Research Designs for Causal Inference

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Background

- The experimental ideal!
- All observational studies require an **identification strategy**
- We've been focusing on conditioning (via matching and/or regression)
- Today's lecture is about quasi-experimental designs

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- Cases on either side of the shock are similar except for the effect of the shock
- Can anyone think of examples?

Design Trumps Analysis

- Observational studies are hard because we need to have a convincing causal theory and have observed all causally relevant variables
- Quasi-Experiments potentially save us from needing a complete and fully observed set of causal variables
- In a quasi-experiment, we can treat our data (almost) as-if they are from an experiment

A Little History of IV

- Have been used for a very long time (since Wright 1928)
- Very popular identification strategy in economics
- Just starting to become widespread in political science
  - Field experiments with noncompliance
  - Mediation analysis

When Would We Use IV?

- We are interested in the effect of $X \rightarrow Y$
- How can we identify the effect $X \rightarrow Y$?

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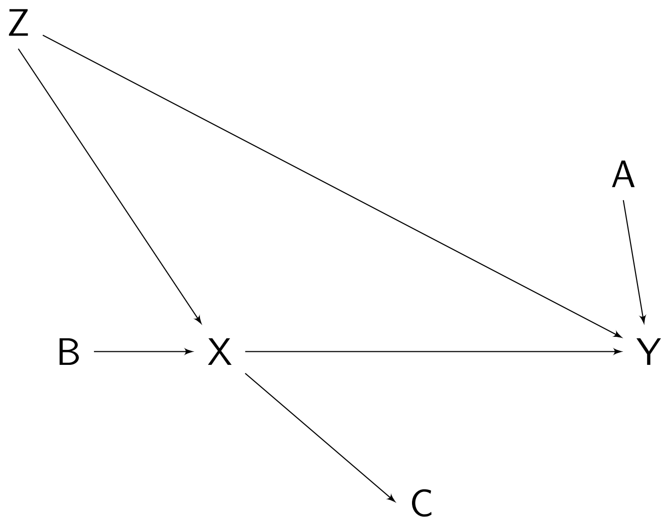
- We are interested in the effect of $X \rightarrow Y$
- How can we identify the effect $X \rightarrow Y$?
- Relationship is confounded by unobservables
- We cannot manipulate X (i.e., no experiments)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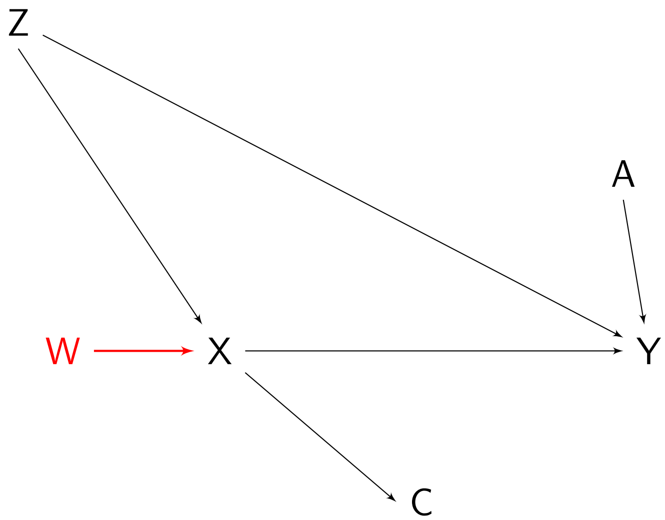
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What is “instrumental”?

1. serving as a crucial means, agent, or tool
2. of, relating to, or done with an instrument or tool
3. relating to, composed for, or performed on a musical instrument
4. of, relating to, or being a grammatical case or form expressing means or agency

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What is “instrumental”?

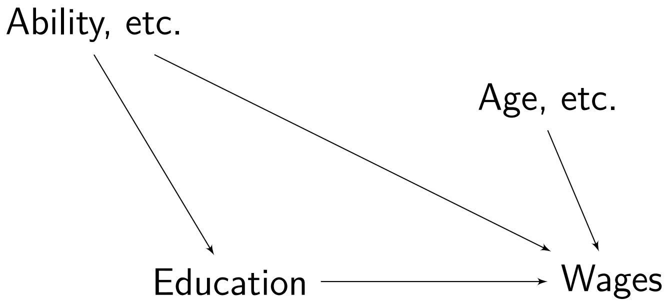
- W must be a crucial cause of X's effect on Y
- W is the quasi-experimental shock to the causal process in our graph
  - It is not caused by X or Y
  - It does not cause Y except through X

Formal Definition

An **instrumental variable** is a variable that satisfies two properties:

1. Exogeneity
   - W temporally precedes X
   - $Cov(B, \epsilon) = 0$
2. Relevance
   - W causes X
   - $Cov(W, X) \neq 0$

Example: Returns to Schooling



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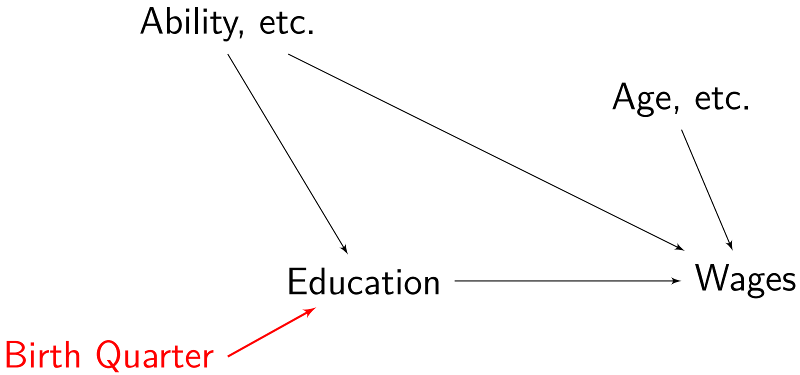

How IV Works I

- Start with case where W is a 0,1 indicator
- To identify the effect $X \to Y$, all we need is W
- We don't need to worry about other omitted variables, because the as-if-random instrument is doing all the heavy lifting for us
- But we don't learn anything about the rest of the causal graph

How IV Works II (Wald)

- Imagine two effects:

$$ITT_y = E[y_i|w_i = 1] - E[y_i|w_i = 0] \qquad (1)$$
$$ITT_x = E[x_i|w_i = 1] - E[x_i|w_i = 0] \qquad (2)$$

- IV estimates the LATE: $\frac{ITT_y}{ITT_x}$

- In a regression, this is:
$E[y_i|w_i] = \beta_0 + \text{LATE} \times E[x_i|w_i]$

How IV Works III (2SLS)

- Regress x on w:
  $\hat{x}_i = \hat{\gamma}_0 + \hat{\gamma}_1 w_i + g_i$
- Regression y on $\hat{x}$:
  $\hat{y}_i = \hat{\beta}_0 + \hat{\beta}_1 \hat{x}_i + e_i$
- Both x and w can be continuous
- We can also have multiple w's and multiple x's
- In Stata:

```
ivregress 2sls Y covariates (X = W), first
```

Standard Errors in IV

- SEs are larger in IV than OLS
- Second-stage can use "robust" SEs to account for heteroskedasticity
- The weaker the instrument, the larger the SEs

IV Diagnostics

- Assess relevance of instrument
    - Examine first-stage equation
    - `estat firststage`

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- Durbin-Wu-Hausman Test (exclusion restriction)
  - Do residuals from the first stage relate to y?
  - If X is exogenous, IV and OLS results should be similar
  - $y = \beta_0 + \beta_1 x_{\text{Confounded}} + \beta_2 \hat{\eta} + e$
  - η are the residuals from the first stage
  - In Stata: `estat endogenous`

IV Diagnostics

- Depending on number of confounded variables and number of instruments, model is:
  - Exactly identified
  - Overidentified
  - Underidentified

- Test of overidentified models:
  - Evaluate null hyp. that all instruments are relevant
  - Rejection means at least one instrument irrelevant
  - In Stata: `estat overid`

- Not applicable in most real-world situations

Local Average Treatment Effect

- IV estimate *local* to the variation in X that is due to variation W (i.e., the LATE)
- This matters if effects are *heterogeneous*
- LATE is effect for those who *comply* with instrument
- Four subpopulations:
  - Compliers: $X = 1$ only if $W = 1$
  - Always-takers: $X = 1$ regardless of W
  - Never-takers: $X = 0$ regardless of W
  - Defiers: $X = 1$ only if $W = 0$

Local Average Treatment Effect

$$\begin{aligned} ITT_y = &\pi_{Compliers} * ITT_{Compliers} \\ &+ \pi_{Always-Takers} * ITT_{Always-Takers} \\ &+ \pi_{Never-Takers} * ITT_{Never-Takers} \\ &+ \pi_{Defiers} * ITT_{Defiers} \end{aligned}$$

- All π sum to 1

Local Average Treatment Effect

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$$\pi_{Complier} = Pr(X=1|W=1) - Pr(X=1|W=0)$$

Local Average Treatment Effect

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- Is this what we want to know?
- Is it externally valid?

Finding Instruments

- Forward, not backward, causal inference
- Most instruments are not things we care about
  - Weather, disasters
  - Geography, borders, climate
  - Lotteries
- A good instrument is one that satisfies both of our conditions, so we need:
  - A good story about exogeneity
  - Evidence that instrument is *strong*

Instrumental Variables Activity

- Read each scenario
- Assess **exogeneity** and **relevance**
- Discuss with the person sitting next to you

Questions about IV?

Example: Maimonides' Rule

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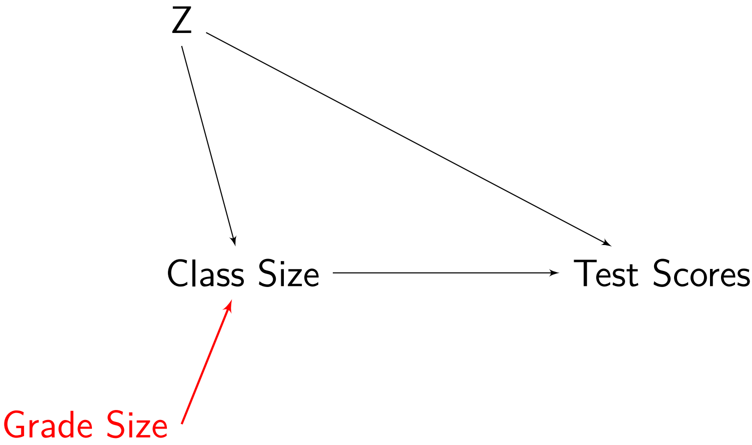
1. What is Maimonides' Rule?
2. Why is it a valid (credible) instrument? (Or why isn't it?)
3. How does it differ from a randomized experiment?


Z
Class Size
Test Scores
Grade Size

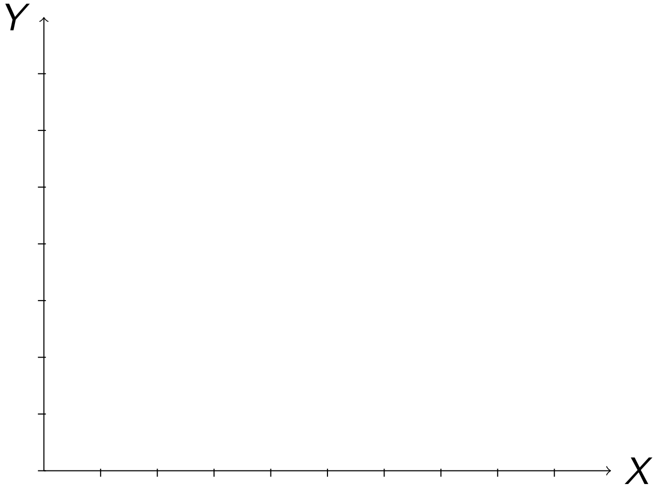
How RDD Works

1. Find a consequential threshold
   - Examples?

2. Causal inference is about comparisons
   - In an experiment, X is randomly assigned
   - In matching or regression, we compare units that differ only in X but are similar in Z

3. In RDD, X is not randomly assigned and there is no *covariate overlap*
   - W causally determines X, so units with different values of X also differ in their value of W
   - compare units that are as similar as possible

Regression Discontinuity



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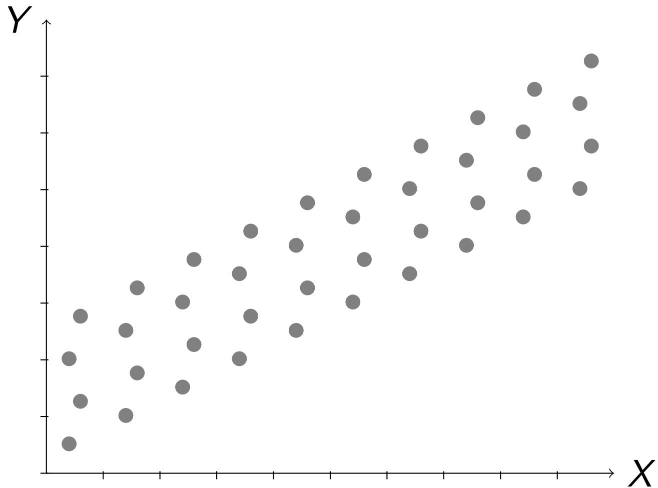

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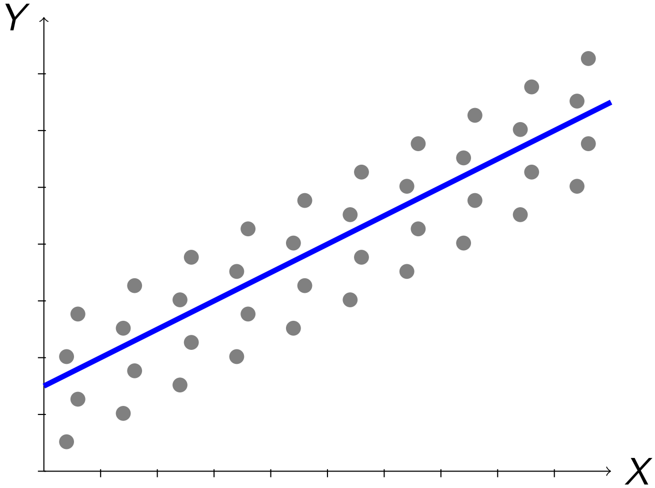

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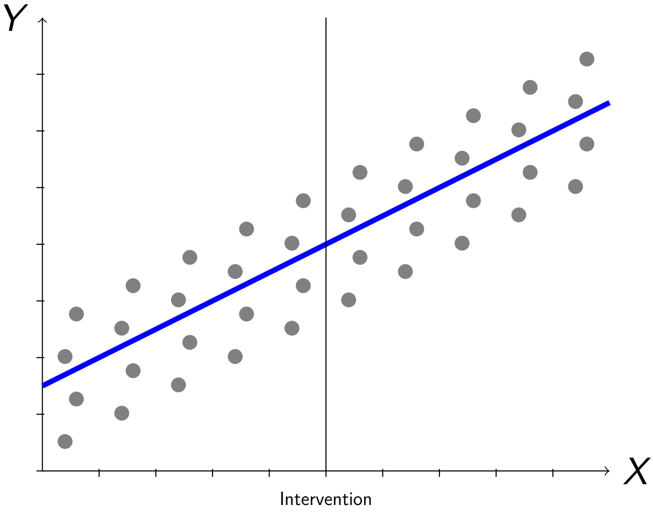

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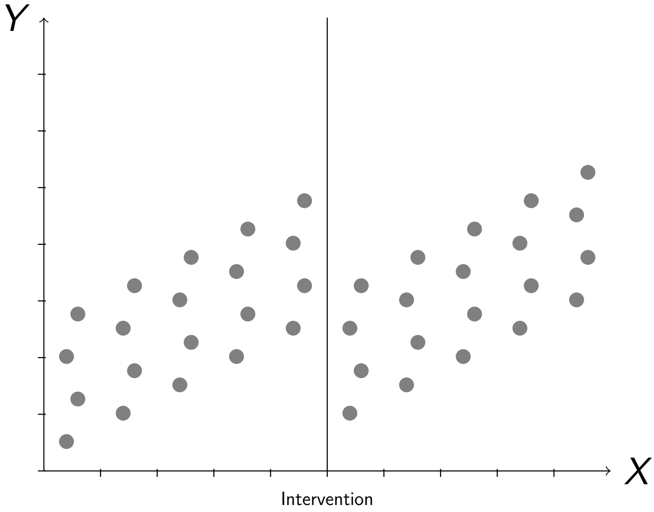

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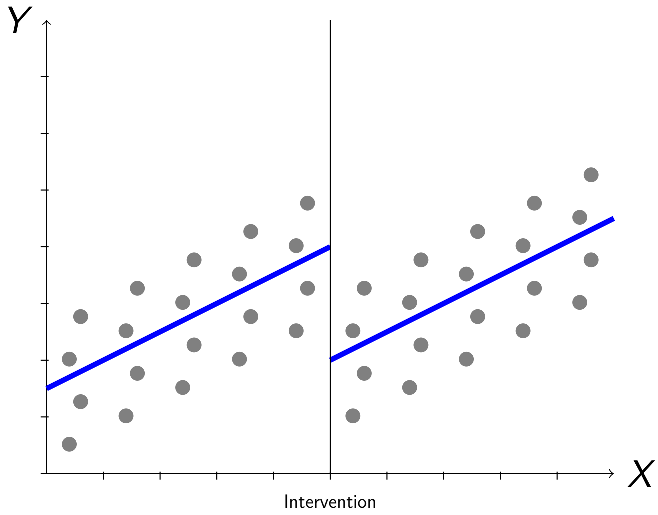

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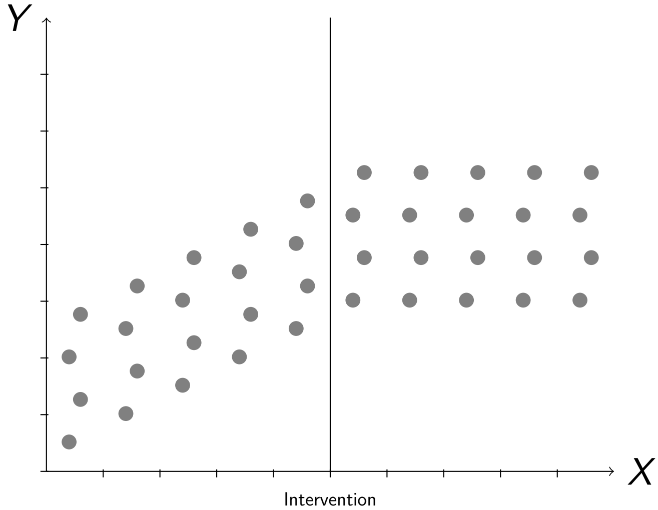

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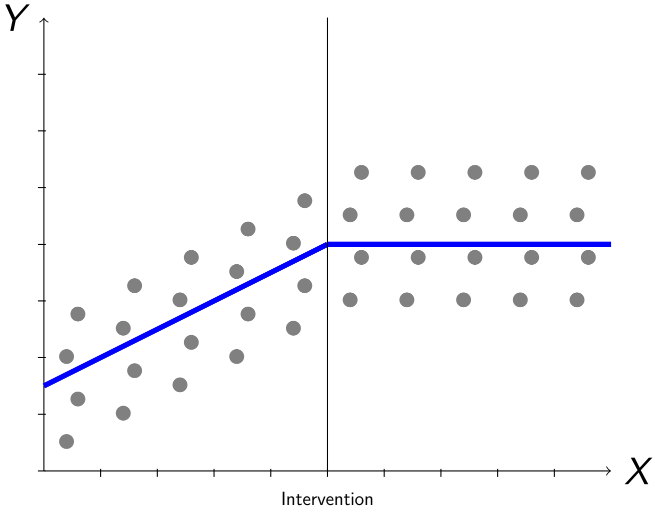

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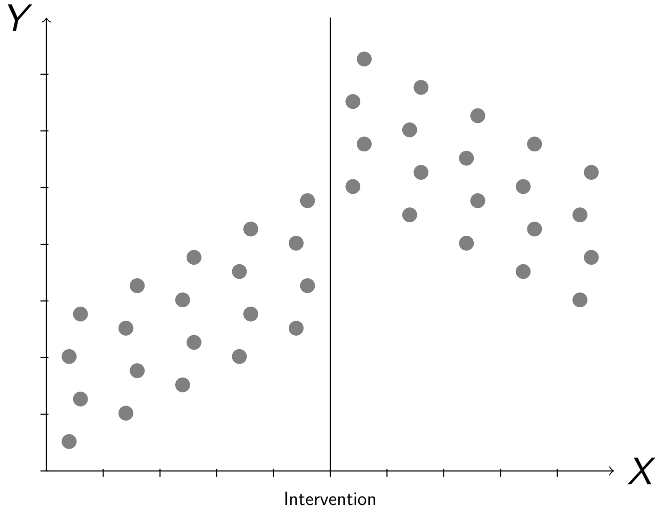

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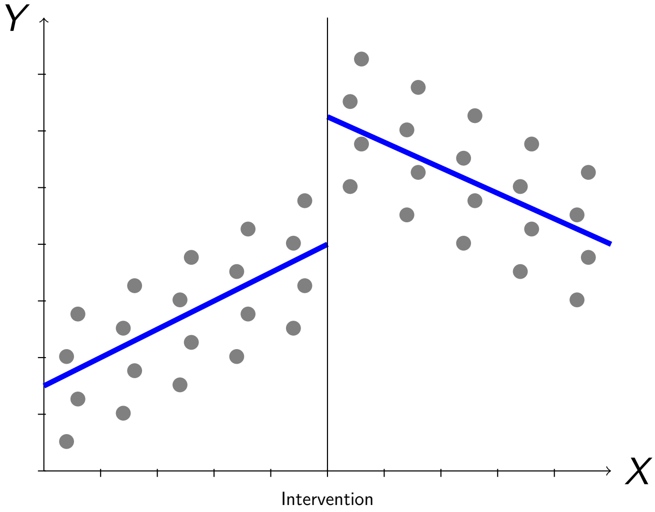

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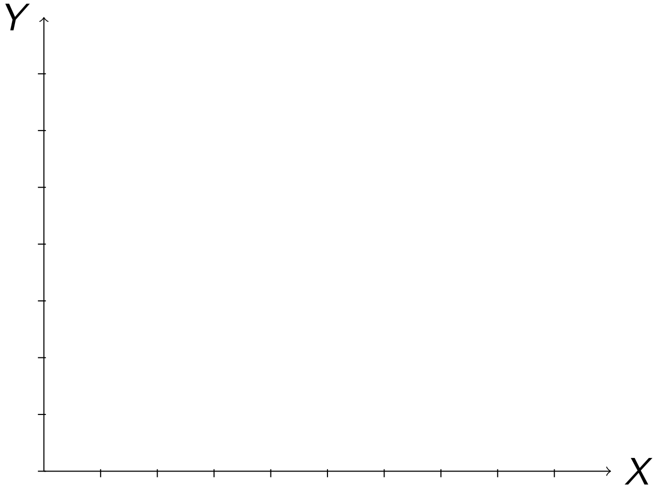

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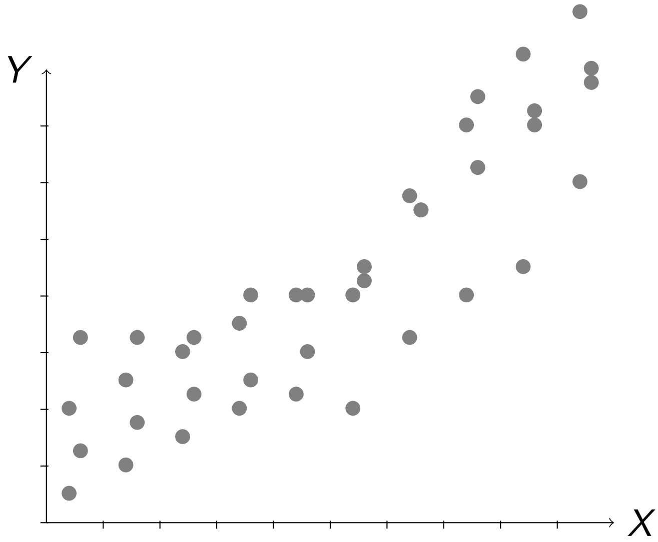

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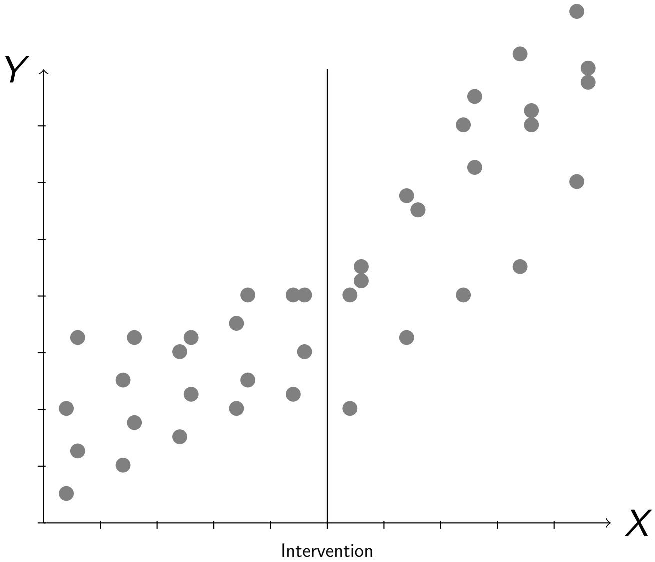

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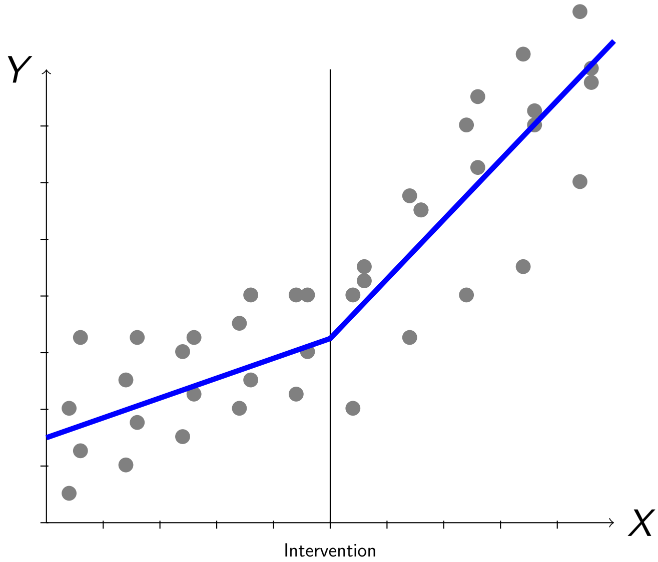

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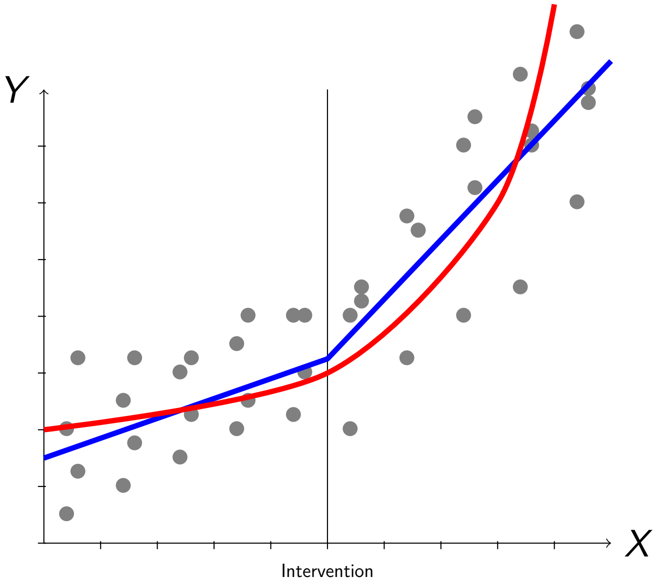

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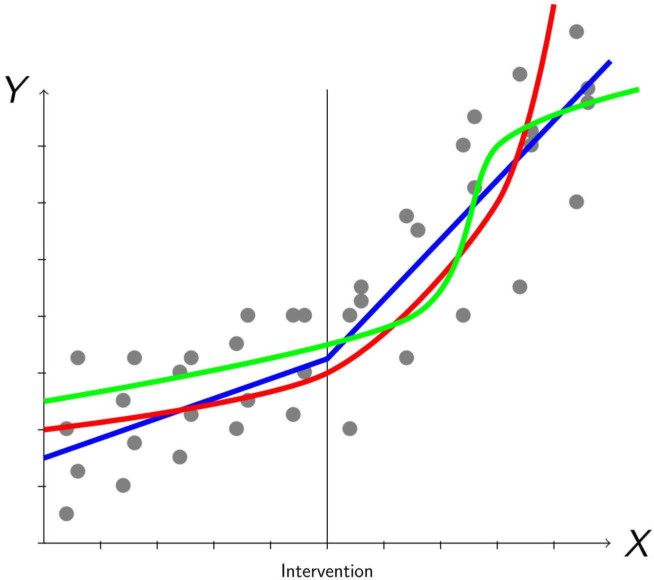

"Sharp" and "Fuzzy" RDD

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$$W = \begin{cases} 1, & \text{if } X > \text{threshold} \\ 0, & \text{if } X < \text{threshold} \end{cases}$$

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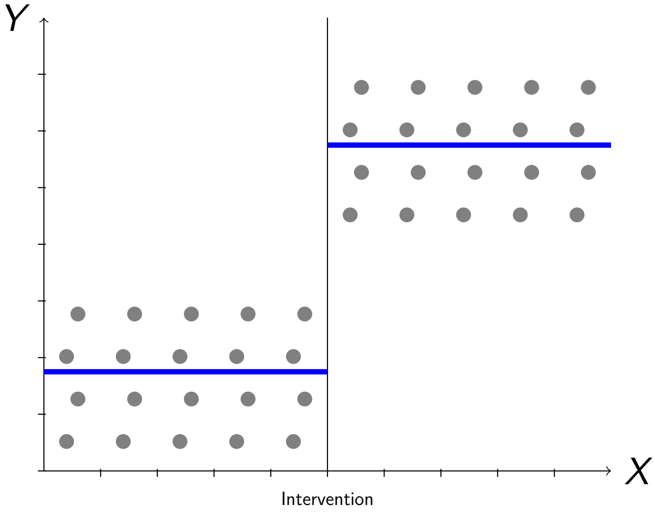
- If a threshold perfectly causes X, then it produces a **sharp** discontinuity
  - Potentially analyze as an experiment
- If a threshold imperfectly (probabilistically) causes X, then it produces a **fuzzy** discontinuity
  - Analyze using Instrumental Variables

$$W = \begin{cases} 1, & \text{if } X > \text{threshold} \\ 0, & \text{if } X < \text{threshold} \end{cases}$$

- Examples?

Sharp vs. Fuzzy RDD



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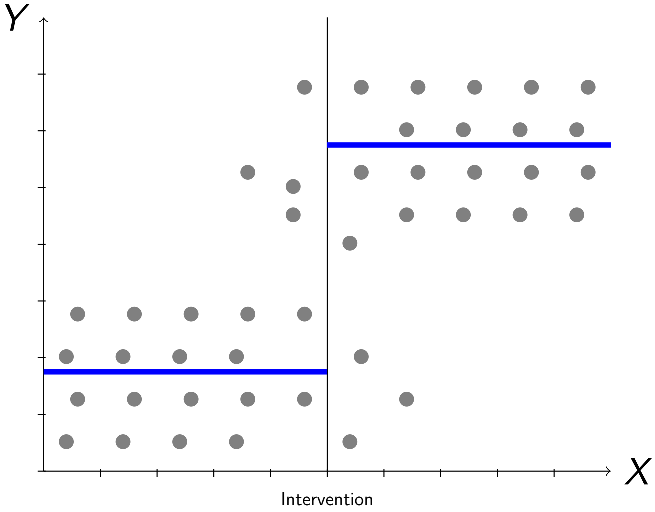
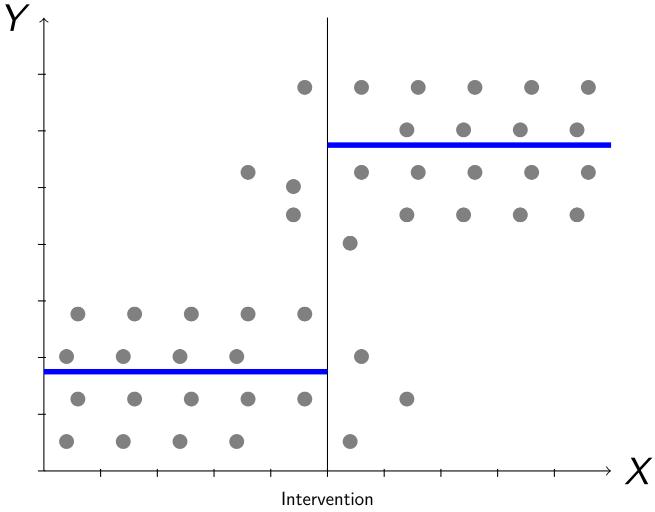


Modelling RDD

- Sharp: Treat threshold as an experiment
- Fuzzy: Treat the threshold as an instrument
  - Not all cases above threshold are treated
  - Not all cases below threshold are untreated
- Effect is estimated at point of discontinuity, which may not reflect effect $X \rightarrow Y$ over the entire domain of X
- Need to choose bandwidths

Sharp vs. Fuzzy RDD



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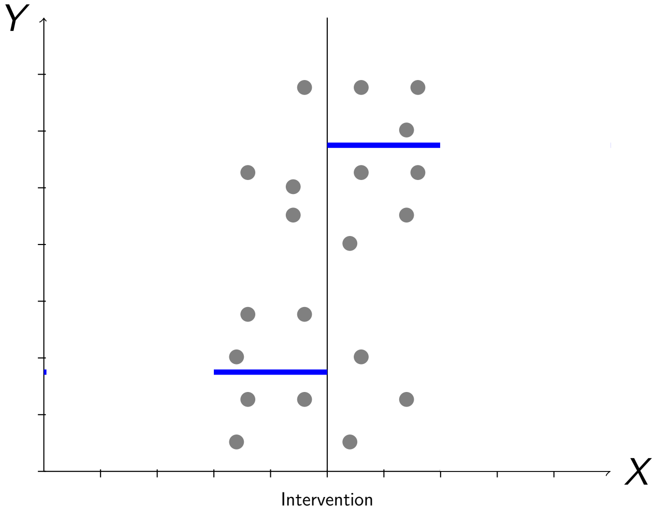

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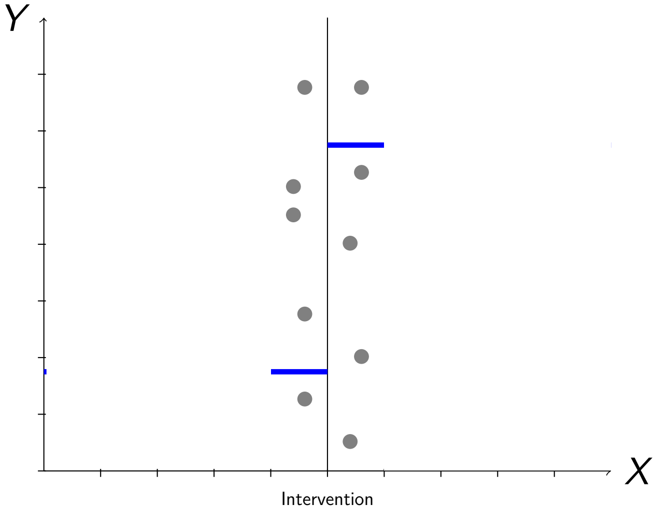

Modelling RDD

- Use bandwidths to subset the data
- Regress Y on X, interacted with W
- Often use polynomial terms:

$$Y = \beta_0 + \beta_1 X + \beta_2 X^2 + \ldots + \beta_3 Z + \beta_4 XZ + \beta_5 X^2 Z + \ldots$$

Problems with Discontinuities

Campbell's Law*: The more any quantitative social indicator (or even some qualitative indicator) is used for social decision-making, the more subject it will be to corruption pressures and the more apt it will be to distort and corrupt the social processes it is intended to monitor.*

Problems with Discontinuities

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- Discontinuities are exploitable

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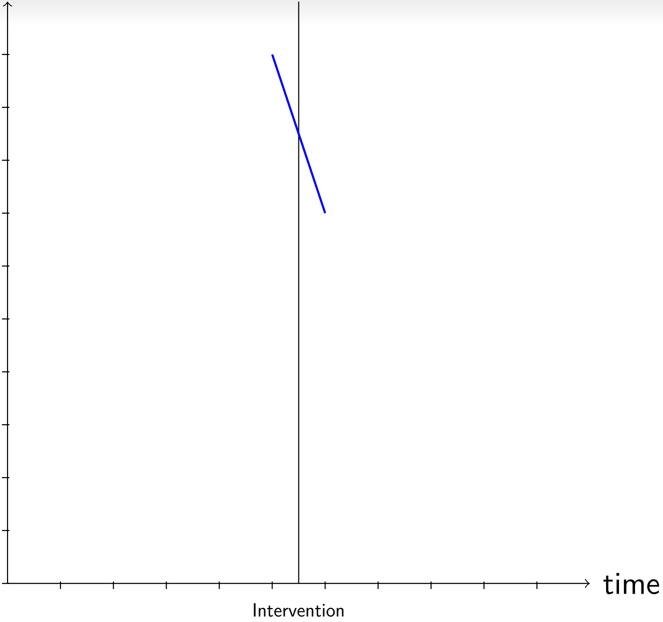
- Discontinuities are exploitable
- Compensatory rivalry and equalization

Questions about RDD?

How ITS Works

- Identify an exogenous shock in X that might affect Y
- Look at Y before (t) and after ($t+1$) the shock
- We only observe one manifest outcome at each point in time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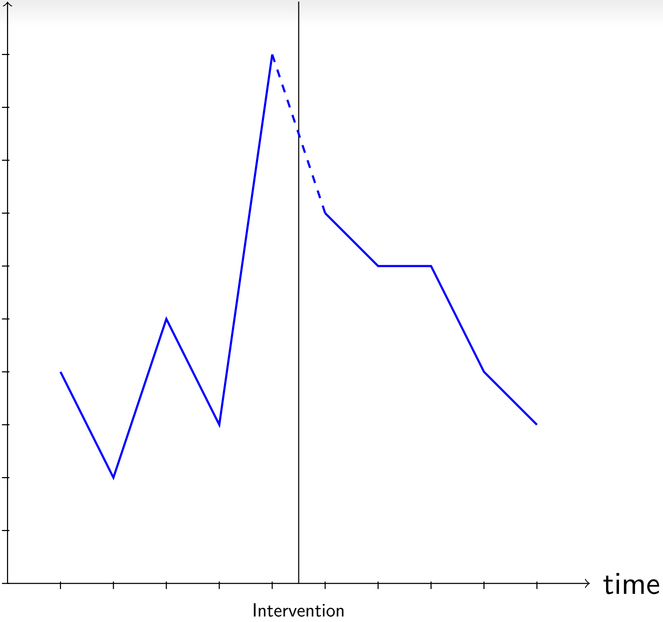
Intervention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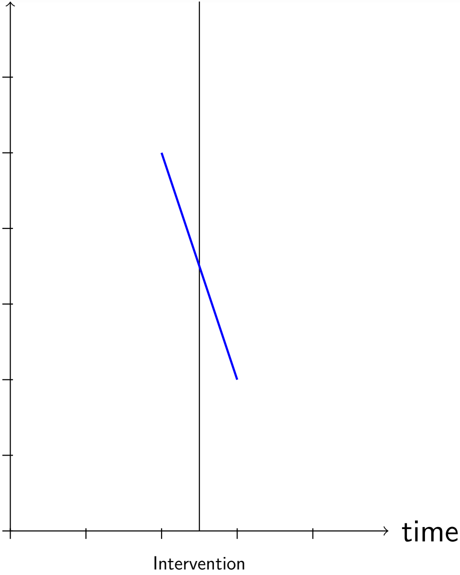
- Identify an exogenous shock in X that might affect Y
- Look at Y before (t) and after ($t+1$) the shock
- We only observe one manifest outcome at each point in time
- To make a causal inference, we need:
  - $Y_{0,t}$ and $Y_{1,t}$, or
  - $Y_{0,t+1}$ and $Y_{1,t+1}$
- Use pre-post comparisons to infer the value of unobserved potential outcomes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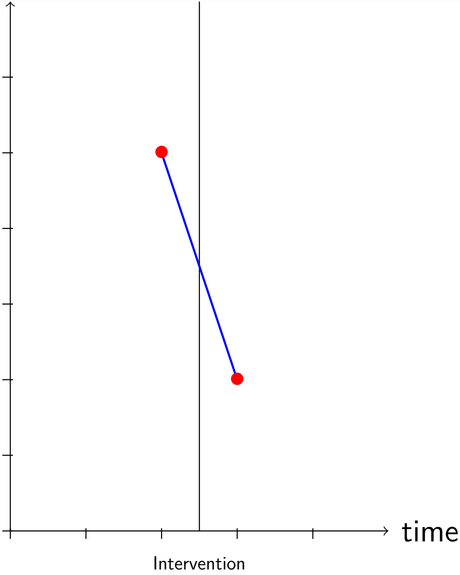
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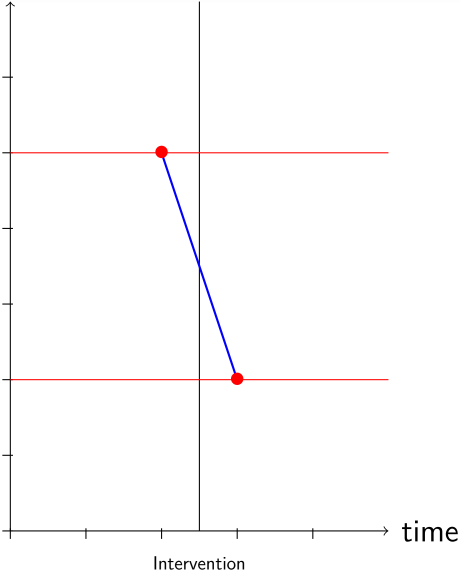
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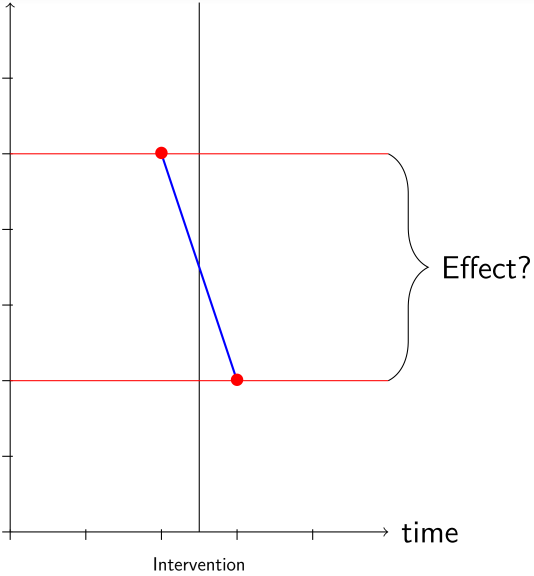
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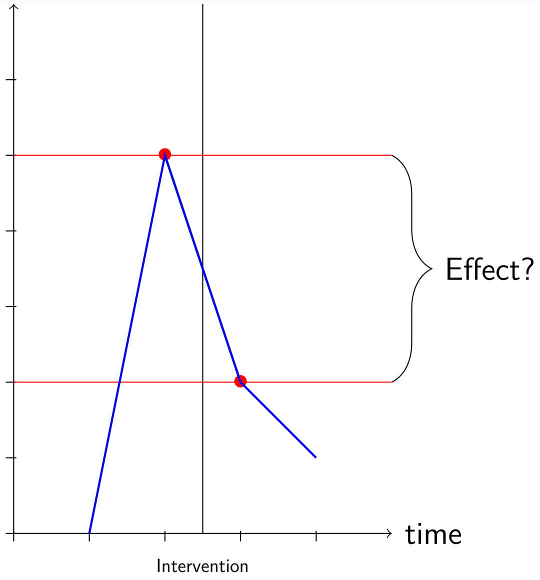
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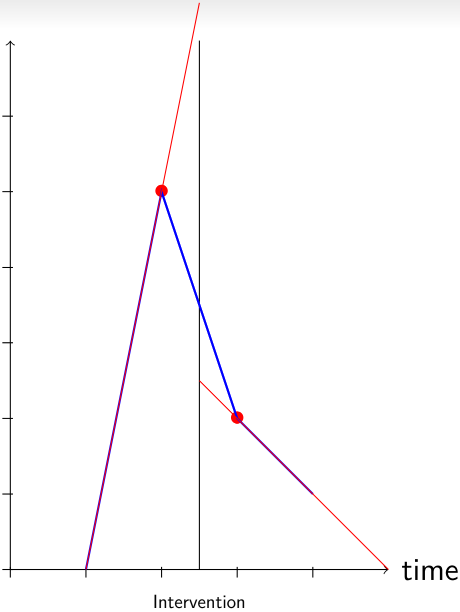

time
Intervention

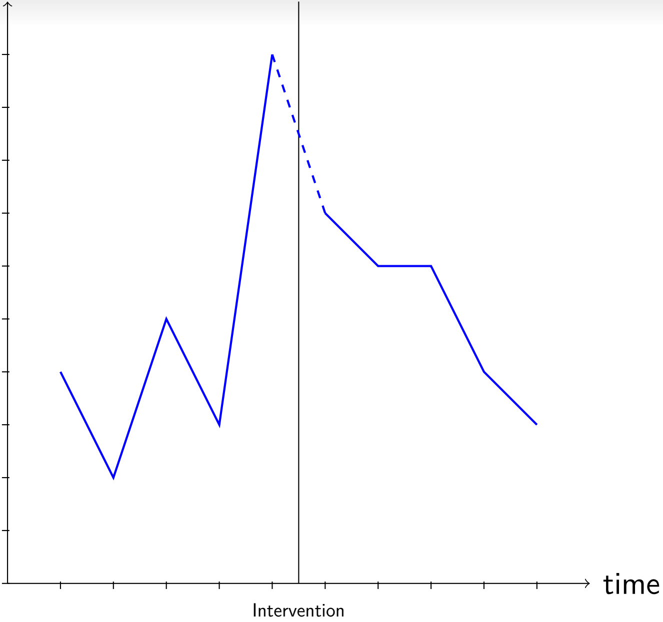

Effect?
time
Intervention

Threats to Inference

- Campbell and Ross talk about six "threats to validity" (i.e., threats to causal inference) related to time-series analysis
- What are those threats?

ITS Considerations

- Changes in *level* and/or *slope*
- Effects can be delayed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ITS Considerations

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- Improving the design (easiest to hardest):

ITS Considerations

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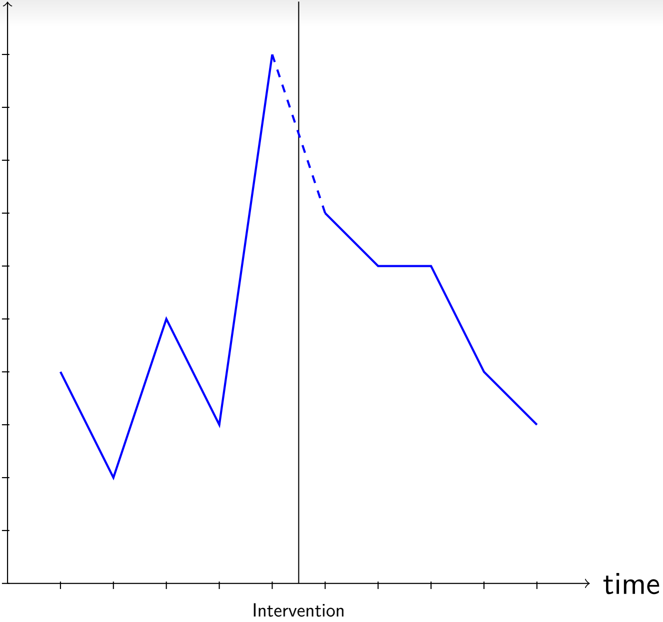
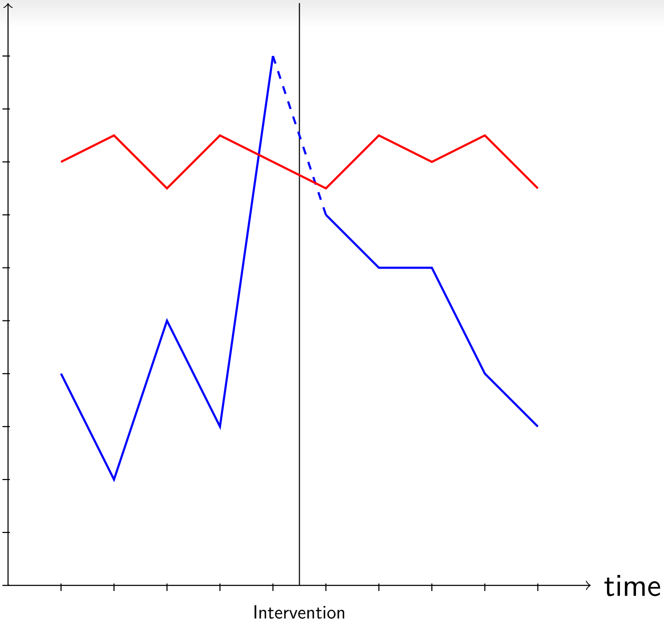
  - Non-equivalent outcome(s) series
  - Longer series
  - Control case(s)


time
Intervention


time
Intervention

Modelling an ITS

- ITS can be expressed as a regression model where **time** is our key X variable
- Intervention W is a pre-post indicator
- We are interested in the coefficients in the marginal effect of time on Y before and after intervention
  - Is there a slope change?
  - Is there an intercept change?

Campbell and Ross

1. What is their research question?
2. How do they analyze the data?
3. What do they find and conclude?

Questions about ITS?

Problem with Inference in ITS

- ITS compares a unit against itself at various points in time (pre- and post-treatment)
- This requires a strong assumption that potential outcomes are constant over-time:

$$Y_{i0t} \equiv Y_{i0t+1}$$
$$Y_{i1t} \equiv Y_{i1t+1}$$

- Campbell and Ross's threats to validity are hugely problematic

Difference-In-Differences

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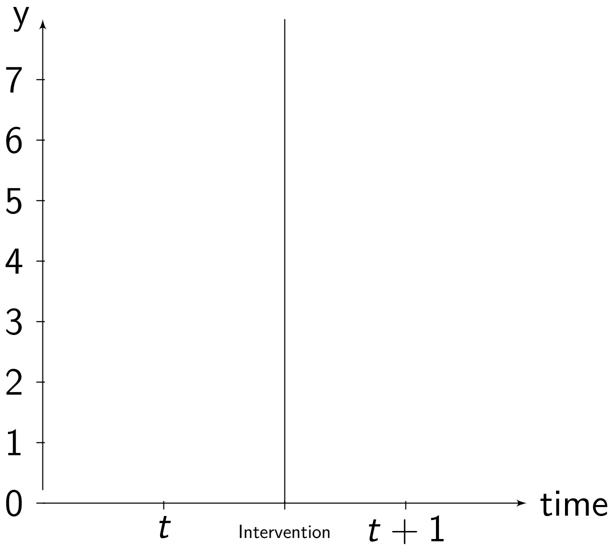
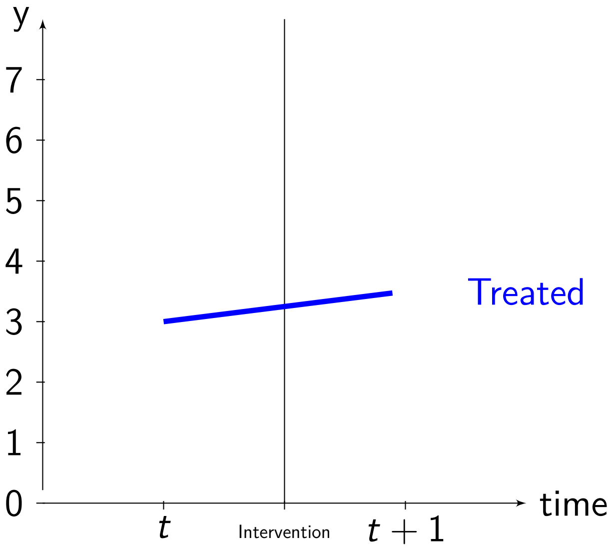
- Use a comparison case (or cases)!

- Instead of using the pre-post difference in Y_i to estimate the causal effect, use the difference in pre-post differences for two units i and j:

$$(Y_{i,t+1} - Y_{i,t}) - (Y_{j,t+1} - Y_{j,t})$$


y
7
6
5
4
3
2
1
0
time
t
Intervention
$t+1$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time
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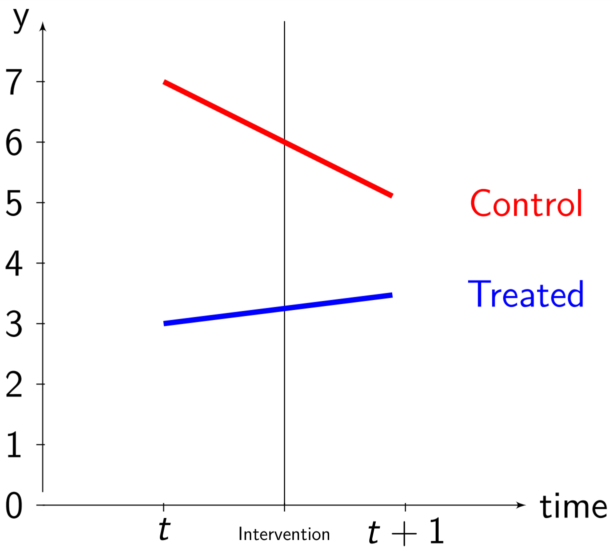
Intervention
$t+1$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Treated
time
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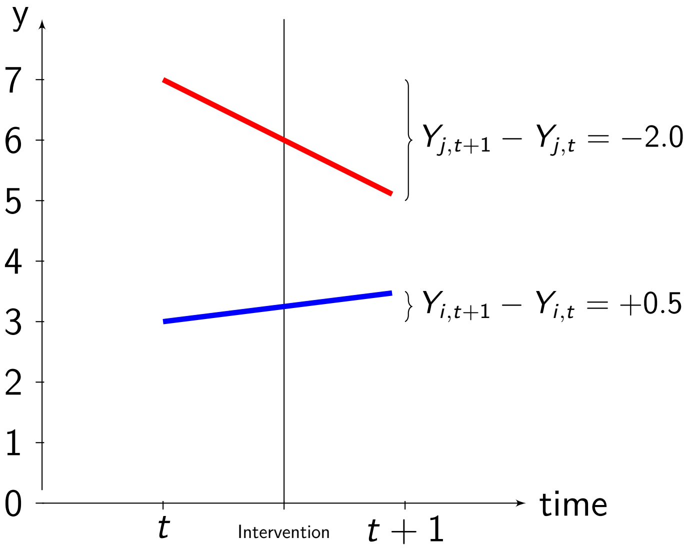
Intervention
$t+1$


y
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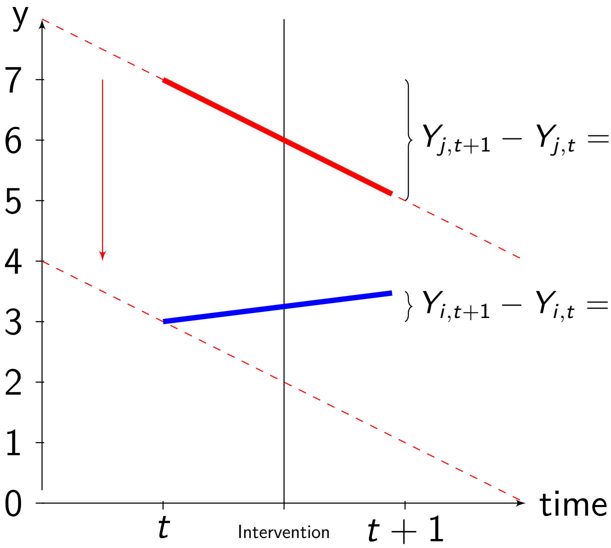
t
Intervention
$t+1$
time
$Y_{j,t+1} - Y_{j,t} = -2.0$
$Y_{i,t+1} - Y_{i,t} = +0.5$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$Y_{j,t+1} - Y_{j,t} = -2.0$
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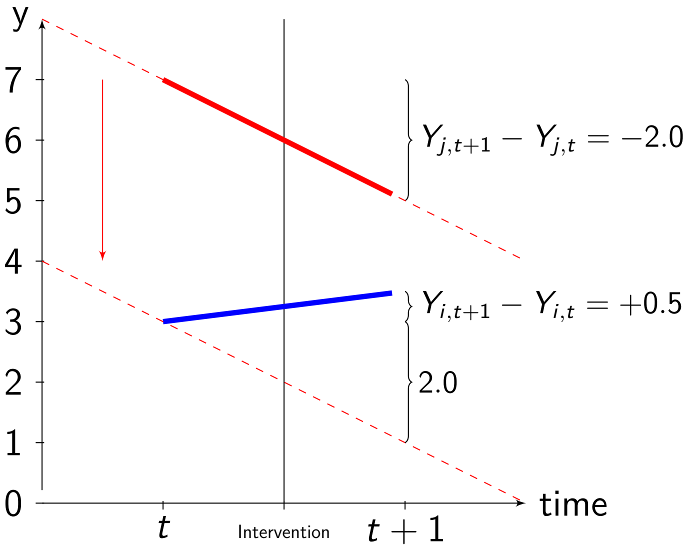
time
t
Intervention
$t+1$


y
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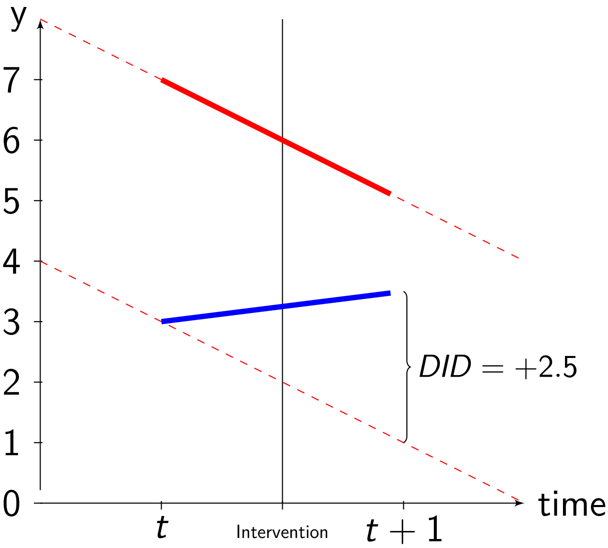
0
$Y_{j,t+1} - Y_{j,t} = -2.0$
$Y_{i,t+1} - Y_{i,t} = +0.5$
2.0
time
t
Intervention
$t+1$


y
7
6
5
4
3
2
1
0
t
Intervention
$t+1$
time
$DID = +2.5$

Lassen and Serritzlew

1. What is their research question?
2. How do they analyze the data?
3. What do they find and conclude?

Causal Inference Over-Time

- In experiments, matching, cross-sectional regression, and RDD, we make causal inferences based on **between-unit** comparisons at the *same* time
- In ITS, DID, and panel analysis (next week), we make causal inferences (also) based on **within-unit** comparisons at *different* times
- This can be really helpful, but also raises new concerns