# Instrumental Variables

## EC 607, Set 8

Edward Rubin
Spring 2020

# Prologue

# Schedule

## Last time

Matching and propensity-score methods

- Conditional independence
- Overlap

## Today

Instrumental variables (and two-stage least squares)

## Upcoming

Assignment 2

# Research designs

# Research designs

## Selection on observables and/or unobservables

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

$$(\mathrm{Y}_{0i},\ \mathrm{Y}_{1i}) \perp\!\!\!\perp \mathrm{D}_i | \mathbf{X}_i$$

for **observable** variables $\mathbf{X}_i$.

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

1. $(\mathrm{Y}_{0i},\ \mathrm{Y}_{1i}) \perp \mathbf{Z}_i$
2. $\mathrm{Cov}(\mathbf{Z}_i,\ \mathrm{D}_i) \neq 0$

# 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).

## Which route?

Which set of research designs is more palatable?

1. There are plenty of bad applications of both sets.
   Violated assumptions, bad controls, *etc.*

2. **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.

3. **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)[†] 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)

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{D}_i + \varepsilon_i \tag{1}$$

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

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

† 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

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{D}_i + \varepsilon_i \tag{1}$$

A valid **instrument** is a variable $\mathbf{Z}_i$ such that

1. $\mathbf{Cov}(\mathbf{Z}_i, \mathbf{D}_i) \neq 0$
   our instrument correlates with treatment (so we can keep part of $\mathbf{D}_i$)

2. $\mathbf{Cov}(\mathbf{Z}_i, \varepsilon_i) = 0$
   our instrument is uncorrelated with other (non-$\mathbf{D}_i$) determinants of $\mathbf{Y}_i$, *i.e.*, $\mathbf{Z}_i$ 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 $\text{Lottery}_i$ denote an indicator for whether $i$ won a lottery scholarship.[†]

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

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

# Instrument variables

## The IV estimator

The IV estimator for our model

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{D}_i + \varepsilon_i \tag{1}$$

with (valid) instrument $\mathbf{Z}_i$ is

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

If you have no covariates, then

$$\hat{\beta}_{\text{IV}} = \frac{\text{Cov}(\mathbf{Z}_i, \mathbf{Y}_i)}{\text{Cov}(\mathbf{Z}_i, \mathbf{D}_i)}$$

# Instrument variables

## The IV estimator

The IV estimator for our model

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{D}_i + \varepsilon_i \tag{1}$$

with (valid) instrument $\mathbf{Z}_i$ is

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

If you have additional (exogenous) covariates $\mathbf{X}_i$, then

$$\mathbf{Z} = \begin{bmatrix} \mathbf{Z}_i & \mathbf{X}_i \end{bmatrix}$$

$$\mathbf{D} = \begin{bmatrix} \mathbf{D}_i & \mathbf{X}_i \end{bmatrix}$$

## Proof: Consistency

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

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{X}_i + \varepsilon_i \tag{1}$$

$$\begin{aligned}
&\operatorname{plim}\left(\hat{\beta}_{IV}\right) \\
&\quad = \operatorname{plim}\left(\left(\mathbf{Z}'\mathbf{D}\right)^{-1}\left(\mathbf{Z}'\mathbf{Y}\right)\right) \\
&\quad = \operatorname{plim}\left(\left(\mathbf{Z}'\mathbf{D}\right)^{-1}\left(\mathbf{Z}'\mathbf{D}\beta + \mathbf{Z}'\varepsilon\right)\right) \\
&\quad = \operatorname{plim}\left(\left(\mathbf{Z}'\mathbf{D}\right)^{-1}\left(\mathbf{Z}'\mathbf{D}\right)\beta\right) + \operatorname{plim}\left(\frac{1}{N}\mathbf{Z}'\mathbf{D}\right)^{-1}\operatorname{plim}\left(\frac{1}{N}\mathbf{Z}'\varepsilon\right) \\
&\quad = \beta \quad ✔
\end{aligned}$$

# 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 $\widehat{\mathbf{D}}_i$.

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

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

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

*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 $\beta_1$.

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

$$\mathbf{P}_{\mathbf{Z}} = \mathbf{Z}\left(\mathbf{Z}'\mathbf{Z}\right)^{-1}\mathbf{Z}'$$

where $\mathbf{D}$ is a matrix of our treatment and predetermined covariates $(\mathbf{X}_i)$ and $\boldsymbol{Z}$ 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.
- *Related:* Nonlinear first stages can mess things up.
- 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. $\mathbf{D}_i = \gamma_1 \mathbf{Z}_i + \gamma_2 \mathbf{X}_i + u_i$
2. $\mathbf{Y}_i = \beta_1 \widehat{\mathbf{D}}_i + \beta_2 \mathbf{X}_i + \varepsilon_i$

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

The **reduced form** regresses the outcome $\mathbf{Y}_i$ (LHS of the second stage) on our instrument $\mathbf{Z}_i$ and covariates $\mathbf{X}_i$ (RHS of the first stage).

$$\mathbf{Y}_i = \pi_1 \mathbf{Z}_i + \pi_2 \mathbf{X}_i + u_i$$

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 $\mathbf{Cov}(\mathbf{Z}_i, \varepsilon_i) = 0$.
- Offers insights into your estimates

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

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.

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

What's the interpretation/intuition?

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

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

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

# 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.*, $\widehat{\gamma}_1 = 0.50$.[†]

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

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

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

† Imagine none of the applicants would have graduated otherwise

# Two-stage least squares

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

## Derivation

$$\widehat{\beta}_1^{\text{IV}} = \left(\mathbf{Z}'\mathbf{D}\right)^{-1}\left(\mathbf{Z}'\mathbf{Y}\right)$$

$$= \left(\widetilde{\mathbf{Z}}'\widetilde{\mathbf{D}}\right)^{-1}\left(\widetilde{\mathbf{Z}}'\mathbf{Y}\right) \quad \text{applying FWL to reduce } \mathbf{D} \text{ and } \mathbf{Z} \text{ to vectors.}$$

$$= \frac{\text{Cov}\left(\widetilde{\mathbf{Z}}_i,\, \mathbf{Y}_i\right)}{\text{Cov}\left(\widetilde{\mathbf{Z}}_i,\, \widetilde{\mathbf{D}}_i\right)} = \frac{\text{Cov}\left(\widetilde{\mathbf{Z}}_i,\, \mathbf{Y}_i\right) / \text{Var}\left(\widetilde{\mathbf{Z}}_i\right)}{\text{Cov}\left(\widetilde{\mathbf{Z}}_i,\, \widetilde{\mathbf{D}}_i\right) / \text{Var}\left(\widetilde{\mathbf{Z}}_i\right)}$$

$$= \frac{\widehat{\pi}_1}{\widehat{\gamma}_1} \quad \checkmark$$

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.[†]

We are interested in the regression model for the effect of some treatment (*e.g.*, blood-pressure medication) on medical outcome $\mathbf{Y}_i$

$$\mathbf{Y}_i = \beta_0 + \beta_1 \mathbf{D}_i + \varepsilon_i$$

$\mathbf{D}_i$ indicates whether $i$ *takes* the treatment (medication). $\varepsilon_i$ captures all other factors that affect $\mathbf{Y}_i$. Or in potential-outcomes framework:

$$\begin{aligned} \mathbf{Y}_i &= \mathbf{Y}_{1i}\mathbf{D}_i + \mathbf{Y}_{0i}(1 - \mathbf{D}_i) \\ \mathbf{Y}_{0i} &= \beta_0 + \varepsilon_i \\ \mathbf{Y}_{1i} &= \mathbf{Y}_{0i} + \beta_1 \end{aligned}$$

† 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 fights against the estimated effect.

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

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

*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 $\text{D}_i$ with intention to treat $\text{Z}_i$.

# IV: Mechanics and intuition

## The IV solution

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

1. $\mathbf{Cov}(\mathbf{Z}_i, \varepsilon_i) = \mathbf{0}$ as $\mathbf{Z}_i$ was randomly assigned (exclusion restriction).
2. $\mathbf{Cov}(\mathbf{Z}_i, \mathbf{D}_i) \neq \mathbf{0}$ 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 $\beta_1$.

## 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

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

*i.e.*, if 50% of treated individuals take the medication, $\widehat{\gamma}_1 = 0.50$.

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

$$\mathbf{Y}_i = \pi_1 \mathbf{Z}_i + v_i$$

which we know IV rescales using the first stage

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

# IV: Mechanics and intuition

## Noncompliance, continued

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

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

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

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

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

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

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

# IV: Mechanics and intuition

## Takeaways

Main points

1. IV **rescales** the causal effect of $\mathbf{Z}_i$ on $\mathbf{Y}_i$ by the causal effect of $\mathbf{Z}_i$ on $\mathbf{D}_i$.

2. 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!).

*Credit* Glen Waddell introduced me to IV via Venn.

Figure 1

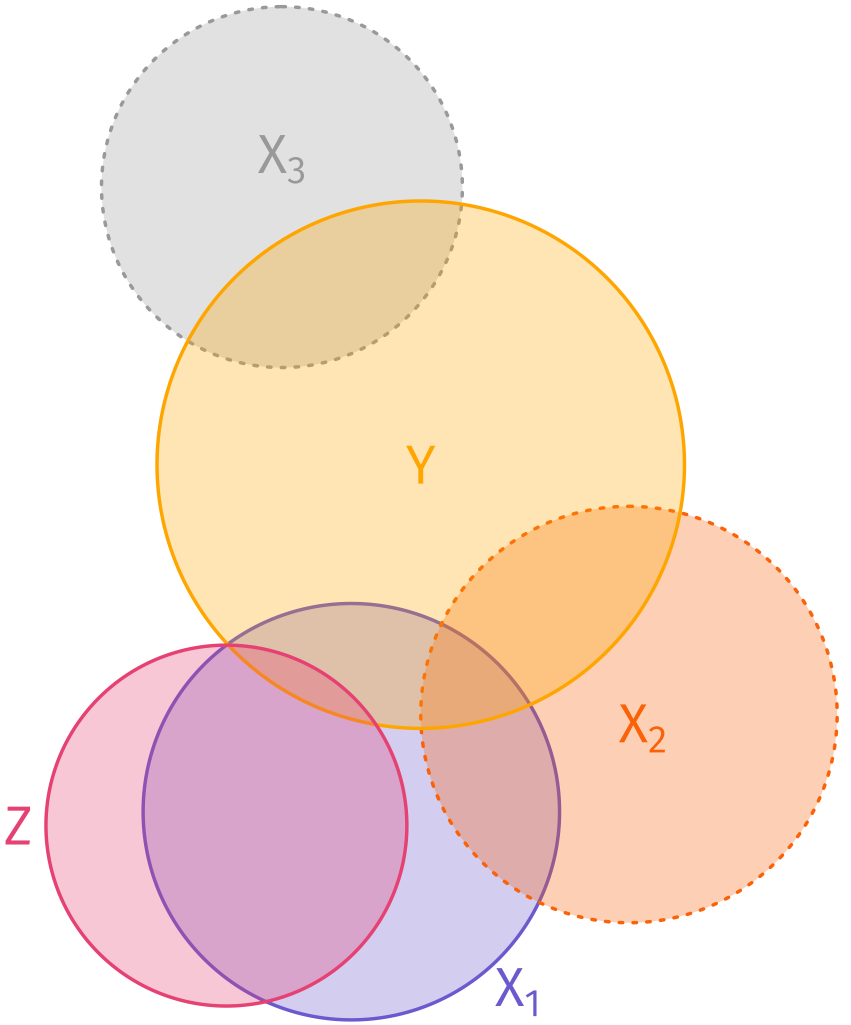


Figure 2

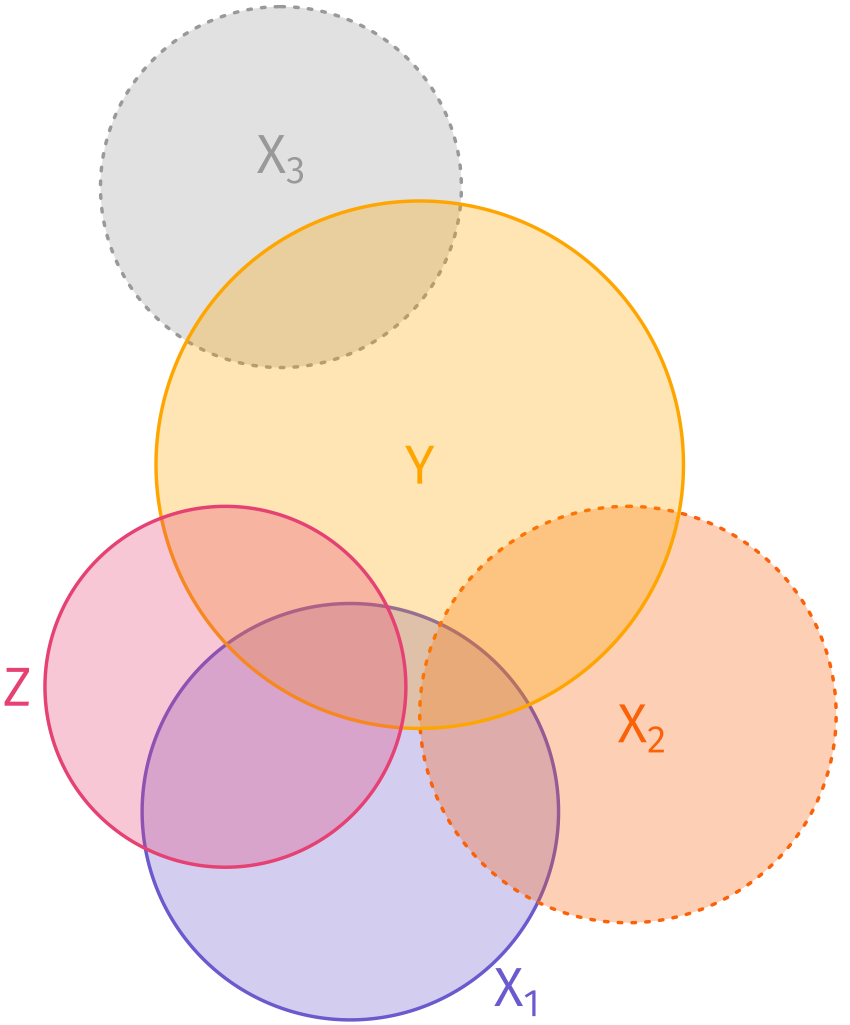


Figure 3

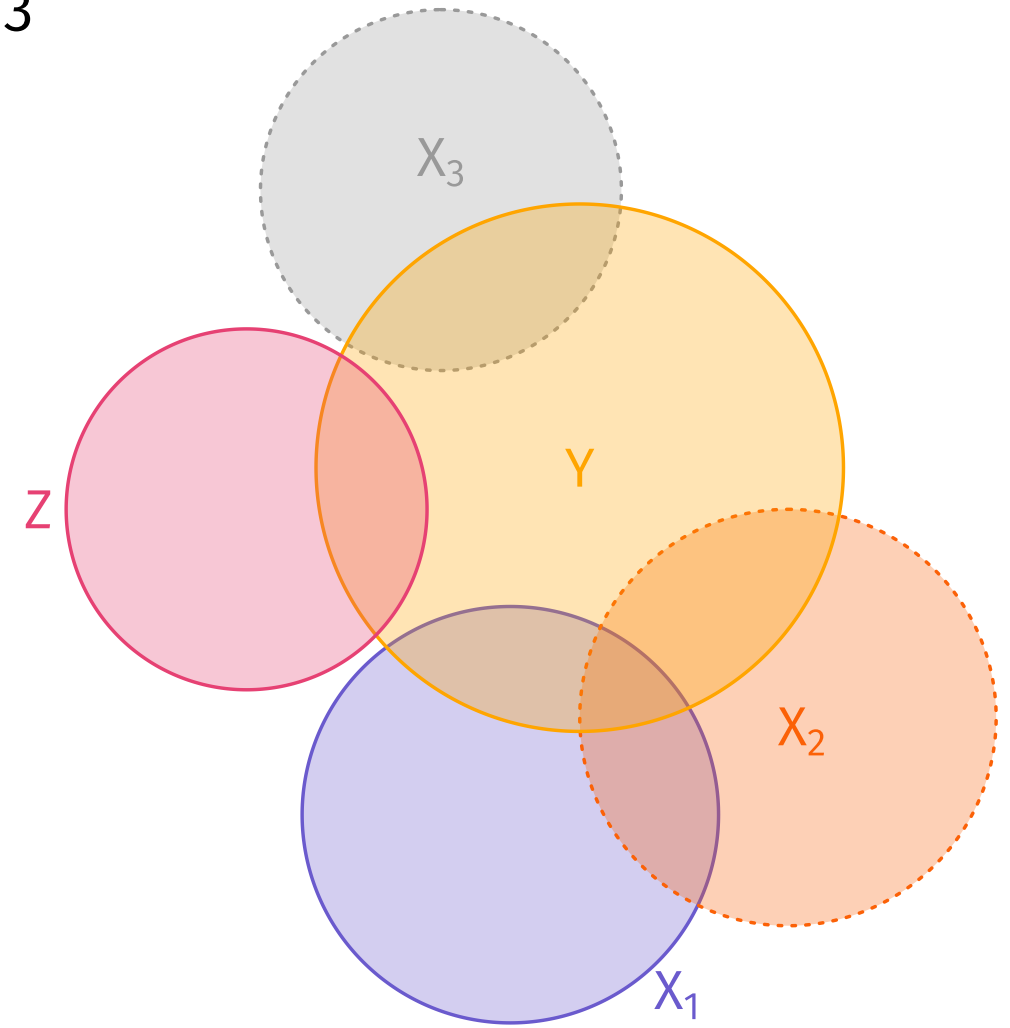


Figure 4

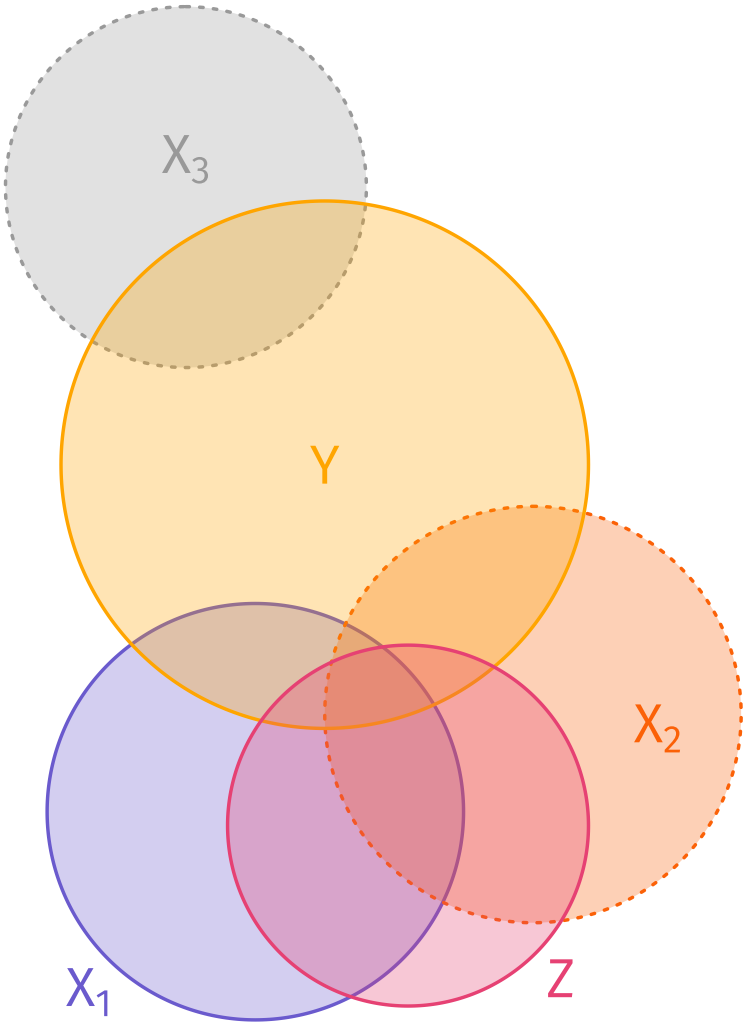
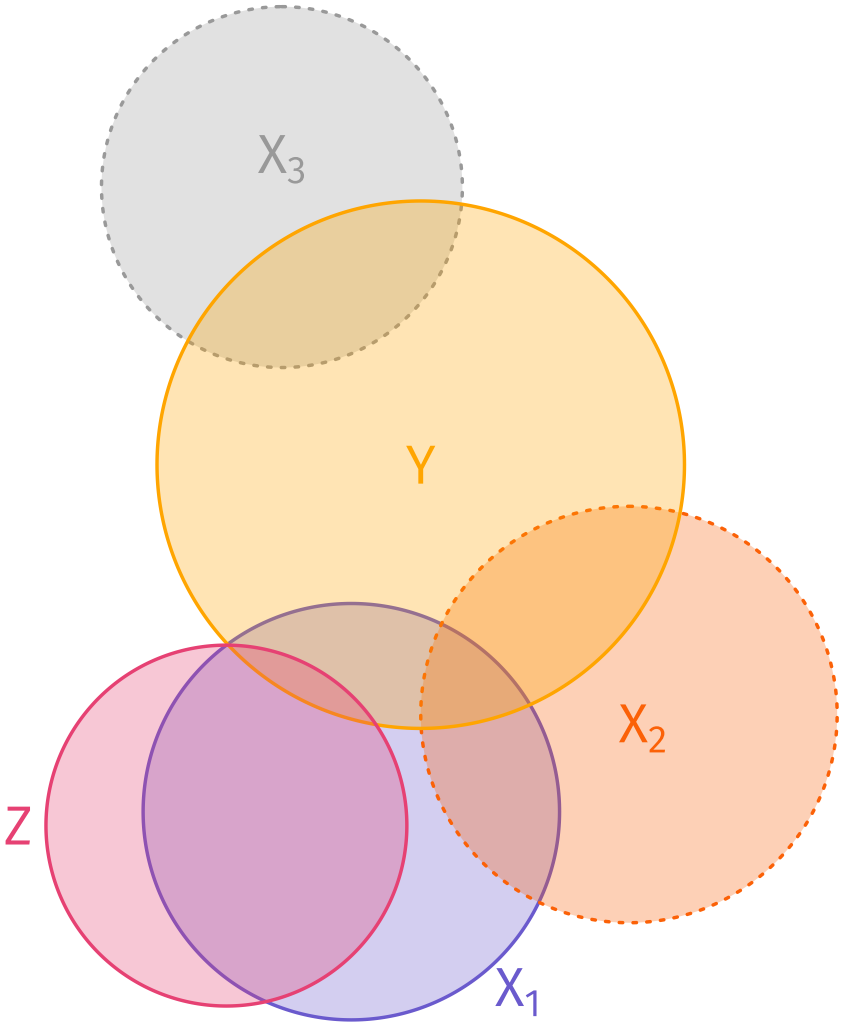


Figure 1



# 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.

2. **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.[†]

† 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 $\mathbf{D}_i$ induced by our instrument $\mathbf{Z}_i$.

In other words: IV focuses on the individuals whose $\mathbf{D}_i$ changes due to $\mathbf{Z}_i$.

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

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

| | |
|---|---|
| 1. **Compliers** $\mathbf{D}_i = 1$ iff $\mathbf{Z}_i = 1$. | Only take pills **when treated**. |
| 2. **Always-takers** $\mathbf{D}_i = 1 \; \forall \mathbf{Z}_i$. | **Always** take pills. |
| 3. **Never-takers** $\mathbf{D}_i = 0 \; \forall \mathbf{Z}_i$. | **Never** take pills. |
| 4. **Defiers** $\mathbf{D}_i = 1$ iff $\mathbf{Z}_i = 0$. | 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 $\left(\widehat{\beta}_1^{\text{IV}} = 0\right)$.

# 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.[†]

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*$_2$ Different instruments have different LATEs.

† 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:* $\mathbf{Cov}(\mathbf{Z}_i, \mathbf{D}_i) > 0$
- *Exclusion restriction:* $\mathbf{Cov}(\mathbf{Z}_i, \varepsilon_i) = 0$

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

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

# 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.[†]

† 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.[†]

† 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

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

# 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[†]

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

† 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).

2. 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[†] 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.

† *E.g.*, Aliprantis (2012)

# 2SLS and R

## `estimatr`

You can implement 2SLS/IV in many ways in R.

Today: `esitmatr` and `iv_robust()`.

Specifically, we give `iv_robust()` the relationship that we want separted from the instrument by `|` , *e.g.*,

```
# Estimate 2SLS
iv_robust(Y ~ D | Z, data = sample_df, se_type = "classical") %>%
  tidy() %>% select(1:5)
```

```
#>          term estimate std.error statistic      p.value
#> 1 (Intercept) 5.786204 2.9744230  1.945320 0.0546020456
#> 2           D 1.107801 0.3043264  3.640173 0.0004372703
```

## Now in two stages!

Of course, we can estimate 2SLS in two stages.

```
# First stage
stage1 = lm_robust(D ~ Z, data = sample_df, se_type = "classical")
# First-stage results
stage1 %>% tidy() %>% select(1:5)
```

```
#>          term  estimate std.error statistic      p.value
#> 1 (Intercept) 8.8226148 0.3169568 27.835389 2.486413e-48
#> 2           Z 0.3257347 0.1031506  3.157857 2.112927e-03
```

## Second stage

We just need to add $\widehat{\mathbf{D}}_i$ to our dataset.

```
# Add fitted (first-stage) values to data
sample_df %<>% mutate(D_hat = stage1$fitted.values)
# Second stage
stage2 = lm_robust(Y ~ D_hat, data = sample_df, se_type = "classical")
# Second-stage results
stage2 %>% tidy() %>% select(1:5)
```

```
#>          term estimate std.error statistic    p.value
#> 1 (Intercept) 5.786204 5.4132099  1.068904 0.28773854
#> 2       D_hat 1.107801 0.5538496  2.000184 0.04824759
```

## Standard errors

However, recall that our second-stage standard errors are not correct.

**Second-stage results**

| Term | Est. | S.E. | t stat. | p-Value |
|---|---|---|---|---|
| Int | 5.786 | 5.413 | 1.07 | 0.2877 |
| D hat | 1.108 | 0.554 | 2.00 | 0.0482 |

**2SLS results**

| Term | Est. | S.E. | t stat. | p-Value |
|---|---|---|---|---|
| Int | 5.786 | 2.974 | 1.95 | 0.0546 |
| D | 1.108 | 0.304 | 3.64 | 0.0004 |

## Conclusions

1. IV/2SLS focus on **isolating some "good" variation** in $\mathbf{D}_i$ via $\mathbf{Z}_i$.
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.

# Table of contents