Instrumental Variables

EC 607, Set 9

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Prologue

Schedule

Last time

Matching and propensity-score methods

- Conditional independence
- Overlap

Today

Instrumental variables (and two-stage least squares)

Upcoming

Next problem set!

Research designs

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Selection on observables and/or unobservables

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

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

for **observable** variables X_i.

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for **observable** variables X_i.

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

1. $(\mathrm{Y}_{0i}, \mathrm{Y}_{1i}) \perp \mathrm{Z}_i$

2. $\mathrm{Cov}(\mathrm{Z}_i, \mathrm{D}_i) \neq 0$

Selection on observables and/or unobservables

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

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   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 $\mathbf{Z}_i$.

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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 β_1, want $\mathbf{Cov}(\mathbf{D}_i,\, \varepsilon_i) = \mathbf{0}$.
In general, this is a heroic assumption.

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Alternative: Estimate β_1 via instrumental variables.

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Instrumental variables

Definition

For our model

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

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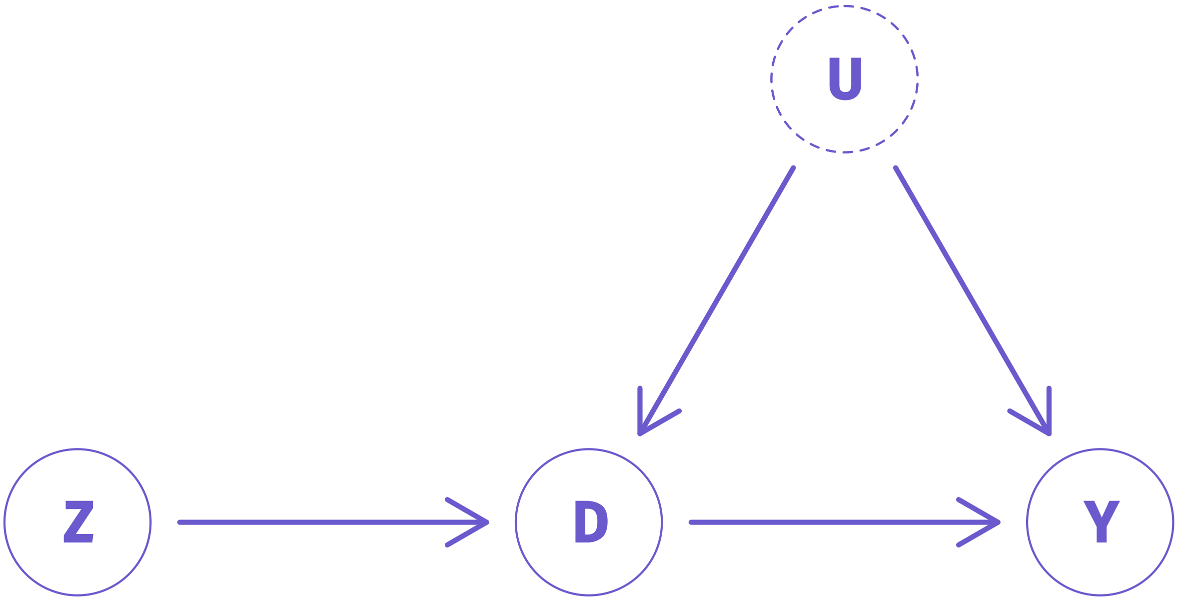
Instrumental variables

The DAG



Instrumental variables

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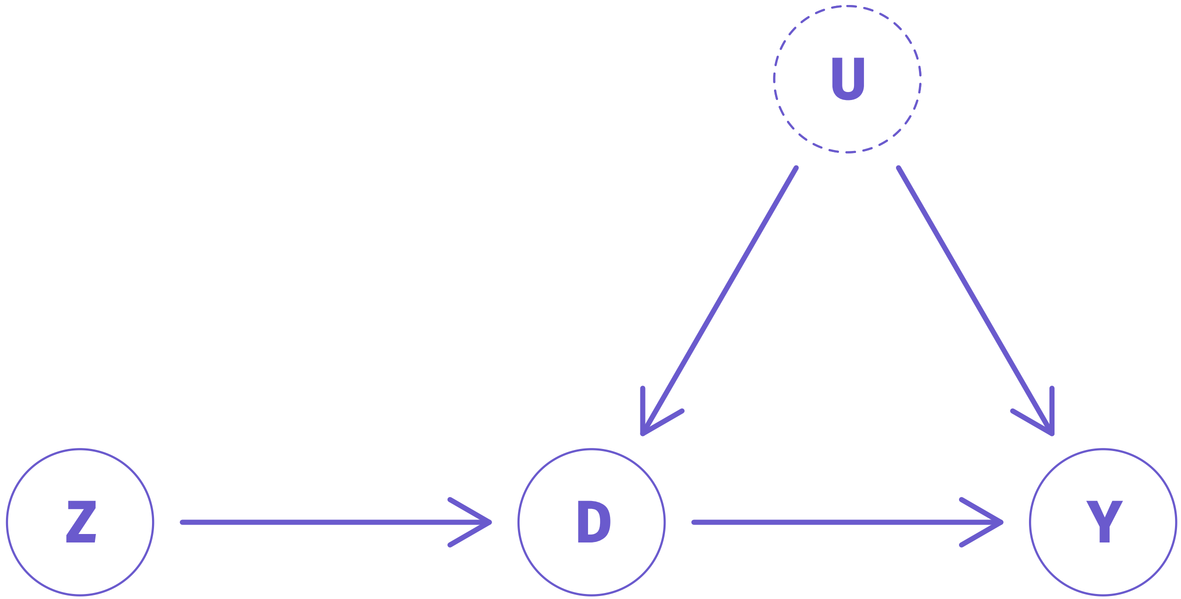

Q How does this DAG illustrate the requirements and identification of IV?

Instrumental variables

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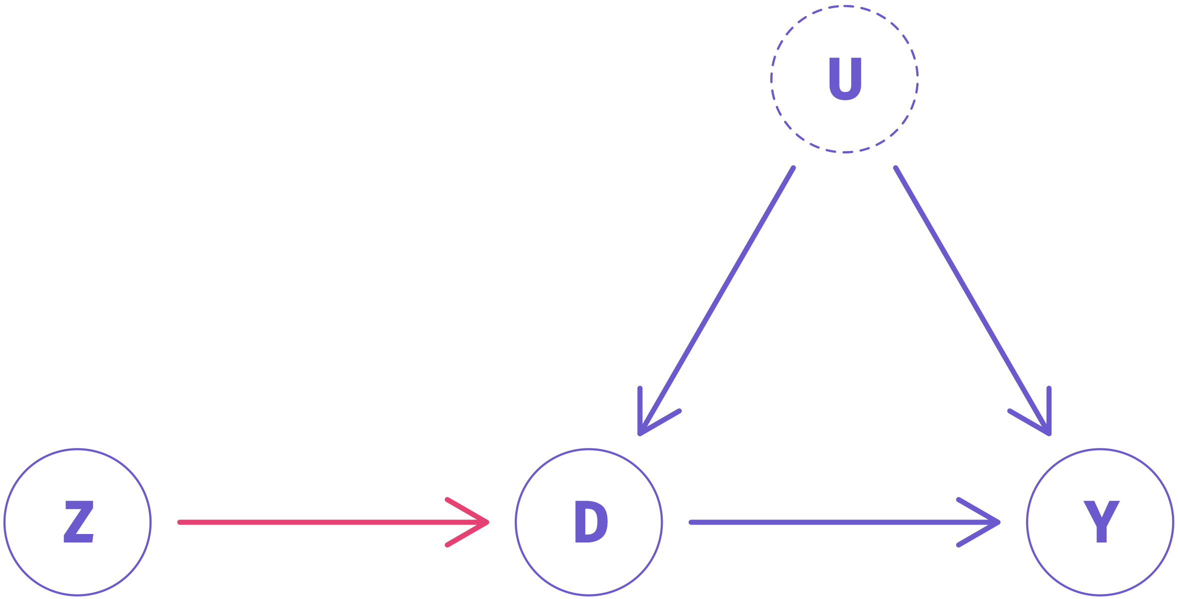

Relevance: **Z** causes an effect in **D**.

Instrumental variables

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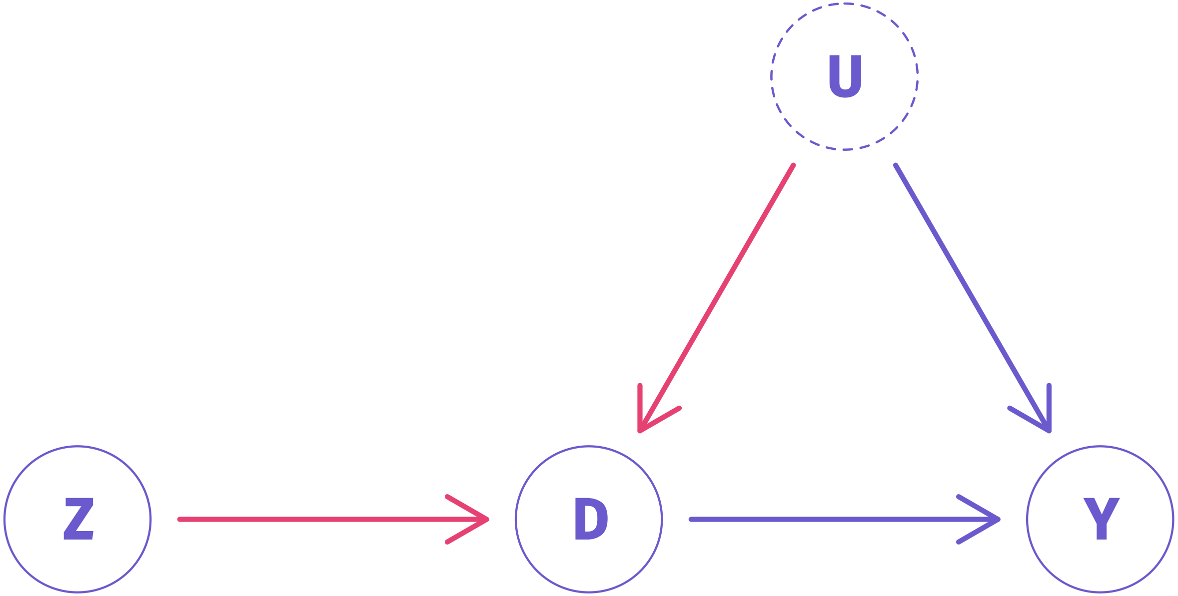

Exclusion restriction:

1. **Z** is **exogenous** (not associated with) **U** because

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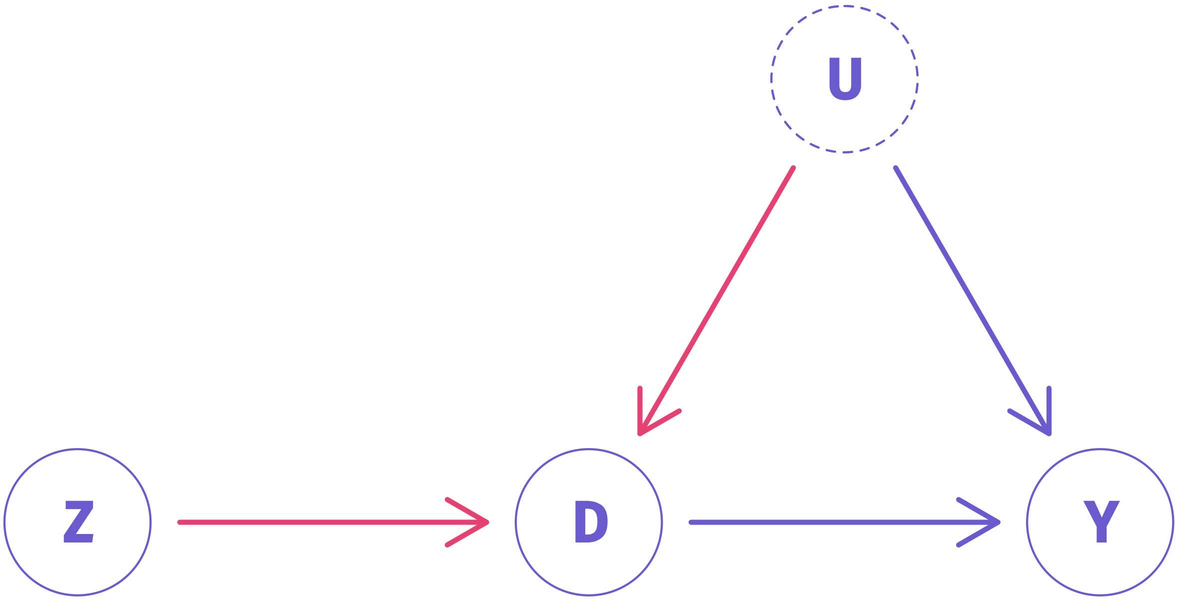

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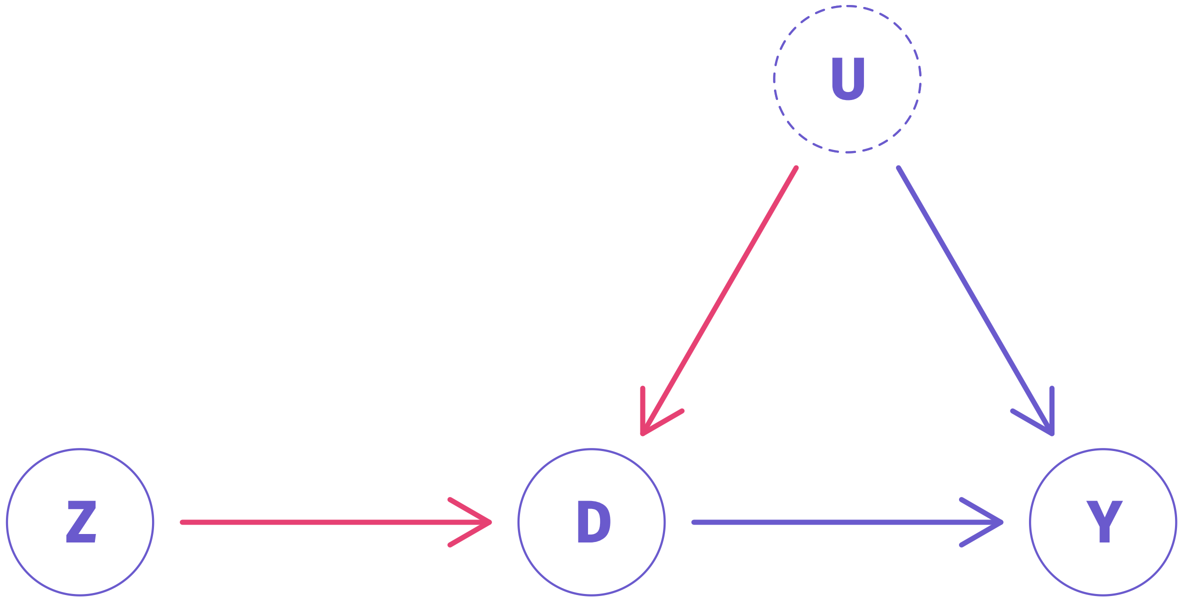

Exclusion restriction:

1. **Z** is **exogenous** (not associated with) **U** because **D** is a collider.
   I.e., **Z** → **D** ← **U** → **Y** is closed without conditioning on (unobservable) **U**.

Instrumental variables

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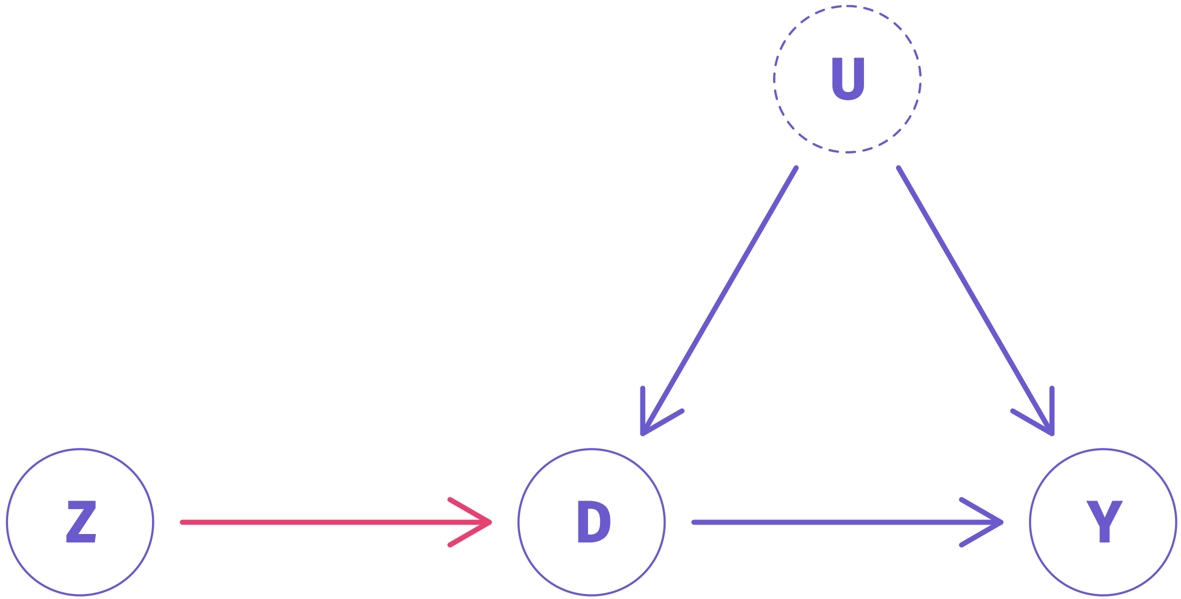

Exclusion restriction:

1. **Z** is **exogenous** (not associated with) **U** because **D** is a collider.
2. Also: **Z** does not directly cause **Y**.

Instrumental variables

Example

Back to the returns to a college degree,

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

OLS is likely biased.

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2. $\text{Cov}(\text{Lottery}_i, \varepsilon_i) = 0$ since the lottery is randomized.

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The IV estimator

The IV estimator for our model

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

with (valid) instrument Z_i is

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

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If you have no covariates, then

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

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

$$\mathbf{Z} = \left[\,\text{Z}_i \quad \mathbf{X}_i\,\right]$$

$$\mathbf{D} = \left[\,\text{D}_i \quad \mathbf{X}_i\,\right]$$

Instrumental variables

Proof: Consistency

With a valid instrument $\mathbf{Z}_i$, $\hat{\beta}_{\text{IV}}$ is a consistent estimator for β_1 in

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

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&= \text{plim}\left(\left(\text{Z}'\text{D}\right)^{-1}\left(\text{Z}'\text{D}\right)\beta\right) + \text{plim}\left(\frac{1}{N}\text{Z}'\text{D}\right)^{-1}\text{plim}\left(\frac{1}{N}\text{Z}'\varepsilon\right)
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&= \beta \quad \checkmark
\end{aligned}$$

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First stage Estimate the effect of the instrument Z_i on our endogenous variable D_i and (predetermined) covariates $\mathbf{X}_i$. Save $\widehat{\text{D}}_i$.

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

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

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

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 β_1.

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

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

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

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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

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

  when you have exactly one instrument.

The reduced form, intuition

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

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Two-stage least squares

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$\widehat{\gamma}_1$ 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.*, $\widehat{\gamma}_1 = 0.50$.[†]

† Imagine none of the applicants would have graduated otherwise

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

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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.

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Two-stage least squares

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

Setup

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

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IV: Mechanics and intuition

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IV: Mechanics and intuition

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$\mathbf{D}_i$ indicates whether i *takes* the treatment (medication). ε_i captures all other factors that affect $\mathbf{Y}_i$.

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IV: Mechanics and intuition

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

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

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IV: Mechanics and intuition

Research design

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IV: Mechanics and intuition

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

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IV: Mechanics and intuition

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$\therefore$ $\mathbf{Z}_i$ is a valid instrument for $\mathbf{D}_i$ and IV consistently estimates β_1.

Noncompliance

Noncompliant individuals do not abide by their treatment assignment.

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

IV: Mechanics and intuition

Noncompliance

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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.

Noncompliance, continued

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

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

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IV: Mechanics and intuition

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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

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Noncompliance, continued

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

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

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

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

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IV: Mechanics and intuition

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IV consistently estimates β_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 $\mathbf{D}_i$.

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.

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

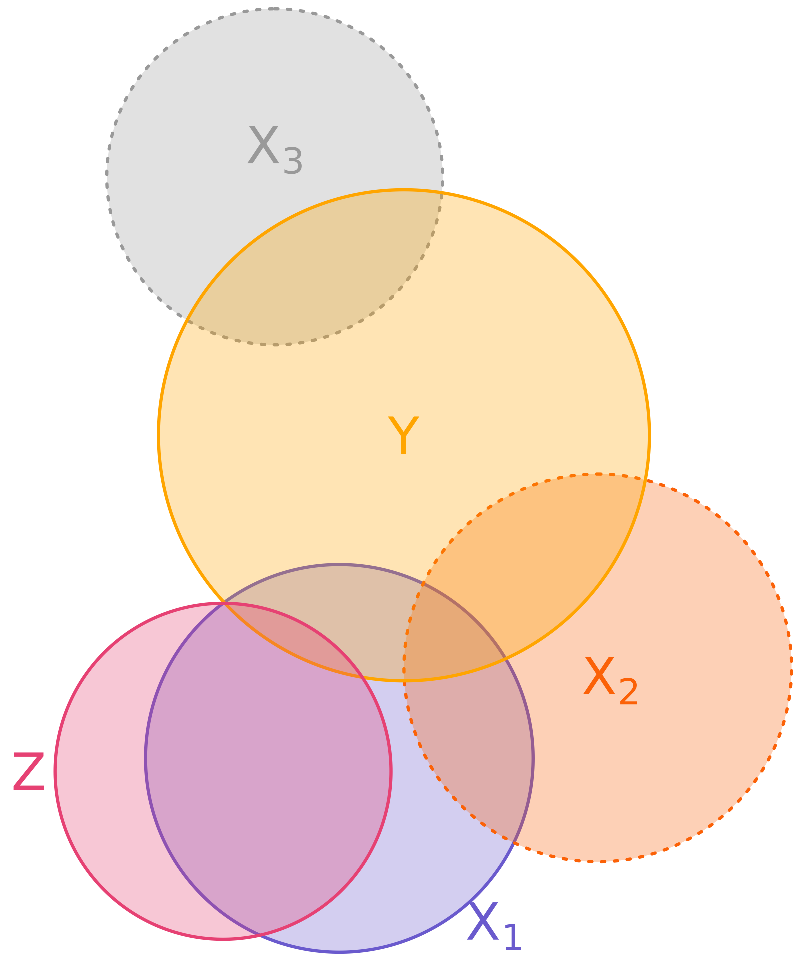


Figure 2

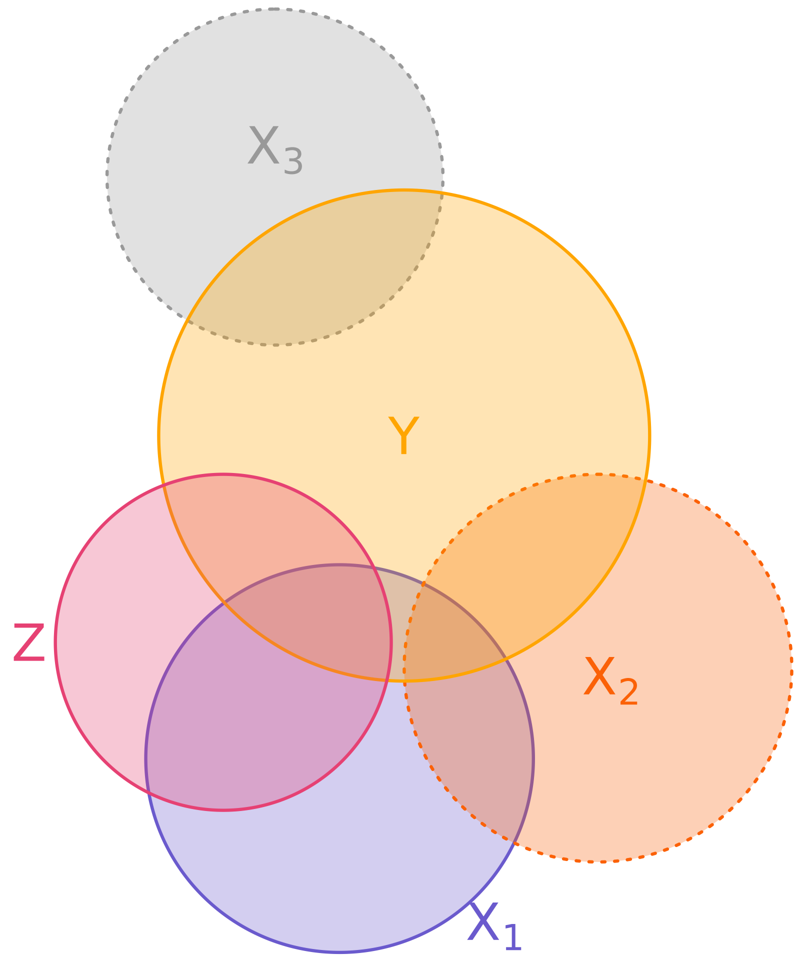


Figure 3

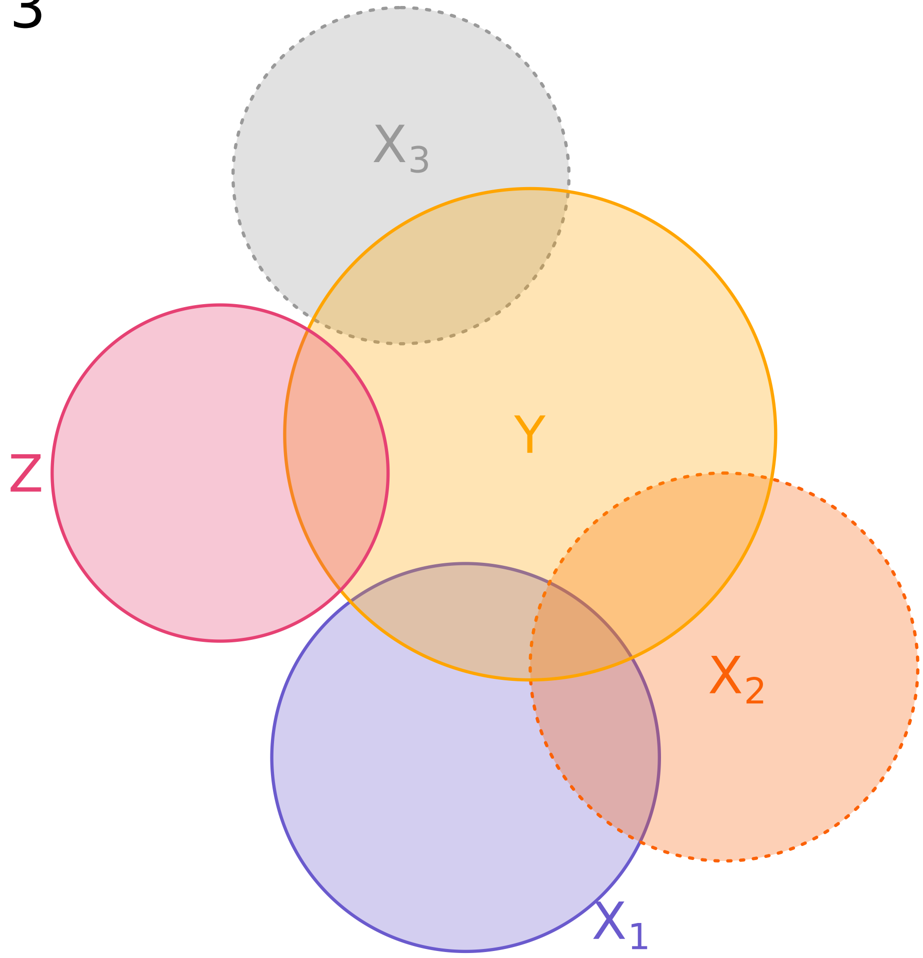
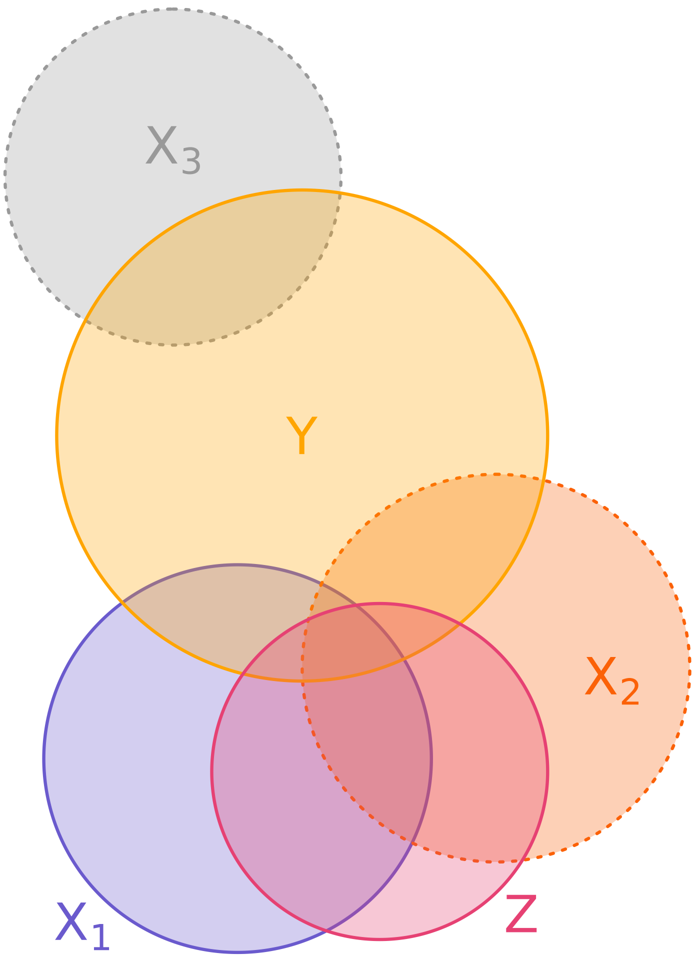


Figure 4



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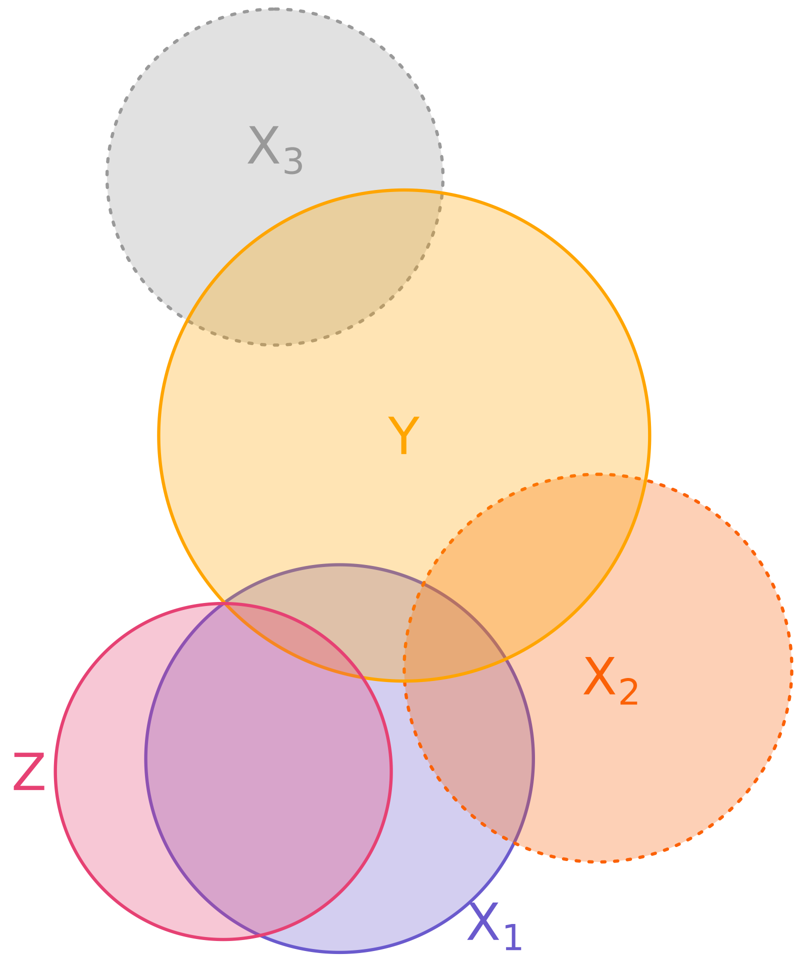

Can you draw the DAGs?

IV + heterogeneity

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A Not ATE. And not TOT. They estimate the LATE.†

† See Angrist, Imbens, and Rubin (1996).

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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**.

IV + heterogeneity

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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** $\text{D}_i = 1$ iff $\text{Z}_i = 1$.
2. **Always-takers** $\text{D}_i = 1\ \forall \text{Z}_i$.
3. **Never-takers** $\text{D}_i = 0\ \forall \text{Z}_i$.
4. **Defiers** $\text{D}_i = 1$ iff $\text{Z}_i = 0$.

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**.

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The LATE

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

The LATE: Medical-trial example

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

Suppose individuals know their response to blood-pressure medication.

IV + heterogeneity

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- $\beta_{1,j} = 0$ 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 $\left(\widehat{\beta}_1^{\text{IV}} = 0\right)$.

The LATE

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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.†

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Takeaway$_2$ Different instruments have different LATEs.

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IV + heterogeneity

Monotonicity

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

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

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

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

IV + heterogeneity

Monotonicity

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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

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Motivation

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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 Z_{1i} and Z_{2i}, or first stage becomes

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

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

while our second stage is still

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

Multiple instruments

Example: Quarter of birth

Back to our quest to estimate the returns to education.

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Multiple instruments

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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[†]

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

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

Multiple instruments

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Multiple instruments

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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.

Multiple instruments

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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:

Multiple instruments

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1. Our estimator is a ratio of the reduced form and the first stage, so a weak first stage essentially blows up the reduced-form estimates (amplifying reduced-form noise/bias).

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2. Many weak instruments lead to a finite-sample issue in which 2SLS is biased toward OLS—our first stage is essentially overfitting.

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

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What about our other requirements for a valid instrument?

Multiple instruments

Example: Quarter of birth

Q2 Is quarter of birth uncorrelated with ε_i (*excludable*)?

Multiple instruments

Example: Quarter of birth

Q2 Is quarter of birth uncorrelated with ε_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 ε_i.

Multiple instruments

Example: Quarter of birth

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Multiple instruments

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Consider December births.

- Original idea: December birthdates will start school at age 5.7, inducing more years of education before 16.

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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

`feols`

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

Today: `fixest` and `feols()`

There are others, *e.g.*, `estimatr::iv_robust()` and `lfe::felm()`

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Specifically, `feols()` wants the exogenous "part" of the equation, a `|`, and the 'link' between the endogenous regressors and the instruments, *e.g.*,

```
# Estimate 2SLS
feols(Y ~ 1 | D ~ Z, data = sample_df) %>% tidy()
```

```
#> # A tibble: 2 × 5
#>   term        estimate std.error statistic  p.value
#>   <chr>          <dbl>     <dbl>     <dbl>    <dbl>
#> 1 (Intercept)     5.79     2.97       1.95 0.0546
#> 2 fit_D           1.11     0.304      3.64 0.000437
```

Now in two stages!

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

```
# First stage
stage1 = feols(D ~ Z, data = sample_df)
# First-stage results
stage1 %>% tidy()
```

```
#> # A tibble: 2 × 5
#>   term        estimate std.error statistic  p.value
#>   <chr>          <dbl>     <dbl>     <dbl>    <dbl>
#> 1 (Intercept)    8.82      0.317     27.8  2.49e-48
#> 2 Z              0.326     0.103      3.16 2.11e- 3
```

2SLS and R

Second stage

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

```r
# Add fitted (first-stage) values to data
sample_df %<>% mutate(D_hat = stage1$fitted.values)
# Second stage
stage2 = feols(Y ~ D_hat, data = sample_df)
# Second-stage results
stage2 %>% tidy()
```

```
#> # A tibble: 2 × 5
#>   term        estimate std.error statistic p.value
#>   <chr>          <dbl>     <dbl>     <dbl>   <dbl>
#> 1 (Intercept)     5.79     5.41       1.07  0.288
#> 2 D_hat           1.11     0.554      2.00  0.0482
```

2SLS and R

Standard errors

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Term	Est.	S.E.	t stat.	p-Value
Int	5.786	5.413	1.07	0.2877
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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