Causal inference for observational data

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The fundamental problem of causal inference and logic of counterfactual inference

Assume student i received tutoring, and had a test score of 82. We'd like to know what this student's score would have been under the *counterfactual* scenario of not receiving tutoring (but we lack a time machine).

For unit i, we only observe $y_i(z = 1)$ *OR* $y_i(z = 0)$, never both. The unobserved value of y is a *potential outcome*, or *counterfactual*.

A counterfactual with observational data?

How can we craft a plausible counterfactual for inference about the effect of a treatment without randomization into treatment?

Assumptions for causal identification

- Clearly defined intervention
- Stable treatment (and counterfactual)
- Temporal ordering
- Ignorability

Some detail on ignorability

The distribution of potential outcomes: $y(z = 1), y(z = 0)$ must be identical across the treatment and control groups

$$y \perp z$$

Randomization, in theory, guarantees this

Ignorability in observational studies

We can satisfy the ignorability assumption by conditioning on *confounding* variables x that may drive variation in y

$$y \perp z | x$$

For causal inference with observational data, we must assume similar distributions of *potential outcomes* across levels of predictors.

Treatment must be as-if random, after conditioning on confounders.

Considerations for ignorability

- Develop a clear understanding of the assignment mechanism
- Balance on observed confounders
- Overlap (empirical support for counterfactual)

We say that a relationship between z and y is *confounded* if there is a third variable x that is associated with both z and y.

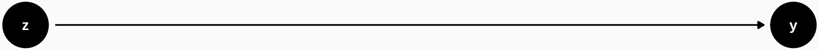
In observational contexts, certain kinds of confounding relationships can *bias* our results if we don't address them properly.

Omitted variable bias is one form of bias, but including the wrong predictors can bias our results too!

Causal graphs

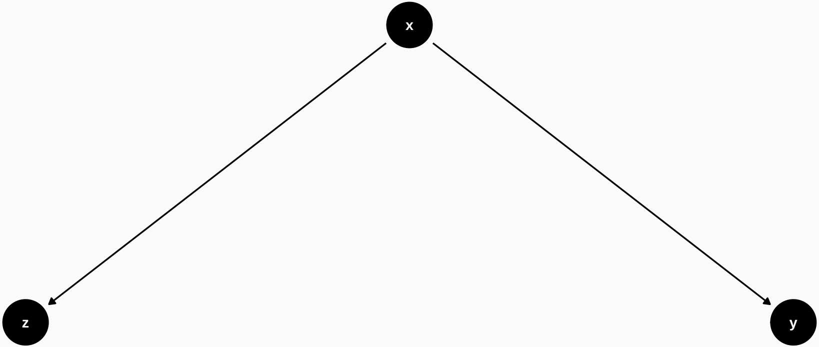
We can use *directed acyclic graphs* (DAGs) to a) explicitly define our causal theory graphically and b) identify appropriate sets of controls to identify a causal effect

DAGs are common in medicine, computer science, increasingly common in social science. Graphical approaches to causality can be contrasted with the *potential outcomes* framework common in economics.



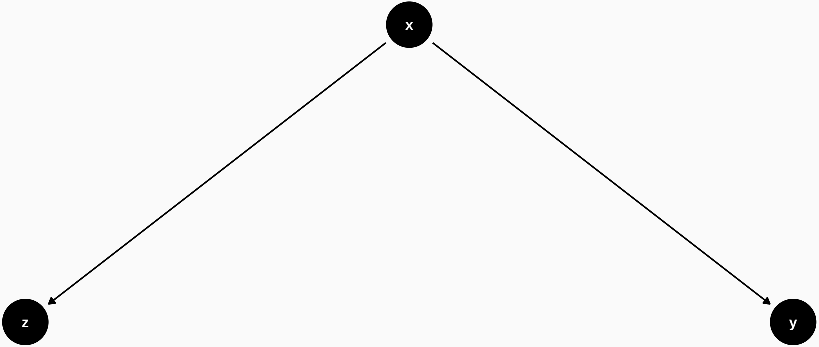
Three common types of confounding: the fork

For the treatment z, outcome y, and confounder x. There is no causal relationship between z and x, but they are associated through their relationship with x.



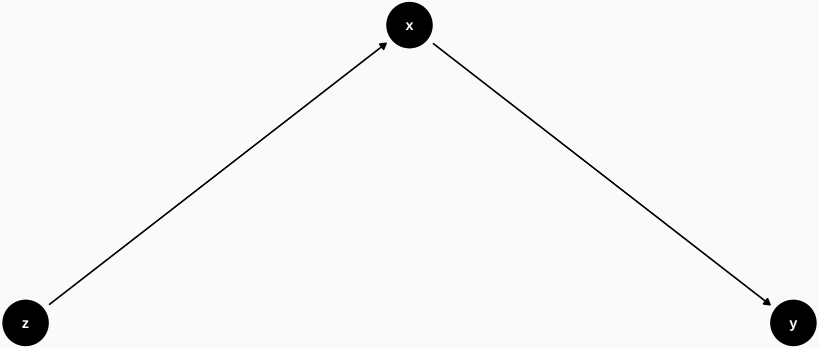
Three common types of confounding: fork

For the treatment z, outcome y, and confounder x. There is no causal relationship between z and x, but they are associated through their relationship with x. If we fail to condition on x, we will obtain a biased estimate of the treatment effect. If we condition on x, we will obtain a valid estimate (no relationship).



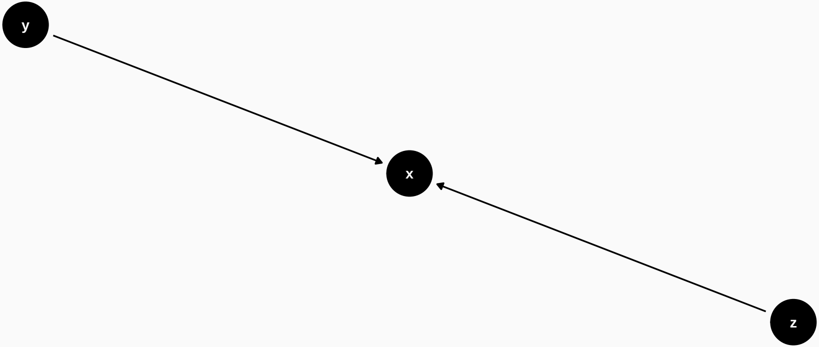
Three common types of confounding: pipe

The causal effect of *z* on *y* is entirely caused by the effect of *z* on a third variable *x*. If we condition on *x*, we won't estimate a relationship between *z* and *y*. This is often called *mediation*, or an *indirect effect* of *z* on *y*.



Three common types of confounding: colliders

There is no causal relationship between *z* and *y*, but both *z* and *y* cause *x*. Conditioning on *x* opens a pathway between *z* and *y*, will result in a statistical association between *y* and *z*. This is often called *selection bias*, and can result in very misleading estimates.

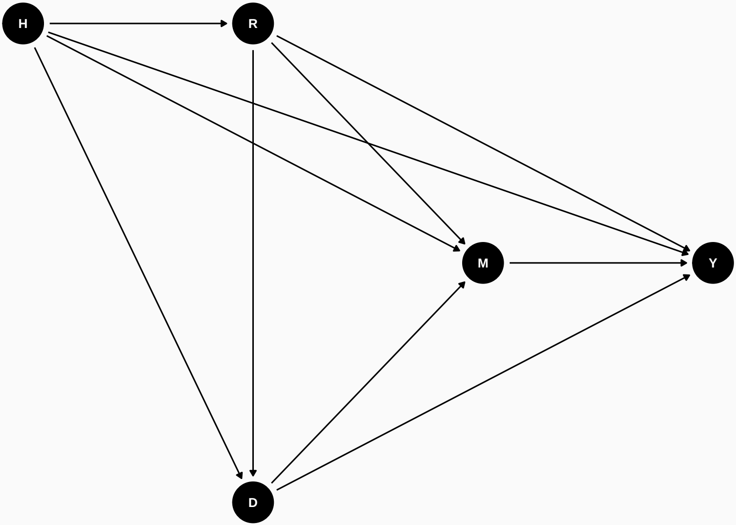


Adjustment sets

We can use DAGs to identify a sufficient set of controls to estimate the effect of z on y. An *adjustment set* closes all *backdoor paths* between confounding variables x and the outcome y.

Causal paths with a fork or pipe are *open*. Causal paths with a collider are *closed*. Conditioning on an open path *blocks* a relationship. Conditioning on a closed path *opens* a relationship.

An example from my research



Adjustment sets and back-door paths

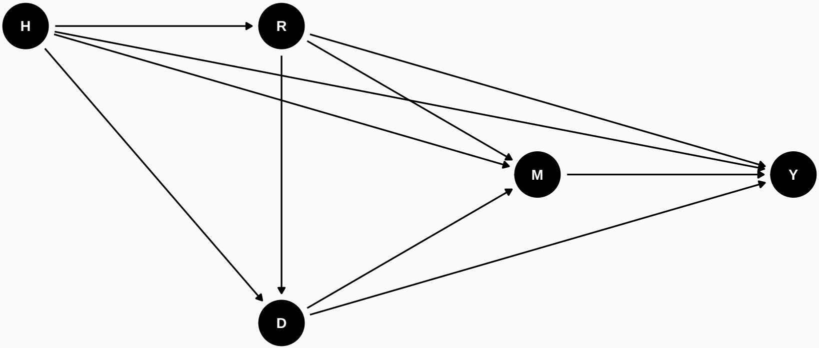
```r
adjustmentSets(g5, effect = "direct")
```

```
## { D, H, M }
```

```r
adjustmentSets(g5, effect = "total")
```

```
## { H }
```

```r
ggdag(g5)
```



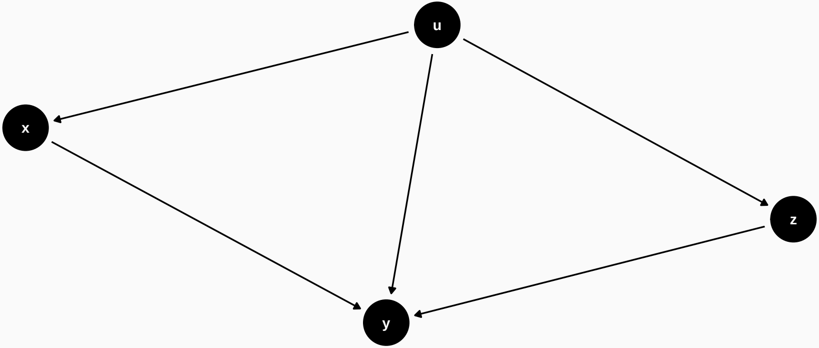
How to proceed when we can measure all confounders

1. Consider what an ideal experiment might look like to clarify your question
2. Draw a DAG
3. Identify adjustment sets to obtain a valid estimate of z on y
4. Identify which causal effect you are interested in (ATE, ATT, ATU, PATE)
5. Decide on a method to produce *ignorability*, like propensity score matching or g-computation
6. Estimate your model(s)

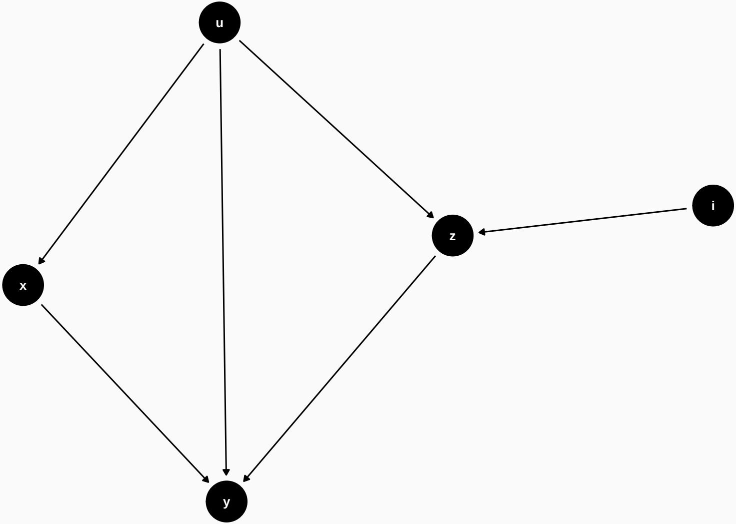
Be very cautious of ‘kitchen sink’ approaches to regression!

Unmeasured confounding

What if we have a DAG like this, where *u* is some unmeasured or unmeasurable confounder?



Econonmetric alternatives: Instrumental variables



Econometric alternatives: Instrumental variables

An *instrumental variable* is associated with exposure to treatment, but not with the outcome. We can use an instrumental variable to emulate randomization on the treatment if we can meet the following assumptions:

- Ignorability of instrument $y(z = 1), y(z = 0) \perp i$
- Monotonicity (one direction of effect)
- Association of instrument with treatment
- Exclusion restriction: no effect of i on y except through z

We can then estimate a *two-stage* regression to estimate the *Local average treatment effect*, the effect of the treatment for those units affected by i

Other econometric approaches to address unmeasured confounding

- Regression discontinuity
- Difference in differences
- Fixed effects