# The Long Term Impacts of Compulsory Schooling: Evidence from a Natural Experiment in School Leaving Dates

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## ABSTRACT

This paper investigates a unique feature of the English educational system to estimate the causal effect of compulsory schooling on labour market outcomes. We examine school leaving rules that allow for discrete variation in exit dates by date of birth within school cohorts. This natural experiment enables a regression discontinuity design that differences out confounding factors discussed in the literature. Individuals compelled to stay in school for as little as three months longer than their classmates tend to achieve significantly higher qualification levels and experience better labour market outcomes. Our analysis of variation of impacts by age of compulsory schooling allows us to provide valuable new insights on the role of education credentials in the labour market.

## NON-TECHNICAL SUMMARY

For a very long time compulsory schooling has been considered a cornerstone of education policy. However, in recent years, the emphasis on requiring students to complete a minimum number of years in school as a policy instrument has dwindled in importance, while focus has shifted to a stronger emphasis on choice and quality. Among economists, compulsory schooling laws have also attracted considerable attention in recent years as part of a widespread effort to find credible instruments to identify the causal effect of schooling on labour market outcomes. Starting with Angrist and Krueger's 1991 seminal paper, a strand of research has attempted to address the causality problem by using "natural experiments" driven by the institutional structure of the education system and its unequal treatment of otherwise similar individuals on the basis of their date of birth.

Most of the institutional settings analysed to date, however, may confuse the "true" schooling effect with other potentially important factors, such as the impact of relative age within a school cohort. This is because identification of schooling effects usually relies on a comparison between the oldest individuals in the class, who according to the rules in place are more likely to leave without completing the full grade, and their younger peers, who are compelled to stay on and attain that year's grade.

This paper approaches such problems by considering a unique set of institutional circumstances which were in place in England and Wales over much of the second half of the twentieth century. This structure was organized such that, depending on their date of birth, individuals within the same school cohort were allowed to leave school only after one of two specific dates upon reaching their final year of compulsory schooling. Since no other institutional differences, apart from school exit rules, apply in the period around the date of birth that marked this separation, we can safely attribute observed differences in performance to the impact of the school exit rule and build our estimation strategy around what is effectively a regression discontinuity design.

We show that this rule had a strong impact on school leaving behaviour, on qualifications achieved by age 16 as well as adult educational outcomes. We then address the question of whether differences in educational outcomes induced by the rule translate into significant differences in adult labour market performance. Our IV estimates suggest significant qualification effects on participation, employment and earnings, which are usually of a higher magnitude than those obtained through OLS. Consistently with other studies, we find that formal education is a more important driver of future economic success for women than it is for men.

Our research makes a contribution to the understanding of regulatory obligations as policy instruments. We show that well-considered interventions of this kind can actually have a positive impact on the subjects whose behaviour is constrained as a result of their enforcement. We argue that policies intended to improve educational outcomes should not fully dismiss the regulatory approach in favour of other financial incentives when both can be powerful complements. The right balance between these two approaches, however, still needs to be addressed.

This study also highlights the interaction between education, as measured by the time input devoted to schooling, and the credentials which are acquired as a result. The rules we consider here induce a fairly small difference in potential length of schooling - approximately three months. We demonstrate that the same school leaving rules imply stronger effects when the school leaving age is timed to compel individuals to complete a year in which they can be awarded nationally recognized qualifications. In other words, our results suggest that the effect of gaining a certification and not just merely length of schooling alone plays an important role in explaining future economic outcomes.

We thus conclude that policies aimed at increasing time in formal education should at least allow individuals to receive credit for the additional learning experienced. While there is no reason to assume that an investment model of schooling is the right framework for understanding teenage behaviour in relation to school, qualifications and skills, it is doubtful they will remain oblivious to the potential risk of not obtaining a labour market premium for the additional efforts.

# I. INTRODUCTION

Over the last couple of centuries, compulsory schooling has become a fundamental feature of education and social policy all over the world. This reflects the broad consensus that uneducated societies are ill-equipped to foster social and economic development, leading countries to implement sustained increases in the age at which students are allowed to leave school. In more recent years, the emphasis on requiring students to complete a minimum number of years in school as education policy instrument has dwindled in importance in public policy debate, while focus has shifted to a stronger emphasis on choice and quality. To a large extent, this paradigm shift is understandable. As the private opportunity cost of schooling increases with age, it becomes harder to defend the rationale for constraining choice as to when individuals may decide to end their formal education.

Nonetheless, governments have policies oriented towards meeting highly publicized targets on the qualification attainment of school leavers. This highlights the need for a better understanding of whether and how extended schooling actually leads to better material and personal outcomes, what constraints are faced by students and their families, and whether the minimum levels of schooling set by law leave individuals equipped to face the opportunities and challenges of a rapidly changing labour market.

Among economists, compulsory schooling laws have attracted considerable attention in recent years as part of a widespread effort to find credible instruments to identify the causal effect of schooling on labour market outcomes. Starting with the seminal paper by Angrist and Krueger [1991], a strand of research has attempted to address the causality problem by using "natural experiments" driven by the institutional structure of the education system and its *unequal* treatment of otherwise similar individuals on the basis of their date of birth.[1]

In the U.S. example analyzed by Angrist and Krueger [1991], the last day of December marks the separation of students into school cohorts while pupils are allowed to leave school just after the birthday in which they have attained their state's minimum school leaving age.[2] As a result, some of the older individuals in the class leave without completing the full grade, whereas their younger peers are compelled to stay on and attain that year's grade.[3] This ground-breaking study immediately attracted considerable attention, but it was subsequently criticised on a number of fronts.[4] Our study addresses the central challenge to this paper, namely that the U.S. natural experiment can potentially confound the "true" schooling effect with other important factors, such as season of birth effects, the impact of absolute and relative age within a school cohort, and the impact of an early school entry age.[5]

In particular, a number of recent papers have pointed out that age at school entry and relative age can have an impact on school achievement and other outcomes which is not necessarily driven by the direct effect of time in formal education

[1] Public education systems assign students to school cohorts on the basis of their date of birth, which is also used to define the enforcement of school entry and exit rules. See Card [1999 and 2001] for a discussion of this approach.

[2] In other words, if the state's minimum school leaving age is 16, an individual would be entitled to drop out from school on the exact day of her 16th birthday. As Angrist and Krueger [1991] note, there are some exceptions to this. In Wisconsin and Texas, for example, students are required to complete the school term in which they reach the legal dropout age.

[3] In a separate paper, Angrist and Krueger [1992] investigate entry patterns by looking at how the age of school entry (influenced by the date of birth) affects educational outcomes. Consistently with previous findings, they show that younger entrants stay in school longer.

[4] In particular, one of the criticisms refers to the statistical problems affecting the property of IV estimators with weak instruments [Bound *et al.*, 1995, and Staiger and Stock, 1997] or the weak identification result under non-parametric conditions [Chesher, 2003].

[5] These effects are discussed at length in Bound and Jaeger [1996]. They review a substantial literature which shows that certain health problems show dependence with respect of birth dates and influence school performance. They also discuss the evidence on the correlation between season of birth and family background presented in Lam and Miron [1991]. However, in a later study Card [2001] does not find any evidence of systematic variation in parental education by quarter of birth in the 1940 U.S. Census, which is the period of time closest to Angrist and Krueger's [1991] sample for which such data is available.

induced by compulsory schooling.[6] As a result, there are grounds for questioning whether these multiple "age effects" can persist into adulthood and manifest themselves into various labour market outcomes. If this is the case, institutional settings considered to date have failed to provide the type of natural experiment that can partial out the confounding effects of age at school entry, relative age effects and minimum length of schooling.[7]

This paper approaches such problems by considering a unique set of institutional circumstances which were in place in England and Wales over much of the second half of the twentieth century. This structure was organized such that, depending on their date of birth, individuals within the same school cohort were allowed to leave school only after one of two specific dates upon reaching their final year of compulsory schooling. Since no other institutional differences, apart from school exit rules, apply in the neighbourhood of the date of birth that marked this separation, we can safely attribute observed differences in performance to the impact of the school exit rule and build our estimation strategy around what is effectively a regression discontinuity design.[8]

This study also highlights the interaction between education as measured by the time input devoted to schooling and the credentials which are acquired as a result.

[6] A close investigation of the educational, psychology and economic literature reveals significant confusion about the nature of impacts identified by comparing individuals born at the margins of thresholds birth dates for cohort assignment. We recommend the reading of Fredriksson and Öckert [2005] for a very detailed explanation of these issues. Related studies are Cahan and Cohen [1989] for Jerusalem students, Mayer and Knutson [1999] on the NLSY, Strøm [2004] for Norway and Plug [2001] for the Netherlands. Key references for England and Wales are Woodhead [1989], Hutchison and Sharp [1999], Sharp and Hutchison [1997] and Daniels *et al.* [2000]. Further references on the role of relative age or maturity effects can be found in Dudink [1994], Musch and Grondin [2001], Sharp and Benefield [1995], Goodman *et al.* [2003] and Skirbekk *et al.* [2004].

[7] We do not try to address directly questions such as whether parents should try to anticipate or delay school entry or whether school entry age should be further reduced. The former question implies a combination of early entry and position in the school cohort, while the latter needs to abstract from the relative age ranking, which a widespread reform cannot alter. Our main concern is about the trade off between potential labour market experience and time in education.

[8] See Thistlewaite and Campbell [1960] and Hahn *et al*. [2001] for more background on regression discontinuity.

The rules we consider here induce a fairly small difference in potential length of schooling - approximately three months. We argue that this is a significant period of time because it includes the point at which the English and Welsh systems assess the achievement of students and provide recognizable academic credentials for their performance. This is supported by the fact that the same school leaving rules identify stronger effects when the school leaving age is timed to compel individuals to complete a year in which they can be awarded nationally recognized qualifications. In other words, our results suggest that the effect of gaining certification and not just merely length of schooling alone plays an important role in explaining future economic outcomes.

Furthermore, our research makes a contribution to the understanding of regulatory obligations as policy instruments. We show that well-considered interventions of this kind can actually have a positive impact on the subjects whose behaviour is constrained as a result of their enforcement.[9] We argue that policies intended to improve educational outcomes should not fully dismiss the regulatory approach in favour of other financial incentives when both can be powerful complements. The right balance between these two approaches, however, still needs to be addressed.

The paper is organized as follows: Section II describes the school leaving institutional framework operating in England and Wales until 1997, and explain how this was in essence a natural experiment allowing a separation of schooling from relative age and other confounding effects. The data sources are described in Section III. Section IV shows how policy-induced differences in length of schooling have a strong effect on school leaving behaviour and the qualification attainment for

[9] We cannot assess whether forcing students to stay at school against their will can lead to increased incidence of classroom disruption and ultimately exert a negative influence their peers' outcomes.

recently-surveyed cohorts of young people. The analysis is then extended to qualifications held by adults. Section V describes the impact of exit rules and schooling on a number of key labour market outcomes. Section VI investigates whether these results can be explained by certification effects. Section VII concludes.

## II. INSTITUTIONAL FRAMEWORK AND IDENTIFICATION STRATEGY

### *II.A. Compulsory Schooling in England and Wales*

Since the 1870 Education Act, the cut-off date for school entry in England and Wales is the first day of September and the official school starting age is the beginning of the term starting after a child's fifth birthday. Although five is already low by international standards, most children start school when they are four because many schools grant admission at the beginning of the year in which children turn five. The system allows for multiple entry dates (traditionally three and at the beginning of each term), but this practice varies across Local Educational Authorities and schools have moved towards a single entry date in September over time [Woodhead, 1989]. Parents have few opportunities to defer school entry of their children and, unlike in other countries, it is very rare for individuals either to be held back a year or to be promoted to the next year group [Sharp *et al.*, 2002].

The current school leaving age of 16 was increased from 15 in 1973 by the Raising of the School Leaving Age (ROSLA) Order of 1972 for England and Wales. This order drew on previous legislation enacted in the 1944 "Butler" Education Act, which foresaw a two-step increase in the age of compulsory schooling from the 1944 level of 14. The first of these steps took place in 1947 with the raising of the school leaving age to 15, but it took another 15 years before the intended leaving age of 16 was introduced.[10]

In England and Wales, the implementation of compulsory schooling differs from other countries' practices in that an individual is not allowed to leave school on the exact date (birthday) in which she attains the school leaving age. Instead, the official rules are rather more complex and have been modified through time. In particular, since the school year 1963-64 until the school year 1996-97 (Education Act of 1962), legislation allowed children born between 1st September and 31st January to leave school at the beginning of the Easter holiday of the school year in which they reached the applicable compulsory schooling age. However, those born between 1st February and 31st August were not considered to have attained the compulsory schooling age until the last week of May, shortly before the end of the summer term.

The dual school leaving date arrangement resulted from the simplification of the previous system which allowed for three different exit dates and which had been designed in order to track as closely as possible individuals' entry date and age.[11] The rationale for staggered exit dates could be explained by the need to avoid the potentially damaging impacts of continuous drop-out in a class while meeting seasonal increases in the demand for labour triggered by the Easter period. A key factor in preventing a move towards a single school leaving date, as it is common in most European countries, was the strong belief among sections of the public that a massive, simultaneous outflow of jobseekers resulting from a single school leaving date would impinge on the functioning of the labour market,[12] given the relatively low

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[10] These changes in the school leaving age have been used in the literature to identify the effects of schooling on earnings [Harmon and Walker, 1995; Chevalier *et al*., 2004], children's education [Chevalier, 2004] and citizenship [Milligan *et al.*, 2003], among others.

[11] The previous Education Act of 1944 established the end of the term in which an individual attained the age of compulsory schooling as the minimum school leaving date.

[12] The headline of an article published on "The Times" of 31st January 1962 read: "Employers and Unions opposed to one school-leaving date: Too many in the labour market at the same time". The author of the article offered a very detailed account of the situation at the time, according to which: "[employers and trade unions] feared that it might be difficult to find all at once jobs for over 500,000

percentage of the population staying on in education beyond the age of compulsory schooling at the time.[13]

To show how exit rules support our identification strategy, we depict the profiles of expected age at school entry and earliest possible age of school exit for different dates of birth in Figure I. Entry rules determine that an age group, school cohort or class consists of children born between the first day of September and the last day of August in the following calendar year. The lighter discontinuous line represents the expected school starting age for individuals born in different months for the cohorts born between 1960 and 1965. A clear discontinuity emerges between those born in August and the consecutive month of September, which is explained by the cut-off date for school entry.

The rules governing school exit imply a double discontinuity on potential school leaving dates. The first one can be located at the same point as the entry age discontinuity: August-born children are forced to stay until the end of the school year while September-born children are allowed to leave at Easter. As we can see, in this case entry date rules induce also a discontinuity in relative age, as August-born children start school almost one-year earlier than September-born children (this discontinuity is analogous to the December-January comparison in the U.S. example considered by Angrist and Krueger, 1991).

The second discontinuity, which is smaller in size, applies to the differences between individuals born in January and February. This gap in the length of minimum

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school-leavers at the end of the summer term". The Minister of Education is also reported as saying: "I felt it was right to make the experiment of having two leaving dates, hoping that within a year or two the experiment will prove fears not to be well grounded", arguing the case for a move from three to (eventually) one school leaving dates. Interestingly, the U.K. government considers at one point (The Times 30th December 1974) the possibility of moving to a single school leaving date set in May, half-way in between the Easter leaving date and the summer end date.

[13] In the early 1960s only about 20 per cent of pupils stayed on in full time education after having reached the minimum school leaving age [McVicar and Rice, 2001].

compulsory schooling occurs *within* the same school cohort, and since it does not take place in conjunction with a jump in school starting age it represents an ideal source of identification of pure compulsory schooling effects.[14]

*II.B. Identification through a Regression Discontinuity Design (RDD) Approach*

Our approach is very much inspired by the seminal contribution by Angrist and Krueger [1991] but at the same time it has its own very distinct advantages. We estimate the impact of compulsory schooling on school attainment and longer term labour market outcomes through observed differences in performance around the discontinuity point between January and February. To the extent that individuals born on the last day of January and the first day of February are identical in all other observable and unobservable characteristics, the difference in their staying-on behaviour will be driven only by the exit rules, allowing us to identify the average impact of qualification attainment on the outcome of interest.

In order to illustrate our identification strategy and the importance of taking into account relative age effects, let us consider a simple linear model in completed schooling:

$$S_i = \gamma \cdot \kappa(Z_i) + \varepsilon_i \,, \tag{1}$$

where an individual *i*'s schooling $S_i$ depends on the level of compulsory schooling $\kappa(Z_i)$ that an individual should complete according to existing rules, and a number of other factors $\varepsilon_i$, some of which will be unobservable. Compulsory schooling is a deterministic function of date of birth, $Z_i$, which (as we just showed) is not

[14] For the sake of simplicity, in Figure I we assumed that all children enter school in September. Although this is not strictly true because of local and time variation in the implementation of entry rules, the main point is that the entry date for children born in January and February has always been the same (i.e. January) so that no entry age discontinuity applies between January and February.

continuous for all possible dates of birth.

We use some basic notation to illustrate the range the comparisons made between individuals born on very similar dates but subject to different schooling rules. Let $H$ be a random variable, and denote the limit difference operator $D$ at point $z$ as:

$$DH(z) \equiv \lim_{\tau \to 0} \{E(H_i \mid Z_i = z - \tau) - E(H_i \mid Z_i = z + \tau)\},$$

i.e. the limit of the conditional expectations of $H$ on two separate dates of birth as they converge to the same value from the left and right. This operator becomes zero when the conditional expectation of our variable of interest is continuous at $z$.[15]

Following from our earlier discussion of Figure I, we know that there are two discontinuities in the relationship between date of birth and compulsory schooling. The main one applies to children born on 31st August and 1st September. If we denote this date as $z^*$, then using equation (1) we can write down the expected gap in completed schooling at this cut-off point as:

$$DS(z^*) = \gamma \cdot D\kappa(z^*) + D\varepsilon(z^*). \tag{2}$$

As can be inferred from Figure I, $D\kappa(z^*) > 0$, with differences in compulsory schooling $D\kappa(z^*)$ approximating a total of three months as entry age differences of one year are offset by a nine month age difference in potential exit dates. The key question is whether all other influences on schooling or educational outcomes cancel out around this threshold. In other words, can we safely assume that $D\varepsilon(z^*) = 0$? According to our earlier discussion, compulsory schooling is not the only source of discontinuity in educational experiences around this point. September-born children will be the oldest in their school cohort and will enter school at a substantially later age, for instance. These factors may well have their own

[15] This presentation focuses on conditional expectations, but the analysis could carry through to any quantiles under the conditions set out by Chesher [2003].

independent impact on educational outcomes.[16]

To understand the implications of relying on this discontinuity for the identification of the labour market effects of schooling let us consider a labour market outcome $Y_i$ expressed as a linear function of completed schooling ($S_i$) and other observable and unobservable individual characteristics ($\omega_i$):

$$Y_i = \rho \cdot S_i + \omega_i . \tag{3}$$

From equations (1) and (2), performing the same comparisons at the limit and using the same notation as above, the difference in labour market performance of individuals born at either side of the threshold is:

$$DY(z^*) = \rho \cdot DS(z^*) + D\omega(z^*). \tag{4}$$

It is clear that in order to identify the parameter of interest $\rho$ we need to impose $D\omega(z^*) = 0$. However, it is possible that relative age within school cohort and age of school entry might influence labour market outcomes independently of schooling.[17] These two additional effects would appear as confounding factors in an RDD estimation approach around the September 1st discontinuity.[18] A simple Wald-IV estimator around the August-September discontinuity point would capture the sample analogue of:

[16] Other confounding factors, such as season of birth, are less worrying as their effects will be smoothed out at the limit.
[17] August-born individuals will enjoy an extra year of potential experience but this only alters the interpretation of the net impact schooling against potential time in the labour market.
[18] The U.S. institutional set up analysed by Angrist and Krueger [1991] is not conducive to an RDD analysis. To understand this, it is important to note that although some of their estimates use differences between adjacent Quarter 4 and Quarter 1 (resembling the August-September discontinuity for England and Wales) their main IV estimates are based on *within* school-year differences across quarters. The identification therefore relies on the absence of systematic impacts of date of birth within the school year or functional form assumptions. With information on the exact date of birth, one could use as an instrument for grade completed the time a student would have to stay on beyond their legal exit date until the end of the final year's grade. However, in the U.S. institutional context even this approach implies no discontinuity. Students born just one day before the summer holiday will face negligible marginal costs of staying on until graduation date and the rank condition is thus likely to fail.

$$\hat{\rho} \Rightarrow \frac{DY(z^*)}{DS(z^*)} = \frac{\rho \cdot DS(z^*) + D\omega(z^*)}{DS(z^*)} = \rho + \frac{D\omega(z^*)}{\gamma \cdot D\kappa(z^*) + D\varepsilon(z^*)}. \quad (5)$$

This equation makes it clear that unless $D\omega(z^*) = 0$, estimates of $\rho$ will be inconsistent. There is also a second and related problem. In small samples, even minor deviations from $D\omega(z^*) = 0$ will magnify the bias if relative age effects work in the opposite direction of compulsory schooling effects. This would happen if, for example, older children were more likely to obtain better schooling outcomes than their younger class peers. We might think of this as an *additional* "weakness" of an RDD approach around this discontinuity.

The virtue of our natural experiment is the availability of an alternative date of birth $z'$ for which the school entry and maturity effects can be fully differenced out from schooling and labour market outcomes. The January-February cut-off date creates a gap in compulsory schooling levels between individuals that belong to the same school cohort. In the absence of other rules driving the school entry process at this point, subjects born minutes before and after the midnight of 31st January will be observationally identical in all respects other than the date at which they are allowed to leave school, so that $D\omega(z') = 0$, $D\varepsilon(z') = 0$, while $D\kappa(z') < 0$.

Our paper does not seek to make specific methodological contributions on RDD estimation techniques. Our main results are simply obtained by means of reduced form and Wald-IV estimates where we: 1) experiment with parsimonious structures to capture the behaviour of individuals at either side of the cut-off birth date, 2) test the robustness of our estimates by choosing different data windows around this discontinuity point, and - when exact date of birth is available – 3) estimate locally weighted linear regressions of the outcomes of interest on both sides of the threshold. Our ultimate objective is to find the best possible sample analogues for $DY(z'), DS(z')$, and $D\kappa(z')$, and to estimate our parameter of interest as $\hat{\hat{\rho}} = \frac{DY(z')}{DS(z')}$.

We should also use some caution in the interpretation of our results. This is because the IV estimator we propose here does not necessarily lead to a consistent estimate of the *average* marginal return to education across the entire population. Using the terminology proposed by Imbens and Angrist [1994], what we estimate here is a Local Average Treatment Effect (LATE). This is a weighted average of the impact of achieving a qualification where positive weights are given to individuals who are induced to obtain a qualification as a result of the school leaving rule.[19] Although this means that our results cannot be easily generalized to a wider group of pupils, in the absence of randomized trials this approach arguably offers one of the best strategies for the identification of the causal effect of schooling on adult labour market outcomes.

## III. DATA SOURCES

We construct three key databases for our analysis of the impact of being entitled to leave at Easter and consider the following outcomes: (i) short-term indicators of educational achievement, such as length of schooling and qualification attained one year after achieving the compulsory schooling age, and (ii) longer-term measures of performance, such as the highest academic qualification achieved, the labour market participation and employment status of individuals as well as their earnings.

[19] For this to be true, it is conceptually required that there are no individuals who move against the tide, i.e. for whom the obligation to stay on represents an incentive to leave school whereas, if they were allowed to do as they pleased, would prefer to stay on to try and obtain a qualification.

The first of our data sources is the Youth Cohort Study of England and Wales, a series of surveys conducted by the U.K. Department for Education and Skills. The surveys sample students from the population of those eligible to leave school in the previous school-year and interview them in three separate occasions, i.e. at age 16-17, age 17-18 and 18-19. Our focus is on cohorts 2, 3 and 4, which correspond to individuals born in 1968-69, 1969-70 and 1971-72 (representing students eligible to leave in the school years 1984-85, 1985-86 and 1987-88, respectively).[20]

The Youth Cohort Study allows us to test whether the date of birth effect is actually reflected in staying-on behaviour as exit rules would predict, and to understand how this translates into qualification achieved by age 16-17. We rely on information gathered in the first sweep only because later sweeps suffer from considerable attrition problems. Although some information on family background is available in this dataset, there is a general lack of consistency in the way these variables are collected throughout the years and only very few controls are available for the entire period analysed. The main limitation of this survey remains its relatively small sample size, which forces us to pull together observations for different cohorts and for women and men.

The second source we consider is the Labour Force Survey for the period from 1993 to 2003. This is a quarterly sample survey of households living at private addresses in Great Britain.[21] Crucially for our purposes, this survey provides information on the exact month and year of birth of individuals together with

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[20] This is because we have information on the exact leaving school date only up to cohort 4 and date of birth is missing for a large and non-random sample of individuals interviewed in cohort 1. The Youth Cohort Study is an ongoing survey. The latest cohort available for analysis is cohort 10, which consists of individuals born in 1982-83 who were entitled to leave in the school-year 1998-99.

[21] The LFS is based on a systematic random sample design which makes it representative of the whole of Great Britain. Every quarter is made up of 5 "waves", each of approximately 12,000 households. Each wave is interviewed in 5 successive quarters, such that in any one quarter, one wave will be receiving their first interview, one wave their second, and so on, with one wave receiving their fifth and final interview. For more details, see http://www.esds.ac.uk/government/lfs/.

information on their level of educational attainment and labour market status. Unfortunately, there is no background information on characteristics such as the socioeconomic status of the individual's parents, type of school attended or even the precise region where they were brought up.

From this survey we select a pool of respondents aged 24 to 60, although in our main results we eventually exclude older individuals so as to abstract from the effect of the increase in school leaving age from 15 to 16 in 1973-74. Individuals who arrived in Great Britain after the age of 10 are not considered, as their schooling experience could have little to do with the schooling rules we analyse in this paper. Since we examine school leaving rules operating in England and Wales, we restrict our attention to the sample of people resident in these countries at the time the survey is carried out. This implies an inevitable degree of noise due to migration from Scotland or Northern Ireland which we cannot account for.

Finally, we use earnings information from the New Earnings Survey (NES) as a complement to our main LFS estimates for adult outcomes. The New Earnings Survey is based on a one per cent sample of employees in employment in Great Britain. Information on their earnings and hours is obtained by the Office for National Statistics (ONS) in confidence from employers. As a result, the quality of the earnings data is higher and the samples are larger than for the LFS. Longitudinal information is also available for as long as the individual works for an employer registered for the national insurance contributions and income tax pay-as-you-earn (PAYE) system.

The NES is available for statistical use by approved researchers under secure conditions at the ONS for the period 1986-2003. Unfortunately, date of birth is unavailable from the ONS files and our requests to get this information have proved unsuccessful to date.[22] In this paper, we use a large complementary database to identify dates of birth for a subsample of the NES. The Joint Unemployment & Vacancies Operating System Cohort (JUVOS) database comprises a 5 per cent sample of all claims for unemployment related benefits registered in the national unemployment benefits payments systems (NUBS). Dates of birth for this sample, for which we have access over the period 1983-2004, can be safely merged to the NES through an anonymised version of the National Insurance number which is present in both samples. This implies that our matched sample includes individuals who at some point between 1984 and 2004 have claimed unemployment benefits at least once and who are observed in employment at any time between 1986 and 2003. Neither NES nor JUVOS include information on educational qualifications, hence we can only perform reduced-form estimates of the impact of Easter leave entitlement.

We acknowledge that the composition of this matched JUVOS-NES sample is not representative of the whole population. This is for reasons related to the administrative nature of the data collection process, but also because our LFS-based findings reveal an impact of date of birth on the probability of employment and labour market participation. Furthermore, the representativeness of the sample is likely to differ substantially for men and women, since the latter have different propensities to claim unemployment benefits and to go back into work after an unemployment spell.

The main advantage of this dataset is that it allows us to investigate in further detail the impact of date of birth on more accurate earnings information for what could be defined as a "vulnerable" subpopulation, and to fully exploit the features of the regression discontinuity design that is enabled by our access to the exact date of

[22] These data certainly exist as the NES includes an "age" variable, expressed as an integer.

birth, as opposed to the month.[23]

## IV. SCHOOL LEAVING RULES AND QUALIFICATION ATTAINMENT

### *IV.A. School Leaving Behaviour and End-of-Year Exams in the Youth Cohort Study*

We use data from the Youth Cohort Study to investigate whether individuals allowed to leave at Easter (ATLE) actually leave school earlier than those who are forced to stay until the end of the school year, and whether this affects their propensity to take exams at the end of the Summer term. Interestingly, the Youth Cohort Study also allows us to see whether an individual left school upon reaching the Easter leaving date and came back to sit the exams at the end of the year. Indeed, the latter form of behaviour was common practice at the time since it was explicitly considered as a separate answer to the question about school leaving date.

In Figure IIa we depict the smoothed profiles of the probability of leaving at Easter by date of birth, having collapsed the three cohorts into a stylized single school cohort in order to improve the power of the estimates. This figure clearly shows a discontinuity in the probability of leaving school of approximately 13 percentage points at the cut-off birthday date of 31st January. As we can see, the estimated probability is higher than zero even for those students who are not entitled to leave at Easter, suggesting imperfect enforcement of exit rules and probably school exclusions. In other words, the school leaving rules analysed in this paper induce what is known as a fuzzy regression discontinuity design [Hahn *et al.*, 2001].

We investigate how this school leaving behaviour translates into educational outcomes. In first place, not all students who leave at Easter give up completely on the

[23] For more details on the JUVOS and NES datasets and the matching procedure see Appendix A.

opportunity of obtaining a qualification as they can return to sit exams.[24] Since we have direct evidence on this kind of behaviour, we can estimate the probability of leaving at Easter without going back to school to take examinations. As Figure IIb illustrates, approximately 8 per cent of ATLE students do not return to take exams while the gap with those that must stay is still significant although the magnitude is considerably smaller, only about 4 percentage points.

Although school-leaving behaviour is an interesting outcome, it represents only *prima facie* evidence that differences in birth dates are associated with differences in educational achievement because of rules governing the length of compulsory schooling. As we have just seen, many students who leave at Easter also come back to take exams, and it is therefore not straightforward to gauge how the "Easter rule" impacts on the probability of achieving a qualification. Figure III therefore plots the probability of achieving an academic qualification by date of birth. As we can see, attainment is significantly lower by more than 2 percentage points for ATLE students as compared to the other group at the margin of the eligibility threshold. This effect is smaller than that found on exam taking behaviour in Figure IIb and suggests that some of the students who leave at Easter and return to take exams achieve satisfactory qualification outcomes, while at the same time not all individuals induced to stay until the end of the school year attain a qualification.

Table I provides some quantitative estimates of the discontinuities observed in our graphical analysis. In particular, we estimate the following OLS regression for different educational outcomes:

$$S_i = \gamma \cdot ATLE_i + X_i \pi + \sum_c C_{ic} \delta_c + \sum_c C_{ic} \cdot A_i \varphi_c + u_i \,, \tag{6}$$

[24] About two-thirds of the students who leave at Easter come back to take exams.

where $S_i$ is individual *i*'s educational attainment, $ATLE_i$ is a dummy with value 1 if the individual is born before the 31st January date, $X_i$ is a vector of covariates, $C_{ic}$ is a vector of dummies for whether individual *i* belongs to school cohort *c*, and $A_i$ is a linear term in age within each school cohort.

To approximate the regression discontinuity design, we consider only subjects born between December and the following March.[25] While the impact on the overall staying-on behaviour is about 12 percentage points, the effect goes down to 5 points once we consider only the probability of leaving and not coming back to sit for exams. The effect of the "Easter rule" entitlement on the probability of obtaining a qualification is significant and approximately minus 2.8 percentage points in magnitude, fairly close to results in the graphical analysis.

The remainder of the table decomposes these effects according to the father's socioeconomic status in order to investigate whether there is heterogeneity with respect to family background factors. As we expect, students with a skilled father are far less affected by the rule although still less likely to stay on, while the impact on attainment for this group is no longer significant. Instead, for those with an unskilled or out of employment father the effect of the rule is much stronger and translates into significant differences in qualification attainment.

In Table II we present OLS and IV estimates of the effect of leaving school ($LS_i$) at Easter on achieving an academic qualification ($Q_i$). The IV regressions use entitlement to leave at Easter as an instrument and show that the effect of birth date is linked to qualification attainment through variation in school-leaving behaviour.

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[25] Ideally we would have liked to consider only individuals born in January and February or take those even closer to the cut-off date (we have precise information on day of birth in the Youth Cohort Study), however the sample becomes too small and we are forced to consider a larger window in order to obtain more robust estimates.

Specifically, in the first stage we estimate:

$$SL_i = \gamma \cdot ATLE_i + X_i \pi + \sum_c C_{ic} \delta_c + \sum_c C_{ic} \cdot A_i \varphi_c + u_i \,, \tag{7}$$

and in the second stage we consider:

$$Q_i = \psi \cdot SL_i + X_i \pi' + \sum_c C_{ic} \delta'_c + \sum_c C_{ic} \cdot A_i \varphi'_c + u'_i \,. \tag{8}$$

The OLS and IV estimates both tell us that school leavers are 22 per cent less likely to have a qualification by the time of the interview, i.e. one year later. Looking at those who leave at Easter but do not go back to take exams, we can see that here the difference induced by birth dates results in 55 per cent lower probability of having a qualification. We would have perhaps predicted an even bigger effect for this group, but we should account for the fact that the cohorts here examined reach the minimum school leaving age in the mid 1980s and this was a period of increasing efforts to induce higher educational levels in the population.

The fact that the inclusion of individual characteristics does not alter the results observed in the graphical analysis is important as it suggests that these attributes do not have a significant effect on the outcome of interest around the discontinuity point. This is equivalent to standard tests for randomisation, with the exception that our experiment holds in the close neighbourhood of the discontinuity point. We confirm this in Table III, where we present the sample means of several variables of interest at either side of the school-leaving threshold. Apart from the already observed differences in school leaving behaviour and educational attainment, there is no evidence of significant differences in background characteristics. Interestingly, labour market outcomes upon leaving full time education are also similar. If there is a return to schooling or qualifications, this does not appear to be immediate. We investigate this further below.

*IV.B. External Validation – Evidence on the Shift to a Single School-Leaving Date*

Despite their intuitive appeal, natural experiments are often criticized on the grounds of their limited scope for external validation. Policy changes provide an additional natural experiment for testing the role of the January-February school leaving rule. In the Circular number 11/97, the Secretary of State for Education used legislation enacted in the 1996 Education Act to converge towards a common school leaving date coinciding with the end of the main examination period. This meant that students in the school year 1996-97 were the last to face a January-February discontinuity in school leaving dates.

We are not aware of any analysis comparable to ours being used by academics or policy officials to inform this decision. What we can do, however, is to conduct an *ex-post* policy appraisal of the change in the rules using our analysis on cohorts 2 to 4 of the Youth Cohort Study. For example, we could say that the expected change in attainment should be broadly equal to the proportion of a school cohort born between September and January (let us assume for simplicity this is 5/12) times the extra probability of obtaining a qualification, which is about 2.5 percentage points according to Table I and II. This implies a predicted increase in attainment of approximately one percentage point.

How does this prediction compare against actual data? In Table IV we present the figures for exam participation and attainment published by the U.K. Department of Education and Skills. We can see that exam entry increases from 94 per cent in 1996-97 to 94.8 per cent in 1997-98, the first single-exit school year. As far as attainment is concerned, this figure changes from 92.3 per cent to 93.4 percent. If we consider that the change between 1995-96 and 1996-97 is only 0.1 percent, and we take this as an approximation to the underlying trend, this leaves us with exactly the one percentage point predicted by our estimates.

*IV.C. Patterns of Adult Qualification Attainment by Date of Birth*

We now turn to the sample derived from the Labour Force Survey, which enables us to investigate longer-term outcomes. Although this survey does not provide direct information on school leaving behaviour, it represents the largest British dataset with information on month of birth and educational attainment.[26]

We first investigate whether date of birth is correlated with educational achievement according to the features of the school leaving rule. To this end, Figure IVa presents the proportion of men in our sample who have attained any qualification, by month of birth for January (J) and February (F) born individuals. We report two separate measures: the share with either an academic or vocational qualification and the share with at least an academic qualification.

A few key features emerge from this figure. Firstly, the academic versus academic-or-vocational attainment gap considerably narrows down in 1957-58. This is due to the increase in the age of compulsory schooling from 15 to 16 in 1973-74, which affected for the first time the cohort of individuals born between September 1957 and August 1958, and raised the level of academic attainment mainly at the expense of apprenticeships. Secondly, there is a marked and consistent academic attainment gap between men born in January and those born in February, and this is evident mainly for the first few years after 1957-58.[27]

We should point out that the starker difference in attainment by month of birth which is observed for the cohorts born after 1957-58 and the contemporaneous shift towards academic qualifications are no coincidence. Although the legal distinction

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[26] The U.K. Census of Population is not available for this type of research, as anonymised microdata is stripped out of date of birth data. The Census does not collect information on income either.

[27] See Appendix B for further details on qualifications levels and type in the LFS.

between January and February was in place before the school leaving age was raised, individuals compelled to stay for the Summer term when the school leaving age was 15 were still far from reaching the bulk of examinations taking place a year later. In other words, the school leaving age of 15 generated a difference in the length of schooling between January and February-born pupils but gave the February-born only a very slight incentive to achieve a qualification by age 16.[28] We will return to discuss this at the end of the paper. Quite plausibly too, the advantage of February-born children appears to be greatest in the years immediately following the raising of the school leaving age. As attainment increased over time, the proportion of students compelled to gain a qualification as a result of the exit rules is seen to diminish.

The situation for women over the same period is similar apart from a few interesting remarks. As we can see in Figure IVb, women are less likely to attain a qualification through the vocational route over the full period, but this is so especially after the raising of the school leaving age. While we do not see any discrete jump in total male educational attainment after 1957-58 (but simply a substitution of academic versus vocational qualifications around a positive trend), in the case of women it is evident that the raising of compulsory schooling age to 16 leads to a positive shift in overall achievement. Furthermore, the attainment gap between those born in January and February appears, if anything, more evident for women than for men.

In Figures Va and Vb we examine the existence of a January-February gap at different levels of academic attainment for men and women, respectively. Here we observe how the January-February gap is concentrated at the lowest level of attainment, with some evidence of an effect for women at qualification level 2, which is also achieved by age 16. At first sight this suggests that compulsory schooling rules

[28] This would be consistent with Pischke [2004], who finds little evidence of an impact from a reduction in the length of the schooling for German individuals following a series of state-level reforms which were implemented to bring uniformity in the school year across German *laender* or states.

compel individuals at the lower end of the ability distribution to stay on for a few months and leave school with an academic certification, rather than inducing high ability individuals to stay on and reach the higher level academic grades.[29] The figures also indicate an increasing trend in attainment at levels 3 (A-levels) and 4 (degree) for those born after 1971. This coincides with a change in the examinations at age 16 which made these exams more accessible and also with a period of expansion of further and higher education on the supply-side.

Next, we examine in more detail the association between date of birth and educational performance at different levels of attainment by pooling together all the different cohorts from the year 1957-58 onwards and looking at the profile of attainment in each month relative to September. As shown in Figure VIa, we observe an increase in male attainment for the lowest qualification level from January to February but no substantial change at higher levels. Interestingly, attainment at the highest levels has a sharp declining profile over time, suggesting that relative age might have a substantial impact on qualification performance. The profile for women in Figure VIb reveals that attainment *at all levels* is higher in February with respect to January. This suggests that the "Easter rule" has considerable effects beyond the compulsory schooling period for women, although this is not the case for men.

It is not clear a priori why we observe this gender difference in the effect of month of birth. In particular, there is no reason why we should expect that the school leaving rule should have a stronger impact on women than men. In Table V we take a closer look at this effect by considering data windows of varying size around the 31st

[29] Notice that the raising of school leaving age reform has a strong impact on both levels 1 and 2 for men and women, though there is no significant spillover effect on individuals with higher levels of attainment as one would predict if medium ability/middle class individuals were expected to incur extra schooling in order to keep the distance with their immediate competitors. Chevalier *et al*. [2004] have interpreted this as evidence of a relatively small value of signalling in education, though it could be the case that individuals can still distinguish from each other using more detailed grade information than available to the econometrician.

January discontinuity point. These estimates show a significant impact on basic qualifications for men, but no impact on higher educational levels. As we saw in the graphical analysis, we find significant effects on all qualification levels only for women. The table makes it clear, however, that the impact of ATLE on the higher attainment levels is considerably smaller than that on attainment at lower grades. A possible explanation is that since the percentage of women who pursue a vocational route is much lower with respect to men the rule has a stronger bite on women, further inducing spillover effects which are not observed in the case of men.[30]

Figures VIIa and VIIb provide an indication of the impact of ATLE and its statistical significance by reporting the estimated coefficients for monthly dummies in a linear regression for the probability of achieving an academic qualification, leaving January as the baseline. Results for men and women are presented separately with their associated confidence intervals. The January-February discontinuity is clearly observed, but it is also worth noting the declining path of attainment away from the discontinuity point, particularly after February. This suggests that, were it not for the school exit rules, September-born individuals would experience a higher level of attainment, presumably as a result of the maturity effect which is partly dismissed by Angrist and Krueger [1991]. This highlights the risks of using age rank measures as instruments.

*IV.D. Robustness Checks: A Cross-Country Comparison*

A simple way to test the effect of the school exit rules is to compare the

[30] Blanchflower and Lynch [1994] show that at age 16, member of the National Child Development Study cohort born in March 1958, 63 per cent men were at work while 55 per cent of women. Of the former, 44 per cent were involved in apprenticeships while an extra 6 per cent had some informal on-the-job training. In contrast, only 8 per cent of women employees were participating in apprenticeships and 4 per cent informal job training. By age 23, 65 per cent of men had had some training while that figure was only 33 per cent for women.

January versus February attainment gap across groups of individuals that differ in their exposure to this rule. It is possible to establish comparisons between England and Wales on the one side and Scotland on the other because school leaving rules in Scotland establish no different exit dates for individuals born in January and February. This provides an even stronger test of the role of compulsory schooling rules in shaping the January-February gap.[31]

In Panel A of Table VI we present the sample probabilities of attaining an academic qualification for men and women born in January and February. We concentrate on the period after ROSLA (cohorts born in 1957-58 and after) in order to draw conclusions from a fairly homogeneous sample as far as compulsory schooling regulations are concerned. For both genders, the gap between February and January is positive and significant in England and Wales, whereas this is not the case for Scotland. The cross-country difference in differences suggests a 4 per cent extra probability of attaining a qualification as a result of compulsory schooling exit rules for men and women alike. Panel B further compares two months for which we are not aware of discontinuities induced by entry or exit rules in either England and Wales or Scotland. As we can see, we find no statistically significant differences in this case.

## V. LABOUR MARKET EFFECTS OF DATE OF BIRTH AND SCHOOLING IN ENGLAND AND WALES

[31] Given the evidence on Scotland's rules we have gathered so far, class composition in Scotland is determined in most cases by children born March to February of the following year. This however is not such a clear-cut separation as in England and Wales for our period of analysis because there is a degree of discretion across Local Education Authorities as to which dates of birth (months leading to February) grant parents the right to defer entry into primary school. Regarding school leaving at 16, Scotland provides a different exit rule discontinuity, whereby people born March to August must stay until the end of the summer term, whereas those born before the end of February must come back to school afterwards for the Autumn term, being allowed to drop out at Christmas. No relevant examinations appear to take place during the Autumn term.

In a labour market affected by frictions and rigidities, there is genuine interest in understanding whether and how enhancing people skills can improve their labour market prospects by making them more employable and more willing to participate in the first place. From this point of view, it is valid to ask whether compulsory schooling rules are an effective substitute or a more efficient incentive than welfare-to-work policies or adult-training programs.

Following Angrist and Krueger [1991], we therefore ask the question of whether observed differences in educational attainment by month of birth translate into long term labour market effects. Unlike them, we are in a position to test the implicit assumption that relative age effects can be excluded from the second stage regression. We concentrate on three key labour market outcomes: participation in the labour market, employment status and earnings. Since our preferred source, the Labour Force Survey, is not dense enough to provide a large sample of individual wages, we complement it with evidence from the New Earnings Survey.[32]

We begin this section by reporting the observed association between month of birth and participation and employment rates as we did for educational attainment. Figure VIII shows the effect of month of birth on the separate probabilities of being active and being employed for men and women. These figures display an increased attachment to work for February-born individuals, although these impacts appear to be imprecisely estimated and are not statistically significantly different from zero. Furthermore, there appears to be some evidence of relative age effects as summer-born men and women are shown to have lower attachment to work than their older peers.

[32] Earnings information is only elicited in the first and fifth (last) quarters from every individual, there is a high and unreliable level of imputation, and self-employed individuals do not report earnings. Nonetheless, we performed a parallel analysis on LFS wages but restricted the sample to individuals whose responses are not derived by proxy. This comes at the cost of a further reduction in efficiency but does yield more stable estimates in terms of sign and point estimates.

In Table VII we seek to obtain estimates of the impact of ATLE and educational attainment ($Q_i$) on participation ($P_i$), employment ($E_i$) and log earnings ($\log W_i$) by reproducing the RDD setting through the evaluation of different samples of individuals around the end of January threshold. The regressions, which are only run on individuals born a given number of months apart from the cut-off point, are specified as follows:

a) Impact of ATLE on the probability of having an academic qualification:

$$Q_{it} = \chi \cdot ATLE_i + \sum_c C_{ic}\vartheta_c + \sum_t T_{it}\eta_t + \zeta_{it} , \quad (9)$$

where $T_{it}$ is a vector of dummies for whether the observation for individual *i* corresponds to a specific year and quarter *t* of the Labour Force Survey;

b) Reduced form impact of ATLE on labour market outcomes:

$$\{P_{it}, E_{it}, \log W_{it}\} = \chi' \cdot ATLE_i + \sum_c C_{ic}\vartheta'_c + \sum_t T_{it}\eta'_t + \zeta'_{it} ; \quad (10)$$

c) OLS and IV estimates of the impact of getting an academic qualification on the aforementioned labour market outcomes:

$$\{P_{it}, E_{it}, \log W_{it}\} = \rho \cdot Q_{it} + \sum_c C_{ic}\alpha_c + \sum_t T_{it}\nu_t + \mu_{it} . \quad (11)$$

Our identification strategy is based on exogenous variation in length of compulsory schooling induced by the “Easter rule”. Table VII reports the coefficients and standard errors for the reduced form impacts of ATLE and the estimated impact of obtaining an academic qualification, $\rho$, obtained through OLS and IV.

Starting with the upper panel of Table VII, and taking men born between November and April (6 month window), we can see that the “Easter rule” has an impact of minus 0.0277 on educational attainment. We then investigate the direct effect on the probability of participation and employment and in each case compare OLS to IV results which use the entitlement variable as instrument. Looking at male participation and employment, we see that IV impacts are larger than OLS and statistically significant, although less precisely estimated. As we restrict the size of the data window, the reduced form and especially the IV coefficients become less precisely estimated, so that it becomes impossible to define a clear ranking between OLS and IV results.

As far as earnings are concerned, we find strong effects of having a qualification on hourly wages in the OLS regression, but IV results are usually insignificant and the estimated coefficients vary widely in magnitude across various data windows. On the other hand, for the matched JUVOS-NES sample we seem to find a stronger impact of ATLE on earnings. This impact is also more precisely estimated, by virtue of the larger sample size. Unfortunately, as this dataset does not offer information on educational qualifications, we cannot directly derive IV estimates in this case.[33]

The results for women are slightly more robust. We observe stronger effects of ATLE on educational attainment than what we find for men. There are negative and sometimes significant effects of ATLE on participation and employment probabilities, and positive although not very precisely estimated effects of an academic qualification on the outcome variables through IV regressions. The impacts on earnings are also usually stronger than what we find for men, in particular for the LFS sample. The overall picture suggests that, amongst women, being allowed to leave at Easter has a significant and negative effect on labour market attachment as well as on hourly wage rates.

We now try to investigate what happens when we use the full school cohort

[33] We refrain from presenting "split sample" IV estimates that we could derive separately from the LFS and JUVOS-NES samples [Angrist and Krueger, 1992]. This is because we cannot reproduce with complete certainty the sample selection process in the JUVOS-NES sample for the LFS, in order to ensure that numerator and denominator in the Wald estimate refer to the same sample.

while reproducing the features of the regression discontinuity design in a more parsimonious fashion. The only difference between this analysis and the previous specifications is that we now include a linear term in relative age within cohort, measured in months for the LFS and days for the matched JUVOS-NES sample.

The first set of results is presented in Table VIII. Here we look at specifications which first ignore and then include the relative age variable. In this way, we try to investigate whether maturity, physical development and other age-related characteristics may affect labour market outcomes through channels other than schooling. Failure to account for these effects can lead to biased estimates, with the sign of the bias being negative if individuals born later in the school cohort are more likely to have worse labour market performance for these unobserved reasons. Furthermore, in the first stage the impact of compulsory schooling for younger individuals could be considerably attenuated if relative age effects have a negative impact on attainment. As we saw in Section II.B, the fact that relative age and compulsory schooling might affect attainment in different directions could considerably weaken the link between the instrument and the probability of achieving an academic qualification, implying undesirable finite sample biases in the IV estimator.

As we can see in Table VIII, the IV estimate for men using Easter entitlement as an instrument but failing to control for relative age implies no effect of attainment on participation or employment. However, once we introduce relative age as an additional control, educational qualification becomes a significant predictor of labour market outcomes. In particular, we find that: (i) the impact of being allowed to leave at Easter on attainment becomes stronger, and (ii) the IV estimates for the impact on participation and employment are positive, statistically significant, and always above the corresponding OLS estimates. For women, the results are very similar and in some cases even more clear-cut.

The first stage of the IV estimates uses a single instrument and the F-tests suggest that this instrument has considerable explanatory power. We need to acknowledge, however, that misspecification may distort our estimates to the extent that, as we have seen from the figures, the impact of the "Easter rule" is not homogeneous over the whole period considered. Ideally we would like to provide separate estimates for impacts over calendar time and by cohort but because our sample size is not very large we prefer not to present these results.[34] We also have no background information to test whether the impacts are larger for individuals who come from less well-off families, so we cannot provide any evidence on the likely importance of heterogeneous effects.

Table IX produces estimates of the impact of educational qualifications on working hours and earnings for the LFS sample and the matched JUVOS-NES sample. The results suggest that ATLE has a negative but always insignificant impact on weekly hours and a negative impact on hourly wages. Using the LFS sample, the impact on wages is only statistically significant for women, while we find the opposite in the matched JUVOS-NES sample.[35] A possible explanation for this result is that the matched JUVOS-NES sample is not entirely representative of the female population. This is because this sample is essentially based on unemployment claimant records, which are notoriously found to underestimate the population of unemployed women in the U.K. (for more details on this see Appendix A).

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[34] We have also estimated specifications that allow for interactions between relative age and each school-cohort and instrument the attainment variable using the full set of interactions between school-cohort dummies and ATLE. The results are in line with and do not significantly add to the evidence presented here.

[35] In the matched JUVOS-NES sample we also find that ATLE leads to individuals holding a significantly lower occupational status. These results are available on request.

The advantage of the matched JUVOS-NES sample is that we have access to exact date of birth, which enables the empirical implementation of more accurate sample analogues for the regression discontinuity design. Indeed, using this dataset we can offer a more detailed graphical representation of the impacts of ATLE on wages. This is presented in Figures IXa and IXb, for men and women respectively. We first run a regression of log hourly wages on a set of dummies covering survey dates and birth cohorts. We then plot the predicted values from locally weighted regressions of the residuals on date of birth, separately for the ATLE and non-ATLE individuals.

The results for men suggest a significant impact of the "Easter rule" accompanied by a marked declining relationship between earnings and relative age. As we saw with the parametric results, in the female sample we do not find any significant difference in wage rates at the discontinuity point. It is however strange to see that hourly wages reach a minimum at the 31st January threshold. We believe that this could be related to the strong impact of the "Easter rule" on female labour force attachment and therefore on the actual probabilities of being included in the matched JUVOS-NES sample.

## VI. IMPACTS OF EXIT RULES PRE AND POST-ROSLA

In this section we investigate the way in which compulsory schooling rules lead to improved labour market outcomes through an examination of the combined impact of the "Easter rule" with the raising of the school leaving age to 16. An increase in the length of schooling is the common direct output to both policy interventions, but because of the way in which examinations are timed, there is some potential for disentangling length of schooling effects from certification effects. We argue this is important because the implications for policy can be very different depending on how these two effects contribute to explaining the impacts discussed earlier.

Under the general assumption made in models of optimal length of schooling, returns tend to be assumed flat or decreasing in order to meet the conditions for an internal solution. Compulsory schooling laws impose corner solutions for a subsection of the population who would have otherwise chosen a lower level of schooling. If this is the case, the three extra months of schooling implied by ATLE should be reflected into similar, possibly lower, estimated returns from schooling for the post-ROSLA cohorts. At the other extreme, a pure certification view of schooling would state that the "Easter rule" impacts on labour market performance should be dependent on individuals improving their qualification attainment. Given the increasing marginal costs of schooling, the extent of staying until the examination date should be higher the closer examinations are to the date the individual is forced to stay in at school. In both models impacts on qualification attainment will only be noticed when the additional spell of schooling overlaps with qualification award dates.[36]

To assess these hypotheses, we compare impacts of ATLE before and after ROSLA in Table X. We take the school cohorts 1948-49 to 1956-57 as representative of the pre-ROSLA period, as the "Easter rule" was in place for the first time for pupils born in 1948-49 and the 1956-57 cohort was the last to experience a minimum school leaving age of 15. The post-ROSLA period is restricted to the cohorts 1957-58 to 1966-67, to keep the comparison between the pre and post-ROSLA as balanced as possible.

[36] We do not seek to exploit differences in the labour market outcomes of individuals that only differ in their exposure to ROSLA because of the potential confounding influence of other cohort effects.

The results of this comparison are broadly similar for men and women and can be summarized as follows. Firstly, we see that the probability of achieving an academic qualification is lower for pupils entitled to leave at Easter both before and after the ROSLA, although the effect of the "Easter rule" is stronger in the post-ROSLA period. Secondly, the impact of ATLE on adult labour market outcomes is observed only for the cohorts born in the post-ROSLA period.

The presence of significant impacts of ATLE on the probability of achieving an academic qualification in the pre-ROSLA period challenges both of the theories discussed above. This is because, as we have seen before, for these cohorts the minimum school leaving age was 15 while examinations took place at age 16. Pupils staying on in education as a result of the "Easter rule" would still have to undergo another year in school before attempting the nationally recognized Certification of Secondary Education (CSE) exams.

One possibility is that there is a significant degree of error in the reporting and coding of academic qualifications in the LFS. Children at the lower ability track schools, i.e. those most likely to be affected by ATLE, usually took no examinations at all, being offered at best a School Leaving Certificate which only in a proportion of cases were backed by a regionally recognised examination boards. However, the Labour Force Survey measures the first level of academic achievement in terms of O-levels (age 16 qualifications for the high ability track) and CSEs (age 16 qualifications for middle/low ability track) only.[37] It is therefore possible that some pupils who had only obtained a School Leaving Certificate or equivalent report having a CSE. We believe this is the case because we find abnormally high levels of reported CSEs for individuals who went through education before these exams were implemented

[37] See Table B.I in Appendix B for details on the type of qualifications recorded by the LFS.

nationally.

Misreporting of educational qualifications pre-ROSLA would be broadly compatible with an interpretation of our estimated impacts as certification effects. Assuming that the type of qualifications valued in the labour market are CSEs and O-levels, and that a significant difference in the probability of achieving these qualifications emerges only with a minimum school leaving age of 16, we would observe significant labour market impacts of the "Easter rule" only in the post-ROSLA period. In the pre-ROSLA period, February-born children would only achieve 3 additional months of schooling and perhaps a less valued certification, such as a School Leaving Certificate, but none of this would translate into a higher labour market attachment or a higher level of earnings.

The above discussion basically treats the pre and post ROSLA estimation samples as reasonable approximations to a natural experiment. However, we should be cautious about the extent one can interpret these results as a robust test of competing theories about the role of education. For example, returns to qualifications by cohort increased considerably over this period. In fact, the years immediately preceding and following the ROSLA were a period of enormous change in the British labour market. The 1974 school cohort came to the labour market in the middle of a significant economic crisis which ultimately led to the transformation of industrial relations and production structures in the U.K. During the 1960s the numbers of unemployed people below 18 years of age were typically under 20,000, but by 1976 there were 199,000 school leavers registered as unemployed. In addition to ROSLA, these simultaneous changes must have reduced incentives to leave school and also reduced opportunities of benefiting from employer-provided training. Provision of vocational qualifications by employers also appears to have been affected, with

Government funded training schemes trying to compensate for the gap.[38]

We also make a final consideration of a more technical nature. The LATE interpretation of IV estimates forces us to consider compositional differences in the subpopulations induced to gain additional schooling in the pre and post-ROSLA periods. Looking at differences in the average attainment levels and differences in the impact on attainment, the post-ROSLA "compliers" cover a wider range of the population and appear to be drawn from a lower point in the hypothetical distribution of academic abilities. Following Imbens and Angrist [1994], our results could be interpreted as evidence of heterogeneity in treatment effects. Building on the results of Heckman and Vytlacyl [2000], we can say that our IV estimates pre and post-ROSLA represents different Local Average Treatment Effects as they involve integration of marginal treatment effects over different supports of the distribution of individual propensities to stay on in school. If this is the case, our results suggest that marginal treatment effects are higher for individuals with lower propensity towards schooling. This would be consistent with a certification effects hypothesis in that these are the individuals most likely to benefit from being pooled with higher ability peers.

## VII. CONCLUSIONS

This paper provides new evidence on the way in which compulsory schooling improves the educational attainment of individuals and how it may have a lasting impact on their performance in the labour market. We examine a natural experiment generated by idiosyncratic institutional features of the education system in England

[38] For example, Blanchflower and Lynch [1994] show that "in 1981, 34.3 per cent of employed males aged 16-19 were taking an apprenticeship at the date of interview. By 1989 this had fallen to 21.3 per cent of the employed males aged 16-19. For females, the decline was much smaller, but started from a significantly lower base."

and Wales which established discrete variation in school leaving dates for individuals within the same broad age group or cohort. We demonstrate that the impact of these rules on educational attainment was strongest in the years following the raising of the school leaving age to 16. We argue that the main effect of the policy is to compel individuals to reach the level at which they may take exams and leave school with a nationally recognized certification.

This natural experiment allows us to identify the genuine effect of compulsory schooling exit rules, netted out of relative age effects that contaminate estimates based on date of birth. Relative age has been discussed in the literature as a non-negligible advantage and we show that it has indeed a positive, independent effect not only on basic achievement but also at levels of education beyond the added period of compulsory schooling.

We also address the question of whether school leaving rules have had a significant impact on the labour market performance of men and women in England and Wales. Since we can safely assume that individuals born in the consecutive months of January and February are observationally identical in all aspects apart from the possible school leaving dates, we can identify the causal employment and participation effect of attaining a qualification on the "marginal" population of students who are compelled to remain in school through examinations. Our instrumental variable estimates suggest treatment effects of a higher magnitude than those obtained through OLS, which are also statistically significant except for the probability of being active for prime age males. This confirms the conventional finding that formal education is a more important driver of employment and participation decisions for women than it is for men. We leave for further work an analysis of features of participation and some of the mechanisms that can potentially explain this pattern, such as the timing of marriage and fertility behaviour.

In the light of our results, we argue that compulsory schooling plays an important role in fostering the opportunities of low-achieving individuals, provided the certification system is sufficiently well in tune with the additional schooling spells. Their joint influence should be considered more seriously in the design of measures intended to foster wider and longer participation through financial incentives, including initiatives such as the U.K.'s Education Maintenance Allowance. Its pilot evaluation [Middleton *et al*., 2003] estimated an increase in 8 percentage points retention rate for the group most affected by the policy, which is comparable to our results for the impact on the probability of leaving and not returning for exams (5 points for the whole population). Interestingly, that evaluation finds no impact on formal achievement.

Our findings raise some questions about the mechanisms through which the additional schooling induced by these policies leads to improved labour market outcomes. We find it difficult to square our results with a textbook human capital interpretation of returns to schooling and conclude that significant certification effects can be at stake. What we are able to conclude here is that policies aimed at increasing time in formal education should at least allow individuals to receive credit for the additional learning experienced. While there is no reason to assume that an investment model of schooling is the right framework for understanding teenage behaviour in relation to school, qualifications and skills, it is doubtful they will remain oblivious to the potential risk of not obtaining a labour market premium for the additional efforts.

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TABLE I
EFFECT OF SCHOOL LEAVING RULE FOR MALE AND FEMALE PUPILS
IN ENGLAND AND WALES

| | *Youth Cohort Study* *Male and Female pupils in England and Wales born December to March* | | |
|---|---|---|---|
| | Left school at Easter | Left at Easter and not back for exams | Obtained an academic qualification |
| Panel A – Effect of school leaving rule | | | |
| ATLE (born December-January) | 0.1173** (0.0149) | 0.0491** (0.0099) | -0.0277* (0.0122) |
| Panel B – Effect of school leaving rule by father's skill group | | | |
| ATLE (born December-January) & Skilled father | 0.0488** (0.0168) | 0.0239* (0.0103) | -0.0166 (0.0122) |
| ATLE (born December-January) & Unskilled father | 0.1421** (0.0158) | 0.0582** (0.0106) | -0.0317* (0.0132) |

Notes: Panel A reports the estimated effect of being allowed to leave at Easter (ATLE) for all pupils, while Panel B reports the effect of being allowed to leave at Easter interacted with father's socio economic group obtained from separate regressions. Estimates based on linear probability model. Controls include gender, father's socio economic group dummies, relative age ranking within cohort (1 for those born 1st September to 365 for those born 31st August) and its interactions with cohort dummies. Sample of individuals from cohorts 2 to 4 of the Youth Cohort Study (school years 1986-1987, 1987-88 and 1989-90) born from December to March only (13,869 observations). Huber-White heteroskedasticity robust standard errors shown in parentheses. Observations are weighted using survey sampling weights. Symbols: ** significant at 1% level; * significant at 5% level.

TABLE II
EFFECT OF SCHOOL LEAVING RULE ON THE PROBABILITY OF ACHIEVING AN ACADEMIC QUALIFICATION

| | *Youth Cohort Study* | |
|---|---|---|
| | *Male and Female pupils in England and Wales born December to March* | |
| | Qualification | Qualification |
| | *OLS* | *IV* |
| Left at Easter | -0.2161** | -0.2360* |
| | (0.0113) | (0.1010) |
| Left at Easter, and not back for exams | -0.5469** | -0.5637* |
| | (0.0203) | (0.2279) |

Notes: Estimated effect of leaving at Easter or leaving at Easter without coming back to take exams on the probability of achieving an academic qualification. Estimates based on separate linear probability regressions. IV estimates use entitlement to leave at Easter (born in December or January) as exclusion restriction. Controls include gender, father's socio economic group dummies, relative age ranking within cohort (1 for those born 1st September to 365 for those born 31st August) and its interactions with cohort dummies. Sample of individuals from cohorts 2 to 4 of the Youth Cohort Study (school years 1986-1987, 1987-88 and 1989-90) born from December to March only (13,869 observations). Huber-White heteroskedasticity robust standard errors shown in parentheses. Observations are weighted using survey sampling weights. Symbols: ** significant at 1% level; * significant at 5% level.

TABLE III
DIFFERENCES IN CHARACTERISTICS OF YCS SAMPLE BY DATE OF BIRTH
(DISCONTINUITY SAMPLE OF STUDENTS BORN DECEMBER TO MARCH)

| | *Youth Cohort Study* *Male and Female pupils in England and Wales born December to March* | | | |
|---|---|---|---|---|
| | Dec-Jan Average | Feb-Mar Average | Difference | Standard Error |
| *Outcome variables* | | | | |
| Left at Easter | 0.250 | 0.135 | 0.115** | 0.013 |
| Left at Easter, and not back for exams | 0.094 | 0.045 | 0.049** | 0.008 |
| Obtained qualification | 0.873 | 0.899 | -0.026** | 0.010 |
| Age 16/17: out of work | 0.081 | 0.091 | -0.010 | 0.009 |
| Age 16/17: training | 0.276 | 0.268 | 0.009 | 0.015 |
| Age 16/17: full-time job | 0.231 | 0.228 | 0.003 | 0.015 |
| Age 16/17: full-time education | 0.398 | 0.401 | -0.003 | 0.017 |
| *Background characteristics (Randomization tests)* | | | | |
| Female | 0.502 | 0.510 | -0.008 | 0.017 |
| Number of brothers | 1.050 | 1.067 | -0.017 | 0.034 |
| Number of sisters | 1.069 | 1.009 | 0.060 | 0.034 |
| Father works | 0.835 | 0.855 | -0.020 | 0.013 |
| Mother works | 0.645 | 0.672 | -0.027 | 0.016 |
| Non-white | 0.077 | 0.087 | -0.009 | 0.009 |
| Father professional | 0.172 | 0.157 | 0.015 | 0.015 |
| Father skilled | 0.288 | 0.295 | -0.007 | 0.018 |
| Attended any comprehensive school | 0.906 | 0.918 | -0.012 | 0.011 |
| Attended comprehensive school 11-16 | 0.284 | 0.266 | 0.017 | 0.015 |
| *Region dummies* | | | | |
| North | 0.065 | 0.071 | -0.007 | 0.009 |
| Yorkshire | 0.110 | 0.105 | 0.005 | 0.010 |
| North West | 0.130 | 0.142 | -0.012 | 0.012 |
| East Midlands | 0.084 | 0.076 | 0.007 | 0.010 |
| West Midlands | 0.117 | 0.120 | -0.003 | 0.011 |
| East Anglia | 0.042 | 0.043 | -0.001 | 0.007 |
| Greater London | 0.100 | 0.108 | -0.009 | 0.011 |
| South East | 0.202 | 0.194 | 0.008 | 0.014 |
| South West | 0.090 | 0.083 | 0.007 | 0.010 |
| Wales | 0.062 | 0.057 | 0.005 | 0.008 |

Notes: Predicted averages of selected variables for individuals born on both sides of entitlement threshold. Estimates based on linear probability model. Controls include cohort relative age ranking within cohort (1 for those born 1st September to 365 for those born 31st August) and its interactions with cohort dummies. Sample of individuals from cohorts 2 to 4 of the Youth Cohort Study (school years 1986-1987, 1987-88 and 1989-90) born from December to March only (13,869 observations). Huber-White heteroskedasticity robust standard errors shown in parentheses. Observations are weighted using YCS survey sampling weights. Symbols: ** significant at 1% level; * significant at 5% level.

TABLE IV
EXAMINATION ENTRIES AND ATTAINMENT OF 15/16 YEAR OLD PUPILS IN ENGLAND AROUND THE SHIFT TO A SINGLE SCHOOL LEAVING DATE

| | Percentage Pupils who entered for at least one GCSE/GNVQ examination | | | Percentage Pupils with at least one A*-G (pass) grades | | |
|---|---|---|---|---|---|---|
| School year | Men | Women | All | Men | Women | All |
| *1995-96* | *92.9* | *94.9* | *93.9* | *91.1* | *93.4* | *92.2* |
| *1996-97* | *93.1* | *95.0* | *94.0* | *91.2* | *93.5* | *92.3* |
| 1997-98 | 93.9 | 95.7 | 94.8 | 92.3 | 94.6 | 93.4 |
| 1998-99 | 94.1 | 95.9 | 95.0 | 93.0 | 95.0 | 94.0 |
| 1999-00 | 94.7 | 96.3 | 95.5 | 93.5 | 95.4 | 94.4 |
| 2000-01 | 94.9 | 96.6 | 95.7 | 93.5 | 95.6 | 94.5 |
| 2001-02 | 95.1 | 96.8 | 95.9 | 93.6 | 95.7 | 94.6 |
| 2002-03 | 95.3 | 97.1 | 96.2 | 93.7 | 95.9 | 94.8 |

Notes: Percentage of pupils entering for one or more GCSE/GNVQ examination and percentage of pupils achieving a pass grade on the number of pupils aged 15 on roll at the start of the academic year. The Easter school leaving date was in place for the cohort of students reaching age sixteen in 1995-96 and 1996-1997 school years (italics). Data from *Statistics of Education*, the Statistical Bulletin published by the U.K. Department for Education and Skills (*DfES)*, various years.

## TABLE V
EFFECT OF SCHOOL LEAVING RULE ON LEVEL OF ACADEMIC ATTAINMENT IN ENGLAND AND WALES, BY GENDER AND SIZE OF WINDOW AROUND ENTITLEMENT THRESHOLD

| | *Labour Force Survey* | | | | | |
|---|---|---|---|---|---|---|
| | Men | | | Women | | |
| | Birth month interval | | | Birth month interval | | |
| Academic qualification level attained | Nov-Apr | Dec-Mar | Jan-Feb | Nov-Apr | Dec-Mar | Jan-Feb |
| Level 1 or higher | -0.0277** | -0.0251** | -0.0219** | -0.0295** | -0.0301** | -0.0349** |
| | (0.0033) | (0.0040) | (0.0057) | (0.0029) | (0.0035) | (0.0051) |
| Level 2 or higher | -0.0053 | -0.0038 | -0.0014 | -0.0103** | -0.0150** | -0.0199** |
| | (0.0038) | (0.0046) | (0.0066) | (0.0035) | (0.0042) | (0.0060) |
| Level 3 or higher | 0.0028 | 0.0009 | -0.0020 | -0.0072* | -0.0097** | -0.0118* |
| | (0.0038) | (0.0047) | (0.0067) | (0.0036) | (0.0044) | (0.0062) |
| Level 4 or higher | 0.0020 | -0.0019 | -0.0025 | -0.0079** | -0.0089** | -0.0116** |
| | (0.0034) | (0.0042) | (0.0060) | (0.0032) | (0.0038) | (0.0054) |

Notes: Each cell reports the estimated effect of being allowed to leave at Easter (ATLE) on the probability of achieving an academic qualification by means of a linear probability model estimated separately for different levels of achievement and for different month intervals. Each regression also includes survey date dummies, school year dummies, and an intercept. Huber-White heteroskedasticity robust standard errors adjusted in order to take into account the presence of multiple observations for each individual shown in parentheses. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 (273,251 [77,065] men and 301,381 [82,385] women born Nov-Apr; 185,559 [52,289] men and 202,817 [55,549] women born Dec-Mar; 90,848 [25,597] men and 100,264 [27,564] women born Jan-Feb). Symbols: ** significant at 1% level; * significant at 5% level. Numbers in square brackets indicate unique individuals.

TABLE VI

DIFFERENCE IN DIFFERENCES ESTIMATES OF EFFECT OF SCHOOL LEAVING RULES ON ACADEMIC ATTAINMENT – ENGLAND AND WALES VERSUS SCOTLAND

| | *Labour Force Survey* | | | |
|---|---|---|---|---|
| | Men | | Women | |
| | England and Wales | Scotland | England and Wales | Scotland |
| Panel A - England and Wales cut-off rule: January-February | | | | |
| January | 0.7764 | 0.7279 | 0.8023 | 0.7294 |
| | (0.0040) | (0.0127) | (0.0037) | (0.0122) |
| February | 0.7984 | 0.7090 | 0.8373 | 0.7240 |
| | (0.0040) | (0.0143) | (0.0035) | (0.0128) |
| Difference Jan-Feb | -0.0219** | 0.0188 | -0.0350** | 0.0053 |
| (within country) | (0.0057) | (0.0191) | (0.0051) | (0.0177) |
| "Diff in diffs" | -0.0407** | | -0.0403** | |
| (E&W-Scotland) | (0.0199) | | (0.0184) | |
| Panel B - Robustness check : Comparison of two adjacent months with no discontinuity | | | | |
| Difference Apr-May | 0.0008 | -0.0122 | -0.0019 | 0.0095 |
| (within country) | (0.0055) | (0.0175) | (0.0048) | (0.0162) |
| Diff. in diff.s | 0.0130 | | -0.0114 | |
| E&W-Scotland | (0.0183) | | (0.0169) | |

Notes: This table reports: (i) probabilities of attaining an academic qualification by month of birth, country of residence and gender, (ii) within-country estimates of the school leaving rule impact, and (iii) difference in differences estimates across countries (standard deviations in parentheses). Both (ii) and (iii) include standard errors clustered in order to take into account the presence of multiple observations for each individual shown in parentheses. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 (England and Wales: 90,848 [25,597] men and 100,264 [27,564] women born Jan-Feb; 93,730 [26,540] men and 105,498 [28,654] women born Apr-May. Scotland: 9,867 [2,874] men and 11,403 [3,240] women born Jan-Feb; 10,263 [3,037] men and 11,617 [3,325] women born Apr-May). In England and Wales the school leaving rule establishes that those born in January can drop out at Easter whereas those born in February must stay until the end of the summer term (Panel A). Scottish rules are more complicated and are intertwined with school entry dates, but involve no discontinuity between these two months. Panel B provides a similar comparison for a pair of consecutive months not affected by any schooling cut-off rule in either of the two countries. Symbols: ** significant at 1% level; * significant at 5% level. Numbers in square brackets indicate unique individuals.

## TABLE VII

EFFECT OF ACHIEVING AN ACADEMIC QUALIFICATION ON PARTICIPATION AND EMPLOYMENT IN ENGLAND AND WALES BY GENDER AND SIZE OF WINDOW WIDTH AROUND THE CUT-OFF POINT IN ENGLAND AND WALES

| | | *Labour Force Survey* | | | | | | | *Labour Force Survey - employees, no proxy respondents* | | | *JUVOS-NES* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Acad. qual. | Participation | | | Employment | | | Log hourly wage | | | Log hourly wage |
| | | *OLS* | *OLS* | *OLS* | *IV* | *OLS* | *OLS* | *IV* | *OLS* | *OLS* | *IV* | *OLS* |
| Panel A - Men | | | | | | | | | | | | |
| Nov-Apr | Acad. qual. | | | 0.0933** | 0.1377** | | 0.1671** | 0.2652** | | 0.3849** | -0.1467 | |
| | | | | (0.0028) | (0.0597) | | (0.0036) | (0.0843) | | (0.0080) | (0.2871) | |
| | ATLE | -0.0277** | -0.0038* | | | -0.0074** | | | 0.0031 | | | -0.0055 |
| | | (0.0033) | (0.0017) | | | (0.0024) | | | (0.0060) | | | (0.0070) |
| Dec-Mar | Acad. qual. | | | 0.0940** | 0.1532 | | 0.1685** | 0.2330* | | 0.3822** | 0.0331 | |
| | | | | (0.0034) | (0.0797) | | (0.0045) | (0.1124) | | (0.0099) | (0.3592) | |
| | ATLE | -0.0252** | -0.0039 | | | -0.0059* | | | -0.0007 | | | -0.0164* |
| | | (0.0040) | (0.0020) | | | (0.0029) | | | (0.0073) | | | (0.0085) |
| Jan-Feb | Acad. qual. | | | 0.0940** | 0.1915 | | 0.1677** | 0.1294 | | 0.3831** | 1.2418 | |
| | | | | (0.0050) | (0.1340) | | (0.0064) | (0.1830) | | (0.0140) | (1.3861) | |
| | ATLE | -0.0220** | -0.0042 | | | -0.0029 | | | -0.0106 | | | -0.0102 |
| | | (0.0056) | (0.0030) | | | (0.0042) | | | (0.0106) | | | (0.0118) |
| Panel B - Women | | | | | | | | | | | | |
| Nov-Apr | Acad. qual. | | | 0.2523** | 0.1134 | | 0.2781** | 0.1755 | | 0.4062** | 0.4686* | |
| | | | | (0.0045) | (0.1033) | | (0.0046) | (0.1091) | | (0.0068) | (0.2310) | |
| | ATLE | -0.0295** | -0.0033 | | | -0.0052 | | | -0.0110* | | | -0.0110 |
| | | (0.0029) | (0.0031) | | | (0.0033) | | | (0.0056) | | | (0.0076) |
| Dec-Mar | Acad. qual. | | | 0.2554** | 0.3665** | | 0.2815** | 0.4360** | | 0.4136** | 0.3783 | |
| | | | | (0.0054) | (0.1215) | | (0.0055) | (0.1295) | | (0.0085) | (0.2935) | |
| | ATLE | -0.0301** | -0.0110** | | | -0.0131** | | | -0.0085 | | | -0.0160 |
| | | (0.0035) | (0.0038) | | | (0.0040) | | | (0.0068) | | | (0.0094) |
| Jan-Feb | Acad. qual. | | | 0.2544** | 0.1790 | | 0.2784** | 0.2662 | | 0.4049** | 0.4461 | |
| | | | | (0.0077) | (0.1503) | | (0.0078) | (0.1586) | | (0.0122) | (0.2898) | |
| | ATLE | -0.0349** | -0.0063 | | | -0.0093 | | | -0.0143 | | | -0.0163 |
| | | (0.0051) | (0.0054) | | | (0.0057) | | | (0.0096) | | | (0.0135) |

Notes: Estimates of impact of academic qualification and Easter leave entitlement (ATLE) on long term educational and labour market outcomes. Estimates based on linear probability model. Controls include survey and cohort dummies and an intercept. Huber-White heteroskedasticity robust standard errors adjusted for clustering within parentheses. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Calculations on participation and employment are based on a sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 (273,251 [77,065] men and 301,381 [82,385] women born Nov-Apr; 185,559 [52,289] men and 202,817 [55,549] women born Dec-Mar; 90,848 [25,597] men and 100,264 [27,564] women born Jan-Feb). Calculations on log hourly wages are based on a sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 which does not include proxy respondents and is restricted to employees (36,069 [29,745] men and 46,885 [37,423] women born Nov-Apr; 24,477 [20,199] men and 31,582 [25,199] women born Dec-Mar; 12,059 [9,933] men and 15,674 [12,509] women born Jan-Feb). Additional calculations on hourly wages are based on the matched JUVOS-NES (1993-2003) sample of individuals born September 1957 to August 1975 (105,254 [19,179] men and 87,369 [17,184] women born Nov-Apr; 72,003 [13,091] men and 58,670 [11,548] women born Dec-Mar; 35,140 [6,405] men and 28,607 [5,582] women born Jan-Feb). Symbols: ** significant at 1% level; * significant at 5% level. Numbers in square brackets indicate unique individuals.

TABLE VIII

EFFECT OF LEAVING RULES AND ACHIEVING AN ACADEMIC QUALIFICATION ON LONG TERM LABOUR MARKET OUTCOMES IN ENGLAND AND WALES

| | *Labour Force Survey* | | | | | | |
|---|---|---|---|---|---|---|---|
| | Academic qual. | Participation | | | Employment | | |
| | *OLS* | *OLS* | *OLS* | *IV* | *OLS* | *OLS* | *IV* |
| Panel A - Men | | | | | | | |
| ATLE | -0.0234** | 0.0002 | | | -0.0004 | | |
| | (0.0023) | (0.0012) | | | (0.0017) | | |
| *F-test* | *99.27* | | | | | | |
| Academic qual. | | | 0.0914** | -0.0091 | | 0.1660** | 0.0152 |
| | | | (0.0020) | (0.0522) | | (0.0026) | (0.0732) |
| ATLE | -0.0308** | -0.0058* | | | -0.0106** | | |
| | (0.0045) | (0.0023) | | | (0.0033) | | |
| *F-test* | *46.51* | | | | | | |
| Academic qual. | | | 0.0915** | 0.1887* | | 0.1662** | 0.3432** |
| | | | (0.0020) | (0.0761) | | (0.0026) | (0.1076) |
| Relative age | -0.0012* | -0.0010** | -0.0005** | -0.0008** | -0.0017** | -0.0008** | -0.0013** |
| | (0.0006) | (0.0003) | (0.0002) | (0.0003) | (0.0005) | (0.0002) | (0.0004) |
| Panel B - Women | | | | | | | |
| ATLE | -0.0245** | 0.0024 | | | 0.0013 | | |
| | (0.0021) | (0.0022) | | | (0.0024) | | |
| *F-test* | *136.60* | | | | | | |
| Academic qual. | | | 0.2539** | -0.0999 | | 0.2789** | -0.0550 |
| | | | (0.0032) | (0.0939) | | (0.0032) | (0.0985) |
| ATLE | -0.0350** | -0.0113** | | | -0.0134** | | |
| | (0.0040) | (0.0043) | | | (0.0046) | | |
| *F-test* | *75.97* | | | | | | |
| Academic qual. | | | 0.2542** | 0.3223** | | 0.2792** | 0.3838** |
| | | | (0.0032) | (0.1195) | | (0.0032) | (0.1271) |
| Relative age | -0.0018** | -0.0023** | -0.0016** | -0.0017** | -0.0025** | -0.0015** | -0.0018** |
| | (0.0006) | (0.0006) | (0.0003) | (0.0004) | (0.0007) | (0.0003) | (0.0005) |

Notes: Estimates of impact of academic qualification and Easter leave entitlement (ATLE) on long term educational and labour market outcomes. Estimates based on linear probability model. F-test reports the standard statistic to assess potential weakness of the excluded instrument -entitlement to leave at Easter- on its impact on the probability of achieving an academic qualification. The upper sections of panels A and B do not include a linear control for relative birth month order position in the school cohort, other controls include survey and cohort dummies and an intercept. Huber-White heteroskedasticity robust standard errors adjusted for clustering within parentheses. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 (546,861 [154,489] men and 605,162 [164,957] women). Symbols: ** significant at 1% level; * significant at 5% level. Numbers in square brackets indicate unique individuals.

## TABLE IX
EFFECT OF LEAVING RULES AND ACHIEVING AN ACADEMIC QUALIFICATION ON LABOUR MARKET OUTCOMES OF EMPLOYEES IN ENGLAND AND WALES

| | *Labour Force Survey – employees, no proxy respondents* | | | | | *JUVOS-NES* |
|---|---|---|---|---|---|---|
| | Academic qual. | Log weekly hours | Log hourly wage | | | Log hourly wage |
| | *OLS* | *OLS* | *OLS* | *OLS* | *IV* | *OLS* |
| Panel A - Men | | | | | | |
| ATLE | -0.0212** | -0.0058 | -0.0062 | | | -0.0240** |
| | (0.0055) | (0.0038) | (0.0084) | | | (0.0095) |
| *F-test* | *14.97* | | | | | |
| Academic qual. | | | | 0.3925** | 0.2931 | |
| | | | | (0.0057) | (0.3844) | |
| Relative age | -0.0005 | -0.0008 | -0.0029* | -0.0029** | -0.0027** | -0.0617** |
| | (0.0008) | (0.0006) | (0.0012) | (0.0006) | (0.0010) | (0.0160) |
| Panel B - Women | | | | | | |
| ATLE | -0.0264** | -0.0075 | -0.0196* | | | -0.0172 |
| | (0.0046) | (0.0082) | (0.0078) | | | (0.0106) |
| *F-test* | *32.97* | | | | | |
| Academic qual. | | | | 0.4178** | 0.7438* | |
| | | | | (0.0050) | (0.2915) | |
| Relative age | -0.0004 | -0.0003 | -0.0025* | -0.0013* | -0.0022* | -0.0261 |
| | (0.0007) | (0.0012) | (0.0011) | (0.0006) | (0.0010) | (0.0181) |

Note: Estimates of impact of academic qualification and Easter leave entitlement (ATLE) on long term educational and labour market outcomes. Estimates based on linear probability model. Controls include survey and cohort dummies and an intercept. Huber-White heteroskedasticity robust standard errors adjusted for clustering within parentheses. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Calculations on qualification levels, log total weekly hours and log gross hourly wages are based on a sample of employees from the Labour Force Survey (1993-2003) born September 1957 to August 1975 which does not include proxy respondents (72,278 observations, 59,448 individuals for men and 93,446 observations, 74,611 individuals for women). Additional calculations on hourly wages based on the matched JUVOS-NES (1993-2003) sample of individuals born September 1957 to August 1975 (211,227 [38,441] men and 175,404 [34,551] women). Symbols: ** significant at 1% level; * significant at 5% level. Numbers in square brackets indicate unique individuals.
Source: ONS

TABLE X

COMPARISON OF SCHOOL LEAVING RULES IMPACT
FOR DIFFERENT SCHOOL LEAVING AGE LEVELS

| | | Men | | | | Women | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | Cohorts 1948-49-1956-57 School leaving age 15 | | Cohorts 1957-58-1966-67 School leaving age 16 | | Cohorts 1948-49-1956-57 School leaving age 15 | | Cohorts 1957-58-1966-67 School leaving age 16 | |
| | Dependent variable | Impact of ATLE | Sample Average | Impact of ATLE | Sample Average | Impact of ATLE | Sample Average | Impact of ATLE | Sample Average |
| *Labour Force Survey* | Any academic qualification | -0.0292** (0.0076) | 0.56 | -0.0373** (0.0055) | 0.76 | -0.0278** (0.0075) | 0.57 | -0.0464** (0.0050) | 0.80 |
| | Any qualification | -0.0097 (0.0052) | 0.85 | -0.0199 ** (0.0038) | 0.89 | -0.0281** (0.0064) | 0.74 | -0.0302** (0.0043) | 0.86 |
| | Participation | 0.0034 (0.0039) | 0.92 | -0.0088** (0.0028) | 0.94 | -0.0011 (0.0057) | 0.80 | -0.0105* (0.0051) | 0.75 |
| | Employment | 0.0032 (0.0048) | 0.87 | -0.0133** (0.0038) | 0.88 | -0.0028 (0.0060) | 0.76 | -0.0112* (0.0054) | 0.72 |
| *Labour Force Survey - employees, no proxy respondents* | Log hourly wages | -0.0046 (0.0130) | 1.79 | -0.0017 (0.0107) | 1.74 | -0.0096 (0.0113) | 1.38 | -0.0163 (0.0100) | 1.41 |
| *JUVOS-NES* | Log hourly wages (nominal) | -0.0181 (0.0184) | 2.13# | -0.0303** (0.0137) | 2.17# | -0.0104 (0.0191) | 1.87# | -0.0199 (0.0146) | 1.93# |

Notes: Estimates of impact of being allowed to leave at Easter (ATLE) on long term educational and labour market outcomes. Estimates based on linear probability model. Controls include dummies for all possible survey date and cohort combinations. Huber-White heteroskedasticity robust standard errors adjusted for clustering within parentheses. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Calculations on qualification levels, employment and participation are based on a sample of individuals from the Labour Force Survey (1993-2003) born September 1948 to August 1967 (Cohorts 1948-49-1956-57: 287,866 [73,752] men and 299,646 [75,508] women; Cohorts 1957-58-1966-67: 362,924 [98,478] men and 397,428 [104,204] women). Calculations on log hourly wages are based on a sample of employees from the Labour Force Survey (1993-2003) born September 1948 to August 1967 which does not include proxy respondents (Cohorts 1948-49-1956-57: 34,374 [27,712] men and 46,594 [36,590] women; Cohorts 1957-58-1966-67: 45,585 [37,401] men and 60,144 [47,689] women). Additional calculations on hourly wages are based on the matched JUVOS-NES (1993-2003) sample of individuals born September 1948 to August 1967 (Cohorts 1948-49-1956-57: 70,561 [11,888] men and 60,094 [10,160] women; Cohorts 1957-58-1966-67: 134,483 [22,161] men 111,459 [20,477] women). Hourly wage rates have been deflated to 1986 UK GBP in the Labour Force Survey sample, while they are nominal rates in the matched JUVOS-NES sample. Symbols: ** significant at 1% level; * significant at 5% level. Numbers in square brackets indicate unique individuals.
Source: ONS

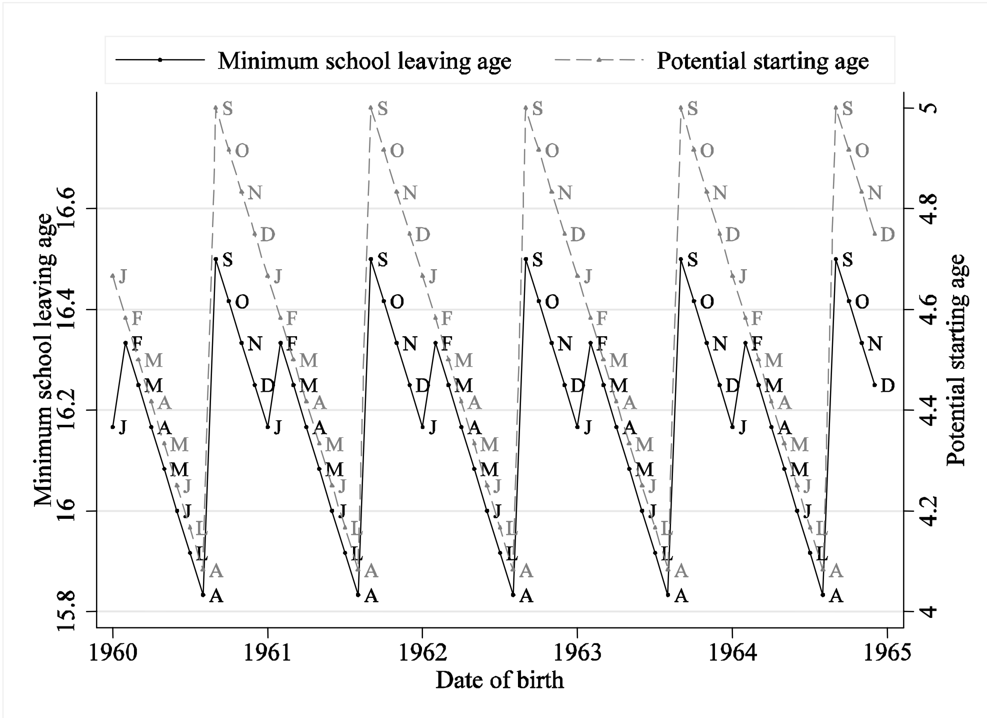



FIGURE I

POTENTIAL STARTING AGE AND MINIMUM SCHOOL LEAVING AGE BY MONTH OF BIRTH

Notes: Stylised depiction of potential starting age and minimum school leaving age according to the policy rules in operation in England and Wales throughout the period between 1973-74 and 1996-97. A uniform single entry date is assumed for presentational simplicity and an arbitrary range of dates of birth is selected for display. Easter is counted as from April and Summer vacation as July. (Example: the minimum school leaving age for a February born child will be 16+4/12). A three-month gap in compulsory schooling arises between January and February born children in the same school cohort. A bigger gap is found between consecutive August and September born children, but an even larger gap is found in potential starting age for these, and subsequently in relative age within cohort.

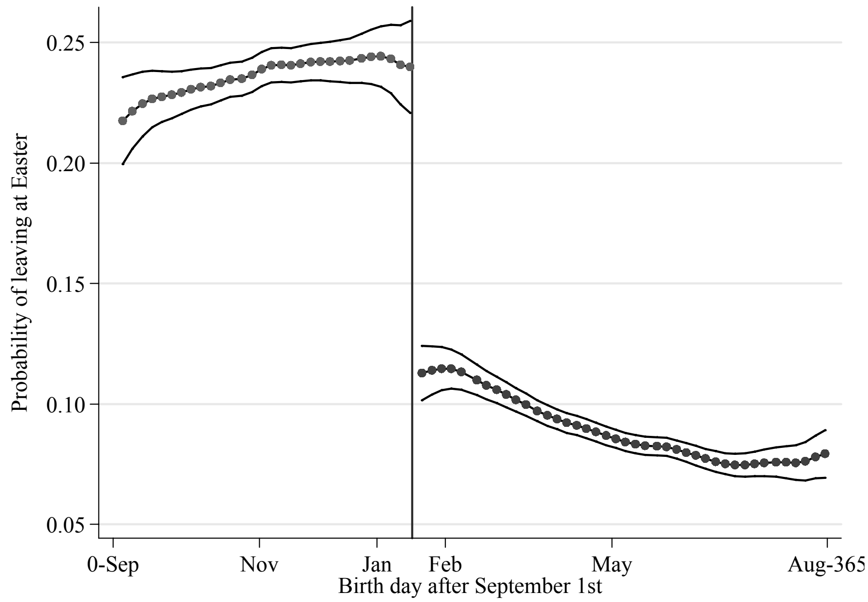



FIGURE IIa
PROBABILITY OF LEAVING AT EASTER BY DATE OF BIRTH IN ENGLAND AND WALES

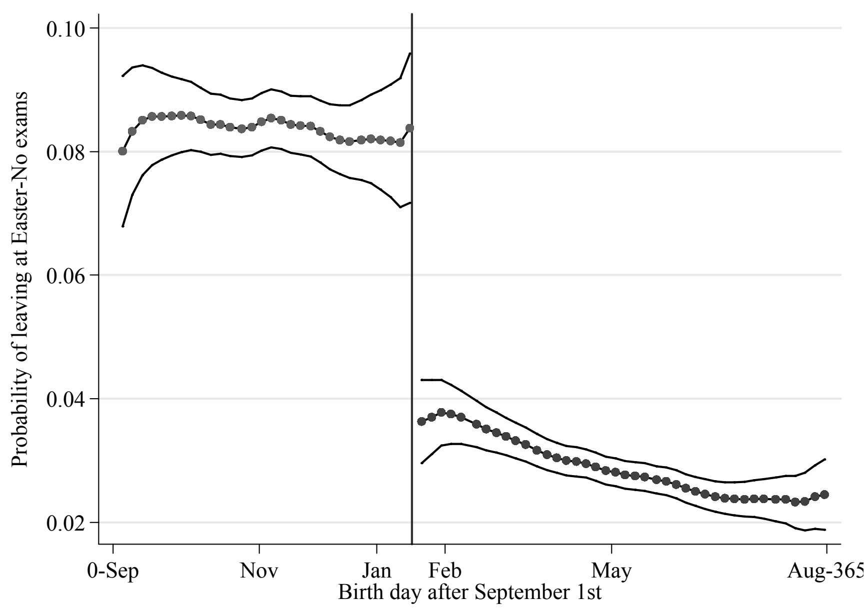



FIGURE IIb
PROBABILITY OF LEAVING AT EASTER AND NOT RETURNING TO TAKE EXAMS
BY DATE OF BIRTH IN ENGLAND AND WALES

Notes: Figure IIa shows the smoothed mean probability of leaving at Easter by birth date of birth for men and women in England and Wales with associated confidence intervals. Figure IIb shows the smoothed mean probability of leaving at Easter and not returning to take exams by date of birth for men and women in England and Wales with associated confidence intervals. Observations are weighted using survey sampling weights. Estimates derived using “running”, a locally weighted regression routine for Stata®. Sample of individuals from cohorts 2-4 of the Youth Cohort Study (42,747 observations)

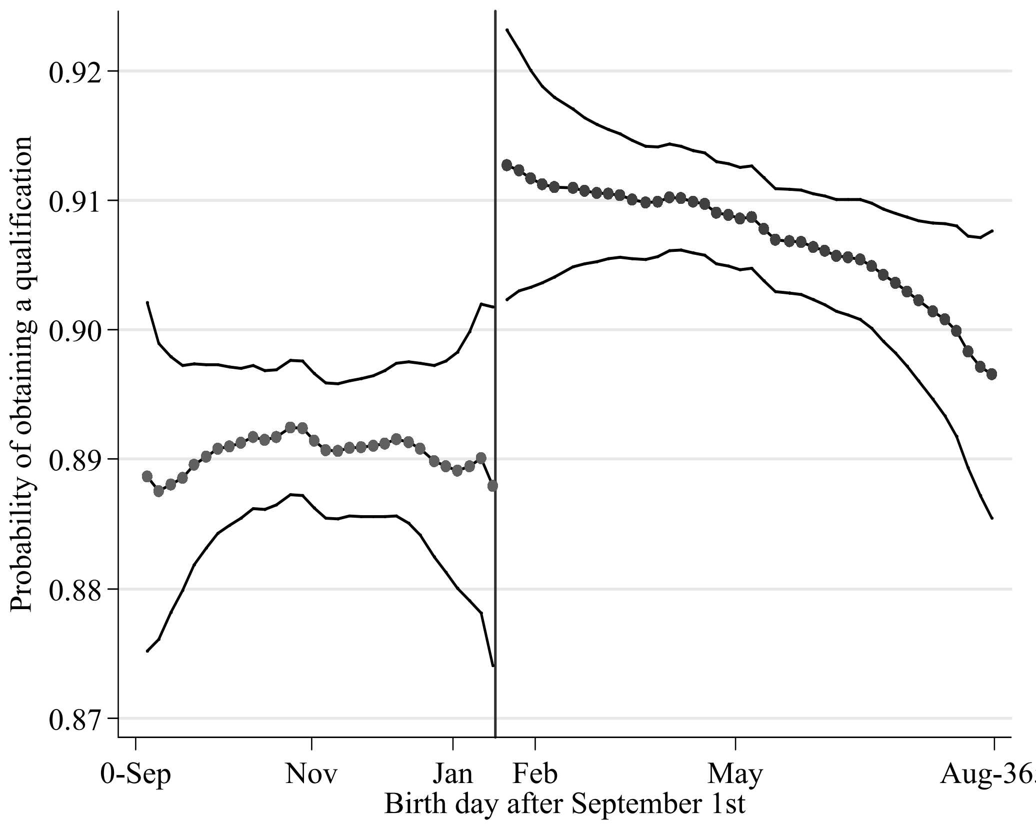



FIGURE III
PROBABILITY OF OBTAINING AN ACADEMIC QUALIFICATION BY DATE OF BIRTH IN ENGLAND AND WALES

Notes: Smoothed mean probability of obtaining an academic qualification by date of birth for men and women in England and Wales with associated confidence intervals. Estimates derived using "running", a locally weighted regression routine for Stata®. Observations are weighted using survey sampling weights. Sample of individuals from cohorts 2-4 of the Youth Cohort Study (42,747 observations)

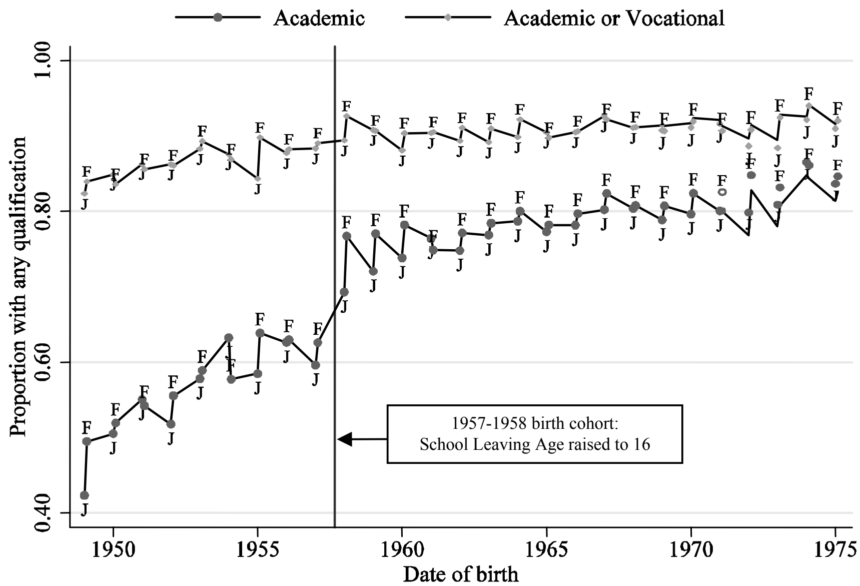



FIGURE IVa
PROPORTION OF MEN WITH ANY QUALIFICATION IN ENGLAND AND WALES, BY DATE OF BIRTH

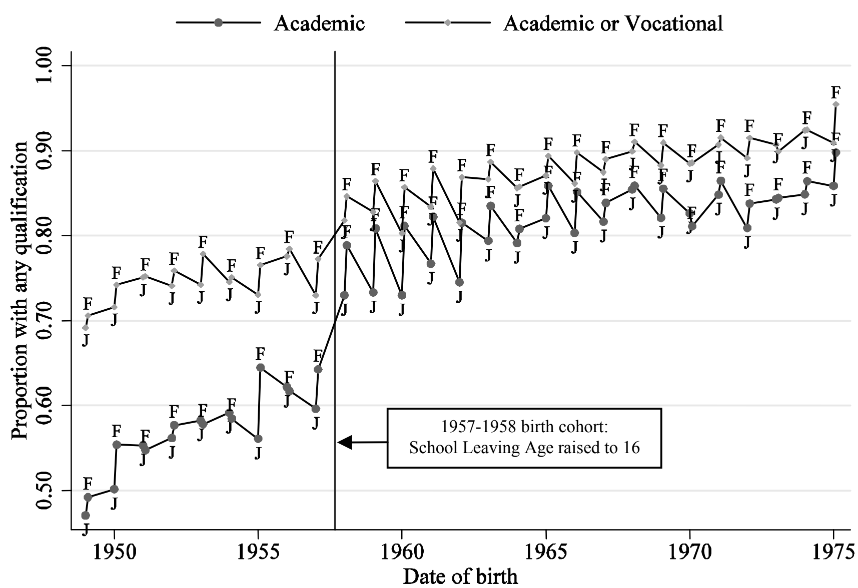



FIGURE IVb
PROPORTION OF WOMEN WITH ANY QUALIFICATION IN ENGLAND AND WALES, BY DATE OF BIRTH

Notes: Cohort profile of men and women who attain any academic qualification or any academic or vocational qualification level shown for sample born in January (J) or February (F). Sample of individuals from the Labour Force Survey (1993-2003) born September 1948 to August 1975 (834,727 [228,241] men and 904,808 [240,465] women). Numbers in square brackets indicate unique individuals.

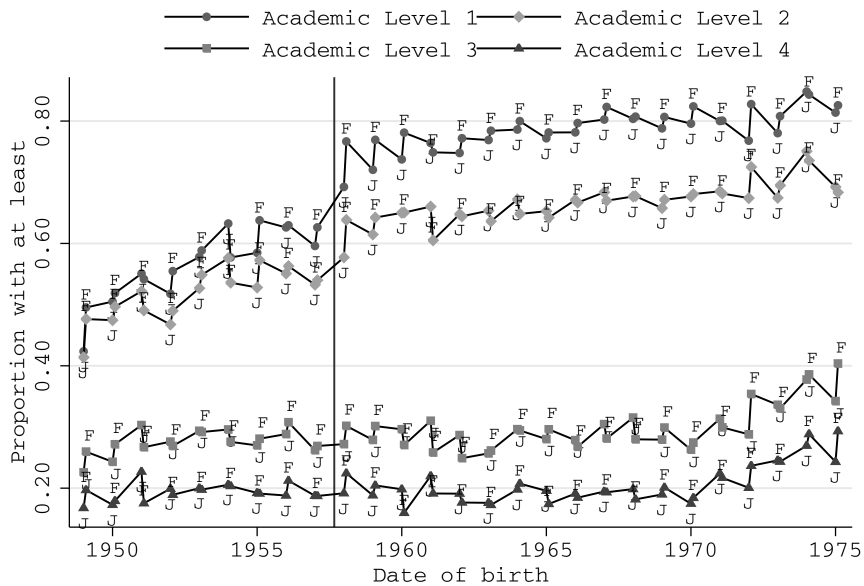



FIGURE Va
PROPORTION OF MEN ATTAINING AN ACADEMIC QUALIFICATION IN ENGLAND AND WALES, BY DATE OF BIRTH AND LEVEL

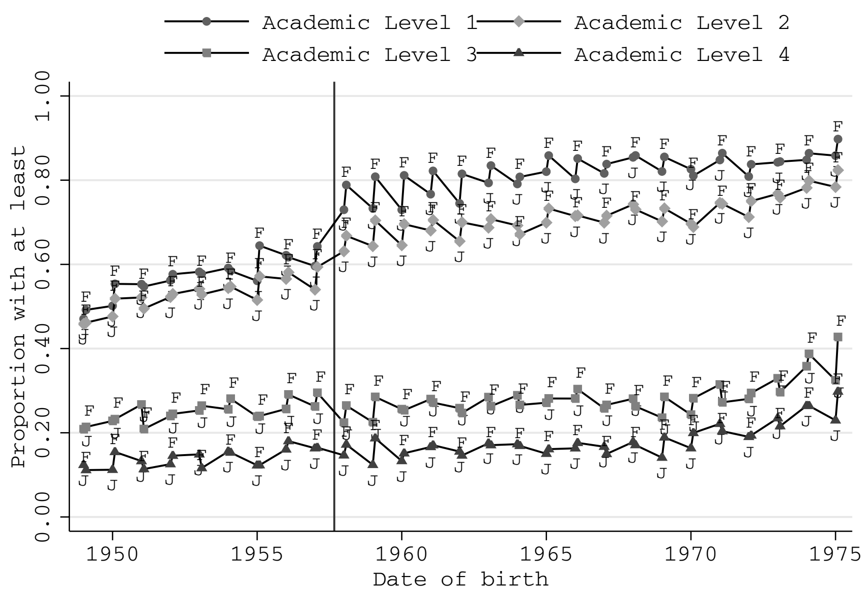



FIGURE Vb
PROPORTION OF WOMEN ATTAINING AN ACADEMIC QUALIFICATION IN ENGLAND AND WALES, BY DATE OF BIRTH AND LEVEL

Notes: Cohort profile of the proportion of men and women who have at least attained each type of academic qualification level for sample born in January (J) or February (F). Sample of individuals from the Labour Force Survey (1993-2003) born September 1948 to August 1975 (834,727 [228,241] men and 904,808 [240,465] women). Numbers in square brackets indicate unique individuals.

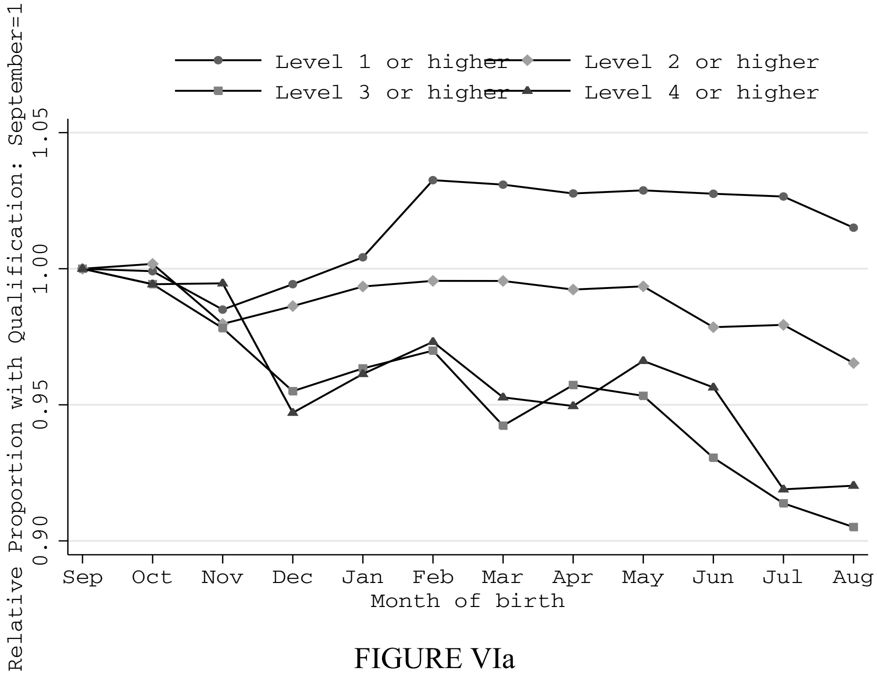



FIGURE VIa
RELATIVE PROPORTION OF MEN ATTAINING AN ACADEMIC QUALIFICATION IN ENGLAND AND WALES, BY MONTH OF BIRTH AND LEVEL

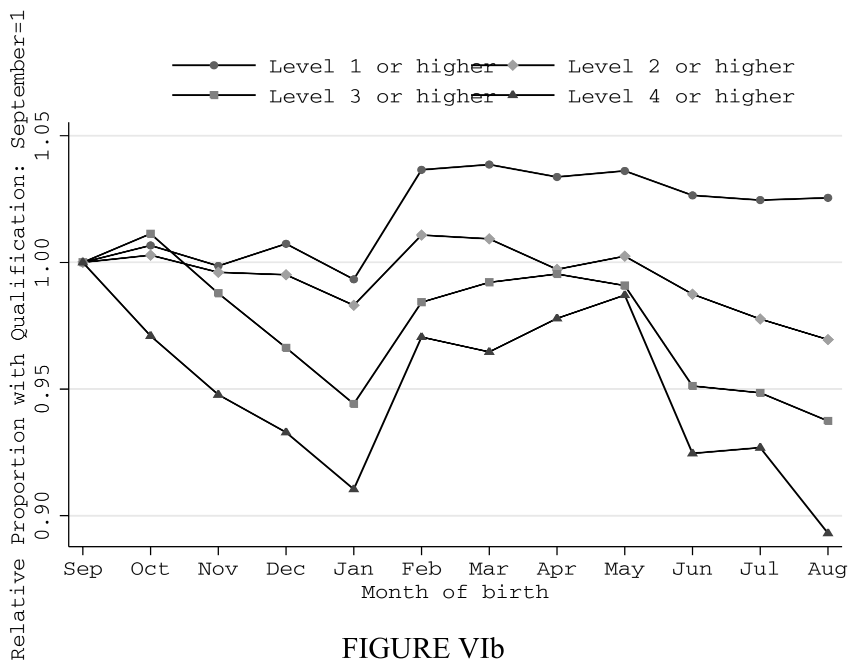



FIGURE VIb
RELATIVE PROPORTION OF WOMEN ATTAINING AN ACADEMIC QUALIFICATION IN ENGLAND AND WALES, BY MONTH OF BIRTH AND LEVEL

Notes: School year profile of the relative proportion of men and women who have at least attained the academic qualification level shown. Sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 (546,861 [154,489] men and 605,162 [164,957] women). Numbers in square brackets indicate unique individuals.

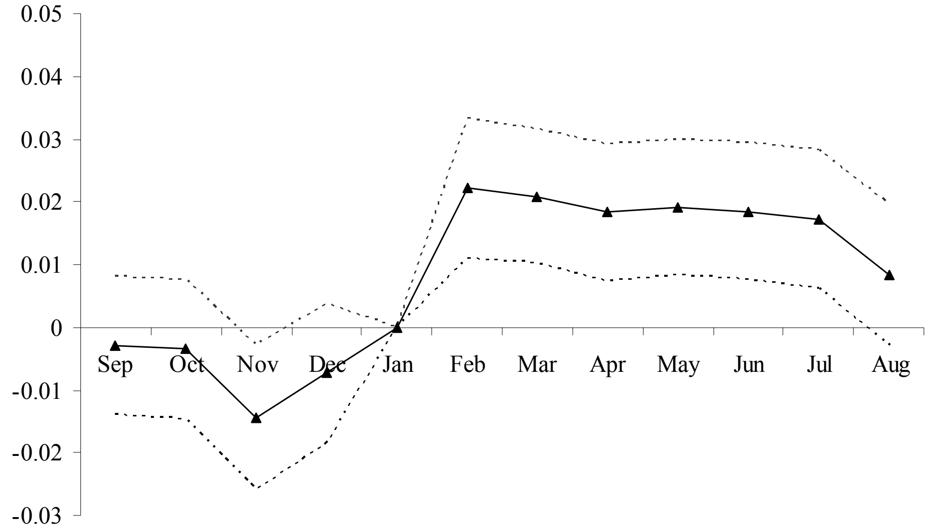



FIGURE VIIa
EFFECT OF MONTH OF BIRTH ON THE PROBABILITY OF ACHIEVING AN ACADEMIC QUALIFICATION FOR MEN IN ENGLAND AND WALES

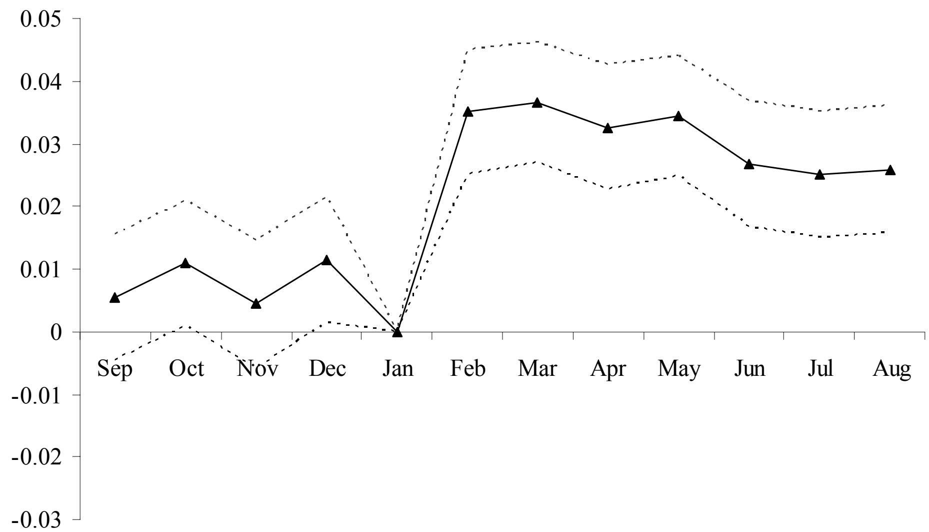
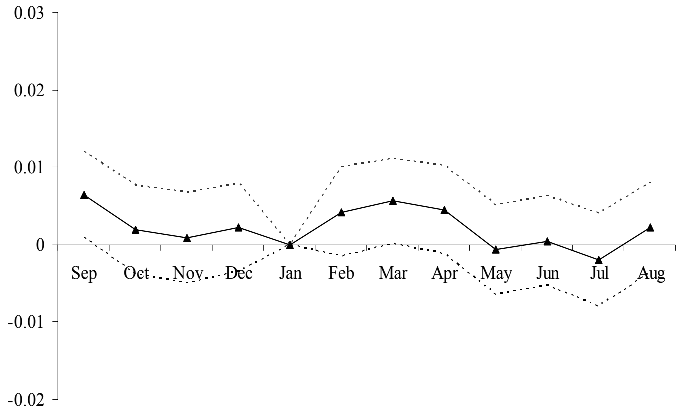
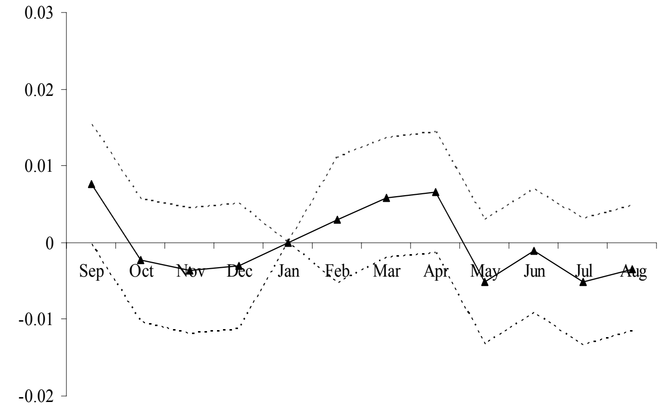



FIGURE VIIb
EFFECT OF MONTH OF BIRTH ON THE PROBABILITY OF ACHIEVING AN ACADEMIC QUALIFICATION FOR WOMEN IN ENGLAND AND WALES

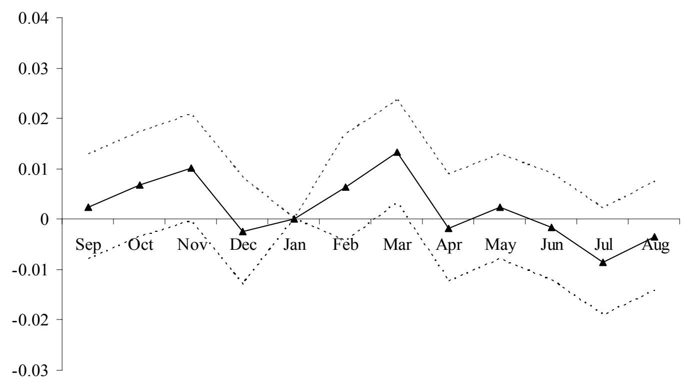
Notes: Estimated effect of month of birth on the probability of achieving an academic qualification (solid line). Effects are estimated by a linear probability model through OLS. Confidence intervals (dotted line) obtained using Huber-White heteroskedasticity robust standard errors adjusted in order to take into account the presence of multiple observations for each individual. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Except for the month dummies, the model includes survey date dummies, school year dummies, and an intercept. The omitted month is January. Sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 (546,861 [154,489] men and 605,162 [164,957] women). Numbers in square brackets indicate unique individuals.



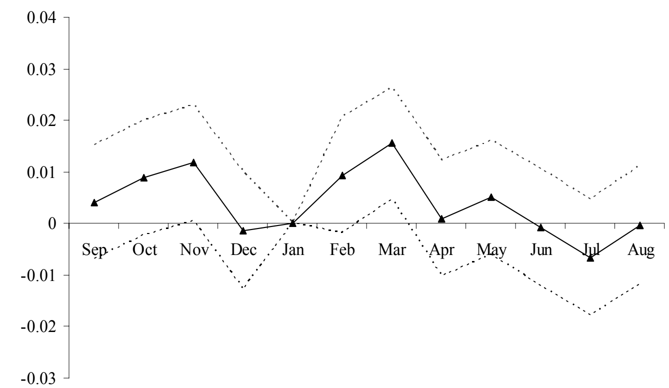

Men – Labour market participation probability




Men – Employment probability




Women – Labour market participation probability




Women – Employment probability

FIGURE VIII

EFFECT OF MONTH OF BIRTH ON THE PROBABILITY OF PARTICIPATION AND EMPLOYMENT

Notes: Estimated effect of month of birth on the probability of participating/being employed in the labour market (solid line). Effects are estimated by a linear probability model through OLS. Confidence intervals (dotted line) obtained using Huber-White heteroskedasticity robust standard errors adjusted in order to take into account the presence of multiple observations for each individual shown by discontinuous lines. Observations are weighted by the inverse probability that an individual belonging to a certain year-month cell is included in the estimation sample. Except for the month dummies, the model includes survey date dummies, school year dummies, and an intercept. The omitted month is January. Sample of individuals from the Labour Force Survey (1993-2003) born September 1957 to August 1975 (546,861 [154,489] men and 605,162 [164,957] women). Numbers in square brackets indicate unique individuals.

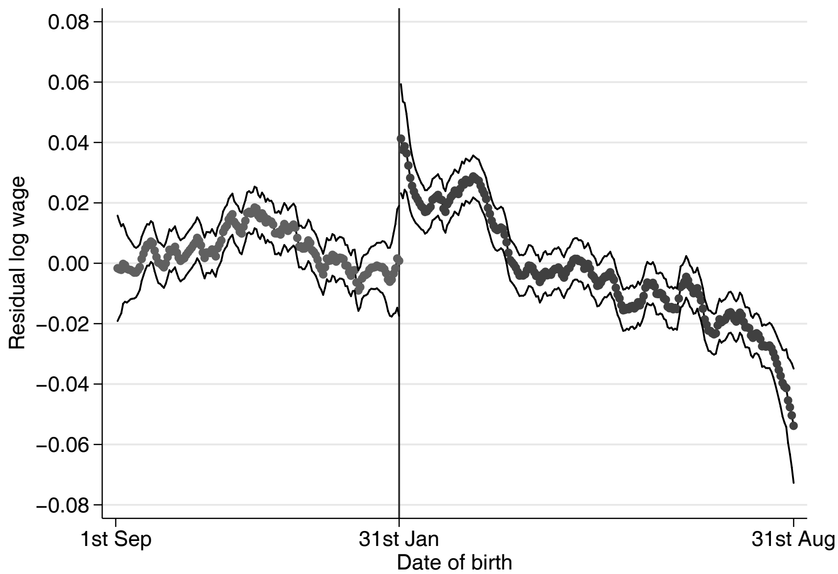



FIGURE IXa
EFFECT OF DATE OF BIRTH ON LOG HOURLY WAGES
FOR MEN IN ENGLAND AND WALES

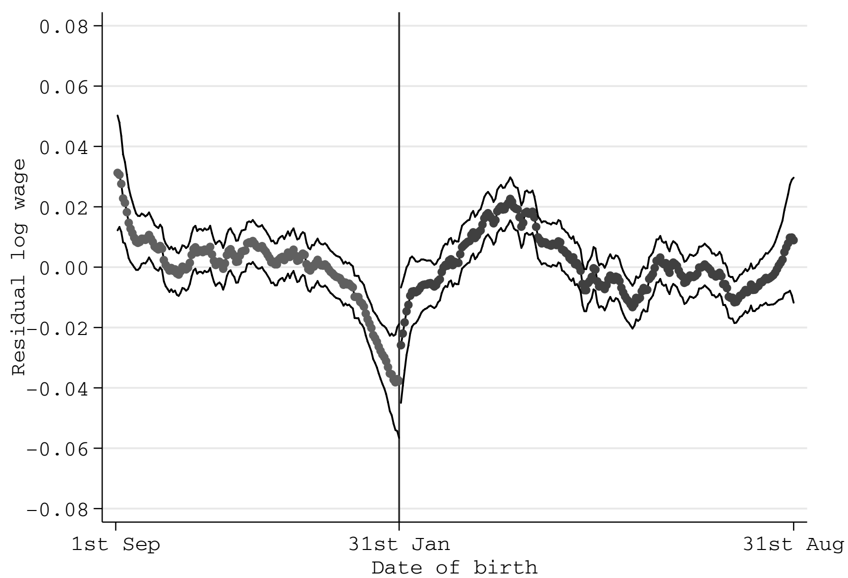



FIGURE IXb
EFFECT OF DATE OF BIRTH ON LOG HOURLY WAGES
FOR WOMEN IN ENGLAND AND WALES

Notes: Smoothed mean value of residual log hourly wages after accounting for survey date and birth cohort effects, by date of birth for men and women in England and Wales with associated confidence intervals. Estimates derived using "running", a locally weighted regression routine for Stata®. Calculation based on the matched JUVOS-NES sample (1993 -2003) of individuals born September 1957 to August 1975 (282,107 [41,607] men and 229,720 [37,286] women). Numbers in square brackets indicate unique individuals.
Source: ONS

APPENDIX A
DETAILS ON THE MATCHED JUVOS-NES DATASET

**The Joint Unemployment & Vacancies Operating System Cohort (JUVOS)**

The Office for National Statistics compiles statistics on U.K. claimant unemployment from which it has established a database of longitudinal information. The database comprises a 5 per cent sample of all computerized claims for unemployment-related benefits (Unemployment Benefit, Income Support and National Insurance credits) selected by reference to a claimant's National Insurance (NI) number and paid through the National Unemployment Benefits System (NUBS).

Data are added to the cohort monthly, so that the database increases in size each month by 5% of the inflow record for that month, approximately 15,000 claims. To date the cumulative sample size is over four million claim records and covers the period between October 1982 and January 2006. The cohort excludes claims paid clerically, since these are handled outside of NUBS. Many of these claims are related to the long-term unemployed, so this exclusion may lead to under-representation of these claimants.

More information on JUVOS is available from:
http://www.statistics.gov.uk/STATBASE/Analysis.asp?vlnk=224&More=Y , and
http://www.data-archive.ac.uk/findingData/snDescription.asp?sn=3721

**The New Earnings Survey (NES)**

The New Earnings Survey (NES) is an annual sample survey of the earnings of employees in Great Britain. The survey consists of a 1 per cent sample of employees registered by their employers into the pay-as-you-earn (PAYE) taxation system. Individuals are selected by reference to the last 2 digits of their National Insurance numbers producing a random sample of those in the system. The same pair of digits has been used since 1975, allowing the construction of a longitudinal database. From October 2004 the New Earnings Survey (NES) was replaced by the Annual Survey of Hours and Earnings (ASHE).

Unfortunately, the survey does not include information on educational attainment. It is designed to collect data on hours worked, industry, occupation, place of work, sex, age and about the levels, distributions and make-up of earnings of employees in all industries and occupations and for the major national collective agreements.

Coverage of full-time employees is virtually complete, but coverage of part-time employment is less comprehensive and many individuals earning below the income tax threshold are not covered at all. This excludes mainly women with part-time jobs and a small proportion of young people. As a result, the level of average wages for part-time workers found in the survey is likely to be overestimated. The survey does not cover the self-employed.

The earnings information relates to gross pay before tax, National Insurance or other deductions, and generally excludes payments in kind. It is restricted to earnings relating to the survey pay period (usually April), and therefore it excludes payments of arrears from another period made during the survey period. Any payments due as a result of a pay settlement but not yet paid at the time of the survey will also be excluded.

Most of the NES analyses relate to employees on adult rates whose earnings for the survey pay period were not affected by absence. Thus they do not include the earnings of those who did not work a full week, and those whose earnings were reduced because of, for example, sickness and short time working.

More information on NES is available from:
http://www.statistics.gov.uk/statbase/Product.asp?vlnk=5749

**Matched JUVOS-NES sample**

We use individual data resulting from the matching of NES records for the period 1986-2003 to the JUVOS database covering the years between 1983 and 2003. This is enabled by a common individual identifier which is an anonymised version of the individual's National Insurance Number. The linking and statistical analysis was carried on-site at the Office for National Statistics Business Data Linking unit, through its Virtual Microdata Lab. As explained in the main text, the linking was required by the need to extract individual dates of birth available through JUVOS to employees' earnings records available from the NES.

The NES file for the period 1986-2003 consists of more than 3 million observations (this is not the full panel, but it is the one available to us through the Office for National Statistics Business Data Linking unit). We succeed in matching 1,205,834 individuals to JUVOS (about 40 per cent). Unmatched individuals are for the most part those who never receive unemployment benefits. After dropping repeated observations in the same year (people with more than one job), we are left with 993,443 records. For comparability with our LFS estimates, we restrict the sample to NES records corresponding to the 1993-2003 period. This restriction leads to 628,360 observations, and has very little impact on the central estimates although tends to make estimates significantly less precise. Most estimates are based on a total of 386,631 observations (211,227 for men and 175,404 for women) corresponding to post-ROSLA school birth cohorts 1957-58 to 1974-75.

For further details on access conditions, replication and confidentiality please see:
http://www.statistics.gov.uk/about/bdl/

**Gender differences in claimant count measures of unemployment**

In the U.K. as well as in many other countries, unemployment benefit register-based measures of unemployment differ from survey-based ones. However, the gap in the UK is wider for women than for men. According to recent figures published by the Office of National Statistics, fewer than half of ILO-unemployed women claim unemployment-related benefits compared with more than three quarters of men [Office for National Statistics, 2006].

The reasons are varied. Firstly, there is a set of explanations related to the benefit system itself. Unemployed women who have been in employment and have paid National Insurance contributions can claim and receive contribution-based benefits (from 1997 these are known as Jobseeker's Allowance payments or JSA). However, after six months the benefit becomes income-based and since this is calculated on a household basis many women living in a couple in which the other partner is still working are unlikely to receive any payment. Secondly, women living in a couple who are looking for work but were not in work immediately before looking (and therefore do not have recent National Insurance contributions) will not receive any JSA payment if they claim. This applies to those who were looking after their families before starting to look for work. There might also be differences in the take-up rate due to differences in behaviour. Women in couples who are looking for work may consider themselves as moving back, albeit temporarily, into roles as housewives rather than as *unemployed* and may be less likely to file a claim.

APPENDIX B
GENERAL FEATURES OF THE U.K. EDUCATIONAL SYSTEM

Many 20th Century reforms to the U.K. education system have been designed to widen access and improve the performance of pupils at the lower end of the attainment distribution, in an attempt to reform what has historically been described as an *elitist* system. The reforms raising the length of compulsory schooling which were enacted in 1947 and 1973 can be interpreted in this context, while other changes in the same direction took place in subsequent decades.

During the 1960s and 1970s, secondary schools in England and Wales underwent a period of radical change, in a further attempt to widen access. Prior to this period, students of differing abilities were sent to different types of schools, receiving very different types of education. More able students (i.e. the top 10-20 per cent) were sent to *elite* schools called grammar schools while the rest would attend so-called "secondary modern" or "technical" schools. The grammar school students were also most likely to go on to higher education. Over the 1960s this selective system was progressively phased out in favour of a comprehensive one, although certain schools can still select their intake on the basis of ability.

In the meantime, the system of national public examinations underwent its own transformations. Since the 1950s, secondary school students who were academically inclined took Ordinary Level (age 16) and Advanced Level (age 18) examinations. O-levels and A-levels remain today an essential requirement to enter higher education. After 1965 less academically oriented pupils had the option of taking the Certificate of Secondary Education (CSE) before they left school. Like O-levels, the CSE was only accessible to those who stayed in education until age 16, although at the time of their introduction compulsory schooling ended at age 15. School Leaving Certificates were accessible to all students upon leaving full-time education but their administration was erratic and mostly down to the schools.[39]

In 1988 the O-level and CSE exams were combined in the GCSE (General Certificate of Secondary Education), still taken at age 16. This change marked a turning point in the achievement of 16 year olds in the U.K., as it was not necessary anymore to choose whether to go for the lower level CSE option or the more difficult O-level examination. This may have encouraged those who were academically on the borderline between CSEs and O-levels to aim for a higher level of attainment. As Vignoles and Hansen [2005] argue, GCSEs have proved more accessible than O- levels and considerably more students now leave school with at least some qualifications.

In addition to the "academic" path of qualification attainment, traditionally characterised by central government control over the content and assessment of qualifications, the system comprises an alternative "vocational" route, which has a more disparate structure of training provision. Here the government has traditionally allowed private (and multiple) institutions greater freedom in determining the content and assessment of qualifications (see Table B.I below).

[39] In a speech, Lord Adonis, Minister for Schools in Tony Blair's Government, reflects on a book by a 1967 book by David Hargreaves, an educationalist, about life in a secondary modern school at the time: "Most of the boys at the school took no external exams at all and gained no qualifications whatsoever, with only a minority in top streams even entered for a local school leaving certificate – for which cheating was widespread among staff and pupils. The new national CSE exam was coming in – below a much inferior form of O-level – but there was no encouragement from the headteacher or most of the teachers on even the brightest pupils to stay at school beyond the school leaving age of 15 to take it. As for O-levels, David Hargreaves wrote: 'Only one member of staff felt strongly that even the best boys, in the top stream, were of sufficient ability to take O-level; most of the other teachers of the higher streams took the view that … to enter them for O-level would be to mislead the pupils with hopes of academic success beyond their powers. There is also little doubt that some of these teachers were reluctant to teach to O-level since they had never done so and were uneasy about their competence to do so'. As for the wider ethos of the school: 'Lessons and exams were treated with contempt by most of the boys. … For many of the teachers and most of the pupils life at the school was a necessary evil. Life was directed towards a reduction of potential conflict by a minimal imposition of demands one upon the other. If the upper streams passed their [school leaving] exam and the lower streams did not riot, the school was for most teachers succeeding.'" See Hargreaves [1967]; the quotations are from pages 86, 87 and 184. The minister's full speech can be downloaded from: http://www.dfes.gov.uk/speeches/media/documents/alevels.doc.

TABLE B.I
QUALIFICATION LEVELS AND TYPE OF EDUCATIONAL AND VOCATIONAL QUALIFICATIONS AS RECORDED BY THE LABOUR FORCE SURVEY

| Qualification level | Type of qualification |
|---|---|
| *Academic Qualifications* | |
| Level 1 | CSE below grade 1, GCSE below grade C |
| Level 2 | O-levels, CSE grade 1, GCSE grades A-C, A/S levels |
| Level 3 | A-levels, SCE Higher, Scottish certificate of 6$^{th}$ year studies |
| Level 4 | Diploma in Higher Education, other HE qualifications below degree level |
| Level 5 | First Degree, Higher Degree |
| *Vocational Qualifications* | |
| Level 1 | YT certificate, SCOTVEC national certificate modules, NVQ level 1, GNVQ foundation, RSA other qualifications, City and Guilds “other”, BTEC first certificate |
| Level 2 | NVQ level 2, GNVQ intermediate, RSA diploma, City and Guilds craft, BTEC first diploma |
| Level 3 | NVQ level 3, GNVQ advanced, RSA higher diploma, City and Guilds advanced craft, BTEC/SCOTVEC national, ONC/OND |
| Level 4 | NVQ level 4, BTEC/SCOTVEC higher, HNC/HND, Nursing qualification, Teaching qualification |
| Level 5 | Other, i.e. member of professional institute |

Note: Details on the type of academic and vocational qualifications recorded by the Labour Force Survey over the period between 1993 and 2003, and our classification of these qualifications into different educational attainment levels.