All Together

Part II

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Today's and Tomorrow's Lecture

- Review most of our tools for causal inference applying them to one policy issue:
    - The effect of education on earnings

We will do this in three steps:

- First we will use selection bias, potential outcomes, RCTs and regression to frame this causal question.

- Then we will learn our last important concept: bad controls.

- Finally we will see how we can use IV, DD and RDD to answers this question.

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- **Finally we will see how we can use IV, DD and RDD to answers this question.**

Use IV, DD and RDD to Answers This Question.

- Study 1 (Reg + IV): Twins, Ability, and Measurement Error
- Study 2 (IV +DD): Compulsory Schooling in the Early 20th Century
- Study 3 (IV): QOB and Schooling
- Study 4 (RDD): Degree Completion and Earnings
- Note: the goal of these examples is to help you solidify concepts already reviewed. Use them to understand key concepts but don't get too frustrated if you don't understand some of the specifics of any of these examples.

Study 1 (Reg + IV): Twins, Schooling and Ability

- As we discuss yesterday and OLS regression with schooling and earnings is probably contaminated by OVB from factors like ability and/or privileged.
- One approach to control for those factors is to look at twins:
  - Share similar upbringing.
  - Share genetic backgrounds.
  - Any difference in schooling between two twins is unrelated to this commonly shared characteristics.
- We can remove the OVB of this other factors by taking the differences between twins.

Twin Differences: Regression 1/2

Given that we are interested in variables at the family level (f) and at the twin level (i), the long equation for this setting:

$$lnY_{if} = \alpha^l + \rho^l S_{if} + \lambda A_{if} + e^l_{if}$$

What this study assumes is that this other factors (A_{if}) are constant within a family (they don't vary between individual i within family f). Given this, we can look at the regression equation of each twin:

$$lnY_{1f} = \alpha^l + \rho^l S_{1f} + \lambda A_f + e^l_{1f}$$
$$lnY_{2f} = \alpha^l + \rho^l S_{2f} + \lambda A_f + e^l_{2f}$$

- Subtracting the equation of one twin from the other:

Twin Differences: Regression 2/2

$$lnY_{1f} - lnY_{2f} = \rho^l(S_{1f} - S_{2f}) + e^l_{1f} - e^l_{2f}$$

- No OVB!
- Column 1: Levels.
- Column 2: Differences.
- OLS points again to 11% return on additional year.
- Difference approach suggest 6%.
- But: all the variation comes from differences in schooling between to twins.

TABLE 6.2
Returns to schooling for Twinsburg twins

	Dependent variable			
	Log wage (1)	Difference in log wage (2)	Log wage (3)	Difference in log wage (4)
Years of education	.110 (.010)		.116 (.011)	
Difference in years of education		.062 (.020)		.108 (.034)
Age	.104 (.012)		.104 (.012)	
Age squared/100	−.106 (.015)		−.106 (.015)	
Dummy for female	−.318 (.040)		−.316 (.040)	
Dummy for white	−.100 (.068)		−.098 (.068)	
Instrument education with twin report	No	No	Yes	Yes
Sample size	680	340	680	340

Notes: This table reports estimates of the returns to schooling for Twinsburg twins. Column (1) shows OLS estimates from models estimated in levels. OLS estimates of models for cross-twin differences appear in column (2). Column (3) reports 2SLS estimates of a levels regression using sibling reports as instruments for schooling. Column (4) reports 2SLS estimates using the difference in sibling reports to instrument the cross-twin difference in schooling. Standard errors appear in parentheses.

Twin Differences: Measurement Error

- One interpretation of the drop from 11% to 6% is that the latter has much more measurement error in the measure of schooling.
- Twins tend to have similar schooling. Differences can emerge for (a) random reasons or (b) misreporting of years of education.
- Measurement error in the regressor of interest (treatment variable) leads to attenuation bias (appendix in Ch6).
- To address this bias, the authors of the study suggest using an instrument that is unlikely to have the same bias: the years of education of one sibling as reported by the other sibling.

Twin Differences IV 1/2

- Is the sibling's report on the other sibling's education a good instrument (for the education of an individual)?
- Relevant: yes, the report of the sibling is probably good at explaining the education of the individual.
- Independent: it definitively is not random, but the argument here is that it is independent to the measurement error.
- Exclusion: probably yes, as the report on education probably affects earnings through education alone.
- The key idea here is that the reduce form and first stage still suffer from attenuation bias, but this bias cancels out when computing the LATE.

Twin Differences IV 2/2

- Columns 3 and 4
- 2SLS estimates
- After correcting for measurement error we are almost back to the OLS estimate!
- In this sample, there doesn't seem to be much of an ability/privilege bias that is common across siblings.

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Study 2 (IV+DD): Compulsory Schooling in the Early 20th Century (US)

- In the first half of the 20th century, several state laws requiring compulsory education were established to prevent child labor.
- The requirements vary between 6th - 9th grades, and were implemented at different times across states.
- We will look at a study that combines two research design tools:
  - Uses the compulsory laws of each states as an instrument for the years of education, and
  - Control for state and year of birth fixed effects, hence generating a DD estimate.
- This instruments are implemented as binary variables for each year of requirement (leaving 6th grade as a reference group)
- Are this compulsory education laws a good instrument?

Compulsory Schooling and Earnings: Assessing the Instrument(s)

- Compulsory laws seem to have an effect on overall years of educations: between 0.2 of a year to 0.4.
- Independence: are they as good as random? they are as long as change in compulsory laws are unrelated to potential earnings in each state.
  - In this study compulsory laws where more quickly and more strictly adopted in the northern states relative to southern states. State specific trends could invalidate independence. Additionally compulsory laws grew over time but so so did economic progress.
- Controlling for state and year fixed effect could address this. And turn our IV estimate into an ID+DD estimate.
- Similarly to the MLDA DD study, here they authors can also control for lack of parallel trends.

Compulsory Schooling and Earnings: Results

- First Stage , Reduced Form and Second Stage (2SLS) estimates
- Three instruments.
- Column 1: all relevant with 9th grade the most relevant (think who are the compliers)
- Column 3 suggests strong estimates
- After controlling for state specific trends, FS and RF effects disappear.
- 2SLS are large but very noisy (denominator in LATE is close to zero)
- After accounting for state trends, the instrument becomes irrelevant!

Table 6.3
Returns to schooling using child labor law instruments

	Dependent variable			
	Years of schooling		Log weekly wages	
	(1)	(2)	(3)	(4)
A. First-stage and reduced-form estimates				
Child labor law req. 7 years	.166 (.067)	−.024 (.048)	.010 (.011)	−.013 (.011)
Child labor law req. 8 years	.191 (.062)	.024 (.051)	.013 (.010)	.005 (.010)
Child labor law req. 9 years or more	.400 (.098)	.016 (.053)	.046 (.017)	.008 (.014)
B. Second-stage estimates				
Years of education			.124 (.036)	.399 (.360)
State of birth dummies × linear year of birth trends	No	Yes	No	Yes

Notes: This table shows 2SLS estimates of the returns to schooling using as instruments three dummies indicating the years of schooling required by child labor laws as a condition for employment. Panel A reports first-stage and reduced-form estimates controlling for year and state of birth effects and for census year dummies. Columns (2) and (4) show the results of adding state-specific linear trends to the list of controls. Panel B shows the 2SLS estimates of the returns to schooling generated by the first-stage and reduced-form estimates in panel A. Sample size is 722,343. Standard errors are reported in parentheses.

Study 3 (IV): Quarter of Birth and Schooling 1/2

- In the US, children must start kindergartner the year they turn 5.
- School years starts in August/September.
- Most states require attendance to school at least until the children turns 16 (some states require 17 and 18).
- This institutional rules introduce quasi-random variation in schooling.

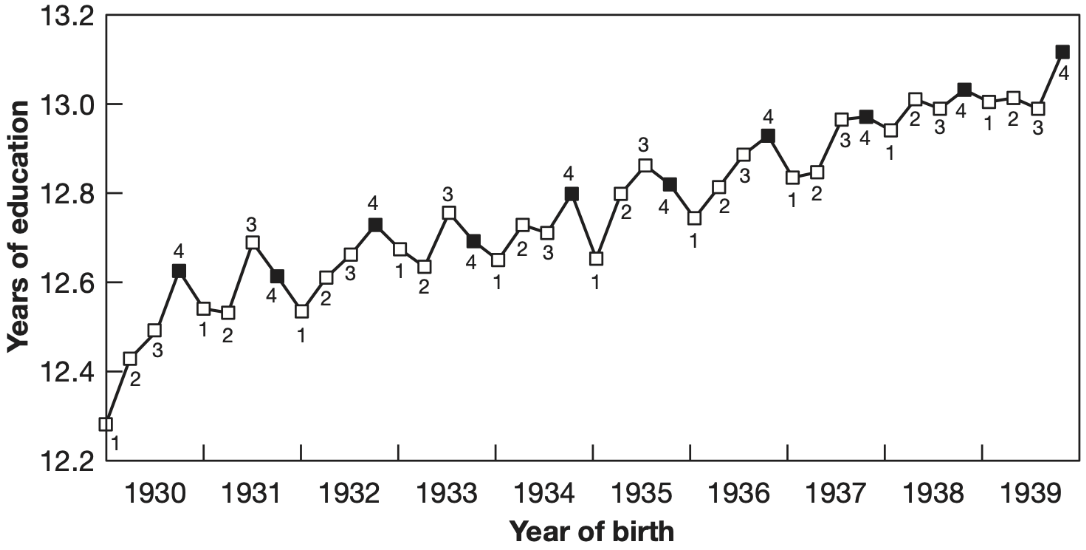
Study 3 (IV): Quarter of Birth and Schooling 2/2

- For example:
  - Jae, born on January 1st, enters kindergartner at age 5 years and 8 months (5.7 years).
  - Dante, born on December 1st, enters kindergartner at age 4 years and 9 months (4.8 years).
  - Let's assume that both want/have to drop out as early as possible:
  - Jae can leave school at the beginning of 10th grade (age 16).
  - Dante can leave school after starting 11th grade (age 16).
  - Because of (random) birth date, Dante gets about one additional year of schooling.
- The study that uses this instrument uses census data, with only records quarter of birth (QOB), hence this is the instrument (instead of date of birth).

Assessing the QOB Instrument

- Relevant: Figure 6.1 suggest yes.
- Independent: Does the season of birth correlates with potential earnings? Surprisingly: maybe. Other studies have shown how maternal schooling peaks in the second quarter. This could introduce OVB in the IV analysis.
- Exclusion restriction: QOB only affects earnings through additional schooling. Might not work if systematically younger children would perform worse in the classroom.

FIGURE 6.1
The quarter of birth first stage



Notes: This figure plots average schooling by quarter of birth for men born in 1930–1939 in the 1980 U.S. Census. Quarters are labeled 1–4, and symbols for the fourth quarter are filled in.

Results 1/2

- First stage?
- Reduce form?
- LATE?
- Who are compliers?

Table 6.4
IV recipe for an estimate of the returns to schooling using a single quarter of birth instrument

	Born in quarters 1–3	Born in quarter 4	Difference
Log weekly wage	5.8983	5.9051	.0068
			(.0027)
Years of education	12.7473	12.8394	.0921
			(.0132)
IV estimate of the returns to schooling			.074
			(.028)

Notes: Sample size is 329,509. Standard errors are reported in parentheses.

Results 1/2

- First stage: $\phi = \mathbf{0.092}$
- Reduce form: $\rho = \mathbf{0.0068}$
- LATE: $\lambda = \frac{0.0068}{0.092} = \mathbf{0.074}$
- Who are compliers: Individuals who only stay in school if required by age, and that drop out when allowed by age.

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Years of education	12.7473	12.8394	.0921
			(.0132)
IV estimate of the returns to schooling			.074
			(.028)

Notes: Sample size is 329,509. Standard errors are reported in parentheses.

Results 2/2

- OLS estimate produces a return to schooling of 7.1% in this sample.
- Simple IV with a binary for fourth quarter yields a 7.5%.
- The estimated coefficient doesn't change much when adding year of birth binaries.
- The effect grows and becomes more precise after instrumenting the other quarters too (3 quarter binaries): 10.5%.

Table 6.5
Returns to schooling using alternative quarter of birth instruments

	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	2SLS (5)
Years of education	.071	.074	.071	.075	.105
	(.0004)	(.028)	(.0004)	(.028)	(.020)
First-stage F-statistic		48		47	33
Instruments	None	Quarter 4	None	Quarter 4	3 quarter dummies
Year of birth controls	No	No	Yes	Yes	Yes

Notes: This table reports OLS and 2SLS estimates of the returns to schooling using quarter of birth instruments. The estimates in columns (3)–(5) are from models controlling for year of birth. Columns (1) and (3) show OLS estimates. Columns (2), (4), and (5) show 2SLS estimates using the instruments indicated in the third row of the table. F-tests for the joint significance of the instruments in the corresponding first-stage regression are reported in the second row. Sample size is 329,509. Standard errors are reported in parentheses.

Study 4 (RDD): Degree Completion and Earnings 1/2

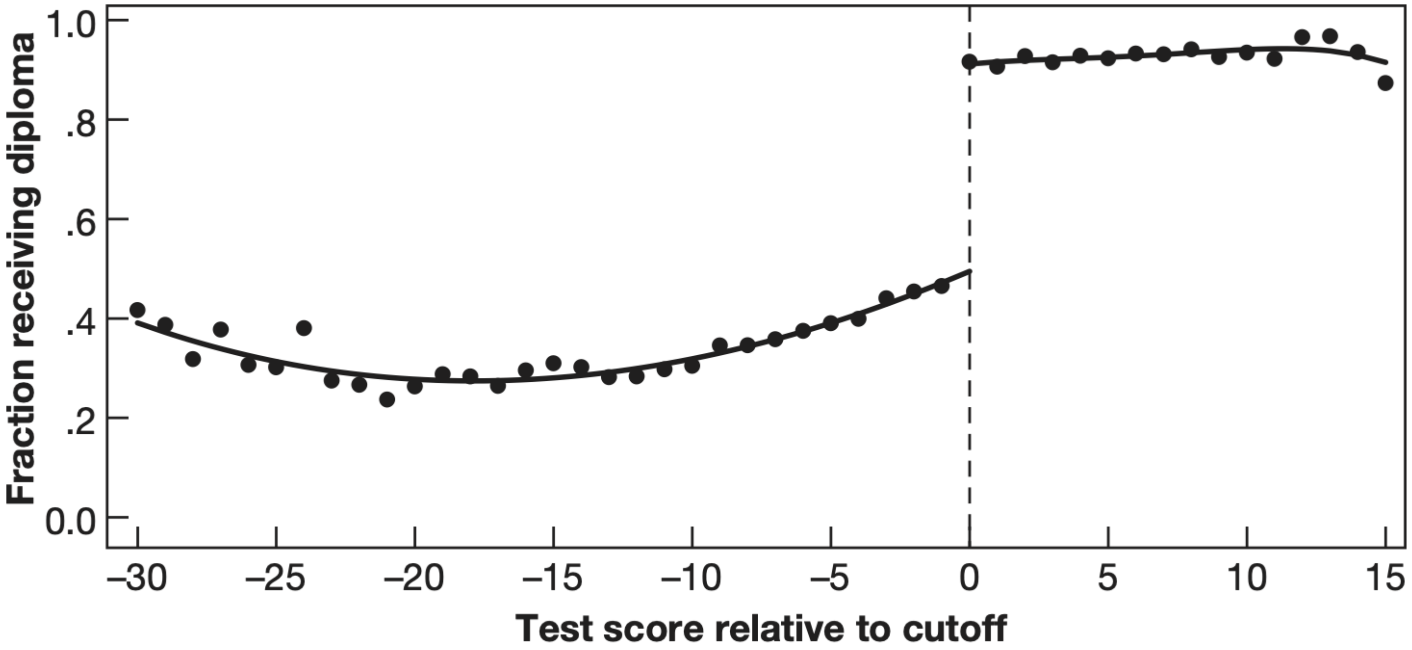
- Throughout these studies we have been assuming that one additional year yields similar return independent of degree completion (same to gain a year from 10 to 11 than from 11 to 12).
- This assumes that there is no Degree/Sheepskin Effect (sheepskin was the original material of diplomas).
- To test the existence of Sheepskin effect in high school, this study compares individuals with and without high school.
- To address selection bias/OVB it uses and RDD design for a "last chance" graduation exam from high school in Texas.

Study 4 (RDD): Degree Completion and Earnings 2/2

- Outcome: Annual earnings 7-11 years after high school.
- Treatment: Graduating high school.
- Instrument: binary variable that takes the value of 1 if score is above the passing cutoff, and 0 otherwise.

Results: First Stage

Figure 6.3
Last-chance exam scores and Texas sheepskin



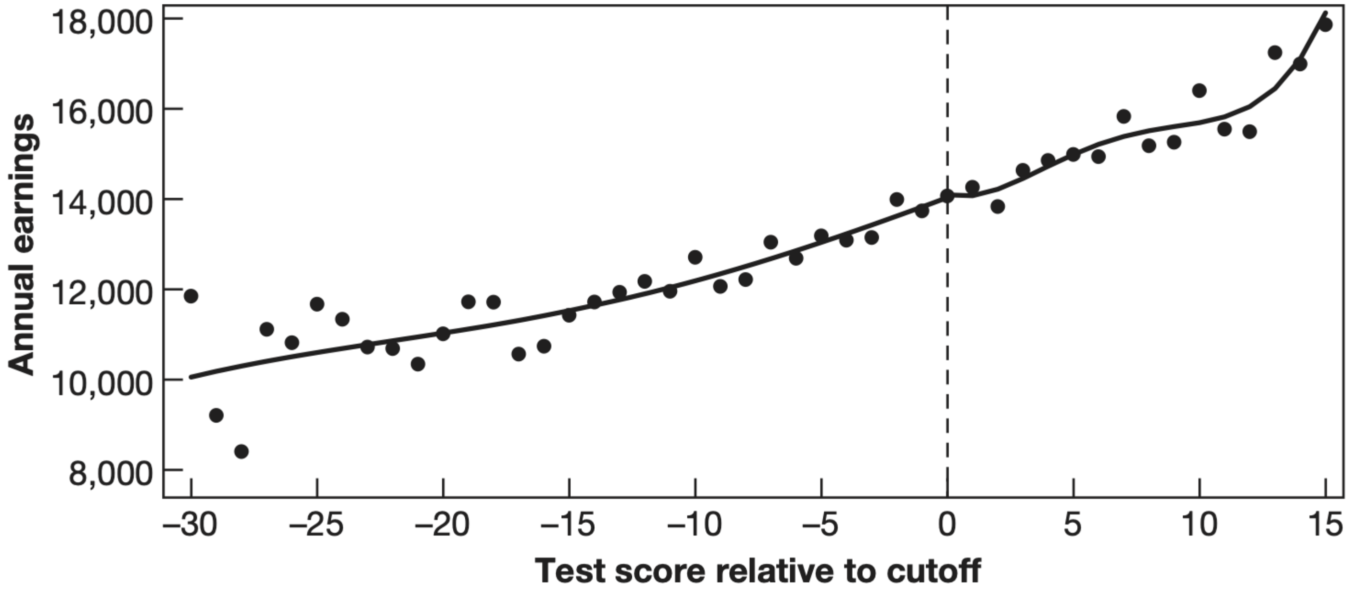
Notes: Last-chance exam scores are normalized relative to passing thresholds. Dots show average diploma receipt conditional on each score value. The solid lines are fitted values from a fourth-order polynomial, estimated separately on either side of the passing cutoff (indicated by the vertical dashed line).

Results: Reduce Form

- No Sheepskin for High School, among compliers, in Texas, for some (unspecified) period in time.

- Notice that MM uses this evidence to implicitly support their (wildly more general) claim that there is not Sheepskin effect *anywhere*.

FIGURE 6.4
The effect of last-chance exam scores on earnings

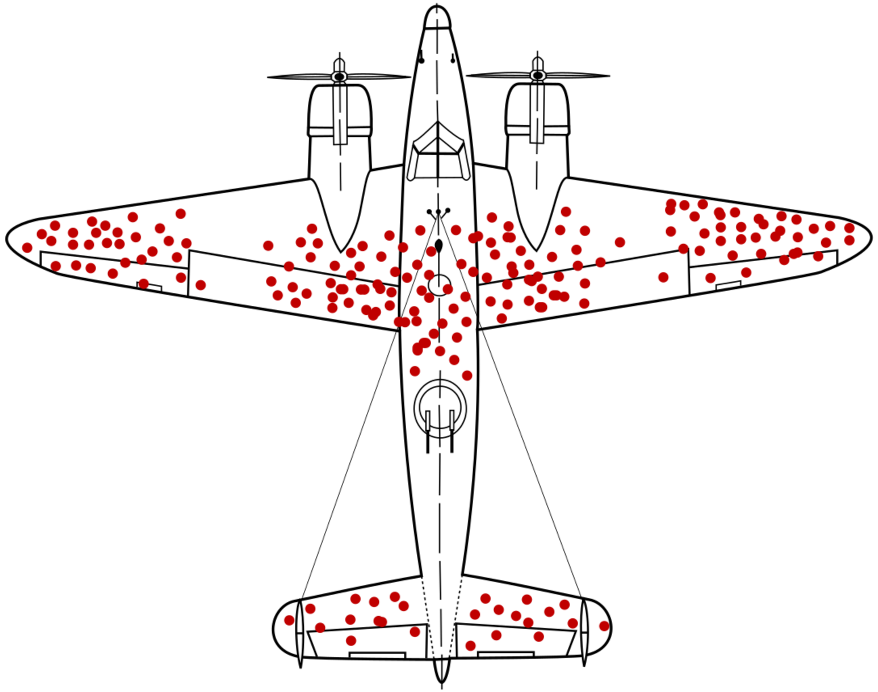


Notes: Last-chance exam scores are normalized relative to passing thresholds. Dots show average earnings conditional on each score value, including zeros for nonworkers. The solid lines are fitted values from a fourth-order polynomial, estimated separately on either side of the passing cutoff (indicated by the vertical dashed line).

Final Thoughts on Earnings and Education:

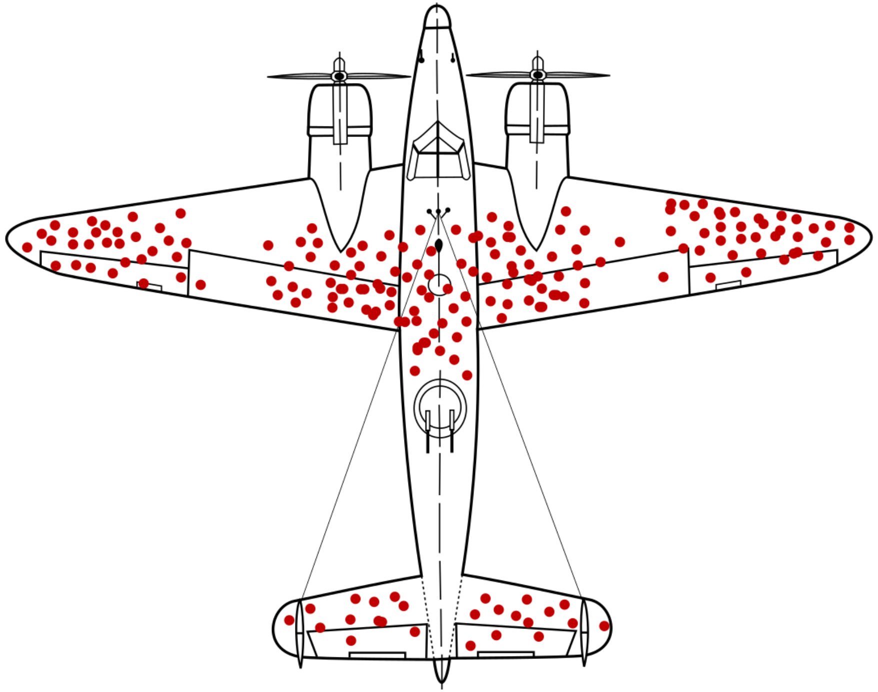
- Why so much interest in education to understand income (growth and inequality)?

- One suggestion: economists are a highly educated population that have a tremendous appreciation for education (regardless of what our models might suggest).



Final Thoughts on Earnings and Education:

- Moreover economists come from highly educated families at a much higher proportion to that of the overall population and of other graduate degrees.
- Hence when looking at what are the important factor that determine income, maybe we are extrapolating for what has been important in our personal experience.
- Maybe bringing in economists from different educational backgrounds could change the focus away from schooling and "ability" and closer to other determinants of earnings.



Acknowledgments

- MM