MY457/MY557: Causal Inference for Observational and Experimental Studies

Week 3: Selection on Observables 1

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Experiments and Observational Studies

Randomized experiments are called the gold standard for (internal validity of) causal inference.

But we cannot (should not?) always randomize!

Enter observational studies: Designs where the assignment mechanism is not known or not under researcher's control.

Goal is to design studies such that we believe causal effects are still identified, and understand and evaluate the assumptions underpinning these designs.

Begin with selection on observables – an assumption-heavy design that provides the ground work for much more.

Pre-Treatment Covariates

Pre-treatment covariate:
Any variable X that is predetermined with respect to the treatment D such that the value of X_i for each unit i does not depend on the value of D_i.

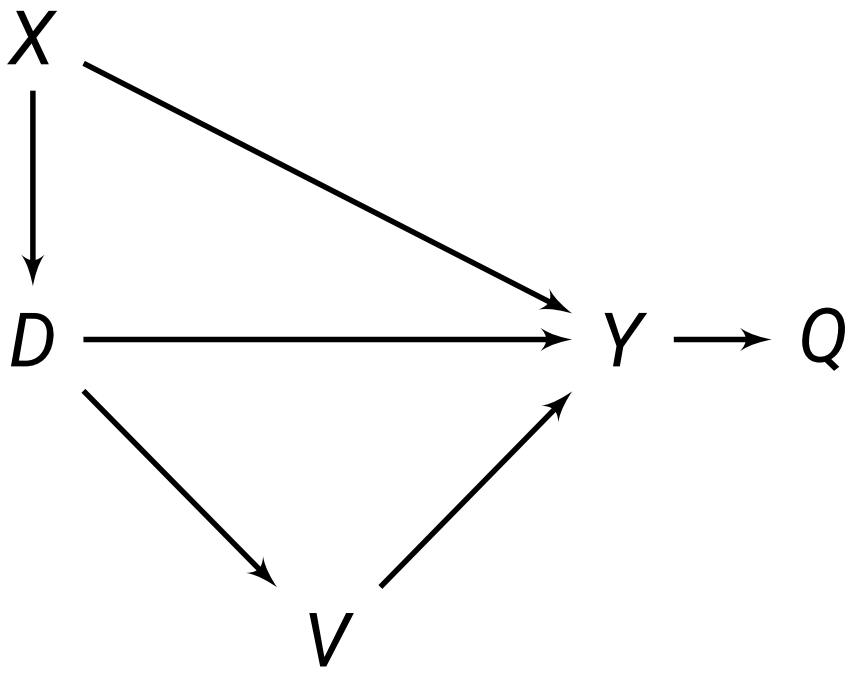
Note: This implies that there are no potential outcomes X_{0i} and X_{1i} with respect to this treatment D, just one value X_i, taken as fixed for the purposes of our analysis.

X and D may still be associated if the treatment assignment for D is associated with or causally affected by X.

X may include characteristics that are immutable (e.g. age) or they may be causally affected by other things (e.g. income).

X may include baseline (pre-treatment) measures of Y.

Pre-Treatment vs. Post-Treatment Covariates



From this perspective, post-treatment covariates are descendents of D. They may be direct descendents (e.g. V above) or indirect descendents, e.g. Q above.

Identification Assumptions

In randomized experiments, D_i satisfies independence (or ignorability):

$$(Y_{0i}, Y_{1i}) \perp\!\!\!\perp D_i$$

What if we cannot assume independence? Instead, we might assume:

1. The conditional ignorability (CI) (a.k.a exogeneity, independence) assumption:

$$(Y_{0i}, Y_{1i}) \perp\!\!\!\perp D_i \mid X_i = x \quad \text{for any} \quad x \in \mathcal{X}$$

Read: Among units with identical values of X_i, D_i is "as-if" random.

2. The common support (a.k.a positivity, overlap) assumption:

$$0 < \Pr(D_i = 1 \mid X_i = x) < 1 \quad \text{for any} \quad x \in \mathcal{X}$$

Read: With any value of X_i, i could have received treatment or control.

Identification Result for ATE

1. Let's first reason about CATEs:

$$
\begin{aligned}
\tau_{CATE}(x) &= \mathbb{E}[Y_{1i} - Y_{0i} \mid X_i = x] \\
&= \mathbb{E}[Y_{1i} \mid X_i = x] - \mathbb{E}[Y_{0i} \mid X_i = x] \\
&= \mathbb{E}[Y_{1i} \mid D_i = 1, X_i = x] - \mathbb{E}[Y_{0i} \mid D_i = 0, X_i = x] \quad \because \text{CI} \\
&= \underbrace{\mathbb{E}[Y_i \mid D_i = 1, X_i = x] - \mathbb{E}[Y_i \mid D_i = 0, X_i = x]}_{\text{conditional difference-in-means}}
\end{aligned}
$$

2. Now, we reason about our true estimand, the ATE:

$$
\begin{aligned}
\tau_{ATE} &= \mathbb{E}[Y_{1i} - Y_{0i}] \\
&= \int \mathbb{E}[Y_{1i} - Y_{0i} \mid X_i = x] \, d\mathbf{P}(X_i) \quad \because \text{common support} \\
&= \int (\mathbb{E}[Y_i \mid D_i = 1, X_i] - \mathbb{E}[Y_i \mid D_i = 0, X_i]) \, d\mathbf{P}(X_i) \\
&\quad \because \text{proof 1 above.}
\end{aligned}
$$

Identification Result for ATE

Result: Under our two assumptions, ATE is nonparametrically identified as:

$$\begin{aligned} \tau_{ATE} &= \mathbb{E}[\hat{\tau}_{CATE}(X_i)] \\ &= \int (\mathbb{E}[Y_i \mid D_i = 1, X_i] - \mathbb{E}[Y_i \mid D_i = 0, X_i])\, d\mathbf{P}(X_i) \end{aligned}$$

where the first $\mathbb{E}$ is taken with respect to the distribution of X_i for all i, $\mathbf{P}(X_i)$.

Read: The ATE is identified as a weighted average of differences in the population regression function for every value of X_i for all i...

$$\hat{\tau}_{CATE}(x) = \mathbb{E}[Y_i \mid D_i = 1, X_i = x] - \mathbb{E}[Y_i \mid D_i = 0, X_i = x]$$

... where the weights correspond to the probability of $X_i = x$.

Identification Result for ATT

ATT is also nonparametrically identified under the conditional ignorability and common support assumptions as:

$$\tau_{ATT} \quad = \quad \mathbb{E}[\hat{\tau}_{CATE}(X_i) \mid D_i = 1]$$

where $\mathbb{E}$ is taken with respect to the distribution of X_i for all i where $D_i = 1$.

However, the identification assumptions may be relaxed for the ATT:

1. $(Y_{0i}) \perp\!\!\!\perp D_i \mid X_i = x$
2. $\Pr(D_i = 1 \mid X_i = x) < 1$ (a.k.a "weak overlap")

Does $\tau_{ATE} = \tau_{ATT}$ necessarily hold when conditional ignorability holds? No!

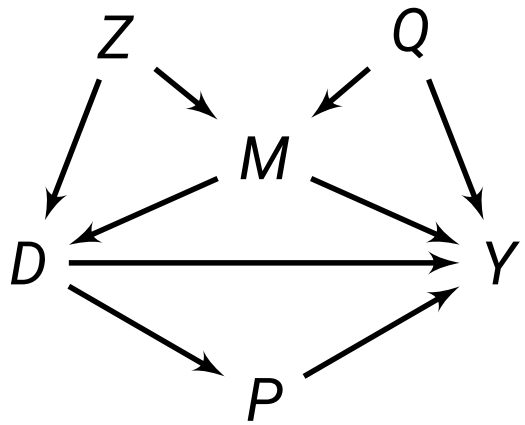
Why? $\mathbb{E}[\hat{\tau}(x) \mid D_i = 1] \neq \mathbb{E}[\hat{\tau}(x)]$ when D_i is not unconditionally random.

Blocked Paths

Blocked paths:

A set of nodes $\{\mathbf{S}\}$ blocks a path $\boldsymbol{p}$ if either

1. $\boldsymbol{p}$ contains at least one *arrow-emitting node* in $\mathbf{S}$, or
2. $\boldsymbol{p}$ contains at least one *collision node* that is outside $\mathbf{S}$ and has no descendant in $\mathbf{S}$.

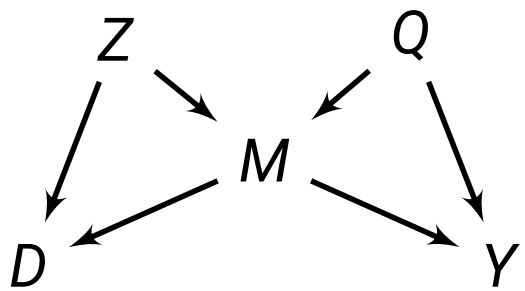


The path $D \rightarrow P \rightarrow Y$ is blocked by $\{P\}$
The path $D \leftarrow M \rightarrow Y$ is blocked by $\{M\}$
The path $D \leftarrow Z \rightarrow M \rightarrow Y$ is blocked by $\{M\}$ or $\{Z\}$ or $\{Z, M\}$
The path $D \leftarrow Z \rightarrow M \leftarrow Q \rightarrow Y$ is blocked by $\{Z\}$ or $\{Q\}$ or $\{\varnothing\}$

d-Separation

d-separation:

1. If **S** *blocks all paths* from D to Y, then **S** d-separates D and Y.
2. If **S** d-separates D and Y, then $D \perp\!\!\!\perp Y \mid \mathbf{S}$.



D and Y are d-separated by $\{Z, M\}$ or $\{Q, M\}$ or $\{Z, Q, M\}$.

The Back-Door Criterion for Causal Identification

The graphical concept of ***d***-separation corresponds to the statistical concept of conditional independence.

Back-door criterion (Pearl, 2000):

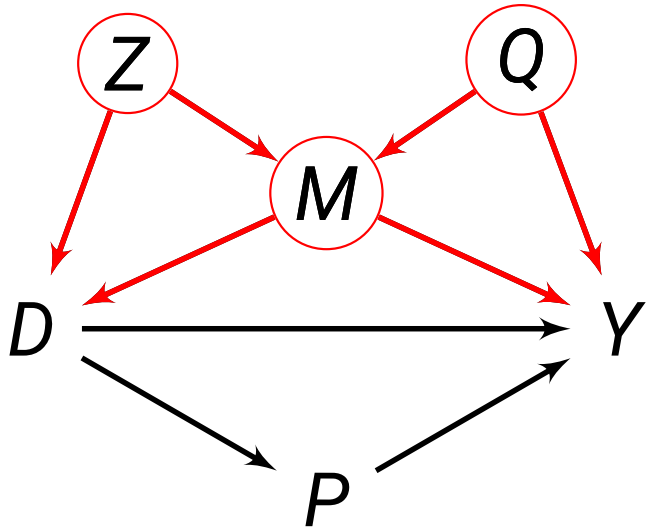
A set **S** is sufficient for adjustment to identify the causal effect of ***X*** on ***Y*** if:

1. No element of **S** is a descendant of ***X***, and
2. The elements of **S** *block all back-door paths* from ***X*** to ***Y***

Note: Pearl (2000) also gives us a front-door criterion for identification, but it is hard to find effective examples in the real world, so we won't dive deeper now. See Glynn & Kashin (2017) and Bellemare et al. (2024).

Identification via Back-Door Criterion: Example

Consider again our DAG:



What conditioning set(s) identify the total effect of D on Y?

$\{Z, M\}$ or $\{M, Q\}$ or $\{Z, Q, M\}$.

Why not $\{M\}$? Only $\{M\}$ opens a back-door path due to the collider M.

Similarly, only $\{Z, Q\}$ (or either alone) leaves a back-door path open.

Good Control, Bad Control?

The graphical framework provides some insights that aren't always apparent when using potential outcomes.

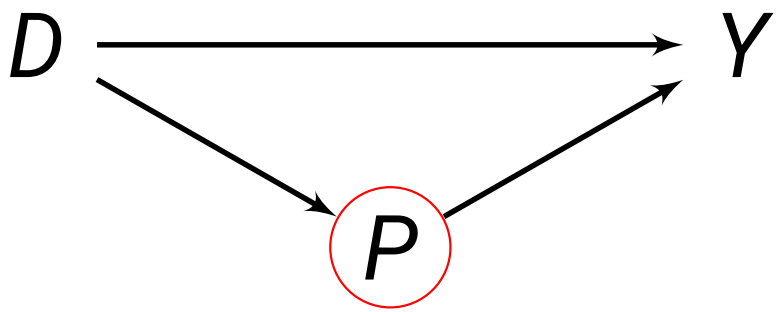
One set of insights relates to whether particular controls are "good", "bad", or "neutral" in terms of identification and efficiency.

Cinelli et al. (2022) provide a survey of multiple example models that demonstrate cases of good, bad, and neutral controls. Very useful reference!

Good controls tend to be those that block backdoor paths (facilitating identification). Good controls can also be those that improve precision (regardless of identification).

Note: These insights assume our DAG is (close to) correct!

Good, Bad, or Neutral?



This is a bad control, a case of overcontrol (or post-treatment) bias. Why?

The total effect (τ_{ATE}) is a combination of $D \rightarrow Y$ and $D \rightarrow P \rightarrow Y$. By adjusting for P we instead identify the controlled direct effect: $D \rightarrow Y$.

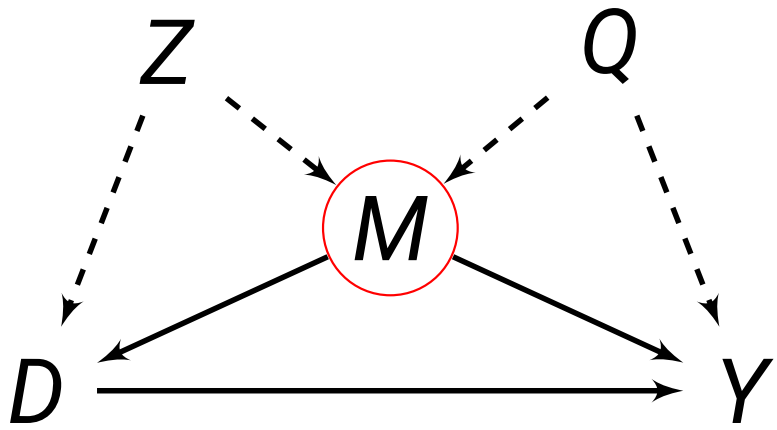
This might be a useful quantity, but it requires our DAG to be correct! See e.g. Acharya et al. (2016) for more.

Good, Bad, or Neutral?

This is a neutral control that may improve efficiency. Why?

In this DAG, Q affects Y, but is unrelated to D. By conditioning on Q we control away noise in Y. All that remains is variation in Y that is induced by D, so efficiency may improve.

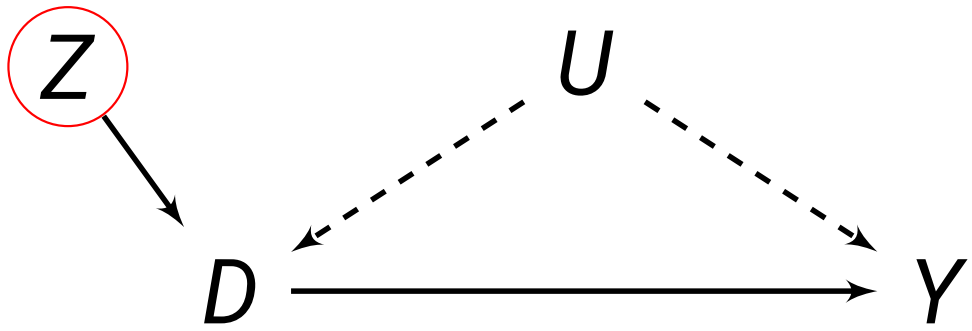
Good, Bad, or Neutral?



This is a bad control, a case of M-bias. Why?

As we saw earlier, by adjusting for M we open a back-door path that was otherwise blocked! In this DAG, no observable conditioning set identifies $D \rightarrow Y$.

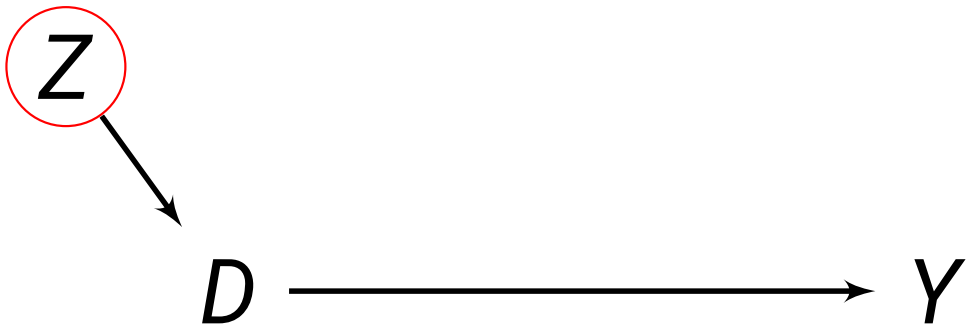
Good, Bad, or Neutral?



This is a bad control, a case of bias amplification. Why?

In this DAG, Z sets D exogenously (to Y). By conditioning on Z we control away exogenous variation in D. All that remains is endogenous variation in D, and so the confounding effect of U is amplified.

Good, Bad, or Neutral?

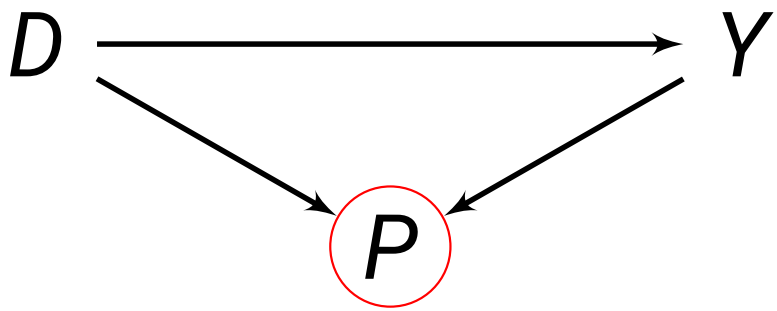


This is a neutral control that may harm efficiency. Why?

In this DAG, *Z* sets *D* exogenously (to *Y*). By conditioning on *Z* we do not threaten identification, but we control away "inferentially helpful" variation in *D* (and by implication *Y*).

General rule of thumb #1: Controlling for predictors of *D* is much less helpful (often harmful) than controlling for predictors of *Y*.

Good, Bad, or Neutral?



This is a bad control, a case of collider stratification bias. Why?

In this DAG, *D* and *Y* both set *P*. By conditioning on *P* we open a back-door path.

General rule of thumb #2: Don't condition on descendents of *D* (post-treatment covariates). There are some instances where this can be appropriate, but they are few and far between.

What Next? From Identification to Estimation

Today we learned the identification assumptions required for selection on observables as a causal research design.

Also learned about different types of covariates and some of the (often non-obvious) implications of conditioning on them.

Next week we will study four broad approaches to estimating causal estimands under selection on observables:

1. Subclassification
2. Matching
3. Weighting
4. Regression