

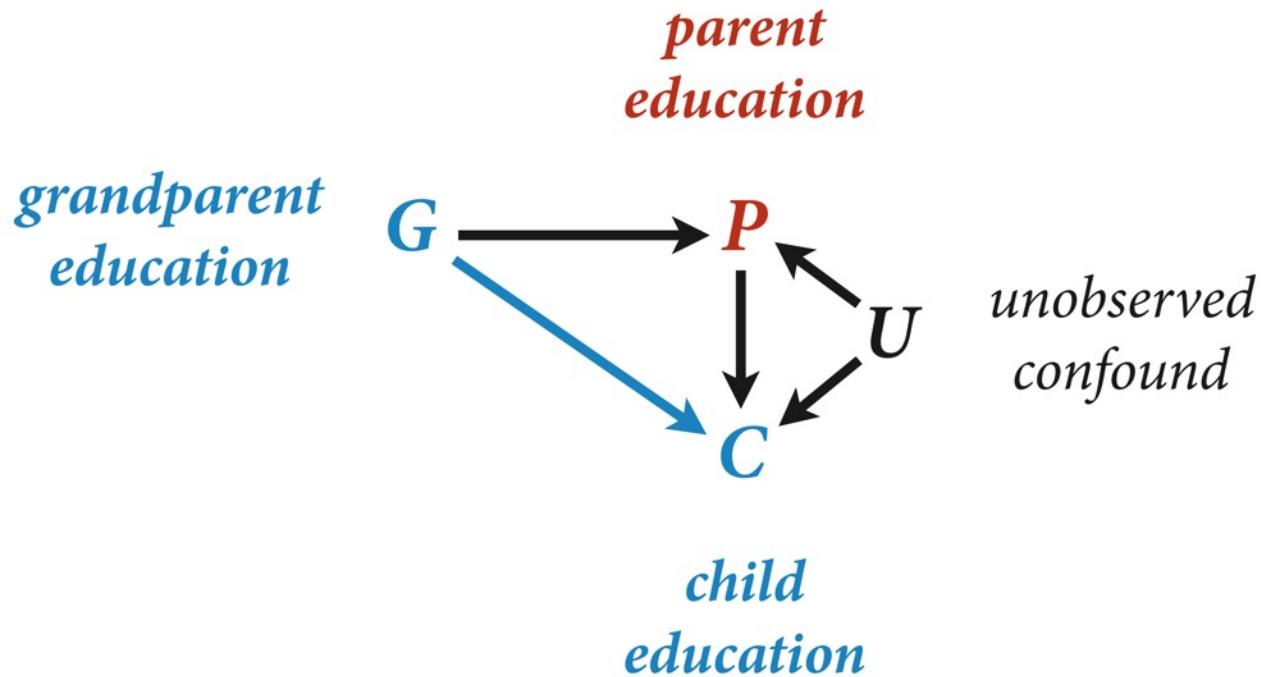
# Methods 4 - 6

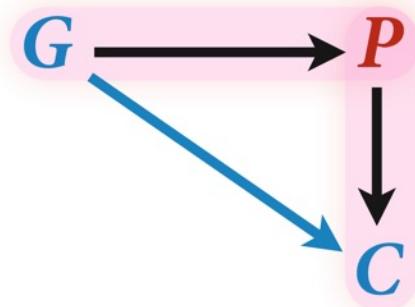
**Chris Mathys**



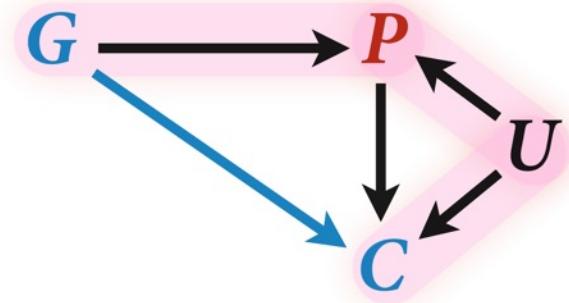
BSc Programme in Cognitive Science

Spring 2022



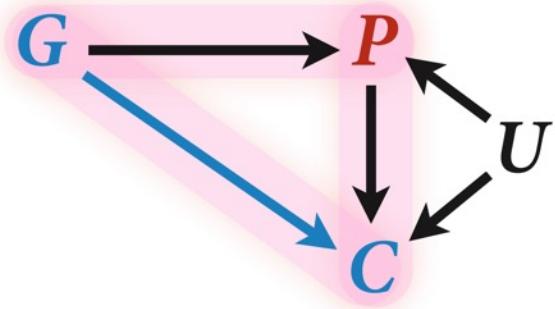


*P* is a mediator



*P* is a collider

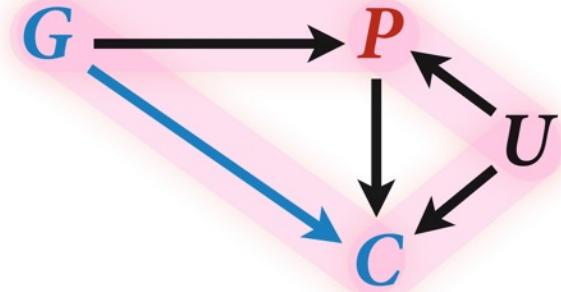
Can estimate **total**  
effect of  $G$  on  $C$



$$C_i \sim \text{Normal}(\mu_i, \sigma)$$

$$\mu_i = \alpha + \beta_G G_i$$

Cannot estimate  
**direct** effect



$$C_i \sim \text{Normal}(\mu_i, \sigma)$$

$$\mu_i = \alpha + \beta_G G_i + \beta_P P_i$$

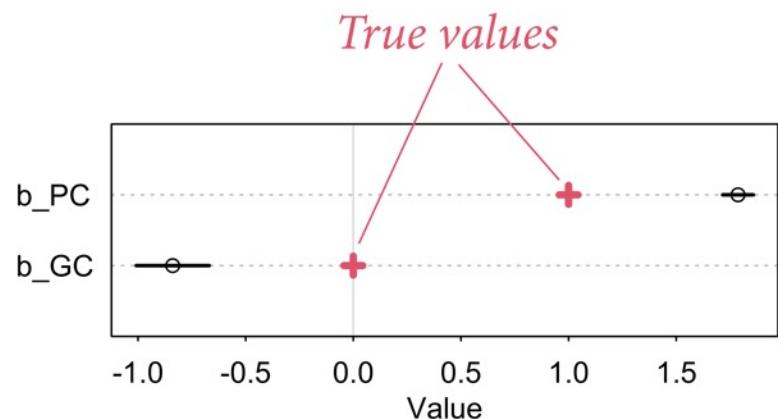
```

N <- 200 # num grandparent-parent-child triads
b_GP <- 1 # direct effect of G on P
b_GC <- 0 # direct effect of G on C
b_PC <- 1 # direct effect of P on C
b_U <- 2 #direct effect of U on P and C

set.seed(1)
U <- 2*rbern( N , 0.5 ) - 1
G <- rnorm( N )
P <- rnorm( N , b_GP*G + b_U*U )
C <- rnorm( N , b_PC*P + b_GC*G + b_U*U )
d <- data.frame( C=C , P=P , G=G , U=U )

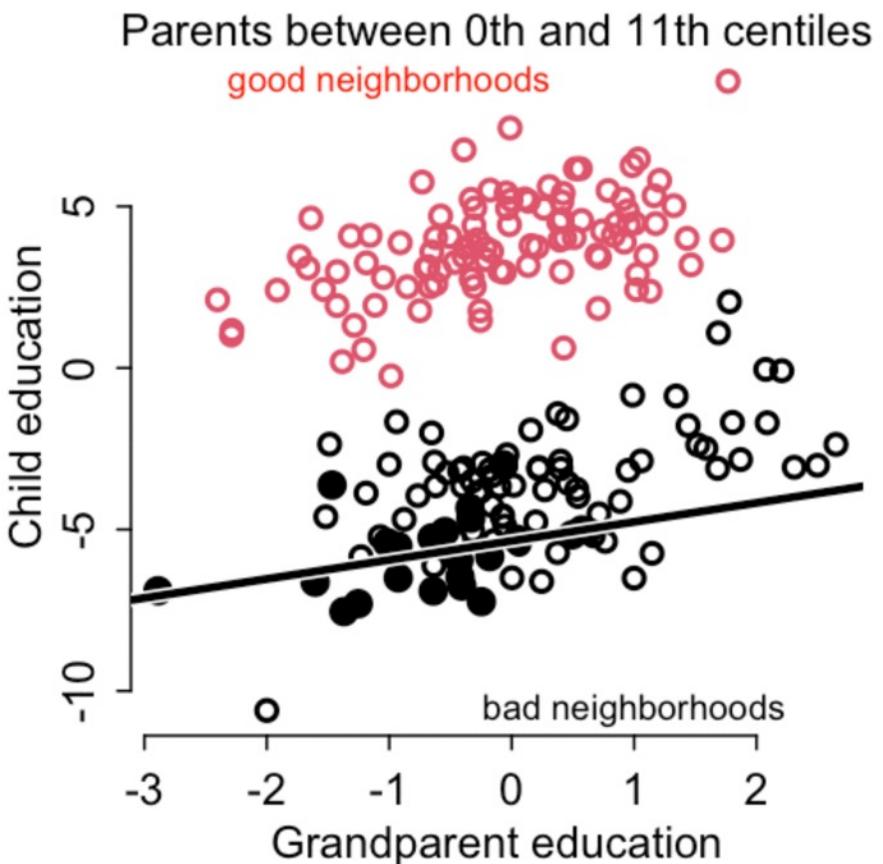
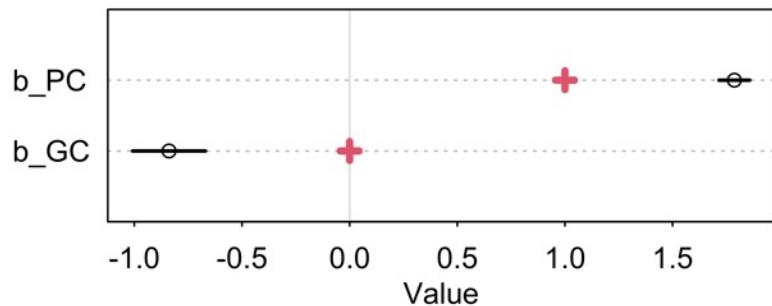
m6.11 <- quap(
  alist(
    C ~ dnorm( mu , sigma ),
    mu <- a + b_PC*P + b_GC*G,
    a ~ dnorm( 0 , 1 ),
    c(b_PC,b_GC) ~ dnorm( 0 , 1 ),
    sigma ~ dexp( 1 )
  ), data=d )

```



Stratify by parent centile  
(collider)

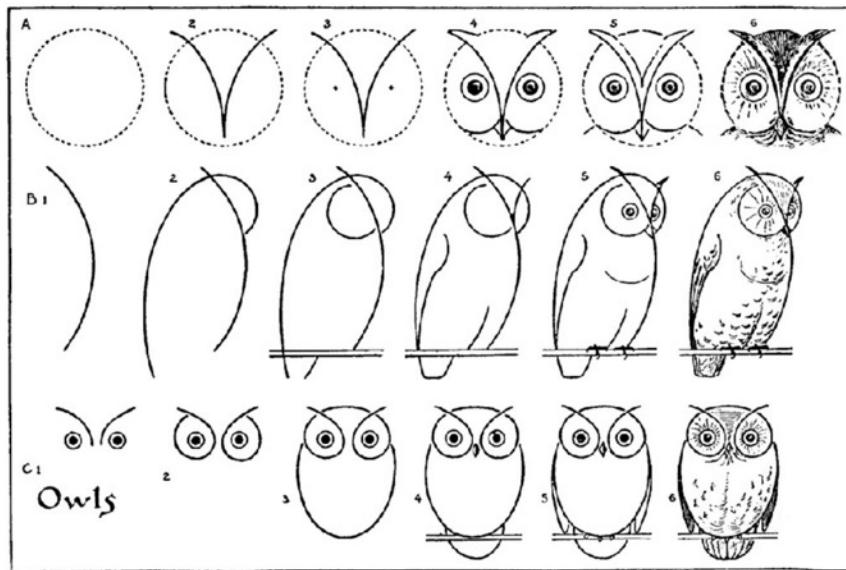
Two ways for parents to  
attain their education: from  
 $G$  or from  $U$



# From Theory to Estimate

Our job is to

- (1) Clearly state assumptions
- (2) Deduce implications
- (3) Test implications



# Avoid Being Clever At All Costs

Being clever is neither reliable nor transparent

Now what?

Given a causal model, can use logic to derive implications

Others can use same logic to verify/challenge your work



*The Fork*

$$X \leftarrow Z \rightarrow Y$$

$X$  and  $Y$  associated  
unless stratify by  $Z$

*The Pipe*

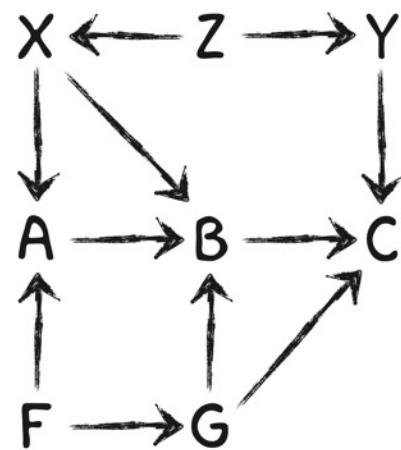
$$X \rightarrow Z \rightarrow Y$$

$X$  and  $Y$  associated  
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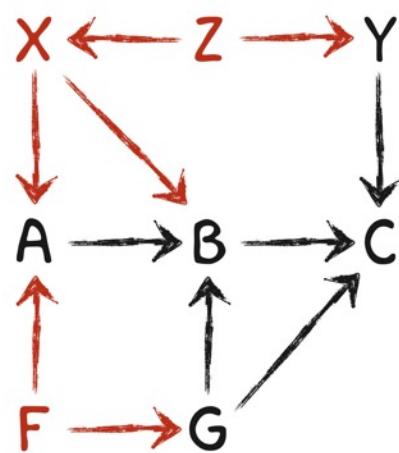
*The Collider*

$$X \rightarrow Z \leftarrow Y$$

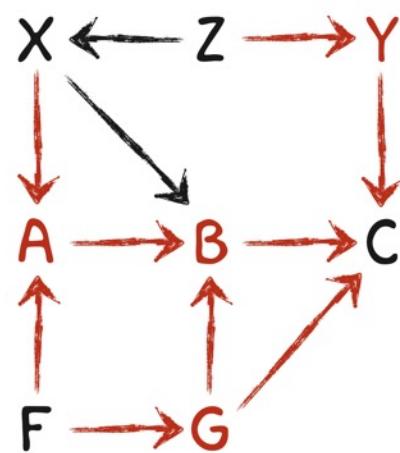
$X$  and  $Y$  not associated  
unless stratify by  $Z$



# Forks



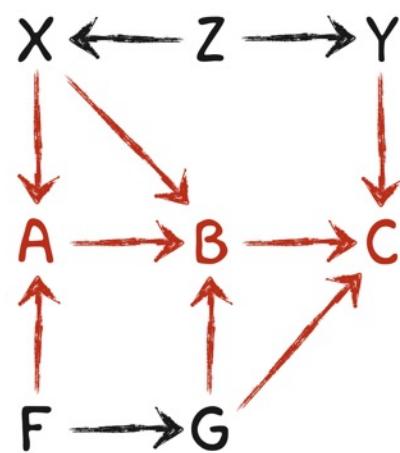
Forks  
Pipes

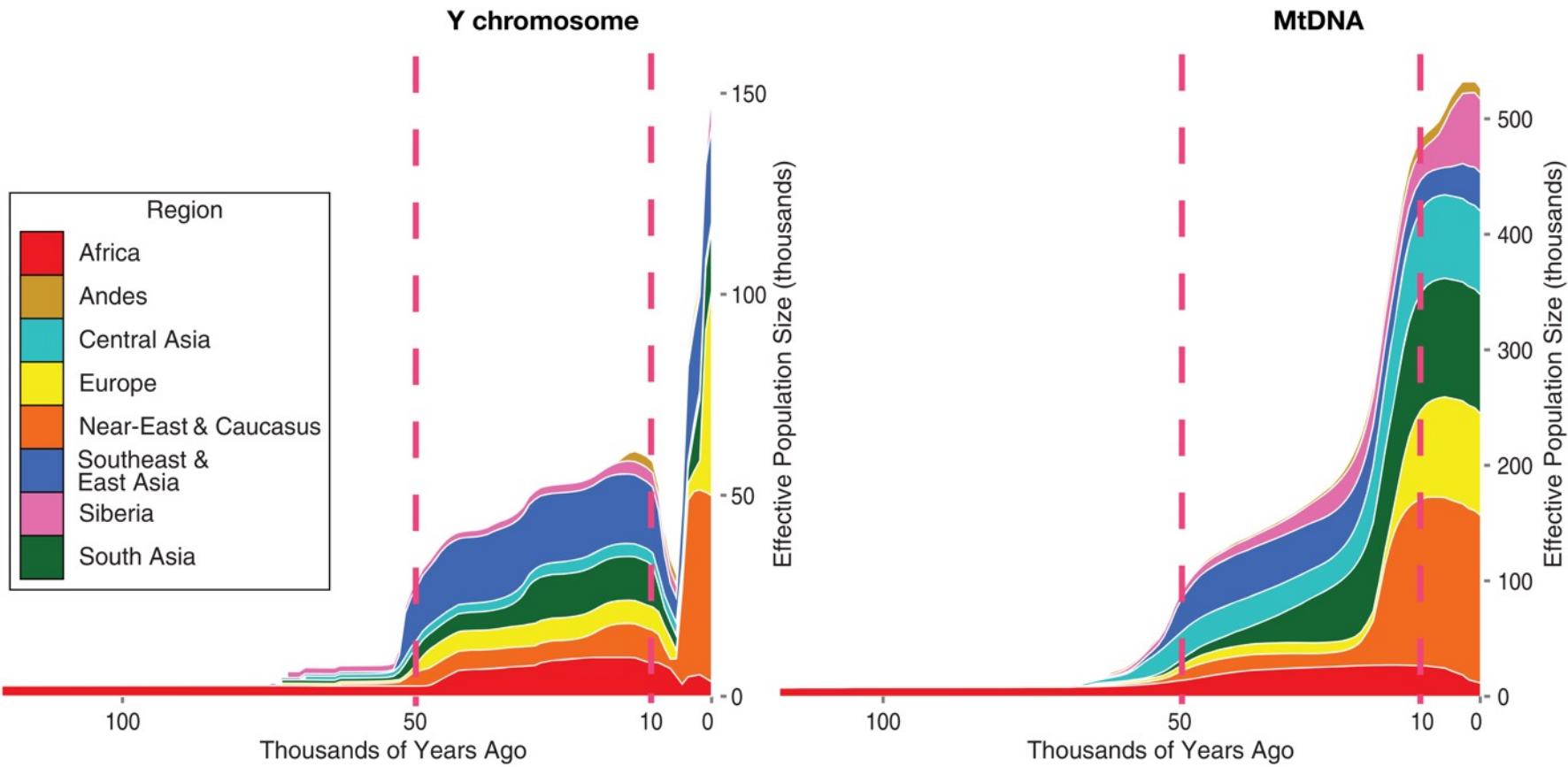


Forks

Pipes

Colliders





Karmin, M., (+100) et al., A recent bottleneck of Y chromosome diversity coincides with a global change in culture, *Genome research* 2015, DOI:10.1101/gr.186684.114

# DAG Thinking

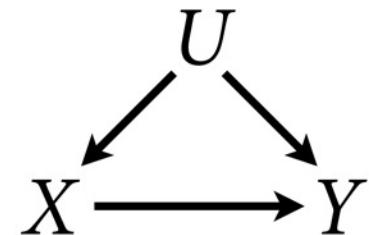
In an experiment, we cut causes of the treatment

We **randomize** (hopefully)

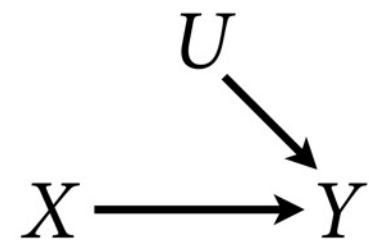
So how does causal inference without randomization ever work?

Is there a statistical procedure that mimics randomization?

*Without randomization*



*With randomization*



# DAG Thinking

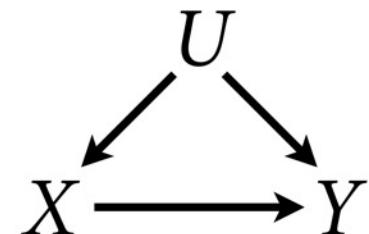
Is there a statistical procedure  
that mimics randomization?

$$P(Y | \text{do}(X)) = P(Y | ?)$$

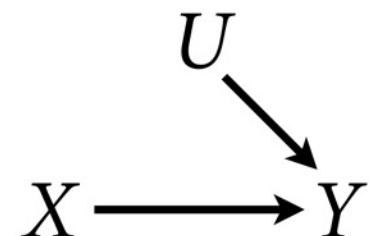
$\text{do}(X)$  means intervene on  $X$

Can analyze causal model to  
find answer (if it exists)

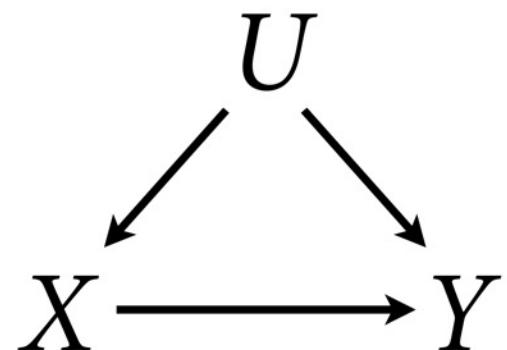
*Without randomization*



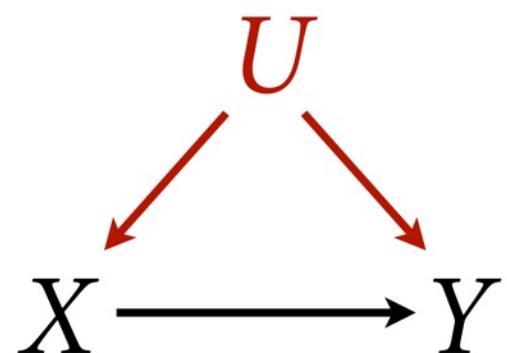
*With randomization*



# Example: Simple Confound



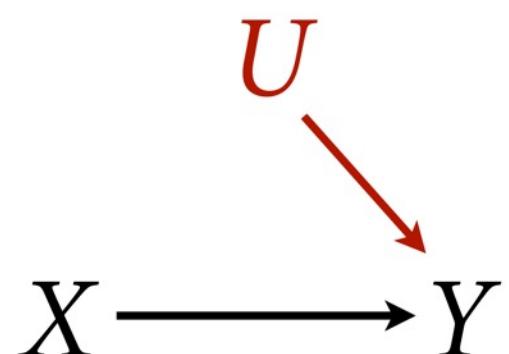
# Example: Simple Confound



*Non-causal path  
 $X <- U -> Y$*

*Close the fork!  
Condition on U*

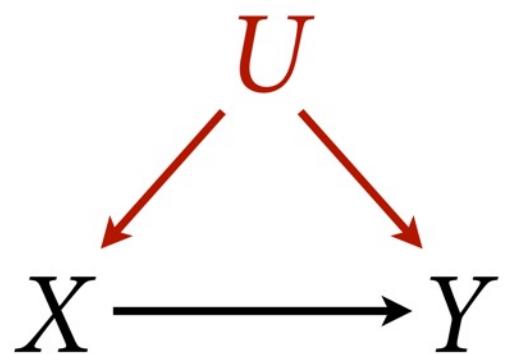
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# Example: Simple Confound



*Non-causal path  
X <- U -> Y*

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Condition on U*

$$P(Y | \text{do}(X)) = \sum_U P(Y | X, U)P(U) = \text{E}_U P(Y | X, U)$$

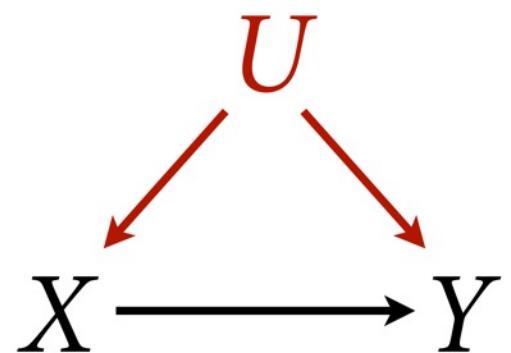
*“The distribution of Y, stratified by X and U,  
averaged over the distribution of U.”*

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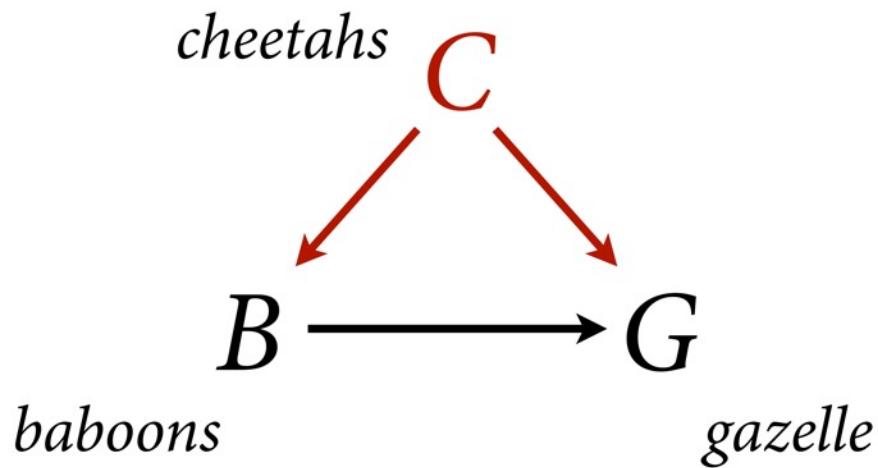
*“The distribution of Y, stratified by X and U, averaged over the distribution of U.”*

The causal effect of  $X$  on  $Y$  is **not** (in general) the **coefficient** relating  $X$  to  $Y$

It is the distribution of  $Y$  when we change  $X$ , **averaged** over the distributions of the control variables (here  $U$ )

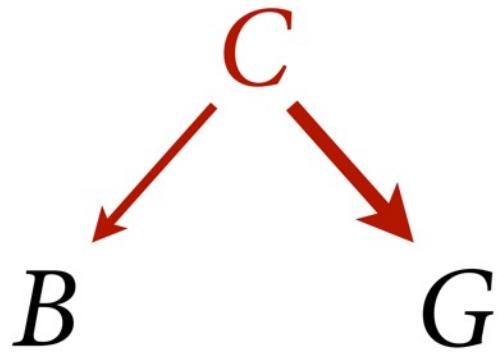


# Marginal Effects Example

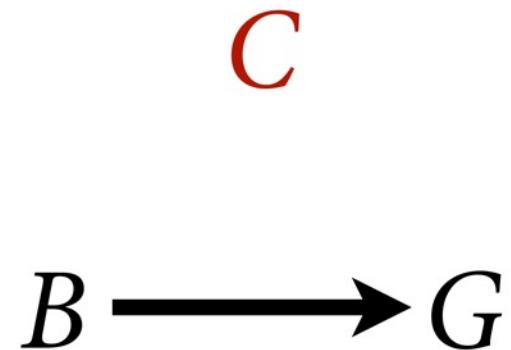


# Marginal Effects Example

*cheetahs present*



*cheetahs absent*



*Causal effect of baboons depends upon distribution of cheetahs*

# do-calculus

For DAGs, rules for finding  
 $P(Y|do(X))$  known as **do-calculus**

do-calculus says what is possible  
to say **before** picking functions

Additional assumptions yield  
additional implications

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THE BOOK OF WHY

## DO-CALCULUS AT WORK

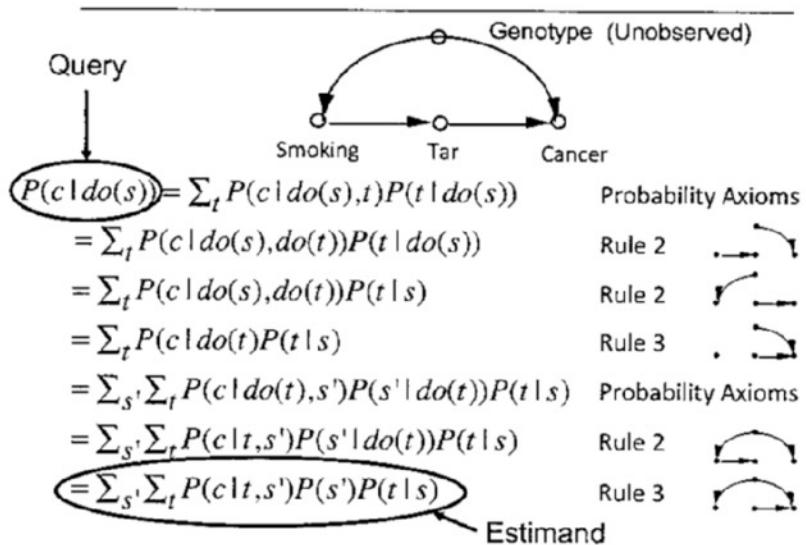
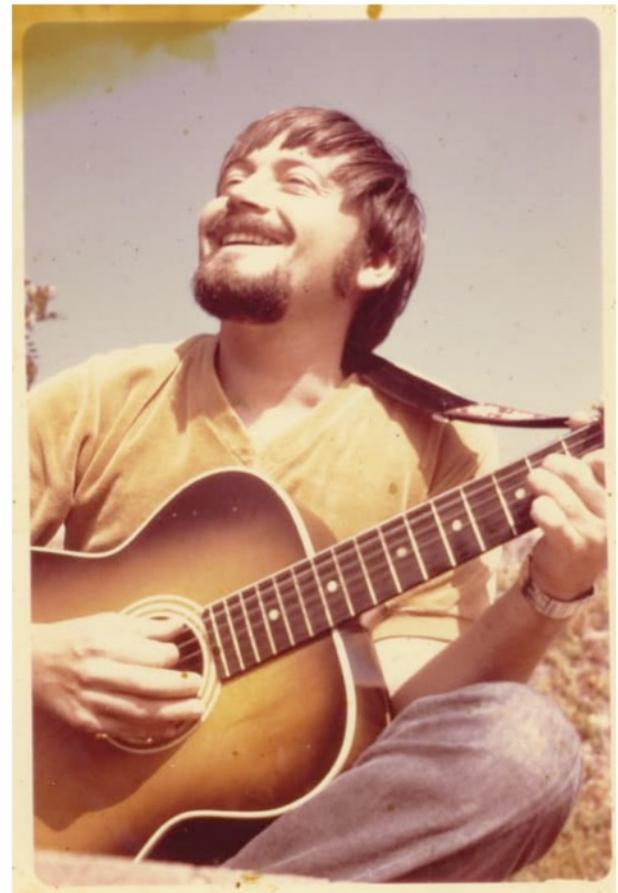


FIGURE 7.4. Derivation of the front-door adjustment formula from the rules of *do*-calculus.

# do-calculus

do-calculus is **worst case**:  
additional assumptions often  
allow stronger inference

do-calculus is **best case**:  
if inference possible by do-  
calculus, does not depend on  
special assumptions



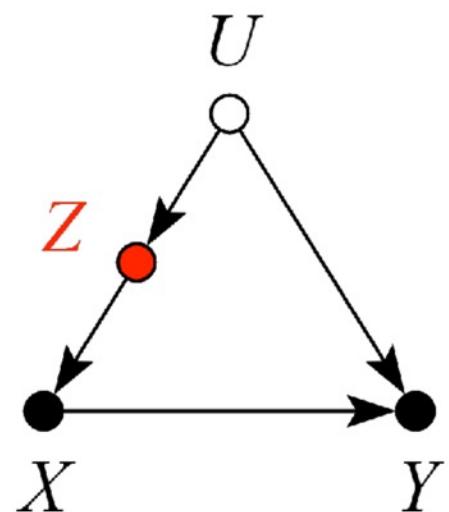
*Judea Pearl, father of  
do-calculus, in 1966*

# Backdoor Criterion

Very useful implication of do-calculus is the **Backdoor Criterion**

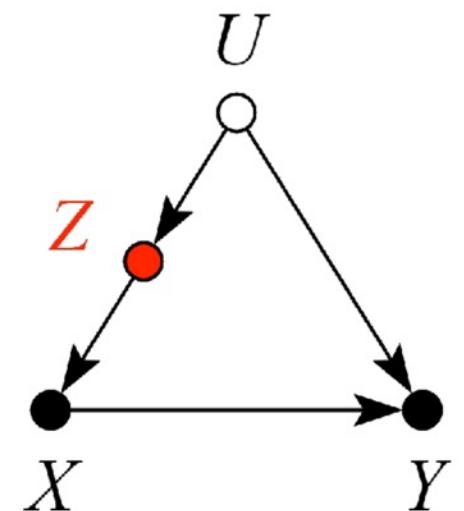
**Backdoor Criterion** is a shortcut to applying rules of do-calculus

Also inspires **strategies** for research design that yield valid estimates



# Backdoor Criterion

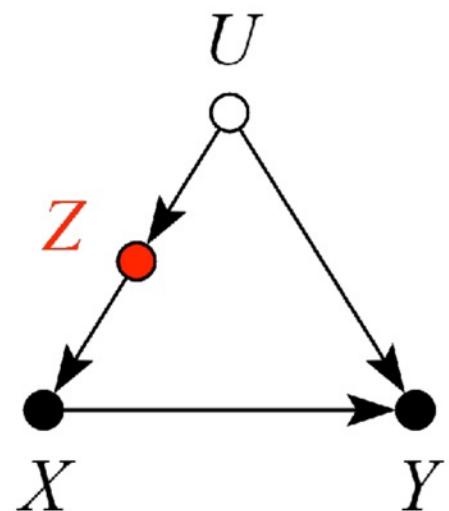
**Backdoor Criterion:** Rule to find a set of variables to stratify (condition) by to yield  $P(Y|\text{do}(X))$



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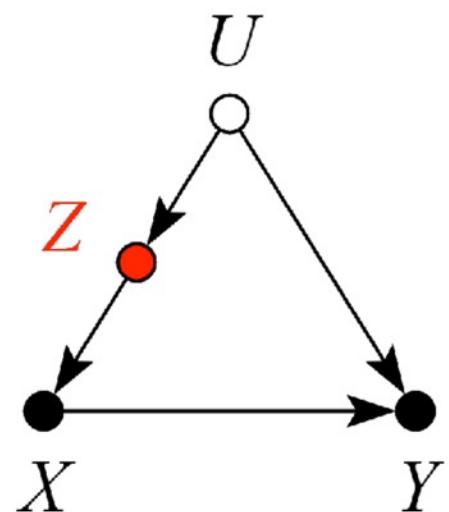
- (1) Identify all **paths** connection the treatment ( $X$ ) to the outcome ( $Y$ )



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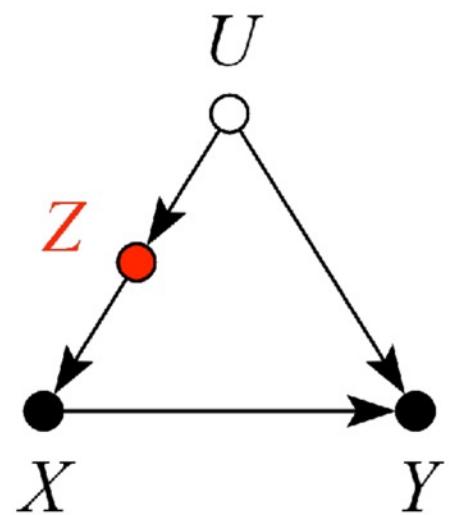
- (1) Identify all **paths** connection the treatment ( $X$ ) to the outcome ( $Y$ )
- (2) Paths with arrows **entering**  $X$  are backdoor paths (non-causal paths)



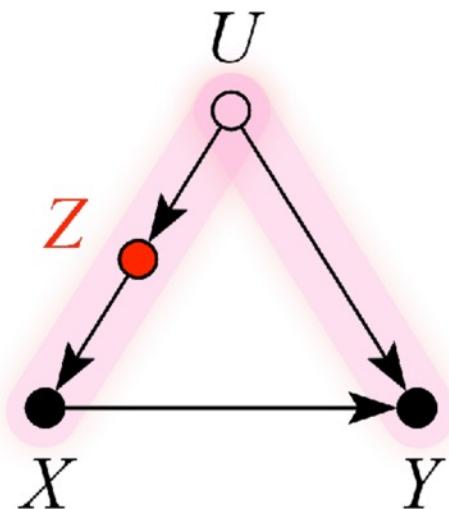
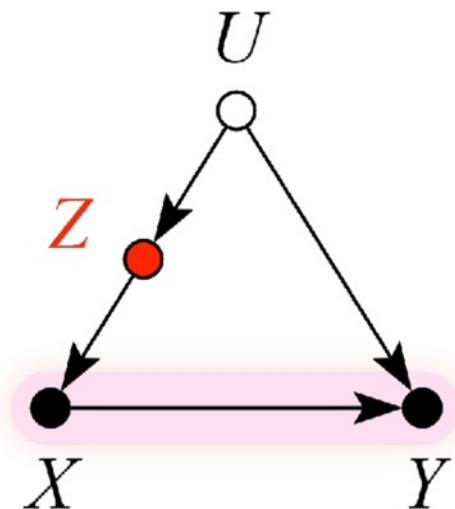
# Backdoor Criterion

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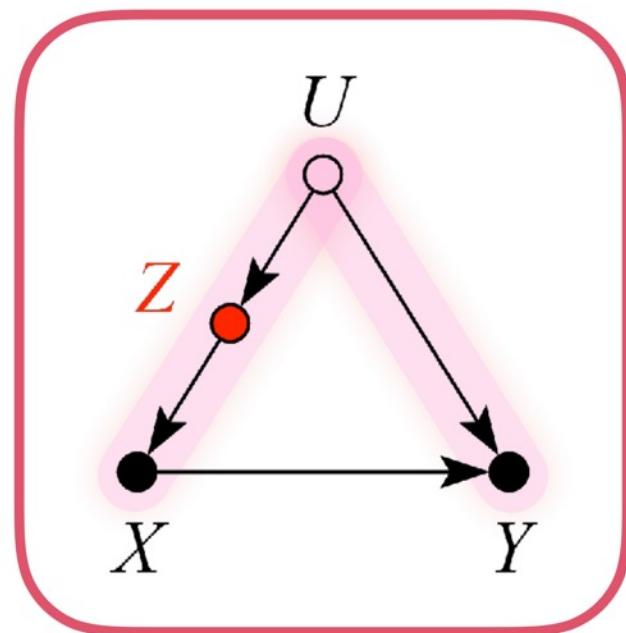
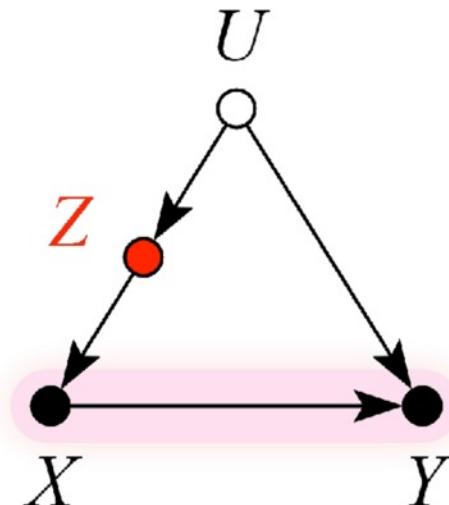
- (1) Identify all **paths** connection the treatment ( $X$ ) to the outcome ( $Y$ )
- (2) Paths with arrows **entering**  $X$  are backdoor paths (non-causal paths)
- (3) Find **adjustment set** that closes/blocks all backdoor paths



(1) Identify all **paths** connection the treatment ( $X$ ) to the outcome ( $Y$ )

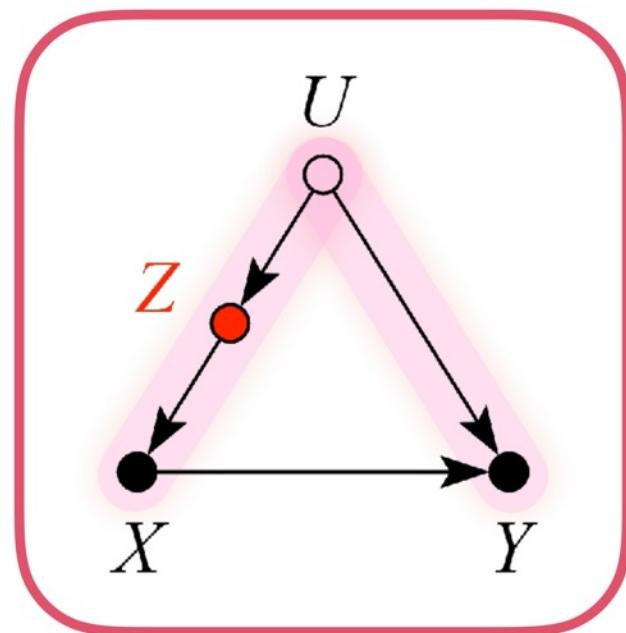


(2) Paths with arrows **entering**  $X$  are backdoor paths (non-causal paths)



(3) Find a set of control variables that close/block all backdoor paths

Block the pipe:  $X \perp\!\!\!\perp U | Z$



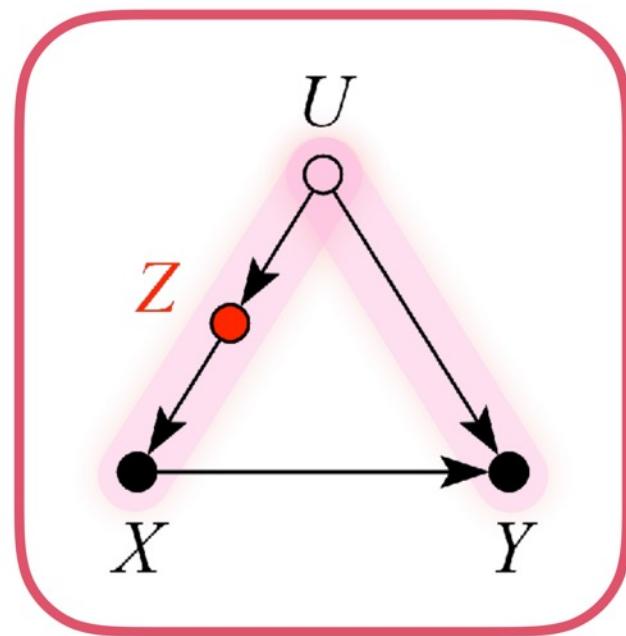
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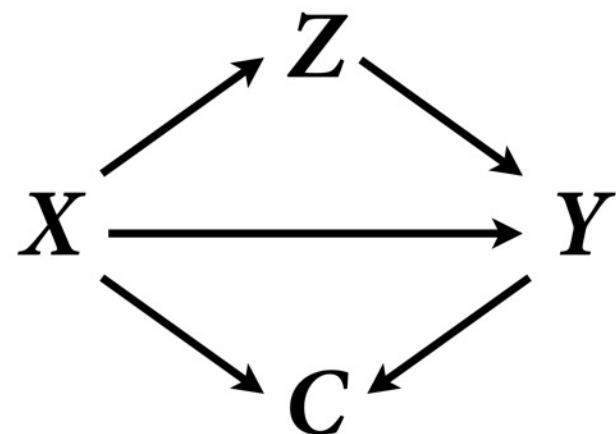
$$P(Y | \text{do}(X)) = \sum_U P(Y | X, Z)P(Z)$$

$$Y_i \sim \text{Normal}(\mu_i, \sigma)$$

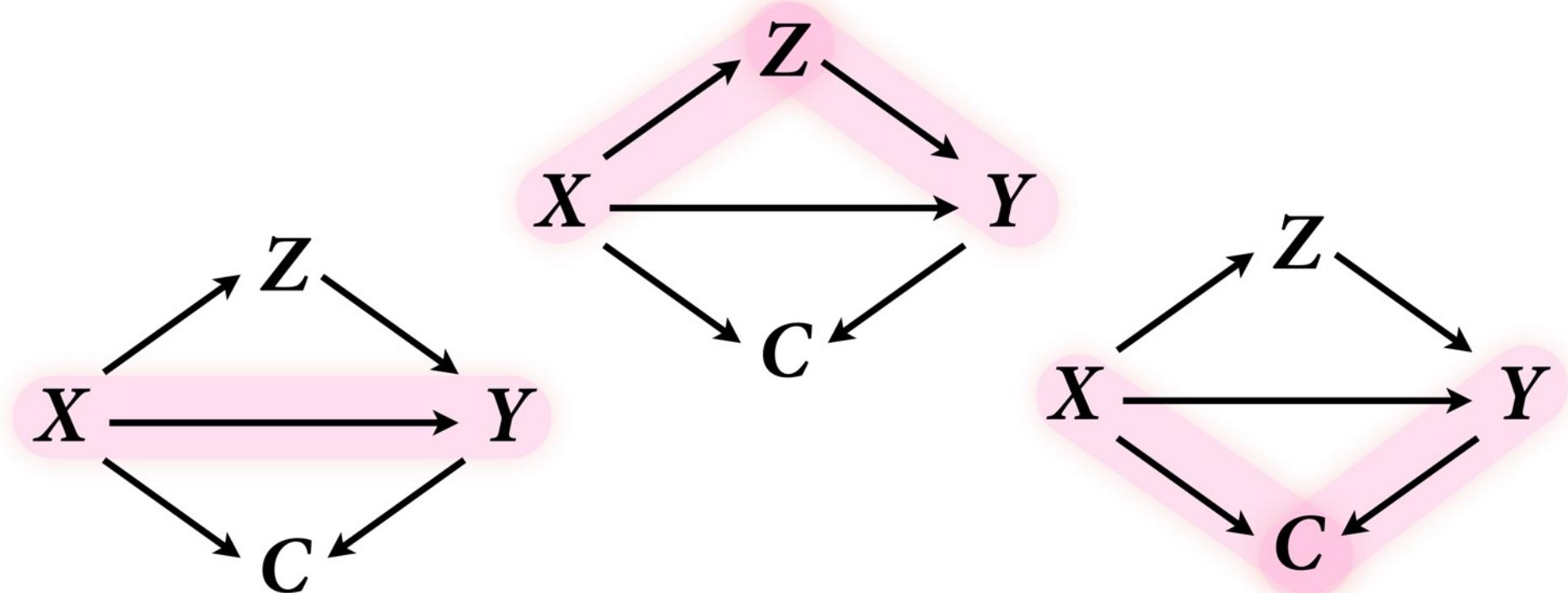
$$\mu_i = \alpha + \beta_X X_i + \beta_Z Z_i$$

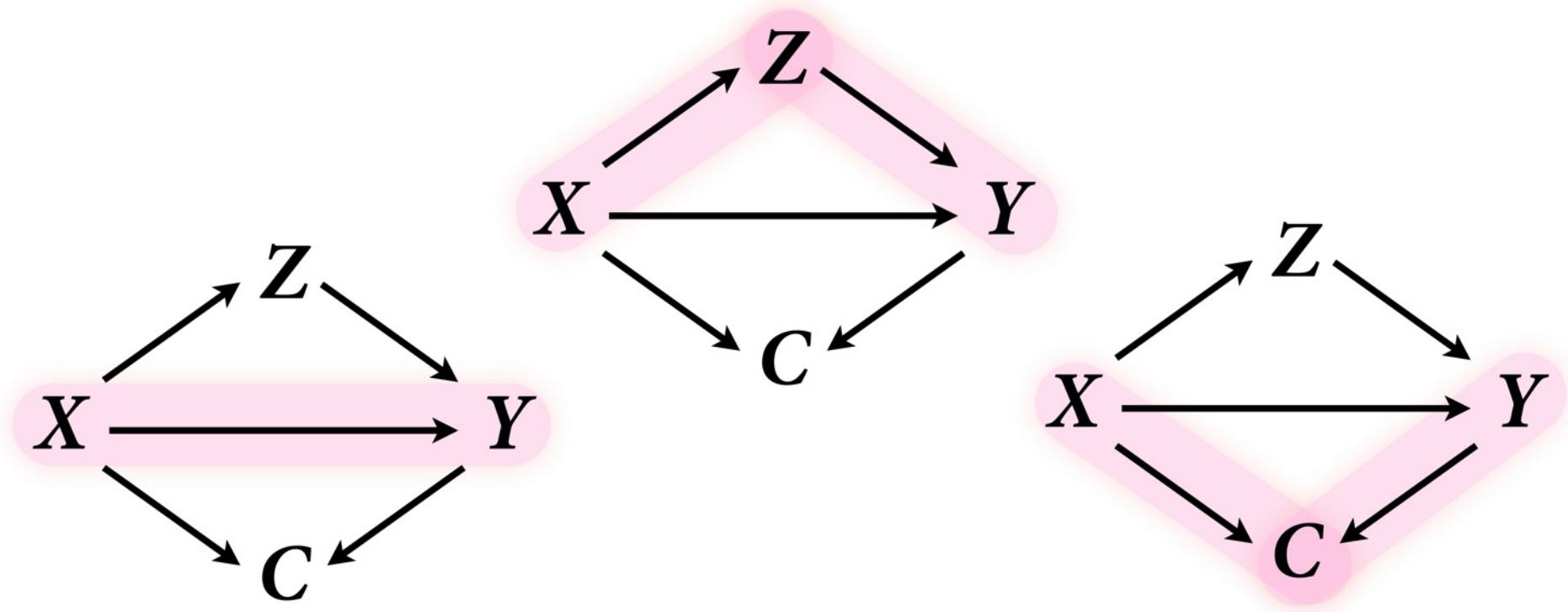


List all the paths connecting **X** and **Y**.  
Which need to be closed to estimate  
effect of **X** on **Y**?



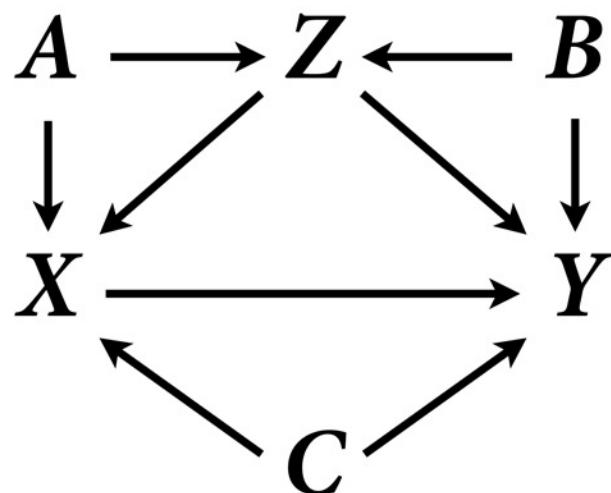
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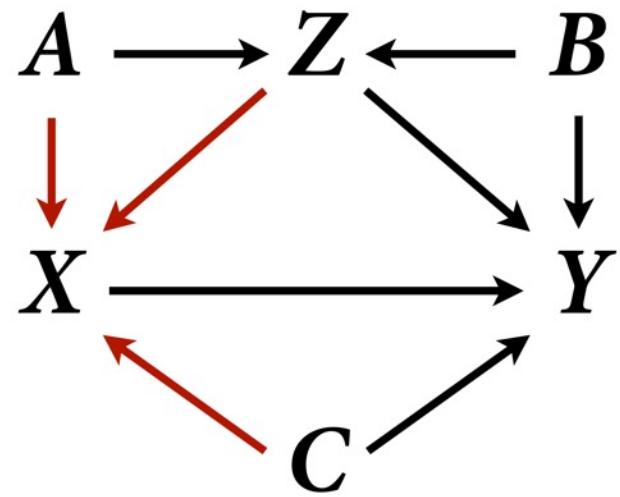
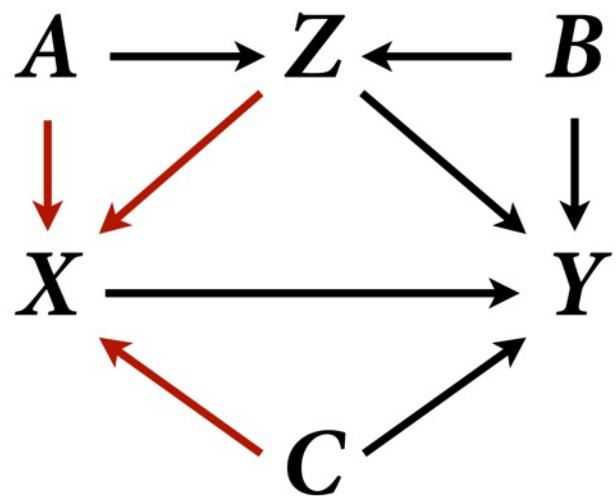


Adjustment set: *nothing!*

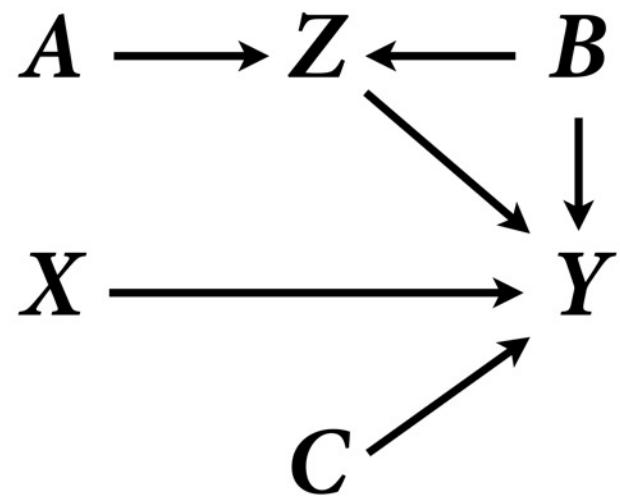
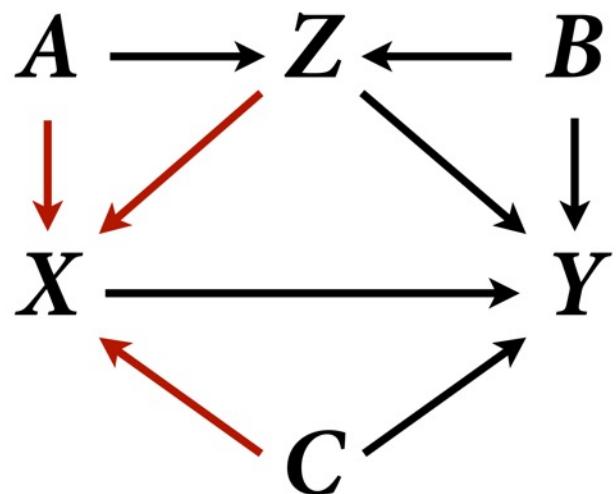
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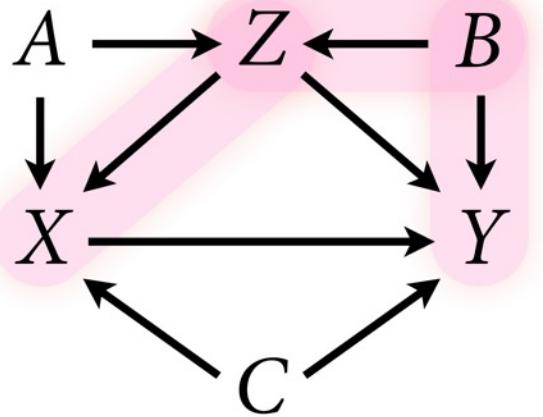
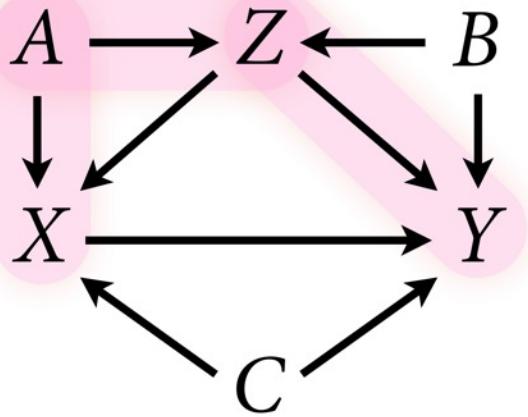
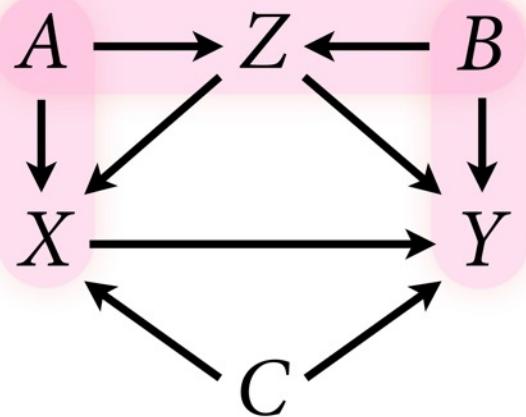
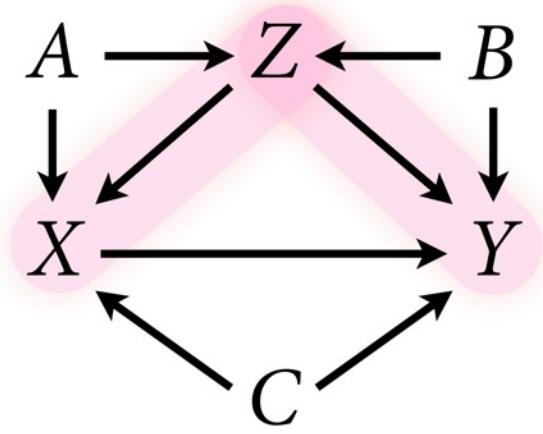
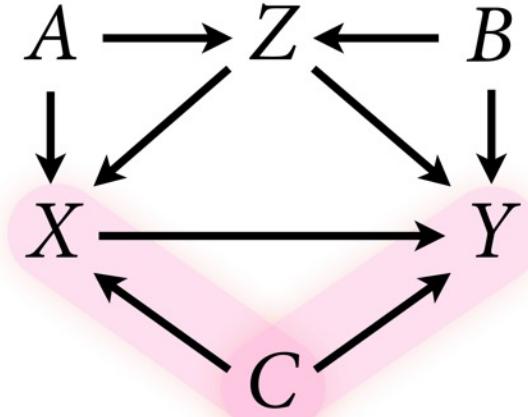
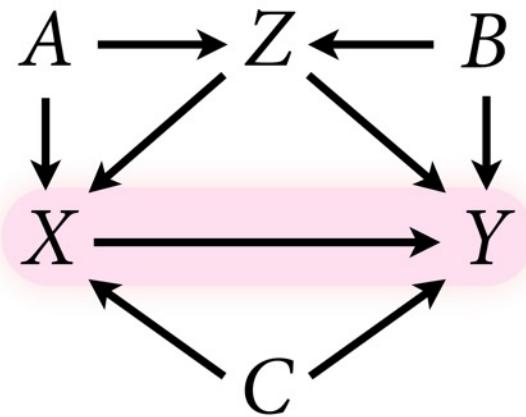


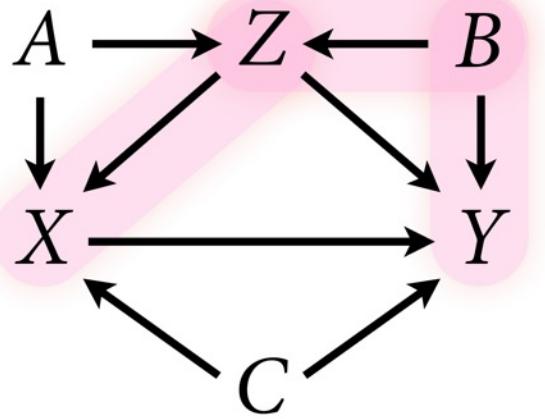
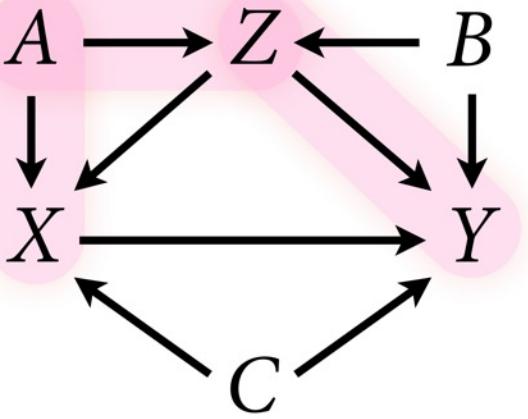
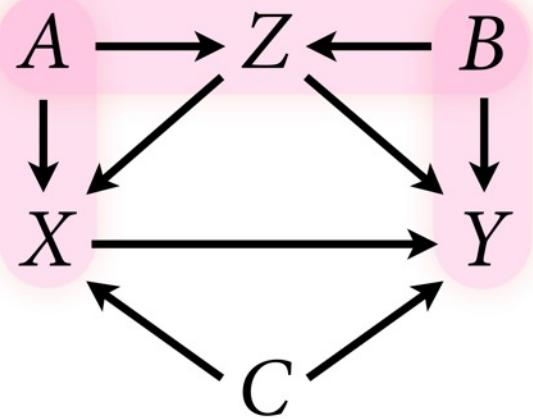
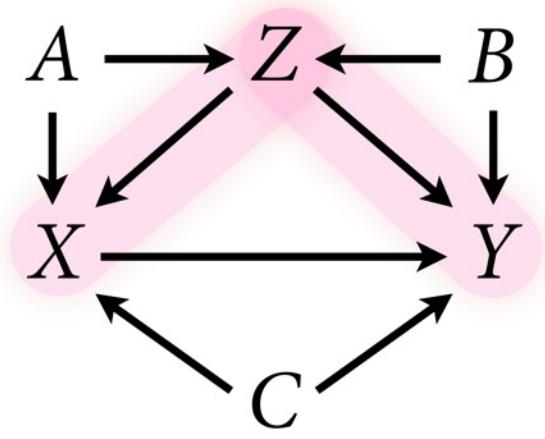
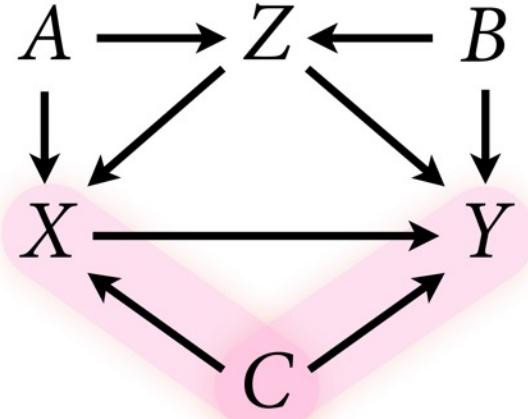
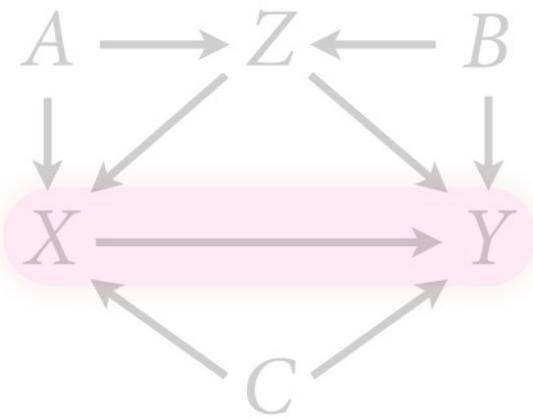
$$P(Y|\text{do}(X))$$

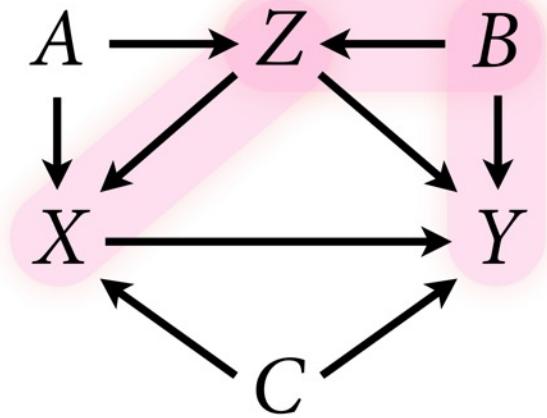
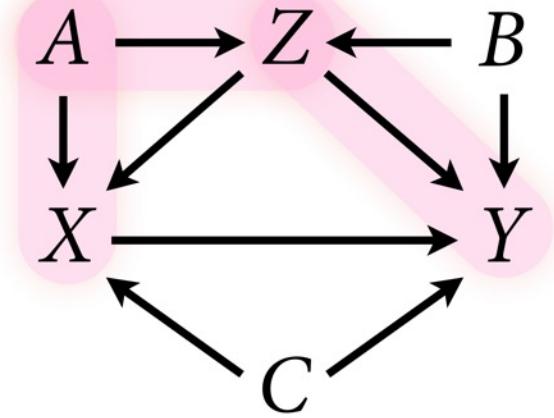
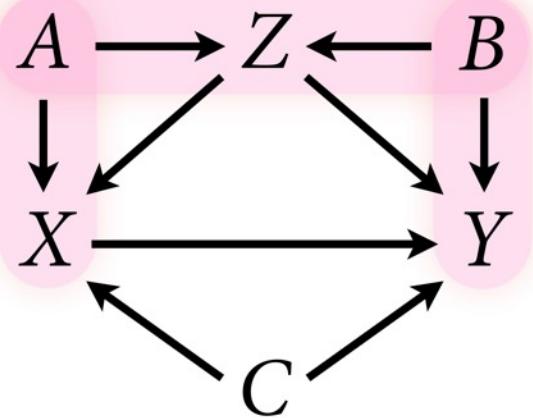
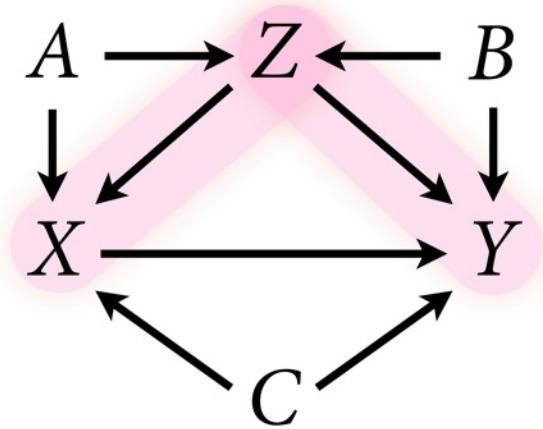
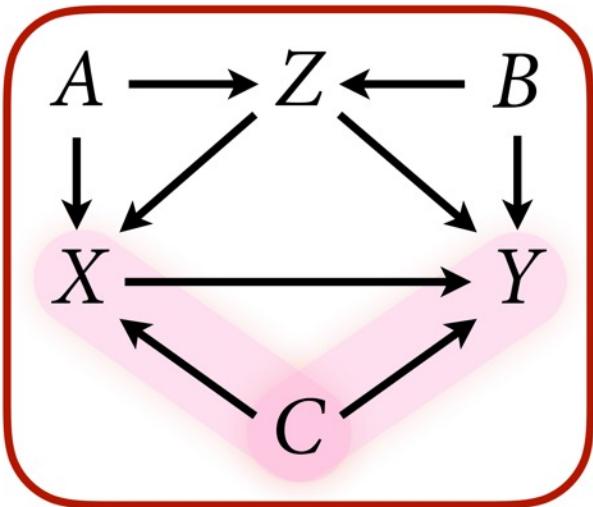
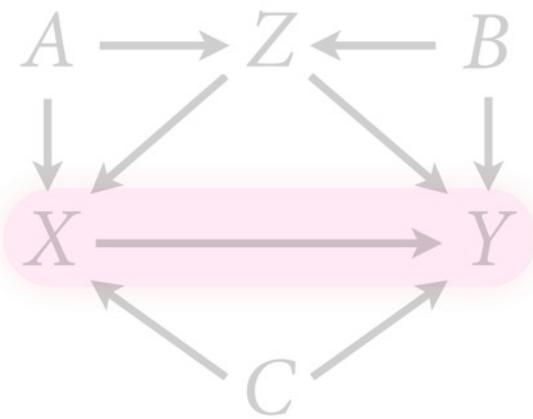


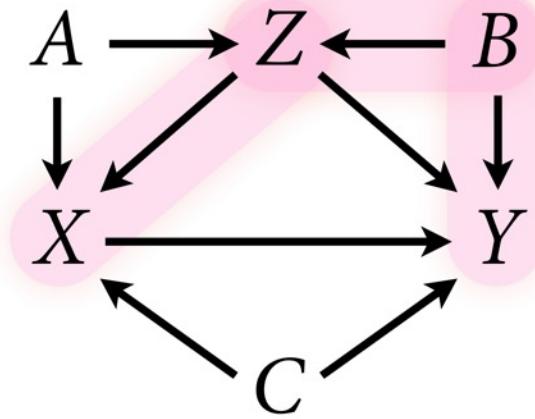
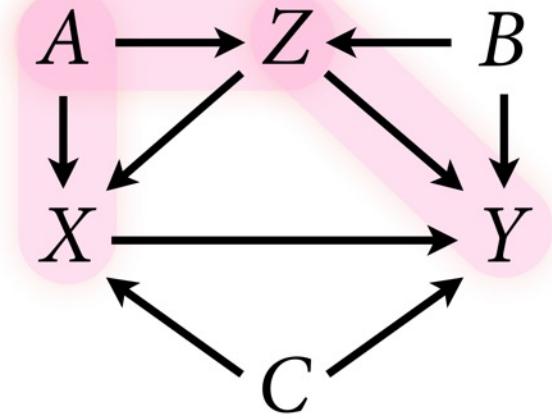
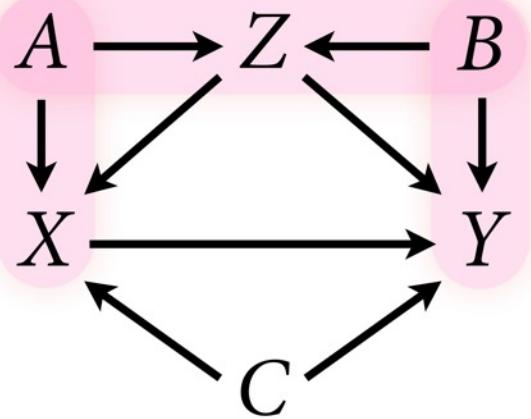
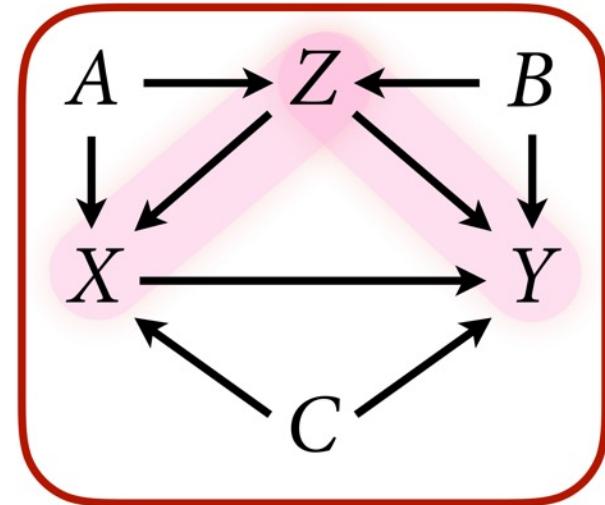
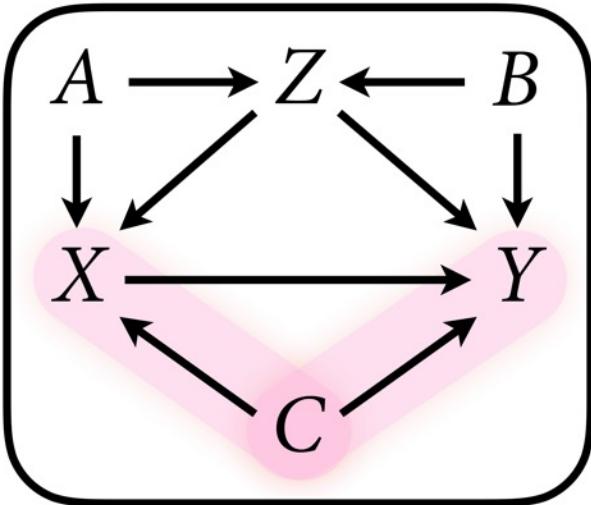
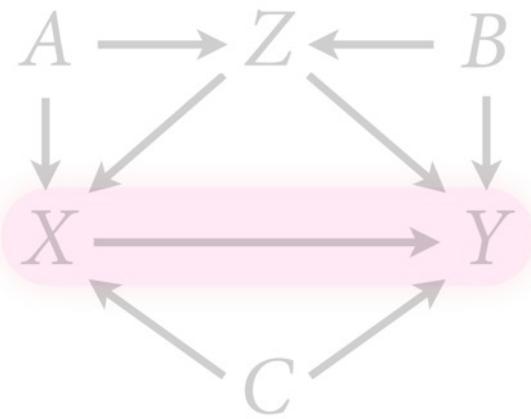
$$P(Y|\text{do}(X))$$

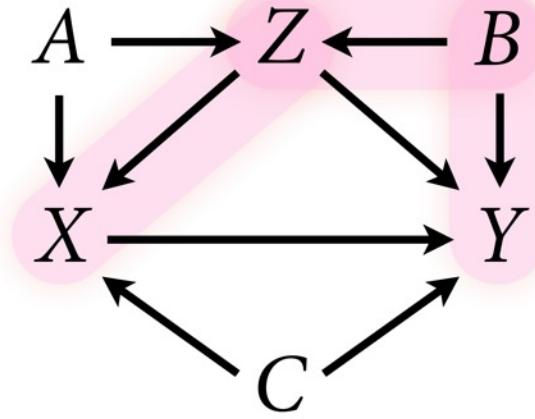
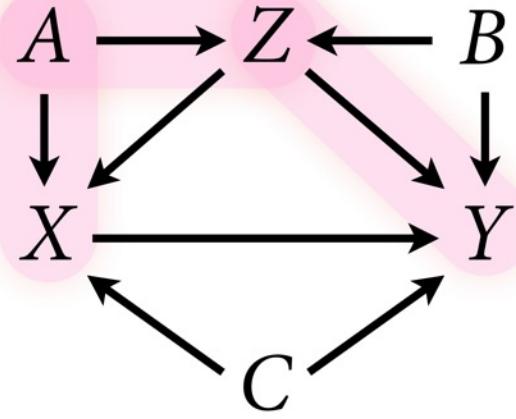
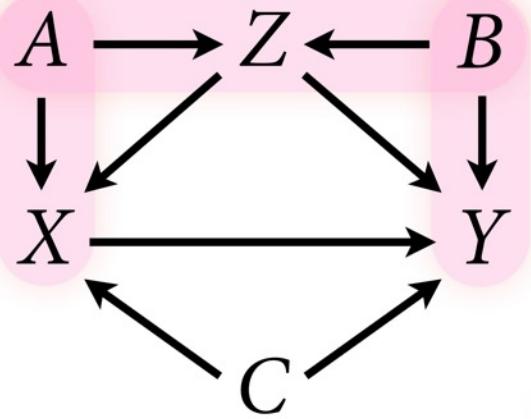
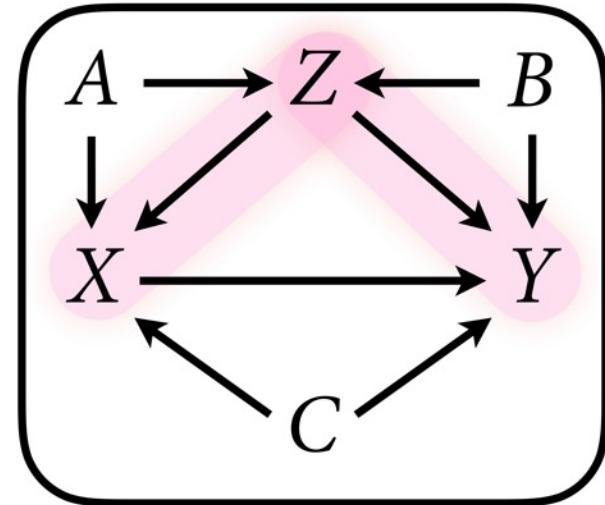
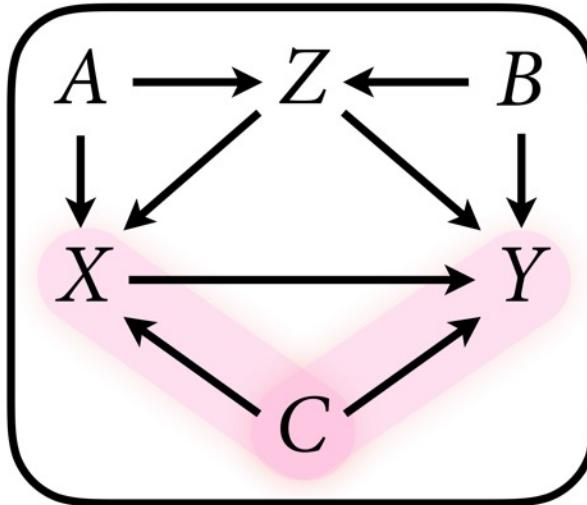
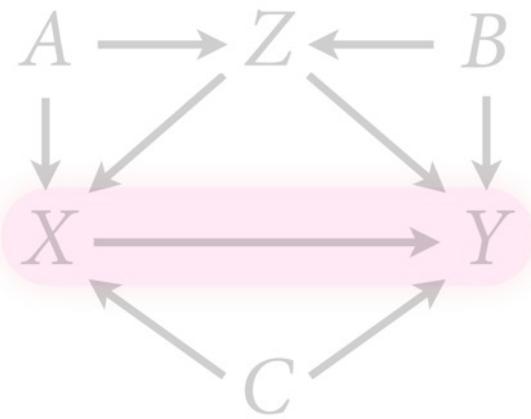


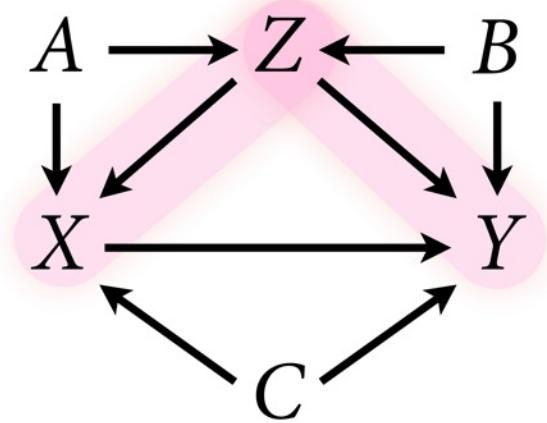
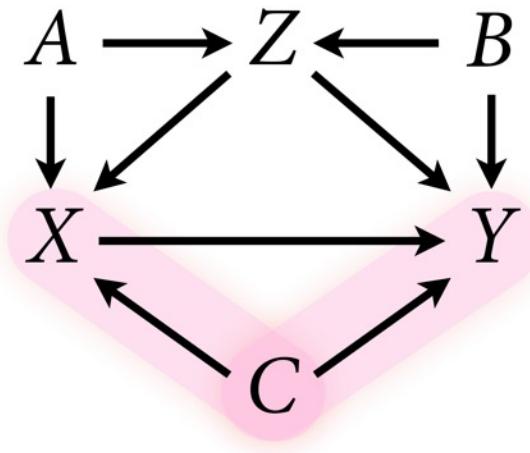
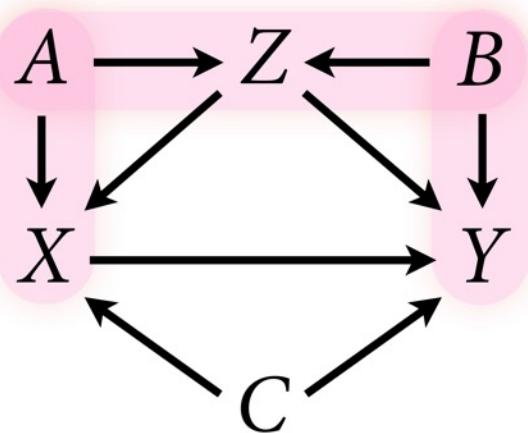












Adjustment set:  $C, Z$ , and either  $A$  or  $B$

( $B$  is better choice)

# www.dagitty.net

Model | Examples | How to ... | Layout | Help

Causal effect identification

Adjustment (total effect) ▾

Minimal sufficient adjustment sets for estimating the total effect of X on Y:

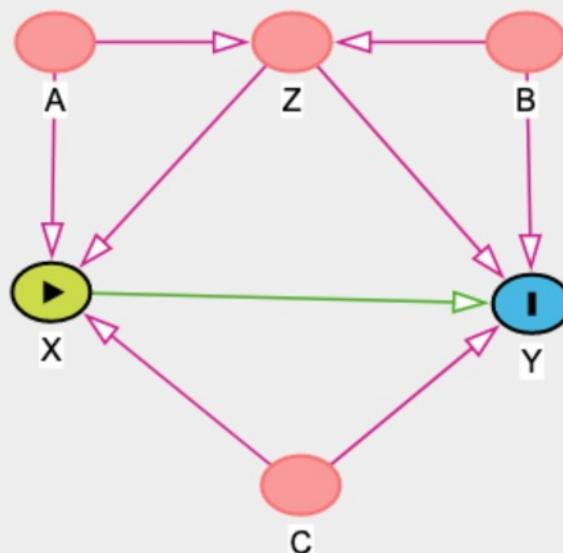
- A, C, Z
- B, C, Z

Testable implications

The model implies the following conditional independences:

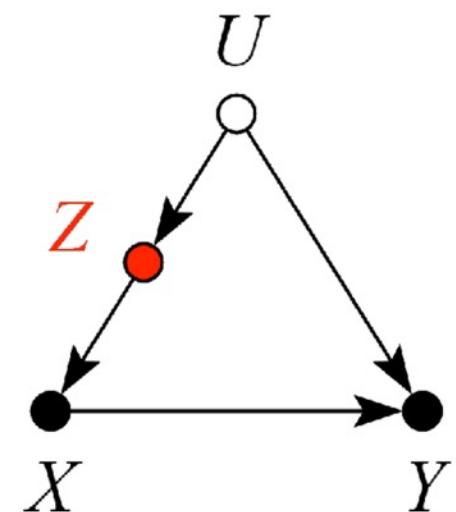
- $X \perp\!\!\!\perp B \mid A, Z$
- $Y \perp\!\!\!\perp A \mid B, C, X, Z$
- $A \perp\!\!\!\perp B$
- $A \perp\!\!\!\perp C$
- $B \perp\!\!\!\perp C$
- $Z \perp\!\!\!\perp C$

[Export R code](#)



# Backdoor Criterion

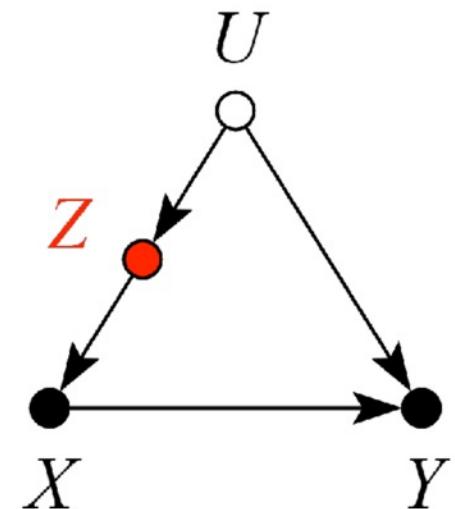
**Backdoor Criterion:** Rule to find adjustment set to yield  $P(Y|\text{do}(X))$



# Backdoor Criterion

**Backdoor Criterion:** Rule to find adjustment set to yield  $P(Y|\text{do}(X))$

Beware non-causal paths that you open while closing other paths!

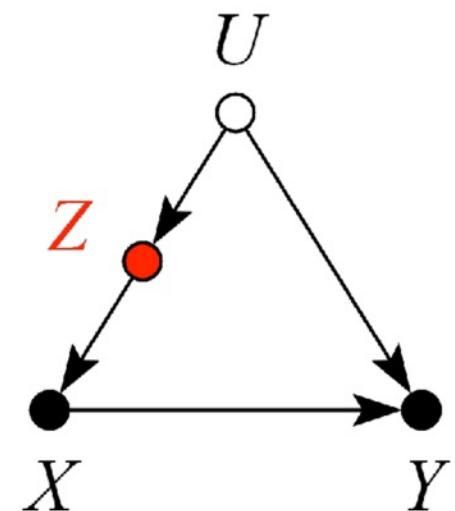


# Backdoor Criterion

**Backdoor Criterion:** Rule to find adjustment set to yield  $P(Y|\text{do}(X))$

Beware non-causal paths that you open while closing other paths!

More than backdoors:



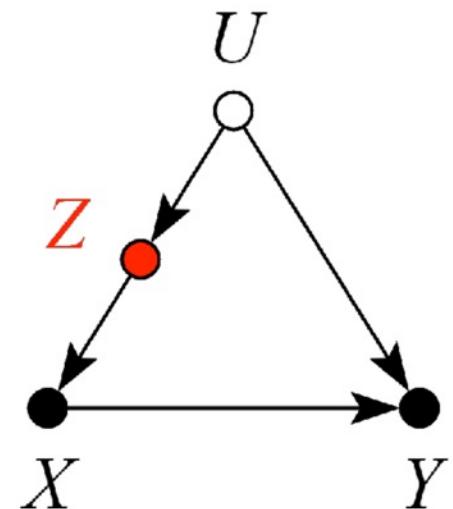
# Backdoor Criterion

**Backdoor Criterion:** Rule to find adjustment set to yield  $P(Y|\text{do}(X))$

Beware non-causal paths that you open while closing other paths!

More than backdoors:

Also solutions with simultaneous equations  
(instrumental variables e.g.)



# Backdoor Criterion

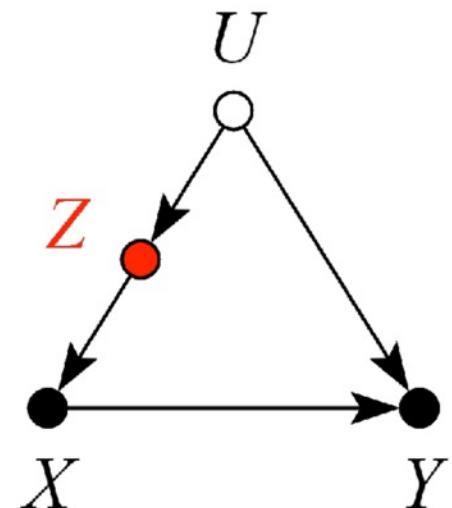
**Backdoor Criterion:** Rule to find adjustment set to yield  $P(Y|\text{do}(X))$

Beware non-causal paths that you open while closing other paths!

More than backdoors:

Also solutions with simultaneous equations  
(instrumental variables e.g.)

Full Luxury Bayes: use all variables, but in separate sub-models instead of single regression



# Good & Bad Controls

**“Control” variable:** Variable introduced to an analysis so that a causal estimate is possible

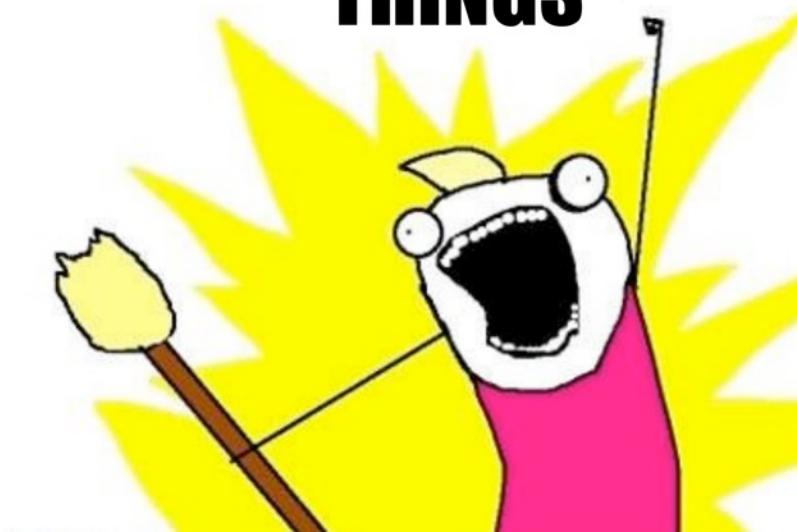
Common **wrong** heuristics for choosing control variables

Anything in the spreadsheet **YOLO!**

Any variables not highly **collinear**

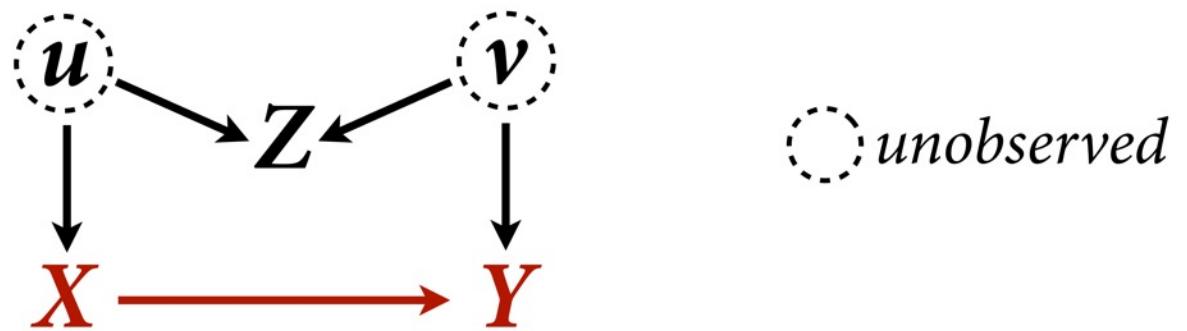
Any **pre-treatment** measurement (baseline)

**CONTROL  
ALL THE  
THINGS**

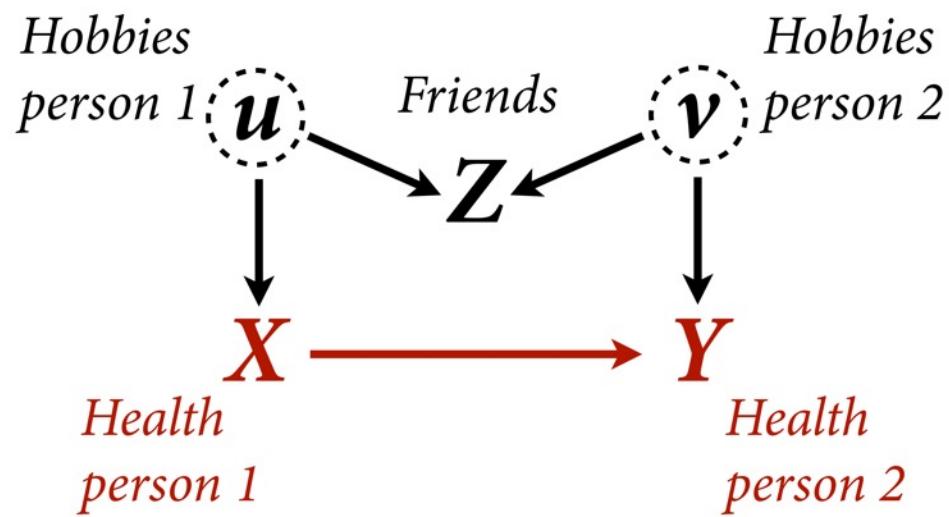


$$X \longrightarrow Y$$

Cinelli, Forney, Pearl 2021 A Crash Course in Good and Bad Controls

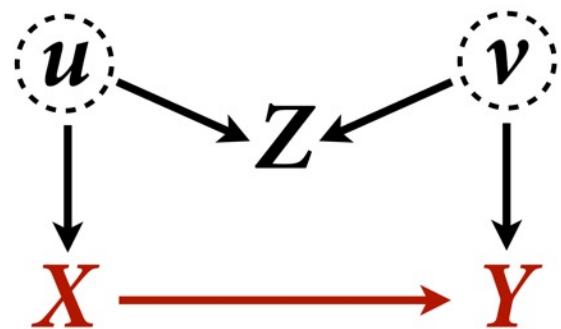


Cinelli, Forney, Pearl 2021 A Crash Course in Good and Bad Controls



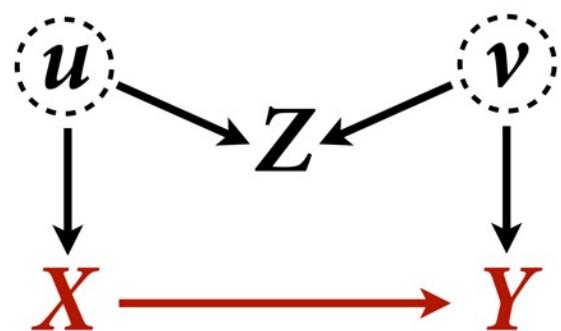
Cinelli, Forney, Pearl 2021 A Crash Course in Good and Bad Controls

(1) List the paths



(1) List the paths

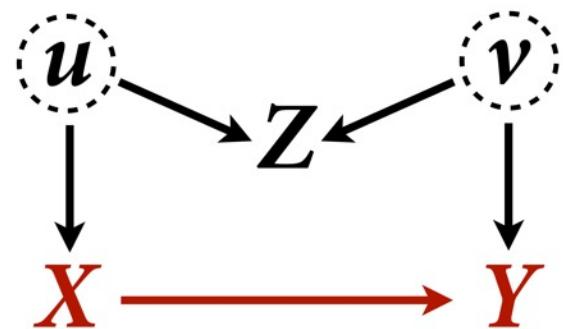
$$X \rightarrow Y$$



(1) List the paths

$$X \rightarrow Y$$

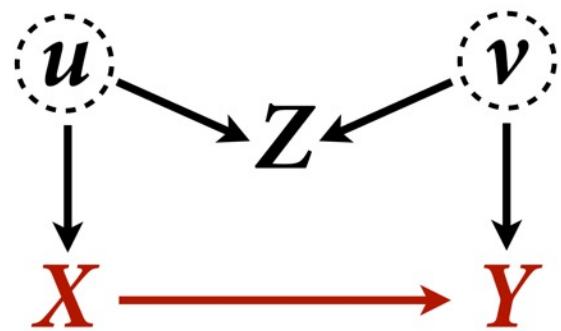
$$X \leftarrow u \rightarrow Z \leftarrow v \rightarrow Y$$



(1) List the paths    (2) Find backdoors

$X \rightarrow Y$   
*frontdoor & open*

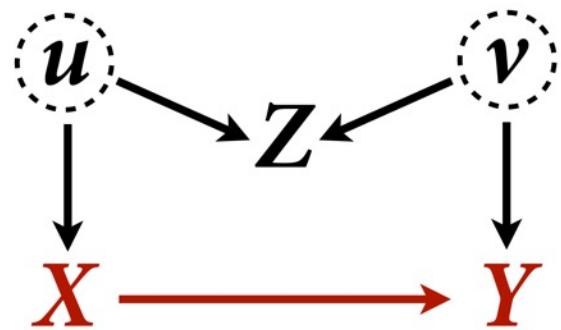
$X \leftarrow u \rightarrow Z \leftarrow v \rightarrow Y$   
*backdoor & closed*



(1) List the paths    (2) Find backdoors

$X \rightarrow Y$   
*frontdoor & open*

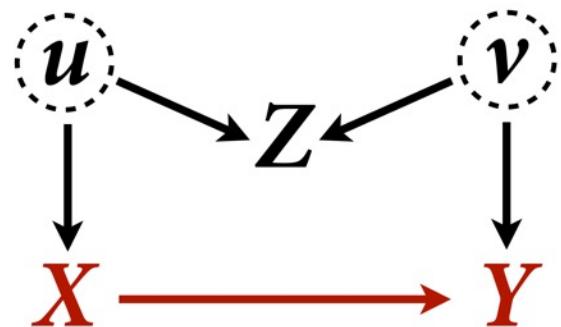
$X \leftarrow u \rightarrow Z \leftarrow v \rightarrow Y$   
*backdoor & closed*



- (1) List the paths    (2) Find backdoors    (3) Close backdoors

$X \rightarrow Y$   
*frontdoor & open*

$X \leftarrow u \rightarrow Z \leftarrow v \rightarrow Y$   
*backdoor & closed*

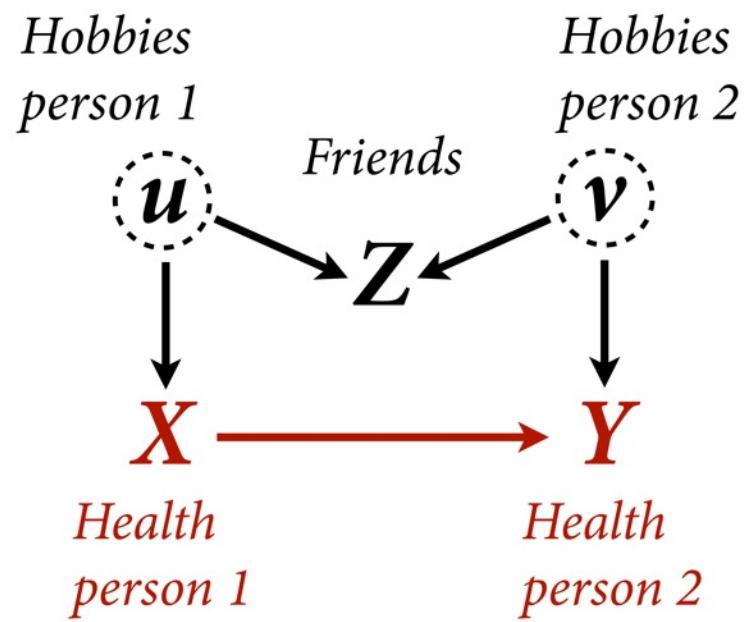


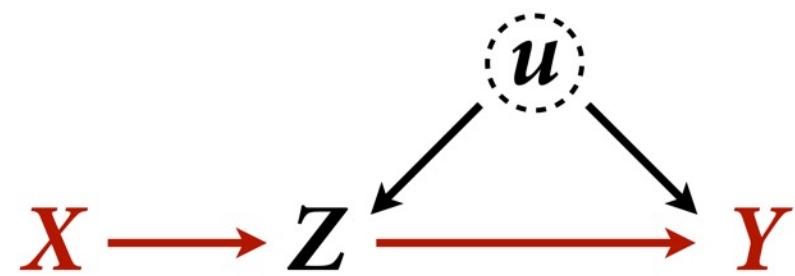
What happens if you stratify by Z?

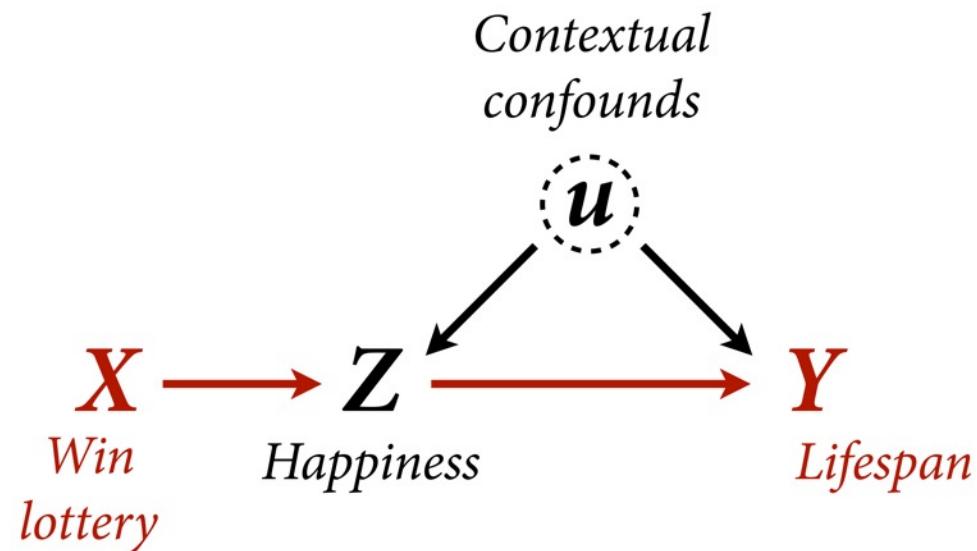
Opens the backdoor path

Z could be a **pre-treatment** variable

Not safe to always control pre-treatment measurements



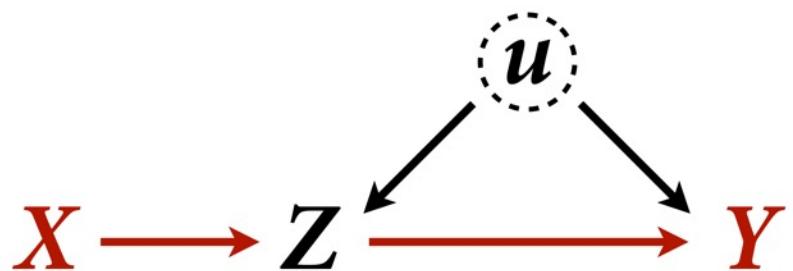




$$X \rightarrow Z \rightarrow Y$$

$$X \rightarrow Z \leftarrow u \rightarrow Y$$

No backdoor, no need  
to control for  $Z$



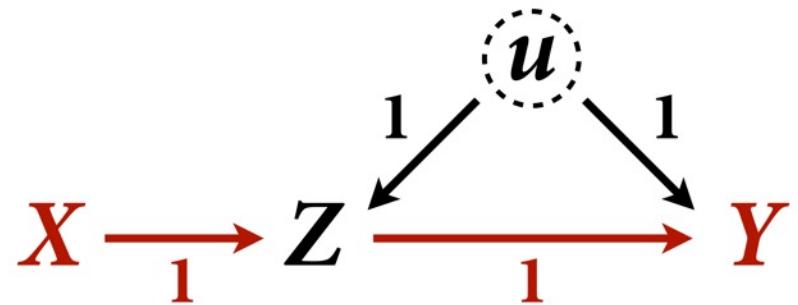
```

f <- function(n=100,bXZ=1,bZY=1) {
  X <- rnorm(n)
  u <- rnorm(n)
  Z <- rnorm(n, bXZ*X + u)
  Y <- rnorm(n, bZY*Z + u )
  bX <- coef( lm(Y ~ X) )['X']
  bXZ <- coef( lm(Y ~ X + Z) )['X']
  return( c(bX,bXZ) )
}

sim <- mcreplicate( 1e4 , f() , mc.cores=8 )

dens( sim[1,] , lwd=3 , xlab="posterior mean" )
dens( sim[2,] , lwd=3 , col=2 , add=TRUE )

```



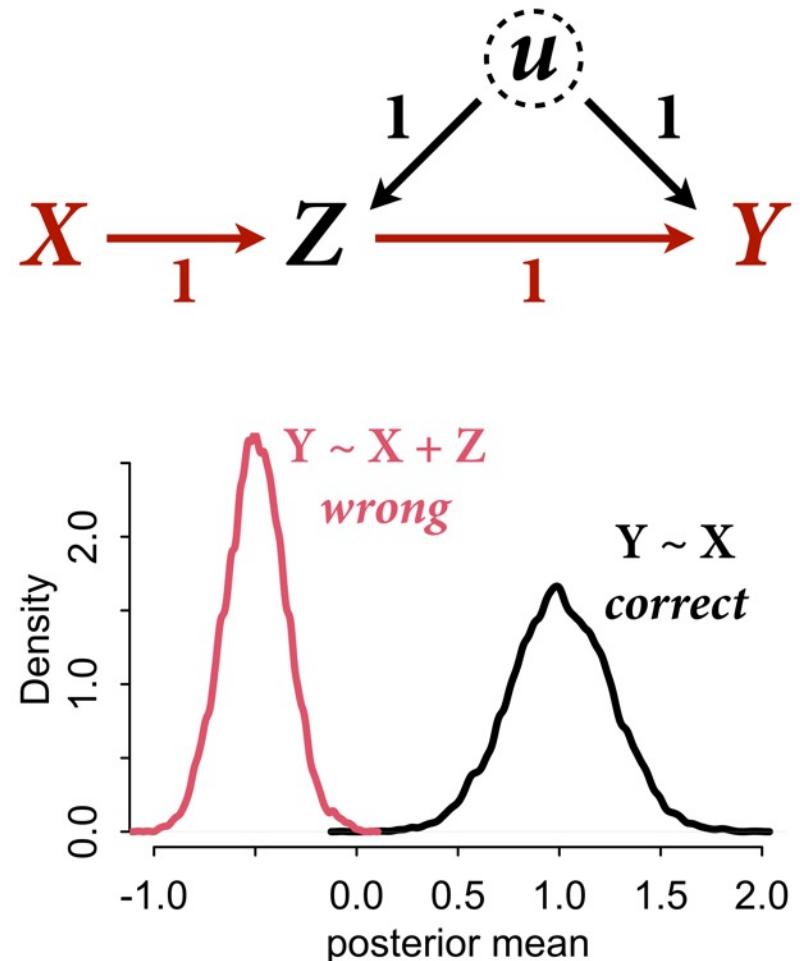
```

f <- function(n=100,bXZ=1,bZY=1) {
  X <- rnorm(n)
  u <- rnorm(n)
  Z <- rnorm(n, bXZ*X + u)
  Y <- rnorm(n, bZY*Z + u )
  bX <- coef( lm(Y ~ X) )['X']
  bXZ <- coef( lm(Y ~ X + Z) )['X']
  return( c(bX,bXZ) )
}

sim <- mcreplicate( 1e4 , f() , mc.cores=8 )

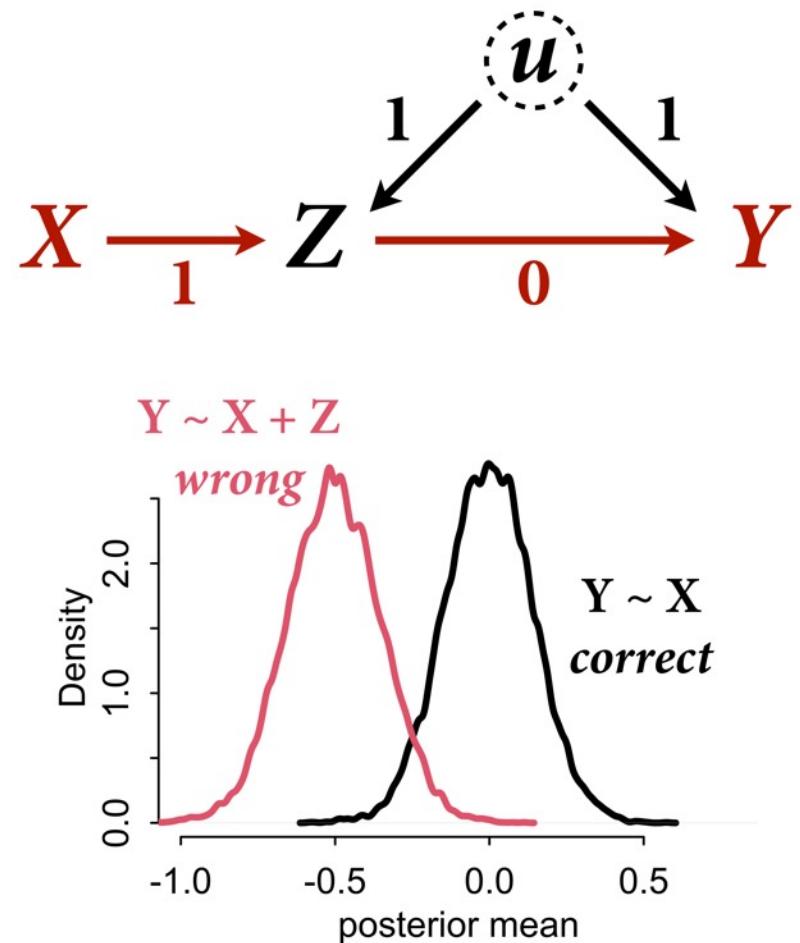
dens( sim[1,] , lwd=3 , xlab="posterior mean" )
dens( sim[2,] , lwd=3 , col=2 , add=TRUE )

```



# Change bZY to zero

```
f <- function(n=100,bXZ=1,bZY=1) {  
  X <- rnorm(n)  
  u <- rnorm(n)  
  Z <- rnorm(n, bXZ*X + u)  
  Y <- rnorm(n, bZY*Z + u )  
  bX <- coef( lm(Y ~ X) )['X']  
  bXZ <- coef( lm(Y ~ X + Z) )['X']  
  return( c(bX,bXZ) )  
}  
  
sim <- mcreplicate( 1e4 , f(bZY=0) , mc.cores=8 )  
  
dens( sim[1,] , lwd=3 , xlab="posterior mean" )  
dens( sim[2,] , lwd=3 , col=2 , add=TRUE )
```



$$X \rightarrow Z \rightarrow Y$$

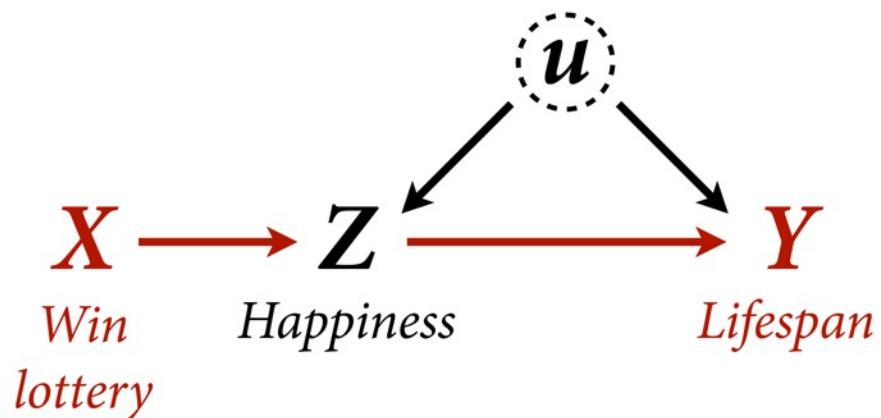
$$X \rightarrow Z \leftarrow u \rightarrow Y$$

Controlling for  $Z$  biases treatment estimate  $X$

Controlling for  $Z$  opens biasing path through  $u$

Can estimate effect of  $X$ ; Cannot estimate mediation effect  $Z$

No backdoor, no need to control for  $Z$

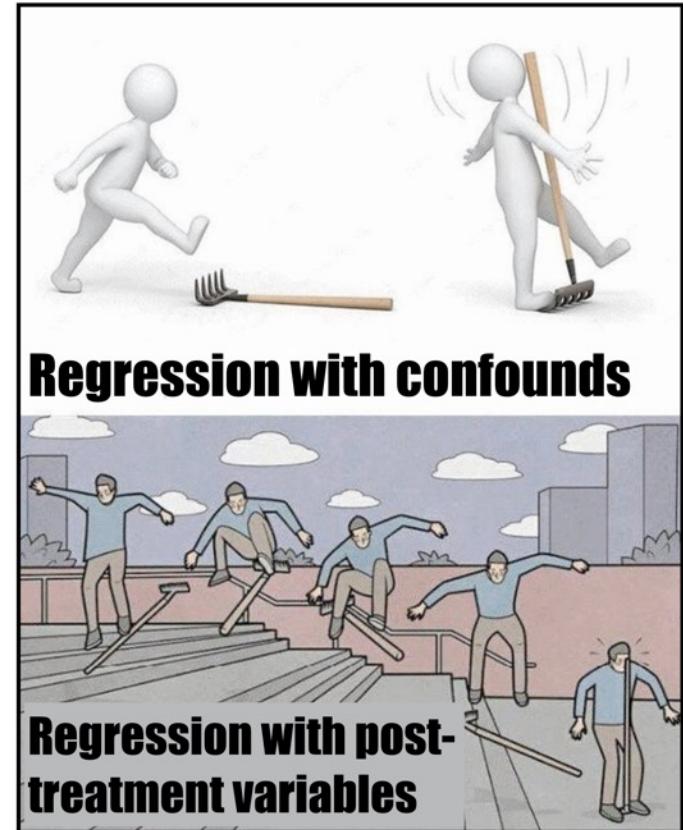


# Post-treatment bias is common

TABLE 1 Posttreatment Conditioning in Experimental Studies

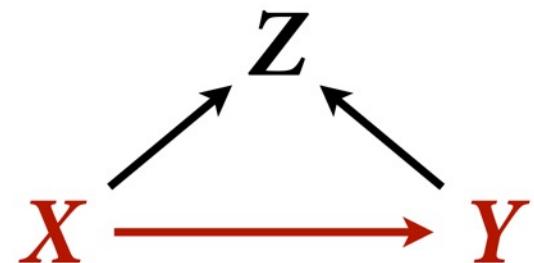
Category	Prevalence
Engages in posttreatment conditioning	46.7%
Controls for/interacts with a posttreatment variable	21.3%
Drops cases based on posttreatment criteria	14.7%
Both types of posttreatment conditioning present	10.7%
No conditioning on posttreatment variables	52.0%
Insufficient information to code	1.3%

Note: The sample consists of 2012–14 articles in the *American Political Science Review*, the *American Journal of Political Science*, and the *Journal of Politics* including a survey, field, laboratory, or lab-in-the-field experiment (n = 75).

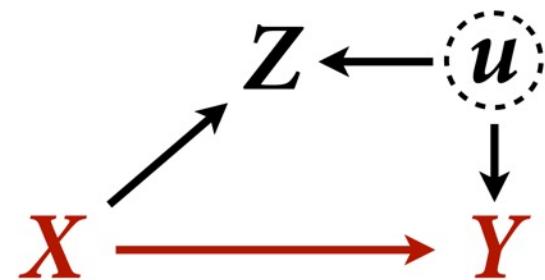


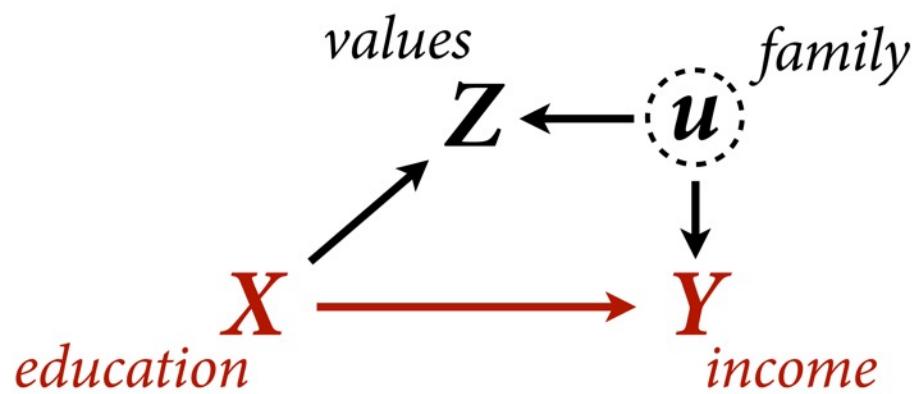
Montgomery et al 2018 How Conditioning on Posttreatment Variables Can Ruin Your Experiment

Do not touch the collider!

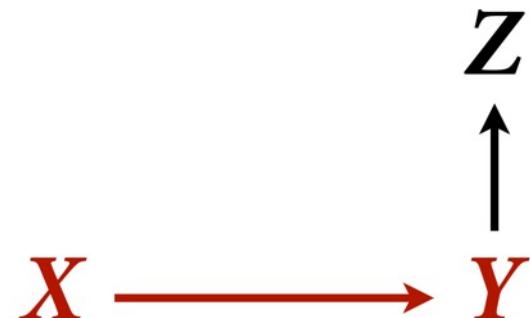


Colliders not always so obvious

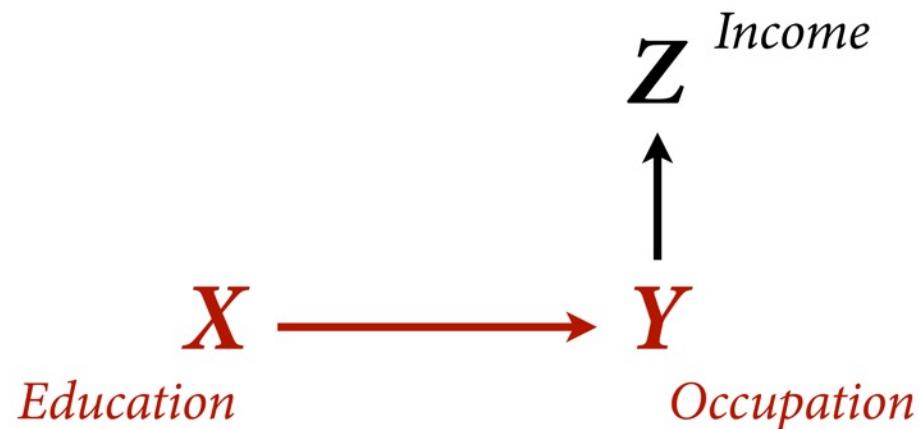




“Case-control bias”

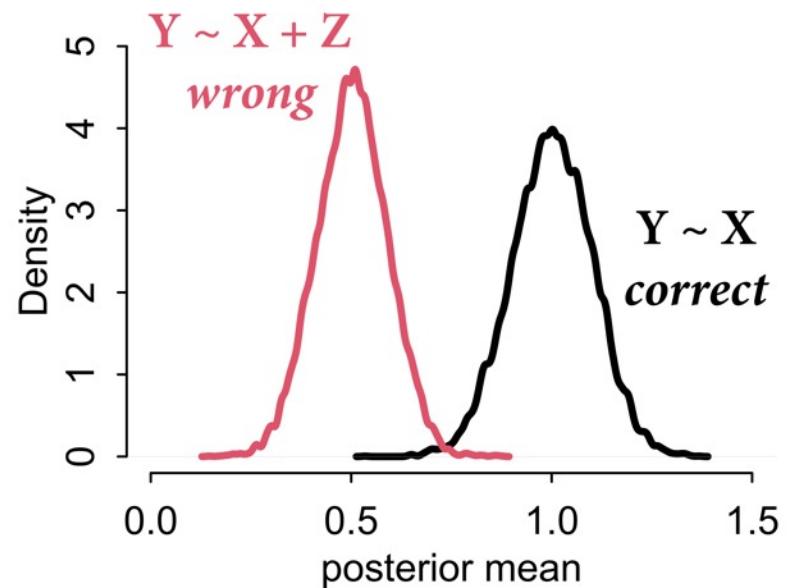


“Case-control bias”



## “Case-control bias”

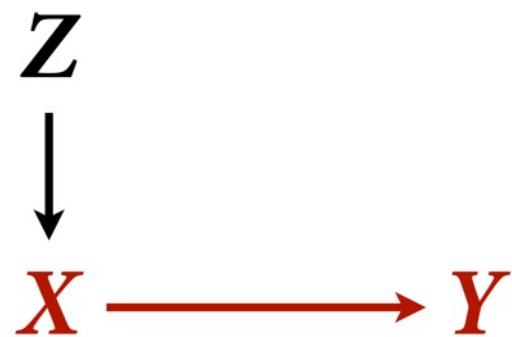
```
f <- function(n=100,bXY=1,bYZ=1) {  
  X <- rnorm(n)  
  Y <- rnorm(n, bXY*X )  
  Z <- rnorm(n, bYZ*Y )  
  bX <- coef( lm(Y ~ X) )['X']  
  bXZ <- coef( lm(Y ~ X + Z) )['X']  
  return( c(bX,bXZ) )  
}  
  
sim <- mcreplicate( 1e4 , f() , mc.cores=8 )  
  
dens( sim[1,] , lwd=3 , xlab="posterior mean" )  
dens( sim[2,] , lwd=3 , col=2 , add=TRUE )
```



“Precision parasite”

No backdoors

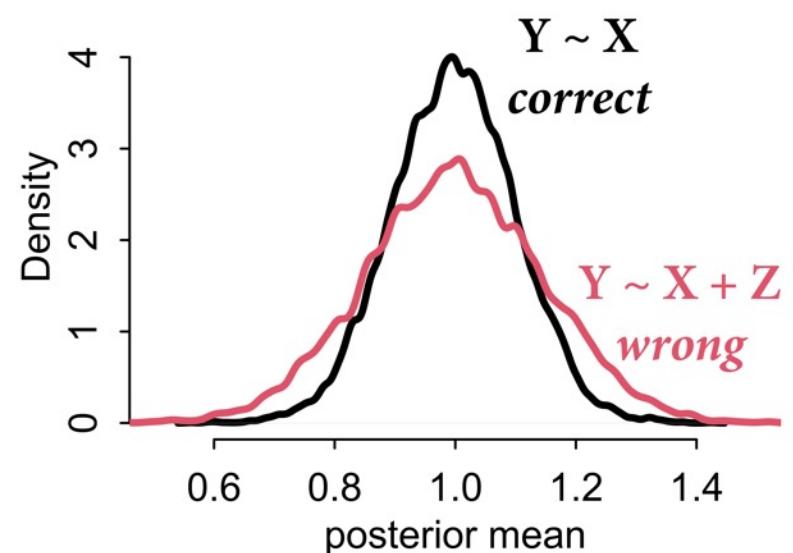
But still not good to  
condition on  $Z$



## “Precision parasite”

```
f <- function(n=100,bZX=1,bXY=1) {  
  Z <- rnorm(n)  
  X <- rnorm(n, bZX*Z )  
  Y <- rnorm(n, bXY*X )  
  bX <- coef( lm(Y ~ X) )['X']  
  bXZ <- coef( lm(Y ~ X + Z) )['X']  
  return( c(bX,bXZ) )  
}  
  
sim <- mcreplicate( 1e4 , f(n=50) , mc.cores=8 )  
  
dens( sim[1,] , lwd=3 , xlab="posterior mean" )  
dens( sim[2,] , lwd=3 , col=2 , add=TRUE )
```

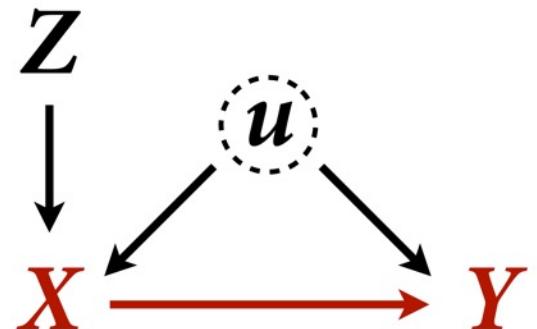
$Z \rightarrow X \rightarrow Y$



“Bias amplification”

$X$  and  $Y$  confounded by  $u$

Something **truly awful** happens  
when we add  $Z$



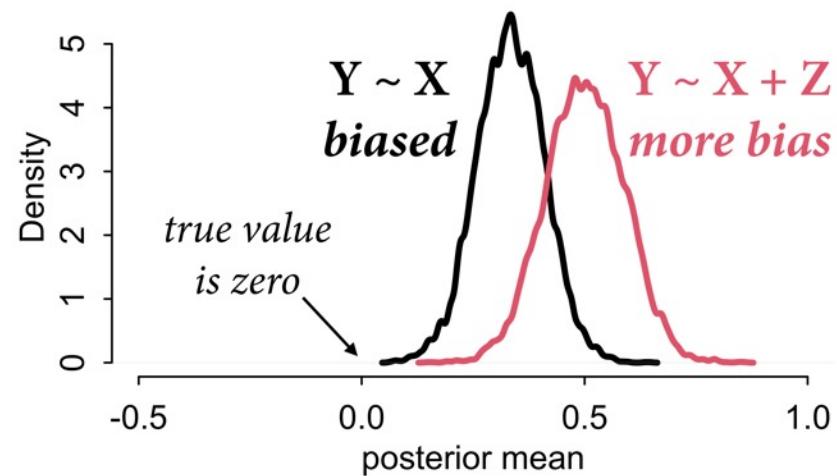
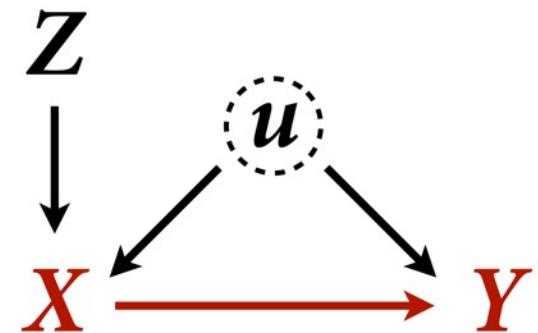
```

f <- function(n=100,bZX=1,bXY=1) {
  Z <- rnorm(n)
  u <- rnorm(n)
  X <- rnorm(n, bZX*Z + u )
  Y <- rnorm(n, bXY*X + u )
  bX <- coef( lm(Y ~ X) )['X']
  bXZ <- coef( lm(Y ~ X + Z) )['X']
  return( c(bX,bXZ) )
}

sim <- mcreplicate( 1e4 , f(bXY=0) , mc.cores=8 )

dens( sim[1,] , lwd=3 , xlab="posterior mean" )
dens( sim[2,] , lwd=3 , col=2 , add=TRUE )

```

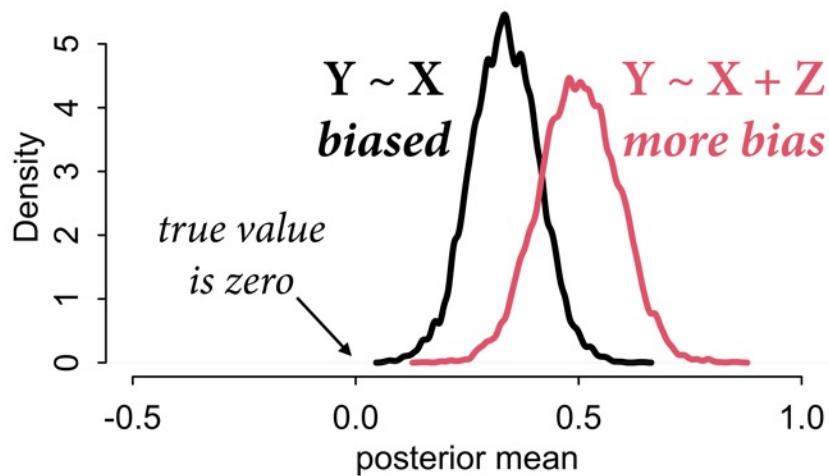
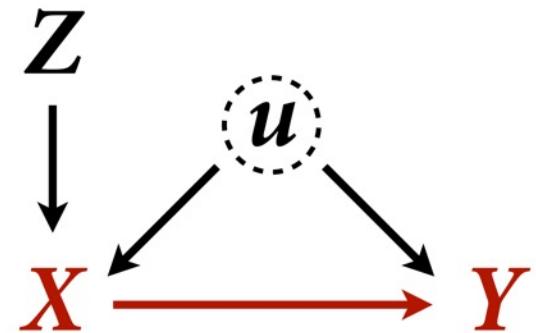


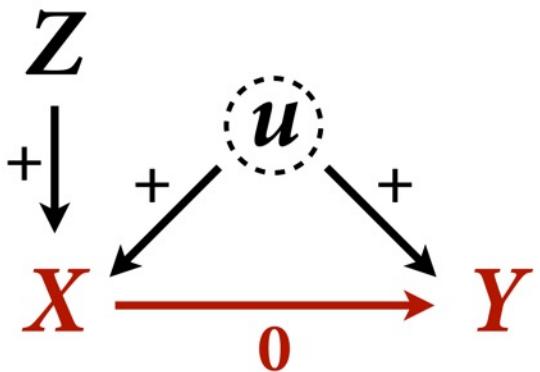
## WHY?

Covariation  $X$  &  $Y$  requires variation in their causes

Within each level of  $Z$ , less variation in  $X$

Confound  $u$  relatively more important within each  $Z$

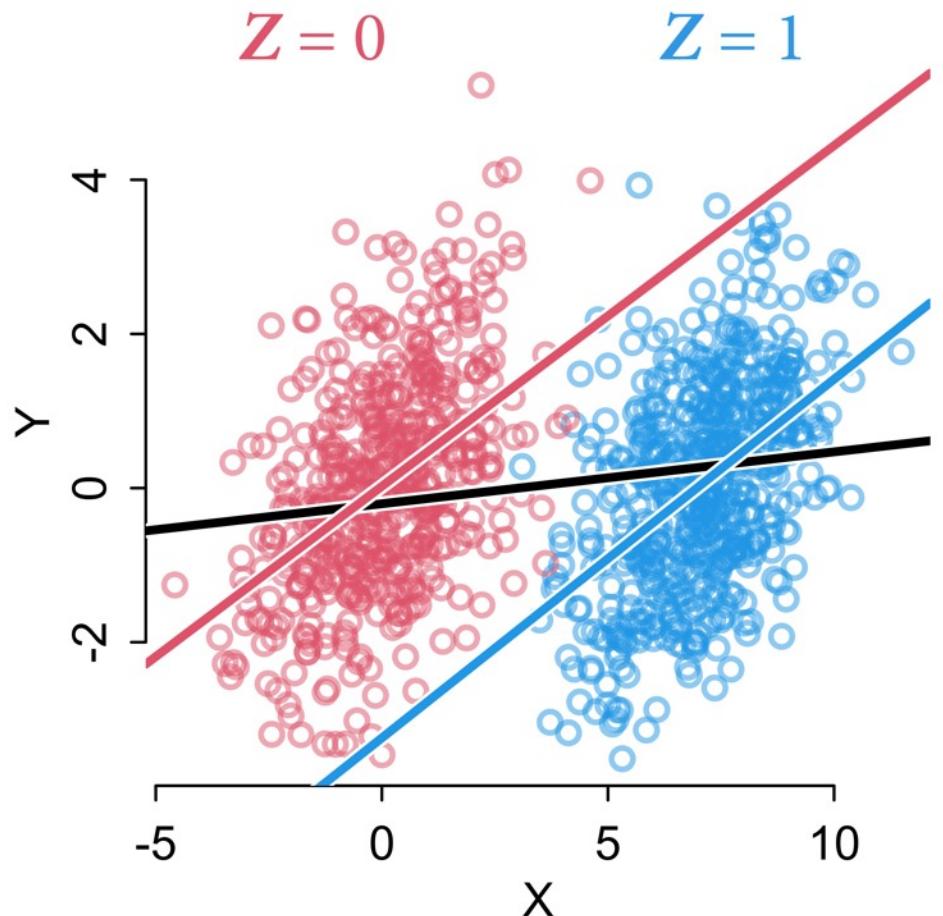




```

n <- 1000
Z <- rbern(n)
u <- rnorm(n)
X <- rnorm(n, 7*Z + u )
Y <- rnorm(n, 0*X + u )

```



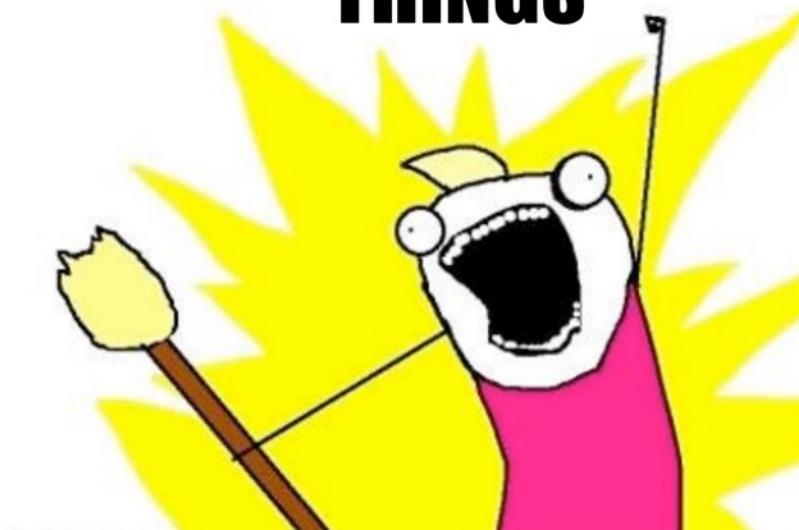
# Good & Bad Controls

**“Control” variable:** Variable introduced to an analysis so that a causal estimate is possible

Heuristics fail — adding control variables can be worse than omitting

Make assumptions explicit

**MODEL  
ALL THE  
THINGS**



# Table 2 Fallacy

Not all coefficients are causal effects

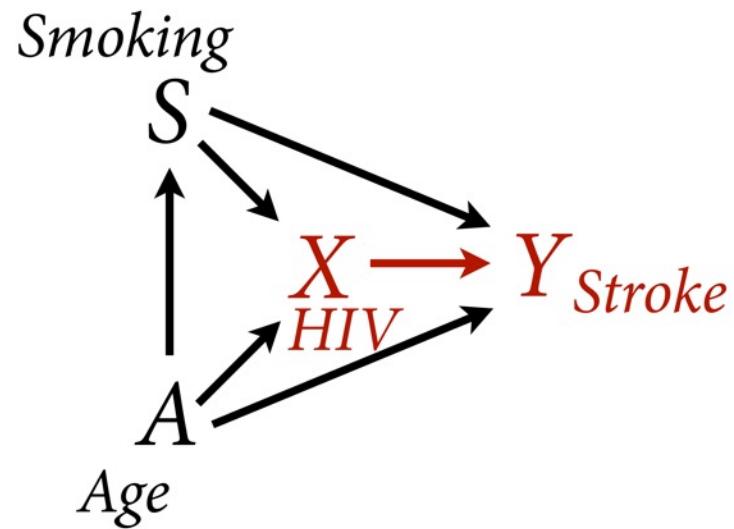
Statistical model designed to identify  $X \rightarrow Y$  will not also identify effects of control variables

Table 2 is dangerous

TABLE 2—ESTIMATED PROBIT MODELS  
FOR THE USE OF A SCREEN

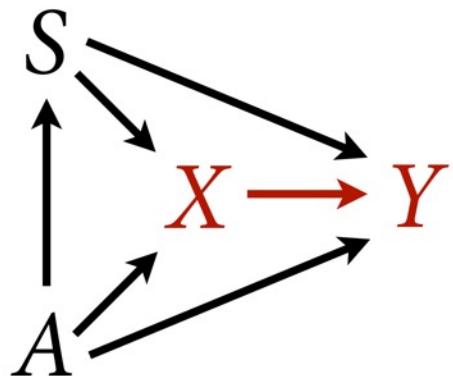
	Preliminaries	Finals	blind
	(1)	(2)	(3)
(Proportion female) <sub>t-1</sub>	2.744 (3.265) [0.006]	3.120 (3.271) [0.004]	0.490 (1.163) [0.011]
(Proportion of orchestra personnel with <6 years tenure) <sub>t-1</sub>	-26.46 (7.314) [-0.058]	-28.13 (8.459) [-0.039]	-9.467 (2.787) [-0.207]
“Big Five” orchestra		0.367 (0.452) [0.001]	
pseudo $R^2$	0.178	0.193	0.050
Number of observations	294	294	434

Westreich & Greenland 2013 The Table 2 Fallacy

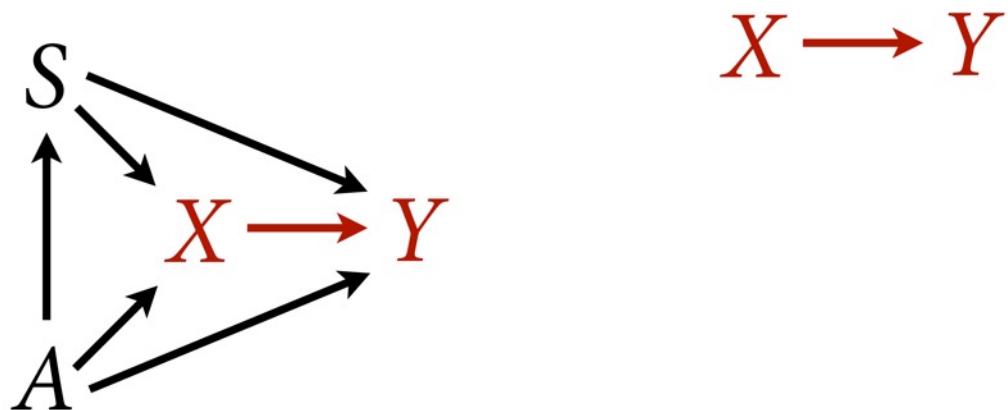


Westreich & Greenland 2013 The Table 2 Fallacy

# Use Backdoor Criterion

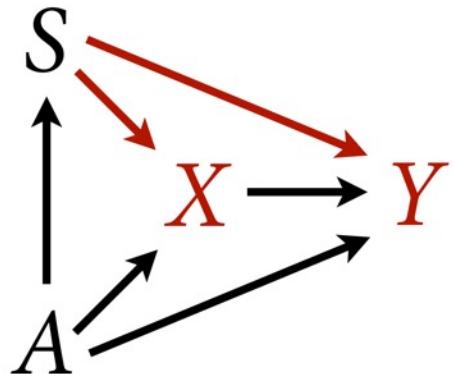


# Use Backdoor Criterion

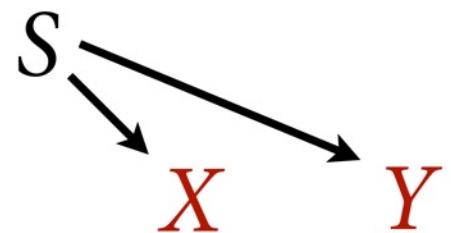


$$X \longrightarrow Y$$

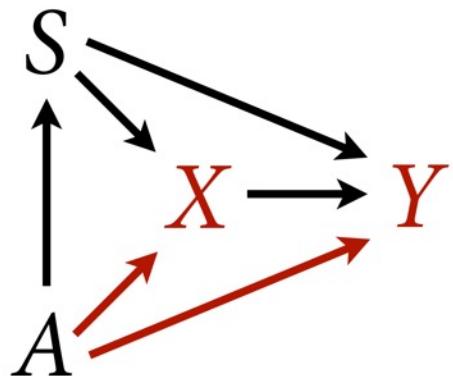
# Use Backdoor Criterion



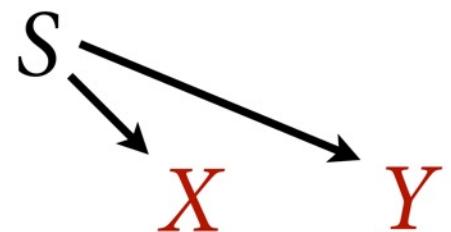
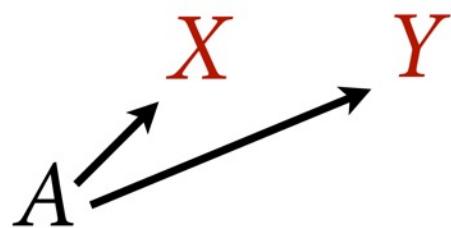
$X \rightarrow Y$



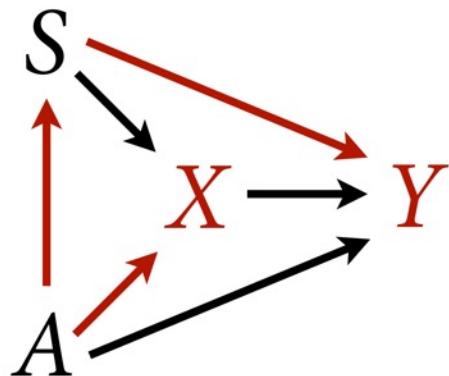
# Use Backdoor Criterion



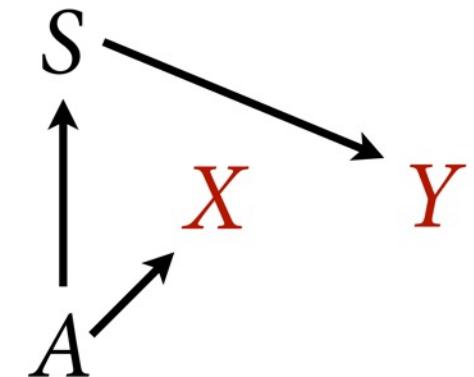
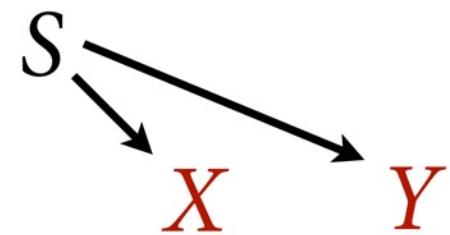
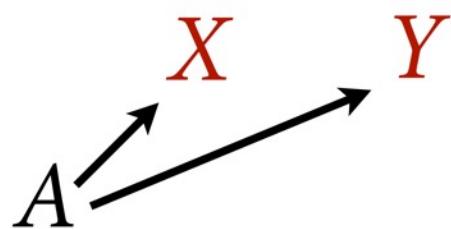
$$X \rightarrow Y$$



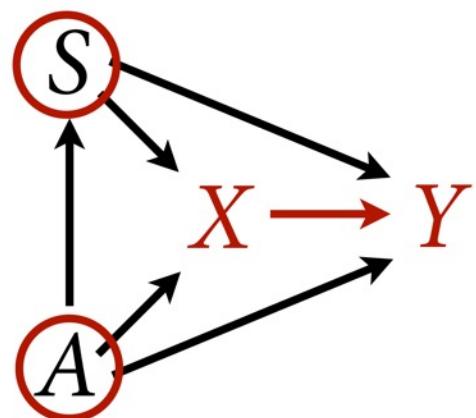
# Use Backdoor Criterion



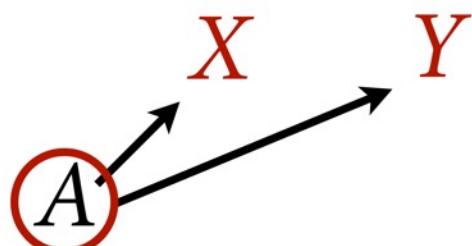
$X \rightarrow Y$



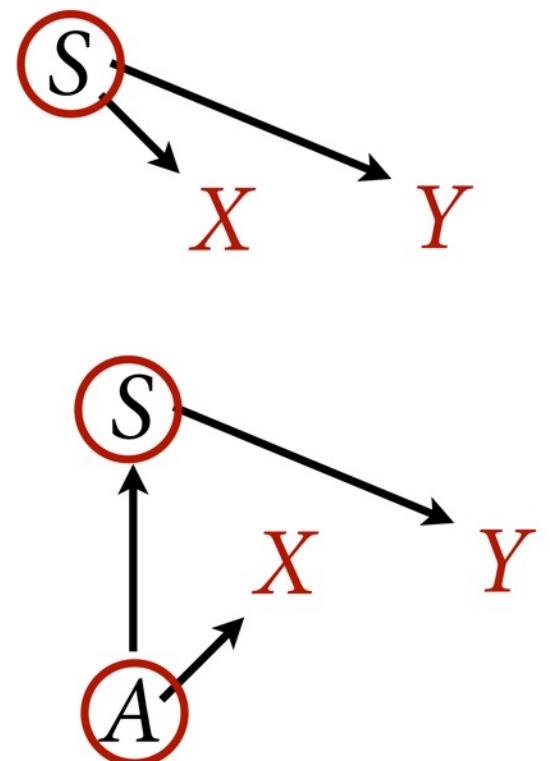
# Use Backdoor Criterion

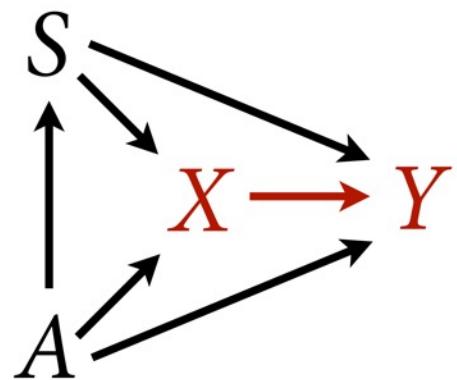


$$X \rightarrow Y$$



$$X \rightarrow Y$$



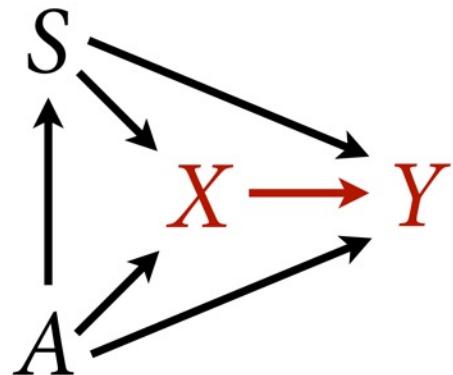


$$Y_i \sim \text{Normal}(\mu_i, \sigma)$$

$$\mu_i = \alpha + \beta_X X_i + \beta_S S_i + \beta_A A_i$$

# $X$

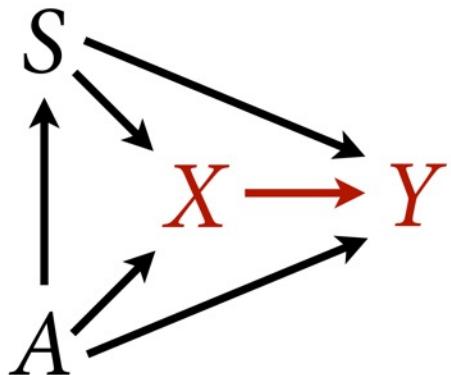
*Unconditional*



Confounded by  $A$   
and  $S$

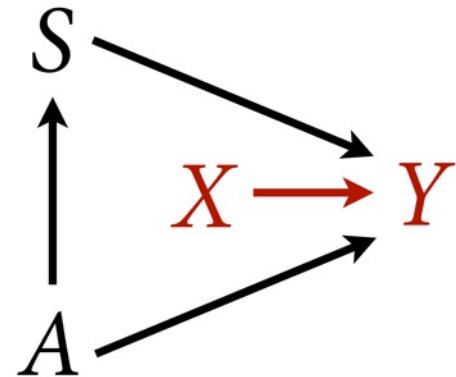
# $X$

*Unconditional*



Confounded by  $A$   
and  $S$

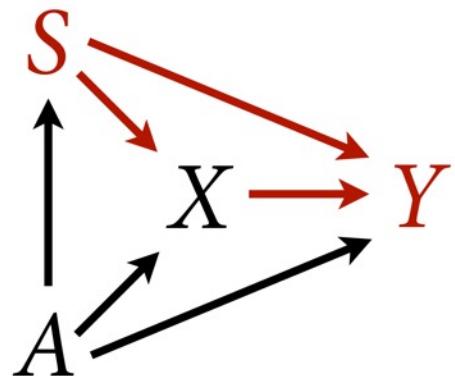
*Conditional on  $A$  and  $S$*



Coefficient for  $\mathbf{X}$ :  
Effect of  $X$  on  $Y$   
(still must  
marginalize!)

**S**

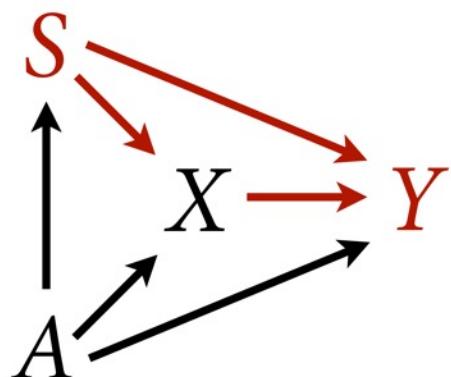
*Unconditional*



Effect of  $S$   
confounded by  $A$

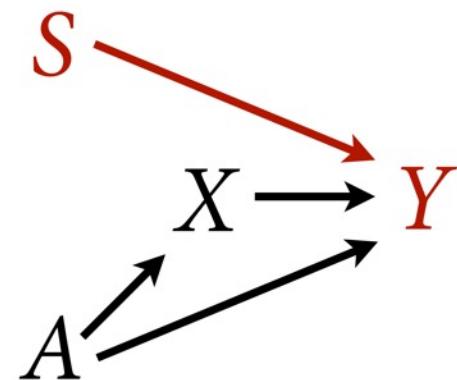
# S

*Unconditional*



Effect of S  
confounded by A

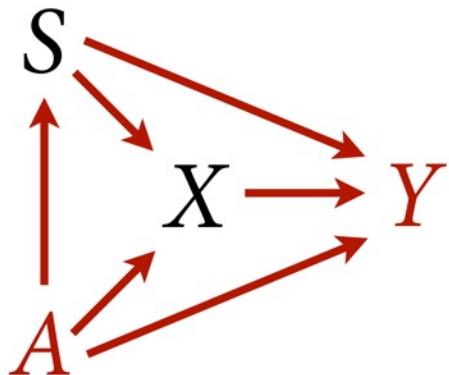
*Conditional on A and X*



Coefficient for S:  
Direct effect of S on Y

# A

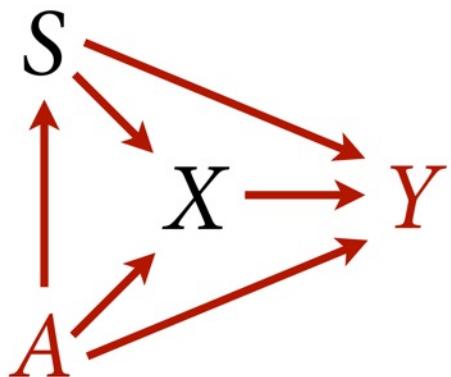
*Unconditional*



Total causal effect  
of  $A$  on  $Y$  flows  
through all paths

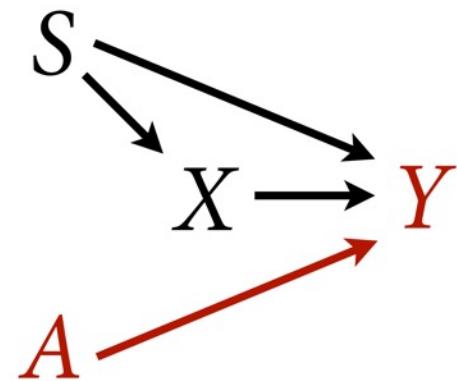
# A

*Unconditional*

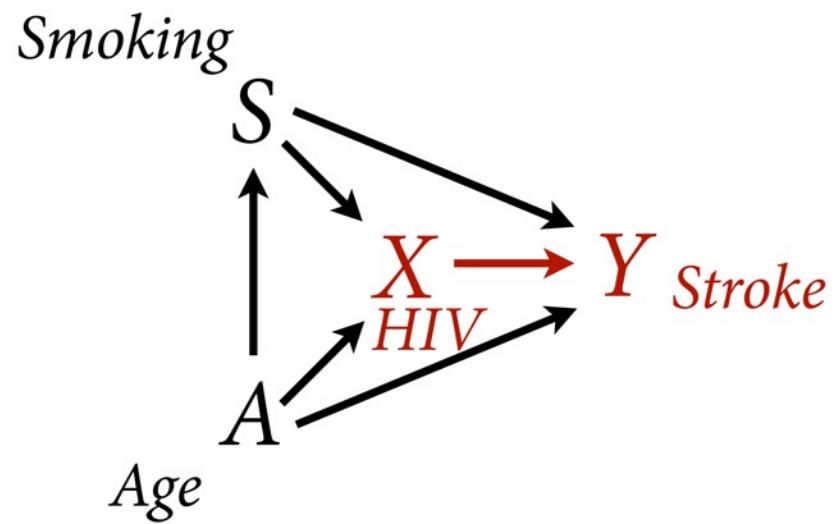


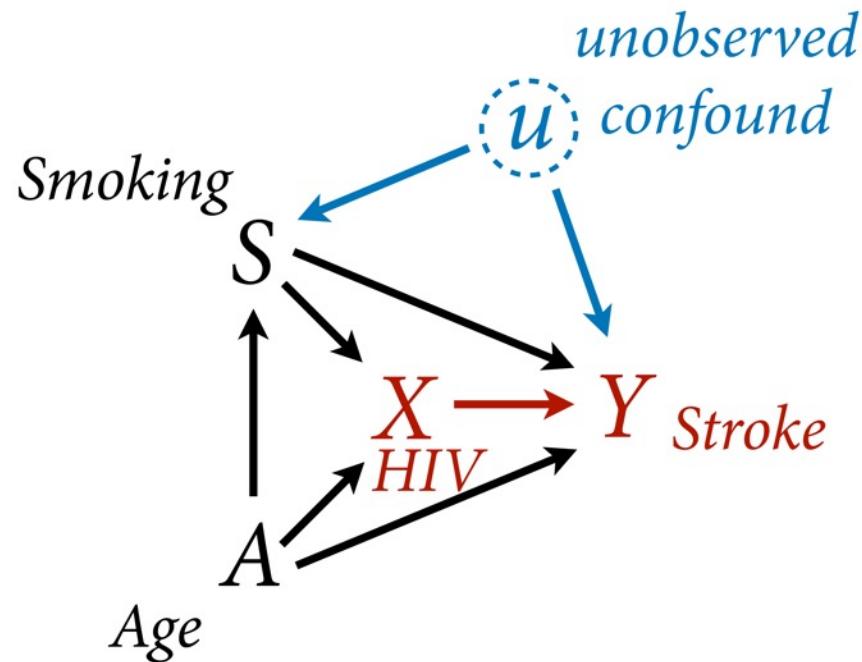
Total causal effect  
of  $A$  on  $Y$  flows  
through all paths

*Conditional on  $X$  and  $S$*



Coefficient for  $A$ :  
**Direct** effect of  $A$  on  $Y$





# Table 2 Fallacy

Not all coefficients created equal

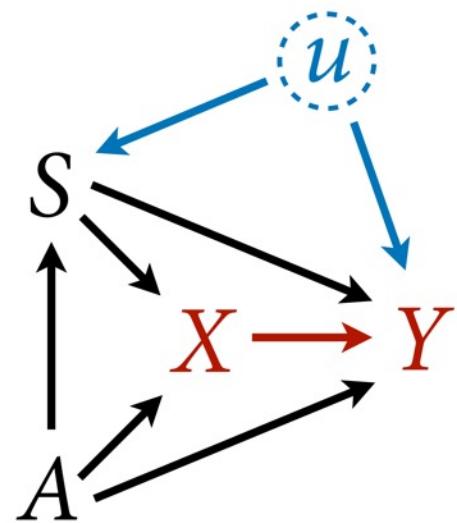
So do not present them as equal

Options:

Do not present control coefficients

Give explicit interpretation of each

No causal model, no interpretation



# Imagine Confounding

Often we cannot credibly adjust for all confounding

Do not give up!

Biased estimate can be better than no estimate

**Sensitivity analysis:** draw the implications of what you don't know

Find natural experiment or design one

