

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

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 .

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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

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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 Y_{0i} and Y_{1i}).

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Research designs

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Which set of research designs is more palatable?

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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)

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

To guarantee consistent OLS estimates for β_1 , want $\text{Cov}(\mathbf{D}_i, \varepsilon_i) = 0$.
In general, this is a heroic assumption.

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Alternative: Estimate β_1 via instrumental variables.

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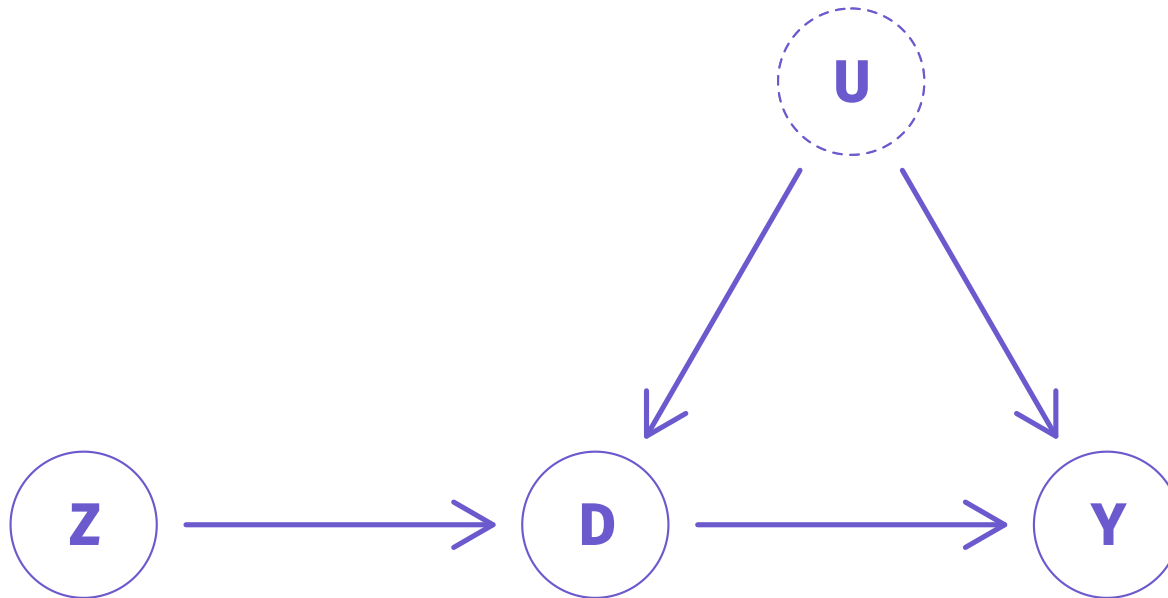
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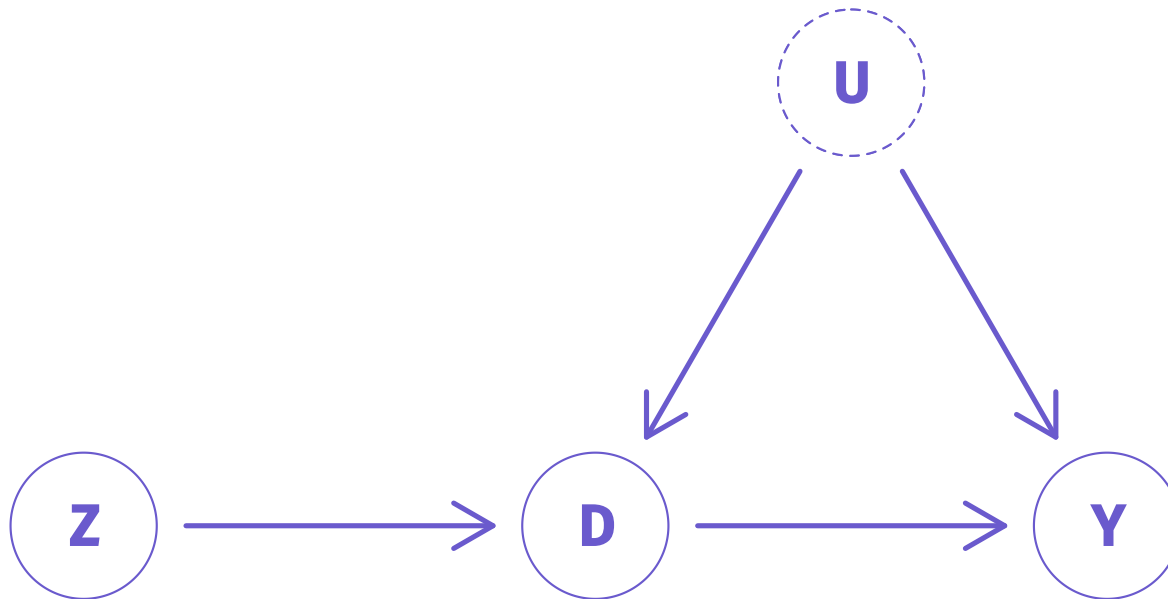
Instrumental variables

The DAG



Instrumental variables

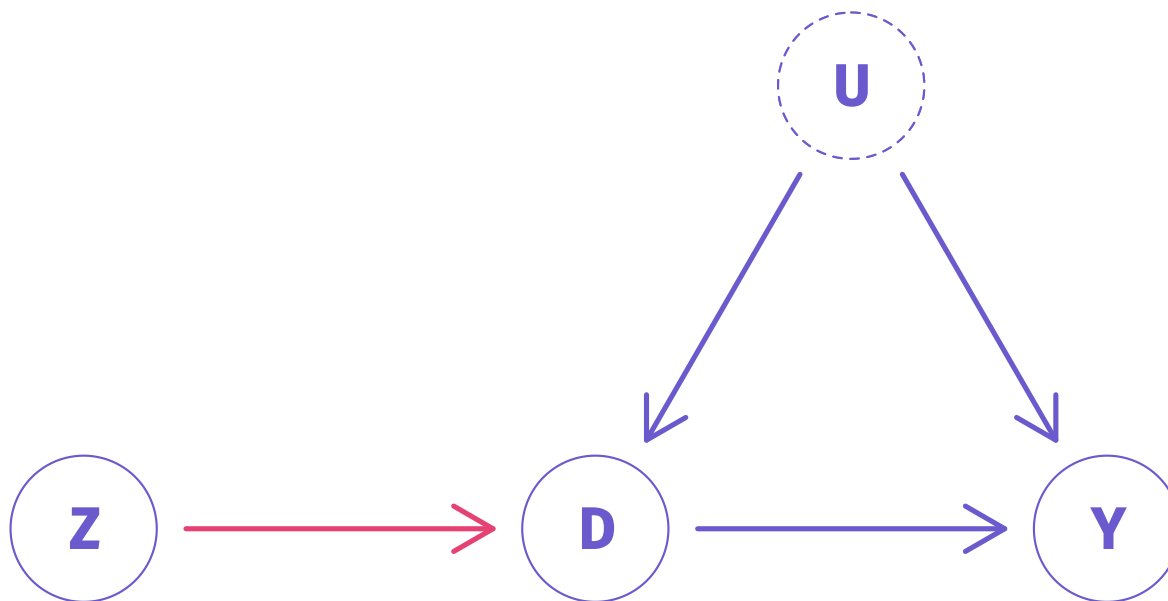
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Q How does this DAG illustrate the requirements and identification of IV?

Instrumental variables

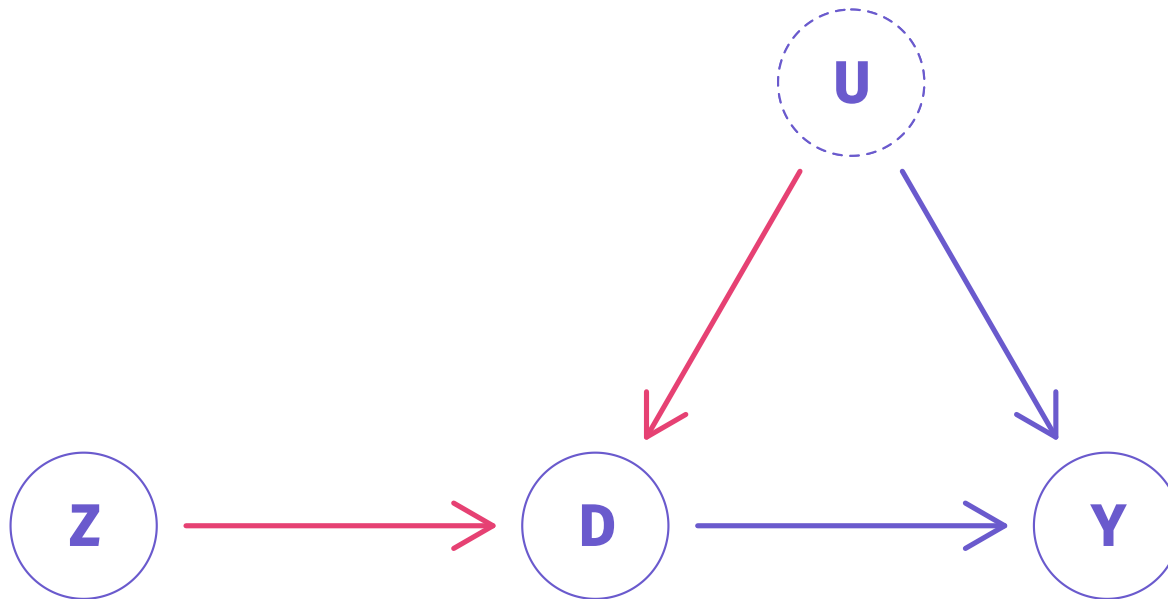
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Relevance: **Z** causes an effect in **D**.

Instrumental variables

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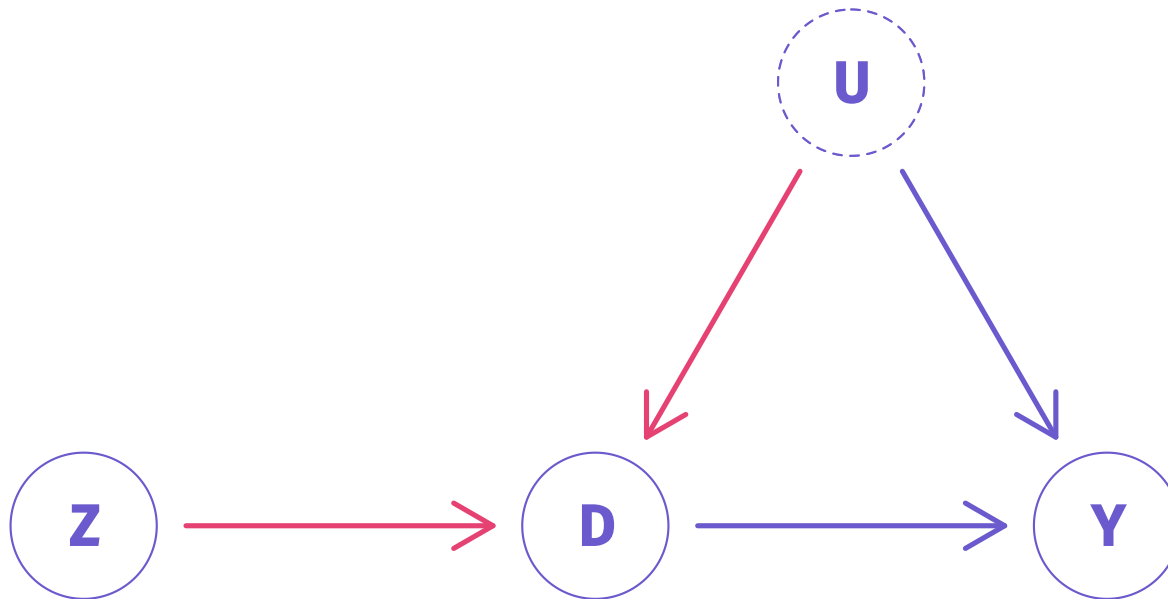


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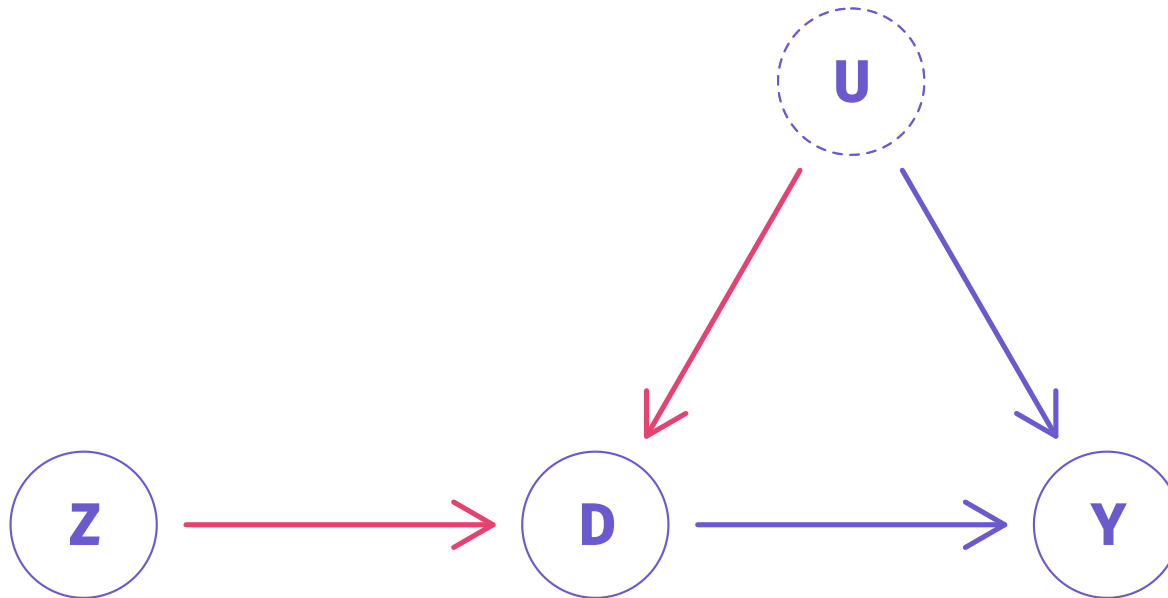


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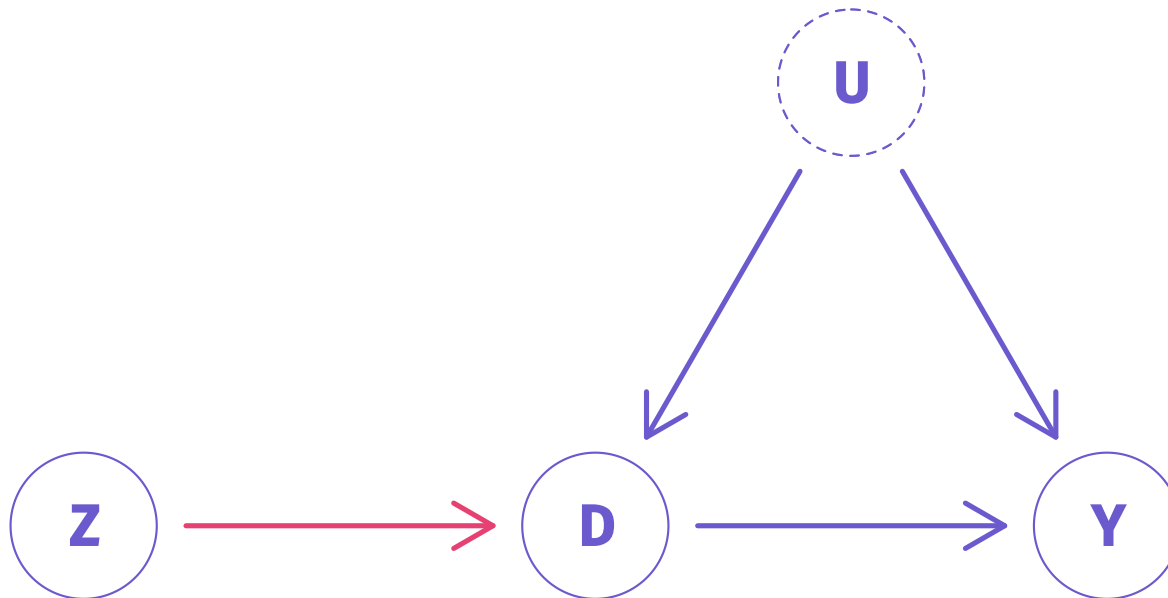


Exclusion restriction:

1. **Z** is **exogenous** (not associated with) **U** because **D** is a collider.
I.e., $\mathbf{Z} \rightarrow \mathbf{D} \leftarrow \mathbf{U} \rightarrow \mathbf{Y}$ is closed without conditioning on (unobservable) **U**.

Instrumental variables

The DAG



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,

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

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Let **Lottery**_{*i*} denote an indicator for whether *i* won a lottery scholarship.[†]

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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

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

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If you have no covariates, then

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

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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 β_1 in

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

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

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Two-stage least squares

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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 $\hat{\mathbf{D}}_i$.

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Second stage Estimate the 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 β_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.
- *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 Y_i (LHS of the second stage) on our instrument Z_i and covariates 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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$$\hat{\beta}_1^{2SLS} = \frac{\hat{\pi}_1}{\hat{\gamma}_1}$$

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.

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

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

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

Two-stage least squares

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

[†] Imagine none of the applicants would have graduated otherwise

Two-stage least squares

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

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

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

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

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Two-stage least squares

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Two-stage least squares

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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.[†]

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

D_i indicates whether i takes the treatment (medication). ε_i captures all other factors that affect Y_i .

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

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IV: Mechanics and intuition

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

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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 .

IV: Mechanics and intuition

The IV solution

First question: Is Z_i a valid instrument for D_i ?

IV: Mechanics and intuition

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IV: Mechanics and intuition

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1. $\text{Cov}(Z_i, \varepsilon_i) = 0$ as Z_i was randomly assigned (exclusion restriction).
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IV: Mechanics and intuition

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1. $\text{Cov}(\mathbf{Z}_i, \varepsilon_i) = 0$ as \mathbf{Z}_i was randomly assigned (exclusion restriction).
2. $\text{Cov}(\mathbf{Z}_i, \mathbf{D}_i) \neq 0$ 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 β_1 .

IV: Mechanics and intuition

Noncompliance

Noncompliant individuals do not abide by their treatment assignment.

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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.

IV: Mechanics and intuition

Noncompliance, continued

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

$$D_i = \gamma_1 Z_i + u_i$$

IV: Mechanics and intuition

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

IV: Mechanics and intuition

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IV: Mechanics and intuition

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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

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

IV: Mechanics and intuition

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

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

IV: Mechanics and intuition

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

IV: Mechanics and intuition

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

IV consistently estimates β_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 .

IV: Mechanics and intuition

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2. IV **does not** compare treated compliers to untreated compliers.

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 .
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

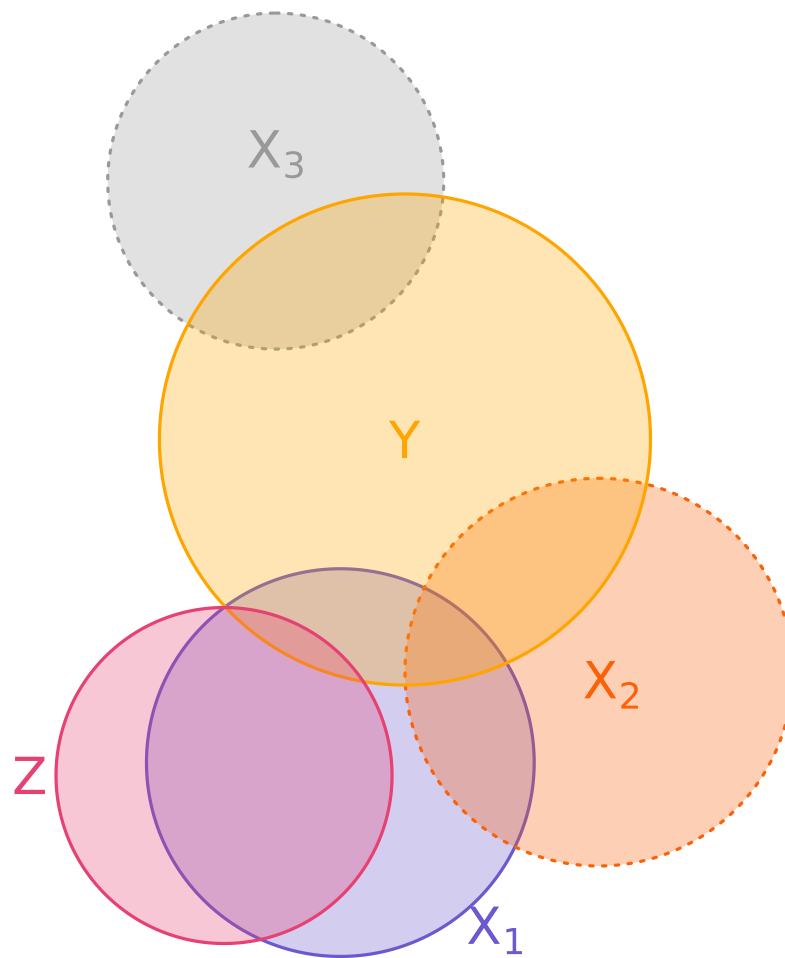


Figure 2

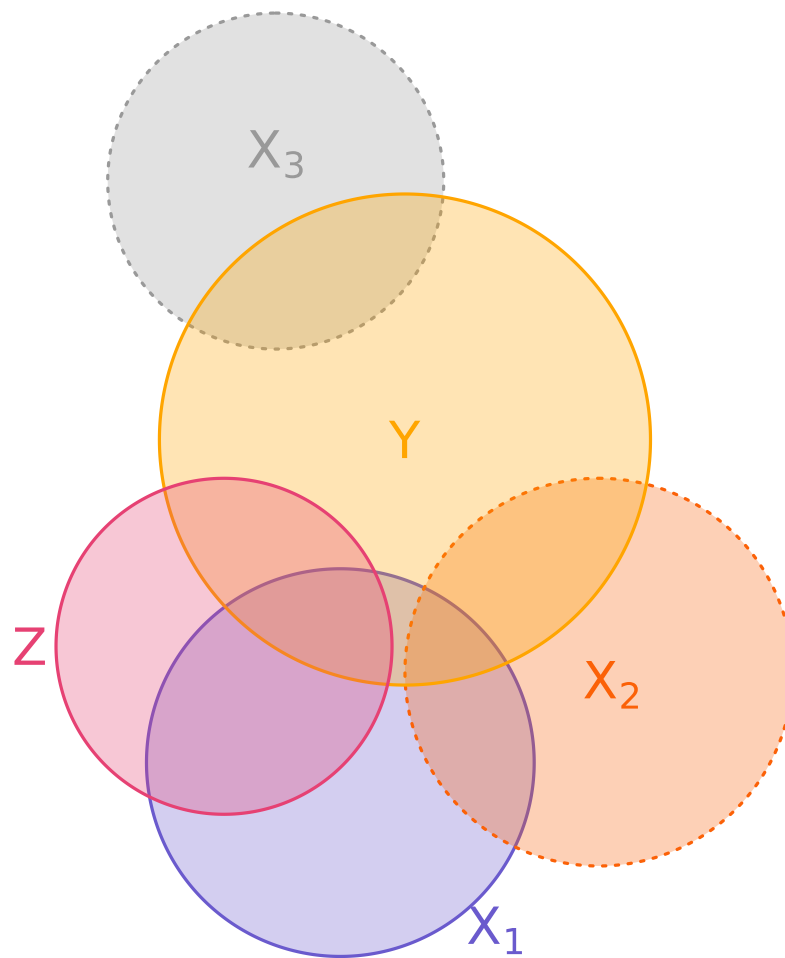


Figure 3

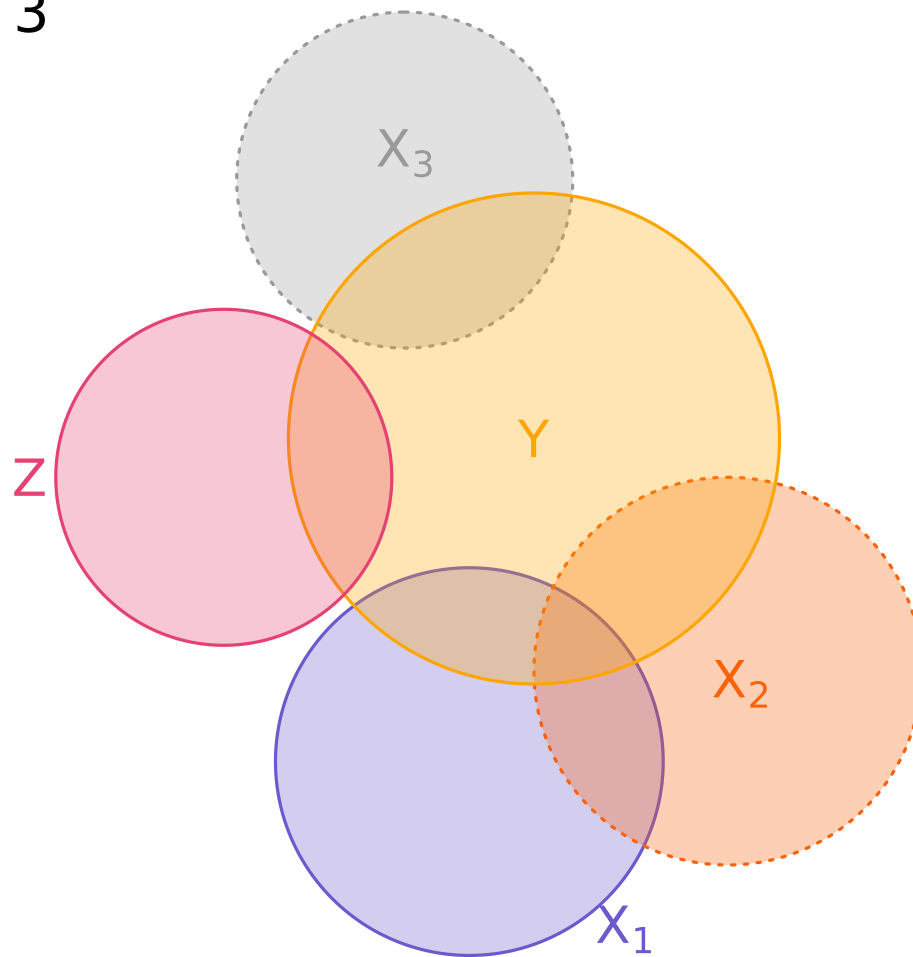


Figure 4

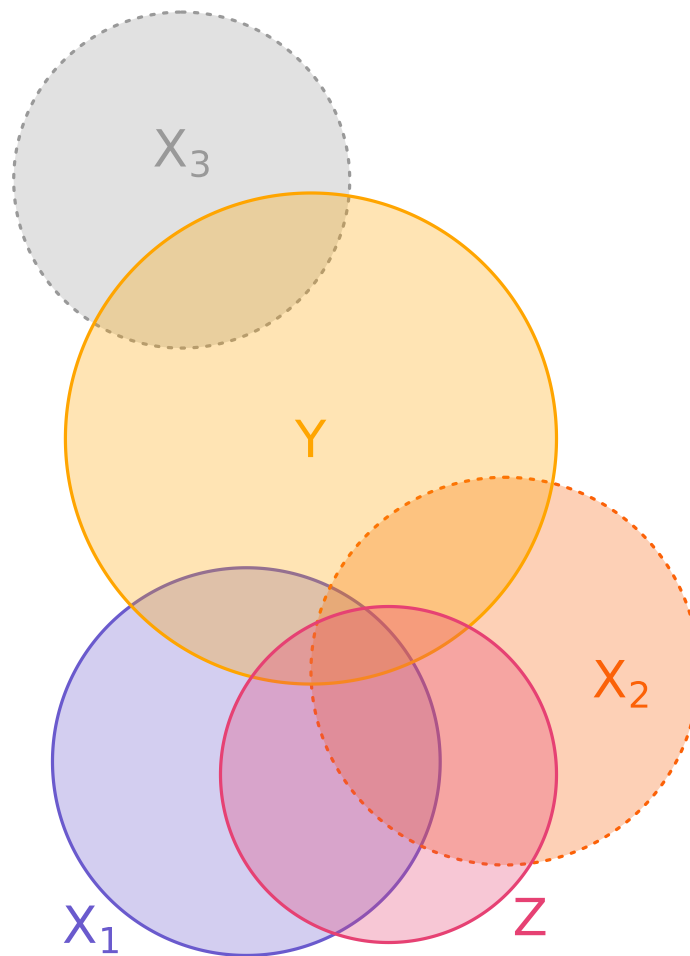
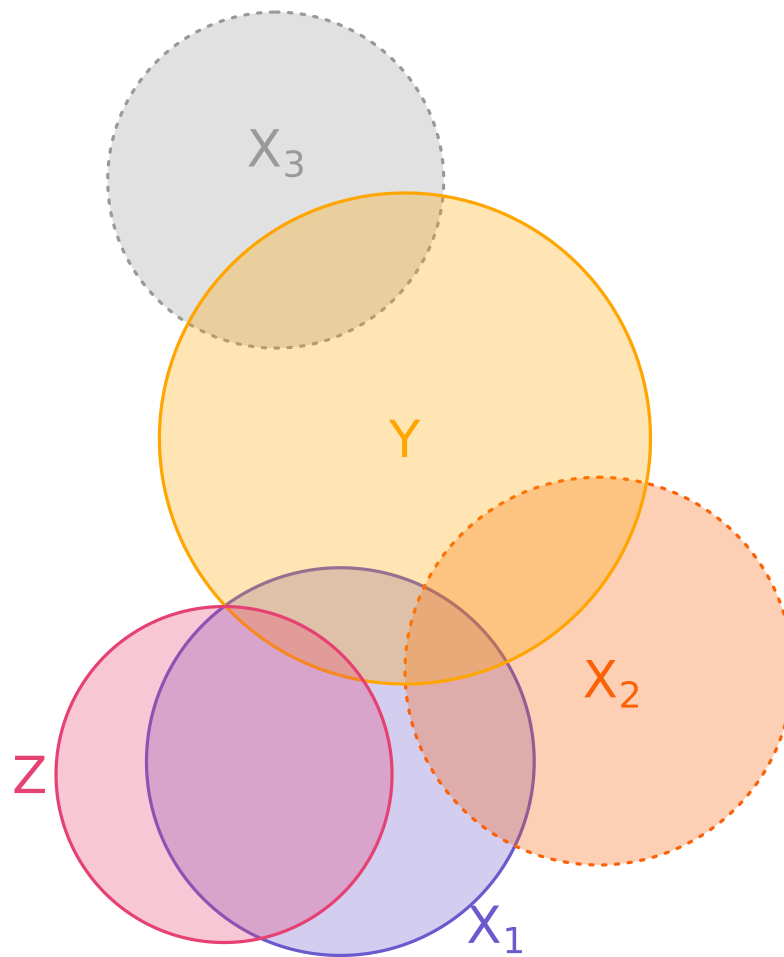


Figure 1



Can you draw the DAGs?

IV + heterogeneity

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IV + heterogeneity

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IV + heterogeneity

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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

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

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IV + heterogeneity

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

1. **Compliers** $D_i = 1$ iff $Z_i = 1$.
2. **Always-takers** $D_i = 1 \forall Z_i$.
3. **Never-takers** $D_i = 0 \forall Z_i$.
4. **Defiers** $D_i = 1$ iff $Z_i = 0$.

IV + heterogeneity

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Only take pills **when treated**.

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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*.

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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.

IV + heterogeneity

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

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

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 $\left(\hat{\beta}_1^{\text{IV}} = 0\right)$.

IV + heterogeneity

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 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.

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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.

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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.

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In this case, the IV estimand is

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

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

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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

Multiple instruments

Motivation

Q Why include multiple instruments?

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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

$$D_i = \gamma_0 + \gamma_1 Z_{1i} + \gamma_2 Z_{2i} + \gamma_3 X_i + u_i$$

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.

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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?

Multiple instruments

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Multiple instruments

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Q Is quarter of birth a valid instrument?

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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

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:

Multiple instruments

Example: Quarter of birth

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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 essentially blows up the reduced-form estimates (amplifying reduced-form noise/bias).

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).
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:

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).
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 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

Q3 Is the effect monotone?

Multiple instruments

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A3 Some[†] argue that monotonicity may be violated in this setting.

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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
```


2SLS and R

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 \hat{D}_i to our dataset.

```
# 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

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

2SLS and R

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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 and R

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

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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