Experimental Design and the Search for Quasi-Experiments

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Principles of causality

1. Correlation
2. Nonconfounding
3. Direction ("temporal precedence")
4. Mechanism
5. (Appropriate level of analysis)

Mill's Method of Difference

If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance save one in common, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or cause, or an necessary part of the cause, of the phenomenon.

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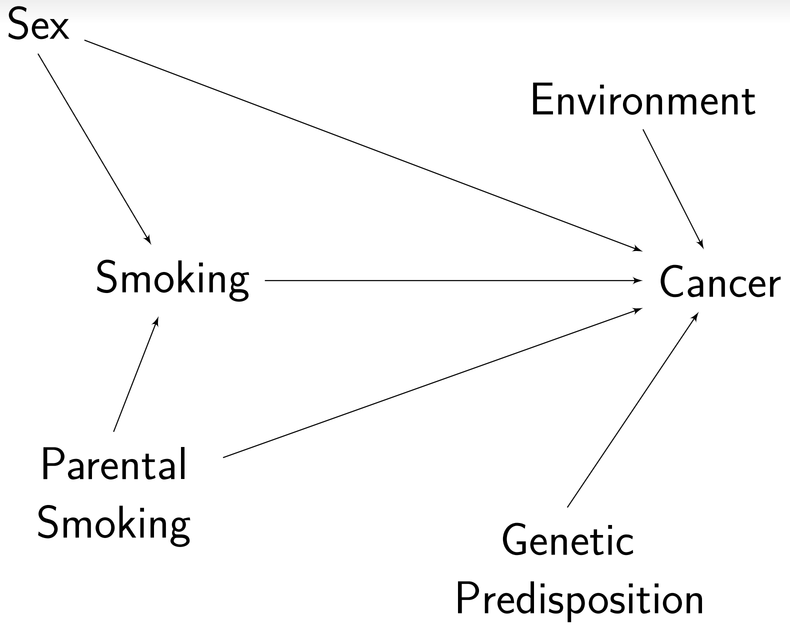
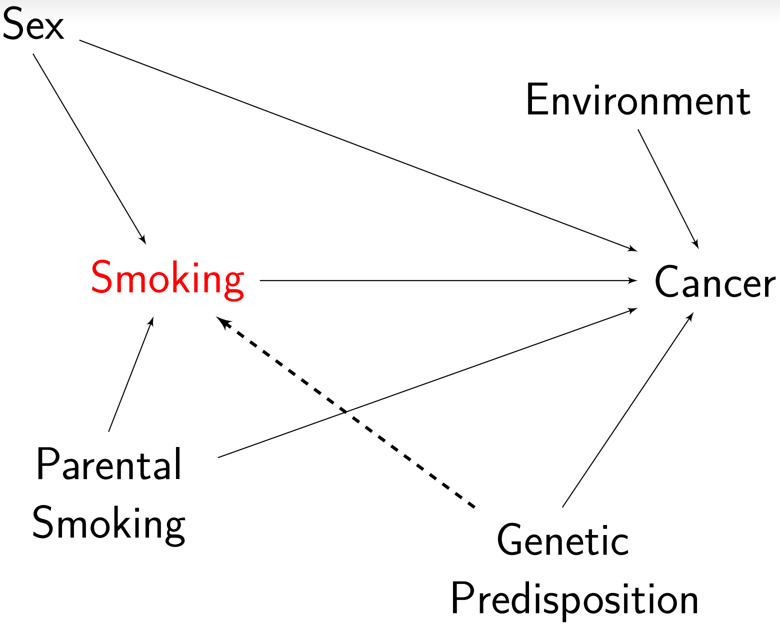
1. Correlate a "putative" cause (X) and an outcome (Y)
2. Identify all possible confounds ($\mathbf{Z}$)
3. "Condition" on all confounds
   - Calculate correlation between X and Y at each combination of levels of $\mathbf{Z}$


Sex
Environment
Smoking
Cancer
Parental Smoking
Genetic Predisposition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The Experimental Ideal

A randomized experiment, or randomized control trial is:

> *The observation of units after, and possibly before, a randomly assigned intervention in a controlled setting, which tests one or more precise causal expectations*

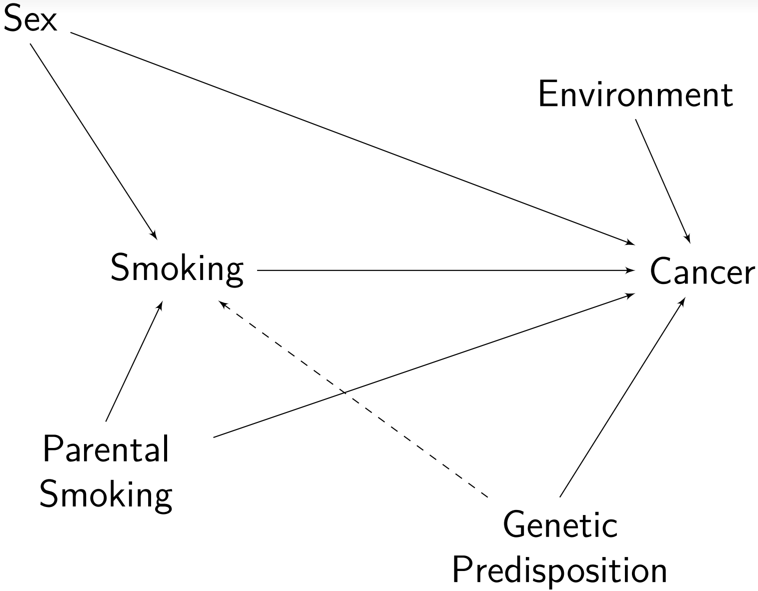
This is Holland's "statistical solution" to the fundamental problem of causal inference

The Experimental Ideal

- It solves both the temporal ordering and confounding problems
  - Treatment (X) is applied by the researcher before outcome (Y)
  - Randomization means there are no confounding (Z) variables
- Thus experiments are sometimes called a “gold standard” of causal inference

Random Assignment

- A physical process of randomization
- Breaks the "selection process"
    - Units only take value of X because of assignment
- This means:
    - All covariates are balanced between groups
    - Potential outcomes are balanced between groups
    - In sum: No confounding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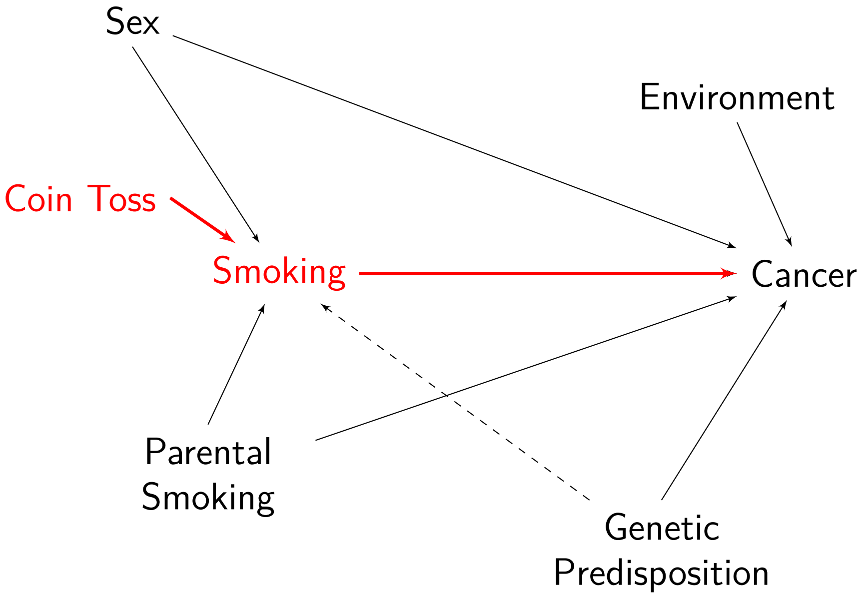
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Experimental Inference I

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- But we still only see one potential outcome for each unit:

$$ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0]$$

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- Is this what we want to know?

Experimental Inference IV

- What we want and what we have:

$$ATE = E[Y_{1i}] - E[Y_{0i}] \tag{1}$$

$$ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0] \tag{2}$$

Experimental Inference IV

- What we want and what we have:

$$ATE = E[Y_{1i}] - E[Y_{0i}] \tag{1}$$

$$ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0] \tag{2}$$

- Are the following statements true?
  - $E[Y_{1i}] = E[Y_{1i}|X = 1]$
  - $E[Y_{0i}] = E[Y_{0i}|X = 0]$

Experimental Inference IV

- What we want and what we have:

$$ATE = E[Y_{1i}] - E[Y_{0i}] \tag{1}$$

$$ATE_{naive} = E[Y_{1i}|X=1] - E[Y_{0i}|X=0] \tag{2}$$

- Are the following statements true?
  - $E[Y_{1i}] = E[Y_{1i}|X=1]$
  - $E[Y_{0i}] = E[Y_{0i}|X=0]$
- Not in general!

Experimental Inference V

- Only true when both of the following hold:

$$E[Y_{1i}] = E[Y_{1i}|X=1] = E[Y_{1i}|X=0] \quad (3)$$
$$E[Y_{0i}] = E[Y_{0i}|X=1] = E[Y_{0i}|X=0] \quad (4)$$

- In that case, potential outcomes are *independent* of treatment assignment

- If true, then:

$$\begin{aligned} ATE_{naive} &= E[Y_{1i}|X=1] - E[Y_{0i}|X=0] \quad (5) \\ &= E[Y_{1i}] - E[Y_{0i}] \\ &= ATE \end{aligned}$$

Experimental Inference VI

- This holds in experiments because of randomization
  - Units differ only in what side of coin was up
  - Experiments randomly reveal potential outcomes

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- This holds in experiments because of randomization
  - Units differ only in what side of coin was up
  - Experiments randomly reveal potential outcomes
- Matching/regression/etc. attempts to eliminate those confounds, such that:

$$E[Y_{1i}|Z] = E[Y_{1i}|X = 1, Z] = E[Y_{1i}|X = 0, Z]$$
$$E[Y_{0i}|Z] = E[Y_{0i}|X = 1, Z] = E[Y_{0i}|X = 0, Z]$$

"The Perfect Doctor"

Unit	Y_0	Y_1
1	?	?
2	?	?
3	?	?
4	?	?
5	?	?
6	?	?
7	?	?
8	?	?
Mean	**?**	**?**

"The Perfect Doctor"

Unit	Y_0	Y_1
1	?	14
2	6	?
3	4	?
4	5	?
5	6	?
6	6	?
7	?	10
8	?	9
Mean	**5.4**	**11**

"The Perfect Doctor"

Unit	Y_0	Y_1
1	13	14
2	6	0
3	4	1
4	5	2
5	6	3
6	6	1
7	8	10
8	8	9
Mean	**7**	**5**

Experimental Analysis I

- The statistic of interest in an experiment is the *sample average treatment effect* (SATE)
- This boils down to being a mean-difference between two groups:

$$SATE = \frac{1}{n_1}\sum Y_{1i} - \frac{1}{n_0}\sum Y_{0i} \qquad (5)$$

- In practice we often estimate this using:
  - t-tests
  - Linear regression

Experimental Analysis II

- We don't just care about the size of the SATE. We also want to know whether it is significantly different from zero (i.e., different from no effect/difference)
- To know that, we need to estimate the *variance* of the SATE
- The variance is influenced by:
  - Total sample size
  - Variance of the outcome, Y
  - Relative size of each treatment group

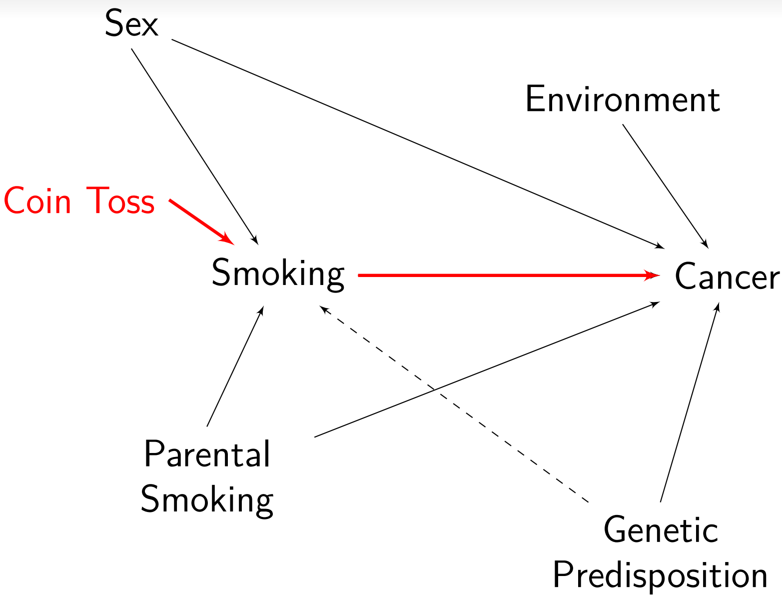
Experimental Analysis III

- Formula for the variance of the SATE is:
$$\widehat{Var}(SATE) = \frac{\widehat{Var}(Y_0)}{N_0} + \frac{\widehat{Var}(Y_1)}{N_1}$$
  - $\widehat{Var}(Y_0)$ is control group variance
  - $\widehat{Var}(Y_1)$ is treatment group variance

- We often express this as the *standard error* of the estimate:
$$\widehat{SE}_{SATE} = \sqrt{\frac{\widehat{Var}(Y_0)}{N_0} + \frac{\widehat{Var}(Y_1)}{N_1}}$$

Compliance

- Compliance is when individuals receive and accept the treatment to which they are assigned:
  - Receive the wrong treatment (cross-over)
  - Fail to receive any treatment
- This causes problems for our analysis because factors other than randomization explain why individuals receive their treatment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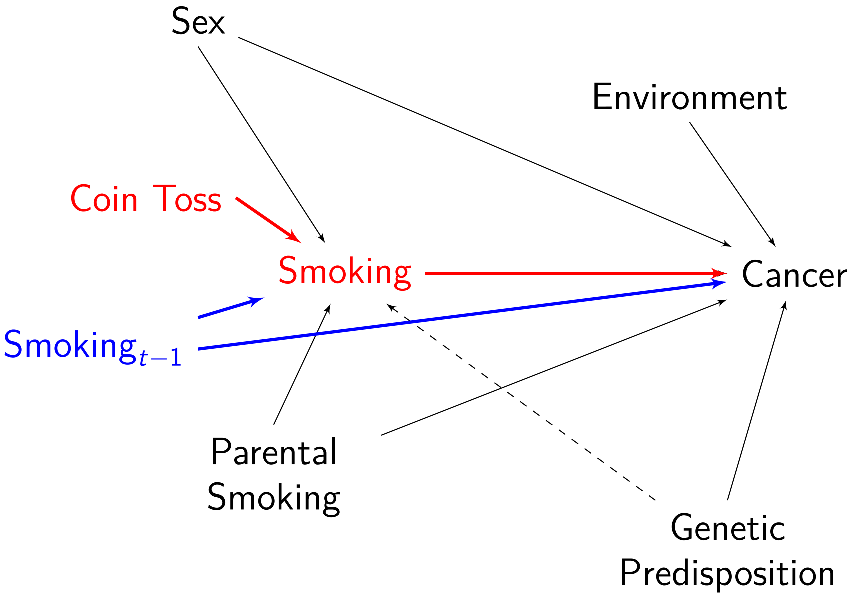

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Smoking_{t-1}
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Ethics

- Experiments raise lots of ethical considerations
- Because we are intervening in peoples' lives, we have to weight harm and benefits of our interventions
- A big question relates to "deception" (are we deceiving our experimental participants? is that a problem?)

Why Quasi-Experiments?

- 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

What is a Quasi-Experiment?

- Quasi-Experiments are situations where randomization-like forces influence the values of independent variables
- Most of the time, these are "natural" experiments where boundaries, discontinuities, or interruptions disrupt a continuous treatment-assignment process
- Analyzing a quasi-experiment involves searching for an "instrument variable"

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
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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

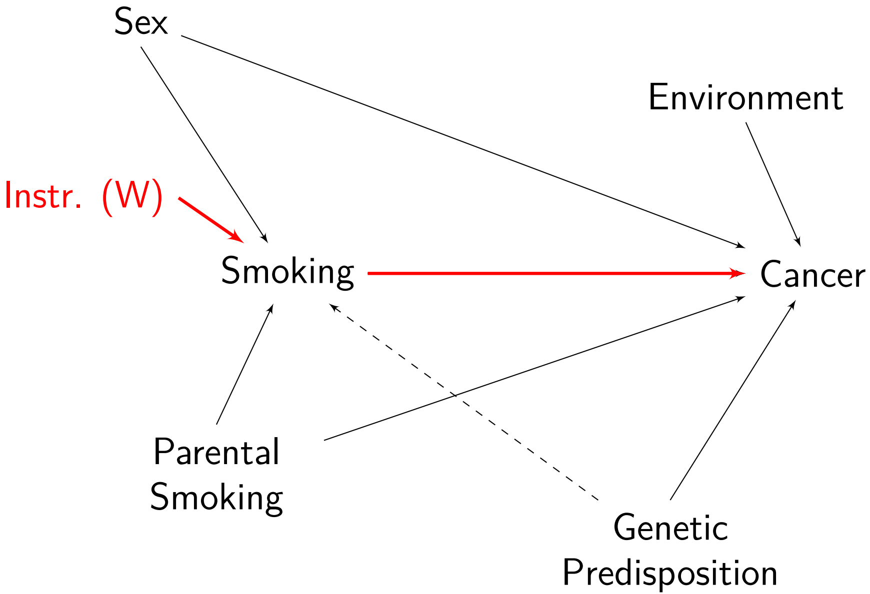
Formal Definition

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
   - $Cov(W, X) \neq 0$

How IV Works I

- Start with case where W is 0,1
- To identify the effect $X \rightarrow Y$, all we need is W
- We don't need to worry about other omitted variables, but we don't learn anything about the rest of the causal graph


Sex
Environment
Instr. (W)
Smoking
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How IV Works II (Wald)

- Imagine two effects:

$$ITT_y = E[y_i|w_i = 1] - E[y_i|w_i = 0] \qquad (6)$$
$$ITT_x = E[x_i|w_i = 1] - E[x_i|w_i = 0] \qquad (7)$$

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

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

Local Average Treatment Effect

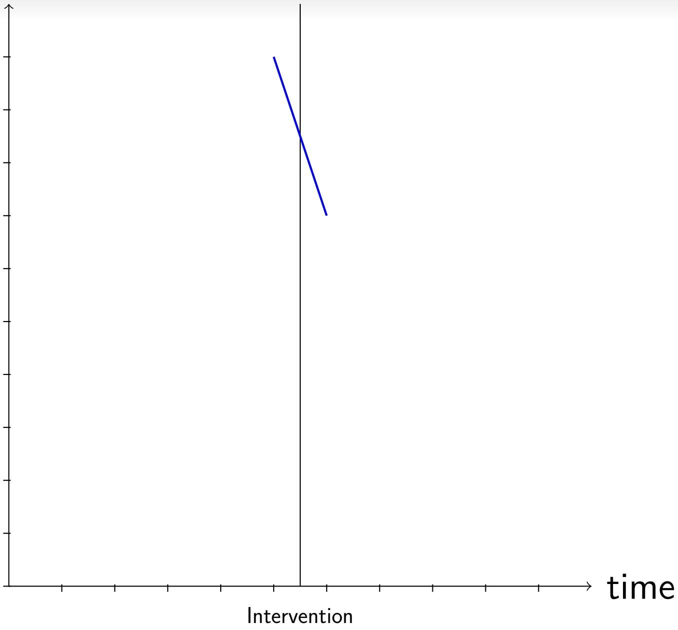
- IV estimate *local* to the variation in X that is due to variation W (i.e., the LATE)
- This matters if effects are *heterogeneous*
- LATE is effect for those who *comply* with instrument
- Four subpopulations:
  - Compliers: $X = 1$ only if $W = 1$
  - Always-takers: $X = 1$ regardless of W
  - Never-takers: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

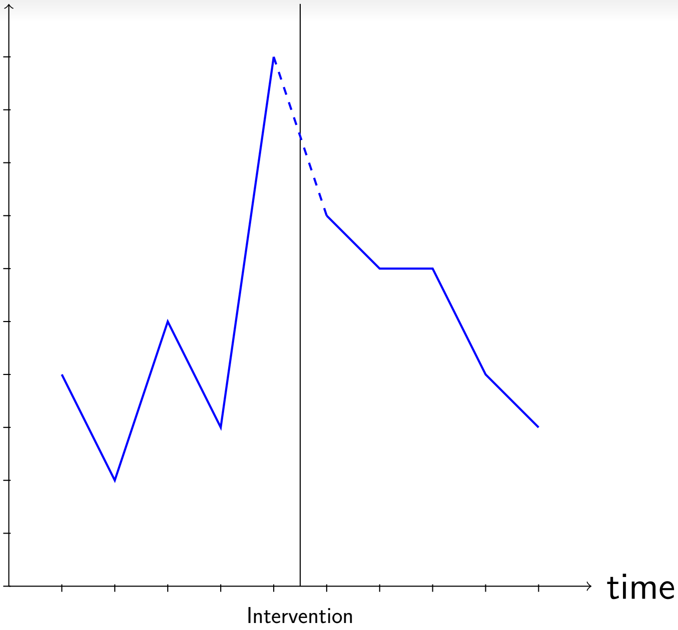
Finding Instruments

- 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*

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


time
Intervention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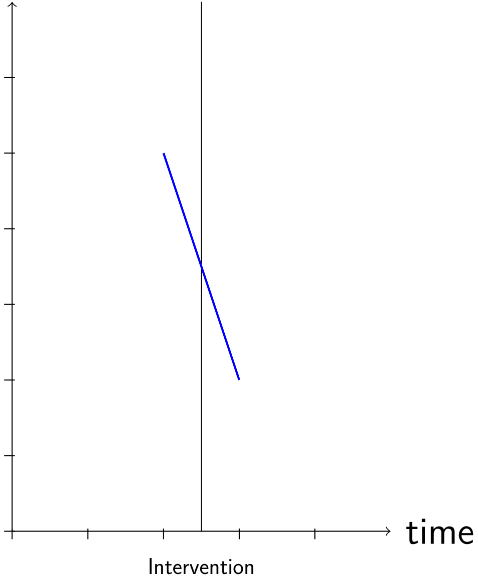
- Look at Y before (t) and after ($t+1$) the shock
- We only observe one manifest outcome at each point in time
- To make a causal inference, we need:
  - $Y_{0,t}$ and $Y_{1,t}$, or
  - $Y_{0,t+1}$ and $Y_{1,t+1}$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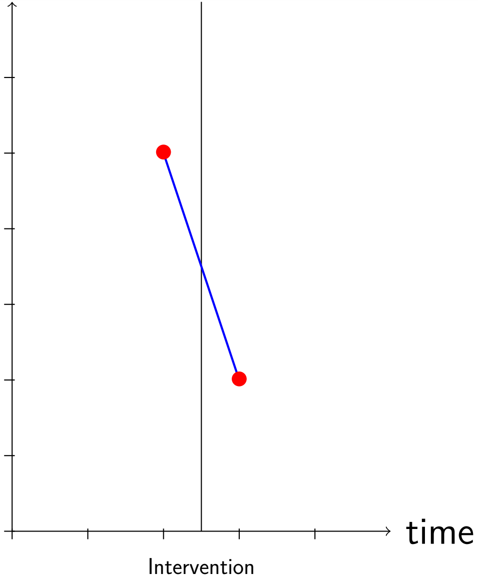
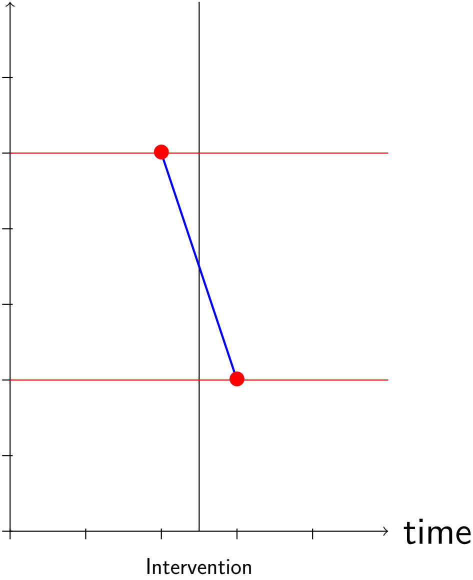
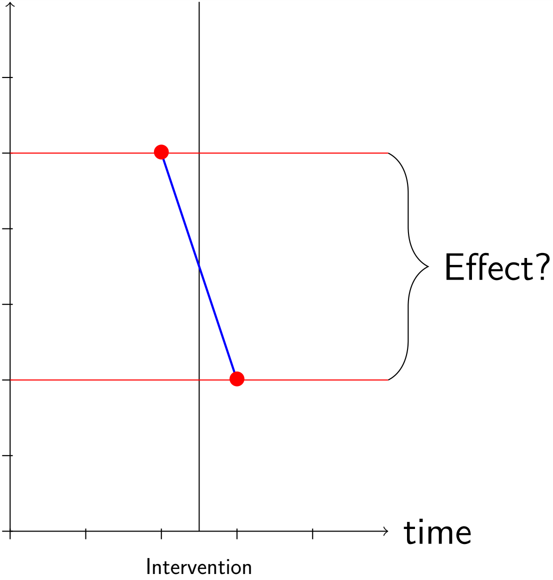
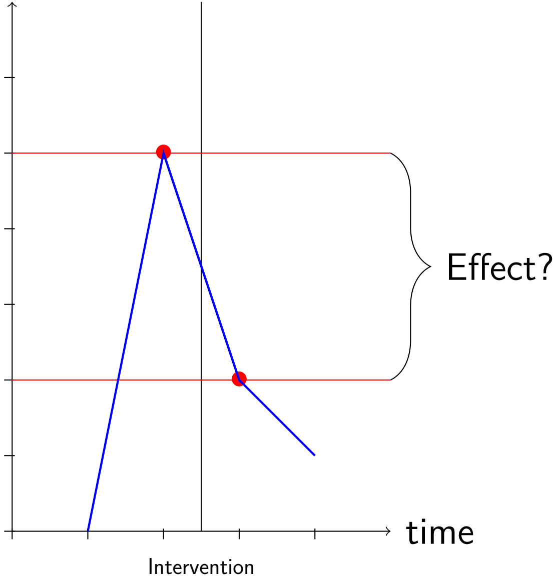
Intervention


time
Intervention


time
Intervention


Effect?
time
Intervention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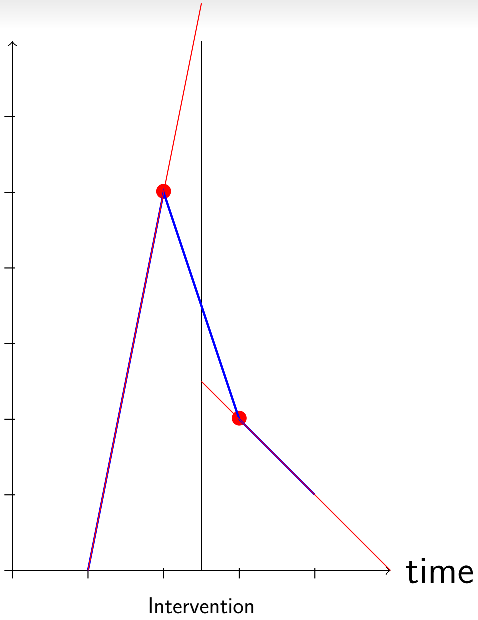
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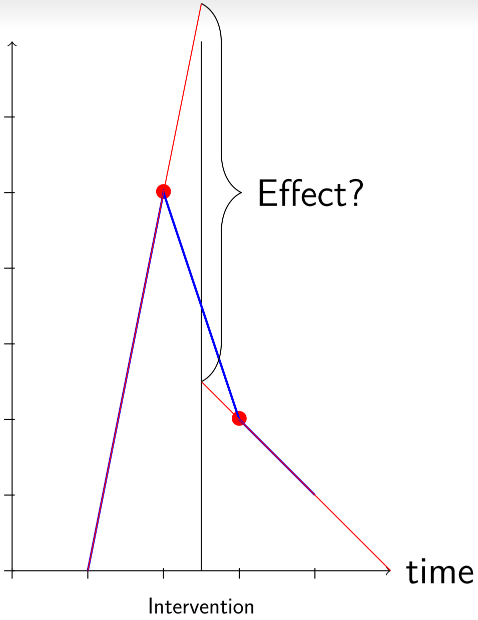
Effect?
time
Intervention

Threats to Validity

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

Threats to Validity

1. History
2. Maturation
3. Testing
4. Instrumentation
5. Instability
6. Regression to the mean

Difference-In-Differences

- How do we know change in Y wasn't due to something else?
  - How do we know $Y_{0,t}$ is a good stand-in for $Y_{0,t+1}$?

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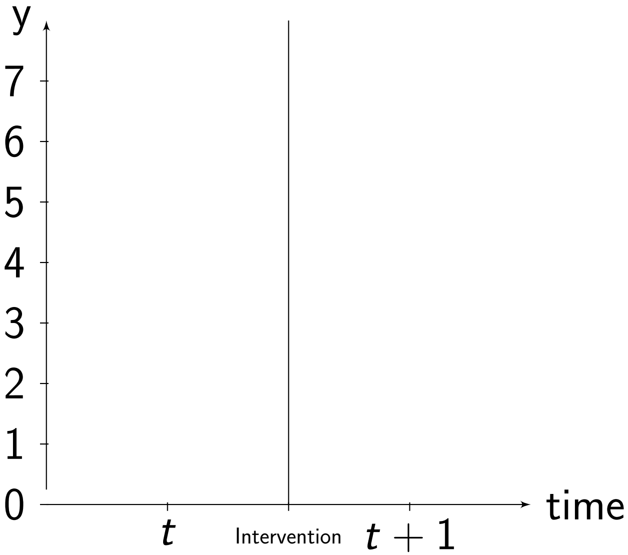
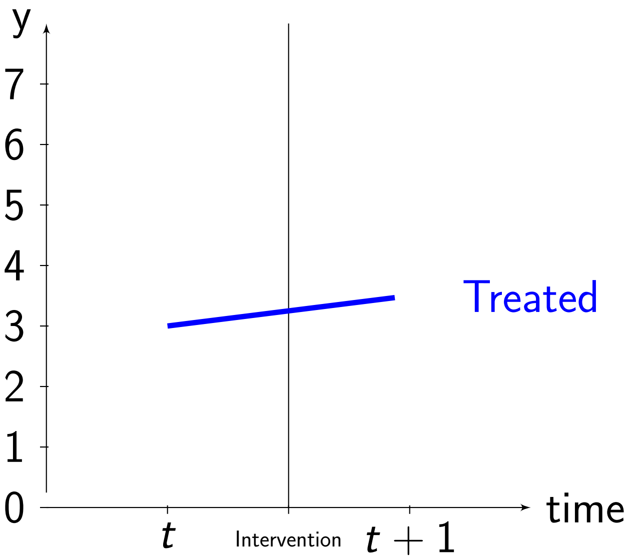
- Use a comparison case (or cases)!

Difference-In-Differences

- How do we know change in Y wasn't due to something else?
  - How do we know $Y_{0,t}$ is a good stand-in for $Y_{0,t+1}$?
- 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
$t+1$


y
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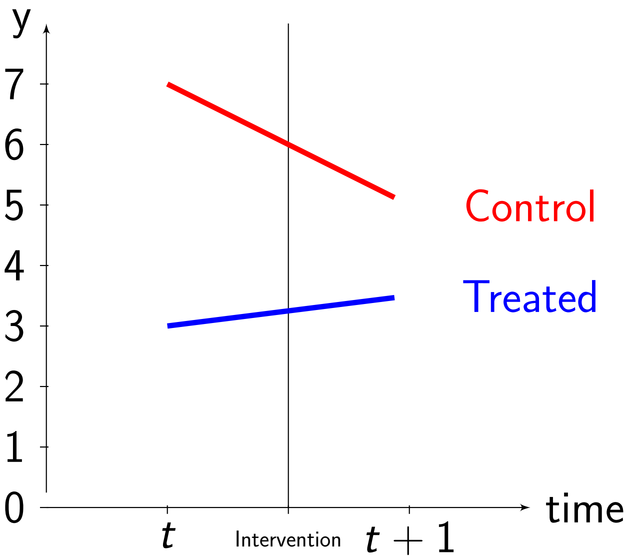
Treated
time
t
Intervention
t + 1


y
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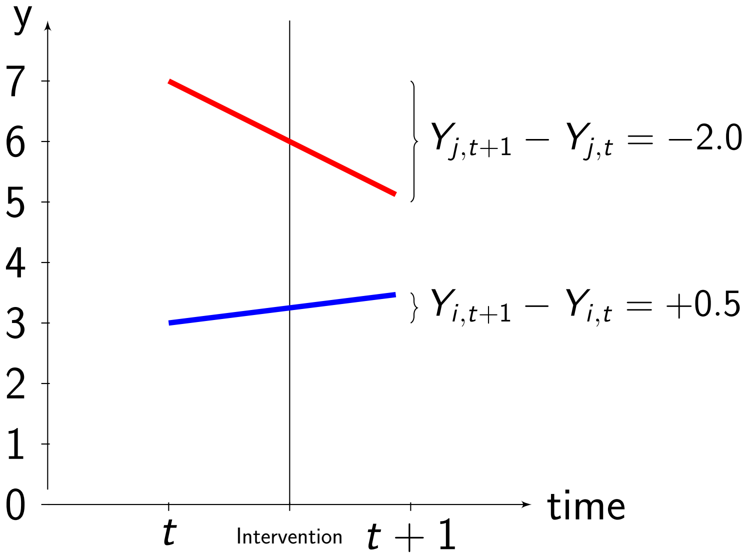
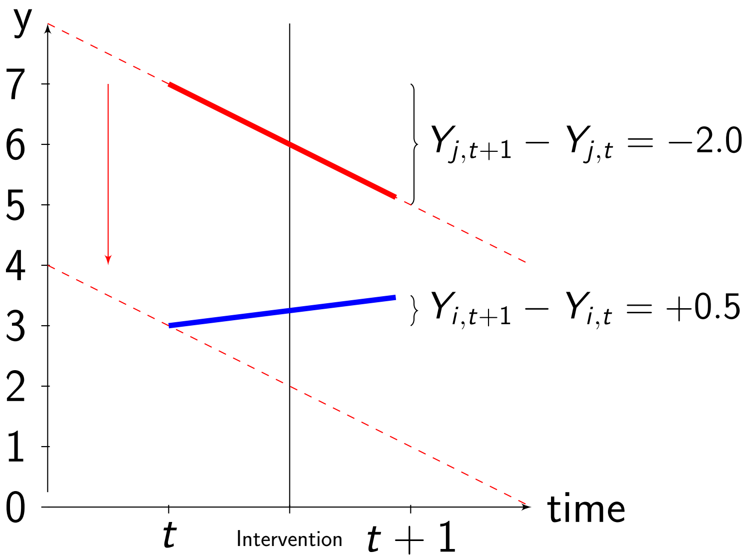
Control
Treated
time
t
Intervention
t + 1


y
7
6
5
4
3
2
1
0
$Y_{j,t+1} - Y_{j,t} = -2.0$
$Y_{i,t+1} - Y_{i,t} = +0.5$
time
t
Intervention
$t+1$


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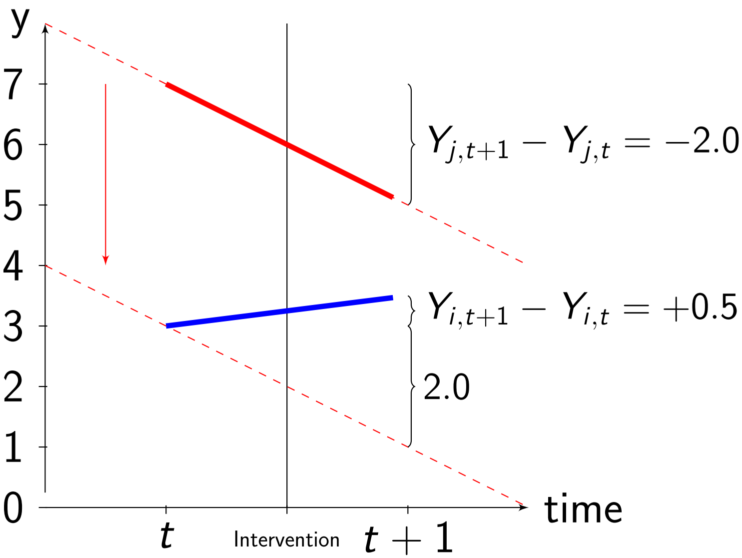
time
t
Intervention
$t + 1$



y
7
6
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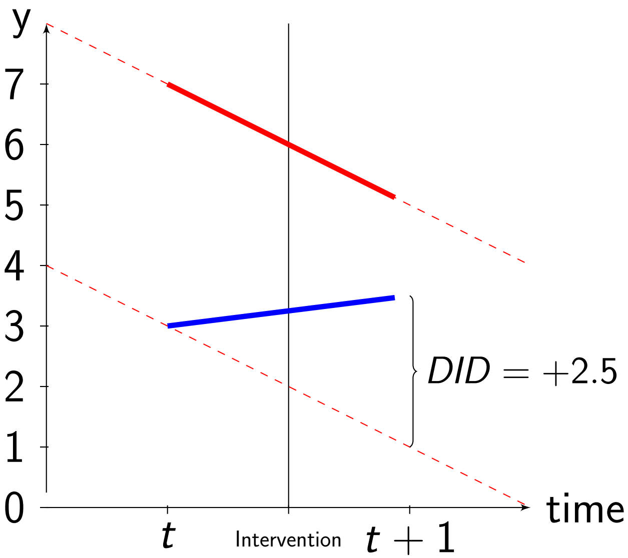
1
0
$Y_{j,t+1} - Y_{j,t} = -2.0$
$Y_{i,t+1} - Y_{i,t} = +0.5$
2.0
time
t
Intervention
$t+1$


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

Conclusion

- In **regression/matching**, we address confounding through conditioning on observable variables
- In **quasi-experiments**, we address confounding and ordering through experiment-like discontinuities or interventions that occur at specific points in time for specific subsets of units
- In **experimentation**, we solve confounding and ordering through *randomized* intervention

Preview

- Next week: The End!!