

Week 10: Causality with Measured Confounding

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Princeton

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¹These slides are heavily influenced by Matt Blackwell, Jens Hainmueller, Erin Hartman, Kosuke Imai and Gary King.

Where We've Been and Where We're Going...

- Last Week
 - ▶ intro to causal inference
- This Week
 - ▶ Monday:
 - ★ experimental Ideal
 - ★ identification with measured confounding
 - ▶ Wednesday:
 - ★ regression estimation
- Next Week
 - ▶ identification with unmeasured confounding
 - ▶ instrumental variables
- Long Run
 - ▶ probability → inference → regression → causal inference

Questions?

- 1 The Experimental Ideal
- 2 Assumption of No Unmeasured Confounding
- 3 Estimation Under No Unmeasured Confounding
- 4 Regression Estimators
- 5 Regression and Causality
- 6 Regression Under Heterogeneous Effects
- 7 Fun with Visualization, Replication and the NYT

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Today's Random Medical News

from the New England Journal of Panic-Inducing Gobbledygook

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Lancet 2001: negative correlation between coronary heart disease mortality and level of vitamin C in bloodstream (controlling for age, gender, blood pressure, diabetes, and smoking)

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Lancet 2002: no effect of vitamin C on mortality in controlled placebo trial (controlling for nothing)

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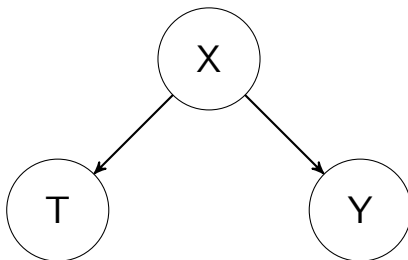


Lancet 2003: comparing among individuals with the same age, gender, blood pressure, diabetes, and smoking, those with higher vitamin C levels have lower levels of obesity, lower levels of alcohol consumption, are less likely to grow up in working class, etc.

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Confounders



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- Cannot always randomize so we do observational studies, where we **adjust** for the **observed covariates** and **hope** that unobservables are balanced
- Better than hoping: **design** observational study to approximate an experiment
 - ▶ “The planner of an observational study should always ask himself: How would the study be conducted if it were possible to do it by controlled experimentation” (Cochran 1965)

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- What is your mode of statistical inference?

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 - ▶ Treatment assignment does not depend on any potential outcomes.
 - ▶ Sometimes written as $D_i \perp\!\!\!\perp (\mathbf{Y}(1), \mathbf{Y}(0))$

Why do Experiments Help?

Remember selection bias?

$$\begin{aligned} & E[Y_i|D_i = 1] - E[Y_i|D_i = 0] \\ &= E[Y_i(1)|D_i = 1] - E[Y_i(0)|D_i = 0] \\ &= E[Y_i(1)|D_i = 1] - E[Y_i(0)|D_i = 1] + E[Y_i(0)|D_i = 1] - E[Y_i(0)|D_i = 0] \\ &= \underbrace{E[Y_i(1) - Y_i(0)|D_i = 1]}_{\text{Average Treatment Effect on Treated}} + \underbrace{E[Y_i(0)|D_i = 1] - E[Y_i(0)|D_i = 0]}_{\text{selection bias}} \end{aligned}$$

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When all goes well, an experiment eliminates selection bias.

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 - ▶ Pick the ideal experiment to this observational study.
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 - ▶ Try to estimate the randomization procedure.
 - ▶ Analyze this as an experiment with this estimated procedure.
- Tries to minimize “snooping” by picking the best modeling strategy before seeing the outcome.

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- Never used our knowledge of the randomization for this quantity.

Stratification Example: Smoking and Mortality (Cochran, 1968)

TABLE 1
DEATH RATES PER 1,000 PERSON-YEARS

Smoking group	Canada	U.K.	U.S.
Non-smokers	20.2	11.3	13.5
Cigarettes	20.5	14.1	13.5
Cigars/pipes	35.5	20.7	17.4

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TABLE 2
MEAN AGES, YEARS

Smoking group	Canada	U.K.	U.S.
Non-smokers	54.9	49.1	57.0
Cigarettes	50.5	49.8	53.2
Cigars/pipes	65.9	55.7	59.7

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- calculate death rates within age subgroups
- average within age subgroup death rates using fixed weights (e.g. number of cigarette smokers)

Stratification: Example

	Death Rates Pipe Smokers	# Pipe- Smokers	# Non- Smokers
Age 20 - 50	15	11	29
Age 50 - 70	35	13	9
Age + 70	50	16	2
Total		40	40

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$$15 \cdot (29/40) + 35 \cdot (9/40) + 50 \cdot (2/40) = 21.2$$

Smoking and Mortality (Cochran, 1968)

TABLE 3
ADJUSTED DEATH RATES USING 3 AGE GROUPS

Smoking group	Canada	U.K.	U.S.
Non-smokers	20.2	11.3	13.5
Cigarettes	28.3	12.8	17.7
Cigars/pipes	21.2	12.0	14.2

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- ▶ $\rightsquigarrow$ cannot stratify to each unique value of X_i :
- Practically, this is massively important: almost always have data with unique values.

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 - ▶ Each unit has its own value of X_i : \$54,134, \$123,043, \$23,842.
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$$\mathbb{E}[Y_i | D_i = 1, X_i = 54134] - \mathbb{E}[Y_i | D_i = 0, X_i = 54134]$$

- ▶ $\rightsquigarrow$ cannot stratify to each unique value of X_i :
- Practically, this is massively important: almost always have data with unique values.

One option is to discretize as we discussed with age, we will discuss more later this week!

Identification Under Selection on Observables

Identification Assumption

- 1 $(Y_1, Y_0) \perp\!\!\!\perp D|X$ (*selection on observables*)
- 2 $0 < \Pr(D = 1|X) < 1$ with probability one (*common support*)

Identification Result

Given selection on observables we have

$$\begin{aligned}\mathbb{E}[Y_1 - Y_0|X] &= \mathbb{E}[Y_1 - Y_0|X, D = 1] \\ &= \mathbb{E}[Y|X, D = 1] - \mathbb{E}[Y|X, D = 0]\end{aligned}$$

Therefore, under the common support condition:

$$\begin{aligned}\tau_{ATE} &= \mathbb{E}[Y_1 - Y_0] = \int \mathbb{E}[Y_1 - Y_0|X] dP(X) \\ &= \int (\mathbb{E}[Y|X, D = 1] - \mathbb{E}[Y|X, D = 0]) dP(X)\end{aligned}$$

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Identification Result

Similarly,

$$\begin{aligned}\tau_{ATT} &= \mathbb{E}[Y_1 - Y_0|D = 1] \\ &= \int (\mathbb{E}[Y|X, D = 1] - \mathbb{E}[Y|X, D = 0]) dP(X|D = 1)\end{aligned}$$

To identify τ_{ATT} the selection on observables and common support conditions can be relaxed to:

- $Y_0 \perp\!\!\!\perp D|X$ (*SOO for Controls*)
- $\Pr(D = 1|X) < 1$ (*Weak Overlap*)

Identification Under Selection on Observables

unit	Potential Outcome under Treatment	Potential Outcome under Control		
i	Y_{1i}	Y_{0i}	D_i	X_i
1	$\mathbb{E}[Y_1 X = 0, D = 1]$	$\mathbb{E}[Y_0 X = 0, D = 1]$	1	0
2			1	0
3	$\mathbb{E}[Y_1 X = 0, D = 0]$	$\mathbb{E}[Y_0 X = 0, D = 0]$	0	0
4			0	0
5	$\mathbb{E}[Y_1 X = 1, D = 1]$	$\mathbb{E}[Y_0 X = 1, D = 1]$	1	1
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$(Y_1, Y_0) \perp\!\!\!\perp D|X$ implies that we conditioned on all confounders. The treatment is randomly assigned within each stratum of X :

$$\begin{aligned}\mathbb{E}[Y_0|X = 0, D = 1] &= \mathbb{E}[Y_0|X = 0, D = 0] \text{ and} \\ \mathbb{E}[Y_0|X = 1, D = 1] &= \mathbb{E}[Y_0|X = 1, D = 0]\end{aligned}$$

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- 1 The Experimental Ideal
- 2 Assumption of No Unmeasured Confounding
- 3 Estimation Under No Unmeasured Confounding
- 4 Regression Estimators
- 5 Regression and Causality
- 6 Regression Under Heterogeneous Effects
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 - ▶ effect of income on voting (confounding: age)
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- No unmeasured confounding assumes that we've measured all sources of confounding.

Big problem

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- Another way: use DAGs and look at back-door paths.

Backdoor paths and blocking paths

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- Backdoor paths between D and $Y \rightsquigarrow$ common causes of D and Y :



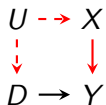
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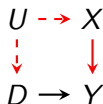
- Here there is a backdoor path $D \leftarrow X \rightarrow Y$, where X is a common cause for the treatment and the outcome.

Other types of confounding



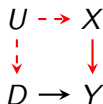
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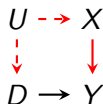
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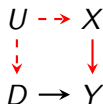
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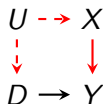
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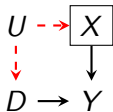
- D is enrolling in a job training program.
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- X is number of job applications sent out.
- Big assumption here: no arrow from U to Y .

Other types of confounding



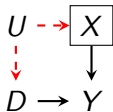
- D is exercise.
- Y is having a disease.
- U is lifestyle.
- X is smoking
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What's the problem with backdoor paths?



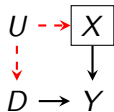
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What's the problem with backdoor paths?



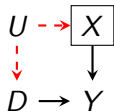
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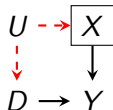
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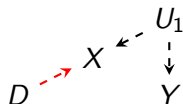
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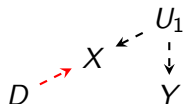
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- In the DAG here, if we condition on X , then the backdoor path is blocked.

Not all backdoor paths



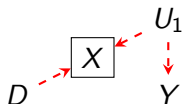
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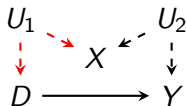
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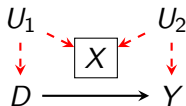
Every time you do, a puppy cries.

M-bias



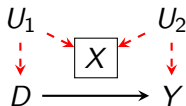
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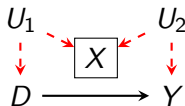
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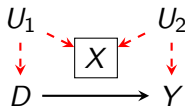
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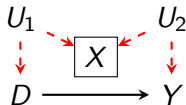
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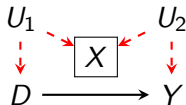
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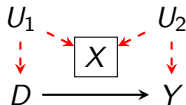
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 - ▶ See the Elwert and Winship piece for more!

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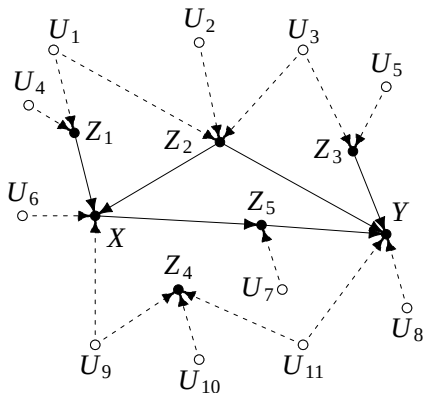
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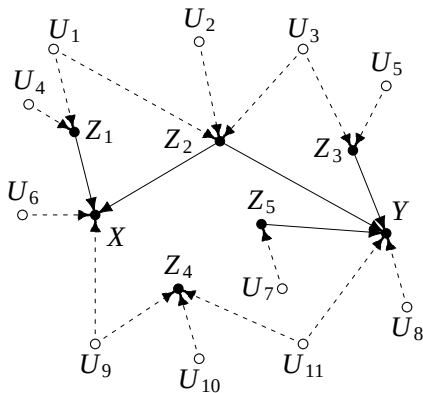
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 - ▶ what variables to condition on to eliminate the confounding.

Example: Sufficient Conditioning Sets



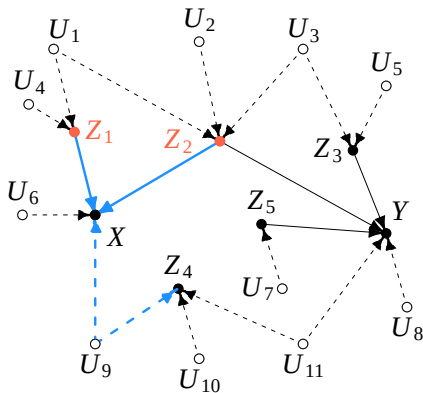
Remove arrows out of X .

Example: Sufficient Conditioning Sets



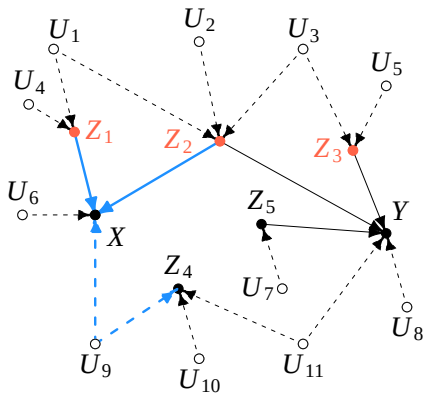
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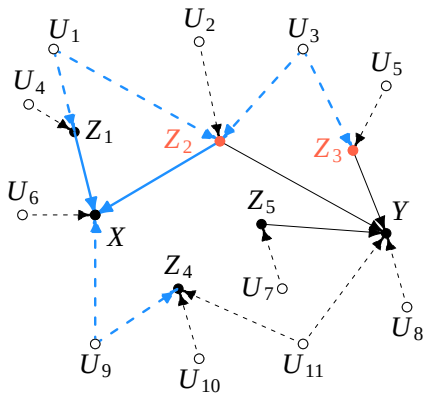
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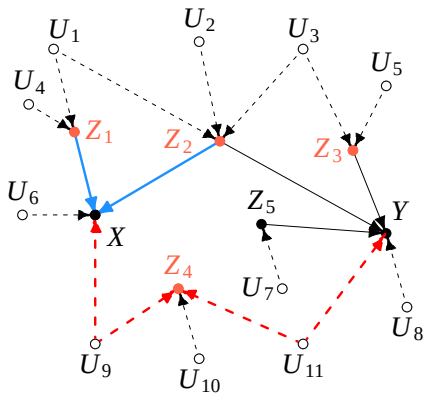
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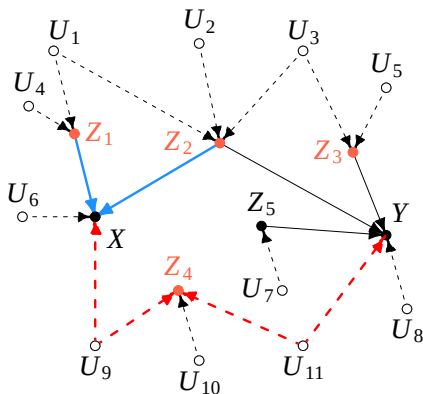
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Example: Non-sufficient Conditioning Sets

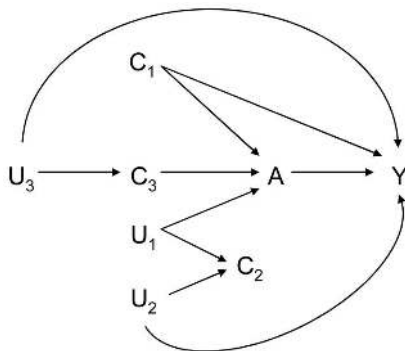


Recall that paths are blocked by “unconditioned colliders” or conditioned non-colliders

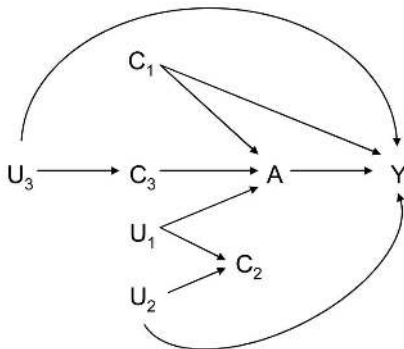
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Implications (via Vanderweele and Shpitser 2011)



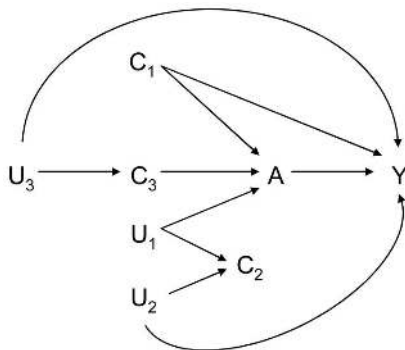
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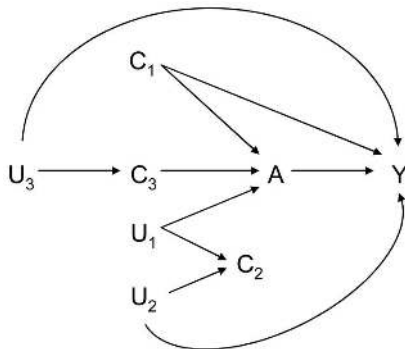
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(would condition on C_2 inducing M-bias)
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(would leave open a backdoor path $A \leftarrow C_3 \leftarrow U_3 \rightarrow Y$.)

No unmeasured confounders is not testable

- No unmeasured confounding places no restrictions on the observed data.

$$\underbrace{(Y_i(0) | D_i = 1, X_i)}_{\text{unobserved}} \stackrel{d}{=} \underbrace{(Y_i(0) | D_i = 0, X_i)}_{\text{observed}}$$

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- With backdoor criterion, you must have the correct DAG.

Assessing no unmeasured confounders

TABLE VI
THE FOX NEWS EFFECT: INTERACTIONS AND PLACEBO SPECIFICATIONS

Dep. var.	Interactions		Placebo specifications		
	Presid. Rep. vote share 2000–1996		Presidential Republican vote share 2000–1996 1996–1992 1992–1988		
	(1)	(2)	(3)	(4)	(5)
Availability of Fox News via cable in 2000	0.0109 (0.0042)***	0.0105 (0.0039)***	0.0036 (0.0016)**	−0.0024 (0.0031)	0.0026 (0.0026)
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- Unconfoundedness could still be violated even if you pass this test!

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 - ▶ All discussed in the next couple of weeks!

Where We've Been and Where We're Going...

- Last Week
 - ▶ intro to causal inference
- This Week
 - ▶ Monday:
 - ★ experimental Ideal
 - ★ identification with measured confounding
 - ▶ Wednesday:
 - ★ regression estimation
- Next Week
 - ▶ identification with unmeasured confounding
 - ▶ instrumental variables
- Long Run
 - ▶ probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causal inference

Questions?

- 1 The Experimental Ideal
- 2 Assumption of No Unmeasured Confounding
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- Identification depends on **assumptions** not statistical models.

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- A big topic I'm skipping over as outside the scope of class is the **propensity score** (conditional expectation of the treatment given the covariates).

Regression

David Freedman:

I sometimes have a nightmare about Kepler. Suppose a few of us were transported back in time to the year 1600, and were invited by the Emperor Rudolph II to set up an Imperial Department of Statistics in the court at Prague. Despairing of those circular orbits, Kepler enrolls in our department. We teach him the general linear model, least squares, dummy variables, everything. He goes back to work, fits the best circular orbit for Mars by least squares, puts in a dummy variable for the exceptional observation - and publishes. And that's the end, right there in Prague at the beginning of the 17th century.

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- When is regression causal? When the **CEF** is causal.
- This means that the question of whether regression has a causal interpretation is a question about **identification**

Identification under Selection on Observables: Regression

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- ① Constant treatment effects and outcomes are linear in X
 - ▶ τ will provide unbiased and consistent estimates of ATE.
- ② Constant treatment effects and unknown functional form
 - ▶ τ will provide well-defined linear approximation to the average causal response function $\mathbb{E}[Y|D=1, X] - \mathbb{E}[Y|D=0, X]$. Approximation may be very poor if $\mathbb{E}[Y|D, X]$ is misspecified and then τ may be biased for the ATE.

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- ③ Heterogeneous treatment effects (τ differs for different values of X)
 - ▶ If outcomes are linear in X , τ is unbiased and consistent estimator for conditional-variance-weighted average of the underlying causal effects. This average can be different from the ATE.

Identification under Selection on Observables: Regression

Identification Assumption

- ① *Constant treatment effect: $\tau = Y_{1i} - Y_{0i}$ for all i*
- ② *Control outcome is linear in X : $Y_{0i} = \beta_0 + X_i'\beta + \epsilon_i$ with $\epsilon_i \perp\!\!\!\perp X_i$ (no omitted variables and linearly separable confounding)*

Identification Result

Then $\tau_{ATE} = \mathbb{E}[Y_1 - Y_0]$ is identified by a regression of the observed outcome on the covariates and the treatment indicator

$$Y_i = \beta_0 + \tau D_i + X_i'\beta + \epsilon_i$$

Ideal Case: Linear Constant Effects Model

Assume **constant linear effects** and **linearly separable confounding**:

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- Under this model, $(Y_1, Y_0) \perp\!\!\!\perp D|X$ implies $\epsilon_i|X \perp\!\!\!\perp D$
- As a result,

$$\begin{aligned} Y_i &= \beta_0 + \tau D_i + \mathbb{E}[\eta_i] \\ &= \beta_0 + \tau D_i + X_i'\beta + \mathbb{E}[\epsilon_i] \\ &= \beta_0 + \tau D_i + X_i'\beta \end{aligned}$$

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- Thus, a regression where D_i and X_i are entered linearly can recover the ATE.

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- **Constant effects** and **linearly separable confounding** aren't very appealing or plausible assumptions
- To understand what happens when they don't hold, we need to understand the properties of regression with minimal assumptions: this is often called an agnostic view of regression.
- The Aronow and Miller book (and lecture 7) provide some context but essentially as long as we have iid sampling, we will asymptotically obtain the best linear approximation to the CEF.

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- The question, then, is when does knowing the CEF tell us something about causality?
- Angrist and Pischke argues that a regression is causal when the CEF it approximates is causal. Identification is king.
- We will show that under certain conditions, a regression of the outcome on the treatment and the covariates can recover a causal parameter, but perhaps not the one in which we are interested.

Linear constant effects model, binary treatment

Now with the benefit of covering agnostic regression, let's review again the simple case.

- Experiment: with a simple experiment, we can rewrite the consistency assumption to be a regression formula:

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- Note that if ignorability holds (as in an experiment) for $Y_i(0)$, then it will also hold for v_i^0 , since $\mathbb{E}[Y_i(0)]$ is constant. Thus, this satisfies the usual assumptions for regression.

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- We will assume a linear model for the potential outcomes:

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- Works with continuous or ordinal D_i if effect of these variables is truly linear.

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- Thus, OLS estimates the ATE with no covariates.

Adding covariates

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- What about the regression estimand, τ_R ? How does it relate to the ATE/ATT?

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- Let's investigate this under a saturated regression model:

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- Linear in X_i by construction!

Investigating the regression coefficient

- How can we investigate τ_R ? Well, we can rely on the regression anatomy:

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$$\tau_R = \frac{\mathbb{E} [\tau(X_i)(D_i - \mathbb{E}[D_i|X_i])^2]}{\mathbb{E}[(D_i - E[D_i|X_i])^2]} = \frac{\mathbb{E}[\tau(X_i)\sigma_d^2(X_i)]}{\mathbb{E}[\sigma_d^2(X_i)]}$$

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- $\sigma_d^2(x) = \text{Var}[D_i|X_i = x]$ is the conditional variance of treatment assignment.

ATE versus OLS

$$\tau_R = \mathbb{E}[\tau(X_i)W_i] = \sum_x \tau(x) \frac{\sigma_d^2(x)}{\mathbb{E}[\sigma_d^2(X_i)]} \mathbb{P}[X_i = x]$$

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- Maximum variance with $\mathbb{P}[D_i = 1|X_i = x] = 1/2$.

OLS weighting example

- Binary covariate:

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Group 1

$$\mathbb{P}[X_i = 1] = 0.75$$

Group 2

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$$\begin{aligned}\tau_R &= \mathbb{E}[\tau(X_i)W_i] \\ &= \tau(1)W(1)\mathbb{P}[X_i = 1] + \tau(0)W(0)\mathbb{P}[X_i = 0]\end{aligned}$$

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$\sigma_d^2(1) = 0.09$	$\sigma_d^2(0) = 0.25$
$\tau(1) = 1$	$\tau(0) = -1$

- Implies the ATE is $\tau = 0.5$
- Average conditional variance: $\mathbb{E}[\sigma_d^2(X_i)] = 0.13$
- $\rightsquigarrow$ weights for $X_i = 1$ are: $0.09/0.13 = 0.692$, for $X_i = 0$: $0.25/0.13 = 1.92$.

$$\begin{aligned}\tau_R &= \mathbb{E}[\tau(X_i)W_i] \\ &= \tau(1)W(1)\mathbb{P}[X_i = 1] + \tau(0)W(0)\mathbb{P}[X_i = 0] \\ &= 1 \times 0.692 \times 0.75 + -1 \times 1.92 \times 0.25\end{aligned}$$

OLS weighting example

- Binary covariate:

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- Incorrect linearity assumption in X_i will lead to more bias.

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- How can we use this?

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- Sometimes called an **imputation estimator**.

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Simple imputation estimator

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- Useful trick: use a model on the entire data and `model.frame()` to get the right design matrix:

```
## heterogeneous effects
y.het <- ifelse(d == 1, y + rnorm(n, 0, 5), y)

mod <- lm(y.het ~ d + X)
mod1 <- lm(y.het ~ X, subset = d == 1)
mod0 <- lm(y.het ~ X, subset = d == 0)
y1.imps <- predict(mod1, model.frame(mod))
y0.imps <- predict(mod0, model.frame(mod))
mean(y1.imps - y0.imps)
```

```
## [1] 0.61
```

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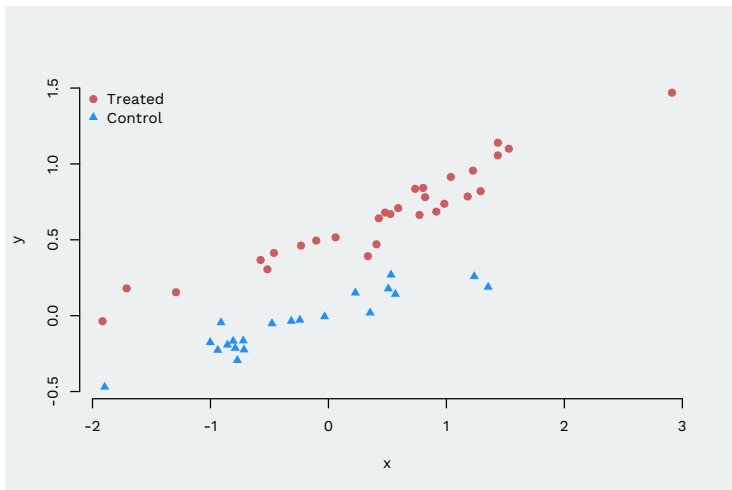
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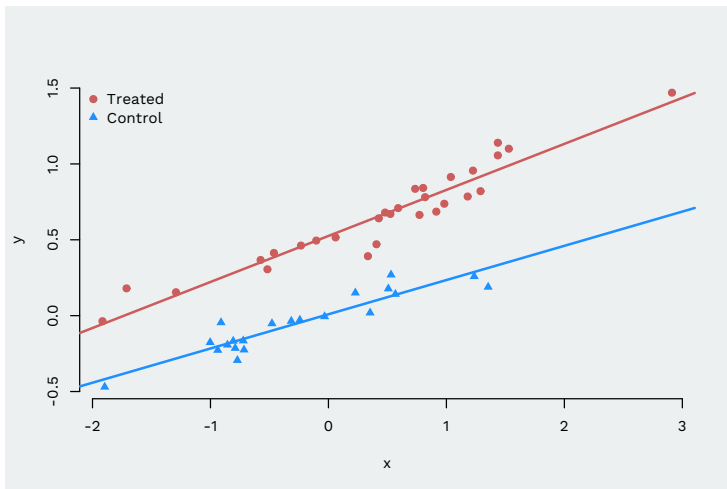
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 - ▶ Easiest is generalized additive models (GAMs)

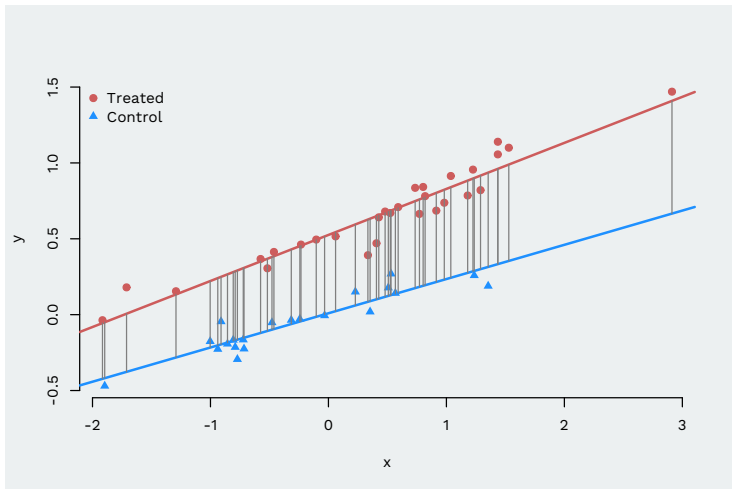
Imputation estimator visualization



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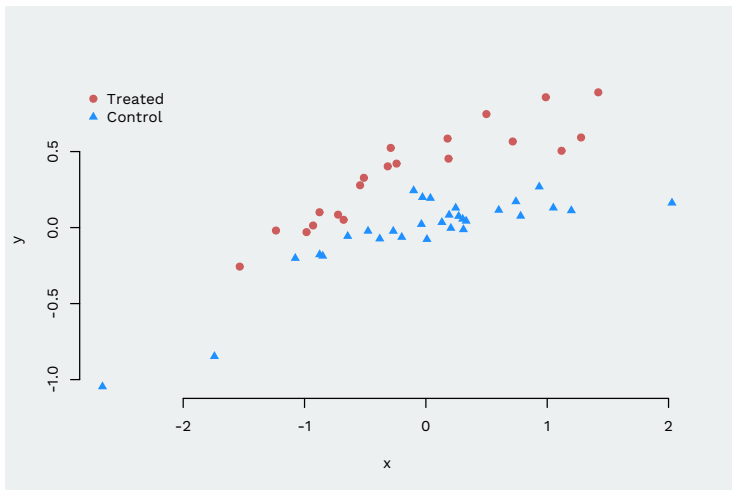


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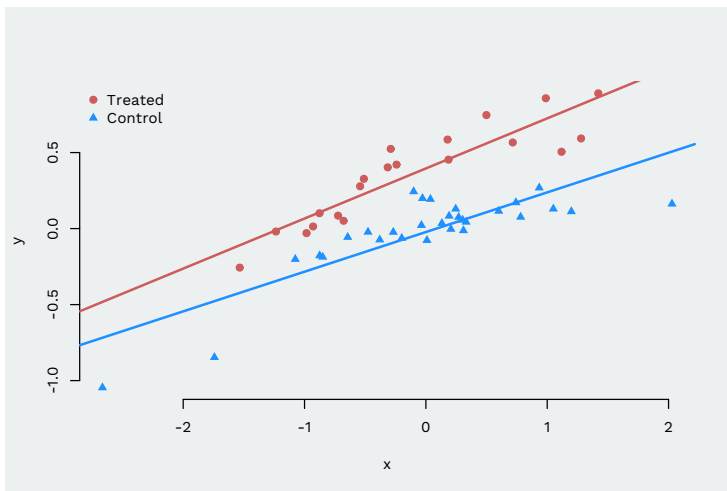
Nonlinear relationships

- Same idea but with nonlinear relationship between Y_i and X_i :



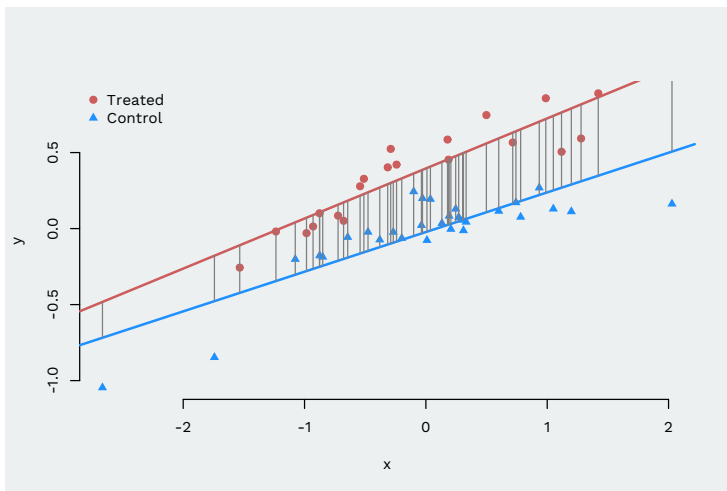
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Using semiparametric regression

- Here, CEFs are nonlinear, but we don't know their form.

```
library(mgcv)
mod0 <- gam(y ~ s(x), subset = d == 0)
summary(mod0)
```

```
##
## Family: gaussian
## Link function: identity
##
## Formula:
## y ~ s(x)
##
## Parametric coefficients:
##             Estimate Std. Error t value Pr(>|t|)
## (Intercept) -0.0225     0.0154   -1.46    0.16
##
## Approximate significance of smooth terms:
##             edf Ref.df    F p-value
## s(x) 6.03     7.08 41.3  <2e-16 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
```

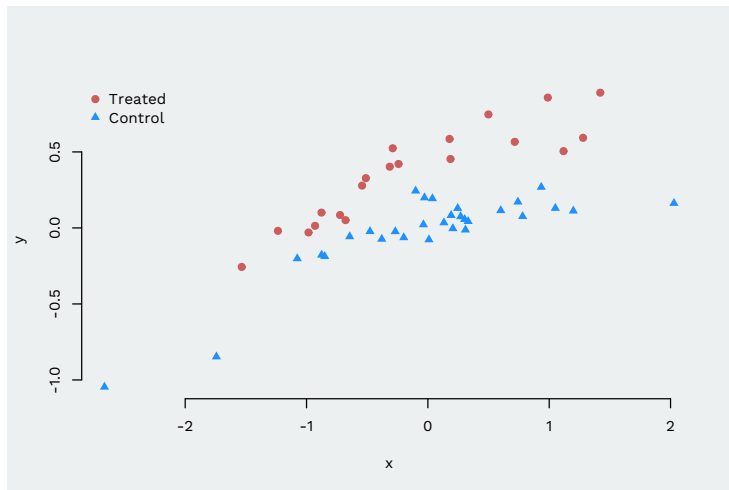
Using semiparametric regression

- Here, CEFs are nonlinear, but we don't know their form.
- We can use GAMs from the `mgcv` package to for flexible estimate:

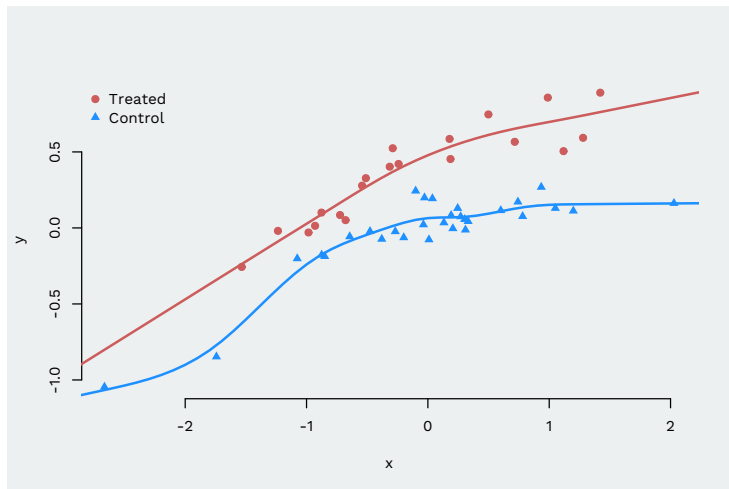
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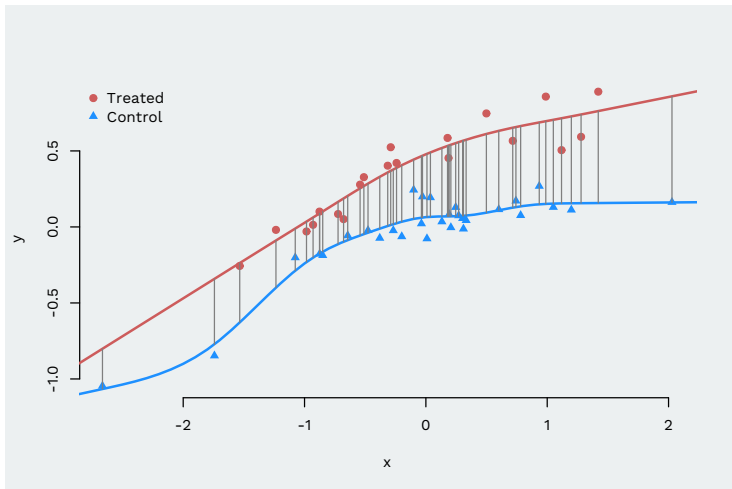
Using GAMs



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Using GAMs



‘Wait...so what are we actually doing most of the time?’

A Discussion

Conclusions

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- Regression is mechanically very simple, but philosophically somewhat complicated
- It is a useful descriptive tool for approximating a conditional expectation function
- Once again though, the estimand of interest isn't necessarily the regression coefficient.
- There are many other approaches to estimation, but **identification** is key.

Next Week

- Causality with Unmeasured Confounding
- Reading:
 - ▶ Angrist and Pischke Chapter 4 Instrumental Variables and Chapter 6 on Regression Discontinuity Designs
 - ▶ Morgan and Winship Chapter 9 Instrumental Variable Estimators of Causal Effects
 - ▶ Optional: Hernan and Robins Chapter 16 Instrumental Variable Estimation

- 1 The Experimental Ideal
- 2 Assumption of No Unmeasured Confounding
- 3 Estimation Under No Unmeasured Confounding
- 4 Regression Estimators
- 5 Regression and Causality
- 6 Regression Under Heterogeneous Effects
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Visualization in the New York Times

AMERICAS

How Stable Are Democracies? ‘Warning Signs Are Flashing

The Interpreter

By AMANDA TAUB NOV. 29, 2016

WASHINGTON — Yascha Mounk is used to being the most pessimistic person in the room. Mr. Mounk, a lecturer in government at Harvard, has spent the past few years challenging one of the bedrock assumptions of Western politics: that once a country becomes a liberal democracy, it will stay that way.

His research suggests something quite different: that liberal democracies around the world may be at serious risk of decline.

Mr. Mounk’s interest in the topic began rather unusually. In 2014, he published a book, [“Stranger in My Own Country.”](#) It started as a memoir of his experiences growing up as a Jew in Germany, but became a broader investigation of how contemporary European nations were struggling to construct new, multicultural national identities.

The Danger of Deconsolidation

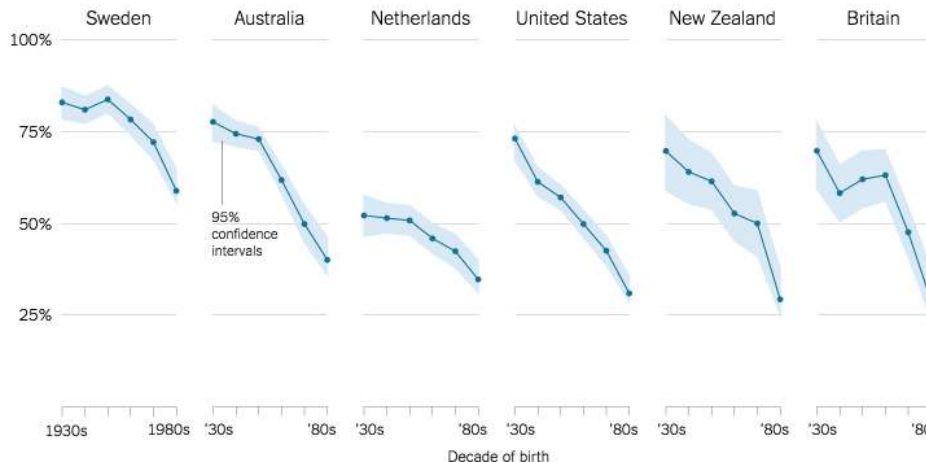
THE DEMOCRATIC DISCONNECT

Roberto Stefan Foa and Yascha Mounk

Roberto Stefan Foa is a principal investigator of the World Values Survey and fellow of the Laboratory for Comparative Social Research. His writing has appeared in a wide range of journals, books, and publications by the UN, OECD, and World Bank. Yascha Mounk is a lecturer on political theory in Harvard University's Government Department and a Carnegie Fellow at New America, a Washington, D.C.-based think tank. His dissertation on the role of personal responsibility in contemporary politics and philosophy will be published by Harvard University Press, and his essays have appeared in Foreign Affairs, the New York Times, and the Wall Street Journal.

Visualization in the New York Times

Percentage of people who say it is “essential” to live in a democracy



Source: Yascha Mounk and Roberto Stefan Foa, "The Signs of Democratic Deconsolidation," *Journal of Democracy* | By The New York Times

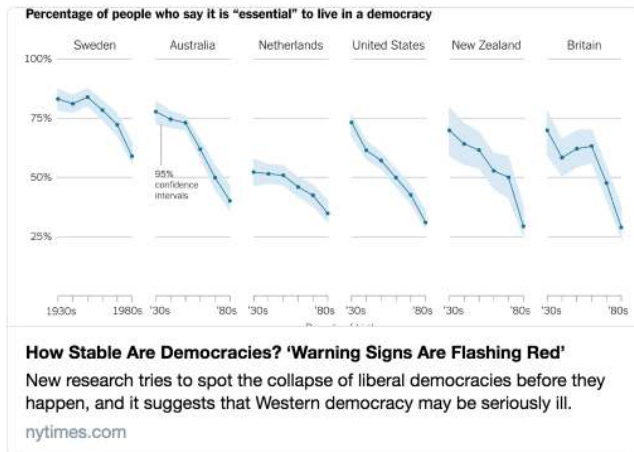
Visualization in the New York Times



Ryan D. Enos @RyanDEnos · 19h

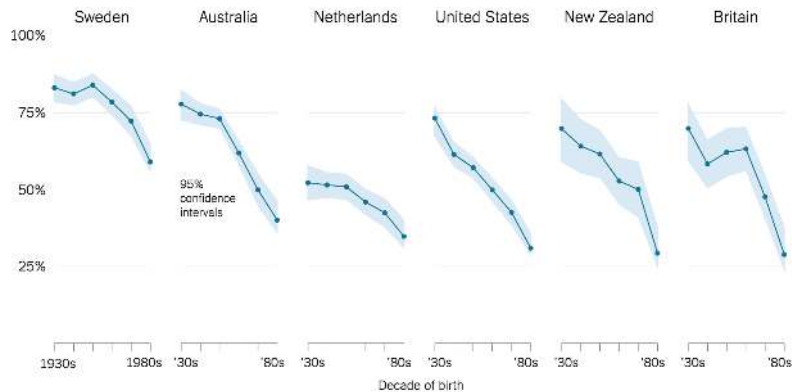


Lots of worried chatter a/b @amandataub article on work of @Yascha_Mounk.
Important, but want to raise cautions 1/



Alternate Graphs

Percentage of people who say it is “essential” to live in a democracy

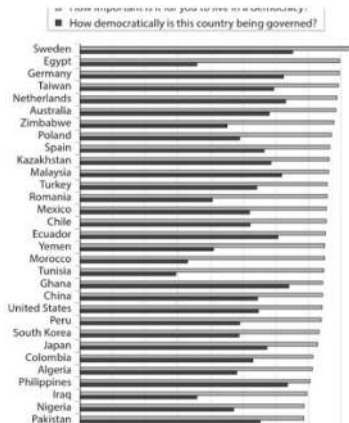
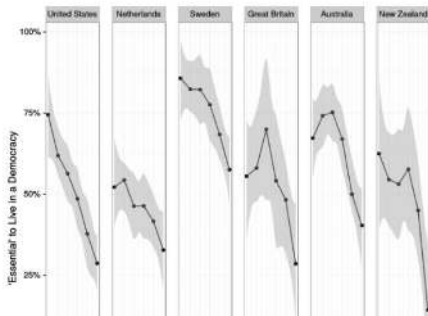


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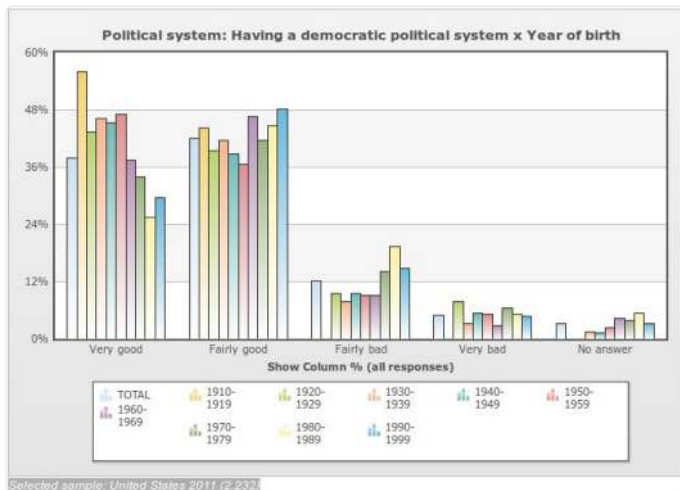
.@RyanDEnos Compare NYT/JoD (left) to the very same data analysed differently by Bartels and Achen (2016) (right). Extreme score vs means.

Across numerous countries, including Australia, Britain, the Netherlands, New Zealand, Sweden and the United States, the percentage of people who say it is "essential" to live in a democracy has plummeted, and it is especially low among younger generations.



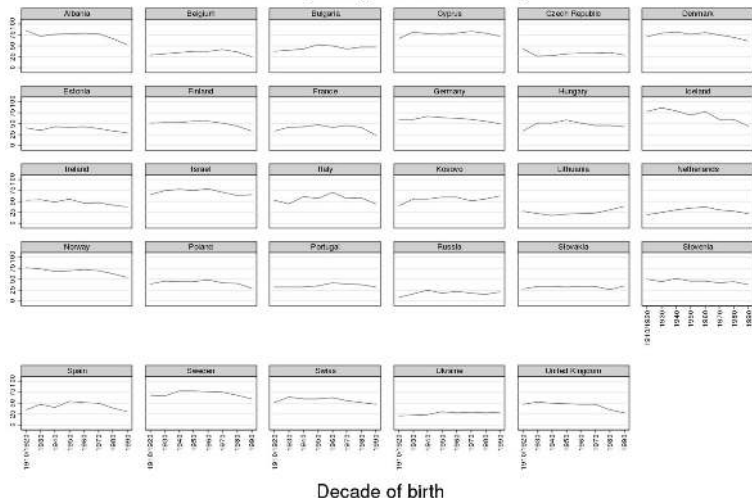
Alternate Graphs

@RyanDEnos They also stop at the 80s cohort.
The data has the 90's as well. I wonder why they would stop there...



Alternate Graphs

Percentage of people who say it is *extremely important* to live in a country that is governed democratically



Source: ESS Wave 8

In reply to Ryan D. Enos

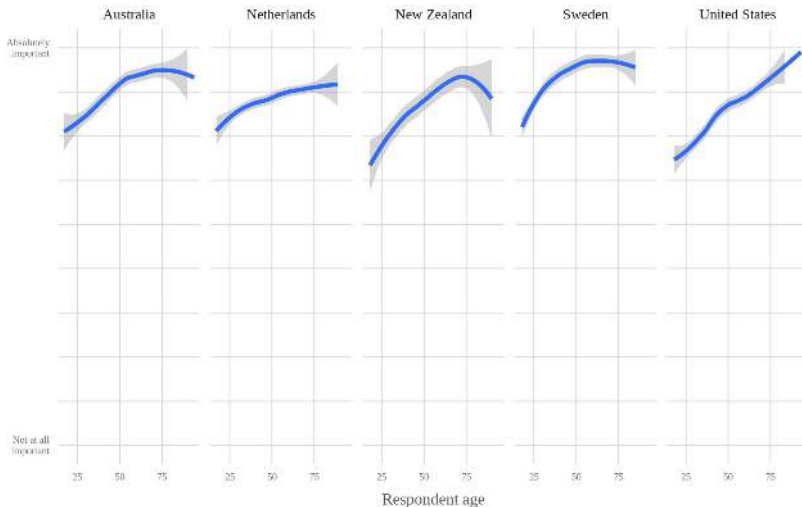


Benjamin Sack @bsack - 15m

@RyanDEnos Same analysis strategy with comparable data from [@ESS_Survey](#) (similar item, 0-10 scale) shows slightly different pattern, too.

Alternate Graphs

How important is it for you to live in a country that is governed democratically?

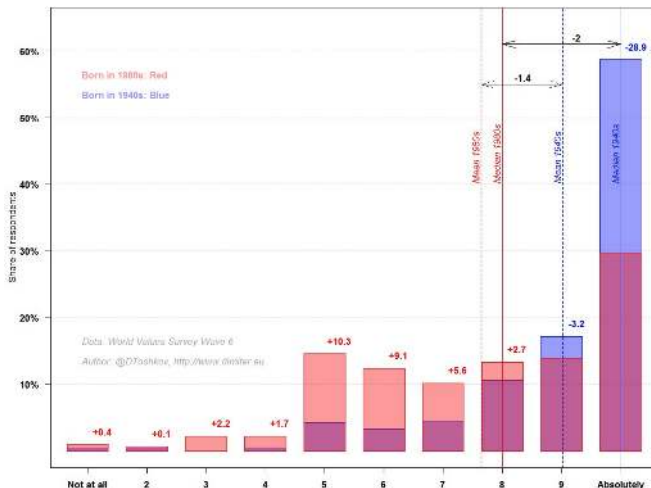


614 Bantam @jacobach · 16h

@FlyanDinos @bshor @nataliemlb @TomWQvdMeer this is a "quick and dirty" plot I did with WVS wave 6. Not quite so terrifying.

Alternate Graphs

How important is it for you to live in a country that is governed democratically? United States, 2011



Dimitar Toshkov @DToshkov · 31m

my take on the democratic deconsolidation graph that scared everyone yesterday. Blue is 1940s cohort, red is 1980s. First, United States

Thoughts

Two stories here:

Thoughts

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- 1 Visualization and data coding choices are important

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Two stories here:

- 1 Visualization and data coding choices are important
- 2 The internet is amazing (especially with replication data being available!)