Economic Tools and Policy Evaluation

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EE469 Guest lecture

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Kremer et al. 2013

Evidence based policy making: Why?

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Evidence based policy making in the policy process

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Evidence based policy making & Economic tools

- Macro/micro
- ST/LT

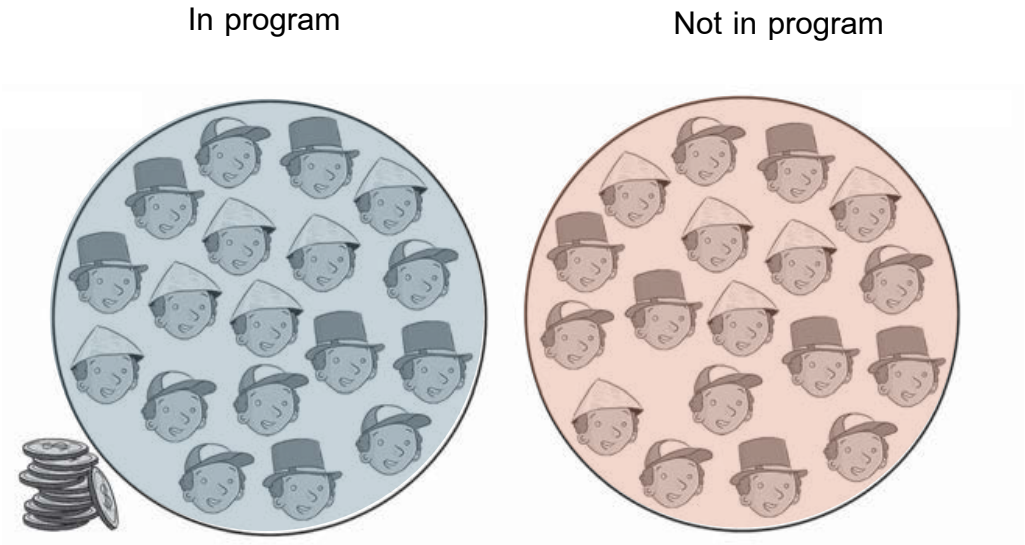
- Mechanism
- Hypothesis

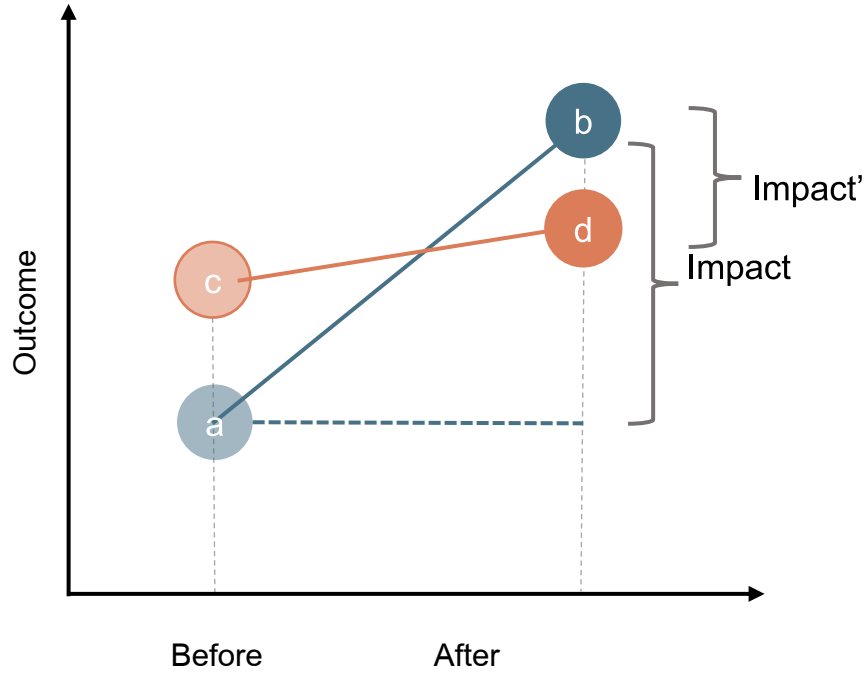
- Existing
- Need to collect

- Difference in difference
- Instrumental variable
- Regression discontinuity
- Matching
- Randomization
- Structural modeling
- **Big data & mixed model**

Main but challenging job for economics

- Challenges in measuring impact of **program** on **outcome(s)**





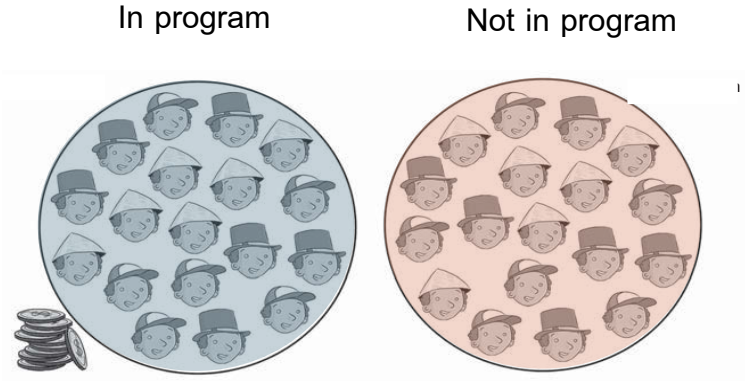
✗ **(After) – (Before):** Impact = b - a but there things also happen during the time?

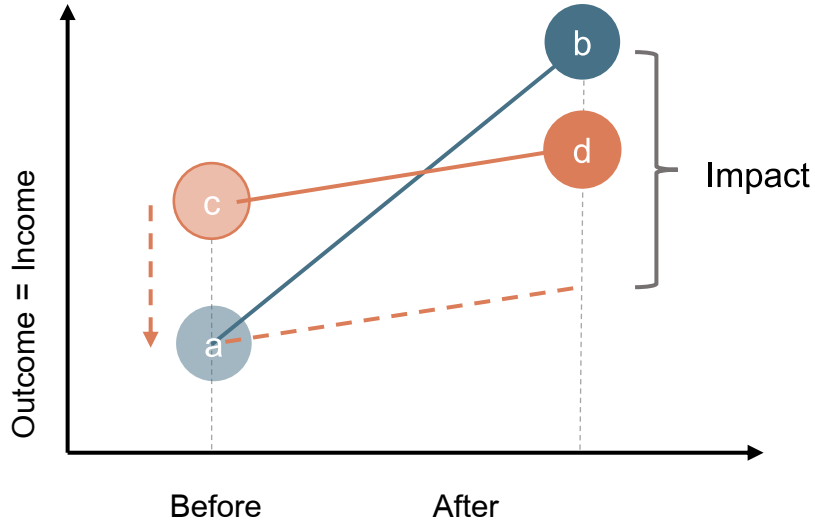
✗ **(In program) – (Not in program):** Impact = d - b but…are co l a

treatment different even before the program? → **selection problem**

If data before and after program available

Difference in difference





- Compare changes in outcomes over time between treatment and control groups

	After	Before	Difference
In school	b	a	b-a
Not in school	d	c	d-c
Difference	b-d	a-c	(b-a)-(d-c)

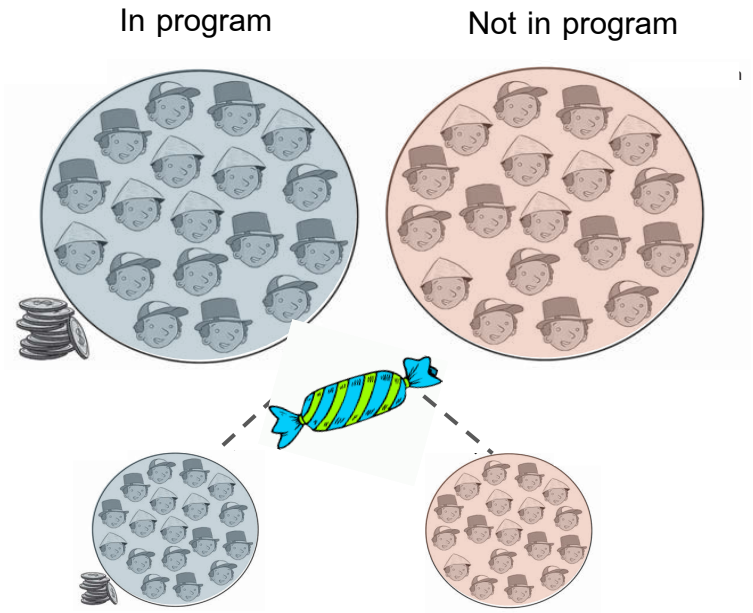
Correct for initial difference of treatment and control groups

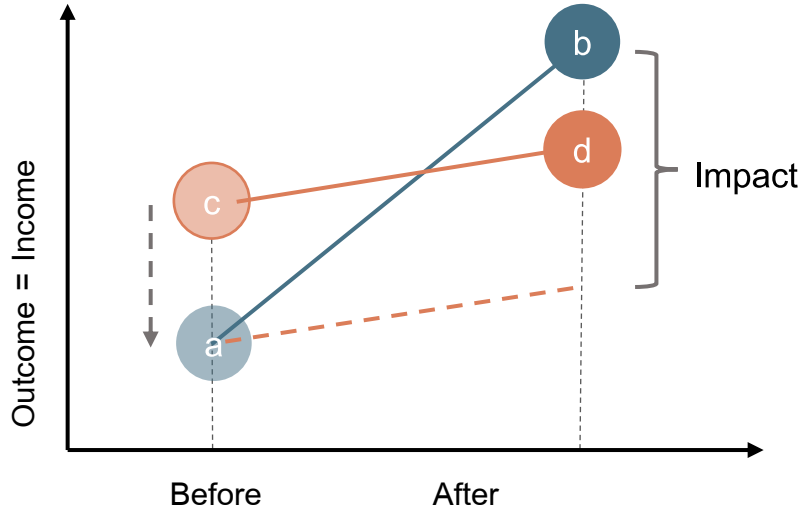
Assume both groups move with similar time trend without program

But…do control and treatment change with same time trend?

If data before program NOT available

Instrument variable (IV)



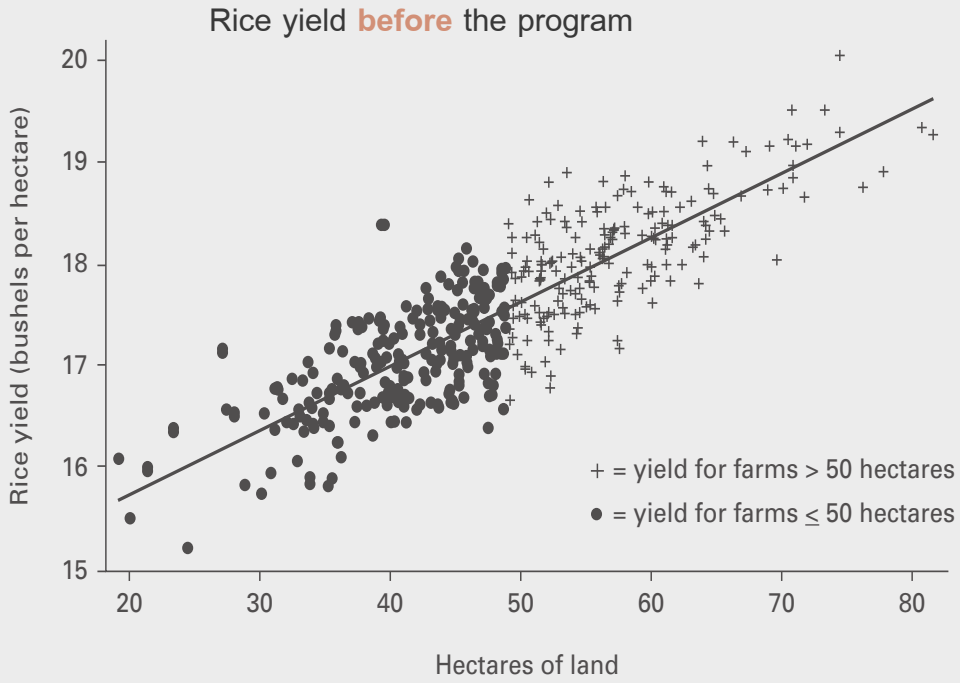


- Find instrument that are not related to outcome but can induce subject into treatment/control groups

  Correct for initial difference of treatment and control groups

  - Ex) Angrist (1990) estimate impacts of military service on labor outcome
    - IV: draft lottery
  - Ex) Angrist and Krueger (1991) estimate effects of compulsory school on earnings
    - IV: quarter of birth in the census

    (In the US, students can enter school when they turn 6 in Jan and have to stay in school until they reach 16)



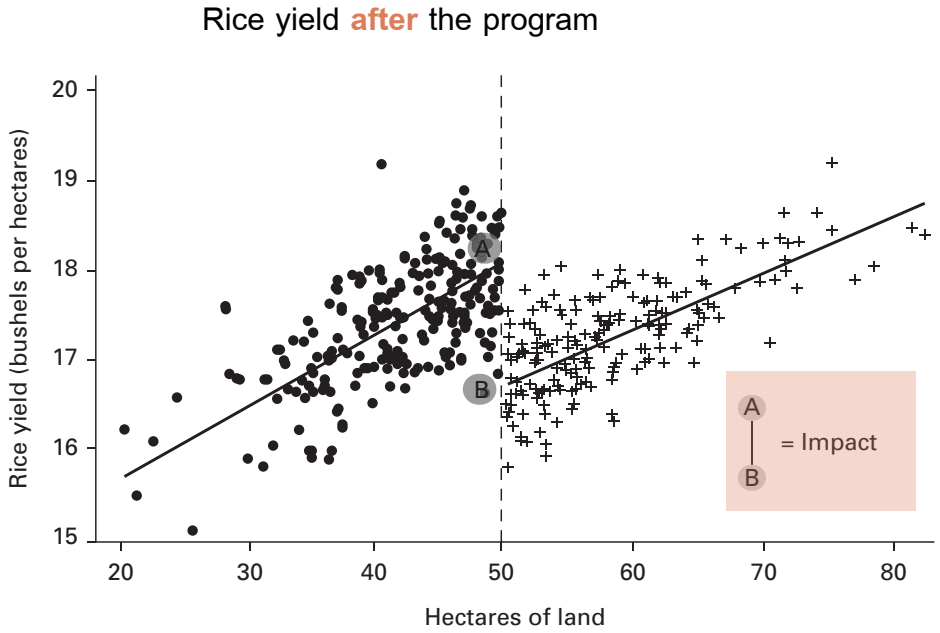
If data before program NOT available

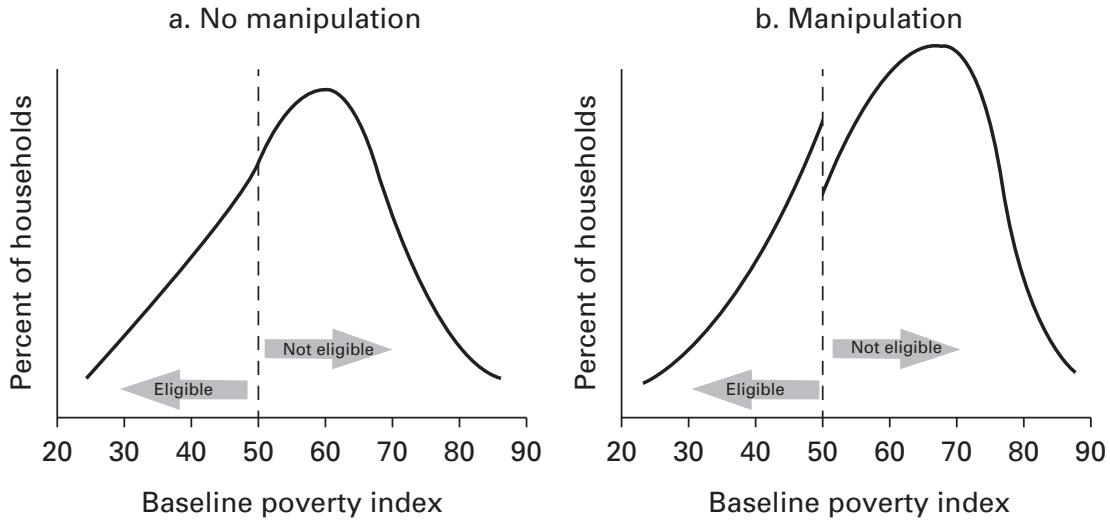
Regression discontinuity

Compare control and treatment within the neighborhood of eligibility threshold

Correct for initial difference of treatment and control groups

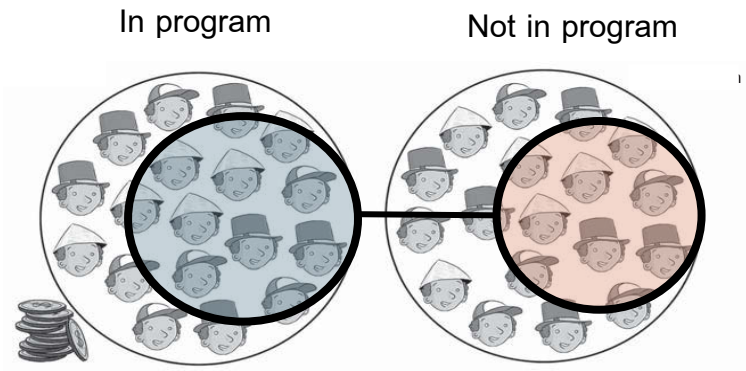
Assume subjects cannot manipulate their eligibility … but





If data before program NOT available

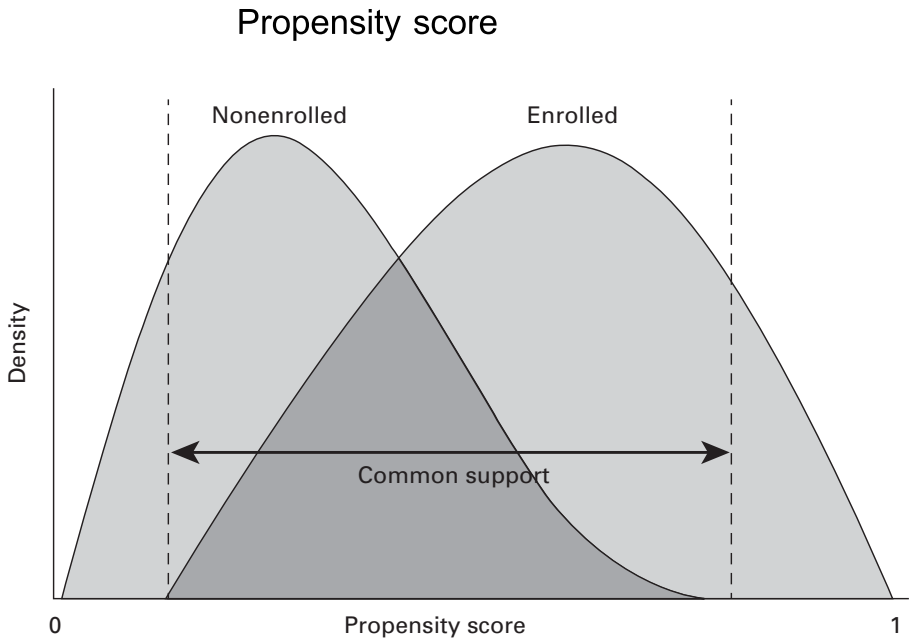
Matching



- Construct control group that have statistically similar to treatment based on observed characteristics

  Correct for initial difference of treatment and control groups

  Assume sorting based on observed characteristics



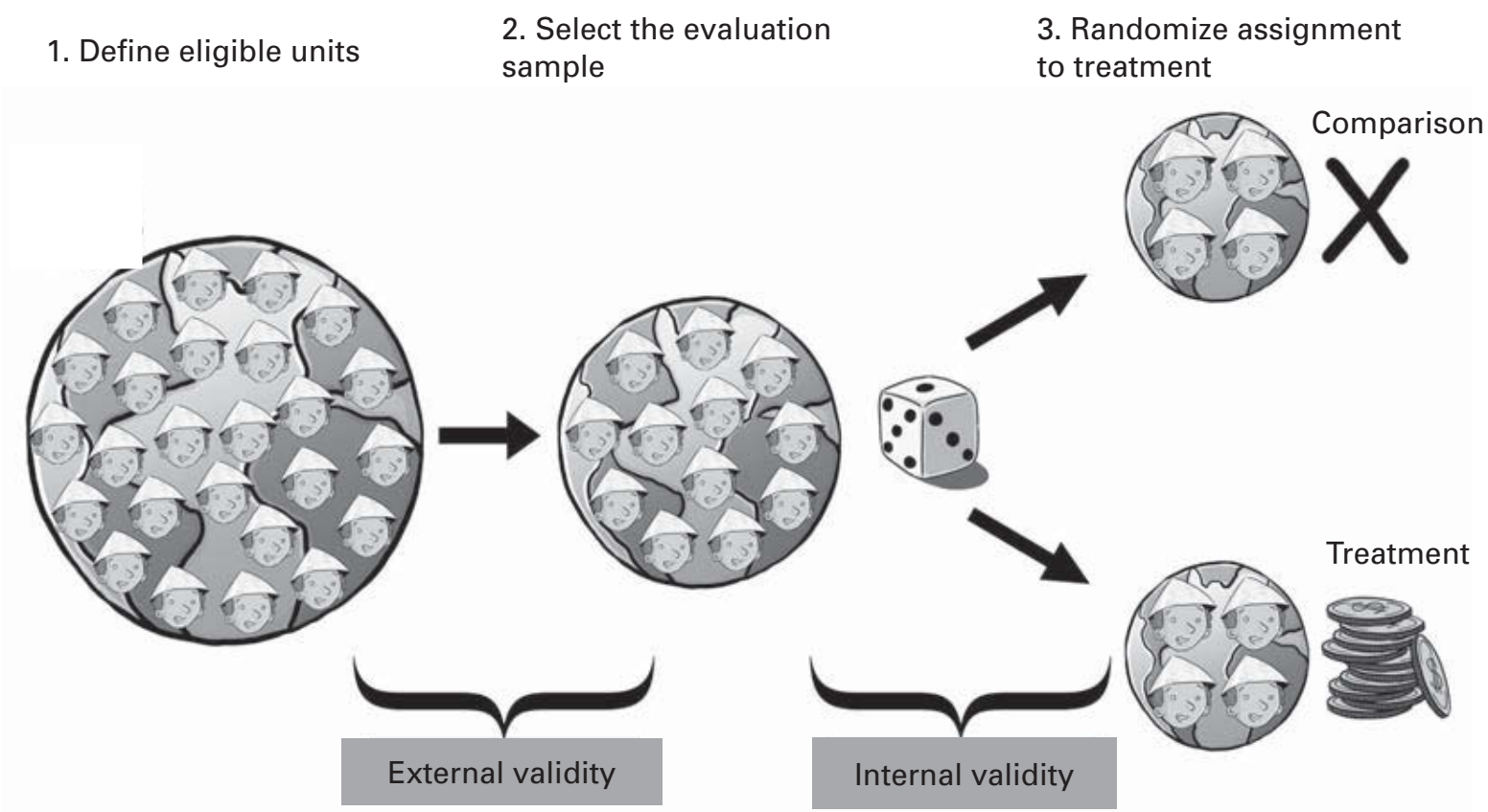
But…how can we be ensure that control and treatment are not different based on unobserved characteristics?

If new data collection is possible

Randomization

- Pure randomization: Randomly assign subject to program (treatment) and control group

  Correct for initial difference of treatment and control groups



Challenges

- Noncompliance → quasi randomization
- feasible?
- Costly?
- Complex, macro policy
- External validity?

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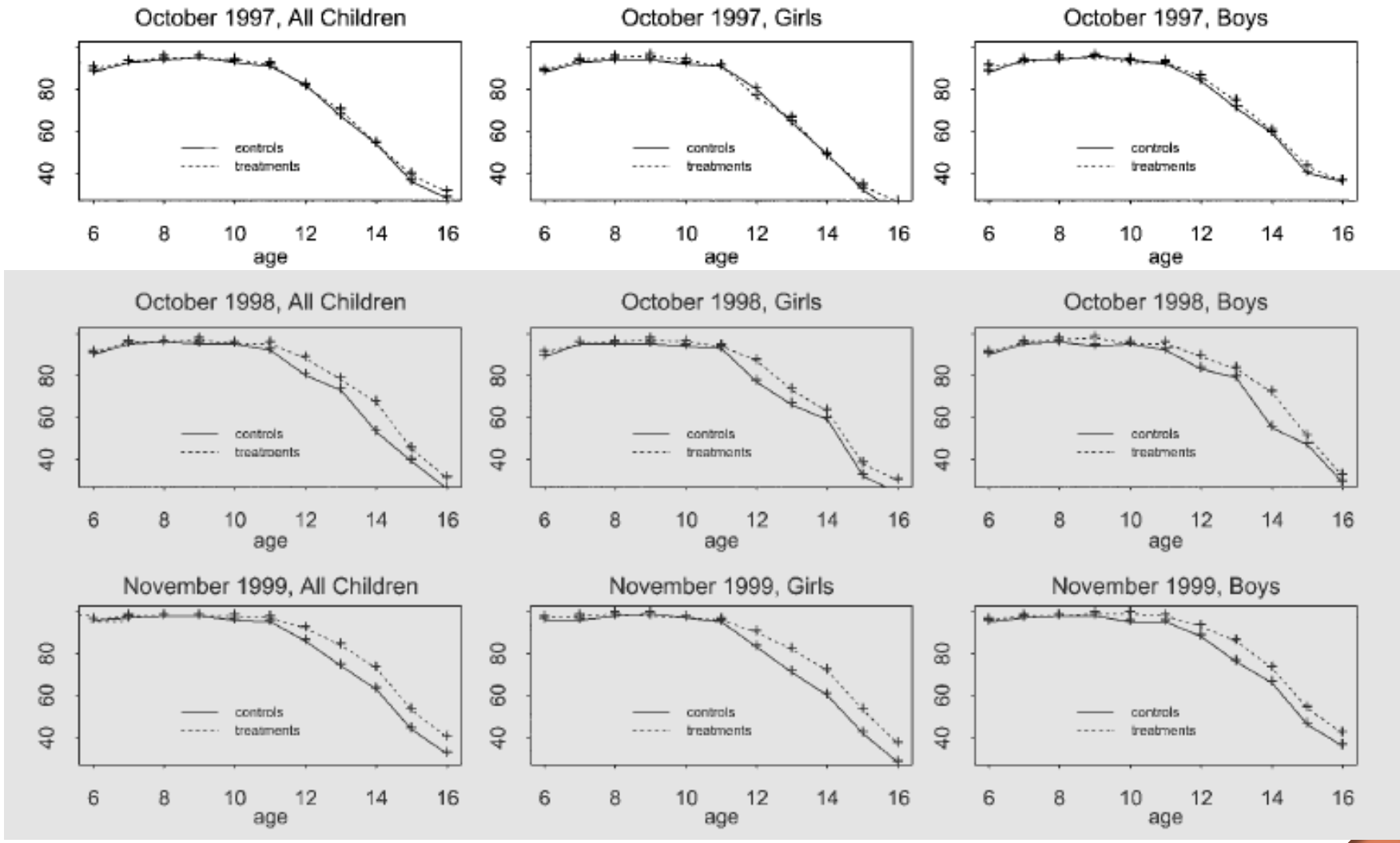
If new data collection is possible

Randomization

Ex) Behrman et al.(2005) evaluate Progressa CCT program

- Randomly select villages in treatment and controls
- Compare eligible households in control and treatment groups



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Other method and the use of big/admin data

- Structural modeling:

1) develop model and calibrate with data with good variations

2) simulate what would happen to outcome with policy options

Ex) Townsend evaluate Thailand village funds

- Big/admin data: ex) researchers in the US have been exploiting census data

1) Cover before and after program

2) track for long-term outcomes

3) large coverage → external validity and impact heterogeneity

Ex) Duflo 2001, Chetty's work with tax records, etc.

Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment

By Esther Duflo*

Between 1973 and 1978, the Indonesian government engaged in one of the largest school construction programs on record. Combining differences across regions in the number of schools constructed with differences across cohorts induced by the timing of the program suggests that each primary school constructed per 1,000 children led to an average increase of 0.12 to 0.19 years of education, as well as a 1.5 to 2.7 percent increase in wages. This implies estimates of economic returns to education ranging from 6.8 to 10.6 percent. (*JEL* I2, J31, O15, O22)

Duflo's famous paper using Indonesia Census

Birthplace / Birthday	Years of education			Log(wages)		
	Level of program in region of birth			Level of program in region of birth		
	High (1)	Low (2)	Difference (3)	High (4)	Low (5)	Difference (6)
Panel A: Experiment of Interest						
Aged 2 to 6 in 1974	8.49	9.76	−1.27	6.61	6.73	−0.12
	(0.043)	(0.037)	(0.057)	(0.0078)	(0.0064)	(0.010)
Aged 12 to 17 in 1974	8.02	9.40	−1.39	6.87	7.02	−0.15
	(0.053)	(0.042)	(0.067)	(0.0085)	(0.0069)	(0.011)
Difference	0.47	0.36	0.12	−0.26	−0.29	0.026
	(0.070)	(0.038)	(0.089)	(0.011)	(0.0096)	(0.015)

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Choosing economic tools to do evidence based policy

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- ST/LT

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