Causal methods with observational data

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Research Design for Social Sciences
MA Computational Social Science, UC3M
Fall 2024

Roadmap

Intro and overview

Methods in brief

Paper discussion

Re-cap and essay guidelines

Re-cap

1. Problem/topic
2. Stories, arguments about mechanisms
3. Research question
4. Proper theory, concepts and operationalization
5. Measurement, unit of analyses, data sources, etc
6. Inference strategy
7. Results & interpretation

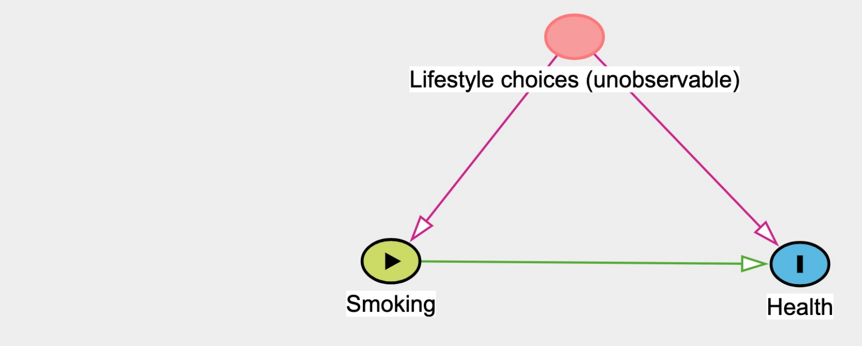
Methods and causal inference

- Most of the time is **impossible** to control for all relevant variables (i.e. not able to close all back-door paths)
- So what do we do? We try to find ways to control for unobserved confounding
- One option is to rely on **additional controlling techniques**
  - → Matching: also depends on observables, but parametric advantages, etc (anyway, not a solution for U)
  - → Fixed effects: can control for group-level unobservables, and correct issues related to ecological fallacy

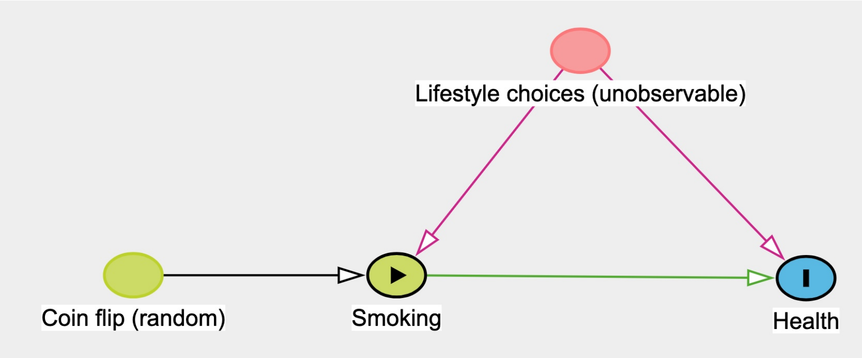
Methods and causal inference

- Another option is to find a source of exogenous variation and exploit
- There are a few methods related to causal inference not because they uncover causal relationships but because they are designed to exploit *'typical' exogenous sources of variation*
- Situations where there is *something* that introduces variation in the treatment that is independent from confounders and you can exploit to analyse
  - $\rightarrow$ Same as randomization (of treatment assignment) in experiments
- (In H-K's *The Effect*, they're called 'template causal diagrams')

Exogenous variation



Exogenous variation



Methods and causal inference

- Five techniques commonly used in causal inference
  - → two of them use for controlling (closing back-doors), and the other three to exploit ‘pre-made‘ causal models
  - → btw, what is controlling? when you control for Z, you remove the variation in X and Y that is *explained by Z* (see this)

Methods and causal inference

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  - → btw, what is controlling? when you control for Z, you remove the variation in X and Y that is *explained by Z* (see this)

1. Fixed effects
2. Difference-in-differences
3. Regression discontinuity design
4. Instrumental variables
5. Matching

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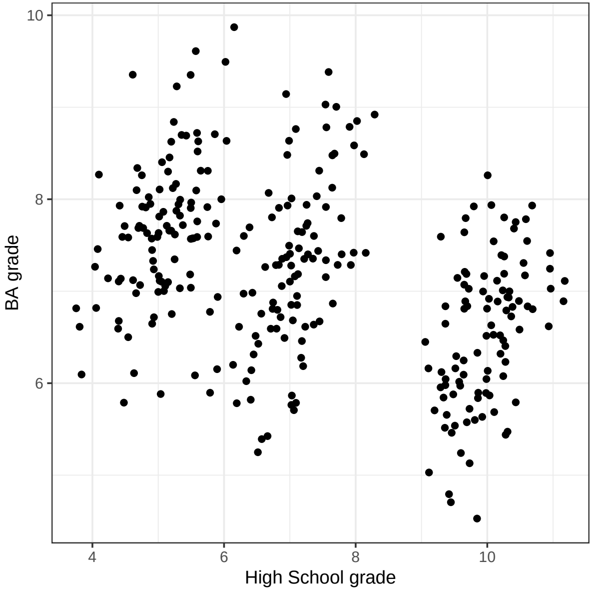
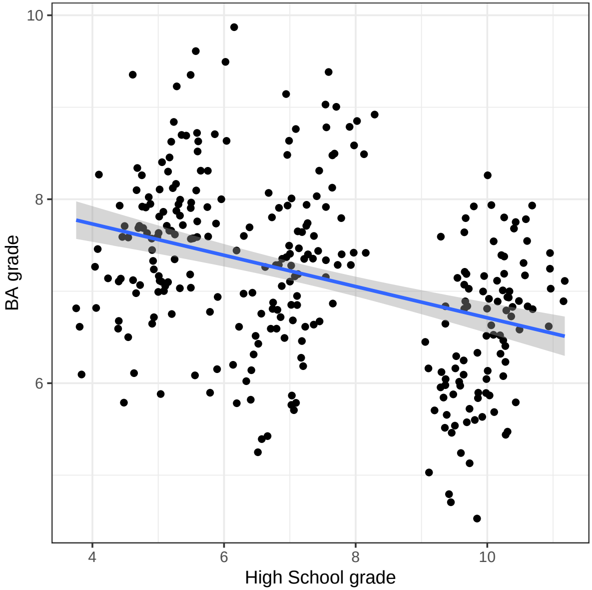
Fixed effects

- The problem with covariate adjustment is that we need to *observe* those variables, but we usually have unobserved confounders
- An strategy in that case is to try within-group comparisons, which blocks all group-wide confounding
- For example, imagine cases when our U variable is:
  - → *city of origin*, in an individual-level analysis; *school effects* in an analyses of students grades; *individual background*, in a panel survey analysis...
- It corrects for ecological fallacy (esp. Simpson's paradox)
- Estimation: dummy variable for each group

Fixed effects



Fixed effects



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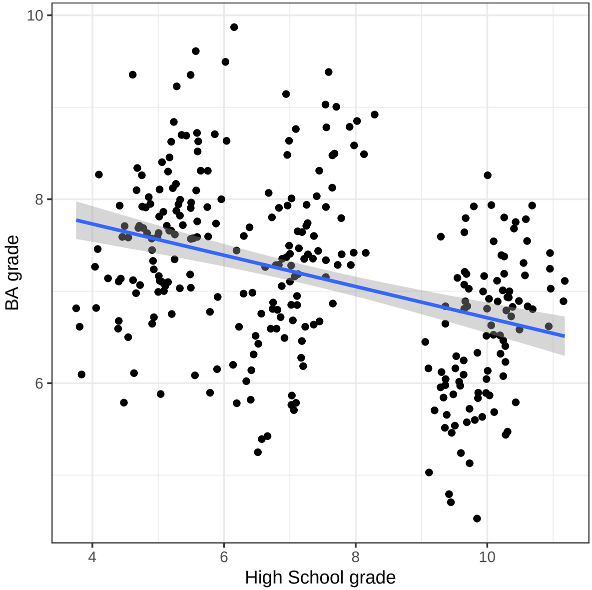

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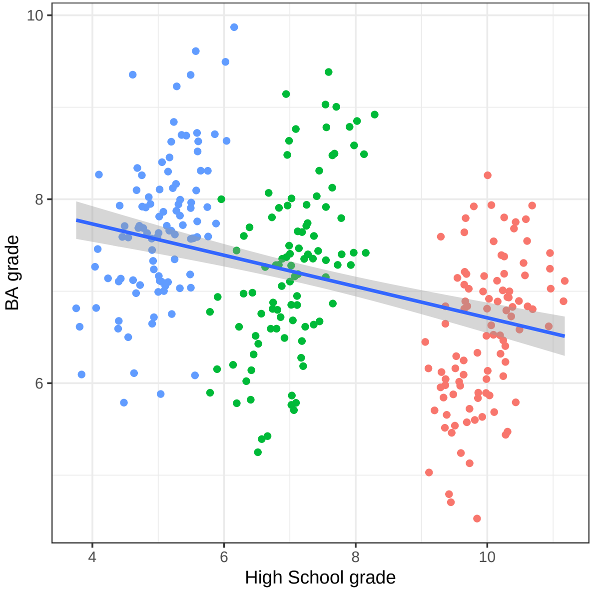
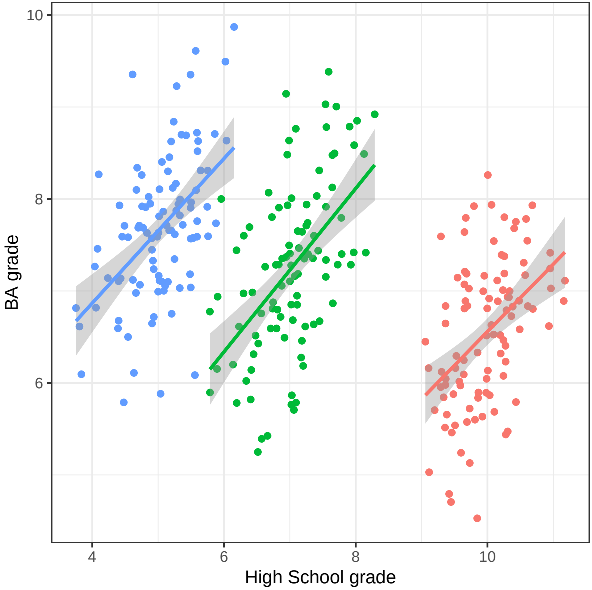
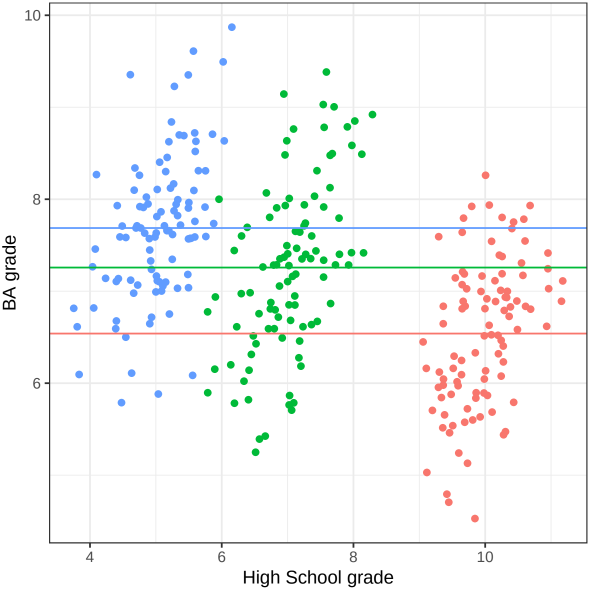


Fixed effects



Fixed effects



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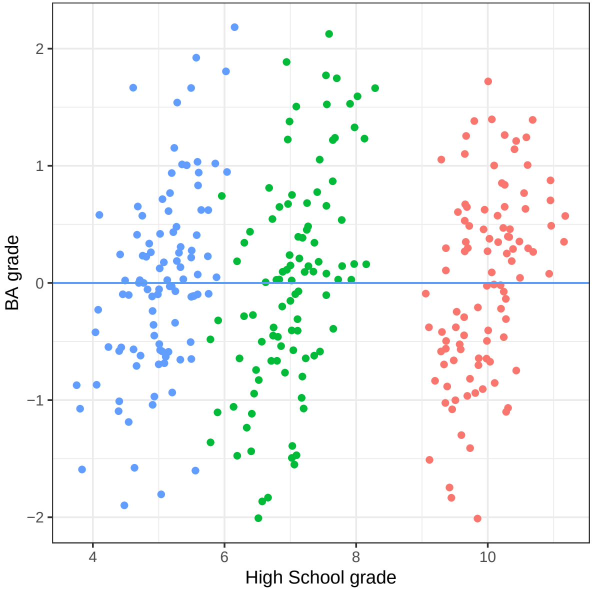
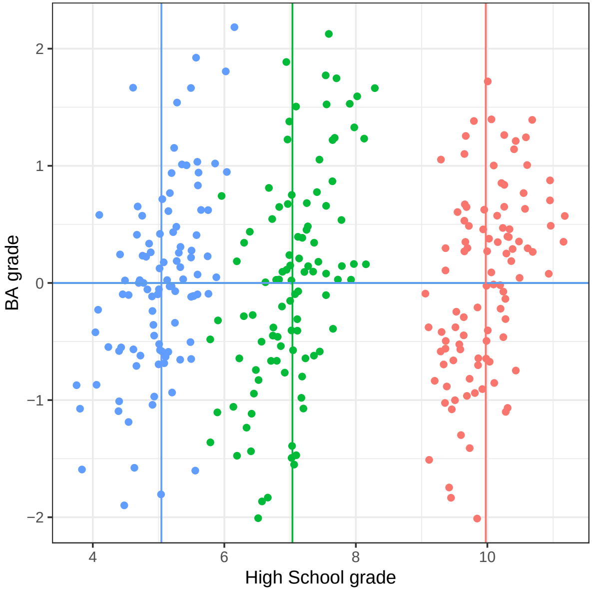
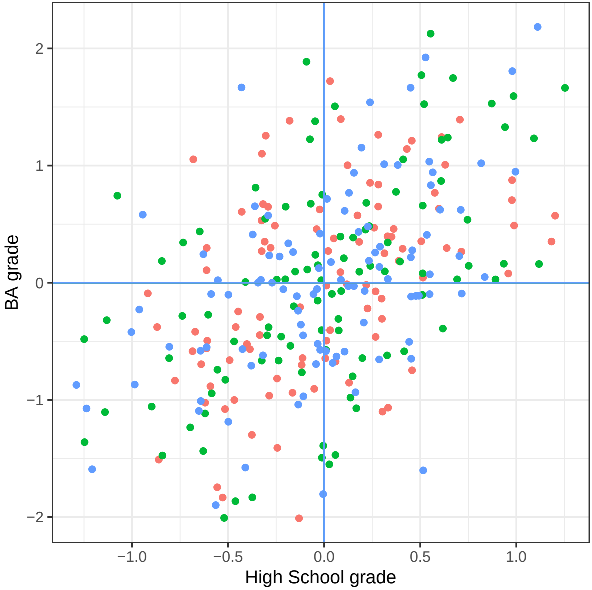


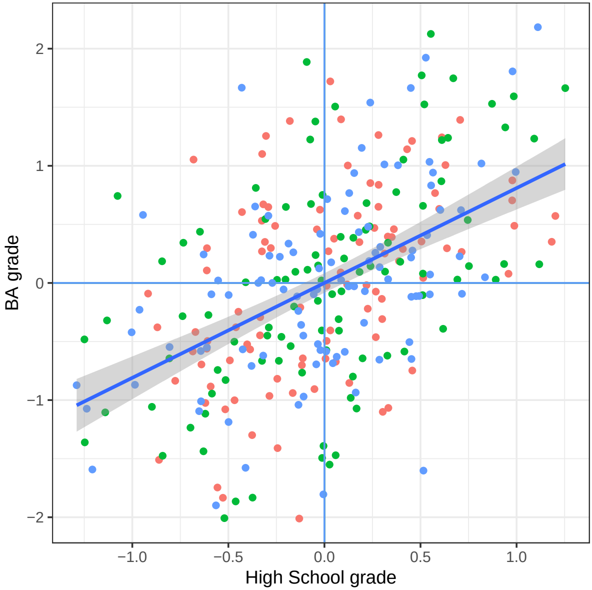
Fixed effects



Fixed effects



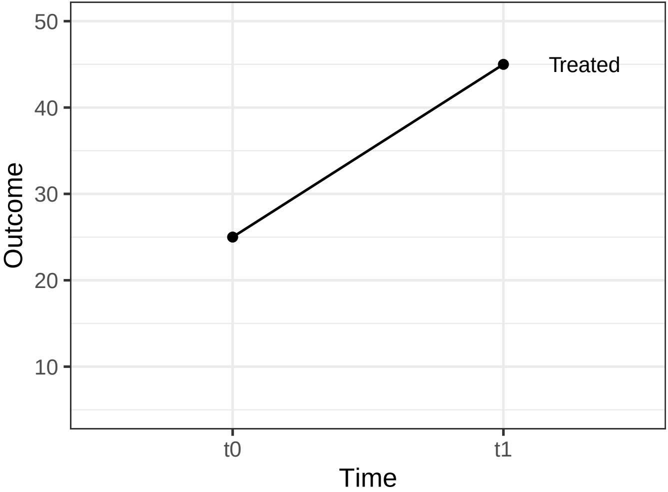
Fixed effects



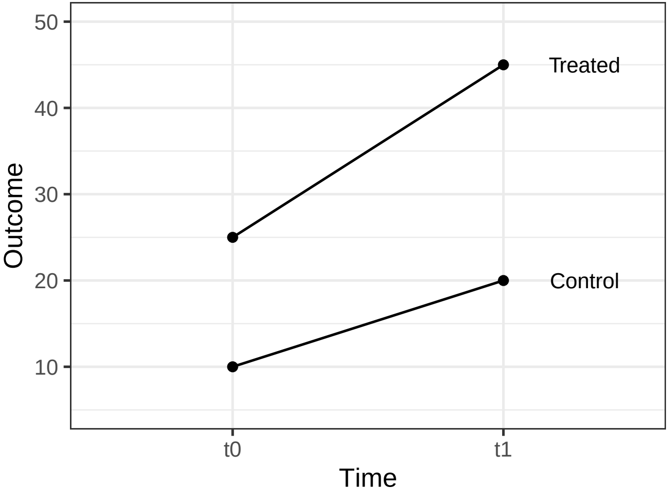
Difference-in-differences

- Treatments usually occur at a particular moment in time, e.g.:
  - → Minimum wage increase, terrorist attacks, influx of refugees, ...
- A naive idea would be to compare how it was *before* the treatment with how it is *after* the treatment, right?
  - → e.g. in municipalities that removed Francoist streets, how much did Vox grow between 2016 and 2019?
- or we could just compare treated vs control *after* treatment
  - → e.g. did Vox get more votes in 2019 in municipalities that removed F streets?
- DiD idea: use a control group that works as a counterfactual for how much would Y have changed without treatment

Difference-in-differences



Difference-in-differences



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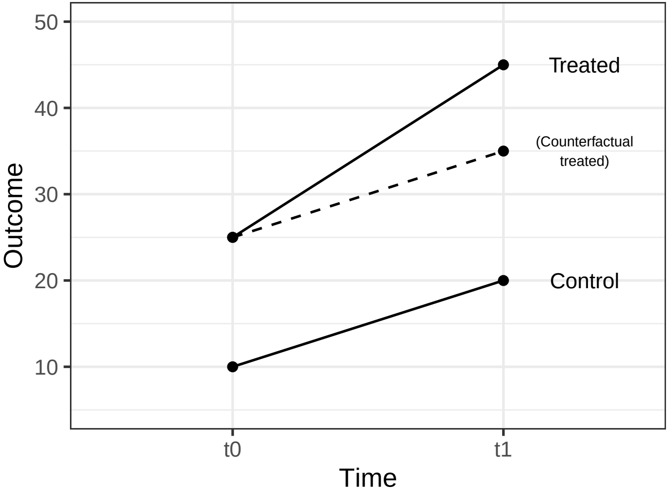
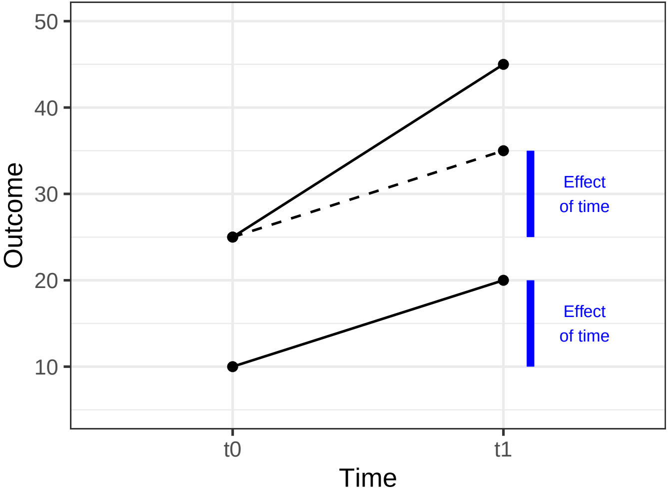
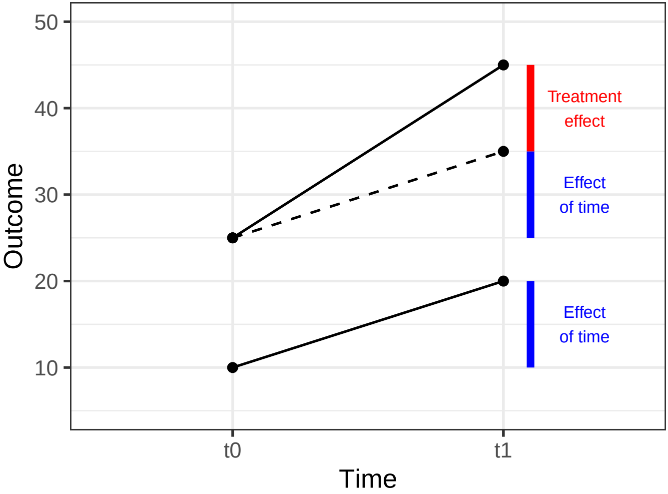


Difference-in-differences



Difference-in-differences



Difference-in-differences estimation

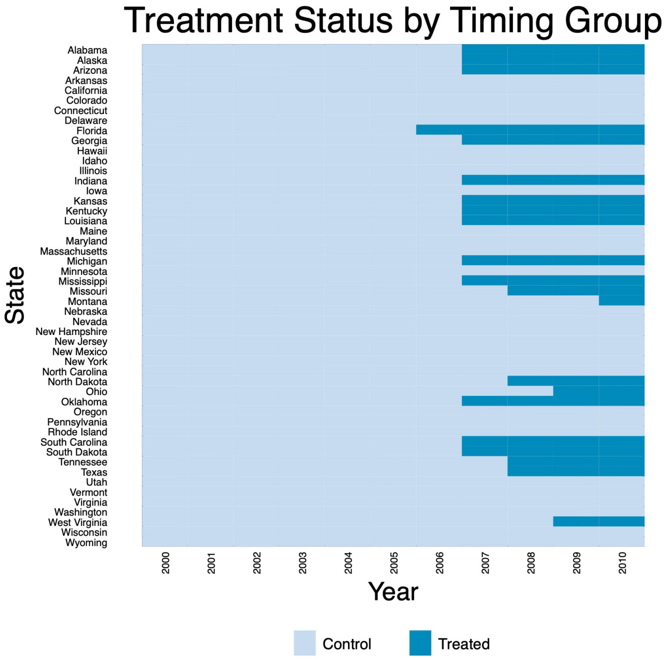
- You normally estimate it using a regression model (OLS or logit, depending on the outcome) on panel data
  - $\rightarrow$ each unit has one observation per period (at least, before/after treatment)
- Variables: time, group (treated or not), and their interaction

$$\begin{aligned} Y_{it} = & \beta_0 + \beta_1 \mathit{Treated}_i + \beta_2 \mathit{After}_t + \\ & \beta_3 (\mathit{Treated}_i \times \mathit{After}_t) + \beta^\top \mathbf{x}_i + \epsilon_{it} \end{aligned} \tag{1}$$

- Key assumption: control group is a good counterfactual
  - $\rightarrow$ checking parallel trends assumption

DiD and staggered treatments

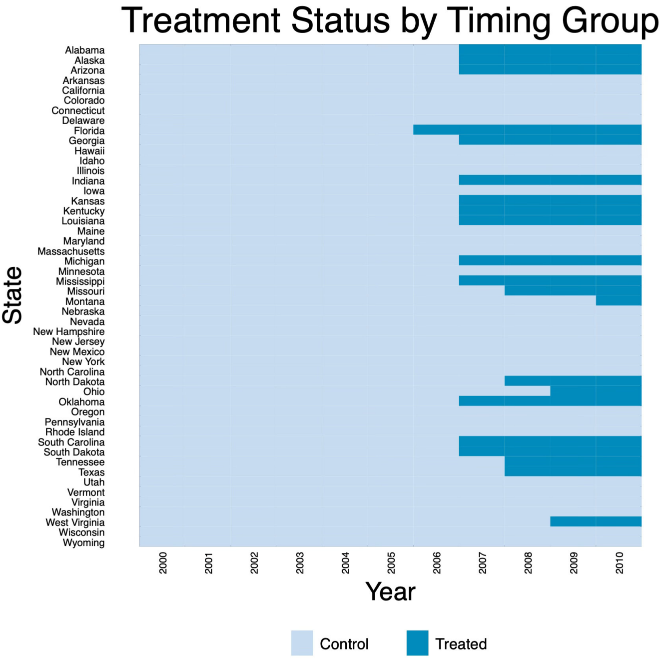
- Normal DiD works on a before/after setting, where treatment affects all units at the same time
- What if treatment is **staggered**?



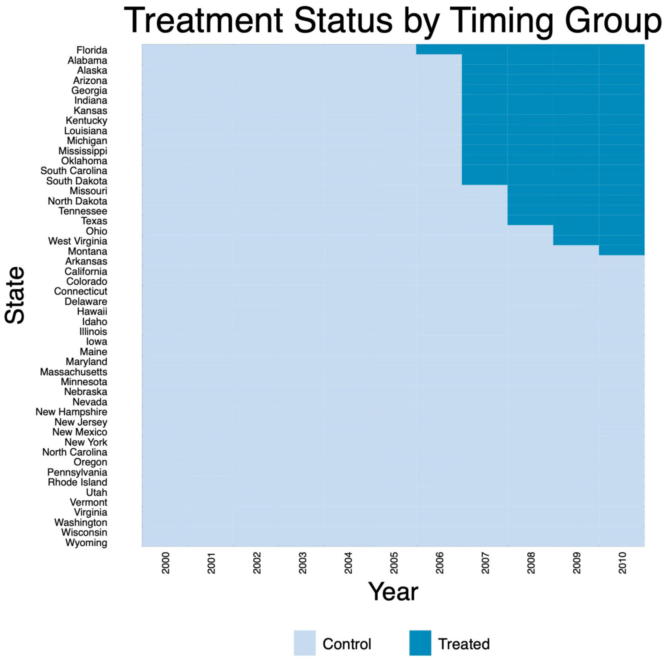
Treatment Status by Timing Group
State
Alabama
Alaska
Arizona
Arkansas
California
Colorado
Connecticut
Delaware
Florida
Georgia
Hawaii
Idaho
Illinois
Indiana
Iowa
Kansas
Kentucky
Louisiana
Maine
Maryland
Massachusetts
Michigan
Minnesota
Mississippi
Missouri
Montana
Nebraska
Nevada
New Hampshire
New Jersey
New Mexico
New York
North Carolina
North Dakota
Ohio
Oklahoma
Oregon
Pennsylvania
Rhode Island
South Carolina
South Dakota
Tennessee
Texas
Utah
Vermont
Virginia
Washington
West Virginia
Wisconsin
Wyoming
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year
Control
Treated

DiD and staggered treatments

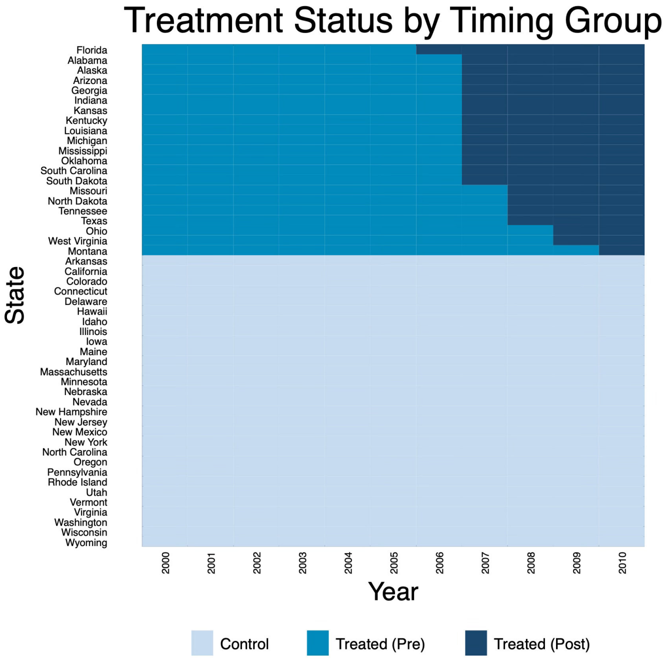
- Different strategies to deal with this
- Naive: Two-Way Fixed Effects (TWFE)
  - → Problem when having heterogenous effects over time
- More recently: DiD estimates under treatment heterogeneity
  - → Goodman-Bacon 2021 ("Difference-in-Differences with Variation in Treatment Timing")
  - → Callaway and Sant'Anna 2021 ("Difference-in-Differences with Multiple Time Periods")
  - → Roth et al. 2023 ("What's Trending in Difference-in-Differences?")



Treatment Status by Timing Group
State
Alabama
Alaska
Arizona
Arkansas
California
Colorado
Connecticut
Delaware
Florida
Georgia
Hawaii
Idaho
Illinois
Indiana
Iowa
Kansas
Kentucky
Louisiana
Maine
Maryland
Massachusetts
Michigan
Minnesota
Mississippi
Missouri
Montana
Nebraska
Nevada
New Hampshire
New Jersey
New Mexico
New York
North Carolina
North Dakota
Ohio
Oklahoma
Oregon
Pennsylvania
Rhode Island
South Carolina
South Dakota
Tennessee
Texas
Utah
Vermont
Virginia
Washington
West Virginia
Wisconsin
Wyoming
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year
Control
Treated



Treatment Status by Timing Group
State
Florida
Alabama
Alaska
Arizona
Georgia
Indiana
Kansas
Kentucky
Louisiana
Michigan
Mississippi
Oklahoma
South Carolina
South Dakota
Missouri
North Dakota
Tennessee
Texas
Ohio
West Virginia
Montana
Arkansas
California
Colorado
Connecticut
Delaware
Hawaii
Idaho
Illinois
Iowa
Maine
Maryland
Massachusetts
Minnesota
Nebraska
Nevada
New Hampshire
New Jersey
New Mexico
New York
North Carolina
Oregon
Pennsylvania
Rhode Island
Utah
Vermont
Virginia
Washington
Wisconsin
Wyoming
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year
Control
Treated


Treatment Status by Timing Group
State
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Mississippi
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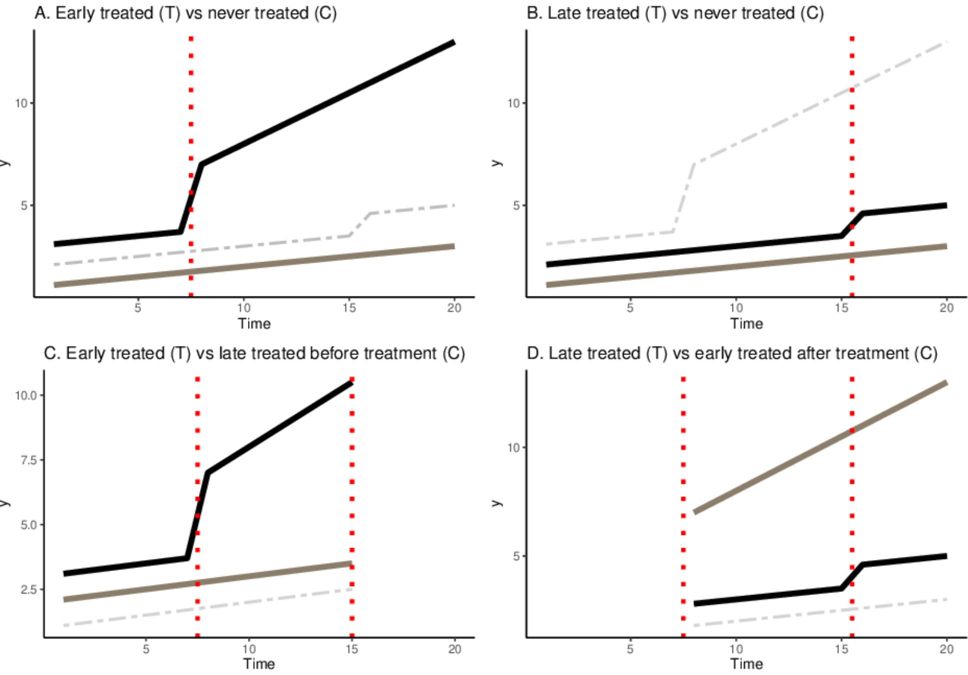
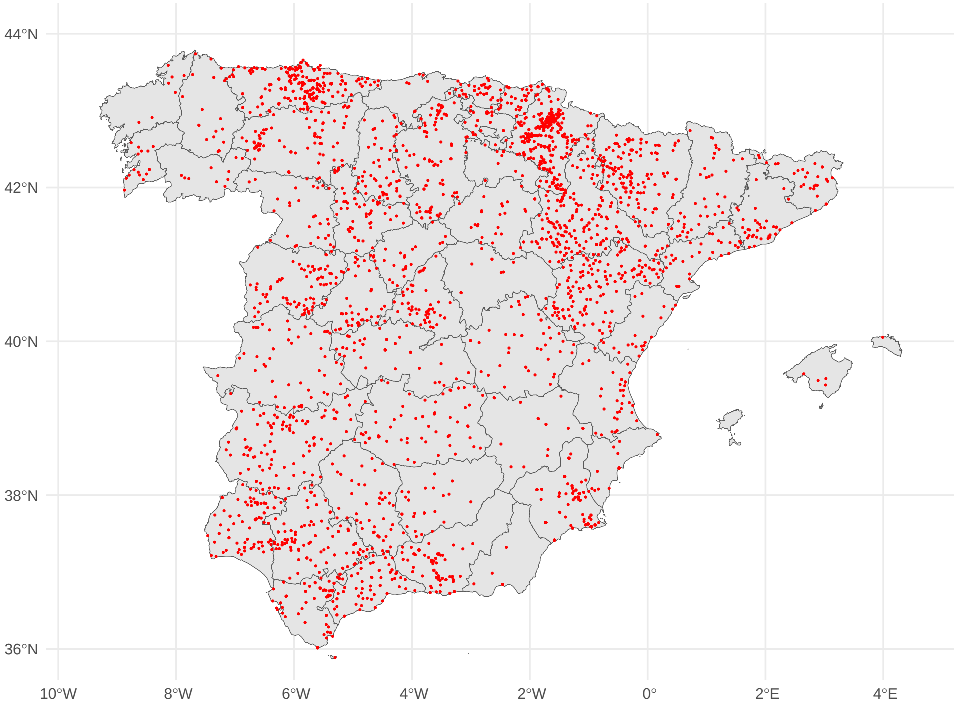
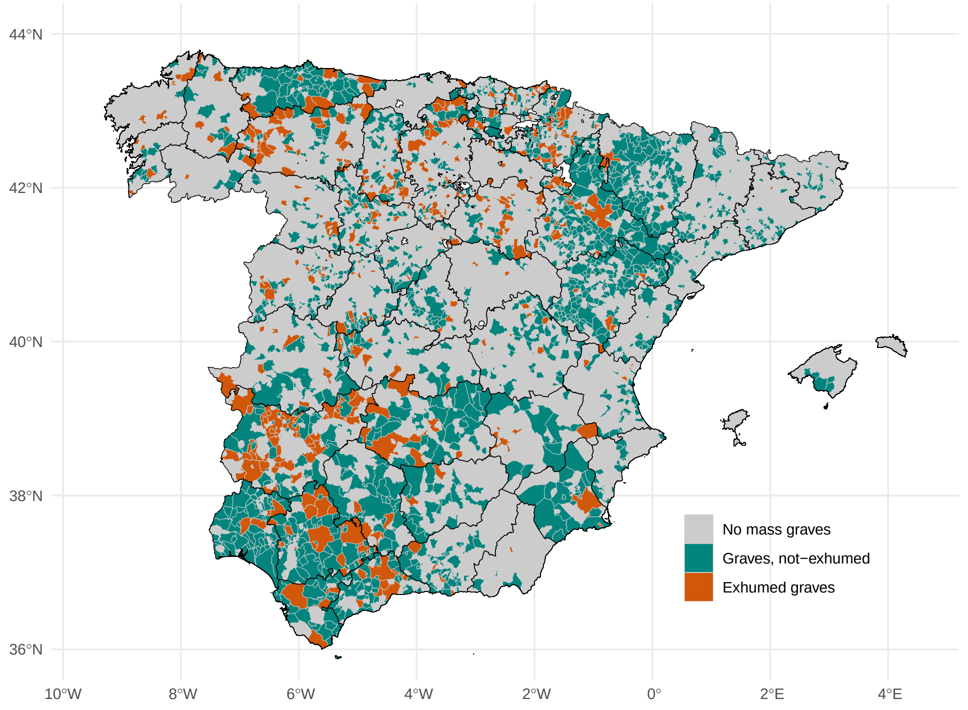
South Carolina
South Dakota
Missouri
North Dakota
Tennessee
Texas
Ohio
West Virginia
Montana
Arkansas
California
Colorado
Connecticut
Delaware
Hawaii
Idaho
Illinois
Iowa
Maine
Maryland
Massachusetts
Minnesota
Nebraska
Nevada
New Hampshire
New Jersey
New Mexico
New York
North Carolina
Oregon
Pennsylvania
Rhode Island
Utah
Vermont
Virginia
Washington
Wisconsin
Wyoming
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Year
Control
Treated (Pre)
Treated (Post)


A. Early treated (T) vs never treated (C)
B. Late treated (T) vs never treated (C)
C. Early treated (T) vs late treated before treatment (C)
D. Late treated (T) vs early treated after treatment (C)
y
Time
5
10
15
20
10.0
7.5
5.0
2.5

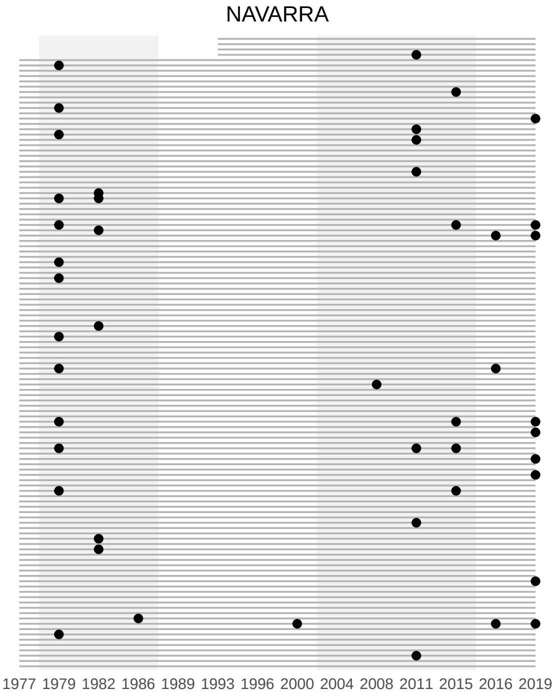
Bringing justice to victims worldwide, one bone fragment at a time

A documentary on the creation of the world-renowned Argentina Forensic Anthropology Team is now streaming on PBS


44°N
42°N
40°N
38°N
36°N
10°W
8°W
6°W
4°W
2°W
0°
2°E
4°E



44°N
42°N
40°N
38°N
36°N
10°W
8°W
6°W
4°W
2°W
0°
2°E
4°E
No mass graves
Graves, not−exhumed
Exhumed graves

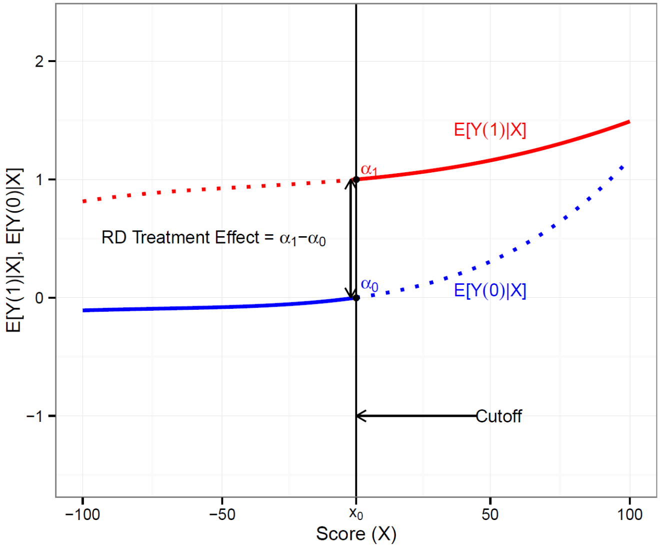

NAVARRA
1977
1979
1982
1986
1989
1993
1996
2000
2004
2008
2011
2015
2016
2019

Regression discontinuity design

- RDD works well when assignment into treatment depends on a cutoff along a running variable
  - → Do incumbent politicians have an electoral advantage? (vote share)
  - → What is the effect of being drafted into the military? (birth year)
  - → Effect of national policies in ethnic identification in Africa? (distance to colonial borders)
- The source of the exogenous variation:
  - → Although many variables confound the relationships between X and Y, nothing should be too different *around the cutoff* between treatment and control groups (local randomization assumption)

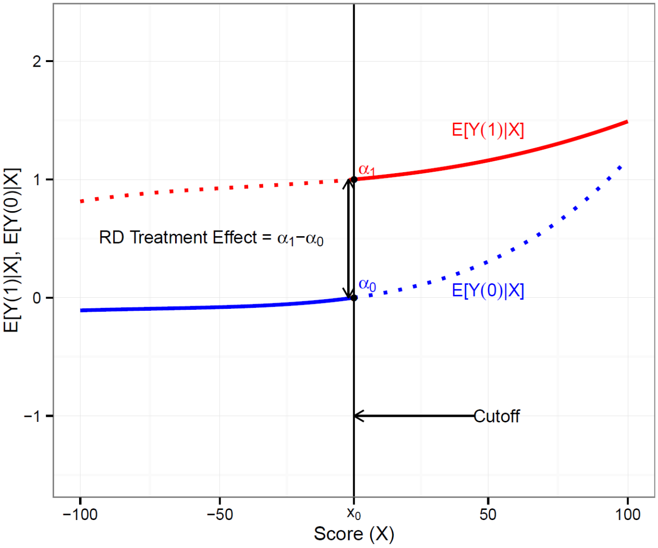
Regression discontinuity design

Figure 2: RD Treatment Effect in Sharp RD Design



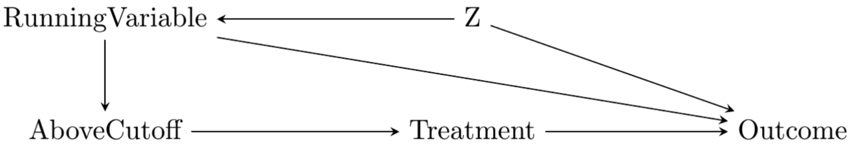
Regression discontinuity design

Figure 2: RD Treatment Effect in Sharp RD Design



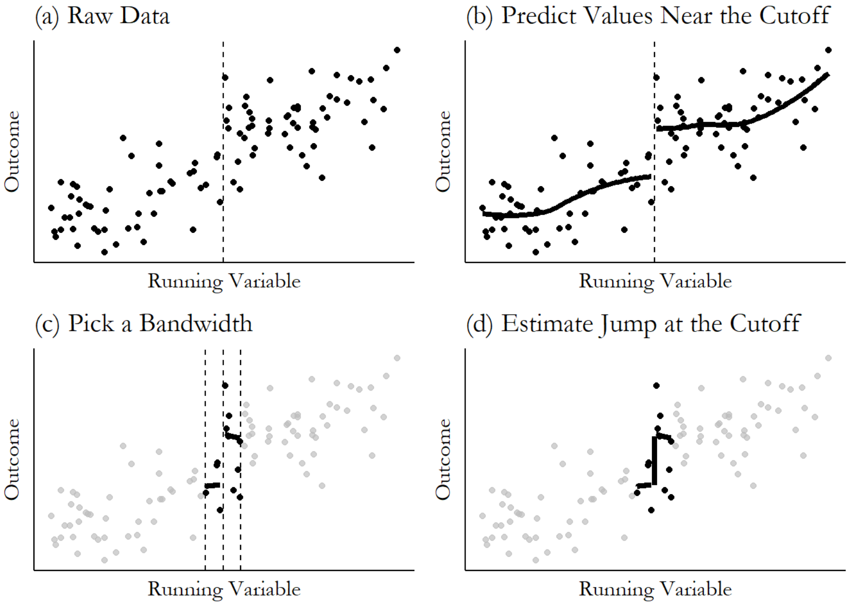
Regression discontinuity design

- Key assumption : other confounders also vary along the running variable, but are independent to the *jump over cutoff*



Huntington-Klein, *The Effect*, p.508

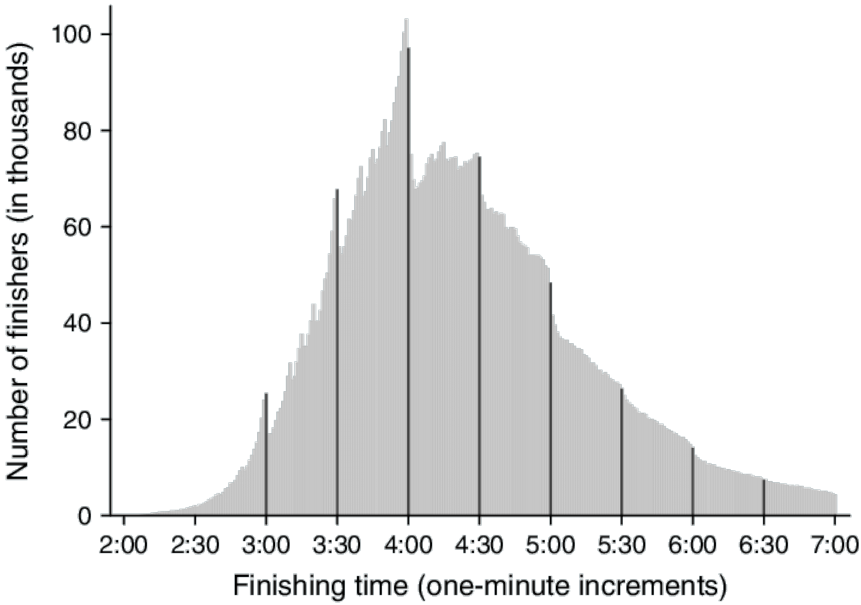
RDD estimates the LATE



Threats in RDD

- Outcome $\rightarrow$ cutoff: what we study might actually affect cutoff (e.g. colonial borders and ethnic ID)
  - $\rightarrow$ could also be a case of confounding, cutoff $\leftarrow Z \rightarrow$ outcome
- Precise sorting: maybe something is *actually* affecting sorting around the cutoff (next example)
- Causal effect independent of treatment: being above/below cutoff affects Y independent of the treatment we're interesting in

Threats in RDD: precise sorting



Instrumental variables

$$\begin{tikzcd} & & U \arrow[dl, dashed] \arrow[dr, dashed] & \\ \texttt{random} \arrow[r] & X \arrow[rr] & & Y \end{tikzcd}$$

- Probably closest idea to a 'natural experiment'

Instrumental variables

- Find an exogenous source of variation in the treatment variable
  → e.g., we want to know the effect of protests on government action, and we use *rainfall* as an instrument
- Isolate that variation and use it to identify the causal effect
  → two-stage least squares, or 2SLS

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Assumptions:

- Relevance: the instrument explains at least some part of the treatment variable
- Validity or exclusion restriction: no back door paths between the instrument and the outcome

Matching

- Main idea behind matching: adding control variables is *not the only way* to control / close back doors
- When you use a subset selecting on a variable, you are controlling for that variable
  - $\rightarrow$ e.g. if you analyze *income* $\leftarrow$ *education* and only use data from individuals who grew up in big cities, you are already controlling for urban/rural
- Matching builds on this, and it is essentially about keeping in the same only comparable observations, or *matched pairs*
- Example of $Y \leftarrow$ *Treatment* matching on X

Matching

- Normally you match treated units on a few control units (so you would get the **ATT**)
- But in some cases you do the opposite, because you have more treated units that control (and you get the **ATC**)
  - → Example from Ukraine survey paper
- Two basic approaches:
  - → Distance matching: you select observations that have similar values on the matching variables
  - → Propensity score matching: we estimate the probability of being treated based on a set of variables, and then control/subset for that propensity score

Overall inference strategy

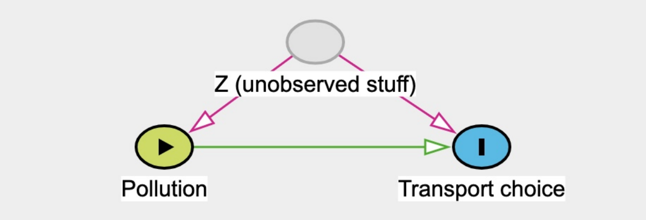
- In ideal experimental setting, we actually don't need any sophisticated statistics
  - $\rightarrow$ We can just compare the *mean* of the treatment group and the *mean* of the control group, that's it

Overall inference strategy

- In ideal experimental setting, we actually don't need any sophisticated statistics
  - $\rightarrow$ We can just compare the *mean* of the treatment group and the *mean* of the control group, that's it
- In observational data, these two types of tools (controlling + exploiting exogenous variation) are often used in combination
- But **always remember** these methods **depend** on a **causal model**
- Let's look at an example (from Huntington-Klein *The Effect*, kind of)

Controlling **and** exploting exogeneity

- Q: **Does pollution determine transport choice?**
- And say we are going to observe variation across *days*
  - → Outcome: car driving
  - → Treatment: pollution levels
- Clear problem of endogeneity, no? (driving in t_{-1}, economic activity, etc)



Controlling **and** exploting exogeneity

Chasing Clean Air: Pollution-Induced Travels in China

Shuai Chen, Yuyu Chen, Ziteng Lei, Jie-Sheng Tan-Soo

Abstract: This study uses "big" data to empirically investigate a highly effective, but underexamined way of reducing one's exposure toward air pollution—short-term travel. We determine subscribers' locations using mobile phones' signals and thereby establish linkages between air pollution and short-term population movements between cities in China. Using an instrumental variable based on daily variation in wind directions and pollution levels from distant upwind locations, we find that a one-unit increase in the origin city's air quality index (AQI) over the destination city's AQI increases short-term population flow from origin to destination by 0.15%. Further analyses provide richer characterizations of the decision-making process behind travel movements. Our findings add to the evidence base by examining in detail an understudied behavioral response toward air pollution.

JEL Codes: O15, Q53, Q56

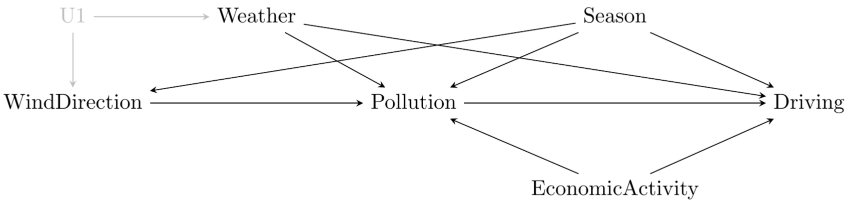
Keywords: air pollution, short-term population movement, avoidance cost, averting

Controlling **and** exploting exogeneity

- But it's not enough with wind direction, no?

Controlling **and** exploting exogeneity

- But it's not enough with wind direction, no?



(And city, in this case)

Controlling **and** exploting exogeneity

- So in this case, we need to do two things:

1. Exploit exogenous variation
   - $\rightarrow$ In this case, using an instrumental variables approach, which isolates the variation in pollution which is explained by variation in wind direction

Controlling **and** exploting exogeneity

- So in this case, we need to do two things:

1. Exploit exogenous variation
   → In this case, using an instrumental variables approach, which isolates the variation in pollution which is explained by variation in wind direction
2. *And* control for weather, season, and city
   → Using a regression with control variables, or matching, or fixed effects

Roadmap

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Paper discussion

Re-cap and essay guidelines

Paper discussion

Research Article

Research and Politics
October-December 2021: 1–7

DOI: 10.1177/20531680211058550
journals.sagepub.com/home/rap
SAGE

Do TJ policies cause backlash? Evidence from street name changes in Spain

Francisco Villamil[1] and Laia Balcells[2]

Abstract
Memories of old conflicts often shape domestic politics long after these conflicts end. Contemporary debates about past civil wars and/or repressive regimes in different parts of the world suggest that these are sensitive topics that might increase political polarization, particularly when transitional justice policies are implemented and political parties mobilize discontentment with such policies. One such policy recently debated in Spain is removing public symbols linked to a past civil war and subsequent authoritarian regime (i.e., Francoism). However, the empirical evidence on its impact is still limited. This article attempts to fill this gap by examining the political consequences of street renaming. Using a difference-in-differences approach, we show that the removal of Francoist street names has contributed to an increase of electoral support for a new far-right party, Vox, mainly at the expense of a traditional right-wing conservative party, PP. Our results suggest that revisiting the past can cause a backlash among those ideologically aligned with the perpetrator, and that some political parties can capitalize on this.

Keywords
Transitional justice, voting, conflict memories, Spain

Roadmap

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Methods in brief

Paper discussion

Re-cap and essay guidelines

Re-cap and final essay

- **Groups?** Send me an email **before Oct 15th**

Re-cap and final essay

- Logistics:
  - → 10-15 slots
  - → 10min presentation + 10min feedback
  - → If less groups, we can increase time
- **Questions?**