Advanced topics & overview

Francisco Villamil

Research Design for Social Sciences
MA Computational Social Science, UC3M
Fall 2023

Roadmap

Questions & review

- Questions?

Questions & review

- Questions?
- 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

Questions & review

- Questions?
- 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

Roadmap

Generalizing results

- The Credibility Revolution in social sciences
- What are we really learning?

Generalizing results

- The Credibility Revolution in social sciences
- What are we really learning?
- Importance of meta-scientific knowledge
    1. Robustness of results and incremental science ($\rightarrow$ meta-analyses)
    2. Generalization of results ($\rightarrow$ construct and external validity)

An example of a meta-analysis

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

An example of a meta-analysis

- 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*

Scholars 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.

An example of a meta-analysis

1. Literature review $\rightarrow$ 3,300 records

An example of a meta-analysis

1. Literature review $\rightarrow$ 3,300 records
2. Look for similar research designs and topics:

   > "the 46 included studies (102 estimates) quantitatively analyze the effect of plausibly exogenous variation in world commodity prices on armed civil conflict by using a generalized difference-in-difference design with unit and time fixed effects"

An example of a meta-analysis

1. Literature review → 3,300 records
2. Look for similar research designs and topics:

   > "the 46 included studies (102 estimates) quantitatively analyze the effect of plausibly exogenous variation in world commodity prices on armed civil conflict by using a generalized difference-in-difference design with unit and time fixed effects"

3. Make studies comparable → standardize coefficients

An example of a meta-analysis

1. Literature review $\rightarrow$ 3,300 records
2. Look for similar research designs and topics:

   "the 46 included studies (102 estimates) quantitatively analyze the effect of plausibly exogenous variation in world commodity prices on armed civil conflict by using a generalized difference-in-difference design with unit and time fixed effects"
3. Make studies comparable $\rightarrow$ 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

	Characteristics		Predicted direction		
Commodity type	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

Meta-analysis findings

- **Pooling studies** across commodity types: no effect

Meta-analysis findings

- **Pooling studies** across commodity types: no effect
- But when **disaggregating by commodity type**: effects coherent with theoretical predictions

Meta-analysis findings

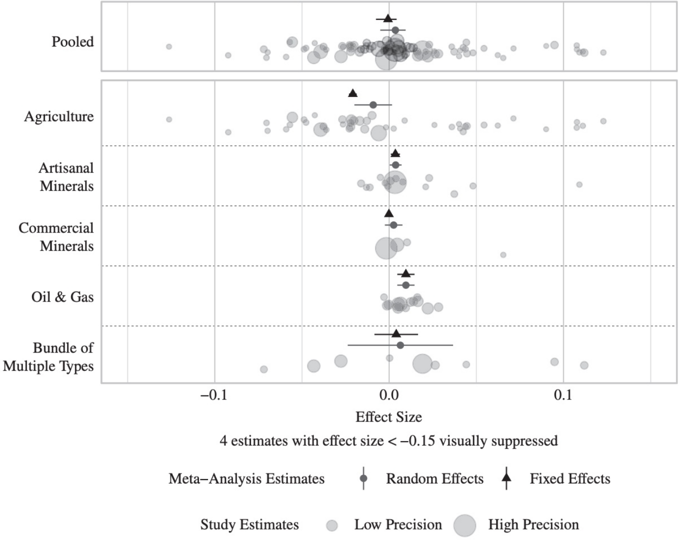
- **Pooling studies** across commodity types: no effect
- But when **disaggregating by commodity type**: effects coherent with theoretical predictions
  → price increases for **agricultural commodities** reduce the likelihood of armed conflict (labor-intensive commodities and opportunity cost)

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

	Africa	Asia	N. America	S. America	Multiple
Agriculture	N = 14	9	3	2	17
Artisanal Minerals	12	1		3	
Commercial Minerals	4				
Oil & Gas	4	2		3	4
Multiple	4				6

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

Construct validity

- This is about *mislabeling* the cause or outcome of a study
- We say we are analyzing the effect of X on Y, but are we really measuring that?

Construct validity

- This is about *mislabeling* the cause or outcome of a study
- We say we are analyzing the effect of X on Y, but are we really measuring that?

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

External validity

- This is about understanding the *conditions* under which X affects Y

  (it is very unlikely that the effect takes place under universal conditions)

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.

Roadmap

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.

Roadmap

Review

Generalizing results

Robustness and inference tests

Mechanism test and additional implications

Extra: Bartik instruments

Extra: Synthetic control methods

Questions

Causal methods again, in detail

Testing the mechanism

Additional implications of the theory

Roadmap

Bartik instruments

Roadmap

Review

Generalizing results

Robustness and inference tests

Mechanism test and additional implications

Extra: Bartik instruments

Extra: Synthetic control methods

Questions

Causal methods again, in detail

Synthetic control methods

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

Original study

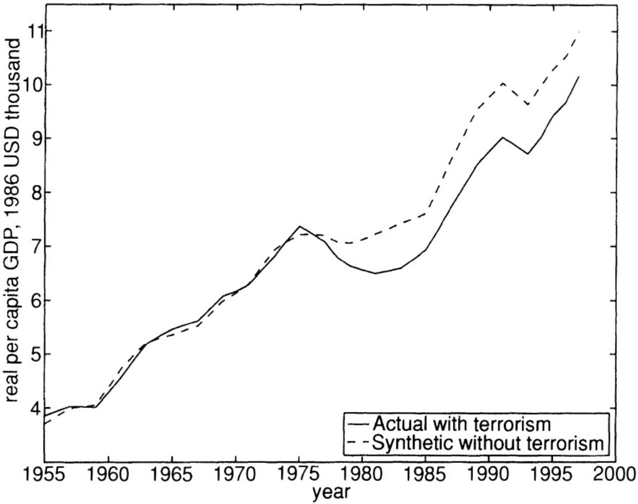


FIGURE 1. PER CAPITA GDP FOR THE BASQUE COUNTRY

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.*

Roadmap

Questions about final essay

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

Roadmap

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 → X → Y ← 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 ← Z → 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: jet 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 men + 12 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

Two approaches to matching

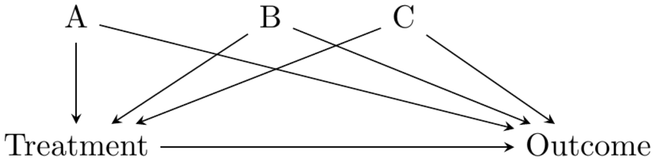
1. **Distance matching**
   - → We want to create a dataset where treatment and control observations have similar values (distance) in the confounding variables
   - → If, say, our confounding variable is income, we'll pick control observations that have a similar value on income to each of the treatment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

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

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



Two approaches to matching

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)

Two approaches to matching

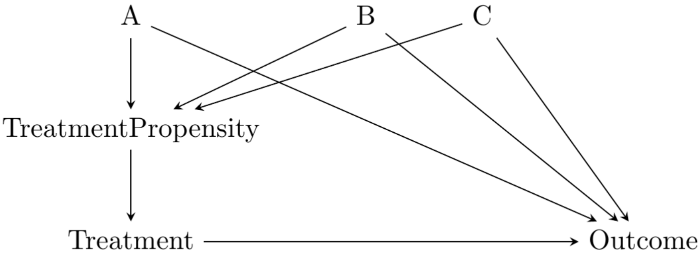
2. **Propensity score matching**
   - → We want to account for the differential likelihood in getting into treatment depending on the value of the confounding variables
   - → We estimate the probability of getting into treatment, usually by doing a regression where the outcome is the treatment and the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

Two approaches to matching

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)

Two approaches to matching

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)



Two approaches to matching: propensity score

```r
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

	Model 1	Model 2	Model 3
(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

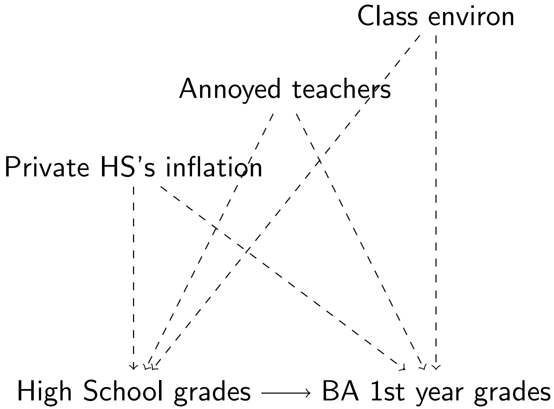
Fixed effects

- 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

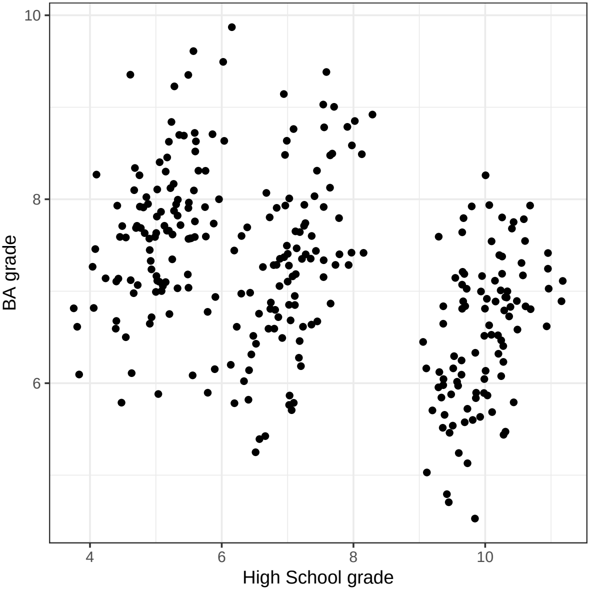
Fixed effects - when do we use them?

$$\begin{tikzcd} & \text{U} \arrow[dl, dashed] \arrow[dr, dashed] & \\ \text{Treatment} \arrow[rr] & & \text{Outcome} \end{tikzcd}$$

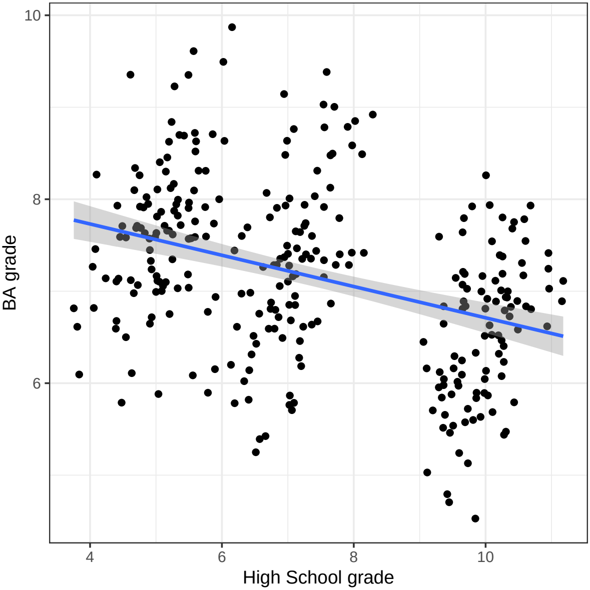
Fixed effects - when do we use them?



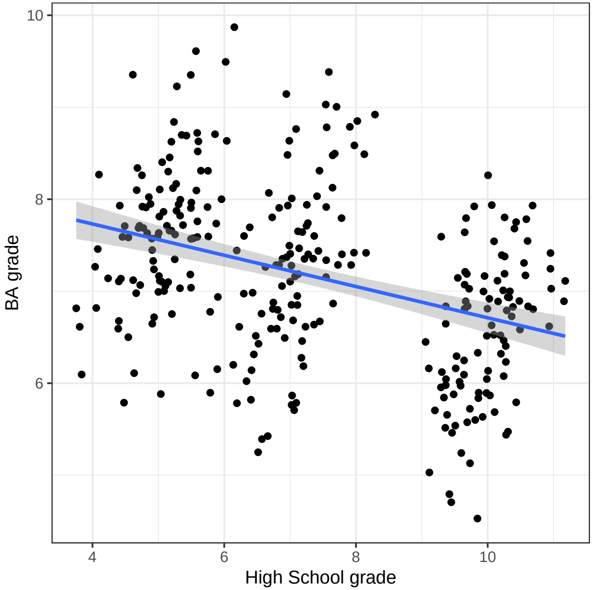
Fixed effects



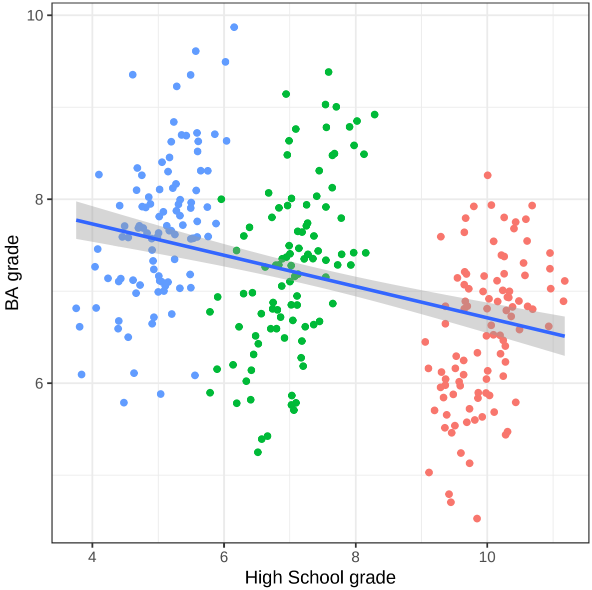
Fixed effects



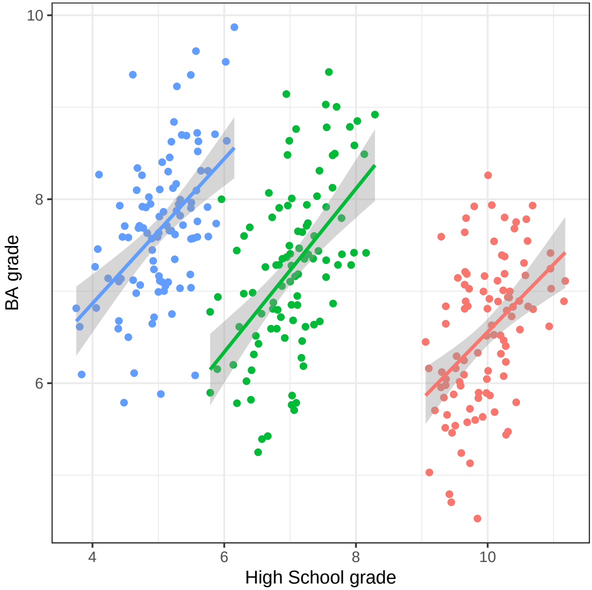
Fixed effects



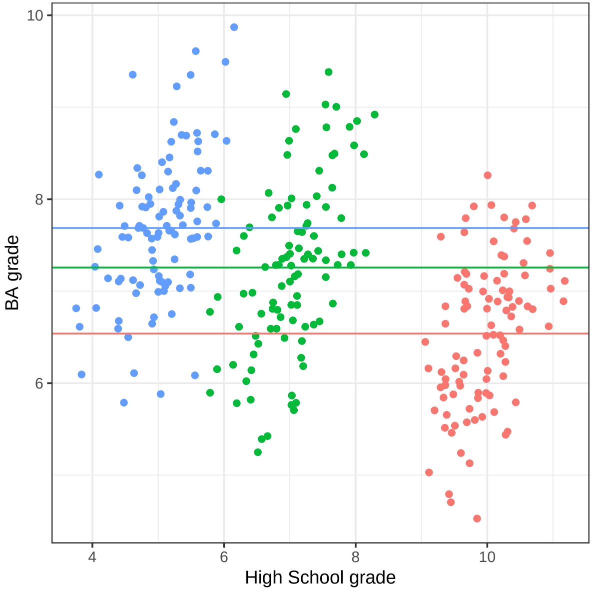
Fixed effects



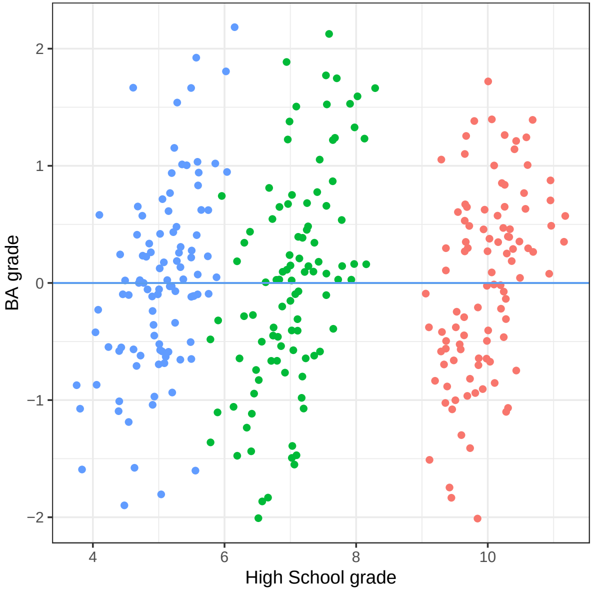
Fixed effects



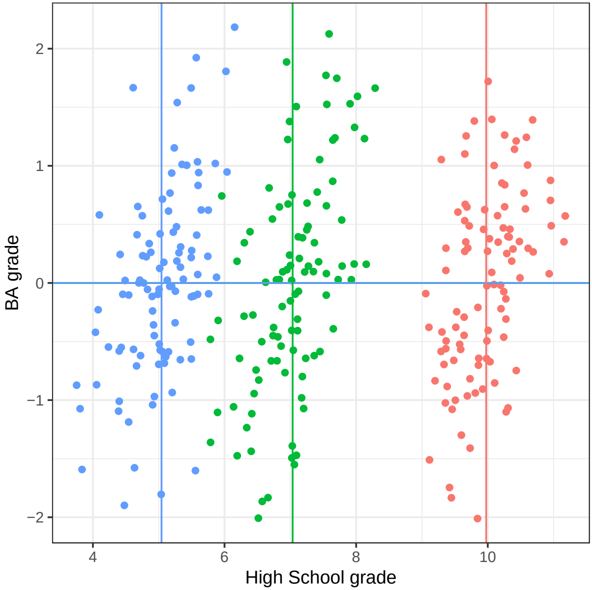
Fixed effects



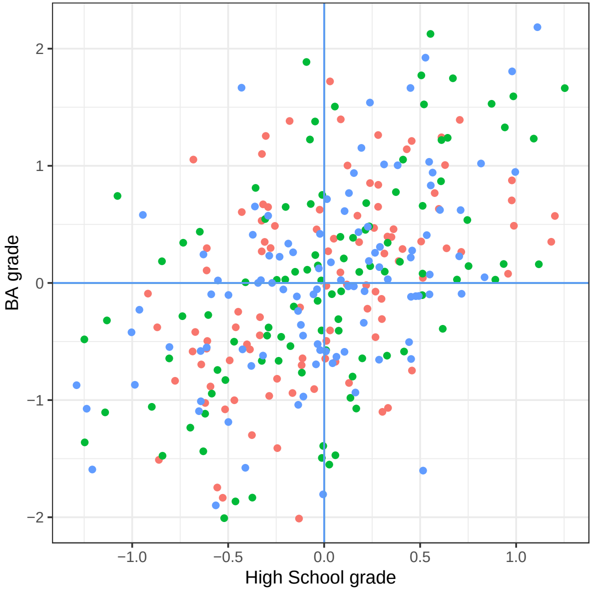
Fixed effects



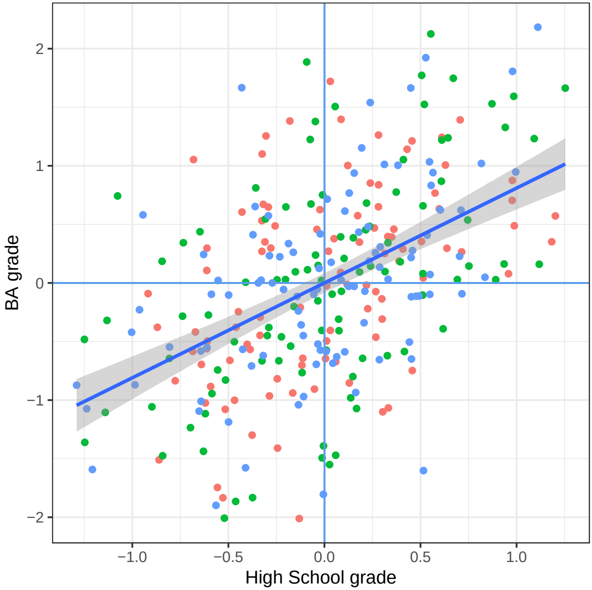
Fixed effects



Fixed effects



Fixed effects



Fixed effects and regression

```r
# 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 0.000000000000000283 ***
---
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: 0.0000000000000002826
```

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: < 0.00000000000000022
```

Difference-in-differences

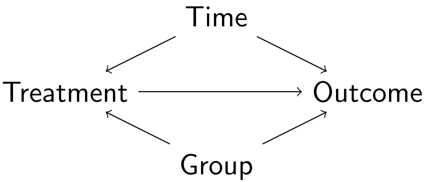
- Treatments usually occur at a particular moment in time, e.g.:
  - → Minimum wage increase
  - → Terrorist attack
  - → Influx of refugees
  - → ...
- In those cases, if we have before & after observations, we have something like this:

$$\begin{tikzcd}
& \text{Time} \arrow[dl] \arrow[dr] & \\
\text{Treatment} \arrow[rr] & & \text{Outcome}
\end{tikzcd}$$

- 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?
  - → ...

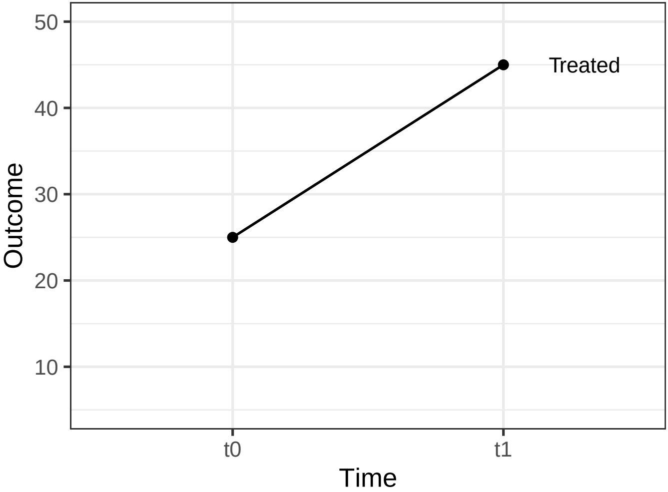


Difference-in-differences

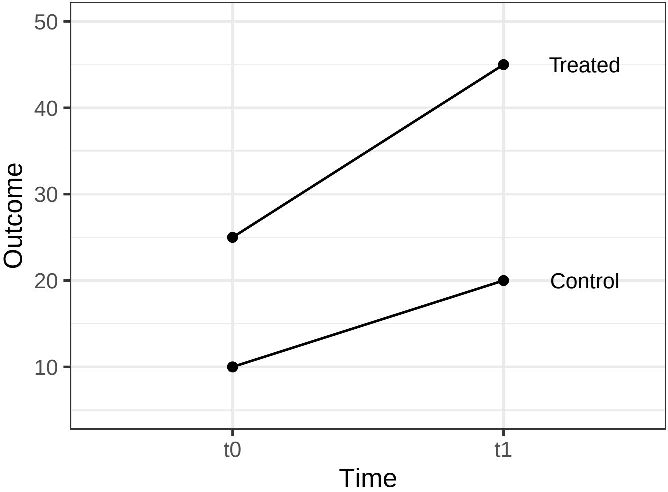
$$\begin{tikzcd}
& \text{Time} \arrow[dl] \arrow[dr] & \\
\text{Treatment} \arrow[rr] & & \text{Outcome} \\
& \text{Group} \arrow[ul] \arrow[ur] &
\end{tikzcd}$$

- 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 closign the other back door through `time`)

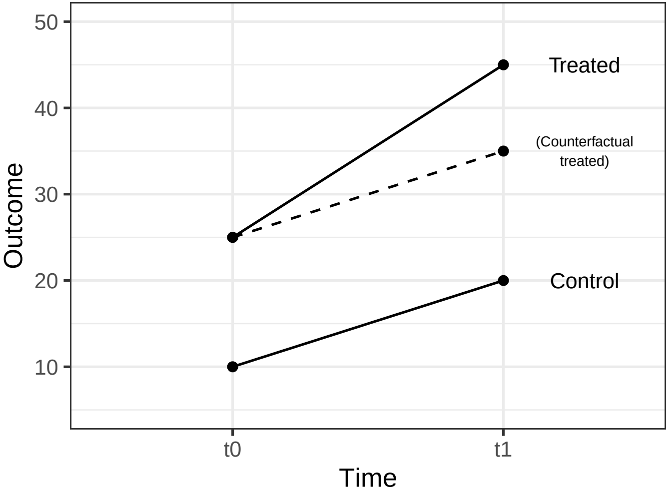
Difference-in-differences



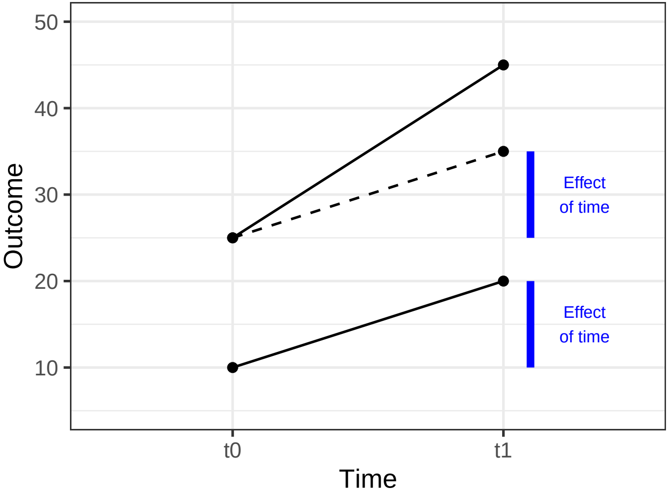
Difference-in-differences



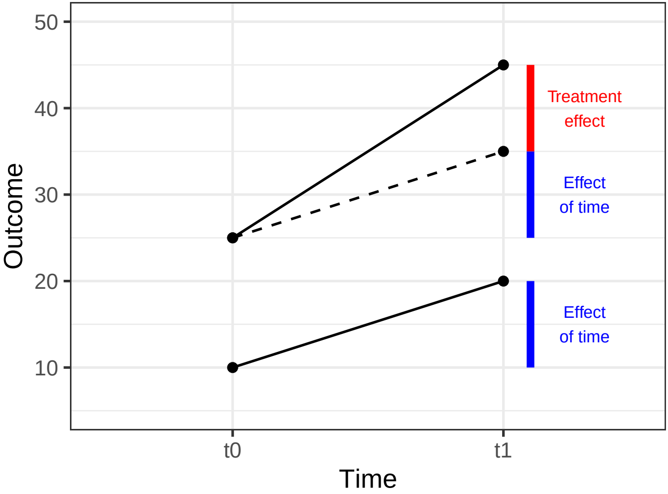
Difference-in-differences



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 1952, Southwark & Vauxhall Company still got it from downstream

Difference-in-differences: Cholera in London

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

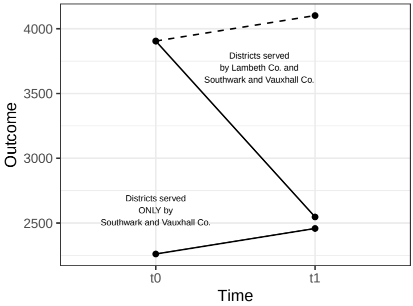


TABLE XII.

Sub-Districts.	Deaths from Cholera in 1849.	Deaths from Cholera in 1854.	Water Supply.
St. Saviour, Southwark	283	371	Southwark & Vauxhall Company only.
St. Olave	157	161	
St. John, Horsleydown	192	148	
St. James, Bermondsey	249	362	
St. Mary Magdalen	259	244	
Leather Market	226	237	
Rotherhithe*	352	282	
Wandsworth	97	59	
Battersea	111	171	
Putney	8	9	
Camberwell	235	240	
Peckham	92	174	
Christchurch, Southwark	256	113	Lambeth Company, and Southwark and Vauxhall Compy.
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 (2nd)	243	117	
Lambeth Church (1st)	215	49	
Lambeth Church (2nd)	544	193	
Kennington (1st)	187	303	
Kennington (2nd)	153	142	
Brixton	81	48	
Clapham	114	165	
St. George, Camberwell	176	132	
Norwood	2	10	Lambeth Company only.
Streatham	154	15	
Dulwich	1	—	
Sydenham	5	12	
First 12 sub-districts	2261	2458	Southwk. & Vauxhall.
Next 16 sub-districts	3905	2547	Both Companies.

Difference-in-differences

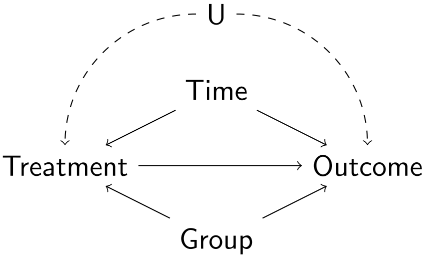
- You can estimate this effect just with group means
- But it is often easier to use regression, also because you can include controls:

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

- 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**

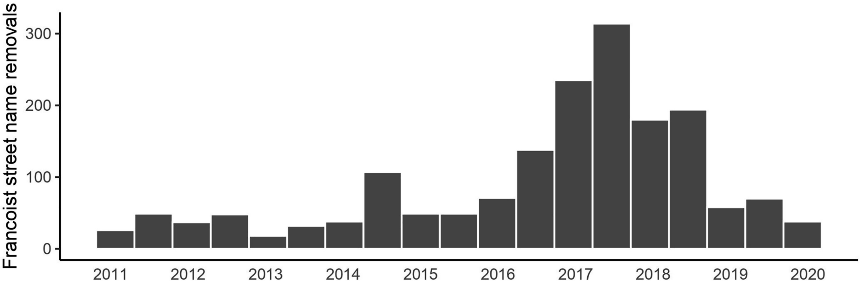


- One way to test this is checking if the **parallel trends assumption** holds (we need data further back in time)

DiD example

- What is the effect of symbolic TJ policies?
- journals.sagepub.com/doi/full/10.1177/20531680211058550

DiD example



Francoist street name removals
300
200
100
0
2011
2012
2013
2014
2015
2016
2017
2018
2019
2020

DiD example

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

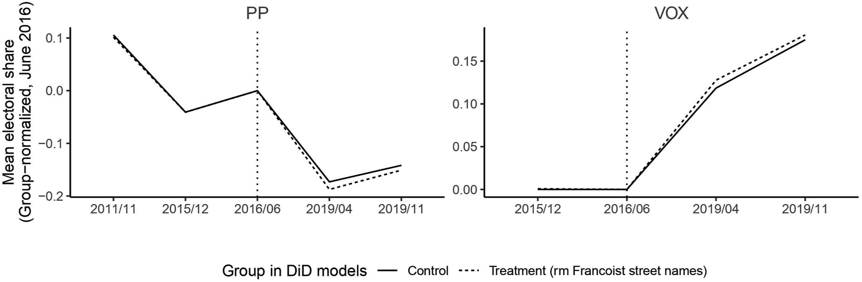
Note: Row percentages. Changes in 2016–2018 refer to the period between 30/06/2 016 and 31/12/2 018.

DiD example

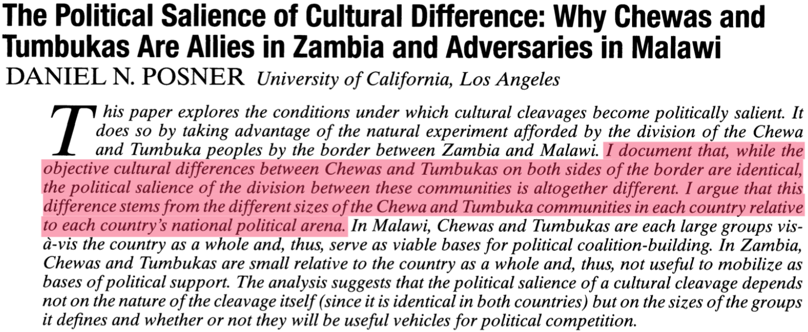
Table 2. Mean electoral share in sample.

	June 2016			April 2019			
Party	***Control***	***Treated***	**Δ**	***Control***	***Treated***	**Δ**	$\Delta_{2019}-\Delta_{2016}$
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


PP
VOX
Mean electoral share
(Group−normalized, June 2016)
0.1
0.0
−0.1
−0.2
2011/11
2015/12
2016/06
2019/04
2019/11
0.15
0.10
0.05
0.00
2015/12
2016/06
2019/04
2019/11
Group in DiD models
Control
Treatment (rm Francoist street names)

Regression discontinuity

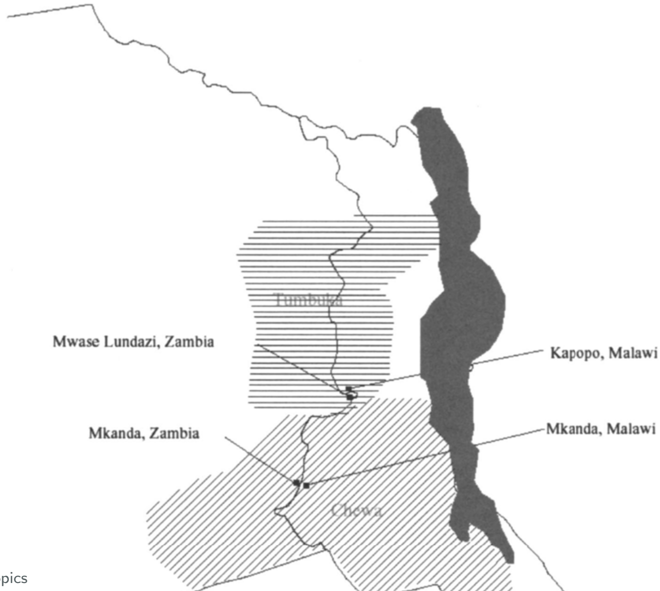

The Political Salience of Cultural Difference: Why Chewas and Tumbukas Are Allies in Zambia and Adversaries in Malawi

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

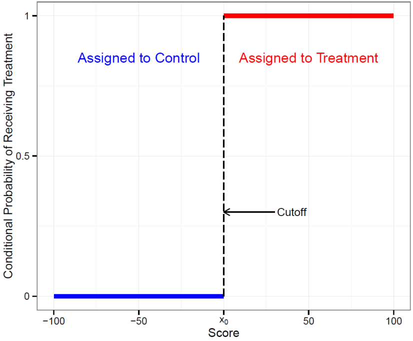


Regression discontinuity

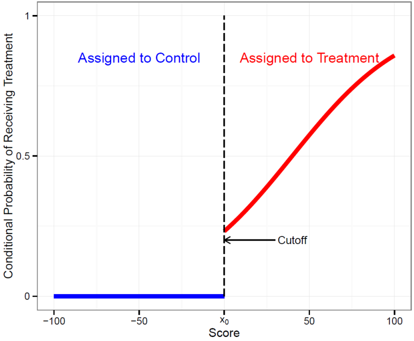
- RDD works well when assignment into treatment depends on a cutoff along a **running variable**
  - → Do incumbent politicians have an electoral advantage? (vote share)
  - → What is the effect of being drafted into the military? (birth year)
  - → Effect of national policies in ethnic identification 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

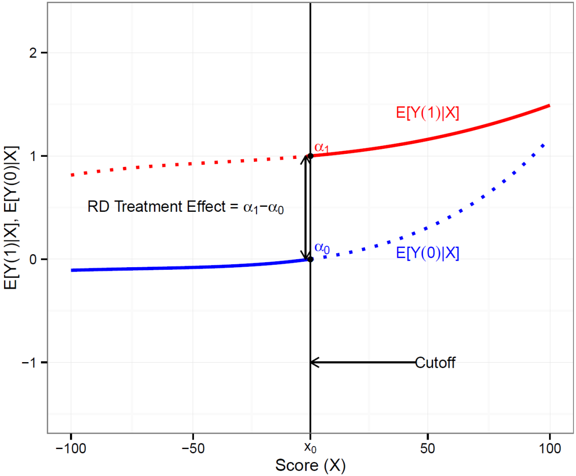


(b) Fuzzy RD (One-Sided)

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

RDD

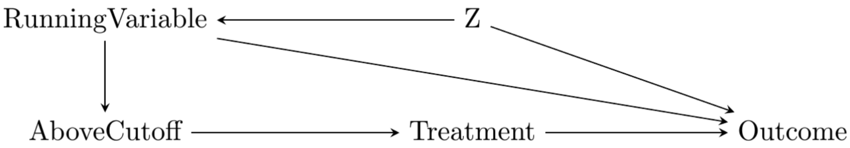
Figure 2: RD Treatment Effect in Sharp RD Design



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

RDD

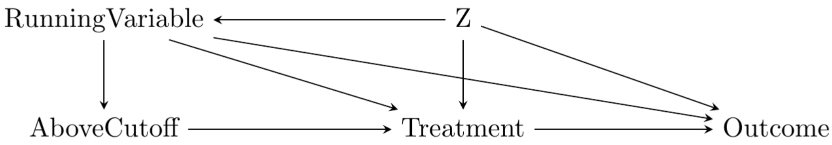
- Underlying assumption: other confounders also vary along the running variable, but are independent to the *jump*



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

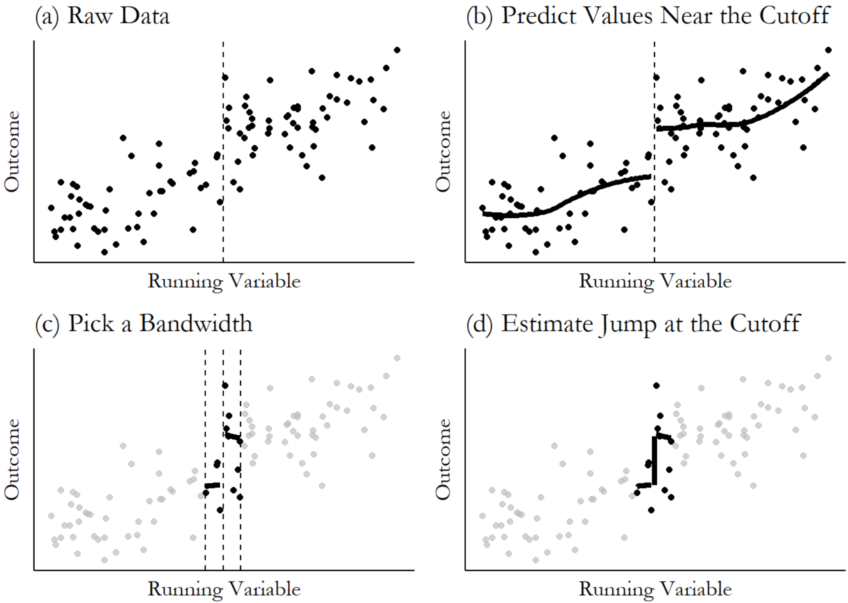
RDD

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



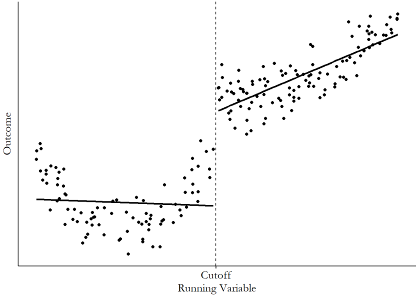
Huntington-Klein, *The Effect*, p.508

RDD implementation



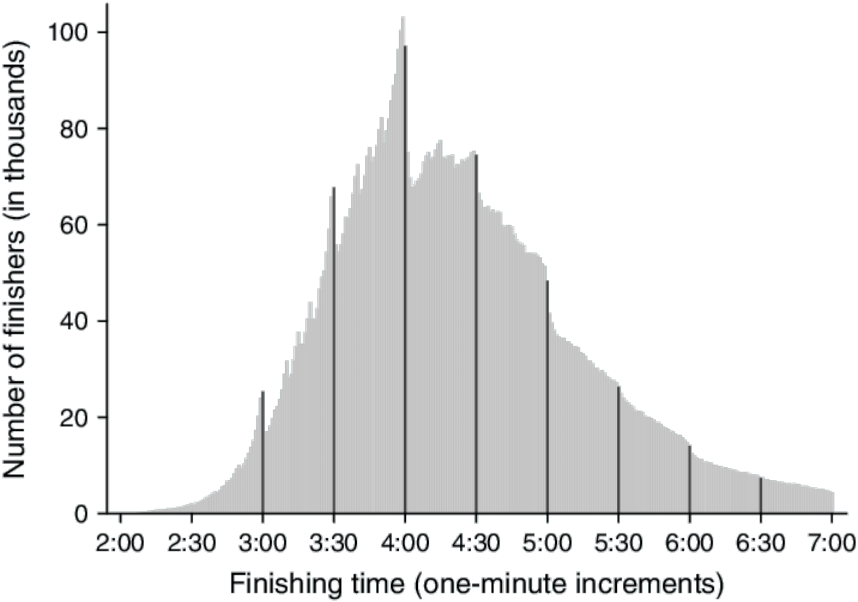
RDD and regression

$$
\begin{aligned}
Y = &\beta_0 + \beta_1 \textit{Distance} + \\
&\beta_2 \textit{Treated} + \\
&\beta_3(\textit{Treated} \times \textit{Distance}) + \beta^\top x_i
\end{aligned} \tag{2}
$$

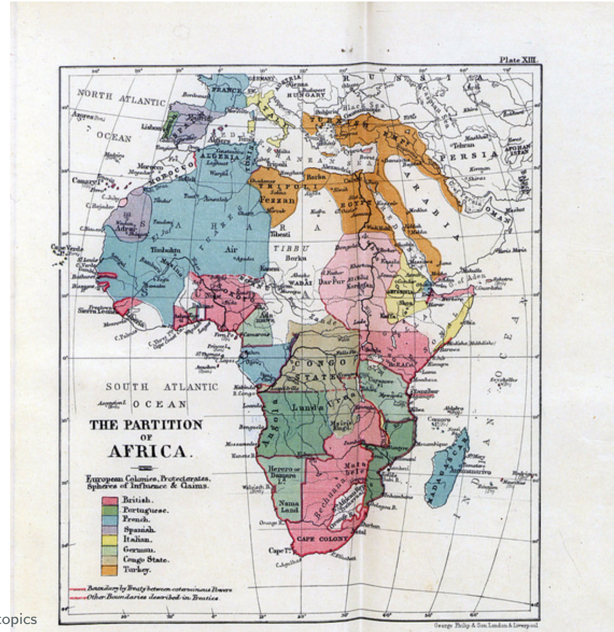


- But there's no need to use linear regression, other methods available as well

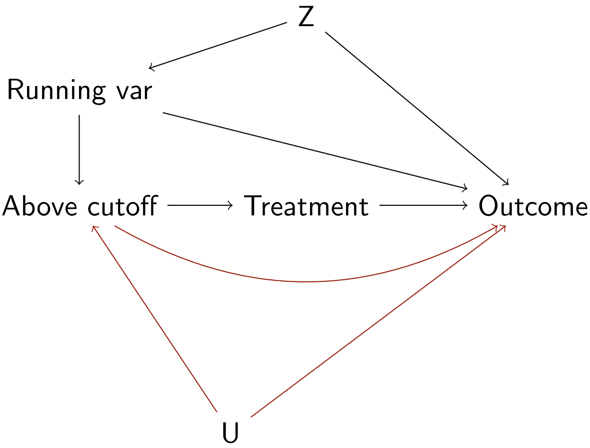
Threats to RDD: precise sorting



Threats to RDD: cutoff $\leftarrow$ 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

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

Instrumental variables

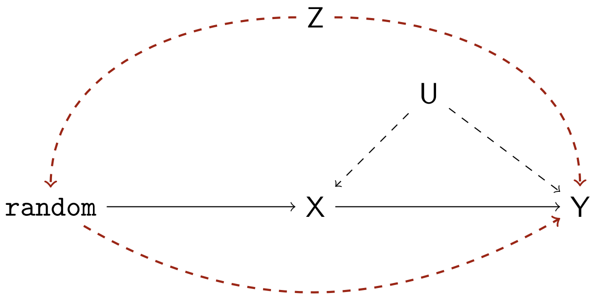
- Find an exogenous source of variation in the treatment variable
- Isolate that variation and use it to identify the causal effect

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

Edward Miguel

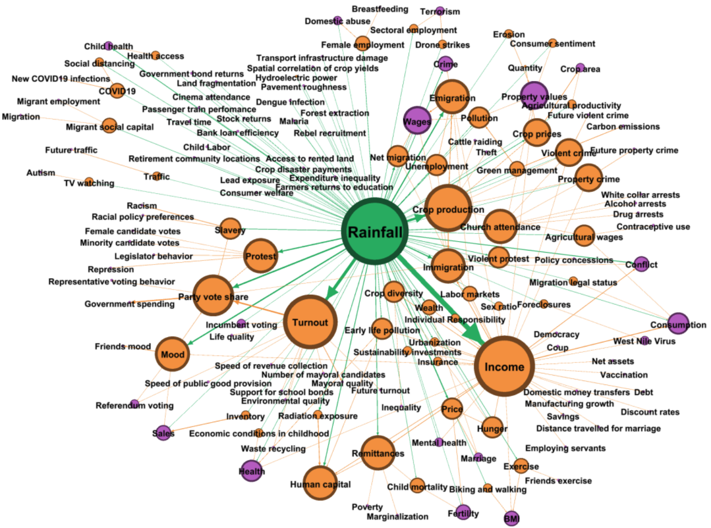
University of California, Berkeley and National Bureau of Economic Research

Shanker Satyanath and Ernest Sergenti

New York University

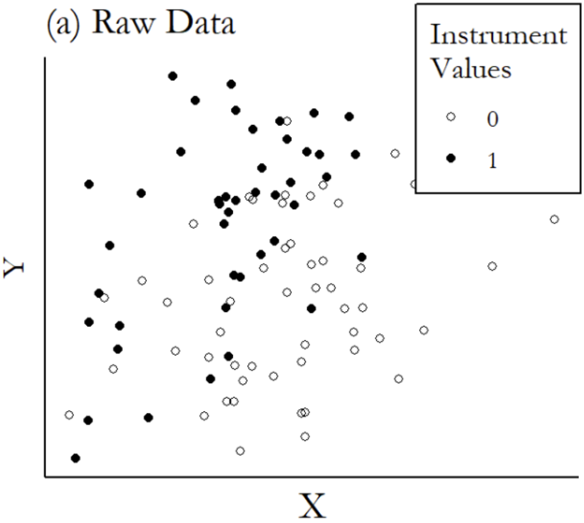
Estimating the impact of economic conditions on the likelihood of civil conflict is difficult because of endogeneity and omitted variable bias. We use rainfall variation as an instrumental variable for economic growth in 41 African countries during 1981–99. Growth is strongly negatively related to civil 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



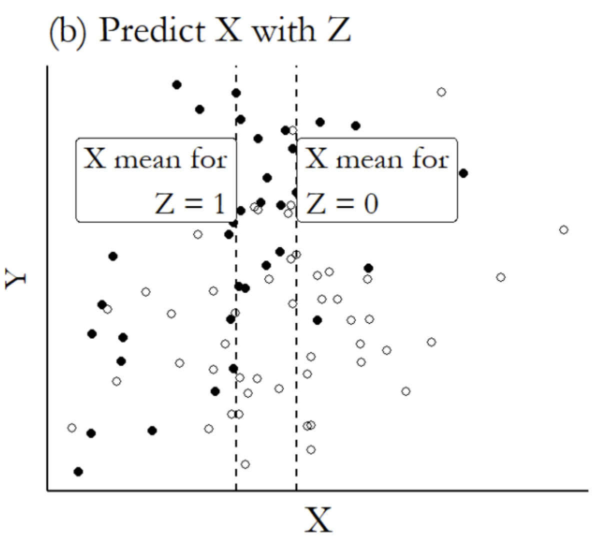
Jonathan Mellon (2022) Rain, Rain, Go Away: 192 Potential Exclusion-Restriction Violations for Studies Using Weather as an Instrumental Variable

How does IV work?



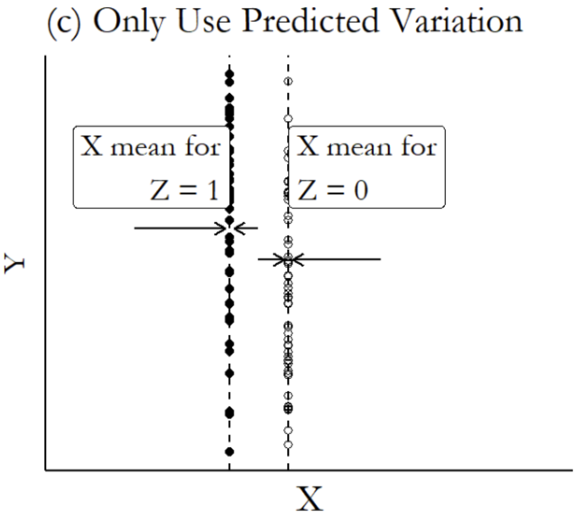
(Huntington-Klein, *The Effect*, p 472)

How does IV work?



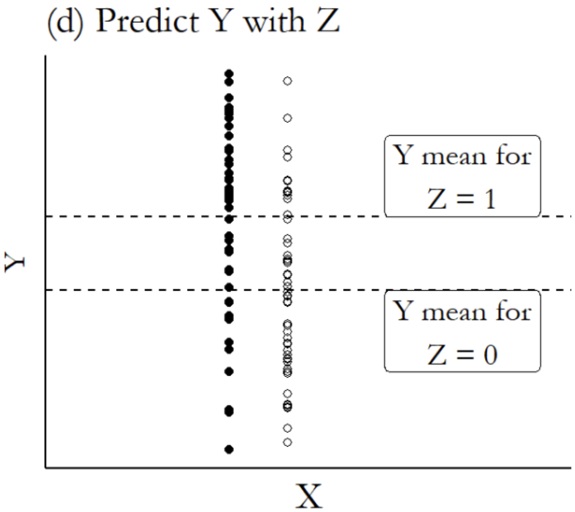
(Huntington-Klein, *The Effect*, p 472)

How does IV work?



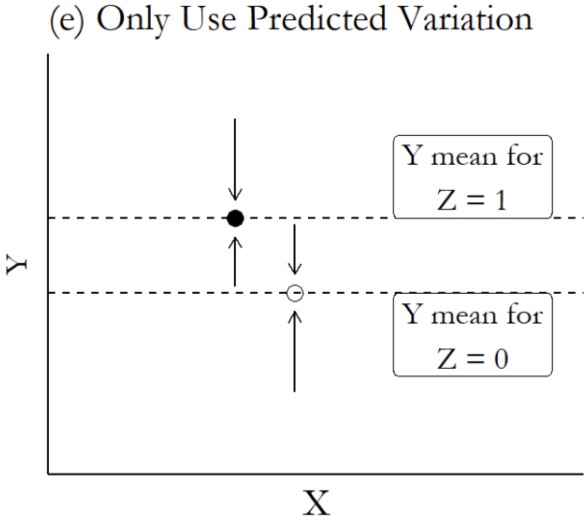
(Huntington-Klein, *The Effect*, p 472)

How does IV work?



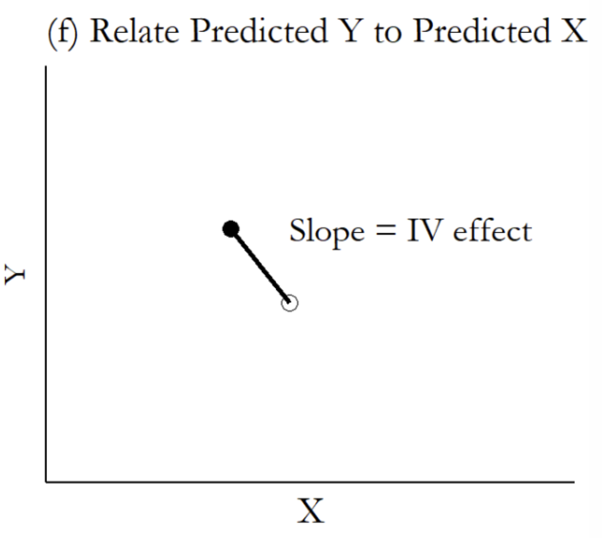
(Huntington-Klein, *The Effect*, p 472)

How does IV work?



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

How does IV work?



(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

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

DOI: 10.1177/0022002720963674
journals.sagepub.com/home/jcr
SAGE

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

Carl Müller-Crepon[1], Philipp Hunziker[2], and Lars-Erik Cederman[3]

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

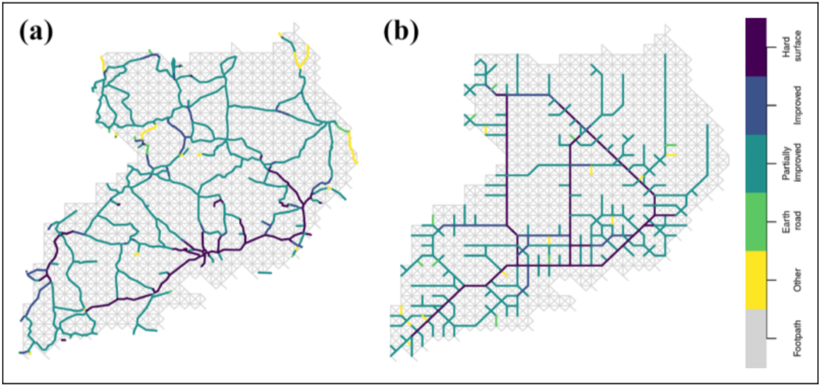


Figure 3. Observed and simulated road network in Uganda, 1966. (a) Observed network. (b) Simulated network.