Week 10: Causality with Measured Confounding

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Princeton

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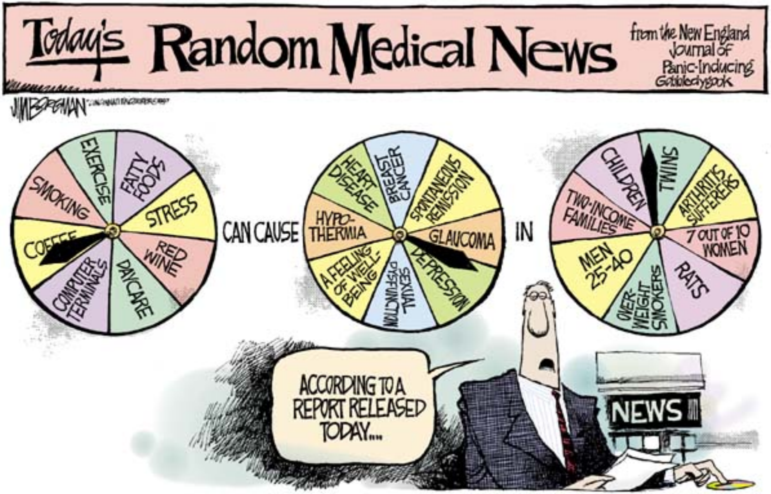
[1]These slides are heavily influenced by Matt Blackwell, Jens Hainmueller, Erin Hartman, Kosuke Imai and Gary King.

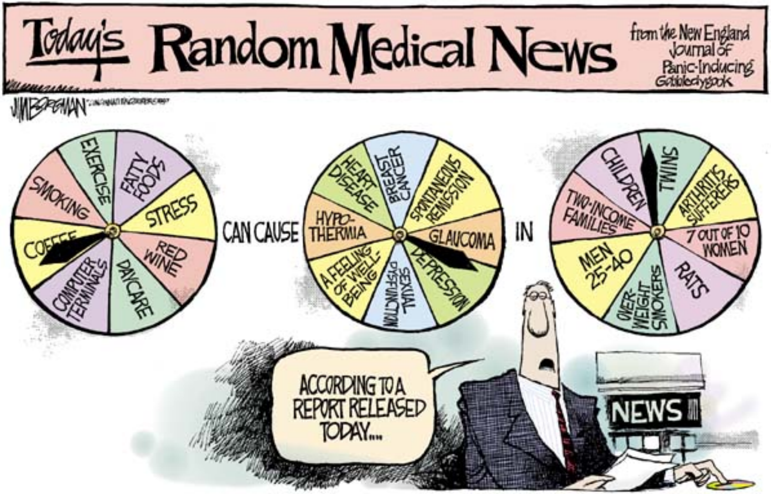
Where We've Been and Where We're Going...

- Last Week
  - regression diagnostics
- This Week
  - Monday:
    - experimental Ideal
    - identification with measured confounding
  - Wednesday:
    - regression estimation
- Next Week
  - identification with unmeasured confounding
  - instrumental variables
- Long Run
  - causality with measured confounding → unmeasured confounding → repeated data

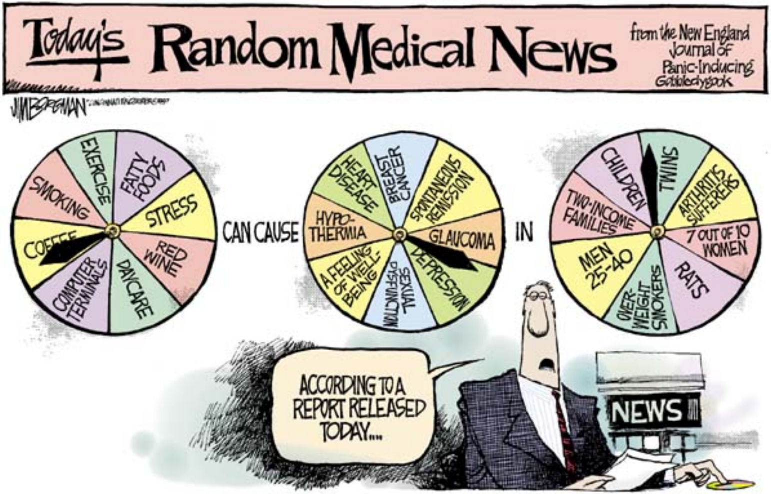
Questions?



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)



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



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.

Why So Much Variation?

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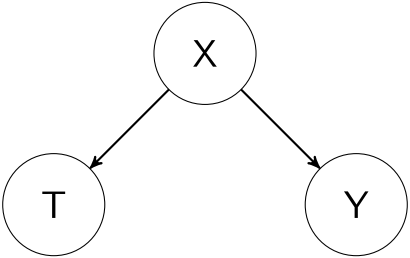
Confounders



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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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   - 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?

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& E[Y_i|D_i = 1] - E[Y_i|D_i = 0] \\
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When all goes well, an experiment eliminates selection bias.

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    - 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

Stratification Example: Smoking and Mortality (Cochran, 1968)

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

What is the average death rate for Pipe Smokers?

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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.
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Cigarettes	28.3	12.8	17.7
Cigars/pipes	21.2	12.0	14.2

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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:

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

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Identification Result

Similarly,

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

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	$\mathbb{E}[Y_1 \vert X = 0, D = 1]$	$\mathbb{E}[Y_0 \vert X = 0, D = 1]$	1	0
2			1	0
3	$\mathbb{E}[Y_1 \vert X = 0, D = 0]$	$\mathbb{E}[Y_0 \vert X = 0, D = 0]$	0	0
4			0	0
5	$\mathbb{E}[Y_1 \vert X = 1, D = 1]$	$\mathbb{E}[Y_0 \vert X = 1, D = 1]$	1	1
6			1	1
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$(Y_1, Y_0) \perp\!\!\!\perp D \mid X$ implies that we conditioned on all confounders. The treatment is randomly assigned within each stratum of X:

$$\begin{aligned}
\mathbb{E}[Y_0 \mid X=0, D=1] &= \mathbb{E}[Y_0 \mid X=0, D=0] \text{ and} \\
\mathbb{E}[Y_0 \mid X=1, D=1] &= \mathbb{E}[Y_0 \mid X=1, D=0]
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$(Y_1, Y_0) \perp\!\!\!\perp D \mid X$ also implies

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- No unmeasured confounding assumes that we’ve measured all sources of confounding.

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

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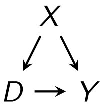
- Backdoor paths between D and Y $\rightsquigarrow$ common causes of D and Y:



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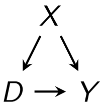
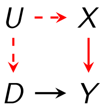


- Here there is a backdoor path $D \leftarrow X \rightarrow Y$, where X is a common cause for the treatment and the outcome.

Other types of confounding



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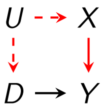

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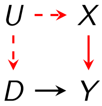

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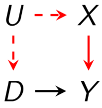

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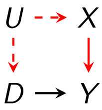

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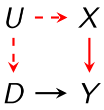
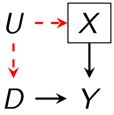


- D is exercise.
- Y is having a disease.
- U is lifestyle.
- X is smoking
- Big assumption here: no arrow from U to Y.

What's the problem with backdoor paths?



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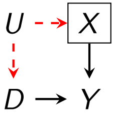

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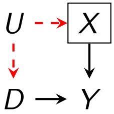

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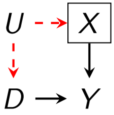

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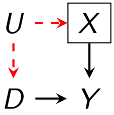

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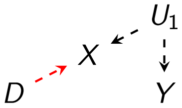
- In the DAG here, if we condition on X, then the backdoor path is blocked.

Not all backdoor paths



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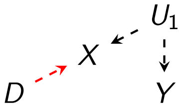

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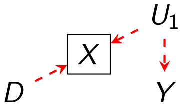

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Don't condition on post-treatment variables

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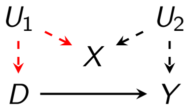
Every time you do, a puppy cries.

M-bias



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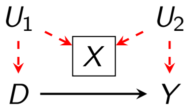

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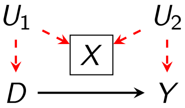

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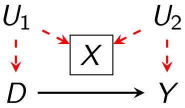

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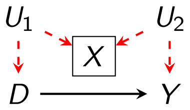

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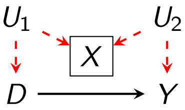

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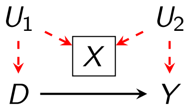

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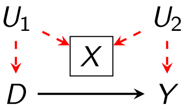

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  - See the Elwert and Winship piece for more!

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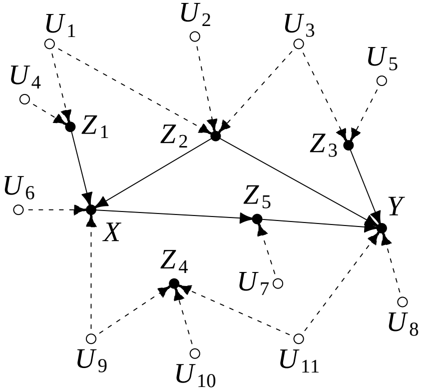
  - what variables to condition on to eliminate the confounding.

Example: Sufficient Conditioning Sets



Remove arrows out of X.

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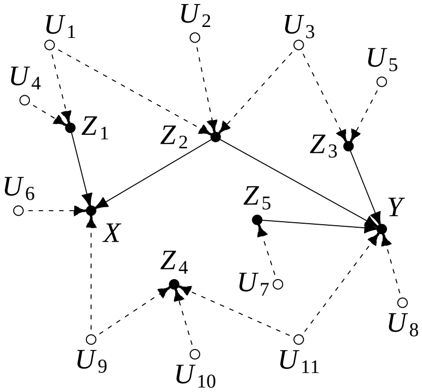

Recall that paths are blocked by "unconditioned colliders" or conditioned non-colliders

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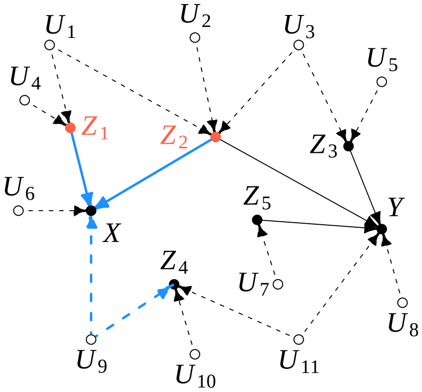

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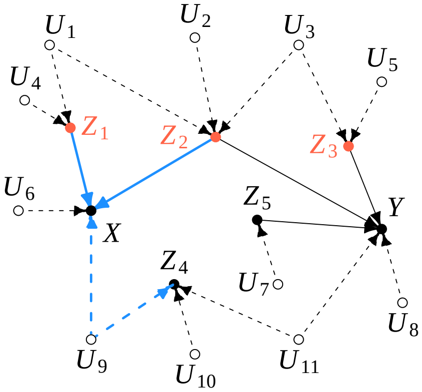

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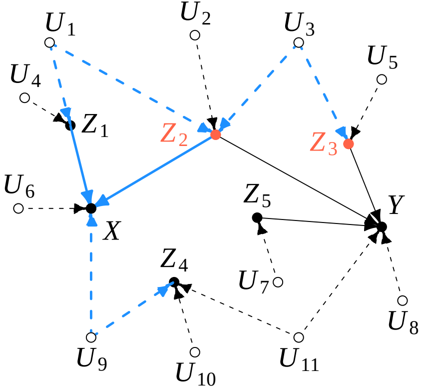

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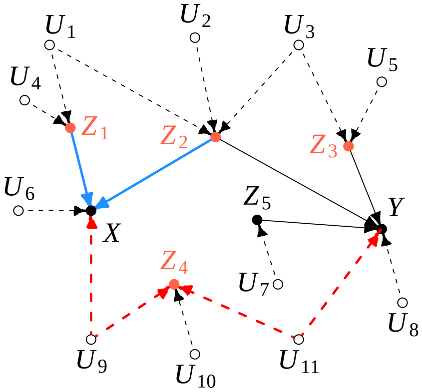

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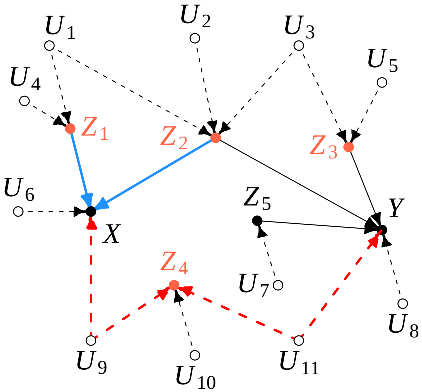

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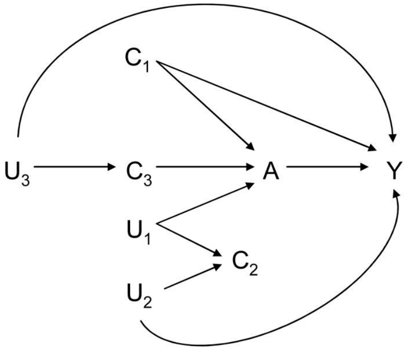

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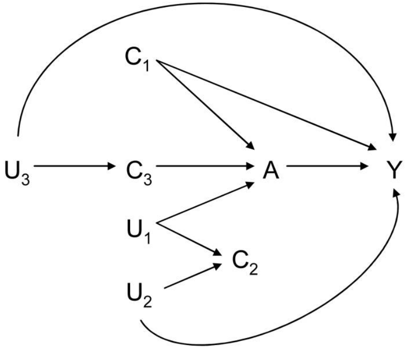

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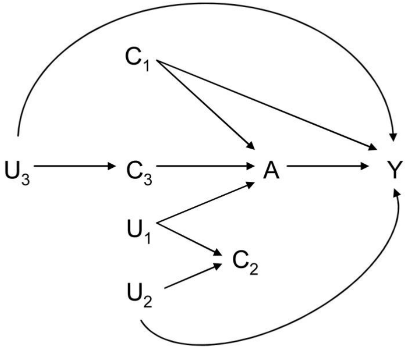

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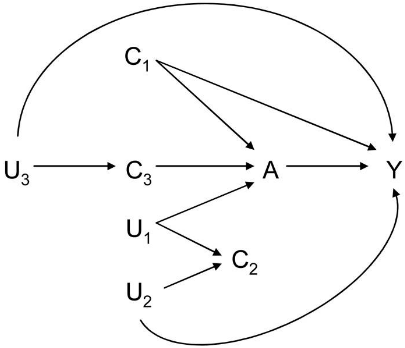

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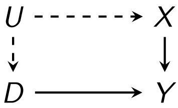
   (would leave open a backdoor path $A \leftarrow C_3 \leftarrow U_3 \rightarrow Y$.)

SWIGs



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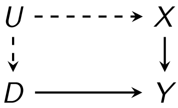

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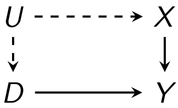

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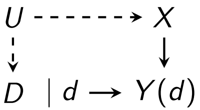

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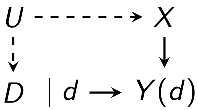

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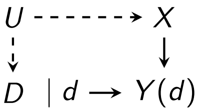

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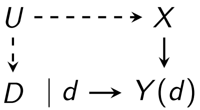

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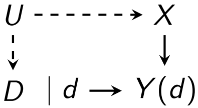

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  - Conditioning on X blocks that backdoor path $\rightsquigarrow D \perp\!\!\!\perp Y(d) | X$

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Assessing no unmeasured confounders

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THE FOX NEWS EFFECT: INTERACTIONS AND PLACEBO SPECIFICATIONS

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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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  - All discussed in the next couple of weeks!

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- Causal inference is hard but worth doing!

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- This line of work is one of my favorites.

Sequence of slides that follow courtesy of King, Pan and Roberts

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Either or both could be right or wrong.

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(They also censor 2 other smaller categories)

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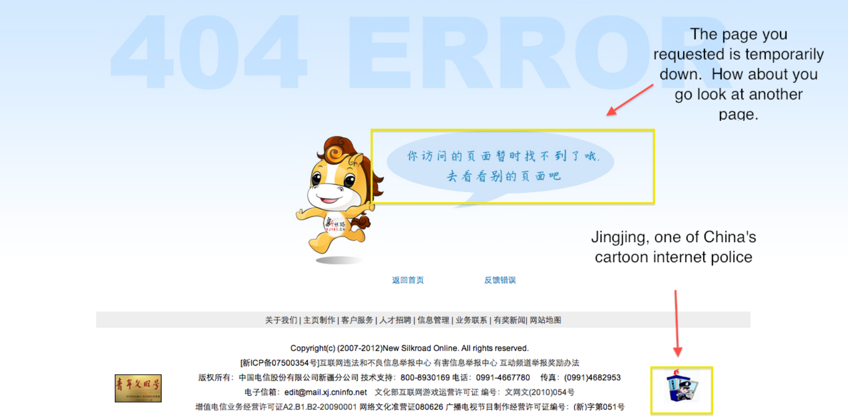
  - Use computer-assisted methods of text analysis (some existing, some new, all adapted to Chinese)

Censorship is not Ambiguous: BBS Error Page



For 2 Unusual Topics: Constant Censorship Effort

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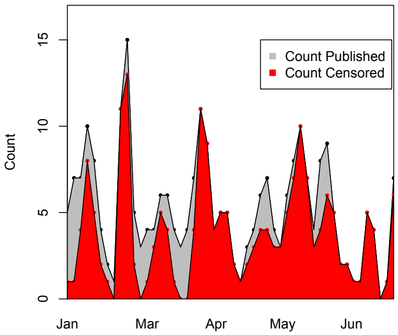
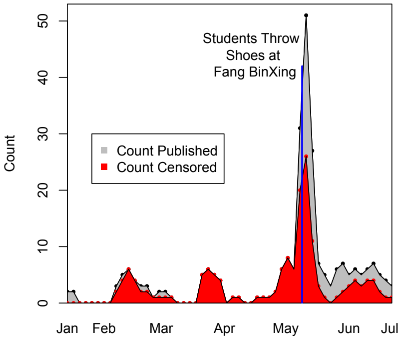


Pornography



Criticism of the Censors

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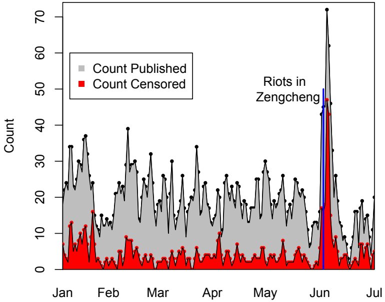

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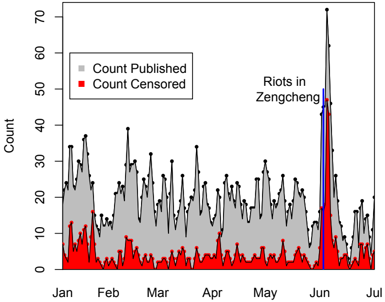

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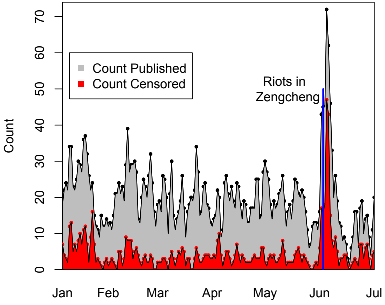

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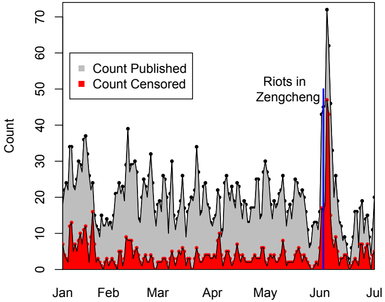

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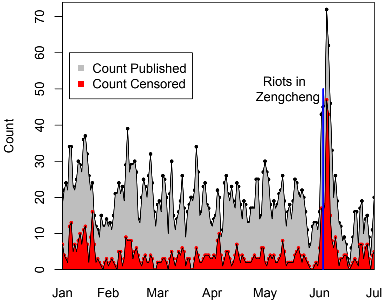

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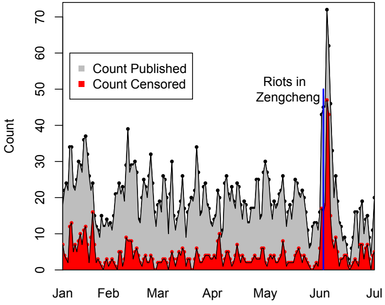

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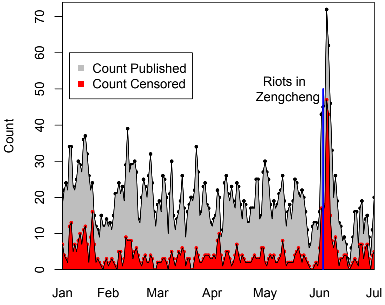

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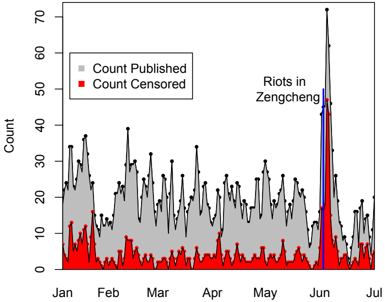

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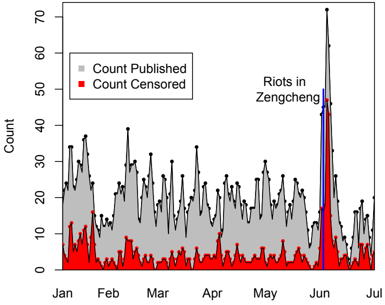

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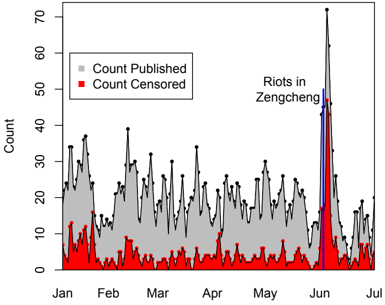

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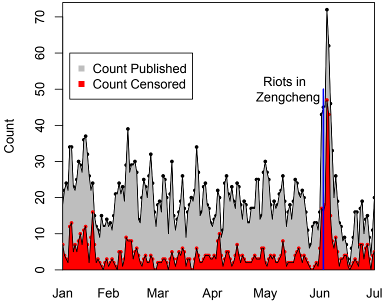

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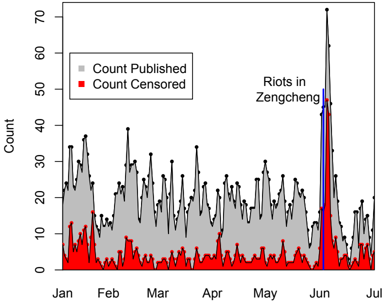

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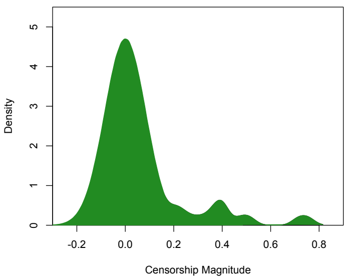

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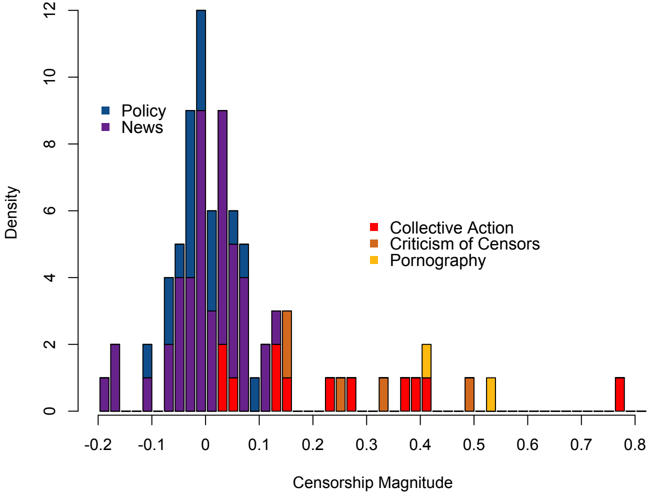

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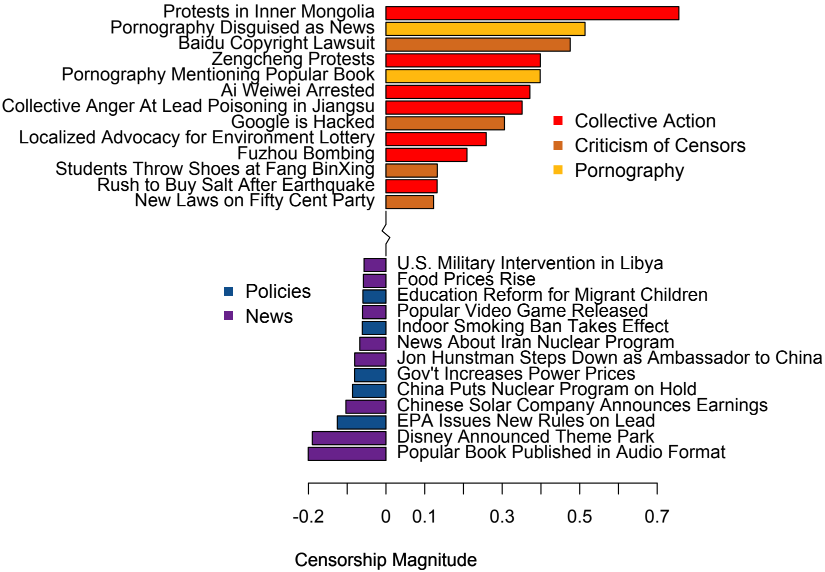
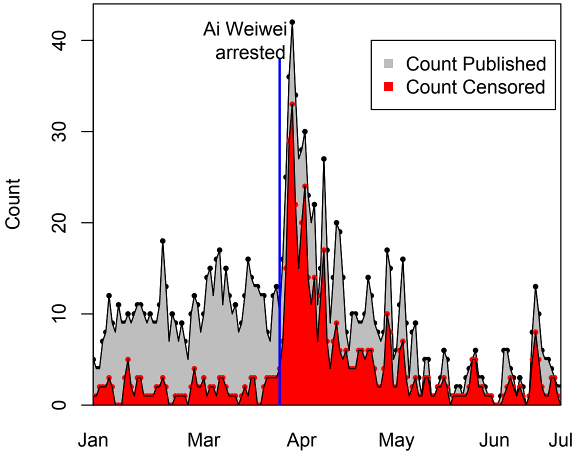
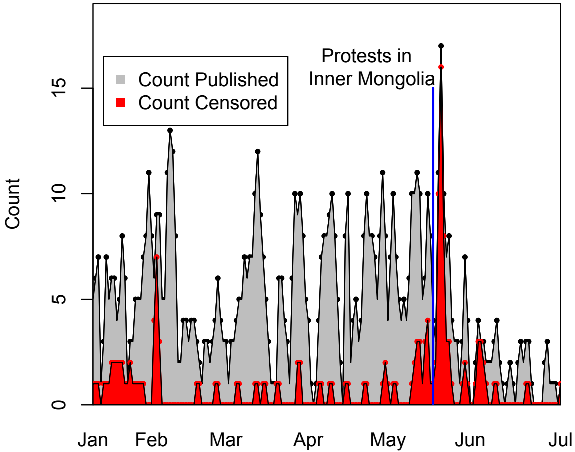


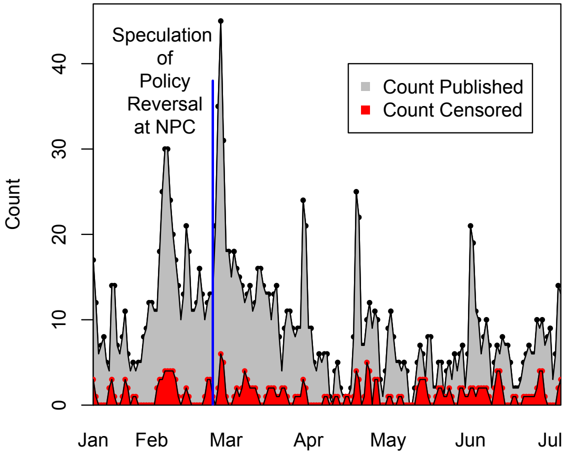
Censoring Collective Action: Ai Weiwei's Arrest



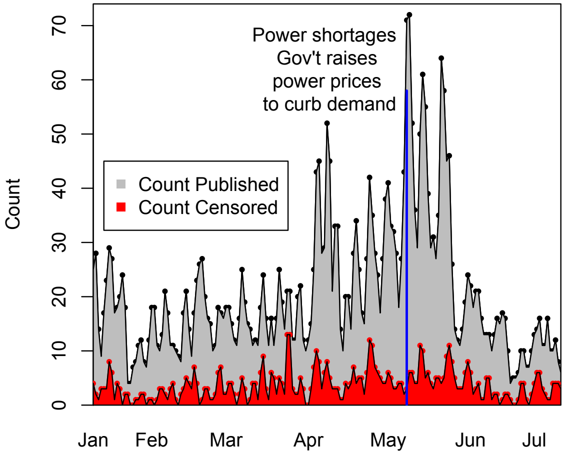
Censoring Collective Action: Protests in Inner Mongolia



Low Censorship on One Child Policy



Low Censorship on News: Power Prices



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

- Last Week
  - regression diagnostics
- This Week
  - Monday:
    - experimental Ideal
    - identification with measured confounding
  - Wednesday:
    - regression estimation
- Next Week
  - identification with unmeasured confounding
  - instrumental variables
- Long Run
  - causality with measured confounding $\rightarrow$ unmeasured confounding $\rightarrow$ repeated data

Questions?

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

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   - 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 is often 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

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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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- The Aronow and Miller book is an excellent introduction to the agnostic view of regression and I recommend checking it out. Here I will give you just a flavor of it.

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- $\rightsquigarrow$ OLS is BLUE, plus normality of the errors and we get small sample SEs.
- What is the basic approach here? It is a model for the conditional distribution of Y_i given X_i:

$$[Y_i|X_i] \sim N(X_i'\beta, \sigma^2)$$

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- Lose the distributional assumptions, focus on the conditional expectation function (CEF):

$$\mu(x) = \mathbb{E}[Y_i|X_i = x] = \sum_y y \cdot \mathbb{P}[Y_i = y|X_i = x]$$

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- In other words, even a non-linear CEF has a "true" linear approximation, even though that approximation may not be great.

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- With more covariates, β is more complicated, but we can still write it like this.
- Let $\tilde{X}_{ki}$ be the residual from a regression of X_{ki} on all the other independent variables. Then, β_k, the coefficient for X_{ki} is:

$$\beta_k = \frac{\text{Cov}(Y_i, \tilde{X}_{ki})}{\mathrm{Var}(\tilde{X}_{ki})}$$

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  1. Outcome and covariates are multivariate normal.
  2. Linear regression model is saturated.
- A model is saturated if there are as many parameters as there are possible combination of the X_i variables.

Saturated model example

- Two binary variables, X_{1i} for marriage status and X_{2i} for having children.

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$$E[Y_i|X_{1i} = 1, X_{2i} = 0]$$
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- We can write the CEF as follows:

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$$
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\end{aligned}
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- Or, a series of dummies for each unique combination of X_i.
- This makes linearity hold mechanically and so linearity is not an assumption.

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```
girls <- foreign::read.dta("girls.dta")
head(girls[, c("name", "totchi", "aauw")])
```

```
##                     name totchi aauw
## 1   ABERCROMBIE, NEIL      0  100
## 2   ACKERMAN, GARY L.      3   88
## 3 ADERHOLT, ROBERT B.      0    0
## 4     ALLEN, THOMAS H.     2  100
## 5  ANDREWS, ROBERT E.      2  100
## 6          ARCHER, W.R.    7    0
```

Linear model

```
summary(lm(aauw ~ totchi, data = girls))
```

```
##
## Coefficients:
##             Estimate Std. Error t value Pr(>|t|)
## (Intercept)    61.31       1.81   33.81   <2e-16 ***
## totchi         -5.33       0.62   -8.59   <2e-16 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## Residual standard error: 42 on 1733 degrees of freedom
##   (5 observations deleted due to missingness)
## Multiple R-squared:  0.0408, Adjusted R-squared:  0.0403
## F-statistic: 73.8 on 1 and 1733 DF,  p-value: <2e-16
```

Saturated model

```
summary(lm(aauw ~ as.factor(totchi), data = girls))
```

```
##
## Coefficients:
##                     Estimate Std. Error t value Pr(>|t|)
## (Intercept)            56.41       2.76   20.42  < 2e-16 ***
## as.factor(totchi)1      5.45       4.11    1.33   0.1851
## as.factor(totchi)2     -3.80       3.27   -1.16   0.2454
## as.factor(totchi)3    -13.65       3.45   -3.95  8.1e-05 ***
## as.factor(totchi)4    -19.31       4.01   -4.82  1.6e-06 ***
## as.factor(totchi)5    -15.46       4.85   -3.19   0.0015 **
## as.factor(totchi)6    -33.59      10.42   -3.22   0.0013 **
## as.factor(totchi)7    -17.13      11.41   -1.50   0.1336
## as.factor(totchi)8    -55.33      12.28   -4.51  7.0e-06 ***
## as.factor(totchi)9    -50.41      24.08   -2.09   0.0364 *
## as.factor(totchi)10   -53.41      20.90   -2.56   0.0107 *
## as.factor(totchi)12   -56.41      41.53   -1.36   0.1745
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## Residual standard error: 41 on 1723 degrees of freedom
##   (5 observations deleted due to missingness)
## Multiple R-squared:  0.0506, Adjusted R-squared:  0.0446
## F-statistic: 8.36 on 11 and 1723 DF,  p-value: 1.84e-14
```

Saturated model minus the constant

```
summary(lm(aauw ~ as.factor(totchi) - 1, data = girls))
```

```
##
## Coefficients:
##                      Estimate Std. Error t value Pr(>|t|)
## as.factor(totchi)0      56.41       2.76   20.42   <2e-16 ***
## as.factor(totchi)1      61.86       3.05   20.31   <2e-16 ***
## as.factor(totchi)2      52.62       1.75   30.13   <2e-16 ***
## as.factor(totchi)3      42.76       2.07   20.62   <2e-16 ***
## as.factor(totchi)4      37.11       2.90   12.79   <2e-16 ***
## as.factor(totchi)5      40.95       3.99   10.27   <2e-16 ***
## as.factor(totchi)6      22.82      10.05    2.27   0.0233 *
## as.factor(totchi)7      39.29      11.07    3.55   0.0004 ***
## as.factor(totchi)8       1.08      11.96    0.09   0.9278
## as.factor(totchi)9       6.00      23.92    0.25   0.8020
## as.factor(totchi)10      3.00      20.72    0.14   0.8849
## as.factor(totchi)12      0.00      41.43    0.00   1.0000
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## Residual standard error: 41 on 1723 degrees of freedom
##   (5 observations deleted due to missingness)
## Multiple R-squared:  0.587,  Adjusted R-squared:  0.584
## F-statistic:  204 on 12 and 1723 DF,  p-value: <2e-16
```

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```
c1 <- coef(lm(aauw ~ as.factor(totchi) - 1, data = girls))
c2 <- with(girls, tapply(aauw, totchi, mean, na.rm = TRUE))
rbind(c1, c2)
```

```
##      0  1  2  3  4  5  6  7   8 9 10 12
## c1 56 62 53 43 37 41 23 39 1.1 6  3  0
## c2 56 62 53 43 37 41 23 39 1.1 6  3  0
```

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- Warning if the CEF is very nonlinear then this approximation could be terrible!!

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&= \mathbb{E}[X_i Y_i] - \mathbb{E}[X_i X_i'\beta] \\
&= \mathbb{E}[X_i Y_i] - \mathbb{E}\left[X_i X_i' \mathbb{E}[X_i X_i']^{-1}\mathbb{E}[X_i Y_i]\right] \\
&= \mathbb{E}[X_i Y_i] - \mathbb{E}[X_i X_i']\mathbb{E}[X_i X_i']^{-1}\mathbb{E}[X_i Y_i] \\
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\end{aligned}$$

- No assumptions on the linearity of $\mathbb{E}[Y_i | X_i]$.

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- If you work through the matrix algebra, this turns out to be:

$$\hat{\beta} = \left(\mathbf{X}'\mathbf{X}\right)^{-1}\mathbf{X}'\mathbf{y}$$

Asymptotic OLS inference

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- No linearity assumption needed!

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- See Aronow and Miller for much more.

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- The question, then, is when does knowing the CEF tell us something about causality?

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Regression and causality

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- Angrist and Pishke 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

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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 satifies the usual assumptions for regression.

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- Consistency assumption allows us to write this as:

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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.

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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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- $\sigma_d^2(x) = \mathrm{Var}[D_i|X_i = x]$ 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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- Average conditional variance: $\mathbb{E}[\sigma_d^2(X_i)] = 0.13$

OLS weighting example

- Binary covariate:

Group 1	Group 2
$\mathbb{P}[X_i = 1] = 0.75$	$\mathbb{P}[X_i = 0] = 0.25$
$\mathbb{P}[D_i = 1 \vert X_i = 1] = 0.9$	$\mathbb{P}[D_i = 1 \vert X_i = 0] = 0.5$
$\sigma_d^2(1) = 0.09$	$\sigma_d^2(0) = 0.25$
$\tau(1) = 1$	$\tau(0) = -1$

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- $\rightsquigarrow$ weights for $X_i = 1$ are: $0.09/0.13 = 0.692$, for $X_i = 0$: $0.25/0.13 = 1.92$.

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$$\begin{aligned} &\text{Group 1} & &\text{Group 2} \\ \mathbb{P}[X_i = 1] &= 0.75 & \mathbb{P}[X_i = 0] &= 0.25 \\ \mathbb{P}[D_i = 1 | X_i = 1] &= 0.9 & \mathbb{P}[D_i = 1 | X_i = 0] &= 0.5 \\ \sigma^2_d(1) &= 0.09 & \sigma^2_d(0) &= 0.25 \\ \tau(1) &= 1 & \tau(0) &= -1 \end{aligned}$$

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OLS weighting example

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&= \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 \\
&= 0.039
\end{aligned}$$

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  - Implies that the OLS weights are 1.
- Incorrect linearity assumption in X_i will lead to more bias.

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- Estimate a regression of Y_i on X_i among the $D_i = d$ group.
- Then, $\hat{\mu}_d(x)$ is just a predicted value from the regression for $X_i = x$.
- How can we use this?

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- Impute the treated potential outcomes with $\widehat{Y}_i(1) = \hat{\mu}_1(X_i)$!

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$$\tau_{imp} = \frac{1}{N} \sum_i \hat{\mu}_1(X_i) - \hat{\mu}_0(X_i)$$

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- Sometimes called an **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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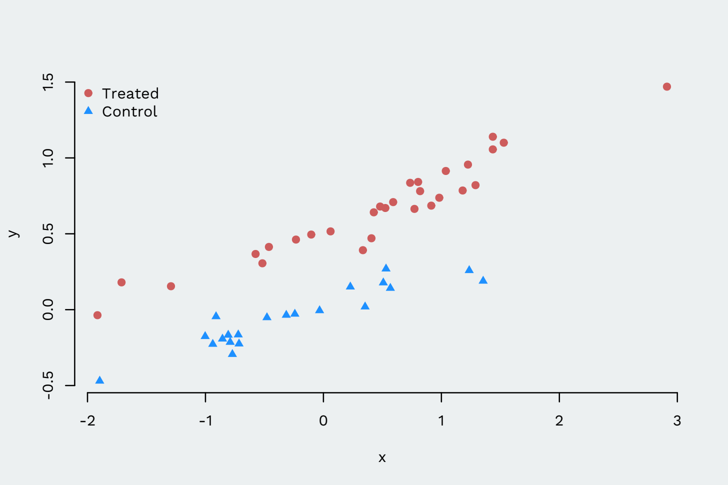
  - Kernel regression, local linear regression, regression trees, etc
  - Easiest is generalized additive models (GAMs)

Imputation estimator visualization



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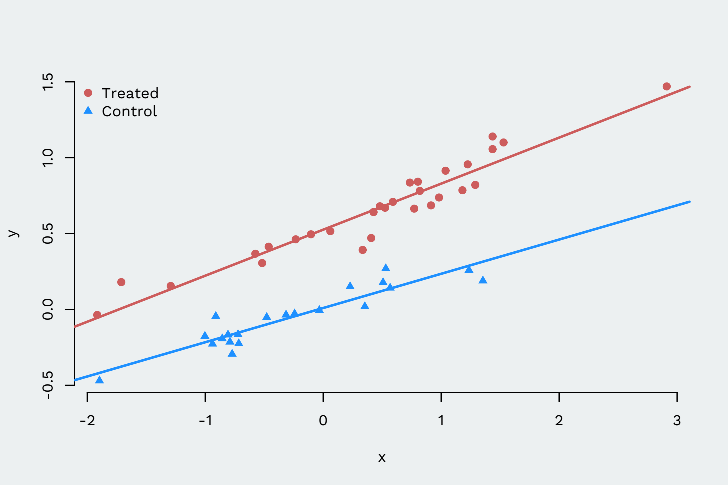

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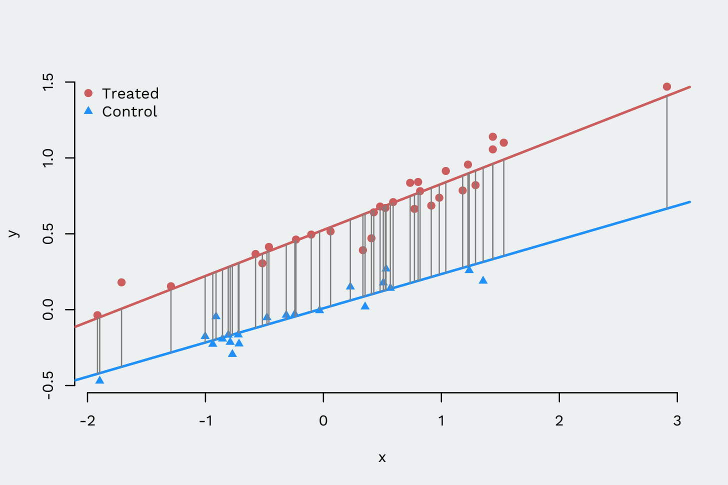
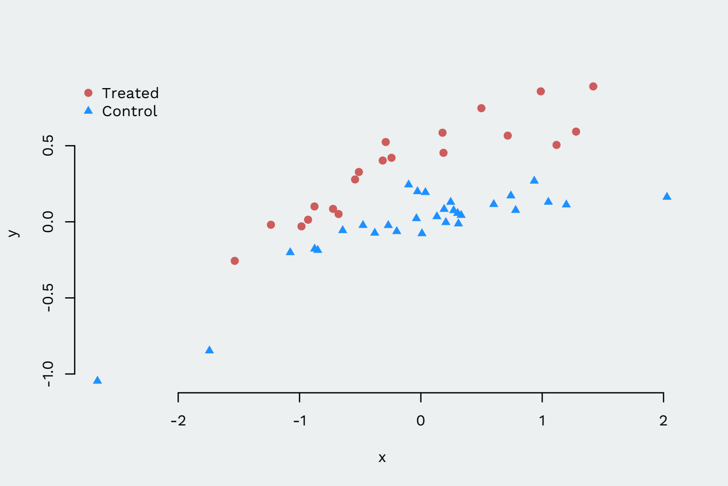


Nonlinear relationships

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



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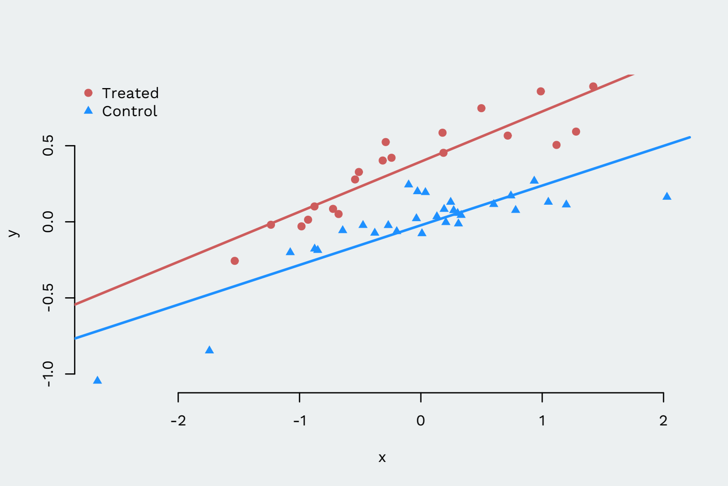

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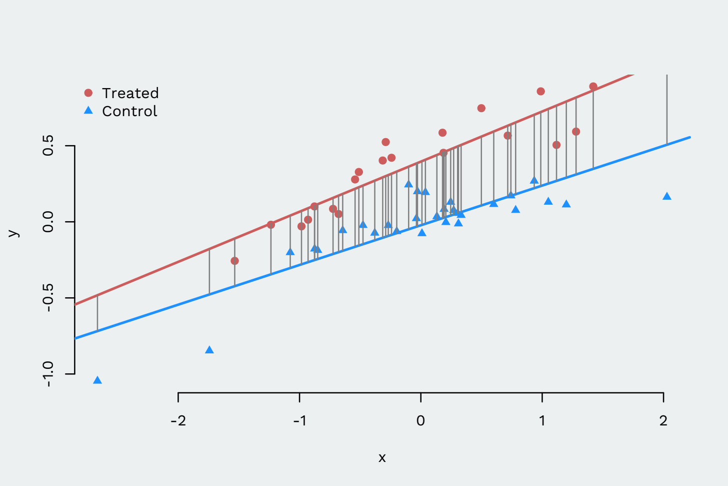

Using semiparametric regression

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

```r
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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library(mgcv)
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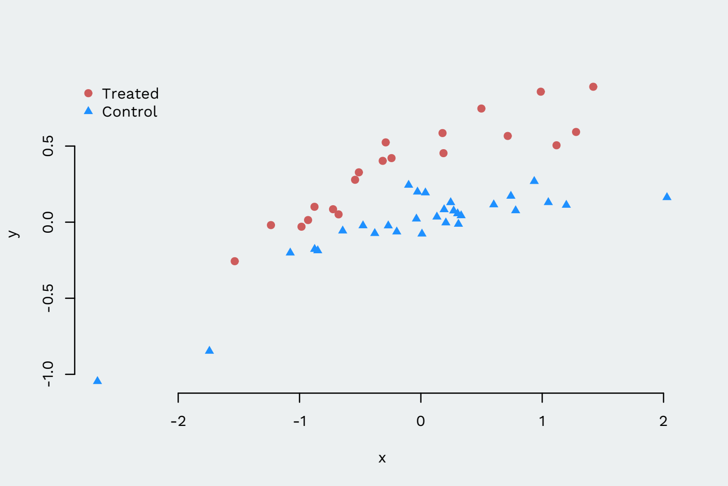

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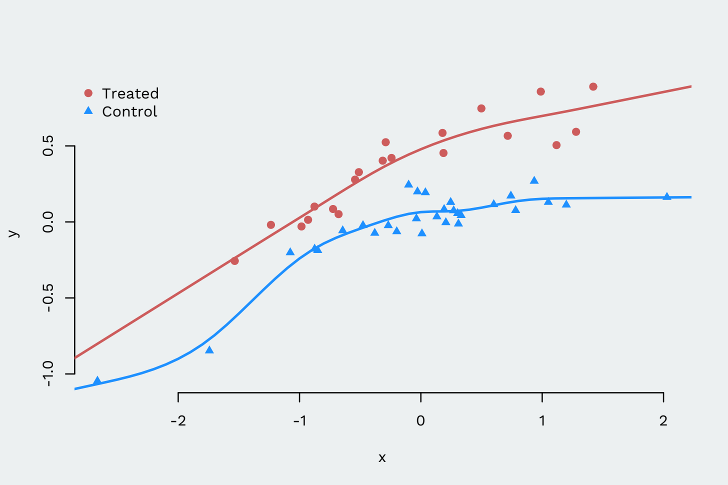

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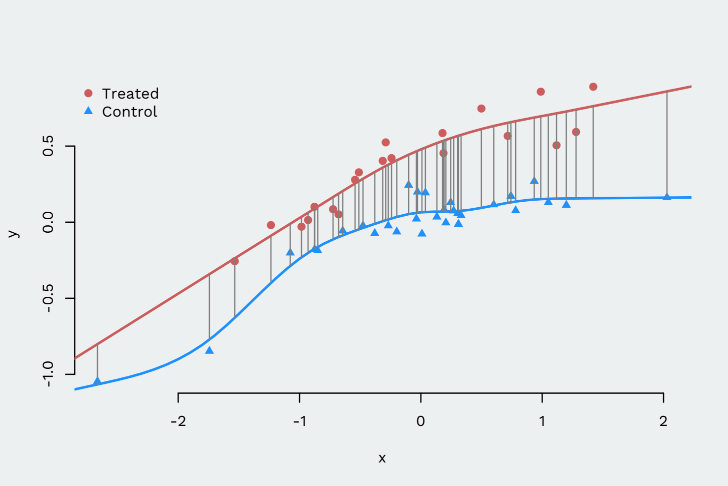

Wait...so what are we actually doing most of the time?

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.

Next Week

- Causality with Unmeasured Confounding
- Reading:
  - Fox Chapter 9.8 Instrumental Variables and TSLS
  - Angrist and Pishke Chapter 4 Instrumental Variables
  - Morgan and Winship Chapter 9 Instrumental Variable Estimators of Causal Effects
  - Optional: Hernan and Robins Chapter 16 Instrumental Variable Estimation
  - Optional: Sovey, Allison J. and Green, Donald P. 2011. "Instrumental Variables Estimation in Political Science: A Readers' Guide." *American Journal of Political Science*

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.

Visualization in the New York Times

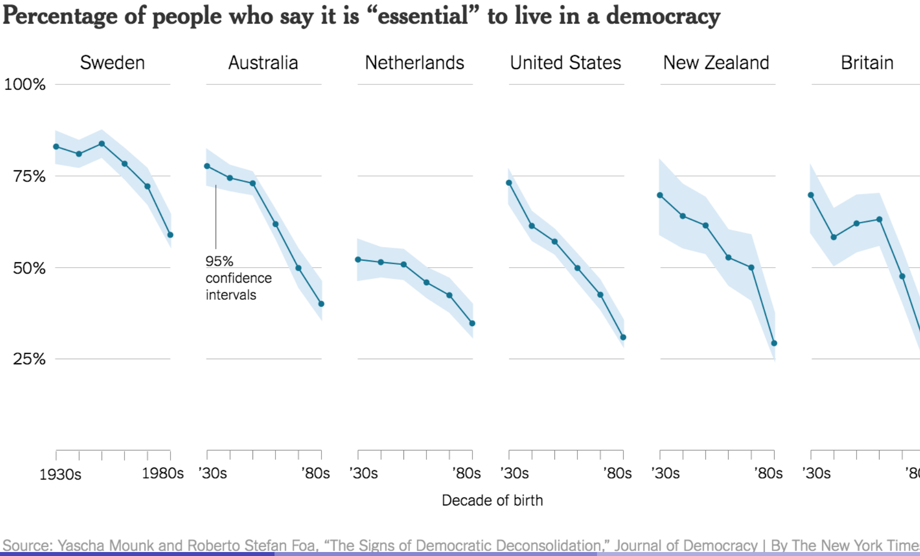
The Danger of Deconsolidation

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.

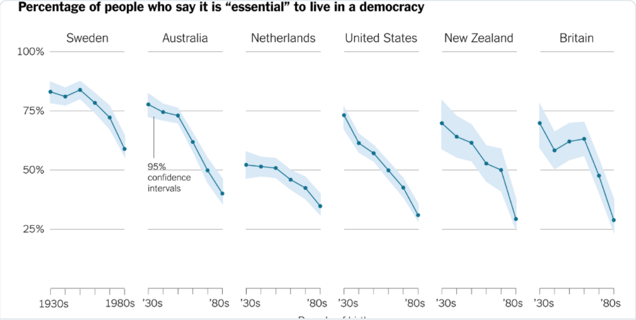
Visualization in the New York Times



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/

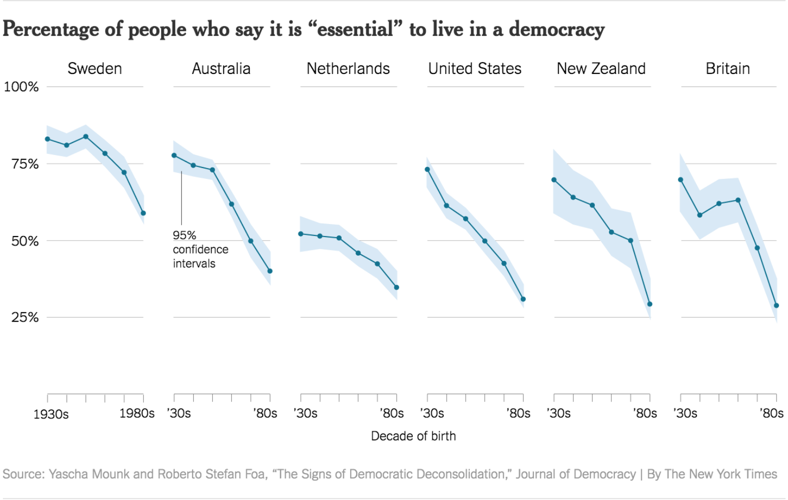


How Stable Are Democracies? 'Warning Signs Are Flashing Red'

New research tries to spot the collapse of liberal democracies before they happen, and it suggests that Western democracy may be seriously ill.

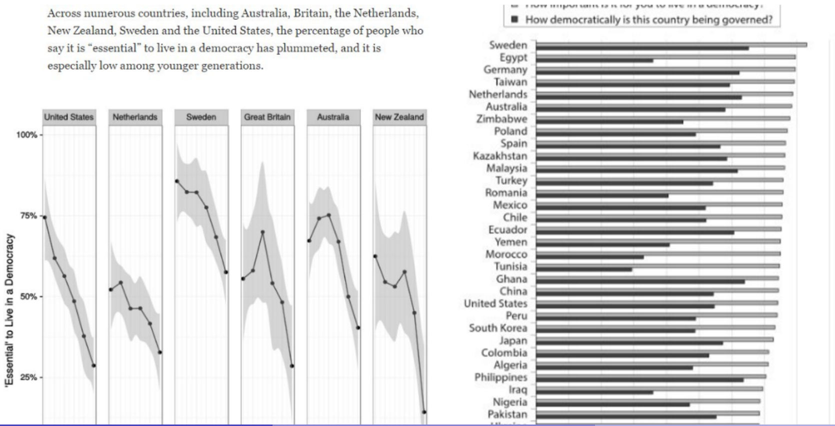
nytimes.com

Alternate Graphs



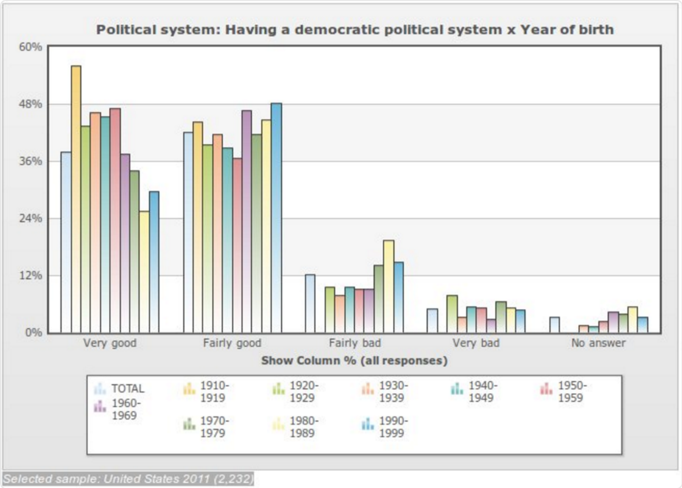
Alternate Graphs

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



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...



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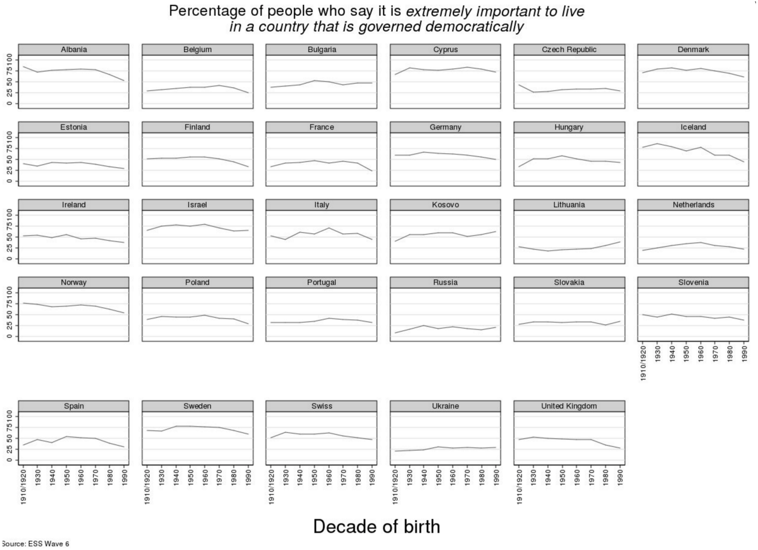

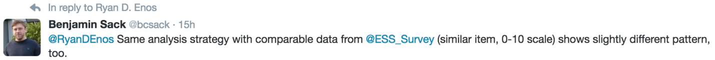

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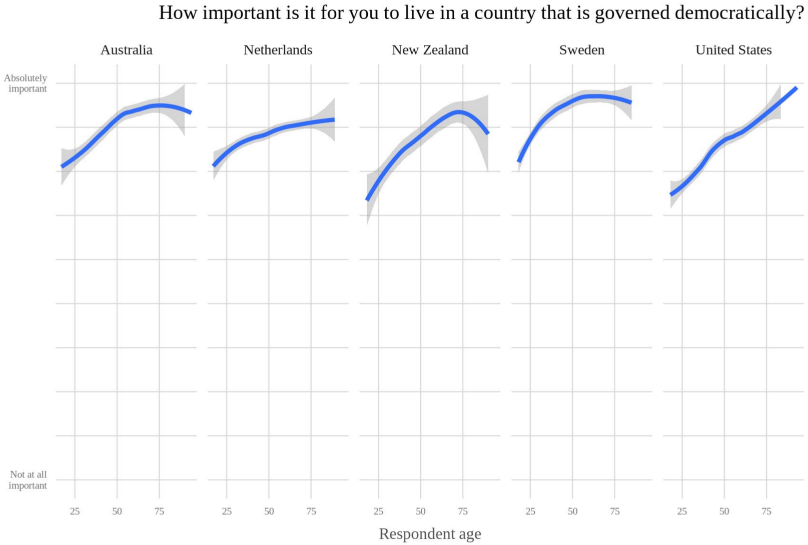
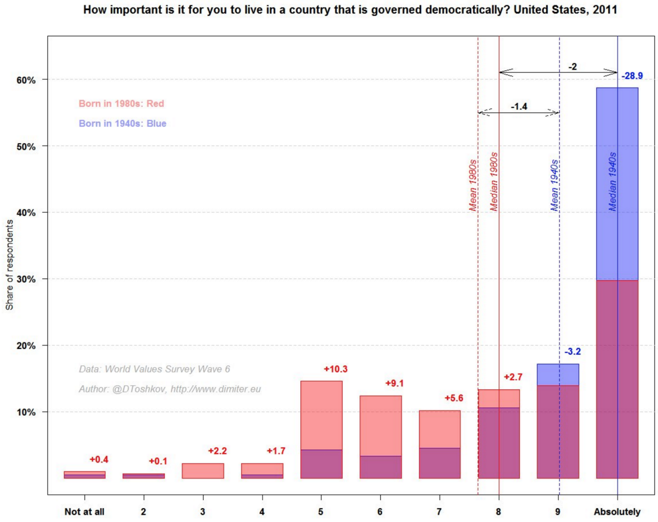


614 Bantam @jpbach · 15h
@RyanDEnos @bshor @nataliemjb @TomWGvdMeer this is a "quick and dirty" plot I did with WVS wave 6. Not quite so terrifying.

Alternate Graphs



Dimiter 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

Thoughts

Two stories here:

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

This Appendix

- The main lecture slides have glossed over some of the details and assumptions for identification
- This appendix contains mathematical results and conditions necessary to estimate causal effects.
- I have also included a section with more details on blocking

Subclassification Estimator

Identification Result

$$\tau_{ATE} = \int \left(\mathbb{E}[Y|X, D=1] - \mathbb{E}[Y|X, D=0] \right) dP(X)$$

$$\tau_{ATT} = \int \left(\mathbb{E}[Y|X, D=1] - \mathbb{E}[Y|X, D=0] \right) dP(X|D=1)$$

Assume X takes on K different cells $\{X^1, ..., X^k, ..., X^K\}$. Then the analogy principle suggests estimators:

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Assume X takes on K different cells $\{X^1, ..., X^k, ..., X^K\}$. Then the analogy principle suggests estimators:

$$\widehat{\tau}_{ATE} = \sum_{k=1}^{K} \left(\bar{Y}_1^k - \bar{Y}_0^k\right) \cdot \left(\frac{N^k}{N}\right); \ \widehat{\tau}_{ATT} = \sum_{k=1}^{K} \left(\bar{Y}_1^k - \bar{Y}_0^k\right) \cdot \left(\frac{N_1^k}{N_1}\right)$$

- N^k is # of obs. and N_1^k is # of treated obs. in cell k
- $\bar{Y}_1^k$ is mean outcome for the treated in cell k
- $\bar{Y}_0^k$ is mean outcome for the untreated in cell k

Subclassification by Age ($K = 2$)

X_k	Death Rate Smokers	Death Rate Non-Smokers	Diff.	# Smokers	# Obs.
Old	28	24	4	3	10
Young	22	16	6	7	10
Total				10	20

What is $\widehat{\tau}_{ATE} = \sum_{k=1}^{K} \left(\bar{Y}_1^k - \bar{Y}_0^k\right) \cdot \left(\frac{N^k}{N}\right)$?

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$\widehat{\tau}_{ATE} = 4 \cdot (10/20) + 6 \cdot (10/20) = 5$

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$\widehat{\tau}_{ATT} = 4 \cdot (3/10) + 6 \cdot (7/10) = 5.4$

Subclassification by Age and Gender ($K = 4$)

X_k	Death Rate Smokers	Death Rate Non-Smokers	Diff.	# Smokers	# Obs.
Old, Male	28	22	4	3	7
Old, Female		24		0	3
Young, Male	21	16	5	3	4
Young, Female	23	17	6	4	6
Total				10	20

What is $\widehat{\tau}_{ATE} = \sum_{k=1}^{K} \left(\bar{Y}_1^k - \bar{Y}_0^k\right) \cdot \left(\frac{N^k}{N}\right)$?

Subclassification by Age and Gender ($K = 4$)

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Total				10	20

What is $\widehat{\tau}_{ATE} = \sum_{k=1}^{K} \left(\bar{Y}_1^k - \bar{Y}_0^k\right) \cdot \left(\frac{N^k}{N}\right)$?

Not identified!

Subclassification by Age and Gender ($K = 4$)

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Total				10	20

What is $\widehat{\tau}_{ATT} = \sum_{k=1}^{K} \left(\bar{Y}_1^k - \bar{Y}_0^k \right) \cdot \left(\frac{N_1^k}{N_1} \right)$?

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Total				10	20

What is $\widehat{\tau}_{ATT} = \sum_{k=1}^{K} \left(\bar{Y}_1^k - \bar{Y}_0^k \right) \cdot \left(\frac{N_1^k}{N_1} \right)$?

$\widehat{\tau}_{ATT} = 4 \cdot (3/10) + 5 \cdot (3/10) + 6 \cdot (4/10) = 5.1$

Selection Bias

Recall the selection problem when comparing the mean outcomes for the treated and the untreated:

Problem

$$\underbrace{\mathbb{E}[Y|D=1] - \mathbb{E}[Y|D=0]}_{\textit{Difference in Means}} = \mathbb{E}[Y_1|D=1] - \mathbb{E}[Y_0|D=0]$$

$$= \underbrace{\mathbb{E}[Y_1 - Y_0|D=1]}_{ATT} + \underbrace{\{\mathbb{E}[Y_0|D=1] - \mathbb{E}[Y_0|D=0]\}}_{BIAS}$$

How can we eliminate the bias term?

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$$= \underbrace{\mathbb{E}[Y_1 - Y_0|D=1]}_{ATT} + \underbrace{\{\mathbb{E}[Y_0|D=1]-\mathbb{E}[Y_0|D=0]\}}_{BIAS}$$

How can we eliminate the bias term?

- As a result of randomization, the selection bias term will be zero
- The treatment and control group will tend to be similar along all characteristics (identical in expectation), including the potential outcomes under the control condition

Identification Under Random Assignment

Identification Assumption

$(Y_1, Y_0) \perp\!\!\!\perp D$ *(random assignment)*

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Problem: $\tau_{ATE} = \mathbb{E}[Y_1 - Y_0]$ *is unobserved. But given random assignment*

$$\begin{aligned} \mathbb{E}[Y|D=1] &= \mathbb{E}[D \cdot Y_1 + (1-D) \cdot Y_0 | D=1] \\ &= \mathbb{E}[Y_1|D=1] \end{aligned}$$

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$$\begin{aligned}
\mathbb{E}[Y|D=1] &= \mathbb{E}[D \cdot Y_1 + (1-D) \cdot Y_0 | D=1] \\
&= \mathbb{E}[Y_1 | D=1] \\
&= \mathbb{E}[Y_1]
\end{aligned}$$

$$\mathbb{E}[Y|D=0] =$$

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$$\begin{aligned} \mathbb{E}[Y|D=0] &= \mathbb{E}[D \cdot Y_1 + (1-D) \cdot Y_0 | D=0] \\ &= \mathbb{E}[Y_0|D=0] \\ &= \mathbb{E}[Y_0] \end{aligned}$$

$$\tau_{ATE} = \mathbb{E}[Y_1 - Y_0] = \mathbb{E}[Y_1] - \mathbb{E}[Y_0] = \underbrace{\mathbb{E}[Y|D=1] - \mathbb{E}[Y|D=0]}_{\textit{Difference in Means}}$$

Average Treatment Effect (ATE)

Imagine a population with 4 units:

i	Y_{1i}	Y_{0i}	Y_i	D_i
1	3	0	3	1
2	1	1	1	1
3	2	0	0	0
4	2	1	1	0

What is $\tau_{ATE} = \mathbb{E}[Y_1] - \mathbb{E}[Y_0]$?

Average Treatment Effect (ATE)

Imagine a population with 4 units:

i	Y_{1i}	Y_{0i}	Y_i	D_i
1	3	0	3	1
2	1	1	1	1
3	2	0	0	0
4	2	1	1	0
$\mathbb{E}[Y_1]$	2			
$\mathbb{E}[Y_0]$		.5		

$$\tau_{ATE} = \mathbb{E}[Y_1] - \mathbb{E}[Y_0] = 2 - .5 = 1.5$$

Average Treatment Effect (ATE)

Imagine a population with 4 units:

i	Y_{1i}	Y_{0i}	Y_i	D_i
1	3	?	3	1
2	1	?	1	1
3	?	0	0	0
4	?	1	1	0
$\mathbb{E}[Y_1]$	?			
$\mathbb{E}[Y_0]$		?		

What is $\tau_{ATE} = \mathbb{E}[Y_1] - \mathbb{E}[Y_0]$?

Average Treatment Effect (ATE)

Imagine a population with 4 units:

i	Y_{1i}	Y_{0i}	Y_i	D_i	$P(D_i = 1)$
1	3	?	3	1	?
2	1	?	1	1	?
3	?	0	0	0	?
4	?	1	1	0	?
$\mathbb{E}[Y_1]$	?				
$\mathbb{E}[Y_0]$		?			

What is $\tau_{ATE} = \mathbb{E}[Y_1] - \mathbb{E}[Y_0]$? In an experiment, the researcher controls the probability of assignment to treatment for all units $P(D_i = 1)$ and by imposing equal probabilities we ensure that treatment assignment is independent of the potential outcomes, i.e. $(Y_1, Y_0) \perp\!\!\!\perp D$.

Average Treatment Effect (ATE)

Imagine a population with 4 units:

i	Y_{1i}	Y_{0i}	Y_i	D_i	$P(D_i = 1)$
1	3	0	3	1	2/4
2	1	1	1	1	2/4
3	2	0	0	0	2/4
4	2	1	1	0	2/4
$\mathbb{E}[Y_1]$	2				
$\mathbb{E}[Y_0]$		.5			

What is $\tau_{ATE} = \mathbb{E}[Y_1] - \mathbb{E}[Y_0]$? Given that D_i is randomly assigned with probability 1/2, we have $\mathbb{E}[Y|D = 1] = \mathbb{E}[Y_1|D = 1] = \mathbb{E}[Y_1]$.

All possible randomizations with two treated units:

Treated Units:	1 & 2	1 & 3	1 & 4	2 & 3	2 & 4	3 & 4
Average $Y|D = 1$:	2	2.5	2.5	1.5	1.5	2

So $\mathbb{E}[Y|D = 1] = \mathbb{E}[Y_1] = 2$

Average Treatment Effect (ATE)

Imagine a population with 4 units:

i	Y_{1i}	Y_{0i}	Y_i	D_i	$P(D_i = 1)$
1	3	0	3	1	$2/4$
2	1	1	1	1	$2/4$
3	2	0	0	0	$2/4$
4	2	1	1	0	$2/4$
$\mathbb{E}[Y_1]$	2				
$\mathbb{E}[Y_0]$		.5			

By the same logic, we have: $\mathbb{E}[Y|D=0] = \mathbb{E}[Y_0|D=0] = \mathbb{E}[Y_0] = .5$.

Therefore the average treatment effect is identified:

$$\tau_{ATE} = \mathbb{E}[Y_1] - \mathbb{E}[Y_0] = \underbrace{\mathbb{E}[Y|D=1] - \mathbb{E}[Y|D=0]}_{\text{Difference in Means}}$$

Average Treatment Effect (ATE)

Imagine a population with 4 units:

i	Y_{1i}	Y_{0i}	Y_i	D_i	$P(D_i = 1)$
1	3	0	3	1	$2/4$
2	1	1	1	1	$2/4$
3	2	0	0	0	$2/4$
4	2	1	1	0	$2/4$
$\mathbb{E}[Y_1]$	2				
$\mathbb{E}[Y_0]$		.5			

Also since $\mathbb{E}[Y|D=0] = \mathbb{E}[Y_0|D=0] = \mathbb{E}[Y_0|D=1] = \mathbb{E}[Y_0]$
we have that

$$\begin{aligned} \tau_{ATT} &= \mathbb{E}[Y_1 - Y_0|D=1] = \mathbb{E}[Y_1|D=1] - \mathbb{E}[Y_0|D=0] \\ &= \mathbb{E}[Y_1] - \mathbb{E}[Y_0] = \mathbb{E}[Y_1 - Y_0] \\ &= \tau_{ATE} \end{aligned}$$

Identification under Random Assignment

Identification Assumption

$(Y_1, Y_0) \perp\!\!\!\perp D$ *(random assignment)*

Identification Result

We have that

$$\mathbb{E}[Y_0|D=0] = \mathbb{E}[Y_0] = \mathbb{E}[Y_0|D=1]$$

and therefore

$$\begin{aligned}\underbrace{E[Y|D=1] - \mathbb{E}[Y|D=0]}_{\text{Difference in Means}} &= \underbrace{E[Y_1 - Y_0|D=1]}_{ATET} + \underbrace{\{\mathbb{E}[Y_0|D=1] - \mathbb{E}[Y_0|D=0]\}}_{BIAS} \\ &= \underbrace{E[Y_1 - Y_0|D=1]}_{ATET}\end{aligned}$$

As a result,

$$\underbrace{E[Y|D=1] - \mathbb{E}[Y|D=0]}_{\text{Difference in Means}} = \tau_{ATE} = \tau_{ATET}$$

Identification in Randomized Experiments

Identification Assumption

Given random assignment $(Y_1, Y_0) \perp\!\!\!\perp D$

Identification Result

Let $F_{Y_d}(y)$ *be the cumulative distribution function (CDF) of* Y_d*, then*

$$\begin{aligned} F_{Y_0}(y) &= \Pr(Y_0 \leq y) = \Pr(Y_0 \leq y | D = 0) \\ &= \Pr(Y \leq y | D = 0). \end{aligned}$$

Similarly,

$$F_{Y_1}(y) = \Pr(Y \leq y | D = 1).$$

So the effect of the treatment at any quantile $\theta \in [0, 1]$ *is identified:*

$$\alpha_\theta = Q_\theta(Y_1) - Q_\theta(Y_0) = Q_\theta(Y | D = 1) - Q_\theta(Y | D = 0)$$

where $F_{Y_d}(Q_\theta(Y_d)) = \theta$.

Estimation Under Random Assignment

Consider a randomized trial with N individuals.

Estimand

$\tau_{ATE} = \mathbb{E}[Y_1 - Y_0] = \mathbb{E}[Y|D=1] - \mathbb{E}[Y|D=0]$

Estimator

Estimation Under Random Assignment

Consider a randomized trial with N individuals.

Estimand

$\tau_{ATE} = \mathbb{E}[Y_1 - Y_0] = \mathbb{E}[Y|D=1] - \mathbb{E}[Y|D=0]$

Estimator

By the analogy principle we use

$$\widehat{\tau} = \bar{Y}_1 - \bar{Y}_0$$

$$\bar{Y}_1 = \frac{\sum Y_i \cdot D_i}{\sum D_i} = \frac{1}{N_1} \sum_{D_i=1} Y_i\,;$$

$$\bar{Y}_0 = \frac{\sum Y_i \cdot (1 - D_i)}{\sum (1 - D_i)} = \frac{1}{N_0} \sum_{D_i=0} Y_i$$

with $N_1 = \sum_i D_i$ *and* $N_0 = N - N_1$.

Under random assignment, $\widehat{\tau}$ *is an unbiased and consistent estimator of* τ_{ATE} *(*$\mathbb{E}[\widehat{\tau}] = \tau_{ATE}$ *and* $\widehat{\tau}_N \xrightarrow{p} \tau_{ATE}$*.)*

Unbiasedness Under Random Assignment

One way of showing that $\widehat{\tau}$ is unbiased is to exploit the fact that under independence of potential outcomes and treatment status, $\mathbb{E}[D] = \frac{N_1}{N}$ and $\mathbb{E}[1 - D] = \frac{N_0}{N}$

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Rewrite the estimators as follows:

$$\widehat{\tau} = \frac{1}{N} \sum_{i=1}^{N} \left(\frac{D \cdot Y_1}{N_1/N} - \frac{(1 - D) \cdot Y_0}{N_0/N} \right)$$

Take expectations with respect to the sampling distribution given by the design. Under the Neyman model, Y_1 and Y_0 are fixed and only D_i is random.

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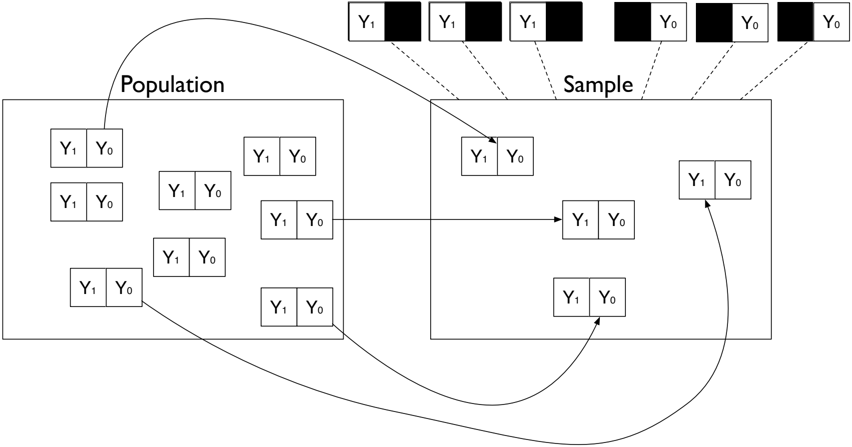
Take expectations with respect to the sampling distribution given by the design. Under the Neyman model, Y_1 and Y_0 are fixed and only D_i is random.

$$\mathbb{E}[\widehat{\tau}] = \frac{1}{N}\sum_{i=1}^{N}\left(\frac{\mathbb{E}[D]\cdot Y_1}{N_1/N} - \frac{\mathbb{E}[(1-D)]\cdot Y_0}{N_0/N}\right) = \frac{1}{N}\sum_{i=1}^{N}(Y_1 - Y_0) = \tau$$

What is the Estimand?

- So far we have emphasized effect estimation, but what about uncertainty?
- In the design based literature, variability in our estimates can arise from two sources:
    1. Sampling variation induced by the procedure that selected the units into our sample.
    2. Variation induced by the particular realization of the treatment variable.
- This distinction is important, but often ignored

What is the Estimand?



SATE and PATE

- Typically we focus on estimating the average causal effect in a particular sample: **S**ample **A**verage **T**reatment **E**ffect (SATE)
  - Uncertainty arises only from hypothetical randomizations.
  - Inferences are limited to the sample in our study.
- Might care about the **P**opulation **A**verage **T**reatment **E**ffect (PATE)
  - Requires precise knowledge about the sampling process that selected units from the population into the sample.
  - Need to account for two sources of variation:
    - ⋆ Variation from the sampling process
    - ⋆ Variation from treatment assignment.
- Thus, in general, $\text{Var}(\widehat{\text{PATE}}) > \text{Var}(\widehat{\text{SATE}})$.

Standard Error for Sample ATE

The standard error is the standard deviation of a sampling distribution:
$SE_{\widehat{\theta}} \equiv \sqrt{\frac{1}{J}\sum_1^J(\widehat{\theta}_j - \overline{\widehat{\theta}})^2}$ (with J possible random assignments).

i	Y_{1i}	Y_{0i}	Y_i	D_i	$P(D_i = 1)$
1	3	0	3	1	2/4
2	1	1	1	1	2/4
3	2	0	0	0	2/4
4	2	1	1	0	2/4

ATE estimates given all possible random assignments with two treated units:

Treated Units:	1 & 2	1 & 3	1 & 4	2 & 3	2 & 4	3 & 4
$\widehat{ATE}$:	1.5	1.5	2	1	1.5	1.5

The average $\widehat{ATE}$ is 1.5 and therefore the true standard error is
$SE_{\widehat{ATE}} = \sqrt{\frac{1}{6}[(1.5-1.5)^2 + (1.5-1.5)^2 + (2-1.5)^2 + (1-1.5)^2 + (1.5-1.5)^2 + (1.5-1.5)^2]} \approx .28$

Standard Error for Sample ATE

Standard Error for Sample ATE

Given complete randomization of N units with N_1 assigned to treatment and $N_0 = N - N_1$ to control, the true standard error of the estimated sample ATE is given by

$$SE_{\widehat{ATE}} = \sqrt{\left(\frac{N-N_1}{N-1}\right)\frac{Var[Y_{1i}]}{N_1} + \left(\frac{N-N_0}{N-1}\right)\frac{Var[Y_{0i}]}{N_0} + \left(\frac{1}{N-1}\right)2Cov[Y_{1i}, Y_{0i}]}$$

with population variances and covariance

$$Var[Y_{di}] \equiv \frac{1}{N}\sum_{1}^{N}\left(Y_{di} - \frac{\sum_1^N Y_{di}}{N}\right)^2 = \sigma^2_{Y_d|D_i=d}$$

$$Cov[Y_{1i}, Y_{0i}] \equiv \frac{1}{N}\sum_{1}^{N}\left(Y_{1i} - \frac{\sum_1^N Y_{1i}}{N}\right)\left(Y_{0i} - \frac{\sum_1^N Y_{0i}}{N}\right) = \sigma^2_{Y_1,Y_0}$$

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Plugging in, we obtain the true standard error of the estimated sample ATE

$$SE_{\widehat{ATE}} = \sqrt{\left(\frac{4-2}{4-1}\right)\frac{.25}{2} + \left(\frac{4-2}{4-1}\right)\frac{.5}{2} + \left(\frac{1}{4-1}\right)2(-.25)} \approx .28$$

Standard Error for Sample ATE

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Given complete randomization of N units with N_1 assigned to treatment and $N_0 = N - N_1$ to control, the true standard error of the estimated sample ATE is given by

$$SE_{\widehat{ATE}} = \sqrt{\left(\frac{N-N_1}{N-1}\right)\frac{Var[Y_{1i}]}{N_1} + \left(\frac{N-N_0}{N-1}\right)\frac{Var[Y_{0i}]}{N_0} + \left(\frac{1}{N-1}\right)2Cov[Y_{1i}, Y_{0i}]}$$

with population variances and covariance

$$Var[Y_{di}] \equiv \frac{1}{N}\sum_1^N \left(Y_{di} - \frac{\sum_1^N Y_{di}}{N}\right)^2 = \sigma^2_{Y_d|D_i=d}$$

$$Cov[Y_{1i}, Y_{0i}] \equiv \frac{1}{N}\sum_1^N \left(Y_{1i} - \frac{\sum_1^N Y_{1i}}{N}\right)\left(Y_{0i} - \frac{\sum_1^N Y_{0i}}{N}\right) = \sigma^2_{Y_1,Y_0}$$

Standard error decreases if:

- N grows
- $Var[Y_1]$, $Var[Y_0]$ decrease
- $Cov[Y_1, Y_0]$ decreases

Conservative Estimator $\hat{SE}_{\widehat{ATE}}$

Conservative Estimator for Standard Error for Sample ATE

$$\widehat{SE}_{\widehat{ATE}} = \sqrt{\frac{\widehat{Var[Y_{1i}]}}{N_1} + \frac{\widehat{Var[Y_{0i}]}}{N_0}}$$

with estimators of the sample variances given by

$$\widehat{Var[Y_{1i}]} \equiv \frac{1}{N_1 - 1} \sum_{i|D_i=1}^{N} \left(Y_{1i} - \frac{\sum_{i|D_i=1}^{N} Y_{1i}}{N_1} \right)^2 = \widehat{\sigma}^2_{Y|D_i=1}$$

$$\widehat{Var[Y_{0i}]} \equiv \frac{1}{N_0 - 1} \sum_{i|D_i=0}^{N} \left(Y_{0i} - \frac{\sum_{i|D_i=0}^{N} Y_{0i}}{N_0} \right)^2 = \widehat{\sigma}^2_{Y|D_i=0}$$

Conservative Estimator $\hat{SE}_{\widehat{ATE}}$

Conservative Estimator for Standard Error for Sample ATE

$$\widehat{SE}_{\widehat{ATE}} = \sqrt{\frac{\widehat{Var[Y_{1i}]}}{N_1} + \frac{\widehat{Var[Y_{0i}]}}{N_0}}$$

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What about the covariance?

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- Conservative compared to the true standard error, i.e. $SE_{\widehat{ATE}} < \widehat{SE}_{\widehat{ATE}}$
- Asymptotically unbiased in two special cases:
- if τ_i is constant (i.e. $Cor[Y_1, Y_0] = 1$)
- if we estimate standard error of population average treatment effect ($Cov[Y_1, Y_0]$ is negligible when we sample from a large population)
- Equivalent to standard error for two sample t-test with unequal variances or "robust" standard error in regression of Y on D

Proof: $SE_{\widehat{ATE}} \leq \hat{SE}_{\widehat{ATE}}$

Upper bound for standard error is when $Cor[Y_1, Y_0] = 1$:

$$Cor[Y_1, Y_0] = \frac{Cov[Y_1, Y_0]}{\sqrt{Var[Y_1]Var[Y_0]}} \leq 1 \iff Cov[Y_1, Y_0] \leq \sqrt{Var[Y_1]Var[Y_0]}$$

$$\begin{aligned}
SE_{\widehat{ATE}} &= \sqrt{\left(\frac{N-N_1}{N-1}\right)\frac{Var[Y_1]}{N_1} + \left(\frac{N-N_0}{N-1}\right)\frac{Var[Y_0]}{N_0} + \left(\frac{1}{N-1}\right)2Cov[Y_1, Y_0]} \\
&= \sqrt{\frac{1}{N-1}\left(\frac{N_0}{N_1}Var[Y_1] + \frac{N_1}{N_0}Var[Y_0] + 2Cov[Y_1, Y_0]\right)} \\
&\leq \sqrt{\frac{1}{N-1}\left(\frac{N_0}{N_1}Var[Y_1] + \frac{N_1}{N_0}Var[Y_0] + 2\sqrt{Var[Y_1]Var[Y_0]}\right)} \\
&\leq \sqrt{\frac{1}{N-1}\left(\frac{N_0}{N_1}Var[Y_1] + \frac{N_1}{N_0}Var[Y_0] + Var[Y_1] + Var[Y_0]\right)}
\end{aligned}$$

Last step follows from the following inequality

$$\begin{aligned}
(\sqrt{Var[Y_1]} - \sqrt{Var[Y_0]})^2 &\geq 0 \\
Var[Y_1] - 2\sqrt{Var[Y_1]Var[Y_0]} + Var[Y_0] &\geq 0 \iff Var[Y_1] + Var[Y_0] \geq 2\sqrt{Var[Y_1]Var[Y_0]}
\end{aligned}$$

Proof: $SE_{\widehat{ATE}} \leq \hat{SE}_{\widehat{ATE}}$

$$
\begin{aligned}
SE_{\widehat{ATE}} &\leq \sqrt{\frac{1}{N-1}\left(\frac{N_0}{N_1}Var[Y_1] + \frac{N_1}{N_0}Var[Y_0] + Var[Y_1] + Var[Y_0]\right)} \\
&\leq \sqrt{\frac{N_0^2 Var[Y_1] + N_1^2 Var[Y_0] + N_1 N_0 (Var[Y_1] + Var[Y_0])}{(N-1)N_1 N_0}} \\
&\leq \sqrt{\frac{(N_0^2 + N_1 N_0) Var[Y_1] + (N_1^2 + N_1 N_0) Var[Y_0]}{(N-1)N_1 N_0}} \\
&\leq \sqrt{\frac{(N_0 + N_1) N_0 Var[Y_1]}{(N-1)N_1 N_0} + \frac{(N_1 + N_0) N_1 Var[Y_0]}{(N-1)N_1 N_0}} \\
&\leq \sqrt{\frac{N\, Var[Y_1]}{(N-1)N_1} + \frac{N\, Var[Y_0]}{(N-1)N_0}} \\
&\leq \sqrt{\frac{N}{N-1}\left(\frac{Var[Y_1]}{N_1} + \frac{Var[Y_0]}{(N_0)}\right)} \\
&\leq \sqrt{\frac{N}{N-1}\left(\frac{\widehat{Var[Y_1]}}{N_1} + \frac{\widehat{Var[Y_0]}}{(N_0)}\right)}
\end{aligned}
$$

So the estimator for the standard error is conservative.

Standard Error for Sample ATE

i	Y_{1i}	Y_{0i}	Y_i
1	3	0	3
2	1	1	1
3	2	0	0
4	2	1	1

$\widehat{SE}_{\widehat{ATE}}$ estimates given all possible assignments with two treated units:

Treated Units:	1 & 2	1 & 3	1 & 4	2 & 3	2 & 4	3 & 4
$\widehat{ATE}$:	1.5	1.5	2	1	1.5	1.5
$\widehat{SE}_{\widehat{ATE}}$:	1.11	.5	.71	.71	.5	.5

The average $\widehat{SE}_{\widehat{ATE}}$ is $\approx .67$ compared to the true standard error of $SE_{\widehat{ATE}} \approx .28$

Example: Effect of Training on Earnings

- Treatment Group:
  - $N_1 = 7,487$
  - Estimated Average Earnings $\bar{Y}_1$: \$16,199
  - Estimated Sample Standard deviation $\widehat{\sigma}_{Y|D_i=1}$: \$17,038
- Control Group :
  - $N_0 = 3,717$
  - Estimated Average Earnings $\bar{Y}_0$: \$15,040
  - Estimated Sample deviation $\widehat{\sigma}_{Y|D_i=0}$: \$16,180
- Estimated average effect of training:

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- Estimated average effect of training:
  - $\widehat{\tau}_{ATE} = \bar{Y}_1 - \bar{Y}_0 = 16,199 - 15,040 = \$1,159$
- Estimated standard error for effect of training:
  - $\widehat{SE}_{\widehat{ATE}} = \sqrt{\frac{\widehat{\sigma}^2_{Y|D_i=1}}{N_1} + \frac{\widehat{\sigma}^2_{Y|D_i=0}}{(N_0)}} = \sqrt{\frac{17{,}038^2}{7{,}487} + \frac{16{,}180^2}{3{,}717}} \approx \$330$
- Is this consistent with a zero average treatment effect $\alpha_{ATE} = 0$?

Testing the Null Hypothesis of Zero Average Effect

- Under the null hypothesis H_0: $\tau_{ATE} = 0$, the average potential outcomes in the population are the same for treatment and control: $\mathbb{E}[Y_1] = \mathbb{E}[Y_0]$.
- Since units are randomly assigned, both the treatment and control groups should therefore have the same sample average earnings
- However, we in fact observe a difference in mean earnings of $\$1,159$
- What is the probability of observing a difference this large if the true average effect of the training were zero (i.e. the null hypothesis were true)?

Testing the Null Hypothesis of Zero Average Effect

- Use a two-sample t-test with unequal variances:

$$t = \frac{\widehat{\tau}}{\sqrt{\frac{\widehat{\sigma}^2_{Y_i|D_i=1}}{N_1} + \frac{\widehat{\sigma}^2_{Y_i|D_i=0}}{N_0}}} = \frac{\$1,159}{\sqrt{\frac{\$17,038^2}{7,487} + \frac{\$16,180^2}{3,717}}} \approx 3.5$$

  - From basic statistical theory, we know that $t_N \xrightarrow{d} \mathcal{N}(0,1)$
  - And for a standard normal distribution, the probability of observing a value of t that is larger than $|t| > 1.96$ is $< .05$
  - So obtaining a value as high as $t = 3.5$ is very unlikely under the null hypothesis of a zero average effect
  - We reject the null hypothesis H_0: $\tau_0 = 0$ against the alternative H_1: $\tau_0 \neq 0$ at asymptotic 5% significance level whenever $|t| > 1.96$.
  - Inverting the test statistic we can construct a 95% confidence interval

$$\widehat{\tau}_{ATE} \pm 1.96 \cdot \widehat{SE}_{\widehat{ATE}}$$

Testing the Null Hypothesis of Zero Average Effect

R Code

```
> d <- read.dta("jtpa.dta")
> head(d[,c("earnings","assignmt")])
  earnings assignmt
1     1353        1
2     4984        1
3    27707        1
4    31860        1
5    26615        0
>
> meanAsd <- function(x){
+   out <- c(mean(x),sd(x))
+   names(out) <- c("mean","sd")
+   return(out)
+ }
>
> aggregate(earnings~assignmt,data=d,meanAsd)
  assignmt earnings.mean earnings.sd
1        0      15040.50    16180.25
2        1      16199.94    17038.85
```

Testing the Null Hypothesis of Zero Average Effect

R Code

```
> t.test(earnings~assignmt,data=d,var.equal=FALSE)

        Welch Two Sample t-test

data:  earnings by assignmt
t = -3.5084, df = 7765.599, p-value = 0.0004533
alternative hypothesis: true difference in means is not equal to 0
95 percent confidence interval:
 -1807.2427  -511.6239
sample estimates:
mean in group 0 mean in group 1
       15040.50        16199.94
```

Regression to Estimate the Average Treatment Effect

Estimator (Regression)

The ATE can be expressed as a regression equation:

$$
\begin{aligned}
Y_i &= D_i\, Y_{1i} + (1 - D_i)\, Y_{0i} \\
&= Y_{0i} + (Y_{1i} - Y_{0i})\, D_i \\
&= \underbrace{\bar{Y}_0}_{\alpha} + \underbrace{(\bar{Y}_1 - \bar{Y}_0)}_{\tau_{Reg}} D_i + \underbrace{\{(Y_{i0} - \bar{Y}_0) + D_i \cdot [(Y_{i1} - \bar{Y}_1) - (Y_{i0} - \bar{Y}_0)]\}}_{\epsilon} \\
&= \alpha + \tau_{Reg} D_i + \epsilon_i
\end{aligned}
$$

- τ_{Reg} could be biased for τ_{ATE} in two ways:

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- τ_{Reg} could be biased for τ_{ATE} in two ways:
  - Baseline difference in potential outcomes under control that is correlated with D_i.
  - Individual treatment effects τ_i are correlated with D_i
  - Under random assignment, both correlations are zero in expectation

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  - Individual treatment effects τ_i are correlated with D_i
  - Under random assignment, both correlations are zero in expectation
- Effect heterogeneity implies "heteroskedasticity", i.e. error variance differs by values of D_i.
  - Neyman model imples "robust" standard errors.
- Can use regression in experiments without assuming constant effects.

Regression to Estimate the Average Treatment Effect

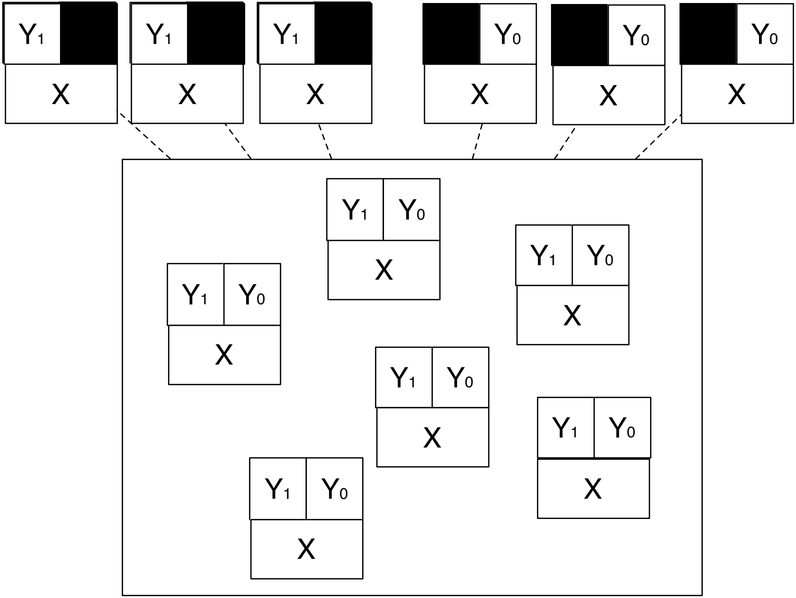
R Code

```
> library(sandwich)
> library(lmtest)
>
> lout <- lm(earnings~assignmt,data=d)
> coeftest(lout,vcov = vcovHC(lout, type = "HC1")) # matches Stata

t test of coefficients:

            Estimate Std. Error t value  Pr(>|t|)
(Intercept) 15040.50     265.38 56.6752 < 2.2e-16 ***
assignmt     1159.43     330.46  3.5085 0.0004524 ***
---
```

Covariates and Experiments



Covariates

- Randomization is gold standard for causal inference because in expectation it balances observed but also unobserved characteristics between treatment and control group.
- Unlike potential outcomes, you observe baseline covariates for all units. Covariate values are predetermined with respect to the treatment and do not depend on D_i.
- Under randomization, $f_{X|D}(X|D=1) \stackrel{d}{=} f_{X|D}(X|D=0)$ (equality in distribution).
- Similarity in distributions of covariates is known as covariate balance.
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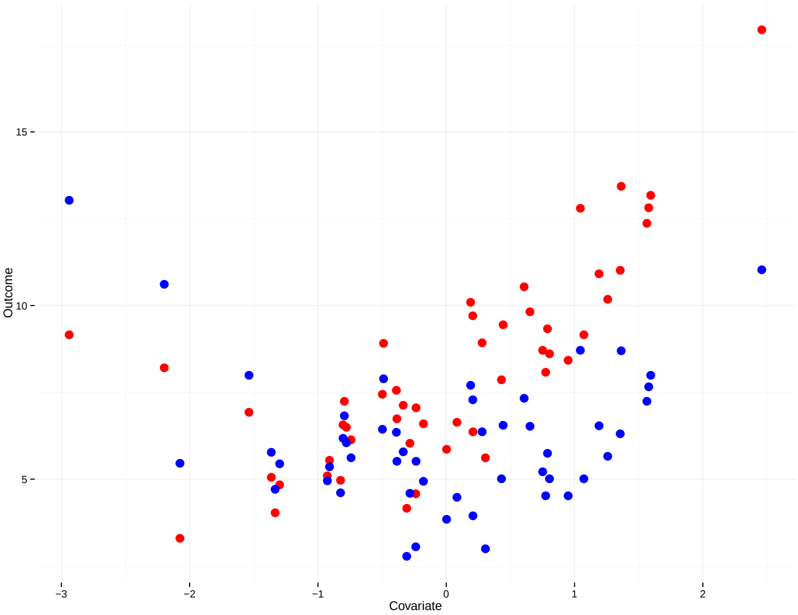
- If this is not the case, then one of two possibilities:
  - Randomization was compromised.
  - Sampling error (bad luck)
- One should always test for covariate balance on important covariates, using so called "balance checks" (eg. t-tests, F-tests, etc.)

Covariates and Experiments



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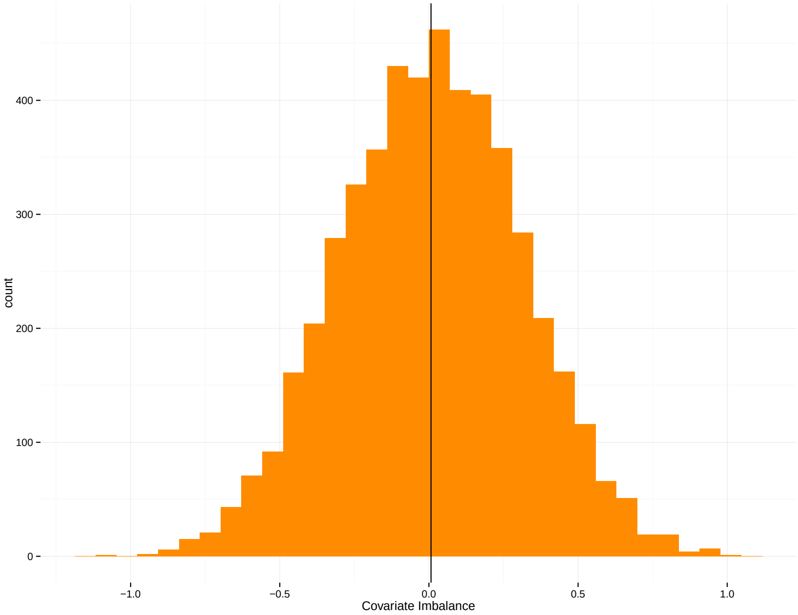

Regression with Covariates

- Practioners often run some variant of the following model with experimental data:

$$Y_i = \alpha + \tau D_i + X_i\beta + \epsilon_i$$

- Why include X_i when experiments "control" for covariates by design?

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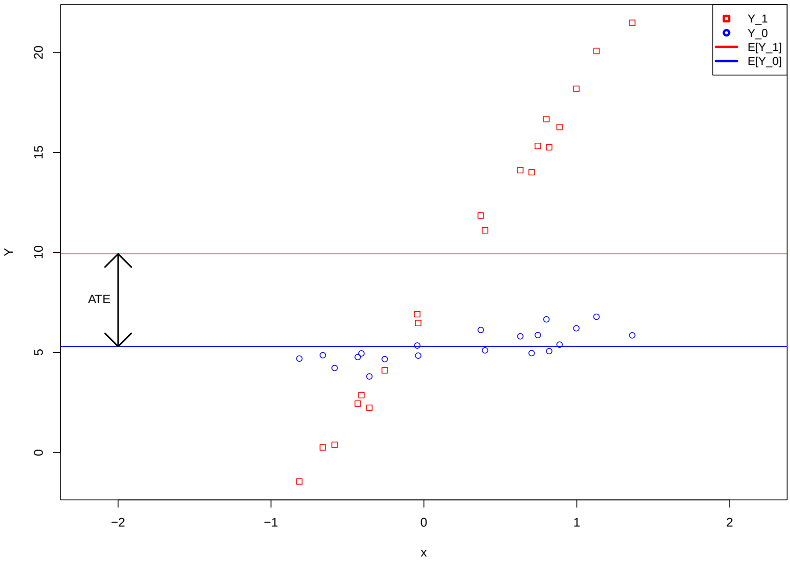
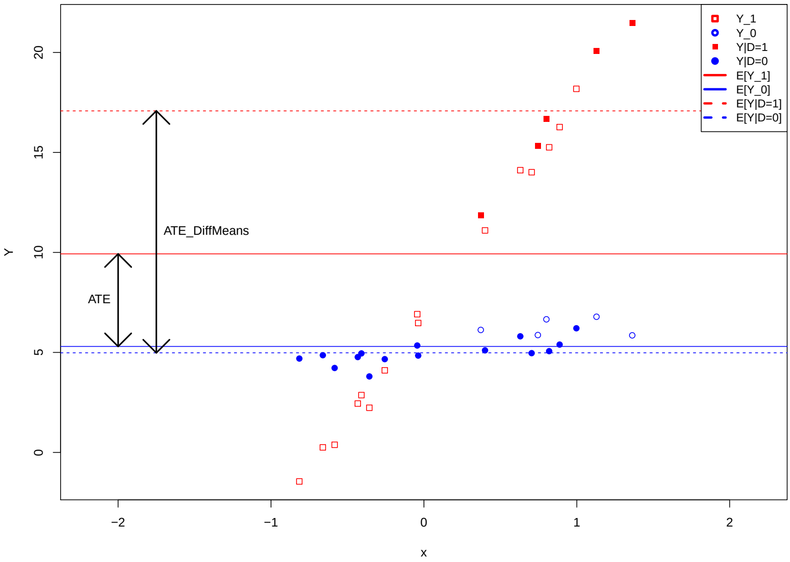
  - Correct for chance covariate imbalances that indicate that $\hat{\tau}$ may be far from τ_{ATE}.
  - Increase precision: remove variation in the outcome accounted for by pre-treatment characteristics, thus making it easier to attribute remaining differences to the treatment.
- ATE estimates are robust to model specification (with sufficient N).
  - Never control for **post-treatment** covariates!

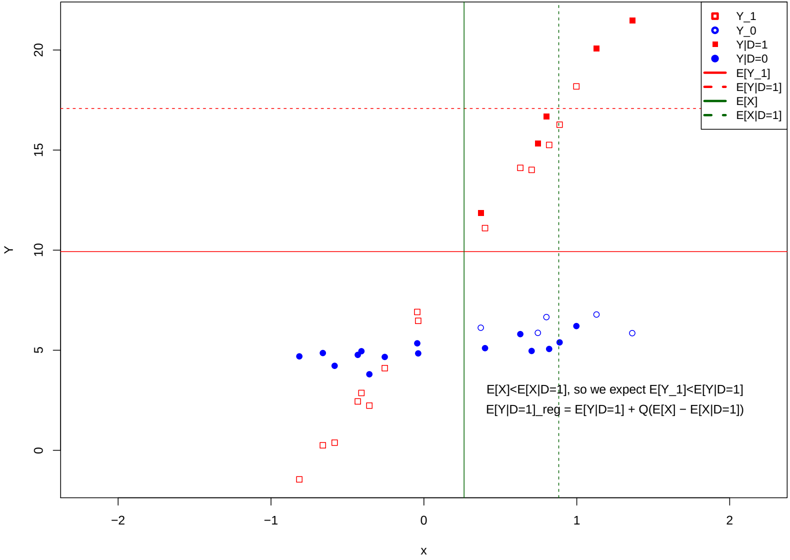
True ATE



True ATE and Unadjusted Regression Estimator



Adjusted Regression Estimator



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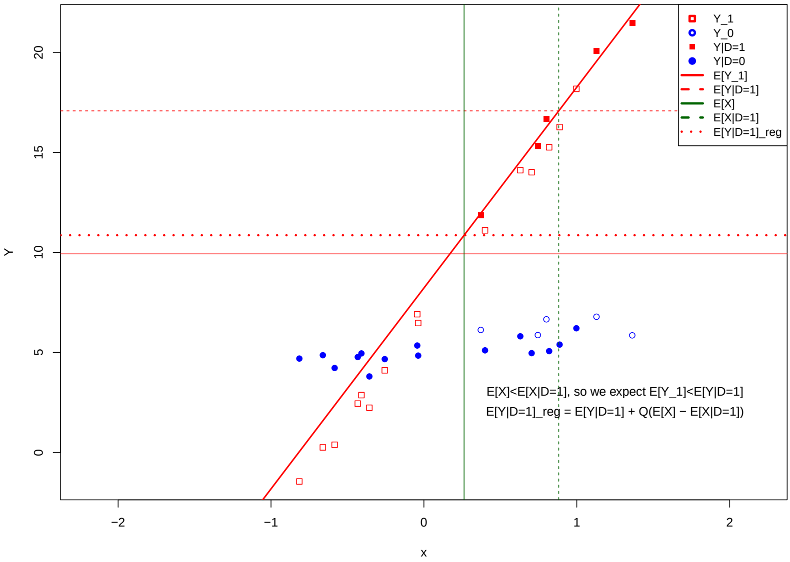

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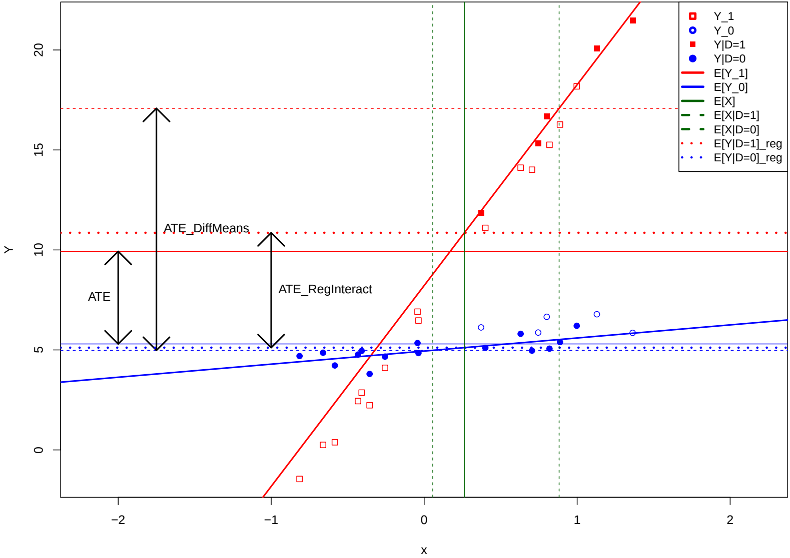


Covariate Adjustment with Regression

Freedman (2008) shows that regression of the form:

$$Y_i = \alpha + \tau_{reg} D_i + \beta_1 X_i + \epsilon_i$$

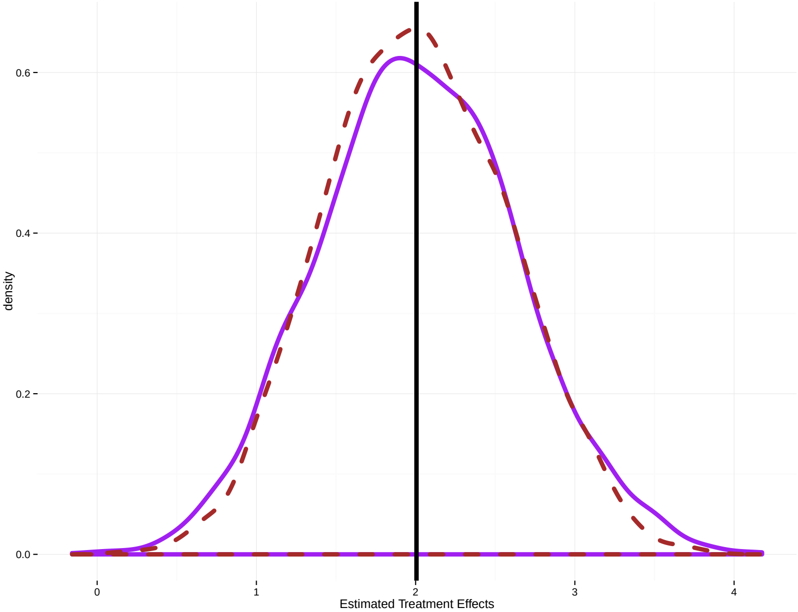
- $\hat{\tau}_{reg}$ is consistent for ATE and has small sample bias (unless model is true)
  - bias is on the order of $1/n$ and diminishes rapidly as N increases
- $\hat{\tau}_{reg}$ will not necessarily improve precision if model is incorrect
  - But harmful to precision only if more than 3/4 of units are assigned to one treatment condition or $\mathrm{Cov}(D_i, Y_1 - Y_0)$ larger than $\mathrm{Cov}(D_i, Y)$.

Lin (2013) shows that regression of the form:

$$Y_i = \alpha + \tau_{interact} D_i + \beta_1 \cdot (X_i - \bar{X}) + \beta_2 \cdot D_i \cdot (X_i - \bar{X}) + \epsilon_i$$

- $\hat{\tau}_{interact}$ is consistent for ATE and has the same small sample bias
- Cannot hurt asymptotic precision even if model is incorrect and will likely increase precision if covariates are predictive of the outcomes.
- Results hold for multiple covariates

Covariate Adjustment with Regression



Why are Experimental Findings Robust to Alternative Specifications?

Note the following important property of OLS known as the Frisch-Waugh-Lovell (FWL) theorem or Anatomy of Regression:

$$\beta_k = \frac{Cov(Y_i, \tilde{x}_{ki})}{Var(\tilde{x}_{ki})}$$

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Let $\tilde{D}_i$ be the residuals after regressing D_i on X_i. For experimental data, on average, what will $\tilde{D}_i$ be equal to?
Since $\tilde{D}_i \approx D_i$, multivariate regressions will yield similar results to bivariate regressions.

Summary: Covariate Adjustment with Regression

- One does not need to believe in the classical linear model (linearity and constant treatment effects) to tolerate or even advocate OLS covariate adjustment in randomized experiments (agnostic view of regression)
- Covariate adjustment can buy you power (and thus allows for a smaller sample).
- Small sample bias might be a concern in small samples, but usually swamped by efficiency gains.
- Since covariates are controlled for by design, results are typically not model dependent
- Best if covariate adjustment strategy is pre-specified as this rules out fishing.
- Always show the unadjusted estimate for transparency.

Testing in Small Samples: Fisher's Exact Test

- Test of differences in means with large N:

$$H_0 : \mathbb{E}[Y_1] = \mathbb{E}[Y_0], \quad H_1 : \mathbb{E}[Y_1] \neq \mathbb{E}[Y_0] \text{ (weak null)}$$

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- Let Ω be the set of all possible randomization realizations.
- We only observe the outcomes, Y_i, for one realization of the experiment. We calculate $\hat{\tau} = \bar{Y}_1 - \bar{Y}_0$.

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$$H_0 : \mathbb{E}[Y_1] = \mathbb{E}[Y_0], \quad H_1 : \mathbb{E}[Y_1] \neq \mathbb{E}[Y_0] \text{ (weak null)}$$

- Fisher's Exact Test with small N:

$$H_0 : Y_1 = Y_0, \quad H_1 : Y_1 \neq Y_0 \qquad \text{(sharp null of no effect)}$$

- Let Ω be the set of all possible randomization realizations.
- We only observe the outcomes, Y_i, for one realization of the experiment. We calculate $\hat{\tau} = \bar{Y}_1 - \bar{Y}_0$.
- Under the sharp null hypothesis, we can compute the value that the difference in means estimator would have taken under any other realization, $\hat{\tau}(\omega)$, for $\omega \in \Omega$.

Testing in Small Samples: Fisher's Exact Test

i	Y_{1i}	Y_{0i}	D_i
1	3	?	1
2	1	?	1
3	?	0	0
4	?	1	0
$\widehat{\tau}_{ATE}$			1.5

What do we know given the sharp null $H_0 : Y_1 = Y_0$?

Testing in Small Samples: Fisher's Exact Test

i	Y_{1i}	Y_{0i}	D_i
1	3	3	1
2	1	1	1
3	0	0	0
4	1	1	0
$\widehat{\tau}_{ATE}$			1.5
$\hat{\tau}(\omega)$			1.5

Given the full schedule of potential outcomes under the sharp null, we can compute the null distribution of ATE_{H_0} across all possible randomization.

Testing in Small Samples: Fisher's Exact Test

i	Y_{1i}	Y_{0i}	D_i	D_i
1	3	3	1	1
2	1	1	1	0
3	0	0	0	1
4	1	1	0	0
$\widehat{\tau}_{ATE}$			1.5	
$\hat{\tau}(\omega)$			1.5	0.5

Testing in Small Samples: Fisher's Exact Test

i	Y_{1i}	Y_{0i}	D_i	D_i	D_i
1	3	3	1	1	1
2	1	1	1	0	0
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$\widehat{\tau}_{ATE}$			1.5		
$\hat{\tau}(\omega)$			1.5	0.5	1.5

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i	Y_{1i}	Y_{0i}	D_i	D_i	D_i	D_i
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Testing in Small Samples: Fisher's Exact Test

i	Y_{1i}	Y_{0i}	D_i	D_i	D_i	D_i	D_i	D_i
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2	1	1	1	0	0	1	1	0
3	0	0	0	1	0	1	0	1
4	1	1	0	0	1	0	1	1
$\widehat{\tau}_{ATE}$			1.5					
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So $\Pr(\hat{\tau}(\omega) \geq \widehat{\tau}_{ATE}) = 2/6 \approx .33$.

Which assumptions are needed?

Testing in Small Samples: Fisher's Exact Test

i	Y_{1i}	Y_{0i}	D_i	D_i	D_i	D_i	D_i	D_i
1	3	3	1	1	1	0	0	0
2	1	1	1	0	0	1	1	0
3	0	0	0	1	0	1	0	1
4	1	1	0	0	1	0	1	1
$\widehat{\tau}_{ATE}$			1.5					
$\hat{\tau}(\omega)$			1.5	0.5	1.5	-1.5	-.5	-1.5

So $\Pr(\hat{\alpha}(\omega) \geq \widehat{\tau}_{ATE}) = 2/6 \approx .33$.

Which assumptions are needed? None! Randomization as "reasoned basis for causal inference" (Fisher 1935)

Blocking

- Imagine you have data on the units that you are about to randomly assign. Why leave it to "pure" chance to balance the observed characteristics?
- Idea in blocking is to pre-stratify the sample and then to randomize separately within each stratum to ensure that the groups start out with identical observable characteristics on the blocked factors.
- You effectively run a separate experiment within each stratum, randomization will balance the unobserved attributes
- Why is this helpful?
  - Four subjects with pre-treatment outcomes of $\{2,2,8,8\}$
  - Divided evenly into treatment and control groups and treatment effect is zero
  - Simple random assignment will place $\{2,2\}$ and $\{8,8\}$ together in the same treatment or control group $1/3$ of the time

Blocking

Imagine you run an experiment where you block on gender. It's possible to think about an ATE composed of two seperate block-specific ATEs:

$$\tau = \frac{N_f}{N_f + N_m} \cdot \tau_f + \frac{N_m}{N_f + N_m} \cdot \tau_m$$

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An unbiased estimator for this quantity will be

$$\hat{\tau}_B = \frac{N_f}{N_f + N_m} \cdot \hat{\tau}_f + \frac{N_m}{N_f + N_m} \cdot \hat{\tau}_m$$

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or more generally, if there are J strata or blocks, then

$$\hat{\tau}_B = \sum_{j=1}^{J} \frac{N_j}{N} \hat{\tau}_j$$

Blocking

Because the randomizations in each block are independent, the variance of the blocking estimator is simply $(\mathrm{Var}(aX + bY) = a^2\mathrm{Var}(X) + b^2\mathrm{Var}(Y))$
:

$$\mathrm{Var}(\hat{\tau}_B) = \left(\frac{N_f}{N_f + N_m}\right)^2 \mathrm{Var}(\hat{\tau}_f) + \left(\frac{N_m}{N_f + N_m}\right)^2 \mathrm{Var}(\hat{\tau}_m)$$

or more generally

$$Var(\hat{\tau}_B) = \sum_{j=1}^{J} \left(\frac{N_j}{N}\right)^2 \mathrm{Var}(\hat{\tau}_j)$$

Blocking with Regression

When analyzing a blocked randomized experiment with OLS and the probability of receiving treatment is equal across blocks, then OLS with block "fixed effects" will result in a valid estimator of the ATE:

$$y_i = \tau D_i + \sum_{j=2}^{J} \beta_j \cdot B_{ij} + \epsilon_i$$

where B_j is a dummy for the j-th block (one omitted as reference category).

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If probabilites of treatment, $p_{ij} = P(D_{ij} = 1)$, vary by block, then weight each observation:

$$w_{ij} = \left(\frac{1}{p_{ij}}\right) D_i + \left(\frac{1}{1 - p_{ij}}\right) (1 - D_i)$$

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Why do this? When treatment probabilities vary by block, then OLS will weight blocks by the variance of the treatment variable in each block. Without correcting for this, OLS will result in biased estimates of ATE!

When Does Blocking Help?

Imagine a model for a complete and blocked randomized design:

$$Y_i = \alpha + \tau_{CR} D_i + \varepsilon_i$$

When Does Blocking Help?

Imagine a model for a complete and blocked randomized design:

$$Y_i = \alpha + \tau_{CR} D_i + \varepsilon_i \quad (1)$$

$$Y_i = \alpha + \tau_{BR} D_i + \sum_{j=2}^{J} \beta_j B_{ij} + \varepsilon_i^* \quad (2)$$

where B_j is a dummy for the j-th block. Then given iid sampling:

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$$Var[\widehat{\tau}_{BR}] = \frac{\sigma_{\varepsilon^*}^2}{\sum_{i=1}^n (D_i - \bar{D})^2 (1 - R_j^2)} \quad \text{with } \widehat{\sigma}_{\varepsilon^*}{}^2 = \frac{\sum_{i=1}^n \widehat{\varepsilon}_i^{*2}}{n-k-1} = \frac{SSR_{\widehat{\varepsilon}^*}}{n-k-1}$$

where R_j^2

When Does Blocking Help?

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where B_j is a dummy for the j-th block. Then given iid sampling:

$$Var[\widehat{\tau}_{CR}] = \frac{\sigma_\varepsilon^2}{\sum_{i=1}^n (D_i - \bar{D})^2} \quad \text{with } \widehat{\sigma}_\varepsilon^2 = \frac{\sum_{i=1}^n \widehat{\varepsilon}_i^2}{n-2} = \frac{SSR_{\widehat{\varepsilon}}}{n-2}$$

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where R_j^2 is R^2 from regression of D on all B_j variables and a constant.

When Does Blocking Help?

$$Y_i = \alpha + \tau_{CR} D_i + \varepsilon_i \quad (3)$$

$$Y_i = \alpha + \tau_{BR} D_i + \sum_{j=2}^{J} \beta_j B_{ij} + \varepsilon_i^* \quad (4)$$

where B_k is a dummy for the k-th block. Then given iid sampling:

$$V[\widehat{\tau}_{CR}] = \frac{\sigma_\varepsilon^2}{\sum_{i=1}^n (D_i - \bar{D})^2} \quad \text{with } \widehat{\sigma}_\varepsilon^2 = \frac{\sum_{i=1}^n \widehat{\varepsilon}_i^2}{n-2} = \frac{SSR_{\widehat{\varepsilon}}}{n-2}$$

$$V[\widehat{\tau}_{BR}] = \frac{\sigma_{\varepsilon^*}^2}{\sum_{i=1}^n (D_i - \bar{D})^2 (1 - R_j^2)} \quad \text{with } \widehat{\sigma}_{\varepsilon^*}{}^2 = \frac{\sum_{i=1}^n \widehat{\varepsilon}_i^{*2}}{n-k-1} = \frac{SSR_{\widehat{\varepsilon}^*}}{n-k-1}$$

where R_j^2 is R^2 from regression of D on the B_k dummies and a constant.
So when is $Var[\widehat{\tau}_{BR}] < Var[\widehat{\tau}_{CR}]$?

When Does Blocking Help?

$$
\begin{aligned}
Y_i &= \alpha + \tau_{CR} D_i + \varepsilon_i && (5)\\
Y_i &= \alpha + \tau_{BR} D_i + \sum_{j=2}^{J} \beta_j B_{ij} + \varepsilon_i^* && (6)
\end{aligned}
$$

where B_k is a dummy for the k-th block. Then given iid sampling:

$$
\begin{aligned}
V[\widehat{\tau}_{CR}] &= \frac{\sigma_\varepsilon^2}{\sum_{i=1}^n (D_i - \bar{D})^2} && \text{with } \widehat{\sigma}_\varepsilon^2 = \frac{\sum_{i=1}^n \widehat{\varepsilon}_i^2}{n-2} = \frac{SSR_{\widehat{\varepsilon}}}{n-2}\\
V[\widehat{\tau}_{BR}] &= \frac{\sigma_{\varepsilon^*}^2}{\sum_{i=1}^n (D_i - \bar{D})^2 (1 - R_j^2)} && \text{with } \widehat{\sigma}_{\varepsilon^*}{}^2 = \frac{\sum_{i=1}^n \widehat{\varepsilon_i^*}^2}{n-k-1} = \frac{SSR_{\widehat{\varepsilon^*}}}{n-k-1}
\end{aligned}
$$

where R_j^2 is R^2 from regression of D on the B_k dummies and a constant.
Since $R_j^2 \approx 0$ $V[\widehat{\tau}_{BR}] < V[\widehat{\tau}_{CR}]$ if $\frac{SSR_{\widehat{\varepsilon^*}}}{n-k-1} < \frac{SSR_{\widehat{\varepsilon}}}{n-2}$

Blocking

- How does blocking help?
  - Increases efficiency if the blocking variables predict outcomes (i.e. they "remove" the variation that is driven by nuisance factors)
  - Blocking on irrelevant predictors can burn up degrees of freedom.
  - Can help with small sample bias due to "bad" randomization
  - Is powerful especially in small to medium sized samples.

Blocking

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  - Increases efficiency if the blocking variables predict outcomes (i.e. they "remove" the variation that is driven by nuisance factors)
  - Blocking on irrelevant predictors can burn up degrees of freedom.
  - Can help with small sample bias due to "bad" randomization
  - Is powerful especially in small to medium sized samples.
- What to block on?
  - "Block what you can, randomize what you can't"
  - The baseline of the outcome variable and other main predictors.
  - Variables desired for subgroup analysis
- How to block?
  - Stratification
  - Pair-matching
  - Check: `blockTools` library.

Analysis with Blocking

- **"As ye randomize, so shall ye analyze"** (Senn 2004): Need to account for the method of randomization when performing statistical analysis.
- If using OLS, strata dummies should be included when analyzing results of stratified randomization.
  - If probability of treatment assignment varies across blocks, then weight treated units by probability of being in treatment and controls by the probability of being a control.
- Failure to control for the method of randomization can result in incorrect test size.