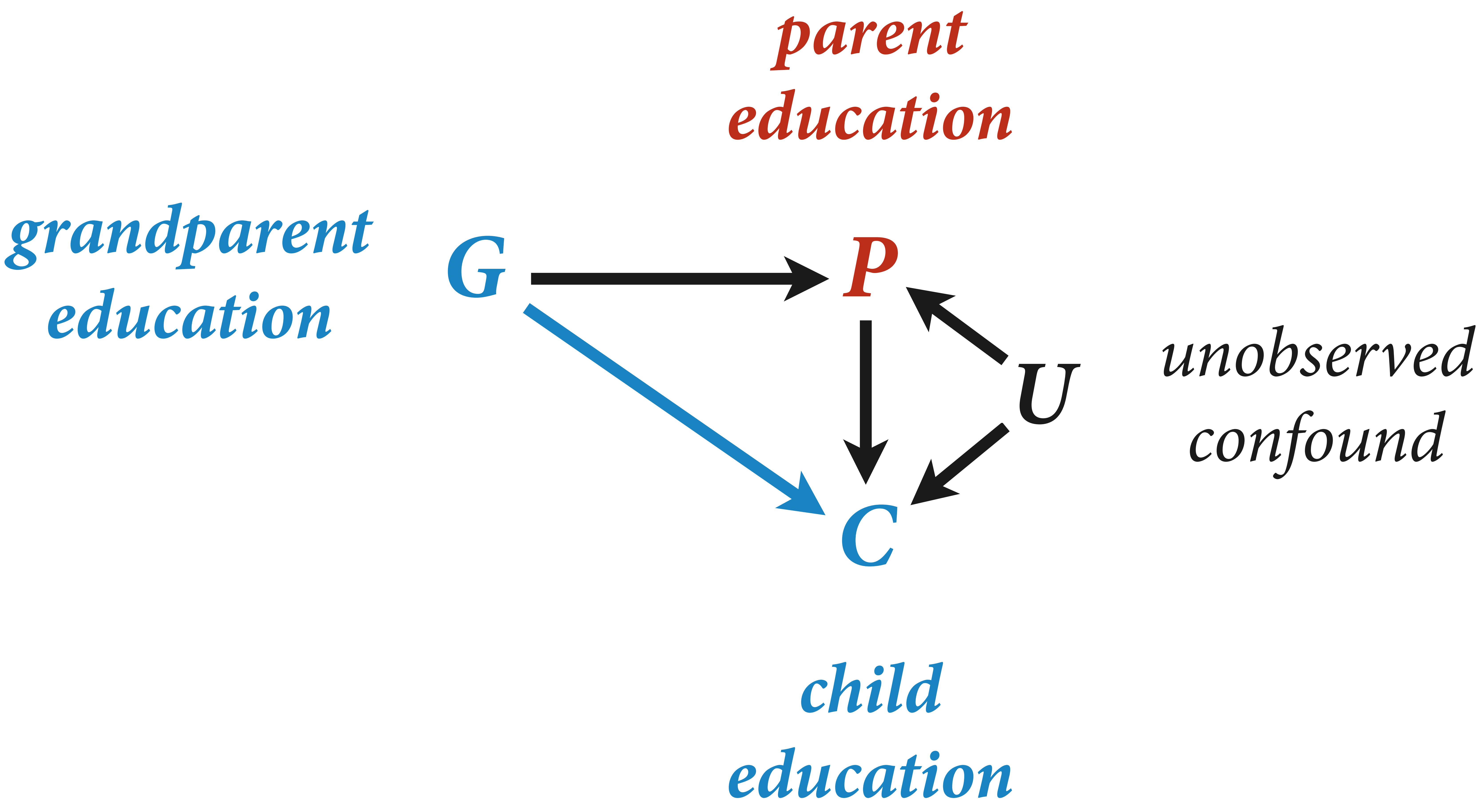
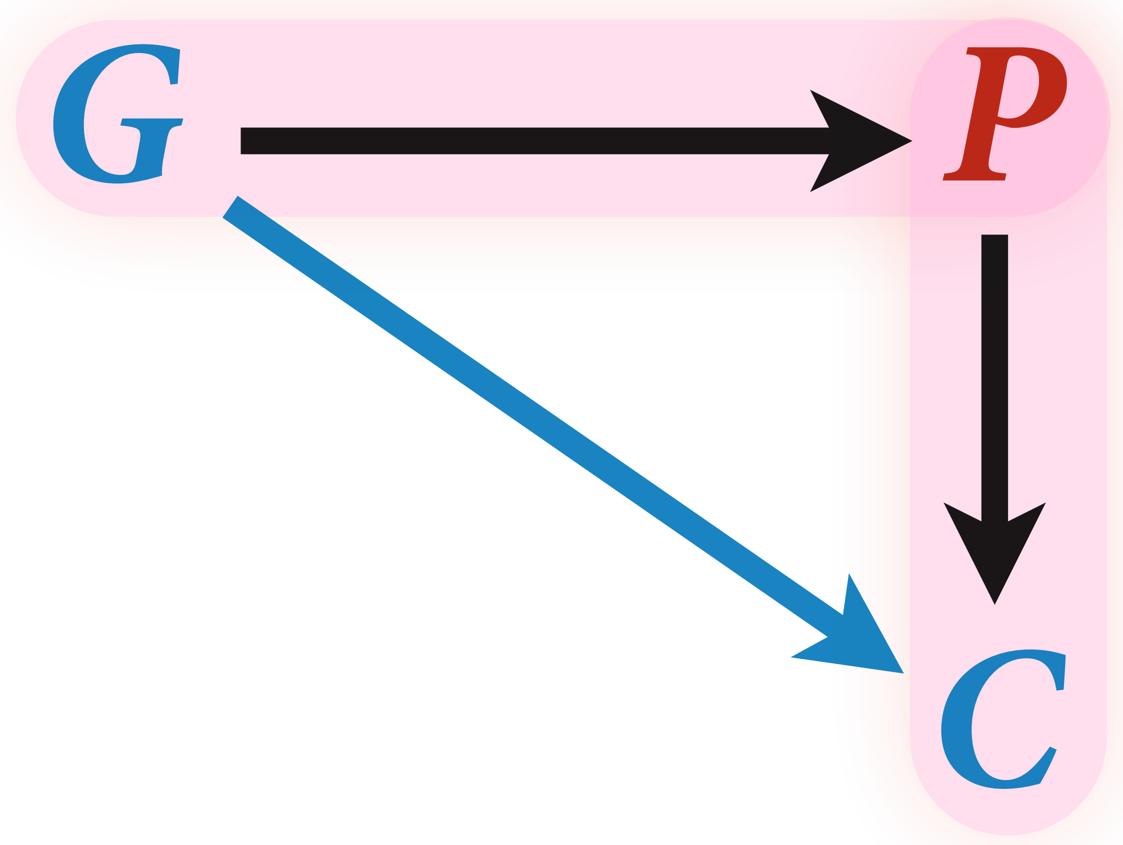
Statistical Rethinking

2022

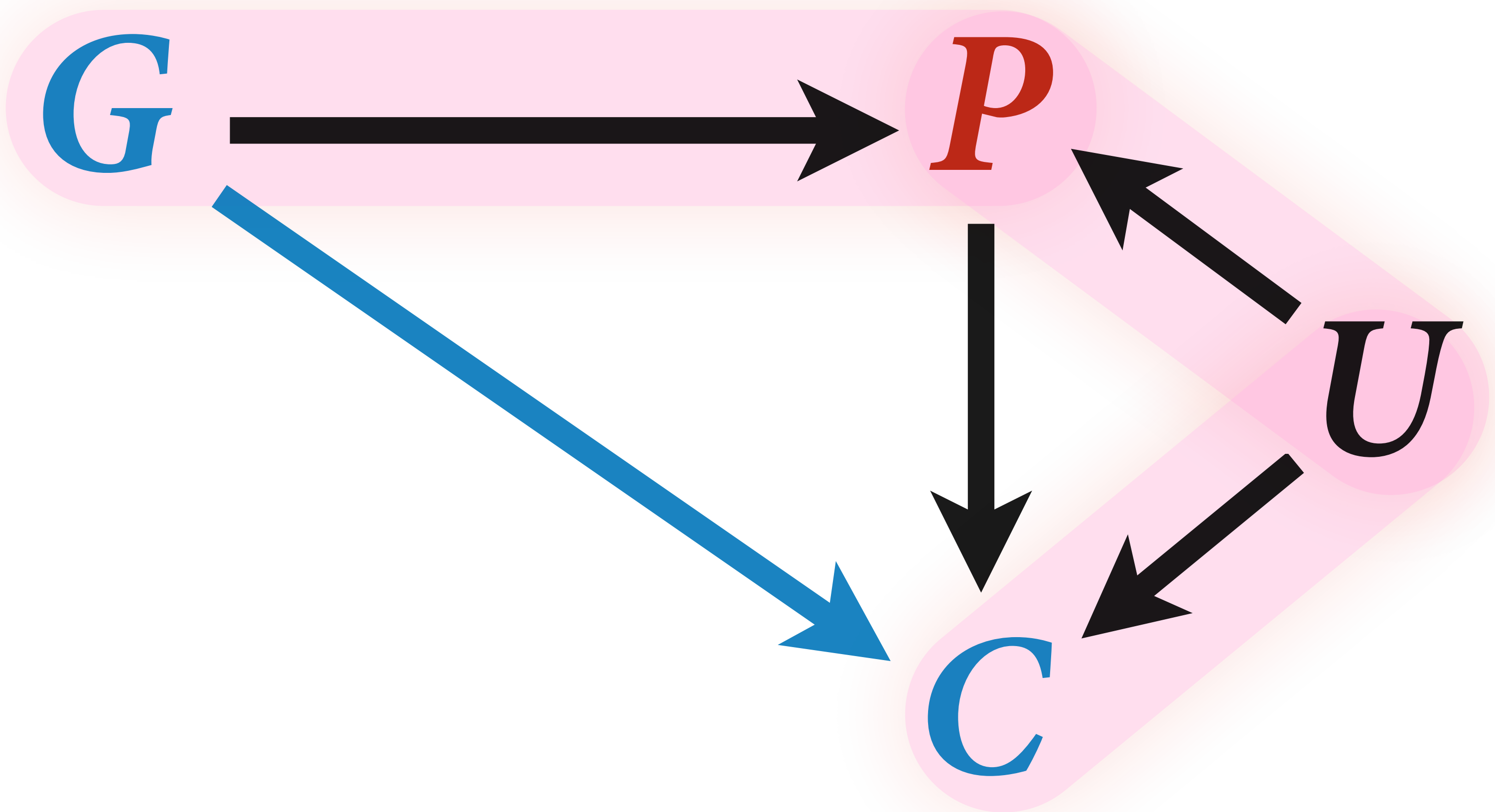
06: Good & Bad Controls



parent
education
grandparent
education
G
P
U
unobserved
confound
C
child
education

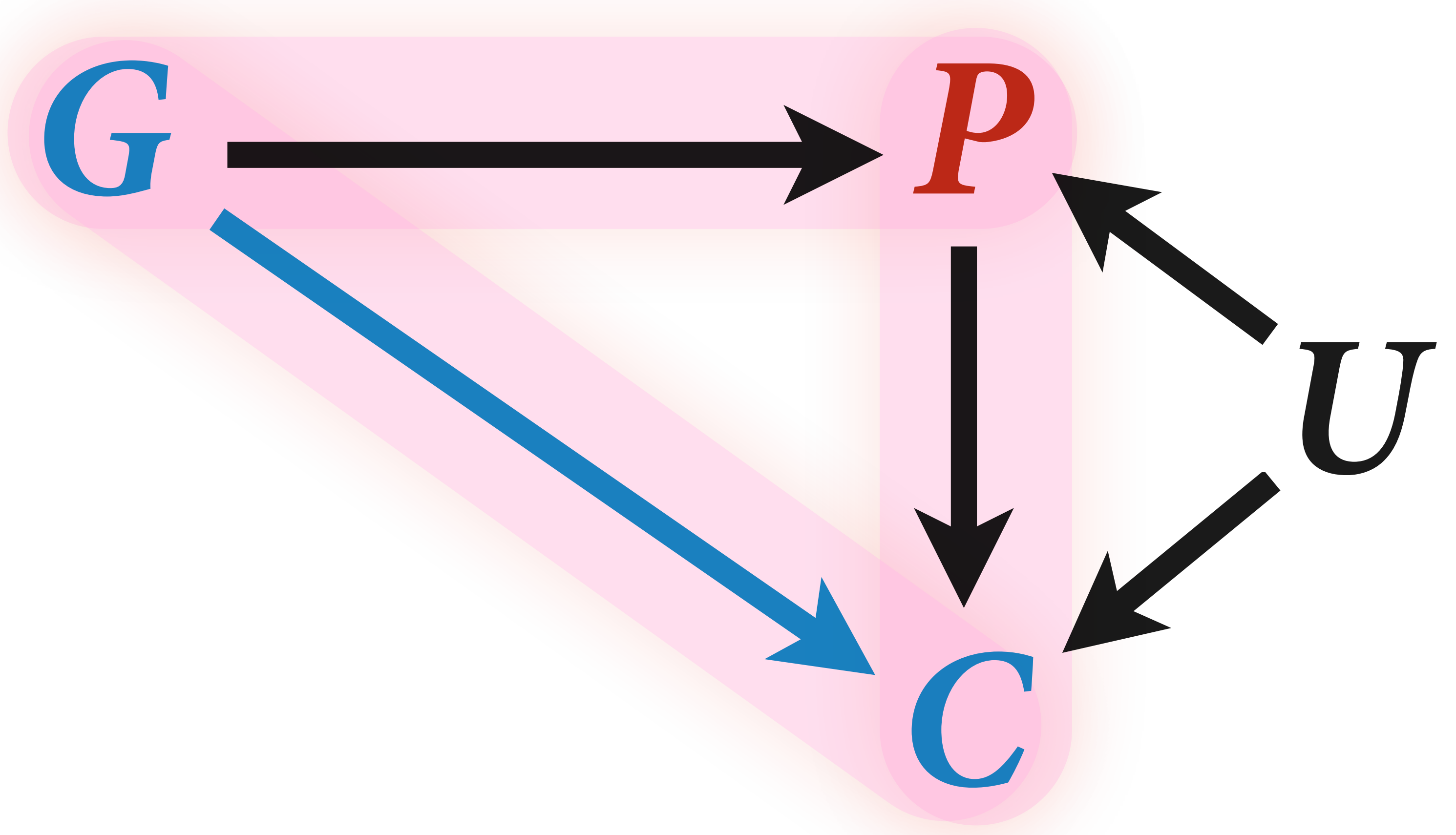


P is a mediator



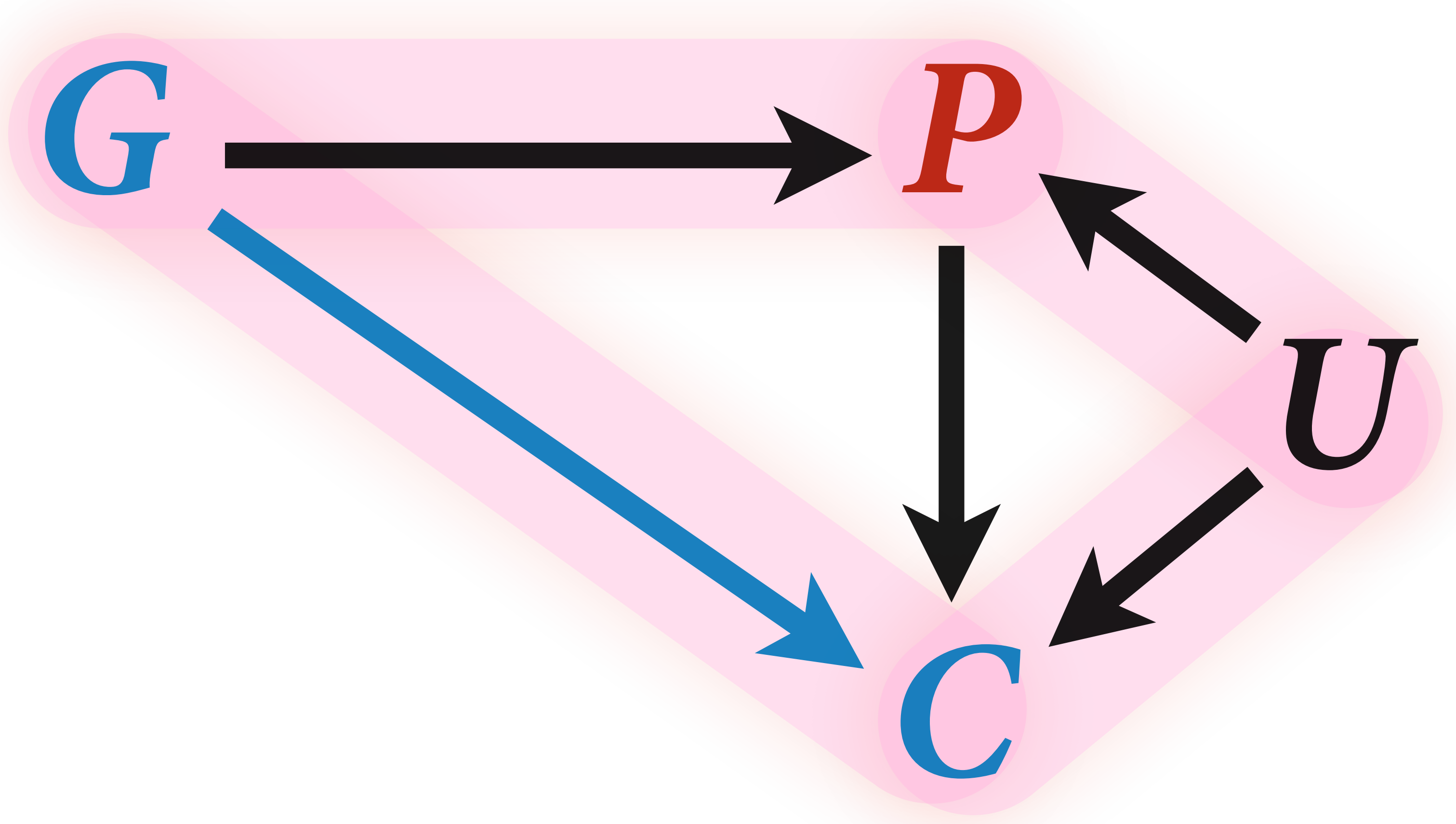
P is a collider

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



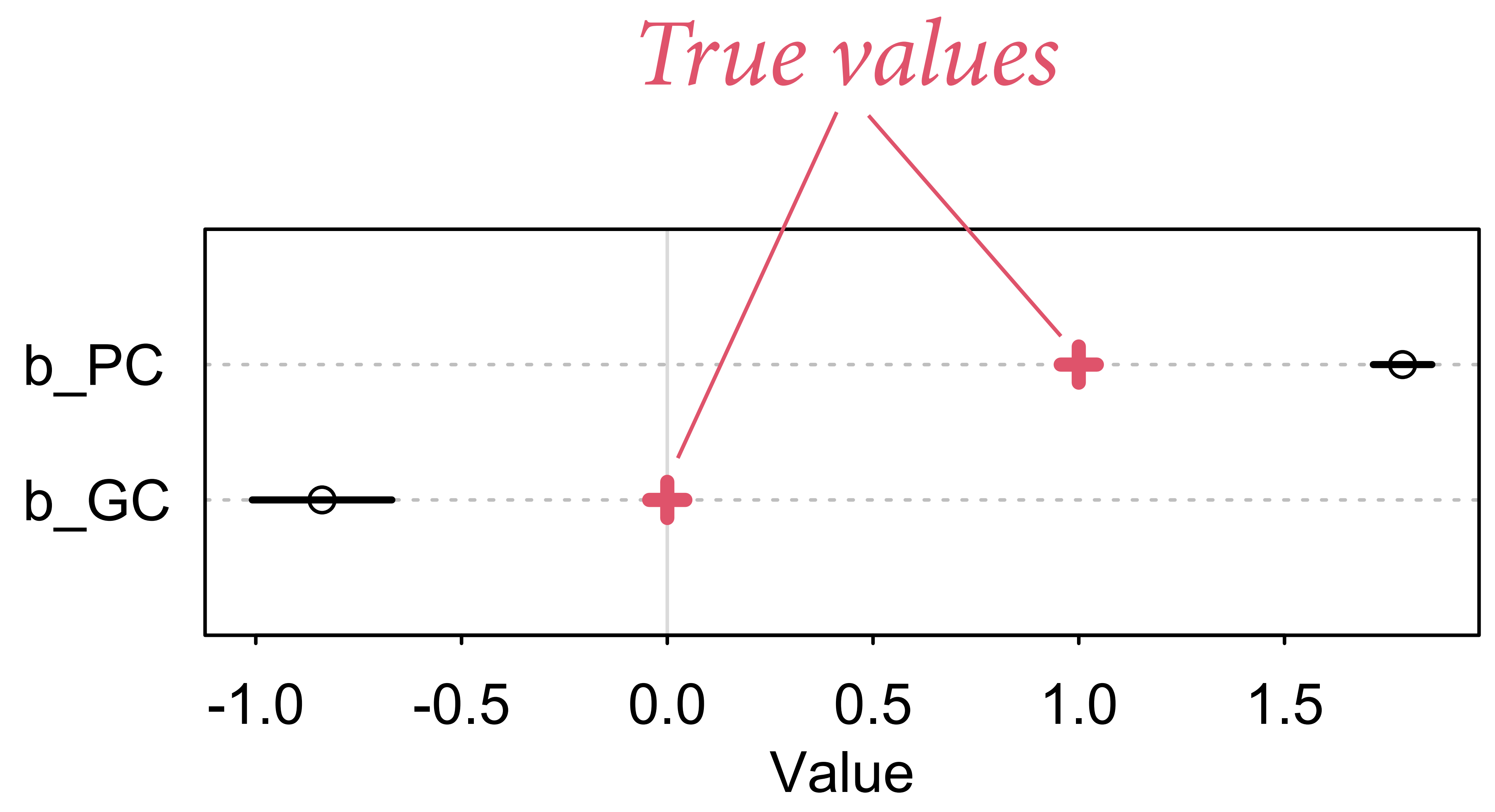
$$C_i \sim \text{Normal}(\mu_i, \sigma)$$
$$\mu_i = \alpha + \beta_G G_i$$

Cannot estimate **direct** effect



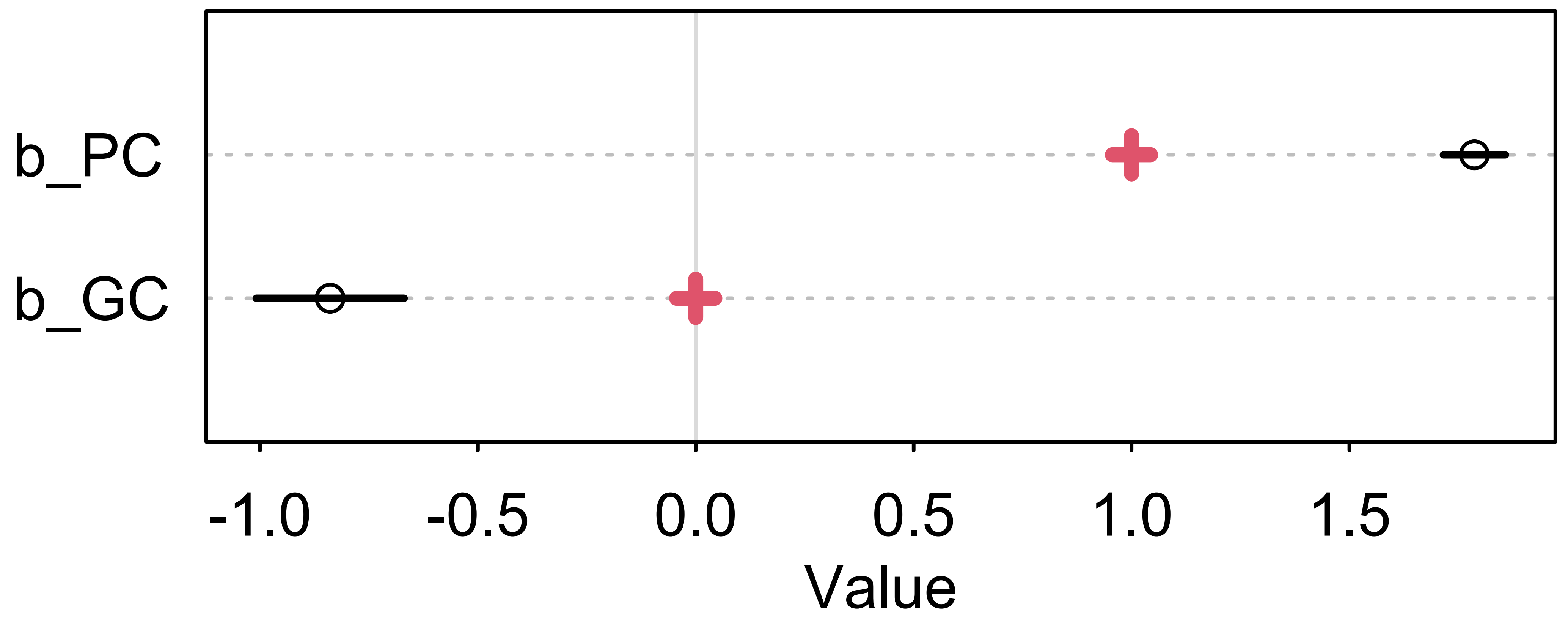
$$C_i \sim \text{Normal}(\mu_i, \sigma)$$
$$\mu_i = \alpha + \beta_G G_i + \beta_P P_i$$

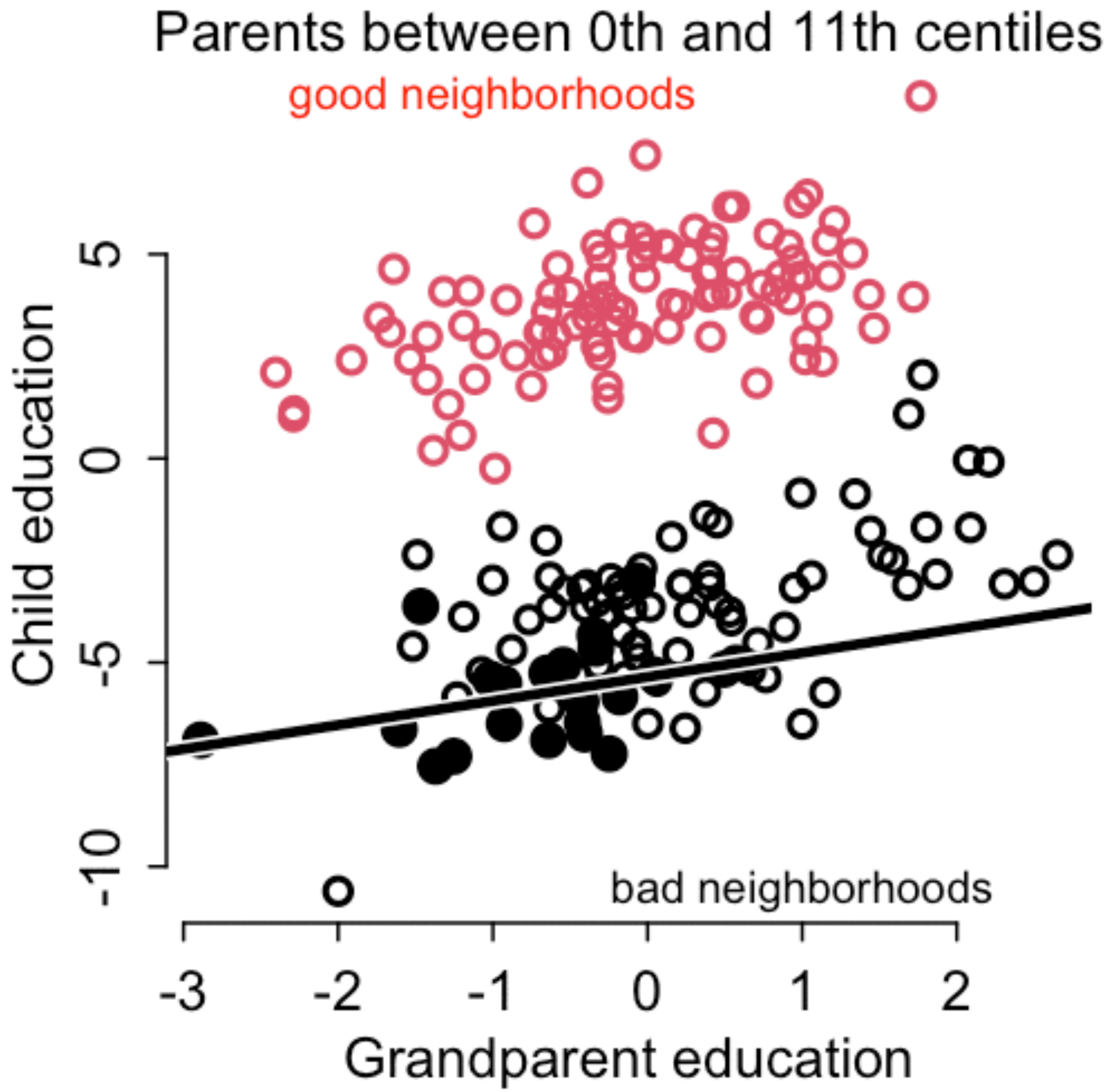
```
N <- 200  # num grandparent-parent-child triads
b_GP <- 1 # direct effect of G on P
b_GC <- 0 # direct effect of G on C
b_PC <- 1 # direct effect of P on C
b_U <- 2 #direct effect of U on P and C

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*





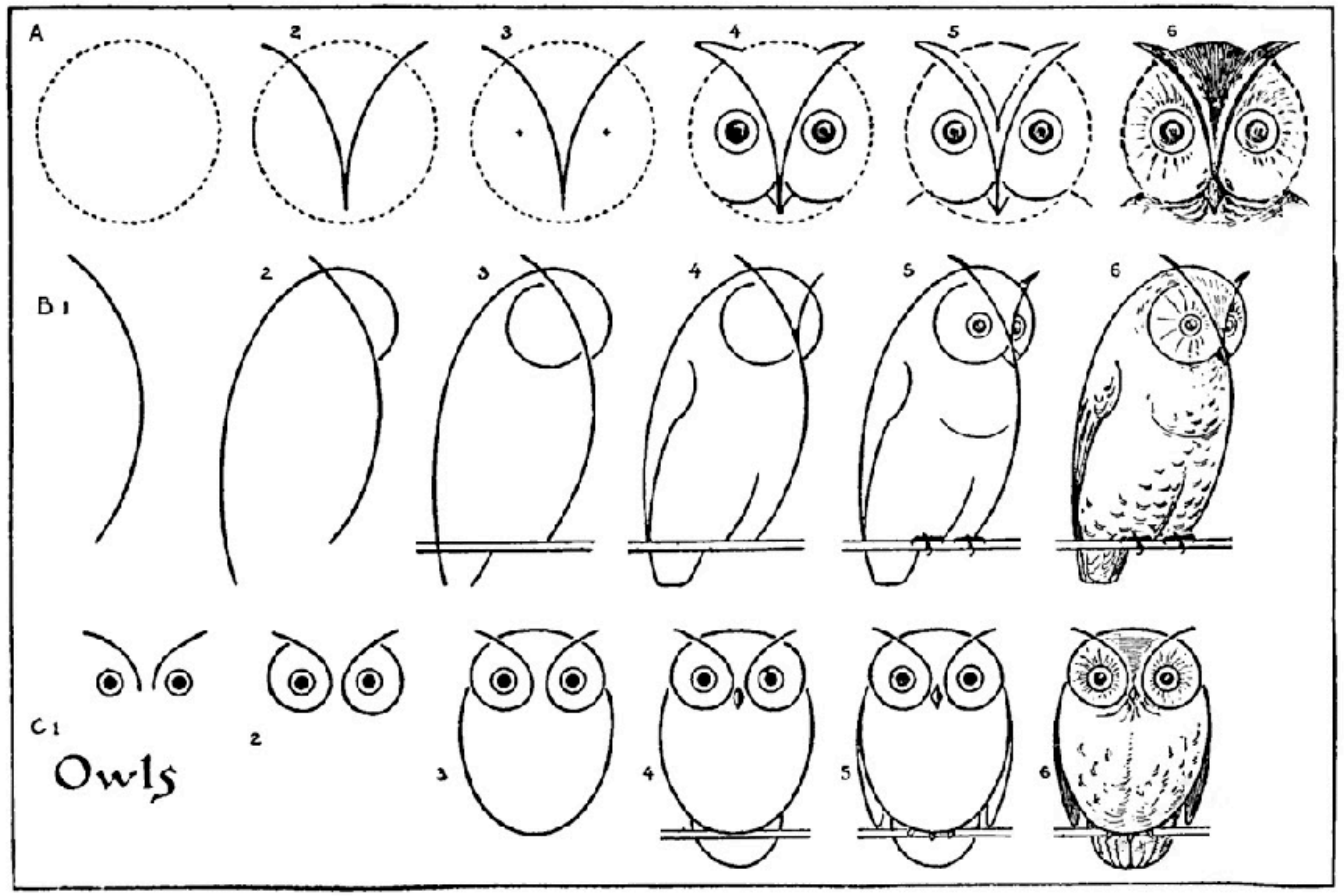
From Theory to Estimate

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
unless stratify by *Z*

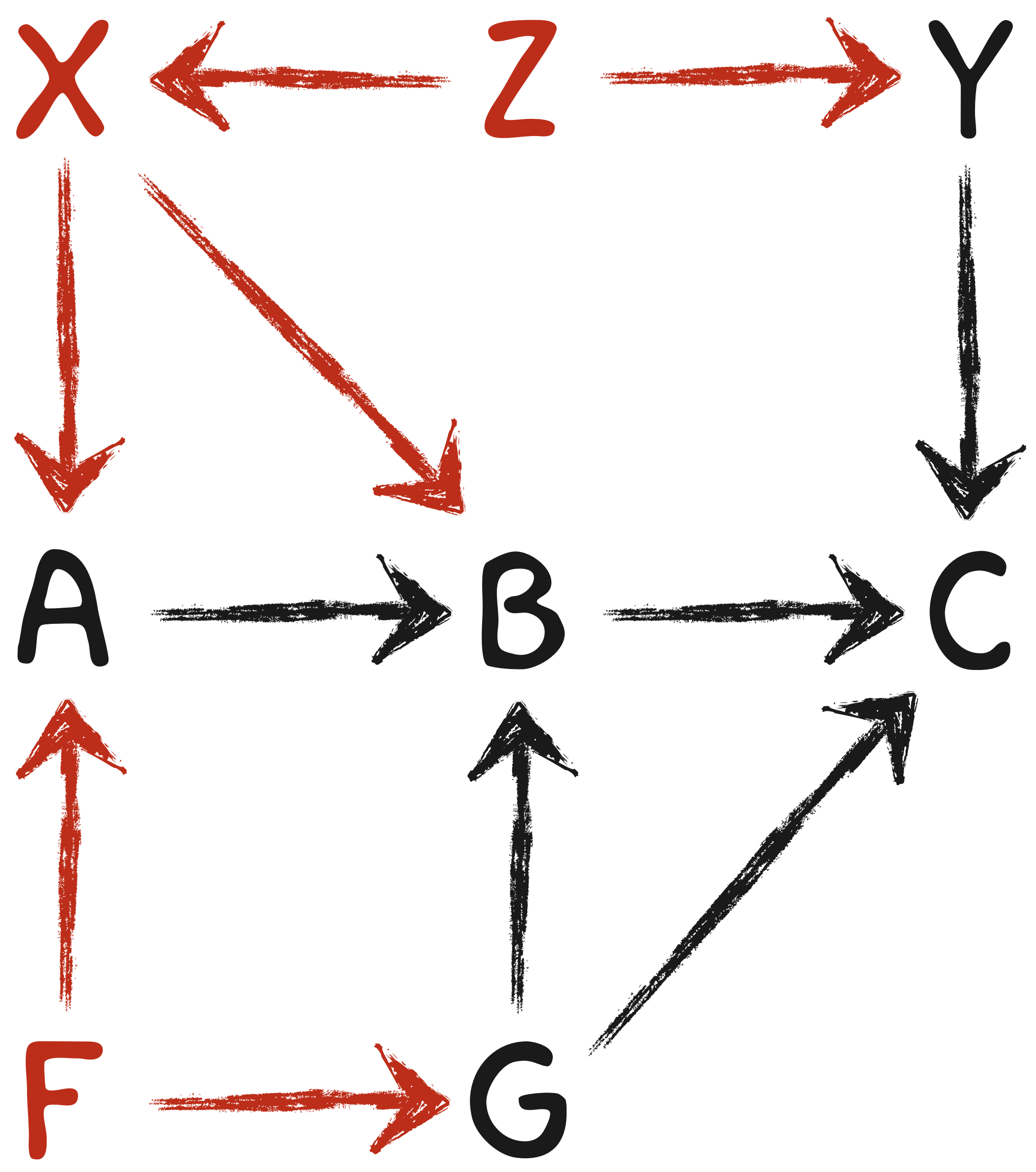
The Collider

$X \rightarrow Z \leftarrow Y$

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

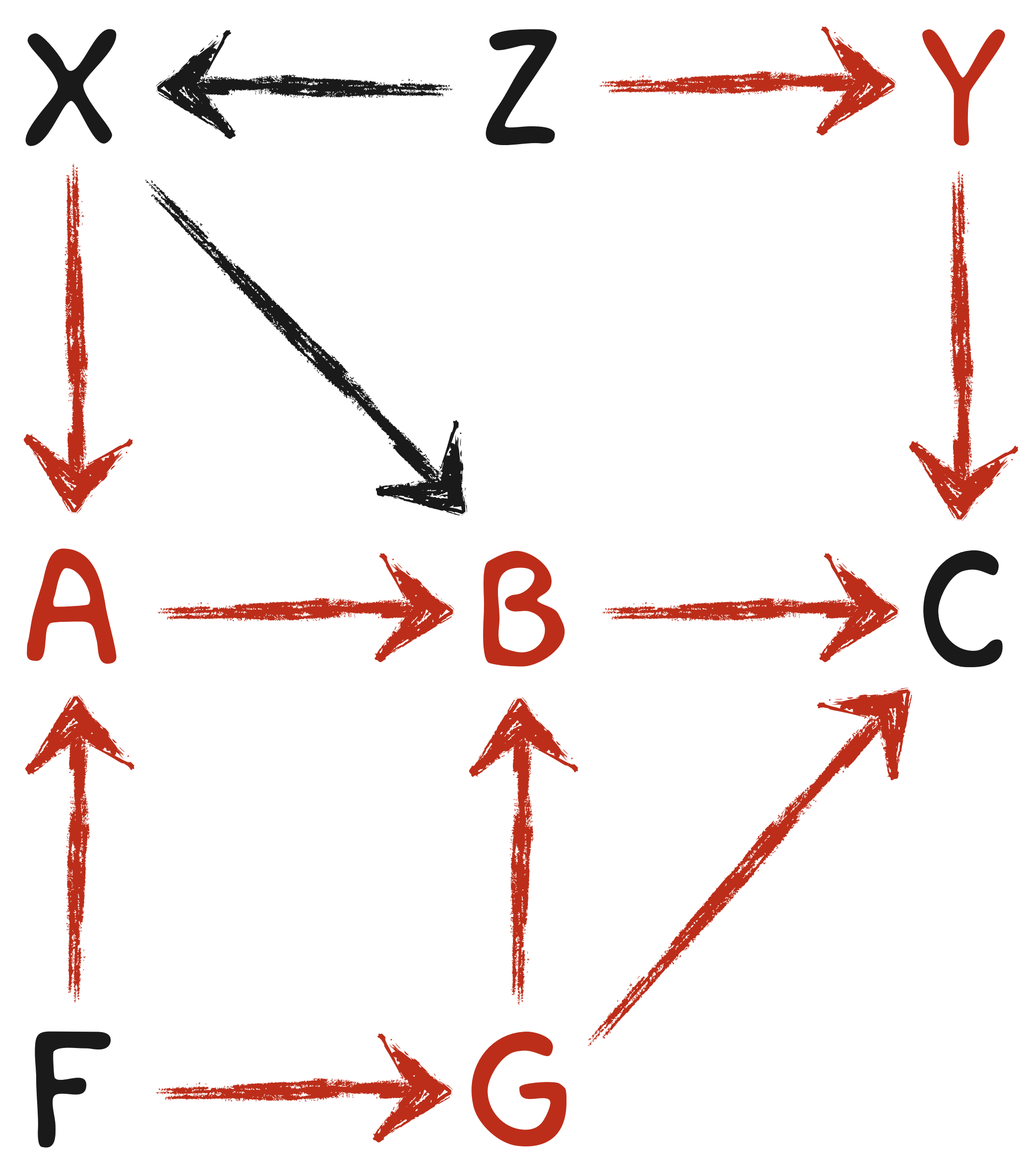
$$
\begin{tikzcd}
X \arrow[d] \arrow[dr] & Z \arrow[l] \arrow[r] & Y \arrow[d] \\
A \arrow[r] & B \arrow[r] & C \\
F \arrow[u] \arrow[r] & G \arrow[u] \arrow[ur] &
\end{tikzcd}
$$

Forks



Forks

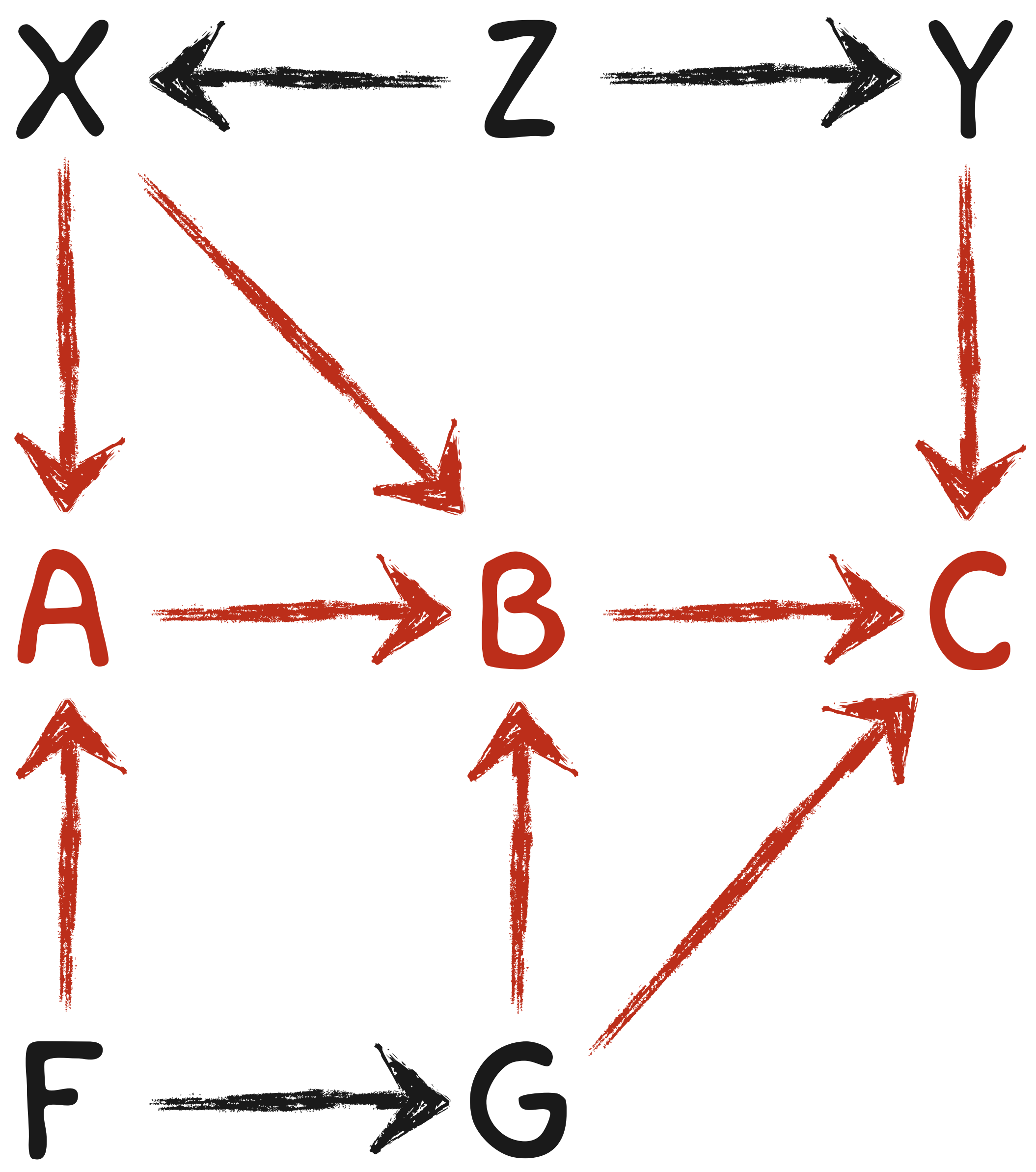
Pipes

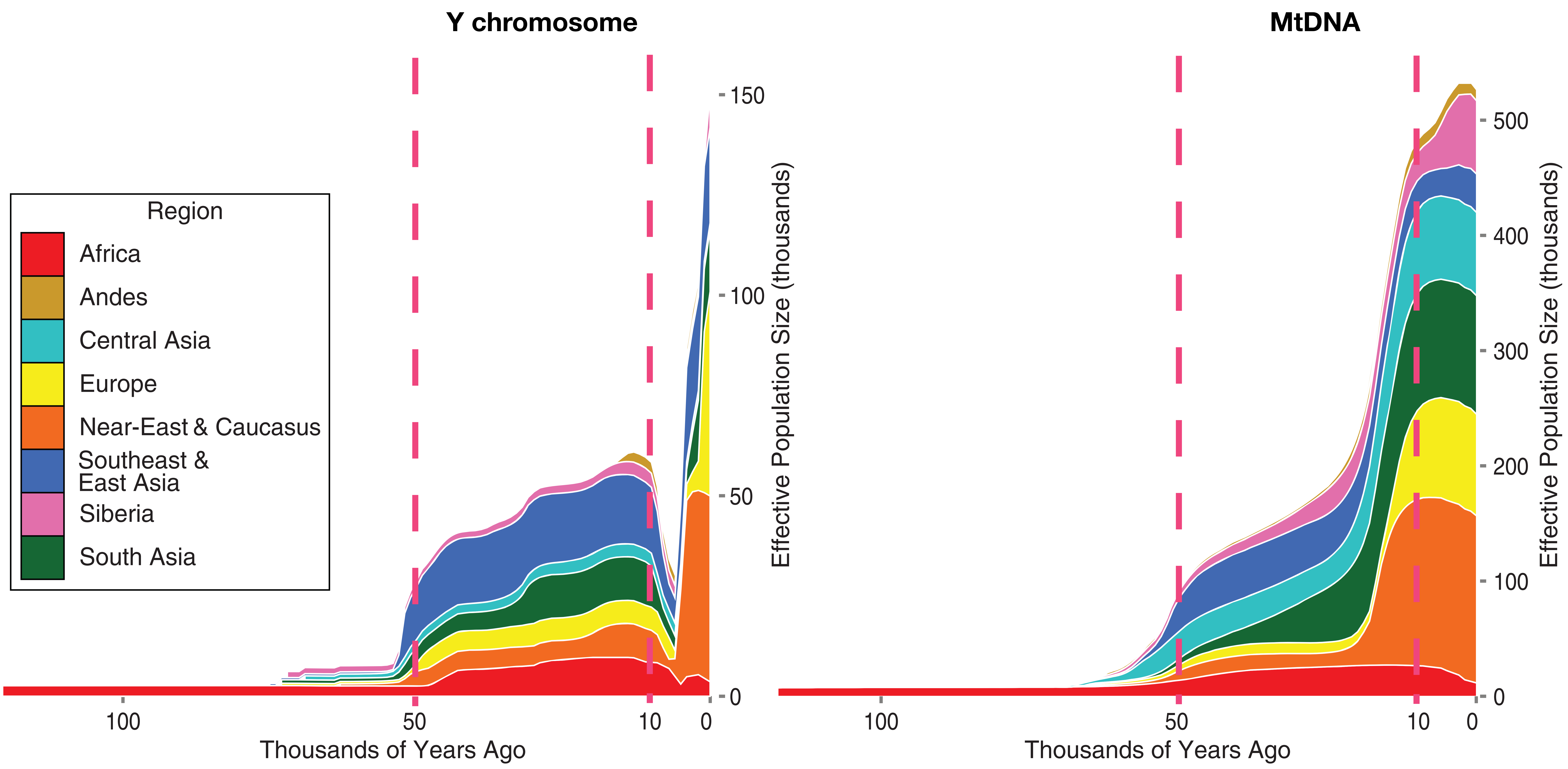


Forks

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

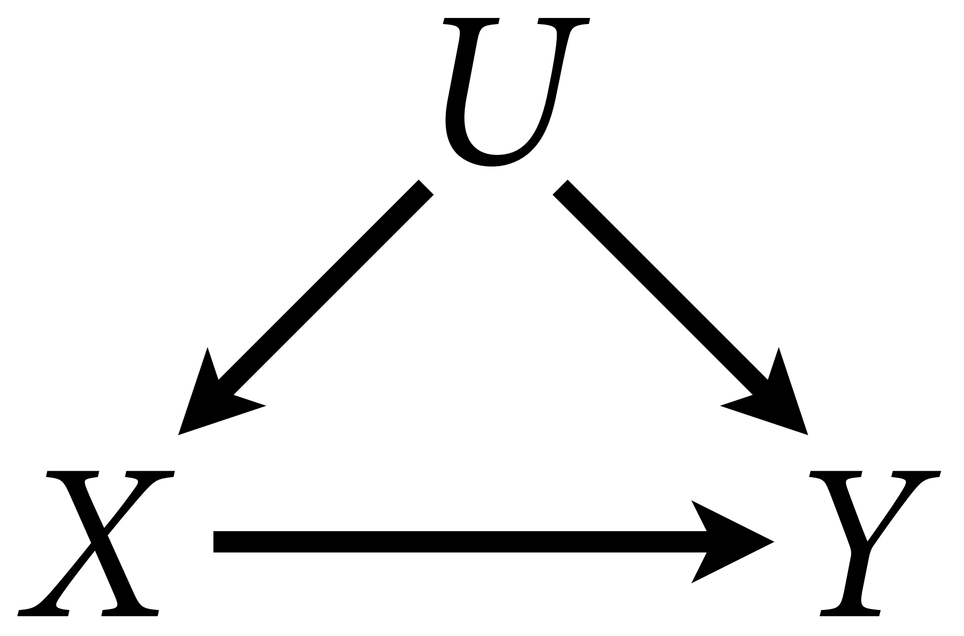
In an experiment, we cut causes of the treatment

We ***randomize*** (hopefully)

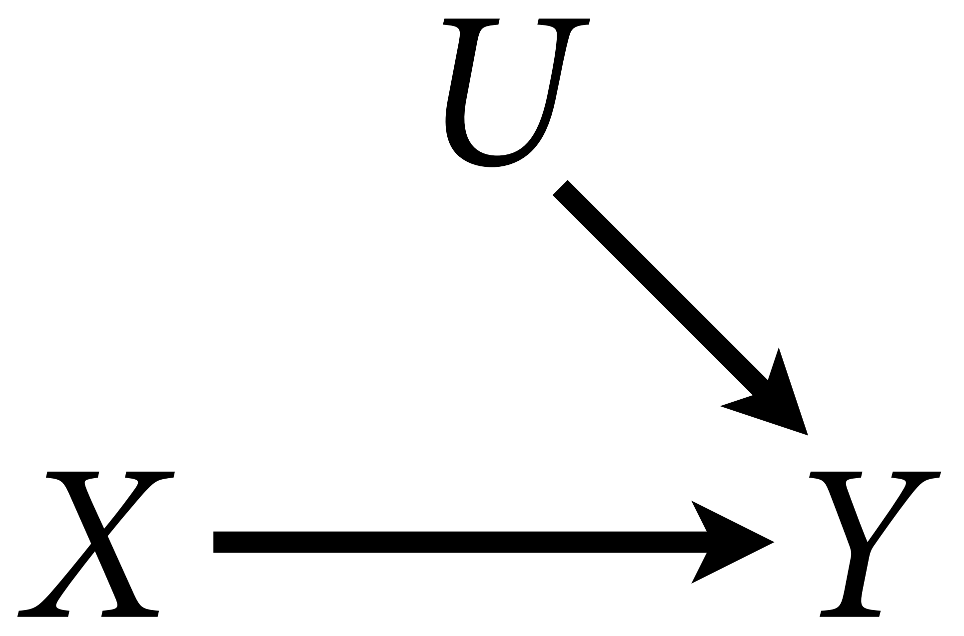
So how does causal inference without randomization ever work?

Is there a statistical procedure that mimics randomization?

Without randomization

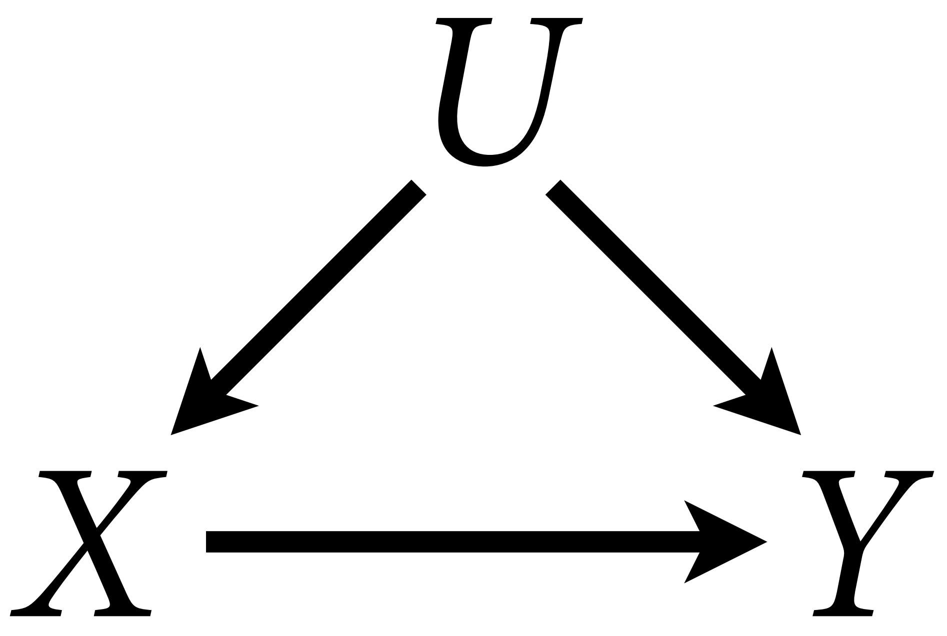


With randomization



DAG Thinking

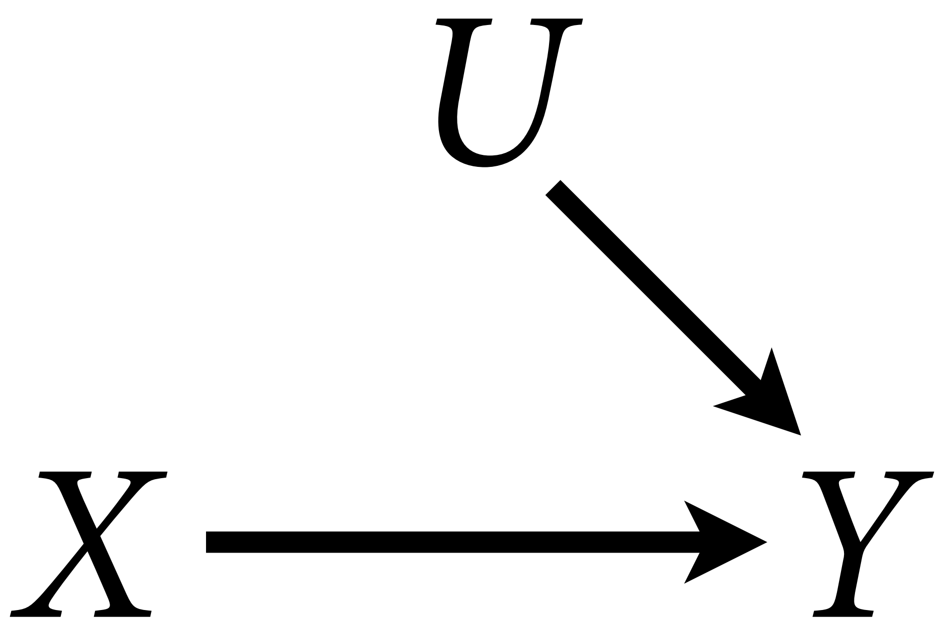
Without randomization



Is there a statistical procedure that mimics randomization?

$$P(Y \mid \mathrm{do}(X)) = P(Y \mid ?)$$

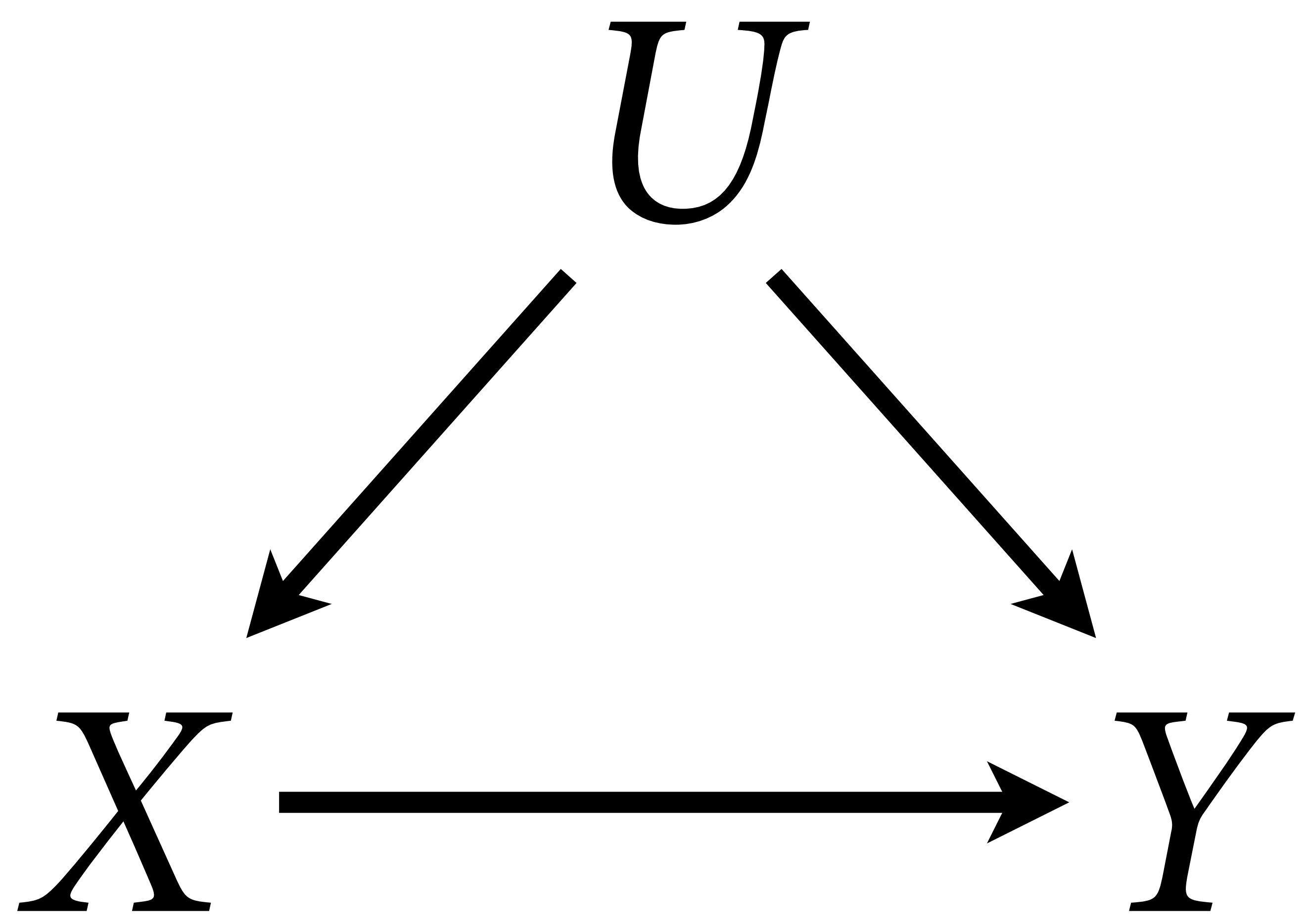
With randomization



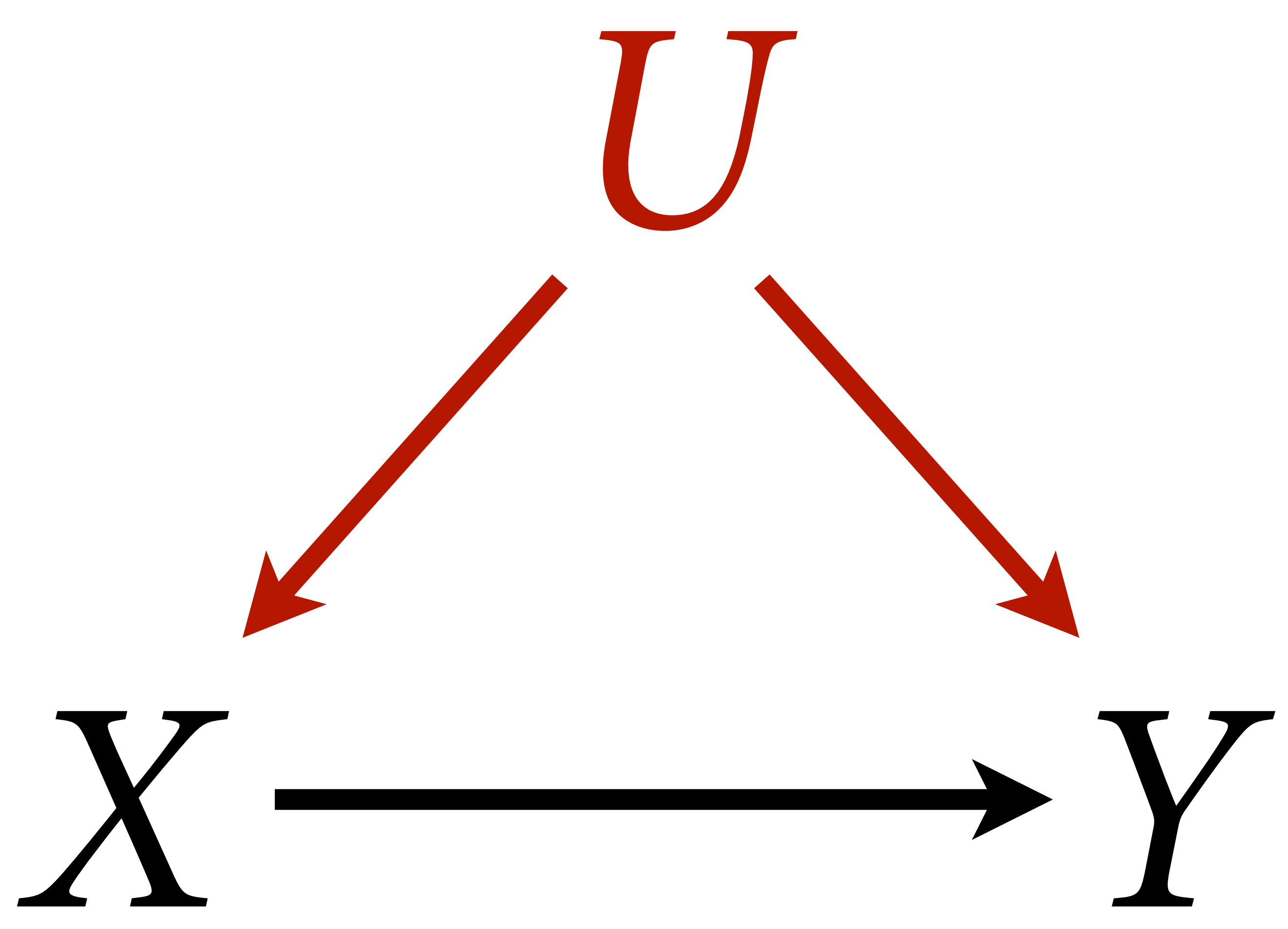
$\mathrm{do}(X)$ means intervene on X

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

Example: Simple Confound



Example: Simple Confound



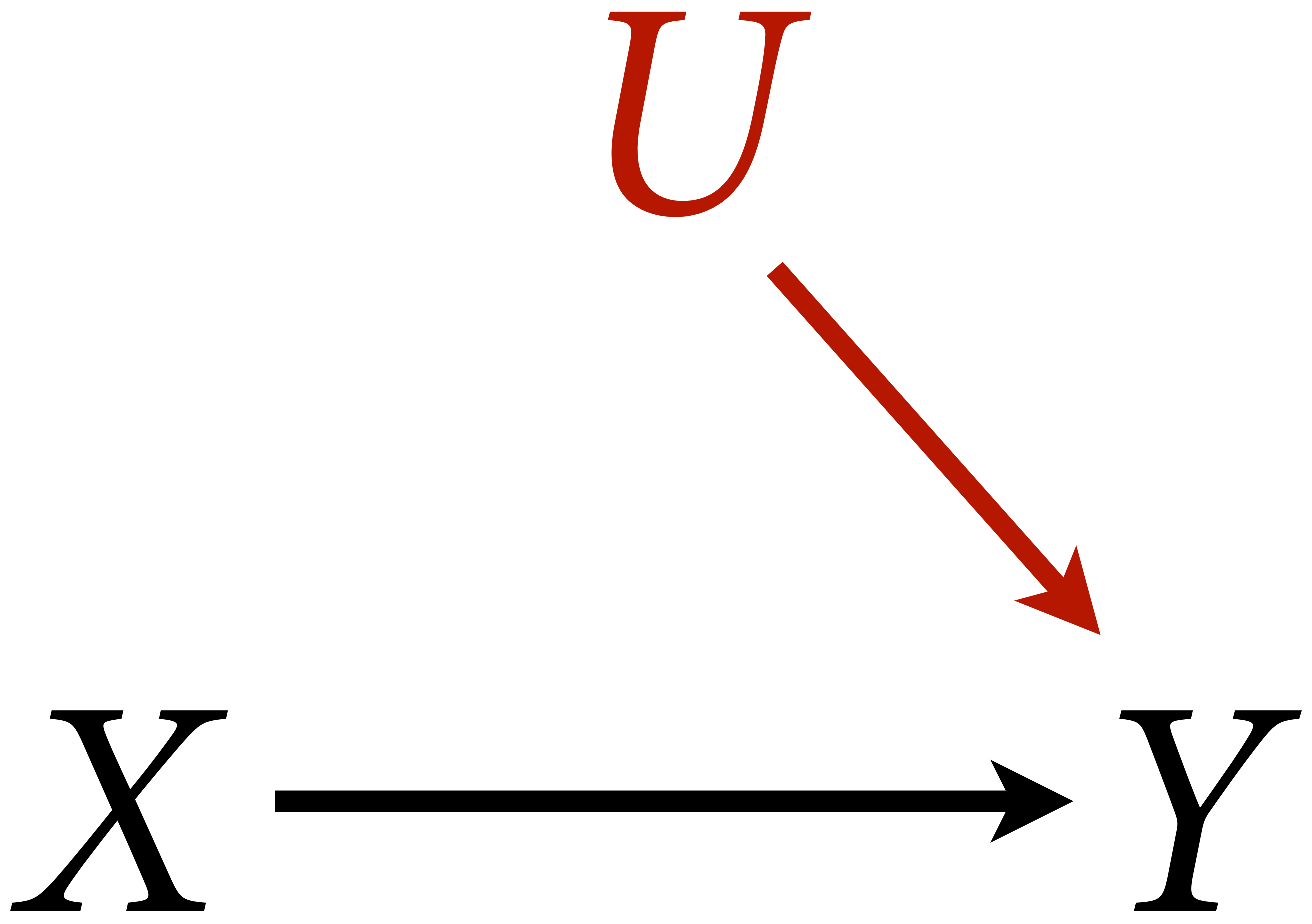
Non-causal path

X <– U –> Y

Close the fork!

Condition on U

Example: Simple Confound



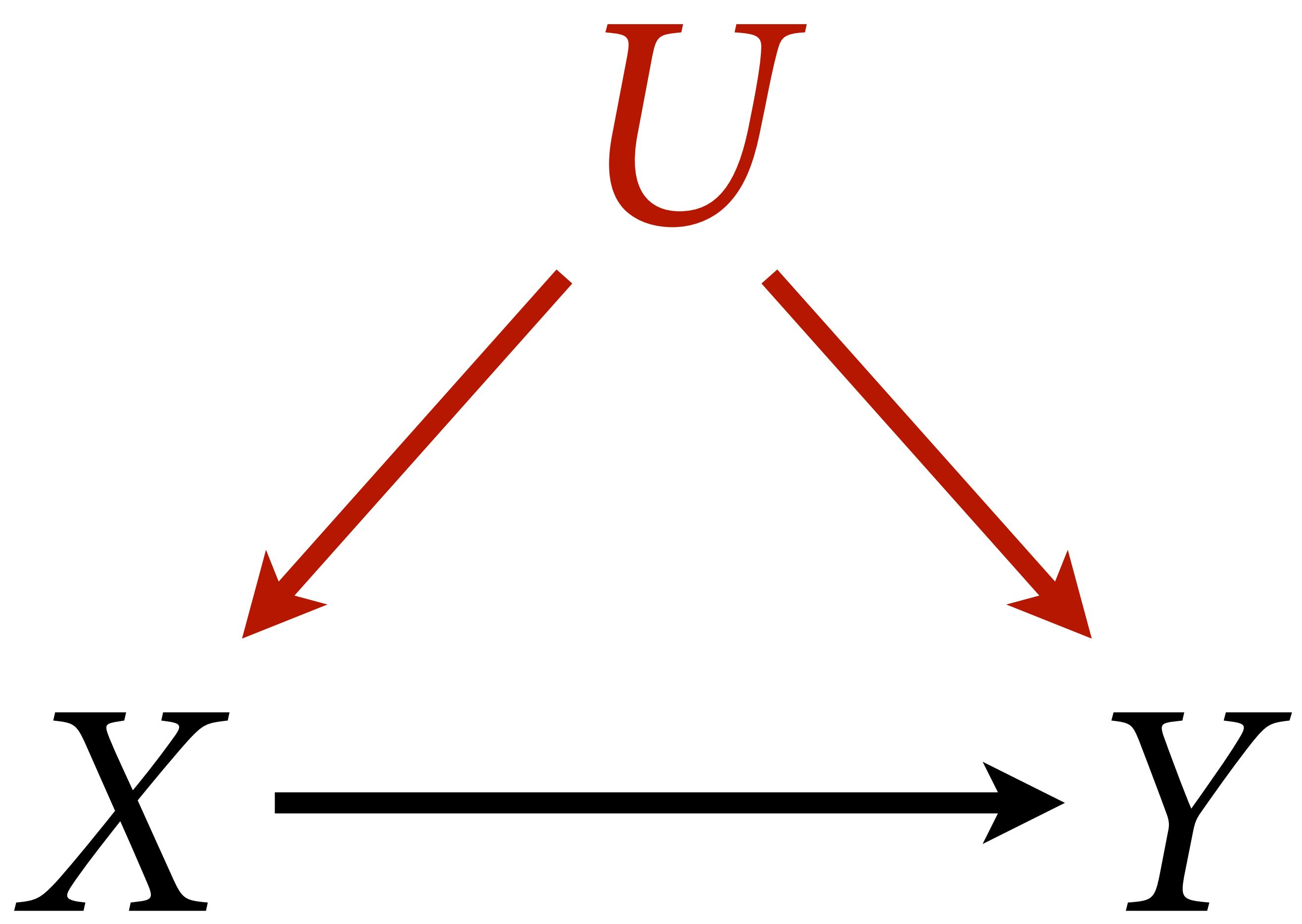
Non-causal path

X <– U –> Y

Close the fork!

Condition on U

Example: Simple Confound



Non-causal path
X <– U –> Y

Close the fork!
Condition on U

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

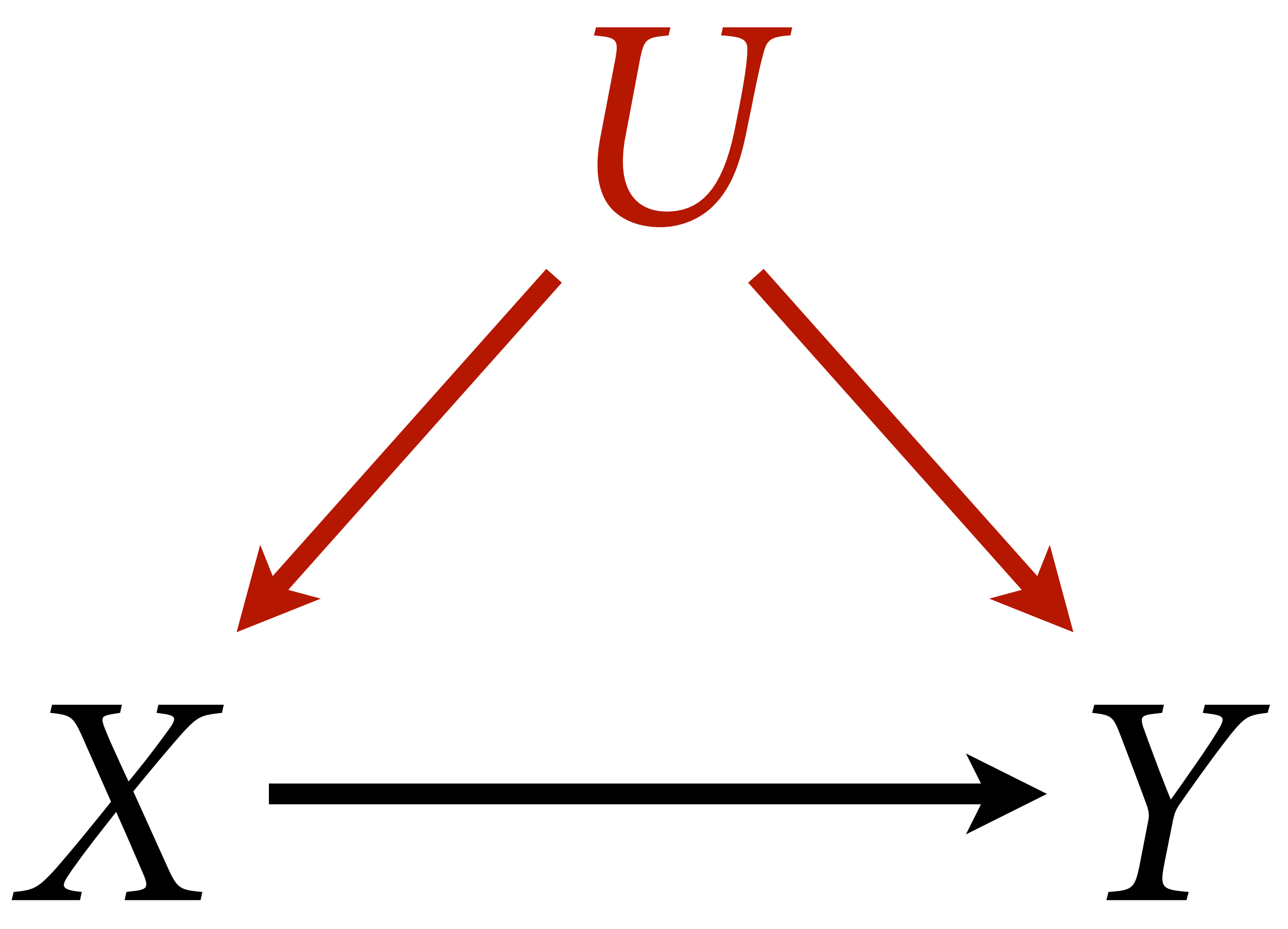
"The distribution of Y, stratified by X and U, averaged over the distribution of U."

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

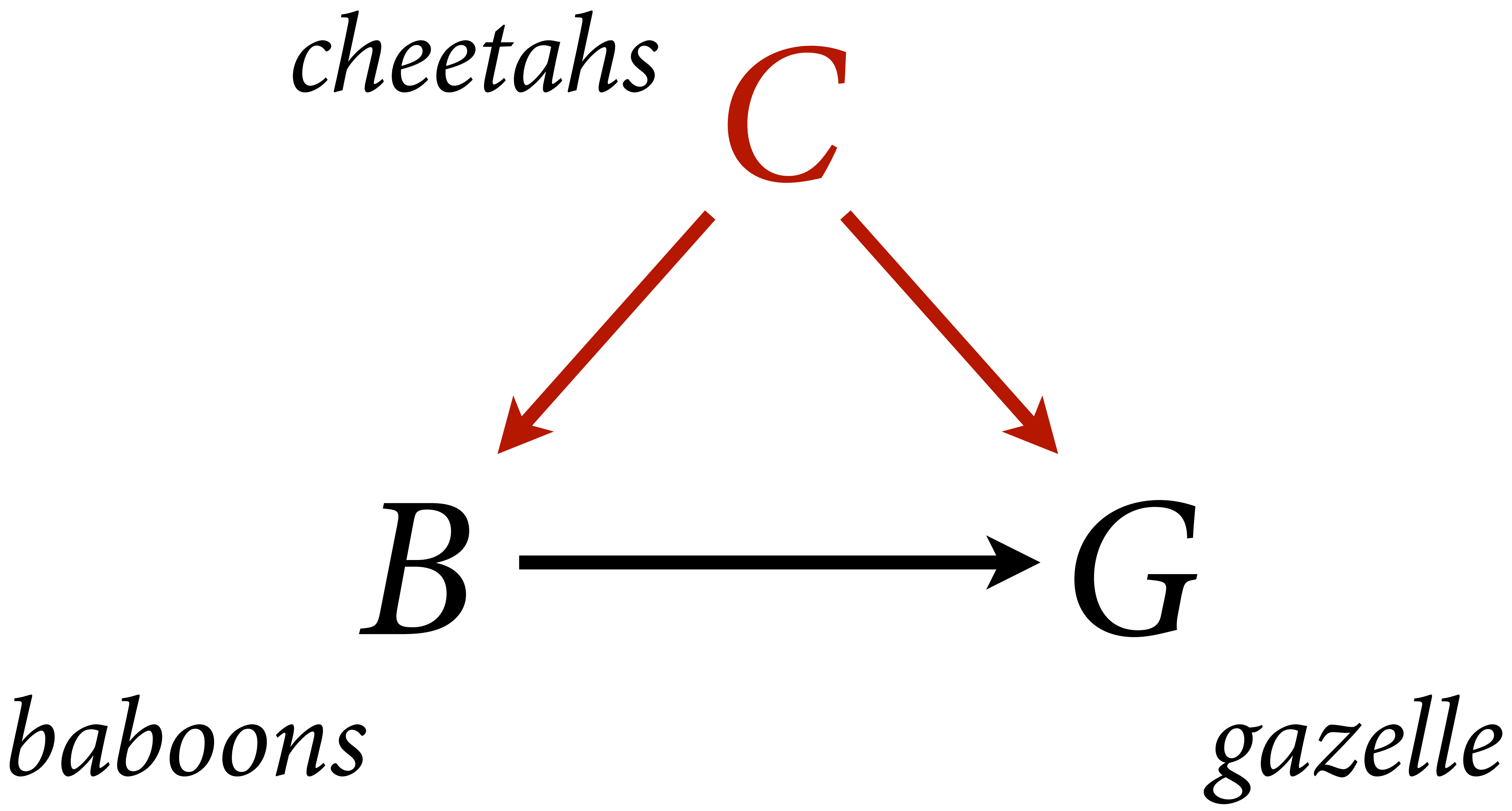
"The distribution of Y, stratified by X and U, averaged over the distribution of U."

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)

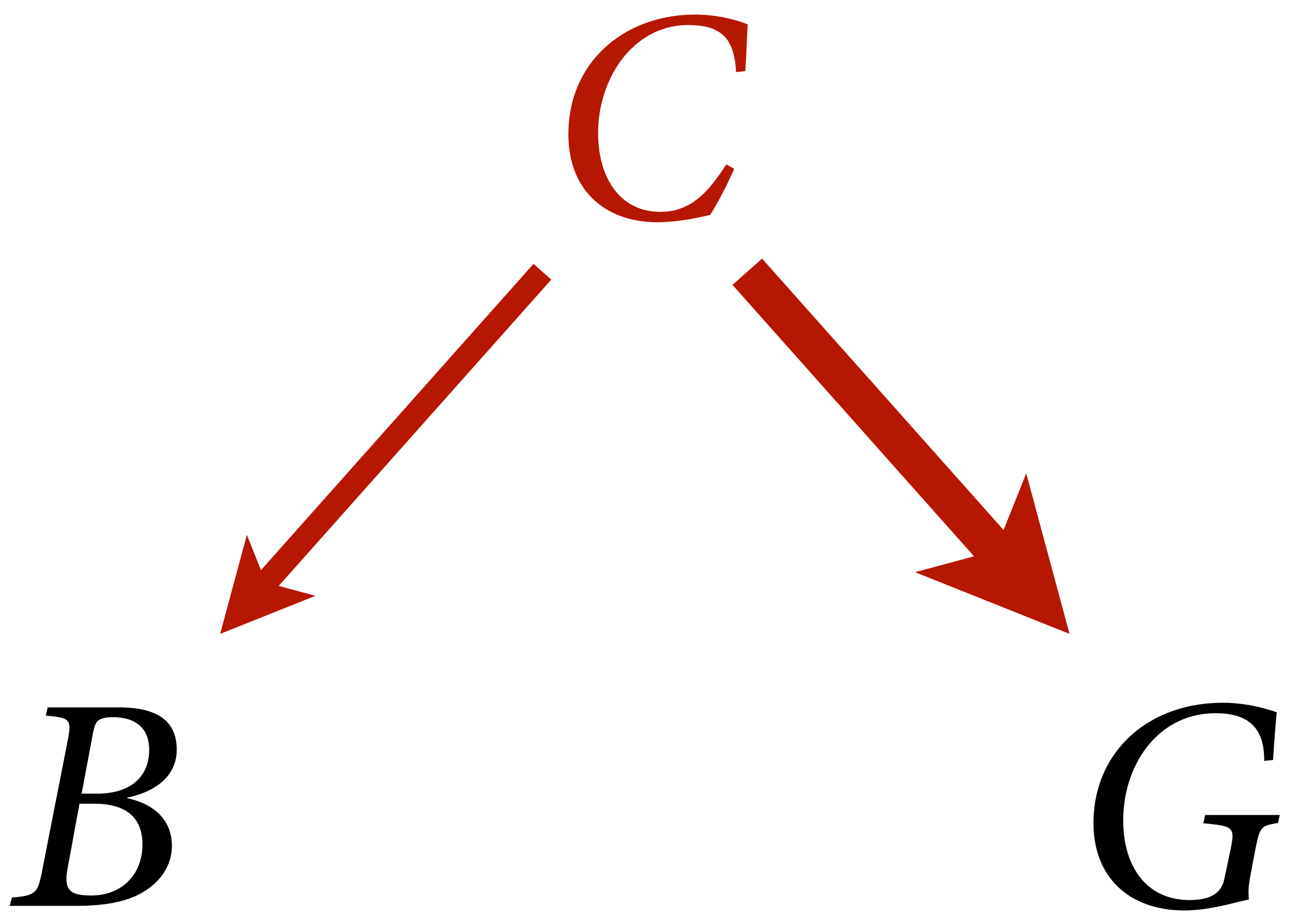


Marginal Effects Example

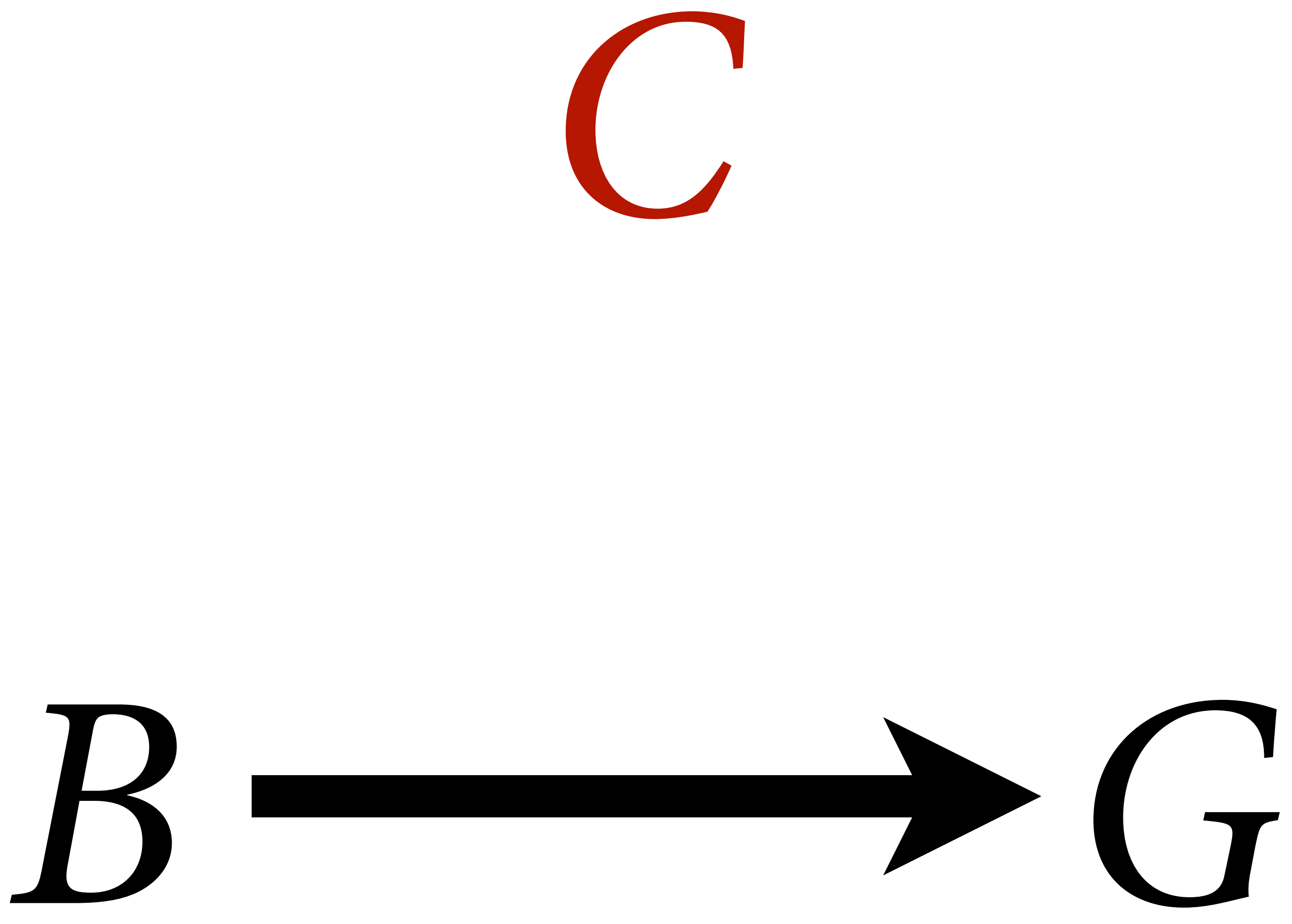


Marginal Effects Example

cheetahs present



cheetahs absent



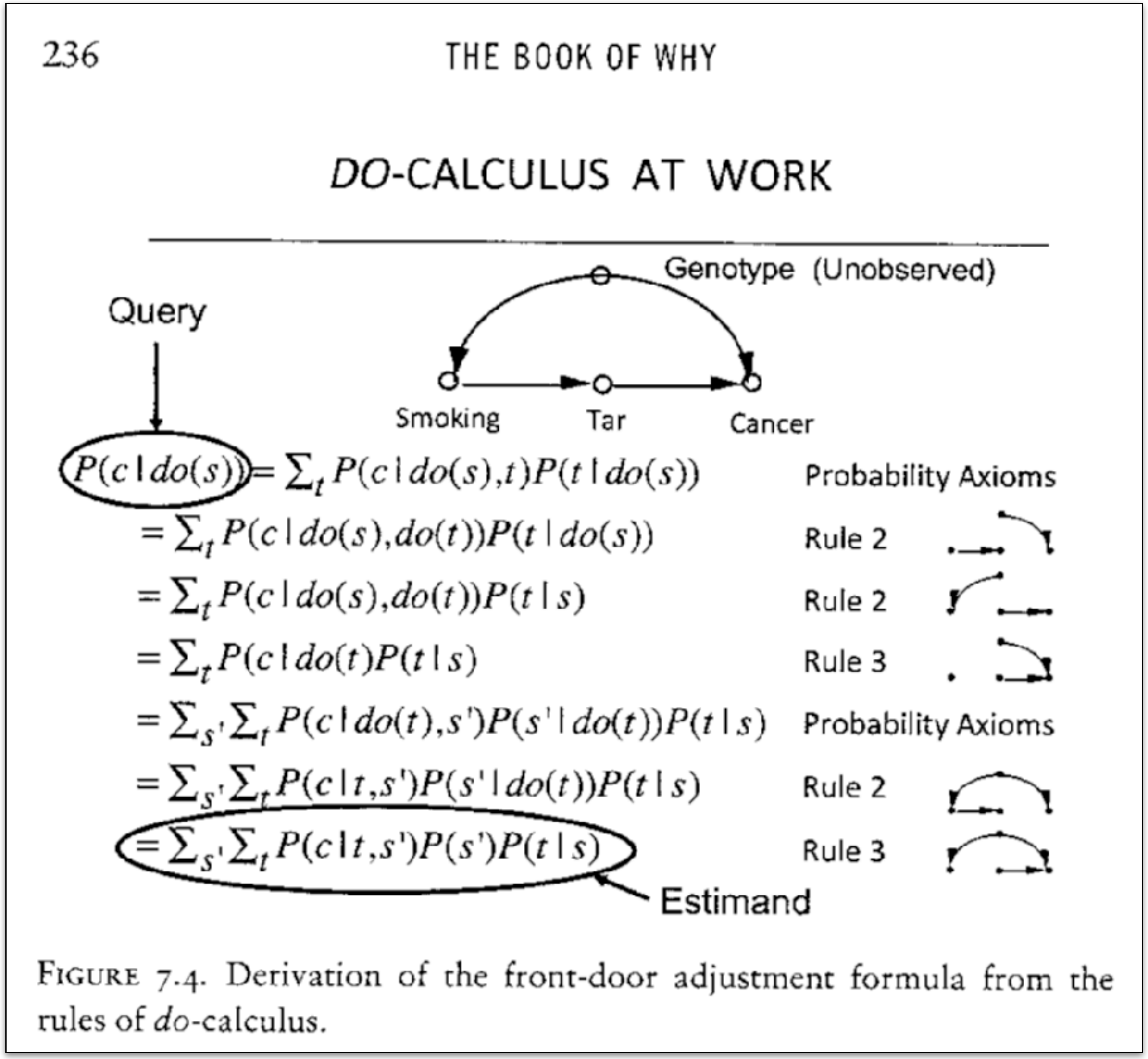
Causal effect of baboons depends upon distribution of cheetahs

do-calculus

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

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

Additional assumptions yield additional implications



236 THE BOOK OF WHY

DO-CALCULUS AT WORK

$$P(c \mid do(s)) = \Sigma_t P(c \mid do(s), t) P(t \mid do(s))$$ Probability Axioms

$$= \Sigma_t P(c \mid do(s), do(t)) P(t \mid do(s))$$ Rule 2

$$= \Sigma_t P(c \mid do(s), do(t)) P(t \mid s)$$ Rule 2

$$= \Sigma_t P(c \mid do(t)) P(t \mid s)$$ Rule 3

$$= \Sigma_{s'} \Sigma_t P(c \mid do(t), s') P(s' \mid do(t)) P(t \mid s)$$ Probability Axioms

$$= \Sigma_{s'} \Sigma_t P(c \mid t, s') P(s' \mid do(t)) P(t \mid s)$$ Rule 2

$$= \Sigma_{s'} \Sigma_t P(c \mid t, s') P(s') P(t \mid s)$$ Rule 3

Estimand

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

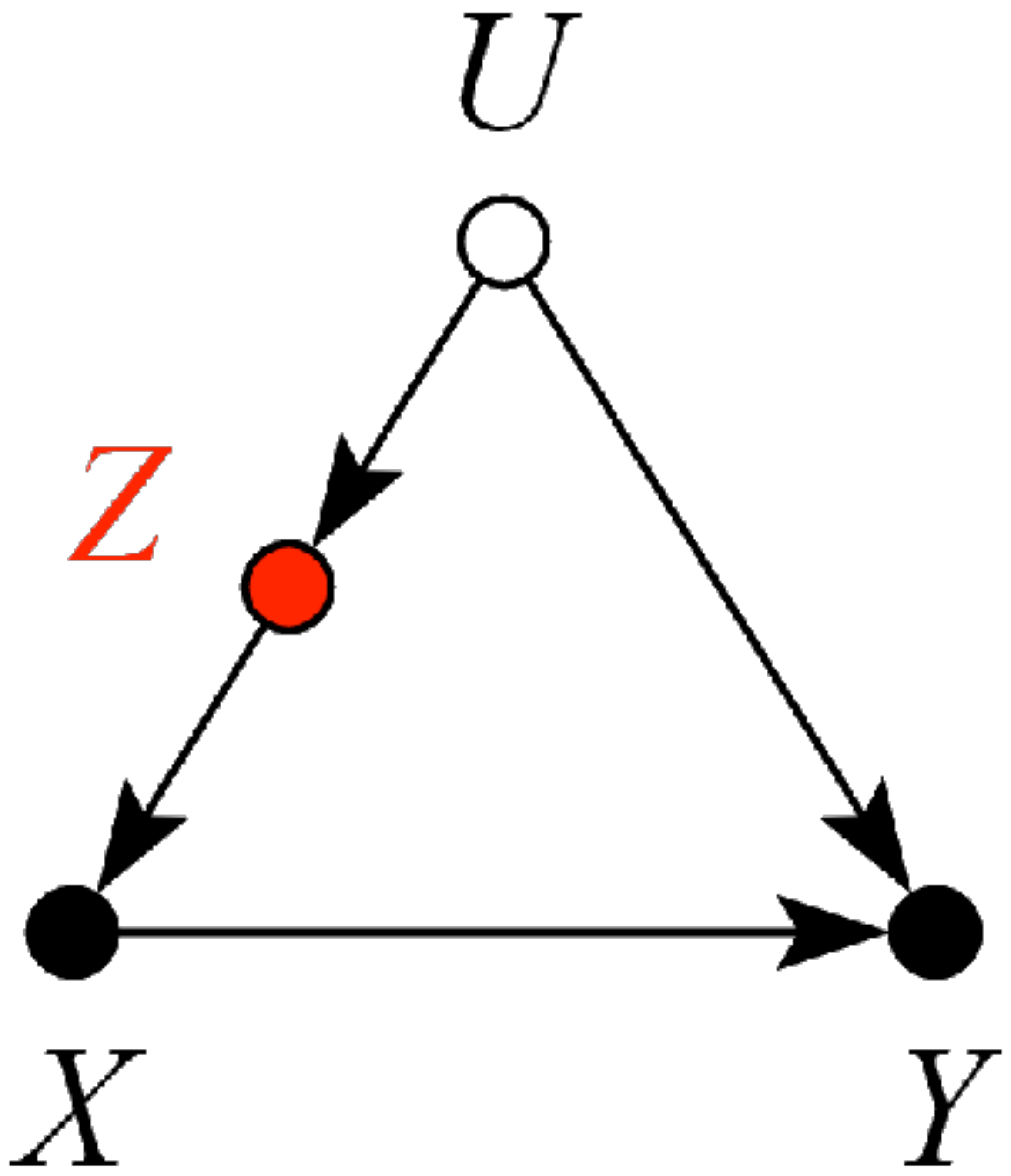
Judea Pearl, father of do-calculus, in 1966

Backdoor Criterion

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

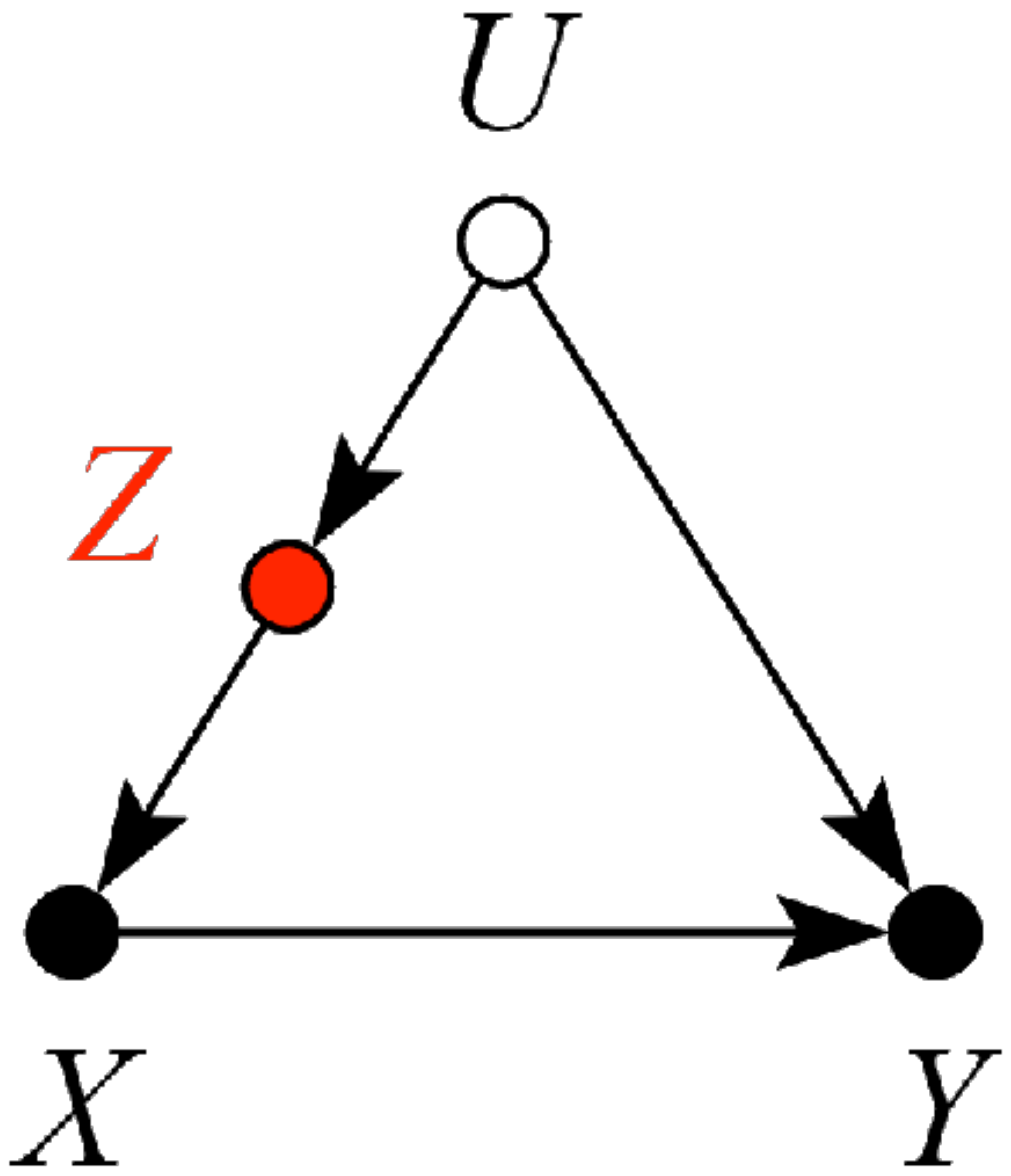
Backdoor Criterion is a shortcut to applying rules of do-calculus

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



Backdoor Criterion

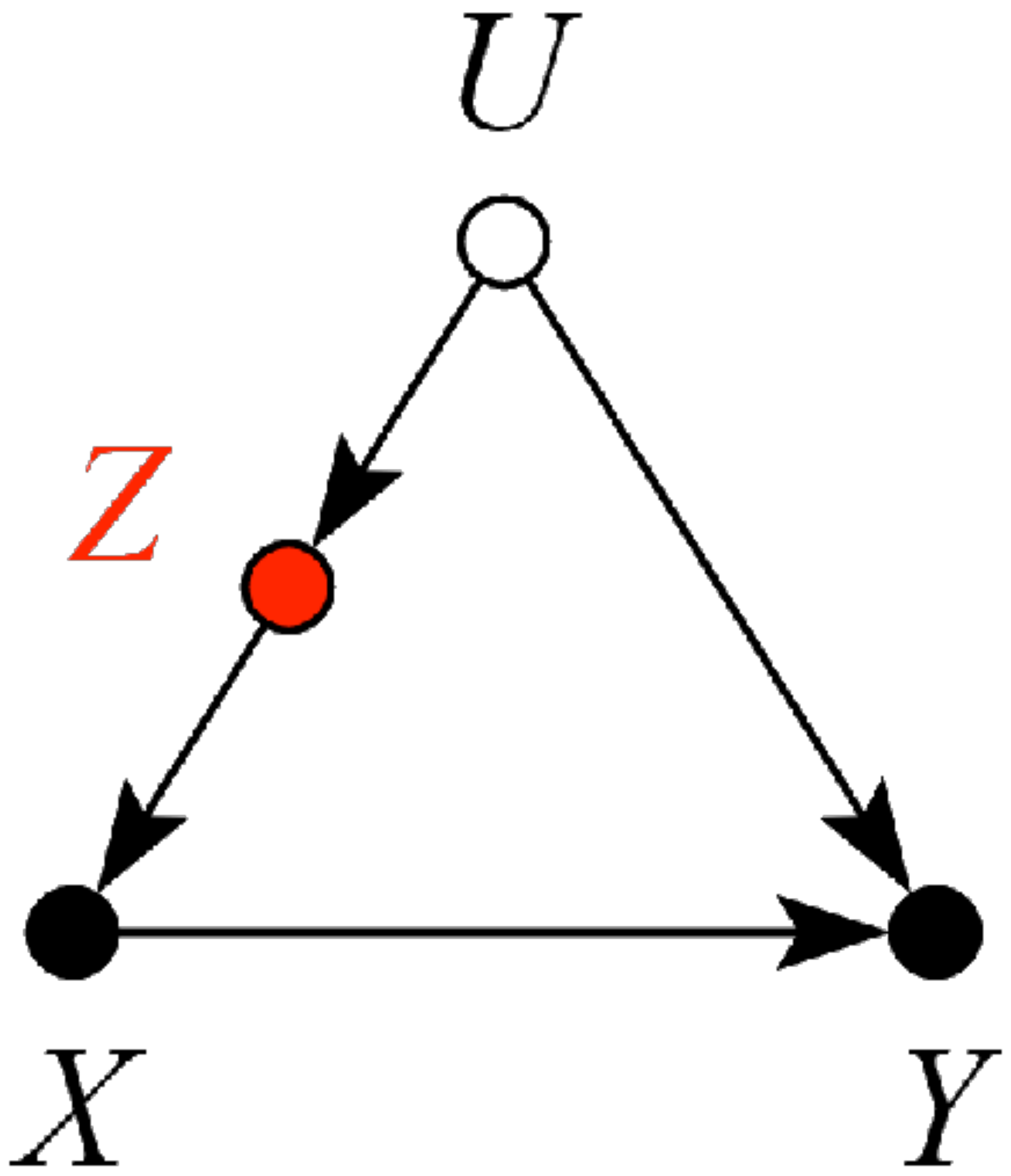
Backdoor Criterion: Rule to find a set of variables to stratify (condition) by to yield $P(Y|\mathrm{do}(X))$



Backdoor Criterion

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

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

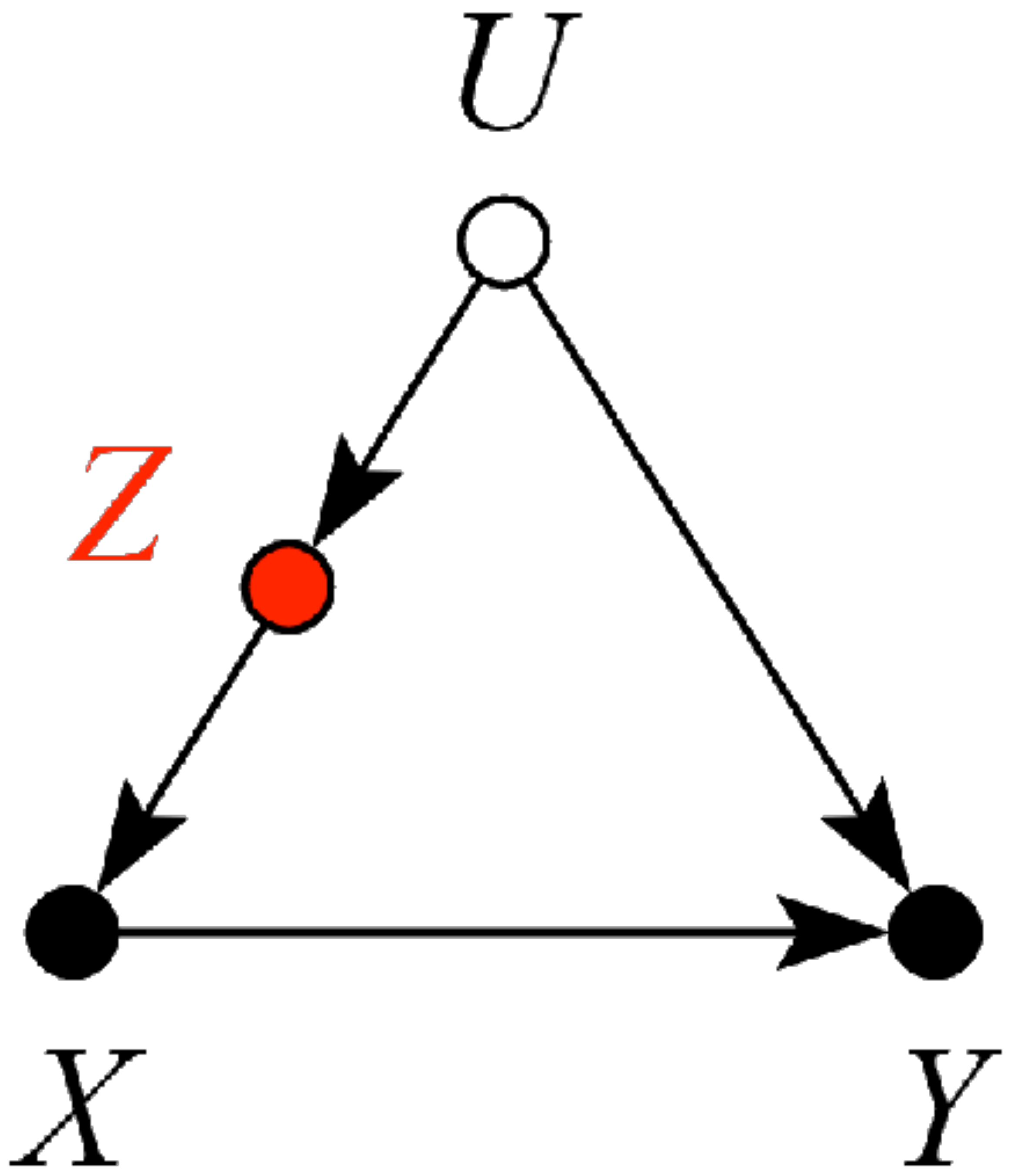


Backdoor Criterion

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

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

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



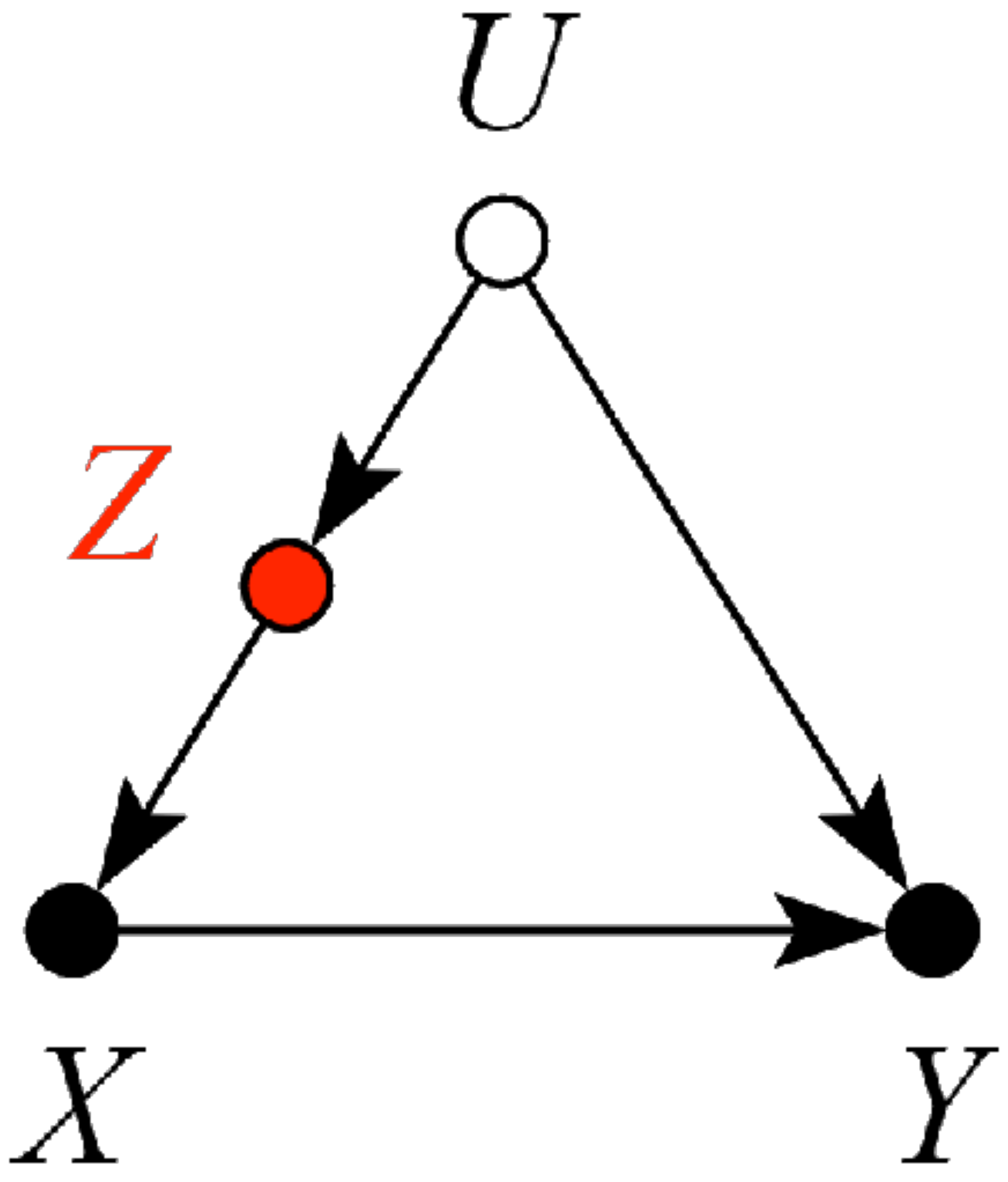
Backdoor Criterion

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

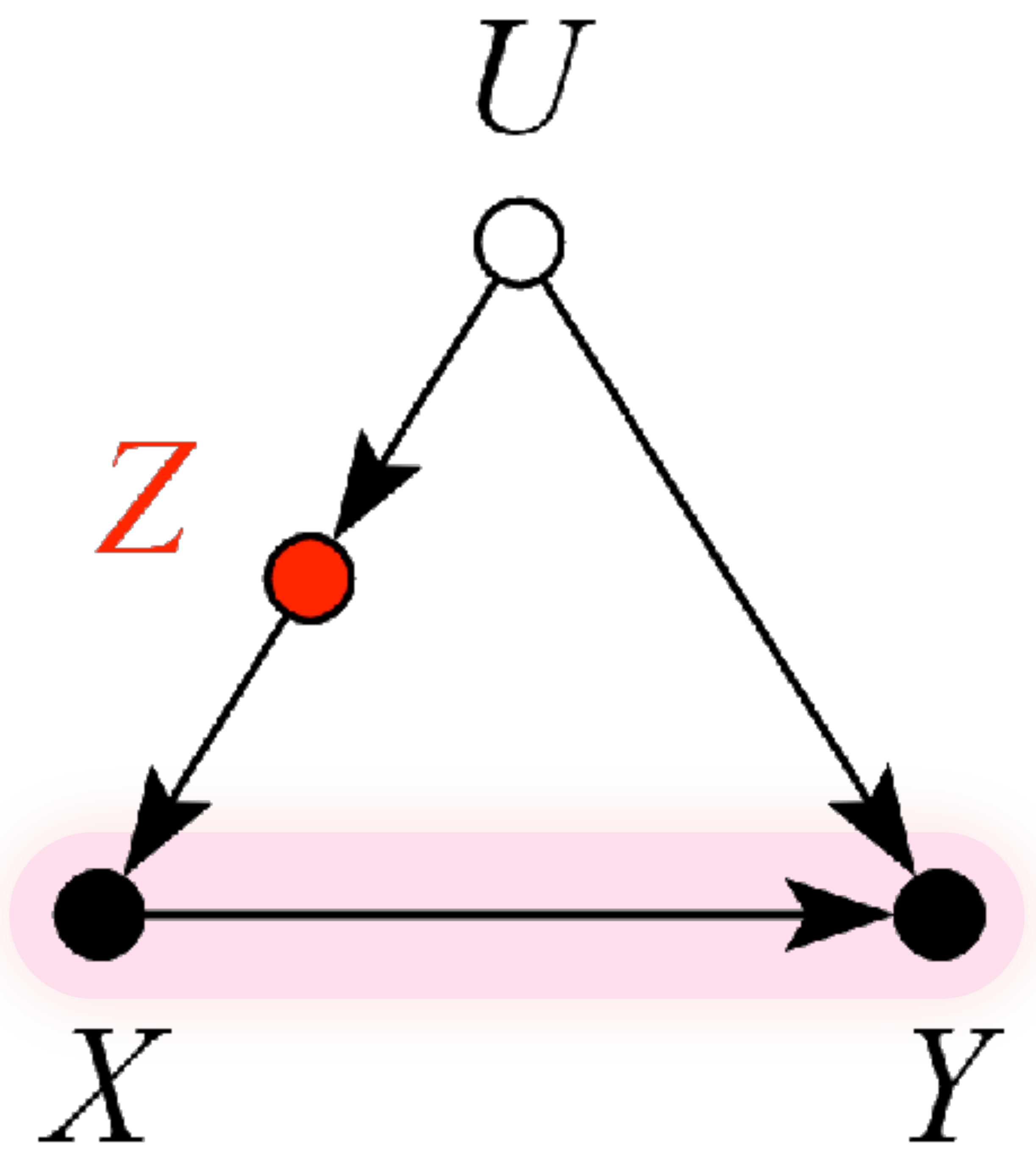
(1) Identify all **paths** connection the treatment (X) to the outcome (Y)

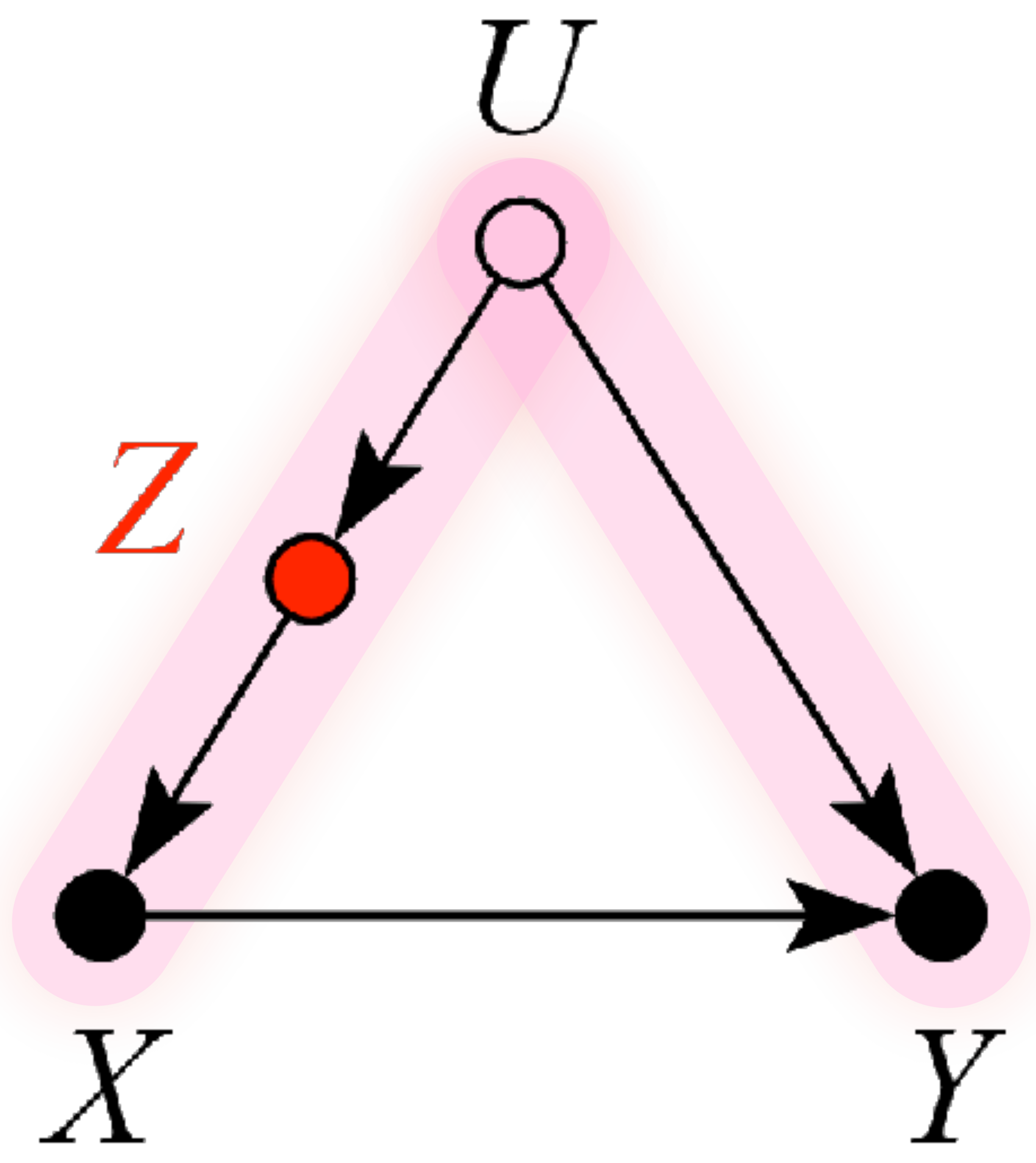
(2) Paths with arrows **entering** X are backdoor paths (non-causal paths)

(3) Find adjustment set that closes/blocks all backdoor paths

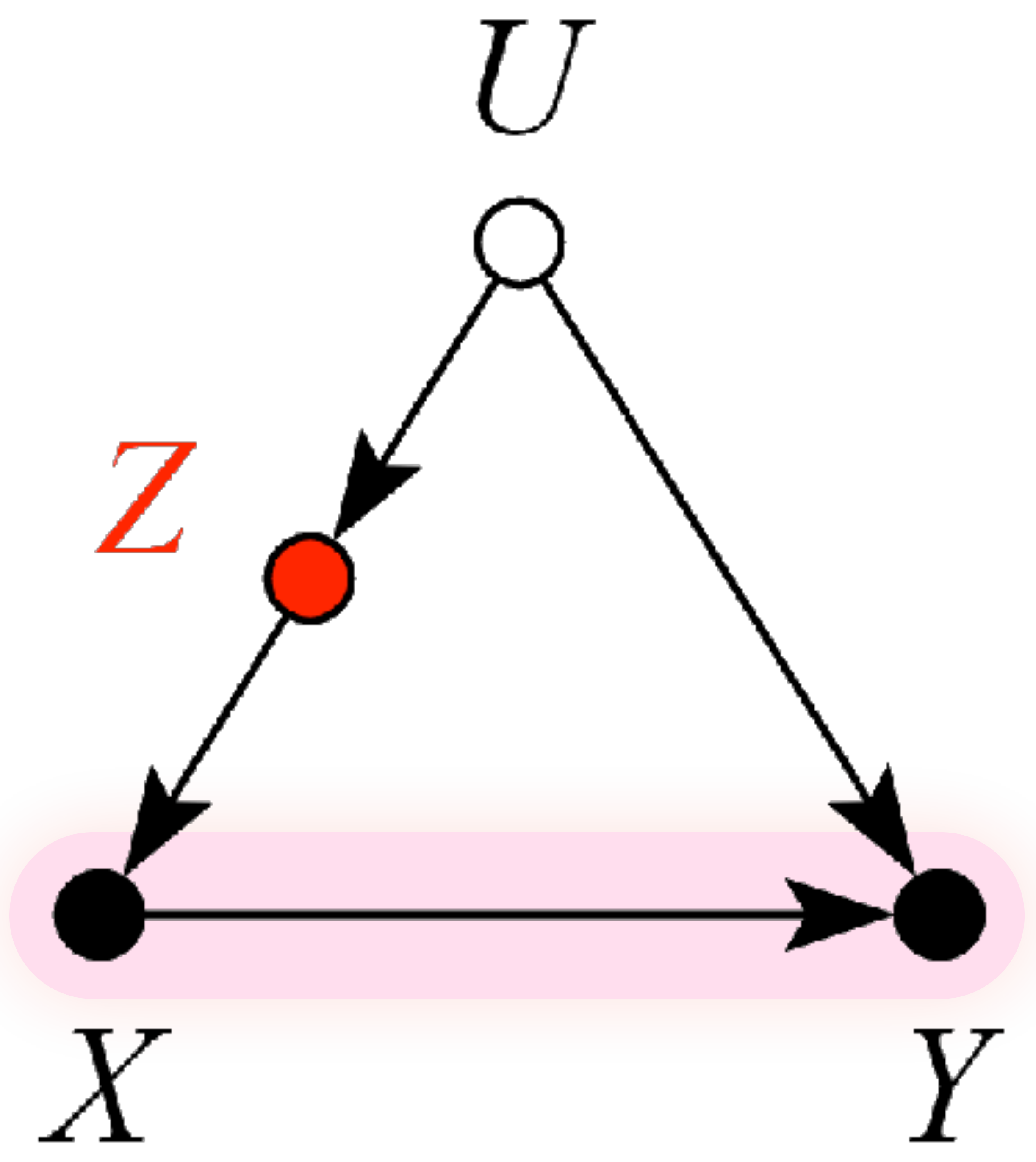


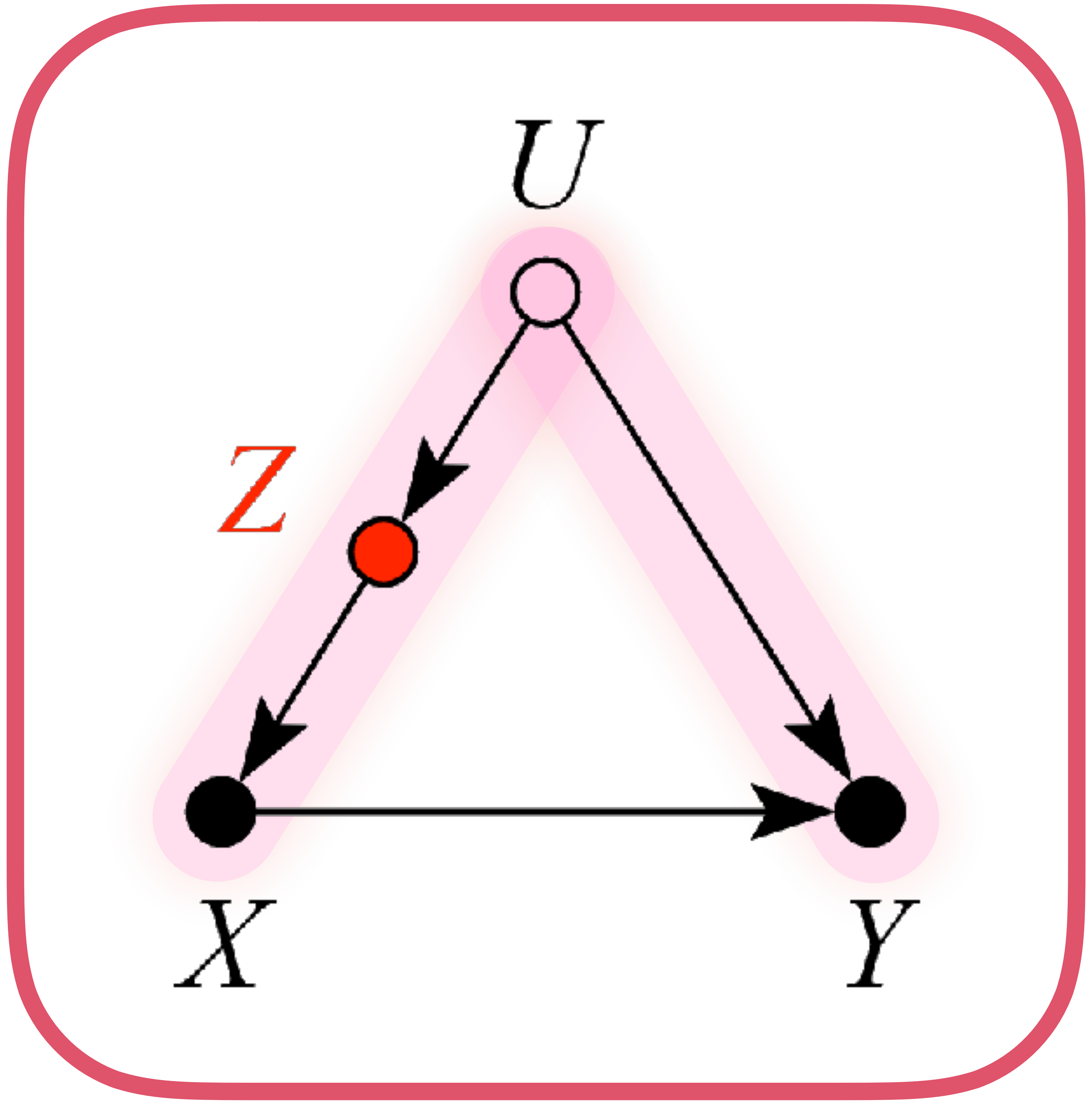
(1) Identify all **paths** connection the treatment (X) to the outcome (Y)





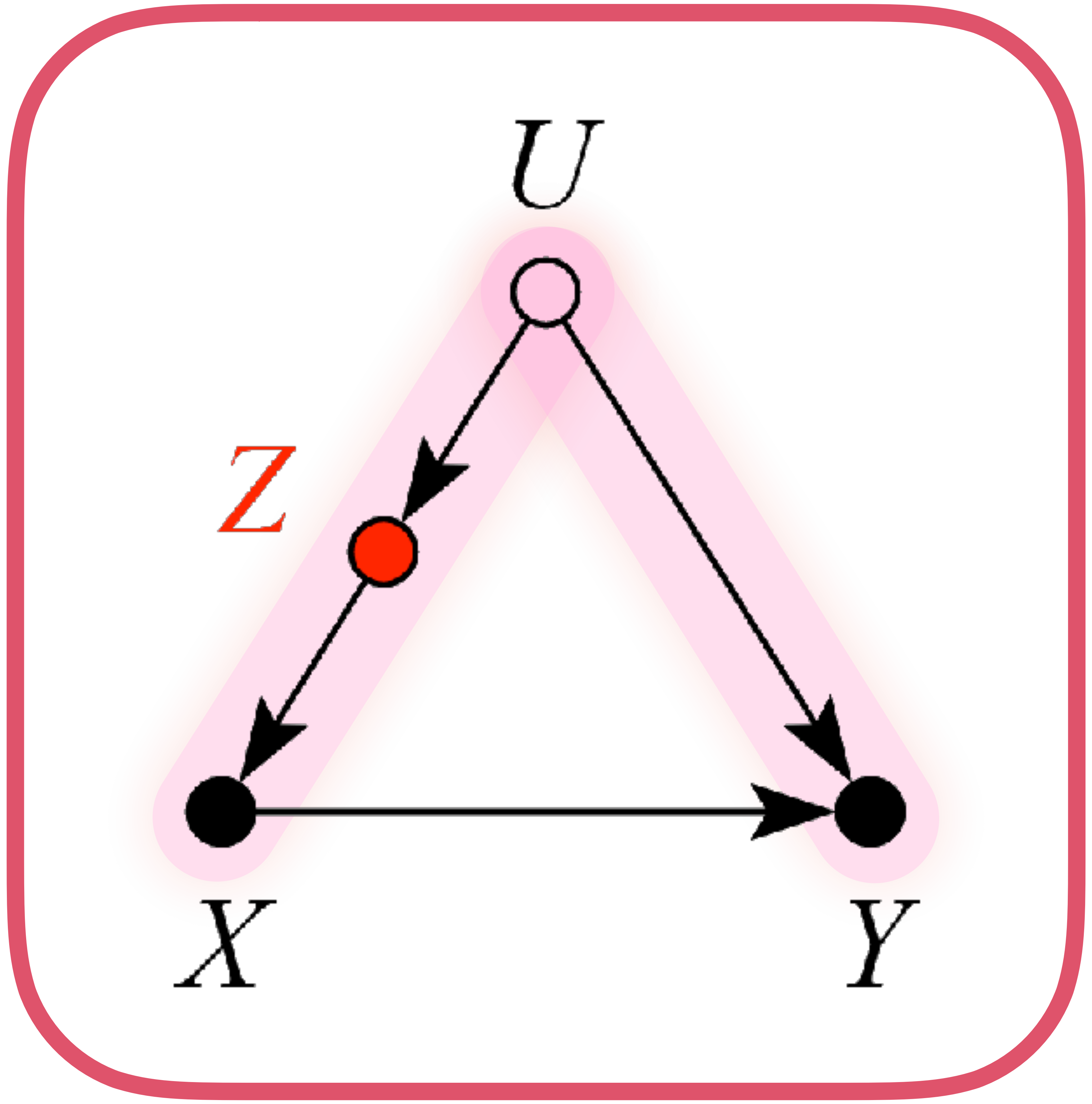
(2) Paths with arrows **entering** X are backdoor paths (non-causal paths)





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

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

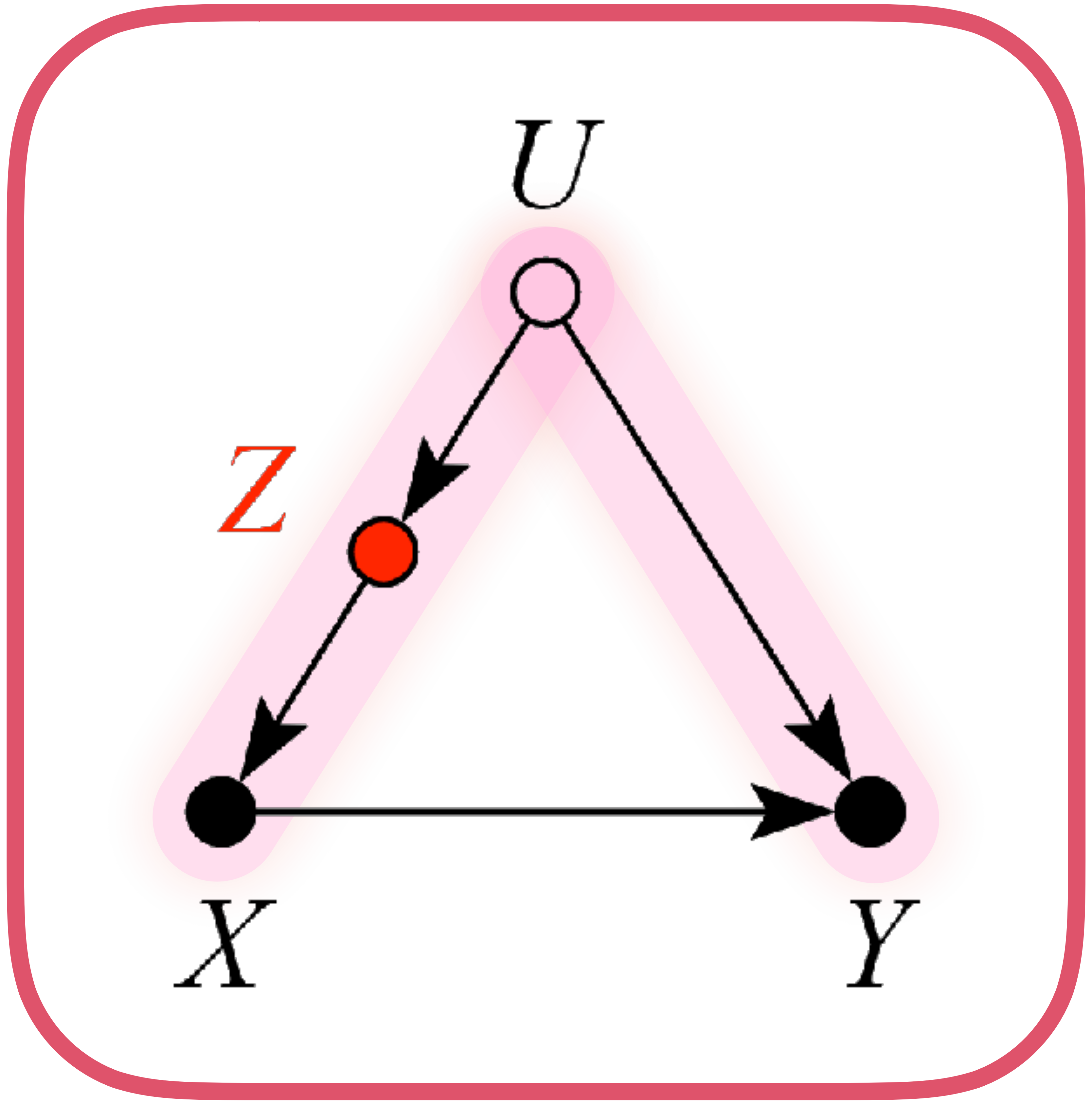


(3) Find a set of control variables that close/block all backdoor paths

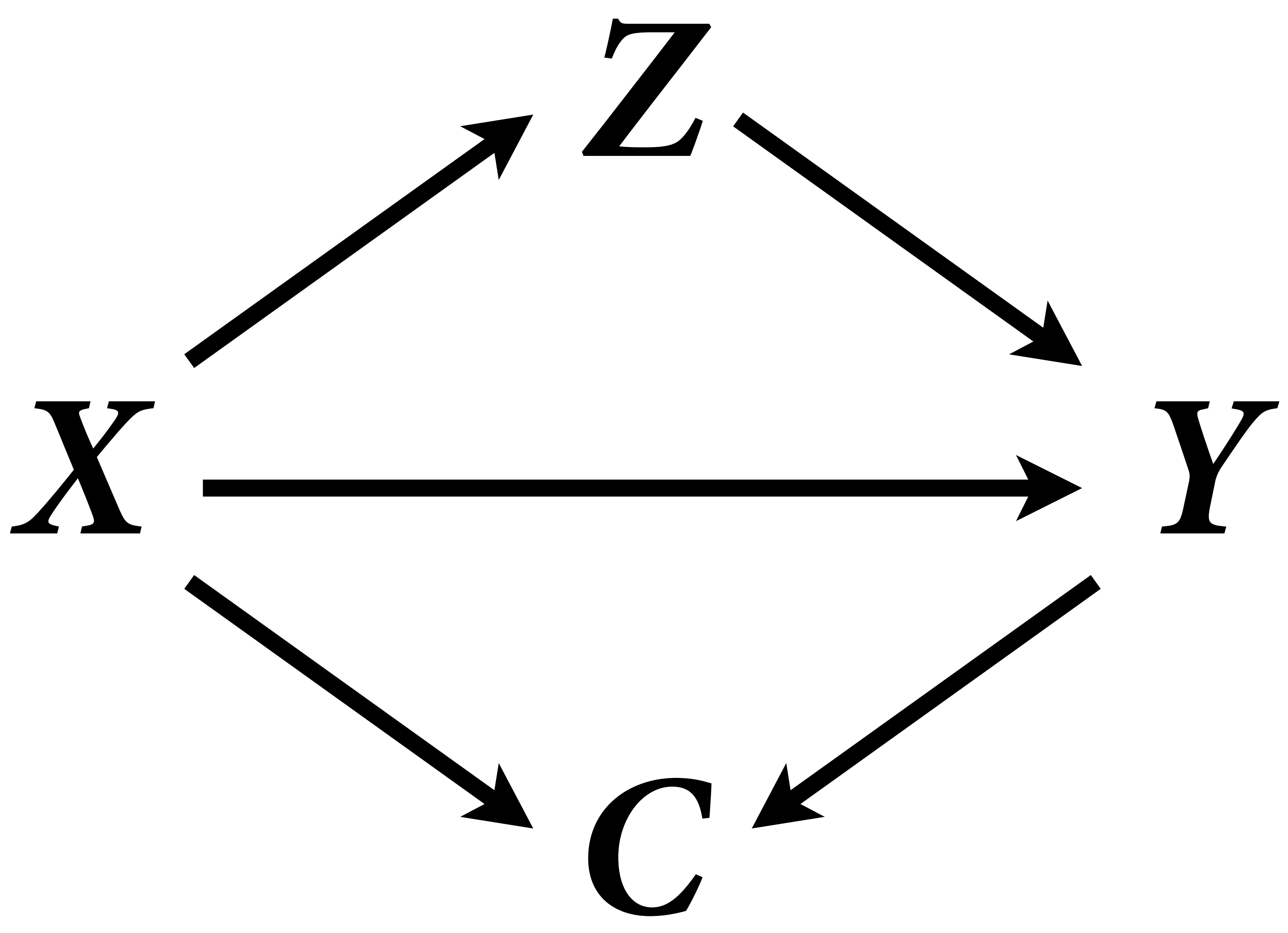
Block the pipe: $X \perp\!\!\!\perp U \mid Z$

$$P(Y \mid \mathrm{do}(X)) = \sum_{U} P(Y \mid X, Z) P(Z)$$

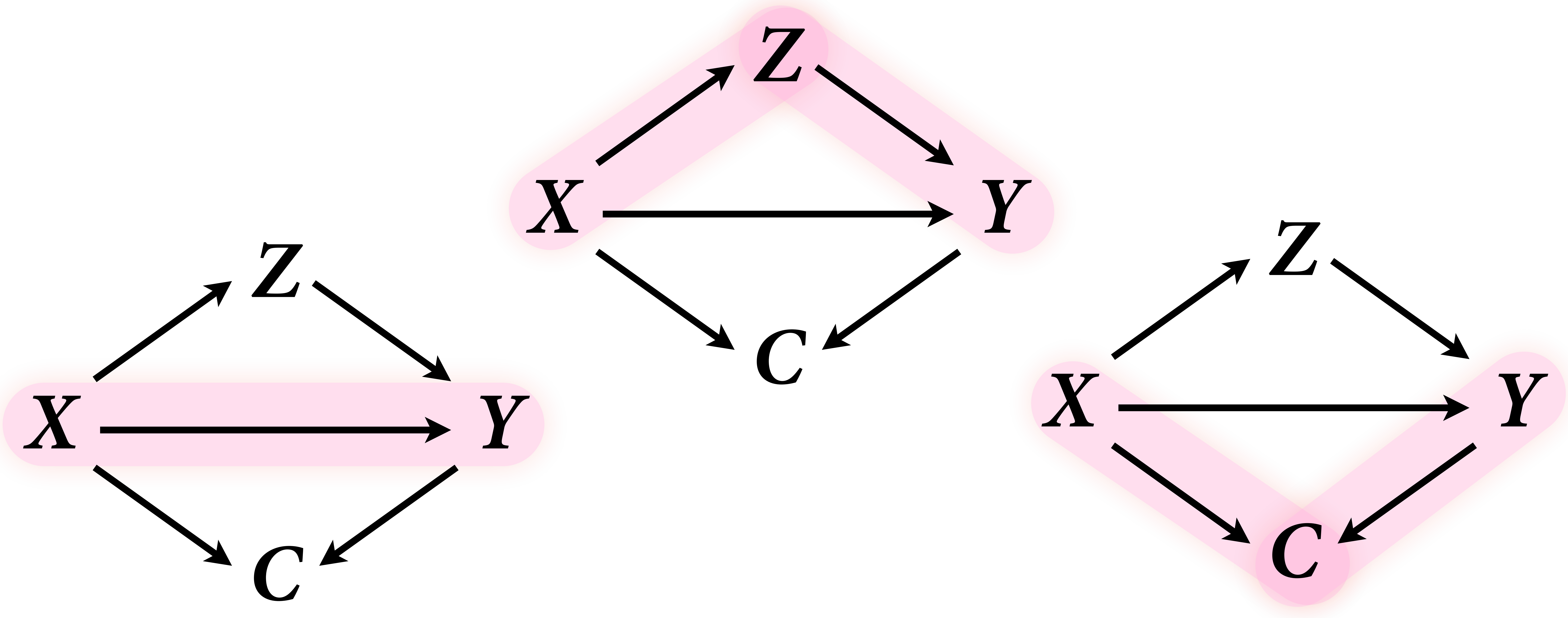
$$Y_i \sim \mathrm{Normal}(\mu_i, \sigma)$$
$$\mu_i = \alpha + \beta_X X_i + \beta_Z Z_i$$

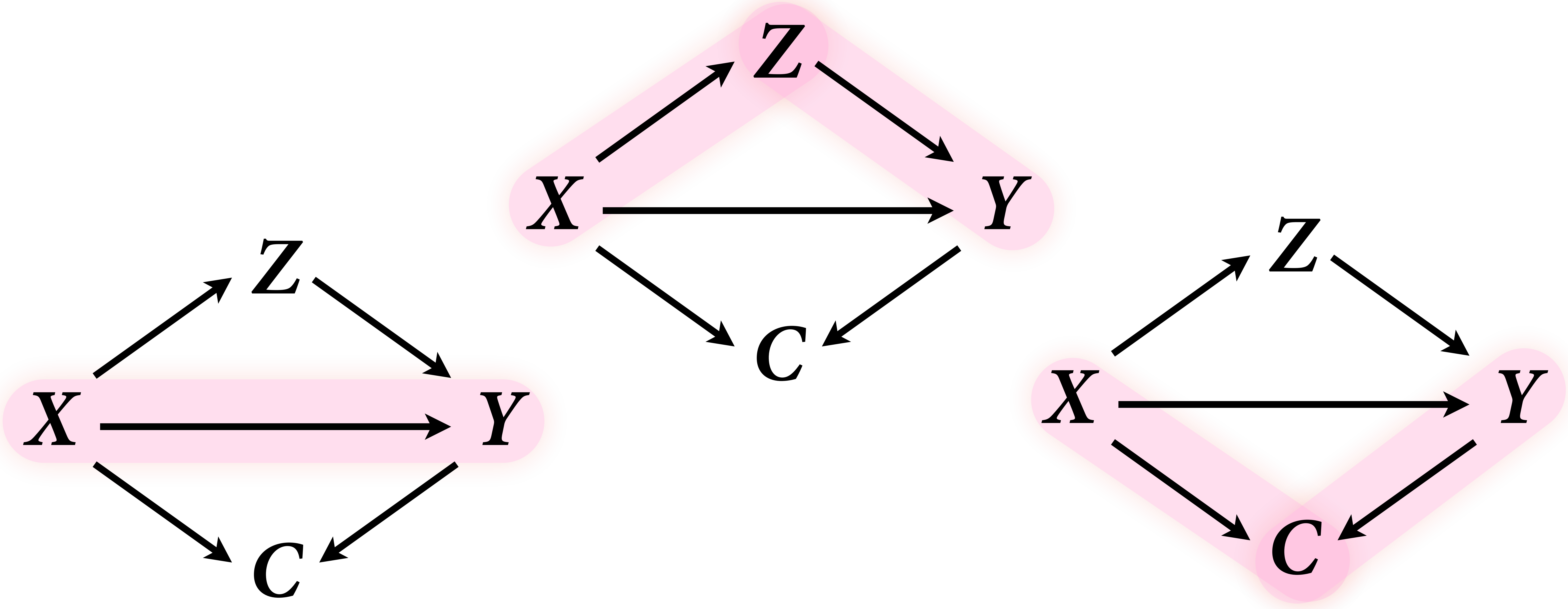


List all the paths connecting **X** and **Y**. Which need to be closed to estimate effect of **X** on **Y**?



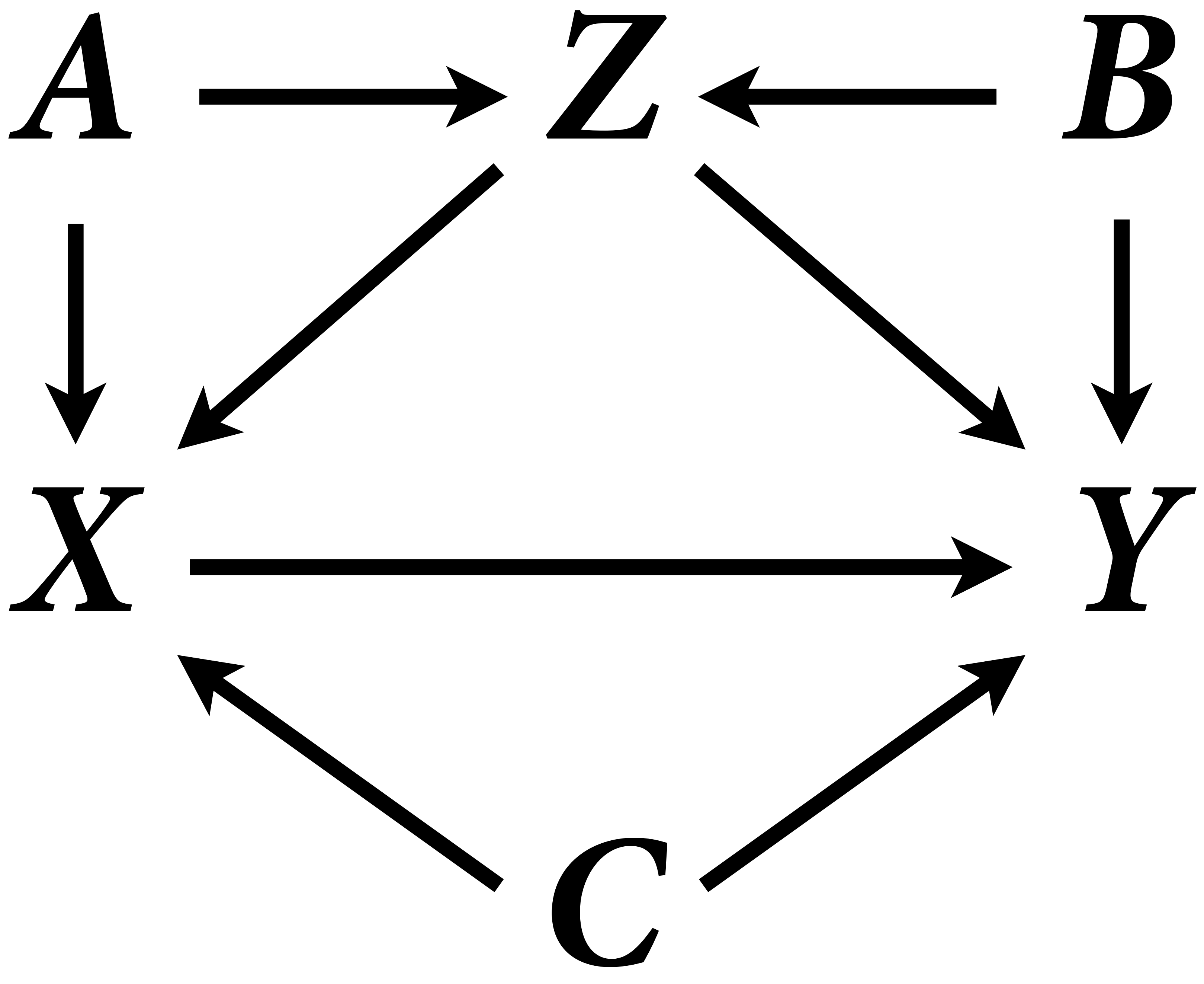
List all the paths connecting **X** and **Y**.
Which need to be closed to estimate effect of **X** on **Y**?

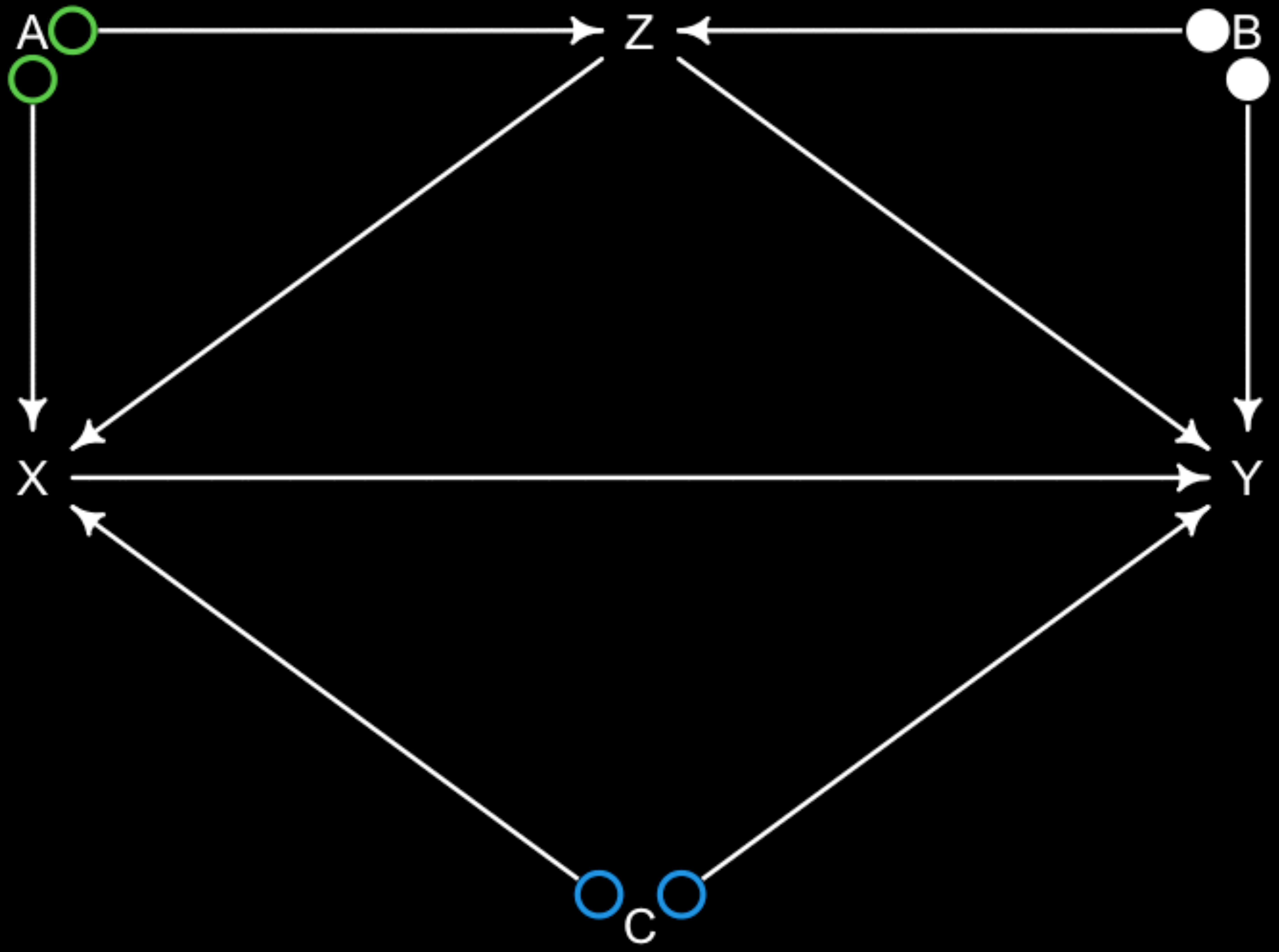




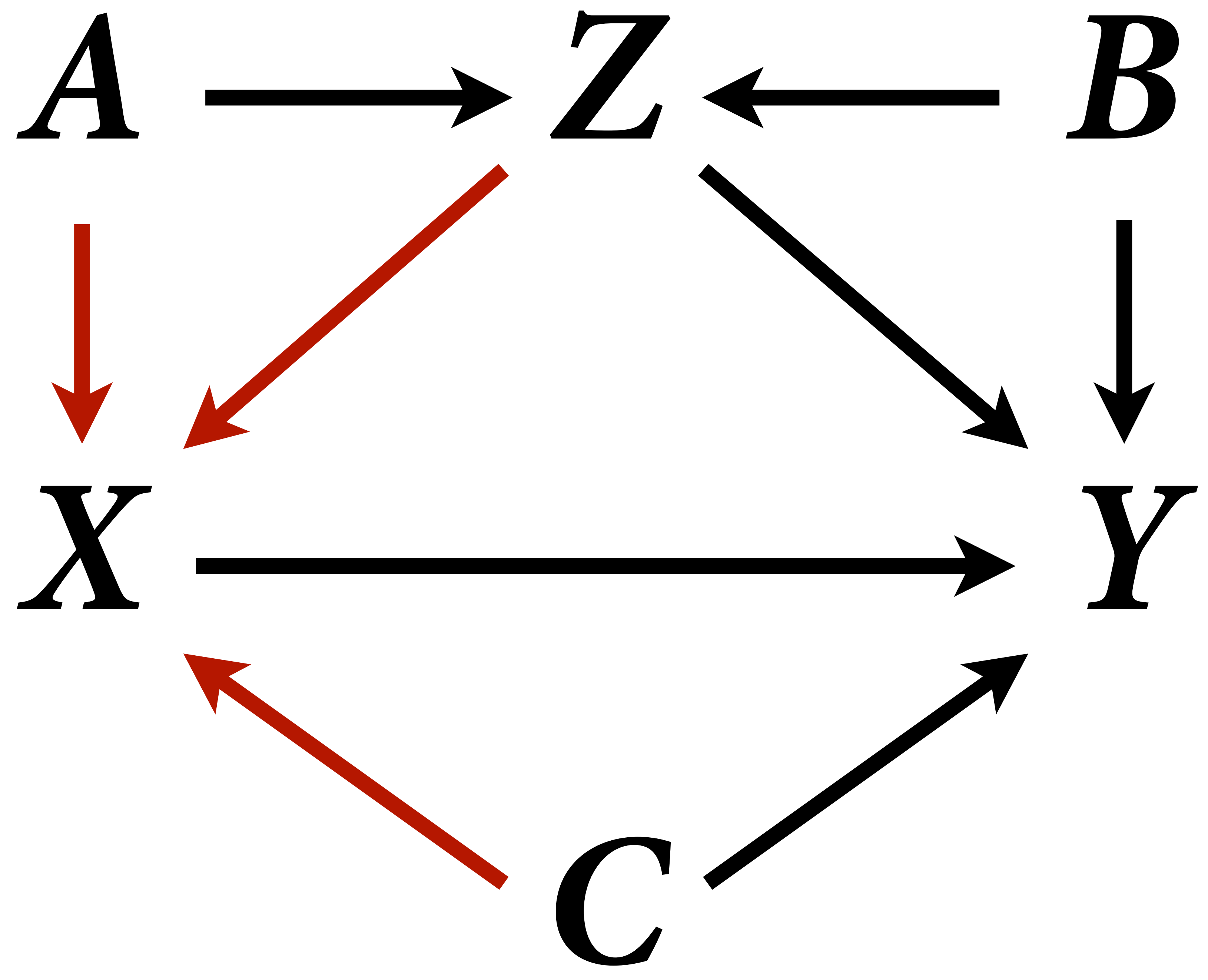
Adjustment set: *nothing!*

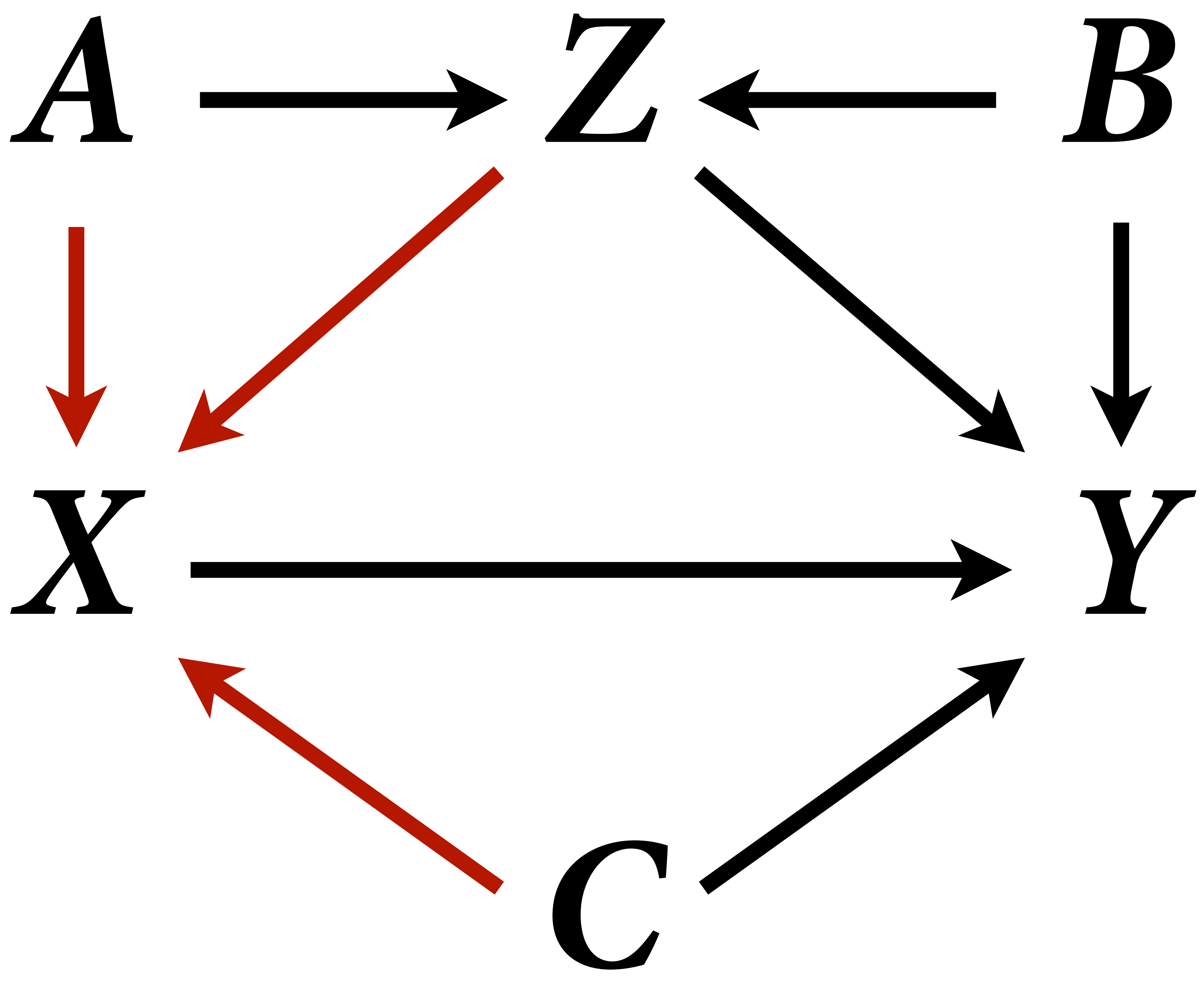
List all the paths connecting **X** and **Y**. Which need to be closed to estimate effect of **X** on **Y**?

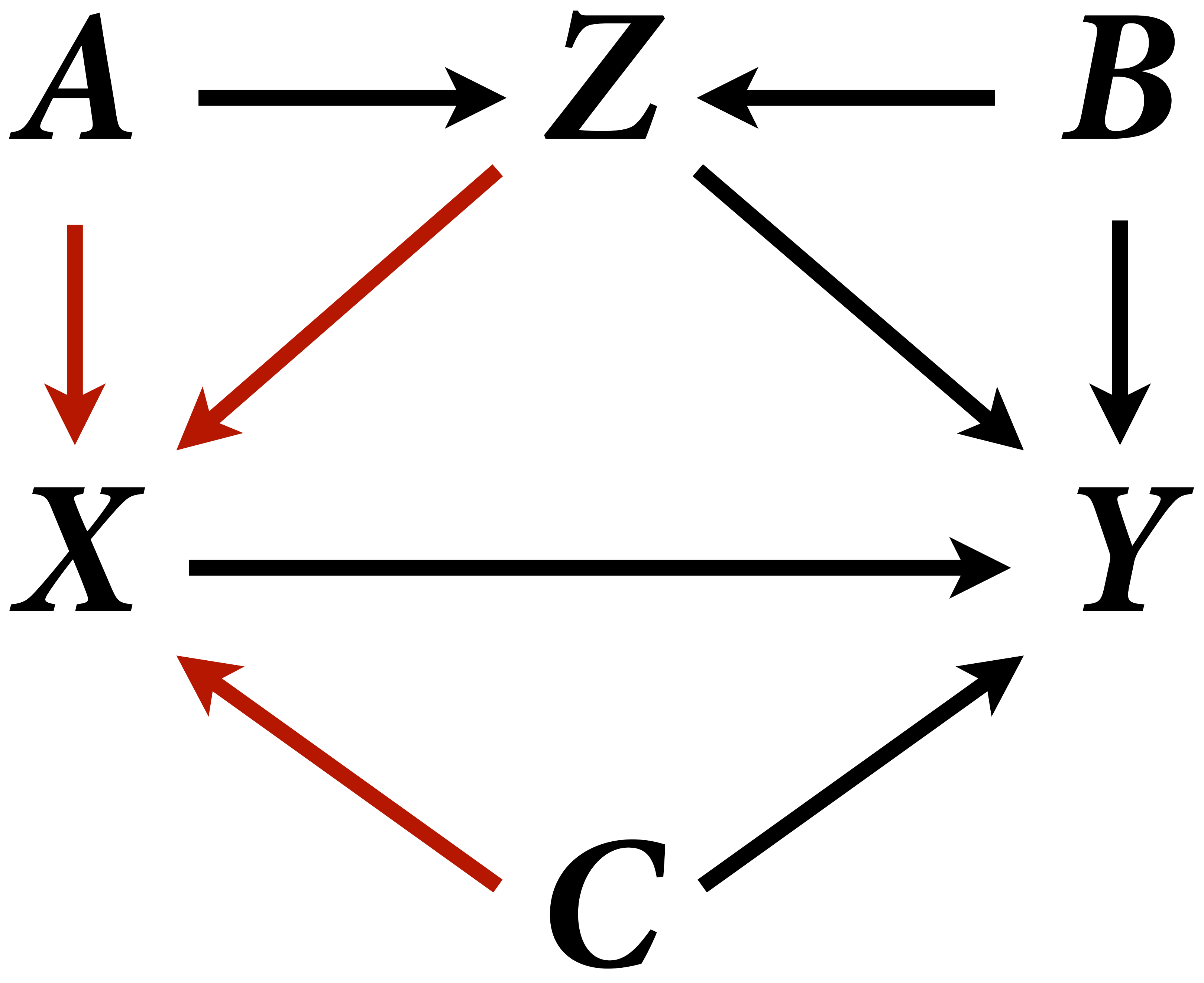



A
Z
B
X
Y
C

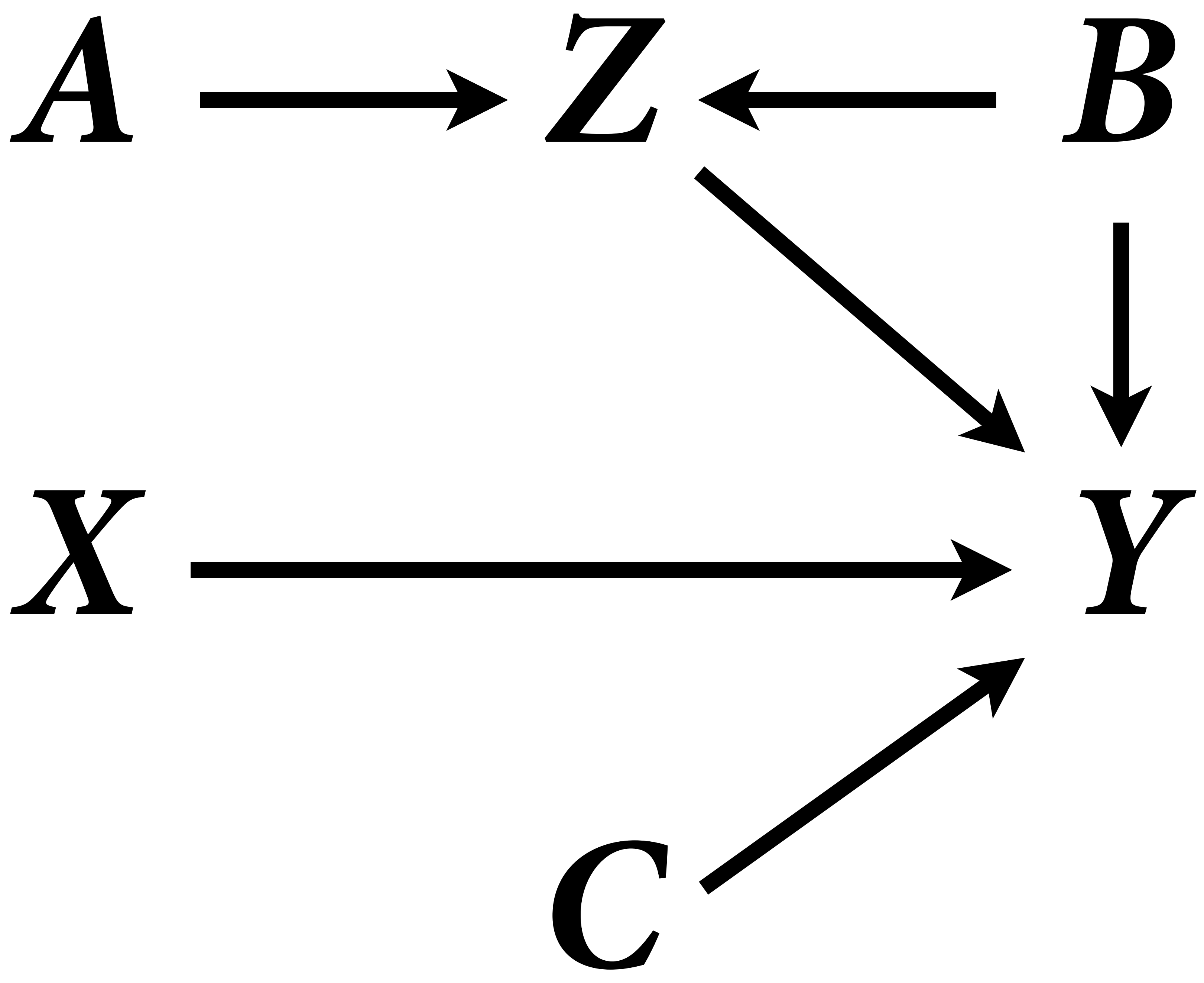
$$P(Y|\mathrm{do}(X))$$

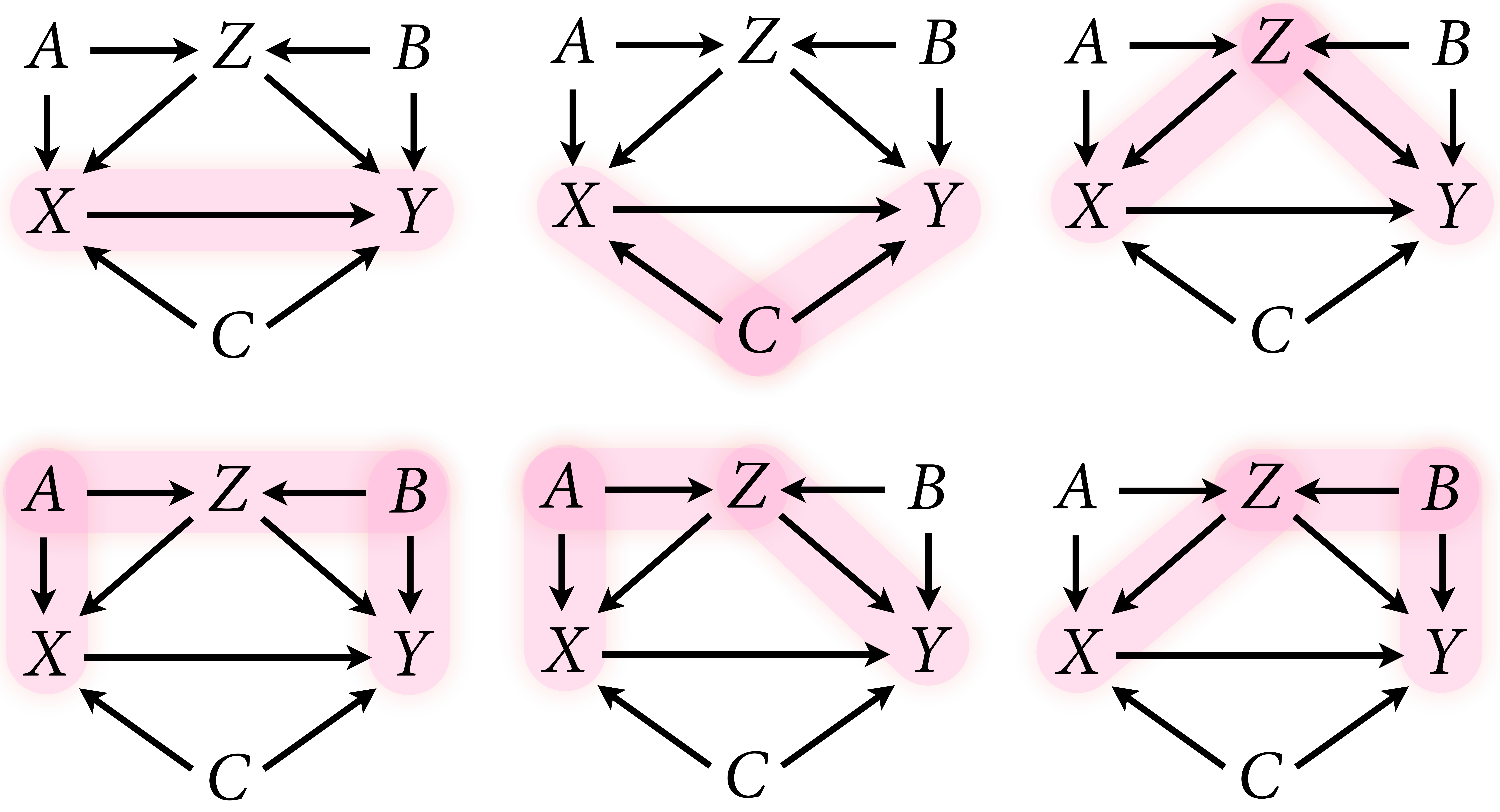


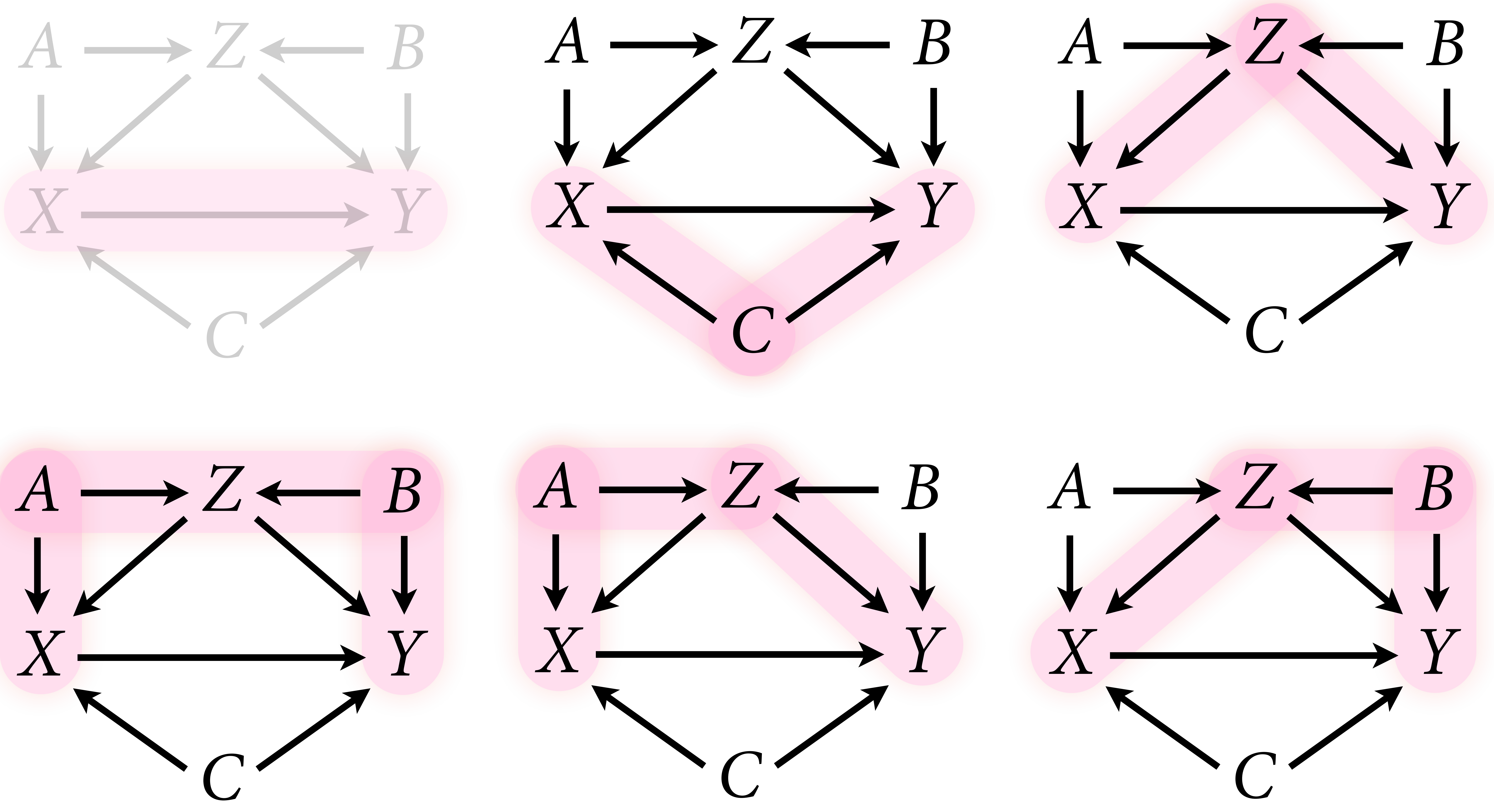


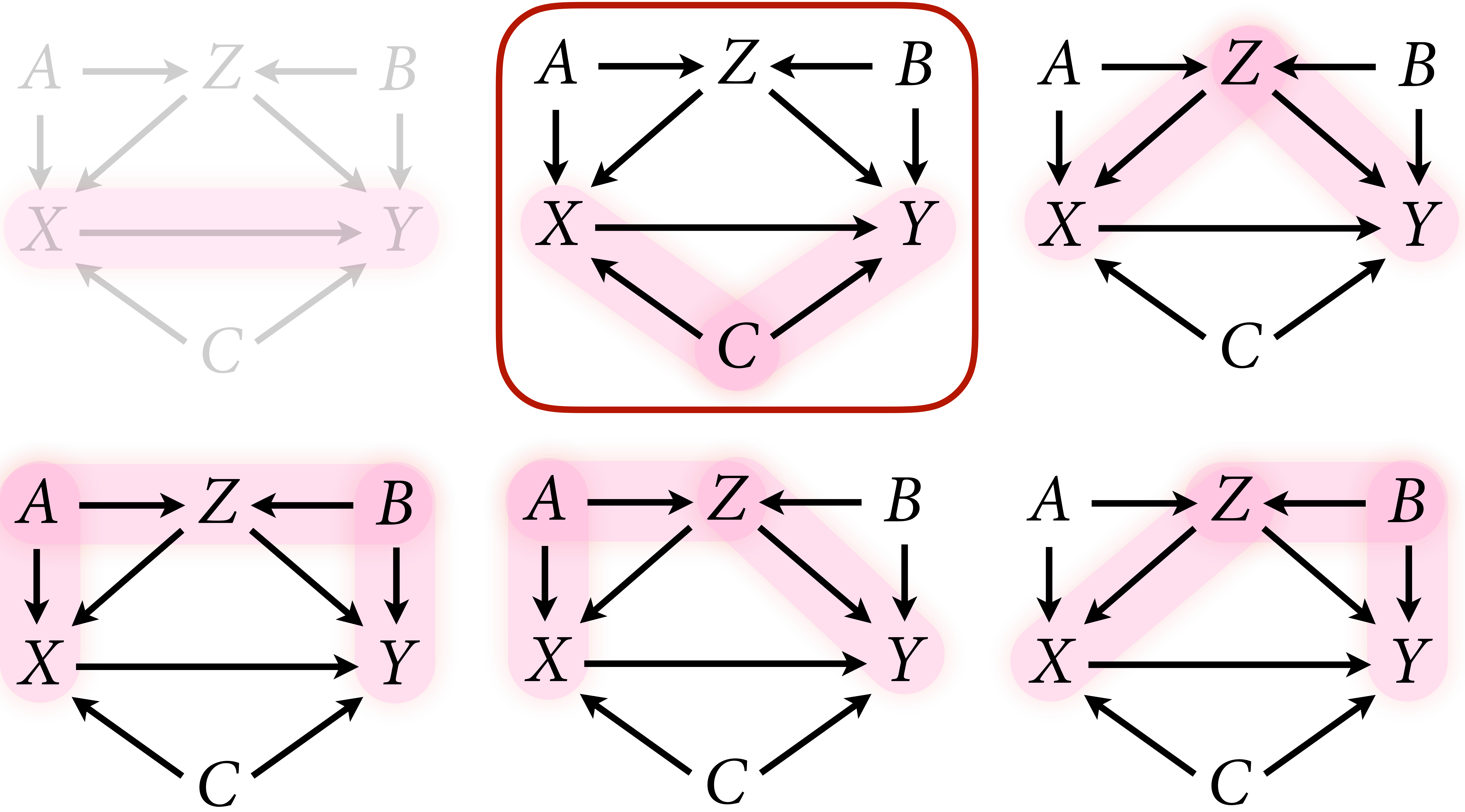


$$P(Y|\mathrm{do}(X))$$

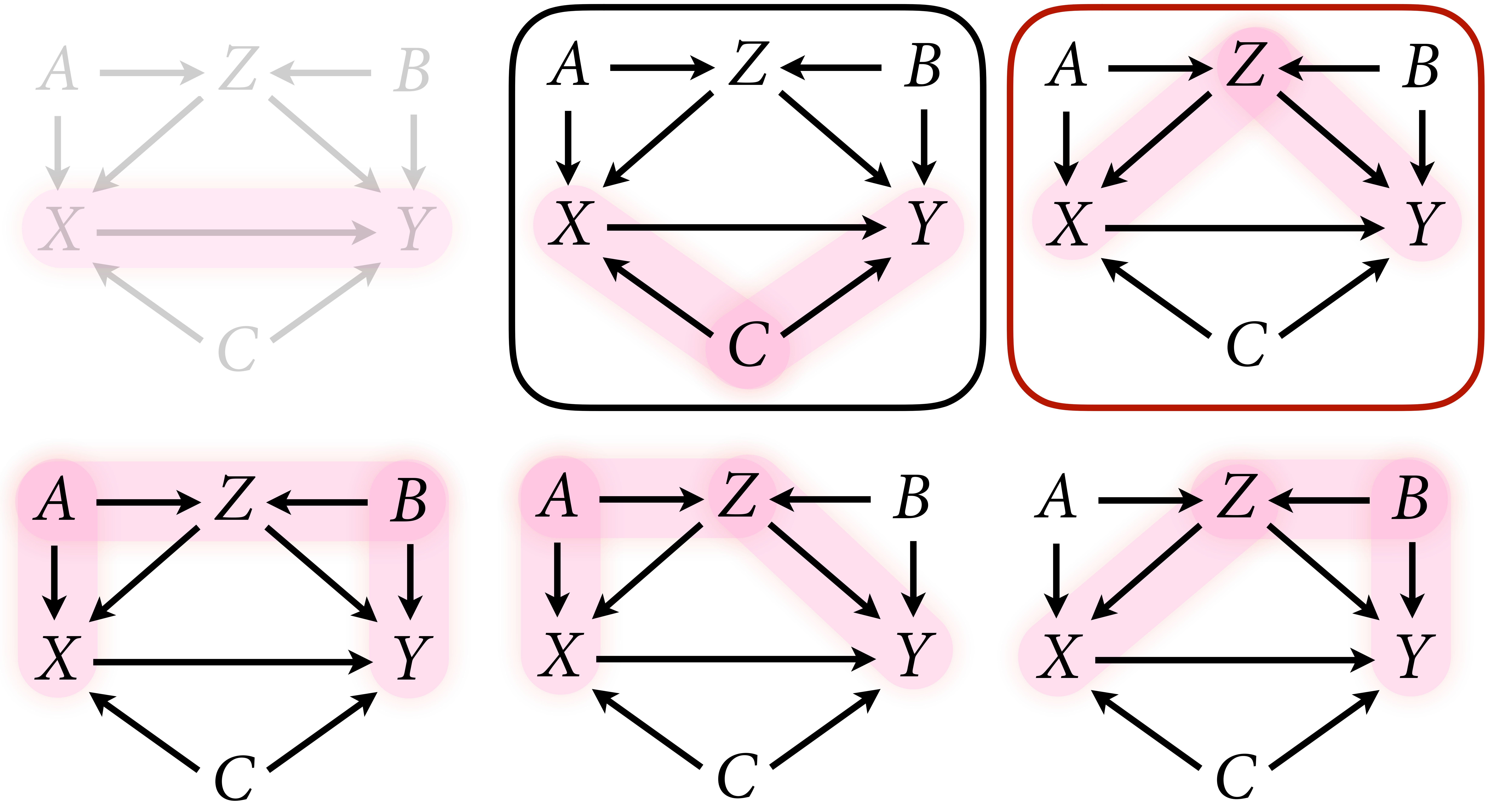


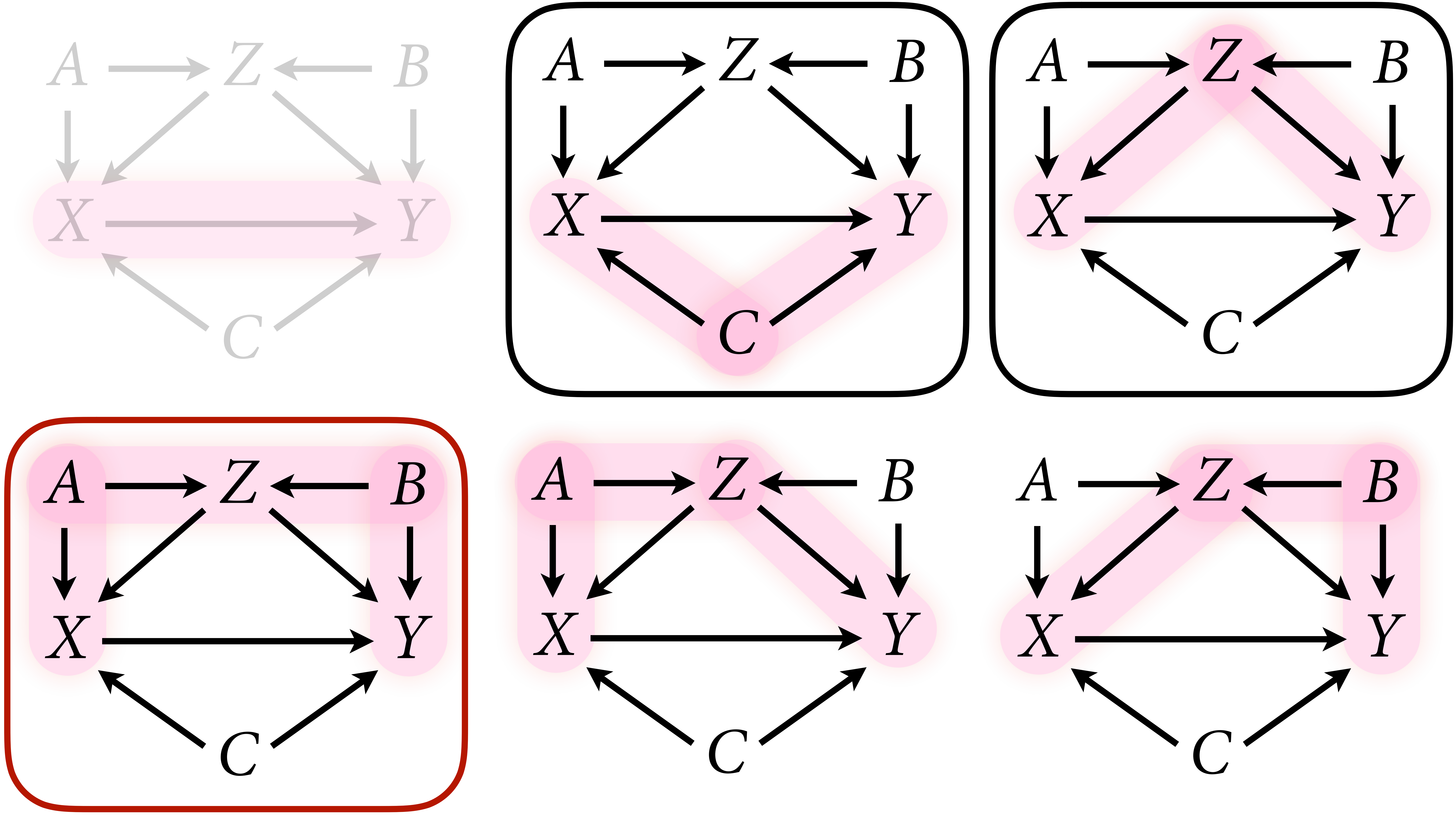

A
Z
B
X
Y
C

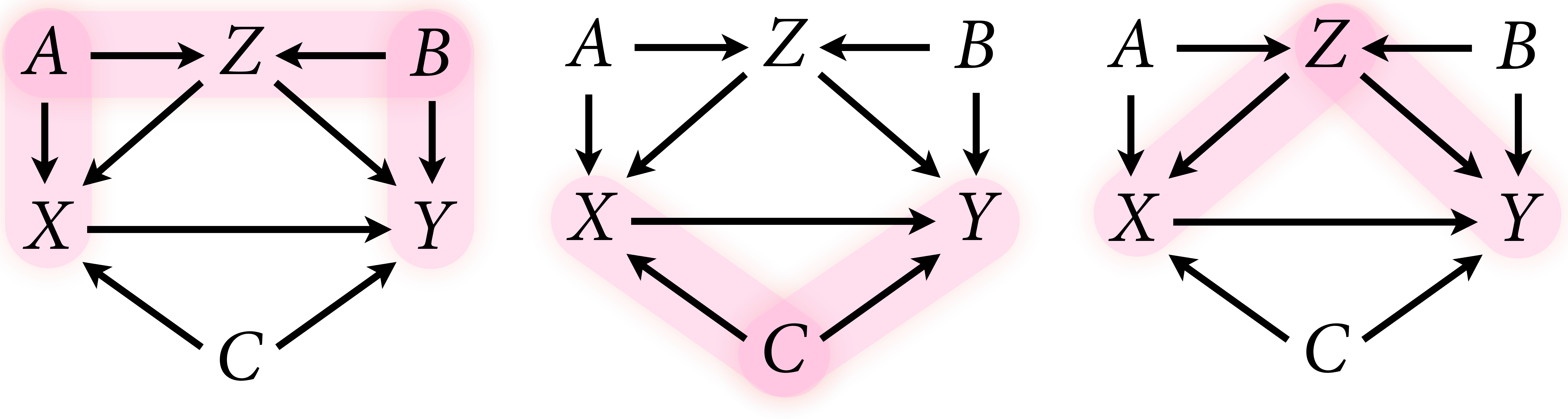

A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C



A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C


A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C
A
Z
B
X
Y
C


A
Z
B
X
Y
C

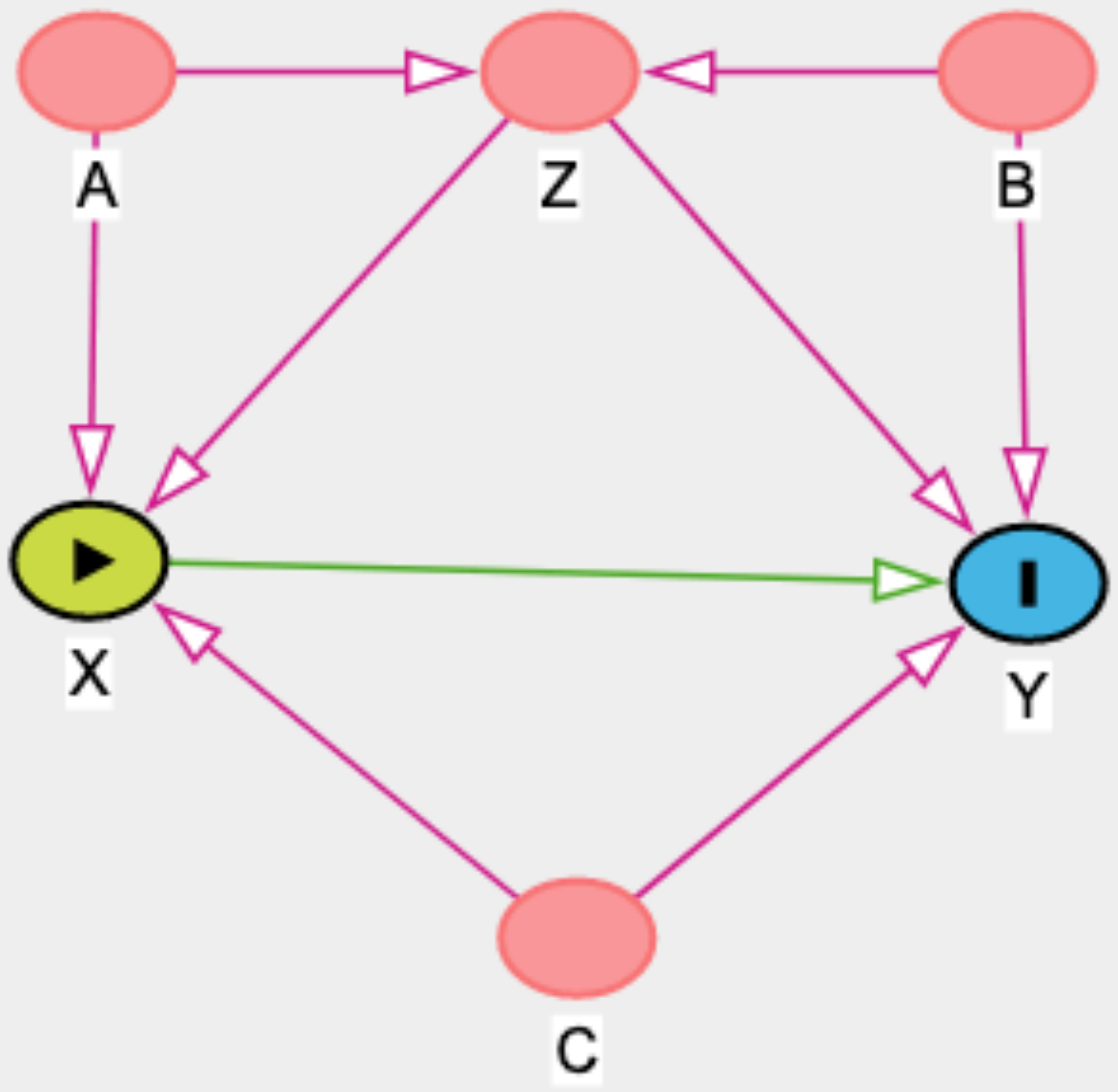


Adjustment set: C, Z, *and either* A *or* B

(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

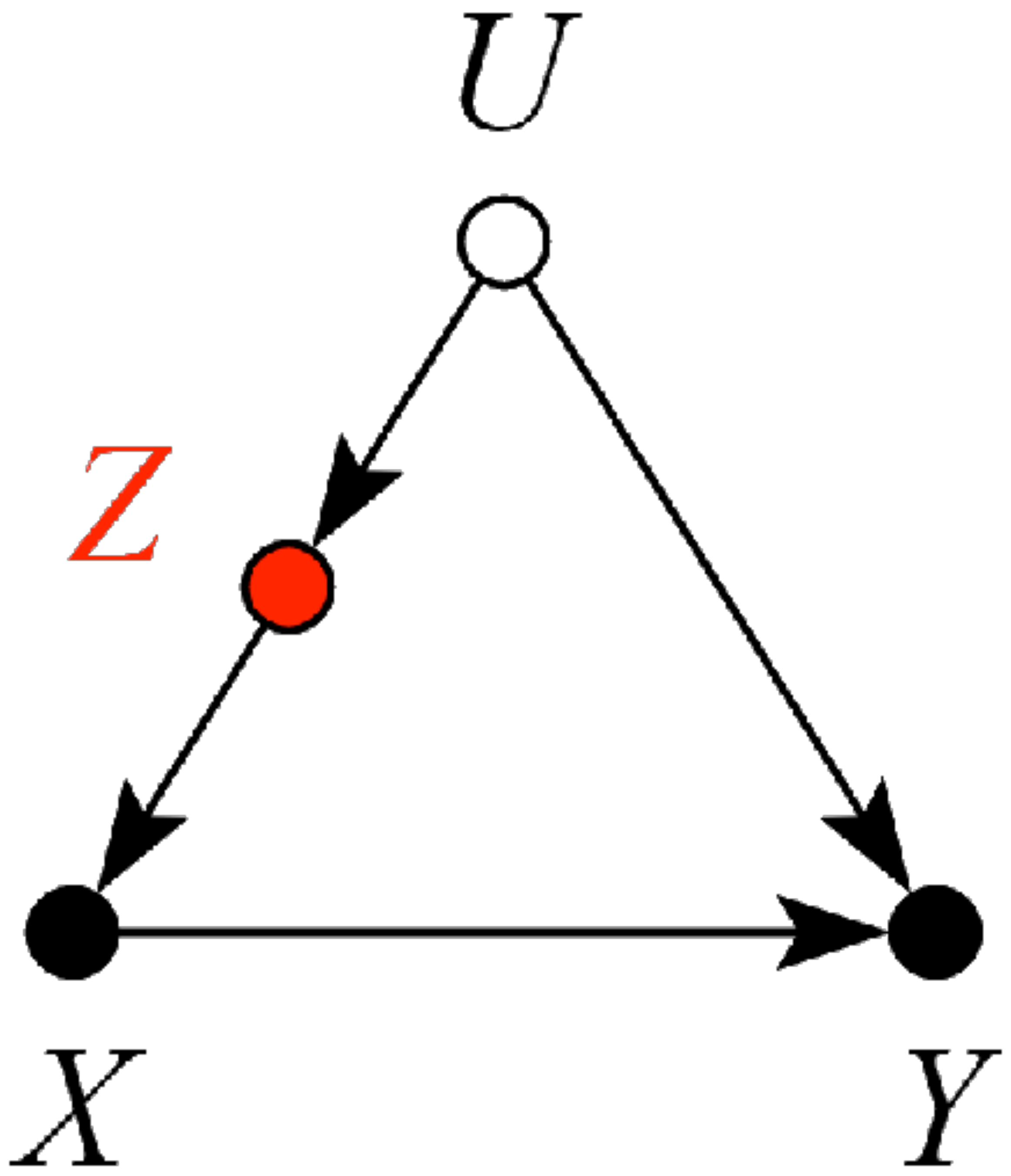
The model implies the following conditional independences:

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

Export R code

Backdoor Criterion

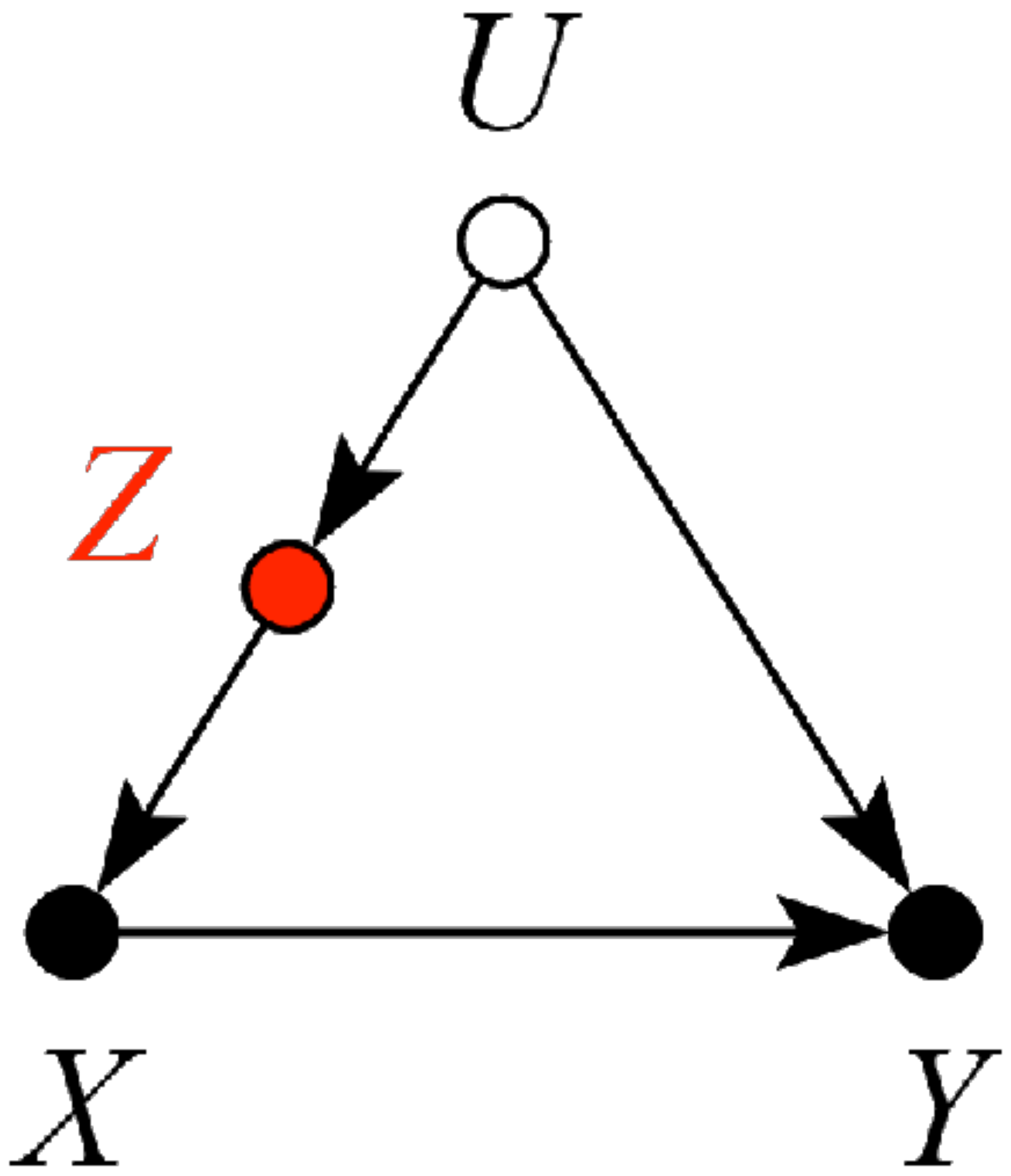
Backdoor Criterion: Rule to find adjustment set to yield $P(Y|\text{do}(X))$



Backdoor Criterion

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

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

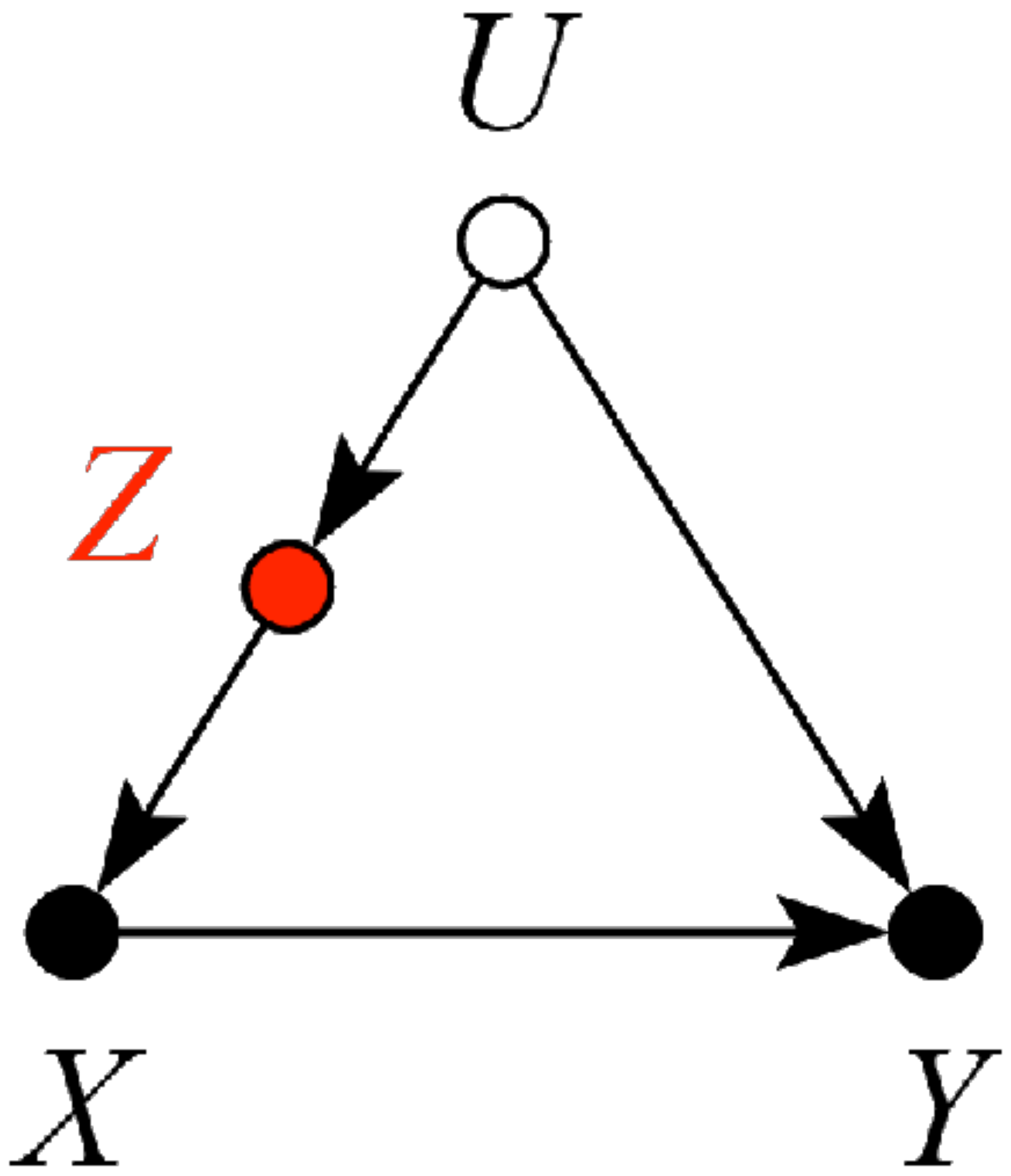


Backdoor Criterion

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:



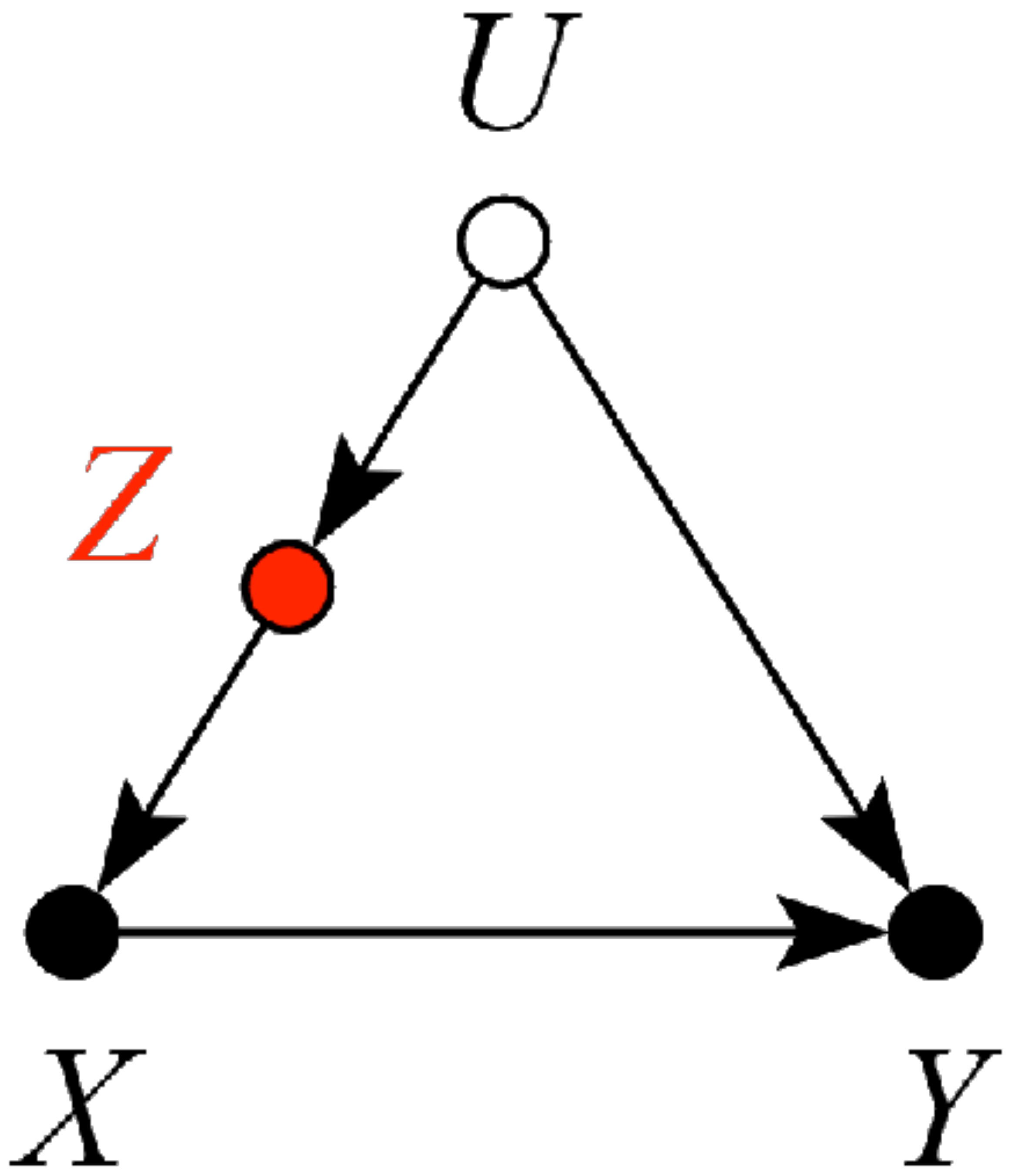
Backdoor Criterion

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

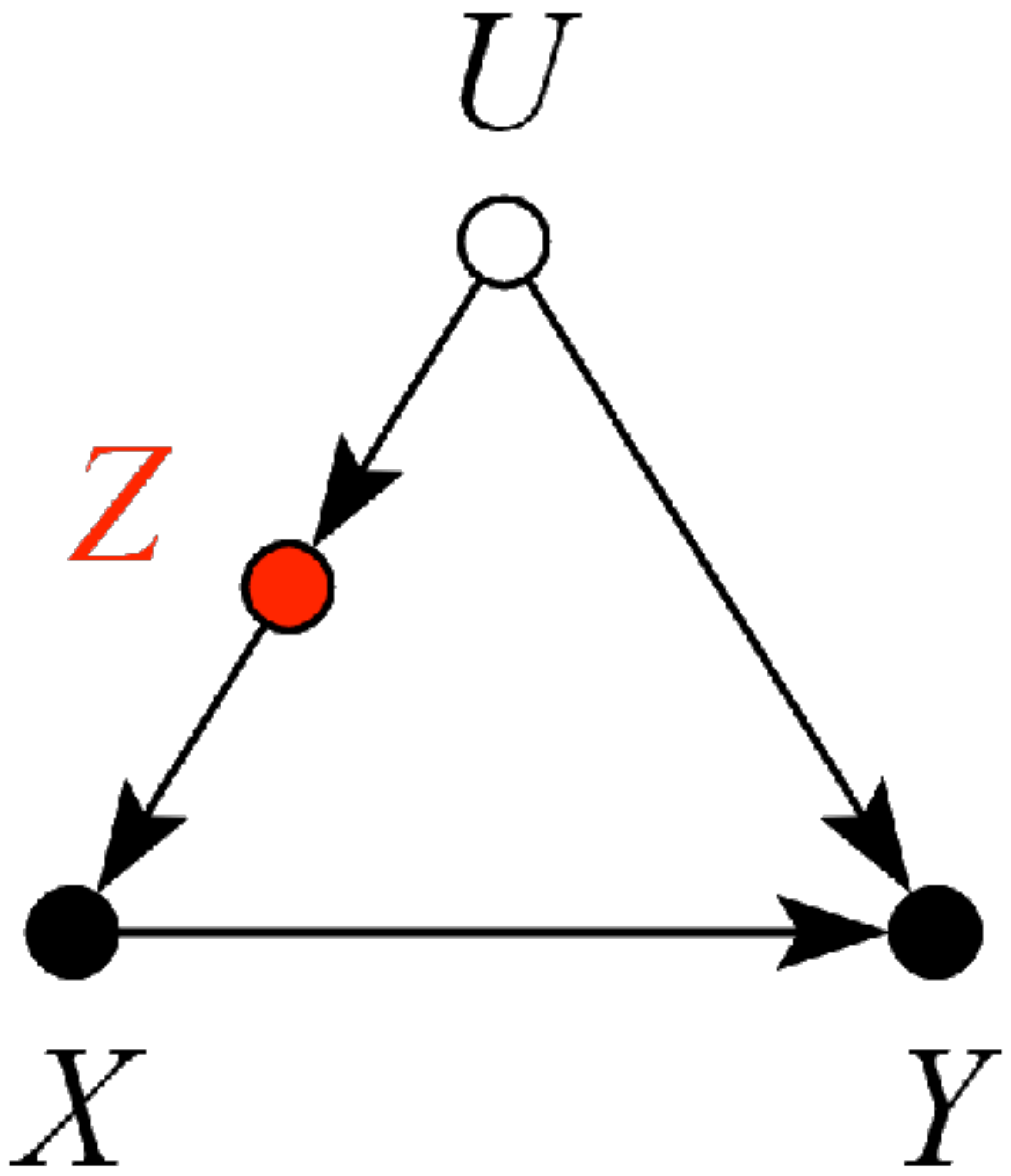
Backdoor Criterion: Rule to find adjustment set to yield $P(Y|\text{do}(X))$

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



PAUSE

http://www.blackswanman.com/

Good & Bad Controls

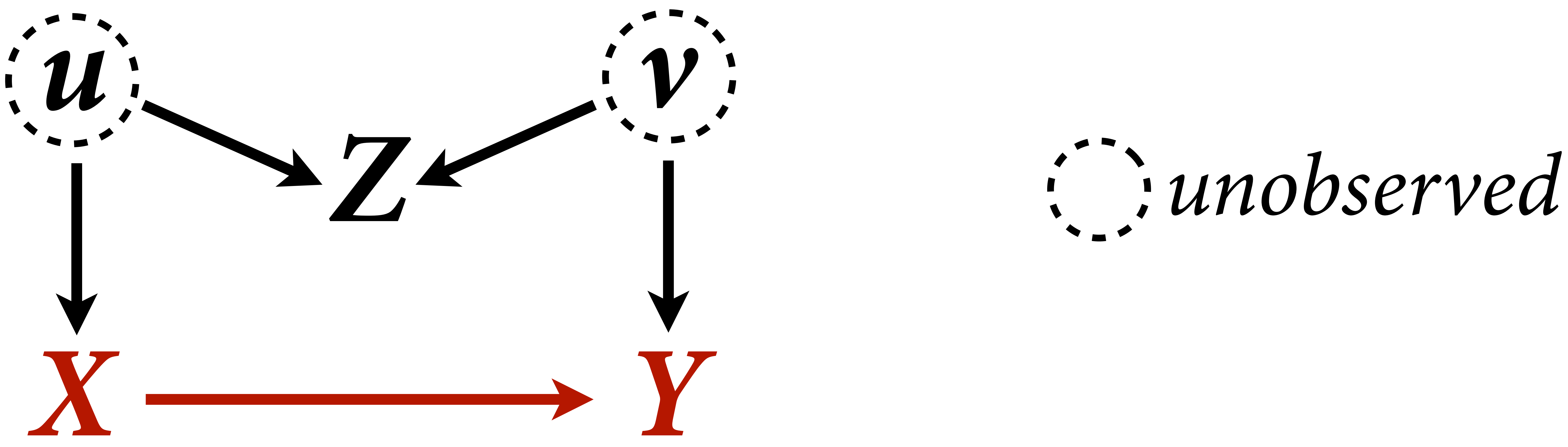
"Control" variable: Variable introduced to an analysis so that a causal estimate is possible

Common **wrong** heuristics for choosing control variables

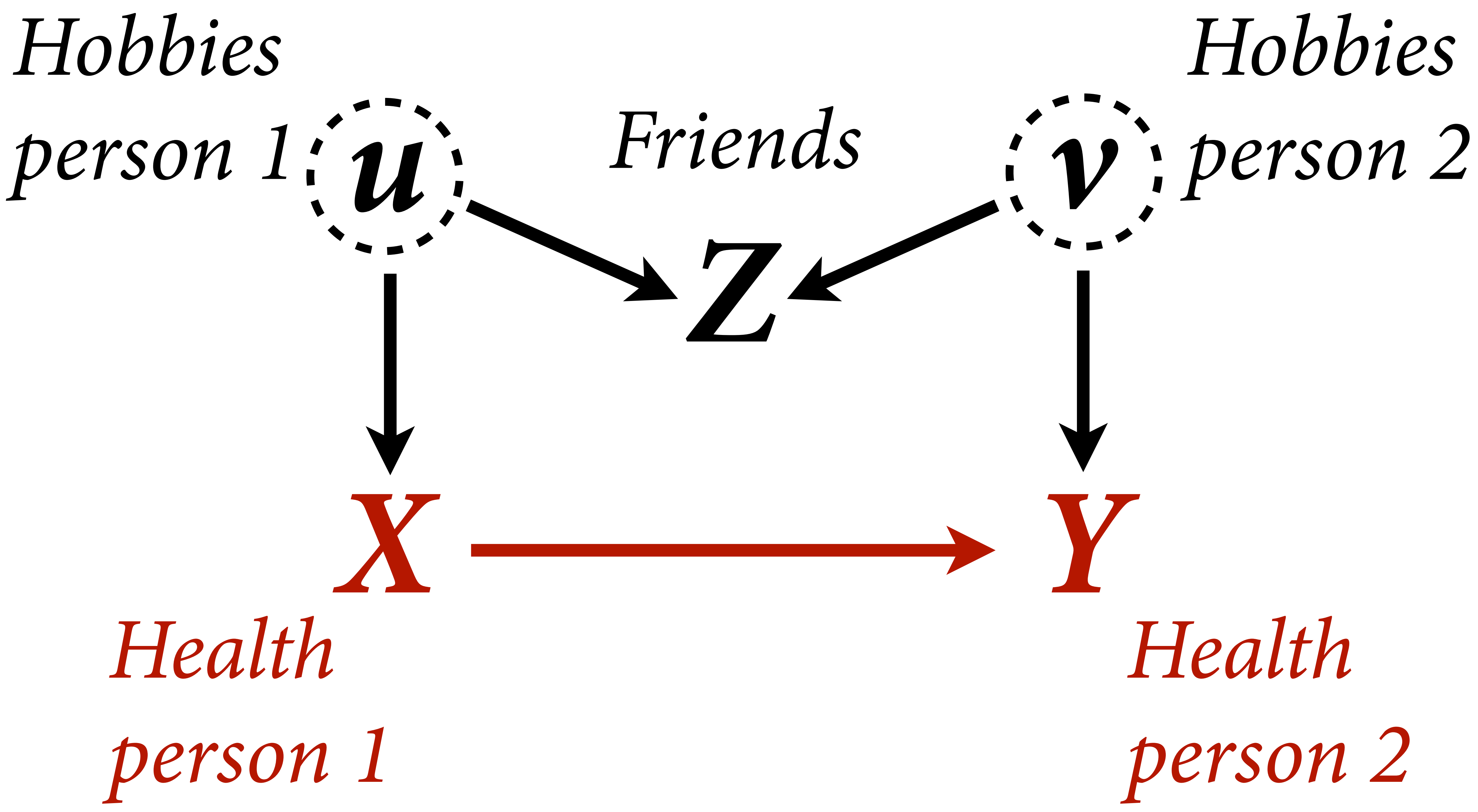
- Anything in the spreadsheet **YOLO**!
- Any variables not highly **collinear**
- Any **pre-treatment** measurement (baseline)

$$X \longrightarrow Y$$

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

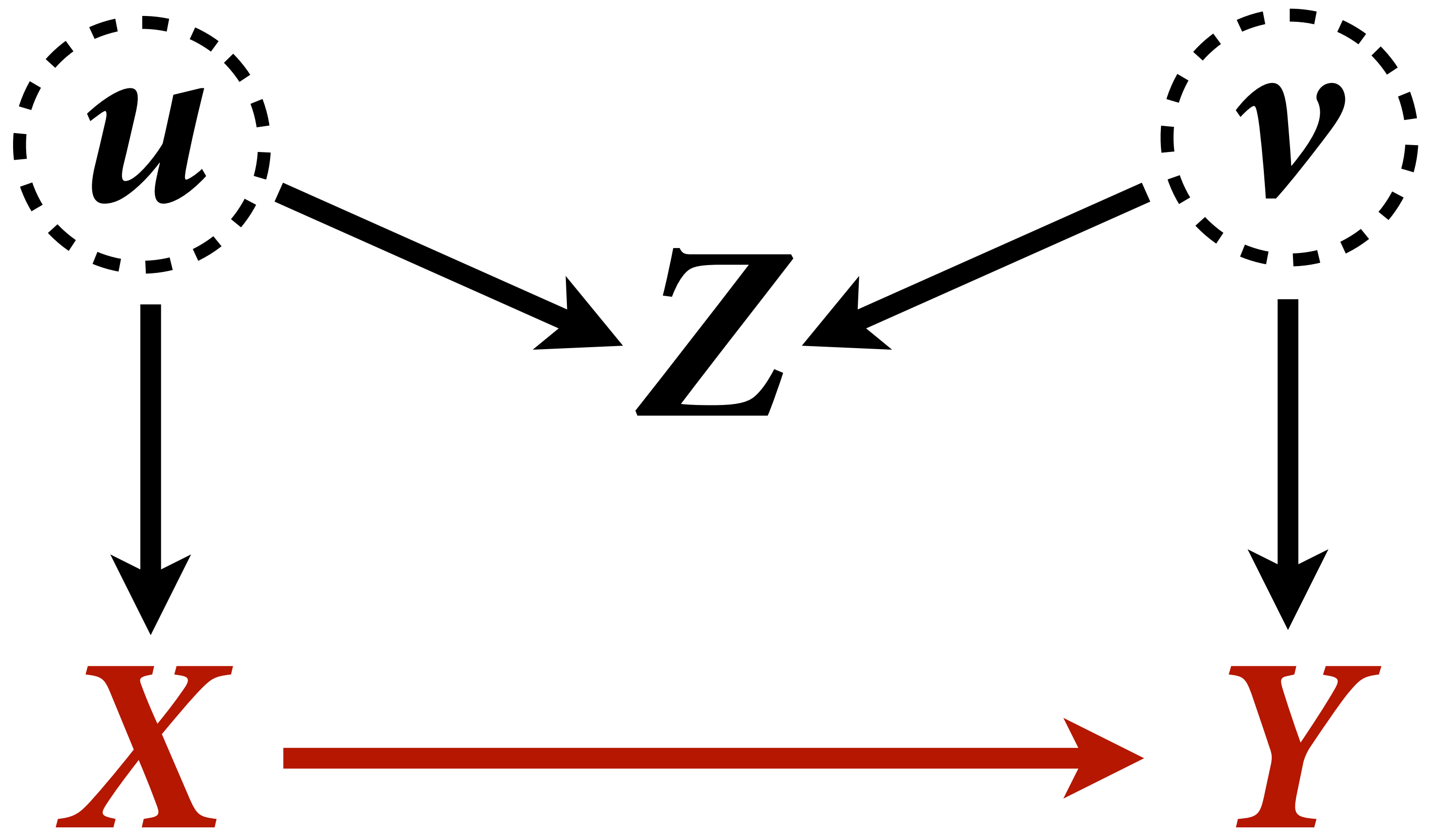


Cinelli, Forney, Pearl 2021 A Crash Course in Good and Bad Controls



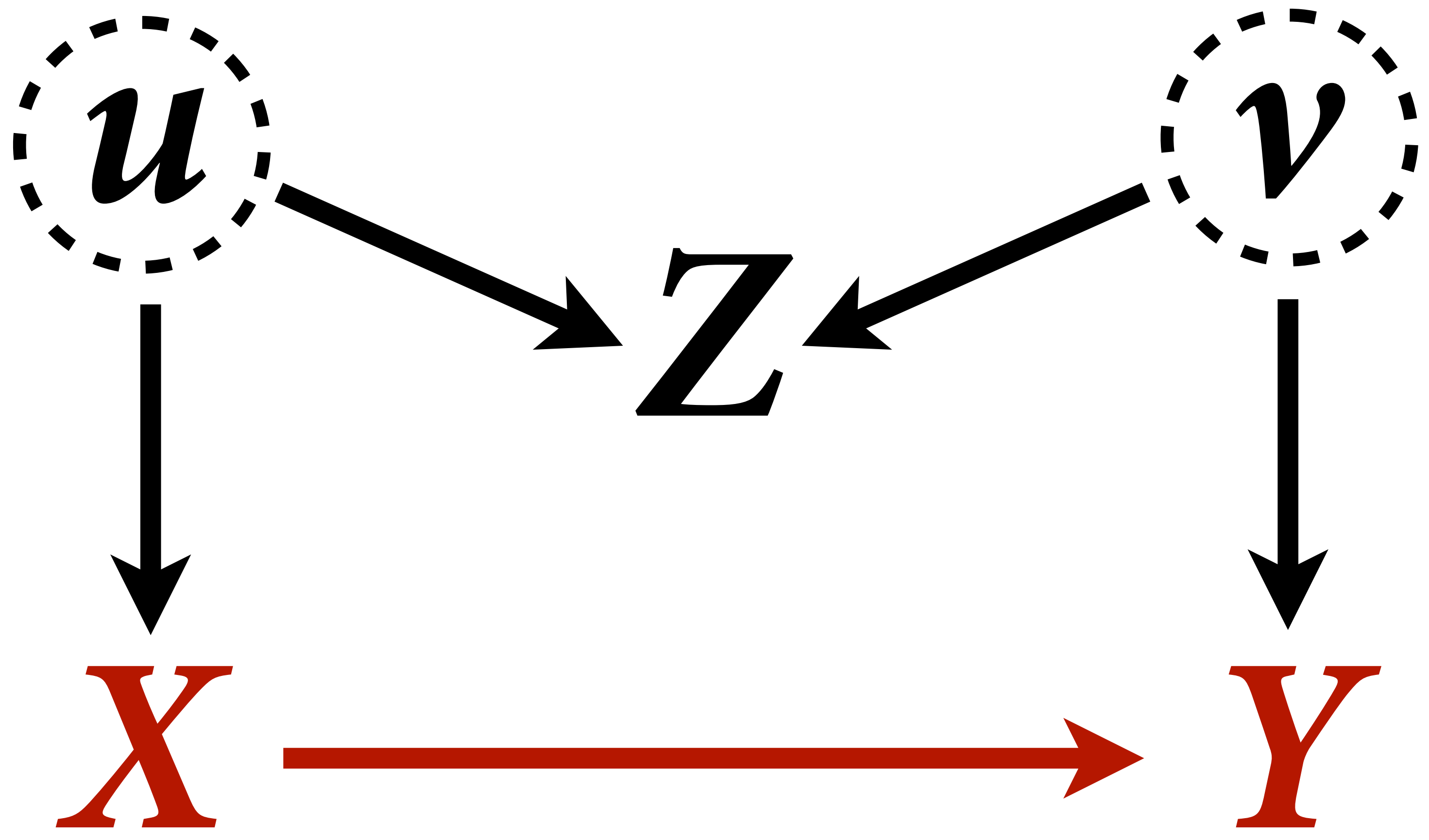
Cinelli, Forney, Pearl 2021 A Crash Course in Good and Bad Controls

(1) List the paths



(1) List the paths

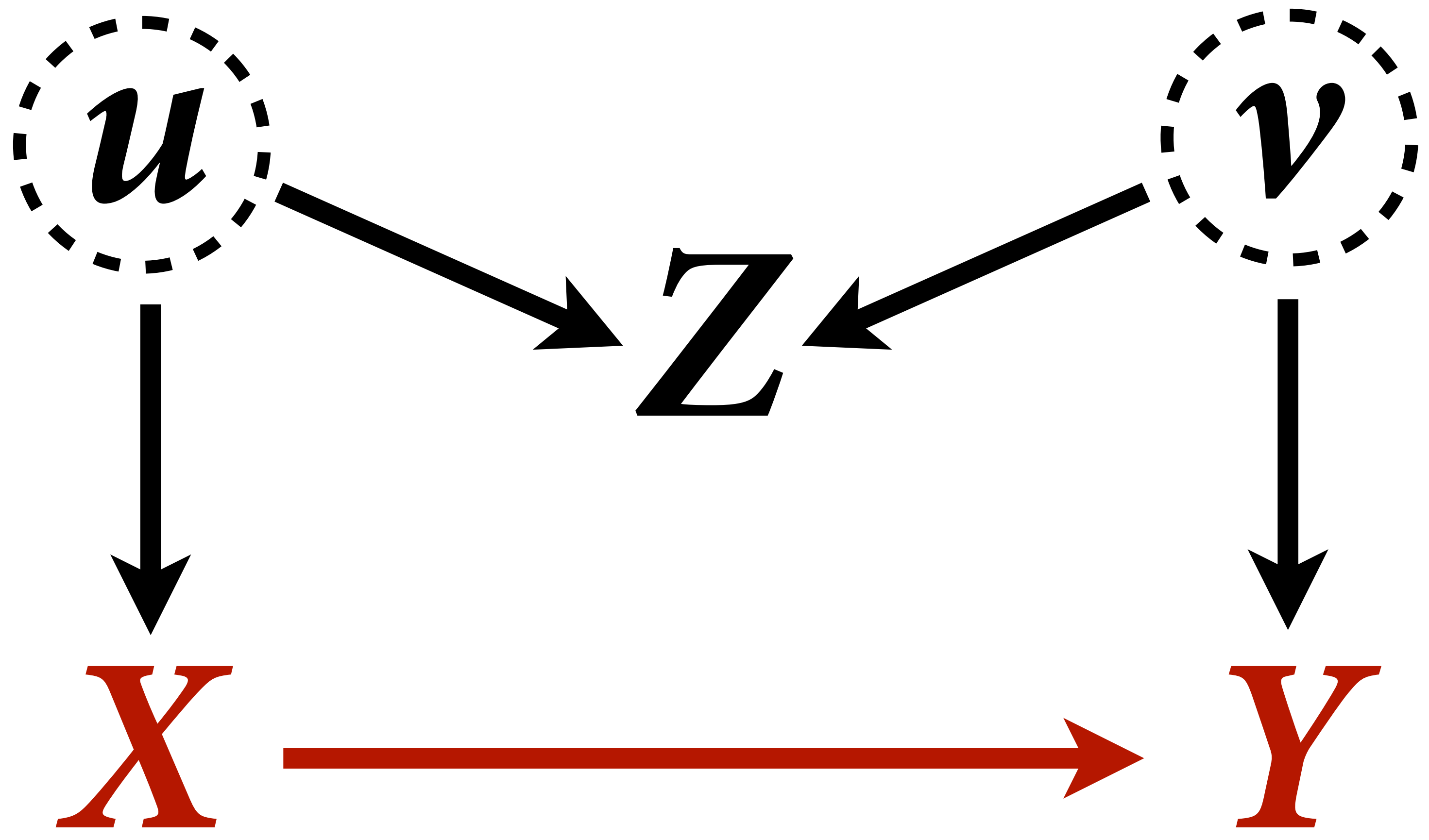
$X \rightarrow Y$



(1) List the paths

$$X \rightarrow Y$$

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



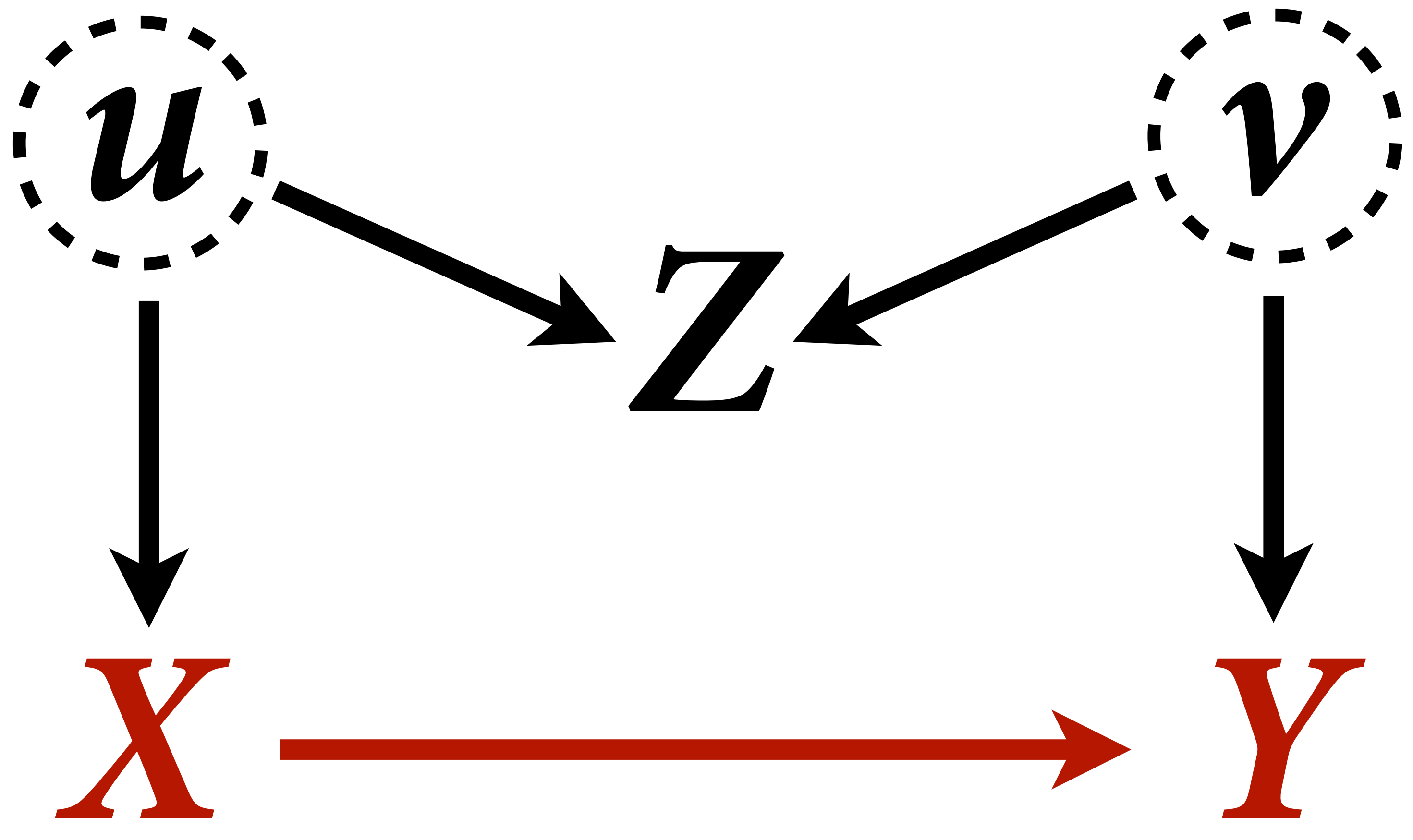
(1) List the paths (2) Find backdoors

$$X \rightarrow Y$$

frontdoor & open

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

backdoor & closed



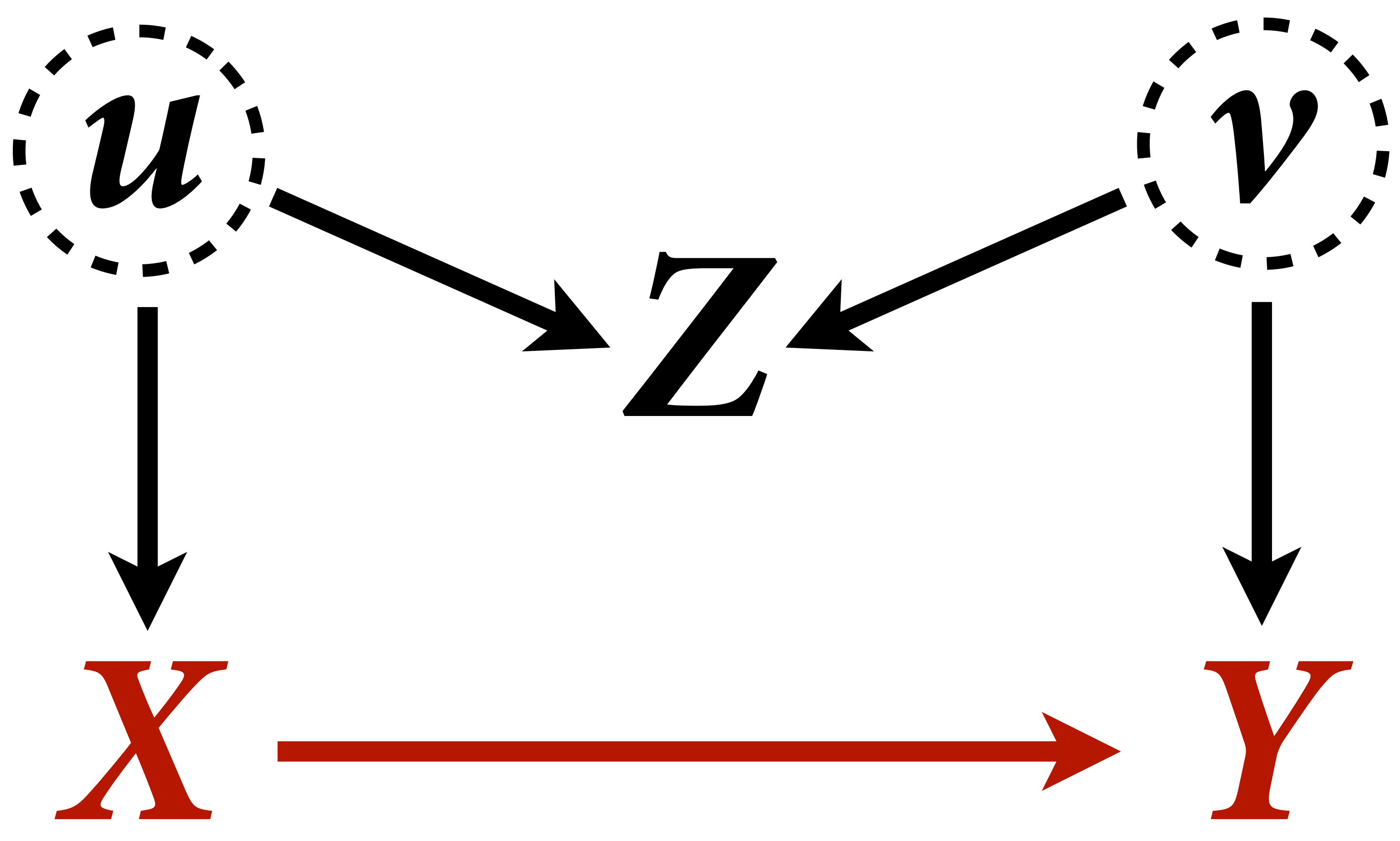
(1) List the paths (2) Find backdoors

$X \rightarrow Y$

frontdoor & open

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

backdoor & closed



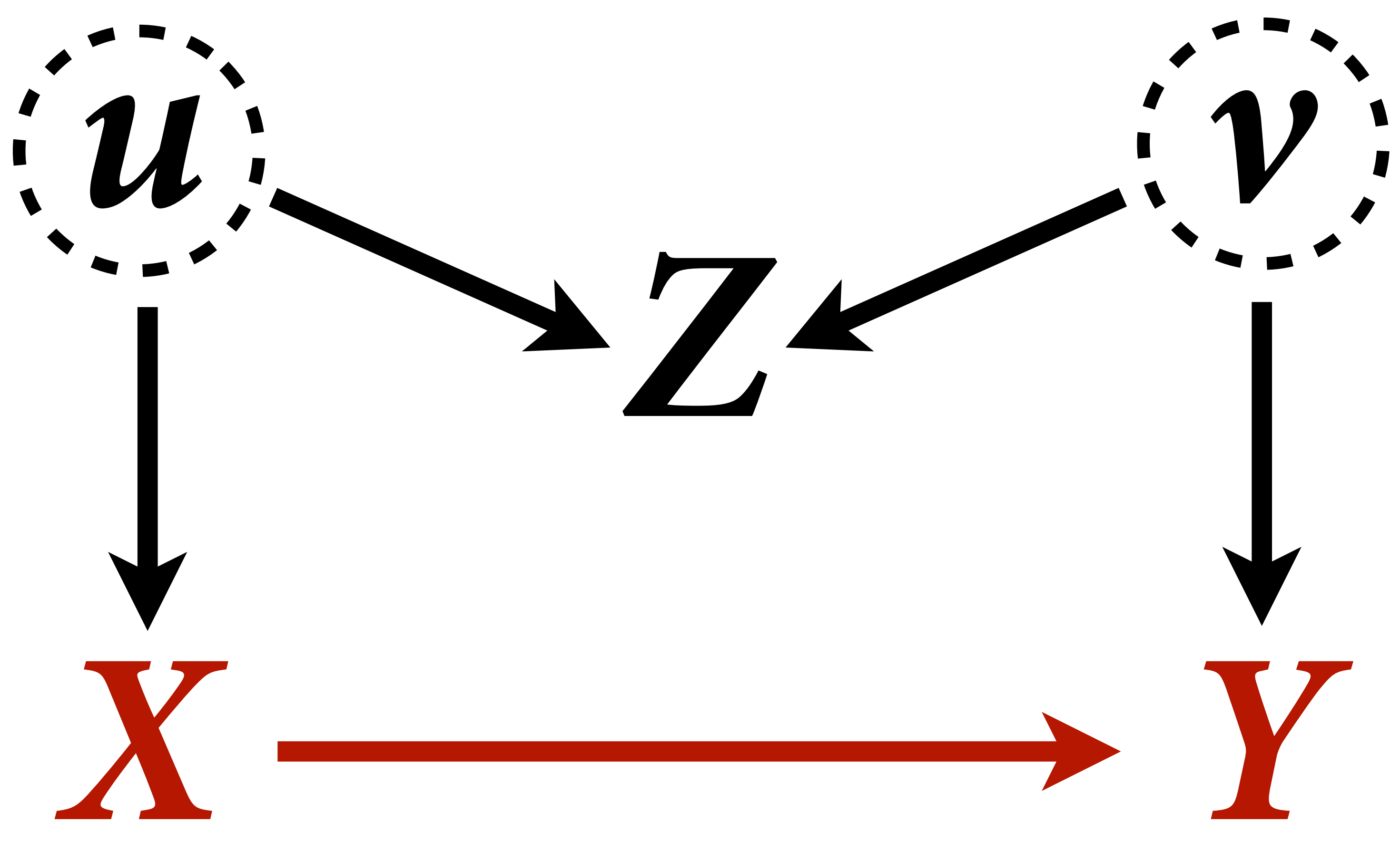
(1) List the paths (2) Find backdoors (3) Close backdoors

$$X \rightarrow Y$$

frontdoor & open

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

backdoor & closed

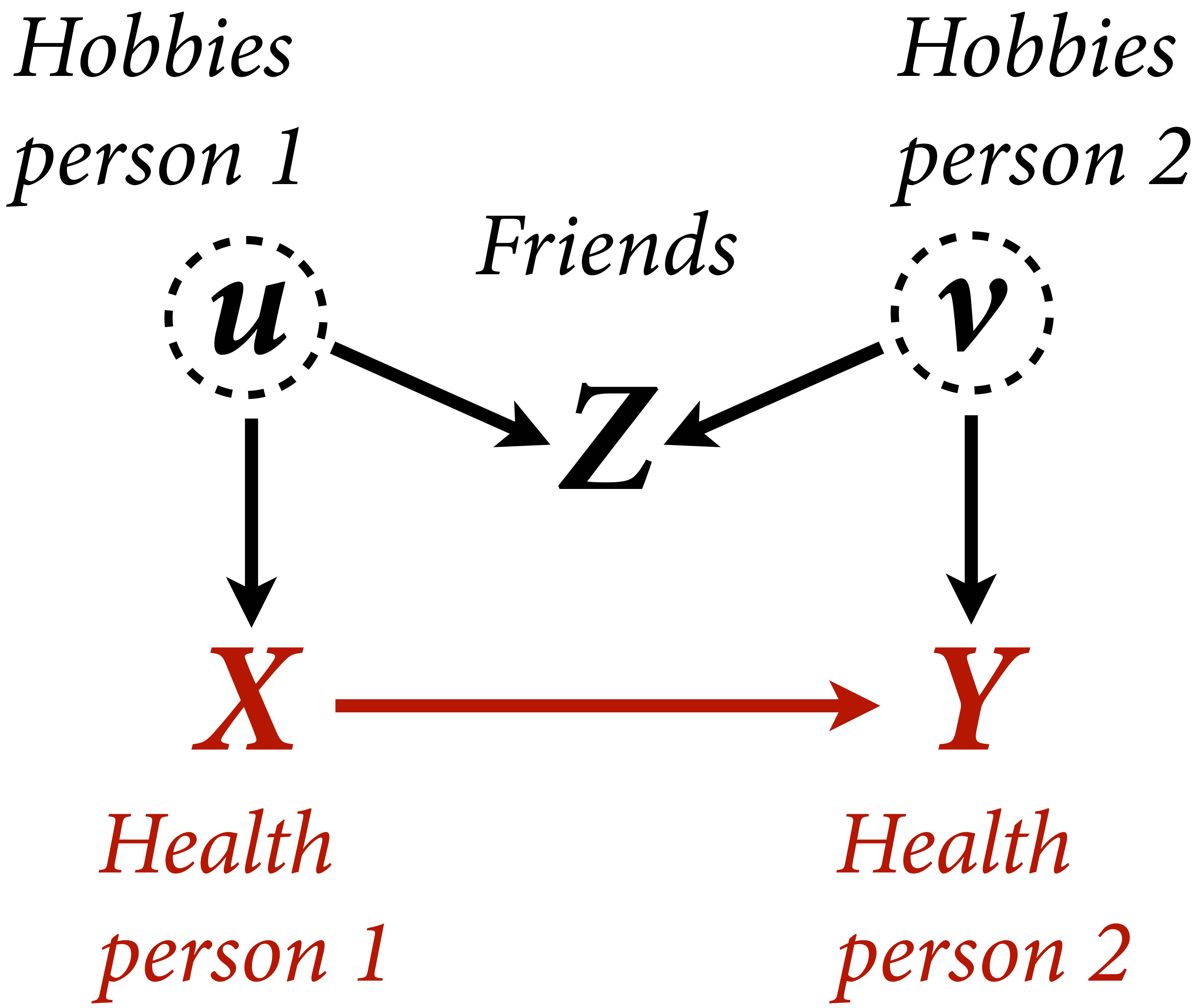


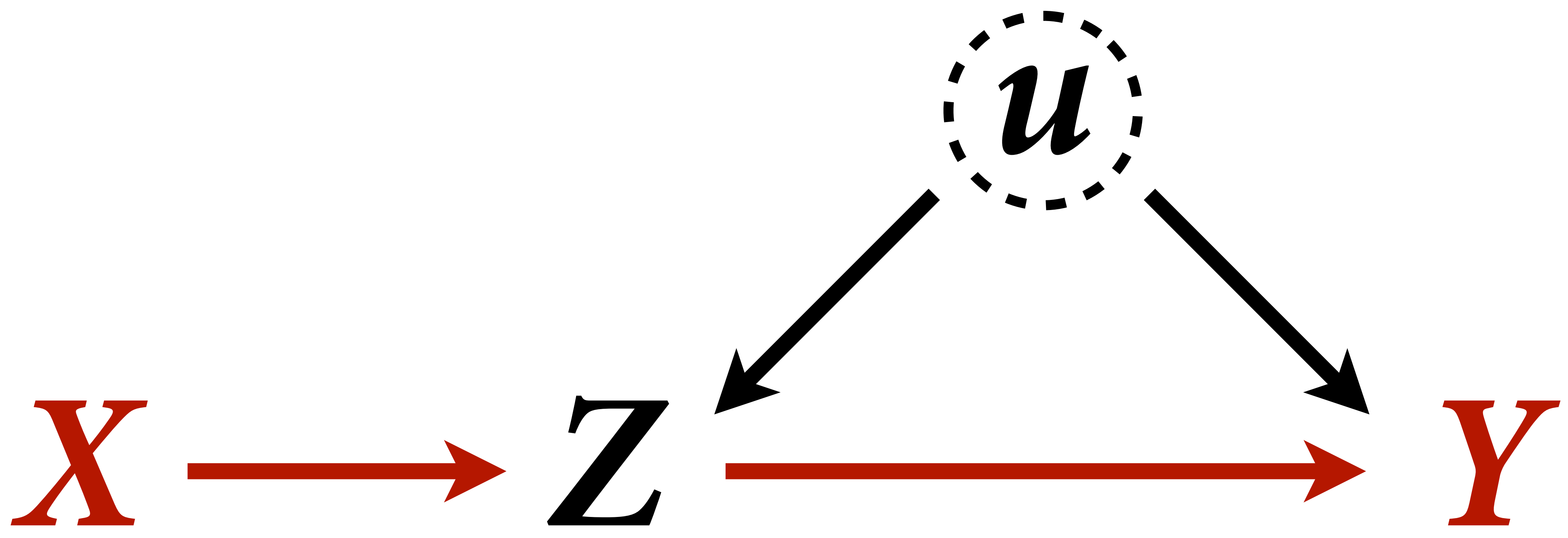
What happens if you stratify by *Z*?

Opens the backdoor path

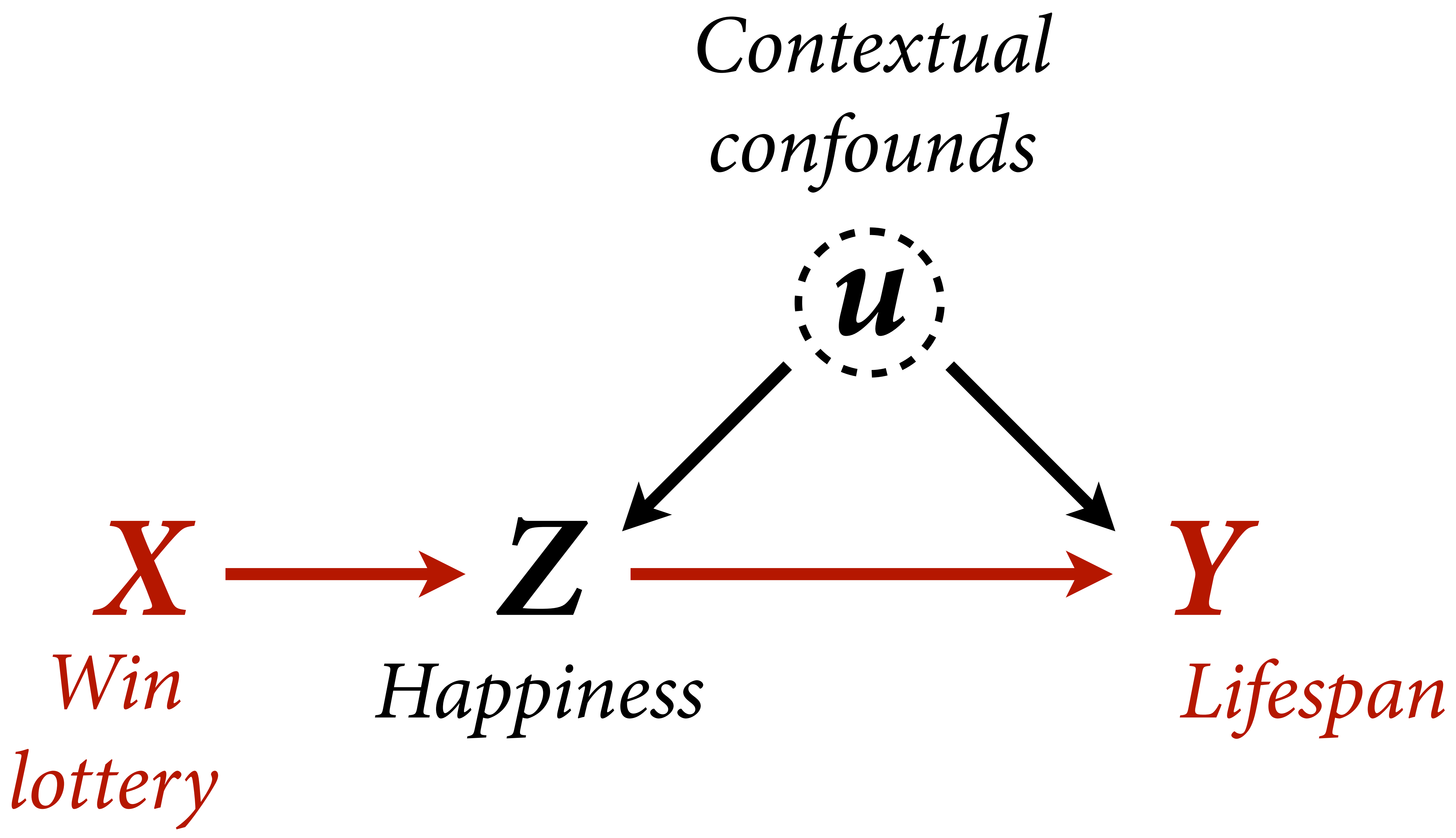
Z could be a **pre-treatment** variable

Not safe to always control pre-treatment measurements





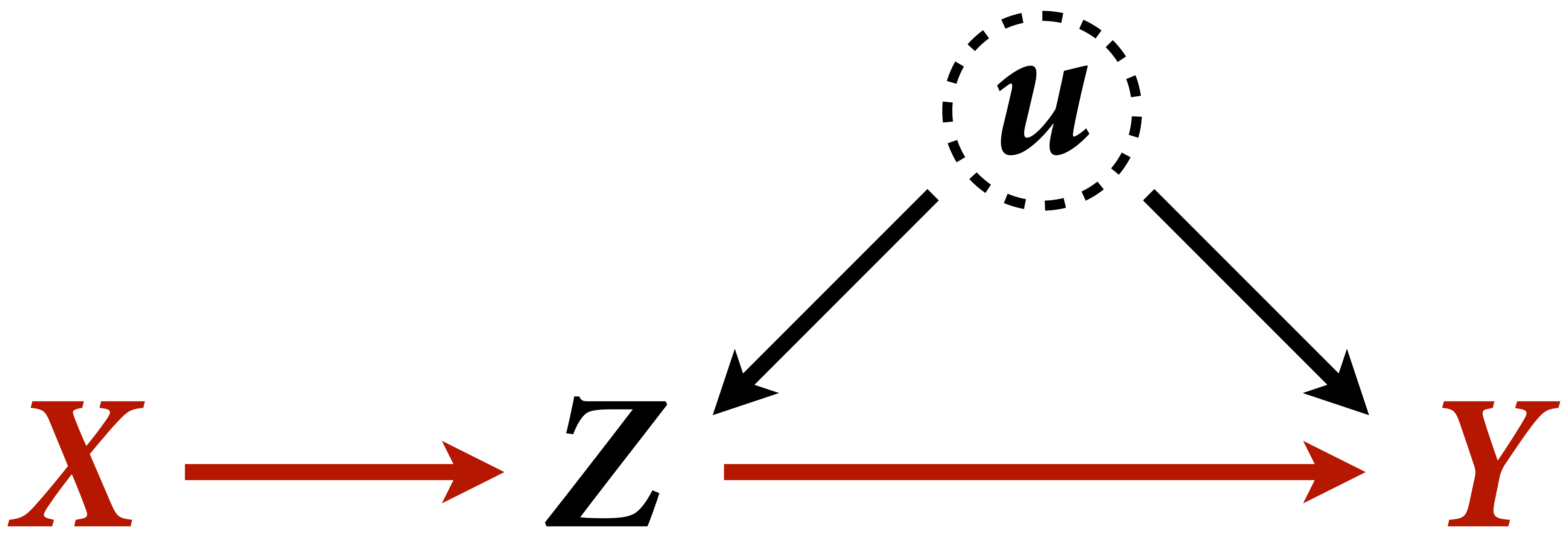
u
X
Z
Y


Contextual confounds
u
X
Z
Y
Win lottery
Happiness
Lifespan

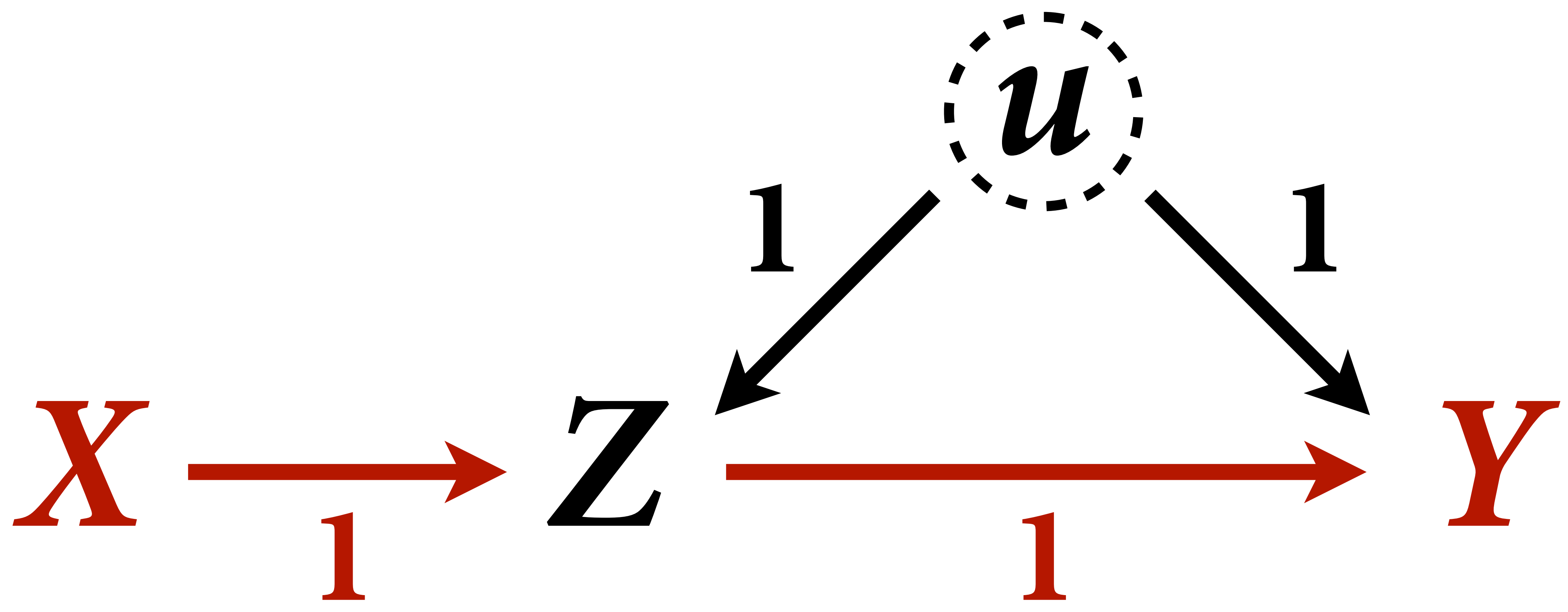
$$X \rightarrow Z \rightarrow Y$$

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

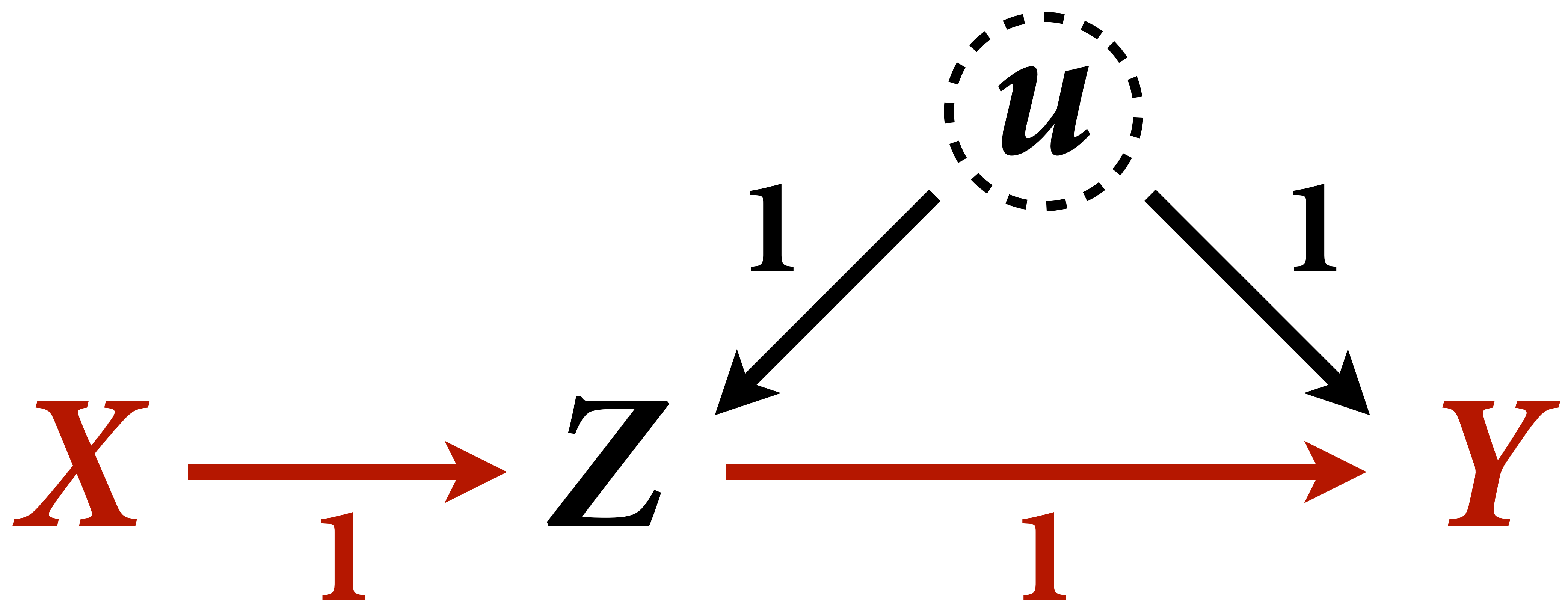
No backdoor, no need to control for Z

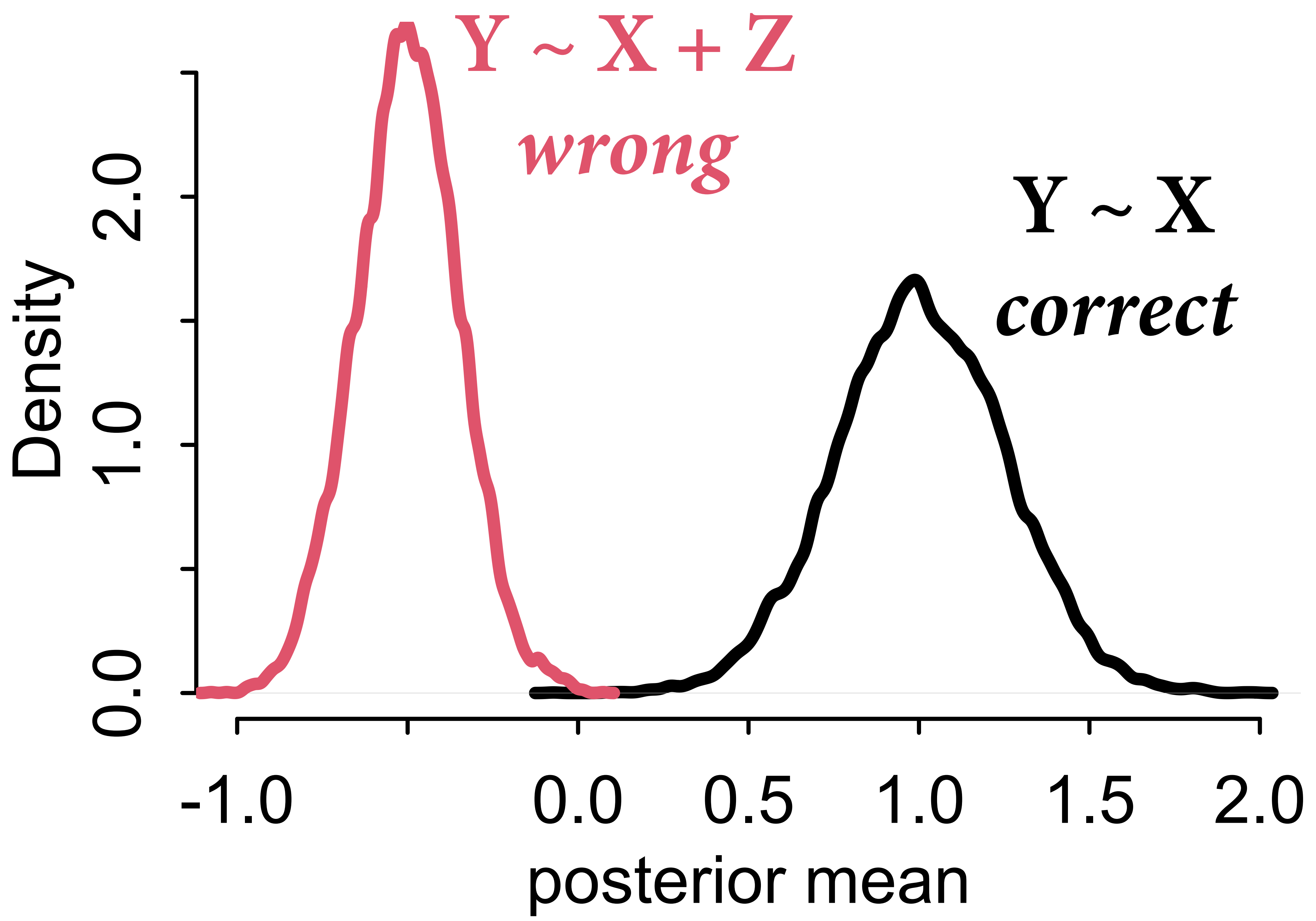


```r
f <- function(n=100,bXZ=1,bZY=1) {
    X <- rnorm(n)
    u <- rnorm(n)
    Z <- rnorm(n, bXZ*X + u)
    Y <- rnorm(n, bZY*Z + u )
    bX <- coef( lm(Y ~ X) )['X']
    bXZ <- coef( lm(Y ~ X + Z) )['X']
    return( c(bX,bXZ) )
}

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

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



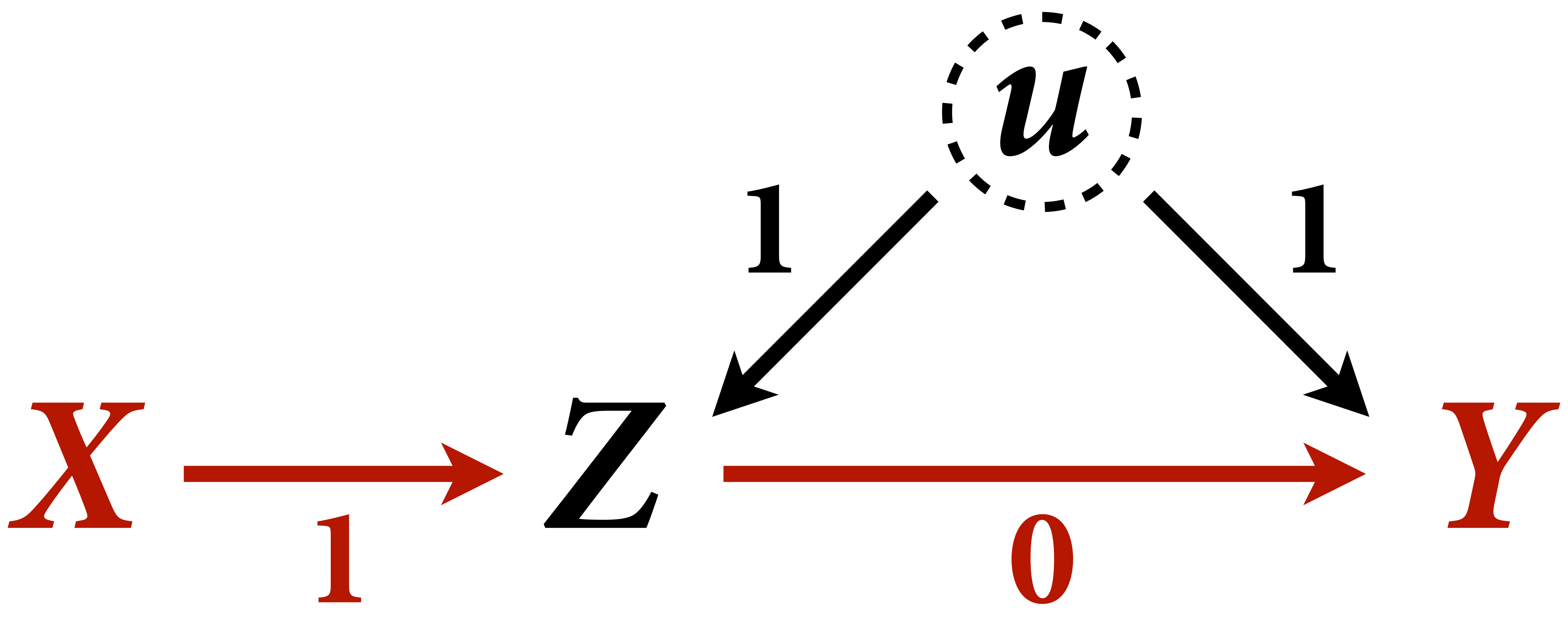
```
f <- function(n=100,bXZ=1,bZY=1) {
    X <- rnorm(n)
    u <- rnorm(n)
    Z <- rnorm(n, bXZ*X + u)
    Y <- rnorm(n, bZY*Z + u )
    bX <- coef( lm(Y ~ X) )['X']
    bXZ <- coef( lm(Y ~ X + Z) )['X']
    return( c(bX,bXZ) )
}

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

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

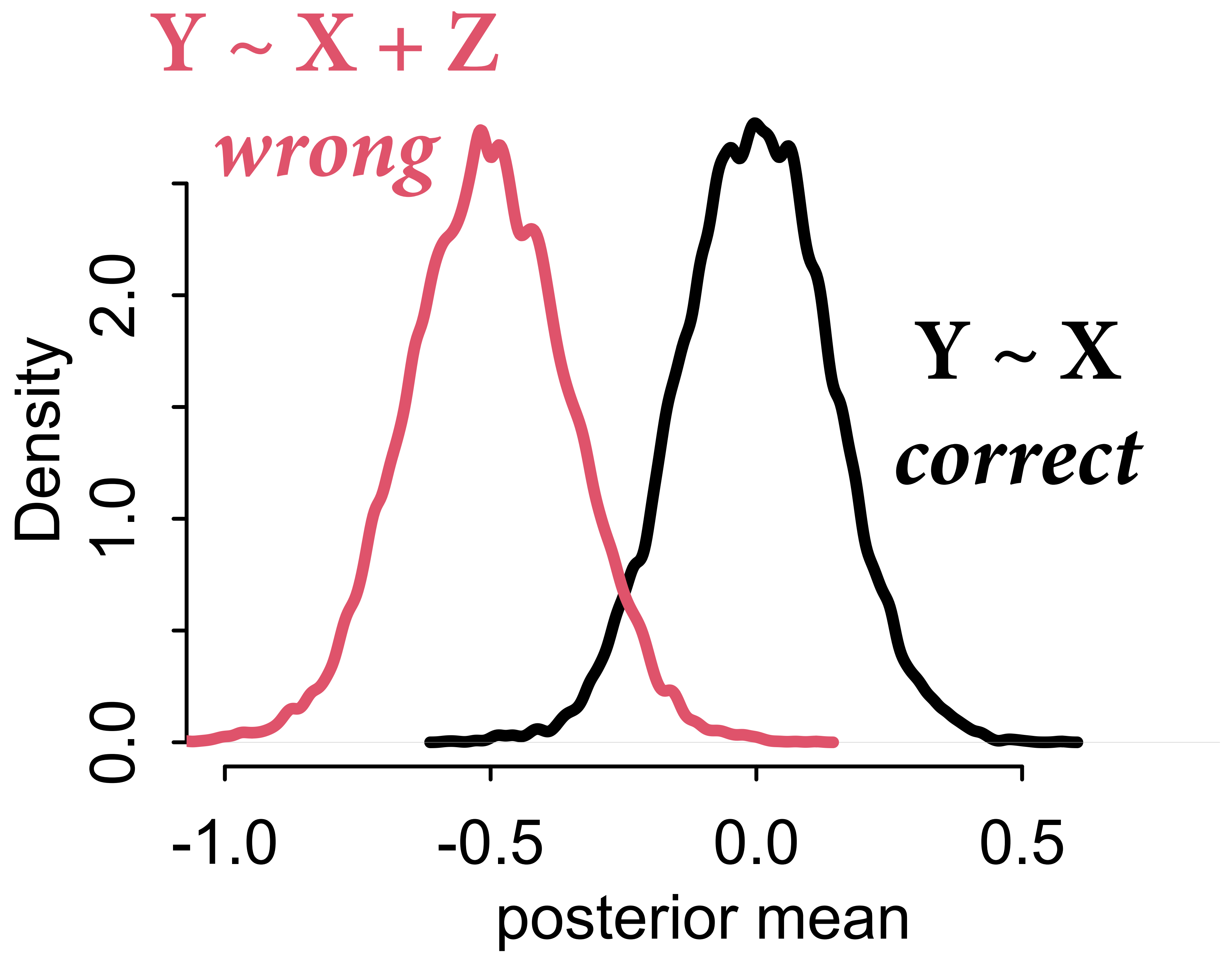




Change bZY to zero

```
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 )
```





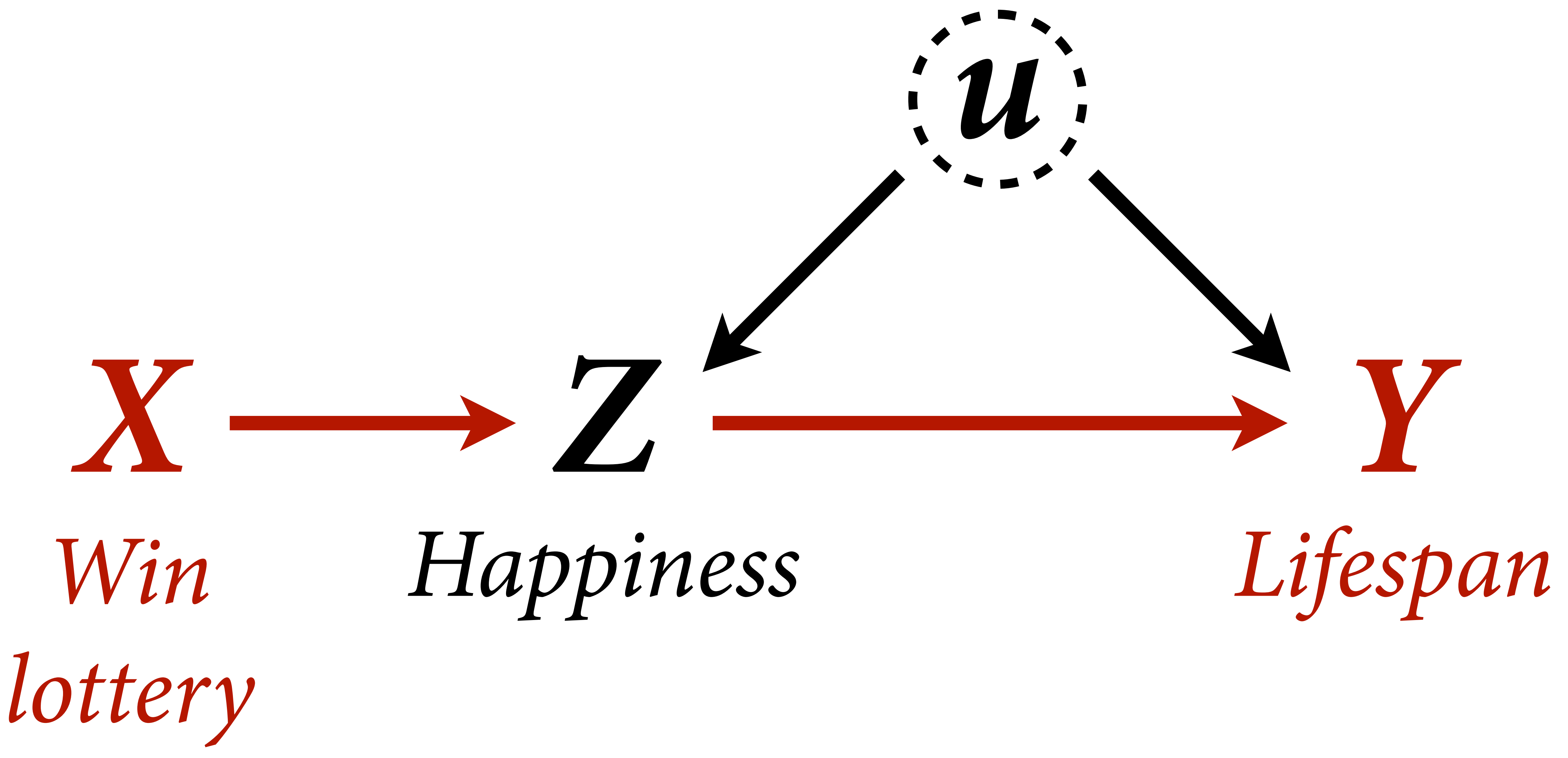
$X \to Z \to Y$
$X \to Z \leftarrow u \to Y$

No backdoor, no need to control for Z

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

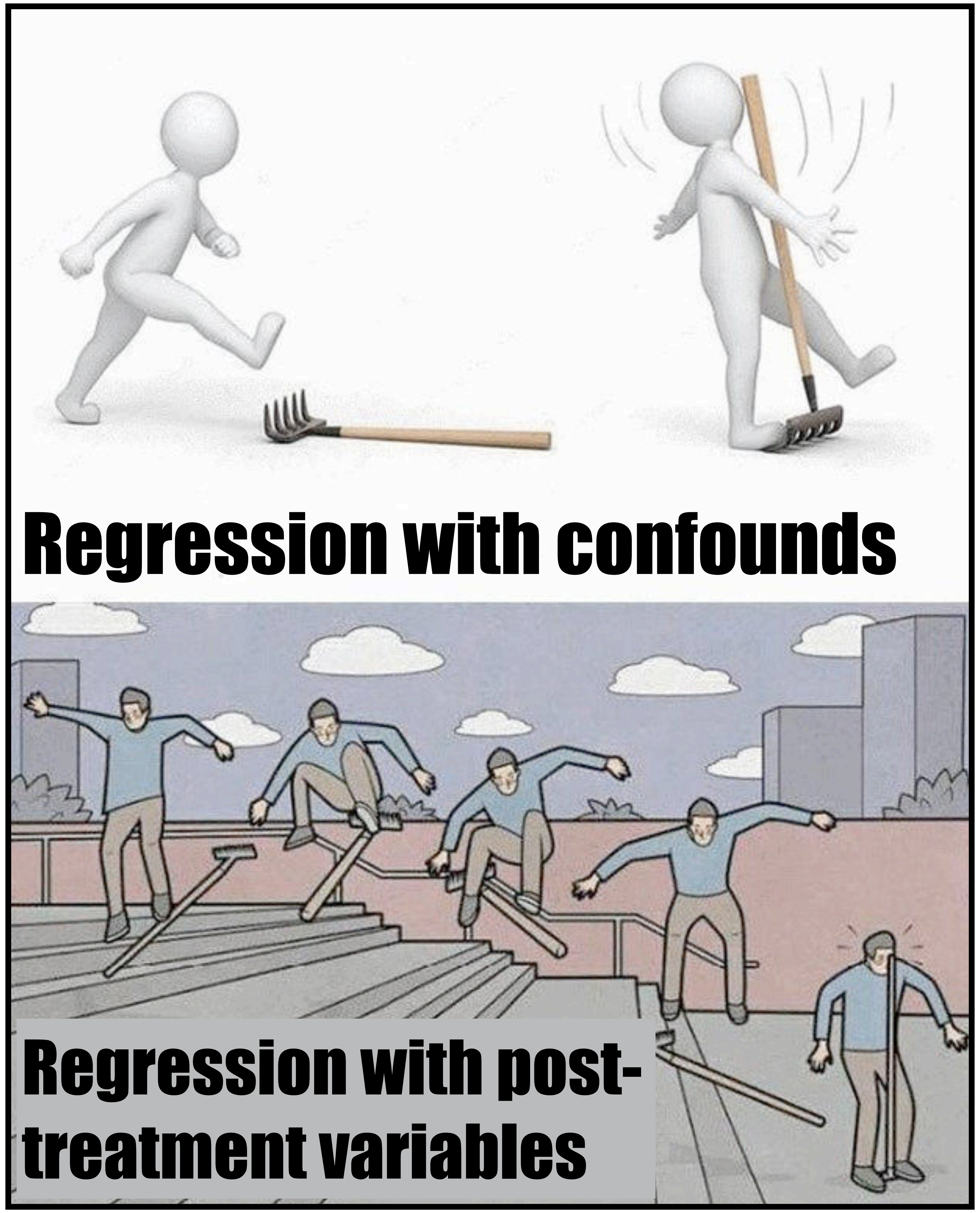


Post-treatment bias is common

TABLE 1 Posttreatment Conditioning in Experimental Studies

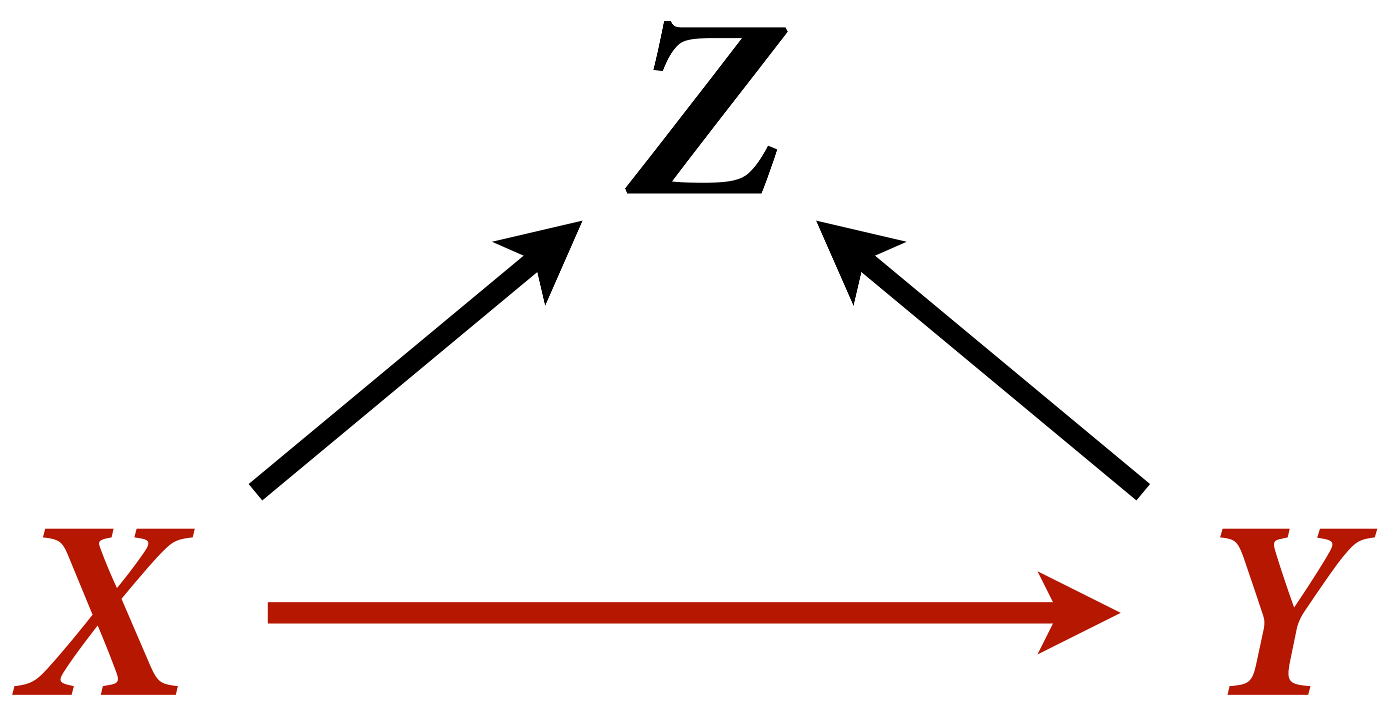
Category	Prevalence
Engages in posttreatment conditioning	46.7%
Controls for/interacts with a posttreatment variable	21.3%
Drops cases based on posttreatment criteria	14.7%
Both types of posttreatment conditioning present	10.7%
No conditioning on posttreatment variables	52.0%
Insufficient information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

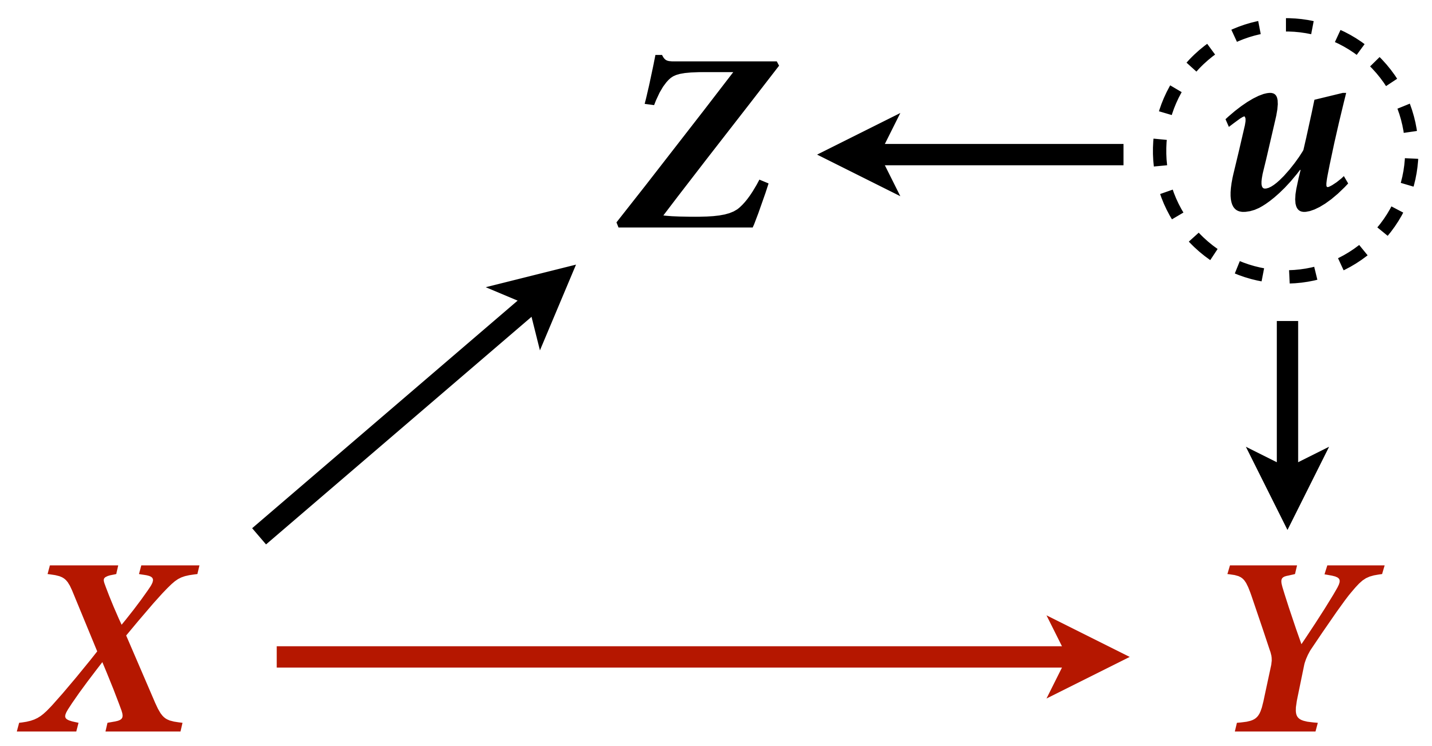


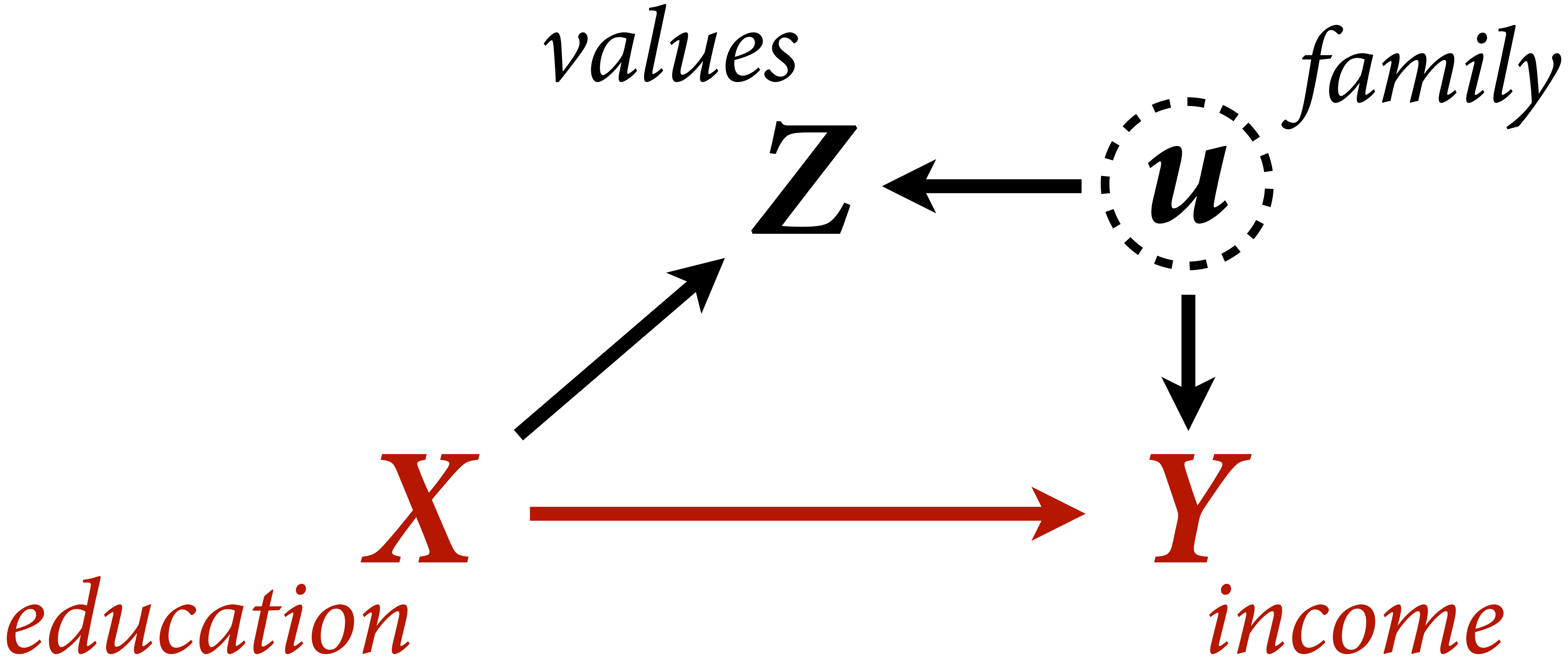
Montgomery et al 2018 How Conditioning on Posttreatment Variables Can Ruin Your Experiment

Do not touch the collider!

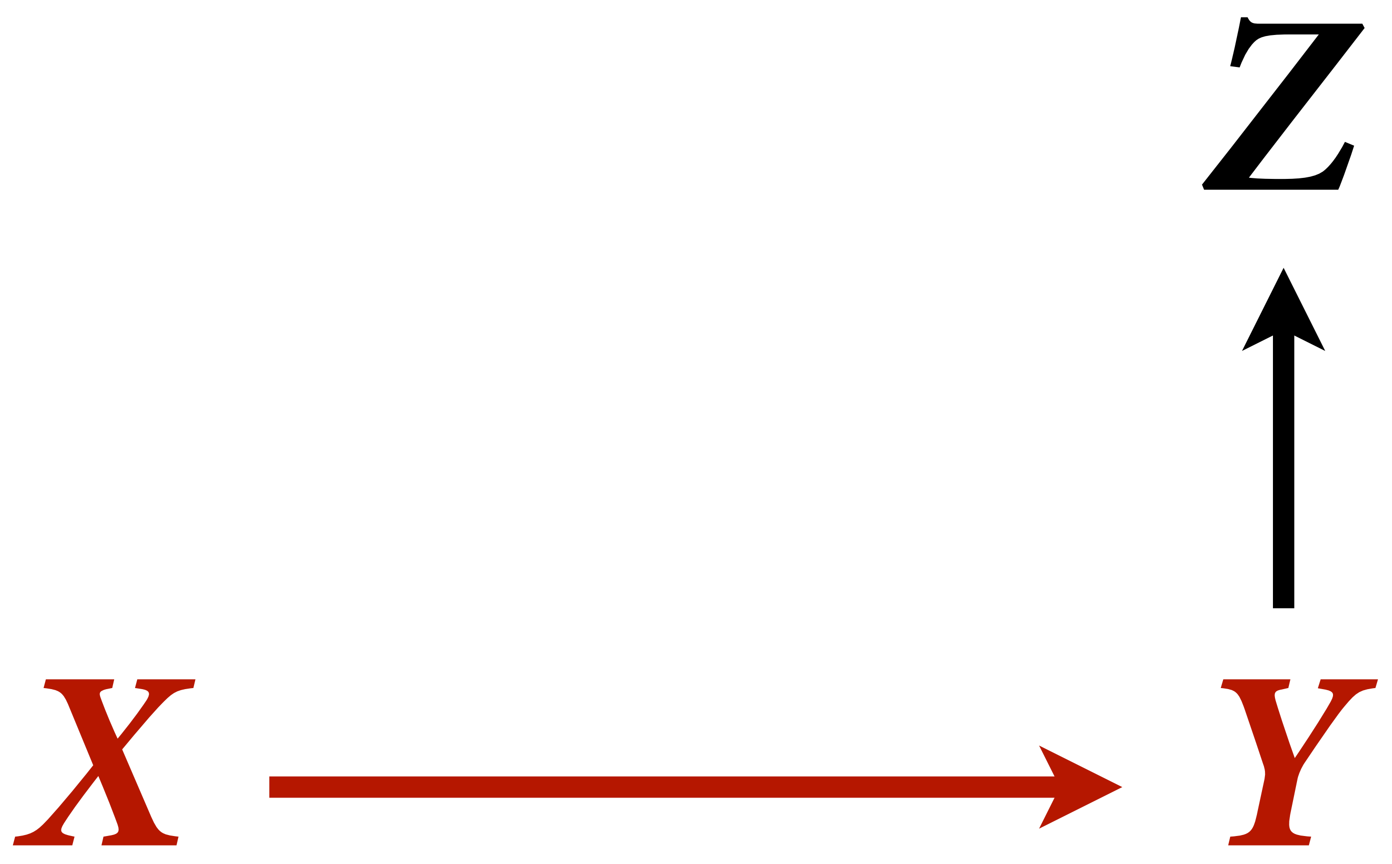


Colliders not always so obvious

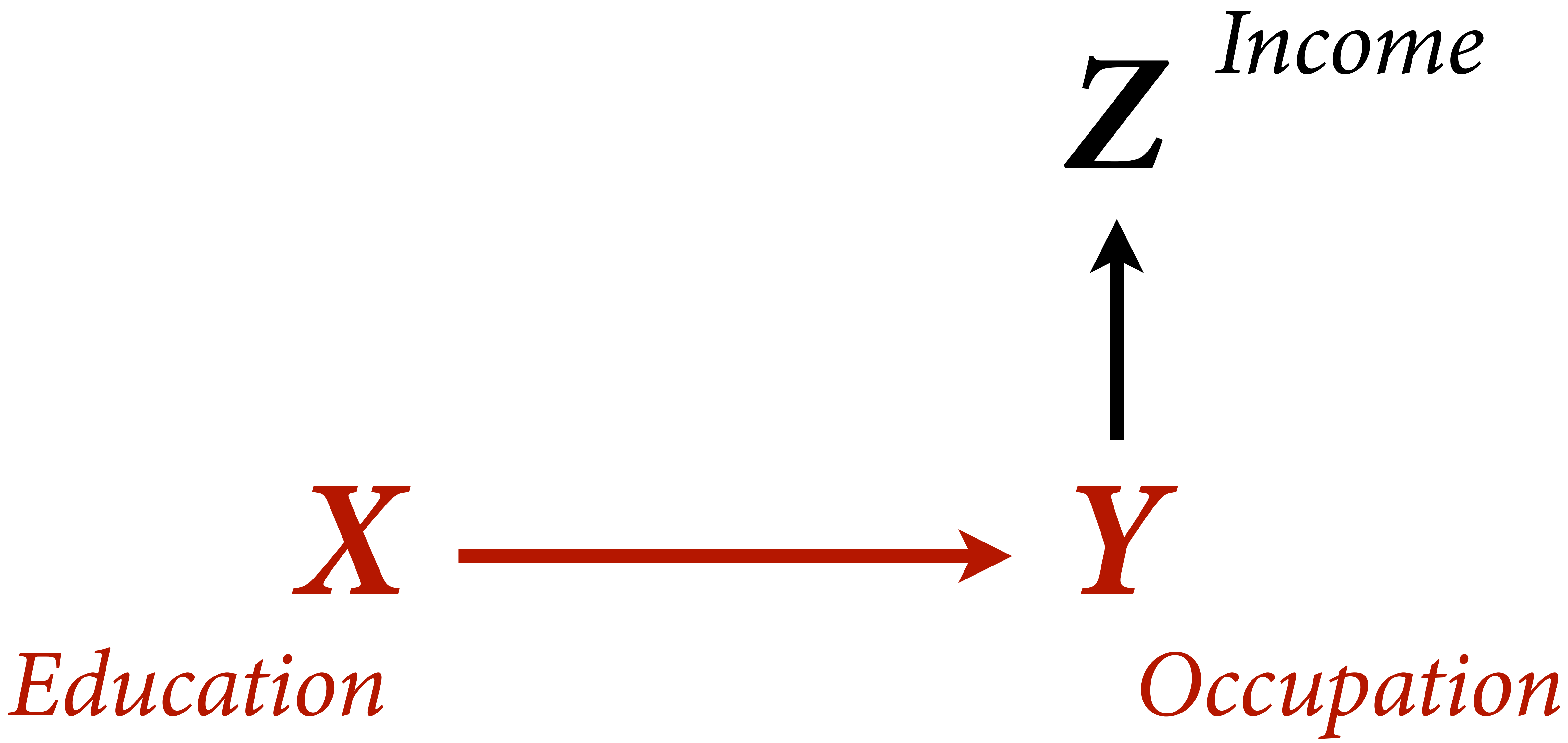



values
family
Z
u
X
Y
education
income

"Case-control bias"



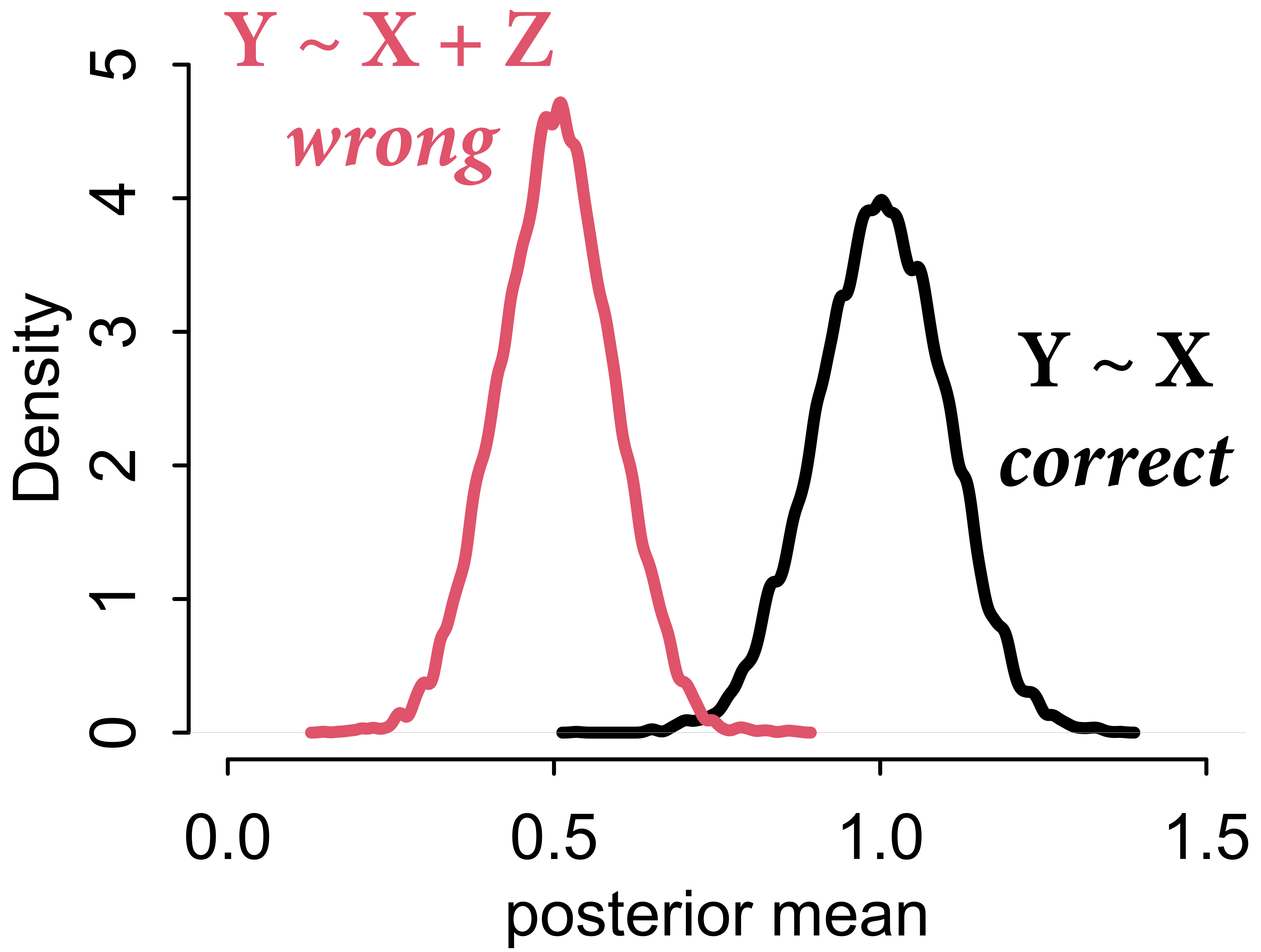
“Case-control bias”



“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 )
```

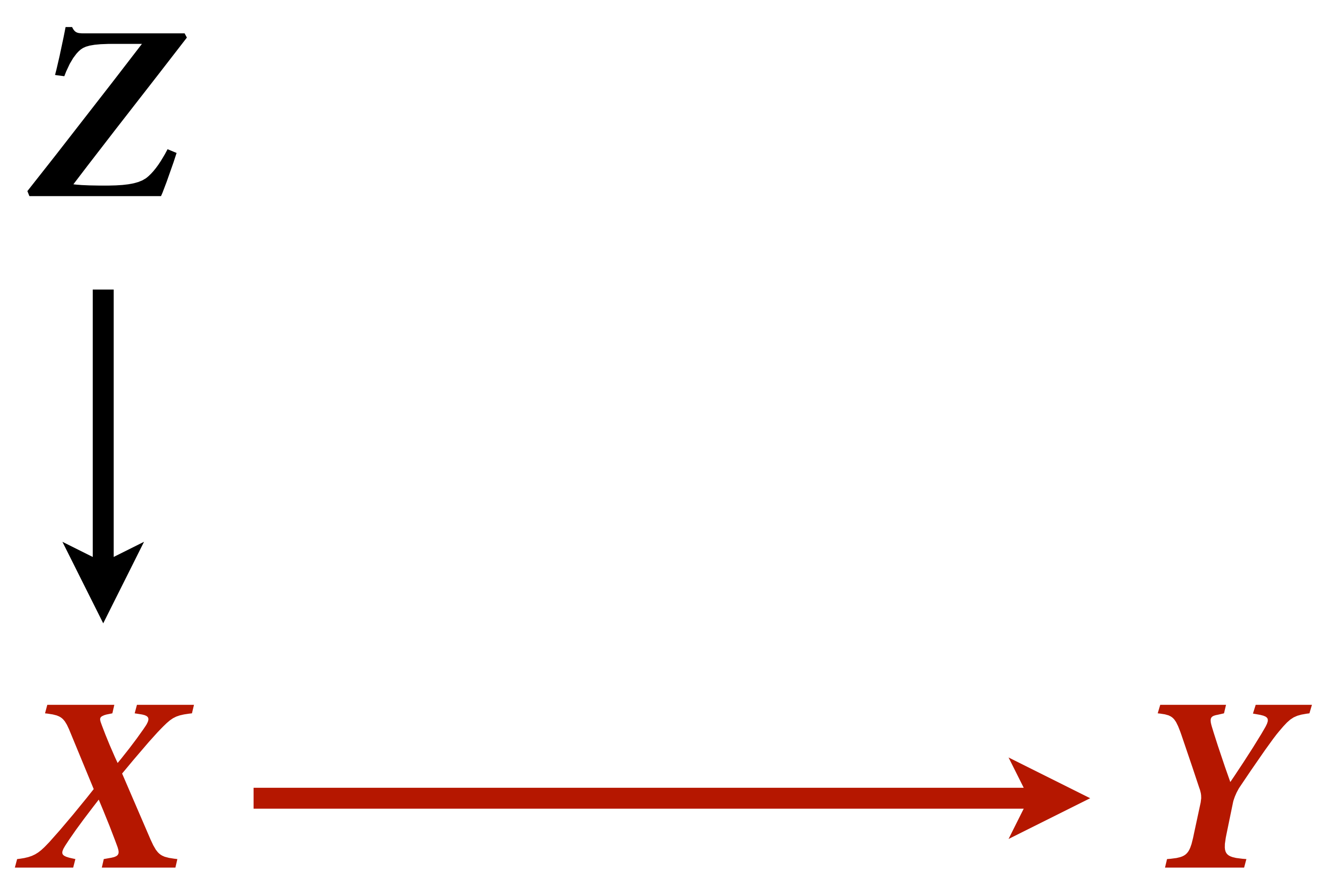
$$X \xrightarrow{1} Y \xrightarrow{1} Z$$



“Precision parasite”

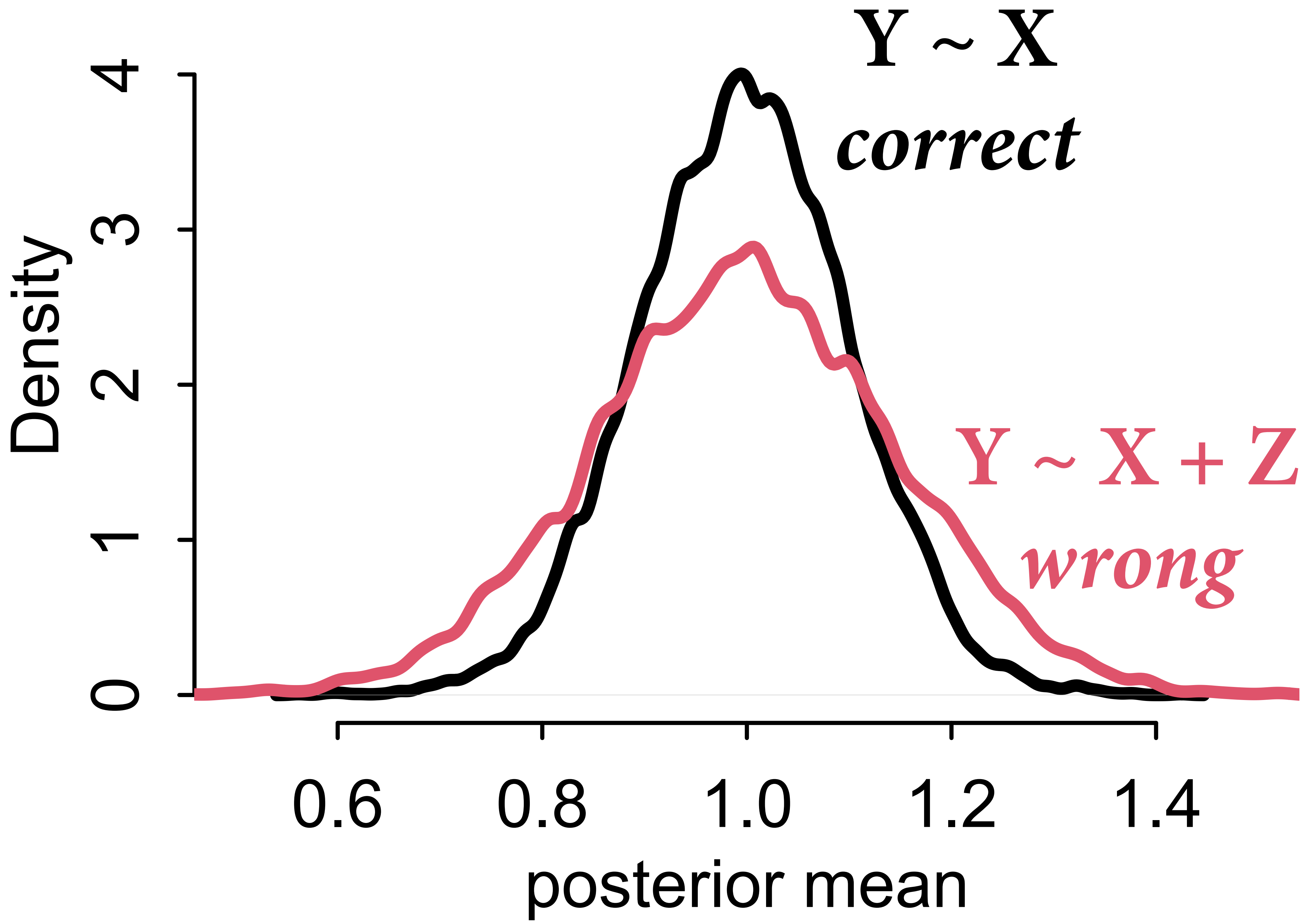
No backdoors

But still not good to condition on *Z*



“Precision parasite”

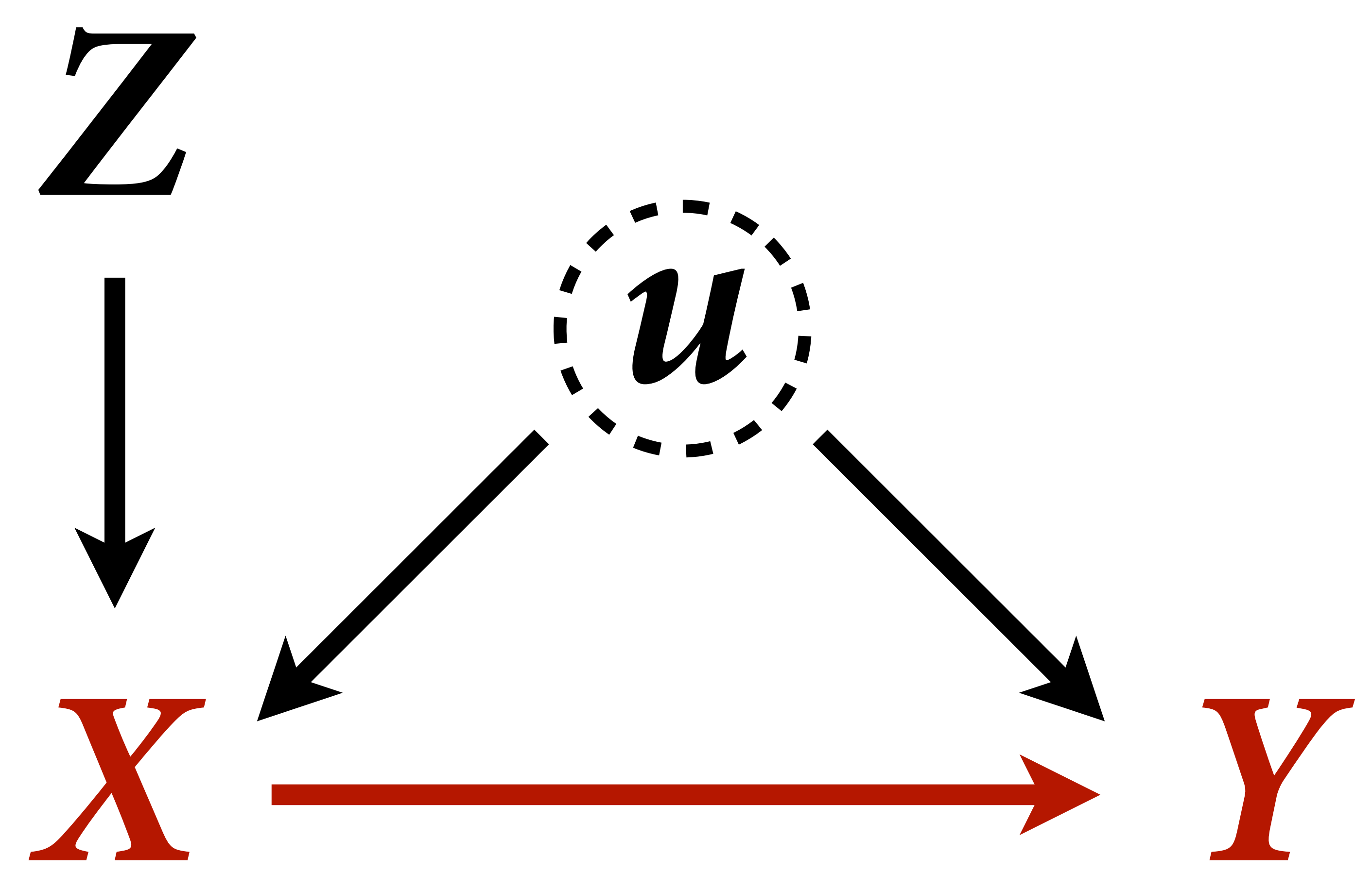
```
f <- function(n=100,bZX=1,bXY=1) {
    Z <- rnorm(n)
    X <- rnorm(n, bZX*Z )
    Y <- rnorm(n, bXY*X )
    bX <- coef( lm(Y ~ X) )['X']
    bXZ <- coef( lm(Y ~ X + Z) )['X']
    return( c(bX,bXZ) )
}

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 )
```



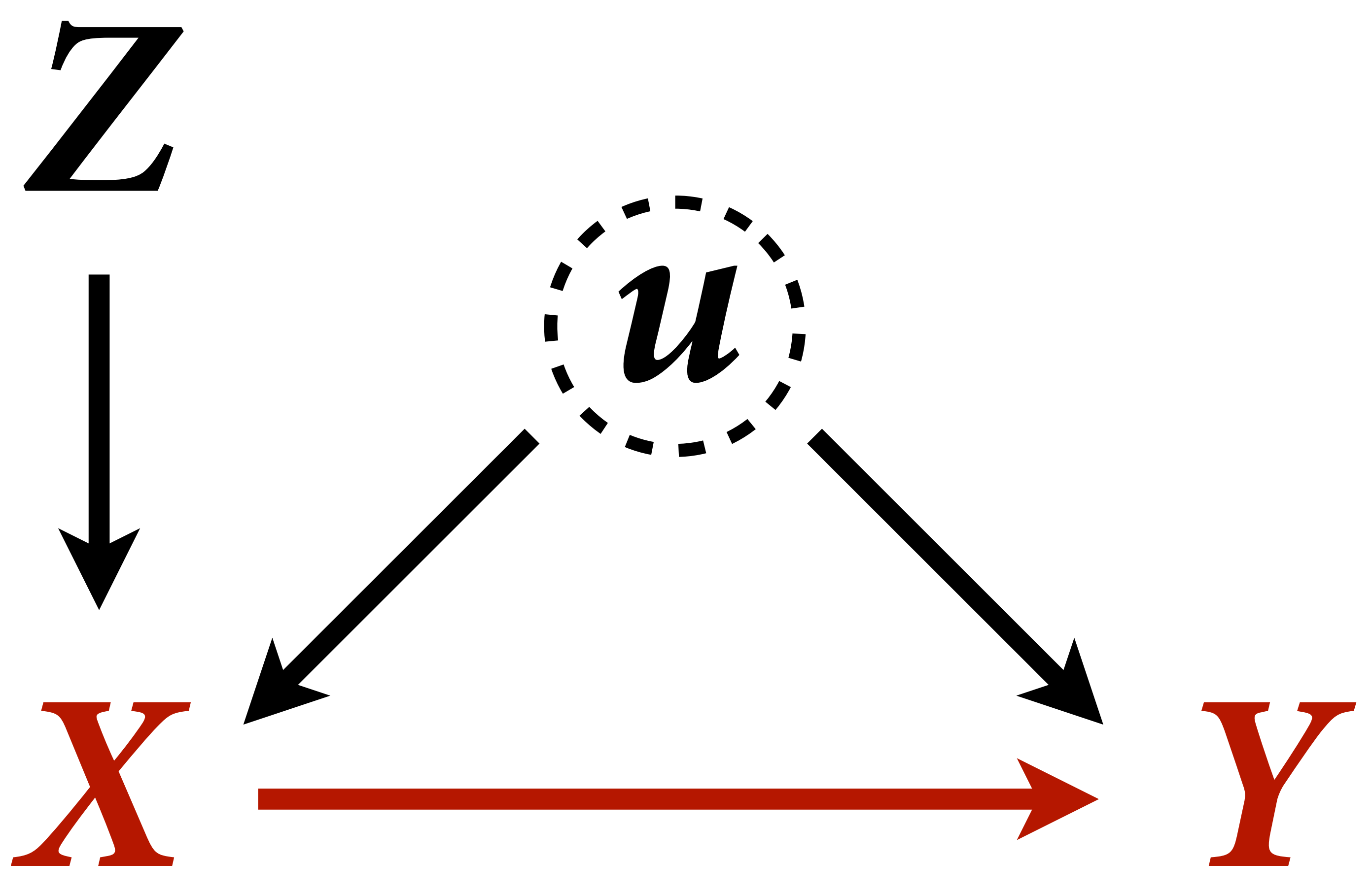
"Bias amplification"

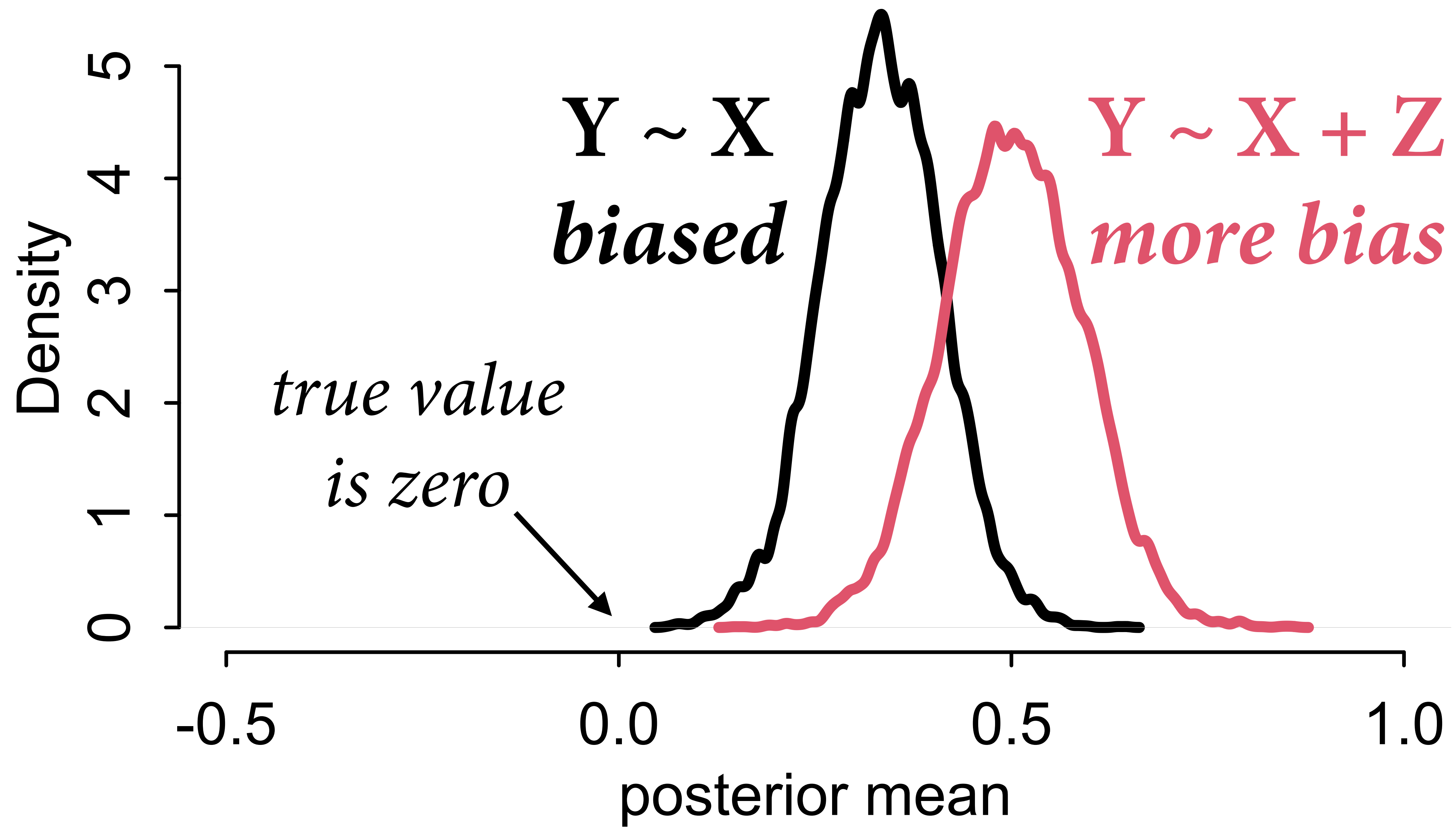
X and ***Y*** confounded by ***u***

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



```r
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 )
```



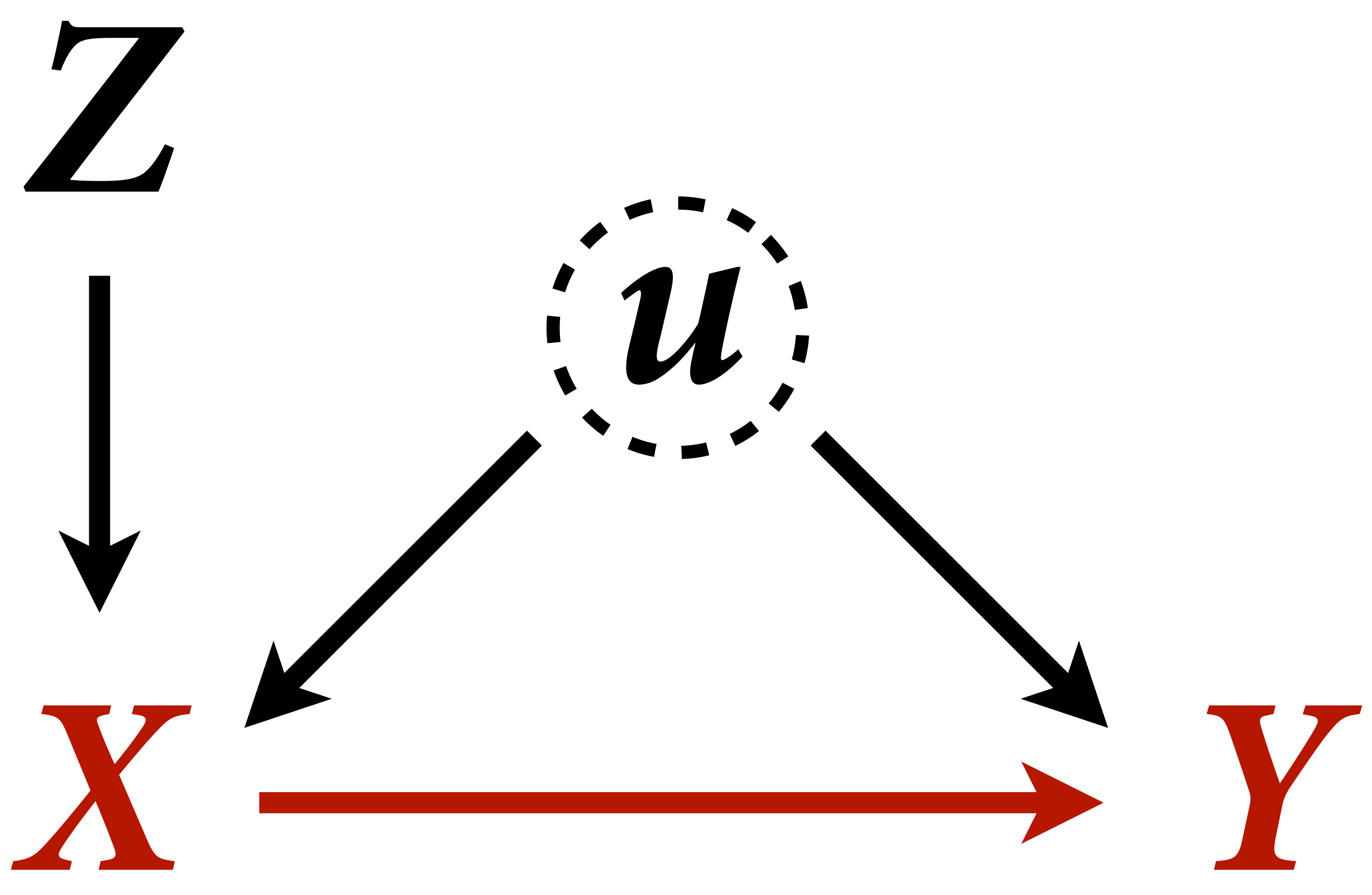


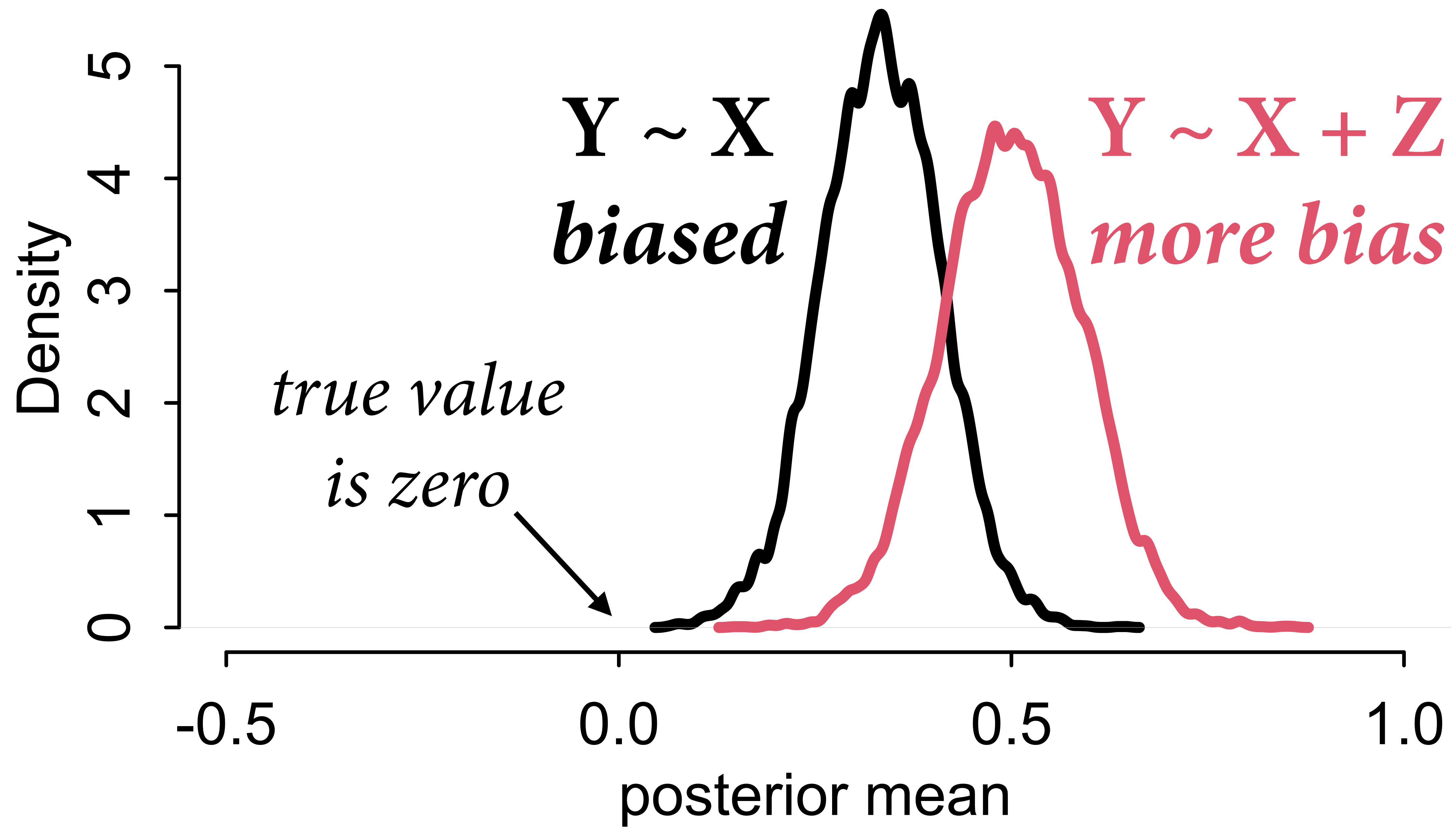
WHY?

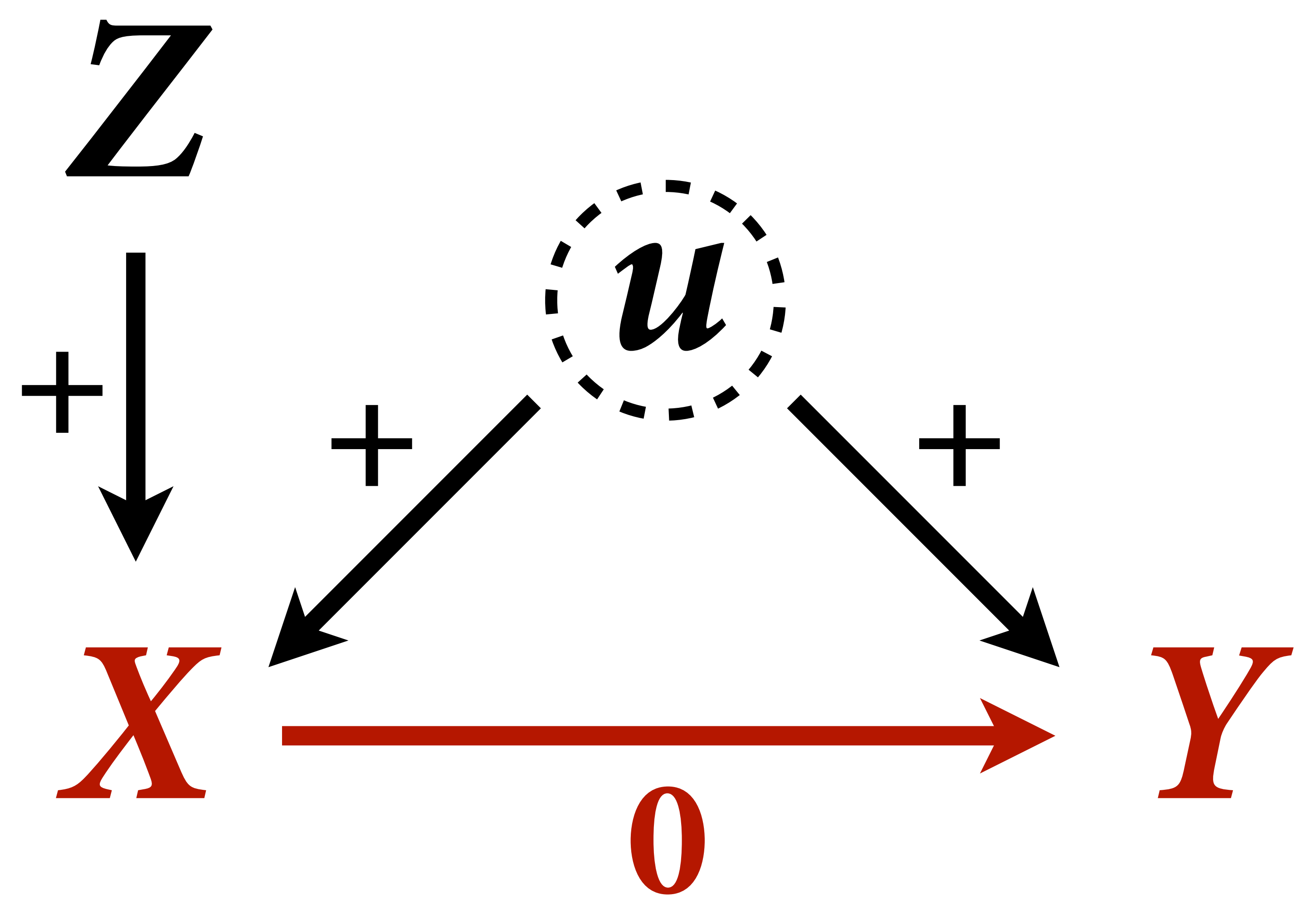
Covariation ***X*** & ***Y*** requires variation in their causes

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

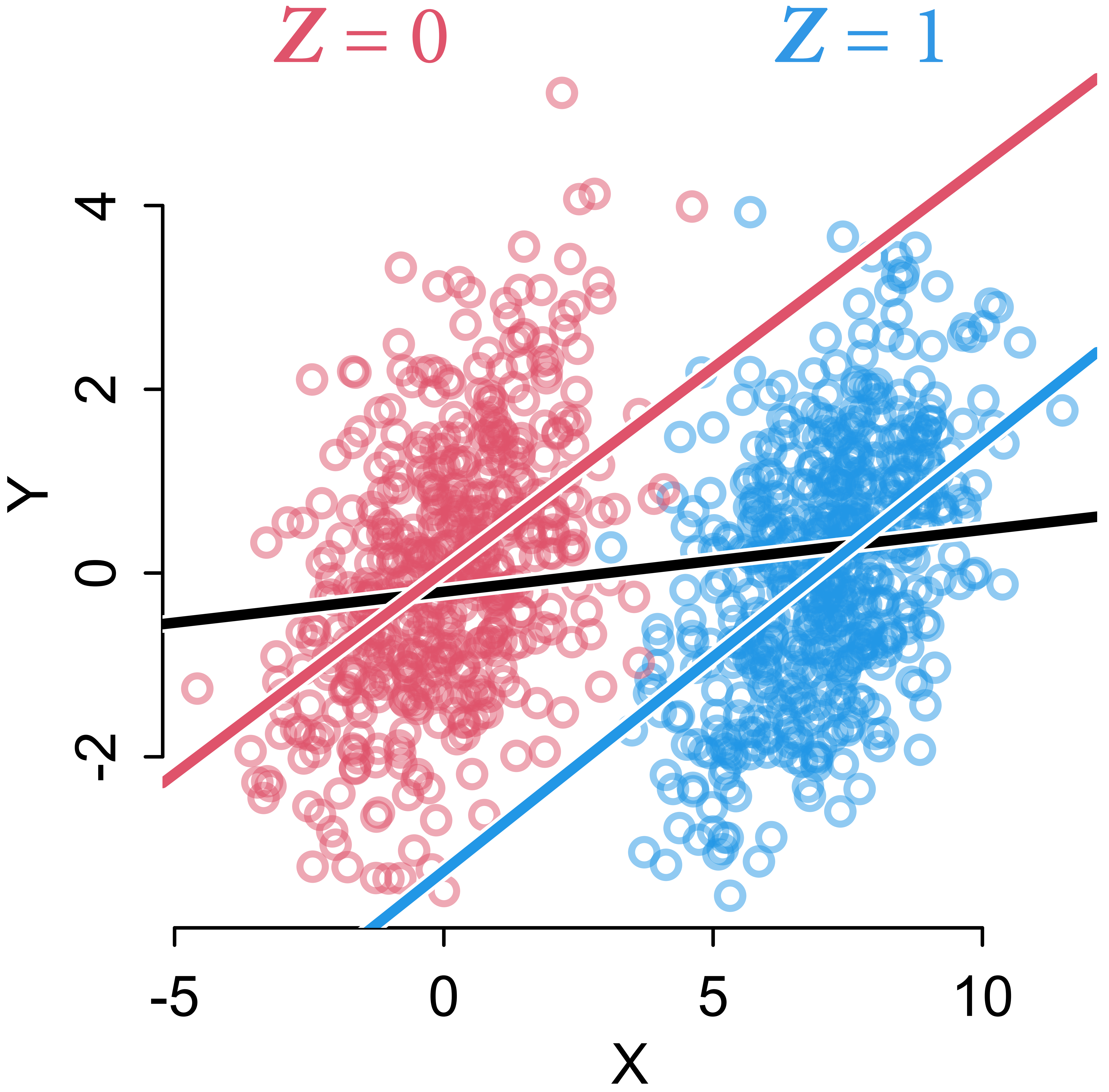
Confound ***u*** relatively more important within each ***Z***








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

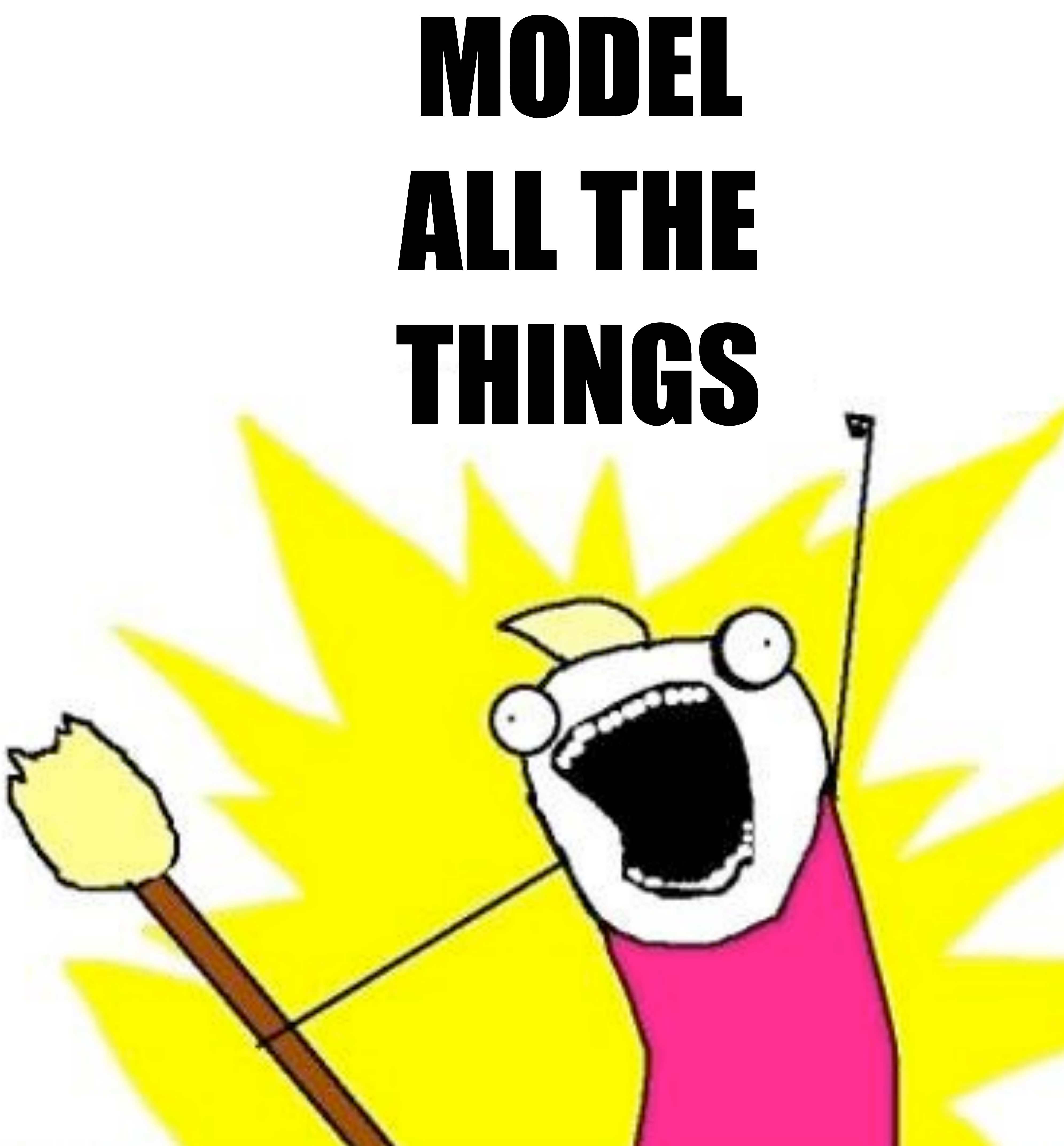


Good & Bad Controls

"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



PAUSE

Table 2 Fallacy

Not all coefficients are causal effects

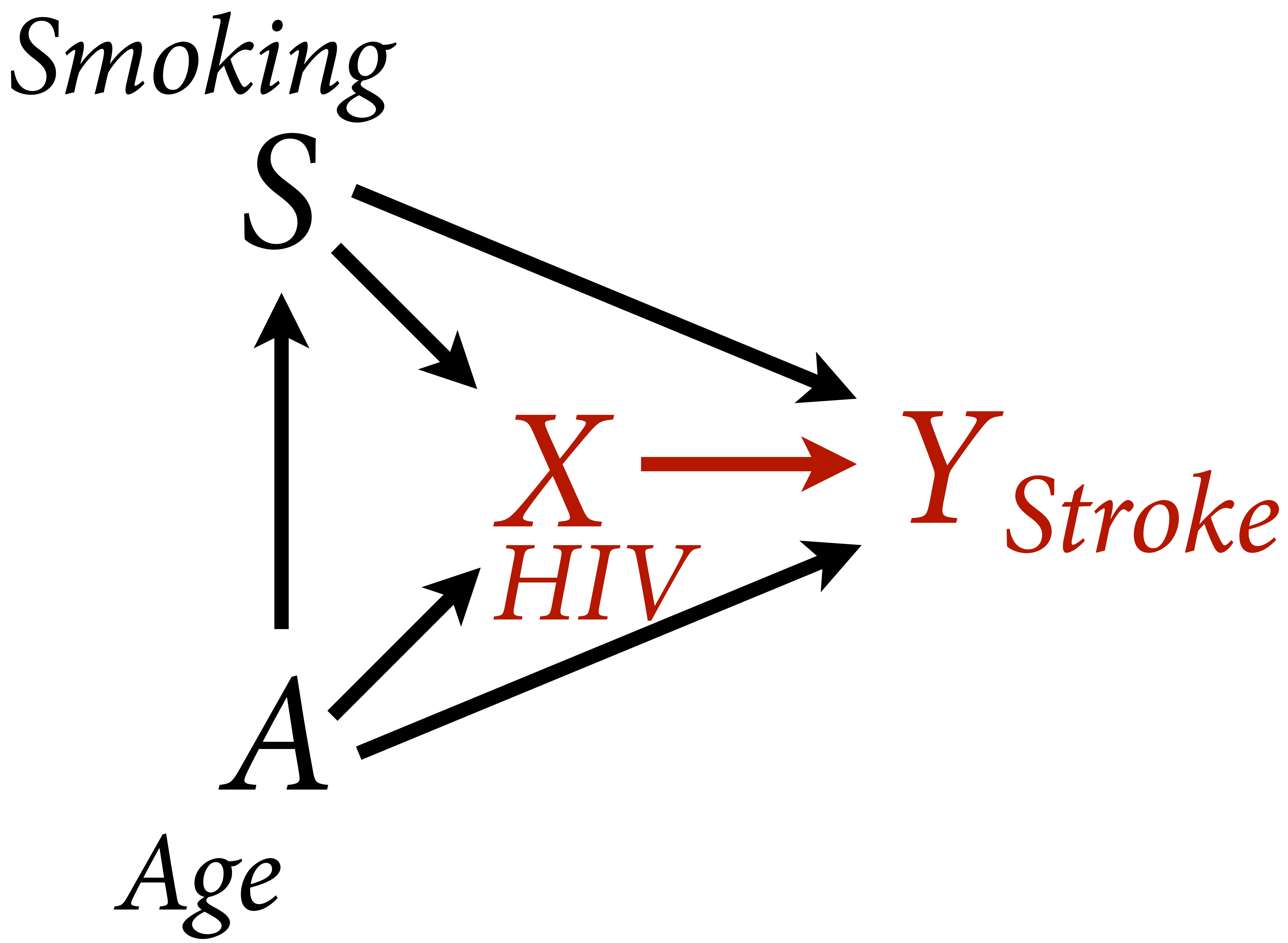
Statistical model designed to identify ***X*** –> ***Y*** will not also identify effects of control variables

Table 2 is dangerous

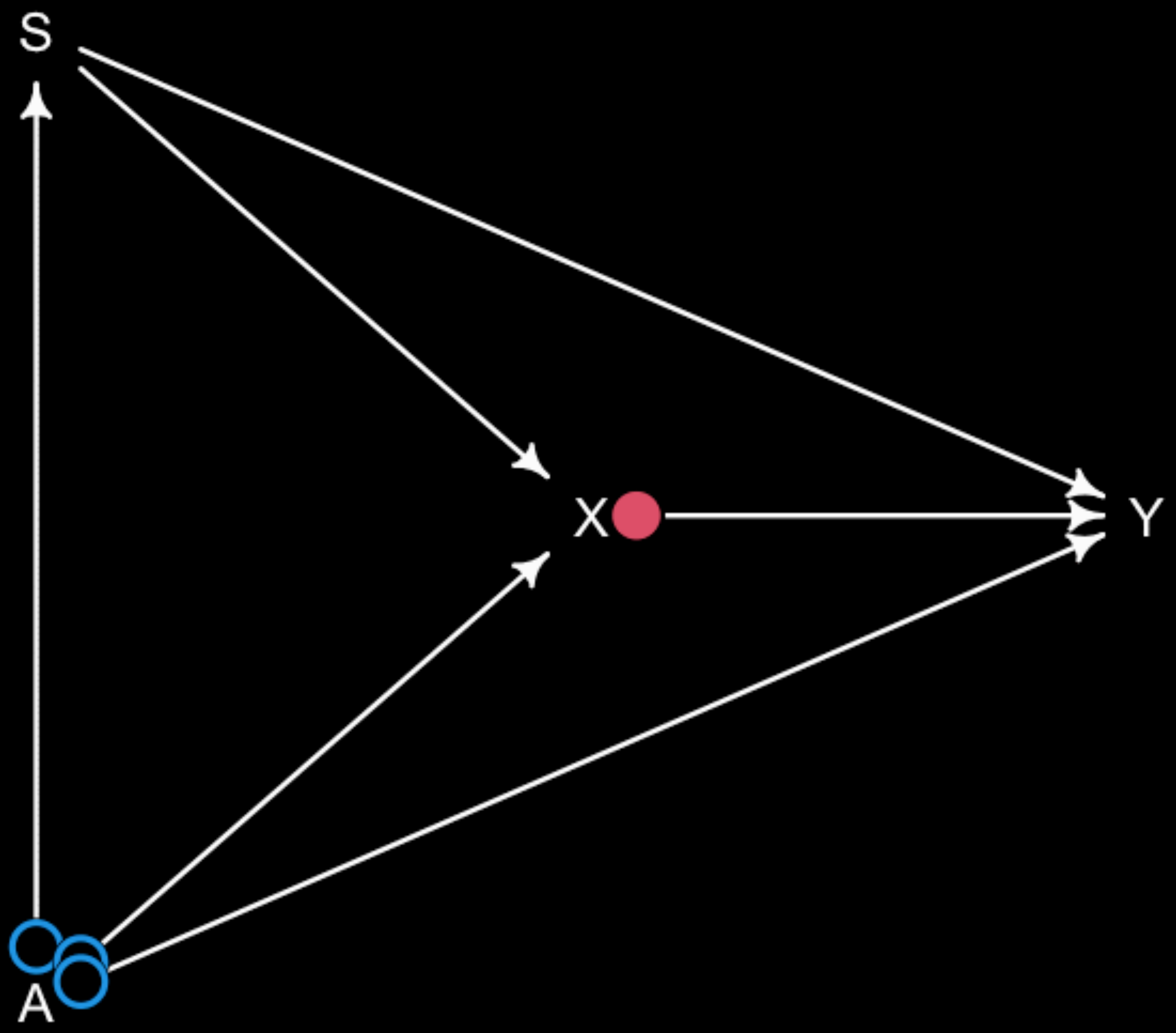
TABLE 2—ESTIMATED PROBIT MODELS FOR THE USE OF A SCREEN

	Preliminaries blind		Finals blind
	(1)	(2)	(3)
(Proportion female)$_{t-1}$	2.744	3.120	0.490
	(3.265)	(3.271)	(1.163)
	[0.006]	[0.004]	[0.011]
(Proportion of orchestra personnel with <6 years tenure)$_{t-1}$	−26.46	−28.13	−9.467
	(7.314)	(8.459)	(2.787)
	[−0.058]	[−0.039]	[−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

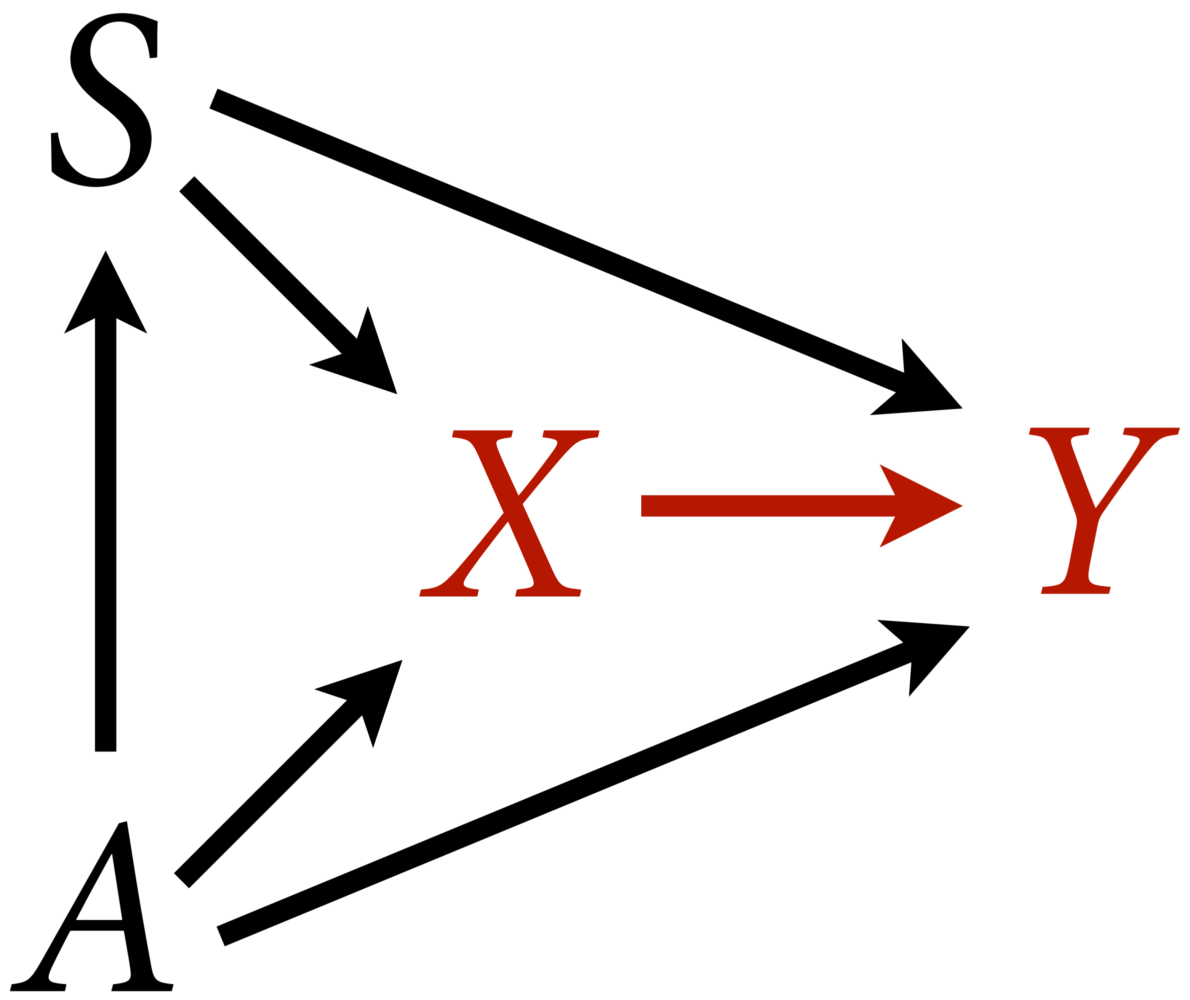
Westreich & Greenland 2013 The Table 2 Fallacy



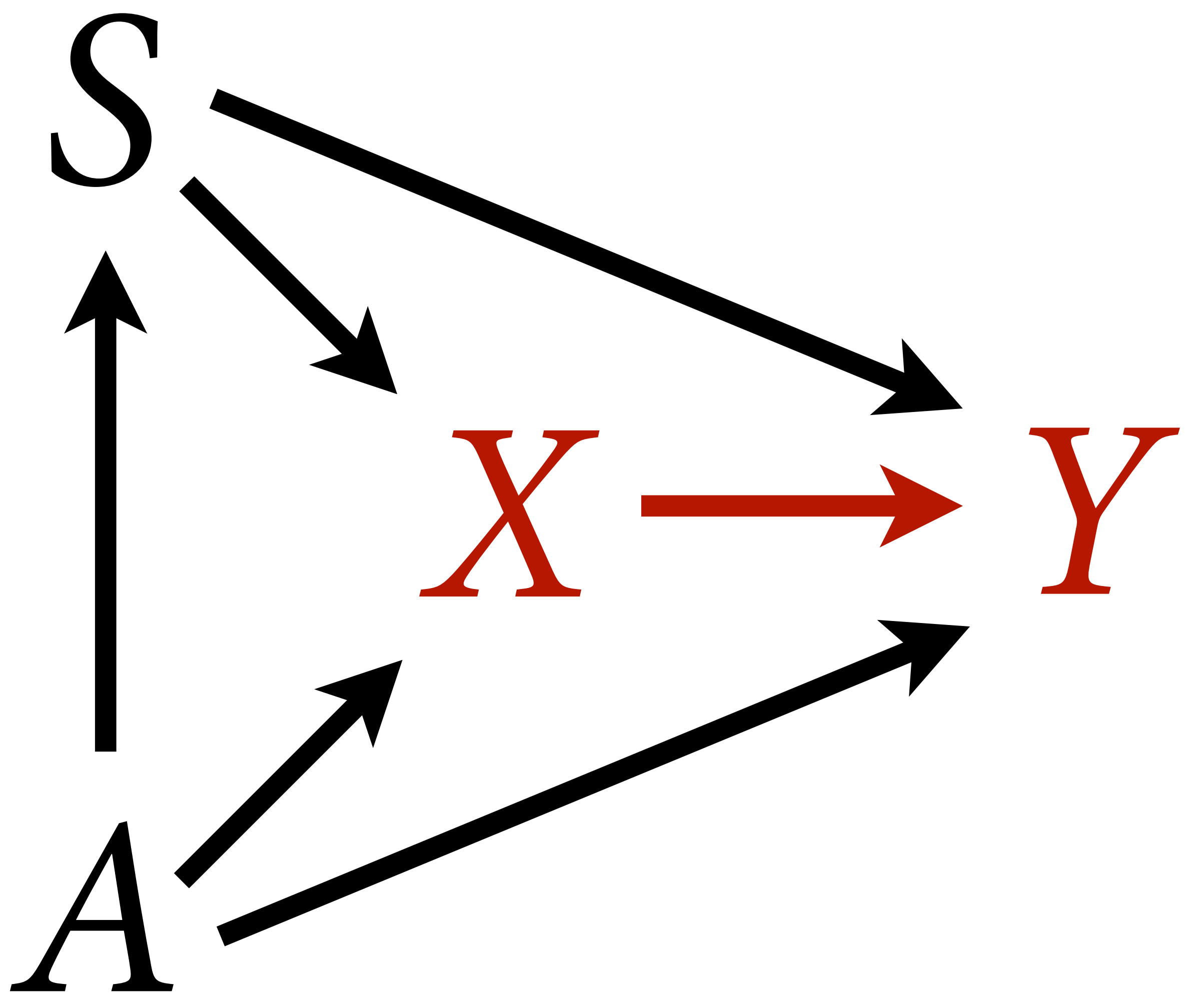
Westreich & Greenland 2013 The Table 2 Fallacy


S
X
Y
A

Use Backdoor Criterion

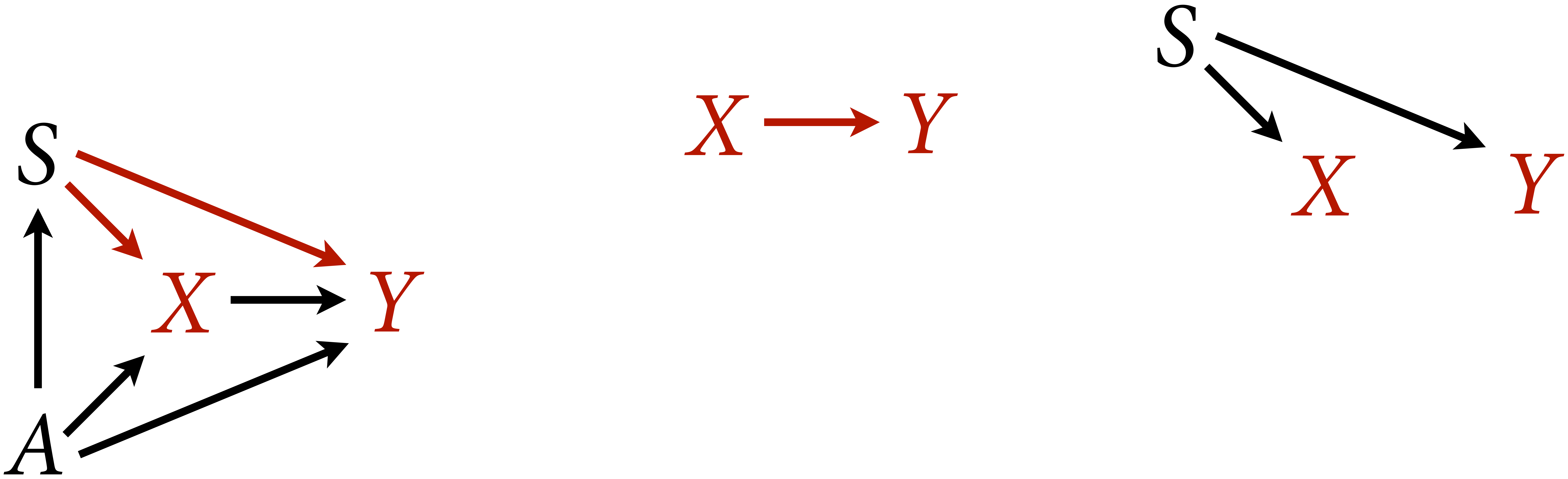


Use Backdoor Criterion

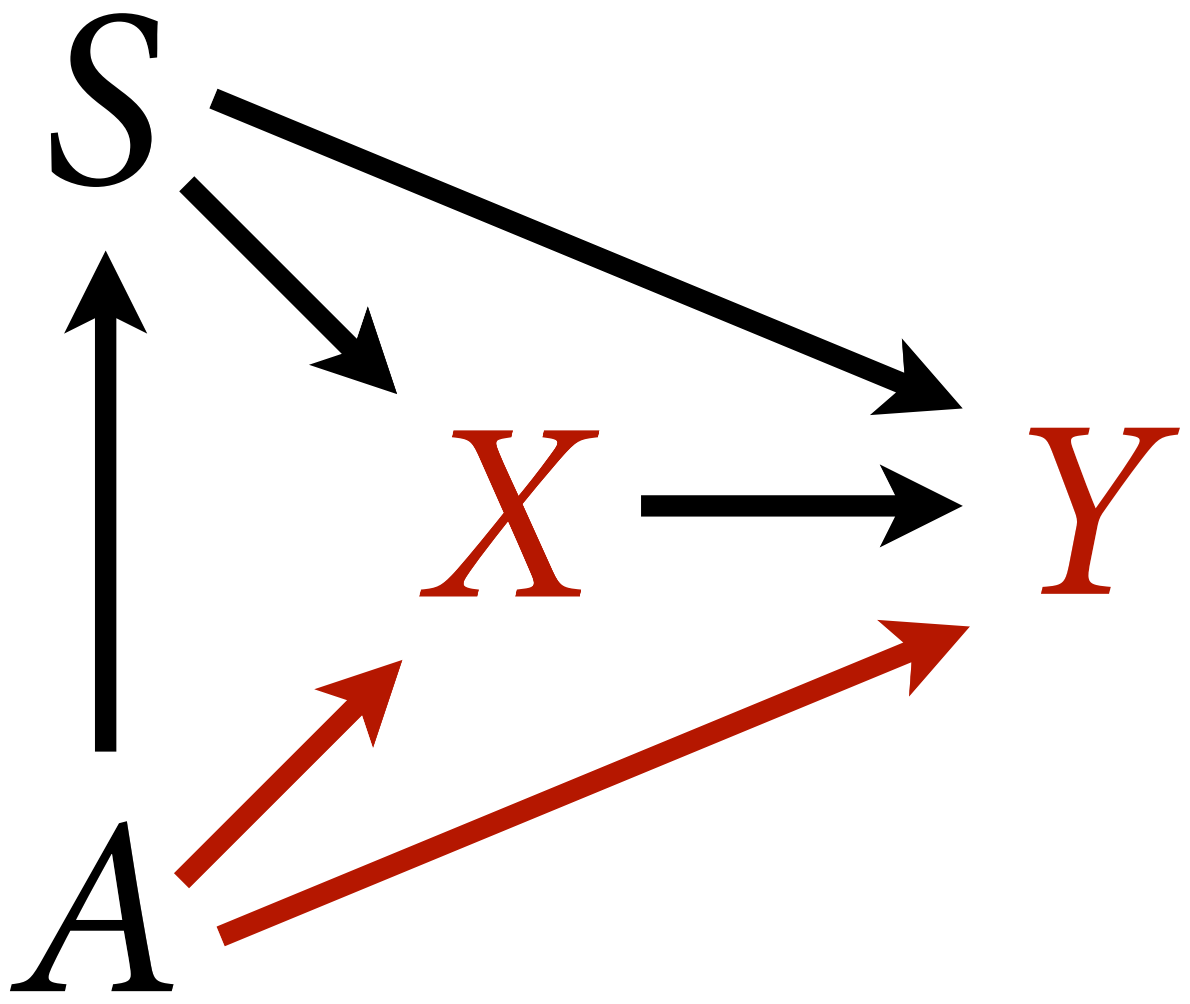


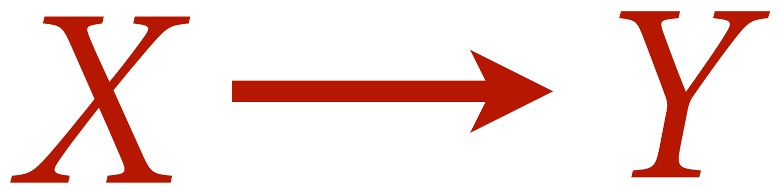
$X \longrightarrow Y$

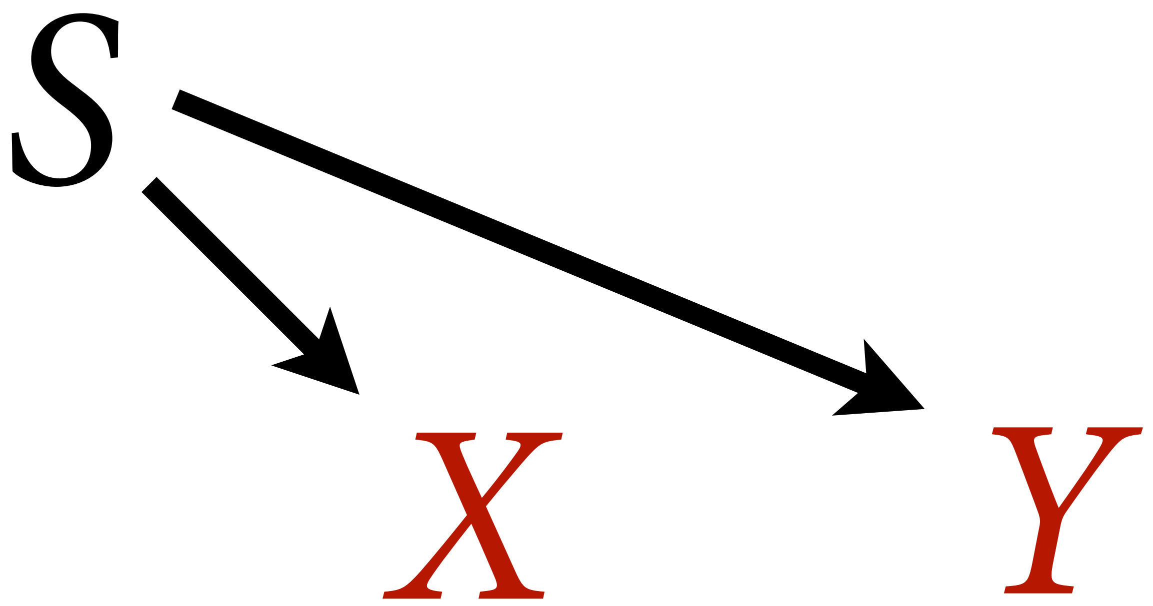
Use Backdoor Criterion

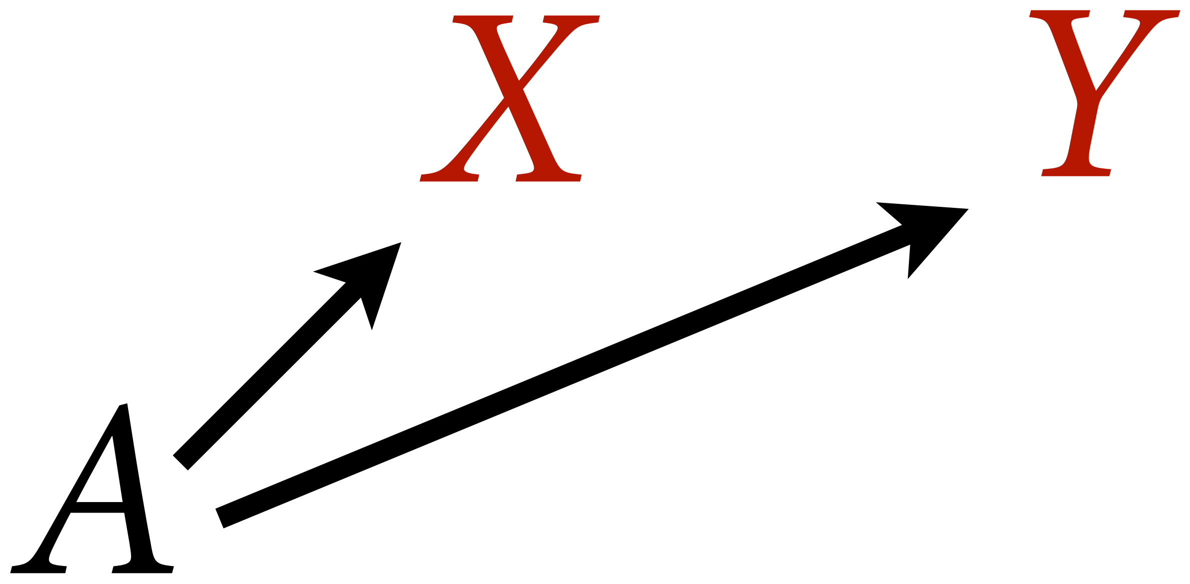


Use Backdoor Criterion

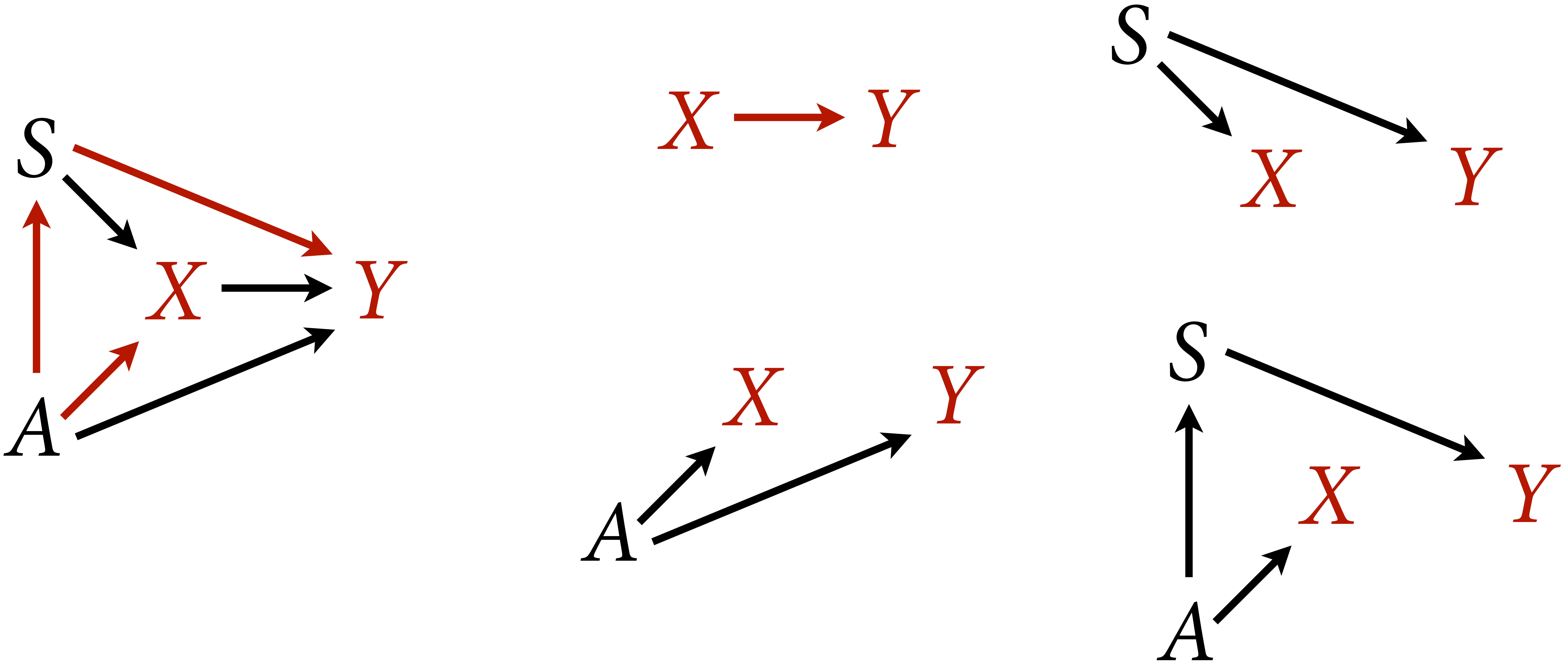




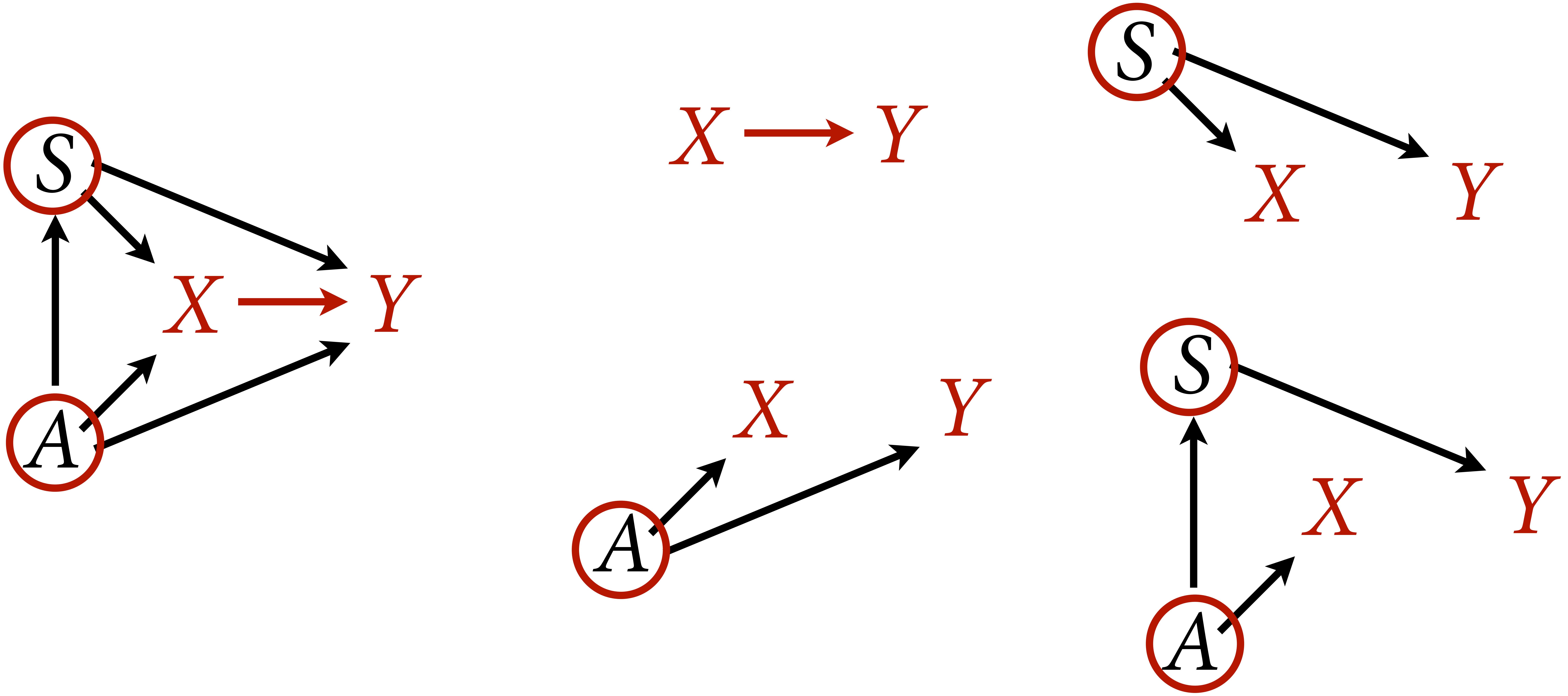


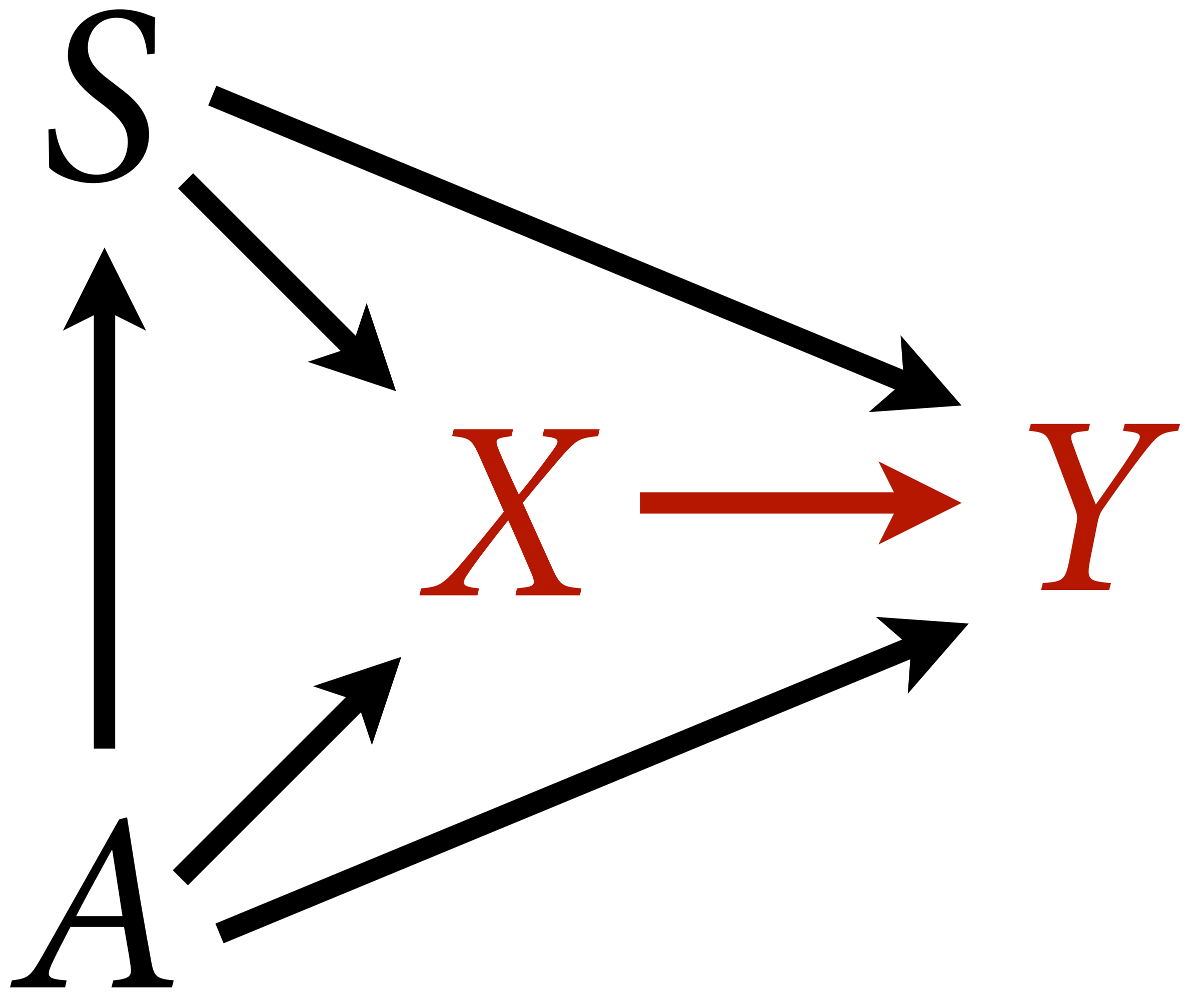


Use Backdoor Criterion



Use Backdoor Criterion

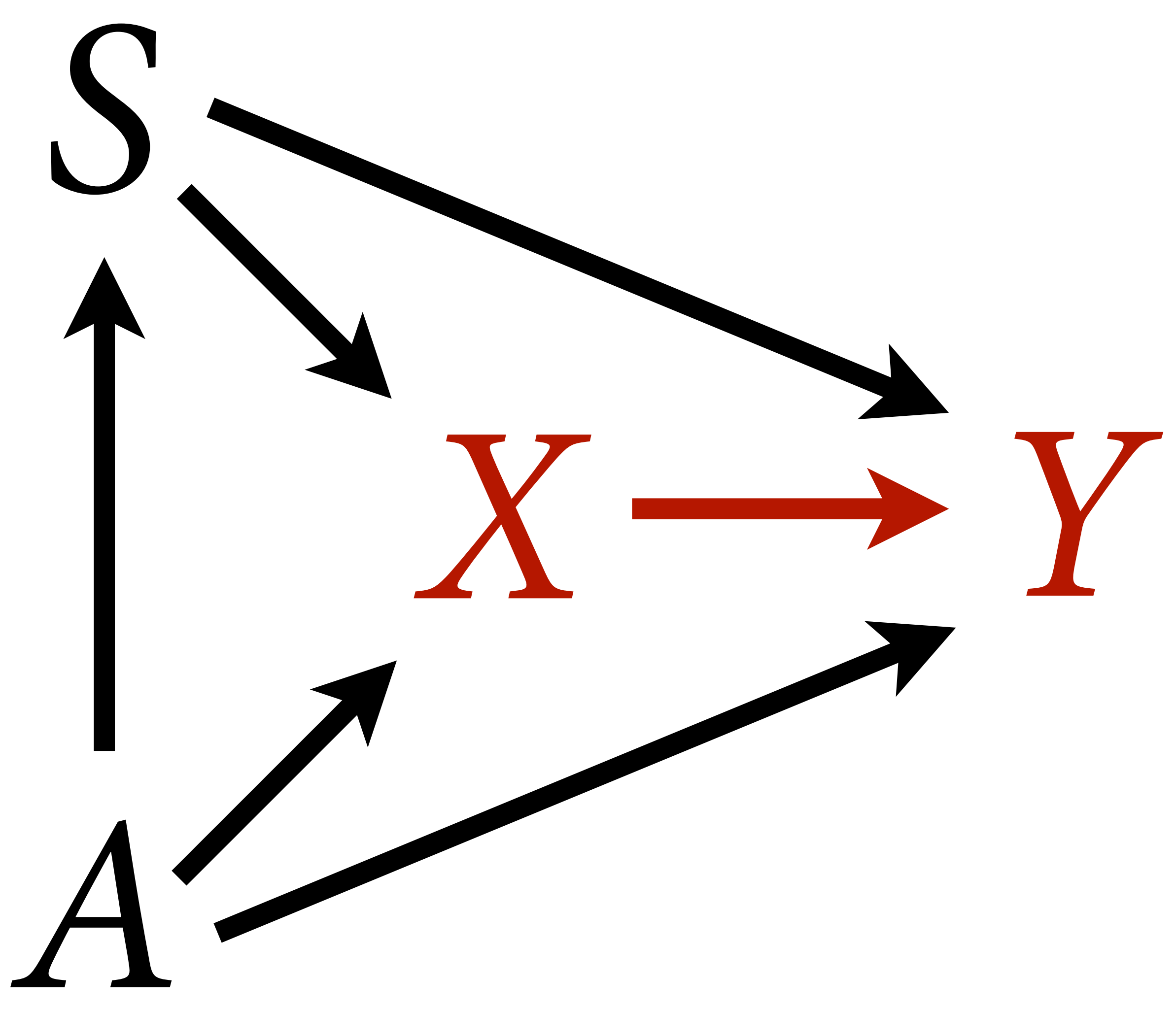




$$Y_i \sim \text{Normal}(\mu_i, \sigma)$$
$$\mu_i = \alpha + \beta_X X_i + \beta_S S_i + \beta_A A_i$$

X

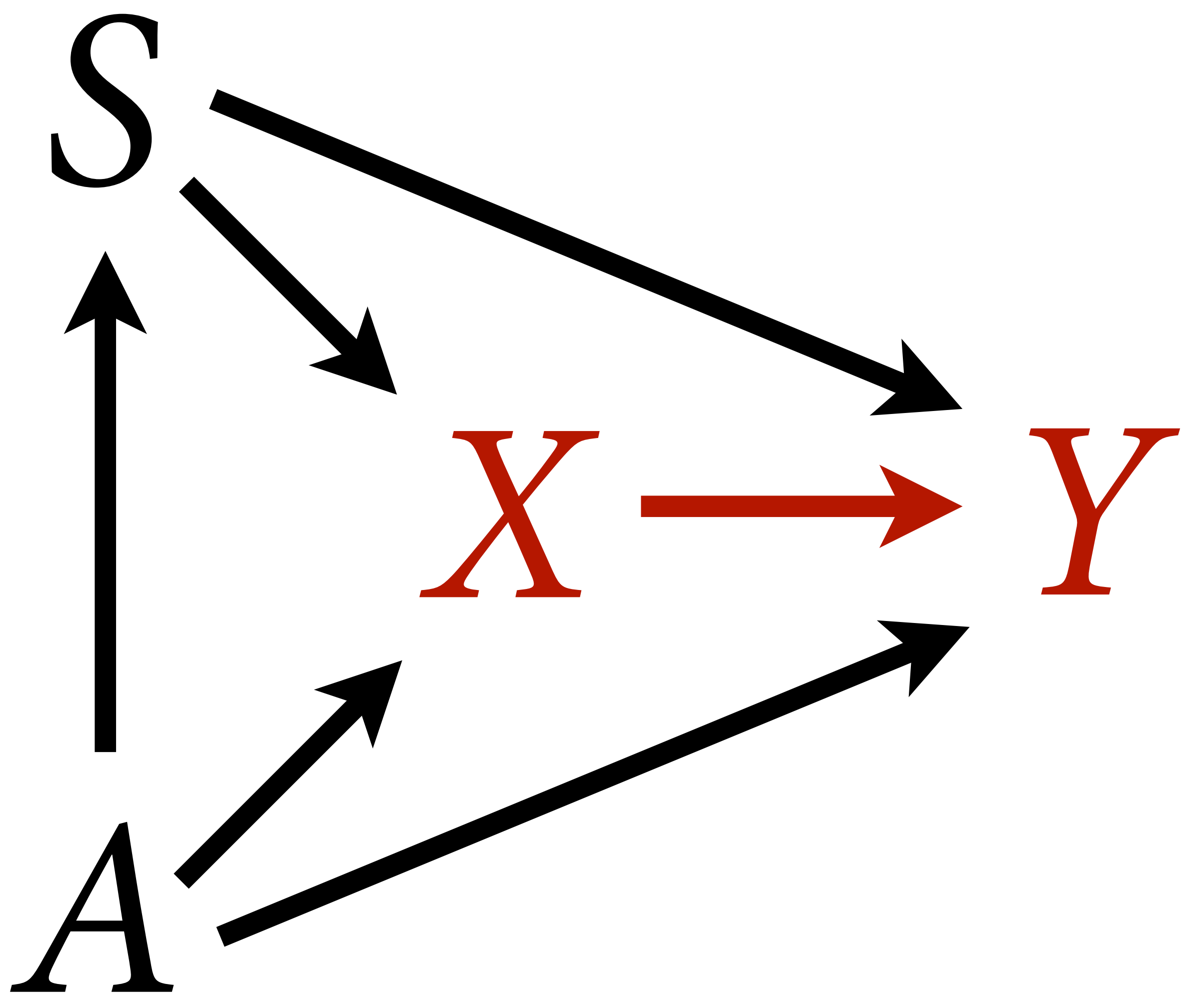
Unconditional



Confounded by A and S

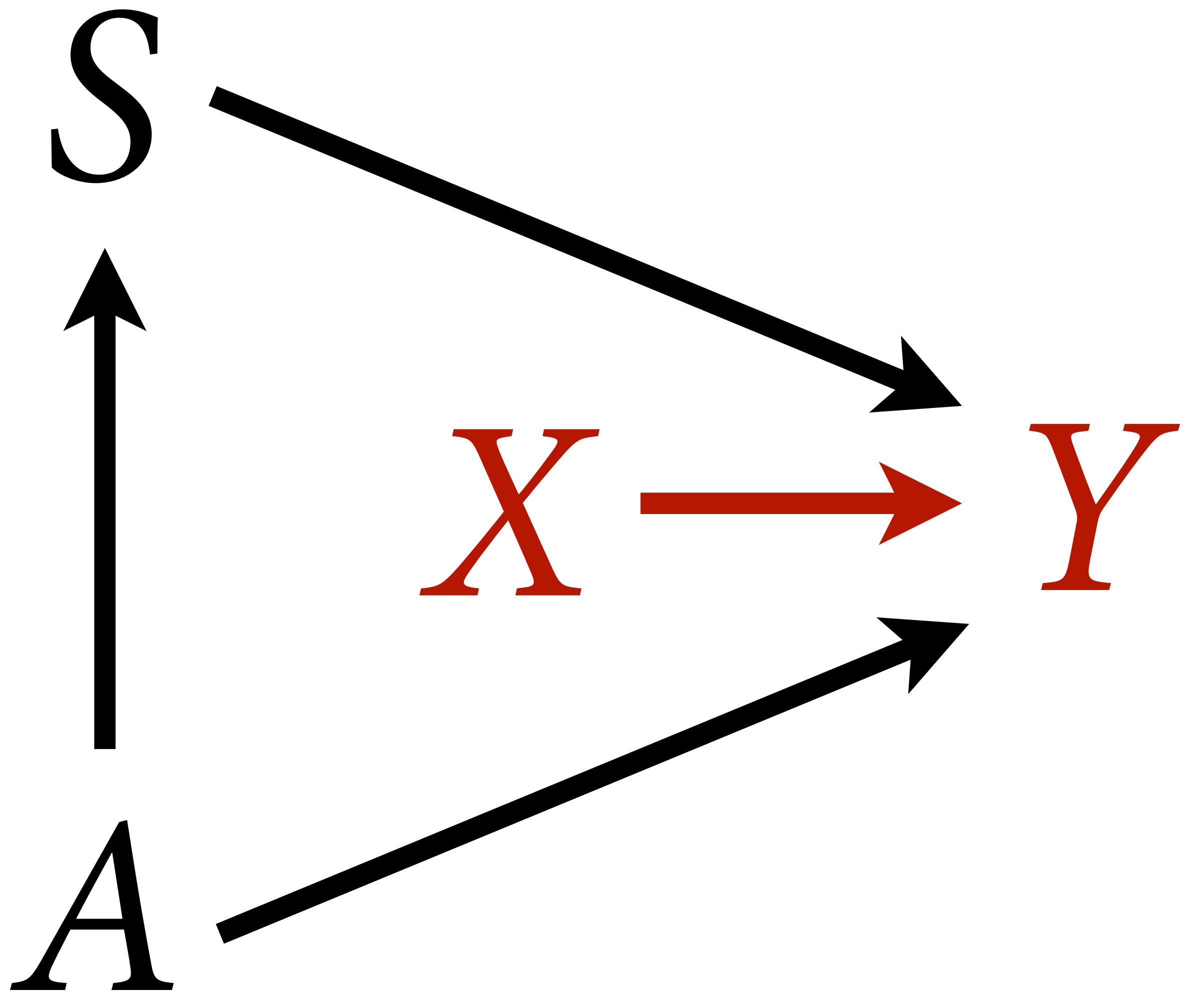
X

Unconditional



Confounded by A and S

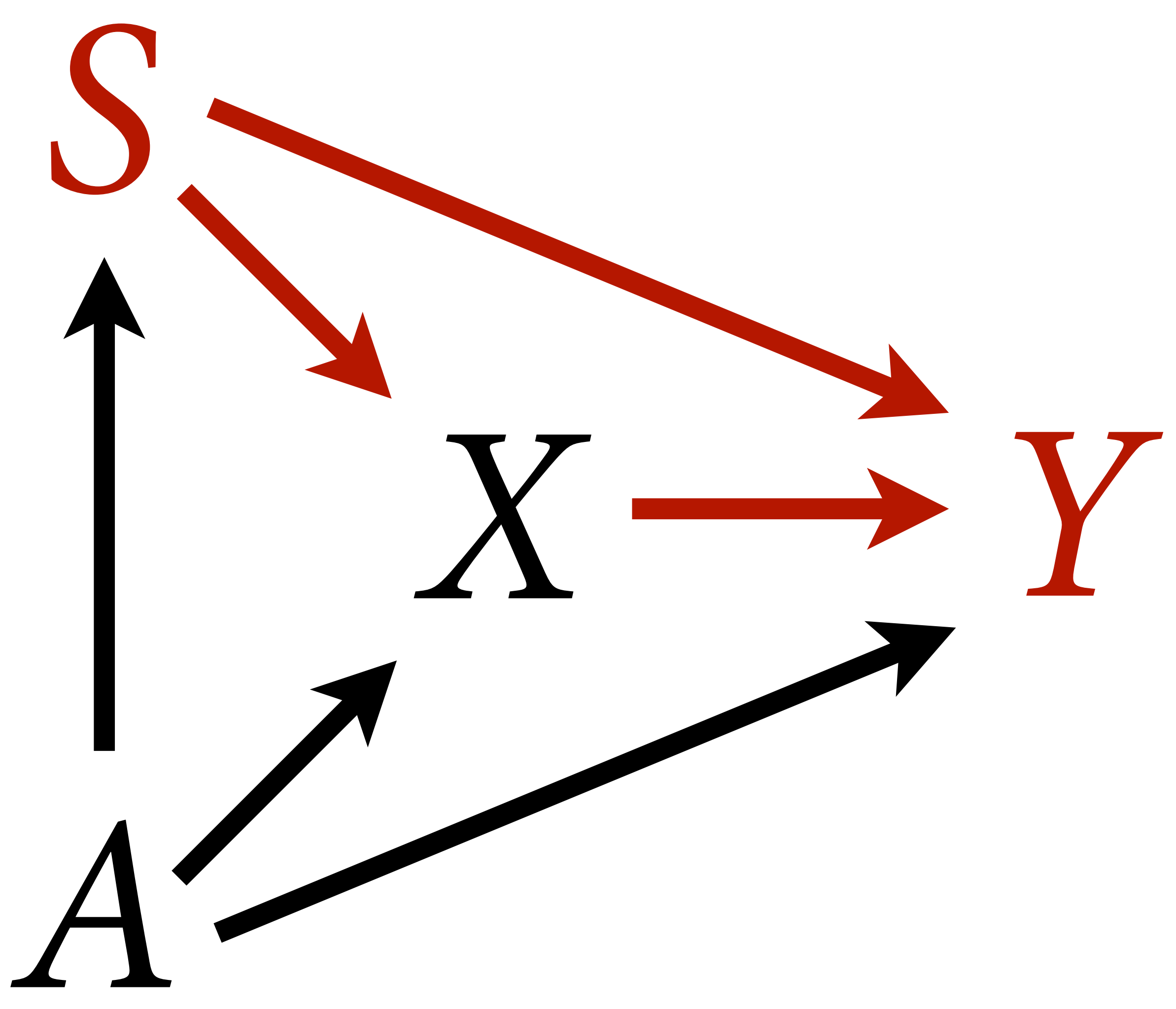
Conditional on A and S



Coefficient for X:
Effect of X on Y
(still must marginalize!)

S

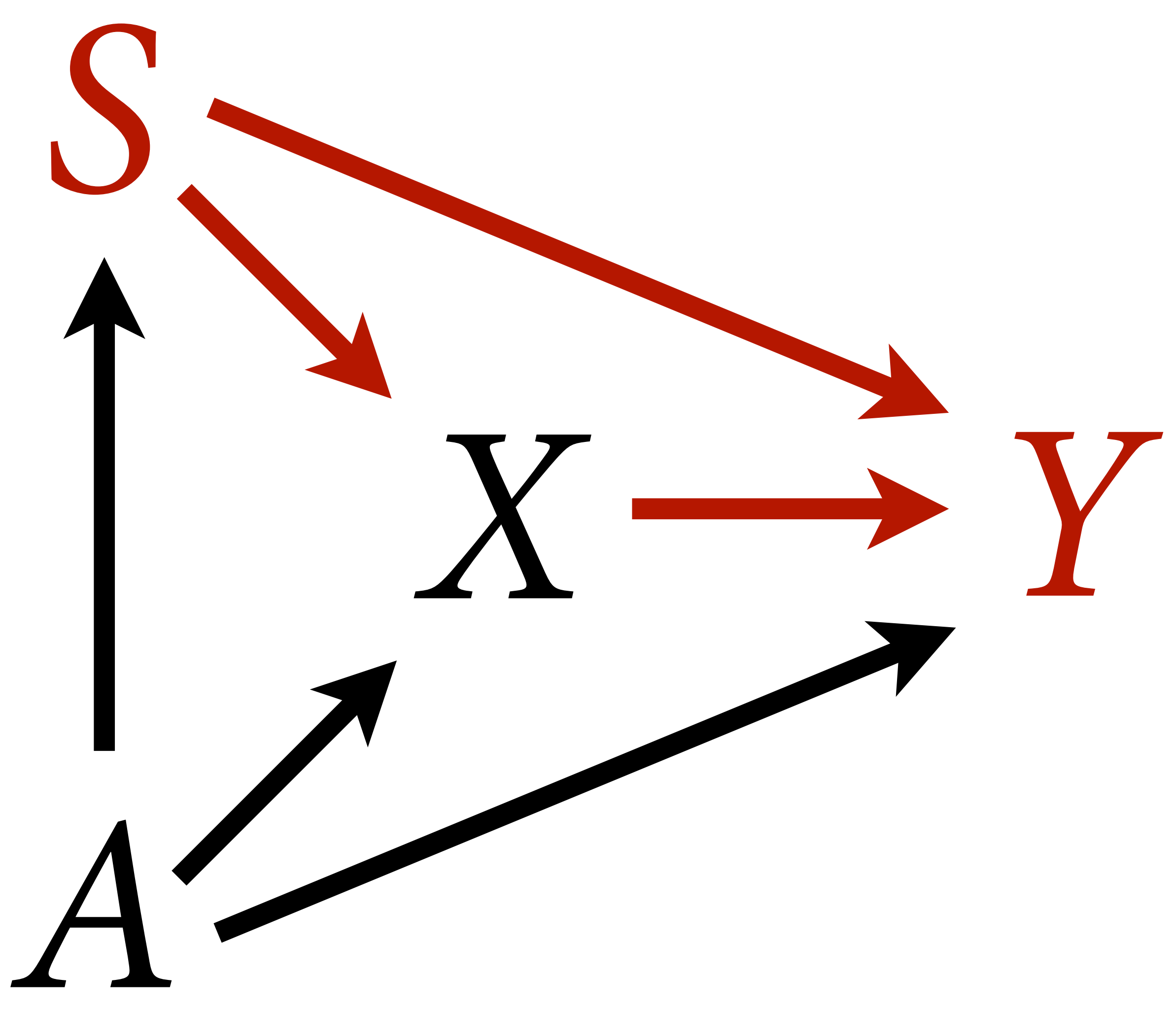
Unconditional



Effect of S
confounded by A

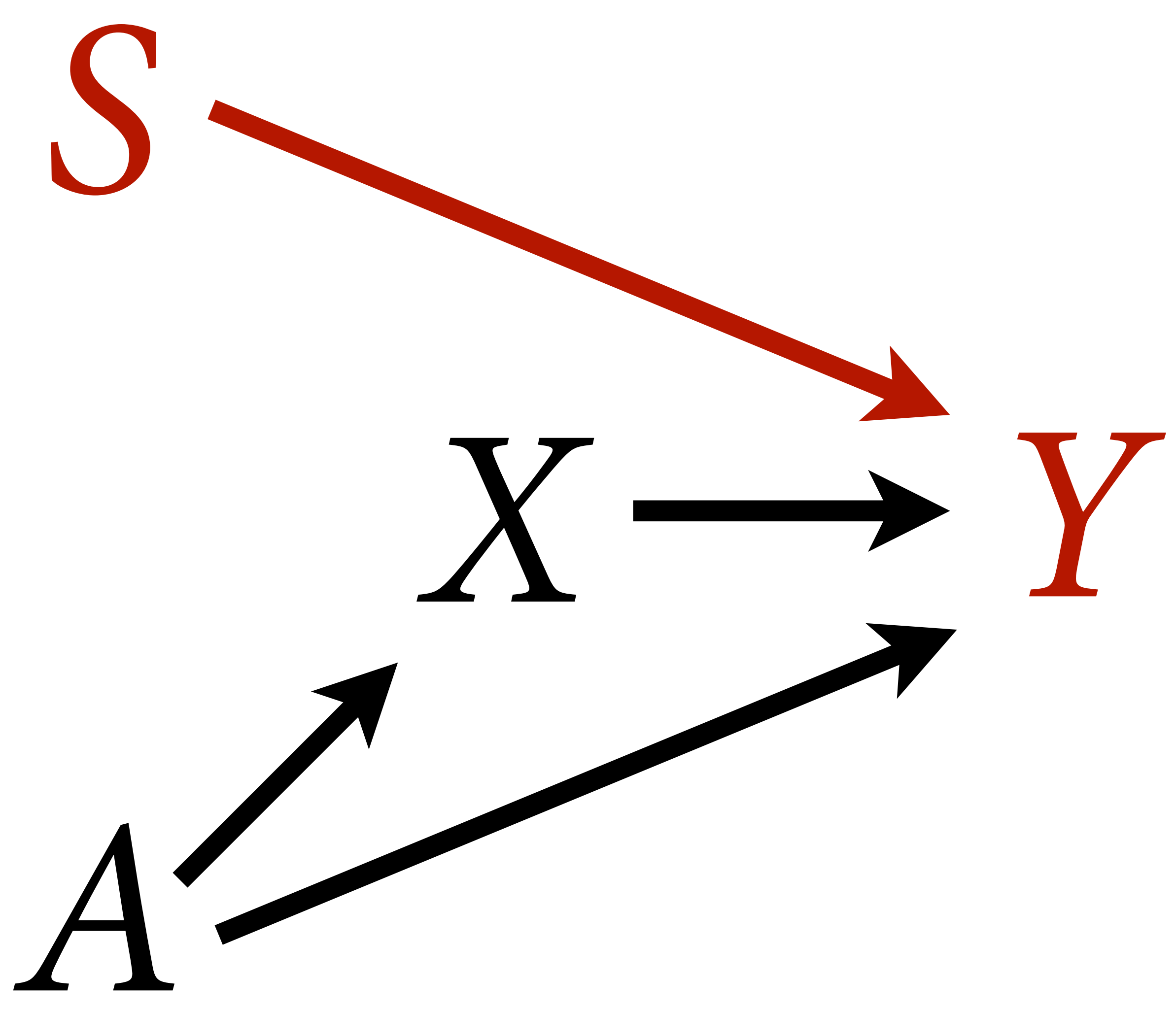
S

Unconditional



Effect of S
confounded by A

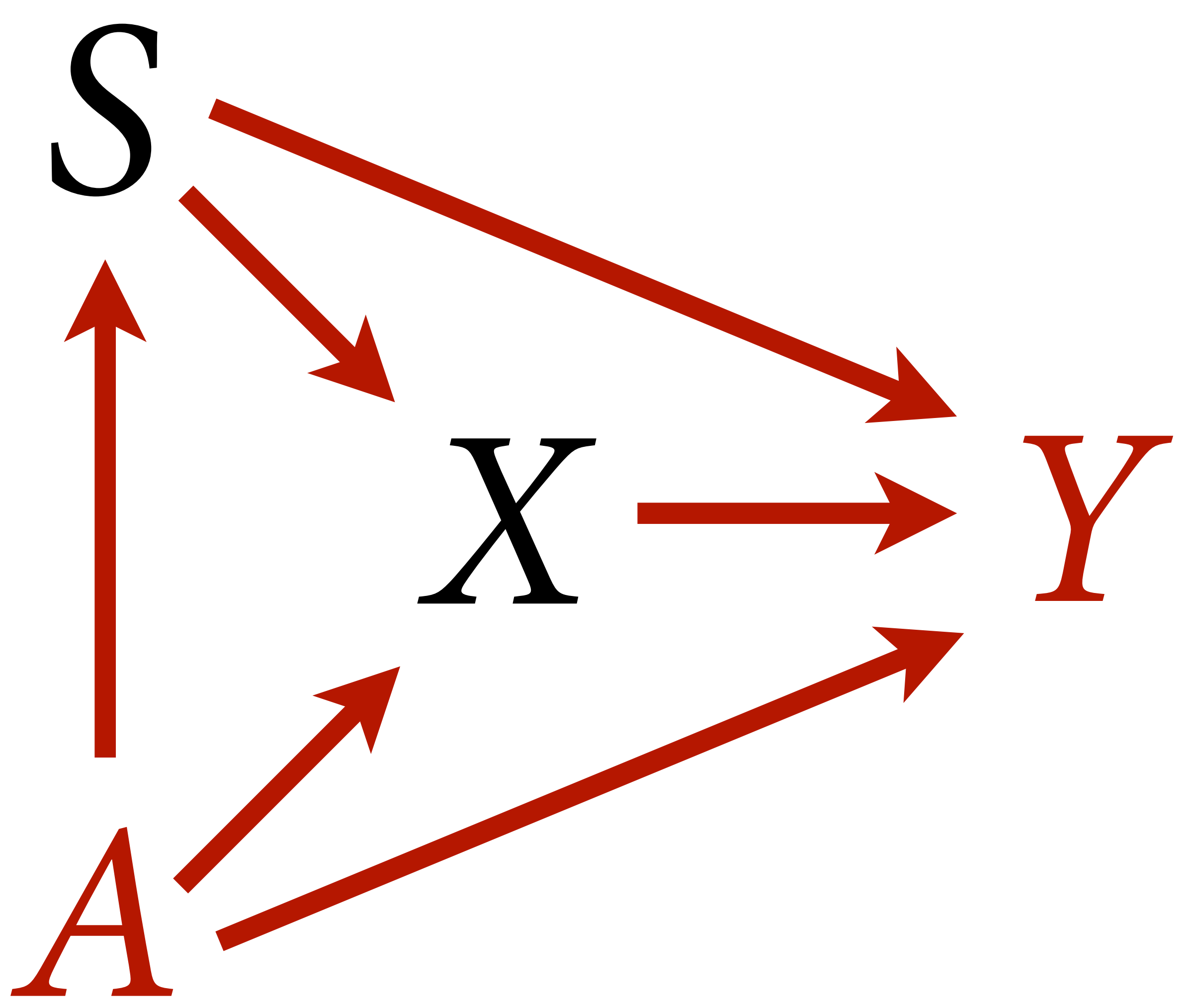
Conditional on A and X



Coefficient for S:
Direct effect of S on Y

A

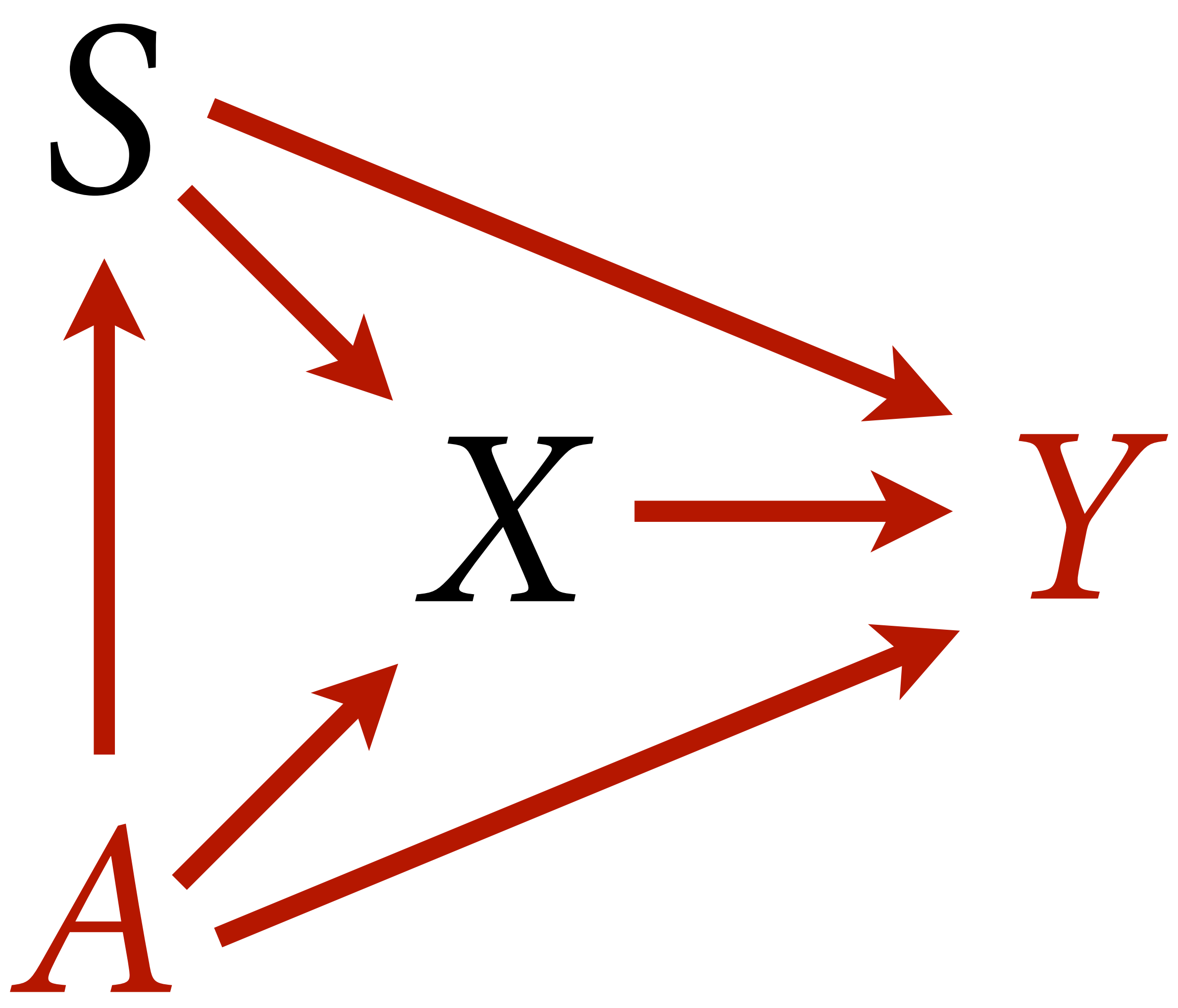
Unconditional



Total causal effect of A on Y flows through all paths

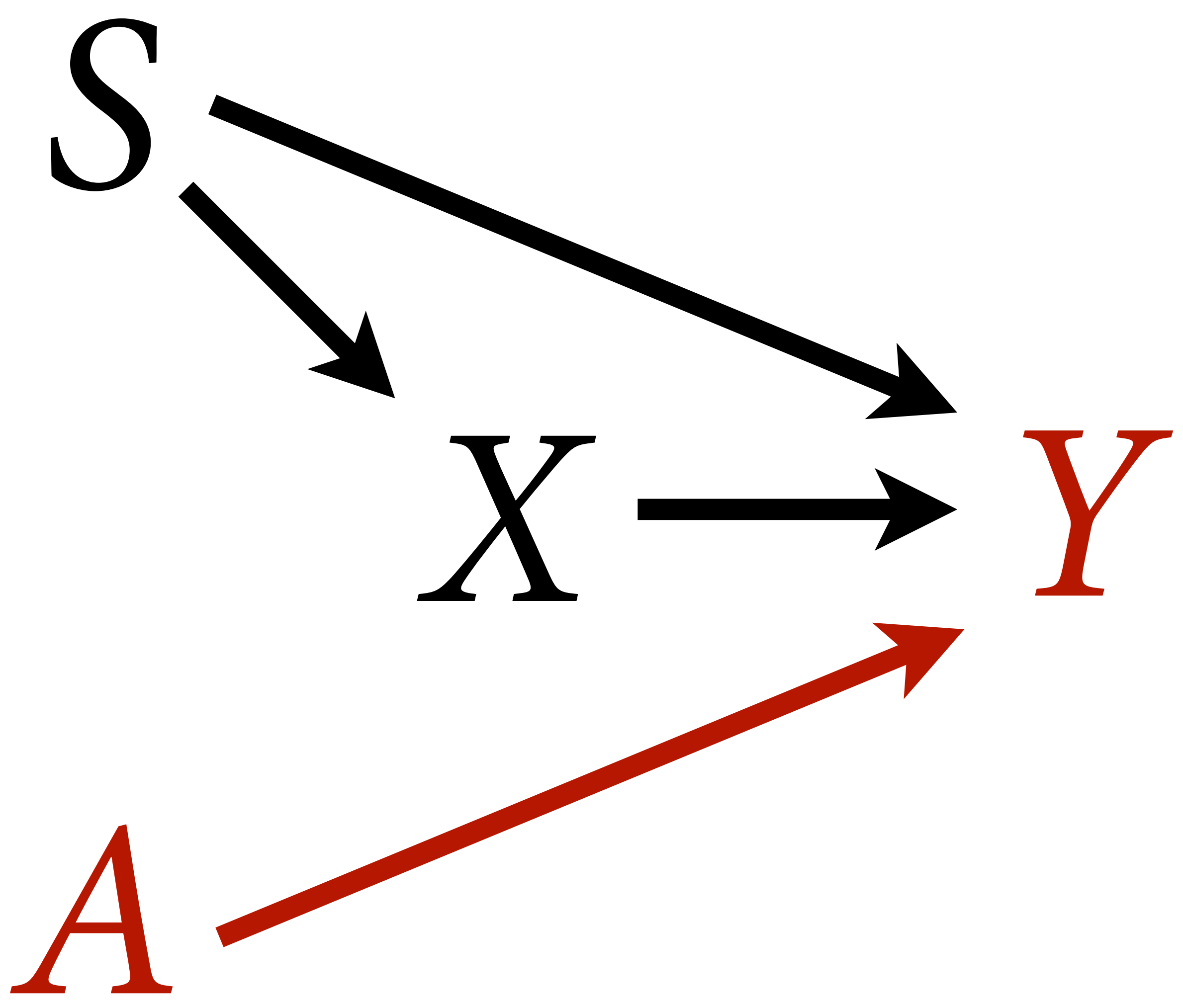
A

Unconditional

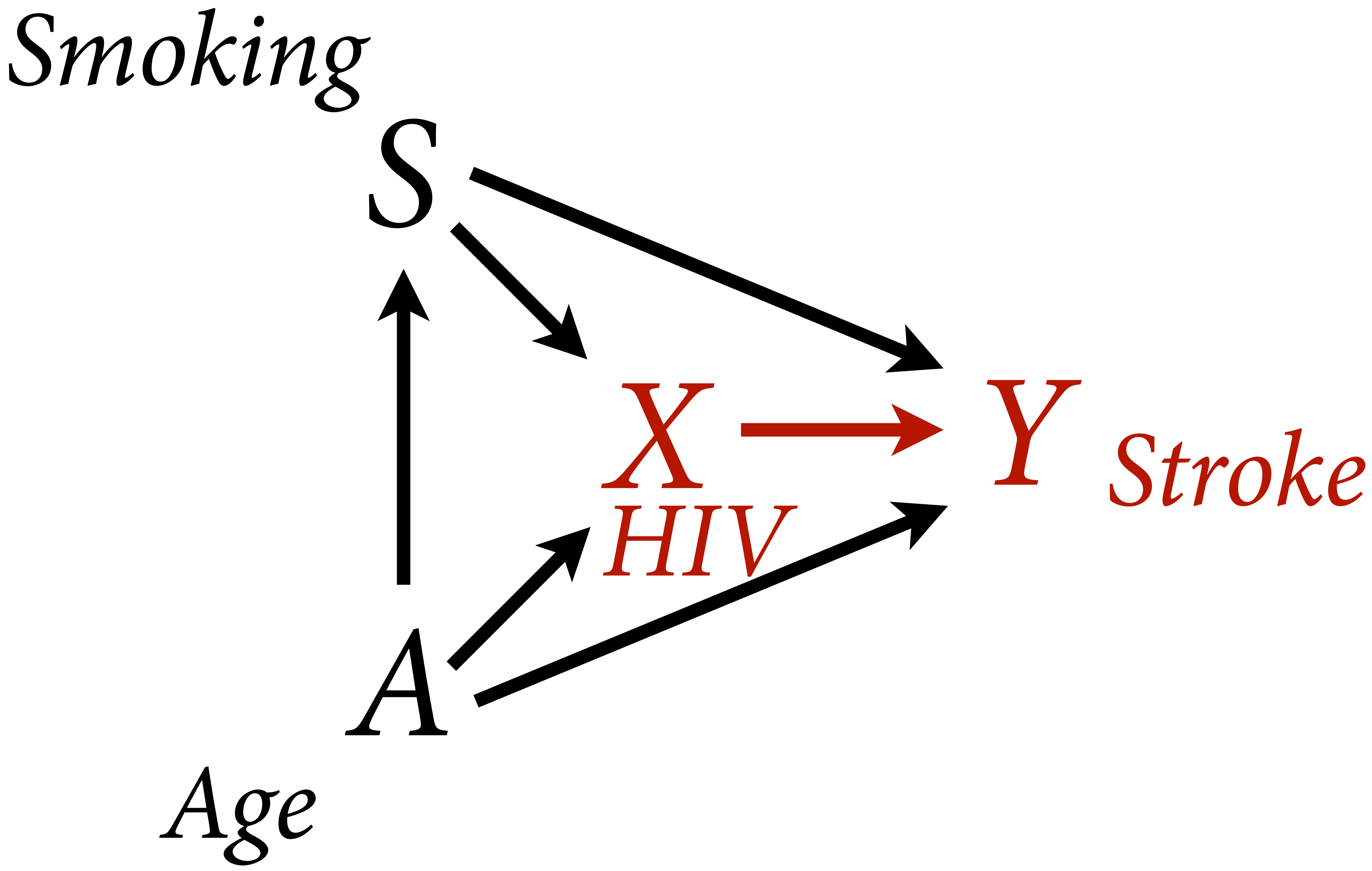


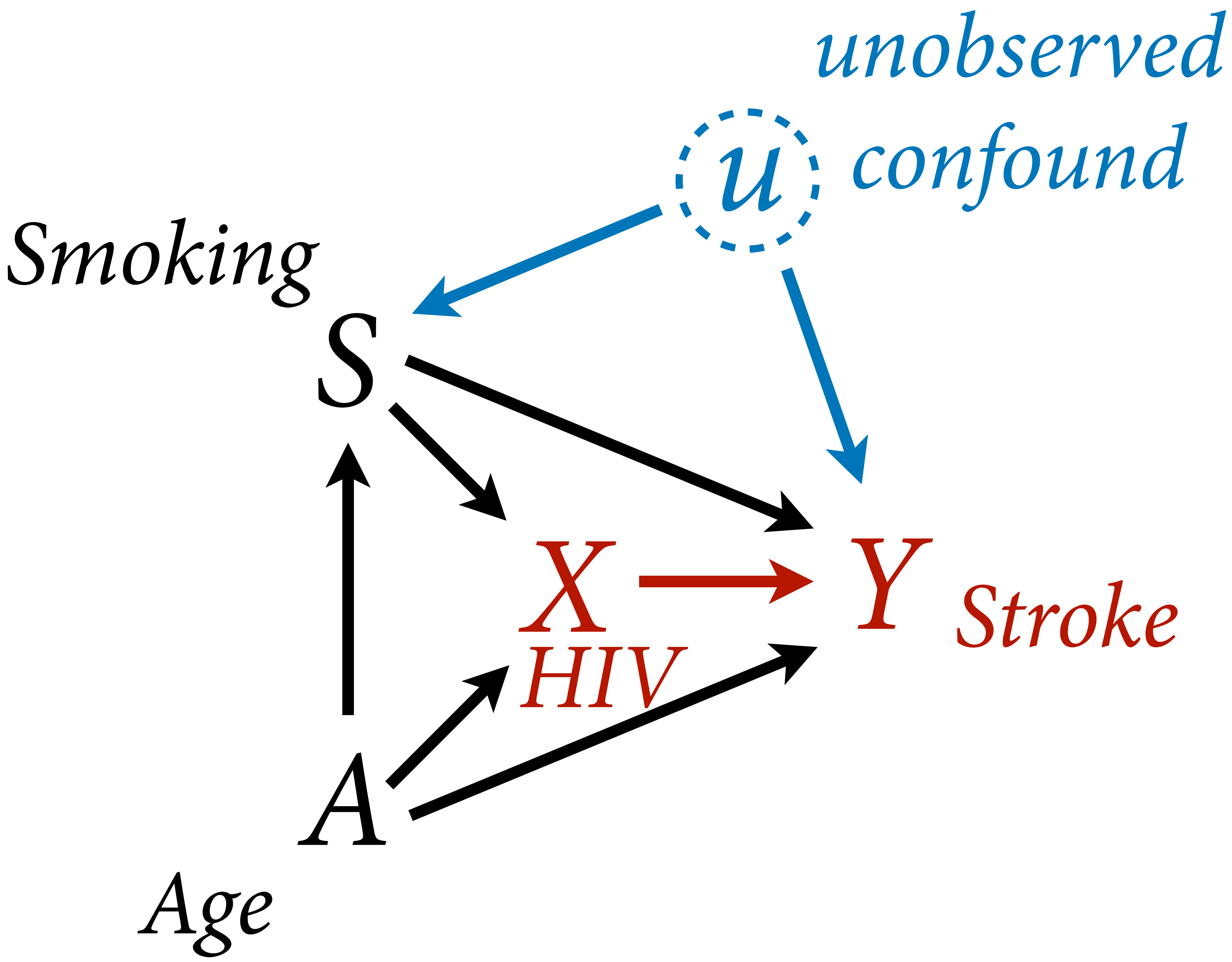
Total causal effect of *A* on *Y* flows through all paths

Conditional on X and S



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


Smoking
S
X
HIV
Y
Stroke
A
Age



unobserved
confound
u
Smoking
S
X
Y
Stroke
HIV
A
Age

Table 2 Fallacy

Not all coefficients created equal

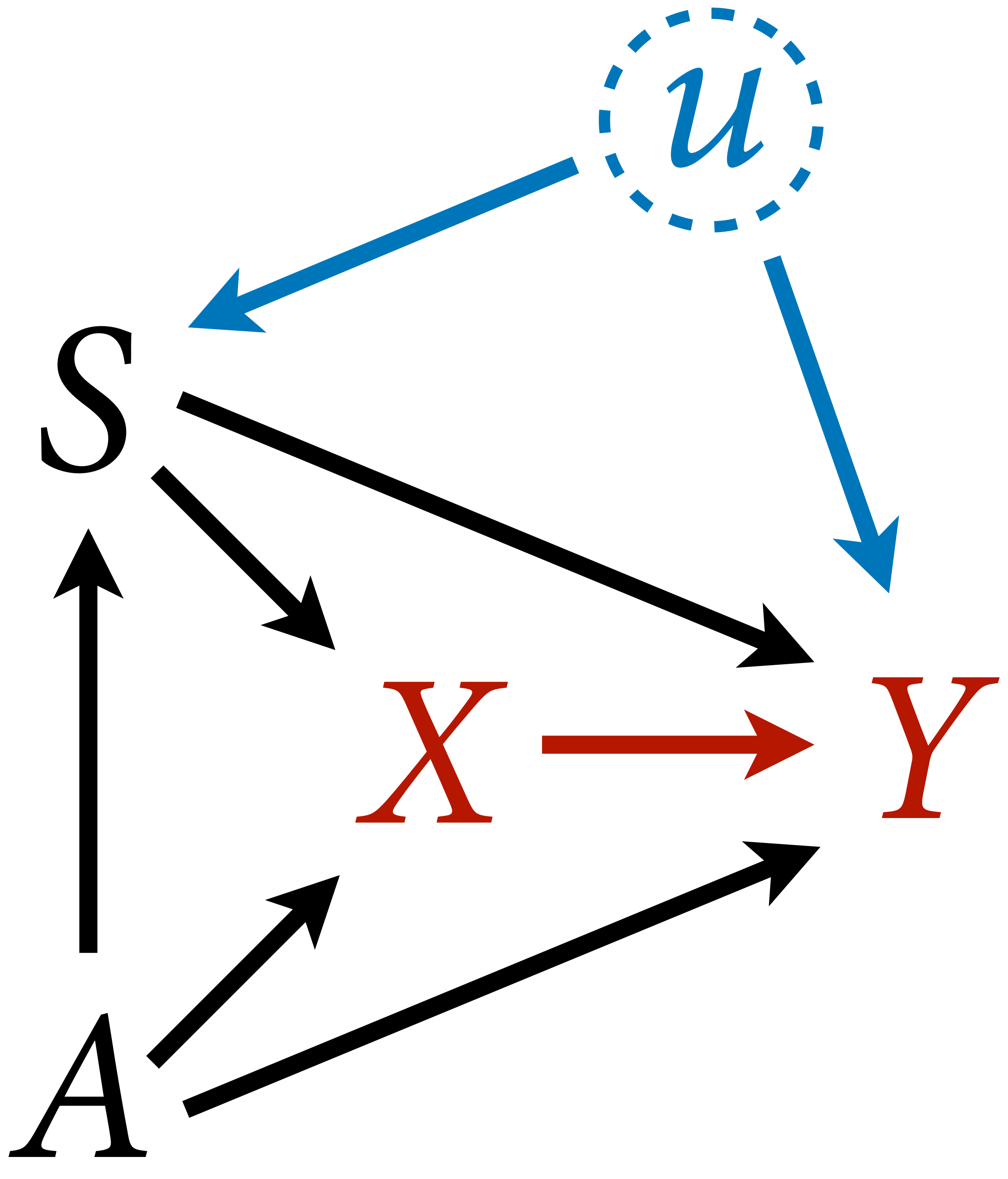
So do not present them as equal

Options:

Do not present control coefficients

Give explicit interpretation of each

No causal model, no interpretation



Imagine Confounding

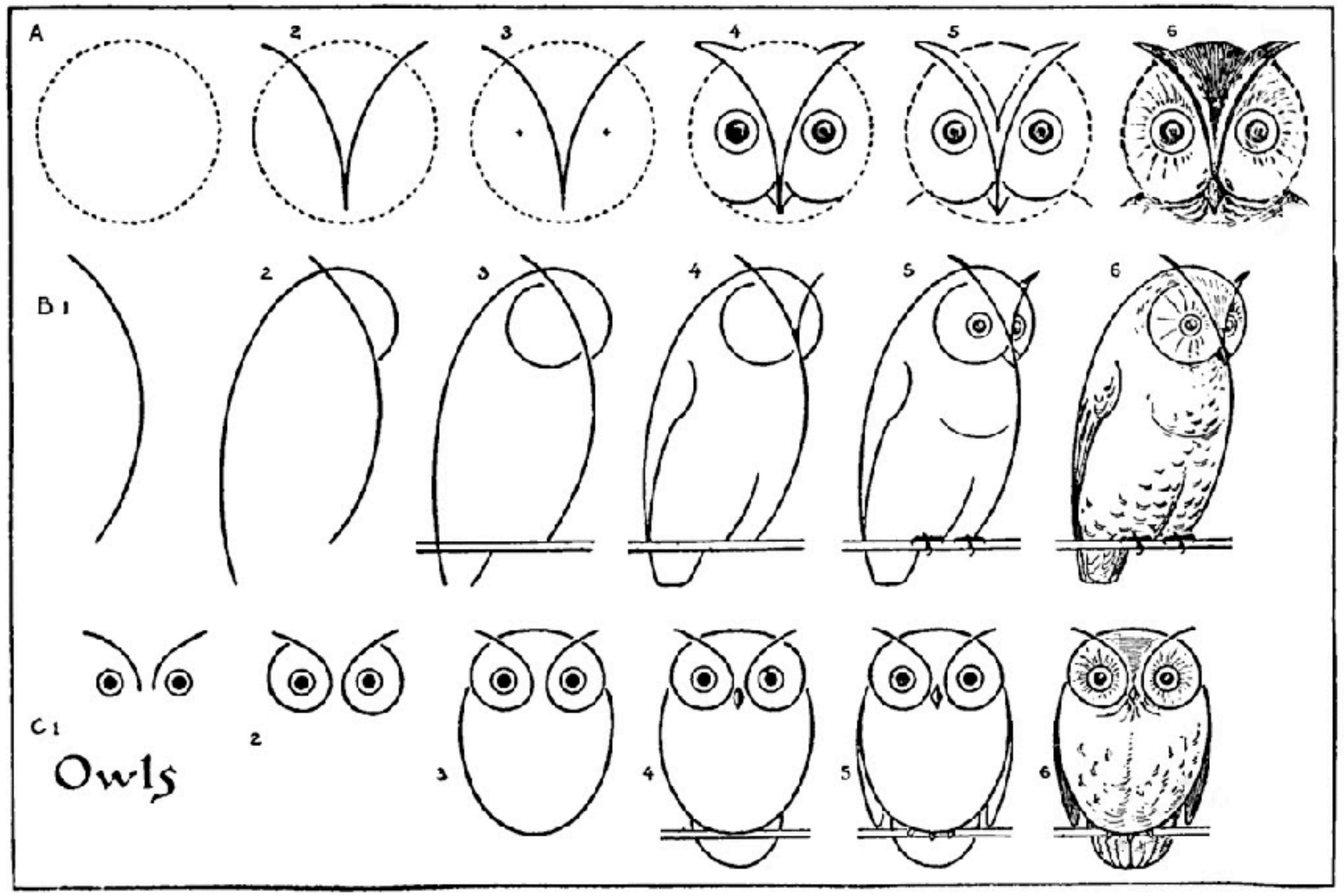
Often we cannot credibly adjust for all confounding

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



Course Schedule

Week 1	Bayesian inference	Chapters 1, 2, 3
Week 2	Linear models & Causal Inference	Chapter 4
Week 3	Causes, Confounds & Colliders	Chapters 5 & 6
Week 4	Overfitting / MCMC	Chapters 7, 8, 9
Week 5	Generalized Linear Models	Chapters 10, 11
Week 6	Integers & Other Monsters	Chapters 11 & 12
Week 7	Multilevel models I	Chapter 13
Week 8	Multilevel models II	Chapter 14
Week 9	Measurement & Missingness	Chapter 15
Week 10	Generalized Linear Madness	Chapter 16

https://github.com/rmcelreath/stat_rethinking_2022