Returns to Schooling

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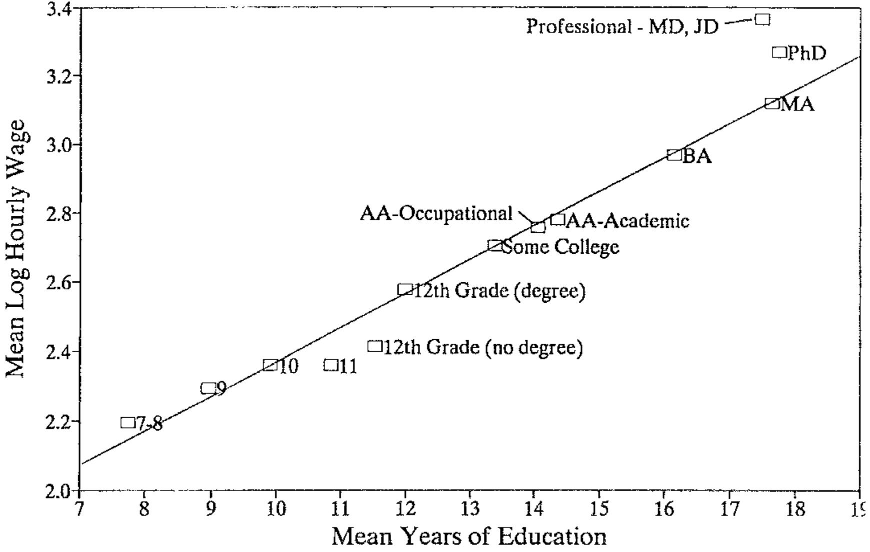
This comes from the Card Handbook Chapter

Lets assume that

$$\log(W_i) = b_0 + \beta S_i + g(X_i) + \theta_i + u_i$$

where W_i is wages, S_i is schooling, X_i is experience, θ_i is unobserved ability, and u_i is other unobservables.

Is schooling Really linear?



Mean Log Hourly Wage
3.4
3.2
3.0
2.8
2.6
2.4
2.2
2.0
7
8
9
10
11
12
13
14
15
16
17
18
Mean Years of Education
Professional - MD, JD
PhD
MA
BA
AA-Occupational
AA-Academic
Some College
12th Grade (degree)
12th Grade (no degree)
10
11
9
7-8

The figure shows that without controlling for ability bias, it seems to be pretty close.

There is a literature on this and there are papers that find evidence of sheepskin effects-but at the very least this is not an unreasonable assumption

We are worried about ability bias we want to use instrumental variables

A good instrument should have two qualities:

- It should be correlated with schooling (S_i)
- It should be uncorrelated with ability (θ_i) as well as other unobservables)

Many different things have been tried. Lets go through some of them

Family Background

If my parents earn quite a bit of money it should be easier for me to borrow for college

Also they might put more value on education

This should make me more likely to go

This has no direct effect on my income-Wisconsin did not ask how much education my Father had when they made my offer

But is family background likely to be uncorrelated with unobserved ability?

Closeness of College

If I have a college in my town it should be much easier to attend college

- I can live at home
- If I live on campus
  - I can travel to college easily
  - I can come home for meals and to get my clothes washed
- I can hang out with my friends from High school

But is this uncorrelated with unobserved ability?

Quarter of Birth

This is the most creative

Consider the following two aspects of the U.S. education system (this actually varies from state to state and across time but ignore that for now),

- People begin Kindergarten in the calender year in which they turn 5
- You must stay in school until you are 16

Now consider kids who:

- Can't stand school and will leave as soon as possible
- Obey truancy law and school age starting law
- Are born on either December 31,1972 or January 1,1973

Those born on December 31 will

- turn 5 in the calender year 1977 and will start school then (at age 4)
- will stop school on their 16th birthday which will be on Dec. 31, 1988
- thus they will stop school during the winter break of 11th grade

Those born on January 1 will

- turn 5 in the calender year 1978 and will start school then (at age 5)
- will stop school on their 16th birthday which will be on Jan. 1, 1989
- thus they will stop school during the winter break of 10th grade

The instrument is a dummy variable for whether you are born on Dec. 31 or Jan 1

This is pretty cool:

- For reasons above it will be correlated with education
- No reason at all to believe that it is correlated with unobserved ability

The Fact that not everyone obeys perfectly is not problematic:

An instrument just needs to be correlated with schooling, it does not have to be perfectly correlated

In practice we can’t just use the day as an instrument, use “quarter of birth” instead

Policy Changes

Another possibility is to use institutional features that affect schooling

Here often institutional features affect one group or one cohort rather than others

TABLE II

OLS AND IV ESTIMATES OF THE RETURN TO EDUCATION WITH INSTRUMENTS BASED ON FEATURES OF THE SCHOOL SYSTEM

Author	Sample and Instrument		Schooling Coefficients OLS	IV
1. Angrist and Krueger (1991)	1970 and 1980 Census Data, Men. Instruments are quarter of birth interacted with year of birth. Controls include quadratic in age and indicators for race, marital status, urban residence.	1920–29 cohort in 1970	0.070 (0.000)	0.101 (0.033)
		1930–39 cohort in 1980	0.063 (0.000)	0.060 (0.030)
		1940–49 cohort in 1980	0.052 (0.000)	0.078 (0.030)
2. Staiger and Stock (1997)	1980 Census, Men. Instruments are quarter of birth interacted with state and year of birth. Controls are same as in Angrist and Krueger, plus indicators for state of birth. LIML estimates.	1930–39 cohort in 1980	0.063 (0.000)	0.098 (0.015)
		1940–49 cohort in 1980	0.052 (0.000)	0.088 (0.018)
3. Kane and Rouse (1993)	NLS Class of 1972, Women. Instruments are tuition at 2 and 4-year state colleges and distance to nearest college. Controls include race, part-time status, experience. Note: Schooling measured in units of college credit equivalents.	Models without test score or parental education	0.080 (0.005)	0.091 (0.033)
		Models with test scores and parental education	0.063 (0.005)	0.094 (0.042)
4. Card (1995b)	NLS Young Men (1966 Cohort) Instrument is an indicator for a nearby 4-year college in 1966, or the interaction of this with parental education. Controls include race, experience (treated as endogenous), region, and parental education	Models that use college proximity as instrument (1976 earnings)	0.073 (0.004)	0.132 (0.049)
		Models that use college proximity × family background as instrument	—	0.097 (0.048)

5. Conneely and Uusitalo (1997)	Finnish men who served in the army in 1982, and were working full time in civilian jobs in 1994. Administrative earnings and education data. Instrument is living in university town in 1980. Controls include quadratic in experience and parental education and earnings.	Models that exclude parental education and earnings	0.085 (0.001)	0.110 (0.024)
		Models that include parental education and earnings	0.083 (0.001)	0.098 (0.035)
6. Harmon and Walker (1995)	British Family Expenditure Survey 1978–86 (men). Instruments are indicators for changes in the minimum school leaving age in 1947 and 1973. Controls include quadratic in age, survey year, and region.		0.061 (0.001)	0.153 (0.015)
7. Ichino and Winter-Ebmer (1998)	Austria: 1983 Census, men born before 1946. Germany: 1986 GSOEP for adult men. Instrument is indicator for 1930–35 cohort. (Second German IV also uses dummy for father's veteran status). Controls include age, unemployment rate at age 14, and father's education (Germany only). Education measure is dummy for high school or more.	Austrian Men	0.518 (0.015)	0.947 (0.343)
		German Men	0.289 (0.031)	0.590/0.708 (0.844) (0.279)
8. Lemieux and Card (1998)	Canadian Census, 1971 and 1981: French-speaking men in Quebec and English-speaking in Ontario. Instrument is dummy for Ontario men age 19–22 in 1946. Controls include full set of experience dummies and Quebec-specific cubic experience profile.	1971 Census:	0.070 (0.002)	0.164 (0.053)
		1981 Census:	0.062 (0.001)	0.076 (0.022)
9. Meghir and Palme (1999)	Swedish Level of Living Survey (SLLS) data for men born 1945–55, with earnings in 1991, and Individual Statistics (IS) sample of men born in 1948 and 1953, with earnings in 1993. Instrument is dummy for attending "reformed" school system at age 13. Other controls include cohort, father's education, and county dummies. Models for IS data also include test scores at age 13.	SLLS Data (Years of education)	0.028 (0.007)	0.036 (0.021)
		IS Data (Dummy for 1–2 years of college relative to minimum schooling)	0.222 (0.020)	0.245 (0.082)

TABLE II—*Continued*

Author	Sample and Instrument		Schooling Coefficients OLS	Schooling Coefficients IV
10. Maluccio (1997)	Bicol Multipurpose Survey (rural Philippines): male and female wage earners age 20–44 in 1994, whose families were interviewed in 1978. Instruments are distance to nearest high school and indicator for local private high school. Controls include quadratic in age and indicators for gender and residence in a rural community.	Models that do not control for selection of employment status or location	0.073 (0.011)	0.145 (0.041)
		Models with selection correction for location and employment status	0.063 (0.006)	0.113 (0.033)
11. Duflo (1999)	1995 Intercensal Survey of Indonesia: men born 1950–72. Instruments are interactions of birth year and targeted level of school building activity in region of birth. Other controls are dummies for year and region of birth and interactions of year of birth and child population in region of birth. Second IV adds controls for year of birth interacted with regional enrollment rate and presence of water and sanitation programs in region.	Model for hourly wage	0.078 (0.001)	0.064/0.091 (0.025) (0.023)
		Model for monthly wage with imputation for self-employed.	0.057 (0.003)	0.064/0.049 (0.017) (0.013)

Notes: See text for sources and more information on individual studies.

Consistently IV estimates are higher than OLS

Why?

- Bad Instruments
- Ability Bias
- Measurement Error
- Publication Bias
- Discount Rate Bias

Discount Rate Bias

This is a simplified version of it (and my version of it)

Lang and Card explain it somewhat differently

Suppose 2 levels of schooling and 2 values of instrument

$$S_i = \begin{cases} 0 & \text{High School} \\ 1 & \text{College} \end{cases}$$

$$Z_i = \begin{cases} 1 & \text{with probability}\rho \\ 0 & \text{with probability}1-\rho \end{cases}$$

$$\log(W_i) = \theta_i + \beta_i S_i + u_i$$

$E(u_i) = 0$ and is uncorrelated with S_i

S_i is potentially correlated with (θ_i, β_i)

Suppose that we have an instrument Z_i which is correlated with S_i but not with (θ_i, β_i, u_i)

$$E(W_i \mid Z_i) = E(\theta_i \mid Z_i) + E(\beta_i S_i \mid Z_i)$$

If $\beta_i = \beta_0$ so it is constant for everyone

$$E(W_i \mid Z_i) = E(\theta_i \mid Z_i) + \beta_0 E(S_i \mid Z_i)$$

so IV works

However if β_i varies across persons then in general

$$E(\beta_i S_i \mid Z_i) \neq E(\beta_i) E(S_i \mid Z_i)$$

Local Average Treatment Effects

To see what it converges to I draw on Imbens and Angrist (EMA, 1994)

Imbens and Angrist (1994) consider the case in which there are not constant treatment effects

We need a “first stage” so Z_i has to be correlated with S_i.

Without loss of generality assume that
$Pr(S_i = 1 \mid Z_i = 1) > Pr(S_i = 1 \mid Z_i = 0)$

There are 4 different types of people those for whom $T_i = 1$ when:

1. $Z_i = 1, Z_i = 0$
2. never
3. $Z_i = 1$ only
4. $Z_i = 0$ only

Imbens and Angrist's monotonicity rules out 4 as a possibility

Let $\mu_1, \mu_2,$ and μ_3 represent the sample proportions of the three groups

and G_i an indicator of the group

Note that

$$\begin{aligned}
\widehat{\beta}_1 \overset{p}{\to} &= \frac{Cov\left(Z_i, W_i\right)}{Cov(Z_i, S_i})\\
&= \frac{Cov\left(Z_i, \theta_i + \beta_i S_i + u_i\right)}{Cov(Z_i, S_i)}\\
&= \frac{Cov\left(Z_i, \beta_i S_i\right)}{Cov(Z_i, S_i)}\\
&= \frac{E\left(Z_i \beta_i S_i\right) - E\left(\beta_i S_i\right) E\left(Z_i\right)}{E\left(Z_i S_i\right) - E\left(S_i\right) E\left(Z_i\right)}
\end{aligned}$$

Recall that ρ denotes the probability that $Z_i = 1$.

Lets look at the pieces

first the numerator

$$
\begin{aligned}
& E(\theta_i S_i Z_i) - E\left(\theta_i S_i\right) E\left(Z_i\right) \\
= & \rho E(\theta_i S_i \mid Z_i = 1) - E\left(\theta_i S_i\right) \rho \\
= & \rho E(\theta_i S_i \mid Z_i = 1) \\
& - \left[\rho E(\theta_i S_i \mid Z_i = 1) + (1-\rho) E(\theta_i S_i \mid Z_i = 0)\right] \rho \\
= & \rho(1-\rho) \left[E(\theta_i S_i \mid Z_i = 1) - E(\theta_i S_i \mid Z_i = 0)\right] \\
= & \rho(1-\rho) \left[E(\theta_i \mid G_i = 1)\mu_1 + E(\theta_i \mid G_i = 3)\mu_3 - E(\theta_i \mid G_i = 1)\mu_1\right] \\
= & \rho(1-\rho) E(\theta_i \mid G_i = 3)\mu_3
\end{aligned}
$$

Next consider the denominator

$$
\begin{aligned}
& E(S_i Z_i) - E\left(S_i\right) E\left(Z_i\right) \\
=& \rho E(S_i \mid Z_i = 1) - E\left(S_i\right) \rho \\
=& \rho E(S_i \mid Z_i = 1) \\
& - \left[\rho E(S_i \mid Z_i = 1) + (1-\rho) E(S_i \mid Z_i = 0)\right] \rho \\
=& \rho(1-\rho) \left[E(S_i \mid Z_i = 1) - E(S_i \mid Z_i = 0)\right] \\
=& \rho(1-\rho) \left[\mu_1 + \mu_3 - \mu_1\right] \\
=& \rho(1-\rho)\mu_3
\end{aligned}
$$

Thus

$$
\begin{aligned}
\widehat{\beta}_1 \overset{p}{\to} & \frac{\rho(1-\rho)E(\beta_i \mid G_i = 3)\mu_3}{\rho(1-\rho)\mu_3} \\
= & E(\beta_i \mid G_i = 3)
\end{aligned}
$$

They call this the local average treatment effect

Thus $\widehat{\beta}_{IV}$ may be high because $E(\beta_i \mid G_i = 3)$ may be high

There are a number of reasons why this might be the case:

- Borrowing Constraints
- Nonlinearities in schooling

Twins

$$\log(w_{if}) = \theta_f + \beta S_{if} + u_{if}$$

The problem is that θ_f is correlated with S_{if}

We can solve by differencing

$$E\left(\log(w_{if}) - \log(w_{jf})\right) = \beta E\left(S_{if} - S_{jf}\right)$$

Use this to get consistent estimates of β

Table 6
Cross-sectional and within-family differenced estimates of the return to education for twins[a]

Author	Sample and specification		Cross-sectional OLS	Differenced	
				OLS	IV
1. Ashenfelter and Rouse (1998)	1991–1993 Princeton Twins Survey. Identical male and female twins. Controls Basic controls include quadratic	Basic	0.110 (0.010)	0.070 (0.019)	0.088 (0.025)
	in age, gender and race. Added controls include tenure, marital status and union status.	Basic + added controls	0.113 (0.010)	0.078 (0.018)	0.100 (0.023)
2. Rouse (1997)	1991–1995 Princeton Twins Survey. Identical male and female twins. Basic controls as above.		0.105 (0.008)	0.075 (0.017)	0.110 (0.023)
3. Miller et al. (1995)	Australian Twins Register. Identical and fraternal twins. Controls include quadratic in	Identical twins	0.064 (0.002)	0.025 (0.005)	0.048 (0.010)
	age, gender, marital status. Incomes imputed from occupation	Fraternal twins	0.066 (0.002)	0.045 (0.005)	0.074 (0.008)
4. Behrman et al. (1994)	NAS-NRC white male twins born 1917–1927, plus male twins born 1936–1955 from Minnesota Twins	Identical twins	0.071 (0.002)	0.035 (0.005)	0.056 –
	Registry. Controls include quadratic in age[b]	Fraternal twins	0.073 (0.003)	0.057 (0.005)	0.071 –
5. Isacsson (1997)	Swedish same-sex twins with both administrative and survey measures of schooling.	Identical twins	0.049 (0.002)	0.023 (0.004)	0.024 (0.008)
	Controls include sex, marital status, quadratic in age, and residence in a large city[c]	Fraternal twins	0.051 (0.002)	0.040 (0.003)	0.054 (0.006)

Problems:

- Twins aren't a random sample of population (and often not a random sample of twins)
- Need to have variation in $S_{if} - S_{jf}$
- Is θ_{if} really the same for identical twins?

In Willis and Rosen two things affect schooling choices:

- r_i
- ability differences (Roy model style)

These should be the same for both

Suppose that $\theta_{if} \neq \theta_{jf}$

We expect that:

- corr$(S_{if}, \theta_{if}) > 0$
- corr$(S_{if} - S_{jf}, \theta_{if} - \theta_{jf}) > 0$

While most of the variation in θ_{if} may be explained by family effects, it may also me that most of the variation in S_{if} is explained by family effects as well

Since

$$
\begin{aligned}
\beta_{OLS} &= \beta + \frac{cov(S_{if}, \theta_{if})}{var(S_{if})} \\
\beta_{FE} &= \beta + \frac{cov(S_{if} - S_{jf}, \theta_{if} - \theta_{jf})}{var(S_{if} - S_{jf})}
\end{aligned}
$$

If $var(S_{if} - S_{jf})$ is small the bias could be large

It is not clear which has bigger bias