UNIVERSITY OF
STIRLING

DEPARTMENT OF
ECONOMICS

Forced to be Rich?

Returns to Compulsory Schooling in Britain

Paul J. Devereux

Robert A. Hart

Stirling Economics Discussion Paper 2008-02

January 2008

Online at http://www.economics.stir.ac.uk

Forced to be Rich? Returns to Compulsory Schooling in Britain [*]

by

Paul J. Devereux
School of Economics and Geary Institute UCD, CEPR and IZA
devereux@ucd.ie

Robert A. Hart
Department of Economics
University of Stirling
r.a.hart@stir.ac.uk

January, 2008

Abstract:
Researchers using changes in compulsory schooling laws as instruments have typically estimated very high returns to additional schooling that are greater than the corresponding OLS estimates. Given that the first order source of bias in OLS is likely to be upward as more able individuals tend to obtain more education, such high estimates are usually rationalized as reflecting the fact that the group of individuals who are influenced by the law change have particularly high returns to education. That is, the Local Average Treatment Effect (LATE) is larger than the average treatment effect (ATE). However, studies of a 1947 British compulsory schooling law change that impacted about half the relevant population (so the LATE approximates the ATE) have also found very high IV returns to schooling (about 15%), suggesting that the ATE of schooling is greater than OLS estimates would suggest. This constitutes a puzzle: How can the OLS return to schooling be a significantly downward biased estimate of the ATE when the primary source of OLS bias should be upward? We utilize a source of earnings data, the New Earnings Survey Panel Data-set (NESPD), that is superior to the datasets previously used and conclude that there is no such puzzle: the IV estimates are small and much lower than OLS. In fact, there is no evidence of any return for women and the return for men is in the 4-7% range. We do, however, find that men benefit from greater schooling through a reduction in earnings variability.

JEL Classification: J30, J31, J24, I20

Keywords: Returns to schooling, British 1947 compulsory schooling law change; Regression discontinuity design.

[*] Devereux gratefully acknowledges financial support from the National Science Foundation.

1. Introduction

Researchers using changes in compulsory schooling laws as instruments have typically estimated very high returns to additional schooling that are greater than the corresponding OLS estimates. Given that the first order source of bias in OLS is likely to be upward as more able individuals tend to obtain more education, such high estimates are usually rationalized as reflecting the fact that the group of individuals who are influenced by the law change have particularly high returns to education. That is, the Local Average Treatment Effect (LATE) is larger than the average treatment effect (ATE).

However, Oreopoulos (2006) examines a 1947 British compulsory schooling law change that impacted about half the relevant population (so the LATE approximates the ATE) and finds very high IV returns to schooling (about 15%), suggesting that the ATE of schooling is greater than OLS estimates would suggest.[1] This constitutes a puzzle: How can the OLS return to schooling be a significantly downward biased estimate of the ATE when the primary source of OLS bias should be upward? We utilize a source of earnings data, the New Earnings Survey Panel Data-set (NESPD), that is superior to the datasets previously used and conclude that there is no such puzzle: the IV estimates are small and much lower than OLS.

There are additional reasons to doubt the very high returns to earnings that have been found using the 1947 reform. While U.S. compulsory schooling laws have been found to influence a range of outcomes including mortality rates, health, probability of voting, criminal behaviour, fertility, and education of offspring (Lleras-Muney 2005; Milligan et al. 2004; Moretti 2004; Black et al. 2007; Oreopoulos et al. 2006), researchers

[1] Other work by Oreopoulos (2007) and earlier work by Harmon and Walker (1995) finds similar estimates using the 1947 change but also other sources of identification.

have struggled to find strong effects of the 1947 British reform on these types of outcomes (Clark and Royer 2007 for mortality; Milligan et al. 2004 for voting; Lindeboom et al. 2006 and Galindo-Rueda 2003 for intergenerational transmission).[2] Given we expect earnings to impact other behaviours and outcomes, we might expect a 15% return to schooling to lead to large impacts in other dimensions.

The NESPD dataset we use has several advantages. First, it covers the period from 1975 to 2001 and so contains earnings data that encompass large parts of the careers of cohorts impacted by the 1947 reform. In contrast, Harmon and Walker use a Family Expenditure Survey (FES) sample that runs from 1978 to 1986, and Oreopoulos (2006) uses the 1983-1998 survey years from the General Household Survey (GHS). Second, the NESPD is a large dataset that draws on a random 1% sample of the British population and allows a much larger sample than in previous research. Third, as it is a legal obligation on employers to complete the survey, and as it is based on the employer's payroll records, a high response rate is obtained and earnings information is likely to be much more accurate than self and proxy reports from household surveys. Fourth, the NESPD is a panel dataset and so if an individual is missed in any one year, for example due to unemployment, they are likely to be picked up in some other year. Thus, the coverage of the survey is potentially better than for household surveys that are repeated cross-sections. The quality of the dataset enables us to use a Regression Discontinuity (RD) design in the analysis.

The NESPD does not have any information on educational attainment. For this reason, we estimate the first stage relationship between the law change and schooling using the GHS. In addition to studying earnings and wages, we exploit the panel structure

[2] On the other hand, Oreopoulos (2006) does find positive effects of the law change on self-reported health.

of the NESPD data to examine the effects of the law change on wage growth, earnings instability, and job transitions. We find evidence that extra schooling reduces the variability of male earnings in addition to modestly increasing earnings levels. There is no evidence that extra schooling increases earnings or reduces earnings variability for women.

2. Analysing the 1947 Law Change

Background

In 1947 there was a major change in compulsory schooling laws in Britain with the minimum school leaving age increasing from 14 to 15. This change arose as a result of the 1944 Education Act that included a plan for free secondary school education in addition to raising the school leaving age within three years. Prior to 1947, students who planned to leave school at age 14 generally stayed in primary school until that age. Students who planned to stay longer typically switched to secondary school at age 12. While some received free secondary education as a result of good exam performance; others paid fees for secondary school that were heavily subsidised by the State. The reform made free secondary schooling available to all and made the first year of secondary school compulsory. The change was accompanied by an increase in the number of teachers, buildings, and furniture to accommodate the rapidly increased student numbers and the pupil/teacher ratio remained quite stable over this period (see Oreopoulos 2006 and Galindo-Rueda 2003 for further details about the reform).

The effect of the law change was that persons born before April 1933 faced a minimum school leaving age of 14, and persons born from April 1933 onwards faced a minimum age of 15. This reform had a very large impact on school leaving behaviour as can be seen in Figures 1 and 2 – the fraction leaving school before age 15 fell from over 60% in 1932 to about 10% in 1934. Oreopoulos (2006) and Clark and Royer (2007) report similar impacts of the law change on schooling attainment.[3]

Interpreting the Effects of the Law Change

Imbens and Angrist (1994) show that, under a monotonicity assumption, the IV estimator provides a Local Average Treatment Effect (LATE). In other words, it calculates the average effect of the treatment for compliers (individuals whose behaviour is changed by the instrument) only. In our case, the monotonicity assumption implies that the increase in the compulsory schooling age from 14 to 15 does not cause anyone who would have stayed until aged 15+ to now leave at age 14. The IV estimate provides no information about the returns to schooling for always-takers (people who would have stayed in school until at least age 15 irrespective of the legal rule) or never-takers (people who drop out before age 15 irrespective of the legal rule). In our case, both of these groups exist as almost 40% stayed until 15+ before the law change, and about 10% dropped out before age 15 after the law change. This means that, without extrapolation, the LATE is uninformative about the ATE. However, as pointed out by Oreopoulos (2006), it is reasonable to suppose that as the number of compliers becomes an increasingly large proportion of the sample, the LATE should converge towards the ATE. Then, if compulsory schooling law changes in the U.S. (which affected maybe 5% of the

[3] The fact that not everybody born after 1933 reports leaving school after age 14 can be ascribed to misreporting, individual non-compliance, and the fact that a few districts failed to provide sufficient school places for a while after the law was enacted.

population[4]) and the 1947 British reform (which affected about half) provide similar estimated returns to schooling, it would suggest that heterogeneous treatment effects are not a major issue and LATE estimates from North America may be similar to the ATE.

Empirical Specification

We estimate the relationships between the law change and our variables of interest (schooling, wages and earnings) using a regression discontinuity approach (see Imbens and Lemieux, 2007 and the references therein). The base specification regresses the particular outcome on a quartic function of year of birth and a dummy variable for the minimum school leaving age being 15. Our unit of analysis is a year-of-birth cell. We reduce the micro-data to this level of aggregation by taking the means of all variables within each cell – obviously year of birth and the law variable are constant within cells. All subsequent regressions are weighted by the number of observations in each cell. The quartic in cohort allows for smooth changes in outcomes over time and the effect of the law change is identified from the discontinuity in the law variable when the reform is implemented. Later, we augment this global polynomial approach with local linear regression. Because schooling is unavailable in the NESPD, we use a two sample 2SLS approach to estimating the return to schooling. This is described in Section 4.

3. Data

NESPD

The New Earnings Survey Panel Dataset (NESPD) is comprised of a random sample of all individuals whose National Insurance numbers end in a given pair of digits.

[4] Lleras-Muney (2005).

Each year a questionnaire is directed to employers, who complete it on the basis of payroll records for relevant employees. The questions relate to a specific week in April. Since the same individuals are in the sample each year, the NESPD is a panel data set that runs from 1975 to 2001. Because National Insurance numbers are issued to all individuals who reach the minimum school leaving age, the sampling frame of the survey is a random sample of the population. Employers are legally required to complete the survey questionnaire so the response rate is very high. Also, individuals can be tracked from region to region and employer to employer through time using their National Insurance numbers.

However, not everyone in the sample frame is captured every year as questionnaires are sent to employers based on the employee's current tax record. Individuals may not have a current tax record if they have very recently changed jobs and the record has not been updated, or if they do not earn enough to pay tax or National Insurance. In the analysis, we reduce the extent of these problems by giving each person equal weight irrespective of how frequently they appear in the 26-year panel. Later, we also do some analysis in which we investigate the impacts of missing out on some of the lowest paid workers in our base specifications. We find no evidence that this leads to any bias.

Since the data are taken directly from the employer's payroll records, the earnings and hours information in the NESPD are considered to be very accurate. The wage measure we use is "gross weekly earnings excluding overtime divided by normal basic hours for employees whose pay for the survey period was not affected by absence."[5] We also estimate weekly earnings specifications in which weekly earnings (including overtime) replace hourly standard rates. We deflate wages and earnings using the British Retail Price

---

[5] Including overtime payments and taking account of overtime hours produced no substantive changes and so we confine attention to standard hourly rates throughout the paper.

Index as it is Britain's most widely used price index and is similar to the U.S. Consumer Price Index (CPI). We drop cases for which hourly wage observations are less than £1 or more than £150 (in 2001 pounds).[6] Note that both full and part time workers are included in estimation. Part-time workers constitute only about 2% of the male sample but make up over 40% of the female sample. We have verified that omitting part time workers from the sample does not change the estimates to any large extent.

The sample includes individuals who are born between 1921 and 1945 – this provides 12 years before the 1933 reform and 12 years subsequent to 1933 -- and who are aged between 25 and 60. By 25 most Britons have completed education and capping the age at 60 limits the influence of early retirement.[7] Descriptive statistics for the sample are in Table 1.

GHS

The General Household Survey (GHS) is a continuous national survey of people living in private households, conducted on an annual basis by the Office for National Statistics (ONS). The GHS started in 1971 and has been carried out continuously since then, except for breaks in 1997-1998 when the survey was reviewed and 1999-2000 when it was redeveloped. We use the 1979-1998 GHS surveys in our analysis.[8] Being a

[6] These exclusions are similar to those used by Card (1999). They imply the exclusion of 75 male observations (459 female) that have wages less than £1 and 54 male observations (0 female) that have wages greater than £150. To put these numbers in context, the minimum wage was £3.70 in 2001. We also exclude the small number of cases where weekly hours are greater than 84 (56 cases).
[7] Many low skilled people quit working before age 65 in Britain – Banks and Blundell (2005) show that, in the 1980-2000 period, the employment rate of men aged 60-64 is only about 40%. In Section 5 below, we show that our results are robust to tightening the age requirements even further.
[8] We exclude the pre-1979 surveys from the analysis as earnings are measured very differently in this early period, referring to the year preceding the survey rather than to earnings in the week preceding the interview. We exclude post 1998 surveys as no survey was held in 1999 and the survey was relaunched in 2000 with a different design.

household survey, the GHS is subject to non-response and reporting error. The response rates have varied over our sample period between a high of 85% in 1988 and a low of 72% in 1998. We use similar sample selection rules in the GHS as in the NESPD.

Unlike the NESPD, the GHS is not a panel but a set of repeated cross-sections. Also, it has information on schooling attainment that is not present in the NESPD. The variable we use is the age at which the person left school. This is appropriate for our purposes as we are estimating the value of an extra year spent at school (as distinct from the value of going to college or doing a PhD).[9]

The earnings data in the GHS are inferior to those in the NESPD due to mis-reporting and non-response. Also, while weekly earnings measures are broadly comparable over time, there have been changes in the exact definition of earnings. The GHS does have information on usual weekly hours so one can construct an hourly earnings variable. One drawback is that the earnings information includes overtime earnings but the hours variable does not include overtime hours. Despite this problem, we report estimates using the hourly wage variable in addition to weekly earnings.[10] Descriptive statistics for the sample are in Table 1.

4. Results

A. OLS Estimates Using the GHS

The NESPD has no information about years of schooling so we require another data source to estimate the OLS relationship between schooling and wages and the first

[9] While there is also a measure of age when the person completed education, it is not very reliable as there appear to be many cases where people add some education after long absences from the system.

[10] Manning (2000) also uses this variable and shows that it is highly correlated with the true hourly wage (correlation=.98) because average overtime hours are relatively short (less than 3 per week) and because overtime hours are very weakly correlated with hourly earnings.

stage relationship between the law change and schooling. Following some recent literature, we use the General Household Survey (GHS) for this purpose.[11] The OLS specification uses individual-level data and regresses log wages (earnings) on age left school, a quartic in cohort, and a full set of age dummies. For comparability with the NESPD sample, we include immigrants and exclude the self-employed. There are 29,217 observations for men and 26,934 observations on women. For hourly wages, the coefficient on age left school is .140 (.002) for men and .165 (.002) for women. For weekly earnings, the analogous coefficients are .134 (.002) and .194 (.004) for men and women respectively. These are large "returns" and suggest that either the value of an additional year in school is very high for these cohorts or there is a lot of selection in terms of who leaves school early.[12] The analysis using the change in the compulsory schooling law is designed to differentiate between these two explanations.

B. First Stage from GHS

Because the cohorts impacted by the law are those born after April 1 1933, approximately 3/4 of the 1933 cohort is impacted by the reform. Given that we observe birth year but not birth month, we define the law variable as being equal to zero for persons born before 1933 and one otherwise. We also explore the robustness of our estimates to allowing the law to have a different impact for the 1933 cohort (who are partially affected) than for subsequent cohorts (who are fully impacted).

---

[11] An alternative would be the Family Expenditure Survey (FES). We have chosen not to use it as the FES does not have information on year of birth (just age), does not have information on country of birth, and also has only about a 60% response rate.

[12] These OLS estimates are possibly so high because they are estimated from variation in the lower tail of the education distribution – age left school will not vary much between college degree holders and people who finish school but do not go to college.

In the NESPD, we know year of birth. In the GHS, year of birth is reported in surveys carried out from 1986 onwards.[13] Unfortunately, 1986 is too late a starting year for the analysis as individuals born in 1921 are aged 65 in that year. Therefore, we use the GHS surveys starting from 1979 to estimate a first stage relationship. In the 1979 to 1985 GHS, we impute year of birth using information on age, survey year, and survey month.[14]

As discussed above, the base specification regresses age left school on a quartic function of cohort and a dummy variable for the minimum school leaving age being 15. We reduce the micro-data to 25 year-of-birth cells by taking the means of all variables within each cohort and weight all regressions by the number of observations in each cell.

For comparability with the NESPD sample, we restrict the sample to persons aged between 25 and 60 who are members of the 1921 to 1945 cohorts. The NESPD sample includes only persons who work either full-time or part-time and excludes the self-employed. For consistency with the NESPD sample exclusions, we report first stage regressions for the sample of employed individuals who are not self-employed and who work between 1 and 84 hours a week. Note that even though the GHS has country of birth indicators, we have not restricted the sample to British-born persons because we cannot impose this restriction in the NESPD.

The first stage estimates are presented in column (1) of Table 2. The effect of the law is to increase the average school leaving age by .37 of a year for men and .46 of a year for women and these are both strongly statistically significant. While not reported in the Table, the law reduced the proportion who finished school at 14 or younger by .41

[13] Year of birth is also available for women aged 16-49 in the 1983-1985 surveys and we use this information where available.

[14] We impute year of birth as being (survey year – age) for persons who are interviewed between July and December, and as being (survey year – age – 1) for persons interviewed between January and June.

(.05) for men and .42 (.05) for women.[15] Figures 1 to 4 below display the estimates graphically (we exclude immigrants when drawing the figures). The polynomial fits in these figures are created using the baseline specification of a quartic in cohort. The break in 1933 is very clear.

Note also that the first stage coefficients are very robust to the specification used. For example, Appendix Table 1 shows estimates when cohort-age cells are used instead of cohort cells, allowing the inclusion of age controls. Adding age to the specification has little impact. The first stage estimates are also very similar irrespective of the exact sample used – i.e. varying the age ranges of respondents, the exact years in the GHS used, excluding or including the non-employed and the self-employed etc. So, it does appear that our GHS sample provides a reliable first stage.

C. Reduced Form Effects on Hourly Wages and Weekly Earnings

The baseline reduced form estimates from the NESPD are in column (3) of Table 2 (Panel A for hourly wages and Panel B for weekly earnings). The results differ between men and women. For men, the reduced form estimate of the effect of the law change on log wages is .021 (.005) so the law increases wages by about 2%. For women, the estimate is a statistically insignificant .001 (.008), suggesting very small effects of the law change. Column (3) in Panel B includes analogous estimates for log weekly earnings (including overtime) and these are very similar to the wage estimates.[16]

[15] The effects of the law are strongly concentrated at age 14 – 15. The effect of the law on probability finish by age 15 is -.030 (.012) for men and -.046 (.020) for women. The equivalent figures for age 16 are small and statistically insignificant -- .010 (.005) for men and .005 (.009) for women.

[16] The reduced form effects are almost identical when we use median wages or earnings in the cohort cell rather than mean earnings as the dependent variable (for men reduced form effects are .022 (.008) and .017 (.008) for wages and earnings respectively; for women the equivalent numbers are -.0006 (.007) and .011 (.017)). This indicates that our estimates are not being strongly impacted by outliers.

Figures 5 and 6 below display the wage estimates graphically. For men, there is a clear break in the series in 1933. For women, it is equally clear that there is no break in the series in 1933.

D. Two Sample Two Stage Least Squares

Given that the first stage and reduced form regressions come from different datasets, it is not possible to do conventional Two Stage Least Squares (2SLS) to estimate the return to an extra year in school. Instead we use Two Sample Two Stage Least Squares (TS2SLS) (Angrist and Krueger 1992; Inoue and Solon 2006). This is implemented by forming the predicted value of schooling using the first stage coefficients estimated in the GHS and the actual explanatory variables from the NESPD. We then use the NESPD to regress the log wage on the predicted value of schooling and the usual explanatory variables.[17] Note that when the law is the only instrument, the TS2SLS estimator is simply the reduced form effect of the law divided by the first stage effect.

The estimates are in column (5) of Table 2. As suggested by the reduced forms, the return to schooling is essentially zero for women but positive for men. The size of the effect for men is .057, implying that an extra year of schooling increases wages by about 5 or 6%.

E. Allowing Effect of Law to Differ for 1933 Cohort

In Table 2, we also report estimates in which there are two exogenous variables of interest – a dummy for whether the person is a member of the 1933 cohort, and a dummy

[17] In calculating the TS2SLS standard errors, we treat the predicted value of schooling as being known. We have experimented with allowing for the fact that this variable is estimated but doing so has little effect on the standard errors.

for coming from a subsequent cohort. As expected, the first stage effect is smaller for the first dummy than for the second. The coefficient on the second dummy, which can be interpreted as the effect of the law fully implemented, is .42 for men and .51 for women. As can be seen in column (6), the TS2SLS estimates when both of these variables are used as instruments are very close to those using just the Law = 1 dummy as instrument, suggesting that the exact specification used for the law change is not critical. The estimates are also very similar (column (7)) when we include the dummy for whether the person is a member of the 1933 cohort in the 2^{nd} stage and use the dummy for coming from a post-33 cohort as the only excluded instrument. Given the similarities, we report only the estimates using the single law change variable in subsequent tables.

5. Robustness Checks

Allowing For Age Effects

Given the life-cycle pattern of earnings, it is possible that adding age controls might matter. In appendix Table 1, we report estimates from specifications that group the individual-level data by cohort-age and include age controls. However, both reduced form and TSTSLS estimates are very little impacted by the addition of controls for a quadratic in age, or by the inclusion of a full set of age dummies. This can be seen in columns (4) to (9) in Appendix Table 1.

Restricting the Sample to Persons aged 35-50

Haider and Solon (2006) show using U.S. data that current income is a reasonable proxy for lifetime income for men aged between their early 30s and late 40s. For this reason (and because in Britain many men retire before age 60), we have checked the robustness of our results to restricting the NESPD sample to men aged between 35 and 50. Note that this age restriction implies that the cohorts followed are now born between 1925 and 1945. The TS2SLS estimates for men fall to .03 (.04) for wages and .01 (.05) for earnings. Thus, the finding of a modest return to additional compulsory schooling is, if anything, strengthened when the sample is restricted to ages in which current earnings are likely to be a good proxy for permanent earnings.

For women, it is less clear which ages provide the most representative earnings due to the issues raised by childbearing and childcare. However, in any case, the estimates for women in the 35-50 age group are -.001 (.03) for wages and .02 (.05) for earnings. These are very similar to the estimates for the broader age range.

Undersampling of Low-Paid in NESPD

The NESPD under-samples individuals who earn less than the PAYE tax threshold in Britain and so are not subject to tax. The threshold has varied over time between £675 per year in 1975 and £4535 per year in 2001. To assess the extent of this problem, we compare the proportion of observations in the NESPD sample that are under the threshold to the equivalent figure from the GHS sample. For men, there are fewer than 1% of these observations in either sample so it is not a relevant issue. For women, the proportions are 27% in the GHS and 18% in the NESPD (once we give each individual the same weight). We have tried reweighting the regressions to get some sense as to what biases might arise. To do so, we gave a weight of 1.5 to observations in the NESPD that were below the tax threshold and a weight of 2/3 to observations that are above the threshold. This exercise had very little effect on the estimates – the reduced form effect of the law on the log wage for women remained at .001 (.007) and the equivalent estimate for log earnings fell to .003 (.018). Thus, all indications suggest that this is not a big issue.[18]

Inclusion of Immigrants in NESPD

There is no information about place of birth in the NESPD and so our sample includes both British-born and immigrants. Since immigrants may not have gone through the British schooling system, ideally the sample would be restricted to British-born. One nice feature of the GHS is that it has information on country of birth. In Table 1, we see that only about 8% of the GHS sample are immigrants.[19] In Table 4, we go further and compare GHS estimates from the British-born sample to those from a sample that includes foreign born people as well (like the NESPD). Given the small percentage of our cohorts that are born abroad and the fact that immigration is unlikely to be systematically related to compulsory schooling laws, one would not expect large biases in 2SLS estimates resulting from contamination by immigration. However, one would expect both the first stage estimates and the reduced form estimates to be lower when foreign-born persons are included in the sample.

The estimates are in Table 3. Initially we restrict the GHS sample to employees as the NESPD sample excludes the self-employed. Comparing column (1) to column (2), we

---

[18] A more fundamental selection problem is that we only observe wages for individuals who work and the law may systematically impact employment probabilities. Interestingly, we find that there is a small but statistically significant negative effect of the law on employment in the GHS for both men and women. This is not just an early retirement effect as it is also present when the sample is restricted to persons aged 35-50. Assuming that the non-employed would tend to have lower wages if they worked, this suggests that our estimates may be biased upwards due to this type of selection.

[19] We have calculated a similar percentage for these cohorts using the British Labour Force Survey.

see that the first stage relationship between the law and schooling increases modestly when immigrants are excluded (from .41 to .45 for men and from .46 to .48 for women).[20] Columns (4) and (5) show the analogous reduced form estimates and indicate that, if anything, excluding immigrants reduces the effects of the law on wages and earnings a little and the 2SLS estimates in columns (7) and (8) tell the same story. There clearly is no evidence that the NESPD estimates are biased down as a result of some immigrants being present in the sample and this provides another reason to have confidence in the NESPD results in Table 2. Interestingly, the GHS estimates are broadly similar to those we found using the NESPD but the standard errors are generally higher.

Exclusion of Self-Employed from NESPD

One can also examine whether the GHS estimates are different when the self-employed are included. These results are in columns (6) and (9) in Table 4. Including the self-employed tends to increase the size of the estimates but not to any large extent. The small change reflects the fact that the self-employed constitute only about 12% of the GHS sample. Also, we have verified that the law variable has no significant effect on the probability that an individual is self-employed for either men or women in the GHS sample. Including the self-employed does tend to reduce the precision of the estimates and this presumably reflects the greater variance of earnings and hours of the self-employed and the likelihood that these are measured more poorly for this group.[21]

---

[20] The first stage coefficients are slightly different from those in Table 2 because there is an additional sample restriction here that persons must have a wage observation.

[21] We have also tried restricting the age range to 35-50 in the GHS as we did with the NESPD. The resultant wage estimates that are equivalent to column (9) in Table 4 are .02 (.05) and .02 (.04) for men and women respectively; the respective earnings estimates are .01 (.05) and -.06 (.07).

Local Linear Regression

In the specifications so far, we have modelled cohort as a global polynomial and looked for a discontinuous effect of the law change. An alternative strategy is to use local linear regression which involves fitting linear regression functions to the observations within a distance h on either side of the discontinuity point. The key is that both the slope and the intercept of the regression function are allowed to differ before versus after the law change. The effect of the law change is then measured as the difference between the two intercepts.

In our specific context, TS2SLS can be estimated using

$$SCH_i = \alpha_0 + \alpha_1 1\{YOB_i \geq 33\}.(YOB_i - 33) + \alpha_2 1\{YOB_i < 33\}.(YOB_i - 33) + \alpha_3 LAW_i + \varepsilon_i$$

where i indexes cohort, SCH is age left school, YOB is year of birth, and 1(.) is the indicator function. This equation is used to form the predicted value of schooling (PREDSCH) for each cohort i and then the return to schooling (β_3) is estimated using

$$\ln Y_i = \beta_0 + \beta_1 1\{YOB_i \geq 33\}.(YOB_i - 33) + \beta_2 1\{YOB_i < 33\}.(YOB_i - 33) + \beta_3 PREDSCH_i + \varepsilon_i$$

Local Linear regression is useful for analyzing points close to the discontinuity. In practice, it is not clear what bandwidth (h) to choose. For this reason, we report estimates for the 30-35 cohorts as well as those from 29-36, 28-37, and 27-38.[22] See Imbens and

[22] We take advantage of the restricted cohort ranges here to calculate the first stage using only GHS observations in which year of birth is known. This is the case from 1986 onwards for men and from 1983 onwards for most women.

Lemieux (2007) for discussion on the importance of testing robustness to the bandwidth chosen.

The estimates are in Table 4. While less precisely estimated, they are quite consistent with the other NESPD estimates from Table 2. There is no evidence of any effect for women but there is some evidence of a modest return to schooling for men in the 4 – 7% range.

6. Reconciling Estimates with the Literature

There are many studies that report estimates of the return to schooling using compulsory schooling laws. For example, Angrist and Krueger (1991) and Oreopoulos (2006) for the U.S., Black et al. (2005) for Norway, Grenet (2005) for France, and Pischke and von Wachter (2005) for Germany. The U.S. estimates are higher than OLS but the estimates from the three papers using European data are all lower than OLS and sometimes very low – Pischke and von Wachter report estimates suggesting zero returns to schooling in Germany. We now turn to the British literature.

Harmon and Walker (1995) use both the 1947 law and a subsequent 1973 law change for identification. They study men only and report returns to schooling of 15 percent, far above their OLS estimate. Their paper has been criticised by Oreopoulos (2006) and by Pischke and von Wachter (2005) because of its failure to adequately control for cohort effects -- they include survey year dummies and a quadratic in age but no controls for cohort (although the linear age variable is in effect a linear cohort variable given they control for survey year). Also, because they use the later 1973 law change that impacted a much smaller proportion of the population, their LATE estimates are not directly comparable to ours. Given all the differences in approach, it is not surprising that our estimates are different from theirs.

Recently Oreopoulos (2006) studies the 1947 law change using GHS files from 1983-1998. His sample includes British-born individuals born between 1921 and 1951 who were aged between 32 and 64 at the time of the survey. He provides both differences-in-differences (DD) and regression discontinuity (RD) evidence. The DD specifications control for a quartic in cohort (cohort dummies in some specifications) and a Northern Ireland (NI) dummy and exploit the fact that the school leaving age was raised later in NI than in the rest of the UK. The DD approach assumes that the cohort effects are the same in Britain and NI and this may be a strong assumption given that Northern Ireland is a unique place with its issues of religious discrimination that do not apply to the rest of the UK. So, our estimates may differ from his DD estimates because of the different identifying assumptions used. While attitudes to identifying assumptions are necessarily subjective, we do not find this approach as compelling as the RD design using British data alone.[23]

Oreopoulos (2006) also uses an RD approach using Britain alone and finds IV estimates of .15 (.06) for all workers, and .15 (.13) for men. These estimates are sufficiently imprecisely estimated to leave open the possibility of very small or very large returns to education. However, it is puzzling is that our RD estimates using the GHS are very different from his. We are currently trying to figure out how the differences in the exact samples used lead to these coefficient differences

[23] It is clear from his paper that Oreopoulos (2006) also considers the RD approach to be particularly compelling.

7. Schooling and Earnings Instability

In addition to studying earnings and wages, we exploit the panel structure of the NESPD data to examine the effects of the law change on earnings growth, earnings instability, and job transitions. These are important outcomes as increased human capital may enable persons to work in less risky jobs or occupations and so increase utility by reducing the variance of earnings over time for any given level of lifetime earnings. However, we have found no causal analysis in the literature concerning the effects of schooling on earnings uncertainty.[24]

To carry out the analysis, we restrict the NESPD sample to cases where there is information on wages in the current year and the previous year (so all 1975 observations are dropped). The estimates for men are in Table 5. First we study the effects of the law on changes in log wages and changes in log earnings between t-1 and t. The estimates are negative and statistically insignificant, suggesting that men with more schooling do not benefit through increased wage growth as well as increased wage levels. It is important to note, however, that men here are aged between 30 and 60 and the benefits to schooling in terms of accelerated wage growth may have already occurred prior to entering our sample.[25]

Even if schooling does not increase wage growth, it may lead to less instability in wages. We examine this question by studying whether the law impacts the absolute value of log wage changes. For men, the estimates for earnings are negative and statistically significant and those for wages are also negative and almost statistically significant. They

[24] See Anderberg and Andersson (2003) for a discussion of some of the literature on the relationship between education and earnings uncertainty. Often earnings variances are found to be higher for more educated people but this has no causal interpretation.

[25] Additionally, when we restrict the sample to persons aged 50 or less, the estimates become much closer to zero.

suggest that an extra year of schooling reduces the absolute wage change by about 1% and the absolute earnings change by about 2.5%. This latter estimate is quite large.[26]

Another measure of instability is job changing. Education may help people obtain better job matches and lower the risk of job loss. We find no statistically significant effect of schooling on the probability of changing jobs between t-1 and t (the job change variable is actually defined as being 1 if the person is in the current job for less than 12 months). However, the estimates for men are negative and close to statistical significance.

Overall, the evidence suggests that additional schooling reduces earnings instability for men. For women, nothing is statistically significant and the coefficients are generally quite small. Thus, there is no evidence that schooling impacts the wage growth or earnings stability of women. Figures 7 and 8 show the average values of the absolute change in log earnings for men and women.

8. Conclusions

The 1947 change in the British compulsory schooling law has enabled us to estimate the returns to extra schooling for men and women in a situation where about half the population leave school at the earliest possible age. We find no evidence of any wage or earnings return for women and the balance of evidence suggests a modest return of 4-7% for men. Additionally, we find that men also benefit from extra schooling through lower earnings variability.

The returns to schooling we find are significantly below comparable OLS estimates. If our estimates are picking up something close to the average treatment effect

[26] These findings are robust to defining instability as the variance or standard deviation of real log wages or earnings for the individual over time. They are also fairly robust to excluding men aged more than 50 from the sample.

in the population, then the ATE is much lower than the OLS estimate. This is consistent with the fact that more able children tend to obtain more schooling. It also suggests that large estimates found using other compulsory schooling law changes that impacted the schooling of much fewer people may be picking up very high returns to schooling for the small number of compliers. Heterogeneous treatment effects appear to be important when estimating the return to schooling.

Our estimates also may help explain the apparent puzzle of why half the British population dropped out of school as early as they could given the returns to schooling are so high. One simple explanation is that the returns to additional schooling were actually quite low and it was rational to leave school early. While it is difficult to quantify the costs of an extra year of schooling, this story may be consistent with what we find in this paper.

References

Anderberg Dan and Fredrik Andersson. (2003). “Investments in human capital, wage uncertainty, and public policy,” *Journal of Public Economics*, 87(7-8: August), 1521-1537.

Angrist Joshua D. and Alan B. Krueger (1991). “Does Compulsory Schooling Attendance Affect Schooling and Earnings?,” *Quarterly Journal of Economics*, 106, 979-1014.

Angrist Joshua D. and Alan B. Krueger (1992). “The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples,” *Journal of the American Statistical Association*, 87, 328-336.

Banks James and Richard Blundell (2005). “Private Pension Arrangements and Retirement in Britain,” *Fiscal Studies* 26(1), 35-53.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. (2005). “Why the Apple Doesn’t Fall Far: Understanding Intergenerational Transmission of Human Capital,” *American Economic Review*, 95(1), 437-449.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. (2007). “Staying in the Classroom and out of the Maternity Ward? The Effect of Compulsory Schooling Laws on Teenage Births,” forthcoming *Economic Journal*.

Card, David (1999). “The Causal Effect of Education on Earnings,” in Orley Ashenfelter and David Card (eds.), *Handbook of Labor Economics*, Volume 3A, North-Holland.

Clark, Damon and Heather Royer (2007). "The Effect of Education on Longevity: Evidence from the United Kingdom," mimeo.

Galindo-Rueda, Fernando (2003). "The Intergenerational Effect of Parental Schooling: Evidence from the British 1947 School Leaving Age Reform," mimeo.

Grenet, Julien (2005) "Is it Enough to Increase Compulsory Schooling to Raise Earnings? Evidence from the French Berthoin Reform (1967)," PJSE Working Paper.

Haider Steven & Gary Solon. (2006). "Life-Cycle Variation in the Association between Current and Lifetime Earnings," *American Economic Review*, American Economic Association, vol. 96(4), pages 1308-1320, September.

Harmon Colm and Ian Walker. (1995), "Estimates of the Economic Return to Schooling for the United Kingdom," *American Economic Review* 85, 1278-1296.

Imbens, Guido and Joshua Angrist. (1994). "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62(2), 467-476.

Imbens, Guido and Thomas Lemieux. (2007). "Regression Discontinuity Designs: A Guide to Practice," NBER Technical Working Paper #337, April.

Inoue, Atsushi, and Gary Solon. (2006). "Two-Sample Instrumental Variables Estimators," Working Paper.

Lindeboom Maarten, Ana Llena Nozal, and Bas van der Klaauw (2006), "Parental Education and Child Health: Evidence from a Schooling Reform," Tinbergen Institute Working Paper 109/3.

Lleras-Muney, Adriana. (2005), "The Relationship Between Education and Adult Mortality in the United States," *Review of Economic Studies* 72, 189-221.

Manning, Alan (2000), “Movin’ on Up: Interpreting the Earnings-Experience Profile,” *Bulletin of Economic Research* 52(4), 261-295.

Milligan Kevin, Moretti Enrico, Oreopoulos Philip. (2004) “Does education improve citizenship? Evidence from the United States and the United Kingdom,” *Journal of Public Economics*, 88:1667–95.

Moretti, Enrico and Lance Lochner. (2004), “The Effect of Education on Criminal Activity: Evidence from Prison Inmates, Arrests and Self-Reports,” *American Economic Review* 94(1).

Oreopoulos, Philip (2006), “Estimating Average and Local Average Treatment Effects of Education When Compulsory Schooling Laws Really Matter, *American Economic Review* 96, 152-175.

Oreopoulos, Philip (2007), “Do Dropouts Drop Out Too Soon? Wealth, Health, and Happiness from Compulsory Schooling,” *Journal of Public Economics*, forthcoming published online February 2007.

Oreopoulos Philip, Marianne E. Page, and Ann Huff Stevens (2006), “The Intergenerational Effects of Compulsory Schooling,” *Journal of Labor Economics* 24(4), 729-760.

Pischke, Jorn-Steffen and Till von Wachter (2005), “Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation, NBER Working Paper 11414.

Table 1: Descriptive Statistics

NESPD (1975 - 2001)

Variable	Obs	Mean	Std. Dev.	Min	Max
Survey Year	1219398	82.71	6.42	75	101
Cohort	1219398	33.43	7.27	21	45
Female	1219398	0.44	0.50	0	1
Age	1219398	48.28	7.29	29	60
Hours Worked	1219398	34.03	9.04	1	84
Log (Hourly Wage)	1219398	1.94	0.49	0.00	5.01
Log (Weekly Earnings)	1219398	5.47	0.71	0.32	8.82
Law mandates school until 15	1219398	0.54	0.50	0	1

These are weighted means with the weights being the inverse of the number of times the individual is in the sample. In total, there are 145883 individuals who are sampled an average of 8.36 times.

General Household Survey (1979 – 1998)

Variable	Obs	Mean	Std. Dev.	Min	Max
Survey Year	72523	85.54	5.07	79	98
Cohort	72523	36.12	6.49	21	45
Female	72523	0.44	0.50	0	1
Age	72523	49.00	6.57	33	60
British Born	72523	0.92	0.27	0	1
Employee	72523	0.88	0.33	0	1
Hours Worked	72523	35.82	13.24	1	84
Log (Hourly Wage)	61549	1.86	0.58	0.00	4.97
Log (Weekly Earnings)	61549	5.32	0.85	0.08	8.99
Age Left School	72523	15.34	1.27	10	24
Left School by age 14	72523	0.24	0.43	0	1
Left School by age 15	72523	0.67	0.47	0	1
Law mandates school until 15	72523	0.71	0.46	0	1

Table 2: Estimated Effect of an extra year of schooling on Hourly Wages and Weekly Earnings

Panel A: Hourly Wages

		First Stage		Reduced Form		TS2SLS		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Men	Law = 1	.369*		.021*				
		(.060)		(.005)				
	Cohort = 1933		.310*		.020*			
			(.040)		(.004)			
	Cohort > 1933		.423*		.022*			
			(.053)		(.007)			
	Schooling (1 Instrument)					.057*		
						(.013)		
	Schooling (2 Instruments)						.054*	.051*
							(.015)	(.016)
Women	Law = 1	.455*		.001				
		(.053)		(.008)				
	Cohort = 1933		.402*		.002			
			(.029)		(.007)			
	Cohort > 1933		.505*		-.0002			
			(.045)		(.011)			
	Schooling (1 Instrument)					.002		
						(.018)		
	Schooling (2 Instruments)						.001	-.0004
							(.019)	(.021)

First Stage regressions are based on 25 cohort cells for men and 25 cohort cells for women from the GHS. Reduced Form regressions are based on 25 cohort cells for men and 25 cohort cells for women from the NESPD. All regressions are weighted by cell size and include a quartic function of year-of-birth.
TS2SLS is Two Sample Two Stage Least Squares.
Robust standard errors in parentheses.
* denotes statistically significant at the 5% level.
Schooling (1 Instrument) is the specification where the instrument is the dummy variable 1(Law =1).
Schooling (2 Instruments) is the specification where the instruments are the dummy variables 1(Cohort = 1933) and 1(Cohort > 1933). In column (7), 1(Cohort = 1933) is included as a control variable.
There are 33978 male observations and 29567 female observations used in the GHS.
There are 718120 male observations (on 81461 men) and 501278 female observations (on 64422 women) used in the NESPD.

Table 2: Estimated Effect of an extra year of schooling on Hourly Wages and Weekly Earnings

Panel B: Weekly Earnings

		First Stage		Reduced Form		TS2SLS		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Men	Law = 1	.369* (.060)		.021* (.006)				
	Cohort = 1933		.310* (.040)		.023* (.004)			
	Cohort > 1933		.423* (.053)		.019* (.007)			
	Schooling (1 Instrument)					.057* (.015)		
	Schooling (2 Instruments)						.050* (.017)	.045* (.016)
Women	Law = 1	.455* (.053)		.007 (.015)				
	Cohort = 1933		.402* (.029)		.017 (.011)			
	Cohort > 1933		.505* (.045)		-.003 (.017)			
	Schooling (1 Instrument)					.015 (.032)		
	Schooling (2 Instruments)						.007 (.031)	-.005 (.034)

First Stage regressions are based on 25 cohort cells for men and 25 cohort cells for women from the GHS. Reduced Form regressions are based on 25 cohort cells for men and 25 cohort cells for women from the NESPD. All regressions are weighted by cell size and include a quartic function of year-of-birth.
TS2SLS is Two Sample Two Stage Least Squares.
Robust standard errors in parentheses.
* denotes statistically significant at the 5% level.
Schooling (1 Instrument) is the specification where the instrument is the dummy variable 1(Law =1).
Schooling (2 Instruments) is the specification where the instruments are the dummy variables 1(Cohort = 1933) and 1(Cohort > 1933). In column (7), 1(Cohort = 1933) is included as a control variable.
There are 33978 male observations and 29567 female observations used in the GHS. There are 718120 male observations (on 81461 men) and 501278 female observations (on 64422 women) used in the NESPD.

Table 3: Estimated Effect of an extra year of schooling on Wages and Earnings (GHS)

	First Stage			Reduced Form			Two Stage Least Squares		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hourly Wages									
Men	.411*	.447*	.426*	.015*	.011	.020	.037*	.025	.047
	(.041)	(.038)	(.037)	(.008)	(.008)	(.012)	(.018)	(.016)	(.026)
Women	.460*	.480*	.481*	.016	.011	.017	.035*	.023	.036*
	(.043)	(.042)	(.043)	(.007)	(.008)	(.010)	(.014)	(.017)	(.017)
Weekly Earnings									
Men	.411*	.447*	.426*	.028*	.026*	.028*	.069*	.058*	.066*
	(.041)	(.038)	(.037)	(.008)	(.009)	(.011)	(.022)	(.020)	(.022)
Women	.460*	.480*	.481*	.017	.002	.005	.037	.005	.011
	(.043)	(.042)	(.043)	(.022)	(.020)	(.019)	(.044)	(.042)	(.038)
Immigrants included	Yes	No	No	Yes	No	No	Yes	No	No
Employees only	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No

First stage, reduced-form, and 2SLS regressions are based on 25 cohort cells for men and 25 cohort cells for women. All regressions are weighted by cell size and include a quartic function of year-of-birth.
Robust standard errors in parentheses.
* denotes statistically significant at the 5% level.
There are 29217 men and 26934 women used in columns (1), (4), and (7).
There are 27016 men and 24876 women used in columns (2), (5), and (8).
There are 30800 men and 26039 women used in columns (3), (6), and (9).

Table 4: Estimated Effect of an extra year of schooling on Wages and Earnings (Local Linear Regression)

	1930 – 1935 Cohorts		1929 – 1936 Cohorts		1928 – 1937 Cohorts		1927 – 1938 Cohorts	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hourly Wages	First Stage	TS2SLS	First Stage	TS2SLS	First Stage	TS2SLS	First Stage	TS2SLS
Men	.283*	.067*	.282*	.058*	.248*	.058*	.311*	.043*
	(.021)	(.024)	(.059)	(.028)	(.051)	(.023)	(.060)	(.018)
Women	.299	-.006	.375*	.007	.332*	.005	.408*	.004
	(.149)	(.058)	(.123)	(.030)	(.099)	(.036)	(.095)	(.024)
Weekly Earnings								
Men	.283*	.069*	.282*	.035	.248*	.051	.311*	.039
	(.021)	(.025)	(.059)	(.040)	(.051)	(.030)	(.060)	(.019)
Women	.299	.023	.375*	.034	.332*	.040	.408*	.026
	(.149)	(.085)	(.123)	(.042)	(.099)	(.045)	(.095)	(.037)

Regressions are carried out on cohort-level cells so there are 6 observations in columns (1) and (2), 8 observations in columns (3) and (4), 10 observations in columns (5) and (6), and 12 observations in columns (7) and (8). All regressions are weighted by cell size and include a linear cohort effect that is allowed to have a different slope after the law change.
First stage is estimated using the GHS. TS2SLS is estimated using the NESPD and the GHS.
TS2SLS is Two Sample Two Stage Least Squares.
Robust standard errors in parentheses.
* denotes statistically significant at the 5% level.

For the 1930-1935 cohorts, we use 3276 (19118) men and 3241 (15263) women in the GHS (NESPD). For the 1929-1936 cohorts, we use 4355 (25446) men and 4461 (20403) women in the GHS (NESPD). For the 1928-1937 cohorts, we use 5364 (31863) men and 5603 (25535) women in the GHS (NESPD). For the 1927-1938 cohorts, we use 6401 (38215) men and 6810 (30673) women in the GHS (NESPD).

Table 5: Estimated Effect of an extra year of schooling on Other Outcomes

	Men		Women	
	Reduced Form	TS2SLS	Reduced Form	TS2SLS
	(1)	(2)	(3)	(4)
Change in Log Hourly Wages	-.003	-.009	-.001	-.003
	(.003)	(.007)	(.003)	(.007)
Change in Log Weekly Earnings	-.004	-.010	-.002	-.005
	(.004)	(.010)	(.003)	(.007)
Absolute Value of Change in log Hourly Wage	-.004	-.010	.0003	.0007
	(.002)	(.006)	(.002)	(.004)
Absolute Value of Change in log Weekly Earnings	-.009*	-.025*	-.004	-.010
	(.003)	(.009)	(.006)	(.012)
Whether Started New Job	-.006	-.016	-.003	-.007
	(.004)	(.011)	(.003)	(.008)

(Unreported) first stage regressions are based on 25 cohort cells from the GHS. Reduced form and Two Sample Two Stage Least Squares (TS2SLS) estimates are based on 25 cohort cells from the NESPD. All regressions are weighted by cell size and include a quartic function of year-of-birth.
Robust standard errors in parentheses.
* denotes statistically significant at the 5% level.

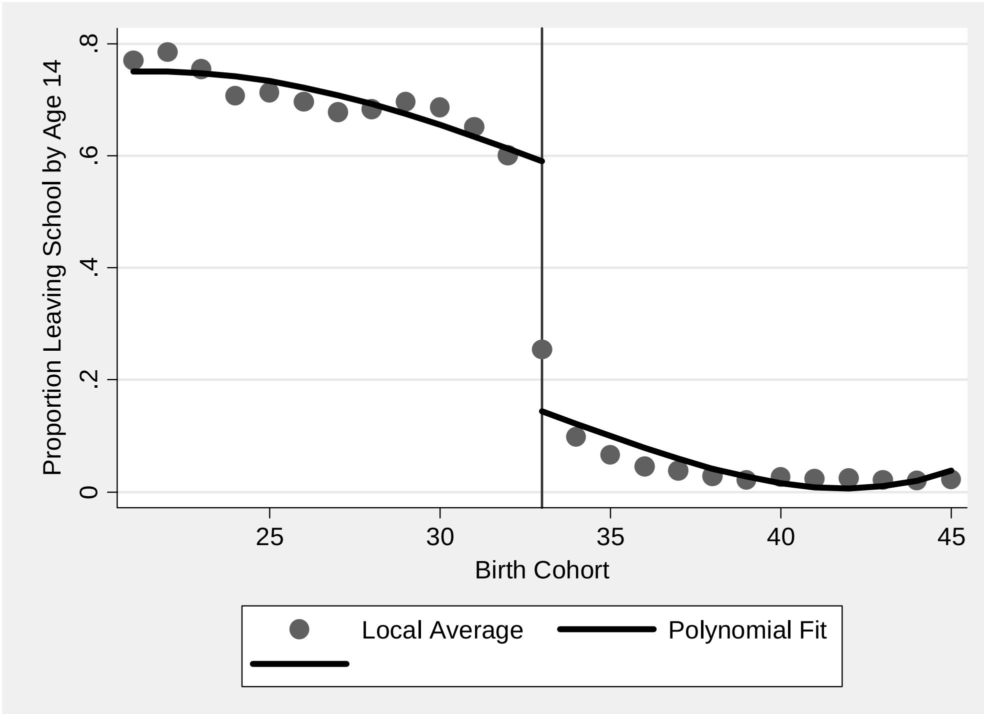
Based on 636659 NESPD observations on 67768 men and 436856 NESPD observations on 52762 women from 1976 to 2001.

Appendix Table 1: Estimated Effect of an extra year of schooling on Wages and Earnings (Grouping into Cohort-Age Cells)

	First Stage: Effect of Law on Age Left School			Reduced Form: Effect of Law on Wages/Earnings			TS2SLS: Effect of Schooling on Wages/Earnings		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hourly Wages									
Men	.369*	.369*	.360*	.021*	.021*	.020*	.056*	.057*	.056*
	(.054)	(.054)	(.055)	(.004)	(.004)	(.005)	(.011)	(.012)	(.014)
Women	.455*	.454*	.451	-.001	.001	.001	-.002	.002	.002
	(.048)	(.047)	(.049)	(.007)	(.007)	(.007)	(.015)	(.014)	(.015)
Weekly Earnings									
Men	.369*	.369*	.360*	.020*	.020*	.019*	.055*	.054*	.052*
	(.054)	(.054)	(.055)	(.004)	(.005)	(.005)	(.012)	(.012)	(.015)
Women	.455*	.454*	.451	.005	.004	.004	.010	.010	.010
	(.048)	(.047)	(.049)	(.013)	(.013)	(.013)	(.028)	(.028)	(.028)
Birth Cohort Controls	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic
Age Controls	None	Quadratic	Age Dummies	None	Quadratic	Age Dummies	None	Quadratic	Age Dummies

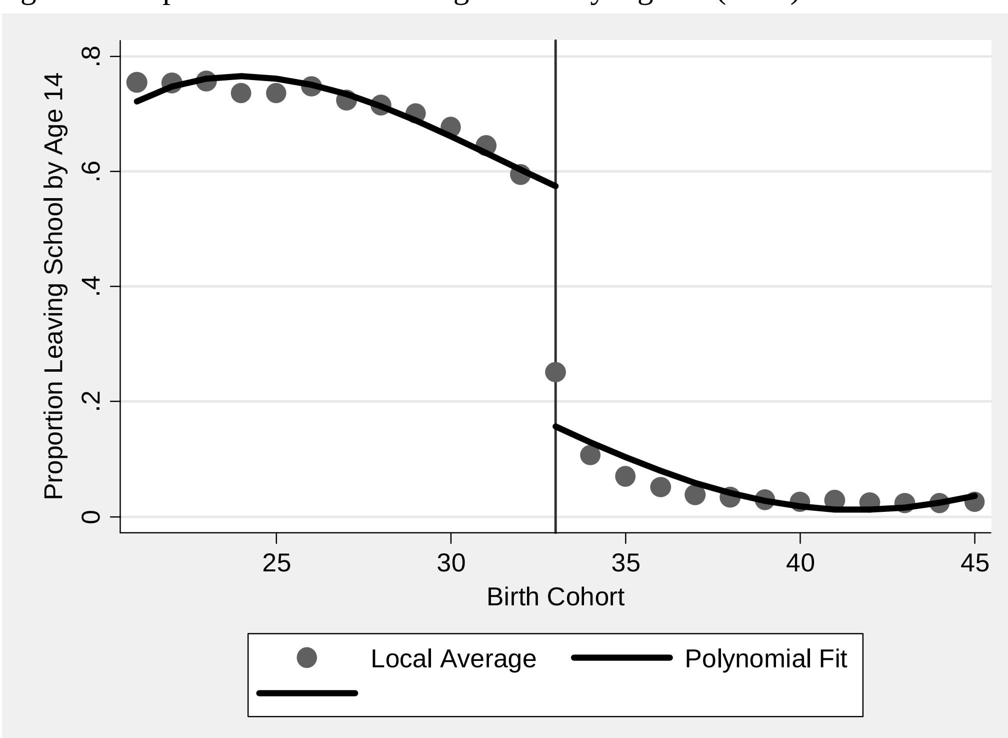
First stage regressions are based on 380 cohort-age cells for men and 381 cohort-age cells for women from the GHS. Reduced form estimates are based on 485 cohort-age cells for both genders from the NESPD. All regressions are weighted by cell size.
TS2SLS is Two Sample Two Stage Least Squares.
Standard errors are clustered by birth cohort so there are 25 clusters.
* denotes statistically significant at the 5% level.
There are 33978 male observations and 29567 female observations used in the GHS. There are 718120 male observations (on 81461 men) and 501278 female observations (on 64422 women) used in the NESPD.

Figure 1: Proportion of Women Leaving School by Age 14 (GHS)



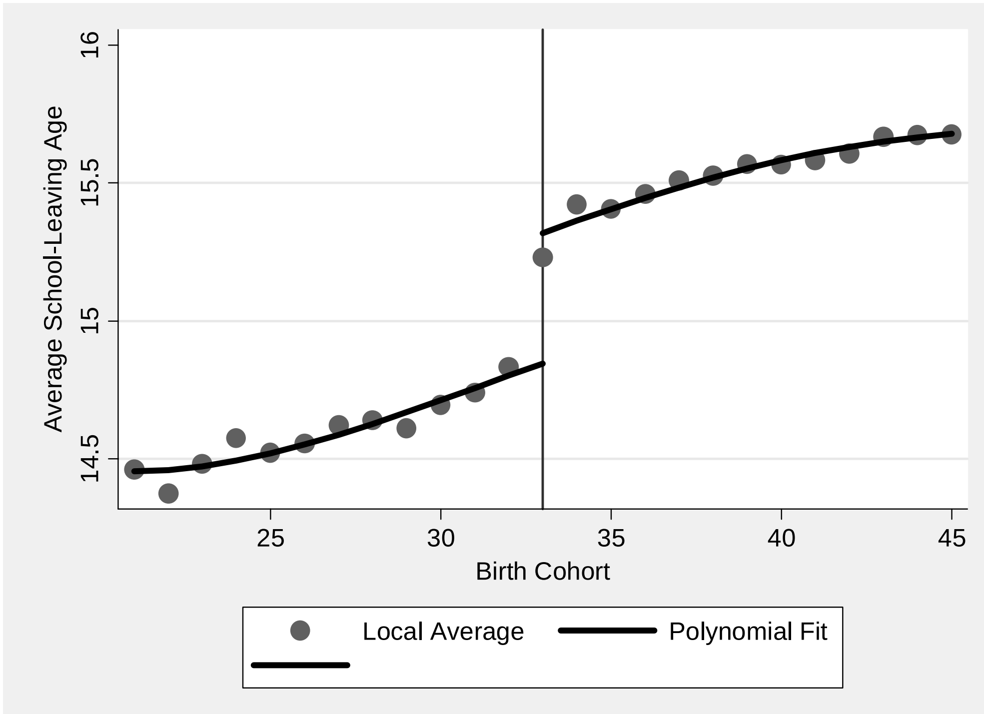
Vertical Line indicates change in school leaving age from 14 to 15

Figure 2: Proportion of Men Leaving School by Age 14 (GHS)



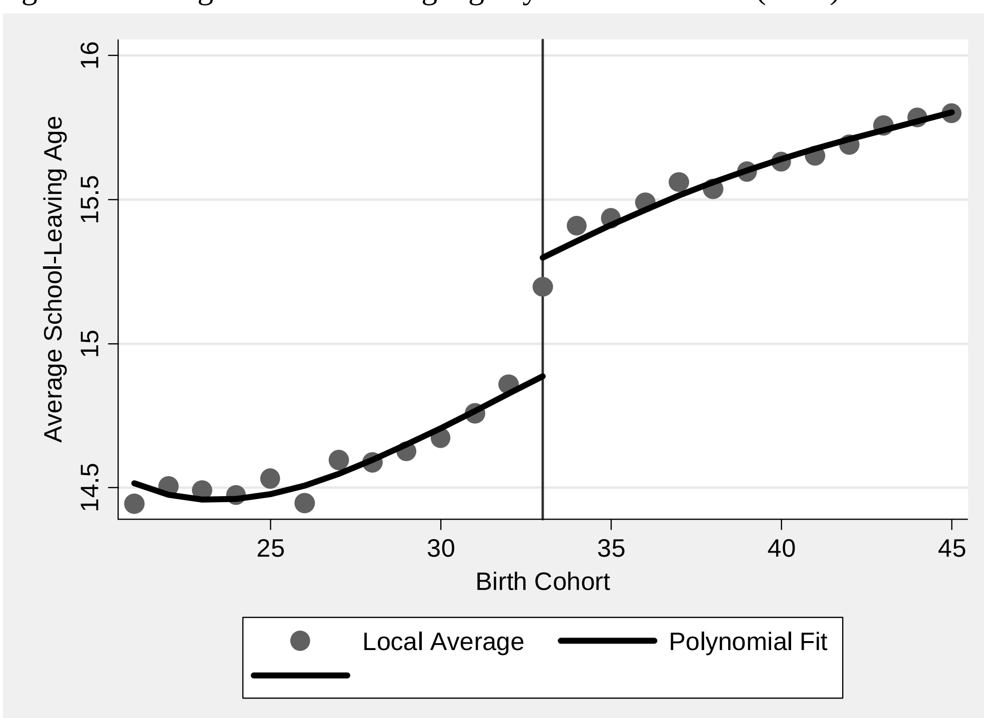
Vertical Line indicates change in school leaving age from 14 to 15

Figure 3: Average School Leaving Age by Cohort for Women (GHS)



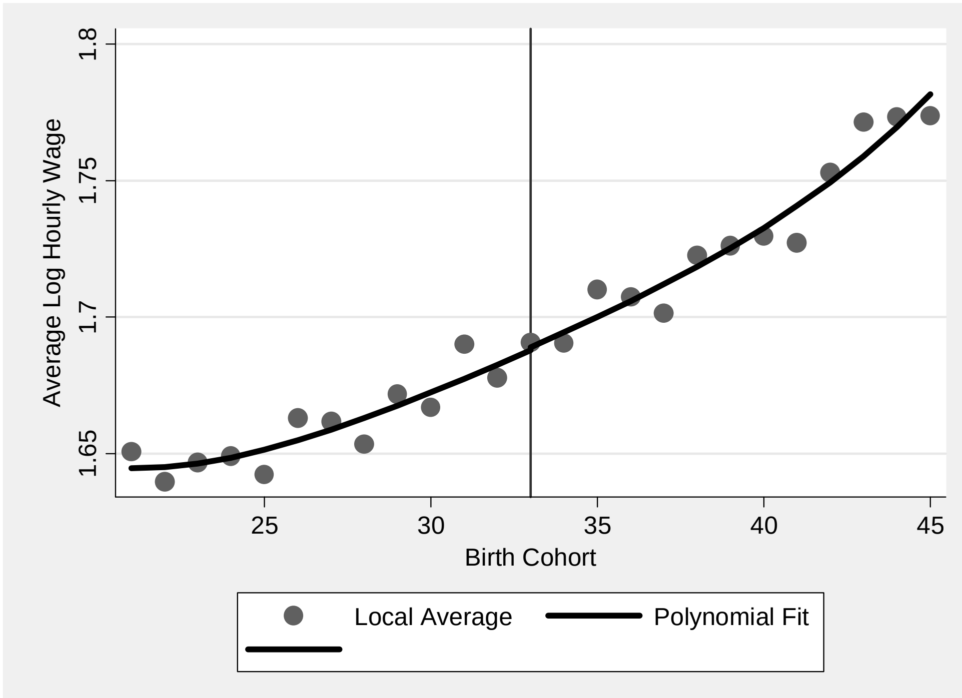
Vertical Line indicates change in school leaving age from 14 to 15

Figure 4: Average School Leaving Age by Cohort for Men (GHS)



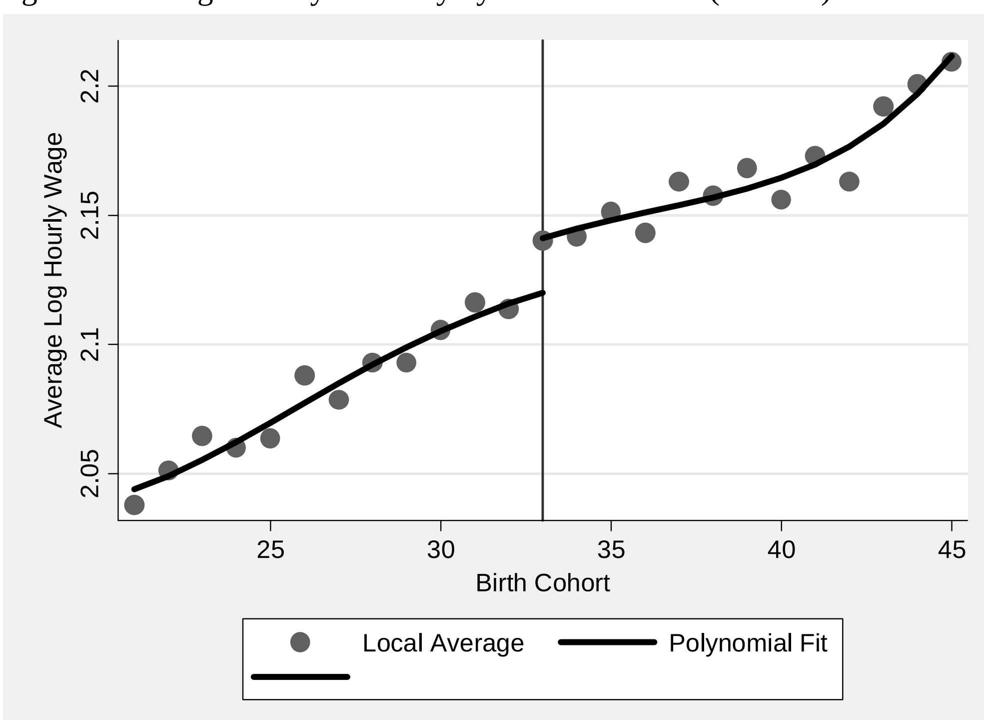
Vertical Line indicates change in school leaving age from 14 to 15

Figure 5: Average Hourly Base Pay by Cohort for Women (NESPD)



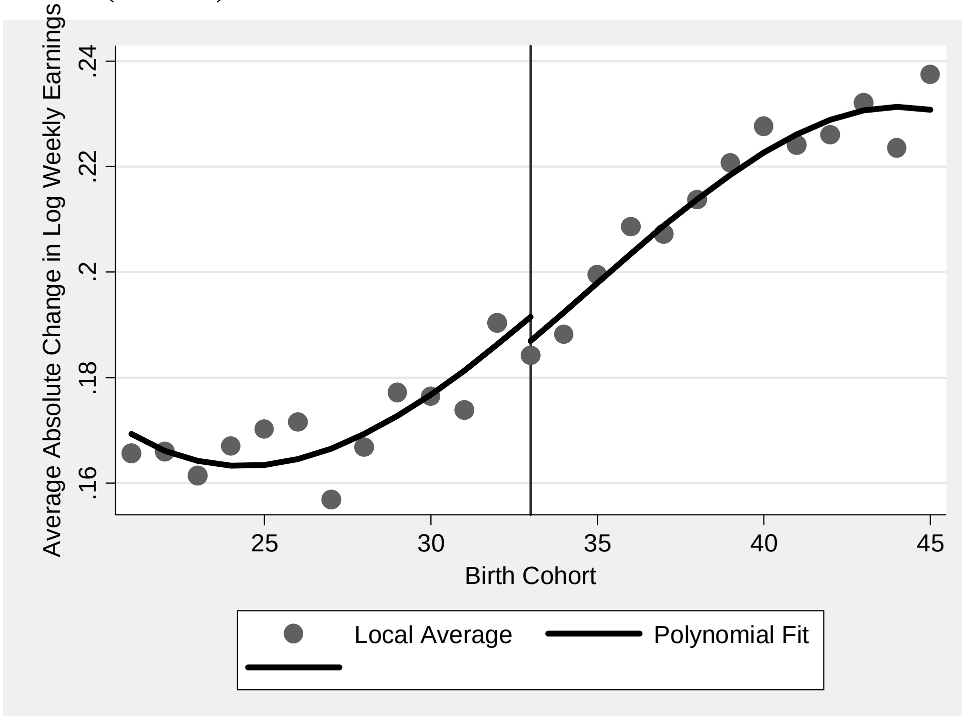
Vertical Line indicates change in school leaving age from 14 to 15

Figure 6: Average Hourly Base Pay by Cohort for Men (NESPD)



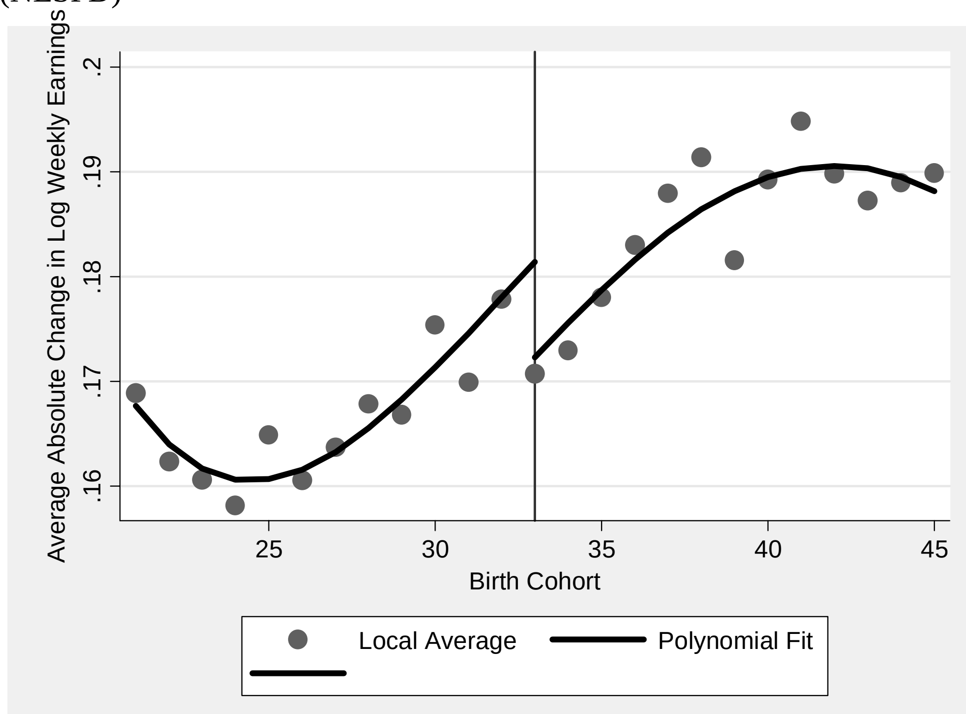
Vertical Line indicates change in school leaving age from 14 to 15.

Figure 7: Average Absolute Value of Change in Log Weekly Earnings by Cohort for Women (NESPD)



Vertical Line indicates change in school leaving age from 14 to 15.

Figure 8: Average Absolute Value of Change in Log Weekly Earnings by Cohort for Men (NESPD)



Vertical Line indicates change in school leaving age from 14 to 15.