Week 11: Causality with Unmeasured Confounding

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Princeton

December 5 and 7, 2016

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

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

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- Last Week
  - selection on observables and measured confounding
- This Week
  - Monday:
    - natural experiments
    - classical view of instrumental variables
  - Wednesday:
    - modern view of instrumental variables
    - regression discontinuity
- The Following Week
  - repeated observations
- Long Run
  - causality with measured confounding $\rightarrow$ unmeasured confounding $\rightarrow$ repeated data

Questions?

Unmeasured Confounding

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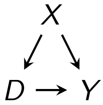

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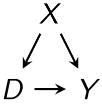

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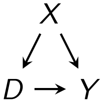

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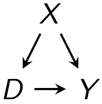

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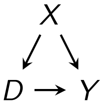

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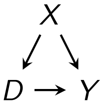

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- Under selection on unobservables we are going to need a different approach which we will talk about over the next two weeks.
- No Free Lunch $\rightsquigarrow$ we can't get something for nothing, we will need new variables, new assumptions and new approaches.
- Goal: give you a feel for what is possible, but note that you will need to do more research if you want to use one of these techniques.

Approaches to Unmeasured Confounding

- Natural Experiments

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Approaches to Unmeasured Confounding

- Natural Experiments (today)
- Interrupted Time-Series (today)
- Instrumental Variables (today and Wednesday)
- Regression Discontinuity (Wednesday)
- Bounding
- Sensitivity Analysis
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- When available, an excellent way to capitalize on randomness in the world to make casual inferences.
- See Dunning (2012) *Natural Experiments in the Social Sciences*

Caution on terminology

It is worth nothing 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)

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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

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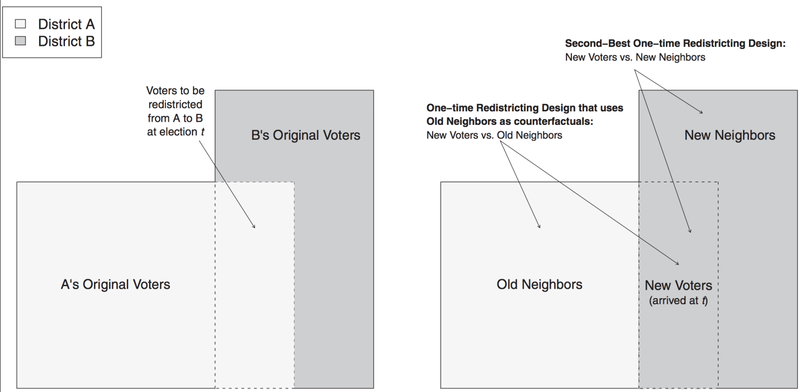
2. "if not, what is the comparison that is guaranteed by the randomization, and how does this comparison relate to the comparison the researcher wishes to make?

Example

Example from Sekhon and Titiunik (2012) discussion of Ansolabehere, Snyder and Stewart (2000)

FIGURE 1. Illustration of Redistricting Research Designs



(a) Before One-time Redistricting (b) After One-time Redistricting

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- The result only applies to a limited population that we may not care a great deal about.
- Be sure to verify that you “as-if-random” assignment is really random (e.g. placebo tests, balance tests)
- Convincingly analyzing a natural experiment takes at least as much careful thought not less!

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- 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 (2017) argues that with always-on digital data collection we will be in better shape moving forward to leverage natural experiments as the opportunities arise.

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$$Y_t = f(t) + D_t\beta + \epsilon_t$$

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

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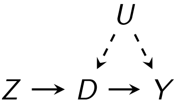
- It is going to turn out that the same construction will let us deal with non-compliance in experiments.

Graphical Model



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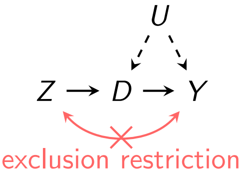

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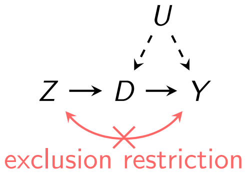

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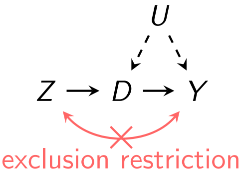

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- First-stage relationship Z affects D

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)
- Levitt (1997): being an election year as IV for police force size (crime 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, Sen (2015): cotton suitability as IV for proportion slave in 1860 (outcome is white attitudes today)

Core Idea

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Subject to an exclusion restriction you may be able to get (approximately) what you want anyway.

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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

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   - Constant treatment effects
   - Linearity in case of a multivalued treatment
2. Potential Outcome Model of IV
   - Heterogeneous treatment effects
   - Focus in Local Average Treatment Effect (LATE)

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2. We may not be able to measure X without error.

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

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$$U_i \sim_{i.i.d} N(0, \sigma_u^2).$$

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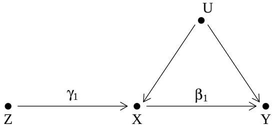
- omitted variables
- measurement error in X
- included variables (post-treatment or M-structures)
- simultaneous equations (endogenous feedback loops)

A Potential Solution: Instrumental Variables (IV)

$$Y_i = \beta_0 + \beta_1 X_i + U_i$$

$$X_i = \gamma_0 + \gamma_1 Z_i + U_i$$



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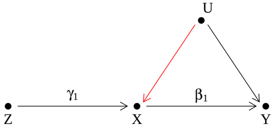

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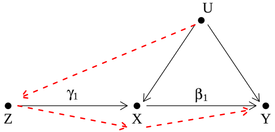

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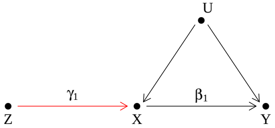
$$Cov[X_i, Z_i] \neq 0$$



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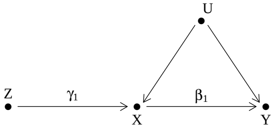

The IV Estimator

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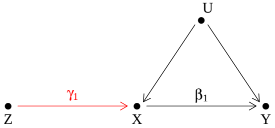

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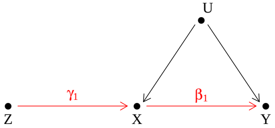

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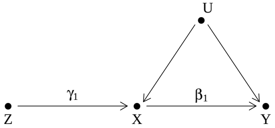
- $\frac{\widehat{\gamma_1 \cdot \beta_1}}{\widehat{\gamma_1}}$ identifies $\frac{\gamma_1 \cdot \beta_1}{\gamma_1} = \beta_1$



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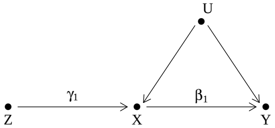

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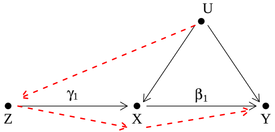
Therefore, if the instrument is weak ($\gamma_1 \approx 0$), and our estimates of γ_1 and $\gamma_1 \cdot \beta_1$ are not perfect, we can get inaccurate estimates of β_1:

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



Preview of Modern Approaches: Relaxing Constant Effects

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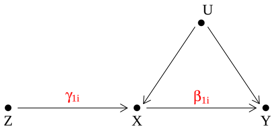
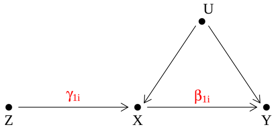
Suppose we believe that the effects of Z and X are different for different units.

$$Y_i = \beta_{0i} + \beta_{1i} X_i + U_i$$

$$X_i = \gamma_{0i} + \gamma_{1i} Z_i + V_i$$



IV Estimator with Heterogeneous Effects



- regressing X on Z now only identifies $\overline{\gamma}_1$

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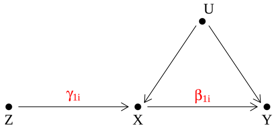

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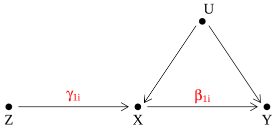

- regressing X on Z now only identifies $\overline{\gamma}_1$
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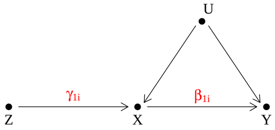

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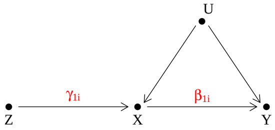

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With additional assumptions ($\gamma_{i1} \geq 0$ for all i), the IV estimator identifies a weighted average effect of X on Y according the effects of Z on X.

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E[\hat{\alpha}_{1,OLS}] &= \alpha_1 + E[\frac{Cov[D, u_2]}{Cov[D, D]}]
\end{aligned}$$

so bias depends on correlation between u and D

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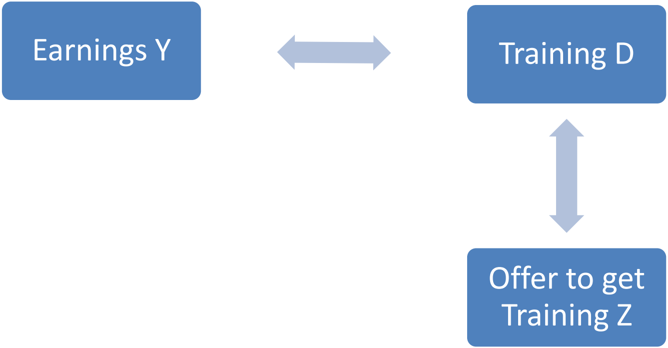
Instrumental Variable Estimator Assumptions



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Based on these IV assumptions we can identify three effects:

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

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\hat{\pi}_1 &= \pi_1 + \frac{Cov[Z, u_1]}{Cov[Z, Z]}
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E[\hat{\pi}_1] &= \pi_1 + E[\frac{Cov[Z, u_1]}{Cov[Z, Z]}] = \pi_1
\end{aligned}$$

$\hat{\pi}_1$ is consistent since $Cov[u_1, Z] = 0$

First Stage Effect in JTPA

First stage effect: Z on D: $\hat{\pi}_1 = \frac{Cov[D,Z]}{V[Z]}$

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

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- Second Stage: $Y = \alpha_0 + \alpha_1 D + u_2$
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Reduced Form/Intent-to-treat Effect: Z on Y: Plug first into second stage:

$$Y = \alpha_0 + \alpha_1(\pi_0 + \pi_1 Z + u_1) + u_2$$

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

$$\begin{aligned} Y &= \alpha_0 + \alpha_1(\pi_0 + \pi_1 Z + u_1) + u_2 \\ Y &= (\alpha_0 + \alpha_1\pi_0) + (\alpha_1\pi_1)Z + (\alpha_1 u_1 + u_2) \end{aligned}$$

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Y &= (\alpha_0 + \alpha_1\pi_0) + (\alpha_1\pi_1)Z + (\alpha_1 u_1 + u_2) \\
Y &= \gamma_0 + \gamma_1 Z + u_3
\end{aligned}
$$

where $\gamma_0 = \alpha_0 + \alpha_1\pi_0$, $\gamma_1 = \alpha_1\pi_1$, and $u_3 = \alpha_1 u_1 + u_2$.

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$$\hat{\gamma}_1 = \frac{Cov[Y, Z]}{Cov[Z, Z]} = \frac{Cov[\gamma_0 + \gamma_1 Z + u_3, Z]}{Cov[Z, Z]}$$

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\hat{\gamma}_1 &= \frac{Cov[Y, Z]}{Cov[Z, Z]} = \frac{Cov[\gamma_0 + \gamma_1 Z + u_3, Z]}{Cov[Z, Z]} \\
E[\hat{\gamma}_1] &= \gamma_1 + E[\frac{Cov[Z, u_3]}{Cov[Z, Z]}] = \gamma_1
\end{aligned}
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$\hat{\gamma}_1$ is consistent since $Cov[u_1, Z] = 0$ and $Cov[u_2, Z] = 0$ implies $Cov[u_3, Z] = 0$

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

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$\hat{\alpha}_1$ is consistent if $Cov[u_2, Z] = 0$ but has a bias which decreases with instrument strength.

Instrumental Variable Effect: Wald Estimator

Instrumental Variable Effect: $\alpha_1 = \frac{\text{Effect of Z on Y}}{\text{Effect of Z on D}} = \frac{Cov[Y,Z]}{Cov[D,Z]}$

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

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The instrumental variable estimator:

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is numerically equivalent to the following two step procedure:

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- α_1 is solely identified based on variation in D 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 $\hat{\pi}_0$ and $\hat{\pi}_1$.

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

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

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so to obtain consistent estimates the instrument Z must be:

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- Failure of either condition is a problem! But both conditions can be hard to satisfy at the same time. There often is a tradeoff.

Instrumental Variable Examples

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

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- Finite sample bias can be considerable (e.g., 20 - 30%), even when the sample size is over 100,000 if the instrument is weak
- Relative bias of $\alpha_{D,IV}$ versus $\alpha_{D,OLS}$ is approximately $1/F$ where F is the F-statistic for testing H_0: $\pi_Z = 0$, i.e. partial effect of Z on D is zero (or against joint zero for multiple instruments)

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- There are more complex competitors to 2SLS:
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  - Imbens and Rosenbaum (2005) robust IV.
- Small sample studies suggest that LIML and robust IV may be superior to 2SLS in small samples (but remains open area of research)

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- SUTVA may be a concern as well

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> *"The general lesson is once again the ultimate futility of trying to avoid thinking about how and why things work"*
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> *"[there is a] risk [of] transforming the methodologic dream of avoiding unmeasured confounding into a nightmare of conflicting biased estimates"*
> *- Hernan and Robins (2006)*

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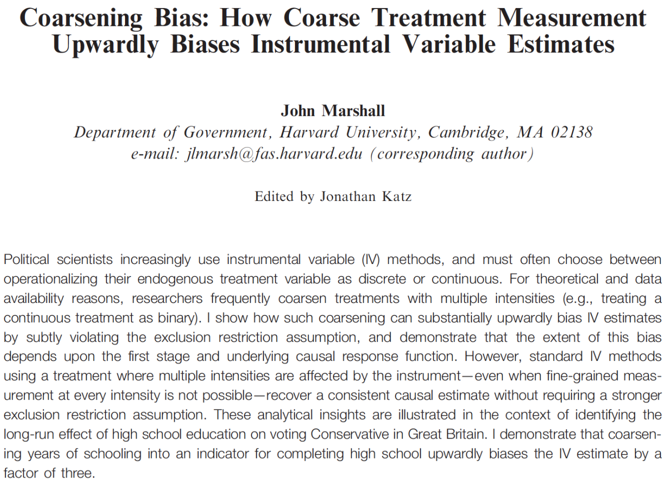
- So far, we have assumed constant treatment effects α_D which seems unrealistic in most settings. Often an instrument affects only a subpopulation of interest and we have little information about treatment effects for other units that may not be affected by the instrument at all.
- Next time we'll discuss modern IV with heterogeneous potential outcomes

Fun With Coarsening Bias


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

John Marshall
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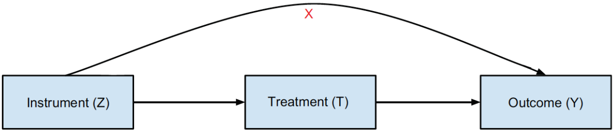
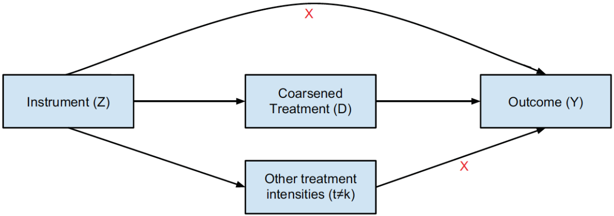
Edited by Jonathan Katz

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



(a) Weak exclusion restriction



(b) Strong exclusion restriction

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

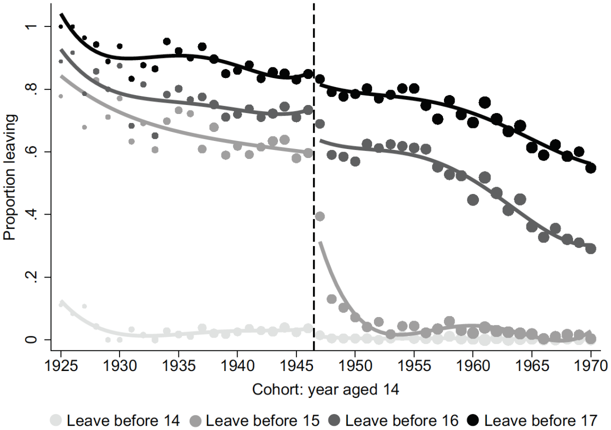


Fig. 3 1947 compulsory schooling reform and student leaving age by cohort.
Notes: Data are from the British Election Survey. Curves represent fourth-order polynomial fits. Gray dots are birth-year cohort averages, and their size reflects their weight in the sample.

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!)

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

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- Last Week
  - selection on observables and measured confounding
- This Week
  - Monday:
    - natural experiments
    - classical view of instrumental variables
  - Wednesday:
    - modern view of instrumental variables
    - regression discontinuity
- The Following Week
  - repeated observations
- Long Run
  - causality with measured confounding $\rightarrow$ unmeasured confounding $\rightarrow$ repeated data

Questions?

Identification with Traditional Instrumental Variables

- Two equations:
  - $Y = \gamma + \alpha D + \varepsilon$ (Second Stage)
  - $D = \tau + \rho Z + \eta$ (First Stage)
- Four Assumptions
  1. Exogeneity: $Cov(Z, \eta) = 0$
  2. Exclusion: $Cov(Z, \varepsilon) = 0$
  3. First Stage Relevance: $\rho \neq 0$
  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 $\alpha = Y_{s,i} - Y_{s-1,i}$.

Instrumental Variables and Potential Outcomes

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  - D_i not randomized, but Z_i is
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- D_i now depends on $Z_i \rightsquigarrow$ two potential treatments: $D_i(1) = D_i(z = 1)$ and $D_i(0)$.
- Outcome can depend on both the treatment and the instrument: $Y_i(d, z)$ is the outcome if unit i had received treatment $D_i = d$ and instrument value $Z_i = z$.

Potential Outcome Model for Instrumental Variables

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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)

D_z indicates potential treatment status given $Z = z$

- $D_1 = 1$ encouraged to take treatment and takes treatment

Assumption

Observed treatments are realized as

$$D = Z \cdot D_1 + (1 - Z) \cdot D_0 \text{ so } D_i = \begin{cases} D_{1i} & \text{if } Z_i = 1 \\ D_{0i} & \text{if } Z_i = 0 \end{cases}$$

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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 need the instrument to be randomized:

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

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- This assumption alone gets us the intent-to-treat (ITT) effect:

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

Assumption 2: Exclusion Restriction

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- This is testable by regressing D on Z
- Note that even a weak instrument can induce a lot of bias. Thus, for practical sample sizes you need a strong first stage effect.

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

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Only one of the potential treatment indicators (D_0, D_1) is observed, so in the general case we cannot identify which group any particular individual belongs to

Monotonicity means no defiers

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- Anyone with $D_i = 1$ when $Z_i = 0$ must be an always-taker and anyone with $D_i = 0$ when $Z_i = 1$ must be a never-taker.

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- 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.

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

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  - Control units are a mix of never takers and compliers.
- Without further assumptions, $\tau_{LATE} \neq \tau_{ATE}$.
- Complier group depends on the instrument $\rightsquigarrow$ different IVs will lead to different estimands.

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- Note: this can be very difficult to do practically in many settings.

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Proof.

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

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

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- 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)

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- Abadie (2003) shows how to use covariate information to calculate other characteristics of the complier group (kappa weighting)

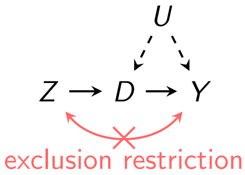
Size of Complier Group

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)	$P[D_1 > D_0 \mid D=0]$ (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.

Falsification tests



- The exclusion restriction cannot be tested directly, but it can be falsified.

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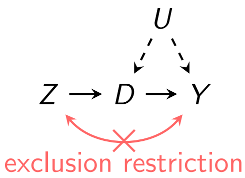

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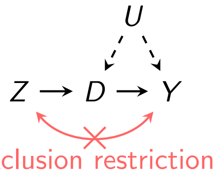

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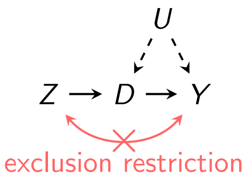

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- Nunn & Wantchekon (2011): use distance to coast as an instrument for Africans, use distance to the coast in an Asian sample as falsification test.

Nunn & Wantchekon falsification test

VOL. 101 NO. 7 NUNN AND WANTCHEKON: THE ORIGINS OF MISTRUST IN AFRICA 3243

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.

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  (if constant effects happen to hold, effects for compliers are by definition same as for entire population.)

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- Be sure to evaluate all conditions and remember randomization of Z does not guarantee the exclusion restriction.

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- 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).

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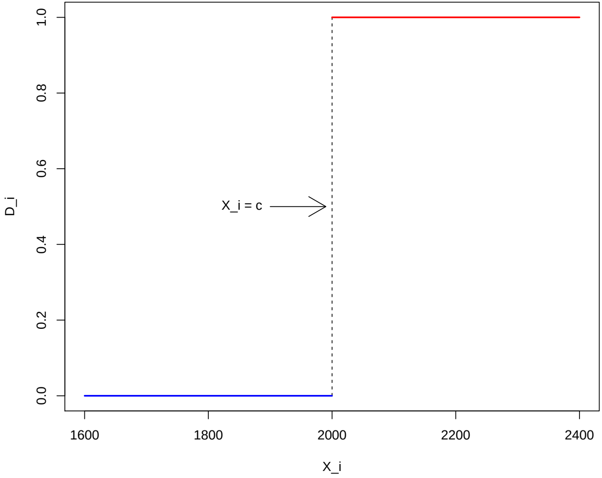
- X_i can be related to the potential outcomes and so comparing treated and untreated units does not provide causal estimates
- assume relationship between X and the potential outcomes Y_1 and Y_0 is smooth around the threshold $\rightsquigarrow$ discontinuity created by the treatment to estimate the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

Graphical Illustration



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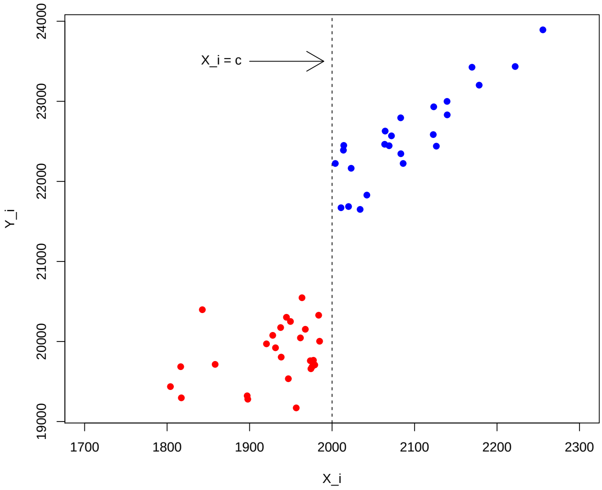

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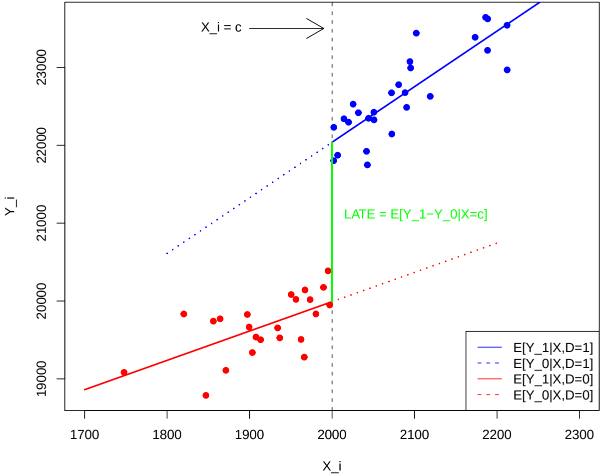

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- Intuitively, we are interested in the discontinuity in the outcome at the discontinuity in the treatment assignment.

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$$\mathbb{P}(D_i = 1 | X_i = c) = 1$$
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- 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: $\lim_{x \downarrow c} E[Y_i | X_i = x] - \lim_{x \uparrow c} E[Y_i | X_i = x]$

Design

- Sharp RD: treatment assignment is a deterministic function of the forcing variable and the threshold.
- Key assumption: no compliance problems (deterministic)
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- 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. $Y_1, Y_0 \perp\!\!\!\perp D | X$ *(trivially met)*
2. $0 < P(D = 1 | X = x) < 1$ *(always violated in Sharp RDD)*
3. $E[Y_1 | X, D]$ *and* $E[Y_0 | X, D]$ *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 α_{SRDD} *is only identified at the threshold.*

Extrapolation and smoothness

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

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

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

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- 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.

Example: Electronic Voting (Hidalgo 2012)

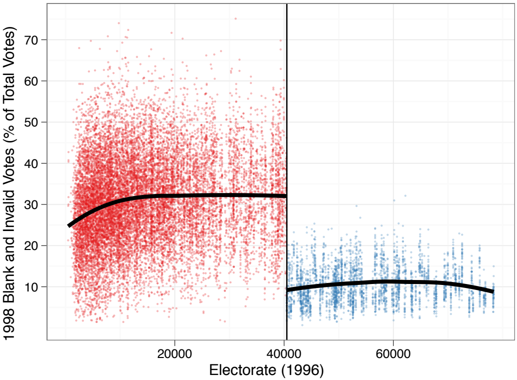

Figure 6: The effect of electronic voting on the percent of null and blank votes. Each dot is a polling station. Polling stations to the left of the vertical black line used paper ballots and polling stations to the right used electronic voting. The black horizontal line is the conditional mean of the outcome estimated with a loess regression.

Other 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

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- 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)

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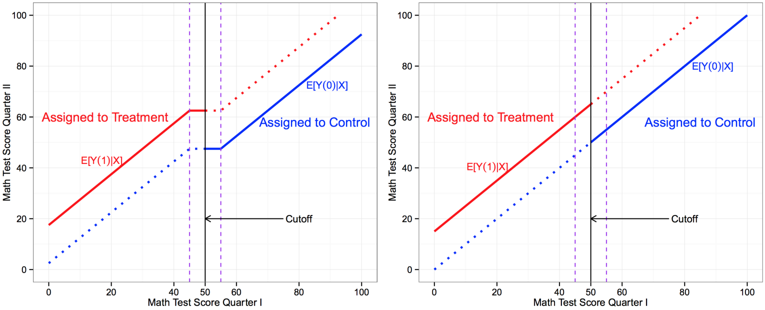
- This has caused a lot of confusion in the literature particularly in testing with background covariates
- Local statistical independence does not imply exclusion restriction (i.e. forcing variable not directly affecting the outcome)
- If you are doing an RDD: be sure to do balance checks and sensitivity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

Local Randomization vs. Continuity (Sekhon and Titiunik 2016)

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

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

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- 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!

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Fuzzy RD assumptions

Assumption 2: Monotoncity

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

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Assumption 3: Local Exogeneity of Forcing Variable

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Basically, in an ε-ball around c, the forcing variable is randomly assigned.

Example: Early Release Program (HDC)

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- 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

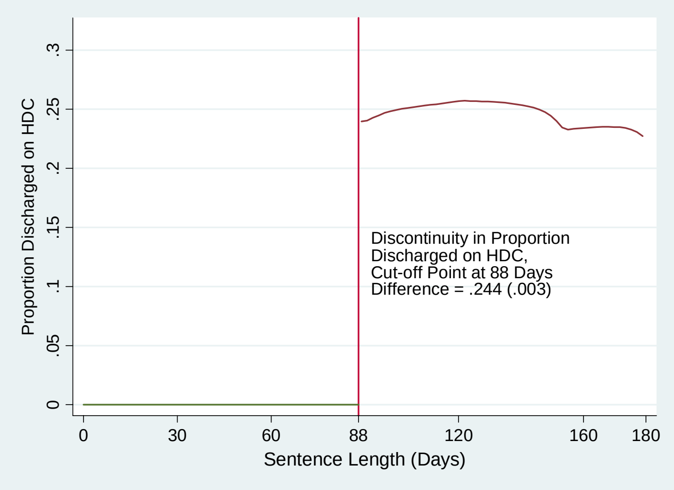
Example: Early Release Program (HDC)

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

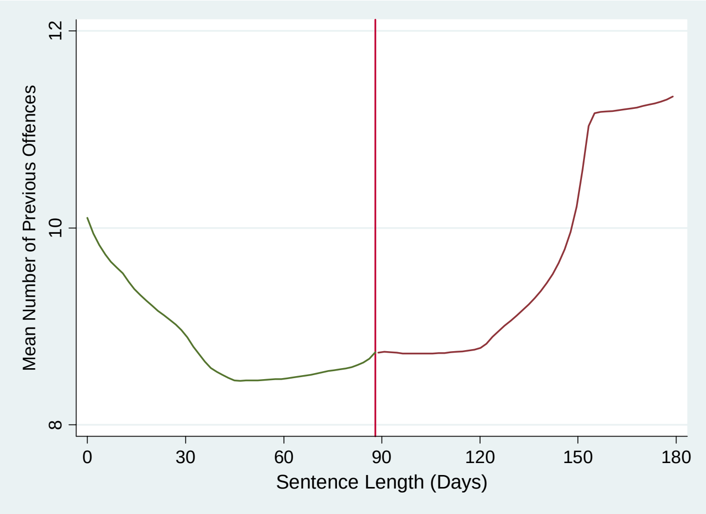
Example: Early Release Program (HDC)

Figure 1: Proportion Discharged on HDC by Sentence Length



Example: Early Release Program (HDC)

Figure 2: Mean Number of Previous Offence by Sentence Length



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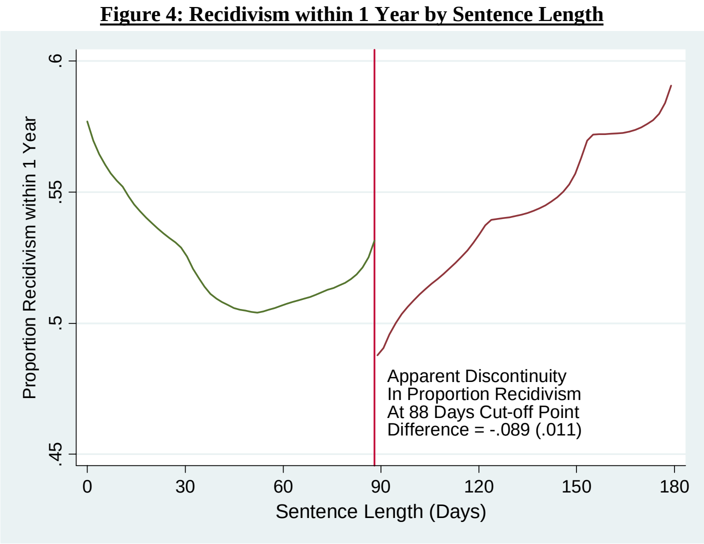
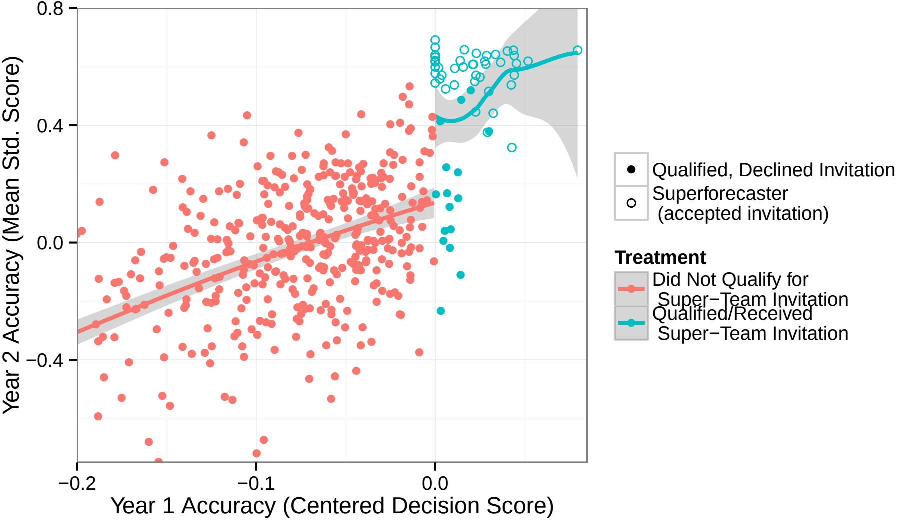


Example: Early Release Program (HDC)

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

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

Example: Teamwork



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- 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!

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- The trick is to exploit some other feature (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 we will talk about more designs for unmeasured confounding.

Next Week

- Causality with Repeated Data
- Reading
  - Angrist and Pishke Chapter 5 Parallel Worlds: Fixed Effects, Differences-in-Differences and Panel Data
  - Optional: Imai and Kim "When Should We Use Linear Fixed Effects Regression Models for Causal Inference with Longitudinal Data"
  - Optional: Angrist and Pishke Chapter 6 Regression Discontinuity Designs
  - Optional: Morgan and Winship Chapter 11 Repeated Observations and the Estimation of Causal Effects

References

- Abadie, Alberto. "Semiparametric instrumental variable estimation of treatment response models." *Journal of econometrics* 113, no. 2 (2003): 231-263.
- Angrist, Joshua D., and Jrn-Steffen Pischke. *Mostly harmless econometrics: An empiricist's companion*. Princeton university press, 2008.
- Morgan, Stephen L., and Christopher Winship. *Counterfactuals and causal inference*. Cambridge University Press, 2014.

Fun with Extremists

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?

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"...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:

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- Evaluates how the preferences of primary voters map to legislature.

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

Findings: Elections Strongly Prefer Moderates

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In the U.S. House, 1980–2010:

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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

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- Primary voters cannot force in extremists.
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- Argument of broader research project: candidate entry key to electing extremist legislators.

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- Compare elections and roll-call voting in "treated" districts vs. "control" districts.

Obstacle to Estimating Effects of Extremist Nominees

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Selection Bias.

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Selection Bias.

- Districts choose extremist nominees because they prefer them.

Close Primaries Offer Variation in Nominee Type

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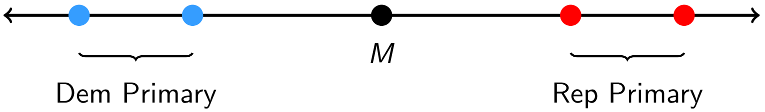
- Regression discontinuity design (RDD) in primary elections.
- Districts with moderate/extremist nominee otherwise identical in expectation.
- Key assumption for RDD: no sorting

"Extremists" Defined



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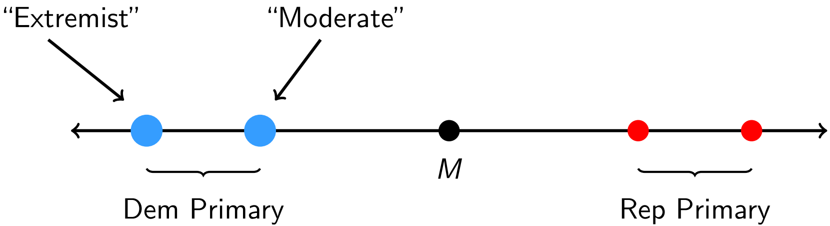

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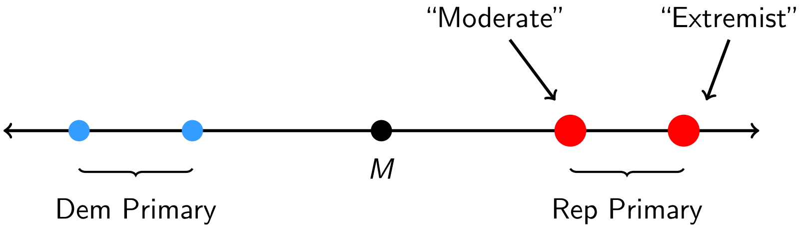

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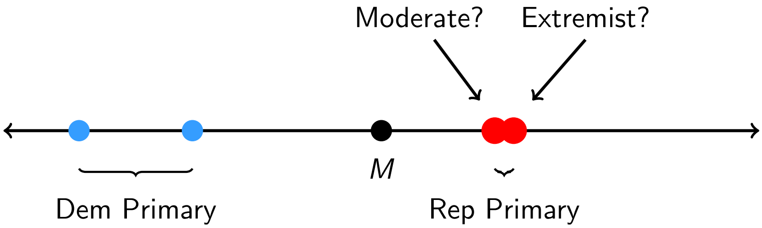

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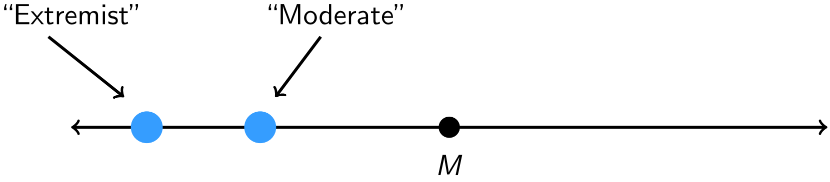

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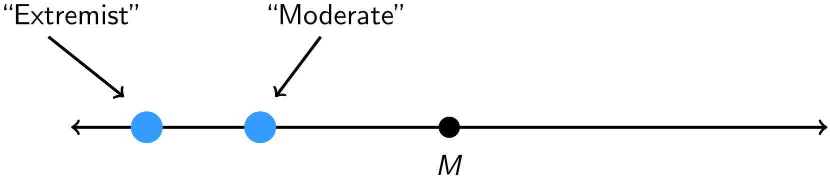

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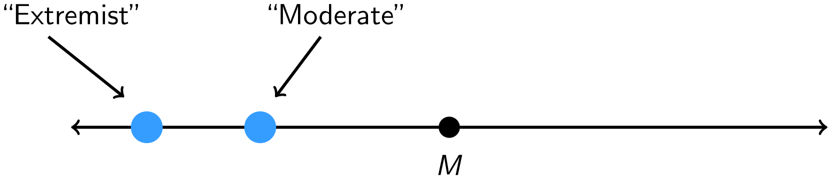

- Calculate distance between moderate and extremist.
- Use races where distance is at or above the median distance.

Quick Example: Robbie Wills vs. Joyce Elliott

Joyce Elliott: -0.33

vs.

Robbie Wills: -0.07

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- Wills sent out mailer calling Elliott an "extremist" who was "unelectable."

Quick Example: Robbie Wills vs. Joyce Elliott

Joyce Elliott: -0.33

vs.

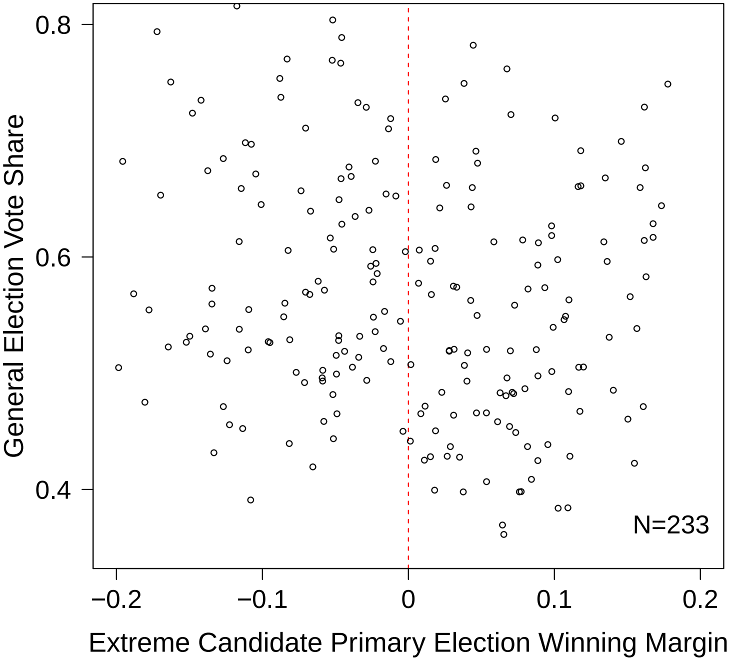
Robbie Wills: -0.07

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

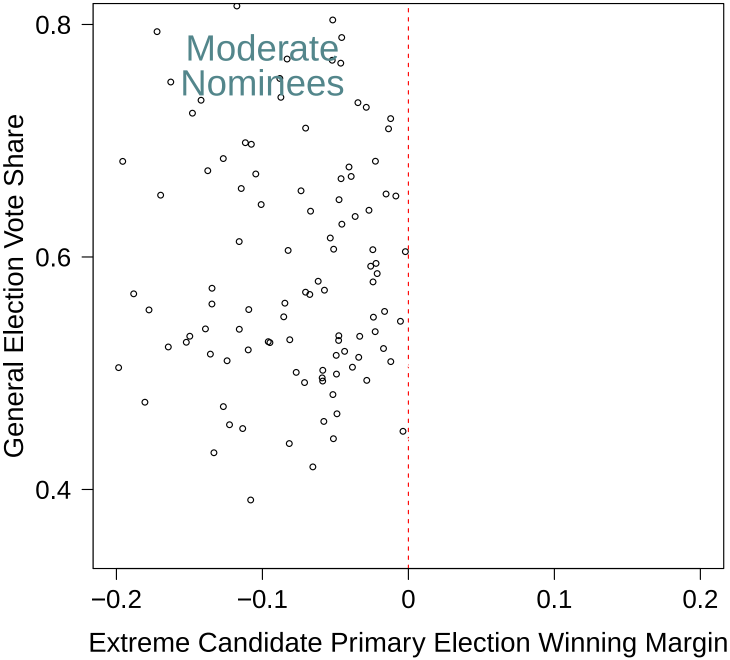
Estimating the RD: Effects of Extremist Nominations

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

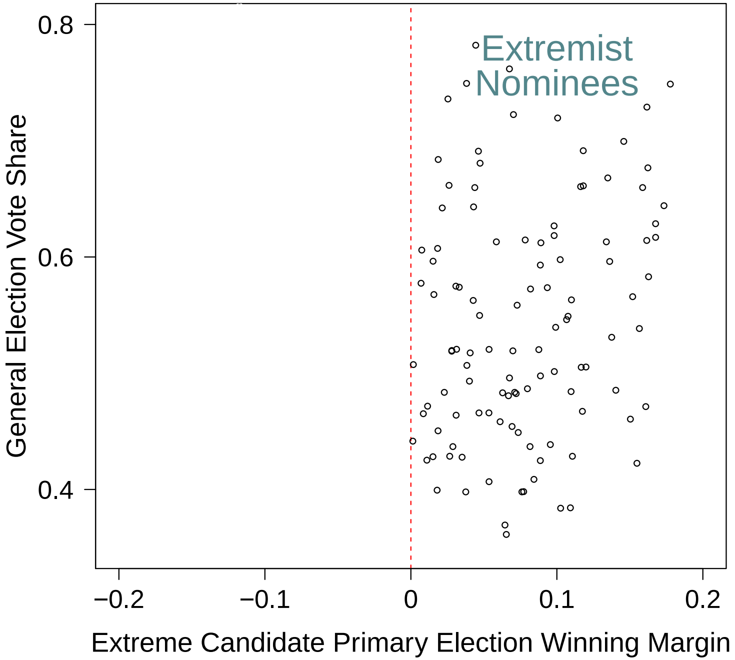
$V_{it} \equiv$ extremist candidate's vote-share winning margin.



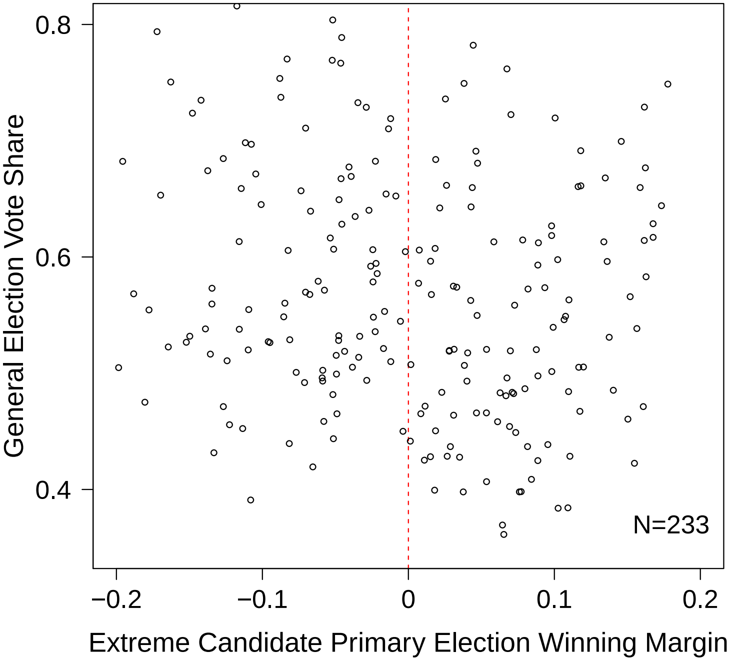
0.8
0.6
0.4
General Election Vote Share
N=233
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin



Moderate
Nominees
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



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



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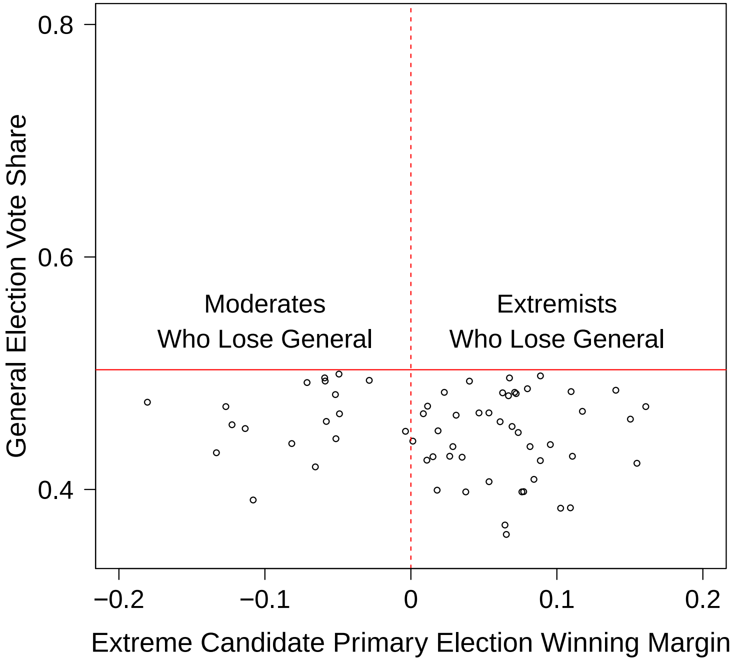
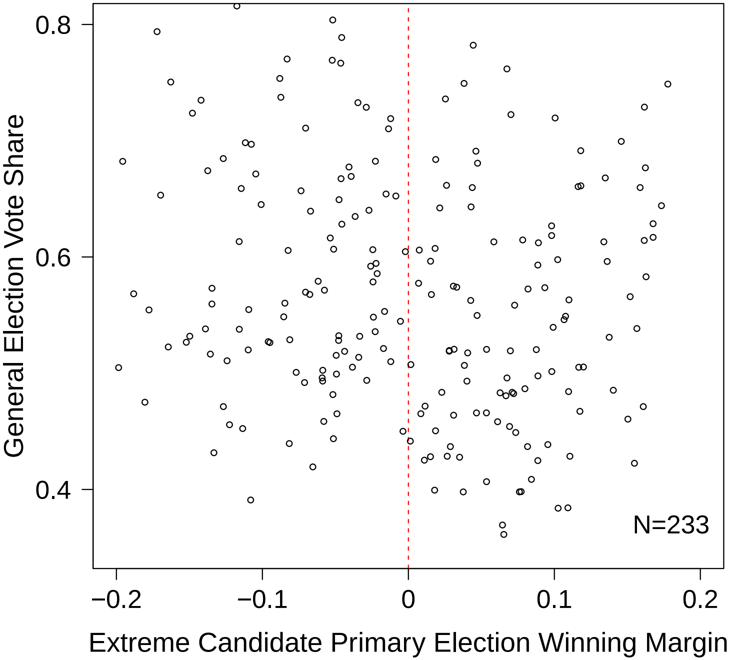


0.8
0.6
0.4
General Election Vote Share
Moderates
Who Lose General
Extremists
Who Lose General
−0.2
−0.1
0
0.1
0.2
Extreme Candidate Primary Election Winning Margin



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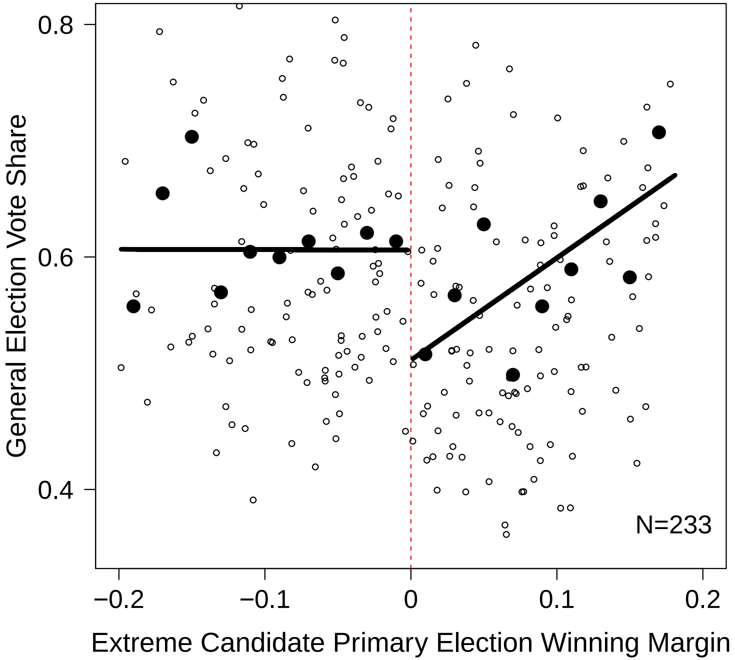

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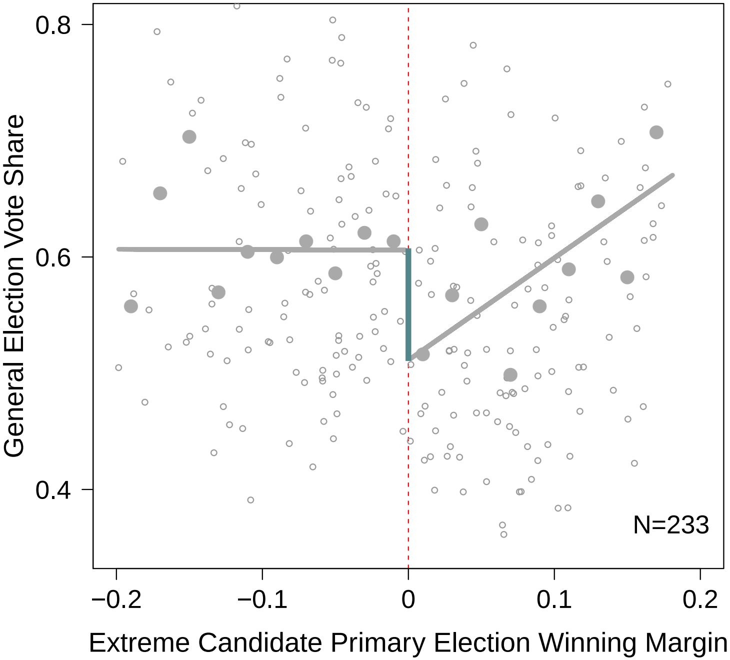

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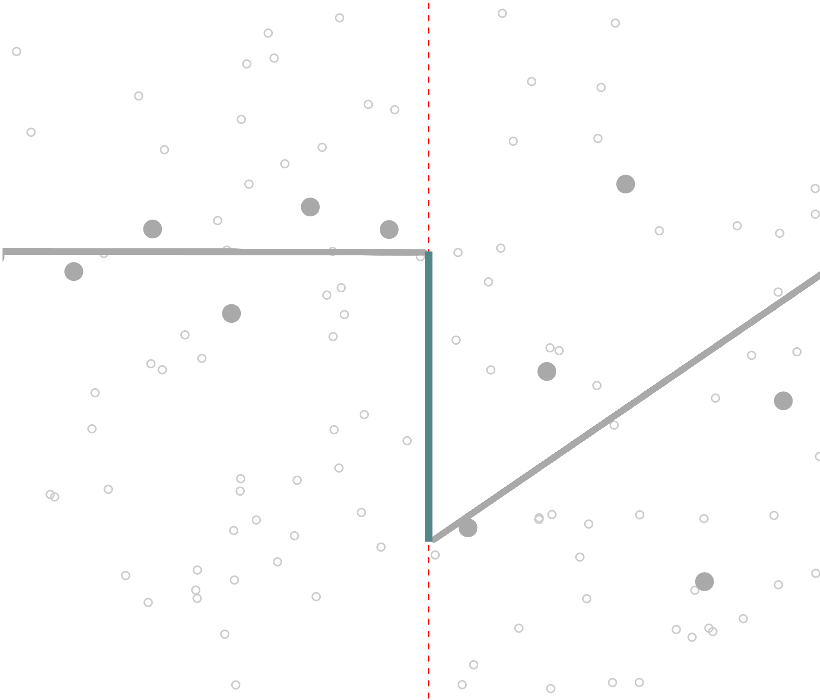

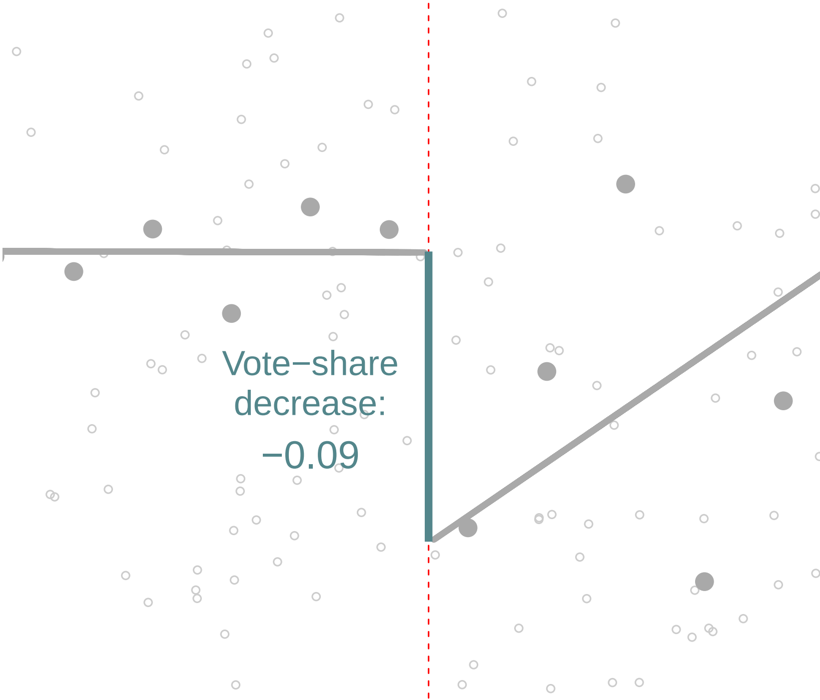
Vote−share
decrease:
−0.09

Large Electoral Penalty to Nominating Extremist

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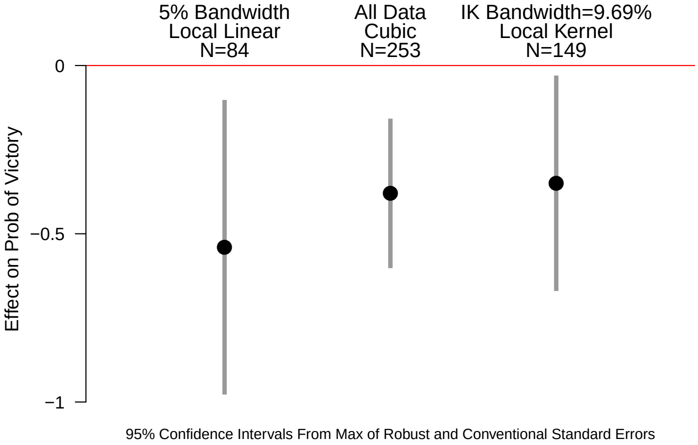

How Does Penalty to Extremists Affect Roll-Calls?

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Knowing general election prefers moderates not sufficient to understand tradeoff.

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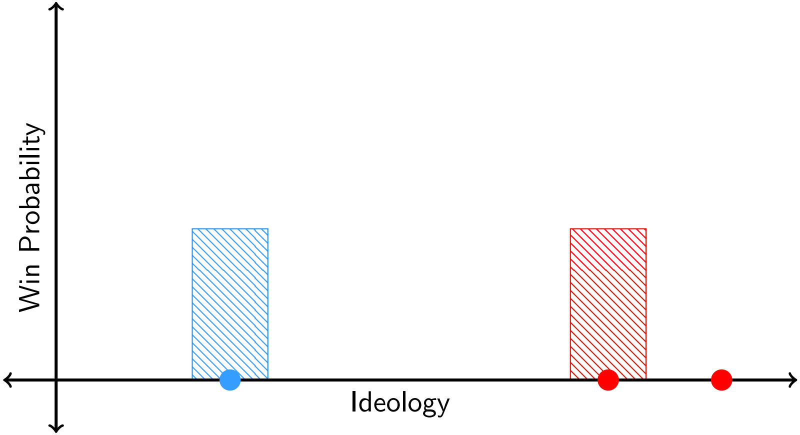

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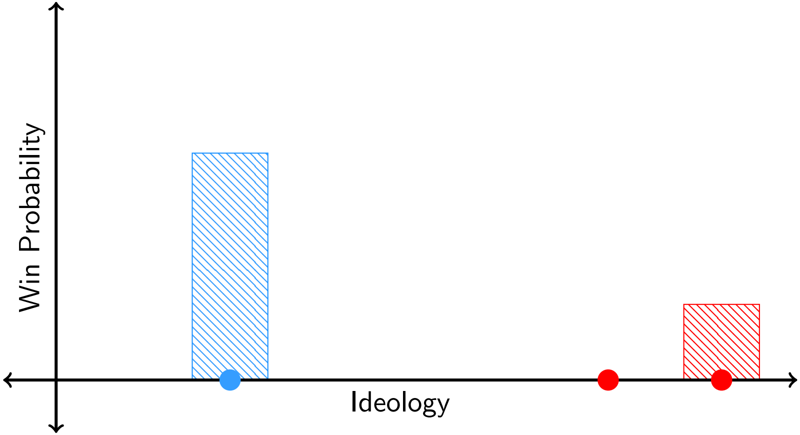
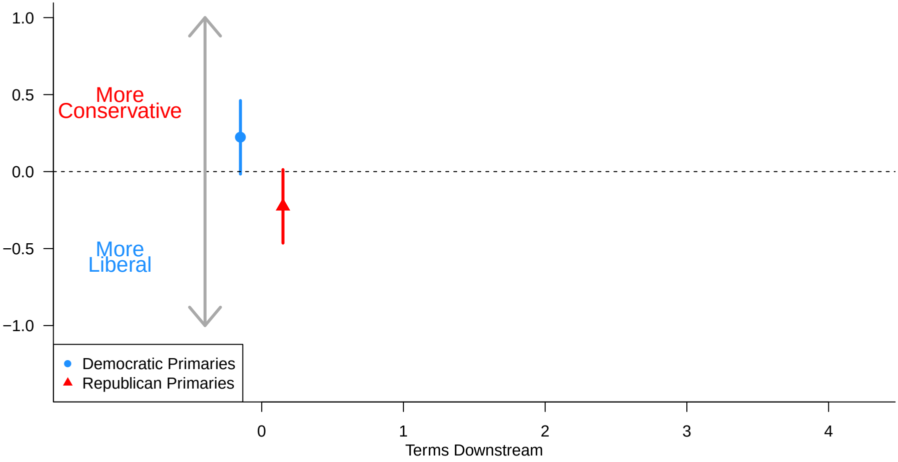


Effect of Extremists on Roll-Call Voting



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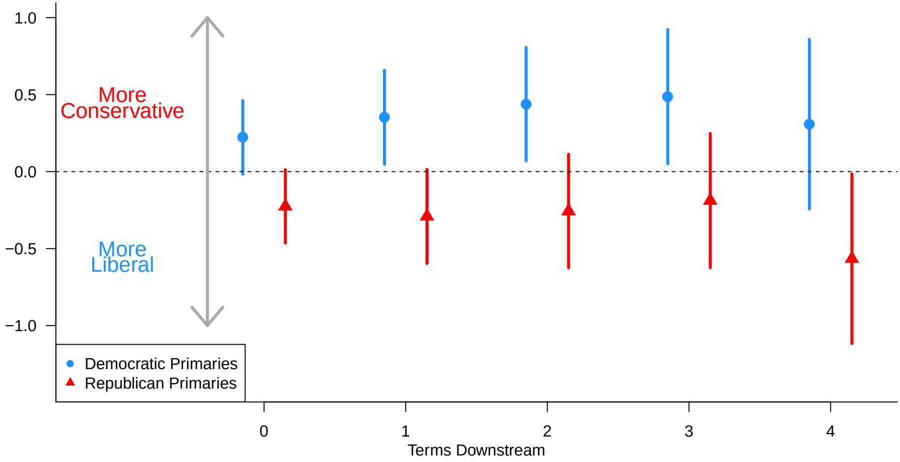

Summary

Summary

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

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- The general election is a huge force for moderation.

Elections: A Limited Force For Moderation

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- U.S. House elections select "moderate extremists."

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.

Fun With Weak Instruments

Ratkovic, Marc, and Yuki Shiraito. Strengthening Weak Instruments by Modeling Compliance. Working Paper.
(Thanks to Yuki Shiraito for sharing these slides with me)

Example: Endogeneity of Institution and Growth

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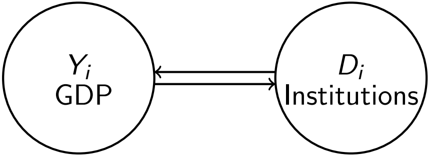
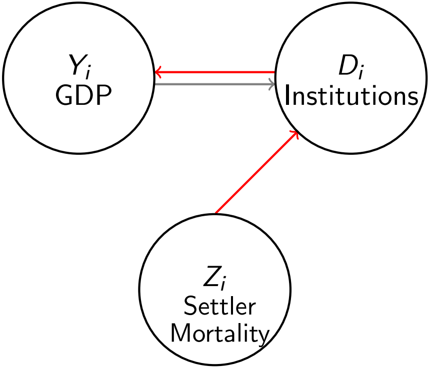
Endogeneity Bias



- Y_i and D_i have direct causal effects on each other
- Ordinary least squares biased

Solution of Acemoglu et.al. (2001)

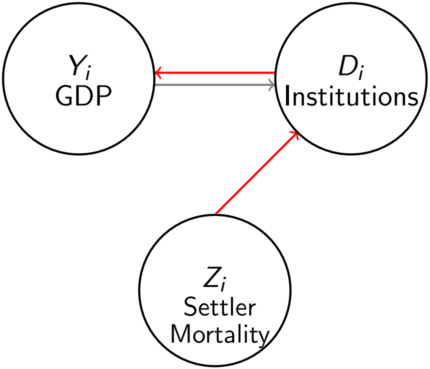
Instrumental Variable Analysis



- Exploiting exogeneity of Z_i

Solution of Acemoglu et.al. (2001)

Instrumental Variable Analysis



- Exploiting exogeneity of Z_i
- Validity: Settlers chose to build stronger (non-extractive) institutions in places they wanted to live

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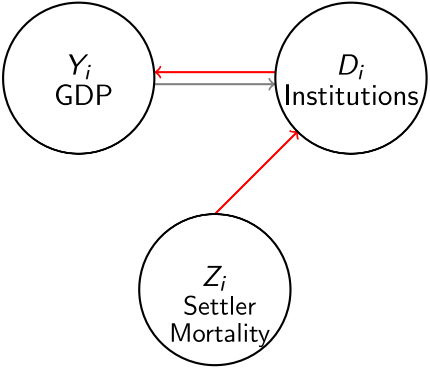

- Exploiting exogeneity of Z_i
- Validity: Settlers chose to build stronger (non-extractive) institutions in places they wanted to live
- Exclusion: But early settler mortality rate does not affect GDP directly

Addressing Endogeneity

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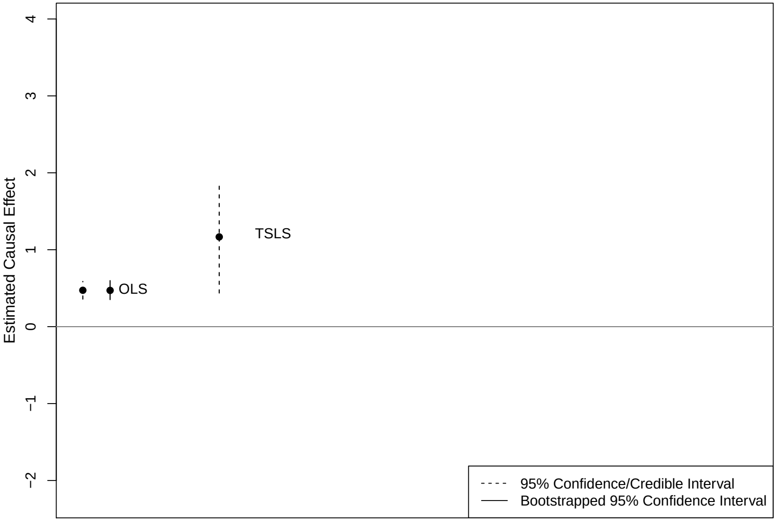
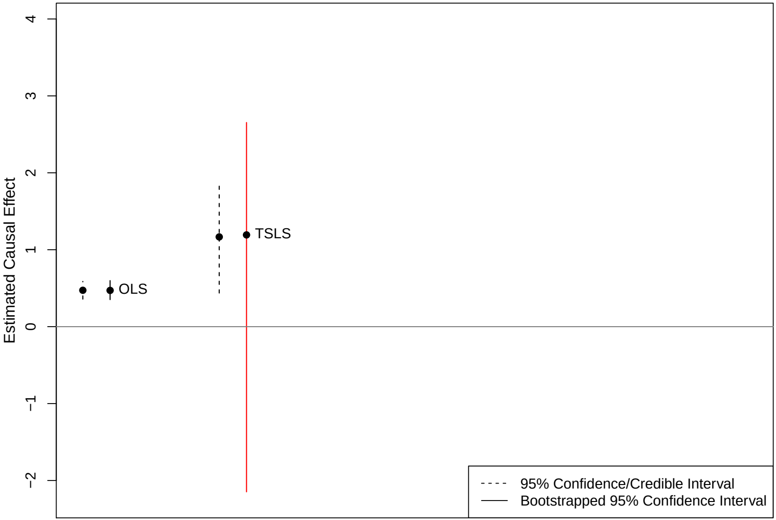
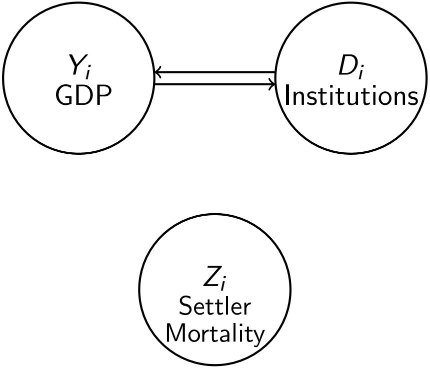


Problem: Bootstrapped Confidence Interval



Problem: Weak Instruments

Non-Compliers



- Non-compliers: For some countries, early settler mortality rate did not affect institutions

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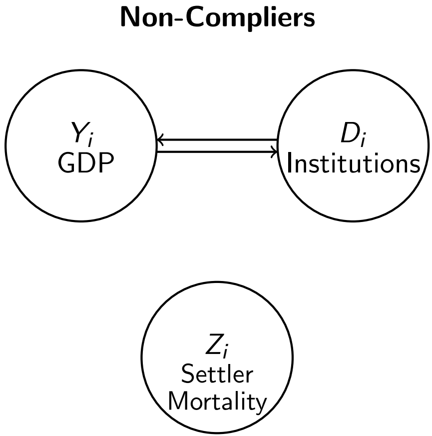

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$$\hat{\beta}_{IV} = \frac{\widehat{Cov(Y_i, D_i)}}{\underbrace{\widehat{Cov(D_i, Z_i)}}_{\approx 0}}$$

Problem: Weak Instruments

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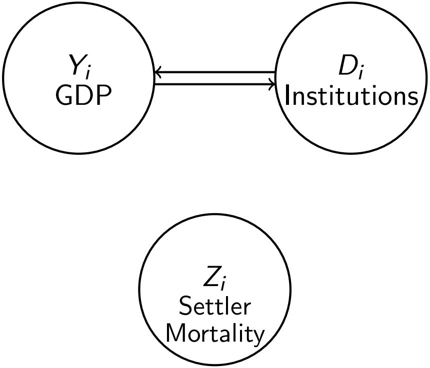

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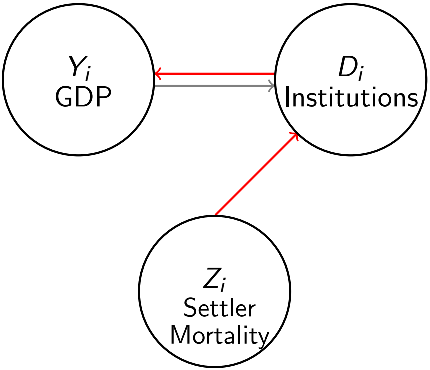
- Arbitrarily misleading estimates

Solution: Complier Instrumental Variable Estimation

Compliers



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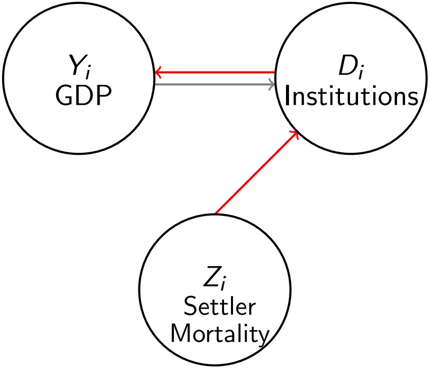

- Want to focus on observations where Z_i moves D_i, compliers

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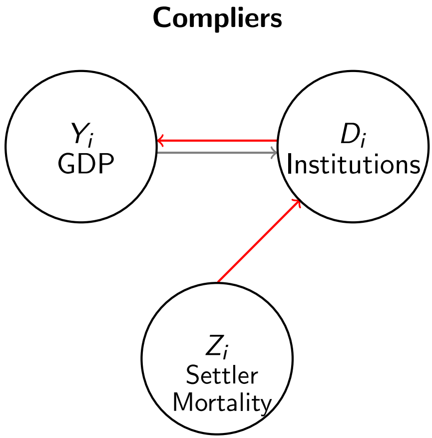

- Want to focus on observations where Z_i moves D_i, compliers
- Can't observe who are compliers

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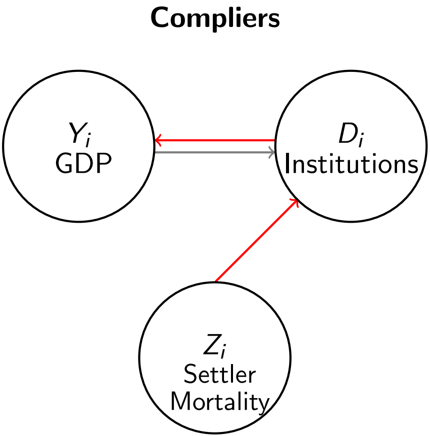

- Want to focus on observations where Z_i moves D_i, compliers
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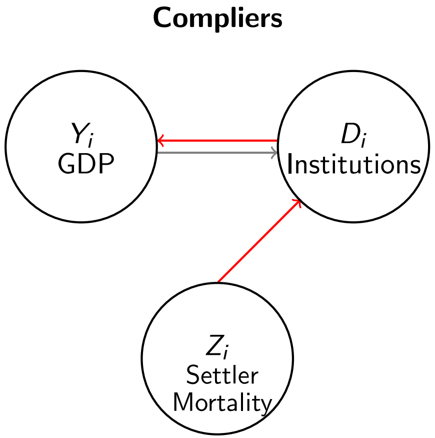

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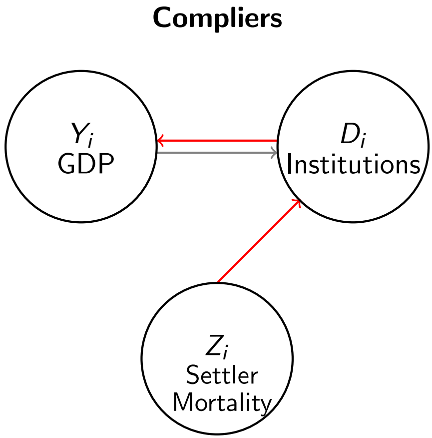

- Want to focus on observations where Z_i moves D_i, compliers
- Can't observe who are compliers
- Estimate latent variable indicating compliance
- Strengthen the instrument through upweighting compliers
- Connect finite mixture modeling of compliance with weak instrument problem (Hirano et.al. 2000)

IV as Simultaneous Equations

Standard model

$$D_i = Z_i^\top \delta + X_i^\top \theta + \eta_i \quad \text{(First Stage)}$$
$$Y_i = D_i^\top \beta + X_i^\top \gamma + \epsilon_i \quad \text{(Second Stage)}$$

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where, for a simple random sample of $i \in \{1, 2, \ldots, N\}$

- Y_i: Outcome
- D_i: Endogenous
- Z_i: Instrument
- X_i: Covariates
- ϵ_i, η_i: Normal errors
- $\text{Cov}(\epsilon_i, \eta_i) \neq 0$

CIV as Simultaneous Equations

CIV model

$$\Pr(C_i = 1) = \Phi\left(W_i^\top \alpha\right) \qquad \text{(Compliance Model)}$$

$$D_i = \begin{cases} \delta_0^C + Z_i^\top \delta + X_i^\top \theta + \eta_i; & C_i = 1 \\ \delta_0^{NC} + X_i^\top \theta + \eta_i; & C_i = 0 \end{cases} \qquad \text{(First Stage)}$$

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- Maximization not straightforward
- Gibbs sampler
- ECM algorithm in paper

Revisiting Acemoglu, Johnson, and Robinson (2001)

Causal effect of property rights on economic growth

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- Covariates: Latitude (absolute value); former French colony (0/1) or British colony (0/1); proportion citizens who are Catholic, Muslim, and neither; whether the country has a French legal origin (0/1)
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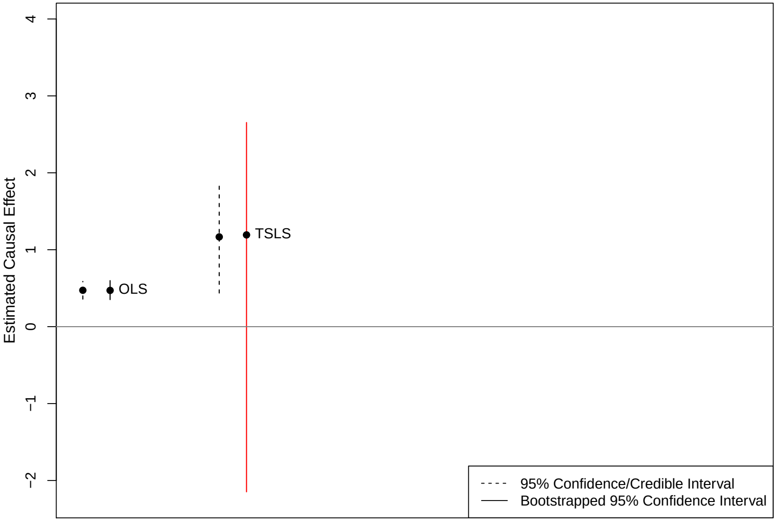
CIV for

- Strengthening a weak instrument
- Characterizing compliers

Causal Estimates



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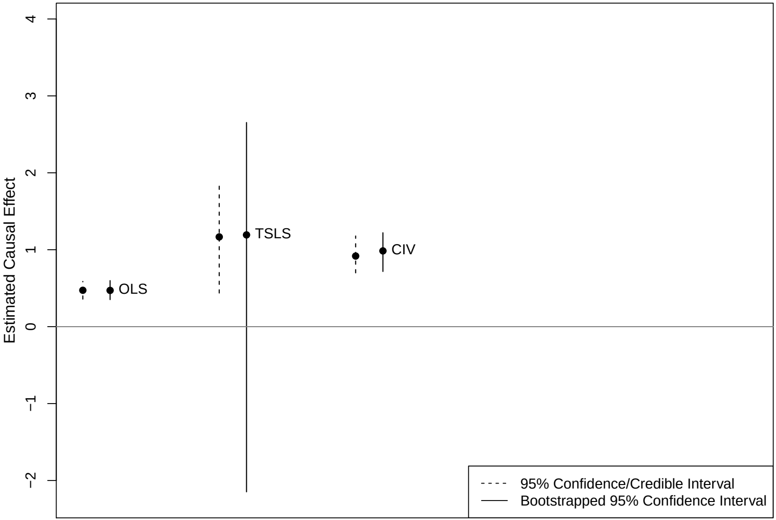

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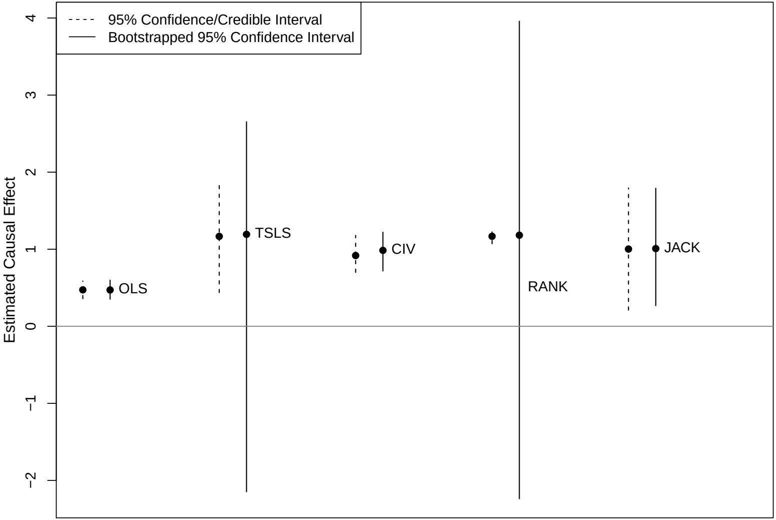
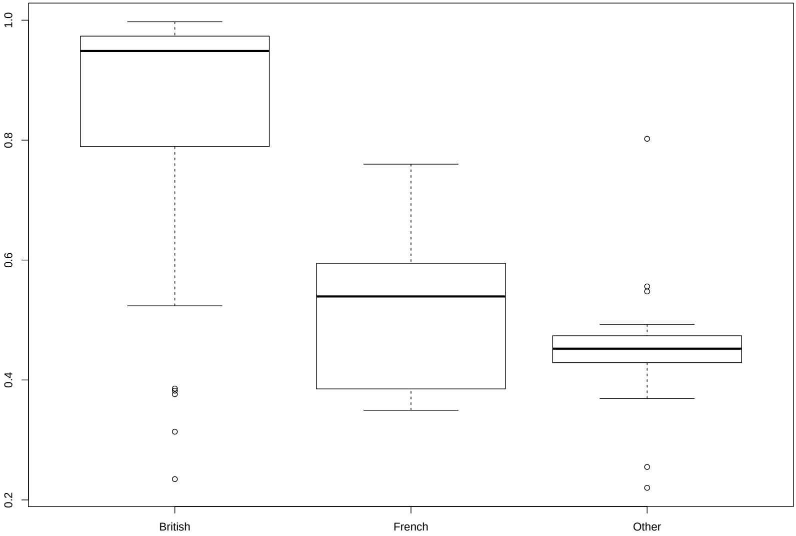


Compliance, by Colonizer



Fun With Conclusion

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  - Complier Instrumental Variable estimation
    1. Estimates latent compliance status
       $\rightarrow$ Characterize who are the compliers
    2. Upweights the compliers
       $\rightarrow$ Strengthens the instrument

Proof of the LATE theorem

- Under the exclusion restriction and randomization,

$$\begin{aligned} E[Y_i|Z_i = 1] &= E[Y_i(0) + (Y_i(1) - Y_i(0))D_i|Z_i = 1] \\ &= E[Y_i(0) + (Y_i(1) - Y_i(0))D_i(1)] \qquad \text{(randomization)} \end{aligned}$$

- The same applies to when $Z_i = 0$, so we have

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

- Thus, $E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] =$

$$\begin{aligned} &E[(Y_i(1) - Y_i(0))(D_i(1) - D_i(0))] \\ =&E[(Y_i(1) - Y_i(0))(1)|D_i(1) > D_i(0)] \Pr[D_i(1) > D_i(0)] \\ +&E[(Y_i(1) - Y_i(0))(-1)|D_i(1) < D_i(0)] \Pr[D_i(1) < D_i(0)] \end{aligned}$$

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

- Thus, $E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] =$

$$\begin{aligned} & E[(Y_i(1) - Y_i(0))(D_i(1) - D_i(0))] \\ =& E[(Y_i(1) - Y_i(0))(1)|D_i(1) > D_i(0)] \Pr[D_i(1) > D_i(0)] \\ +& E[(Y_i(1) - Y_i(0))(-1)|D_i(1) < D_i(0)] \Pr[D_i(1) < D_i(0)] \\ =& E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)] \Pr[D_i(1) > D_i(0)] \end{aligned}$$

Proof of the LATE theorem

- Under the exclusion restriction and randomization,

$$\begin{aligned} E[Y_i|Z_i = 1] &= E[Y_i(0) + (Y_i(1) - Y_i(0))D_i|Z_i = 1] \\ &= E[Y_i(0) + (Y_i(1) - Y_i(0))D_i(1)] \qquad \text{(randomization)} \end{aligned}$$

- The same applies to when $Z_i = 0$, so we have

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

- Thus, $E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] =$

$$\begin{aligned} &E[(Y_i(1) - Y_i(0))(D_i(1) - D_i(0))] \\ =&E[(Y_i(1) - Y_i(0))(1)|D_i(1) > D_i(0)] \Pr[D_i(1) > D_i(0)] \\ +&E[(Y_i(1) - Y_i(0))(-1)|D_i(1) < D_i(0)] \Pr[D_i(1) < D_i(0)] \\ =&E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)] \Pr[D_i(1) > D_i(0)] \end{aligned}$$

- The third equality comes from monotonicity: with this assumption, $D_i(1) < D_i(0)$ never occurs.

Proof (continued)

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)] \Pr[D_i(1) > D_i(0)]$$

- We can use the same argument for the denominator:

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

- Dividing these two expressions through gives the LATE.