Week 8: Regression in the Social Sciences

Brandon Stewart

Princeton

November 7 and 9, 2016

These slides are heavily influenced by Matt Blackwell, Justin Grimmer, Jens Hainmueller, Erin Hartman, Kosuke Imai and Gary King.
Where We’ve Been and Where We’re Going…

Last Week
- matrix form of linear regression
- inference and F-tests

This Week
- Monday: making an argument in social sciences
- Wednesday: causal inference

Next Week
- regression diagnostics

Long Run
- regression → diagnostics → causal inference

Questions?

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Questions?
1. Thousand Foot View
2. Power
3. Problems with $p$-Values
4. Visualization and Quantities of Interest
5. A Preview of Causal Inference
6. Fun With Censorship
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Why Are We Doing All of This Again?

We are all here because we are trying to do some social science, that is, we are in the business of knowledge production. Quantitative methods are an increasingly big part of that so whether you are reading or actively doing quantitative analysis it is going to be there.

So why all the math?

We are taking a future-oriented approach. We want to prepare you for the next big thing. Methods that became popular in the social sciences since I took the equivalent of this class: machine learning, text-as-data, Bayesian nonparametrics, design-based inference, DAG-based causal inference. A technical foundation prepares you to learn new methods for the rest of your career. Trust me, now is the time to invest. Knowing how methods work also makes you a better reader of work.
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DO ALL THE MATH
Three components of quantitative social science:

1. Argument
2. Research Design
3. Presentation

This week we will cover a few issues in these areas:
▶ power (research design)
▶ problems with \( p \)-values (argument, design, presentation)
▶ visualization and quantities of interest (argument, presentation)
▶ identification and causal inference (argument, design)

We will mostly talk about statistical methods here (it is a statistics class!) but the best work is a combination of substantive and statistical theory.
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Are Most Published Findings False?

The backdrop for this is numerous studies with a dim view of the veracity of the average academic article.

Ioannidis (2005) "Why most published research findings are false" PLOS Medicine

Begley and Ellis (2012) "Drug development: raise standard for preclinical cancer research" Nature

Johnson (2013) "Revised standards for statistical evidence" PNAS

Franco, Malhotra, Simonovits (2014) "Publication Bias in the Social Sciences: Unlocking the File Drawer" Science


Leek and Jager (2017) "Is Most Published Research Really False?" Annual Review of Statistics and Its Applications
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Thousand Foot View

Power

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Fun With A Bundle of Sticks
Matt Salganik in his book *Bit by Bit* argues that it is an ethical imperative to only use as many subjects as we need in an experiment.
An Example Gerber, Green and Larimer (2008)

- Matt Salganik in his book *Bit by Bit* argues that it is an ethical imperative to only use as many subjects as we need in an experiment.
- Why did GGL use sample sizes of 38,000? Could they have used fewer? How would we know?

<table>
<thead>
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<th>TABLE 2. Effects of Four Mail Treatments on Voter Turnout in the August 2006 Primary Election</th>
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<td>Experimental Group</td>
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Why did GGL use sample sizes of 38,000? Could they have used fewer? How would we know?

We choose the sample size that can ensure that we can **detect** what we think is the true treatment effect (i.e. reject the null of no effect).

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<tr>
<td>N of Individuals</td>
<td>191,243</td>
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Small effects will require more observations than large effects. But how many more?

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Statistical Power

Type 1 errors—false discovery
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Power will (in general) depend on four factors (particularly for \( t/ \) normally distributed test statistics):

1. Type I error rate (\( \alpha \))
2. Effect size
3. Variance
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Suppose (again) we’re running an experiment, sampling from two normal distributions (treatment and control).
Power and Hypothesis Tests

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Testing hypotheses:

\[ H_0 : \mu_t - \mu_c = \mu_{\text{diff}} = 0 \]

\[ H_1 : \mu_{\text{diff}} \neq 0 \]

Test statistic:

\[ t = \frac{T - C}{\sqrt{\hat{\sigma}_t^2/n_t + \hat{\sigma}_c^2/n_c}} \]
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Key question: given true value of $\mu_{\text{diff}}^* \neq 0$ what is the probability $t$ falls in “fail to reject” region?

Pr(Type 2 error) = $P(-1.96 < t < 1.96)$

\[ \text{Power} = 1 - \text{Pr(Type 2 error)} \]

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![Graph showing power analysis](image-url)
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- not evidence of no effect!
Before starting they did a **power analysis**
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Steps to a power analysis

1. Pick some hypothetical effect size $\mu_y - \mu_x = 0.05$
2. Calculate the distribution of test statistic $T$
3. Calculate the probability of rejecting the null under that distribution
4. Repeat for different effect sizes

Let's say we want to run another experiment where we believe the true effect is $\mu_y - \mu_x = 0$.

The variances are $\sigma_y^2 = \sigma_x^2 = 0.2$.

We can only afford to send out 500 mailers. Should we run the experiment?
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Let’s say we want to run another experiment where we believe the true effect is $\mu_y - \mu_x = 0.05$ and the variances are $\sigma_y^2 = \sigma_x^2 = 0.2$

We can only afford to send out 500 mailers. Should we run the experiment?
Before starting they did a **power analysis**

Steps to a power analysis:
1. Pick some hypothetical effect size \( \mu_y - \mu_x = 0.05 \)
2. Calculate the distribution of test statistic \( T \) under that effect size
3. Calculate the probability of rejecting the null under that distribution
4. Repeat for different effect sizes

Let’s say we want to run another experiment where we believe the true effect is \( \mu_y - \mu_x = 0.05 \) and the variances are \( \sigma_y^2 = \sigma_x^2 = 0.2 \)

We can only afford to send out 500 mailers. Should we run the experiment?

To the board!
A Short Case Study of Retrospective Power Analysis

Durante, Arsena and Griskevicius (2013) publish a study in Psychological Science on menstrual cycles and political attitudes. They report a 17 point swing in voting preferences in the 2012 election.

▶ for context polling showed Obama's support varying by 7 points during the entire general campaign (Gallup 2012)

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Gelman and Carlin (2014) show how the study design should cause us to question the finding.

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A Troubling Figure (via Gelman)

This is what "power = 0.06" looks like.
Get used to it.

Type S error probability:
If the estimate is statistically significant, it has a 24% chance of having the wrong sign.

Exaggeration ratio:
If the estimate is statistically significant, it must be at least 9 times higher than the true effect size.

Estimated effect size
1. Thousand Foot View
2. Power
3. Problems with $p$-Values
4. Visualization and Quantities of Interest
5. A Preview of Causal Inference
6. Fun With Censorship
7. Neyman-Rubin Model of Causal Inference
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Problems with $p$-values

$p$-values are extremely common in the social sciences and are often the standard by which the value of the finding is judged.

$p$-values are not:

▶ an indication of a large substantive effect
▶ the probability that the null hypothesis is true
▶ the probability that the alternative hypothesis is false

A large $p$-value could mean either that we are in the null world OR that we had insufficient power.
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So what is the basic idea?

The idea was to run an experiment, then see if the results were consistent with what random chance might produce. Researchers would first set up a ‘null hypothesis’ that they wanted to disprove, such as there being no correlation or no difference between groups. Next, they would play the devil’s advocate and, assuming that this null hypothesis was in fact true, calculate the chances of getting results at least as extreme as what was actually observed. This probability was the P value. The smaller it was, suggested Fisher, the greater the likelihood that the straw-man null hypothesis was false.

(Nunzo 2014, emphasis mine)
I’ve got 99 problems...
I’ve got 99 problems.

*p*-values are hard to interpret, but even in the best scenarios they have some key problems:
I’ve got 99 problems...

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- they remove focus from data, measurement, theory and the substantive quantity of interest
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- “significant covariates” aren’t even necessarily good predictors (Ward et al 2010, Lo et al 2015)
- they lead to publication filtering on arbitrary cutoffs.
Arbitrary Cutoffs

Figure 1a: Histogram of Z-Statistics, APSR & AJPS (Two-Tailed)

Gerber and Malhotra (2006) Top Political Science Journals
Arbitrary Cutoffs

Figure 1
Histogram of z Statistics From the American Sociological Review, the American Journal of Sociology, and The Sociological Quarterly (Two-Tailed)

Gerber and Malhotra (2008) Top Sociology Journals
Arbitrary Cutoffs

Figure 1. The graphs show the distribution of 3,627 p values from three major psychology journals.

Masicampo and Lalande (2012) Top Psychology Journals
Still Not Convinced?
The Real Harm of Misinterpreted $p$-values

Viewpoint

The harm done by tests of significance

Ezra Hauer*

35 Merton Street, Apt. 1706, Toronto, Ont., Canada M4S 3G4

Abstract

Three historical episodes in which the application of null hypothesis significance testing (NHST) led to the mis-interpretation of data are described. It is argued that the pervasive use of this statistical ritual impedes the accumulation of knowledge and is unfit for use.

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Keywords: Significance; Statistical hypothesis; Scientific method
Example from Hauer: Right-Turn-On-Red

### Table 1
The Virginia RTOR study

<table>
<thead>
<tr>
<th>Category</th>
<th>Before RTOR signing</th>
<th>After RTOR signing</th>
</tr>
</thead>
<tbody>
<tr>
<td>Fatal crashes</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Personal injury crashes</td>
<td>43</td>
<td>60</td>
</tr>
<tr>
<td>Persons injured</td>
<td>69</td>
<td>72</td>
</tr>
<tr>
<td>Property damage crashes</td>
<td>265</td>
<td>277</td>
</tr>
<tr>
<td>Property damage (US$)</td>
<td>161243</td>
<td>170807</td>
</tr>
<tr>
<td>Total crashes</td>
<td>308</td>
<td>337</td>
</tr>
</tbody>
</table>
The Point in Hauer

Two other interesting examples in Hauer (2004) are:

Core issue is that lack of significance is not an indication of a zero effect, it could also be a lack of power (i.e. a small sample size relative to the difficulty of detecting the effect). On the opposite end, large tech companies essentially never use significance testing because they have huge samples which essentially always find some non-zero effect. But that doesn't make the effect significant in a colloquial sense of important.
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On the opposite end, large tech companies essentially never use significance testing because they have huge samples which essentially always find some non-zero effect. But that doesn’t make the effect significant in a colloquial sense of important.
P-values and Confidence Intervals

P-values one of most used tests in the social sciences—and you’re telling me not to rely on them?
P-values and Confidence Intervals

P-values one of most used tests in the social sciences—and you’re telling me not to rely on them?

- Basically, yes.
P-values and Confidence Intervals

What’s the matter with you?

Two reasons not to worship p-values [of many]

1) Statistical: they represent a very specific quantity under a null distribution. If you don’t really care about rejecting just that null, then you should focus on providing more information.

2) Substantive: p-values are divorced from your quantity of interest—which almost always should relate to how much an intervention changes a quantity of social scientific interest (Grandparent rule).
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P-values and Confidence Intervals

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1) Me too, good luck.
P-values and Confidence Intervals

But I want to assess the probability that my hypothesis is true—why can’t I use a p-value?

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2) That’s not what p-values measure
P-values and Confidence Intervals

But I want to assess the probability that my hypothesis is true—why can’t I use a p-value?

1) Me too, good luck.
2) That’s not what p-values measure
3) No one study is going to eliminate an entire hypothesis; even if that study generates a really small p-value, you’d probably want an entirely different infrastructure
P-values and Confidence Intervals

I like to estimate the certainty of my results using a p-value, do you find this problematic?
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- Yes
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- Yes

- What could be wrong with this?
P-values and Confidence Intervals

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OK—I get it.
P-values and Confidence Intervals

OK—I get it. No p-values

Why?
1) Substantive significance and statistical significance simultaneously
2) Make comparisons across factors approximately and accurately (though exercise caution)
3) Harder to hide weird looking effects
OK—I get it. No p-values (but I’m still going to use them anyways.)
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Confidence Intervals, Graphical Presentation of a quantity of interest
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But Let’s Not Obsess Too Much About \( p \)-values

From Leek and Peng (2015) “\( P \) values are just the tip of the iceberg” *Nature.*
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An Intro Motivation
An Intro Motivation

[Image of a pie chart showing the 2012 presidential run with percentages:
- Back Palin: 70%
- Back Huckabee: 63%
- Back Romney: 60%]
Visualization

Visualization is hard but ultimately extremely important. It is absurd that we spend months collecting data, weeks analyzing it and five minutes slapping it into an unreadable table.

Visualization can be used for many purposes:

▶ drawing people into a topic/dataset
▶ presenting evidence
▶ exploration/model checking

Three steps involved:

1. clearly define the goal
2. estimate quantities of interest
3. present those quantities in a compelling way

Good design involves thinking carefully about the audience.

Stewart (Princeton)

Week 8: Regression in the Social Sciences
November 7 and 9, 2016
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- Good design involves thinking carefully about the **audience**
Examples via Gelman
Examples via Gelman

One may be better for drawing people in, the other for evidence.
A classic infographic by Nightingale. Dramatizes the problem.
Examples via Gelman

A simpler version where it is easier to see the patterns.
How Not to Present Statistical Results

This one is typical of current practice, not especially bad.

What do these numbers mean?

Why so much whitespace? Can you connect cols A and B?
How Not to Present Statistical Results

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How Not to Present Statistical Results

TABLE 1
Predicting Which Ethnic Group Conquered Most of Bosnia

<table>
<thead>
<tr>
<th></th>
<th>Coefficient</th>
</tr>
</thead>
<tbody>
<tr>
<td>Attention to Bosnia crisis</td>
<td>.609**</td>
</tr>
<tr>
<td>Age</td>
<td>.007**</td>
</tr>
<tr>
<td>Education</td>
<td>.289**</td>
</tr>
<tr>
<td>Family income</td>
<td>.151**</td>
</tr>
<tr>
<td>Race (non-White/White)</td>
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</tr>
<tr>
<td>Gender (female/male)</td>
<td>.789**</td>
</tr>
<tr>
<td>Region (South/non-South)</td>
<td>.076</td>
</tr>
<tr>
<td>Network coverage</td>
<td>.000</td>
</tr>
<tr>
<td>Education × Time</td>
<td>-.003*</td>
</tr>
<tr>
<td>Time in months</td>
<td>.078**</td>
</tr>
<tr>
<td>Constant</td>
<td>-9.257**</td>
</tr>
<tr>
<td>Number</td>
<td>7,021</td>
</tr>
<tr>
<td>-2 log-likelihood</td>
<td>7,215.231</td>
</tr>
<tr>
<td>Goodness of fit</td>
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</tr>
<tr>
<td>Cox &amp; Snell $R^2$</td>
<td>.212</td>
</tr>
<tr>
<td>Nagelkerke $R^2$</td>
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</tr>
<tr>
<td>Overall correct classification (%)</td>
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</table>

NOTE: Unstandardized coefficients for logistic regression. Dependent variable is knowledge of which group conquered most of Bosnia.
*p ≤ .05, two-tailed. **p ≤ .01, two-tailed.
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How Not to Present Statistical Results

This one is typical of current practice, not especially bad.

What do these numbers mean?
How Not to Present Statistical Results

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- Why so much whitespace? Can you connect cols A and B?
How Not to Present Statistical Results: Continued

What does the star-gazing add?
Can any be interpreted as causal estimates?
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<tr>
<td>Number</td>
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<td>-2 log-likelihood</td>
<td>7,215.231</td>
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2. For example: Other things being equal, an additional year of education would increase your annual income by $1,500 on average, plus or minus about $500.

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We will discuss more general recipes in Soc504 but essentially three approaches: analytic, parametric simulation, non-parametric simulation.

Analytic method involve finding the quantity and using the properties of variance and covariances to calculate a confidence interval. Can be challenging for complicated quantities.

Parametric simulation uses the estimated coefficients and variance-covariance matrix to simulate outcomes (we will cover this in Soc504).

Non-parametric simulation uses the bootstrap. Calculate the quantity of interest within each bootstrap sample and then aggregate to get an estimate of the variance.
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Visualization

Looking at data
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- Most basic method of inference
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Four (related) reasons
Reason 1: Models Lie

Remember Anscombe’s quartet?
## Reason 2: Delivery of Information

<table>
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<tr>
<th></th>
<th>x</th>
<th>y</th>
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<td>3</td>
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Reason 2: Delivery of Information
Reason 3: Model Skepticism and Checking
Reason 3: Model Skepticism

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All inferences rest on assumption—visualization is a particularly reliable method for identifying obvious violations
Example from Justin Grimmer’s work.
Reason 4: Presentation and Persuasion

Lincoln Chafee, (R-RI)
33% Credit Claiming
26% Position Taking
Airport Grants

Example from Justin Grimmer’s work.
Reason 4: Presentation and Persuasion

Susan Collins, (R-RI)
29% Credit Claiming
20% Position Taking

Airport Grants

Example from Justin Grimmer’s work.
Reason 4: Presentation and Persuasion

Two Party Vote Share, Bush
Prop Credit Claiming - Prop. Position Taking

Mike Enzi, (R-WY)
18% Credit Claiming
35% Position Taking
Tax Reform

Example from Justin Grimmer's work.
Reason 4: Presentation and Persuasion

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John Tester, (D-MT)
43% Credit Claiming
20% Position Taking
Water Grants
Reason 4: Presentation and Persuasion

Sheldon Whitehouse, (D-RI)
8% Credit Claiming
54% Position Taking
Iraq War

Example from Justin Grimmer’s work.
There are a number of problems with \textit{p}-values and significance testing as currently applied. At the core of these problems is that \textit{p}-values don't mean quite what we want them to mean. A solution? Generate quantities of interest connected to your theory and present those. Tools such as the bootstrap and simulation techniques described today can help get the right quantities. We will expand on this next semester. Visualizations can serve many purposes including a compelling way to present these quantities. Many of these concerns have motivated a turn towards causal inference.
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1 Thousand Foot View
2 Power
3 Problems with $p$-Values
4 Visualization and Quantities of Interest
5 A Preview of Causal Inference
6 Fun With Censorship
7 Neyman-Rubin Model of Causal Inference
8 Complications
9 ATE and Other Estimands
10 Graphical Models
11 Fun With A Bundle of Sticks
What is Causal Inference?

A causal inference is a statement about counterfactuals. The difference between prediction and causal inference is the intervention on the system under study. Like it or not, social science theories are almost always expressed as causal claims: e.g. "an increase in $X$ causes an increase in $Y$". The study of causal inference helps us understand the assumptions we need to make this kind of claim.
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A quantity of interest is identified when access to infinite data would result in the estimate taking on only a single value. For example, having all dummy variables in a linear model is not statistically identified because they cannot be distinguished from the intercept.

Causal identification is what we can learn about a causal effect from available data. If an effect is not identified, no estimation method will recover it. This means the relevant question is “what’s your identification strategy?” or what are the set of assumptions that let you claim you’ve estimated a causal effect? As we will see this is not a conversation about estimation (in other words the answer cannot be “regression”).
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Stewart (Princeton)
Identification vs. Estimation

- **Identification**: How much can you learn about the estimand if you have an infinite amount of data?
- **Estimation**: How much can you learn about the estimand from a finite sample?

The role of assumptions:
- Often identification requires (hopefully minimal) assumptions
- Even when identification is possible, estimation may impose additional assumptions (i.e. regression)

Law of Decreasing Credibility (Manski): The credibility of inference decreases with the strength of the assumptions maintained.
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- This line of work is one of my favorites.

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

The largest selective suppression of human expression in history
Chinese Censorship

The largest selective suppression of human expression in history:

- implemented manually,
- by \( \approx 200,000 \) workers,
- located in government and inside social media firms
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Either or both could be right or wrong.
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Collect 3,674,698 social media posts in 85 topic areas over 6 months
Random sample: 127,283
(Repeat design; Total analyzed: 11,382,221)

⇝

For each post (on a timeline in one of 85 content areas):
▶ Download content the instant it appears
▶ (Carefully) revisit each later to determine if it was censored
▶ Use computer-assisted methods of text analysis (some existing, some new, all adapted to Chinese)
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Censorship is not Ambiguous: BBS Error Page

The page you requested is temporarily down. How about you go look at another page.

Jingjing, one of China’s cartoon internet police
For 2 Unusual Topics: Constant Censorship Effort
For 2 Unusual Topics: Constant Censorship Effort

**Pornography**

**Criticism of the Censors**

Students Throw Shoes at Fang BinXing

**Stewart (Princeton)**  Week 8: Regression in the Social Sciences  November 7 and 9, 2016
All other topics: Censorship & Post Volume are “Bursty”

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They monitored 85 topic areas (Jan–July 2011)

Found 87 volume bursts in total

Identified real world events associated with each burst

Their hypothesis: The government censors all posts in volume bursts associated with events with collective action (regardless of how critical or supportive of the state)
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Begin with 87 volume bursts in 85 topics areas

For each burst, calculate change in % censorship inside to outside each volume burst within topic areas – censorship magnitude

If goal of censorship is to stop collective action, they expect:

1. On average, % censored should increase during volume bursts
2. Some bursts (associated with politically relevant events) should have much higher censorship

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<th>Censorship Magnitude</th>
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<tr>
<td>-0.2</td>
<td>0</td>
</tr>
<tr>
<td>0.0</td>
<td>1</td>
</tr>
<tr>
<td>0.2</td>
<td>2</td>
</tr>
<tr>
<td>0.4</td>
<td>3</td>
</tr>
<tr>
<td>0.6</td>
<td>4</td>
</tr>
<tr>
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Stewart (Princeton)

Week 8: Regression in the Social Sciences

November 7 and 9, 2016
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![Censorship Magnitude vs Density Chart]

Stewart (Princeton)

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![Graph showing distribution of censorship magnitude](image)
Observational Test 2: The Event Generating Volume Bursts
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Event classification (each category can be +, −, or neutral comments about the state)
Observational Test 2: The Event Generating Volume
Bursts

Event classification (each category can be +, −, or neutral comments about the state)

1. Collective Action Potential

- Protest or organized crowd formation outside the Internet
- Individuals who have organized or incited collective action on the ground in the past;
- Topics related to nationalism or nationalist sentiment that have incited protest or collective action in the past.

Stewart (Princeton)
Observational Test 2: The Event Generating Volume Bursts

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What Types of Events Are Censored?

[Graph showing the density of censorship magnitude with categories: Collective Action, Criticism of Censors, Pornography, Policy, and News. Each category is represented by bars of different colors and heights. The x-axis represents censorship magnitude ranging from -0.2 to 0.8, and the y-axis represents density.]
What Types of Events Are Censored?

- Protests in Inner Mongolia
- Pornography Disguised as News
- Baidu Copyright Lawsuit
- Zengcheng Protests
- Pornography Mentioning Popular Book
- Ai Weiwei Arrested
- Collective Anger At Lead Poisoning in Jiangsu
- Google is Hacked
- Localized Advocacy for Environment Lottery
- Fuzhou Bombing
- Students Throw Shoes at Fang BinXing
- Rush to Buy Salt After Earthquake
- New Laws on Fifty Cent Party

Censorship Magnitude

- Policies
- News
- U.S. Military Intervention in Libya
- Food Prices Rise
- Education Reform for Migrant Children
- Popular Video Game Released
- Indoor Smoking Ban Takes Effect
- News About Iran Nuclear Program
- Jon Huntsman Steps Down as Ambassador to China
- Gov't Increases Power Prices
- China Puts Nuclear Program on Hold
- Chinese Solar Company Announces Earnings
- EPA Issues New Rules on Lead
- Disney Announced Theme Park
- Popular Book Published in Audio Format

Censorship Magnitude
Censoring Collective Action: Ai Weiwei’s Arrest

![Graph showing the counts of published and censored articles about Ai Weiwei's arrest over time.]

- **Count Published**
- **Count Censored**

Ai Weiwei arrested
Censoring Collective Action: Protests in Inner Mongolia

Protests in Inner Mongolia

Count
Jan Feb Mar Apr May Jun Jul
Count Published
Count Censored

Protests in Inner Mongolia
Low Censorship on One Child Policy

Speculation of Policy Reversal at NPC

Count Published
Count Censored
Low Censorship on News: Power Prices

Power shortages
Gov't raises power prices to curb demand

Count
Count Published
Count Censored

Week 8: Regression in the Social Sciences
November 7 and 9, 2016
References

This Lecture:

- Gelman and Carlin (2014). “Beyond Power Calculations: Assessing Type S (Sign) and Type M (Magnitude) Errors”
- Kastellec and Leoni (2007). “Using Graphs Instead of Tables in Political Science.” *Perspectives on Politics*
Where We’ve Been and Where We’re Going...

- Last Week
  - matrix form of linear regression
  - inference and F-tests

- This Week
  - Monday: making an argument in social sciences
  - Wednesday: causal inference

- Next Week
  - regression diagnostics

Long Run
- regression → diagnostics → causal inference

Questions?

Stewart (Princeton)
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Questions?
1. Thousand Foot View
2. Power
3. Problems with $p$-Values
4. Visualization and Quantities of Interest
5. A Preview of Causal Inference
6. Fun With Censorship
7. Neyman-Rubin Model of Causal Inference
8. Complications
9. ATE and Other Estimands
10. Graphical Models
11. Fun With A Bundle of Sticks
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7 **Neyman-Rubin Model of Causal Inference**
8 Complications
9 ATE and Other Estimands
10 Graphical Models
11 Fun With A Bundle of Sticks
Causation

What’s a cause?

- Time precedence
- Constant Conjunction
- Correlation is not causation
- We will see that correlation based definitions can lead to consideration of nonsensical causes
- Method of difference
- Identify identical units, except for treatment. Attribute cause to difference
- Granger Cause
- Forecast based definition of cause
- Problem of common causes
Causation

What’s a cause?
- Time precedence
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Causation

I used to think correlation implied causation.

Then I took a statistics class. Now I don't.

Sounds like the class helped.

Well, maybe.
Neyman-Rubin Model

Two possible conditions:

- Treatment condition $T = 1$
- Control condition $T = 0$

Suppose that we have an individual $i$.

Key assumption: we can imagine a world where individual $i$ is assigned to treatment and control conditions.

Potential Outcomes: responses under each condition, $Y_i(T)$

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Definition: no differences between treatment and control worlds.
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Job Training Programs:
- Treatment: Receive job training ($T = 1$)
- Control: No job training ($T = 0$)

Response: Income (Dollars per year)

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Fundamental Problem of Causal Inference (Holland (1986)):

It is impossible to observe both $Y_i(1)$ and $Y_i(0)$.
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Stewart (Princeton)

Week 8: Regression in the Social Sciences

November 7 and 9, 2016
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Week 8: Regression in the Social Sciences
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Some Useful Terms
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**Definition (Treatment)**

\[ D_i: \text{Indicator of treatment intake for unit } i \]

\[ D_i = \begin{cases} 
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Definition (Potential Outcome)

\(Y_{0i}\) and \(Y_{1i}\): Potential outcomes for unit \(i\)

\[Y_{di} = \begin{cases} 
Y_{1i} & \text{Potential outcome for unit } i \text{ with treatment} \\
Y_{0i} & \text{Potential outcome for unit } i \text{ without treatment} 
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Some Useful Terms

Definition (Causal Effect)

Causal effect of the treatment on the outcome for unit $i$ is the difference between its two potential outcomes:

$$\tau_i = Y_{1i} - Y_{0i}$$

Assumption

Observed outcomes are realized as $Y_i = D_i \cdot Y_{1i} + (1 - D_i) \cdot Y_{0i}$ so $Y_i = \begin{cases} 
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Causal Inference as a Missing Data Problem

Realized Outcome: \( Y_i = D_i Y_{1i} + (1-D_i) Y_{0i} \)

Fundamental Problem of Causal Inference

Cannot observe both potential outcomes, so we how can we calculate \( \tau_i = Y_{1i} - Y_{0i} \)?

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Causal Inference as a Missing Data Problem

Causal inference is difficult because it involves missing data. How can we calculate $\tau_i = Y_1^i - Y_0^i$?

Homogeneity is one solution:

- If $\{Y_1^i, Y_0^i\}$ is constant across individuals, then cross-sectional comparisons will recover $\tau_i$.
- If $\{Y_1^i, Y_0^i\}$ is constant across time, then before and after comparisons will recover $\tau_i$.

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The Selection Problem

- Why is this difficult? selection bias

\[ E[Y_i | D_i = 1] - E[Y_i | D_i = 0] = E[Y_i(1) | D_i = 1] - E[Y_i(0) | D_i = 1] + E[Y_i(0) | D_i = 1] - E[Y_i(0) | D_i = 0] \]

Average Treatment Effect on Treated

Naive estimator = Average Treatment Effect on Treated + Selection Bias

Selection bias: how different the treated and control groups are in terms of their potential outcome under control.
The Selection Problem

- Why is this difficult? **selection bias**
- The core idea is that the people who get treatment might look different from those who get control and thus they are not good **counterfactuals** for each other.
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- Let's look at what we get from a naive difference in means with a binary treatment:

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Selection Makes Us Care About Assignment Mechanisms

The process that determines which units receive which treatments, hence which potential outcomes are realized and thus can be observed, and, conversely, which potential outcomes are missing.

(Imbens and Rubin, 2015, p. 31)

Key Assumptions:

- Individualistic assignment: Limits the dependence of a particular unit's assignment probability on the values of the covariates and potential outcomes for other units.
- Probabilistic assignment: Requires the assignment mechanism to imply a non-zero probability for each treatment value, for every unit.
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2 Power
3 Problems with $p$-Values
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Assumptions: Be Careful When Defining Treatment

1) There is only one version of the treatment, not \( T_1, T_2, \ldots \)
- Drug trial
- Private Schooling
- Practical Advice: are there hidden versions of your treatment? (suggests different interpretations)

2) Potential outcomes depend only on my treatment status (\( Y(1) \), not \( Y(1,0,0,0,1,0, \ldots ,0,1) \) or \( Y(T) \))
- Survey experiment
- AIDS drug trials
- Practical Advice: design study to avoid spillovers and contamination (unless question of interest, see Nickerson (2008) and Gerber, Green, and Larimer (2008))

Together: (1) and (2) constitute: SUTVA: Stable Unit Treatment Value Assumption
Also sometimes referred to as the "No Interference" assumption.
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The Trouble with Interference

Let $D = \{D_i, D_j\}$ be a vector of treatment assignments for two units $i$ (me) and $j$ (you).

How many elements in $D$?

$D = \{(D_i = 0, D_j = 0), (D_i = 1, D_j = 0), (D_i = 0, D_j = 1), (D_i = 1, D_j = 1)\}$

How many potential outcomes for unit $i$?

$Y_{1i}(D) = \{Y_{1i}(1, 1), Y_{1i}(1, 0), Y_{0i}(0, 1), Y_{0i}(0, 0)\}$
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How many potential outcomes for unit $i$?

$$Y_{1i}(\mathbf{D}) = \begin{cases} Y_{1i}(1, 1) \\ Y_{1i}(1, 0) \end{cases} \quad Y_{0i}(\mathbf{D}) = \begin{cases} Y_{0i}(0, 1) \\ Y_{0i}(0, 0) \end{cases}$$
Potential Outcomes with Interference

How many causal effects for unit \( i \)?

\[
\tau_i(D) = \begin{cases} 
Y_{1i}(1,1) - Y_{0i}(0,0) \\
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\end{cases}
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How many potential outcomes are observed for unit \( i \)?

Since we only observe one of the four potential outcomes, the missing data problem for causal inference is even more severe.
Potential Outcomes with Interference

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The No Interference assumption states that unit $i$'s potential outcomes depend on $D_i$, not $D$:

$Y_{1i}(1, 1) = Y_{1i}(1, 0)$ and $Y_{0i}(0, 1) = Y_{0i}(0, 0)$

This assumption furthermore allows us to define the effect for unit $i$ as $\tau_i = Y_{1i} - Y_{0i}$.

No interference is an example of an exclusion restriction. We rely on outside information to rule out the possibility of certain causal effects (e.g., you taking the treatment has no effect on my potential outcomes).
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Some Examples of Interference:
- Contagion
- Displacement
- Communication
- Deterrence

Causal inference in the presence of interference between subjects is an area of active research. Specially tailored experimental designs have been developed to study these interactions, e.g. Miguel and Kremer (2004) and Sinclair, McConnell, and Green (2012).
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ATE and Other Estimands
What Gets to Be a Cause?

We can imagine a world where individual is assigned to treatment and control conditions.

What is the Hypothetical Experiment?

Problem: Immutable (or difficult to change) characteristics
- Effect of gender on promotion
- Effect of race on income

Consider causal effect of gender on promotion:
- Do we mean gender reassignment surgery?
- Do we mean randomly assigning at birth? (a lot of other stuff different)
- One idea: manipulate perceptions–women evaluated differently on paper

No Causation Without Manipulation
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Stewart (Princeton)

Week 8: Regression in the Social Sciences

November 7 and 9, 2016
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Caveats and Implications

- Does not dismiss claims of discrimination on immutable characteristics as legitimate
- Pervasive effects of racism/sexism in society
- Suggests: we need a different empirical strategy to evaluate claims
- What facet of institutionalized racism (or its consequences) causes racial disparities?
  
  - Correlation problem (1):
    - Regression models can estimate coefficients for immutable characteristics
    - But are necessarily imprecise: what do scholars have in mind in models?

  - Design Principle:
    - Pretend you're God designing experiment
    - If that experiment does not exist, be concerned
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Back to the Neyman Urn Model
Move the goal posts: Focus on estimating Average Treatment Effect (ATE)

Suppose we have $N$ observations in population ($i = 1, \ldots, N$)

$$\text{ATE} = \frac{1}{N} \sum_{i=1}^{N} (Y_i(1) - Y_i(0)) = \mathbb{E}[Y(1) - Y(0)]$$

Average over population!!!

- Population parameter
- It is fixed and unchanging
Average Treatment Effects

Move the goal posts:
Average Treatment Effects

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Focus on estimating Average Treatment Effect (ATE)
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\[= E[Y(1) - Y(0)] \text{ Average over population!!!} \]
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= E[Y(1) - Y(0)] \text{ Average over population}!!!
\]

- Population parameter
- It is fixed and unchanging
Estimating ATE under Random Assignment

Estimator for ATE:

\[ \hat{ATE} = \frac{1}{N} \sum_{i=1}^{N} [Y_i(1)T_i - Y_i(0)(1-T_i)] \]

\[ = E[Y(1)|T=1] - E[Y(0)|T=0] \]
Estimating ATE under Random Assignment

Estimator for ATE:
Estimating ATE under Random Assignment

Estimator for ATE:

\[ \widehat{ATE} = \text{Average (Treated Units)} - \text{Average (Control Units)} \]
Estimating ATE under Random Assignment

Estimator for ATE:

\[ \hat{ATE} = \text{Average (Treated Units)} - \text{Average (Control Units)} \]

\[ = \frac{\sum_{i=1}^{N} Y_i(1) T_i}{\sum_{i=1}^{N} T_i} - \frac{\sum_{i=1}^{N} Y_i(0)(1 - T_i)}{\sum_{i=1}^{N} (1 - T_i)} \]
Estimating ATE under Random Assignment

Estimator for ATE:

\[ \hat{\text{ATE}} = \text{Average (Treated Units)} - \text{Average (Control Units)} \]

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\[ = \sum_{i=1}^{N} \left[ \frac{Y_i(1) T_i}{n_t} - \frac{Y_i(0)(1 - T_i)}{n_c} \right] \]
Estimating ATE under Random Assignment

Estimator for ATE:

\[
\hat{\text{ATE}} = \text{Average (Treated Units)} - \text{Average (Control Units)}
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\[
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\[
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\]

\[
= E[Y(1)|T = 1] - E[Y(0)|T = 0]
\]
Estimands

Because $\tau_i$ are unobservable, we shift what we are interested to:

**Definition (Average Treatment Effect (ATE))**

$$\tau_{ATE} = \text{Average of all treatment potential outcomes} - \text{Average of all control potential outcomes}$$

or

$$\tau_{ATE} = \frac{\sum_i^N Y_{1i}}{N} - \frac{\sum_i^N Y_{0i}}{N}$$

or

$$\tau_{ATE} = E[Y_{1i} - Y_{0i}]$$

or

$$\tau_{ATE} = E[\tau_i]$$
Other Estimands

Definition (Average treatment effect on the treated (ATT))

\[ \tau_{ATT} = E[Y_{1i} - Y_{0i}|D_i = 1] \]
Other Estimands

Definition (Average treatment effect on the treated (ATT))

\[ \tau_{ATT} = E[Y_{1i} - Y_{0i} | D_i = 1] \]

Definition (Average treatment effect on the controls (ATC))

\[ \tau_{ATC} = E[Y_{1i} - Y_{0i} | D_i = 0] \]
Other Estimands

Definition (Average treatment effect on the treated (ATT))

\[ \tau_{ATT} = E[Y_{1i} - Y_{0i}|D_i = 1] \]

Definition (Average treatment effect on the controls (ATC))

\[ \tau_{ATC} = E[Y_{1i} - Y_{0i}|D_i = 0] \]

Definition (Average treatment effects for subgroups)

\[ \tau_{ATE(X)} = E[Y_{1i} - Y_{0i}|X_i = x] \]

or

\[ \tau_{ATT(X)} = E[Y_{1i} - Y_{0i}|D_i = 1, X_i = x] \]
Imagine a study population with 4 units:

<table>
<thead>
<tr>
<th>i</th>
<th>$D_i$</th>
<th>$Y_{1i}$</th>
<th>$Y_{0i}$</th>
<th>$\tau_i$</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1</td>
<td>10</td>
<td>4</td>
<td>6</td>
</tr>
<tr>
<td>2</td>
<td>1</td>
<td>1</td>
<td>2</td>
<td>-1</td>
</tr>
<tr>
<td>3</td>
<td>0</td>
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What is the ATE?

$$E\left[Y_{1i} - Y_{0i}\right] = \frac{1}{4} \times (6 + -1 + 0 + 3) = 2$$

Note: Average effect is positive, but $\tau_i$ are negative for some units!
Average Treatment Effect

Imagine a study population with 4 units:

<table>
<thead>
<tr>
<th>i</th>
<th>$D_i$</th>
<th>$Y_{1i}$</th>
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<th>$\tau_i$</th>
</tr>
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<tbody>
<tr>
<td>1</td>
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What is the ATT and ATC?
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What is the ATT and ATC?

\[
E[Y_{1i} - Y_{0i}|D_i = 1] = 1/2 \times (6 + -1) = 2.5
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Naive Comparison: Difference in Means

Comparisons between observed outcomes of treated and control units can often be misleading. Units which select treatment may not be like units which select control. That is, selection into treatment is often associated with the potential outcomes. This means we have violated the assumption of unconfoundness ($Y(1), Y(0) \perp D$).
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Selection Bias

Example: Church Attendance and Political Participation
Church goers likely to differ from non-Church goers on a range of background characteristics (e.g. civic duty). Given these differences, turnout for churchgoers could be higher than for non-churchgoers even if church had zero mobilizing effect.

Example: Gender Quotas and Redistribution Towards Women
Countries with gender quotas are likely countries where women are politically mobilized. Given this difference, policies targeted towards women would be more common in quota countries even if these countries had not adopted quotas.
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Most statistical models of causal inference attain identification of treatment effects by restricting the assignment mechanism in some way.
No causation without manipulation?
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Always ask:
what is the experiment I would run if I had infinite resources and power?
Causal Inference Workflow

- Data
- Estimator
- Estimate

Identification Strategy

Quantity of Interest

Ideal Experiment

Causal Relationship
Summing Up: Neyman-Rubin causal model

Useful for studying the "effects of causes", less so for the "causes of effects".

No assumption of homogeneity, allows for causal effects to vary unit by unit.

No single "causal effect", thus the need to be precise about the target estimand.

Distinguishes between observed outcomes and potential outcomes.

Causal inference is a missing data problem: we typically make assumptions about the assignment mechanism to go from descriptive inference to causal inference.

Stewart (Princeton)
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Summary: Observational Studies and Causal Inference

Experimental studies:

- Treatment under control of analyst
- Units (people, countries) control their treatment status
- Selection: treatment and control groups differ systematically

\[ E[Y(1) | T = 1] \neq E[Y(1) | T = 0], \]
\[ E[Y(0) | T = 0] \neq E[Y(0) | T = 1] \]

- Observables: things we can see, measure, and use in our study
- Unobservables: not observables (big problem)
- Naive difference in means will be biased
- Many, many, potential strategies for limiting bias
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Can regression be also used for causal inference?

To interpret $\beta$ as a causal effect of $X$ on $Y$, we need very specific and often unrealistic assumptions:

1. $E[Y|X]$ is correctly specified as a linear function (linearity)
2. There are no other variables that affect both $X$ and $Y$ (exogeneity)

1. can be relaxed by:
   ⋆ Using a flexible nonlinear or nonparametric method
   ⋆ "Preprocessing" data to make analysis robust to misspecification

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We will discuss more in a few weeks.

For now while doing diagnostics, it is safest to treat $\beta$ as a purely descriptive/predictive quantity.
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Summary: Regression as a Causal Model

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Graphical Models

A general framework for representing causal relationships based on directed acyclic graphs (DAG). The work we discuss here comes out of developments by Judea Pearl and others. Particularly useful for thinking through issues of identification. Provides a graphical representation of the models and a set of rules (do-calculus) for identifying the causal effect. Nice software that takes the graph and returns an identification strategy: DAGitty at http://dagitty.net.
Graphical Models

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Components of a DAG

- nodes → variables
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- absence of nodes → **no common causes** of any pair of variables

![Diagram showing components of a DAG with nodes U, X, T, Y, Z, M and arrows indicating relationships and orientations.](image-url)
Components of a DAG

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  (Unobserved typically called U or V)
- **(Directed) Arrows** → **Causal Effects**
- Absence of nodes → **No Common Causes** of any pair of variables
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Positioning conveys no mathematical meaning but often is oriented left-to-right with causal ordering for readability.

Dashed lines are used in context-dependent ways. All relationships are non-parametric.
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Relationships in a DAG

- Parents (Children): directly causing (caused by) a node

\[
\begin{align*}
U & \rightarrow X \\
X & \rightarrow T \\
T & \rightarrow Y \\
Z & \rightarrow T \\
Z & \rightarrow M \\
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- **We will talk in depth about two types of relationships:** confounders and colliders.
- $X$ is a confounder (or common cause)
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- Even without a **causal** effect or directed edge between $T$ and $Y$ they will have a **marginal** associational relationship
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*Conditional* on $X$, $T$ and $Y$ are unrelated in this graph.
Confounders

- $X$ is a confounder (or common cause)
- Even without a causal effect or directed edge between $T$ and $Y$ they will have a marginal associational relationship
- **Conditional** on $X$, $T$ and $Y$ are unrelated in this graph.
- We can think of conditioning on a confounder as blocking the flow of association.
Colliders

- $X$ is now a collider because two arrows point into it.
Colliders

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- In this scenario $T$ and $Y$ are **not marginally associated**.
Colliders

- $X$ is now a collider because two arrows point into it.
- In this scenario $T$ and $Y$ are not marginally associated.
- If we control for $X$ they become associated and create a connection between $T$ and $Y$. 
Colliders are scary because you can induce dependence.

Endogenous Selection Bias: The Problem of Conditioning on a Collider Variable

Felix Elwert\textsuperscript{1} and Christopher Winship\textsuperscript{2}

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**Keywords**
causality, directed acyclic graphs, identification, confounding, selection

**Abstract**
Endogenous selection bias is a central problem for causal inference. Recognizing the problem, however, can be difficult in practice. This article introduces a purely graphical way of characterizing endogenous selection bias and of understanding its consequences (Hernán et al. 2004). We use causal graphs (direct acyclic graphs, or DAGs) to highlight that endogenous selection bias stems from conditioning (e.g., controlling, stratifying, or selecting) on a so-called collider variable, i.e., a variable that is itself caused by two other variables, one that is (or is associated with) the treatment and another that is (or is associated with) the outcome. Endogenous selection bias can result from direct conditioning on the outcome variable, a post-outcome variable, a post-treatment variable, and even a pre-treatment variable. We highlight the difference between endogenous selection bias, common-cause confounding, and overcontrol bias and discuss numerous examples from social stratification, cultural sociology, social network analysis, political sociology, social demography, and the sociology of education.
From Confounders to Back-Door Paths

Identify causal effect of $T$ on $Y$ by conditioning on $X$, $Z$, or $X$ and $Z$.

We can formalize this logic with the idea of a back-door path.

A back-door path is "a path between any causally ordered sequence of two variables that begins with a directed edge that points to the first variable." (Morgan and Winship 2013)

Two paths from $T$ to $Y$ here:

1. $T \rightarrow Y$ (directed or causal path)
2. $T \leftarrow X \rightarrow Z \rightarrow Y$ (back-door path)

Observed marginal association between $T$ and $Y$ is a composite of these two paths and thus does not identify the causal effect of $T$ on $Y$.

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We want to block the back-door path to leave only the causal effect
Colliders and Back-Door Paths

- $Z$ is a collider and it lies along a back-door path from $T$ to $Y$

[Diagram of a causal graph with nodes U, V, Z, T, Y, and arrows indicating causal relationships]
Colliders and Back-Door Paths

- \( Z \) is a collider and it lies along a back-door path from \( T \) to \( Y \)
- Conditioning on a collider on a back-door path does not help and in fact causes new associations

![Diagram showing the relationships between variables U, V, Z, T, and Y.](image)
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Here we are fine unless we condition on $Z$ which opens a path $T \leftarrow V \leftrightarrow U \rightarrow Y$ (this particular case is called $M$-bias).
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- Conditioning on a collider on a back-door path does not help and in fact causes new associations.
- Here we are fine unless we condition on $Z$ which opens a path $T \leftarrow V \leftrightarrow U \rightarrow Y$ (this particular case is called $M$-bias).
- So how do we know which back-door paths to block?
Graphs provide us a way to think about conditional independence statements. Consider disjoint subsets of the vertices $A$, $B$ and $C$. A path $p$ is said to be blocked by a set of vertices $C$ if and only if at least one of the following conditions holds:

1. $p$ contains a chain structure $a \rightarrow c \rightarrow b$ or a fork structure $a \leftarrow c \rightarrow b$ where the node $c$ is in the set $C$.
2. $p$ contains a collider structure $a \rightarrow y \leftarrow b$ where neither $y$ nor its descendents are in $C$.

If $A$ is not $D$-separated from $B$ by $C$, we say that $A$ is $D$-connected to $B$ by $C$. 
Graphs provide us a way to think about conditional independence statements. Consider disjoint subsets of the vertices $A$, $B$ and $C$.

- $A$ is **$D$-separated** from $B$ by $C$ if and only if $C$ blocks every path from a vertex in $A$ to a vertex in $B$.
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- Generally we want to know if we can nonparametrically identify the average effect of $T$ on $Y$ given a set of possible conditioning variables $X$.
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- See also Frontdoor Criterion in the social sciences in work by Glynn and Kashin
Thoughts on DAGs and Potential Outcomes

Two very different languages for talking about and thinking about causal inferences. Potential outcomes is very focused on thinking about the treatment assignment mechanism. Potential outcomes is also less of a “foreign language” for most statisticians, but in my experience lumps together a lot of identification assumptions in opaque ignorability conditions.

Graphical Models with DAGs are very visually appealing but the operations on the graph can be challenging. DAGs very helpful for thinking through identification and the entire causal process.

Note that both are about non-parametric identification and not estimation. This is good and bad.

▶ Good: provides a very general framework that applies in non-linear scenarios and interactions
▶ Bad: identification results for identification only holds when variable is completely controlled for (which may be difficult!)
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Next “Week” (Three Classes)

Diagnostics

Unusual and Influential Data → Robust Estimation (Day 1)

Nonlinearity → Generalized Additive Models (Day 2)

Unusual Errors → Sandwich Standard Errors/Block Bootstrap (Day 3)

Reading:

▶ Fox Chapters 11-13

▶ Optional: Fox Chapter 19 Robust Regression


▶ Optional: Aronow and Miller Chapters 4.2-4.4 (Inference, Clustering, Nonlinearity)

▶ Optional: Angrist and Pishke Chapter 8 (Nonstandard Standard Error Issues)
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Fun with a Bundle of Sticks

No Causation Without Manipulation

One of the difficulties that students have with causal inference is the need for manipulation or an ideal experiment. In many areas, the key variables are immutable such as race or gender. Sen and Wasow argue that we can improve our empirical work on this by seeing race/ethnicity as a composite variable or ‘a bundle of sticks’ which can be manipulated separately.
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The Trouble with Race As Treatment

There are three problems with race as a treatment in the causal inference sense:

1. Race cannot be manipulated. Without the capacity to manipulate the question is arguably ill-posed and the estimand is unidentified.
2. Everything else is post-treatment. Everything else comes after race which is perhaps unsatisfying. This also presumes we are only interested in the total effect.
3. Race is unstable. There is substantial variance across treatments which is a SUTVA violation.
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There are three problems with race as a treatment in the causal inference sense:

1. Race cannot be manipulated
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3. Race is unstable
   - there is substantial variance across treatments which is a SUTVA violation
The Bundle of Sticks

- Race
  - Dialect
  - Genes
  - Region of ancestry
  - Religion
  - Wealth
  - Diet
  - Social status
  - Norms
  - Class
  - Power relations

Mutability: More to Less

- Name
- Neighborhood
- Dialect
- Facial features
- Genes
Design 1: Exposure Studies

- **Approach**
  
  a) “one or more elements of race is identified as a relevant cue”
  
  b) “subjects are treated by exposure to the racial cue”
  
  c) “unit of analysis is the individual or institution being exposed”
  
  Examples
  
  ▶ Psychology (Steele 1997 on stereotype threat)
  
  ▶ Audit/Correspondence Studies (Pager 2003, Bertrand and Mullainathan 2004)
  
  ▶ Survey Experiments with Racial Cues (Mendelberg 2001)
  
  ▶ Field Experiments with Racial Cues (Green 2004, Enos 2011)
  
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Design 2: Within-Group Studies

- Approach: identify variation within the racial group along constitutive element.

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- Example: Sharkey (2010) exploiting temporal variation in local homicides in Chicago to identify a significant neighborhood effect of proximity to violence on cognitive performance of African-American children
We can study race with causal inference, it just takes very careful design.

### Table 2  Overview of exposure and within-group research designs

<table>
<thead>
<tr>
<th>Exposure</th>
<th>Within-Group</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unit</td>
<td>Members of a particular group</td>
</tr>
<tr>
<td>Racial cue or signal (e.g., include distinctively ethnic names on a resume)</td>
<td>Constitutive element of the composite of race (e.g., address anxiety about social belonging in college)</td>
</tr>
<tr>
<td>One “stick” is a proxy for the bundle (e.g., in a phone call with a landlord, dialect signals many traits associated with race)</td>
<td>One “stick” explains part of the bundle (e.g., Middle Passage might partly explain high rates of hypertension among African-Americans)</td>
</tr>
<tr>
<td>Correspondence and audit studies, Implicit Association Tests</td>
<td>Experimental manipulation of a constitutive psychological dimension of race Within-race matching</td>
</tr>
</tbody>
</table>