Research Design and Causal Analysis with R

Data Science Summer School
Julian Schuessler

Post-Doc, Institut for Statskundskab, Aarhus Universitet

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Section 1

Intro

Modus operandi

- I'm Julian, pol sci post-doc @ Aarhus U
  - Causal inference methods, experiments, public opinion, discrimination
- *Ask questions*...in the chat
- Dedicated Q&A slot after lunch break
- 10–12 w/ short breaks in between; 12.15–14.15
- Polls & pen & paper & R
- Your own R or colab
  - In colab: `install.packages` now!

Topics

- Choosing control variables (d-separation, back-door criterion, post-treatment bias)
- Sensitivity of OLS estimates to unobserved confounding (in `R`)
- Basic mediation analysis (causal mechanisms) (in `R`)
- Instrumental variables
- All with *causal graphs*, often incl. *sensitivity analysis*

Section 2

Control Variables: Yes or No?

Control Variables: Yes or No?

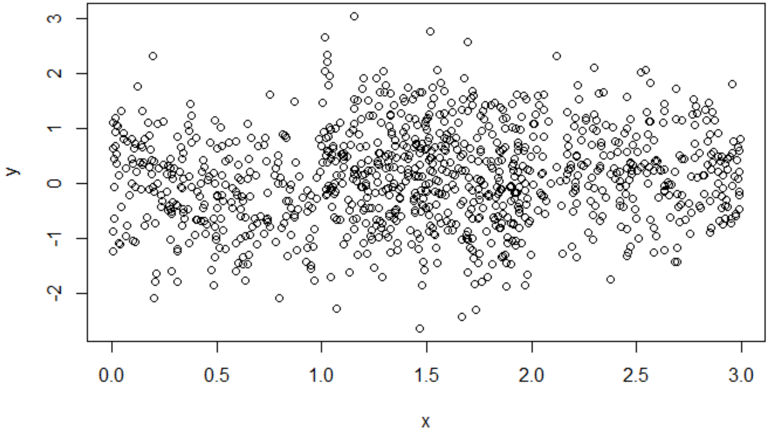


Figure: What a nice Scatterplot

Running the Regression

```
summary(lm(y ~ x))
```

	Dependent variable:
	y
x	0.110***
	(0.035)
Constant	−0.064
	(0.059)
Observations	1,000
R^2	0.010
Adjusted R^2	0.009
Residual Std. Error	0.863 (df = 998)
F Statistic	10.169*** (df = 1; 998)
Note:	*p<0.1; **p<0.05; ***p<0.01

Plotting the Regression

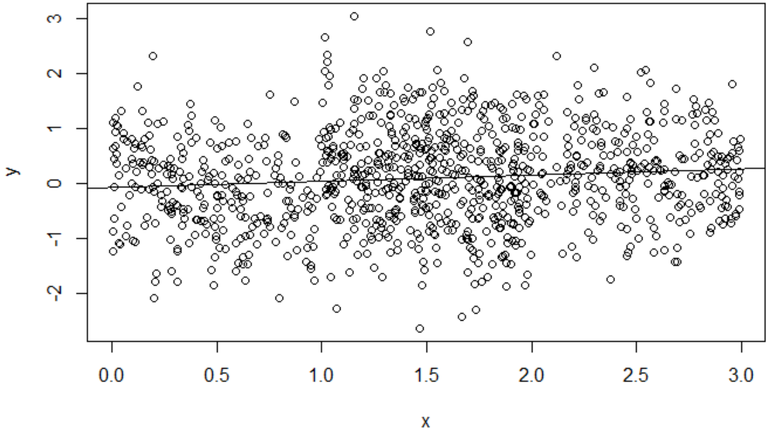


Figure: Nice Scatterplot plus Nice OLS Line

Forgot this one control variable...

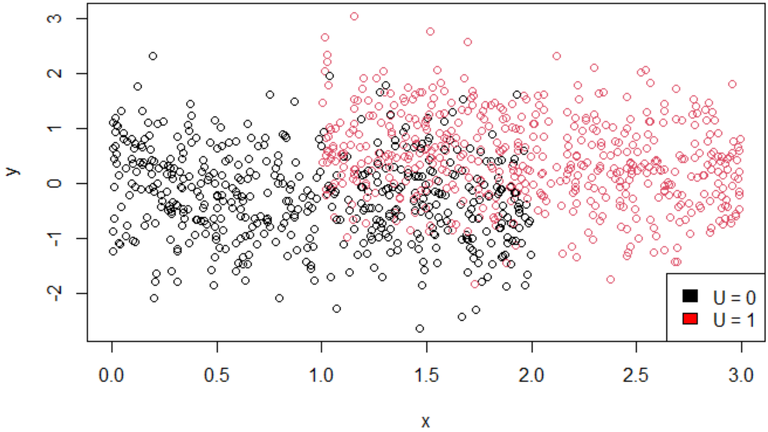


Figure: Coloring in the U. Is it correlated with X / Y?

Forgot this one control variable...

```
summary(lm(y ~ x + u))
```

	Dependent variable:	
	y	
	(1)	(2)
x	0.110***	−0.307***
	(0.035)	(0.041)
u		1.001***
		(0.065)
Constant	−0.064	0.044
	(0.059)	(0.053)
Observations	1,000	1,000
R^2	0.010	0.200
Adjusted R^2	0.009	0.198
Residual Std. Error	0.863 (df = 998)	0.776 (df = 997)
F Statistic	10.169*** (df = 1; 998)	124.289*** (df = 2; 997)
Note:		*p<0.1; **p<0.05; ***p<0.01

Forgot this one control variable...

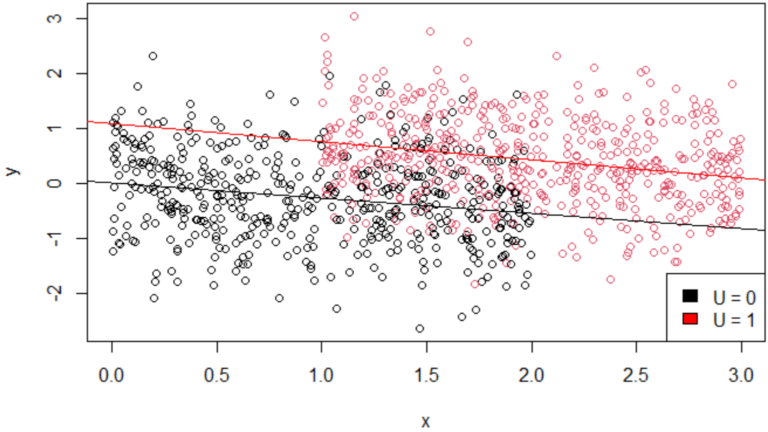


Figure: Controlling for U

Simpson's Paradox

- Dependencies/correlations "switching" when controlling for additional variables: "Simpsons's Paradox"
- "Paradox" b/c it defies intuition

Simulation

Code underlying the data:

```
u <- rbinom(n = 1000, size = 1, prob = 0.5)
x <- u + runif(n = 1000, min = 0, max = 2)
y <- -0.25*x + u + rnorm(n = 1000, sd = 0.75)
```

OLS & OVB

- Comparing linear regressions:
- $Y = a_{res} + b_{res}X + e_{res}$
- $Y = a + bX + cU + e$
- b_{res} and b will differ by $c \times \frac{cov(X,U)}{var(X)}$
- c is regression coefficient for U when controlling for X
- $\frac{cov(X,U)}{var(X)}$ is "imbalance": Regression of left-out U on independent variable X
- "impact times imbalance" (Cinelli/Hazlett 2020)
- "omitted variable bias"
- But is it really "bias"? Should we control for U if we can?

Which Regression is Correct?

- Which regression is correct? The one without U? The one with U? Neither?
- Poll!
- The answer depends on (mostly) on two things:
- The substantive question we are asking
- The assumptions we are willing to make
- So let's talk about **questions** first, and then about **assumptions**. All via causal graphs.

Section 3

DAGs & Causal & Non-causal Questions

First Running Example

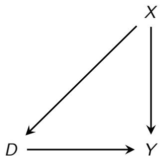
- Darfur (Sudan): Mass violence against civilians in 2003/04, killing an estimated 200,000
- Indicments of genocide and other crimes in the International Criminal Court
- Does "violence beget violence"? Or do individual experiences of violence lead people to demand peace?
- *What is the causal effect of experiencing violence on peace attitudes?* (Hazlett 2020)

A Possible Causal Graph for Hazlett 2020

$$D \longrightarrow Y$$

- Self-reported harm through violence D
- Beliefs about prospects of peace Y

Another Possible Causal Graph for Hazlett 2020



- violence D
- attitudes Y
- gender, village X

Causal & Non-Causal Questions

- Causal question: Effect of violence on attitudes
- Non-causal question: Correlation/dependence between violence and attitudes
- Non-causal question: Dependence between violence and attitudes, controlling for (conditioning on, given the same) gender
- Causal question: Effect of violence on attitudes among women (conditional/subgroup effect)
- Causal question: Effect of gender on attitudes through violence (mediation, more later)

Directed Acyclic Graphs

- These were two directed acyclic graphs (DAGs)
- Directed: Every connection has a direction (no simple lines, no arrows going both ways)
- Acyclic: No cycles in the graph - no "mutual causality", "feedback loops"
- First used by biologist Sewall Wright in the 1920s, important for traditional structural equation modelling in the 60s–80s, resurgence due to work by computer scientist Judea Pearl in 1995
- Popular framework for "working with" causality in machine learning/AI, statistics, political science/sociology...

Causal DAGs

- Causal DAGs visualize causal assumptions
- Important assumptions are the *arrows left out*
  - No arrow = assume that no such causal effect exists, period
- Drawing an arrow just implies that there *might* be a causal effect
- Bad assumptions in, bad results out
- Good assumptions in, good results out
- Causal inference without assumptions is impossible

Hazlett 2020: Basic Result

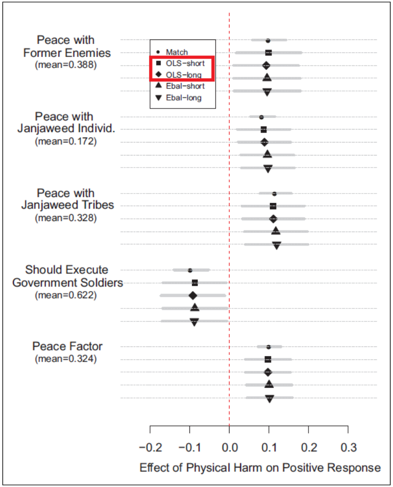


Figure: Fig. 2 from Hazlett 2020.

Causal DAGs

- We've seen the possible consequences of controlling for third variables
- We've seen causal versus non-causal questions
- We've seen some basic DAGs
- So how can DAGs tell us what to control for, given that we ask a causal question?

Section 4

Causal Assumptions & Choosing Control Variables

Choosing Control Variables

- You are interested in the causal effect of violence D on attitudes Y
- You consider statistical control for other variables X
- A good rule for choosing whether to include X would have two properties:
  - It tells you which X you must control for
  - It never tells you to control for a X which actually introduces bias

Choosing Control Variables

- What do you think are good rules for choosing control variables?
- Control for X if...
  - X associated with Y
  - X associated with D
  - X associated with D and Y
  - Unaffected by D and associated with D and Y (K. Imai)
  - affects Y
  - $D \perp\!\!\!\perp Y | X$
  - $D \perp\!\!\!\perp Y(d) | X$
- Vote in the poll!

Choosing Control Variables

- *All* of these rules, except the last, violate the two requirements
- We will use causal graphs to find a rule that is easier to understand
- But first, we need to understand how causation (causal graphs) creates correlation

Section 5

d-separation

d-separation I

- In this graph, do D and Y correlate?
  - Yes
- Do D and Y correlate when I control for / condition on M?
  - No
- The path is *open*. Conditional on M, it is *blocked*
- Simulation in R:

```
D <- rnorm(1000)
M <- 0.4*D + rnorm(1000)
Y <- -0.6*M rnorm(1000)
lm(Y ~ D)
lm(Y ~ D + M)
```

d-separation II

- In this graph, do D and Y correlate?
  - Yes
- Do D and Y correlate when I control for / condition on M?
  - No
- The path is *open*. Conditional on M, it is *blocked*

d-separation III

- In this graph, do D and Y correlate?
  - No
- Do D and Y correlate when I control for / condition on M?
  - Yes
- The path is *blocked*. Conditional on M, it is *open*
- M acts as a *collider*

Collider: Example 1

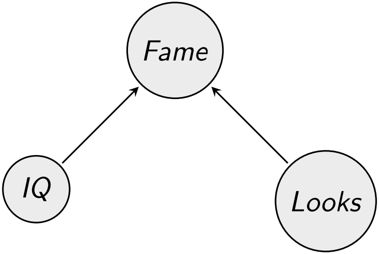
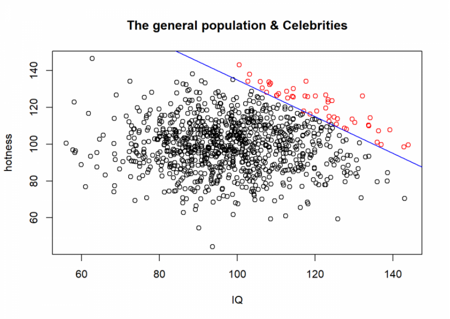
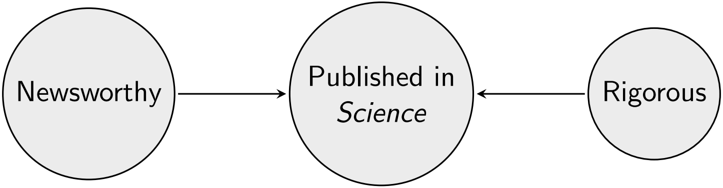


Image: von Jouanne-Diedrich, https://blog.ephorie.de/



Collider: Example 2



- If study is newsworthy and published in science...
- ... it is probably less rigorous

d-separation: Summary

- Chain of mediation: Path is open unconditionally, but blocked conditional on the middle node. $D \not\perp\!\!\!\perp Y$ but $D \perp\!\!\!\perp Y | M$.
- Common cause/fork: Path is open unconditionally, but blocked conditional on the middle node. $D \not\perp\!\!\!\perp Y$ but $D \perp\!\!\!\perp Y | M$.
- Collider: Path is blocked unconditionally, but open conditional on the middle node or one of its descendants. $D \perp\!\!\!\perp Y$ but $D \not\perp\!\!\!\perp Y | M$.
- What if there are multiple, longer paths between D and Y? Will D and Y be (conditionally) independent? **d-separation** gives the answer

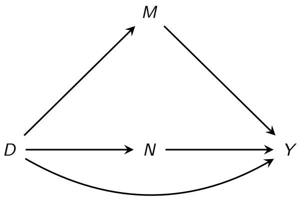
d-separation: Definition

- A path p is blocked by a set of nodes Z if and only if
  1. p contains a chain of nodes $X \rightarrow M \rightarrow Y$ or a fork $X \leftarrow M \rightarrow Y$ such that the middle node M is in Z (i.e., M is conditioned on), or
  2. p contains a collider $X \rightarrow M \leftarrow Y$ such that the collision node M is not in Z, and no descendant of B is in Z
- If Z blocks every path between two nodes X and Y, then X and Y are **d-separated**, conditional on Z, and thus are independent conditional on Z
- **testable implication** of the graph
- "d-separation" = "directional separation" (in directed graphs)
- Path p may be very long, but as long as you block sub-path, you block the whole path
- If testable implication does not hold, something about the graph is wrong

d-separation: Practice & DAGitty

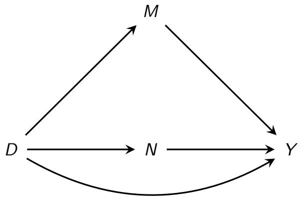
- Needs practice
- Automated: `http://dagitty.net/`
- Also `R` package `dagitty`

Exercise: d-separation



- This graph makes many strong assumptions. Are they testable?
- That is, is there a regression you could run using some (or all) of the variables to show that this graph is wrong?
- Take a sheet of paper & 5 minutes, then poll

Exercise: d-separation



- Almost all pairs of variables are directly connected $\implies$ will likely correlate
- Except for M and N
- Connected through D and Y
- Y is always a collider on these paths, e.g. $M \rightarrow Y \leftarrow N$
- Only open path is $M \rightarrow D \leftarrow N$
- D is "confounder", controlling for D blocks the path, does not open other paths
- So M and N should be independent, given D
- E.g., `lm(M ~ N + D)`

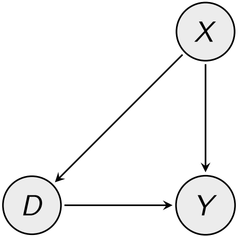
Section 6

Back-Door Criterion

From d-separation to Identification

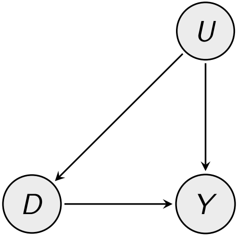
- We now know which graphs create (non-)correlations/dependencies
- How can we use this knowledge to determine valid control variables?

Intuition



- Which paths does the association between D and Y consist of?
- 1) causal effect of D on Y and 2) confounding due to X
- We want to estimate $E[Y|do(D = d)] = E[Y(d)]$
- If you cannot $do(d)$ in reality, find control variables such that
  - "Bad", "spurious", "non-causal" paths between D and Y are blocked
  - All "causal" paths are left open
  - No new "non-causal" paths are opened up (colliders...)

Intuition



- If you cannot intervene, find control variables such that
  - "Bad", "spurious", "non-causal" paths between D and Y are blocked
  - All "causal" paths are left open
  - No new "non-causal" paths are opened up (colliders...)
- This is UNRELATED to d-separation: d-separation is for testing graphs; and if two variables are d-separated, by definition all paths between them are blocked
- But for identifying causal effects, we certainly want to leave certain paths open (although we also want to block *some*)

The Back-Door Criterion

- Given an ordered pair of variables (D, Y) in a DAG, a set of variables X satisfies the backdoor criterion relative to (D, Y) if

  1) no node in X is a descendant of D, and

  2) X blocks every path between D and Y that contains an arrow into D

- *Ordered* pair because D is cause, Y is effect
- A path that starts with an arrow into D is called a **back-door path**
- Blocking back-door paths makes sure we block "bad" paths
- Not conditioning on descendants of D makes sure we leave all "good" causal paths open and that we do not open up new bad paths
- Holds for any DAG $\Longrightarrow$ non-parametric, distribution-free

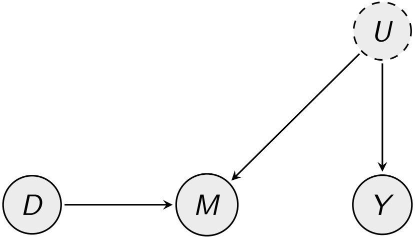
Section 7

Post-Treatment Bias

Post-Treatment Variables: Problem 1

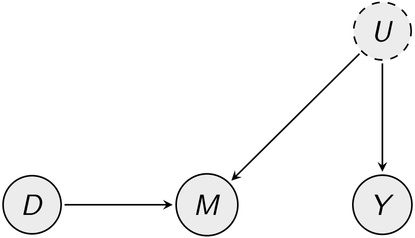
- Which set of variables in this graph satisfy the BDC wrt effect of D on Y?
- The empty set $\emptyset$ - no controls necessary
- $E[Y|do(D=1)] - E[Y|do(D=0)] =$
  $E[Y|D=1] - E[Y|D=0]$ (correlation is causation)
- No paths into D - as if we intervened on it
- Does M correlate with D and Y?
- "M correlates with D and Y. I've learned in stats that I need to control for it. Otherwise, I have omitted-variable bias"
- Bad idea: Conditional on M, D and Y are d-separated! Even though D may have an effect on Y
- Montgomery et al. 2018 AJPS estimate that 50 % of political science experiments do this. Huge problem.

Post-Treatment Variables: Problem 2



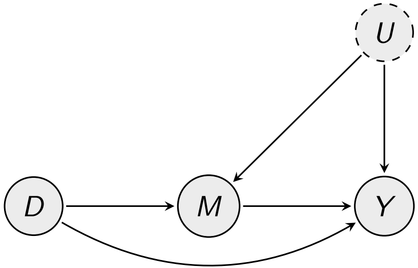
- It gets worse. Which set of variables in this graph satisfy the BDC wrt effect of D on Y?
- The empty set - no controls necessary
- Also, no causal effect of D on Y!

Post-Treatment Variables: Problem 2



- "M correlates with D and Y. I've learned in stats that I need to control for it. Otherwise, I have omitted-variable bias"
- Bad idea: Conditional on M, D and Y are d-connected! Collider!
- See simulation

Post-Treatment Variables: General Case



- This graph applies to situations where there are no back-door paths into D. Perhaps via randomization, or you block them by conditioning on X (not shown).
- Conditioning on M is forbidden by the BDC and will have two consequences:
- 1. You block a causal path, which you do not want
- 2. You open up a non-causal path, which you do not want
- This introduces bias, and it can go in any direction

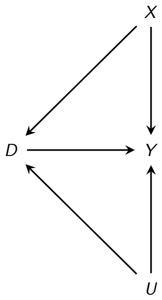
Post-Treatment Variables: Remarks

- Although clear using causal graphs, the fact that conditioning on the descendants of the treatment may actually introduce bias is not well-known
- Usually not mentioned in textbooks that do not use causal graphs
- Even if mentioned, not really explained (see for example "Mostly Harmless Econometrics", section on "Bad Control")

Section 8

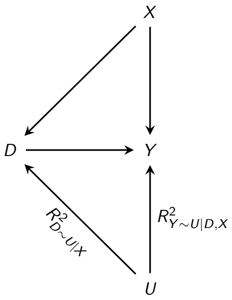
Sensitivity Analysis for Unobserved Confounding: sensemakr

Another Possible Causal Graph for Hazlett 2020



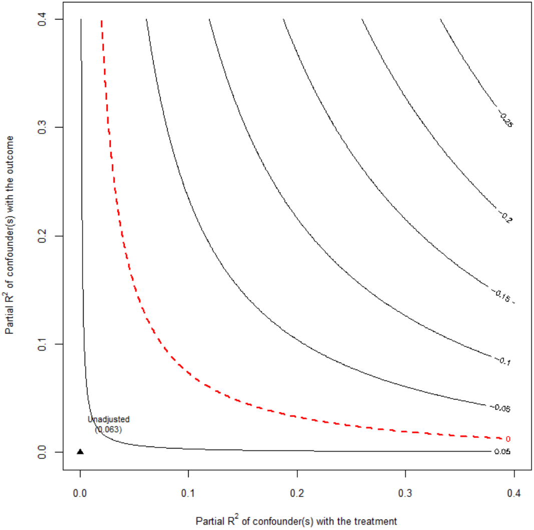
- violence D
- attitudes Y
- gender, village X
- Unobserved U?

Another Possible Causal Graph for Hazlett 2020



- violence D
- attitudes Y
- gender, village X
- Unobserved U?

Sensitivity Analysis via sensemakr (Cinelli/Hazlett)



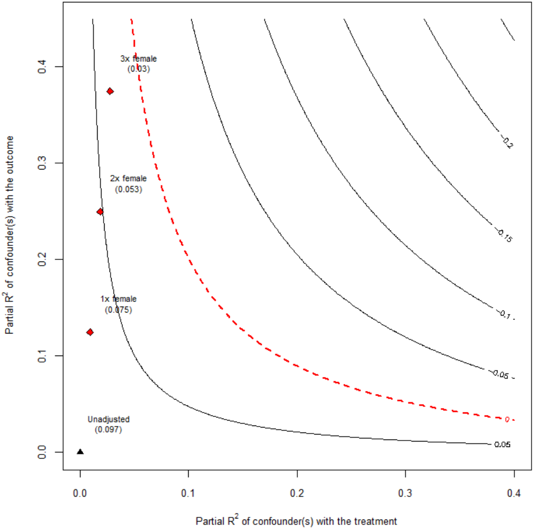
sensemakr: Exercise

- Add `gender` and `village` as control variables
- Use `gender` for "benchmarking"
- Plot the results

  ```
  sensitivity.2 <- sensemakr(model.2,
  treatment = "d", benchmark_covariates = "x, kd =
  1:3)
  plot(sensitivity.2)
  ```

- What does the plot tell us?

sensemakr: Benchmarking



sensemakr: Comments

- Approach by `sensemakr` relies on assumption of linear "target" regression that includes U
- Otherwise, U may contain many, many variables that impact on D and Y in complicated ways
- Sensitivity of significance tests (t-values) by using `plot(sensitivity.1, sensitivity.of = "t-value")`

Section 9

Interim Summary

Interim Summary

- Statistical control for additional variables may make a huge difference
- W/o a clearly articulated question and assumptions impossible to justify whether additional control is "good" or "bad"
- Causal graphs visualize causal assumptions
- Causal assumptions imply certain (non-)correlations via d-separation
- Given a causal question & DAG, can tell what kind of control is (not) necessary via back-door criterion
- Danger of post-treatment bias
- If unobserved confounding suspected: Sensitivity analysis

Section 10

Causal Mechanisms: Mediation Analysis

Substantive Examples for (in)direct effects

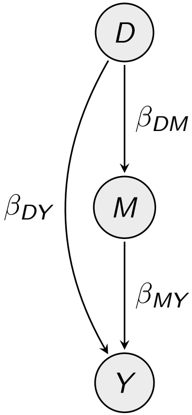
- Do macroeconomic conditions affect the vote for the incumbent mostly through individual evaluations of the economy?
- Does the incumbency effect exist because strong incumbents scare off high-quality challengers?
- Do PR systems redistribute more because of different coalition dynamics?
- Are hiring processes discriminatory; i.e., is there a direct effect of socio-economic background/gender/race...on the probability to receive a job?
- Do some genes cause lung cancer only through their effect on smoking behaviour?
- Does Cognitive Behavioral Therapy only work because it leads people to use anti-depressants more often?

History of (in)direct effects

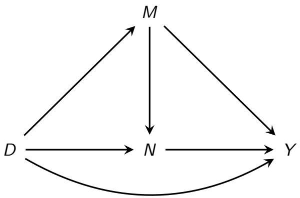
- Pretty clear in linear structural models
- Generalization of direct and indirect effects in Pearl 2001
- Followed by increased interest in statistics, epidemiology, sociology, political science
- E.g., Imai et al. 2010ff. implementation in `mediation` package

Direct and indirect effects in linear models

- What is the direct, what is the indirect effect of D on Y in this model?
- Direct: β_{DY}, indirect: $\beta_{DM}\beta_{MY}$
- Linear models allow for easy estimation strategies using series of linear regressions
- But many things are nonlinear...

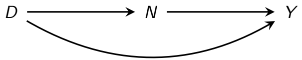


Brader et al.



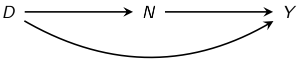
- randomized treatment D
- cost/benefit beliefs M
- anxiety N
- opposition to immigration Y

Simplified Mediation Graph



- randomized treatment D
- (cost/benefit beliefs M)
- anxiety N
- opposition to immigration Y

Estimation in the Easiest Case



- **If this graph is correct and you stick to linear regressions...**
- Regression of Y on D: ATE of D
- Regression of Y on D and N: Direct effect of D
- ATE - Direct Effect = Indirect effect
- Or: Regression of N on D $\times$ Regression of Y on N and D = IE

Differencing Approach

Table:

	Dependent variable:	
	Opposition to Immigration (1-4)	
	(1)	(2)
tone_eth	0.439*** (0.134)	0.161 (0.116)
emo		0.188*** (0.018)
Constant	2.914*** (0.068)	1.674*** (0.134)
Observations	265	265
R^2	0.040	0.315
Adjusted R^2	0.036	0.309
Residual Std. Error	0.949 (df = 263)	0.803 (df = 262)
F Statistic	10.820*** (df = 1; 263)	60.162*** (df = 2; 262)
Note:		*p<0.1; **p<0.05; ***p<0.01

$$\text{IE} = 0.439 - 0.161 = 0.278$$

Product Approach

Table:

	Dependent variable:	
	emo	immigr
	(1)	(2)
tone_eth	1.480*** (0.380)	0.161 (0.116)
emo		0.188*** (0.018)
Constant	6.594*** (0.193)	1.674*** (0.134)
Observations	265	265
R^2	0.054	0.315
Adjusted R^2	0.051	0.309
Residual Std. Error	2.703 (df = 263)	0.803 (df = 262)
F Statistic	15.143*** (df = 1; 263)	60.162*** (df = 2; 262)
Note:		*p<0.1; **p<0.05; ***p<0.01

$$\text{IE} = 1.480 * 0.188 = 0.278$$

Problems with the Classic Linear Approach

- What if there is unobserved confounding? Other problems?
- Sensitivity analysis?
- Unclear how to implement this for nonlinear models (e.g., logit)
- No standard errors for indirect effect

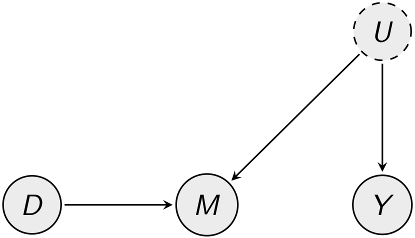
Section 11

Mediation: Identification & Post-Treatment Confounding

Natural Effects: Identification

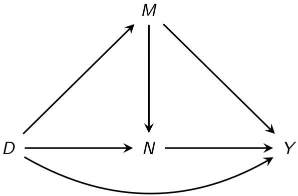
- I am skipping the (intricate) general definition of direct and indirect effects
- Instead, focus again on identification
- I.e., when can we estimate direct/indirect effects from data? How can we think about choosing control variables?

Recap: Post-Treatment Variables: Problem 2



- Conditional on M, D and Y are d-connected! Collider!
- Control for U necessary to correctly infer zero direct effect of D on Y

A Possible Causal Graph for the Brader et al. Study

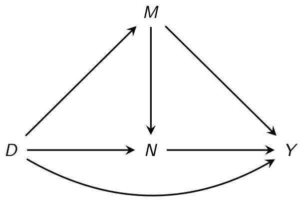


- randomized treatment D
- cost/benefit beliefs M
- anxiety N
- opposition to immigration Y
- If we want "direct effect that does not go through N", control for M?
- Poll

Identification of Natural Direct Effects

- Graphical version of Sequential Ignorability (Imai et al. 2010) due to Pearl 2014:
- There are covariates X such that
- 1. X and D block all D-avoiding back-door paths from N to Y
- 2. X blocks all back-door paths from D to N and from D to Y, and no member of of X is descendant of D
- In essence: Control for confounders of D and Y,
  D and N, and
  N and Y
- And: "no member of X is descendant of D"

A Possible Causal Graph for the Brader et al. Study



- If we want "direct effect not through N", control for M?
- This effect is fundamentally unidentifiable! (w/o further assumptions)
  - Control for M: Block part of effect that goes through N via M
  - Do not control: Open confounding path $N \leftarrow M \rightarrow Y$
- M acts as a *post-treatment confounder*
- This is an issue even if M is measured!

Interim Summary

- "When I measure all mediators and all other relevant variables ("confounders"), I can disentangle the effect of the treatment into different indirect effects"
  - No, because other mediators may act as post-treatment confounders

Section 12

Mediation Analysis: Estimation & Sensitivity Analysis

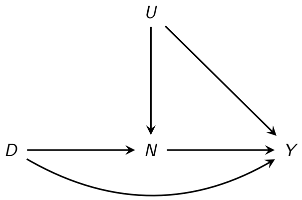
Imai et al.: "mediation" package

- R: `install.packages("mediation")`
- General idea:
  - Fit a regression of Y on D and M (plus controls) (outcome model)
  - Fit a regression of M on D (plus controls) (mediator model)
  - Package calculates Total, Direct, Indirect effect from that
- Supports many, many models in R

Basic Usage

```
model.m <- lm(m ~ d, data=df)

model.y <- lm(y ~ d + m, data=df)

out.1 <- mediate(model.m, model.y,
sims=1000, treat="d",
mediator="m",
boot=FALSE)

plot(out.1)

summary(out.1)
```

Unobserved Mediator-Outcome Confounding



- randomized treatment D
- (cost/benefit beliefs M)
- anxiety N
- opposition to immigration Y
- U unobserved confounder

Sensitivity Analysis in mediation Package

```r
sensout.1 <- medsens(
          out.1, sims=10000, rho.by=.01)

summary(sensout.1)
plot(sensout.1, sens.par="R2")
plot(sensout.1)
```

Sensitivity Results

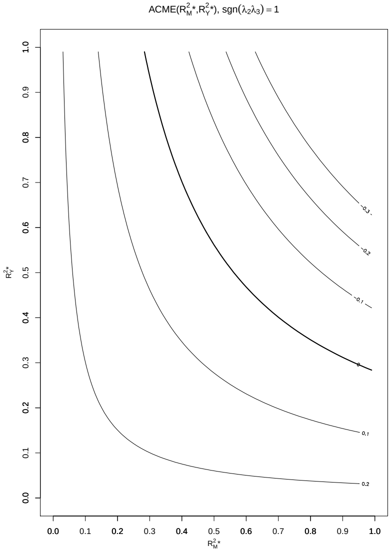


Figure: Output from `plot(sensout.1, sens.par="R2")`

Sensitivity Results

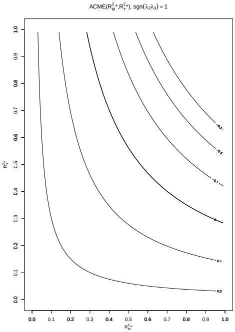


Figure: Output from `plot(sensout.1, sens.par="R2")`

- Original point estimate not shown at $(0, 0)$
- As in `sensmakr`, contour lines point estimates for varying combinations of R^2 that unobserved confounder explains (in M and in Y)

Alternative Sensitivity Results

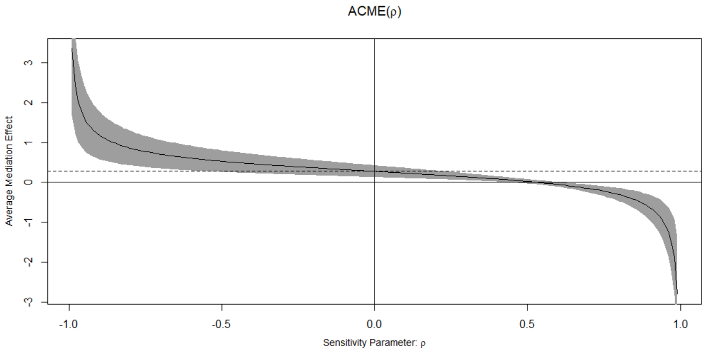


Figure: Output from `plot(sensout.1)`

- Dashed horizontal line is original point estimate
- Solid black line point estimate for varying ρ
- Shaded area are confidence intervals

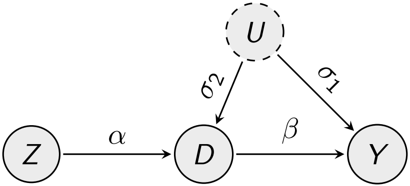
Basic Mediation: Summary

- In "simple" studies where aim is to estimate the total causal effect, control for post-treatment variables/mediators may create bias
  - Control away part of the causal effect of interest and/or
  - Open up non-causal paths (colliders)
- If aim is to estimate direct/indirect effects, control for mediators seems to make sense
- Unbserved confounders of M and Y still create problems; can use sensitivity analysis
- New (and unique) problem: Post-treatment confounding
- Not solvable without stronger assumptions
- Sensitivity analysis under stronger assumptions possible: `multimed`

Section 13

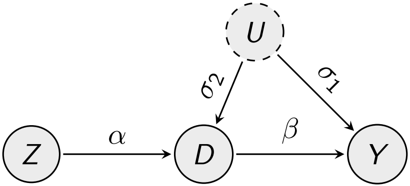
Instrumental Variables via DAGs

IV in Linear Case



- Regard graph as depiction of *structural* or *causal* equations:
  $D = \alpha Z + U$
  $Y = \beta D + U$
- These are not regressions, but "nature" or "society"
- E.g., in our simulations, we played nature
- β is a causal effect that may or may not be *identified* via the regression of Y on D

IV in Linear Case



- $cov(Z, D) = \alpha$
- $cov(Z, Y) = \alpha \cdot \beta$
- We want β. We can estimate $cov(Z, D)$ and $cov(Z, Y)$
- So: $\beta = \dfrac{cov(Z, Y)}{cov(Z, D)}$
- Here, Z acts as an **instrumental variable for the effect of D on Y**

Cyrus Samii's IV Greatest Hits Collection

- Draft lottery numbers $\rightarrow$ military service $\rightarrow$ income (Angrist 1990)
- Quarter of birth $\rightarrow$ schooling $\rightarrow$ income (Angrist & Krueger 1991)
- Election year $\rightarrow$ number of police $\rightarrow$ crime (Levitt 1997)
- Sibling sex composition $\rightarrow$ number of children $\rightarrow$ labor supply (Angrist & Evans 1998)
- Settler mortality $\rightarrow$ investment in institutions $\rightarrow$ avg. income (Acemoglu et al. 2001)
- Rain $\rightarrow$ avg. income $\rightarrow$ civil war (Miguel et al. 2004)
- Density of railroads $\rightarrow$ segregation $\rightarrow$ inequality (Ananat 2011)

Alternative Derivation

- In fact, only structural model for Y needs to be linear:
- $Y = \beta D + U$, where U and D correlate (back-door path)
- Using this equation, $cov(Z, Y)$ is
- $= cov(Z, \beta D + U) = \beta cov(Z, D) + cov(Z, U)$
- The graph says $Z \perp\!\!\!\perp U$, so $cov(Z, U) = 0$
- Solve for $\beta = \dfrac{cov(Z, Y)}{cov(Z, D)}$
- We have made no assumption on structural model for D!

2nd Alternative/Two-Stage Least Squares

- Let $D = \mu + \delta_1 Z + \epsilon$ be the **linear projection** of D on Z
- This is not structural, nor a regression, but a **linear approximation** to $E[D|Z]$ that almost always exists (OLS in the population)
- $cov(Z, \epsilon) = 0$ by construction (as for regression error when indep. vars are discrete)
- Insert into structural model for Y:
- $Y = \beta(\mu + \delta 1 Z + \epsilon) + U$
- $= \beta\mu + \beta\delta 1 Z + \beta\epsilon + U$
- This is a mix of structural and linear-projection coefficients
- Could be estimated via OLS if $cov(Z, \beta\epsilon + U) = \beta cov(Z, \epsilon) + cov(Z, U) = 0$, which is true by construction (ϵ)/by assumption (U)

Two-Stage Least Squares

- $\beta\mu + \beta\delta 1 Z + \beta\epsilon + U$
- $= \beta\mu + \beta\widehat{D} + \beta\epsilon + U$
- Where $\widehat{D} = \delta 1 Z$ are fitted values from first-stage linear projection
- This suggests:
- OLS of D on Z, regardless of what kind of variables D and Z are. Generate $\widehat{D}$.
- OLS of Y on $\widehat{D}$. Coefficient is consistent estimate of causal effect β
- **Two-Stage Least Squares**
- Implemented in standard statistical software (which also gives correct standard errors)

Implementation of 2SLS

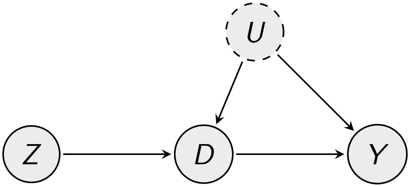
```r
library(estimatr)

iv_robust(Y ~ D + X | Z + X, data = dat)
```

- Y is outcome
- D is treatment
- Z is instrument
- X are controls

Section 14

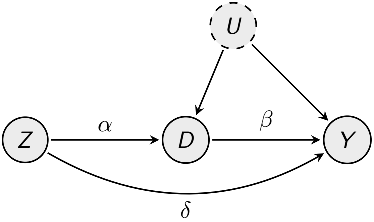
IV Assumptions & Covariates

Basic IV



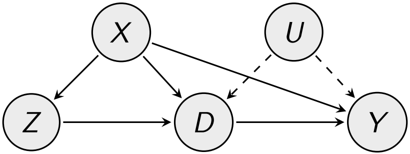
- Z does not directly affect Y ("exclusion restriction", "no direct effect")
- No variables impacting Z and Y or Z and D $\implies$ no back-door paths from instrument to treatment or outcome
- E.g. Z US Vietnam war draft lottery, D actually serving in Vietnam war, Y wages after return, U unobserved ability (Angrist 1990)

IV with Direct Effect of Instrument



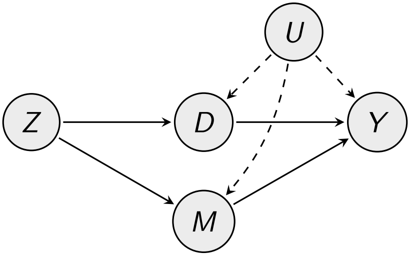
- What is the IV estimator $\dfrac{cov(Z, Y)}{cov(Z, D)}$ in this case?
- $cov(Z, D) = \alpha$
- $cov(Z, Y) = \alpha\beta + \delta$
- $\widehat{\beta} = \dfrac{\alpha\beta + \delta}{\alpha} = \beta + \dfrac{\delta}{\alpha}$
- Asymptotic bias $\dfrac{\delta}{\alpha}$. Z not a valid IV
- Larger if instrument is "weaker" (smaller α)

IV with Covariates



- X: birth year – demand of military varied from year to year
- Control for X blocks back-door path – all good

Post-Instrument Covariates



- M attending college to defer the draft
- Is Z still a valid IV? Must we control for M? Poll!
- If no control for M: Violation of exclusion restriction
- But conditioning on M creates non-causal association between Z and Y (collider!)
- $\implies Z$ not a valid instrument
- See Schuessler et al. 2021 for deeper discussion

Section 15

Instrumental Variables: (Almost) Nonparametric Case

From Linear to Nonparametric Case

- IV in linear case is very easy
- In nonparametric case, complications occur
- The problem comes from heterogeneity in causal effects
- In linear causal models, everyone has the same individual causal effect $Y_1(u) - Y_0(u) = \beta$
- In more realistic nonparametric case, $Y_1(u) - Y_0(u)$ varies across u/across individuals i

Nonparametric Case

- With binary Z and D, we can write the structural equations of the simple IV model as
- $Y_i = \mu_1 + \beta_i D_i + \epsilon_i$
  $D_i = \mu_2 + \alpha_i Z_i + \epsilon_i$
  $Z_i \perp\!\!\!\perp \epsilon_i$
- Where $\beta_i = Y_{D=1}(u) - Y_{D=0}(u)$ and $\alpha_i = D_{Z=1}(u) - D_{Z=0}(u)$ are unit-level causal effects
- Very easy to show that $E[Y|Z=1] - E[Y|Z=0] = E[\alpha_i \beta_i]$ by structural definition of counterfactuals & BDC
- Also clear that $E[D|Z=1] - E[D|Z=0] = E[\alpha_i]$

Nonparametric Case

- $Y_i = \mu_1 + \beta_i D_i + \epsilon_i$
  $D_i = \mu_2 + \alpha_i Z_i + \epsilon_i$
- In this model, $\alpha_i, \beta_i, \epsilon_i$ are all part of U_i, the unobserved confounders that influence
  - D and Y and
  - how D reacts to Z and Y reacts to D (interactions!)
- So α_i and β_i correlate
- With binary D, $\alpha_i = D_{Z=1}(u) - D_{Z=0}(u)$ can only take on three values: 1, 0, -1
- It turns out that now, bad things can happen with our usual IV estimator

Nonparametric Case

- $\frac{cov(Z,Y)}{cov(Z,D)} = \frac{E[Y|Z=1]-E[Y|Z=0]}{E[D|Z=1]-E[D|Z=0]} =$
- $\frac{E[\alpha_i \beta_i]}{E[\alpha_i]}$ is the IV estimator if our graph is correct
- Now let's say for people with $\alpha_i = 1$ and $\alpha_i = -1$, $\beta_i = 1$; for $\alpha_i = 0$ units, $\beta_i = 0$. All α_i equally likely ($\frac{1}{3}$)
- Then by LIE, ATE $= E[\beta_i] = E[\beta_i|\alpha_i = 1]P(\alpha_i = 1) + E[\beta_i|\alpha_i = -1]P(\alpha_i = -1) + E[\beta_i|\alpha_i = 0]P(\alpha_i = 0) = \frac{2}{3}$
- But also by LIE: $E[\alpha_i \beta_i] = E[\alpha_i \beta_i|\alpha_i = 1]P(\alpha_i = 1) + E[\alpha_i \beta_i|\alpha_i = -1]P(\alpha_i = -1) + E[\alpha_i \beta_i|\alpha_i = 0]P(\alpha_i = 0)$
- $= E[\beta_i|\alpha_i = 1]P(\alpha_i = 1) + E[-\beta_i|\alpha_i = -1]P(\alpha_i = -1)$
- $= 1 \cdot \frac{1}{3} - 1 \cdot \frac{1}{3} = 0!$
- IV estimator will be 0 even though ATE is $\frac{2}{3}$!

Parametric Solutions

- Solutions by making stronger assumptions:
- $E[\alpha_i \beta_i] = E[\alpha_i]E[\beta_i]$. This is almost like assuming away confounding (uncorrelated effect heterogeneity)
- $\beta_i = \beta$, a constant, so $E[\alpha_i \beta_i] = E[\alpha_i]\beta$. This is similar to linearity (constant causal effects)
- Most common in political science/econ: Assume away that people exist with $\alpha_i = -1$
- Since their choice of D reacts to Z "in the opposite way", $\alpha_i = -1$ units are also called **defiers**
- Assumption also sometimes called **monotonicity**, because Z may not have positive AND negative impact on D
- Since this restricts the structural function for D, **it is a parametric assumption**

The LATE Model

- $Z \perp\!\!\!\perp Y_{D=1}, Y_{D=0}, D_{Z=1}, D_{Z=0} \implies Z \perp\!\!\!\perp \beta_i, \alpha_i$
- This is the **old** instrumental assumption from linear case (no back-door paths to or direct effect on Y), plus BDC for $Z \rightarrow D$ (**new!**)
- No defiers: $P(\alpha_i = -1) = 0$ (**new!**)
- Relevance/first-stage: $E[D|Z = 1] - E[D|Z = 0] \neq 0$ (**old**)
- Then the IV estimator by above reasoning evaluates to
- $\frac{E[\alpha_i \beta_i | \alpha_i = 1] P(\alpha_i = 1)}{E[\alpha_i]} =$
- $\frac{E[\beta_i | \alpha_i = 1] P(\alpha_i = 1)}{P(\alpha_i = 1)} =$
- $E[\beta_i | \alpha_i = 1]$
- $= E[Y_{D=1} - Y_{D=0} | D_{Z=1} - D_{Z=0} = 1]$

The LATE Model

- $E[\beta_i|\alpha_i = 1] = E[Y_{D=1} - Y_{D=0}|D_{Z=1} - D_{Z=0} = 1]$
- The average effect of D on Y for those units whose choice of D reacts to Z
- **Local** Average Treatment Effect (LATE), Complier Average Causal Effect (CACE)

LATE is not ATE

- In our example, $\alpha_i = 1 \implies \beta_i = 1$ and $\alpha_i = 0 \implies \beta_i = 0$
- If $P(\alpha_i = 1) = 0.5$ and no defiers, this would mean $ATE = 0.5 \neq LATE = 1$
- ATE is usually more relevant for policy and science
- Compliers may be small part of overall population. Fortunately, first-stage is share of compliers: $E[\alpha_i] = P(\alpha_i = 1)$, so we can check this
- Plus, we cannot directly observe who a complier is, because we cannot observe α_i; so LATE is not really a covariate-specific effect
- In general, people debate whether LATE is useful to know or whether we should care about ATE (e.g. Heckman)
- It turns out that using an instrument, even without monotonocity, one can at least **partially identify/bound** the ATE (Balke/Pearl 1997)

Section 16

Summary

Summary

- We've covered:
  - Causal Graph basics incl. d-separation
  - Back-door criterion, post-treatment bias
  - Sensitivity analysis for OLS
  - Basic mediation analysis incl. sensitivity analysis
  - IV from a DAG perspective

Section 17

Literature & Further Material

Literature

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