



Barcelona School of Economics

Assignment 7

Econometrics

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1 Question 1: Replication of Figures 4 and 6

1.1 Figure 4: Education Attainment

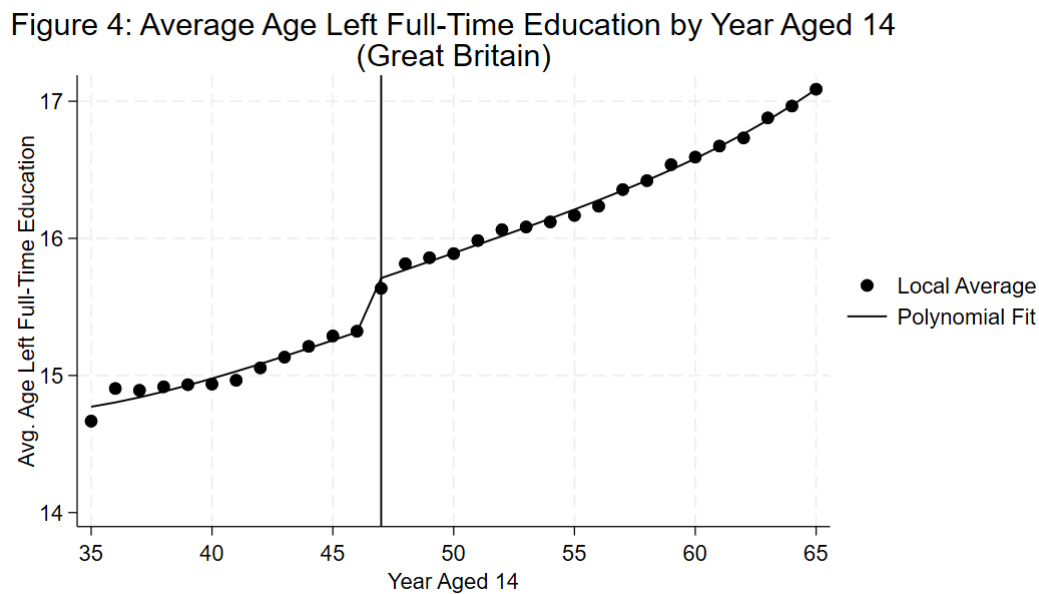


Figure 1: Average Age Left Full-Time Education by Year Aged 14 (Great Britain)
The figure shows local averages (dots) and a quartic polynomial fit (line). The vertical line indicates 1947, when the minimum school-leaving age increased from 14 to 15.

1.2 Figure 6: Earnings

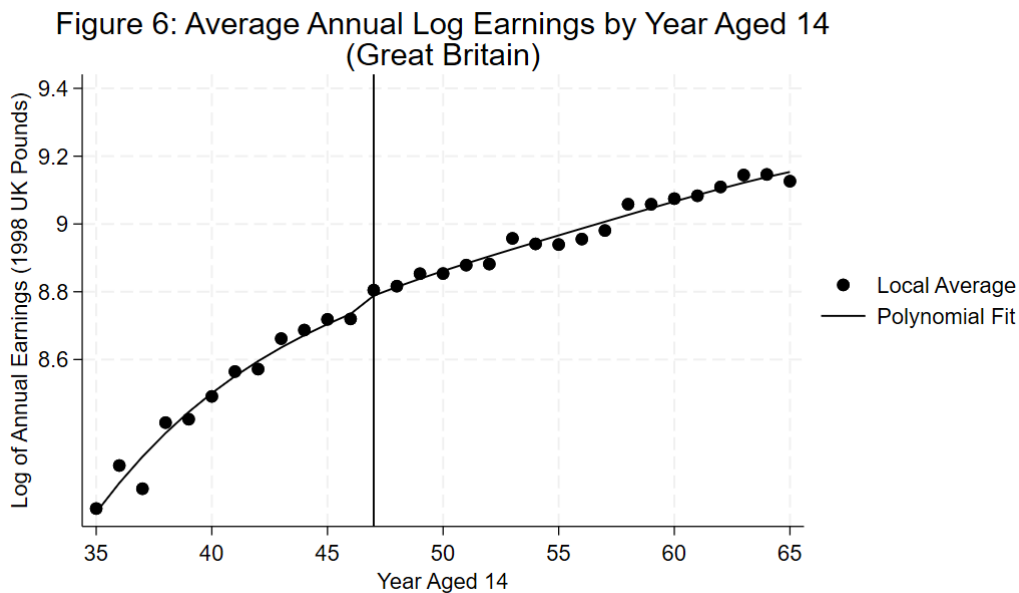


Figure 2: Average Annual Log Earnings by Year Aged 14 (Great Britain)

The figure shows local averages (dots) and a quartic polynomial fit (line). Earnings are measured in 1998 UK pounds. The vertical line indicates 1947, when the minimum school-leaving age increased from 14 to 15.

Table 1: RD–IV Estimates for Great Britain (Ages 25–64)

	(4) Cohort quartic	(5) Cohort + age quartic	(6) Cohort + age FE
Dependent variable: <code>log(earnings)</code>			
Age left full-time education (<code>agelfted</code>)	0.1108 (0.0328)***	0.1147 (0.0330)***	0.1191 (0.0393)***
Observations		13,118	
Clusters (birth cohorts)		31	

Notes: 2SLS estimates. `agelfted` instrumented with `drop15`. Weighted by `wght`. Cohort polynomials centered at their midpoint. Standard errors clustered by birth cohort. *** $p < 0.01$.

Interpretation of RD–IV Estimates

The results show that the compulsory schooling reform that raised the minimum school-leaving age from 14 to 15 led to substantial increases in earnings for the affected cohorts.

Across all specifications, the IV coefficient on `agelfted` lies between 0.1108 and 0.1191, implying that an additional year of schooling increases annual earnings by approximately 11.7–12.6%, using the exact transformation $100(\exp(\hat{\beta}) - 1)$.

Column (4): Baseline specification. With only a quartic in birth cohort as the control function, the estimated return to an additional year of education is 0.1108 (SE = 0.0328). This estimate implies an 11.72% earnings increase and is precisely estimated. Because identification arises from variation at the cohort cutoff corresponding to the reform, the estimate reflects the return for individuals whose schooling decisions were altered by the legal change.

Column (5): Adding flexible age variation. Including a quartic polynomial in age produces a nearly identical coefficient (0.1147), corresponding to a 12.15% effect. The similarity to Column (4) indicates that differential age profiles across cohorts do not materially affect the estimated impact of schooling. The stability of the coefficient suggests that age-related earnings gradients are not confounding the schooling effect.

Column (6): Replacing the age polynomial with age fixed effects. Using age dummies absorbs all age-related wage variation nonparametrically. The resulting estimate, 0.1191 (12.65%), again remains close to the previous columns. Although this specification is more flexible, it produces only a modest increase in the point estimate, and the standard error grows slightly due to the larger number of parameters. The persistence of a 12% earnings return across such different age controls reinforces the robustness of the result.

Overall assessment. All three IV estimates are tightly clustered around 0.11–0.12 and statistically significant at the 1% level. The consistency across specifications implies that neither cohort trends nor age-related earnings patterns drive the observed effects. Instead, the results provide strong evidence that the additional compulsory year of schooling generated by the reform substantially increased adult earnings. The magnitudes are large relative to typical cross-sectional estimates, which is consistent with the interpretation that compulsory schooling reforms identify returns for individuals at the lower end of the schooling distribution, where marginal returns are often higher.

In summary, the RD–IV evidence for Great Britain indicates that the 1947 school-leaving age reform produced sizeable and robust gains in adult earnings, with estimated returns to schooling of approximately 12% per additional year for the affected cohorts.

2 Replication of Table 1 (Great Britain)

2.1 Results

Table 2: Replication of Table 1 for Great Britain

	(1)	(2)	(3)	(4)	(5)	(6)
	FS: YOB poly	FS: +Age poly	FS: Age FE	RF: YOB poly	RF: +Age poly	RF: Age FE
drop15	0.469*** (0.067)	0.469*** (0.066)	0.475*** (0.065)	0.055*** (0.015)	0.052*** (0.014)	0.056*** (0.017)
Observations	22574	22574	22574	13118	13118	13118

Standard errors in parentheses

*** $p < 0.01$

Table 1 presents the effect of the compulsory schooling law raising the minimum leaving age to 15 in Great Britain. The first-stage regressions indicate that being subject to the reform (**drop15**) increased the age of leaving school by approximately 0.47 years across specifications (columns 1–3), with standard errors around 0.065–0.067, confirming a strong instrument. The reduced-form regressions show that the reform raised log annual earnings by about 5–5.6% (columns 4–6), with standard errors between 0.014 and 0.017. These findings are robust to different model controls, including year-of-birth quartic polynomials, age quartic polynomials, and age fixed effects. The implied local average treatment effect (LATE), computed as the ratio of the reduced-form to the first-stage coefficients, suggests that one additional year of schooling due to the reform increased earnings by roughly 11–12% for compliers, highlighting the economically meaningful impact of the policy. The stability of the results across specifications suggests that the estimated effects are not driven by specific functional form assumptions and provide credible evidence that raising the minimum school-leaving age had meaningful long-term benefits in terms of earnings.

3 RD-IV Estimates (Table 2, Great Britain)

Table 3: TABLE 2 – RD-IV ESTIMATES: RETURNS TO (COMPULSORY) SCHOOLING FOR LOG ANNUAL EARNINGS (Great Britain, ages 25–64)

	(4)	(5)	(6)
	Quartic cohort	Quartic cohort + age quartic	Quartic cohort + age dummies
Age left full-time education (agelfted)	0.1108 [0.0328]***	0.1147 [0.0330]***	0.1191 [0.0393]***
Cohort polynomial controls	Quartic	Quartic	Quartic
Age controls	None	Quartic	Dummies
Observations		13,118	
Clusters (birth cohorts)		31	

Notes: The dependent variable is log annual earnings. Each regression is estimated by 2SLS, where **agelfted** (age left full-time education) is instrumented by **drop15** (an indicator that the cohort faced a minimum school-leaving age of 15 when age 14). Regressions use individual-level observations, are weighted by the survey/cell weight **wght**, and standard errors are clustered by birth cohort. Standard errors are in brackets. Significance: *** $p < 0.01$.

RD–IV estimates and interpretation

The table above presents our RD–IV estimates of the causal return to one additional year of (full-time) schooling for Great Britain. The three columns replicate the specifications in Table 2 of Oreopoulos (2006) corresponding to (4) a baseline specification with a quartic in birth cohort, (5) the same plus an age quartic, and (6) the same plus age fixed effects (age dummies).

Point estimates and economic interpretation. The IV estimates of the effect of schooling on log annual earnings are:

- Column (4): $\hat{\beta}_{IV} = 0.1108$ (SE = 0.0328) — implies an **11.72%** increase in annual earnings per additional year of schooling (exact percent: $100(e^{0.1108} - 1) = 11.717\%$).
- Column (5): $\hat{\beta}_{IV} = 0.1147$ (SE = 0.0330) — implies **12.15%** increase ($100(e^{0.1147} - 1) = 12.154\%$).
- Column (6): $\hat{\beta}_{IV} = 0.1191$ (SE = 0.0393) — implies **12.65%** increase ($100(e^{0.1191} - 1) = 12.648\%$).

All three estimates are statistically significant at the 1% level in our estimation. The magnitudes indicate economically large returns to an extra year of schooling induced by the compulsory school-leaving reform.

Identification and instrument validity. Identification exploits the exogenous increase in the compulsory school-leaving age (the 1947 reform that raised the legal leaving age from 14 to 15) as an instrument. The key identifying assumptions are:

1. **Relevance:** The instrument (`drop15`) must strongly predict years of schooling. The first-stage regression confirms a positive and statistically significant effect of `drop15` on `agelfted`.
2. **Exogeneity / exclusion:** Conditional on the cohort polynomial and age controls, the only channel through which `drop15` affects earnings is via schooling.

Under these assumptions, the 2SLS coefficients are a Local Average Treatment Effect (LATE): they measure the return to education for *compliers* — individuals whose schooling decision was changed by the reform.

Robustness and specification differences. The three columns show the estimates are robust to adding flexible age controls. Column (5) includes an age quartic and Column (6) controls with age dummies; the IV point estimates remain close (0.11–0.12) across these specifications, suggesting the result is not driven by a particular choice of age control. This increases confidence that the estimated effect captures a causal schooling return rather than an artifact of age or cohort trends.

Caveats and remarks.

- The estimate is a LATE for the cohorts affected by the reform (those induced to stay in school because the legal leaving age increased). It may not equal the average return to schooling for other groups or in other contexts.
- The exact numerical values differ from Oreopoulos (2006), nevertheless, the magnitude and statistical significance are comparable and point to a strongly positive return to compulsory schooling in this historical setting.

Conclusion. The RD–IV estimates for Great Britain indicate that one additional year of (compulsory) schooling induced by the 1947 reform raises annual earnings by about 11–13% for the affected cohorts. Estimates are stable across the three specifications, supporting a robust positive effect of increased compulsory schooling on earnings.