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ABSTRACT

What Determines the Return to Education: An Extra Year or a Hurdle Cleared?*

The 1973 Raising of the School Leaving Age in England and Wales has been used to identify returns to years' schooling. However, the reform affected the proportion with qualifications, as well as schooling length. To shed light on whether the returns reflect extra schooling or qualifications, we exploit another institutional rule – the Easter Leaving Rule – to obtain unbiased estimates of the effect of qualifications. We find sizeable returns to academic qualifications – increasing the probability of employment by 40 percentage points. This is more than 70% of the estimated return based on RoSLA, suggesting that qualifications drive most – but not all – of the returns to education.

JEL Classification: I21, I28, J24

Keywords: returns to education, RoSLA, qualifications

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NON-TECHNICAL SUMMARY

The raising of the minimum school leaving age in England and Wales in 1973 has previously been used to estimate the economic returns to an additional year of education. However, this policy change had two effects on members of the affected cohorts: it increased their length of schooling and also increased the probability that they attained at least some academic qualifications.

Another school leaving rule that was in place at the time allows us to isolate the effect of having some academic qualifications versus having none on the outcomes of individuals with the same length of schooling.

This latter exercise suggests that there is a sizeable return to having academic qualifications, increasing the probability of employment by 40 percentage points. This is more than 70% of the estimated return to qualifications based on the raising of the minimum school leaving age – implying that the majority of the policy effect came through the impact on qualifications, with a smaller additional effect due to schooling length.

This has implications for plans to further raise the education leaving age, suggesting that returns will be higher if students are compelled to take nationally recognised exams at the end of their final year of education. Thus raising the minimum school leaving age to 18 – when A-levels and equivalent exams are taken – should have a much greater effect on outcomes than raising the minimum leaving age to 17, an age at which traditionally there are not nationally recognised exams taken.

1. Introduction

It is well known that identifying the *causal* effect of education on labour market and other outcomes is problematic given the endogeneity of schooling choice. Changes in compulsory schooling laws, which occurred in the UK in 1947 (when the school leaving age changed from 14 to 15) and 1973 (when it increased again to 16), are natural candidates for instruments and have been widely exploited (see Harmon and Walker, 1995, Devereux and Hart, 2010, Grenet, 2009, for earnings effects and Silles, 2009, Clark and Royer, 2010, for effects on health outcomes). Typically, estimates focus on the returns to the length of schooling. However, raising the school leaving age, particularly from 15 to 16, affected not only years' education but also the qualifications that people obtained. The extent to which the estimated returns reflect the benefit of an increase in length of schooling, or the returns to gaining specific qualifications is unclear. Yet this is crucially important to policy-makers. For example, the school leaving age is planned to increase again in the UK from 16 to 17 in 2013.The issue is whether this will raise employment and wages among those affected when the high stakes exams are typically taken at age 18.

The aim of this paper is to shed light on the issue of what drives returns to increased education – whether there is a benefit to increased years of schooling or whether it is qualifications that are key. To do this, we exploit another institutional rule – the Easter Leaving Rule (ELR) – that determined exactly when in the school year people could leave school. Rather than being allowed to leave on the day of reaching the minimum age, children faced one of two possible leaving dates – the end of the Easter term or the end of the summer term – depending on their birthday. Specifically, those born between 1st September – 31st January could leave at Easter while those born between 1st February – 31st August had to stay until the end of the summer, exam-taking term. After the school leaving age rose to 16, the age at which high stakes exams are typically taken in the UK, late leavers were significantly more likely to obtain academic qualifications. We exploit this discontinuity to identify the effect of qualifications on later outcomes, focusing on employment and participation in the labour market. We then compare the unbiased estimates of the effect of qualifications using the ELR as an instrument with the potentially biased estimates of the effect of qualifications using the 1973 Raising of the School Leaving Age (RoSLA) as an instrument. This allows us to say something about whether what matters is qualifications or years of schooling.

The plan of the paper is as follows: the next section discusses related literature on estimating returns to education using RoSLA as well as studies that have attempted to estimate the returns to qualifications directly. Section 3 discusses the institutional rules, and our empirical strategy, in more detail. Section 4 describes the data, while section 5 presents the main regression results. Section 6 concludes.

2. Related literature

Our paper is related to two existing literatures. First, a number of papers estimate the returns to education by exploiting changes in the school leaving age in the UK. The majority employ variants of the traditional Mincer human capital earnings function in which education is measured in terms of completed years of schooling. Very few of these studies explicitly consider the extent to which the increase in qualifications matters. There is a second literature that does focus on quantifying the returns to specific qualifications or equivalent levels, especially in the UK context. We briefly discuss both in turn.

Harmon and Walker (1995) were the first to exploit changes in the minimum school leaving age in the UK to identify the causal effect of increased education on wages. They exploited the 1947 increase from age 14 to 15, affecting school cohorts from 1933 onwards, and the 1973 increase from 15 to 16, affecting cohorts born from 1st September 1957 onwards, to derive instrumental variables estimates of the return to years' schooling. They estimated a large positive return of 15.3 per cent, but since they did not control for cohort effects, this estimate may be upward-biased, capturing the effect of increasing education among successive cohorts and conflating the actual return to one single additional year of education. A second potential concern is that the estimates derived from school leaving age reforms provide a local average treatment effect (LATE) that may be limited to the specific group of compliers, making interpretation of the Harmon and Walker estimate potentially problematic since it combines the effects of two leaving rule changes.

In order to sharpen the estimate of the wage return to the additional education induced by the 1947 increase in the minimum school leaving age, Devereux and Hart (2010) employ a regression discontinuity design allowing comparison of wages for the cohorts born just before and just after the law change. Using data from the General Household Survey (GHS), Devereux and Hart estimate a return to education for men of approximately 6 per cent for weekly earnings. Using the larger and more accurate New Earnings Survey Panel Data-set (NESPD), the corresponding estimates are between 3-4 per cent in both cases. The combination of the vast dataset (in excess of 1 million observations in the samples using the NESPD) and the identification exploiting a clear and sizeable discontinuity in schooling allows us to be confident that the wage return to education for those induced to gain additional schooling by the 1947 RoSLA is in fact limited to 3-4 per cent.

Focusing on the 1973 change, Grenet (2009) uses the British Labour Force Survey and similarly implements a regression discontinuity design to estimate the wage return to education. Grenet estimates a return for men of approximately 6-8 per cent in hourly earnings and suggests that the return for compliers at this margin is higher than that found at the 1947 law change because compulsion to remain in school until age 16 brings students to the point at which high stakes exams are taken. Hence the change in minimum schooling requirements adds not only to the number of years of schooling but also impacts the probability of attaining credentials and this is an important factor for wage outcomes. However, Grenet does not test this formally.

Interestingly, Devereux and Fan (2010) also in this issue, exploit the expansion in higher education participation in the UK between the late 1980s and the mid-1990s to instrument for education and derive estimates of the return to additional year of schooling. The expansion shifted the whole distribution of education upwards, increasing average education by approximately one year, with the resulting IV estimate of the male return to education being 6%. This is above the estimated effect for the 1947 RoSLA but around or just below the estimate from Grenet for the 1973 RoSLA. This makes sense as the expansion from the late 1980s saw an increase in average schooling length which should entail increased qualifications for some though not all of the men affected.

The literature on returns to qualifications typically finds strong, positive effects. Dearden (1999) uses the rich National Child Development Survey (NCDS) data and finds that leaving school at 16 with 5 or more O-levels compared to zero qualifications increases wages (at age 33) by approximately 20-26% for men. Blundell, Dearden and Sianesi (2005) again using the NCDS compare various models and methods of estimation and find that, compared to leaving at 16 with no qualifications, having O-levels or GCSEs gives a wage return for men of 14-20%, with 18% being the average in the population. Using LFS data, Chevalier, Harmon, Walker and Zhu (2004) consider signalling versus human capital explanations for the return to education and estimate the male wage return to O-levels versus no qualifications of approximately 25% – though these come from OLS specifications which may suffer some positive ability bias. Whilst rejecting pure signalling explanations of the returns to qualifications, they suggest that “sheep-skin” effects (i.e. credentials) are important after controlling for years of education. This supports earlier work by Chevalier and Walker (2002) who use both the British Household Panel Survey and the General Households Survey to estimate the returns to specific qualifications. They find that compared to no qualifications, attaining GCSEs is associated with approximately 25% higher wages, and this is after accounting for years of education – again suggesting that, even conditional on the length of schooling, the margin between getting some qualifications and not is important for later outcomes.

3. Empirical Strategy

In order to disentangle the effect of an additional year of schooling from the effect of credentials gained in school, we exploit a former institutional rule in England and Wales that determined exactly when individuals could leave school – the Easter Leaving Rule.

Since the Education Act of 1870 a September 1st cut-off has determined which school cohort a child belongs to in England and Wales: thus school cohort *t* comprises all children born between 1st September in year *t* and 31st August in year *t+1*. The same Act also established that children are legally bound to attend school from the start of the first academic term following their fifth birthday. For almost all children, this practically meant starting school at the start of the academic term – and in most cases the academic year – in which they turn five.[2] This means that those born in later months of a school year (June, July, August) will be barely older than four when they begin school in the September and younger in absolute age when each set of exams is taken since exams are taken at a set point within the year, not at a specific age of pupil. This leads to a "summer-born penalty" (see Crawford et al, 2010) and controlling for within-cohort age is therefore important in our analysis below. However, crucially for our analysis, there is no 31st January cut-off for determining when children start school.

School leaving dates were established by the Education Act of 1962 which stipulated that individuals born in the first five months of the school year, i.e. 1st September – 31st January, attained the minimum school leaving age at the end of the Easter term in the academic year that they turn 15 (or latterly 16, following the 1973 reform). Those born between 1st February and 31st August were not deemed to have reached the minimum school leaving age until the end of the summer term – typically the end of May/start of June.[3] This is known as the Easter Leaving Rule (ELR). The discontinuity at 31st January in school leaving date *within* a school cohort implies a slightly longer duration of schooling for the younger-born within the school year: depending on when Easter falls the increased education duration implied by the rule is between 33 and 61 days – of which 24 to 44 would be school days.

However, more significant is the fact that "high stakes" exams are taken in the summer term. For the cohorts we look at, these exams included the General Certificate of Education Ordinary level (GCE

[2] In practice, school start dates vary by local authority with some operating a dual start date (September and January) and others a single, September start date. Ideally, we would have this information but our dataset does not have any information on where respondents were educated.

[3] The Education-School Leaving Act 1976 made the "May" school leaving date explicit as the Friday before the last Monday in May – see Del Bono and Galindo-Rueda (2004)

O-level) exams, taken by secondary school students who were more academically oriented and the Certificate of Secondary Education (CSE) exams, taken by less academically inclined students. Both O-level and CSE exams were taken by most students at age 16, despite the latter being introduced (in 1965) when the minimum leaving age was still 15. And, for both exams, the exam-taking period was May-June. As we show below, the requirement for individuals born between 1st February and 31st August to remain in school until the end of the exam-taking term increased the likelihood that they took the exams and gained some academic qualification before leaving school, particularly when the minimum school leaving age was 16.

School children born either side of the 31st January/1st February discontinuity are in the same school cohort, begin school at approximately the same age and are approximately the same level of maturity within cohort. This discontinuity does not align with any other institutional factor that could affect educational attainment and undermine the identification strategy. It is therefore credible that children born either side of the discontinuity point at 31st January/1st February are identical with respect to their unobserved characteristics, such that any difference in their educational attainment is driven solely by the institutional rules governing when they are allowed to leave. We would argue that the difference in the length of schooling between children each side of this cut-off is negligible (approximately 30 days). Instead, the main effect of the leaving rule is on the probability of obtaining academic qualification, particularly for cohorts born after the raising of the school leaving age (RoSLA) from 15 to 16. We would therefore argue that this discontinuity can be exploited to identify the effect of qualifications.

We are not the first to adopt this identification strategy. Anderberg and Zhu (2010) use it to estimate the effect for women of holding academic qualifications on the probability of being married and on the probability of the husband holding qualifications and being economically active. Closer to our study, Del Bono and Galindo-Rueda (2004, 2006) use it to estimate the effect of qualifications on wages, employment and participation. Specifically, Del Bono and Galindo-Rueda (2006) use data from the LFS (1993-2003), the Youth Cohort Study and a dataset combining information from the New Earnings Survey and the Joint Unemployment and Vacancies Operating System Cohort to study primarily the cohorts born from September 1957 up to the last cohorts affected by the ELR (born before the end of August 1981). The Education Act of 1996 replaced the two leaving dates with a single leaving date – the last Friday in June of the school year that the individual reaches age 16.

Del Bono and Galindo-Rueda explicitly focus on cohorts born after the 1973 reform in order to abstract from the effects of raising the school leaving age. In contrast, our focus is on the cohorts immediately before and after the reform. Our main aim is to shed light on whether the main driver of the returns for the post-RoSLA cohort was the increase in the length of schooling or the increase in qualifications. We do this by comparing the estimated returns to qualifications (exploiting the ELR for the post-RoSLA cohorts) with the estimated effect of the RoSLA itself, which affected both length of schooling and qualifications.

A narrow focus on cohorts around the 1973 RoSLA is justified by the pattern of increasing attainment of academic qualifications in the cohorts born since the 1970s. As it becomes more common for the majority of individuals to attain at least some academic qualifications, the power of the ELR to identify the return to qualifications diminishes – if all in a cohort gain qualifications then this identification strategy will fail. Moreover, as the group of individuals whose behaviour may be influenced by the ELR falls in size the estimated LATE is driven by an ever narrower and specific stratum of those with very little taste for education. Similarly, the further we move away in time from the 1973 RoSLA the more problematic it is to compare the post-RoSLA cohorts to those born before who were able to leave at 15 – the implicit regression discontinuity design weakening with each year that we move away.

More formally, we are interested in estimating the effect of qualifications on labour market outcomes, i.e.

$$Y_{it} = \beta Q_i + X_{it}\gamma + u_{it}$$

where Y_{it} is labour market outcome for individual *i* at time *t* (we look at employment, participation and wages), Q_i is an indicator for whether the individual has any academic qualifications and X_{it} is a vector of control variables, including age within year, year of birth (dummies), a quadratic in age, plus region and survey quarter*year dummies.

OLS estimates of β are likely to suffer from endogeneity bias. Both the 1973 RoSLA that increased the school leaving age from 15 to 16 and the ELR (post-RoSLA) are potential instruments since they affect the probability of obtaining any academic qualifications. We would argue that the impact of the ELR on outcomes comes solely through the effect on qualifications since the approximate 30 day difference in schooling for those either side of the 31st January discontinuity is too small to have an impact. This will therefore provide an unbiased estimate of the effect of qualifications. However, the RoSLA also affected the length of schooling by up to one year for affected cohorts and, if this has a separate effect on labour market outcomes, means the estimate of the effect of qualification will be subject to upward bias. Comparing estimates obtained using these two instruments can therefore tell us something about the size of the bias caused by the length of schooling effect, and the relative importance of qualifications and length of schooling on labour market outcomes.

In brief, therefore, our strategy is to obtain – and compare – estimates of β using RoSLA and ELR as instruments. We would expect that $\beta_{RoSLA} \geq \beta_{ELR}$ since RoSLA affected both the probability of gaining qualifications and the length of schooling. If $\beta_{RoSLA} = \beta_{ELR}$ this implies that the effect of RoSLA is driven solely by qualifications (there is no upward bias caused by the increase in length of schooling); if $\beta_{RoSLA} > \beta_{ELR}$ then the length of schooling additionally matters as well as qualifications.

One assumption here is that the estimates are comparable. In practice, both are local average treatment effects for those who were induced by the institutional rule to gain qualifications (the “compliers”). For the RoSLA, compliers are people who gain qualifications because they are required to stay on in school from 15 to 16. In the case of the ELR, compliers are people who gain qualifications because they are required to stay on in school from Easter until the end of May. We make an implicit assumption that the effect of gaining qualifications on outcomes is similar for the two groups in order to be able to say anything concrete about what drives the RoSLA effect. This seems reasonable given that both groups of compliers are within the same cohort and are people induced by institutional rules to obtain academic qualifications. Because both estimates are local average treatment effects, they may not be informative of the average treatment effect of academic qualifications. However, the groups of individuals at the margin of gaining any academic qualifications are important from a policy perspective – especially given plans to raise the school leaving age to 17 in 2013 and up to 18 in 2015.

4. Data and descriptives

Our data come from the Quarterly Labour Force Survey (LFS), pooled from 1993 quarter one to 2010 quarter two inclusive. The LFS is the largest regular household survey in Great Britain and is designed to be representative of the population living in private households, with approximately 60,000 households responding each quarter. The survey is a rotating panel with each household interviewed in five successive quarters and is designed such that, in each quarter, one fifth of the households are undertaking their first interview, one fifth their second interview and so forth. The LFS provides the necessary information on each individual’s year and month of birth[4] in addition to their highest educational qualifications, age when completed full-time education and current labour market status. In their first and fifth interviews respondents are also asked to provide information on their earnings, although this is missing for many observations[5]. To keep samples consistent we use only information from an individual’s first interview in all of the results presented.

[4] From 1993 onwards the month of birth is available only in the Special Licence QLFS datasets.

[5] We deflate wages using the quarterly RPI 1993Q1=1 and trim the wage distribution to exclude the lowest and highest 2% of the hourly wage distribution; we first remove the lowest and highest 2% of the hours distribution.

For the wage effects analysis we include full-time employees only and exclude the self-employed. When looking at the probability of employment we again exclude the self-employed but do allow part-time employees in the employed category; the unemployed category captures both the registered unemployed and the economically inactive. For the case where participation is the dependent variable, we allow the self-employed and unemployed to be included as participating, the non-participating group comprising solely the economically inactive. In all cases we only include information from individuals who have completed the survey themselves, excluding all proxy respondents.

To avoid any issues in modelling female labour market participation, we restrict our analysis to men. We focus on cohorts born ten years before and after the RoSLA – from September 1947 to August 1967. With data from LFS waves from 1993 – 2010 this means that our sample contains men aged 25 to 62[6]. We restrict our analysis to those leaving school at age 16 or younger.[7] Implicitly we assume that the RoSLA and the ELR induced the compliers to stay only up to the minimum leaving date and did not cause them to stay even longer; we also assume that the effects of RoSLA were restricted to individuals at the lower end of the education distribution and that there was not a ripple effect upwards. Consistent with previous research (see *inter alia* Chevalier *et al* (2004)), we provide evidence that this was the case.

Table 1 contains summary statistics for our estimation sample, by school cohort. The effect of RoSLA on years of schooling and academic qualifications is clear: in the 1956/7 cohort mean years of schooling is 10.51, increasing to 10.87 in the 1957/8 cohort, while the proportion with academic qualifications increases from 0.459 to 0.609. To identify the effects on employment outcomes, we need to remove age and cohort effects. Using data from 20 cohorts, and surveys from 18 years (and four quarters in all but one of the years[8]) means that we have variation in age by cohort (and multiple cohorts at the same age). Moreover, though we only retain each individual's first observation, the rolling panel nature of the QLFS means that we have variation in ages and cohorts at each quarter*year of the survey, allowing quarter*year time effects to also be identified.

Figure 1 illustrates the effect of RoSLA on attainment of different qualification levels using the National Vocational Qualification equivalence scale (see Appendix Table 1). This is shown for the full sample (rather than just the estimation sample of those leaving school at 16 or before) allowing us to explore whether RoSLA had any effect on later school leavers. The proportion attaining no

[6] We also exclude men who moved to the UK after their secondary schooling would have begun (i.e. after age 11).

[7] We have confirmed all of the main results with the full sample, available from the authors on request.

[8] In 2010 we only have data available from the first two quarters.

academic qualifications was falling steadily across cohorts both before and after RoSLA but there is a discontinuity at the point of RoSLA. The proportion attaining no academic qualifications fell from 0.286 in the 1956/57 cohort to 0.216 in the 1957/58 cohort. Similarly there is a steady upward trend in the proportion attaining level 1 qualifications before and after RoSLA but a discontinuity at the RoSLA point, the proportion attaining level 1 qualifications increasing from 0.045 to 0.093. For level 2 qualifications the jump at 1957/58 is from 0.194 to 0.234. For levels 3 and upwards – academic qualifications equivalent to A-levels or above – the patterns are unaffected by RoSLA.

Figure 2 illustrates the same effects amongst our estimation sample of those who leave school at age 16 or younger. The proportion attaining no academic qualifications falls from 0.541 in the 1956/57 cohort to 0.391 in the 1957/58 cohort. The proportion attaining level 1 qualifications discontinuously jumps at the RoSLA point from 0.071 to 0.166, while for level 2 qualifications the jump is from 0.253 to 0.322. For levels 3 and upwards the patterns are completely flat across all cohorts – as we would expect, among those leaving school at 16 or younger, there is very little attainment of level 3 or higher qualifications.

Figure 3 illustrates both the effect of RoSLA across cohorts (marked by the vertical red line) and the effect within each cohort of the ELR. The post-RoSLA increase in academic qualifications is clear for both "early leavers" (born 1st September – 31st January) and "late leavers" (born 1st February – 31st August). Looking within each school cohort, late leavers are clearly more likely to have academic qualifications after RoSLA – the pattern before RoSLA is mixed. The difference post-RoSLA is in line with what we would expect given that the main exams are taken at age 16.

Figure 4 illustrates this further comparing just those born in January and those born in February. This is the discrete jump in qualifications probability that will drive the identification in our estimates using the Easter Leaving Rule. We control in all specifications for the smooth effects of relative age within cohort, using a linear trend; the jump around the 31st January discontinuity point, post-RoSLA, provides exogenous variation in qualifications attainment. The figure shows clearly the greater attainment for the February born in the post-RoSLA period, a much clearer pattern than in the pre-RoSLA cohorts. Moreover we see again the up-shift in qualification attainment across the board for the cohorts affected by the RoSLA.

5. Regression results

Effect of RoSLA and ELR on academic qualifications

Table 2 quantifies the effects of both rules on the probability of attaining academic qualifications in each of the three samples corresponding to the employment outcomes we look at (log wages, employment, participation) using simple linear probability models. "Late leaver" is an indicator that takes the value 1 if the individual is born between 1st February and 31st August i.e. compelled to remain in school until the end of the Summer term, otherwise zero. RoSLA is an indicator equal to 1 for individuals born after 1st September 1957, otherwise zero. No additional controls are included in these specifications; including a quadratic in age, a linear control for relative age within year (September=12, October=11, ..., August=1), year of birth dummies and dummies for region of residence, ethnicity and survey quarter*year leaves the key results unchanged[9].

The results in Table 2 confirm the evidence from Figures 3 and 4 on the effect of the ELR. The more closely focused around the 31st January cut-off point the sample is, the more credible the assumption that individuals differ only in their qualification attainment due to the ELR. Panel (a) of Table 2 considers only January and February-born individuals and, for the larger employment and participation samples, the ELR has a significant effect in increasing the probability of attaining academic qualifications by around 7 percentage points in the post-RoSLA period, but no effect in the pre-RoSLA period. For the smaller wage sample, there is no ELR effect either pre- or post-RoSLA. In all samples, there is a sizeable effect of RoSLA itself, raising the probability of attaining qualifications by between 22 and 27 percentage points. These regression results confirm that both the RoSLA and the ELR are potentially relevant instruments for academic qualifications, although these regressions do not include any other covariates.

Panel (b) includes all individuals born within each cohort; this is the sample used in the IV regressions. For each sample the effect of RoSLA is to raise the probability of attaining qualifications by around 25 percentage points and for each sample there is a significant effect of the ELR of between 2 and 4 percentage points in the post-RoSLA period – exactly as we would expect. However, in the employment and participation analysis, there is also a significant positive effect of the ELR – of approximately 2 percentage points – on qualifications in the pre-RoSLA period. This is puzzling, as those not eligible to leave at Easter would have to remain for an entire further year before they reached the age at which nationally recognised academic qualification exams are taken. This has previously been found to be a feature of the LFS (see Del Bono and Galindo-Rueda, 2006), and may be due to mis-reporting of qualifications in the LFS. Individuals who remained until the end of the Summer term pre-RoSLA but left at 15 will in some cases have received a “School Leaving Certificate” and it may be that some individuals who left at 15 in the pre-RoSLA period report having CSE equivalent level qualifications (and therefore would count as having academic qualifications) in error. Mis-reporting of education is known to be a problem in the LFS, see Thomson *et al* (2010) and references therein. By excluding all proxy respondents we hoped to limit this problem however it does still remain to some extent. Further analysis (nor reported) provided some support for this mis-

[9] Results available from the authors on request.

reporting explanation – reduced-form estimates showed that while the ELR may have an effect on qualification attainment pre-RoSLA, this does not translate into any effects in the labour market which is consistent with CSE level qualifications having been erroneously recorded.

IV estimates using RoSLA and ELR

As discussed in section 3, the main aim of this paper is to shed light on whether the effect of RoSLA is driven by the increase in qualifications or by the increase in the length of schooling. We do this by estimating an unbiased effect of qualifications using ELR as an instrument and comparing this with the potentially (upward) biased estimate of the effect of qualifications (if length of schooling also matters) using RoSLA. Table 3 pursues this idea, presenting IV estimates of the effect of qualifications using each instrument separately.[10] In order to limit the issues surrounding the potential mis-reporting of qualifications in the pre-RoSLA period, we restrict the estimates using the Easter Leaving Rule to the first 10 cohorts affected by RoSLA.

When including all other covariates, the instruments used separately are too weak to identify the causal effects of academic qualifications on log wage in the sample available. Neither RoSLA nor ELR is statistically significant in the first stage regression. Grenet (2009) uses a larger sample from the LFS and does find a reduced form effect of the 1973 RoSLA on later log wages of 1.6% to 2.1% – which is similar to the point estimate in our sample, not reported – but statistically significant in his larger sample.[11] Del Bono and Galindo-Rueda (2006) also fail to find a statistically significant effect of ELR using a larger sample of post-RoSLA cohorts (from September 1957 to August 1975); in our smaller sample it is therefore perhaps not surprising that we do not identify an effect on log wages. We therefore focus on the results for employment and participation.

In the larger samples for employment and participation, each instrument is sufficiently strong to generate statistically significant variation in academic qualification attainment, allowing more precise IV estimates of the causal effect of qualifications on employment and participation. In each specification the first stage *F*-statistic on the exclusion of the instrument exceeds the rule-of-thumb value of 10 for non-weak instruments.

[10] The reduced form, Wald estimates, might have been a more obvious point of departure. However, none of the Intention to Treat estimates of employment effects using the ELR was statistically significant; we therefore move straight to presenting the IV results.

[11] Grenet uses LFS data from 1993-2004 and cohorts born between 1949 and 1967; his sample includes individuals who leave at 18 or younger (ours is 16 or younger) and we are constrained to include only individuals for whom highest qualification is recorded which further reduces our sample relative to Grenet's.

RoSLA is estimated to increase the probability of attaining academic qualifications by 9.4 percentage points (employment analysis sample) and 8.5 pp (participation analysis) in each case significant at the 1% level. The RoSLA IV estimate of the effect of qualifications on employment is an increase of 55.8 percentage points (significant at the 5% level). This is a large effect, more than double the OLS estimate of 24.3 pp. The story is similar for participation: the RoSLA IV estimate is that academic qualifications increase the probability of participating in the labour market by 45.0 pp. This is approximately three times greater than the OLS estimate of 15.2pp. One possible explanation for the larger IV estimates is that they capture a LATE for those who only gained qualifications because of the constraint of RoSLA. They may also be upward-biased estimates of the true effect of qualifications because of the increase in years' schooling which may separately have affected employment and participation.

The estimates on the right hand side of Table 3 provide some evidence on the extent to which qualifications, rather than the extra year in education, account for the RoSLA returns. In these estimates, we identify the effect of qualifications using the ELR (and only the post-RoSLA cohorts). We argue that this provides unbiased estimates of the effects of qualifications because the ELR had a negligible effect on years' schooling received. The first stage estimates indicate that being a late leaver increases the probability of attaining academic qualifications by 5.9 pp in the employment analysis sample and 6.6 pp in the participation sample (both significant at the 1% level). For employment, the IV estimated return to academic qualifications is a 39.7 pp increase in employment probability, compared to the OLS estimate of 27.0 pp. For participation, the ELR estimated IV return is 26.0 pp compared with the OLS estimate of 15.2 pp. These results are similar to Del Bono and Galindo-Rueda (2006).

The comparison of note is between the IV estimates using RoSLA and the IV estimates using ELR. For both employment and participation, instrumenting qualifications using RoSLA yields larger estimates. This is consistent with there being some upward bias because of an additional effect on years' of schooling. However, the difference between these estimates and the IV estimates using ELR is not large – particularly for employment. The estimate based on the ELR is more than 70% of the estimated based on RoSLA, suggesting that most of the return associated with RoSLA is being driven by the return to academic qualifications, rather than the additional year of education. This goes some way to answering whether it is time in school or credentials that matter most for the cohorts affected by RoSLA.

6. Concluding Remarks

In this paper, we have used one institutional rule – the Easter Leaving Rule (ELR) – to shed light on what drives the effect on employment outcomes of another institutional rule – the 1973 RoSLA from

15 to 16. This reform had two effects on educational outcomes that could impact employment: it both increased the length of time that children spent in school by up to one year and made it more likely that they would leave school with some academic qualifications since the relevant exams are taken at age 16 in the UK. Using RoSLA to instrument years' schooling (as has been done in previous studies) is therefore likely to produce biased estimates of the returns to years' schooling in the purest sense because of this qualification effect.

The ELR defined exactly when children could leave school – at Easter (children born between 1st September – 31st January) or at the end of the summer term (children born between 1st February – 31st August). We show that for post-RoSLA cohorts, late leavers were significantly more likely to obtain academic qualifications since they were forced to stay until the end of the exam-taking term. We exploit this discontinuity to obtain an unbiased estimate of the effect of qualifications on later employment outcomes, focusing on employment and participation. Consistent with previous studies (Dearden, 1999, Blundell et al, 2005 and Del Bono and Galindo-Rueda, 2006), we find that qualifications have a large, positive effect on later outcomes, increasing the probability of employment by 40 percentage points, although this must be caveated that it is a local average treatment effect for those who were induced by the ELR to obtain academic qualifications.

Comparing the estimates of the returns to qualifications (on employment and participation) using the ELR as an instrument with the estimates using the RoSLA as an instrument, we show that most of the returns to RoSLA appear to be driven by qualifications, but that there is some (small) additional return to increasing the length of schooling.

Our findings have several implications. First, they help to reconcile previous estimates of the returns to education associated with the 1947 RoSLA, which raised the leaving age from 14 to 15, and the 1973 RoSLA, which raised the leaving age from 15 to 16. Estimates of the former (Devereux and Hart, 2010) are smaller than the latter (Grenet, 2009): a 3-4 per cent boost in earnings, compared to a 6-8 per cent. Grenet had previously suggested that the fact that the 1973 reform saw a sizeable increase in the proportion leaving with academic qualifications may account for this difference; we provide direct evidence to support this claim. Importantly, this does mean that the two reforms should be kept separate in obtaining estimates of the returns to education since their effects on educational outcomes were different.

Secondly, our results strongly suggest that qualifications drive much of the estimated returns to raising the school leaving age from 15 to 16. This is potentially relevant to current UK government policy which is to raise the school leaving age again from 16 to 17 in 2013. The second set of high stakes exams is typically not taken until age 18; there is therefore a potential concern that simply increasing the length of time that pupils spend in education without a corresponding increase in qualifications would have substantially less of an effect than if pupils both gained an extra year and left with some credentials. Another consideration is that requiring pupils to spend another year in full-time education is costly in terms of resources; by comparison, requiring them to take exams and increasing the probability of leaving with qualifications could be a more cost-effective approach.

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Table 1: Descriptive Statistics, Mean and Standard Deviation (lower figure)

School Cohort	Age	Academic Quals.	Years of Schooling	Employed (0,1)	Participating (0,1)	N
47-48	52.8	0.365	10.33	0.676	0.734	2287
	5.2	0.482	0.56	0.468	0.442	
48-49	51.7	0.373	10.36	0.697	0.769	2073
	5.1	0.484	0.59	0.460	0.422	
49-50	50.7	0.394	10.39	0.686	0.760	1874
	5.1	0.489	0.59	0.464	0.427	
50-51	49.7	0.394	10.39	0.722	0.786	1696
	5.1	0.489	0.60	0.448	0.410	
51-52	48.8	0.405	10.43	0.694	0.767	1715
	5.1	0.491	0.60	0.461	0.423	
52-53	47.5	0.431	10.43	0.718	0.793	1736
	5.1	0.495	0.65	0.450	0.405	
53-54	46.7	0.437	10.47	0.735	0.809	1658
	5.1	0.496	0.61	0.441	0.393	
54-55	45.8	0.462	10.51	0.735	0.817	1603
	5.1	0.499	0.58	0.441	0.387	
55-56	44.9	0.470	10.52	0.774	0.834	1716
	5.1	0.499	0.60	0.418	0.372	
56-57	43.6	0.459	10.51	0.745	0.825	1750
	5.1	0.498	0.59	0.436	0.380	
57-58	42.5	0.609	10.87	0.770	0.852	1933
	5.1	0.488	0.39	0.421	0.356	
58-59	41.7	0.625	10.89	0.770	0.850	1963
	5.1	0.484	0.36	0.421	0.357	
59-60	40.5	0.650	10.85	0.755	0.851	2101
	5.0	0.477	0.42	0.430	0.357	
60-61	39.3	0.659	10.85	0.782	0.865	2074
	5.2	0.474	0.40	0.413	0.341	
61-62	38.3	0.670	10.87	0.790	0.863	2217
	5.0	0.470	0.37	0.407	0.344	
62-63	37.4	0.691	10.85	0.763	0.860	2140
	5.1	0.462	0.45	0.426	0.347	
63-64	36.4	0.701	10.85	0.786	0.886	2318
	5.1	0.458	0.42	0.410	0.318	
64-65	35.3	0.734	10.86	0.797	0.887	2218
	5.0	0.442	0.40	0.402	0.317	
65-66	34.6	0.746	10.82	0.786	0.881	2072
	5.1	0.436	0.44	0.410	0.324	
66-67	33.7	0.743	10.87	0.798	0.890	2122
	5.0	0.437	0.40	0.402	0.313	
Total	42.8	0.561	10.66	0.750	0.831	39266
	7.9	0.496	0.55	0.433	0.375	

Table 2: The effect of the 1973 RoSLA and the Easter Leaving Rule on the probability of attaining academic qualifications, three estimation samples

Linear probability model regression results. Dependent variable = academic qualifications (0/1)

			Wage sample	**Employed sample**	**Participation sample**
(a) Individuals born January and February in each cohort	**Post-RoSLA**	*coeff*	0.265***	0.225***	0.222***
		st. err.	0.029	0.018	0.017
	post-RoSLA*Late Leaver	*coeff*	-0.010	0.071***	0.067***
		st. err.	0.041	0.026	0.024
	Late Leaver	*coeff*	0.053	0.014	0.022
		st. err.	0.034	0.020	0.018
	Constant	*coeff*	0.498***	0.412***	0.408***
		st. err.	0.024	0.014	0.012
	R-squared		0.078	0.072	0.070
	Obs.		2112	5512	6422
(b) All Individuals in each cohort	**post-RoSLA**	*coeff*	0.259***	0.246***	0.244***
		st. err.	0.014	0.009	0.008
	post-RoSLA*Late Leaver	*coeff*	0.021**	0.043***	0.038***
		st. err.	0.017	0.011	0.010
	Late Leaver	*coeff*	0.031	0.020**	0.024***
		st. err.	0.014	0.008	0.008
	Constant	*coeff*	0.504***	0.402***	0.401***
		st. err.	0.011	0.006	0.006
	R-squared		0.085	0.077	0.075
	Obs.		12806	33161	38695

Table 3: IV estimates

		Log hourly wage				Log hourly wage		
		OLS	IV	First stage		OLS	IV	First stage
Academic Quals.	*coeff*	**0.291*****	0.142		*coeff*	**0.244*****	-0.558	
	st. err.	0.008	0.581		*st. err.*	0.011	0.906	
	coeff		**post-RoSLA**	0.064	*coeff*		**Late Leaver**	0.027
	st. err.			0.043	*st. err.*			0.020
F				2.273				1.902
R-squared		0.143	0.119	0.106		0.119	-----	0.035
Obs.		12806	12806	12806		7498	7498	7498

		Employed (0,1)				Employed (0,1)		
		OLS	IV	First stage		OLS	IV	First stage
Academic Quals.	*coeff*	**0.243*****	**0.558****		*coeff*	**0.270*****	**0.397***	
	st. err.	0.005	0.273		*st. err.*	0.007	0.216	
	coeff		**post-RoSLA**	**0.094*****	*coeff*		**Late Leaver**	**0.059*****
	st. err.			0.028	*st. err.*			0.014
F				11.571				17.664
R-squared		0.109	0.002	0.101		0.111	0.093	0.038
Obs.		33161	33161	33161		17996	17996	17996

		Participating (0,1)				Participating (0,1)		
		OLS	IV	First stage		OLS	IV	First stage
Academic Quals.	*coeff*	**0.152*****	**0.450***		*coeff*	**0.152*****	**0.260***	
	st. err.	0.004	0.240		*st. err.*	0.006	0.143	
	coeff		**post-RoSLA**	**0.085*****	*coeff*		**Late Leaver**	**0.066*****
	st. err.			0.026	*st. err.*			0.013
F				10.806				25.040
R-squared		0.092	-----	0.096		0.072	0.050	0.035
Obs.		38695	38695	38695		20844	20844	20844

Note: Late leavers are born 1st February to 31st August inclusive. Controls included for age, age^2, age-within-year, dummies for year of birth, dummies for region of residence, ethnicity, survey quarter*year.

*** p<0.01, ** p<0.05, * p<0.10. Robust standard errors.

Figure 1: Proportion with academic qualifications at each NVQ level, by school cohort

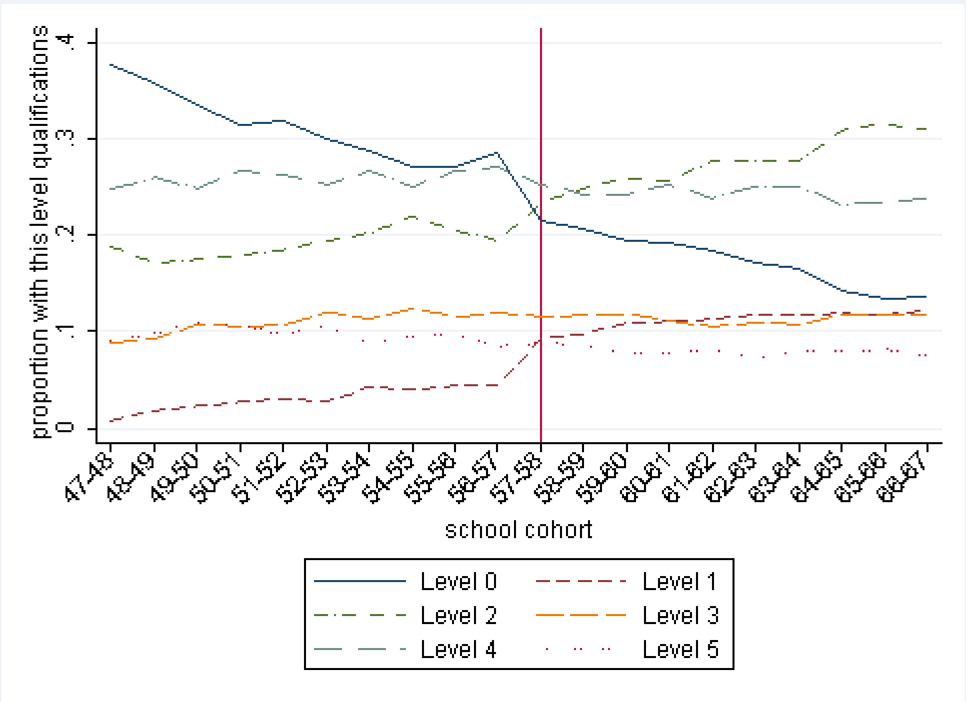


Figure 2: Proportion with academic qualifications at each NVQ level amongst leavers by age 16 or younger, by school cohort

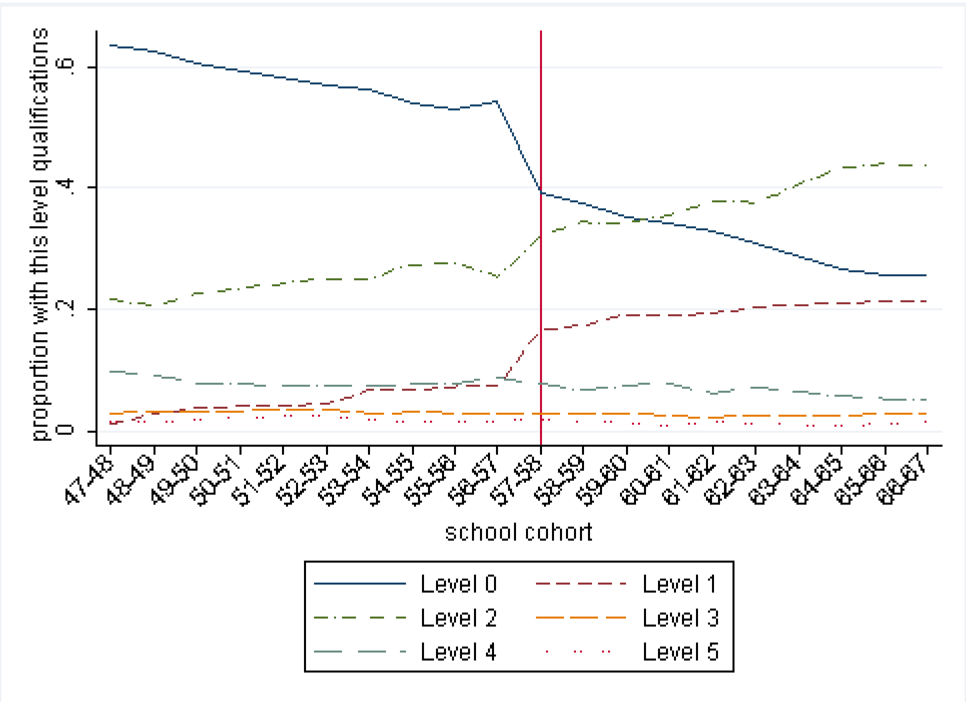


Figure3: Probability of attaining academic qualifications, by school cohort: September to January born versus February to August born

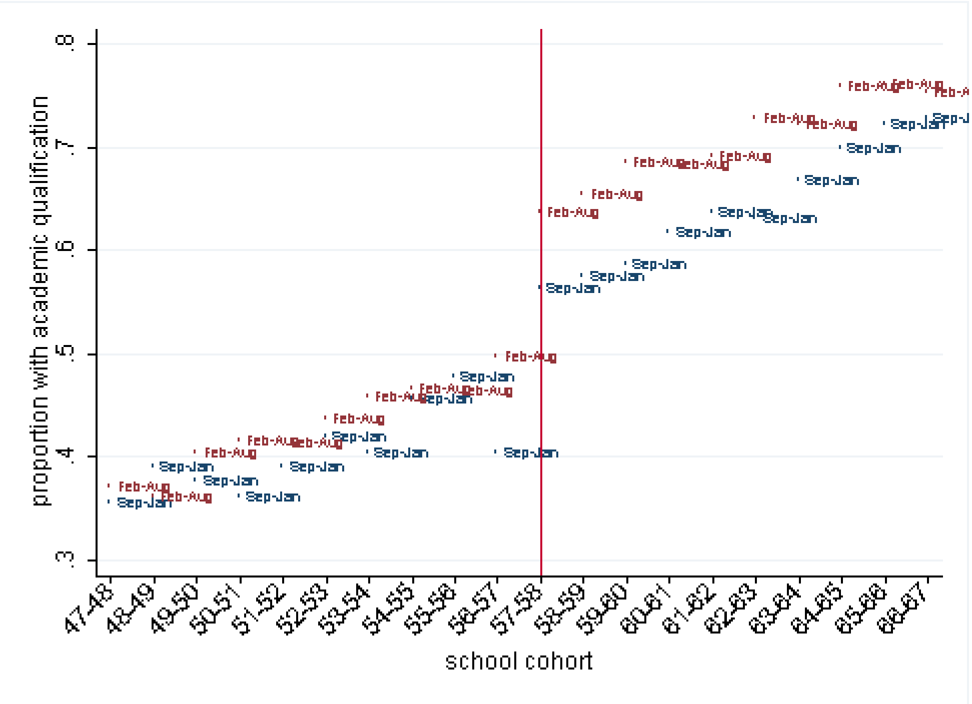
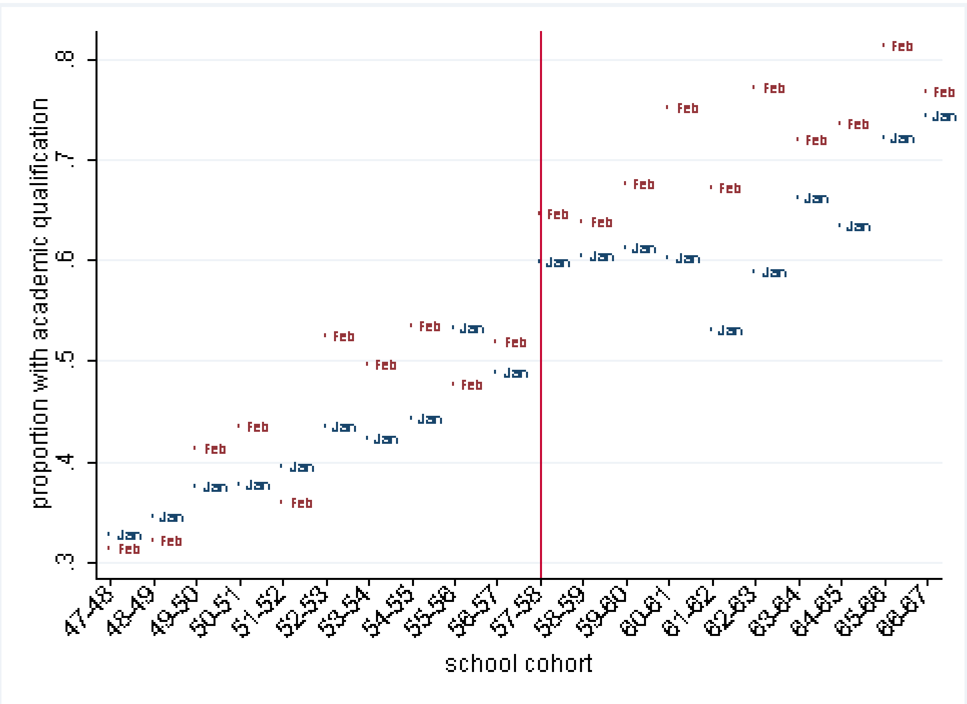


Figure 4: Probability of attaining academic qualifications, by school cohort: January born versus February born



Appendix

Table A1 NVQ Equivalent Qualifications Classification

NVQ equivalent	Academic qualification
Level 0	No nationally recognised academic qualifications
Level 1	CSE below grade 1, GCSE below grade C
Level 2	CSE grade 1, O-levels, GCSE grade A-C
Level 3	A-levels, A/S levels, SCE Higher, Scottish certificate of sixth year studies, international baccalaureate
Level 4	First/foundation degree, other degree, diploma in higher education
Level 5	Higher degree