Heterogeneous returns over the life-cycle? Or nothing at all?  
Re-examining the returns to education in the UK

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Abstract
This paper uses data from the UK Labour Force Surveys 1986-2011, the New Earnings Survey and the Annual Survey of Hours and Earnings 1975-2011 to re-examine the returns to education from the 1972 Raising of the School Leaving Age (RoSLA) reform. Importantly, the span of our data allows us to investigate whether the labour market returns from an additional year of schooling vary over the lifecycle.

Keywords: Returns to education, life-cycle, earnings, employment

JEL classification: I21, I28, J24

1. Introduction
A recent analysis of newly released UK household data suggests that the impact of the 1972 RoSLA reform had a long-run and permanent impact on hourly wages (Buscha and Dickson, 2012) for both men and women. This evidence shows that individuals who were affected by the school leaving age reform in 1972 earned on average 5% more in their early to mid-50s than similar individuals who were not affected by the ‘additional year of schooling’. Whilst this is good news for policy makers and believers in human capital theory – especially in light of the forthcoming raising of the participation ages to 17 and up to 18 in 2013 and 2015 respectively – these results are somewhat at odds with recent evidence on the returns to education in the UK. In fact, recent re-appraisals of the causal return to education (Devereux and Hart, 2010; Grenet, 2012 forthcoming) using more sophisticated identification techniques and larger datasets seem to suggest that the effect of an additional year of schooling is less than half of what was commonly estimated in studies from the 1990s and 2000s. The UK returns literature is thus in a state where ‘causal’ returns to schooling apparently vary between 3% and 20%.

To rationalise this emerging disparity in the returns to education Buscha and Dickson (2012) conjecture that life-cycle effects may play an important role in reconciling such differences. In other words, the returns to education could be non-constant over the life-cycle
and therefore the varying returns estimated in the UK literature can be explained by the fact that many of these studies use various data sources over different time periods. Furthermore, recent developments in the literature by Haider and Solon (2006) and Bhuller, et al. (2011) suggests that life-cycle effects are an important component in the assessment of the return to education and that constant returns should not be taken for granted. Bhuller et al. specifically warn of the danger of life-cycle bias and argue “… that it is necessary to pay close attention to difference in age composition when comparing estimates of the returns to schooling across countries, subgroups, or time” (Bhuller, et al. 2011: 20). However, although life-cycle bias is one possible way to explain the varying estimates of the return to education in the UK literature, other possible explanations exist, such as: non-causal identification across different studies, methodological differences (for example, different functional forms estimated and controls employed), dissimilarities in the derivation of the earnings variables, different educational variables, and different data sources (of which some may be more weaker than others).

In this paper we attempt to reconcile the varying estimates in the UK education literature by examining the return from the 1972 RoSLA over the life-cycle using a number of data sources. By exploiting the 1972 RoSLA we are able to make stronger causal claims on whether the return to education varies as individuals become older, and in addition, we make use of regression discontinuity design which has become a popular empirical strategy to achieve more robust causal inference. Moreover, we use two homogenous data sources covering approximately the same time period which allows us cross-validate our findings and exclude survey specific effects from our analysis. Following Grenet (2012) we use data from the Labour Force Survey to analyse the return to education in each year for the years 1993 to 2011. Likewise, following Devereux and Hart (2010), we also use data from the New Earnings Survey in addition to data from the Annual Survey of Hours and Earnings to present the return to education for each year for the years 1975 to 2011.

Our results suggest that significant heterogeneity in the return to education is apparent in our data sources. Our analysis of the LFS data shows that the impact of the 1972 RoSLA reform was at its highest when individuals were in their mid-30s to early-40s and that this return subsequently drops when individuals age beyond this. These findings are important as they suggest that a ‘catch-up’ effect exists by who have less education. Moreover, these findings explain some of the diverging results in the UK literature and we thus argue that lifecycle effects in the returns to education are an important component in the general analysis of returns to education.

The remainder of this paper is structured as follows. Section 2 reviews the literature whilst section 3 describes the raising of the school leaving age reform in 1972. Section 4 describes

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5 Robust employment information is available from 1986 onwards and we also make use of this. Grenet’s data was limited to the period 1993 to 2006.
the data and key measures used in our analysis. Section 5 details the empirical strategy whilst section 6 presents the results. Finally, section 7 concludes.

2. Literature Review – the returns to education

The literature on the returns to schooling has made great strides forwards since the seminal articles from the 1950s, 1960s and 1970s as evidenced by several high profile survey and review articles that have been published (such as Card, 1999 and 2001; Harmon et al., 2003; Heckman et al. 2003; Lemieux, 2006 and Polacheck, 2007). Although issues still remain, there have been a number of significant strides that have taken place since the middle of the last century when labour economists’ interest in causally identifying the returns to education began in earnest.

At the forefront of these innovations are the empirical implementation of instrumental variable techniques that attempt to causally estimate the effect of additional schooling on earnings. Specifically, papers using the exogenous variation in the school leaving age laws (Angrist and Krueger, 1991 and Harmon and Walker, 1995) began to make convincing arguments that it is possible to estimate robust causal returns to education. Various other techniques, such as twin studies, 2nd moments or different instruments (see Klein and Vella, 2009) have also been proposed by the literature but to date instrumental variable regressions using school leaving age laws remain the most widely accepted form of estimating causal returns to education. As such, there have been a wide range of studies making use of this method to analyse not only wage returns to education but also the causal effect of education on a number of other outcomes inter alia health, employment and crime (see next section for an overview).

Within this instrumental variable literature a revisionist view has recently emerged: early UK evidence on the returns to education by Harmon and Walker (1995), Harmon and Walker (1999) and Chevalier and Walker (2002) suggested that one additional year of schooling is associated with relatively large returns to earnings (in the order of 15-20%). However, more recent evidence on the matter from Devereux and Hart (2010) and Grenet (2012) appears to suggest smaller causal estimates in the range of 3-8%, per additional year of schooling. One argument for reconciling these different estimates is that larger and better data sources in addition to newer and more precise regression techniques (regression discontinuity design) have allowed for accurate picture to emerge – one that significantly reduces the causal return to education.

A different explanation, put forward by Buscha and Dickson (2012), is that due to data constraints and cross-sectional pooling, various papers have examined returns to education at different points on the lifecycle. This is exemplified in Figure 1 which illustrates clearly that the UK education literature has not consistently estimated returns at similar points across the lifecycle. Therefore, another explanation in reconciling these two strands of the UK returns

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6 It should be noted that the interpretation of IV estimates must be seen within the context of a Local Average Treatment Effect (LATE) which limits the causal effects to only a subsample of compliers.
literature is that returns may vary over the lifecycle and that the impact of additional education diminishes as individuals become older. However, Buscha and Dickson (2012) were unable to bring clarity to this debate as they use a relatively small sample size to estimate returns to education late in the lifecycle and this resulted in a large standard error around their estimate.

Nevertheless, the suggestion that lifecycle bias may play an important role when estimating the returns to education has also been proposed by Buller et al. (2011) who show clearly that the return to schooling for Norwegian men varies across the lifecycle. Using administrative data and using school leaving age as identification method they show that the return to one additional year of schooling is non-existent at age 28, 10% at age 33, peaks at 15% at age 43 and then diminishes to 13% at age 58. Therefore, in this paper, we aim to test the hypothesis that returns to education vary across the lifecycle in the UK.

3. Raising of the School Leaving Age in 1972

The raising of the school leaving age (RoSLA) in the UK has occurred twice since the end of the Second World War: to some controversy in the immediate post-war period in 1947 and most recently in 1972. The original foundation for both pieces of legislation can be found in the Education Act of 1944 which increased the compulsory schooling leaving age from 14 to 15 on
the 1st April 1947 whilst at the same time providing the President of the Board of Education with the powers to raise the school leaving age to 16 as soon as it was deemed practicable (Education Act, 1944). This finally occurred in 1971 and from 1st September 1972 all children attending schools in England and Wales were required to stay on until the age of 16.7

The 1972 RoSLA event therefore affected all individuals who born on or after the 1st September 1957. Anybody born after this date was subject to a minimum of 11 years of schooling whilst those born before could have received a minimum of 10 years of schooling. In addition, because the nature of the UK schooling system implies that the minimum entry and exit ages are regulated at a national level there was little scope for regional variations to exist (in contrast the United States education system).

The RoSLA events that took place in 1947 and 1972 provide quasi-experimental variation in the average number of years of education for cohorts born around the discontinuities induced by the policy changes – i.e. the cohorts born either side of 1st April 1933 and 1st September 1957. This variation has been exploited in a number of studies examining not only the impact on hourly wages but also the effect on alternative outcomes such as health, crime and voting behaviour. Table 1 summarises the various findings, the upper panel focusing on earnings effects while the lower panel documents impacts on the additional outcomes examined in the literature.

7 Interestingly, there is a certain amount of nuance in the decision to set RoSLA in the year 1972. The original commitment to introduce “secondary education for all” was taken in 1964 but due to various budgetary issues, projected staffing and building shortages in addition to continued societal debate about the merits of increased compulsory schooling it was decided to let many of post-war the baby boomers pass through the education system first. More of the historical context can be found in McCulloch, et al. (2012).
### Table 1: A Review of the Impact of the School Leaving Age Reforms of 1947 and 1972 and its effects

<table>
<thead>
<tr>
<th>Study</th>
<th>Data</th>
<th>RoSLA</th>
<th>Effect of RoSLA on education</th>
<th>Effect of RoSLA on outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Earnings</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Harmon and Walker (1995)</td>
<td>Pooled Family Expenditure Survey 1978 to 1986</td>
<td>1947 and 1972</td>
<td>0.541 additional years of schooling for the 1947 change and 0.110 additional years for the 1972 change</td>
<td>IV estimate of 15% on hourly wages for men</td>
</tr>
<tr>
<td>Deveraux and Hart (2010)</td>
<td>Pooled General Household Survey’s (GHS) 1979 to 1998 and the Pooled New Earnings Survey Panel (NESPD) 1975 to 2001</td>
<td>1947</td>
<td>0.469/0.397 (GHS/NESPD) additional years of schooling for men 0.550/0.511 (GHS/NESPD) additional years of schooling for women</td>
<td>The 1947 reform had a reduced form effect of 2% for hourly wages for men. No effect for women.</td>
</tr>
<tr>
<td>Dickson and Smith (2011)</td>
<td>Pooled Quarterly Labour Force Survey 1993 to 2010</td>
<td>1972</td>
<td>Proportion with no academic qualifications fell by 0.071; those with level 1 qualifications increased by 0.047; those with level 2 qualifications increased by 0.041. No effect on level 3+ qualifications</td>
<td>No effect on log hourly wages A positive (reduced form) employment effect of 9%</td>
</tr>
<tr>
<td>Grenet (2012)</td>
<td>Pooled Quarterly Labour Force Survey 1993 to 2004</td>
<td>1972</td>
<td>0.274 additional years of schooling for men 0.317 additional years of schooling for women</td>
<td>The 1972 reform has a reduced from effect of 2-3% on hourly wages for men.</td>
</tr>
<tr>
<td>Dickson (2012)</td>
<td>British Household Panel Survey 1991 to 2006</td>
<td>1972</td>
<td>0.564 additional years of schooling for men (men only examined)</td>
<td>An additional year of schooling (IV estimate) increases hourly wages by 10% for men.</td>
</tr>
<tr>
<td>Study</td>
<td>Data</td>
<td>RoSLA</td>
<td>Effect of RoSLA on education</td>
<td>Effect of RoSLA on outcome</td>
</tr>
<tr>
<td>-----------------------</td>
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</tr>
<tr>
<td>Buscha and Dickson (2012)</td>
<td>UK Household Longitudinal Study 2011</td>
<td>1972</td>
<td>0.225 additional years of schooling for men</td>
<td><strong>Hourly wages</strong> increased by 5% for men and 6% for women.</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>0.356 additional years of schooling for women</td>
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<td></td>
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<td><strong>Hourly wages</strong> increased by 5% for men and 6% for women.</td>
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<td><strong>Hourly wages</strong> increased by 5% for men and 6% for women.</td>
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<td></td>
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<td></td>
<td>Relative to those aged 14 the 1947 change increased the</td>
<td>There is no effect on the probability of voting</td>
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<td></td>
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<td></td>
<td>average age before drop out by 0.512 and the 1972 change by</td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td>0.953</td>
<td></td>
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<tr>
<td>Milligan <em>et al.</em> (2004)</td>
<td>Pooled British Election Studies 1964, 74, 79, 83, 87, 92 and 97</td>
<td>1947 and 1972</td>
<td>0.593 additional years of schooling for the 1947 change and 0.186 additional years for the 1972 change</td>
<td>One more year of education (IV estimate) increase probability of self-reported good health by approximately 7%</td>
</tr>
<tr>
<td>Siles (2009)</td>
<td>Pooled General Household Survey’s 1980 to 2004</td>
<td>1947 and 1972</td>
<td>0.593 additional years of schooling for the 1947 change and 0.186 additional years for the 1972 change</td>
<td>One more year of education (IV estimate) increase probability of self-reported good health by approximately 7%</td>
</tr>
<tr>
<td>Clark and Royer (2010)</td>
<td>Pooled Health Survey of England 1991 to 2004</td>
<td>1947 and 1972</td>
<td>1947 = +0.420yrs of schooling for men 1947 = +0.527yrs of schooling for women 1972 = +0.318yrs of schooling for men 1972 = +0.252yrs of schooling for women</td>
<td>No significant effect on health indicators</td>
</tr>
<tr>
<td>Machin <em>et al.</em> (2011)</td>
<td>Pooled General Household Survey’s 1972 to 1996</td>
<td>1972</td>
<td>5.7% drop in the proportion of individuals with no qualification and an increase of 0.221 years of additional schooling</td>
<td>RoSLA significantly impacted crime rates. The reduced from effect on the conviction rate is - 5%.</td>
</tr>
<tr>
<td>Siles (2011)</td>
<td>Pooled General Household Survey’s 1978 to 2004</td>
<td>1947 and 1972</td>
<td>0.466 additional years of schooling for the 1947 change and 0.233 additional years for the 1972 change</td>
<td>No effect on early childbirth from 1947 reform. 1972 reform reduced the probability of a teen birth by 15%</td>
</tr>
</tbody>
</table>
4. Data

Our data come from a number of sources. Firstly, the British Labour Force Survey (LFS). We combine the annual LFS datasets from 1986 to 1991 and supplement this with the Quarterly LFS data, pooled from 1992 quarter one to 2011 quarter four 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 are taking their second interview and so on. The annual LFS did not contain earnings information, however this is available in the quarterly LFS data thus the earnings analysis is restricted to the years after 1991.\(^8\) Earnings information is asked of individuals in their first and fifth wave interviews potentially providing two observations per individual. However, there is a great deal of missing data which may not be missing at random, therefore to avoid including individuals more than once, we restrict the earnings estimations to include only the information from an individual's fifth wave interview.\(^9\) We select individuals resident in England and Wales and exclude those living in Scotland or Northern Ireland and those who were born outside of England and Wales unless they moved to Britain prior to commencing their secondary education. For the earnings analysis we exclude the self-employed and those who first left full-time education after the age of 25. We deflate earnings into 2005 £s and trim the earnings distribution to remove the top and bottom 1% of the distribution by gender.

5. Methodology

To causally identify the effect of education on wages and employment we follow the now standard approach of using instrumental variable (IV) methods (see for example, Machin, et al. 2011). In this approach a causal effect of education is achieved via the inclusion of a RoSLA 1972 dummy variable in the first stage education regression in a two-stage least square framework. By arguing that the RoSLA 1972 event was an exogenous occurrence which increased levels of education randomly we can obtain a causal IV estimate as follows:

\[ E_i = \alpha_1 + \beta_1 \text{RoSLA}72_i + \mathbf{x}' \gamma + \epsilon_i \]  

\[ y_i = \alpha_2 + \beta_2 \text{RoSLA}72_i + \mathbf{x}' \gamma + \epsilon_i \] 

\(^8\) The earnings information is not present in the QLFS for 1992 thus our earnings analysis is carried out on data from 1993 quarter 1 onwards. Month of birth is only included in the special licence datasets from 2003 onwards hence we acknowledge the support of the Office for National Statistics and the Economic and Social Data Service in providing access to this special licence data.

\(^9\) This decision was governed by the fact that there is less missing earnings data amongst the fifth wave observations and also in order to maintain comparability with Grenet (2012) who uses only wave five observations.
where (1) and (2) are the reduced form equations for education $E_i$ and labour market outcome $y_i$ (log hourly wages or employment), $\beta_1$ is the estimate of the RoSLA 1972 dummy on education (years of schooling) whilst $\beta_2$ is the estimated effect of the RoSLA 1972 event on labour market outcomes. If needed, $x'_i$ can be vector of additional control variables (such as age and gender) with parameter estimates of $\gamma$. Finally, $e_i$ is a normally distributed error term with mean zero. The structural form for labour market outcomes $y_i$ is then given by:

$$y_i = \alpha_3 + \beta_3 E_i + x'_i \gamma + e_i$$ (3)

where the IV estimate of $\beta_3$ in (3) is given by the ratio of the reduced form coefficients in (1) and (2), $\beta_3 = \beta_2 / \beta_1$.

As highlighted in Table 1, the UK RoSLA literature has generally agreed that the estimated effect of 1972 event on education was to raise average years of schooling by approximately 0.25 years ($\hat{\beta}_1 = 0.25$). It is thus possible to obtain some estimated measure of $\hat{\beta}_3$ by only estimating the reduced form equation (1) – in order to obtain $\hat{\beta}_1$ – and then using out-of-sample estimation for $\beta_2$ (Devereux and Hart, 2010). However, because so many studies have consistently estimated $\hat{\beta}_2$ using various datasets it can be argued that not even an out-of-sample estimate for $\beta_2$ is necessary. It is for this reason that we focus much of our attention on estimating a precise effect of $\hat{\beta}_1$ in the reduced form equation (2), although we will also produce estimates of $\hat{\beta}_2$ using the LFS.

Assignment to the ‘treatment’ of the 1972 RoSLA is based on an individual’s date of birth and although the date of birth is exogenous for individual $i$, it would make little sense to compare individuals who were born too many years apart due to cohort, generational and/or lifecycle effects which contaminate exogenous differences between such individuals. It is possible to control for such differences by including a series of age or cohort controls in the vector $x'_i$ and traditionally, $\beta_1$ and $\beta_2$ are estimated like this using a 2SLS framework (see inter alia Harmon and Walker, 1995). However, a disadvantage of 2SLS is in its linear and parametric restrictions. Given the large sample qualities of our data (LFS and particularly ASHE), we are able to implement a regression discontinuity design (RDD) in order to estimate the parameters $\beta_1$ and
Following the approach outlined in Imbens and Lemieux (2008), and implemented on LFS data by Grenet (2012), we use non-parametric techniques where the effect of RoSLA 1972 on education and labour market outcomes is estimated by local linear regression in a local region near the discontinuity.

To estimate the values $\beta_1$ and $\beta_2$ we fit a linear regression function to observations within distance $h$ on either side of the discontinuity point (1957 September 1st)

$$
\min_{\alpha_{pre-1957};\beta_{pre-1957}} \sum_{i \in \mathcal{X}_i \leq \mathcal{C} \leq \mathcal{X}_i \leq h} (Y_i - \alpha_{pre-1957} - \beta_{pre-1957} \cdot (X_i - c))^2
$$

and

$$
\min_{\alpha_{post-1957};\beta_{post-1957}} \sum_{i \in \mathcal{X}_i \leq \mathcal{C} \leq \mathcal{X}_i \leq h} (Y_i - \alpha_{post-1957} - \beta_{post-1957} \cdot (X_i - c))^2
$$

Where $Y_i$ is the outcome variable in question (years of schooling and labour market outcomes), $X_i$ is the number of months and individual is born before/after the 1957 September 1st discontinuity, the $\alpha$s and $\beta$s are the regression intercepts and slope values computed for data in the region surrounding the discontinuity, $c$, with bandwidth, $h$. These values are calculated twice, once for the left hand side of $c$ (born pre-1957) and once for the right hand side (born post-1957) of $c$. The intercept values at the discontinuity, $\mu_{pre-1957}(c)$ and $\mu_{post-1957}(c)$, can then computed by:

$$
\hat{\mu}_{pre-1957}(c) = \hat{\alpha}_{pre-1957} + \hat{\beta}_{pre-1957} \cdot (c - \mathcal{C}) = \hat{\alpha}_{pre-1957}
$$

and

$$
\hat{\mu}_{post-1957}(c) = \hat{\alpha}_{post-1957} + \hat{\beta}_{post-1957} \cdot (c - \mathcal{C}) = \hat{\alpha}_{post-1957}
$$

Given these estimates we can then compute the treatment effect of RoSLA 1972 on education $E_i$ and labour market outcomes $y_i$ as follows:

$$
\hat{\beta}_1 = \hat{\alpha}_{pre-1957} - \hat{\alpha}_{post-1957} \text{ if } Y = E_i
$$

$$
\hat{\beta}_2 = \hat{\alpha}_{pre-1957} - \hat{\alpha}_{post-1957} \text{ if } Y = y_i
$$

These non-parametric estimates of $\beta_1$ and $\beta_2$ can be made more sophisticated by using weights that decrease smoothly as the distance from the cut-off point increases (for example, Machin et al, 2011 use of inverse distance weighting where observations nearer to $c$ contribute more than observations far away) although Imbens and Lemieux (2008) suggest that additional
weighting in local linear regression rarely matters. They suggest that from a practical point of view a simple rectangular kernel should be used and that the robustness of the results be verified by using different choice of bandwidths. We therefore report results for varying bandwidths but limit these to multiples of 12 for LFS data \( h = [12, 24, 36, 48, 60, 72] \) to avoid possible contamination by within-year month of birth effects. For ASHE data the absence of precise month of birth information implies using yearly bandwidths (with all those born in 1957 excluded because we cannot assign accurately assign them to treatment/non-treatment) so that \( h = [1, 2, 3, 4, 5, 6] \). Finally, because local linear regression techniques can be problematic when bandwidth sizes are low, we complement our estimation of \( \beta_1 \) and \( \beta_2 \) with mean-differences for a smaller set of bandwidths which are more local to the discontinuity. Finally, we produce the parameters estimates of \( \hat{\beta}_1 \) and \( \hat{\beta}_2 \) for each year from 1985-2011. This allows us assess to what extent the RoSLA 1972 education reform influenced labour market outcomes across the life-cycle.

6. Results

**Pooled QLFS**

Firstly we present estimates of the impact of the 1972 RoSLA on education and wages pooling the long range of QLFS datasets. Table 2a presents our RDD estimates of the reduced form impact of RoSLA 1972 on years of education and log hourly wages for bandwidths from 12 months up to 72 months. Moreover, we are able to compare a more traditional parametric approach but restricting our sample to include just those individuals born in the school cohort immediately before and cohort immediately after the 1972 RoSLA. Given the size of each QLFS cross-sections even with this restriction we still maintain a sample of over 10,000 observations. As these individuals are all approximately the same age at each survey and in contiguous cohorts, we are able to circumvent issues of bias introduced by lifecycle effects and cohort effects. Table 2b presents these more traditional “pooled” reduced form estimates, with a full set of year dummies included to remove survey year effects.

As can be seen in these Tables, we find a consistent picture of results from the RDD on pooled data and the more traditional pooled reduced form results. Following Lee and Lemieux (2010) we concentrate on the higher bandwidths which you more data and should allow a more accurate estimate of the discontinuity in outcomes at the point of RoSLA. The narrower bandwidths could be in danger of distorting the estimate by including age within school-year effects and are

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10 For the QLFS analysis we use rolling 3-year samples from the LFS so rather than looking at each year from 1993 to 2011, the 1993 sample contains 1993, 1994 and 1995, the 1994 sample contains 1994, 1995 and 1996 and so forth.
therefore likely to be somewhat upward biased. For bandwidths of 48, 60 and 72, we find a significant reduced form impact on wages of around 2%. The impact on years of education is also statistically significant and about 0.3 years – suggesting that just under one-third of the men in the immediate post-RoSLA cohorts were bound by the reform. Similarly the more traditional reduced form approach estimates in Table 2b show a first stage impact of 0.3 additional years of education induced by RoSLA and a reduced form impact of 1.9% on wages. These estimates, using a slightly longer dataset than that used by Grenet (2012), confirm the more recent findings in the UK literature: that the reduced form impact of RoSLA 1972 – averaged across almost two decades of the lifecycle – is approximately a significant 2% increase in hourly wages. As it is estimated that just under one third of the men in these cohorts were in the complier group for this reform, the implied local average treatment effect estimate of the return to an additional year of education is approximately 6%.

Table 2a: Regression Discontinuity Design Estimates of the effect of the 1972 RoSLA on log hourly wages and years of schooling, various bandwidths; pooled Quarterly LFS data, 1993q1-2011q4

<table>
<thead>
<tr>
<th>Bandwidth</th>
<th>log hourly wage</th>
<th>Years of schooling</th>
</tr>
</thead>
<tbody>
<tr>
<td>12</td>
<td>Coeff. 0.040**</td>
<td>0.407***</td>
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<tr>
<td></td>
<td>Std. Err. 0.021</td>
<td>0.100</td>
</tr>
<tr>
<td>24</td>
<td>Coeff. 0.031**</td>
<td>0.430***</td>
</tr>
<tr>
<td></td>
<td>Std. Err. 0.015</td>
<td>0.069</td>
</tr>
<tr>
<td>36</td>
<td>Coeff. 0.020**</td>
<td>0.353***</td>
</tr>
<tr>
<td></td>
<td>Std. Err. 0.012</td>
<td>0.056</td>
</tr>
<tr>
<td>48</td>
<td>Coeff. 0.026**</td>
<td>0.341***</td>
</tr>
<tr>
<td></td>
<td>Std. Err. 0.010</td>
<td>0.048</td>
</tr>
<tr>
<td>60</td>
<td>Coeff. 0.019**</td>
<td>0.303***</td>
</tr>
<tr>
<td></td>
<td>Std. Err. 0.009</td>
<td>0.043</td>
</tr>
<tr>
<td>72</td>
<td>Coeff. 0.014**</td>
<td>0.270***</td>
</tr>
<tr>
<td></td>
<td>Std. Err. 0.009</td>
<td>0.039</td>
</tr>
</tbody>
</table>

* p<0.10, ** p<0.05, *** p<0.01

There is a UK literature showing that children born later in the school year suffer a penalty in education and later labour market outcomes as a result of being up to a year younger than some of their peers within their school year, see Crawford et al (2011). There is a danger therefore that lower bandwidths compare August born and September born children who are not directly comparable because of these within-year effects.
Table 2b: Reduced form and Two-Stage Least Squares Estimates of the effect of the 1972 RoSLA; pooled Quarterly LFS data, 1993q1-2011q4

<table>
<thead>
<tr>
<th></th>
<th>log hourly wage</th>
<th>Years of schooling</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Reduced Form</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Coeff.</td>
<td>0.019*</td>
<td>0.301***</td>
</tr>
<tr>
<td>Std. Err.</td>
<td>0.010</td>
<td>0.048</td>
</tr>
<tr>
<td><em>F-statistic on instrument</em></td>
<td></td>
<td>39.32</td>
</tr>
<tr>
<td><strong>2SLS Results</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Coeff.</td>
<td>0.064**</td>
<td></td>
</tr>
<tr>
<td>Std. Err.</td>
<td>0.031</td>
<td></td>
</tr>
<tr>
<td><strong>N = 10124</strong></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* p<0.10, ** p<0.05, *** p<0.01

Notes: for Table 2b only individuals born in +/- 12 months around 1 Sept 1957 included in the sample; a full-set of survey year dummies included in each regression

Lifecycle Results

However, as outline above, by exploiting the large datasets we have constructed, we are able to examine the first stage and reduced form effects at each point of the lifecycle – using LFS data from 1993 to 2011 corresponding to ages from 36 to 54 for the relevant cohorts born just before and just after September 1957. We now present these results visually via a series of plots illustrating the estimated effects when different bandwidths around the discontinuity event are employed.

Figures 2 and 3 show for men the impact of RoSLA on average years of schooling and log hourly wages when estimating using the RDD methodology outlined in section 5. As above, results presented use six different bandwidths around the discontinuity and again we will concentrate our analysis on the bandwidths in the lower panel of each picture: estimates that include observations +/- 48, 60 and 72 months either side of the discontinuity. The first observation that we make is that the estimated first stage effect of RoSLA on years of education is consistently estimated for the majority of surveys in the data – averaging around 0.35 additional years of education induced by the policy change. As Table 1 confirms, this is consistent with the majority of findings in the literature to date. It is worth noting however that there is variation in the size of the first stage effect, depending on at what age the respondents are asked about their education. Given that these estimates are all taken from random samples from the same
Figure 2: Estimated impact of RoSLA72 on average years of schooling

![Graphs showing the estimated impact of RoSLA72 on average years of schooling for different ages (12, 24, 36, 48, 60, 72 years) using local linear regression.](image)

- Coeff
- Smoother
- Upper 95% CI
- Lower 95% CI

Graphs by bandwidth. Rolling 3-year data. Uses LFS income weights.

Figure 3: Estimated impact of RoSLA72 on log hourly wages, RDD estimates using local linear regression

![Graphs showing the estimated impact of RoSLA72 on log hourly wages for different ages (12, 24, 36, 48, 60, 72 years) using local linear regression.](image)

- Coeff
- Smoother
- Upper 95% CI
- Lower 95% CI

Graphs by bandwidth. Rolling 3-year data. Uses LFS income weights.
population but interviewed in different years, and that in each case the samples are between 5,000
and 10,000 individuals, this suggests that sampling variation may explain a portion of this
variance however we should be concerned about the extent to which Two-Stage Least Squares
estimates in the literature to date may be influenced by this variation in the first stage impact of
RoSLA.

Moving on to Figure 3, again concentrating on the estimates for bandwidths 48, 60 and 72, a
similar pattern emerges in each graph: there is a positive and significant (at 5%) reduced form
impact of RoSLA on wages of around 5% for the years when the affected individuals are in their
mid 30s to early 40s. After these ages the estimated impact falls and remains insignificantly
different from zero. Figure 4 illustrates the same pattern, this time using just mean comparisons
either side of the discontinuity. In this case, as there is no local linear smoothing either side of the
discontinuity, it is important to use a narrower bandwidth. Reassuringly, the 12-month bandwidth
gives estimates almost exactly matching those in the lower panel of Figure 3. As the bandwidth is
increased in Figure 4 the estimates are distorted by business cycle effects owing to the data
coming from different LFS surveys hence the greater variation induced.

Figure 4: Estimated impact of RoSLA72 on log hourly wages, RDD estimates using mean
comparison

![Graphs by bandwidth. Rolling 3-year data. LFS income weights used]
Table 3 presents the individual RDD coefficient estimates for ages from 36 to 53. Note that these ages represent samples that include individuals at the specified age or up to two years older i.e. the RDD for age 36 includes individuals aged 36, 37 or 38. The Table presents the estimates when a bandwidth of 60 is used.

Table 3: Regression Discontinuity Design Estimates of the Reduced Form impact of RoSLA 1972 on log hourly wages at different points in the life-cycle; bandwidth 60 months

<table>
<thead>
<tr>
<th>Age</th>
<th>Coeff.</th>
<th>Std. Err.</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>36</td>
<td>0.028</td>
<td>0.025</td>
<td>7349</td>
</tr>
<tr>
<td>37</td>
<td>0.060</td>
<td>0.023</td>
<td>8039</td>
</tr>
<tr>
<td>38</td>
<td>0.062</td>
<td>0.023</td>
<td>8762</td>
</tr>
<tr>
<td>39</td>
<td>0.056</td>
<td>0.022</td>
<td>9175</td>
</tr>
<tr>
<td>40</td>
<td>0.035</td>
<td>0.022</td>
<td>9411</td>
</tr>
<tr>
<td>41</td>
<td>0.054</td>
<td>0.022</td>
<td>9259</td>
</tr>
<tr>
<td>42</td>
<td>0.038</td>
<td>0.023</td>
<td>8904</td>
</tr>
<tr>
<td>43</td>
<td>0.030</td>
<td>0.024</td>
<td>8499</td>
</tr>
<tr>
<td>44</td>
<td>0.005</td>
<td>0.025</td>
<td>7995</td>
</tr>
<tr>
<td>45</td>
<td>-0.013</td>
<td>0.025</td>
<td>7604</td>
</tr>
<tr>
<td>46</td>
<td>-0.019</td>
<td>0.026</td>
<td>7161</td>
</tr>
<tr>
<td>47</td>
<td>-0.014</td>
<td>0.027</td>
<td>6831</td>
</tr>
<tr>
<td>48</td>
<td>0.031</td>
<td>0.028</td>
<td>6262</td>
</tr>
<tr>
<td>49</td>
<td>0.016</td>
<td>0.030</td>
<td>5518</td>
</tr>
<tr>
<td>50</td>
<td>-0.005</td>
<td>0.033</td>
<td>4561</td>
</tr>
<tr>
<td>51</td>
<td>-0.019</td>
<td>0.038</td>
<td>3797</td>
</tr>
<tr>
<td>52</td>
<td>0.024</td>
<td>0.047</td>
<td>3145</td>
</tr>
<tr>
<td>53</td>
<td>0.017</td>
<td>0.074</td>
<td>2534</td>
</tr>
</tbody>
</table>

* p<0.10, ** p<0.05, *** p<0.01

The table confirms the size and significance of the impacts illustrated in Figure 3: for ages from 37 to 42 there is a significant positive impact of the 1972 RoSLA on wages, varying in size between 3.5% and 6.2% and for the most part this is significant at the 5% level or the 1% level. The average size of the estimated reduced form effect across these ages is 5.1%. These estimates derive from RDD regressions with more than 8,000 observations, the size of available samples not falling substantially until later ages – thus the lack of significance in estimates after age 42 does not appear to be due to insufficient sample size.12

12 These estimates are robust to the choice of kernel (we use rectangular as per the best practice advised by Lee and Lemieux, 2010) however using a triangular kernel gives comparable estimates. Estimates for bandwidths 48 and 72 are very similar to the results in Table 3, see Figure 3 for visual confirmation of this.
The evidence therefore from these RDD regressions estimated for different points in the lifecycle suggest that the pooled reduced form estimate of approximately 2% reported in the recent literature – and replicated here with our data – masked substantial heterogeneity in the impact of RoSLA over the lifecycle. Early years see a substantial wage premium in the range of 5% which would imply a LATE estimate of the return to a year of education of 15%. Estimates for ages between 42 and 50 are much smaller, even negative in some cases, close to zero and are not close to being statistically significant. The corresponding LATE estimates would of course therefore be zero.

<<ASHE and NES results to be inserted here>>

7. Discussion and Conclusion

In this paper we have revisited some of the evidence on the returns to education and attempted to provide an explanation for the varying returns to education reported in the UK literature. Of particular interest was whether (causal) returns vary over the lifecycle and initial results appear to suggest that this is indeed the case. Making use of 20 years of LFS data and exploiting the exogenous variation in the leaving of the school leaving age reform in 1972 we find an average reduced form effect of 2 percentage points (which implies a 6% LATE effect) which is robust to a choice of various bandwidths. Such results are in line with recent evidence by Grenet (2012) and Devereux and Hart (2010) which have both suggested that the returns to education are lower than previously assumed.

However, on further inspection there is strong evidence to suggest that much of this return materialises at points in the lifecycle between mid-30s and early-40s and that the effect of additional education disappears as individuals when individuals are young or old. There is some evidence to suggest that earning rise again in the mid-50s – which corresponds to similar findings produced Buscha and Dickson (2012) – however, this evidence remains marginal. This finding may offer an explanation for the divergent findings in the UK literature; early papers examining the return to schooling would only have been able to look at early lifecycle effects whilst later papers included the falling rate of return to education as individual’s age.

We therefore conclude that returns to education are heterogeneous over the lifecycle and that individuals who receive a positive educational shock will not maintain their wage advantage into the long-run. Although the total wealth accumulation by individuals with more education will be higher compared to those with less education, their final hourly pay does not appear to be significantly different. Moreover, our results are in line with recent evidence produced by Bhuller et al. (2011) who also find variation in returns to education over the lifecycle for Norwegian cohorts.
In seeking to explain these apparent non-constant returns to education over the lifecycle one argument may be that the increased skill depreciation by more educated individuals leads to varying returns i.e. for whatever reason, educated individual lose skills at a faster pace than non-educated individuals. Alternatively, it might be that educational returns over the lifecycle are closely linked with macro-economic business cycles; where education favours individuals during poor times whilst it plays a lesser role during good times when opportunities are plentiful. However, we are unable to address these questions in this paper and argue that this is likely to be a fruitful avenue for future research

References


