

# Advanced topics & overview

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Research Design for Social Sciences  
MA Computational Social Science, UC3M

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# Roadmap

Review

Generalizing results

Mechanism test and additional implications

Robustness and inference tests

Extra: Synthetic control methods

Questions

Causal methods again, in detail

## Questions & review

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- Take away from workshop:
  - Need to think in a **broader way**
  - Do not stay fixated on a method
  - There's never a single solution/strategy

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- Take away from workshop:
  - Need to think in a **broader way**
  - Do not stay fixated on a method
  - There's never a single solution/strategy
- Key thing I'd like you to take from this course:  
learn how to **plan**, execution will come later

# Unit of analysis

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- **Level at which we have our observations**

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  - The UA is the level at which you are measuring your variables
  - Not all variables can be measured at the same level
  - Example: **cancer mortality**. At which levels can we measure this?
- We can change the UA of a dataset (usually from bottom to top)
  - Example: let's say we have an individual-level dataset of diseases and outcome (death)

# Unit of analysis

Most important things:

- Be able to identify it
- **Choose the right unit of analysis** depending on the theory and mechanisms we are testing
  - Examples: democratic peace, trade flows

# Unit of analysis

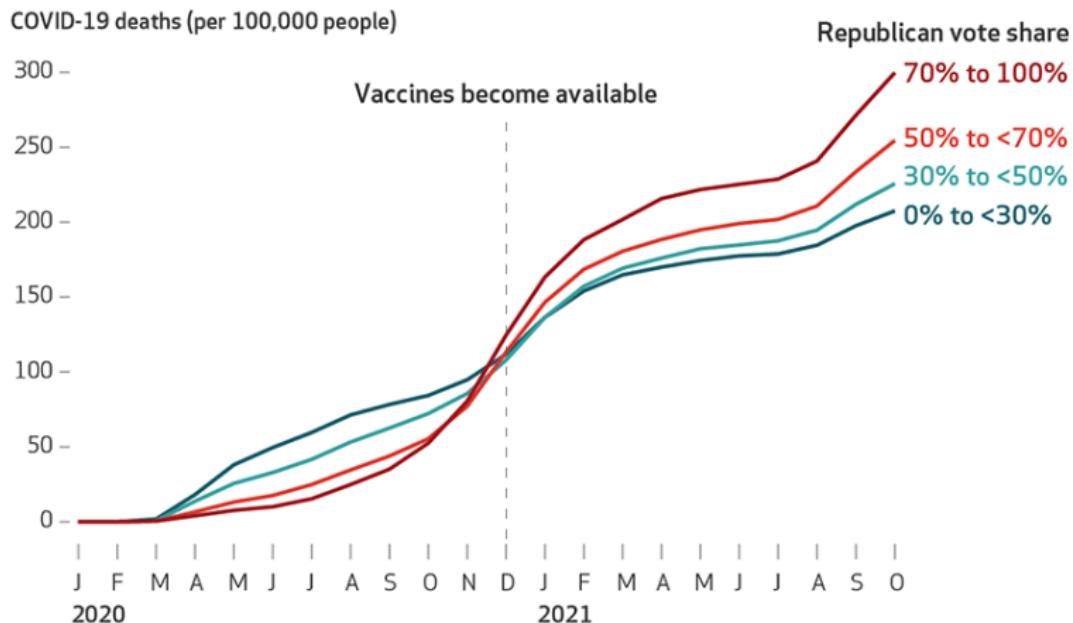
Most important things:

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- How many variables and what's the unit of analyses in the next dataset?

muni_code	municipio	PP2016_06	PSOE2016_06	part2016_06	PP2019_11	PSOE2019_11	part2019_11
40099	Honrubia de la Cuesta	0,595	0,262	0,846	0,476	0,31	0,815
50252	Tauste	0,461	0,26	0,709	0,316	0,313	0,695
25087	Esterri de Cardos	0	0,116	0,717	0	0,133	0,833
40033	Brieva	0,373	0,176	0,879	0,212	0,192	0,786
26098	Munilla	0,438	0,137	0,83	0,349	0,27	0,685
28021	Berrueco, El	0,371	0,121	0,668	0,264	0,168	0,639
37059	Buenamadre	0,516	0,29	0,758	0,305	0,4	0,812
50203	Pastriz	0,292	0,241	0,719	0,173	0,284	0,767
06901	Valdelacalzada	0,389	0,375	0,736	0,224	0,405	0,729
04031	Cantoria	0,334	0,499	0,758	0,213	0,537	0,737
17199	Torroella de Montgrí	0,089	0,103	0,605	0,041	0,116	0,704
07027	Inca	0,362	0,23	0,631	0,203	0,263	0,595
08240	Sant Sadurní d'Anoia	0,075	0,158	0,697	0,04	0,18	0,776
19256	Sienes	0,639	0,167	0,837	0,556	0,278	0,783
40157	Pelayos del Arroyo	0,477	0,227	0,936	0,333	0,3	0,769
28104	Paracuellos de Jarama	0,381	0,136	0,794	0,236	0,207	0,795
31195	Orbaizeta	0,211	0,062	0,714		0,065	0,639
06135	Trujillanos	0,508	0,313	0,775	0,265	0,343	0,781
08259	Santa María de Palautordera	0,09	0,122	0,695	0,041	0,145	0,768
12095	Ribesalbes	0,349	0,296	0,781	0,275	0,361	0,761

# How many variables? Unit of analysis?

Cumulative COVID-19 deaths per 100,000 people, by county proportion of Republican presidential popular vote in the 2020 election, January 1, 2020–October 31, 2021

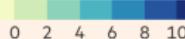
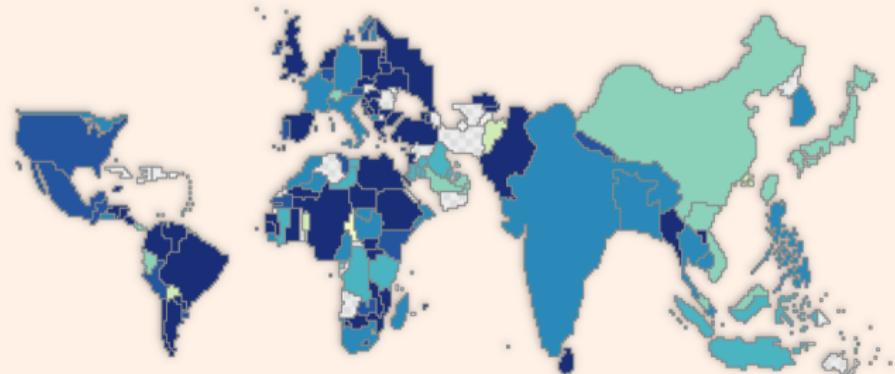


# How many variables? Unit of analysis?

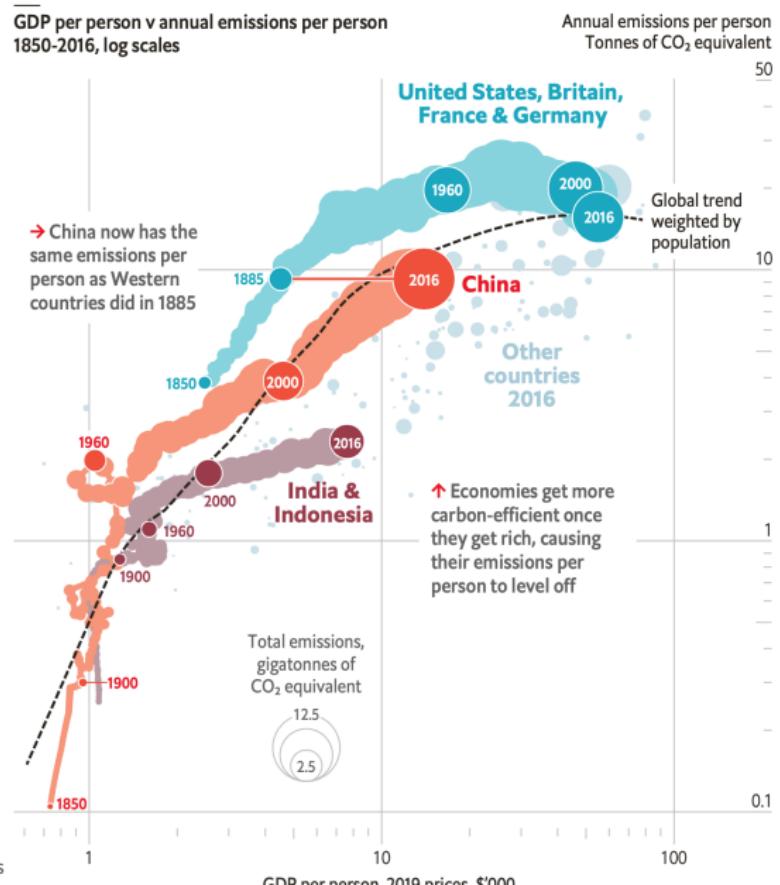
A population-adjusted snapshot of global inflation

Annual % change in consumer price indices, latest figures available (select a country for details).

Each country on the map is sized according to total population in 2018

 Search..

## How many variables? Unit of analysis?



*Carte Figurative des pertes successives en hommes de l'Armée Française dans la Campagne de Russie 1812-1813.*  
 Dessiné par M. Minard, Inspecteur Général des Ponts et Chaussées en retraite.

Les nombres d'hommes perdus sont représentés par les longueurs des zones colorées à raison d'une millimètre pour six mille hommes; ils sont de plus écrits en lettres des zones. Le rouge désigne les hommes qui entrent en Russie; le noir ceux qui sortent. — Les renseignements qui ont servi à dresser la carte ont été pris dans les ouvrages de M. M. Chiers, de Léger, de Fessardat, de Chambray et le journal intime de Jacob, pharmacien de l'Armée depuis le 28 Octobre. Pour mieux faire juger à l'œil la diminution de l'armée, j'ai supposé que les corps de Prince Napoléon et du Maréchal Davout qui avaient été détachés aux Malibors et aux régions vers Orelcha et Wilejek, avaient toujours marché avec l'armée.

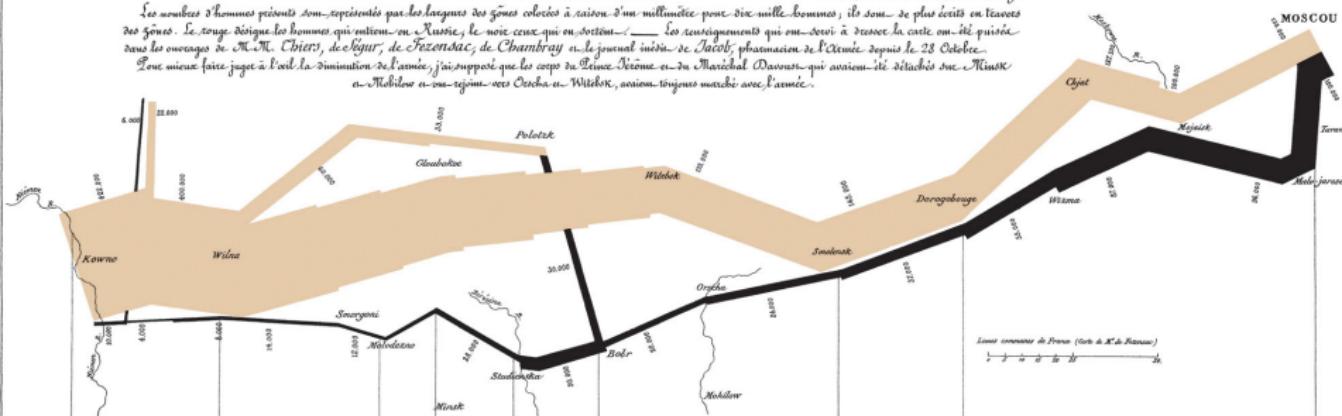


TABLEAU GRAPHIQUE de la température en degrés du thermomètre de Réaumur au dessous de zéro.

Les Campagnes passent au gelé  
le Nilovaï gelé.



Avec les Registres, t. 1er, 1<sup>re</sup> partie, 3<sup>e</sup> édition à Paris.

Imp. Léb. Regnier et Desordet.

Ligne moyenne de France (Carte de M. de Blanque)

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26

Pluie 24 h.<sup>ans</sup>

Zéro 18 h.<sup>ans</sup>

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Degrés

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# Generalizing results

- The Credibility Revolution in social sciences
  - Internally valid studies, context-specific
  - Accumulating generalizable knowledge by repetition
  - 'Valid' studies and synthesis studies
  - Practice-oriented, Esther Duflo: 'The Economist as Plumber'
- What are we really learning?

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- What are we really learning? How much *knowledge* we gain?
- Importance of meta-scientific knowledge
  1. Robustness of results and incremental science (→ meta-analyses)
  2. Generalization of results (→ construct and external validity)

# An example of a meta-analysis

- Effect of commodity prices on conflict
- What's the causal inference problem here?

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- Effect of commodity prices on conflict
- What's the causal inference problem here?
- But there are options to solve this, right?

# An example of a meta-analysis

## **Do Commodity Price Shocks Cause Armed Conflict? A Meta-Analysis of Natural Experiments**

GRAEME BLAIR *University of California, Los Angeles*

DARIN CHRISTENSEN *University of California, Los Angeles*

AARON RUDKIN *University of California, Los Angeles*

**S**cholars of the resource curse argue that reliance on primary commodities destabilizes governments: price fluctuations generate windfalls or periods of austerity that provoke or intensify civil conflict. Over 350 quantitative studies test this claim, but prominent results point in different directions, making it difficult to discern which results reliably hold across contexts. We conduct a meta-analysis of 46 natural experiments that use difference-in-difference designs to estimate the causal effect of commodity price changes on armed civil conflict. We show that commodity price changes, on average, do not change the likelihood of conflict. However, there are cross-cutting effects by commodity type. In line with theory, we find price increases for labor-intensive agricultural commodities reduce conflict, while increases in the price of oil, a capital-intensive commodity, provoke conflict. We also find that price increases for lootable artisanal minerals provoke conflict. Our meta-analysis consolidates existing evidence, but also highlights opportunities for future research.

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3. Make studies comparable → standardize coefficients
4. Perform meta-analysis method

fixed-effects meta-analysis model & random effects m-a model

# Theoretical background on natural resources

H1: NR are the 'prize' to be won by fighting (+)

H2: Higher prices change opportunity cost (- for labor-intensive, + for capital-intensive)

H3: Lootable NR can be captured by fighting (+)

**TABLE 1. Commodity Classifications and Predicted Effect Direction from Each Hypothesis**

Commodity type	Characteristics		Predicted direction		
	Labor-intensive	Lootable	(H1)	(H2)	(H3)
Pooled (average of commodities)	Mix	Mix	+	+/-	+/0
Agriculture	✓		+	-	0
Artisanal minerals	✓	✓	+	-	+
Commercial minerals			+	+	0
Oil & gas		✓	+	+	+/0
Bundle of multiple types	Mix	Mix	+	+/-	+/0

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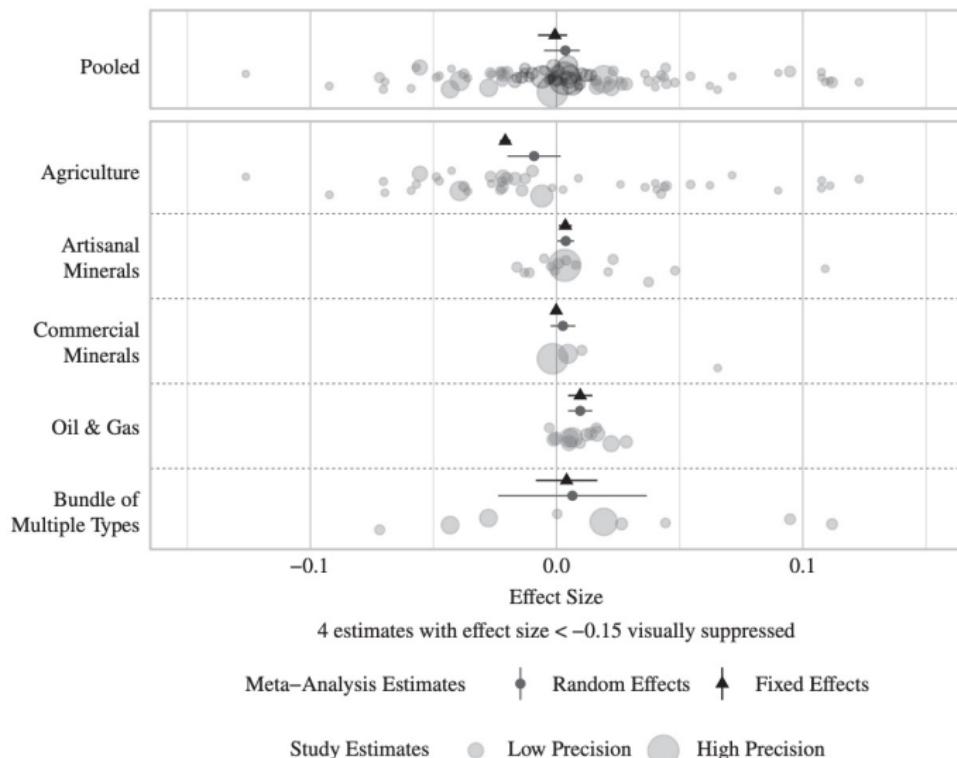
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# Meta-analysis findings

- **Pooling studies** across commodity types: no effect
- But when **disaggregating by commodity type**: effects coherent with theoretical predictions
  - price increases for **agricultural commodities** **reduce** the likelihood of armed conflict (labor-intensive commodities and opportunity cost)
  - price increases for **oil and gas** **increase** likelihood of conflict (capital-intensive commodity and returns to fighting)

# Meta-analysis findings

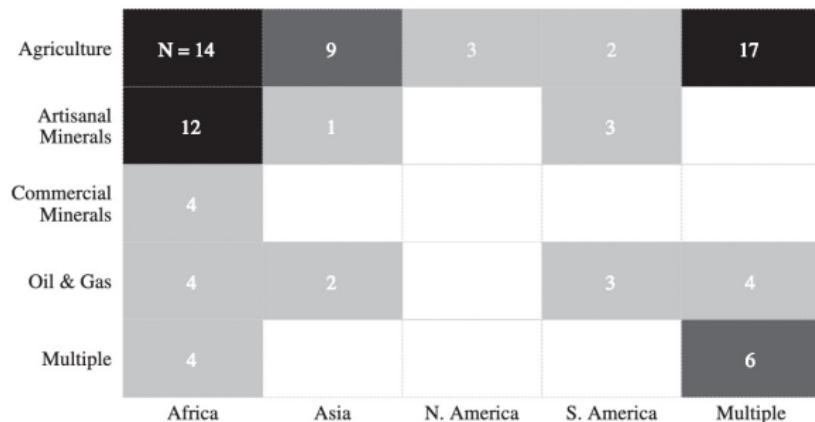
**FIGURE 1. Effects of Commodity Prices on Conflict by Commodity Type**



# Learning about gaps

- A meta-analysis can be used to identify **gaps**, e.g.
  1. **Spatial or temporal validity:** where and when do findings come from?
  2. **Publication bias:** are only positive findings being published?

**FIGURE 2. Evidence Gap Map (Number of Estimates) by Commodity Type and Continent**



# Funnel plots

- Looking for **publication bias**
  - are people only submitting/publishing certain results? (so we'd not have the full picture)

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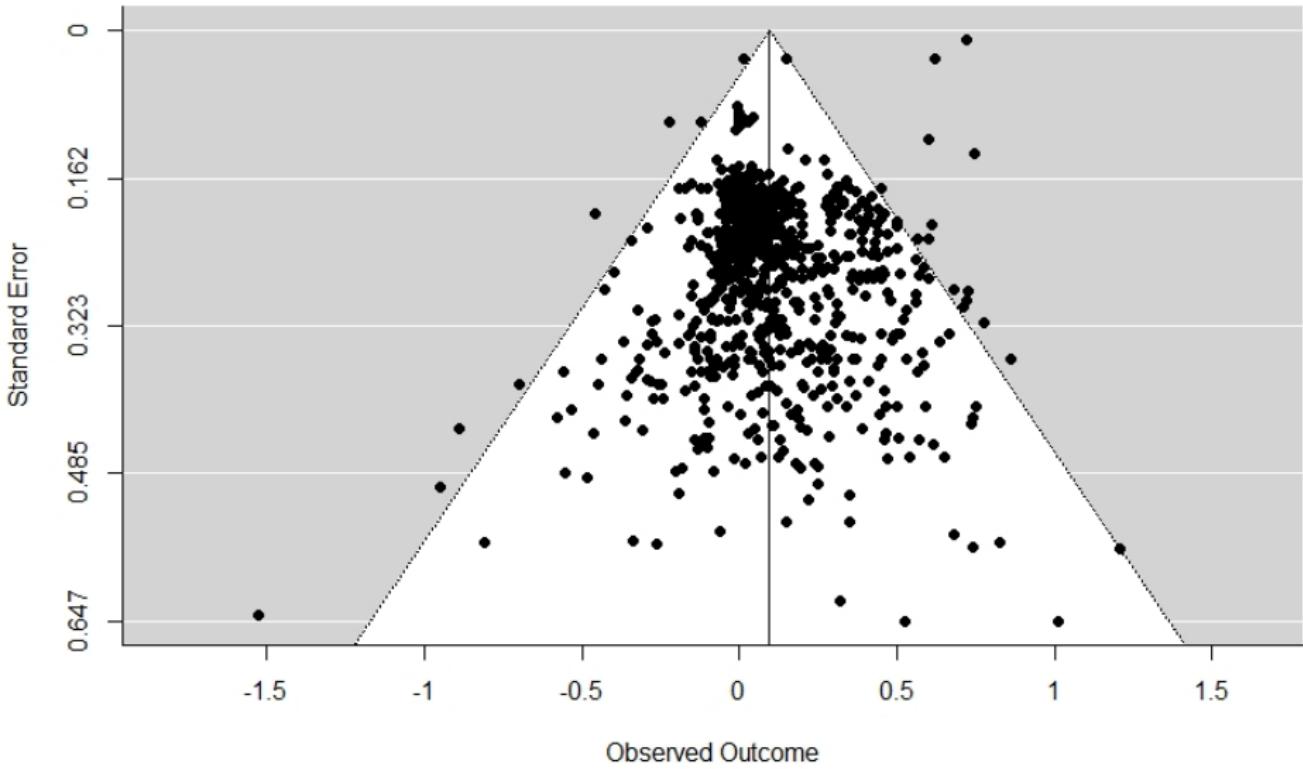
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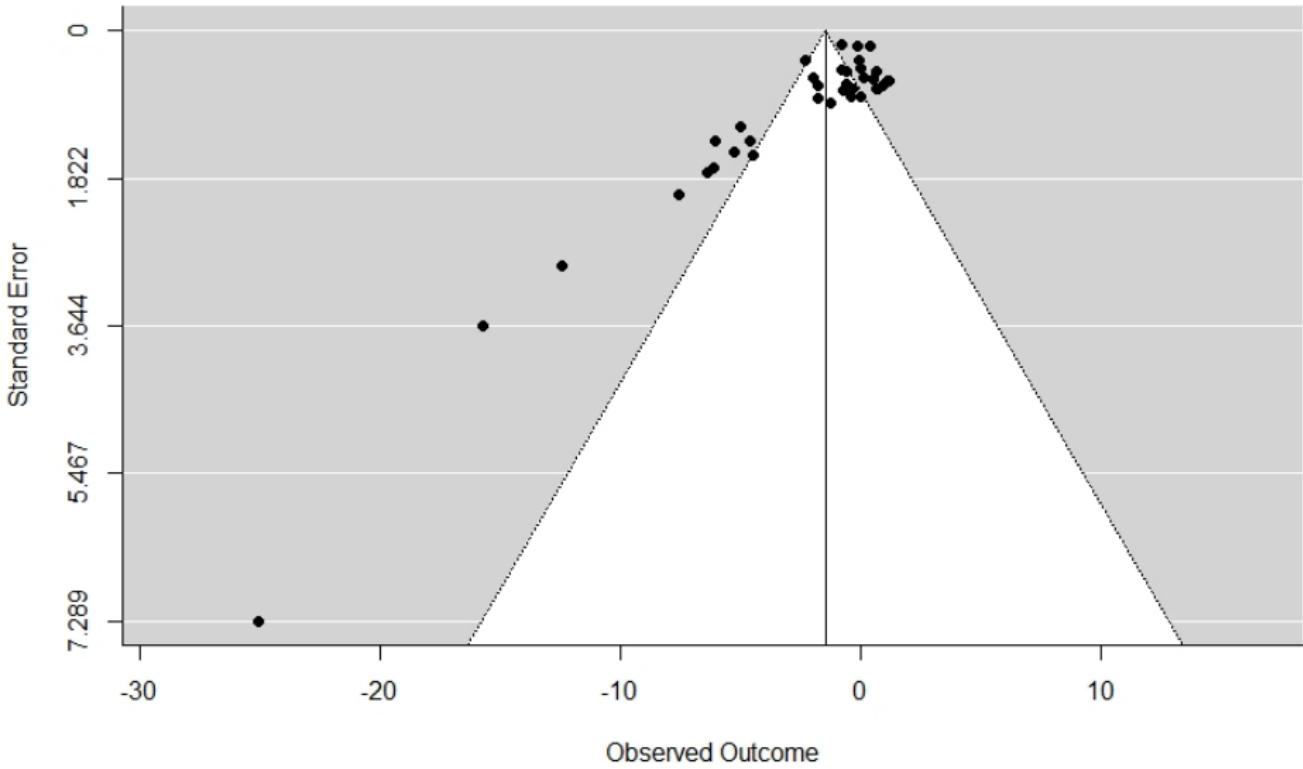
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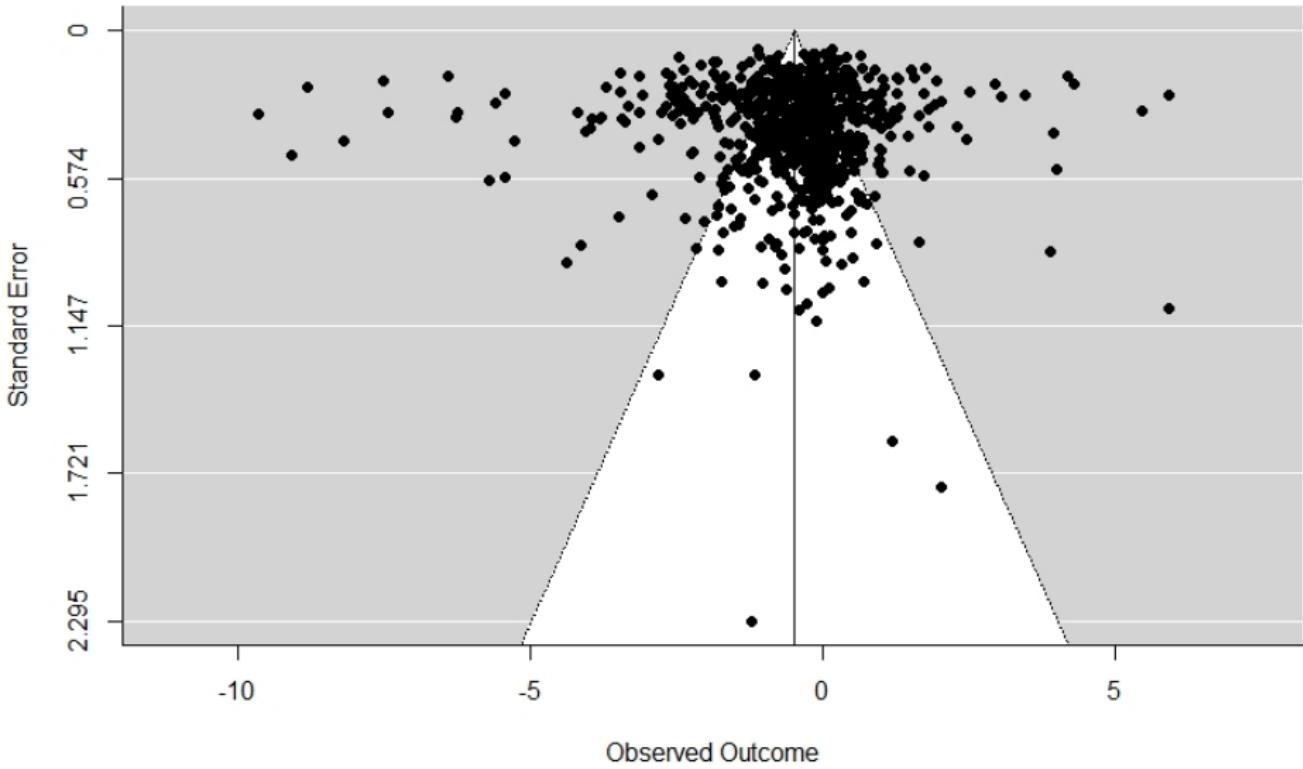
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- If there's publication bias, e.g. we won't see points in the bottom right/left (only positive or negative effects are submitted/published)





# Inverted funnel plots?

- We could observe an inverted funnel plot
- That could be the case e.g. if people don't submit 'imprecise' estimates but still can't find the true effect (unaccounted for heterogeneity, as in the natural resources example?)
-



## Funnel plots in meta-analyses

<https://www.bmjjournals.org/content/343/bmj.d4002>

<https://journals.sagepub.com/doi/pdf/10.1177/1536867X0400400204>

## Generalizing results

- Meta-analyses are **not** enough
- We need some kind of formal approach to understand the validity of studies

# Generalizing results

- **Construct validity**: are we really measuring what we intent to?
  - is the treatment doing what we theoretically expect it to? and are we measuring the outcome correctly?
- **External validity**: would we get the same results if we replicate this in another context?
  - especially: temporal and spatial validity

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1. Construct validity of the **cause**: bundled treatments, exclusion restriction (assignment only affects through treatment), SUTVA, etc
  2. Construct validity of the **outcome**: similar, making sure the empirical measure mirrors the theoretical construct

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(it is very unlikely that the effect takes place under universal conditions)
- ‘Effect-generalization’ and the softer ‘sign-generalization’
- Different types of external validity:
  - Populations ( $X$ -validity)
  - Treatments ( $T$ -validity)
  - Outcomes ( $Y$ -validity)
  - Contexts and settings ( $C$ -validity)

**TABLE 1. Summary of Typology**

	Practical concerns (examples)	Causal assumptions (formalization)
X-validity	Convenience samples, survey non-response, attrition	Ignorability of sampling and treatment effect heterogeneity (Assumption 1)
T-validity	Realistic treatments, bundled treatments, difference in implementations	Ignorable treatment variations (Assumption 2)
Y-validity	Behavioral or hypothetical survey outcomes, short- or long-term outcomes	Ignorable outcome variations (Assumption 3)
C-validity	Mechanisms differ across time, geography, political institutions, and so on	Contextual exclusion restriction (Assumption 4)

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  - Example of studies on dissent and the Arab Spring & importance of smartphones

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  - Example of studies on dissent and the Arab Spring & importance of smartphones
- How do we solve this? Importance of qualitative studies and theory

## References on external validity

- Egami and Hartman (2023) **Elements of External Validity: Framework, Design, and Analysis.** *APSR* 117(3): 1070–1088.
- Munger (2023) **Temporal validity as meta-science.** *Res&Pol* 10(3).  
→ See also this post: Generalizing Knowledge of Twitter to “X” .
- Esterling, Brady, & Schwitzgebel (2023) **The Necessity of Construct and External Validity for Generalized Causal Claims: A Critical Review of the Literature on Quantitative Causal Inference.** Preprint.

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# Testing the mechanism

## Additional implications of the theory

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# Basic guidelines on robustness tests

## Inference tests: placebo tests

- Reference:
  - AC Eggers, G Tuñón, A Dafoe (2023) **Placebo Tests for Causal Inference**. *American Journal of Political Science*, published online.

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# Synthetic control methods

- Imagine combining diff-in-diff with matching
  - treatments goes into effect at one time, and only affects one group
  - you can use pre-T data to make control and treated comparable
  - you estimate effect from post-T divergence

# Original study

## The Economic Costs of Conflict: A Case Study of the Basque Country

By ALBERTO ABADIE AND JAVIER GARDEAZABAL\*

*This article investigates the economic effects of conflict, using the terrorist conflict in the Basque Country as a case study. We find that, after the outbreak of terrorism in the late 1960's, per capita GDP in the Basque Country declined about 10 percentage points relative to a synthetic control region without terrorism. In addition, we use the 1998–1999 truce as a natural experiment. We find that stocks of firms with a significant part of their business in the Basque Country showed a positive relative performance when truce became credible, and a negative relative performance at the end of the cease-fire. (JEL D74, G14, P16)*

*American Economic Review, 2003*

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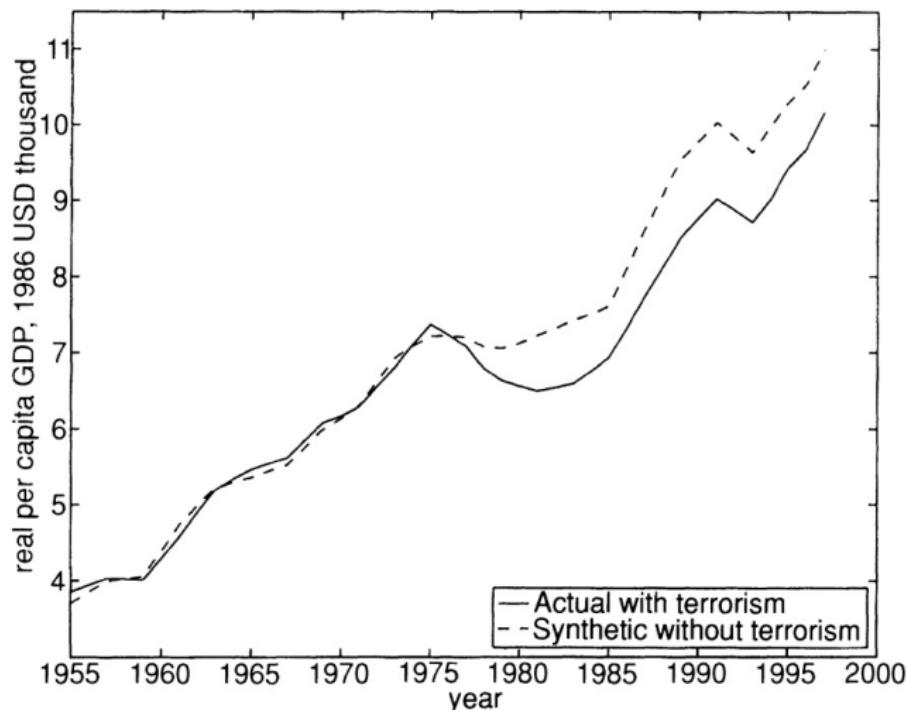


FIGURE 1. PER CAPITA GDP FOR THE BASQUE COUNTRY

## Reference example

- A Abadie, A Diamond & J Hainmueller (2010) Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association.*

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## Questions about final essay

- Review something?
- Format and guidelines
- Group vs individual essays
- Logistics: deadline, where to submit

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## Controlling in regression

- We probably already know this
- We've seen what it means controlling: just adjusting for the variation already predicted by the other variables
- One limitation though: we need to *observe* those variables

# Controlling

- We are going to see two further methods of controlling widely used:
  - Matching
  - Fixed effects

# Matching

- Adding control variables is not the only way to control / close back doors
- Imagine we have the following model:
  - $Z \rightarrow X \rightarrow Y \leftarrow Z$
  - Where  $Z$  is whether someone is retired or not
- If we select a sample of *only* retired people, we are closing that back door
  - $(X \leftarrow Z \rightarrow Y)$
- Matching is something like this, it's basically about creating groups of comparison where  $Z$  (which can be several variables) does not vary

## How matching works

- We have a treatment group and a control group (so: *binary* treatment variables)
- The main idea: give different *weights* to treated and control observations, so we eliminate
- We get these weights by using one or more *matching variables* (i.e. confounding variables)

# How matching works

- Imagine we have:
  - Treatment: get some specific skill training
  - Outcome: get a job afterwards
  - Confounding variable: gender
- Control group: 80 men and 20 women
  - 75% of men get a job, 60% of women do
- Treatment group: 500 men and 500 women
  - 70% of men get a job, 55% of women do
- Comparing within each group, we know the treatment effect is a 5% increase in the odds of getting a job ( $70 \rightarrow 75$ ,  $55 \rightarrow 60$ )
- But if we do the global comparison, it's almost 10%
  - $60 \text{ men} + 12 \text{ women out of } 100 = 72/100 = 72\%$ , vs  $(350 + 275)/1000 = 62.5\%$ , so a difference of 9.5 points

## How matching works

- The problem is we have 4 times more men in the treated group than women, whereas we have equal proportion on the control group
- So we'll weight the control observations by gender, giving *more* weight to the men observations, so it looks more similar to the treatment group
- $(4 * 350 + 1 * 275) / (4 * 500 + 1 * 500) = 67\%$
- Now, the unweighted difference in the treatment group was 72%, and now the weighted difference in the control group is 67%
- The difference is 5 points, the same as the within-group calculation we did before

# Two approaches to matching

## 1. Distance matching

- We want to create a dataset where treatment and control observations have similar values (distance) in the confounding variables
- If, say, our confounding variable is income, we'll pick control observations that have a similar value on income to each of the treatment observations
- <https://nickchk.com/causalgraphs.html>

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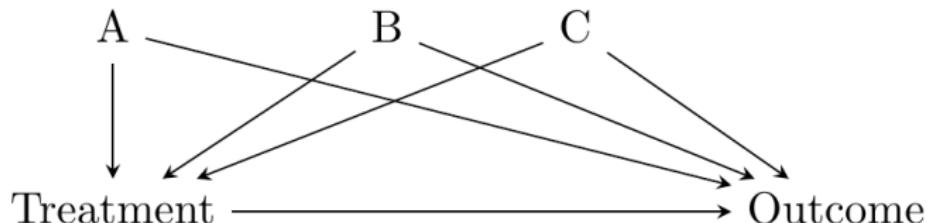
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## 2. Propensity score matching

- We want to account for the differential likelihood in getting into treatment depending on the value of the confounding variables
- We estimate the probability of getting into treatment, usually by doing a regression where the outcome is the treatment and the right-hand variables are the confounders
- We control for the propensity score matching, or select based on it (or both)

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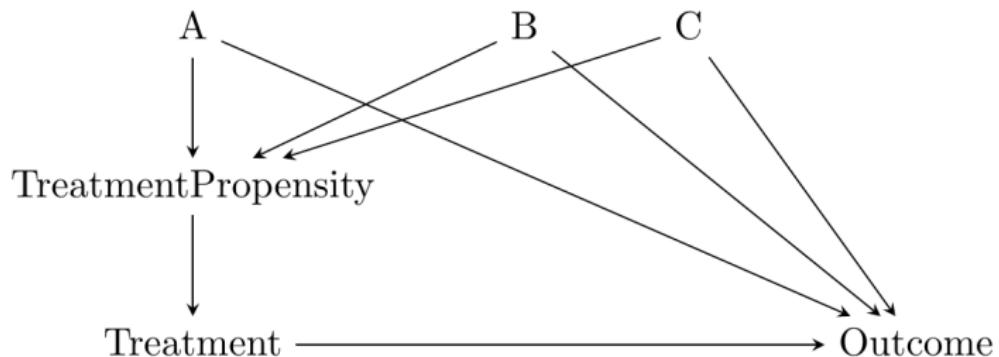
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## Two approaches to matching: propensity score

```
df = data.frame(  
    treat = c(rep(1, 100), rep(0, 1000)),  
    gender = c(rep("M", 80), rep("F", 20), rep(c("M","F"), each = 500)),  
    y = NA  
)  
  
df$y[df$treat == 1 & df$gender == "M"] = rbinom(80, 1, 0.75)  
df$y[df$treat == 1 & df$gender == "F"] = rbinom(20, 1, 0.6)  
df$y[df$treat == 0 & df$gender == "M"] = rbinom(500, 1, 0.7)  
df$y[df$treat == 0 & df$gender == "F"] = rbinom(500, 1, 0.55)  
  
m1 = glm(y ~ treat, data = df)  
m2 = glm(y ~ treat + gender, data = df)  
modelsummary(list(m1, m2))  
  
ps = glm(treat ~ gender, data = df)  
df$propensity_score = predict(ps, newdata = df)  
m3 = glm(y ~ treat + propensity_score, data = df)  
modelsummary(list(m1, m2, m3))
```

## Two approaches to matching: propensity score

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
(Intercept)	0.623	0.550	0.493
	(0.015)	(0.021)	(0.030)
treat	0.007	-0.037	-0.037
	(0.051)	(0.051)	(0.051)
genderM		0.147	
		(0.029)	
propensity_score			1.478
			(0.296)
Num.Obs.	1100	1100	1100

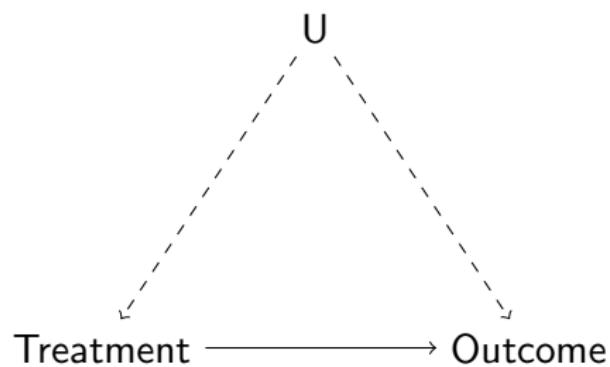
## Matching vs regression

- Matching and regression are complementary approaches
- e.g. regression doesn't waste any information, but has a linearity assumption
- It's usual to use both at the same time

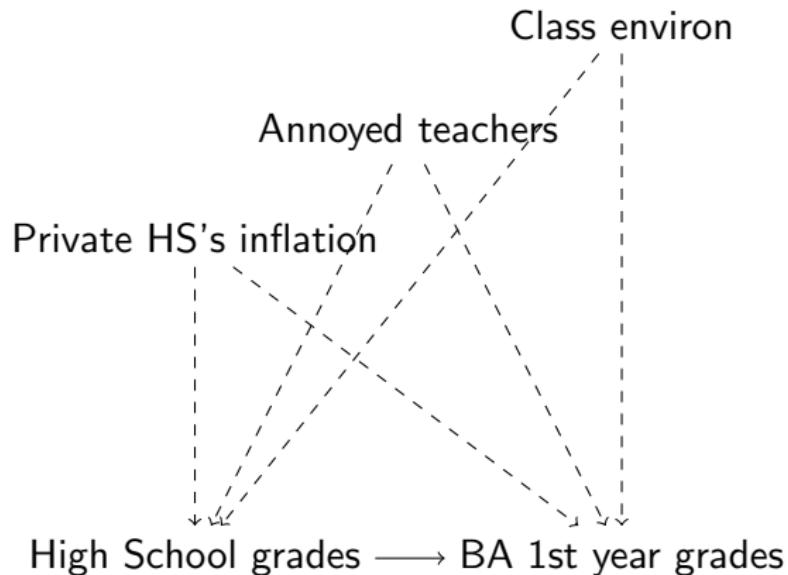
## Fixed effects

- The problem with covariate adjustment, regardless of whether we use regression or matching, is that we need to *observe* those variables
- But another strategy when we have unobserved confounders is to try within-group comparisons, which will work when the unobserved variance is contact within some group
- For example, imagine cases when our  $U$  variable is:
  - *Country history*, in a cross-national analysis
  - *City of origin*, in an individual-level analysis
  - *Individual background*, in a panel survey analysis
  - *Company effects*, if we look at the effects of English courses on internal promotion using individual data from many different companies
  - etc

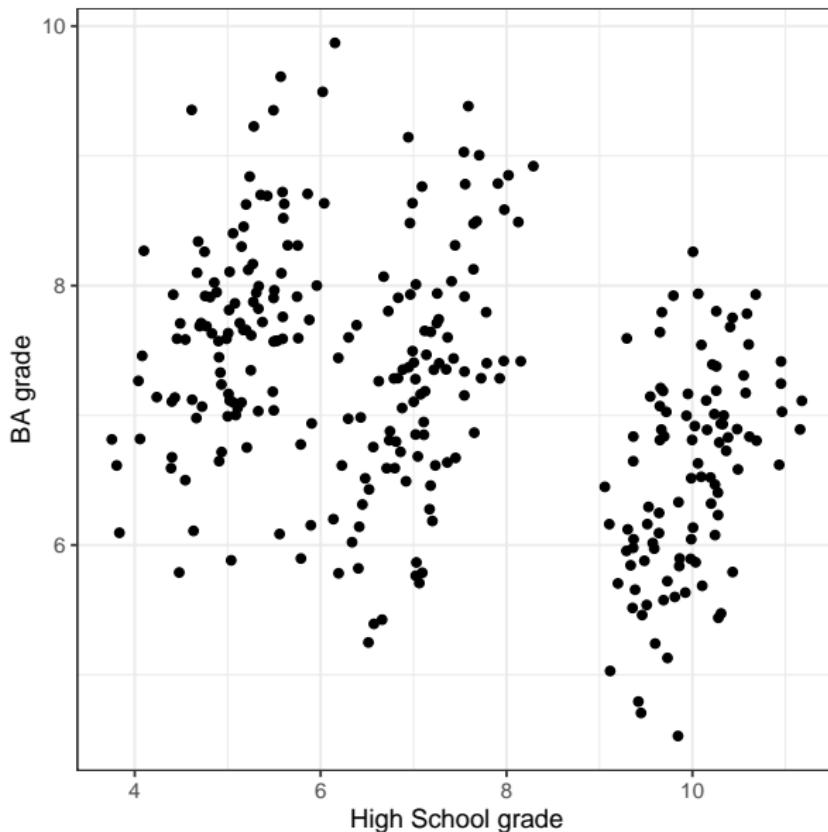
# Fixed effects - when do we use them?



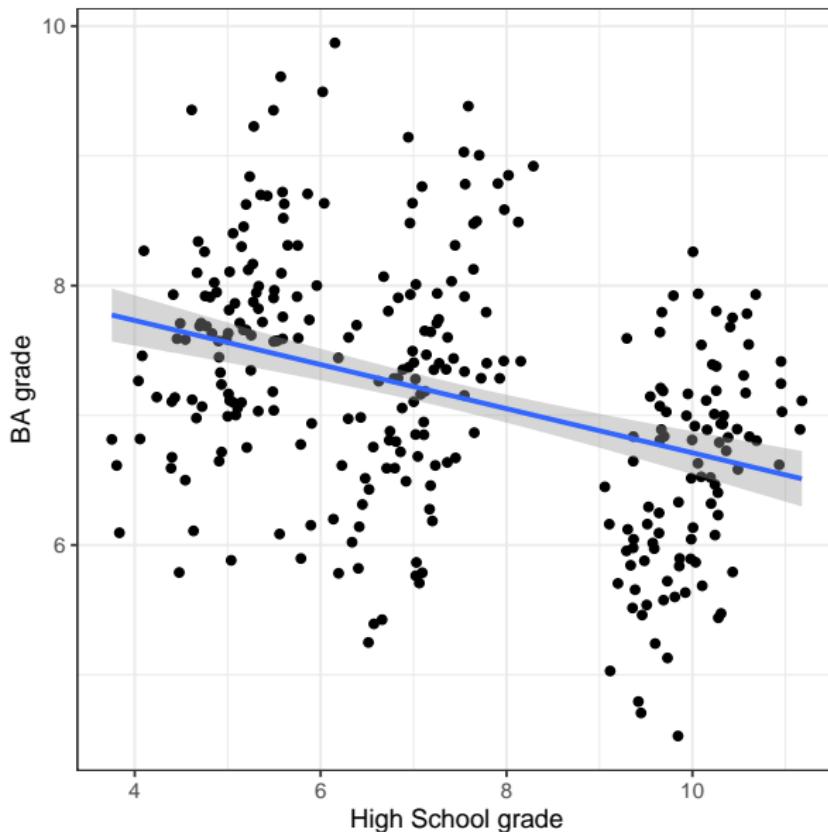
## Fixed effects - when do we use them?



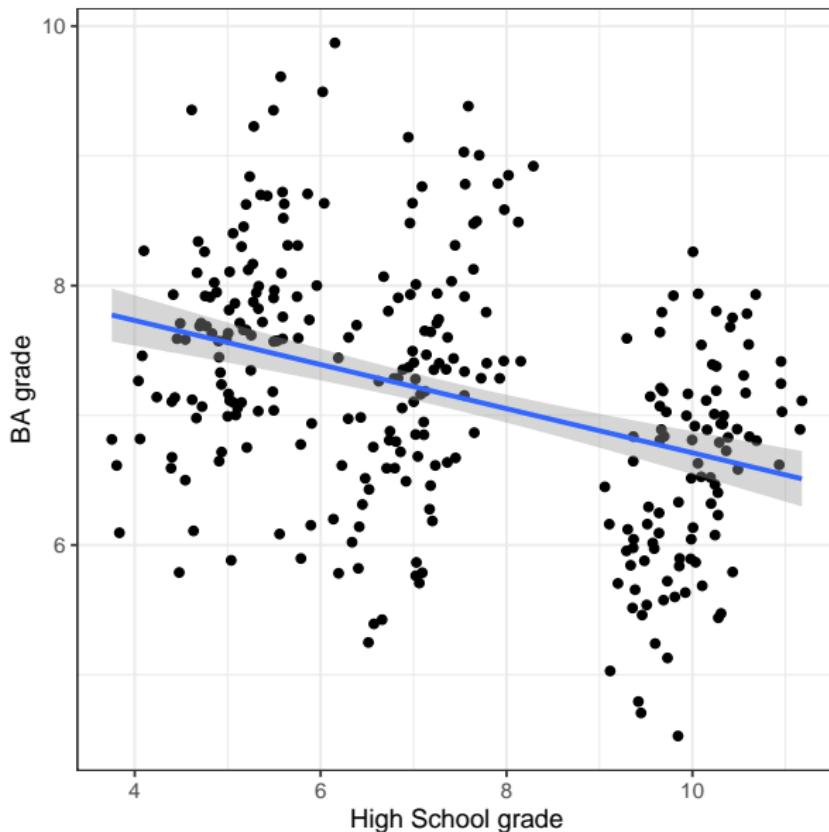
## Fixed effects



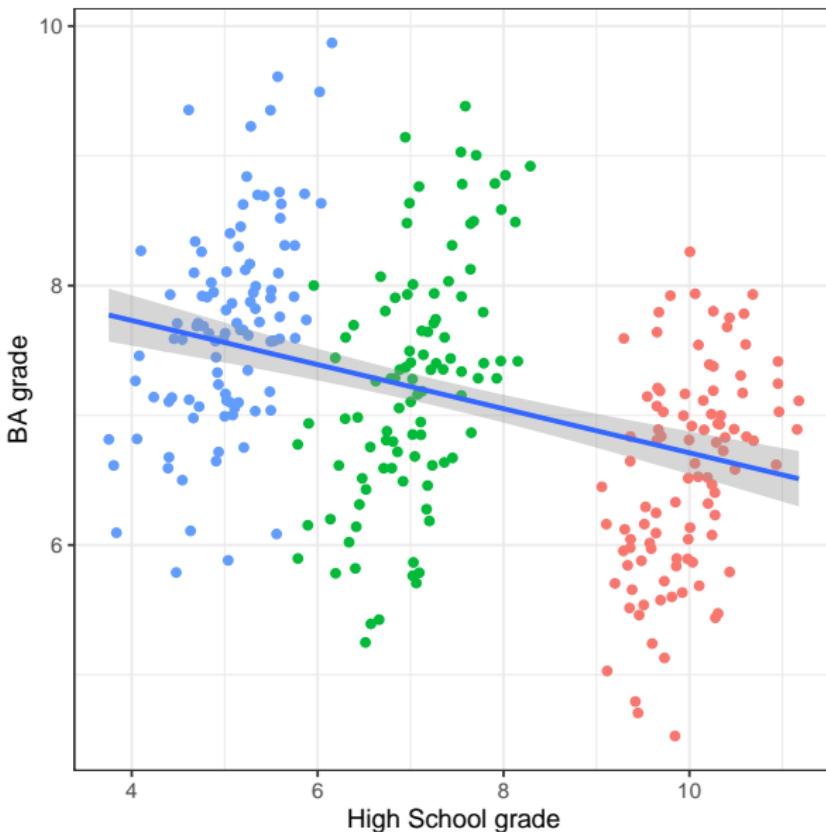
## Fixed effects



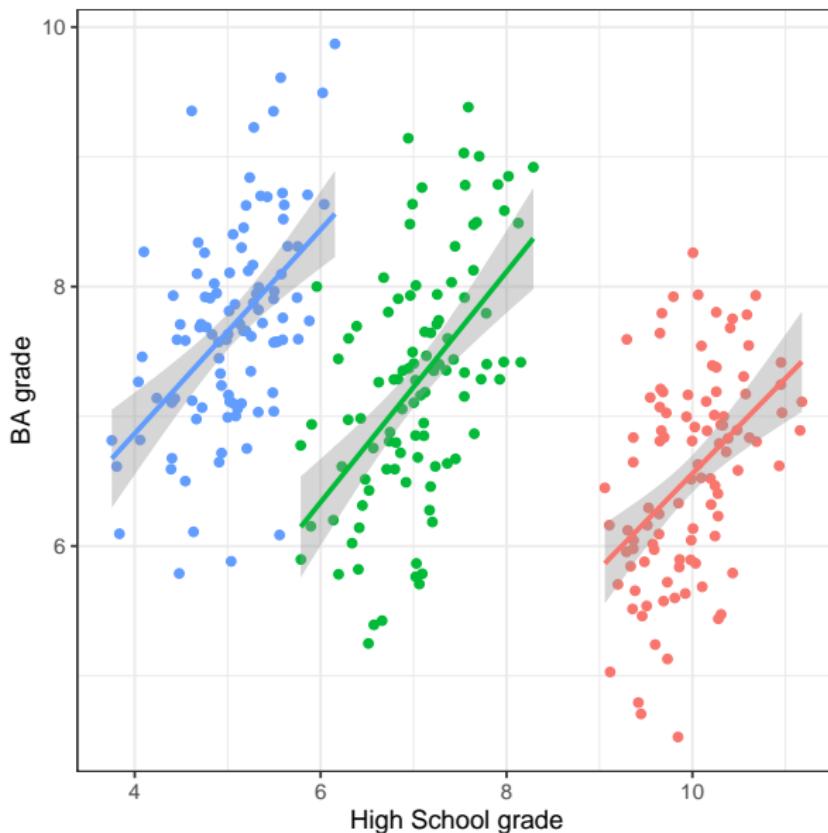
## Fixed effects



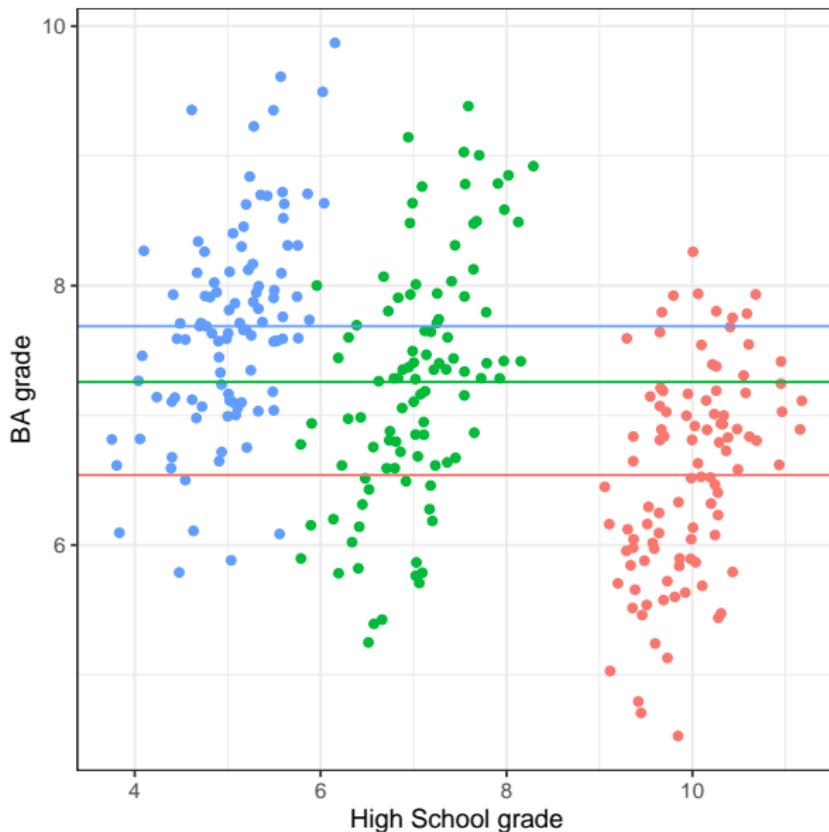
## Fixed effects



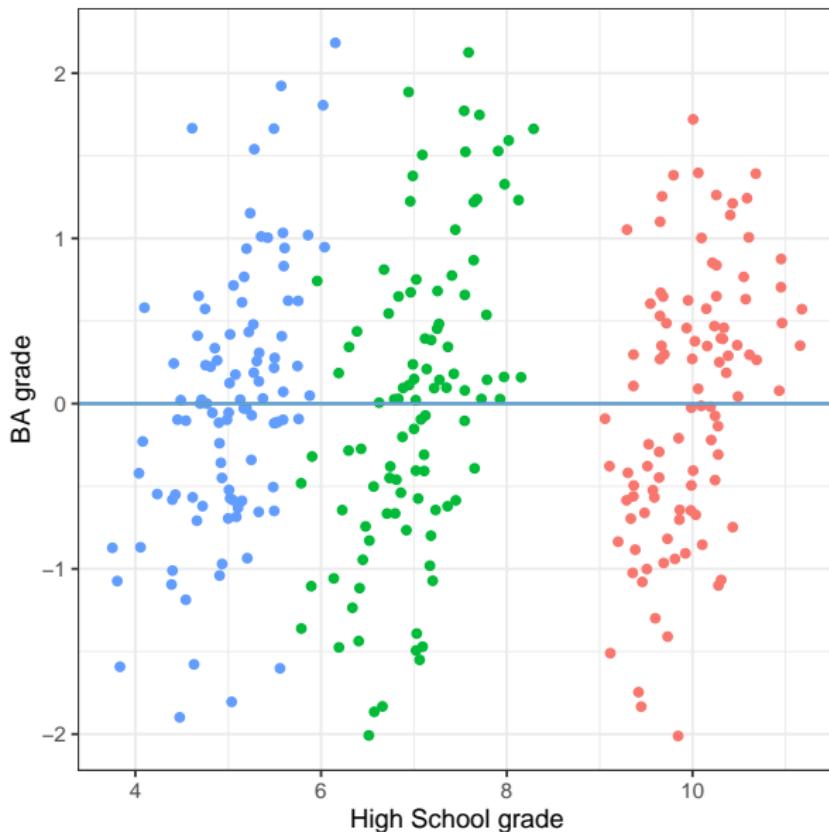
## Fixed effects



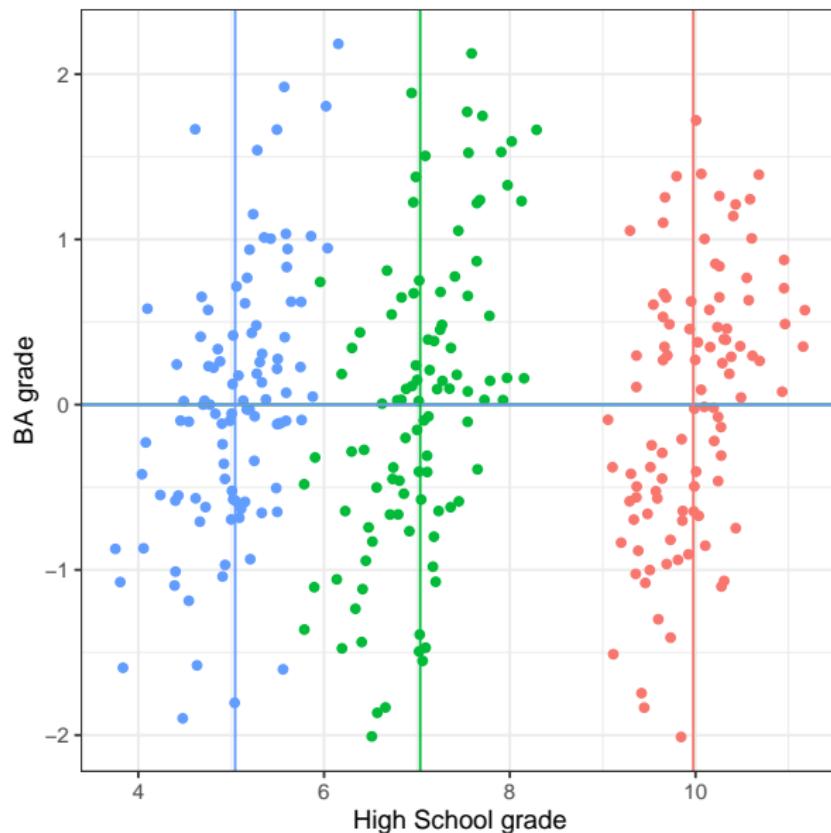
## Fixed effects



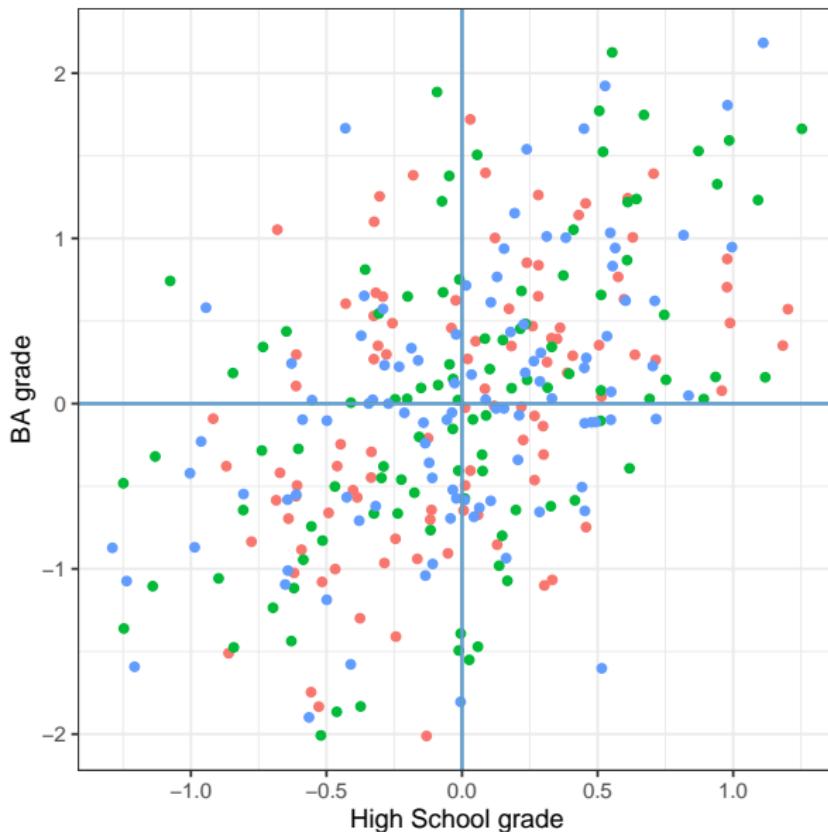
## Fixed effects



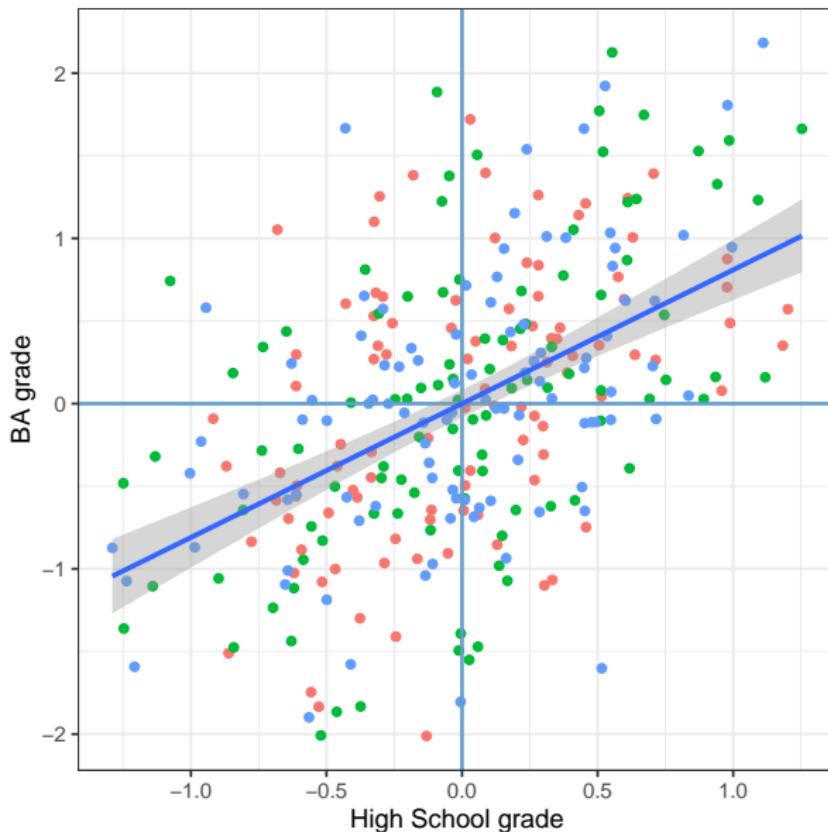
## Fixed effects



# Fixed effects



# Fixed effects



# Fixed effects and regression

```
# Simulate data: 300 students from three high schools,  
# normally distributed grades  
df = data.frame(high_school = rep(c("A", "B", "C"), each = 100)) %>%  
  group_by(high_school) %>%  
  mutate(hs_grade = rnorm(100, 7, 0.5)) %>%  
  ungroup()  
  
# But school A inflated grades a lot, and C was notoriously difficult  
df$hs_grade[df$high_school == "A"] = df$hs_grade[df$high_school == "A"] + 3  
df$hs_grade[df$high_school == "C"] = df$hs_grade[df$high_school == "C"] - 2  
  
# First-year BA grades are a function of the HS grade +/- the school inflation  
df$ba_grade = 2 + 0.75 * df$hs_grade + rnorm(300, 0, 0.75)  
df$ba_grade[df$high_school == "A"] = df$ba_grade[df$high_school == "A"] - 3  
df$ba_grade[df$high_school == "C"] = df$ba_grade[df$high_school == "C"] + 2
```

Our true causal model

# Fixed effects and regression

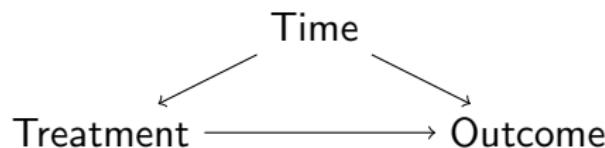
```
Call:  
lm(formula = ba_grade ~ hs_grade, data = df)  
  
Residuals:  
    Min      1Q  Median      3Q     Max  
-2.42897 -0.57050 -0.05923  0.66227  2.76385  
  
Coefficients:  
            Estimate Std. Error t value          Pr(>|t|)  
(Intercept)  8.70808   0.19412  44.86 < 0.0000000000000002 ***  
hs_grade     -0.22059   0.02544  -8.67 0.0000000000000283 ***  
---  
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1  
  
Residual standard error: 0.9061 on 298 degrees of freedom  
Multiple R-squared:  0.2014, Adjusted R-squared:  0.1988  
F-statistic: 75.17 on 1 and 298 DF,  p-value: 0.00000000000002826
```

# Fixed effects and regression

```
Call:  
lm(formula = ba_grade ~ hs_grade + factor(high_school), data = df)  
  
Residuals:  
    Min      1Q  Median      3Q     Max  
-2.0124 -0.5464  0.0042  0.4984  2.2539  
  
Coefficients:  
              Estimate Std. Error t value          Pr(>|t|)  
(Intercept) -1.68390   0.98549 -1.709          0.0886 .  
hs_grade      0.80803   0.09871  8.186 0.00000000000000811 ***  
factor(high_school)B 3.23797   0.30984 10.451 < 0.0000000000000002 ***  
factor(high_school)C 5.26454   0.49388 10.659 < 0.0000000000000002 ***  
---  
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1  
  
Residual standard error: 0.7719 on 296 degrees of freedom  
Multiple R-squared:  0.4243, Adjusted R-squared:  0.4185  
F-statistic: 72.73 on 3 and 296 DF,  p-value: < 0.0000000000000022
```

# Difference-in-differences

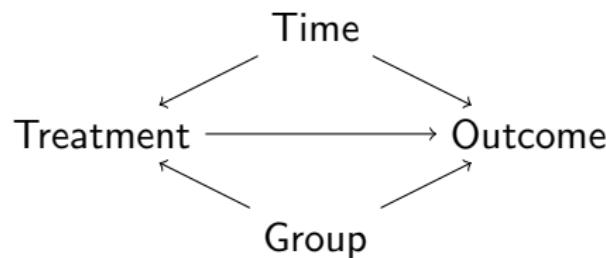
- Treatments usually occur at a particular moment in time, e.g.:
  - Minimum wage increase
  - Terrorist attack
  - Influx of refugees
  - ...
- In those cases, if we have before & after observations, we have something like this:



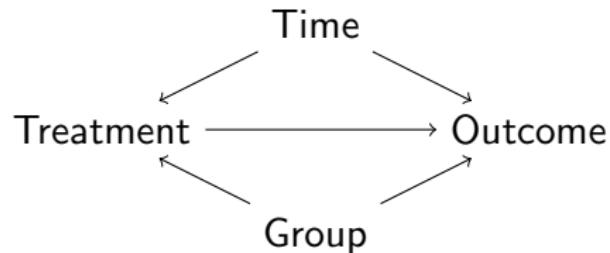
- The **problem** is that **all** the variation goes through time, so if we close that back door, we're left with nothing

# Difference-in-differences

- So one strategy we can use is to bring additional group that is *not treated* and for which we also have before/after observations
  - Minimum wage increase: maybe those earning above MW?
  - Influx of refugees: other countries? regions far from the border?
  - Terrorist attack: do we have a control (untreated) group?
  - ...

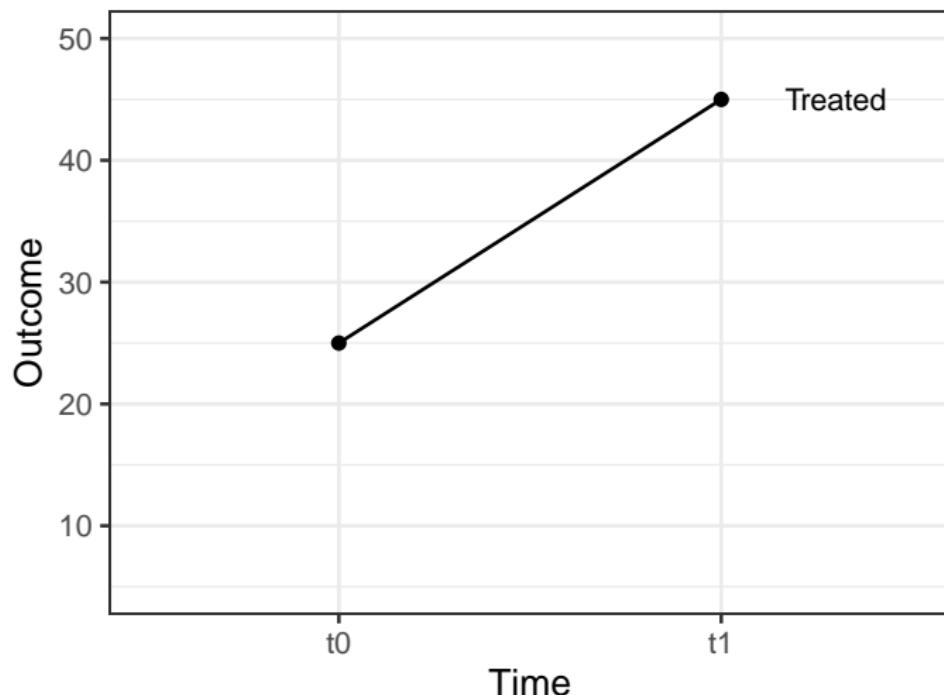


# Difference-in-differences

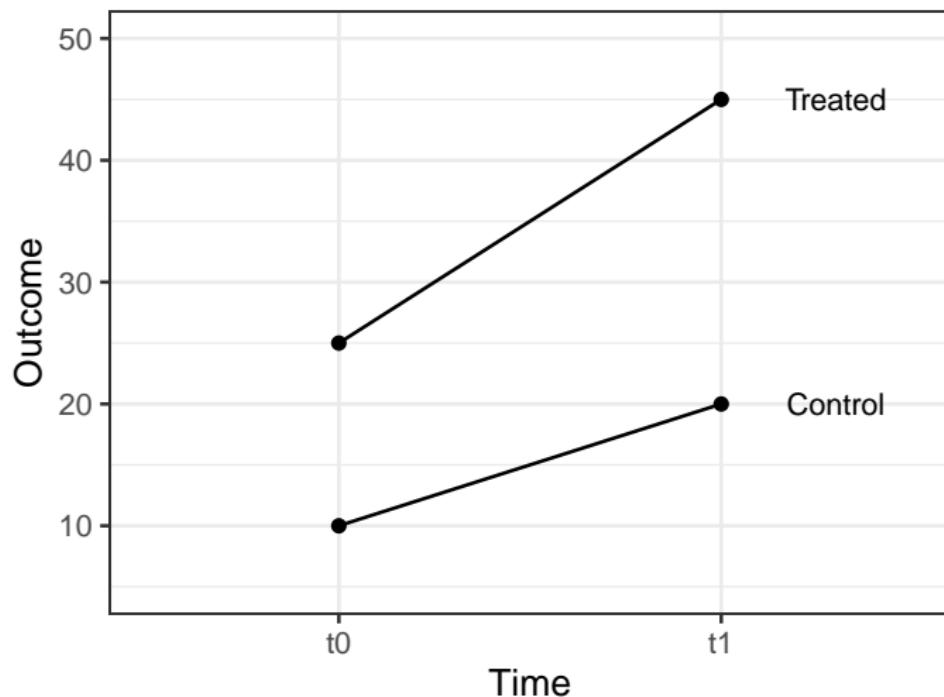


- We can compare changes across time *within* the treated and control groups (closing the back door through group)
- Compare within-group variation between treated and control (since time affects both ‘within-variations’ the same way, we are closing the other back door through time)

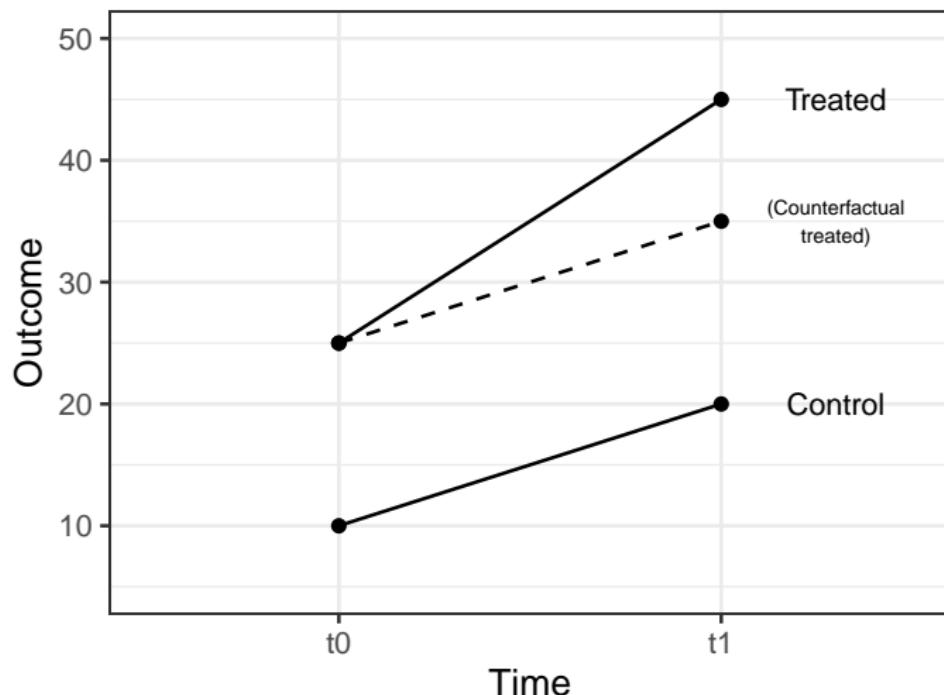
## Difference-in-differences



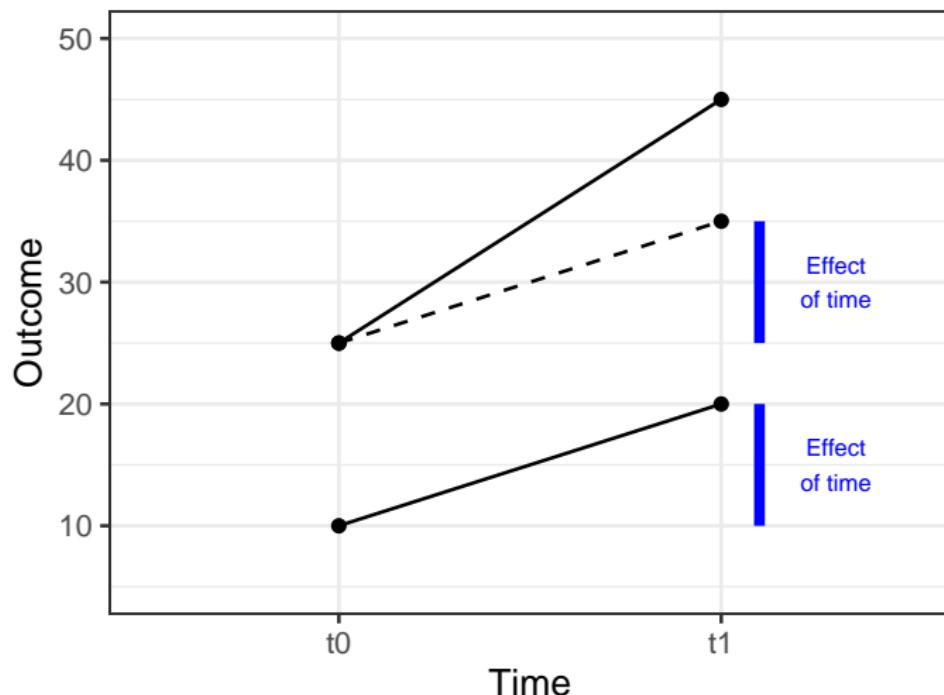
## Difference-in-differences



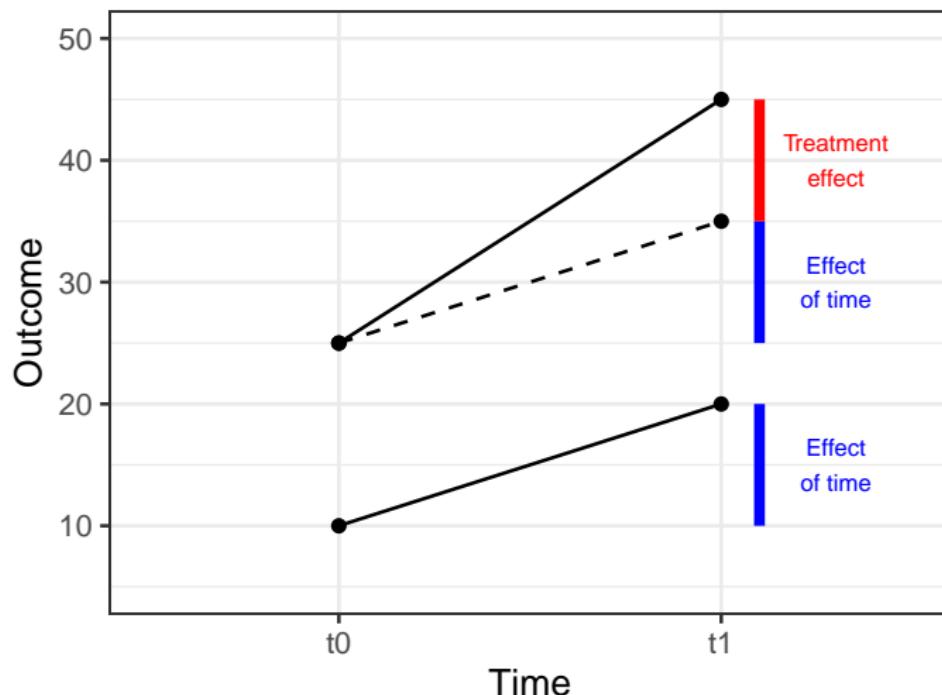
# Difference-in-differences



# Difference-in-differences



# Difference-in-differences



# Difference-in-differences: Cholera in London

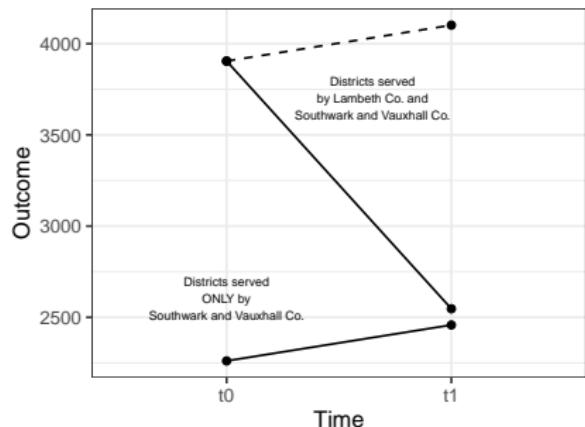


- Probably first use of DiD and natural experiments?
- Lambeth Company moved water intake upriver in 1852, Southwark & Vauxhall Company still got it from downstream

# Difference-in-differences: Cholera in London

TABLE XII.

2261	2458	Southwk. & Vauxhall.
3905	2547	Both Companies.
162	37	Lambeth Company.



Sub-Districts.	Deaths from Cholera in 1849.	Deaths from Cholera in 1854.	Water Supply.
St. Saviour, Southwark .	263	371	
St. Olave .	157	161	
St. John, Horsleydown .	192	148	
St. James, Bermondsey .	949	363	
St. Mary Magdalene .	259	244	
Leather Market .	226	237	
Rotherhithe* .	352	282	
Wandsworth .	97	59	
Battersea .	111	171	
Putney .	8	9	
Camberwell .	935	240	
Peckham .	92	174	
Christchurch, Southwark	256	113	
Kent Road .	267	174	
Borough Road .	312	270	
London Road .	257	93	
Trinity, Newington	318	210	
St. Peter, Walworth	446	388	
St. Mary, Newington	143	92	
Waterloo Road (1st)	193	58	
Waterloo Road (3rd)	243	117	
Lambeth Church (1st)	915	49	
Lambeth Church (2nd)	544	193	
Kennington (1st)	187	303	
Kennington (2nd)	153	142	
Brixton .	81	48	
Clapham .	114	165	
St. George, Camberwell	176	133	
Norwood .	9	10	
Streatham .	154	15	Lambeth Company only.
Dulwich .	1	—	
Sydenham .	5	12	
First 12 sub-districts .	2261	2458	Southwk. & Vauxhall.
Next 16 sub-districts .	3905	2547	Both Companies.

# Difference-in-differences

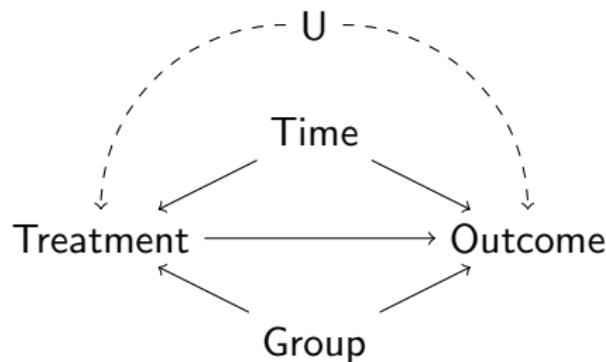
- You can estimate this effect just with group means
- But it is often easier to use regression, also because you can include controls:

$$Y_{it} = \beta_0 + \beta_1 Treated_i + \beta_2 After_t + \beta_3 (Treated_i \times After_t) + \beta^\top \mathbf{x}_i + \epsilon_{it} \quad (1)$$

- But why do we need all this?

# Difference-in-differences

- Because DiD identification **depends** on the assumption that the **control group is a good counterfactual to the treated group**

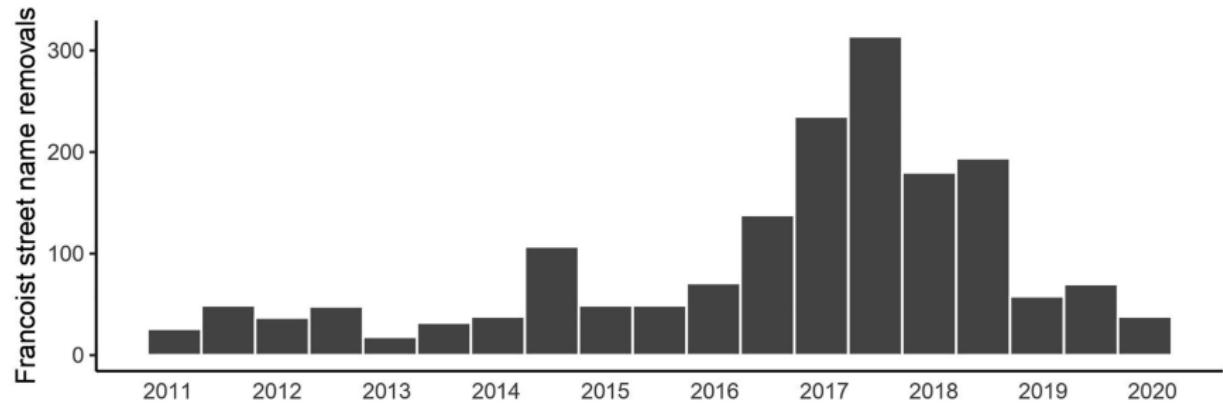


- One way to test this is checking if the **parallel trends assumption** holds (we need data further back in time)

## DiD example

- What is the effect of symbolic TJ policies?
- [journals.sagepub.com/doi/full/10.1177/20531680211058550](https://journals.sagepub.com/doi/full/10.1177/20531680211058550)

# DiD example



# DiD example

Francoist names	Removed Francoist names, 2016–2018?	
In June 2016?	No	Yes
No	6455 (100%)	0 (0%)
	1184 (72%)	454 (28%)

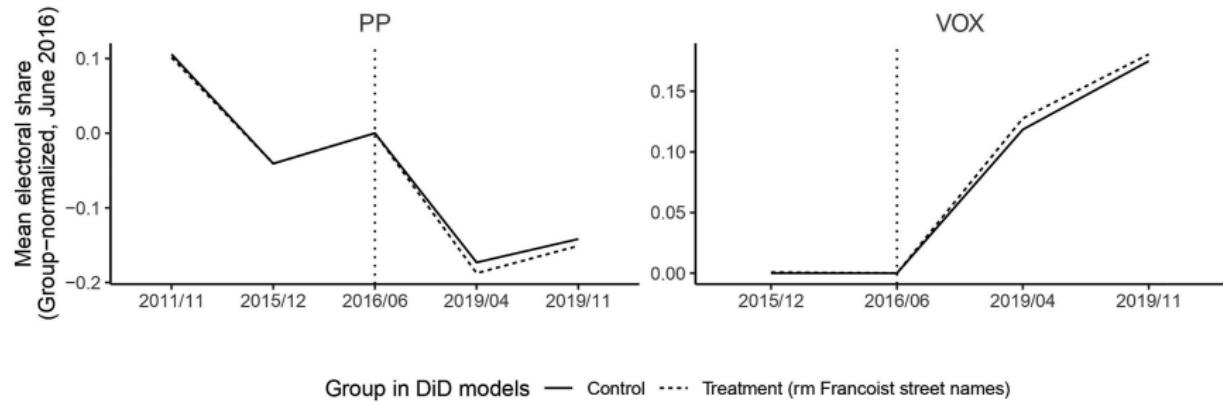
*Note:* Row percentages. Changes in 2016–2018 refer to the period between 30/06/2016 and 31/12/2018.

# DiD example

Table 2. Mean electoral share in sample.

Party	June 2016			April 2019			
	<i>Control</i>	<i>Treated</i>	$\Delta$	<i>Control</i>	<i>Treated</i>	$\Delta$	$\Delta_{2019} - \Delta_{2016}$
Vox	0.21	0.21	0	12.54	13.28	0.74	0.74
PP	41.22	46.77	5.55	23.83	27.68	3.85	-1.7
PSOE	29.13	28.01	-1.12	33.38	32.03	-1.35	-0.23

# DiD example



# Regression discontinuity

## The Political Salience of Cultural Difference: Why Chewas and Tumbukas Are Allies in Zambia and Adversaries in Malawi

DANIEL N. POSNER *University of California, Los Angeles*

This paper explores the conditions under which cultural cleavages become politically salient. It does so by taking advantage of the natural experiment afforded by the division of the Chewa and Tumbuka peoples by the border between Zambia and Malawi. I document that, while the objective cultural differences between Chewas and Tumbukas on both sides of the border are identical, the political salience of the division between these communities is altogether different. I argue that this difference stems from the different sizes of the Chewa and Tumbuka communities in each country relative to each country's national political arena. In Malawi, Chewas and Tumbukas are each large groups vis-à-vis the country as a whole and, thus, serve as viable bases for political coalition-building. In Zambia, Chewas and Tumbukas are small relative to the country as a whole and, thus, not useful to mobilize as bases of political support. The analysis suggests that the political salience of a cultural cleavage depends not on the nature of the cleavage itself (since it is identical in both countries) but on the sizes of the groups it defines and whether or not they will be useful vehicles for political competition.

# Regression discontinuity

**FIGURE 1. Research Sites**

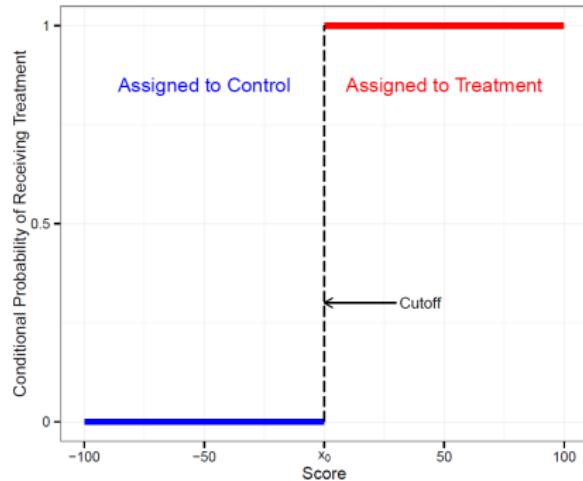


# Regression discontinuity

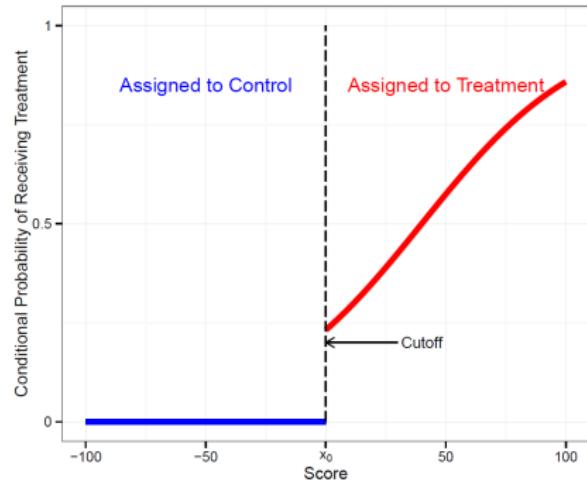
- RDD works well when assignment into treatment depends on a cutoff along a **running variable**
  - Do incumbent politicians have an electoral advantage? (vote share)
  - What is the effect of being drafted into the military? (birth year)
  - Effect of national policies in ethnic identification in Africa? (distance to colonial borders)
- This is the source of the exogenous variation (or if you will, the natural experiment):
  - 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)
  - Sometimes you look at different *bandwidths* to check this

# RDD

Figure 1: Conditional Probability of Receiving Treatment in Sharp vs. Fuzzy RD Designs



(a) Sharp RD

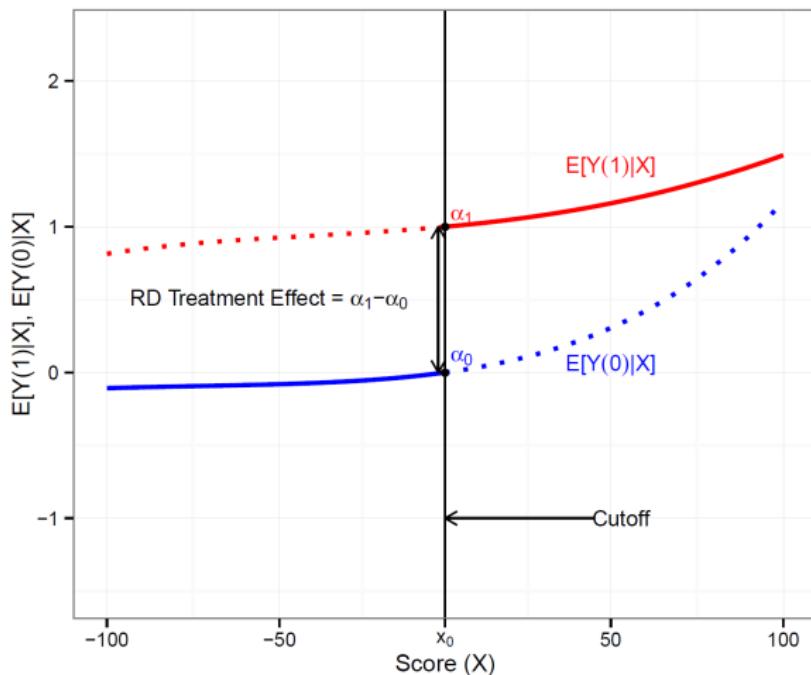


(b) Fuzzy RD (One-Sided)

<https://bookdown.org/paul/applied-causal-analysis/rddbasics2.html>

# RDD

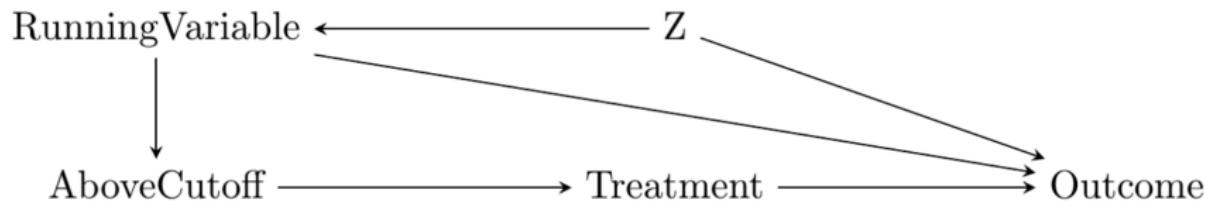
Figure 2: RD Treatment Effect in Sharp RD Design



<https://bookdown.org/paul/applied-causal-analysis/rddbasics3.html>

# RDD

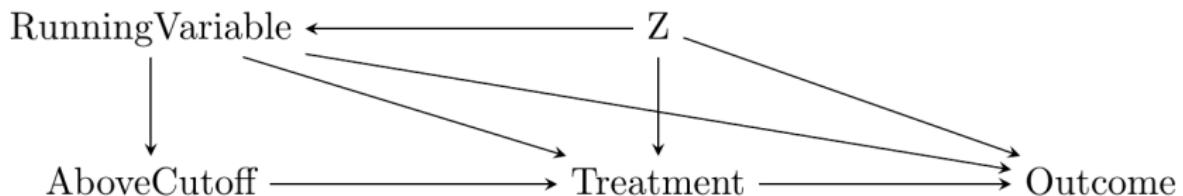
- Underlying assumption: other confounders also vary along the running variable, but are independent to the *jump*



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

# RDD

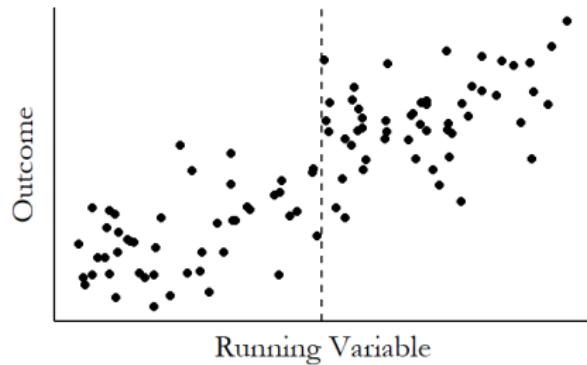
- Underlying assumption: other confounders also vary along the running variable, but are independent to the *jump*
- Even if in a *fuzzy* design



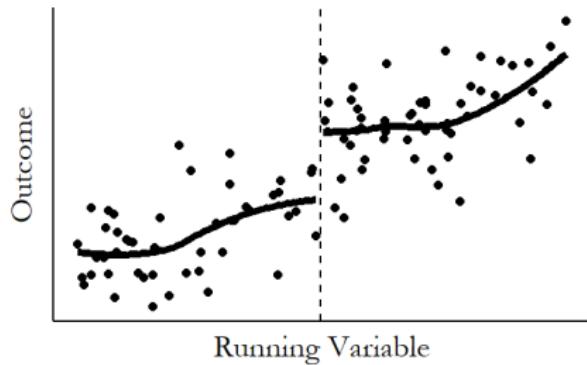
Huntington-Klein, *The Effect*, p.508

# RDD implementation

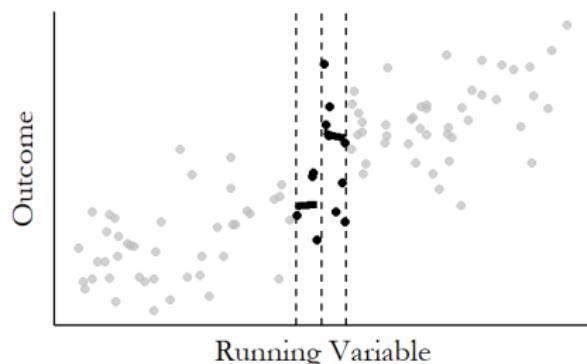
(a) Raw Data



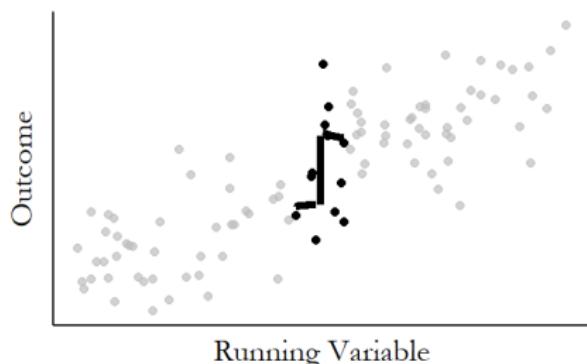
(b) Predict Values Near the Cutoff



(c) Pick a Bandwidth

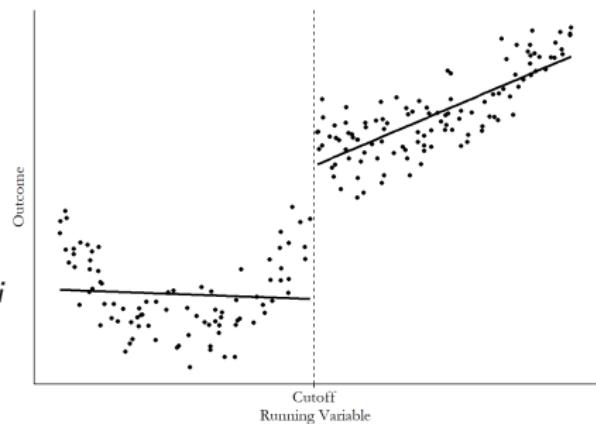


(d) Estimate Jump at the Cutoff



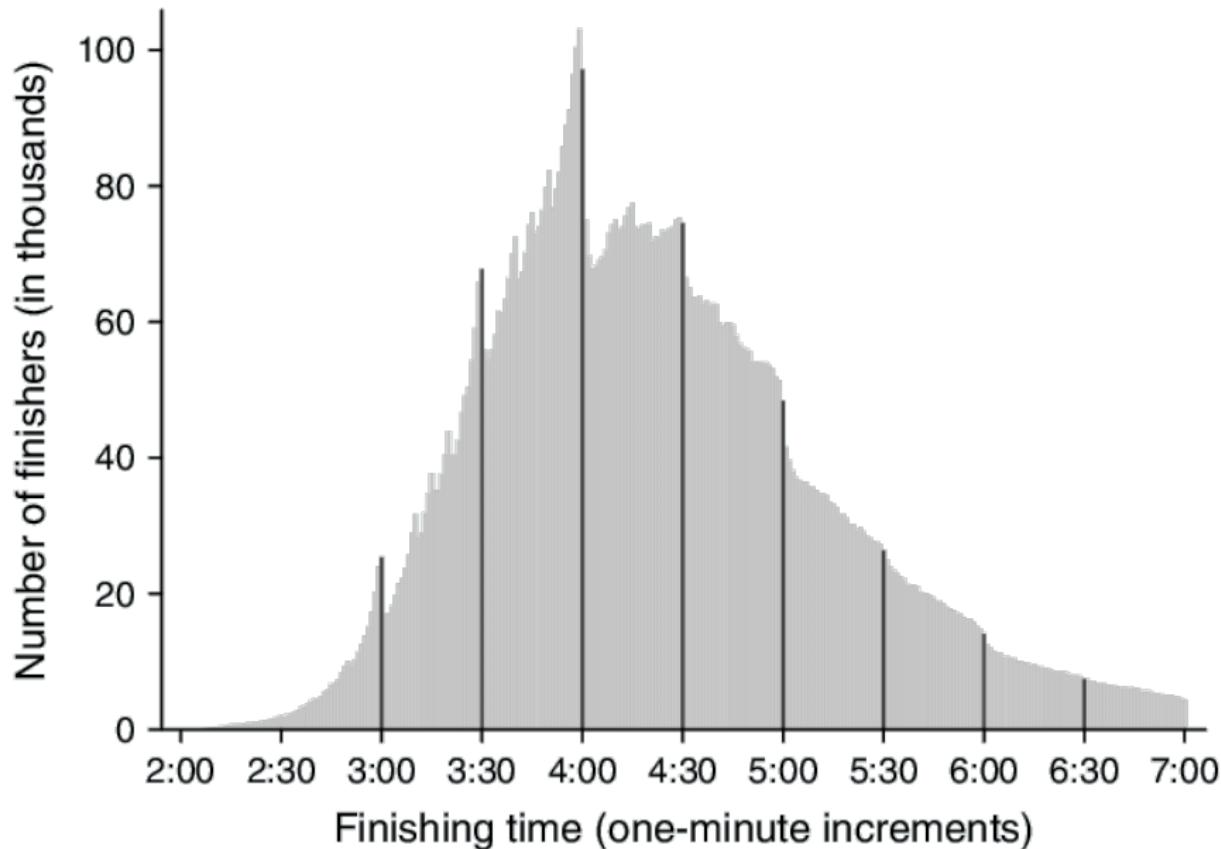
# RDD and regression

$$Y = \beta_0 + \beta_1 Distance + \\ \beta_2 Treated + \\ \beta_3 (Treated \times Distance) + \beta^T \mathbf{x}_i \\ (2)$$

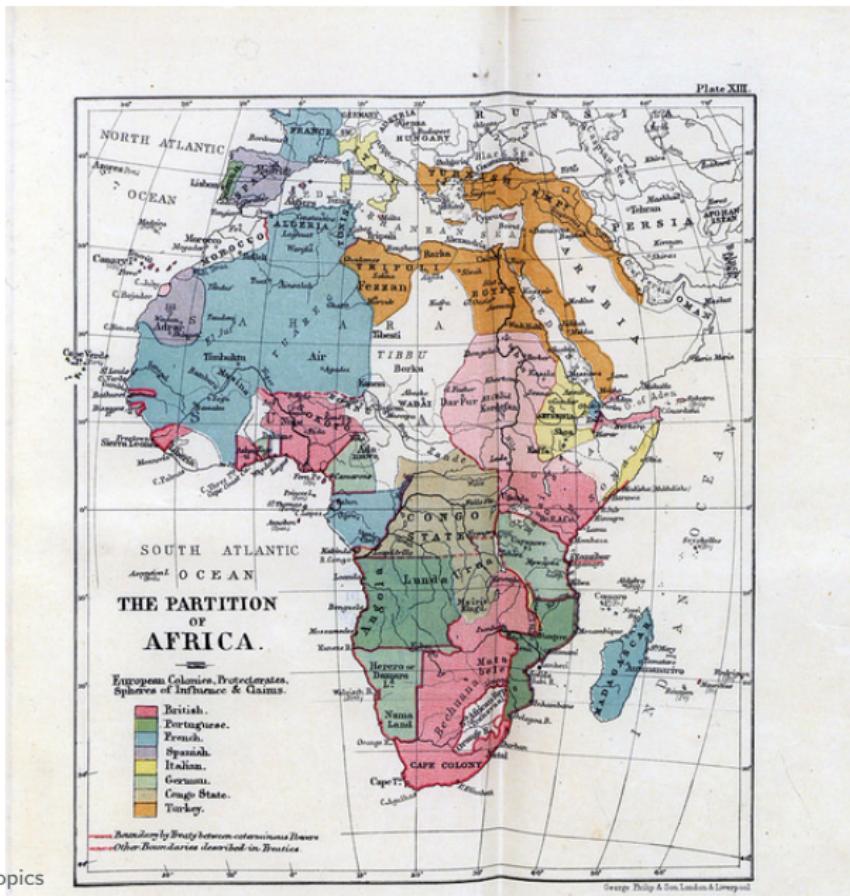


- But there's no need to use linear regression, other methods available as well

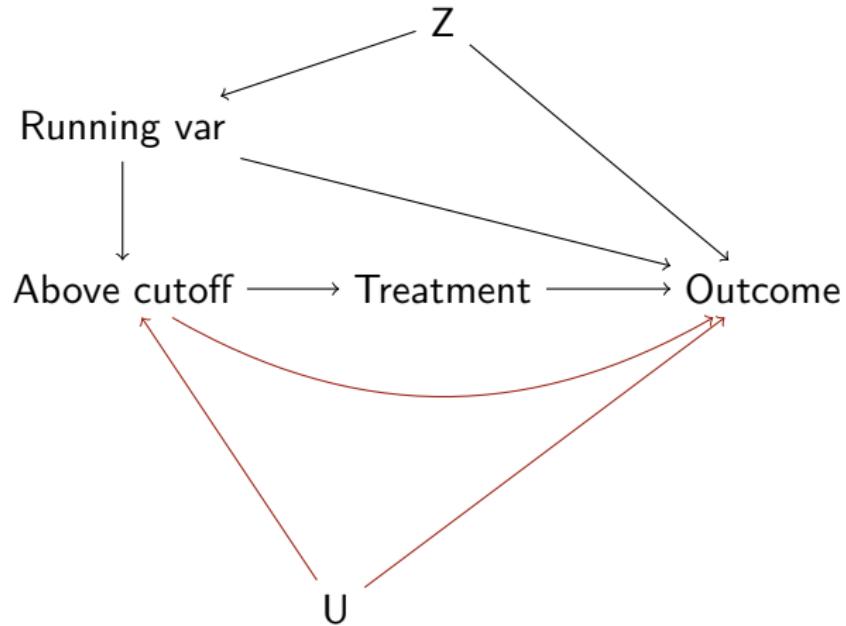
## Threats to RDD: precise sorting



# Threats to RDD: cutoff ← outcome



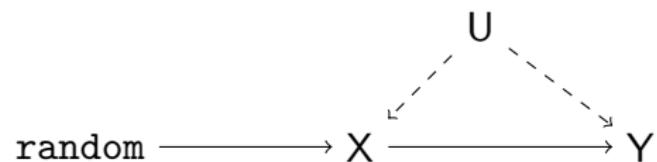
## Threats to RDD



## Some extensions & combinations

- DiD with multiple treatment periods (units being treated at different times)
- Matched DiD
- Difference-in-discontinuities

# Instrumental variables



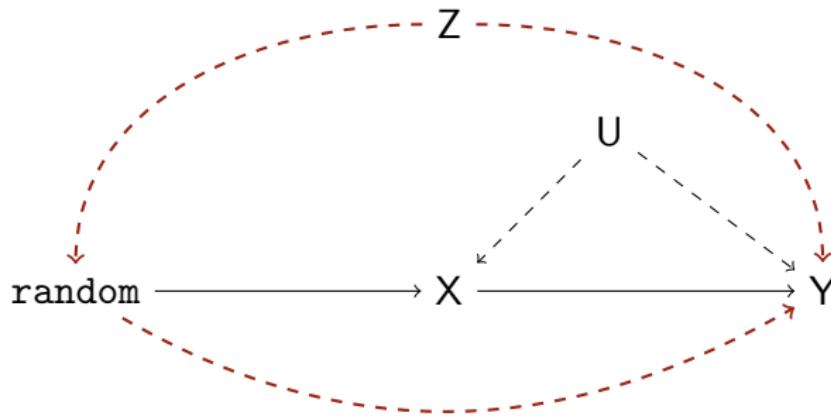
## Instrumental variables

- Find an exogenous source of variation in the treatment variable
- Isolate that variation and use it to identify the causal effect

# Instrumental variables

- Find an exogenous source of variation in the treatment variable
- Isolate that variation and use it to identify the causal effect
- 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

# Instrumental variables



## IV threats

### Economic Shocks and Civil Conflict: An Instrumental Variables Approach

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Edward Miguel

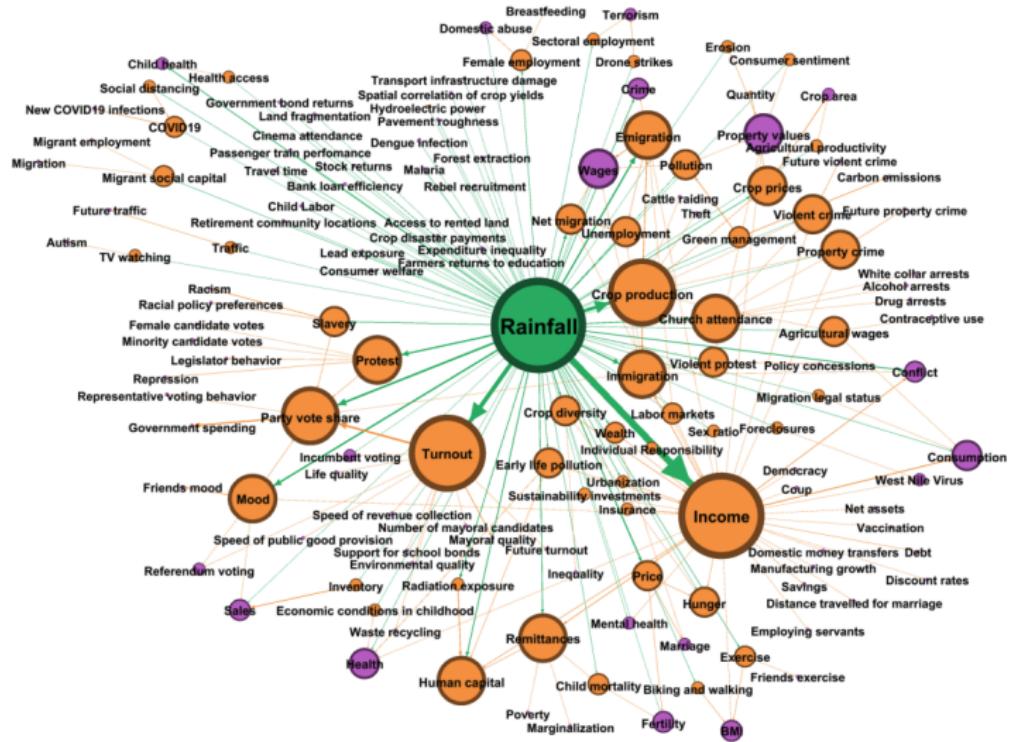
*University of California, Berkeley and National Bureau of Economic Research*

Shanker Satyanath and Ernest Sergenti

*New York University*

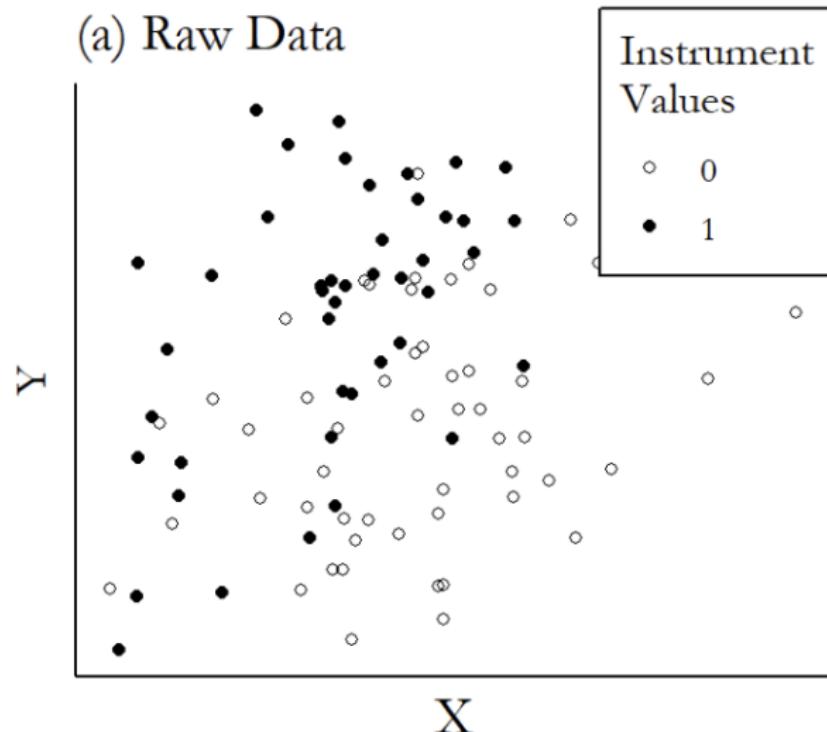
Estimating the impact of economic conditions on the likelihood of civil conflict is difficult because of endogeneity and omitted variable bias. We use rainfall variation as an instrumental variable for economic growth in 41 African countries during 1981–99. Growth is strongly negatively related to civil conflict: a negative growth shock of five percentage points increases the likelihood of conflict by one-half the following year. We attempt to rule out other channels through which rainfall may affect conflict. Surprisingly, the impact of growth shocks

## IV threats



Jonathan Mellon (2022) Rain, Rain, Go Away: 192 Potential Exclusion-Restriction

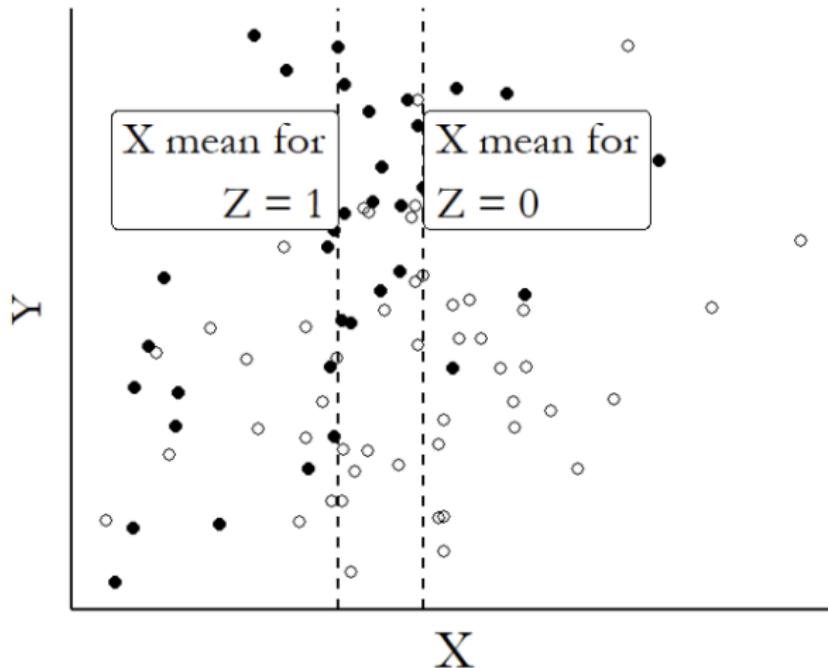
# How does IV work?



(Huntington-Klein, *The Effect*, p 472)

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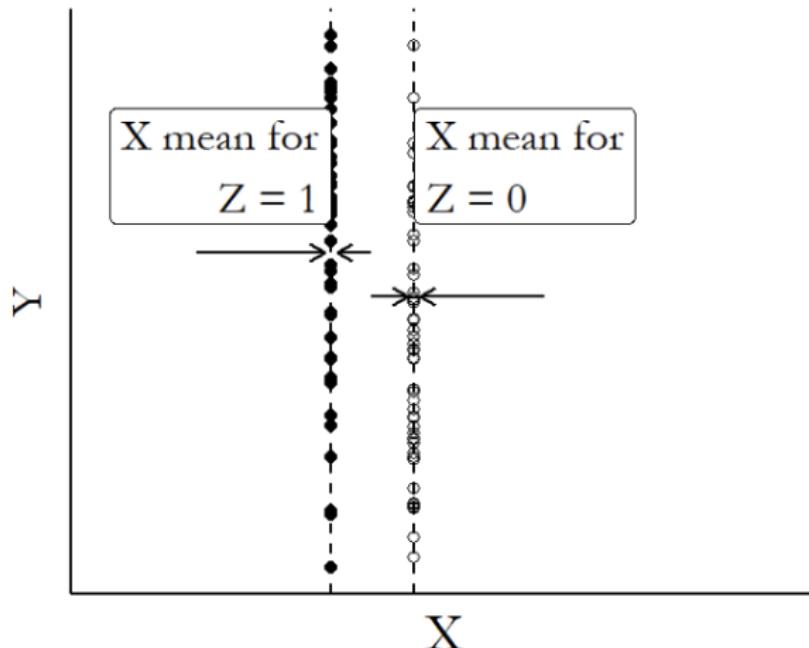
(b) Predict X with Z



(Huntington-Klein, *The Effect*, p 472)

# How does IV work?

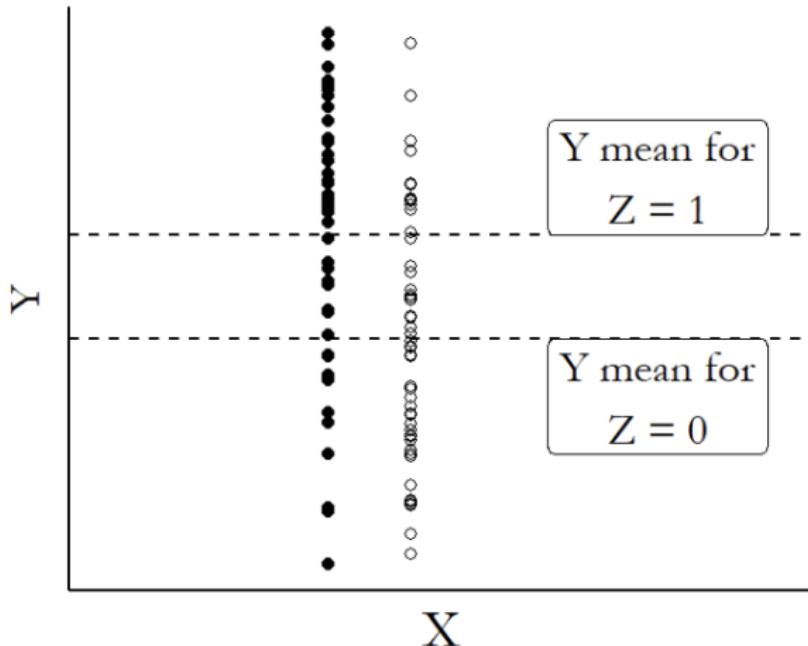
## (c) Only Use Predicted Variation



(Huntington-Klein, *The Effect*, p 472)

# How does IV work?

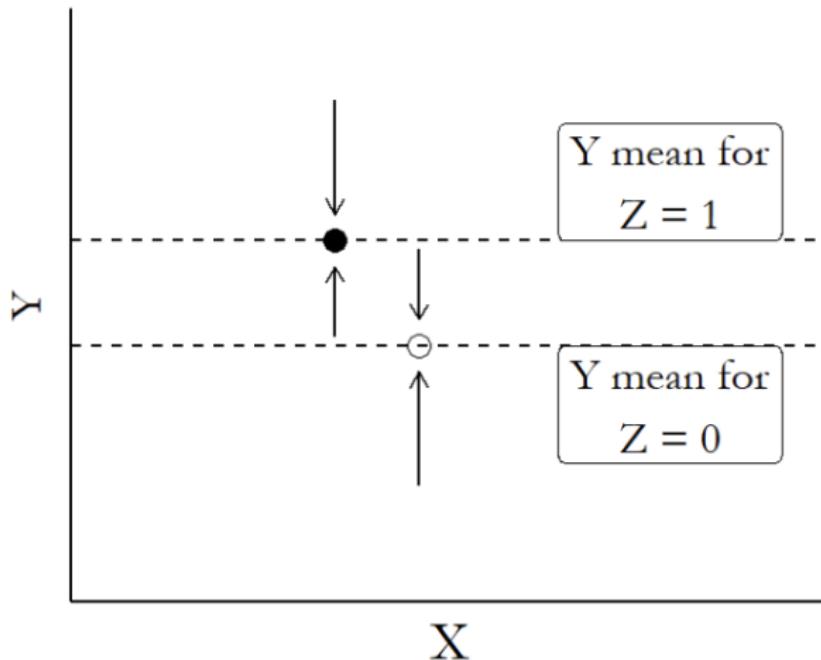
(d) Predict Y with Z



(Huntington-Klein, *The Effect*, p 472)

# How does IV work?

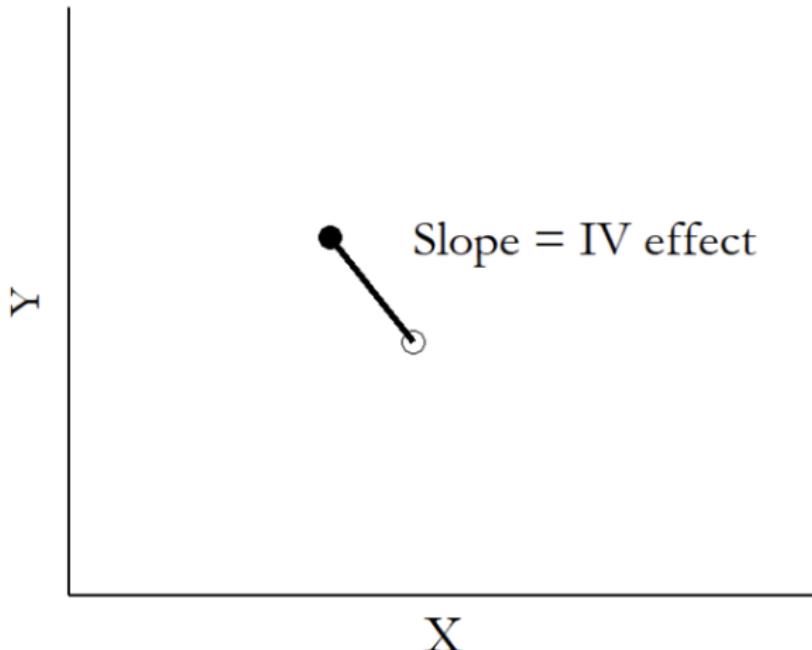
(e) Only Use Predicted Variation



(Huntington-Klein, *The Effect*, p 472)

# How does IV work?

(f) Relate Predicted Y to Predicted X



(Huntington-Klein, *The Effect*, p 472)

# How does IV work?

- Usually: two-stage least squares, or **2SLS**
1. Run a 'first-stage' regression to predict the treamtnet with the instrument
  2. Use the predicted values to predict the outcome in the 'second-stage'

# Alternative approaches to IV: build your own

## Roads to Rule, Roads to Rebel: Relational State Capacity and Conflict in Africa

Carl Müller-Crepon<sup>1</sup> , Philipp Hunziker<sup>2</sup>,  
and Lars-Erik Cederman<sup>3</sup>

Journal of Conflict Resolution  
2021, Vol. 65(2-3) 563-590

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### Abstract

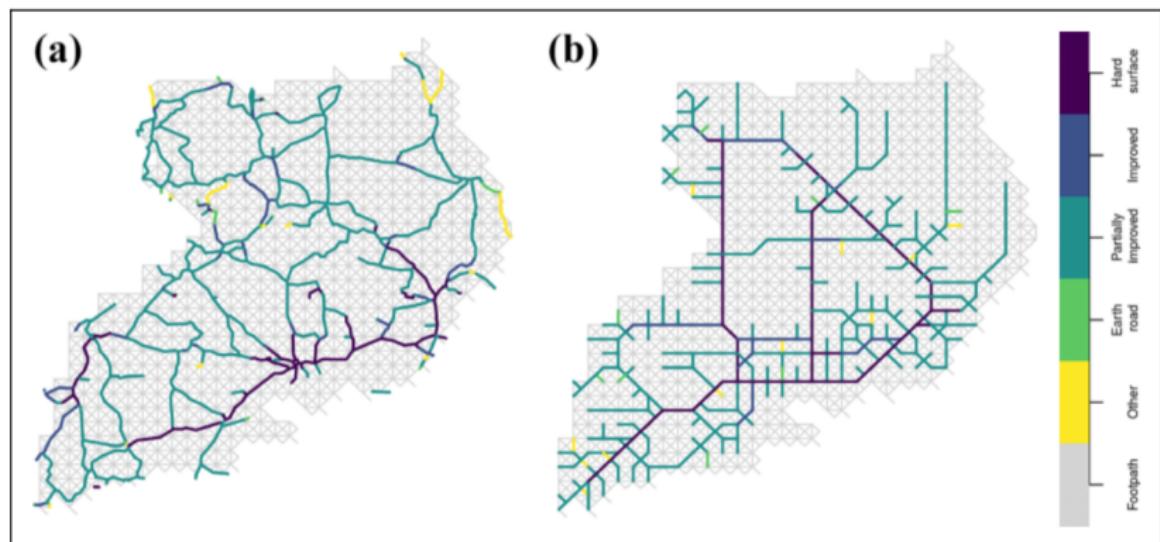
Weak state capacity is one of the most important explanations of civil conflict. Yet, current conceptualizations of state capacity typically focus only on the state while ignoring the relational nature of armed conflict. We argue that opportunities for conflict arise where relational state capacity is low, that is, where the state has less control over its subjects than its potential challengers. This occurs in ethnic groups that are poorly accessible from the state capital, but are internally highly interconnected. To test this argument, we digitize detailed African road maps and convert them into a road atlas akin to Google Maps. We measure the accessibility and internal connectedness of groups via travel times obtained from this atlas and simulate road networks for an instrumental variable design. Our findings suggest that

# Alternative approaches to IV: build your own

## *Instrumental Variable Approach*

We complement our robustness checks with an instrumental variable (IV) strategy that addresses potential omitted variable biases not captured by the previous tests. In particular, there might be hitherto unmeasured group-level characteristics that have affected colonial road building and recent conflict. To address such endogeneity as well as potential systematic measurement bias in the Michelin maps, our IV approach exploits variation from road networks simulated on the basis of countries' population distribution. Our IV approach improves identification by isolating the component of RSC that is due to the spatial population distribution within a country. While population distributions are less malleable than road networks, populations are not randomly distributed. We must therefore rely on the assumption that the population distributions that produce our simulated road networks are conditionally exogenous to conflict. We address potential violations of this assumption below.

# Alternative approaches to IV: build your own



**Figure 3.** Observed and simulated road network in Uganda, 1966. (a) Observed network.  
(b) Simulated network.