Assessing the impact of the minimum wage on young people using Longitudinal Education Outcomes data

LE
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Executive Summary

This study investigates the impact of the minimum wage on employment and participation in education, focusing on the introduction of the National Living Wage (NLW) in April 2016. Understanding the impact on educational participation in particular is important for policymakers to understand for two key reasons, besides expanding the currently sparse evidence base.

- ■ Firstly, any impact that the minimum wage has on young people's **human capital investment may have long-term consequences** for their future labour market outcomes.
- ■ Secondly, it provides policymakers with an understanding of how the minimum wage may **impact short-term labour supply**, especially among young people.

There are several potential ways that changes to the minimum wage could impact young people's educational participation. For example, higher minimum wages may result in a **truncated pay distribution** that reduces the incentive for some to undertake education and training with smaller associated pay increases.

This study is the first to use the Longitudinal Education Outcomes (LEO) data tables to estimate the impact of the minimum wage on employment, earnings, and participation in education. LEO comprises **detailed information on individuals' educational journey from Early Years education through all key stages, as well as further and higher education**. Given that the data is administrative, LEO captures every individual born after 1985/86 who has engaged with the school system in England at some point. The data also contains detailed personal characteristics as well as information on employment, earnings, sector and location from DWP and HMRC tax records. However, it is **not possible to estimate hourly pay** using the information available in LEO.

The **introduction of the NLW in April 2016**, and its coverage of those aged 25 and older, was used to estimate the impact of the minimum wage by tracking the outcomes of those just eligible for the NLW in April 2016 (those aged 25 at the time) and comparing them to those just ineligible (those aged 24 at the time).

The analysis does not estimate **any statistically significant effects of the NLW on educational participation**, neither for overall education nor on participation in further education, undergraduate education, postgraduate education, or apprenticeships. In addition, the analysis **does not estimate a statistically significant impact on labour market outcomes** such as earnings or stability of employment.

The lack of an estimated impact could be driven by a variety of factors, such as the relatively small proportion of 24- and 25-year-olds enrolled in education or the difficulty in focusing on those most likely to be impacted by changes to the minimum wage without hourly pay information.

When more recent data is available, further research could use LEO to assess the **impact of more recent changes to the minimum wage among younger people who are more likely to be enrolled in education**, such as the reduction of the age of entitlement from age 25 to 23 in April 2021 and from age 23 to 21 in April 2024.

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1 Introduction

1.1 Purpose and scope of the research

This study explores the use of the **Longitudinal Education Outcomes (LEO)** data tables in assessing the impact of the minimum wage on young people, focusing on the introduction of the National Living Wage in April 2016 on young people's educational participation and labour market outcomes.

While the impact of changes to the minimum wage on young people's labour market outcomes (such as hours worked, earnings, or employment) is widely studied (London Economics, 2022), the literature on the **impact of minimum wage changes on educational participation** in the UK (Marioni et al., 2024) as well as long-term labour market outcomes is relatively scarce, highlighting a key contribution of this study.

In addition, this research is **the first study to use the Longitudinal Education Outcomes (LEO) data to investigate the impact of minimum wage changes on young people**. As a result, this piece of research also aims to evaluate the potential of LEO data for future research on the effect of the minimum wage on young people.

1.2 Recent trends in the minimum wage

Wages for younger people have increased substantially in recent years due to a combination of

- increases in the relevant rates of the **National Minimum Wage (NMW)**, and
- **lowering the age of entitlement** to the **National Living Wage (NLW)**.

These policy changes aim to improve living standards for younger people, reduce financial disparity, and encourage economic participation, while simplifying wage structures and promoting fairness in the labour market.

The **NLW was introduced in April 2016 for individuals aged 25 and over**, resulting in a 7.5% increase from £6.70 to £7.20 in April, following the previous increase from £6.50 to £6.70 in October 2015. Hence, those **aged 25 and older experienced a 10.8% increase in their minimum wage rate within a year**. Within that same time period, those aged 21 to 24 only experienced a 3.1% increase from £6.50 to £6.70 in October 2015, no uplift in April 2016, and a 3.7% increase in October 2016.

The introduction of the NLW in April 2016 resulted in a **significant increase in the minimum wage rate for people aged 25 and older relative to the minimum wage for those aged 24 and younger.** This research **investigates the impact of the introduction of the NLW on young people** by exploiting the age eligibility threshold of 25 years.

More recently, following the Low Pay Commission's remit to maintain the NLW at **two-thirds of the median hourly earnings**, the NLW was increased by 9.8% in April 2024, to £11.44. The NLW was further raised by 6.7% in April 2025 to £12.21. Additionally, the **minimum age for eligibility NLW had been reduced from 25 to 23 in April 2021**, **and further to 21 in April 2024**. The most recent change resulted in a **12.4% increase in the minimum wage rate for those aged 21-22 in absolute terms**. However, before this change, the 21-22 rate was only 24p below the NLW (a difference of only 2.3%), so most of the gap between rates had already been bridged.

1.3   Recent trends in young people's labour market outcomes

When defining low-paid jobs as those being paid below two-thirds of median hourly earnings, the proportion of all employees (across all ages) that are low paid is at record lows (2.5% in April 2025[1]). Those at the bottom end of the wage distribution tend to be younger (e.g., aged 16-21), which suggests that further extensions of eligibility may result in further wage compression.

Looking specifically at youth unemployment, recent trends show a **steady increase in the unemployment rate for 18- to 24-year-olds since mid-2022**, with the rate for this group now standing at 12.7%[2], although there have been consistent quality concerns raised about the Labour Force Survey over this period[3]. The unemployment rate among economically active 16- and 17-year-olds has been rising since mid-2023 and currently stands at 36.0%[4]. It is important to understand to what extent these increases can be attributed to concurrent increases in the minimum wage for young people.

A key contribution of this study is the impact of changes in the minimum wage on **education participation**. For example, in the UK, the total number of students in further education at further education colleges and higher education providers increased in both 2021/22 (by 4.8%) and 2022/23 (by 4.1%) after a constant decline in the years before, with a 20.4% decrease from 2016/17 to 2021/22 that included pre-pandemic decreases of 2.7% and 3.2% in 2017/18 and 2018/19[5]. The number of higher education students also increased in those years but at a slower rate, with the proportion of 18- to 20-year-olds enrolled in higher education increasing from 27% to 32% between 2014/15 and 2022/23[6].

However, the proportion of young people who are **not in education, employment, or training (NEET)** has also been increasing since mid-2022, with **12.8% of 16- to 24-year-olds classified as NEET** in the second quarter of 2025[7].

As existing evidence on the impacts of changes in the minimum wage on education participation is sparse, **investigating this relationship contributes to policymakers' understanding of strategies to reduce NEET rates** and how changes to the minimum wage interact with **investment in human capital** that have long-term consequences.

1.4   Potential impacts of the minimum wage on young people's labour market outcomes and educational participation

Figure 1 provides an overview of the key impacts (in relation to the specific research objectives) following a change in minimum wage.[8]

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[1] ONS (2025). Low and high pay in the UK: 2025. Statistical bulletin. Link here.

[2] Unemployment rate (aged 18-24). Q3 2025. ONS (2025). Link here.

[3] Economics Observatory (2025), link here.

[4] ONS (2025). Educational status and labour market status for people aged from 16 to 24 (seasonally adjusted). Link here.

[5] Department for Education (2025). Education and training statistics for the UK. Link here.

[6] Social Mobility Commission (2024). State of the Nation – Entry to higher education. Link here.

[7] Department for Education (2025). Education and training statistics for the UK. Link here.

[8] There are a range of other impacts from a minimum wage increase (not explored in detail here), including: productivity and quality of service. prices; public finances, income inequality and poverty reduction.

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■ A higher minimum wage can lead to a **higher weekly salary** for employees if the working hours stay constant. This higher disposable income of low-wage workers can potentially have effects throughout the supply chain, leading to job creation. On the other hand, employers may decide to reduce their demand for **working hours** to offset the higher cost of labour, resulting in unchanged weekly salaries.

■ There might also be **spillover effects** to employees' wages, as workers higher up in the wage distribution may demand a higher wage. However, if wage rates at the bottom of the distribution rise at a faster rate due to uprates in the NLW, this will lead to a **compression in the wage distribution.**

■ Increases in the **minimum wage may increase or decrease education participation, highlighting the importance of research on potential causal mechanisms**. For example, a potential **reduction in the earnings premium** attained through higher/further education and the **increase in the opportunity cost of education** (foregone earnings) may reduce demand for education, as young people may decide to enter or stay in the labour market instead (**substitution effect**). Alternatively, a higher minimum wage might allow more people to finance their education while working a minimum wage job alongside their education and hence have a positive effect on educational participation (**income effect**).

■ Where employees are paid at minimum wage or where spillover effects occur, a higher minimum wage implies **higher costs to employers**. In theory, this could lead to **job losses, a reduction in the demand for hours worked** or **worker substitution**, especially in low-wage sectors, where employees are predominantly young and female. Additionally, employers in the production sector can decide to shift work towards **other age groups** or to more capital-intensive and **less labour-intensive production**. The higher cost to employers can also impact end customers if prices are passed on.

■ Firms can either increase investments in **on-the-job training for higher-paid employees** to use their labour more efficiently, or they can **reduce the investment in training opportunities to save costs**, particularly for low-wage workers, especially in sectors with high turnover. Higher labour costs can also result in fewer entry-level positions available, which might delay **career development**.

Figure 1   Overview of potential key impacts of an increase in minimum wage
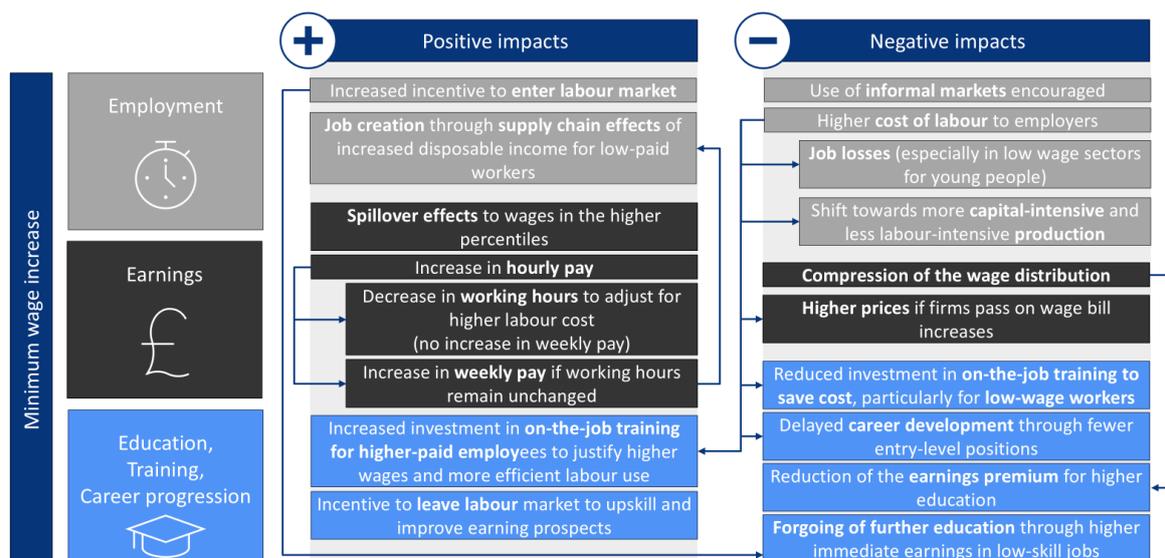
Note: This diagram aims to give a high-level overview of key impacts only, so the list is not exhaustive.

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1.5   Existing evidence on the impact on young people's labour market outcomes and educational participation

1.5.1   Existing evidence on the impact on young people's labour market outcomes

London Economics (2022) investigated the **reduction in the age of entitlement to the NLW from 25 to 23**, implemented in April 2021. The study employed a difference-in-differences approach, comparing the employment and wage outcomes of 23- and 24-year-olds, newly eligible for the NLW, to those of similar ages who were not affected by the policy change. The results of the study suggested that the reduction in the age of NLW eligibility had **no significant impact on employment or earnings of 23- and 24-year-olds** overall. However, a **negative impact on hours worked** was identified for individuals employed in low-pay sectors, where the reduction in hours was greater than the increase in hourly earnings. This was particularly the case for part-time workers, with the transition from full-time to part-time work driving these effects. Additionally, the negative impact on hours and the increase in part-time employment were more pronounced amongst female workers. The research also highlighted a key challenge, which was the identification of a plausible counterfactual group. Giupponi and Machin (2022) also studied the impact of the NLW introduction by exploiting the **age-specific wage change** and found positive effects on wages but little evidence of adverse employment effects, and Giupponi and Machin (2018) found evidence of positive wage spillovers on workers under the age of 25.

Dickens, Riley, and Wilkinson (2010) found evidence of a positive impact on the likelihood of low-skilled individuals being employed by exploiting the relative rate increase individuals experience when they turn 22 and become eligible for the adult rate (before the age threshold change in 2010). Fidrmuc and Tena (2013) found no negative effect of turning 22 on employment but found that firms may lay off younger workers in anticipation of higher minimum wages.

Previous research has not found negative employment effects of minimum wage changes on young people in the UK. Looking at the impact of the change in the age threshold of the NMW adult rate in October 2010 from 22 to 21, London Economics (2015) found no evidence of negative employment effects for 21-year-olds. Crawford, Greaves, Jin, Swaffield, and Vignoles (2011) found no causal effects of the changes in the 16-17 youth rate or the 18-21 development rate on the employment choices of young people. There is some evidence of a negative impact on hours in low-paying sectors and positive effects on wages, including wages of younger workers.

More generally across the entire labour market, **evidence for significant employment losses following minimum wage increases remains mixed**, with some arguing that clear evidence of negative employment effects is still elusive (Manning, 2021) since most research has found no significant negative effects of the NMW on overall employment, despite significant positive effects on wages (Dube, 2019). Despite some literature suggesting positive effects, other studies indicate an own-wage elasticity of employment estimated at -0.20, indicating minimal job losses (Giupponi et al., 2024). Some literature suggests **negative employment effects**, which are more likely to affect women, who are disproportionally represented in the low-pay workforce (Cribb et al., 2021). Supporting these findings, negative employment impacts following the introduction of the NLW are found among part-time women and retail workers, although no significant impacts on employment retention and hours worked were identified (Aitken, Dolton & Riley, 2019).

A change in the minimum wage is also associated with positive **spillover effects**, where wage increases for the low-pay workers may lead to wage gains up to the middle of the household income distribution, with proportional impacts similar for the poorest and the middle-income households

(Giupponi et al., 2024). The existing literature supports findings of spillover effects up to the 20th percentile (Cribb et al., 2021).

1.5.2   Existing evidence on the impact on educational participation

While most studies focus on employment and wage effects, research on the impacts of the minimum wage on **educational participation, training, and career progression remains sparse**. There is, however, evidence from outside the UK suggesting that minimum wage policies can influence decisions around education and training. For instance, evidence from Brazil shows that increases in the absolute minimum wage can reduce demand for education among young people by **reducing the skill premium and lowering the returns to education**. Conversely, for workers at the middle of the wage distribution, relative increases in minimum wage (compared to the average wage in the region) have been associated with **higher investment in on-the-job training** (Marioni, 2024). Looking at **long-run impacts**, evidence from the US shows that the introduction of the minimum wage **increased school enrolment rates**, as the opportunity costs of staying in school are reduced with minimum wage jobs being increasingly offered to those with more experience or education (Sutch, 2010). More recent findings from the US suggest that increases in the minimum wage reduced the likelihood of a low-socioeconomic-status teen dropping out of high school (Smith, 2021).

In the UK, Crawford et al. (2011) looked at the introduction of the 16-17 rate of the NMW in 2004, the impact of the 18-21 development rate, and the reduction in the age of entitlement from 22 to 21 in 2010 on education choices of young people. Using data from the LFS and ASHE and exploiting geographical variation in wage rates, the authors find that **local youth wage rates do not appear to affect the main education or employment choices made by young people between the ages of 16 and 19**. There is some evidence of a negative effect on the likelihood of being in full-time education for men when considering the development rate, but no impact of the youth rate on the probability of staying in full-time education or the probability of being NEET.

1.5.3   Methodological approaches and challenges

Most studies applied one of the following **methodologies**:
- **Difference-in-differences**: comparing the employment or wage outcomes for groups affected by minimum wage changes against those in unaffected regions or sectors;
- **Regression discontinuity design**: where a minimum wage increase affects only certain age groups, by exploiting age thresholds (e.g., 18-20, 21-24, and 25+); or,
- **Panel data analysis**: panel data to track individual or firm-level outcomes over time, controlling for time-invariant characteristics that may affect employment or wages.

Challenges remain in assessing the full impact of minimum wage policies. The main challenges are **data limitations**, such as the decline in the data quality of the Labour Force Survey (LFS), while the availability of data on **training and career progression for specifically low-wage workers** is sparse. Moreover, the interaction between minimum wage policies and other **labour market interventions**, such as training schemes or work experience programmes, remains underexamined.

2 Longitudinal Education Outcomes (LEO) data tables

2.1 LEO data tables

The LEO data tables are a combination of several administrative datasets that provide a picture of **pupils' educational journey through the English school system and can be linked to their labour market outcomes**. The LEO data tables cover every individual born from 1985/86 who has engaged with the school education system in England at least once. Anyone who completed their entire education in England can be traced throughout their entire educational journey due to the longitudinal nature of the LEO data tables.

The data brings together information on

- individuals' educational journey through the English school system and detailed personal characteristics from the **National Pupil Database** (NPD),
- individuals' educational journey through further education via the **Individual Learner Records** (ILR),
- higher education via the **Higher Education Statistics Agency** (HESA) data, and
- information on annual earnings, employment and benefits from HMRC and DWP records.

The key advantage of LEO is its **large sample size** with approximately 500,000 individuals per cohort. Another key advantage of LEO compared to other data sources is the **wealth of personal information in the educational datasets**, including information on attainment and detailed scores at each key stage, information on subjects and course characteristics, absence or exclusion, as well as personal characteristics such as Free School Meal eligibility, local area deprivation, Special Educational Needs status, gender, ethnicity, disability, and other demographics.

Education information can be linked to labour market outcomes information, such as duration of employment spells, annual earnings, receipt of benefits, industry, and geography from individuals' tax records.

Since the LEO data tables only capture individuals who participate in the English school system, individuals studying solely outside England or those born before 1985 are not captured in the data. Crucially, **earnings are reported by tax year, while employment is captured in spells (measured in days)**, meaning there is **no record of hours worked or hourly pay**. Also, although the employment data contains employer characteristics and geographical variables, there is **no information on occupation**.

In addition, since the educational data is captured by academic year while the labour market data is captured by tax year, there is a temporal mismatch between these two. Figure 2 below presents a summary of the key features of the LEO data.

Further information about alternative data sources and comparisons with LEO data can be found in the Annex.

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Figure 2   **Summary of the characteristics of the LEO data**

Sample size	Detail and quality of information	Employment	Pay and hours worked
• **500,000** sample size for each cohort • Does not include **older individuals** and those **studying solely outside England** • Incomplete picture for those **spending time outside of the English system**	• **Administrative** data • **Detailed educational information** (participation and progression) • **Employer information** (location/industry/size but no occupation information)	• Employment **spells** • Employment **stability** (number of spells, length of employment) • Can also observe **receipt of benefits**	• Pay by **financial year** • **No information on hourly pay** • Not possible to reliably estimate hourly pay (no information on **hours worked**) • **Progression up the income distribution**

2.2   Outcomes of interest

To investigate the impact of the introduction of the NLW on **labour market outcomes,** we focus on the following outcomes for every individual in our sample:

- whether an individual was **employed** for any duration throughout a given tax year or not (binary), and
- the **number of employment spells** an individual had within a given tax year

For those individuals who were employed in some capacity within a given tax year, we also estimate the impact on

- their **annual earnings** within a given tax year (cannot be estimated on an hourly basis), and
- their **longest employment spell** within a given tax year (in days).

To understand the impact of the introduction of the NLW on **educational participation**, we look at the following outcomes (each of which is treated as a binary variable):

- Whether an individual was **enrolled** in any type of qualification (dormant, inactive, or suspended qualifications were not included), irrespective of whether the qualification was completed or not;
- The type of the qualification is based on the level of the main qualification undertaken within a given academic year (see Section for more details on how the main qualification was identified):
  - Qualifications up to Level 3, mostly consisting of **further education** (for those aged 18 and younger this will capture A-Level qualifications and equivalent attained while at school, which is relevant for younger cohorts at the beginning of the chosen period of analysis);

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- □ Qualifications between Level 4 to 6, mostly consisting of **undergraduate qualifications**;
- □ Qualifications at Level 7 or 8, mostly consisting of **postgraduate qualifications**.
- ■ Whether the individual undertook an **apprenticeship** of any level (the level of the apprenticeship is captured in the analysis by type, as apprenticeships can be Level 3 up to Level 7.

2.3 Educational participation outcomes in LEO

The analysis focuses on

- ■ whether individuals were **undertaking any form of education** during a given year,
- ■ **what type of education** they undertook based on the level of the qualification, and
- ■ **whether that qualification started in the given academic year** or continued from the previous year.

To obtain this information, we combine data from **Higher Education Statistics Agency (HESA)** and the **Individualised Learner Record (ILR) Aims data**, both of which are part of LEO. The education data is captured in **academic years** rather than tax years.

However, **not all information is available consistently**. In particular, some qualifications in the ILR do not have information on the level of the qualification. In both HESA and the ILR, **for some qualifications information on the start and end date is not available**.

2.3.1 Multiple qualifications in an academic year

Some individuals undertake **multiple qualifications within a given academic year**, either because they complete one and start another, or they switch to another, or do multiple qualifications simultaneously. Hence, one individual can have multiple records in the ILR for a given academic year or can be present in both the ILR and HESA for the same academic year.

Qualifications from **HESA are generally based on academic years, spanning either one or multiple academic years**, unless people quit or drop out. However, qualifications in the **ILR can be shorter in duration and do not all start in September**.

The analysis combines information from both data sources into academic years for each individual, with **one main qualification** (if any) identified for each individual-academic year pair.

To **identify the main qualification** for individuals undertaking more than one within an academic year,

- ■ **exclude** qualifications that **do not have information on the level** of the qualification available,
- ■ identify the qualification as the main qualification in the academic year as
  - □ the qualification the individual **spent the most time on** (using start and end dates) within that academic year, or
  - □ the qualification with the **highest level** if start and end date information is not available.

2.3.2   Classification of qualifications

We also want to analyse whether the introduction of the NLW affected participation in certain types of education. Classifying qualifications by type can be done in different ways. One possibility is to group qualifications into **academic and vocational qualifications**. However, some qualifications, such as degree apprenticeships are **ambiguous as they take place both at the workplace and higher/further education providers**. Similarly, some further education providers offer degree qualifications. Hence, the **distinction between vocational/academic or higher/further education might not always be clear**.

Therefore, this analysis defines qualification types based on the level of qualification, grouping together

- Levels 0-3 (mostly **further education**),
- Levels 4-6 (mostly **undergraduate** or equivalent), and
- Levels 7-8 (**postgraduate** or equivalent).

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3 Methodology

3.1 Identification strategy

The identification strategy, which estimates the impact of the introduction of the National Living Wage in April 2016, sets out:

- the **treatment and control groups** used in the analysis,
- the main **econometric specification** used to measure the potential impact, and
- further statistical tests used to assess the **sensitivity of the findings** from the main econometric specification.

3.1.1 Treatment and control cohorts

Given the longitudinal nature of the LEO data, treatment and control groups can be defined as different cohorts of young people assigned to either group depending on their eligibility for the NLW in April 2016 and tracked over time (before and after April 2016). In fact, those aged 25 and older were eligible for the new NLW (£7.20 per hour) when it was first introduced in April 2016, while those aged 21-24 remained eligible for a lower national minimum wage rate (£6.70 per hour, which was set in October 2025 and remained unchanged until October 2016). Using the policy change occurring in April 2016, it is possible to identify the two following groups:

- The **treatment** cohort, consisting of individuals who were aged **25** in April 2016 when the NLW was introduced (i.e., those born between April 1990 and March 1991).
- The **control** cohort of those aged **24** in April 2016 (i.e., born between April 1991 and March 1992).

We track both cohorts from 2010/11 until 2019/20 (academic years when looking at the educational outcomes, tax years for the labour market outcomes). As a result, all individuals in the control cohort were aged 18 at the start of the 2010/11 tax year and 27 at the start of the 2019/20 tax year, or 18 to 19 (depending on the month of birth) at the start of the 2010/11 academic year and 27 to 28 at the start of the 2019/20 academic year. The treatment cohort is one year older.

The **treatment and control groups would ideally be as similar as possible**, aside from their NLW eligibility in April 2016, since our identification strategy depends on the extent to which **both cohorts would have followed similar trends in the absence of the introduction of the NLW**.

In the main specification, we compare those aged 25 in April 2016 with those aged 24 at the same point in time to ensure the cohorts are **as similar as possible**. However, this means the **control group became eligible for the NLW over the course of the following 12 months** and was entirely eligible for the NLW by the following April, removing differences in minimum wage eligibility with the treatment group by then. Hence, as a robustness check, we also compare those aged 25 with those aged 23 in April 2016, as individuals in this cohort did not become eligible for the NLW for at least 13 months (although there may be larger differences between these two cohorts, as they are more than 12 months apart in age).

This means that the main treatment and control cohorts consist of individuals available in the Key Stage 4 dataset (containing information on learners in England by the end of year 11 of schooling[9]) and born between April 1990 and March 1991 (treatment group) and April 1991 and March 1992 (control group). We also capture those born between April 1989 and March 1990 to be able to compare those aged 26 in April 2016 with the main treatment cohort as a falsification test, and those born between April 1992 and March 1993 to compare the treatment cohort with a younger control cohort as a robustness check. Thus, our 'spine' consists of every individual who was born between April 1989 and March 1993 and has a Key Stage 4 record in England (i.e., those who are aged between 23 and 26 in April 2016).

Figure 3 shows the age and NLW eligibility of the main treatment and control cohorts over time. T1 is the main treatment cohort (aged 25 in April 2016), and C1 is the main control cohort (aged 24 in April 2016).

Figure 3   Treatment/control cohorts across tax years

Tax year / Age	2010-11	2011-12	2012-13	2013-14	2014-15	2015-16	2016-17	2017-18	2018-19	2019-20
28										T1
27									T1	C1
26								T1	C1	C2
25							T1	C1	C2	
24						T1	C1	C2		
23					T1	C1	C2			
22				T1	C1	C2				
21			T1	C1	C2					
20		T1	C1	C2						
19	T1	C1	C2							
18	C1	C2								
17	C2									

Note: Cells highlighted in grey indicate cohorts eligible for the 21+ National Minimum Wage up to April 2016. Cells highlighted in orange indicate cohorts eligible for the National Living Wage from April 2016. Cells highlighted in light blue indicate cohorts eligible for the 21- to 24-year-old National Minimum Wage from April 2016. C1 is the main control cohort, C2 is used as a robustness check.

As a **falsification test**, we also compare outcomes

■ between those aged 26 and 25 in April 2016 (both eligible) and

■ between those aged 24 and 23 in April 2016 (both ineligible).

3.1.2   Cohort characteristics

For every individual, we gathered information on a range of **personal characteristics** by combining information from various educational datasets within LEO: the School Census, YPMAD (Young Person's Matched Administrative Dataset), Key Stage 2 and Key Stage 4 assessments. This information is used to assess whether the treatment and control cohorts are broadly similar besides their difference in age. We also use these details to control for heterogeneity in the analysis and to restrict the analysis to certain sub-groups only in selected specifications, focusing on individuals who might be more likely to be exposed to the minimum wage.

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[9] Aged 15 at the start of the academic year

Information used from the LEO data tables include:

- year and month of birth (to assign the individual to the treatment or control group, and understand how close to the age threshold they were in April 2016),
- gender,
- ethnic group (white or non-white),
- first language (English or non-English),
- IDACI[10] (Income Deprivation Affecting Children Index) score, based on the pupil's postcode at age 15, by decile (grouped into 20% of areas with the highest rates of income deprivation among children and the other 80%),
- Free school meal eligibility at age 15;
- special educational needs status at age 15 (none, with statement, without statement), and
- historical educational attainment, in particular:
    - attainment at Key Stage 2 (assessment of learners when pupils are aged 11), in particular whether the pupil achieved the expected level in English and maths by the end of year 6, and
    - attainment by age 19: whether the individual achieved a level 2 qualification or higher by age 19.

For characteristics that can change over time, such as free school meal eligibility, IDACI score, or special educational needs status, we capture these at age 15 since we use presence in Key Stage 4 to define our cohort. However, this means that those not attending English state schools at age 15 will not have this information available (in particular pupils attending independent schools[11]). Individuals were also excluded from the analysis if information about relevant characteristics was not available for them (for example pupils having no Key Stage 2 record[12]) or was inconsistent across multiple sources (e.g., inconsistent personal characteristics across the KS4, KS2, YPMAD, and/or School Census datasets).[13]

From the initial spine, information on personal characteristics was available and consistent for 83% of individuals. The resulting set of personal characteristics is constant for each individual across our period of analysis, since it is either a **snapshot value** (e.g., measured at one point in time, at age 15) or **unchanged over time** (e.g., year of birth). We then define our four cohorts based on how old Individuals are in April 2016, based on the month and year of birth.

This results in the following **cohort sizes**:

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[10] The Income Deprivation Affecting Children Index (IDACI) score captures the percentage of children aged 0-15 living in income deprived families, i.e. an IDACI score of 0.5 would mean that 50% of children aged 0-15 live in income-deprived families in that area.

[11] Pupils attending independent schools are not covered by the main School Census data collection.

[12] For example pupils who were not in the English school system at age 11 and then subsequently attended Key Stage 4 in English state schools

[13] Following previous research using LEO data tables, see Anderson & Nelson, 2021 (here).

Table 1   Cohort size

Total	Age 26 in April 2016 (born 1989/90)	Age 25 in April 2016 (born 1990/91) – main treatment group	Age 24 in April 2016 (born 1991/92) – main control group	Age 23 in April 2016 (born 1992/93)
2,192,509	544,895	551,040	554,015	543,560

Note: Based on cohort size and availability of personal characteristics.

Source: LE analysis of LEO data tables. data (School Census, Key Stage 2, Key Stage 4, YPMAD)

3.1.3   Econometric specification

To measure the effect of the introduction of the NLW on educational participation and labour market outcomes, we use a **difference-in-differences design with multiple periods before and after treatment, similar to an event study design** where treatment occurs at the same point in time for all treated individuals.

This design allows us to see **whether there are any significant differences in trends between the treatment and control group before the introduction of the NLW**, even when including controls. It also means we can look at the treatment effects over time. As the control group eventually became eligible for the NLW, the estimated impact may be relatively short-term.

The **key assumption is that the control and treatment groups would follow a similar trend in the absence of the introduction of the NLW**. Comparing their pre-treatment trends assesses the plausibility of the assumption.

The data for the labour market outcomes analysis is organised by tax year, so the introduction of the NLW in April 2016 coincided with the start of the 2016/17 tax year, making this the first post-treatment period. Information on education outcomes are organised by academic year (September to August[14]), so the introduction of the NLW occurred midway through the 2015/16 academic year. However, we would expect individuals to make their decisions for the upcoming academic year in the months before September, so we assume that the first post-treatment period is the academic year 2016/17 (i.e., starting in September 2016).

The 2015/16 tax/academic year is used as the baseline period, so the econometric specification estimates the difference between treatment and control groups **relative to the difference between treatment and control groups in that baseline period**. The treatment and control groups are defined based on the individual's age in April 2016.

We estimate the following specification

$$y_{igt} = \alpha_t + \delta_g + \sum_{k \neq 2015/16} \beta_k \big(D_g \times \mathbb{1}\{t = k\} \big) + \gamma DistApr_i + \varphi_{it} + \boldsymbol{X_i}\boldsymbol{\theta} + \epsilon_{igt}$$

For an individual i belonging to cohort g at time t,

- y_{it} is the **outcome of interest** for individual i in cohort g at time t;
- α_t is a vector of dummy variables for **each tax/academic year** from 2010/11 until 2019/20 (except the baseline 2015/16), or time fixed effects;

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[14] August to July for information on Further Education collected in the Individualised Learner Record

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- δ_g are **cohort fixed effects** (i.e., whether the individual is in the **control or treatment group**);
- D_g indicates whether an individual **belongs to the treatment group**;
- $\mathbb{1}\{t = k\}$ is an indicator for the **year t**;
- $\boldsymbol{\beta_k}$ **are the coefficients of interest,** estimated for each year except 2015-16. For $t <$ 2016/17 (i.e., before treatment), the coefficient would be expected to be zero in the absence of any anticipation effects, model misspecification, or omitted confounding variables,[15] with any treatment effects expected to be found for $t \geq 2016/17$;
- $DistApr_i$ indicates how many months someone's month of birth is **away from the threshold**;
- φ_{it} are **fixed effects for someone's age in September of each year**. We use age in September so that it is not collinear with the treatment assignment, and since for educational outcomes, age in September is likely to matter more than age in April; and
- X_i is a set of **control variables** available in LEO data, including sex, ethnic group (white/non-white), free school meal eligibility at 15 (binary indicator), Special Educational Needs Status at age 15 (none, with statement, without statement), first language (English or other than English), highest educational attainment at age 19 (Level 2 or above, or Level 1 or below), and deprivation among children in the area where someone lived at age 15 (among bottom 20% of postcodes in England or not).

Standard errors in this specification are clustered at the year and month of birth level. For the labour market outcomes among those who are employed (annual earnings and duration of longest employment spell), we also control for geography (Local and Unitary Authority (LAUA) level) and for industry (SIC07 code) of main job, and cluster standard errors by LAUA instead.

The main analysis includes all individuals in our sample, while we also undertook additional analysis focusing on **sub-groups that are more likely to earn at or near the minimum wage based on their personal characteristics**, in particular:

- individuals who were eligible for **free school meals** at age 15,
- individuals who lived in areas when aged 15 that were among the **20% local areas with the highest rates of child deprivation** in England,
- individuals who have **not achieved to reach a qualification beyond Level 1 by age 19**,
- **female non-white** individuals, and
- **male non-white** individuals.

Previous analysis published by the Low Pay Commission showed that these characteristics are more often associated with being in a low pay group[16].

For binary outcomes, such as whether an individual is employed, any estimated treatment effect can be interpreted as the change in the probability that an individual is employed that can be attributed to an individual being eligible for the NLW when it was introduced. For continuous

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[15] See Miller, D.J. (2023). An introductory guide to event study models. *Journal of Economic Perspectives, 37(2), 203-230.* Link.

[16] Previous analysis published by the Low Pay Commission has shown that these characteristics are more often associated with being in a low pay group see the 'National Minimum Wage Low Pay Commission Report 2024' here (Chapter 3).

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outcomes, the treatment effect captures the effect on outcomes (e.g., log-transformed earnings) that can be attributed to treatment.

3.1.4 Robustness analysis

We estimate alternative specifications to test the sensitivity of the conclusions drawn from the baseline specification.

- **Redefine the control group as those who were 23 years old in April 2016** rather than those who were 24 years old:
    □ Those who were 24 years old in April 2016 would have become eligible for the NLW within twelve months (i.e., within the 2016-17 tax year), reducing the difference in minimum wage treatment between cohorts even within the first tax year after the introduction of the NLW.
    □ However, those who were 23 years old in April 2016 would not become eligible for the NLW in the 2016-17 tax year, but may be more different to the treatment group (those aged 25 in April 2016), as they are more than 12 months apart in age.
- Focusing on the **first year of enrolment** in education rather than including those continuing their studies as changes to the minimum wage may have a greater impact on the decision to begin an educational qualification rather than the decision to continue studying.
- Defining cohorts **by age at the start of the academic year** rather than at the start of the tax year as individuals may not consider their relevant minimum wage rate in April (when the rates change) when making their education decisions for the following academic year, but instead consider the relevant minimum wage rate in September when the academic year starts. In this case the control group consists of individuals aged 24 in September 2016, and the treatment group consists of individuals aged 25 in September 2016.
- Focusing on the earnings/employment outcomes of those working in **low-pay sectors**[17] as these groups are more likely to be impacted by changes to the minimum wage.
- **Falsification tests** comparing two age groups that do not vary by treatment status.
    □ Comparing those who were 25 and 26 in April 2016 (both age groups eligible for the NLW in April 2016).
    □ Comparing those who were 23 and 24 in April 2016 (both age groups ineligible for the NLW in April 2016).

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[17] As defined by the Low Pay Commission (link here).

4   Descriptive statistics

This section presents educational participation and labour market trends for the different cohorts used in the analysis. This provides context for the econometric findings, providing an indication of whether the assumption of parallel trends is likely to hold. In addition, the descriptive trends illustrate the wealth of information available in the LEO data and some of its potential applications.

The trends presented in this section are consistent with each cohort gradually transitioning from educational participation into employment, and entering into more stable employment with higher annual earnings as they grow older.

4.1   Trends in educational participation

4.1.1   Trends in enrolment

Figure 4 shows the **proportion of individuals participating in any form of education within a given academic year** from the **2010/11** academic year to the **2019/20** academic year.

Across all four cohorts, the share of individuals enrolled in education **declines as they get older**. In 2010/11, **around 60% and 55%** of individuals were participating in education in the main control cohort (light blue line) and the main treatment cohort (red line), respectively. By 2016/17, only around **15%** are undertaking some form of education. In general, the youngest cohort considered (those aged 23 in April 2016) has the highest proportion of individuals who are undertaking some form of education in every academic year from 2010/11 (when they are 17 or 18 at the start of the academic year) until 2019/20 (when they are 26 or 27 at the start of the academic year), compared to the older cohorts.
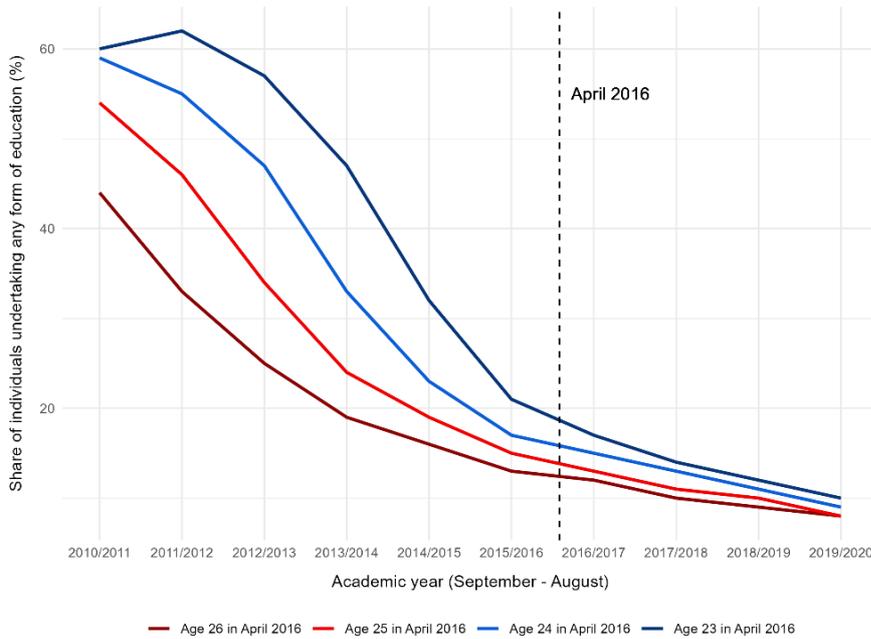
For the youngest cohort, there was an increase in the participation rate between the 2010/11 and 2011/12 academic year (which corresponds to age 18/19, as shown in Figure 5). This could be in anticipation of the tuition fee cap increase for undergraduate degrees to a maximum of £9,000 per year[18] introduced in the 2012/13 academic year, with an associated reduction in the number of young people taking a gap year before starting university. Consistent with this, there was a small increase in UCAS application rates for the January deadline between January 2010 and January 2011 for 18-year-olds[19] and applicants were much more likely to start straight away rather than defer to 2012/13[20].

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[18] Lewis, J., Bolton, P. & Wilson, S (2024). Tuition fees in England: History, debates, and international comparisons. (Link)

[19] See Table 1 in UCAS Analysis and Research: 2016 cycle (2016). UK application rates by the January deadline. (Link)

[20] See UCAS (2014). UCAS Analysis answers five key questions on the impact of the 2012 tuition fees increase in England. (Link)

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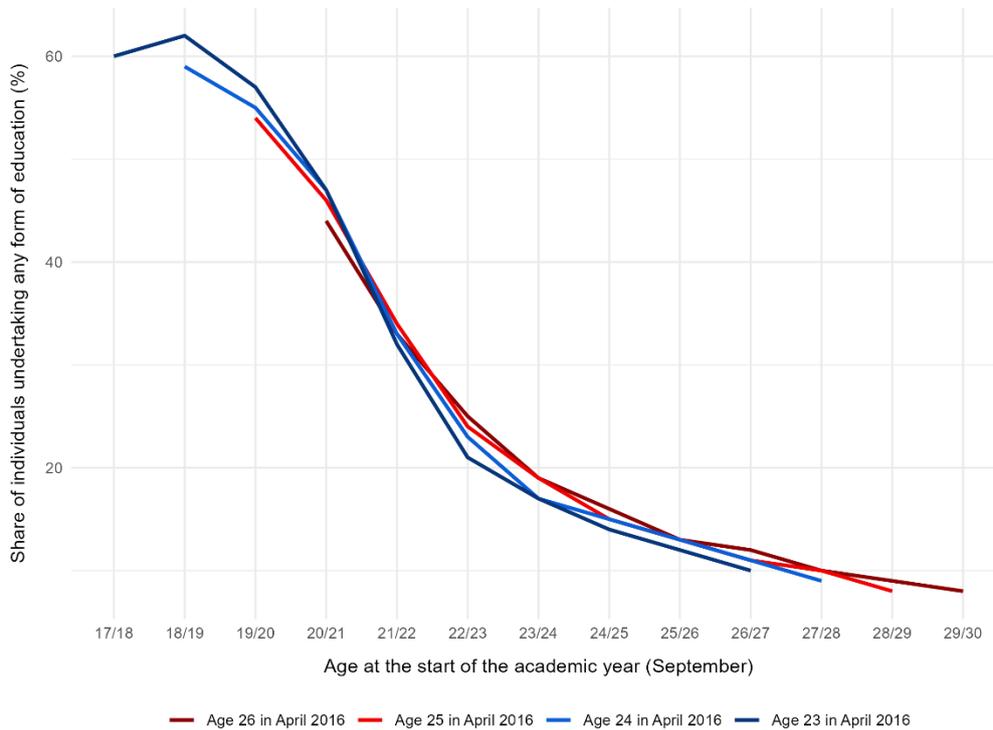
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Figure 4   Enrolment by academic year

Source: LE analysis of LEO data tables.

To allow for easier comparisons between cohorts, Figure 5 shows the same pattern but **by age rather than by academic year**, since every cohort reaches a certain age at a different point. Overall, the **four cohorts appear to follow a similar pattern** where the share of people enrolled in some type of qualification declines with age. However, there appears to be a small divergence between the main treatment and control cohort (aged 25 and 24 in April 2016, respectively) around the age of 23 and 24.

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Figure 5   Enrolment by age

Source: LE analysis of LEO data tables.

4.1.2   Enrolment by qualification type

Among those who are enrolled in some type of education, we can differentiate between the **types of qualifications individuals** undertake.[21] Specifically, we distinguish between

- **up to Level 3** (mostly further education, FE) qualifications – for those aged 18 and younger this will capture A-Level qualifications and equivalent attained while at school, which is relevant for younger cohorts at the beginning of the chosen period of analysis,
- **Level 4-6** (mostly undergraduate, UG) qualifications, and
- **Level 7-8** (postgraduate, PG) qualifications.

Figure 6 illustrates **trends by qualification type** for each of the four cohorts over time. The main treatment and control groups are on the right-hand side of the figure (those who were aged 25 and 24 in April 2016, respectively).

Enrolment in further education and undergraduate qualifications both decline over time. However, in 2010/11, undergraduate qualifications made up more than half of all qualifications that individuals were enrolled in among cohorts aged 24 and 25 in April 2016. By the 2019/20 academic year, undergraduate qualifications made up less than a third of all qualifications that individuals

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[21] The qualification level could not be identified in the LEO data for a small number of qualifications (excluded from this disaggregated analysis).

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were enrolled in. As a proportion of those enrolled in education, participation in further education increased between 2010/11 and 2019/20.
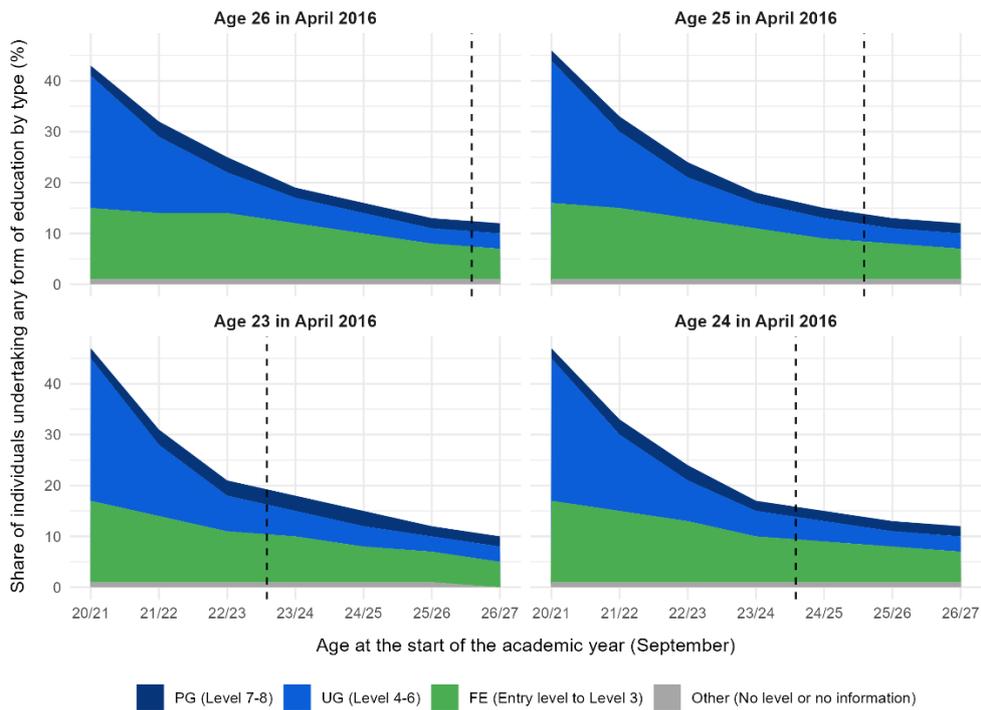
Figure 6   Enrolment by qualification type: by academic year
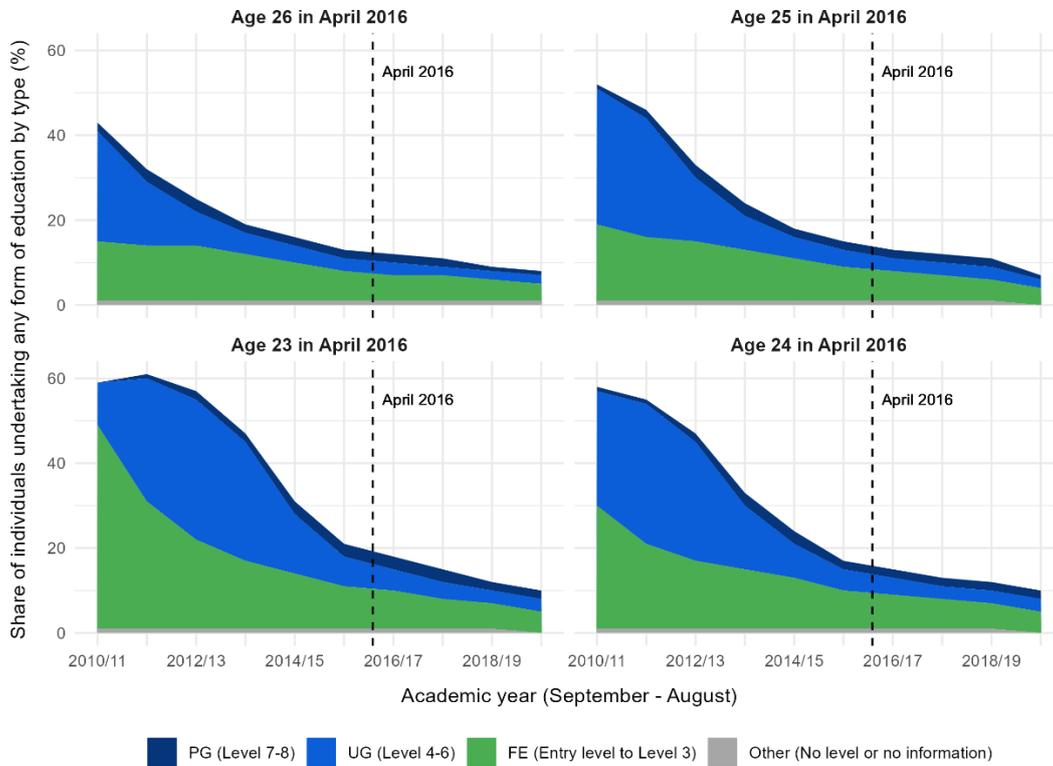
Source: LE analysis of LEO data tables.

Figure 7 also shows **enrolment by qualification type across ages** (not by academic year). The cohorts reach a certain age at different points in time, and by definition, each cohort has a different age when the NLW is introduced in April 2016, as illustrated by the vertical line. As expected, participation in undergraduate qualifications was much higher when people were aged 20 to 21 than when they were 26 to 27. The four cohorts' educational participation trends appear relatively similar but the proportion of 20/21-year-olds in education is slightly higher in the younger cohorts compared to the older cohorts.

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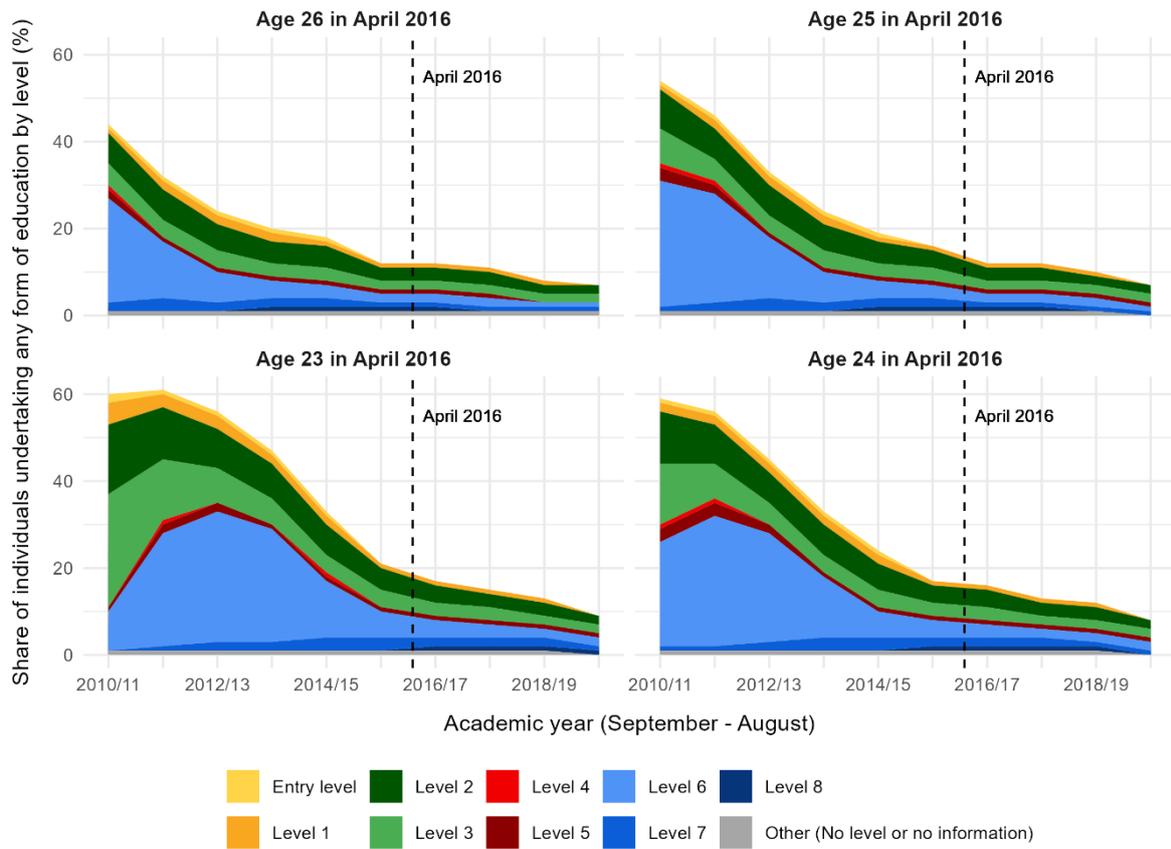**Figure 7   Enrolment by qualification type: by age**

Source: LE analysis of LEO data tables.

Figure 8 further disaggregates the qualification types and shows **enrolment by level** by year. The large proportion of individuals enrolled in undergraduate or equivalent qualifications is driven by Level 6 qualifications (First degree or equivalent). Particularly in the years between 2010/11 and 2012/13, enrolment by qualification type differs across the four cohorts, as individuals transition from 16-19 education into higher education qualifications, as shown in Figure 8. When comparing enrolment by level by age, not by year (see Figure 27 in Annex 2), the trends across cohorts appear to be similar, suggesting the differences are mostly age related not cohort specific.

Younger individuals tend to undertake more qualifications spanning several years. We would expect the **level of the minimum wage to play a bigger role in the decision-making process for individuals who are starting a new qualification** compared to those who are continuing a qualification they started in a previous year. Individuals continuing their qualifications may be more hesitant to 'drop out' due to having already invested significant time and resources compared to individuals who have just started. Figure 8 disaggregates qualifications based on new and continued qualifications, shown by year (not age). In 2010/11 and 2011/12, the control group (aged 24 in April 2016) had a much higher proportion of individuals in their first year compared to the treatment group, where a higher proportion of individuals was continuing a multi-year qualification. Figure 28 in Annex 2 shows the proportion of individuals in first year and not in first year by age rather than by year. Again, cohorts look similar when controlling for age.

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Figure 8   Enrolment by qualification level: by year

Source: LE analysis of LEO data tables.

Figure 9 **First year of enrolment status: by year**
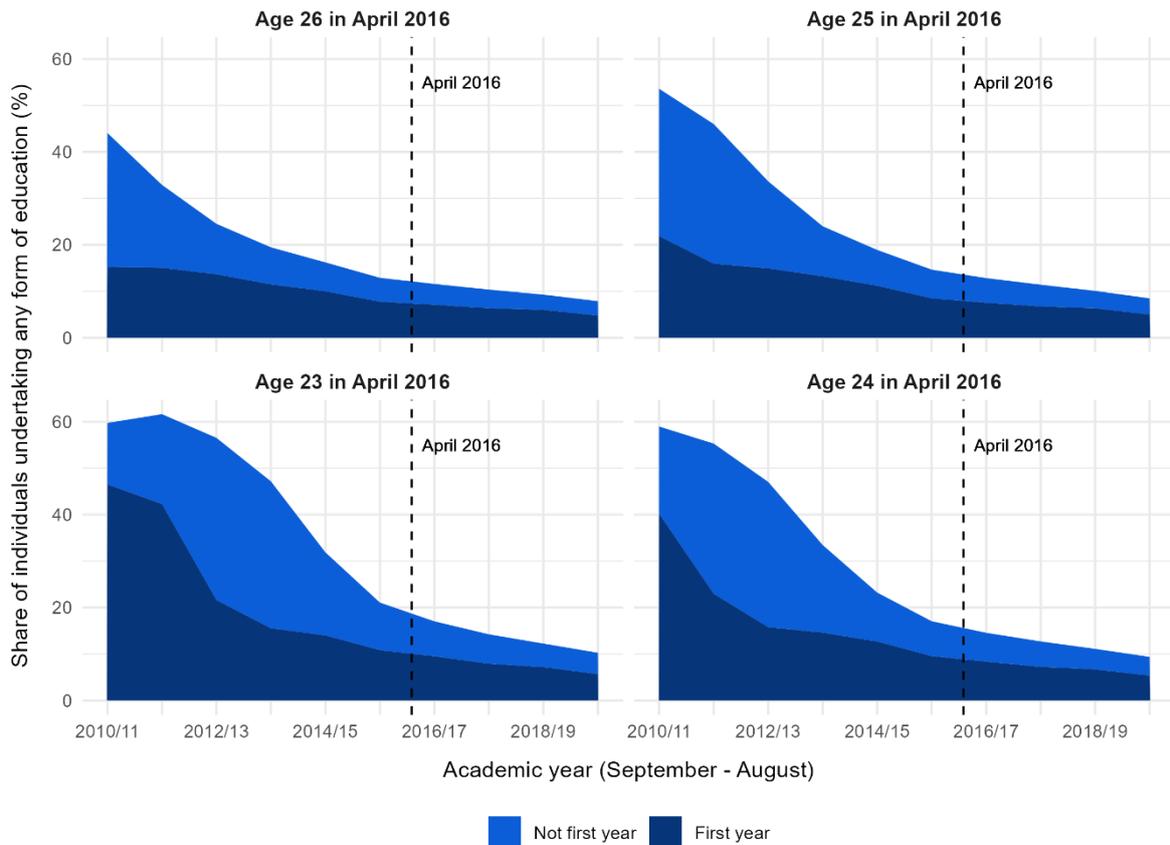
Source: LE analysis of LEO data tables.

Annex A2.1 contains additional information on how cohorts compare based on their personal characteristics, such as ethnicity or highest qualification achieved by age 19.

4.2 Trends in labour market outcomes

The LEO data tables do not include hourly pay information, and it is **not possible to estimate hourly wages as information on hours worked is not available**. Instead, we use personal characteristics such as free school meal eligibility, gender, ethnicity, or prior educational attainment to try and identify those who might have a higher likelihood of earning at or near the minimum wage. The LEO data tables include

- information on **earnings** (if any) for each tax year, and
- the **start and end dates** for each employment spell, which can be used to calculate the **length and number of employment spells within a tax year**.

Employment spells in more detail can give us insight into the employment stability of the four cohorts. Figure 10 tracks the proportion of individuals in each cohort who are in **the same employment spell for the entire tax year, which increases over time**. For a given tax year, older cohorts tend to have a higher proportion of year-long employment spells, although the differences between the different cohorts become smaller over time. Similarly, the proportion of individuals in

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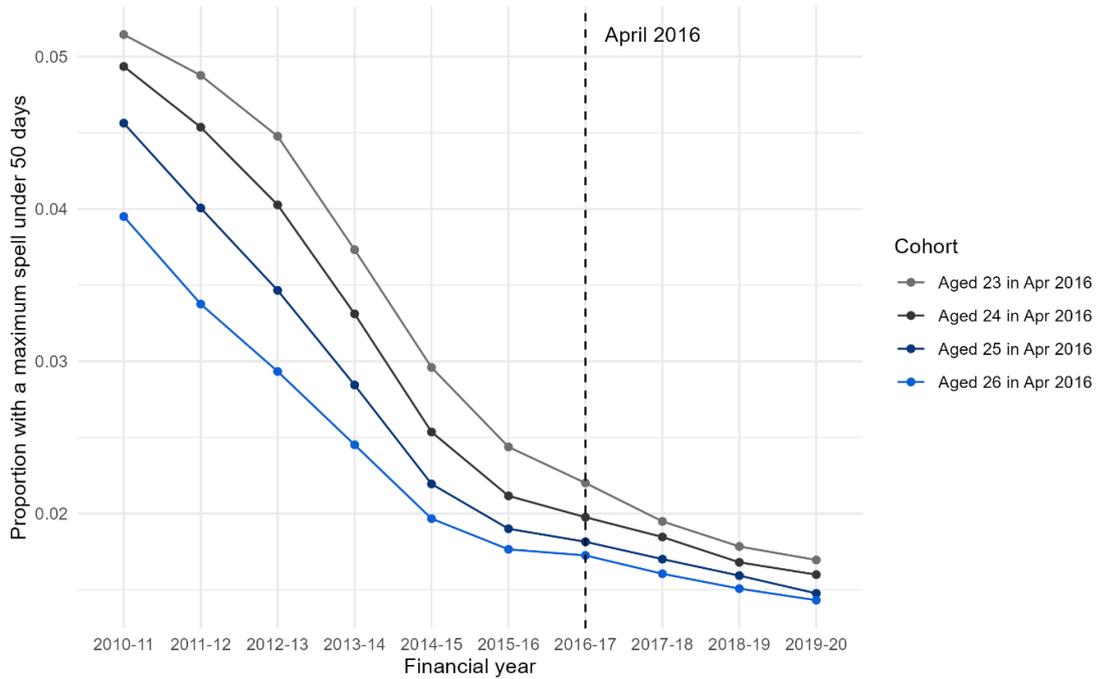each cohort whose **longest employment spells in a tax year are short (less than 50 days) declines across time to under two per cent** (Figure 11).

Figure 10   Proportion of the cohort employed in the same job for the whole year
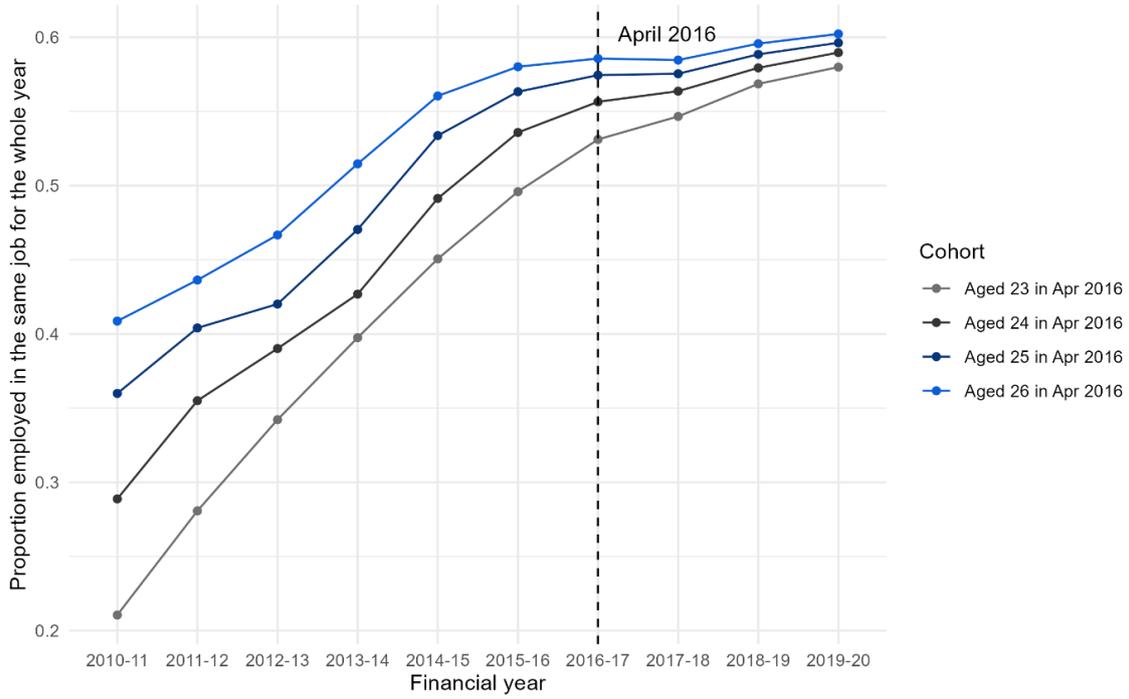
Source: LE analysis of LEO data tables.

Figure 11   Proportion of the cohort with a maximum employment spell of under 50 days

Source: LE analysis of LEO data tables.

Figure 12 shows the distribution of annual earnings (within the tax year) among those who were employed for each of the four cohorts. The filled circles report the median annual earnings within each cohort and a given tax year, while the whiskers show annual earnings at the third and seventh deciles. Within each cohort, annual earnings increase over time as the cohorts get older. Within a given tax year, the older cohorts have higher earnings than the younger cohorts, although **these differences become smaller over time**.

Figure 12   Annual earnings distribution

Source: LE analysis of LEO data tables.

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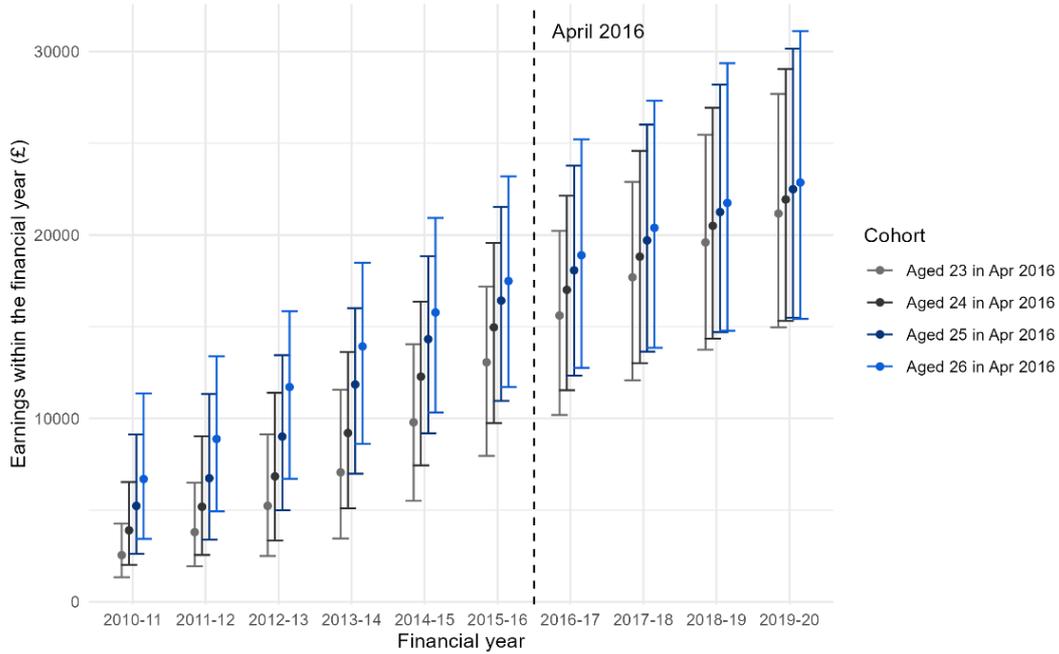
5 Econometric findings

The following section presents the results of the analysis.

- The specifications are first run for **all individuals** in the relevant treatment and control groups.
- The analysis then focuses on **five sub-groups that we expect to have a higher likelihood of minimum wage exposure, based on their personal characteristics**.

These five sub-groups are:

- Individuals who were eligible for free school meals at age 15 (**FSM**);
- Individuals who, at age 15, lived in areas with higher levels of income deprivation affecting children, placing them among the 20% of all lower-layer super output areas (LSOAs) in England with the highest IDACI[22] score (**IDACI**);
- Those who did not achieve a qualification of Level 2 (GCSE grades A*-C) or more by age 19 (**Low educ**);
- Female non-white individuals (**Female NW**); and
- Male non-white individuals (**Male NW**).

5.1 Educational participation

5.1.1 Baseline results

The treatment and control cohorts are defined as 25- and 24-year-olds, respectively, based on individuals' age in April 2016. The model controls for year and cohort, as well as age in September, to capture most of the age-related differences.

Figure 13 presents the coefficients of the event-study design, where the period before the introduction of the NLW (2015/16) is taken as the baseline year, so the **estimates for all other years are measured relative to this baseline**. The dots represent the point estimates of the coefficients and report the estimated change in the difference between the treatment group and the control group relative to the difference between the treatment and control group in the baseline period (2015/16). The **whiskers illustrate the 95% confidence interval** of each point estimate.

Ideally, the treatment and the control groups would follow a similar trend before the introduction of the NLW, consistent with an insignificant difference between the two cohorts before 2015/16, and the coefficients before 2015/16 are not statistically significantly different from zero. If they are, the estimates of the coefficients from 2016/17 onwards can be interpreted as the impact of the introduction of the NLW.

Figure 13 shows the **estimates of the introduction of the NLW on the likelihood that an individual is enrolled in any form of education within a given academic year**. Looking at the estimates for all individuals in Panel (1), there are some statistically significant differences between the treatment

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[22] The Income Deprivation Affecting Children Index (IDACI) score captures the percentage of children aged 0-15 living in income deprived families, i.e. an IDACI score of 0.5 would mean that 50% of children aged 0-15 live in income-deprived families in that area.

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and control group in 2010/11 and 2014/15 despite controlling for year-, cohort-, and age-specific effects.

In these years, the treatment group appears to have a slightly lower likelihood than the control group of being enrolled in education, compared to 2015/16, around 1.5 percentage points less likely than the control group to be in education, relative to 2015/16. However, due to the large sample size, the coefficients are estimated with a high level of precision, resulting in small estimated standard errors, such that even very small coefficients can be statistically significant. **In the years closer to the introduction of the NLW, the treatment and control groups appear to be following a mostly similar trend**.

One reason for the differences in the age-related changes in enrolment between cohorts. **In the 2010/11 academic year, the treatment group was aged 19-20 and the control group was aged 18-19**. Around this age, some people transition from school into university, while others might have already finished a post-compulsory vocational education qualification. **Differences between those aged 18-19 and 19-20** (as the cohorts were in 2010/11) **are likely to be larger than differences between those aged 24 or 25**, **as the cohorts were in April 2016, during the baseline period**.

As Figure 6 shows, the proportion of undergraduate qualifications among overall enrolment differs between the treatment and control group, particularly around 2010/11 to 2012/13. While the proportion of undergraduate enrolment among overall enrolment is highest in the treatment group in 2010/11 and then declines over time, the proportion of undergraduate enrolment among is initially lower in the control group, then increases from 2010/11 to 2011/12 and then declines in the following years. For qualifications up to Level 3, the control group initially has a higher proportion than the treatment group.

Another important factor may be the differential impact of the increase of the tuition fee cap to £9,000 in 2012/13, as there could have been an incentive to enrol the year before. The proportion of individuals starting a new qualification is higher among the younger cohort (the control group) than among the treatment group in 2010/11 and 2011/12 (as shown in Figure 9). Thus, **there could have been a differential impact of the fee cap increase on the treatment and control group before 2012/13**, with a larger incentive for the control group to start their undergraduate qualification in 2011/12 rather than in 2012/13.

If the tuition fee cap increase did have a larger impact on the control group by incentivising individuals to enrol in an undergraduate degree in 2011/12 rather than 2012/13, we would likely see this via a negative coefficient in 2011/12 when looking at undergraduate enrolment, i.e. a higher proportion of the control group enrolling than the treatment group compared to 2015/16.

However, given the age-related changes in enrolment in the earlier years of 2010/11 and 2011/12, there is much higher variability between cohorts and from one year to the next as shown in Figure 8. Even when controlling for age in September there might be some differences in enrolment by type that differ from the difference between treatment and control group in 2015/16 if the trend in enrolment is not fully identical from one year to the next. When looking at overall enrolment in Figure 13 it is unclear which of these factors is driving the differences in 2010/11 and 2011/12.

From 2016/17 onwards, the coefficients capture the impact of the introduction of the NLW on those individuals who are eligible for the NLW (those aged 25 in April 2016, the treatment group). The estimates from 2016/17 are essentially zero and not statistically significant, suggesting there was **no impact on overall educational participation by the introduction of the NLW on eligible individuals**.

When looking at the results by **sub-groups**, the **estimates are slightly less precise since the cohorts are smaller**. For those with lower prior educational achievement (Panel 4) and those who grew up in more deprived areas (Panel 3), the treatment group was also less likely to be in education than the control group in 2010/11 compared to in 2015/16. The introduction of NLW **did not appear to have a statistically significant impact on overall educational participation for any of the sub-groups**. The estimates are slightly positive but not statistically significant, so they do not provide evidence that the NLW had an impact on educational participation.

Looking at the impact on enrolment in specific types of qualifications, Figure 14 presents the estimated impact on enrolment in **further education (up to Level 3).** There are differences between the treatment and control group in the earlier years after 2010/11, both when looking at all individuals and when looking at each sub-group in the other panels. **In the years closer to the introduction of the NLW, these differences are much smaller and mostly insignificant**, suggesting the treatment and control groups follow a more similar trend. From 2016/17 onwards, the estimated coefficients are again close to zero and statistically insignificant, suggesting there was no impact of the introduction of the NLW on participation in further education.

Looking at the impact of the introduction of the NLW on **participation in undergraduate education** (including all qualifications from Level 4 to 6), the estimates presented in Figure 15 show that the effects from 2016/17 onwards are near zero and statistically not significant. Before the introduction of the NLW, the difference in undergraduate participation between the treatment control group was initially slightly larger than in 2015/16, but closer to the introduction of the NLW, the two cohorts were following a more similar trend (except among non-white men and women).
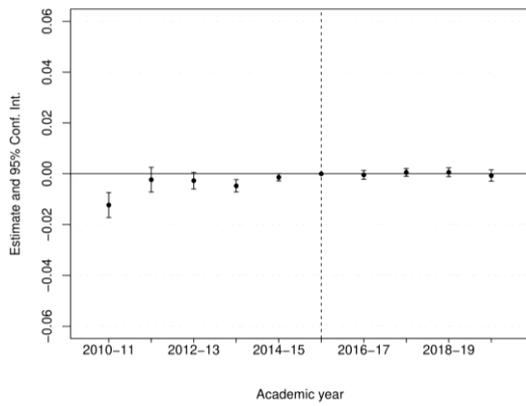
The positive coefficients in 2010/11 and 2011/12 when looking at undergraduate enrolment suggest that the differences might be related to the changes in enrolment patterns by age rather than a response to the tuition fee cap increase. The positive coefficients show that the difference in the proportion of undergraduate enrolment between the control group and the treatment group is bigger in 2010/11 and 2011/12 than the base year 2015/16. Due to the sign of the coefficient, it does not appear that these differences are due to the control cohort responding more strongly to the tuition cap increase by enrolling earlier compared to the treatment cohort, as the treatment group has a higher proportion of undergraduate enrolment than the control group in those years. Instead, these coefficients are likely driven by the age-specific changes in enrolment and enrolment by level, as shown in section 4.1.2.

The tuition fee cap increase potentially incentivised some individuals enrolling in new qualifications to start a further education qualification instead of an undergraduate qualification. This could explain why the treatment group had a higher proportion enrolling in undergraduate qualifications in the earlier years, while the control group had a higher proportion enrolling in further education qualifications in those years.

Figure 16 shows the impact of the NLW on the likelihood of individuals undertaking apprenticeships at any level. Similar to the results presented for further education and overall enrolment in education, there is a slightly negative difference in 2010/11, suggesting participation in apprenticeships is lower among the treatment group than the control group in these years relative to 2015/16. Similar to the other types of qualification, the effect of the introduction to the NLW is relatively small and statistically insignificant.

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Figure 13   Baseline results: Enrolled in education (any level)

(1) All

(2) Free school meals
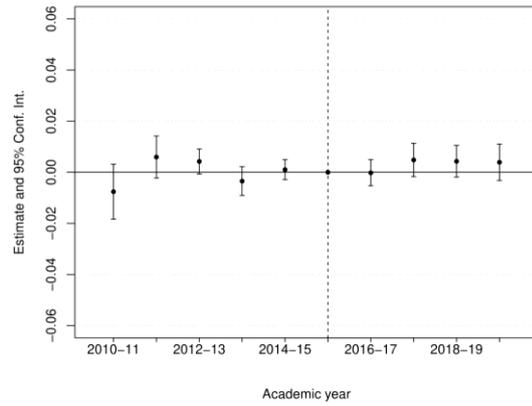
(3) Living in a deprived area at 15

(4) Lower than Level 2 qualification by 19

(5) Female and non-white

(6) Male and non-white

Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables

Figure 14   Baseline results: Further education (up to Level 3)

(1) All

(2) Free school meals
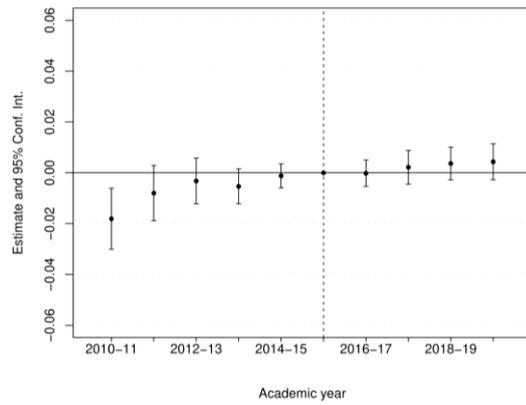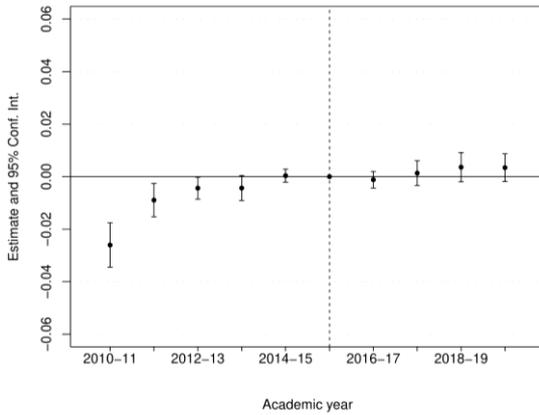
(3) Living in a deprived area at 15

(4) Lower than Level 2 qualification by 19

(5) Female and non-white
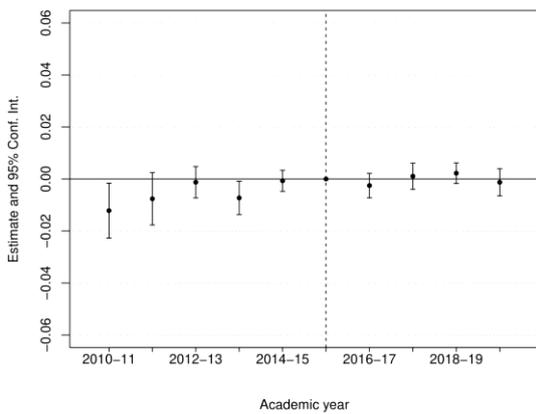
(6) Male and non-white
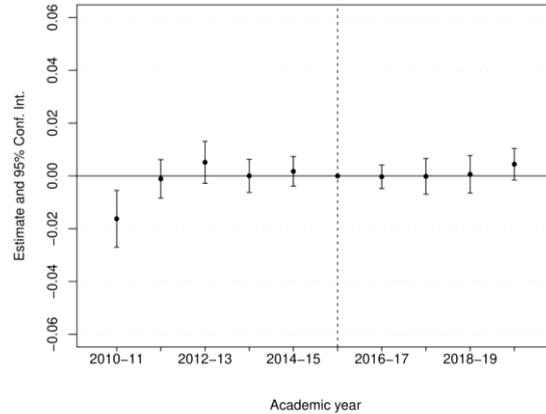
Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

Figure 15   Baseline results: Undergraduate education (Levels 4-6)

(1) All

(2) Free school meals
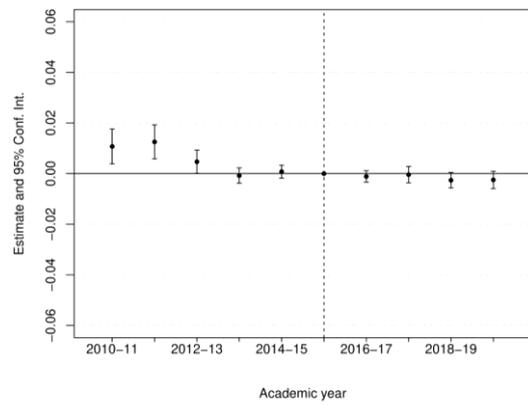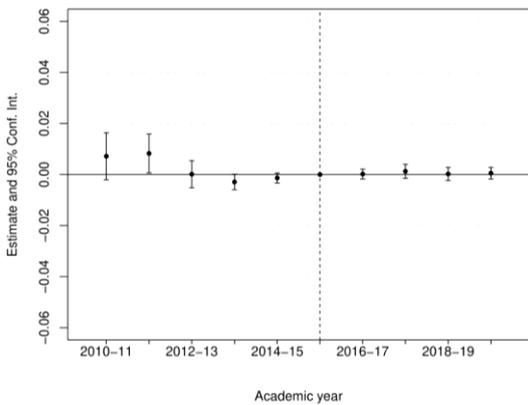
(3) Living in a deprived area at 15

(4) Lower than Level 2 qualification by 19

(5) Female and non-white

(6) Male and non-white

Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

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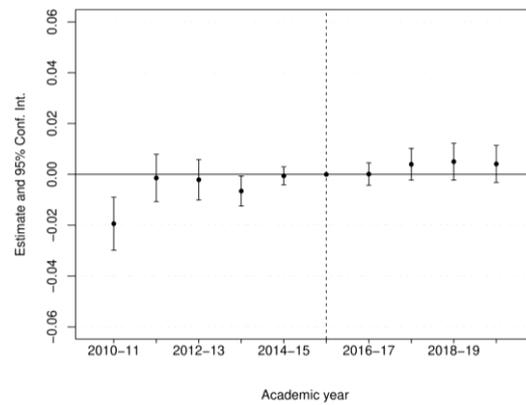**31**
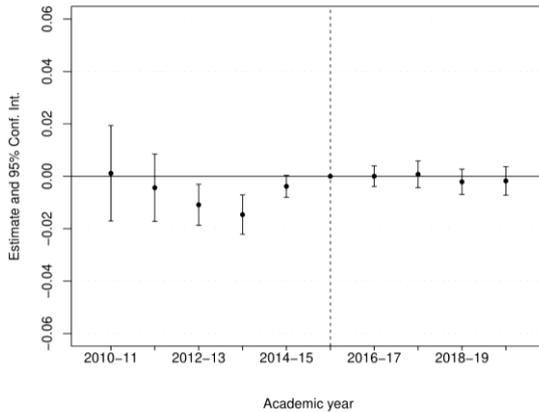
Figure 16   Baseline results: Apprenticeship
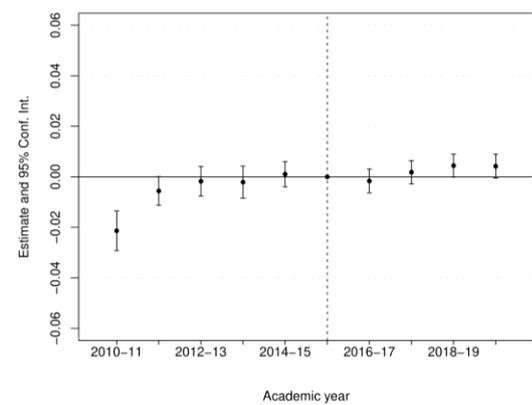
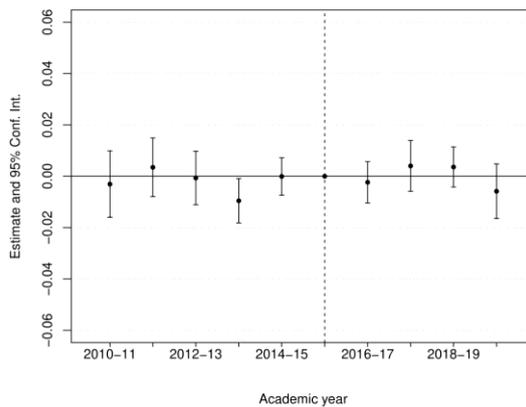**(1) All**

(2) Free school meals
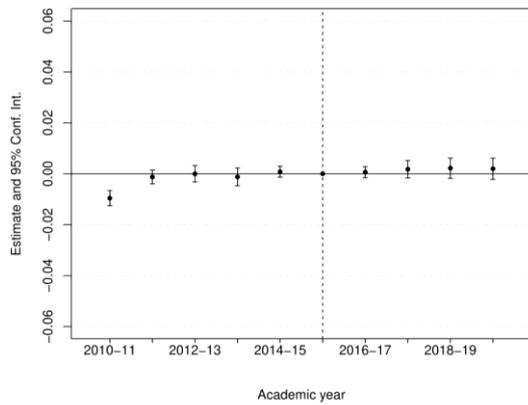
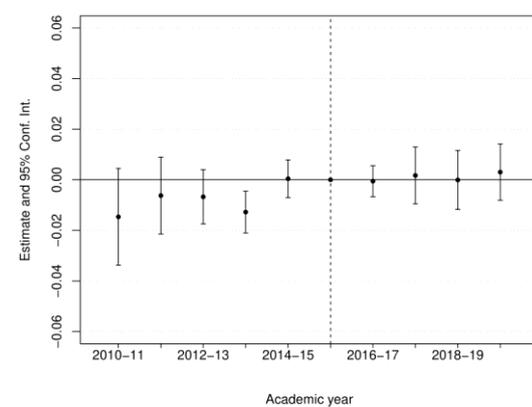**(3) Living in a deprived area at 15**

(4) Lower than Level 2 qualification by 19

(5) Female and non-white
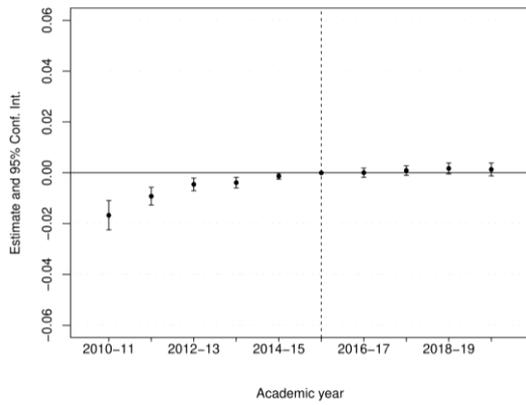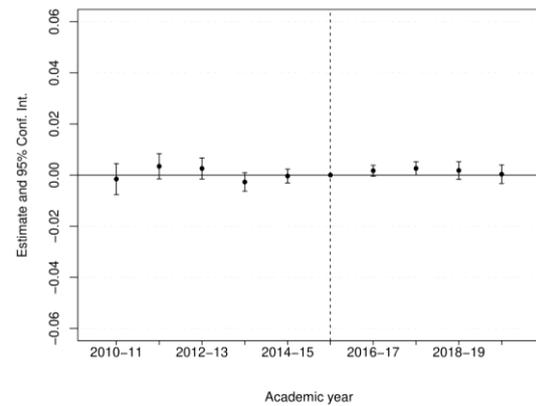

(6) Male and non-white
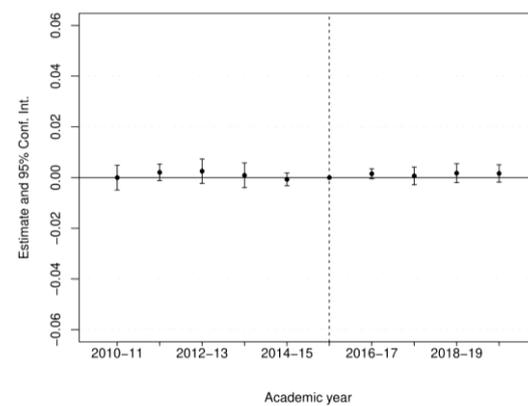
Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

5.1.2 Additional specifications and robustness checks

The analysis presented in this section assesses the sensitivity of the conclusions drawn from the baseline analysis and the validity of the baseline specification.

This includes

- Replacing those who were 24 in April 2016 with **those who were 23 in April 2016 in the control group**.
- Focusing on the **first year of enrolment**.
- Defining cohorts based on **age in September** (rather than April).
- **Falsification tests** comparing cohorts that do not differ in minimum wage treatment in April 2016.

Comparing 25 and 23-year-olds

In the baseline results, those aged 25 in April 2016 are compared to those aged 24 in April 2016.

Since those aged 24 turned 25 and became eligible for the NLW within the year following the introduction of the NLW, an alternative specification uses a younger control group aged 23 in April 2016, who were not eligible for the NLW for at least another year after April 2016, but may be less similar to the treatment group than the original control group.

Figure 17 shows the results of the NLW on the various outcomes when comparing all those who were 25 in April 2016 with those who were 23 in April 2016. The full results by sub-group can be found in the Annex. Differences between the treatment and control group before 2015/16 are larger than in the baseline analysis, consistent with the **alternative control group differing more with the treatment group than the control group in the baseline analysis**, even after controlling for year, cohort, and age in September. However, these pre-April 2016 differences are smaller closer to the introduction of the NLW.

Similar to the baseline analysis, the impact of the introduction of the NLW on overall enrolment, participation in undergraduate education, and postgraduate education appears to be zero and is not statistically significant. The impact on further education and apprenticeship participation appears to be slightly positive and marginally significant. However, **given differences between the treatment and control group before the introduction of the NLW, even after controlling for a variety of factors, these findings should be treated with caution.**

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Figure 17   25 vs. 23-year-olds results for all individuals: all outcomes
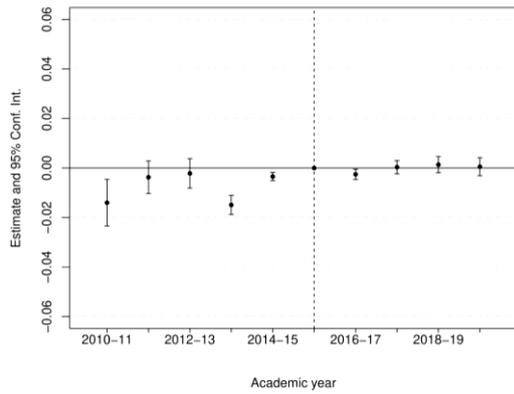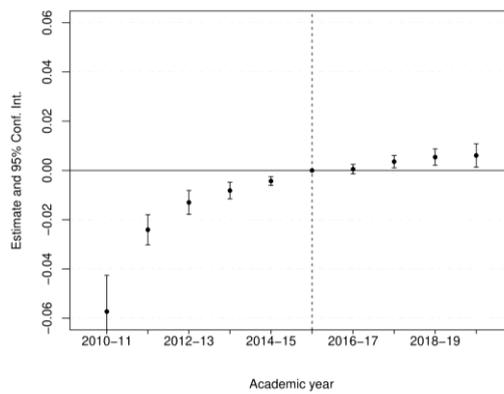
(1) Enrolled

(2) Further education

(3) Undergraduate education

(4) Postgraduate education
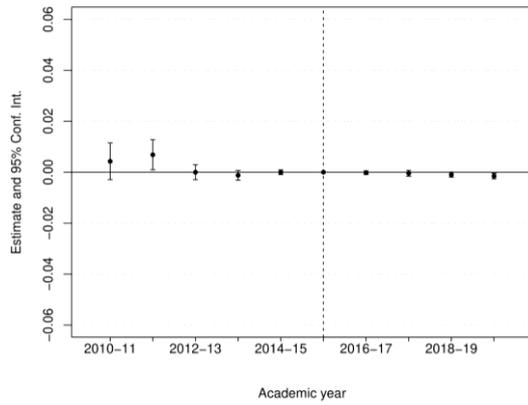

(5) Apprenticeship
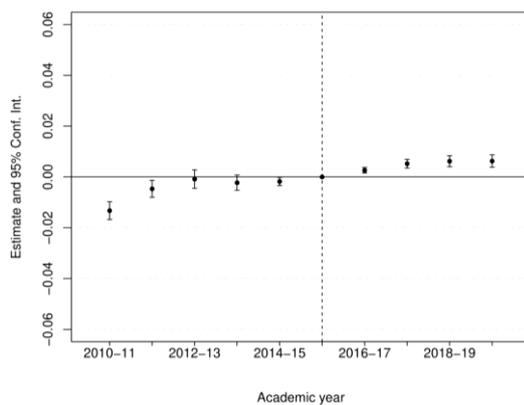
Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

Individuals in the first year of enrolment

Individuals may be more likely to be influenced by the minimum wage when deciding whether or not to **start a qualification** than when deciding whether to continue a qualification they started in a previous year. Figure 18 presents the estimated impact on the type of qualification started, **limiting the sample to those in their first year of a qualification**. As a result, the impact on overall enrolment is not estimated.[23] Panel (1) shows the impact of the introduction of the NLW on the **likelihood of participating in the first year of a further education qualification, conditional on beginning a qualification that year**. The impact following the introduction of the NLW in 2016 is **close to zero and statistically not significant**, with some significant differences before April 2016.

Figure 18   First year of enrolment
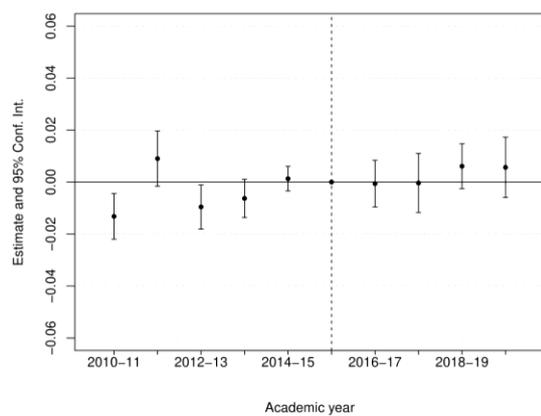
(1) Further education   **(2) Undergraduate education**
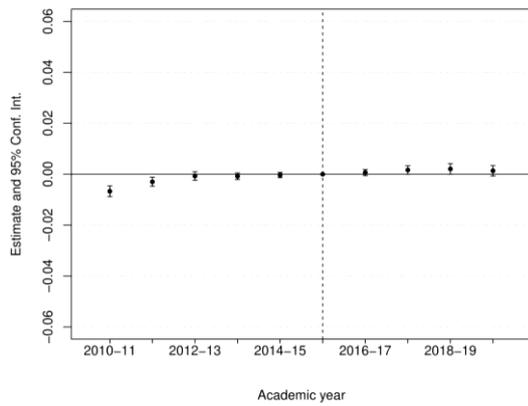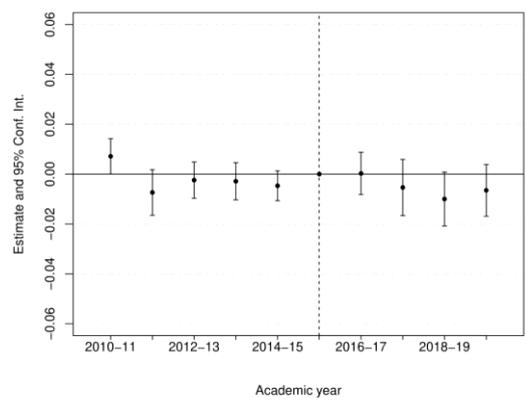

(3) Apprenticeship

Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).
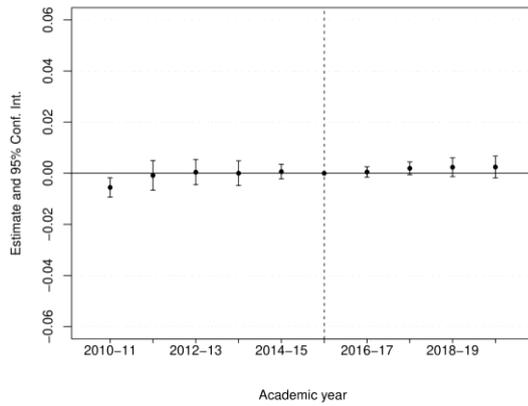*Source: LE analysis of LEO data tables.*

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[23] We also do not estimate this for postgraduate qualifications, since most postgraduate qualifications only take one year. Full results, including those for sub-groups, can be found in the Annex.

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Two years after the introduction of the NLW, the impact appears to be slightly positive but insignificant. In addition, the control group is also eligible for the NLW at that point, so there is no difference in treatment status between the treatment and control groups, and it is difficult to directly attribute this difference to the introduction of the NLW. Since this analysis focuses on individuals in the first year of a qualification, we would not expect to see any impacts of the minimum wage change that occurred several years prior.

Looking at the impact on undergraduate education in Panel (2), there appears to be a very small negative impact from 2017/18 onwards, although this is statistically insignificant. The impact on apprenticeship participation in Panel (3) appears to be slightly positive immediately after the introduction of the NLW, but again, this effect is also statistically insignificant.

Defining cohorts based on age in September

In the baseline analysis, the treatment and control cohorts were defined based on their age in April 2016, when the NLW was introduced. However, since the education data is captured by academic years starting in September, some individuals in the control group (24 years old in April 2016) turned 25 by September 2016 and became eligible for the NLW.

Individuals may consider their eligibility for the NLW in September, when they would start a new qualification, rather than their eligibility in April. We also estimate the impact of the introduction of the NLW on educational participation when defining the treatment and control group based on their age in September 2016, rather than their age in April 2016. The results are reported in the Annex.

Differences in **overall enrolment** after the introduction of the NLW appear to be slightly positive but close to zero and statistically insignificant, except among non-white women, where the effects from 2016/17 are positive and significant. However, the treatment and control groups do not appear to follow a fully similar trend before 2015/16.

Looking at **undergraduate education** participation, there remain significant differences between 25- and 24-year-olds despite controlling for cohort and year differences, which could be driven by age-specific trends, which we cannot control for in this specification. These differences are highly significant in 2010/11 and 2011/12 but only marginally significant in the years closer to the introduction of the NLW. The specifications that include all individuals, those from more deprived areas, non-white men, and non-white women, find a small positive effect of the introduction of the NLW on participation in undergraduate education. However, since the assumption of parallel trends before treatment is not fully met, these estimates should be treated with caution.

The estimated impact is also near zero and statistically insignificant for **postgraduate education** and **apprenticeships**.

Falsification tests

Previously, the baseline analysis compared those who are eligible for the NLW in April 2016 (25-year-olds) to those who are not eligible (24-year-olds in the baseline analysis, and 23-year-olds in the additional analysis). As a **falsification test**, comparing two cohorts which were both eligible in April 2016 (26 and 25-year-olds) and two cohorts which were both ineligible in April 2016 (24 and 23-year-olds).

In the first case, we would not expect to find any effects of the introduction of the NLW. In the second case, we might expect to see some delayed effects of the NLW, since 24-year-olds became eligible for a different minimum wage rate (the NLW) from April 2017 onwards, followed by 23-year-olds in 2018. Figure 19 shows the impact of the NLW on the various educational participation outcomes for all individuals, comparing 26 to 25-year-olds. As expected, **the estimated difference after the introduction of the NLW is close to zero and not statistically significant across all outcomes**.

Similarly, we compare **24- and 23-year-olds**, where neither the control nor the treatment group is eligible for the NLW initially. However, 24-year-olds become eligible for the NLW by April 2017 latest, and the control group by 2018 at the latest; there could have been a delayed effect of the introduction of the NLW.

Figure 20 shows the results for all individuals in the sample; the results by sub-group can be found in the Annex. There are significant negative differences between 24- and 23-year-olds in overall enrolment, further education participation, and apprenticeship participation, and positive differences in undergraduate participation in the years prior to the introduction of the NLW, particularly in 2010/11 and 2011/12. The cohorts are more similar closer to the introduction of the NLW.

After the introduction of the NLW in 2016, the effects are mostly insignificant and very close to zero. However, when looking at further education participation and apprenticeship participation, there appear to be very small positive and marginally significant effects from 2017/18 onwards (i.e., when 24-year-olds are eligible for the NLW).

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Figure 19 Falsification results (26 vs. 25-year-olds) for all individuals: All outcomes
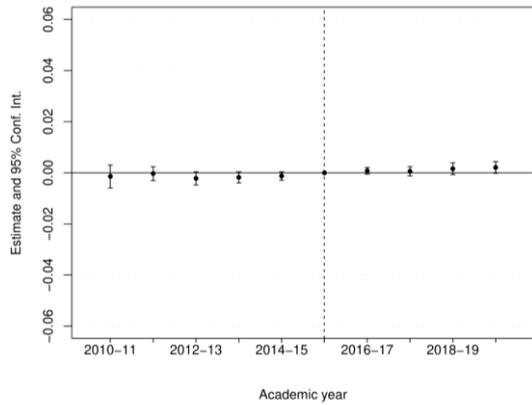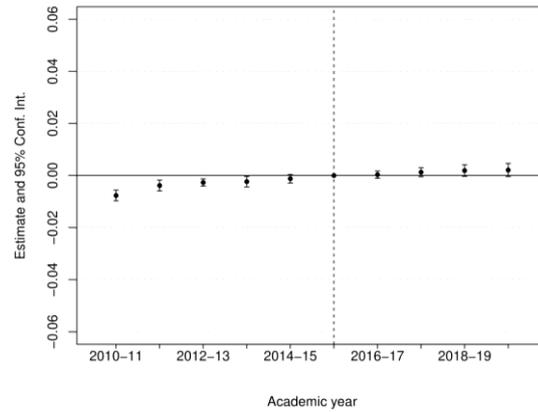
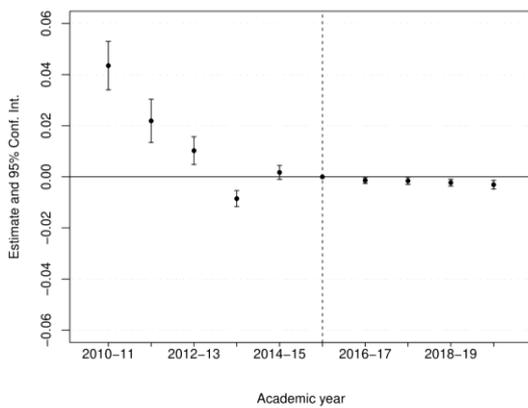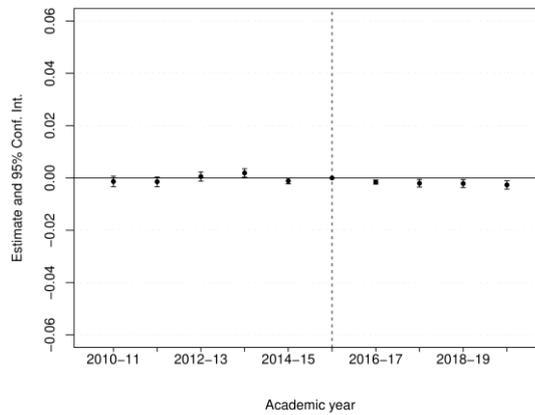### (1) Enrolled

### (2) Further education

(3) Undergraduate education

(4) Postgraduate education
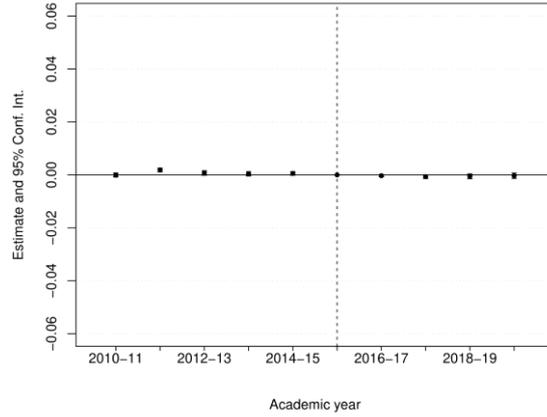
(5) Apprenticeship
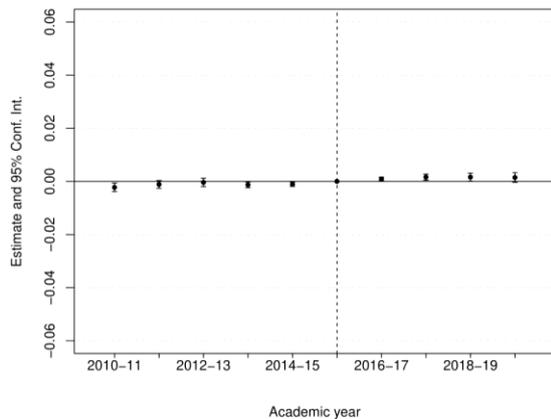
Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

Figure 20   Falsification results (24 vs. 23-year-olds) for all individuals: All outcomes
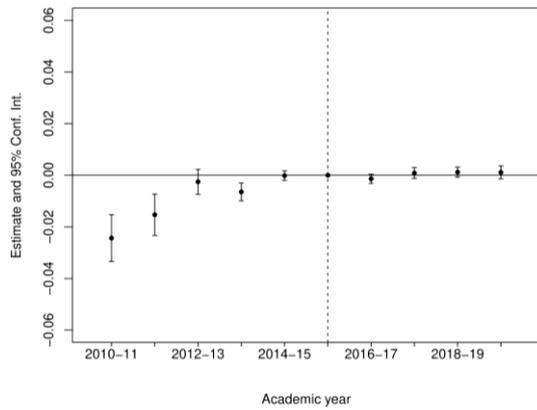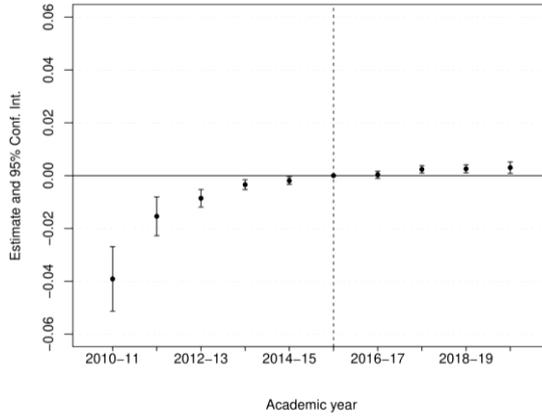
(1) Enrolled
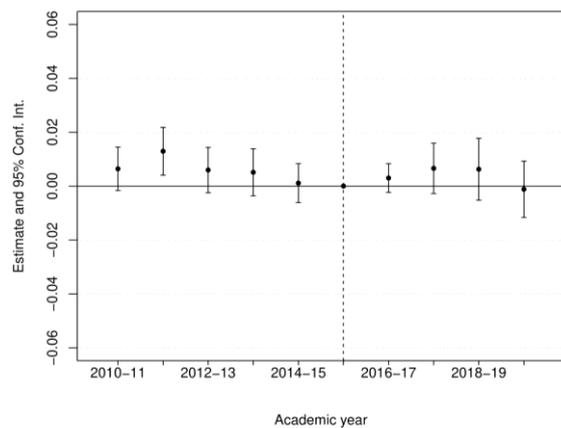

(2) Further education

(3) Undergraduate education

(4) Postgraduate education

(5) Apprenticeship

Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference in enrolment between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

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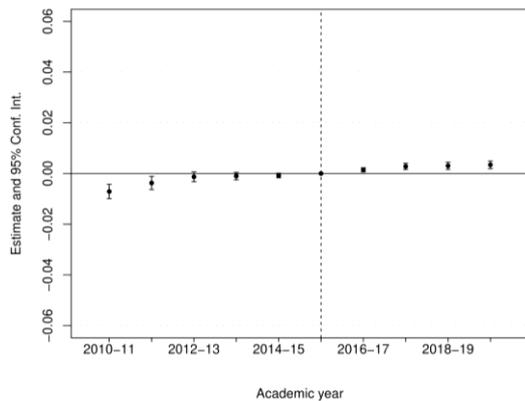
As expected, there are **no significant differences between 26- and 25-year-olds attributable to the introduction of the NLW**, as both groups are already eligible for the NLW. When comparing 24- and 23-year-olds, the results suggest there might be **marginally significant positive effects of the NLW once the 24-year-olds are eligible for the NLW on further education and apprenticeship participation,** but no impact on the other educational participation outcomes.

5.2 Labour market participation

We also explore the potential for LEO data tables to analyse the impact of minimum wage changes on labour market outcomes. These include

- **employment**,
- duration of the **longest employment stint** within a given tax year,
- the **number of employment stints** within a tax year, and
- **annual earnings** (for those who were employed).

Unlike other sources used to estimate the impact of minimum wage changes on labour market outcomes (such as ASHE), **LEO does not contain any information on hours worked, and cannot be used to estimate hourly wages.**

As shown in the descriptive statistics in Section 4.2, the cohorts differ in regard to their employment outcomes in the years closer to 2010/11, where a one-year age difference appears to be associated with large differences in employment rates, annual earnings, and employment stability.

However, as both the treatment and control cohorts grow older, the **differences tend to get smaller**. Ideally, the difference between the treatment and control group would remain relatively stable throughout the period of analysis in the absence of the treatment. **Since the descriptive trends suggested *changes in the difference* between the treatment and control groups, not just a difference in the levels, the assumption of parallel trends before the treatment introduction does not appear to be fully met.**

5.3 Baseline results

As shown in Figure 21, the differences between treatment and control groups (relative to the baseline) before the introduction of the NLW are significant in the earlier years. Closer to the introduction of the NLW, the differences between the treatment and control group tend to become smaller, similar to previous results concerning participation in education.

When including all individuals, there is **no estimated effect of the introduction of the NLW on employment**. In addition, the treatment effects appear to be slightly negative but insignificant across the different sub-groups. Given the treatment and control group do not follow similar trends before the introduction of the NLW, and since the descriptive findings showed that the differences between both groups become smaller over time, these slightly negative estimates might partly capture the *declining difference* between treatment control group relative to 2015/16 in the years after the introduction of the NLW rather than a negative impact of the NLW on employment. The regression tables can be found in the Annex.

Figure 21   Baseline results: Employment
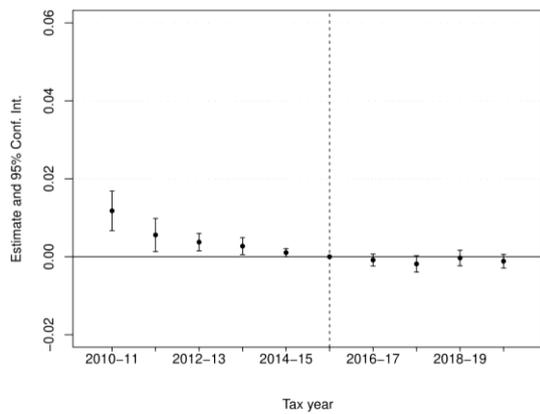
(1) All

(2) Free school meals
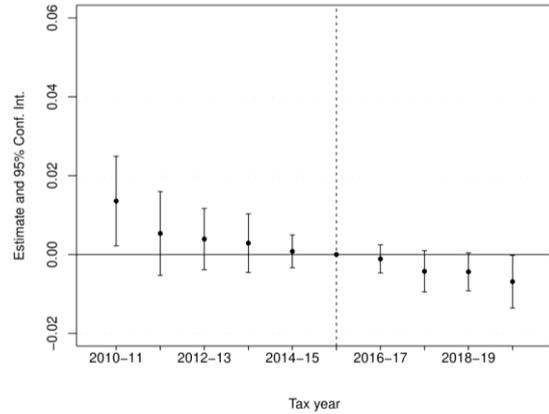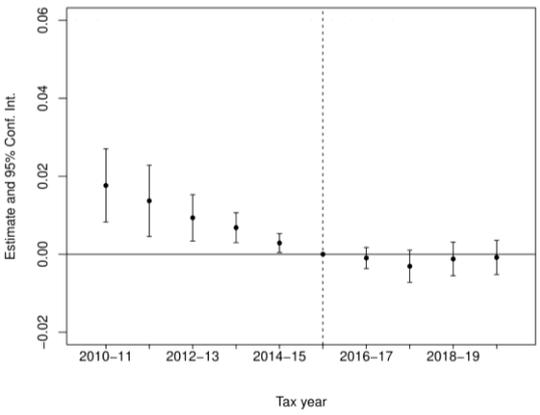
(3) Living in a deprived area at 15

(4) Lower than Level 2 qualification by 19

(5) Female and non-white
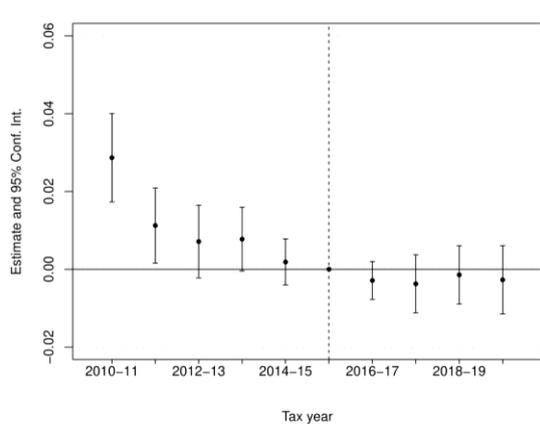
(6) Male and non-white
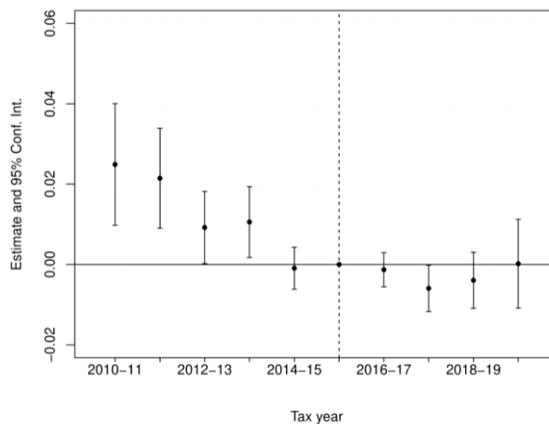
Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference between treatment and control group in each year relative to the difference in 2015/16 (the baseline).
Source: LE analysis of LEO data tables.

Figure 22 shows the results of the impact of the introduction of the NLW on the **number of employment stints within a tax year**. A larger number of employment stints is generally an indicator of more volatile employment conditions.

Similarly to employment, the differences between the treatment and control group appear to be larger around 2010/11, when the cohorts were 18 and 19. Closer to the introduction of the NLW, the differences become smaller as both groups get older. Including all individuals, there is no effect on the number of employment stints from 2016/17 onwards. For some of the sub-groups, the estimated coefficients are slightly negative but insignificant. Given that the treatment and control groups, **when focusing on sub-groups, do not seem to follow a similar trend in the period before the introduction of the NLW,** it is not possible to say whether the introduction of the NLW actually led to more stable employment among certain sub-groups.

When looking at the impact of the introduction of the NLW on **annual earnings for those individuals who are employed** (see Figure 23), the estimates are positive in the years at the beginning of the period of analysis (2010/11). This means the difference between the treatment and control groups was larger in 2010/11 compared to the difference in 2015/16. This is consistent with the descriptive findings in Figure 12, where the difference in annual earnings between cohorts was larger at the start of the period of analysis.

The post-2016 estimates suggest no impact of the NLW on annual earnings when looking at all individuals (Panel 1) or those from more deprived areas (Panel 3). Among the other sub-groups, the estimates are slightly negative but insignificant. A negative impact of the NLW on annual earnings could be due to a reduction in hours as a result of the NLW. However, since we cannot observe hours worked in LEO data tables, we are unable to confirm this. The slightly negative differences (compared to the baseline year) could also be due to the **underlying differences in the trend between the treatment and control groups that are not captured by controlling for year, cohort, age, sector, and area**.

Figure 24 shows the estimates of the impact of the NLW on **the duration of the longest employment spell,** again only for those individuals who are employed at some point within a given tax year. A longer employment duration would be an indicator of more stable employment. The estimates following the introduction of the NLW are generally **not statistically significant but slightly positive**. This could be an indicator that employment becomes slightly less volatile following the introduction of the NLW, in line with the findings on the number of employment stints from Figure 22.

However, since the treatment and control groups do not appear to follow a similar trend in the entire pre-treatment period (except non-white men), it **cannot be concluded from these estimates that the NLW had a causal impact on employment stability**.

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Figure 22   Baseline results: Number of employment stints
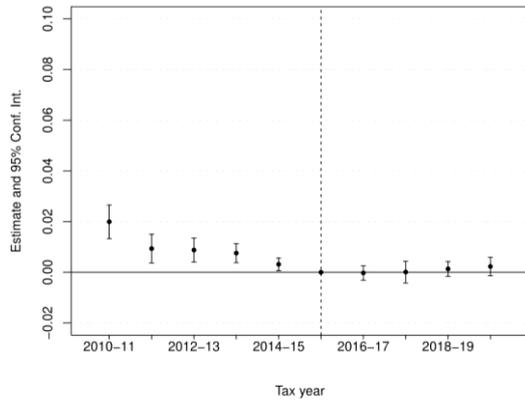
(1) All

(2) Free school meals
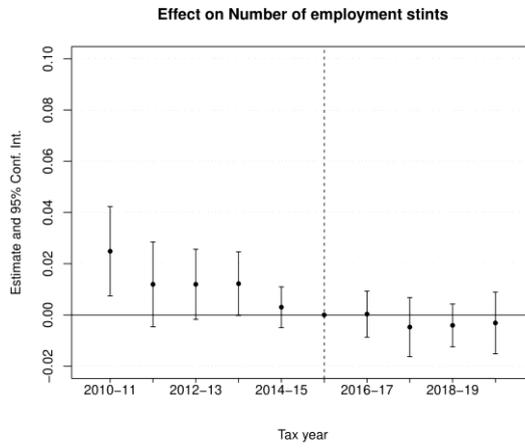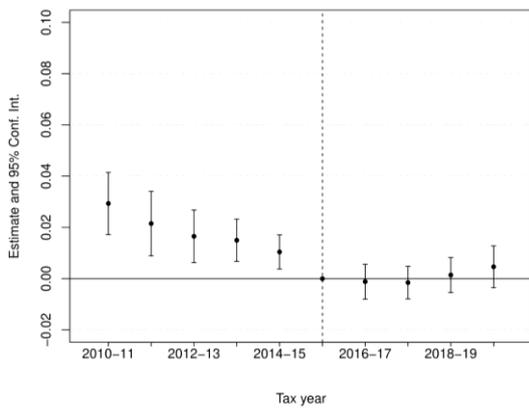
(3) Living in a deprived area at 15

(4) Lower than Level 2 qualification by 19

(5) Female and non-white

(6) Male and non-white

Note: Filled circles represent the coefficient point estimates, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

*Source: LE analysis of LEO data tables.*

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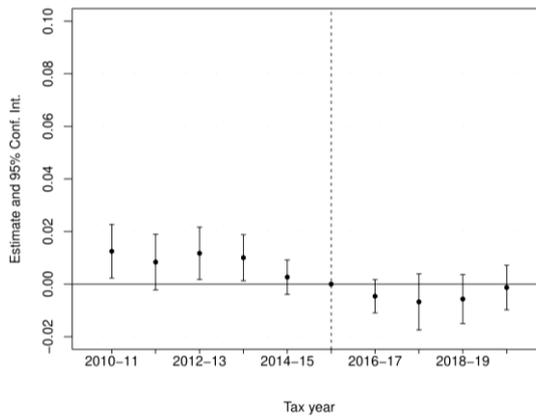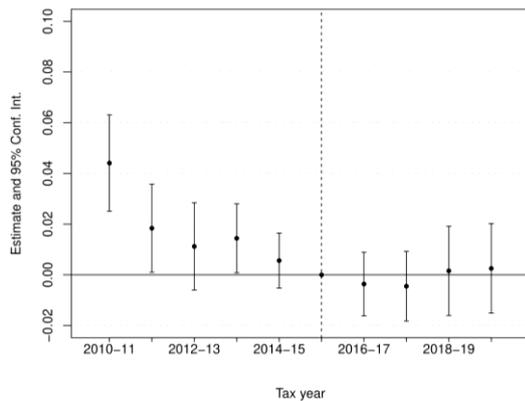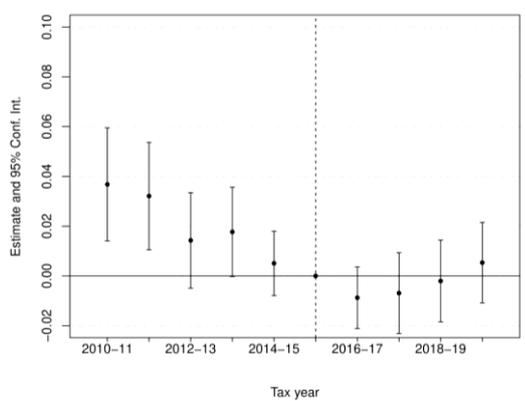
Figure 23   Baseline results: Earnings (log)
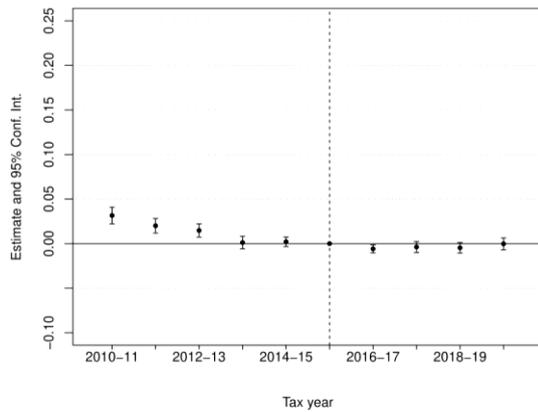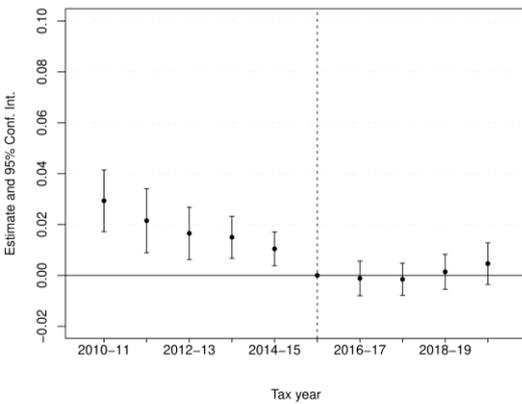
(1) All

(2) Free school meals

(3) Living in a deprived area at 15

(4) Lower than Level 2 qualification by 19
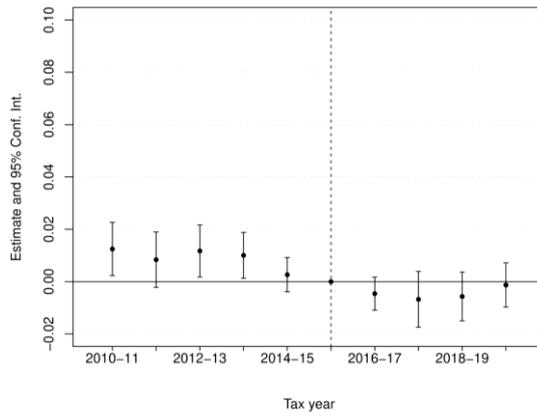
(5) Female and non-white
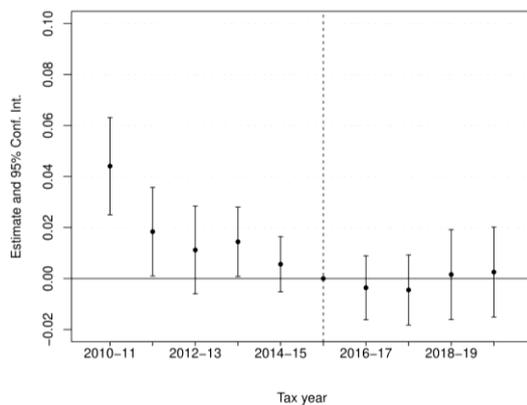
(6) Male and non-white
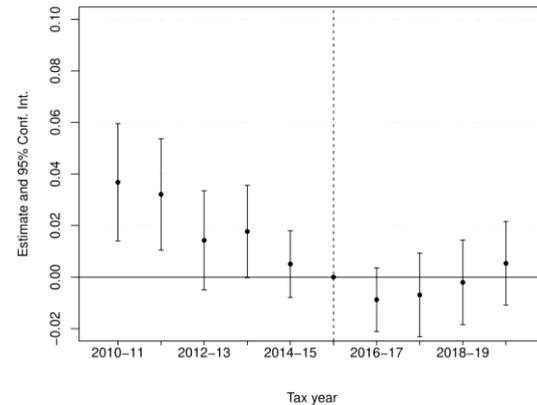
Note: Filled circles represent the coefficient point estimates, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

Figure 24   Baseline results: Duration of longest employment stint
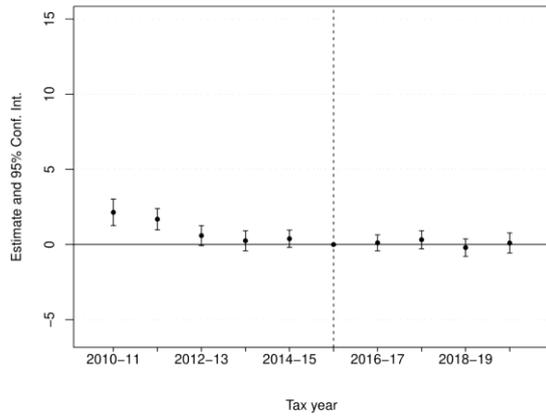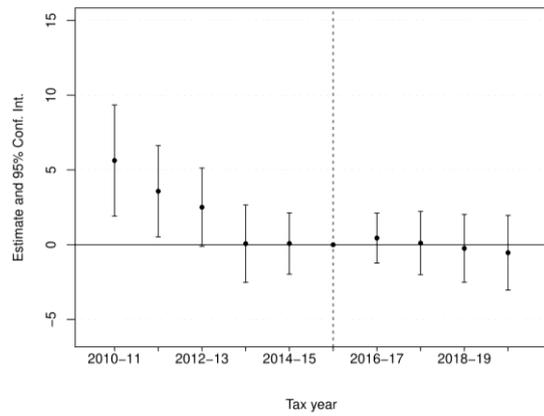
(1) All

Tax year

(2) Free school meals

Tax year

(3) Living in a deprived area at 15

Tax year

(4) Lower than Level 2 qualification by 19

Tax year

(5) Female and non-white

Tax year

(6) Male and non-white

Tax year

Note: Filled circles represent the coefficient point estimates, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

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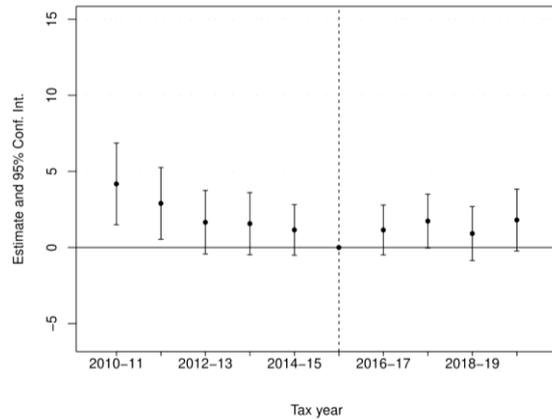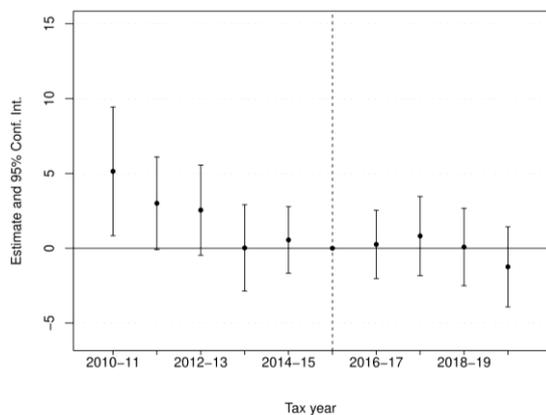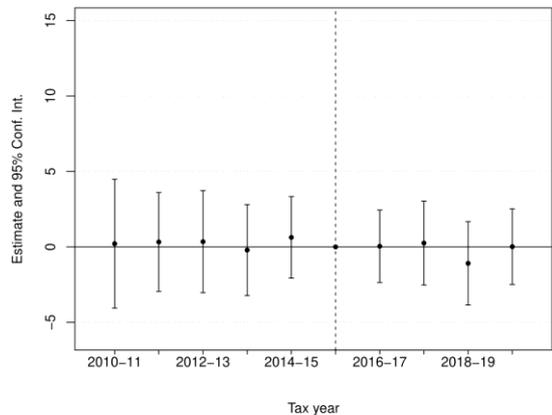
In summary, these **findings need to be treated with caution, since the treatment and control groups do not appear to follow a similar trend at the beginning of the period of analysis**. The estimates after the introduction of the NLW are generally statistically insignificant and essentially zero when looking at all individuals. When looking at the effects among sub-groups who are more likely to be exposed to lower wages, the effects are still insignificant, but the direction of the effects might indicate a slightly negative effect on overall employment, less volatile employment (longer duration, fewer different employment stints within a year), and potentially a negative impact on *annual* earnings.

5.4   Additional results

5.4.1   Comparing 25 to 23-year-olds

Since the control group used in the baseline analysis, aged 24 when the NLW is introduced, becomes eligible for the NLW within the 12 months following the introduction of the NLW, we also compare the treatment group to those aged 23 when the NLW is introduced, since they are not eligible for the NLW for another year. However, **these two cohorts may have been less similar than 25 and 24-year-olds**, based on the descriptive findings shown in Section 4.2.

The regression results are shown in the Annex for all individuals in the cohort. There are significant differences in the trend between the treatment and control groups before the introduction of the NLW, suggesting they may not be a good comparison. The estimated change after the introduction of the NLW is near zero for employment, earnings, and the number of employment stints. Results by sub-groups show similar patterns.

While 23-year-olds are not eligible for the NLW for another year after 24-year-olds from the original control group are eligible, **the 23-year-old control group does not appear to be a suitable control group for 25-year-olds** when looking at employment outcomes.

5.4.2   Low-pay industries

For individuals who are employed at some point within a tax year, LEO data tables contain information on their **main industry (SIC2007)**.

Any potential impact of the introduction of the NLW is more likely to be observed when limiting the sample to those working in industries that are classified as low-pay industries by the Low Pay Commission[24]. Figure 25 shows the impact of the introduction of the NLW on earnings, for all individuals and for the five sub-groups (more details can be found in the Annex). Similar to the baseline results, there was a significant difference between those in the treatment group and those in the control group (those who were 25 and 24 years old in April 2016, respectively) relative to 2015/16, before the minimum wage introduction. This difference gradually declines in the years leading up to the introduction of the NLW, except among non-white women.

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[24] Low Pay Commission (2023). Low-paying sectors review. Link.

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**Figure 25   Low-paying sectors results: Earnings (log)**
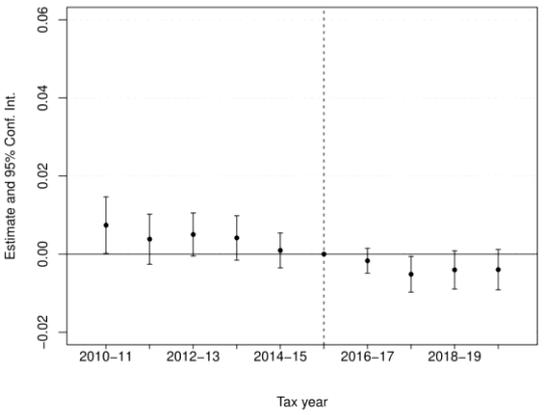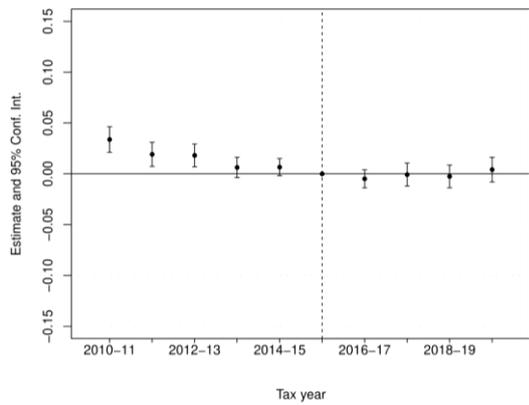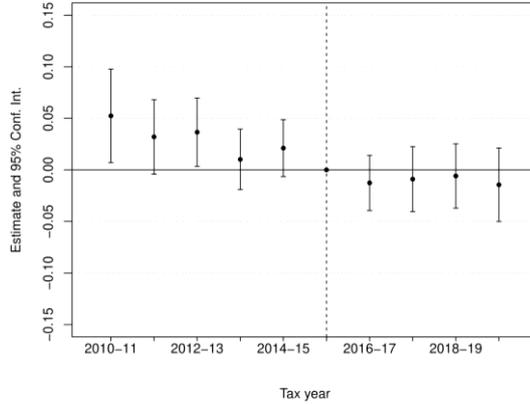
(1) All

(2) Free school meals

(3) Living in a deprived area at 15

(4) Lower than Level 2 qualification by 19

(5) Female and non-white

(6) Male and non-white

Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

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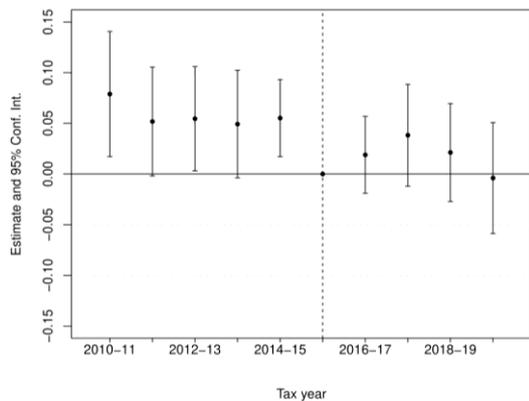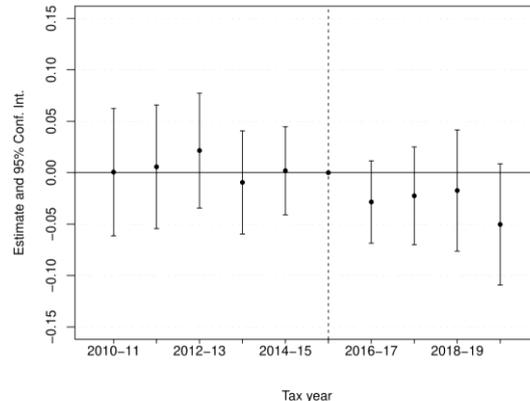
There does not appear to be any effect of the introduction of the NLW on annual earnings in low-pay industries. Among individuals who were eligible for free school meals, those with lower prior educational achievements, those from more deprived areas, and non-white men, the post-treatment effect is slightly negative but generally insignificant, except for those from more deprived areas in 2016/17. Among non-white women, there is a small positive but insignificant effect of the introduction of the NLW. However, as non-white women in the treatment and control group did not appear to follow a similar trend before the introduction of the NLW, these estimates cannot be taken as evidence for a positive impact of the NLW on earnings.

Due to the remaining differences between treatment and control groups before the introduction of the NLW, all of these estimates need to be treated with caution. One possibility why earnings could be lower following the introduction of the NLW is if the introduction of the NLW had a negative impact on hours worked. Even with higher hourly pay, this could result in lower annual earnings. However, as the LEO data tables do not contain information on hours worked, this cannot be tested in this analysis.

Figure 26 shows the impact on the **duration of the longest employment stint in a given tax year**. In earlier years (e.g., 2010/11), there was a significant difference between treatment and control cohorts. At the start of these years, the treatment cohort is 19 years old and the control cohort 18 years old, so the difference concerning labour market outcomes from one year to the next is likely to be larger than the difference in 2015/16 (the baseline year), when they are 24 and 23, respectively. Indeed, the cohorts are more similar in the years closer to the introduction of the NLW.

Generally, the introduction of the NLW did not appear to have an impact on the longest employment duration, neither for all individuals nor for sub-groups. There is a small positive but insignificant effect when looking at individuals who eligible for eligible for free school meals in the past, those with lower prior educational achievements, or non-white women. Among non-white men, there appears to be a small but insignificant negative impact on their longest employment duration.

Overall, this analysis found **no evidence of any statistically significant effects of the introduction of the NLW on labour market outcomes**. The direction of the results could suggest there might have been a small negative impact on employment and annual earnings, as well as a positive impact on employment stability.

However, even when controlling for cohort, year, and age, as well as area and sector (when focusing only on those who are in employment), the treatment and control groups do not always follow a similar trend in the years prior to the introduction of the NLW. Given these remaining differences that cannot be controlled for in the current specification, any non-zero estimates in the years after the introduction of the NLW cannot be interpreted as causal evidence for an impact of the NLW.

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Figure 26   Low-paying sectors results: Duration of longest employment stint
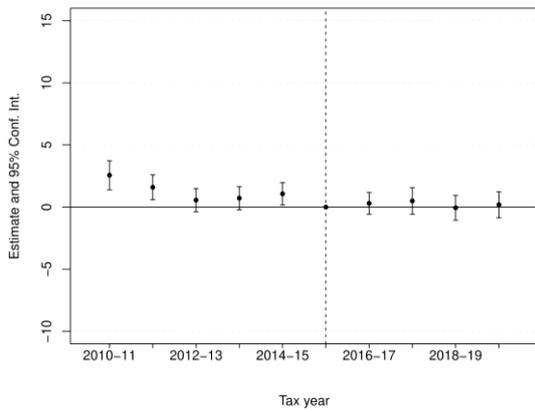
(1) All

**(2) Free school meals**
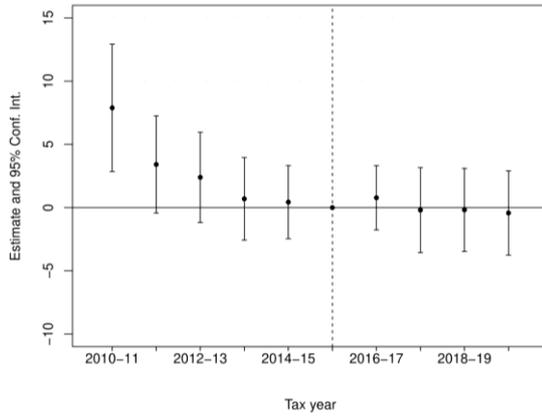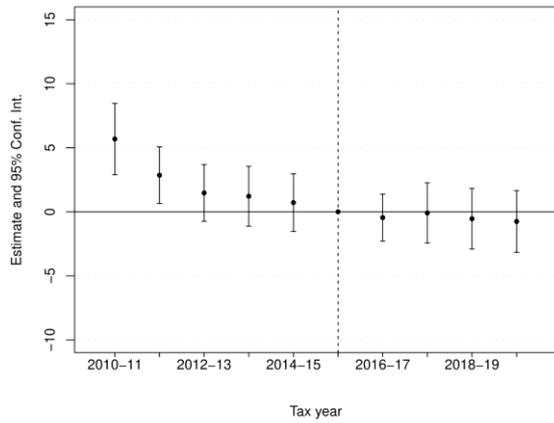
**(3) Living in a deprived area at 15**

(4) Lower than Level 2 qualification by 19

(5) Female and non-white

(6) Male and non-white

Note: Filled circles represent the point estimates of the coefficients from the regression analysis, and the whiskers represent the 95% confidence interval. Coefficients estimate the difference between treatment and control group in each year relative to the difference in 2015/16 (the baseline).

Source: LE analysis of LEO data tables.

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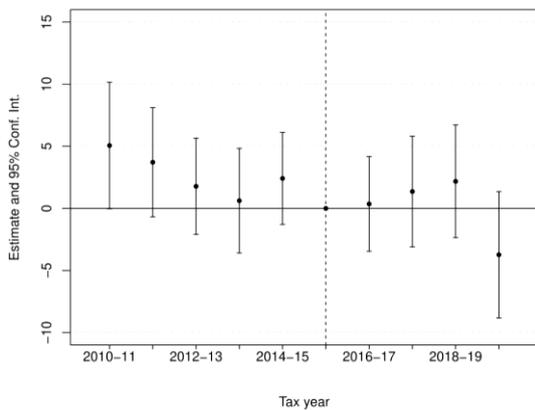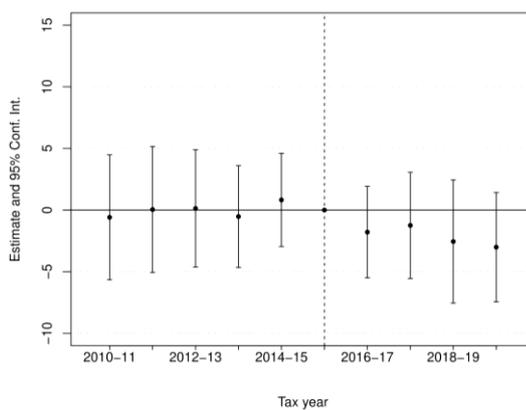
6   Discussion and caveats

The findings suggest that there was **no significant impact of the NLW on young people's labour market outcomes or their decisions regarding education**. This is the case when comparing **entire single-year age cohorts** as well as when focusing on more **specific sub-groups** who are more likely to be impacted by changes to the minimum wage.

There are several potential reasons why no significant impact may have been observed.

The descriptive analysis showed that enrolment in education is much higher when individuals are aged 18 or 19 and gradually declines as people grow older. When the NLW was introduced in 2016, **only around 15% of individuals in the treatment and control groups (aged 24/25 at the time) still participated in some form of education, compared to around 45% in 2011/12, when individuals in the treatment and control group were aged 20/21**. With relatively fewer individuals considering education at age 24/25 compared to younger age groups, younger cohorts might be a better group to understand the impact of minimum wage changes on educational participation. Those who are still in education at age 24/25 might also have different motivation or considerations than those aged 20/21. As a result, more recent minimum wage chan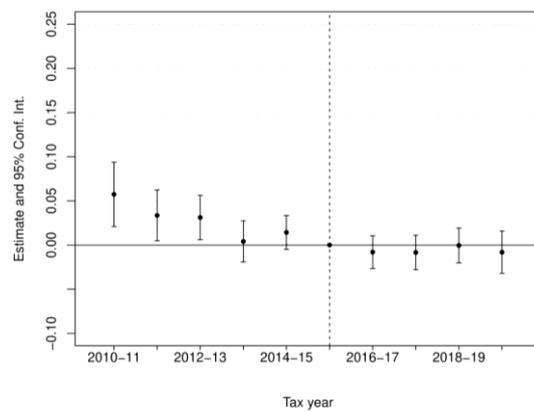ges that impacted younger age groups (e.g., the expansion of entitlement to the NLW in April 2024 to 21- and 22-year-olds) may have had a more significant impact on educational participation than the introduction of the NLW in April 2016.

Another reason may be an important limitation of the LEO data tables. While the impact on sub-groups more likely to be impacted by changes to the minimum wage has been investigated, the lack of hourly pay or hours worked data **limits the ability of the analysis to focus on minimum wage workers.**

In addition, the lack of an impact may be driven by **many in the control group** (those aged 24 in April 2016) **becoming eligible for the NLW soon after April 2016**, which somewhat limits the difference in treatment status between the treatment and control groups. In addition, the NLW introduction might have had spillovers on to younger workers, as Giupponi and Machin (2018) found. Hence, individuals in the control group might have also been able to earn higher wages following the NLW introduction.

One of the key caveats for the analysis of educational participation in this analysis is that there is a **temporal mismatch between the introduction of the NLW in April** (consistent with the start of the tax year) **and the start of the academic year**. Since the **education data is captured by academic year, the data is not fully consistent with the timing of the treatment**. The implications for our analysis depend on when decisions about education in the subsequent academic year are made, which may differ significantly depending on the type, start date, and length of qualification.

The analysis also **focuses on a 'main qualification' in each academic year**, but changes to the minimum wage may impact those working towards multiple qualifications within an academic year differently than others.

Finally, there may be **other factors that are not sufficiently controlled for in the analysis**, as evidenced by a lack of parallel trends before the introduction of the NLW for some outcomes and samples, in particular at the start of the period of analysis in 2010/11.

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7   Conclusion

7.1   Summary of findings

This research analysed the **introduction of the National Living Wage (NLW) in April 2016** on educational participation and labour market outcomes of young people. While the impact of changes to the minimum wage in the UK has been widely studied (London Economics, 2022), the **literature on the impact on educational outcomes is relatively scarce**.

This analysis is the **first to use the Longitudinal Education Outcomes (LEO) data** to study the impact of changes to the minimum wage. LEO comprises **detailed information on individuals' educational journey from Early Years education through all key stages, as well as further and higher education**. Given that the data is administrative, LEO captures every individual born after 1985/86 who has engaged with the school system in England at some point. The data also contains detailed personal characteristics as well as information on employment, earnings, sector and location from DWP and HMRC tax records.

The advantage of LEO is the **level of detail and size of the cohorts**, as each cohort consists of roughly 500,000 individuals, and given its administrative nature, it does not suffer from the same issues as survey data. One of the main drawbacks is that LEO **does not contain information on hours worked or hourly wages, only annual earnings**.

The analysis uses the LEO data to estimate the impact of the NLW introduction in 2016, as this presented a significant increase in the minimum wage rate for those who were eligible for it. We compare those who were eligible for the NLW (aged 25 in April 2016) to those who were not yet eligible for the NLW (aged 24 in April 2016) using a differences-in-differences design with multiple periods before and after the NLW introduction (similar to an event study design).

The analysis does not show **any statistically significant effects of the NLW on educational participation**, neither for overall education nor on participation in further education, undergraduate education, postgraduate education, or apprenticeships. This also holds when looking at certain sub-groups only, who we expect to have a higher likelihood of minimum wage exposure. However, as **only around 15% of individuals in the control and treatment groups are still participating in some form of education at age 24/25** when the NLW is introduced, **it seems unlikely that the NLW would have an impact on educational participation among this age group**.

With respect to labour market outcomes, the **analysis investigated employment, annual earnings, the number of separate employment spells, and the duration of the longest employment spell within a tax year**. The analysis does **not find any statistically significant effects** on any of these labour market outcomes. This also holds when looking at certain sub-groups with a higher likelihood of earning lower wages or when looking only at sectors that are classified as low-paying sectors. Given the absence of hours worked in the LEO data, it is not possible to analyse the impact of the NLW introduction on employment regarding hours worked.

7.2   Recommendations for further research

A similar analysis on the effect on educational participation could be repeated to look **at more recent NLW changes,** exploiting the changes in eligibility for the NLW from 25 to 23 or 23 to 21, once more recent LEO data is available, as a larger proportion of those younger age groups are either

in education or considering education. However, as shown in section 4.1.2, younger individuals around the age of 20 not only have higher rates of educational enrolment, but there is also more variation across qualification types and levels around that time. When comparing different cohorts based on age, this is important to note when looking at the impact of the NLW on younger individuals.

Both HESA and the ILR contain considerable detail about individuals' qualifications, such as attainment, subject, type of course or degree, whether a qualification was undertaken at the part-time or full-time level (in HESA), or whether a qualification was workplace-based or community-based learning (in the ILR). Further research could investigate the impact of the **type of qualification in more detail** than in this study (which has focused on the level of qualification), which would allow researchers to **understand the transmission mechanism more clearly**.

For example, further research may explore educational participation by focusing on a particular type of qualification (from HESA) and investigating the impact on **part-time and full-time study**. Individuals may move to **part-time study while working in a minimum wage job in order to finance their studies**. Alternatively, future research could look at whether there are any **shifts in subjects or course types** related to minimum wage changes.

When looking only at qualifications recorded in the ILR, future research could explore the relation between changes to the minimum wage and **workplace-based learning**. Given that many qualifications in the ILR do not take a full academic year, future research could organise the data at the monthly level by using start and end dates of qualifications to study the impact of minimum wage changes monthly. Due to the size of the cohorts, this would be computationally intensive, but when only looking at a short period before and after a minimum wage change or focusing only on certain types of qualifications, this could capture any short-term impacts. This would also overcome the issue of the temporal mismatch between the education data and tax data and the timing of minimum wage changes.

In this analysis, the **treatment and control groups did not always appear to follow a fully similar trend before April 2016**. If there are remaining differences between the control and treatment groups that cannot be captured in the model, the **honest differences-in-differences approach** developed by Rambachan and Roth[25] could be used to assess whether any estimated treatment effects are 'credible enough' given the pre-treatment differences or if they could just be noise given the unaccounted pre-treatment variation.

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[25] Rambachan, A. & Roth, J. (2023). A more credible approach to parallel trends. Review of Economic Studies, 90, 2555-2591. doi:10.1093/restud/rdad018

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Annex 1   Alternative data sources

The following data sources are not used in the analysis, so the following discussion solely focuses on their relative strengths and limitations compared to the LEO data tables.

A1.1   Labour Force Survey (LFS) and Annual Population Survey (APS)

The LFS is the largest household study in the UK recording labour market outcomes, with sample sizes of 63,000 individuals in Q3 (July to September) 2025 (down from around 80,000 in Q3 2015). The LFS is a representative sample of the UK resident population, and each respondent in the survey is tracked over five consecutive quarters.

However, due to declining response rates[26] and sample sizes and increased volatility, there are **key concerns about the reliability of recent LFS estimates**[27]. In fact, due to these quality concerns, LFS and APS are currently considered designated as "official statistics in development". In addition, around 37% of responses in Q3 2025 provided by a proxy respondent living in the same household. These rates are much higher for young people, the key group of interest in this analysis. Moreover, the LFS is based on self-reported information, with potential measurement error. The Annual Population Survey (APS) comprises information from the Labour Force Survey (a subset of LFS variables), plus an APS sample boost, and it is organised on an annual basis.

A1.2   Annual Survey of Hours and Earnings (ASHE)

The ASHE is an annual survey completed by employers, carried out in April each year and based on a 1% sample of employee jobs taken from HM Revenue & Customs (HMRC) PAYE records[28]. The sample size of ASHE up to 2019 was around 180,000 and remains large with a sample size of around 174,000 in 2025, having recovered from less than 150,000 in 2020, 2021, and 2022. ASHE provides information on earnings, hours of work, as well as occupation, region and industry. Recent research[29] suggested that the percentage of jobs paid at or below the National Minimum Wage has been under-estimated in ASHE, while the bite of the National Living Wage is greater than previously understood. Information from ASHE can be used to identify employees receiving the National Living Wage and the pay structure more generally. ASHE only covers employees, so it cannot be used to look at changes in employment.

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[26] LFS response rates declined from around 45% ten years ago (Q3 2015) to 23% in the most recent quarter available (Q3 2025). ONS link here.

[27] See ONS and Resolution Foundation

[28] ASHE has also been linked to HMRC data (link) up to 2022 (ASHE) and 2019 (PAYE)

[29] ESCOE link here.

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A1.3 Comparison of data sources

Table 2 Summary of data tables

	Longitudinal Education Outcomes (LEO)	ASHE	Labour Force Survey	Annual Population Survey
Type	Linked administrative data	Survey of employers	Household survey	Household survey
Coverage	Individuals in the English educational system	1% sample of PAYE employee jobs	UK resident population	UK resident population
Organised by	Academic year/Tax year	Tax year	Quarter	Year
Sample size	500,000 per cohort	150,000	55,000	150,000
Timeliness	Ad hoc (new release in the pipeline, but no specific date)	Collected in April, published the following February	6-8 weeks after the end of the reference period	3 months after the end of the reference period
Includes	KS4 cohorts of students linked to subsequent labour market and educational outcomes	Employees only	Employees, self-employed and those not in employment	Employees, self-employed and those not in employment
Key strengths	Quality of data and information, size	Sample size, employer survey	Range of variables	Range of variables
Key weaknesses	No information on hours or occupation. English educational system only	Limited personal characteristics. Sample bias	Concerns about sample size and data quality	Concern about sample size and data quality
Information on hours	Not available	Yes (from payroll)	Yes (self-reported)	Yes (self-reported)

Annex 2   Additional descriptive statistics

A2.1   Trends in educational participation by age

Figure 27   Enrolment by qualification level: by age

Source: LE analysis of LEO data tables.

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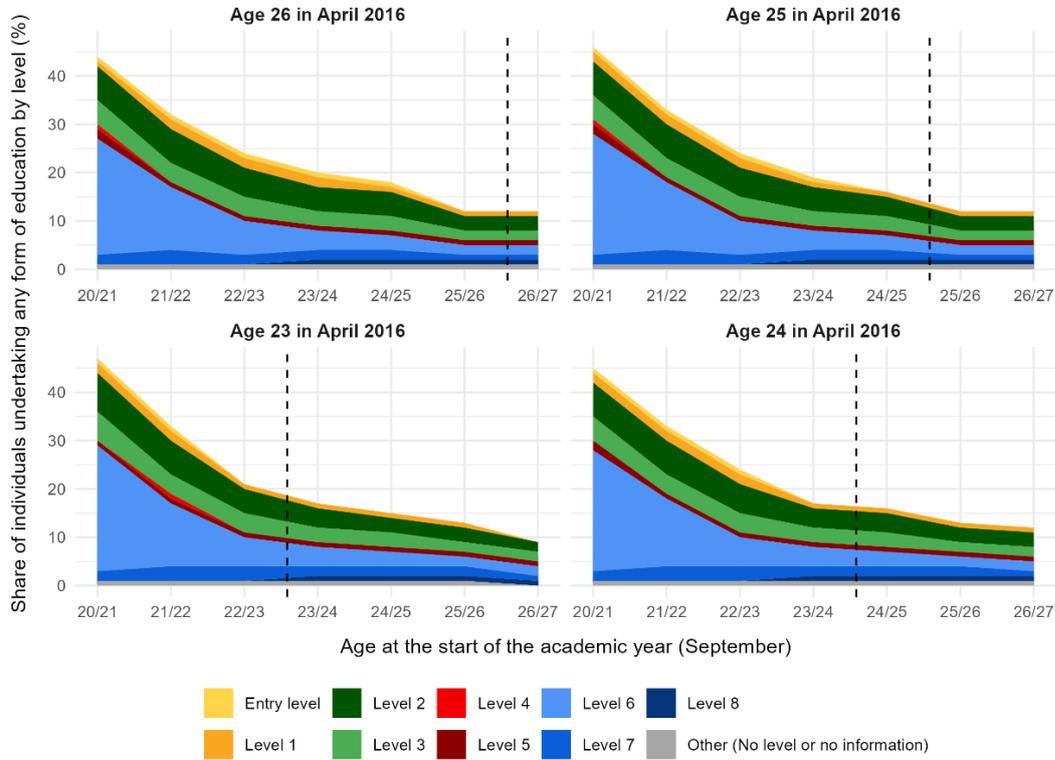
Figure 28   First year of enrolment status: by age
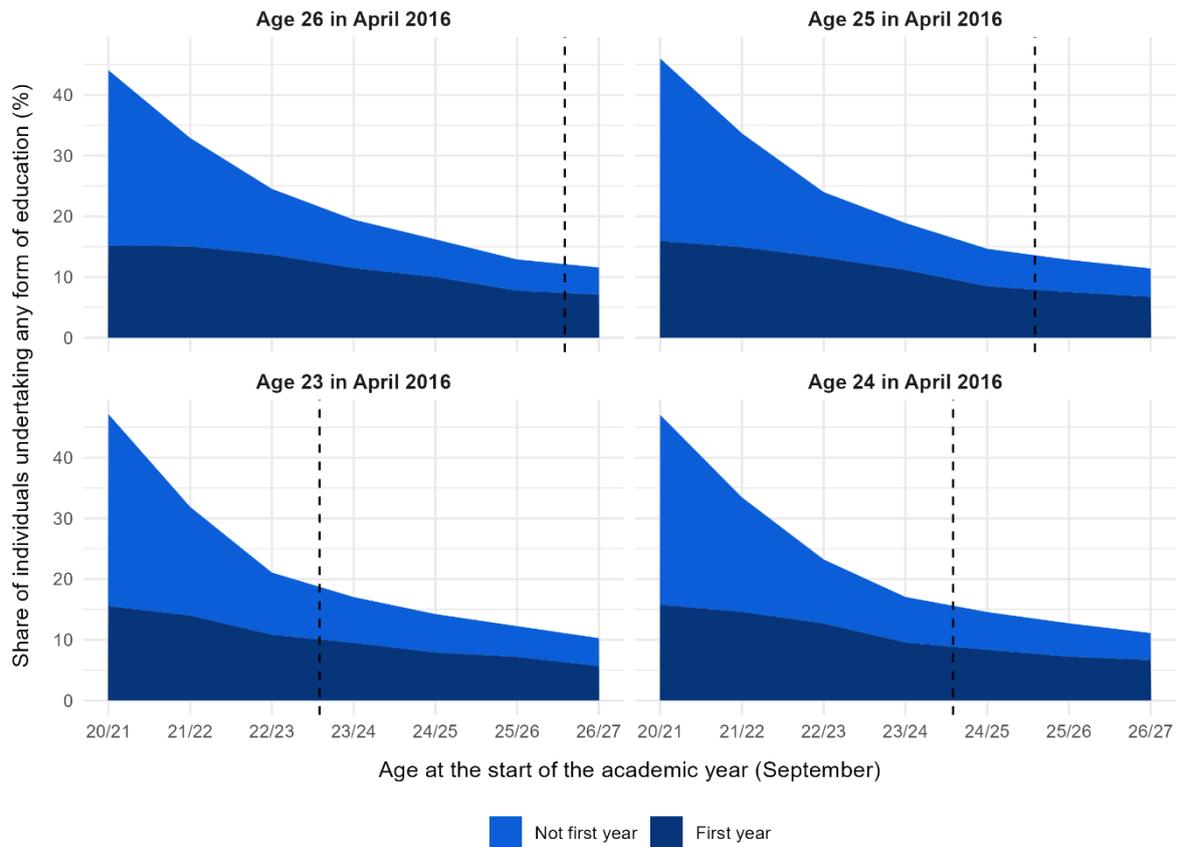
Source: LE analysis of LEO data tables.

A2.2   Personal characteristics by cohort

Our econometric specification is based on the extent to which the treatment and control groups would follow similar trends in the absence of the introduction of the NLW. This would be supported by similarities in educational, socioeconomic, and demographic characteristics. All four cohorts (aged 24 and 25 in April 2016, the main control and treatment groups, respectively, and those aged 23 and 26 in April 2016 are compared based on a range of personal characteristics.

When looking at the **highest level of education achieved at age 19**, our cohorts appear to be broadly similar (see Figure 29). Around half of individuals in each cohort achieved a Level 3 qualification by age 19 (A-levels or equivalent). Among younger cohorts, the proportion of individuals within each cohort who have achieved Level 3 by age 19 is slightly higher than in the older cohorts, suggesting there might be a slight shift towards higher qualifications over time.

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Figure 29   Highest level of education achieved at age 19
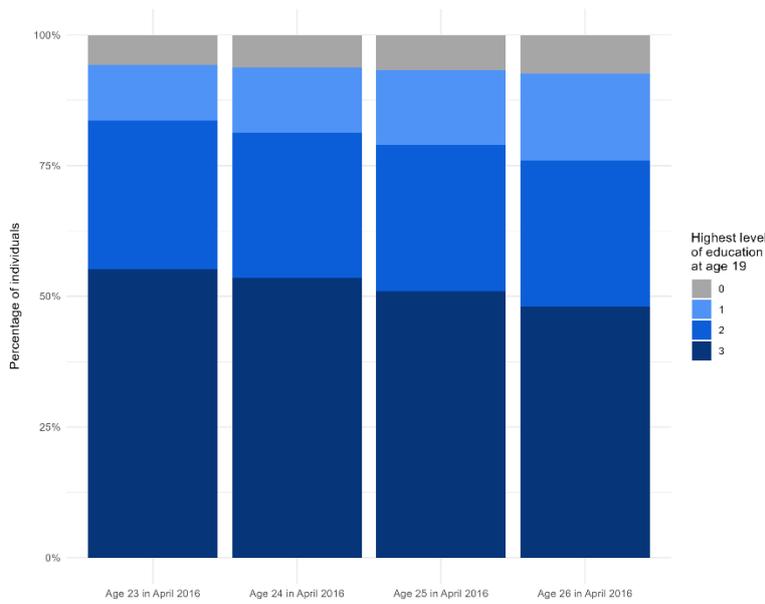
Source: LE analysis of LEO data tables.

We compare the four cohorts based on the distribution of individuals in each cohort according to the relative child deprivation of the area where individuals lived at age 15. Child deprivation is measured by the Income Deprivation Affecting Children Index (IDACI), which measures the proportion of children living in low-income families within a specific area in England. Each postcode in England is ranked based on its IDACI score to create deciles. As shown in Figure 30, the four cohorts are very similar according to the distribution of how areas where individuals lived at age 15 rank.

Figure 30   Local area deprivation (IDACI score decile) of postcode at age 15

Source: LE analysis of LEO data tables.

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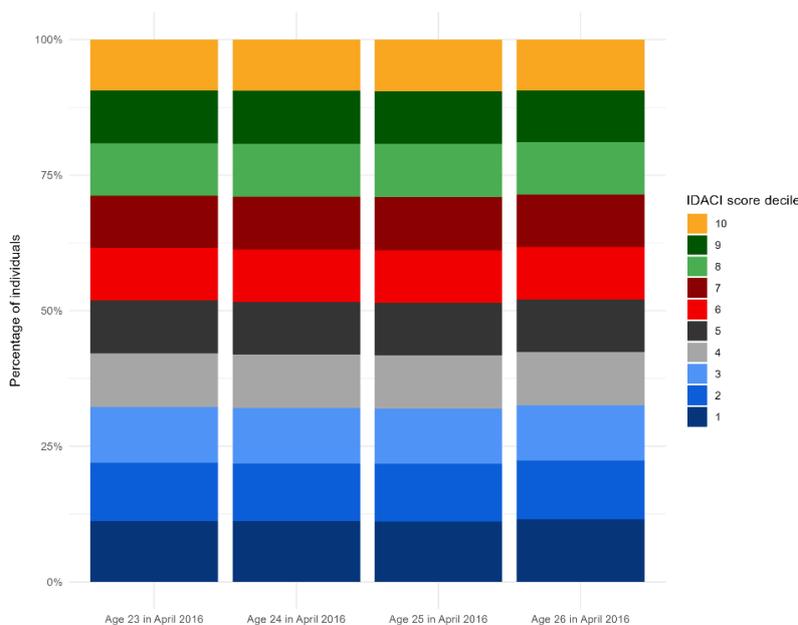
Figure 31   Ethnicity
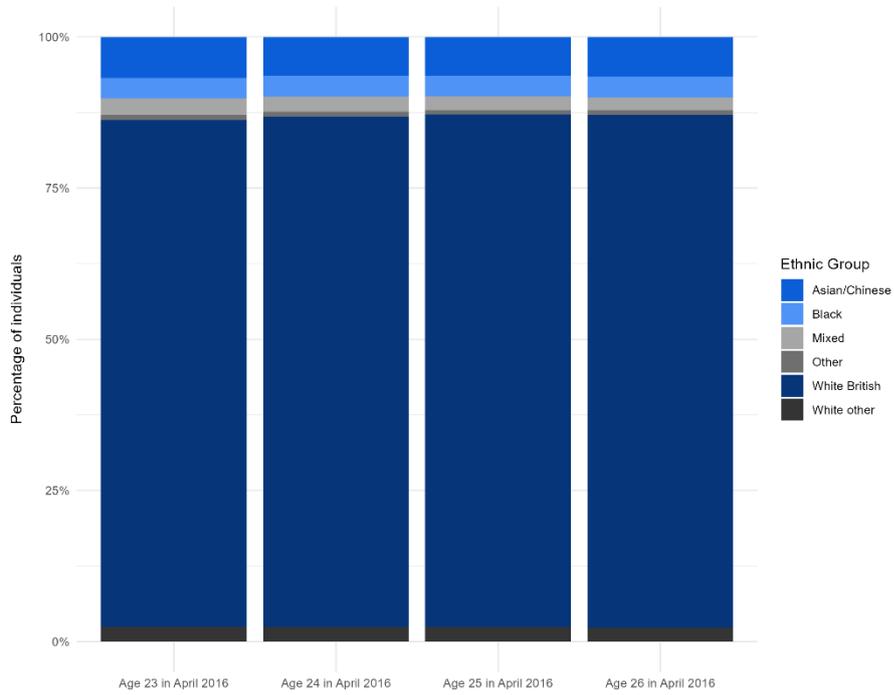
Source: LE analysis of LEO data tables.

Figure 32   First language
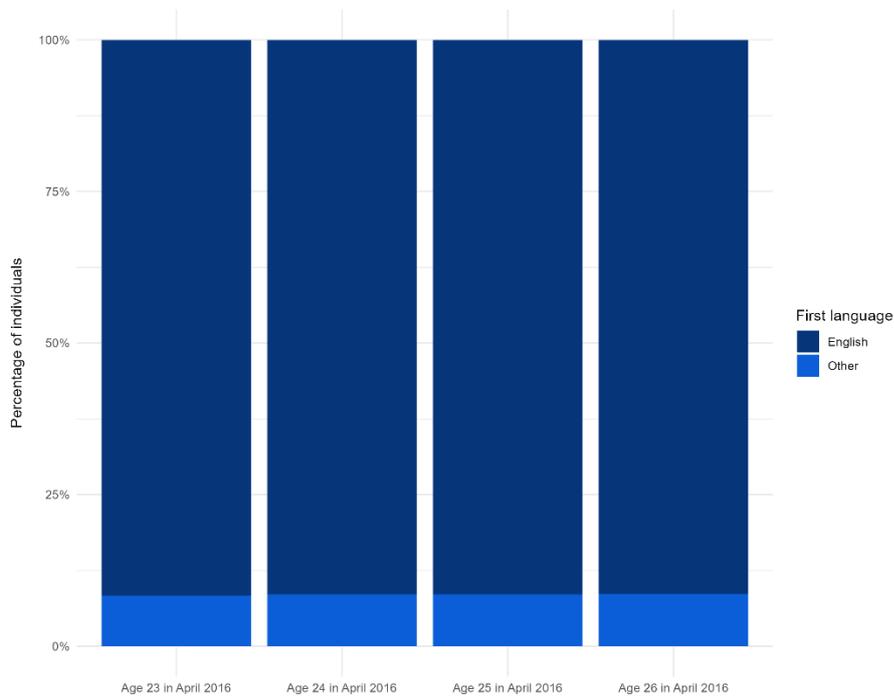
Source: LE analysis of LEO data tables.

The four cohorts are also similar based on the distribution across ethnic groups (Figure 31). White British individuals account for the majority within each cohort, followed by Asian/Chinese individuals. The proportion of individuals whose first language was English is also similar across all cohorts (Figure 32).

The four cohorts appear to be broadly similar based on these characteristics. The only difference is that there appears to be a trend among younger cohorts (those aged 23 or 24 in April 2016) towards completing A-levels or equivalent qualifications by age 19, compared to the older cohorts (aged 25 or 26 in April 2016), as shown in Figure 29. But since we are controlling for year in our specification, this will capture any time-varying trends in education that affect all cohorts.

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Annex 3   Additional econometric results

A3.1   Participation in education

A3.1.1   Baseline results

Table 3   Baseline results: Enrolled

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.012***	-0.008	-0.019***	-0.021***	-0.003	-0.015
	(0.002)	(0.005)	(0.005)	(0.004)	(0.006)	(0.009)
2011-12 x treated	-0.002	0.006	-0.001	-0.006+	0.003	-0.006
	(0.002)	(0.004)	(0.005)	(0.003)	(0.006)	(0.007)
2012-13 x treated	-0.003+	0.004+	-0.002	-0.002	-0.001	-0.007
	(0.002)	(0.002)	(0.004)	(0.003)	(0.005)	(0.005)
2013-14 x treated	-0.005***	-0.003	-0.007*	-0.002	-0.010*	-0.013**
	(0.001)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
2014-15 x treated	-0.001+	0.001	-0.001	0.001	0	0
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
2016-17 x treated	0	0	0	-0.002	-0.002	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)
2017-18 x treated	0.001	0.005	0.004	0.002	0.004	0.002
	(0.001)	(0.003)	(0.003)	(0.002)	(0.005)	(0.005)
2018-19 x treated	0.001	0.004	0.005	0.004+	0.004	0
	(0.001)	(0.003)	(0.004)	(0.002)	(0.004)	(0.006)
2019-20 x treated	-0.001	0.004	0.004	0.004+	-0.006	0.003
	(0.001)	(0.003)	(0.004)	(0.002)	(0.005)	(0.005)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.153	0.098	0.111	0.035	0.244	0.228
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 4   Baseline results: Further education (up to Level 3)

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.017***	-0.018**	-0.026***	-0.020***	-0.012*	-0.016**
	(0.003)	(0.006)	(0.004)	(0.003)	(0.005)	(0.005)
2011-12 x treated	-0.009***	-0.008	-0.009**	-0.007*	-0.008	-0.001
	(0.002)	(0.005)	(0.003)	(0.003)	(0.005)	(0.004)
2012-13 x treated	-0.005**	-0.003	-0.004*	-0.004	-0.001	0.005
	(0.001)	(0.004)	(0.002)	(0.003)	(0.003)	(0.004)
2013-14 x treated	-0.004***	-0.005	-0.004+	-0.004	-0.007*	0
	(0.001)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
2014-15 x treated	-0.001*	-0.001	0	-0.001	-0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
2016-17 x treated	0	0	-0.001	-0.002	-0.003	0
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
2017-18 x treated	0.001	0.002	0.001	0.001	0.001	0
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
2018-19 x treated	0.002	0.004	0.004	0.004+	0.002	0.001
	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
2019-20 x treated	0.001	0.004	0.003	0.005**	-0.001	0.004
	(0.001)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.041	0.048	0.045	0.036	0.04	0.052
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 5   Baseline results: Undergraduate education (Levels 4-6)

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.004	0.011**	0.007	-0.001	0.009	0.001
	(0.003)	(0.003)	(0.004)	(0.001)	(0.006)	(0.009)
2011-12 x treated	0.007*	0.013***	0.008*	0	0.012+	-0.004
	(0.003)	(0.003)	(0.004)	(0.001)	(0.006)	(0.006)
2012-13 x treated	0	0.005*	0	0	-0.003	-0.011**
	(0.001)	(0.002)	(0.003)	(0.001)	(0.004)	(0.004)
2013-14 x treated	-0.001	-0.001	-0.003+	0	-0.006+	-0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)
2014-15 x treated	0	0.001	-0.001	0	0.001	-0.004+
	(0)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
2016-17 x treated	0	-0.001	0	-0.001	0	0
	(0)	(0.001)	(0.001)	(0)	(0.001)	(0.002)
2017-18 x treated	0	0	0.001	-0.001+	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
2018-19 x treated	-0.001+	-0.003+	0	-0.001*	0	-0.002
	(0)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)
2019-20 x treated	-0.002**	-0.002	0.001	-0.002**	-0.003	-0.002
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.128	0.054	0.072	0.001	0.231	0.183
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 6   Baseline results: Postgraduate education (Levels 7-8)

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0	0	0	0	0	-0.001
	(0)	(0.001)	(0.001)	(0)	(0.003)	(0.003)
2011-12 x treated	0	0.001	0	0	0	-0.001
	(0)	(0.001)	(0.001)	(0)	(0.003)	(0.002)
2012-13 x treated	0.001*	0.002*	0.002*	-0.000*	0.002	0
	(0.001)	(0.001)	(0.001)	(0)	(0.002)	(0.002)
2013-14 x treated	0	0.001	0.001	0	0.002	0
	(0)	(0.001)	(0.001)	(0)	(0.002)	(0.002)
2014-15 x treated	-0.000+	0.001	0	0	0	0.001
	(0)	(0.001)	(0.001)	(0)	(0.002)	(0.001)
2016-17 x treated	-0.001	0	0	0	-0.001	0
	(0)	(0.001)	(0.001)	(0)	(0.002)	(0.001)
2017-18 x treated	-0.001	0.002+	0	0	0	0.002
	(0.001)	(0.001)	(0.001)	(0)	(0.002)	(0.002)
2018-19 x treated	-0.001+	0.001	-0.001	0	-0.002	0.001
	(0)	(0.001)	(0.001)	(0)	(0.002)	(0.002)
2019-20 x treated	-0.001+	0.001	-0.001	0	-0.002	0.001
	(0)	(0.001)	(0.001)	(0)	(0.003)	(0.002)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.002	0.001	0.001	0	0.003	0.003
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 7   **Baseline results: Apprenticeship**

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.007***	-0.006**	-0.010***	-0.010***	-0.002	0
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.002)
2011-12 x treated	-0.003**	-0.001	-0.001	-0.002	0.003	0.002
	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
2012-13 x treated	-0.001	0	0	0.001	0.003	0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
2013-14 x treated	-0.001	0	-0.001	0	-0.003	0.001
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
2014-15 x treated	0	0.001	0.001	0.001	0	-0.001
	(0)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2016-17 x treated	0.001	0	0.001	-0.001	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2017-18 x treated	0.002+	0.002	0.002	0.001	0.003*	0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2018-19 x treated	0.002+	0.002	0.002	0.003*	0.002	0.002
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
2019-20 x treated	0.001	0.002	0.002	0.004*	0	0.002
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.012	0.007	0.008	0.007	0.003	0.003
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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A3.1.2   Robustness checks

Comparing 25- and 23-year-olds

Table 8   25 vs. 23-year-olds results: Enrolled

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.014**	-0.050***	-0.055***	-0.067***	-0.005	-0.011
	(0.005)	(0.008)	(0.009)	(0.010)	(0.008)	(0.010)
2011-12 x treated	-0.004	-0.009	-0.012+	-0.027***	0.012	-0.004
	(0.003)	(0.006)	(0.006)	(0.004)	(0.008)	(0.009)
2012-13 x treated	-0.002	-0.003	-0.006	-0.016**	0.008	-0.008
	(0.003)	(0.004)	(0.005)	(0.005)	(0.008)	(0.008)
2013-14 x treated	-0.015***	-0.009*	-0.015**	-0.008+	-0.018*	-0.021***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.007)	(0.005)
2014-15 x treated	-0.003***	-0.004+	-0.007*	-0.003	-0.007	0.002
	(0.001)	(0.002)	(0.003)	(0.003)	(0.005)	(0.004)
2016-17 x treated	-0.003*	-0.004	0.001	-0.003	-0.002	0
	(0.001)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)
2017-18 x treated	0	0.004	0.006+	0	0.006	0.008
	(0.001)	(0.004)	(0.003)	(0.003)	(0.005)	(0.007)
2018-19 x treated	0.001	0.003	0.007+	0.004	0.008	0.013
	(0.002)	(0.004)	(0.004)	(0.004)	(0.005)	(0.008)
2019-20 x treated	0.001	0.001	0.006	0.004	0	0.011
	(0.002)	(0.004)	(0.005)	(0.004)	(0.007)	(0.009)
Num.Obs.	10936010	1312960	2392100	2048120	721460	728810
R2	0.172	0.132	0.141	0.053	0.251	0.239
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 9   25 vs. 23-year-olds results: Further education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.057***	-0.089***	-0.080***	-0.065***	-0.066***	-0.066***
	(0.007)	(0.010)	(0.010)	(0.010)	(0.008)	(0.009)
2011-12 x treated	-0.024***	-0.039***	-0.028***	-0.029***	-0.025***	-0.024***
	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
2012-13 x treated	-0.013***	-0.021***	-0.012**	-0.020***	-0.010*	-0.005
	(0.002)	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)
2013-14 x treated	-0.008***	-0.008*	-0.005	-0.010*	-0.011*	0.001
	(0.002)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)
2014-15 x treated	-0.004***	-0.003	-0.002	-0.006+	-0.005+	0.006*
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
2016-17 x treated	0.001	-0.002	0	-0.003	0	-0.004
	(0.001)	(0.003)	(0.001)	(0.003)	(0.003)	(0.003)
2017-18 x treated	0.004**	0.003	0.003	-0.002	0.003	-0.001
	(0.001)	(0.003)	(0.002)	(0.004)	(0.003)	(0.004)
2018-19 x treated	0.005**	0.006	0.006	0.003	0.006+	0.004
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
2019-20 x treated	0.006*	0.005	0.007	0.006	0.004	0.006
	(0.002)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
Num.Obs.	10936010	1312960	2392100	2048120	721460	728810
R2	0.096	0.108	0.102	0.056	0.103	0.117
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 10   25 vs. 23-year-olds results: Undergraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.043***	0.036***	0.025***	0	0.063***	0.050***
	(0.005)	(0.006)	(0.004)	(0.001)	(0.011)	(0.009)
2011-12 x treated	0.022***	0.031***	0.018***	0	0.041***	0.020*
	(0.004)	(0.005)	(0.004)	(0.001)	(0.008)	(0.009)
2012-13 x treated	0.010***	0.018***	0.007*	0	0.022**	-0.003
	(0.003)	(0.004)	(0.003)	(0.001)	(0.007)	(0.008)
2013-14 x treated	-0.009***	-0.001	-0.010***	0	-0.007	-0.025***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.006)	(0.006)
2014-15 x treated	0.002	0	-0.004**	0	0.002	-0.006+
	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.003)
2016-17 x treated	-0.001*	0	0.003*	-0.001	0.004	0.006*
	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.002)
2017-18 x treated	-0.002*	-0.001	0.005**	-0.001	0.008*	0.006
	(0.001)	(0.002)	(0.002)	(0.001)	(0.004)	(0.004)
2018-19 x treated	-0.002**	-0.004	0.002	-0.002	0.008	0.004
	(0.001)	(0.002)	(0.002)	(0.001)	(0.005)	(0.004)
2019-20 x treated	-0.003**	-0.004	0.001	-0.003*	0.007	0.005
	(0.001)	(0.002)	(0.002)	(0.001)	(0.006)	(0.005)
Num.Obs.	10936010	1312960	2392100	2048120	721460	728810
R2	0.129	0.056	0.076	0.001	0.233	0.184
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 11   25 vs. 23-year-olds results: Postgraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.001	0	-0.001	0	-0.004	0.001
	(0.001)	(0.001)	(0.001)		(0.004)	(0.003)
2011-12 x treated	-0.001	0	-0.002	0	-0.004	0.001
	(0.001)	(0.001)	(0.001)		(0.004)	(0.003)
2012-13 x treated	0.001	0.001	-0.001	0	-0.003	0.002
	(0.001)	(0.001)	(0.001)		(0.004)	(0.003)
2013-14 x treated	0.002*	0.001	0	0	-0.003	0.003
	(0.001)	(0.001)	(0.001)		(0.004)	(0.003)
2014-15 x treated	-0.001*	0.001	-0.001*	0	-0.005	0.003
	(0.001)	(0.001)	(0.001)		(0.003)	(0.002)
2016-17 x treated	-0.002***	0	-0.001	0	-0.004*	0
		(0.001)	(0.001)		(0.002)	(0.002)
2017-18 x treated	-0.002**	0.001	-0.002*	0	-0.005+	0.003
	(0.001)	(0.001)	(0.001)		(0.003)	(0.002)
2018-19 x treated	-0.002**	0.002	-0.002*	-0.001*	-0.008**	0.003
	(0.001)	(0.001)	(0.001)		(0.002)	(0.002)
2019-20 x treated	-0.003**	0.001	-0.002*	-0.001	-0.012***	0.001
	(0.001)	(0.001)	(0.001)		(0.003)	(0.002)
Num.Obs.	10936010	1312960	2392100	2048120	721460	728810
R2	0.003	0.001	0.002	0	0.005	0.004
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.

Table 12   25 vs. 23-year-olds results: Apprenticeship

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.013***	-0.013**	-0.014***	-0.018***	-0.007+	-0.009+
	(0.002)	(0.004)	(0.004)	(0.002)	(0.004)	(0.005)
2011-12 x treated	-0.005**	-0.003	0	-0.008**	-0.001	-0.002
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)
2012-13 x treated	-0.001	0.002	0.006*	-0.002	0.002	0.005
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
2013-14 x treated	-0.002	-0.001	0.002	-0.002	-0.002	0.007*
	(0.001)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
2014-15 x treated	-0.002*	-0.002	0	-0.001	0	0.003
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
2016-17 x treated	0.003***	0.002	0.003**	0.002	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2017-18 x treated	0.005***	0.004*	0.005**	0.004*	0.002	0.004+
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
2018-19 x treated	0.006***	0.005+	0.007**	0.007**	0.002	0.006*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
2019-20 x treated	0.006***	0.005+	0.007*	0.008***	0	0.006*
	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Num.Obs.	10936010	1312960	2392100	2048120	721460	728810
R2	0.014	0.01	0.011	0.008	0.004	0.005
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Individuals in the first year of enrolment

Table 13   First year only results: Further education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.013**	-0.008	-0.017	-0.017*	-0.013	0.009
	(0.004)	(0.020)	(0.011)	(0.007)	(0.017)	(0.018)
2011-12 x treated	0.009+	0.01	0.014	-0.008	0.003	0.066***
	(0.005)	(0.017)	(0.010)	(0.006)	(0.018)	(0.015)
2012-13 x treated	-0.010*	0.002	-0.001	-0.011	-0.017	0.050**
	(0.004)	(0.017)	(0.010)	(0.007)	(0.016)	(0.017)
2013-14 x treated	-0.006+	-0.002	0.006	-0.010+	-0.018	0.026
	(0.004)	(0.013)	(0.009)	(0.006)	(0.015)	(0.018)
2014-15 x treated	0.001	-0.003	0.007	-0.015**	0.018	0.009
	(0.002)	(0.012)	(0.007)	(0.005)	(0.014)	(0.016)
2016-17 x treated	-0.001	-0.001	-0.01	-0.012	-0.025+	0.005
	(0.004)	(0.013)	(0.008)	(0.008)	(0.014)	(0.022)
2017-18 x treated	0	-0.017	-0.012	-0.016+	0.001	0.012
	(0.005)	(0.017)	(0.009)	(0.008)	(0.023)	(0.021)
2018-19 x treated	0.006	0.009	0.009	-0.004	0.002	0.019
	(0.004)	(0.021)	(0.010)	(0.009)	(0.014)	(0.018)
2019-20 x treated	0.006	0.003	-0.005	0.012	-0.008	0.035
	(0.006)	(0.022)	(0.008)	(0.009)	(0.020)	(0.031)
Num.Obs.	1406030	200640	355685	296120	104885	102740
R2	0.017	0.011	0.013	0.013	0.015	0.026
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 14   First year only results: Undergraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.007*	-0.003	0.009	-0.005	0.003	-0.015
	(0.003)	(0.012)	(0.007)	(0.005)	(0.017)	(0.015)
2011-12 x treated	-0.007	-0.011	-0.01	-0.005	-0.003	-0.057***
	(0.004)	(0.009)	(0.007)	(0.005)	(0.016)	(0.014)
2012-13 x treated	-0.002	-0.014	-0.011+	-0.004	-0.01	-0.045**
	(0.004)	(0.009)	(0.006)	(0.005)	(0.018)	(0.015)
2013-14 x treated	-0.003	-0.012	-0.011	-0.006	-0.019	-0.044**
	(0.004)	(0.009)	(0.007)	(0.005)	(0.014)	(0.013)
2014-15 x treated	-0.005	-0.006	-0.011+	-0.004	-0.01	-0.023**
	(0.003)	(0.009)	(0.006)	(0.004)	(0.011)	(0.007)
2016-17 x treated	0	-0.015*	-0.006	-0.002	0.006	-0.01
	(0.004)	(0.006)	(0.006)	(0.005)	(0.016)	(0.016)
2017-18 x treated	-0.005	-0.009	-0.005	-0.001	-0.01	-0.018
	(0.005)	(0.010)	(0.005)	(0.005)	(0.013)	(0.013)
2018-19 x treated	-0.010+	-0.027*	-0.022***	-0.011+	-0.024	-0.038***
	(0.005)	(0.013)	(0.005)	(0.006)	(0.016)	(0.009)
2019-20 x treated	-0.007	-0.012	0.004	-0.011	0.005	-0.028
	(0.005)	(0.016)	(0.008)	(0.007)	(0.019)	(0.022)
Num.Obs.	1406030	200640	355685	296120	104885	102740
R2	0.08	0.032	0.044	0.012	0.114	0.099
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 15   First year only results: Apprenticeship

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.006	0.01	0.008	0.010+	0.020*	0.01
	(0.004)	(0.012)	(0.007)	(0.006)	(0.009)	(0.011)
2011-12 x treated	0.013**	0.006	0.01	0.008	0.029**	0.013
	(0.004)	(0.013)	(0.007)	(0.006)	(0.010)	(0.011)
2012-13 x treated	0.006	0	0.004	0.006	0.011	0.01
	(0.004)	(0.011)	(0.008)	(0.006)	(0.010)	(0.013)
2013-14 x treated	0.005	0.011	0.013+	0.011	0.007	0.001
	(0.004)	(0.011)	(0.008)	(0.009)	(0.009)	(0.012)
2014-15 x treated	0.001	-0.003	0.003	0.003	0.018+	-0.005
	(0.003)	(0.008)	(0.007)	(0.006)	(0.010)	(0.012)
2016-17 x treated	0.003	0.004	0.008	-0.01	0.022**	0.019
	(0.003)	(0.007)	(0.007)	(0.007)	(0.007)	(0.015)
2017-18 x treated	0.007	-0.011	0	0.007	0.016	-0.006
	(0.005)	(0.010)	(0.010)	(0.006)	(0.011)	(0.016)
2018-19 x treated	0.006	0	0.001	0.013+	-0.002	0.021
	(0.006)	(0.013)	(0.010)	(0.007)	(0.014)	(0.016)
2019-20 x treated	-0.001	-0.002	0.004	0.009	0.013	-0.005
	(0.005)	(0.013)	(0.008)	(0.010)	(0.012)	(0.018)
Num.Obs.	1406030	200640	355685	296120	104885	102740
R2	0.006	0.005	0.005	0.004	0.01	0.011
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Defining cohorts based on age in September

Table 16   September cohort results: Enrolled

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.012	-0.021	-0.012	0.002	0.051***	0.041***
	(0.008)	(0.018)	(0.018)	(0.009)	(0.007)	(0.006)
2011-12 x treated	0.037	0.006	0.008	-0.003	0.074**	0.040*
	(0.022)	(0.015)	(0.018)	(0.006)	(0.024)	(0.016)
2012-13 x treated	-0.025	-0.01	-0.023	-0.001	-0.027	-0.001
	(0.031)	(0.015)	(0.020)	(0.004)	(0.041)	(0.029)
2013-14 x treated	-0.039*	-0.016	-0.026+	0	-0.048+	-0.031
	(0.019)	(0.011)	(0.013)	(0.003)	(0.027)	(0.024)
2014-15 x treated	-0.010+	-0.007	-0.010*	0.004	-0.01	-0.008
	(0.005)	(0.004)	(0.005)	(0.002)	(0.009)	(0.009)
2016-17 x treated	0.003	0.003	0.002	0.001	0.013**	0.008
	(0.002)	(0.003)	(0.003)	(0.002)	(0.004)	(0.005)
2017-18 x treated	0.005	0.006	0.005	0.003	0.019**	0.014+
	(0.003)	(0.004)	(0.004)	(0.003)	(0.007)	(0.008)
2018-19 x treated	0.005	0.007	0.006	0.002	0.014*	0.017+
	(0.004)	(0.005)	(0.005)	(0.004)	(0.007)	(0.009)
2019-20 x treated	0.007	0.008	0.008	0.005	0.016*	0.022*
	(0.004)	(0.005)	(0.005)	(0.003)	(0.008)	(0.009)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.146	0.093	0.105	0.034	0.232	0.219
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept						

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 17   September cohort results: Further education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.052*	-0.065+	-0.059+	0.005	-0.058+	-0.064+
	(0.025)	(0.032)	(0.030)	(0.009)	(0.029)	(0.033)
2011-12 x treated	-0.020*	-0.027*	-0.024*	-0.003	-0.024*	-0.026*
	(0.009)	(0.012)	(0.010)	(0.007)	(0.010)	(0.012)
2012-13 x treated	-0.005+	-0.008+	-0.006+	-0.002	-0.001	-0.006
	(0.003)	(0.004)	(0.003)	(0.005)	(0.002)	(0.004)
2013-14 x treated	-0.002	-0.001	-0.001	-0.002	0.007**	0
	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)
2014-15 x treated	0	-0.001	0	0.003	0.006**	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
2016-17 x treated	0	0.001	0	0.001	0.003+	-0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
2017-18 x treated	0.001	0	0	0.003	0.007***	0
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
2018-19 x treated	0.002	0.002	0.002	0.003	0.005***	0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)
2019-20 x treated	0.002	0.002	0.002	0.004	0.005*	0.003
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.037	0.043	0.04	0.035	0.033	0.044
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept						

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 18   September cohort results: Undergraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.060***	0.041**	0.045***	-0.002	0.107**	0.096**
	(0.016)	(0.012)	(0.011)	(0.001)	(0.031)	(0.031)
2011-12 x treated	0.064***	0.034***	0.035**	0.001	0.109***	0.069***
	(0.017)	(0.006)	(0.010)	(0.001)	(0.021)	(0.010)
2012-13 x treated	-0.031	-0.007	-0.022	0	-0.043	-0.011
	(0.032)	(0.014)	(0.020)	(0.001)	(0.046)	(0.030)
2013-14 x treated	-0.038*	-0.016+	-0.025*	0.001	-0.057*	-0.036
	(0.017)	(0.009)	(0.012)	(0.001)	(0.026)	(0.022)
2014-15 x treated	-0.009*	-0.006+	-0.009*	0.001	-0.015*	-0.009
	(0.004)	(0.003)	(0.004)	(0.000)	(0.007)	(0.008)
2016-17 x treated	0.003*	0.003	0.003*	0	0.009*	0.009+
	(0.001)	(0.002)	(0.002)	(0.000)	(0.003)	(0.004)
2017-18 x treated	0.004+	0.004	0.005+	0	0.012*	0.010+
	(0.002)	(0.003)	(0.002)	(0.001)	(0.005)	(0.006)
2018-19 x treated	0.003	0.003	0.004	-0.001*	0.011*	0.011
	(0.002)	(0.003)	(0.003)	(0.001)	(0.005)	(0.007)
2019-20 x treated	0.004	0.005+	0.005+	0	0.011*	0.014+
	(0.002)	(0.003)	(0.003)	(0.001)	(0.005)	(0.007)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.117	0.049	0.065	0	0.213	0.169
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept						

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 19 **September cohort results: Postgraduate education**

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.002*	0.001	0.001	0	0.003	0.008*
	(0.001)	(0.001)	(0.001)	0.000	(0.003)	(0.003)
2011-12 x treated	-0.006+	-0.002	-0.003	-0.000+	-0.010+	-0.002
	(0.003)	(0.002)	(0.002)	0.000	(0.005)	(0.005)
2012-13 x treated	0.011**	0.003+	0.004*	0	0.015**	0.015***
	(0.003)	(0.002)	(0.002)	0.000	(0.005)	(0.003)
2013-14 x treated	0.001	0.001	0	0	0.002	0.002
	(0.001)	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2014-15 x treated	-0.001+	-0.001*	-0.001**	0	-0.001	-0.001
	(0.001)	(0.001)	0.000	0.000	(0.002)	(0.002)
2016-17 x treated	0	0	0	0	0	0.002
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.001)
2017-18 x treated	0.001	0	-0.001	0	-0.001	0.003+
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2018-19 x treated	0	0.001	0	-0.000**	-0.002	0.003+
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2019-20 x treated	0.001	0	0	0	-0.001	0.005*
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.002	0.001	0.001	0	0.003	0.002
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept						

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 20   September cohort results: Apprenticeship

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0	-0.002	0.001	0.008**	0.004*	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
2011-12 x treated	-0.003	-0.002	-0.002	0.001	0.001	-0.002
	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
2012-13 x treated	-0.003*	-0.003*	-0.002	0	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
2013-14 x treated	-0.002+	-0.002	-0.002	0	0.002	0
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
2014-15 x treated	-0.001	-0.001	0	0.001	0.003**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2016-17 x treated	0	0.001	0	0	0	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2017-18 x treated	0.001	0	-0.001	0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
2018-19 x treated	0.001	0.001	0	0.002	0.002	0
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
2019-20 x treated	0.001	0.001	0	0.002	0.001	0
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.011	0.007	0.008	0.007	0.003	0.003
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept						

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Falsification tests: 25- and 26-year-olds

Table 21   26 vs. 25 results: Enrolled

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.001	-0.008*	-0.003	-0.011***	-0.007	-0.011+
	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.005)
2011-12 x treated	0	0	-0.006**	-0.004	-0.006	-0.013*
	(0.001)	(0.003)	(0.002)	(0.002)	(0.006)	(0.006)
2012-13 x treated	-0.002+	-0.003	-0.008***	-0.004	-0.006	-0.010+
	(0.001)	(0.003)	(0.002)	(0.002)	(0.005)	(0.005)
2013-14 x treated	-0.002	-0.002	-0.003	-0.003	0.002	-0.002
	(0.001)	(0.003)	(0.002)	(0.003)	(0.005)	(0.004)
2014-15 x treated	-0.001	-0.004	-0.003*	-0.002	0.001	-0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
2016-17 x treated	0.001	0.003	-0.001	0.002	0.007*	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
2017-18 x treated	0.001	0	-0.001	0	0.007	-0.001
	(0.001)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
2018-19 x treated	0.002	0.001	-0.001	0.002	0.005	0
	(0.001)	(0.003)	(0.002)	(0.002)	(0.004)	(0.005)
2019-20 x treated	0.002+	0.001	-0.002	0.005*	0.005	0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
Num.Obs.	10959370	1338360	2417830	2468560	696980	710930
R2	0.114	0.066	0.074	0.026	0.19	0.184
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 22   26 vs. 25 results: Further education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.008***	-0.014**	-0.012***	-0.012***	-0.011**	-0.008+
	(0.001)	(0.004)	(0.002)	(0.003)	(0.004)	(0.004)
2011-12 x treated	-0.004**	-0.004	-0.008**	-0.004	-0.006+	-0.004
	(0.001)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)
2012-13 x treated	-0.003***	-0.005+	-0.005**	-0.004	-0.006+	-0.002
	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)
2013-14 x treated	-0.002*	-0.004+	-0.002	-0.004	0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
2014-15 x treated	-0.001	-0.003	-0.001	0	-0.002	0
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
2016-17 x treated	0	0.003	-0.001	0	0.004+	0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
2017-18 x treated	0.001	0.004	0	0.001	0.005+	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
2018-19 x treated	0.002+	0.001	0	0.002	0.001	0
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
2019-20 x treated	0.002	0.001	-0.002	0.004	0.003	0
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Num.Obs.	10959370	1338360	2417830	2468560	696980	710930
R2	0.02	0.023	0.022	0.026	0.015	0.021
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 23   26 vs. 25 results: Undergraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.007*	0.009*	0.012***	0	0.002	-0.001
	(0.003)	(0.003)	(0.002)	(0.001)	(0.006)	(0.005)
2011-12 x treated	0.002	0.004*	0.002	-0.001	-0.006	-0.010*
	(0.001)	(0.002)	(0.002)	(0.001)	(0.006)	(0.005)
2012-13 x treated	0	0.001	-0.001	-0.001+	-0.003	-0.012**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)	(0.004)
2013-14 x treated	0	0.001	-0.001	0	0	-0.003
	(0.001)	(0.002)	(0.001)	(0.001)	(0.005)	(0.003)
2014-15 x treated	0	0.001	0	0	0.001	0
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.001)
2016-17 x treated	0	0	0.001*	0	0.005**	-0.001
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2017-18 x treated	0	-0.001	0.001	-0.001	0.005*	0
	0.000	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
2018-19 x treated	0	0.001	0.002	-0.001	0.006*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.002)
2019-20 x treated	0	0.001	0.001	0	0.003	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Num.Obs.	10959370	1338360	2417830	2468560	696980	710930
R2	0.119	0.053	0.064	0	0.215	0.18
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 24   26 vs. 25 results: Postgraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0	-0.001	-0.001	0.000**	0.003+	-0.002
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2011-12 x treated	0.002***	0	0	0.000***	0.006*	0
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2012-13 x treated	0.001	0	-0.001	0.000**	0.003	0.002
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2013-14 x treated	0	0	0	0.000***	0.003	0
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2014-15 x treated	0.001	-0.001	0	0	0.003+	-0.002
	0.000	(0.001)	0.000	0.000	(0.002)	(0.001)
2016-17 x treated	0	-0.001	-0.002**	0	-0.002	-0.002
	0.000	(0.001)	(0.001)	0.000	(0.001)	(0.001)
2017-18 x treated	-0.001+	-0.002*	-0.002***	0	-0.002	-0.001
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2018-19 x treated	-0.001	-0.001+	-0.002**	0	-0.001	-0.002
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2019-20 x treated	0	0	-0.001	0.000*	0	-0.002
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
Num.Obs.	10959370	1338360	2417830	2468560	696980	710930
R2	0.119	0.053	0.064	0	0.215	0.18
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 25   26 vs. 25 results: Apprenticeship

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.002**	-0.004	-0.001	-0.004*	-0.001	0.001
	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)	(0.003)
2011-12 x treated	-0.001	0	-0.002	-0.001	-0.002	0
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
2012-13 x treated	0	-0.001	-0.001	-0.002	-0.003	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
2013-14 x treated	-0.001*	-0.002	0	-0.002	-0.001	-0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)
2014-15 x treated	-0.001*	-0.002	-0.001	-0.001	-0.003+	0
	0.000	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2016-17 x treated	0.001*	0.002+	0.001	0.001+	0.002*	0.001
	0.000	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2017-18 x treated	0.002**	0.003	0.002**	0.002*	0.001	0
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
2018-19 x treated	0.002*	0.001	0.002**	0.003**	-0.001	0
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
2019-20 x treated	0.001	0.001	0.001	0.003*	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Num.Obs.	10959370	1338360	2417830	2468560	696980	710930
R2	0.008	0.005	0.006	0.006	0.002	0.002
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Falsification tests: 23- and 24-year-olds

Table 26   24 vs. 23-year-olds results: Enrolled

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.024***	-0.047***	-0.042***	-0.048***	-0.022***	-0.018**
	(0.004)	(0.006)	(0.006)	(0.008)	(0.005)	(0.006)
2011-12 x treated	-0.015***	-0.023***	-0.016***	-0.023***	-0.009*	-0.012
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
2012-13 x treated	-0.003	-0.014***	-0.005*	-0.014***	-0.002	-0.005
	(0.002)	(0.004)	(0.002)	(0.003)	(0.004)	(0.007)
2013-14 x treated	-0.006***	-0.010*	-0.005*	-0.005+	-0.012*	-0.006
	(0.002)	(0.004)	(0.003)	(0.002)	(0.005)	(0.005)
2014-15 x treated	0	-0.005+	-0.002	-0.002	-0.007+	0.001
	(0.001)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
2016-17 x treated	-0.001	-0.004	-0.001	-0.001	0	0.003
	(0.001)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)
2017-18 x treated	0.001	-0.001	0.001	0	0	0.007
	(0.001)	(0.003)	(0.002)	(0.003)	(0.004)	(0.005)
2018-19 x treated	0.001	-0.003	0.001	0.001	0.002	0.011*
	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)	(0.005)
2019-20 x treated	0.001	-0.004	0.001	0.003	0.006	0.008
	(0.001)	(0.004)	(0.002)	(0.003)	(0.004)	(0.005)
Num.Obs.	10965720	1327340	2402950	1927620	729530	740380
R2	0.179	0.138	0.15	0.056	0.258	0.244
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 27   24 vs. 23-year-olds results: Further education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.039***	-0.060***	-0.054***	-0.050***	-0.042***	-0.040***
	(0.006)	(0.009)	(0.008)	(0.008)	(0.006)	(0.007)
2011-12 x treated	-0.015***	-0.027***	-0.021***	-0.024***	-0.010+	-0.017*
	(0.004)	(0.006)	(0.005)	(0.004)	(0.005)	(0.007)
2012-13 x treated	-0.009***	-0.018***	-0.010**	-0.014***	-0.006+	-0.007
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
2013-14 x treated	-0.003***	-0.004	-0.002	-0.005+	-0.003	0.003
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)
2014-15 x treated	-0.002**	-0.002	-0.001	-0.003	-0.004+	0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
2016-17 x treated	0	-0.001	0	-0.002	0.005+	0
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
2017-18 x treated	0.002**	0.002	0.002	-0.001	0.002	0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
2018-19 x treated	0.003**	0.001	0.002	0.001	0.001	0.005+
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
2019-20 x treated	0.003**	0.001	0.003	0.002	0.004	0.004
	(0.001)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Num.Obs.	10965720	1327340	2402950	1927620	729530	740380
R2	0.099	0.113	0.108	0.059	0.106	0.122
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 28   24 vs. 23-year-olds results: Undergraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.014***	0.013**	0.011***	0.001	0.019*	0.021***
	(0.004)	(0.004)	(0.003)	(0.001)	(0.008)	(0.005)
2011-12 x treated	0.001	0.008+	0.007*	0	0.002	0.008
	(0.005)	(0.005)	(0.003)	(0.001)	(0.007)	(0.008)
2012-13 x treated	0.007*	0.007	0.007*	0	0.007	0.004
	(0.003)	(0.004)	(0.003)	(0.001)	(0.005)	(0.007)
2013-14 x treated	-0.005**	-0.003	-0.003	0.001	-0.006	-0.008+
	(0.002)	(0.003)	(0.002)	(0.001)	(0.005)	(0.005)
2014-15 x treated	0.002+	-0.003*	-0.001	0	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
2016-17 x treated	-0.001	0.001	0.001	0	0.001	0.005*
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2017-18 x treated	-0.001	-0.001	0.001	0.001	0.002	0.006+
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)
2018-19 x treated	0	-0.001	0	0.001	0.003	0.007+
	(0.001)	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)
2019-20 x treated	0	-0.001	-0.001	0	0.005	0.007+
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.004)
Num.Obs.	10965720	1327340	2402950	1927620	729530	740380
R2	0.125	0.053	0.075	0.001	0.224	0.176
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 29   24 vs. 23-year-olds results: Postgraduate education

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.001+	-0.001	-0.001	0.000*	0	-0.002
	0.000	(0.001)	(0.001)	0.000	(0.003)	(0.001)
2011-12 x treated	-0.001	-0.001	-0.001	0.000+	0	-0.002
	0.000	(0.001)	(0.001)	0.000	(0.003)	(0.002)
2012-13 x treated	-0.001*	-0.001	-0.002+	0	0	-0.002
	0.000	(0.001)	(0.001)	0.000	(0.003)	(0.002)
2013-14 x treated	0.002**	0	0	0	-0.001	0
	(0.001)	(0.001)	(0.001)	0.000	(0.003)	(0.002)
2014-15 x treated	0	0	-0.001	0	-0.002	0.001
	0.000	(0.001)	(0.001)	0.000	(0.003)	(0.001)
2016-17 x treated	-0.001	-0.001	-0.001	0	-0.002	-0.001
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2017-18 x treated	-0.001	-0.001	-0.002*	0	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	0.000	(0.001)	(0.002)
2018-19 x treated	-0.001+	0	-0.001	0	0	-0.001
	0.000	(0.001)	(0.001)	0.000	(0.002)	(0.002)
2019-20 x treated	-0.002**	0	-0.001	0	-0.002	-0.004+
	(0.001)	(0.001)	(0.001)	0.000	(0.003)	(0.002)
Num.Obs.	10965720	1327340	2402950	1927620	729530	740380
R2	0.125	0.053	0.075	0.001	0.224	0.176
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 30   24 vs. 23-year-olds results: Apprenticeship

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	-0.007***	-0.012***	-0.011***	-0.012***	-0.007**	-0.008*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
2011-12 x treated	-0.004**	-0.007***	-0.005**	-0.009***	-0.006**	-0.004
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
2012-13 x treated	-0.001	-0.001	0.001	-0.003*	-0.001	0.001
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
2013-14 x treated	-0.001	0	0.002	-0.002+	0	0.004*
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
2014-15 x treated	-0.001+	0	0	-0.002+	-0.001	0.001
	0.000	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
2016-17 x treated	0.001**	0	0.002+	0.002	0.002	0.001
	0.000	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
2017-18 x treated	0.003***	0.002	0.004**	0.001	0.002	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
2018-19 x treated	0.003***	0.001	0.005**	0.001	0.001	0.004+
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
2019-20 x treated	0.003***	0.001	0.005***	0.002	0.001	0.005*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Num.Obs.	10965720	1327340	2402950	1927620	729530	740380
R2	0.014	0.009	0.011	0.008	0.003	0.004
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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A3.2   Labour market outcomes

A3.2.1   Baseline results

Table 31   Baseline results: Employment

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.012***	0.014*	0.018***	0.007*	0.029***	0.025**
	(0.002)	(0.005)	(0.005)	(0.004)	(0.005)	(0.007)
2011-12 x treated	0.006*	0.005	0.014**	0.004	0.011*	0.021**
	(0.002)	(0.005)	(0.004)	(0.003)	(0.005)	(0.006)
2012-13 x treated	0.004**	0.004	0.009**	0.005+	0.007	0.009*
	(0.001)	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)
2013-14 x treated	0.003*	0.003	0.007**	0.004	0.008+	0.011*
	(0.001)	(0.004)	(0.002)	(0.003)	(0.004)	(0.004)
2014-15 x treated	0.001*	0.001	0.003*	0.001	0.002	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
2016-17 x treated	-0.001	-0.001	-0.001	-0.002	-0.003	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
2017-18 x treated	-0.002+	-0.004	-0.003	-0.005*	-0.004	-0.006*
	(0.001)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
2018-19 x treated	0	-0.004+	-0.001	-0.004	-0.001	-0.004
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)
2019-20 x treated	-0.001	-0.007*	-0.001	-0.004	-0.003	0
	(0.001)	(0.003)	(0.002)	(0.003)	(0.004)	(0.005)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.006	0.011	0.011	0.003	0.03	0.033
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 32   Baseline results: Number of employment stints

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.020***	0.025**	0.029***	0.012*	0.044***	0.037**
	(0.003)	(0.008)	(0.006)	(0.005)	(0.009)	(0.011)
2011-12 x treated	0.009**	0.012	0.022**	0.008	0.018*	0.032**
	(0.003)	(0.008)	(0.006)	(0.005)	(0.008)	(0.010)
2012-13 x treated	0.009***	0.012+	0.017**	0.012*	0.011	0.014
	(0.002)	(0.007)	(0.005)	(0.005)	(0.008)	(0.009)
2013-14 x treated	0.008***	0.012+	0.015**	0.010*	0.014*	0.018+
	(0.002)	(0.006)	(0.004)	(0.004)	(0.007)	(0.009)
2014-15 x treated	0.003*	0.003	0.010**	0.003	0.006	0.005
	(0.001)	(0.004)	(0.003)	(0.003)	(0.005)	(0.006)
2016-17 x treated	0	0	-0.001	-0.005	-0.004	-0.009
	(0.001)	(0.004)	(0.003)	(0.003)	(0.006)	(0.006)
2017-18 x treated	0	-0.005	-0.002	-0.007	-0.004	-0.007
	(0.002)	(0.006)	(0.003)	(0.005)	(0.007)	(0.008)
2018-19 x treated	0.001	-0.004	0.001	-0.006	0.002	-0.002
	(0.001)	(0.004)	(0.003)	(0.005)	(0.009)	(0.008)
2019-20 x treated	0.002	-0.003	0.005	-0.001	0.003	0.005
	(0.002)	(0.006)	(0.004)	(0.004)	(0.009)	(0.008)
Num.Obs.	11050550	1304280	2408710	2194100	711110	723970
R2	0.003	0.007	0.006	0.002	0.02	0.024
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 33   Baseline results: Earnings (log)

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.032***	0.057**	0.044***	0.042***	0.064**	0.005
	(0.005)	(0.019)	(0.010)	(0.012)	(0.024)	(0.023)
2011-12 x treated	0.020***	0.034*	0.033***	0.038**	0.029	0.014
	(0.004)	(0.015)	(0.010)	(0.012)	(0.018)	(0.020)
2012-13 x treated	0.015***	0.031*	0.025**	0.035**	0.027	0.008
	(0.004)	(0.013)	(0.009)	(0.011)	(0.017)	(0.019)
2013-14 x treated	0.001	0.004	0.007	0.013	0.016	-0.018
	(0.004)	(0.012)	(0.008)	(0.010)	(0.015)	(0.016)
2014-15 x treated	0.002	0.014	0.009	0.01	0.016	0.004
	(0.003)	(0.010)	(0.007)	(0.007)	(0.011)	(0.013)
2016-17 x treated	-0.006*	-0.008	-0.011+	-0.005	0.005	-0.018
	(0.002)	(0.009)	(0.005)	(0.008)	(0.011)	(0.011)
2017-18 x treated	-0.004	-0.008	-0.009	0.002	0.007	-0.01
	(0.003)	(0.010)	(0.007)	(0.010)	(0.013)	(0.013)
2018-19 x treated	-0.005	-0.001	-0.01	-0.003	0.009	-0.005
	(0.003)	(0.010)	(0.007)	(0.009)	(0.013)	(0.015)
2019-20 x treated	0	-0.008	-0.016*	-0.001	0.01	-0.017
	(0.003)	(0.012)	(0.007)	(0.011)	(0.013)	(0.017)
Num.Obs.	7795120	750205	1536710	1144755	490320	469840
R2	0.296	0.239	0.253	0.212	0.337	0.345
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X
FE: LAUA	X	X	X	X	X	X
FE: SIC2007	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 34   **Baseline results: Duration of longest employment stint**

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	2.142***	5.632**	4.875***	4.176**	5.138*	0.213
	(0.446)	(1.892)	(1.142)	(1.364)	(2.185)	(2.170)
2011-12 x treated	1.688***	3.578*	3.319***	2.897*	3.002+	0.326
	(0.360)	(1.551)	(0.930)	(1.200)	(1.574)	(1.663)
2012-13 x treated	0.595+	2.507+	1.538+	1.656	2.546+	0.345
	(0.338)	(1.330)	(0.856)	(1.063)	(1.533)	(1.719)
2013-14 x treated	0.252	0.07	0.49	1.565	0.021	-0.212
	(0.338)	(1.317)	(0.904)	(1.037)	(1.466)	(1.533)
2014-15 x treated	0.386	0.082	0.191	1.153	0.554	0.631
	(0.292)	(1.042)	(0.728)	(0.845)	(1.130)	(1.372)
2016-17 x treated	0.113	0.447	0.252	1.149	0.253	0.043
	(0.269)	(0.850)	(0.619)	(0.833)	(1.159)	(1.219)
2017-18 x treated	0.321	0.107	0.108	1.735+	0.818	0.252
	(0.305)	(1.077)	(0.717)	(0.901)	(1.344)	(1.413)
2018-19 x treated	-0.206	-0.243	-1.364+	0.915	0.079	-1.088
	(0.294)	(1.154)	(0.784)	(0.905)	(1.313)	(1.403)
2019-20 x treated	0.1	-0.537	-1.276+	1.803+	-1.245	0.016
	(0.340)	(1.270)	(0.757)	(1.033)	(1.360)	(1.272)
Num.Obs.	7646670	732550	1505815	1101540	482820	461565
R2	0.078	0.095	0.09	0.098	0.079	0.103
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X
FE: LAUA	X	X	X	X	X	X
FE: SIC2007	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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A3.2.2   Robustness checks

Table 35   25 vs. 23-year-olds results for all individuals: All labour market outcomes

	(1) Employment	(2) Number stints	(3) Earnings (log)	(4) Longest stint
2010-11 x treated	0.039***	0.060***	0.113***	6.897***
	(0.007)	(0.011)	-0.008	(0.732)
2011-12 x treated	0.017***	0.022***	0.053***	4.774***
	(0.003)	(0.004)	-0.007	(0.610)
2012-13 x treated	0.010***	0.016***	0.031***	2.372***
	(0.002)	(0.004)	-0.006	(0.518)
2013-14 x treated	0.009***	0.015***	0.023***	0.276
	(0.001)	(0.003)	-0.005	(0.453)
2014-15 x treated	0.002*	0.004**	0.004	0.193
	(0.001)	(0.001)	-0.003	(0.339)
2016-17 x treated	0	0.002+	0.000	-0.126
	(0.001)	(0.001)	-0.003	(0.326)
2017-18 x treated	-0.001	0.002	(0.001)	-0.026
	(0.001)	(0.001)	-0.004	(0.384)
2018-19 x treated	-0.002	0.002	(0.006)	-0.622
	(0.001)	(0.002)	-0.004	(0.450)
2019-20 x treated	-0.004*	-0.001	0.000	0.113
	(0.002)	(0.003)	-0.005	(0.513)
Num.Obs.	10936010	10936010	7675420	7541125
R2	0.017	0.011	0.323	0.084
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X
FE: year	X	X	X	X
FE: AgeSept	X	X	X	X
FE: LAUA			X	X
FE: SIC2007			X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 36   Low-paying sectors results: Earnings (log)

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	0.034***	0.052*	0.050***	0.051**	0.079*	0.001
	(0.006)	(0.023)	(0.013)	(0.018)	(0.031)	(0.031)
2011-12 x treated	0.019**	0.032+	0.028*	0.029+	0.052+	0.006
	(0.006)	(0.018)	(0.013)	(0.017)	(0.027)	(0.031)
2012-13 x treated	0.018**	0.037*	0.027*	0.037*	0.055*	0.021
	(0.006)	(0.017)	(0.011)	(0.015)	(0.026)	(0.028)
2013-14 x treated	0.006	0.01	0.013	0.012	0.049+	-0.009
	(0.005)	(0.015)	(0.012)	(0.013)	(0.027)	(0.026)
2014-15 x treated	0.007	0.021	0.014	0.009	0.055**	0.002
	(0.004)	(0.014)	(0.011)	(0.010)	(0.019)	(0.022)
2016-17 x treated	-0.005	-0.013	-0.021*	-0.017	0.019	-0.029
	(0.004)	(0.014)	(0.008)	(0.012)	(0.019)	(0.020)
2017-18 x treated	-0.001	-0.009	-0.003	-0.004	0.038	-0.022
	(0.006)	(0.016)	(0.012)	(0.013)	(0.026)	(0.024)
2018-19 x treated	-0.003	-0.006	-0.003	-0.003	0.021	-0.017
	(0.006)	(0.016)	(0.012)	(0.014)	(0.025)	(0.030)
2019-20 x treated	0.004	-0.014	-0.014	0	-0.004	-0.050+
	(0.006)	(0.018)	(0.012)	(0.015)	(0.028)	(0.030)
Num.Obs.	3821840	439540	855375	683190	238010	241970
R2	0.187	0.156	0.16	0.15	0.2	0.22
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X
FE: LAUA	X	X	X	X	X	X
FE: SIC2007	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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Table 37   **Low-paying sectors results: Longest employment stint**

	(1) All	(2) FSM	(3) IDACI	(4) Low educ	(5) Female NW	(6) Male NW
2010-11 x treated	2.567***	7.888**	5.683***	4.653**	5.063+	-0.59
	(0.594)	(2.559)	(1.418)	(1.757)	(2.592)	(2.577)
2011-12 x treated	1.594**	3.412+	2.858*	1.9	3.716+	0.041
	(0.508)	(1.951)	(1.123)	(1.515)	(2.234)	(2.597)
2012-13 x treated	0.559	2.396	1.475	1.066	1.776	0.13
	(0.474)	(1.814)	(1.126)	(1.363)	(1.972)	(2.416)
2013-14 x treated	0.718	0.691	1.215	1.182	0.618	-0.527
	(0.474)	(1.661)	(1.187)	(1.275)	(2.142)	(2.101)
2014-15 x treated	1.068*	0.434	0.716	0.25	2.412	0.82
	(0.457)	(1.470)	(1.143)	(1.142)	(1.884)	(1.922)
2016-17 x treated	0.299	0.779	-0.456	0.16	0.357	-1.791
	(0.448)	(1.292)	(0.932)	(1.165)	(1.937)	(1.886)
2017-18 x treated	0.496	-0.196	-0.091	1.253	1.36	-1.246
	(0.545)	(1.710)	(1.198)	(1.243)	(2.266)	(2.190)
2018-19 x treated	-0.056	-0.181	-0.542	0.148	2.177	-2.555
	(0.508)	(1.667)	(1.203)	(1.307)	(2.307)	(2.539)
2019-20 x treated	0.19	-0.426	-0.752	0.648	-3.734	-3.009
	(0.531)	(1.691)	(1.225)	(1.363)	(2.585)	(2.253)
Num.Obs.	3769685	434390	844160	674980	235000	238905
R2	0.068	0.085	0.081	0.096	0.057	0.086
Std.Errors (cluster)	Year and month	Year and month	Year and month	Year and month	Year and month	Year and month
FE: treated	X	X	X	X	X	X
FE: year	X	X	X	X	X	X
FE: AgeSept	X	X	X	X	X	X
FE: LAUA	X	X	X	X	X	X
FE: SIC2007	X	X	X	X	X	X

Note: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

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