

# Statistics II

Week 5: **Instrumental Variables**

# Content for Today

1. Info about final exam review session
2. Review of core concepts from lecture
3. Application paper
4. Instrumental variables in R

# Final Exam Review

# Final Exam Review Sessions

- The labs during the last week of class will serve as review sessions for the final exam (because there is no assignment for that week).
- We will send out a list of all of the “topics” you should know about for the final exam in advance (likely during the midterm week).
- Send us your questions/areas you aren’t clear about **in advance**, by March 29th.
- We will prepare material based on those questions.

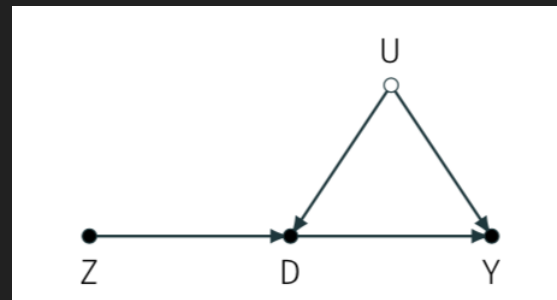
# Lecture Review

# Motivation

- Often we cannot force subjects to take a treatment, and those who choose to take a treatment may systematically differ from those who do not (selection bias).
- So, we can focus on **randomized encouragement** to take the treatment instead.
- **Instrumental variable studies** are the observational-study cousin of randomized encouragement.

# Instrumental Variables

- **Basic idea**: If you're trying to estimate the effect of  $D$  on  $Y$ , but there are unobserved confounders  $U$ , we can use an exogenous variable  $Z$  that isn't affected by  $U$  to measure the unique variation in  $D$  from  $Z$ .
- To study this, we split the variation of  $D$  into **two parts**:
  - One **potentially related to** (potentially unobserved) **confounders  $U$**
  - One **truly exogenous**
- To find the portion of  $D$  unrelated to confounders, one needs a variable  $Z$  (the instrumental variable) that is (as if) randomly assigned and related to  $D$



# For Example

- Let's say you wanted to investigate the link between depression (D) and smoking (Y).
- Lack of job opportunities (Z) could lead to depression, but it is only associated with smoking through its association with depression (i.e. there isn't a direct correlation between lack of job opportunities and smoking).
- This third variable, Z (lack of job opportunities), can generally be used as an instrumental variable if it can be measured and its behavior can be accounted for.



# Intent to Treat Effect (ITT)

- Effect of encouragement itself on the outcome regardless of actual treatment. It only considers assignment to the treatment or control groups.
- Because  $Z_i$  is randomized, ITT is identified by difference in means between the encouraged and unencouraged:

$$ITT = E(Y_i | Z_i = 1) - E(Y_i | Z_i = 0)$$

# Compliance Types

- Some people will always take the treatment, regardless of whether they are in treatment or control (**always-takers**),
- and some never will (**never-takers**).
- Some will always do as their told (**compliers**),
- and some will always do the opposite (**defiers**).

	$Z_i = 0$	$Z_i = 1$
$D_i = 0$	Complier/Never-taker	Defier/Never-taker
$D_i = 1$	Defier/Always-taker	Complier/Always-taker

We cannot directly identify the group to which any particular respondent belongs.

# IV Assumptions

1. Relevance, or nonzero average encouragement effect.
  - Encouragement needs to make a difference.
  - Testable - we can see if there's is a difference between treatment and control groups.
2. Exogeneity/Ignorability of the instrument: Potential treatments and outcomes must be independent of  $Z$  (no OVB).
  - Hypothetical potential outcomes shouldn't be related to  $Z$ .
  - Given by quasi-randomization of encouragement; matter of plausibility.
  - Not empirically testable.

# IV Assumptions

3. Exclusion restriction: Instrument affects outcome only via treatment.
  - a. Potential outcome under encouragement should be same as potential outcome under control regardless of whether treated with encouragement or not.
  - b. Implies that variation in the instrument does not change the potential outcomes other than through its effect on D
  - c. Implies zero ITT effect for always-takers/never-takers.
  - d. Hardly testable!
4. Monotonicity: No defiers.
  - a. Also hardly testable; matter of plausibility

# LATE (with binary D and Y)

- ITT can be decomposed into different subgroups.
- Global ITT is the sum of:
  - The intent to treat for those who comply +
  - The intent to treat for always-takers +
  - The ITT for never-takers +
  - The ITT for defiers,
  - based on the proportion of how many are in each group.
- Under monotonicity and exclusion restriction, this simplifies as:
$$\text{ITT}_{\text{compliers}} \times \Pr(\text{compliers})$$
- $\text{ITT}_{\text{c}}$  can then be interpreted as the Local Average Treatment Effect (LATE)
  - However, compliers are defined in terms of principal strata, so we can never identify who they actually are, and different encouragement (instrument) yields different compliers.

# Calculating LATE

LATE can be calculated using the **Wald Estimator**:

$$(\text{Cov}(Y_i, Z_i) / \text{Cov}(D_i, Z_i))$$

or **Two-Stage Least Squares (2SLS)**:

1. Regress cause D on instrument Z
2. Regress outcome Y on estimate of cause D

2SLS only retains the variation in D that is generated by quasi-experimental variation in Z.

# Opium for the Masses

(Kern & Hainmueller, 2009)

# Research Setup

In this case study of the impact of West German television on public support for the East German communist regime, we evaluate the conventional wisdom in the democratization literature that foreign mass media undermine authoritarian rule. We exploit formerly classified survey data and a natural experiment to identify the effect of foreign media exposure using instrumental variable estimators. Contrary to conventional wisdom, East Germans exposed to West German television were more satisfied with life in East Germany and more supportive of the East German regime. To explain this surprising finding, we show that East Germans used West German television primarily as a source of entertainment. Behavioral data on regional patterns in exit visa applications and archival evidence on the reaction of the East German regime to the availability of West German television corroborate this result.



# Compliance Types

We follow Imbens and Angrist (1994) in conceptualizing IV identification in terms of potential treatment indicators. Let  $D_z$  represent potential treatment status given  $Z = z$ . For example,  $D_0 = 0$  and  $D_1 = 1$  means that a respondent would not watch West German television if she lived in the Dresden district but that she would watch West German television if she did not live in the Dresden district. The treatment status indicator can then be expressed as  $D = ZD_1 + (1 - Z) \cdot D_0$ . Similar to the missing data problem for potential outcomes, we only get to observe  $Z$  and  $D$  (and therefore  $D_z$  for individuals with  $Z = z$ ) but never both potential treatment indicators for the same individual. Following the terminology in Angrist, Imbens, and Rubin (1996), we can distinguish four groups of respondents:

**Compliers:**  $D_0 = 0$  and  $D_1 = 1$ . Compliers are respondents who watch West German television if they do not live in Dresden but who do not watch West German television if they live in Dresden.

**Always-takers:**  $D_1 = D_0 = 1$ . These are respondents who always watch West German television, no matter where they live.

**Never-takers:**  $D_1 = D_0 = 0$ . Similarly, these are respondents who never watch West German television, no matter where they live.

**Defiers:**  $D_0 = 1$  and  $D_1 = 0$ . These are respondents who watch West German television if they live in Dresden but who do not watch West German television if they do not live in Dresden.

# Assumptions

- **Relevance:** Is there an observable difference between support for the East German regimes in areas with and without access to West German tv?
  - Yes. Respondents living in the Dresden district were much less likely to watch West German television than respondents living in other districts.
- **Ignorability** of the instrument: Place of residence “as good as” randomly assigned?
  - Would be challenged if East Germans who desired to watch West German television might have moved away from the Dresden district. If interest in West German television was correlated with regime support, which seems likely, this kind of sorting behavior would invalidate our instrument.

# Assumptions

- **Exclusion** restriction: Does area of residence change support for the regime in other ways besides access to West German tv?
  - Would be in trouble if Dresden was systematically different from other districts.
- **Monotonicity**: Were there East Germans who would have watched West German television if they had lived in Dresden but who would not have watched West German television if they had not lived in Dresden?
  - Probably not.

# Findings

Our main result is that **exposure to West German television increased support for the East German regime**. We argue that the best explanation for this counterintuitive finding is that **East Germans used West German television primarily as a source of entertainment**. In a society with a very limited number of entertainment options, the ability to watch West German television offered them a **vicarious escape** from the scarcities, the queues, and the ideological indoctrination, making life under communism more bearable and the East German regime more tolerable. We do not necessarily argue that West German television's political content did not undermine public support for the East German regime at all. However, the evidence shows that the net effect of West German television exposure was an increase in regime support.

Questions?