The Birthdate Effect: Does It Persist Beyond Education?

EC331: Research in Applied Economics
Tutor: Dr Mingli Chen

April 24, 2019

1605585

Department of Economics,
University of Warwick,
Coventry,
UK

Word Count: 4,995

Abstract

Substantial evidence has proven the existence of the birthdate effect during schooling years, with older students in a given academic cohort having, on average, higher educational attainment. This paper sheds light on the scarce topic of its existence post-education and into early-stage career earnings. Using the new Next Steps cross-sectional data set, we find evidence to suggest the birthdate effect is present in female earnings at age 25, with no evidence to support such a claim for males.

Acknowledgements

I would like to personally thank my tutor Dr Mingli Chen, who has provided valuable feedback on this dissertation throughout the year, and has helped shape the course of this piece of research.

I would also like to thank my peers Robert Brown and Finn Meinecke for their valuable pointers regarding the econometrics used in this report, and Ahmed Sorour and Karina Chopra for helping to support me during the project.

Contents

1 Introduction **3**
1.1 Research Question . . . 3
1.2 Motivation . . . 4
1.3 Structure . . . 5

2 Literature Review **6**
2.1 Relative Age Approach . . . 6
2.2 School Starting Age Method . . . 7
2.3 Contribution to the Literature . . . 10

3 Data **11**
3.1 Context: The UK Education System . . . 11
3.2 The Data Set . . . 11

4 Methodology **14**
4.1 The Core Variables . . . 15
4.2 Controls and Robustness tests . . . 17

5 Results **20**
5.1 Robustness Checks . . . 22
5.2 Limitations . . . 23

6 Conclusion **25**

Appendices **29**

1 Introduction

The phenomenon of the birthdate effect is evident when participation in high outcome categories is higher (lower) amongst those born early (late) in the selection period than would be expected given a uniform distribution of births. The birthdate effect is prominent and pervasive in the UK education system due to the presence of a cut-off date between academic year groups. This cut-off date produces an educational attainment inequality between those born earlier in the academic year and those born later; with the relatively older peers, on average, performing to higher academic standards (Sykes et al., 2016). This educational inequality is most often attributed to the relative-age difference between student peers (Pigeon, 1965; Crawford et al., 2007). This age-gap yields a disparity in mental maturity that births educational differences, which then persist throughout their time in education. However, the full causal nature of the educational gap is still debated (Sykes et al., 2016; Crawford et al., 2007). The birthdate effect is most apparent in early academic years due to a larger proportional difference in ages between peers, however, there is substantial evidence that the effect continues to persist into GCSE's, A levels, and higher education, albeit in a diminishing magnitude (Russel and Startup, 1986; Sharp, 1995; Massey et al., 1996; Alton and Massey, 1998; HEFCE, 2005; Crawford et al., 2007, 2013b). If this age-related performance gap continues to disappear as individuals age, then so does its importance, however, if it persists into adulthood, then it may have important implications on a persons' adult outcomes, most notably their earnings.

1.1 Research Question

The main research question this report hopes to address is if there is evidence that suggests the birthdate effect persists beyond education and impacts early adult outcomes, specifically an individuals' earnings at age 25. The birthdate effects' existence beyond education seems plausible; the previously noted correlation between educational attainment and relative age is dominant in the literature, and the acknowledged relationship between educational attainment and earnings (Psacharopoulos and Patrinos, 2004, 1994) would suggest it ought to be present. Therefore, if true, we would expect to see that those born earlier in the academic year, who have, on average, benefited from greater schooling, will have markedly higher income levels than their younger, less educated peers. Furthermore, this report hopes to promote discussion on the topic of the birthdate effect and advocates for future analysis into the exact nature of the UK birthdate effect.

1.2 Motivation

The motivation for this report follows from that of the educational literature. The arbitrary factor of the selection period, and its influence on an individuals' relative age, should not impact the life outcomes of a child. This stems from the original philosophical basis of the educational system, which is to give each child an equal opportunity to succeed in life, yet, if age effects generated from the education system's cut-off date are unfairly benefiting older students, then it is failing this cause.

Secondly, from an economic standpoint, it is because of the high likelihood that an individuals' well-being is closely related to an individuals' earnings that it is important to identify if the birthdate effect continues to persist. This income well-being effect is most notable at lower incomes (Blanchflower and Oswald, 2004), which predominantly occur during the early stage of an individuals' career. If the birthdate effect is persistent, and noticeable differences in income levels occur depending on the relative age of an individual, then it is likely to alter the choices, experiences, and achievements that the child will have in adulthood, and possibly be detrimental to their well-being.

1.3 Structure

This paper will follow the subsequent structure: Section 2 will set out the findings in the literature regarding the relationship between the relative age of an individual during schooling and their earnings. Section 3 will describe the UK education system, and features of the data, while Section 4 will outline the models and variables used in the reports analysis. Section 5 will present the results from our models, which strongly indicate the presence of a female birthdate effect at age 25, and finally, Section 6 will present concluding remarks.

2 Literature Review

As mentioned earlier, there is robust evidence across the literature to suggest that there is an educational advantage in being the relatively oldest in the academic year (Sykes et al., 2016). This will likely impact adult outcomes as younger, less educated students will likely underperform compared to their older, more educated counterparts. However, the literature on the longer-term earnings effects of relative schooling age is less extensive and less conclusive than its educational counterpart, with UK-based evidence even more scarce. The existing literature on the topic can be split into two varying methods, one favouring a relative-age within-year approach, and the other using a school starting age-based proxy.

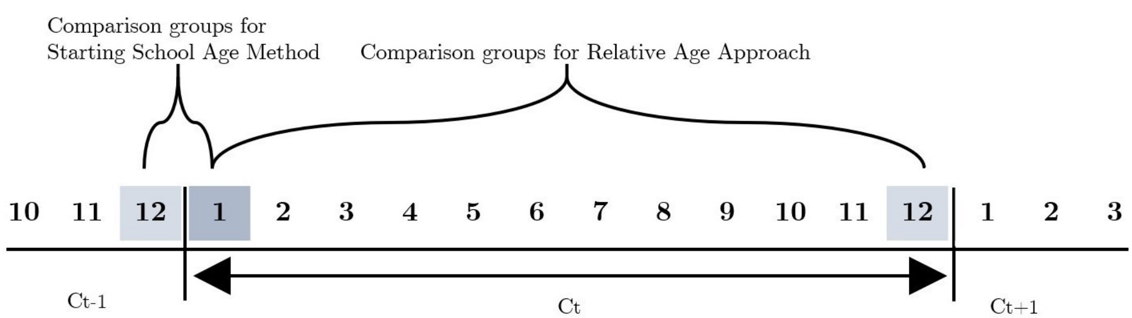
2.1 Relative Age Approach

This approach uses basic OLS regressions with birth month dummy variables expressed as the relative difference in months compared to the academic cut-off date. The cohort members move in-step throughout the education system and are compared together at a set point in time (See Figure 1 for clarification).

The only UK-focused research completed on the topic was by Crawford et al. (2013a), which used generational Labour Force Survey and Understanding Society Survey panel data sets to conclude that relative age had no statistically significant effect on lifetime labour market outcomes. They achieve this while simultaneously re-confirming the widely accepted conclusions that the relatively older in an academic cohort are more likely to have a degree, and the presence of an hourly wage premium when possessing said degree. These conclusions ought to support the existence of a significant long-run earnings difference. Interestingly, when examining individual generations, the relatively younger peers in younger cohorts (currently in their late 20s or early 30s) displayed slightly higher hourly earnings when compared to their relatively older peers. This conclusion is opposite to the expected hypothesis outlined in the introduction. Crawford et al. (2013a) put this down to the short-term positive earnings effect of having more initial labour force experience, as younger peers, on average, entered the working world earlier than their more educated, older peers, as they are less likely to have entered non-compulsory further education.

However, as noted by Crawford et al. (2013a), by using sample data and not population data, the reliability of their results is affected. This may explain why the report conflicts with evidence from other countries. Solli (2012) used the extensive Norwegian registrar population data base and found that, on average, those born in January (post-cut-off) have earnings approximately 4% higher at age 30 when compared to their younger, December-born peers, with the effect more clear in men than women. Likewise, Kawaguchi (2011) found similar results to Solli (2012) for Japanese men aged between 30-34. He found an earnings difference of 3.9% between those born in April-June (Post-cut-off) and their younger January-March born peers.

Figure 1: Method Comparison



2.2 School Starting Age Method

Conversely, most research on the topic has primarily focused on the effect school starting age has on earnings. School starting age is inherently driven by the relationship between a school systems' cut-off date and the birth month of a student and thus can be used to measure relative age differences, irrespective of academic year group. Bedard and Dhuey (2012) exploited the varying US school admission policies in the late 20th Century and found that starting school one year later (hence relatively older to the control group) had a small positive effect on average lifetime earnings for men only. They state that a one-month delay in school starting age produces a 0.6% increase, on average, in lifetime hourly wages. Additionally, these international-based papers highlight a potential gender asymmetry in relation to the birthdate effect, with it being more pronounced in men than women.

Moreover, other papers often use the discontinuity around the cut-off date between academic years to compare the potential impact of starting school at a different age when of close biological age (See Figure 1 for clarification). However, this method allows for more drastic variations in labour market experience as you are comparing individuals in different academic year groups. Using this method, Fredriksson and Öckert (2005) in Sweden, found a slight positive effect on the average lifetime earnings for individuals who started school a year later, compared to their younger-starting peers. Both Bedard and Dhuey (2012) and Fredriksson and Öckert (2005)'s findings support our initial thesis, and suggest that being relatively older when starting school results in significant wage gains. However, given that relatively older students enter the labour workforce later than their younger starting peers; Fredriksson and Öckert (2005) found that they experience a negative wage effect at the early stage of their career, and cumulatively earn less over their lifetimes than their earlier school-starting peers (as they retire at the same biological age). Yet, this study contains only one year of earnings data taken in 2000, and is therefore unable to distinguish between cohort and age specific effects, as different cohorts may experience varying lifetime earnings, so it's results may be biased.

On the other hand, Solli (2012); Larsen and Solli (2012) also incorporate this regression discontinuity methodology and found that there is no statistically significant difference in earnings at age 30 between those born before or after the academic cut-off date. They suggest that the additional year in the labour force - due to an earlier school starting age and, on average, an earlier entrance into the labour force - produces a positive impact on earnings and adequately counterbalances the previously noted negative effect of being the relatively youngest within an academic year. This is in-line with the cumulative earnings and recent cohort findings by Fredriksson and Öckert (2005) and the conclusive reasoning given by Crawford et al. (2013a). The Solli (2012); Larsen and Solli (2012) findings is further supported by more detailed work by Black et al. (2011) using the same Norwegian population data set. These results are more reliable than Fredriksson and Öckert (2005) as they cover a ten-year period (ages 24-34); which allows for a more detailed and distinguishable year-by-year analysis. Black et al. (2011) found a short-run positive earnings effect for earlier school-starting students that disappears at age 30. This further supports the hypothesis of Solli (2012), Larsen and Solli (2012), and Crawford et al. (2013a), that an earlier entrance into the labour market effectively counteracts the negative educational effect on earnings of the relatively youngest. The Solli (2012) approach is the optimal method when analysing the birthdate effect. However, it requires a population data set with multiple years of earnings data, that the UK lacks, therefore it is not possible to implement such a strategy in this report.

To summarise, the literature regarding the birthdate effect beyond education is inconclusive. Some papers imply a positive relationship between relative age and earnings, both at an early-career stage and throughout an individual's lifetime. Whereas most seem to favour the conclusion that there exists a short-term negative relationship between relative-age and early-career earnings - caused by early labour force experience of the relatively young - who's relationship becomes inconsequential beyond age 30. However, regardless of its nature, most papers point to the existence of the birthdate effect, at some point, beyond education.

2.3 Contribution to the Literature

This paper hopes to shed light on the issues noted in the literature, specifically aiming to corroborate international findings within the context of the UK. It will delve into a topic that is rarely highlighted by thinkers and provide another body of research to the existing literature on the birthdate effect. By utilising a new data set, it will present the first UK-based research supporting the suggestion that the birthdate effect does persist beyond education, atleast for females. Furthermore, it will provide important insights into understanding the long-term consequences of a child's month birth on adult outcomes.

3 Data

3.1 Context: The UK Education System

In the UK, the education cut-off date is heavily dependent on the constituent country the child is educated in, with nations having varying selection periods. In England and Wales, the academic year begins September 1st and ends on August 31st. This means those born in September are the relatively oldest in the year group, with August births the relatively youngest. Whereas in Northern Ireland and Scotland, the oldest in the academic year group are born in July and March respectively. This is because education in the UK is a devolved matter, with country governments having responsibility over their own nations education system. In all UK constituent countries, a child must be in compulsory schooling in the academic year that they turn five years old, and they must remain in schooling until the year that they turn 16.

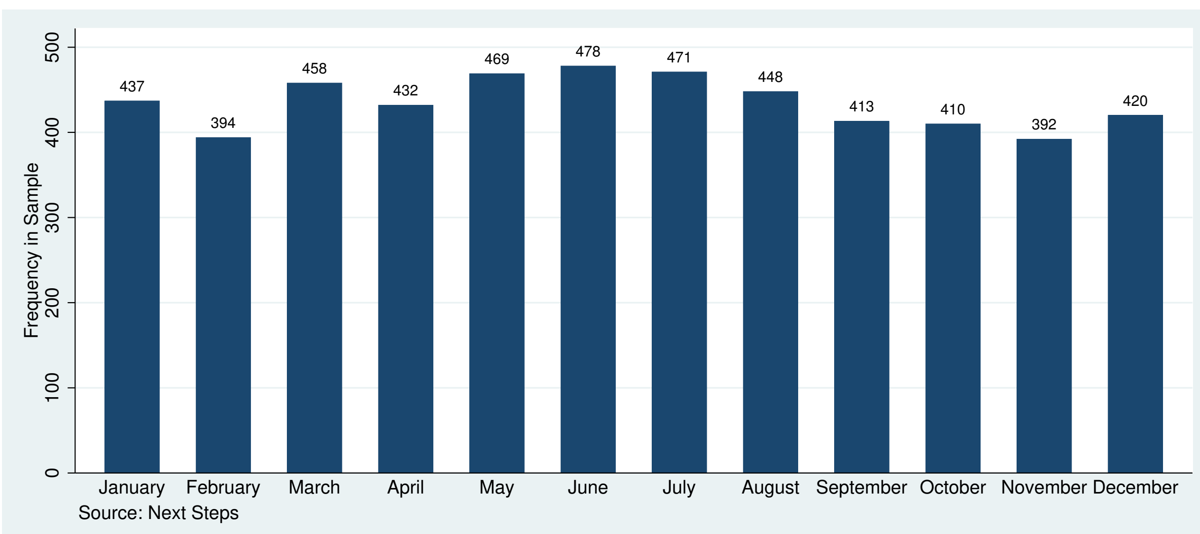
3.2 The Data Set

The data used in this report is from the Next Steps data set, previously known as the Longitudinal Study of Young People in England. It is a wave-based panel data set that surveys 16,122 young people in England, born between September 1st, 1989 and August 31st, 1990. This survey places all members within the same academic cohort and focuses specifically on the nation of England. Given the devolved nature of the education system in the UK, analysis of the UK would require data from each individual nation, to control for individual nation differences, yet, given England's large relative size in the UK, it will still allow for an accurate reading of the entire country.

The data set has been used in previous literature focusing on the birthdate effect during education (Crawford et al., 2013b). It was discontinued in 2011 but has been revived with a further 8th interview period that includes earnings data. It was completed between August 2015 and September 2016, with original cohort members aged either 25 or 26. This 8th wave allows for up to 10 years of labour force experience to have taken place and will therefore allow cross-sectional analysis of the birthdate effect on early-stage career earnings. This report is the first to use this data set in the context of the birthdate effect post education.

However, the data set suffers from attrition, as only 7,707 data points of the original 16,122 are re-surveyed in 2015/16. This level of attrition may bias coefficients downwards and lead to over-representation of the high-earning relatively young. This is because it is noted in the literature that those that are more likely to be attritors tend to have lower levels of earnings and education (Fitzgerald et al., 1998).

Figure 2: Distribution of birth month



The distribution of data is relatively equal across all birth months, this can be seen in Figure 2 above, with slightly higher representation evident in the warmer months of June and July, and slightly lower representation in the colder months of November and February.

However, these slight differences are not large enough to cause any data analysis issues.

4 Methodology

This report uses the relative age approach mentioned in the literature, as having very low variation in labour market experience is necessary when evaluating very early-stage career earnings, which this approach is best suited for. Our analysis utilises an OLS regression model, like that used by Crawford et al. (2013a); Solli (2012), as it allows the lowest sum of squared errors and hence the best fit for our analysis. The basic log linear regression used in this analysis is as follows:

$$lhourpay_i = \alpha + \sum_{n=1}^{11} \beta_n RelAge_i + \sum_{j=12}^{24} \beta_j RelInt_i + \beta_{25} Female_i + \epsilon_i \tag{1}$$

Where *lhourpay* is defined as the logged gross hourly earnings of an individual. *RelAge* is defined as a series of 11 birth month dummies that are unity if the individual is born in the relative birth month, and zero if not (control August), and *RelInt* is a series of 13 control dummies for the relative month the interview took place in (control August 2015) and finally *Female* which is a dummy variable for if the individual is female or not. The summary statistics for core variables are below in Table 1, and all variable summary statistics can be found in Appendix 1.

Table 1: Core Summary statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Average hours worked per week	5,222	38.48	10.32	0.100	168
Average gross weekly pay	5,222	671.8	13,879	0.0800	1,000,000
Average gross hourly pay	5,222	25.15	818.3	0.006	58,824
Logged average gross hourly pay	5,222	2.432	0.476	0.00623	10.98
Relative Age	5,222	6.675	3.449	1	12

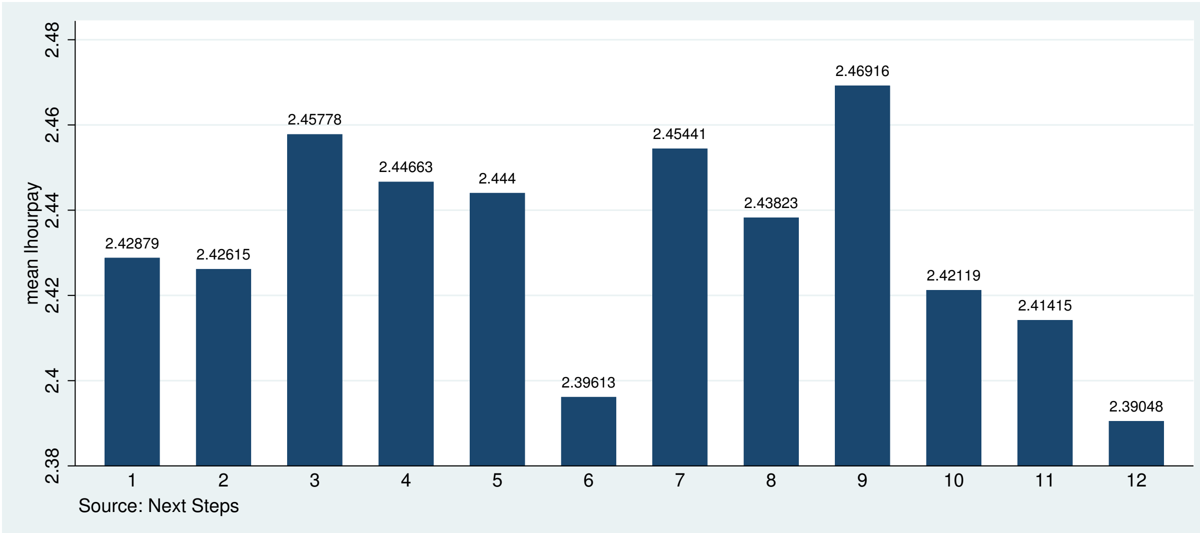
By using this method, we are inherently testing the birthdate effect on earnings, as we are allowing the educational benefits from a relatively early birth to act through the proxy variable of relative age. This means the endogenous effect of education is factored into the relative age variable. We make the assumption that this is the only reason why we may see significant results on earnings for different birth months, as there is nothing inherent about a child's birth month that implies higher or lower future earnings.

The approach outlined above hopes to explain the interesting features seen in the preliminary analysis of the data. Figure 3 below shows a bar chart of mean logged gross hourly pay against the relative age of an individual, a rough trend can be seen. With those who have a higher relative age (the relatively youngest) having lower gross hourly wages, on average, than their relatively older peers.

4.1 The Core Variables

The dependent variable is constructed by dividing the self-reported gross pay per week with the self-reported usual hours worked per week to produce a gross hourly pay

Figure 3: Mean logged gross hourly pay against relative age

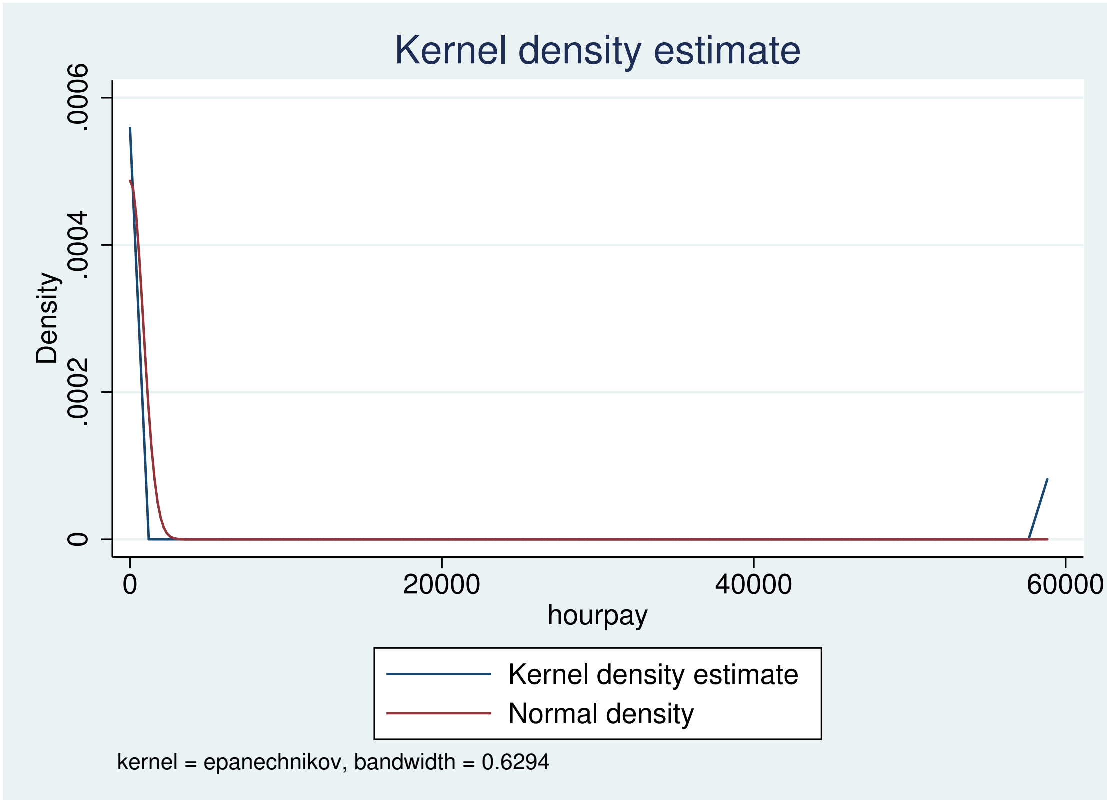


variable. The self-reported nature of these inputs into the dependent variable does not pose a substantial threat to the unbiasedness and consistency of the OLS estimates, as we expect the errors to have a mean of zero and no covariance to be present between individual errors.

Figure 4 below shows the skewness of gross hourly pay against a normal distribution, and provides the basis for the use of logged gross hourly earnings variable to combat the heavy skewness seen in the earnings data. Furthermore, to avoid losing observations that have a gross hourly wage below £1 when transformed, this report uses the widely accepted tool of a transformed dependent variable of the form log(y+1). This should not affect our results, as it maintains consistent variance while still allowing the dependent variable to approach a normal distribution.

The explanatory variable of interest is the relative age of cohort members, which is represented as a series of birth month dummy variables. This allows the assessment of individual birth month effects to be compared. The dummy variables are coded 1 to 12 depending on the relative position of the month in the academic year, with 1 being those born in September in 1989, the relatively oldest, and 12 being those born in August in 1990,

Figure 4: Skewness of gross hourly pay



the relatively youngest. Further analysis will combine birth months based on yearly seasons, this will allow the verification of the monthly findings, and allow direct comparison to various pieces of the literature that only include seasonable birth data, such as Kawaguchi (2011).

4.2 Controls and Robustness tests

One of the control variables used in our analysis is the relative month the interview took place in, this ranges from 1 to 14, with 1 corresponding to the earliest interviews in August 2015, and 14 representing those interviewed in September 2016. It is expected that those that are interviewed later, and hence are older when the survey is completed, will have higher earnings on average than if they are completed the survey earlier. This stems from the accepted assumption that earnings continue to rise with age during the early stages of an individual's career. The use of interview period as a control is universal in the literature (Black et al., 2011; Crawford et al., 2013a; Fredriksson and Öckert, 2005; Kawaguchi, 2011; Solli, 2012; Larsen and Solli, 2012)

This report also uses other controls as robustness checks, such as the inclusion of dummies for an individual's gender. It also uses further controls such as interactive dummies between a person's gender and relative age. The use of gender as a control is because it is not expected to heavily influence an individual's relative age but is expected to affect an individual's earnings. The first will highlight the gender earnings difference, while the second will allow a deep analysis into the male and female specific birthdate effect. The use of this background characteristic is common in the literature, to allow a more accurate reading of the birthdate effect (Solli, 2012; Kawaguchi, 2011; Crawford et al., 2013a).

We further use heteroscedasticity-constant standard errors in our regression to alleviate the issue of potential heteroscedasticity in our dependent error terms, this is due to the high likelihood of correlated error terms as our data set contains observations from individuals in the same schools and areas. These sub-populations may have different variability when compared to others, which is evidenced in Appendix 2 using a Breusch-Pagan test on equation (2), where we reject the null hypothesis of constant variance.

Table 2: Results

EQUATION VARIABLES	(1) Both	(2) Male	(2) Female	(3) Male	(3) Female
Relative Age: 1	0.0323	-0.0369	0.0961**		
	(0.0295)	(0.0408)	(0.0426)		
Relative Age: 2	0.0346	0.0159	0.0493		
	(0.0305)	(0.0470)	(0.0401)		
Relative Age: 3	0.0664**	0.0248	0.0992**		
	(0.0296)	(0.0438)	(0.0403)		
Relative Age: 4	0.0538*	0.00976	0.0888**		
	(0.0315)	(0.0432)	(0.0448)		
Relative Age: 5	0.0537	0.0131	0.0853*		
	(0.0340)	(0.0523)	(0.0448)		
Relative Age: 6	0.00455	0.0329	-0.0165		
	(0.0285)	(0.0430)	(0.0381)		
Relative Age: 7	0.0640*	0.0220	0.0970**		
	(0.0328)	(0.0495)	(0.0438)		
Relative Age: 8	0.0448	0.0179	0.0663		
	(0.0315)	(0.0474)	(0.0418)		
Relative Age: 9	0.0800**	0.0722	0.0868**		
	(0.0373)	(0.0646)	(0.0439)		
Relative Age: 10	0.0275	0.0322	0.0205		
	(0.0295)	(0.0458)	(0.0381)		
Relative Age: 11	0.0229	0.00518	0.0371		
	(0.0315)	(0.0533)	(0.0383)		
Autumn				-0.0142	0.0612***
				(0.0260)	(0.0224)
Winter				0.00461	0.0345
				(0.0277)	(0.0236)
Spring				0.0238	0.0643***
				(0.0321)	(0.0238)
Observations	5,222	5,222	5,222	5,222	5,222
R-squared	0.012	0.015	0.015	0.012	0.012
Interview Month Dummies	Yes	Yes	Yes	Yes	Yes
Interactive Gender Dummies	No	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

5 Results

According to the very basic model specified in (1), with results shown in Table 2, we find that the birthdate effect may be apparent beyond age 25, as two months have statistical significance at the 5% level when compared with August (Relative Age=12), these months are November (Relative Age=3) and May (Relative Age=9). Those born in November earn, on average, 6.9% more than their August-born peers, while for May-born individuals this figure is higher at 8.3%. These two months show inconclusive evidence of a potential birthdate effect, as they are not clustered at the cut-off date of the academic selection period as hypothesised, but do seem to show that older students may have an advantage over their August-born peers.

If we include 11 relative age and gender interactive dummies, as shown in equation (2), then we can begin to analyse gender specific birthdate effects, and discover if this is why we find inconclusive results. Below, *FemaleRelAge* is defined as the Female variable multiplied by RelAge. The results of these regressions can be found in Table 2.

$$lhourpay_i = \alpha + \sum_{n=1}^{11} \beta_n RelAge_i + \sum_{j=12}^{24} \beta_j RelInt_i + \beta_{25} Female_i + \sum_{k=26}^{36} \beta_k FemaleRelAge_i + \epsilon_i \tag{2}$$

If we look at men specifically, we can see that regardless of birth month, there is no significant impact on earnings between being born in an earlier month when compared to being born in August. Therefore, the results suggest that being relatively older in the academic year group has no bearing on your earnings at age 25 for men.

However, when we look at women specifically, we can begin to see the potential presence of the birthdate effect. Women who are born in 5 of the months in the academic year have statistically significant hourly wage differences at the 5% level compared to those born in August. The statistically significant months are predominantly at the beginning of the academic year, with 3 of the 5 being in the first 4 months and November being the most significant. These statistically significant months have wage differences between 9.1% and 10.4% when compared to their relatively younger female peers born in August.

If we take our results at face-value, we have no evidence to support the conclusion that the birthdate effect is apparent for English men at age 25, as we see no noticeable differences between male earnings. However, the birthdate effect may be present for English women at age 25, as a large portion of months have significant earnings differences when compared to the relatively youngest.

This noted gender effect is very much the opposite when compared to the current literature. All three papers that mentioned gender previously find the opposite gender effect, Bedard and Dhuey (2012)'s paper, finds that starting school a month later, hence one month relatively older, results in a 0.6% increase in adult earnings, for men only, with no discernible differences for women. Likewise, when looking at relative age effects in Japan and Norway, Kawaguchi (2011) and Solli (2012) found that only males experience the earnings boost brought about by the birthdate effect at ages 30-34. Our results are very interesting in this regard, as it shows that relatively younger men in England are valued the same in the labour market when compared to their older counterparts at age 25, whereas relatively younger women are not.

A possible explanation for this difference to the literature may stem from the age that the earnings data is being analysed at - which is earlier than that of the literature - rather than individual country differences. Previous educational literature has conclusively stated that women are more likely to be highly educated than their male peers, regardless of relative age (HEIPR, 2017), and so, on average, have substantially less work experience than their male equivalents. Women may not have had ample time in the labour market for their true value to be assessed, which may produce large variations in their earnings. Thus, women may require a longer time frame for their wages to accurately reflect their value in the working world.

5.1 Robustness Checks

So far, our analysis has solely focused on comparing certain birth months to the base case of August, however, it is not just August births who may be significantly affected by the academic selection period, but all individuals who are relatively younger than others, as we would expect them to be worse off than anyone who is relatively older than them. By combining months into seasons, we can begin to see wider relative age effects and have a more conclusive understanding of the birthdate effect, as our initial analysis only concludes that female August births are severely worse-off. This further analysis will help support our initial findings and serve as a robustness check for our results.

If we replace relative age dummies for seasonal dummies of Autumn, Winter, and Spring, as shown in equation (3), we can further analyse our data set using the base case of Summer. The results are outlined in Table 2.

$$lhourpay_i = \alpha + \sum_{n=1}^{3} \beta_n Season_i + \sum_{j=4}^{16} \beta_j RelInt_i + \beta_{17} Female_i + \sum_{k=18}^{20} \beta_k FemaleSeason_i + \epsilon_i \tag{3}$$

Our results show that for women, those born in Autumn have markedly higher earnings than those born in Summer at age 25, with an average of a 6.3% increase in earnings. Likewise, for those born in Spring we see markedly higher wage levels compared to their younger Summer peers, of 6.6%. These results are very strong, and are significant to the 1% level. Interestingly, those born in Winter do not show a significant difference in hourly wages. For men, we see the same pattern as previous, with no statistically significant effect on earnings for men born in any season when compared with their Summer-born peers.

5.2 Limitations

A clear limitation of this approach is the nature of the data set, as mentioned in the literature review, the ideal method of completing such analysis would involve a population data set, with substantial cohort earnings data, as completed by Solli (2012). A wider data set will reduce the standard errors and allow a more accurate analysis of the birthdate effect. Likewise, multiple years of earnings data from various cohorts would provide further support of the accuracy of the earnings data, as years where economic conditions are favourable or unfavourable are evened out, furthermore, it would allow deeper analysis into the exact nature of the birthdate effect across time. The UK currently does not have such a data set available, and so future work on the topic will again suffer the same issues. Due to this data discrepancy, the results presented are weak regarding the exact nature of the birthdate effect, however, as we are not claiming to have evidence of the exact nature of the birthdate post-education, just that it likely exists.

Another potential weakness of this analysis is our failure to control for family based characteristics that may bias the coefficients on our explanatory variables. In the literature, further variables regarding an individuals socio-economic status are often used, to eliminate potential biases created. These biases may be caused by more advantaged families timing their births earlier in the academic year, and hence the wage gains associated with relative age are over-represented and are caused by the well-offness of these families instead. An example of these kinds of controls is the use of Mother fixed effects controls by Solli (2012) or family size, mother's education, or birth order controls by Black et al. (2011). However, these controls had little impact on the nature of the birthdate effect in the literature, so it is safe to assume that by not including them we are not producing heavily biased results.

6 Conclusion

From our empirical results, we have concluded that the birthdate effect may persist beyond education and into early adult outcomes, specifically the earnings of an individual at age 25. However, we have only evidence to support its presence for women, as men have no discernible differences in earnings dependent on their relative birth month to the academic cut-off date. We have further concluded that this birthdate effect may be most apparent for women born in the earlier months of the academic year, when compared to those born at the end of the academic year. Likewise, when months are combined into seasons, we continue to have evidence of the effect, as Summer-born women experience 6% lower earnings compared to their Autumn-born peers.

This report hopes to have established a precedent for future work in the field, as we believe it is necessary for future research to be undertaken to assert the exact nature of the birthdate effect beyond education. We believe this can only be achieved if a population panel data set, with multiple years of earnings, can be constructed. This data would allow a clear picture of the nature of the UK birthdate effect. Furthermore, this report has provided important insight into our understanding of the long-term consequences of a child's relative schooling age on early adult outcomes.

References

Alton, A. and Massey, A. (1998). Date of birth and achievement in GCSE and GCE A level. *Educational Researcn*, 40:105–109.

Bedard, K. and Dhuey, E. (2012). School-Entry Policies and Skill Accumulation Across Directly and Indirectly Affected Individuals. *Journal of Human Resources*, 47(3):643–683.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2011). Too Young to Leave the Nest? The Effects of School Starting Age. *The Review of Economics and Statistics*, 93(2):455–467.

Blanchflower, D. G. and Oswald, A. J. (2004). Well-being over time in Britain and the USA. *Journal of Public Economics*, 88(7-8):1359–1386.

Crawford, C., Dearden, L., and Greaves, E. (2013a). The impact of age within academic year on adult outcomes. DoQSS Working Papers 13-05, Department of Quantitative Social Science - UCL Institute of Education, University College London.

Crawford, C., Dearden, L., and Greaves, E. (2013b). When you are born matters: evidence for England. Doqss working papers, Department of Quantitative Social Science - UCL Institute of Education, University College London.

Crawford, C., Dearden, L., and Meghir, C. (2007). When You Are Born Matters: The Imapct of Date of Birth on Child Cognitive Outcomes in England. CEE Discussion Papers 0093, Centre for the Economics of Education, LSE.

Fitzgerald, J., Gottschalk, P., and Moffitt, R. (1998). An Analysis of Sample Attrition in Panel Data: The Michigan Panel Study of Income Dynamics. NBER Technical Working Papers 0220, National Bureau of Economic Research, Inc.

Fredriksson, P. and Öckert, B. (2005). Is Early Learning Really More Productive? The

Effect of School Starting Age on School and Labor Market Performance. IZA Discussion Papers 1659, Institute for the Study of Labor (IZA).

HEFCE (2005). Young Participation in Higher Education. Ref report, HEFCE.

HEIPR (2017). Participation Rates In Higher Education:Academic Years 2006/2007-2015/2016 . Statistical first release, Department For Education.

Kawaguchi, D. (2011). Actual age at school entry, educational outcomes, and earnings. *Journal of the Japanese and International Economies*, 25(2):64–80.

Larsen, E. and Solli, I. (2012). Born to Run Behind? Persistent Relative Age Effects on Earnings. UiS Working Papers in Economics and Finance 2012/10, University of Stavanger.

Massey, A., Elliot, G., and Ross, E. (1996). Season of birth, sex and success in GCSE English, mathematics and science: some long-lasting effects from the early years? *Research Papers in Education*, 11:129–150.

Pigeon, D. (1965). Date of Birth and Scholastic Performance. *Educational Research*, 8:3–7.

Psacharopoulos, G. and Patrinos, H. A. (1994). Returns to investment in education: A global update. *World Development*, 22(9):1325–1343.

Psacharopoulos, G. and Patrinos, H. A. (2004). Returns to investment in education: a further update. *Education Economics*, 12(2):111–134.

Russel, R. and Startup, M. (1986). Month of birth and academic achievement. *Personality and Individual Differences*, 7:839–846.

Sharp, C. (1995). What's age got to do with it? A study of patterns of school entry and the impact of season of birth on school attainment. *Educational Research*, 37:251–265.

Solli, I. (2012). Left behind by birth month. UiS Working Papers in Economics and Finance 2012/8, University of Stavanger.

Sykes, E. D. A., Bell, J. F., and Rodeiro, C. V. (2016). Birthdate Effects: A Review of the Literature from 1990-on. Cambridge assessment, University of Cambridge.

Appendix 1: Summary statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Average hours worked per week	5,222	38.48	10.32	0.100	168
Average gross weekly pay	5,222	671.8	13,879	0.0800	1,000,000
Average gross hourly pay	5,222	25.15	818.3	0.006	58,824
Logged average gross hourly pay	5,222	2.432	0.476	0.00623	10.98
Relative Age	5,222	6.675	3.449	1	12
Female	5,222	0.553	0.497	0	1
Male	5,222	0.447	0.497	0	1
Female Relative Age	5,222	3.711	4.200	0	12
Male Relative Age	5,222	2.964	4.032	0	12
Winter	5,222	0.240	0.427	0	1
Autumn	5,222	0.233	0.423	0	1
Spring	5,222	0.260	0.439	0	1
Summer	5,222	0.268	0.443	0	1
Male Autumn interaction dummy	5,222	0.107	0.309	0	1
Male Winter interaction dummy	5,222	0.105	0.306	0	1
Male Spring interaction dummy	5,222	0.116	0.320	0	1
Male Summer interaction dummy	5,222	0.119	0.324	0	1
Female Autumn interaction dummy	5,222	0.125	0.331	0	1
Female Winter interaction dummy	5,222	0.135	0.342	0	1
Female Spring interaction dummy	5,222	0.144	0.352	0	1
Female Summer interaction dummy	5,222	0.148	0.355	0	1
Relative Interview month	5,222	6.615	2.906	1	14

Appendix 2: Breusch-Pagan Test

Chi-Squared Statistic	Prob > Chi(2)	Reject H0?
133.30	0.00	Evidence to Reject H0 H0: Constant Variance