Assessing the impacts of the reduction in the age of entitlement to the National Living Wage from age 25 to age 23

London Economics

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Executive Summary

London Economics were commissioned by the Low Pay Commission to **assess the impact of the reduction the age of entitlement to the National Living Wage (NLW) from the age of 25 to the age of 23 in April 2021.**

Since the introduction of the NLW in 2016, those aged 25 and older have been eligible for a higher minimum wage than those between the age of 21 and 24. In April 2021, the age of entitlement to the NLW was cut from 25 to 23, allowing 23- and 24-year-olds entitlement to a higher minimum wage. This resulted in an **8.7% increase in their minimum wage at the start of April 2021**. This increase was significantly higher than for other age groups: 21- and 22-year-olds experienced an increase in their minimum wage of 2% while those aged 25 and older experienced a 2.2% increase.

As part of the government's remit to the Low Pay Commission, the age of entitlement to the NLW will be reduced further from the age of 23 to the age of 21 by 2024 (Low Pay Commission, 2022b), so it is important to understand the impact of the most recent reduction in the age of the entitlement that took place in April 2021.

We investigate the impact on the labour market outcomes of those newly eligible for the NLW (23- and 24-year-olds), with the key metrics including **employment, hours worked, and hourly earnings**. We employ a difference-in-differences approach which compares changes in the trends experienced by 23- and 24-year-olds after April 2021 to the changes in trends experienced by those of similar ages who were not newly eligible for the NLW. We find that 22- and 25-year-olds follow different trends in employment and hours to 23- and 24-year-olds, which limits their suitability as a comparison group. As a result, we use 26-year-olds as a comparison group, a choice supported both by visual inspection of pre-treatment trends and by more formal testing.

We find no evidence of a negative impact of the reduction in the age of entitlement on the employment of 23- and 24-year-olds relative to 26-year-olds, with the estimated impact on **hours worked and hourly earnings being statistically insignificantly different from zero**. The insignificance of some results may be **driven in part by the inclusion of workers who are unlikely to be affected by the minimum wage**, such as those paid significantly higher than the minimum wage.

Focusing on subsets of the sample, we find a **negative and significant impact of the change in eligibility on hours worked for those working in low pay sectors and occupations**. The estimated decrease in average hours worked is 5.2% and 5.5% for those working in low pay sectors and occupations, respectively. This is larger than the estimated increase in hourly earnings for these groups. We find evidence suggesting that this decrease **is driven by an increase in the proportion of workers who are in part-time employment**, rather than a change in the hours worked by those in full-time employment. The negative impact on hours and an increase in the proportion of workers working part time is **greater for female workers**.

We investigate the impact of the reduction in the age of entitlement on other subsets of the sample. Across most subgroups we find a positive but insignificant impact on employment and hourly earnings, although it is difficult to make meaningful conclusions due to **small sample sizes**, while the **absence of a consistently significant impact on hourly earnings limit the causal interpretation of the findings using some subsets of the sample**.

However, it is not possible to determine whether this a result of those previously in full-time employment shifting to part-time employment or the result of additional workers who disproportionately enter part-time employment and bring down the average hours worked without significant impact on those previously in full-time employment. Further, part of the estimated impacts may be the result of changes experienced by 26-year-olds around the time of the reduction in the age of entitlement to the NLW in addition to any changes experienced by 23- and 24-year-olds as a result of the reduction.

We undertake a set of **robustness checks to validate the choice of treatment and control groups** and find that the **results are not sensitive to a range of different specifications** (such as the exclusion of the 2020-21 tax year which was heavily impacted by the COVID-19 pandemic).

1 Introduction and context

The age of entitlement to the National Living Wage (NLW) was reduced from the age of 25 to 23 in April 2021. Those aged 23 and 24 years old experienced a significant increase in their minimum wage of 8.7%, which was considerably greater than the relevant minimum wages of other age groups.

Investigating how this reduction in the age of entitlement to the NLW has influenced young people and their labour market outcomes provides an evidence base for future policy options relating to possible reductions in the age of entitlement, such as the potential reduction in the age of eligibility to 21 by 2024 (Low Pay Commission, 2022b)

Despite coinciding with significant economic events such as Brexit and the COVID-19 pandemic, the policy change also provides plausibly exogenous variation between age groups that can be used to evaluate the impact of minimum wage increases on labour market outcomes among young people.

We exploit the age discontinuity in the age of entitlement to compare changes in employment, pay, and hours worked, for those newly eligible to the NLW in April 2021 to individuals of similar ages unaffected by the policy change.

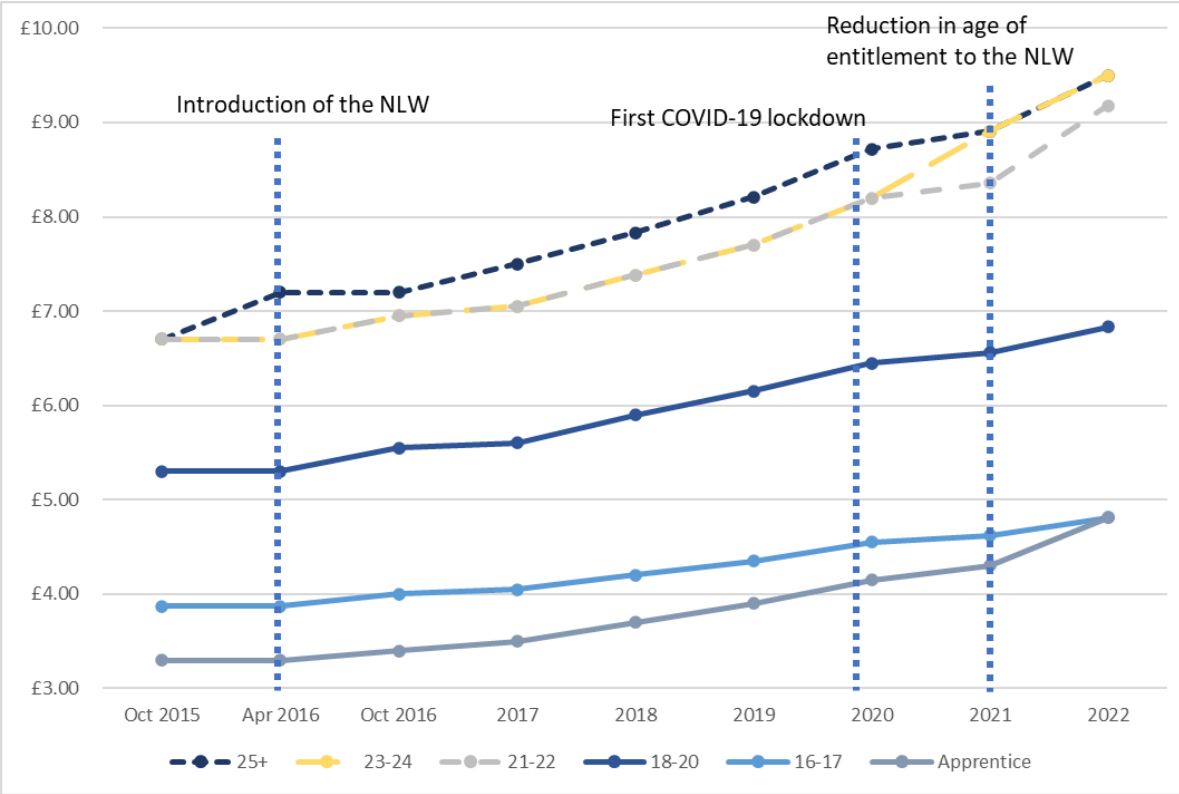
1.1 Recent changes to the National Minimum Wage and the National Living Wage

Since 2015 there has been two structural changes in the minimum wage regime. The first involved **the introduction of the NLW in April 2016 for those aged 25 and above**. In fact, before April 2016 all individuals aged 21 and above were eligible to the adult rate of the National Minimum Wage, which was set at £6.70 (since October 2015). In April 2016 those aged 25 and older became eligible to the NLW, which was set at £7.20 and corresponded to a step-change of 7.5% (£0.50) compared to the National Minimum Wage (NMW) rate they were previously eligible for. The NMW for those aged between 21 and 24 years old (inclusive) was initially left unchanged at £6.70 and then increased to £6.95 in October 2016. Since 2017, all minimum wage rates have been uprated in April of each year, and in the following four years (April 2017 to March 2021), the average gap between the NLW for those aged 25 and above and the minimum wage for 21 to 24-year-olds stood at 6.4%.

The second structural change was the reduction in the age of entitlement to the NLW from 25 to 23 years old in April 2021. This provided individuals aged 23 to 24 years old with the top (adult) rate of the minimum wage for the first time since the introduction of the NLW in April 2016.

Figure 1 illustrates the changes to the minimum wage across different groups (with some age bands have been split to accommodate changes in the age band structure over time). The figure shows that, after the initial change in 2016, the minimum wage rates for individuals above the age of 25 and 21-24 year olds were uprated at a similar pace, increasing on average by 5.2% per annum between April 2017 and April 2020. In comparison, the average change observed in the average weekly earnings over the same time period was 1.7% per annum, while the Consumer Price Index (CPI) increased by 1.8% per annum.

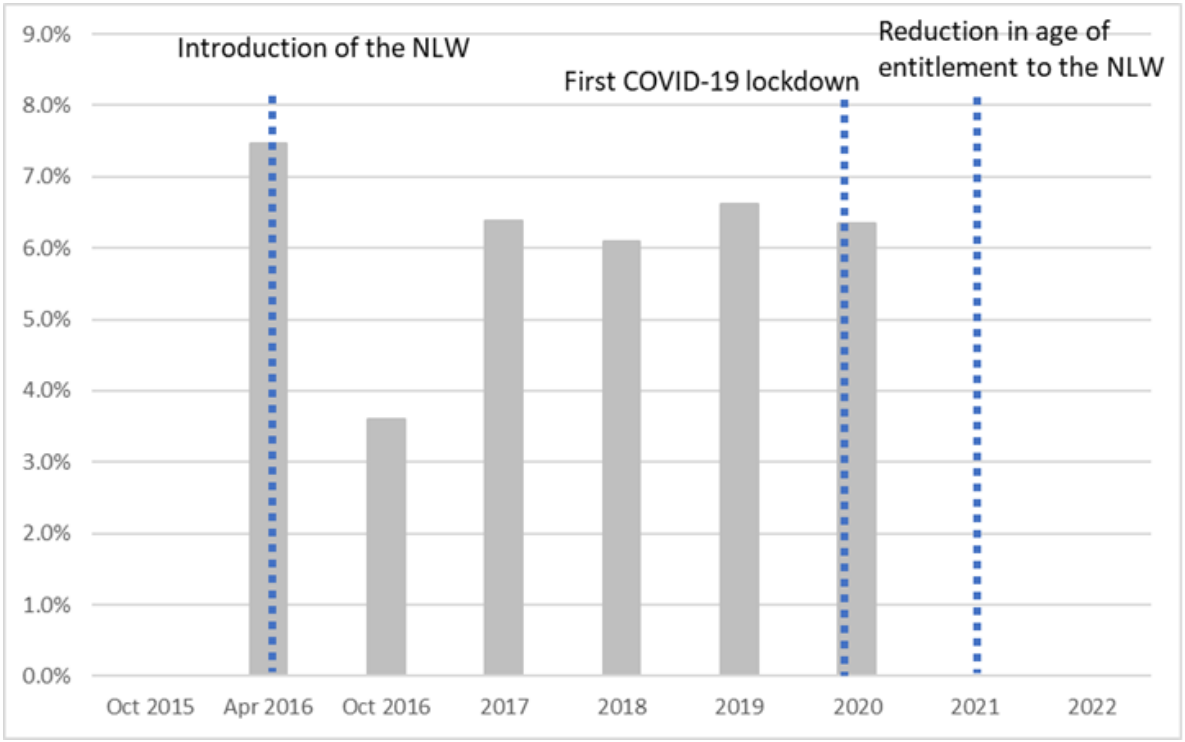
Figure 1 Evolution of minimum wages across age groups



Source: London Economics based on Low Pay Commission Reports (2015-2021)

Figure 2 shows the percentage gap between the minimum wage for 23- to 24-year-olds and the NLW from October 2015 to 2022. The minimum wage gap between 23- to 24-year-olds and those aged 25 and older was 7.5% in April 2016 when the NLW was introduced (although it declined to 3.6% in October 2016) and ranged between 6.1% and 6.6% from April 2017 up until the reduction in the age of entitlement in April 2021. The elimination of this gap for 23- and 24-year-olds in April 2021 was therefore a significant policy change in relative minimum wage rates across age groups. This was the first time that a change in the age eligibility of the relevant minimum wage had been implemented since the reduction in the adult rate of the NMW (from 22 to 21), and corresponding change in eligibility of the Youth Development rate (in October 2010).

Figure 2 Gap between the 23-24 rate and the NLW from October 2015 to April 2022



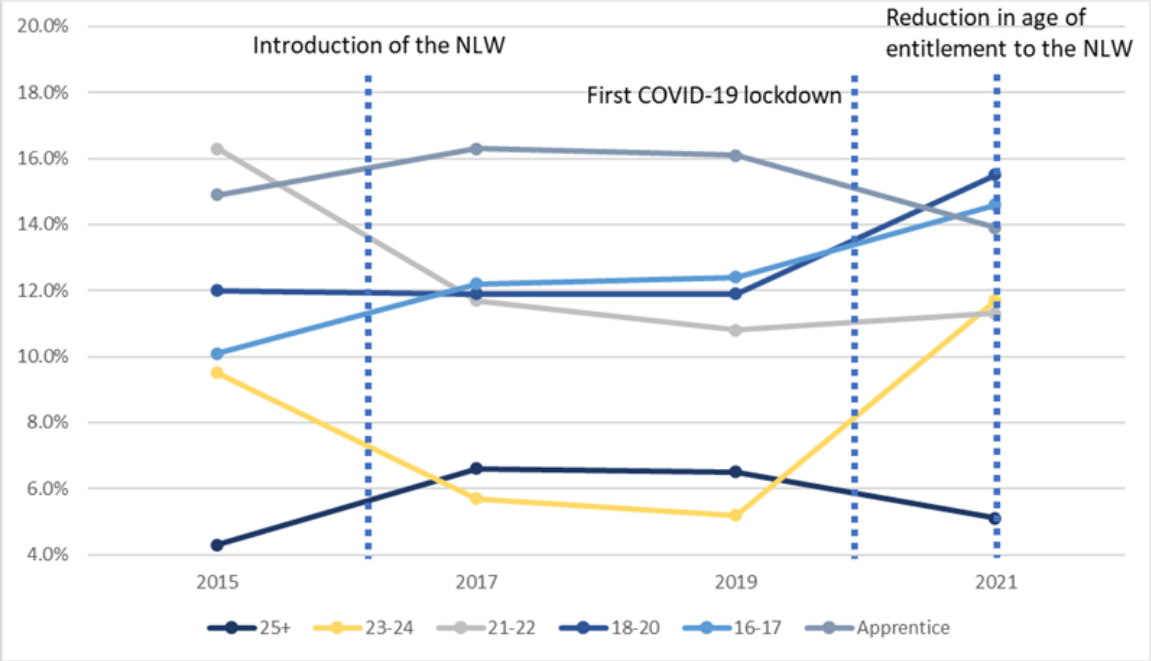
Source: London Economics based on Low Pay Commission Reports (2015-2021)

The reduction in the age of entitlement to the NLW closed this gap, and **raised the minimum wage for those aged 23 and 24 years old by 8.7% (from £8.20 to £8.91)**, which represents the largest rate of growth for 23-24 year olds since the introduction of the NLW. Without the policy change they would have been entitled to the 2% increase experienced by 21- and 22-year-olds (from £8.20 to £8.32). The growth rate for those aged 25 and above was 2.2%, very similar to the wage growth rate experienced by the youngest age group, so those aged 23 and 24 were the only group that experienced a substantial change in their rate around April 2021[1].

Further, as shown in Figure 3, the change in eligibility more than doubled the proportion of 23- and 24-year-olds covered by the minimum wage from 5.2% in 2019 to 11.7% in 2021. The policy change had a direct impact on a considerable proportion of 23- and 24-year-olds, even before considering potential spillover effects to other age groups and across the wage distribution.

[1] It should be noted that the group of 21- and 22-year-olds saw an increase of 9.8% in April 2022, compared to 6.6% for the older age group, meaning that the existing wage gap between the two rates stands at just 3.5% compared to between 6.1% and 6.6% previously. The ambition is that this gap will be completely removed by April 2024 when all individuals aged 21 and above will be eligible for the NLW (see link here).

Figure 3 Percentage of workers covered by the relevant minimum wage, by group



Source: Low Pay Commission Reports (2015-2021)

1.2 Employment and earnings trends among young people

The COVID-19 pandemic had a significant impact on young people and lower earners as a result of both the economic recession resulting from the pandemic, but also measures implemented by the government to limit unemployment (e.g., the Coronavirus Job Retention Scheme (CJRS) that ran from March 2020 to September 2021). The associated business closures and the support provided through the CJRS varied over time, peaking in June 2020, falling through the summer and increasing again in November 2020 and January 2021, and subsequently declining substantially after March 2021[2]. The support given and the timing of the support (and its withdrawal) varied significantly across different sectors of the economy and, as a consequence, affected differently young workers compared to older workers.

For example, the day before the reduction in the age of entitlement to the NLW (31st March 2021), 20% of workers between the ages of 18 and 24 were on furlough compared to 14% for those aged between 25 and 34[3]. Therefore, it is important to account for these events when estimating the impact of the reduction in the age of entitlement to the NLW.

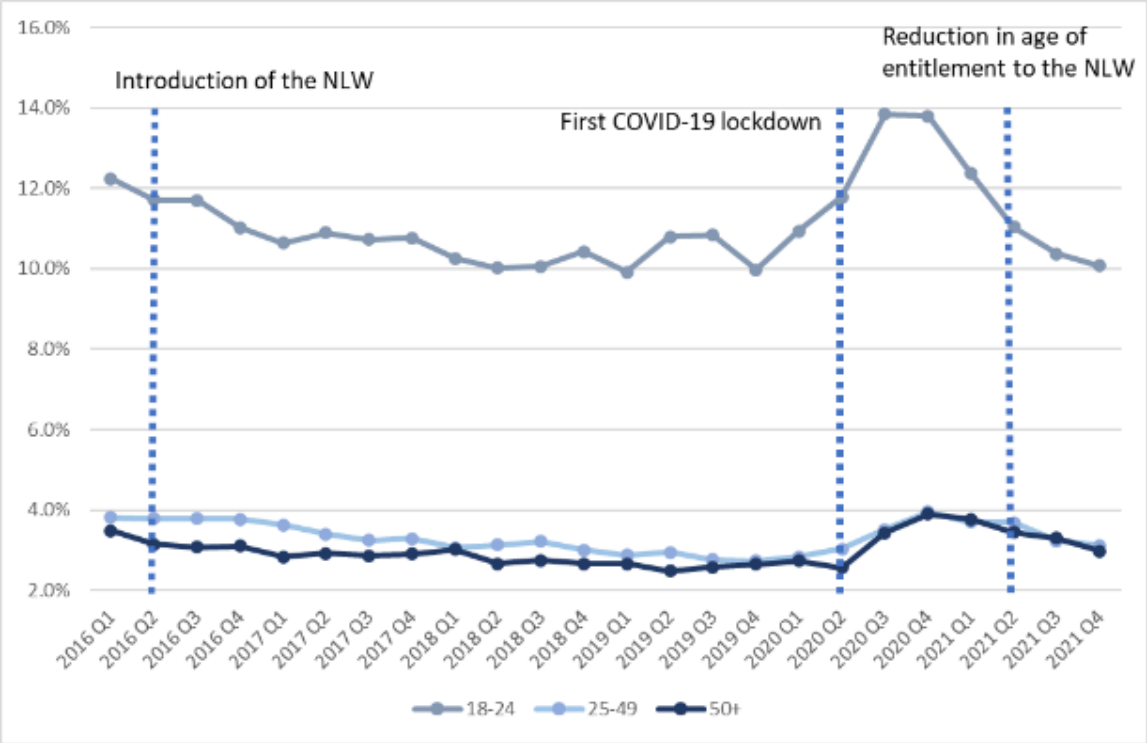
Moreover, as shown in Figure 4, the unemployment rate for young people (18 to 24-year-olds) is generally much higher compared to those observed for older age groups (such as 25 to 49-year-olds). In fact, at the end of 2019 the unemployment rate for the younger age group was around 10% compared to 2.7% for 25- to 49-year-olds and 2.6% for those above the age of 50[4].

Furthermore, the impact of economic recessions on unemployment and non-employment tend to be greater for younger people (Escalonilla et al., 2021; Henehan, 2021; Liu et al., 2016). This is

[2] See link here.
[3] See link here (Table 7a CJRS time series: employments on furlough take-up rate by employee age).
[4] See link here.

consistent with trends during the COVID-19 recession shown in Figure 4. The unemployment rate for young people rose by 3.9 percentage points between the fourth quarter of 2020 and third quarter of 2021, but by only 0.8 percentage points for 25 to 49-year-olds and for those older than 50.

Figure 4 Unemployment rates by age group, 2016 to 2021



Source: ONS (Labour Force Survey: Unemployment by age and duration)

These differences in employment trends highlight the need to control for different trends across age groups. To achieve this, we minimise potential differences across age groups by comparing the treatment group (23- and 24-year-olds) with those of similar ages. This approach is complemented with region- and sector-specific time fixed effects that control for different trends in employment, pay, and hours worked across regions and sectors (the latter for hours worked and hourly earnings), and allow for differences in the impact of the COVID-19 pandemic and the CJRS across different regions and sectors.

1.3 Recent evidence supporting the suggested methodology

The impact of minimum wages on employment and wages has been investigated using a variety of methodologies. The most relevant studies on the impact of changing the age of entitlement to different rates of the minimum wage include previous work by London Economics (2015), Crawford et al. (2011), Dickens et al. (2010), and Giupponi and Machin (2018). These analyses use the discontinuity in the age of entitlement for the adult rate of the minimum wage and found little or no evidence of a negative impact on employment and hours. The London Economics analysis (2015) involved comparing differences in outcomes between 21-year-olds (who were newly eligible for the adult rate in 2010) and two control groups: 20-year-olds (who were not eligible for the adult rate in 2010) and 22-year-olds (who were already eligible before 2010).

Dickens et al. (2010) exploit the age discontinuity in changes in the minimum wage at the age threshold of 22 years old. They find a significant positive effect at the adult NMW threshold on the

probability of being employed for low-skilled individuals. That is, similar individuals were more likely to be employed when the minimum wage was higher. However, when using a sample including individuals slightly further from the threshold age, Fidrmuc and Tena (2013) found no statistically significant discontinuity effect. However, they find a significant negative effect one year before the threshold age and argue this could be because firms lay off workers in anticipation of the minimum wage increase at the threshold age. Fidrmuc and Tena (2013) applied the same methodology but split the data according to firm size and industry, but found few significant results, which may be due to a limited number of observations once the data is divided by firm size or industry.

More recent evidence of changes in the minimum wage structure includes the work by Giupponi and Machin (2018), who investigate the impact of in the increase in the minimum wage for those aged 25 years and above through the introduction of the NLW. They found little evidence of adverse employment effects. Aitken et al. (2019) use a difference-in-differences approach and also find no significant impact of the introduction of the NLW on employment retention and hours worked, although some negative impacts on women working part time and the lowest paid workers in retail industries were identified.

In addition, a significant body of work has since explored the impact of minimum wages on employment, and in a review of the literature, Manning (2021) suggests that clear evidence of a negative impact remains "elusive". For example, (Chen & Teulings, 2022) estimated positive employment effects and strong compression effects in the lower half of the wage distribution as a result of the minimum wage. While some international studies provide evidence of a negative impact of the minimum wage on employment (Neumark and Shirley, 2021), there has been little evidence of a negative impact in the United Kingdom. Dube (2019) suggested that most studies in a UK context do not find that the minimum wage or changes in the NLW had a significantly negative impact on employment.

More generally, minimum wages have been found to have reduced wage inequality in the lower sections of the wage distribution (Autor et al., 2016; DiNardo et al., 1996; Lee, 1999), and in some cases, spillover effects up to the 10th and 20th percentiles (Card et al., 1993; Cribb et al., 2021). While earlier work in the literature suggests that the spillover effects are minimal in the UK (Dickens & Manning, 2004), more recent evidence argues that the UK does experience a positive spillover effect from changes in the minimum wage (Nanos, 2011). Bunching methods have also been used to estimate distributional effects (Cengiz et al., 2019; Cribb et al., 2021; Harasztosi & Lindner, 2019). Reflecting the significant recent commentary on the impact of the minimum wage across the wage distribution, we explored the differentiated impact of the minimum wage across different groups as part of our heterogeneity analysis.

2 Methodology and data

We use a difference-in-differences framework to estimate the impact of the reduction in the age of entitlement to the NLW from 25 years old to 23 years old that took place in April 2021 by comparing a treatment group (23- and 24-year-olds) who were newly eligible for the NLW in April 2021 with similar age groups unaffected by the policy change.

This analysis is consistent with previous analyses of changes to the NMW and NLW (Crawford et al. 2011, London Economics 2015), which both exploited discontinuities in age of entitlement to evaluate previous changes in the minimum wage regime.

2.1 Treatment and control groups

We use a **treatment group that consists of 23- and 24-year-olds in each time period** (before and after the change in the age eligibility threshold). They were not eligible for the NLW before April 2021 but became newly eligible in April 2021. They experienced an 8.7% increase in their minimum wage as a result of the policy change.

The combination of two (similar) age groups within the treatment group increases the sample size. This is particularly important for the heterogeneity analysis where we focus on the impact on a subset of the population. For example, the Labour Force Survey (LFS) contains around 2,500 observations after the policy change in April 2021 for each single year of age with information about labour force status. Approximately 1,600-2,000 observations (for each single year of age) include information about usual hours worked, however, only 300-500 observations (for each single year of age) that include information about hourly wages. This presents a challenge when undertaking heterogeneity analysis.

As counterfactual, we use a **control group** of those who were not directly impacted by the policy change but were nonetheless of a similar age to the treatment group. **We explore the suitability of groups of a similar age to the treatment group as a control group from the pool of 21- to 26-year-olds**. There were no changes in entitlement for these groups in April 2021. The minimum wage for 21- and 22-year-olds was still the NMW before and after April 2021 (associated with an increase of 2%), while the minimum wage for 25- and 26-year-olds increased by 2.2% in April 2021 (through the uprating of the NLW). We judge the suitability of different ages as control groups by comparing pre-treatment trends in labour market outcomes.

2.2 Baseline specification

The baseline specification seeks to identify the impact of the reduction in the age of entitlement by comparing the differences in changes in labour market outcomes between the treatment and control groups.

2.2.1 Extensive margin specification

When investigating extensive margin outcomes (such as employment status), we implement the following baseline specification

$$y_{it} = \alpha_{\mathrm{rt}} + \beta_1 T_i + \beta_2 POST_t + \beta_3 (T_i \times POST_t) + X\boldsymbol{\gamma} + \epsilon_{it} \quad (1)$$

where y_{it} is the outcome variable; $POST_t$ is a binary variable that takes a value of one if t is after the policy change on 1st April 2021 (and zero otherwise); T_i is a binary variable that takes the value of one if observation i is in the treatment group (and zero if in the control group); α_{rt} are region-time fixed effects, where regions are defined as the 12 UK ITL1 regions; X is a battery of controls which are discussed later in more detail (we also include a binary variable indicating whether the individual was on **furlough** at the time as a control variable, as this affected a considerable number of workers' employment statues and pay).

We use the 2017, 2018, 2019, and 2020 tax years as pre-treatment years. Tax years, rather than calendar years, are referred to in this study as since April 2017 changes in the minimum wage coincided with the start of the UK tax year at the start of April. The inclusion of more pre-treatment years allows for a better assessment of the **'parallel trends' assumption** and improves estimation accuracy of some variables. This is important given the significant changes in the 2020 tax year (coinciding with the first year of the COVID-19 pandemic), so the use of the 2020 tax year alone may not be representative of pre-treatment trends. We begin the pre-treatment window with the 2017 tax year, as any further back risks contamination of the pre-treatment window by the introduction of the NLW in April 2016.

2.2.2 Intensive margin specification

For intensive margin outcomes (such as hours of work and hourly earnings), we include sector-time fixed effects (α_{st}) where the baseline specification is

$$y_{it} = \alpha_{\mathrm{rt}} + \alpha_{st} + \beta_1 T_i + \beta_2 POST_t + \beta_3 (T_i \times POST_t) + X\boldsymbol{\gamma} + \epsilon_{it} \qquad (2)$$

Sector-time fixed effects are included in the intensive margin specification as information about individual i's sector of work is available, where sectors are defined as the ONS' SIC sections. This is not the case for those who are not in work, so sector-time fixed effects are not included in the extensive margin specification.

With the sole difference being the inclusion or exclusion of sector-time fixed effects, **both the intensive and extensive margin specifications control for a variety of differences**:

- **pre-existing differences** between the control and treatment groups (β_1),
- **nationwide differences across time** before and after the policy change (β_2),
- **sector- (for the intensive margin specification) and region-specific time fixed effects** to capture differences in changes across regions and sectors (α_{rt} and α_{st}), and
- the **impact of different individual characteristics** (γ).

The coefficient of interest is β_3, the difference between the control and treatment groups after the reduction in the age of entitlement. Table 1 presents the expected outcomes for the different groups.

Table 1 Average outcomes pre- and post-treatment: difference-in-differences approach

	Before	After
Treatment group	$X + \beta_1$	$X + \beta_1 + \beta_2 + \beta_3$
Control group	X	$X + \beta_2$

Note: Each cell presents the expected outcome according to the baseline specification, holding fixed effects and individual-level characteristics constant. X is the average outcome for the control group before the treatment.

It is important to note that the effects estimated by the above model specification are the average effects of eligibility for the NLW. Since many individuals, are unlikely to be affected by the minimum wage regime, the effect on individuals that are affected by the minimum wage regime may be significantly greater than that identified for the entire age groups. Estimates of β_3, therefore, can be interpreted as average treatment effect (ATE) of the policy change on young people. Heterogeneity analysis, focusing on groups such as those in low pay sectors and occupations, may allow for a better estimate of the average treatment effect of the treated (ATT), and is considered in a later section of this report.

2.2.3 Outcome variables: employment, hours worked, and hourly wages

A change in the minimum wage may impact many different labour market outcomes, so we investigate the impact of the reduction in the age of entitlement on

- **employment**,
- **actual and usual hours worked** (both log-transformed), and
- **hourly earnings**[5] (log-transformed).

Both actual and usual hours worked are included in the analysis, as they capture short-term and long-term changes in working hours. They are used to capture the impact on the average number of hours worked by those in work. In the Labour Force Survey (LFS), a respondent is asked about the number of hours worked in a reference week (actual hours worked) and the number of hours that they usual work (usual hours worked).

We also explore other labour market outcomes to better understand the driving factors behind changes in employment, actual and usual hours worked, and hourly earnings. These include **full-time and part-time employment** and **enrolment in education**. Part-time working and enrolment in education are associated with fewer hours worked, which may help to explain observed changes in hours worked.

We include those who are employees and those who are unemployed in the sample for the extensive margin specifications, and those who are employees for the intensive margin specifications. Those who are self-employed are not included in the analysis as the statutory minimum wage does not apply to them.

Observing the impact of the reduction on hourly earnings is particularly important. Statistically significant estimated impact on hours worked (for example) in the absence of a statistically significant estimated impact on hourly wages would question the assumption that relative changes in labour market outcomes are driven by changes in the minimum hourly wage. This is analogous to a test of the relevance assumption where age is an instrument for the hourly wage, where the employment and hours worked specifications are reduced form specifications.

2.2.4 Control variables

Different datasets include different levels of detailed information about individuals. We implement the main analysis using the LFS as there are more individual characteristics that can be controlled for

[5] Hours worked and hourly earnings are log-transformed to focus on percentage changes in those outcomes. Hourly earnings are deflated using CPI (April 2021 indices for all items taken from Table 20a, available here).

- **gender**,
- **ethnicity** (whether the individual is from a non-white ethnic background),
- **marital status** (whether they are married or not),
- **children** (whether they have any dependent children),
- **disability status** (whether they have a disability or not),
- **low educational attainment** (whether their highest educational attainment is Level 2 or below – the equivalent of GCSEs and below),
- **high educational attainment** (whether their highest educational attainment is Level 6 and above – the equivalent of degree level and above), and
- **furlough status**[6].

Region and sector (the latter only for intensive margin outcomes) are controlled for using region-time and sector-time fixed effects. Regions are defined as ITL1 statistical regions (for example, North East England or Wales), while sectors are defined as Standard Industrial Classification (SIC) sections (for example, A: Agriculture, Forestry and Fishing or L: Real Estate Activities). For the avoidance of doubt, examples of region-time fixed effects and sector-time fixed effects are 'Scotland-2018' and 'Financial and Insurance Activities-2018', respectively.

As a robustness check, we also include **month of birth** to control for differences within age group as well as a quadratic term to control for distance from the relevant age threshold (for example, how far away a 25-year-old is from the previous threshold or how far away a 23-year-old is from the current threshold). As a further robustness check, we re-run some specification using data from the **Annual Survey of Hours and Earnings** (ASHE), although this data source contains fewer individual characteristics, and as such, only gender and furlough status could be included as controls.

While all control variables potentially allow for more precise and less biased estimates, compared to previous analyses it is particularly important to control for furlough status, given the considerable number of workers on the CJRS throughout the COVID-19 pandemic. Even if there were no significant differences in furlough statuses between treatment and control groups, including the furlough variable allows for more precise estimation of the impact of the policy change.

2.3 Heterogeneity analysis

While our initial treatment group combines the entire sample of individuals aged 23 and 24 (and respective counterfactual groups), we also refine and break down the analysis by different characteristics. This reflects our expectations that some individuals are unlikely to be affected by changes in NLW eligibility, such as those on high hourly earnings. The heterogeneity analysis estimates the impact of the policy change on groups mostly likely to be impacted by changes to NLW eligibility.

To highlight the effect on the most relevant groups, we investigate the following categories:

- differences across individual characteristics, such as
  - **gender**, and
  - **educational attainment** (such those with lower educational attainment);
- differences across sectors of the economy, such as

[6] Data sources and potential limitations are discussed in Section 2.6.3.

- **shutdown sectors** (as defined by the Institute for Fiscal Studies[7], which also includes sectors such as hospitality and non-food, non-pharmaceutical retail) that were more affected by the COVID-19 pandemic,
- **low pay sectors** (as defined by the Low Pay Commission (LPC)[8], being those more likely to be affected by change in the NLW),
- **low pay occupations** (as defined by the LPC); and

- differences across regions and local areas, such as
  - **rural and urban areas** (as defined by the ONS),
  - **deprivation**, looking at the 20% most deprived areas[9] (matching the information on Lower layer super output area contained in the secure access LFS with information on the Index of Multiple Deprivation in England).

2.4 Assumptions and potential endogeneity concerns

The critical assumption of the proposed methodology is that the differences in trends in labour market outcomes between treatment and control groups are solely driven by their differentiated treatment through the policy change.

Bias in the estimated coefficient (β_3) would arise from a factor that influences treatment (e.g., 24-year-olds) and control (e.g., 25-year-olds) significantly differently that is not already controlled for in the baseline specification. This is known as the **common trends assumption**: that **once the factors in the baseline specification are accounted for, the trends of the treatment group would follow that of the control group in the absence of the policy change**.

While the common trends assumption cannot be directly tested, it is possible to undertake some analysis to test whether the assumptions are reasonable given the available data. These robustness checks and falsification tests are described in Section 2.5.

The baseline model controls for region-time and (for the hours and wages specification) sector-time fixed effects that supplement the individual-level characteristics that are discussed in the previous section. The region-time and sector-time fixed effects control for variation in economic conditions across regions and industries.

One example of how the baseline specification may control for these events, the Coronavirus Job Retention Scheme (CJRS) is presented below.

Box 1 Controlling for the Coronavirus Job Retention Scheme

One significant economic event that coincided with the reduction in the age of entitlement was the **Coronavirus Job Retention Scheme (CJRS)** which began in March 2020 and ended in September 2021. Further, the structure of **the CJRS changed six times in that period**, including after April 2021 when the CJRS was gradually wound down by increasing employers' wage contributions (in July 2021 (to 10%) and in August 2021 (to 20%))[10].

[7] See link here.
[8] See link here.
[9] Indices of Multiple Deprivation are published separately by UK home nations, so we restrict the analysis to England.
[10] See House of Commons Library (2021) link here.

As a result, there may be concerns that the estimated impact of NLW policy changes may also include the impact of changes in the CJRS. For this to be the case, the changes in the CJRS **would need to impact the treatment group significantly differently to the control group in a way that is not already controlled for in the baseline specification** (for example if they are likely to work in different sectors).

This would bias the estimated impact of NLW policy changes, as differences in outcomes between treatment and control groups may also be driven by differences in the sectors that the groups work in and the different impacts of the pandemic of those sectors. However, the baseline specification allows for region-time (for intensive and extensive margin specifications) and sector-time (for the intensive margin specifications) fixed effects which control for differentiated impacts of CJRS changes across regions and sectors. This differentiated impact could be in the way that regions and sectors are differently impacted by the introduction of the CJRS, changes to the CJRS, and the ending of the CJRS.

In addition, time fixed effects control for nationwide changes in the CJRS, region-time fixed effects control for the different impacts that the CJRS may have across regions (potentially reflecting the different concentration of sectors impacted by the pandemic), and control variables account for underlying differences in labour outcomes across individual-level characteristics such as gender and educational attainment.

In order for the estimated coefficient of interest to be biased, there would need to be a difference between treatment and control groups that

- is relevant after April 2021 (pre-existing differences are accounted for),
- is not accounted for by the inclusion of control variables, and
- is not linked to the differentiated impact of macroeconomic events such as Brexit and COVID-19 across sectors and regions (as sector- and region- fixed effects are included).

2.5 Robustness checks and falsification tests

We undertake three sets of robustness checks to test whether differences in the trends in outcomes between the treatment and control groups are in fact driven by differences in minimum wage regime by

1. comparing **pre-treatment trends** in outcome variables between treatment and control groups,
2. comparing differences in outcomes between **placebo treatment and control groups** who have not been differentially treated by the policy change, and
3. **using ASHE** as an alternative data source to the LFS.

The use of ASHE is discussed in further detail in the following section in the discussion about the data sources used in the analysis.

2.5.1 Pre-treatment differences in trends between treatment and control groups

The first robustness check **tests the significance of differences in trends between treatment and control groups before April 2021**. The common trends assumption requires similar trends between treatment and control groups – in the absence of the policy change, the treatment group would have followed a similar trend to that of the control group. While it is not possible to test this

assumption after April 2021, the absence of significant differences between treatment and control groups before April 2021 would reduce some concerns about the hypothetical differences between them in the absence of the policy change after April 2021.

Trends in the outcome variable before 2021 can be compared between 23-year-olds and 22-year-olds (for example). If the differences in employment, hours worked, and hourly wages between the two groups are **solely driven by differences in treatment by the minimum wage regime** (once other factors are controlled for in the baseline specification), then **there should not be significant differences between the two groups before 2021**.

We inlcude year dummies and year-treatment interactions for years up to the policy change to test for pre-treatment differences in trends.

$$\begin{aligned} y_{it} = \alpha_{\mathrm{rt}} + \alpha_{st} + \beta_1 T_i + \beta_{2018} Year2018_t + \beta'_{2018}(T_i \times Year2018_t) \\ + \beta_{2019} Year2019_t + \beta'_{2019}(T_i \times Year2019_t) \\ + \beta_{2020} Year2020_t + \beta'_{2020}(T_i \times Year2020_t) + X\boldsymbol{\gamma} + \epsilon_{it} \end{aligned} \quad (3)$$

β_{2018}, β_{2019}, and β_{2020} represent UK-wide changes in outcomes across the 2018, 2019, and 2020 tax years (relative to the base 2017 tax year), respectively, while β'_{2018}, β'_{2019}, and β'_{2020} illustrate the differences in trends between treatment and control groups in those respective tax years. **The latter set of coefficients are not expected to be significantly different to zero if the treatment and control groups are suitable comparators**.

There are two significant events before 2021, as well as other macroeconomic and labour market changes, that may drive differences in trends before the policy change in April 2021.

The first is the **start of the COVID-19 lockdowns in March 2020**. Testing differences in pre-treatment trends would test whether, even if after the controls and fixed effects included, there are signficant differences in trends between treatment and control groups as a result of the COVID-19 pandemic. The absence of significant differences in trends between groups in the 2020 tax year would alleviate concerns that differences in trends between groups were driven by the impact of the COVID-19 pandemic.

The second event is the **announcement of the reduction** in the age of entitlement to the NLW in 2019. Testing differences in pre-treatment trends would identify whether there were **anticipatory effects** that resulted in labour substitution away from those who would be 23 and 24 years old in April 2021 before the actual policy change date in April 2021. When the policy change was announced in 2019, an employer may substitute away from hiring 22-year-olds (who, in 2021, would be 24 and newly eligible for the NLW) to hiring 23-year-olds (who would be 25 in 2021 and also eligible for the NLW). The announcement of the policy change means that cohorts are covered by the same rate of the minimum wage from 2019 onwards, so there may be substitution towards the more experienced cohort as a result of the announcement.

2.5.2 Alternative treatment and control groups

A second falsification test is run where the control and treatment groups are **replaced with two groups that were not differently impacted by changes** to the minimum wage in April 2021.

For example, the control group could be changed to 21-year-olds and the treatment group changed to 22-year-olds. Both groups experienced the same increase in their relevant minimum wage, so if

the estimated coefficient represents the impact of differentiated minimum wages, no difference in outcomes should be expected. We undertake these falsification tests across a range of alternative pairs of treatment and control groups (presented in Table 2), testing the hypothesis that the coefficient of interest, β_3, is not significantly different to zero (i.e., the groups are not differently impacted by the policy change).

Table 2 Examples of alternative treatment and control groups

	Treatment group	Control group
Neither group eligible in April 2021	22-year-olds	21-year-olds
Both groups already eligible in April 2021	25-year-olds	26-year-olds
	26-year-olds	27-year-olds

However, **spillover effects** may be observed from the reduction in the age of entitlement, which we test for by comparing the relative outcomes of 25- and 26-year-olds to 21- and 22-year-olds. The sample includes those who are 21, 22, 25, and 26 years old and the specification (this example is used for intensive margin outcomes) is adapted to use 25- and 26-year-olds as a treatment group and 21- and 22-year-olds as a control group.

$$y_{it} = \alpha_{\mathrm{rt}} + \alpha_{st} + \beta_1 Age25or26_i + \beta_2 POST_t + \beta_3 (Age25or26_i \times POST_t) + X\boldsymbol{\gamma} + \epsilon_{it} \qquad (4)$$

β_1 represents the pre-existing differences in outcomes between the older age group (25- and 26-year-olds) and the younger age groups (21- and 22-year-olds), while β_3 illustrates the relative spillover effect between age groups. If $\beta_3 < 0$, this suggests that the spillover effect from the policy change benefits the younger age group more than the older age group, and vice versa. Among others, there are two reasons why spillover effects may be observed.

As the minimum wages of 23- and 24-year-olds increases by 8.7% in April 2021 (compared to around 2% for other age groups), there may be **substitution of labour away from those age groups to other young people**. If the substitution to other age groups is not homogeneous (e.g. if the group of 22-year-olds is more likely to act as a substitute for 23-year-olds compared to those aged 21), then there may be a differential impact across the alternative treatment and control groups.

In addition, there may be **anticipatory spillover effects for 22-year-olds who will soon be 23 years old**. The policy change may influence their labour market outcomes as it brings forwards the date they will be eligible for the NLW from 26 months away (for example) to 2 months away, which may influence employers' decisions. As a result, the policy change may impact 22-year-olds more than 21-year-olds. We include distance from the age threshold in months (from the secure access LFS data) as a robustness check to control for this effect, as well as age differences within year groups.

Differences between age groups would be expected as, for example, 27-year-olds will have on average one more year's experience in work than 26-year-olds. However, the **change in trends between those groups after April 2021** would not be expected to be as large as the change in trends between a group that was newly eligible to the NLW in April 2021 and a group that was not newly eligible. The spillover effects outlined above are expected to be indirect spillover effects compared to the direct impact of the NLW on the baseline treatment groups.

2.6 Data sources

The primary data source used in the analysis is the **Quarterly Labour Force Survey** (LFS, both End User Licence and Secure Access versions) up to the end of 2021-22 tax year. We also use the **Annual Survey of Hours and Earnings (ASHE)** up to the 2021 tax year[11].

The respective characteristics, strengths and weaknesses of the different data sources are discussed in more detail in the next sections. However it should be noted that the main difference is that the LFS is a survey of the UK resident population (covering both those in employment (including self-employed) and those not in employment), with information reported based on self-responses, while ASHE is based on a 1% sample of employee jobs PAYE records (excluding the self-employed), which results in a larger sample size than the LFS and it reports information returned by employers based on payroll data (hence it is considered to be more accurate).

Both the LFS and ASHE have a longitudinal component, but the sample sizes for single age groups were too small for meaningful analysis, especially when undertaking heterogeneity analysis.

2.6.1 Labour Force Survey (LFS)

The LFS is the official survey in the UK recording labour market outcomes in each month with around 80,000-90,000 respondents per quarter (although the number of respondents declined significantly during the period affected by COVID-19). The LFS is a representative sample of the UK resident population and each respondent in the survey is tracked over five consecutive quarters.

The analysis was primarily undertaken using the LFS, which reports information on a variety of personal and other characteristics such as age, gender, educational attainment, marital status, ethnicity, region of residence as well as labour market information on employment status, hours worked, hourly earnings, industry, occupation, firm size, length of employment and whether on furlough. This enabled us to look at all individuals in the different age bands considered and their outcomes before and after the policy change. We analysed data up to the end of the 2021-22 tax year, before the introduction of new minimum wages in April 2022 (where 21- and 22-year-olds experienced a large (9.8%) increase in their relevant minimum wage).

Although we also considered exploiting the panel component of the LFS (longitudinal LFS), tracking individuals over time before and after April 2021, sample sizes were extremely limited when looking at single year of ages and were not suitable for the analysis.

One disadvantage of the LFS is that the number of respondents has declined in recent years, leading to smaller sample sizes available for analysis. Also, around one third of responses are provided by a proxy respondent living in the same household (with an effect on the quality of the information provided). Moreover, the LFS is based on self-reported information, with potential measurement error.

2.6.2 Annual Survey of Hours and Earnings (ASHE)

The ASHE is an annual survey carried out in April each year and based on a 1% sample of employee jobs taken from HM Revenue & Customs (HMRC) PAYE records and has a larger sample size than the

[11] It should be noted that the reference period for ASHE 2021 (the date that the data refers to) is only a few weeks after the policy change at the start of April 2021. While the legal requirement to comply with the new minimum wage would have been immediate, the longer-term impact on employment decisions may not be captured in the data.

LFS. It provides information on earnings, hours of work as well as other job and some personal characteristics (but not education nor the rich variety of characteristics captured by the LFS). The data is gathered directly from employers. The ASHE also contains information on region and lower geographical levels, occupation types and industry. A question was added in 2021 to the survey concerning the employee's furlough status (not on furlough, part furlough, or furlough). ASHE does not include the self-employed, those who are unemployed (nor does it report information on full-time education, or whether economically inactive), so it is used to analyse changes at the intensive margin (i.e., change in hours and earnings).

The annual release of ASHE is usually in autumn, so the most recent data available is ASHE 2021. The reference period for ASHE 2021 (the date that the data refers to) is only a few weeks after the policy change at the start of April 2021. While the legal requirement to comply with the new minimum wage would have been immediate, the longer-term impact on employment decisions may not be captured in the data.

2.6.3 Furlough indicators

The reduction in the age of entitlement occurred during the presence of the COVID-19 Job Retention Scheme (CJRS). While some of the implications have been discussed in a previous section, we discuss the data limitations when controlling for the impact of the CJRS on labour market outcomes.

The ONS suggest that the quality of the variables indicating whether an individual's labour market outcomes had been influenced by the CJRS had improved across time, in both the LFS and ASHE. In the LFS between April and June 2020, an individual was classified as being in furlough if they were temporarily away from paid work, had worked fewer hours than usual, and that the reason they worked fewer hours was linked to coronavirus[12]. From July 2020 onwards, the LFS identified those in furlough from a direct question.

In ASHE 2020, the furlough variable is provided by HMRC and indicates whether an individual has been furloughed on the CJRS. The 2021 release included a direct question to the employee concerning whether they were furloughed on the CJRS, reducing the need to link individuals to HMRC data. The ONS suggest that linking individuals with HMRC data in 2020 underestimated the number of employees on furlough by approximately 20%[13].

Although not ideal, the difference in the quality of furlough indicators for both LFS and ASHE may not have a significant impact on the estimation of the coefficient of interest (differences between 23-/24-year-olds and 26-year-olds after the policy change compared to before the policy change) if the improvement in quality of the variable was sufficiently homogeneous. Even if this was not the case, some of the heterogeneity may be captured in the sector-time and region-time fixed effects included in the baseline specification. The use of two data sources where the furlough variable was derived in different ways may provide a test of the robustness of the findings with respect to different changes to the quality of the furlough variable across time.

[12] For further discussion of the quality of the LFS furlough variables see link here (Data sources and quality).
[13] For further discussion of the quality of the ASHE furlough variables see link here (Changes to the 2021 datasets).

Table 3 Data sources - summary

	Labour Force Survey	ASHE
Type	Household survey	Employer survey
Coverage	UK resident population	1% sample of PAYE employee jobs
Size (overall)	80,000-90,000 per quarter[14]	140,000[15]
Size by single year of age	Approximately 700 per quarter	Approximately 3,000 per year
Timeliness	Published monthly. A six- to seven-week gap between the end of the reference period and the publication date.	Yearly, collected in April each year, with results published in Autumn.
Includes	Employees, self-employed and those not in employment.	Employees only
Information on hours	Yes (self-reported)	Yes (from payroll)
Latest release used	Last quarter of the 2021-22 tax year	April 2021

[14] See link here.
[15] See link here.

3 Labour market trends among young people

Before turning to the econometric analysis, this section provides a **descriptive analysis of the trends in labour market outcomes and personal characteristics of those impacted by the age threshold reduction** (23- and 24-year-olds) and **adjacent age groups** (21-, 22-, 25-, and 26-year-olds). The descriptive analysis provides preliminary motivation for the choice of a suitable control group as a comparator for the treatment group of 23- and 24-year-olds. Those age groups whose labour market outcomes follow similar trends to the treatment group are likely to be more suitable control groups.

It is important to highlight that the **econometric analysis considers a shorter time period than presented in the following descriptive statistics**. More specifically, the baseline econometric analysis compares employment, hours worked, and hourly earnings from the 2017, 2018, 2019, and 2020 tax years (prior to the policy change) relative to the 2021 tax year (after the policy change).

As a result, it is particularly important for the treatment and control groups to have similar trends in the pre-treatment period for the common trends assumption to be plausible when controlling for other differences in the econometric analysis. Although similarities in the labour market trends of the treatment and control groups from 2013 to 2018 would further strengthen the argument, these outcomes do not affect the baseline econometric analysis.

This section also investigates the sample sizes available in the LFS for the treatment and control groups. Although modelling single-year age groups minimises any differential effect of age on labour market outcomes, the sample sizes for single-year age groups in the LFS may be too small to undertake a meaningful heterogeneity analysis in the econometric analysis.

3.1 Similarities in labour market outcomes and personal characteristics

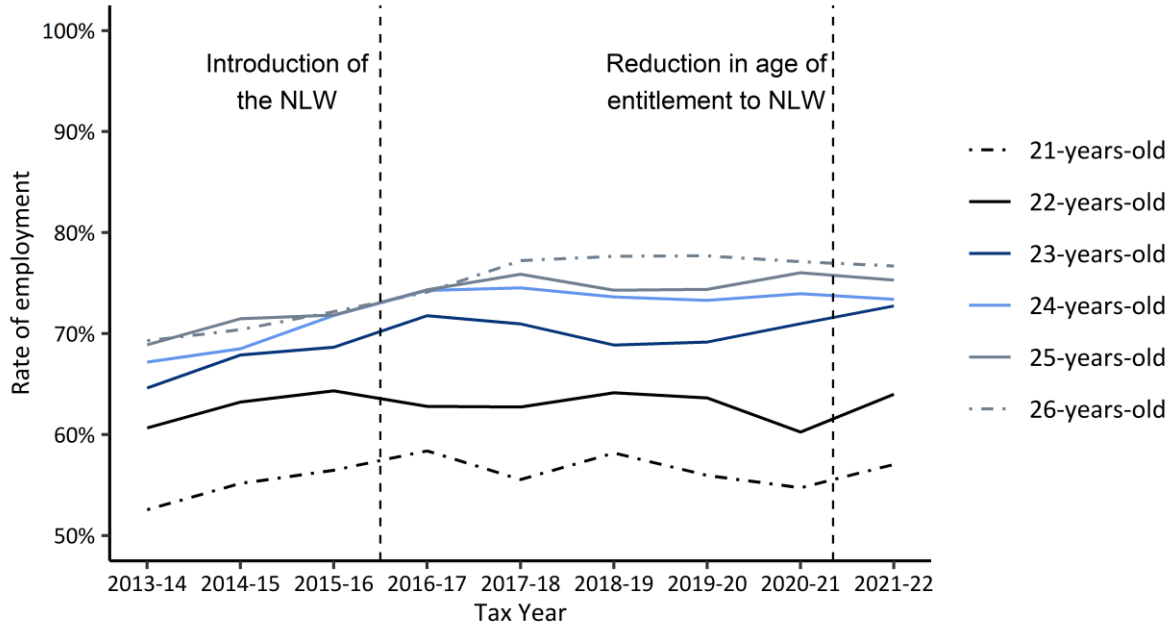
3.1.1 Employment rates of treatment and control groups

Figure 5 and Figure 6 present the employment rate and the change in the employment rate for the baseline treatment and control groups over the past 9 tax years, respectively. Across all age groups there has been an increase in the proportion in employment. In the 2013 tax year, approximately 60.7% of 22-year-olds, 64.6% of 23-year-olds, 67.2% of 24-year-olds and 68.9% of 25-year-olds were in employment whereas, in the 2021 tax year, approximately 64.0% of 22-year-olds, 72.7% of 23-year-olds, 73.4% of 24-year-olds and 75.3% of 25-year-olds were in employment.

Figure 5 presents differences in trends between 22- and 23-year-olds the period: there are very few tax years in which the employment rate has moved in the same direction for 22- and 23-year-olds. Most concerningly for the analysis, in the year before the change of the age eligibility of the National Living Wage (i.e., from 2020 to 2021), the employment rate of 22-year-olds fell by 5.3% whereas the employment rate of 23-year-olds rose by 2.6%[16].

[16] We test pre-trend differences in labour market trends between groups more formally as a robustness check in addition to observing descriptive statistics, as changes such as these may not be statistically significant and suffer from a small sample problem.

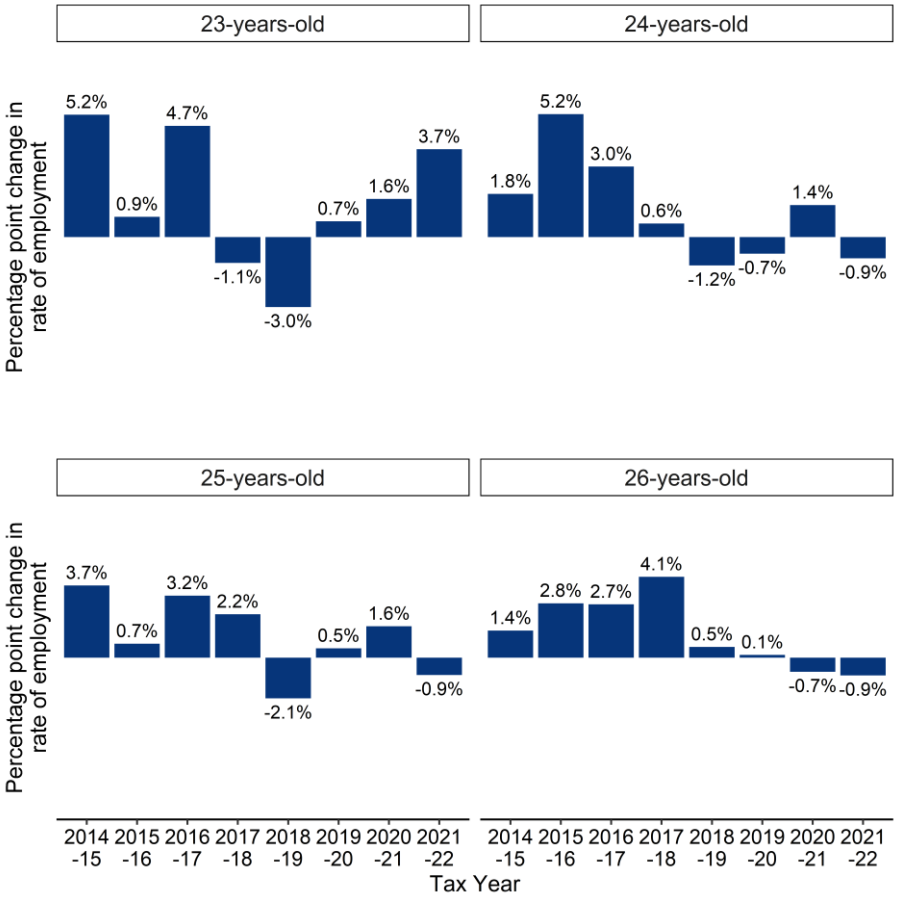
Figure 5 Employment rate of 21- to 26-year-olds from 2013 to 2021



Note: Employment rates exclude self-employed and unpaid family workers.

Source: LFS and London Economics' own calculations.

Figure 6 Annual changes in the employment rate of 23- to 26-year-olds from 2013 to 2021



Note: Employment rates exclude self-employed and unpaid family workers.

Source: LFS and London Economics' own calculations

A potential explanation for the differences between 22- and 23-year-olds is the greater proximity of 22-year-olds to the higher education system relative to 23-year-olds. More specifically, in the 2020-21 tax year, there was a 13.9 percentage point difference in the proportion of 22-year-olds with a Level 6 qualification relative to 21-year-olds, however there was only a 4.1 percentage point difference between 22- and 23-year-olds. This suggests that a larger proportion of individuals complete their Level 6 qualification at the age of 22 than at the age of 23 and therefore a greater number of 22-year-olds have recent experience of the higher education system relative to 23-year-olds.

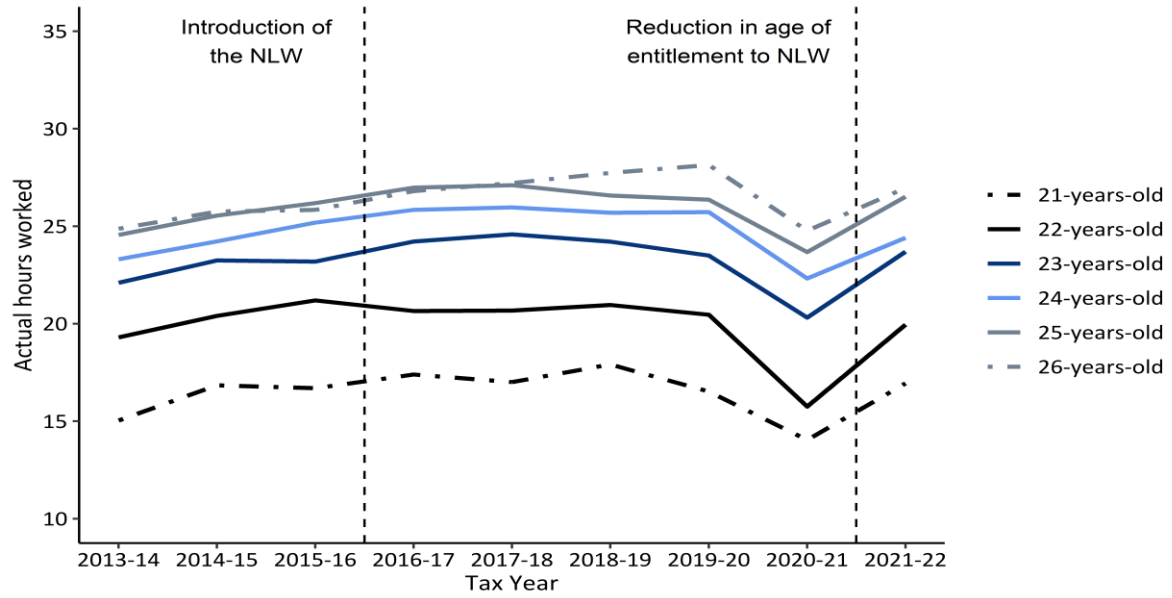
As a result of this proximity to the higher education system, the barriers to continuing or re-entering higher education are likely to be lower for 22-year-olds relative to 23-year-olds. Therefore, in the face of an economic shock, 22-year-olds could be more likely to continue in (or return to) higher education than 23-year-olds. For example, from 2019-20 to 2020-21, faced with the uncertainties of the labour market during the COVID-19 pandemic, the proportion of 22-year-olds reporting education or training as their labour force status increased by 4.2 percentage points whereas the proportion of 23-year-olds decreased by 12.2 percentage points. In contrast, the employment trends of 26-year-olds follows a more similar pattern to the treatment group, and in particular to 24-year-olds.

3.1.2 Hours worked by treatment and control groups

Of those in employment in 2021, 22-year-olds worked for an average of 20.0 hours per week, 23-year-olds for 23.8 hours, 24-year-olds for 24.6 hours and 25-year-olds for 26.6 hours. The trend in the average number of hours worked and the percentage change in the average hours worked by the treatment and control groups over the past 9 years is illustrated in Figure 7 and Figure 8, respectively.

It is important to note that actual hours worked (rather than usual hours worked) is analysed, therefore an individual can report working zero hours despite being in employment. This means that there is a significant dip between 2019 and 2020 as individuals on the Coronavirus Job Retention Scheme reported being in employment whilst working no hours (or very few hours).

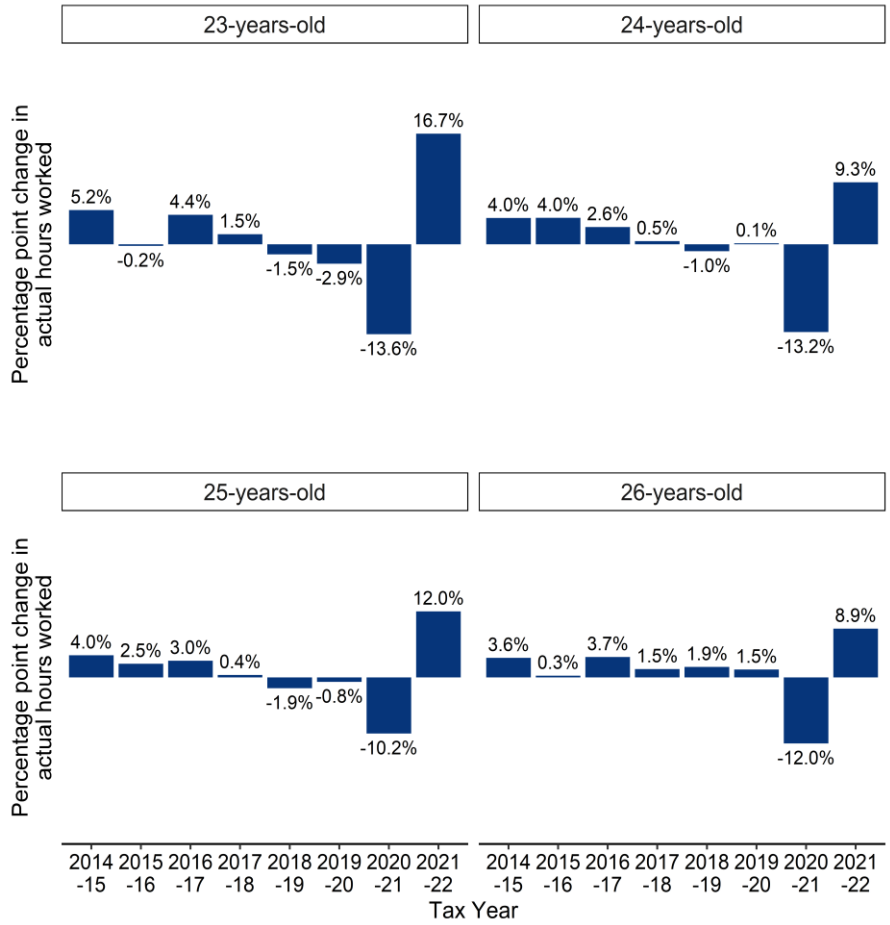
Figure 7 Hours worked by 21- to 26-year-olds from 2013-14 to 2021-22



Note: This is the average actual hours worked in main and second job.

Source: LFS and London Economics' own calculations

Figure 8 Annual changes in actual hours worked by 23- to 26-year-olds from 2013-14 to 2021-22



Note: This is the average actual hours worked in main and second jobs.

Source: LFS and London Economics' own calculations

As with trends in employment, the trends in the number of hours worked by 22-year-olds are quite different to those of older age groups. For example, the decrease in actual hours worked by 22-year-olds (22.3%) was almost double that of the decrease faced by 23- and 24-year-olds (11.7% and 12.5%, respectively). This suggests that there may have been a range of factors that influenced 22-year-olds differently to those of other age groups.

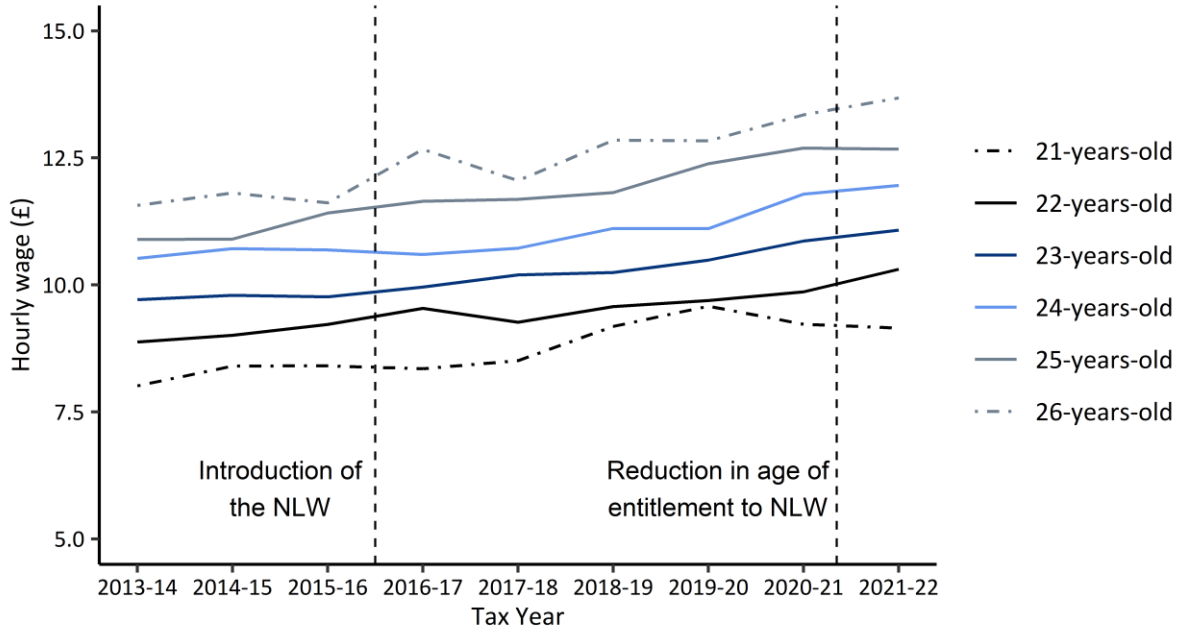
Further, Figure 8 provides evidence that the trends followed by 25-year-olds differs from other adjacent age groups. Despite matching the actual hours trends of 26-year-olds almost identically up to the 2017-18 tax year, there is a considerable divergence in after 2017-18 as the number of hours worked by 25-year-olds began to fall well before the COVID-19 pandemic, especially when compared to 24-year-olds. In contrast, the trends in average hours worked by 26-year-olds follows a more similar pattern to the treatment group, and in particular to 24-year-olds.

3.1.3 Hourly wages of treatment and control groups

Looking at the hourly wages of those in employment, Figure 9 shows that the average (mean) wage for both treatment and control groups has grown over the past 9 years. In the 2013 tax year, the average hourly wage for 22-year-olds was approximately £8.88, for 23-year-olds it was £9.71, for 24-year-olds it was £10.52 and for 25-year-olds it was £10.90 (in 2021 tax year prices). By 2021, these average hourly wages had risen to £10.31 for 22-year-olds, £11.08 for 23-year-olds, £11.96 for 24-year-olds and £12.67 for 25-year-olds.

It is important to highlight that there are some issues associated with the use of the hourly wage observations in the LFS for descriptive and econometric analyses. First, the LFS is based responses reported by the individual or someone else in their household, potentially leading to measurement error. Second, respondents are only asked about earnings on two out of the five occasions that they are surveyed, which means that the sample size can be quite small. As a result, ASHE data is used as a robustness check for the analysis on hours worked and hourly earnings undertaken using the primary data source (the LFS).

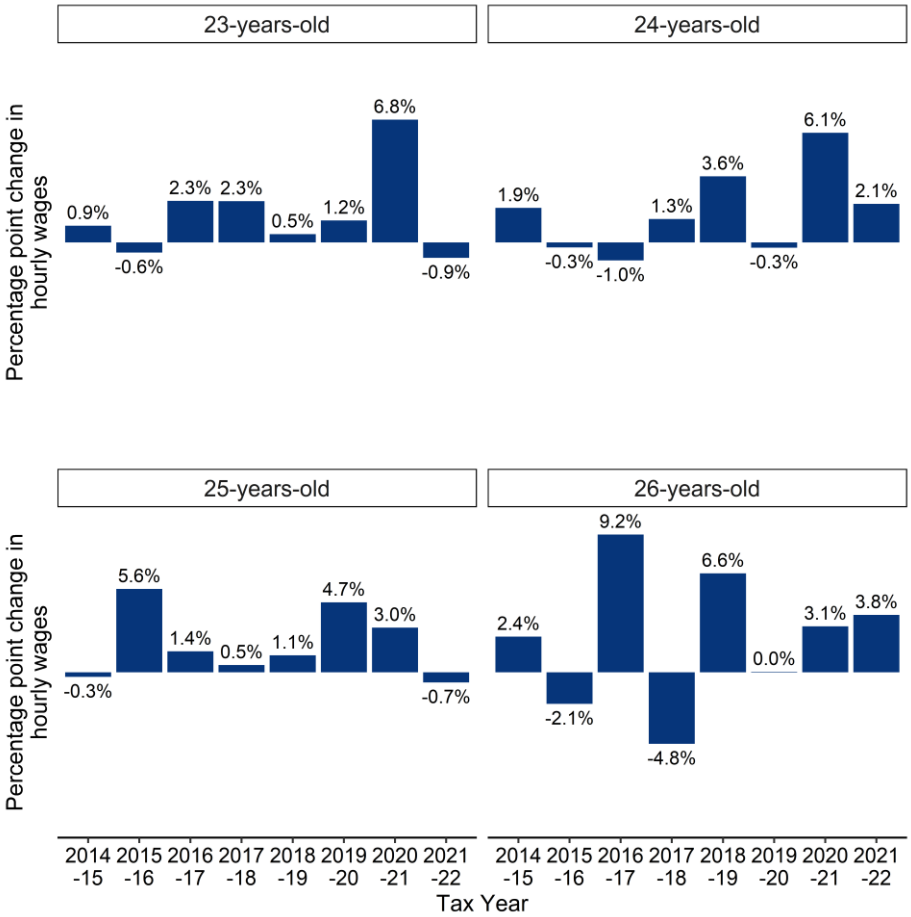
Figure 9 Hourly wages of 21- to 26-year-olds from 2013-14 to 2021-22



Note: This is the average hourly wages in main job. Wages adjusted to 2021-22 prices using CPI deflator.

Source: LFS and London Economics' own calculations.

Figure 10 Annual changes in the hourly wages of 23- to 26-year-olds from 2014-15 to 2021-22



Note: This is the average hourly wages in main job.

Source: LFS and London Economics' own calculations

3.1.4 Personal characteristics

The treatment and potential control groups have extremely similar personal characteristics. More specifically:

- **Race:** There has been a gradual increase in the proportion of individuals who were non-white in each age group from approximately 14% to 17% over the past 9 years.
- **Disability:** There has been a steady rise in the proportion of individuals reporting a work limiting disability in each age group from approximately 12% to 21% from 2013 to 2021.
- **Region:** Across all age groups, over the past 9 years there has been a relatively consistent regional distribution. More specifically, in 2013 and 2021, the region with the greatest proportion of 23- to 25-year-olds was London, whereas the regions with the smallest proportion of 22- to 25-year-olds were the North East and Northern Ireland.
- **Highest educational qualification:** There has been a similar trend in the highest qualification obtained by age group, although the distribution itself varies across age groups. For instance, from 2013 to 2021, there has been an increase in the proportion of 22- and 23-year-olds with a first degree or equivalent as their highest qualification, although the proportion of 22-year-olds with a first degree or equivalent was lower than the proportion of 23-year-olds (23.8% relative to 28.8% in 2021).

3.1.5 Treatment and control groups for the main analysis

Differences in trends between 22-year-olds and the treatment group and 25-year-olds and the treatment group suggest that despite their proximity in age they may not be ideal comparators, so in the main analysis we provide use **26-year-olds** as the control group. We provide evidence that supports this choice by more formal testing of pre-treatment differences in trends between the treatment group and potential control groups.

4 Results

In this section, we present the results, beginning with the results of the baseline specification followed by robustness checks to assess the suitability of 26-year-olds as the preferred control group. To identify the impact of the policy change on those most likely to be affected, we undertake heterogeneity analysis to focus on the impact of the reduction in the age of entitlement on specific groups (across individual, sector, and local area characteristics). Finally, we test the robustness of the results using ASHE, focusing on those working in low pay sectors and occupations.

4.1 Main results

The baseline specification outlined in Section 2.2 compares how changes in labour market trends differ between the treatment group (23- and 24-year-olds, who were newly eligible for the NLW in April 2021) and the control group of 26-year-olds (with the descriptive analysis and more formal tests suggesting that 26-year-olds are the most suitable comparator) following the reduction in the age of entitlement.

Table 4 reports the main results using the baseline specification. **The coefficient of interest in the first column ('Treatment X Post April 2021') suggests that there is a small, positive, and statistically insignificant impact on employment** probabilities of 0.7 percentage points for 23- and 24-year-olds relative to 26-year-olds after the policy change. The impact on actual hours worked is small, negative, and also statistically insignificant (implying a 0.8% decrease in average hours worked as the hours data is log-transformed). The point estimate relating to usual hours reported in third column is smaller (0.4%), which may be explained by the less volatile nature of the usual hours measure compared to actual hours.

Table 4 Impact of the reduction in age of entitlement

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.022 ***	-0.025 ***	-0.029 ***	-0.119 ***
	(0.003)	(0.005)	(0.004)	(0.009)
Treatment X Post April 2021	0.007	-0.008	-0.004	0.017
	(0.006)	(0.012)	(0.011)	(0.023)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.039	0.108	0.135	0.226
No. observations	35841	28921	32446	8197

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

The **estimated effect on hourly earnings for 23- and 24-year-olds is positive** (1.7% increase in hourly earnings relative to 26-year-olds as the hourly earnings data is log-transformed) **but is not statistically significant**. The positive impact is expected given the differences in increases in the minimum wage for the treatment and control groups. The statistical insignificance of the estimate is possibly driven by the smaller sample size for hourly earnings data, which is around a quarter of

the sample size compared to the employment and hours worked regressions. The estimated robust standard error of 2.3% suggests that the impact on hourly earnings would have to be considerable for a statistically significant impact to be detected. In contrast, the expected impact may be relatively small. The minimum wage for 23- and 24-year-olds rose by 6.5 percentage points more than the minimum wage for those aged 25 and older. If this relative increase was relevant for fewer than (for example) 10 percent of the age group (and holding other differences in hourly earnings growth between age groups constant), this would result in less than a 0.65% relative increase in hourly earnings for 23- and 24-year-olds (compared to 26-year-olds).

The statistical insignificance of the results in Table 4 may reflect the sample that the baseline specification uses. Even after the reduction in the age of entitlement the minimum wage coverage for 23- and 24-year-olds was around 11.7%, leaving a **significant number who are unlikely to be impacted by minimum wage changes**, even accounting for spillover effects across the wage distribution. As a result, heterogeneity analysis is undertaken to identify the impact of the policy change on those who are most likely to have been impacted by the change.

The coefficient estimates reported in the first row of Table 4 (those for 'Treatment', estimating pre-treatment differences between treatment and control groups) are negative and significant across all outcomes, with 26-year-olds 2.2 percentage points more likely to be employed, work 2.5%-2.9% more hours, and earn 11.9% more per hour relative to 23- and 24-year-olds. This is expected as 26-year-olds have, on average, two or three more years of experience in the labour market.

These results are a robust to a range of other specifications, such as:

- **the inclusion of the month of birth** (presented in Table 24 in the Annex), which controls for both age differences within age groups (as someone who is 24 years and 1 month old will have close to a year's less experience than someone who is 24 years and 11 months old), as well as distance from the age of entitlement threshold through a quadratic form.
- a **regression discontinuity design** methodology and results which are presented in Annex A1.3, where 25-year-olds are included, and the impact of age (and its quadratic form) is allowed to differ on either side of discontinuity[17]. The results suggest a negative impact on actual hours worked and a positive employment effect.
- **the exclusion of the 2020-21 tax year**. The 2020-21 tax year began shortly after the first COVID-19 pandemic lockdowns in the UK, with significant impacts on the UK economy where differences in the labour market outcomes of different age groups during that tax year may not be representative. However, as presented in Table 26 in the Annex, this exclusion does not impact the main results.
- the **exclusion of the furlough variable**. It is possible that poor controls may bias the estimates. For example, an increase in the minimum wage may increase the likelihood that workers are furloughed, and more so for younger workers, resulting in reduced hours for 23- and 24-year-olds relative to older workers. The inclusion of a furlough variable as an explanatory variable may result in an underestimation of the impact of changes to the minimum wage on hours worked for 23- and 24-year-olds. The estimates when the furlough variable is excluded are reported in Table 27 in Annex A1.5. The point estimates are similar to those found using the baseline specification above.

[17] More detail on the methodology used in the regression discontinuity can be found in Annex A1.3.

Further, there are **no differences in the impact of the policy change between age groups within the treatment group (i.e., between 23- and 24-year-olds)** within the treatment group, as reported in Table 23 in the Annex.

Table 5 presents the estimated impact of the policy change on those in work. The sample is **restricted to those respondents who are employees**. The first specification changes the outcome variable to a dummy variable that takes a value of one if **the individual is in full-time employment and a value of zero if in part-time employment**. The coefficient of interest is negative, suggesting a shift in part time work of 1.5 percentage points, but is not statistically significant. An increase in the proportion of workers who are in part-time employment may reflect either workers who previously worked full time changing to part-time employment or the influx of new workers in part-time employment.

The specification presented in the second column also focuses on those studying while working: the sample is **restricted to employees and the outcome variable takes a value of one if the individual is enrolled in education and a value of one if not**. The point estimate is small and not statistically significant, so there is insufficient evidence to suggest that the reduction in the age of entitlement causes 23- and 24-year-olds to be more or less likely to enrol in education while in work.

Table 5 Employment status and enrolment in education while working

	Full-time employment	Enrolment in education
Treatment	-0.020 ***	0.028 ***
	(0.004)	(0.004)
Treatment X Post April 2021	-0.015	0.005
	(0.010)	(0.009)
Treatment group (ages)	23 & 24	23 & 24
Control group (ages)	26	26
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y
R-squared	0.158	0.019
No. observations	33276	33464

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. The LFS sample for both specifications includes those who are employees. Full-time employment indicates whether an individual is in full-time employment (rather than part-time employment) and enrolment in education indicates whether an individual is enrolling in education (full-time or part-time) while working. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

4.2 Robustness checks

This section presents the robustness checks undertaken to support the choice of treatment and control group. We test differences in pre-treatment trends between the treatment and potential control groups, and test differences between placebo treatment and control groups.

4.2.1 Differences in pre-treatment trends

As described in Section 2.5.1, the suitability of treatment and control groups can be tested by estimating differences in trends before the treatment, akin to a placebo treatment across time. **The absence of significant differences before the treatment would support the common trends assumption**, namely that in the absence of the policy change in April 2021, the labour market trends of the treatment group would have been similar to those of the control group.

We first test differences in the pre-treatment trends between the preferred treatment (23- and 24-year-olds) and control (26-year-olds) groups. Table 6 presents the annual growth rates of the relevant minimum wages for 23-/24-year-olds and for those 25 years old and older[18]. Although there are some differences in growth rates before 2021, these differences are small relative to the 6.6 percentage point difference or divergence in 2021. As a result, if observed differences between treatment and control groups after April 2021 were driven by changes in minimum wage policy, then significant differences should not be observed before April 2021.

Table 6 Minimum wage growth rates for 23-/24-year-olds and those aged 25 and older

	23- and 24-year-olds	Aged 25 and older
2017	5.2%	4.2%
2018	4.7%	4.4%
2019	4.3%	4.9%
2020	6.5%	6.2%
2021	8.7%	2.2%

Source: Low Pay Commission (2021)

Table 7 reports the pre-treatment differences in trends between the preferred treatment (23- and 24-year-olds) and control (26-year-olds) groups. The estimates of the first coefficient ('Treatment') suggests that 23- and 24-year-olds are 1.9 percentage points less likely to be in employment and are paid 11.7% less in hourly earnings compared to 26-year-olds in 2017-18, although there are no significant differences in hours worked. **Critically, there are no significant differences in trends in the following years, which supports the use of 26-year-olds as the preferred control group**.

Table 7 Differences in pre-treatment trends (T = 23 & 24, C = 26)

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.019 ***	-0.009	-0.009	-0.117 ***
	(0.005)	(0.010)	(0.008)	(0.017)
Treatment X 2018-19 tax year	0.002	-0.005	-0.012	-0.001
	(0.008)	(0.013)	(0.012)	(0.025)
Treatment X 2019-20 tax year	-0.002	-0.020	-0.017	-0.007
	(0.008)	(0.014)	(0.012)	(0.026)
Treatment X 2020-21 tax year	-0.006	-0.015	-0.020	0.006
	(0.008)	(0.016)	(0.013)	(0.029)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.040	0.113	0.142	0.219
No. observations	29453	23662	26614	6827

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

[18] For example, the value of 8.7% in 2021 for 23- and 24-year-olds indicates an increase in the minimum wage for 23- and 24-year-olds from the 2020-21 tax year to 2021-22 tax year of 8.7% (as a result of changes in April 2021, the start of the 2021-22 tax year).

The same robustness check is undertaken for other potential control groups. Table 8 selects 22-year-olds as the control group, while Table 9 compares pre-treatment trends with 25-year-olds added to the control group of 26-year-olds.

As suggested in the descriptive statistics, 22-year-olds follow a different trend in employment compared to 23- and 24-year-olds, as shown in the first column of Table 8. In particular, in 2019-20, employment probabilities for 23- and 24-year-olds rose by 2.3 percentage points relative to 22-year-olds. There are also differences in employment trends after the addition of 25-year-olds in the control group, with 23- and 24-year-olds facing a 1.5 percentage point fall in employment probability relative to 25- and 26-year-olds in the COVID-19 pandemic-impacted 2020-21 tax year. Further, there is a significant difference in trends in usual hours worked in 2018-19.

Table 8 and Table 9 **suggest that 26-year-olds are a more suitable comparator for the treatment group, especially compared to 22- and 25-year-olds**.

Table 8 Differences in pre-treatment trends (T = 23 & 24, C = 22)

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	0.028 ***	0.054 ***	0.060 ***	0.073 ***
	(0.007)	(0.012)	(0.011)	(0.020)
Treatment X 2018-19 tax year	-0.015	0.010	0.014	0.007
	(0.010)	(0.017)	(0.016)	(0.029)
Treatment X 2019-20 tax year	0.023 **	0.005	0.001	-0.009
	(0.010)	(0.018)	(0.017)	(0.032)
Treatment X 2020-21 tax year	0.001	0.000	0.015	-0.013
	(0.012)	(0.022)	(0.019)	(0.035)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	22	22	22	22
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.042	0.107	0.130	0.173
No. observations	33395	26380	29417	7150

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

Table 9 Differences in pre-treatment trends (T = 23 & 24, C = 25 & 26)

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.020 ***	-0.012	0.00	-0.097 ***
	(0.005)	(0.008)	(0.007)	(0.015)
Treatment X 2018-19 tax year	0.005	0.004	-0.017 *	0.008
	(0.007)	(0.011)	(0.010)	(0.021)
Treatment X 2019-20 tax year	-0.002	-0.016	-0.006	-0.008
	(0.007)	(0.012)	(0.011)	(0.023)
Treatment X 2020-21 tax year	-0.015 **	-0.019	-0.018	-0.001
	(0.007)	(0.014)	(0.011)	(0.024)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	25 & 26	25 & 26	25 & 26	25 & 26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.038	0.103	0.125	0.197
No. observations	35198	28372	31689	7966

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

Besides comparing pre-treatment trends between treatment and potential control groups, we also **test differences in pre-treatment trends between the two age groups that make up the treatment group**. Testing the differences between 23- and 24-year-olds assesses the suitability of using the combination of the year groups as a single treatment group.

Table 10 presents the results when testing the differences between 23- and 24-year-olds before the policy change in April 2021. Differences in employment trends are not statistically significant while the point estimates are small in magnitude. The point estimates are larger and suggests that growth in average hours worked by 24-year-olds was greater than for 23-year-olds, although importantly this difference is not statistically significant. This is not the case for hourly earnings, where there appears to be a statistically significant difference in trends between the two treatment year groups in 2018-19, despite noisy estimates from a smaller hourly earnings sample size.

Table 10 Differences in pre-treatment trends (T = 24, C = 23)

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	0.004	0.013	0.008	0.008
	(0.007)	(0.012)	(0.010)	(0.021)
Treatment X 2018-19 tax year	-0.002	0.003	0.007	0.074 **
	(0.010)	(0.017)	(0.015)	(0.032)
Treatment X 2019-20 tax year	-0.001	0.011	0.005	-0.031
	(0.010)	(0.018)	(0.016)	(0.034)
Treatment X 2020-21 tax year	0.001	0.021	0.013	-0.033
	(0.012)	(0.021)	(0.017)	(0.036)
Treatment group (ages)	24	24	24	24
Control group (ages)	23	23	23	23
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.039	0.102	0.125	0.189
No. observations	22944	18327	20467	5032

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

4.2.2 Placebo treatment and control groups

Another robustness check undertaken is the comparison of placebo treatment and control groups using the baseline specifications, where both the treatment and control group were similarly impacted by the minimum wage changes introduced in April 2021.

We compare three pairs of placebo treatment and control groups:

- 22- and 21-year-olds (neither age group eligible for the NLW before and after April 2021),
- 25- and 26-year-olds (both age groups eligible for the NLW before and after April 2021), and,
- 26- and 27-year-olds (both age groups eligible for the NLW before and after April 2021).

If both age groups were similarly impacted by the minimum wage, we would not expect there to be significant differences in trends between placebo treatment and control group after the policy change in April 2021. **Differences in trends after the policy change may occur due to heterogeneous spillover effects** (for example, employers may substitute away from 23-year-olds to 22-year-olds rather than to 21-year-olds as the former have, on average, another year of experience) **but is unlikely given the insignificant impact found in the main results**.

The results shown in Table 11 **further suggest problems with using 25-year-olds as a control group**. The first and third columns of Table 11 suggest that there are no significant differences between placebo treatment and control groups after the policy change, while in the second column there is a positive and significant increase in actual and usual hours worked after the policy change for 25- relative to 26-year-olds. No such differences arise when comparing 26-year-olds to 27-year-olds in the third column.

Table 11 Comparison of placebo treatment and control groups

	T = 22, C = 21	T = 25, C = 26	T = 26, C = 27
Treatment group	22-year-olds	25-year-olds	26-year-olds
Control group	21-year-olds	26-year-olds	27-year-olds
Panel A. Employment			
Treatment X Post April 2021	0.011	-0.001	0.000
	(0.012)	(0.008)	(0.007)
R-squared	0.047	0.039	0.045
No. observations	10740	13779	14819
Panel B. Actual hours			
Treatment X Post April 2021	0.005	0.033 **	-0.015
	(0.024)	(0.015)	(0.014)
R-squared	0.047	0.039	0.045
No. observations	8008	11228	12151
Panel C. Usual hours			
Treatment X Post April 2021	-0.014	0.032 ***	-0.014
	(0.022)	(0.013)	(0.012)
R-squared	0.165	0.152	0.174
No. observations	7935	11137	12048
Panel D. Hourly earnings			
Treatment X Post April 2021	0.029	0.014	0.021
	(0.035)	(0.027)	(0.030)
R-squared	0.127	0.215	0.233
No. observations	2107	3328	3708

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The Treatment binary variable is also included in the specification but omitted from the summary table for the sake of clarity. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

4.3 Heterogeneity analysis

While the main results do not indicate any significant impact of the reduction in the age of entitlement to the NLW on labour market outcomes, the samples used for those groups include many individuals who are unlikely to be impacted by changes to the minimum wage, such as those paid significantly higher than the NLW.

The estimated coefficients reported in Table 4 may mask heterogeneity in the impact of the policy change across groups. In this section, we present estimates across different subsets of the population: by **individual characteristics**, by **sector**, and by **local area characteristics**.

The following discussion of the robustness checks for the heterogeneity analysis also **present caveats** for the causal interpretation of the results. Firstly, the **lack of a statistically significant**

estimated impact on hourly earnings for some subgroups limits the causal interpretation of the heterogeneity analysis, even in the cases where a statistically significant impact on hours or employment is estimated. Further, there are some **potential inconsistencies between different subgroups**, where some subgroups with relatively larger estimated hourly earnings effects have relatively smaller estimated impacts on hours worked. This would be unexpected if variation in hours worked was solely driven by differences in the minimum wage. While some of the subgroups analysed may be made up of a higher proportion of workers earning the minimum wage (or near minimum wage) workers, the **subgroups still may contain many workers whose labour market outcomes are unaffected by changes to the minimum wage**. Finally, there are some subgroups where the **small sample size** contributes to large estimated standard errors.

4.3.1 Impact across individual characteristics

To estimate the impact of the reduction in the age of entitlement to the NLW on different groups, we restrict the sample to only include a group with a certain characteristic, and 23- and 24-year-olds are compared to 26-year-olds within that subsample. Table 12 presents the results of the heterogeneity analysis when focusing on **male workers, female workers, and non-white workers**.

Table 12 Heterogeneity analysis: male, female, and non-white workers

	Employment	Actual hours	Usual hours	Hourly earnings
Panel A. Male workers				
Treatment	-0.030 ***	-0.030 ***	-0.039 ***	-0.135 ***
	(0.004)	(0.007)	(0.006)	(0.015)
Treatment X Post April 2021	0.012	-0.016	-0.003	0.053
	(0.010)	(0.017)	(0.014)	(0.034)
R-squared	0.047	0.096	0.127	0.237
No. observations	17392	14183	15526	3784
Panel B. Female workers				
Treatment	-0.016 ***	-0.027 ***	-0.028 ***	-0.089 ***
	(0.004)	(0.008)	(0.006)	(0.010)
Treatment X Post April 2021	0.001	0.003	0.002	-0.021
	(0.008)	(0.018)	(0.015)	(0.025)
R-squared	0.037	0.138	0.161	0.223
No. observations	18449	14738	16920	6010
Panel C. Non-white workers				
Treatment	-0.027 **	-0.060 ***	-0.078 ***	-0.116 ***
	(0.011)	(0.019)	(0.016)	(0.037)
Treatment X Post April 2021	-0.048 *	-0.105 **	0.020	0.027
	(0.027)	(0.043)	(0.036)	(0.064)
R-squared	0.065	0.181	0.178	0.298
No. observations	4022	3008	3361	1073

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable – gender is not included for the male and female specifications and ethnicity is not included for the non-white specification) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

Panels A and B suggest that splitting the sample by gender **may still result in groups that are too broad to detect an impact of changes to the minimum wage**, with no statistically significant estimates across labour market outcomes. This may be expected, as although the coverage of the NLW for women is almost double that of men, the vast majority of women are paid above the NLW.

However, there is a **statistically significant decrease in employment for non-white workers**. Non-white workers aged 23 and 24 years old are 4.8 percentage points less likely to be in employment after April 2021, relative to non-white workers aged 26 years old. Across all groups explored, this is the only incidence of a negative estimated impact on employment. There is also a **significant and negative estimated impact on actual hours worked of around 10.5%** (by far the most negative impact across groups investigated), although the impact on usual hours worked (a less volatile measure) is statistically insignificant. Although pre-treatment trends differences in labour market outcomes are not significant for non-white workers, this may mask significant heterogeneity in labour market outcomes across different ethnicities. The estimated impact may also reflect changes in the composition of non-white workers, such as if ethnicities facing the largest decrease in hours worked made up a larger proportion of 23-/24-year-old non-white workers (relative to 26-year-old non-white workers) after the COVID-19 pandemic began compared to before.

Table 13 reports the heterogeneity analysis for disabled workers (Panel A), workers with at least one dependent child (Panel B), and those with low educational attainment (Panel C).

There are positive but insignificant impacts on employment and hours worked for disabled workers and workers with at least one dependent child, with the exception of actual hours worked by workers with at least one dependent child. **The treatment group of 23- and 24-year-olds with at least one dependent child experienced a 7.6% increase in hours worked relative to 26-year-olds** with at least one dependent child, although this is smaller and not statistically significant for usual hours.

Those with low educational attainment are more likely to be affected by changes to the minimum wage, with over a quarter (25.9%) of those without qualifications covered by the NLW[19] (and are often used as a proxy for minimum wage workers in the minimum wage literature). However, no significant impact on any labour market outcome is estimated. This may be the result of a small sample size or the group still being too broad to detect the impact of minimum wage changes.

There are positive, although statistically insignificant, impacts on hourly earnings across the groups, and in particular they are considerably larger than the impact on hourly earnings for the baseline sample. This suggests that **restricting the sample is focusing better on those most likely to be impacted by changes to the minimum wage, although at the cost of sample sizes**. For example, on average there are only around 400 disabled workers in each age group with hourly earnings data across the entire period from 2017-18 to 2021-22.

[19] In 2020-21 for those aged 25 and above: Low Pay Commission (2022) link here.

Table 13 Heterogeneity analysis: disabled workers, workers with children, and those with low educational attainment

	Employment	Actual hours	Usual hours	Hourly earnings
Panel A. Disabled workers				
Treatment	-0.034 ***	-0.042 **	-0.048 ***	-0.124 ***
	(0.011)	(0.020)	(0.016)	(0.031)
Treatment X Post April 2021	0.031	0.036	0.049	0.095
	(0.023)	(0.047)	(0.040)	(0.075)
R-squared	0.072	0.131	0.160	0.282
No. observations	4807	3527	4115	1158
Panel B. Workers with children				
Treatment	-0.029 ***	0.030 *	0.022 *	-0.129 ***
	(0.008)	(0.016)	(0.012)	(0.027)
Treatment X Post April 2021	0.011	0.076 *	0.038	0.088
	(0.020)	(0.040)	(0.033)	(0.062)
R-squared	0.063	0.261	0.272	0.203
No. observations	6066	4353	5394	1417
Panel C. Low educational attainment				
Treatment	-0.019 **	0.012	0.008	-0.064 ***
	(0.008)	(0.013)	(0.011)	(0.017)
Treatment X Post April 2021	0.001	-0.016	0.001	0.014
	(0.018)	(0.032)	(0.029)	(0.050)
R-squared	0.067	0.216	0.233	0.163
No. observations	7237	5483	6250	2046

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable – disability status is not included for the disability specification, the children binary indicator is excluded from the specification focusing on those with children, and educational attainment factors are excluded when focusing on those with low educational attainment) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

4.3.2 Impact across sectors

The analysis across a large number of sectors (for example, SIC sections such as 'real estate activities') is not feasible given the sample size. Consequently, we focus on three (not mutually exclusive) categories of sectors that are likely to have been impacted by changes to the minimum wage to a greater extent that other sectors.

The three sectors include **low pay sectors, low pay occupations, and shutdown sectors**. Low pay sectors and low pay occupations are defined by the Low Pay Commission[20] across Standard Industrial Classifications and Standard Occupation Classifications (both defined by the ONS). By construction, those working on the minimum wage consist of a larger proportion of these sectors and occupations than any others across the economy, so these groups of sectors and occupations

[20] An explanation and list of low pay sectors and occupations can be found in Appendix 1 of Low Pay Commission (2022a) – link here.

are suitable subsamples. Shutdown sectors refer to those most impacted by the COVID-19 lockdowns, as identified by the Institute for Fiscal Studies[21]. The COVID-19 pandemic impacted these sectors to a much greater extent, so the impact of a significant increase in the minimum wage may be more negative in these sectors than in others (for example, those sectors may be less able to soak up minimum wages increases through cutting profits).

As individuals who are unemployed cannot be accurately assigned to a sector, we undertake the analysis at solely at the intensive margin: **actual hours worked, usual hours worked, and hourly earnings**.

Table 14 Heterogeneity analysis: Low pay sectors, low pay occupations, and shut down sectors

	Actual hours	Usual hours	Hourly earnings
Panel A. Low pay sectors			
Treatment	-0.028 ***	-0.030 ***	-0.102 ***
	(0.009)	(0.008)	(0.015)
Treatment X Post April 2021	-0.007	-0.003	0.033
	(0.023)	(0.021)	(0.040)
R-squared	0.127	0.137	0.165
No. observations	11930	13613	3312
Panel B. Low pay occupations			
Treatment	-0.011	-0.009	-0.047 ***
	(0.011)	(0.009)	(0.016)
Treatment X Post April 2021	-0.041	-0.050 **	0.031
	(0.027)	(0.024)	(0.047)
R-squared	0.145	0.161	0.130
No. observations	9924	11400	2691
Panel C. Shutdown sectors			
Treatment	-0.040 ***	-0.047 ***	-0.084 ***
	(0.014)	(0.012)	(0.022)
Treatment X Post April 2021	-0.028	-0.010	0.072
	(0.037)	(0.034)	(0.074)
R-squared	0.113	0.126	0.157
No. observations	6102	7094	1715
Treatment group (ages)	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26
Tax year X Region FE	Y	Y	Y
Tax year X Sector FE	Y	Y	Y

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

[21] An explanation and list of shutdown sectors can be found in the IFS's Briefing Note BN278 – link here.

The estimated impact of the reduction in the age of entitlement on these sectors are presented in Table 14, the results for low pay sectors in Panel A, low pay occupations in Panel B, and shutdown sectors in Panel C. Across all panels there is a positive point estimate of the impact of the policy change on hourly earnings. However, largely due to a small sample size leading to large standard errors, this is not statistically significant.

For low pay sectors in Panel A, the impact of the policy change on actual and usual hours is not significant, and the point estimates are small. The point estimates are larger in Panel C for shutdown sectors, but again are not statistically significant. However, the **estimated impact on actual hours and usual hours worked for those in low pay occupations is more negative** (reductions of 4.1% and 5.0%, respectively) and**, in the case of usual hours worked, statistically significant at the 5 percent level**.

These results are supported by analysis using ASHE that is presented in Section 4.4.

4.3.3 Impact across local area characteristics

Regional differences may also be important in understanding the impact of the reduction in the age of entitlement to the NLW. There may be a proportion of workers who are paid the minimum wage or close to the minimum wage in areas that are less affluent than others. We split the sample into individuals who live in the bottom quintile of the Index of Multiple Deprivation (least deprived areas) and those who live in the top quintile of the Index of Multiple Deprivation (most deprived areas). The Index of Multiple Deprivation aggregates information about income, employment, health and disability, education, crime, housing, and the living environment about the local area (Lower-Layer Super Output Areas – LSOAs). Although similar measures exist in other Home Nations, due to the lack of direct comparability, we restrict the sample to English LSOAs to ensure consistency.

Table 15 presents the results, with estimates for the least deprived areas in Panel A and estimates for the most deprived areas in Panel B. There are no significant impacts on labour market outcomes in the least deprived areas, which is somewhat expected as these are areas that are likely to have the smallest proportions of minimum wage or near-minimum wage workers. However, there is a positive impact on employment for the most deprived areas in Panel B, **an increase in employment probability of 3.7 percentage points**. In context, this closes the majority of the employment gap between 23-/24-year-olds and 26-year-olds before April 2021 of 5.4 percentage points. There is no significant estimated impact on actual hours, usual hours worked or hourly earnings, although the point estimate for hourly earnings is large (8.9%).

Table 15 Heterogeneity analysis: Least and most deprived areas

	Employment	Actual hours	Usual hours	Hourly earnings
Panel A. Least deprived areas				
Treatment	-0.026 ***	-0.028 **	-0.008	-0.104 ***
	(0.007)	(0.012)	(0.019)	(0.025)
Treatment X Post April 2021	0.000	-0.019	-0.045	-0.035
	(0.014)	(0.034)	(0.045)	(0.055)
R-squared	0.040	0.131	0.244	0.305
No. observations	5454	4451	1277	1282
Panel B. Most deprived areas				
Treatment	-0.054 ***	-0.023	0.000	-0.124 ***
	(0.009)	(0.014)	(0.025)	(0.027)
Treatment X Post April 2021	0.037 *	-0.005	0.017	0.089
	(0.021)	(0.036)	(0.064)	(0.057)
R-squared	0.061	0.163	0.270	0.278
No. observations	5536	4248	1223	1226
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y

Note: The least deprived areas include those in the bottom 20% of the distribution of the index of multiple deprivation 2019 (IMD 2019) and the most deprived areas include those in the top 20% of the distribution. For consistency, only LSOAs in England have been included. All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

The specification reported in Table 16 splits the sample by rural or urban areas. Suffering from a small sample issue, the standard errors for estimated impact on those living in rural areas is not significant despite a large negative estimated effect on actual hours (6.9%). Further, besides a positive employment effect in urban areas, there are no significant impact on actual hours, usual hours, or hourly earnings in urban areas. Again, this is potentially likely to be the result of a combination of a small sample as well as including many of those who are less likely to be impacted by changes in the minimum wage.

Table 16 Heterogeneity analysis: Rural and urban areas

	Employment	Actual hours	Usual hours	Hourly earnings
Panel A. Rural areas				
Treatment	-0.003	-0.011	-0.004	-0.097 ***
	(0.007)	(0.014)	(0.024)	(0.025)
Treatment X Post April 2021	0.009	-0.069 **	-0.006	-0.007
	(0.017)	(0.032)	(0.062)	(0.056)
R-squared	0.050	0.148	0.233	0.272
No. observations	5301	4275	1230	1237
Panel B. Urban areas				
Treatment	-0.026 ***	-0.025 ***	-0.015	-0.123 ***
	(0.003)	(0.006)	(0.010)	(0.010)
Treatment X Post April 2021	0.013 *	0.006	0.020	0.025
	(0.007)	(0.014)	(0.023)	(0.026)
R-squared	0.037	0.102	0.149	0.243
No. observations	28291	22867	6476	6495
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y

Note: Rural and urban areas are defined by the 2011 ONS classification (link here). All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

4.3.4 Testing differences in pre-treatment trends for the heterogeneity analysis

As with the baseline specification and the main results in the previous section, we test for differences in pre-treatment trends between age groups for subsets that we focus on in the heterogeneity analysis. The full results are reported in Annex A1.7 in Table 29 to Table 37, using the same specification as when testing for differences in pre-treatment trends for the entire sample.

The estimates of differences in pre-treatment trends are noisy across all subsets of the sample with relatively large estimated standard errors, in particular for hourly earnings. There are some statistically significant differences before April 2021, but not consistently for any groups across time or across labour market outcomes. However, **they do present a caveat to the causal interpretation of the heterogeneity analysis reported**. For example, 23- and 24-year-olds in low pay occupations experienced a statistically significant decrease in actual hours worked (and a large point estimate for usual hours worked) relative to 26-year-olds in the 2020-21 tax year. If this decrease (relative to 26-year-olds) persisted to the 2021-22 tax year, this would result in an overestimation in the negative impact of the policy on hours worked on 23- and 24-year-olds.

4.4 Results using Annual Survey of Hours and Earnings

This section presents the results of estimating the baseline specification using ASHE data. This data is a survey of employees, so as such is restricted to analysis at the internal margin: **hours worked and hourly earnings**. The baseline specification is undertaken using the entire sample (of 23-, 24-, and 26-year-olds) before focusing on low pay sectors and occupations.

4.4.1 Main results

Although the LFS provides a variety of information about personal characteristics and allowed for analysis at the extensive margin, the analysis using the LFS often suffered from a small sample size problem, especially for hourly earnings. To complement the LFS analysis, we undertake analysis using ASHE, using annual data from 2017 to 2021, the most recent version at the time of writing. **The robustness checks, as presented in Annex A2.3 and Annex A2.4, support the use of 26-year-olds as the preferred control group**, and the results are not sensitive to the inclusion of 25-year-olds in the control group.

Table 17 presents the results of estimating the baseline specification using data from ASHE. The first column suggests that there is a negative (-1.1%) impact on hours worked by 23- and 24-year-olds compared to 26-year-olds after April 2021, but this is not significant. Further, the impact on hourly earnings is small and precisely estimated.

Table 17 Baseline specification using ASHE

	Hours worked	Hourly earnings
Treatment	-0.039***	-0.088***
	(0.005)	(0.003)
Treatment X Post April 2021	-0.011	0.008
	(0.013)	(0.008)
Treatment group (ages)	23 & 24	23 & 24
Control group (ages)	26	26
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y
R-squared	0.098	0.216
No. observations	50301	50285

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

Like the main results that used LFS data, the lack of a significant estimated impact is somewhat expected as this includes the entire sample of the included age groups. To estimate the impact of the policy change more precisely on the treated (those who are minimum wage or near-minimum wage workers), the next section focuses on those in low pay sectors and occupations.

4.4.2 Impact on low pay sectors and occupations

The results in Table 18 focus on the impact on those working in low pay sectors and occupations. The heterogeneity analysis focusing on low pay sectors and occupations using the LFS found a negative impact on hours worked for those in low pay occupations, but in some cases small sample sizes hindered the precise estimation of other effects. However, the results in Table 18 show a **negative and statistically significant**

impact on hours worked by those in low pay sectors and occupations: a decrease in hours of 5.2% and 5.5%, respectively. This **more than doubles the gap in hours worked between 23-/24-year-olds compared to 26-year-olds** (the pre-treatment gap is estimated to be 4.3% for low pay sectors and 3.9% for low pay occupations).

There is also a **positive and statistically significant increase in hourly earnings for low pay sectors and occupations of 2.1% and 2.5%, respectively**. This is smaller in magnitude than the decrease in hours worked and is smaller than the difference in increases in the minimum wage between the treatment and control groups. The increase in hourly earnings for 23- and 24-year-olds compared to 26-year-olds cuts the pre-existing gap in earnings between those groups by 29.6% and 67.6% for those in low pay sectors and occupations, respectively.

However, while there are no significant differences in pre-treatment trends in hours worked within low pay sectors and occupations (presented in Table 44 in the Annex), 23- and 24-year-olds experienced a significantly more negative trend in hourly earnings in 2019-20 compared to the control group of 26-year-olds. If 23- and 24-year-olds consistently experienced negative trends in hourly earnings relative to 26-year-olds (including in the hypothetical scenario where they did not become newly eligible to the NLW), this would suggest a potential underestimation of the increase in hourly earnings experienced by 23- and 24-year-olds.

Table 18 Low pay sectors and occupations (ASHE)

	Hours worked	Hourly earnings
Panel A. Low pay sectors		
Treatment	-0.043***	-0.071***
	(0.009)	(0.005)
Treatment X Post April 2021	-0.055***	0.021**
	(0.021)	(0.010)
R-squared	0.058	0.176
No. observations	23685	23681
Panel B. Low pay occupations		
Treatment	-0.039***	-0.037***
	(0.009)	(0.004)
Treatment X Post April 2021	-0.052**	0.025**
	(0.022)	(0.010)
R-squared	0.101	0.088
No. observations	21418	21412
Treatment group (ages)	23 & 24	23 & 24
Control group (ages)	26	26
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

This estimated impact on hours worked is **robust to the inclusion of 25-year-olds as part of the control group**, with a decrease in hours worked for the treatment group of 4.6% (low pay sectors) and 4.2% (low pay occupations), as presented in Table 38 in the Annex.

We further investigate the negative impact on hours worked by estimating the impact of the policy change on full-time employment, and the results in Table 19 suggest that **the decrease in average hours worked is driven by an increase in the proportion of workers who work part time**.

The first column of Table 19 presents the estimated impact of the policy change in low pay sectors and occupations on full-time employment, where the dependent variable takes a value of one if the individual is in full-time employment and a value of zero if the individual is in part-time employment. There is an estimated **decrease in the relative (not absolute) full-time employment probability of 5.0 percentage points (low pay sectors) and 4.3 percentage points (low pay occupations)**. For context, the gap between 23- and 24-year-olds and 26-year-olds in full-time employment probability was 3.3 percentage points (low pay sectors) and 2.2 percentage points (low pay occupations) before April 2021. This supports the analysis using the LFS in Table 5, although the point estimate was not significant.

Table 19 Full-time employment and hours worked by those in full-time employment in low pay sectors and occupations (ASHE)

	Full-time employment	Hours (full-time)
Panel A. Low pay sectors		
Treatment	-0.033***	-0.005*
	(0.007)	(0.003)
Treatment X Post April 2021	-0.050***	0.002
	(0.017)	(0.006)
R-squared	0.077	0.124
No. observations	23962	21675
Panel B. Low pay occupations		
Treatment	-0.022***	-0.006*
	(0.007)	(0.003)
Treatment X Post April 2021	-0.043**	0.004
	(0.018)	(0.007)
R-squared	0.101	0.088
No. observations	21418	21412
Treatment group (ages)	23 & 24	23 & 24
Control group (ages)	26	26
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

The second column takes the baseline specification with hours worked as the dependent variable and focuses on the subsample of those in full-time employment. There is no significant impact on hours worked for those in full-time employment. The point estimates in the second column are small and precisely estimated zeros.

The results of Table 19 suggest that the decrease in average hours worked is driven by a decrease in the proportion of workers who are working full-time rather than a decrease in the hours worked

of those in full-time employment. **It is important to note that the decrease in proportion of workers in full-time employment could be the result of different effects.**

One possibility is a **shift of previously full-time employees to part-time employment**, which would also reduce the average number of hours worked. Another possibility is the **addition of more workers in employment**, of which a disproportionately large number are in part-time employment. The latter would not reduce the hours worked for those already in employment but would reduce the average number of hours worked across those in employment.

The positive employment point estimates found in the analysis using the LFS, albeit rarely statistically significant, as well as a suggestion of a greater proportion of employees working part time in Table 5 and Table 19 is consistent with the latter effect where the addition of part-time workers reduces average hours worked, although not conclusively.

These **results are also robust to the inclusion of 25-year-olds in the control group**, as presented in Table 39 and Table 40 in the Annex. Further, there are **no significant differences in the impact on 23-year-olds compared to the impact on 24-year-olds**, as presented in Annex A2.5.

4.4.3 Impact on male and female workers in low pay sectors and occupations

Given the larger sample in ASHE, we are able to further break down the sample across gender as well as across sector. Table 20 presents the estimated impact of the policy on male and female workers in low pay sectors and occupations.

Table 20 Male and female workers in low pay sectors and occupations

	Hours worked	Hourly earnings	Hours worked	Hourly earnings
Sample	Male	Male	Female	Female
Panel A. Low pay sectors				
Treatment X Post April 2021	-0.044	0.018	-0.061**	0.025*
	(0.030)	(0.016)	(0.029)	(0.014)
R-squared	0.049	0.136	0.059	0.212
No. observations	10,412	10,412	13,273	13,269
Panel B. Low pay occupations				
Treatment X Post April 2021	-0.027	0.033**	-0.082**	0.018
	(0.028)	(0.013)	(0.035)	(0.014)
R-squared	0.098	0.101	0.077	0.087
No. observations	10,744	10,744	10,674	10,668
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	Y	Y	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

The first two columns present the estimated impact on hours worked. Although the point estimates are negative, the impact on hours worked is not significant for male workers, whereas for female workers it is negative, significant, and larger than the average impact estimated in Table 19. In low pay sectors, **23- and 24-year-old women experience a 6.1% decrease in hours worked compared to their 26-year-old counterparts, and an 8.2% decrease in low pay occupations**. The decrease is consistent with women making up a greater proportion of those working part-time than men (Irvine et al., 2022), while **we find that the increase in the proportion of workers working part time is greater for female workers** in Table 38 in the Annex.

The relative impact on hourly earnings between male and female workers is less clear, with the point estimate for hourly earnings greater for women in low pay sectors (2.5% increase) and greater for men in low pay occupations (3.3% increase).

5 Conclusions

We investigate the impact of the reduction in the age of entitlement to the NLW from the age of 25 to the age of 23 in April 2021. In particular, we estimate the impact of the policy on 23- and 24-year-olds, who were newly eligible for the NLW April 2021 and **experienced a significant increase in their relevant minimum wage compared to other age groups**. Those of similar ages who were not newly eligible for the NLW in April 2021 are explored as potential counterfactuals for 23- and 24-year-olds. Visual inspection of descriptive statistics and more formal testing through the comparison of pre-treatment differences in trends suggest that 22- and 25-year-olds are not suitable comparators, so 26-year-olds are chosen as the preferred control group.

There appears to be **no significant impact on labour market outcomes when comparing the entire sample of 23- and 24-year-olds to the entire sample of 26-year-olds.** We do not find any evidence of a negative impact on employment. In fact, we consistently find a small positive point estimate for employment across almost all subgroups as well as the entire sample of 23- and 24-year-olds, although these estimates are statistically insignificant.

This may also be potentially driven by the sample including many who earn significantly above the relevant minimum wage and are unlikely to be affected by changes to the minimum wage. As a result, we undertake heterogeneity analysis, in some cases focusing on groups that may be more likely to be affected by changes to the minimum wage and find no negative employment effects in almost all groups. We undertake a number of robustness checks to check the sensitivity of the main results.

In some cases, the sample sizes are not sufficiently large to make meaningful conclusions about the impact of the policy change on specific groups. Further, the lack of a significant estimated impact on hourly earnings may limit the causal interpretation of estimates focusing on some subgroups. However, we estimate **a negative and significant impact on hours worked by those in low pay sectors and low pay occupations**, which is greater in magnitude than the statistically significant increase in hourly earnings. In the case of low pay occupations, the same conclusion is reached using different data sources (the LFS and ASHE).

The **negative impact on hours worked is particularly large for female workers**. We find evidence that this is driven by an **increase in the proportion of employees who are in part-time employment**, with no significant impact on the hours worked by those in full-time employment. However, it is not possible to cleanly identify the extent to which it is driven by workers previously working full time who are now working part time or by the addition of new employees into the active labour force who are working on a part time basis to a disproportionate extent and so reduce the average hours worked across the sample.

Further research would explore the longer-term impact of the reduction on labour market outcomes. In addition, further work would investigate the mechanisms driving labour market outcomes as a result of the reduction, such the decrease in average hours worked for some subsets of the population.

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Index of Tables, Figures and Boxes

Tables

Figures

Boxes

Annex 1 Supplementary analysis using LFS data

A1.1 Alternative treatment and control groups

Table 21 Impact of the reduction in age of entitlement: including 25-year-olds

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.020 ***	-0.020 ***	-0.023 ***	-0.095 ***
	(0.002)	(0.004)	(0.004)	(0.008)
Treatment X Post April 2021	0.009	-0.020 *	-0.014	0.009
	(0.006)	(0.010)	(0.009)	(0.018)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	25 & 26	25 & 26	25 & 26	25 & 26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.038	0.108	0.133	0.216
No. observations	48095	38966	43668	11131

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 22 Impact of the reduction in age of entitlement (22- and 23-year-olds)

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	0.028 ***	0.053 ***	0.065 ***	0.052 ***
	(0.005)	(0.009)	(0.006)	(0.010)
Treatment X Post April 2021	-0.016 *	-0.009	-0.023	-0.016
	(0.010)	(0.019)	(0.014)	(0.029)
Treatment group (ages)	23	23	23	23
Control group (ages)	22	22	22	22
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.043	0.114	0.137	0.166
No. observations	21694	17029	18944	4579

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

A1.2 Differences in impact between 23- and 24-years-olds

Table 23 Testing for heterogeneity in impact between 23- and 24-year-olds

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	0.003	0.024 ***	0.018 ***	0.041 ***
	(0.004)	(0.007)	(0.006)	(0.012)
Treatment X Post April 2021	0.012	-0.010	0.006	0.008
	(0.009)	(0.016)	(0.014)	(0.027)
Treatment group (ages)	24	24	24	24
Control group (ages)	23	23	23	23
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.039	0.102	0.124	0.188
No. observations	22944	18327	20467	5032

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

A1.3 Inclusion of month of birth and regression discontinuity analysis

Table 24 Inclusion of month of birth

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.005	0.009	0.012	-0.038
	(0.009)	(0.017)	(0.014)	(0.032)
Treatment X Post April 2021	0.008	-0.005	-0.003	0.017
	(0.006)	(0.013)	(0.011)	(0.023)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.037	0.103	0.131	0.225
No. observations	35514	28633	32140	8147

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Month of birth is included (normalised by the age of entitlement threshold) as well as its quadratic form. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Regression discontinuity design

As a robustness check, we estimate the impact of the reduction using a regression discontinuity design. Like the baseline specification, the identification strategy exploits the differences in minimum wage policy between age groups. Unlike the baseline specification, 25-year-olds are included and the impact of month of birth (and its quadratic) are allowed to differ on either side of the age threshold (exactly 25 years). For the sake of clarity, the identification strategy can be summarised as

$$y_{it} = \alpha_{\mathrm{rt}} + \alpha_{st} + \beta_1 T_i + \beta_2 POST_t + \beta_3 (T_i \times POST_t) + X\boldsymbol{\gamma} + \delta_1 (\mathrm{M_{it}} - 25 \times 12) + \delta_2 (\mathrm{M_{it}} - 25 \times 12)^2 + \delta_3 \, \mathrm{T}_i \times (\mathrm{M_{it}} - 25 \times 12) + \delta_4 \, \mathrm{T}_i \times (\mathrm{M_{it}} - 25 \times 12)^2 + \epsilon_{it} \quad (5)$$

where M_{it} is the age of the individual in months, and an individual is $M_{it} - 25 \times 12$ months older than 25 years, the previous NLW age threshold. The last two terms allow for different impacts of the month of birth on either side of the age threshold (25 years old).

The results of the regression discontinuity design are reported in Table 25. The point estimates in Panel A (entire sample of 23- to 26-year-olds) are similar to the baseline specification results, but the employment effect is positive and significant (0.9 percentage points) and the actual hours effect is negative and significant (1.9% decrease). However, the lack of a significant impact on hourly earnings may limit the causal interpretation between the policy change and the significant changes in other labour market outcomes.

Panels B and C focus on those working in low pay sectors and occupations. There are insignificant effects on the labour market outcomes of low pay sector workers, but a significant and negative effect on actual and usual hours worked by those in low pay occupations (8.5% decrease in actual

hours worked and a 9.2% decrease in usual hours worked). Hourly earnings are estimated to have risen by 13% for the treatment group relative to the control group, but this is statistically insignificant. The relatively large negative impact on hours is consistent with our findings using ASHE data for low pay occupations.

The difference in results when focusing on low pay sectors and low pay occupations suggests that these results may be sensitive to the inclusion or exclusion of a subset of workers, as there is a considerable overlap between those working in low pay sectors and those working in low pay occupations.

Table 25 Regression discontinuity design

	Employment	Actual hours	Usual hours	Hourly earnings
Panel A. Whole sample				
Treatment	-0.005	-0.005	-0.012	0.015
	(0.007)	(0.012)	(0.010)	(0.021)
Treatment X Post April 2021	0.009 *	-0.019 *	-0.013	0.007
	(0.006)	(0.010)	(0.009)	(0.018)
R-squared	0.037	0.104	0.130	0.216
No. observations	47643	38562	43247	11058
Panel B. Low pay sectors				
Treatment		-0.020	-0.028	0.019
		(0.021)	(0.019)	(0.031)
Treatment X Post April 2021		-0.029	-0.016	0.008
		(0.019)	(0.017)	(0.032)
R-squared		0.122	0.134	0.151
No. observations		15666	17872	4386
Panel B. Low pay occupations				
Treatment		0.000	-0.022	-0.024
		(0.026)	(0.023)	(0.034)
Treatment X Post April 2021		-0.085 *	-0.092 **	0.130
		(0.044)	(0.040)	(0.085)
R-squared		0.147	0.167	0.118
No. observations		10820	12385	2968
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	25 & 26	25 & 26	25 & 26	25 & 26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Month of birth is included (normalised by the age of entitlement threshold) as well as its quadratic form, and interactions with the treatment variable are included for both variables to allow for the impact of month of birth and its quadratic form to vary on either side of the age of entitlement threshold. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

A1.4 Excluding the 2020-21 tax year

Table 26 Excluding 2020-21 tax year

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.019 ***	-0.019 ***	-0.021 ***	-0.120 ***
	(0.003)	(0.006)	(0.005)	(0.011)
Treatment X Post April 2021	0.004	-0.013	-0.010	0.021
	(0.007)	(0.013)	(0.011)	(0.023)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.037	0.114	0.138	0.234
No. observations	29660	24329	26888	6841

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

A1.5 Excluding the furlough variable

Table 27 Excluding the furlough variable

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.022 ***	-0.024 ***	-0.029 ***	-0.120 ***
	(0.003)	(0.005)	(0.004)	(0.011)
Treatment X Post April 2021	0.006	-0.006	-0.004	0.021
	(0.006)	(0.013)	(0.011)	(0.023)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.038	0.103	0.133	0.222
No. observations	35841	28921	32446	8197

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, but not an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

A1.6 Spillover effects on adjacent age groups

Table 28 Relative spillover effects (25- and 26-year-olds compared to 21- and 22-year-olds)

	Employment	Actual hours	Usual hours	Hourly earnings
25/26 years old	0.053 ***	0.105 ***	0.118 ***	0.173 ***
	(0.003)	(0.006)	(0.005)	(0.010)
25/26 years old X Post April 2021	-0.015 **	0.002	-0.011	-0.023
	(0.007)	(0.013)	(0.011)	(0.021)
Treatment group (ages)	25 & 26	25 & 26	25 & 26	25 & 26
Control group (ages)	21 & 22	21 & 22	21 & 22	21 & 22
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.047	0.133	0.163	0.244
No. observations	44863	35758	40001	10039

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

A1.7 Differences in pre-treatment trends for different groups

Table 29 Differences in pre-treatment trends: male workers

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.027 ***	-0.019	-0.031 ***	-0.113 ***
	(0.008)	(0.012)	(0.010)	(0.025)
Treatment X 2018-19 tax year	0.010	-0.007	0.003	-0.052
	(0.012)	(0.017)	(0.015)	(0.039)
Treatment X 2019-20 tax year	-0.011	-0.001	-0.011	0.041
	(0.012)	(0.018)	(0.015)	(0.042)
Treatment X 2020-21 tax year	-0.026 **	-0.034	-0.023	-0.015
	(0.013)	(0.021)	(0.016)	(0.045)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.048	0.097	0.127	0.238
No. observations	17392	14183	15526	3784

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 30 Differences in pre-treatment trends: female workers

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.014 *	-0.015	-0.004	-0.121 ***
	(0.007)	(0.014)	(0.012)	(0.024)
Treatment X 2018-19 tax year	-0.006	0.012	-0.012	0.044
	(0.010)	(0.020)	(0.017)	(0.033)
Treatment X 2019-20 tax year	0.008	-0.047 **	-0.027	-0.048
	(0.010)	(0.021)	(0.018)	(0.034)
Treatment X 2020-21 tax year	-0.010	0.002	-0.015	0.009
	(0.011)	(0.024)	(0.019)	(0.038)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.037	0.138	0.162	0.230
No. observations	18449	14738	16920	4413

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 31 Differences in pre-treatment trends: non-white workers

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.014	-0.076 **	-0.102 ***	-0.154 *
	(0.020)	(0.034)	(0.029)	(0.079)
Treatment X 2018-19 tax year	-0.014	0.029	0.043	0.045
	(0.029)	(0.046)	(0.042)	(0.113)
Treatment X 2019-20 tax year	-0.008	-0.006	-0.026	-0.117
	(0.031)	(0.046)	(0.042)	(0.110)
Treatment X 2020-21 tax year	0.000	-0.017	0.033	0.117
	(0.036)	(0.063)	(0.052)	(0.150)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.065	0.181	0.179	0.396
No. observations	4022	3008	3361	775

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 32 Differences in pre-treatment trends: disabled workers

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.025	-0.074 **	-0.018	-0.107 **
	(0.022)	(0.037)	(0.031)	(0.045)
Treatment X 2018-19 tax year	-0.025	0.075	-0.010	-0.094
	(0.030)	(0.053)	(0.043)	(0.080)
Treatment X 2019-20 tax year	0.017	-0.060	-0.074 *	0.043
	(0.028)	(0.053)	(0.043)	(0.095)
Treatment X 2020-21 tax year	0.001	0.028	0.071	0.134
	(0.029)	(0.056)	(0.045)	(0.097)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.072	0.132	0.161	0.286
No. observations	4807	3527	4115	1158

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 33 Differences in pre-treatment trends: workers with children

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.027 **	0.057 **	0.039 *	-0.164 ***
	(0.013)	(0.026)	(0.021)	(0.047)
Treatment X 2018-19 tax year	-0.005	-0.023	0.001	0.055
	(0.019)	(0.038)	(0.032)	(0.065)
Treatment X 2019-20 tax year	-0.003	-0.053	-0.051	0.010
	(0.021)	(0.039)	(0.034)	(0.074)
Treatment X 2020-21 tax year	0.014	0.064	0.021	-0.035
	(0.025)	(0.054)	(0.038)	(0.083)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.063	0.262	0.273	0.204
No. observations	6066	4353	5394	1417

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 34 Differences in pre-treatment trends: low educational attainment

	Employment	Actual hours	Usual hours	Hourly earnings
Treatment	-0.012	0.045 **	0.016	-0.106 ***
	(0.014)	(0.021)	(0.018)	(0.036)
Treatment X 2018-19 tax year	0.004	-0.031	0.001	0.080
	(0.020)	(0.032)	(0.029)	(0.055)
Treatment X 2019-20 tax year	-0.022	-0.003	-0.005	-0.058
	(0.022)	(0.035)	(0.032)	(0.056)
Treatment X 2020-21 tax year	-0.009	-0.077 *	-0.047	0.013
	(0.027)	(0.042)	(0.035)	(0.057)
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	N	Y	Y	Y
R-squared	0.067	0.217	0.234	0.193
No. observations	7237	5483	6250	1529

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 35 Differences in pre-treatment trends: low pay sectors

	Actual hours	Usual hours	Hourly earnings
Treatment	-0.023	-0.020	-0.094 ***
	(0.016)	(0.014)	(0.026)
Treatment X 2018-19 tax year	0.019	0.010	0.009
	(0.024)	(0.021)	(0.037)
Treatment X 2019-20 tax year	-0.040	-0.033	-0.014
	(0.025)	(0.022)	(0.039)
Treatment X 2020-21 tax year	-0.015	-0.016	-0.051
	(0.030)	(0.025)	(0.049)
Treatment group (ages)	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26
Tax year X Region FE	Y	Y	Y
Tax year X Sector FE	Y	Y	Y
R-squared	0.127	0.137	0.165
No. observations	11930	13613	3312

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 36 Differences in pre-treatment trends: low pay occupations

	Actual hours	Usual hours	Hourly earnings
Treatment	-0.001	-0.038	-0.006
	(0.016)	(0.027)	(0.018)
Treatment X 2018-19 tax year	0.017	-0.001	0.017
	(0.024)	(0.040)	(0.027)
Treatment X 2019-20 tax year	-0.024	0.009	-0.032
	(0.026)	(0.042)	(0.029)
Treatment X 2020-21 tax year	-0.059 **	-0.078	-0.033
	(0.029)	(0.050)	(0.036)
Treatment group (ages)	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26
Tax year X Region FE	Y	Y	Y
Tax year X Sector FE	Y	Y	Y
R-squared	0.145	0.161	0.131
No. observations	9924	11400	2691

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: LFS and London Economics' own calculations

Table 37 Differences in pre-treatment trends: shutdown sectors

	Actual hours	Usual hours	Hourly earnings
Treatment	-0.033	-0.019	-0.073 **
	(0.024)	(0.021)	(0.035)
Treatment X 2018-19 tax year	0.043	0.000	0.000
	(0.035)	(0.032)	(0.053)
Treatment X 2019-20 tax year	-0.081 **	-0.069 **	-0.011
	(0.038)	(0.034)	(0.059)
Treatment X 2020-21 tax year	-0.046	-0.002	-0.044
	(0.048)	(0.038)	(0.076)
Treatment group (ages)	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26
Tax year X Region FE	Y	Y	Y
Tax year X Sector FE	Y	Y	Y
R-squared	0.115	0.127	0.157
No. observations	6102	7094	1715

Note: All specifications include control variables (gender, disability status, ethnicity, marital status, children, educational attainment, and an indication of whether furlough influenced the outcome variable) and region-time fixed effects. Sector-time fixed effects are not included for the employment specification as the sample includes those who are not in employment and therefore not attributable to a particular sector. The LFS sample includes those who are employees and unemployed. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: LFS and London Economics' own calculations

Annex 2 Supplementary analysis using ASHE data

A2.1 Full-time employment and hours worked by gender

Table 38 Full-time employment and hours worked by those in full-time employment (male and female workers in low pay sectors and occupations)

	Full-time employment	Full-time employment	Hours (full-time employment)	Hours (full-time employment)
Sample	Male	Female	Male	Female
Panel A. Low pay sectors				
Treatment X Post April 2021	-0.020	-0.063**	0.007	0.001
	(0.030)	(0.027)	(0.011)	(0.009)
R-squared	0.062	0.082	0.044	0.042
No. observations	6,977	9,158	4,837	5,476
Panel B. Low pay occupations				
Treatment X Post April 2021	-0.007	-0.055*	0.011	-0.013
	(0.029)	(0.032)	(0.011)	(0.012)
R-squared	0.111	0.099	0.066	0.071
No. observations	7,162	7,100	5,068	3,704
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	Y	Y	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

A2.2 Inclusion of 25-year-olds in the control group

Table 39 Low pay sectors and occupations (ASHE) – including 25-year-olds

	Hours worked	Hourly earnings
Panel A. Low pay sectors		
Treatment	-0.045***	-0.059***
	(0.007)	(0.004)
Treatment X Post April 2021	-0.046***	0.016*
	(0.017)	(0.009)
R-squared	0.054	0.176
No. observations	31,597	31,587
Panel B. Low pay occupations		
Treatment	-0.043***	-0.031***
	(0.008)	(0.004)
Treatment X Post April 2021	-0.042**	0.014
	(0.019)	(0.009)
R-squared	0.100	0.086
No. observations	28,387	28,378
Treatment group (ages)	23 & 24	23 & 24
Control group (ages)	25 & 26	25 & 26
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

Table 40 Full-time employment and hours worked by those in full-time employment in low pay sectors and occupations (ASHE) – including 25-year-olds

	Full-time employment	Hours (full-time)
Panel A. Low pay sectors		
Treatment	-0.034***	-0.006***
	(0.006)	(0.002)
Treatment X Post April 2021	-0.040***	-0.000
	(0.014)	(0.005)
R-squared	0.074	0.044
No. observations	31,974	19,963
Panel B. Low pay occupations		
Treatment	-0.024***	-0.006**
	(0.006)	(0.002)
Treatment X Post April 2021	-0.039***	0.004
	(0.015)	(0.006)
R-squared	0.123	0.058
No. observations	28,736	17,377
Treatment group (ages)	23 & 24	23 & 24
Control group (ages)	25 & 26	25 & 26
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

A2.3 Differences in pre-treatment trends

Table 41 Testing differences in pre-treatment trends (Control group: 26-year-olds)

	Hours worked	Hourly earnings
Treatment	-0.030***	-0.088***
	(0.010)	(0.006)
Treatment X 2018-19 tax year	-0.006	-0.006
	(0.014)	(0.009)
Treatment X 2019-20 tax year	-0.015	0.003
	(0.015)	(0.009)
Treatment X 2020-21 tax year	-0.020	0.005
	(0.016)	(0.010)
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y
R-squared	0.097	0.211
No. observations	42,118	42,103

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

Table 42 Testing differences in pre-treatment trends (Control group: 22-year-olds)

	Hours worked	Hourly earnings
Treatment	0.075***	0.061***
	(0.013)	(0.006)
Treatment X 2018-19 tax year	0.031*	0.001
	(0.018)	(0.009)
Treatment X 2019-20 tax year	0.027	0.012
	(0.019)	(0.009)
Treatment X 2020-21 tax year	0.007	-0.008
	(0.020)	(0.011)
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y
R-squared	0.113	0.185
No. observations	38,379	38,361

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

Table 43 Testing differences in pre-treatment trends (Control group: 25-year-olds)

	Hours worked	Hourly earnings
Treatment	-0.019*	-0.061***
	(0.010)	(0.006)
Treatment X 2018-19 tax year	-0.007	-0.001
	(0.015)	(0.009)
Treatment X 2019-20 tax year	-0.030**	0.015*
	(0.015)	(0.009)
Treatment X 2020-21 tax year	-0.031*	-0.001
	(0.016)	(0.010)
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y
R-squared	0.097	0.203
No. observations	41,641	41,624

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

Table 44 Testing differences in pre-treatment trends (Control group: 26-year-olds) for low pay sectors and occupations

	Hours worked	Hourly earnings	Hours worked	Hourly earnings
Sample	Low pay sectors	Low pay sectors	Low pay occupations	Low pay occupations
Treatment	-0.025	-0.059***	-0.024	-0.024***
	(0.015)	(0.008)	(0.016)	(0.008)
Treatment X 2018-19 tax year	-0.023	-0.020	-0.010	-0.018
	(0.023)	(0.012)	(0.024)	(0.012)
Treatment X 2019-20 tax year	-0.021	-0.022*	-0.026	-0.025**
	(0.024)	(0.011)	(0.026)	(0.011)
Treatment X 2020-21 tax year	-0.035	-0.008	-0.031	-0.009
	(0.027)	(0.015)	(0.029)	(0.015)
R-squared	0.055	0.166	0.098	0.079
No. observations	19,825	19,821	17,972	17,966
Treatment group (ages)	23 & 24	23 & 24	23 & 24	23 & 24
Control group (ages)	26	26	26	26
Tax year X Region FE	Y	Y	Y	Y
Tax year X Sector FE	Y	Y	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

A2.4 Placebo treatment and control groups

Table 45 Full-time employment and hours worked by those in full-time employment in low pay sectors and occupations (ASHE)

	Hours worked	Hourly earnings
Panel A. Treatment = 22, Control = 21		
Treatment	0.101***	0.048***
	(0.009)	(0.004)
Treatment X Post April 2021	-0.018	-0.007
	(0.023)	(0.011)
R-squared	0.152	0.125
No. observations	25,857	25,845
Panel B. Treatment = 25, Control = 26		
Treatment	-0.005	-0.030***
	(0.006)	(0.004)
Treatment X Post April 2021	0.010	0.010
	(0.014)	(0.009)
R-squared	0.092	0.215
No. observations	35,687	35,672
Panel C. Treatment = 26, Control = 27		
Treatment	0.004	-0.029***
	(0.006)	(0.004)
Treatment X Post April 2021	0.002	-0.007
	(0.014)	(0.009)
R-squared	0.095	0.225
No. observations	36731	36715
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

Source: ASHE and London Economics' own calculations

A2.5 Testing differences between 23- and 24-year-olds

Table 46 Baseline specification using ASHE (23- and 24-year-olds)

	Hours worked	Hourly earnings
Treatment	0.034***	0.039***
	(0.007)	(0.004)
Treatment X Post April 2021	-0.004	-0.003
	(0.016)	(0.009)
Treatment group (ages)	24	24
Control group (ages)	23	23
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y
R-squared	0.104	0.197
No. observations	32,075	32,066

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

Table 47 Low pay sectors and occupations (ASHE) (23- and 24-year-olds)

	Hours worked	Hourly earnings
Panel A. Low pay sectors		
Treatment	0.052***	0.028***
	(0.011)	(0.005)
Treatment X Post April 2021	0.007	-0.010
	(0.025)	(0.011)
R-squared	0.062	0.147
No. observations	15,639	15,636
Panel B. Low pay occupations		
Treatment	0.035***	0.012**
	(0.011)	(0.005)
Treatment X Post April 2021	0.011	-0.004
	(0.028)	(0.011)
R-squared	0.102	0.085
No. observations	14,456	14,453
Treatment group (ages)	24	24
Control group (ages)	23	23
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

Table 48 Full-time employment and hours worked by those in full-time employment in low pay sectors and occupations (ASHE) (23- and 24-year-olds)

	Full-time employment	Hours (full-time)
Panel A. Low pay sectors		
Treatment	0.042***	0.022***
	(0.008)	(0.008)
Treatment X Post April 2021	0.009	0.022
	(0.020)	(0.021)
R-squared	0.079	0.127
No. observations	15,805	14,617
Panel B. Low pay occupations		
Treatment	0.003	0.005
	(0.003)	(0.004)
Treatment X Post April 2021	0.002	-0.007
	(0.008)	(0.009)
R-squared	0.054	0.073
No. observations	9,437	8,547
Treatment group (ages)	24	24
Control group (ages)	23	23
Tax year X Region FE	Y	Y
Tax year X Sector FE	Y	Y

Note: All specifications include control variables (gender and an indication of whether furlough influenced the outcome variable) and region-time and sector-time fixed effects. Robust standard errors are presented in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.10.

Source: ASHE and London Economics' own calculations

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