Economics 270B
Ph.D. Development Economics

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University of California, Berkeley

Lecture 3 – February 9, 2015

I. Overview of International Economic Development
Lecture 1: Understanding economic growth and development (1/26)
Lecture 1B: Persistence of historical institutions and shocks
(read during holiday week of 2/16)

Lecture 2: The Psychology of Poverty (2/2)

II. Human Capital in Economic Development
Lectures 3-4: Education (2/9, 2/23)
Lectures 5-7: Health and nutrition (3/2, 3/9, 3/16)

III. Political economy
Lectures 8-9: Democracy, Corruption and Development (3/30, 4/6)
(guest lectures by Prof. Fred Finan)

Lecture 10: Ethnic and Social Divisions (4/13)
Lectures 11-12: The Political Economy of Conflict (4/20, 4/27)

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- Prerequisites: Graduate economic theory, econometrics

- Grading:
  Four referee reports – 40%
  → Report #1 on Schilbach paper due today (2/9)
  → Report #2 on Dizon-Ross paper due in two weeks (2/23)

  Two problem sets – 20%
  Research proposal – 30%
  Class participation – 10%
  No final exam

- All readings are available on bCourses

Any questions?

Lecture 3 outline

(1) Overview of human capital in economic development

-- Baird et al (2009) on measurement error in education micro-data in Kenya

(2) Jensen (2010) on the perceived returns to schooling and the demand for education

(3) Measurement issues in the study of education

-- Krueger and Lindahl (2001): education and growth

(4) Duflo (2001) on the returns to schooling in Indonesia

-- Looking forward: research design issues

(1) Human capital in economic development

- There have been massive increases in literacy and schooling attainment around the world – Africa, Asia, Latin America – during the past 60 years

- Perhaps unexpectedly, at the regional level increased schooling does not line up well with faster economic growth rates, e.g., Sub-Saharan Africa versus South Asia

HDI rank	Human Development Index (HDI)	Life expectancy at birth	Mean years of schooling	Expected years of schooling	Gross national income (GNI) per capita
	Value	(years)	(years)	(years)	(2005 PPP $)
	2012	2012	2010[a]	2011[b]	2012
Regions					
Arab States	0.652	71.0	6.0	10.6	8,317
East Asia and the Pacific	0.683	72.7	7.2	11.8	6,874
Europe and Central Asia	0.771	71.5	10.4	13.7	12,243
Latin America and the Caribbean	0.741	74.7	7.8	13.7	10,300
South Asia	0.558	66.2	4.7	10.2	3,343
Sub-Saharan Africa	0.475	54.9	4.7	9.3	2,010

(1) Human capital in economic development

- There have been massive increases in literacy and schooling attainment around the world – Africa, Asia, Latin America – during the past 60 years
- Perhaps unexpectedly, at the regional level increased schooling does not line up well with faster economic growth rates, e.g., Sub-Saharan Africa versus South Asia
- This is consistent with the view that institutions and technology (“A”) matter more for growth than physical or human capital investments. But in the “short-run” boosting human capital would still increase income levels – and it could change/improve lives in many ways

(1) Human capital in economic development

- This week: what is the return to schooling in less developed countries? And what do people *think* it is?

(1) Human capital in economic development

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- Next lecture: what does the education production function look like? Which inputs lead to more “human capital”?

(1) Human capital in economic development

- Why focus on education?

- In many poor countries, education spending is the largest single recurrent discretionary budget expenditure. E.g., in some African countries it is one third of discretionary expenditures → policy importance

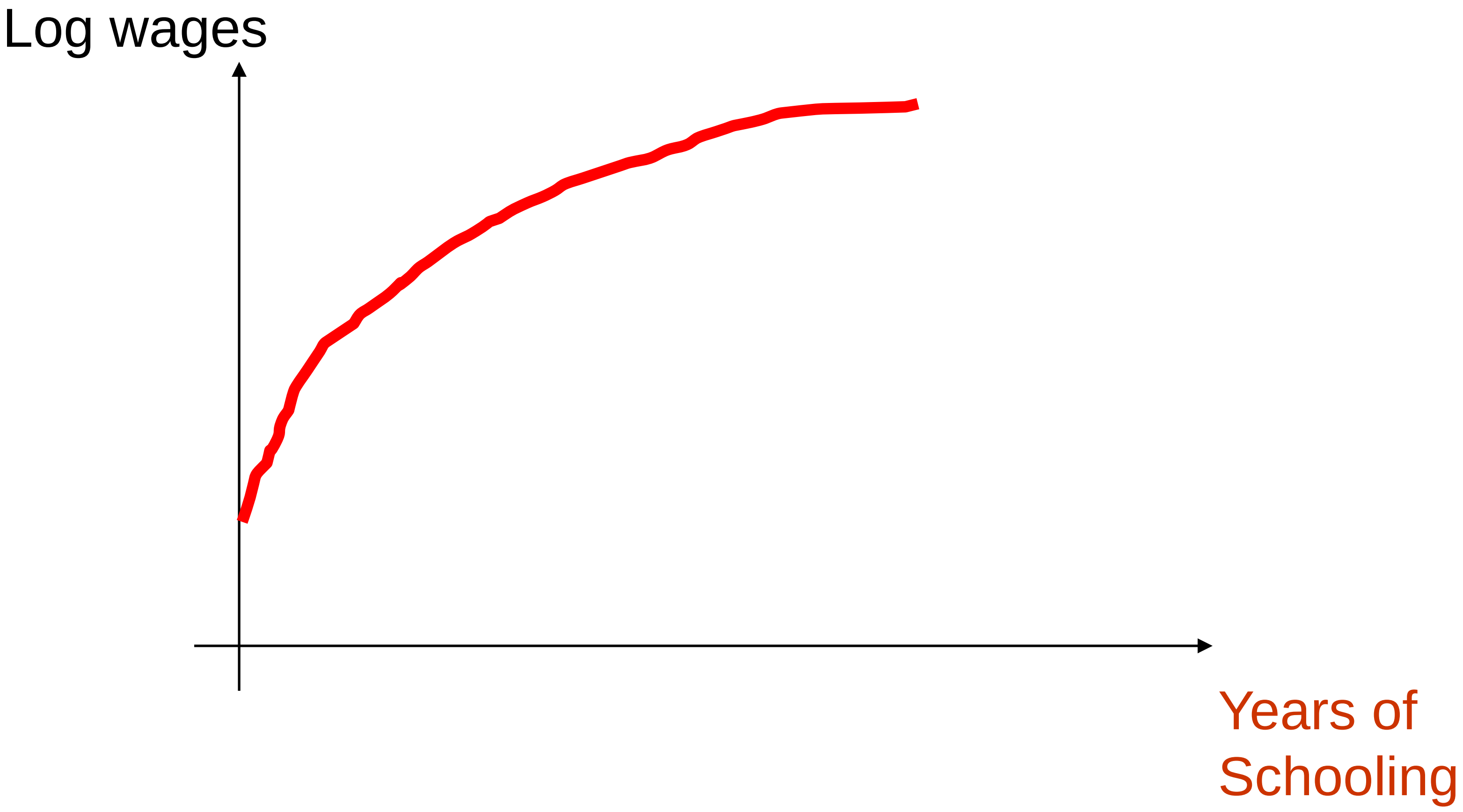
- As one of the largest and richest empirical literatures in all of economics (including development), it serves as a useful introduction to issues of research design, data and measurement.

(1) Different conceptions of education

- Benefits of education could include:
  -- Higher wages (“human capital”)
  -- Education as a signal of ability
  -- Education as consumption (reading Shakespeare)

- Costs: Opportunity cost of time studying; tuition costs

(1) The returns to schooling



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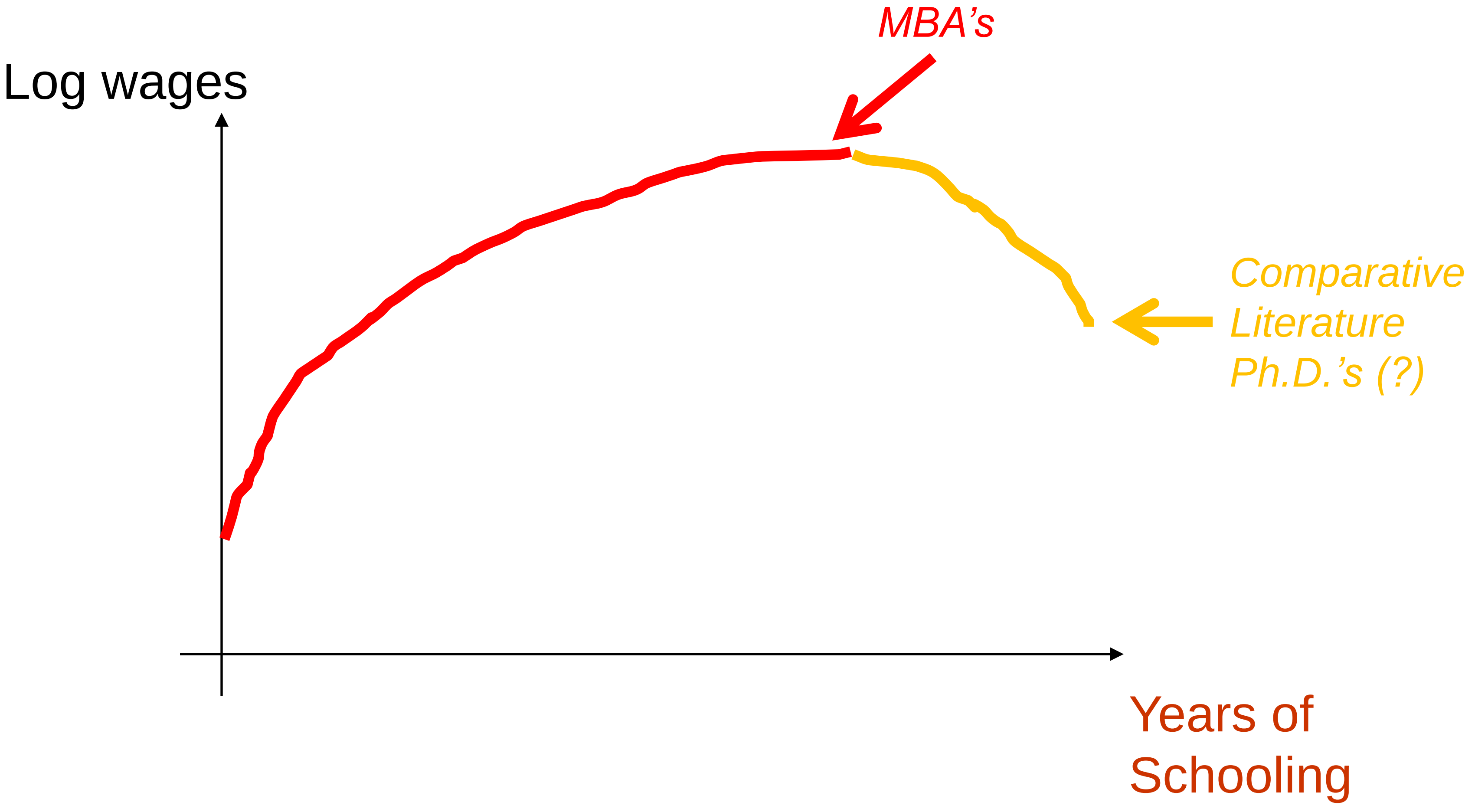

(1) Different conceptions of education

- Link back to last week's lecture (on the **"psychology"** of poverty): education potentially affects many psychological processes:

  -- Information / knowledge

  -- Processing of new information

  -- "Mindset" / attitudes

  -- Aspirations / self-image / self-esteem

  -- (Is there a direct neurophysiological effect?)

(1) Different conceptions of education

- Possible **social** benefits include labor productivity spillovers, a “better” functioning democracy (?), less crime (?), better child health (?), others?

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- Possible **social** benefits include labor productivity spillovers, a “better” functioning democracy (?), less crime (?), better child health (?), others?

- Socially suboptimal investments if there are spillovers (i.e., within the workplace), or within household agency problems (parent-child)

(1) Estimating Mincerian wage regressions

- The Mincerian wage regression:

  $$ln(w_i) = b_0 + b_1 S_i + b_2 X_i + b_3 X_i^2 + e_i$$

  where w is the individual wage, S is years of schooling, and X is years of experience, for individual i

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- This has been run in literally dozens of countries, and estimates of b_1 usually fall in the range 0.05-0.15

- Reliably estimating this equation has been central to labor economics for 50+ years. Possible upward selection / omitted variables bias ("ability"), and possible downward attenuation bias due to **measurement error**

(1) Measurement error and attenuation bias

- Imagine the exact (but unmeasured) variable X* is imperfectly captured by the (measured) variable X:

  $$X_i = X^*_i + u_i$$

  where u_i is an i.i.d. normally distributed random variable. This is classical measurement error

  -- X is reported years of schooling

  -- X* is real schooling or skills

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- We want to run the regression $Y_i = a + bX^*_i + e_i$ but due to data limitations have to run $Y_i = \alpha + \beta X_i + \varepsilon_i$. How does β^{OLS} relate to b?

(1) Measurement error and attenuation bias

- The coefficient of interest is *b*, where OLS delivers:
  $b^{OLS} = Cov(X^*,Y)/Var(X^*)$

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- But we end up estimating:
  $\beta^{OLS} = Cov(X,Y)/Var(X)$
  $= [Cov(X^*, Y) + Cov(u, Y)] / [Var(X^*) + Var(u)]$
  $= [Cov(X^*, Y)] / [Var(X^*) + Var(u)]$
  $= [Cov(X^*,Y)*Var(X^*)/Var(X^*)] / [Var(X^*)+Var(u)]$
  $= b^{OLS}*\{Var(X^*)/ [Var(X^*) + Var(u)]\}$

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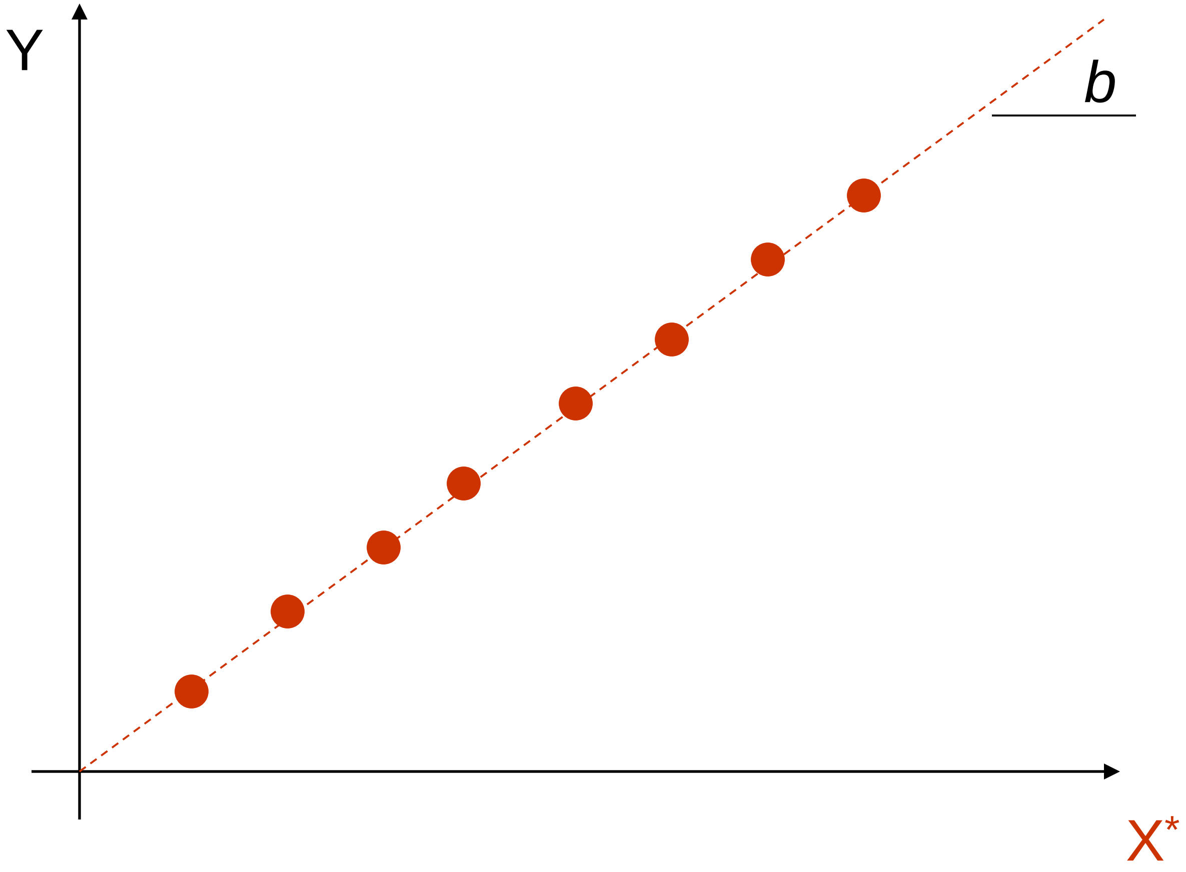
- **Bias towards zero** as a function of the “signal-noise ratio”, i.e., if half the variation in X is noise, bias is 50%

(1) Measurement error and attenuation bias



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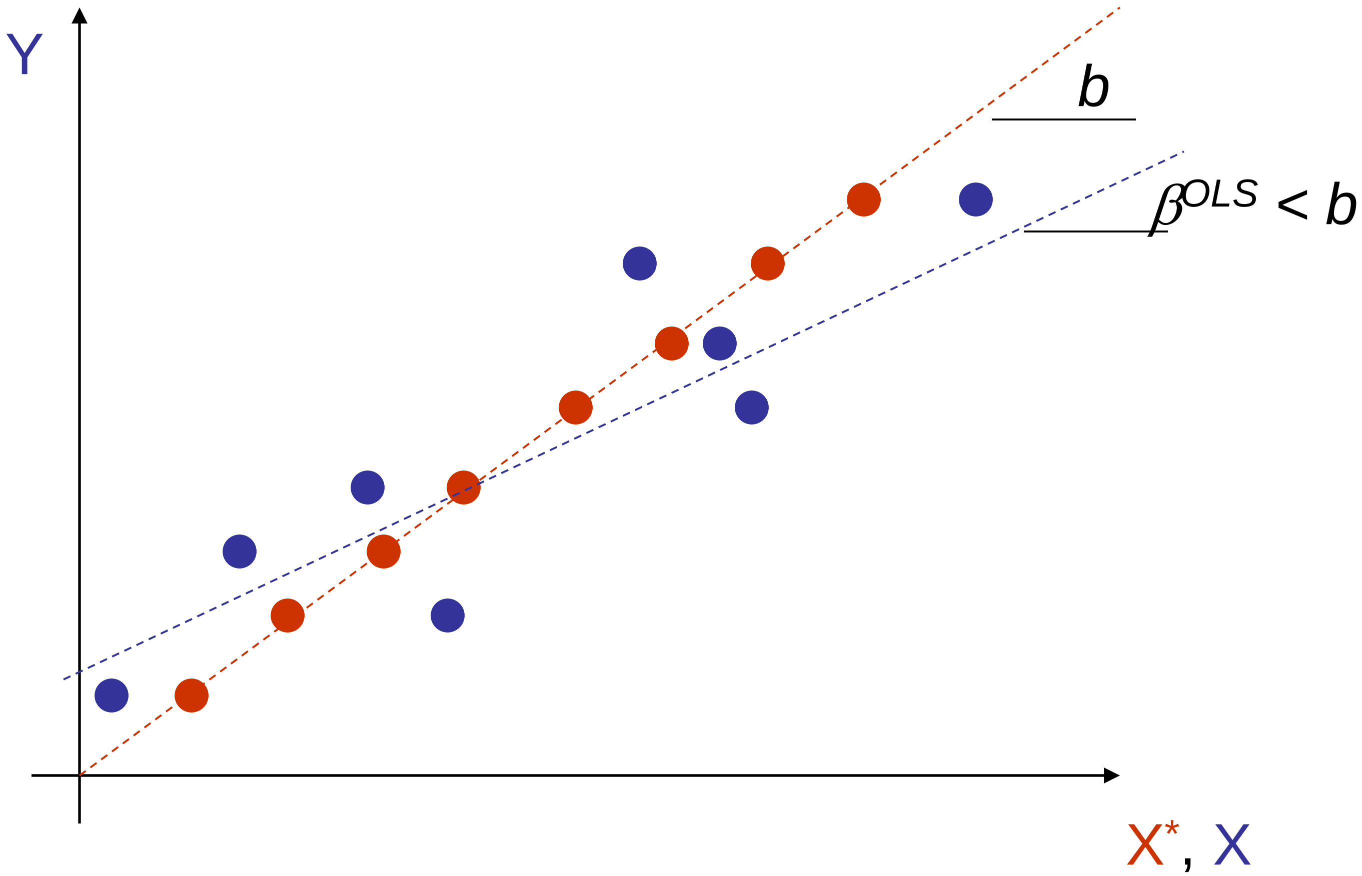

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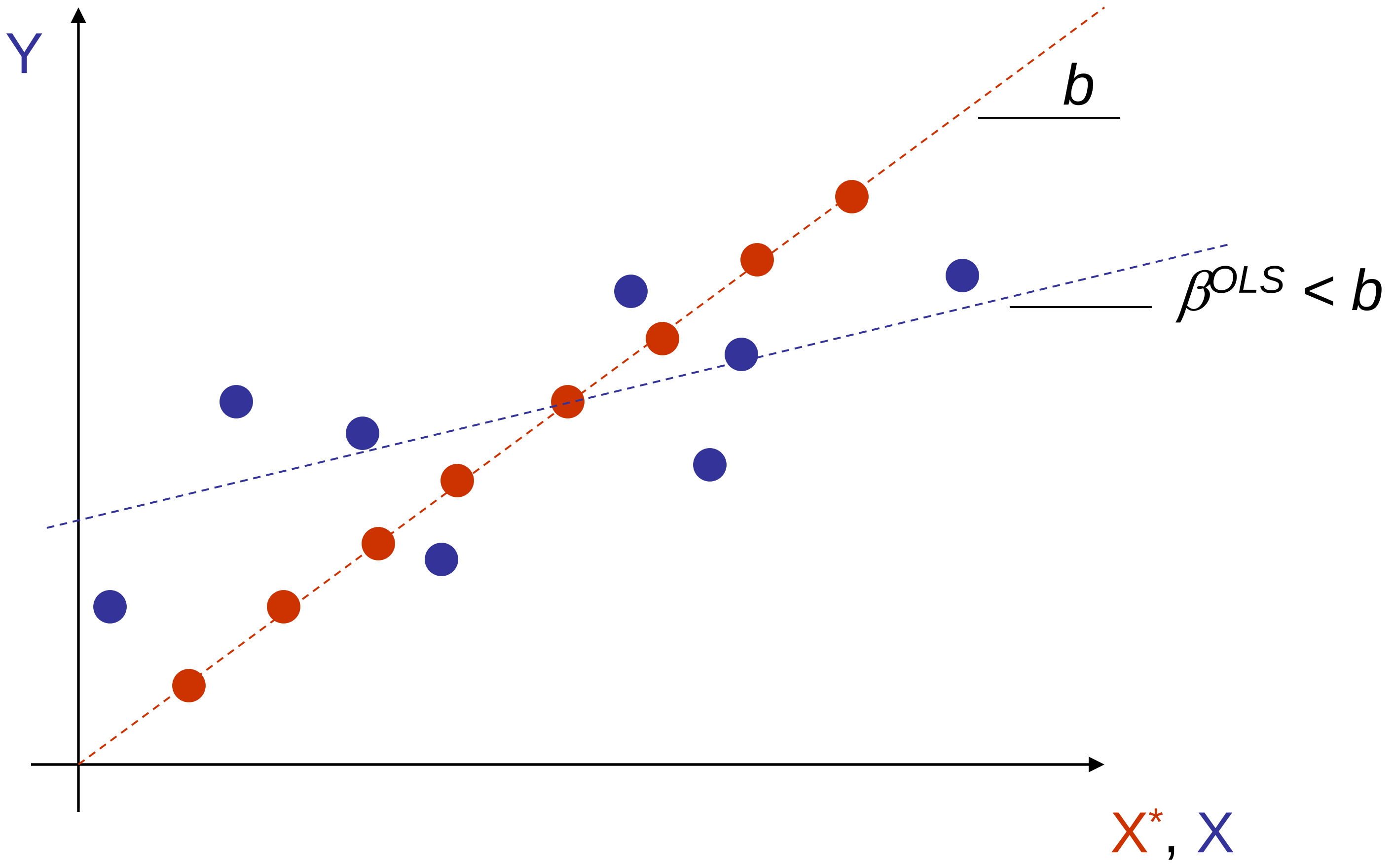

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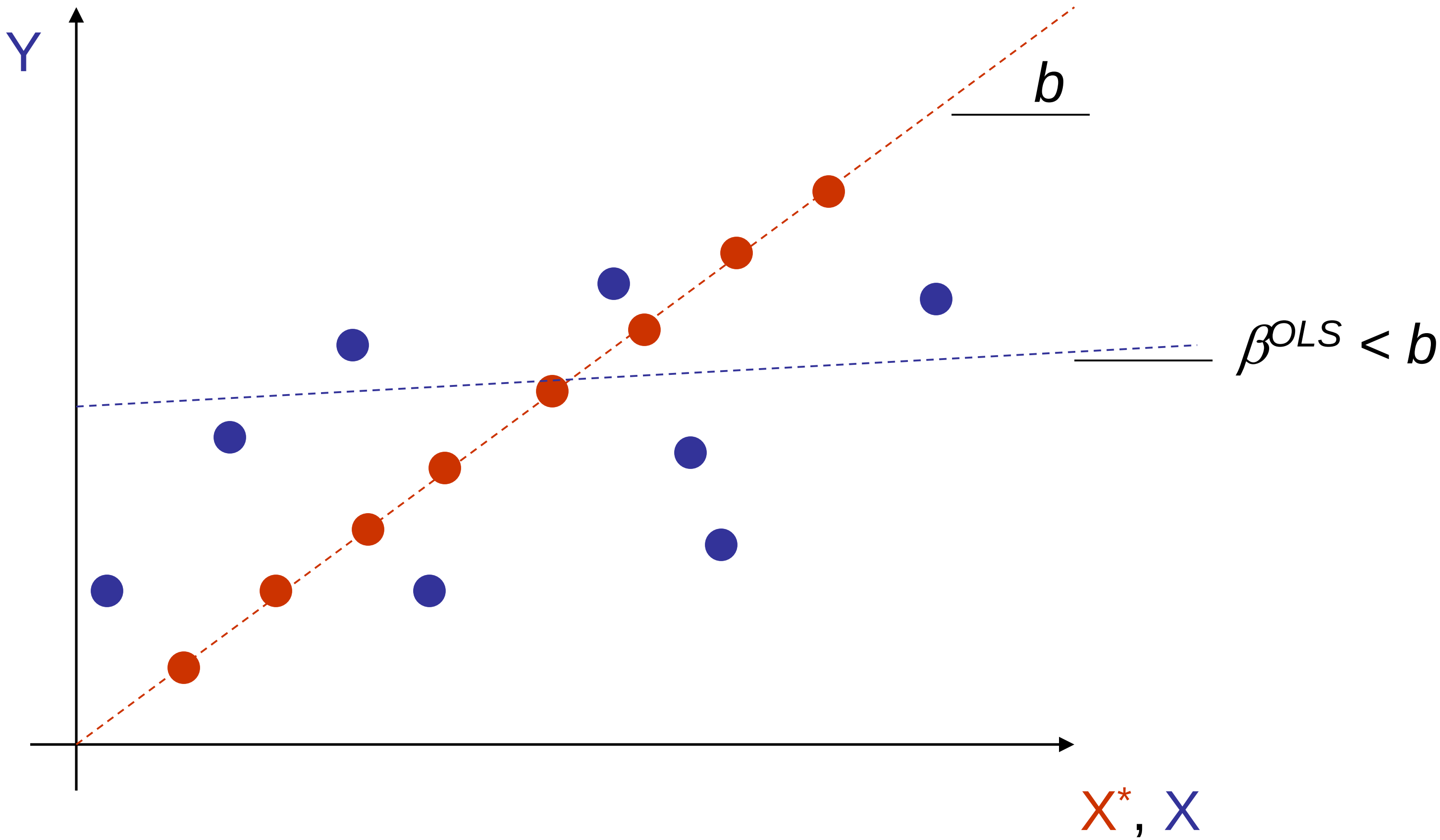


(1) An example from Kenyan microdata

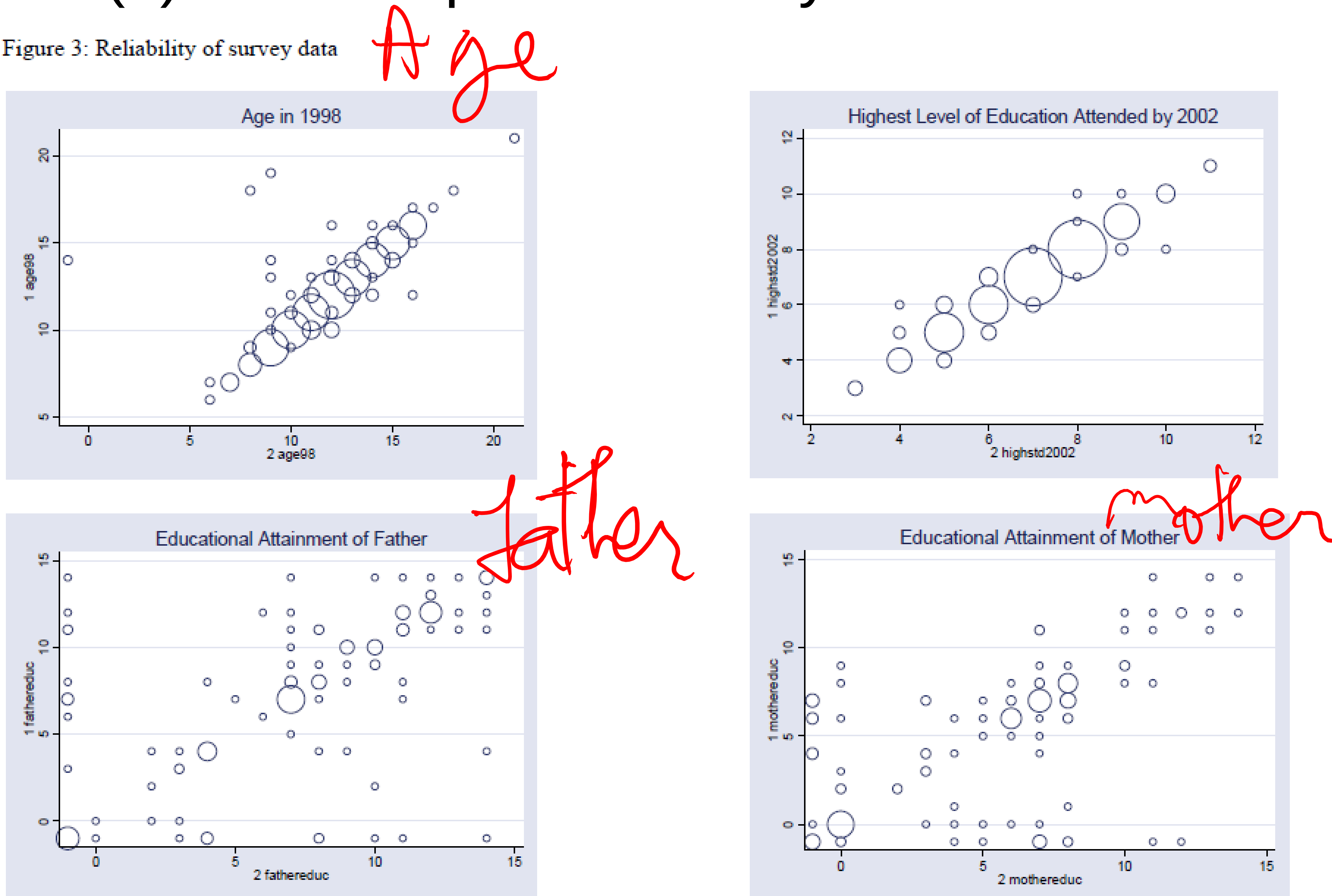
- How noisy is education data in household survey data? Few reliable estimates of measurement error exist.

- One example: as part of the Kenya Life Panel Survey (KLPS), collected in rural western Kenya since 2003, we carry out representative “re-surveys” of respondents, i.e., ask them a subset of questions a few weeks later.
- This allows us to assess the **reliability** of the data as captured in the correlation across multiple measures

- Regardless of your research design, a study with poorly designed measures is unlikely to yield useful results

(1) An example from Kenyan microdata

Figure 3: Reliability of survey data



(1) An example from Kenyan microdata

Table 21: Survey-resurvey response comparison

	Sub-Tribe	Age in 1998	Highest Grade Attended by 2002	Indicator for Ever Left Local Areas	Educational Attainment of Father	Educational Attainment of Mother
Mean [std dev] of survey response[a]	1.40	12.10	6.95	0.09	8.72	5.84
	[0.83]	[2.55]	[1.59]	[0.29]	[3.55]	[3.84]
Mean [std dev] of resurvey response[a]	1.39	11.94	6.93	0.09	8.62	5.77
	[0.82]	[2.50]	[1.60]	[0.29]	[3.30]	[3.77]
Difference [std err][a]	0.01	0.16*	0.02	0.00	0.11	0.07
	[0.05]	[0.09]	[0.03]	[0.02]	[0.22]	[0.22]
Fraction pairs with matching responses[a]	0.948	0.756	0.859	0.914	0.529	0.505
Fraction pairs with responses within one year[a]	--	0.922	0.985	--	0.750	0.705
Fraction survey responses of "don't know"	0	0	0	0	0.154	0.127
Fraction resurvey responses of "don't know"	0	0.005	0	0	0.154	0.119
Fraction survey responses that are impossible values	0	0.005	0	0	0	0
Fraction resurvey responses that are impossible values	0	0	0	0	0.007	0.008
Number survey-resurvey pairs	135	207	206	208	136	134
Pairwise correlation coefficient[a]	--	0.869	0.964	--	0.797	0.819

Reliability ratio of father's education in Kenya ≈ 0.80; in US, Angrist and Krueger (1999) estimate 0.86.

(1) IV and local average treatment effects

- Another important issue in estimating the returns to schooling arises when using instrumental variables (IV): most IV approaches that rely on exogenous shifts in attained schooling identify effects only for the population affected by the shift in attainment (Angrist, Imbens and Rubin 1996) → local average treatment effect (LATE)

  -- The relevant **population** for which IV coefficients are estimated may thus be different than for OLS

- Conceptually, this is nearly the same as the **"external validity"** versus "internal validity" issue in randomized experiments

(1) Returns to schooling in poor countries

- Given these concerns over identification, measurement error, and external validity, relatively few studies in developing countries have rigorously estimated returns to schooling in less developed countries. How should we interpret Mincerian regressions?

  -- Duflo (2001) was an early exception (discussed later)

- Today's lecture explores the demand for education, and macro vs. micro estimates, focusing on issues of measurement and identification.
- Next week's lecture focuses on understanding supply side issues and the education production function.

(2) Jensen (2010, *QJE*)

- What are the drivers of individual (and household) educational investment choices?
- In particular, how knowledgeable are people about the relevant returns to schooling? If **perceived returns** differ systematically from actual returns, people may make "incorrect" investment choices.

(2) Jensen (2010, *QJE*)

- What are the drivers of individual (and household) educational investment choices?
- In particular, how knowledgeable are people about the relevant returns to schooling? If **perceived returns** differ systematically from actual returns, people may make "incorrect" investment choices.

- In-class experiment: how many of you know the mean and standard deviation of salaries for economists in academic research institutions?
- What is the difference in average salaries between those with an MA in Economics versus a Ph.D.?

(2) Jensen (2010, *QJE*)

- Key questions: What are **perceived returns** to schooling in the Dominican Republic (DR)? And how do they affect educational investment choices?

- The DR setting: 80-90% primary completion but only 25-30% secondary school completion.
- Returns to schooling are hard to estimate (as we will discuss later on), but in the cross-section, secondary school grads **earn 40% more** than primary school only
- Given opportunity cost of work and a 5% annual discount, the return to secondary schooling is 15%.
- Minimal official DR government data had been released on schooling and labor market outcomes

(2) Jensen (2010, *QJE*)

- Research design:
- Sample: 8th grade males (average age 14) in non-rural areas of the DR, N=2,250
- Randomization of information (regarding schooling returns) across 150 school clusters nation-wide

(2) Jensen (2010, *QJE*)

- Research design: Zwane et al
- Sample: 8th grade males (average age 14) in non-rural areas of the DR, N=2,250
- Randomization of information (regarding schooling returns) across 150 school clusters nation-wide
- Four year panel dataset (2001-2005), with 90% follow-up
- **Baseline survey** (mid-2001), with information on returns to schooling in treatment clusters; elicit information on perceived returns to schooling from everyone
- First follow-up survey (late-2001), collects information on perceived returns to schooling, enrollment in secondary
- **Second follow-up survey** (mid-2005), educational attainment over time from individuals plus school records

(2) Jensen (2010, *QJE*)

Perceived returns to schooling:

Students were also asked to estimate the earnings of current thirty- to forty-year-old workers with different levels of education:

> Now, we would like you to think about adult men who are about 30 to 40 years old and who have completed only [primary school/secondary school/university]. Think not just about the ones you know personally, but all men like this throughout the country. How much do you think they earn in a typical week, month or year?

(2) Jensen (2010, *QJE*)

The information treatment:

II.B. The Intervention

At the end of the student survey, each respondent at a randomly selected subset of schools was given information on earnings by education from the household survey and the absolute and percent return implied by those values, as reported above:

> Before we end, I would like to provide you with some information from our study. In January, we interviewed adults living in this community and all over the country. We asked them about many things, including their earnings and education. We found that the average earnings of a man 30 to 40 years old with only a primary school education was about 3,200 pesos per month. And the average income of a man the same age who completed secondary school, but did not attend university, was about 4,500 pesos per month. So the difference between workers with and without secondary school is about 1,300 pesos per month; workers who finish secondary school earn about 41 percent more than those who don't. And people who go to university earn about 5,900 pesos per month, which is about 85 percent more than those who only finish primary school.

(2) Jensen (2010, *QJE*)

- Table 1: randomization produced comparable treatment and control groups
- Table 3: perceived earnings “gains” due to secondary schooling are much smaller than observed differences
- (How meaningful are these observed differences?)

(2) Jensen (2010, *QJE*)

TABLE I
MEANS, STANDARD DEVIATIONS, AND TEST OF TREATMENT–CONTROL COVARIATE BALANCE

	All	Control	Treatment	Difference
Age	14.3	14.3	14.4	0.02
	[0.79]	[0.79]	[0.79]	(0.04)
School performance	2.64	2.66	2.62	–0.04
	[1.45]	[1.46]	[1.45]	(0.06)
Father finished secondary	0.38	0.39	0.38	–0.01
	[0.49]	[0.49]	[0.49]	(0.05)
Log (income per capita)	8.16	8.17	8.15	–0.04
	[0.32]	[0.31]	[0.32]	(0.05)
Round 1 expected earnings (self)				
Primary (only)	3,516	3,548	3,484	–64
	[884]	(116)	(124)	(165)
Secondary (only)	3,845	3,884	3,806	–78
	[1,044]	(132)	(145)	(191)
Implied perceived returns (self)	329	336	322	–14
	[403]	(25)	(27)	(36)
Round 1 expected earnings (others)				
Primary (only)	3,478	3,509	3,447	–62
	[863]	(112)	(120)	(160)
Secondary (only)	3,765	3,802	3,728	–73
	[997]	(126)	(143)	(185)
Implied perceived returns (other)	287	293	281	–12
	[373]	(23)	(29)	(36)

(2) Jensen (2010, *QJE*)

TABLE III
MEASURED AND PERCEIVED MONTHLY EARNINGS, MALES AGED 30–40

	(1) Measured mean	(2) Perceived (self)	(3) Perceived (others)
Primary	3,180	3,516	3,478
	[1,400]	[884]	[863]
Secondary	4,479	3,845	3,765
	[1,432]	[1,044]	[997]
Tertiary	9,681	5,127	5,099
	[3,107]	[1,629]	[1,588]
Secondary – primary	1,299	329	287
		[403]	[373]
Tertiary – secondary	5,202	1,282	1,334
		[1,341]	[1,272]

(2) Jensen (2010, *QJE*)

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!

(2) Jensen (2010, *QJE*)

- Table 1: randomization produced comparable treatment and control groups
- Table 3: perceived earnings “gains” due to secondary schooling are much smaller than observed differences
- (How meaningful are these observed differences?)
- Table 2: conditional on other individual factors, high perceived returns to secondary schooling are associated with more school enrollment (cross-sectionally in the control group)

(2) Jensen (2010, *QJE*)

TABLE II

IMPLIED PERCEIVED RETURNS AND

Panel A. Round 1 implied perceived returns (control group only)

	(1) Returned next year	(2) Returned next year	(3) Finished school	(4) Finished school	(5) Years of schooling	(6) Years of schooling
Implied perceived returns	0.11***	0.083**	0.14***	0.092**	0.53***	0.37**
	(0.030)	(0.034)	(0.036)	(0.038)	(0.13)	(0.14)
Log (inc. per capita)		0.090		0.25***		0.76***
		(0.062)		(0.063)		(0.24)
School performance		0.015		0.015		0.093**
		(0.014)		(0.011)		(0.045)
Father finished secondary		0.036		−0.014		0.045
		(0.041)		(0.044)		(0.16)
Age		−0.017		0.006		−0.045
		(0.024)		(0.025)		(0.093)
R^2	.008	.016	.017	.048	.016	.042
Observations	1,003	1,003	1,003	1,003	918	918

(2) Jensen (2010, *QJE*)

- Why are perceived returns so different than observed?
- (A possibility is that cross-sectional differences are not meaningful due to bias, but put that aside for now…)

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- Theoretical framework in online appendix:
- Imagine people are "local econometricians" and get most information from their neighborhood
- Neighborhoods are highly segregated by income
- Implies that "local" estimates of returns to schooling will be **biased towards zero**: due to selection, people in poor (rich) areas observe high and low education individuals all with relatively low (high) incomes
- (Little systematic government data release in DR.)

(2) Jensen (2010, *QJE*)

- Builds on Wilson (1987), who suggests that U.S. youths in poor urban areas see little evidence of a link between education and earnings around them
- Lifetime earnings (Y) depend on education (S, $S \in \{0,1\}$), ability (A), random shock (ε) (as in Mincer):

$$Y = \alpha + \beta A + \gamma S + \varepsilon$$

- Cost of schooling (c) is decreasing in ability ($\beta_C < 0$):

$$c = \alpha_C + \beta_C A + \varepsilon_C$$

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- Attend secondary school (S=1) iff:

$$E[\, Y \mid A, S=1] - E[\, Y \mid A, S=0] = \gamma \geq c$$

- For inference, assume Y, A and S are observable; unobserved A could bias estimates upward (opposite).

(2) Jensen (2010, *QJE*)

- Imagine the stark case in which those with high incomes ($Y > Y^*$) live in one neighborhood and those with lower incomes ($Y < Y^*$) live in another area
- Assume individuals base return to schooling estimates only on neighborhood data, i.e., they are not aware of the sorting mechanism (or are unable to correct for it)

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- Imagine the stark case in which those with high incomes ($Y > Y^*$) live in one neighborhood and those with lower incomes ($Y < Y^*$) live in another area
- Assume individuals base return to schooling estimates only on neighborhood data, i.e., they are not aware of the sorting mechanism (or are unable to correct for it)
- The observed difference between educated vs. uneducated in poor areas is less than γ, as "truncation" lowers mean income for the educated more:

$$E[Y|A, S=1, \varepsilon<Y^*-\alpha-\beta A-\gamma]$$
$$- E[Y|A, S=0, \varepsilon<Y^*-\alpha-\beta A] < \gamma$$

- Similarly for rich areas (truncation raises mean income for uneducated more)

(2) Jensen (2010, *QJE*)

- Case 1: no residential mobility
  → correct inference

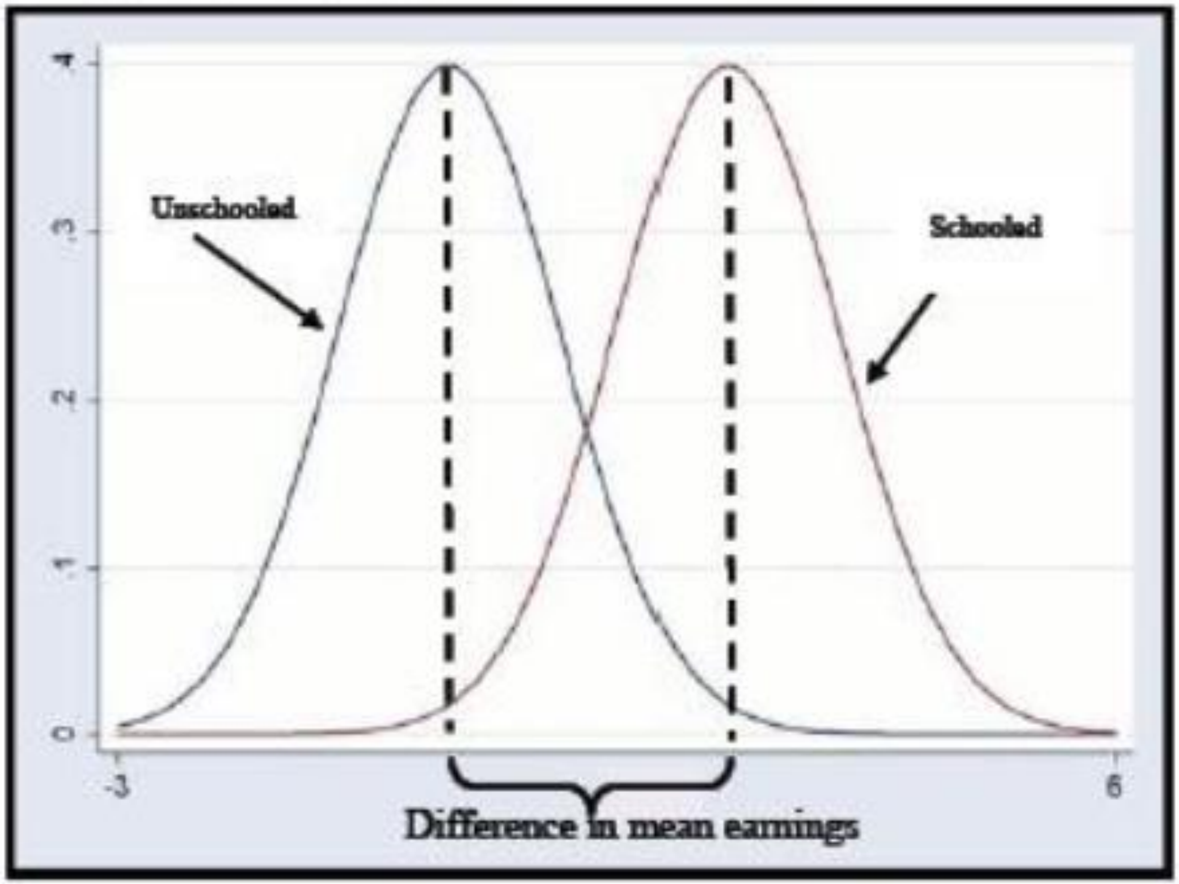
- Case 2: residential mobility leads those with income above a threshold to live in a “rich” neighborhood (presumably high land rental prices keep out the poor)
  → incorrect inference (understate returns)

(2) Jensen (2010, *QJE*)

Online Appendix

FIGURE A.1 RESIDENTIAL SEGREGATION AND MEAN EARNINGS BY EDUCATION

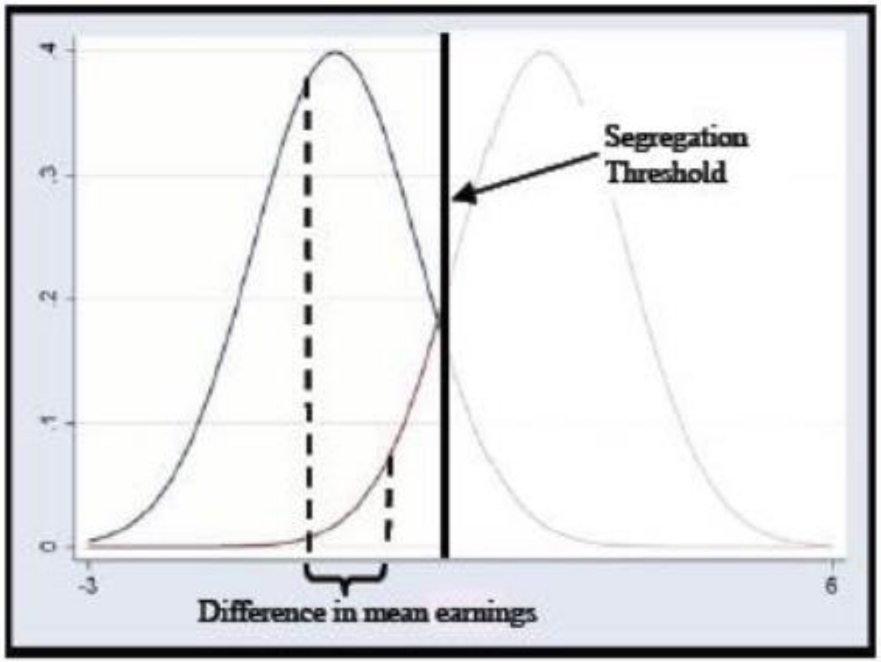
A. Full Population (no residential segregation)



Online Appendix

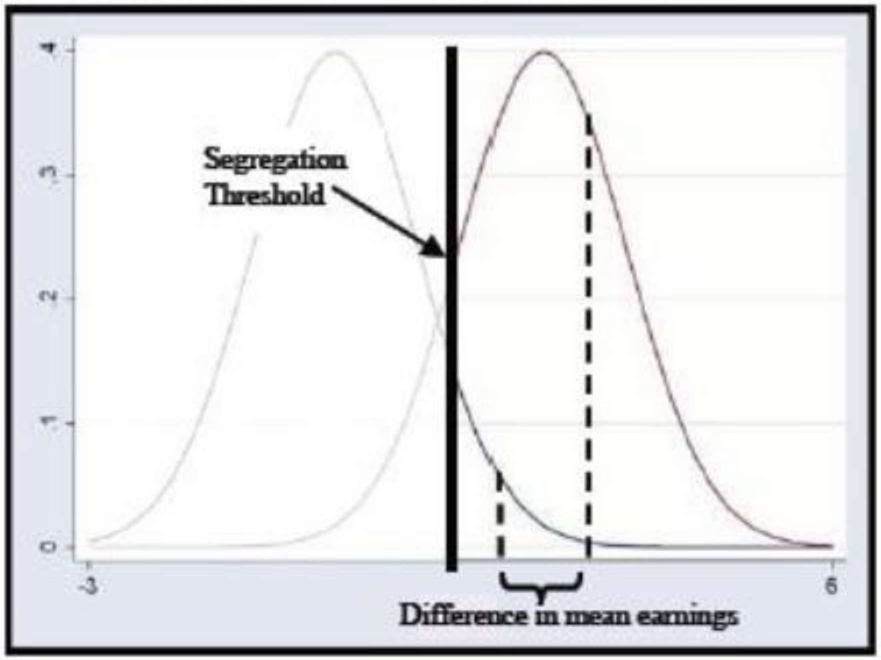
(2) Jensen (2010, *QJE*)

B. Income Segregation—Poor Neighborhood



Y < Y*

C. Income Segregation—Rich Neighborhood



Y > Y*

(2) Jensen (2010, *QJE*)

- Econometric identification strategy: instrumental variable approach
- Information treatment (IV)
  - → Perceived returns (endogenous variable)
  - → Educational investment (outcome variable)

(2) Jensen (2010, *QJE*)

- Econometric identification strategy: instrumental variable approach
- Information treatment (IV)
  → Perceived returns (endogenous variable)
  → Educational investment (outcome variable)

- First stage: Information → Perceived returns
- Reduced form: Information → Educational investment
- Structural relationship (second stage):
  Perceived returns → Education

(2) Jensen (2010, *QJE*)

- Table 4: the information treatment is associated with significantly higher perceived returns to secondary schooling 4-6 months later. (“First stage”)

(2) Jensen (2010, *QJE*)

TABLE IV

EFFECT OF THE INTERVENTION ON EXPECTED RETURNS AND SCHOOLING: NO COVARIATES

	Panel A. Perceived returns to school				
	Round 1		Round 2		
	Control	Treatment	Control	Treatment	Difference-in-difference
Expected earnings (self):					
Primary (only)	3,548	3,484	3,583	3,230	−284***
	(116)	(124)	(118)	(92)	(43)
Secondary (only)	3,884	3,806	4,001	3,995	82*
	(132)	(145)	(132)	(114)	(44)
Implied perceived returns	336	322	418	765	366***
	(25)	(27)	(24)	(34)	(29)
Expected earnings (others):					
Primary (only)	3,509	3,447	3,546	3,204	−274***
	(112)	(120)	(113)	(92)	(41)
Secondary (only)	3,802	3,728	3,892	3,916	102**
	(126)	(143)	(120)	(111)	(45)
Implied perceived returns	293	281	346	712	377***
	(23)	(29)	(22)	(31)	(26)
Number of observations	1,003	1,022	922	977	1,859

(2) Jensen (2010, *QJE*)

- Table 4: the information treatment is associated with significantly higher perceived returns to secondary schooling 4-6 months later. (“First stage”)

- Table 2 (cols 7-12): increase perceived returns to schooling boost educational investments considerable
- A 1 SD increase in perceived returns (400 DR pesos) boosts schooling attainment by 0.25 years.
- (Possible exclusion restriction violations?)

Table 2

(2) Jensen (2010, *QJE*)

	Panel B. Round 2 implied perceived returns (full sample)					
				Instrumental variables		
	(7) Returned next year	(8) Finished school	(9) Years of schooling	(10) Returned next year	(11) Finished school	(12) Years of schooling
Implied perceived returns	0.095***	0.088***	0.37***	0.16**	0.096*	0.63***
	(0.21)	(0.019)	(0.075)	(0.071)	(0.055)	(0.22)
Log (inc. per capita)	0.044	0.18***	0.61***	0.023	0.18***	0.52***
	(0.045)	(0.048)	(0.17)	(0.049)	(0.051)	(0.17)
School performance	0.014	0.021**	0.087**	0.013	0.021**	0.086**
	(0.010)	(0.008)	(0.034)	(0.010)	(0.008)	(0.034)
Father finished secondary	0.067**	0.045	0.21*	0.066**	0.045	0.20*
	(0.032)	(0.029)	(0.12)	(0.032)	(0.029)	(0.12)
Age	−0.011	0.004	−0.006	−0.011	0.004	−0.003
	(0.019)	(0.016)	(0.066)	(0.019)	(0.016)	(0.067)
R^2	.027	.050	.053	.022	.050	.046
Observations	1,899	1,899	1,809	1,899	1,899	1,809

(2) Jensen (2010, *QJE*)

- Two additional patterns of interest:

1. Table 5: Poor (below median income) treatment households have the same response as richer (above median) households in terms of perceived returns to schooling (344 vs. 386 pesos), BUT much smaller gains in years of schooling attained (0.04 vs. 0.33). Why? (Credit constraints? Something else?)

2. Table 6: Individuals with smaller changes in perceived returns over time (<1000 peso change) show much smaller gains in schooling than those with larger changes. (Why not focus on the **interaction** between low baseline perceived returns and treatment?)

(2) Jensen (2010, *QJE*)

1. Key limitation: we do not know what the returns to schooling actually are in Dominican Republic. Working assumption that they are large and positive.

2. How much do these results generalize?

- Nguyen (2008) finds similar impacts on schooling in Madagascar (short time horizon)
- Specific conditions needed: widespread perception of low returns to schooling, and respondents not so poor that credit constraints bind even with better information
- E.g., vocational education project in Kenya (Hicks et al. 2015) did not find lasting impacts of information on returns in male-dominated trades on enrollment.

(2) Jensen (2010, *QJE*)

- Bottom line: is providing more information on returns an attractive education policy option?

(3) Krueger and Lindahl (2001, *JEL*)

(3) Krueger and Lindahl (2001, *JEL*)

- Some researchers have focused on the macroeconomic evidence using cross-country regression methods

- One possible advantage of the macro approach is the ability to **capture social benefits** of schooling, e.g., labor productivity spillovers missed using individual data

  -- This would suggest macro estimates should be larger than micro estimates (unless education just serves a signaling / credential purpose)

  -- From a public economics and policy point of view, social benefits are more important to understand than private benefits, since they would justify public subsidies and intervention.

(3) Krueger and Lindahl (2001)

- Existing cross-country studies regressing income growth on human capital find positive impacts of lagged schooling stocks on growth, but small and not very large effects of changes in educational attainment, say **4% per average year of schooling** – not what we would expect

TABLE 1
REPLICATION AND EXTENSION OF BENHABIB AND SPIEGEL (1994)
DEPENDENT VARIABLE: ANNUALIZED CHANGE IN LOG GDP, 1965–85

	Log Schooling			Linear Schooling		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Log S	–.072	.178	.614	—	—	—
	(.058)	(.112)	(.162)			
Log S_{65}	—	.010	.026	—	—	—
		(.004)	(.005)			
ΔS	—	—	—	.012	.039	.151
				(.023)	(.024)	(.034)
S_{65}	—	—	—	—	.003	.004
					(.001)	(.001)
Log Y_{65}	–.009	–.012	–.015	–.008	–.014	–.014
	(.002)	(.002)	(.003)	(.002)	(.002)	(.004)
Δ Log Capital	.523	.461	—	.521	.465	—
	(.048)	(.052)		(.051)	(.052)	
Δ Log Work Force	.175	.232	—	.110	.335	—
	(.164)	(.160)		(.160)	(.167)	
R^2	.694	.720	.291	.688	.726	.271

Notes: All change variables were divided by 20, including the dependent variable. Sample size is 78 countries. Standard errors are in parentheses. All equations also include an intercept. S_{65} is Kyriacou's measure of schooling in 1965; Δ Log S is the change in log schooling between 1965 and 1985, divided by 20; and Y_{65} is GDP per capita in 1965. Mean of the dependent variable is .039; standard deviation of dependent variable is .020.

(3) Krueger and Lindahl (2001)

- Existing cross-country studies regressing income growth on human capital find positive impacts of lagged schooling stocks on growth, but small and not very large effects of changes in educational attainment, say **4% per average year of schooling** – not what we would expect

- Are the micro estimates (5-15%) just hopeless biased (upwards) by omitted variables / selection?

  -- Or could measurement error in national educational data be (partially) to blame? (This is their main claim.)

(3) Krueger and Lindahl (2001)

- Sources of measurement error in macro education data:

  -- A widely used UNESCO database, based on Ministry of Education statistics. These may be unreliable due to a lack of trained statistical personnel, resources

  -- UNESCO data use enrollment at start of school year

  -- Children educated abroad not counted (this is particularly problematic for secondary, higher education)

  -- Differences in **schooling quality** across countries (e.g., there are big differences even across U.S. towns)

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  -- Differences in **schooling quality** across countries (e.g., there are big differences even across U.S. towns)

- Measurement error may be **exacerbated in first differenced** specifications, like growth regressions

(3) Krueger and Lindahl (2001)

- Consider the first differenced regression equivalent to above, ΔY_i on ΔX_i. The estimate of β becomes:

  $\beta^{OLS} = Cov(\Delta X, \Delta Y)/Var(\Delta X)$

  $= b^{OLS} * \{Var(X^*)/ [Var(X^*) + Var(u)^*\Omega]\}$

  where $\Omega = (1 - \rho_u) / (1 - \rho_{X^*})$, where ρ captures the extent of serial correlation across time in a variable, i.e., $Corr(u_t, u_{t-1}) = Cov(u_t, u_{t-1})/Var(u)$

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  -- First differencing exacerbates attenuation when there is more serial correlation in schooling than measurement error. **"Differencing out" signal leaves mainly noise**. Over short periods, schooling levels are nearly fixed but noise is not, e.g. thought experiment of daily data.

(3) Krueger and Lindahl (2001)

- Recall $\beta^{OLS} = Cov(\Delta X, \Delta Y)/Var(\Delta X)$

  $= b^{OLS}*\{Var(X^*)/ [Var(X^*) + Var(u)*\Omega]\}$

  where $\Omega = (1 - \rho_u) / (1 - \rho_{X^*})$

- An example: if ρ_{X^*}= 0.7 and ρ_u= 0.1, then Ω = (0.9/0.3) = 3. If $Var(X^*) = Var(u)$ = 1, then the attenuation bias effect goes from 0.5 (=1/2) to 0.25 (=1/4).

  -- Using the Barro-Lee and Kyriacou data, they estimate that ρ_{X^*} = 0.97 > 0.61 = ρ_u. This data seems pretty poor, with very non-idiosyncratic errors

(3) Krueger and Lindahl (2001)

- The existence of two different cross-country education series (Barro and Lee; Kyriacou) allows them to validate the accuracy of the data. Assume that there is classical measurement error in both series. A higher correlation between the two series → greater reliability

  -- These data series are quite highly correlated in levels, but **much less so in first differences**. There appears to be substantial measurement error in the first differenced education series, likely leading to major attenuation bias

- The reliability ratio captures the extent of attenuation bias: $R_i = Cov(S_i, S_j) / Var(S_i)$

TABLE 2
RELIABILITY OF VARIOUS MEASURES OF YEARS OF SCHOOLING

A. Estimated Reliability Ratios for Barro-Lee and Kyriacou Data

	Reliability of Barro-Lee Data	Reliability of Kyriacou Data
Average years of schooling, 1965	.851	.964
	(.049)	(.055)
Average years of schooling, 1985	.773	.966
	(.055)	(.069)
Change in years of schooling, 1965–85	.577	.195
	(.199)	(.067)

B. Estimated Reliability Ratios for Barro-Lee and World Values Survey Data

	Reliability of Barro-Lee Data	Reliability of WVS Data
Average years of schooling, 1990	.903	.727
	(.115)	(.093)
Average years of secondary and higher schooling, 1990	.719	.512
	(.167)	(.119)

Notes: The estimated reliability ratios are the slope coefficients from a bivariate regression of one measure of schooling on the other. For example, the .851 entry in the first row is the slope coefficient from a regression in which the dependent variable is Kyriacou's schooling variable and the independent variable is Barro-Lee's schooling variable. The .964 ratio in the second column is estimated from the reverse regression. In panel B, the reliability ratios are estimated by comparing the Barro-Lee and WVS data. In the WVS data set, secondary and higher schooling is defined as years of schooling attained *after 8 years of schooling*.

Sample size for panel A is 68 countries. Sample size for panel B is 34 countries. Standard errors are reported in parentheses.

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(3) Krueger and Lindahl (2001)

- Examine the relationship between economic growth and education growth over different time periods. Since the underlying education stock is slow moving, over shorter intervals Ω is likely to be larger thus exacerbating measurement error

  -- Using the best data, a longer time period (20 years), and correcting for attenuation bias yields a “return” of **30%** to an additional year of education attained (on average)

TABLE 3
THE EFFECT OF SCHOOLING ON GROWTH
DEPENDENT VARIABLE: ANNUALIZED CHANGE IN LOG GDP PER CAPITA

	5-year changes			10-year changes			20-year changes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
S_{t-1}	.004 (.001)	—	.004 (.001)	.003 (.001)	—	.004 (.001)	.005 (.001)	—	.005 (.001)
ΔS	—	.031 (.015)	.039 (.014)	—	.075 (.026)	.086 (.024)	—	.184 (.057)	.182 (.051)
Log Y_{t-1}	–.005 (.003)	.004 (.002)	–.006 (.003)	–.003 (.003)	.004 (.001)	–.005 (.003)	–.010 (.003)	–.001 (.002)	–.013 (.003)
R^2	.197	.161	.207	.242	.229	.284	.184	.103	.281
N	607	607	607	292	292	292	97	97	97

Notes: First six columns include time dummies. Equations were estimated by OLS. The standard errors in the first six columns allow for correlated errors for the same country in different time periods. Maximum number of countries is 110. Columns 1–3 consist of changes for 1960–65, 1965–70, 1970–75, 1975–80, 1980–85, 1985–90. Columns 4–6 consist of changes for 1960–70, 1970–80, 1980–90. Columns 7–9 consist of changes for 1965–85. Log Y_{t-1} and S_{t-1} are the log GDP per capita and level of schooling in the initial year of each period. ΔS is the change in schooling between t – 1 and t divided by the number of years in the period. Data are from Summers and Heston and Barro and Lee. Mean (and standard deviation) of annualized per capita GDP growth is .021 (.033) for columns 1–3, .022 (.026) for columns 4–6, and .022 (.020) for columns 7–9.

(3) Krueger and Lindahl (2001)

- Examine the relationship between economic growth and education growth over different time periods. Since the underlying education stock is slow moving, over shorter intervals Ω is likely to be larger thus exacerbating measurement error

  -- Using the best data, a longer time period (20 years), and correcting for attenuation bias yields a "return" of 30% to an additional year of education attained (on average), **where 0.182/0.577 $\approx$ 0.3**

  -- Alternatively, one measure can IV for the other (Table 4), but standard errors are very large in that case.

(3) Krueger and Lindahl (2001)

- The social return to education – or endogeneity / OVB?

- Bottom line: there is not much we can say about the causal effect of more schooling on economic growth using cross-country data, due to both measurement and identification issues

  -- More convincing work on human capital externalities has been micro-level or region-level (i.e. Moretti 2004)

(4) Duflo (2001, *AER*)

- The **ideal experiment** would randomize educational chances (by varying subsidies, perhaps) across individuals, and across regions, to estimate externalities
- See Glennerster and Takavarasha (2013); next week.

- Duflo (2001) is among the first reliable estimates of returns to education in a less developed country

  -- Studies the impact of a massive school building campaign in Indonesia during the oil-rich 1970s. What impact did this expansion have on later schooling attainment? On later wages?

  -- Not a randomized experiment: how credible?

(4) Duflo (2001, *AER*)

- Between 1973-1978 the government built 61,000 additional primary schools, doubling the number of classrooms in the country. The number of teachers also increased by 43% (!) during this period. This could be thought of as a sharp **drop in the price** of primary education for many households (e.g., travel costs)

- Poor areas were supposed to be targeted, but building did not exactly follow the formula. Schools were supposed to be built in proportion to the number of children **out of school** in 1973 (Table 2)

TABLE 2—THE ALLOCATION OF SCHOOLS

	Log(INPRES schools)[a]
Log of number of children aged 5–14 in the region	0.78
	(0.027)
Log(1 − enrollment rate in primary school in 1973)[b]	0.12
	(0.038)
Number of observations	255
R^2	0.78

Notes: Standard errors are in parentheses.

[a] The dependent variable is the log of the number of INPRES schools built between 1973 and 1978.

[b] The enrollment rate in primary school is the number of children enrolled in primary school in 1973 (obtained from the Ministry of Education and Culture) divided by the number of children aged 5–14 in the region in 1973.

(4) Duflo (2001, *AER*)

- Focuses on the 1995 labor market outcomes of men born between 1950-1972 (using the SUPAS intercensal household survey), N=200,000 households

- Difference in differences strategy: compare cohorts too old to benefit to those who benefited from the program, across areas with more versus fewer schools built

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- Difference in differences strategy: compare cohorts too old to benefit to those who benefited from the program, across areas with more versus fewer schools built

- IV-2SLS estimation:

  School construction (instrumental variable)

  → educational attainment (endogenous variable)

  → wages (outcome variable)

(4) Duflo (2001, *AER*)

- Consider the impact of the program on school attainment in the first stage:

$$S_{ijk} = c + \alpha_j + \beta_k + (P_j{*}T_i)\gamma + (Z_j{*}T_i)\delta + \varepsilon_{ijk}$$

where S is the amount of schooling for an individual i, in region j and age cohort k. Let c be a constant, α_j be an indicator for district of individual birth, β_k be cohort indicator variables, P_j denotes program intensity in region j , Z_j are other regional controls, and T is an indicator taking on a value of one if the individual was young enough to benefit from the program ("treated")

(4) Duflo (2001, *AER*)

- An identification concern is the exclusion restriction: other targeted programs in the same areas

  -- Would there have been convergence across regions even in the absence of the school-building program?

  -- Did quantity **and quality** of education change?

- The performance of older cohorts in programs districts serves as a sort of internal control to capture local trends

- Bottom line: returns to schooling in Indonesia in 1995 between 6-10% per year

  -- Poor and low density regions appear to benefit most

TABLE 4—EFFECT OF THE PROGRAM ON EDUCATION AND WAGES: COEFFICIENTS OF THE INTERACTIONS BETWEEN COHORT DUMMIES AND THE NUMBER OF SCHOOLS CONSTRUCTED PER 1,000 CHILDREN IN THE REGION OF BIRTH

First stage

Reduced form

		Dependent variable					
		Years of education			Log(hourly wage)		
	Observations	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Experiment of Interest: Individuals Aged 2 to 6 or 12 to 17 in 1974 (Youngest cohort: Individuals ages 2 to 6 in 1974)							
Whole sample	78,470	0.124	0.15	0.188			
		(0.0250)	(0.0260)	(0.0289)			
Sample of wage earners	31,061	0.196	0.199	0.259	0.0147	0.0172	0.0270
		(0.0424)	(0.0429)	(0.0499)	(0.00729)	(0.00737)	(0.00850)
Panel B: Control Experiment: Individuals Aged 12 to 24 in 1974 (Youngest cohort: Individuals ages 12 to 17 in 1974)							
Whole sample	78,488	0.0093	0.0176	0.0075			
		(0.0260)	(0.0271)	(0.0297)			
Sample of wage earners	30,225	0.012	0.024	0.079	0.0031	0.00399	0.0144
		(0.0474)	(0.0481)	(0.0555)	(0.00798)	(0.00809)	(0.00915)
Control variables:							
Year of birth*enrollment rate in 1971		No	Yes	Yes	No	Yes	Yes
Year of birth*water and sanitation program		No	No	Yes	No	No	Yes

Notes: All specifications include region of birth dummies, year of birth dummies, and interactions between the year of birth dummies and the number of children in the region of birth (in 1971). The number of observations listed applies to the specification in columns (1) and (4). Standard errors are in parentheses.

Wald estimate = 0.027/0.259 ≈ 10%

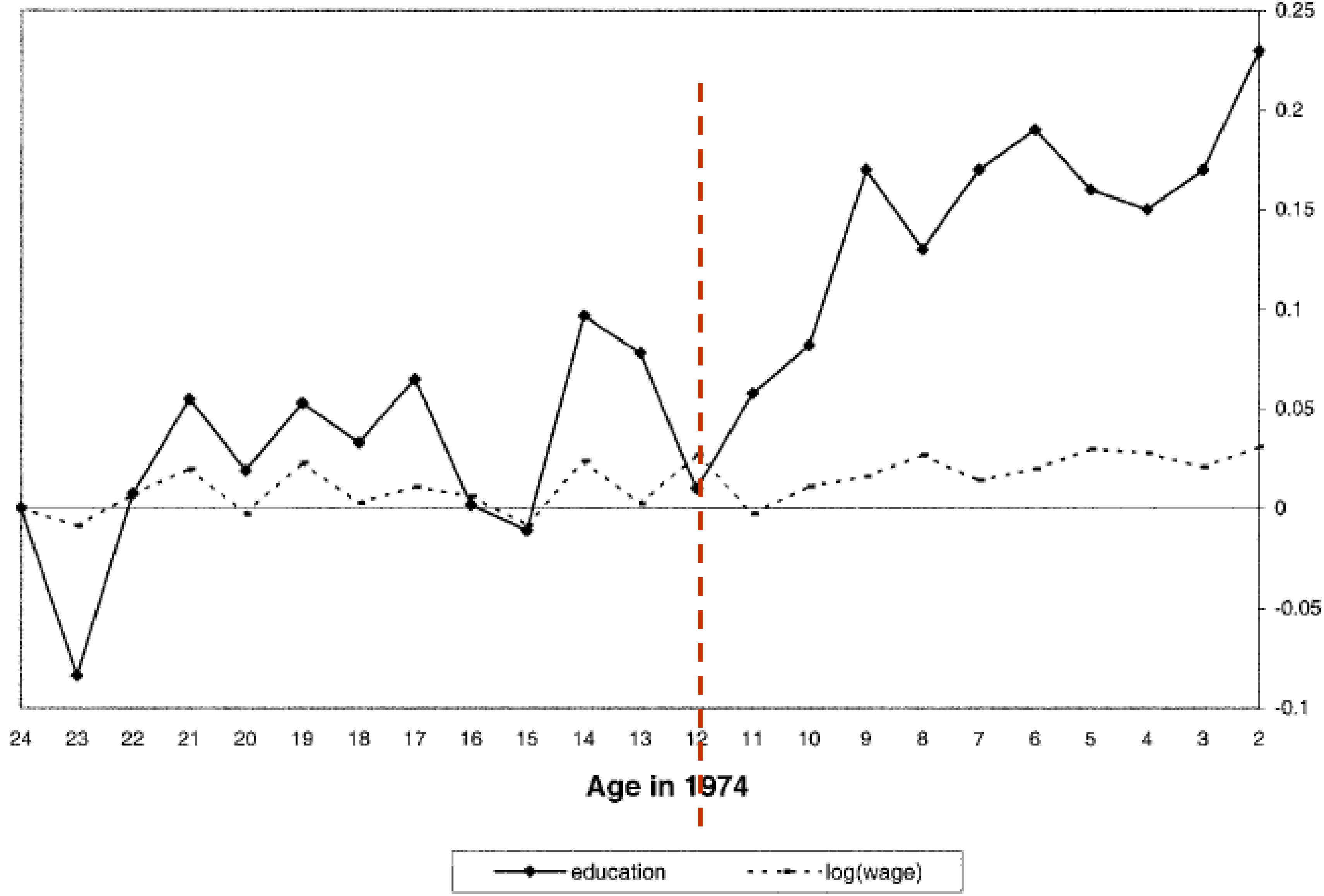

FIGURE 3. COEFFICIENTS OF THE INTERACTIONS AGE IN 1974* PROGRAM INTENSITY IN THE REGION OF BIRTH IN THE WAGE AND EDUCATION EQUATIONS

Table 6—Program Effect and Returns to Education by Categories of Region of Birth

		Characteristics of region of birth					
		Density[a]		1976 Poverty[b]		Preprogram education[c]	
	Whole sample (1)	<Median (2)	>Median (3)	High (4)	Low (5)	<Median (6)	>Median (7)
Panel A: Effect of the Program on Education							
Dependent variable: Years of education. Sample: individuals ages 2 to 6 or 12 to 17 in 1974							
Interaction	0.15	0.19	−0.014	0.13	0.083	0.14	0.13
(2–6 in 1974)*program intensity in region of	(0.026)	(0.035)	(0.048)	(0.058)	(0.035)	(0.040)	(0.036)
Panel B: Effect of the Program on Wages							
Dependent variable: log(hourly wage). Sample: individuals ages 2 to 6 or 12 to 17 in 1974 (wage earners)							
Interaction	0.017	0.032	−0.00084	0.051	−0.00083	0.028	0.0046
(2–6 in 1974)*program intensity in region of	(0.0074)	(0.011)	(0.012)	(0.017)	(0.0094)	(0.013)	(0.0095)
Panel C: Returns to Education							
Dependent variable: log(hourly wage). Sample: wage earners							
Years of education	0.078	0.11	No First stage	0.10	No First stage	0.12	0.029
	(0.00062)	(0.026)		(0.028)		(0.032)	(0.052)
	[0.9]	[0.86]		[0.88]		[0.72]	[0.83]

Table 7—Effect of Education on Labor Market Outcomes: OLS and 2SLS Estimates

Control function

Method	Instrument	(1)	(2)	(3)	(4)
Panel A: Sample of Wage Earners					
Panel A1: Dependent variable: log(hourly wage)					
OLS		0.0776	0.0777	0.0767	
		(0.000620)	(0.000621)	(0.000646)	
2SLS	Year of birth dummies*program intensity in region of birth	0.0675	0.0809	0.106	0.0908
		(0.0280)	(0.0272)	(0.0222)	(0.0541)
		[0.96]	[0.9]	[0.93]	[0.9]
2SLS	(Aged 2–6 in 1974)*program intensity in region of birth	0.0752	0.0862	0.104	
		(0.0338)	(0.0336)	(0.0304)	
		(0.0338)	(0.0336)	(0.0304)	
Panel A2: Dependent variable: log(monthly earnings)					
OLS		0.0698	0.0698	0.0689	
		(0.000601)	(0.000602)	(0.000628)	
2SLS	Year of birth dummies*program intensity in region of birth	0.0756	0.0925	0.0913	0.134
		(0.0280)	(0.0278)	(0.0219)	(0.0631)
		[0.73]	[0.63]	[0.58]	[0.7]
Panel B: Whole Sample					
Panel B1: Dependent variable: participation in the wage sector					
OLS		0.0328	0.0327	0.0337	
		(0.00311)	(0.000311)	(0.000319)	
2SLS	Year of birth dummies*program intensity in region of birth	0.101	0.118	0.0892	
		(0.0210)	(0.0197)	(0.0162)	
		[0.66]	[0.93]	[1.12]	
Panel B2: Dependent variable: log(monthly earnings), imputed for self-employed individuals					
OLS		0.0539	0.0539	0.0539	
		(0.000354)	(0.000354)	(0.000355)	
2SLS	Year of birth dummies*program intensity in region of birth	0.0509	0.0745	0.0346	
		(0.0157)	(0.0136)	(0.0138)	
		[0.68]	[0.58]	[1.16]	
Control variables:					
Year of birth*enrollment rate in 1971		No	Yes	Yes	Yes
Year of birth*water and sanitation program		No	No	Yes	No
Propensity score, propensity score squared		No	No	No	Yes

(4) Duflo (2001, *AER*)

- What is the rate of return of the program?

  -- Estimated returns are highly sensitive to post-construction income growth in Indonesia

  -- Under fast growth (like that observed in 1970s-1990s), education investments relatively high rates of return, 8.8 to 12%. Under slow growth, returns to this program probably would have been small or even negative

(4) Duflo (2001, *AER*)

- What is the rate of return of the program?

  -- Estimated returns are highly sensitive to post-construction income growth in Indonesia

  -- Under fast growth (like that observed in 1970s-1990s), education investments relatively high rates of return, 8.8 to 12%. Under slow growth, returns to this program probably would have been small or even negative
- Given this finding, forward looking governments' **education investments might be endogenous** to growth prospects – further complicating cross-country results. E.g., school enrollment and literacy in India started growing in the 1990s after growth had increased

TABLE 8—EVALUATION OF THE PROGRAM'S NET RETURN

	Deadweight loss 0.2 (1)	Deadweight loss 0.2 (2)
Panel A: Results		
Control for year of birth*enrollment rate	No	Yes
First year where benefit > costs (discount rate = 5 percent)		
In annual value	1996	1996
In discounted sum	2005	2002
Discounted sum of net benefits in 2050 (growth rate after 1997 = 5 percent, discount rate 5 percent)		
In million 1990 U.S.$	13,025	13,096
As a fraction of Indonesia's GDP in 1973	0.30	0.36
Divided by initial costs	24.1	24.2
Discounted sum of net benefits in 2050 (growth rate after 1997 = 2 percent, discount rate 5 percent)		
In million 1990 U.S.$	6,691	11,589
As a fraction of Indonesia's GDP in 1973	0.18	0.32
Divided by initial costs	12.4	21.4
Discounted sum of net benefits in 2050 (growth rate from 1973 = 2 percent, discount rate 5 percent)		
In million 1990 U.S.$	−631.6	1,200
As a fraction of Indonesia's GDP in 1973	−0.017	0.033
Divided by initial costs	−1.16	2.22
Internal rate of return[a]		
Growth rate after 1997 = 5 percent	0.102	0.118
Growth rate after 1997 = 2 percent	0.088	0.106
Growth rate from 1973 = 2 percent	0.0443	0.059
Panel B: Assumptions and Parameters		
Population growth rate after 1997	0.015	
Yearly teacher's salary in 1973 (1990 U.S. dollars)	363	
Yearly teacher's salary in 1995 (1990 U.S. dollars)	2,467	
Total recurrent costs/teacher salary	1.25	
Total cost of construction (million 1990 U.S. dollars)	522	
Number of schools constructed	61,800	
Lifetime of the schools (years)	20	
Share of labor income in GDP	0.7	

(4) Duflo (2001, *AER*)

- Looking ahead:

  -- If education does have sizeable private (and perhaps even larger social) returns, should more public resources be spent on education in less developed countries? If so, what types of investments should be made?

  -- Pupil-teacher ratios, textbooks, the organization of the school system / teacher's unions, incentives for teachers, students, parents, ….

- Building a sense of civic responsibility, democratic values, national identity and cohesion is a social return to education that may be important but is hard to estimate with micro-econometric methods


RUSSIA
CANADA
U.S.A.
PACIFIC OCEAN
ATLANTIC OCEAN
KAZAKSTAN
MONGOLIA
CHINA
JAPAN
TURKEY
IRAQ
IRAN
ALGERIA
LIBYA
EGYPT
SAUDI ARABIA
INDIA
MALI
NIGER
CHAD
SUDAN
NIGERIA
ETHIOPIA
SOMALIA
COLOMBIA
BRAZIL
BOLIVIA
ANGOLA
ZAMBIA
NAMIBIA
SOUTH AFRICA
INDIAN OCEAN
INDONESIA
PAPUA NEW GUINEA
AUSTRALIA
NEW ZEALAND
SOUTHERN OCEAN
ANTARCTICA
The Hobo-Dyer Equal Area Projection

Next week

- For next week's lecture, please focus on the Muralidharan and Sundararaman (2011) and Baird et al (2011) articles.

- The second referee report is due in two weeks (February 23rd), on the Rebecca Dizon-Ross article.