The wage returns to education over the life-cycle: Heterogeneity and the role of experience*

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Abstract

This paper considers the implications of using regression discontinuity methods (RDD) on pooled data to estimate the causal effect of education on earnings, in particular when exploiting changes in minimum school leaving age requirements. RDD methods estimate the return to schooling inclusive of the impact of lost (potential) labour market experience as a result of being in school for an additional year (i.e. the "net" return). This is in contrast to the more parametric approaches to instrumental variables estimation used in the literature of the 1990s and 2000s, which estimated the "gross" return, conditioning out differences in (potential) experience. This has implications for interpreting the returns to schooling and we demonstrate the importance of the distinction using the 1972 Raising of the School Leaving Age (RoSLA) in England and Wales. Using data from the New Earnings Survey Panel Dataset 1975-2011, we illustrate the effect of the lost labour market experience at the RoSLA discontinuity on estimates of the return to education. When the "experience penalty" is not taken into account, RDD estimates suggest a significant *negative* return to additional education whilst when the experience component is taken into account, a positive effect is found, in line with earlier parametric studies. Moreover, we show that there is substantial variation in the impact of RoSLA over the lifecycle, which is often masked by studies that estimate the return over many years of pooled data. Together these factors explain the range of estimates of the return to education in the UK literature.

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

JEL classification: I21, I28, J24

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1. Introduction

The 1990s saw a new wave of UK literature on the returns to education, with studies for the first time exploiting minimum school leaving age reforms to derive causal estimates of the impact of additional schooling on earnings.[3] The consensus across these studies was that the return to education for UK men lay in the range of 15-20%. Whilst this is good news for policy makers

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[3] See Harmon and Walker, 1995; Harmon and Walker, 1999; Chevalier and Walker, 2002.

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and believers in human capital theory – especially in light of the raising of the participation age in England and Wales to 17 in 2013 and up to 18 in 2015 – these early results have since been called into question. In fact, recent re-appraisals of the causal return to education (Devereux and Hart, 2010; Grenet, 2013) 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 previously estimated. The UK returns literature is therefore in a state where 'causal' returns to an extra year of schooling apparently vary between 3% and 20%.

One possible explanation for this emerging disparity in the returns to education is the role of life-cycle effects. That is, the returns to education could be non-constant over the life-cycle and therefore the varying returns estimated may be explained by the fact that many of these studies use differing 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 caution regarding the necessity to pay close attention to differences in age composition when comparing estimates of the returns to schooling. 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 literature by examining the return over the life-cycle from the 1972 RoSLA, using a number of data sources but the same methodology and variable definitions. The range of the data available allows a large proportion of the lifecycle of the relevant cohorts to be examined. Moreover, by exploiting the 1972 RoSLA we are able to make stronger causal claims on whether the return to education varies as people age. In addition, we make use of regression discontinuity design (RDD) which has become the standard empirical strategy to achieve more robust causal inference. We use two homogenous data sources covering approximately the same time period, which allows us to cross-validate our findings and exclude survey specific effects from our analysis. Following Grenet (2013) we use data from the Labour Force Survey (LFS) to analyse the return to education in each year for the years 1993 to 2011.[4] Likewise, following Devereux and Hart (2010), we also use data from the New Earnings Survey Panel Dataset (NESPD) to present the return to education for each year for the years 1975 to 2011.

[4] Terminal education age information is available from 1986 onwards and we also make use of this. Grenet's data was limited to the period 1993 to 2006.

Our results suggest that there is significant lifecycle heterogeneity in the return to education. Analysis of the NESPD data shows that the impact of the 1972 RoSLA was initially *negative*: individuals affected earning lower hourly wages from their late teenage years through to their early 30s. Between the mid-30s and early 40s there is not a significant difference in earnings as a result of RoSLA, however from the mid-40s onwards, those affected by the reform earn significantly more than those immediately prior to it. The earnings pattern shows that the RoSLA affected both the intercept (negatively) and the slope (positively) of the age earnings profile, such that individuals were initially disadvantaged before eventually the greater returns to experience closed the earnings gap and then opened up a positive premium in earnings later in life.

More generally we make two contributions. Firstly, we show that the now standard approach to assessing the impact of compulsory schooling reform by pooling data and implementing a RDD will mask lifetime heterogeneity and potentially lead to inaccurate policy inference depending on the age distribution of the estimating data. Secondly, we show that the negative effect of the loss of experience as a result of additional education is potentially substantial amongst the complier group for such reforms. Therefore RDD estimates can never control for the experience difference and in this sense are not able to compare 'like with like' in the way that more traditional parametric approaches attempt to do. 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 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

2.1 The returns to education in the UK

The literature on estimating the returns to education has made great strides forwards since the seminal articles from the 1950s, 1960s and 1970s and this is evidenced by numerous high profile survey and review articles that summarize collective research findings to date (such as Card, 1999 and 2001; Harmon *et al*, 2003; Heckman *et al.* 2003; Lemieux, 2006 and Polacheck, 2007, Oreopoulos and Petronijevic, 2013). Although many issues still remain within this literature, there have been a number of significant advances that have taken place in the last few decades.

At the forefront of such innovation is 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. Although various other identification approaches such as twin studies, alternative instruments or estimators exploiting differences in 2nd moments (Klein and Vella, 2009) have also been proposed, to date instrumental variable regressions using school leaving age laws remain the most widely accepted technique for estimating causal returns to education.[5] 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.

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 from Devereux and Hart (2010) and Grenet (2013) appears to contradict such findings and suggests 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 estimation techniques (RDD) have allowed for a more accurate picture to emerge – one that significantly reduces the causal return to education.

An alternative 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 in the lifecycle – this is illustrated in Figure 1. Considering all of these estimates, a potential explanation to reconcile the two strands of the UK returns literature is that returns may vary over the lifecycle and this translates into different causal estimates depending on the age distribution of the sample. Using cross-sectional data from the UK-HLS, Buscha and Dickson (2012) estimate returns to education at a single point late in the lifecycle and suggest that the 1972 RoSLA continues to be associated with a significant positive impact on earnings for the men (and women) affected.[6]

The suggestion that lifecycle bias may play an important role when estimating the returns to education is echoed by Haider and Solon (2006) and Bhuller, *et al.*, (2011) who support the notion that constant returns should not be so readily assumed and that it is vital to incorporate potential lifecycle effects into any assessment of the returns to education. Bhuller *et al.* (2011), for example, show clearly that the return to schooling for Norwegian men varies across the lifecycle. Using administrative data and exploiting school leaving age reforms for identification, 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. Building on such previous work, here we test the hypothesis that returns to education vary across the lifecycle in the UK.

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[5] 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.

[6] Reduced form estimates of 5% for both men and women, implying a LATE return to education of 15-20% for the complier group.

2.2 The 1972 Raising of the School Leaving Age

The raising of the school leaving age (RoSLA) in the UK has occurred twice[7] 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[8] 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, section 35). This finally occurred in 1972 and from 1st September that year all children attending schools in England and Wales were required to stay on until the age of 16.[9]

The 1972 RoSLA event therefore affected all individuals who were born on or after the 1st September 1957. Anybody born after this point 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. As alluded to above, the estimates of the wage returns to education derived using RoSLA as an instrument range from the very low or zero (Dickson and Smith, 2011), through estimates of 6-7% (Grenet, 2013) to the much higher estimates of 15% to 20% (Harmon and Walker, 1995; Chevalier and Walker, 2002).

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[7] We do include the most recent raising of the *participation* age in 2013.

[8] Also known as the "Butler Act" after the President of the Board of Education, Rab Butler, it was passed on 3rd August 1944.

[9] 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

Study	Data	RoSLA	Effect of RoSLA on education	Effect of RoSLA on **outcome**
Earnings				
Harmon and Walker (1995)	Pooled Family Expenditure Survey 1978 to 1986	1947 and 1972	0.541 additional years of schooling for the 1947 change and 0.110 additional years for the 1972 change	IV estimate of 15% **on hourly wages** for men
Chevalier and Walker (2002)	British Household Panel Survey 1991 to 1996	1972	Not reported	IV estimate of 17-20% **in hourly wages** for men
Oreopoulos (2006)	Pooled General Household Survey's 1983 to 1998	1947 and 1972	Combined effect of 0.453 additional years of schooling	Combined reduced form effect on **hourly wages** of 6% and an IV effect of 15% for men.
Devereux and Hart (2010)	Pooled General Household Survey's (GHS) 1979 to 1998 and the Pooled New Earnings Survey Panel (NESPD) 1975 to 2001	1947	0.469/0.397 (GHS/NESPD) additional years of schooling for men 0.550/0.511 (GHS/NESPD) additional years of schooling for women	The 1947 reform had a reduced form effect of 2% for **hourly wages** for men. No effect for women.
Dickson and Smith (2011)	Pooled Quarterly Labour Force Survey 1993 to 2010	1972	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	No effect on log **hourly wages** A positive (reduced form) **employment** effect of 9%
Grenet (2013)	Pooled Quarterly Labour Force Survey 1993 to 2004	1972	0.274 additional years of schooling for men 0.317 additional years of schooling for women	The 1972 reform has a reduced from effect of 2-3% on **hourly wages** for men.
Dickson (2013)	British Household Panel Survey 1991 to 2006	1972	0.564 additional years of schooling for men (men only examined)	An additional year of schooling (IV estimate) increases **hourly wages** by 10% for men.

Study	Data	RoSLA	Effect of RoSLA on education	Effect of RoSLA on **outcome**
Buscha and Dickson (2012)	UK Household Longitudinal Study 2011	1972	0.225 additional years of schooling for men 0.356 additional years of schooling for women	**Hourly wages** increased by 5% for men and 6% for women.
***Other** outcomes*				
Milligan *et al.* (2004)	Pooled British Election Studies 1964, 74, 79, 83, 87, 92 and 97	1947 and 1972	Relative to those aged 14 the 1947 change increased the average age before drop out by 0.512 and the 1972 change by 0.953	There is no effect on the probability of **voting**
Siles (2009)	Pooled General Household Survey's 1980 to 2004	1947 and 1972	0.593 additional years of schooling for the 1947 change and 0.186 additional years for the 1972 change	One more year of education (IV estimate) increase probability of self-reported **good health** by approximately 7%
Machin *et al.* (2011)	Pooled General Household Survey's 1972 to 1996	1972	5.7% drop in the proportion of individuals with no qualification and an increase of 0.221 years of additional schooling	RoSLA significantly impacted **crime** rates. The reduced from effect on the conviction rate is -5%.
Siles (2011)	Pooled General Household Survey's 1978 to 2004	1947 and 1972	0.466 additional years of schooling for the 1947 change and 0.233 additional years for the 1972 change	No effect on **early childbirth** from 1947 reform. 1972 reform reduced the probability of a teen birth by 15%
Wilson (2012)	Pooled Labour Force Survey's 1975 to 2006	1972	0.506 additional years of schooling for women (women only examined)	Reduction in probability of **a teen birth** by 7%.
Clark and Royer (2013)	Pooled Health Survey of England 1991 to 2004	1947 and 1972	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	No significant effect on **health** indicators (objective) or self-reported health, mortality, or health-related behaviours

3. Data

3.1 The LFS and NESPD

Our data come from a number of sources. Firstly, to replicate and examine recent findings from Grenet (2013), we use the British Labour Force Survey. We use Quarterly LFS data, pooled from 1993 quarter one to 2011 quarter four inclusive. The LFS is the largest regular household survey in the UK and is designed to be representative of the population living in private households, with approximately 60,000 households responding each quarter. Following Grenet (2013) we restrict the earnings estimations to include only information from an individual's last interview. 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. We also exclude those who are self-employed and those who first left full-time education after the age of 25. We calculate gross hourly pay excluding overtime and deflate earnings into 2013 £s. Finally, we trim the earnings distribution to remove the top and bottom 1% of the distribution.

Secondly, following Devereux and Hart (2010), we make use of the New Earnings Survey Panel Dataset which is a 1% sample of employees in Great Britain. Based on the last two digits of an individual's National Insurance number the NESPD follows the earnings of approximately 170,000 individuals over the period 1975 to 2011. The administrative nature of the NESPD provides a high quality companion to LFS data as earnings information is obtained via employer payroll records. Employers are legally obliged to provide this information, which results in a very high response rate. Moreover, because individuals are tracked via their NI number, attrition from the survey due to unemployment, withdrawal from labour force or failure of sample location does not result in permanent attrition from the data since such individuals will likely be captured again in later years.

However, not everyone is included in the sample frame of the NESPD. The self-employed, those who switch jobs around the time of the questionnaire date and individuals who earn less than the minimum Pay As You Earn (PAYE) tax threshold are not included in this survey. Self-employed individual are excluded from our analysis in any case and those switching jobs at the time of survey are unlikely to significantly impact our estimates. The failure to include individuals who earn below the PAYE threshold is potentially a more serious issue and one which was investigated in some detail by Devereux and Hart (2010) and Dickens (1999). They both concur that this is not actually a substantial issue and that controlling for this under sampling has a negligible effect on estimates.

Two important differences exist between our study and that of Devereux and Hart (2010). Devereux and Hart pool their data together to obtain one combined estimate of the effect of schooling on earnings. To overcome the fact that some individuals have more responses than other individuals they weight each observation by the inverse of the number of times the person appears in the data. Because the focus of our analysis is specifically on life-cycle effects we estimate year-by-year effects, which do not require this type of re-weighting. Secondly, Devereux and Hart focus on the 1947 RoSLA event which raised the school leaving age from 14 to 15 whilst we analyse the 1972 RoSLA event which raised the school leaving age from 15 to 16.

In operationalizing this data we use the same dependent variable as Devereux and Hart (2010): gross hourly pay excluding overtime deflated to 2013 £s. We cut the top 1% of the earnings distributions but leave the bottom 1% due to the aforementioned PAYE selection.

3.2 Descriptive Statistics

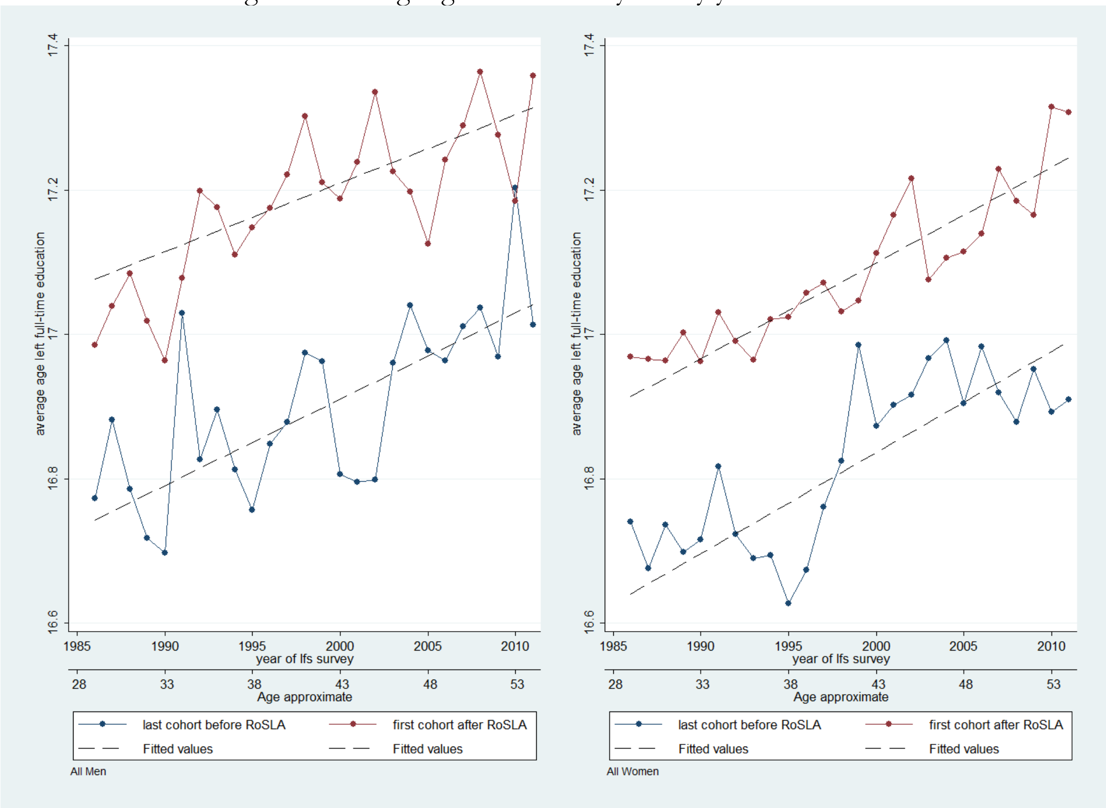
This section presents descriptive statistics on our two data sources. Because we are principally interested in the last cohort before the schooling leaving age was raised and the first cohort after the school leaving age was raised[10] we limit our descriptive overview to this range of the data for both the LFS and NESPD. In addition, we forego presenting detailed wage descriptives for the LFS as our preferred data source, the NESPD, has a larger sample size, spans more survey years, and provides higher quality wage data.[11] Ultimately, we only use the LFS for creating a reference point by replicating Grenet (2012) and briefly highlighting the education discontinuity of RoSLA 1972.

In terms of educational differences caused by the RoSLA 1972 event we are only able to use the LFS since only this data contains information on education. However, as we can see from Table 1, it is by now well-established that the 1972 school leaving age reform significantly increased the amount of schooling received for the treated cohorts: on average by between one quarter and one third year of extra schooling. Unsurprisingly we find a similar result in our LFS data, estimating an average increase of 0.30 years for men and 0.23 years for women as a result of the 1972 RoSLA. However, within the context of our study it is worth exploring whether the additional schooling of the treated cohort remains stable over successive LFS waves. To the best of our knowledge no study has yet examined this issue and one would expect, *a priori*, the educational difference to remain stable over time. Figure 2 presents these results.

[10] It should be noted that due to measurement differences the concepts of cohorts between the two data sources are not identical. The LFS contains month of birth information which means we define the cohorts to run from Sep 1956 to Aug 1957 and from Sep 1957 to Aug 1958. For the NESPD birth information is only provided at an annual level and we thus define the cohorts as all those born in 1956 and all those born in 1958 (leaving out the year 1957 as we cannot be sure whether individuals were affected by RoSLA event).

[11] For example, we find that LFS wage results are very sensitive to the inclusion of proxy responses, which compromise almost 1/3rd of the data. Results for this can be found in the appendix.

Figure 2: Average age left school by survey year in the LFS



Source: LFS 1986-2011

Figure 2 offers a surprising finding. Even though, on average, the educational distance between the two cohorts remains stable over time, there appears to be substantial year-by-year variation in average reported schooling levels. This is concerning because if estimation is carried out on non-pooled (or relatively small pooled) data then the estimated education effect is likely to vary greatly depending on what portion of the data one is analysing. Moreover, it appears that substantial recall bias is creeping into the LFS over time. Although the LFS is not a true long-run panel data source, the relevant cohorts across the successive waves ought to be identical due to the large sampling framework of the LFS. Moreover, because we are specifically using information on the age at which individual's left full-time education, there is no scope for lifelong learning practices to push up the average age left education over time. This, however, is not the case in the observed data: average levels of age-left full time education are rising over time for both cohorts. Fortunately, the suggestion of recall bias does not invalidate our – or the literature's – results since these are driven by the difference in education which appears to stay stable over time. Nevertheless, the existence of strong fluctuations combined with evidence of recall bias makes the usage of schooling in first-stage estimates a potentially hazardous endeavour and this is one of the reasons why we primarily prefer to focus on reduced forms effects.

We now turn to the NESPD data which provides a high quality wage data. Moreover, unlike the LFS, the NESPD data is not limited to earnings information post-1992 which implies we can obtain information about a greater part of the lifecycle and how earnings vary across it. Specifically, we can look at the early part of the age earnings profile that has to date not been examined in any study of RoSLA 1972. Figure 3 presents average log gross hourly pay by cohort and by year of survey:

Figure 3: Average log gross hourly pay by survey year in the NESPD

Several things are apparent in examining Figure 3. First, the NESPD data does not appear to have such a high year-on-year variance as the LFS data which results in a smoother trend for both cohorts.[12] Second, the cohort with more education does not consistently report higher earnings across the lifecycle. Third, the pay gap between cohorts is high during the early parts of the lifecycle (and in favour of the *pre*-reform cohort) and narrows during the middle and later parts. Fourth, the lifecycle profile of both cohorts, although roughly inverted u-shaped, contain dips in earnings during the 1990 recession and the 2008 recession. Fifth, there is a suggestion that the lifecycle profile of both cohorts varies by gender with women experiencing a relative higher pay difference during the mid-20s to mid-30s which could be attributed to delayed child-birth and longer labour market participation.

[12] Not shown in paper. Similar results for LFS are shown in the appendix.

Although these graphs are descriptive in nature they foreshadow some of our results. Importantly, analysis of the NESPD suggests that there is a penalty incurred by an additional year of schooling. The first cohort after the 1972 RoSLA reports consistently lower gross hourly pay until they reach their 30s and the most obvious explanation for this phenomenon is the relative lack of labour market experience of this later cohort. Indeed, because the later cohort is on average one year younger and had to stay in school for one additional year they lack on average *two* years of potential labour market experience compared with their peers from the school year above. This is not without consequence; as previously shown by Angrist (1990), the loss of potential labour market experience for U.S. Vietnam lottery draftees resulted in permanently lower earnings throughout their lives. In other words, the loss of an additional year's potential labour market experience is likely to have a long-run impact on lifetime earnings.

If this is indeed the case then this suggests that much of the recent literature has not compared "like-with-like" since at the point of the 'discontinuity' both cohorts differ not only in terms of schooling but also in terms of potential labour market experience. While this loss of experience – an opportunity cost of the additional education – is part of the net return, the spirit of the early literature exploiting RoSLA was to control for labour market experience, usually by proxying it with age and thereby estimating something that is more akin to a gross return. The intention was to consider the 'pure' effect of the additional education on earnings, holding constant labour market experience. These earlier studies were in effect estimating the schooling coefficient in a classic Mincerian human capital earnings function, with differences in labour market experience controlled for. Since RDD methods have been brought to bear on the question, controlling for labour market experience is no longer part of the estimation: the estimand is different. The comparison of cohorts via RDD gives the *net* effect on wages of the additional education *and* the reduction in potential labour market experience. This is part of the explanation as to why the more recent studies – using RDD – have resulted in lower estimates than the IV literature from the 1990s: the latter recovers a parameter closer to the gross education effect, while the RDD estimates the net effect, not taking into account the discontinuity in experience at the point of RoSLA. Including polynomial controls in age in the RDD framework will not provide a solution because of the discontinuity in the relationship between age and experience at exactly the discontinuity point for schooling.

4. Methodology

4.1 Causal Identification

To examine the above outlined conjecture we first aim to causally identify the effect of education on wages. To do this we follow the 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 1972 RoSLA 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:

$$E_i = \alpha_1 + \beta_1 RoSLA72_i + \boldsymbol{x}_i'\gamma_1 + \varepsilon_{1i} \quad (1)$$

$$Y_i = \alpha_2 + \beta_2 RoSLA72_i + \boldsymbol{x}_i'\gamma_2 + \varepsilon_{2i} \quad (2)$$

where (1) and (2) are the reduced form equations for education E_i and log hourly wages Y_i, β_1 is the estimate of the RoSLA 1972 dummy on education (measured in years of schooling) whilst β_2 is the estimated effect of the RoSLA 1972 event on log hourly wages. If chosen to be included, $\boldsymbol{x}_i'$ can be vector of additional control variables (such as age, gender and regional dummies) with parameter estimates γ. Finally, ε_{1i} and ε_{2i} are two normally distributed error terms with mean zero. The structural form for labour market outcomes y_i is then given by:

$$y_i = \alpha_3 + \beta_3 E_i + \boldsymbol{x}_i'\gamma_3 + \varepsilon_{3i} \quad (3)$$

where the IV estimate of β_3 in (3) is given by the ratio of the reduced form coefficients in (1) and (2), $\beta_3 = \beta_2/\beta_1$.

Although the NESPD does not contain any measures of education, it is possible to obtain some estimated measure of $\hat{\beta}_3$ by estimating the reduced form equation (2) – in order to obtain $\hat{\beta}_2$ – and then using an out-of-sample estimation for β_1 (Devereux and Hart, 2010). However, because so many studies have consistently estimated $\hat{\beta}_1$ we argue that not even out-of-sample estimates for β_1 are necessary. As highlighted in Table 1, the UK literature has generally agreed that the estimated effect of 1972 RoSLA on education was to raise average years of schooling by approximately 0.3 years. Using our LFS data, we estimate similar values: $\hat{\beta}_1 = 0.3$ for men, $\hat{\beta}_1 = 0.23$ for women. It is for this reason that we focus much of our attention on estimating a precise value of β_2 in the reduced form equation (2).

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. To some extent it is possible to control for such differences by including a series of age or cohort controls in the vector $\boldsymbol{x}_i'$ and traditionally, β_1 and β_2 are estimated like this using a 2SLS framework (see *inter alia* Harmon and Walker, 1999). However, a disadvantage of 2SLS is in its linear and parametric restrictions. Given the large sample qualities of our data (particularly NESPD), we are able to implement a regression discontinuity design in order to estimate the β_1 and β_2. Following the approach outlined in Imbens and Lemieux (2008), and implemented on LFS data by Grenet (2013) and on NESPD data by Devereux and Hart (2010), we use non-parametric techniques where the effect of RoSLA 1972 on education and earnings is estimated by local linear regression in a local region near the discontinuity.

To estimate the values β_2 we fit a linear regression function to observations within distance *h* on either side of the discontinuity point (1st September 1957)

$$\min_{\alpha_{pre-1957}:\beta_{pre-1957}} \sum_{i:c-h<X_i<c} \left(Y_i - \alpha_{pre-1957} - \beta_{pre-1957}.(X_i - c)\right)^2 \quad (4)$$

and

$$\min_{\alpha_{post-1957}:\beta_{post-1957}} \sum_{i:c-h<X_i<c} \left(Y_i - \alpha_{post-1957} - \beta_{post-1957}.(X_i - c)\right)^2 \quad (5)$$

Where Y_i is log hourly wages, X_i is the number of months an individual is born before/after the 1st September 1957 discontinuity, the αs and βs 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 be computed by:

$$\hat{\mu}_{pre-1957}(c) = \hat{\alpha}_{pre-1957} + \hat{\beta}_{pre-1957}.(c - c) = \hat{\alpha}_{pre-1957} \quad (6)$$

and

$$\hat{\mu}_{post-1957}(c) = \hat{\alpha}_{post-1957} + \hat{\beta}_{post-1957}.(c - c) = \hat{\alpha}_{post-1957} \quad (7)$$

Given these estimates we can then compute the treatment effect of RoSLA 1972 log hourly wages Y_i as follows:

$$\hat{\beta}_2 = \hat{\alpha}_{pre-1957} - \hat{\alpha}_{post-1957} \quad (8)$$

Imbens and Lemieux (2008) suggest that from a practical point of view a simple rectangular kernel should be used to estimate β_1 and that robustness be verified by using different choice of bandwidths. We therefore report results for varying bandwidths but limit these to multiples of 12 months for LFS data $h = [12, 24, 36, 48, 60, 72]$ to avoid possible contamination by within-year month of birth effects. For NESPD 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 $h = [1,2,3,4,5,6]$. In addition, because local linear regression techniques can be problematic when bandwidth sizes are low, we complement our estimation of β_2 with mean-differences for a smaller set of bandwidths which are more local to the discontinuity. Finally, we produce the parameters estimates of β_2 for each year of data.[13] This allows us assess to what extent the RoSLA 1972 education reform influenced labour market outcomes across the life-cycle.

4.2 Correcting for the experience loss

However, as mentioned in section 3, there is a suggestion that the effect of the RoSLA 1972 could result in negative outcomes, potentially suggesting that additional schooling is associated with negative returns to education for both men and women in the early part of the lifecycle. Clearly, if we assume that additional schooling does not lead to net negative human capital accumulation, this cannot be true and other confounding factors must play a role since the singular impact of additional schooling on wages should at worst be zero.

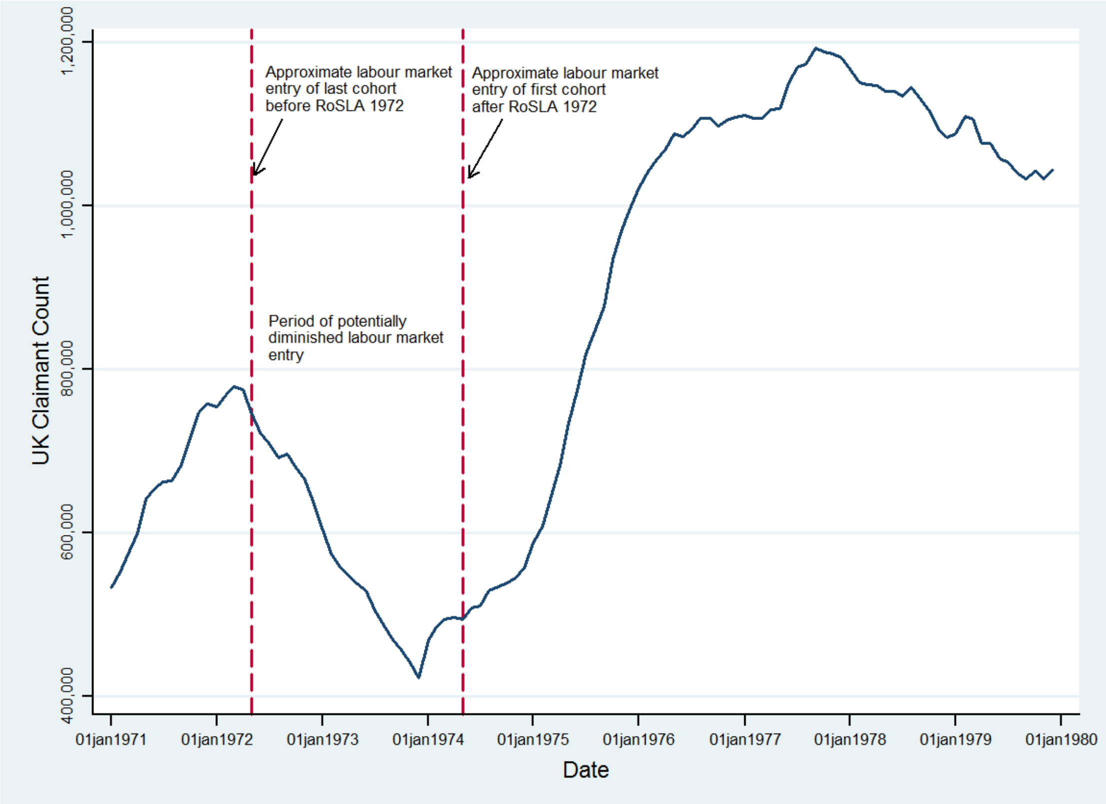
Although the literature generally argues that at the point of discontinuity the treated and non-treated cohorts are alike in all characteristics except for the additional schooling, this is not quite the case. At best RDD and other discontinuity estimates must rely on cohort averages around the discontinuity, since all RoSLA events are contaminated by within-school year effects if one moves 'too close' to the discontinuity. On top of such intra-year education differences, there are therefore two further possible differences between the pre- and post-RoSLA cohorts: macro-economic circumstances which affect cohorts differentially and the loss in potential experience for the post-RoSLA cohort.

The relevant macro-economic circumstances relate to the 1973-1975 recession caused by the oil crisis and it conceivable that youths entering the labour market post 1972 RoSLA were faced with a tougher labour market when compared to the previous cohort. This may have caused lower average earnings in the post-RoSLA cohort as youth unemployment rose and wages fell. However, this argument is predicated on the assumption that wages for the pre-RoSLA cohort would have been unaffected, possibly due to their already entrenched position in the labour market. Whilst we are unable to test such a hypothesis, unemployment and claimant count statistics for the period suggest that the proportion of individuals seeking work dropped substantially in 1973 and 1974 when compared to 1971 or 1972 (Denman and McDonald, 1995). The economic recession did not 'bite' into unemployment statistics until 1975 onwards and it

[13] To smooth our results for the LFS and NESPD analysis we use rolling 3-year samples from the data, 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. The NESPD sample size allows single year analysis which confirms the rolling 3-year results, see appendix A.

thus appears that school leavers who were part of the first post 1972 RoSLA cohort would have faced a period of relatively good labour market conditions. A pictorial representation of this is shown below in Figure 4.

Figure 4: UK Claimant Count in the 1970s (seasonally adjusted)



Source: Office of National Statistics

The loss of potential labour market experience is likely to be a more significant factor in explaining the previously found negative (descriptive) results. As can be clearly seen in Figure 4, the introduction of RoSLA 1972 led to a significant period of potentially diminished labour market entry. Staying-on rates increased dramatically during this period and previous studies have shown that the proportion of youths leaving the education system at age 16 or more increased from ??% to over 95% (**cite**) from one cohort to the next. This suggests that the experience loss incurred by an extra year of schooling, in addition to the potential experience difference caused by the age difference between the older pre-1972 RoSLA cohort and the younger post-1972 RoSLA cohort, could be major factor in explaining the negative descriptive findings.

In other words, sophisticated regression discontinuity designs do not, and cannot, compare 'like with like' when the identification comes from a cohort based approach within a returns to schooling context. This in turn may explain the divergence in recent findings where RDD estimates (Devereux and Hart, 2010 and Grenet, 2013) return low values whilst more traditional 2SLS estimates, which attempt to control for the experience differences via age polynomials, return high values.

It is notable that the early literature on returns to education was explicitly implementing Mincer's (1974) human capital earnings function, as derived from Becker's (1964) model of investment in human capital. One of the explicit assumptions of the model is that time in school is independent of time in the work-place – that is, that schooling does not have a deleterious impact on experience. Therefore the only opportunity costs of additional schooling are the foregone wage for that year plus any tuition costs. The experience opportunity cost is not allowed for – in fact it is explicitly assumed not to exist. As such, the model aims to control for any experience differences by including a polynomial (traditionally quadratic) in experience in the estimated earnings function. Thus the estimated return in the model is intended to be the 'gross' return to education, having controlled out differences in experience. This type of argumentation is carefully highlighted in Rosenzweig and Wolpin (2000:p832-853) who demonstrate the importance of omitted experience in identifying Mincer functions when using natural experiments, including school leaving age reforms.

However, and crucially, modern RDD methodology – in assuming that the only discontinuity at the relevant threshold between cohorts is in education – does not allow differences in experience to be controlled and thus estimates the 'net' return to education, including the (likely negative) impact of experience loss.

In a cross-sectional setting there is relatively little that can be done to correct for any experience difference at the discontinuity since identification of the educational discontinuity comes from age, which in turn is highly correlated with experience. However, within a panel data setting it is possible to attempt a correction. We do this by replacing current values of earnings with future values of earnings for the post-1972 RoSLA cohort only. Assuming that earnings growth after schooling is only a function of experience and not schooling levels (i.e. there is additive separability in the Mincer framework) then replacing current earnings with future earnings $\left(y_{it_{post-1957}} = y_{it+n_{post-1957}}\right)$ should correct any experience loss present at the identification discontinuity. At minimum the cohort age difference implies that a value of $n = 1$ should be used whilst at maximum an additional year of schooling (in addition to the age difference) implies a value of $n = 2$. In reality, however, the experience loss from RoSLA was not 1 full year but 0.3 years and therefore a value of $n = 1.3$ is likely to be the most appropriate.

5. Results

5.1 Pooled LFS and Pooled NESP

Firstly we present estimates of the impact of the 1972 RoSLA on education and wages by pooling the LFS survey years and NESPD survey years. In part this is to ensure that we can reconcile our estimates with those of Grenet (2013) but also to set a reference point for when we explore lifecycle effects. Table 2 presents our RDD estimates of the reduced form impact of 1972 RoSLA on years of education and log hourly wages for various relevant bandwidths.

As can be seen, we find a consistent picture of results from the RDD estimates on pooled data. For the LFS, the narrower bandwidths could be in danger of distorting the estimate by including age within school-year effects and are therefore likely to be somewhat upward biased.[14] Looking initially at the results for men[15], the bandwidths of 48, 60 and 72 for the LFS and corresponding bandwidths 4, 5, and 6 for the NESPD show a significant reduced form impact on wages of around 2%. The impact on years of education in the LFS is also statistically significant and about 0.3 years[16] – suggesting that just under one-third of the men in the immediate post-RoSLA cohorts were bound by the reform. These estimates, using a slightly longer dataset than that used by Grenet (2013), confirm the more recent findings in the UK literature: that the reduced form impact of 1972 RoSLA – 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%. This is a far cry from earlier estimates of 15-20% and the replication of such results using two different data sources adds substantial weight to this school of new evidence. However, when we extend the available data range beyond what is measurable in the LFS and include the years 1975 to 1992 in our analysis of the NESPD, we immediately see a dramatic change in coefficients. The estimated reduced form impact on wages from RoSLA 1972 is now approximately -3% for men (-2% for women)[17]. Extrapolating such results suggests an implied local average treatment effect of approximately -9% for an additional year of education, an estimate that has never been seen before in any related literature.

Two important findings can be taken from Table 2. The first is that any significant difference in the returns to education between the two data ranges suggests the presence of strong lifecycle effects that has previously been masked in prior empirical studies (Grenet, 2013; Devereux and Hart, 2010 and Oreopolous, 2006 amongst others). Second, the negative estimate is clearly driven by other factors as we refuse to believe that the gross return to education can ultimately be less than zero. Nonetheless, the empirical evidence suggest that, ultimately, the next impact of increasing the school leaving from 15 to 16 in 1972 was to the *detriment* of those affected by it.

[14]There is a danger that lower bandwidths give too much weight to a comparison of August born and September born children who are not directly comparable because of within-year birth effects.
[15] Pooled results for women are available from the authors on request. To match the majority of the literature, we concentrate on impacts for men.
[16] Not shown, see appendix
[17] Not shown, see appendix

Table 2: RDD Estimates of the effect of the 1972 RoSLA on log hourly wages and years of schooling, various bandwidths: pooled LFS data and pooled NESPD data; men only

		LFS 1993-2011					NESPD 1993-2011			NESPD 1975-2011	
Bandwidth (mths)		Log hourly wage	Years of schooling	N	Bandwidth (yrs)		Log hourly wage	N		Log hourly wage	N
12	*Coeff.*	0.029	0.435***	10,536	1	*Coeff.*	0.015***	64,945	*Coeff.*	-0.035***	128,790
	Std. Err.	(0.022)	(0.109)			*Std. Err.*	(0.004)		*Std. Err.*	(0.003)	
24	*Coeff.*	0.025*	0.362***	21,049	2	*Coeff.*	0.028***	131,220	*Coeff.*	-0.039***	259,979
	Std. Err.	(0.015)	(0.075)			*Std. Err.*	(0.007)		*Std. Err.*	(0.009)	
36	*Coeff.*	0.025**	0.360***	31,436	3	*Coeff.*	0.015	198,686	*Coeff.*	-0.020**	390,793
	Std. Err.	(0.013)	(0.062)			*Std. Err.*	(0.008)		*Std. Err.*	(0.010)	
48	*Coeff.*	0.023**	0.335***	42,076	4	*Coeff.*	0.014	268,444	*Coeff.*	-0.027***	523,386
	Std. Err.	(0.011)	(0.053)			*Std. Err.*	(0.005)		*Std. Err.*	(0.007)	
60	*Coeff.*	0.021**	0.313***	53,127	5	*Coeff.*	0.028***	336,944	*Coeff.*	-0.024***	652,124
	Std. Err.	(0.010)	(0.047)			*Std. Err.*	(0.005)		*Std. Err.*	(0.006)	
72	*Coeff.*	0.018**	0.294***	63,883	6	*Coeff.*	0.024***	406,765	*Coeff.*	-0.032***	781,308
	Std. Err.	(0.009)	(0.043)			*Std. Err.*	(0.004)		*Std. Err.*	(0.007)	

* p<0.10, ** p<0.05, *** p<0.01. Estimates based on local linear regression using rectangular kernel. Bandwidth 1 for NESPD data cannot be estimated using local linear regression since only one data point exists. We complemented this by using mean differences for bandwidth 1. LFS: 1993q1-2011q4, NESPD: 1993-2011.

5.2 Lifecycle Results

We now turn to exploring the divergence in our pooled results by estimating reduced form effect of RoSLA 1972 at each point of the lifecycle – using the NESPD from 1975 to 2011 corresponding to the approximate ages of 18 to 54 for the relevant cohorts born just before and just after September 1957.

Table 3 presents estimates of the impact of RoSLA using mean differences based on 1 year bandwidths for the NESPD data. The NESPD is large enough to support bandwidth analysis based on 1 year implying that these results are not contaminated by further outlying cohorts. To reduce the year-by-year noise levels we decide to smooth our estimates by estimating the effect of 1972 RoSLA on rolling 3-year data bands (see footnote 11), though estimates are robust to using single-year data bands (see appendix A).

Table 3: Estimates of the Reduced Form impact of RoSLA 1972 on log hourly wages at different points in the life-cycle, NESPD

	NESPD						NESPD experience adjusted (*n* = 2)					
	Men			Women			Men			Women		
Year	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N	Coeff	S.E.	N
1975	-0.261***	(0.007)	9740	-0.159***	(0.007)	8982	0.096***	(0.006)	9245	0.073***	(0.006)	8505
1976	-0.197***	(0.006)	10244	-0.131***	(0.006)	9327	0.075***	(0.006)	9746	0.050***	(0.006)	8889
1977	-0.136***	(0.006)	10579	-0.116***	(0.006)	9234	0.081***	(0.006)	10114	0.064***	(0.006)	8792
1978	-0.107***	(0.006)	10843	-0.093***	(0.007)	8989	0.073***	(0.006)	10430	0.083***	(0.006)	8527
1979	-0.092***	(0.006)	11150	-0.074***	(0.007)	8759	0.083***	(0.006)	10827	0.098***	(0.007)	8296
1980	-0.085***	(0.006)	11423	-0.058***	(0.007)	8496	0.069***	(0.006)	11099	0.089***	(0.008)	8045
1981	-0.073***	(0.006)	11440	-0.046***	(0.008)	8073	0.073***	(0.007)	11102	0.080***	(0.008)	7685
1982	-0.066***	(0.006)	11360	-0.026***	(0.008)	7613	0.073***	(0.007)	10984	0.085***	(0.009)	7243
1983	-0.056***	(0.007)	11035	-0.014	(0.009)	7179	0.079***	(0.007)	10755	0.094***	(0.010)	6800
1984	-0.044***	(0.007)	11004	0.010	(0.010)	6901	0.084***	(0.008)	10722	0.118***	(0.010)	6525
1985	-0.037***	(0.007)	11110	0.033***	(0.010)	6819	0.075***	(0.008)	10746	0.134***	(0.011)	6445
1986	-0.032***	(0.008)	11390	0.050***	(0.010)	7024	0.064***	(0.008)	10935	0.138***	(0.011)	6673
1987	-0.031***	(0.008)	11429	0.053***	(0.011)	7248	0.060***	(0.009)	10971	0.138***	(0.011)	6969
1988	-0.031***	(0.009)	11413	0.050***	(0.011)	7575	0.071***	(0.009)	11043	0.150***	(0.011)	7314
1989	-0.020***	(0.009)	11217	0.049***	(0.011)	7874	0.083***	(0.009)	10930	0.158***	(0.011)	7672
1990	-0.008	(0.009)	11043	0.043***	(0.011)	8170	0.075***	(0.009)	10809	0.136***	(0.011)	8008
1991	0.006	(0.009)	10730	0.045***	(0.011)	8323	0.056***	(0.010)	10649	0.097***	(0.011)	8296
1992	0.004	(0.010)	10585	0.038***	(0.011)	8401	0.045***	(0.010)	10645	0.073***	(0.011)	8471
1993	0.008	(0.010)	10714	0.034***	(0.011)	8832	0.057***	(0.010)	10754	0.079***	(0.011)	8842
1994	0.002	(0.010)	10926	0.020*	(0.011)	9295	0.069***	(0.010)	10775	0.094***	(0.011)	9190
1995	0.006	(0.010)	10866	0.011	(0.011)	9634	0.079***	(0.010)	10556	0.102***	(0.011)	9411
1996	-0.003	(0.010)	10780	0.007	(0.011)	9681	0.069***	(0.011)	10453	0.102***	(0.011)	9476
1997	-0.006	(0.010)	10695	-0.001	(0.010)	9741	0.068***	(0.011)	10294	0.099***	(0.011)	9465
1998	-0.008	(0.010)	10804	-0.008	(0.010)	9907	0.067***	(0.011)	10208	0.091***	(0.010)	9514
1999	-0.005	(0.011)	10573	-0.001	(0.010)	9997	0.061***	(0.011)	9517	0.086***	(0.011)	9206
2000	-0.001	(0.011)	10380	0.006	(0.010)	10100	0.045***	(0.012)	8928	0.074***	(0.011)	8942
2001	0.007	(0.011)	10402	0.007	(0.010)	10288	0.045***	(0.012)	9108	0.064***	(0.011)	9277
2002	0.010	(0.011)	10490	-0.002	(0.010)	10545	0.048***	(0.011)	9635	0.057***	(0.010)	9864
2003	0.018*	(0.011)	10684	-0.003	(0.010)	10973	0.054***	(0.011)	10135	0.045***	(0.010)	10543
2004	0.023**	(0.011)	10567	-0.004	(0.009)	11205	0.055***	(0.011)	9858	0.040***	(0.010)	10576
2005	0.024**	(0.011)	9923	-0.009	(0.010)	10621	0.060***	(0.012)	9112	0.036***	(0.010)	9930
2006	0.025**	(0.012)	9069	-0.006	(0.010)	9782	0.053***	(0.012)	8240	0.036***	(0.010)	9065
2007	0.036***	(0.012)	8910	-0.004	(0.010)	9751	0.034***	(0.013)	7806	0.013	(0.011)	8711
2008	0.046***	(0.012)	9247	-0.003	(0.010)	10401	0.029**	(0.014)	6740	-0.004	(0.012)	7638
2009	0.048***	(0.011)	9722	0.004	(0.009)	11200	0.060***	(0.018)	5833	0.004	(0.015)	6700
2010	0.042***	(0.014)	6410	0.010	(0.011)	7466	-	-	-	-	-	-
2011	0.046**	(0.020)	3223	0.022	(0.016)	3791	-	-	-	-	-	-

Standard errors in parenthesis. Results based on rolling three year data bands except for last two years. LFS estimates based on 60mths bandwidth local linear regression with rectangular kernel. NESPD Estimates based on 1-year bandwidth mean differences.

Results in the left-hand panel of Table 3 confirm our previous descriptive findings: specifically that the NESPD suggests the 1972 RoSLA was associated with substantially lower average earnings for those who left-school at age. However, gender differences are also apparent. The negative effect of additional schooling lasted until 1990 for men (when they were in their early 30s) whilst the negative effect for women was much shorter and lasted only until 1982 (when they were aged mid-20s). Moreover, for men there is long catch-up process with individuals who received additional schooling only receiving statistically higher earnings after 2003 – with the return reaching a size of 4-5%. For women the positive return from additional schooling is apparent much earlier and starts in 1985 and also reaches a size of 5%. However, the positive earnings effect experienced by women is temporary and disappears once women reach their late 30s, after which the return to additional schooling becomes statistically insignificant. This phenomenon may be related to fertility timing decisions but it has to be acknowledged that estimating education returns for women is complicated by the non-random selection into the labour market on factors including education. Nonetheless, it is somewhat surprising that the NESPD suggests the effect of additional human capital is only transitory for women and completely disappears after reaching the middle of the lifecycle. Finally, it is interesting to observe that the pooled estimated return for men (in Table 2) apparently masks significant heterogeneity in the return to education – suggesting that our initial assumption of heterogeneity in the lifecycle is correct.

Adjusting for the experience penalty

Results from the right hand panel of Table 3 show that, in stark contrast to results from Table 3, the reduced form impact of 1972 RoSLA was strongly positive at all points over the lifecycle. The highly negative effects in the early part of the lifecycle are replaced by positive coefficients of circa 7-10%. Moreover, there appears much less evidence of the strong lifecycle effects exhibited in Table 3 in the returns to education for both men and women. Men receive an hourly earnings premium of approximately 7-9% in the early part of their lifecycle whilst for the later part this is approximately 4-7%. Women see an average return of 7-9% in the early part of their lifecycle which then rises to 13-15% during the middle stage before falling back to 0-3% in the later part of their careers. It should be noted that we believe that the reported values in Table 4 are upper bounds since reduced form estimates of 10% would suggest LATE returns of approximately 40% and 30% for men and women respectively – estimates which seem excessively high in comparison to the literature. Nonetheless, there is a strong suggestion from Table 4 that the true returns to education are more homogenous than Table 3 would suggest, although some evidence of heterogeneity remains, especially for women.

6. Discussion and conclusions

In this paper we have revisited some of the evidence on the returns to education and attempted to provide an explanation for the varying estimates reported in the UK literature. Of particular interest was whether (causal) returns vary over the lifecycle and preliminary results suggested that this is indeed the case. Using 19 years of LFS data and 36 years of NESPD data we exploit the exogenous variation induced by the 1972 school leaving age reform and 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 replicate recent findings of Grenet (2013) and Devereux and Hart (2010) which have both suggested that the returns to education are considerably lower than previously thought. However, on further inspection there is strong evidence that these 'averaged' returns are not homogenous over the lifecycle since both data sources suggest that the returns to education vary depending on what point in the lifecycle they are measured.

However, we also found that there is some discrepancy between the two data sources, as both appear to provide contradictory estimates on how the return to education varies over the lifecycle. In part this due to LFS estimates being subject to larger standard errors and increased measurement error. Additionally, there is simply less data available in the LFS and therefore no possibility to analyse early parts of the lifecycle. We thus favour estimates from the NESPD which suggested that there is strong heterogeneity in the returns to education over the lifecycle. We found significant negative effects in the early part of work-life and positive effects in the later part (although the timing of this differed by gender). However, in rationalising such results we argued that even the most sophisticated RDD designs, where the evaluation of 1972 RoSLA takes places right at the boundary of the discontinuity, cannot overcome the experience bias induced by a cohort identification approach.

Attempting to correct for this experience bias induced by RDD methods, we conclude that the variation in the returns to education over the lifecycle is less extreme than initially estimated. The returns for men are fairly homogenous over time with men experiencing only a small decrease in the returns to education as they age. The returns for women are more concave with medium returns early in life, large returns during the mid-course and low returns later in life. This is very similar to recent evidence on lifecycle returns for Norway provided by Bhuller *et al.* (2011) who also find evidence of concavity. In order to better contextualise such results we computed the estimated effect of the 1972 RoSLA on annual earnings in 2013 prices. These data are taken from gross weekly wages which are multiplied by 52 and then adjusted by the estimated coefficients in Table 3 and 4. Although these figures represent 'back-of-the-envelope' calculations they nonetheless provide a useful contextual narrative in which to view the effect of an additional year of schooling under the various RDD identification regimes.

Table 5. Estimated effect of 1972 RoSLA on average yearly earnings in 2013 prices.

Year	Men			Women		
	Average yearly earnings, 1956 cohort	Estimated average yearly earnings, 1958 cohort	Estimated average yearly earnings 1958 cohort, experience adjusted	Average yearly earnings, 1956 cohort	Estimated average yearly earnings, 1958 cohort	Estimated average yearly earnings 1958 cohort, experience adjusted
1975	£12,192	£9,010	£13,048	£9,200	£7,737	£9,935
1976	£13,883	£11,148	£14,710	£10,432	£9,066	£11,104
1977	£14,646	£12,654	£15,666	£10,914	£9,648	£11,692
1978	£15,998	£14,287	£17,036	£11,554	£10,480	£12,316
1979	£15,995	£14,524	£17,189	£11,330	£10,491	£12,192
1980	£17,824	£16,309	£18,939	£12,469	£11,746	£13,273
1981	£18,111	£16,789	£19,321	£13,288	£12,677	£14,202
1982	£19,703	£18,403	£21,032	£13,550	£13,198	£14,503
1983	£20,768	£19,605	£22,304	£13,982	£13,787	£15,062
1984	£21,793	£20,834	£23,522	£14,160	£14,302	£15,347
1985	£22,809	£21,965	£24,437	£14,026	£14,489	£15,100
1986	£24,136	£23,364	£25,612	£14,019	£14,720	£14,949
1987	£25,512	£24,721	£26,985	£14,180	£14,931	£15,069
1988	£27,286	£26,440	£29,160	£14,142	£14,849	£15,195
1989	£28,182	£27,619	£30,452	£14,455	£15,163	£15,700
1990	£28,488	£28,260	£30,602	£14,158	£14,767	£15,263
1991	£29,553	£29,730	£31,204	£14,806	£15,473	£15,665
1992	£31,031	£31,155	£32,407	£15,622	£16,216	£16,338
1993	£31,841	£32,096	£33,655	£15,919	£16,460	£16,849
1994	£31,884	£31,948	£34,084	£16,046	£16,367	£17,174
1995	£32,139	£32,332	£34,667	£16,342	£16,522	£17,634
1996	£33,477	£33,377	£35,770	£16,741	£16,858	£17,899
1997	£33,501	£33,300	£35,747	£17,317	£17,300	£18,484
1998	£34,845	£34,566	£37,145	£17,451	£17,311	£18,603
1999	£35,291	£35,114	£37,431	£18,670	£18,651	£19,807
2000	£36,417	£36,380	£38,025	£19,576	£19,694	£20,447
2001	£36,798	£37,055	£38,450	£19,136	£19,270	£19,995
2002	£37,704	£38,081	£39,520	£20,295	£20,255	£21,261
2003	£37,482	£38,156	£39,505	£20,762	£20,700	£21,860
2004	£37,117	£37,971	£39,188	£20,727	£20,644	£21,853
2005	£38,248	£39,165	£40,593	£21,808	£21,612	£23,103
2006	£37,056	£37,983	£39,039	£21,840	£21,709	£22,973
2007	£37,212	£38,552	£38,486	£21,383	£21,298	£22,087
2008	£38,465	£40,234	£39,621	£22,224	£22,158	£22,861
2009	£37,532	£39,334	£39,864	£22,341	£22,431	£23,671
Total earnings	£994,919	£982,461	£1,054,417	£564,867	£562,977	£599,466
Effect of RoSLA 1972		-£12,459	£59,498		-£1,891	£34,598
Effect of 1 year of extra schooling		-£41,529	£198,327		-£8,221	£150,426
Total earnings (1 extra year of schooling)	£994,919	£953,391	£1,193,246	£564,867	£556,646	£715,294

Table 5 shows that the total lifetime earnings for the pre-1972 RoSLA cohort was approximately £1m for men and £0.5m for women. According to our estimates the reduced form effect of the 1972 RoSLA is thus bounded between £-12,000 and £60,000 for men and £-2,000 and £35,000 for women. Dividing these values by 0.30 years of schooling for men and 0.23

years of schooling for women suggests a LATE effect ranging between £-42,000 and £200,000 for men and £-8,000 and £150,000 for women. As mentioned before, clearly these values present minimum lower and maximum upper bounds in which to see the impact of additional schooling. Taking a mid-range estimate of our results, then suggests that the average return to 1972 RoSLA was approximately £24,000 for men and £15,500 for women whilst the LATE effect was £79,000 for men and £71,000 for women.

Our results are important for a variety of reason. First, we argue that recent estimates of the return to education in the UK underestimate the true return to an additional year of schooling due to their failure to compensate for any experience differences between the cohorts. This 'bias' will affect any estimates of returns to education that exploit compulsory school leaving age reforms and estimate returns using RDD. That experience matters in the long-run and over the lifecycle has already been shown by Angrist (1990) and controlling for it in a cohort identification framework remains a high priority. It is arguable that this experience loss is part of the return – part of the opportunity cost of the additional year of schooling – and as such should be netted into the estimates of the return to education. While this is true, it remains important to decompose the return into the 'pure' (positive) education effect and the necessary (negative) experience loss effect. Each of these components may change over time and it is important for wider policy to know the return to the skills associated with education, abstracted from the experience costs of gaining more education. Moreover, should a reform be considered that increases education without reducing experience – for example by lowering the school start age – it is important to be able to estimate the likely return to this sort of pure education change. Second, our results show that there is evidence of heterogeneous returns to education over the lifecycle, especially for women. Returns to education studies are thus advised to consider the possible impact of their data range on their final estimates; and if not empirically at least contextually. Third, the evidence presented here provides an important linkage between the recent 'revisionist' returns literature and the older returns literature in the UK. Although we do not claim to have found the 'true causal' effect of additional education, we do believe that we have at least bounded the empirical literature in the UK to date and argue that lifecycle effects are likely to be a fruitful avenue for future research, especially within context of the latest raising of the participation age from 16 to 17 and 17 to 18 in 2013 and 2015 respectively.

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Appendix A – NESPD robustness checks

Table A1: Regression Discontinuity Design Estimates of the Reduced Form impact of the 1972 RoSLA on log hourly wages at different points in the life-cycle; NESPD, single year data-bands.

	NESPD					
	Men			Women		
Year	Coeff	S.E.	N	Coeff	S.E.	N
1975	-0.356***	(0.010)	2966	-0.228***	(0.010)	2724
1976	-0.268***	(0.009)	3295	-0.173***	(0.010)	3048
1977	-0.185***	(0.008)	3479	-0.125***	(0.008)	3210
1978	-0.130***	(0.009)	3470	-0.112***	(0.009)	3069
1979	-0.093***	(0.009)	3630	-0.107***	(0.010)	2955
1980	-0.102***	(0.010)	3743	-0.071***	(0.010)	2965
1981	-0.079***	(0.010)	3777	-0.074***	(0.012)	2839
1982	-0.081***	(0.010)	3903	-0.036***	(0.013)	2692
1983	-0.065***	(0.011)	3760	-0.030**	(0.014)	2542
1984	-0.047***	(0.011)	3697	-0.008	(0.016)	2379
1985	-0.053***	(0.012)	3578	0.008	(0.016)	2258
1986	-0.033***	(0.012)	3729	0.037**	(0.018)	2264
1987	-0.021*	(0.013)	3803	0.060***	(0.018)	2297
1988	-0.038***	(0.014)	3858	0.057***	(0.018)	2463
1989	-0.034**	(0.015)	3768	0.045**	(0.019)	2488
1990	-0.019	(0.015)	3787	0.047**	(0.019)	2624
1991	-0.008	(0.016)	3662	0.056***	(0.019)	2762
1992	0.000	(0.016)	3594	0.031	(0.020)	2784
1993	0.024	(0.017)	3474	0.046**	(0.020)	2777
1994	-0.014	(0.017)	3517	0.037*	(0.019)	2840
1995	0.013	(0.017)	3723	0.019	(0.020)	3215
1996	0.005	(0.018)	3686	0.006	(0.019)	3240
1997	-0.002	(0.018)	3457	0.007	(0.019)	3179
1998	-0.012	(0.018)	3637	0.004	(0.018)	3262
1999	-0.005	(0.018)	3601	-0.015	(0.018)	3300
2000	-0.008	(0.018)	3566	-0.013	(0.018)	3345
2001	-0.002	(0.019)	3406	0.027	(0.018)	3352
2002	0.010	(0.019)	3408	0.000	(0.017)	3403
2003	0.013	(0.018)	3588	-0.011	(0.017)	3533
2004	0.007	(0.018)	3494	0.007	(0.016)	3609
2005	0.031*	(0.018)	3602	-0.004	(0.016)	3831
2006	0.029	(0.019)	3471	-0.011	(0.016)	3765
2007	0.009	(0.021)	2850	-0.009	(0.018)	3025
2008	0.036*	(0.022)	2748	0.004	(0.018)	2992
2009	0.059***	(0.019)	3312	-0.007	(0.016)	3734
2010	0.039**	(0.020)	3187	-0.003	(0.016)	3675
2011	0.045***	(0.020)	3223	0.021	(0.016)	3791

Standard errors in parenthesis. Results based on single year data bands. NESPD estimates based on 1 year bandwidth mean differences.

Appendix B – LFS robustness checks

Figure B1: Average age left school by survey year in LFS (non-proxy respondents only)

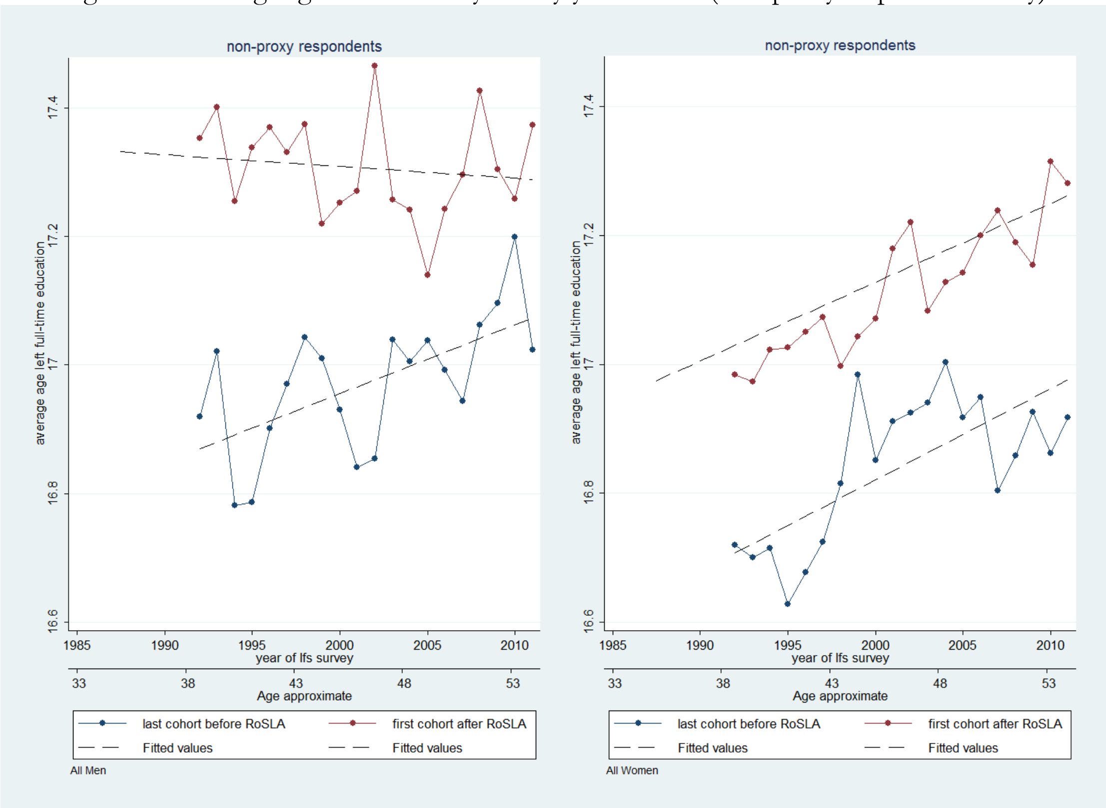

Figure B2: Average log gross hourly pay by survey year in LFS (non-proxy respondents only)

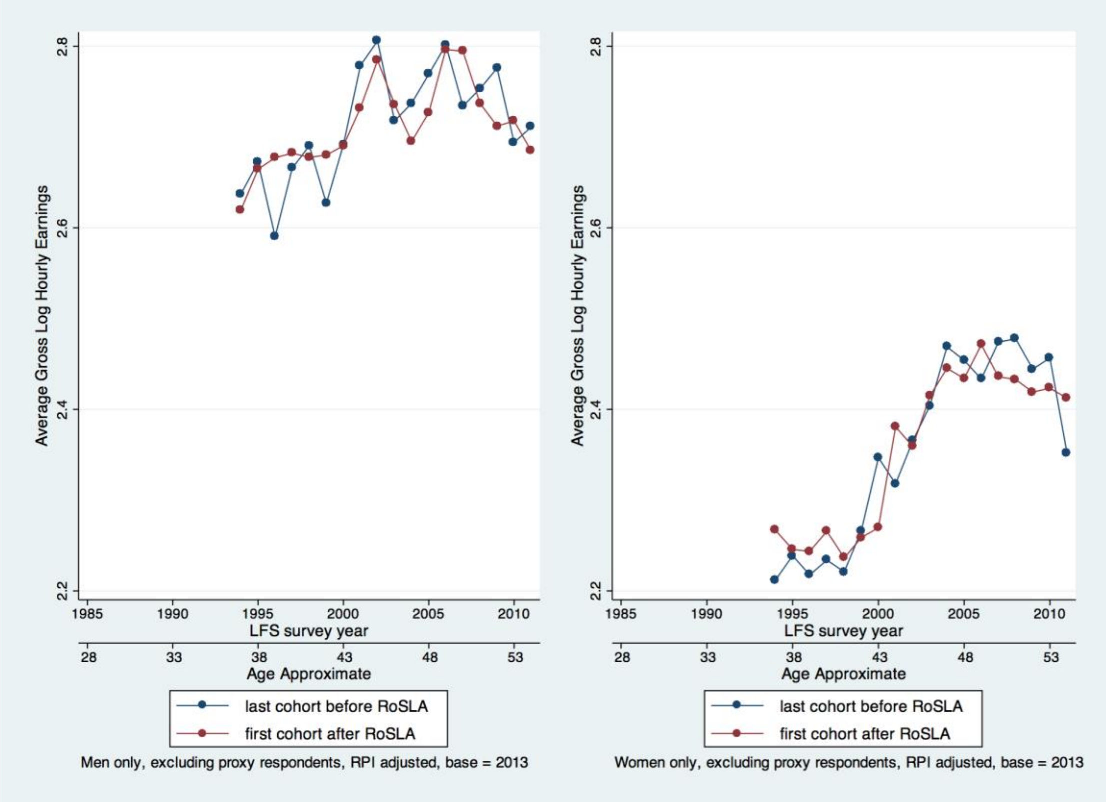

Table B1: Regression Discontinuity Design Estimates of the Reduced Form impact of the 1972 RoSLA on log hourly wages and years of schooling, various bandwidths; pooled LFS data, 1993q1-2011q4. Men only, excluding proxy respondents

		LFS		
Bandwidth (mths)		Log hourly wage	Years of schooling	N
12	*Coeff.*	0.013	0.417***	7,136
	Std. Err.	(0.027)	(0.146)	
24	*Coeff.*	0.004	0.347***	14,267
	Std. Err.	(0.018)	(0.100)	
36	*Coeff.*	0.011	0. 381***	21,303
	Std. Err.	(0.015)	(0.082)	
48	*Coeff.*	0.011	0. 366***	28,435
	Std. Err.	(0.013)	(0.071)	
60	*Coeff.*	0.010	0. 344***	35,942
	Std. Err.	(0.012)	(0.063)	
72	*Coeff.*	0.008	0.313***	43,139
	Std. Err.	(0.011)	(0.058)	

* p<0.10, ** p<0.05, *** p<0.01. Estimates based on local linear regression using rectangular kernel.

Table B2: Regression Discontinuity Design Estimates of the Reduced Form impact of the 1972 RoSLA on log hourly wages at different points in the life-cycle; LFS, by year. Men only, excluding proxy respondents

	Men		
Year	Coeff	S.E.	N
1993	-0.013	(0.026)	6,332
1994	0.031	(0.025)	7,052
1995	0.051*	(0.026)	6,986
1996	0.052**	(0.026)	7,413
1997	0.027	(0.026)	7,161
1998	0.030	(0.025)	6,930
1999	0.023	(0.026)	6,218
2000	-0.003	(0.027)	5,973
2001	-0.011	(0.029)	5,601
2002	-0.009	(0.028)	5,838
2003	-0.012	(0.029)	5,386
2004	-0.020	(0.031)	5,047
2005	0.019	(0.032)	4,672
2006	0.021	(0.032)	4,518
2007	0.013	(0.033)	4,386
2008	-0.028	(0.035)	4,284
2009	-0.011	(0.037)	3,968
2010	0.003	(0.046)	2,565
2011	0.039	(0.064)	1,185

* p<0.10, ** p<0.05, *** p<0.01. Estimates based on local linear regression using rectangular kernel, bandwidth 60 months.