# Week 11: Causality with Unmeasured Confounding


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[1] These slides are heavily influenced by Matt Blackwell, Adam Glynn and Jens Hainmueller.

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

- Last Week
  - selection on observables and measured confounding
- This Week
  - natural experiments
  - instrumental variables
  - regression discontinuity
- The Following Week
  - repeated observations and wrap up
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causal inference

# Unmeasured Confounding

- Last week we considered cases of measured confounding

$$\begin{tikzcd} & X \arrow[dl] \arrow[dr] & \\ T \arrow[rr] & & Y \end{tikzcd}$$

- In this case we block the backdoor path $T \leftarrow X \rightarrow Y$ by conditioning on $X$.
- What happens in the general case where $X$ is unobserved?
- Under selection on unobservables we are going to need a different approach. We will talk about several over the next two weeks.
- There is No Free Lunch $\rightsquigarrow$ we can't get something for nothing, we will need new variables, new assumptions and/or new approaches.
- Goal: give you a feel for what is possible, but note that you will need to do work beyond class if you want to use one of these techniques. Think "on ramp" more than "comprehensive tutorial."

# Approaches to Unmeasured Confounding

- Natural Experiments (this video)
- Interrupted Time-Series (this video)
- Instrumental Variables (next two videos)
- Regression Discontinuity (last video)
- Bounding
- Sensitivity Analysis
- Front Door Adjustment

# Natural Experiments

- Broadly speaking an "experiment" where the treatment is randomized but the randomization was not controlled by the researcher.
- Leverages an exogenous (external to the system) event to measure the effect of an otherwise endogenous (internal to the system) phenomenon.
- Trickier to analyze than regular experiments
  - we ought to be suspicious of randomization we don't control
  - nature may not choose exactly the treatment we want
  - not immediately obvious which groups are comparable
  - valid comparison may not estimate the causal effect of interest
- When available, a useful way to capitalize on randomness in the world to make causal inferences.
- See Dunning (2012) *Natural Experiments in the Social Sciences*

# Caution on terminology

*It is worth noting that the label "natural experiment" is perhaps unfortunate. As we shall see, the social and political forces that give rise to as-if random assignment of interventions are not generally "natural" in the ordinary sense of that term. Second, natural experiments are observational studies, not true experiments, again, because they lack an experimental manipulation. In sum, natural experiments are neither natural nor experiments.*

—Dunning (2012) pg 16

# Natural Experiment Examples (True Randomization)

| Randomness | Focus | Citation |
|---|---|---|
| Vietnam draft | labor market | Angrist 1990 |
| randomized quotas | female leadership in Indian village council presidencies | Chattopadhyay & Duflo 2004 |
| randomized term lengths | tenure in office on legislative performance | Dal Bo & Rossi 2010 |
| lottery | effect of winnings on political attitudes | Doherty, Green & Gerber 2006 |
| randomized ballot order | ballot order effects in CA | Ho & Imai 2008 |

# Natural Experiment Examples (As If Randomization)

| Randomness | Focus | Citation |
|---|---|---|
| child abduction by LRA | child soldering affecting political participation | Blattman 2008 |
| election monitor assignment | international election monitoring on fraud | Hyde 2007 |
| random shelling by drunk soldiers | indiscriminate violence on rebellion | Lyall 2009 |
| hurricane | study of friendship formulation | Phan and Airoldi 2015 |
| 2006 Israel-Hezbollah war | stress on unborn babies | Torche and Shwed 2015 |
| Snowden revelations | reading behavior on wikipedia | Penney 2016 |
| terrorist attacks | perception of immigrants | Legewie 2013 |

# Questions to Ask Yourself

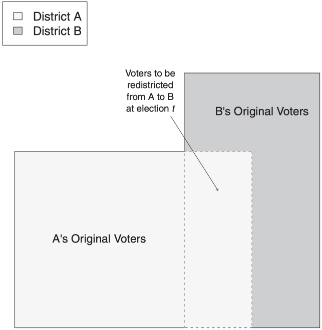
From Sekhon and Titiunik (2012) "When Natural Experiments Are Neither Natural nor Experiments" *American Political Science Review*

1. "is the proposed treatment-control comparison guaranteed to be valid by the assumed randomization?"
2. "if not, what is the comparison that is guaranteed by the randomization, and how does this comparison relate to the comparison the researcher wishes to make?"

# Example: Redistricting

Sekhon and Titiunik 2012 discussion of Ansolabehere et al. 2000

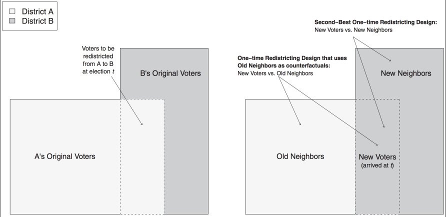
- problem: difficult to estimate incumbency advantage.
- proposed solution: redistricting as a natural experiment
- two types of variation: temporal (voting before and after) and cross-sectional (some voters move)
- key claim: if district is randomly redrawn, we can attribute voting differences between new voters in a district and their new neighbors to a lack of incumbency advantage
- idea is that two groups have same incumbent, same challenger, same campaign environment, but different histories with incumbent




(a) Before One-time Redistricting

# Example: Redistricting

FIGURE 1. Illustration of Redistricting Research Designs




(a) Before One-time Redistricting

(b) After One-time Redistricting

1) Is the comparison of B's old voters and B's new voters guaranteed to be valid if we assume that voters are redistricted randomly?
   - No! only voters in A were subject to randomization
   - Natural experiment creates three distinct groups: new voters, new neighbors, old neighbors and not all comparisons are valid.
2) What comparison is guaranteed to be valid if redistricting is done at random?
   - random redistricting guarantees that old neighbors and new voters are comparable.
   - need to find a new design (see Sekhon and Titiunik 2012 for more)

# Pitfalls

- We still really need theory to guide our thinking
- Understanding the assignment and causal process is extremely important (was this really random? is the treatment really what we care about?)
- The result only applies to a limited population — is it significant?
- Be sure to verify that your "as-if-random" assignment is really random (e.g. placebo tests, balance tests)
- If possible, use sensitivity tests to evaluate susceptibility to unobserved confounding (e.g. Cinelli and Hazlett 2020)
- Convincingly analyzing a natural experiment takes at least as much careful thought not less!

# Reasons to Be Excited

- Now that you know what to look for you may see more natural experiments out there
- Exogenous randomization can help us make credible causal inferences in places where we never could have run an experiment
- It is often pretty easy to communicate these kinds of methods to non-experts
- Salganik (2018) argues that with always-on digital data collection we will be in better shape moving forward to leverage natural experiments as the opportunities arise.

# Interrupted Time Series

- A simple construction often used with natural experiment is the Interrupted Time Series (ITS)
- ITS designs convey the basic intuition that when an event abruptly occurs, we can compare results immediately before and immediately afterwards.
- We can write this as a model indexing the date with $d$:

$$Y_d = f(d) + T_d\beta + \epsilon_d$$

- The key identifying assumption is that the observed values of $y_d$ before the treatment status switches at $d^*$ can be used to specify $f(d)$ for the rest of the series used.

# Interrupted Time Series Example

*American Political Science Review* (2018) 112, 3, 621–636
doi:10.1017/S0003055418000084 

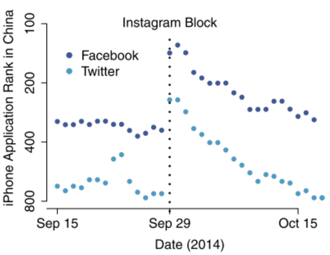
## How Sudden Censorship Can Increase Access to Information

WILLIAM R. HOBBS *Northeastern University*
MARGARET E. ROBERTS *University of California, San Diego*

*Conventional wisdom assumes that increased censorship will strictly decrease access to information. We delineate circumstances when increases in censorship expand access to information for a substantial subset of the population. When governments suddenly impose censorship on previously uncensored information, citizens accustomed to acquiring this information will be incentivized to learn methods of censorship evasion. These evasion tools provide continued access to the newly blocked information—and also extend users' ability to access information that has long been censored. We illustrate this phenomenon using millions of individual-level actions of social media users in China before and after the block of Instagram. We show that the block inspired millions of Chinese users to acquire virtual private networks, and that these users subsequently joined censored websites like Twitter and Facebook. Despite initially being apolitical, these new users began browsing blocked political pages on Wikipedia, following Chinese political activists on Twitter, and discussing highly politicized topics such as opposition protests in Hong Kong.*

# Interrupted Time Series Example

**FIGURE 3. Left: The Instagram block's effect on the rank of Facebook and Twitter on iPhones from mainland China, from AppAnnie.com. Right: Comparison of tweets per day from Mainland China and Hong Kong before and after the Instagram block.**




The left panel of this figure shows the change in download ranks for Facebook and Twitter before and after Instagram was blocked. The right panel of this figure shows that the Chinese Twitter users in our sample increased 30% the same day that we observe a spike in Instagram mentions and several days after the beginning of the Hong Kong protests. This increase only occurred in China and not in Hong Kong. The lines in this panel were fit using a smoothing spline.

# We Covered

- Natural Experiments
- Interrupted Time Series

Want to learn more about natural experiments?

- Dunning, Thad. (2012), *Natural Experiments in the Social Sciences: A Design-Based Approach*.
- Titiunik, Rocío. (2020) "Natural Experiments" in *Advances in Experimental Political Science*.

Next Time: Instrumental Variables Part 1 (of 2)

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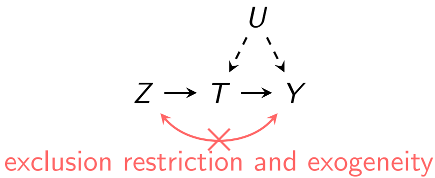
# Motivating Instrumental Variables

- Last week we saw how to identify and estimate effects under measured confounding. We just saw what happened when nature happens to randomize the treatment we want.
- Are we doomed if neither happens?
- Instrumental variables (IV) allow us to exploit an exogenous source of variation that drives the treatment but does not otherwise affect the outcome.
- If we have an instrument, we can deal with unmeasured confounding in the treatment-outcome relationship.
- It is going to turn out that the same construction will let us deal with non-compliance in experiments.

Angrist (1990): Draft lottery as an instrument to study the relationship between military service and income

https://en.wikipedia.org/wiki/Draft_lottery_(1969)#/media/File:1969_draft_lottery_photo.jpg

# Graphical Model




- Notation: $Z$ is the instrument, $T$ is the treatment, and $U$ is the unmeasured confounder
- (Approximate) assumptions (3 of 4)
  1) instrument-treatment and instrument-outcome don't share unmeasured common causes (exogeneity of the instrument)
  2) no direct or indirect effect of the instrument on the outcome not through the treatment (exclusion restriction)
  3) $Z$ affects $T$ (first stage relationship)
- We will need one more later which we will come back to.

# Some Examples

- Angrist (1990): Draft lottery as an IV for military service (income as outcome)
- Acemoglu et al (2001): settler mortality as an IV for institutional quality (GDP/capita as outcome)
- Miguel, Satayanath & Sergenti (2004): lagged rainfall as IV for GDP per capita effect (outcome is civil war onset).
- Kern & Hainmueller (2009): having West German TV reception in East Berlin as an instrument for West German TV watching (outcome is support for the East German regime)
- Nunn & Wantchekon (2011): historical distance of ethnic group to the coast as a instrument for the slave raiding of that ethnic group (outcome are trust attitudes today)
- Acharya, Blackwell and Sen (2017): cotton suitability as IV for proportion slaves in 1860 (outcome is white attitudes today)

# Core Idea

The world has randomized something (the instrument) just maybe not the thing you want (the treatment).

Subject to four assumptions you may be able to get (approximately) what you want anyway.

# Non-Compliance Motivation for Instrumental Variables

## Problem

- *Often we cannot force subjects to take specific treatments*
- *Units choosing to take the treatment may differ in unobserved characteristics from units that refrain from doing so*

Example: Non-Compliance in JTPA Experiment

| | Not Enrolled in Training | Enrolled in Training | Total |
|---|---|---|---|
| Assigned to Control | 3,663 | 54 | 3,717 |
| Assigned to Training | 2,683 | 4,804 | 7,487 |
| Total | 6,346 | 4,858 | 11,204 |

# Two Views on Instrumental Variables

1. Traditional Econometric Framework
   - strong assumptions
     - constant effects
     - linearity in case of a continuous treatment
   - Identifies the average treatment effect
2. Potential Outcome Model of IV
   - Weaker assumptions
     - monotonicity
     - allows heterogeneous treatment effect
   - Only identifies Local Average Treatment Effect (LATE)

# The Problem (informal)

Suppose want to know the average effect of T on Y.

Two Problems:

1. We may not be able to measure all variables that affect both T and Y.
2. We may not be able to measure T without error.

Both of these conditions will induce bias in our effect estimates.

# The Problem (formal)

Assume a linear structural equation model but suppose that the classical "exogeneity" condition ($E[U_i|T_i] = 0$) does not hold:

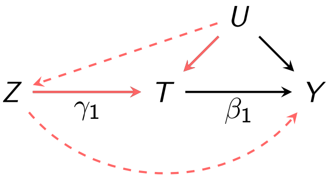
$$Y_i = \beta_0 + \beta_1 T_i + \underbrace{\beta_2 U_i + \eta_i}_{u_Y}$$

This can happen for a number of reasons including:

- omitted variables (which is a common cause of $T$ and $Y$)
- measurement error in $T$
- included variables (post-treatment or M-structures)
- simultaneous equations (endogenous feedback loops)

We will typically formulate the problem as resulting from omitted confounding.

# A Potential Solution: Instrumental Variables (IV)




$$Y_i = \beta_0 + \beta_1 T_i + u_Y$$
$$E[U_i | T_i] \neq 0$$
$$T_i = \gamma_0 + \gamma_1 Z_i + u_T$$
$$E[U_i | Z_i] = 0$$
$$Cov[T_i, Z_i] \neq 0$$

# Commonly Used Instrumental Variables

- Assigned status in randomized trials with noncompliance
  - assigned vs. enrolled in job training
  - "received" versus "read" the mailing in the social pressure experiment
- Rainfall, earthquakes, ...
- ...

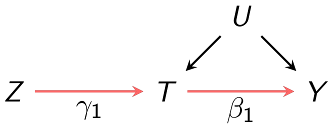
# The IV Estimator

With our assumed model,

$$Y_i = \beta_0 + \beta_1 T_i + u_Y$$
$$T_i = \gamma_0 + \gamma_1 Z_i + u_T$$

Assuming the model is true,

- regressing $T$ on $Z$ consistently estimates $\gamma_1$
- regressing $Y$ on $Z$ consistently estimates $\gamma_1 \cdot \beta_1$
- $\frac{\widehat{\gamma_1 \cdot \beta_1}}{\widehat{\gamma_1}}$ consistently estimates $\frac{\gamma_1 \cdot \beta_1}{\gamma_1} = \beta_1$



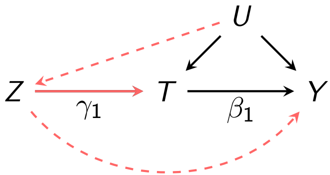
# Preview: The Problem of Weak Instruments

Notice that the IV technique uses a ratio $\frac{\gamma_1 \cdot \beta_1}{\gamma_1} = \beta_1$.

Dividing by zero (or near zero) makes things blow up.

Therefore, if the instrument is weak ($\gamma_1 \approx 0$), and our estimates of $\gamma_1$ and $\gamma_1 \cdot \beta_1$ are not perfect, we can get inaccurate estimates of $\beta_1$:

- medium sample size $\Rightarrow$ high variance
- small violations of assumptions $\Rightarrow$ large bias

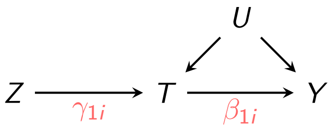


# Preview of Modern Approaches: Relaxing Constant Effects

Suppose we believe that the effects of Z and T are different for different units.

$$Y_i = \beta_{0i} + \beta_{1i} T_i + u_{Yi}$$

$$T_i = \gamma_{0i} + \gamma_{1i} Z_i + u_{Ti}$$



# IV Estimator with Heterogeneous Effects

$$\begin{tikzcd} & & U \arrow[dl] \arrow[dr] & \\ Z \arrow[r, "\gamma_{1i}"'] & T \arrow[rr, "\beta_{1i}"'] & & Y \end{tikzcd}$$

- regressing $X$ on $Z$ now only identifies $\overline{\gamma}_1$
- regressing $Y$ on $Z$ identifies only $\overline{\gamma_1 \cdot \beta_1}$
- $\overline{\gamma_1 \cdot \beta_1} \neq \overline{\gamma_1} \cdot \overline{\beta_1}$
- Therefore the IV estimator does not estimate even the average $\beta_1$ ($\frac{\overline{\gamma_1 \cdot \beta_1}}{\overline{\gamma}_1} \neq \overline{\beta_1}$)

With additional assumptions ($\gamma_{i1} \geq 0$ for all $i$), the IV estimator identifies a weighted average effect of $T$ on $Y$ according to the effects of $Z$ on $T$.

# Why IV?

- True model: $Y = \beta_0 + \beta_1 T + u_Y$
  - $T$ is the treatment variable (e.g. training)
  - $T$ may be endogenous so that $Cov[T, U] \neq 0$

- Recall that the OLS estimator for $\beta_1$ is given by:

$$
\begin{aligned}
\hat{\beta}_{1,OLS} &= \frac{\widehat{Cov}[Y, T]}{\widehat{Var}[T]} = \frac{\widehat{Cov}[\beta_0 + \beta_1 T + u_Y, T]}{\widehat{Var}[T]} \\
&= \frac{\beta_1 \widehat{Var}[T] + \widehat{Cov}[T, u_Y]}{\widehat{Var}[T]} \\
&= \beta_1 + \frac{\widehat{Cov}[T, u_Y]}{\widehat{Var}[T]} \\
E[\hat{\beta}_{1,OLS}] &= \beta_1 + E\left[\frac{\widehat{Cov}[T, u_Y]}{\widehat{Var}[T]}\right]
\end{aligned}
$$

so bias depends on correlation between $u_Y$ and $T$

# Instrumental Variable Estimator Assumptions

Imagine we have two equations:

- Second Stage: $Y = \beta_0 + \beta_1 T + u_Y$
- First Stage: $T = \gamma_0 + \gamma_1 Z + u_T$
  - $Z$ is our instrumental variable (e.g. randomized encouragement)
  - $\gamma_1$ is effect of $Z$ on $T$

Instrumental Variable Assumptions:

1. $\gamma_1 \neq 0$ so $Z$ creates some variation in $T$ (called first stage or relevance)
2. $Z$ is exogenous meaning $Cov[u_T, Z] = 0$ and $Cov[u_Y, Z] = 0$. The latter is an exclusion restriction, it implies that the only reason why $Z$ is correlated with $Y$ is through the correlation between $Z$ and $T$. So $Z$ has no independent effect on $Y$.

# Instrumental Variable Estimator Assumptions

- Second Stage: $Y = \beta_0 + \beta_1 T + u_Y$
- First Stage: $T = \gamma_0 + \gamma_1 Z + u_T$
- IV assumptions: $Cov[u_T, Z] = 0$, $\gamma_1 \neq 0$, and $Cov[u_Y, Z] = 0$

Based on these IV assumptions we can identify three effects:

1. The first stage effect: Effect of $Z$ on $T$.
2. Reduced form or intent-to-treat effect: Effect of $Z$ on $Y$.
3. The instrumental variable treatment effect: Effect of $T$ on $Y$, using only the exogenous variation in $T$ that is induced by $Z$.

# First Stage Effect

- Second Stage: $Y = \beta_0 + \beta_1 T + u_Y$
- First Stage: $T = \gamma_0 + \gamma_1 Z + u_T$
- IV assumptions: $Cov[u_T, Z] = 0$, $\gamma_1 \neq 0$, and $Cov[u_Y, Z] = 0$

First stage effect: $Z$ on $D$

$$
\begin{aligned}
\hat{\gamma}_1 &= \frac{\widehat{Cov}[D, Z]}{\widehat{V}[Z]} = \frac{\widehat{Cov}[\gamma_0 + \gamma_1 Z + u_T, Z]}{\widehat{V}[Z]} \\
\hat{\gamma}_1 &= \frac{\gamma_1 \widehat{Cov}[Z, Z] + \widehat{Cov}[Z, u_T]}{\widehat{V}[Z]} \\
\hat{\gamma}_1 &= \gamma_1 + \frac{\widehat{Cov}[Z, u_T]}{\widehat{V}[Z]} \\
E[\hat{\gamma}_1] &= \gamma_1 + E\left[\frac{\widehat{Cov}[Z, u_T]}{\widehat{V}[Z]}\right] = \gamma_1
\end{aligned}
$$

$\hat{\gamma}_1$ is consistent since $Cov[u_T, Z] = 0$

# First Stage Effect in JTPA

First stage effect: $Z$ on $T$: $\hat{\gamma}_1 = \frac{\widehat{Cov}[T,Z]}{\widehat{V}[Z]}$

R Code

```
> cov(d[,c("earnings","training","assignmt")])
             earnings    training    assignmt
earnings 2.811338e+08 685.5254685 257.0625061
training 6.855255e+02   0.2456123   0.1390407
assignmt 2.570625e+02   0.1390407   0.221713
```

R Code

```
> 0.1390407/0.2217139
[1] 0.6271177
```

# First Stage Effect in JTPA

R Code

```
> summary(lm(training~assignmt,data=d))

Call:
lm(formula = training ~ assignmt, data = d)

Residuals:
     Min       1Q   Median       3Q      Max
-0.64165 -0.01453 -0.01453  0.35835  0.98547

Coefficients:
            Estimate Std. Error t value Pr(>|t|)
(Intercept) 0.014528   0.006529   2.225   0.0261 *
assignmt    0.627118   0.007987  78.522   <2e-16 ***
---
Signif. codes:  0 *** 0.001 ** 0.01 * 0.05 . 0.1  1

Residual standard error: 0.398 on 11202 degrees of freedom
Multiple R-squared: 0.355,        Adjusted R-squared: 0.355
F-statistic:  6166 on 1 and 11202 DF,  p-value: < 2.2e-1
```

# Reduced Form/Intent-to-treat Effect

- Second Stage: $Y = \beta_0 + \beta_1 T + u_Y$
- First Stage: $T = \gamma_0 + \gamma_1 Z + u_T$
- IV assumptions: $Cov[u_T, Z] = 0$, $\gamma_1 \neq 0$, and $Cov[u_Y, Z] = 0$

Reduced Form/Intent-to-treat Effect: $Y$ on $Z$: Plug first into second stage:

$$\begin{aligned} Y &= \beta_0 + \beta_1(\gamma_0 + \gamma_1 Z + u_T) + u_Y \\ Y &= (\beta_0 + \beta_1\gamma_0) + (\beta_1\gamma_1)Z + (\beta_1 u_T + u_Y) \end{aligned}$$

Note that

$$\begin{aligned} \widehat{\beta_1\gamma_1} &= \frac{\widehat{Cov}[Y, Z]}{\widehat{Cov}[Z, Z]} = \frac{\widehat{Cov}[\beta_0 + \beta_1\gamma_0) + (\beta_1\gamma_1)Z + (\beta_1 u_T + u_Y), Z]}{\widehat{Cov}[Z, Z]} \\ E[\widehat{\beta_1\gamma_1}] &= \beta_1\gamma_1 + E\left[\frac{\widehat{Cov}[Z, (\beta_1 u_T + u_Y)]}{\widehat{Cov}[Z, Z]}\right] = \beta_1\gamma_1 \end{aligned}$$

$\widehat{\beta_1\gamma_1}$ is consistent since $Cov[u_T, Z] = 0$ and $Cov[u_Y, Z] = 0$ implies $Cov[Z, (\beta_1 u_T + u_Y)] = 0$

# Reduced Form/Intent-to-treat Effect

R Code

```
> summary(lm(earnings~assignmt,data=d))

Call:
lm(formula = earnings ~ assignmt, data = d)

Residuals:
   Min     1Q Median     3Q    Max
-16200 -13803  -4817   8950 139560

Coefficients:
            Estimate Std. Error t value Pr(>|t|)
(Intercept)  15040.5      274.9  54.716  < 2e-16 ***
assignmt      1159.4      336.3   3.448 0.000567 ***
---
Signif. codes:  0 *** 0.001 ** 0.01 * 0.05 . 0.1   1

Residual standard error: 16760 on 11202 degrees of freedom
Multiple R-squared: 0.00106,        Adjusted R-squared: 0.000971
F-statistic: 11.89 on 1 and 11202 DF,  p-value: 0.000566
```

# Instrumental Variable Effect: Wald Estimator

- Second Stage: $Y = \beta_0 + \beta_1 T + u_Y$
- First Stage: $T = \gamma_0 + \gamma_1 Z + u_T$
- IV assumptions: $Cov[u_T, Z] = 0$, $\gamma_1 \neq 0$, and $Cov[u_Y, Z] = 0$

IV Effect: $T$ on $Y$ using exogenous variation in $T$ that is induced by $Z$. Recall

$$Y = (\beta_0 + \beta_1\gamma_0) + (\beta_1\gamma_1)Z + (\beta_1 u_T + u_Y)$$

Given this, we can identify $\beta_1$:

$$\begin{aligned}
\beta_1 &= \frac{\beta_1\gamma_1}{\gamma_1} = \frac{\text{Effect of Z on Y}}{\text{Effect of Z on T}} = \frac{Cov[Y,Z]/Cov[Z,Z]}{Cov[T,Z]/Cov[Z,Z]} = \frac{Cov[Y,Z]}{Cov[T,Z]} \\
&= \frac{Cov[\beta_0 + \beta_1 T + u_Y, Z]}{Cov[T,Z]} = \frac{\beta_1 Cov[T,Z] + Cov[u_Y,Z]}{Cov[T,Z]} = \beta_1 + \frac{Cov[u_Y,Z]}{Cov[T,Z]} \\
E[\hat{\beta}_1] &= \beta_1 + E\left[\frac{\widehat{Cov}[u_Y,Z]}{\widehat{Cov}[T,Z]}\right]
\end{aligned}$$

$\hat{\beta}_1$ is consistent if $Cov[u_Y, Z] = 0$ but has a bias which decreases with instrument strength.

# Instrumental Variable Effect: Wald Estimator

Instrumental Variable Effect: $\beta_1 = \frac{\text{Effect of Z on Y}}{\text{Effect of Z on T}} = \frac{Cov[Y,Z]}{Cov[T,Z]}$

R Code

```
> cov(d[,c("earnings","training","assignmt")])
             earnings    training    assignmt
earnings 2.811338e+08 685.5254685 257.0625061
training 6.855255e+02   0.2456123   0.1390407
assignmt 2.570625e+02   0.1390407   0.221713
```

R Code

```
> 257.0625061/0.1390407
[1] 1848.829
```

# Instrumental Variable Effect: Two Stage Least Squares

The instrumental variable estimator:

$$\hat{\beta}_1 = \frac{\widehat{\beta_1 \gamma_1}}{\hat{\gamma}_1} = \frac{\widehat{Cov}[Y,Z]}{\widehat{Cov}[T,Z]}$$

is numerically equivalent to the following two step procedure:

1. Fit first stage and obtain fitted values $\hat{T} = \hat{\gamma}_0 + \hat{\gamma}_1 Z$
2. Plug into second stage:

$$\begin{aligned} Y &= \beta_0 + \beta_1 \hat{T} + u_Y \\ Y &= \beta_0 + \beta_1(\hat{\gamma}_0 + \hat{\gamma}_1 Z) + u_Y \\ Y &= (\beta_0 + \beta_1\hat{\gamma}_0) + \beta_1(\hat{\gamma}_1 Z) + u_Y \end{aligned}$$

- $\beta_1$ is solely identified based on variation in $T$ that comes from $Z$
- Point estimates from second regression are equivalent to IV estimator, the standard errors are not quite correct since they ignore the estimation uncertainty in $\hat{\gamma}_0$ and $\hat{\gamma}_1$.

# Instrumental Variable Effect: Two Stage Least Squares

R Code

```
> training_hat <- lm(training~assignmt,data=d)$fitted
> summary(lm(earnings~training_hat,data=d))

Call:
lm(formula = earnings ~ training_hat, data = d)

Residuals:
   Min     1Q Median     3Q    Max
-16200 -13803  -4817   8950 139560

Coefficients:
             Estimate Std. Error t value Pr(>|t|)
(Intercept)   15013.6      281.3  53.375  < 2e-16 ***
training_hat   1848.8      536.2   3.448 0.000567 ***
---
Signif. codes:  0 *** 0.001 ** 0.01 * 0.05 . 0.1  1

Residual standard error: 16760 on 11202 degrees of freedom
Multiple R-squared: 0.00106,        Adjusted R-squared: 0.000971
F-statistic: 11.89 on 1 and 11202 DF,  p-value: 0.0005669
```

# Instrumental Variable Effect: Two Stage Least Squares

R Code

```
> library(AER)
> summary(ivreg(earnings ~ training | assignmt,data = d))
Call:
ivreg(formula = earnings ~ training | assignmt, data = d)
Residuals:
   Min     1Q Median     3Q    Max
-16862 -13716  -4943   8834 140746
Coefficients:
            Estimate Std. Error t value Pr(>|t|)
(Intercept)  15013.6      280.6  53.508  < 2e-16 ***
training      1848.8      534.9   3.457 0.000549 ***
---
Residual standard error: 16720 on 11202 degrees of freedom
Multiple R-Squared: 0.00603,        Adjusted R-squared: 0.005941
Wald test: 11.95 on 1 and 11202 DF,  p-value: 0.0005491
```

# Judging the Credibility of IV Estimates

- The probability limit of the IV estimator is given by:

$$plim\, \beta_{T,IV} = \beta_T + \frac{Cov(Z, u_Y)}{Cov(Z, T)}$$

  so to obtain consistent estimates the instrument $Z$ must:

- Be Relevant: $Cov(Z, T) \neq 0$ (testable)
  - if $Cov(Z, T)$ is small, the instrument is weak.
  - weak instruments increases bias, but estimator remains consistent.
  - bias can be substantial even for very large sample sizes when the instrument is weak.
- Satisfy Exclusion Restriction: $Cov(Z, u_Y) = 0$ (untestable)
  - if $Z$ has an independent effect on $Y$ other than through $T$ we have $Cov(Z, u_Y) \neq 0$
  - if assumption not met estimates are inconsistent
  - small violations can lead to significant large sample bias unless instruments are strong
- Failure of either condition is a problem! But both conditions can be hard to satisfy at the same time. There often is a tradeoff.

# When Should We Believe The Exclusion Restriction?

- Does a randomly assigned instrument $Z$ always satisfy $Cov(Z, u_Y) = 0$?
- No! Encouragement may still have independent effect on outcome other than through the treatment.For example,
  - being draft eligible might encourage people to go to school and that might impact their earnings (Angrist 1990, 330)
  - having a low draft number might encourage an employer to invest less in the employee because of concerns they will be drafted (Wooldridge 2010, 94)
- When designing an encouragement experiment we need to be careful to design encouragements so that they are relevant, but also narrowly constructed to only create variation in treatment intake.
- In observational work, imagining the ideal experiment (and associated compliance problem) can be helpful.
- Requires understanding of the context!

# General Words of Caution

These methods are not a panacea. Even if someone calls them a "natural experiment" that doesn't actually make it like an experiment.

> *"The general lesson is once again the ultimate futility of trying to avoid thinking about how and why things work"*
> *- Angus Deaton (2010)*

> *"[there is a] risk [of] transforming the methodologic dream of avoiding unmeasured confounding into a nightmare of conflicting biased estimates"*
> *- Hernán and Robins (2006)*

# Conclusion

- Core logic of IV: inflate the intention to treat effect by the inverse of the compliance.
- IV works only under very specific circumstances
  (e.g. well designed encouragement design experiments)
- Often, it will be difficult to find instruments that are both relevant (strong enough) and satisfy the exclusion restriction
- Violations of assumptions can lead to large biases and estimation theory is complicated
- So far, we have assumed constant treatment effects which seems unrealistic in most settings. Often an instrument affects only a subpopulation of interest and we have little information about treatment effects for other units that may not be affected by the instrument at all.

Want to learn more about instrumental variables? Here are some things to read:

- Angrist, Imbens, and Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables" *Journal of the American Statistical Association*, `https://www.jstor.org/stable/2291629`
- Sovey and Green. 2011. "Instrumental Variables Estimation in Political Science: A Readers' Guide" *American Journal of Political Science*, `https://doi.org/10.1111/j.1540-5907.2010.00477.x`
- Swanson and Hernan. 2013. "Commentary: How to Report Instrumental Variable Analyses (Suggestions Welcome)" *Epidemiology*, `https://www.jstor.org/stable/23486750`
- Morgan and Winship. 2014. Chapter 9: Instrumental Variable Estimators of Causal Effects. `https://doi.org/10.1017/CBO9781107587991.010`
- Angrist and Pische. Chapter 4: Instrumental Variables in Action: Sometimes You Get What You Need

# We Covered

- IV under constant effects

Next Time: Modern IV with heterogeneous effects

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

- Last Week
  - selection on observables and measured confounding
- This Week
  - natural experiments
  - instrumental variables
  - regression discontinuity
- The Following Week
  - repeated observations and wrap up
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causal inference

# Identification with Traditional Instrumental Variables

- Two equations:
  - $Y = \beta_0 + \beta_1 T + u_Y$ (Second Stage)
  - $T = \gamma_0 + \gamma_1 Z + u_T$ (First Stage)
  - IV assumptions: $Cov[u_T, Z] = 0$, $\gamma_1 \neq 0$, and $Cov[u_Y, Z] = 0$
- Four Assumptions
  1. Exogeneity: $Cov[u_T, Z] = 0$
  2. Exclusion: $Cov[u_Y, Z] = 0$
  3. First Stage Relevance: $\gamma_1 \neq 0$
  4. Homogeneity: $\alpha = Y_{1,i} - Y_{0,i}$ constant for all units $i$.
     Or in the case of a multivalued treatment with $s$ levels we assume $\beta_1 = Y_{s,i} - Y_{s-1,i}$.

Angrist (1990): Draft lottery as an instrument to study the relationship between military service and income

https://en.wikipedia.org/wiki/Draft_lottery_(1969)#/media/File:1969_draft_lottery_photo.jpg

# Instrumental Variables and Potential Outcomes

- Basic idea of IV:
  - $T_i$ not randomized, but $Z_i$ is
  - $Z_i$ only affects $Y_i$ through $T_i$
- $T_i$ now depends on $Z_i \rightsquigarrow$ two potential treatments: $T_i(1) = T_i(z = 1)$ and $T_i(0) = T_i(z = 0)$.
- Outcome can depend on both the treatment and the instrument: $Y_i(t, z)$ is the outcome if unit $i$ had received treatment $T_i = t$ and instrument value $Z_i = z$.

# Potential Outcome Model for Instrumental Variables

## Definition (Instrument)

$Z_i$: Binary instrument for unit $i$.

$$Z_i = \begin{cases} 1 & \text{if unit } i \text{ ``encouraged'' to receive treatment} \\ 0 & \text{if unit } i \text{ ``encouraged'' to receive control} \end{cases}$$

## Definition (Potential Treatments)

$T(z)$ indicates potential treatment status given $Z = z$

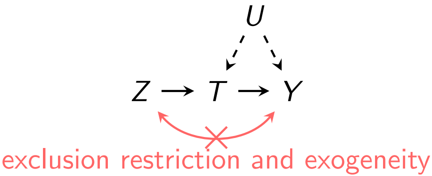
- $T_i(1) = 1$ encouraged to take treatment and takes treatment

## Assumption

*Observed treatments are realized as*

$$T_i = Z_i \cdot T_i(1) + (1 - Z_i) \cdot T_i(0) \text{ so } T_i = \begin{cases} T_i(1) & \text{if } Z_i = 1 \\ T_i(0) & \text{if } Z_i = 0 \end{cases}$$

# Key Assumptions in the Modern Approach




Assumptions:

1. Exogeneity of the Instrument
2. Exclusion Restriction
3. First-stage relationship
4. Monotonicity

You may sometimes see assumptions 1 and 2 collapsed into an assumption called something like "Ignorability of the Instrument". I find it helpful to assess them separately though.

# Assumption 1: Exogeneity of the Instrument

- Essentially we want the instrument to be randomized:

$$[\{Y_i(t,z), \forall t,z\}, T_i(1), T_i(0)] \perp\!\!\!\perp Z_i$$

- We can weaken this to conditional ignorability. But why believe conditional ignorability for the instrument but not the treatment?
- Best instruments are truly randomized.
- This assumption alone gets us the intent-to-treat (ITT) effect:

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] = E[Y_i(T_i(1), 1) - Y_i(T_i(0), 0)]$$

- Sometimes the ITT is interesting in its own right and should probably be reported regardless.

# Assumption 2: Exclusion Restriction

- The instrument has no direct effect on the outcome, once we fix the value of the treatment.

$$Y_i(t,1) = Y_i(t,0) \quad \text{for } t = 0,1$$

- Given this exclusion restriction, we know that the potential outcomes for each treatment status only depend on the treatment, not the instrument:

$$Y_i(1) \equiv Y_i(1,1) = Y_i(1,0)$$
$$Y_i(0) \equiv Y_i(0,1) = Y_i(0,0)$$

- Random assignment of the instrument is not sufficient for exclusion
- NOT A TESTABLE ASSUMPTION

# Assumption 3: First Stage Relationship

- The instrument must have an effect on the treatment.

$$E[T_i(1) - T_i(0)] \neq 0$$

- Implies that
  - $\text{Cov}(T, Z) \neq 0$ (instrument and treatment are linearly related)
  - $0 < P(Z_i = 1) < 1$ (all units have some chance of getting instrument)
  - $P(T(1) = 1) \neq P(T(0) = 1)$ (proportion treated would be different if all received instrument than if all didn't)
- This is testable by regressing $T$ on $Z$ (or making a scatter plot of $T$ and $Z$)
- Note that the finite-sample bias of the IV estimator depends inversely on the strength of the instrument. Thus, for practical sample sizes you need a strong first stage effect.

# Assumption 4: Monotonicity

- To allow for heterogenous effects we need to make a new assumption about the relationship between the instrument and the treatment.
- Monotonicity says that the presence of the instrument never dissuades someone from taking the treatment:

$$T_i(1) - T_i(0) \geq 0$$

- Note if this holds in the opposite direction $T_i(1) - T_i(0) \leq 0$, we can always rescale $T_i$ to make the assumption hold.

# Principal Strata

Following Angrist, Imbens, and Rubin (1996), we can define four subpopulations (for cases with a binary treatment and a binary instrument):

**Definition**

- Compliers: $T_i(1) > T_i(0)$ ($T_i(0) = 0$ and $T_i(1) = 1$).
- Always-takers: $T_i(1) = T_i(0) = 1$.
- Never-takers: $T_i(1) = T_i(0) = 0$.
- Defiers: $T_i(1) < T_i(0)$ ($T_i(0) = 1$ and $T_i(1) = 0$).

Only one of the potential treatment indicators $(T_i(0), T_i(1))$ is observed, so in the general case we cannot identify exactly which group any particular individual belongs to (although we can rule some out).

# Monotonicity means no defiers

| Name | $T_i(1)$ | $T_i(0)$ |
|---|---|---|
| Always Takers | 1 | 1 |
| Never Takers | 0 | 0 |
| Compliers | 1 | 0 |
| Defiers | 0 | 1 |

- We sometimes call assumption 4 no defiers because the monotonicity assumption rules out the existence of defiers.
- This means we can now sometimes identify the subgroup
- Anyone with $T_i = 1$ when $Z_i = 0$ must be an always-taker and anyone with $T_i = 0$ when $Z_i = 1$ must be a never-taker.

# Local Average Treatment Effect (LATE)

- Under these four assumptions, we can use the Wald estimator to estimate the local average treatment effect (LATE) — sometimes called the complier average treatment effect.
- This is the ATE among the compliers: those that take the treatment when encouraged to do so.
- That is, the LATE theorem, states that:

$$\frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[T_i|Z_i = 1] - E[T_i|Z_i = 0]} = E[Y_i(1) - Y_i(0)|T_i(1) > T_i(0)]$$

- This may seem mundane in that we have simply changed our assumptions and not our estimation, but this fact was a massive intellectual jump in our understanding of IV. Angrist, Imbens and Rubin (1996) is amazing, you should read it!

# Who are the Compliers?

| Study | Outcome | Treatment | Instrument |
|---|---|---|---|
| Angrist and Evans (1998) | Earnings | More than 2 Children | Multiple Second Birth (Twins) |
| Angrist and Evans (1998) | Earnings | More than 2 Children | First Two Children are Same Sex |
| Levitt (1997) | Crime Rates | Number of Policemen | Mayoral Elections |
| Angrist and Krueger (1991) | Earnings | Years of Schooling | Quarter of Birth |
| Angrist (1990) | Earnings | Veteran Status | Vietnam Draft Lottery |
| Miguel, Satyanath and Sergenti (2004) | Civil War Onset | GDP per capita | Lagged Rainfall |
| Acemoglu, Johnson and Robinson (2001) | Economic performance | Current Institutions | Settler Mortality in Colonial Times |
| Cleary and Barro (2006) | Religiosity | GDP per capita | Distance from Equator |

# Is the LATE useful?

- Once we allow for heterogeneous effects, all we can estimate with IV is the effect of treatment among compliers.
- This is an unknown subset of the data.
    - Treated units are a mix of always takers and compliers.
    - Control units are a mix of never takers and compliers.
- Without further assumptions, Local Average Treatment Effect (LATE) and Average Treatment Effect (ATE) are not equal.
- Complier group depends on the instrument $\rightsquigarrow$ different IVs will lead to different estimands!
- How much we care largely depends on our theory and what the instrument is.
- The traditional framework "cheats" by assuming that the effect is constant, so it is the same for compliers and non-compliers.

# Randomized Trials with One-Sided Noncompliance

- Will the LATE ever be equal to a usual causal quantity?
- When non-compliance is one-sided, then the LATE is equal to the ATT.
- Think of a randomized experiment:
  - Randomized treatment assignment = instrument ($Z_i$)
  - Non-randomized actual treatment taken = treatment ($T_i$)
- One-sided noncompliance: only those assigned to treatment (control) can actually take the treatment (control). Or

$$T_i(0) = 0 \forall i \quad \rightsquigarrow \quad P(T_i = 1 | Z_i = 0) = 0$$

- Maybe this is because only those treated actually get pills or only they are invited to the job training location. But this can be very difficult in many settings.
- See also, Imbens 2010. "Better LATE Than Nothing: Some Comments on Deaton (2009) and Heckman and Urzua (2009)" `http://dx.doi.org/10.1257/jel.48.2.399`

# Benefits of one-sided noncompliance

One-sided noncompliance $\rightsquigarrow$ no "always-takers" and since there are no defiers,

- Treated units must be compliers.
- ATT is the same as the LATE.

**Proof.**

$$
\begin{aligned}
E[Y_i|Z_i=1] - E[Y_i|Z_i=0] =& E[Y_i(0) + (Y_i(1) - Y_i(0))T_i|Z_i=1] - E[Y_i(0)|Z_i=0] \\
& \text{(exclusion restriction + one-sided noncompliance)} \\
=& E[Y_i(0)|Z_i=1] + E[(Y_i(1) - Y_i(0))T_i|Z_i=1] - E[Y_i(0)|Z_i=0] \\
=& E[Y_i(0)] + E[(Y_i(1) - Y_i(0))T_i|Z_i=1] - E[Y_i(0)] \\
& \text{(randomization)} \\
=& E[Y_i(1) - Y_i(0)|T_i=1, Z_i=1]P(T_i=1|Z_i=1) \\
& \text{(law of iterated expectations + binary treatment)} \\
=& E[Y_i(1) - Y_i(0)|T_i=1]P(T_i=1|Z_i=1) \\
& \text{(one-sided noncompliance)}
\end{aligned}
$$

Noting that $P(T_i=1|Z_i=0)=0$, then the Wald estimator is just the ATT: $\frac{E[Y_i|Z_i=1]-E[Y_i|Z_i=0]}{P(T_i=1|Z_i=1)} = E[Y_i(1) - Y_i(0)|T_i=1]$ Thus, under the additional assumption of one-sided compliance, we can estimate the ATT using the usual IV approach $\square$

# Example: The Vietnam Draft Lottery (Angrist (1990))

- Effect of military service on civilian earnings
- Simple comparison between Vietnam veterans and non-veterans are likely to be a biased measure
- Angrist (1990) used draft-eligibility, determined by the Vietnam era draft lottery, as an instrument for military service in Vietnam
- Draft eligibility is random and affected the probability of enrollment
- Estimate suggest a 15% negative effect of veteran status on earnings in the period 1981-1984 for white veterans born in 1950-51; although the estimators are quite imprecise
- This is only identified for compliers (i.e. those who if draft eligible would serve but otherwise would not)

# Wald Estimates for Vietnam Draft Lottery (Angrist (1990))

| | | Draft-Eligibility Effects in Current $ | | | | |
|---|---|---|---|---|---|---|
| Cohort | Year | FICA Earnings (1) | Adjusted FICA Earnings (2) | Total W-2 Earnings (3) | $\hat{p}^e - \hat{p}^n$ (4) | Service Effect in 1978 $ (5) |
| 1950 | 1981 | −435.8 | −487.8 | −589.6 | 0.159 | −2,195.8 |
| | | (210.5) | (237.6) | (299.4) | (0.040) | (1,069.5) |
| | 1982 | −320.2 | −396.1 | −305.5 | | −1,678.3 |
| | | (235.8) | (281.7) | (345.4) | | (1,193.6) |
| | 1983 | −349.5 | −450.1 | −512.9 | | −1,795.6 |
| | | (261.6) | (302.0) | (441.2) | | (1,204.8) |
| | 1984 | −484.3 | −638.7 | −1,143.3 | | −2,517.7 |
| | | (286.8) | (336.5) | (492.2) | | (1,326.5) |
| 1951 | 1981 | −358.3 | −428.7 | −71.6 | 0.136 | −2,261.3 |
| | | (203.6) | (224.5) | (423.4) | (0.043) | (1,184.2) |
| | 1982 | −117.3 | −278.5 | −72.7 | | −1,386.6 |
| | | (229.1) | (264.1) | (372.1) | | (1,312.1) |
| | 1983 | −314.0 | −452.2 | −896.5 | | -2,181.8 |
| | | (253.2) | (289.2) | (426.3) | | (1,395.3) |
| | 1984 | −398.4 | −573.3 | −809.1 | | −2,647.9 |
| | | (279.2) | (331.1) | (380.9) | | (1,529.2) |
| 1952 | 1981 | −342.8 | −392.6 | −440.5 | 0.105 | −2,502.3 |
| | | (206.8) | (228.6) | (265.0) | (0.050) | (1,556.7) |
| | 1982 | −235.1 | −255.2 | −514.7 | | −1,626.5 |
| | | (232.3) | (264.5) | (296.5) | | (1,685.8) |
| | 1983 | −437.7 | −500.0 | −915.7 | | −3,103.5 |
| | | (257.5) | (294.7) | (395.2) | | (1,829.2) |
| | 1984 | −436.0 | −560.0 | −767.2 | | −3,323.8 |
| | | (281.9) | (330.1) | (376.0) | | (1,959.3) |

# Estimating the Size of the Complier Group

- Since we never observe both potential treatment assignments for the same unit, we cannot identify individual units as compliers
- However, we can easily identify the proportion of compliers in the population using the first stage effect:

$$\begin{aligned} P(T_i(1) > T_i(0)) &= E[T_i(1) - T_i(0)] = E[T_i(1)] - E[T_i(0)] \\ &= E[T_i|Z_i = 1] - E[T_i|Z_i = 0] \end{aligned}$$

- Using a similar logic we can identify the proportion of compliers among the treated or controls only. For example:

$$P(T_i(1) > T_i(0)|T_i = 1) = \frac{P(Z_i = 1)(E[T_i|Z_i = 1] - E[T_i|Z_i = 0])}{P(T_i = 1)}$$

- Note: this estimate is pinned down entirely by the assumptions of monotonicity and exogeneity
- Abadie (2003) shows how to use covariate information to calculate other characteristics of the complier group (kappa weighting)

# Size of Complier Group

TABLE 4.4.2
Probabilities of compliance in instrumental variables studies

| Source (1) | Endogenous Variable (D) (2) | Instrument (Z) (3) | Sample (4) | $P[D = 1]$ (5) | First Stage, $P[D_1 > D_0]$ (6) | $P[Z = 1]$ (7) | Compliance Probabilities $P[D_1 > D_0 \mid D = 1]$ (8) | Compliance Probabilities $P[D_1 > D_0 \mid D = 0]$ (9) |
|---|---|---|---|---|---|---|---|---|
| Angrist (1990) | Veteran status | Draft eligibility | White men born in 1950 | .267 | .159 | .534 | .318 | .101 |
| | | | Non-white men born in 1950 | .163 | .060 | .534 | .197 | .033 |
| Angrist and Evans (1998) | More than two children | Twins at second birth | Married women aged 21–35 with two or more children in 1980 | .381 | .603 | .008 | .013 | .966 |
| | | First two children are same sex | | .381 | .060 | .506 | .080 | .048 |
| Angrist and Krueger (1991) | High school graduate | Third- or fourth-quarter birth | Men born between 1930 and 1939 | .770 | .016 | .509 | .011 | .034 |
| Acemoglu and Angrist (2000) | High school graduate | State requires 11 or more years of school attendance | White men aged 40–49 | .617 | .037 | .300 | .018 | .068 |

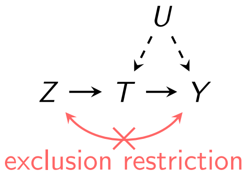
*Notes*: The table computes the absolute and relative size of the complier population for a number of instrumental variables. The first stage, reported in column 6, gives the absolute size of the complier group. Columns 8 and 9 show the size of the complier population relative to the treated and untreated populations.

# Do we care about the complier effect?

IV estimates the effect for compliers. How do we feel about that?

- Pros
  - it is identified (i.e. it is what we can get)
  - if the instrument is the policy lever, these are the people we can help
  - we can compute proportion of compliers (and under strong assumptions) characterize the compliers in terms of observed variables
- Cons
  - we don't actually know who the compliers are and every instrument yields a different group
  - monotonicity is a strong unit-level assumption
    (i.e. it is unlikely to hold when decision to treat is the result of multiple criteria that includes risks and benefits, see Hernán and Robins 2018, pg 63)
  - 'relatively minor violations of conditions [Assumptions 1-4] for IV estimation may result in large biases of unpredictable or counter-intuitive direction'
    (Hernán and Robins 2018)

# Falsification tests




- The exclusion restriction cannot be tested directly, but it can be falsified.
- Falsification test Test the reduced form effect of $Z_i$ on $Y_i$ in situations where it is impossible or extremely unlikely that $Z_i$ could affect $T_i$.
- Because $Z_i$ can't affect $T_i$, then the exclusion restriction implies that this falsification test should have 0 effect.
- Nunn & Wantchekon (2011): use distance to coast as an instrument for Africans, use distance to the coast in an Asian sample as falsification test.

# Nunn & Wantchekon falsification test



TABLE 7—REDUCED FORM RELATIONSHIP BETWEEN THE DISTANCE FROM THE COAST AND TRUST WITHIN AFRICA AND ASIA

| | Trust of local government council | | | |
|---|---|---|---|---|
| | Afrobarometer sample | | Asiabarometer sample | |
| | (1) | (2) | (3) | (4) |
| Distance from the coast | 0.00039*** | 0.00031*** | −0.00001 | 0.00001 |
| | (0.00009) | (0.00008) | (0.00010) | (0.00009) |
| Country fixed effects | Yes | Yes | Yes | Yes |
| Individual controls | No | Yes | No | Yes |
| Number of observations | 19,913 | 19,913 | 5,409 | 5,409 |
| Number of clusters | 185 | 185 | 62 | 62 |
| $R^2$ | 0.16 | 0.18 | 0.19 | 0.22 |

*Notes:* The table reports OLS estimates. The unit of observation is an individual. The dependent variable in the Asiabarometer sample is the respondent's answer to the question: "How much do you trust your local government?" The categories for the answers are the same in the Asiabarometer as in the Afrobarometer. Standard errors are clustered at the ethnicity level in the Afrobarometer regressions and at the location (city) level in the Asiabarometer and the WVS samples. The individual controls are for age, age squared, a gender indicator, education fixed effects, and religion fixed effects.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

# Other Extensions to IV

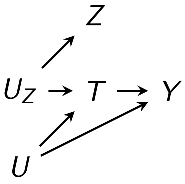
- Multiple instruments
- Covariates and conditional ignorability (see Glynn and Rueda 2018 on post-instrument bias though)
- Overidentification tests (in constant effects)
- Compliance modeling for weak instruments
- Conditional effects and causal interaction models

# Classical Vs. Modern Instrumental Variables

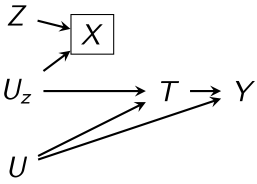
- We dropped the constant effects assumption (assumption 4), which is usually unrealistic.
- We added a weaker monotonicity assumption.
- We defined a set of subpopulations: compilers, always-takers, never-takers, defiers
- We clarify that the effects are identified only for a particular subpopulation — the "complier" subpopulation.
  (if constant effects happen to hold, effects for compliers are by definition same as for entire population.)

# Wait, What Were The Assumptions?

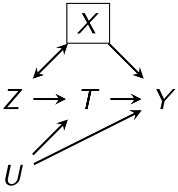
- Assumptions given have typically been sufficient but not necessary.
- Easy to state assumptions for linear structural equation models (where we can use covariance of error and variable), harder in general.
- Technically can use the following in place of assumptions 1-3: (see technical point 16.1 of Hernán and Robins)
  1) Exogeneity: $Y_i(t,z) \perp\!\!\!\perp Z_i$ for all $t, z$.
  2) Exclusion: $Y_i(t,z) = Y_i(t,z') = Y_i(t)$ for all $z, z', d$ and $i$
  3) Relevance: $Z \not\!\perp\!\!\!\perp T$
     - all possibly conditional on $X$
- This allows some graphs that don't meet our original conditions, but satisfy new assumptions given particular edge configurations.




(proxy instrument)




(instrument via collider)




(exclusion via control)

# Concluding Thoughts on Instrumental Variables

- Strong assumptions but powerful results
- Enormous care is required in the interpretation
- Questions to Always Ask
  1. is the instrument weak?
     (does $Z$ predict $T$)
  2. is the instrument exogenous?
     (was $Z$ randomly assigned?)
  3. does the exclusion restriction hold?
     (is there a path from $Z$ to $Y$ not through $T$)
  4. do we believe monotonicity?
     (are there units where the instrument discourages treatment)
  5. do the assumptions identify an effect for the subpopulation of interest?
     (is the instrument such that we care about compliers?)
- Be sure to evaluate all conditions and remember randomization of $Z$ does not guarantee the exclusion restriction.

# Fun With Coarsening Bias

## Coarsening Bias: How Coarse Treatment Measurement Upwardly Biases Instrumental Variable Estimates

**John Marshall**
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Edited by Jonathan Katz

Political scientists increasingly use instrumental variable (IV) methods, and must often choose between operationalizing their endogenous treatment variable as discrete or continuous. For theoretical and data availability reasons, researchers frequently coarsen treatments with multiple intensities (e.g., treating a continuous treatment as binary). I show how such coarsening can substantially upwardly bias IV estimates by subtly violating the exclusion restriction assumption, and demonstrate that the extent of this bias depends upon the first stage and underlying causal response function. However, standard IV methods using a treatment where multiple intensities are affected by the instrument—even when fine-grained measurement at every intensity is not possible—recover a consistent causal estimate without requiring a stronger exclusion restriction assumption. These analytical insights are illustrated in the context of identifying the long-run effect of high school education on voting Conservative in Great Britain. I demonstrate that coarsening years of schooling into an indicator for completing high school upwardly biases the IV estimate by a factor of three.

# The Idea

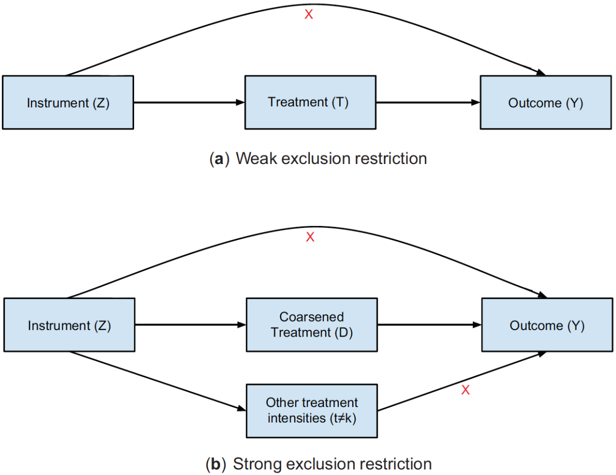



**Fig. 1** Graphical representation of weak and strong exclusion restrictions.

# Design

- Data: British Election Survey 1979-2010
- Outcome: voting for conservative party in most recent election
- Instrument: respondents turning 14 in 1947 or later who were affected by the 1947 school leaving reform (increased age from 14 to 15)
- Treatment: either years of schooling or coarsened indicator for completed high school or not

# Data

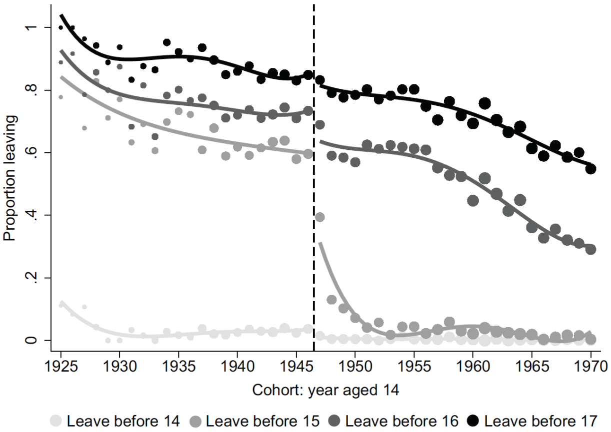



**Fig. 3** 1947 compulsory schooling reform and student leaving age by cohort.
*Notes*: Data are from the British Election Survey. Curves represent fourth-order polynomial fits. Gray dots are birth-year cohort averages, and their size reflects their weight in the sample.

# Findings

- Finding: Using the dichotomous version of the treatment inflates the result by a factor of three
- Suggestion: Use the linear version of the treatment (although see the article for more details!)

Want to learn more about instrumental variables? Here are some things to read:

- Angrist, Imbens, and Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables" *Journal of the American Statistical Association*, `https://www.jstor.org/stable/2291629`
- Sovey and Green. 2011. "Instrumental Variables Estimation in Political Science: A Readers' Guide" *American Journal of Political Science*, `https://doi.org/10.1111/j.1540-5907.2010.00477.x`
- Swanson and Hernan. 2013. "Commentary: How to Report Instrumental Variable Analyses (Suggestions Welcome)" *Epidemiology*, `https://www.jstor.org/stable/23486750`
- Morgan and Winship. 2014. Chapter 9: Instrumental Variable Estimators of Causal Effects. `https://doi.org/10.1017/CBO9781107587991.010`
- Angrist and Pische. Chapter 4: Instrumental Variables in Action: Sometimes You Get What You Need

# We Covered

- IV with heterogeneous effects

Next Time: Regression Discontinuity

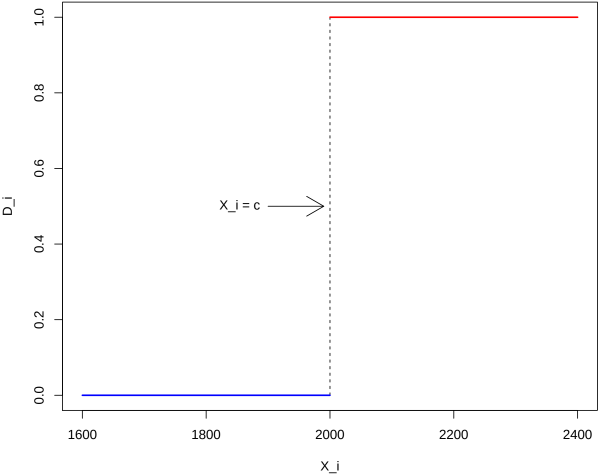
# Where We've Been and Where We're Going...

- Last Week
  - selection on observables and measured confounding
- This Week
  - natural experiments
  - instrumental variables
  - regression discontinuity
- The Following Week
  - repeated observations and wrap up
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causal inference

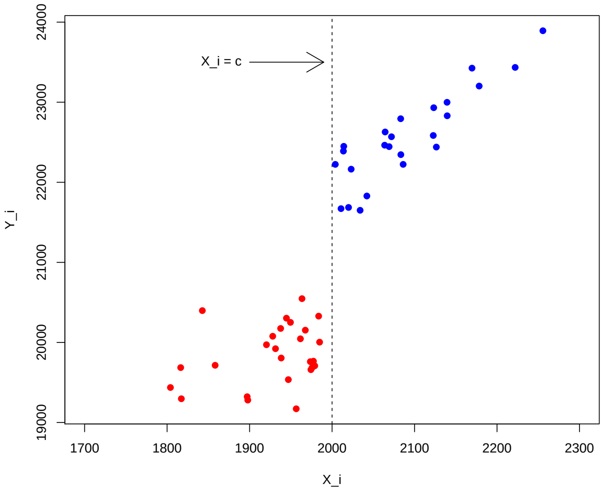
# Regression Discontinuity

- A different strategy where the core intuition is that identification comes in a discontinuity in treatment assignment
- A widely applicable strategy in rule-based systems or allocations of limited resources (e.g. administrative programs, elections, admission systems)
- It is a fairly old idea, generally credited to education research by Thistlethwaite and Campbell 1960 but with a dynamic and interesting recent history (Hahn et al 2001 and Lee 2008 were big jumps forward).
- The goal here is to get you up to speed with the core idea: if you want to know how to do this in practice read *A Practical Introduction to Regression Discontinuity Designs* Volumes I and II by Matias Cattaneo, Nicolás Idrodo and Rocío Titiunik

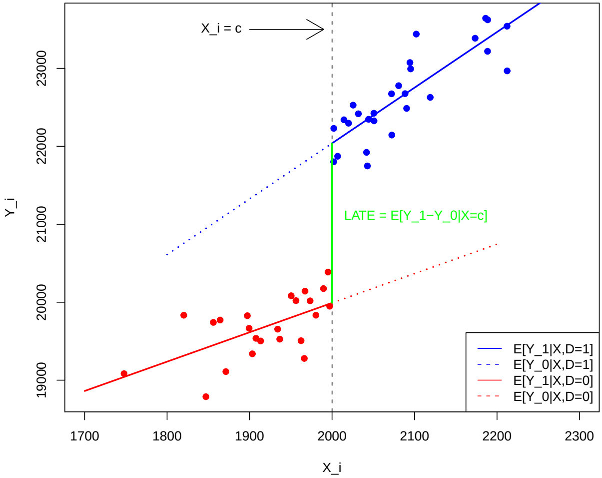
# Graphical Illustration



# Graphical Illustration



# Graphical Illustration



# Setup

- The basic idea behind RDDs:
  - $X_i$ is a forcing variable.
  - Treatment assignment is determined by a cutoff in $X_i$.

$$T_i = 1\{X_i > c\} \text{ so } T_i = \begin{cases} T_i = 1 & \text{if } X_i > c \\ T_i = 0 & \text{if } X_i < c \end{cases}$$

- $X_i$ can be related to the potential outcomes and so comparing treated and untreated units does not provide causal estimates
- assume relationship between $X$ and the potential outcomes $Y_1$ and $Y_0$ is smooth around the threshold $\rightsquigarrow$ discontinuity created by the treatment to estimate the effect of $T$ on $Y$ at the threshold

# Design

- Sharp RD: treatment assignment is a deterministic function of the forcing variable and the threshold.
- Key assumption: no compliance problems (deterministic)
- At the threshold, $c$, we only see treated units and below the threshold, we only see control values:

$$P(T_i = 1 | X_i = c) = 1$$
$$P(T_i = 1 | X_i < c) = 0$$

- Intuitively, we are interested in the discontinuity in the outcome at the discontinuity in the treatment assignment.
- We want to investigate the behavior of the outcome around the threshold: $\lim_{x \downarrow c} E[Y_i | X_i = x] - \lim_{x \uparrow c} E[Y_i | X_i = x]$
- Under certain assumptions, this quantity identifies the ATE at the threshold: $\tau_{SRD} = E[Y_i(1) - Y_i(0) | X_i = c]$

# Identification

## Identification Assumption

1. $Y(1), Y(0) \perp\!\!\!\perp T|X$ *(trivially met by construction)*
2. $0 < P(T = 1|X = x) < 1$ *(always violated in Sharp RDD)*
3. $E[Y(1)|X, T]$ *and* $E[Y(0)|X, T]$ *are continuous in* $X$ *around the threshold* $X = c$ *(individuals have imprecise control over* $X$ *around the threshold)*

## Identification Result

*The treatment effect is identified at the threshold as:*

$$
\begin{aligned}
\alpha_{SRDD} &= E[Y(1) - Y(0)|X = c] \\
&= E[Y(1)|X = c] - E[Y(0)|X = c] \\
&= \lim_{x \downarrow c} E[Y(1)|X = x] - \lim_{x \uparrow c} E[Y(0)|X = x]
\end{aligned}
$$

*Without further assumptions* $\alpha_{SRDD}$ *is only identified at the threshold.*

# Extrapolation and smoothness

- Remember the quantity of interest here is the effect at the threshold:

$$\begin{aligned}\tau_{SRD} &= E[Y_i(1) - Y_i(0)|X_i = c] \\ &= E[Y_i(1)|X_i = c] - E[Y_i(0)|X_i = c]\end{aligned}$$

- But we don't observe $E[Y_i(0)|X_i = c]$ ever due to the design, so we're going to extrapolate from $E[Y_i(0)|X_i = c - |\varepsilon|]$ for $\varepsilon \approx 0$.
- Extrapolation, even at short distances, requires smoothness in the functions we are extrapolating.

# What can go wrong?

- If the potential outcomes change at the discontinuity for reasons other than the treatment, then smoothness will be violated.
- For instance, if people sort around threshold, then you might get jumps other than the one you care about.
- If things other than the treatment change at the threshold, then that might cause discontinuities in the potential outcomes.

# Recent RDD Examples

- class size on student achievement
  - Angrist and Lavy 1999
- wage increase on performance of mayors

  Ferraz and Finan 2011; Gagliarducci and Nannicini 2013
- colonial institutions on development outcomes

  Dell 2009
- length of postpartum hospital stays on mother and infant mortality

  Almond and Doyle 2009
- naturalization on political integration of immigrants

  Hainmueller and Hangartner 2015
- financial aid offers on college enrollment

  Van der Klaauw 2002
- access to Angel funding on growth of start-ups

  Kerr, Lerner and Schoar 2010
- RDD that exploits "close" elections is workhorse model for electoral research:

  Lee, Moretti and Butler 2004, DiNardo and Lee 2004, Hainmueller and Kern 2008, Leigh 2008, Pettersson-Lidbom 2008, Broockman 2009, Butler 2009, Dal Bó, Dal Bó and Snyder 2009, Eggers and Hainmueller 2009, Ferreira and Gyourko 2009, Uppal 2009, 2010, Cellini, Ferreira and Rothstein 2010, Gerber and Hopkins 2011, Trounstine 2011, Boas and Hidalgo 2011, Folke and Snyder Jr. 2012, and Gagliarducci and Paserman 2012

# General estimation strategy

- The main goal in RD is to estimate the limits of various CEFs such as:

$$\lim_{x \uparrow c} E[Y_i | X_i = x]$$

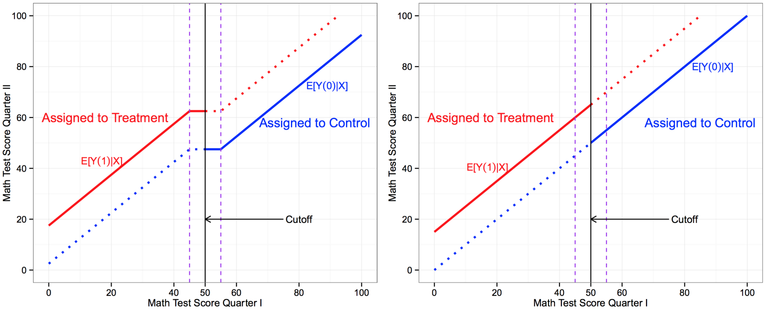
- It turns out that this is a hard problem because we want to estimate the regression at a single point and that point is a boundary point.
- As a result, the usual kinds of nonparametric estimators perform poorly (polynomials and kernels are particularly bad)
- In general, we are going to have to choose some way of estimating the regression functions around the cutpoint.
- Using the entire sample on either side will obviously lead to bias because those values that are far from the cutpoint are clearly different than those nearer to the cutpoint.
- $\rightarrow$ restrict our estimation to units close to the threshold.
- Local linear regression is a good way to go: see `rdrobust` package in `R` (Calonico et al 2015)

# Misconceptions

- Continuity of the potential outcomes does not imply local randomization
- This has caused a lot of confusion in the literature particularly in testing with background covariates
- Local statistical independence does not imply exclusion restriction (i.e. forcing variable not directly affecting the outcome)
- If you are doing an RDD: be sure to do balance checks and sensitivity checks (read-up on best practices first!)

# Local Randomization vs. Continuity (Sekhon and Titiunik 2017)

Figure 1: Two Scenarios with Randomly Assigned Score




(a) Test scores locally unrelated to potential outcomes

(b) Test scores locally related to potential outcomes

# Fuzzy RD

- With fuzzy RD, the treatment assignment is no longer a deterministic function of the forcing variable, but there is still a discontinuity in the probability of treatment at the threshold:

**Assumption FRD**

$$\lim_{x \downarrow c} P[T_i = 1 | X_i = x] \neq \lim_{x \uparrow c} P[T_i = 1 | X_i = x]$$

- In the sharp RD, this is also true, but it further required the jump in probability to be from 0 to 1.
- Fuzzy RD is often useful when the a threshold encourages participation in program, but does not actually force units to participate.
- Sound familiar? Fuzzy RD is just IV!

# Fuzzy RD is IV

- Forcing variable is an instrument: affects $Y_i$, but only through $T_i$ (at the threshold)
- Let $T_i(x)$ be the potential value of treatment when we set the forcing variable to $x$, for some small neighborhood around $c$.
- $T_i(x) = 1$ if unit $i$ would take treatment when $X_i$ was $x$
- $T_i(x) = 0$ if unit $i$ would take control when $X_i$ was $x$

# Fuzzy RD assumptions

**Assumption 2: Monotoncity**

There exists $\varepsilon$ such that $T_i(c+e) \geq T_i(c-e)$ for all $0 < e < \varepsilon$

No one is discouraged from taking the treatment by crossing the threshold.

**Assumption 3: Local Exogeneity of Forcing Variable**

In a neighborhood of $c$,

$$\{\tau_i, T_i(x)\} \perp\!\!\!\perp X_i$$

Basically, in an $\varepsilon$-ball around $c$, the forcing variable is randomly assigned.

# Example: Early Release Program (HDC)

- Prison system in many countries is faced with overcrowding and high recidivism rates after release.
- Early discharge of prisoners on electronic monitoring has become a popular policy
- Difficult to estimate impact of early release program on future criminal behavior: best behaved inmates are usually the ones to be released early
- Marie (2008) considers Home Detention Curfew (HDC) scheme in England and Wales:
- Fuzzy RDD: Only offenders sentenced to more than three months (88 days) in prison are eligible for HDC, but not all those with longer sentences are offered HDC

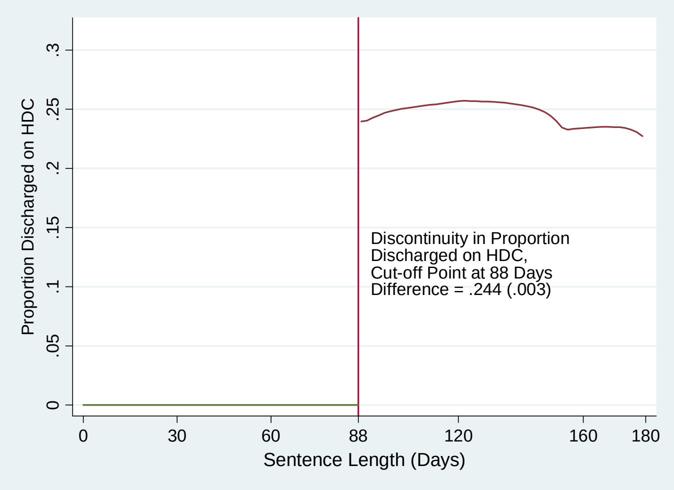
# Example: Early Release Program (HDC)

**Table 2: Descriptive Statistics for Prisoners Released by Length of Sentence and HDC and Non HDC Discharges and +/-7 Days Around Discontinuity Threshold**

| | | | |
|---|---|---|---|
| **Panel A - Released +/- 7 Days of 3 Months (88 Days) Cut-off:** | | | |
| **Discharge Type** | **Non HDC** | **HDC** | **Total** |
| Percentage Female | 10.5 | 9.7 | 10.3 |
| Mean Age at Release | 28.9 | 30.7 | 29.3 |
| Percentage Incarcerated for Violence | 19.8 | 18.2 | 19.4 |
| Mean Number Previous Offences | 9.5 | 5.7 | 8.7 |
| Recidivism within 12 Months | 54.6 | 28.1 | 48.8 |
| Sample Size | 18,928 | 5,351 | 24,279 |
| | | | |
| **Panel B - Released +/- 7 Days of 3 Months (88 Days) Cu-off:** | | | |
| **Day of Release around Cut-off** | **- 7 Days** | **+ 7 Days** | **Total** |
| Percentage Female | 11 | 10.2 | 10.3 |
| Mean Age at Release | 28.8 | 29.4 | 29.3 |
| Percentage Incarcerated for Violence | 17.1 | 19.7 | 19.4 |
| Mean Number Previous Offences | 9.1 | 8.6 | 8.7 |
| Recidivism within 12 Months | 56.8 | 47.9 | 48.8 |
| Percentage Released on HDC | 0 | 24.4 | 22 |
| Sample Size | 2,333 | 21,946 | 24,279 |

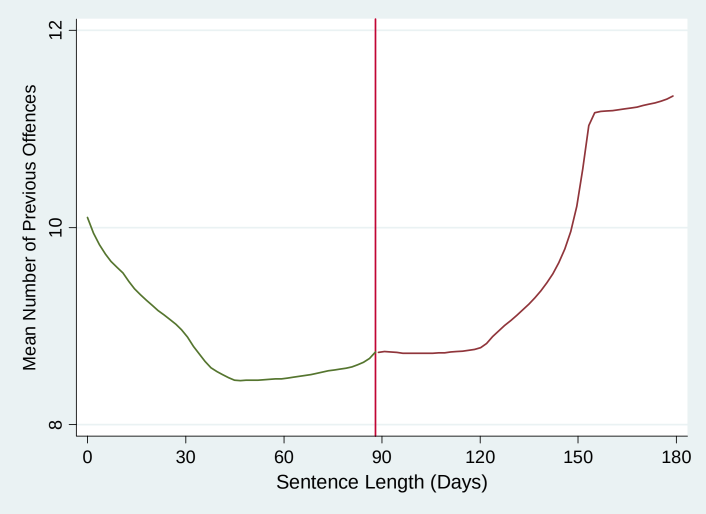
# Example: Early Release Program (HDC)

**Figure 1: Proportion Discharged on HDC by Sentence Length**



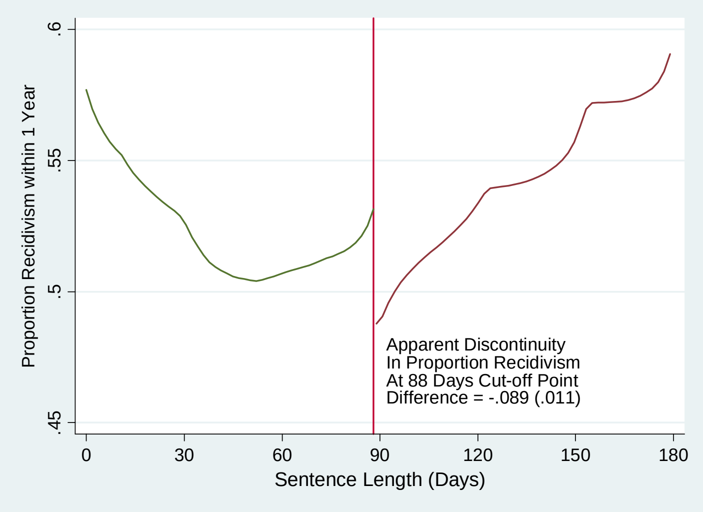
# Example: Early Release Program (HDC)

**Figure 2: Mean Number of Previous Offence by Sentence Length**



# Example: Early Release Program (HDC)

**Figure 4: Recidivism within 1 Year by Sentence Length**

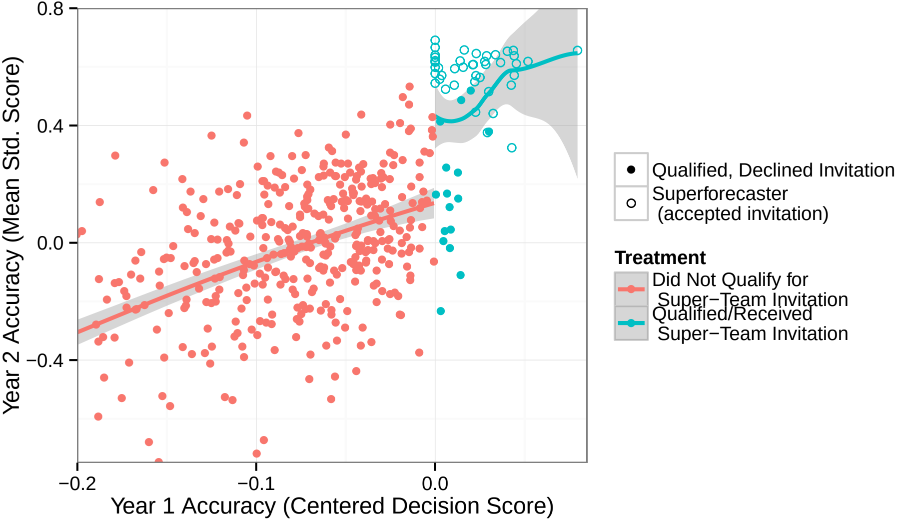


# Example: Early Release Program (HDC)

**Table 4: RDD Estimates of HDC Impact on Recidivism – Around Threshold**

| | Dependent Variable = Recidivism Within 12 Months<br><br>Estimation on Individuals Discharged +/- 7 Days of 88 Days Threshold | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| **Estimated Discontinuity of HDC Participation at Threshold ( $HDC^{+} - HDC^{-}$ )** | .243<br>(.009) | .223<br>(.009) | .243<br>(.003) |
| **Estimated Difference in Recidivism Around Threshold ( $Rec^{+} - Rec^{-}$ )** | -.089<br>(.011) | -.059<br>(.009) | -.044<br>(.014) |
| **Estimated Effect of HDC on Recidivism Participation ($Rec^{+} - Rec^{-}$ )/ ($HDC^{+} - HDC^{-}$ )** | -.366<br>(.044) | -.268<br>(.044) | -.181<br>(n.a.) |
| **Controls** | No | Yes | No |
| **PSM** | No | No | Yes |
| **Sample Size** | 24,279 | 24,279 | 24,279 |

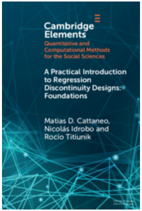
# Example: Teamwork



# Regression Discontinuity Conclusions

- Key idea is to exploit an arbitrary assignment rule to identify a causal quantity.
- Remember that we are only identifying an effect at the boundary.
- There are many other nuances to estimation and choosing an appropriate bandwidth for the comparison- be sure to read more before trying this at home.
- There is an interesting literature on geographic regression discontinuity designs as well. These are harder but can be useful!

# What to read next?




- A Practical Introduction to Regression Discontinuity Designs, Volumes I and II by Matias Cattaneo, Nicolas Idrodo and Rocio Titunik
- Angrist and Pishke Chapter 6 Regression Discontinuity Designs

# Fun with Extremists

Hall, Andrew. "What Happens When Extremists Win Primaries?" 2015. *American Political Science Review*.

I'm grateful to Andy Hall for sharing the following slides with me.

# What are the Effects of Extremists Winning Primaries?

*"...getting a general-election candidate who can win is the only thing we care about."*
—*Nat'l Republican Senatorial Committee*

vs.

*"The road to hell is paved with electable candidates."* —*Conservative Blogger*

There is a tradeoff between ideology and electability:

- Evaluates how the preferences of primary voters map to legislature.
- Shows how general elections react to moderates vs. extremists.

## Findings: Elections Strongly Prefer Moderates

In the U.S. House, 1980–2010:

- Extremist causes 38 percentage-point decrease in win probability on average.
- On average, roll-call voting farther away from primary voters when they nominate extremists.

# Elections Select Moderate Extremists

- Primary voters cannot force in extremists.
- House elections choose moderates, but constrained by candidate pool.
- Argument of broader research project: candidate entry key to electing extremist legislators.

# Empirical Approach

- Quantity of interest: effect of extremist nominees
- Ideal experiment: randomly assign districts extremist or moderate nominees.
- Compare elections and roll-call voting in "treated" districts vs. "control" districts.

# Obstacle to Estimating Effects of Extremist Nominees

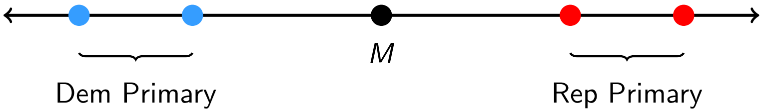
Selection Bias.

- Districts choose extremist nominees because they prefer them.

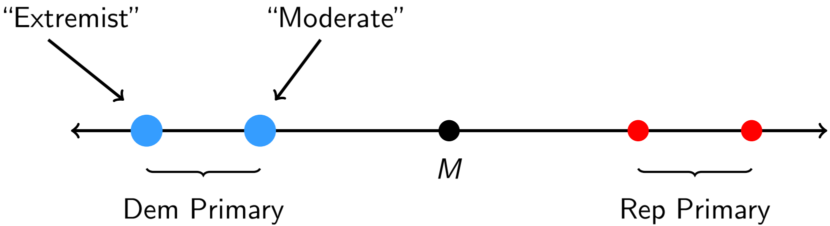
# Close Primaries Offer Variation in Nominee Type

- Regression discontinuity design (RDD) in primary elections.
- Districts with moderate/extremist nominee otherwise identical in expectation.
- Key assumption for RDD: no sorting

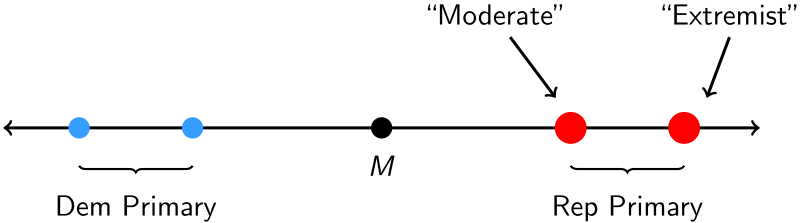
# "Extremists" Defined



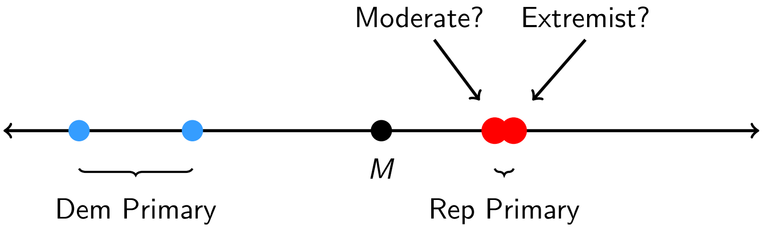
# “Extremists” Defined



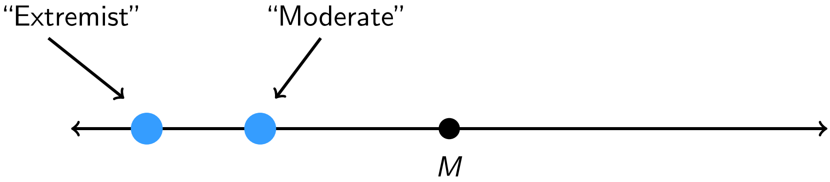
# "Extremists" Defined



# "Extremists" Defined



# "Extremists" Defined




- Calculate distance between moderate and extremist.
- Use races where distance is at or above the median distance.

# Quick Example: Robbie Wills vs. Joyce Elliott

Joyce Elliott: -0.33

vs.

Robbie Wills: -0.07

# Quick Example: Robbie Wills vs. Joyce Elliott

Joyce Elliott: -0.33


vs.

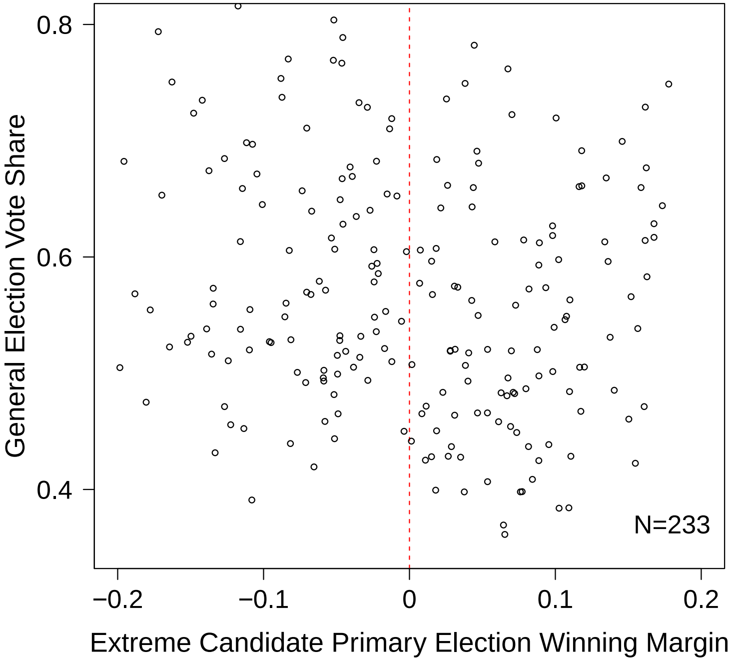
Robbie Wills: -0.07


- Wills sent out mailer calling Elliott an "extremist" who was "unelectable."
- Elliott won close runoff primary and lost general election 62% to 38%.

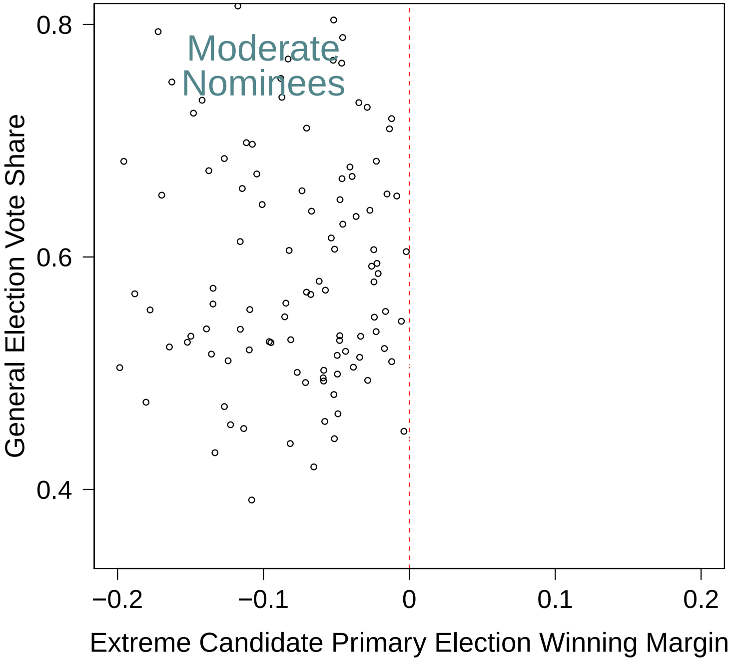
# Estimating the RD: Effects of Extremist Nominations

$$Y_{it} = \beta_0 + \beta_1 Extremist\ Primary\ Win_{it} + f(V_{it}) + \epsilon_{it}$$

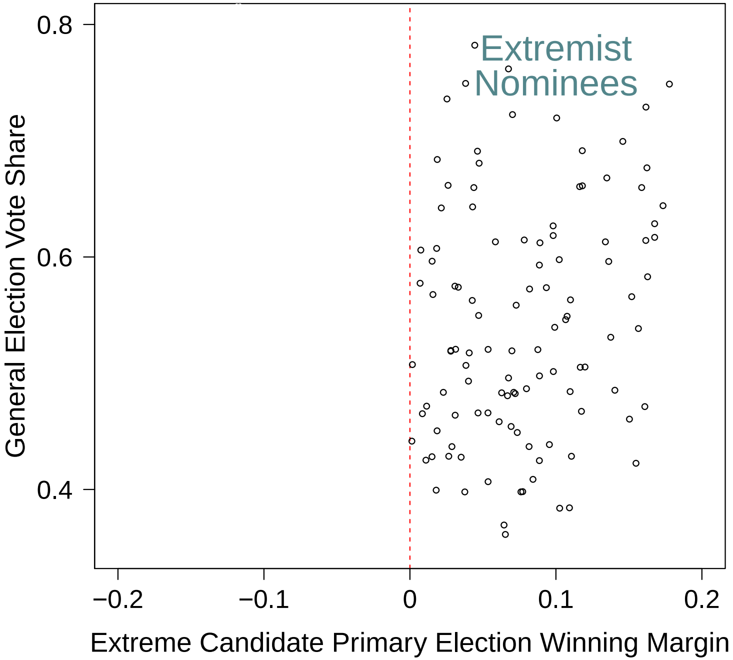
$V_{it} \equiv$ extremist candidate's vote-share winning margin.

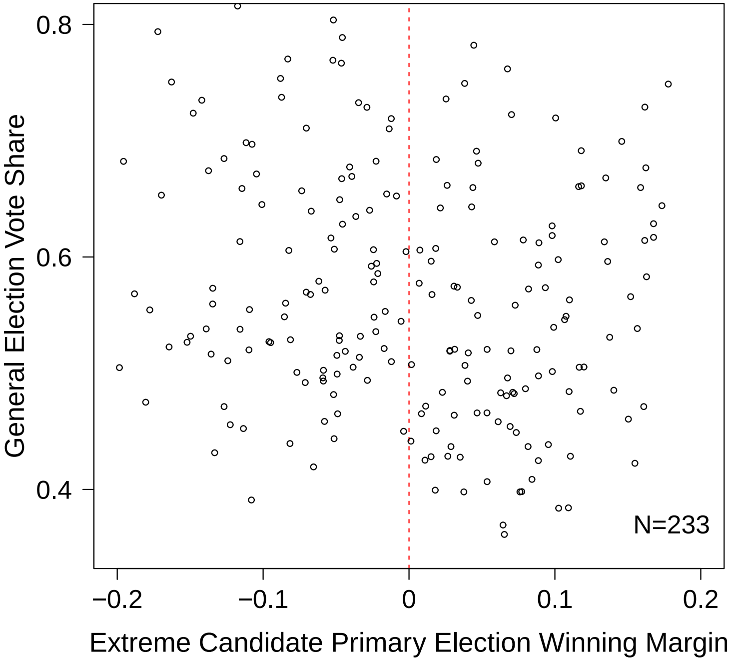


0.8
0.6
0.4
General Election Vote Share
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin
N=233

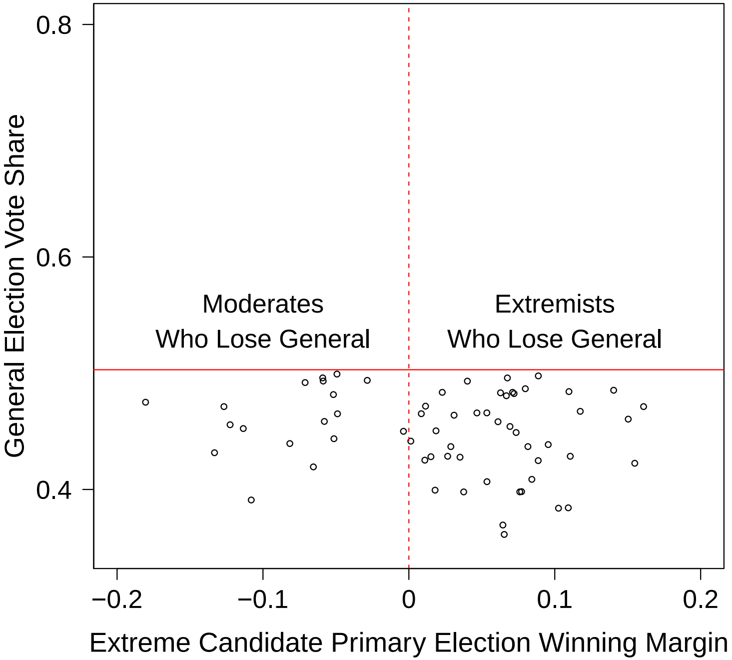


Moderate
Nominees
0.8
0.6
0.4
−0.2
−0.1
0
0.1
0.2
General Election Vote Share
Extreme Candidate Primary Election Winning Margin

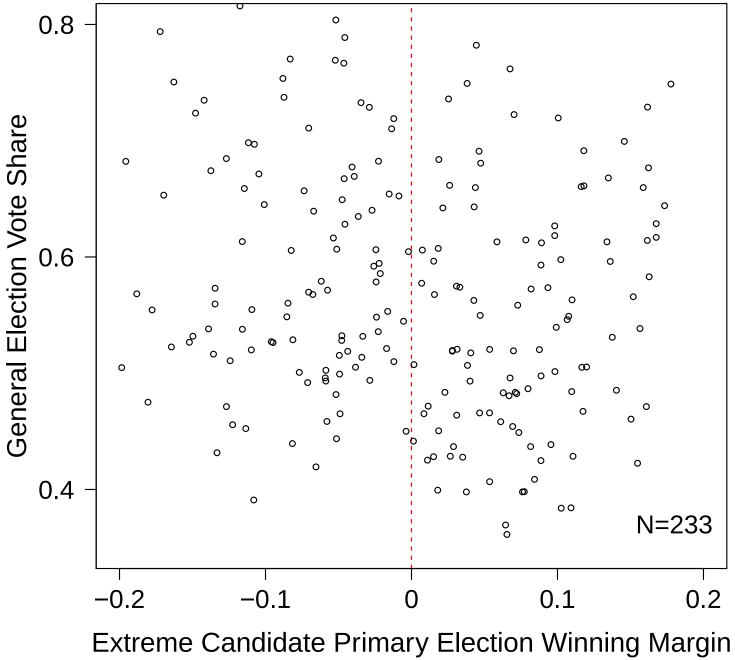

Extremist
Nominees
General Election Vote Share
0.8
0.6
0.4
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin

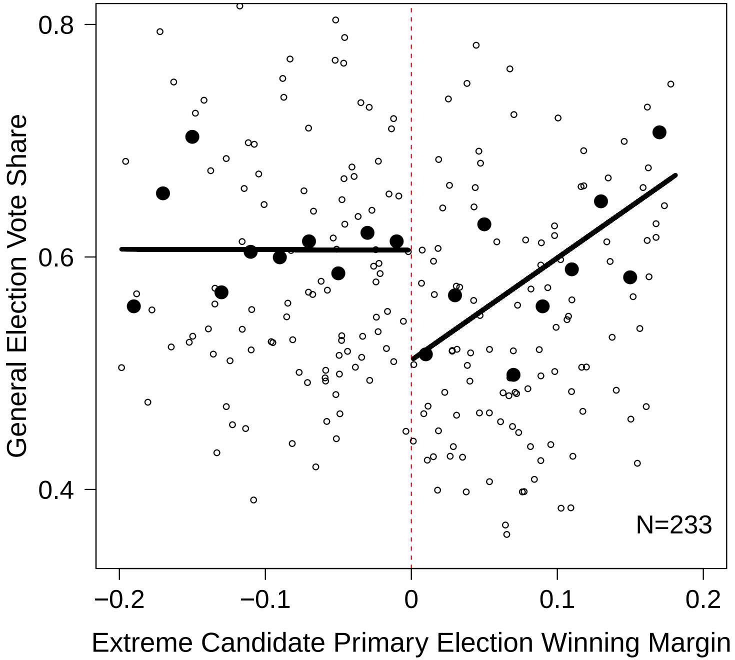


0.8
0.6
0.4
General Election Vote Share
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin
N=233

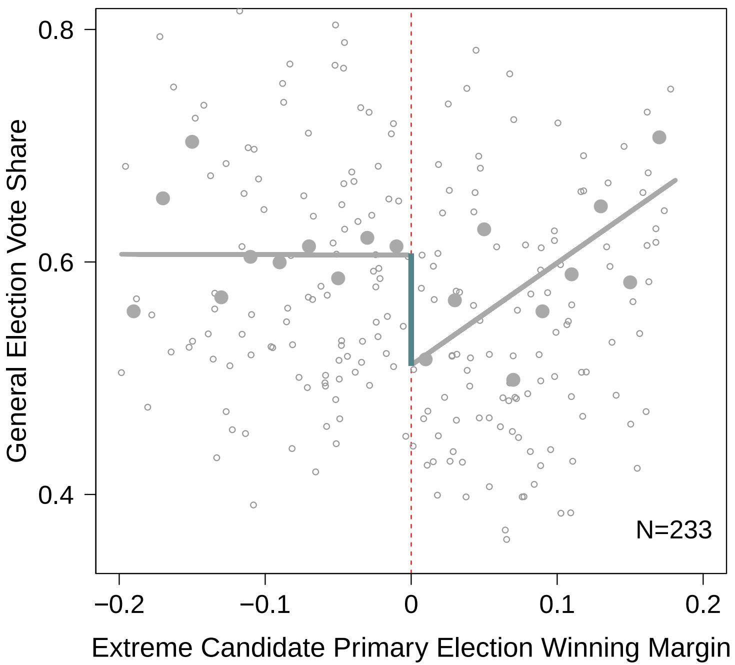


0.8
0.6
0.4
General Election Vote Share
Moderates
Who Lose General
Extremists
Who Lose General
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin

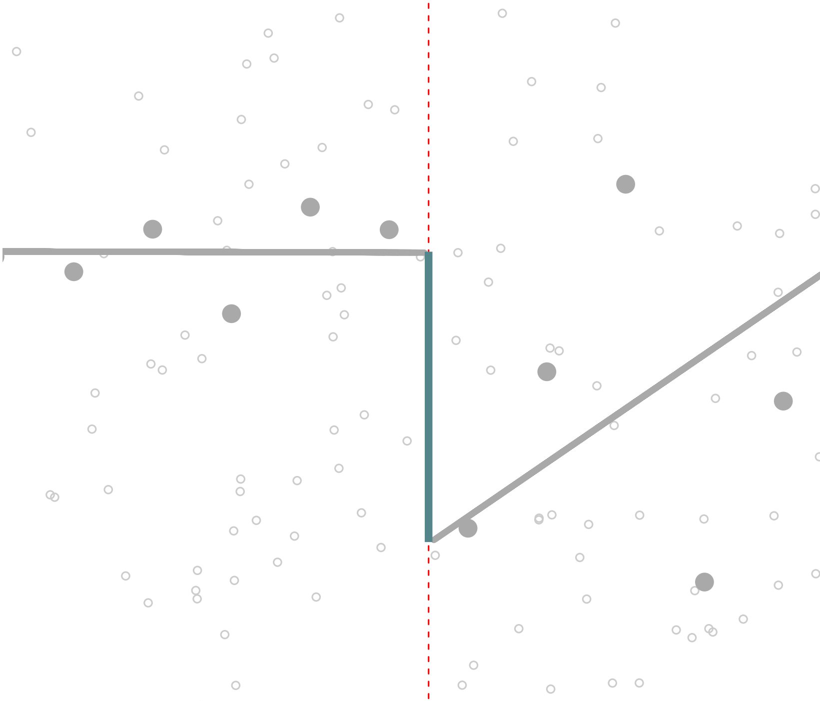

0.8
0.6
0.4
General Election Vote Share
N=233
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin

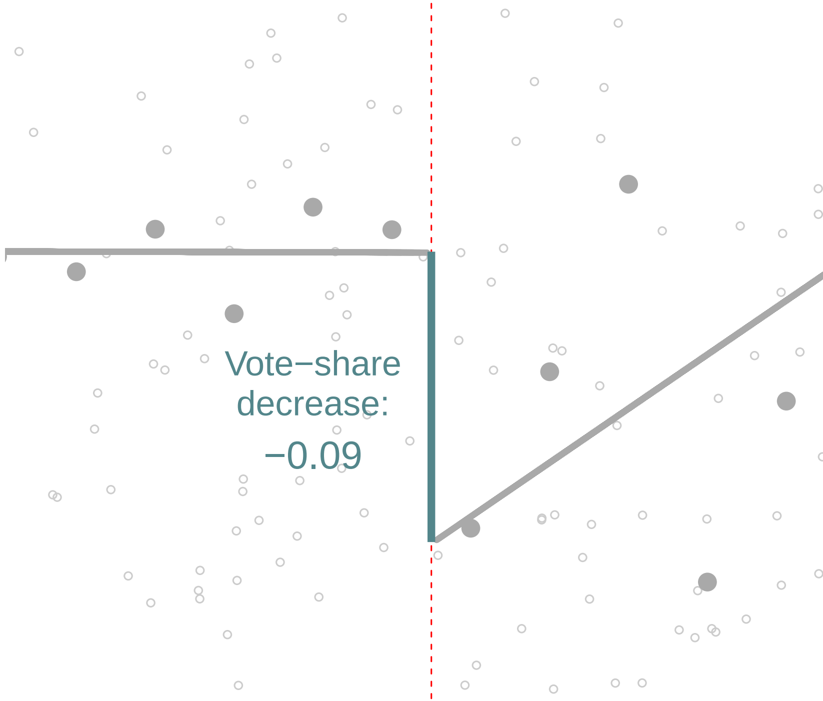


0.8
0.6
0.4
General Election Vote Share
N=233
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin

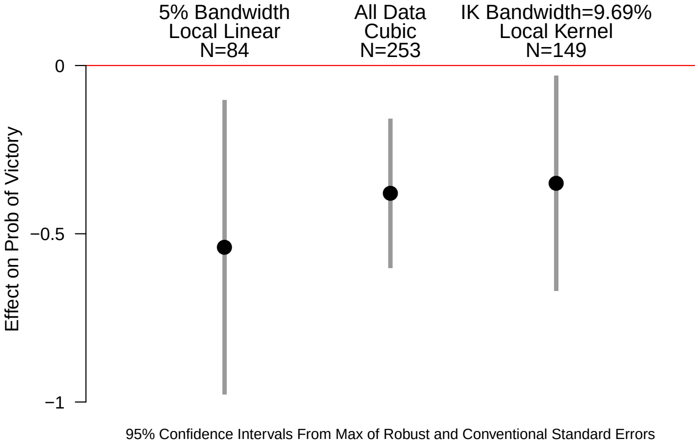


0.8
0.6
0.4
General Election Vote Share
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin
N=233




Vote−share
decrease:
−0.09

# Large Electoral Penalty to Nominating Extremist



# Summary

- Primary voters do not make legislature more extreme by forcing in extreme candidates.

- The general election is a huge force for moderation.

# Elections: A Limited Force For Moderation

- U.S. House elections select "moderate extremists."
- Argument: Differential entry of extremist candidates forces voters to elect extremists.

# Fun With Related Work


Hall and Snyder. 2013. Candidate Ideology and Electoral Success. Working Paper.

Eggers, Andrew, Anthony Fowler, Jens Hainmueller, Andrew B. Hall, and James M. Snyder, Jr. On the Validity of the Regression Discontinuity Design for Estimating Electoral Effects: Evidence From Over 40,000 Close Races. *American Journal of Political Science*, 2015.

Hall, Andrew B. "What Happens When Extremists Win Primaries?" *American Political Science Review*. 2015.

# This Week in Review

- This week we covered approaches to unmeasured confounding
- The trick is to exploit some other feature but there is No Free Lunch.
- Now that you have seen a few examples, hopefully you can be on the lookout for your own research.
- We talked about natural experiments, instrumental variables and regression discontinuity.

Next Week: Causality with Repeated Data