Quasi-Experiments

POLI SCI 210

Introduction to Empirical Methods in Political Science

AI Prompts

- Quasi-experiment (vs. natural experiment)
- Regression discontinuity
- Difference-in-differences
- Political science applications of these designs
- *Robust* standard errors
- *Clustered* standard errors

So far

- **Two weeks ago:** Experiments as a framework to think about causal inference (potential outcomes)
- **Last week:** Regression as a flexible method to estimate conditional means/slopes
- **This week:**

Strategies for causal inference

- **Strategy 1:** Random assignment
- **Strategy 2:** Ignorability assumption (conditional independence)

Ignorability is hard to justify:

1. Need to account for all relevant confounding variables
2. Should not include more than 2-3 controls in regression

Roadmap

Quasi-experiments as research designs for *credible* causal inference in observational studies

Tuesday: Ignorability satisfied by design (regression discontinuity)

Thursday: Replace ignorability with a more plausible assumption (difference-in-differences)

Quasi-experiments

- *Observational studies*
- Conditions (treatment, control) assigned in a manner that is **sufficiently independent** from potential outcomes
- Enough to satisfly *ignorability* or *conditional independence*
- $\neq$ **Natural experiments** (no random assignment)
- We can *think* of them as experiments but they **are not**

Regression discontinuity design (RDD)

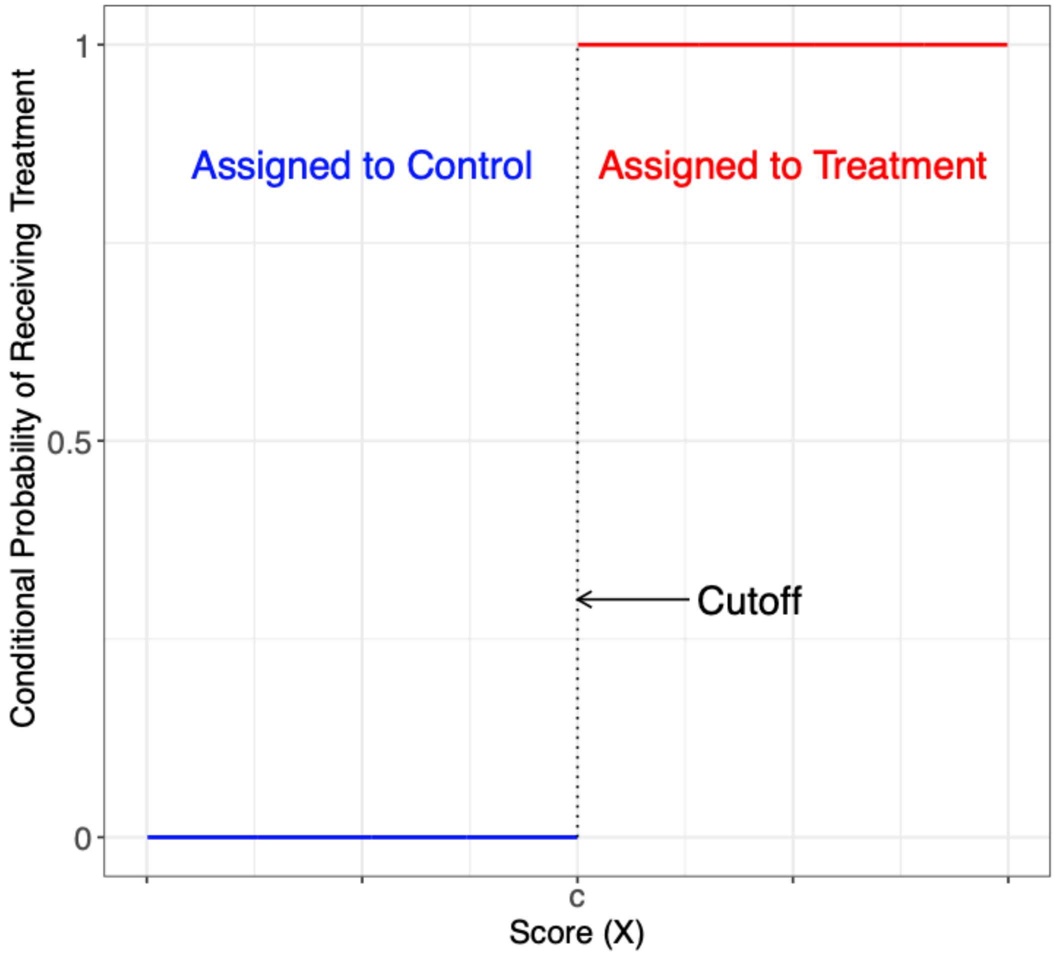
Ingredients

- Y: Outcome
- X: **Score** or **running variable** (numerical continuous)
- c: **Cutoff** or **threshold**
- T: **Treatment** indicator (1, 0)

Potential outcomes

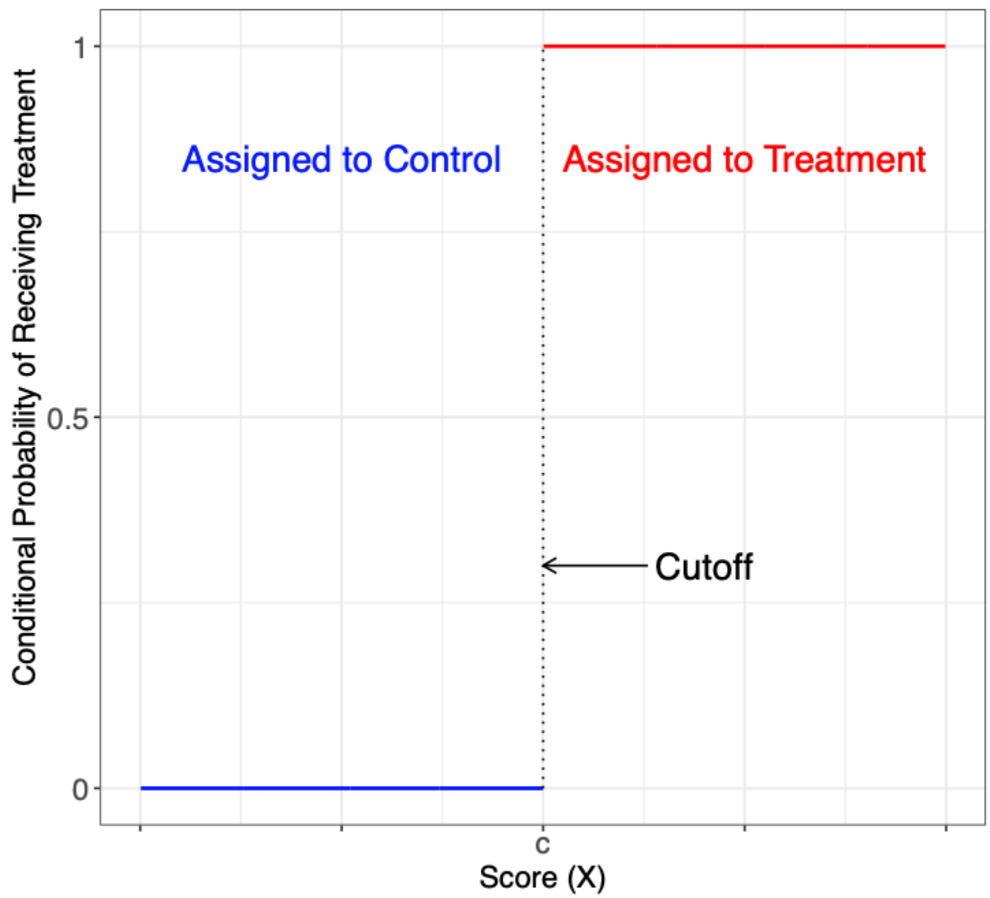
$$Y = (1 - T)Y(0) + TY(1) = \begin{cases} Y(0) & \text{if } X < c \\ Y(1) & \text{if } X \geq c \end{cases}$$

Sharp RDD



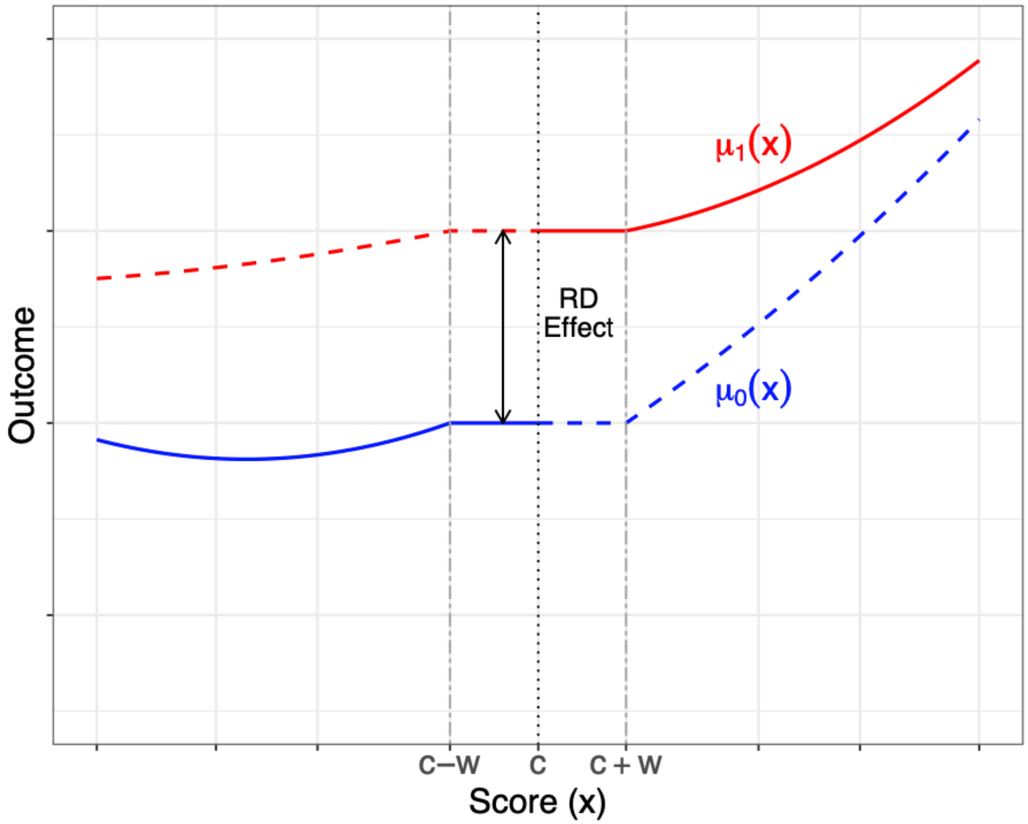
Called *sharp* because treatment assignment is *deterministic* at the cutoff

Interpretation: Two approaches



1. Local randomization
2. Continuity-based

Local randomization approach

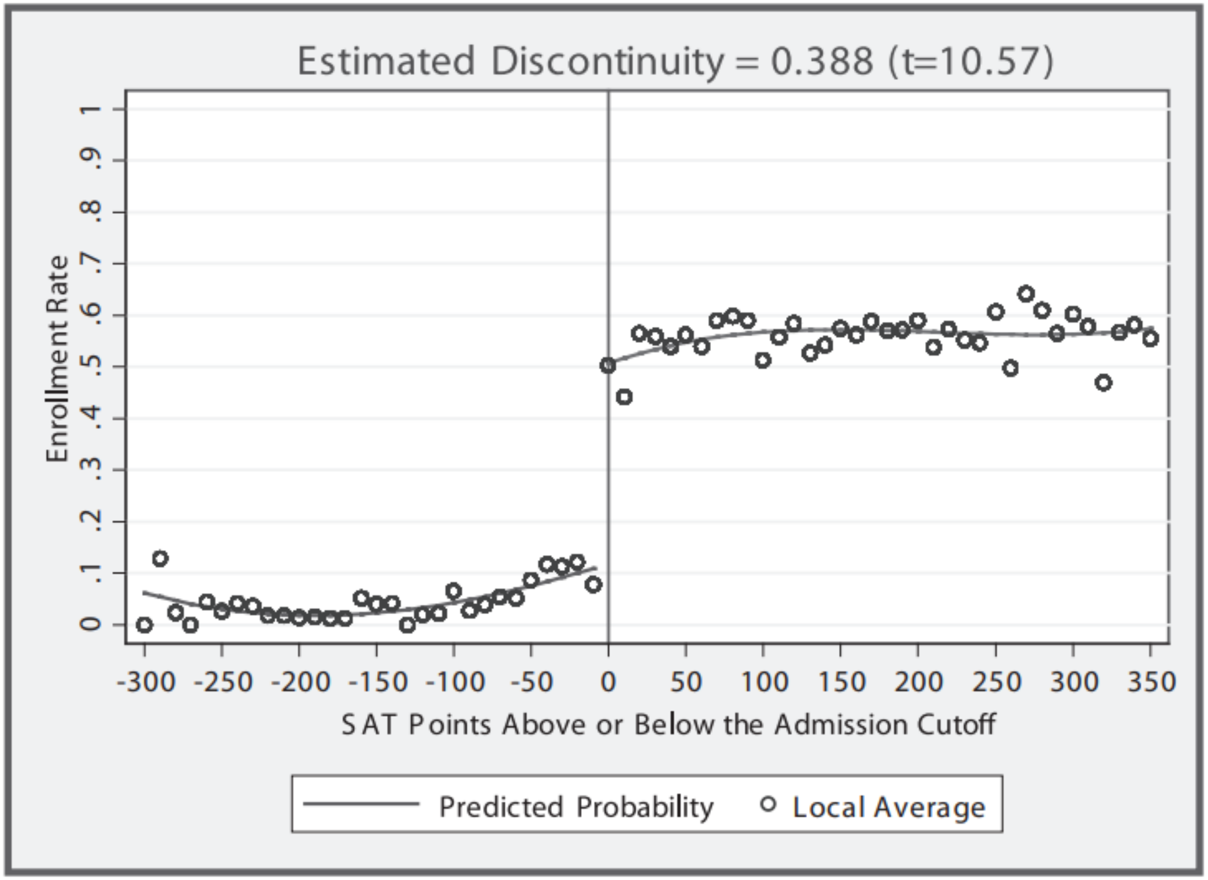


- Potential outcomes are not random because they depend on the *score*
- But around the *cutoff*, treatment assignment is **as-if random**

We can pretend we have an experiment within a **bandwidth** around the *cutoff*

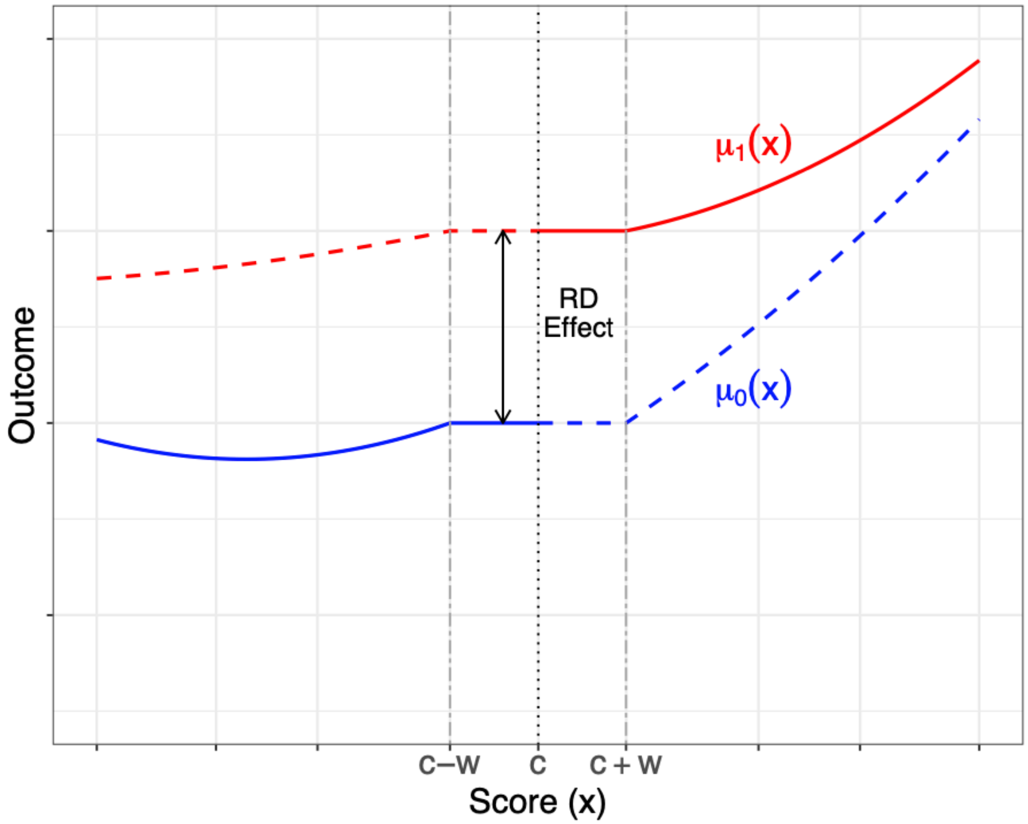
Example

Figure 1.—Fraction Enrolled at the Flagship State University



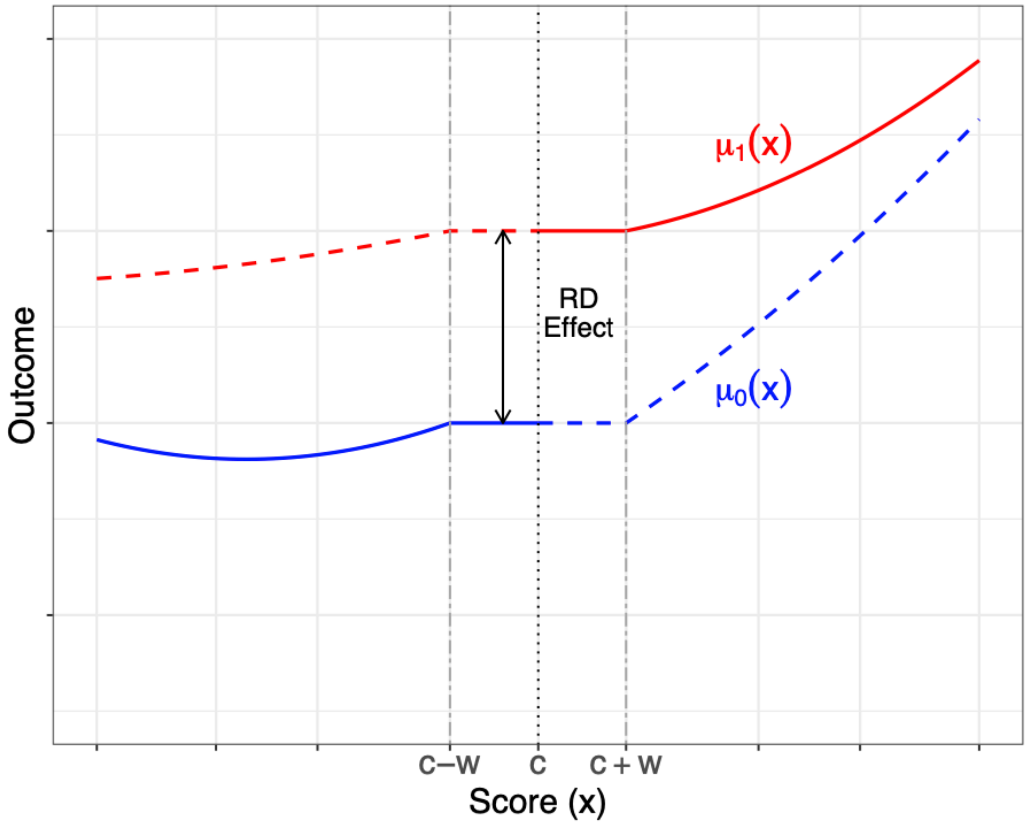
Source: Hoekstra, Mark. 2009. "The Effect of Attending the Flagship State University on Earnings: A Discontinuity-Based Approach." *The Review of Economics and Statistics* 91 (4): 717-724

Local randomization approach



- Bandwidth $\mathcal{W} = [c - w, c + w]$
- Treatment **as-if** random within $\mathcal{W}$
- ATE *identified* within $\mathcal{W}$

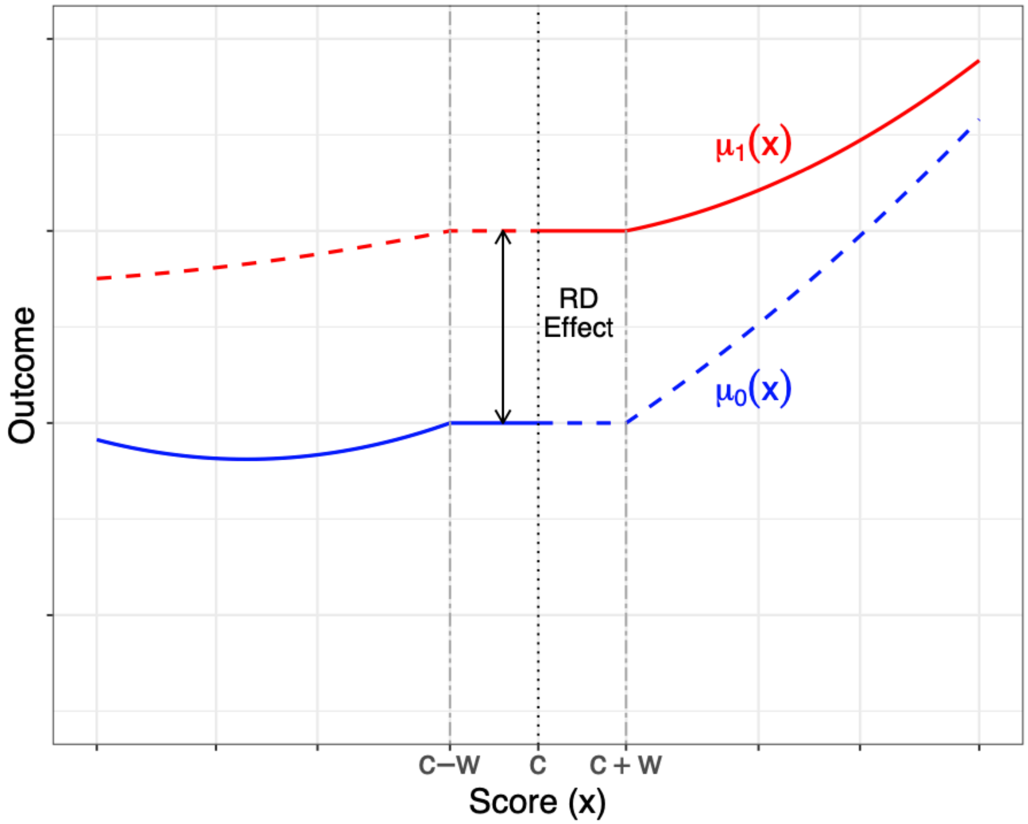
Local randomization approach



Requirements

1. Known probability distribution of scores within $\mathcal{W}$ ($\equiv$ random assignment)
2. Potential outcomes **not affected by scores** within $\mathcal{W}$

Local randomization approach



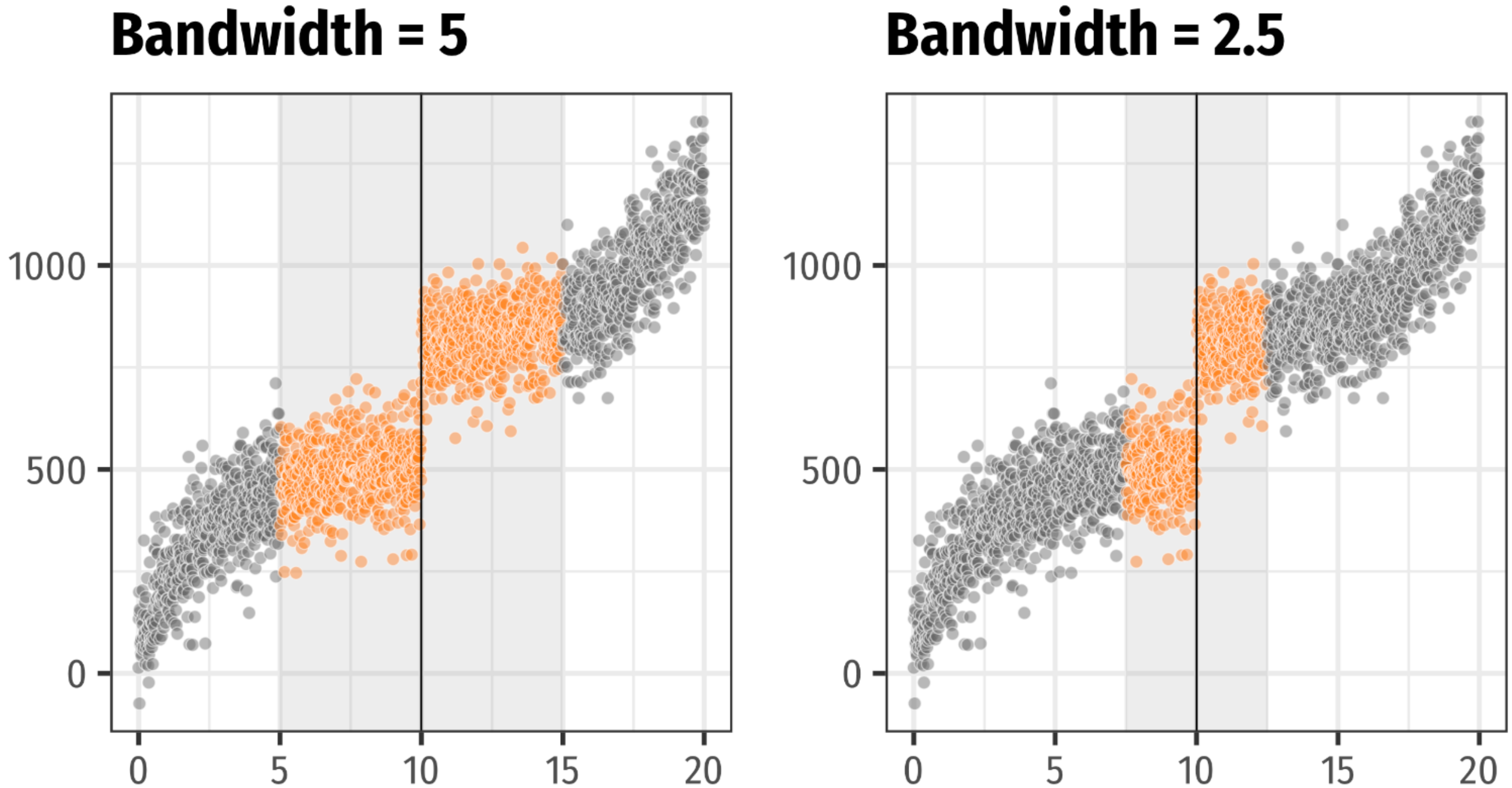
Estimation

- Difference in means within $\mathcal{W}$

Inference

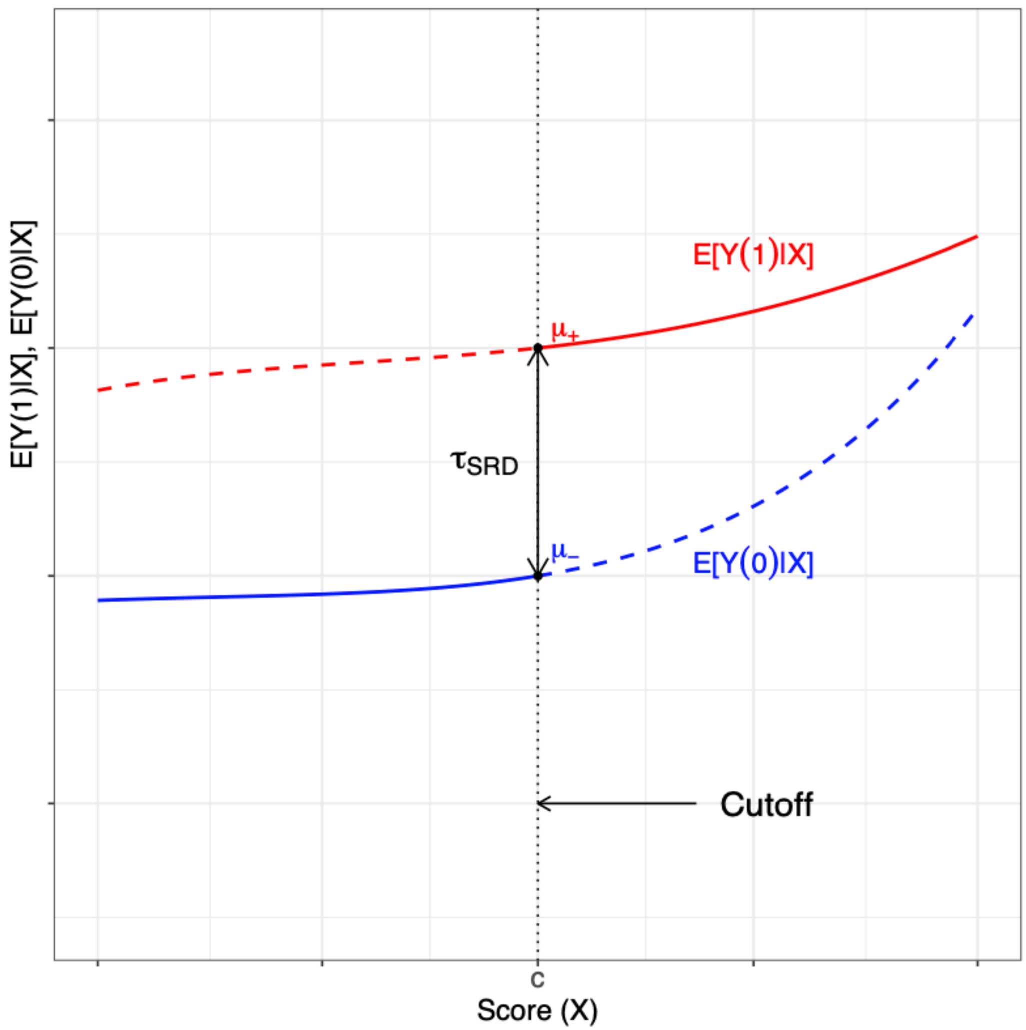
1. CLT approximation (needs a *super-population*)
2. Simulation (*randomization inference*)

Challenge: choosing a bandwidth



A narrow bandwidth has low bias but high variance. A wider bandwidth has lower variance but more bias. Narrow makes more sense but may yield wide confidence intervals.

Continuity-based approach



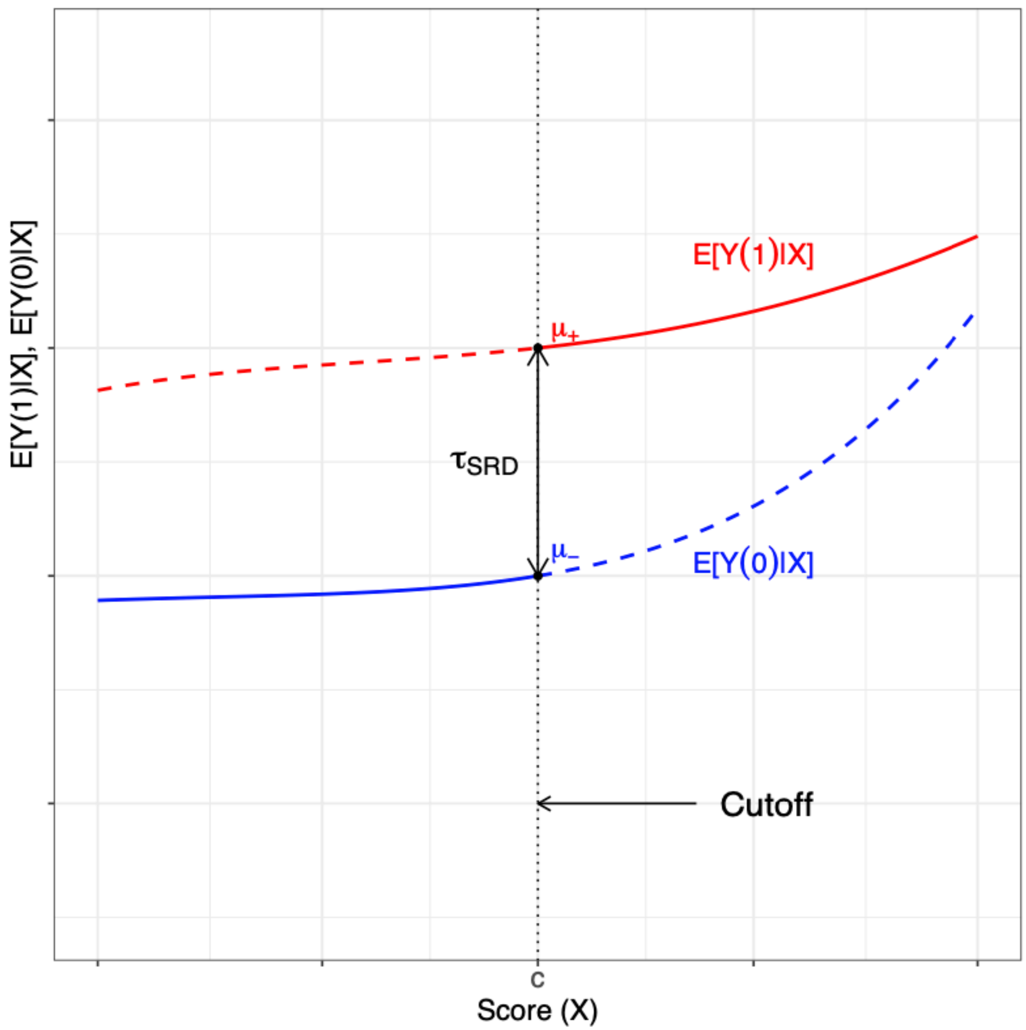
- Treatment assignment **deterministic** at cutoff
- ATE *identified* **exactly** at c
- $\tau_{SRD} \equiv E[Y_i(1) - Y_i(0)$
- But it does not exist!

Example: Evanston school district

Terms

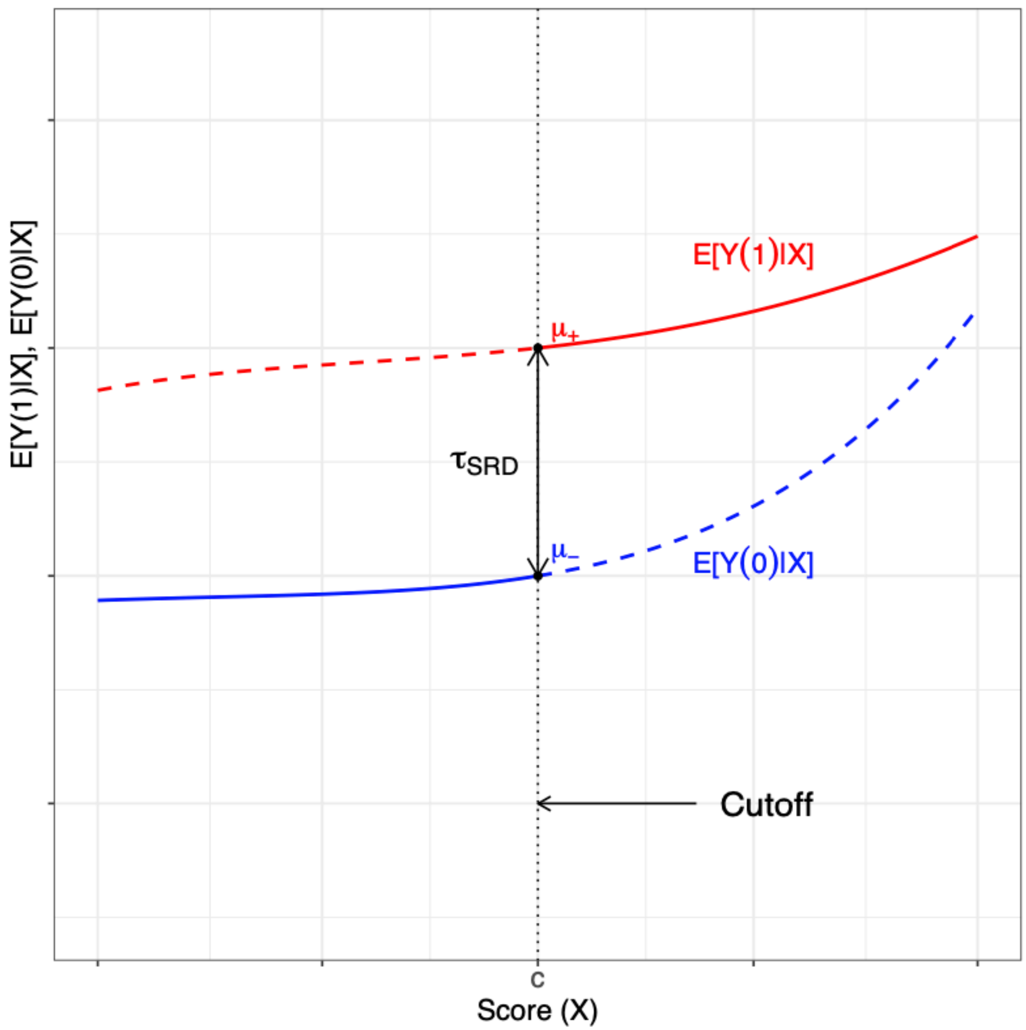
This map was created by a user. Learn how to create your own.

Continuity-based approach



- ATE is *identified* but *nonexistent* at cutoff
- Still, we can approximate gap

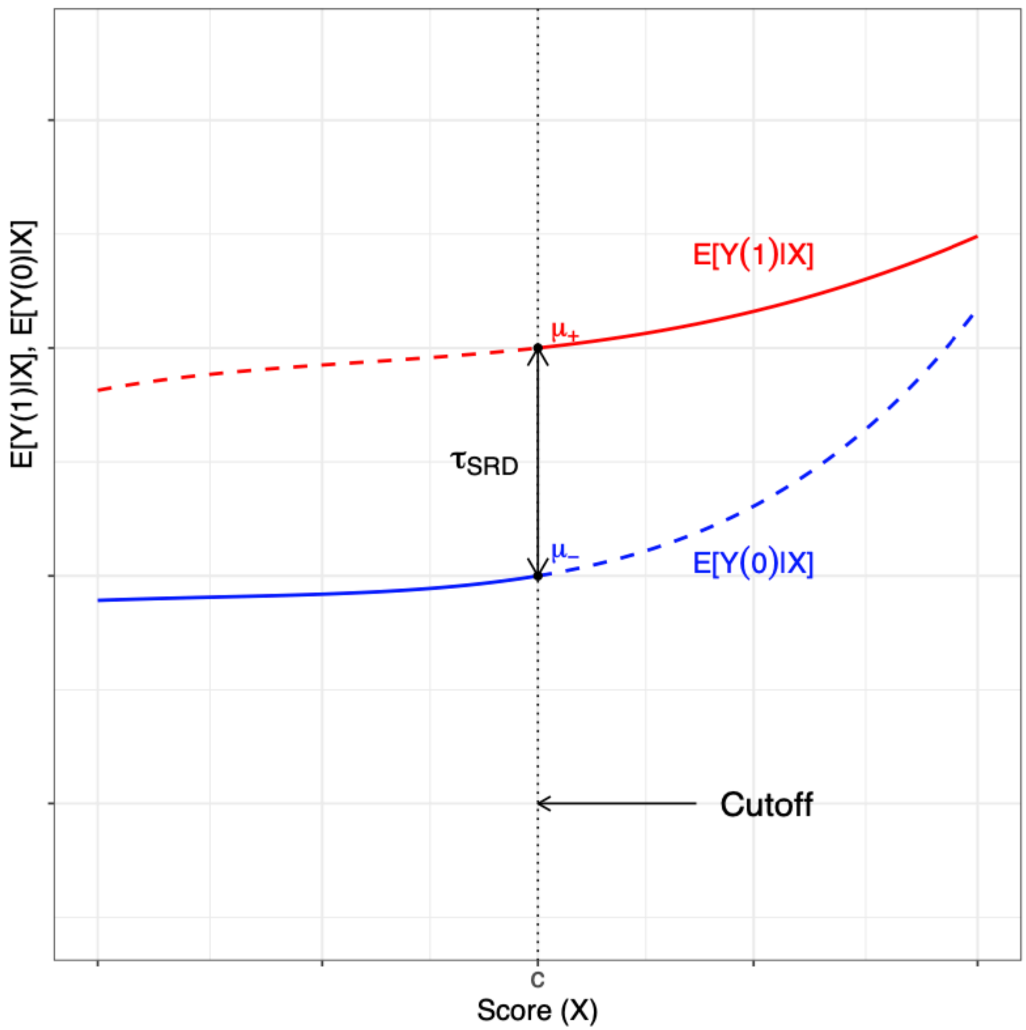
Continuity-based approach



- ATE is *identified* but *nonexistent* at cutoff
- Still, we can approximate gap

$$\lim_{x \downarrow c} E[Y|X = x] - \lim_{x \uparrow c} E[Y$$

Continuity-based approach



- ATE is *identified* but *nonexistent* at cutoff
- Still, we can approximate gap

$$\lim_{x\downarrow c} E[Y|X = x] - \lim_{x\uparrow c} E[Y$$

- This becomes a **line-drawing** problem

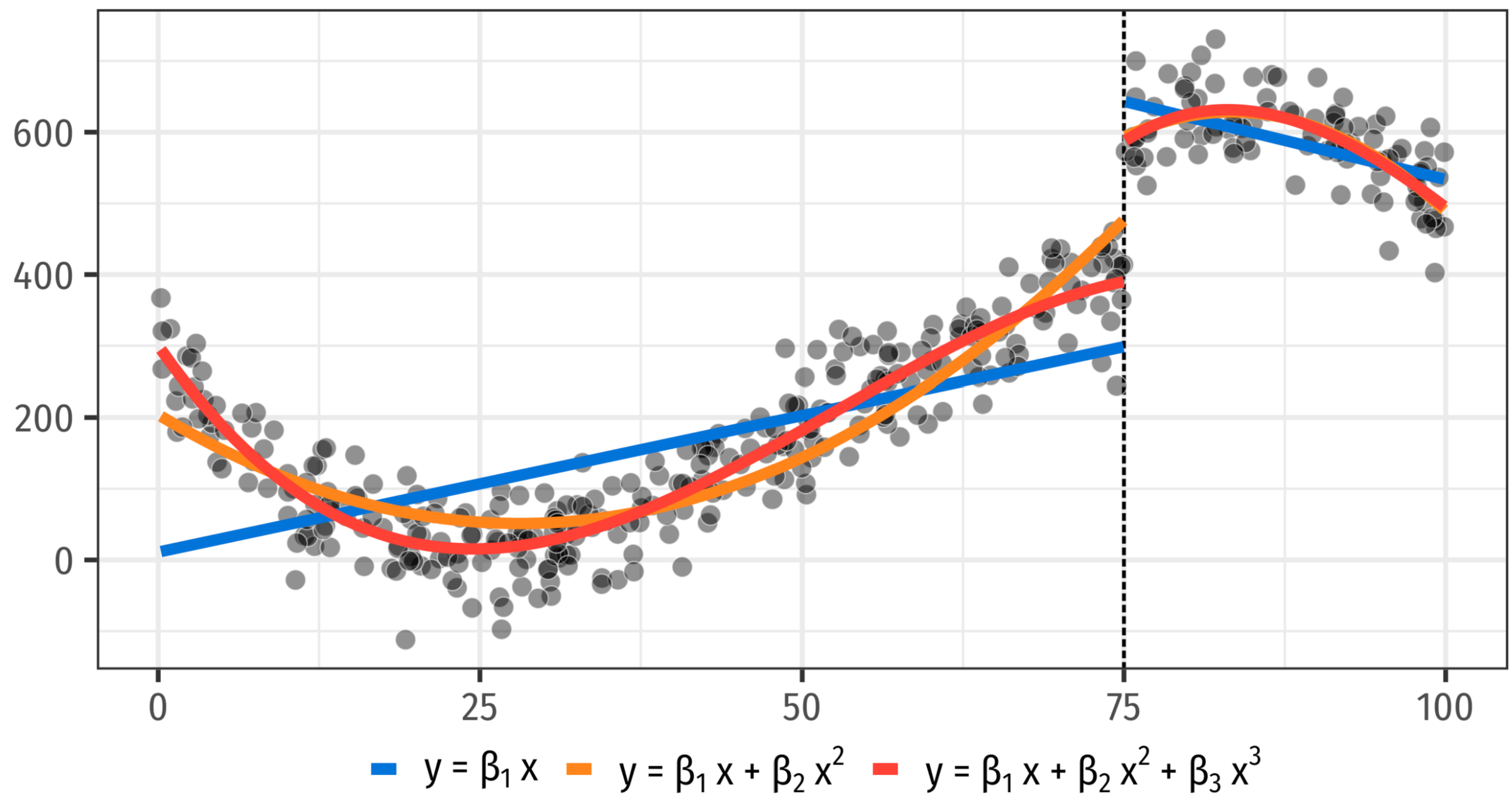
Local polynomial point estimation

Steps

1. Choose polynomial p
2. Choose kernel function $K(\cdot)$
3. Choose bandwidth h
4. Fit $\widehat{\mu}_+$ and $\widehat{\mu}_-$ via *weighted least-squares*
5. Estimate: $\widehat{ATE} = \widehat{\mu}_+ - \widehat{\mu}_-$
6. Inference: Correct for adaptive bandwidth selection

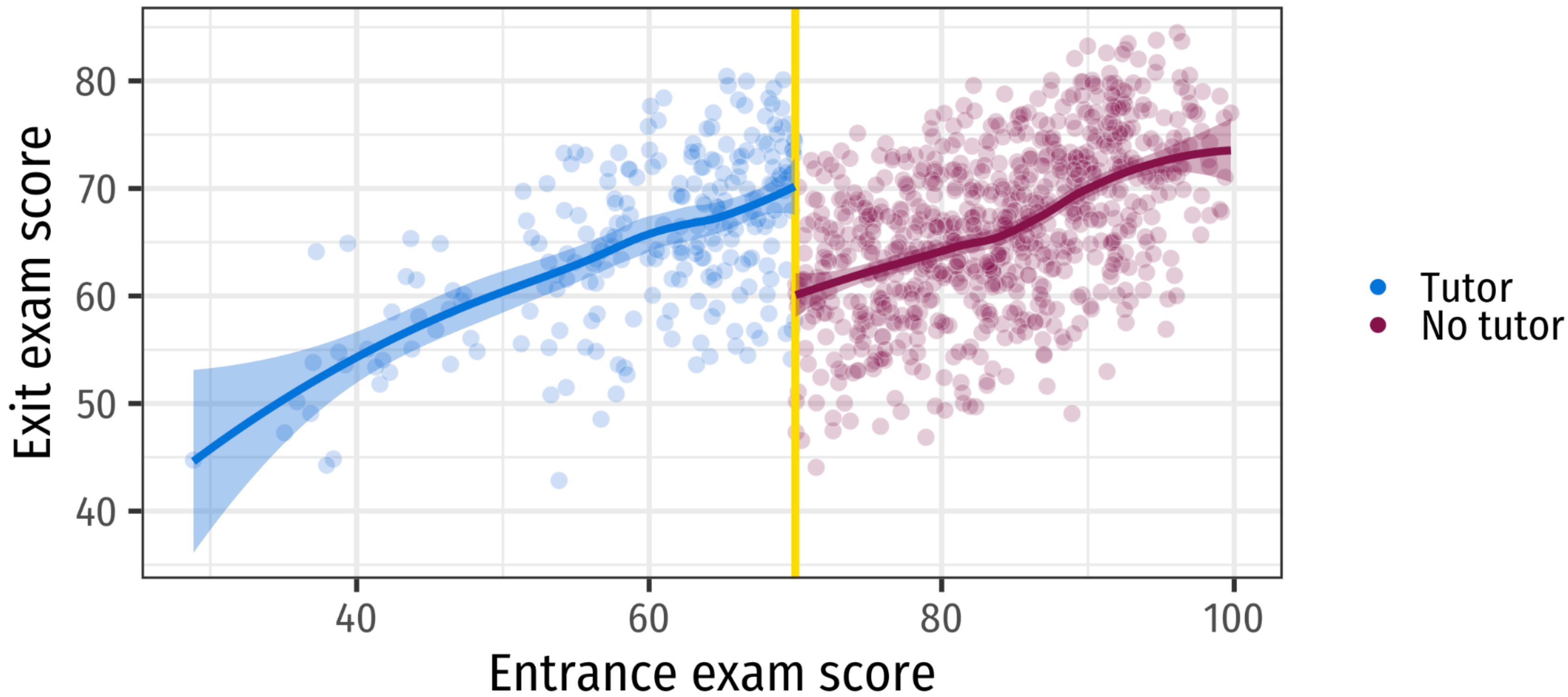
This a non-parametric procedure since most choices are automated

Line drawing: Parametric



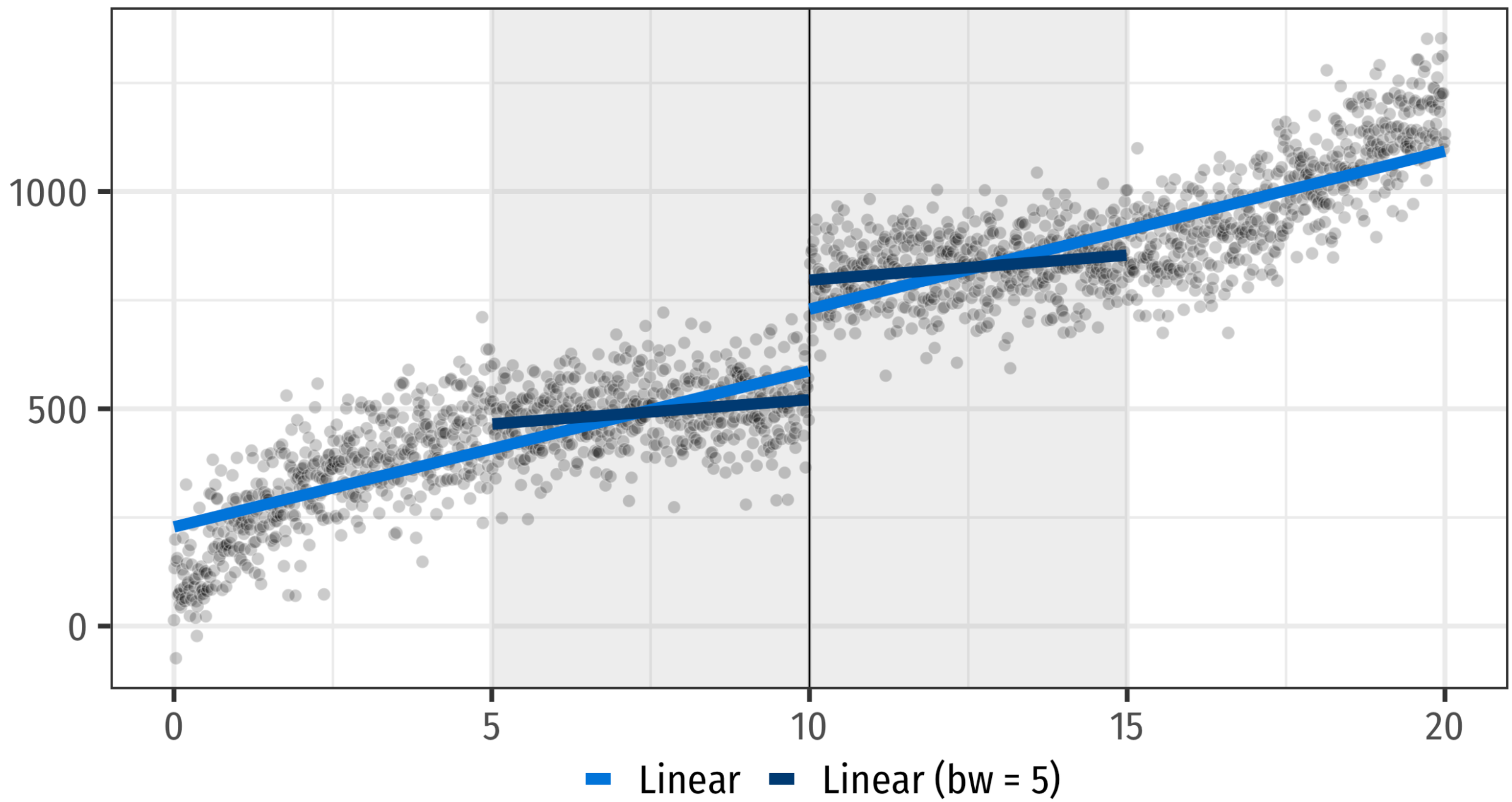
Different functional forms change the size of the gap

Line drawing: Nonparametric



These lines are made by an algorithm that calculates the local average at many windows or bins of data

Line drawing: Bandwidth



The size of the bandwidth determines the data you use to draw lines

Practice

Econometrica, Vol. 82, No. 1 (January, 2014), 229–269

ISLAMIC RULE AND THE EMPOWERMENT OF THE POOR AND PIOUS

BY ERIK MEYERSSON[1]

Does Islamic political control affect women's empowerment? Several countries have recently experienced Islamic parties coming to power through democratic elections. Due to strong support among religious conservatives, constituencies with Islamic rule often tend to exhibit poor women's rights. Whether this reflects a causal relationship or a spurious one has so far gone unexplored. I provide the first piece of evidence using a new and unique data set of Turkish municipalities. In 1994, an Islamic party won multiple municipal mayor seats across the country. Using a regression discontinuity (RD) design, I compare municipalities where this Islamic party barely won or lost elections. Despite negative raw correlations, the RD results reveal that, over a period of six years, Islamic rule increased female secular high school education. Corresponding effects for men are systematically smaller and less precise. In the longer run, the effect on female education remained persistent up to 17 years after, and also reduced adolescent marriages. An analysis of long-run political effects of Islamic rule shows increased female political participation and an overall decrease in Islamic political preferences. The results are consistent with an explanation that emphasizes the Islamic party's effectiveness in overcoming barriers to female entry for the poor and pious.

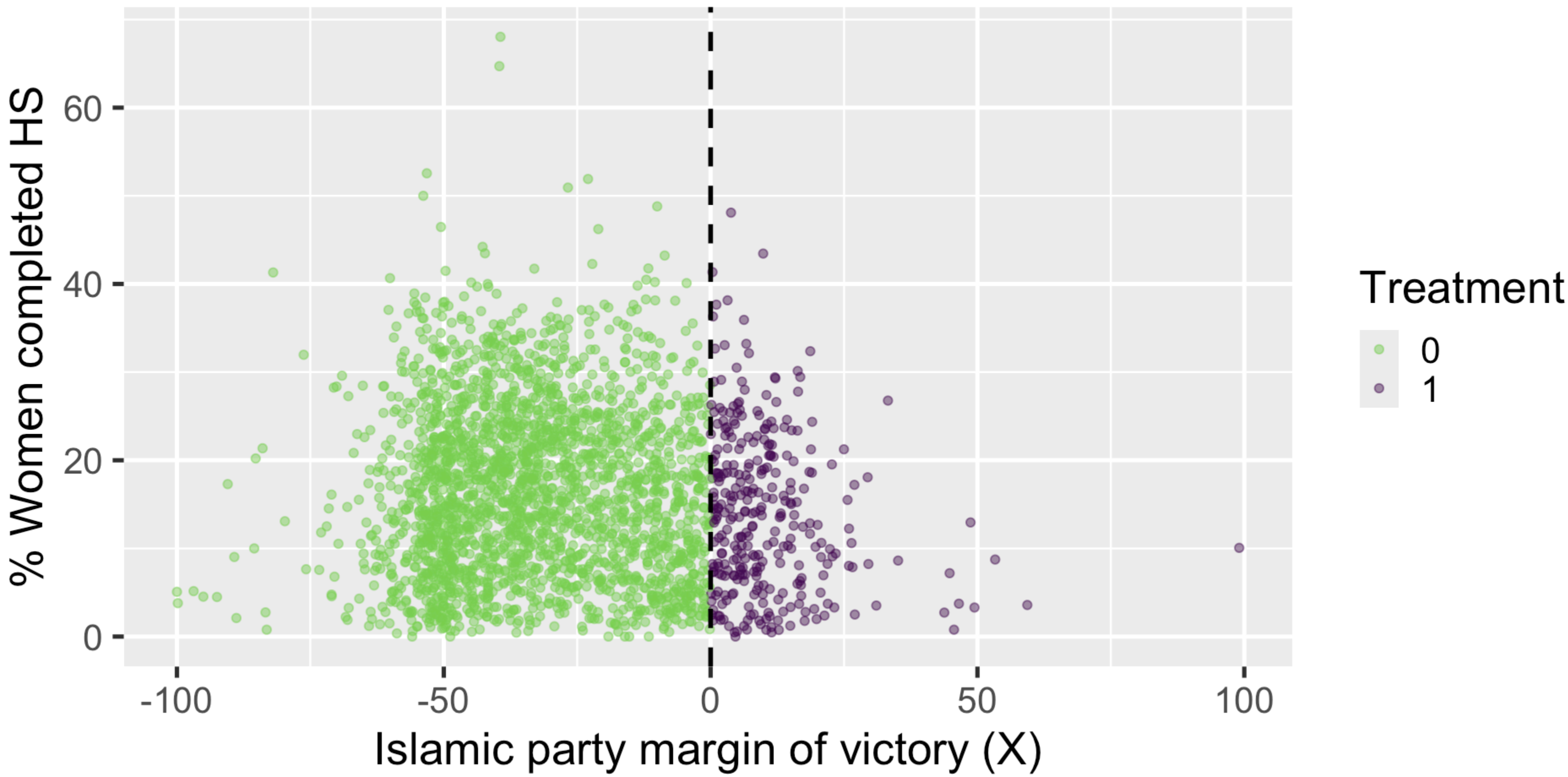
KEYWORDS: Political Islam, regression discontinuity, education.

https://doi.org/10.3982/ECTA9878

Ingredients

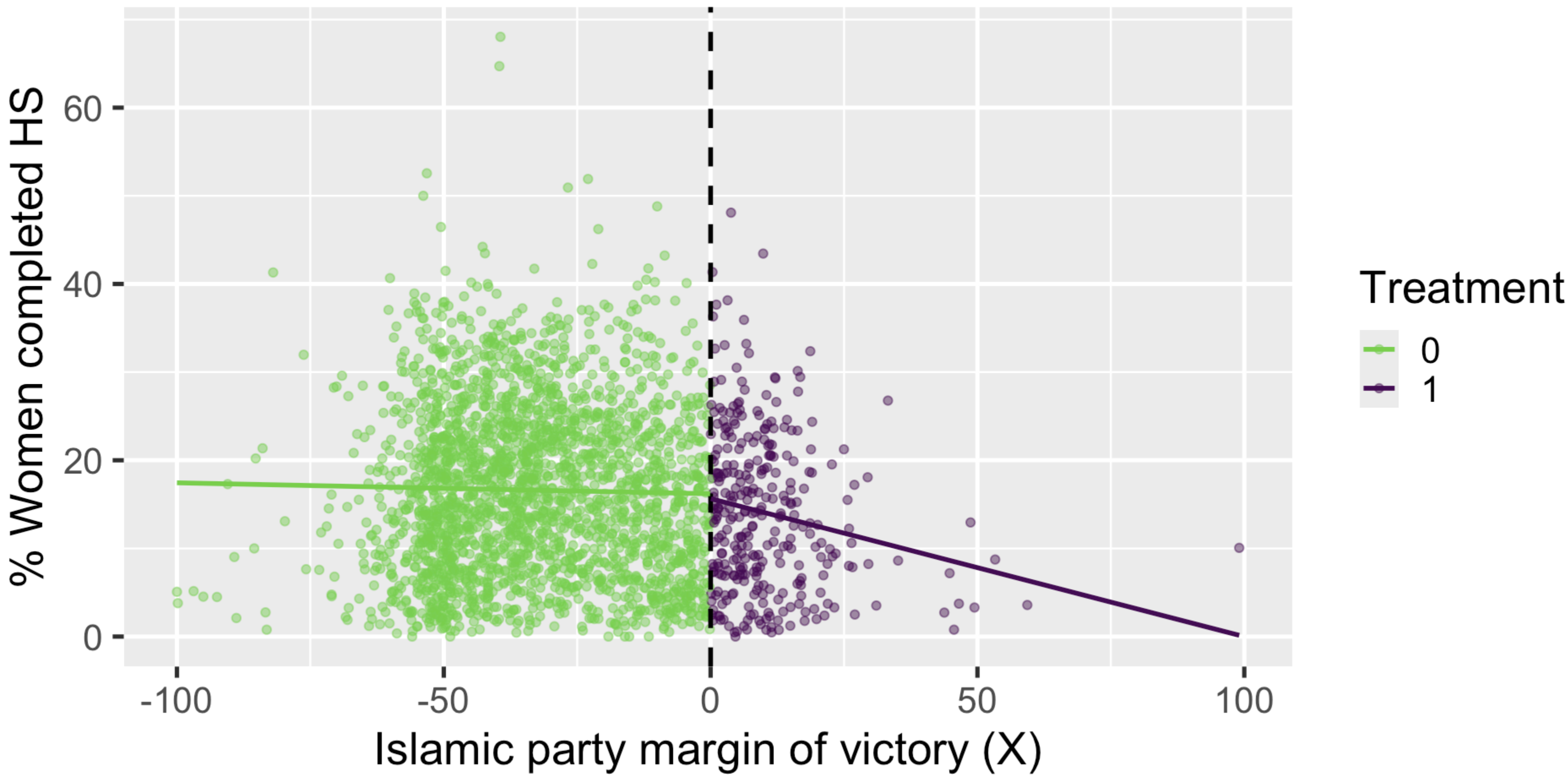
- Y: Percentage of young women who had completed high school by 2000 (outcome)
- X‘: Islamic parties’ margin of victory in the 1994 mayoral election (score)
- c: Implicit in score being centered at 0
- T: Whether a mayor from an Islamic party was elected in the 1994 election (treatment)

Visualize



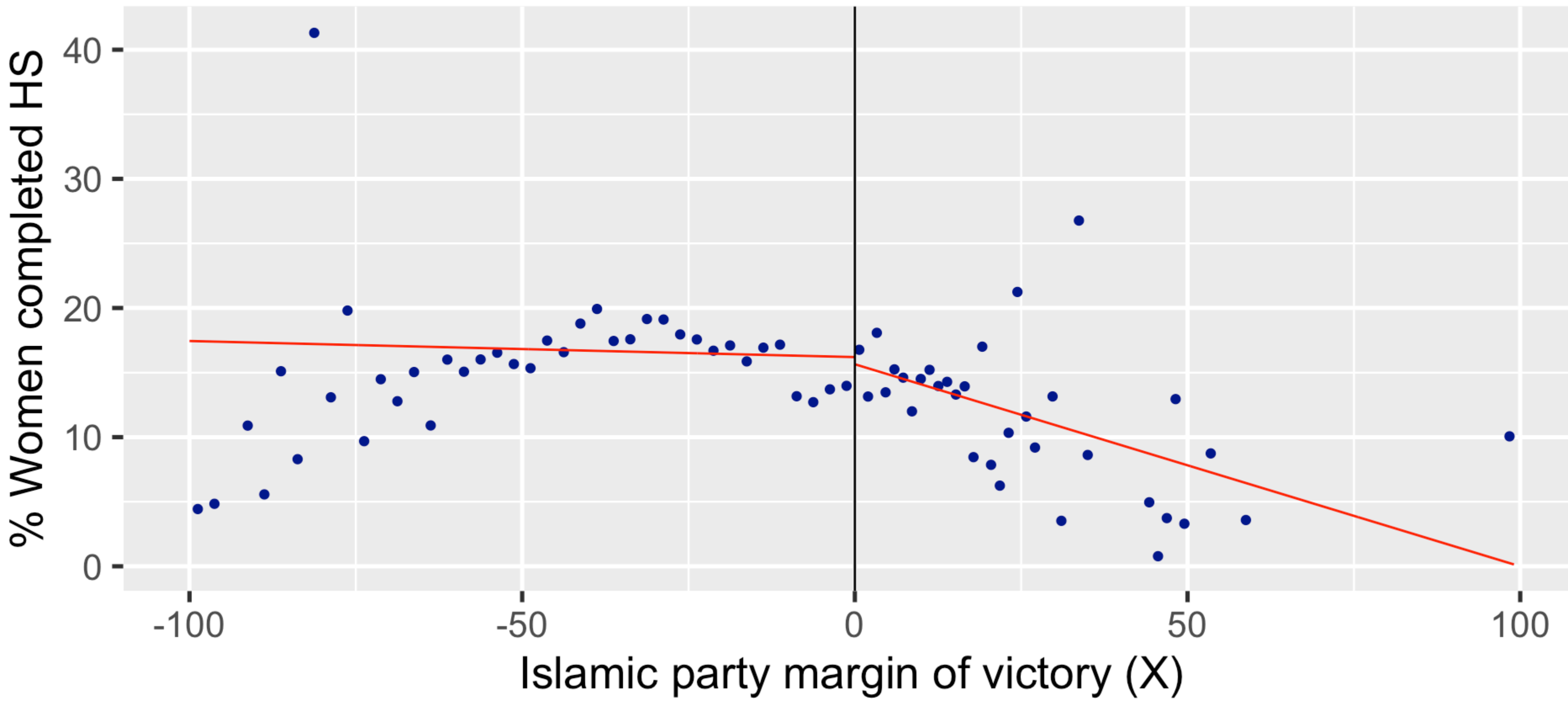
% Women completed HS
60
40
20
0
-100
-50
0
50
100
Islamic party margin of victory (X)
Treatment
0
1

Visualize



% Women completed HS
60
40
20
0
-100
-50
0
50
100
Islamic party margin of victory (X)
Treatment
0
1

Bin observations



Models

OLS baseline: $\widehat{Y} = \beta_0 + \beta_1 T + \beta_2 X$

Local randomization: 5% and 10% bandwidth

Continuity-based: Default automation

Results

Approach	bw	estimate	std.error	p.value

Results

Approach	bw	estimate	std.error	p.value
Baseline		-1.73	0.747	0.0204

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Baseline		-1.73	0.747	0.0204
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Local randomization	10	1.37	0.836	0.1013

Results

Approach	bw	estimate	std.error	p.value
Baseline		-1.73	0.747	0.0204
Local randomization	5	1.54	1.179	0.1924
Local randomization	10	1.37	0.836	0.1013
Continuity	17.2	3.02	1.427	0.0344

Summary

- RDD as a credible way to justify ignorability assumption
- Because local randomization and continuity-based approach
- Effects are **causal** but **local**
- Do they apply to other cases?
- Do they exist beyond the discontinuity?

Quasi-experiments

POLI SCI 210

Introduction to Empirical Methods in Political Science

Last time

- RDD as an example of how to justify ignorability by design
- But it is a very specific design!
- **Today:** Take advantage of before/after comparisons to circumvent ignorability assumption
- **Difference-in-differences** design

Example

[Ep1] The Battle with Cholera - John Snow [Public Health Influencers]

https://youtu.be/D2DzDDyKjR4?si=5h2JXBOPXtPZ5bG0

King of the Pump

How did John Snow prove that it was the water?

- Before/after comparison (1849-1854)
- **In between:** Lambeth water company forced to move upstream (away from dirty water)

Results

	Cholera deaths per 10,000	
Supplier	1849	1854

Adapted from: Coleman, Thomas. 2019. "Causality in the Time of Cholera: John Snow as a Prototype for Causal Inference."

Results

	Cholera deaths per 10,000	
Supplier	**1849**	**1854**
Lambeth (dirty to clean)	85	19

Adapted from: Coleman, Thomas. 2019. "Causality in the Time of Cholera: John Snow as a Prototype for Causal Inference."

Results

	Cholera deaths per 10,000	
Supplier	**1849**	**1854**
Lambeth (dirty to clean)	85	19
Others (dirty to dirty)	135	147

Moving away from the 💩 certainly helped!

Adapted from: Coleman, Thomas. 2019. “Causality in the Time of Cholera: John Snow as a Prototype for Causal Inference.”

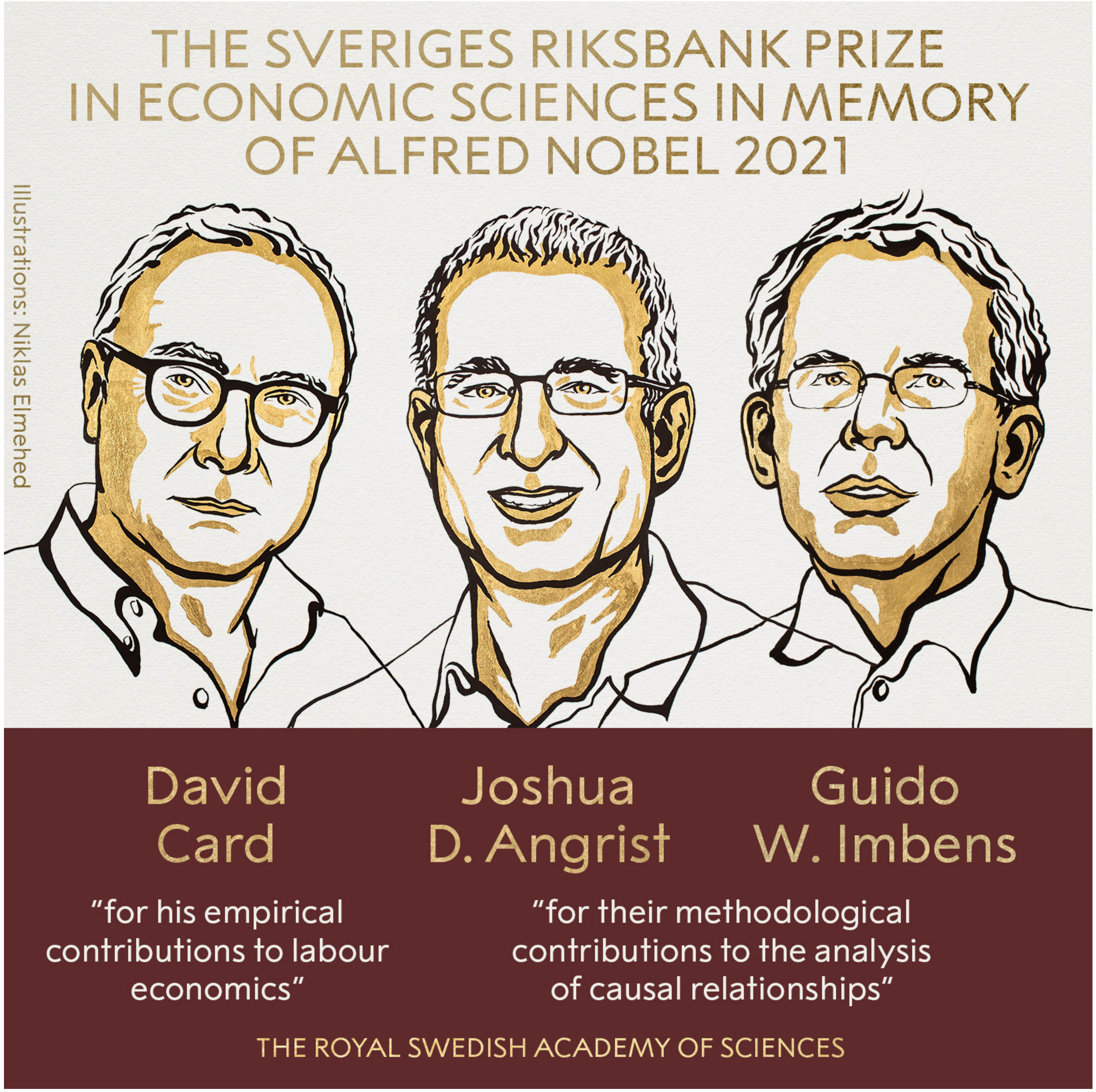
Another example

Minimum Wages and Employment:
A Case Study of the Fast-Food Industry
in New Jersey and Pennsylvania

By David Card and Alan B. Krueger*

On April 1, 1992, New Jersey's minimum wage rose from $4.25 to $5.05 per hour. To evaluate the impact of the law we surveyed 410 fast-food restaurants in New Jersey and eastern Pennsylvania before and after the rise. Comparisons of employment growth at stores in New Jersey and Pennsylvania (where the minimum wage was constant) provide simple estimates of the effect of the higher minimum wage. We also compare employment changes at stores in New Jersey that were initially paying high wages (above $5) to the changes at lower-wage stores. We find no indication that the rise in the minimum wage reduced employment. (*JEL* J30, J23)

Card, David and Alan B. Krueger (1994). "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania." *American Economic Review* 84 (4): 772-793


THE SVERIGES RIKSBANK PRIZE
IN ECONOMIC SCIENCES IN MEMORY
OF ALFRED NOBEL 2021
Illustrations: Niklas Elmehed
David
Card
Joshua
D. Angrist
Guido
W. Imbens
"for his empirical
contributions to labour
economics"
"for their methodological
contributions to the analysis
of causal relationships"
THE ROYAL SWEDISH ACADEMY OF SCIENCES

Minimum wage and employment

- **Agreement:** Increasing minimum wage reduces employment
- **New Jersey 1992:** \$4.25 → \$5.05
- **Pennsylvania:** Stay at \$4.25
- Look at **full time employment** (FTE) in *fast food restaurants*
- Compare February-November 1992

Results

Variable
FTE before
FTE after
Change

Results

Variable	PA
FTE before	23.300
FTE after	21.147
Change	-2.160

Results

Variable	PA	NJ
FTE before	23.300	20.44
FTE after	21.147	21.03
Change	-2.160	0.59

Results

Variable	PA	NJ	NJ-PA
FTE before	23.300	20.44	-2.89
FTE after	21.147	21.03	-0.14
Change	-2.160	0.59	2.76

Increasing minimum waged created **more** jobs in NJ!

Difference-in-differences design

- Time periods: $t = \{1, 2\}$ (Before/after treatment)
- Treatment: $D_i = \{0, 1\}$
- Potential outcomes: $Y_{i,t}(0) = Y_{i,t}(0, 0)$ and $Y_{i,t}(1) = Y_{i,t}(0, 1)$

Switching equation

$$Y_{i,t} = D_i Y_{i,t}(1) + (1 - D_i) Y_{i,t}(0)$$

aka DD, DiD, diff-in-diffs for short

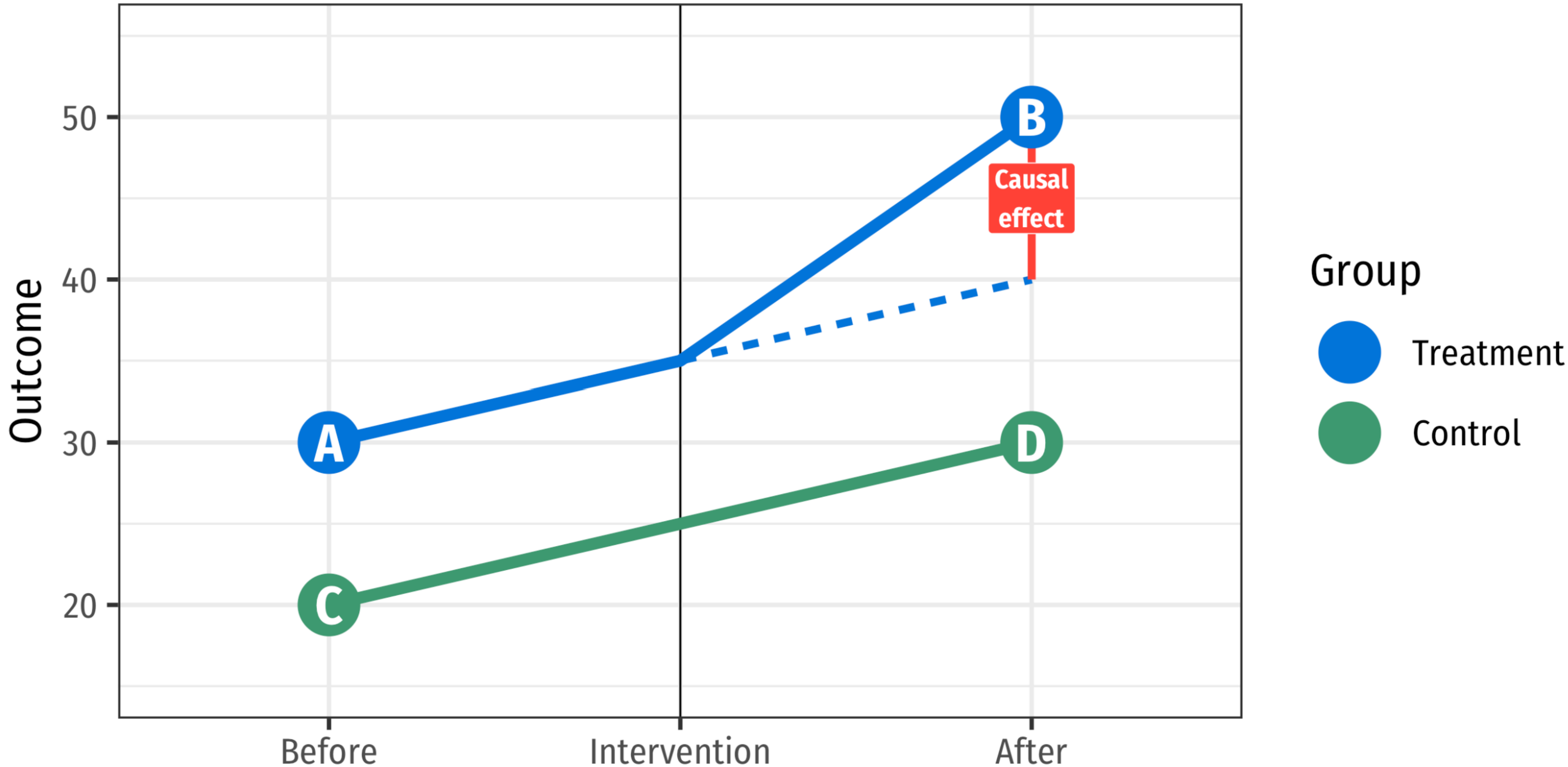
Target quantity

Average treated effect on the treated (ATT) in $t = 2$

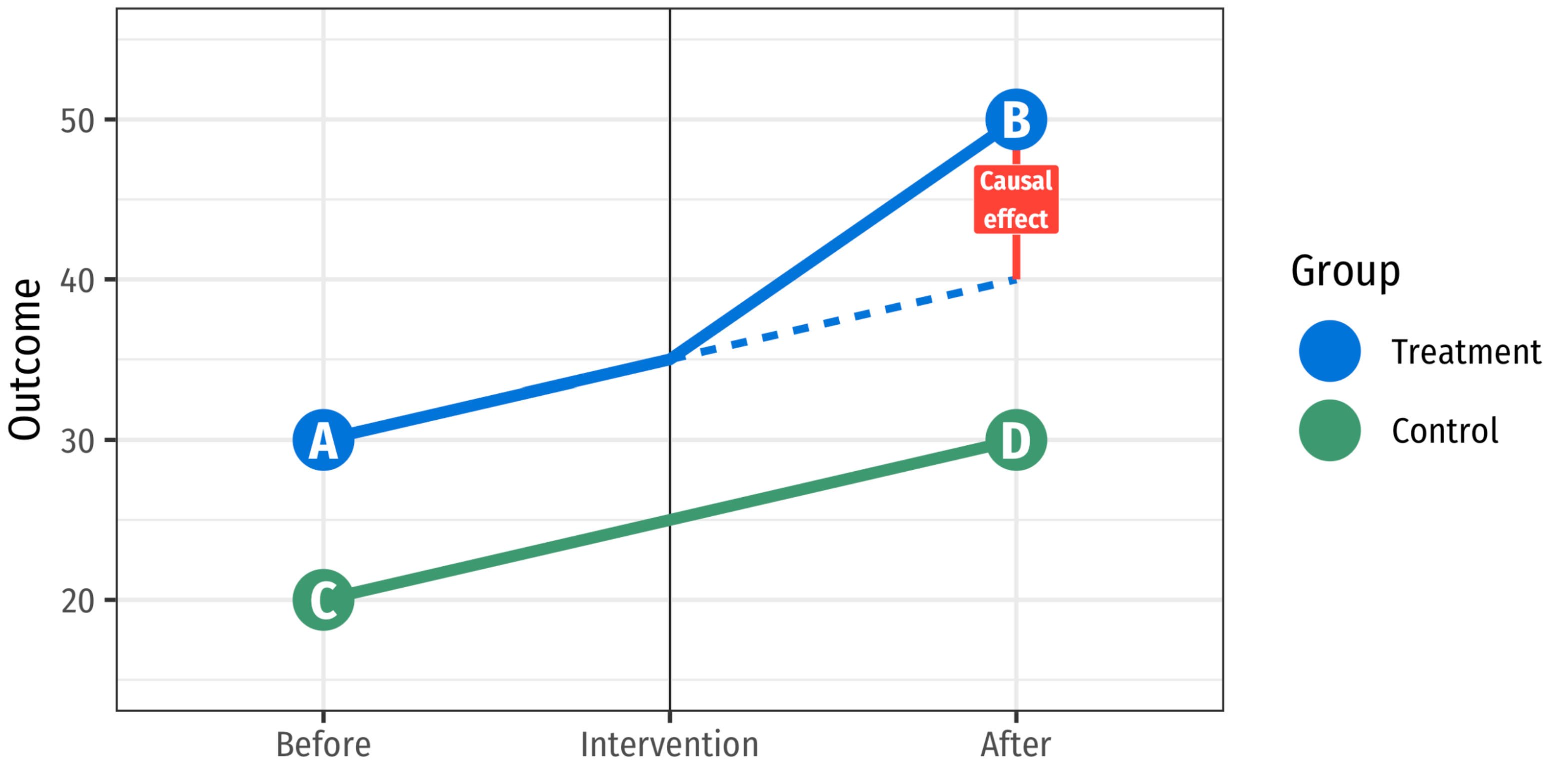
$$\tau_2 = E[Y_{i,2}(1) - Y_{i,2}(0) | D_i = 1]$$

- Cannot observe directly
- Cannot avoid *selection bias*
- But before/after setup allows for credible estimation

Target quantity

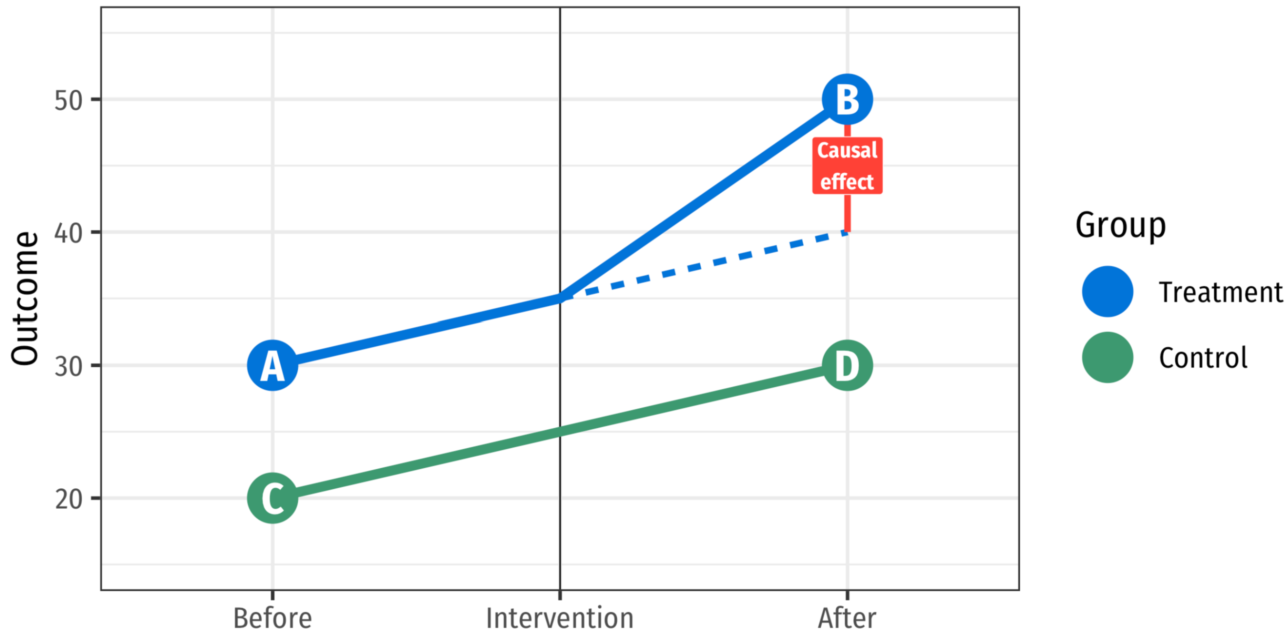


DID estimation



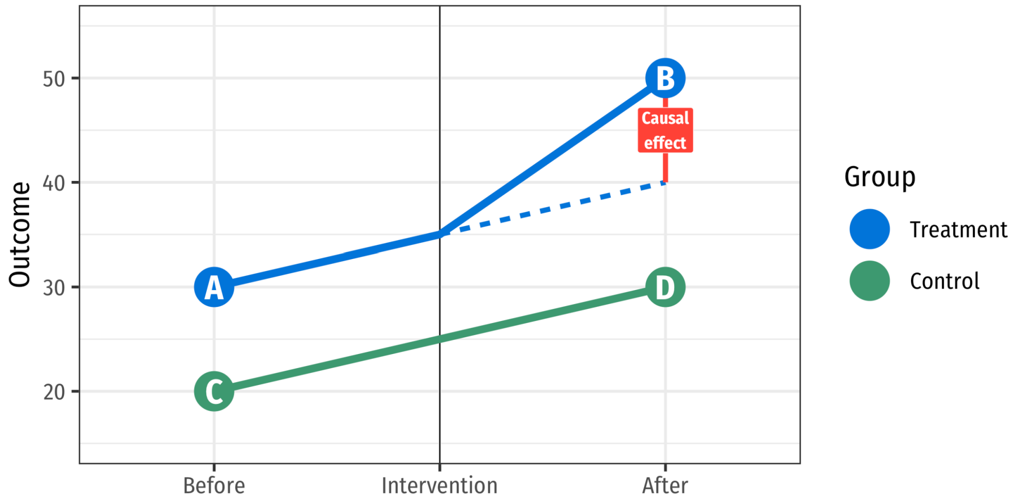
$$\widehat{ATT} = [\text{Mean}(B) - \text{Mean}(A)] - [\text{Mean}(D) - \text{Mean}(C)]$$

DID estimation



$$\widehat{ATT} = \underbrace{[\text{Mean}(B) - \text{Mean}(A)]}_{\text{Difference}} - \underbrace{[\text{Mean}(D) - \text{Mean}(C)]}_{\text{Difference}}$$

DID estimation



$$\widehat{ATT} = \underbrace{\underbrace{[\text{Mean}(B) - \text{Mean}(A)]}_{\text{Difference}} - \underbrace{[\text{Mean}(D) - \text{Mean}(C)]}_{\text{Difference}}}_{\text{Difference-in-differences}}$$

Rewrite as a regression

Two time periods

$$
\begin{aligned}
Y =& \beta_0 + \beta_1 \text{Treated} + \beta_2 \text{Post-treatment} + \\
& \color{purple}{\beta_3 \text{Treated} \times \text{Post-treatment}}
\end{aligned}
$$

- β_0: Avg. control group, before treatment
- $\beta_0 + \beta_1$: Avg. treatment group, before treatment
- $\beta_0 + \beta_2$: Avg. control group, post-treatment
- $\beta_0 + \beta_1 + \beta_2 + \beta_3$: Avg. treatment group, post-treatment

β_3 gives the **difference-in-differences**

Rewrite as a regression

Multiple time periods

$$Y = \alpha_i + \alpha_t + \beta_1 \text{Treated}$$

- α_i: Control for variation **within units**
- α_t: Control for variation **over time**
- α_i and α_t are called **fixed-effects**

This is a *two-way fixed-effects* estimator (TWFE)

Example with multiple periods

NBER WORKING PAPER SERIES

DON'T TAKE 'NO' FOR AN ANSWER:
AN EXPERIMENT WITH ACTUAL ORGAN DONOR REGISTRATIONS

Judd B. Kessler
Alvin E. Roth

Working Paper 20378
http://www.nber.org/papers/w20378

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2014

https://www.nber.org/papers/w20378

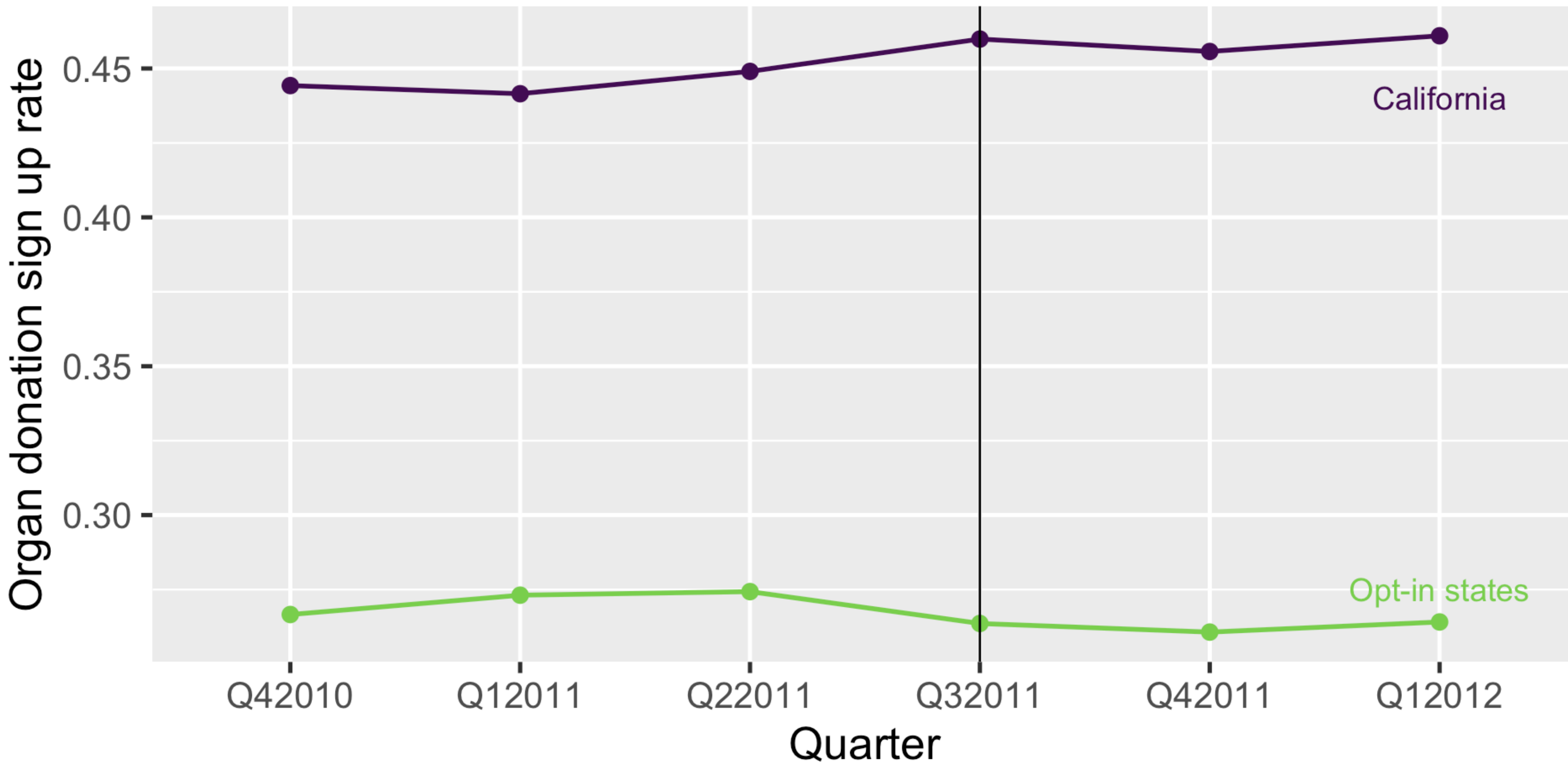
Table 1: Organ Donor Registration Questions by State

Active Choice		
Positive Wording	*Negative Wording*	*States*
"Yes"	"No"	AK, CT, GA, HI, IA, LA, MA, MS, NE, NV, NJ, NM, ND, OR, PA, RI, TX, UT, VT, WV, WY
"YES, add my name to the donor registry"	"I do not wish to register at this time"	CA
"Yes, add my name"	"No, not at this time"	MD
"Yes"	"Skip this question"	NY
"Yes"	"Not now"	MT
Verbal question: No fixed response		AR, CO, DE, FL, ID, IL, IN, KS, KY, ME, MI, MO, NC, OH, OK, WA
Opt-In		
Positive Wording		*States*
"Yes"		TN, WI, DC
"I want to be an organ and tissue donor. By checking this box, Donor Network of AZ will add me to the Donate Life AZ Registry"		AZ
"I want my license or ID card to show that I choose to be an organ and tissue donor under the Uniform Anatomical Gift Act"		MN
"Check here to consent to organ & tissue donation"		NH
"YES, I want to be an organ and tissue donor."		SC
"In the event of my death, I would like to be an organ/tissue donor."		SD
"Yes, I would like to remain or become an organ, eye and tissue donor."		VA

Intervention

- **2011:** California switches from *opt-in* → *active choice*
- Compare with states that *remain opt-in* (AZ, DC, MN, NH, TN, SC, SD, VA, WI)
- **Outcome:** Average sign-up rates per quarter per state

Comparison over time



Opt-in states: AZ, DC, MN, NH, TN, SC, SD, VA, WI

Models

Two-periods:

$$\widehat{\texttt{rate}} = \beta_0 + \beta_1 \texttt{california} + \beta_2 \texttt{post} + \beta_3 \texttt{california} \times$$

Two-wave fixed-effects:

$$\widehat{\texttt{rate}} = \alpha_{\texttt{state}} + \alpha_{\texttt{quarter}} + \beta_1 \texttt{treated}$$

Models

Two-periods:

$$\widehat{\texttt{rate}} = \beta_0 + \beta_1 \texttt{california} + \beta_2 \texttt{post} + \beta_3 \texttt{california} \times$$

Two-wave fixed-effects:

$$\widehat{\texttt{rate}} = \beta_0 + \beta_1 \texttt{california-post} + \beta_2 \texttt{state} + \beta_3 \texttt{quarte}$$

Use **clustered standard errors** for statistical inference

Results

Model	estimate	conf.low	conf.high	p.value

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Model	estimate	conf.low	conf.high	p.value
Two-periods (no clustering)	-0.0225	-0.2699	0.225	0.857941

Results

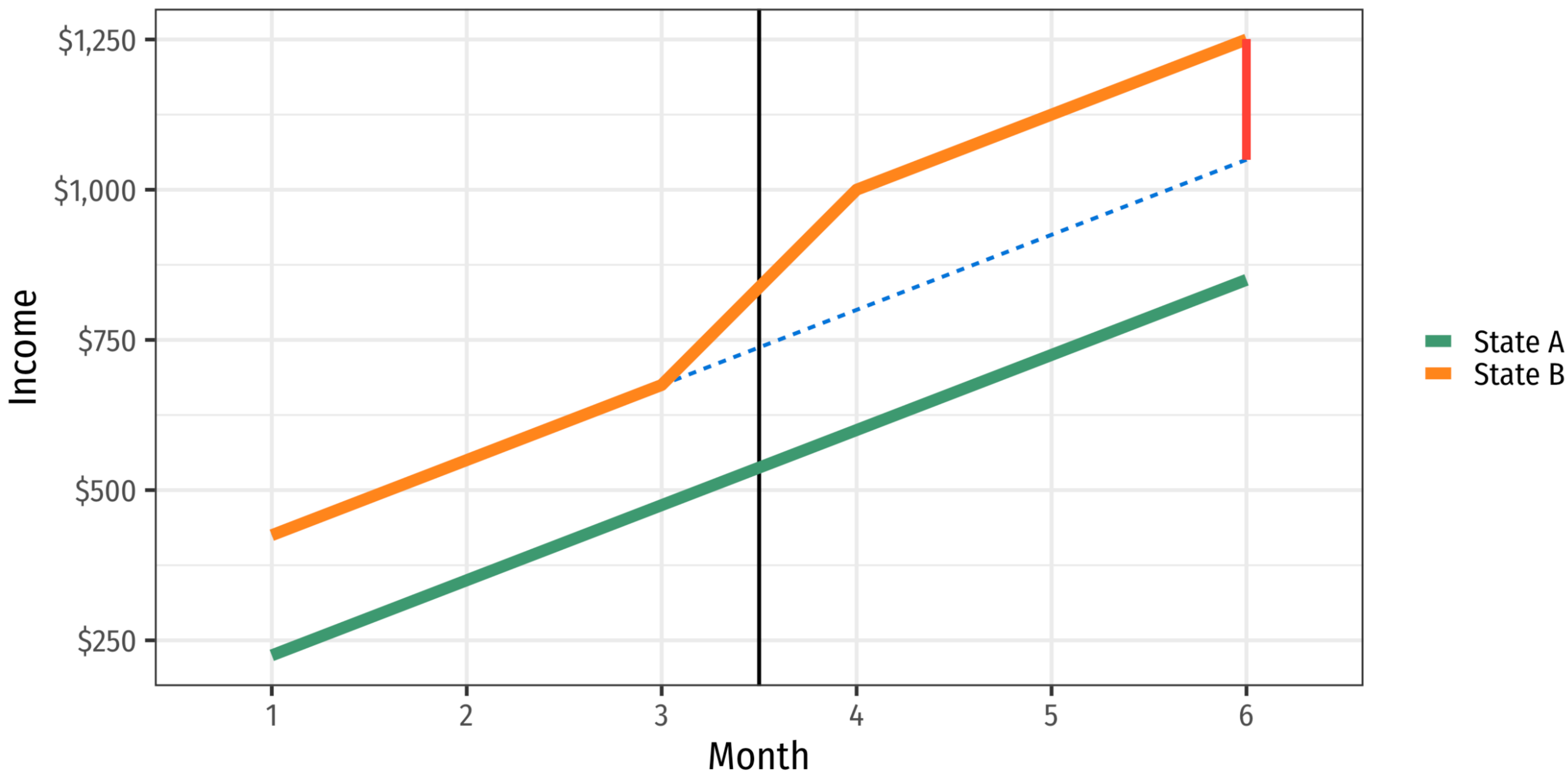
Model	estimate	conf.low	conf.high	p.value
Two-periods (no clustering)	-0.0225	-0.2699	0.225	0.857941
Two-periods (clustered)	-0.0225	-0.0349	-0.0101	0.000986

Results

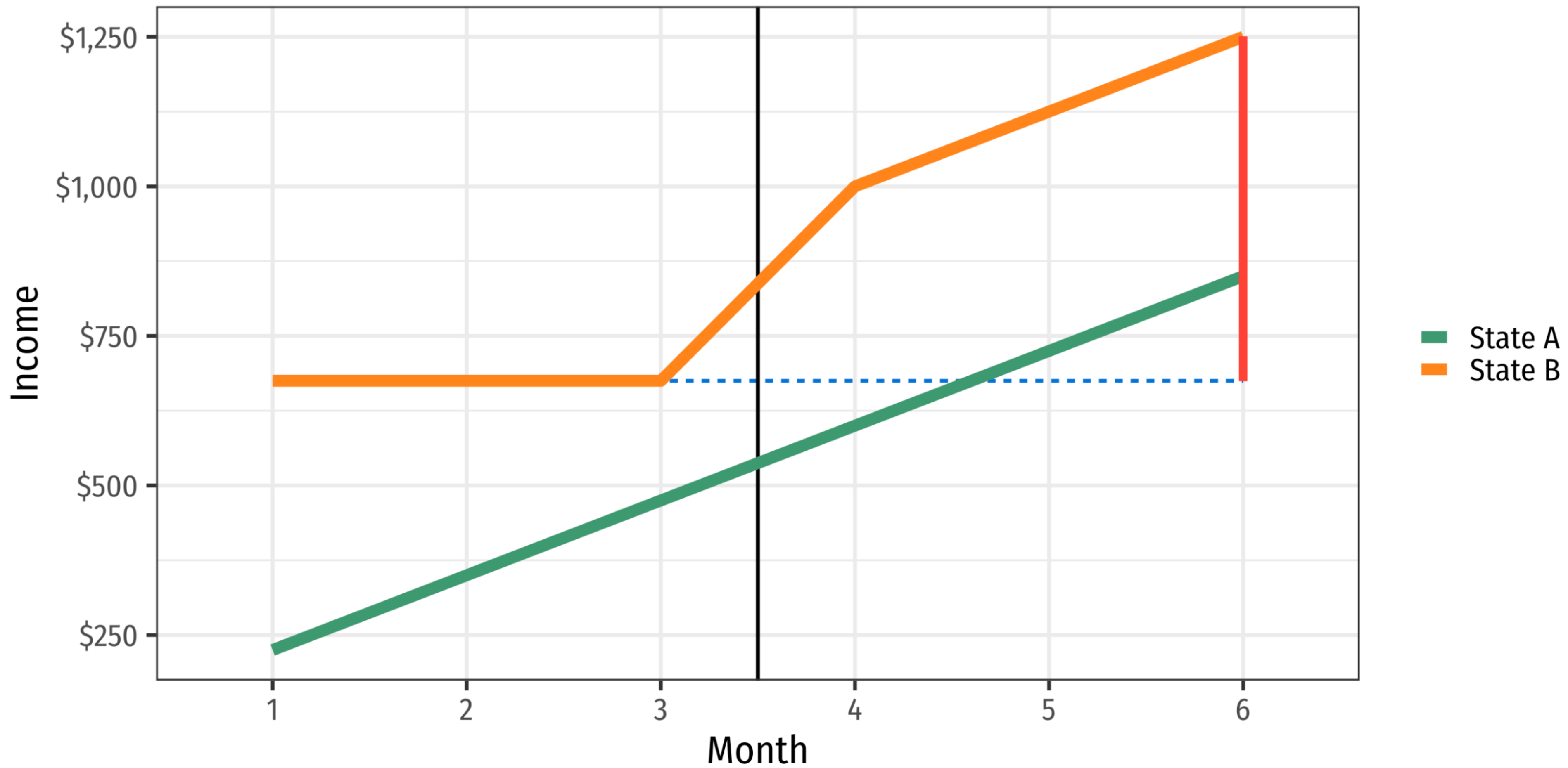
Model	estimate	conf.low	conf.high	p.value
Two-periods (no clustering)	-0.0225	-0.2699	0.225	0.857941
Two-periods (clustered)	-0.0225	-0.0349	-0.0101	0.000986
Two-way FE	-0.0225	-0.0349	-0.0101	0.000986

These estimates are only *valid* under a **BIG assumption**

Assumption: Parallel trends



What if we break parallel trends?



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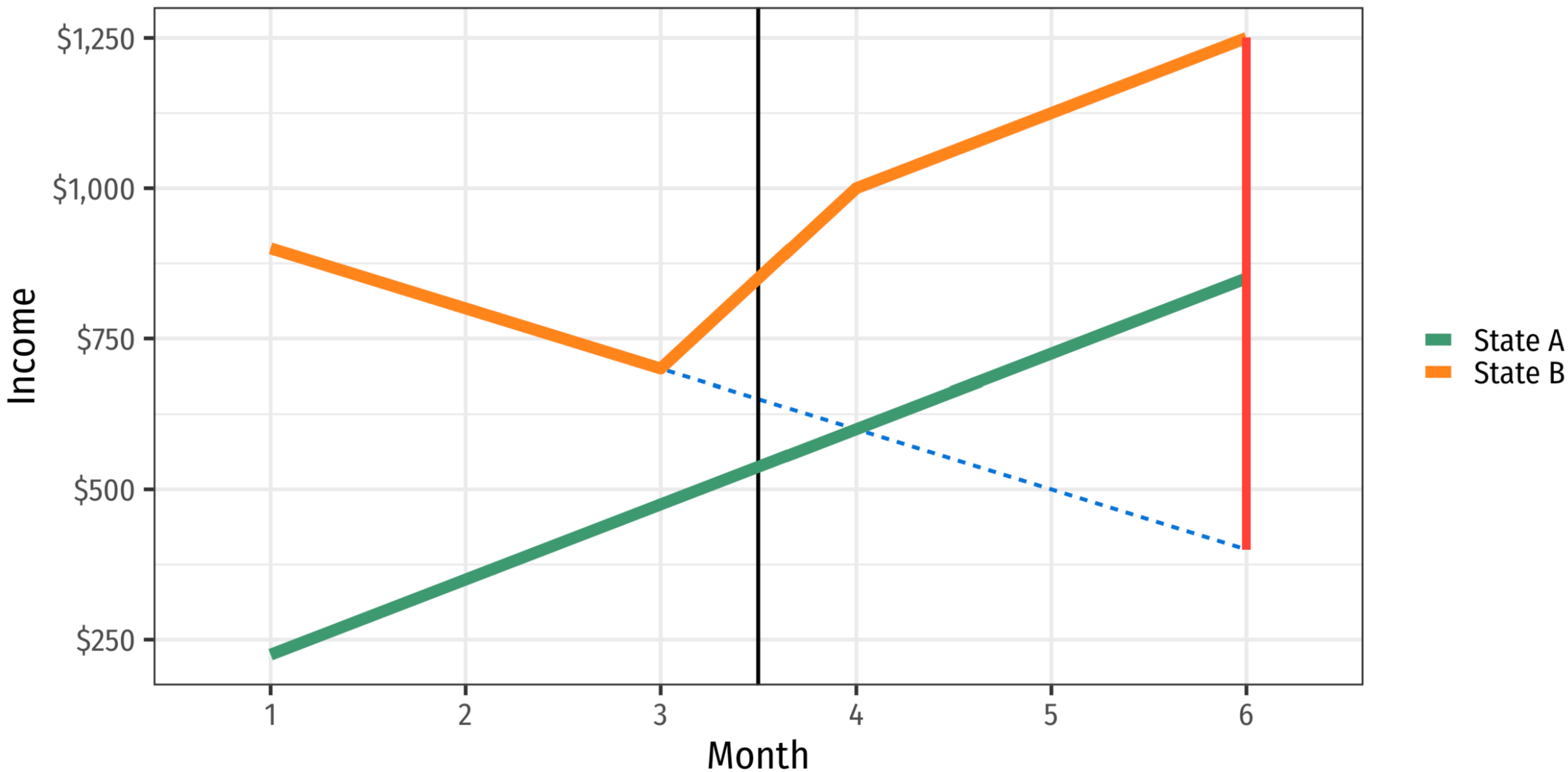
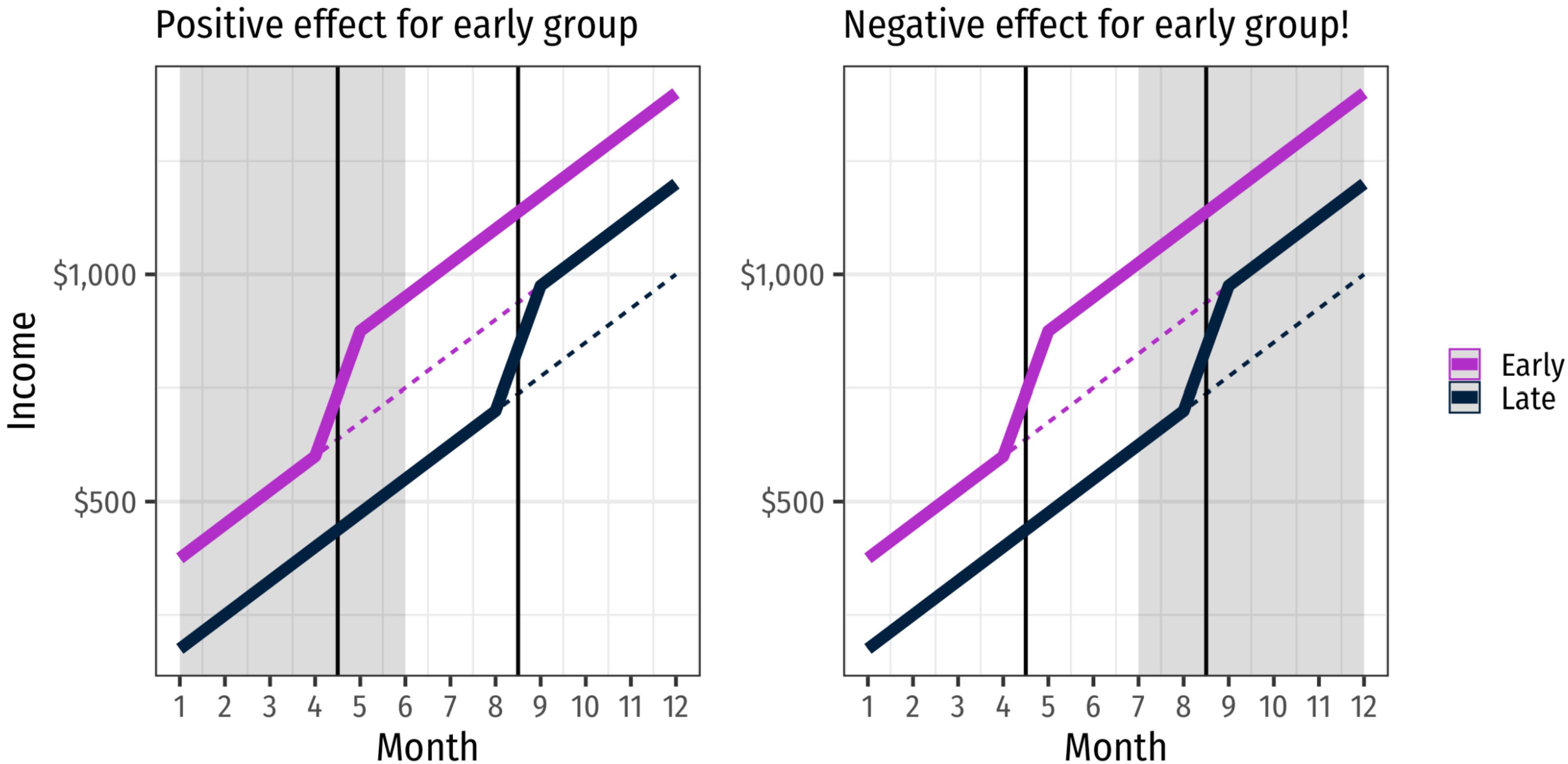


Challenge: Staggered adoption



Summary

- Difference in difference leverages before-after comparisons between treatment and control group to tease out causal effects
- **Highly local:** Focus on average treatment effect *on the treated*
- Could always change the control group
- Parallel trends assumption is very important!