

Project Module papers

Winter 2019

These notes will span the collection of papers focused on human capital and inter-generational mobility.

1 Life Cycle Earnings, Education Premiums and Internal Rates of Return¹

- **Question:** Tackles three fundamental questions in economics:
 1. What do the education premiums look like over the life cycle?
 2. What is the impact on schooling on earnings over time?
 3. How does the internal rate of return (IRR) compare with the opportunity cost of funds? And does considering progressive taxes lessen the incentive to invest in education?
- **Data:** Only Males. 1943-1963 birth cohorts. Norwegian Panel Data 1967-2010 merged with 1960 census data. Public domain. Linking child to parent possible. Schooling measured until age 40. three measures: (1) Earnings over the life-cycle (discretized) (2) IRR (3) Lifetime Education Premium (mean lifetime earnings)
- **Methods:** OLS, IV
- **Identification Strategies**
 1. compulsory schooling reform as an instrument for education (it is exogenous, thus extremely useful BUT it just increases **required** schooling from 7 to 9 years)
 2. Controlling of course for ability test scores to account for selection bias ('more able' students will get more schooling).
 3. Within-twin-pair estimation (to control for environment/genetics)
- **Conclusion:** Additional Schooling gives higher lifetime earnings and steeper age-earnings profile. IRR around 10% after considering taxes and earnings related to entitlements.

1.1 Notes

- **Mincer Regression:** Assumes *exogenous* variation in schooling, quadratic potential experience in explaining log earnings. Mincer also assumes college students do not have any earnings and post-schooling employment is exogenous. (both relaxed here to compare estimates and make bounds).
- **Education Premium:** Interpreting the Mincer regression as the price to pay for a laborer given a certain education level, we can interpret the schooling's coefficient as the *education premium*. With respect to the paper, this is done at each age. They consider the education premium's mean and present value. The IRR is rate ρ which satisfies the typical PV problem:

$$\sum_{a=17}^{62} \frac{\beta_a}{(1 + \rho)^{a-16}} = 0$$

They compare this rate to the real-interest rate (proxy for opportunity cost). Thus, trying to answer the question: Is it more profitable to invest in education or invest in the market at the inflation-adjusted rate? Furthermore, they use the **ordinary annuity** expression to compute the lifetime education premium. The **average real interest rate of 2.3%** was used for 1967-2010 (should another rate be considered?)

- **First Identification Strategy - IV:** Since schooling is endogenous, we seek to extract only the exogenous portion from the first stage regression. They instrument using a boolean variable for compulsory schooling. Estimation results from equation (7) show a strong first stage with an estimated coefficient on the instrument of 0.213. This means that exposure to the compulsory schooling reform increased years of schooling by about one-fth of a year. The F-statistic for the instrument is around 93, implying that weak instrument bias is not a concern for our analysis.
- **Second Identification Strategy - Controlling for ability:** The second method controls for ability to mitigate selection bias. **The exam:** The arithmetic test mirrors the test in the Wechsler Adult Intelligence Scale (WAIS); the word test is similar to the vocabulary test in WAIS; and the gures test is comparable to the Raven Progressive Matrix test. They use **test score indicators**. Problem: only those that tried for the military took the exam (for 1950 or later).
- **Third Identification Strategy - Within-twin-pair estimation:** To control for environment and genetics - used as a fixed effect.
- **Unbalanced Panel:** When the number of time periods are *not* the same for all individuals. They wish to study those born in 1943-1963 during the period 1967-2010 for ages 17 to 62. Problem:
 1. The person born in 1963 at most will be 47, thus we don't know what happens between 48-62. A difference exists if the cohort was born

after 1949. A robustness check (using a balanced panel) is done to reassure the results.

• **Table 2 and Table 3:** The main results show the following:

1. IRR robust to the specification. Between 10%-14%
2. Returns to education negative for 17-24 year olds (since they are giving up earnings compared to those going straight to work). Returns increase (still negative) when considering those in the workforce having to pay taxes on their wages.
3. For ages 25-44, positive, but again for the IV estimate, really noisy (standard errors 15 times larger than that of the twins estimation). This is typically a sign of weak instruments, but the F-stat, t-stat and the fact that the instrument is exogenous helps.
4. For ages 45-62, IV returns no difference and again high standard errors.
5. All estimates range between 2% and 9% of the mean.
6. **Table 3** shows that the IRR results are robust after considering taxes and pension income considering all ages. Looking at Figure 5, we can see that taxes decreases the negative effect of going to college since those who did not go to college and chose to work, must pay taxes on their earnings. The pension entitlements were estimated based on earnings information.
7. As part of the sensitivity analysis, they had to make sure that cohort trends in earnings were unrelated to the reform. In other words, that nothing else made this earnings ‘jump’. This is proven by creating a specification where they use interaction terms for years and municipality characteristics (plus the fixed effects) to explain the timing of the reform (e.g. poor municipalities would be the first to implement the reform. No evidence of such a relationship was found.
8. Controlling for pre-reform trends, the IRR stays sizeable.
9. In considering different ways in treating the missing values, whether by imputing zeros or past earnings, results are robust and considering a balanced panel also rendered consistent results.
10. Mincer Regression estimates are lower because of the restriction of no earnings while in school and exogenous post schooling employment accounts also for the downward bias.

2 Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital²

- **Question:** What goes into the composition of the variation when trying to explain the relationship between the education levels parents attain compared to their children's education levels? Is it mostly family characteristics and inherited ability (selection theory explanation) or education spillovers (being educated has implications on parenting and leads to your children having more educational outcomes)?
- **Significance:** If it is the case that education spillovers exist, education policy seems to be the recourse. Thus, this paper proposes to provide evidence on the *causation* argument and relieve some questions on the intergenerational transmission of education.
- **Data:** Norwegian data and children data from 2000 aged 20-35. Census data from 1960, 1970, 1980. Educational attainment from Statistics Norway. Cohorts of parents born between 1947 and 1958. Analysis mostly focused on parents with less than 9 years of schooling.
- **Methods:** OLS, IV
- **Identification Strategies:**
 1. They instrument for *parental* education using the compulsory schooling reform (1960s Norway) since it is exogenous to parental ability.
 2. They make two assumptions:
 - (a) That individuals who get 9 years of education after the reform would have gotten less than or equal to 9 before the reform.
 - (b) Those who got less than 9 years of education before the reform, get 9 years of education after the reform.
- **Conclusion:** There is little evidence of a causal relationship between the father's education and the children's education. A mother and son comparison generates a weakly significant result, but no causal relationship between mother's and daughter's educations.

2.1 Notes

- They checked for significant determinants with respect to the timing of the reform (as did Paper 1) such as average earnings of the people, taxable income, size of the municipality, unemployment rates, urbanization, labor force composition, and education levels and found no relationship.

- Their estimation uses parental education, children age, sex, and fixed effects for parents' age and parents' municipality. The fixed effect for parents' age is their to control for secular (persisting nearly indefinitely) changes in educational attainment over time that may be completely unrelated to the reform. The municipality effects control for variation in the timing of the reform across municipalities that may not have been exogenous to parents' educational choice like them seeking higher education because their municipality required highly skilled workers. As long as we fix these characteristics, make sure they are not correlated with the timing of the reform nor the schooling of the children of this generation.

- **Small things to be aware of**

1. They assume in cases of one-parent households that the child lived with the mom. This comes up because the mother's parent place of residence can be found in the 1960 census. Thus, they make the 'safe' assumption that the child lived their too (this is of course relative to the epoch).
 2. **Selective Migration:** There could be cases where the parents anticipated a change in another municipality and moved there. This affects the estimates, but not the consistency - thus with large enough sample size, the estimate is fine.
- Looking at the distribution of parents and their education levels plus minus 2 years from the reform shows that the *cumulative* distribution of people getting ≤ 9 years of education is constant. Thus, the reform itself does not have spillover effects.
 - Looking at family characteristics, there is no evidence of compositional change.

- **Results**

1. OLS - positive and significant results (5% level) for discrete and Boolean education DV.
2. 2SLS - insignificant, large s.e.'s, but this is due to the focus on all parents and not those in the bottom of the distribution where the reform has 'more bite'. The instrument is weak and the t-stat, although showing variation exists between the reform and parental education, is mostly minor at the higher education levels.
3. Focusing on the sample of parents who received less than 10 years of education, 2SLS shows mother's education has an impact over on child's education level and son. Standard errors decreased after subsetting, this the estimate is more precise.
4. Overall, results indicate that the positive correlation between parent and child and their education levels mostly comes from other positively correlated factors with education like ability, family background, income or other factors. The causal effect is weak.

5. In the literature, high IV estimates are rationalized by heterogeneous returns to education (a particular group whose behavior is impacted by the instrument i.e. child goes to school longer).
 6. To control for the fact that educated women marry educated men, they control for father's education and thus, both parents, in explaining child's educational attainment. They also only include cases where at least 1 parents attained less than 10 years of education. IV estimates still weak.
- **Robustness/Specification Checks:** (1) Municipality-specific time trends, county cohort fixed effects (2) In case they were inaccurate in selecting the reform year, they removed data for that year, and the years preceding it and following it. (3) Sample selection from 20-35 to 25-35 years old. The initial reason for 20-35 comes from the fact that less educated parents have less educated children. Thus, since the reform hits the lower distribution, the children were expected to be finished by 20 years of age. To be sure they are finished, they consider 25-35 years. Still, significant, positive result for mother/son, but nothing else. (4) Subsetting to the highly educated rendered nothing. (5) Conditional on human capital, high able women are less likely to have children. Thus, when the reform hits, some woman will experience an increase in human capital. These women will not have children and the group of women left hit by the reform having children have **less ability** than those mothers not hit by the reform. To check whether the reform increases one's probability to have children, they run a linear probability model with the DV equal to 1 if parent has child and 0 if not. Not significant; but, could also use probit/logit. (6) Sibling fixed effects. This is supposed to capture some unobserved ability. In the first stage regression, if unobserved ability is positively correlated with parental education, then of course the OLS estimates will decrease with including the covariate. First, they consider the sample of parents who have brothers or sisters and seeing how they impact the same-sex child. The results, as hypothesized, reduce the estimates in the OLS regression; implying the OLS estimates are upward biased by unobserved ability.
 - **Exploring the mechanisms for explaining why mother's education affects child's education:** (1) Children tend to spend more time with their mother. Naturally, extra schooling for the mothers increases their own human capital, thus their optimal human capital *choice* of children. (2) To see if there exists a relationship between mother's education and father's education (testing the hypothesis that higher educated women marry highly educated men), they ran a regression using the reform as an instrument for mother's education and again, nothing. This implies that the greater education induced by the reform did not have any major effect on the type of father chosen by the mother. (8) To test whether women with more education have less children and invest more in each of them, testing the quality/quantity trade off, they see whether increasing

mother's education has an effect on the number of children they have. There is a lack of evidence on this (except for mother's on the lower end of the educational attainment distribution

References

- [1] Manudeep Bhuller, Magne Mogstad, and Kjell G Salvanes. "Life-cycle earnings, education premiums, and internal rates of return". In: *Journal of Labor Economics* 35.4 (2017), pp. 993–1030.
- [2] Sandra E Black, Paul J Devereux, and Kjell G Salvanes. "Why the apple doesn't fall far: Understanding intergenerational transmission of human capital". In: *American economic review* 95.1 (2005), pp. 437–449.