Lecture 22: Regression discontinuity

Criminology 250

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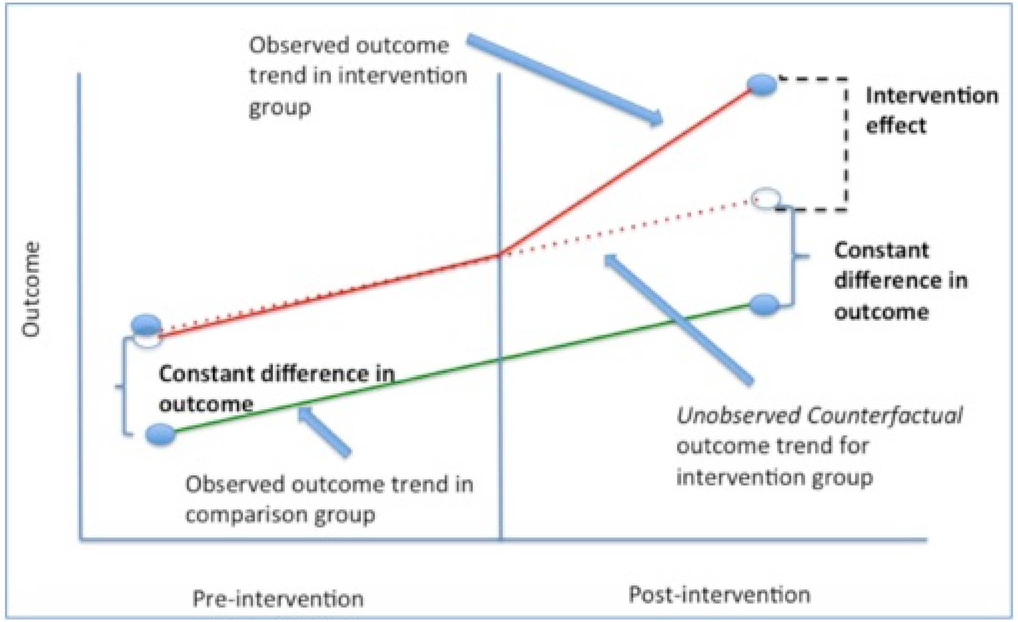
University of Pennslyvania

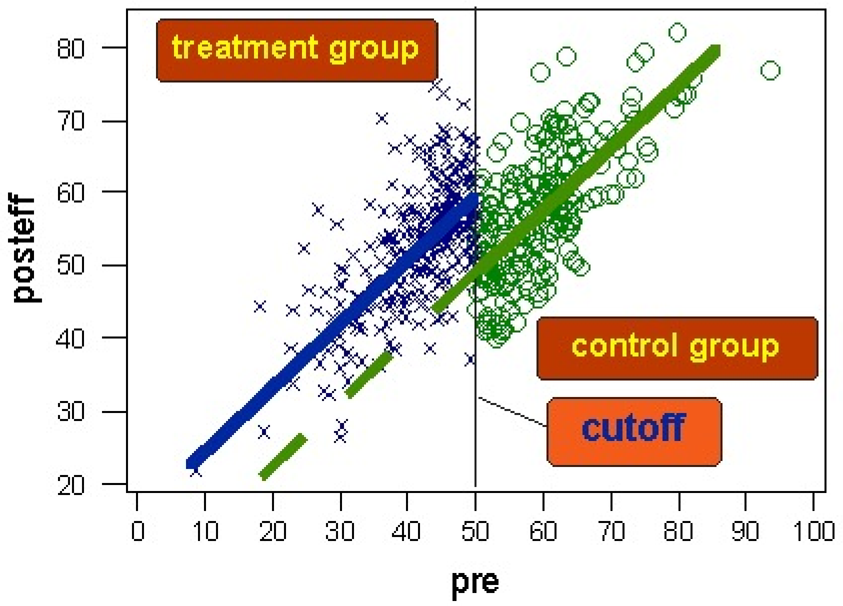
Review of diff-in-diff



- Observational data: Use when we don't have experimental data, and we want to compare two groups (or individuals) that only have one difference (the treatment) between them. *Note: Need before and after measurements.*
- Examples:
  - Comparing rates of malaria in two towns when mosquito nets were given out,
  - comparing the effects on education of having fathers die after a tsunami,
  - comparing rates of lung cancer in two towns, one of which had a garbage incinerator built next to it.
- Main assumptions: Key two assumptions are parallel trends and SUTVA (no interference and consistency).

Regression discontinuity

Regression discontinuity



- Observational data: Use when we don't have experimental data, and we want to compare two groups (or individuals) that only have one difference (the treatment) between them, *and this difference was assigned randomly right around a cutoff*.

- Examples:

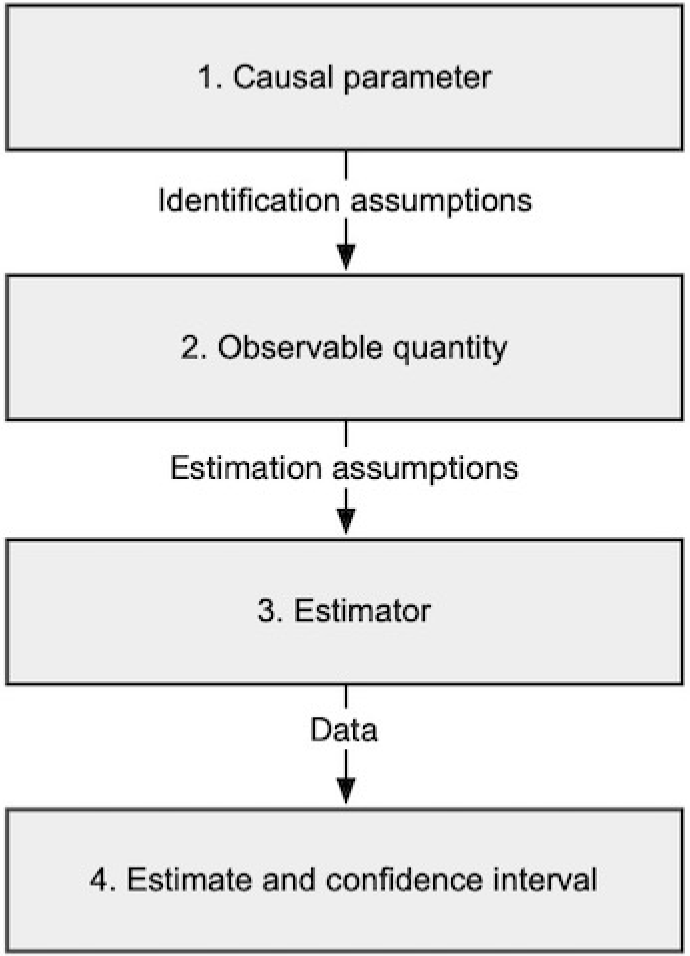
  - Evaluating the impact of extra tutoring on educational levels of students (cutoff based on SAT score) - there are many variations on this,
  - evaluating the effect of getting a DUI on reoffending (cutoff based on BAC level) - Hansen 2015,
  - evaluating the effect of having additional police officers on crime (cutoff based on university boundary) - MacDonald et al 2016.

- Assumptions: Key assumption is continuity.

Natural experiment

- Null hypothesis: The null hypothesis is continuity, and any discontinuity necessarily implies some cause.
- Continuity is natural: The tendency for things to change gradually is what we have come to expect in nature.
- Jumps are unnatural, or artificial: Jumps are so unnatural that when we see them happen, they beg for explanation. Charles Darwin, in his On the Origin of Species, summarized this by saying Natura non facit saltum, or “nature does not make jumps.”

How do we estimate a causal parameter?



Parameter of interest

Local average treatment effect:

$LATE = E(Y^1 - Y^0 | X = c_0)$.

A way to estimate this using a parametric model is:

$Y = \alpha + \beta_1 x_i + \beta_2 c_i + \beta_3 c_i^2 + \beta_4 c_i^3 + \epsilon$

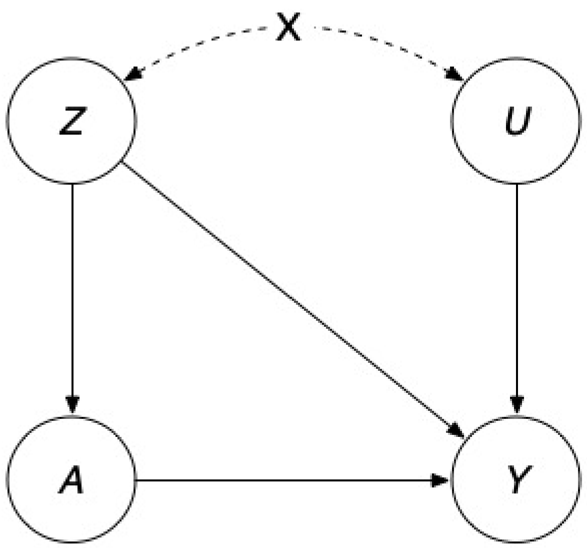
where assignment X is

$x_i = 1$ if $c_i \geq c_0$ and $x_i = 0$ if $c_i < c_0$.

and c_0 is the treatment cutoff. The polynomial can be shortened or extended according to the shape of the data.

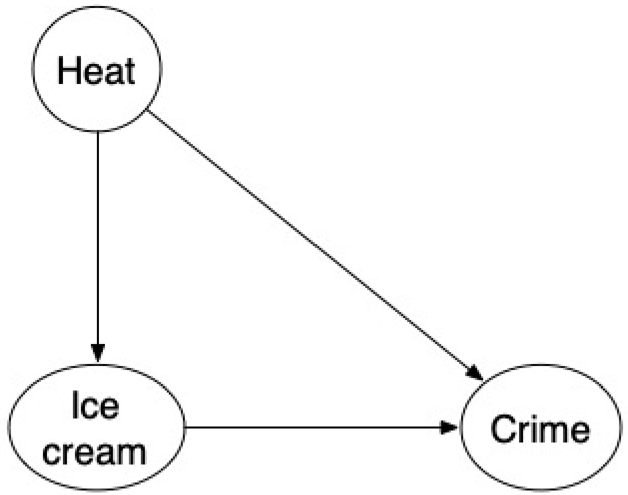
To see how to implement this in R you can go to: https://mixtape.scunning.com/regression-discontinuity.html

Full DAG

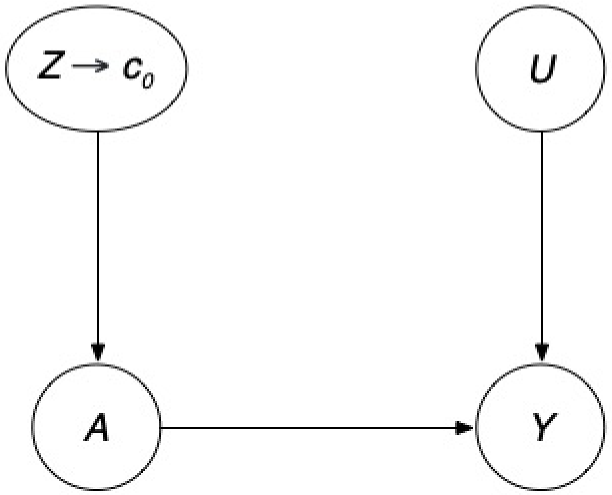


- The assignment or "running" variable Z is an observable confounder because it causes both A and Y (recall the ice-cream/crime example).
- Let's think about this using the educational program example. The assignment determines whether the person gets the treatment (tutoring), *and* the outcome of how well they do (the score *is* whether they do well educationally).
- We need to get rid of the arrow from Z to Y.

Recall the ice-cream/crime example



DAG near a cutoff c_0



- We do this by focusing on a small neighborhood right around the cutoff c_0.
- Focusing on this specific region makes it so we don't have a direct cause from Z to Y. In other words, we don't think the students who get 10 points above or below 1000 (990 - 1010) in their SATs do that differently in terms of educational outcome. Near c_0, they're all the same.
- So, Z around the neighborhood (c_0) acts as if it were randomly assigned. Thus, there is no arrow into Z or from Z to Y.

Main assumption

The most important assumption in regression discontinuity is "continuity".

- Continuity: Potential outcomes must be continuous at the cutoff. This is reflected graphically by the absence of an arrow from Z to Y because the cutoff c_0 has cut it off. At c_0 the assignment variable Z no longer has a direct effect on Y.
    - Basically, you assume that, without the arbitrary cutoff that determines who gets the treatment and who doesn't, the outcome should have been continuous and had no jumps.

Example: More police → less crime?

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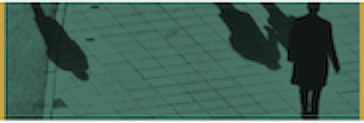

Journal of the Royal Statistical Society
Statistics in Society
Series A

J. R. Statist. Soc. A (2016)
179, *Part* 3, *pp.* 831–846

The effect of private police on crime: evidence from a geographic regression discontinuity design

John M. MacDonald, Jonathan Klick and Ben Grunwald

University of Pennsylvania, Philadelphia, USA

[Received April 2014. Final revision July 2015]

Summary. Research demonstrates that police reduce crime. We study this question by using a natural experiment in which a private university increased the number of police patrols within an arbitrarily defined geographic boundary. Capitalizing on the discontinuity in patrols at the boundary, we estimate that the extra police decreased crime in adjacent city blocks by 43–73%. Our results are consistent with findings from prior work that used other kinds of natural experiment. The paper demonstrates the utility of the geographic regression discontinuity design for estimating the effects of extra public or private services on a variety of outcomes.

Keywords: Crime; Geographic regression discontinuity; Private police

Example: More police → less crime?

Fig. 1. University City district crime patterns: - - - - -, University City boundary; ———, outer Penn campus; ———, inner Penn campus; □, 0–25 crime incidents; ■, 26–50 crime incidents; ■, 51–100 crime incidents; ■, 101–200 crime incidents; ■, 201–491 crime incidents

Example: More police → less crime?

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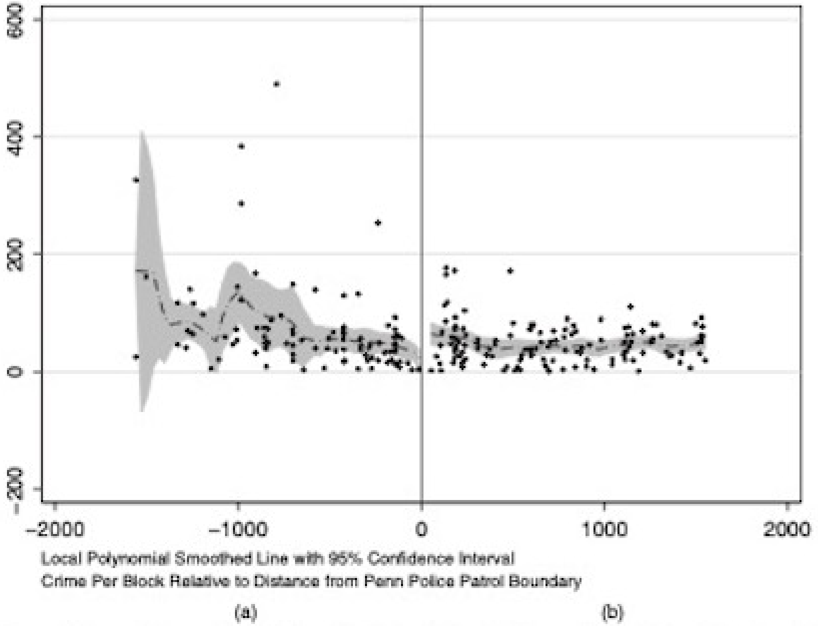

Fig. 2. Local polynomial smoothing of crime by block relative to distance from the Penn boundary: (a) inside boundary; (b) outside boundary

Example: County mask mandates → fewer Covid-19 cases and deaths?

IMF Working Paper

Mask Mandates Save Lives

by Niels-Jakob H. Hansen and Rui C. Mano

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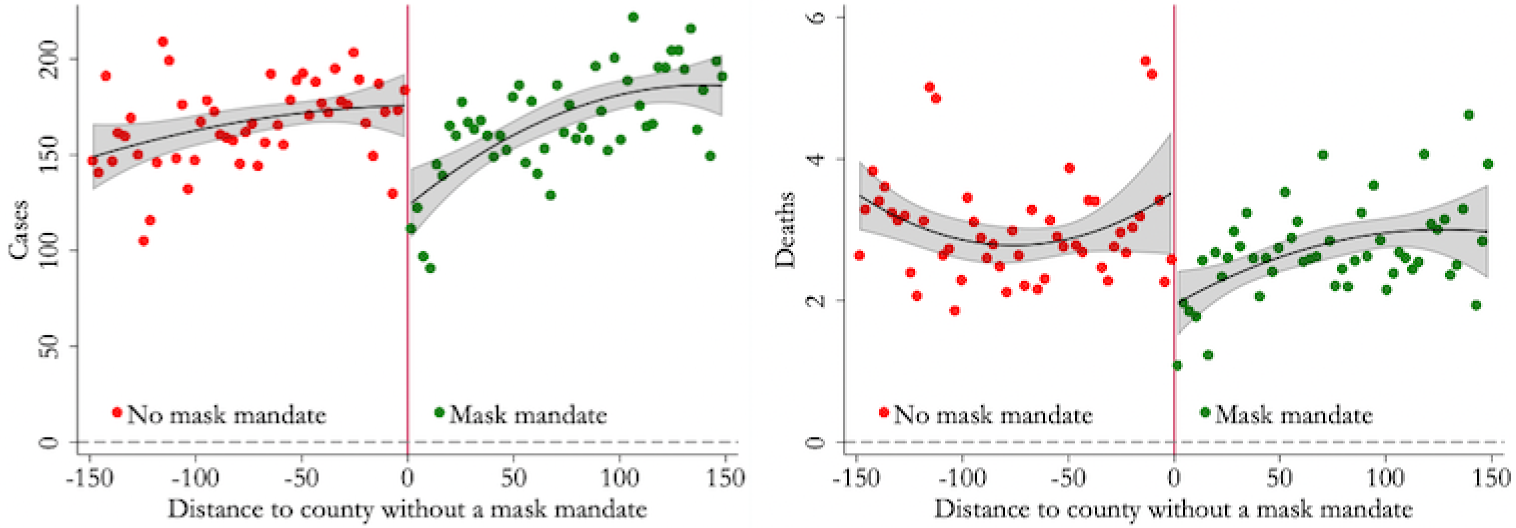
INTERNATIONAL MONETARY FUND

Example: County mask mandates → fewer Covid-19 cases and deaths?

Figure 3: New weekly COVID-19 cases and deaths per 100,000 inhabitants



(a) Cases (b) Deaths

Note: These charts show raw data of new weekly COVID-19 cases and deaths and thus do not account for county or time fixed effects as done in our econometric analysis. Data is binned in intervals of three miles.

Instrumental variables

Nobel Memorial Prize in Economic Sciences, 2021

Identification of Causal Effects Using Instrumental Variables

Joshua D. ANGRIST, Guido W. IMBENS, and Donald B. RUBIN

We outline a framework for causal inference in settings where assignment to a binary treatment is ignorable, but compliance with the assignment is not perfect so that the receipt of treatment is nonignorable. To address the problems associated with comparing subjects by the ignorable assignment—an "intention-to-treat analysis"—we make use of instrumental variables, which have long been used by economists in the context of regression models with constant treatment effects. We show that the instrumental variables (IV) estimand can be embedded within the Rubin Causal Model (RCM) and that under some simple and easily interpretable assumptions, the IV estimand is the average causal effect for a subgroup of units, the compliers. Without these assumptions, the IV estimand is simply the ratio of intention-to-treat causal estimands with no interpretation as an average causal effect. The advantages of embedding the IV approach in the RCM are that it clarifies the nature of critical assumptions needed for a causal interpretation, and moreover allows us to consider sensitivity of the results to deviations from key assumptions in a straightforward manner. We apply our analysis to estimate the effect of veteran status in the Vietnam era on mortality, using the lottery number that assigned priority for the draft as an instrument, and we use our results to investigate the sensitivity of the conclusions to critical assumptions.

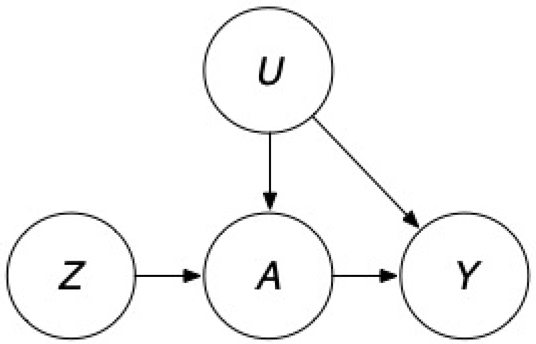
KEY WORDS: Compliers; Intention-to-treat analysis; Local average treatment effect; Noncompliance; Nonignorable treatment assignment; Rubin-Causal-Model; Structural equation models.

1. INTRODUCTION

Economists are typically interested in estimating causal effects rather than mere associations between variables. Potentially interesting causal effects include the effects of education on employment and earnings, the effects of employment training programs on subsequent labor market histories, and the effects of a firm's inputs on its output. The dominant approach to making inferences about causal effects in economics over the last four decades is based on

single individual or unit of observation is the comparison (e.g., difference) between the value of the outcome if the unit is treated and the value of the outcome if the unit is not treated. The target of estimation, the estimand, is typically the average causal effect, defined as the average difference between treated and untreated outcomes across all units in a population or in some subpopulation (e.g., males or females). For this definition of causality to be applicable to samples with units already exposed to treatments, we must be able to imagine observing outcomes on a unit in cir-

Motivating example: Noncompliance

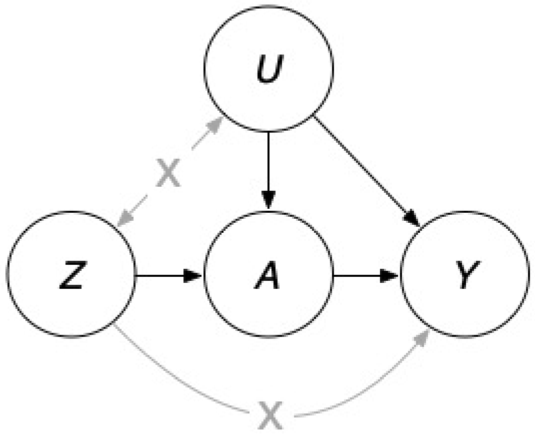


Suppose there is a double-blind randomized trial with noncompliance (i.e., not all people took the treatment properly):

- Z is the randomization assignment indicator (1: treatment, 0: placebo)
- A is an indicator for actually receiving treatment (1: yes, 0: no)
- Y is the outcome
- U is all factors (some unmeasured) that affect both the outcome and the adherence to the assigned treatment.

Suppose we want to consistently estimate the average causal effect of A on Y.

Instrumental variables (IV)

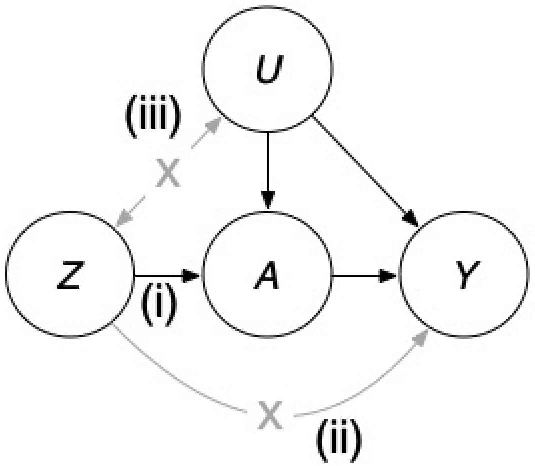


Instrumental variables (IV) may be used to identify the average causal effect of A on Y even if we did not measure the variables normally required to adjust for the confounding caused by U.

To perform their magic, IV methods need an instrumental variable Z, or an *instrument*. A variable Z is an instrument because it meets three instrumental conditions:

- (i) Relevance: Z and A are associated
- (ii) Exclusion restriction: Z affects Y only through A
- (iii) No confounding of Z on Y: Z and Y do not share causes

Instrumental variables (IV)



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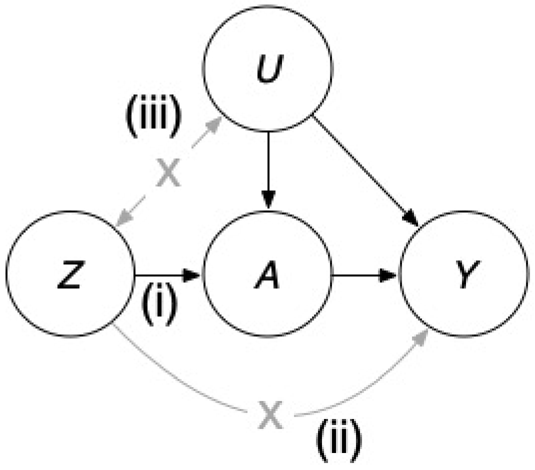

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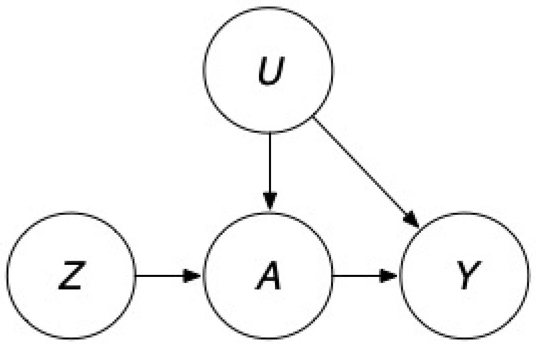
Monotonicity: There is a "fourth assumption" of monotonicity (cannot be seen in DAG). There are no defiers (i.e. assigned to treatment, take placebo, and vice versa).

Why do we need double blind experiments?

Note that if we don't have a double-blind study, the exclusion restriction, condition (ii), is violated, i.e. $Z \to Y$, (e.g. the effect of vaccine assignment could be that person acts more recklessly).

Example: Noncompliance



In the double-blind randomized trial described above, the randomization indicator Z is an instrument. Why?

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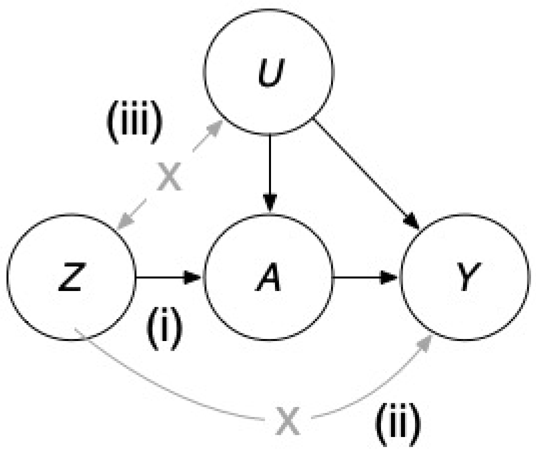


In the double-blind randomized trial described above, the randomization indicator Z is an instrument. Why?

- Condition (i) is met because trial participants are more likely to receive treatment if they were assigned to treatment,
- condition (ii) is expected by the double-blind design, and
- condition (iii) is expected by the random assignment of Z.

The figure in this slide depicts a special case in which the instrument Z has a causal effect on the treatment A. We then refer to Z as a *causal instrument*.

Parameter of interest

There are different parameters of interest. For instance, in the noncompliance example, the intention to treat effect (ITT) is what we care about:

> The effect of Z does not measure "the effect of treating with A" but rather "the effect of assigning participants to being treated with A" or "the effect of having the intention of treating with A," which is why the causal effect of randomized assignment Z is referred to as the intention-to-treat effect. (Hernan and Robins, 2020).

(For estimation, see two-stage least squares (2SLS) in a few slides.)

Example: Effect of smoking tobacco on health

- RA Fisher's opinion: Suppose a researcher wishes to estimate the causal effect of smoking on general health. Correlation between health and smoking does not imply that smoking causes poor health because other variables, such as depression, may affect both health and smoking, or because health may affect smoking. It is at best difficult and expensive to conduct controlled experiments on smoking status in the general population.
- Using IV: The researcher may attempt to estimate the causal effect of smoking on health from observational data by using the tax rate for tobacco products as an instrument for smoking.
- IV conditions met: The tax rate for tobacco products is a reasonable choice for an instrument because the researcher assumes that it can only be correlated with health through its effect on smoking (exclusion). If the researcher then finds tobacco taxes and state of health to be correlated, this may be viewed as evidence that smoking causes changes in health.

Example: Effect of tutoring on GPA

- Question of interest: Suppose that we wish to estimate the effect of a university tutoring program on grade point average (GPA). The relationship between attending the tutoring program and GPA may be confounded by a number of factors.
- Candidate for instrument: If students are assigned to dormitories at random, the proximity of the student's dorm to the tutoring program is a natural candidate for being an instrumental variable.
- Does not satisfy exclusion: However, what if the tutoring program is located in the college library? In that case, Proximity may also cause students to spend more time at the library, which in turn improves their GPA. Thus, proximity does not qualify as an instrumental variable because it is connected to GPA.

Reference: https://en.wikipedia.org/wiki/Instrumental_variables_estimation#cite_note-Leigh:00-9

Common instruments in observational studies

Some commonly used categories of candidate instruments are:

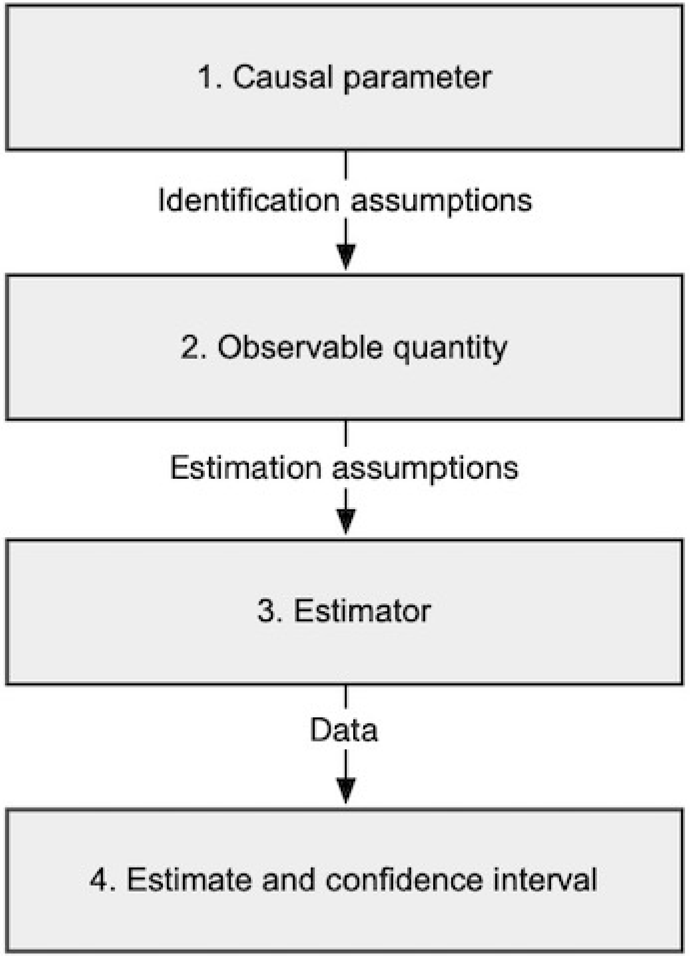
- Genetic factors: For example, when estimating the effects of alcohol intake on the risk of coronary heart disease, Z can be a polymorphism associated with alcohol metabolism (say, ALDH2 in Asian populations). Causal inference from observational data via IV estimation using genetic variants is part of the framework known as *Mendelian randomization*.

- Access: The proposed instrument Z is a measure of access to the treatment. The idea is that access impacts the use of treatment A but does not directly affect the outcome Y. For example, physical distance or travel time to a facility has been proposed as an instrument for treatments available at such facilities (McClellan et al. 1994, Card 1995, Baiocchi et al. 2010). Another example: calendar period has been proposed as an instrument for a treatment whose accessibility varies over time (Hoover et al. 1994, Detels et al. 1998).

- Arbitrary cutoffs: Look out for these! (e.g. letter of the alphabet to get into a class, date of birth for getting into a grade).

References: Hernan and Robins.

How do we estimate a causal parameter?



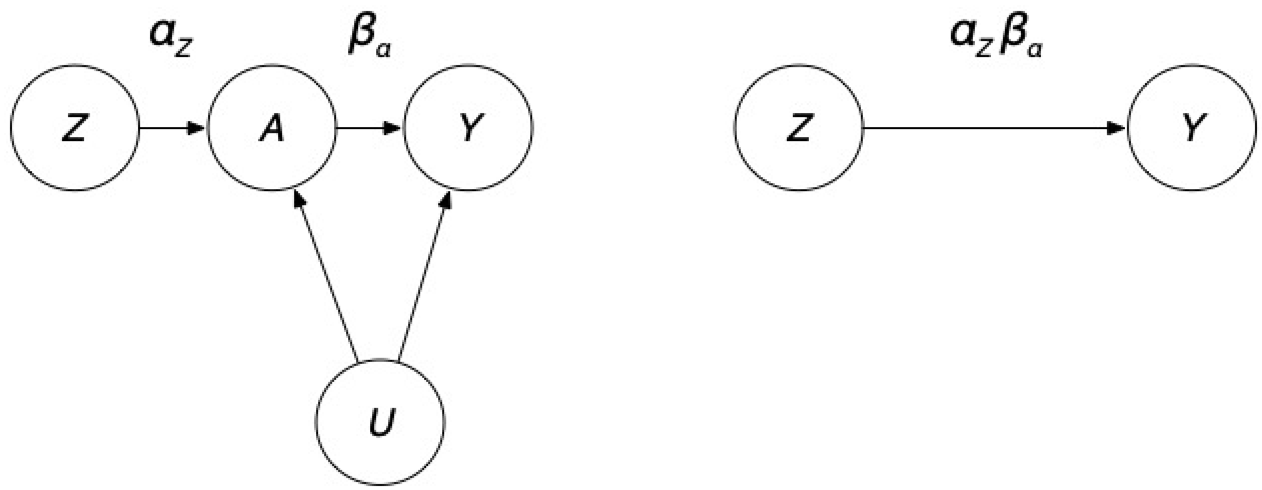
Estimation by using two-stage least squares

What causal effects can I get out of the graph? Regression of A on Z, and regression of Y on that result.

Two-stage least squares (2SLS):

Stage 1: Regress A on Z: $A = Z\alpha + \epsilon_1$, and save the predicted values from that regression.

Stage 2: Regress Y on the predicted values from the first stage: $Y = \hat{A}\beta + \epsilon_2$.



Instrument strength

- Need the instrument to be "strong". What does this mean?
  - We need the effect of A on Z to be strong because we will be dividing by it. If you divide by a tiny number sometimes the β_A effect will blow up.
  - Lots of new research about what to do if you have a weak IV instrument.
- There's a lot more on this topic, but we are out of time!