

# Adjustment Criteria for Generalizing Experimental Findings

Juan D. Correa, Jin Tian and Elias Bareinboim

**PURDUE**  
UNIVERSITY®

IOWA STATE  
UNIVERSITY

**ICML | 2019**

Long Beach, CA

# Causal Effects and Experiments

# Causal Effects and Experiments

- Science is about understanding the laws of nature, which are usually expressed in terms of [cause and effect relationships](#).

# Causal Effects and Experiments

- Science is about understanding the laws of nature, which are usually expressed in terms of **cause and effect relationships**.
- **Controlled experimentation** is the pillar on top of which empirical science is built upon.

# Causal Effects and Experiments

- Science is about understanding the laws of nature, which are usually expressed in terms of **cause and effect relationships**.
- **Controlled experimentation** is the pillar on top of which empirical science is built upon.
- Dozens of billions of dollars are spent every year in performing controlled experiments in the context of the empirical sciences (health sciences, economics, social sciences).

# Causal Effects and Experiments

- Science is about understanding the laws of nature, which are usually expressed in terms of **cause and effect relationships**.
- **Controlled experimentation** is the pillar on top of which empirical science is built upon.
- Dozens of billions of dollars are spent every year in performing controlled experiments in the context of the empirical sciences (health sciences, economics, social sciences).
- Inferring and reasoning with causal relations are central for decision-making, explainability, and reinforcement learning.

# Motivating Example (1)

(Why is this problem non-trivial?)

# Motivating Example (1)

## (Why is this problem non-trivial?)

[Greenhouse et al. 2008] In the context of pediatric patients treated with antidepressant:

# Motivating Example (1)

## (Why is this problem non-trivial?)

[Greenhouse et al. 2008] In the context of pediatric patients treated with antidepressant:

- The FDA was interested in assessing the effect of antidepressant drugs on suicidality.

# Motivating Example (1)

## (Why is this problem non-trivial?)

[Greenhouse et al. 2008] In the context of pediatric patients treated with antidepressant:

- The FDA was interested in assessing the effect of antidepressant drugs on suicidality.
- Historically, drugs had been prescribed by doctors taking into account background information of the patients and the assessment of their baseline risk.

# Motivating Example (1)

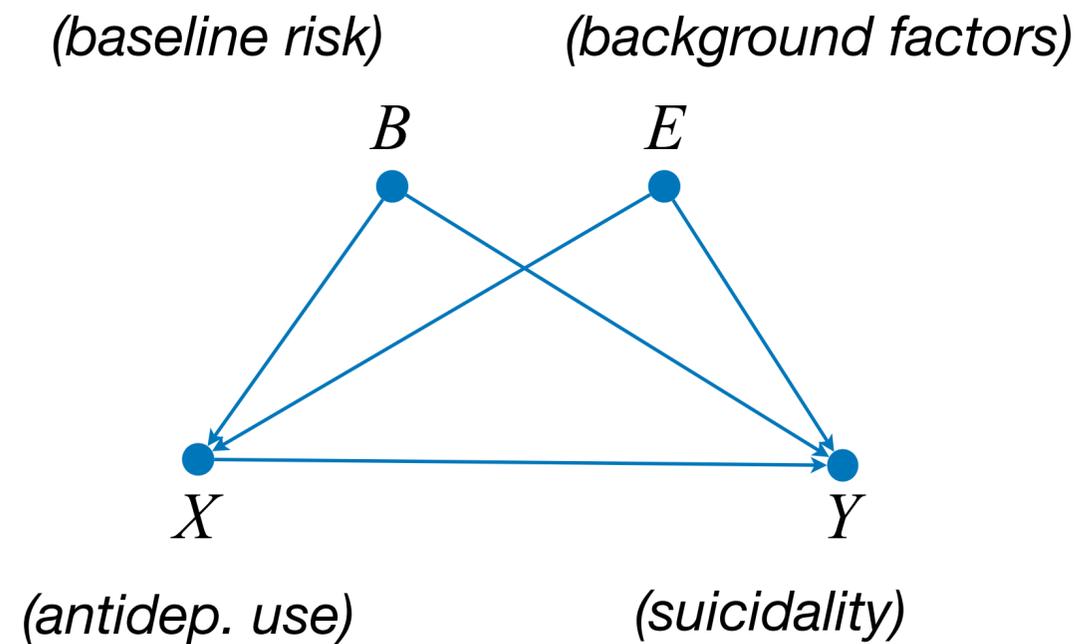
## (Why is this problem non-trivial?)

[Greenhouse et al. 2008] In the context of pediatric patients treated with antidepressant:

- The FDA was interested in assessing the effect of antidepressant drugs on suicidality.
- Historically, drugs had been prescribed by doctors taking into account background information of the patients and the assessment of their baseline risk.
- Since the prescription and the outcome are both affected by the background factors, a controlled experiment is used to identify the unconfounded effect of the antidepressants.

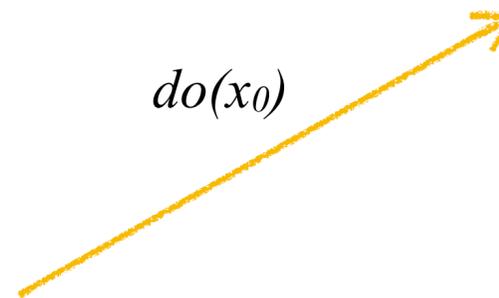
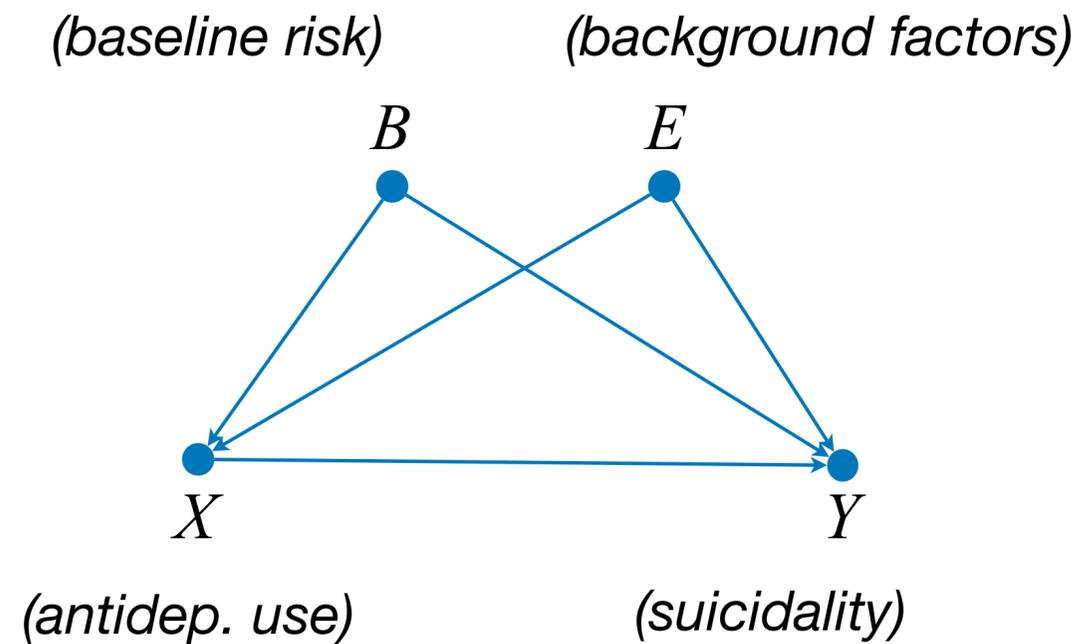
# Controlled Experimentation — Randomization

Natural world (confounded)

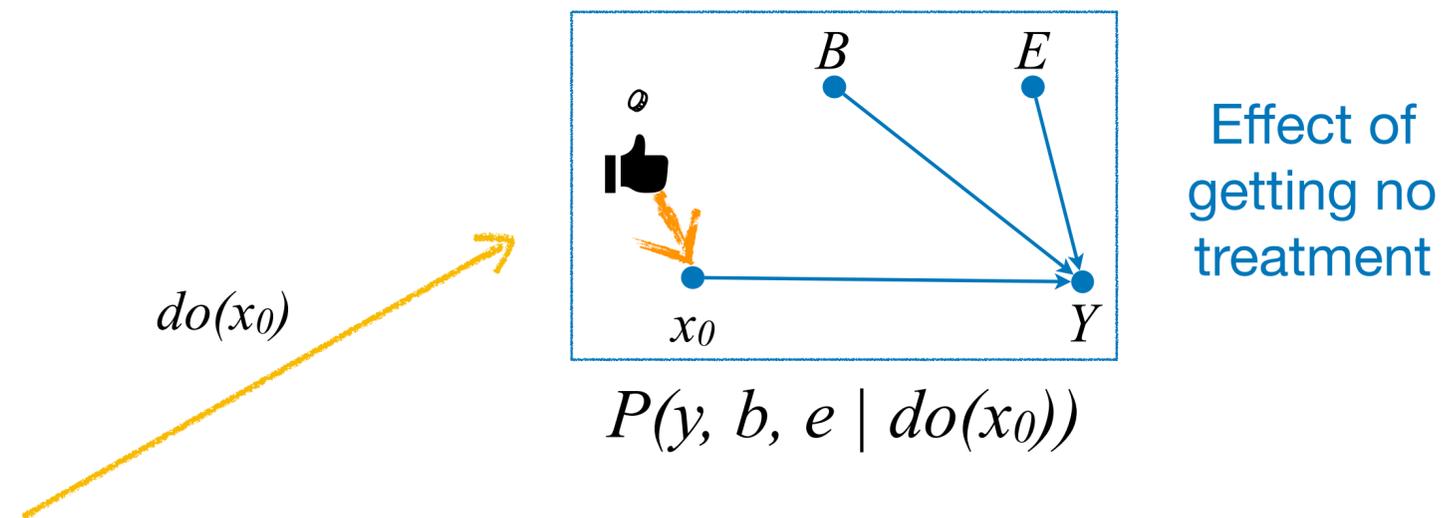
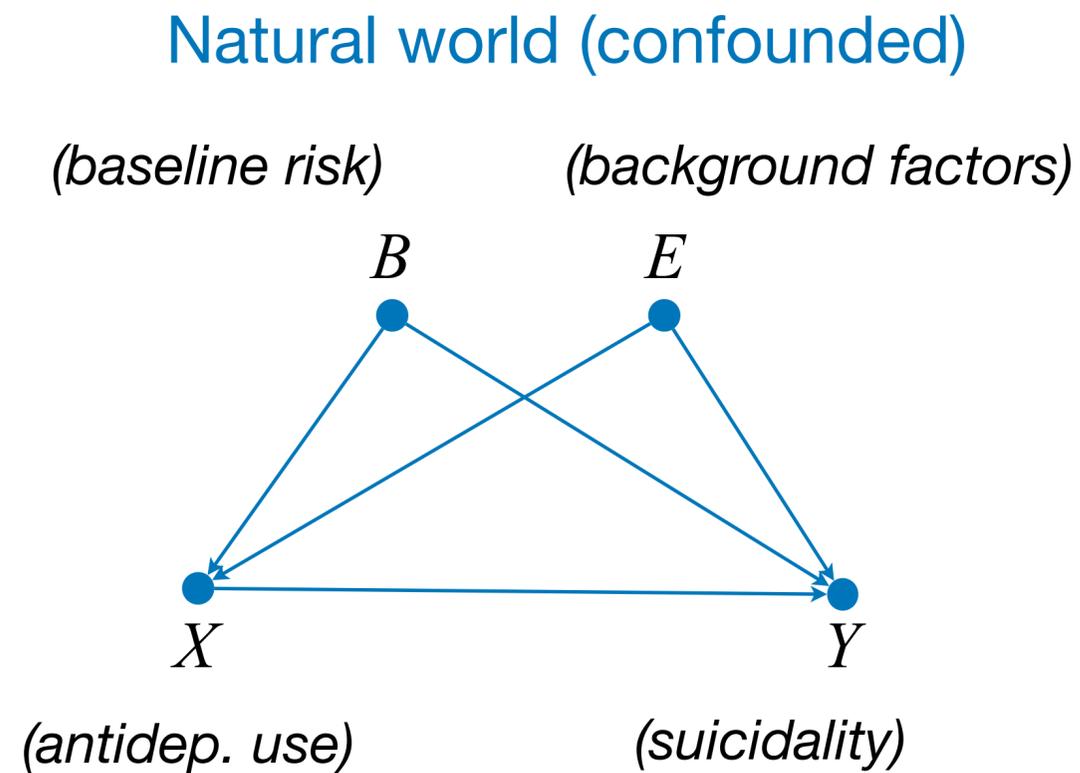


# Controlled Experimentation — Randomization

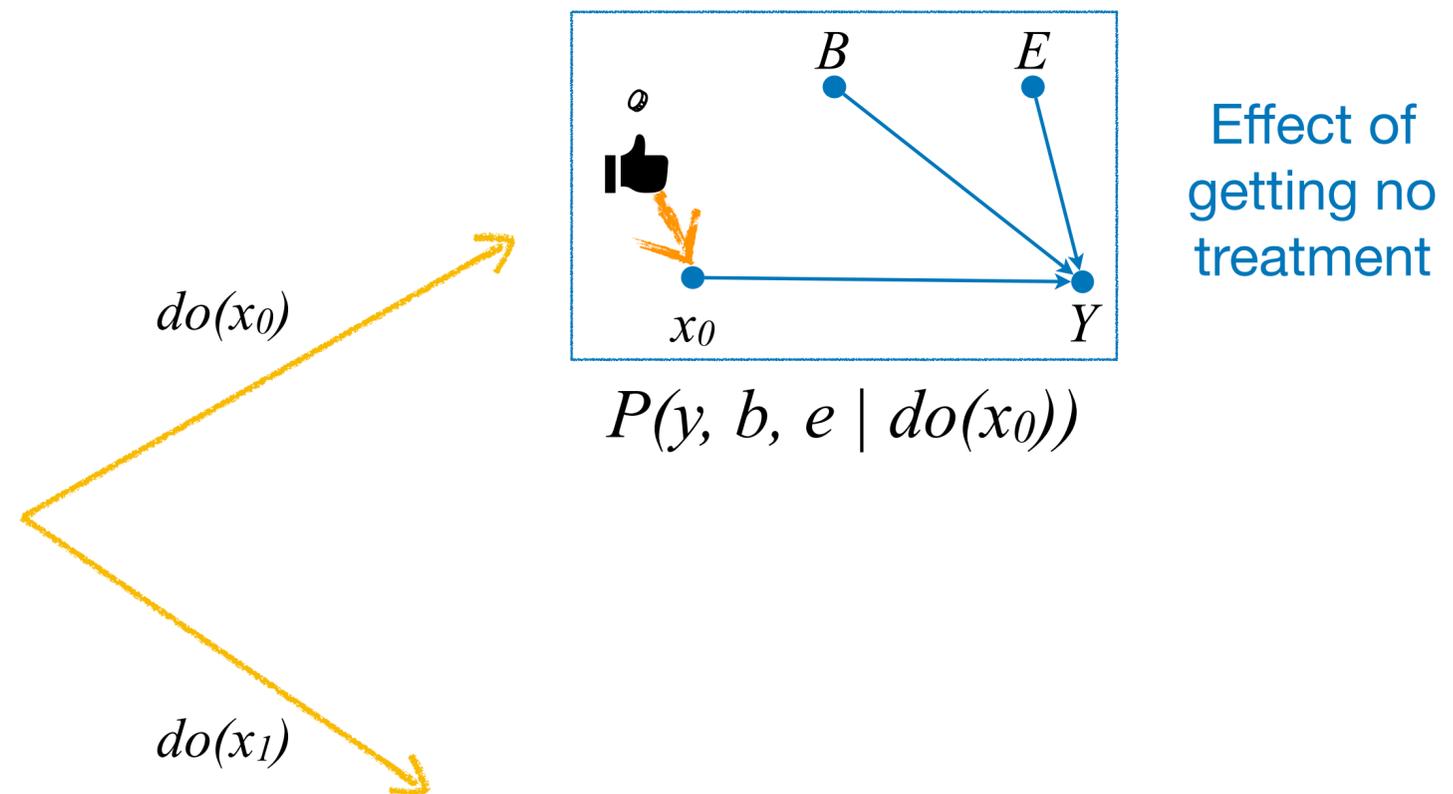
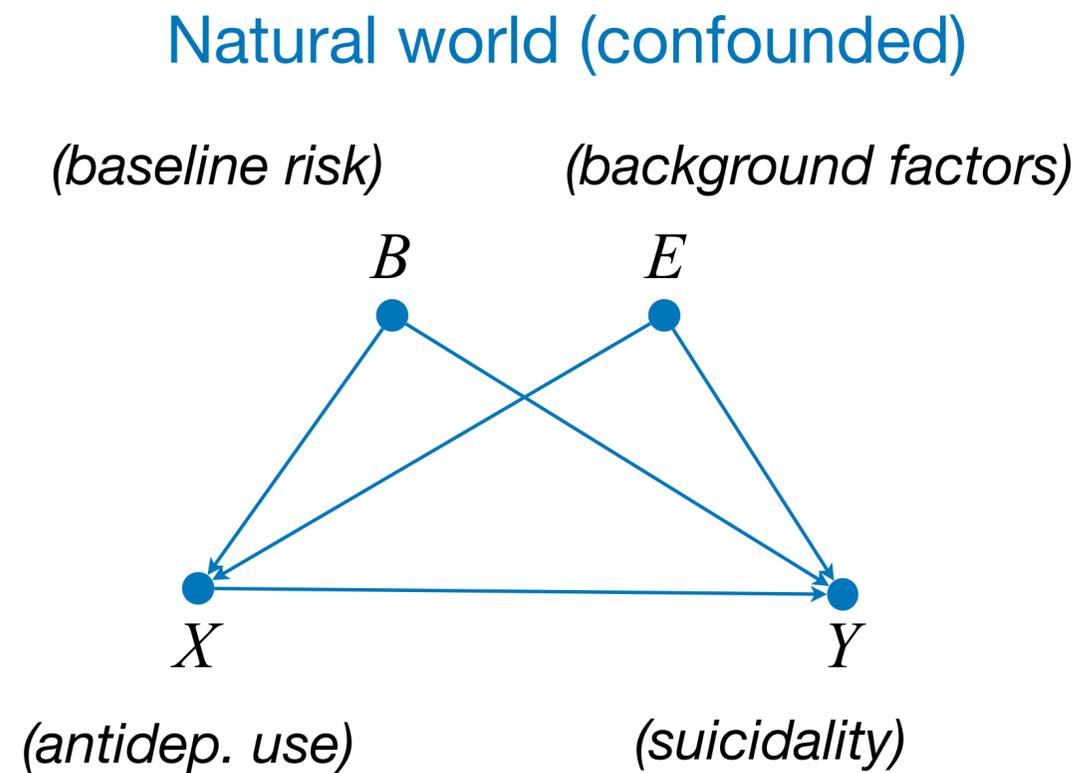
Natural world (confounded)



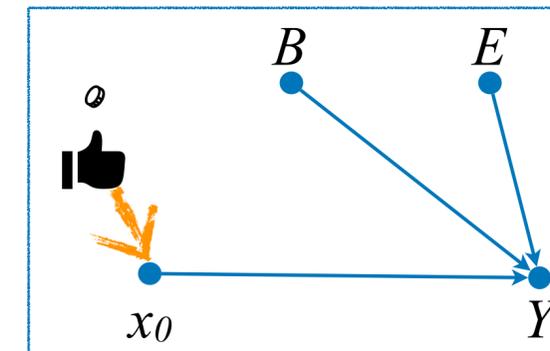
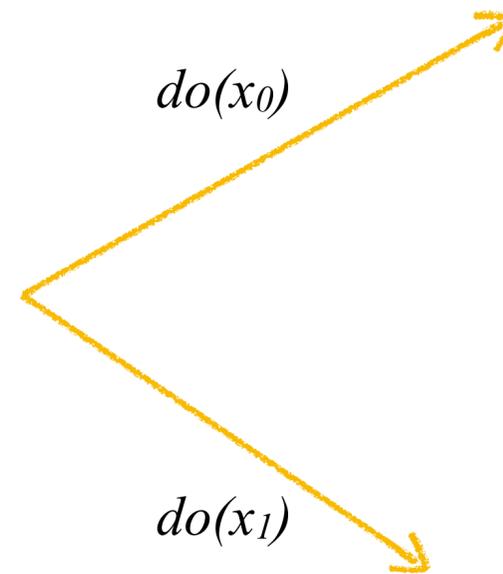
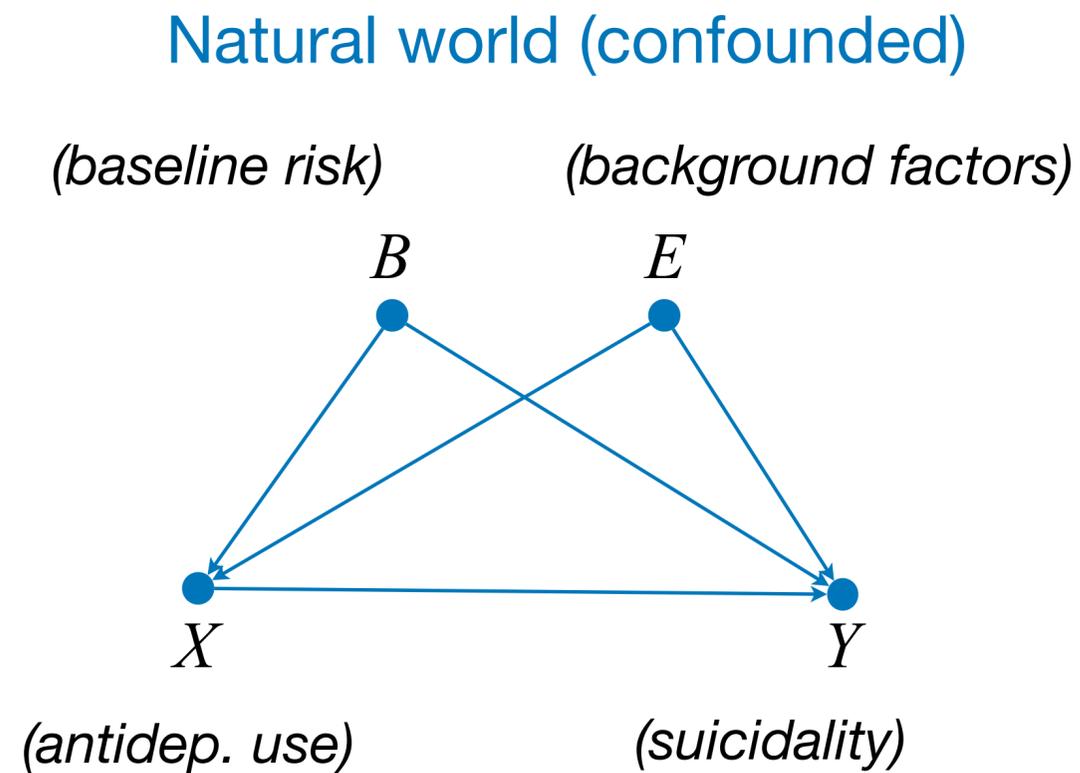
# Controlled Experimentation — Randomization



# Controlled Experimentation — Randomization

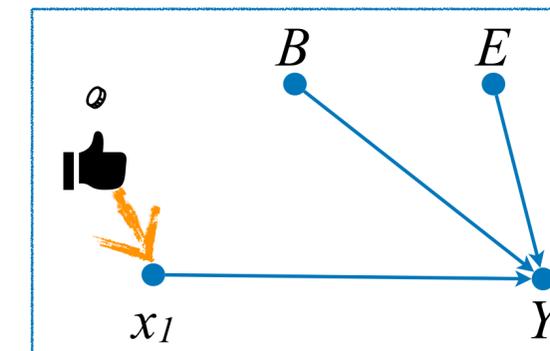


# Controlled Experimentation — Randomization



$$P(y, b, e | do(x_0))$$

Effect of getting no treatment



$$P(y, b, e | do(x_1))$$

Effect of getting treatment

# Randomization is not all there is!

## Motivating Example (2)

# Randomization is not all there is!

## Motivating Example (2)

[Greenhouse et al. 2008] On the risk of suicidality among pediatric antidepressant users:

# Randomization is not all there is!

## Motivating Example (2)

[Greenhouse et al. 2008] On the risk of suicidality among pediatric antidepressant users:

- The FDA performed several RCTs finding that youths receiving antidepressants ( $do(x_1)$ ) **had approximately twice the amount** of suicidal thoughts and behaviors compared to the control groups ( $do(x_0)$ ).

$$P(Y = 1 | do(x_1)) > P(Y = 1 | do(x_0))$$

# Randomization is not all there is!

## Motivating Example (2)

[Greenhouse et al. 2008] On the risk of suicidality among pediatric antidepressant users:

- The FDA performed several RCTs finding that youths receiving antidepressants ( $do(x_1)$ ) **had approximately twice the amount** of suicidal thoughts and behaviors compared to the control groups ( $do(x_0)$ ).  
$$P(Y = 1 | do(x_1)) > P(Y = 1 | do(x_0))$$
- Results led to the addition of a **strict warning** to the drug's label.

# Randomization is not all there is!

## Motivating Example (2)

[Greenhouse et al. 2008] On the risk of suicidality among pediatric antidepressant users:

- The FDA performed several RCTs finding that youths receiving antidepressants ( $do(x_1)$ ) **had approximately twice the amount** of suicidal thoughts and behaviors compared to the control groups ( $do(x_0)$ ).

$$P(Y = 1 | do(x_1)) > P(Y = 1 | do(x_0))$$

- Results led to the addition of a **strict warning** to the drug's label.
- Surprisingly, following the warning, a **decrease in prescription** was reported together with **an increase of suicidal events** in the corresponding age groups.

$$P^*(Y = 1 | do(x_1)) < P^*(Y = 1 | do(x_0))$$

# Randomization is not all there is!

## Motivating Example (2)

[Greenhouse et al. 2008] On the risk of suicidality among pediatric antidepressant users:

- The FDA performed several RCTs finding that youths receiving antidepressants ( $do(x_1)$ ) **had approximately twice the amount** of suicidal thoughts and behaviors compared to the control groups ( $do(x_0)$ ).

$$P(Y = 1 | do(x_1)) > P(Y = 1 | do(x_0))$$

- Results led to the addition of a **strict warning** to the drug's label.
- Surprisingly, following the warning, a **decrease in prescription** was reported together with **an increase of suicidal events** in the corresponding age groups.

$$P^*(Y = 1 | do(x_1)) < P^*(Y = 1 | do(x_0))$$

- Several **observational studies** reported **positive results** for patients using the same antidepressants, even after accounting for access to mental health-care and other confounding factors.

$$\tilde{P}^*(Y = 1 | do(x_1)) < \tilde{P}^*(Y = 1 | do(x_0))$$

# Randomization is not all there is!

## Motivating Example (2)

[Greenhouse et al. 2008] On the risk of suicidality among pediatric antidepressant users:

- The FDA performed several RCTs finding that youths receiving antidepressants ( $do(x_1)$ ) **had approximately twice the amount** of suicidal thoughts and behaviors compared to the control groups ( $do(x_0)$ ).

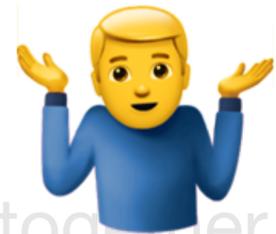
$$P(Y = 1 | do(x_1)) > P(Y = 1 | do(x_0))$$

- Results led to the addition of a **strict warning** to the drug's label.
- Surprisingly, following the warning, a **decrease in prescription** was reported together with an **increase of suicidal events** in the corresponding age groups.

$$P^*(Y = 1 | do(x_1)) < P^*(Y = 1 | do(x_0))$$

- Several **observational studies** reported **positive results** for patients using the same antidepressants, even after accounting for access to mental health-care and other confounding factors.

$$\tilde{P}^*(Y = 1 | do(x_1)) < \tilde{P}^*(Y = 1 | do(x_0))$$



What is going on here?

# What are we missing?

## Motivating Example (3)

# What are we missing?

## Motivating Example (3)

- Were the experiments conducted erroneously ?

# What are we missing?

## Motivating Example (3)

- Were the experiments conducted erroneously ?
- Randomization guarantees **internal validity**, that is, causal conclusions are true for the population that was studied.

# What are we missing?

## Motivating Example (3)

- Were the experiments conducted erroneously ?
- Randomization guarantees **internal validity**, that is, causal conclusions are true for the population that was studied.
- Most experimental findings are intended to be **generalized** to a broader, or even different, **target domain** (in other words, population, setting, environment).

# Two Challenges

- Some possible explanations for the discrepancy in those results are:

# Two Challenges

- Some possible explanations for the discrepancy in those results are:

## 1. Transportability

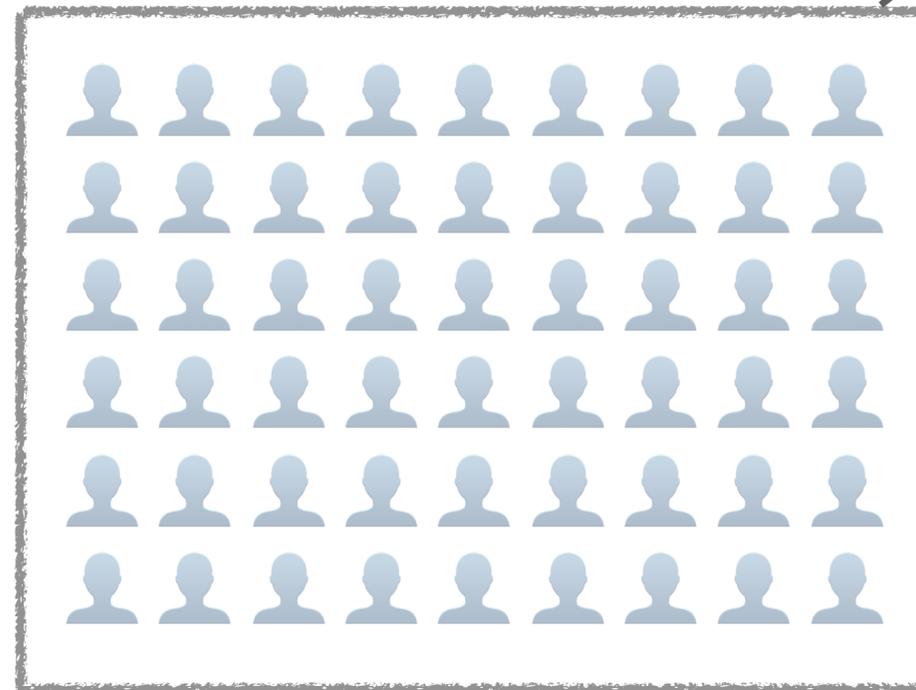
There is a mismatch between the study population  $\pi$  and the general clinical population  $\pi^*$  regarding ethnicity, race, and income (covariates named  $E$ ).

# Two Challenges

- Some possible explanations for the discrepancy in those results are:

## 1. Transportability

There is a mismatch between the study population  $\pi$  and the general clinical population  $\pi^*$  regarding ethnicity, race, and income (covariates named  $E$ ).



entire/target  
population

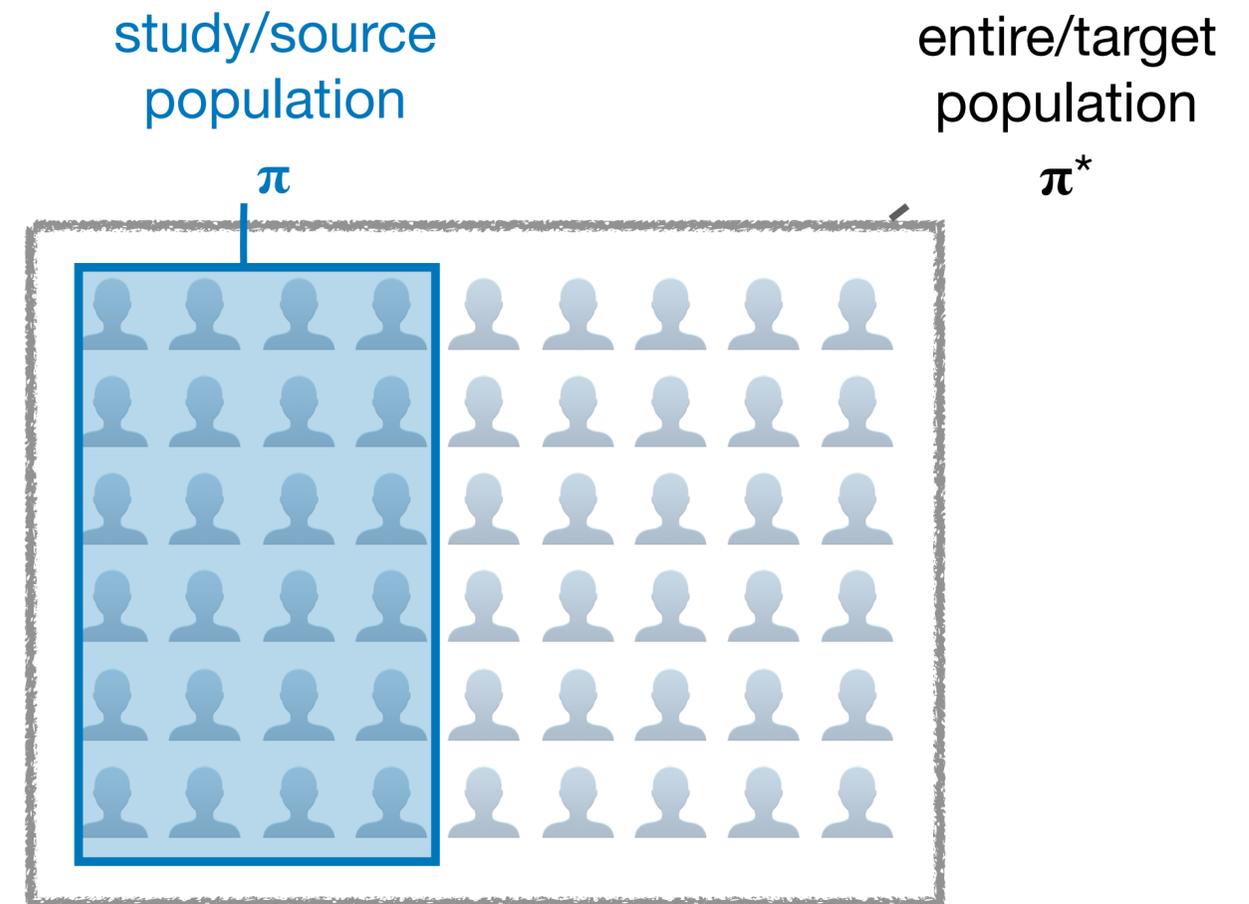
$\pi^*$

# Two Challenges

- Some possible explanations for the discrepancy in those results are:

## 1. Transportability

There is a mismatch between the study population  $\pi$  and the general clinical population  $\pi^*$  regarding ethnicity, race, and income (covariates named  $E$ ).

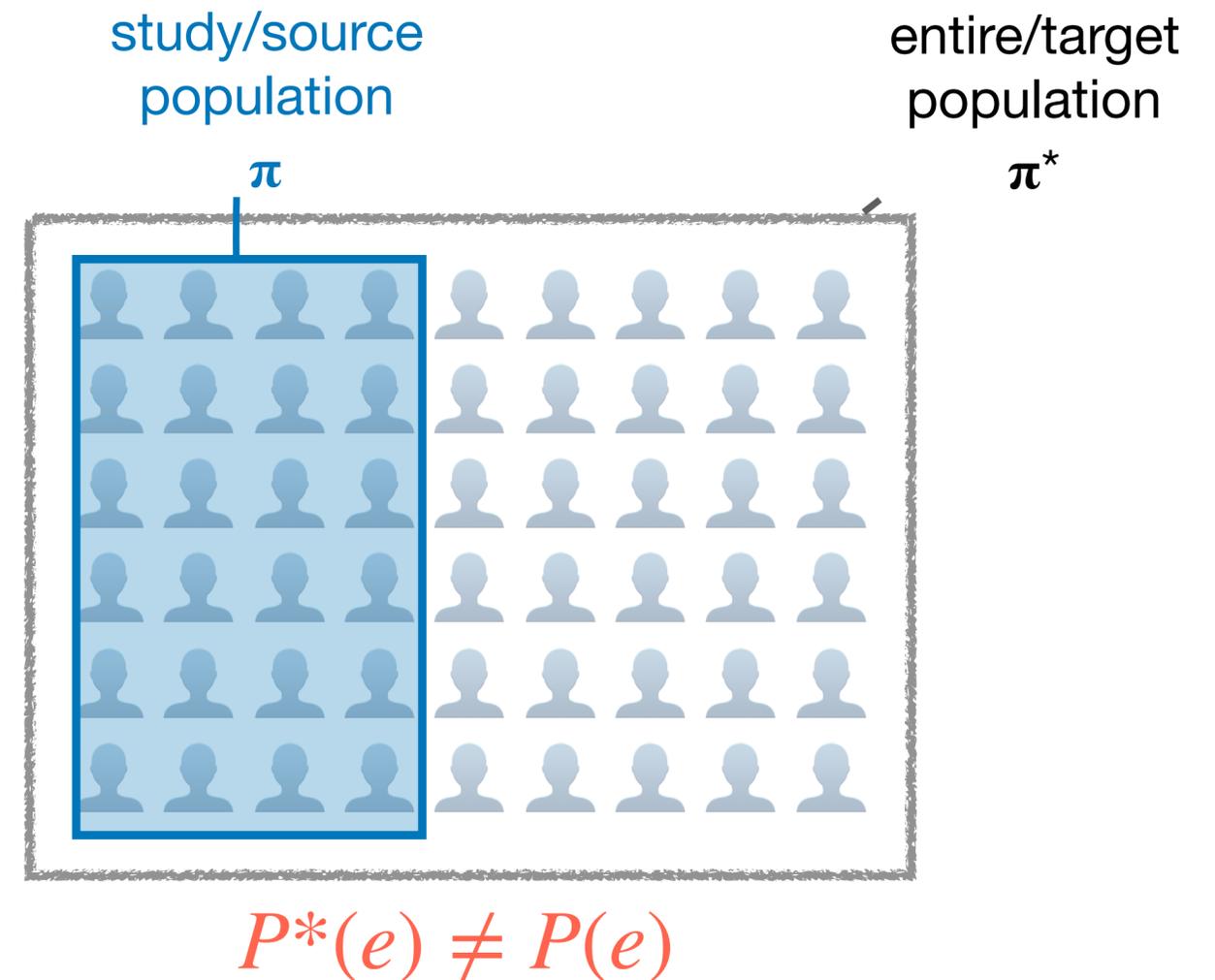


# Two Challenges

- Some possible explanations for the discrepancy in those results are:

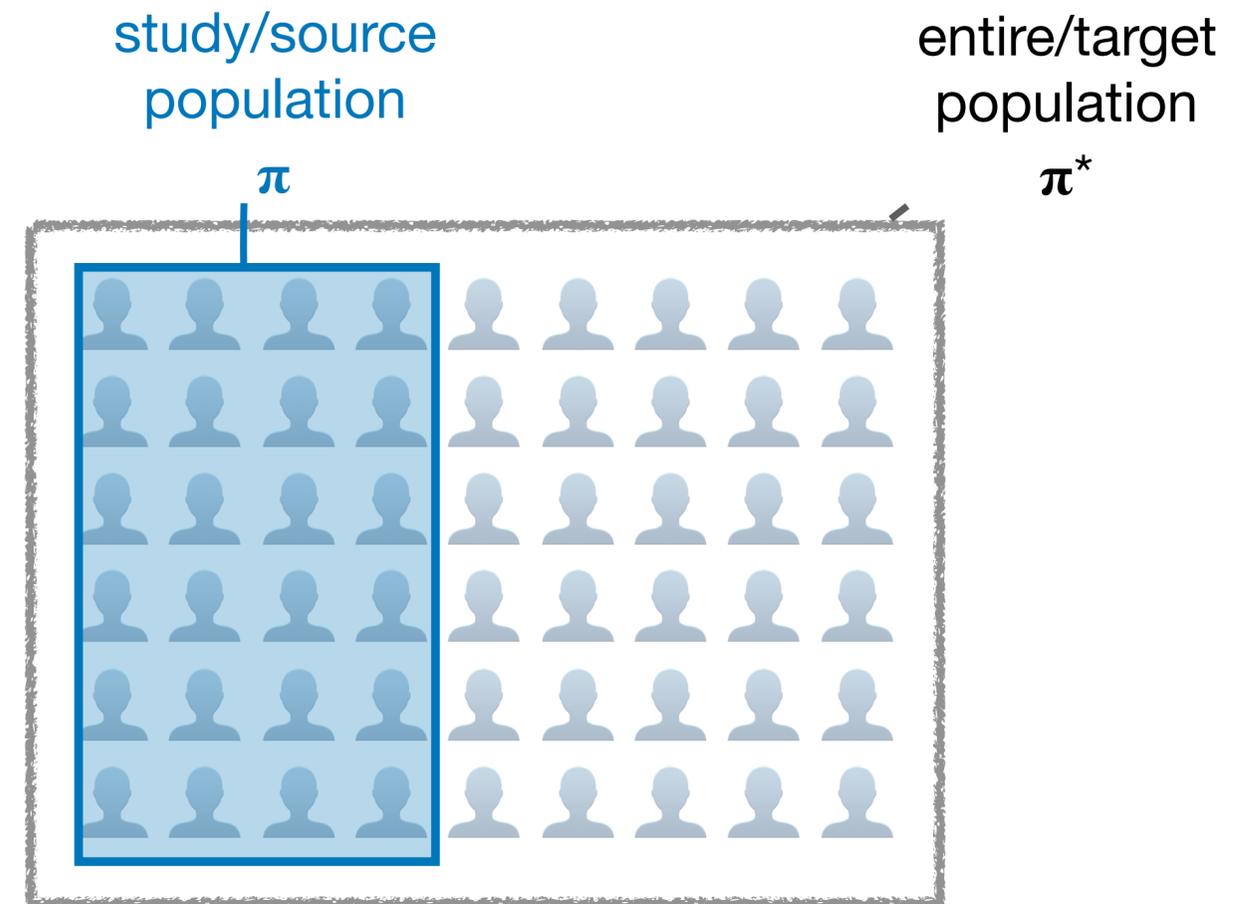
## 1. Transportability

There is a mismatch between the study population  $\pi$  and the general clinical population  $\pi^*$  regarding ethnicity, race, and income (covariates named  $E$ ).



# Two Challenges

- Some possible explanations for the discrepancy in those results are:

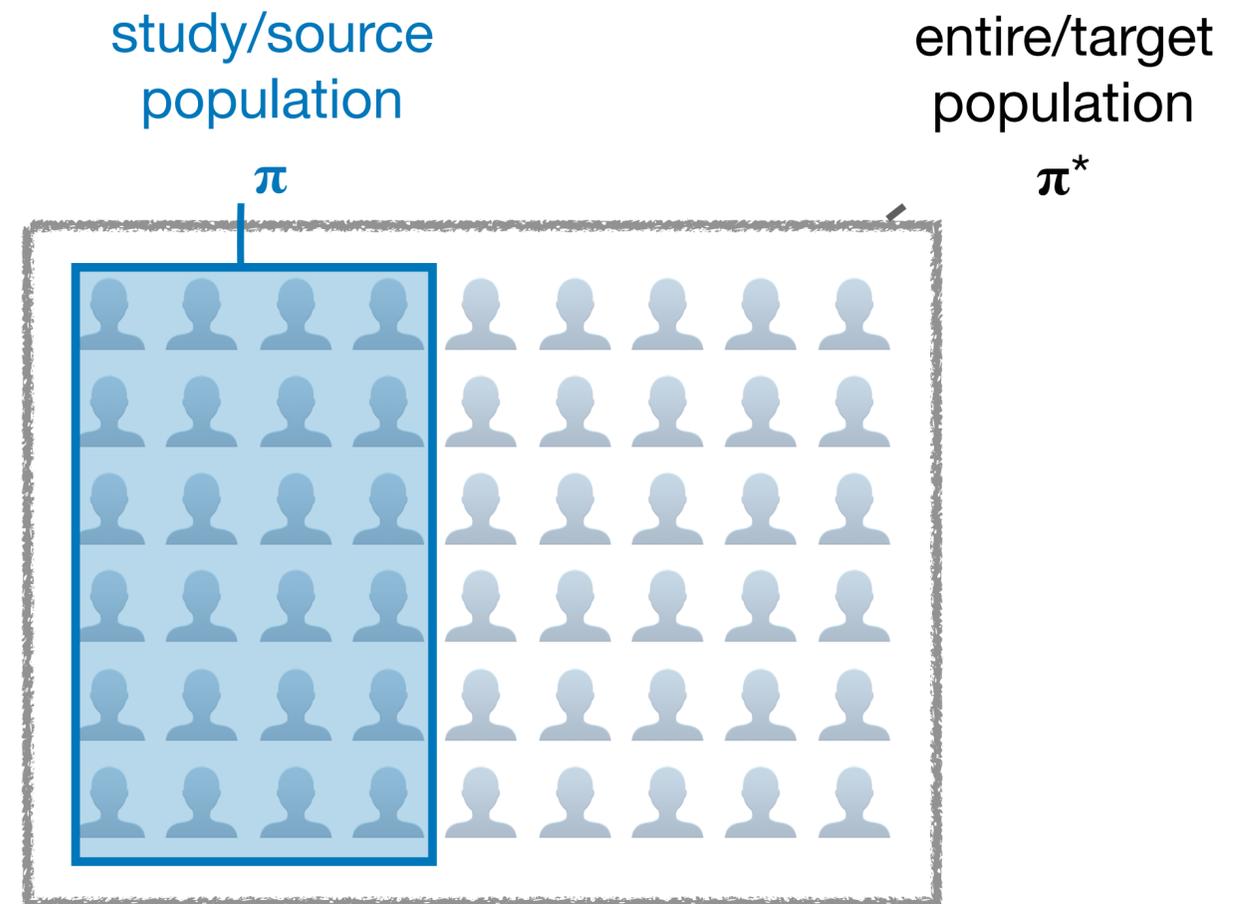


# Two Challenges

- Some possible explanations for the discrepancy in those results are:

## 2. Selection Bias

FDA's studies sampled from a distinct population by excluding youths with elevated baseline risk for suicide (B) from their cohorts.



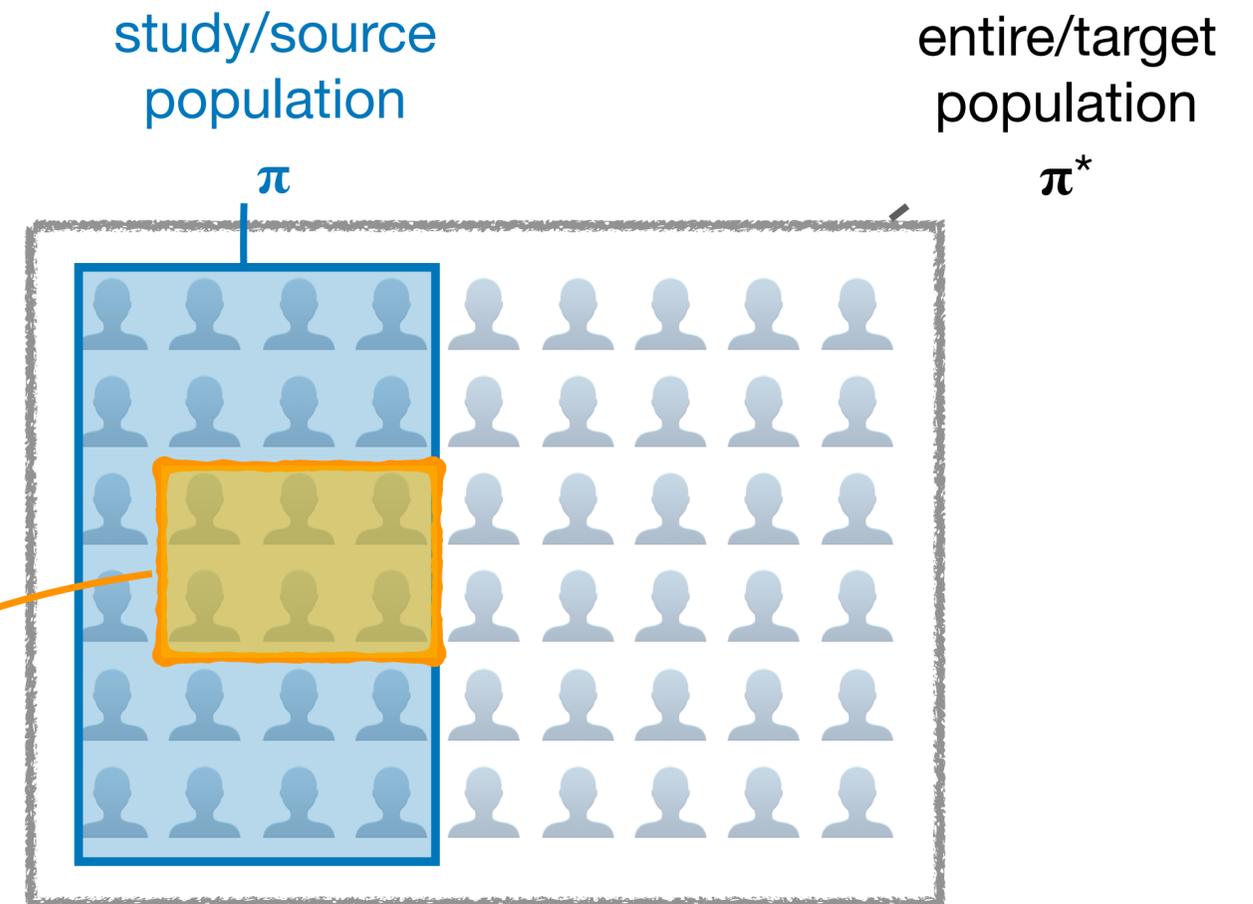
# Two Challenges

- Some possible explanations for the discrepancy in those results are:

## 2. Selection Bias

FDA's studies sampled from a distinct population by excluding youths with elevated baseline risk for suicide (B) from their cohorts.

sampled individuals  
(S=1)

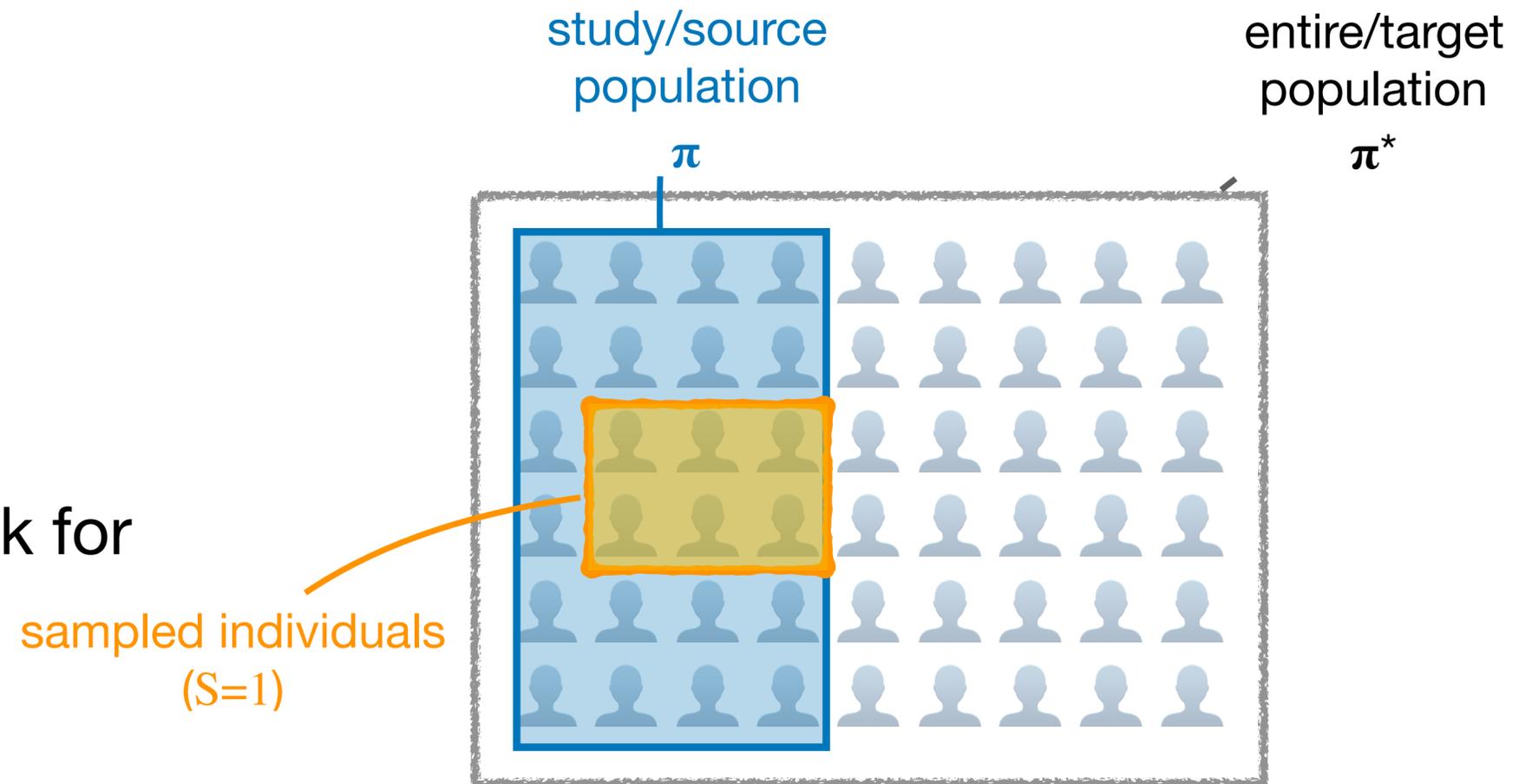


# Two Challenges

- Some possible explanations for the discrepancy in those results are:

## 2. Selection Bias

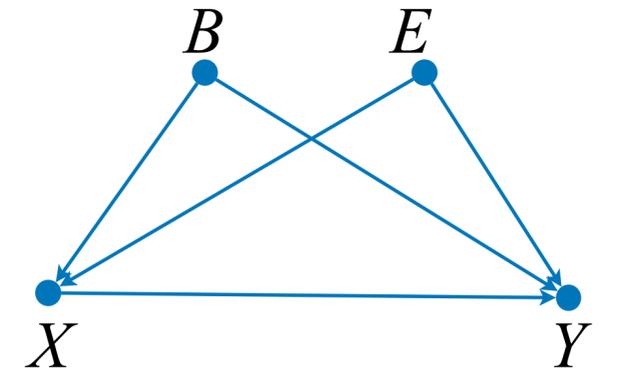
FDA's studies sampled from a distinct population by excluding youths with elevated baseline risk for suicide (B) from their cohorts.



$$P(y, b, e | do(x), S = 1) \neq P(y, b, e | do(x))$$

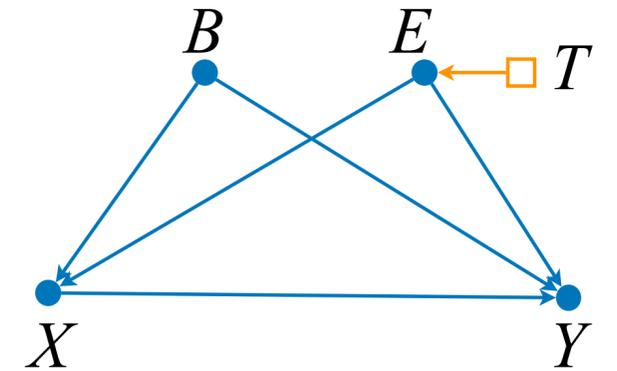
$$P(x, y, b, e | S = 1) \neq P(x, y, b, e)$$

# Formalizing the Problem



# Formalizing the Problem

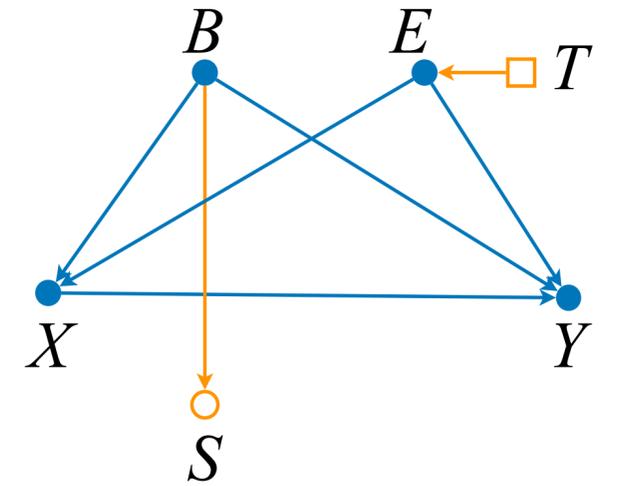
We use indicator  $\square$  named  $T$  to mark variables with differences between domains  $\pi$  and  $\pi^*$ .



# Formalizing the Problem

We use indicator  $\square$  named  $T$  to mark variables with differences between domains  $\pi$  and  $\pi^*$ .

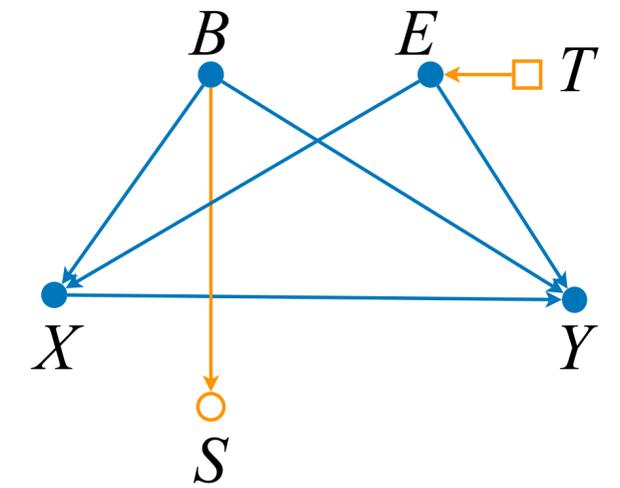
Similarly, the indicator  $\circ$ , named  $S$ , is defined such that  $S=1$  for every unit sampled in the study, and 0, otherwise.



# Formalizing the Problem

We use indicator  $\square$  named  $T$  to mark variables with differences between domains  $\pi$  and  $\pi^*$ .

Similarly, the indicator  $\circ$ , named  $S$ , is defined such that  $S=1$  for every unit sampled in the study, and 0, otherwise.



(called selection diagram  $D$ )

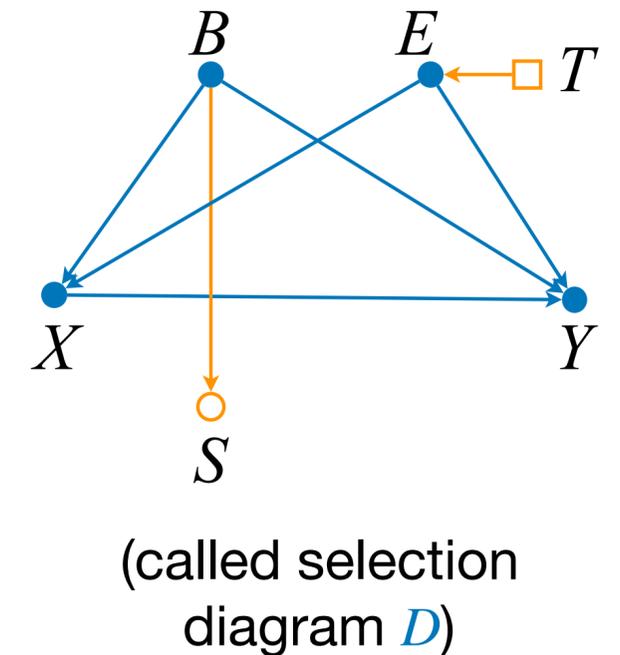
# Formalizing the Problem

We use indicator  $\square$  named  $T$  to mark variables with differences between domains  $\pi$  and  $\pi^*$ .

Similarly, the indicator  $\circ$ , named  $S$ , is defined such that  $S=1$  for every unit sampled in the study, and 0, otherwise.

In this example, the causal effect can be estimated by recalibrating the experimental findings using observations from the target domain

$$P^*(y | do(x)) = \sum_{b,e} P(y | do(x), b, e, S = 1) P^*(b, e)$$

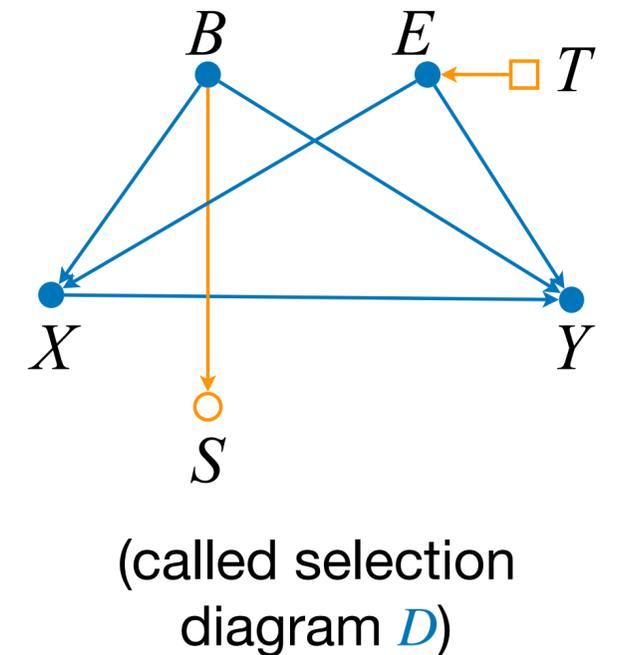


# Formalizing the Problem

We use indicator  $\square$  named  $T$  to mark variables with differences between domains  $\pi$  and  $\pi^*$ .

Similarly, the indicator  $\circ$ , named  $S$ , is defined such that  $S=1$  for every unit sampled in the study, and 0, otherwise.

In this example, the causal effect can be estimated by recalibrating the experimental findings using observations from the target domain



$$\boxed{P^*(y | do(x))} = \sum_{b,e} P(y | do(x), b, e, S = 1) P^*(b, e)$$

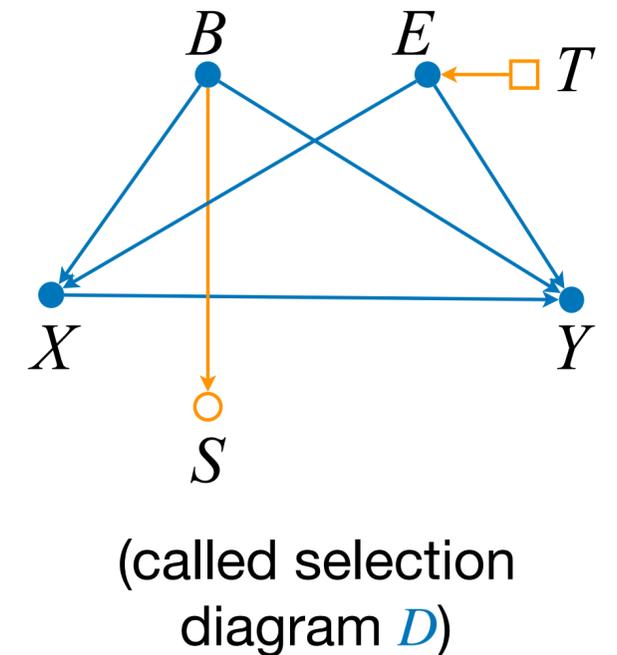
causal effect  
in target domain

# Formalizing the Problem

We use indicator  $\square$  named  $T$  to mark variables with differences between domains  $\pi$  and  $\pi^*$ .

Similarly, the indicator  $\circ$ , named  $S$ , is defined such that  $S=1$  for every unit sampled in the study, and 0, otherwise.

In this example, the causal effect can be estimated by recalibrating the experimental findings using observations from the target domain



$$\boxed{P^*(y | do(x))} = \sum_{b,e} \boxed{P(y | do(x), b, e, S = 1)} P^*(b, e)$$

causal effect in target domain

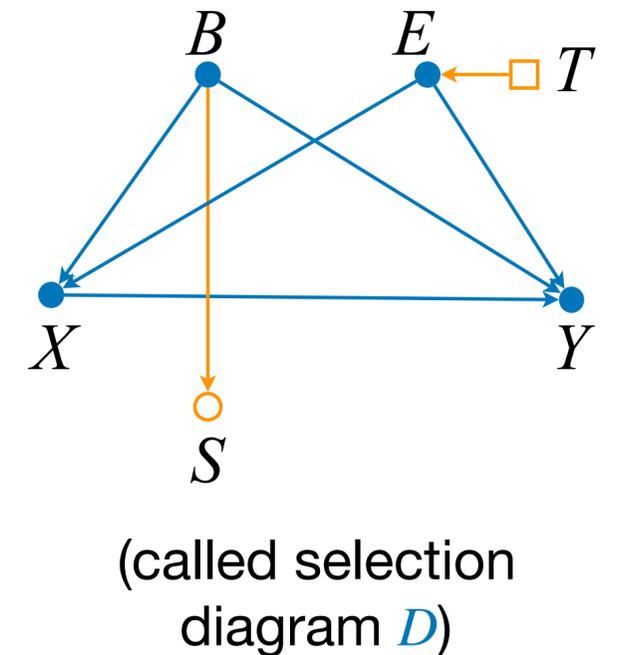
experimental data from the source under selection bias

# Formalizing the Problem

We use indicator  $\square$  named  $T$  to mark variables with differences between domains  $\pi$  and  $\pi^*$ .

Similarly, the indicator  $\circ$ , named  $S$ , is defined such that  $S=1$  for every unit sampled in the study, and 0, otherwise.

In this example, the causal effect can be estimated by recalibrating the experimental findings using observations from the target domain



$$P^*(y | do(x)) = \sum_{b,e} P(y | do(x), b, e, S = 1) P^*(b, e)$$

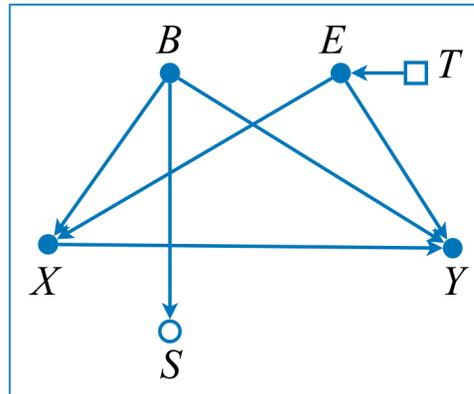
causal effect in target domain

experimental data from the source under selection bias

Observations from the target domain

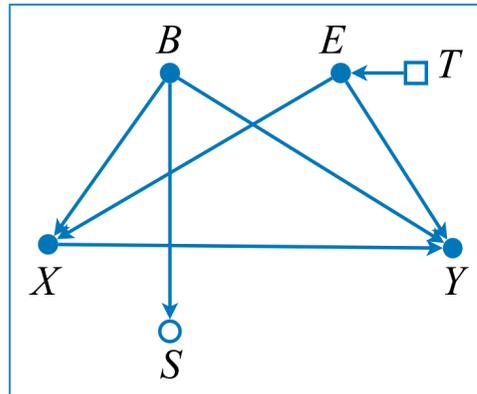
# Problem Statement

# Problem Statement



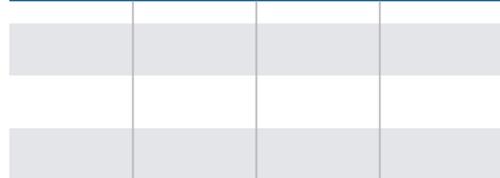
Selection Diagram  $D$

# Problem Statement



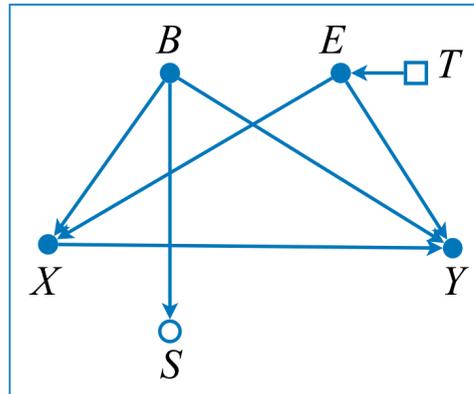
Selection Diagram  $D$

$$P(\mathbf{v} \mid do(x), S = 1)$$



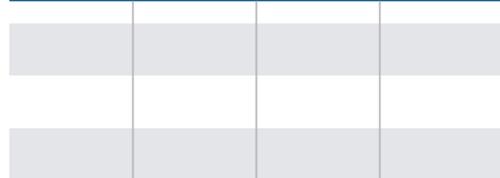
Selection-biased Exp.  
Distribution  $P_1$  from  $\pi$

# Problem Statement



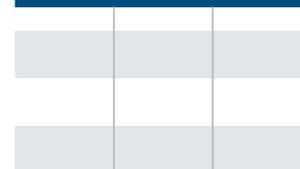
Selection Diagram  $D$

$$P(\mathbf{v} \mid do(x), S = 1)$$



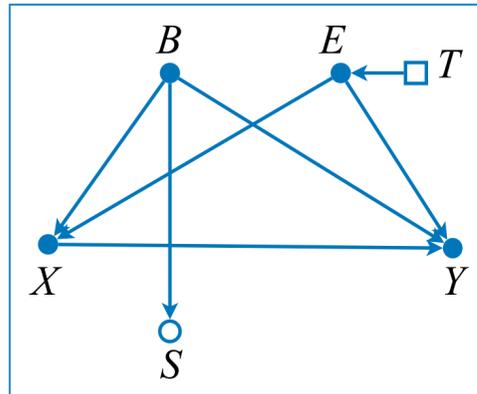
Selection-biased Exp. Distribution  $P_1$  from  $\pi$

$$P^*(\mathbf{w})$$



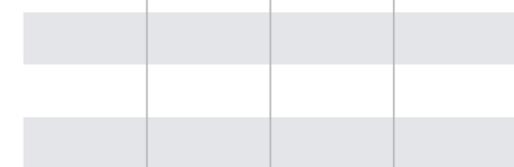
Covariate Distribution  $P_2$  from  $\pi^*$

# Problem Statement



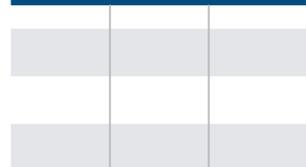
Selection Diagram  $D$

$$P(\mathbf{v} \mid do(x), S = 1)$$



Selection-biased Exp.  
Distribution  $P_1$  from  $\pi$

$$P^*(\mathbf{w})$$

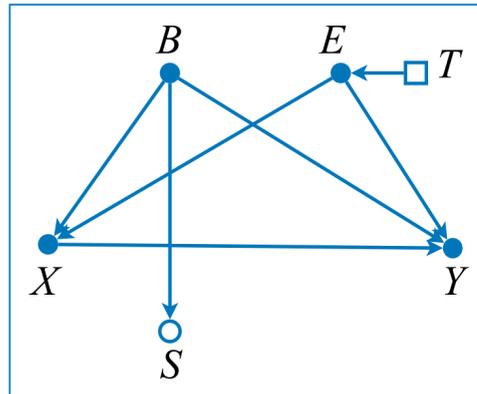


Covariate Distribution  
 $P_2$  from  $\pi^*$

Is there a function  $f$  such that

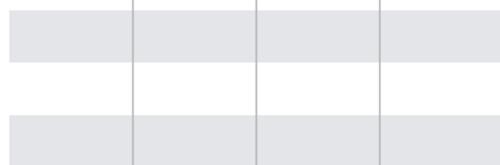
$$P^*(y \mid do(x)) = f(P_1, P_2)$$

# Problem Statement



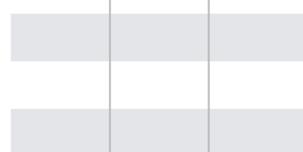
Selection Diagram  $D$

$$P(\mathbf{v} \mid do(x), S = 1)$$



Selection-biased Exp.  
Distribution  $P_1$  from  $\pi$

$$P^*(\mathbf{w})$$



Covariate Distribution  
 $P_2$  from  $\pi^*$

Is there a function  $f$  such that

$$P^*(y \mid do(x)) = f(P_1, P_2)$$

yes ( $f$ ) / no



# Related Work

# Related Work

confounding   type of input   selection bias   transportability   complete

---

# Related Work

confounding    type of input    selection bias    transportability    complete

---

**Backdoor Criterion** [Pearl '93]  
**Extended Backdoor** [Pearl and Paz '10]

---



obs.

# Related Work

	confounding	type of input	selection bias	transportability	complete
<b>Backdoor Criterion</b> [Pearl '93] <b>Extended Backdoor</b> [Pearl and Paz '10]	✓	obs.			
<b>Adjustment Criterion</b> [Shpitser et al. '10; Perkovic et al. '15,'18]	✓	obs.			✓

# Related Work

	confounding	type of input	selection bias	transportability	complete
<b>Backdoor Criterion</b> [Pearl '93] <b>Extended Backdoor</b> [Pearl and Paz '10]	✓	obs.			
<b>Adjustment Criterion</b> [Shpitser et al. '10; Perkovic et al. '15,'18]	✓	obs.			✓
<b>Selection Backdoor</b> [Bareinboim, Tian and Pearl '14]	✓	obs.	✓		

# Related Work

	confounding	type of input	selection bias	transportability	complete
<b>Backdoor Criterion</b> [Pearl '93] <b>Extended Backdoor</b> [Pearl and Paz '10]	✓	obs.			
<b>Adjustment Criterion</b> [Shpitser et al. '10; Perkovic et al. '15,'18]	✓	obs.			✓
<b>Selection Backdoor</b> [Bareinboim, Tian and Pearl '14]	✓	obs.	✓		
<b>Generalized Adjustment Criterion</b> [Correa, Tian and Bareinboim '18]	✓	obs.	✓		✓

# Related Work

	confounding	type of input	selection bias	transportability	complete
<b>Backdoor Criterion</b> [Pearl '93] <b>Extended Backdoor</b> [Pearl and Paz '10]	✓	obs.			
<b>Adjustment Criterion</b> [Shpitser et al. '10; Perkovic et al. '15,'18]	✓	obs.			✓
<b>Selection Backdoor</b> [Bareinboim, Tian and Pearl '14]	✓	obs.	✓		
<b>Generalized Adjustment Criterion</b> [Correa, Tian and Bareinboim '18]	✓	obs.	✓		✓
<b>st-Adjustment Criterion</b> [Correa, Tian and Bareinboim '19]	—	exp.	✓	✓	✓

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$P^*(y | do(x))$$

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$\underline{P^*(y | do(x))}$$

unbiased target  
effect in  $\pi^*$

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$\underbrace{P^*(y | do(x))}_{\substack{\text{unbiased target} \\ \text{effect in } \pi^*}} = \sum_{\mathbf{z}} P(y | do(x), \mathbf{z}, S = 1)P^*(\mathbf{z})$$

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$\underbrace{P^*(y | do(x))}_{\text{unbiased target effect in } \pi^*} = \sum_{\mathbf{z}} \underbrace{P(y | do(x), \mathbf{z}, S = 1)}_{\text{experiment results in source domain } \pi} P^*(\mathbf{z})$$

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$\underbrace{P^*(y | do(x))}_{\text{unbiased target effect in } \pi^*} = \sum_{\mathbf{z}} \underbrace{P(y | do(x), \mathbf{z}, S = 1)}_{\text{experiment results in source domain } \pi} \underbrace{P^*(\mathbf{z})}_{\text{observations from the target domain } \pi^*}$$

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$\underbrace{P^*(y | do(x))}_{\text{unbiased target effect in } \pi^*} = \sum_{\mathbf{z}} \underbrace{P(y | do(x), \mathbf{z}, S = 1)}_{\text{experiment results in source domain } \pi} \underbrace{P^*(\mathbf{z})}_{\text{observations from the target domain } \pi^*}$$

- Questions:

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$\underbrace{P^*(y | do(x))}_{\text{unbiased target effect in } \pi^*} = \sum_{\mathbf{z}} \underbrace{P(y | do(x), \mathbf{z}, S = 1)}_{\text{experiment results in source domain } \pi} \underbrace{P^*(\mathbf{z})}_{\text{observations from the target domain } \pi^*}$$

- Questions:
  1. How to **determine** if *st-adjustment* holds for a set of covariates  $\mathbf{Z}$ ?

# Solution: Covariate *st*-Adjustment

- Strategy: Recalibrate the results from experiments in the the studied population using observations from the target population.

$$\underbrace{P^*(y | do(x))}_{\text{unbiased target effect in } \pi^*} = \sum_{\mathbf{z}} \underbrace{P(y | do(x), \mathbf{z}, S = 1)}_{\text{experiment results in source domain } \pi} \underbrace{P^*(\mathbf{z})}_{\text{observations from the target domain } \pi^*}$$

- Questions:
  1. How to **determine** if *st-adjustment* holds for a set of covariates  $\mathbf{Z}$ ?
  2. How to **find** admissible covariate sets?

# Challenge I. Covariate Admissibility

# Challenge I. Covariate Admissibility

- In general, adjusting for some variables that are affected by the treatment could introduce more bias, instead of controlling for the current, existent ones.

# Challenge I. Covariate Admissibility

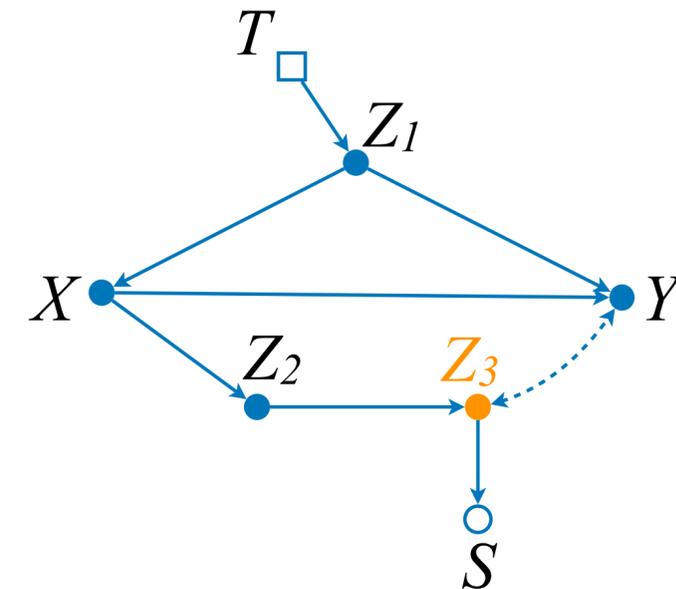
- In general, adjusting for some variables that are affected by the treatment could introduce more bias, instead of controlling for the current, existent ones.
- In our setting, in particular, special attention needs to be paid to these covariates (*affected by the treatment*) that are *correlated with the outcome* given pre-treatment covariates.

# Challenge I. Covariate Admissibility

- In general, adjusting for some variables that are affected by the treatment could introduce more bias, instead of controlling for the current, existent ones.
- In our setting, in particular, special attention needs to be paid to these covariates (*affected by the treatment*) that are *correlated with the outcome* given pre-treatment covariates.
- Let's call this set  $Z_p$ .

# Challenge I. Covariate Admissibility

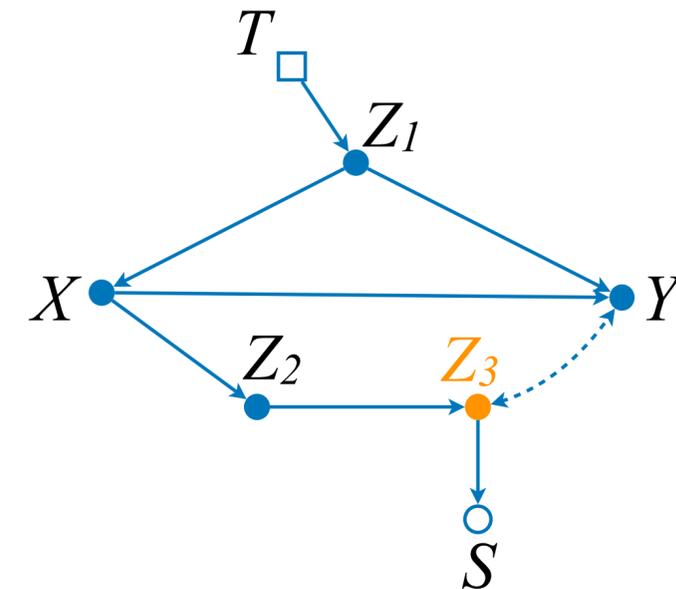
- In general, adjusting for some variables that are affected by the treatment could introduce more bias, instead of controlling for the current, existent ones.
- In our setting, in particular, special attention needs to be paid to these covariates (*affected by the treatment*) that are *correlated with the outcome* given pre-treatment covariates.
- Let's call this set  $Z_p$ .



# Challenge I. Covariate Admissibility

- In general, adjusting for some variables that are affected by the treatment could introduce more bias, instead of controlling for the current, existent ones.
- In our setting, in particular, special attention needs to be paid to these covariates (*affected by the treatment*) that are *correlated with the outcome* given pre-treatment covariates.

- Let's call this set  $Z_p$ .
- For example if adjusting for  $Z = \{Z_1, Z_2, Z_3\}$  in this model  $Z_p = \{Z_3\}$ .



# **Main Result I: Complete Graphical Condition**

# Main Result I:

## Complete Graphical Condition

A set of covariates  $Z$  is admissible for *st-adjustment* in  $D$  relative to treatment  $X$  and outcome  $Y$  if:

# Main Result I:

## Complete Graphical Condition

A set of covariates  $Z$  is admissible for *st-adjustment* in  $D$  relative to treatment  $X$  and outcome  $Y$  if:

- (i) Variables in  $Z_p$  are independent of the treatment given all other covariates, and

# Main Result I:

## Complete Graphical Condition

A set of covariates  $Z$  is admissible for *st-adjustment* in  $D$  relative to treatment  $X$  and outcome  $Y$  if:

- (i) Variables in  $Z_p$  are independent of the treatment given all other covariates, and
- (ii) The outcome  $Y$  is independent of all the transportability (T) and selection bias nodes (S) given the covariates  $Z$  and the treatment  $X$ .

# Main Result I: Complete Graphical Condition

A set of covariates  $Z$  is admissible for *st-adjustment* in  $D$  relative to treatment  $X$  and outcome  $Y$  if:

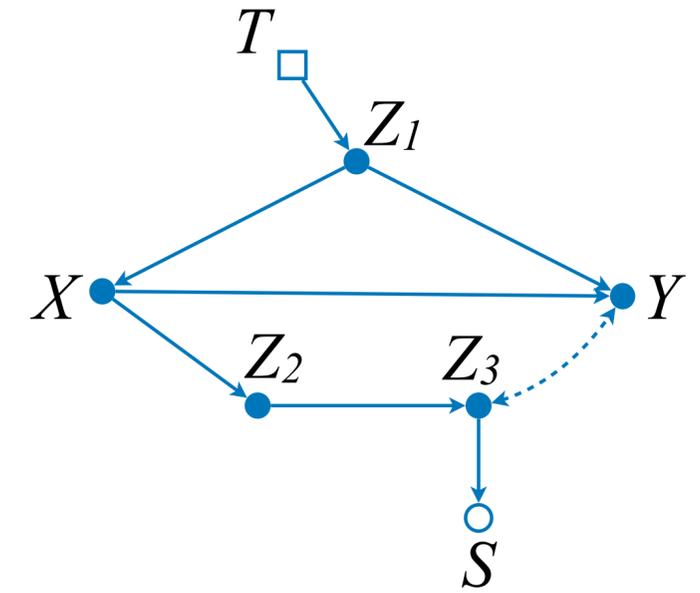
- (i) Variables in  $Z_p$  are independent of the treatment given all other covariates, and
- (ii) The outcome  $Y$  is independent of all the transportability (T) and selection bias nodes (S) given the covariates  $Z$  and the treatment  $X$ .

**Thm.** The causal effect  $P^*(y \mid \text{do}(x))$  is identifiable by *st-adjustment* on a set  $Z$  with  $D$  if and only if the conditions above hold for  $Z$  relative to  $X$  and  $Y$ .

# Understanding the criterion

# Understanding the criterion

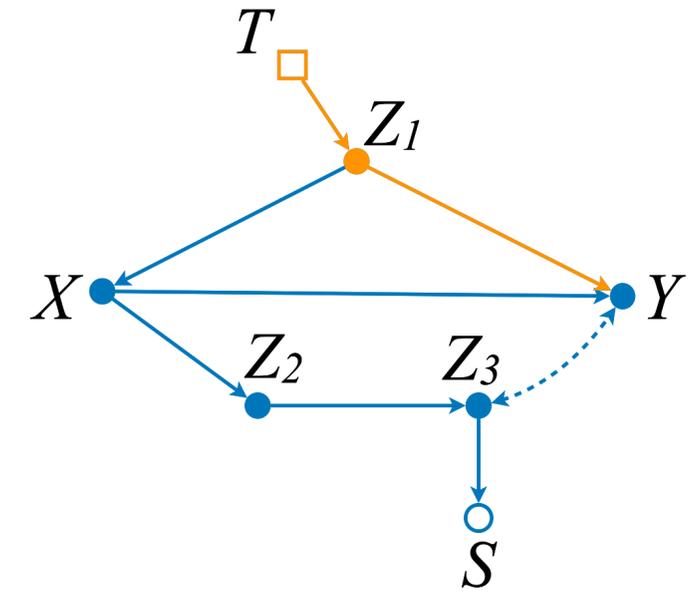
Task: Compute  $P^*(y \mid \text{do}(x))$



# Understanding the criterion

Task: Compute  $P^*(y \mid \text{do}(x))$

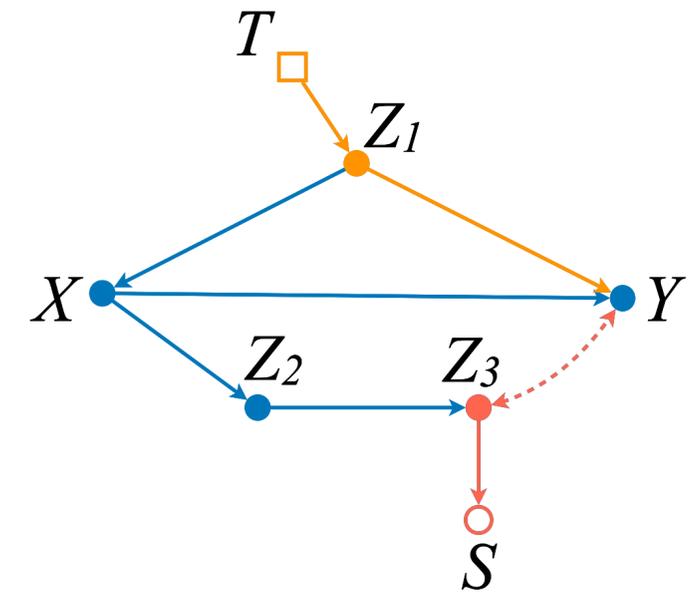
- The outcome  $Y$  is affected by differences in the distribution of  $Z_1$  between the source and target domains.



# Understanding the criterion

Task: Compute  $P^*(y \mid \text{do}(x))$

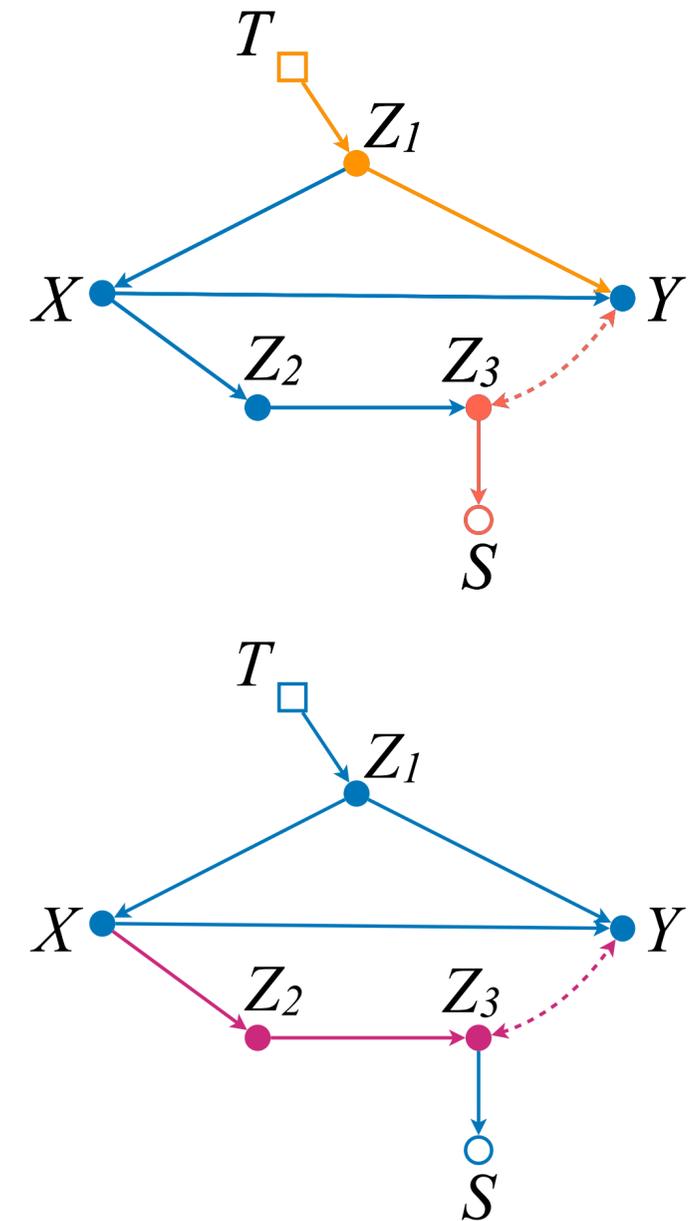
- The outcome  $Y$  is affected by differences in the distribution of  $Z_1$  between the source and target domains.
- The variable  $Z_3$  affects the likelihood of units being sampled.



# Understanding the criterion

Task: Compute  $P^*(y \mid \text{do}(x))$

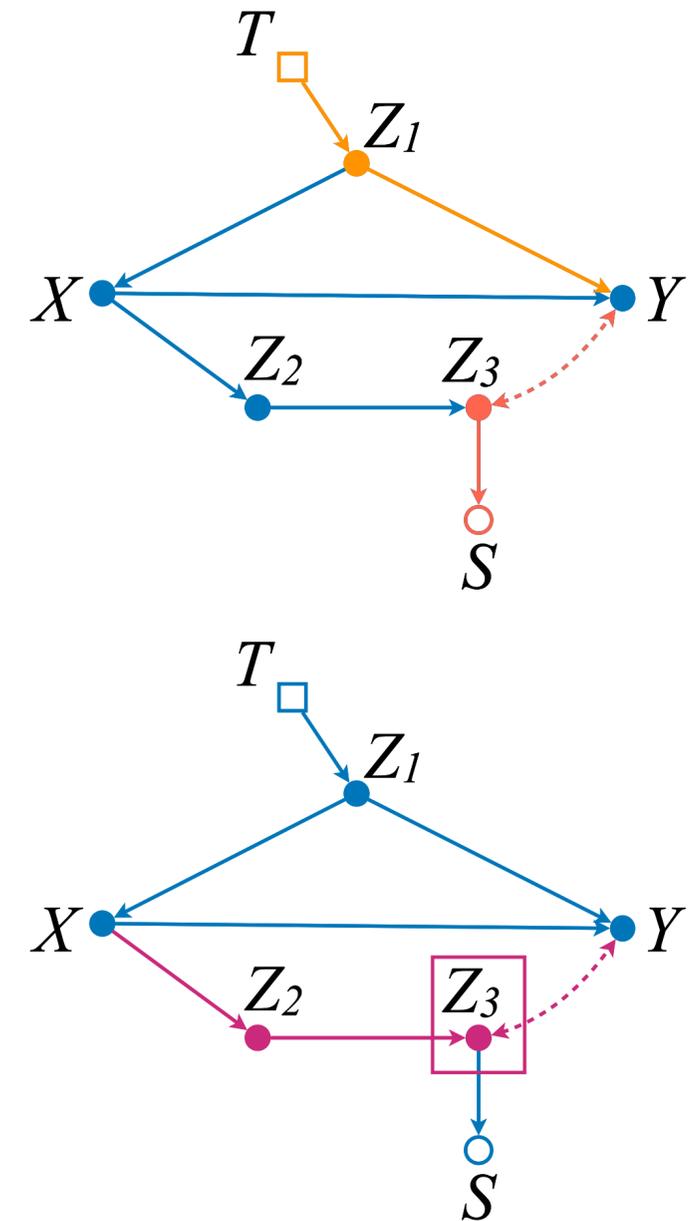
- The outcome  $Y$  is affected by differences in the distribution of  $Z_1$  between the source and target domains.
- The variable  $Z_3$  affects the likelihood of units being sampled.
- If we adjust for  $Z_3$  to control for selection bias, we introduce spurious correlation. Hence, we should also control for  $Z_2$ .



# Understanding the criterion

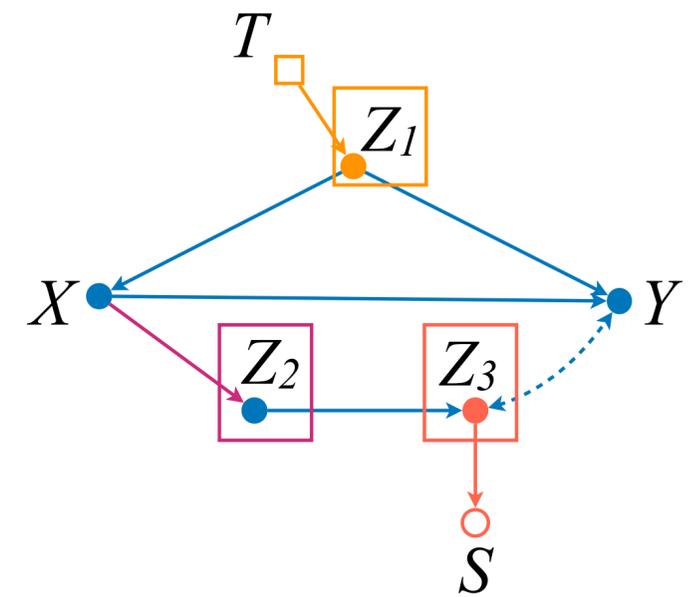
Task: Compute  $P^*(y \mid \text{do}(x))$

- The outcome  $Y$  is affected by differences in the distribution of  $Z_1$  between the source and target domains.
- The variable  $Z_3$  affects the likelihood of units being sampled.
- If we adjust for  $Z_3$  to control for selection bias, we introduce spurious correlation. Hence, we should also control for  $Z_2$ .



# Getting the intuition behind the rules

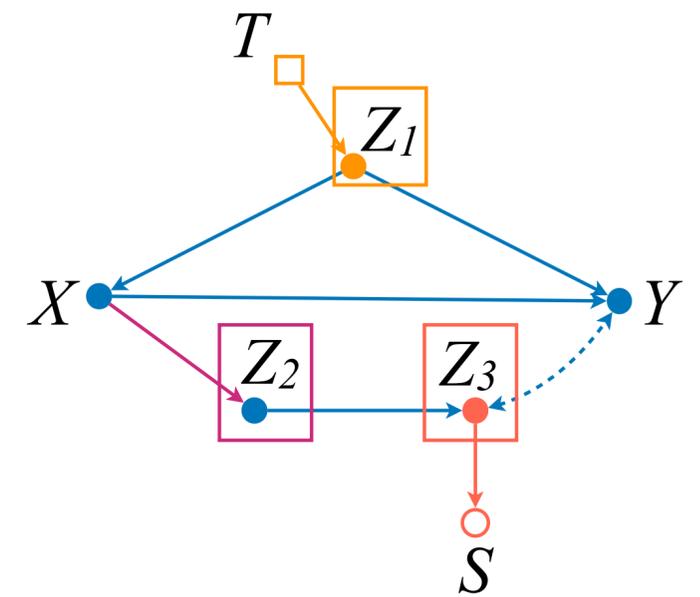
## Example



# Getting the intuition behind the rules

## Example

By making  $\mathbf{Z} = \{Z_1, Z_2, Z_3\}$ , we can verify the *st-adjustment* conditions, i.e.:

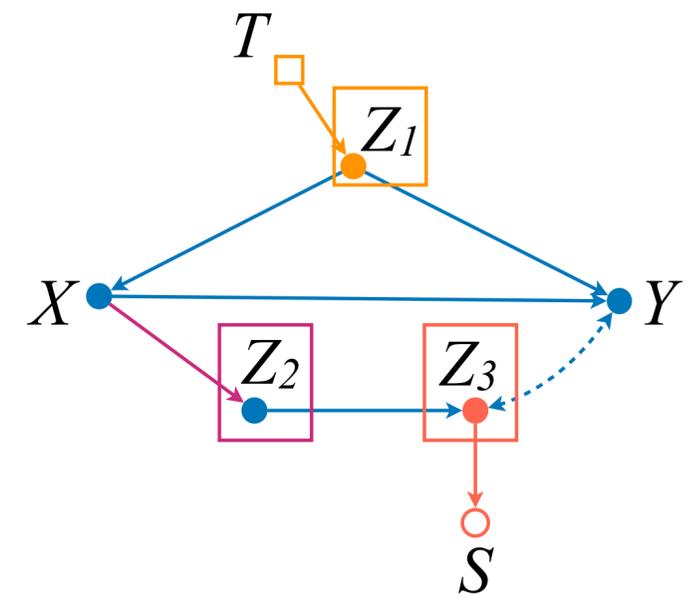


# Getting the intuition behind the rules

## Example

By making  $\mathbf{Z} = \{Z_1, Z_2, Z_3\}$ , we can verify the *st-adjustment* conditions, i.e.:

- (i) The variable in  $\mathbf{Z}_p = \{Z_3\}$  is independent of  $X$  given the other covariates  $\{Z_1, Z_2\}$ .

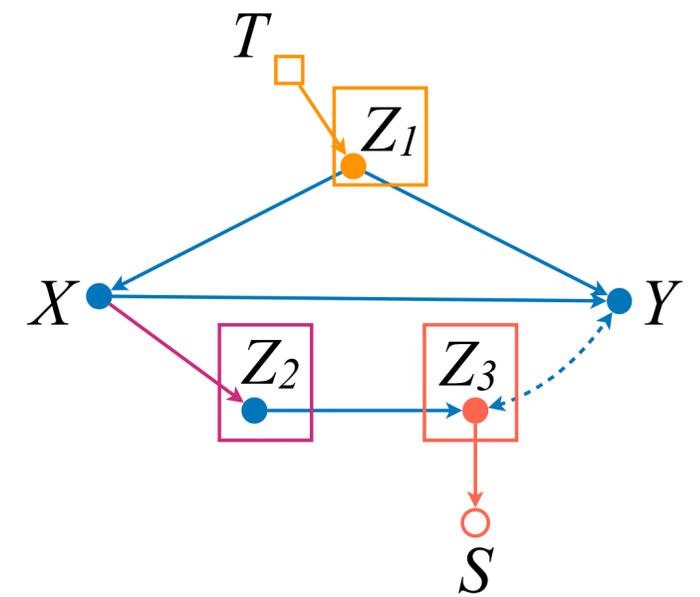


# Getting the intuition behind the rules

## Example

By making  $\mathbf{Z} = \{Z_1, Z_2, Z_3\}$ , we can verify the *st-adjustment* conditions, i.e.:

- (i) The variable in  $\mathbf{Z}_p = \{Z_3\}$  is independent of  $X$  given the other covariates  $\{Z_1, Z_2\}$ .
- (ii) The outcome  $Y$  is independent of  $S$  and  $T$  given  $\mathbf{Z}$ .

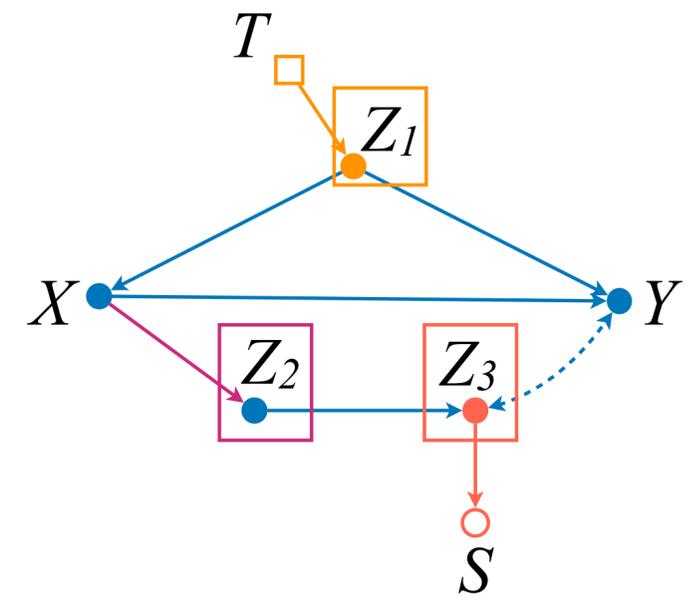


# Getting the intuition behind the rules

## Example

By making  $\mathbf{Z} = \{Z_1, Z_2, Z_3\}$ , we can verify the *st-adjustment* conditions, i.e.:

- (i) The variable in  $\mathbf{Z}_p = \{Z_3\}$  is independent of  $X$  given the other covariates  $\{Z_1, Z_2\}$ .
- (ii) The outcome  $Y$  is independent of  $S$  and  $T$  given  $\mathbf{Z}$ .



Hence, the *st-adjustment* is guaranteed to hold, i.e.:

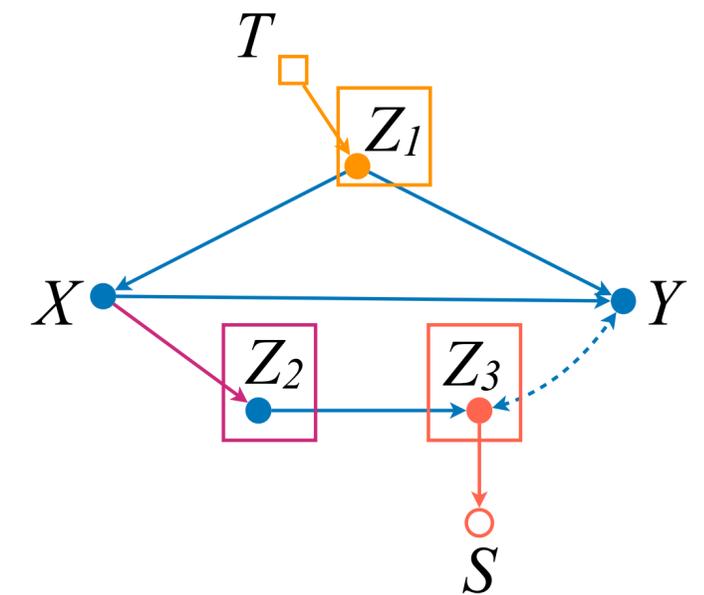
$$P^*(y | do(x)) = \sum_{z_1, z_2, z_3} P(y | do(x), z_1, z_2, z_3, S = 1) P^*(z_1, z_2, z_3)$$

# Getting the intuition behind the rules

## Example

By making  $\mathbf{Z} = \{Z_1, Z_2, Z_3\}$ , we can verify the *st-adjustment* conditions, i.e.:

- (i) The variable in  $\mathbf{Z}_p = \{Z_3\}$  is independent of  $X$  given the other covariates  $\{Z_1, Z_2\}$ .
- (ii) The outcome  $Y$  is independent of  $S$  and  $T$  given  $\mathbf{Z}$ .



Hence, the *st-adjustment* is guaranteed to hold, i.e.:

$$P^*(y | do(x)) = \sum_{z_1, z_2, z_3} P(y | do(x), z_1, z_2, z_3, S = 1) P^*(z_1, z_2, z_3)$$

causal effect in target domain      experimental data from the source under selection bias      measurements from the target domain

# Challenge II.

## Searching for Admissible Sets

# Challenge II.

## Searching for Admissible Sets

- Given a candidate set  $Z$ , we have a condition to determine if it is admissible or not.

# Challenge II.

## Searching for Admissible Sets

- Given a candidate set  $Z$ , we have a condition to determine if it is admissible or not.
- The natural question that follows is how to find an admissible set without resorting to trial and error. There could be exponentially many candidates (and even valid ones!).

# Challenge II.

## Searching for Admissible Sets

- Given a candidate set  $Z$ , we have a condition to determine if it is admissible or not.
- The natural question that follows is how to find an admissible set without resorting to trial and error. There could be exponentially many candidates (and even valid ones!).
- How to determine the existence of at least one admissible set?

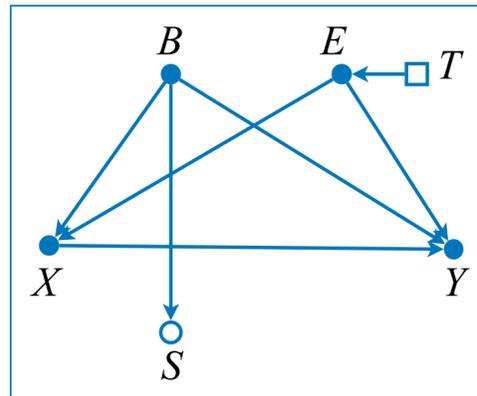
# Challenge II.

## Searching for Admissible Sets

- Given a candidate set  $Z$ , we have a condition to determine if it is admissible or not.
- The natural question that follows is how to find an admissible set without resorting to trial and error. There could be exponentially many candidates (and even valid ones!).
- How to determine the existence of at least one admissible set?
- There are sets that could be preferred among other admissible ones due to certain properties (e.g., cost, variance).

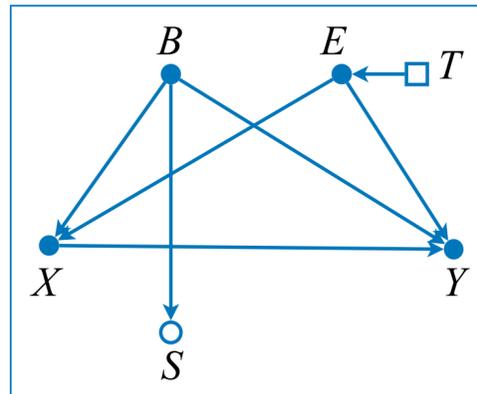
# Main Result II: Listing Algorithm

# Main Result II: Listing Algorithm



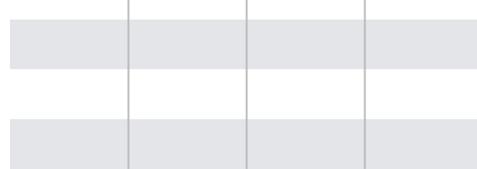
Selection Diagram  $D$

# Main Result II: Listing Algorithm



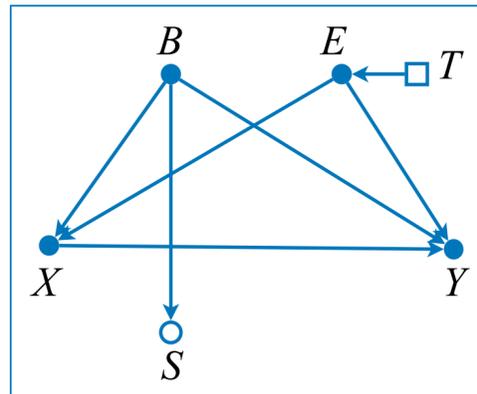
Selection Diagram  $D$

$$P(\mathbf{v} \mid do(x), S = 1)$$



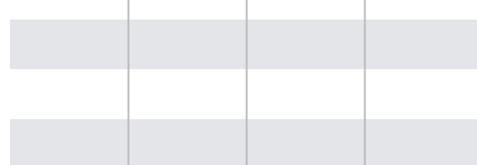
Selection-biased Exp.  
Distribution from  $\pi$

# Main Result II: Listing Algorithm



Selection Diagram  $D$

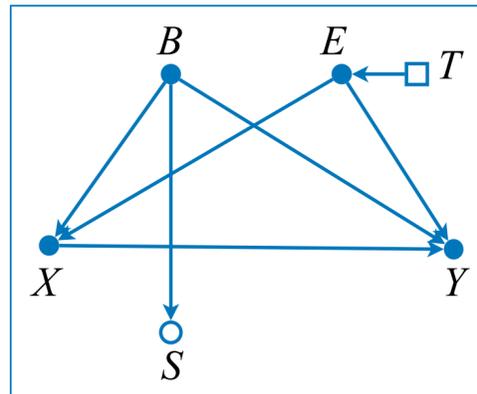
$$P(\mathbf{v} \mid do(x), S = 1)$$



Selection-biased Exp.  
Distribution from  $\pi$

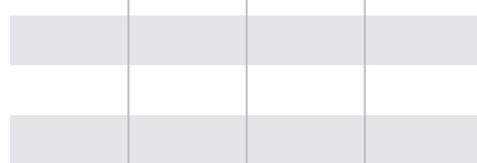
Set  $\mathbf{W}$  of covariates  
measurable in  $\pi^*$

# Main Result II: Listing Algorithm



Selection Diagram  $D$

$$P(\mathbf{v} \mid do(x), S = 1)$$

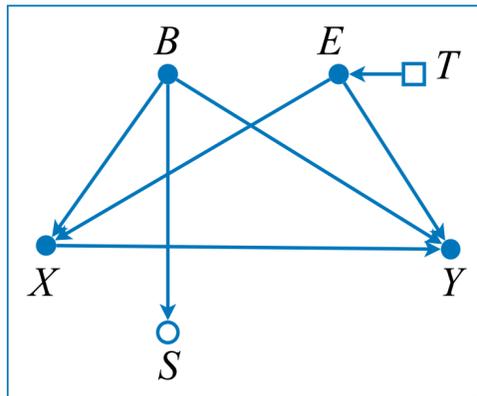


Selection-biased Exp.  
Distribution from  $\pi$

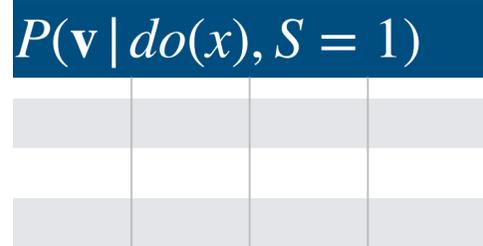
Set  $\mathbf{W}$  of covariates  
measurable in  $\pi^*$

What are all the admissible sets  
satisfying *st-adjustment*?

# Main Result II: Listing Algorithm



Selection Diagram  $D$



Selection-biased Exp. Distribution from  $\pi$

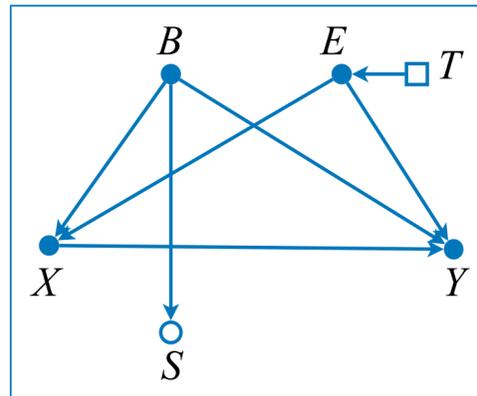
Set  $W$  of covariates measurable in  $\pi^*$

What are all the admissible sets satisfying *st-adjustment*?

List of of sets  $Z_1, Z_2, \dots \subseteq W$  such that for each  $Z_i$ :

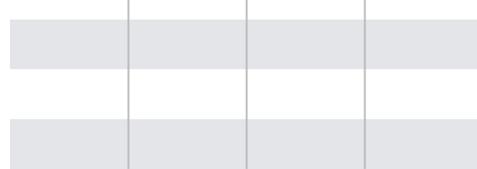
$$P^*(y | do(x)) = \sum_{z_i} P(y | do(x), z_i, S = 1) P^*(z_i)$$

# Main Result II: Listing Algorithm



Selection Diagram  $D$

$$P(\mathbf{v} \mid do(x), S = 1)$$



Selection-biased Exp. Distribution from  $\pi$

Set  $W$  of covariates measurable in  $\pi^*$

What are all the admissible sets satisfying *st-adjustment*?

List of of sets  $Z_1, Z_2, \dots \subseteq W$  such that for each  $Z_i$ :

$$P^*(y \mid do(x)) = \sum_{z_i} P(y \mid do(x), z_i, S = 1)P^*(z_i)$$

We provide an algorithm (Alg. 2) that works with polynomial delay (Thm. 6)

# Conclusions

# Conclusions

- Given a selection diagram, we describe complete conditions to determine whether adjusting by a given set of covariates is admissible for the identification of causal effects from experimental results in a source domain and some observations from the target domain.

# Conclusions

- Given a selection diagram, we describe complete conditions to determine whether adjusting by a given set of covariates is admissible for the identification of causal effects from experimental results in a source domain and some observations from the target domain.
- We provide a procedure to list valid adjustment sets given a set of variables that can be measured.

# Conclusions

- Given a selection diagram, we describe complete conditions to determine whether adjusting by a given set of covariates is admissible for the identification of causal effects from experimental results in a source domain and some observations from the target domain.
- We provide a procedure to list valid adjustment sets given a set of variables that can be measured.
- We hope the formal and transparent dressing given to the problem by our results can help researchers in health sciences, econometrics, reinforcement learning, marketing and others where extrapolating experimental results is crucial.

**Thank you!**



**#76**

**Poster**

# Polynomial delay [Takata '10]

- Time passing between the start of the execution and first output or failure is polynomial.
- Time between outputs is also polynomial.

