Week 11: Causality with Unmeasured Confounding

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Princeton

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[1]Many slides are from Matt Blackwell, Chris Felton, Adam Glynn, Jens Hainmueller and Ian Lundberg.

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

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- Last Week
  - selection on observables and measured confounding
- This Week
  - natural experiments
  - instrumental variables
  - other approaches
- The Following Week
  - final week
  - reading discussion
- Long Run
  - probability → inference → regression → causal inference

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- 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."

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- 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)

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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**
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

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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?"

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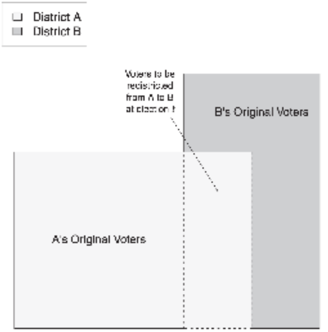

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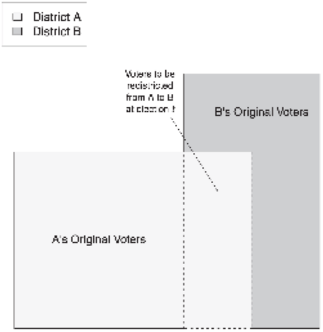

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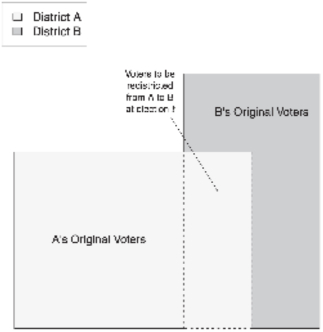

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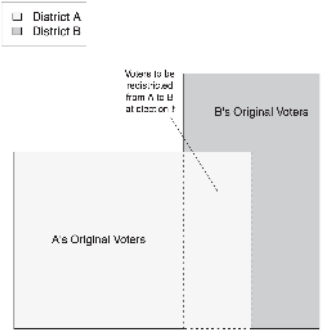
- idea is that two groups have same incumbent, same challenger, same campaign environment, but different histories with incumbent



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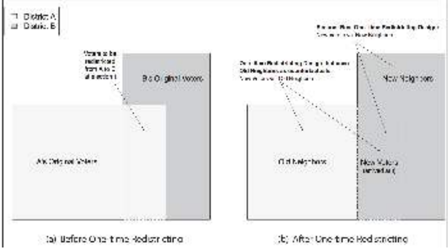

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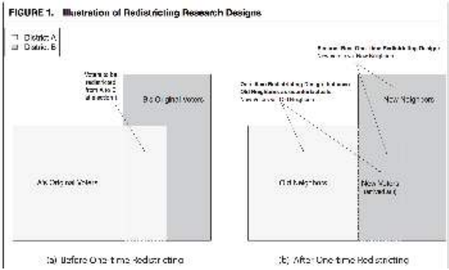

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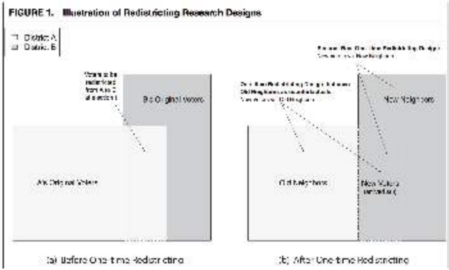

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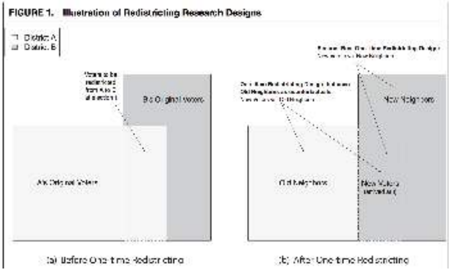

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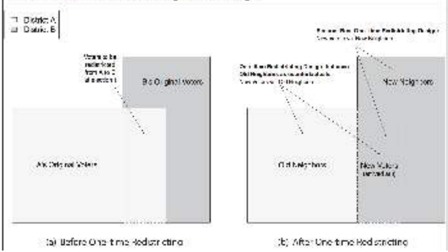

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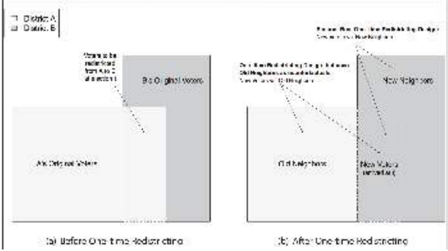

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    - random redistricting guarantees that old neighbors and new voters are comparable.
    - need to find a new design (see Sekhon and Titiunik 2012 for more)

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- Convincingly analyzing a natural experiment takes at least as much careful thought not less!

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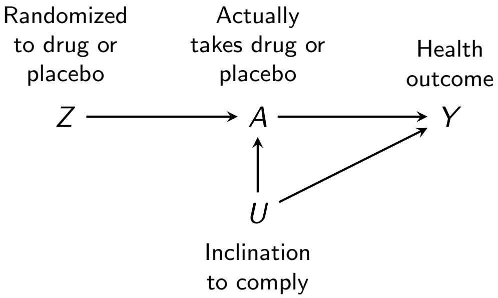
- It is often pretty easy to communicate these kinds of methods to non-experts
- Salganik (2018) argues that with always-on digital data collection we will be in better shape moving forward to leverage natural experiments as the opportunities arise.

Instrumental variables: Experiment with noncompliance



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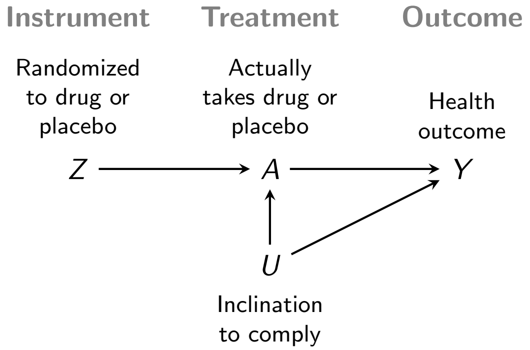

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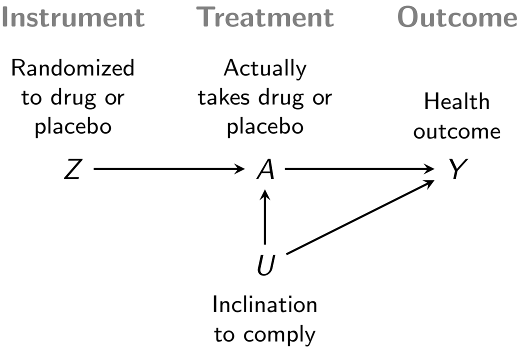
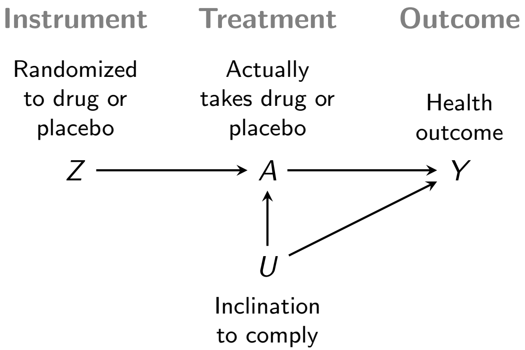


Two ideas

1. Intent to treat effect
2. Average effect among compliers

Instrumental variables: 1) Intent to treat effect



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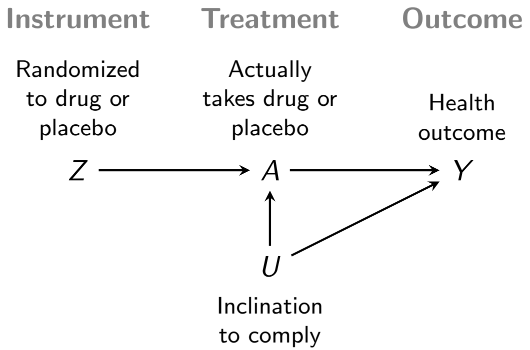

Ignore A. What is the effect of Z on Y?

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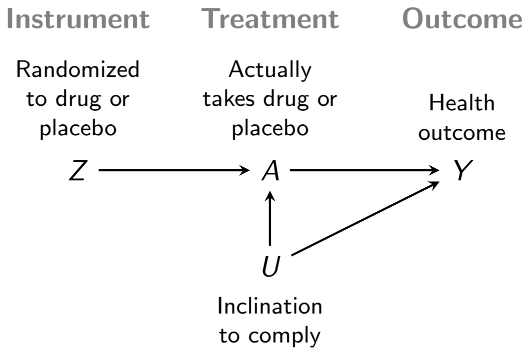

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$$E(\underset{\substack{\text{Outcome}\\ \text{under}\\ Z=1}}{Y^1} - \underset{\substack{\text{Outcome}\\ \text{under}\\ Z=0}}{Y^0})$$

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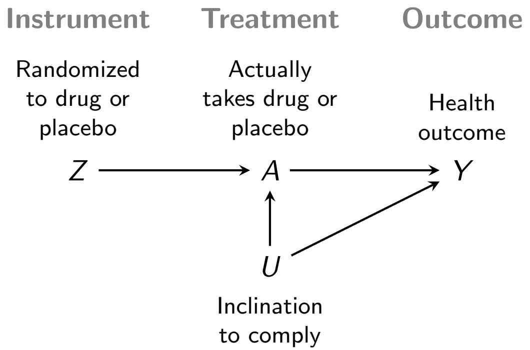

Ignore A. What is the effect of Z on Y?

$$E(Y^1 - Y^0) =$$

Outcome under $Z = 1$

Outcome under $Z = 0$

By Positivity Consistency Exchangeability for Z

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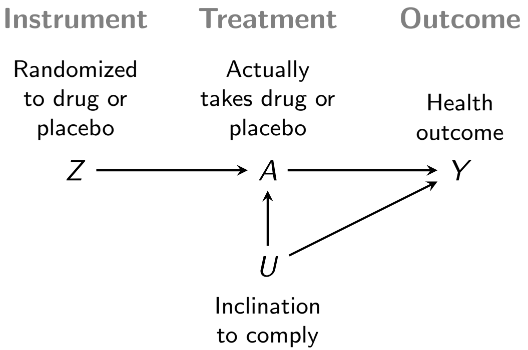

Ignore A. What is the effect of Z on Y?

$$E(Y^1 - Y^0) = E(Y \mid Z = 1) - E(Y \mid Z = 0)$$

Outcome under $Z = 1$

Outcome under $Z = 0$

By Positivity Consistency Exchangeability for Z

Mean difference in observable outcomes

Instrumental variables: 2) Average effect among compliers

Imbens, G., & Angrist, J. (1994). Identification and estimation of local average treatment effects. Econometrica, 62(2), 467-475.

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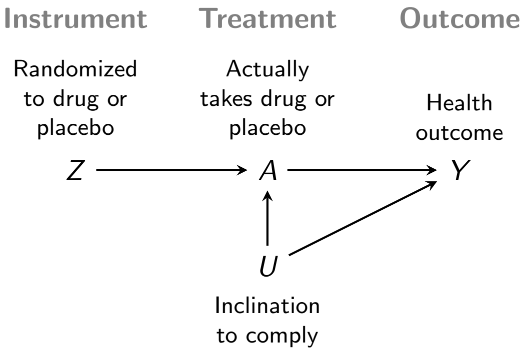

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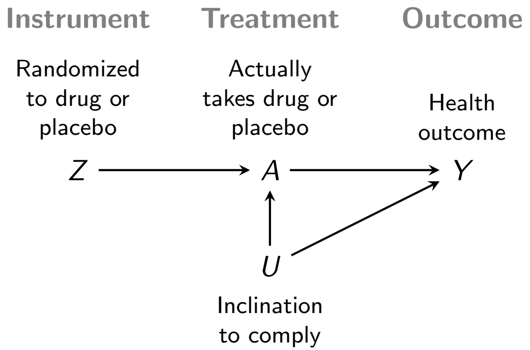
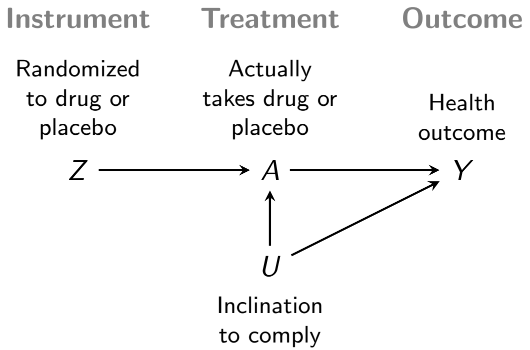


Key insight: The effect of Z on Y operates entirely through A

Instrumental variables: 2) Average effect among compliers



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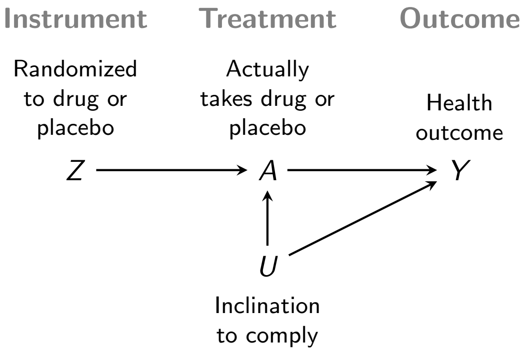
Instrumental variables: 2) Average effect among compliers



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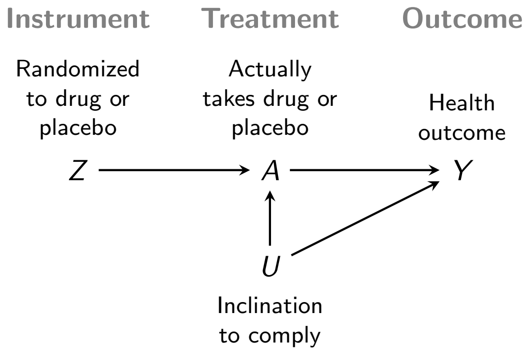

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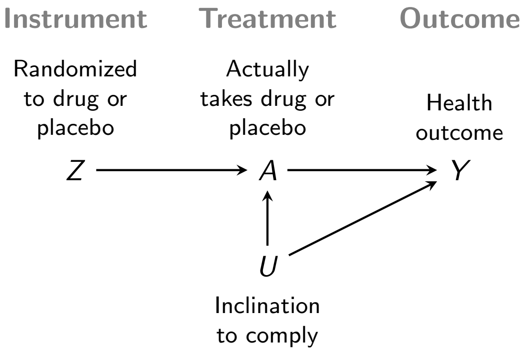
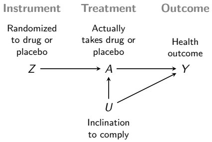


Key insight: The effect of Z on Y operates entirely through A

1. Study the effect of $Z \rightarrow Y$ (we just did)
2. Study the effect of $Z \rightarrow A$
3. Learn about $A \rightarrow Y$ since $Z \rightarrow Y$ is $Z \rightarrow A \rightarrow Y$

Instrumental variables: 2) Average effect among compliers



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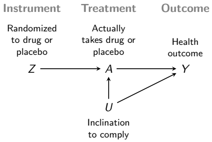

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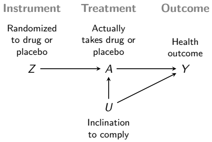

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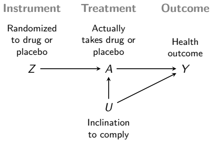
Compliers $\quad A^{Z=0} = 0 \quad A^{Z=1} = 1 \quad$ (follow assignment)

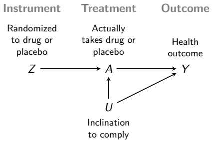
Instrumental variables: 2) Average effect among compliers



The effect $Z \to A$ has four **principal strata**:
latent sets of people who respond to Z a particular way

Compliers	$A^{Z=0} = 0$	$A^{Z=1} = 1$	(follow assignment)
Always takers	$A^{Z=0} = 1$	$A^{Z=1} = 1$	(always take treatment)

Instrumental variables: 2) Average effect among compliers



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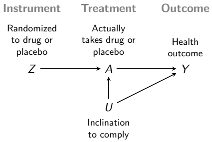

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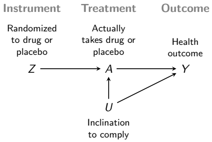
Instrumental variables: 2) Average effect among compliers



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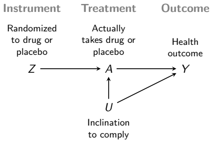
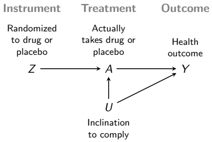
Discuss: In which strata is the effect $Z \to Y$ zero?

Instrumental variables: 2) Average effect among compliers



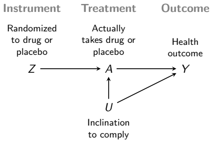
Among **always takers** and **never takers**,

Instrumental variables: 2) Average effect among compliers



Among **always takers** and **never takers**,
Z does not affect A

Instrumental variables: 2) Average effect among compliers



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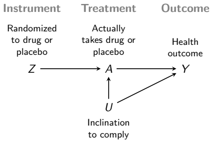

Among **always takers** and **never takers**,
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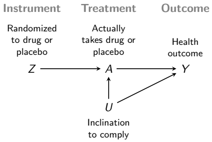
Z only affects Y through A

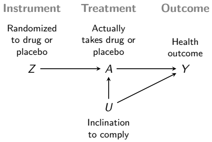
In these strata, Z does not affect Y

Instrumental variables: 2) Average effect among compliers



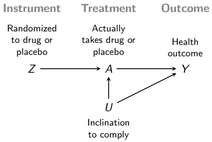
Among **compliers**,

Instrumental variables: 2) Average effect among compliers



Among **compliers**,
$Z = 1$ implies $A = 1$ and
$Z = 0$ implies $A = 0$

Instrumental variables: 2) Average effect among compliers



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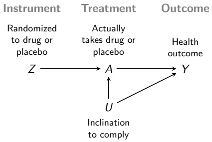

Among **compliers**,
$Z = 1$ implies $A = 1$ and
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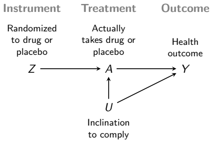
In these strata, $Z = A$

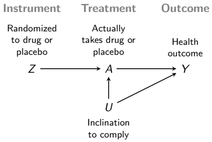
$Z \rightarrow Y$ and $A \rightarrow Y$ are the same

Instrumental variables: 2) Average effect among compliers



Among **defiers**,

Instrumental variables: 2) Average effect among compliers



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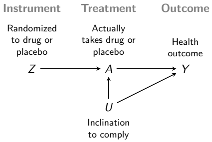

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In these strata, $Z = 1 - A$

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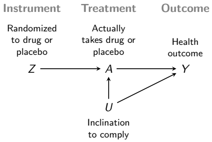

Among **defiers**,
$Z = 1$ implies $A = 0$ and
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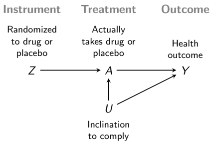
In these strata, $Z = 1 - A$

$Z \to Y$ and $A \to Y$ are the same magnitude
but have opposite signs

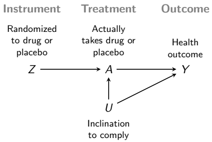
Instrumental variables: 2) Average effect among compliers



Four principal strata

Compliers	$(Z \to A) = +1$	$(Z \to Y) = (A \to Y)$
Always takers	$(Z \to A) = 0$	$(Z \to Y) = 0$
Never takers	$(Z \to A) = 0$	$(Z \to Y) = 0$
Defiers	$(Z \to A) = -1$	$(Z \to Y) = -(A \to Y)$

Instrumental variables: 2) Average effect among compliers



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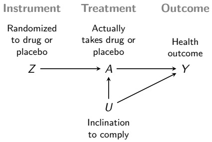
Assume **no defiers** in the population

Instrumental variables: 2) Average effect among compliers



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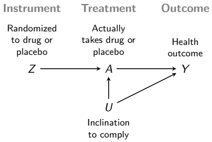

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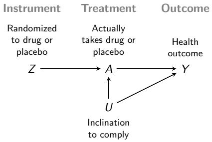
Instrumental variables: 2) Average effect among compliers



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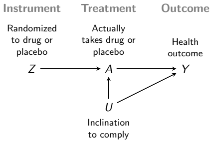
Hypothetical:

Population is 50% compliers, 25% always takers, 25% never takers

Instrumental variables: 2) Average effect among compliers



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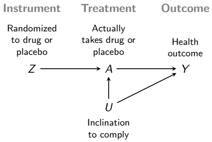
Hypothetical:

Population is 50% compliers, 25% always takers, 25% never takers

Average effect of $Z \to Y$ among compliers is 0.6

Instrumental variables: 2) Average effect among compliers



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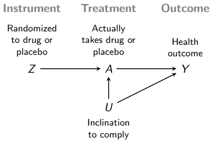
Hypothetical:
Population is 50% compliers, 25% always takers, 25% never takers
Average effect of $Z \to Y$ among compliers is 0.6

What is the average effect of $Z \to Y$ in the population?

Instrumental variables: 2) Average effect among compliers



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Hypothetical:

Population is 50% compliers, 25% always takers, 25% never takers
Average effect of $Z \to Y$ among compliers is 0.6

What is the average effect of $Z \to Y$ in the population? **0.3**

Instrumental variables: 2) Average effect among compliers

Deriving the general case:

Instrumental variables: 2) Average effect among compliers

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$$E(Y^{Z=1} - Y^{Z=0})$$

Instrumental variables: 2) Average effect among compliers

Deriving the general case:

$$
\begin{aligned}
&E(Y^{Z=1} - Y^{Z=0}) \\
&= \sum_s E(Y^{Z=1} - Y^{Z=0} \mid S = s) \underbrace{\mathbb{P}(S = s)}_{\substack{\text{Denote} \\ \pi_s}}
\end{aligned}
$$

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$$
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& = E(Y^{Z=1} - Y^{Z=0} \mid S = \text{Complier}) \pi_{\text{Complier}} \\
& \quad + E(Y^{Z=1} - Y^{Z=0} \mid S = \text{Always-Taker}) \pi_{\text{Always-Taker}} \\
& \quad + E(Y^{Z=1} - Y^{Z=0} \mid S = \text{Never-Taker}) \pi_{\text{Never-Taker}} \\
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&= E(Y^{Z=1} - Y^{Z=0} \mid S = \text{Complier})\pi_{\text{Complier}} \\
&\qquad + E(Y^{Z=1} - Y^{Z=0} \mid S = \text{Always-Taker})\pi_{\text{Always-Taker}} \qquad (= 0) \\
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$$

Instrumental variables: 2) Average effect among compliers

$$E(Y^{Z=1} - Y^{Z=0}) = E(Y^{Z=1} - Y^{Z=0} \mid S = \text{Complier})\pi_{\text{Complier}}$$

Instrumental variables: 2) Average effect among compliers

Average effect of instrument	=	Average among compliers	×	Proportion who comply

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Rearrange to get the complier average treatment effect

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$$E(Y^{A=1} - Y^{A=0} \mid S = \text{Complier}) = \frac{E(Y^{Z=1} - Y^{Z=0})}{\pi_{\text{Complier}}}$$

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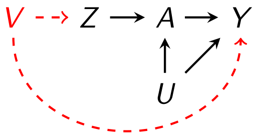
$$\begin{aligned} E(Y^{A=1} - Y^{A=0} \mid S = \text{Complier}) &= \frac{E(Y^{Z=1} - Y^{Z=0})}{\pi_{\text{Complier}}} \\ &= \frac{E(Y^{Z=1} - Y^{Z=0})}{E(A^{Z=1} - A^{Z=0})} \end{aligned}$$

Three assumptions that make IV work

$$\begin{tikzcd} Z \arrow[r] & A \arrow[r] & Y \\ & U \arrow[u] \arrow[ur] & \end{tikzcd}$$

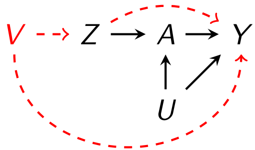
Three assumptions that make IV work



Exogeneity:
Instrument must be
unconfounded
(this path cannot exist)

Three assumptions that make IV work

Exclusion Restriction:
Instrument must affect outcome only through treatment
(this edge cannot exist)



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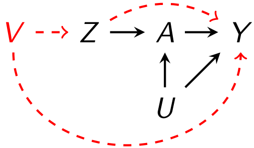

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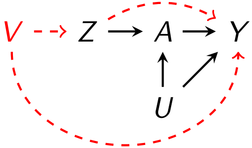

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Monotonicity:
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Given these extra assumptions,
the average effect among compliers
is identified by
$E(Y^{A=1} - Y^{A=0} \mid S = \text{Complier})$

$= \frac{E(Y^{Z=1} - Y^{Z=0})}{E(A^{Z=1} - A^{Z=0})}$ $\begin{array}{l} \leftarrow \text{Effect of } Z \text{ on } Y \\ \leftarrow \text{Effect of } Z \text{ on } A \end{array}$

$= \frac{E(Y \mid Z=1) - E(Y \mid Z=0)}{E(A \mid Z=1) - E(A \mid Z=0)}$

Two equivalent estimators for the simple setting

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We have been studying the **Wald estimator**

$$\frac{E(Y \mid Z = 1) - E(Y \mid Z = 0)}{E(A \mid Z = 1) - E(A \mid Z = 0)}$$

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$$\frac{E(Y \mid Z = 1) - E(Y \mid Z = 0)}{E(A \mid Z = 1) - E(A \mid Z = 0)} = \frac{\mathrm{Cov}(Z, Y)}{\mathrm{Cov}(Z, A)}$$

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where the version at the right generalizes continuous variables in a linear system

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A numerically equivalent estimator is **two-stage-least-squares**:

1. Regress A on Z
2. Predict $\hat{A}$
   - Intuition: Isolates Z-generated variation in A

Two equivalent estimators for the simple setting

We have been studying the **Wald estimator**

$$\frac{E(Y \mid Z=1) - E(Y \mid Z=0)}{E(A \mid Z=1) - E(A \mid Z=0)} = \frac{\mathrm{Cov}(Z,Y)}{\mathrm{Cov}(Z,A)}$$

where the version at the right generalizes continuous variables in a linear system

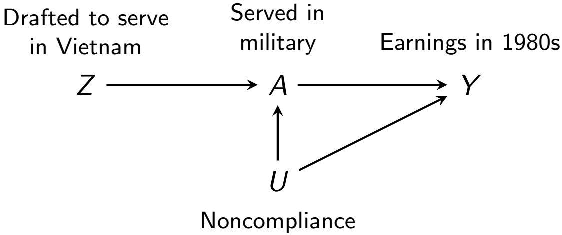
A numerically equivalent estimator is **two-stage-least-squares**:

1. Regress A on Z
2. Predict $\hat{A}$
   - Intuition: Isolates Z-generated variation in A
3. Regress Y on $\hat{A}$

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

Assumptions are less plausible in observational studies



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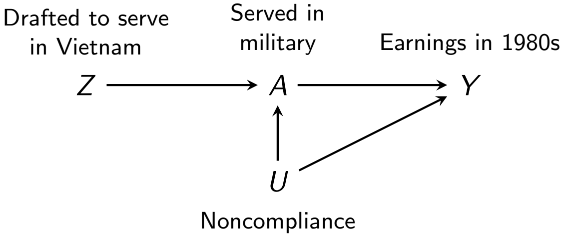

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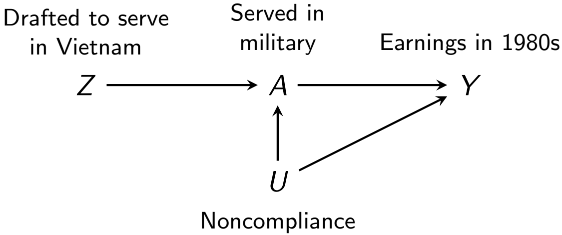

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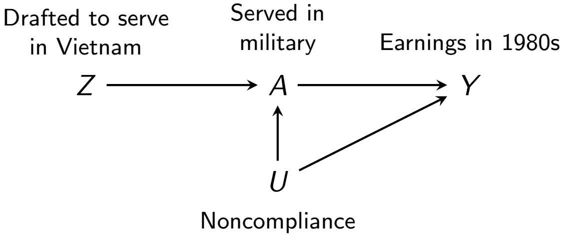

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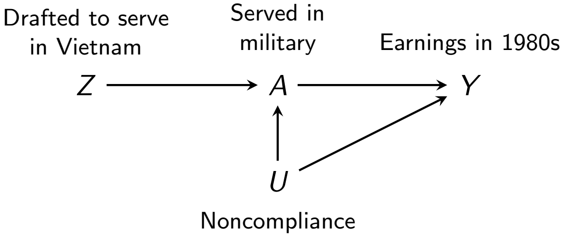

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- Exogeneity: Randomly selected draft numbers
- Exclusion: Being drafted affects earnings only through service
- Monotonicity: No one joins the military in defiance of not being drafted

Angrist, J. D. (1990). Lifetime earnings and the Vietnam era draft lottery: Evidence from Social Security administrative records. The American Economic Review, 313-336.

A difficult example: Genes as instruments

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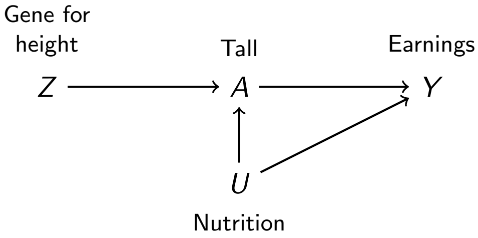

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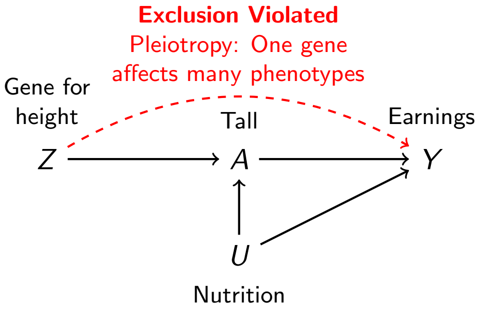

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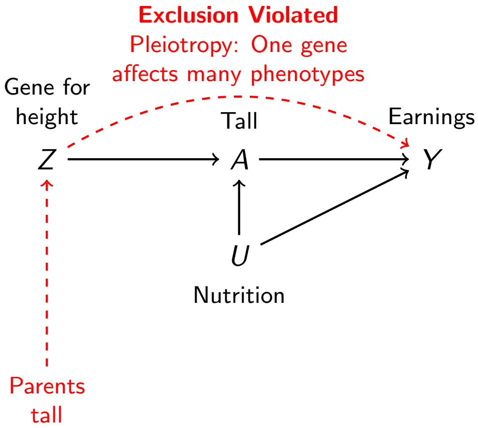

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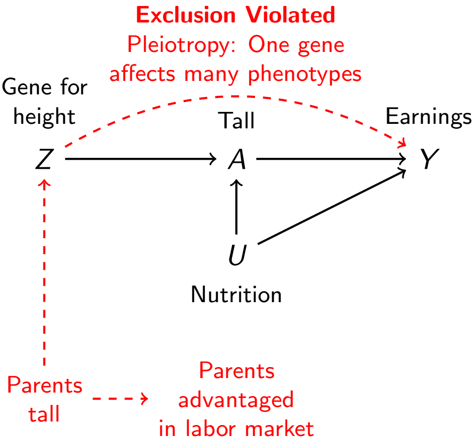

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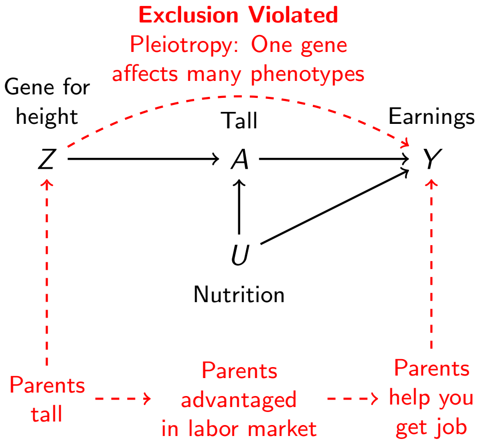

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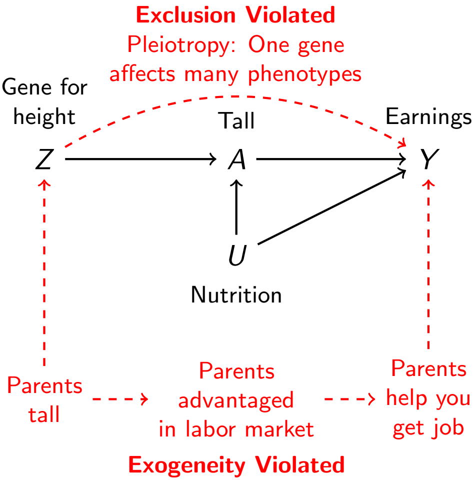
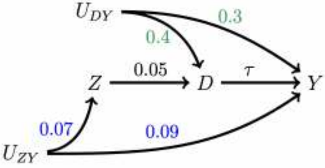


Warning: IV is very sensitive to assumptions



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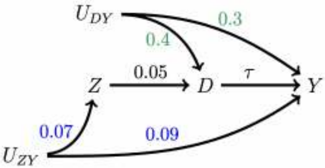
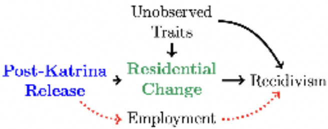


Asymptotic Bias of OLS
$= 0.3 \times 0.4 = \mathbf{0.12}$

Asymptotic Bias of IV
$= (0.07 \times 0.09)/0.05 = \mathbf{0.126}$

Study	Causal Structure	Identification Assumptions
Kirk (2009)		(1) Post-Katrina release induces moves away from a parolee's home neighborhood. (2) Post-Katrina release affects recidivism *only through* residential change and not, e.g., employment or police resources. (3) The timing of release shares no common causes with recidivism. (4) Post-Katrina release *never discourages* residential change.

Laidley and Conley (2018)

(1) More sunlight causes kids to exercise more. (2) Sunlight affects test scores *only through* exercise and not, e.g., mood. (3) When person fixed effects are included, within-person sunlight variation shares no common causes with test scores. (4) Sunnier weather *never discourages* exercise.

Sampson and Winter (2018)

(1) Proximity to a smelting plant increases lead exposure. (2) Proximity affects deliquency *only through* lead exposure. (3) Proximity shares no common causes—such as neighborhood disadvantage—with deliquency. (4) Moving closer to a smelting plant *never* causes someone to experience less lead exposure.

Harding et al. (2018)

$$\begin{tikzcd} & \text{Job Prospects} \arrow[d] \arrow[dr, bend left] & \\ \text{Judge} \arrow[r] & \text{Incarceration} \arrow[r] & \text{Employment} \end{tikzcd}$$

(1) Judge assignment affects the probability of being incarcerated. (2) Judge assignment affects employment *only through* incarceration. (3) Judge assignment shares no common causes with future employment. (4) If assigned to a more lenient judge, a defendant will *never* receive a harsher sentence.

Effects for Compliers

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

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  How much we care about LATE turns on what the instrument is!

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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- 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`

Appendix: Proof

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 $\square$

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.

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$$
\begin{tikzcd}
 & Z & \\
U_Z \arrow[ur] \arrow[r] & A \arrow[r] & Y \\
U \arrow[ur] \arrow[urr] & &
\end{tikzcd}
$$

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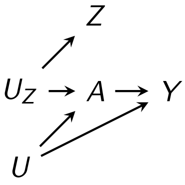

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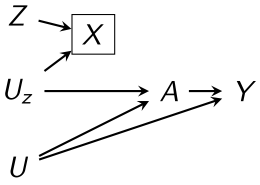

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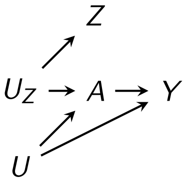

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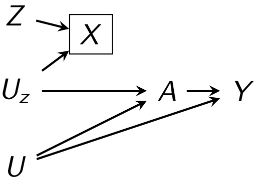

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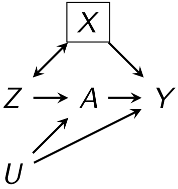

(exclusion via control)

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     (is the instrument such that we care about compliers?)

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 A)
  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 A)
  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.

To Read More About IV

Original Article

Sociological Methods & Research
1–48

DOI: 10.1177/00491241241235900
journals.sagepub.com/home/smr

Handle with Care: A Sociologist's Guide to Causal Inference with Instrumental Variables

Chris Felton[1] and Brandon M. Stewart[2]

To Read More About IV

Instrumental Variables Checklist

1. State the Theoretical Estimand

a. State the estimand: a Local Average Treatment Effect (the "Monotonicty" section) or weighted average of LATEs (Online Appendix F). Explain how compliers may differ from non-compliers and why we should be interested in the complier average treatment effect.

2. Explain Assumptions

a. State the identification assumptions in clear, non-technical language. unconfoundedness, exclusion restriction, monotonicity, instrument relevance, SUTVA, and positivity (the "Identification and Estimation" section).

b. Discuss plausible violations of any identification assumptions. It is unlikely that each and every assumption is beyond reproach.

c. If using a *proxy* instrument, be sure to state the *causal* instrument (Online Appendix B). The estimand and identification assumptions are defined with respect to the causal instrument—*not* the proxy instrument.

3. Report Results with Weak-Instrument-Robust Confidence Intervals

a. Report the estimated first-stage effect, "reduced-form" or ITT effect, and treatment effect with confidence intervals (the "Weakness" section).

b. Report *VtF*, Anderson–Rubin, and bootstrapped confidence intervals for the treatment effect (the "Estimation Bias" subsection).

4. Assess Bias and Expected Type-M Error

a. Conduct bias analysis for violations of unconfoundedness or the exclusion restriction (the "Identification Bias" subsection).

b. Report the results of placebo tests and scaled balance checks if possible (Online Appendix C).

c. Report a heteroskedasticity-robust partial *F*-statistic for the instrument in the first-stage regression (the "Estimation Bias" subsection).

d. Report the expected Type-M error, possibly across a range of hypothetical effect sizes (the "Type-M Error" subsection).

1. Natural Experiments
2. Instrumental Variables
3. Regression Discontinuity
4. Interrupted Time-Series
5. Difference-in-Differences
6. Synthetic Controls

Regression Discontinuity

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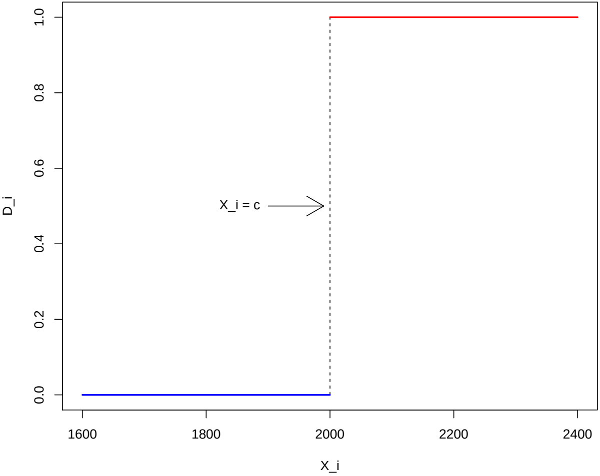
- It is a fairly old idea, generally credited to education research by Thistlethwaite and Campbell 1960 but with a dynamic and interesting recent history (Hahn et al 2001 and Lee 2008 were big jumps forward).
- The goal here is to get you up to speed with the core idea: if you want to know how to do 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



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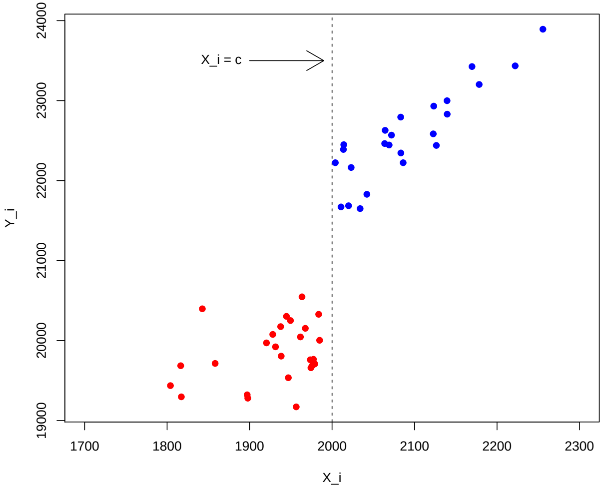

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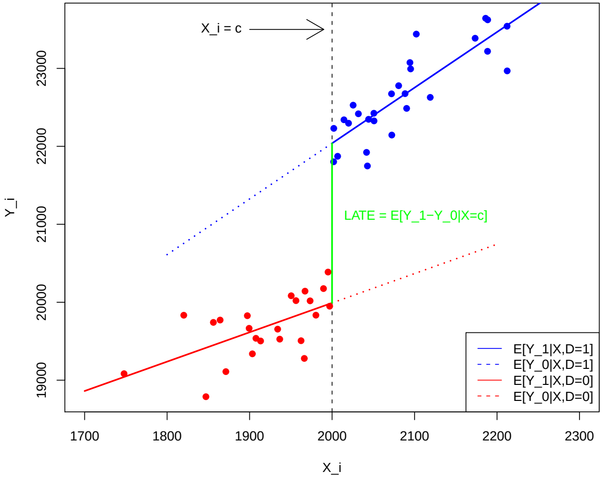

Setup

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

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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 T on Y at the threshold

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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. $Y(1), Y(0) \perp\!\!\!\perp T | X$ *(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 α_{SRDD} *is only identified at the threshold.*

Extrapolation and smoothness

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

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- 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.

General estimation strategy

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

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

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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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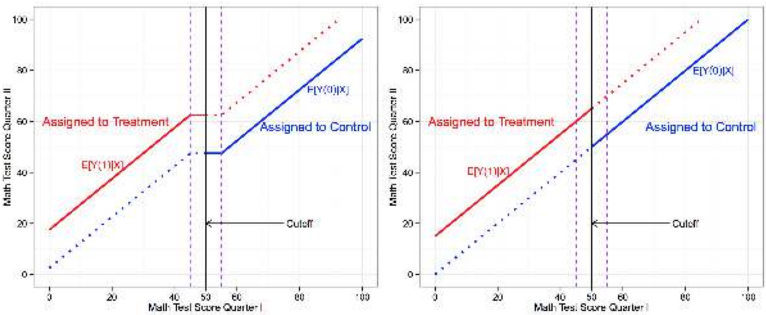
- Local statistical independence does not imply exclusion restriction (i.e. forcing variable not directly affecting the outcome)
- If you are doing an RDD: be sure to do balance checks and sensitivity checks (read-up on best practices first!)

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 1 of Fuzzy RD

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

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- Sound familiar? Fuzzy RD is just IV!

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- $T_i(x) = 0$ if unit i would take control when X_i was x

Fuzzy RD assumptions

Assumption 2: Monotonicity

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

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

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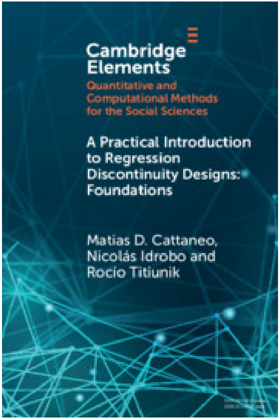
- Remember that we are only identifying an effect at the boundary.
- There are many other nuances to estimation and choosing an appropriate bandwidth for the comparison- be sure to read more before trying this at home.
- There is an interesting literature on geographic regression discontinuity designs 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

Interrupted Time Series

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- 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.

ITS Example 1 (Hobbs and Roberts 2018)

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

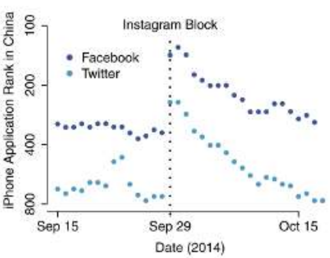
How Sudden Censorship Can Increase Access to Information

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.

ITS Example 1 (Hobbs and Roberts 2018)

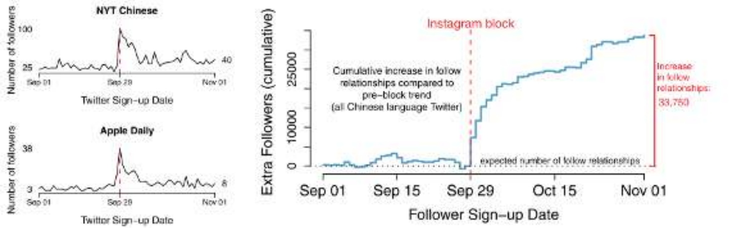
FIGURE 3. Left: The Instagram block's effect on the rank of Facebook and Twitter on iPhones from mainland China, from AppAnnie.com. Right: Comparison of tweets per day from Mainland China and Hong Kong before and after the Instagram block.



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.

ITS Example 1 (Hobbs and Roberts 2018)

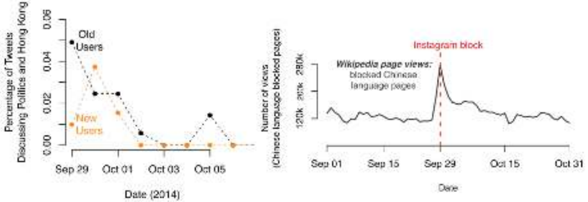
FIGURE 4. Left: Daily new followers to *New York Times* Chinese and *Apple Daily* Twitter accounts (based on new user sign-up dates). Right: Cumulative increase in followers, compared to preblock trend, of any Chinese language user (based on new user sign-up dates) compared to expected increase in followers.



The left panel of this figure shows the sign-up dates of followers of the *New York Times* Chinese and *Apple Daily* Twitter accounts. Many followers of these accounts signed up for Twitter immediately following the Instagram block. This increase in sign-ups—users who eventually followed *NYT* Chinese and *Apple Daily*—continues long after the Instagram block. The right panel of this figure shows that all Chinese language Twitter users accumulated approximately 33,750 more followers from new Twitter sign-ups than what we would expect based on pre-block trends. This cumulative increase was calculated using a cumulative sum of the number of new followers minus the number of expected followers, where the expected followers was the mean daily number of new followers prior to the Instagram block.

ITS Example 1 (Hobbs and Roberts 2018)

FIGURE 5. Left: Tweets that mention politics in Hong Kong, comparison of new users and old users. Right: Page views for Chinese language Wikipedia pages blocked in China. Bottom: Changes in Wikipedia views.



Largest increases in views September 28th to 29th *First day of Instagram block*	Largest increases in views September 29th to 30th *Second day of Instagram block*
Occupy Central	**People's Republic of China blocked websites list**
Hong Kong	**Jiang Zemin**
June 4th Incident	Radio Australia
Hong Kong Independence Movement	**Hu Jintao**
pan-Democrats	**Zeng Qing**
Xi Jinping	**Wang Weilin (Tank Man)**
Sunflower student movement	**Li Peng**
Guo Baixiong	**Ling Jihua**
Article 23 Hong Kong	**Tiananmen Square Incident**
Zhang Xuan (celebrity who supported HK movement)	**Zhou Yongkang**
Alliance for True Democracy	**Wu'erkaixi (June 4 leader)**
Bo News (dissident Chinese news network, located in U.S.)	**Zhang Dejiang**
Chai Ling (June 4 leader)	YouTube
Jasmine Revolution	**Wen Jiabao**
Hu Yaobang	AV Actress (Japanese porn stars)
Taiwan	**Mao Zedong**
Jiang Zemin	**Deng Xiaoping**
People's Republic of China blocked websites list	**Ling Gu (son of Ling Jihua)**
Democracy	**Wu Bangguo**
Wu'erkaixi (June 4 leader)	**Hua Guofeng**

The left panel of this figure shows that users who signed up for Twitter after the Instagram block began mentioning protest events in Hong Kong about a day after their arrival on the site. The right panel of this figure shows page views of Chinese language Wikipedia pages that were blocked in China before and after Instagram was blocked. The increase in Wikipedia page views shows that the Instagram block facilitated increased access to information for some users. In the bottom table, the change in Wikipedia views shows that new viewers accessed pages that had long been censored including those related to the 1989 Tiananmen Square protests (general political and historical events are in bold).

ITS Example 2 (Felton 2023)

sociological science

13 Reasons Why Probably Increased Emergency Room Visits for Self-Harm among Teenage Girls

Chris Felton

Harvard University

Abstract: I present evidence that the release of Netflix's *13 Reasons Why*—a fictional series about the aftermath of a teenage girl's suicide—caused a temporary spike in emergency room (ER) visits for self-harm among teenage girls in the United States. I conduct an interrupted time series analysis using monthly counts of ER visits obtained from a large, nationally representative survey. I estimate that the show caused an increase of 1,297 self-harm visits (95 percent CI: 634 to 1,965) the month it was released, a 14 percent (6.5 percent, 23 percent) spike relative to the predicted counterfactual. The effect persisted for two months, and ER visits for intentional cutting—the method of suicide portrayed in the series—were unusually high following the show's release. The findings indicate that fictional portrayals of suicide can influence real-life self-harm behavior, providing support for contagion-based explanations of suicide. Methodologically, the study showcases how to make credible causal claims when effect estimates are likely biased.

Keywords: suicide; self-harm; contagion; imitation; interrupted time series; causal inference

ITS Example 2 (Felton 2023)

Used to train forecasting model

Month	t	*13RW* Released	$Y_t(1)$	$Y_t(0)$	$\tau_t = Y_t(1) - Y_t(0)$
Jan 2006	1	0	—	5,101	—
⋮	⋮	⋮	⋮	⋮	⋮
Feb 2017	$T-2$	0	—	8,099	—
Mar 2017	$T-1$	0	—	9,450	—
Apr 2017	T	1	10,460	?	10,460 − ?

Imputed using forecasting model

Figure 2: The logic of interrupted time series (ITS). *Notes:* Annotated table illustrating the logic of ITS. A forecasting model is trained on the pre-treatment time series, which represents a series of potential outcomes under control ($Y_t(0)$). This model is then used to forecast, or impute, the potential outcome under control in the post-treatment period. The treatment effect is estimated by subtracting the imputed potential outcome under control ($\widehat{Y}_T(0)$) from the observed potential outcome under treatment ($Y_T(1)$).

ITS Example 2 (Felton 2023)

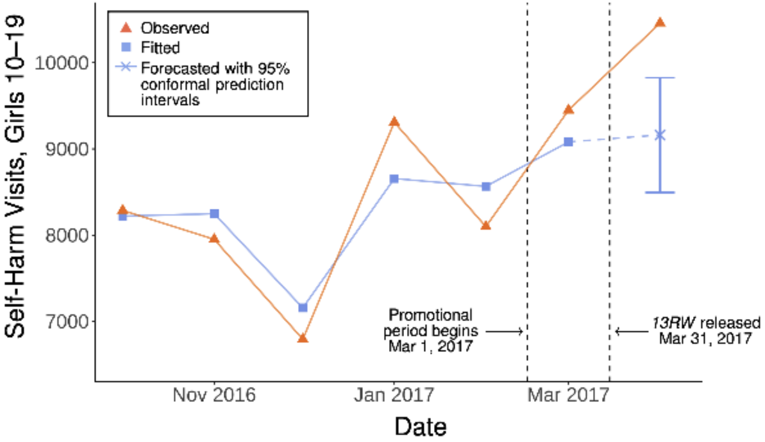


Figure 3: Main ITS results. *Notes:* Plot showing the final 6 pre-treatment periods and first post-treatment period. The model is fit to 122 differenced pre-treatment periods, but all values are de-differenced for visualization. The estimated treatment effect is an increase of 1,297 self-harm visits, with 95 percent conformal confidence intervals ranging from 634 to 1,965. This is a 14 percent (6.5 percent, 23 percent) spike relative to the predicted counterfactual.

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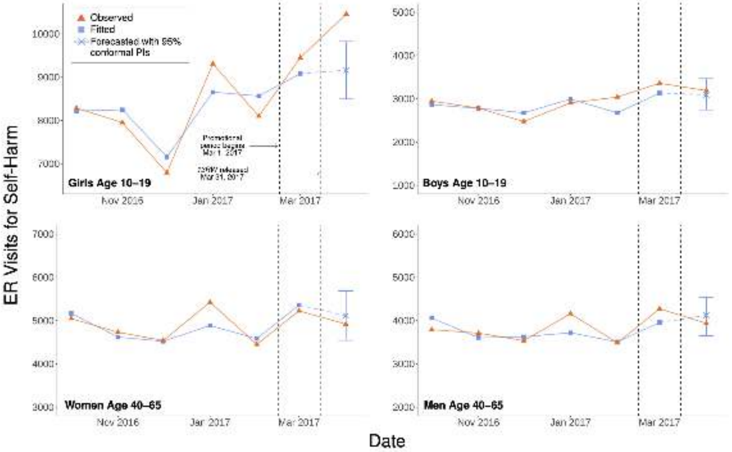

Figure 4: Estimated treatment effects are negligible for teen boys and older adults. *Notes*: For each group, I perform AIC-guided stepwise selection to choose an ARIMA specification using only the pre-treatment series. All models include first- and seasonal-differencing to account for secular trends and seasonality. The result for boys fits with the gender "paradox" in suicide, and the analyses for adults serve as placebo tests for the ITS method. Predicted values for each group except teen girls lie within the prediction intervals, boosting confidence in the out-of-sample predictive accuracy of the model-selection procedure. Errorbars show 95 percent conformal prediction intervals.

ITS Example 2 (Felton 2023)

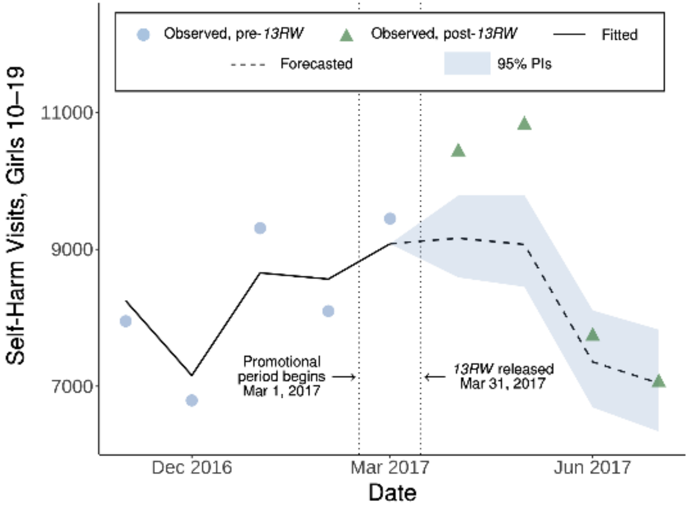

Figure 5: The treatment effect persists for two months. *Notes*: Multi-step-ahead forecasts with 95 percent prediction intervals constructed from simulated paths using bootstrapped residuals. The duration of the effect is consistent with trends in *13 Reasons Why*–related social media posts. The prediction intervals likely undercover the potential outcomes under control, but analyses in "Methodological Concerns" provide more evidence that the effect persisted for two months.

ITS Example 2 (Felton 2023)

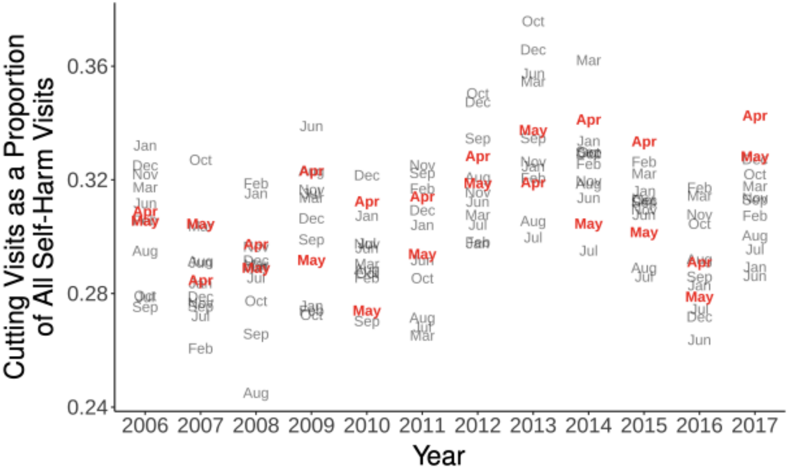

Figure 6: ER visits for intentional cutting are particularly high following 13 Reasons Why's release. *Notes*: The plot shows the monthly proportions of self-harm visits that are for intentional cutting among teen girls. Each month is labeled with its abbreviation, and April and May are highlighted in red. The proportions show noticeable variation by year, likely due to yearly changes in the sample of hospitals, but in no year other than 2017 do April and May have the two highest counts.

ITS Example 2 (Felton 2023)

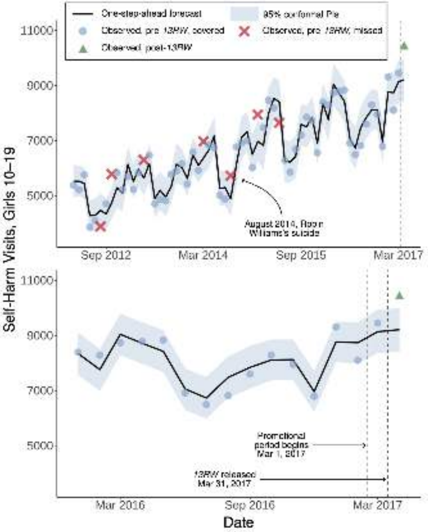

Figure 9: Repeated placebo test results. *Notes.* One-step-ahead forecast plots with 95 percent conformal prediction intervals. The black line represents out-of-sample forecasts, not fitted values. The ARIMA specification was chosen using the first half of the time series only. For the initial test period, the model is trained on the first half of the series. For each subsequent test period, an additional observation is added to the training series. Of 61 placebo tests, 7 fail, indicating good but imperfect coverage. Excluding the Robin Williams period, the numbers are 60 and 6, respectively.

ITS Example 2 (Felton 2023)

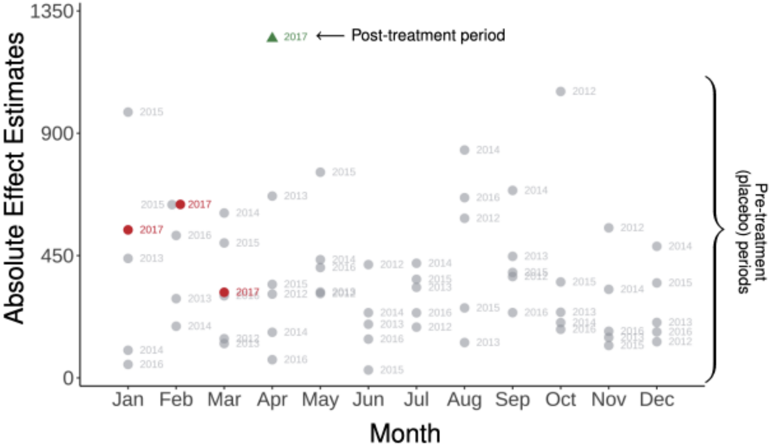

Figure 11: Repeated placebo test results by month. *Notes*: Placebo effect estimates in absolute value organized by month. Pre-treatment estimates from 2017 are red circles, and the post-treatment estimate is a green triangle. Although the estimates for January–March of 2017 are slightly elevated, they are not especially high for those months. The effect estimates for February 2015 and February 2017 are almost exactly equal and thus plotted side-by-side. The model appears to predict more accurately for the final three months of the year.

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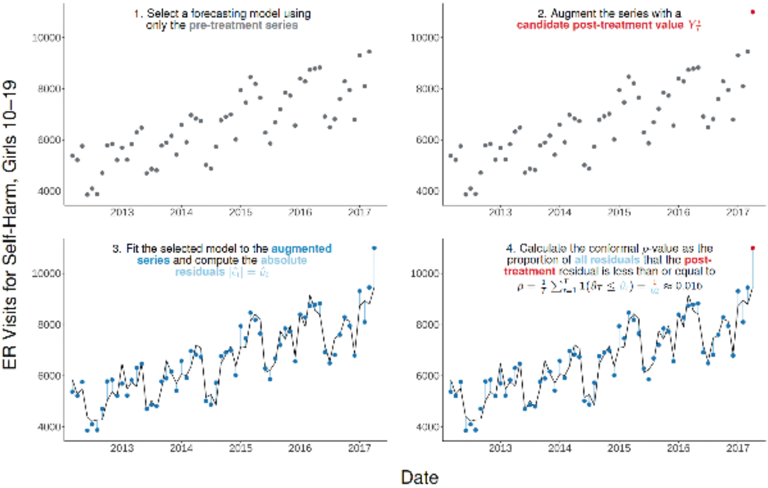

Figure E.2: Grid of plots illustrating how to calculate a conformal p-value for a candidate value for Y_T, denoted $Y_T^\dagger$. Here $Y_T^\dagger = 11{,}000$. Note that the set of all residuals includes the residual for $Y_T^\dagger$, $\hat{\epsilon}_T$.

Motivation: Studying the Minimum Wage

Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania

By DAVID CARD AND ALAN B. KRUEGER*

On April 1, 1992, New Jersey's minimum wage rose from \$4.25 to \$5.05 per hour. To evaluate the impact of the law we surveyed 410 fast-food restaurants in New Jersey and eastern Pennsylvania before and after the rise. Comparisons of employment growth at stores in New Jersey and Pennsylvania (where the minimum wage was constant) provide simple estimates of the effect of the higher minimum wage. We also compare employment changes at stores in New Jersey that were initially paying high wages (above \$5) to the changes at lower-wage stores. We find no indication that the rise in the minimum wage reduced employment. (*JEL* J30, J23)

`https://www.jstor.org/stable/2118030`

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- "What would a skeptic consider convincing evidence?" David Card
- "There was a time when we thought econometric techniques would solve a lot of the data problems. Now I think the feeling is that there are a lot of problems for which it is easier to get better data." Alan Krueger

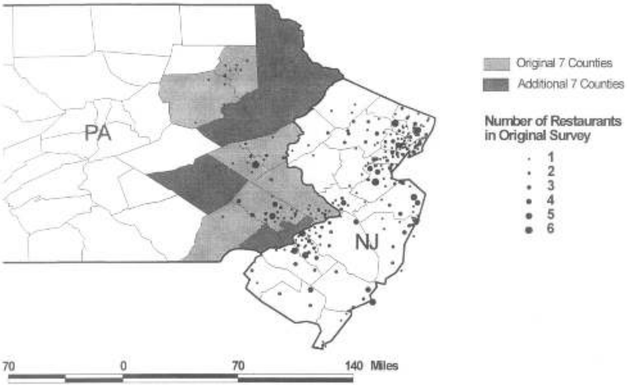


FIGURE 1. AREAS OF NEW JERSEY AND PENNSYLVANIA COVERED BY ORIGINAL SURVEY AND BLS DATA

Source: Card and Krueger 2000

Conversations / *David Card and Alan Krueger*

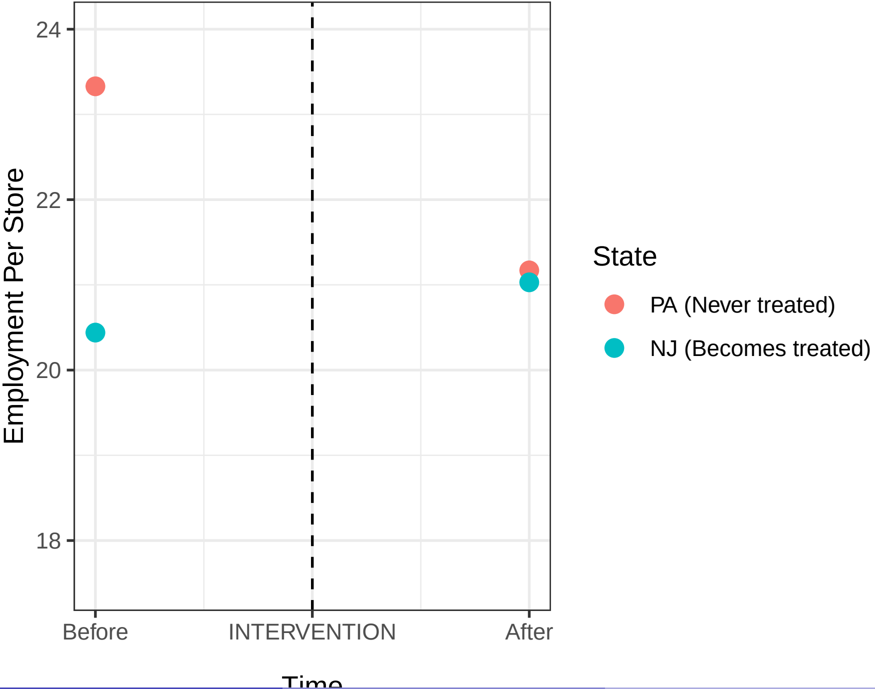
Two Economists Catch Clinton's Eye By Bucking the Common Wisdom

Princeton economists David Card, left, and Alan Krueger.

They use control groups in their research, and test minimum-wage theories by surveying the managers of fast-food restaurants.

The New Jersey Test

'Let's Not Assume'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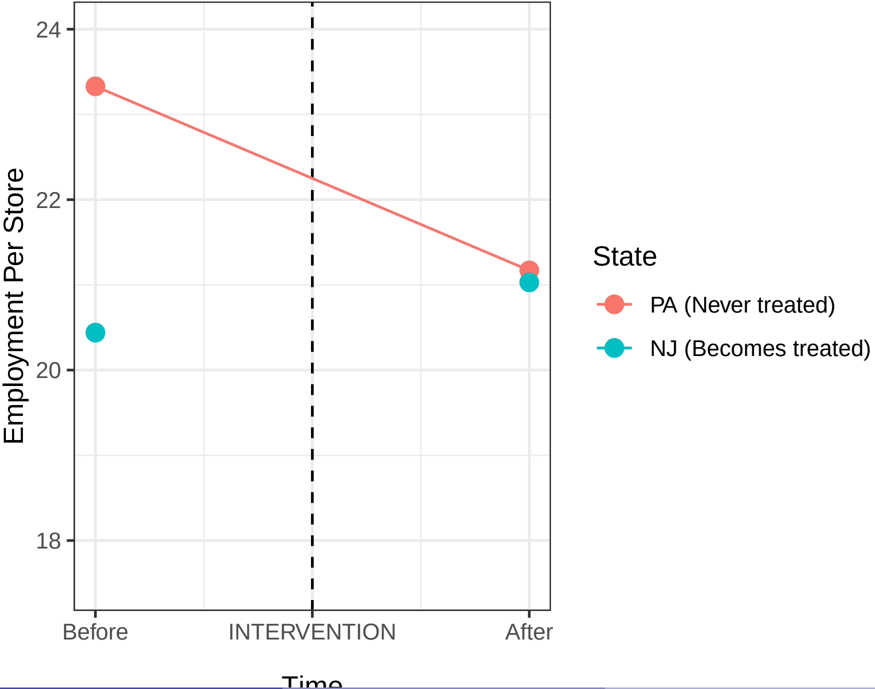
Employment Per Store
Before
INTERVENTION
After
Time
State
PA (Never treated)
NJ (Becomes treated)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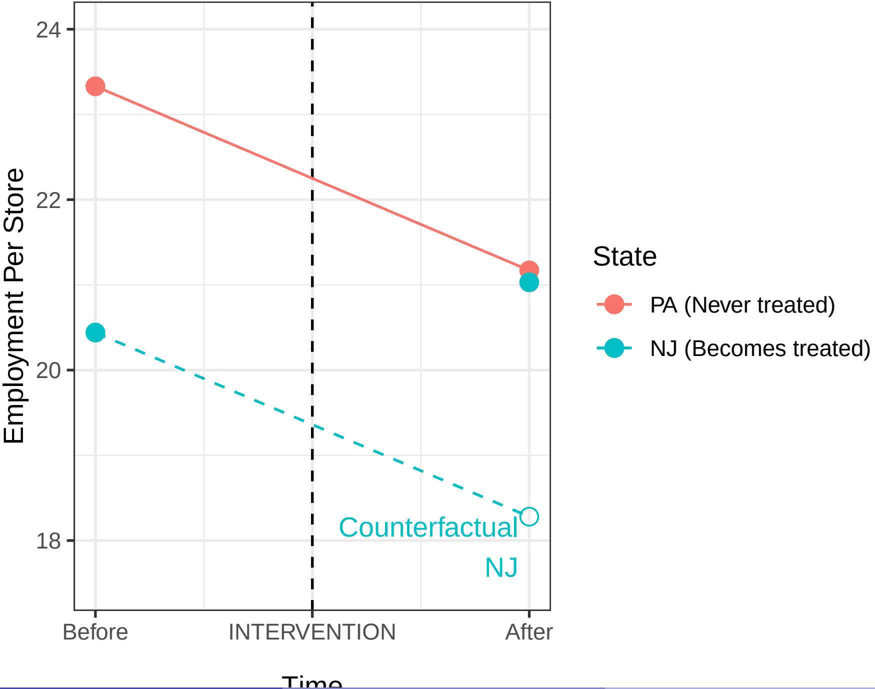
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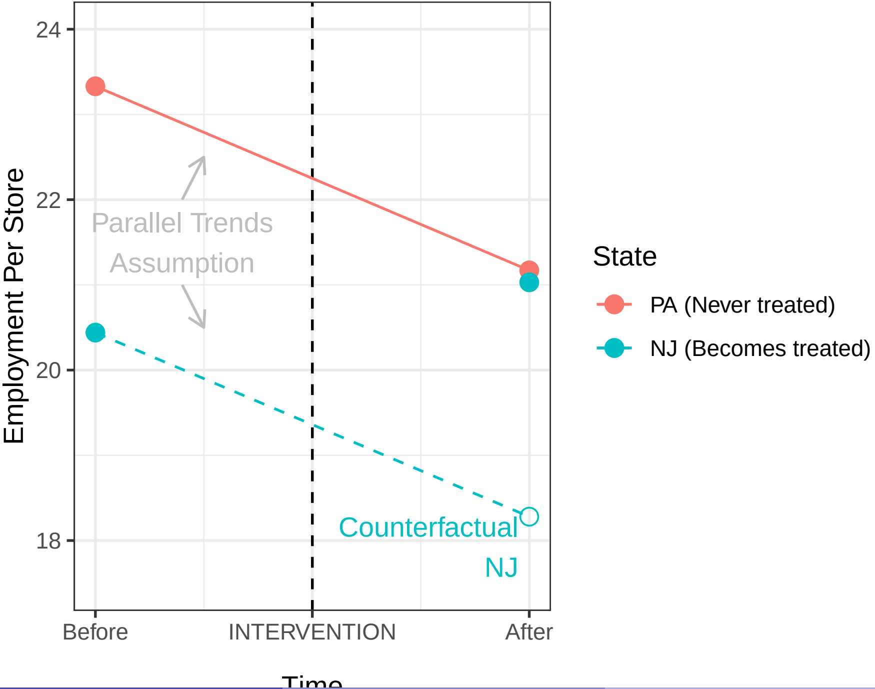

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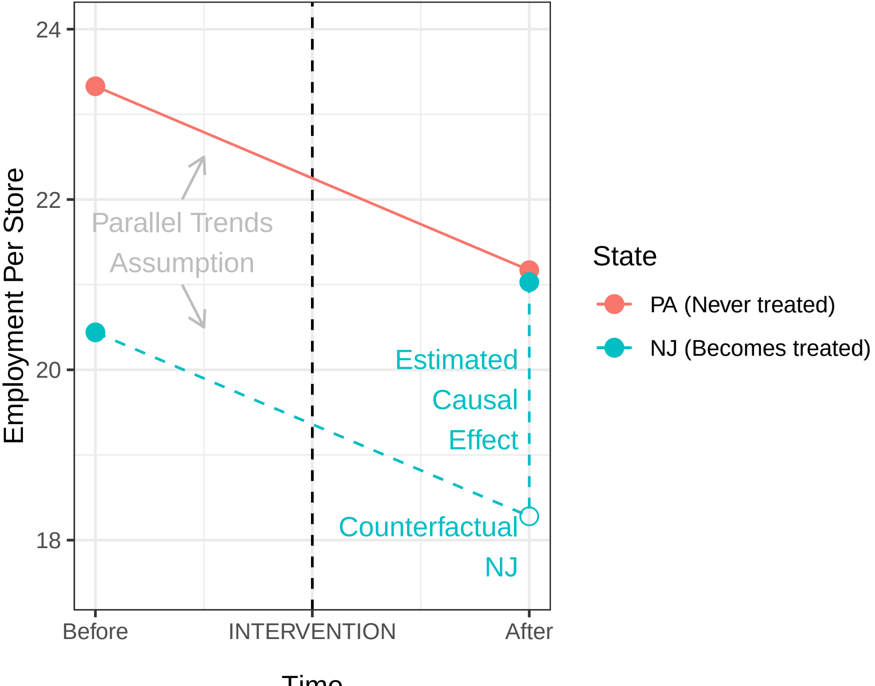

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Example: Card and Krueger (2000)

- Do increases to the minimum wage depress employment at fast-food restaurants?

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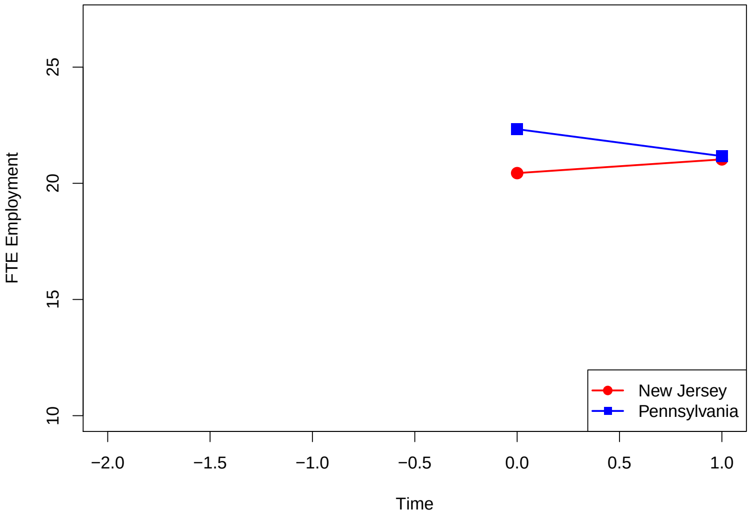
- NJ_i indicates which stores received the treatment of a higher minimum wage at time period $t = 2$

Parallel Trends?



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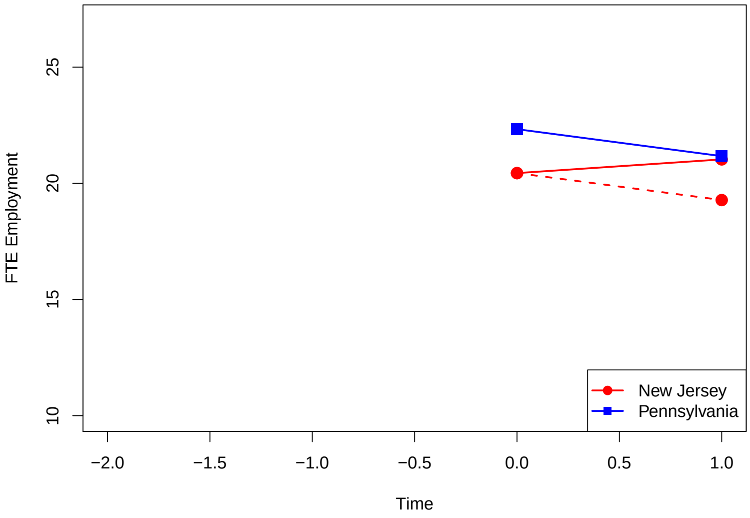

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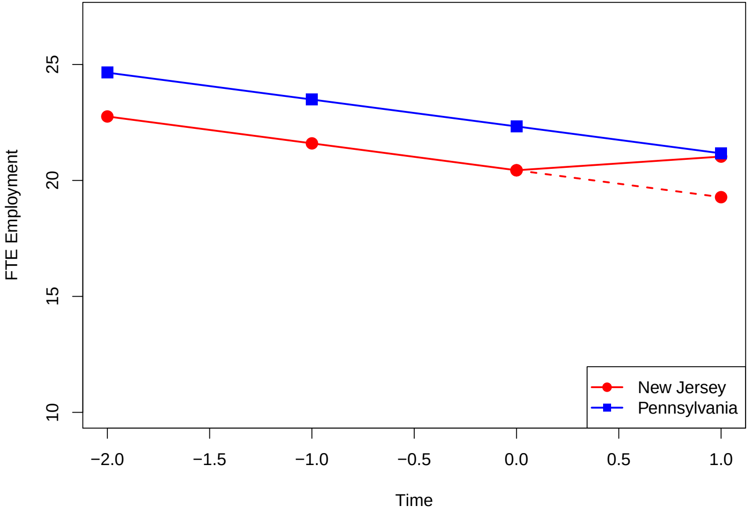

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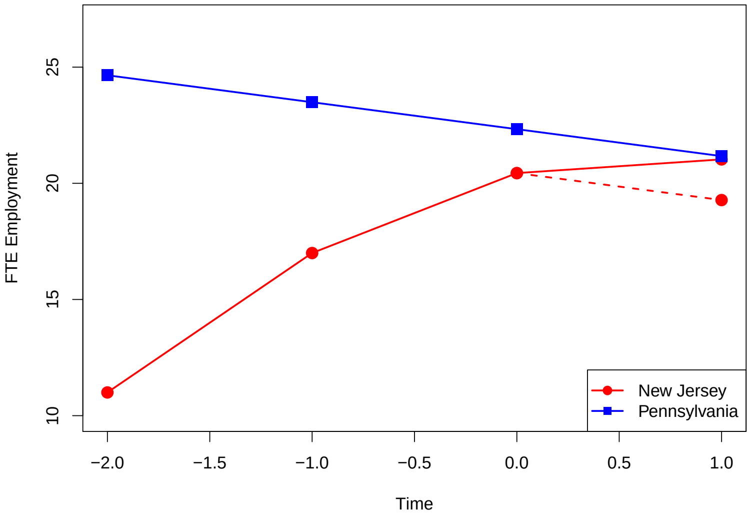
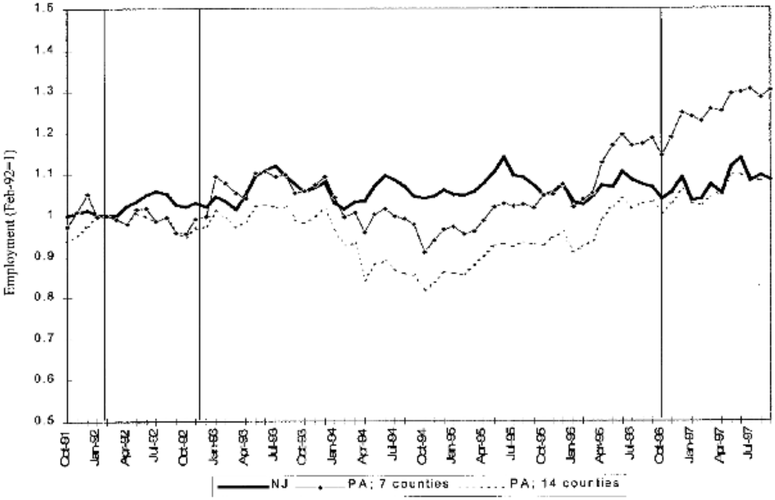


Longer Trends in Employment (Card and Krueger 2000)



First two vertical lines indicate the dates of the Card-Krueger survey. October 1996 line is the federal minimum wage hike which was binding in PA but not NJ

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4) Long-term vs. Short-term Effects
   parallel trends are less credible over a long time horizon, but many policies need time to take effect.

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   difference in levels and difference in logs can be quite different
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Synthetic control

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How much did it reduce CA cigarette sales in 1990? 1995? 2000?

Synthetic control

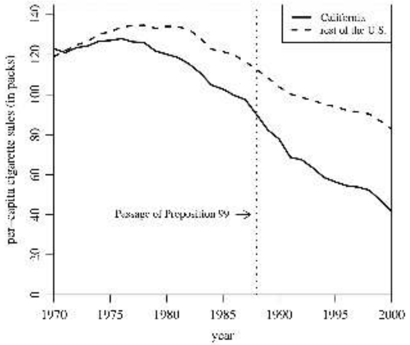

Figure 1. Trends in per-capita cigarette sales: California vs. the rest of the United States.

Can't use RDD
— Effect at 1988 not of interest

Can't use ITS
— Hard to extrapolate Y^0 trend

Can't use DID
— No other state like CA

Idea: Create a **synthetic CA** to estimate $Y^0_{\text{CA,t}}$ for $t \geq 1988$

A Synthetic California

Table 1. Cigarette sales predictor means

Variables	California Real	California Synthetic	Average of 38 control states
Ln(GDP per capita)	10.08	9.86	9.86
Percent aged 15–24	17.40	17.40	17.29
Retail price	89.42	89.41	87.27
Beer consumption per capita	24.28	24.20	23.75
Cigarette sales per capita 1988	90.10	91.62	114.20
Cigarette sales per capita 1980	120.20	120.43	136.58
Cigarette sales per capita 1975	127.10	126.99	132.81

NOTE: All variables except lagged cigarette sales are averaged for the 1980–1988 period (beer consumption is averaged 1984–1988). GDP per capita is measured in 1997 dollars, retail prices are measured in cents, beer consumption is measured in gallons, and cigarette sales are measured in packs.

A Synthetic California

Table 2. State weights in the synthetic California

State	Weight	State	Weight
Alabama	0	Montana	0.199
Alaska	–	Nebraska	0
Arizona	–	Nevada	0.234
Arkansas	0	New Hampshire	0
Colorado	0.164	New Jersey	–
Connecticut	0.069	New Mexico	0
Delaware	0	New York	–
District of Columbia	–	North Carolina	0
Florida	–	North Dakota	0
Georgia	0	Ohio	0
Hawaii	–	Oklahoma	0
Idaho	0	Oregon	–
Illinois	0	Pennsylvania	0
Indiana	0	Rhode Island	0
Iowa	0	South Carolina	0
Kansas	0	South Dakota	0
Kentucky	0	Tennessee	0
Louisiana	0	Texas	0
Maine	0	Utah	0.334
Maryland	–	Vermont	0
Massachusetts	–	Virginia	0
Michigan	–	Washington	–
Minnesota	0	West Virginia	0
Mississippi	0	Wisconsin	0
Missouri	0	Wyoming	0

Synthetic control

Theoretical Estimand: $\tau(t) = Y^1_{\text{CA},t} - Y^0_{\text{CA},t} \qquad t \geq 1988$

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$$\text{Empirical Estimand:} \quad \theta(t) = Y^1_{\text{CA},t} - Y^0_{\text{SyntheticCA},t} \qquad t \geq 1988$$

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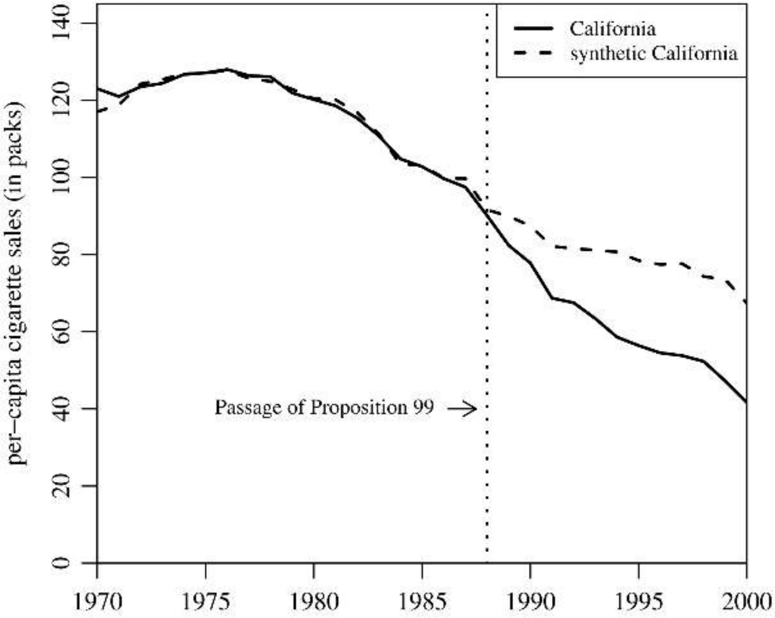
Empirical Estimand: $\theta(t) = Y^1_{\text{CA},t} - Y^0_{\text{SyntheticCA},t}$ $\quad t \geq 1988$

Identifying Assumption: $\underbrace{Y^0_{\text{CA},t}}_{\text{Counterfactual}} = \underbrace{Y^0_{\text{SyntheticCA},t}}_{\text{Factual}}$ $\quad t \geq 1988$

Synthetic control



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To read more about core SCM: Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. "Comparative politics and the synthetic control method." *American Journal of Political Science* 59.2 (2015): 495-510.

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- We talked about natural experiments, instrumental variables, regression discontinuity, interrupted time series, and differences and differences

Next Week: Final Week and Synthesis! (Please Read!)