

# Instrumental Variables

EC 425/525, Set 8

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15 May 2019

# Prologue

# Schedule

## Last time

Matching and propensity-score methods

- Conditional independence
- Overlap

## Today

Instrumental variables (and two-stage least squares)

## Upcoming

- Assignment due Sunday
- Proposal due Wednesday 5/22
- Midterm?

# Research designs

# Research designs

## Selection on observables and/or unobservables

We've been focusing on **selection-on-observables designs**, i.e.,

$$(Y_{0i}, Y_{1i}) \perp\!\!\!\perp D_i | X_i$$

for **observable** variables  $X_i$ .

**Selection-on-unobservable designs** replace this assumption with two new (but related) assumptions

1.  $(Y_{0i}, Y_{1i}) \perp Z_i$
2.  $\text{Cov}(Z_i, D_i) \neq 0$

# Research designs

## Selection on observables and/or unobservables

Our main goal in causal-inference minded (applied) econometrics boils down to isolating **"good" variation** in  $\mathbf{D}_i$  (exogenous/as-good-as-random) from **"bad" variation** (the part of  $\mathbf{D}_i$  correlated with  $\mathbf{Y}_{0i}$  and  $\mathbf{Y}_{1i}$ ).

(We want to avoid selection bias.)

- **Selection-on-observables designs** assume that we can control for all *bad variation* (selection) in  $\mathbf{D}_i$  through a known (observed)  $\mathbf{X}_i$ .
- **Selection-on-unobservables designs** assume that we can extract part of the *good variation* in  $\mathbf{D}_i$  (generally using some  $\mathbf{Z}_i$ ) and then use this *good part* of  $\mathbf{D}_i$  to estimate the effect of  $\mathbf{D}_i$  on  $\mathbf{Y}_i$ . We throw away the *bad variation* in  $\mathbf{D}_i$  (it's bad).

# Research designs

## Which route?

So set of research designs is more palatable?

1. There are plenty of bad applications of both sets.

Violated assumptions, bad controls, *etc.*

1. **Selection on observables** assumes we know *everything* about selection into treatment—we can identify *all* of the good (or bad) variation in  $\mathbf{D}_i$ .

Tough in non-experimental settings. Difficult to validate in practice.

1. **Selection on unobservables** assumes we can isolate *some* good/clean variation in  $\mathbf{D}_i$ , which we then use to estimate the effect of  $\mathbf{D}_i$  on  $\mathbf{Y}_i$ .

Seems more plausible. Possible to validate. May be underpowered.

# Instrumental variables

## Introduction

**Instrumental variables** (IV)<sup>†</sup> is the canonical selection-on-unobservables design—isolating *good variation* in  $\mathbf{D}_i$  via some magical **instrument**  $\mathbf{Z}_i$ .

Consider some model (structural equation)

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{D}_i + \varepsilon_i \quad (1)$$

To guarantee consistent OLS estimates for  $\beta_1$ , want  $\text{Cov}(\mathbf{D}_i, \varepsilon_i) = 0$ .  
In general, this is a heroic assumption.

*Alternative:* Estimate  $\beta_1$  via instrumental variables.

<sup>†</sup> For the moment, we're lumping together IV and two-stage least squares (2SLS) together—as many people do—even though they are technically different.



# Instrumental variables

## Definition

For our model

$$Y_i = \beta_0 + \beta_1 D_i + \varepsilon_i \quad (1)$$

A valid **instrument** is a variable  $Z_i$  such that

1.  $\text{Cov}(Z_i, D_i) \neq 0$

our **instrument** correlates with treatment (so we can keep part of  $D_i$ )

1.  $\text{Cov}(Z_i, \varepsilon_i) = 0$

our **instrument** is uncorrelated with other (non- $D_i$ ) determinants of  $Y_i$ ,  
i.e.,  $Z_i$  is excludable from equation (1). (**exclusion restriction**)

# Instrumental variables

## Example

Back to the returns to a college degree,

$$\text{Income}_i = \beta_0 + \beta_1 \text{Grad}_i + \varepsilon_i$$

OLS is likely biased.

What if that state conducts a (random) **lottery** for scholarships?

Let **Lottery**<sub>*i*</sub> denote an indicator for whether *i* won a lottery scholarship.<sup>†</sup>

1.  $\text{Cov}(\text{Lottery}_i, \text{Grad}_i) \neq 0$  ( $> 0$ ) if scholarships increase grad. rates.
1.  $\text{Cov}(\text{Lottery}_i, \varepsilon_i) = 0$  since the lottery is randomized.

<sup>†</sup> We'll have to focus on families who were eligible/who applied.

# Instrument variables

## The IV estimator

The IV estimator for our model

$$Y_i = \beta_0 + \beta_1 D_i + \varepsilon_i \quad (1)$$

with (valid) instrument  $Z_i$  is

$$\hat{\beta}_{IV} = (Z'D)^{-1} (Z'Y)$$

If you have no covariates, then

$$\hat{\beta}_{IV} = \frac{\text{Cov}(Z_i, Y_i)}{\text{Cov}(Z_i, D_i)}$$

# Instrument variables

## The IV estimator

The IV estimator for our model

$$Y_i = \beta_0 + \beta_1 D_i + \varepsilon_i \quad (1)$$

with (valid) instrument  $Z_i$  is

$$\hat{\beta}_{IV} = (Z'D)^{-1} (Z'Y)$$

If you have additional (exogenous) covariates  $X_i$ , then

$$Z = [Z_i \quad X_i]$$

$$D = [D_i \quad X_i]$$

# Instrumental variables

## Proof: Consistency

With a valid instrument  $\mathbf{Z}_i$ ,  $\hat{\beta}_{IV}$  is a consistent estimator for  $\beta_1$  in

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{X}_i + \varepsilon_i \quad (1)$$

$$\text{plim}(\hat{\beta}_{IV})$$

$$= \text{plim} \left( (\mathbf{Z}'\mathbf{D})^{-1} (\mathbf{Z}'\mathbf{Y}) \right)$$

$$= \text{plim} \left( (\mathbf{Z}'\mathbf{D})^{-1} (\mathbf{Z}'\mathbf{D}\beta + \mathbf{Z}'\varepsilon) \right)$$

$$= \text{plim} \left( (\mathbf{Z}'\mathbf{D})^{-1} (\mathbf{Z}'\mathbf{D}) \beta \right) + \text{plim} \left( \frac{1}{N} \mathbf{Z}'\mathbf{D} \right)^{-1} \text{plim} \left( \frac{1}{N} \mathbf{Z}'\varepsilon \right)$$

$$= \beta \quad \checkmark$$

# Two-stage least squares

# Two-stage least squares

## Setup

You'll commonly see IV implemented as a two-stage process known as **two-stage least squares** (2SLS).

**First stage** Estimate the effect of the instrument  $\mathbf{Z}_i$  on our endogenous variable  $\mathbf{D}_i$  and (predetermined) covariates  $\mathbf{X}_i$ . Save  $\hat{\mathbf{D}}_i$ .

$$\mathbf{D}_i = \gamma_1 \mathbf{Z}_i + \gamma_2 \mathbf{X}_i + u_i$$

**Second stage** Estimate model we wanted—but only using the variation in  $\mathbf{D}_i$  that correlates with  $\mathbf{Z}_i$ , i.e.,  $\hat{\mathbf{D}}_i$ .

$$\mathbf{Y}_i = \beta_1 \hat{\mathbf{D}}_i + \beta_2 \mathbf{X}_i + \varepsilon_i$$

*Note* The controls  $\mathbf{X}_i$  must match in the first and second stages.

# Two-stage least squares

## IV estimation

This two-step procedure, with a valid instrument, produces an estimator  $\hat{\beta}_1$  that is consistent for  $\beta_1$ .

$$\hat{\beta}_{2SLS} = (\mathbf{D}'\mathbf{P}_Z\mathbf{D})^{-1} (\mathbf{D}'\mathbf{P}_Z\mathbf{Y})$$

$$\mathbf{P}_Z = \mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'$$

where  $\mathbf{D}$  is a matrix of our treatment and predetermined covariates ( $\mathbf{X}_i$ ) and  $\mathbf{Z}$  is a matrix of our instrument and our predetermined covariates.



# Two-stage least squares

## IV estimation

### Important notes

- The controls ( $\mathbf{X}_i$ ) must match in the first and second stages.
- If you have exactly **one instrument** and exactly **one endogenous variable**, then 2SLS and IV are identical.
- Your second-stage standard errors are not correct.

# Two-stage least squares

## The reduced form

In addition to the regressions within the two stages of 2SLS

$$1. D_i = \gamma_1 Z_i + \gamma_2 X_i + u_i$$

$$2. Y_i = \beta_1 \hat{D}_i + \beta_2 X_i + \varepsilon_i$$

there is a third important and related regression: the reduced form.

The **reduced form** regresses the outcome  $Y_i$  (LHS of the second stage) on our instrument  $Z_i$  and covariates  $X_i$  (RHS of the first stage).

$$Y_i = \pi_1 Z_i + \pi_2 X_i + u_i$$

Thus, the reduced form provides a consistent estimate of the causal effect of our instrument on the outcome.

# Two-stage least squares

## The reduced form, continued

While the reduced form estimates the causal effect of the instrument on our outcome, we're often actually interested in the effect of *treatment* ( $\mathbf{D}_i$ ).

That said, the reduced form is still incredibly helpful/important:

- Clarifies your source of identifying variation.
- Does not suffer from *weak instruments* problems.
- Only requires  $\text{Cov}(\mathbf{Z}_i, \varepsilon_i) = 0$ .
- Offers insights into your estimates

$$\hat{\beta}_1^{2SLS} = \frac{\hat{\pi}_1}{\hat{\gamma}_1}$$

when you have exactly one instrument

# Two-stage least squares

## The reduced form, intuition

This expression for the 2SLS (and IV) estimator can be very helpful.

$$\hat{\beta}_1^{2SLS} = \frac{\hat{\pi}_1}{\hat{\gamma}_1} = \frac{\text{Reduced-form estimate}}{\text{First-stage estimate}}$$

What's the interpretation/intuition?

Back to our example:  $\hat{\beta}_1$  = est. effect of college graduation on income.

$\hat{\pi}_1$  gives the estimated causal effect of the scholarship lottery on income ,  
but what share of lottery winners graduate? We need to rescale if  $< 100\%$ .

$\hat{\gamma}_1$  estimates the effect of winning the scholarship lottery on graduation —  
the share of winners who graduated due to winning. We can scale with  $\hat{\gamma}_1$ !

# Two-stage least squares

## The reduced form, example

To see why this scaling makes sense, imagine that 50% of lottery winners graduate from college due to the lottery, *i.e.*,  $\hat{\gamma}_1 = 0.50$ .<sup>†</sup>

Our reduced-form estimate of  $\hat{\pi}_1 = \$5,000$  says that lottery winners make \$5,000 more than the control group, on average.

However, half of the winners did not graduate, so  $\hat{\pi}_1$  "underestimates" the effect of college graduation by combining graduates by nongraduates.

Thus, we want to double  $\hat{\pi}_1$ , *i.e.*, divide by  $\hat{\gamma}_1$ :  $\hat{\pi}_1 / \hat{\gamma}_1 = \$5,000 / 0.5 = \$10,000$ .

<sup>†</sup> Imagine none of the applicants would have graduated otherwise

# Two-stage least squares

Q How do we get this magical expression?  $\left( \hat{\beta}_1^{\text{IV}} = \frac{\hat{\pi}_1}{\hat{\gamma}_1} \right)$

## Derivation

$$\begin{aligned}\hat{\beta}_1^{\text{IV}} &= (\mathbf{Z}'\mathbf{D})^{-1} (\mathbf{Z}'\mathbf{Y}) \\ &= (\tilde{\mathbf{Z}}'\tilde{\mathbf{D}})^{-1} (\tilde{\mathbf{Z}}'\mathbf{Y}) \quad \text{applying FWL to reduce } \mathbf{D} \text{ and } \mathbf{Z} \text{ to vectors.} \\ &= \frac{\text{Cov}(\tilde{\mathbf{Z}}_i, \mathbf{Y}_i)}{\text{Cov}(\tilde{\mathbf{Z}}_i, \tilde{\mathbf{D}}_i)} = \frac{\text{Cov}(\tilde{\mathbf{Z}}_i, \mathbf{Y}_i) / \text{Var}(\tilde{\mathbf{Z}}_i)}{\text{Cov}(\tilde{\mathbf{Z}}_i, \tilde{\mathbf{D}}_i) / \text{Var}(\tilde{\mathbf{Z}}_i)} \\ &= \frac{\hat{\pi}_1}{\hat{\gamma}_1} \quad \checkmark\end{aligned}$$

Let's push a bit deeper into IV's mechanics and intuition.

# IV: Mechanics and intuition

## Setup

In this section, we'll use medical trials as a working example.<sup>†</sup>

We are interested in the regression model for the effect of some treatment (e.g., blood-pressure medication) on medical outcome  $Y_i$

$$Y_i = \beta_0 + \beta_1 D_i + \varepsilon_i$$

$D_i$  indicates whether  $i$  takes the treatment (medication).  $\varepsilon_i$  captures all other factors that affect  $Y_i$ . Or in potential-outcomes framework:

$$\begin{aligned} Y_i &= Y_{1i} D_i + Y_{0i} (1 - D_i) \\ Y_{0i} &= \beta_0 + \varepsilon_i \\ Y_{1i} &= Y_{0i} + \beta_1 \end{aligned}$$

<sup>†</sup> Credit/thanks go to [Michael Anderson](#) for this example—and much of these notes.



# IV: Mechanics and intuition

## Research design

*Goal* **Estimate the effect of blood-pressure medication** on blood pressure.

*Challenge* **Selection bias**: Even if treatment reduces blood pressure, selection bias will fight against the estimated effect.

*Solution* **Randomized medical trial**: Ask randomly chosen individuals in treatment group to take the pill. Control individual get placebo (or nothing).

*Analysis 1* **Intention to treat (ITT)**:  $\hat{\beta}_1^{\text{ITT}} = \bar{Y}_{\text{Trt}} - \bar{Y}_{\text{Ctrl}}$

*ITT problem* **Bias from noncompliance**: People don't always follow rules. E.g., treated folks who don't take pills; control folks who take pills.

*Analysis 2* **IV!** Instrument medication  $D_i$  with intention to treat  $Z_i$ .

# IV: Mechanics and intuition

## The IV solution

First question: Is  $\mathbf{Z}_i$  a valid instrument for  $\mathbf{D}_i$ ?

1.  $\text{Cov}(\mathbf{Z}_i, \varepsilon_i) = 0$  as  $\mathbf{Z}_i$  was randomly assigned (exclusion restriction).
1.  $\text{Cov}(\mathbf{Z}_i, \mathbf{D}_i) \neq 0$  if assignment to treatment changes the likelihood you take the pills (first stage).

$\therefore \mathbf{Z}_i$  is a valid instrument for  $\mathbf{D}_i$  and IV consistently estimates  $\beta_1$ .

# IV: Mechanics and intuition

## Noncompliance

**Noncompliant** individuals do not abide by their treatment assignment.

Let's see how IV "solves" this problems.

First, assume noncompliance only affects treated individuals—*i.e.*, treated folks sometimes don't take their pills; control folks never take pills.

# IV: Mechanics and intuition

## Noncompliance, continued

The **first stage** recovers the share of treatment individuals who take the pill

$$D_i = \gamma_1 Z_i + u_i$$

*i.e.*, if 50% of treated individuals take the medication,  $\hat{\gamma} = 0.50$ .

The **reduced form** estimates the *ITT*

$$Y_i = \pi_1 Z_i + v_i$$

which we know IV rescales using the first stage

$$\hat{\beta}_1^{\text{IV}} = \frac{\hat{\pi}_1}{\hat{\gamma}_1} = \frac{\hat{\pi}_1}{0.50} = 2 \times \hat{\pi}_1$$

# IV: Mechanics and intuition

## Noncompliance, continued

IV solves the noncompliance issue by rescaling by the rate of compliance.

If everyone perfectly complies, then  $\hat{\gamma}_1 = 1$  and  $\hat{\beta}_1^{\text{IV}} = \hat{\pi}_1/1 = \hat{\beta}_1^{\text{ITT}}$ .

Further example  $N_{\text{Trt}} = 10$ ; trt. compliance = 50%; ctrl. compliance = 100%.

$$\bar{Y}_{\text{Trt}} = \frac{5(\beta_0 + \beta_1) + 5(\beta_0)}{10} = \beta_0 + \frac{\beta_1}{2} \text{ and } \bar{Y}_{\text{Ctrl}} = \beta_0.$$

So our reduced-form estimate (the ITT) is  $\hat{\gamma}_1 = \frac{\beta_1}{2}$  (half the true effect).

IV consistently estimates  $\beta_1$  via rescaling the ITT by the rate of compliance

$$\hat{\beta}_1^{\text{IV}} = \frac{\pi}{\gamma} = \frac{\beta_1/2}{1/2} = \beta_1$$

# IV: Mechanics and intuition

## Takeaways

### Main points

1. IV **rescales** the causal effect of  $Z_i$  on  $Y_i$  by the causal effect of  $Z_i$  on  $D_i$ .
1. IV **does not** compare treated compliers to untreated compliers.  
Such a comparison/estimator would re-introduce selection bias.

Thus far, we assumed homogeneous treatment effects.

Q What happens **when treatment effects are heterogeneous**?

**A** Let's recall what our instruments are doing (with Venn diagrams!).



Figure 1

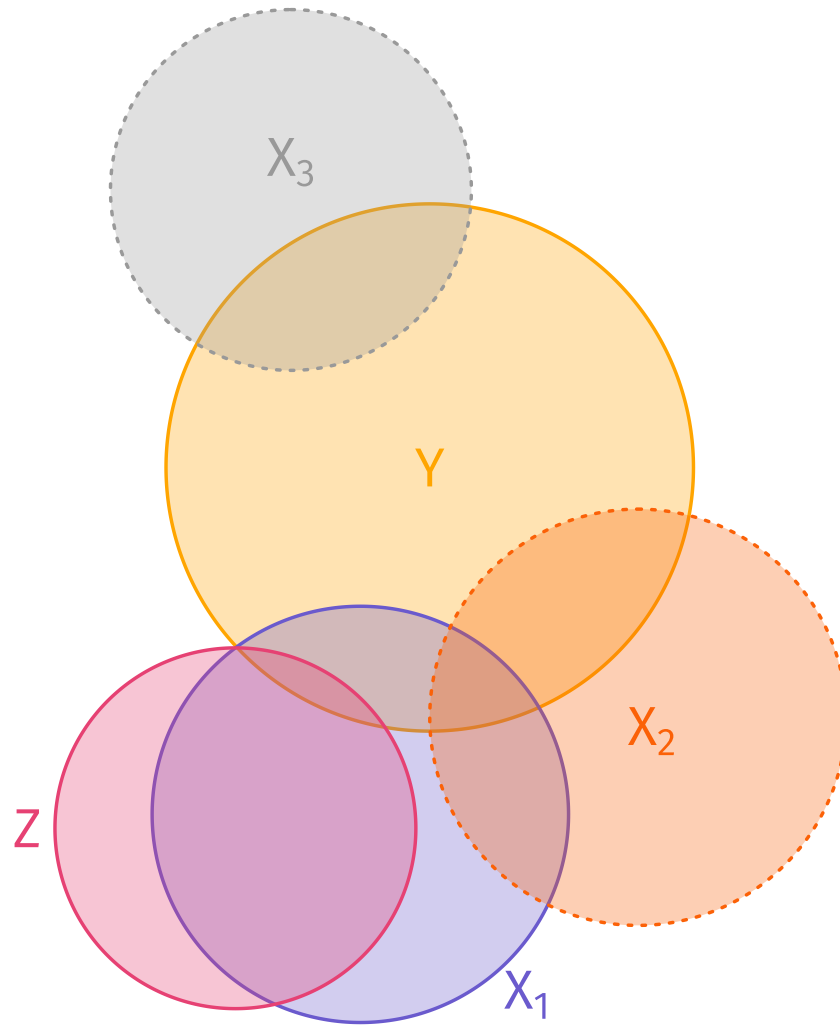


Figure 2

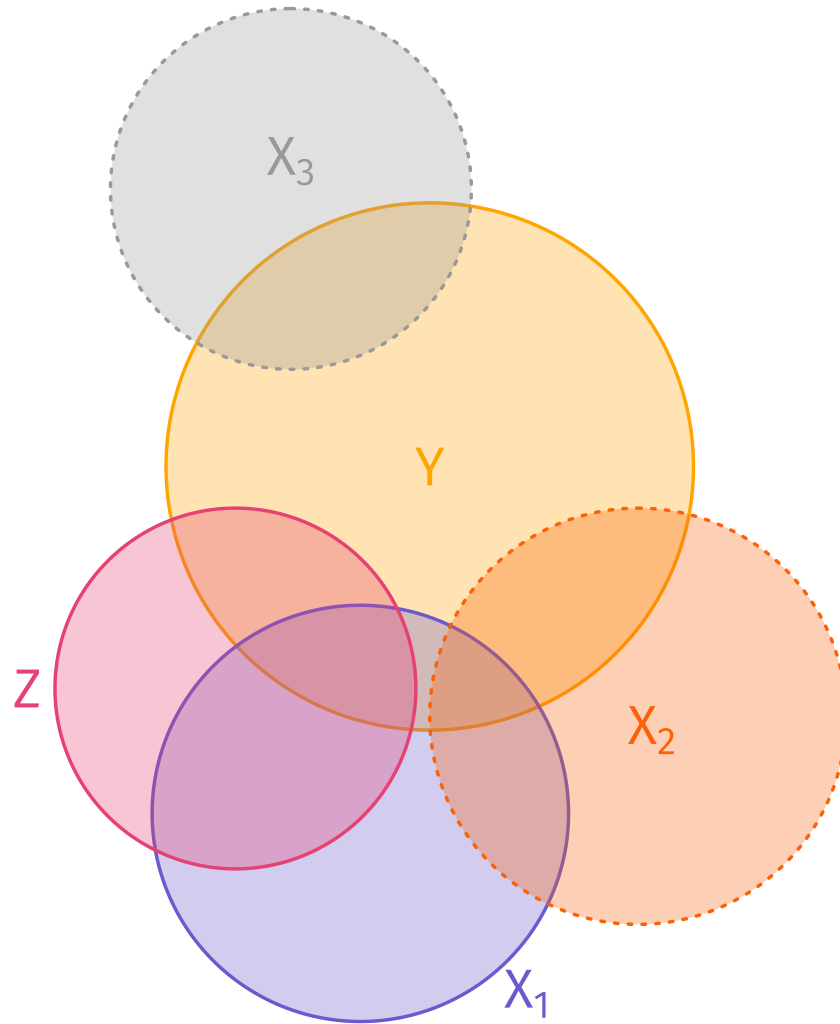


Figure 3

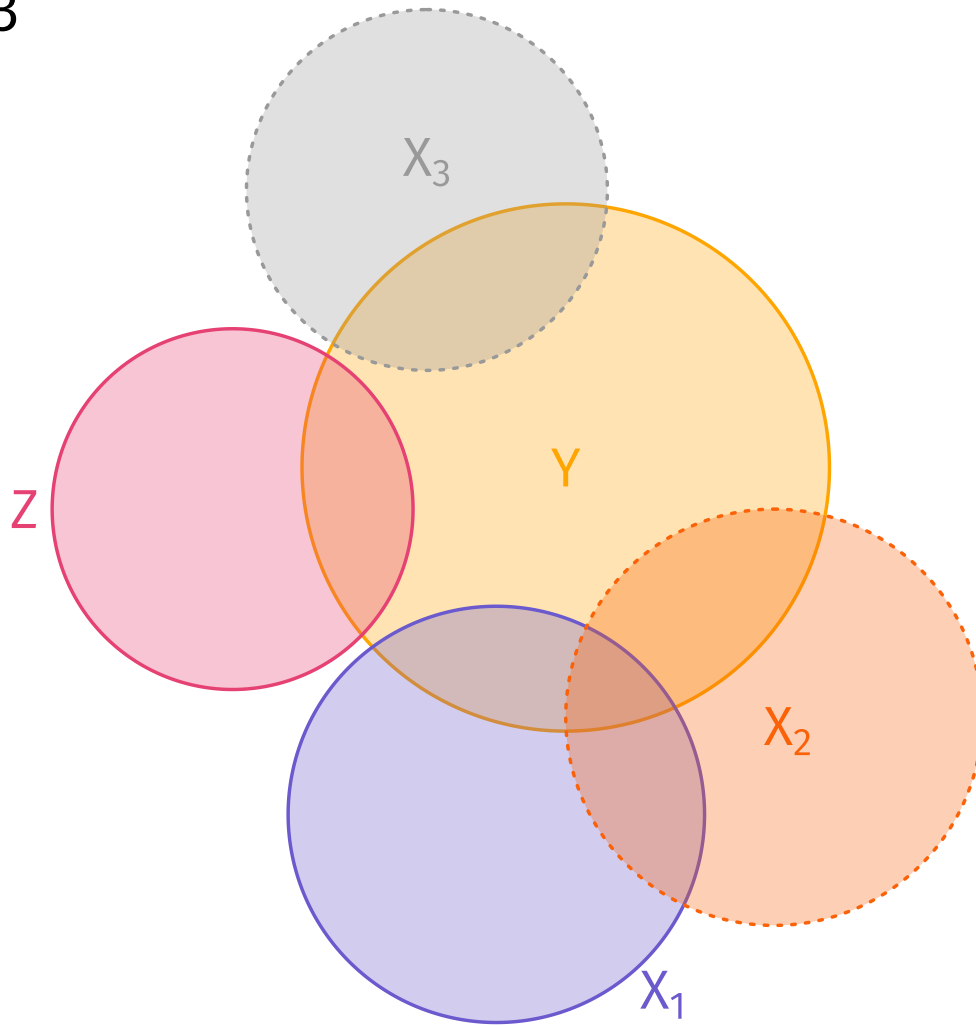


Figure 4

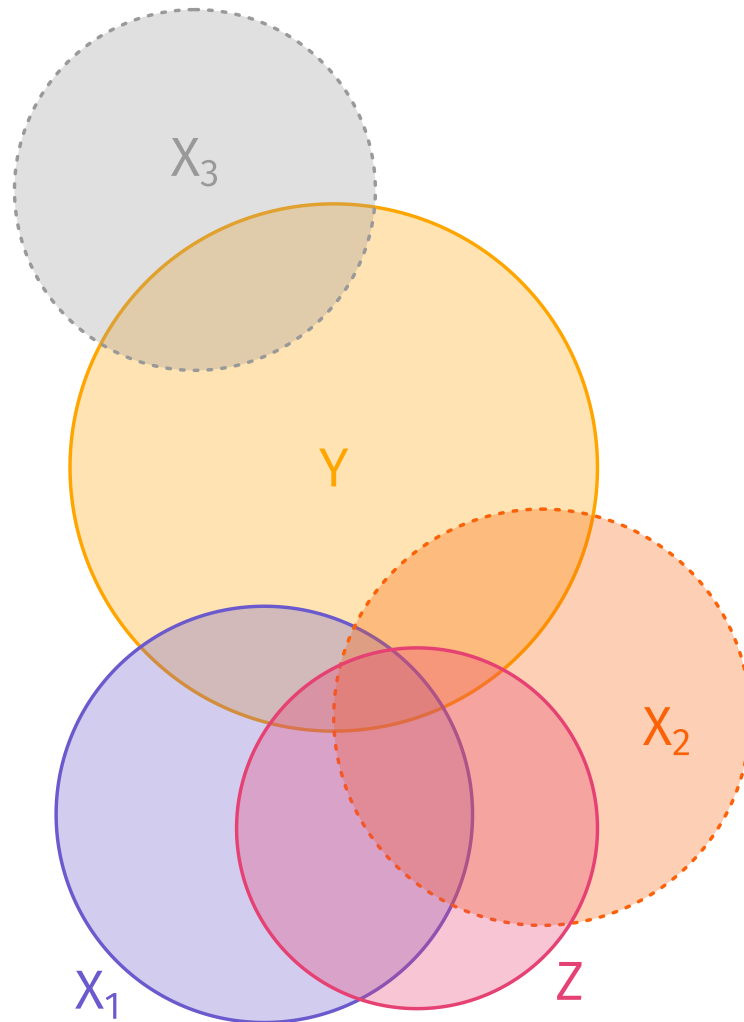
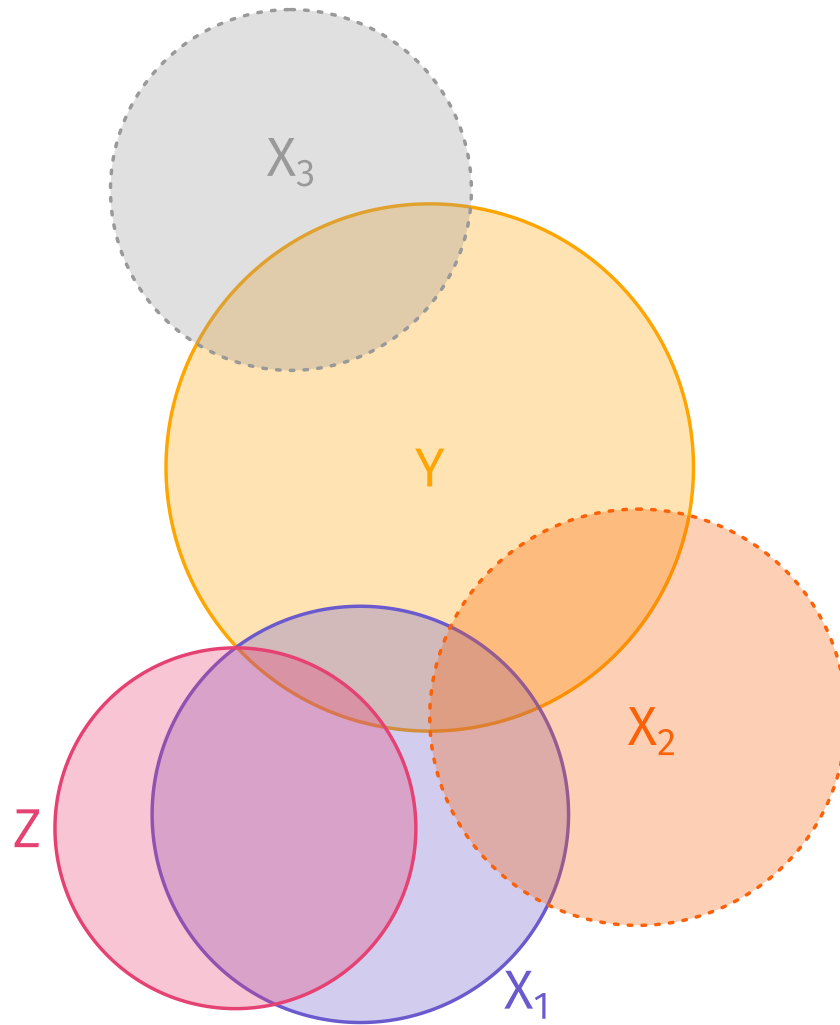


Figure 1



# IV + heterogeneity

## Recap

Throughout the course, we've discussed two concepts of treatment effects.

1. **Average treatment effect (ATE)** The average treatment effect for an individual randomly drawn from our sample.
1. **Treatment on the treated (TOT)** The average treatment effect for a ***treated*** individual randomly drawn from our sample.

When we assume homogeneous/constant treatment effects,  $ATE = TOT$ .

**Q** If treatment effects vary, then what do IV and 2SLS estimate?

**A** Not ATE. And not TOT. They estimate the LATE.<sup>†</sup>

<sup>†</sup> See Angrist, Imbens, and Rubin (1996).

# IV + heterogeneity

## The LATE

IV generally estimates the **LATE**—the **Local Average Treatment Effect**.

*Recall* IV "works" by isolating variation in  $D_i$  induced by our instrument  $Z_i$ .

In other words: IV focuses on the individuals whose  $D_i$  changes due to  $Z_i$ .

Angrist, Imbens, and Rubin (1996) call these folks **compliers**.

However, *compliers* are only one of four possible groups.

1. **Compliers**  $D_i = 1$  iff  $Z_i = 1$ .

Only take pills **when treated**.

2. **Always-takers**  $D_i = 1 \forall Z_i$ .

**Always** take pills.

3. **Never-takers**  $D_i = 0 \forall Z_i$ .

**Never** take pills.

4. **Defiers**  $D_i = 1$  iff  $Z_i = 0$ .

Only take pills **when untreated**.

# IV + heterogeneity

## The LATE

Because IV only uses variation in  $\mathbf{D}_i$  that correlates with  $\mathbf{Z}_i$ , IV mechanically drops *always-takers* and *never-takers*.

Most IV derivations/applications assume away the existence of *defiers*.

Thus, IV estimates a treatment effect **using only compliers**.

Hence the "local" in *local average treatment effect*.



# IV + heterogeneity

## The LATE: Medical-trial example

Imagine treatment works for some ( $\beta_{1,i} < 0$ ) and not for others ( $\beta_{1,j} = 0$ ).

Suppose individuals know their response to blood-pressure medication.

- $\beta_{1,i} < 0$  individuals always take the pill.
- $\beta_{1,j} = 0$  individuals only take the pill when treated.

Then our compliers will be individuals for whom  $\beta_{1,j} = 0$ .

Thus, IV's LATE will indicate no treatment effect ( $\hat{\beta}_1^{\text{IV}} = 0$ ).

# IV + heterogeneity

## The LATE

**Q** So is IV actually inconsistent?

**A** It depends what you are trying to estimate (and how you interpret it).

IV doesn't estimate the ATE or TOT, so it would be inconsistent for them.<sup>†</sup>

IV estimates the *local* average treatment effect.

*Takeaway* Because IV identifies off of compliers, it estimates an average treatment effect for these individuals (who *comply* with the instrument).

*Takeaway<sub>2</sub>* Different instruments have different LATEs.

<sup>†</sup> Just as the TOT is not consistent for the ATE.

# IV + heterogeneity

## Monotonicity

We've already written down the two classical IV/2SLS assumptions

- *First stage*:  $\text{Cov}(Z_i, D_i) > 0$
- *Exclusion restriction*:  $\text{Cov}(Z_i, \varepsilon_i) = 0$

but we need a third assumption to get ensure IV's complier-based LATE interpretation.

- **Monotonicity (Uniformity)**:  $D_i(z) \geq D_i(z')$  or  $D_i(z) \leq D_i(z') \quad \forall i$   
**Heckman**: *Uniformity of responses across persons.*  
**Imbens and Angrist (1994)**: Instrument has monotone effect on  $D_i$ .

# IV + heterogeneity

## Monotonicity

If "defiers" exist, then monotonicity/uniformity is violated.

In this case, the IV estimand is

$$\frac{\tau_c \Pr(\text{complier}) - \tau_d \Pr(\text{defier})}{\Pr(\text{complier}) - \Pr(\text{defier})}$$

which is not bound between  $\tau_c$  and  $\tau_d$ .

Example  $\tau_c = 1$  and  $\tau_d = 2$ .  $\Pr(\text{complier}) = 2/3$  and  $\Pr(\text{defier}) = 1/3$ .

Then the "LATE" is 0.<sup>†</sup>

<sup>†</sup> Some people would instead say that there is no LATE when you violate monotonicity.

Until now, we've focused on using a single instrument.

The 2SLS estimator accomodates multiple instruments.<sup>†</sup>

<sup>†</sup> Whether you can find multiple valid instruments is another question.

Multiple instruments

# Multiple instruments

## Motivation

**Q** Why include multiple instruments?

**A** Multiple instruments can capture more variation in  $\mathbf{D}_i$  (efficiency).

Using terminology from the *system-of-equations* literature,

- one instrument for one endogenous variable: **just identified**
- multiple instruments for one endogenous variable: **over identified**

# Multiple instruments

## In practice

With (valid) instruments  $\mathbf{Z}_{1i}$  and  $\mathbf{Z}_{2i}$ , or first stage becomes

$$\mathbf{D}_i = \gamma_0 + \gamma_1 \mathbf{Z}_{1i} + \gamma_2 \mathbf{Z}_{2i} + \gamma_3 \mathbf{X}_i + u_i$$

while our second stage is still

$$\mathbf{Y}_i = \beta_0 + \beta_1 \hat{\mathbf{D}}_i + \beta_2 \mathbf{X}_i + v_i$$



# Multiple instruments

## Example: Quarter of birth

Back to our quest to estimate the returns to education.

Angrist and Krueger (1991) proposed *quarter of birth* as a set of instruments for years of schooling.

Accordingly, their first stage looks something like<sup>†</sup>

$$\begin{aligned}\text{Schooling}_i = & \gamma_0 + \gamma_1 \mathbb{I}(\text{Born Q1})_i + \gamma_2 \mathbb{I}(\text{Born Q2})_i \\ & + \gamma_3 \mathbb{I}(\text{Born Q3})_i + \gamma_4 \mathbb{I}(\text{Born Q4})_i \\ & + \gamma_5 \mathbf{X}_i + u_i\end{aligned}$$

<sup>†</sup> We need to drop one of the quarter-of-birth indicators to avoid perfect collinearity.

# Multiple instruments

## Example: Quarter of birth

**Q** Is quarter of birth a valid instrument?

**Q1** Why would quarter of birth affect schooling? (*First stage*)

**A1** Students cannot drop out of school until a certain age, and quarter of birth affects your age at the time you begin school.

**Example** Some states require students to stay in school until they are 16.

- Students who start school at age **6** drop out after **10** years of schooling.
- Students who start school at age **5** drop out after **11** years of schooling.

# Multiple instruments

## Example: Quarter of birth

If students must begin school in calendar year in which they turn 6

- December birthdates: begin school at 5.75; drop out with 10.25 yrs.
- January birthdates: begin school at 6.75; drop out with 9.25 yrs.

For some group, quarter of birth may affect the number of years in school.

# Multiple instruments

## Example: Quarter of birth

It turns out that the first stage is also pretty weak in this setting.

**Weak instruments** can cause several problems for 2SLS/IV:

1. Our estimator is a ratio of the reduced form and the first stage, so a weak first stage can blow up reduced-form estimates (amplifying reduced-form noise/bias).
1. Many weak instruments lead to a finite-sample issue in which 2SLS is biased toward OLS—our first stage is essentially overfitting.

What about our other requirements for a valid instrument?

# Multiple instruments

## Example: Quarter of birth

**Q2** Is quarter of birth uncorrelated with  $\varepsilon_i$  (*excludable*)?

**A2** While quarter of birth may be fairly arbitrary for some families, other families might time births.

If these birth timers differ from other couples along other dimensions (*e.g.*, income or education), then quarter of birth may correlate with  $\varepsilon_i$ .

# Multiple instruments

## Example: Quarter of birth

**Q3** Is the effect monotone?

**A3** Some<sup>†</sup> argue that monotonicity may be violated in this setting.

Consider December births.

- Original idea: December birthdates will start school at age 5.7, inducing more years of education before 16.
- *Redshirting* idea: Parents hold back December kids so they can be older (*i.e.*, 6.7), inducing fewer years of education before 16.

<sup>†</sup> *E.g.*, Aliprantis (2012)

# IV and 2SLS

## Conclusions

1. IV/2SLS focus on **isolating some "good" variation** in  $D_i$  via  $Z_i$ .
2. Important **requirements**: strong first stage, excludability, monotonicity.
3. IV and 2SLS **rescale the reduced form** with the first stage.
4. Estimates are **LATE from compliers**.
5. Different instruments can produce **different LATEs**.
6. A **weak first stage** can lead to problems.

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