

# Lecture 2: Instrumental Variables

Fabian Waldinger

# Topics Covered in Lecture

- ① Instrumental variables basics.
- ② Example: IV in the return to education literature: Angrist & Krueger (1991).
- ③ IV with heterogeneous treatment effects - LATE.
- ④ Weak instruments.

# Why Use IV?

- Instrumental Variables methods are typically used to address the following problems encountered in OLS regression:
  - ① Omitted variable bias.
  - ② Measurement error.
  - ③ Simultaneity or reverse causality.

## Example: Ability Bias in the Returns to Education

- Labour economists have been studying returns to education for a very long time.
- The typical model that is analyzed (going back to Mincer; here we abstract from the returns to experience) is:

$$Y_i = \alpha + \rho S_i + \gamma A_i + v_i$$

$Y_i$  = log of earnings.

$S_i$  = schooling measured in years.

$A_i$  = individual ability.

- Typically the econometrician cannot observe  $A_i$ .
- Suppose you therefore estimate the short regression:

$$Y_i = \alpha + \rho S_i + \eta_i$$

were  $\eta_i = \gamma A_i + v_i$

# Derivation of Ability Bias

- The OLS estimator for  $\rho$  in this simple case is:

$$\hat{\rho} = \frac{\text{Cov}(Y, S)}{\text{Var}(S)}$$

- plugging in true model for Y:

$$\hat{\rho} = \frac{\text{Cov}([\alpha + \rho S_i + \gamma A_i + v_i], S)}{\text{Var}(S)}$$

- Taking expectations we get:

$$\rho + \gamma \frac{\sigma_{A,S}}{\sigma_S}$$

$\frac{\sigma_{A,S}}{\sigma_S}$  is the regression coefficient if we regressed S on A.

- This is the classic ability bias if  $\gamma > 0$  and  $\sigma_{A,S} > 0$  the coefficient on schooling in the short regression would be upward biased.

# How IV Can be Used to Obtain Unbiased Estimates?

- How can we estimate the true  $\rho$  if ability is unobserved?
- Use an instrument  $Z$ .
- 2 important conditions for a valid IV:
  - ①  $\text{Cov}(S, Z) \neq 0$  (first stage exists).
  - ②  $\text{Cov}(Z, \eta) = 0$  (exclusion restriction:  $Z$  is uncorrelated with any other determinants of the dependent variable).
- While we can test whether the first condition is satisfied the second condition cannot be tested. As a researcher you have to try to convince your audience that it is satisfied.
- With one endogenous variable and one instrument the IV estimator is:

$$\rho^{IV} = \frac{\text{Cov}(Y_i, Z_i)}{\text{Cov}(S_i, Z_i)}$$

## IV is Consistent if IV Assumptions Are Satisfied

- Because thinking in regression coefficients can sometimes be easier we can divide both denominator and numerator by  $V(Z_i)$  to get:

$$\frac{\text{Cov}(Y_i, Z_i) / V(Z_i)}{\text{Cov}(S_i, Z_i) / V(Z_i)}$$

- The coefficient of interest is the ratio of the population regression of  $Y_i$  on  $Z_i$  (*reduced form*) to the population regression of  $S_i$  on  $Z_i$  (first stage).
- The IV estimator is consistent if the IV assumptions are satisfied:
- Substitute true model for  $Y$ :

$$\hat{\rho}^{IV} = \frac{\text{Cov}([\alpha + \rho S_i + \gamma A_i + v_i], Z)}{\text{Cov}(S, Z)} = \rho \frac{\text{Cov}([S], Z)}{\text{Cov}(S, Z)} + \gamma \frac{\text{Cov}([A], Z)}{\text{Cov}(S, Z)} + \frac{\text{Cov}([v_i], Z)}{\text{Cov}(S, Z)}$$

- Taking plims:

$$\text{plim } \hat{\rho}^{IV} = \rho$$

- because  $\text{Cov}([A], Z) = 0$  and  $\text{Cov}([v_i], Z) = 0$  due to the exclusion restriction, and  $\text{Cov}(S, Z) \neq 0$  (due to the first stage)

## IV Jargon

- Causal relationship of interest:

$$Y_i = \alpha + \rho S_i + \eta_i$$

- First-Stage regression:

$$S_i = \alpha + \gamma Z_i + \xi_i$$

- Second-Stage regression:

$$Y_i = \alpha + \rho \hat{S}_i + v_i$$

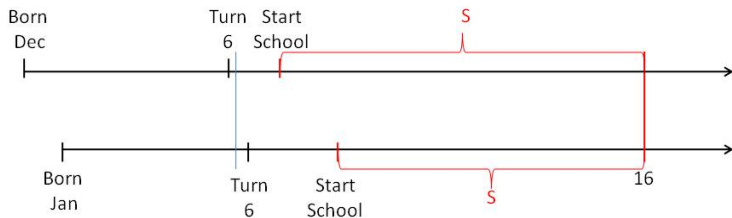
- Reduced form:

$$Y_i = \alpha + \delta Z_i + \varepsilon_i$$



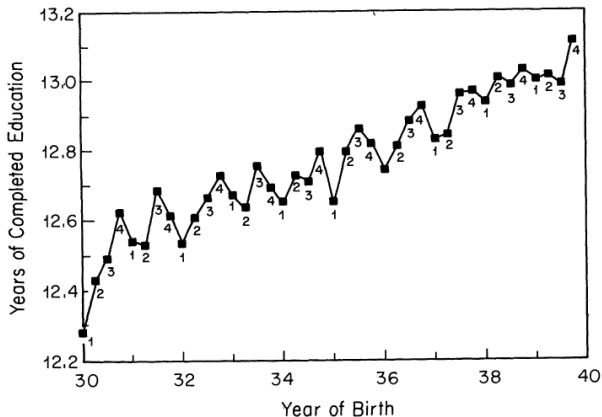
# Instrument for Education using Compulsory Schooling Laws

- In practice it is often difficult to find convincing instruments (in particular because many potential IVs do not satisfy the exclusion restriction).
- In the returns to education literature Angrist and Krueger (1991) had a very influential study where they used quarter of birth as an instrumental variable for schooling.
- In the US you could drop out of school once you turned 16.
- Children have different ages when they start school and thus different lengths of schooling at the time they turn 16 when they can potentially drop out.



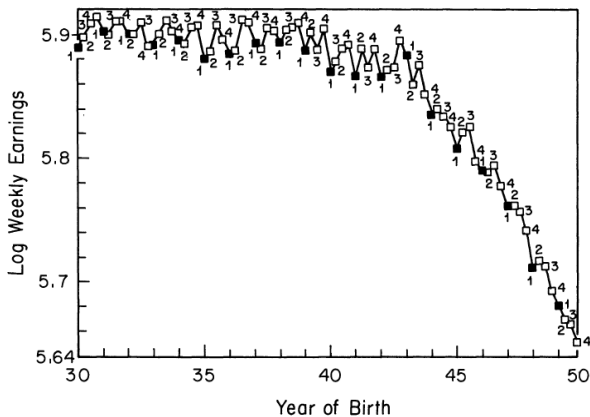
# First Stages

- Men born earlier in the year have lower schooling. This indicates that there is a first stage.



# Reduced Form

- Do differences in schooling due to different quarter of birth translate into different earnings?



# Two Stage Least Squares

- The first stage regression is:

$$S_i = X' \pi_{10} + \pi_{11} Z_i + \zeta_{1i}$$

- The reduced form regression is:

$$Y_i = X' \pi_{20} + \pi_{21} Z_i + \zeta_{2i}$$

- The covariate adjusted IV estimator is the sample analog of the ratio  $\frac{\pi_{21}}{\pi_{11}}$ .
- In practice one often estimates IV as Two-stage-least squares (2SLS).
- It is called 2SLS because you could estimate it as follows:

- ① Obtain the first stage fitted values:

$$\hat{S}_i = X' \hat{\pi}_{10} + \hat{\pi}_{11} Z_i$$

where  $\hat{\pi}_{10}$  and  $\hat{\pi}_{11}$  are OLS estimates of the first stage regression.

- ② Plug the first stage fitted values into the "second-stage equation".

$$Y_i = X' \alpha + \rho \hat{S}_i + error$$

# Two Stage Least Squares

- Despite the name the estimation is usually not done in two steps (if you would do that the standard errors would be wrong).
- STATA or other regression softwares are usually doing the job for you (and get the standard errors right).
- The intuition of 2SLS, however, is very useful:  
2SLS only retains the variation in  $S$  that is generated by quasi-experimental variation (and thus hopefully exogenous).
- Angrist and Krueger use more than one instrumental variable to instrument for schooling: they include a dummy for each quarter of birth.
- Their estimated first-stage regression is therefore:

$$S_i = X_i' \pi_{10} + \pi_{11} Z_{1i} + \pi_{12} Z_{2i} + \pi_{13} Z_{3i} + \zeta_{1i}$$

- The second stage is the same as before but the fitted values are from the new first stage.

# First Stage Regressions in Angrist & Krueger (1991)

Outcome variable	Birth cohort	Mean	Quarter-of-birth effect <sup>a</sup>			<i>F</i> -test <sup>b</sup> [ <i>P</i> -value]
			I	II	III	
Total years of education	1930–1939	12.79	-0.124 (0.017)	-0.086 (0.017)	-0.015 (0.016)	24.9 [0.0001]
	1940–1949	13.56	-0.085 (0.012)	-0.035 (0.012)	-0.017 (0.011)	18.6 [0.0001]
High school graduate	1930–1939	0.77	-0.019 (0.002)	-0.020 (0.002)	-0.004 (0.002)	46.4 [0.0001]
	1940–1949	0.86	-0.015 (0.001)	-0.012 (0.001)	-0.002 (0.001)	54.4 [0.0001]
Years of educ. for high school graduates	1930–1939	13.99	-0.004 (0.014)	0.051 (0.014)	0.012 (0.014)	5.9 [0.0006]
	1940–1949	14.28	0.005 (0.011)	0.043 (0.011)	-0.003 (0.010)	7.8 [0.0017]
College graduate	1930–1939	0.24	-0.005 (0.002)	0.003 (0.002)	0.002 (0.002)	5.0 [0.0021]
	1940–1949	0.30	-0.003 (0.002)	0.004 (0.002)	0.000 (0.002)	5.0 [0.0018]

# First Stage Regressions in Angrist & Krueger (1991)

Outcome variable	Birth cohort	Mean	Quarter-of-birth effect <sup>a</sup>			<i>F</i> -test <sup>b</sup> [ <i>P</i> -value]
			I	II	III	
Total years of education	1930–1939	12.79	-0.124 (0.017)	-0.086 (0.017)	-0.015 (0.016)	24.9 [0.0001]
	1940–1949	13.56	-0.085 (0.012)	-0.035 (0.012)	-0.017 (0.011)	18.6 [0.0001]
High school graduate	1930–1939	0.77	-0.019 (0.002)	-0.020 (0.002)	-0.004 (0.002)	46.4 [0.0001]
	1940–1949	0.86	-0.015 (0.001)	-0.012 (0.001)	-0.002 (0.001)	54.4 [0.0001]
Years of educ. for high school graduates	1930–1939	13.99	-0.004 (0.014)	0.051 (0.014)	0.012 (0.014)	5.9 [0.0006]
	1940–1949	14.28	0.005 (0.011)	0.043 (0.011)	-0.003 (0.010)	7.8 [0.0017]
College graduate	1930–1939	0.24	-0.005 (0.002)	0.003 (0.002)	0.002 (0.002)	5.0 [0.0021]
	1940–1949	0.30	-0.003 (0.002)	0.004 (0.002)	0.000 (0.002)	5.0 [0.0018]

- Quarter of birth is a strong predictor of total years of education.

# First Stage Regressions in Angrist & Krueger (1991)

Some Indication of the Validity of the Exclusion Restriction

Outcome variable	Birth cohort	Mean	Quarter-of-birth effect <sup>a</sup>			<i>F</i> -test <sup>b</sup> [ <i>P</i> -value]
			I	II	III	
Total years of education	1930–1939	12.79	-0.124 (0.017)	-0.086 (0.017)	-0.015 (0.016)	24.9 [0.0001]
	1940–1949	13.56	-0.085 (0.012)	-0.035 (0.012)	-0.017 (0.011)	18.6 [0.0001]
High school graduate	1930–1939	0.77	-0.019 (0.002)	-0.020 (0.002)	-0.004 (0.002)	46.4 [0.0001]
	1940–1949	0.86	-0.015 (0.001)	-0.012 (0.001)	-0.002 (0.001)	54.4 [0.0001]
Years of educ. for high school graduates	1930–1939	13.99	-0.004 (0.014)	0.051 (0.014)	0.012 (0.014)	5.9 [0.0006]
	1940–1949	14.28	0.005 (0.011)	0.043 (0.011)	-0.003 (0.010)	7.8 [0.0017]
College graduate	1930–1939	0.24	-0.005 (0.002)	0.003 (0.002)	0.002 (0.002)	5.0 [0.0021]
	1940–1949	0.30	-0.003 (0.002)	0.004 (0.002)	0.000 (0.002)	5.0 [0.0018]

- Reassuringly quarter of birth does not affect the probability of graduating from college.



# IV Results

## IV Estimates Birth Cohorts 20-29, 1980 Census

Independent variable	(1) OLS	(2) TSLS
Years of education	0.0711 (0.0003)	0.0891 (0.0161)
Race (1 = black)	—	—
SMSA (1 = center city)	—	—
Married (1 = married)	—	—
9 Year-of-birth dummies	Yes	Yes
8 Region-of-residence dummies	No	No
Age	—	—
Age-squared	—	—
$\chi^2$ [dof]	—	25.4 [29]

# IV Results - Including Some Covariates

IV Estimates Birth Cohorts 20-29, 1980 Census

Independent variable	(1) OLS	(2) TSLS	(3) OLS	(4) TSLS
Years of education	0.0711 (0.0003)	0.0891 (0.0161)	0.0711 (0.0003)	0.0760 (0.0290)
Race (1 = black)	—	—	—	—
SMSA (1 = center city)	—	—	—	—
Married (1 = married)	—	—	—	—
9 Year-of-birth dummies	Yes	Yes	Yes	Yes
8 Region-of-residence dummies	No	No	No	No
Age	—	—	-0.0772 (0.0621)	-0.0801 (0.0645)
Age-squared	—	—	0.0008 (0.0007)	0.0008 (0.0007)
$\chi^2$ [dof]	—	25.4 [29]	—	23.1 [27]

## IV Results - Including More Covariates and Interacting Quarter of Birth

- They also include specifications where they use 30 (quarter of birth  $\times$  year) dummies and 150 (quarter of birth  $\times$  state) dummies as IVs (intuition: the effect of quarter of birth may vary by birth year or state).
- This reduces standard errors.
- But also comes at the cost of potentially having a weak instruments problem (see below).

Independent variable	(1) OLS	(2) TOLS	(3) OLS	(4) TOLS
Years of education	0.0673 (0.0003)	0.0928 (0.0093)	0.0673 (0.0003)	0.0907 (0.0107)
Race (1 = black)	—	—	—	—
SMSA (1 = center city)	—	—	—	—
Married (1 = married)	—	—	—	—
9 Year-of-birth dummies	Yes	Yes	Yes	Yes
8 Region-of-residence dummies	No	No	No	No
50 State-of-birth dummies	Yes	Yes	Yes	Yes
Age	—	—	-0.0757 (0.0617)	-0.0880 (0.0624)
Age-squared	—	—	0.0008 (0.0007)	0.0009 (0.0007)

# Short Description of Angrist (1990) Veteran Draft Lottery Example

- In the following we will often refer to an example from Angrist's paper on the effects of military service on earnings.
- Angrist (1990) uses the Vietnam draft lottery as in IV for military service.
- In the 1960s and early 1970s, young American men were drafted for military service to serve in Vietnam.
- Concerns about the fairness of the conscription policy lead to the introduction of a draft lottery in 1970.
- From 1970 to 1972 random sequence numbers were assigned to each birth date in cohorts of 19-year-olds.
- Men with lottery numbers below a cutoff were drafted while men with numbers above the cutoff could not be drafted.
- The draft did not perfectly determinate military service:
  - Many draft-eligible men were exempted for health and other reasons.
  - Exempted men volunteered for service.

# Summary of Findings on Vietnam Draft Lottery

- ① First stage results:  
Having a low lottery number (being eligible for the draft) increases veteran status by about 16 percentage points (the mean of veteran status is about 27 percent).
- ② Second stage results:  
Serving in the army lowers earnings by between \$2,050 and \$2,741 per year.

# IV with Heterogeneous Treatment Effects

- Up to this point we only considered models where the causal effect was the same for all individuals (homogenous treatment effects):  
 $Y_{1i} - Y_{0i} = \rho$  for all  $i$ .
- We now try to understand what IV estimates if treatment effects are heterogeneous.
- This will inform us about two types of validity characterizing research designs:
  - ① Internal validity: Does the design successfully uncover causal effects for the population studied?
  - ② External validity: Do the study's results inform us about different populations?

# IV with Heterogeneous Treatment Effects

- Variables used in this setup:
  - $Y_i(d, z)$  = potential outcome of individual  $i$ .
  - $D_i$  = treatment dummy.
  - $Z_i$  = instrument dummy.
- Causal chain is:

$$Z_i \rightarrow D_i \rightarrow Y_i$$

- Notation for  $D_i$  :
  - $D_{1i}$  =  $i$ 's treatment status when  $Z_i = 1$
  - $D_{0i}$  =  $i$ 's treatment status when  $Z_i = 0$
- Observed treatment status is therefore:

$$D_i = D_{0i} + (D_{1i} - D_{0i})Z_i = \pi_0 + \pi_{1i}Z_i + \xi_i$$

$$\pi_0 = E[D_{0i}]$$

$\pi_{1i} = (D_{1i} - D_{0i})$  is the heterogeneous causal effect of the IV on  $D_i$ .

The average causal effect of  $Z_i$  on  $D_i$  is  $E[\pi_{1i}]$ .

# Key Assumptions in the Heterogeneous Effects Framework

## ① Independence assumption:

- The IV is independent of the vector of potential outcomes and potential treatment assignments (i.e. as good as randomly assigned):  
 $\{Y_i(D_{1i}, 1), Y_i(D_{0i}, 0), D_{1i}, D_{0i}\} \perp Z_i$
- The independence assumption is sufficient for a causal interpretation of the reduced form:

$$\begin{aligned} E[Y_i | Z_i = 1] - E[Y_i | Z_i = 0] \\ &= E[Y_i(D_{1i}, 1) | Z_i = 1] - E[Y_i(D_{0i}, 0) | Z_i = 0] \\ &= E[Y_i(D_{1i}, 1)] - E[Y_i(D_{0i}, 0)] \end{aligned}$$

- Independence also means that the first stage captures the causal effect of  $Z_i$  on  $D_i$  :

$$\begin{aligned} E[D_i | Z_i = 1] - E[D_i | Z_i = 0] &= E[D_{1i} | Z_i = 1] - E[D_{0i} | Z_i = 0] \\ &= E[D_{1i} - D_{0i}] \end{aligned}$$



## ② Exclusion restriction:

- $Y_i(d, z)$  is a function of  $d$  only. Or formally:  
 $Y_i(d, 0) = Y_i(d, 1)$  for  $d=0,1$ .
- In the Vietnam draft lottery example: an individual's earnings potential as a veteran or non-veteran are assumed to be unchanged by draft eligibility status.
- The exclusion restriction would be violated if low lottery numbers may have affected schooling (e.g. to avoid the draft). If this was the case the lottery number would be correlated with earnings for at least two cases:
  - ① through its effect on military service.
  - ② through its effect on educational attainment.
- The fact that the lottery number is randomly assigned (and therefore satisfies the independence assumption) does not ensure that the exclusion restriction is satisfied.

# Key Assumptions in the Heterogeneous Effects Framework

- Using the exclusion restriction we can define potential outcomes indexed solely against treatment status:

$$Y_{1i} = Y_i(1, 1) = Y_i(1, 0)$$

$$Y_{0i} = Y_i(0, 1) = Y_i(0, 0)$$

- In terms of potential outcomes we can write:

$$Y_i = Y_i(0, Z_i) + [Y_i(1, Z_i) - Y_i(0, Z_i)]D_i$$

$$Y_i = Y_{0i} + [Y_{1i} - Y_{0i}]D_i$$

- Random coefficients notation for this is:

$$Y_i = \alpha_o + \rho_i D_i$$

$$\text{with } \alpha_o = E[Y_{0i}] \text{ and } \rho_i = Y_{1i} - Y_{0i}$$

## ③ **First Stage:**

As in the constant coefficients model we also need that the instrument has to have a significant effect on treatment:

$$E[D_{1i} - D_{0i}] \neq 0$$

# Key Assumptions in the Heterogeneous Effects Framework

## 4 Monotonicity:

Either  $\pi_{1i} \geq 0$  for all  $i$  or  $\pi_{1i} \leq 0$  for all  $i$ .

- While the instrument may have no effect on some people, all those who are affected are affected in the same way.
- In the draft lottery example: draft eligibility may have had no effect on the probability of military service. But there should also be no one who was kept out of the military by being draft eligible.  $\rightarrow$  this is likely satisfied.
- In the quarter of birth example for schooling the assumption may not be satisfied (see Barua and Lang, 2009):  
Being born in the 4th quarter (which typically increases schooling) may have reduced schooling for some because their school enrollment was held back by their parents.
- Without monotonicity, IV estimators are not guaranteed to estimate a weighted average of the underlying causal effects of the affected group,  $Y_{1i} - Y_{0i}$ .

# IV Estimates LATE

- If all 4 assumptions are satisfied, IV estimates LATE (Local Average Treatment Effect).
- LATE is the average effect of  $X$  on  $Y$  for those whose treatment status has been changed by the instrument  $Z$ .
- In the draft lottery example: IV estimates the average effect of military service on earnings for the subpopulation who enrolled in military service because of the draft but would not have served otherwise. (This excludes volunteers and men who were exempted from military service for medical reasons for example).
- We have reviewed the properties of IV with heterogeneous treatment effects using a very simple dummy endogenous variable, dummy IV, and no additional controls example. The intuition of LATE generalizes to most cases where we have continuous endogenous variables and instruments, and additional control variables.

# Some LATE Framework Jargon

- The LATE framework partitions any population with an instrument into potentially 4 groups:
  - ① *Compliers*: The subpopulation with  $D_{1i} = 1$  and  $D_{0i} = 0$ .  
Their treatment status is affected by the instrument in the right direction.
  - ② *Always-takers*: The subpopulation with  $D_{1i} = D_{0i} = 1$ .  
They always take the treatment independently of  $Z$ .
  - ③ *Never-takers*: The subpopulation with  $D_{1i} = D_{0i} = 0$ .  
They never take the treatment independently of  $Z$ .
  - ④ *Defiers*: The subpopulation with  $D_{1i} = 0$  and  $D_{0i} = 1$ .  
Their treatment status is affected by the instrument in the "wrong" direction.
- These terms come from an analogy to the medical literature where the treatment is taking a pill for example.

# Monotonicity Ensures That There Are No Defiers

- Monotonicity ensures that there are no defiers.
- Why is it important to not have defiers?
  - If there were defiers, effects on compliers could be (partly) cancelled out by opposite effects on defiers.
  - One could then observe a reduced form which is close to 0 even though treatment effects are positive for everyone (but the compliers are pushed in one direction by the instrument and the defiers in the other direction).

# What Does IV Estimate and What Not?

## LATE and ATE

- As outlined above, with all 4 assumptions satisfied IV estimates the average treatment effect for compliers.
- Without further assumptions (e.g. constant causal effects) LATE is not informative about effects on never-takers or always-takers because the instrument does not affect their treatment status.
- In most applications we would be mostly interested in estimating the average treatment effect on the whole population (ATE).

$$E[Y_{1i} - Y_{0i}]$$

- This is usually not possible with IV.



## Other Potentially Interesting Treatment Effects

- Another effect which we may potentially be interested in estimating is the *average treatment effect on the treated* (ATT).
- Treatment status is:  $D_i = D_{0i} + (D_{1i} - D_{0i})Z_i$
- By monotonicity we cannot have  $D_{0i} = 1$  and  $(D_{1i} - D_{0i}) = 1$  since  $D_{0i} = 1$  implies  $D_{1i} = 1$ .
- The treated therefore either have  $D_{0i} = 1$  (always-takers) or  $(D_{1i} - D_{0i}) = 1$  and  $Z_i = 1$  (compliers)
- It follows that LATE is not the same as ATT.

$$\begin{aligned} E[Y_{1i} - Y_{0i} | D_i = 1] &= E[Y_{1i} - Y_{0i} | D_{0i} = 1] P[D_{0i} = 1 | D_i = 1] \\ &\quad \text{Effect on treated} \qquad \qquad \text{Effect on always takers} \\ &\quad + E[Y_{1i} - Y_{0i} | D_{1i} > D_{0i}] P[D_{1i} > D_{0i}, Z_i = 1 | D_i = 1] \\ &\qquad \qquad \qquad \text{Effect on compliers} \end{aligned}$$

- If there are no always takers we can, however, estimate ATT which is equal to LATE in that case.

# IV in Randomized Trials

- The use of IV methods can also be useful when evaluating a randomized trial.
- In many randomized trials, participation is voluntary among those randomly assigned to treatment.
- On the other hand people in the control group usually do not have access to treatment.
  - only those who are particularly likely to benefit from treatment will actually take up treatment (leads almost always to positive selection bias)
  - if you just compare means between treated and untreated individuals (using OLS) you will obtain biased treatment effects.
- Solution:
- Instrument for treatment with whether you were offered treatment.
  - you estimate LATE.

# IV in Randomized Trials - Example: Training Programme

- David Autor calculates the different effects for the JTPA training programme.
- The programme provides job training to people facing barriers for employment (e.g. dislocated workers, disadvantaged young adults).
- The programme was randomly offered.
- Only 60 percent of those who were offered the training actually received it.
- 2 percent of people in the control group also received training.
- Autor evaluates differences in earnings in the 30 month period after random assignment.

# IV in Randomized Trials - Example Training Programme

Table 4.4.1: Results from the JTPA experiment: OLS and IV estimates of training impacts

	Comparisons by Training Status		Comparisons by Assignment Status		Instrumental Variable Estimates	
	Without Covariates (1)	With Covariates (2)	Without Covariates (3)	With Covariates (4)	Without Covariates (5)	With Covariates (6)
A. Men	3,970 (555)	3,754 (536)	1,117 (569)	970 (546)	1,825 (928)	1,593 (895)
B. Women	2,133 (345)	2,215 (334)	1,243 (359)	1,139 (341)	1,942 (560)	1,780 (532)

- Columns (1) and (2) show OLS estimates.
- Columns (3) and (4) show ITT (reduced form) estimates.
- Columns (5) and (6) show IV estimates.
- Here we actually estimate the ATT (not only LATE) because there are almost no always-takers.

# Weak Instruments

- As you will know from your econometrics courses IV is consistent but not unbiased.
- For a long time researchers estimating IV models never cared much about the small sample bias.
- In the early 1990s a number of papers, however, highlighted that IV can be severely biased in particular if instruments are weak (i.e. the first stage relationship is weak) and if you use many instruments to instrument for one endogenous variable (i.e. there are many overidentifying restrictions).
- In the worst case, if the instruments are so weak that there is no first stage the 2SLS sampling distribution is centered on the probability limit of OLS.

# Weak Instruments - Bias Towards OLS

- Let's consider a model with a single endogenous regressor and a simple constant treatment effect.
- The causal model of interest is:

$$y = \beta x + \eta \quad (1)$$

- The  $N \times Q$  matrix of instrumental variables is  $Z$  with the first stage equation:

$$x = \mathbf{Z}'\pi + \zeta \quad (2)$$

- If  $\eta_i$  and  $\zeta_i$  are correlated, estimating (1) by OLS would lead to biased results.
- The OLS bias is:

$$E[\beta^{OLS} - \beta] = \frac{\text{Cov}[\eta, x]}{\text{Var}[x]}$$

- If  $\eta_i$  and  $\zeta_i$  are correlated the OLS bias is therefore:  $\frac{\sigma_{\eta\zeta}}{\sigma_x^2}$

# Weak Instruments - Bias Towards OLS

- It can be shown that the bias of 2SLS is approximately:

$$E[\hat{\beta}_{SLS} - \beta] \approx \frac{\sigma_{\eta\xi}}{\sigma_{\xi}^2} \frac{1}{F+1}$$

F is the population analogue of the F-statistic for the joint significance of the instruments in the first stage regression.

See MHE pp. 206-208 for a derivation.

- If the first-stage is weak (i.e.  $F \rightarrow 0$ ) the bias of 2SLS approaches  $\frac{\sigma_{\eta\xi}}{\sigma_{\xi}^2}$ .
- This is the same as the OLS bias as for  $\pi = 0$  in equation (2) (i.e. there is no first stage relationship)  $\sigma_x^2 = \sigma_{\xi}^2$  and therefore the OLS bias  $\frac{\sigma_{\eta\xi}}{\sigma_x^2}$  becomes  $\frac{\sigma_{\eta\xi}}{\sigma_{\xi}^2}$ .
- If the first stage is very strong ( $F \rightarrow \infty$ ) the IV bias goes to 0.

# Weak Instruments - Adding More Instruments

- Adding more weak instruments will increase the bias of 2SLS. By adding further instruments without predictive power the first stage F-statistic goes towards 0 and the bias increases.
- If the model is just identified, weak instrument bias is less of a problem: (in MHE p. 209 they write it is approximately unbiased - this is only true if the first stage is not 0; see [http://econ.lse.ac.uk/staff/spischke/mhe/josh/solon\\_justid\\_April14.pdf](http://econ.lse.ac.uk/staff/spischke/mhe/josh/solon_justid_April14.pdf)).
- Bound, Jaeger, and Baker (1995) highlighted this problem for the Angrist & Krueger study. A&K present findings from using different sets of instruments:
  - ① quarter of birth dummies  $\rightarrow$  3 instruments.
  - ② quarter of birth + (quarter of birth)  $\times$  (year of birth) dummies  $\rightarrow$  30 instruments.
  - ③ quarter of birth + (quarter of birth)  $\times$  (year of birth) + (quarter of birth)  $\times$  (state of birth)  $\rightarrow$  180 instruments.



# Adding Instruments in Angrist & Krueger

Table from Bound, Jaeger, and Baker (1995) - 3 and 28/30 IVs

	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
Coefficient	.063 (.000)	.142 (.033)	.063 (.000)	.081 (.016)	.063 (.000)	.060 (.029)
F (excluded instruments)		13.486		4.747		1.613
Partial $R^2$ (excluded instruments, $\times 100$ )		.012		.043		.014
F (overidentification)		.932		.775		.725
<i>Age Control Variables</i>						
Age, Age <sup>2</sup>	x	x			x	x
9 Year of birth dummies			x	x	x	x
<i>Excluded Instruments</i>						
Quarter of birth		x		x		x
Quarter of birth $\times$ year of birth				x		x
Number of excluded instruments		3		30		28

- Adding more weak instruments reduced the first stage F-statistic and moves the coefficient towards the OLS coefficient.

# Adding Instruments in Angrist & Krueger

Table from Bound, Jaeger, and Baker (1995) - 180 IVs

	(1) OLS	(2) IV
Coefficient	.063 (.000)	.083 (.009)
F (excluded instruments)		2.428
Partial $R^2$ (excluded instruments, $\times 100$ )		.133
F (overidentification)		.919
<i>Age Control Variables</i>		
Age, Age <sup>2</sup>		
9 Year of birth dummies	x	x
<i>Excluded Instruments</i>		
Quarter of birth		x
Quarter of birth $\times$ year of birth		x
Quarter of birth $\times$ state of birth		x
Number of excluded instruments		180

- Adding more weak instruments reduced the first stage F-statistic and moves the coefficient towards the OLS coefficient.

# What Can You Do If You Have Weak Instruments?

- With weak instruments you have the following options:
  - ① Use a just identified model with your strongest IV.  
If the instrument is very weak, however, your standard errors will probably be very large.
  - ② Use a limited information maximum likelihood estimator (LIML).  
This is approximately median unbiased for overidentified constant effects models. It provides the same asymptotic distribution as 2SLS (under constant effects) but provides a finite-sample bias reduction. (LIML is programmed for STATA 10/11 incorporated in the `ivregress` command).
  - ③ Find stronger instruments.

# Practical Tips For IV Papers

- ① Report the first stage.
  - Does it make sense?
  - Do the coefficients have the right magnitude and sign?
- ② Report the F-statistic on the excluded instrument(s).
  - Stock, Wright, and Yogo (2002) suggest that F-statistics above 10 indicate that you do not have a weak instrument problem (but this is of course not a proof).
  - If you have more than one endogenous regressor for which you want to instrument, reporting the first stage F-statistic is not enough (because 1 instrument could affect both endogenous variables and the other could have no effect - the model would be underidentified). In that case you want to report the Cragg-Donald EV statistic (more on this next week in Waldinger, 2010).

# Practical Tips For IV Papers

- ③ If you have many IVs pick your best instrument and report the just identified model (weak instrument problem is much less problematic).
- ④ Check overidentified 2SLS models with LIML.
- ⑤ Look at the Reduced Form.
  - The reduced form is estimated with OLS and is therefore unbiased.
  - If you can't see the causal relationship of interest in the reduced form it is probably not there.