Applied Microeconometrics I

Lecture 6: Instrumental variables

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Lecture Slides

What did we do last time?

- Threats to validity:
  - Omitted variable bias, OVB (lecture 4)
  - Bad controls (lecture 4)
  - Measurement error (ME) in dependent variable
  - ME in the independent variable
  - ME in the controls

What did we do last time?

1. **ME in the *dependent* variable**: not an issue, it only affects precision of estimates (not consistency)

2. **ME in the *independent* variable**:
   - "Classical measurement error" setting
   - Downward/attenuation bias

3. **ME in the *control variables***:
   - Idea behind CIA is that we can control for selection
   - The sensitivity of estimated parameter of interest to the inclusion of additional controls is an indication of OVB
   - However, the use of proxies introduces ME in the control variables (and downward bias).
   - This can lead to wrongly conclude that OVB is not an issue
   - A valid test for selection bias is to regress the proxy on the independent variable of interest (since 1. is not an issue)

Today: Instrumental Variables

- **Motivation**: when/why to use IV?
- **Instrument validity**: which assumptions are needed?
- **Interpretation**
  - potential outcomes framework
  - RCT with imperfect/partial compliance
  - LATE and its interpretation
- **Examples**:
  - Angrist et al. (2012): Who benefits from charter schools?
    RCT with partial compliance
  - Angrist (2006): Instruments and criminology
    LATE and average treatment effects

Motivation

- Arriving at causal estimates by simply controlling for observables (CIA) is challenging if not impossible
- Our data are unlikely to be rich enough to allow for credible *ceteris paribus* claims
- *Instrumental variables* are an often used strategy to arrive at causal inference when controlling for observables is not enough
- Idea: Look for variables (instruments) that generate partial or incomplete random assignment to our treatment of interest

Motivation

- Suppose we are interested in the effect of D (college education) on Y (earnings)
- People select into the treatment status via unobservable characteristics U that also affect Y

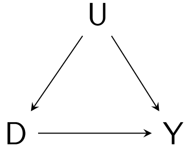
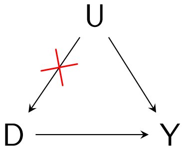


FIGURE 1 – Non-random treatment

Motivation

- If we could randomize D participation, we would break the $U \to D$ link and be able to estimate $D \to Y$



Notes: A red cross denotes a non-existing causal link.

FIGURE 2 – Randomized treatment

- But in most cases:
  - D participation cannot be assigned randomly via a RCT
  - We are unable to control for all the factors that are correlated with D and have an effect on Y

Motivation

- Instrument Z is a variable that **incompletely** plays the part of the RCT: Z assigns treatment randomly to *some units*
- Idea: exploit "causal chain reaction" $Z \to D \to Y$

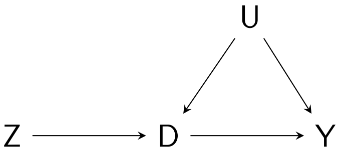


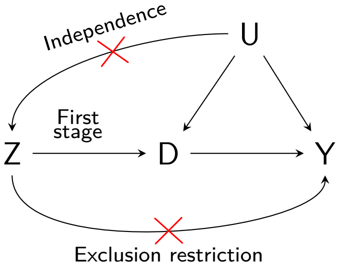
FIGURE 3 – Instrumental variables

- What assumptions do we need to make?

Instrument validity

A valid Z must fulfill:

i. *First stage*: Z has a causal effect on D
ii. *Independence/exogeneity*: Z is as good as randomly assigned
iii. *Exclusion restriction*: Z affects Y only via D (no other channels)



Notes: A red cross denotes a non-existing causal link. Z is assigned as good as randomly and has an effect on D without having a direct effect on Y.

FIGURE 4 – Instrumental variables

Instrument validity

Examples

- Think at the *first stage*, *independence*, *exclusion restriction* assumptions in Table 1. Are these valid instruments?

Candidate Z	D	Y	Validity
Draft lottery	Military service	Income	Yes
Twin births	Family size	Income	Probably
Parental education	Child's education	Child's income	No
Judge' leniency	Incarceration	Recidivism	Probably

TABLE 1 – Candidate Instruments' validity

- Best instruments are like RCT's that (incompletely) allocate the variable of interest D across units

LATE

Angrist et al. (1996)

- Consider a case where $Z_i = \{0, 1\}$ and $D_i = \{0, 1\}$
  - Z_i denotes randomized treatment assignment
  - D_i is the treatment status (endogenous)

- **First stage**: Z assigns some individuals to treatment D

$$\phi = E[D_i|Z_i = 1] - E[D_i|Z_i = 0]$$

- **Reduced form**: Causal effect of Z on Y:

$$\rho = E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]$$

- With the FS and RF we can calculate the **Local Average Treatment Effect (LATE)**

$$\lambda = \frac{\rho}{\phi} = \frac{E[Y_i|Z_i=1]-E[Y_i|Z_i=0]}{E[D_i|Z_i=1]-E[D_i|Z_i=0]}$$

- LATE is the causal effect that instrumental variables identify

LATE interpretation

- Think about IV as an RCT with **incomplete compliance**
- Randomly drawing $Z_i = 1$ assigns unit i to treatment $D_i = 1$. However, we cannot fully enforce that:
  - That all those with $Z_i = 1$ actually get the treatment $D_i = 1$
  - That none of those with $Z_i = 0$ gets the treatment $D_i = 1$
- Counterfactual/potential treatment statuses:
  - $D_{zi} \equiv D_i(Z_i = z)$, with $Z_i = \{0, 1\}$, $D_{zi} = \{0, 1\}$
  - $D_i = D_{0i} + (D_{1i} - D_{0i})Z_i$
  - what we observe in the data is D_i and Z_i
  - for a given i, we observe either D_{1i} or D_{0i} (never both)
- Under imperfect compliance, we cannot rule out the existence of **never-takers** and **always-takers:**

$$D_{1i} = D_{0i} = 0 \text{ for never-takers,}$$

$$D_{1i} = D_{0i} = 1 \text{ for always-takers}$$

LATE interpretation

- **Setting**:
  - People are free to choose whether to take D_i or not
  - Z_i randomly assigns people to be treated or not (treatment *eligibility/offer*)
  - Our interest is in $D \to Y$, but cannot simply regress Y_i on D_i: the actual treatment participation/take-up is endogenous

- **Potential treatment status**, conditional on Z_i:

	Not assigned to treatment: $Z_i=0$	Assigned to treatment: $Z_i=1$
Does not take the treatment: $D_i=0$	$D_{0i}=0$	$D_{1i}=0$
Does take the treatment: $D_i=1$	$D_{0i}=1$	$D_{1i}=1$

LATE interpretation

- Suppose we can rule out the existence of those who do not take the treatment when assigned and take the treatment when not assigned:

$$D_{1i} = 0 \text{ and } D_{0i} = 1$$

- These people are called **defiers** and the assumption that rules them out is referred to as **monotonicity**
  - *All* i's that are affected by Z are either more likely or less likely to take D (Z moves everyone in the same direction).

- Those who only take the treatment when assigned are called **compliers**:

$$D_{1i} = 1 \text{ and } D_{0i} = 0$$

LATE interpretation

- *Heterogeneous treatment effects:* Treatment effect may vary across **compliers, always-takers, never-takers, defiers**
- Two *assignments to treatment*, and two *potential treatment statuses*:
  - $Z_i = \{0, 1\}$
  - $\{D_{0i}, D_{1i}\}$, with $D_{zi} \equiv D_i(Z_i = z)$
- Four *treatment-assignment* combinations:
  - If $Z_i = 0$: $D_{0i} = \{0, 1\}$
  - If $Z_i = 1$: $D_{1i} = \{0, 1\}$
- Four *potential outcome-treatment* combinations, $Y(d, z) \equiv Y(D_{zi} = d, Z_i = z)$:
  - If $Z_i = 0$: $Y_i(0, 0), Y_i(1, 0)$
  - If $Z_i = 1$: $Y_i(0, 1), Y_i(1, 1)$

LATE interpretation

The four groups

- Suppose that Z fulfills *first stage*, *independence*, and *exclusion restriction*
- We have **four potential outcome-treatment** combinations:

		Not assigned to treatment: Z_i=0	
		Does not take the treatment: D_i=0	Does take the treatment: D_i=1
Assigned to treatment: Z_i=1	Does not take the treatment: D_i=0	*Never-takers* $Y_i(0,1)-Y_i(0,0)=0$	*Defiers* $Y_i(0,1)-Y_i(1,0)=$ $-[Y_i(1)-Y_i(0)]$
	Does take the treatment: D_i=1	*Compliers* $Y_i(1,1)-Y_i(0,0)=$ $[Y_i(1)-Y_i(0)]$	*Always-takers* $Y_i(1,1)-Y_i(1,0)=0$

- If also *monotonicity* is fulfilled, the treatment effect is identified for the compliers

LATE interpretation

- More formally:
  1. Independence: $\{Y_i(D_{1i},1), Y_i(D_{0i},0), D_{1i}, D_{i0}\} \perp Z_i$
  2. First stage:* $E[D_i|Z_i=1] - E[D_i|Z_i=0] \neq 0$
  3. Exclusion: $Y_i(d,0) = Y_i(d,1) \equiv Y_{di}$, for $d = 0,1$
  4. Monotonicity: $D_{1i} \geq D_{0i}, \forall i$ (or vice versa)

  *under 1., the FS assumption becomes $E[D_{1i} - D_{0i}] \neq 0$

- Which of these assumptions can we test?

  1. Partly testable, 2. Testable, 3., 4. Not testable

- Under these assumptions IV estimates:

$$E[Y_i(1) - Y_i(0)|D_{1i} > D_{i0}] = \frac{E[Y_i|Z_i=1]-E[Y_i|Z_i=0]}{E[D_i|Z_i=1]-E[D_i|Z_i=0]}$$

- IV estimate the effect of the treatment on those who only take the treatment because they were assigned to it by Z_i (and they would not take if not assigned to it by Z_i)
  - these are *compliers*, those "moved" by the instrument
  - with 4., all i's moved by Z_i are moved in the same direction

LATE interpretation

- Why might LATE differ from the ATET?
- Never-takers, always-takers, and compliers probably have a reason to behave as they do:
  - Never-takers don't want treatment under any circumstances
  - Always-takers want it no matter what
  - The compliers only take it if our instrument tells them to
- Potential outcomes, and hence the treatment effects, may differ across these groups (*heterogeneous treatment effects*)
- IV only give us the treatment effect for compliers
- Whether that is interesting or not depends on the application

Example: Who benefits from charter schools?

Angrist et al. (2012)

- Controversial (in the US) topic: Charter schools
- Is attending charter schools good for student achievement?
- Kids attending charter schools are a selected group
- Student achievement is affected by a myriad of factors that may also affect the probability of attending a charter
- In Massachusetts entry to over-subscribed charter schools is decided by lottery

Example: Who benefits from charter schools?

Angrist et al. (2012)

- Angrist at al obtain data on the applicants who participated in a lottery to gain entry to a KIPP Charter School in Lynn, MA
- They use winning the entry lottery (= gaining entry) as an instrument for attending KIPP
- Think about the following questions
  1. Is winning the entry lottery as good as randomly assigned?
  2. Does winning the entry lottery have a direct effect on student achievement?
  3. Does winning lottery have an effect on attending KIPP?
  4. Are there likely to be defiers?

Observable characteristics

Angrist et al. (2012)

Table 1: Descriptive Statistics and Covariate Balance

	Means			Balance regressions	
	Lynn Public 5th graders	KIPP Lynn 5th graders	KIPP Lynn lottery applicants	No controls	Demographic controls
	(1)	(2)	(3)	(4)	(5)
Hispanic	0.418	0.565	0.538	-0.052	-
				(0.053)	
Black	0.173	0.235	0.256	0.033	-
				(0.044)	
White	0.296	0.168	0.179	-0.017	-
				(0.040)	
Asian	0.108	0.021	0.022	0.028*	-
				(0.015)	
Female	0.480	0.474	0.489	-0.002	-
				(0.054)	
Free/reduced price lunch	0.770	0.842	0.825	-0.031	-
				(0.041)	
Special Education	0.185	0.189	0.200	-0.009	-
				(0.043)	
Limited English Proficiency	0.221	0.172	0.206	-0.074	
				(0.047)	
Baseline Math Score	-0.307	-0.336	-0.389	0.097	0.034
				(0.114)	(0.107)
Baseline Verbal Score	-0.356	-0.399	-0.443	0.039	-0.036
				(0.119)	(0.105)
F-value from joint test				0.814	0.184
p-value from F-test				0.615	0.832
N for demographics	3964	285	446	446	446
N for baseline Math	3808	284	435	435	435
N for baseline ELA	3805	284	436	436	436

Notes: Columns (1), (2), and (3) report means of the variable indicated in each row. Column (1) reports 4th grade means for students that

Lottery to attend charter school

Angrist et al. (2012)

Table 2: KIPP Academy Lynn Lotteries

Lottery Cohort (1)	Calendar years observed (2)	Grades observed (3)	Number of applicants (4)	Number of applicants in lottery sample (5)	Percent offered (6)	Percent attended (7)	Average years at KAL (winners) (8)
2005-2006	2006-2009	5-8	138	106	0.925	0.670	2.56
2006-2007	2007-2009	5-7	117	86	0.674	0.535	2.29
2007-2008	2008-2009	5-6	167	118	0.627	0.534	1.68
2008-2009	2009	5	207	136	0.537	0.397	0.70
All cohorts	2006-2009	5-8	629	446	0.679	0.525	1.85

Lottery to attend charter school

Angrist et al. (2012)

- Things seem work beautifully:
  - observable characteristics are balanced across lottery winners and losers
  - we wouldn't expect winning a lottery to have any direct effects on achievement
- However, not all lottery winners actually attend:
  - 303 children (=0.679*446) were offered a slot
  - only 221 (73%) of winners actually attend and somehow 5 (3.5%) losers also attend

Lottery to attend charter school

Angrist et al. (2012)

- Lottery is not a standard RCT that assigns KIPP enrollment but an **RCT with imperfect compliance**, i.e. an instrument
  - First stage: clear effect of winning a lottery on attending
  - Reduced form: compare outcomes of losers and winners
  - LATE: divide the reduced form with the first stage
- The lottery randomizes the *eligibility* to enroll to KIPP (*not enrollment* itself, which is an individual decision)
- Angrist et al are interested in the effects on test scores in standard deviation units

Effects on test scores

Angrist et al. (2012)

Table 4: Lottery Results

		all applicants				Lynn public schools at baseline	
		First Stage	Reduced Form	2SLS	OLS	2SLS	OLS
Subject	Controls	(1)	(2)	(3)	(4)	(5)	(6)
Math	Basic	1.218***	0.437***	0.359***	0.301***	0.352***	0.304***
		(0.065)	(0.117)	(0.096)	(0.048)	(0.110)	(0.054)
		842	842	842	842	683	683
	Demographics	1.225***	0.399***	0.325***	0.312***	0.324***	0.332***
		(0.067)	(0.106)	(0.084)	(0.041)	(0.099)	(0.046)
		842	842	842	842	683	683
	Demographics & Baseline Scores	1.221***	0.430***	0.352***	0.314***	0.352***	0.344***
		(0.068)	(0.067)	(0.053)	(0.032)	(0.064)	(0.038)
		833	833	833	833	675	675
ELA	Basic	1.218***	0.189	0.155	0.169***	0.224*	0.166***
		(0.065)	(0.118)	(0.096)	(0.049)	(0.115)	(0.057)
		843	843	843	843	684	684
	Demographics	1.228***	0.124	0.101	0.170***	0.159*	0.179***
		(0.068)	(0.098)	(0.078)	(0.041)	(0.092)	(0.046)
		843	843	843	843	684	684
	Demographics & Baseline Scores	1.228***	0.164**	0.133**	0.174***	0.150**	0.185***
		(0.068)	(0.073)	(0.059)	(0.031)	(0.069)	(0.036)
		833	833	833	833	677	677

Effects on test scores

Angrist et al. (2012)

- The first stage implies that attendance increase by 1.2 years (perfect compliance would imply 1.75)
- Winners score about 0.4 sd's higher than losers in math
- The resulting LATE is 0.35 sd's
- This result is robust to including controls

Judge leniency design

Bhuller et al. (2020)

- **Question:** What is the causal effect of incarceration on recidivism and labor market outcomes?
  - Incarceration rates are increasing in most developed countries.
  - Prison time can deter crime/rehabilitate or be criminogenic
  - Ex-convicts have high recidivism/weak labor market attachment, but unobservables confound incarceration effect
- *Judge leniency design*:
  1. Random assigment of judges to cases (conditional on court-year FEs)
  2. Judges vary systematically in how strict they are
- Setting:
  - $D_i = \{0, 1\}$: incarceration of defendant i
  - $Z_{j(i)} \in [0, 1]$: average incarceration rate in *other cases* that judge j has handled (*leave-out mean*)

Judge leniency design

Bhuller et al. (2020)

- Is the instrument valid?
  1. **First stage**:
     - Being assigned to judge with 10 ppt higher incarceration rate increases the own incarceration probability by 5 ppt
  2. **Independence**:
     - Case and defendants' pre-determined X's are uncorrelated with Z
     - Adding controls to the FS doesn't affect estimates
  3. **Exclusion**: Does the judge leniency to incarceration affect defendants' outcomes only through the incarceration channel? (and not directly in other ways)
- Assumption 3. potentially problematic. Model extensions:
  - Control for judge stringency in other dimensions
  - Include an instrument for other trial sentencing decisions