The 'weighted average' column reports the weighted average treatment effects across all treatment cohorts. The average treatment effects for each treated cohort are summarised in each column. 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 at the 0.05 level in a 2-tailed test. The p-value column denotes the probability values for the Wald test of parallel trend assumption as reported by the 'att_gt' function from the 'did' package. The estimates are obtained using the doubly robust estimator *(dripw)* with the standard errors clustered at the primary sampling unit level.

the size of workers that received NLW in subsequent cohorts. This is expected given that the NLW was first introduced in 2016 with an eligible age cut-off for workers aged 25 and above. Besides, the 2016 basic NLW rate was the largest rise in the UK's minimum wage's history, and it has a higher coverage rate than the previous NMW and subsequent NLW uprating in 2017, 2018, and 2019 (Low Pay Commission, 2022). Besides, the Low Pay Commission estimates of hourly wage underpayment as a proportion of coverage for eligible NLW workers is lower in 2016 than in subsequent years (Low Pay Commission, 2019).

The group-time average treatment effects results

The estimated treatment effects of the NLW policy on mental health using the never-treated group[3] are summarised in Table 3.2. 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

[3]We additionally estimate the treatment effects using the not-yet-treated group. The results are summarised in Appendix B (See Tables B.3)

the treatment effects estimation. The estimated p-values 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 pre-treatment periods.

The aggregate weighted 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 which ranges between 0 and 100 with 100 indicating the highest mental health state, only increased by about 0.79 index points (less than one) for those in the treatment cohorts compared to the comparison group following the NLW policy between 2016 and 2019. Also, the 2016 - 2019 columns in Table 3.2 summarise the effect of the NLW based on all individuals who 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. However, the disaggregated estimates for each treatment period are not statistically significant when separately considered (see Table 3.2).

The average treatment effects by the length of time the NLW policy has been in place are summarised in Appendix B Table B.1. Additionally, we provide the estimated results using the not-yet-treated group as the comparison group. The results are summarised in Table B.3, and the results are similar to the main results using the never-treated group. Overall, the disaggregated treatment effects estimated by cohort and time show consistency in the positive mental health effects of the NLW policy across the different treatment cohorts and periods they were 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.

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 of the benefits freeze program could disproportionately affect low-paid workers. Besides, the government's attempt to reduce reliance on benefits and shift the cost burden to employers through higher wages could worsen the precarious conditions of low-income workers. Hence, we evaluate the impacts of the benefits freeze program on the mental health effects of the NLW policy. Receiving the in-work welfare benefits that were frozen could be endogenously related to the NLW policy given that both policies are targeted at low-income individuals. As such the receipt of welfare benefits could be seen as an indicator of being a low-wage earner. Nonetheless, we re-estimated the group-time average treatment effects separately for the group that was receiving any of the frozen working-age benefits and, as a result, were affected by the benefits freeze policy and the other group that was not on any of the frozen benefits. The separate analysis would provide insights into the variations in the impacts of receiving the NLW for the different groups. The estimated average group-time treatment effects are summarised in Table 3.3. Panel A shows the average treatment effects across treatment cohorts and calendar years for the workers who 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 parameters 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 who reportedly did not receive any of the affected frozen benefits are summarised in Panel B. The results show positive and significant estimates for the weighted average parameter. The disaggregated

Table 3.3 NLW treatment effects – receiving vs not-receiving work-related benefits

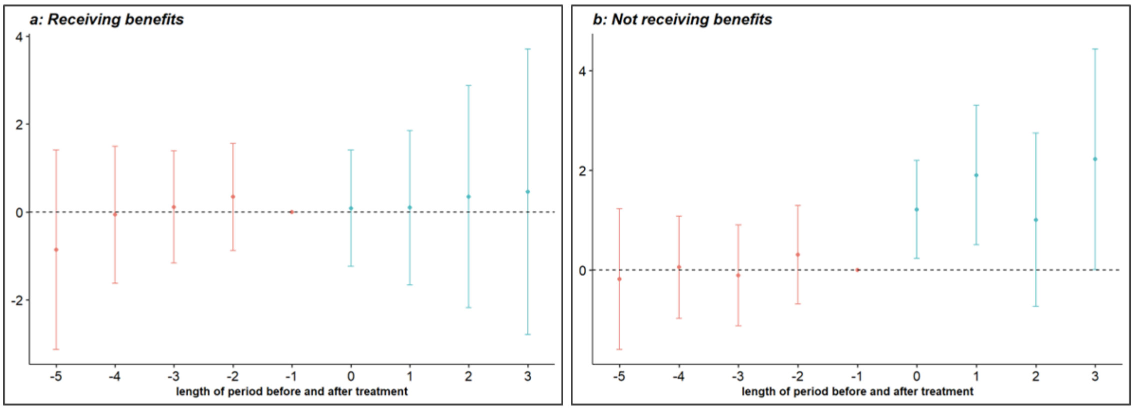
	Outcome: MCS	2016	2017	2018	2019	Weighted average	P-value
Panel A	***Receiving benefits***	0.1337	1.7451	-0.4847	1.2733	0.6930	0.7777
		(0.7725)	(0.7740)	(1.0447)	(0.8752)	(0.4851)	
Panel B	***Not receiving benefits***	1.5046	1.5514	1.6205	1.6022	1.5287**	0.8398
		(0.5979)	(0.8135)	(0.9975)	(0.8485)	(0.4578)	

Note: The Table summarises the group-time average treatment effects of the NLW policy for individuals affected by the 2016 welfare benefits freeze (Panel A). The estimated parameters for those not receiving the affected welfare benefits are summarised in Panel B. All the parameters are estimated under the unconditional parallel trend assumptions without including the covariates, using the estimation approach proposed by Callaway and Sant'Anna (2021) and implemented by their 'did' R package. The 'weighted average' column reports the weighted average treatment effects across all treatment cohorts. The average treatment effects for each treated cohort are summarised in each column. The aggregated parameters across time are summarised in Appendix B Table B.2. 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 at the 0.05 level in a 2-tailed test. The p-value column denotes the probability values for the Wald test of parallel trend assumption as reported by the 'att_gt' function from the 'did' package. The estimates are obtained using the doubly robust estimator *(dripw)* with the standard errors clustered at the primary sampling unit level.

parameters across all the treatment cohorts are also positive indicating 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 3.2 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 pre-treatment periods includes 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. However, it is important to acknowledge that the relatively small sample of the group of workers affected by the welfare benefits freeze policy and additionally receiving the NLW could affect the statistical power of the estimates.

Furthermore, Figure 3.2b 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 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 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 well-being (see Reeves et al., 2017).



(a) Receiving benefits (b) Not receiving benefits

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 pre-treatment periods. Blue lines are the point estimates of the NLW on mental health, and the lines represent their 95% confidence bands. The estimates are obtained using the doubly robust estimator *(dripw)* with the standard errors clustered at the primary sampling unit level.

Fig. 3.2 Average treatment effects by the length of exposure to treatment.

3.5.1 Robustness checks and additional results

Sensitivity analysis of the parallel trend assumption

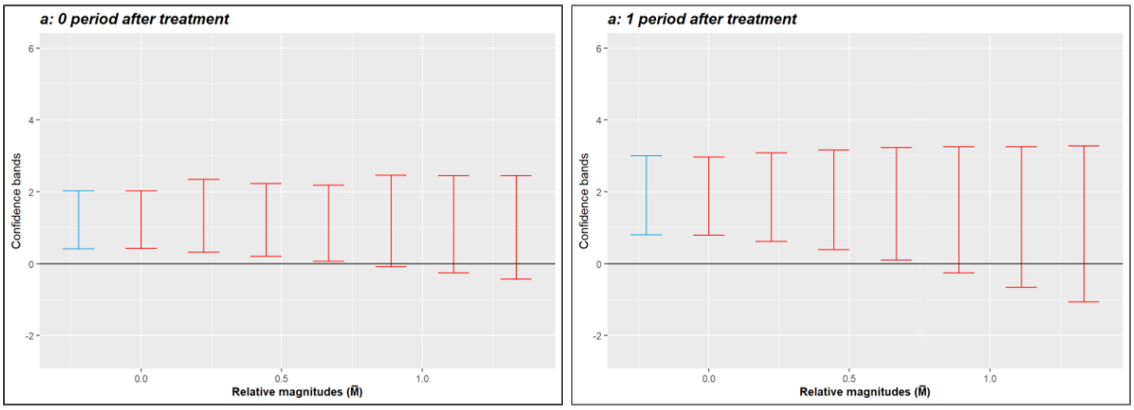
Next, we conduct the sensitivity analysis of our results to violations of the parallel trend assumption. We follow the standard practice in the literature by conducting some robustness checks to evaluate the validity and robustness of the estimated results. First, we evaluate the sensitivity of our DID 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 and Sant'Anna, 2021; Rambachan and Roth, 2023). 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 (2023) proposed, given its strengths in addressing issues related to making inferences without relying on the exact pre-treatment 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 pre-treatment periods. Additionally, the aggregate group-time event-study estimates may be biased by other socioeconomic and welfare policies and programs 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 3.3 shows sensitivity plots of the estimated robust confidence set in the periods after treatment using the significant post-treatment estimates from 3.2b. The plots show that the robust confidence intervals allow for up to 50% of the maximal pre-treatment violation in parallel trends in the post-treatment periods (see Figure 3.3). 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 pre-treatment violations by up to 50%.

NLW policy effect on the labour market and well-being outcomes

In this section, we evaluate the NLW policy effect on some selected labour markets and well-being outcomes, particularly those that could serve as potential mechanisms linking wage policy



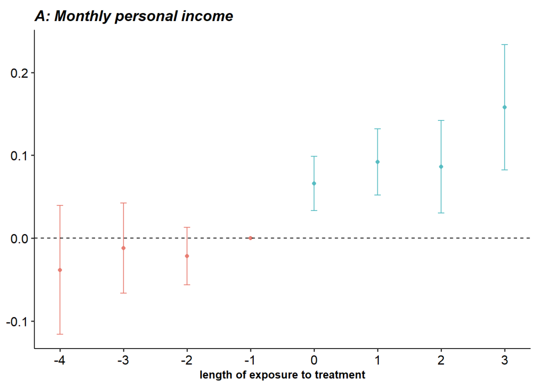
Note: The figure shows the sensitivity analysis plot using the event study from the estimated aggregate group-time average treatment effects. The plot is based on the "relative magnitude" restrictions. The blue line indicates the estimated event-study confidence intervals one period after treatment, as reported in Figure 3.2. 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 pre-treatment periods.

Fig. 3.3 Sensitivity analysis plots.

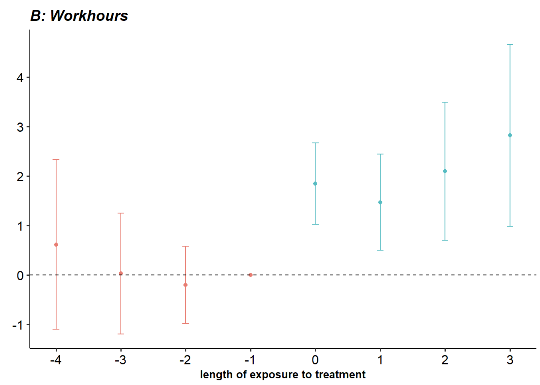
to mental health. As discussed in the review section, there are interconnections in the pathways linking wage policies to health outcomes. Consistent with previous literature, we considered the effects of NLW policy on two labour market outcomes, self-reported earned income and work hours, and two aspects of work-related well-being: job satisfaction and satisfaction with leisure time. The estimated heterogeneous treatment effects are summarised in Table 3.4.

Row I in Table 3.4 shows the estimated average treatment effects on monthly personal earned income disaggregated across the treated cohorts and the period they were 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 row II of Table 3.4 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 rows III and IV of Table 3.4. Following a large body of literature that has employed self-reported measures as a construct of well-being (Akanni et al., 2022b; Flint et al., 2014; Gülal and Ayaita, 2020; Kuroki, 2018), we collect data on the job and leisure time satisfaction from the USoc 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 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 well-being (Gülal and Ayaita, 2020). However, the estimated results show insignificant NLW effects on leisure time satisfaction.

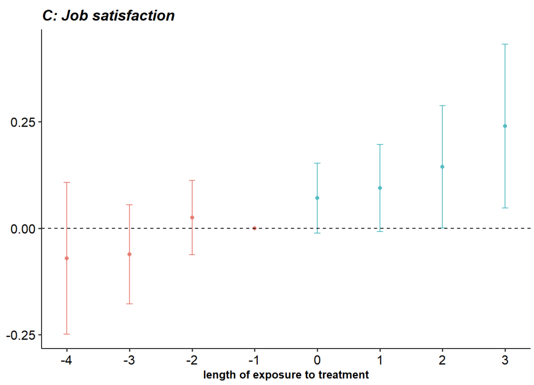
Figure 3.4 depicts the event study aggregates and the simultaneous confidence bands for the estimated coefficients for each outcome. The positive and significant policy effects on income (Figure 3.4a), work hours (Figure 3.4b), and job satisfaction (Figure 3.4c) lend support to our main findings that 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 well-being (Leigh et al., 2019). First, the significant effect estimates for earned income and affected workers' job satisfaction corroborate 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 (Figure 3.4d) 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 (Capuano et al., 2019; Connolly and Gregory, 2002).



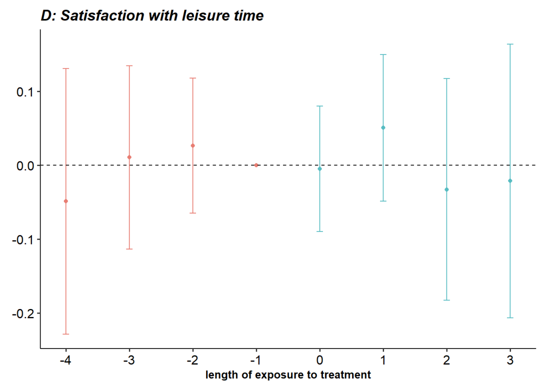
(a) Monthly personal income



(b) Workhours



(c) Job satisfaction



(d) Satisfaction with leisure time

Note: The figures show the dynamic average effects of the NLW policy aggregated by event time on the selected labour market and work-related well-being 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

Fig. 3.4 Aggregate treatment effects on labour market and well-being outcomes.

Table 3.4 NLW policy effects on labour market and well-being outcomes

	Outcome	2016	2017	2018	2019	Weighted average	P-value
I	*Earned income*	0.1152**	0.0644**	0.1065**	0.0943**	0.0950**	0.4124
		(0.0220)	(0.0267)	(0.0265)	(0.0240)	(0.0140)	
II	*Work hours*	2.4558**	2.1619**	1.4244	1.2133	1.8946**	0.4068
		(0.5947)	(0.5818)	(0.6472)	(0.5959)	(0.3224)	
III	*Job satisfaction*	0.0869	0.2236**	0.1062	0.0908	0.1286**	0.1070
		(0.0636)	(0.0663)	(0.0683)	(0.0556)	(0.0333)	
IV	*Leisure satisfaction*	0.0139	0.0091	0.0238	0.0099	0.0137	0.5558
		(0.0562)	(0.0562)	(0.0638)	(0.0496)	(0.0281)	

Note: The Table summarises the average group-time treatment effects of NLW on earned income, work-hours, job satisfaction and satisfaction with leisure time under the unconditional parallel trend assumptions, that is, without covariates. Standard errors are presented in parenthesis, and 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 column summarises the probability values for the Wald test of parallel trend assumption. The estimates are obtained using the doubly robust estimator *(dripw)* with the standard errors clustered at the primary sampling unit level.

Additional results - mental health measured using GHQ-12

We consider an additional measure of mental health which has been widely used in literature to measure mental health. We employed the GHQ-12 for robustness purposes. Unlike the MCS, GHQ-12 has 12 components, each asking participants about their conditions. The GHQ-12 score converts valid responses to the 12-item questionnaire to a single Likert scale. Each question has a four-point Likert scale in descending order from 1 to 4, with 1 indicating better mental health status. The score is obtained by recoding and then summing the values to give a scale running from 0 to 12, from the least distressed to the most distressed respectively. On the other hand, the caseness score sums the valid 12 responses after they are recoded, and the value ranges from 0 to 36. Hence, GHQ-12 has a discrete scale and is not a continuous outcome (see Kelly et al., 2008). Previous studies aggregate the GHQ score by summing across responses to each component, and this could create measurement error (Brewer et al., 2019). Also, Brown et al. (2018) using the Understanding Society study data shows that aggregating the GHQ-12 score by summing across the different components creates measurement error.

Hence, we employed 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. The factor analysis equation is given as:

$$SCGHQ_{i,j} = \alpha_j + \beta_j \psi_{i,j} + \varepsilon_{i,j} \tag{3.4}$$

where $SCGHQ_{i,j}$ is the $j-th$ component of the mental health measures of individual i. α_j and β_j are respectively the intercept and factor loading for each mental health component. $j = 1,2,.3,...,12$ and each component measures different aspects of mental health including *"concentration"; "loss of sleep"; "playing a useful role"; "being capable of making decisions"; "constantly under strain"; "problem overcoming difficulties"; "enjoy day-to-day activities"; "ability to face problems"; "unhappy or depressed"; "losing confidence"; "believe worthless"; and "general happiness"*. $\varepsilon_{i,j}$ is a measure-specific error component and it has a zero mean and is independently distributed across individuals. $\psi_{i,j}$ is the latent factor for the extracted subjective well-being component of each GHQ which is identifiable and extractable by setting the factor mean to zero and β of the first component equals one.

Table 3.4 summarises the factor analysis results. Concentration ($SCGHQa$) is set as the reference loading with a fixed score of 1. *Being unhappy or depressed* ($SCGHQi$) reports the highest factor loading scoring 2.39 whilst the lowest loading is recorded for *(capable of making decisions)* ($SCGHQd$) at 0.78 (see Table 3.4). We extract the latent factor ($\psi_{i,j}$ in equation 3.4), the measures of mental health, and a higher score indicates better mental health.

The estimated average group-time treatment effects using the GHQ-12 mental health measure are summarised in Table 3.6. Compared to the main results presented in Tables 3.2 and 3.3, none of the results using GHQ-12 report any statistically significant average treatment effects of the NLW on mental health. The differences in the estimated group-time average treatment effects of the NLW on mental health for the two measures - MCS and

GHQ-12, could be explained by the differences in their constructs and the nature of the wage policy under consideration which changes every year. The MCS captures long-term health domains which partly explains the significant effects of the wage increase as presented in table 3.2. The MCS is constructed from the Short Form 36 (SF-36) Health Survey which includes 36 questions assessing the functional health and well-being of an individual over a longer term. Also, it focuses on eight longer-term health domains ranging from physical functioning, role limitations due to physical problems, bodily pain, general health, vitality, social functioning, role limitations due to emotional problems, and mental health, with the eight domains contributing to the physical component summary (PCS) and mental component summary (MCS) scores. The GHQ-12 on the other hand has a short time reference, usually two weeks. For example, the self-completion GHQ questionnaire module used in the Understanding Society study assesses the participants' feelings on each of the 12 domains over the last few weeks before the interview date.

Additionally, we compare the group-time average treatment effects for individuals who were affected by the welfare benefits freeze and those not affected by the policy. While the estimated average treatment effects are also not statistically significant, the results show a negative weighted average treatment effects for the individuals receiving benefits, but positive for those not receiving any of the frozen welfare benefits. The results are also indicative of the negative impacts of the welfare benefits freeze on the mental health of the affected workers, despite receiving the wage increment from the NLW policy.

3.6 Discussion

The estimated impacts of the NLW on mental health presented in this chapter show the positive effects of receiving the national living wage on mental health. The group-time average treatment effects and the event-study aggregate both confirm the positive effects. Also, the positive effect is significant and cumulatively increasing over the considered length of treatment exposure.

Table 3.5 Factor analysis results

GHQ label	Mental health measure	Factor loading	Signal
SCGHQa	*Concentration*	1.0000	0.1596
SCGHQb	*Loss of sleep*	1.8316	0.3293
SCGHQc	*Playing a useful role*	0.9908	0.1828
SCGHQd	*Capable of making decisions*	0.7807	0.1338
SCGHQe	*Constantly under strain*	1.9307	0.2800
SCGHQf	*Problem overcoming difficulties*	1.9569	0.2333
SCGHQg	*Enjoy day-to-day activities*	1.1407	0.1546
SCGHQh	*Ability to face problems*	0.9458	0.1394
SCGHQi	*Unhappy or depressed*	2.3935	0.2138
SCGHQj	*Losing confidence*	2.3107	0.2176
SCGHQk	*Believe worthless*	1.9818	0.2333
SCGHQl	*General happiness*	2.2733	0.1780

Note: The Table summarises

Table 3.6 Average treatment effects using GHQ-12 factor score

	2016	2017	2018	2019	Weighted average	P-value
Main results	0.0379	-0.0703	0.1493	-0.1006	0.0061	0.2917
	(0.0876)	(0.0912)	(0.0996)	(0.0871)	(0.0465)	
Receiving benefits	0.0229	-0.1928	0.2681	0.0086	0.0198	0.2695
	(0.1323)	(0.1425)	(0.1612)	(0.1398)	(0.0773)	
Not receiving benefits	-0.0322	0.0037	0.0394	-0.1791	-0.0328	0.4010
	(0.1249)	(0.1090)	(0.1295)	(0.1059)	(0.0606)	

Note: The Table summarises the group-time treatment effect parameters using the extracted GHQ-12 factor score. The 'weighted average' column reports the weighted average using the cohort size for all the group-time average treatment effects. Each row summarises average treatment effects by the timing that each group received the NLW. Standard errors are in parenthesis, and the p-value denotes the probability values for the Wald test of parallel trend assumption. The estimates are obtained using the doubly robust estimator *(dripw)* with the standard errors clustered at the primary sampling unit level.

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. Kronenberg et al. (2017) conclude that a larger increase in wages could lead to improvements in mental health. Our findings contrast those 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 canceled out the positive benefits of wage policies, especially on the mental health outcome. While it appears that the NLW policy seems to have achieved some of its set-out objectives as set out by the government which is to increase earnings for low-paid workers.[4] On the other, the NLW was also 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 build better lives without the additional supplementation (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) found that the minimum wage policy in the US, which provides 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

[4] Aitken et al. (2019) documents that the NLW introduction and uratings have increased wages for low-paid workers with little adverse impact on employment retention.

socioeconomic and welfare policies toward income expansions worked together with wage policies to improve the low-wage workers' situations.

3.7 Limitations

The analysis in this chapter has some limitations that are worth highlighting in this section. First, using hourly wage information to identify individuals eligible for treatment meant our sample composition is made up of only workers who remain employed, and it does not account for individuals who lost their jobs during the periods considered. Empirical investigations of the effects of the NLW policy on labour market outcomes indicate no significant decline in employment or work hours (see Aitken et al., 2019; Brewer et al., 2019; Dube, 2019). However, these findings do not rule out the fact that minimum wages and welfare benefits freezes might have some effects on unemployment.

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) show that attrition in the Understanding Society study is greater amongst younger age groups, men, black people, and people on 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 and 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, impose other bias on the estimated results, and compromise the statistical power. It also leads to underestimating the treatment effects by dropping people from the treatment and control sample. Besides, the 25 years eligibility age criteria to receive the NLW also restricts the worker's sample considered in our study to older adults while it excludes young workers between 18- and 24 years of age, who are more likely to be new entrants into the labour market, in their early career, likely earning around the minimum wage, and form parts of the cohorts considered in most minimum wage and health literature (Leigh, 2021).

Lastly, another constraint of this study is its inability to integrate the welfare benefit freeze into the same heterogeneous difference-in-differences framework to simultaneously evaluate the annual roll-out of the national living wage. Future studies would benefit from extensions to the staggered difference-in-differences method that accommodates a triple-difference design. This is currently receiving attention in the quasi-experimental methodology literature (see Sant'Anna, 2022; Strezhnev, 2023)

3.8 Conclusion

In this chapter, we provide empirical evidence of the causal impacts of the introduction and subsequent annual increments in the NLW on the mental health of affected workers. We consider 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 wage policy. We find evidence of positive effects of the national living wage on mental health, but with constricted impacts from the counteracting welfare benefits freeze policy, which stagnates or reduces the affected workers' income. 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, 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 trade-offs risk diluting the effectiveness of wage policies. Overall, the chapter contributes to the importance of evaluating the health impacts of wage floor changes amid other counteracting welfare policies.

Chapter 4

The effects of the National Living Wage on Informal Carers Work Hours and Health

4.1 Introduction

Demand for informal (unpaid) carers is increasing due to demographic changes including population ageing, rising adult dependency ratio, improvements in life expectancy for children and people with disabilities, and migration. Within this context of an increasingly ageing population and the continuous improvements in the life expectancy of children and young people with disabilities needing care and support, developed and middle-income countries currently face the major challenge of ensuring their health and social systems are capable of accommodating the demographic shift. The health and social care systems in these countries continuously require individuals to provide care for their family members and acquaintances. Besides, publicly funded care and institutionalised support services have not kept pace with the growing demand for social care services (Thorlby et al., 2018).

There are also pressures emanating from shifts towards public policies that favoured community supply of care away from the formal care institutions (Colombo et al., 2011). On the other hand, the prevailing labour market and welfare policies are simultaneously putting pressure on these unpaid carers to increase their participation in labour market activities to promote economic growth, increase competitiveness, and address potential labour shortages as a result of the changing population demographics (Lewis and Giullari, 2005).[1] Examples of these policies include pension reforms and changes to the retirement age, as well as the minimum wage and living wage policies.

While several studies have considered the effects of retirement reforms on informal care (see Carrino et al., 2019; Zhu and Onur, 2022), there are few studies on the effects of minimum wage policies on informal care provision (Jutkowitz et al., 2022).[2] Unpaid care can have a dual impact on household finances, since it may limit carers' earning potential while care-receivers are also less likely to earn. Following conjectures within the social policy literature, caring responsibilities increase the tendency for higher absence and sick days, making the informal carers more likely to be less productive than noncarers (see Carmichael and Charles, 1998). Besides, because of the limited time resources, there is an inverse relationship between the number of hours devoted to providing care, leisure and labour supply. As a result, informal carers often opt for paid jobs that are relatively less time-demanding, part-time roles, or positions that are below the level of their skills or experience.

There are reasonable concerns on the part of the government and policymakers to ensure that the supply of labour is adequate for the continuous functioning of the economy. The UK government introduced the National Living Wage (NLW) policy in 2016 and subsequently increased the base hourly wage rate every year. NLW is explicitly about ensuring high levels of employment. Also, the policy aims to reduce government welfare benefits spending, particularly those costs associated with augmenting wages and low income through work-related support and tax credits. The government specifically wants to move from a low-wage, high-tax, and

[1] Although informal caregiving and unpaid carers are often not the direct focus of conversations about the social care systems and policies.

[2] On the contrary, the focus in the literature has largely been on how caring responsibilities affect the labour market participation decisions (Heitmueller, 2007; Lilly et al., 2010).

high-welfare society to a higher-wage, lower-tax, lower-welfare society by shifting the burdens of welfare spending to employers in the form of higher wages.[3]

Furthermore, evidence abounds on the impacts of wage policies in general, and NLW in particular, on the general population. Little is known about the impact on informal carers given their very specific situation. Increases in wage floors have direct connections with the income of low-paid workers but also have implications on informal caring decisions. A systematic review of existing studies showed that caregivers are more likely to earn low wages than non-informal carers (see Lilly et al., 2007). Besides, as NLW increases annually in the UK, the minimum earnings requirement to receive the carer's allowance is also changing. However, the low earnings threshold of most informal carers will likely force them to work fewer hours in order to keep claiming carer's allowance (CarersUK, 2019). The acquisition of the minimum level of income necessary to survive sometimes depends on assessing a complex web of provisions by low-income individuals with caring responsibilities (Mosley, 2021). Carers would often have to rely on state benefits and associated services in order to balance the number of hours devoted to care and paid work. Hence, informal carers are more exposed to the effects of policies affecting wage floors and other welfare benefit rates.

The aim of this chapter is to evaluate the impact of the national living wage policy on informal carers. There is literature coverage on the relationship between informal caring responsibilities and wages (see Jutkowitz et al., 2022). The empirical evidence on the impacts is inconclusive (Fevang et al., 2008; Van Den Berg et al., 2004). This study relates to several strands of literature. First, we contribute to the strands of empirical literature that evaluate wage policies and their unintended outcomes by exploring the impact on informal carers. A limited number of empirical studies have been devoted to investigating the effects of labour market reforms and policies on the provision of informal care in general (Carrino et al., 2019; Zhu and Onur, 2022), and carers' health in particular. Besides, the reconciliation of caring duties and employment remains an increasingly important health, socioeconomic and welfare policy

[3] See The National Living Wage policy paper

issue globally. Second, our article provides insight into the findings on the trade-off between labour force participation and caring responsibilities as a result of a major labour market reform. Previous studies show that caregivers are generally equally as likely to be in the labour force as non-caregivers and they are more likely to work in jobs requiring fewer hours to meet their caring commitments (Lilly et al., 2007).

Moreover, Target 5.4 of the United Nations Sustainable Development Goals solicits the recognition of the value of unpaid carers in the value chain of providing the services necessary for the well-being of the entire society including children, elderly, ill and people living with disabilities among others (Perry, 2022). Our study aim is related to three of the six major policy challenges identified by the United Nations to support informal carers including the importance of employment, earnings, and their health and well-being (United Nations Economic Commission for Europe, 2019). The three related policy challenges are (i) reconciling caring with employment, (ii) the ensuring adequacy of carers' income and social security, and (iii) protecting carers' health and well-being. Other challenges identified include acknowledging the contributions of informal carers, providing access to community-based services, and providing access to information and training (United Nations Economic Commission for Europe, 2019).

Additionally, we contribute to the literature on caring responsibilities and carers' health. Previous literature indicates that the relationship between caregiving and carers' health could be mediated by a range of factors, including wages.[4] However, while empirical evidence seems to be more consistent that increased minimum wage positively affects general workers' health (Akanni et al., 2022a; Leigh, 2019), little is known about the effects on the health of workers with caring responsibilities.

The remainder of the chapter is organised as follows: the next section provides a review of the empirical literature. Section 4.3 describes the model including the data, identification

[4]Other determinants discussed in the literature include being in employment, care intensity, co-residency with care-recipient, and available support from other family members, the wider community and formal services (see Becker and Sempik, 2019).

strategy, and empirical model. The results are discussed in Section 4.4, and Section 4.6 concludes the chapter.

4.2 Literature review

Empirical research has convincingly documented that caregivers are equally as likely to participate in the labour force as non-caregivers. It is also clear from the literature that increased time devoted to caring appears to leave carers with less time for labour force participation and leisure. Hence, they are more likely to work fewer hours or in part-time roles (Lilly et al., 2007). Besides, unpaid carers have been found willing to give up work as their caring commitments increase or when their caring duties conflict with employment (see Carmichael and Ercolani, 2016). As a result, many countries have devised and implemented different policies and programmes to reduce the work-care conflict by financially supporting unpaid carers. Hypothetically, these financial compensations for the hours devoted to providing care would allow carers to either reduce work hours, exit employment or combine their work with care roles.

However, there are fundamental questions regarding the relief offered through financial compensation in terms of work–care conflict, as employment reductions may worsen the long-run position of the carers in the labour market (Raiber et al., 2022). Questions have also been raised about the restrictions in the eligibility conditions and adequacy of these financial transfers to unpaid carers compared to being engaged in the labour market. For example, the current rates of the carers' allowance and carers' credit, which are the two main welfare support for unpaid carers in the UK, are among the lowest benefits of their kind compared to other welfare benefits in the UK including employment support allowance, lone parent allowance and job seeker's allowance (Powell, 2019). The most important eligibility condition to receive the carer's allowance is a weekly minimum of 35 hours devoted to unpaid care. Consequently, at the current rate of £69.70 per week using 2022/2023 rates, it implies about £1.99 per hour.

On the other hand, the carer's credit provides national insurance credits to carers to fill gaps in the National Insurance record to ensure their ability to qualify for the State Pension. It also requires at least 20 hours of providing unpaid care in a week.[5]

Furthermore, both empirical research and policy literature are replete with studies on the effects of informal care provision on various outcomes including employment, earnings and the health of the care providers (see Brimblecombe et al., 2020; Kolodziej et al., 2018). In their systematic review of studies that estimated the causal impact of informal caregiving, Bom et al. (2019) found a vast literature supporting the evidence of the negative effects of informal caring on physical and mental health, with the different impact intensities across different socioeconomic backgrounds, and the demographic group of caregivers. Kaschowitz and Brandt (2017) summarises the pathways through which informal caregiving could affect carers' health. First, the physical burdens and time-demanding tasks associated with caring might lead to declining health and well-being for the carers. Secondly, the level of the adverse impacts of caring on health depends on kinship and intimacy between the carers and care receivers (Gormley, 1996; Litwin et al., 2014), and the cultural motives for providing informal care (Zarzycki et al., 2022). For example, children, siblings, spousal, and parental relationships are all found to have differential health effects from providing informal care (Broese van Groenou et al., 2013; Oshio, 2014). Additionally, contextual factors including socioeconomic, demographic and support systems could moderate the burdens associated with providing informal care. Do et al. (2014) using data from the United States Behavioral Risk Factor Surveillance System found that the relationship between informal care and health is impacted by income, race, and ethnicity.

Methodologically, empirical studies have employed various methods to evaluate the efficacy of policies and interventions to support informal caregiving, and those policies aimed at improving the health of caregivers. Prominent among these methods are simultaneous equation models including the two-stage least squares and instrumental variables methods (Bom et al.,

[5] see https://www.gov.uk/carers-credit/eligibility

2019). However, the main empirical challenge in these studies largely emanates from individual self-selection involved in providing informal care, and the selection bias that arises from comparing outcomes for individuals with and without care responsibilities. Studies have attempted to address these issues by employing different matching approaches including one-on-one matching, propensity score matching, and coarsened exact matching (Bom and Stöckel, 2021; Stöckel and Bom, 2022). The various matching techniques compare caregivers to individuals who are non-caregivers, based on observable characteristics. Studies have also employed other quasi-experimental approaches to investigate the effects of interventions on informal caregiving. However, attention in most of these studies is largely focused on policies and interventions that are directly and indirectly related to supporting informal caregiving (see Courtin et al., 2014). Calvó-Perxas et al. (2018) employed survey data from 12 European countries to assess the association between financial and other non-financial support policies of support to caregivers and the effects on their health. They found non-financial support measures and policies, especially those regarding the provision of free time away from caring duties, dealing with emotional burdens of caregiving, and acquisition of caring skills, all have a larger protective impact on the health of caregivers than the financial support measures across the considered countries.

On the contrary, few studies have employed quasi-experimental approaches to evaluate the causal impact and indirect consequences of labour market policies on informal caring decisions and carers' health. The common theme across these few studies is their focus on policies related to retirement decisions and career elongation programmes. Zhu and Onur (2022) employed the Australian Household, Income and Labour Dynamics in Australia (HILDA) survey data to analyse the effects of retirement status and duration on informal care provided by older individuals. Their results show that postponing retirement through the Australian pension age reform, which was aimed at lengthening working careers, did not crowd out the supply of informal care by older adults. Carrino et al. (2019) considered the increase in the state pension qualifying age for women in the UK on the intensity of informal care provision. Using data from the Understanding Society longitudinal survey, they found that the increased employment associated with the policy substantially reduces the informal care intensity and the probability of providing intensive care.

Theoretically, the motives for providing informal care have been identified and categorised under different themes, including altruism, social norms, reciprocity, duty, direct payments, and strategic bequest motives. However, regardless of the motive, caring has been identified as a combination of satisfaction and challenges, with both positive and negative consequences for the carers (Fevang et al., 2012). Besides, there is consensus on the inverse trade-off between the amount of time devoted to caring and available time for labour market participation and leisure (Kuhn and Nuscheler, 2011). Also, the impacts of providing unpaid care can vary based on the net effects of counteracting forces: the substitution and income effects. First, given that time is scarce, an increase (decrease) in wages will increase (decrease) the opportunity costs of leisure and providing informal care. Hence, there is an increase in time devoted to labour supply. On the other hand, as wages increase, the informal carer does not have to devote as much time to work to earn the same income, leaving more time for leisure and providing care. Fevang et al. (2012) find that the effects on labour supply and caregiving are indeterminate, and it is not known which effect dominates. Furthermore, there is also a possibility that informal carers may engage in employment as a way of gaining respite from the pressures and demands associated with caring (Carmichael and Charles, 1998). Besides, contrary to the traditional notion of separating work from leisure unpaid carers can utilise their being in paid employment as a respite space for leisure (Joseph and Joseph, 2019). By implication, such respite effects would further counteract the substitution effects with regard to the carers' decision on labour market participation, and their response to changes in the wage policy.

This study seeks to contribute and extend the empirical evidence to the effects of other labour market and welfare policies on unpaid care. Theoretically, wages have been documented to have mixed effects on informal care, leisure and labour supply. Increased wages could also provide respite for carers and moderate the health and well-being effects of informal care duties. Jutkowitz et al. (2022) using data the Health and Retirement Study found that increasing the minimum wage across US states between 2010 and 2014 did not affect the amount of caregiving received by US adults above 64 years. However, the policy may potentially increase wages for lower-income home care workers and could lead to improved care receivers' outcomes. Also, Hampton and Totty (2023) found that minimum wage has small positive effects on the labour supply of key workers aged between 62 and 70, which forms part of the increasing age cohort of unpaid carers in the United States. Additionally, the declining welfare benefits arising from the work-related benefit freeze that was simultaneously implemented with the NLW policy could further constrict the positive effects of the NLW (see Akanni et al., 2022a). Thus, our study attempts to provide empirical evidence reflecting the twin pressures from the conflicting policies of increasing labour market participation and increasing needs for informal carers.

4.3 Data and Empirical Model

4.3.1 Data source

We used data from the Understanding Society (USoc) UK Household Longitudinal Study (University of Essex, Institute for Social and Economic Research, 2022). USoc provides a longitudinal dataset of over 40000 individuals selected across households in the UK and followed every year since its inception in 2009. One of the merits of the USoc for our study is that it provides information on the basic hourly wage for the survey participants. The hourly wage information is useful to identify workers who are eligible to receive increased wages each year following the NLW increases. Additionally, the USoc includes the caring questionnaire module with detailed questions regarding informal caring activities. It includes survey questions that self-identify respondents who are residential and nonresidential informal carers. It also includes other care-related issues including the number of hours per week spent caring and how caring affects paid work (see Section 4.3.1 for a discussion and definition of informal carer).

The longitudinal nature of the USoc data also allows us to follow the informal carers over time providing a long-term perspective on their lives. The underlying intuition for employing an individual method in this study is to compare changes in individual outcomes such as the impact of caring on LFP decisions and changes in mental and physical health after the NLW. The USoc data collection follows an overlapping panel structure where data for each wave cover at least 24 month period but each respondent is surveyed every consecutive 12 months. The survey also provides information on the interview dates. Thus, given that the NLW became effective on the 1st of April each year, we matched individual information from different overlapping waves for each year starting from April 01 to March 31 the following year (see Kaminska and Lynn, 2019). In line with the main objective of the study which is to evaluate the effects of receiving the NLW for the group of individuals who became informal carers. Also, in line with the matching method used to identify the comparison groups, we consider the analysis from 2015 to 2020, using data commencing from 01 April 2015 which is the year before the first NLW group received treatment to 31 March 2020.

Dependent variables

We consider three dependent variables including work hours[6], and physical and mental health, capturing the different dimensions of labour market participation and health outcomes. These outcomes have been well-considered in the literature and they are mostly connected to informal carers' choice between labour force participation, leisure, and caregiving. Besides, the importance of intensive margin of labour supply is relevant given the changing nature of work with the emergence of more service-related and freelance and hybrid roles (see Cai, 2021), and the

[6]We considered the intensive margin of labour supply as a result of the objective of this chapter which involves estimating the effects of providing unpaid care and receiving the national living wage. More importantly, we identified the treatment eligibility for the latter using self-reported hourly wage information. By implication, only individuals who are reportedly working, earned wages and remained in employment following the events are considered in the analysis.

peculiarities of informal carers who may not necessarily need to completely withdraw from the labour market when faced with caring responsibilities.

We measured work hours using the self-reported number of hours normally worked per week. On the other hand, physical health and mental health are respectively measured using the 12-item Short-Form Survey (SF-12) physical component summary score (PCS) and the mental component summary score (MCS). Both PCS and MCS are two components of the SF-12, which convert valid responses to the SF-12 questions into a single mental and physical functioning score. Both measures are on a continuous scale between 0 and 100, and were based on item weights proposed by Ware et al. (1998). Both measures are extracted from the eight domains of the SF-36 using orthogonal factor rotation. They have also been widely used as screening tools to monitor the presence and severity of physical and mental disorders in clinically defined groups in addition to targeting treatment and prevention (Vilagut et al., 2013).

Definition of informal carer

Our definition of informal carers considers every individual who reported in the USoc survey that they provide unpaid care regardless of the age or relationship of the care receivers and the location where they provided the care. There are usually different forms of relationships between the informal carers and the care receivers, including parents, spouses, parents-in-law, grandparents, relatives, friends, neighbours, and clients for volunteering organisations (Department of Health and Social Care, 2018). The caring questionnaire module in the USoc asks the survey participants the following questions to identify residential and nonresidential unpaid carers: (i) Non-residential carers - *"do you provide some regular service or help for any sick, disabled or elderly person not living with you? (excluding help that was provided in the course of paid employment)"*, (ii) Residential carers - *"is there anyone living with you who is sick, disabled or elderly whom you look after or give special help to (for example, a sick, disabled or elderly relative, husband, wife or friend etc)?"*. USoc also collects information regarding the various welfare benefits received by the survey participants which include the carers' allowance. Hence, for completeness, we include respondents who reported receiving the carers' allowance benefit but have missing data in the survey on whether they provide any form of residential or nonresidential unpaid care.

4.3.2 Treatment identification

The effects of the NLW

As discussed in the background section, this study seeks to estimate the effects of the introduction and subsequent upratings of the NLW on informal carers' health outcomes and work hours. We restrict the empirical analysis only to periods before the emergence of the Covid-19 pandemic, as the precise magnitude of the impacts of Covid-19 is hard to disentangle. Besides, the effects of the pandemic on informal caring, labour market outcomes and health, particularly its mental health consequences, have been discussed in other studies (see Banks et al., 2021; Jiskrova, 2022). Recently, Costi et al. (2023) found that the mental health of individuals who started providing care during the Covid-19 pandemic disproportionately deteriorated especially during the lockdowns and social restrictions. Also, Madia et al. (2023) find similar pandemic effects in mental health as well as the exacerbated decline in work hours for informal carers compared to those who never provided care. Therefore, including periods covering the Covid-19 pandemic could bias or crowd out the causal effects of the NLW. We follow extant literature on the causal effects of wage policies in the UK to identify the treated workers affected by the NLW policy and the control group (Akanni et al., 2022a; Kronenberg et al., 2017; Lenhart, 2017a,b; Maxwell et al., 2022; Reeves et al., 2017). The NLW policy between 2016 and 2019 sets out the eligibility criteria for the NLW to include workers aged 25 and above. Effective from 1st April 2021, the NLW policy was extended to include workers that are aged 23 and 24. However, we did not include this age group since they were not captured in the policy during the period under consideration. Besides, their inclusion was during the Covid-19 period, which

Table 4.1 Hourly wage cutoffs for treatment identification

Year	Treatment group	Control group
2016	£7.20	£12.16
2017	£7.50	£12.47
2018	£7.83	£12.77
2019	£8.21	£13.28

Note: The treated group comprises workers who are earning below the NLW base wage rate each year, while the control group consists of workers earning up to the annual median hourly wage.

was not included in our analysis. Accordingly, the treated group comprises individuals who became informal carers[7] that are aged 25 years and above, and whose prevailing basic hourly wage was below the stipulated NLW threshold for each year between 2016 and 2019 (See Table 4.1).

On the other hand, the control group are also informal carers who are aged 25 but who were already earning above the NLW rate. We capped the upper wage band for the control group to the median hourly wage for each period.[8] The selection of the median hourly wage is to some degree in line with the government mandate given to the Low Pay Commission when the NLW policy was set out in 2016 to recommend the level of the path of the national living wage targeting 60% of median earnings by 2020 (UK Department for Business Innovation and Skills, 2015).

Identifying the informal carers and comparison groups

In estimating the effects of becoming informal carers, the main issue of concern is the identification of appropriate counterfactuals to evaluate the causal effects of becoming informal carers. People do not randomly choose to become carers but there are inherent characteristics and conditions. Self-selection into carer further creates potential endogeneity issues, particularly

[7]See Section 4.3.2

[8]The median hourly pay is defined using the Office of National Statistics (ONS) Annual Survey of Hours and Earnings estimates available at https://www.ons.gov.uk/.

on the effects of becoming informal carers on employment and health outcomes. We addressed this self-selection issue using a matching technique to identify non-informal carers using the observable set of pre-treatment characteristics that could affect employment and health outcomes, and the decision to provide informal care (Stöckel and Bom, 2022). The preprocessing of the observed data for both the treatment and comparison group using matching, we seek to reduce or eliminate the selection bias due to using the set of pre-treatment covariates that render subsequent parametric adjustment either irrelevant or less important (Negri, 2023).

We employed the Coarsened Exact Matching (CEM) proposed by Iacus et al. (2012). CEM has become an increasingly popular method employed in the causal effects literature to mitigate endogeneity problems between treatment and outcome (Amaral et al., 2018). Similar to popular matching techniques widely used in the empirical literature such as the PSM, CEM reduces the covariate imbalances in the treatment and comparison samples to allow a more robust test of the causal effect of the policy under consideration, and in the case of this study, the effects of becoming an informal carer. Additionally, it reduces model dependence and estimation error using coarsened variables of covariates to increase the number of matched samples to maximise the sample size and statistical power (Blackwell et al., 2009). CEM technique follows three procedures: (i) coarsen the covariates into a class according to user-defined cutoffs or automatic binning algorithm, (ii) sort each unique coarsened covariates into a stratum, and (iii) discard strata that do not contain at least one treated and one control unit (see Iacus et al., 2012). Iacus et al. (2011) introduced the $\mathscr{L}1$ statistic to measure the degree of imbalance between the treated and comparison groups. Unlike the approach in most matching applications that check the balance between the treated and untreated group by comparing the univariate absolute difference in their means, the $\mathscr{L}1$ statistic is a useful measure of the distance between the empirical distributions of the pre-treatment covariates between the treated and untreated group. The multivariate (or overall) $\mathscr{L}1$ index provides a comprehensive measure of global imbalance, including the imbalances for all the covariates considered. The final stage of the CEM matching process is to employ the matched cases to estimate the causal effect of becoming an unpaid carer using the weights from the CEM. The remaining imbalance from the matched sample can also be addressed by further including the covariates as control variables (Iacus et al., 2019).

The main shortcoming of the CEM approach is its inability to account for unobserved covariates that affect both the outcome and the treatment assignment. By matching only on the observable and measured covariates, it does not eliminate treatment endogeneity caused by unobservables, and hence it does not reduce or eliminate bias in estimated outcomes associated with the unobserved covariates. However, in selecting the covariates included in the matching model, we follow the intuition proposed by Schmitz and Westphal (2017) and Stöckel and Bom (2022) regarding the underlying motivations and preconditions to provide informal care: (i) the necessity to provide care (ii) the willingness to provide care, and (iii) the ability to provide care. First, the decision to provide informal care is primarily premised on the need for care from close acquaintances, usually, a family member or close associates, and sometimes there are limited or absent alternatives. Secondly, the willingness of the potential informal carer to provide the care, and this depends on their personality traits, socioeconomic characteristics and other individual factors that affect their inclination towards providing care. The third precondition is the ability to provide care and it relates to factors such as the informal carers' pre-existing health status (Stöckel and Bom, 2022). In line with these three preconditions and data availability, we considered covariates including age, gender, number of children, highest education qualification, employment type, general self-reported health status, the presence of long-standing illness or disability, and whether the individual's father or mother is alive. More importantly in selecting the covariates, it is important to note that increasing or reducing the number of included covariates in the matching process does not necessarily ensure a better matching outcome (Giuliani, 2023). Besides, it is not recommended because a much larger reduction in the number of observations as a result of increasing or reducing covariates increases the risks of losing more individuals from treatment cohorts, which could result in underestimating the causal effects (Iacus et al., 2019).

For the robustness of the results obtained using the CEM procedure, we employed the PSM method to match individuals who became informal carers to the never-carer groups. We estimated a one-to-one matching given the large number of respondents who never provided informal care in our sample and similar to the method employed by Costi et al. (2023). On the choice of the functional form of the propensity score model, we estimate different binomial models for each of the treatment groups and the comparison group following the procedure recommended in Caliendo and Kopeinig (2008), and given that different individuals became informal carers at different times (see also Lechner, 2001).[9]. The covariates included in the propensity-matching model are similar to those considered in the CEM procedure, and their inclusion was informed by economic theory, previous related research and information about the institutional settings as previously discussed in the CEM procedure. The pairwise t-tests for imbalances as well as the estimated treatment effects using the PSM-matched sample are summarised in Appendix C.

4.3.3 Estimation method

The main objective of this chapter as set out in the introduction (see Section 4.1) is to evaluate the effects of the introduction and annual uprating of the NLW for individuals who became informal carers during the period under consideration. To do this, we estimate separate treatment effects regressions for the group of informal carers who received the NLW and the comparison group of unpaid carers already earning above the wage thresholds. We follow the standard procedure in recent papers by estimating by first estimating the two-way fixed effects (TWFE) model for separate groups, and the model is specified as:

[9]The alternative is to estimate a multinomial probit model. However, the approach is established to show little differences in its relative performance compared to estimating a series of binomial models, which in addition is robust to misspecification (see Caliendo and Kopeinig, 2008, pp. 37)

$$Y_{i,t} = \alpha_i + \lambda_t + \beta_{twfe} W_{it} + \gamma X_{it} + \varepsilon_{i,t} \tag{4.1}$$

where $Y_{i,t}$ is the outcome for individual i at period t. α_i is the individual fixed effects, λ_t is the period fixed effects, and W is the treatment indicator, and it has a value of 1 indicating when an informal carer received the NLW and 0 otherwise. β_{twfe} is the treatment effects parameter indicating the effect of becoming an unpaid carer. and X_{it} is the vector of covariates included in the estimation and ε is the error term, which are clustered at the primary sampling unit level (see also Costi et al., 2023).

One limitation of equation 4.1 as shown in recent difference-in-differences literature is that the treatment estimates from β_{twfe} may provide biased estimates when there is variation in the policy rollout across individuals and over time (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2022; Sun and Abraham, 2021). As such and as illustrated in Chapter 2, the NLW policy can be treated as a roll-out of events with individuals reaching the eligible age every year and the basic wage threshold also changing. Hence, we extended the analysis by employing an estimation procedure involving multiple treatment cohorts and variations in treatment periods. We followed the Callaway and Sant'Anna (2021) approach which allows for multiple treatment periods. The model assumptions and parameters have been previously discussed in Section 3.5, and they include the treatment irreversibility and no anticipation assumptions, among others. An advantage of the estimator is that it allows for the evaluation of heterogeneous treatment effects, providing both the disaggregated and cumulative effects across when individuals became treated and over time.

We consider individuals who become informal carers based on whether they received the National Living Wage (treatment group) or otherwise (comparison group). The aim is to evaluate whether receiving the NLW affects the work-care decisions or provides any health respite for the informal carers. The observed and potential outcomes (Callaway and Sant'Anna, 2021) can be expressed as:

$$Y_{it}^{C} = Y_{it}^{C}(0) + \sum_{g=2}^{T} (Y_{it}^{C}(g) - Y_{it}^{C}(0))\dot{G}_{ig} \tag{4.2}$$

$Y_{it}(0)$ denotes potential outcomes for informal carers (C) that do receive the NLW. $Y_{it}(g)$ is the potential outcome of individual i at time t when they first participate in the treatment and g captures the group of informal carers that received the NLW at the same time. We employed 'did' R package provided by Callaway and Sant'Anna (2021) to estimate the "group-time average treatment effect" parameter $ATT(g,t)^{C}$, and the $ATT(g,t)^{C}$ equation is given as:

$$ATT_{g,t}^{C} = \mathbb{E}[Y_{t}^{C}(g) - Y_{t}^{C}(0)|G_{g} = 1] \tag{4.3}$$

The $ATT(g,t)$ in Equation 4.3 provides the aggregate average treatment effect estimate of becoming an informal carer.

4.4 Results

4.4.1 Matching results and summary statistics

Table 4.2 summarises the results of the overall imbalance test in the unmatched and matched sample of informal caregivers and non-carers using the pre-treatment covariates as discussed in Section 4.3.2. The $\mathscr{L}1$ index ranges between 0 and 1 with the former indicating a perfect balance across covariates and 1 denoting a perfect imbalance between the treated and control group. However, interpreting the value of a $\mathscr{L}1$ statistic does not provide valuable insights on its own, rather a comparison between the $\mathscr{L}1$ statistic for pre-matched and matched samples to assess the increase in the balance due to the matching solution from that difference (Blackwell et al., 2009).

The multivariate imbalances in the pre-treatment covariates show that there is a high degree of imbalance in the pre-treatment covariates between the treatment groups and the pool of individuals who were never-carers and from which the comparison group would be matched. The index is high across all the groups with the lowest value recorded for the 2016 group at 0.66 (see Table 4.2). However, the post-matching imbalances following the CEM procedure indicate a lower multivariate $\mathscr{L}1$ index for the matched samples. The multidimensional imbalance between the treatment and matched control group after considering all the covariates shows lower imbalances. For example, while the pre-treatment imbalance index for the 2016 treatment group is 0.66, the post-matched index is 0.37. The results indicate a more balanced sample than the pre-matched sample. A similar reduction in the $\mathscr{L}1$ statistic is observed for other pre-treatment periods.

Additionally, we evaluated the pre- and post-matching differences in the covariates between the treatment and the matched control group. We determine the extent of balancing between the treatment groups and the matched control group using the matching weights obtained from the CEM procedure. The results are summarized in Table 4.3. The pairwise t-tests of the differences before and after matching largely indicate no statistically significant differences in the observed covariates between the treatment and comparison groups after the matching. However, some variables including whether the father or mother is alive and the number of children in the household still exhibit some significant differences after matching (see Table 4.3).

We present the descriptive statistics of the variables in Table 4.4. The table provides the mean and standard deviation statistics for each of the treatment cohorts, i.e., individuals who started providing unpaid care at different periods, and those who were never carers. The table shows that the average age of individuals who started providing unpaid care is higher than the never-carer group. Also, the proportion of females that are informal carers across all the periods is more than in the non-informal carers. Although there are more females in both categories of

Table 4.2 Test for imbalances in the raw and matched carers sample

	Raw sample		Matched sample	
Period	$\mathscr{L}1$	**No. treated**	$\mathscr{L}1$	**No. treated**
2016	0.6631	2116	0.3647	1916
2017	0.6977	1433	0.3719	1326
2018	0.7154	1133	0.3445	1019
2019	0.7280	876	0.3889	784

Note: The $\mathscr{L}1$ denotes the multivariate $\mathscr{L}1$ index and it measures the degree of imbalance between the treated and comparison groups in the sample before and after matching. The detailed results of the imbalance tests are reported in the Appendix C.

informal and never carers. This is in line with various literature and reports documenting that females are more likely to be informal carers than males (see Milletler, 2020). Table 4.4 further shows that most individuals who became informal carers in the different periods were married or living as a couple. Overall, the matched sample identified sufficiently similar observations between the treatment and control groups based on the set of selected covariates.

4.4.2 Average treatment effects results

This section discusses the results of the effects of becoming informal carers and also receiving the national living wage. We commence the analysis with the two-way fixed effect regression, which estimates the changes in outcomes (mental and physical health and work hours) between individuals who became carers and the matched sample of individuals who were never carers o, before and after entering into informal care. The informal caregiving column in Table 4.5 summarises the TWFE results for each period across the three outcomes. The results suggest mixed effects of becoming an informal carer across the different outcomes and periods considered. Becoming an informal carer is associated with a decline in mental and physical health as well as work hours for each year considered, except for mental health in 2017 and physical health in 2019. However, the estimates show mixed statistical significance.

Table 4.3 Covariates differences before and after CEM

	2016		2017		2018		2019	
	Unmatched	**matched**	**Unmatched**	**matched**	**Unmatched**	**matched**	**Unmatched**	**matched**
Age	13.72	-0.6	10.82	-0.47	8.24	-0.79	6.19	-0.18
	[0.000]	[0.552]	[0.000]	[0.638]	[0.000]	[0.427]	[0.000]	[0.854]
Number of children in HH	-5.09	1.3	-4.27	0.95	-2.38	1.22	-2.99	1.78
	[0.000]	[0.193]	[0.000]	[0.344]	[0.017]	[0.224]	[0.003]	[0.076]
Gender (female)	6.01	0.2	2.09	0.09	3.07	0.91	2.83	-0.49
	[0.000]	[0.840]	[0.036]	[0.930]	[0.002]	[0.362]	[0.005]	[0.626]
Marital status (married)	7.28	-1.44	4.28	-0.8	4.99	-0.72	2.85	-0.23
	[0.000]	[0.149]	[0.000]	[0.424]	[0.000]	[0.469]	[0.004]	[0.822]
Education qualification (degree)	-1.32	1.05	-3.56	0.68	-0.46	0.75	-0.65	-0.32
	[0.187]	[0.295]	[0.000]	[0.500]	[0.645]	[0.452]	[0.515]	[0.749]
Job type (part-time)	4.25	1.34	5.05	0.83	1.94	1.46	1.08	1.31
	[0.000]	[0.180]	[0.000]	[0.408]	[0.052]	[0.146]	[0.282]	[0.189]
Self-rated health (fair or poor)	5.83	1.25	5.57	1.66	6.7	1.64	2.53	1.99
	[0.000]	[0.211]	[0.000]	[0.098]	[0.000]	[0.102]	[0.011]	[0.046]
Longstanding illness or disability	8.45	1.05	5.93	0.44	7.93	0.82	4.76	0.8
	[0.000]	[0.294]	[0.000]	[0.661]	[0.000]	[0.414]	[0.000]	[0.424]
Father alive	-3.84	1.78	-4.78	1.83	-0.69	1.32	-0.16	0.91
	[0.000]	[0.075]	[0.000]	[0.068]	[0.489]	[0.188]	[0.872]	[0.362]
Mother alive	2.71	2.48	-0.91	2.2	2.55	1.58	3.12	1.78
	[0.007]	[0.013]	[0.365]	[0.028]	[0.011]	[0.114]	[0.002]	[0.075]

Note: The table reports the pairwise t-tests (absolute values) before and after the CEM. The columns denote the period that each informal carer first reported providing unpaid care. Before and After respectively indicate before and after the matching process. The p-values for the differences between the treatment and control groups are reported in square brackets. The highlighted p-values denote covariates showing statistical differences post-matching. The results are generated using the *'pstest'* command in Stata and the detailed matching results are reported in the Appendix C.

We estimate the impacts of receiving the national living wage for unpaid carers and noncarers alike. We re-estimated the fixed effects model separately for the group who became unpaid carers during the period under consideration, and the never-carer group. Columns II and III in Table 4.5 present the results for the two groups respectively. While the 2016 estimates in column I show that becoming an unpaid carer led to a significant decline in work hours, for those that received the NLW and compared to the comparison groups that did not, there is a marginal increase in work hours among the sample of carers and noncarers that received the national living wage in 2016, 2017 and only 2018 for the noncarers. However, none of the estimates is significant for both mental and physical health outcomes.

Table 4.6 summarises the group-time average treatment effects of receiving the NLW for the group of informal caregivers estimated using the multiple periods difference-in-differences

Table 4.4 Summary statistics

	2016	2017	2018	2019	Control
Age	52.636	52.062	51.432	49.850	46.681
	(17.364)	(17.102)	(16.809)	(16.478)	(19.351)
Sex (Female)	0.578	0.553	0.573	0.564	0.510
	(0.494)	(0.497)	(0.495)	(0.496)	(0.500)
Marital status (Married)	0.682	0.672	0.699	0.701	0.601
	(0.466)	(0.470)	(0.459)	(0.458)	(0.490)
Educational qualification (Degree)	0.361	0.343	0.395	0.398	0.376
	(0.481)	(0.475)	(0.489)	(0.490)	(0.484)
Job type (part-time)	0.312	0.308	0.285	0.254	0.253
	(0.463)	(0.462)	(0.452)	(0.436)	(0.435)
Self-rated health (fair or poor)	0.233	0.241	0.245	0.196	0.180
	(0.423)	(0.428)	(0.430)	(0.397)	(0.385)
Longstanding illness or disability	0.399	0.388	0.420	0.384	0.310
	(0.490)	(0.487)	(0.494)	(0.487)	(0.463)
Number of children in HH	0.283	0.299	0.316	0.311	0.337
	(0.450)	(0.458)	(0.465)	(0.463)	(0.473)
Father alive	0.180	0.212	0.215	0.211	0.216
	(0.384)	(0.409)	(0.411)	(0.409)	(0.412)
Mother alive	0.271	0.261	0.296	0.280	0.244
	(0.445)	(0.439)	(0.457)	(0.449)	(0.430)
SF-12 Mental Component Summary	49.085	48.970	48.679	48.823	49.435
	(10.234)	(10.722)	(10.342)	(10.097)	(10.004)
SF-12 Physical Component Summary	47.899	48.209	48.327	50.039	50.259
	(11.904)	(11.776)	(11.425)	(10.833)	(10.908)
Work hours	32.087	32.175	32.117	33.449	33.110
	(11.665)	(11.497)	(11.844)	(10.790)	(11.041)
No. of Obs.	2116	1433	1133	876	26000

Note: The table summarises covariates mean and standard deviations (reported in parenthesis) for the sample of informal carers and the matched non-carers in the pre-treatment period. The treatment group include individuals who became carers in each successive period, and the control group includes the matched sample as discussed in Section 4.3.2. Standard deviations are reported in parentheses.

model. [10] The pre-trend column summarises the Wald pre-test for parallel trends assumption, and it indicates that we cannot reject the null hypothesis that the parallel trend assumption holds in the treatment effects estimation. The estimated p-values are larger than 0.05 for the three outcomes, and each suggests that the parallel trends assumption holds in the pre-treatment periods.

The disaggregated average treatment effects over the periods different groups of informal carers received the NLW indicate variations in the effects of NLW on work hours, mental health and physical health with none showing statistical significance. On the other hand, the simple average group-time treatment effect summarised in the weighted average column in Table 4.6 shows the mixed effects of receiving the NLW. While the result is not significant for health outcomes, the estimated weighted average treatment effect shows a significant increase in work hours for informal carers who received the NLW compared to informal carers who did not receive the wage increase. However, it is important to note that none of the disaggregated estimates is statistically significant (see Table 4.6).

4.5 Effects of becoming informal carer - using heterogeneous DID estimator

Individuals enter into informal caring at different times giving rise to time-variant treatment exposure. Hence, we explore the heterogeneity in the treatment effects of becoming an informal carer on mental health, physical health and mental health using the staggered-adoption design. We summarised the estimated treatment effects in Table 4.7. We re-estimate the treatment effects of becoming unpaid carers under the unconditional parallel trend assumptions, that is

[10] Additionally, we estimate the group-time average treatment effects of receiving the NLW for the group of individuals who were never informal carers over the period under consideration. The results are summarised in Appendix C (See Table C.2).

Table 4.5 Two-way fixed effects results - informal caregiving and NLW

	I - Informal caregiving			**II - NLW & unpaid carers**			**III - NLW & noncarers**		
Period	**Mh**	**Ph**	**Wh**	**Mh**	**Ph**	**Wh**	**Mh**	**Ph**	**Wh**
2016	-0.179	-0.436*	-0.905***	-0.786	-0.463	1.334*	0.228	-0.572	1.481**
	(0.305)	(0.255)	(0.219)	(0.854)	(0.857)	(0.755)	(0.603)	(0.513)	(0.586)
2017	0.213	-0.417*	-0.220	-0.311	-0.898	1.698*	-0.101	0.044	1.316**
	(0.311)	(0.250)	(0.416)	(0.699)	(0.683)	(0.873)	(0.489)	(0.391)	(0.557)
2018	-0.422	-0.028	-0.280	-0.259	-0.886	-0.364	-0.125	-0.074	1.115**
	(0.350)	(0.280)	(0.247)	(0.740)	(0.668)	(0.781)	(0.493)	(0.404)	(0.497)
2019	-0.936**	0.100	-0.204	-0.164	-0.075	0.791	0.628	-0.248	0.603
	(0.426)	(0.262)	(0.285)	(0.629)	(0.516)	(0.688)	(0.408)	(0.335)	(0.440)

Note: The table summarises the main parameter of interest (β_{twfe}) in Equation 4.1 indicating the average treatment effects. column I summarises the ATE of becoming an unpaid carer, while columns II and II respectively summarise the estimated effects of the NLE for unpaid carers and those who were never carers. The acronyms respectively indicate: Mh - mental health; Ph - physical health; Wh - work hours. All the specifications include individual and year-fixed effects and are estimated using the 'reghdfe' command (Correia, 2019) using Stata 17. Robust standard errors are presented in parenthesis and clustered at the primary sampling unit level. ***, **, and * indicates statistical significance at 1%, 5% and 10% respectively.

Table 4.6 Group-time difference-in-differences results - NLW effects

	2016	**2017**	**2018**	**2019**	**Weighted average**	**Pretrend χ^2**
Mental health	-0.230	0.342	-0.348	-0.555	-0.220	1.906
	(1.072)	(0.982)	(1.457)	(1.036)	(0.564)	[0.928]
Physical health	-0.939	-0.963	-0.540	-1.083	-0.709	5.439
	(0.1.081)	(0.855)	(0.946)	(0.738)	(0.456)	[0.489]
Work hours	2.2024	2.4901	-0.0555	1.5951	1.7280**	0.5439
	(0.9150)	(1.1834)	(1.3012)	(1.5439)	(0.6532)	

Note: The Table summarises the group-time treatment effects of receiving the NLW for the group of individuals that become informal carers. All estimations are carried out using the 'csdid' package in Stata (Callaway and Sant'Anna, 2021). Each row summarises the average treatment effects on each outcome and indexed by the period each cohort first received the NLW. The 'weighted average' column provides a single parameter weighted average group-time average treatment effect. Robust standard errors are presented in parenthesis and clustered at the primary sampling unit level, 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 Pretrend χ^2 column summarises the Wald test of parallel trend assumption with a null hypothesis that all pre-treatment is equal to zero and the corresponding probability values in squared brackets.

Table 4.7 Group - time average treatment effects of becoming informal carer

	2016	2017	2018	2019	Weighted average	P-value
Mental health	-0.416	-0.208	-0.490	-0.754	-0.395**	2.571
	(0.269)	(0.353)	(0.384)	(0.485)	(0.180)	[0.860]
Physical health	0.322	-0.295	-0.285	-0.089	0.046	3.255
	(0.240)	(0.272)	(0.329)	(0.324)	(0.154)	[0.776]
Work hours	-0.867***	-0.129	-0.183	-0.749*	-0.556***	6.542
	(0.253)	(0.294)	(0.338)	(0.389)	(0.163)	[0.365]

Note: The table summarises the group-time treatment effects of becoming informal carers under the unconditional parallel trends assumptions, that is, without including the covariates. All estimations are carried out using the 'csdid' package in Stata (Callaway and Sant'Anna, 2021). Each row summarises the average treatment effects on each outcome and indexed by the period each cohort first received the NLW. The 'weighted average' column provides a single parameter weighted average group-time average treatment effect. Robust standard errors are presented in parenthesis and clustered at the primary sampling unit level, 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 pretrend χ^2 column summarises the Wald test of parallel trend assumption with a null hypothesis that all pre-treatment are equal to zero and the corresponding probability values in squared brackets.

without including the covariates. First, the estimated p-values of the Wald statistics are larger than 0.05, suggesting that the parallel trends assumption holds in the pre-treatment periods.

The aggregate group-time average treatment effects show that the estimated weighted average summary parameter is negative and statistically significant for mental health and work hours. However, not statistically significant for physical health. In context, the estimated average treatment effects show that work hours and mental health declined for individuals who became informal carers between 2016 and 2019, compared to their matched comparison group who did not provide unpaid care over the considered period. Overall, the estimates show supportive evidence of the impact of becoming an informal carer on declining labour force participation and mental health outcomes. Our results is similar to Costi et al. (2023) who find a significant effect of becoming an informal carer on declining work hours and increased mental health issues in the UK following the Covid-19 pandemic (also see Madia et al., 2023).

4.6 Discussion and conclusion

The impacts of informal caregiving on labour market participation and health outcomes have been discussed in the literature (Fevang et al., 2008). In addition, studies have considered the effects of interventions, including financial and non-financial incentives on unpaid carers and caring activities (Zhu and Onur, 2022). However, the empirical literature is limited and not clear on the impacts of minimum wage on informal caregiving (Jutkowitz et al., 2022). This chapter exploits the introduction of the UK NLW in 2016 and the subsequent annual increases to investigate the impacts of increased wage floors on the work hours and health effects of becoming unpaid carers. We employed the longitudinal richness in the Understanding Society data, and the detailed responses on unpaid care activities, self-reported health outcomes and earnings information.

We presented estimated average treatment effects using the canonical difference-in-differences estimation. Additionally, we extend the analysis to account for the heterogeneous effects of becoming unpaid carers at different periods using the difference-in-differences setting that allows for the disaggregated and interpretable impact of a staggered policy design. The results show that the effect of becoming an informal carer on work hours is negative, but the mental and physical health effects are mixed across the different periods considered. Informal carers who received the NLW tend to increase their work hours when compared to other informal carers who are already earning above the wage threshold. Overall, our results suggest the marginal increase in NLW could lead to adjustments in work hours among eligible unpaid carers compared to other informal carers who did not receive the wage increase.

Chapter 5

Conclusions

5.1 Summary of Contributions and Implications

The trajectories of income over time and exogeneous shocks to income such as increase in wage floors and changes in state welfare benefits are important determinants of health and well-being outcomes. Low-pay individuals are very susceptible to policy changes that marginally affects income and earnings. Additionally, increasing minimum wages impacts workers' earnings, and reduces earnings inequality and in-work poverty, all of which have implications on the workers' health and overall economic well-being. In this thesis, we revisit income-related policy reforms and its nexus with health, well-being and informal caregiving in the UK, focusing on the effects of the introduction and annual upratings in the national living wage.

We reconsidered income-health nexus in chapter 2 by evaluating changes in income dynamics on health and well-being outcomes. Studies have shown current income as a major cause of inequalities in health and well-being outcomes using cross-sectional and longitudinal data. We contribute to this literature by employing the recently developed fixed-effects ordered logit model, that allows ordered health and well-being responses, which enhances reliability and validity of estimated results, and model with fixed effects. The estimation approach also relaxes the distributional and independence assumptions in the random effects model for ordered outcomes. Additionally, we considered downward and upwards trends in income, and their effects on health and well-being. We employed different constructs of income experiences including income stability, income volatility, as well as low and high income spells using data from the Understanding Society UK Household Longitudinal Study. We also considered different self-reported health and well-being measures including general health, mental health, long-standing illness or disability, and satisfaction with life and leisure.

The empirical results confirmed the positive effects of increasing family income on health which is also in line with findings in the literature. Additionally, we find that stability in income position is strongly associated with improved health and well-being. While income volatility is connected to increased odds of reporting poor health outcomes. The association between volatile income and health is considerably more significant for individuals from low-income households. Also, we find that more years spent in a lower-income quartile reduces the odds of reporting improved self-rated health. The chapter concludes with a disaggregated analysis using data samples partitioned into periods before and periods after 2016, coinciding with the introduction of the National Living Wage policy. The significant Wald tests results between estimates in the two periods highlight the significant shifts in the effects of income trajectories on self-reported health and well-being following the implementation of the national living wage policy. One of the implications of these findings is that policies designed to address health and well-being problems must consider income volatility as an important source of risks. The significance of volatile incomes on health and well-being, especially for individuals from low-income households also suggests that designing social policies and safety net programmes targeting low income individuals should encompass measures to deal with associated rising income volatility.

In chapter 3, we considered the effects of the introduction and subsequent increases in the national living wage on mental health. Previous studies on the NLW policy effects focused on the labour market and employment outcomes. However, we considered the health-effects by focusing on mental health. Our choice of mental health is premised on its focal impact on all other aspects of health and well-being. Another major contribution in this chapter is the consideration of the counteracting effects of the welfare benefits freeze policy. The welfare freeze policy freezes the main rates of all working age benefits and tax credits and was simultaneously implemented with the NLW in 2016. Identifying perfect comparison group for quasi-experimental evaluation of wage policies in the UK is challenging. Contrary to decentralised minimum wage policy across states and provinces in the US, Canada and other countries with similar regional variations and disaggregated wage policies, minimum wage policy in the UK is centralised. We defined treatment as individuals who are eligible for the NLW, that is aged 25 and above, and earn below the stipulated wage threshold. On the other hand, the comparison groups include individuals earned above the NLW threshold but not more than the annual median hourly wage. We employed the difference-in-differences approach proposed by Callaway and Sant'Anna (2021) which allows for the heterogeneity in the policy design.

Our results showed that NLW leads to positive improvements in mental health. By contrast, the positive effect on NLW on mental health is constricted by the welfare benefits freeze policy. We also found that the NLW increase job satisfaction, supporting the psychosocial hypothesis that associates increase in job satisfaction with improvements in mental health. We also find empirical evidence supporting the substitution effects between work hours and leisure satisfaction following increase in wages. The results show that to achieve and sustain the positive effects of increased wage floors, they should not be accompanied by other interventions that lead to overall stagnation or reduction in earnings for the affected workers. Our findings also 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 earnings. Rather than considering wages increase and government welfare benefits as alternatives, the two should be complementary. Besides, their prospects of reducing earnings inequality, in-work poverty and generating liveable income for families may be more effective in combination rather than reducing one for the other.

In Chapter 4, We extend the investigation of the health-effects of the UK national living wage to the informal care sector. Informal carers do not only make enormous contribution to the acquaintances they support, they form an integral part of the country's health and social system. However, the motives for providing unpaid care have been identified as a combination of satisfaction and challenges, with both having positive and negative consequences for the caregivers. There is also a dual impact on labour supply and the overall economy, given the very specific situations of unpaid carers. While unpaid carers clearly have the capacity to work, the interactions of the need for care from close relatives or friends, and the willingness and ability to provide care on the part of the carer, often bring about the need to combine work with caring responsibilities. We began the empirical analyses first considering the effects of becoming informal carers on work hours and physical and mental health. We observed significant decline in work hours in periods following the start of informal caregiving. Similarly, our results show negative effects of informal caring on physical health outcome, while the effects on mental health is not significant. Next we examined the effects of receiving the NLW by the informal carers on three outcomes. We found increase in work hours for the group of informal carers that received the national living wage compared to informal carers already earning above the wage threshold. On the other hand, the NLW effects is not significant for the carers' health outcomes.

While the effects of NLW is not significant on informal carers' health outcomes, there is an interesting pattern observed on their work hours. the overall effects of becoming informal carer on work hours is negative. However, the effect is positive effects for the group of informal carers that received the NLW compared to carers already earning above the NLW threshold. The results suggests that different informal carers do what makes sense for them depending on their socioeconomic circumstances. While there is an overall decrease in work hours from becoming an unpaid carer, some carers increase their hours in response to NLW increase. Above all, the NLW policy appears to provide a positive employment outcome through increased labour force participation even among workers with caring responsibilities.

5.2 Directions for Future Research

There are several dimensions to contributions and extensions of the literature on wage legislation and their effects on health and well-being outcomes. For example, the emergence of Covid-19 was unprecedented as it affects every facets of human lives and the economy. While the empirical analyses in this thesis were restricted to periods before the Covid-19 pandemic, it would be beneficial to consider in future studies, the effects of the coronavirus pandemic on the health impacts of minimum wage legislation.

Moreover, during the pandemic, the government introduced the Coronavirus Job Retention Scheme (CJRS) otherwise popularly referred to as the Furlough Scheme. Under the scheme, the government pays certain percentage of usual wages of employees that temporarily stopped working due to the coronavirus pandemic through their employers. The scheme helped to prevent the mass layoffs that was imminent as a result of the lockdown measures in place during the pandemic and the significant decline in production and productivity. Nonetheless, the beneficiaries of the furlough scheme were disproportionately young and low paid workers, and mostly in small businesses and sectors including hospitality, arts and recreation (Pope and Shearer, 2021). In addition, the government wage support is capped in percentage terms at £2500 or up to 80% of the usual employee pay, with a choice for the employer to top up the pay, if they so wished (HM Revenue and Customs, 2021). Future studies would benefit from investigating the intersectionality of national living wage policy and the furlough scheme on health and well-being outcomes.

The heterogeneous difference-in-differences methodology employed in Chapters 2 and 3 would also benefit from future extensions to the quasi-experimental techniques and coverage.

One of such extensions is the incorporation of staggered adoption treatment designs to the triple difference estimator which is currently receiving attention in methodological literature (see Strezhnev, 2023). Additionally, future studies would benefit from using other data sources that allows the linkage of wage information to health and well-being outcomes. More accurate wage information using data extracted from actual payrolls and administrative sources and linking such data to patient health data would further strengthen the accuracy of the estimated causal effects of the national living wage policy on health and well-being.

Furthermore, Chapter 4 on the effects of the national living wage on informal caregiving points towards other extensions in the future. It would be beneficial to consider the effects of other array of reforms and policies that specifically affects unpaid carers. One of such policies in the UK is the Scottish Carer's Allowance Supplement, which provides extra payment for unpaid carers that are resident in Scotland who are in receipt of the UK-wide Carer's Allowance. Such reforms in specific and targeted policies could make substantial differences to the lived experience of carers and people receiving care (Cantillon and Kirk, 2020). However, while studies in the literature have considered the impacts of the main Carer's Allowance (see Brimblecombe et al., 2018), the extension by the Scottish government, which increases the allowance for affected carers by up to 13%, is yet to receive attention in empirical literature. Addressing the aforementioned research directions, among others, could lead to more informed decisions in wage policy making. Also, public authority and public health experts can make more informed decisions regarding minimum wage policies and their impact on population health, and ultimately working towards improved well-being and social equity.

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Appendix A

Chapter 2 Appendix

Table A.1 Descriptive statistics

Variable	Full sample		2011 - 2015		2016 - 2019	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Household disposable income (log)	1,879.82	1,950.72	1,866.94	2,074.77	1,895.92	1,783.42
General health	3.36	1.06	3.45	1.07	3.25	1.04
Mental health	11.37	2.93	11.38	2.89	11.35	2.98
Leisure satisfaction	4.88	1.66	4.79	1.69	4.99	1.62
Life satisfaction	5.23	1.43	5.22	1.45	5.24	1.42
Age	53.61	15.98	51.60	15.83	56.11	15.81
Gender						
Male	0.44	0.50	0.44	0.50	0.44	0.50
Female	0.56	0.50	0.56	0.50	0.56	0.50
Marital status						
Never married	0.14	0.34	0.14	0.35	0.13	0.33
Married or cohabiting	0.71	0.46	0.71	0.46	0.70	0.46
Unmarried	0.16	0.37	0.15	0.36	0.17	0.38
Educational qualification						
No qualification	0.11	0.32	0.11	0.32	0.11	0.31
Other qualification	0.10	0.30	0.10	0.30	0.10	0.30
GCSE; A-level; etc	0.38	0.49	0.39	0.49	0.37	0.48
Degree and above	0.41	0.49	0.40	0.49	0.43	0.49
Ethnicity						
White	0.90	0.30	0.90	0.30	0.90	0.30
Non-white	0.10	0.30	0.10	0.30	0.10	0.30
No employed in household	1.24	1.06	1.27	1.04	1.19	1.08
No of observations	148,588		78,466		69,201	

Table A.2 Moderating effects of labour supply on household income and leisure satisfaction

	No interaction	With interaction
Household disposable income (log)	-0.071***	-0.011
	(0.016)	(0.024)
Number employed in household (Ref: *None*)		
At least one person	-0.405***	0.312
	(0.031)	(0.211)
Interaction term		-0.100***
(*labour supply and household income*)		(0.029)
Age	-0.295***	-0.294***
	(0.026)	(0.026)
Age-squared	0.006***	0.006***
	(0.001)	(0.001)
Age-cubed	-0.000***	-0.000***
	(0.000)	(0.000)
Marital status (Ref: *Never married*)		
Married or Cohabiting	-0.053	-0.053
	(0.050)	(0.050)
Unmarried	0.147**	0.149**
	(0.059)	(0.059)
Education Attainment (Ref: *No qualification*)		
Other qualification	-0.307**	-0.309**
	(0.153)	(0.153)
GCSE; A-level; etc	-0.053	-0.054
	(0.176)	(0.176)
Degree and other higher degrees	-0.232	-0.232
	(0.186)	(0.186)
Region dummies	Yes	Yes

Table A.3 Income stability on health and wellbeing

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Average income	0.282***	0.121***	0.201***	0.280***
	(0.019)	(0.023)	(0.020)	(0.020)
Age	-0.173***	-0.160***	-0.366***	-0.285***
	(0.034)	(0.034)	(0.032)	(0.035)
Age-squared	0.003***	0.003***	0.007***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Age-cubed	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Gender				
(Ref: *Male*)				
female	-0.010	-0.335***	-0.012	0.051
	(0.032)	(0.036)	(0.031)	(0.031)
Marital status				
(Ref: *Never married*)				
Married or Cohabiting	0.250***	0.316***	0.075	0.616***
	(0.058)	(0.060)	(0.057)	(0.059)
Unmarried	0.026	0.001	-0.093	0.154**
	(0.069)	(0.072)	(0.069)	(0.070)
Education Attainment				
(Ref: *No qualification*)				
Other qualification	0.282***	0.047	0.003	0.008
	(0.078)	(0.085)	(0.075)	(0.083)
GCSE; A-level; etc	0.418***	0.143**	0.150**	0.057
	(0.065)	(0.073)	(0.064)	(0.073)
Degree and above	0.804***	0.074	0.255***	0.215***
	(0.069)	(0.075)	(0.066)	(0.073)
Ethnicity				
(Ref: *White*)				
Non-white	-0.205***	0.057	-0.144**	-0.192**
	(0.074)	(0.082)	(0.073)	(0.077)
Number employed in the HH	0.192***	0.117***	-0.183***	0.044*
	(0.024)	(0.026)	(0.022)	(0.023)
Region dummies	Yes	Yes	Yes	Yes

Table A.4 Income volatility on health and wellbeing

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Income volatility	-0.008	-0.057***	-0.031*	-0.085***
	(0.017)	(0.018)	(0.016)	(0.017)
Age	-0.194***	-0.173***	-0.383***	-0.314***
	(0.034)	(0.034)	(0.031)	(0.035)
Age-squared	0.003***	0.004***	0.007***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Age-cubed	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Gender				
(Ref: *Male*)				
female	-0.032	-0.343***	-0.028	0.028
	(0.032)	(0.035)	(0.031)	(0.031)
Marital status				
(Ref: *Never married*)				
Married or Cohabiting	0.331***	0.345***	0.131**	0.685***
	(0.058)	(0.060)	(0.057)	(0.060)
Unmarried	0.006	-0.006	-0.106	0.138*
	(0.069)	(0.072)	(0.069)	(0.071)
Education Attainment				
(Ref: *No qualification*)				
Other qualification	0.348***	0.076	0.049	0.073
	(0.077)	(0.085)	(0.074)	(0.082)
GCSE; A-level; etc	0.542***	0.195***	0.241***	0.179**
	(0.064)	(0.071)	(0.062)	(0.071)
Degree and other higher degrees	1.096***	0.201***	0.466***	0.507***
	(0.065)	(0.070)	(0.062)	(0.069)
Ethnicity				
(Ref: *White*)				
Non-white	-0.309***	0.024	-0.211***	-0.275***
	(0.073)	(0.081)	(0.073)	(0.077)
Number employed in the HH	0.231***	0.132***	-0.156***	0.078***
	(0.024)	(0.026)	(0.022)	(0.023)
Region dummies	Yes	Yes	Yes	Yes

Table A.5 Below median income on health and wellbeing

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Below median income quartile	-0.066***	-0.032***	-0.047***	-0.050***
	(0.007)	(0.007)	(0.006)	(0.007)
Age	-0.172***	-0.160***	-0.366***	-0.291***
	(0.034)	(0.034)	(0.031)	(0.035)
Age-squared	0.003***	0.003***	0.007***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Age-cubed	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Gender				
(Ref: *Male*)				
female	-0.019	-0.337***	-0.019	0.038
	(0.032)	(0.035)	(0.031)	(0.031)
Marital status				
(Ref: *Never married*)				
Married or Cohabiting	0.172***	0.274***	0.018	0.572***
	(0.061)	(0.063)	(0.060)	(0.062)
Unmarried	0.012	-0.007	-0.109	0.133*
	(0.069)	(0.072)	(0.069)	(0.071)
Education Attainment				
(Ref: *No qualification*)				
Other qualification	0.301***	0.055	0.016	0.036
	(0.077)	(0.085)	(0.075)	(0.083)
GCSE; A-level; etc	0.454***	0.158**	0.180***	0.118
	(0.065)	(0.072)	(0.064)	(0.072)
Degree and above	0.922***	0.120*	0.342***	0.378***
	(0.068)	(0.073)	(0.065)	(0.072)
Ethnicity				
(Ref: *White*)				
Non-white	-0.269***	0.030	-0.190***	-0.266***
	(0.074)	(0.081)	(0.073)	(0.076)
Number employed in the hh	0.153***	0.097***	-0.211***	0.025
	(0.025)	(0.028)	(0.023)	(0.024)
Region dummies	Yes	Yes	Yes	Yes

Table A.6 Above median income on health and wellbeing

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Above median income quartile	0.073***	0.036***	0.041***	0.049***
	(0.011)	(0.011)	(0.010)	(0.010)
Age	-0.117**	-0.100**	-0.384***	-0.273***
	(0.050)	(0.051)	(0.049)	(0.053)
Age-squared	0.002*	0.002**	0.007***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Age-cubed	-0.000	-0.000**	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Gender				
(Ref: *Male*)				
female	-0.044	-0.343***	-0.030	0.052
	(0.051)	(0.056)	(0.050)	(0.052)
Marital status				
(Ref: *Never married*)				
Married or Cohabiting	0.227**	0.240**	-0.053	0.578***
	(0.099)	(0.102)	(0.086)	(0.098)
Unmarried	0.105	0.050	-0.157	0.195*
	(0.110)	(0.111)	(0.104)	(0.110)
Education Attainment				
(Ref: *No qualification*)				
Other qualification	0.176	0.029	-0.053	-0.011
	(0.127)	(0.137)	(0.124)	(0.134)
GCSE; A-level; etc	0.329***	0.066	0.141	0.077
	(0.109)	(0.117)	(0.106)	(0.117)
Degree and above	0.894***	0.093	0.287***	0.364***
	(0.113)	(0.118)	(0.106)	(0.117)
Ethnicity				
(Ref: *White*)				
Non-white	-0.164	-0.092	-0.256**	-0.331***
	(0.102)	(0.121)	(0.106)	(0.110)
Number employed in the hh	0.153***	0.144***	-0.181***	0.038
	(0.035)	(0.039)	(0.034)	(0.039)
Region dummies	Yes	Yes	Yes	Yes

Table A.7 Income gradients and health & well-being outcomes - gender disaggregation

	SRH	GHQ-12	Leisure satisfaction	Life satisfaction
Panel I: **Male**				
Household disposable income (log)	0.038	0.146***	-0.077***	0.106***
	(0.024)	(0.030)	(0.024)	(0.025)
Income stability	0.343***	0.182***	0.226***	0.324***
	(0.029)	(0.034)	(0.029)	(0.027)
Income volatility	-0.038	-0.046*	-0.068***	-0.087***
	(0.025)	(0.027)	(0.023)	(0.024)
Below median income	-0.090***	-0.062***	-0.059***	-0.065***
	(0.010)	(0.010)	(0.009)	(0.010)
Above median income	0.090***	0.062***	0.059***	0.065***
	(0.010)	(0.010)	(0.009)	(0.010)
Panel II: **Female**				
Household disposable income (log)	0.077***	0.156***	-0.059***	0.101***
	(0.023)	(0.024)	(0.022)	(0.022)
Income stability	0.227***	0.070**	0.176***	0.235***
	(0.025)	(0.027)	(0.025)	(0.027)
Income volatility	0.019	-0.067***	-0.003	-0.082***
	(0.020)	(0.022)	(0.021)	(0.022)
Below median income	-0.045***	-0.006	-0.036***	-0.037***
	(0.009)	(0.009)	(0.008)	(0.009)
Above median income	0.045***	0.006	0.036***	0.037***
	(0.009)	(0.009)	(0.008)	(0.009)

Note: ***, ** and * denotes statistical significance at 1%, 5% and 10% levels respectively. Standard errors are presented in parentheses. Household disposable income (log) is the log of the equivalised and inflation-adjusted after-tax household income SRH denotes self-rated health, while GHQ-12 is the mental health indicator. All models are estimated using the ordered logit model with the corresponding covariates considered in each estimation.

Table A.8 Attrition probit test

	2011 - 2016	2011 - 2019
General health	0.035	-0.031
	(0.042)	(0.022)
Mental health	0.006	0.005
	(0.015)	(0.008)
Life satisfaction	0.014	0.003
	(0.032)	(0.017)
Leisure satisfaction	-0.025	-0.021
	(0.026)	(0.014)
Income group (Ref: *lowest quintile*)		
Second quintile	-0.003	0.107
	(0.144)	(0.077)
Third quintile	0.026	0.117
	(0.146)	(0.077)
Fourth quintile	-0.088	0.143*
	(0.147)	(0.078)
Highest quintile	-0.079	0.218***
	(0.154)	(0.082)
Age	-0.013***	-0.026***
	(0.004)	(0.002)
Marital status (Ref: *Never married*)		
Married or Cohabiting	0.014	-0.007
	(0.114)	(0.058)
Unmarried	-0.065	0.067
	(0.169)	(0.084)
Education (Ref: *No qualification*)		
Other qualification	0.219	-0.169
	(0.219)	(0.130)
GCSE; A-level; etc	0.128	-0.191*
	(0.188)	(0.108)
Degree and other higher degrees	0.137	-0.315***
	(0.190)	(0.108)
Ethnicity (Ref: *White*)		
Non-white	0.094	0.108*
	(0.114)	(0.065)
Sex (Ref: *male*)		
Female	0.115*	-0.003
	(0.068)	(0.032)
Job term (Ref: *permanent job*)		
Non- permanent	-0.225*	0.072
	(0.134)	(0.070)
Job type (Ref: *employee*)		
Self-employed	0.133	0.056
	(0.109)	(0.056)
Long-term illness or disability (Ref: *No*)		
Yes	-0.086	-0.300***
	(0.166)	(0.098)
Employment status		
Not employed	-0.161*	-0.017
	(0.095)	(0.047)
Number of children in HH	-0.024	-0.061***
	(0.040)	(0.024)
Region of residence (Ref: *North East*)		
North West	0.065	-0.064
	(0.305)	(0.134)
Yorkshire and the Humber	0.248	0.144
	(0.303)	(0.132)
East Midlands	0.307	-0.007
	(0.303)	(0.138)
West Midlands	0.151	0.036
	(0.307)	(0.136)
East of England	0.470	-0.018
	(0.297)	(0.130)
London	0.356	0.006
	(0.295)	(0.137)
South East	0.002	-0.034
	(0.293)	(0.130)
South West	0.040	-0.133
	(0.301)	(0.138)
Wales	0.174	0.221
	(0.306)	(0.156)
Scotland	0.335	0.142
	(0.297)	(0.140)
Northern Ireland	0.646**	0.000
	(0.299)	(.)
Constant	-0.377	1.238***
	(0.430)	(0.228)
Pseudo R^2	0.036	0.059

Appendix B

Chapter 3 Appendix

Table B.1 NLW policy effects on mental health

	Outcome: MCS	**2016**	**2017**	**2018**	**2019**
Panel A	*Without covariates*	0.2560	0.4548	0.7402	1.2251**
		(0.6113)	(0.5234)	(0.5132)	(0.4942)
Panel B	*With covariates*	03090	0.7549	0.8985	1.4134**
		(0.7050)	(0.6547)	(05569)	(0.5476)

Note: The Table summarises the group-time average treatment effect of receiving the national living wage by the length of time that the policy has been in place. The parameters are estimated under the conditional and unconditional parallel trends assumptions. Similar to the cohort effects summarised in Table 3.2, the estimated results across time also show that the cumulative effects of the NLW policy on mental health are positive.

Table B.2 NLW and welfare benefits freeze

	Outcome: MCS	2016	2017	2018	2019
Panel A	*Receiving benefits*	0.3663	-0.4927	-0.3200	0.8542
		(0.9972)	(0.7674)	(0.8508)	(0.7622)
Panel B	*Not receiving benefits*	1.3841	1.4161	0.9761	2.1113**
		(0.6937)	(0.6704)	(0.6330)	(0.6397)

Note: The Table summarises the group-time average treatment effect of receiving the national living wage by period aggregation. Panel A denotes individuals affected by the 2016 welfare benefits freeze, and those not receiving the affected welfare benefits are summarised in Panel B. All the parameters are estimated under the unconditional parallel trend assumptions i.e. without including the covariates

Table B.3 Treatment effects estimates of the NLW policy (Not yet treated group)

Outcome: MCS	2016	2017	2018	2019	Weighted average	P-value
Full sample	0.7372	1.5086**	0.6279	1.2549	0.9853**	0.0586
	(0.4414)	(0.5469)	(0.7097)	(0.6200)	(0.3578)	
Receiving benefits	0.2841	1.8718	-1.5479	0.7464	0.4658	0.2983
	(0.6889)	(0.7637)	(1.1837)	(1.0459)	(0.5197)	
Not receiving benefits	1.2049	1.1536	2.0195	0.5929	1.1975**	0.2991
	(0.5475)	(0.7160)	(0.8749)	(0.7074)	(0.3825)	

Note: The Table summarises the group-time average treatment effect parameters using the "not-yet-treated" as the comparison grop. The ‘weighted average’ column reports the weighted average treatment effects across all treatment cohorts. The average treatment effects for each treated cohort are summarised in each column. 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 at the 0.05 level in a 2-tailed test. The p-value column denotes the probability values for the Wald test of parallel trend assumption as reported by the ‘att_gt‘ function from the ‘did’ package. The estimates are obtained using the doubly robust estimator *(dripw)* with the standard errors clustered at the primary sampling unit level.

Appendix C

Chapter 4 Appendix

Matching results

Table C.1 Covariates imbalance test pre- and post-matching using PSM

	2016		2017		2018		2019	
Matching covariates	**Before**	**After**	**Before**	**After**	**Before**	**After**	**Before**	**After**
Age	13.72	-0.85	9.97	-0.86	7.76	-0.42	4.63	-0.5
	[0.0000]	[0.3950]	[0.0000]	[0.3910]	[0.0000]	[0.6720]	[0.0000]	[0.6160]
Number of children	-5.09	0.55	-2.88	0.4	-1.39	-0.31	-1.52	0.61
	[0.0000]	[0.5820]	[0.0040]	[0.6910]	[0.1630]	[0.7560]	[0.1290]	[0.5400]
Female	6.01	0.81	3.07	0.33	3.98	-0.25	3.02	0.2
	[0.0000]	[0.4190]	[0.0020]	[0.7410]	[0.0000]	[0.8060]	[0.0030]	[0.8430]
Married	7.28	-1.09	5.16	-0.93	6.26	-0.5	5.73	-0.67
	[0.0000]	[0.2770]	[0.0000]	[0.3550]	[0.0000]	[0.6200]	[0.0000]	[0.5000]
Degree	-1.32	0.8	-2.39	-0.94	1.22	-0.43	1.26	-0.33
	[0.1870]	[0.4230]	[0.0170]	[0.3480]	[0.2240]	[0.6690]	[0.2070]	[0.7420]
Job type (part-time)	4.25	0.43	3.23	0.3	1.71	0.83	0.05	1.08
	[0.0000]	[0.6690]	[0.0010]	[0.7650]	[0.0880]	[0.4080]	[0.9630]	[0.2820]
Self-rated health (fair or poor)	5.83	1.73	5.52	1.14	5.17	1.66	1.13	1.69
	[0.0000]	[0.0830]	[0.0000]	[0.2550]	[0.0000]	[0.0970]	[0.2570]	[0.0910]
Longstanding illness or disability	8.45	0.7	5.94	0.18	7.46	-0.2	4.48	1.01
	[0.0000]	[0.4860]	[0.0000]	[0.8570]	[0.0000]	[0.8440]	[0.0000]	[0.3140]
Father alive	-3.84	0.8	-0.36	0.24	-0.09	0.48	-0.31	1.49
	[0.0000]	[0.4250]	[0.7160]	[0.8100]	[0.9310]	[0.6350]	[0.7540]	[0.1350]
Mother alive	2.71	0.36	1.36	0.06	3.7	0.76	2.3	1.45
	[0.0070]	[0.7170]	[0.1740]	[0.9550]	[0.0000]	[0.4500]	[0.0210]	[0.1480]
SF-12 MCS	-1.47	-2.8	0.115	-0.65	-2.26	-1.46	-1.65	-1.15
	[0.1420]	[0.0050]	[1.1500]	[0.5170]	[0.0240]	[0.1460]	[0.0990]	[0.2500]
SF-12 PCS	-9.01	-2.76	0.00	-2.02	-5.28	-1.66	-0.55	-0.73
	[0.0000]	[0.0060]	[1.1700]	[0.0430]	[0.0000]	[0.0970]	[0.5860]	[0.4650]
Work hours	-2.74	0.375	0.042	0.159	-1.96	-0.43	0.64	-1.27
	[0.0060]	[1.0500]	[1.0800]	[0.8900]	[0.0500]	[0.6660]	[0.5240]	[0.2040]

Note: The table reports the pairwise t-tests before and after the propensity matching. Before and After respectively indicate before and after the propensity score matching. The p-values for the differences between the treatment and control groups are reported in square brackets and indicate that at the 5% significance level, there are no statistically significant differences in the observed covariates between the treatment and comparison groups.

Coarsened Exact Matching Results

2016

Multivariate L1 distance: .66309703

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.20043	5.2086	0	7	7	4	-8
kids	.03963	-.03963	0	0	0	0	0
female	.09295	.09295	0	0	1	0	0
married	.03766	.03766	0	0	0	0	0
degree	.0275	-.0275	0	0	0	0	0
part_time	.0657	.0657	0	0	0	0	0
fair	.04219	.04219	0	0	0	0	0
longhealth	.07182	.07182	0	0	0	1	0
falive	.03207	-.03207	0	0	0	0	0
malive	.0472	.0472	0	0	0	0	0

(using the scott break method for imbalance)

Matching Summary:

Number of strata: 4292

Number of matched strata: 936

	0	1
All	26600	2116
Matched	17571	1916
Unmatched	9029	200

Multivariate L1 distance: .36474301

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.04414	.05758	0	0	0	0	1
kids	7.0e-15	8.8e-15	0	0	0	0	0
female	1.5e-14	1.5e-14	0	0	0	0	0
married	5.5e-15	-3.8e-15	0	0	0	0	.
degree	1.0e-14	1.4e-14	0	0	0	0	.
part_time	1.7e-14	4.0e-15	0	0	.	.	.
fair	5.4e-15	6.6e-15	0	0	0	0	.
longhealth	1.0e-14	1.5e-14	0	0	0	0	0
falive	4.1e-15	5.1e-15	0	0	0	0	.
malive	4.8e-15	8.2e-15	0	0	0	0	.

```
2016             Unmatched |       Mean               %reduct |     t-test    |  V(T)/
Variable           Matched | Treated Control    %bias  |bias| |    t    p>|t| |  V(C)
--------------------------+----------------------------------+---------------+----------
dvage                   U  | 52.636   46.681     32.4         |  13.72  0.000 |  0.81*
                        M  | 52.636   52.876     -1.3    96.0 |  -0.60  0.552 |  0.98
kids                    U  | .28261   .33677    -11.7         |  -5.09  0.000 |     .
                        M  | .28261   .26931      2.9    75.4 |   1.30  0.193 |     .
female                  U  | .57798   .51015     13.6         |   6.01  0.000 |     .
                        M  | .57798   .57568      0.5    96.6 |   0.20  0.840 |     .
married                 U  | .68184   .60143     16.8         |   7.28  0.000 |     .
                        M  | .68184   .69713     -3.2    81.0 |  -1.44  0.149 |     .
degree                  U  | .36143   .37591     -3.0         |  -1.32  0.187 |     .
                        M  | .36143    .3499      2.4    20.4 |   1.05  0.295 |     .
part_time               U  | .31151   .25272     13.1         |   4.25  0.000 |     .
                        M  | .31151   .29171      4.4    66.3 |   1.34  0.180 |     .
fair                    U  |  .2328   .18044     13.0         |   5.83  0.000 |     .
                        M  |  .2328   .22053      3.0    76.6 |   1.25  0.211 |     .
longhealth              U  | .39905   .31028     18.6         |   8.45  0.000 |     .
                        M  | .39905   .38727      2.5    86.7 |   1.05  0.294 |     .
falive                  U  | .18006   .21607     -9.0         |  -3.84  0.000 |     .
                        M  | .18006   .16462      3.9    57.1 |   1.78  0.075 |     .
malive                  U  | .27106   .24432      6.1         |   2.71  0.007 |     .
                        M  | .27106   .24614      5.7     6.8 |   2.48  0.013 |     .
mcs                     U  | 49.085   49.435     -3.5         |  -1.47  0.142 |  1.05
                        M  | 49.085   49.879     -7.9  -126.8 |  -3.27  0.001 |  1.05
pcs                     U  | 47.899   50.259    -20.7         |  -9.01  0.000 |  1.19*
                        M  | 47.899   48.545     -5.7    72.6 |  -2.27  0.023 |  1.03
wrkhrs                  U  | 32.087    33.11     -9.0         |  -2.74  0.006 |  1.12*
                        M  | 32.087    32.31     -2.0    78.2 |  -0.58  0.562 |  1.11*
-----------------------------------------------------------------------------------------
* if variance ratio outside [0.92; 1.09] for U and [0.92; 1.09] for M
------------------------------------------------------------------------------------
 Sample     | Ps R2   LR chi2   p>chi2   MeanBias   MedBias      B      R     %Var
-----------+------------------------------------------------------------------------
 Unmatched | 0.035    221.73    0.000      13.1      13.0      53.1*   0.99     75
 Matched   | 0.004     23.55    0.036       3.5       3.0      17.2    1.16     25
------------------------------------------------------------------------------------
* if B>25%, R outside [0.5; 2]
```

2017

Multivariate L1 distance: .71167181

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.22163	6.0278	0	9	7	5	-3
kids	.03422	-.03422	0	0	0	0	0
female	.04966	.04966	0	0	1	0	0
married	.0435	.0435	0	0	0	0	0
degree	.04549	-.04549	0	0	0	0	0
part_time	.05315	.05315	0	0	0	0	0
fair	.07087	.07087	0	0	0	0	0
longhealth	.0991	.0991	0	0	0	1	0
falive	.00823	-.00823	0	0	0	0	0
malive	.02077	.02077	0	0	0	0	0

(using the scott break method for imbalance)

Matching Summary:

Number of strata: 4416

Number of matched strata: 706

	0	1
All	26326	1433
Matched	15376	1326
Unmatched	10950	107

Multivariate L1 distance: .37186146

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.04743	.07313	0	0	1	0	0
kids	1.9e-14	-1.7e-14	0	0	0	0	0
female	2.7e-14	-2.5e-14	0	0	0	0	0
married	2.0e-14	-1.9e-14	0	0	0	0	0
degree	1.8e-14	-1.9e-14	0	0	0	0	.
part_time	2.9e-14	-8.0e-15	0	0	.	.	.
fair	9.3e-15	-1.1e-14	0	0	0	0	.
longhealth	1.9e-14	-1.8e-14	0	0	0	0	0
falive	1.1e-14	-6.4e-15	0	0	0	0	.
malive	1.2e-14	-9.3e-15	0	0	0	0	.

2017 Variable	Unmatched Matched	Mean Treated	Control	%bias	%reduct |bias|	t-test t	p>|t|	V(T)/ V(C)
dvage	U	52.221	46.596	30.6		10.82	0.000	0.83*
	M	52.221	52.449	-1.2	95.9	-0.47	0.638	0.99
kids	U	.28542	.34016	-11.8		-4.27	0.000	.
	M	.28542	.27376	2.5	78.7	0.95	0.344	.
female	U	.54571	.51734	5.7		2.09	0.036	.
	M	.54571	.54449	0.2	95.7	0.09	0.930	.
married	U	.65852	.60168	11.8		4.28	0.000	.
	M	.65852	.66893	-2.2	81.7	-0.80	0.424	.
degree	U	.34373	.39123	-9.9		-3.56	0.000	.
	M	.34373	.33486	1.8	81.3	0.68	0.500	.
part_time	U	.33618	.25114	18.7		5.05	0.000	.
	M	.33618	.32093	3.4	82.1	0.83	0.408	.
fair	U	.24441	.18449	14.6		5.57	0.000	.
	M	.24441	.22496	4.8	67.5	1.66	0.098	.
longhealth	U	.38295	.30842	15.7		5.93	0.000	.
	M	.38295	.37707	1.2	92.1	0.44	0.661	.
falive	U	.1331	.18351	-13.8		-4.78	0.000	.
	M	.1331	.11665	4.5	67.4	1.83	0.068	.
malive	U	.2	.2101	-2.5		-0.91	0.365	.
	M	.2	.17652	5.8	-132.5	2.20	0.028	.
mcs	U	48.611	49.028	-3.9		-1.43	0.153	1.18*
	M	48.611	49.324	-6.6	-71.1	-2.37	0.018	1.15*
pcs	U	47.973	50.145	-19.5		-7.12	0.000	1.12*
	M	47.973	48.472	-4.5	77.0	-1.50	0.134	0.96
wrkhrs	U	31.587	32.966	-12.4		-3.01	0.003	1.03
	M	31.587	31.588	-0.0	99.9	-0.00	0.998	0.99

* if variance ratio outside [0.90; 1.11] for U and [0.90; 1.11] for M

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.035	165.31	0.000	13.2	12.4	55.5*	1.07	75
Matched	0.002	6.58	0.922	3.0	2.5	11.1	1.11	25

* if B>25%, R outside [0.5; 2]

2018

Multivariate L1 distance: .71073914

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.22928	6.1102	0	10	7	5	-5
kids	.00983	-.00983	0	0	0	0	0
female	.03982	.03982	0	0	1	0	0
married	.0713	.0713	0	0	0	0	0
degree	.0025	-.0025	0	0	0	0	0
part_time	.02693	.02693	0	0	0	0	0
fair	.06221	.06221	0	0	0	0	0
longhealth	.11292	.11292	0	0	0	1	0
falive	.02439	-.02439	0	0	0	0	0
malive	.03414	.03414	0	0	0	0	0

(using the scott break method for imbalance)

Matching Summary:

Number of strata: 4063

Number of matched strata: 629

	0	1
All	23784	1133
Matched	13073	1019
Unmatched	10711	114

Multivariate L1 distance: .34447423

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.0644	.09666	0	0	0	1	-2
kids	3.5e-15	4.3e-15	0	0	0	0	0
female	9.4e-15	1.3e-14	0	0	0	0	0
married	9.2e-15	1.3e-14	0	0	0	0	.
degree	8.5e-15	5.5e-15	0	0	0	0	.
part_time	9.2e-15	2.7e-15	0	0	0	.	.
fair	6.8e-15	3.3e-15	0	0	0	0	.
longhealth	8.2e-15	7.5e-15	0	0	0	0	0
falive	4.8e-15	4.1e-15	0	0	0	0	.
malive	6.8e-15	4.4e-15	0	0	0	0	.

2018 Variable	Unmatched Matched	Mean Treated	Mean Control	%bias	%reduct |bias|	t-test t	t-test p>|t|	V(T)/ V(C)
dvage	U	51.809	47.01	26.0		8.24	0.000	0.85*
	M	51.809	52.244	-2.4	90.9	-0.79	0.427	1.00
kids	U	.30362	.33787	-7.3		-2.38	0.017	.
	M	.30362	.28656	3.7	50.2	1.22	0.224	.
female	U	.56752	.52085	9.4		3.07	0.002	.
	M	.56752	.55348	2.8	69.9	0.91	0.362	.
married	U	.68085	.60665	15.5		4.99	0.000	.
	M	.68085	.69125	-2.2	86.0	-0.72	0.469	.
degree	U	.39727	.40424	-1.4		-0.46	0.645	.
	M	.39727	.38577	2.3	-65.1	0.75	0.452	.
part_time	U	.28498	.2495	8.0		1.94	0.052	.
	M	.28498	.25752	6.2	22.6	1.46	0.146	.
fair	U	.2693	.18755	19.6		6.70	0.000	.
	M	.2693	.24698	5.3	72.7	1.64	0.102	.
longhealth	U	.42958	.31686	23.5		7.93	0.000	.
	M	.42958	.41708	2.6	88.9	0.82	0.414	.
falive	U	.18677	.19516	-2.1		-0.69	0.489	.
	M	.18677	.17129	3.9	-84.7	1.32	0.188	.
malive	U	.25559	.22306	7.6		2.55	0.011	.
	M	.25559	.23465	4.9	35.7	1.58	0.114	.
mcs	U	47.736	48.847	-10.3		-3.37	0.001	1.18*
	M	47.736	49.062	-12.3	-19.3	-3.92	0.000	1.16*
pcs	U	47.882	50.284	-21.6		-7.07	0.000	1.17*
	M	47.882	48.066	-1.7	92.3	-0.49	0.624	0.98
wrkhrs	U	31.652	32.901	-11.2		-2.48	0.013	1.04
	M	31.652	33.111	-13.1	-16.8	-2.94	0.003	1.13*

* if variance ratio outside [0.89; 1.12] for U and [0.89; 1.12] for M

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.030	118.56	0.000	12.6	10.3	53.0*	0.90	75
Matched	0.008	25.05	0.023	4.9	3.7	23.5	1.29	50

* if B>25%, R outside [0.5; 2]

2019

Multivariate L1 distance: .74574817

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.18182	4.0627	1	6	5	2	-14
kids	.04229	-.04229	0	0	0	0	0
female	.05458	.05458	0	0	1	0	0
married	.06076	.06076	0	0	0	0	0
degree	.02963	-.02963	0	0	0	0	0
part_time	.00267	.00267	0	0	0	0	0
fair	.01454	.01454	0	0	0	0	0
longhealth	.10742	.10742	0	0	0	1	0
falive	.02631	-.02631	0	0	0	0	0
malive	.01805	.01805	0	0	0	0	0

(using the scott break method for imbalance)

Matching Summary:

Number of strata: 3909

Number of matched strata: 491

	0	1
All	22026	876
Matched	11245	784
Unmatched	10781	92

Multivariate L1 distance: .38887372

Univariate imbalance:

	L1	mean	min	25%	50%	75%	max
dvage	.04375	-.00064	0	0	0	0	-3
kids	1.0e-14	2.2e-15	0	0	0	0	0
female	6.4e-15	1.1e-14	0	0	0	0	0
married	9.1e-15	1.4e-14	0	0	0	0	0
degree	4.8e-15	4.4e-16	0	0	0	0	.
part_time	5.8e-15	1.4e-15	0	0	0	.	.
fair	1.0e-14	2.4e-15	0	0	0	0	.
longhealth	6.9e-15	4.2e-15	0	0	0	0	0
falive	9.4e-15	2.1e-15	0	0	0	0	.
malive	1.2e-14	2.7e-15	0	0	0	0	.

2019 Variable	Unmatched Matched	Mean Treated	Control	%bias	%reduct |bias|	t-test t	p>|t|	V(T)/ V(C)
dvage	U	51.666	47.565	22.2		6.19	0.000	0.84*
	M	51.666	51.781	-0.6	97.2	-0.18	0.854	0.98
kids	U	.27968	.32798	-10.5		-2.99	0.003	.
	M	.27968	.25255	5.9	43.8	1.78	0.076	.
female	U	.57192	.5232	9.8		2.83	0.005	.
	M	.57192	.58036	-1.7	82.7	-0.49	0.626	.
married	U	.66208	.61433	9.9		2.85	0.004	.
	M	.66208	.66582	-0.8	92.2	-0.23	0.822	.
degree	U	.40164	.41281	-2.3		-0.65	0.515	.
	M	.40164	.40722	-1.1	50.1	-0.32	0.749	.
part_time	U	.27371	.25163	5.0		1.08	0.282	.
	M	.27371	.24638	6.2	-23.8	1.31	0.189	.
fair	U	.2242	.18924	8.6		2.53	0.011	.
	M	.2242	.19582	7.0	18.8	1.99	0.046	.
longhealth	U	.39886	.32204	16.0		4.76	0.000	.
	M	.39886	.3852	2.9	82.2	0.80	0.424	.
falive	U	.17517	.17731	-0.6		-0.16	0.872	.
	M	.17517	.16324	3.1	-459.5	0.91	0.362	.
malive	U	.24594	.20223	10.5		3.12	0.002	.
	M	.24594	.21979	6.3	40.2	1.78	0.075	.
mcs	U	47.657	48.363	-6.7		-1.88	0.060	1.02
	M	47.657	48.612	-9.0	-35.2	-2.50	0.012	1.02
pcs	U	48.885	50.32	-13.0		-3.81	0.000	1.20*
	M	48.885	49.166	-2.6	80.4	-0.69	0.490	1.06
wrkhrs	U	32.943	32.94	0.0		0.01	0.996	1.03
	M	32.943	33.282	-3.0	-11632.2	-0.63	0.528	1.17*

* if variance ratio outside [0.88; 1.14] for U and [0.88; 1.14] for M

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.030	101.64	0.000	8.9	9.8	52.2*	1.02	50
Matched	0.004	11.79	0.545	3.9	3.0	17.8	1.14	25

* if B>25%, R outside [0.5; 2]

* if B>25%, R outside [0.5; 2]

Propensity Scores Matching Results

2016 Variable	Unmatched Matched	Mean Treated	Mean Control	%bias	%reduct |bias|	t-test t	t-test p>|t|	V(T)/V(C)
dvage	U	52.636	46.681	32.4		13.72	0.000	0.81*
	M	45.068	45.353	-1.6	95.2	-0.48	0.634	0.94
kids	U	.50709	.59632	-9.3		-4.05	0.000	0.92
	M	.66974	.61105	6.1	34.2	1.29	0.198	1.20*
female	U	.57798	.51015	13.6		6.01	0.000	.
	M	.61105	.57998	6.3	54.2	1.32	0.187	.
married	U	.68184	.60143	16.8		7.28	0.000	.
	M	.70886	.69735	2.4	85.7	0.52	0.600	.
degree	U	.36143	.37591	-3.0		-1.32	0.187	.
	M	.45109	.42002	6.4	-114.5	1.31	0.192	.
part_time	U	.31151	.25272	13.1		4.25	0.000	.
	M	.30495	.29574	2.0	84.3	0.42	0.676	.
fair	U	.2328	.18044	13.0		5.83	0.000	.
	M	.13809	.1473	-2.3	82.4	-0.55	0.583	.
longhealth	U	.39905	.31028	18.6		8.45	0.000	.
	M	.27848	.26237	3.4	81.9	0.76	0.450	.
falive	U	.18006	.21607	-9.0		-3.84	0.000	.
	M	.28539	.28193	0.9	90.4	0.16	0.873	.
malive	U	.27106	.24432	6.1		2.71	0.007	.
	M	.40276	.40161	0.3	95.7	0.05	0.961	.
mcs	U	49.085	49.435	-3.5		-1.47	0.142	1.05
	M	48.985	49.115	-1.3	62.9	-0.28	0.782	0.94
pcs	U	47.899	50.259	-20.7		-9.01	0.000	1.19*
	M	51.52	51.259	2.3	88.9	0.58	0.559	0.98
wrkhrs	U	32.087	33.11	-9.0		-2.74	0.006	1.12*
	M	31.679	32.115	-3.8	57.3	-0.79	0.429	1.01

* if variance ratio outside [0.92; 1.09] for U and [0.88; 1.14] for M

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.035	222.29	0.000	12.9	13.0	53.2*	0.98	60
Matched	0.003	6.98	0.903	3.0	2.3	12.7	0.98	20

* if B>25%, R outside [0.5; 2]

2017 Variable	Unmatched Matched	Mean Treated	Mean Control	%bias	%reduct |bias|	t-test t	t-test p>|t|	V(T)/ V(C)
dvage	U	52.062	46.681	29.5		9.97	0.000	0.78*
	M	45.781	46.076	-1.6	94.5	-0.41	0.683	1.05
kids	U	.53668	.59632	-6.1		-2.17	0.030	1.00
	M	.6311	.56239	7.0	-15.2	1.26	0.207	1.14
female	U	.55314	.51015	8.6		3.07	0.002	.
	M	.58228	.60217	-4.0	53.7	-0.67	0.501	.
married	U	.67217	.60143	14.7		5.16	0.000	.
	M	.71429	.73237	-3.8	74.4	-0.67	0.502	.
degree	U	.34337	.37591	-6.8		-2.39	0.017	.
	M	.44665	.47378	-5.7	16.6	-0.90	0.366	.
part_time	U	.30769	.25272	12.3		3.23	0.001	.
	M	.29656	.27848	4.0	67.1	0.66	0.507	.
fair	U	.2413	.18044	15.0		5.52	0.000	.
	M	.15552	.16275	-1.8	88.1	-0.33	0.743	.
longhealth	U	.38756	.31028	16.3		5.94	0.000	.
	M	.30741	.2821	5.3	67.2	0.92	0.356	.
falive	U	.21182	.21607	-1.0		-0.36	0.716	.
	M	.30922	.30561	0.9	14.9	0.13	0.896	.
malive	U	.26094	.24432	3.8		1.36	0.174	.
	M	.38517	.37432	2.5	34.7	0.37	0.710	.
mcs	U	48.97	49.435	-4.5		-1.58	0.115	1.15*
	M	49.038	48.523	5.0	-10.8	0.85	0.395	0.85
pcs	U	48.209	50.259	-18.1		-6.37	0.000	1.17*
	M	51.825	52.397	-5.0	72.1	-1.06	0.291	1.14
wrkhrs	U	32.175	33.11	-8.3		-2.03	0.042	1.08
	M	31.563	32.001	-3.9	53.2	-0.66	0.512	0.92

* if variance ratio outside [0.90; 1.11] for U and [0.85; 1.18] for M

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.034	154.38	0.000	11.1	8.6	56.3*	0.84	60
Matched	0.005	8.32	0.822	3.9	4.0	17.3	1.03	0

* if B>25%, R outside [0.5; 2]

2018 Variable	Unmatched Matched	Mean Treated	Mean Control	%bias	%reduct |bias|	t-test t	t-test p>|t|	V(T)/ V(C)
dvage	U	51.432	46.681	26.2		7.76	0.000	0.75*
	M	46.045	46.532	-2.7	89.8	-0.60	0.549	1.03
kids	U	.54033	.59632	-5.8		-1.80	0.071	0.93
	M	.67647	.64932	2.8	51.5	0.44	0.662	1.08
female	U	.57337	.51015	12.7		3.98	0.000	.
	M	.56787	.52715	8.2	35.6	1.22	0.224	.
married	U	.69892	.60143	20.5		6.26	0.000	.
	M	.74434	.76923	-5.2	74.5	-0.86	0.389	.
degree	U	.39471	.37591	3.9		1.22	0.224	.
	M	.48416	.5	-3.3	15.8	-0.47	0.638	.
part_time	U	.2847	.25272	7.2		1.71	0.088	.
	M	.25792	.23982	4.1	43.4	0.62	0.534	.
fair	U	.24519	.18044	15.9		5.17	0.000	.
	M	.16063	.15611	1.1	93.0	0.18	0.854	.
longhealth	U	.42023	.31028	23.0		7.46	0.000	.
	M	.32353	.30995	2.8	87.7	0.43	0.665	.
falive	U	.21493	.21607	-0.3		-0.09	0.931	.
	M	.29412	.29638	-0.6	-98.3	-0.07	0.941	.
malive	U	.29552	.24432	11.5		3.70	0.000	.
	M	.37557	.38688	-2.6	77.9	-0.35	0.730	.
mcs	U	48.679	49.435	-7.4		-2.26	0.024	1.07
	M	49.157	49.189	-0.3	95.8	-0.05	0.959	1.05
pcs	U	48.327	50.259	-17.3		-5.28	0.000	1.10
	M	51.811	51.959	-1.3	92.3	-0.25	0.802	1.01
wrkhrs	U	32.117	33.11	-8.7		-1.96	0.050	1.15*
	M	32.248	33.414	-10.2	-17.5	-1.59	0.113	1.31*

* if variance ratio outside [0.88; 1.13] for U and [0.83; 1.21] for M

Sample	Ps R2	LR chi2	p>chi2	MeanBias	MedBias	B	R	%Var
Unmatched	0.034	130.26	0.000	12.3	11.5	56.9*	0.85	40
Matched	0.005	5.74	0.955	3.5	2.8	16.1	1.36	20

* if B>25%, R outside [0.5; 2]

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2019              Unmatched |       Mean               %reduct |     t-test    |  V(T)/
Variable            Matched | Treated Control    %bias  |bias| |    t    p>|t| |  V(C)
--------------------------+----------------------------------+---------------+----------
dvage                   U  |  49.85   46.681     17.6         |   4.63  0.000 |  0.73*
                        M  | 44.487   44.288      1.1    93.7 |   0.23  0.815 |  0.87
kids                    U  | .55515   .59632     -4.2         |  -1.18  0.237 |  1.02
                        M  | .60048   .70702    -10.8  -158.8 |  -1.63  0.104 |  0.79*
female                  U  | .56373   .51015     10.8         |   3.02  0.003 |      .
                        M  | .57143   .59564     -4.9    54.8 |  -0.71  0.481 |      .
married                 U  | .70098   .60143     21.0         |   5.73  0.000 |      .
                        M  | .71671   .72881     -2.6    87.8 |  -0.39  0.698 |      .
degree                  U  | .39777   .37591      4.5         |   1.26  0.207 |      .
                        M  | .45278   .43584      3.5    22.5 |   0.49  0.625 |      .
part_time               U  | .25367   .25272      0.2         |   0.05  0.963 |      .
                        M  | .24213   .25908     -3.9 -1690.4 |  -0.56  0.575 |      .
fair                    U  |  .1962   .18044      4.0         |   1.13  0.257 |      .
                        M  | .10654   .10896     -0.6    84.6 |  -0.11  0.911 |      .
longhealth              U  | .38405   .31028     15.5         |   4.48  0.000 |      .
                        M  | .31235   .30751      1.0    93.4 |   0.15  0.881 |      .
falive                  U  | .21144   .21607     -1.1         |  -0.31  0.754 |      .
                        M  | .28087   .27361      1.8   -57.1 |   0.23  0.816 |      .
malive                  U  | .27985   .24432      8.1         |   2.30  0.021 |      .
                        M  | .37288   .38257     -2.2    72.7 |  -0.29  0.774 |      .
mcs                     U  | 48.823   49.435     -6.1         |  -1.65  0.099 |  1.02
                        M  | 49.555   49.298      2.6    58.0 |   0.42  0.671 |  1.03
pcs                     U  | 50.039   50.259     -2.0         |  -0.55  0.586 |  0.99
                        M  | 52.602   52.243      3.3   -63.0 |   0.61  0.543 |  0.95
wrkhrs                  U  | 33.449    33.11      3.1         |   0.64  0.524 |  0.96
                        M  | 33.094   32.959      1.2    60.1 |   0.18  0.855 |  1.00
-------------------------------------------------------------------------------------------
* if variance ratio outside [0.87; 1.15] for U and [0.82; 1.21] for M

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 Sample     | Ps R2   LR chi2   p>chi2   MeanBias   MedBias      B      R     %Var
-----------+-----------------------------------------------------------------------
 Unmatched | 0.023     83.45    0.000      7.6       4.5      46.7*   0.94     20
 Matched   | 0.005      5.18    0.971      3.0       2.6      15.8    0.80     20
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* if B>25%, R outside [0.5; 2]
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Table C.2 Average treatment effects of the NLW (Non-informal carers)

	2016	**2017**	**2018**	**2019**	**Weighted average**	**P-value**
Work hours	0.8293	1.3224	2.0104	1.5975	1.2651**	0.5307
	(0.5492)	(0.7366)	(0.6952)	(0.7386)	(0.3548)	
Mental health	0.2478	0.2076	-0.5230	0.7402	0.1679	0.6440
	(0.5279)	(0.6792)	(0.5832)	(0.6136)	(0.3276)	
Physical health	-0.4892	-0.9660	-0.2098	-0.5298	-0.5396	0.5786
	(0.4599)	(0.5054)	(0.5793)	(0.4992)	(0.2790)	