Making Causal Critiques
Day 3 - Assessing Causal Evidence

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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

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

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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$$= E(Y_1 - Y_0) \tag{6}$$

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  - We cannot measure potential outcomes
  - 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

- Limitations of Field Experiments: **Internal Validity**

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?

Lab/Survey Experiments

- ▸ Why lab and survey experiments?
    - ▸ 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?

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- ► What can we do when the treatment assignment mechanism is not random?
- ► An 'instrument' is a variable which assigns 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

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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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        - Save the predicted values from this regression: $\hat{D} = D \sim Instrument$
        - Estimate how the predicted values affect the outcome: $Y \sim \hat{D}$
        - Interpret the coefficient on $\hat{D}$

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- ▶ IV Interpretation:
  - ▶ 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

Regression Discontinuities

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- ▶ As always, we need some 'as-if' random variation in assignment to treatment to get plausible counterfactuals
- ▶ 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
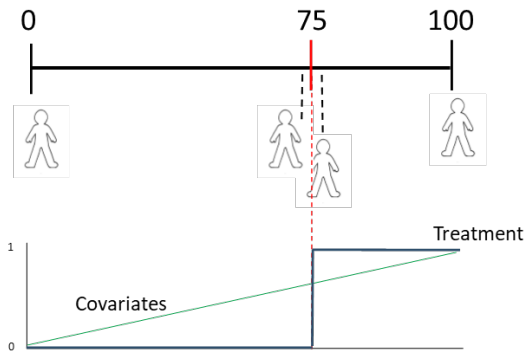
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   ▶ So we can compare them directly

- ▶ 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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  - ▸ Units are not aware of the threshold
  - ▸ The threshold is decided after units make choices
  - ▸ 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
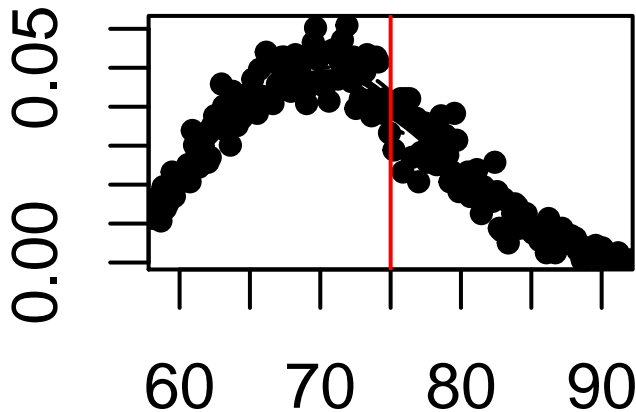
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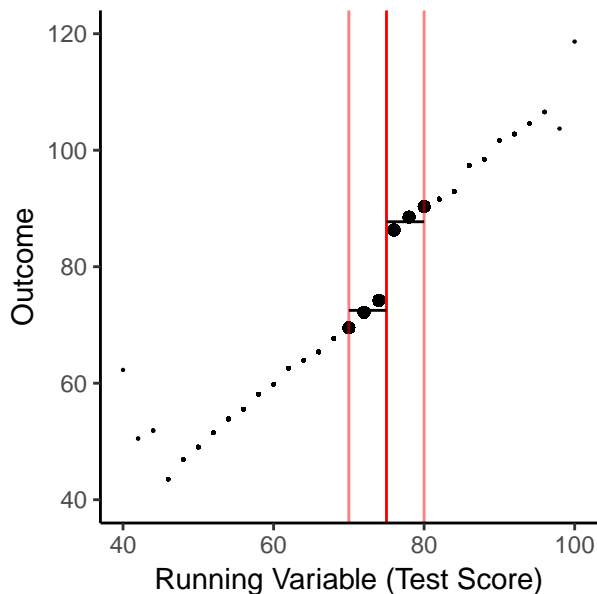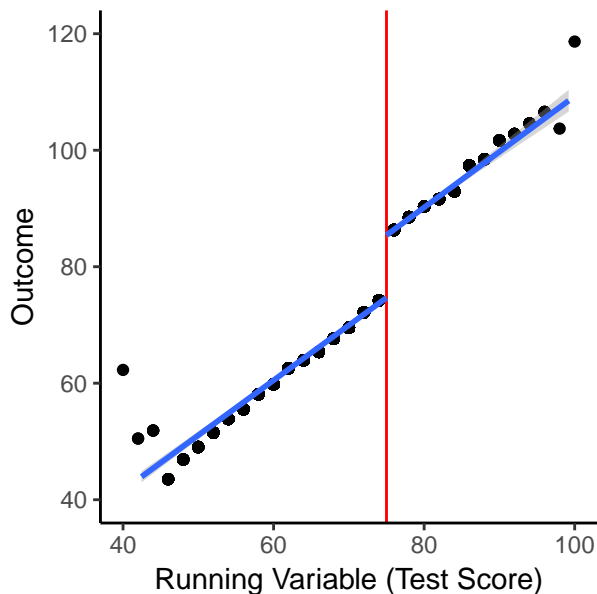 ▸ We may need to make the running variable non-linear
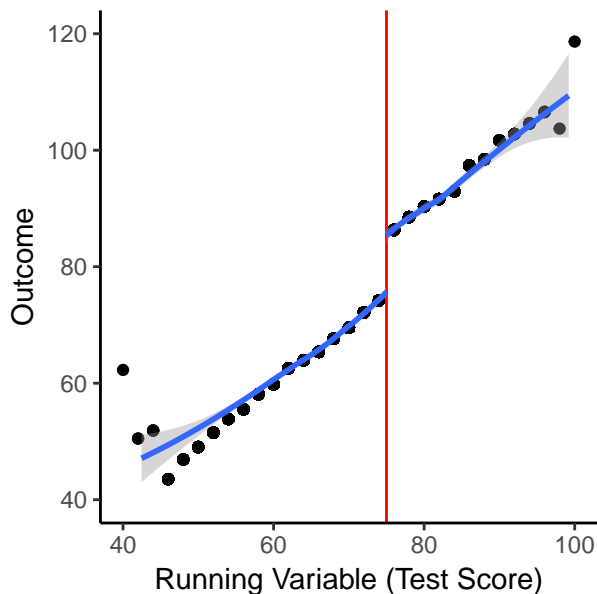
Raw Data
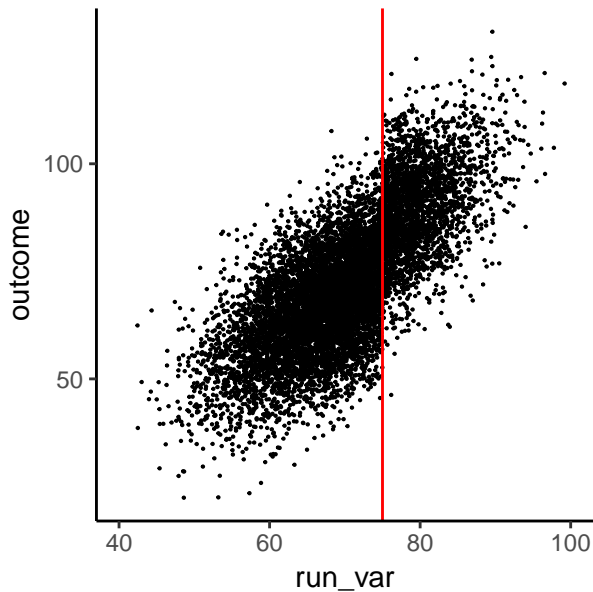
'Binned' Data
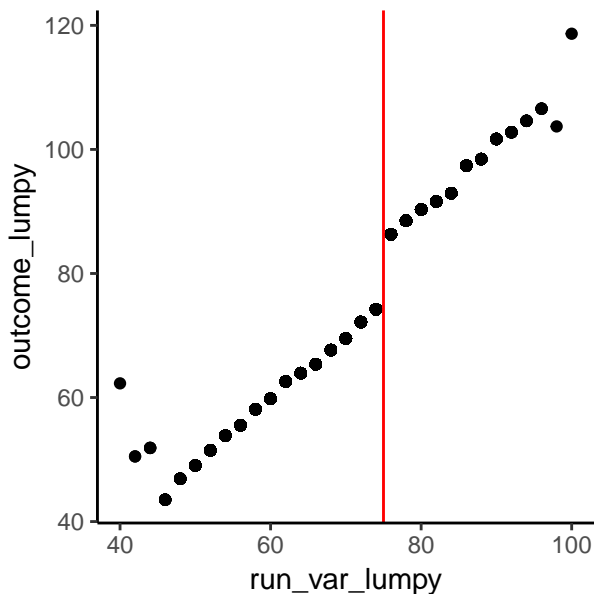
Difference-in-Means

Parametric Regression - Linear

Parametric Regression - Non-linear

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- ▸ Why does RD estimate a **Local** Average Treatment Effect?
  - ▸ Treatment assignment is only random at the threshold
  - ▸ Our estimates only apply to units close to the threshold
  - ▸ 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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  ▶ They have extremely detailed information to predict vote results
  ▶ So potential outcomes are not balanced
  ▶ But no other case (9 countries) has this problem

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

Difference-in-Differences

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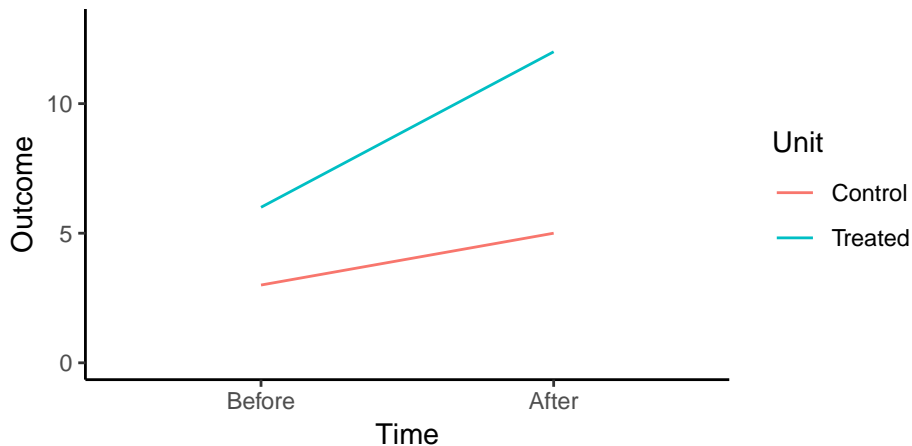
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- ▶ But what if we combine the time-series and cross-section variation?
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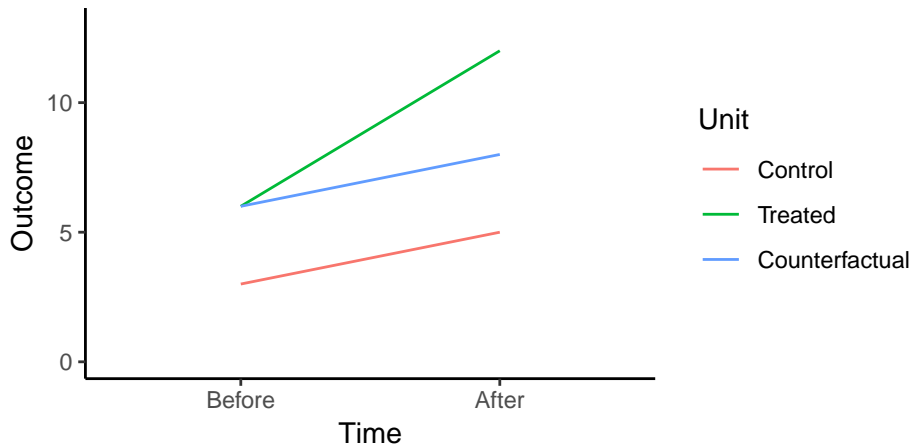- ▸ There is nothing 'random' here, but we are more easily able to limit the risk of omitted variables

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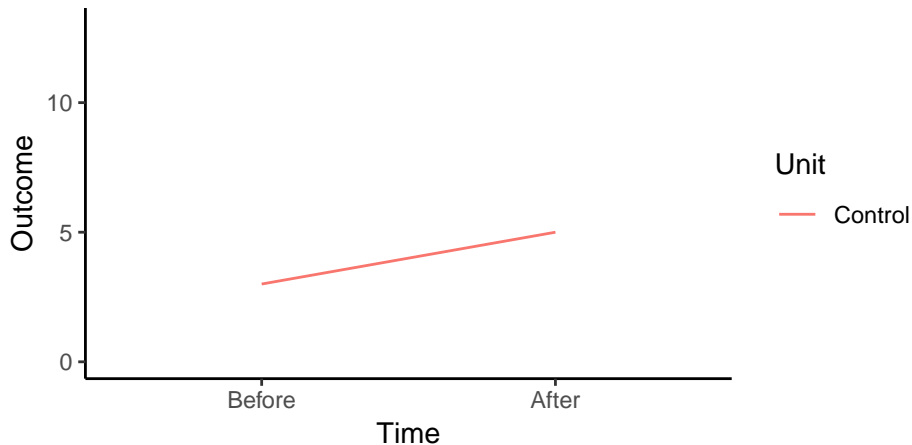
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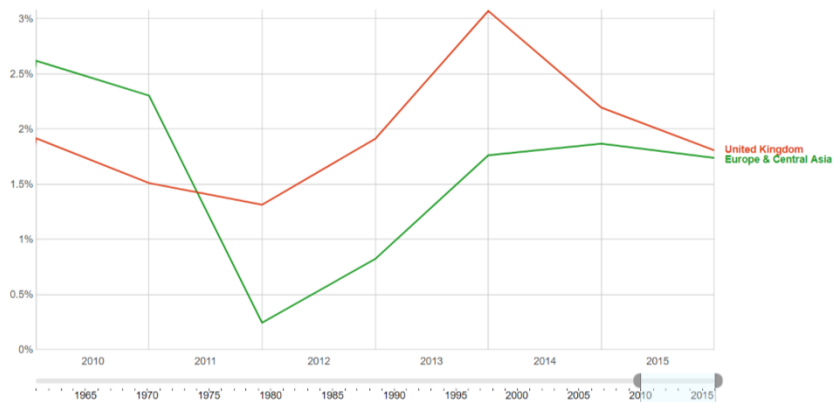 - The net effect of Brexit is -0.35%

Difference-in-Differences

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- 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 time-varying confounders (Parallel trends)
    2. 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
    3. Groups are stable (eg. no migration due to treatment)

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- ▶ One test of this is to check if **pre-treatment trends are parallel**
- ▶ Then our counterfactual makes sense

Difference-in-Differences
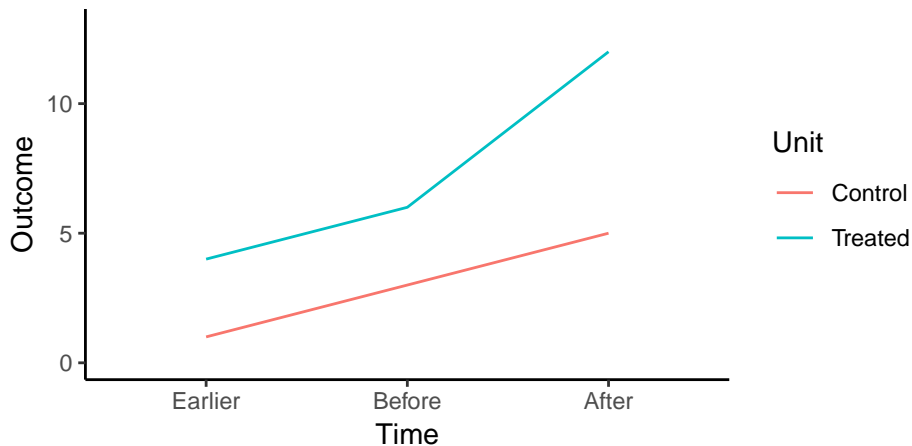
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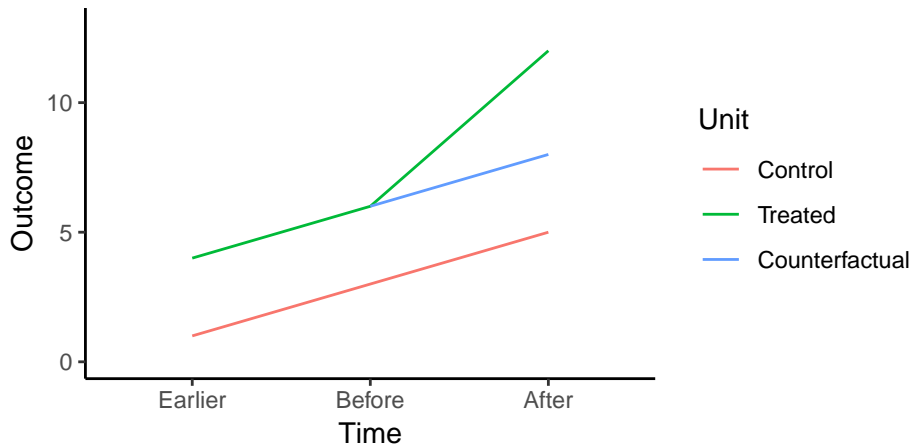
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- ► Eg. training program participants' income has usually fallen a lot in the past few months

Assumptions

Causal Methodology Assumptions

Research Design	Assumptions required for valid causal inference
Field Experiments	No spillovers, Randomization implemented correctly, Randomization complied with, No Hawthorne Effects
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