Introduction: Research design

17.871

Statistics

- **1. a.** In early use, that branch of political science dealing with the collection, classification, and discussion of facts (especially of a numerical kind) bearing on the condition of a state or community. In recent use, the department of study that has for its object the collection and arrangement of numerical facts or data, whether relating to human affairs or to natural phenomena. (OED)
- First usage: 1770

Etymology of *statistics*

- From German *Statistik*, political science, from New Latin *statisticus*, of state affairs, from Italian *statista*, person skilled in statecraft, from *stato*, state, from Old Italian, from Latin *status*, position, form of government.

-American Heritage Dictionary of the American Language

The Biggest Problem in Research: Establishing Causality

- Example: HIV and circumcision
  - Observational studies suggest that male circumcision may provide protection against HIV infection

Why is causality such a problem?

- In observational studies, selection into "treatment" and "control" cases rarely random
  - Schooling examples (private vs. public)
  - Voting examples (pro-choice versus pro-life)
- Treatment and control cases may thus differ in other ways that affect the outcome of interest
- The two primary drivers of selection are
  - Confounding variables
  - Reverse causation

How to Establish Causality
(i.e., how to rule out alternatives)

- How do we establish causality? By ruling out alternative explanations
  - Legal analogy: prosecutor versus defense
- Run a field experiment! (best approach)
- HIV and circumcision: field experiment possible?

Post-test only experiment

- Donald Campbell and Julian Stanley, *Experimental and Quasi-Experimental Designs for Research* (1963)
- Summary:

R	X	O
R		O

- No prior observation
- Classical scientific and agricultural experimentalism

Field Experiment example: HIV and male circumcision

- 3,274 uncircumcised men, aged 18–24, volunteered!
  - Randomly assigned to a control or an intervention group with
  - Follow-up visits at months 3, 12, and 21
- Did it work?
  - Control group: 2.1 per 100 person-years
  - Treatment group: 0.85 per 100 person-years
- Problems?
  - Internally valid!
    - Because of randomization intervention, no bias from nonrandom selection into the treatment group. That is,
      - No differences between the treatment and control group on confounding variables (only comparing apples with apples, no apples with oranges)
      - No possibility of reverse causation
    - Alternative interpretations of the treatment?
  - External validity?
  - Could the difference have occurred by chance?
    - Unlikely: $p < 0.001$ on difference

HIV and male circumcision

- When controlling for behavioral factors, including sexual behavior that increased slightly in the intervention group, condom use, and health-seeking behavior, the protection was
  - 61% (95% CI: 34%–77%).
- Male circumcision provides a degree of protection against acquiring HIV infection, equivalent to what a vaccine of high efficacy would have achieved.
- Male circumcision may provide an important way of reducing the spread of HIV infection in sub-Saharan Africa.

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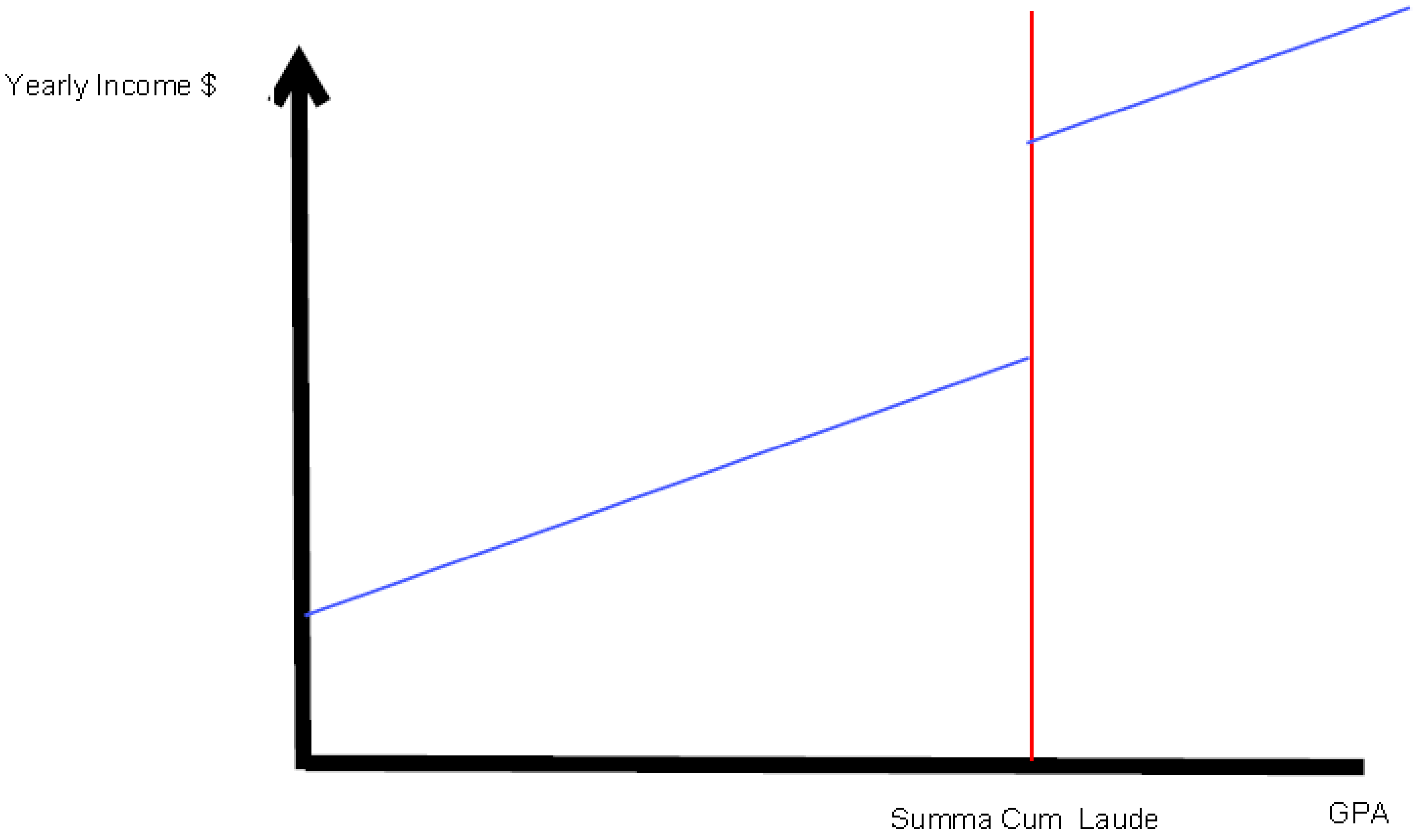
How to Establish Causality
(i.e., how to rule out alternatives)

- But, running an experiment is often impossible
  - Try anyway: e.g., HIV and circumcision
- If you can't run an experiment: natural experiment
  - Exploit something that is exogenous
    - Accidental deaths
    - Timing of Senate elections
    - Imposition of new voting machines
    - 9/11 terrorist attacks
    - Geographical boundaries
  - Exploit a discontinuity
    - *Summa Cum Laude*'s effect on income
    - Regression discontinuity (RD) design

Regression Discontinuity: The Picture



Regression discontinuity

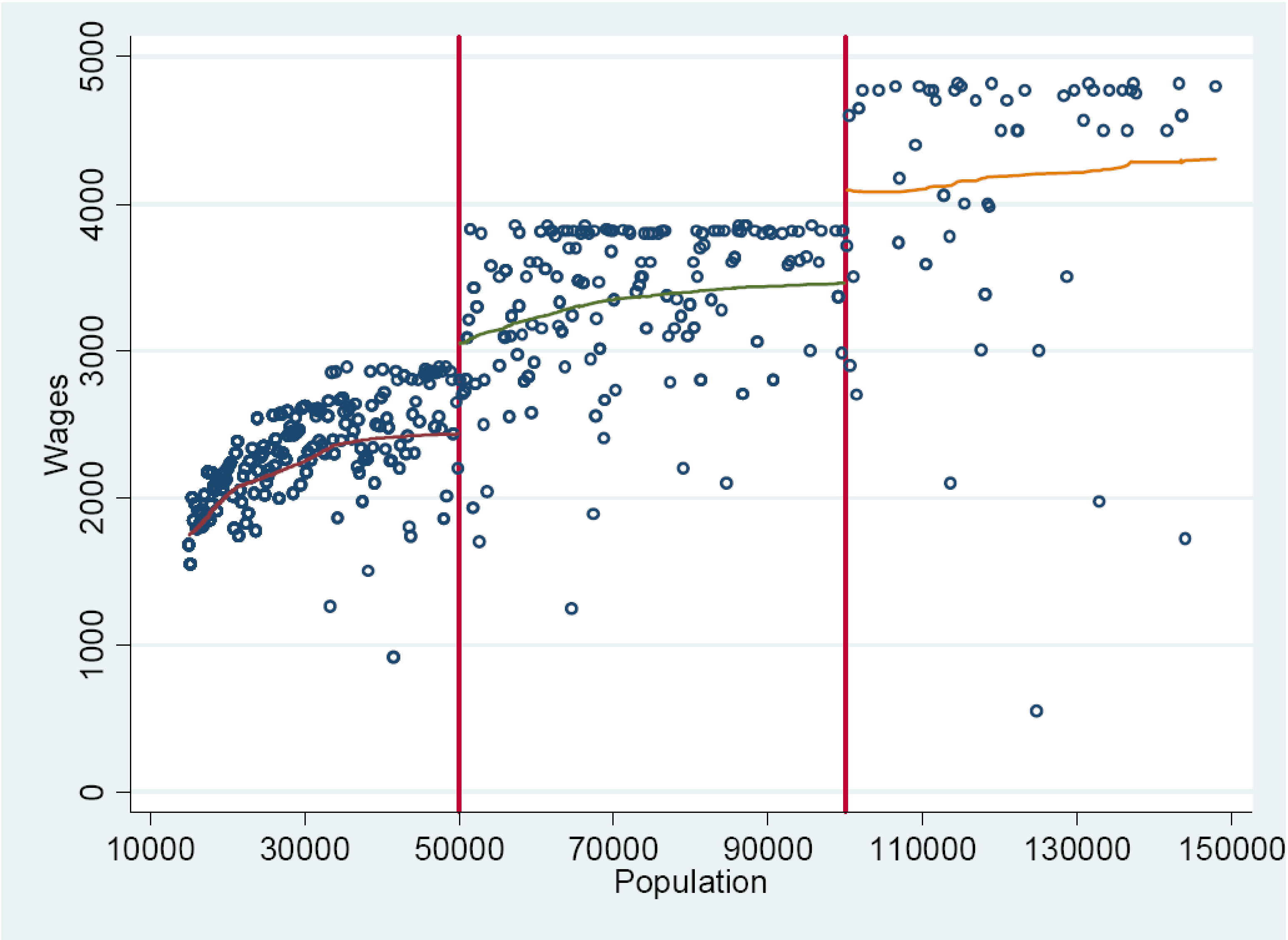
Example from Brazil

Motivating Politicians:

The Impacts of Monetary Incentives on Quality and Performance[*]

Claudio Ferraz[†]
PUC-Rio

Frederico Finan[‡]
UCLA



Wages
Population
0
1000
2000
3000
4000
5000
10000
30000
50000
70000
90000
110000
130000
150000

Table 10. The Effects of Wages on Public Good Provision

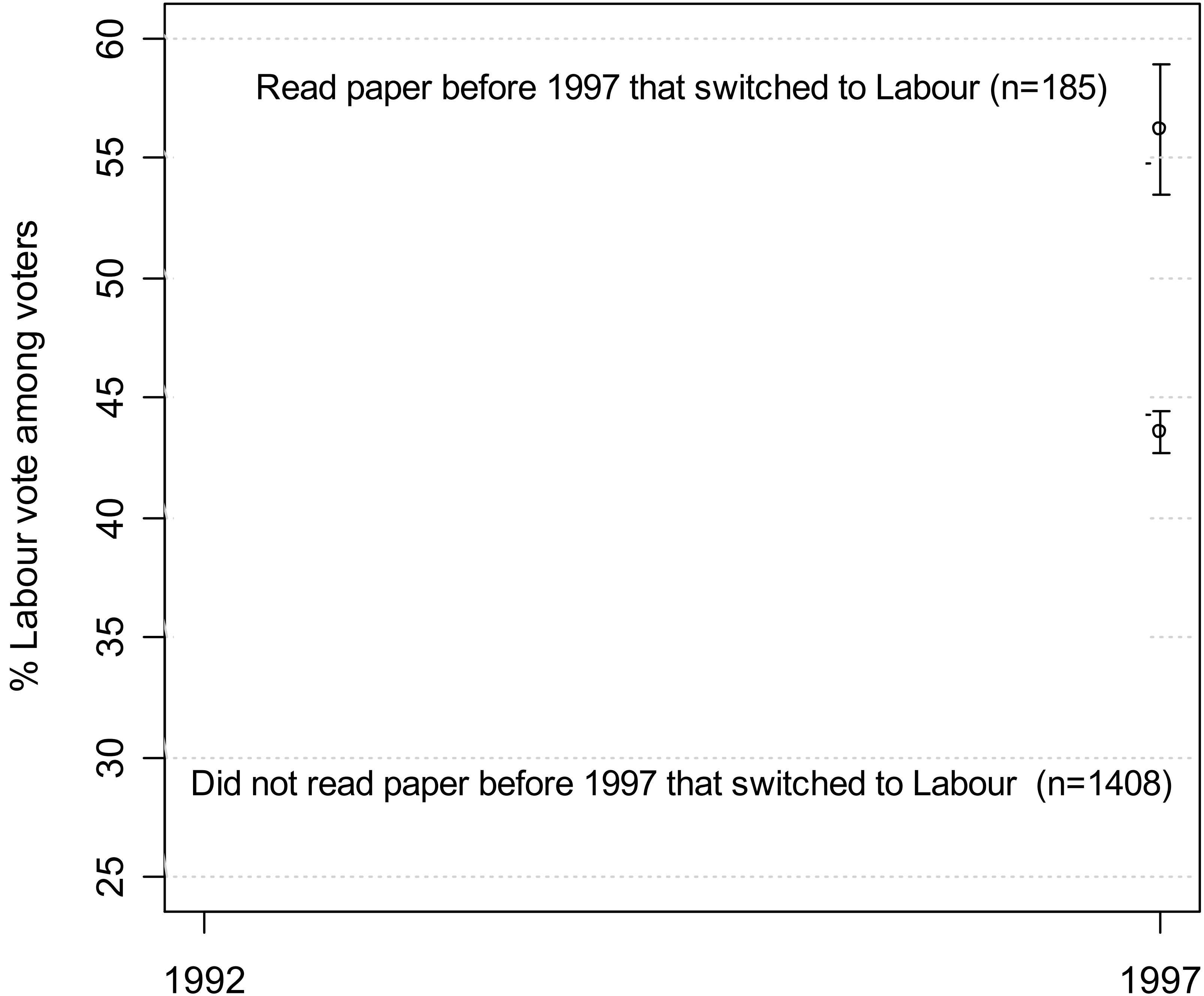
	Education			Health			Sanitation and Water			
Dependent variable:	Number of schools per school aged child (x1000)	Share of schools with a science lab	Share of schools with a computer lab	Health Clinic	Number of doctors per capita (x1000)	Average number of doctor visits	Sanitation network extension per capita (x100)	Water network extension per capita (x100)	Sanitation connections per capita (x100)	Water connections per capita (x100)
	(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: IV estimates										
Log Wages	1.27	0.068	0.175	0.242	0.639	0.521	0.073	0.028	3.201	0.457
	[0.688]*	[0.029]**	[0.073]**	[0.129]*	[0.279]**	[0.241]**	[0.036]**	[0.052]	[2.353]	[1.741]
F-test (exc. instruments)	26.21	26.21	26.21	27.31	28.59	30.23	21.54	26.49	22.05	26.68
Panel B: OLS estimates										
Log Wages	-0.292	-0.015	-0.064	0.104	0.142	-0.088	-0.033	-0.063	-2.371	-2.028
	[0.093]***	[0.005]***	[0.011]***	[0.016]***	[0.033]***	[0.035]**	[0.005]	[0.006]	[0.328]	[0.221]
R-squared	0.48	0.07	0.54	0.04	0.31	0.16	0.27	0.27	0.31	0.50
Municipal characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4736	4736	4736	4012	4857	4825	3479	3928	3483	3935

How to Establish Causality
(i.e., how to rule out alternatives)

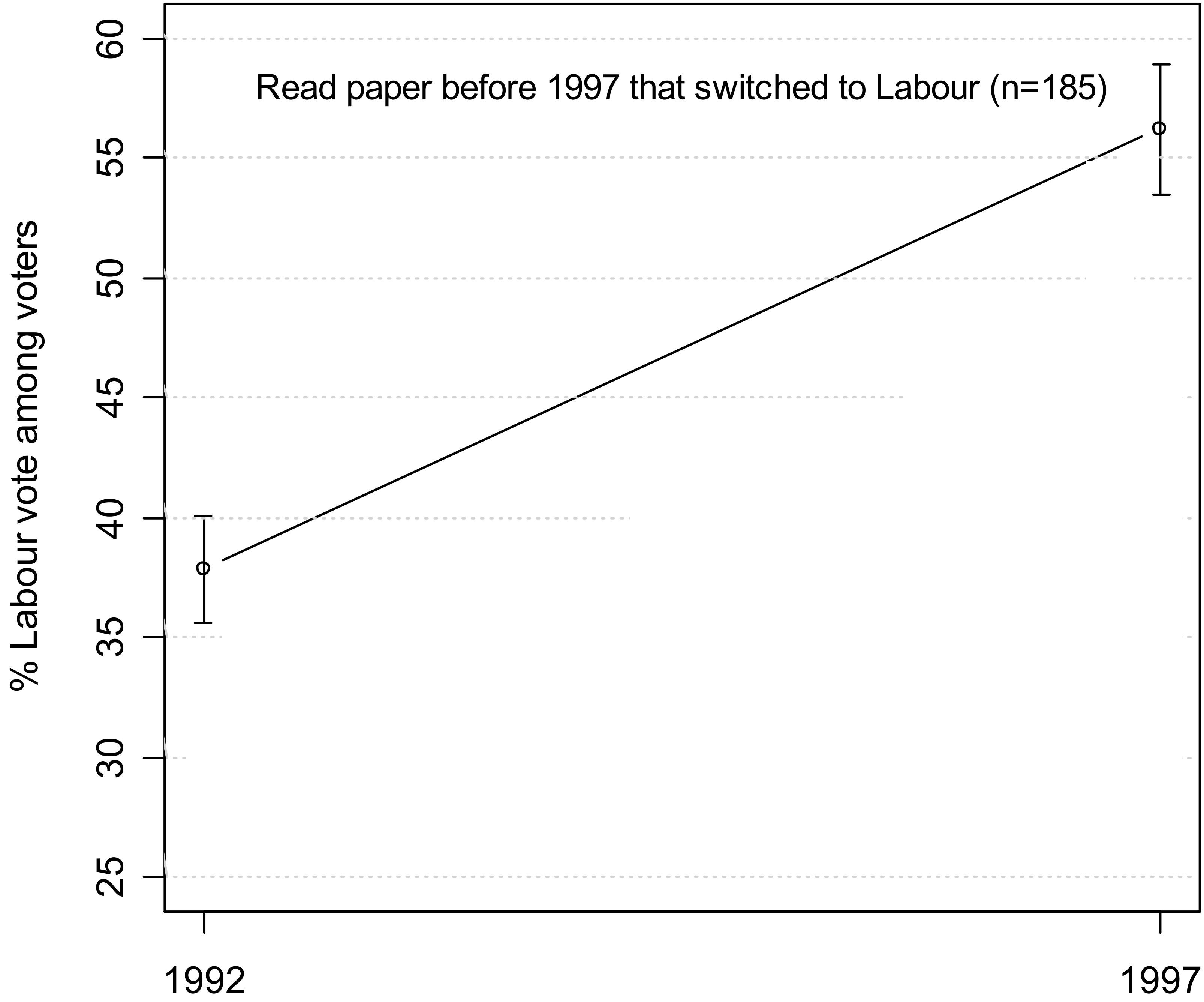
- If you can't run an experiment or find a natural experiment/discontinuity
  - Control for confounding variables
    - Difference-in-differences (DD)
    - Matching
    - Controlling for variables with parametric models, e.g., regression
  - Eliminate reverse causation
    - Exploit time with panel data, i.e., measure the outcome before and after some treatment

Difference-in-differences

- Media effects example
  - Endorsement changes in the 1997 British election
  - Illustrates
    - difference-in-differences, which reduces bias from confounding variables
    - Panel data, which can help rule out reverse causation



Read paper before 1997 that switched to Labour (n=185)
Did not read paper before 1997 that switched to Labour (n=1408)
% Labour vote among voters
60
55
50
45
40
35
30
25
1992
1997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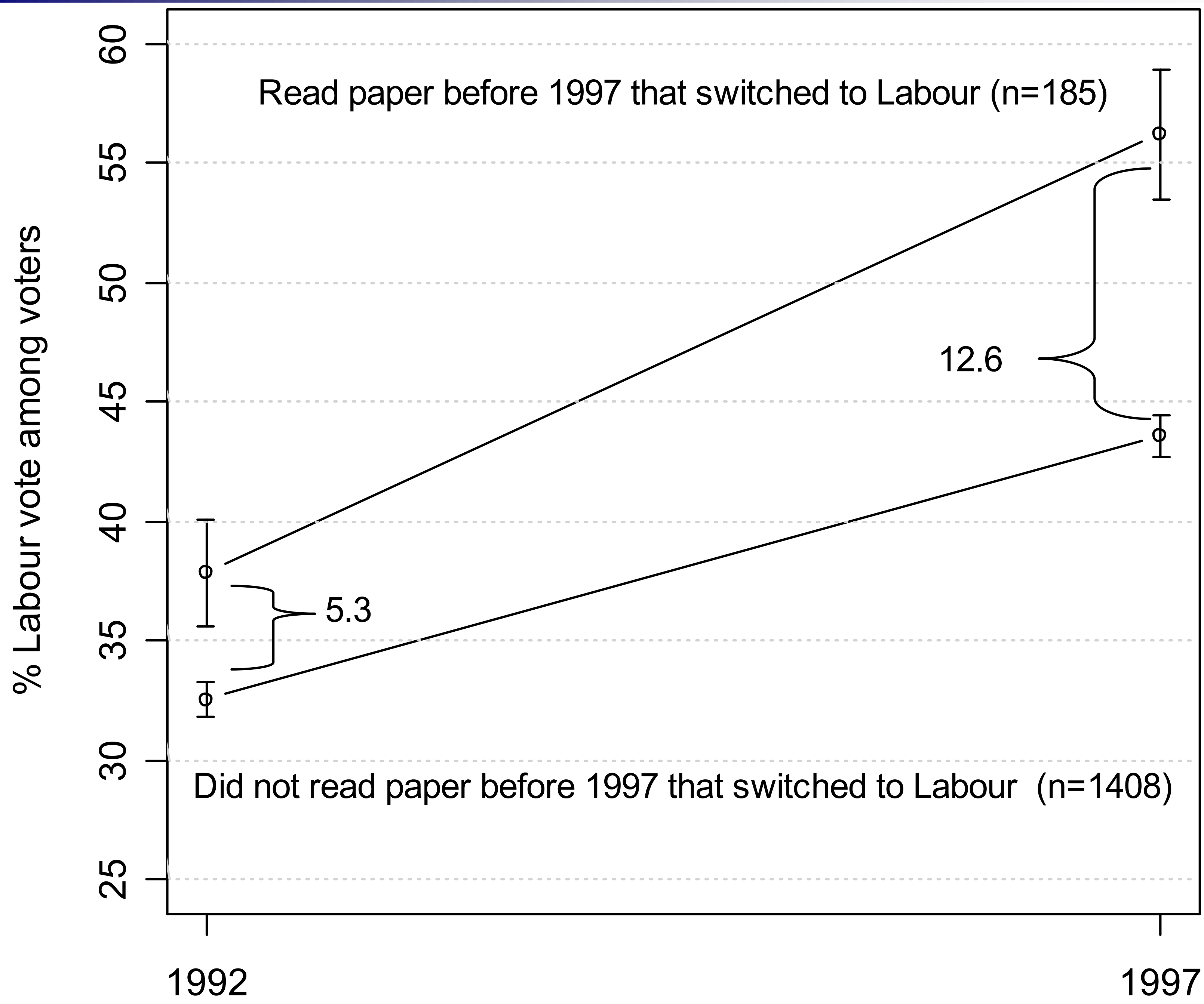


% Labour vote among voters
60
55
50
45
40
35
30
25
Read paper before 1997 that switched to Labour (n=185)
12.6
5.3
Did not read paper before 1997 that switched to Labour (n=1408)
1992
1997

How to Establish Causality
(i.e., how to rule out alternatives)

- If you can't run an experiment or find a natural experiment
  - Control for confounding variables
    - Difference-in-differences (DD)
    - Matching
    - **Controlling for variables with parametric models, e.g., regression**
  - Eliminate reverse causation
    - Exploit time with panel data, i.e., measure the outcome before and after some treatment

Much of 17.871 is about this

Summary

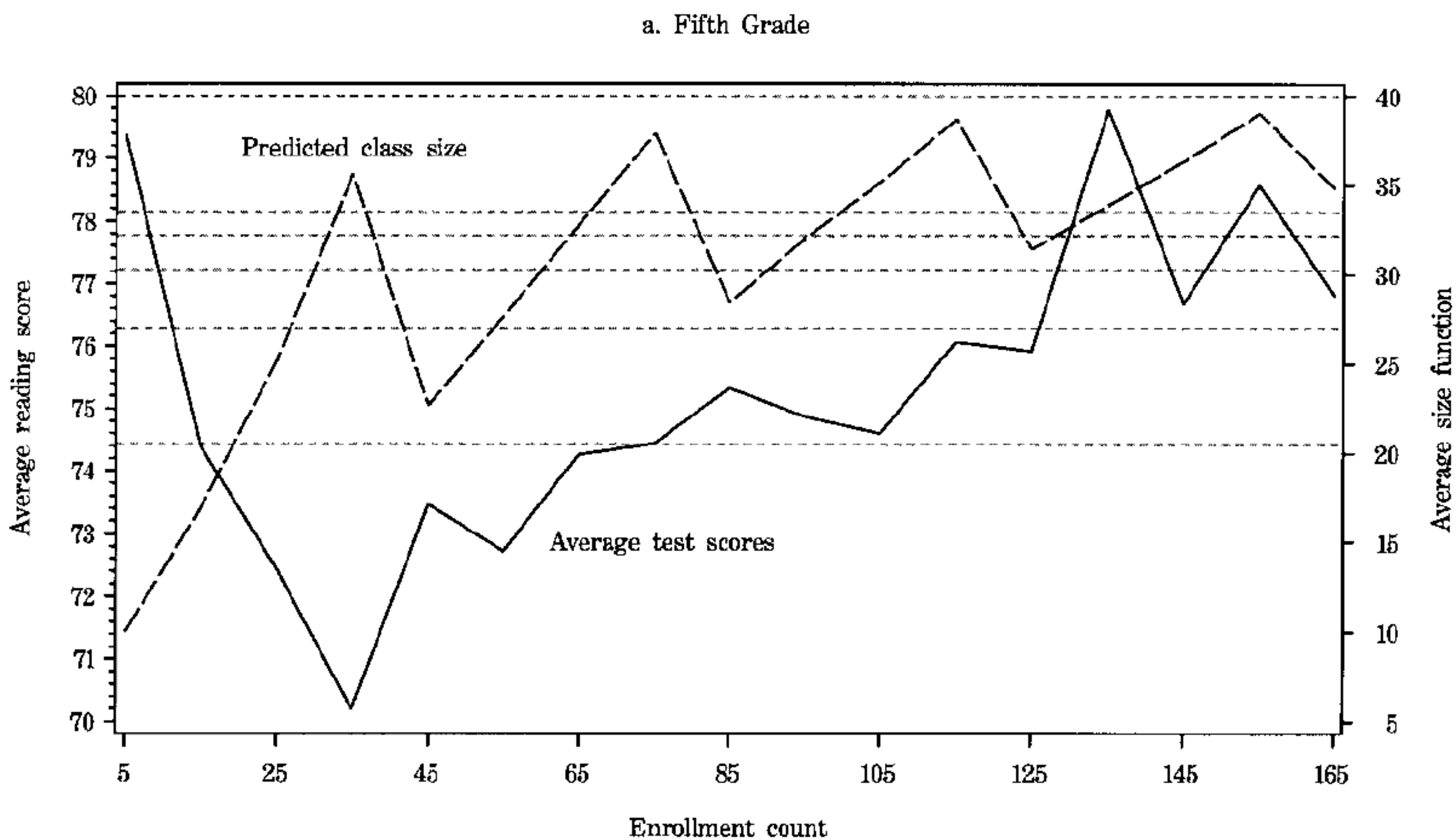
- Classical experimentation unlikely, but always preferred
  - Always keep a classical experiment in mind when designing observational studies
- Strive for "natural" or quasi-experiments
  - Alternating years of standardized testing
  - Timing of Senate elections
  - Imposition of new voting machines
  - 9/11 terrorist attacks
  - Use Regression-discontinuity designs
    - Geographical boundaries (e.g., minimum wage study)
- Use Difference-in-differences designs
- Gather as much cross-time data as possible (panel studies)
- If you only have cross-sectional data, be humble!

Example: Class size and achievement

Hypothesis: Maimonides's Rule: Class sizes large than 40 students are bad for kids.

Context: In Israel kindergarten class sizes are never greater than 40.

What is the pattern predicted? 0-40 grow as you expect, then at 41 average size 20.5, increase again to 80, at 81 avg. 27.



(Angrist and Lavy, 1999)