Recitation, Week 13

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POL-850

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Outline

1. Why Quasi-Experiments?
2. Instrumental Variable (IV)
3. Regression Discontinuity (RD)
4. Differences-in-Differences (DID)

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  Causation for RCTs and correlation at best for observational studies
- Then why we don't always do RCTs?
  Experiments can be impractical/too costly/unethical

Why Quasi-Experiments?

- Motivation: we want to estimate a causal effect of X on Y (not just merely correlation) with observational data
- We can do quasi-experiments when subjects are assigned to treatment/control in a way that is "as if" random
- They are different from RCTs, because?

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- They are different from RCTs, because?
  No explicit, deliberate, random assignment by the researcher

Figure: Ogaden National Liberation Front during the Somali civil war

- Miguel, Satyanath, and Sergenti "Economic Shocks and Civil Conflict: An Instrumental Variables Approach"

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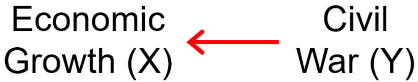
  1. Reverse causality



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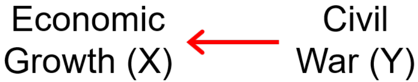
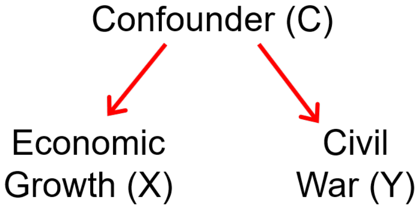


    2. Confounder (C) (that we can't observe! Hence, we can't control for it!)



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- Could you explain how rainfall causes the "as-if" random assignment of economic growth?

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Rainfall (Z) $\longrightarrow$ Economic Growth (X) $\longrightarrow$ Civil War (Y)

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1. **Valid first-stage**: Strong association between Z and X
2. **Independence**: Z is randomly assigned
3. **Exclusion restriction**: Z affects Y only through X

Instrumental Variable: Potential Problems

- Violation of 1 (Valid First-Stage): Weak instruments

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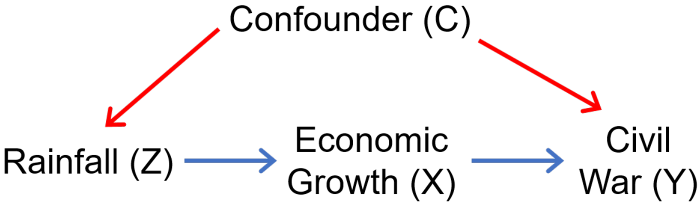
Instrumental Variable: Potential Problems

- Violation of 1 (Valid First-Stage): Weak instruments

- When there is weak association between Z and X
- Can easily check this out using F-test ($F > 10$)
- When do you think this would not hold?
- Countries in the sample are advanced economies

- Violation of 2 (Independence)



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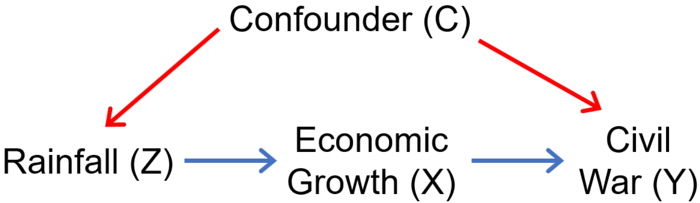

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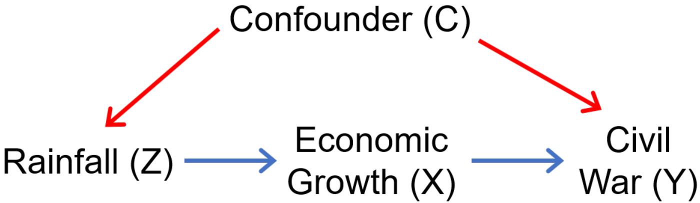
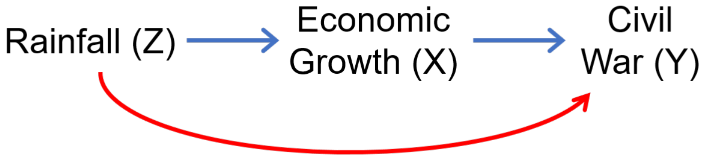


- When Z is not randomly assigned
- Can you think of a case where this assumption does not hold?
- Fossil fuel industry leads to more Z (due to more greenhouse gas emission) and less Y (more to lose compared to agrarian society)–though least likely

- Violation of 3 (Exclusion restriction)



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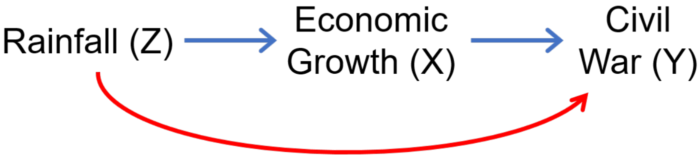

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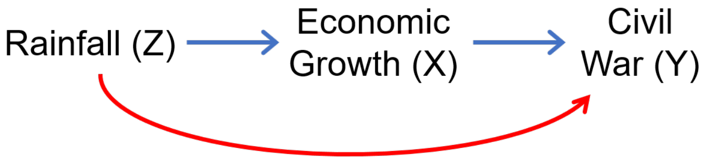

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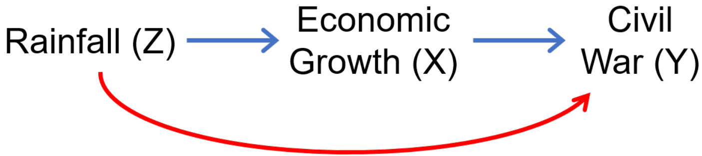

- When Z affects Y directly or through other channels than X
- Can you think of those channels?
- Z destroys road network; consequently less frequent conflict
- Z has impact on income inequality and that leads to more war

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- Therefore, what we estimate is not causal effect of X on Y
- Unfortunately, we cannot prove that independence and exclusion restriction assumptions hold
- Thus we need to argue with circumstantial evidence that it holds

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- She notices that the economy in districts that are downstream from irrigation dams are not really affected by rainfall
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- There are other channels through which rainfall affects civil conflicts

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- Baez and Camacho "Assessing the Long-term Effects of Conditional Cash Transfers on Human Capital: Evidence from Colombia"

Regression Discontinuity

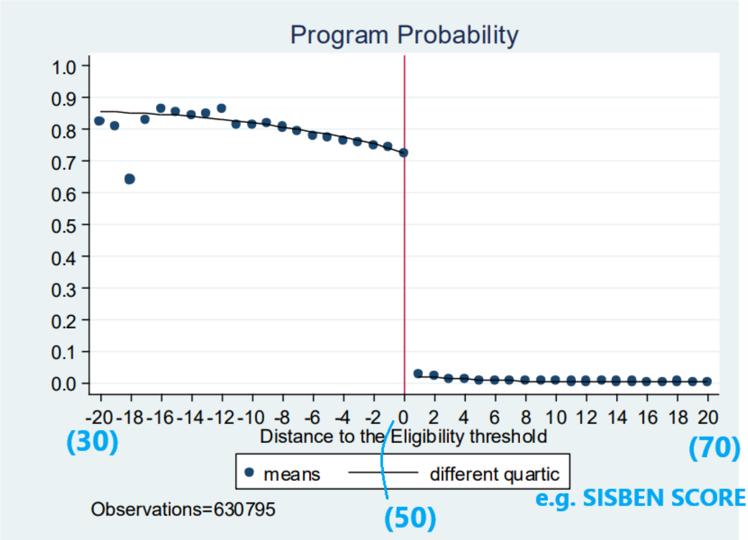
- Why "conditional"?: Within each municipality FA targets the poorest households based on a poverty index score called **SISBEN (running variable)** that is used to identify the most vulnerable population
- **SISBEN**: An index which runs from 0 to 100 which measures economic well-being of a household given the consumption of durable goods, human capital endowments, and current income

Sisbén
SISTEMA DE IDENTIFICACIÓN DE POTENCIALES
BENEFICIARIOS DE PROGRAMAS SOCIALES

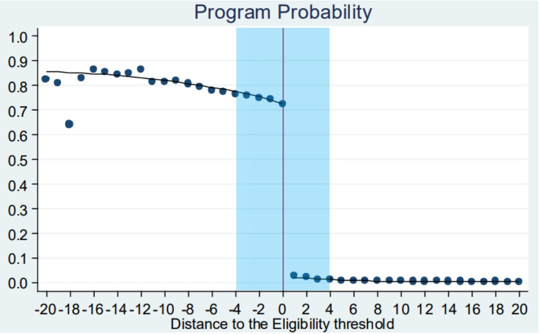
Regression Discontinuity

- "Cash transfer" (X): Local banks deliver the cash transfers to beneficiaries every two months
- "Human capital" (Y): Measured by high school completion rate

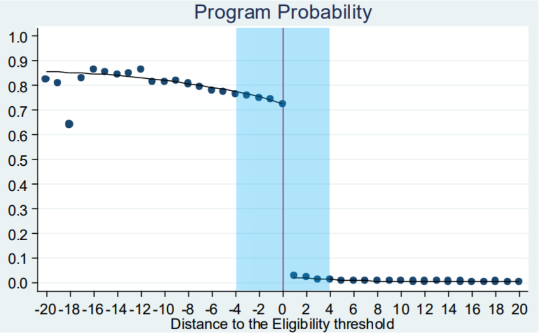
Figure 1. Effects of the SISBEN Score on Participation in the Program



Notes: The X axis presents the normalized distance of each child's proxy-means score to the cutoff that is used to classify households as SISBEN 1 and determines eligibility to the program. The Y axis presents the program participation probability.



- What's the basis of "as-if" random assignment of treatment? Suppose 50 SISBEN points was the **cutoff value**
  1. Households right below 50 (control)
  2. Households right above 50 (treatment)



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- Therefore, the two groups are equal in every aspect other than receiving treatment!

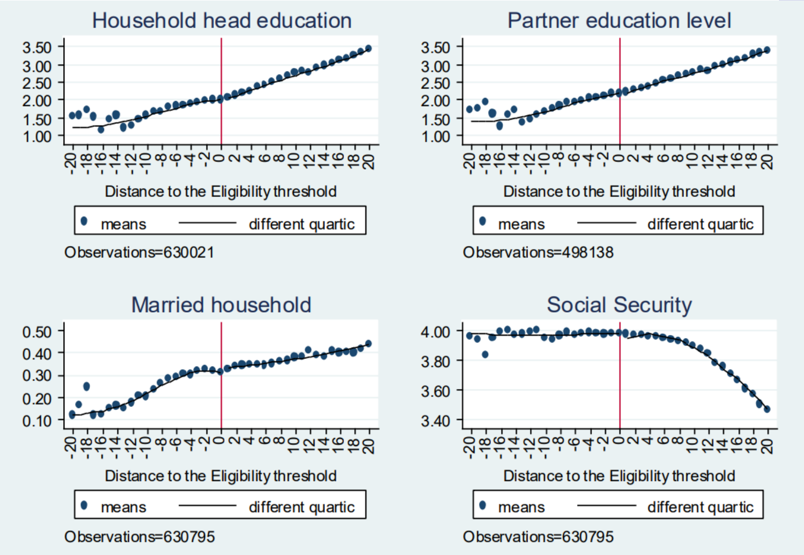
Household head education
Distance to the Eligibility threshold
means
different quartic
Observations=630021
Partner education level
Distance to the Eligibility threshold
means
different quartic
Observations=498138
Married household
Distance to the Eligibility threshold
means
different quartic
Observations=630795
Social Security
Distance to the Eligibility threshold
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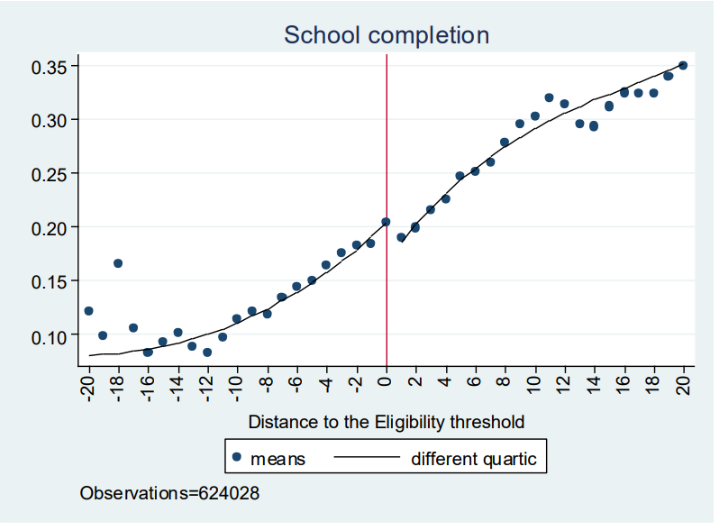
Figure 2. Impacts of FA on High School Completion (RD Analysis)



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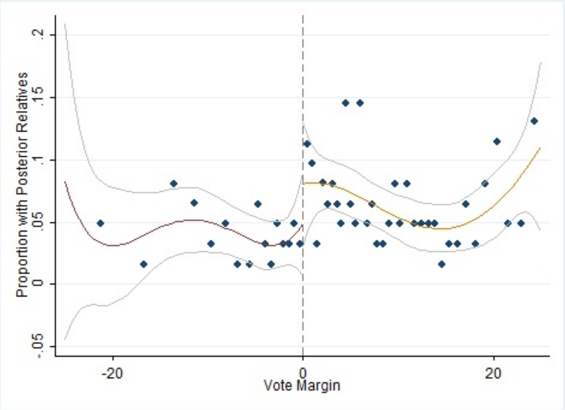
Proportion with Posterior Relatives
-.05
0
.05
.1
.15
.2
-20
0
20
Vote Margin

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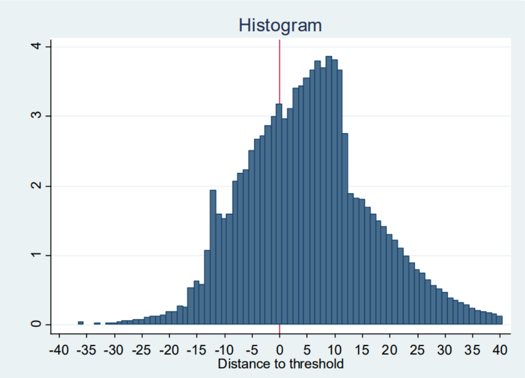
- "Sorting" can result from strategic behavior of participants (or administrators of the program)
- **What could be a potential cause of sorting in this FA example?**
- Households just above the threshold (of 50) may lobby with local authorities to have a score just below 50 and access the program
- If this happens to be the case, would our estimate lead to under- or overestimation of the effect?

Figure 4. Distribution of the SISBEN Score (total and by gender and area)



- **Would you say sorting happened given the histogram?**

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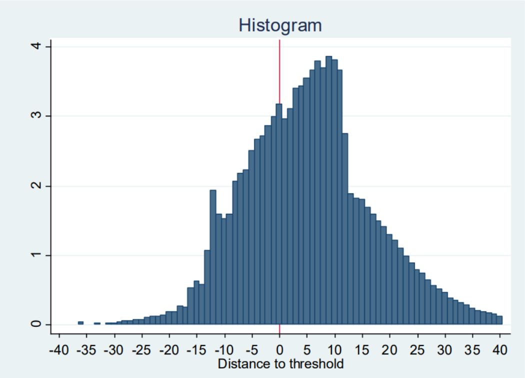

- **Would you say sorting happened given the histogram?**
- **What would have happened to this histogram if sorting happened?**

Differences-in-Differences

- Suppose we observe a positive change in the outcome variable of the treatment group
- Would we expect this positive change in the absence of treatment?

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- Suppose we observe a positive change in the outcome variable of the treatment group
- Would we expect this positive change in the absence of treatment? We need a counterfactual
- DD allows us to estimate the actual change in the outcome of the treatment group

NB: the following example is a hypothetical example!

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  (in other words, what would be a confounding factor?)

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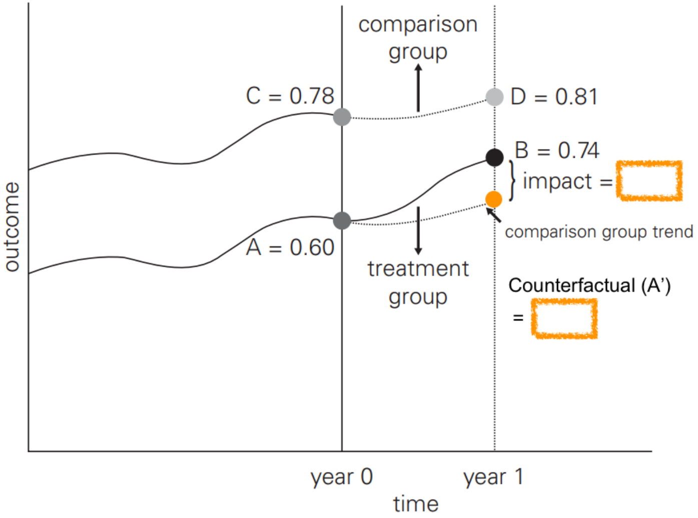
- Policy makers decide that vocational training schools in less privileged communities receive the treatment
- Y: Employment rate upon graduation
- Suppose you compare Y of those schools before and after the treatment and find a positive change
- Why is it hard to believe the naive estimate?
  (in other words, what would be a confounding factor?)
  Increase in the labor demand in those areas?, better economic conditions?
- Let's compare the treatment group with schools which did not receive the treatment (in more privileged communities)

Differences-in-Differences



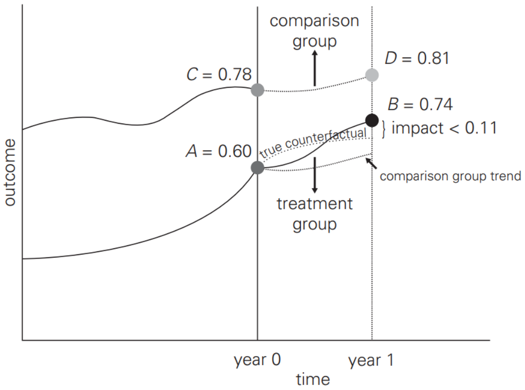
Differences-in-Differences

- How to estimate the effect of X on Y using DD?

	After	Before	Difference
Treatment enrolled	0.74	0.60	*0.14*
Comparison/ nonenrolled	0.81	0.78	*0.03*
Difference	*−0.07*	*−0.18*	*DD = 0.14 − 0.03 = 0.11*

- The effect of X on Y is an 11 percentage point increase!
- Note that we are assuming parallel trends
- What's this **assumption**? In the absence of the treatment, treated unit would have trended (changed over time) in the way the control unit did (in terms of the "slope").

Differences-in-Differences



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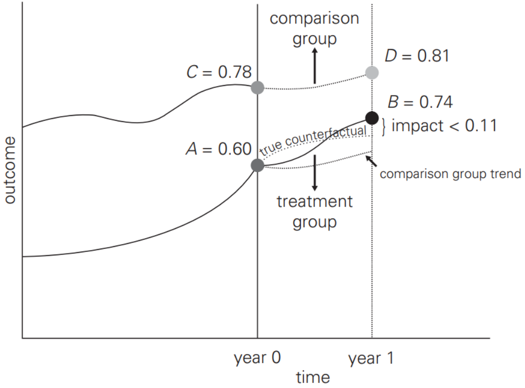

- Could you explain how the parallel trends assumption is violated in this case?
- Would this lead to an underestimation or overestimation of the true treatment effect?

Key takeaways

- Quasi-experiments (natural experiments) leverage "as-if random" assignment of treatment to claim a causal effect of X on Y
- This can be seen as a combination of the advantages of experiments and observational studies
- Yet, these methods in general depend on assumptions that cannot be proven
- This is why scholars provide some suggestive evidence—either qualitative or quantitative (validity checks)—on the validity of their argument on "as-if random" assignment of the treatment