# Part II: Economics of Education

Jinpeng Shi

September 21, 2026

# Motivation

- Speaking of economics of education, what questions come your mind immediately?

# Motivation

- Speaking of economics of education, what questions come your mind immediately?
- Are private schools better than public schools?
- First, how to measure student performance?
- If really is better, is it because of the teachers, students, curriculum, or school resources?
- How to measure teacher performance?
- Are there any ways (public interventions) to improve student outcomes at public schools?
- ...

# Lecture Structure

- For the next 3 weeks, we will ask a series of questions:
- Topics from K-12 education to college admission, field of study,...
- Everything that is relevant until your labor market entry.
- We examine many important aspects that are essential during your human capital accumulation process.
- If we have extra time, we will end the topic with some economic models. (optional)

# 1. School Choice:

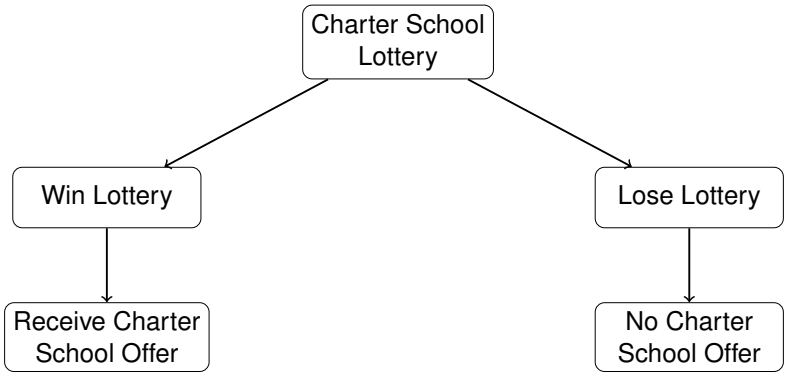
# Does Charter School Attendance Raise Your Academic Outcomes?

# Background

- Charter school is a type school that funded by government.
- They consists both primary and secondary institutions in U.S.
- They are independent from public school system.
- But they have contracts (charters) with local governments in terms of how to operate.
- Introduction to Instrumental Variable
- Waiting for Superman

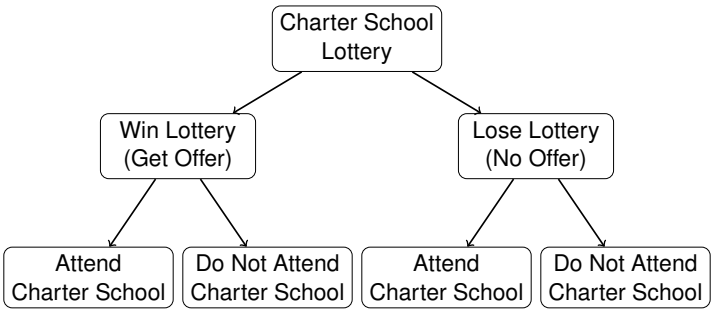
# A Recap

- Recall from the video, we have a lottery for charter school seats.
  - If you win the lottery, you get a seat offer to charter school.
  - If you lose, you don't get an offer.



# Charter School Attendance

- After receiving the lottery outcome, you decide whether you attend the charter school or not.
- You can **accept** or **reject** the seat offer.
- You can manage to attend charter schools through other means even if you lose the lottery.



# Types of Students

Depending on whether you received the seat offer and your school choice:

- Compliers: students attend charter school if win the lottery, do not attend charter school if lose the lottery.
- Always takers: students who always attend charter school regardless of their lottery outcome.
- Never takers: students who always go to other schools regardless of their lottery outcome.
- Defiers: students who attend charter school if they lose the lottery, and do not attend charter school if win the lottery.

# What IV (Instrumental Variable) Can Identify?

- Compliers' school choice are affected by the instrument (lottery results).
- Thus, IV can only identify the causal effect of charter school attendance on student outcomes for **compliers**.
- For an IV to be valid, we are assuming **no defiers** in the student population.
  - It is **unlikely** that lottery winning induce student treatment status change like defiers.
- We call this **local average treatment effect**.

# LATE vs ATT vs ATE

- LATE: local average treatment effect, the average treatment effect on compliers.
- ATT: average treatment effect on treated.
  - Note that both compliers and always-takers are treated.
- ATE: average treatment effect, the entire student poplation.
  - when can we calculate this effect: if we observe both potential outcomes for each individual.

# Selection Bias in Charter School Attendance

- Note that the lottery outcome is randomly assigned, but not the attendance choice.
- If we run the following regression, the result might be biased:

$$\text{Student Outcome}_i = \beta_0 + \beta_1 \text{Charter School Attendance}_i + \mu_i$$

- Because students who enroll in charter school may be different to begin with.
- We call this equation as **second stage**.

# First Stage

- Thus, we use charter school seat offer as an **instrument ($z$)**.
- The first stage captures the effect of lottery winning of charter school attendance:

$$\text{Charter School Attendance}_i = \gamma_0 + \gamma_1 \text{Lottery Winning}_i + \nu_i$$

- When you cannot randomly assign people to treatment (charter school attendance), you randomly assign students to instrument (lottery winning) that changes the status of treatment.

# What Makes a Valid Instrument?

- Relevance condition: the instrument must be **strong**. (a strong first stage)
- Exclusion restriction: the instrument must only influence the outcome through the channel of endogenous variable (to be instrumented).
  - In other words, lottery winning can't affect student outcomes through other factors, **only charter school seat offer**.
- Empirically, it is always hard to prove your exclusion restriction to be valid.
- Can you think of a violation to exclusion restriction?

# Reduced Form

- So, what is the causal effect of charter school attendance on student outcomes using instrumental variable approach?

$$\text{Student Outcome}_i = \alpha_0 + \alpha_1 \text{Lottery Winning}_i + \varepsilon_i$$

- Recall that:

Effect of lottery winning on charter school attendance $\times$ Effect of charter school attendance on student outcomes $=$ Effect of lottery winning on student outcomes

- Thus,

$$\beta_1 = \frac{\alpha_1}{\gamma_1}$$

- $\beta_1$ was biased initially, but not anymore.
- Empirically, we should also add a set of controls in regression.

# The Causal Effect of Lecture Attendance Redux

Does your attendance to today's lecture increase your final grade on this course?

- Suppose we can't randomly assign students to today's lecture,
- We can take a compromise by running the following regression:

$$\text{Final Grade}_i = \beta_0 + \beta_1 \text{Attendance}_i + \gamma \text{Controls} + \varepsilon_i$$

- Is $\beta_1$ the causal effect of lecture attendance on final grade?

# Instrumental Variable

Now, suppose “Attendance” is an endogenous variable, since we cannot control motivation, prior academic background correctly.

- We can solve this problem by instrumenting the endogenous variable.
- We can find an instrument $z$ that is correlated to “Attendance” and only affects “Final Grade” through the channel of “Attendance”.

For an IV to be valid, we need two conditions:

- **Relevance Condition**: $z$ should be correlated to the endogenous variable
- **Exclusion Restriction**: $z$ affects “Final Grade” only through “Attendance”

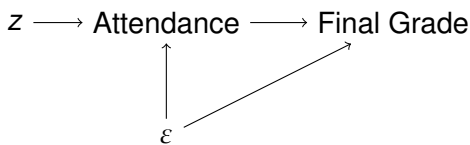
# IV Diagrams

Relevance condition: $z$ should be correlated with the endogenous variable

$$\text{Cov}(z, \text{Attendance}) \neq 0 \tag{1}$$

Exclusion restriction: $z$ only affects Final Grade through Attendance

$$\text{Cov}(z, \varepsilon) = 0 \tag{2}$$



# IV vs OVB

What if $\text{Cov}(z, \varepsilon) \neq 0$?

- Instrument affects outcome through other channels beyond Attendance.
- And it's affecting outcome through channels that were not included in our previous regression.
- Then this instrument is not valid:
  - Violation to exclusion restriction.
  - Omitted variable bias not solved by IV.
- Our estimates are **biased**!

# What could become a valid IV?

A simple nudge:

- Suppose I have a group of students that are comparable, I randomly select some of the students to send out an email to nudge them to attend the lecture.
- Whether students receive the email or not will serve as an indicator for the probability of attending today's lecture.

Do you believe this instrument? Why or why not?

# What could become a valid IV, cont?

A natural experiment:

- If today's lecture is live streaming, and an internet outage happened in certain dorms/regions of the city at the same time and only last until the end of the lecture,
- Whether having the internet outage will predict your attendance of today's lecture.
- And does not affect your future access to later live streaming lecture and course materials.

Do you believe this instrument? Why or why not?

# Do Highly Motivated Students Finish Stronger?

- We have been arguing for a long time:
  - more motivated students are more likely to do better in class.
- But how can we estimate the **causal effect** of motivation on your final class outcomes:

$$\text{Final Grade}_i = \beta_0 + \beta_1 \text{Motivation}_i + \gamma \text{Controls} + \varepsilon_i$$

- I can extract time spent on Canvas for each time you logged in.
- But we still suffer from measurement error.

# An Instrument for Motivation

- Suppose I add additional locks to course materials on Canvas:
  - You have to click or complete previous tasks in order to view the subsequent materials.
  - How many modules you unlocked serve as an indicator for your motivation.
- Do you believe the relevance condition?
- What about the exclusion restriction?

# Was Charter School Actually Effective?

# Charter School Project was Effective

According to Angrist et al. (2013)

- Boston and Lynn charter middle schools increase student achievement by:
  - $0.4\sigma$ per year in math;
  - $0.2\sigma$ per year in English Liberal Arts.
- Boston charter high schools increase student achievement by:
  - $0.3\sigma$ per year in math;
  - $0.2\sigma$ per year in English Liberal Arts.
- Research shows similar results for charter school projects in DC, New York and Chicago.

# Is That True Nationwide?

- However, most of these projects were developed in **urban** setting
- Do we expect to find similar results in **nonurban** setting?
  - Angrist et al. (2011) found **little** evidence of achievement gains outside the **high-poverty** urban areas.
  - Some estimates in nonurban setting show significant **negative** effects.
  - Gleason et al. (2010) found similar results from a multi-state study.
- So, what happened?

# Lottery Estimates

TABLE 4—LOTTERY ESTIMATES OF CHARTER EFFECTS

| Subject | All charter schools: First stage (1) | All charter schools: 2SLS (2) | Urban charter schools: First stage (3) | Urban charter schools: 2SLS (4) | Nonurban charter schools: First stage (5) | Nonurban charter schools: 2SLS (6) |
|---|---|---|---|---|---|---|
| *Panel A. Middle school* | | | | | | |
| Math | 1.02*** | 0.213*** | 1.03*** | 0.321*** | 1.01*** | −0.123*** |
| | (0.040) | (0.028) | (0.051) | (0.031) | (0.074) | (0.047) |
| N | 16,543 | | 11,941 | | 4,602 | |
| ELA | 1.02*** | 0.075*** | 1.04*** | 0.146*** | 1.00*** | −0.144*** |
| | (0.040) | (0.025) | (0.051) | (0.028) | (0.074) | (0.039) |
| N | 16,285 | | 11,649 | | 4,636 | |
| *Panel B. High school* | | | | | | |
| Math | 0.565*** | 0.273*** | 0.508*** | 0.339*** | 1.13*** | −0.020 |
| | (0.085) | (0.071) | (0.090) | (0.077) | (0.197) | (0.071) |
| N | 4,050 | | 3,519 | | 531 | |
| ELA | 0.565*** | 0.206*** | 0.508*** | 0.264*** | 1.14*** | −0.046 |
| | (0.086) | (0.060) | (0.090) | (0.067) | (0.196) | (0.059) |
| N | 4,103 | | 3,567 | | 536 | |

# What Explains the Urban Charter Premium?

- Urban schools typically come with **high minority, high poverty** enrollment.
- See Table 5
- Does this mean urban and nonurban are targeting different student populations?
- Or it's simply the urban programs were more effective?
- Can we distinguish these two: **different demographics vs heterogeneous treatment effect**?

# Blinder-Oaxaca Decomposition

- We decompose the observed differences in treatment effect into:
  - differences in student population demographics;
  - and differences within the same demographics.

TABLE 7—DECOMPOSING DIFFERENCES IN MIDDLE SCHOOL CHARTER EFFECTS

| | | Decomposition 1 (urban loading) | | Decomposition 2 (nonurban loading) | |
|---|---|---|---|---|---|
| Subject | Urban versus nonurban difference in effects (1) | Due to diffs in cov. levels (2) | Due to diffs in cov. specific effects (3) | Due to diffs in cov. levels (4) | Due to diffs in cov. specific effects (5) |
| Math | 0.633*** | 0.322*** | 0.312*** | 0.250 | 0.383* |
| | (0.079) | (0.075) | (0.094) | (0.163) | (0.200) |
| N | 5,731 | | | | |
| ELA | 0.389*** | 0.184** | 0.205** | 0.197 | 0.192 |
| | (0.073) | (0.077) | (0.095) | (0.146) | (0.180) |
| N | 5,734 | | | | |

# No Excuse Doctrine

- Around half of the urban advantage was due to student demographics.
- So what makes the urban schools more effective?
- No Excuse instructional approach: See Table 8.
  - use uniforms.
  - cold call in the classroom.
  - use math drills.
  - extended math instructional time.
  - reward system.
  - ...

# 2. How to Measure Teacher Performance?

# Motivation

Recall our findings from charter school attendance:

- Charter school program was effective in raising student achievement (e.g., test scores).
- However, the effect mainly came from urban charter school program.
- And we show, half of the urban-nonurban treatment effect differences were caused by differences in student population demographics.
- The remaining half came from different instructional method adopted by urban charter school programs.

Q: How can we measure teacher quality?

# Teacher Value-Added

The most common method to evaluate teacher performance:

- Their impacts on student test scores $\Rightarrow$ **Value-added (VA)**.

The debate:

- Do differences in test score gains reflect the **causal effect** of teachers or **selection bias** (student sorting)?
- Do teachers who raise the test scores improve students' **long run outcomes** or simply better at **teaching to the test**?

# The Causal Impact of Teachers

The gap:

- Previous research found conflicting results on VA bias.

Q:

- Estimate the degree of bias in teacher VA.

Policy relevance:

- Biased estimates of VA may reward or penalize teachers systematically.

# Data

Test scores and teacher assignments in grade 3-8 from a urban school district in US.

- Including 2.5M students.
- 18M tests of Math and English spanning 1989-2009.

US tax record:

- 90% matched to US tax records spanning 1996-2011.
- Parental characteristics:
  - household income;
  - retirement savings;
  - mother's age at child's birth.

# Constructing VA Estimates

- Define students' test scores in each subject in class (c) at school (s) in year $t$ with assigned teacher (j): $A_{ijcst}$
- We know information on students' characteristics from the school districts: covariates $\mathbb{X}_{ijcst}$
  - Prior year's test scores;
  - Students' age, race, gender;
  - Indicator for special education, grade repetition, lagged suspensions and absences;
  - ...
- We regress students' test scores on observed characteristics:

$$A_{ijcst} = \beta \mathbb{X}_{ijcst} + \nu_{ijcst}$$

# Constructing VA Estimates, cont

Now, we residualize the test scores on observed students characteristics:

$$\tilde{A}_{ijcst} = A_{ijcst} - \beta \mathbb{X}_{ijcst}$$

We can decompose our test score residuals into three parts:

- teacher value-added;
- class-level shocks;
- idiosyncratic shocks/ random noise.

$$\tilde{A}_{ijcst} = \mu_{jt} + \theta_c + \varepsilon_{ijcst}$$

The last two terms are unobserved errors in test scores that are uncorrelated to teacher quality.

# Constructing VA Estimates, cont

Now, let's aggregate student-level test score residuals to the teacher level:

$$\bar{A}_{jt} = \sum \tilde{A}_{ijcst}$$

- Note that we do not observe $\mu_{jt}$!
- And if we use test score residuals in year $t$ as a measure of teacher value-added, then it is **biased**:
  - It includes other shocks that are unrelated to teacher quality.

# Constructing VA Estimates, cont

So we use aggregated test scores in other years to predict teacher value-added:

- For instance, suppose we have aggregated test scores in the following year:

| Year | 2001 | 2002 | 2003 | 2004 | 2005 |
|---|---|---|---|---|---|
| $\bar{A}_{jt}$ | 0.20 | 0.10 | 0.30 | 1.2 | 0.8 |

- If year 2003 is our target, then we use the rest of four years to construct VA estimates.
- Principle: the closer to the target year, the more predictive of past/future test score residuals.
- We them construct covariance matrix to estimate the best linear predictor of test score residuals.

# Constructing VA Estimates, cont

It is equivalent to running the following regression:

$$\bar{A}_{j2003} = \alpha + \beta_1 \bar{A}_{j2001} + \beta_2 \bar{A}_{j2002} + \beta_4 \bar{A}_{j2004} + \beta_5 \bar{A}_{j2005} + e_{j2003}$$

- Then we use our estimates to construct VA estimates in year 2003.
- We replicate this procedure for every other year.
- We are assuming other shocks in other years' test score residuals are **uncorrelated** to teacher quality in year $t$.

# Definition of VA Bias

A hypothetical experiment:

- Suppose we could randomly assign teachers within the public school district;
- What would be the effect of teacher quality on student test score residual gains?

$$\tilde{A}_{ijcst} = \lambda \hat{\mu}_{jt} + \psi_{ijcst}$$

- $\lambda$ represents the true causal effect.
- So bias is defined as:

$$B = 1 - \lambda$$

- If $B = 0$, there is no bias in VA estimates.

# Results

- VA estimate is an **unbiased** estimate of teacher quality.
- There is little evidence of student sorting:
  - sorting more likely to appear between schools rather than within school.

TABLE 3—ESTIMATES OF FORECAST BIAS USING PARENT CHARACTERISTICS AND LAGGED SCORES

| | Score in year $t$ (1) | Pred. score using parent chars. (2) | Score in year $t$ (3) | Pred. score using year $t-2$ score (4) |
|---|---|---|---|---|
| Teacher VA | 0.998 | 0.002 | 0.996 | 0.022 |
| | (0.0057) | (0.0003) | (0.0057) | (0.0019) |
| Parent chars. controls | | | X | |
| Observations | 6,942,979 | 6,942,979 | 6,942,979 | 5,096,518 |

# How to Interpret VA Estimates?

Now, we get an unbiased estimate of teacher quality based on students' test scores:

- Does that translate to effect in the long run;
- Or just better at teaching students to test?

# The Long-run Impact of Teacher Quality

To estimate the effect of VA on outcomes, we first look at the determination of outcomes:

$$Y_{ijcst} = \beta^Y \mathbb{X}_{ijcst} + \nu_{ijcst}$$

We then residualize outcome $Y$ as we did when constructing VA estimates:

$$\tilde{Y}_{ijcst} = Y_{ijcst} - \beta^Y \mathbb{X}_{ijcst}$$

So, what's long run impact of VA on outcomes?

$$\tilde{Y}_{ijcst} = a + \kappa \mathring{\mu}_{jt} + \eta_{ijcst}$$

- $\kappa$ is our parameter of interest;
- $\mathring{\mu}_{jt}$ is standardized teacher VA.

# Effect on College Attendance

TABLE 2— IMPACTS OF TEACHER VALUE-ADDED ON COLLEGE ATTENDANCE

| | College at age 20 (%) (1) | College at age 20 (%) (2) | College at age 20 (%) (3) | College quality at age 20 ($) (4) | College quality at age 20 ($) (5) | College quality at age 20 ($) (6) | High quality college (%) (7) | Four or more years of college, ages 18–22 (%) (8) |
|---|---|---|---|---|---|---|---|---|
| Teacher VA | 0.82 (0.07) | 0.71 (0.06) | 0.74 (0.09) | 298.63 (20.74) | 265.82 (18.31) | 266.17 (26.03) | 0.72 (0.05) | 0.79 (0.08) |
| Mean of dep. var. | 37.22 | 37.22 | 37.09 | 26,837 | 26,837 | 26,798 | 13.41 | 24.59 |
| Baseline controls | X | X | X | X | X | X | X | X |
| Parent chars. controls | | X | | | X | | | |
| Lagged score controls | | | X | | | X | | |
| Observations | 4,170,905 | 4,170,905 | 3,130,855 | 4,167,571 | 4,167,571 | 3,128,478 | 4,167,571 | 3,030,878 |

# Effect on Earnings

TABLE 3—IMPACTS OF TEACHER VALUE-ADDED ON EARNINGS

| | Earnings at age 28 ($) | Earnings at age 28 ($) | Earnings at age 28 ($) | Working at age 28 (%) | Total income at age 28 ($) | Wage growth ages 22–28 ($) |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Teacher VA | 349.84 | 285.55 | 308.98 | 0.38 | 353.83 | 286.20 |
| | (91.92) | (87.64) | (110.17) | (0.16) | (88.62) | (81.86) |
| Mean of dep. var. | 21,256 | 21,256 | 21,468 | 68.09 | 22,108 | 11,454 |
| Baseline controls | X | X | X | X | X | X |
| Parent chars. controls | | X | | | | |
| Lagged score controls | | | X | | | |
| Observations | 650,965 | 650,965 | 510,309 | 650,965 | 650,965 | 650,943 |

# Effect on Other Outcomes

TABLE 4—IMPACTS OF TEACHER VALUE-ADDED ON OTHER OUTCOMES (*percent*)

| | Teenage birth (1) | Percent college grad in zip at age 28 (2) | Have 401(k) at age 28 (3) |
|---|---|---|---|
| Teacher VA | −0.61 | 0.25 | 0.55 |
| | (0.06) | (0.04) | (0.16) |
| Mean of dep. var. | 13.24 | 13.81 | 19.81 |
| Baseline controls | X | X | X |
| Observations | 2,110,402 | 468,021 | 650,965 |

# 3. Returns to Years of Schooling:

# Does Compulsory Schooling Law Increase Your Future Earnings?

# The Mincer Equation

Jacob Mincer (1974) introduced a landmark “human capital earnings function”,

$$\log Y = \log Y_0 + rS + \beta_1 X + \beta_2 X^2$$

- $Y$ is earnings;
- $Y_0$ is the earnings for someone with no education and experience;
- $S$ is years of schooling;
- $X$ is years of potential labor market experience.

# The Mincer Equation: Regression

$$\log Y_i = \alpha + rS_i + \beta_1 X_i + \beta_2 X_i^2 + \varepsilon_i$$

- $\alpha$ is the constant;
- $\varepsilon$ is the error term;
- $r$ is the return to education;
- $\beta_0$, $\beta_1$ are returns to work experience.

Q: Can we claim $r$ as the **causal effect** of returns to human capital investment?

# A Signaling Explanation

Degree as a signal in the labor market:

- People with higher **innate ability** tend to complete more years of schooling.
- The same group tends to do well in terms of labor market reward.
- Higher innate ability workers complete more years of schooling to send a signal about their ability in the job market.
- Thus, returns to education is not a reward to human capital investment, rather a signaling problem.

# Potential Bias in Mincer Regression

Two competing economic models: **human capital investment** vs **signaling**.

1. If schooling is mostly signaling:
   - Mincer regression suffers from Omitted Variable Bias (OVB).
   - We should expect the return to education **go down** once we controlled innate ability $\Rightarrow$ **overestimation**;
2. If schooling is mostly human capital investment:
   - The parsimonious Mincer regression may suffer from measurement error.
   - We should expect that we **underestimate** the return to education.
   - This is called **attenuation bias**.

Q: Can we find a way to causally estimate the return to education? Which model fits the empirical evidence better?

# Compulsory Schooling Laws

Angrist and Krueger (1991):

- Most states require school enrollment on the **calendar year** when students turn 6.
- With birthday cutoffs (thus, no rolling admissions), students who were born in the different quarters of the year enter school at different age.
- Born in the **fourth** quarter $\Rightarrow$ **shortly before 6**;
- Born in the **first** quarter $\Rightarrow$ **around** $6\frac{1}{2}$.
- Students cannot drop out until they reach age 16.

# Quarter of Birth: An Example

Consider two students: Alex and Amy,

- Alex was born on December 31st, 2005:
  - The calendar year he turned 6 is year 2011;
  - According to state law, he enter the first grade on September, 2011.
  - He can only drop out after grade 12.
- Amy was born on January 1st, 2005:
  - The calendar year she turned 6 is year 2011;
  - According to state law, she enter the first grade on September, 2011.
  - She can only drop out after grade 11.

# Quarter of Birth: An Example, cont

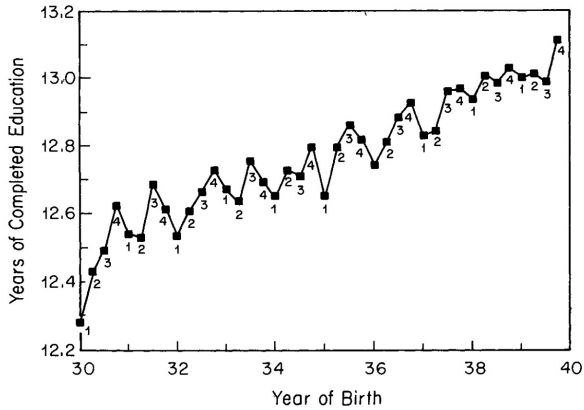
Consider two students: Alex and Laura,

- Alex was born on December 31st, 2005:
  - The calendar year he turned 6 is year 2011;
  - According to state law, he enter the first grade on September, 2011.
  - He can only drop out after grade 12.
- Laura was born on January 1st, 2006:
  - The calendar year she turned 6 is year 2012;
  - According to state law, she enter the first grade on September, 2012.
  - She can only drop out after grade 11.

Even if their dates of birth are only one day apart, they enter school at different year, and compulsory schooling law affects them differently.

# Quarter of Birth

- If students are affected by compulsory schooling laws, we would expect different quarters of birth end up at different grades at legal drop out age



# Quarter of Birth beyond Compulsory Schooling Laws?

TABLE I
THE EFFECT OF QUARTER OF BIRTH ON VARIOUS EDUCATIONAL OUTCOME VARIABLES

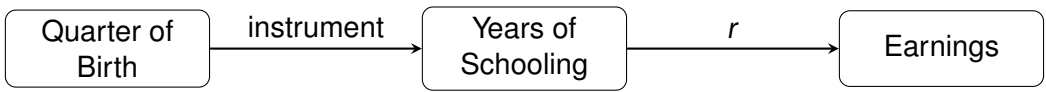
| Outcome variable | Birth cohort | Mean | Quarter-of-birth effect[a] I | II | III | $F$-test[b] [$P$-value] |
|---|---|---|---|---|---|---|
| Total years of education | 1930–1939 | 12.79 | −0.124 | −0.086 | −0.015 | 24.9 |
| | | | (0.017) | (0.017) | (0.016) | [0.0001] |
| | 1940–1949 | 13.56 | −0.085 | −0.035 | −0.017 | 18.6 |
| | | | (0.012) | (0.012) | (0.011) | [0.0001] |
| High school graduate | 1930–1939 | 0.77 | −0.019 | −0.020 | −0.004 | 46.4 |
| | | | (0.002) | (0.002) | (0.002) | [0.0001] |
| | 1940–1949 | 0.86 | −0.015 | −0.012 | −0.002 | 54.4 |
| | | | (0.001) | (0.001) | (0.001) | [0.0001] |
| Years of educ. for high school graduates | 1930–1939 | 13.99 | −0.004 | 0.051 | 0.012 | 5.9 |
| | | | (0.014) | (0.014) | (0.014) | [0.0006] |
| | 1940–1949 | 14.28 | 0.005 | 0.043 | −0.003 | 7.8 |
| | | | (0.011) | (0.011) | (0.010) | [0.0017] |
| College graduate | 1930–1939 | 0.24 | −0.005 | 0.003 | 0.002 | 5.0 |
| | | | (0.002) | (0.002) | (0.002) | [0.0021] |
| | 1940–1949 | 0.30 | −0.003 | 0.004 | 0.000 | 5.0 |
| | | | (0.002) | (0.002) | (0.002) | [0.0018] |
| Completed master's degree | 1930–1939 | 0.09 | −0.001 | 0.002 | −0.001 | 1.7 |
| | | | (0.001) | (0.001) | (0.001) | [0.1599] |
| | 1940–1949 | 0.11 | 0.000 | 0.004 | 0.001 | 3.9 |
| | | | (0.001) | (0.001) | (0.001) | [0.0091] |
| Completed doctoral degree | 1930–1939 | 0.03 | 0.002 | 0.003 | 0.000 | 2.9 |
| | | | (0.001) | (0.001) | (0.001) | [0.0332] |
| | 1940–1949 | 0.04 | −0.002 | 0.001 | −0.001 | 4.3 |
| | | | (0.001) | (0.001) | (0.001) | [0.0050] |

# Quarter of Birth as an Instrument

Recall Mincer regression:

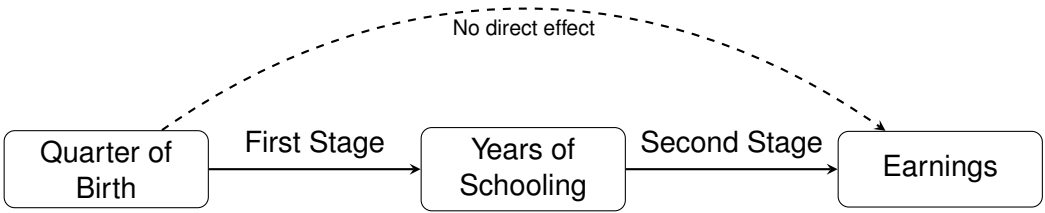
$$\log Y_i = \alpha + rS_i + \beta_1 X_i + \beta_2 X_i^2 + \varepsilon_i$$

- We do not control innate ability.
- Our estimate of $r$ is **biased**.
- So we use quarter of birth as an instrument for years of schooling.



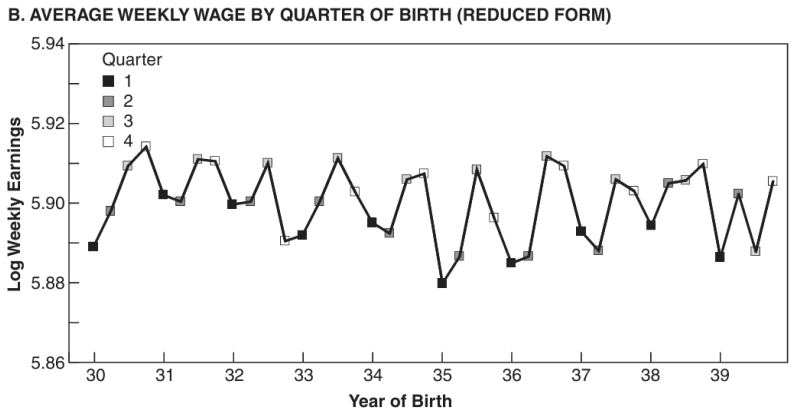
# IV Validity

- **Relevance condition**: compulsory schooling laws force students who were born in different quarters of birth completing different years of schooling
- **Exclusion restriction**: quarter of birth is unlikely to be correlated to innate ability, motivation, and family connection, etc.



# Reduced Form Correlation

- Seasonal pattern of log weekly earnings.



# Results

- Estimates are similar to conventional estimates.
- Little evidence on omitted variable bias.

TABLE IV

OLS AND TSLS ESTIMATES OF THE RETURN TO EDUCATION FOR MEN BORN 1920–1929: 1970 CENSUS[a]

| Independent variable | (1) OLS | (2) TSLS | (3) OLS | (4) TSLS | (5) OLS | (6) TSLS | (7) OLS | (8) TSLS |
|---|---|---|---|---|---|---|---|---|
| Years of education | 0.0802 | 0.0769 | 0.0802 | 0.1310 | 0.0701 | 0.0669 | 0.0701 | 0.1007 |
| | (0.0004) | (0.0150) | (0.0004) | (0.0334) | (0.0004) | (0.0151) | (0.0004) | (0.0334) |
| Race (1 = black) | — | — | — | — | 0.2980 | −0.3055 | −0.2980 | −0.2271 |
| | | | | | (0.0043) | (0.0353) | (0.0043) | (0.0776) |
| SMSA (1 = center city) | — | — | — | — | 0.1343 | 0.1362 | 0.1343 | 0.1163 |
| | | | | | (0.0026) | (0.0092) | (0.0026) | (0.0198) |
| Married (1 = married) | — | — | — | — | 0.2928 | 0.2941 | 0.2928 | 0.2804 |
| | | | | | (0.0037) | (0.0072) | (0.0037) | (0.0141) |
| 9 Year-of-birth dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| 8 Region of residence dummies | No | No | No | No | Yes | Yes | Yes | Yes |
| Age | — | — | 0.1446 | 0.1409 | — | — | 0.1162 | 0.1170 |
| | | | (0.0676) | (0.0704) | | | (0.0652) | (0.0662) |
| Age-squared | — | — | −0.0015 | −0.0014 | — | — | −0.0013 | −0.0012 |
| | | | (0.0007) | (0.0008) | | | (0.0007) | (0.0007) |
| $\chi^2$ [dof] | — | 36.0 [29] | — | 25.6 [27] | — | 34.2 [29] | — | 28.8 [27] |

# Human Capital Investment vs Signaling Revisited

- Our results in Table IV show **little** evidence of bias in Mincer regression.
- Thus, we are in favor of human capital investment hypothesis.
- One more year of schooling translates into 7% wage premium in the labor market.
- In 1970, men born in the first quarter of the year earned **0.7 percent lower** weekly wage and had completed **0.126 fewer** years of education than men born in the last three quarters of the year.

# IV Validity Revisited

Relevance condition:

- Bound et al. (1995) claim that quarter of birth is a weak instrument.
- This can be tested with test statistics in first stage.

Exclusion Restriction:

- Month of birth has been consistently found associated with later outcomes: health, education attainment, earnings, mortality.
- Could some other variables like family characteristics influence the pattern on season of birth?

# Season of Birth and Family Background

The consensus:

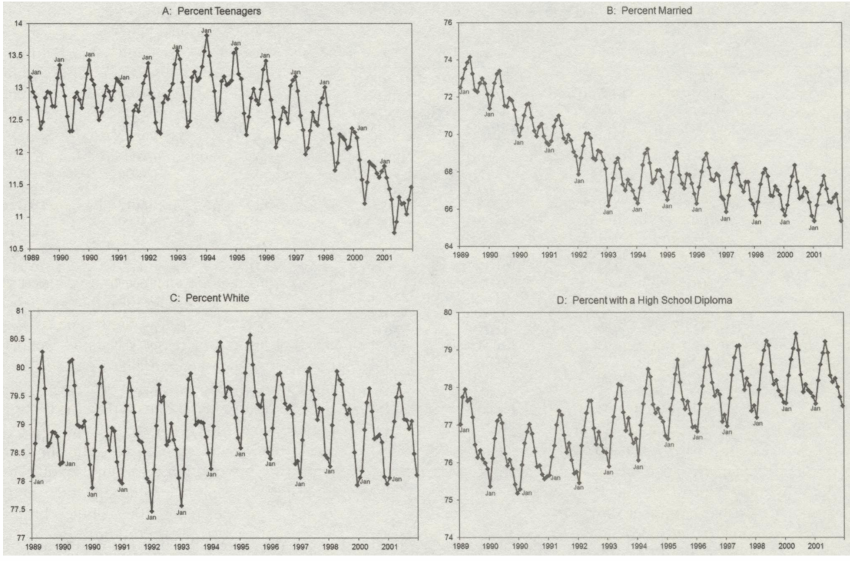
- One's birthday is unlikely to be correlated with personal attributes other than age at school entry. Hoogerheide et al. (2007), Card (1999).

Buckles and Hungerman (2013):

- Q: Whether the typical woman give birth in the winter looks **different** from the woman give birth at the other times of year?
- They find that women give birth in the winter:
  - younger, less educated, less likely to be married.

# Maternal Characteristics by Month

FIGURE 1.—MATERNAL CHARACTERISTICS BY MONTH, NATALITY FILES, 1989–2001




The sample for each figure includes all births in the Natality Detail Files from 1989 to 2001, for 52,041,054 observations.

# Season of Birth and Family Background, cont

Then they want to see whether variation in family background can account for variation in outcomes ascribed to season of birth.

- The relationship between season of birth and later outcomes are largely driven by **fertility patterns across socioeconomic groups**.
- The fact that family background correlates to both season of birth and later outcomes **violates** exclusion restriction!

# Why the Seasonal Pattern Exists?

- Maybe women from high socioeconomic status have strong preferences for nonwinter birth or better at timing birth away from winter;
- Maybe change in weather affect risky sexual behavior and such effect vary across socioeconomic groups.

The short answer:

- Seasonal fertility patterns are driven by women wanting a birth;
- One's season of birth is part of a fertility choice made by parents.
- This would make IV estimation even more problematic!

# IV Validity All Over Again

- The fact that fertility planning is part of the reason on seasonal birth patterns,
- It suggests compulsory schooling laws are less important reason for later outcomes related to season of birth.

What could we do?

- Control family background in the first stage.

# 4. Field of Study:

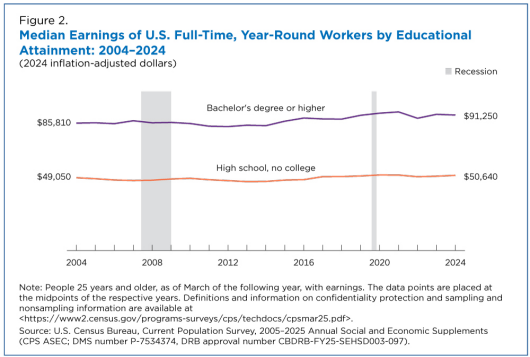
# Does Your Major Influence Your Earning Trajectory?

# Motivation

Last lecture, we show that one more year of schooling leads to 7% increase in labor market earnings:

- Naturally, we would expect people who earn higher educational degree to earn more in the labor market, on average.

- Here's the college vs non-college wage gap:



# Motivation, cont

However, even within the same level of years of schooling completed, we observe substantial wage gaps:

- Post-Secondary Employment Outcomes
- Collge Scoreboard
- Hamiliton Project

# Research Question

- Majority of young adults enroll in post-secondary education in OECD countries.
- They have to make decisions regarding field of study or college major when they enroll.
- And we observe sustained wage gaps across college majors.

Q:

- Does your major choice **causally** affect your earning trajectories?
- Is your major choice **more important** than where you attend your college?

# Selection Bias

- Suppose we want to estimate the causal effect of your college major on your future earnings:

$$\text{Earnings}_i = \alpha + \beta \text{Major}_i + \gamma X + \varepsilon_i$$

  - we include a set of controls: cohort, sex, race, region, institution, etc.

- our regression estimate may be **biased**!
- Individuals enroll in high earning jobs may have higher potential (skills, innate ability, network, etc.) to begin with.
- We need an apple to apple comparison.

# The Ideal Experiment

- In an ideal world, we would want to randomly assign students to different college majors, and measure their earnings 20 years later.
- That is not doable given ethic concerns.
- Can we think of some natural or quasi random experiment that are effectively random for college enrollment.

# Institutional Background in Norway: Kirkeboen et al. (2016)

- It covers almost all universities and colleges.
  - It has 4 public universities, and a number of public and private university colleges.
  - The majority of students attend public institutions.
  - Universities all offer a wide selection of fields.
  - While university colleges rarely offer fields like law, science, medicine and technology;
  - They tend to offer professional degrees like engineering, health, business and teaching.
  - There are generally no tuition fee for attending a postsecondary education.

# Centralized Admission Process in Norway

- Prospective students apply to a field and institution simultaneously.
- They rank up to 15 choices.
- Applicants are scored by a central organization based on their GPA.
- Applicants are ranked based on their score.
- Then places are then assigned in turn.

# The Timeline

- Each fall, the central agency decides on funding to each field at every institution.
- It effective determines the slots offered by institutions.
- The deadline to submit student ranked choices is mid April.
- They can adjust rankings until July.
- They cannot add new ones, but can drop them.
- Offers are made after ranking if final.

# Sequential Order Decision

We define a field of study in an institution as course:

- The highest ranked applicant receives an offer for her preferred course.
- The second highest ranked applicant receives and offer for her highest ranked course among the remaining courses.
- ...
- Repeat this procedure until slots run out, or applicants run out.

# Timeline Continued

- This procedure generates a first set of offers sent out in late July.
- Applicants have a week to accept the offer, remaining on the waiting list or withdraw from the application process.
- The slots remained after first round are then allocated in the second round in early August.
- It follows the same sequential offer procedure and only for those who remain on the waiting list.
- Students begin their study in mid August.

# More on Admission Process

- Students can only move up in the offer sequence.
- In the second round, it's either their first round offer, or an offer from a higher ranked course which they remain on the waiting list in the first round.
- If students want to change major or institution, they need to participate in next year's centralized admission process.

# The Effective Randomization

- For courses with excess demand, cutoffs are implemented.
- Students above a certain threshold are more likely to receive an offer they preferred than students who are marginally below the cutoff.
- Because cutoffs are unpredictable, this discontinuity creates a random assignment.
- You cannot make strategic rankings since you don't know all comparable applicants' choices in Norway.
- Random assignment of student into different field of study: **above vs below the cutoffs.**

# Regression Discontinuity Design (RDD)

Thistlethwaite and Campbell (1960) study the impact of merit awards on student future academic outcomes:

- Measured by **career aspirations**, **enrollment in postgraduate programs**, etc.
- If we run the following regression:

$$\text{Future Academic Outcome}_i = \alpha + \beta \text{Merit Award Status}_i + \gamma X + \varepsilon_i$$

- $\beta$ may be biased due to selection bias.
- Ideally, we would want to explore some type of randomization.

# Thistlethwaite and Campbell (1960) Design

- The authors exploit the fact that these awards were allocated based on observed student test scores.
- Student with scores ($S_i$) **greater** than the cutoff $c$, received the award.

$$\text{Merit Award Status}_i = \begin{cases} \text{Received}, & \text{if } S_i \geq c, \\ \text{Declined}, & \text{if } S_i < c. \end{cases}$$

- This creates a **sharp** discontinuity of merit award status around the cutoff $c$.

# The Causal Claim

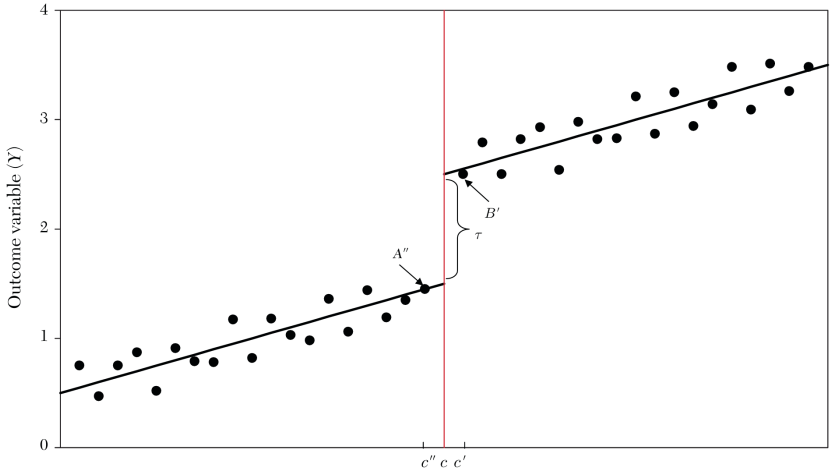
We know merit award status is a function of test score:

- but there is a sharp discontinuity around the cutoff.
- In the meantime, we see no other reasons other than merit award status,
- drives future academic outcomes to be a **discontinuous** function of test score.
- So, we attribute this discontinuous jump of future academic outcome at cutoff as the **causal effect** of receiving merit award.

# The Effective Randomization Again

- Suppose we choose a window around the cutoff: $[c'', c']$.
- We have, say, Alex ended up in point $B'$;
- while Amy ended up in point $A''$.
- Since they are very close the cutoff: **just above** vs **just below**;
- The cutoff score $c$ randomly assigns Alex and Amy into different merit award status.
- The two students are otherwise similar in characteristics.

# Discontinuous Jump in Outcomes



# The Trade-off in Practice

- The window matters!
- If we choose a wider window, the students are less similar in terms of test score.
- If we move closer to $c$, essentially we have less data.
- If we choose a narrower window inside $[c'', c']$, we have no observations at all.
- Thus, we choose a reasonable, and estimate the “average treatment effect”.
- As we move away from the cutoff, the results are sensitive to functional form.

# Important Caveat

- To get the causal claim, we are assuming outcomes have no other reasons to be a discontinuous function of test score.
- We assume all other factors determining outcomes must be **smooth** in test scores.
- If the other variables jump at cutoff $c$, then our estimate is **biased**.

# Regression Discontinuity in Potential Outcome Framework

Let's re-frame the RDD design into a potential outcome framework:

- Let's define merit award status as the treatment $D_i$:

$$D_i = \begin{cases} 1, & \text{if } S_i \geq c, \\ 0, & \text{if } S_i < c. \end{cases}$$

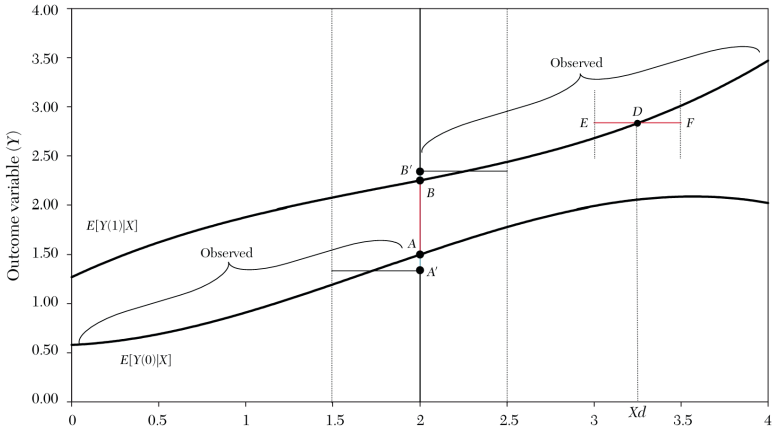
- Ideally, to calculate the average treatment effect, we take the difference of two potential outcomes:

$$E[Y_{1i}|D_i = 1] - E[Y_{0i}|D_i = 0]$$

- But we only observe one potential outcome for each treatment status:

$$Y_i = \begin{cases} Y_{1i}, & \text{if} D_i = 1, \\ Y_{0i}, & \text{if} D_i = 0. \end{cases}$$

# Graphical Illustration



# Identification: Field of Study

| Course Ranking | Inst. | Field | Cutoff |
|---|---|---|---|
| Panel A: Fields | | | |
| 1st best | A | 1 | 57 |
| 2nd best | B | 1 | 52 |
| 3rd best | A | 2 | 48 |
| 4th best | A | 3 | 45 |
| | Application score = 49 | | |
| Local Course Ranking | | | |
| Preferred | A | 2 | Yes |
| Next-best | A | 3 | No |
| | Application score = 47 | | |
| Local Course Ranking | | | |
| Preferred | A | 2 | No |
| Next-best | A | 3 | Yes |

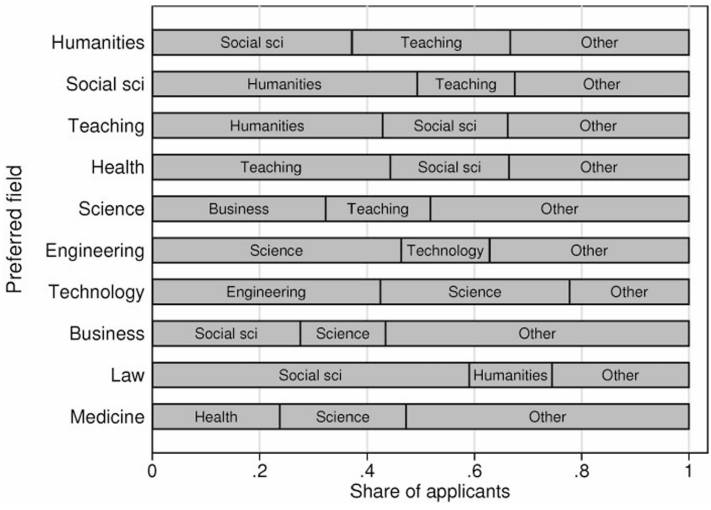
# Identification: Institution

| | | | |
|---|---|---|---|
| Panel B: Institutions | | | |
| 1st best | B | 1 | 52 |
| 2nd best | A | 2 | 48 |
| 3rd best | B | 2 | 46 |
| 4th best | B | 3 | 43 |
| | Application score = 49 | | |
| Local Course Ranking | | | |
| Preferred | A | 2 | Yes |
| Next-best | B | 2 | No |
| | Application score = 47 | | |
| Local Course Ranking | | | |
| Preferred | A | 2 | No |
| Next-best | B | 2 | Yes |

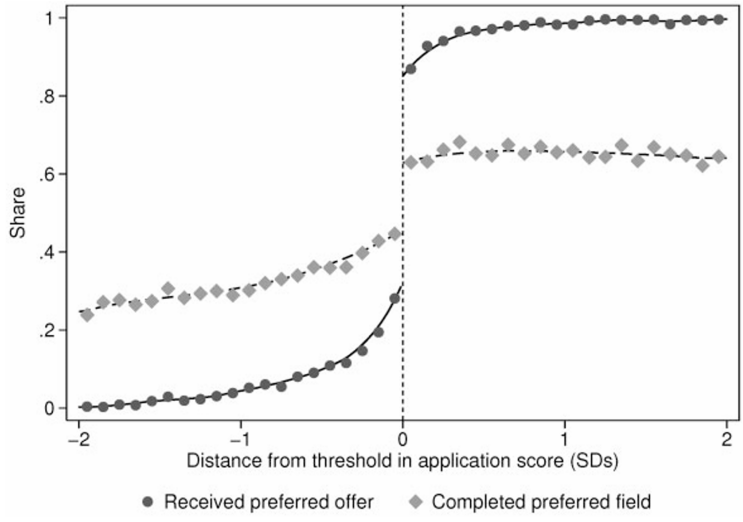
# Identification

- The cutoffs randomly assign students into different fields and institutions;
- And we need to compare students with similar course preferences.
- Are Alex and Amy comparable:
  - Suppose both Alex and Amy chose Science as their preferred field;
  - Alex's next best field is Teaching;
  - Amy's next best field is Social Science;
  - Then we are comparing apples to oranges.

# Next Best Fields



# Graphical Cutoffs

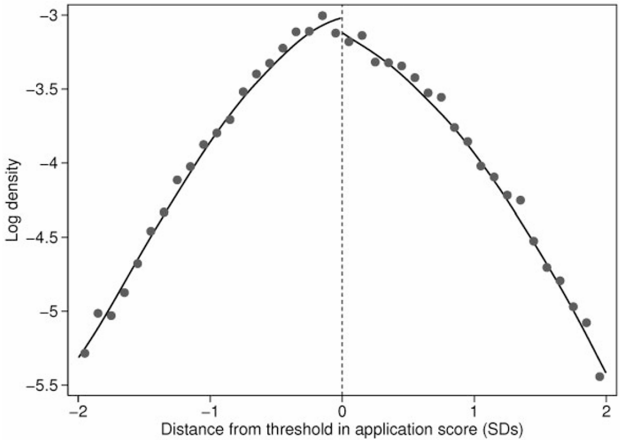


# Offer Received vs Field Completed

- We see a sharp jump in receiving an offer from the preferred field.
- But discontinuity is smaller for those who graduate from the preferred the field.
- That's because students can reapply their preferred field in the second year if they did not get it the first time.
- Our treatment is **graduating from the field**.

# Student Sorting and Admission Cutoffs

- Students might sort above the cutoffs to receive an offer from preferred field.



# Results: Different Field Does Pay Differently

| | Next Best Alternative ($k$) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Humanities | Soc Science | Teaching | Health | Science | Engineering | Technology | Business | Law |
| Completed field ($j$) | | | | | | | | | |
| Humanities | | 21.4* | −4.7 | −22.9* | 5.0 | −38.5** | 6.9 | −42.2** | −156.3 |
| | | (11.0) | (9.8) | (12.1) | (11.9) | (14.7) | (48.3) | (10.6) | (437.3) |
| Social Science | 18.7** | | 9.8 | −10.8 | 55.5** | −55.4** | −110.4 | −28.4** | −76.1 |
| | (6.7) | | (11.6) | (13.0) | (21.5) | (20.6) | (103.0) | (10.7) | (86.4) |
| Teaching | 22.3** | 31.4** | | 1.8 | 23.5** | −33.9** | −35.3 | −21.1** | 22.8 |
| | (5.0) | (7.9) | | (6.6) | (9.5) | (12.5) | (37.1) | (7.1) | (127.9) |
| Health | 18.8** | 30.7** | 7.7** | | 28.9** | −27.9** | −43.4** | −17.4** | −55.2 |
| | (6.3) | (7.6) | (2.8) | | (7.6) | (10.4) | (20.8) | (4.0) | (97.7) |
| Science | 53.7** | 69.6** | 38.6** | 29.6** | | −2.2 | 16.8 | −4.9 | 148.3 |
| | (18.4) | (22.4) | (14.2) | (11.5) | | (14.6) | (18.1) | (10.5) | (276.2) |
| Engineering | 59.8 | −5.5 | 75.2** | 0.2 | 52.4** | | −46.0 | −13.0 | −57.7 |
| | (50.6) | (58.2) | (37.5) | (16.4) | (21.0) | | (43.9) | (23.7) | (166.6) |
| Technology | 41.9** | 58.7** | 22.1* | 32.5** | 68.1** | −5.6 | | 7.0 | −53.1 |
| | (10.8) | (10.1) | (12.4) | (10.1) | (9.6) | (12.0) | | (9.5) | (147.5) |

# Results: Continued

(CONTINUED)

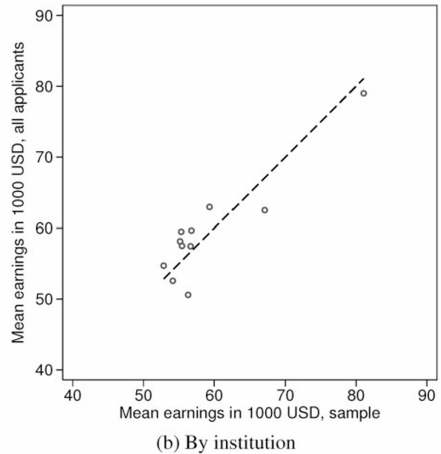
| | Next Best Alternative ($k$) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Humanities | Soc Science | Teaching | Health | Science | Engineering | Technology | Business | Law |
| Business | 48.1** | 61.9** | 31.0** | 30.2** | 58.0** | −3.4 | 28.5* | | 3.5 |
| | (11.3) | (12.0) | (8.8) | (10.9) | (10.5) | (12.6) | (15.6) | | (83.0) |
| Law | 46.3** | 55.6** | 36.6** | 21.5* | 40.1** | −27.5 | −15.6 | −1.4 | |
| | (7.2) | (8.3) | (11.6) | (11.5) | (9.7) | (18.3) | (18.0) | (8.7) | |
| Medicine | 83.3** | 79.4** | 62.6** | 45.6** | 81.3** | 21.1 | 40.1** | 23.3** | 14.8 |
| | (9.8) | (10.7) | (9.0) | (7.0) | (9.7) | (20.7) | (11.7) | (8.8) | (83.6) |
| Female | −7.0** | −6.3** | −10.3* | −5.6** | −5.3** | −5.1** | −4.1** | −7.0** | −10.6 |
| | (1.1) | (1.6) | (1.3) | (0.9) | (1.3) | (1.0) | (1.6) | (3.5) | (6.9) |
| Application score | −0.6 | 4.3** | 4.0** | 1.6** | −0.7 | 1.1* | −0.1 | 0.1 | 13.8 |
| | (0.8) | (1.6) | (0.9) | (0.6) | (0.7) | (0.6) | (1.3) | (2.8) | (14.6) |
| Average $y^k$ | 30.0 | 23.4 | 46.2 | 51.8 | 27.3 | 87.9 | 78.4 | 75.6 | 105.8 |
| Observations | 8,391 | 11,030 | 10,987 | 3,269 | 6,422 | 3,085 | 1,245 | 4,403 | 1,251 |

# Discussion

- Does field of study accumulate field-specific human capital or this is just a reflection of exposure to different peers?
  - Controlling average student application score, estimates barely changed.
- Different fields take different years to complete, maybe the earning gap is just a reflection of labor market experience?
  - Controlling predicted years of experience, results are robust.
- Maybe the gap was simply driven by institution quality?
  - Institution-specific field of study effect remains.

# More on Institutional Effect

- The institutional difference in Norway is modest.
- But we see large effect on college quality in other countries.
  - Dale and Krueger(2002), Hoxby(2009), Deming et al. (2012)



(b) By institution

# 5. College Admissions Policy: The Effect of Affirmative Action

# Motivation

- Affirmative action is a type of policy that are designed to address systematic discrimination.
- It create **quotas** for a particular protected groups during college admission.
  - racial quotas, gender quotas, reserved political positions, etc.
- Hope to bridging inequality in labor market, increasing access to education, etc.
- It also faces criticisms regarding its lawfulness and discrimination to other groups.
  - Students to Fair Admissions vs Harvard
  - Bleemer on Affirmative Action

# The Potential Promises

- Affirmative action explicitly favor historically disadvantaged groups;
- Leveling the playing field in the short-term: e.g., reserve quotas during college admissions.
- Lessons we learned so far:
  - more years of schooling raise future labor market earnings;
  - access to highly rewarded field of study accumulate field-specific human capital which leads to future labor market success.
- Q: Can affirmative action generate long run effects that potentially eradicate achievement gaps (like, educational attainment, income) caused by historic discrimination?

# The Potential Limitations

It always creates controversy:

- They alter the allocation of scarce resources.
- “Opportunity cost”: the preferential admission to one student can result in the exclusion of some other student to a particular college.
- Mismatch problem:
  - Affirmative action can harm targeted students by placing them into an institution they are poorly suited.
  - Students with weak preparation may perform poorly in challenging environment (e.g., selected colleges).

# The Empirical Gap

But it is difficult to evaluate the effect of affirmative action:

- In U.S., each university and college has its own admission policy.
- Often based on factors that are difficult to quantify:
  - essays, interviews, extracurricular activities, recommendation letters, etc.
- It's difficult to identify the beneficiaries: who gets treated by affirmative action.

We need an ideal setting:

- The common or centralized admission process;
- Identification of who gets treated.

# The Empirical Gap Continued

However, credible comparison is still difficult across students:

- When evaluating the effect of affirmative action, we typically look at academic outcomes;
- But subjective grading standards vary across faculty members, departments, and institutions.
- Grades and even graduation rates are not comparable across institutions.

We need an ideal setting:

- Standardized examinations and centralized system.

# Badge et al. (2016)

They study the effect of affirmative action in India:

- The Indian caste system divides society into closed hereditary groups or castes.
- The set of castes known as **Scheduled Castes (SC)** are at the bottom of caste hierarchy, and have suffered most discrimination.
- Additionally, there are **Scheduled Tribes (ST)** that live remote from educational opportunities.
- In 1950, Indian constitution mandates affirmative action to SC and ST groups.

# More on Castes

- In addition to SC/ST, certain socially and educationally disadvantaged groups are designated as **Backward Classes (BC)**.
- It was then divided into four different groups: A, B, C, D.
- Affirmative action is then extended by law to these groups as well.
- It is intended to improve access to secondary and higher education.

# More on Affirmative Action

- Quota system is substantial in India.
- 15% for SC; 6% for ST; 7% each for BC-A, BC-D; 10% for BC-B; 1% for BC-C.
- Castes not accorded to quotas, are designated as Open.
- One-third of seats within each castes are reserved for women.

# Institutional Context

- Colleges have discretion on up to 20% of their available seats.
- The remaining seats are allocated based on a common admission system.
- The affirmative action mandates certain proportion of seats must be set aside on the basis of caste and gender.
- Of 215 engineering colleges they studied, they share a common curriculum and administer externally-graded examinations to all students after first year.

# The Empirical Gap All Over Again

- Even so, applicants are not obliged to accept an offer of admission.
- Even if students receive an offer from the engineering college they studied: they can opt to other institutions or not attending colleges at all.
- We need to control students' preferences to make sure it's apple to apple comparison.
- The authors were able to control factors that influence attendance decisions.

# Sequential Admission Process

- Priority for admission to engineering colleges are based on student's rank on common entry examination and on caste and gender.
- The highest ranked individual in a caste/gender group can choose any colleges.
- Then the second highest chooses.
- ...
- Open seats are allocated first based on entry exam score.
- If an individual qualify a seat without invoking a quota, the quota vacated may be taken by another student within the same caste/gender group.

# Entry Exam Rank vs Effective Rank

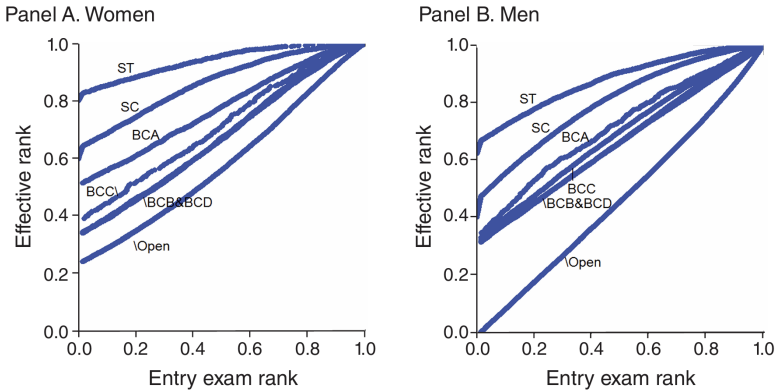



FIGURE 1. RELATIONSHIP BETWEEN ENTRY EXAM RANK AND EFFECTIVE RANK BY CASTE AND GENDER

# College Attendance

- The probability of matriculation in engineering college is **monotonically increasing** in effective rank.

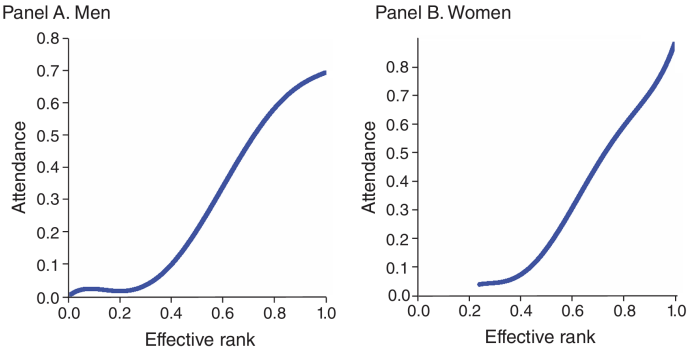



FIGURE 2. RELATIONSHIP BETWEEN EFFECTIVE RANK AND COLLEGE ATTENDANCE

# College Attendance Continued

- Conditional on effective rank, the matriculation is an inverted U-shape in students' latent aptitude:
  - low aptitude students will not enroll in colleges;
  - high aptitude students opt for more selective colleges.

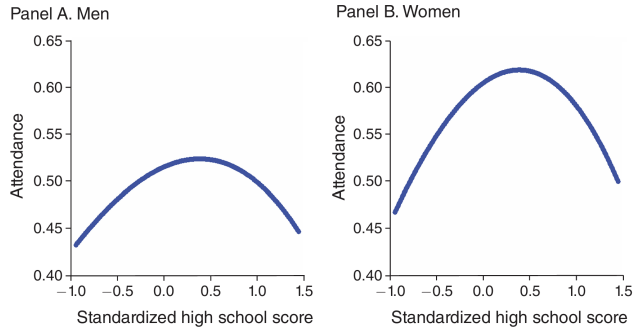



FIGURE 3. RELATIONSHIP BETWEEN HIGH SCHOOL SCORE AND ATTENDANCE

# Identification

We use effective rank as an instrument:

- Relevance condition: effective rank is a good indicator on quality of engineering colleges students enrolled.
- Exclusion restriction: effective rank affect student's academic outcomes only through the channel of attending a better college.
- Conditional on students' latent aptitude, we expect no differences on student's preference in college attendance.

# Results

- Affirmative action improves students effective rank;
- Which allows students to attend a higher-quality school;
- That affects students' first year exam score;
- They found no effect on college graduation rate for particular groups.
- See Table 4 and Table 5.

**No Mismatch caused by Affirmative Action.**

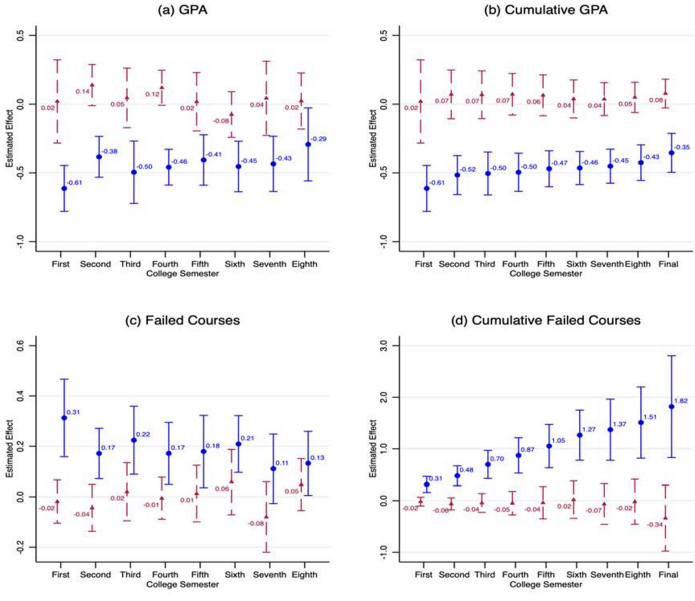
# Is That True Elsewhere?

Oliveira et al. (2023)

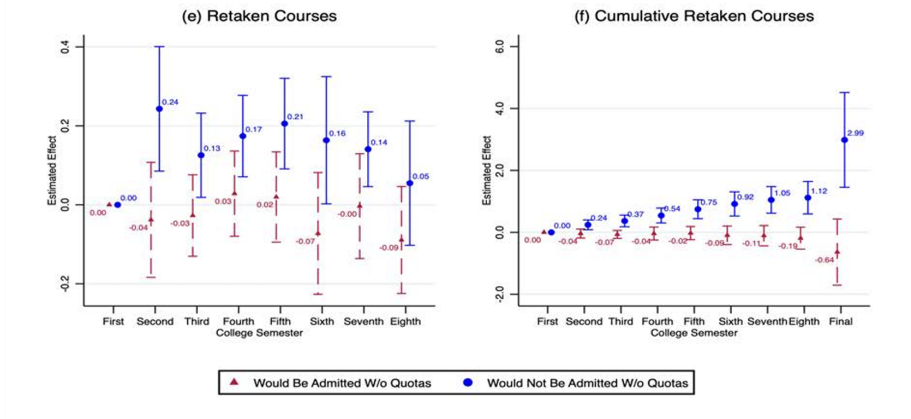
- They study affirmative action in Brazil in 2005.
- Comparing to non-quota peers, quota beneficiaries are less likely to progress smoothly through colleges;
- They are less likely to graduate from colleges.
- The results were driven by those who would not be admitted by colleges otherwise.

**There is a mismatch problem!**

# Effect of Admission via Quotas



# Effect of Admission via Quotas, cont.



# Does it matter who gets treated?

Chen et al. (2026) study affirmative action in China:

- Affirmative action does increase probability of students getting admitted to colleges or even the selective ones;
- But reduce their overall academic ranking;
- Shifting toward lower-earning majors and lower life-satisfaction.
- Heterogeneity analysis reveal that the dose of affirmative action matters:
  - lower dose beneficiaries actually benefit from the affirmative action;
  - but not the higher dose receivers.