

27–28. Semester Review

Ian Lundberg

Cornell Info 6751: Causal Inference in Observational Settings
Fall 2022

28 and 30 Nov 2022

Learning goals for the semester

At the end of the semester, you will be able to:

- ▶ evaluate the credibility of causal claims
- ▶ answer causal questions in their own research
- ▶ engage with new methods for causal inference

Causal Inference in Observational Settings

Causal claims require an argument

Causal inference without models (nonparametric)

- Consistency

- Exchangeability

- Positivity

Causal inference with models (parametric)

- Outcome modeling: The parametric g-formula

- Treatment modeling: Propensity scores

- Marginal structural models

Treatments that unfold over time

- Treatments in many time periods

- Controlled direct effects

- Natural direct effects

Complexities that arise in real settings

- When treatment causes death: Principal stratification

- When variables are measured with error

When exchangeability does not hold

- Bounds

- Difference in difference

- Regression discontinuity

- Synthetic control

- Instrumental variables

Recap: Causal inference in observational settings

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Causal claims require an argument



Left photo: By Fernando Frazão/Agência Brasil -

http://agenciabrasil.ebc.com.br/sites/_agenciabrasil2013/files/fotos/1035034-_mg_0802_04.08.16.jpg, CC BY 3.0 br, <https://commons.wikimedia.org/w/index.php?curid=50548410>

Right photo: By Agencia Brasil Fotografias - EUA levam ouro na ginástica artística feminina; Brasil fica em 8 lugar, CC BY 2.0, <https://commons.wikimedia.org/w/index.php?curid=50584648>

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1. Statistical evidence

- Simone Biles swung on the uneven bars. She won a gold medal.

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	Do you win gold if you:		Causal effect
	Swing	Do not swing	of swinging
Simone Biles	Yes (1)	?	?
Ian	?	No (0)	?

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Consistency

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$Y_i^1 \quad Y_i^2$
 $Y_i^3 \quad Y_i^4$

Potential Outcomes

Consistency

$$\begin{array}{cc} Y_i^1 & Y_i^2 \\ Y_i^3 & Y_i^4 \end{array}$$

Potential Outcomes

$$Y_i$$

Factual Outcomes

Consistency

Consistency Assumption

$$Y_i^{A_i} = Y_i$$

$$\begin{matrix} Y_i^1 & Y_i^2 \\ Y_i^3 & Y_i^4 \end{matrix}$$

Potential Outcomes

$$Y_i$$

Factual Outcomes

Consistency

$$Y_i = Y_i^{A_i}$$

The factual outcome Y_i equals the potential outcome $Y_i^{A_i}$ under the factual treatment A_i ;

Requires

1. Potential outcomes are well-defined
2. Treatments are measured in observed data

Consistency Threat: Interference

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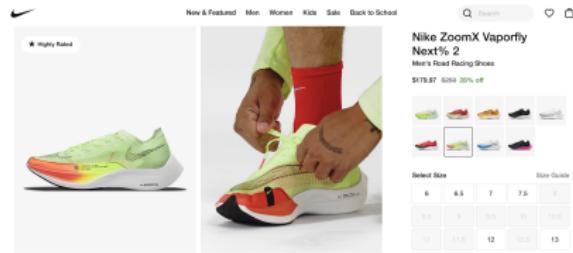


Image source: Nike

Consistency Threat: Interference

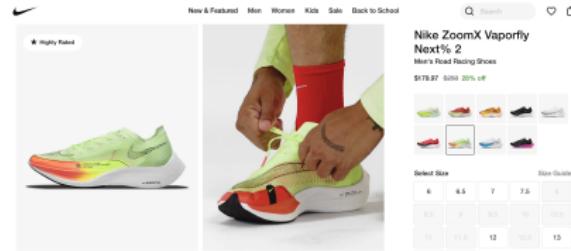


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Suppose my challenger is a tiny bit faster than me

Consistency Threat: Interference

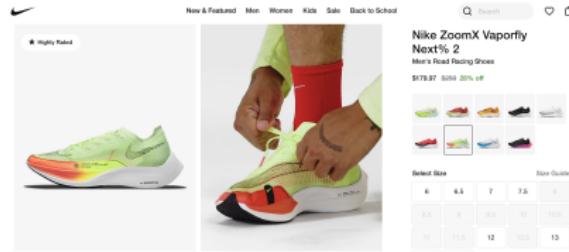


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Suppose my challenger is a tiny bit faster than me

I wear springy shoes

Challenger wears
springy shoes

Yes No

Yes

No

Consistency Threat: Interference

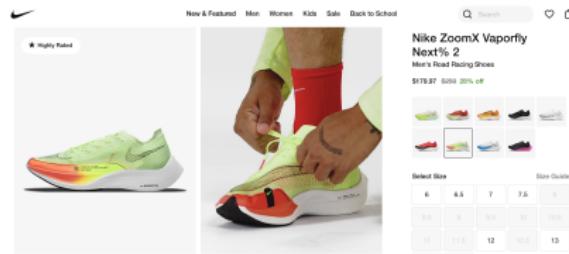


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	Yes	No
Challenger wears springy shoes	Yes	I lose I lose
	No	

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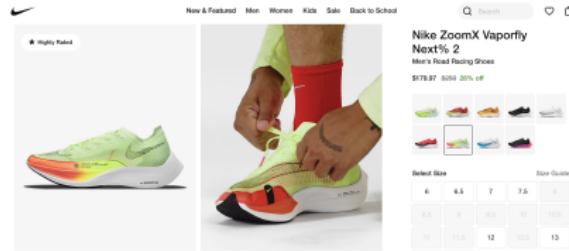


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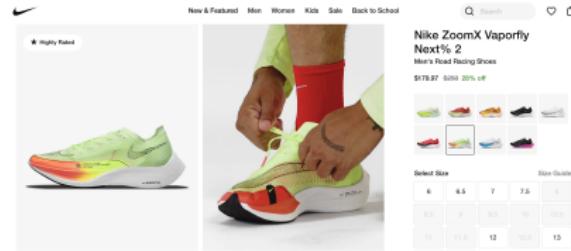


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Interference:

The outcome of unit i depends on the treatment of unit j

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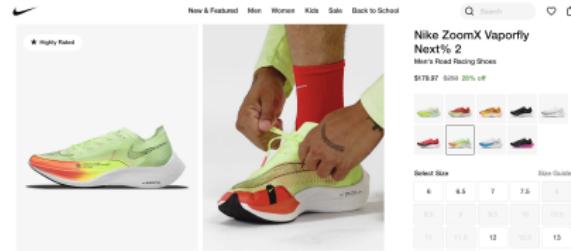


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Suppose my challenger is a tiny bit faster than me

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Interference:

The outcome of unit i depends on the treatment of unit j

$Y_i^{A_i=a}$ is undefined

Need $Y_i^{A_i=a, A_j=a'}$

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 - ▶ Pay us lots of money
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Consistency Threat: Variations of Treatment

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Consistency Threat: Variations of Treatment

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Problem: The college you attend is relevant

Formally, you can collapse treatments only if we believe treatment variation irrelevance ([VanderWeele 2009](#))

$$Y_i^{a,k_a} = Y_i^{a,k'_a} \quad \text{for all } k_a, k'_a$$

where

- ▶ a is the treatment college degree
- ▶ k_a and k'_a are versions which college

Consistency. Summary

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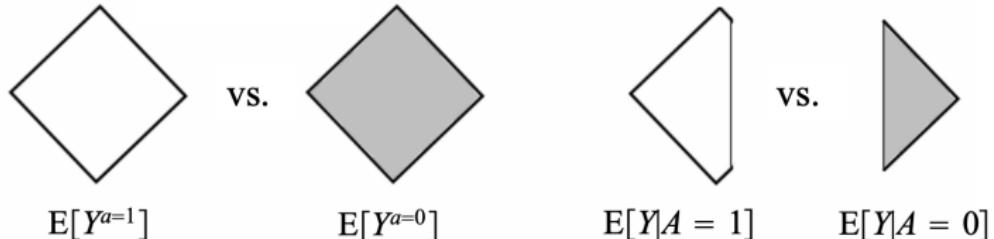
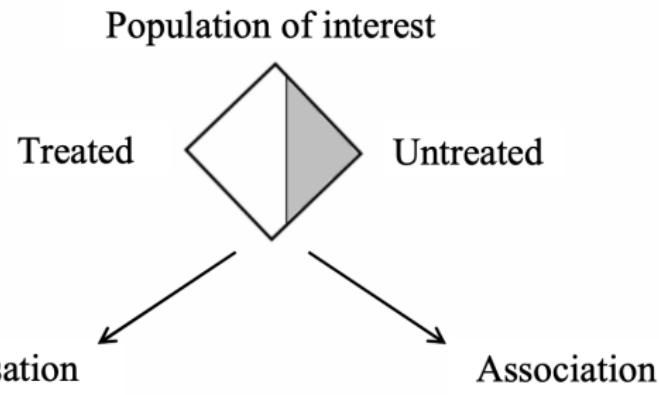
Those settings require careful definitions of the potential outcomes

Important: Consistency is a concern that **precedes** confounding

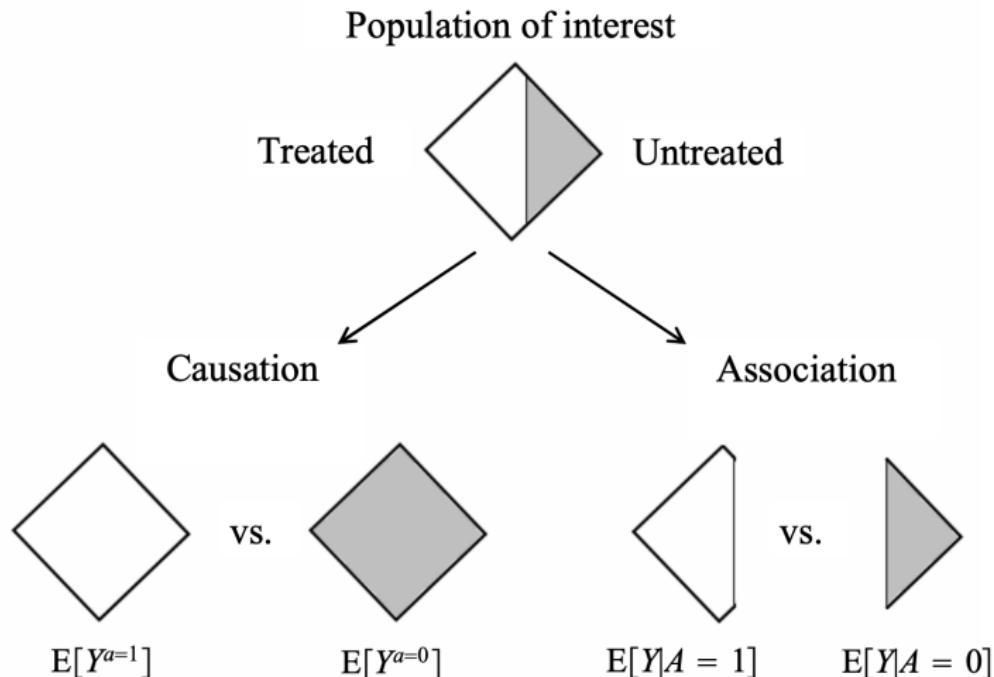
- ▶ First define the treatment and outcome
- ▶ Then move on to subsequent assumptions

Exchangeability

Exchangeability



Exchangeability



Exchangeability. Under randomization, the left equals the right:

$$E(Y^{a=1} - Y^{a=0}) = E(Y | A = 1) - E(Y | A = 0)$$

Exchangeability: In math

Exchangeability: $A \perp\!\!\!\perp \{Y^a\}$

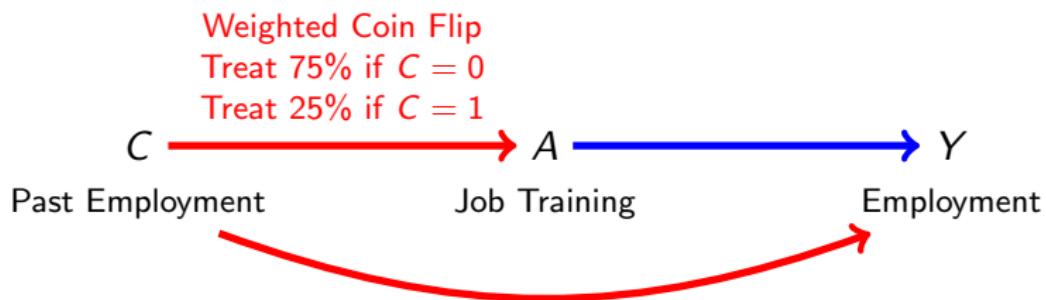
- ▶ Treatment is independent of potential outcomes
- ▶ Achieved in expectation in a simple randomized experiment

Conditional exchangeability: $A \perp\!\!\!\perp \{Y^a\} \mid \vec{L}$

- ▶ Treatment is independent of potential outcomes within strata of \vec{L}
- ▶ Achieved in expectation in a randomized experiment with unequal assignment probabilities that are a function of \vec{L}

Exchangeability: Using DAGs

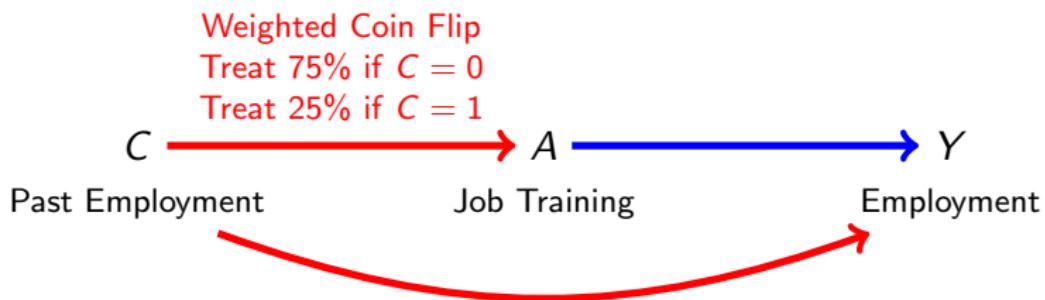
A and Y are associated through two paths



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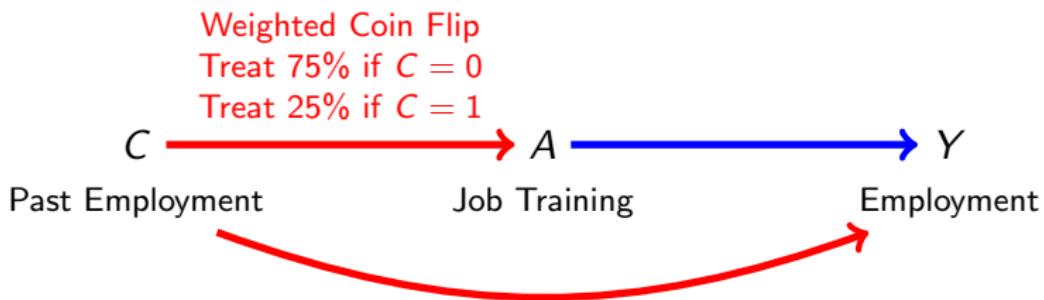
- A **causal path** $A \rightarrow Y$



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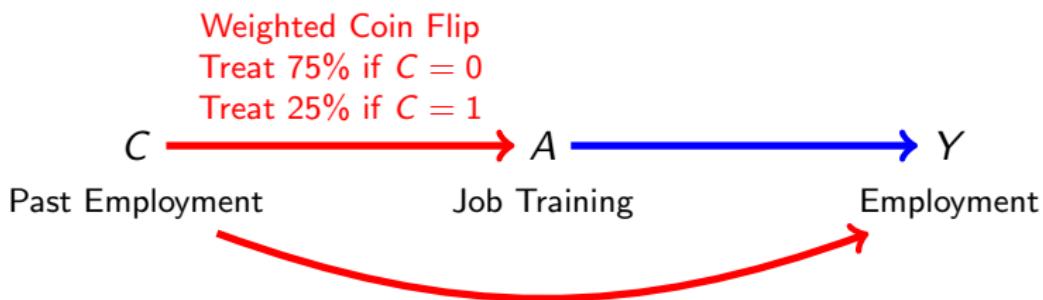
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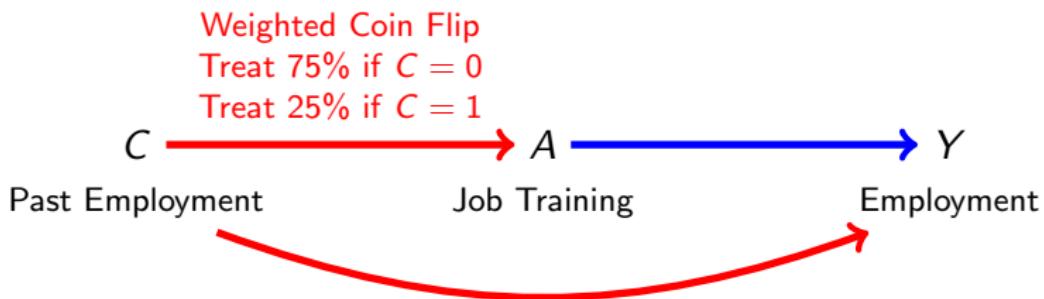
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 - ▶ Think of a house at A



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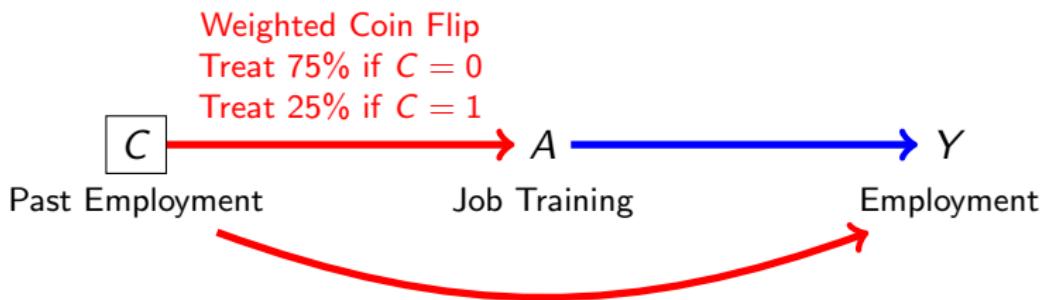
- ▶ A **causal path** $A \rightarrow Y$
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Exchangeability: Using DAGs

A and Y are associated through two paths

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- ▶ A **backdoor path** $A \leftarrow C \rightarrow Y$
 - ▶ Think of a house at A
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To block the backdoor path, condition on C

- ▶ Analyze within strata of C
- ▶ Denoted by the box

Exchangeability: Using DAGs

Three sources of association in DAGs

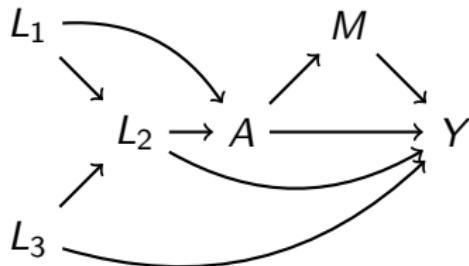
1. Causal path: $A \rightarrow Y$, $A \rightarrow M \rightarrow Y$, etc.
2. Backdoor path: $A \leftarrow L \rightarrow Y$
 - ▶ Conditioning on L blocks the path
3. Path through an adjusted collider: $A \rightarrow [C] \leftarrow Y$
 - ▶ Conditioning on C opens the path

The effect $A \rightarrow Y$ is identified if the only source of association between A and Y comes from causal paths

Exchangeability: Using DAGs

What adjustment set identifies the total effect of A on Y ?

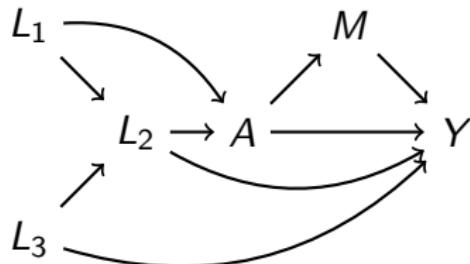
- (3 correct answers)



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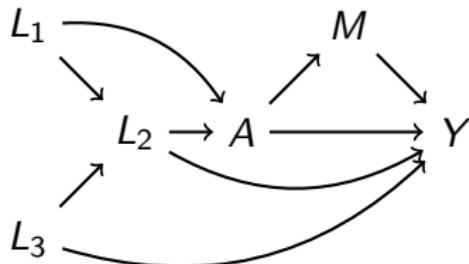


- We have to condition on L_2 to block $A \leftarrow L_2 \rightarrow Y$

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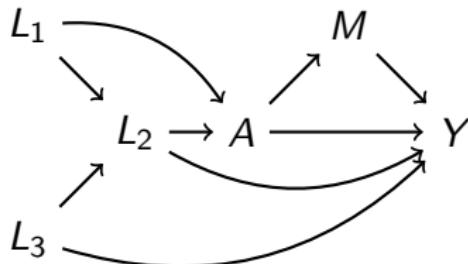


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- That opens the path $A \leftarrow L_1 \rightarrow \boxed{L_2} \leftarrow L_3 \rightarrow Y$

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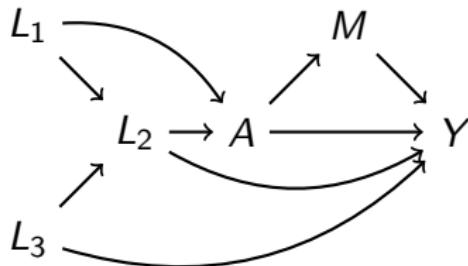


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Three sufficient sets: $\{L_1, L_2\}$, $\{L_1, L_3\}$, $\{L_1, L_2, L_3\}$

Positivity

For every value a of the treatment A
and every confounder value $\vec{\ell}$ for the sufficient adjustment set \vec{L} ,

$$P(A = a \mid \vec{L} = \vec{\ell}) > 0$$

Examples where positivity does not hold:

- ▶ $A = a$ is employment as a surgeon
 $\vec{L} = \vec{\ell}$ is completion of medical school
- ▶ $A = a$ is chewing a tough steak
 $\vec{L} = \vec{\ell}$ is being an infant with no teeth

What to do:

Redefine the causal question to only involve counterfactuals for which positivity holds

Causal inference by adjustment for confounding: Three key assumptions

1. Consistency
2. Exchangeability
3. Positivity

$$\begin{aligned} Y_i &= Y_i^{A_i} \\ A \perp\!\!\!\perp \{Y^a\} \mid \vec{L} \\ P(A = a \mid \vec{L} = \vec{\ell}) &> 0 \end{aligned}$$

Addressed in that order: each requires the previous

Nonparametric identification: A proof

Linking counterfactual to factual outcomes

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$$E(Y^a)$$

Nonparametric identification: A proof

Linking counterfactual to factual outcomes

$$E(Y^a) = \sum_{\vec{\ell}} P(\vec{L} = \vec{\ell}) E(Y^a | \vec{L} = \vec{\ell}) \quad \text{rules of probability}$$

Nonparametric identification: A proof

Linking counterfactual to factual outcomes

$$\begin{aligned} E(Y^a) &= \sum_{\vec{\ell}} P(\vec{L} = \vec{\ell}) E(Y^a \mid \vec{L} = \vec{\ell}) && \text{rules of probability} \\ &= \sum_{\vec{\ell}} P(\vec{L} = \vec{\ell}) E(Y^a \mid \vec{L} = \vec{\ell}, A = a) && \text{exchangeability} \end{aligned}$$

Nonparametric identification: A proof

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$$\begin{aligned} \mathbb{E}(Y^a) &= \sum_{\vec{\ell}} P(\vec{L} = \vec{\ell}) \mathbb{E}(Y^a \mid \vec{L} = \vec{\ell}) && \text{rules of probability} \\ &= \sum_{\vec{\ell}} P(\vec{L} = \vec{\ell}) \mathbb{E}(Y^a \mid \vec{L} = \vec{\ell}, A = a) && \text{exchangeability} \\ &= \sum_{\vec{\ell}} P(\vec{L} = \vec{\ell}) \mathbb{E}(Y \mid \vec{L} = \vec{\ell}, A = a) && \text{consistency} \end{aligned}$$

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Nonparametric identification: A proof

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Intuition

For each unit i , take the mean outcome among units who look like them $\vec{L} = \vec{\ell}_i$ but who got the treatment of interest $A = a$.

Positivity ensures that there are some such units

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Nonparametric Estimation

$\hat{E}()$ is the sample
mean in the subgroup



$$E(Y^a) = \frac{1}{n} \sum_{i=1}^n E(Y | \vec{L} = \vec{\ell}_i, A = a)$$



Parametric Estimation

$\hat{E}()$ is a prediction
from a regression model

The parametric g-formula: Simple case



Parametric model $E(Y | L, A) = \alpha + \gamma L + \beta A$

The parametric g-formula: Simple case



Parametric model $E(Y | L, A) = \alpha + \gamma L + \beta A$

Estimator for the potential outcome under treatment ($A = 1$):

$$\hat{E}(Y^1) = \frac{1}{n} \sum_{i=1}^n \left(\hat{\alpha} + \hat{\gamma} \ell_i + \hat{\beta} \times 1 \right)$$

Estimator in words:

1. Estimate the regression model
2. Change all treatment values to 1
3. Predict for everyone
4. Take the sample mean

The parametric g-formula: Any estimator

Let $\hat{f}(\vec{\ell}, a)$ be a prediction function

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Any prediction function is allowable

Hill, Jennifer L. 2011.

“Bayesian nonparametric modeling for causal inference.”

Journal of Computational and Graphical Statistics 20.1:217-240.

- ▶ Binary treatment (simulated)
- ▶ Continuous confounder X (simulated)

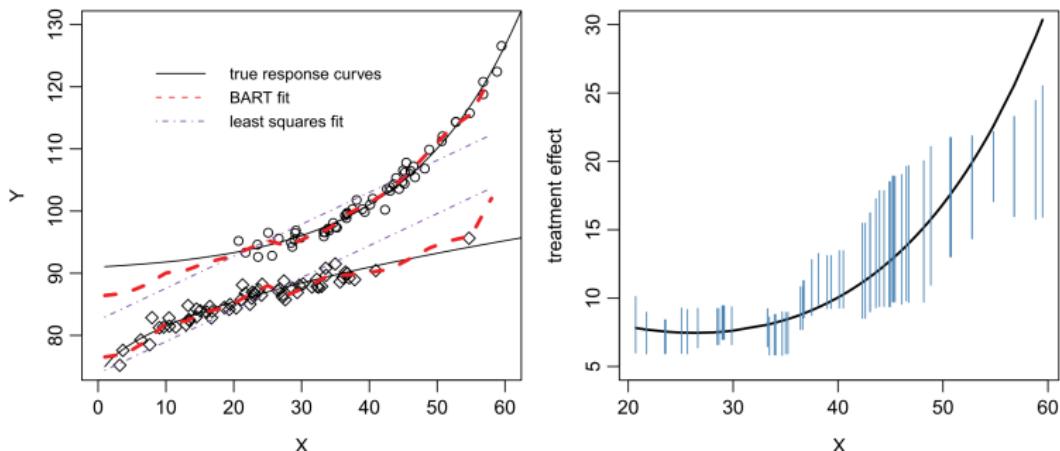


Figure 1. Left panel: simulated data with linear regression and BART fits. Right panel: BART inference for treatment effect on the treated. A color version of this figure is available in the electronic version of this article.

Automated versus Do-It-Yourself Methods for Causal Inference: Lessons Learned from a Data Analysis Competition¹

Vincent Dorie, Jennifer Hill, Uri Shalit, Marc Scott and Dan Cervone

Abstract. Statisticians have made great progress in creating methods that reduce our reliance on parametric assumptions. However, this explosion in research has resulted in a breadth of inferential strategies that both create opportunities for more reliable inference as well as complicate the choices that an applied researcher has to make and defend. Relatedly, researchers advocating for new methods typically compare their method to at best 2 or 3 other causal inference strategies and test using simulations that may or may not be designed to equally tease out flaws in all the competing methods. The causal inference data analysis challenge, “Is Your SATT Where It’s At?”, launched as part of the 2016 Atlantic Causal Inference Conference, sought to make progress with respect to both of these issues. The researchers creating the data testing grounds were distinct from the researchers submitting methods whose efficacy would be evaluated. Results from 30 competitors across the two versions of the competition (black-box algorithms and do-it-yourself analyses) are presented along with post-hoc analyses that reveal information about the characteristics of causal inference strategies and settings that affect performance. The most consistent conclusion was that methods that flexibly model the response surface perform better overall than methods that fail to do so. Finally new methods are proposed that combine features of several of the top-performing submitted methods.

Key words and phrases: Causal inference, competition, machine learning, automated algorithms, evaluation.

1. INTRODUCTION

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In the absence of a controlled randomized or natural experiment,² inferring causal effects involves the difficult task of constructing fair comparisons between ob-

Quantitative Research, Los Angeles Dodgers, Dodger Stadium, 1000 Vin Scully Ave., Los Angeles, California 90012, USA (e-mail: dcervone@gmail.com).

¹Discussed in 10.1214/18-STS684; 10.1214/18-STS680; 10.1214/18-STS690; 10.1214/18-STS689; 10.1214/18-STS679; 10.1214/18-STS682; 10.1214/18-STS688

²We use natural experiment to include (1) studies where the causal variable is randomized not for the purposes of a study (for instance, a school lottery), (2) studies where a variable is randomized but the causal variable of interest is downstream of this (e.g., plays the role of an instrumental variable), and (3) regression discontinuity designs.

The parametric g-formula by matching



For each i with $A = 1$,

- ▶ Find a unit j with $A = 0$ that minimizes

$$d(\vec{\ell}_i, \vec{\ell}_j)$$

for some distance metric d

- ▶ Impute $E(Y | \vec{L} = \vec{\ell}_i, A = 0)$ with the observed Y_j
- ▶ Estimate $\hat{\delta}_i = Y_i - Y_j$

$$\text{Estimate } \hat{E}(Y^1 - Y^0 | A = 1) = \frac{1}{n_1} \sum_{i: A_i=1} \hat{\delta}_i$$

Outcome modeling and treatment modeling

Outcome modeling



- 1) Model $\{L, A\} \rightarrow Y$
- 2) Impute unobserved Y^a

Outcome modeling and treatment modeling

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Treatment modeling



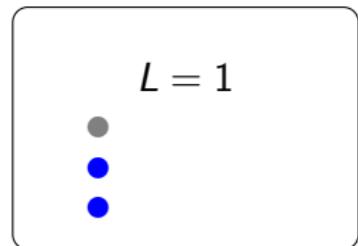
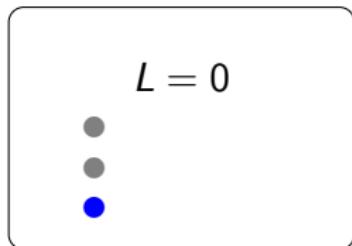
- 1) Model $L \rightarrow A$
- 2) Create a pseudo-population where $A \perp\!\!\!\perp L$
- 3) Take means in the pseudo-population

Intuition: Weight to a pseudo-population

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Factual population:

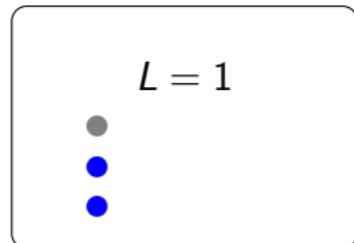
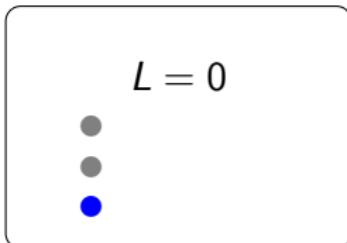
- Untreated
- Treated



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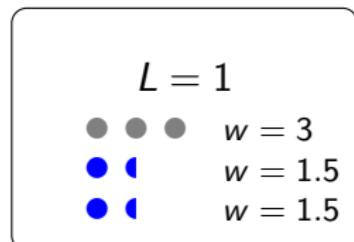
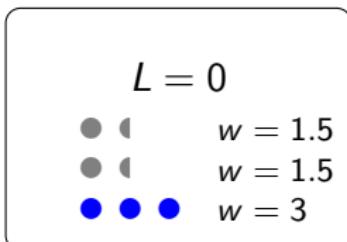
Factual population:

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Pseudo-population:

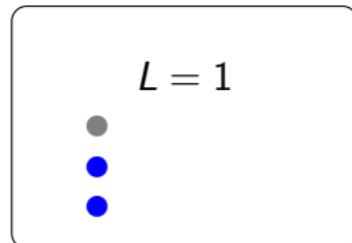
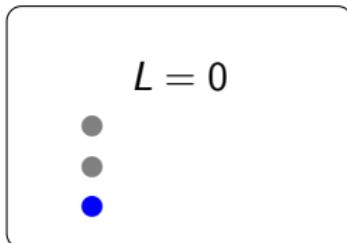
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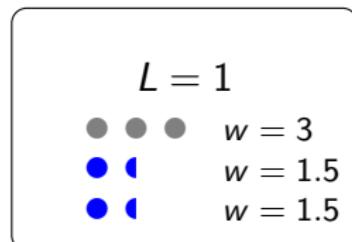
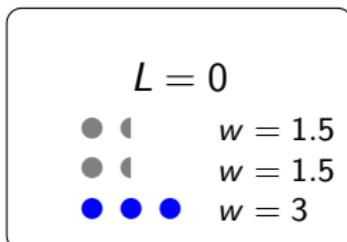
Factual population:

- Untreated
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Pseudo-population:

- Untreated
- Treated



Each weight is the inverse probability of the observed treatment:

$$w_i = \frac{1}{P(A = a_i \mid L = \ell_i)}$$

Intuition: Weight to a pseudo-population

In the factual population In the pseudo-population



Marginal structural models

In the factual population In the pseudo-population



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In the pseudo-population, estimate

$$\mathbb{E}(Y^a) = g(a)$$

for some parametric function $g()$

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In the pseudo-population, estimate

$$\mathbb{E}(Y^a) = g(a)$$

for some parametric function $g()$

Example:

$$\mathbb{E}(Y^a) = \alpha + \beta a$$

Marginal structural models

Useful when

- ▶ Treatment A contains values that are sparsely populated
 - ▶ Need to pool information from other treatment values
- ▶ You have theory about the shape of the causal response surface $E(Y^a)$
- ▶ You are able to model the treatment assignment rule $\vec{L} \rightarrow A$ to create the weights

Less useful when

- ▶ Outcome under $A = a$ is not informative about outcome under $A = a'$
- ▶ It is hard to model $P(A = a | \vec{L})$
 - ▶ Example: Continuous rather than categorical A

Causal Inference in Observational Settings

Causal claims require an argument

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- Exchangeability

- Positivity

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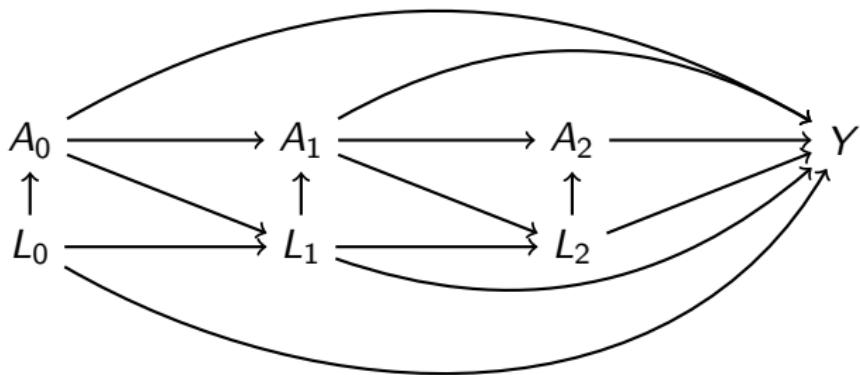
We first addressed treatments at one time point



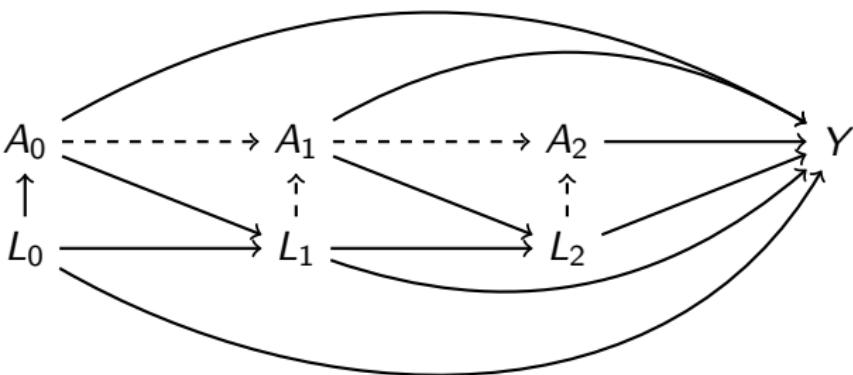
We first addressed treatments at one time point



Then, we addressed treatments at many time points



Treatments at many time points: Estimation strategy



- 1) Weight to a pseudo-population
without the dashed arrows

$$w_i = \prod_{t=0}^2 \frac{P(A_t=A_{ti})}{P(A_t=A_{ti} | \bar{L}_{t,i} \bar{A}_{t-1})}$$

weight for i

product over periods

stabilizing term

treatment probability at period t

- 2) Apply a marginal structural model

$$E(Y^{\vec{a}}) = g(\vec{a})$$

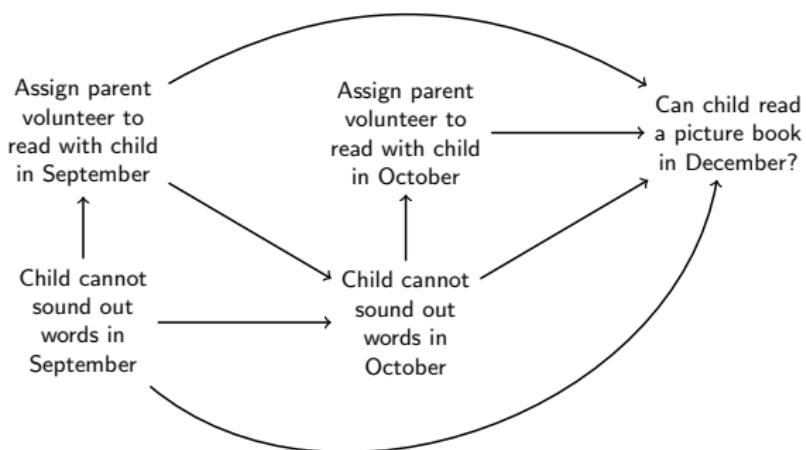
Example:

$$\alpha + \beta \left(\frac{a_0+a_1+a_2}{3} \right)$$

Step back: When would I study treatments over time?

- ▶ Treatment is assigned at many time points
- ▶ as a function of observables up to that point

Our example from class:



Mediation

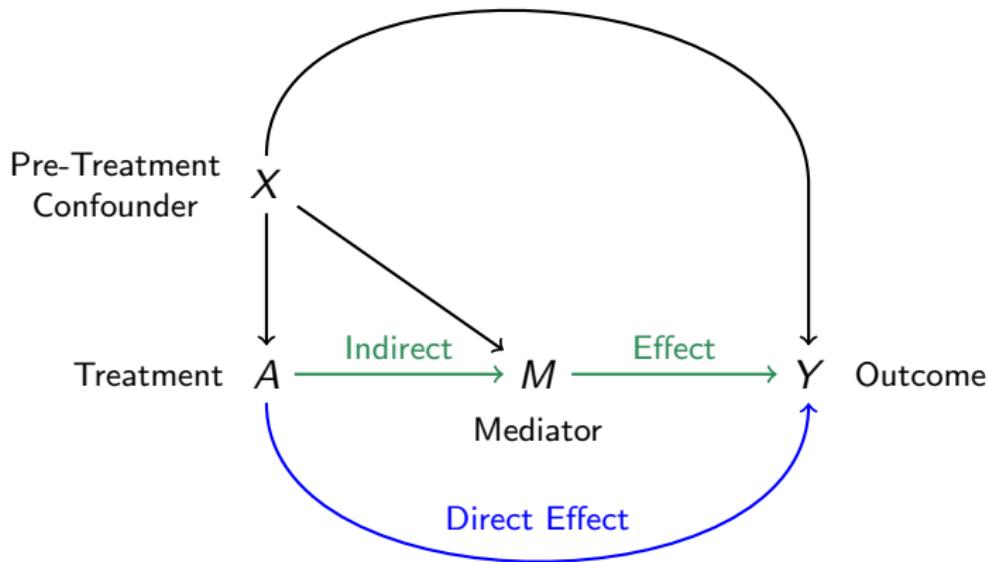
Mediation

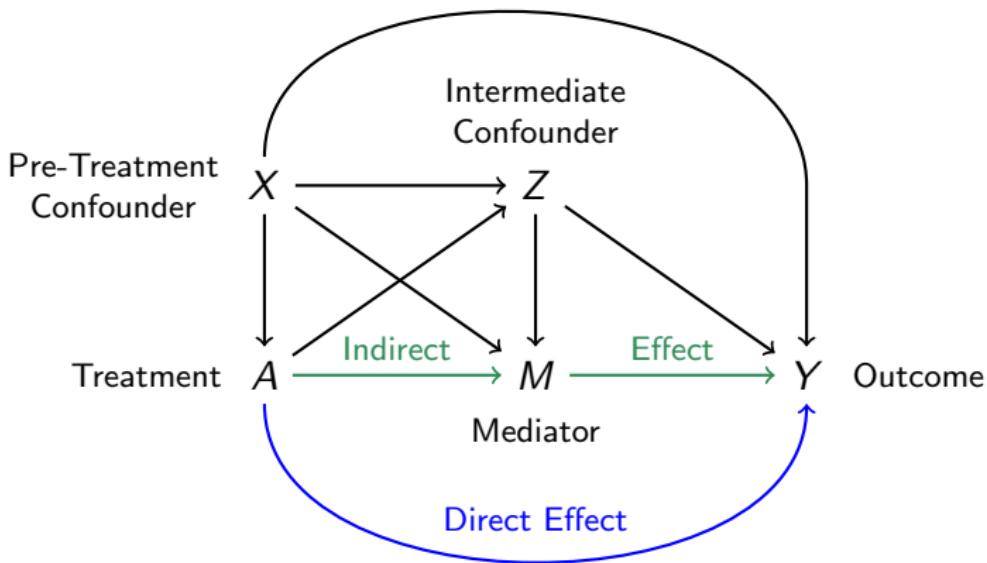
Methods to this point have emphasized total effects

- ▶ How big is the effect of A on Y ?
- ▶ In what subgroups of \vec{L} is that effect large?

Mediation asks questions about how those effects come to be

- ▶ By what causal process does A affect Y ?





Mediation: Controlled direct effects

Test for causal paths that remain after one is blocked by an intervention.

Causal claim

If the school librarian visits a class, that causes kids to read more.

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“How” statement

Happens because they become more likely to visit the school library

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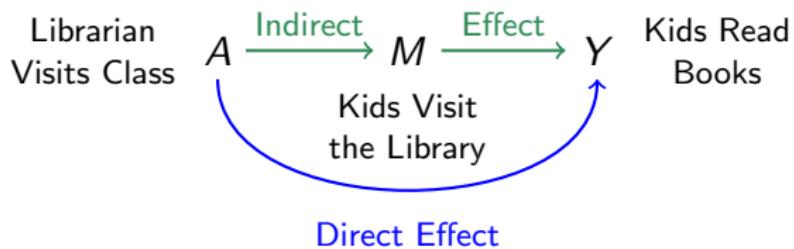
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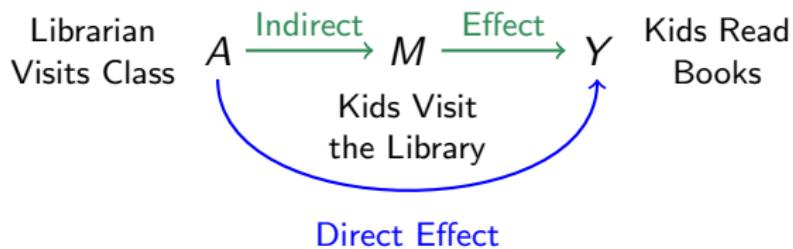
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Experimental test (0)

Close the library. Does A affect Y ?

Experimental test (1)

Mandate library visits. Does A affect Y ?

Note: Both physically manipulate M

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Decompose a total effect into direct and indirect effects

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Decompose a total effect into direct and indirect effects

Each child has a mediator value realized under each treatment

- ▶ M_i^0 : Would they visit the library if $A = 0$?
- ▶ M_i^1 : Would they visit the library if $A = 1$?

We cannot observe both mediator values.

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$$E(Y^1 - Y^0) \quad \text{Total effect}$$

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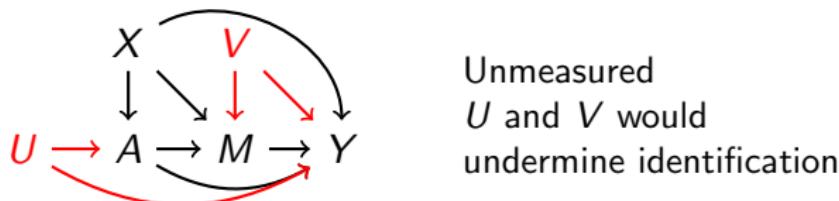
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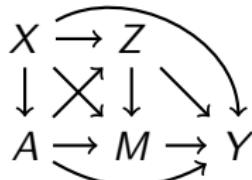
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Identification: Controlled and natural direct effects

All mediation estimands require no unmeasured confounding



CDE allows a measured intermediate confounder Z
NDE does not allow this



Examples of mediation questions

Mediation questions examine how an effect arises:
the causal process by which a treatment affects an outcome

- ▶ Zhou (2022): The effect of enrolling in college (A) on earnings (Y) only partially operates through BA completion (M)
- ▶ Wodtke & Parbst (2017): The effect of neighborhood disadvantage (A) on children's academic achievement (Y) does not operate through school poverty (M)
- ▶ Brand, Moore, Song, & Xie (2019): The effect of parental divorce (A) on educational attainment (Y) operates partially through a drop in family income (M)

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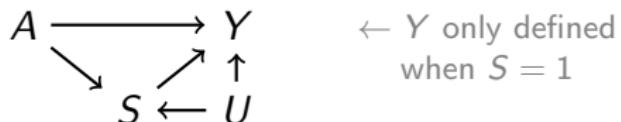
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When treatment causes death: Principal stratification



- ▶ Suppose you have a new exercise program $A = 1$ vs $A = 0$
- ▶ You study the effect on heart rate after 12 months $Y^1 - Y^0$
- ▶ But only some of the people survive $S = 1$
 - ▶ And survival was higher in your program

When treatment causes death: Principal stratification

Four principal strata

1. Would survive regardless of treatment
 - Our focal group: Effect of exercise on heart rate is well-defined!
2. Would not survive regardless of treatment
3. Would survive if and only if they receive the exercise program
4. Would survive if and only if they do not receive the program

Difficulty: We don't observe the strata.

When treatment causes death: Principal stratification

Assume **monotonicity**:

Exercise never hurts people. It only does nothing or helps.

Survivors in the control condition would have survived regardless

Survivors in the treatment condition are a mix of

- ▶ Survive regardless Proportion π_1
 - ▶ Survive if and only if treated Proportion π_3

By estimating $A \rightarrow S$, you can estimate π_1 and π_3

Then, we can set-identify a range of values for

$E(Y^1 - Y^0 \mid S = \text{Survive Regardless})$

When treatment causes death: Principal stratification

The big idea

1. Sometimes your sample is inherently selected (e.g., survival)
2. That needs to go on the DAG
3. If treatment causes selection, then you unavoidably condition on a post-treatment variable
 - Which you know causes bad problems!
4. Principal stratification is the tool to set-identify an effect in a pre-treatment subgroup (e.g., survive regardless) for whom the selection variable is not post-treatment



When variables are measured with error

When variables are measured with error,
the DAG should include the true variable X
and the measured variable X^*

When variables are measured with error

Hernan & Cole (2009) Fig 2

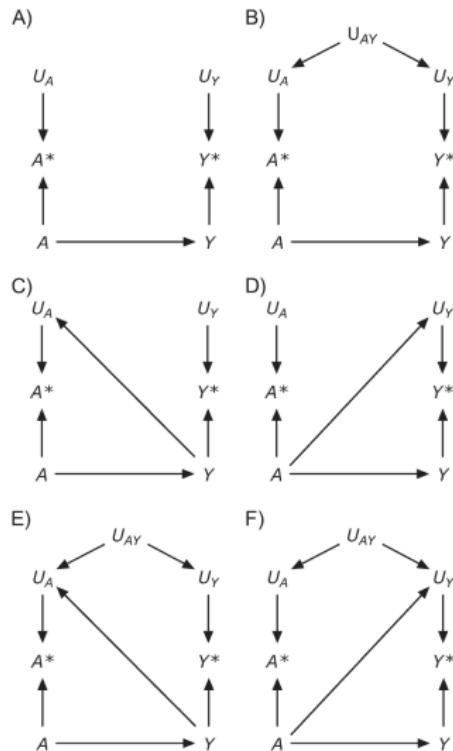


Figure 2. A structural classification of measurement error.

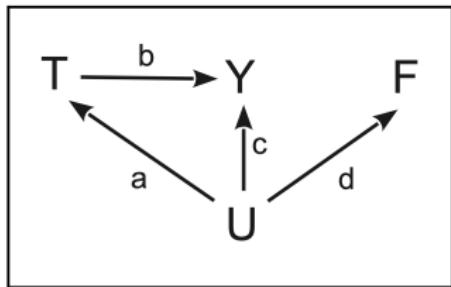
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In some cases, you can make strong assumptions to achieve identification despite measurement error

When variables are measured with error

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One example: [Elwert & Pfeffer 2022](#)

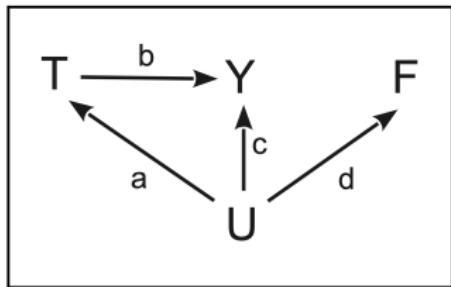


$$E(Y | T, F) = \alpha + \beta T + \gamma F$$

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Difference estimator

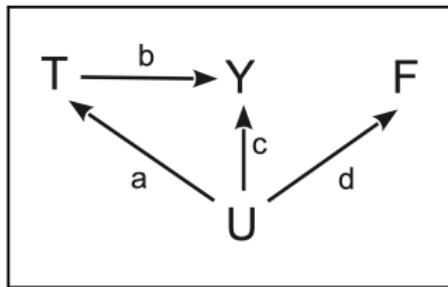
$$\beta - \gamma = b + \underbrace{ac}_{\substack{\text{Bias} \\ \text{without} \\ \text{control}}} \underbrace{\frac{a - d}{a - a^2 d}}_{\substack{\text{Bias} \\ \text{Multiplier} \\ |M|}}$$

Eliminates bias in special case

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Eliminates bias in special case

Control estimator

$$\beta = b + \underbrace{\frac{ac}{1 - a^2 d^2}}_{\substack{\text{Bias} \\ \text{without} \\ \text{control}}} \underbrace{\frac{1 - d^2}{1 - a^2 d^2}}_{\substack{\text{Multiplier} \\ |M| < 1}}$$

Always reduces bias

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When exchangeability does not hold: Bounds

You will meet critics who do not believe exchangeability.
You will want to show them bounds!

Setting:

- ▶ A binary treatment
- ▶ Y bounded outcome in range $y_{\text{Min}} \leq Y \leq y_{\text{Max}}$

To simplify notation,

- ▶ Let $\hat{y}_a = \hat{E}(Y | A = a)$ be the sample mean of Y among those with $A = a$
- ▶ Let $\hat{\pi}_a = \hat{P}(A = a)$ be the sample proportion with $A = a$

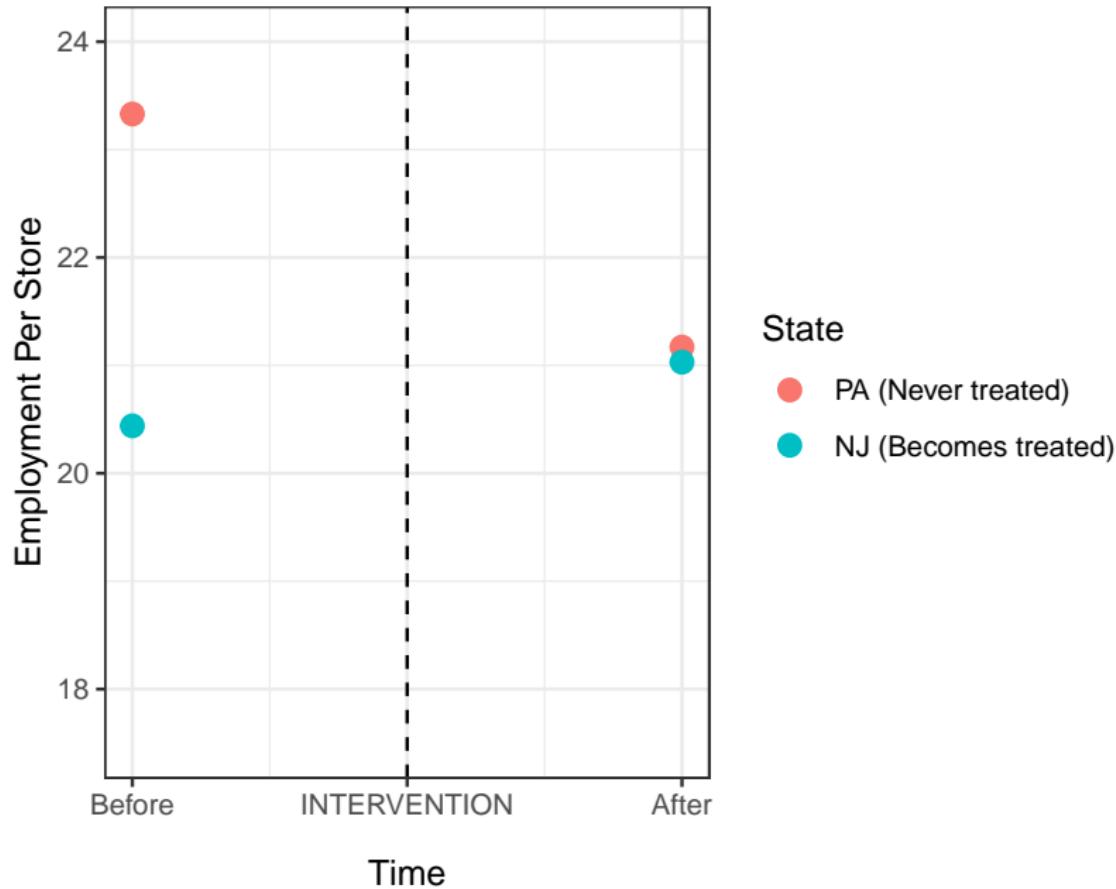
$$\hat{E}^{\text{LowerBound}}(Y^a) = \hat{\pi}_a \hat{y}_a + (1 - \hat{\pi}_a) y_{\text{Min}}$$

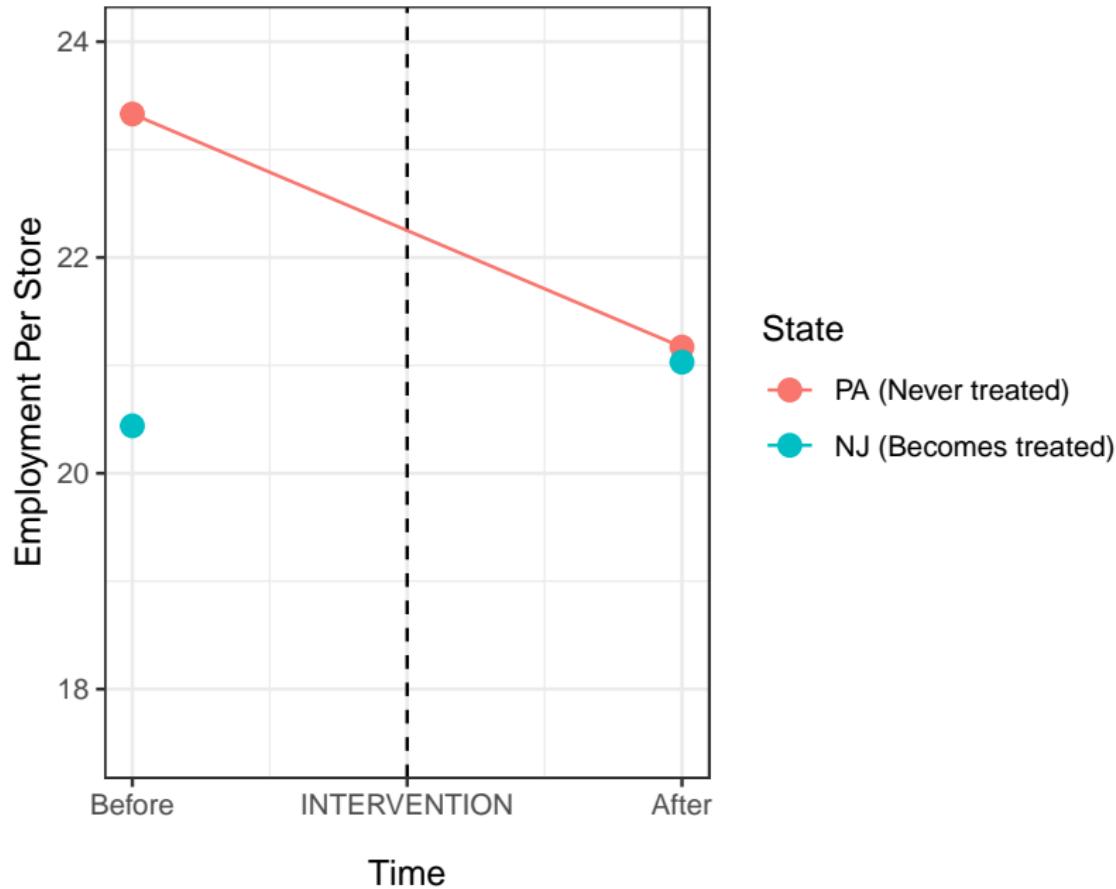
$$\hat{E}^{\text{UpperBound}}(Y^a) = \hat{\pi}_a \hat{y}_a + (1 - \hat{\pi}_a) y_{\text{Max}}$$

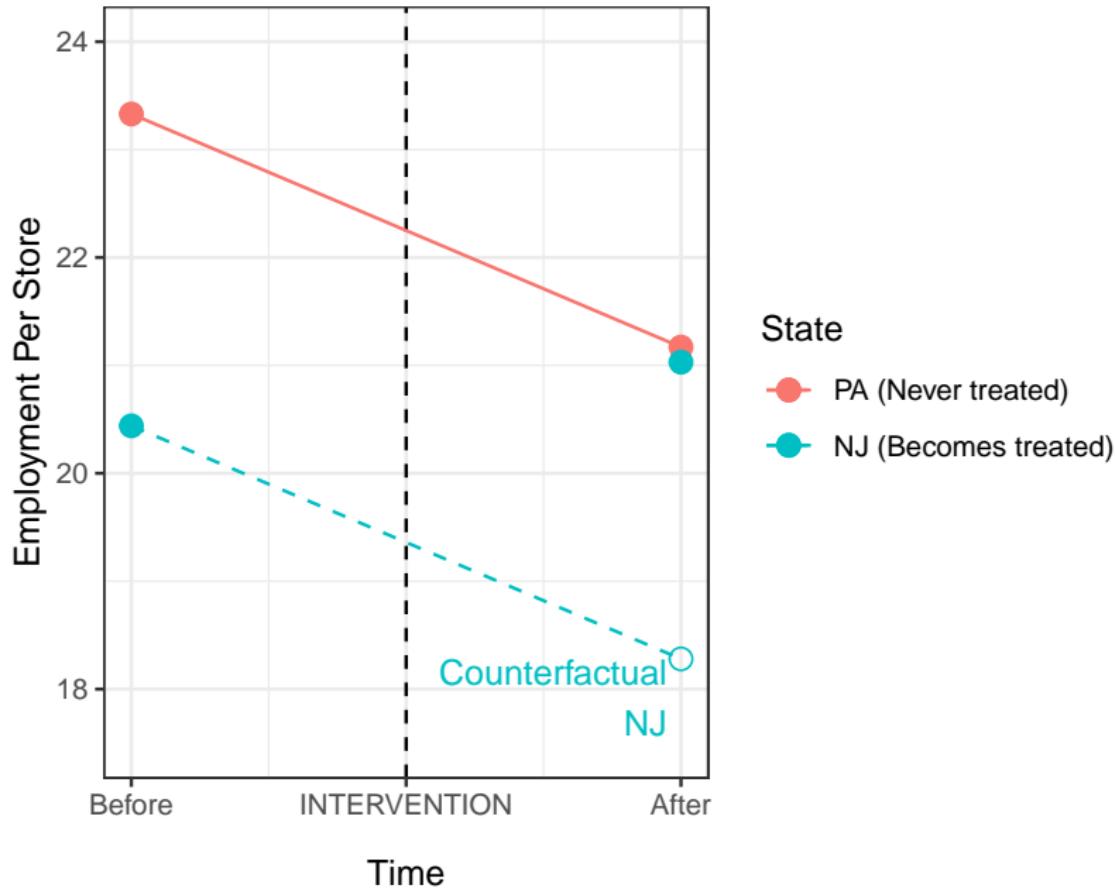
Difference in difference

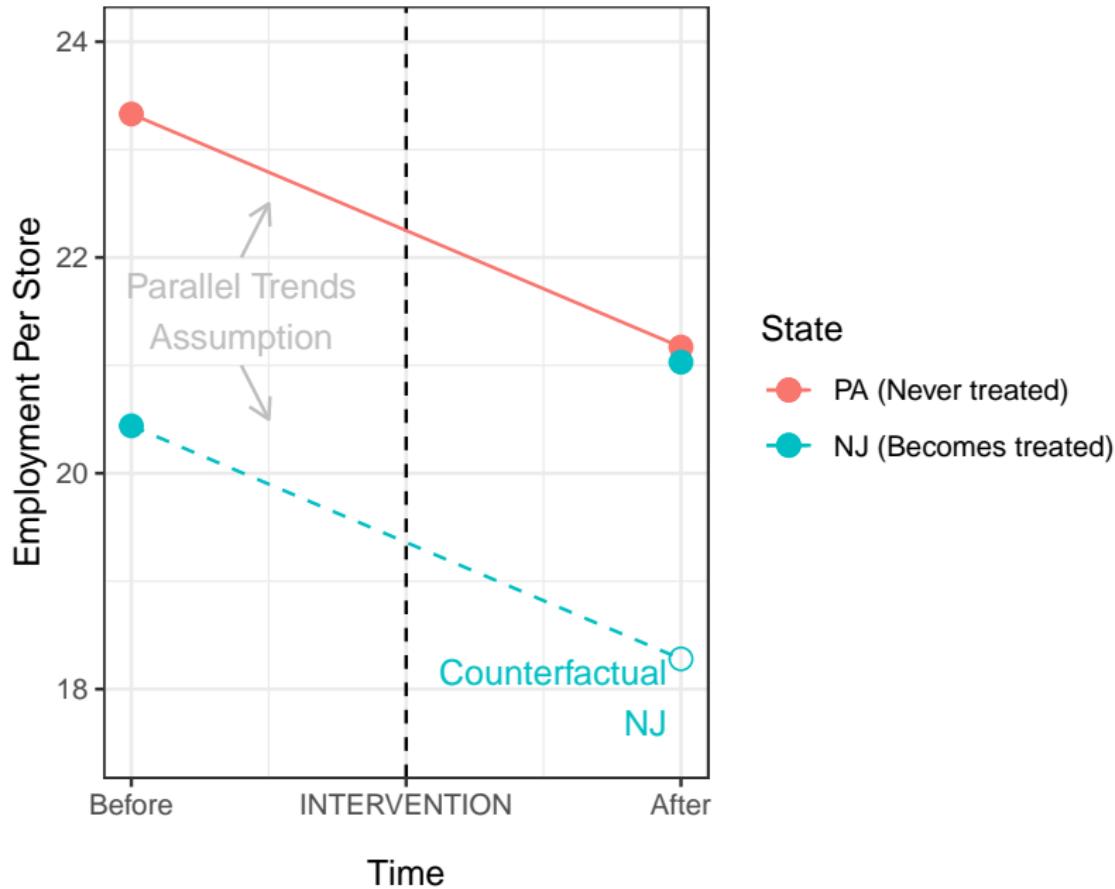


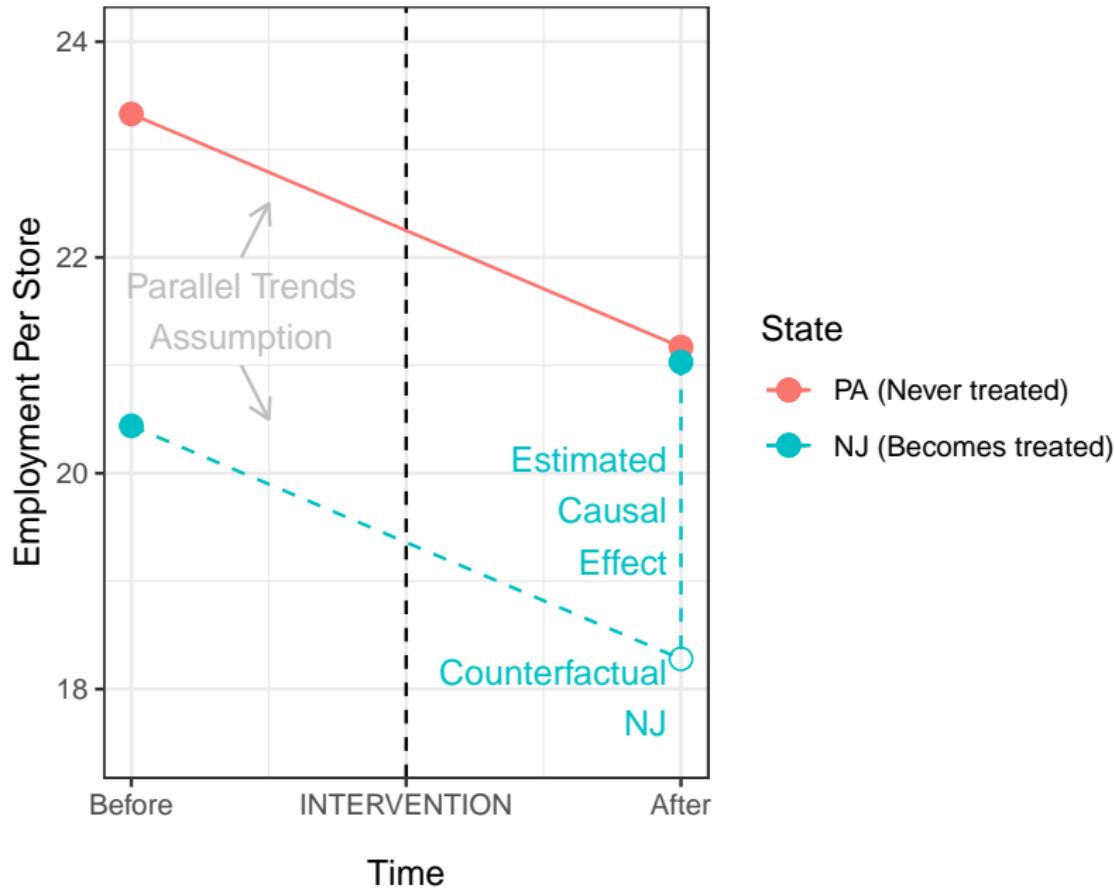
Photo by James Loesch - <https://www.flickr.com/photos/jal33/49113053632/>
CC BY 2.0, <https://commons.wikimedia.org/w/index.php?curid=87207834>





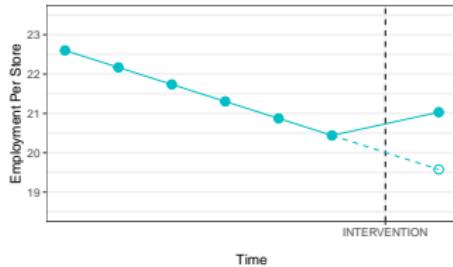






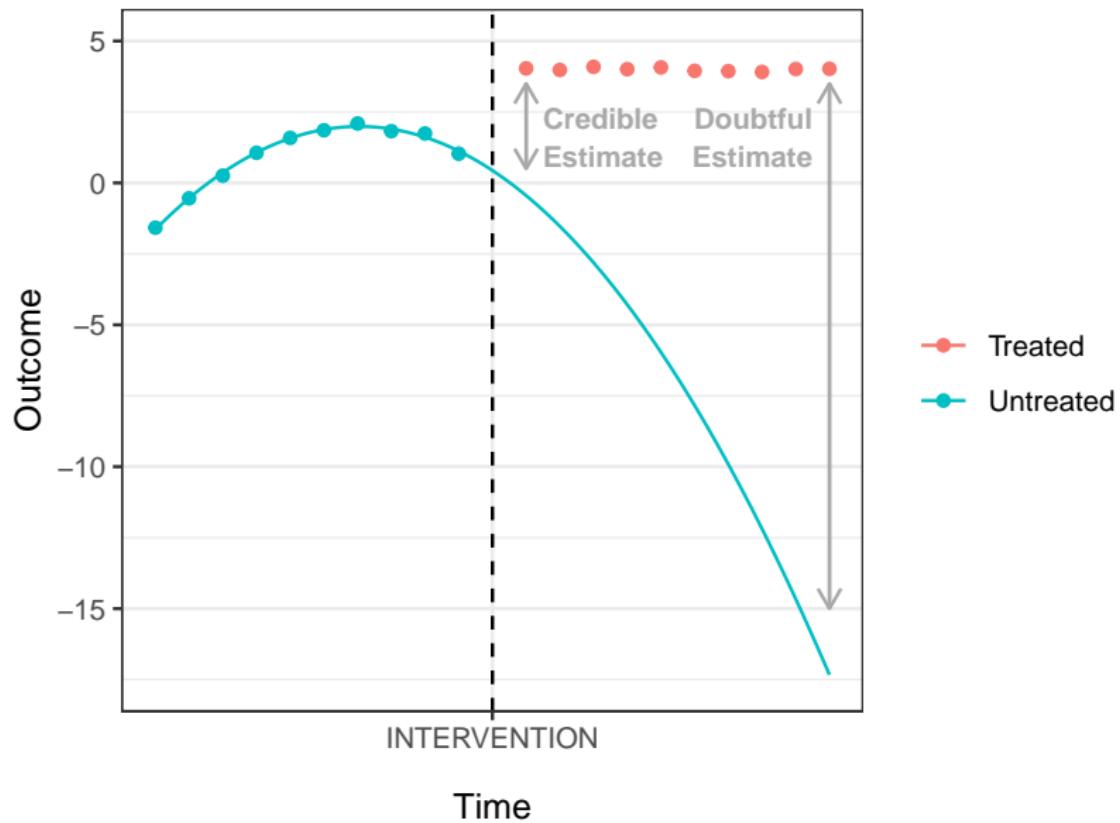
Interrupted time series¹

You study one unit. It is untreated. Then it is treated.



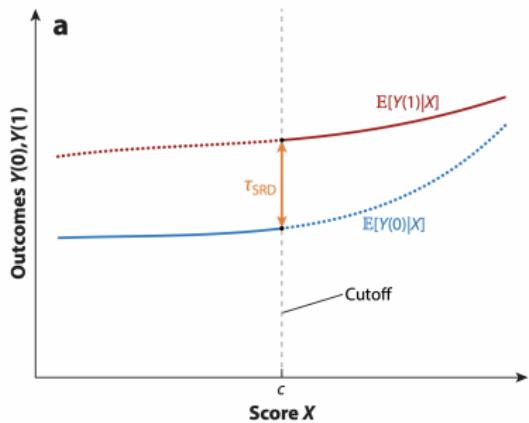
¹Bernal, J. L., Cummins, S., & Gasparrini, A. (2017). [Interrupted time series regression for the evaluation of public health interventions: A tutorial](#). International Journal of Epidemiology, 46(1), 348-355.

Interrupted time series: When it becomes doubtful



Regression discontinuity²

Cattaneo & Titiunik 2022 Fig 1a



Theoretical Estimand
 $E(Y(1) - Y(0) | X = c)$

Empirical Estimand

$$\lim_{x \downarrow c} E(Y | X = x)$$

—

$$\lim_{x \uparrow c} E(Y | X = x)$$

Identifying Assumptions

$$E(Y(1) | X = x)$$
 and

$$E(Y(0) | X = x)$$
 are

continuous at $x = c$

and $f_X(x) > 0$ for x near c

²Cattaneo, M. D., & Titiunik, R. (2022). [Regression discontinuity designs](#). Annual Review of Economics, 14, 821-851.

Synthetic control³

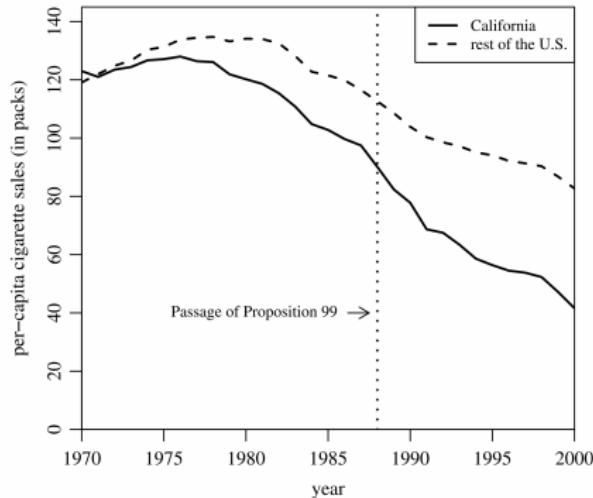


Figure 1. Trends in per-capita cigarette sales: California vs. the rest of the United States.

Can't use RD

- Effect at 1988 not of interest

Can't use ITS

- Hard to extrapolate Y^0 trend

Can't use DID

- No other state like CA

Idea: Create a
synthetic CA
to estimate
 $Y_{CA,t}^0$ for $t \geq 1988$

³Abadie, A., Diamond, A., & Hainmueller, J. (2010). *Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program*. Journal of the American Statistical Association, 105(490), 493-505.

Synthetic control⁴

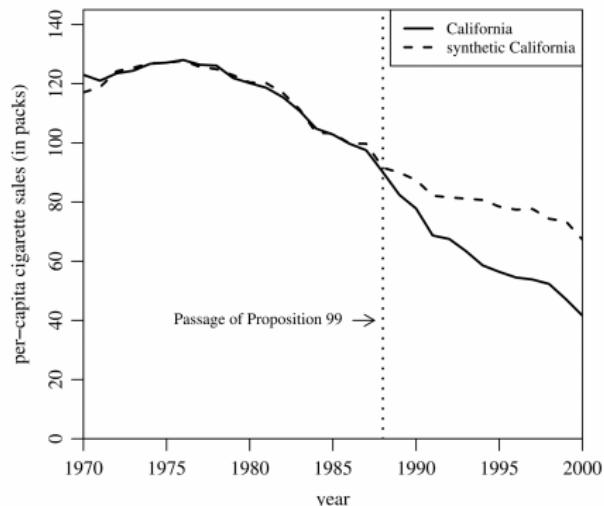
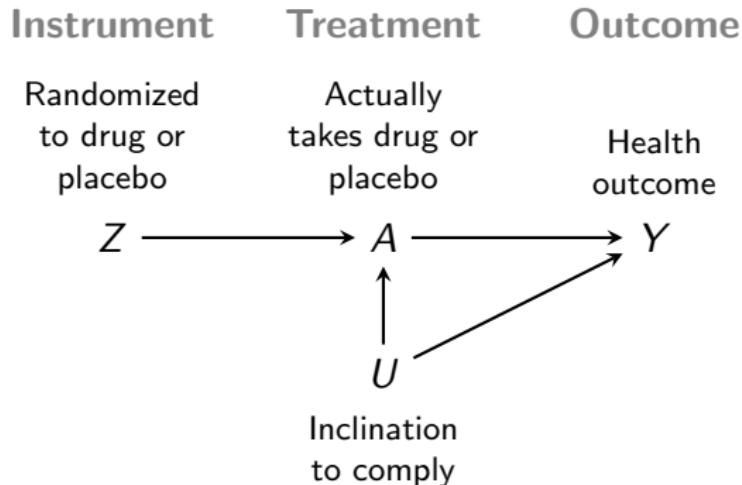


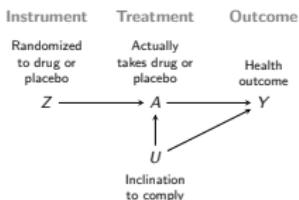
Figure 2. Trends in per-capita cigarette sales: California vs. synthetic California.

⁴Abadie, A., Diamond, A., & Hainmueller, J. (2010). *Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program*. Journal of the American Statistical Association, 105(490), 493-505.

Instrumental variables: Experiment with noncompliance



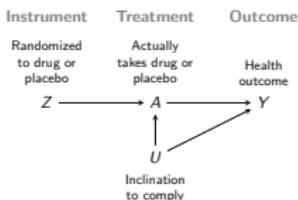
Instrumental variables



Four principal strata

Compliers	$(Z \rightarrow A) = +1$	$(Z \rightarrow Y) = (A \rightarrow Y)$
Always takers	$(Z \rightarrow A) = 0$	$(Z \rightarrow Y) = 0$
Never takers	$(Z \rightarrow A) = 0$	$(Z \rightarrow Y) = 0$
Defiers	$(Z \rightarrow A) = -1$	$(Z \rightarrow Y) = -(A \rightarrow Y)$

Instrumental variables

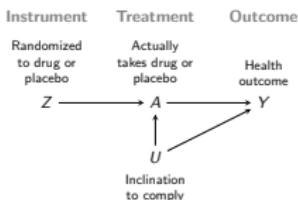


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- The effect $Z \rightarrow Y$ arises entirely from compliers

Instrumental variables

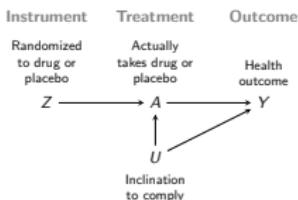


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- The effect $Z \rightarrow Y$ arises entirely from compliers
- The effect $Z \rightarrow A$ tells us how many compliers there are

Instrumental variables



Four principal strata

Compliers	$(Z \rightarrow A) = +1$	$(Z \rightarrow Y) = (A \rightarrow Y)$
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- The effect $Z \rightarrow Y$ arises entirely from compliers
- The effect $Z \rightarrow A$ tells us how many compliers there are

$$E(Y^{a=1} - Y^{a=0} | S = \text{Complier}) = \frac{E(Y^{z=1} - Y^{z=0})}{E(A^{z=1} - A^{z=0})}$$

Causal Inference in Observational Settings

Causal claims require an argument

Causal inference without models (nonparametric)

- Consistency

- Exchangeability

- Positivity

Causal inference with models (parametric)

- Outcome modeling: The parametric g-formula

- Treatment modeling: Propensity scores

- Marginal structural models

Treatments that unfold over time

- Treatments in many time periods

- Controlled direct effects

- Natural direct effects

Complexities that arise in real settings

- When treatment causes death: Principal stratification

- When variables are measured with error

When exchangeability does not hold

- Bounds

- Difference in difference

- Regression discontinuity

- Synthetic control

- Instrumental variables

Recap: Causal inference in observational settings

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Recap: Causal inference in observational settings

Randomized experiments are the gold standard in causal inference,
and for many good reasons



Phase 3 Clinical Trial of Investigational Vaccine for COVID- 19 Begins

Multi-Site Trial to Test Candidate Developed by
Moderna and NIH

July 27, 2020

5

⁵Published 27 July 2020. <https://www.niaid.nih.gov/news-events/phase-3-clinical-trial-investigational-vaccine-covid-19-begins>

An experimental protocol clarifies the causal estimand

- ▶ Target population ✓
- ▶ Well-defined contrast between treatments ✓
- ▶ Well-defined outcome after follow-up period ✓
- ▶ Attrition is transparent ✓
- ▶ Assumptions (randomized design) support causal claims ✓

⁶Hernán, M. A., & Robins, J. M. (2016). Using big data to emulate a target trial when a randomized trial is not available. American Journal of Epidemiology, 183(8), 758-764.

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An observational study cannot randomize.

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An experimental protocol clarifies the causal estimand

- ▶ Target population ✓
- ▶ Well-defined contrast between treatments ✓
- ▶ Well-defined outcome after follow-up period ✓
- ▶ Attrition is transparent ✓
- ▶ Assumptions (randomized design) support causal claims ✓

An observational study cannot randomize.

Otherwise, an observational study can do all of the above.⁶

⁶Hernán, M. A., & Robins, J. M. (2016). Using big data to emulate a target trial when a randomized trial is not available. American Journal of Epidemiology, 183(8), 758-764.

A few key principles for causal inference in observational settings

When you **evaluate a causal claim**, ask yourself

1. What is the unit of analysis?
2. What is the treatment?
3. Do the potential outcomes make sense? consistency
4. Can I draw the DAG? exchangeability
5. Are all treatment values plausible? positivity

When you answer causal questions in **your own research**, consider

1. What is my causal contrast?
 - ▶ What would it mean to assign someone to my treatment?
2. What is my target population?
3. What assumptions do I need for inference?
 - ▶ If these assumptions, then this causal conclusion
 - ▶ The if-then statement should be airtight!
 - ▶ Then argue for those assumptions

When you **engage with new methods**, ask yourself

1. Counterfactuals always require assumptions.
What assumption does this method make?
2. In what settings would this method perform well?
3. In what settings would this method perform poorly?
4. How does this method invoke key concepts I already know?

Learning goals for the semester

At the end of the semester, you will be able to:

- ▶ evaluate the credibility of causal claims
- ▶ answer causal questions in their own research
- ▶ engage with new methods for causal inference

What comes next?

- ▶ Comments on two peers' proposals due 5pm Monday Dec 5
- ▶ Course evaluations
- ▶ Go out and conduct causal research!