Interpreting and Critiquing Causal Evidence

Day 3 - Assessing Causal Evidence

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Section 1

Introduction

Solving the Problem of Causal Inference

- We cannot!
- But we can try and minimize the risks
- Selecting units that provide appropriate counterfactuals, avoiding:
  - Omitted variable bias
  - Selection Bias
  - Reverse Causation

Solving the Problem of Causal Inference

- Experiments
  - Field Experiments
  - Lab Experiments
  - Survey Experiments
- Quasi-Experiments
  - Instrumental Variables
  - Regresssion Discontinuity
  - Difference-in-DIfferences

Causal Inference

Types of Research Design:

	Researcher controls the treatment assignment	Treatment assignment mechanism likely to create comparable potential outcomes ('Conditional Independence')
Controlled Experiments	Yes	Yes
Natural Experiments	No	Yes
Observable Studies	No	No

Section 2

Experiments

Field Experiments

- Field experiments provide confidence because treatment assignment is **controlled by the researcher**
- But still take place in real-world environments, so they identify (hopefully) meaningful treatment effects

Field Experiments

- Why does randomization help us achieve causal inference?

Field Experiments

- Why does randomization help us achieve causal inference?
  - A treatment assignment mechanism that **balances potential outcomes**
  - Every unit has **exactly the same** probability of treatment
  - If treatment is randomly distributed, **so are potential outcomes**
- Potential outcomes are - on average - the same for treated and control units
  - No omitted variable bias
  - No self-selection
  - No reverse causation

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  - But we can assess balance in *observable* covariates
  - What if some covariates are imbalanced?

Field Experiments

- Analysing field experiments
  - Comparison of means: t-test to test significance
  - Regression achieves the same thing
  - $Y_i \sim \alpha + \beta D_i + \epsilon_i$

Field Experiments

- Assumptions
  - **Compliance with randomization** - Treatment was truly random and accepted
  - **No Spillovers (SUTVA)** - Treatment of one unit doesn't affect potential outcomes of other units

Field Experiments

- Limitations of Field Experiments:

Field Experiments

- Limitations of Field Experiments:
  - Small sample sizes still prevent inference
  - Ethics
  - Logistics/Finance
  - Some treatments can't be manipulated (history)
  - Lack of control over treatment content and context - is it informative?
  - Long-term/scale effects/adaptation?

Field Experiments

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Field Experiments

- Limitations of Field Experiments: **Internal Validity**
  - No guarantee of actual balance
  - Unbiased but imprecise; variation still high if lots of other variables also affect Y
  - Hawthorne effect: participants adapt behaviour in experiments
  - Biased measurement if not double-blind
  - *Average* Treatment Effect can be skewed by Outliers
  - Complications of non-compliance, attrition

Field Experiments

- All these complications mean we need lots of assumptions and background knowledge
- Just as with other methodologies

Lab/Survey Experiments

- Why lab and survey experiments?

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  - Treatments we cannot administer in reality
  - Outcome measurements that are hard to take in reality
  - Random treatment assignment not permitted in reality

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- **Treatment Assignment**: Same as a Field Experiment
- **Treatment**: Not a manipulation of real world political or economic processes, but establishing controlled 'lab' conditions
  - The advantage: Control over context helps isolate mechanisms
  - The disadvantage: Can we generalize to the real world from this artificial context?

Section 3

Instrumental Variables

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- An 'instrument' is a variable which assigns *part of* treatment in an 'as-if' random way
  - Or at least in a way which is 'exogenous' - not related to omitted variables
  - Even if other variables **also** affect treatment

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- We can use the instrument to isolate 'as-if' random variation in treatment, and use that to estimate the effect of treatment on the outcome
- NOT the effect of the instrument on the outcome

Instrumental Variables

- Example Instruments:
  - Rainfall for conflict
  - Sex-composition for effect of third child
  - Distance from the coast for exposure to slave trade

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  - **Strong First Stage:** The Instrument must **affect** the treatment
  - We can test this with a simple regression: $Treatment \sim Instrument$
  - The instrument should be a significant predictor of treatment
  - Rule-of-thumb: $F - statistic > 10$

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     - **We cannot test or prove this assumption!**
- Theory and qualitative evidence needed to argue that the instrument is not correlated with any other factors affecting the outcome

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     - Estimate how the predicted values affect the outcome: $Y \sim \hat{D}$
     - Interpret the coefficient on $\hat{D}$

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  - Your coefficient is a causal estimate ONLY for units that were actually treated **because of the instrument**
  - They don't tell us about the causal effect for other units that never responded to the instrument
  - We call our causal effect estimate a 'Local Average Treatment Effect' (LATE)
  - 'Local' to the units whose treatment status actually changed

Section 4

Regression Discontinuities

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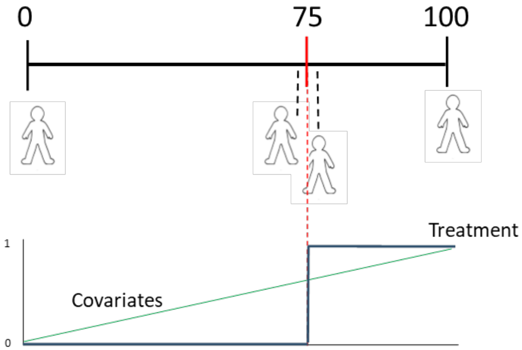
- Regression discontinuities take advantage of social rules that **treat similar people differently**
- Specifically, similar people with slightly different 'scores' are assigned to treatment/control

Regression Discontinuities



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  - For units just above and below the threshold:
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    - Their potential outcomes are (on average) almost the same
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  - So we can compare them directly

- Example thresholds:
  - Exam cutoffs
  - Age cutoffs
  - Policy eligibility rules
  - Close elections
  - Adminsitrative boundaries

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  - **Treatment,** D_i**:** Binary 0/1 depending on whether the running variable is above or below the threshold ($x_i >= \bar{x}$)
  - **Outcome,** Y_i**:** Any subsequent outcome you have measured

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  5. No compound treatments

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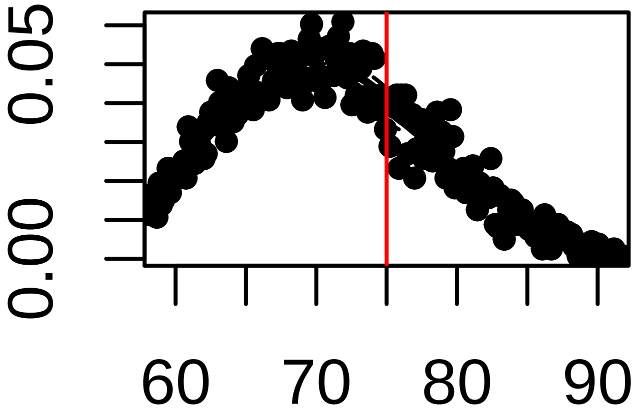
  - The running variable is hard to manipulate precisely
- We need qualitative evidence to support these assumptions

- We can check for sorting with a density test
- If units are bunched just above the threshold, this suggests manipulation



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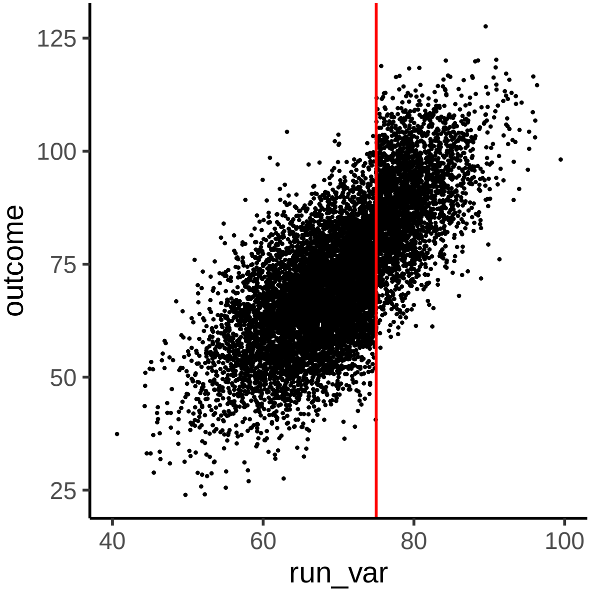
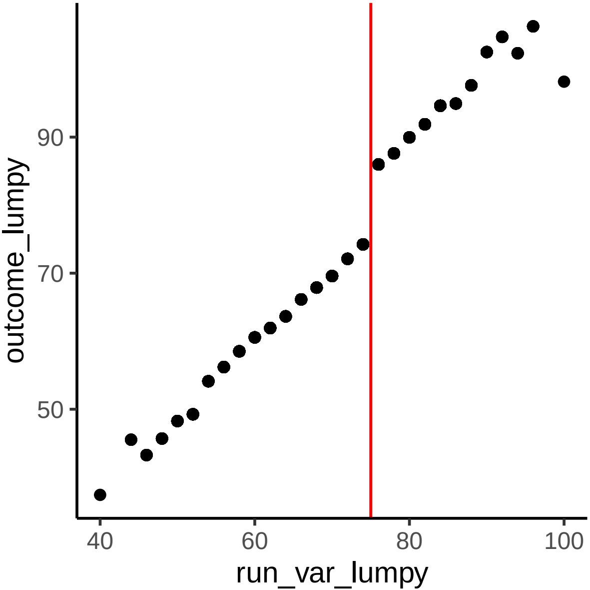
  - We may need to make the running variable non-linear

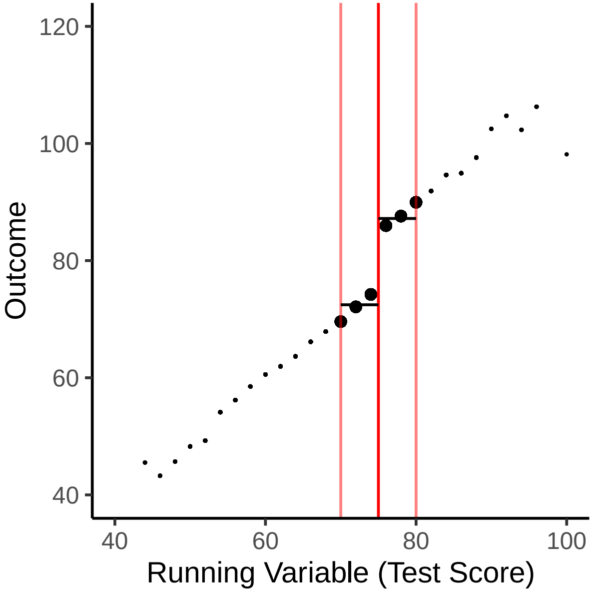
Raw Data



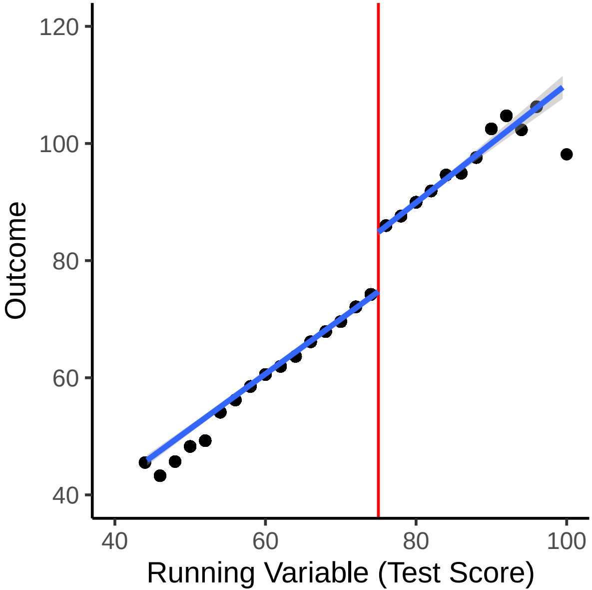
'Binned' Data



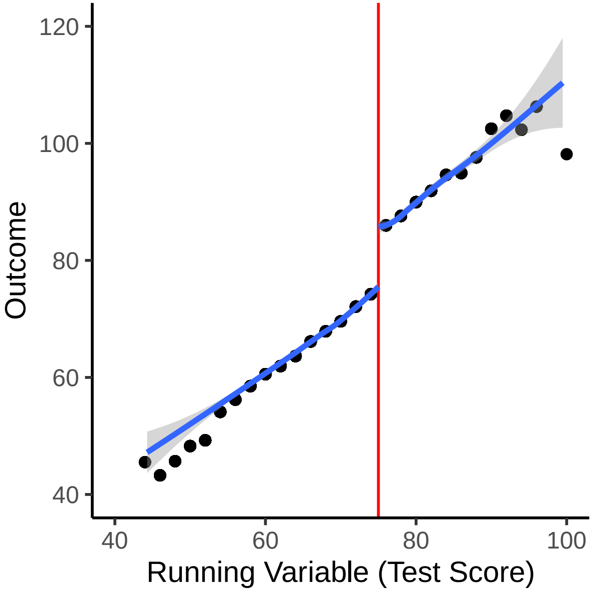
Difference-in-Means



Parametric Regression - Linear



Parametric Regression - Non-linear



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  - Units far from the threshold are very different for a reason, and causal effects are likely to be different

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- Particularly useful for understanding the effects of political power
  - **Running Variable:** Margin of victory
  - **Treatment:** Winning a close election
  - **Control:** Losing a close election
  - **Outcome:** Anything that happens later...

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  - Politicians (incumbents, the wealthy) can control whether they win, even when it's a tight race
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  - So potential outcomes are not balanced
  - But no other case (9 countries) has this problem

Section 5

Difference-in-Differences

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- Some treatments happen at a specific point in time
  - Can't we compare the same unit before and after treatment?
  - Surely this limits the number of omitted variables - Chile today is very similar to Chile tomorrow
- But No!
  - Other factors influencing the outcome might also have changed between our measurements (eg. any news event!)
  - Eg. a worldwide recession

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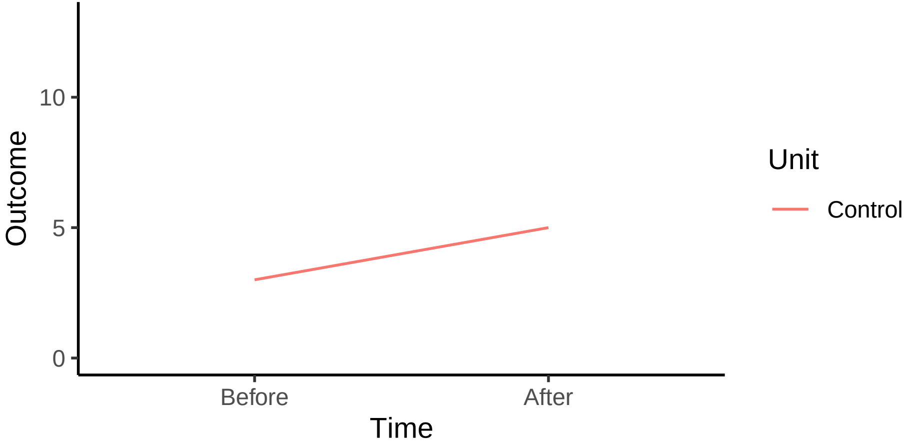
- But what if we combine the time-series and cross-section variation?
- We can keep lots of variables fixed if we compare the same unit before and after treatment
- We can measure how much other factors changed over time if we have units that were not exposed to treatment
- There is nothing 'random' here, but we are more easily able to limit the risk of omitted variables

Difference-in-Differences



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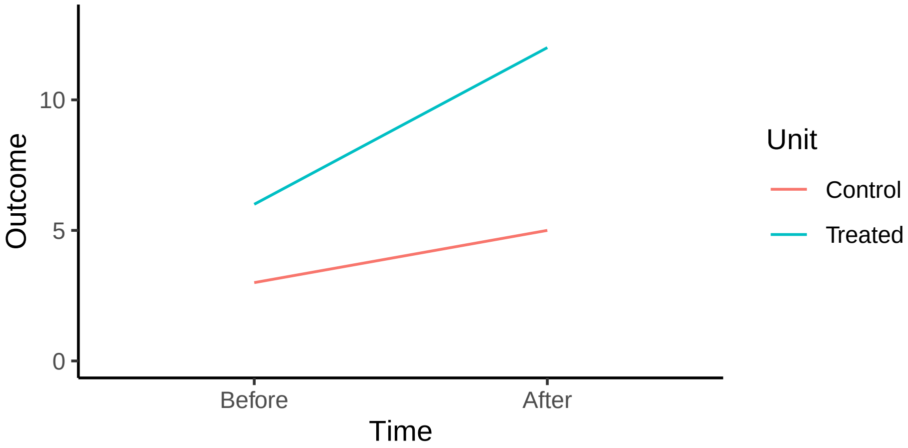

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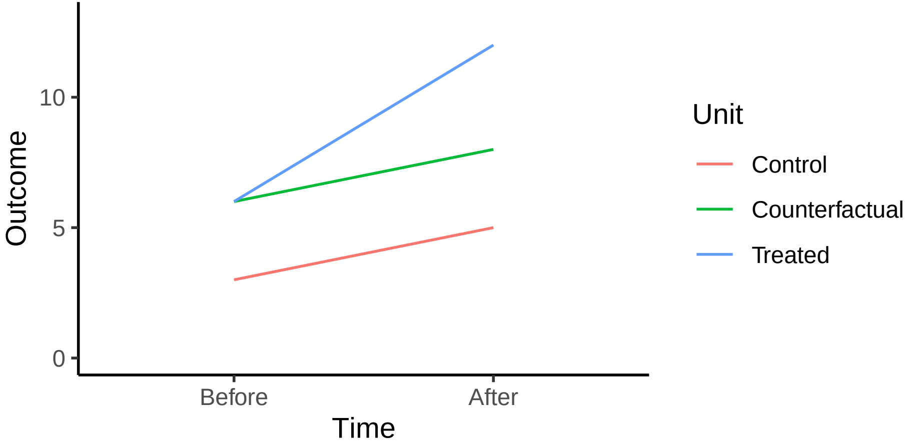

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  - Comparing before and after Brexit is biased - the world economy improved around the same time as Brexit (coincidentally)
  - But compare how European growth changes (-0.05%) and UK growth changed (-0.4%)
  - The net effect of Brexit is -0.35%

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    - Comparing with European growth rates is biased - UK growth is influenced by oil, different labour laws etc.
    - Comparing before and after Brexit is biased - the world economy improved around the same time as Brexit (coincidentally)
    - But compare how European growth changes (-0.05%) and UK growth changed (-0.4%)
    - The net effect of Brexit is -0.35%
    - That's two differences
        - **Difference 1:** Between before and after (over time)
        - **Difference 2:** Between treated and control units

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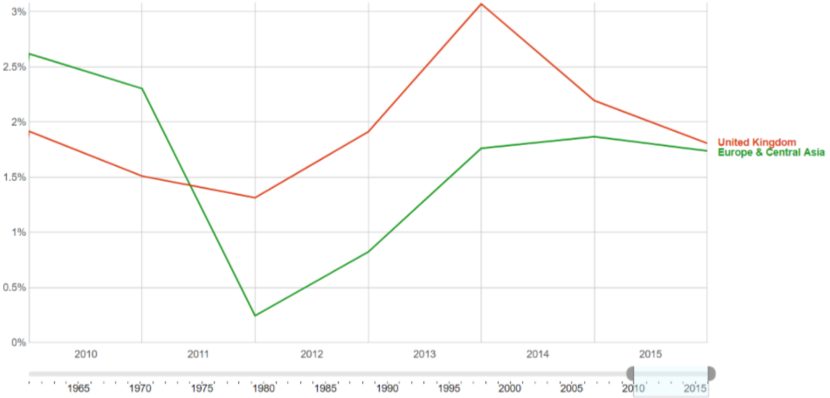

Difference-in-Differences

- We're now comparing *changes* (differences), not *levels* of the outcome
  - Most omitted variables affect 'levels', so this makes our counterfactuals more plausible
    - Eg. different laws affect growth rates, not the change in growth over time
  - And crucially, we can remove omitted variables even for *unobserved* confounders

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- Factors that affect the **trend** in the outcome *differentially* in treated and control units
- Eg. Even before Brexit, the UK had falling growth while growth in the eurozone was improving

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- The difference-in-differences estimate is just the *interaction* of time and treatment status

$$Y_{it} = \alpha + \gamma D_i + \delta T_t + \beta D_i * T_t$$

- β is our causal effect estimate

Difference-in-Differences

- Assumptions Required:
  1. **No Spillovers** (SUTVA)
  2. **No time-varying confounders** (Parallel trends)
  3. **Well-defined treatment** (many things changed at the same time!)
     - Eg. The UK also announced new rules to regulate the banking sector on the same day as Brexit
  4. **Groups are stable** (eg. no migration due to treatment)

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- Eg. Participants who join a training program usually experience income falls in the previous few months

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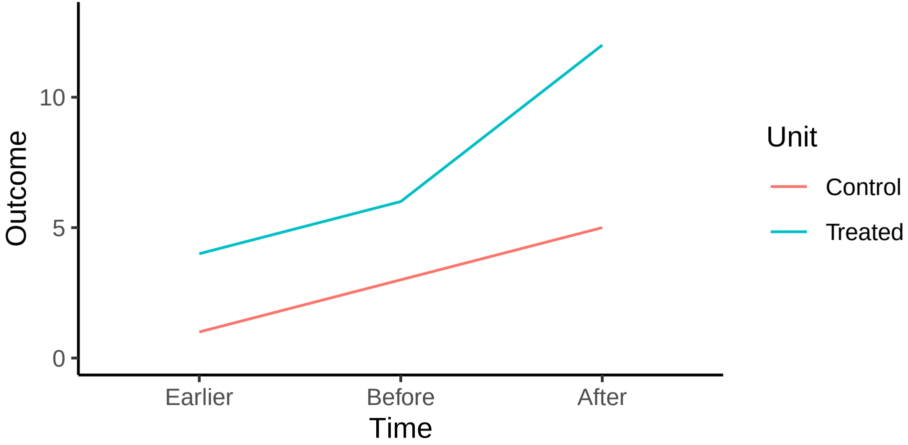
  - So any difference in trend is only due to treatment
- One test of this is to check if **pre-treatment trends are parallel**
- Then our counterfactual makes sense

Difference-in-Differences



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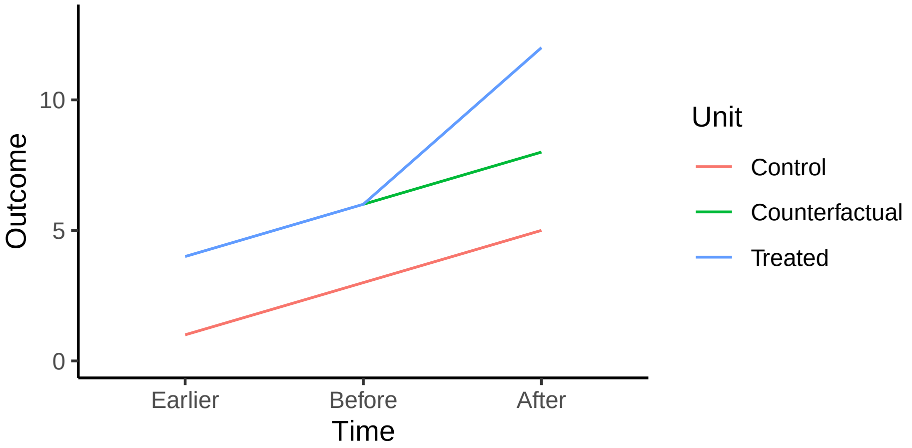

Assumptions

Causal Methodology Assumptions

Research Design	**Assumptions required for valid causal inference**
Field/Lab/Survey Experiments	No spillovers, Randomization implemented correctly, Randomization complied with, No Hawthorne Effects
Instrumental Variables	No Spillovers, First stage predicts treatment, Exclusion restriction
Regression Discontinuities	No Spillovers, Continuity (balance) of covariates, No precise manipulation, No strategic threshold, No compounding discontinuities
Difference-in-Differences	No Spillovers, No time-varying confounders (parallel trends), Well-defined treatment, Stable groups