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The impact of age within academic year on adult outcomes

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Abstract

Children born at the end of the academic year have lower educational attainment, on average, than those born at the start of the academic year. Previous research has shown that the difference is most pronounced early in pupils' school lives, but remains evident and statistically significant in high-stakes exams taken at the end of compulsory schooling. Those born later in the academic year are also significantly less likely to participate in post-compulsory education than those born at the start of the year. We provide the first evidence on whether these differences in childhood outcomes translate into differences in the probability of employment, occupation and earnings for adults in the UK. We also examine whether there are differences in broader measures of well-being such as self-perceived health and mental health. We find that the large and significant differences observed in educational attainment do not lead to pervasive differences in adulthood; those born towards the end of the academic year are more likely to experience unemployment (which is particularly true for females and those that don't achieve a degree level qualification) but in general there are few substantial or statistically significant differences in terms of occupation, earnings and self-perceived health and mental health. It is not clear why this should be the case, but if employers reward productivity equally as they learn more about their workers, irrespective of their educational attainment, then this lack of significant differences may not be surprising.

Key words: Month of birth, wages, employment, educational attainment

JEL classification: I21, J24

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1 Introduction

Children born at the end of the academic year have lower educational attainment, on average, than those born at the start of the academic year. This finding, documented in the UK[5] and elsewhere[6], is most pronounced early in pupils' school lives, but remains evident and statistically significant in high-stakes exams taken at the end of compulsory schooling (Crawford et al., 2007, 2011). Moreover, the month in which a child is born not only affects their national achievement test scores, but also affects how they feel about their own ability and the degree to which they believe they can influence their future (Crawford, Dearden & Greaves, 2011). These findings clearly have important implications for child wellbeing, which may itself be a rationale for policy action in this area.

However, it is also vital to understand whether an individual's position within their academic cohort has a lasting impact once individuals have left compulsory education. Previous studies have documented that differences exist in the progression to, and attainment at, higher education. Relevant to the UK context, Crawford, Dearden & Greaves (2011) show that month of birth matters for the educational choices made by young people participating in further education (such as the decision to take vocational qualifications) and Crawford, Dearden & Meghir (2010) show that month of birth matters for young people's decision about whether or not to go to university. HEFCE (2005) show the same result using alternative administrative data for England and Scotland. Given that education choices are likely to have potentially far-reaching consequences for employment, wages and health, it seems plausible that an individual's choices, experiences and achievements during adulthood may be affected by the month in which they were born.

We are not the first to consider this issue, but we are the first to consider the impact of age relative to academic year cut-off on labour market outcomes in the UK, and the first to consider its role in explaining wider measures of wellbeing such as health. To date, the literature has focused primarily on the effect of school starting age (which is driven by the relationship between month of birth and the academic year cut-off). Bedard & Dhuey (2012) exploit variation in school admissions policies across U.S. states and find a small positive effect of starting school older on later earnings. Using a state of birth level repeated cross-section of men born 1959-1981, combined with school entry laws from 1964-1986, Bedard & Dhuey find that shifting the school entry cutoff date by one month (so that all pupils are one month older when they start school) increases male hourly earnings by approximately 0.5 percent.

Similarly, applying a regression discontinuity design around the sharp school entry cut-off in Sweden, Fredriksson & Ockert (2005) find that starting school older has a small positive effect on earnings; however, they also point out that because starting school later means entering the labour market later, the net earnings effect over the life-cycle is in fact negative. Solli (2011) finds a similar result using Norwegian register data.

By contrast, other studies find little effect of school starting age on labour market outcomes, at least in the long run. Black, Devereux and Salvanes (2008) – using the same Norwegian register data as Solli (2011) – find a short-run positive effect on earnings of starting school at a younger age, although this effect is no longer present age 30. Dobkin & Ferreira (2010) find no differences in earnings, as well as no differences in family income, house ownership, house value and marital status according to the timing of entry to

[5] See, for example, Russell and Startup (1986), Bell and Daniels (1990), Sharp, Hutchison and Whetton (1994), Thomas (1995) and Alton and Massey (1998).

[6] See studies for the US (Datar, 2006; Elder and Lubotsky, 2009; Aliprantis, 2011; Robertson, 2011), Canada (Smith, 2009 and 2010), Germany (Jurges and Schneider, 2007; Puhani and Weber, 2007; Mühlenweg and Puhani, 2010), Sweden (Fredriksson and Ockert, 2005), Norway (Strom, 2004), Chile (McEwan and Shapiro, 2008), Australia (Buddelmeyer and Le, 2011), Italy (Ponzo and Scoppa, 2011), Japan (Kawaguchi, 2011), Hungary (Hamori and Kollo, 2011), Malta (Borg and Falzon, 1995) and Brazil (Sampaio et al., 2011). Studies using cross-country international data sets include Bedard and Dhuey (2006), Borghans and Diris (2010) and Sprietsma (2010).

school. Kawaguchi (2011) uses the sharp discontinuity in academic cohorts in Japan and finds no statistically significant difference between the relatively oldest and youngest in terms of earnings, the employment rate, or the probability of marriage. Zweimüller (2011) finds that although there are short-term differences in wages, wage profiles converge after three years of labour market experience.

To investigate whether month of birth continues to affect outcomes into adulthood in the UK, we use simple linear and log-linear regression to determine whether there is a statistical relationship between age relative to academic year cut-off and a range of adult outcomes. To do so, we use two complementary sources of data: Understanding Society, one of the largest longitudinal studies in the world, designed to track the attitudes and circumstances of 100,000 individuals living in 40,000 households in the UK today, and the Labour Force Survey, which is largest household survey in the UK and provides the official measures of employment and unemployment. The benefit of the Labour Force Survey is the large sample size (around 46,200 households in each wave) although the range of information collected about respondents is less broad than for Understanding Society, given the focus of the data on labour market outcomes. In contrast, Understanding Society has a smaller achieved sample size (around 30,000 households in the first wave) but collects a wide range of information, such as highest educational qualification; current employment status and employment history; current occupation, wages and income; health and other measures of self-reported wellbeing. Importantly, for our purposes, month of birth is observable in each dataset.

The main research question to be addressed is thus whether an individual's age relative to academic year cut-off continues to affect outcomes observed in adulthood. Variation in relative age occurs from those born in different months of the year within UK countries, and between those born in the same months of the year across UK countries (as the academic year is different). The academic year in England and Wales runs from 1 September to 31 August and is split into three terms (autumn, spring and summer). It is a statutory requirement for children in England and Wales to start school by the beginning of the term after they turn 5, but within these confines school admissions policies are set by local (rather than central) authorities, and in most cases children start school considerably earlier than this.[7] In Scotland, children born in March are the oldest in the academic year (and children born in February the youngest), while in Northern Ireland the oldest and youngest are those born in July and June respectively. In Scotland, it is possible for parents to delay their child's entry to primary school for one year if they are born between August and February (although only those born in January and February are guaranteed a state funded nursery place in the intervening period).[8]

Section 2 outlines the simple methodology we adopt, while Section 3 discusses the data we use; Section 4 presents our results and Section 5 concludes.

[7] It is possible for parents to defer the date on which their child starts school up to and including the statutory date (with the agreement of their local authority). This means that their child can start school later, but would be placed into the correct academic year for their age, thus reducing the amount of time that they spend in school overall. This means it is relatively rare for parents to do this. (http://media.education.gov.uk/assets/files/pdf/s/school%20admissions%20code%201%20february%202012.pdf.)

[8] Source: http://www.edinburgh.gov.uk/info/851/nurseries_and_playgroups/566/pre-school_nursery_education/4.

2 Methodology

Like previous work in this area, our methodology is straightforward. We regress the outcome of interest on age relative to academic year cut-off, entered as a set of binary variables for month of birth relative to the oldest in the academic year-group (which allows the impact of relative age to be non-linear). In robustness checks we also include background characteristics of the individuals, but our results are unaffected by their inclusion. For all continuous outcomes we use linear regression (apart from gross hourly wages, where we use log-linear regression), where the dependent variable (or log of the dependent variable) is the outcome of interest and independent variables include relative age and background characteristics (that aren't affected by a person's relative age).

The coefficients on the set of binary variables for relative age are therefore interpreted as the difference (on average) between those that were relatively young in their academic cohort and the relatively oldest. The linear regression takes the following form:

$$y_{ict} = \alpha + \boldsymbol{RELAGE}'_{ict}\boldsymbol{\beta}_R + \boldsymbol{X}'_{ic}\boldsymbol{\beta}_x + \varepsilon_{it} \quad (1)$$

where y is the outcome of interest for individual i in country c measured at age t, **RELAGE** is a vector of dummy variables indicating the relative age of individuals within their academic cohort (which is specific to their country of schooling) and $\boldsymbol{X}$ is a vector of individual characteristics and controls for academic cohort of birth and quarter of interview. The log-linear regression takes the form:

$$\ln(y_{ict}) = \alpha + \boldsymbol{RELAGE}'_{ict}\boldsymbol{\beta}_R + \boldsymbol{X}'_{ic}\boldsymbol{\beta}_x + \varepsilon_{it} \quad (2)$$

where the coefficients on the set of binary variables for relative age are interpreted as the percentage difference (on average) between those that were relatively young in their academic cohort and the relatively oldest, rather than the difference.

For binary outcomes the regression takes the form:

$$\begin{aligned} P(y_{ict} = 1|RELAGE, X) &= \alpha + \boldsymbol{RELAGE}'_{ict}\boldsymbol{\beta}_R + \boldsymbol{X}'_{ic}\boldsymbol{\beta}_x + \varepsilon_{it} \\ &= F(\alpha + \boldsymbol{RELAGE}'_{ict}\boldsymbol{\beta}_R + \boldsymbol{X}'_{ic}\boldsymbol{\beta}_x) \\ &= F(\boldsymbol{X}'_{ic}\boldsymbol{\beta}) \end{aligned} \quad (3)$$

Where ε_{it} is assumed to be normally distributed and as such F represents the normal cumulative distribution function. We use the estimated vector of coefficients $\widehat{\boldsymbol{\beta}}_R$ and $\widehat{\boldsymbol{\beta}}_x$ to compute marginal effects, which for the set of binary variables for relative age are interpreted as the impact of moving from a particular relative age to the relatively oldest. For example, the marginal effect for the relatively youngest ($\boldsymbol{RELAGE_1}$) is given by:

$$P(y_{ict} = 1|RELAGE, X) = F(\boldsymbol{X}'_{ic}\boldsymbol{\beta}|\ \boldsymbol{RELAGE_1} == 1) - F(\boldsymbol{X}'_{ic}\boldsymbol{\beta}|\ \boldsymbol{RELAGE_1} == 0) \quad (4)$$

RELAGE is constructed on the basis of age relative to academic year cut-off in the country in which they were born. This means that pupils born in August in England and Wales, June in Northern Ireland and February in Scotland would be assigned a relative age of one (the relatively youngest), while pupils born in September in England and Wales, July in Northern Ireland and March in Scotland would be assigned a relative age of twelve (the relatively oldest).

To calculate the total effect of age within cohort on adult outcomes, it is important that $\boldsymbol{X}'_{ict}$ excludes characteristics that can be affected independently, as in this case the regression would underestimate the impact of relative age on adult outcomes. Understanding Society is particularly helpful in this regard, as retrospective information about individuals' family background when they were age 14 – such as father's occupation and whether their mother worked – was collected.

Finally, we are mindful of compulsory school leaving laws as well as school admissions policies. Pupils in England and Wales are currently required to remain in school until the last Friday in June of the academic year in which they turn 16. There have been some changes to the school leaving age and dates over time, however, which are summarised in Appendix Table 1. For example, before 1998, young people in England and Wales could leave school at the end of the spring term following their 16th birthday if they were born between 1st September and 31st January, but had to stay in school until the last Monday in May if they were born between 1st February and 31st August. This means that it is important for us to account for birth cohort in our analysis[9].

In all cases standard errors are corrected to account for the correlation of error terms ε_{it} within household, as both the LFS and Understanding Society are household surveys.

[9] The educational context is likely to influence the relationship between relative age within cohort and individuals' outcomes. For example, the difference in the probability of acquiring qualifications may be reduced where those born earlier in the academic year are able to leave compulsory schooling before the exam period: the disadvantage of being born relatively late in the year may be partly offset if forced to complete qualifications. Similarly, the expansion of universities may reduce the inequality in university participation between those born relatively late or early in the academic cohort.

3 Data

The Labour Force Survey is a survey of households living at private addresses in the UK. Its focus is on employment and earnings, as its main purpose is to provide information on the UK labour market which is used for official measures of employment and unemployment.[10] As such, it includes a relatively limited set of background characteristics, but it does include information on educational qualifications. The LFS has a large sample size – around 46,200 households are interviewed each quarter[11] – and uses a rotational sampling design, in which a household is retained in the sample for a total of five consecutive quarters and then replaced. Information about employment status is asked in each wave, but details of earnings and income are requested only in the first and fifth waves. We focus our attention on individuals surveyed for the first time between 2002 and 2011 and who were born in England, Wales, Scotland or Northern Ireland.

Understanding Society is a large longitudinal panel study following around 40,000 households in the UK. It collects information on a wide range of topics, including educational qualifications, labour market status and history, and self-reported health and well-being.[12] Information is collected from all individuals aged 16 and upwards in the household, with a separate questionnaire for 10- to 15-year-olds. Panel members are followed if they leave the household (as are members of their new household), and new members of existing households are also added to the survey. The first wave of Understanding Society – which we use – took place between January 2009 and January 2011, although it built on and extended the long-running British Household Panel Survey (BHPS), with households due to be surveyed approximately annually going forwards.

In both surveys we focus attention on those aged 25-64 and infer the country of schooling primarily from the country of birth, as this information is the only proxy available for the LFS, and for all those born before 1981 for Understanding Society. This assumption appears to be reasonable, as there is a high cross-over between country of birth and country of school for the individuals surveyed as part of Understanding Society born after 1981 (who were asked the country in which they went to school); 98.9% and 100% of those that were born in England and Wales, respectively, were subject to the school admissions system operating in their country of birth (that is, report that their school was in England or Wales). For those born in Scotland and Northern Ireland this figure is slightly lower, at 92.6% and 95.0% respectively, but the cross-over remains high.

The distribution of month of birth is very similar between the two datasets, with a slightly higher proportion of March births in both sources. Figure 1 shows that the distribution of individuals by year of birth varies across the datasets, with Understanding Society having a relatively large sample of younger individuals[13]. To take account of the different composition of ages, we include a set of binary variables in equations 1 to 4 for academic cohort of birth (where five academic cohorts are grouped). We restrict out

[10] See http://www.ons.gov.uk/ons/guide-method/method-quality/specific/labour-market/labour-market-statistics/volume-1---2011.pdf.

[11] The LFS has been carried out quarterly since 1992. Before this, it occurred annually (from 1986), but information on wages was not collected prior to 1992.

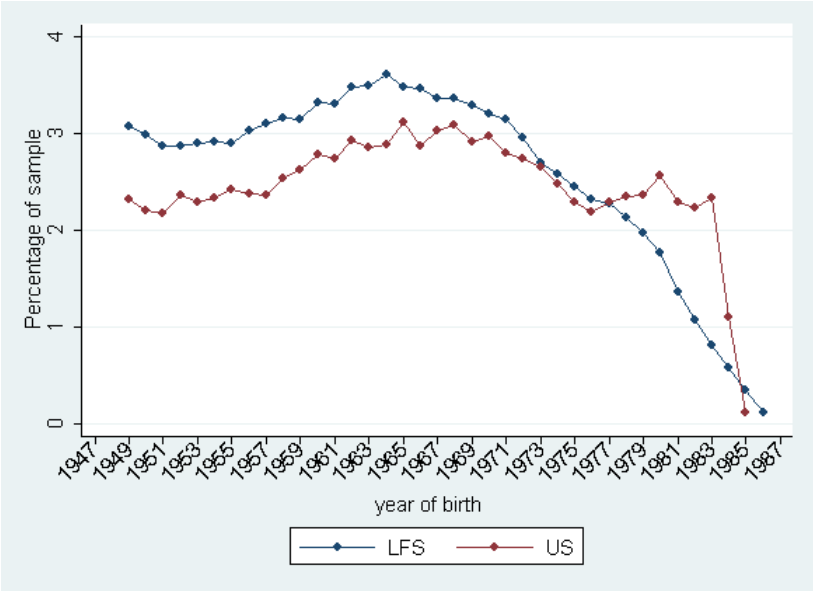
[12] For more details on Understanding Society, see https://www.understandingsociety.ac.uk/.

[13] This is unlikely to be due to sampling of those of younger ages most likely to be at university, as our sample selection includes those between age 25 and 64 at the time of the survey. Even without this restriction, the LFS and Understanding Society took a similar approach to sampling those living away for education: the LFS target population is all people "resident in private households, resident in National Health Service accommodation, and young people living away from the parental home in a student hall of residence or similar institution during term time"13, while Understanding Society also attempted to get responses (including proxy responses) from absent household members that are absent for school, university, work or in an institution or hospital (Understanding Society project instructions).

sample to those aged between 25 and 64 at the time of interview to ensure that all young people would have finished full-time education.

We also include a set of binary variables for year and quarter of interview to account for differences in outcomes over time.

Figure 1: Distribution of year of birth



Source: LFS and Understanding Society

Table 1: Summary statistics for those born 1948 to 1987

	Understanding Society *interviewed between 2009 and 2011*			LFS *interviewed between 2009 and 2011, wave 1*			LFS *interviewed between 2002 and 2011, wave 1*		
Outcome	N	**Mean**	SD	N	**Mean**	SD	N	**Mean**	SD
No qualifications	26,424	**0.22**	0.41	81,043	**0.13**	0.34	332,346	**0.14**	0.35
Achieved education level: A level (or equivalent)	26,424	**0.43**	0.5	81,043	**0.55**	0.5	332,346	**0.53**	0.5
Achieved education level: Degree +	26,424	**0.24**	0.43	81,043	**0.23**	0.42	332,346	**0.21**	0.41
Employment status: ILO unemployed	26,534	**0.07**	0.25	82,277	**0.05**	0.21	342,747	**0.03**	0.18
Employment status: employed	26,534	**0.63**	0.48	82,277	**0.67**	0.47	342,747	**0.69**	0.46
Employment status: self-employed	26,534	**0.10**	0.3	82,277	**0.11**	0.31	342,747	**0.11**	0.31
Current job national statistics socio-economic classification: professional	25,334	**0.28**	0.45	74,714	**0.30**	0.46	313,511	**0.29**	0.45
Current job national statistics socio-economic classification: routine	25,334	**0.19**	0.39	74,714	**0.18**	0.38	313,511	**0.18**	0.39
Usual gross hourly pay: current job	13,307	**14.15**	33.18	44,224	**13.28**	10.4	184,931	**12.28**	10.52

The summary statistics in Table 1 show that outcome variables are reasonably well matched across the two sources of data, as the mean and standard deviation for most outcome variables are similar for Understanding Society and LFS, with the exception of the proportion of respondents with no educational qualifications, the proportion with A level (or equivalent) and the standard deviation of usual gross hourly pay. The difference in qualification is likely to be driven by the more detailed coding of qualifications in the LFS, where more low level qualifications are included, in addition to the relatively young sample in Understanding Society.

The benefit of the larger sample size of the LFS is evident: the number of observations is around four times that when the interview period is constrained to be the same as that for Understanding Society, and around thirteen times that when the interview period is increased.

4 Adult outcomes for the relatively oldest and youngest in their academic cohort

Figure 2 to Figure 8 show the differences in outcomes for relatively young and relatively old individuals born in different years (and therefore subject to different education systems). The outcomes (on average) for those born towards the end of the academic year (the relatively young) are shown in the darker blue, while outcomes for the relatively old are shown in the lighter yellow. The dashed lines represent the upper and lower 95% confidence intervals, which are evidently larger for Understanding Society where the sample size is smaller.

This graphical presentation demonstrates the general trends in education participation and unemployment over time, which may affect the presence of differences in adult outcomes for the relatively old and young within a cohort. For example, as the probability of having no formal qualifications decreases (perhaps as a result of the requirement for all pupils to stay in school until after the exam period) the difference in qualification rates between the relatively old and relatively young (on average) may increase, as the relatively old (and less academically inclined) can no longer leave school before the exam period.

The figures also demonstrate the similarity of trends over time observed in the LFS and Understanding Society, for all outcomes. As noted previously, however, the proportion of individuals with no educational qualifications is higher in Understanding Society, which is likely to be as a result of the less detailed questions in the survey. The larger sample size of LFS is also evident through the smaller confidence intervals and slightly smoother trends in outcomes across the birth cohorts.

There is some evidence in the LFS that the probability of having no educational qualifications is higher for the relatively young in more recent academic cohorts, but slightly lower in older academic cohorts. This perhaps reflects changes in the compulsory school leaving age and dates over time; the relatively old in cohorts born before 1980-1981 were able to leave school before the exam period (see Appendix Table 1) which may reduce the proportion of the relatively old acquiring qualifications.

Figure 3 shows that the relatively old are more likely to achieve a degree qualification (or higher) than the relatively young, which is true for the majority of cohorts. This is clearly and precisely evident in the LFS, and evident (although less precise) in Understanding Society.

Figure 4 shows some evidence that the relatively young are more likely to be unemployed for most cohorts, but Figure 5 shows some evidence that the probability of being employed or self-employed depends to some extent on the cohort of birth; the relatively young in the oldest cohorts are more likely to be employed, but the opposite is true for more recent cohorts. It is plausible that this is an effect of labour market experience: if those born at the end of the academic year are more likely to have left education and entered the labour market earlier than those born at the start of the academic year, then they may be reaping the rewards of this additional experience in terms of the likelihood of being in work at a given age.

Figure 6 and Figure 7 show some evidence that the probability of having a professional occupation is lower, and the probability of having a routine occupation is higher, for the relatively young in some cohorts, but there is no consistent pattern.

Similarly, while for some cohorts (log) gross hourly wages (in 2012 prices) are lower for the relatively young, this is not consistent across all cohorts (Figure 8), and the differences are not statistically significant. In line with the findings for employment described above, however, those born at the end of the academic year seem to earn slightly *more* per hour than those born at the start of the academic year amongst the most recent birth cohorts. Again, it is plausible that this is an effect of labour market experience. This is similar to the explanation offered by Black, Devereux and Salvanes (2008), who found

evidence of a small positive effect on earnings of starting school younger, which had disappeared by around age 30.

Although there appears to be some variation in the impact of relative age for those in different academic cohorts, we present the results of our empirical analysis for the cohorts overall. Noticeable differences across academic cohorts are described in the text and presented in full in the online appendix. All results discussed below are robust to the inclusion of background characteristics. These results are available on request from the authors.

Table 2 presents the results for the LFS. These results are likely to be reliable, given the large and representative sample. There is evidence that those that are relatively young in their academic cohort are less likely to achieve a degree level qualification; those that are the relatively youngest in their cohort are 1.2 percentage points less likely to achieve a degree than those that are the relatively oldest, for example. The marginal effects for all months (where the reference category is the relatively oldest) are negative, suggesting that this finding is robust and affects pupils born across the academic year. This finding is consistent with previous research: Crawford, Dearden and Meghir (2010) find that young people born in August in England (the relatively youngest) are 2.3 percentage points less likely to go to university at age 18 or 19 (i.e. straight after finishing further education or following a single gap year) than young people born in September (the relatively oldest); HEFCE (2005) find a similar result using different administrative data. The finding from the LFS adds to this literature by suggesting that the relatively young in the academic cohort do not acquire further education later in life, as the significant difference in the probability of having a degree remains for older adults. The results from Understanding Society are also consistent; although the difference is not statistically significant: Table 5 shows that the relatively youngest in the cohort are 2.1 percentage points less likely to achieve a degree than the relatively oldest, on average. Like the LFS, the majority of marginal effects are negative, suggesting that individuals born across the academic year are affected by their relative age in this domain.

The relatively young are also less likely to have no educational qualifications (as well as less likely to have higher level qualifications). The marginal effect of being relatively younger in both the LFS and Understanding Society is negative across most of the academic cohort (although not always statistically significant). Closer investigation reveals that this negative impact is driven by older cohorts, for whom the compulsory school leaving age fell before the final exams for the relatively older pupils (see Table 8 in the online appendix). It is therefore unlikely that this finding is generalisable to the present cohort of school leavers and more recent cohorts of adults, where all pupils are required to remain in school until after final year assessments.

If there are returns to attending higher education, we may expect that individuals that are relatively young within their academic cohort have lower earnings, probability of employment and occupational status, on average, than relatively old individuals. In fact, there is little evidence that the higher proportion of relatively old individuals achieving a degree translates into better earnings or employment prospects, on average. In the LFS, those that are relatively young in their cohort tend to have lower gross hourly pay, are more likely to have a routine rather than professional occupation, and are slightly more likely to be unemployed, but these differences are rarely statistically significant despite the large sample size. The most consistent impact appears to be on the probability of being unemployed (according to the ILO definition where participation on a government scheme or unpaid labour for a family business are included as employment) where amongst the sample as a whole, the relatively youngest in their academic cohort are 0.4 percentage points more likely to be unemployed than the relatively oldest.

There is some suggestion of variation in the impact on gross hourly pay (in 2012 prices) across older and younger cohorts: amongst the most recent birth cohorts, the relatively young earn significantly *more* per hour than the relatively old (see online appendix Table 7). This may be due to returns to labour market experience: the relatively young may be more likely to accrue years of work experience at a younger age,

which is supported by the finding that most recent birth cohorts are significantly *less* likely to be unemployed early in their careers, while older birth cohorts are significantly more likely to be unemployed).[14] A similar explanation was offered by Black et al. (2008), who found evidence of a small positive effect on earnings of starting school younger, which had disappeared by around age 30.

Results from Understanding Society largely support this conclusion, although in this sample relatively young individuals tend to be more likely to have a professional occupation, and differences are not statistically significant in general. Understanding Society reveals that household income, as well as hourly pay, is largely unaffected by an individual's relative age.

Do these findings imply that there are no returns to education? Figure 9 to Figure 13 demonstrate that those with at least a degree level of education have better earnings and employment prospects than those without a degree: Figure 9 shows that those that do not achieve a degree are more likely to be unemployed in both the LFS and Understanding Society samples; Figure 11 shows the sizeable difference in the probability of having a professional occupation for those with and without a degree; Figure 13 shows that those with a degree have higher gross pay per hour (although this margin seems to decline for more recent cohorts). These descriptive summaries of course do not account for the unobservable differences in those that choose to continue in further education and those that do not: although it appears that there are sizeable returns to acquiring a degree, it may be that the observed gains are entirely driven by underlying differences in the ability (or productivity) of those that choose different routes.[15]

Of those that choose to complete a degree or not, there appear to be very few differences between the relatively older and relatively younger individuals in the cohort. Table 5 confirms that there are no significant differences between the group of individuals with and without a degree in the impact of relative age on the probability of being unemployed, employed or self-employed, having a professional or routine occupation, or gross hourly pay. There is some suggestion that relative age has a larger impact on the probability of having a professional occupation for those that acquire a degree, however, which could be driven by differences in previous leadership experience (Dhuey and Lipscomb, 2008). This is also consistent with recent research that finds that relative age affects the probability of becoming a CEO (Du et al, 2012) and politician in the US (Muller and Page, 2013). Table 5 also provides some suggestion that the higher probability of unemployment for the relatively young in the academic cohort is driven by those that do not acquire a degree, suggesting that the relatively young that do not advance to higher education are particularly disadvantaged.

The figures and tables discussed so far clearly demonstrate the superiority of the LFS in its larger sample size, as the results are precise enough to detect significant differences in relatively small effects. The benefit of the Understanding Society sample, however, is the ability to observe whether there are differences in wider outcomes that are typically unobservable to researchers, such as self-assessed general and mental health, and intergenerational mobility (defined here as the probability of having a higher occupational status than ones father). As expected, the standard errors for the Understanding Society sample are large, which limits the conclusions we can draw. However, it seems clear that there are no systematic patterns in terms of self-reported general health, and no large differences in self-assessed mental health or intergenerational mobility. The direction of these effects do seem to be consistent in that the relatively young are more likely to have poorer mental health and lower intergenerational mobility, but there is not sufficient evidence to conclude that adults' well-being is affected by their relative age in their classroom.

[14] This assumes that the effects seen for older birth cohorts at older ages would be similar to those found for more recent cohorts at older ages.

[15] There is a large literature that aims to estimate the returns to education, accounting for omitted variable bias. See Psacharopoulos and Patrinos (2004) for a recent review of this long literature.

Table 6 reports the variation in the impact of relative age for males and females in the LFS, where the larger sample size permits such analysis. There is some evidence that females are disproportionately affected by their relative age in terms of the probability of acquiring any and degree level qualifications, for example the relatively youngest females are 1.6 percentage points less likely to achieve a degree, compared to 1.1 percentage points for the relatively youngest males (a difference that appears to be larger for more recent cohorts). The impact of relative age on the probability of unemployment also appears to be driven by females, where the relatively youngest are 0.6 percentage points more likely to be unemployed compared to 0.2 percentage points for the relatively youngest males, for example. The impact of relative age on the probability of having a professional occupation also appears to be larger for females, although these differences (like all differences between males and females discussed here) are not statistically significant. The explanation for these patterns in the data is unclear: it may be that females have, on average, particular non-cognitive traits that make them more susceptible to negative effects of relative age, or that females are particularly affected by their experience in school.

5 Conclusion

Previous research in the UK has shown that there are substantial differences between children born earlier and later in the academic year in terms of educational attainment, attitudes and behaviours observed during childhood (Crawford et al., 2011), and further and higher education decisions and outcomes (Crawford et al., 2007, 2010). This paper has provided new evidence on the extent to which these differences persist into adulthood.

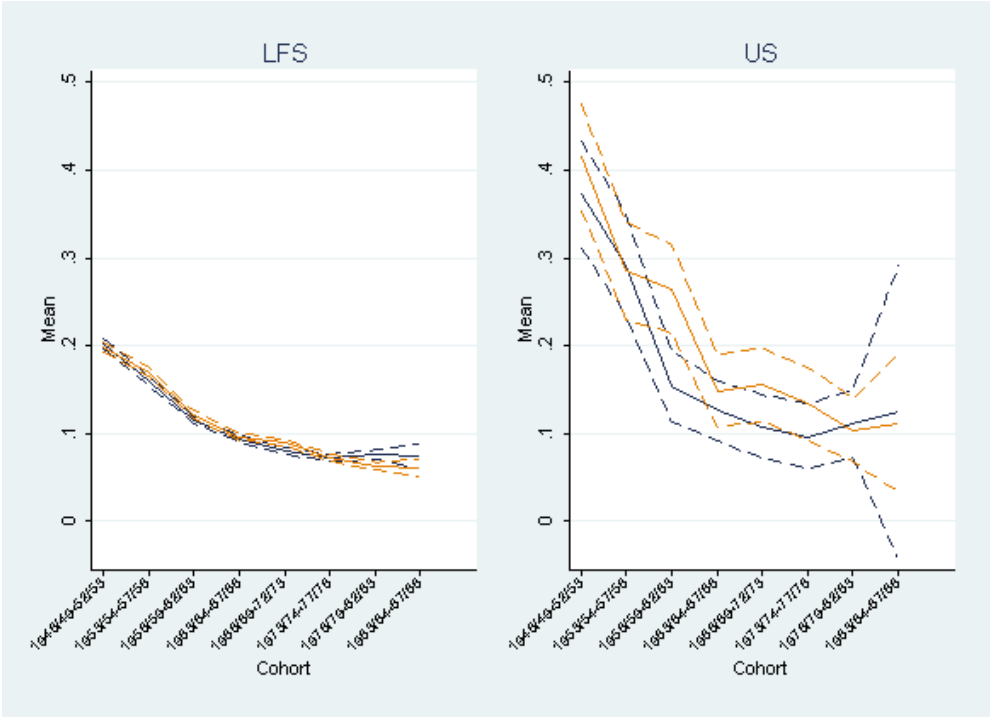
We find strong evidence that the differences in degree completion that we found for younger cohorts are still present for older age groups. This suggests that those who are relatively young in their academic cohort do not "catch up" with their peers by acquiring additional education later in life, as the significant difference in the probability of having a degree remains even for older adults.

In terms of other outcomes, however, we find little evidence of any consistent differences between those both earlier and later in the academic year during adulthood: when we look across our sample as a whole, our analysis suggests that differences in labour market status and wider wellbeing between individuals born in different months are, in general, both economically and statistically insignificant in the UK.

There are some interesting differences by birth cohort, however: in particular, we find that, amongst the most recent birth cohorts (those that we observe in their late 20s and early 30s), individuals born at the end of the academic year are slightly *more* likely to be in work, *less* likely to be unemployed and seem to earn slightly *more* per hour than those born at the start of the academic year. It is plausible that this is an effect of labour market experience: if those born at the end of the academic year are more likely to have left education and entered the labour market earlier than those born at the start of the academic year, then they may be reaping the rewards of this additional experience in terms of the likelihood of being in work and of earning higher wages at a given age. These effects do not persist, however, such that – when looking across 25-64 year olds as a whole – there are very few significant differences between individuals born at the start and end of the academic year.

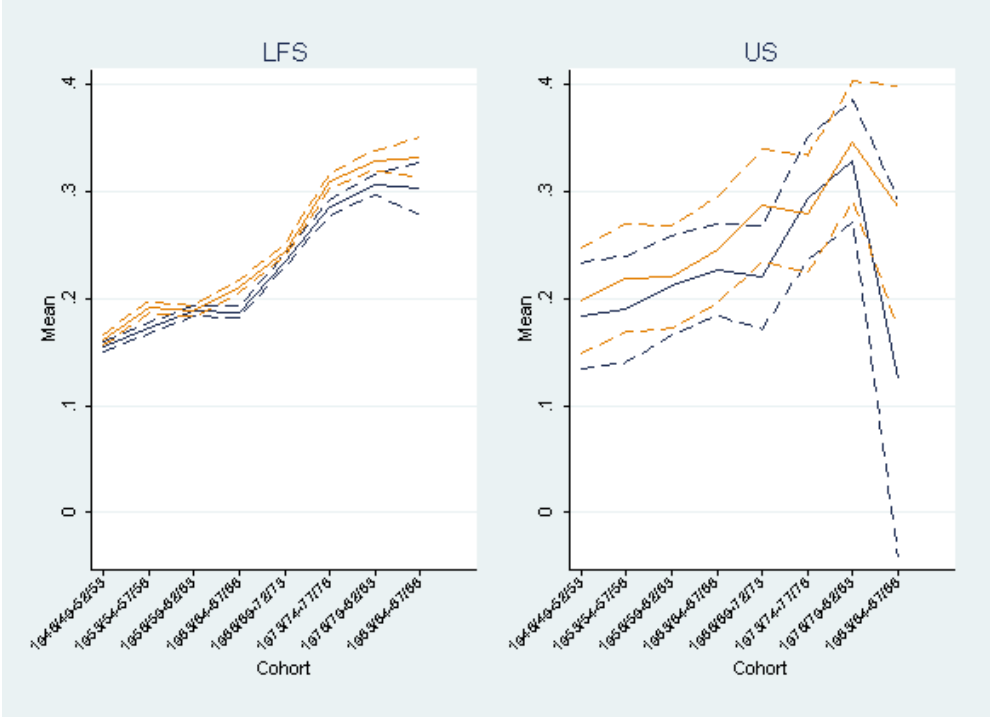
It is not completely clear why the sizeable differences in educational attainment that we observe between those born at the start and end of the academic year are not reflected in the labour market. It is possible that if we had population data – such as that available to Fredriksson & Ockert (2005) and Black et al. (2008) – then we might find evidence of significant differences: the overall point estimates described above certainly all point in the "expected" direction, with the relatively young less likely to be in work, more likely to be unemployed and likely to earn less, on average, than the relatively old. On the other hand, it may simply be the case that further and higher education are not productivity enhancing for the marginal individual, or that workers' productivity levels out at older ages. In this case, the lack of significant differences between individuals born at different times of the year is not surprising.

Figure 2: The probability of having no educational qualifications



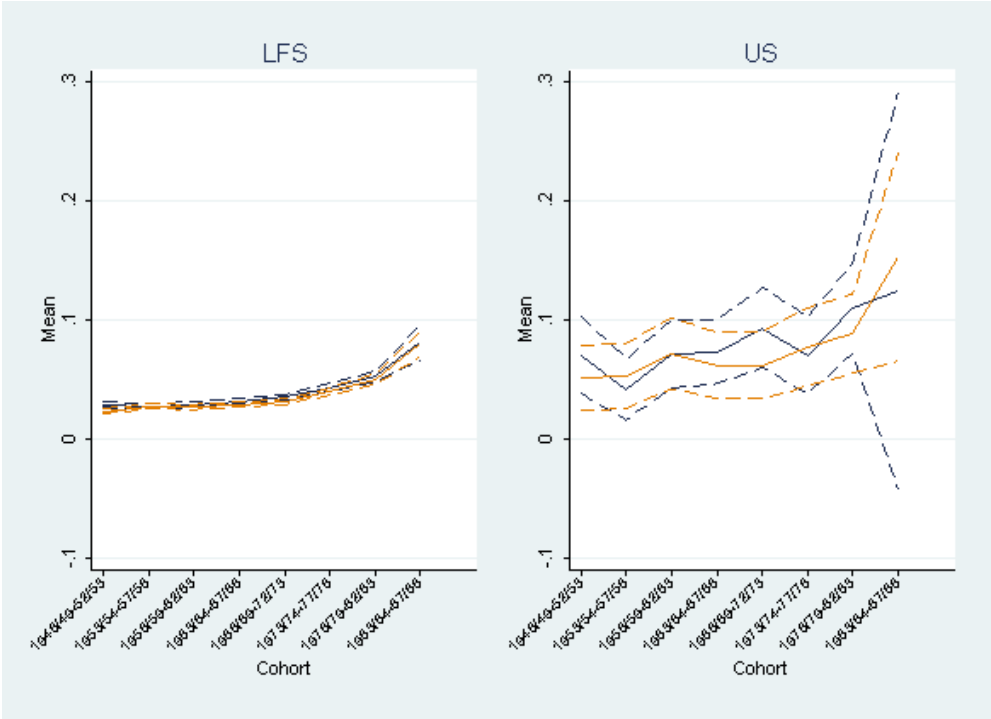
Note: the solid darker blue bar represents those that are relatively young in their academic cohort. The solid lighter yellow bar represents those that are relatively old. The dashed lines represent upper and lower 95% confidence intervals. Source: LFS and Understanding Society

Figure 3: The probability of acquiring a degree level qualification



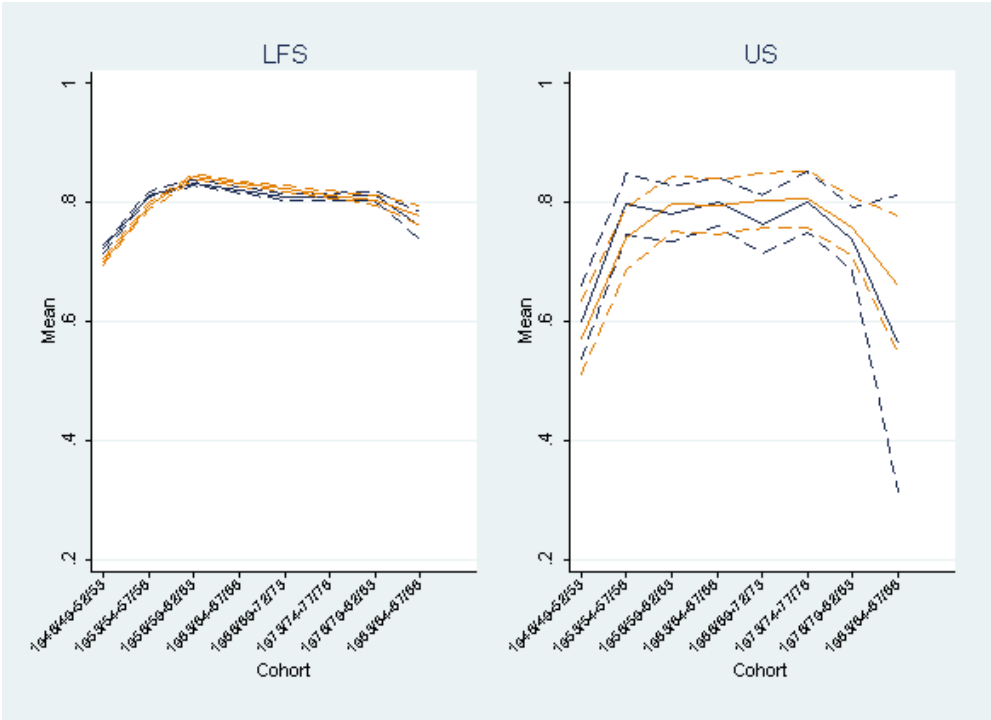
Note: see note to Figure 2.

Figure 4: The probability of being unemployed



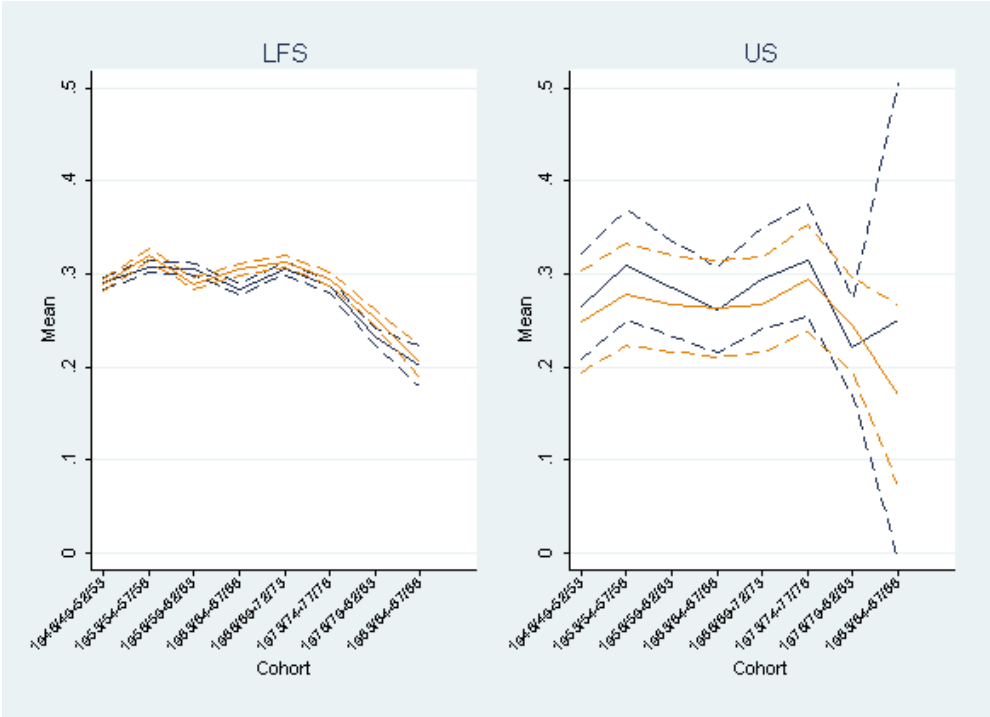
Note: see note to Figure 2.

Figure 5: The probability of being employed or self-employed



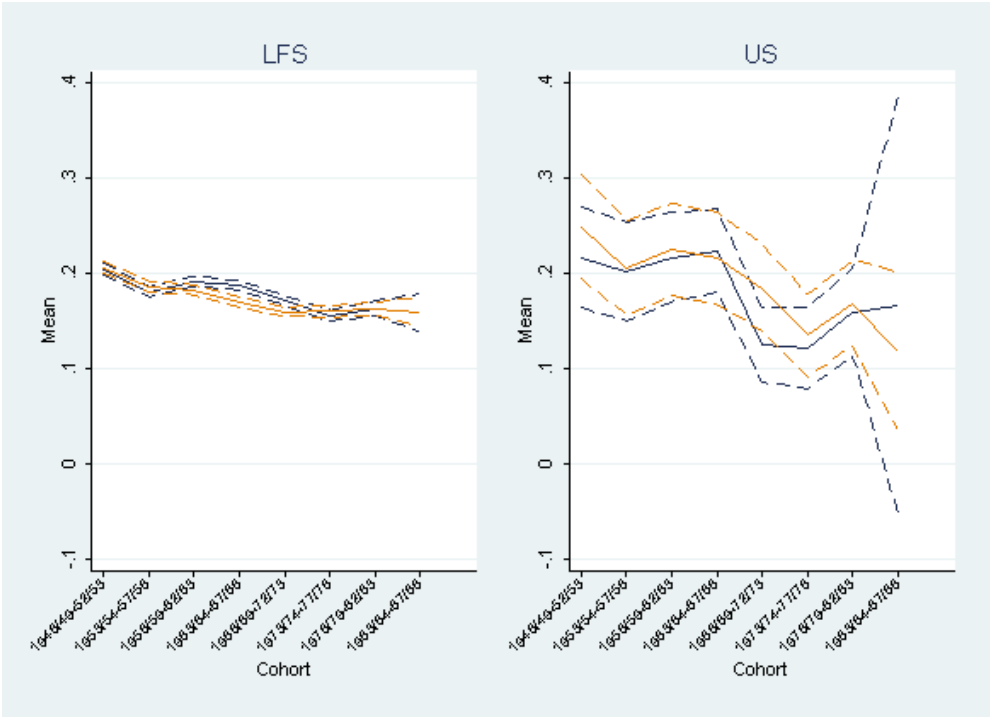
Note: see note to Figure 2.

Figure 6: The probability of having a professional occupation



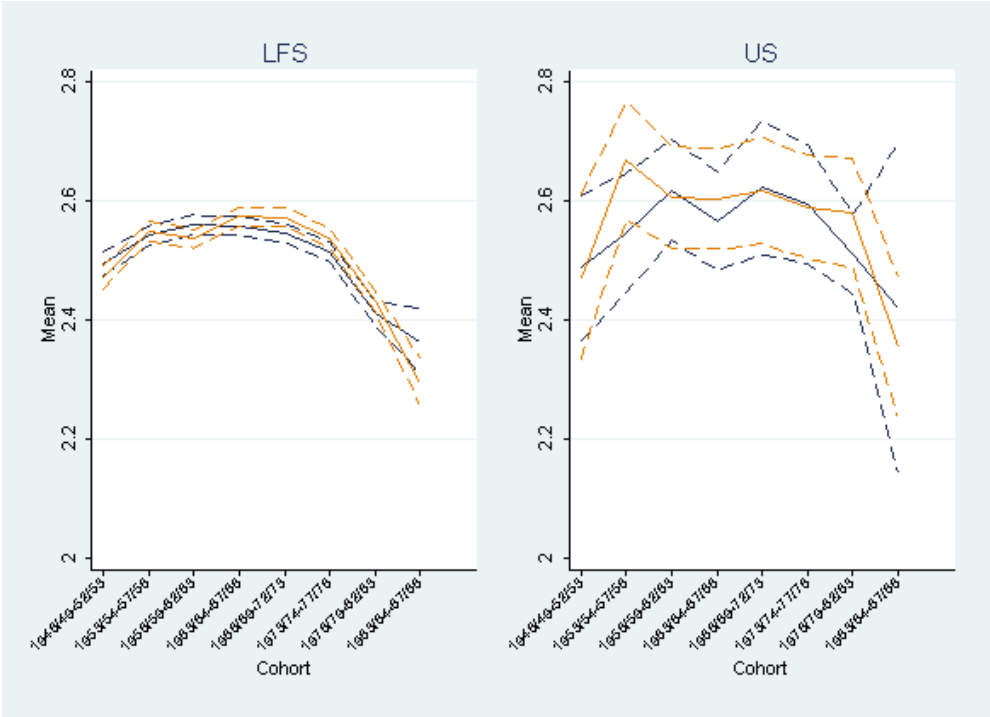
Note: see note to Figure 2.

Figure 7: The probability of having a routine occupation



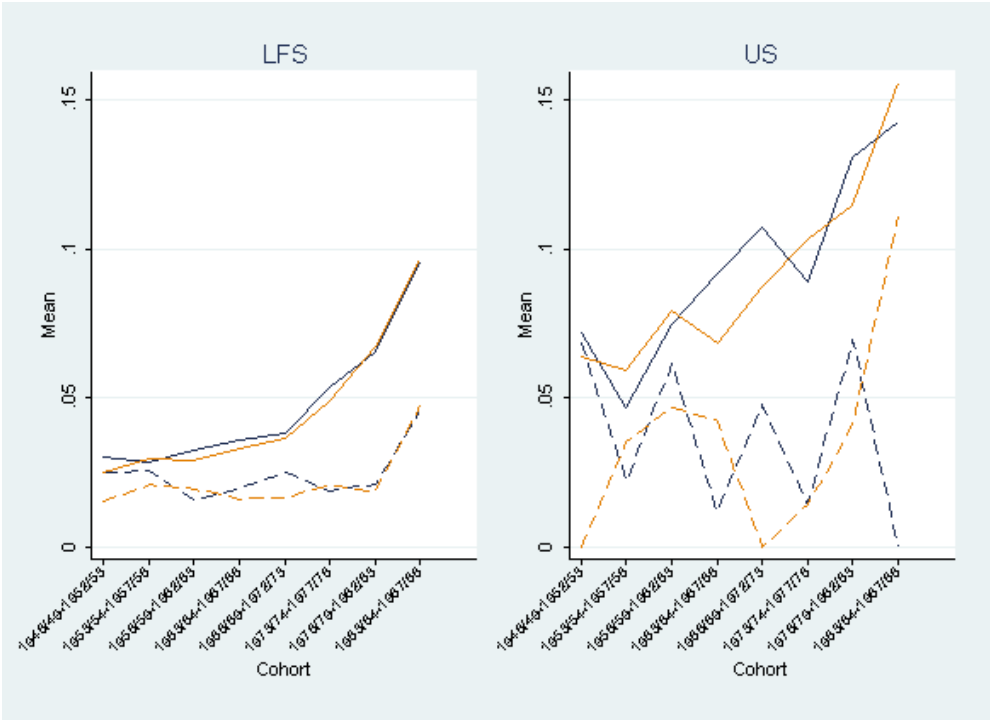
Note: see note to Figure 2.

Figure 8: Log gross pay per hour (in 2012 prices)



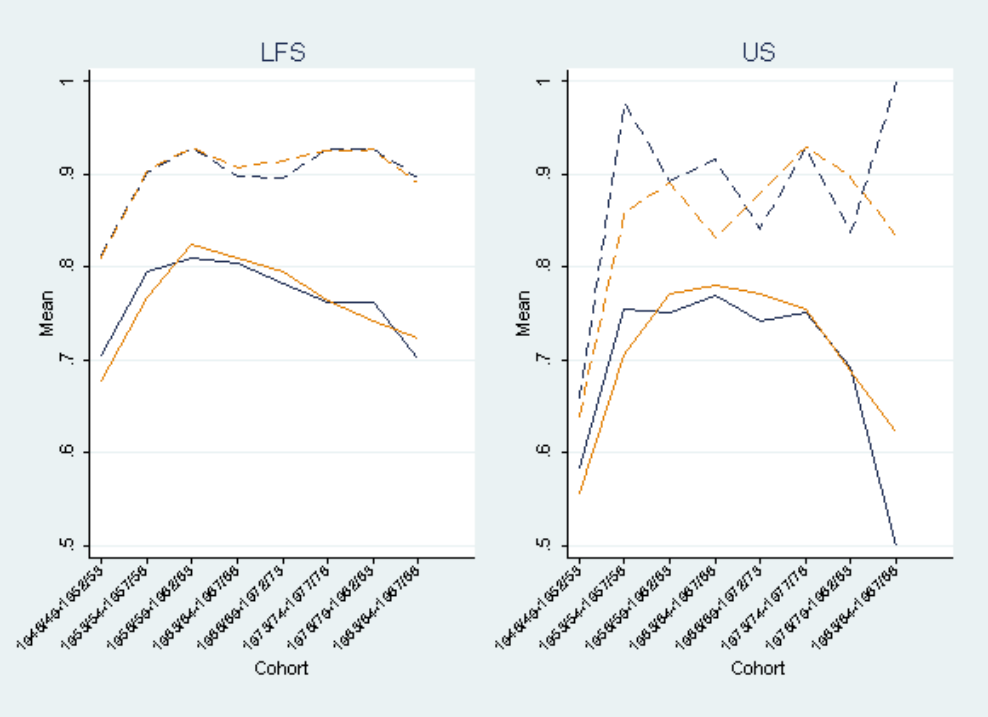
Note: see note to Figure 2.

Figure 9: The probability of being unemployed for those that do and don't achieve a degree level qualification



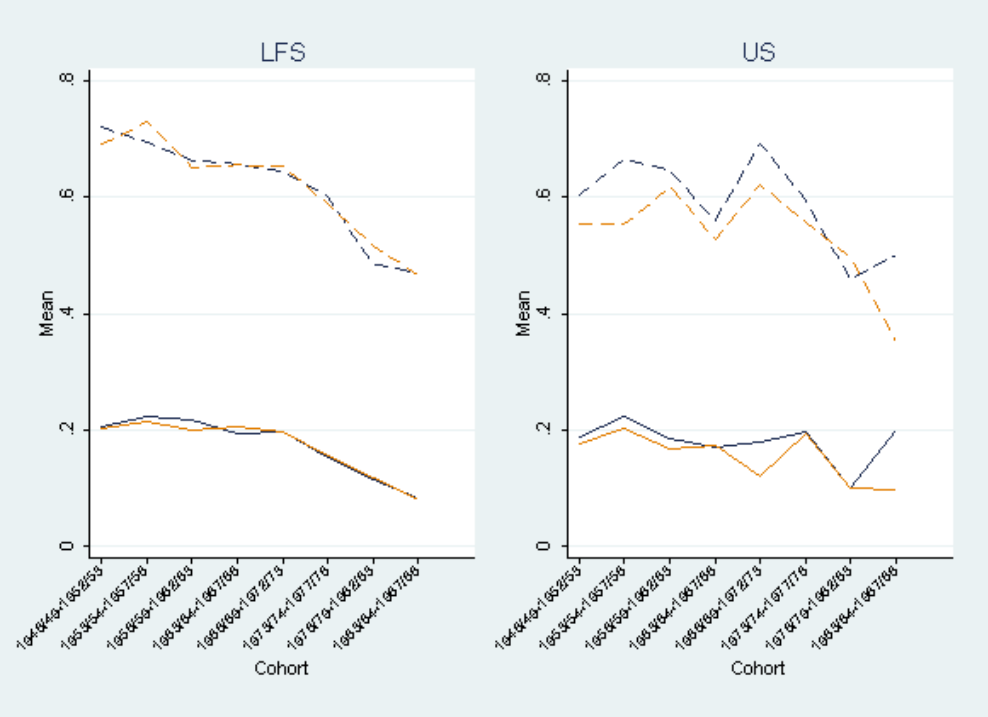
Note: the solid lines represent those that do not achieve a degree; the darker solid line represents those that are relatively young, the lighter solid line represents those that are relatively old. The dashed lines represent those that achieve a degree; the darker dashed line representing the relatively young. Source: LFS and Understanding Society

Figure 10: The probability of being employed or self-employed for those that do and don't achieve a degree level qualification



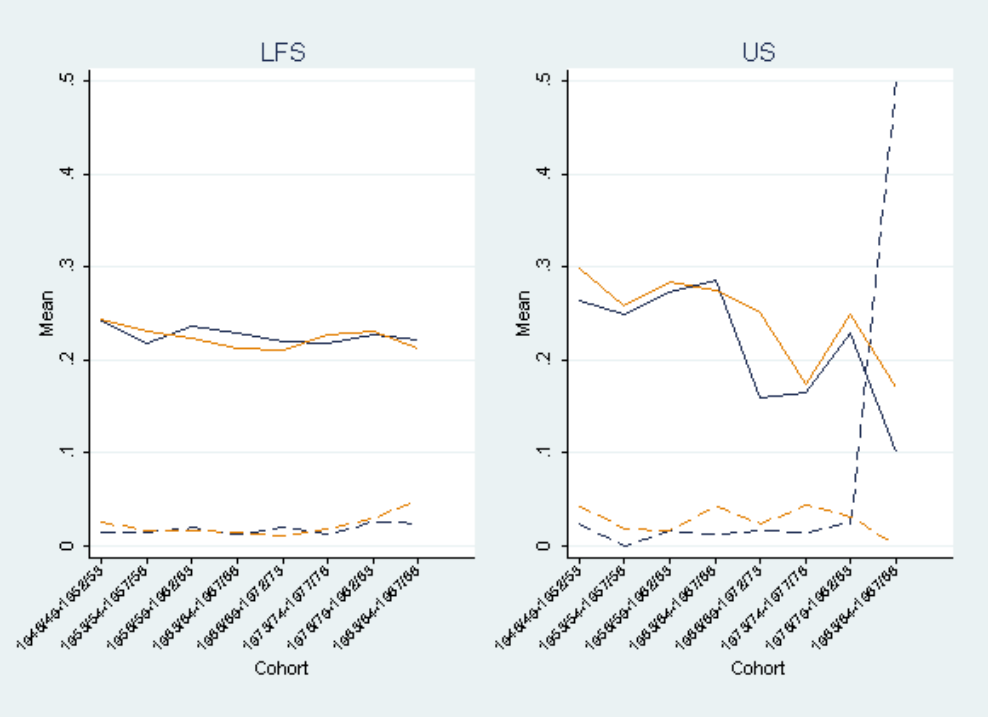
Note: see note to Figure 9.

Figure 11: The probability of having a professional occupation for those that do and don't achieve a degree level qualification



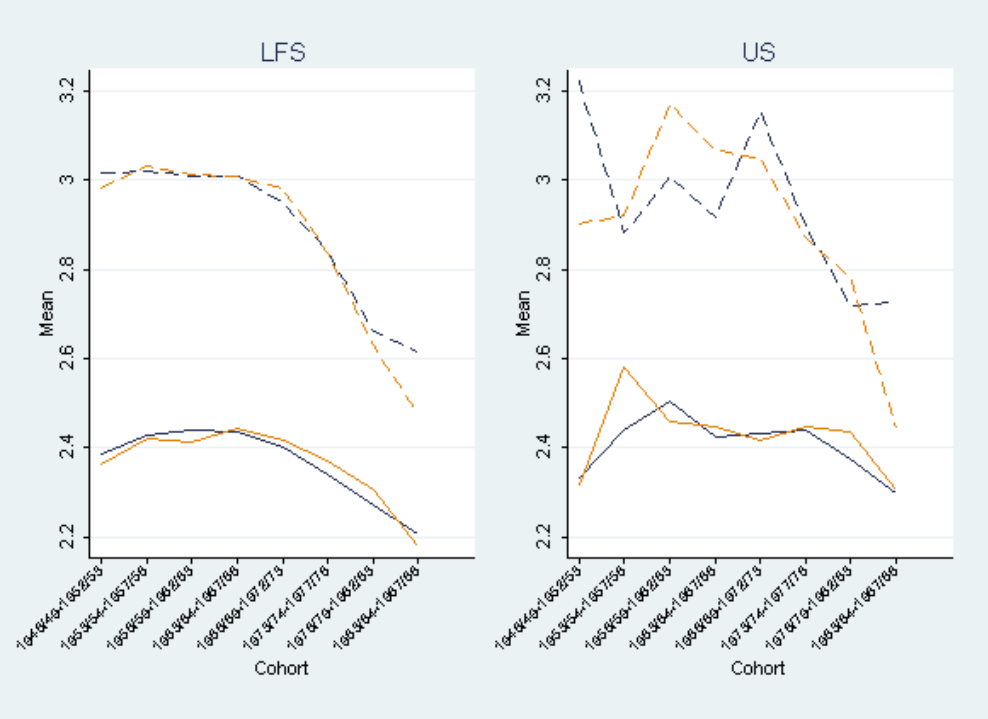
Note: see note to Figure 9.

Figure 12: The probability of having a routine occupation for those that do and don't achieve a degree level qualification



Note: see note to Figure 9.

Figure 13: Log gross pay per hour (in 2012 prices) for those that do and don't achieve a degree level qualification



Note: see note to Figure 9.

Table 2: Impact of relative age on adult outcomes: LFS

	Labour Force Survey (full sample)						
	No qualifications	Achieved a degree	Unemployed	Employed or self-employed	Professional occupation	Routine occupation	Gross hourly pay (in logs)
Relative age: 1	-0.003	-0.012***	0.004*	0.001	-0.006	0.003	-0.011
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 2	-0.004	-0.010**	0.002	-0.000	-0.008*	0.001	-0.013*
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 3	-0.003	-0.000	0.003	0.002	-0.001	-0.004	0.003
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 4	-0.009**	-0.004	0.002	0.003	0.004	-0.001	-0.003
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 5	-0.006*	-0.007*	0.005**	-0.001	-0.004	0.002	0.000
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 6	-0.007*	-0.001	0.004**	0.001	0.005	-0.003	0.009
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 7	-0.008**	-0.009*	0.003*	-0.002	-0.001	-0.001	0.001
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 8	0.002	-0.006	0.002	-0.002	-0.009*	0.002	-0.003
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 9	0.002	-0.006	0.003*	-0.003	-0.002	-0.001	-0.007
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 10	0.003	-0.003	0.003*	-0.002	-0.001	0.005	-0.001
	[0.003]	[0.004]	[0.002]	[0.003]	[0.004]	[0.003]	[0.006]
Relative age: 11	-0.001	-0.000	0.001	-0.003	-0.001	0.002	-0.002
	[0.003]	[0.003]	[0.001]	[0.003]	[0.004]	[0.003]	[0.006]
Academic cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	No	No	No	No	No	No	No
Sex	No	No	No	No	No	No	No
N	332346	332346	342747	342747	313511	313511	184931

Note: Sample includes all those born between 1948 and 1987 who were interviewed for the first time as part of the UK's Labour Force Survey between 2002 and 2011. Standard errors are clustered at the household level. * denotes p<0.05, ** denotes p<0.01 and *** denotes p<0.001.

Table 3: Impact of relative age on adult outcomes: Understanding Society

	Understanding Society (full sample)										
	No qualifications	Achieved a degree	Unemployed	Employed or self-employed	Professional occupation	Routine occupation	Gross hourly pay (in logs)	Household income (in logs)	Higher occupation than father	Excellent general health	Mental health (standard deviations)
Relative age: 1	-0.035**	-0.021	0.009	-0.001	0.013	-0.014	-0.030	-0.026	0.001	0.015	-0.007
	[0.012]	[0.014]	[0.008]	[0.014]	[0.015]	[0.013]	[0.025]	[0.026]	[0.016]	[0.013]	[0.035]
Relative age: 2	-0.001	-0.014	0.007	-0.022	0.012	-0.020	-0.007	0.002	-0.002	-0.006	-0.024
	[0.013]	[0.014]	[0.008]	[0.014]	[0.014]	[0.012]	[0.026]	[0.025]	[0.016]	[0.013]	[0.034]
Relative age: 3	-0.010	-0.015	0.004	-0.001	0.016	-0.018	-0.051*	-0.008	-0.005	-0.020	-0.018
	[0.012]	[0.014]	[0.008]	[0.013]	[0.014]	[0.012]	[0.026]	[0.025]	[0.015]	[0.012]	[0.034]
Relative age: 4	-0.016	0.011	-0.002	0.009	0.023	-0.011	-0.020	0.006	-0.006	0.003	-0.014
	[0.012]	[0.014]	[0.008]	[0.013]	[0.014]	[0.013]	[0.025]	[0.025]	[0.015]	[0.013]	[0.034]
Relative age: 5	-0.018	-0.008	-0.008	0.000	0.017	-0.000	-0.031	0.001	-0.004	-0.020	-0.011
	[0.012]	[0.014]	[0.008]	[0.013]	[0.014]	[0.013]	[0.027]	[0.026]	[0.015]	[0.012]	[0.034]
Relative age: 6	-0.008	-0.024	0.000	-0.010	0.005	-0.003	-0.062*	-0.012	-0.010	-0.009	-0.055
	[0.012]	[0.013]	[0.008]	[0.013]	[0.014]	[0.013]	[0.027]	[0.025]	[0.015]	[0.012]	[0.034]
Relative age: 7	-0.020	-0.016	-0.001	-0.003	0.000	-0.014	-0.029	-0.009	-0.005	-0.021	-0.047
	[0.012]	[0.014]	[0.008]	[0.014]	[0.014]	[0.013]	[0.026]	[0.026]	[0.016]	[0.012]	[0.034]
Relative age: 8	-0.007	-0.012	0.000	-0.001	0.024	-0.015	-0.040	0.007	-0.018	-0.011	-0.044
	[0.012]	[0.014]	[0.008]	[0.013]	[0.014]	[0.013]	[0.027]	[0.025]	[0.015]	[0.012]	[0.034]
Relative age: 9	-0.001	0.004	-0.003	0.011	0.008	-0.019	-0.002	0.022	-0.013	-0.002	-0.041
	[0.013]	[0.014]	[0.008]	[0.013]	[0.014]	[0.013]	[0.026]	[0.025]	[0.016]	[0.013]	[0.034]
Relative age: 10	-0.007	-0.014	-0.009	0.015	0.015	-0.000	-0.064*	-0.006	0.005	-0.023	0.026
	[0.013]	[0.014]	[0.008]	[0.013]	[0.015]	[0.013]	[0.026]	[0.026]	[0.016]	[0.013]	[0.034]
Relative age: 11	-0.009	0.004	-0.003	0.001	0.029*	-0.011	-0.013	0.011	-0.004	-0.006	0.005
	[0.012]	[0.014]	[0.008]	[0.013]	[0.014]	[0.013]	[0.027]	[0.026]	[0.016]	[0.013]	[0.034]
Academic cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	No	No	No	No	No	No	No	No	No	No	No
Sex	No	No	No	No	No	No	No	No	No	No	No
Further background characteristics	No	No	No	No	No	No	No	No	No	No	No
N	24265	24265	24368	24368	23291	23291	12812	24280	24372	24346	19668

Note: Sample includes all those born between 1948 and 1987 who were interviewed for the first time as part of Understanding Society between 2009 and 2011 (wave 1). Standard errors are clustered at the household level. * denotes p<0.05, ** denotes p<0.01 and *** denotes p<0.001.

Table 4: Impact of relative age on adult outcomes for those with/without a degree: LFS

	Labour Force Survey (full sample)									
	Unemployed		Employed or self-employed		Professional occupation		Routine occupation		Gross hourly pay (in logs)	
	No degree	Degree	No degree	Degree	No degree	Degree	No degree	Degree	No degree	Degree
Relative age: 1	0.004*	0.001	0.002	0.002	0.005	-0.013	0.001	-0.000	-0.002	-0.009
	[0.002]	[0.003]	[0.004]	[0.006]	[0.004]	[0.009]	[0.004]	[0.002]	[0.007]	[0.012]
Relative age: 2	0.002	0.002	0.003	-0.007	0.000	-0.014	-0.000	-0.002	-0.011	0.007
	[0.002]	[0.003]	[0.004]	[0.006]	[0.004]	[0.009]	[0.004]	[0.002]	[0.006]	[0.011]
Relative age: 3	0.003	0.001	0.003	-0.003	0.002	-0.009	-0.005	0.003	0.008	-0.012
	[0.002]	[0.002]	[0.004]	[0.005]	[0.004]	[0.009]	[0.004]	[0.002]	[0.007]	[0.011]
Relative age: 4	0.004*	-0.001	0.003	0.002	0.009*	-0.002	-0.003	0.004	-0.002	-0.001
	[0.002]	[0.002]	[0.004]	[0.005]	[0.004]	[0.009]	[0.004]	[0.003]	[0.006]	[0.011]
Relative age: 5	0.004*	0.004	-0.000	-0.002	0.001	-0.002	0.001	-0.001	0.005	0.003
	[0.002]	[0.003]	[0.004]	[0.005]	[0.004]	[0.009]	[0.004]	[0.002]	[0.006]	[0.011]
Relative age: 6	0.005**	-0.000	0.001	0.003	0.007	0.004	-0.004	0.000	0.006	0.010
	[0.002]	[0.002]	[0.004]	[0.005]	[0.004]	[0.009]	[0.004]	[0.002]	[0.006]	[0.011]
Relative age: 7	0.003	-0.001	-0.001	-0.004	0.005	-0.004	-0.003	0.002	0.005	0.002
	[0.002]	[0.003]	[0.004]	[0.006]	[0.004]	[0.009]	[0.004]	[0.003]	[0.007]	[0.012]
Relative age: 8	0.002	0.001	-0.000	-0.007	-0.005	-0.008	0.002	0.000	-0.003	0.008
	[0.002]	[0.003]	[0.004]	[0.006]	[0.004]	[0.009]	[0.004]	[0.002]	[0.006]	[0.011]
Relative age: 9	0.004*	-0.001	-0.003	-0.002	0.004	-0.008	-0.003	-0.001	-0.008	-0.001
	[0.002]	[0.003]	[0.004]	[0.005]	[0.004]	[0.009]	[0.004]	[0.002]	[0.007]	[0.012]
Relative age: 10	0.004*	0.000	-0.003	0.001	0.001	-0.002	0.007	0.001	-0.003	0.003
	[0.002]	[0.003]	[0.004]	[0.005]	[0.004]	[0.009]	[0.004]	[0.002]	[0.007]	[0.012]
Relative age: 11	-0.000	0.004	-0.002	-0.011*	0.002	-0.010	0.003	0.002	-0.001	0.001
	[0.002]	[0.003]	[0.004]	[0.006]	[0.004]	[0.009]	[0.004]	[0.002]	[0.007]	[0.011]
Academic cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	No	No	No	No	No	No	No	No	No	No
Sex	No	No	No	No	No	No	No	No	No	No
N	262149	70197	262149	70197	235514	68568	235514	68568	135531	45073

Note: Sample includes all those born between 1948 and 1987 who were interviewed for the first time as part of the UK's Labour Force Survey between 2002 and 2011. Standard errors are clustered at the household level. * denotes p<0.05, ** denotes p<0.01 and *** denotes p<0.001.

Table 5: Impact of relative age on adult outcomes for males and females: LFS

	Labour Force Survey (full sample)													
	No qualifications		Achieved a degree		Unemployed		Employed or self-employed		Professional occupation		Routine occupation		Gross hourly pay (in logs)	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
Relative age: 1	-0.003	-0.006	-0.011	-0.016**	0.002	0.006**	0.005	-0.009	-0.004	-0.012	-0.001	0.007	-0.011	-0.012
	[0.005]	[0.005]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 2	-0.002	-0.009*	-0.017**	-0.005	0.002	0.005*	-0.004	-0.004	-0.008	-0.009	-0.001	0.003	-0.006	-0.005
	[0.005]	[0.004]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 3	0.002	-0.005	-0.006	0.002	0.004	0.005*	-0.002	-0.005	-0.002	-0.002	-0.005	0.002	0.003	0.006
	[0.005]	[0.004]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.005]	[0.006]	[0.006]	[0.006]	[0.004]	[0.010]	[0.009]
Relative age: 4	-0.009*	-0.011*	-0.005	-0.007	0.002	0.004	0.004	-0.004	0.011	-0.006	-0.007	0.001	0.008	-0.009
	[0.004]	[0.004]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.006]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 5	-0.004	-0.007	-0.011	-0.006	0.002	0.009***	0.003	-0.013*	-0.001	-0.013*	-0.001	0.005	0.002	0.003
	[0.005]	[0.004]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 6	-0.004	-0.012**	0.002	-0.003	0.005	0.005*	0.002	-0.004	0.009	-0.000	-0.004	-0.003	0.007	0.013
	[0.004]	[0.004]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.005]	[0.006]	[0.006]	[0.006]	[0.004]	[0.010]	[0.009]
Relative age: 7	-0.005	-0.012**	-0.011	-0.005	0.002	0.006**	-0.002	-0.011	0.001	-0.004	-0.003	0.002	0.008	0.007
	[0.005]	[0.005]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 8	0.005	0.001	-0.010	-0.005	0.003	0.003	-0.000	-0.011*	-0.009	-0.014*	0.001	0.002	-0.006	0.006
	[0.005]	[0.005]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 9	0.003	-0.000	-0.007	-0.007	0.009***	0.001	-0.009	-0.004	0.003	-0.010	-0.008	0.003	-0.000	-0.009
	[0.005]	[0.005]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 10	0.003	0.003	-0.007	-0.001	0.006*	0.003	0.001	-0.009	0.002	-0.006	-0.000	0.011*	-0.003	-0.000
	[0.005]	[0.005]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Relative age: 11	-0.000	-0.004	-0.005	0.003	0.002	0.001	-0.000	-0.008	-0.000	-0.002	0.002	0.004	-0.001	0.004
	[0.005]	[0.005]	[0.006]	[0.005]	[0.003]	[0.002]	[0.005]	[0.006]	[0.007]	[0.006]	[0.006]	[0.005]	[0.010]	[0.009]
Academic cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of interview	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Sex	No	No	No	No	No	No	No	No	No	No	No	No	No	No
N	158405	173941	158405	173941	163750	178997	163750	178997	154953	158558	154953	158558	87224	97707

Note: Sample includes all those born between 1948 and 1987 who were interviewed for the first time as part of the UK's Labour Force Survey between 2002 and 2011. Standard errors are clustered at the household level. * denotes p<0.05, ** denotes p<0.01 and *** denotes p<0.001.

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Appendix Table 1: Education context for different cohorts of pupils

Academic cohort	School leaving age[a,b]	Exam system[c]	Higher education[d]
1934–35 to 1947–48	15	O	
1948–49 to 1956–57	15 (Easter/summer)	O/CSE	
1957–58 to 1959–60	16 (Easter/summer)	O/CSE	
1960–61 to 1970–71	16 (Easter/May)	O/CSE	
1971–72 to 1973–74	16 (Easter/May)	GCSE	
1974–75 to 1980–81	16 (Easter/May)	GCSE	University expansion (polytechnic)
1981–82 to 1996–97	16 (summer)	GCSE	University expansion (polytechnic)
1997–98[e]	17 (summer)	GCSE	University expansion (polytechnic)
1998–99 onwards[e]	18	GCSE	University expansion (polytechnic)

[a] The Education Act of 1944 raised the school leaving age to 15 (which was enforced from April 1947). This meant that all those born in or after April 1932 had to remain in school until age 15. From 1 September 1973, the compulsory school leaving age was 16. This meant that those born in or after September 1957 could not leave school before age 16.

[b] Between 1976 and 1997, those born between September and January could leave school at the end of the Easter term and those born February to August could leave on the last Monday in May. This applied to those born between September 1960 and August 1981. Prior to this (from September 1963), the two dates had been the 'end of the Easter term' and the 'end of the summer term' – affecting those born in or after September 1948 – (Education Act 1962); prior to this, a pupil was able to leave school on their fifteenth birthday (Education Act 1944). The current school leaving date ('summer') is the last Friday in June in the academic year in which the pupil turns 16. This has been in place from academic year 1997–98, affecting those born in or after September 1981 (Education Act 1996).

[c] GCSEs were first taken in academic year 1987–88, and so affected those born in or after September 1971. O Levels were first taken in academic year 1950–51, and so affected those born in or after September 1934. CSEs were first taken in academic year 1964–65, and so affected those born in or after September 1948.

[d] Polytechnics were able to become universities in 1992, following the Further and Higher Education Act 1992. This would have affected all those born around 1974.

[e] For these cohorts of pupils, 'participation leaving age' should be read in place of 'school leaving age', as young people may be employed full-time with part-time education or training, or take an apprenticeship (Education and Skills Act 2008).

Source: Education Act 1944, Education Act 1962, Further and Higher Education Act 1992, Education Act 1996 and Education and Skills Act 2008.