

9. Instrumental variables

LPO 8852: Regression II

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IV - introduction

An **instrumental variable** can be used to isolate exogenous variation in a explanatory variable that would otherwise be endogenous. Some useful applications:

- Addressing OVB when adequate controls or panel data are unavailable
- “Natural experiments”
- Partial or incomplete random assignment
- Fuzzy regression discontinuity (RD)
- Correcting for measurement error

Some rules of covariance

Recall that for random variables X , Y , and Z and constant a :

- 1 $Cov(aX, Y) = aCov(X, Y)$
- 2 $Cov(X + c, Y) = Cov(X, Y)$
- 3 $Cov(X + Y, Z) = Cov(X, Z) + Cov(Y, Z)$
- 4 $Cov(X, X) = Var(X)$

All of these follow from the definition of covariance:

$$Cov(X, Y) = \sigma_{XY} = E[(X - E(X))(Y - E(Y))]$$

Method of moments estimation of β_1

Covariance algebra is one way to arrive at the OLS estimator of β_1 .
Consider the simple regression:

$$Y_i = \beta_0 + \beta_1 X_i + u_i$$

The covariance of Y and X can be written:

$$\begin{aligned} Cov(Y, X) &= Cov(\beta_0 + \beta_1 X + u, X) \\ &= \beta_1 Cov(X, X) + Cov(u, X) \\ \sigma_{YX} &= \beta_1 \sigma_X^2 + \sigma_{Xu} \end{aligned}$$

And so:

$$\beta_1 = \frac{\sigma_{YX}}{\sigma_X^2} + \frac{\sigma_{Xu}}{\sigma_X^2}$$

Method of moments estimation of β_1

What this says is that the population slope β_1 is the population covariance between Y and X (σ_{YX}) divided by the population variance of X (σ_X^2), plus a bias term.

$$\beta_1 = \frac{\sigma_{YX}}{\sigma_X^2} + \frac{\sigma_{Xu}}{\sigma_X^2}$$

If the population covariance between X and u is 0 (σ_{Xu}), we get the traditional expression for the population slope:

$$\beta_1 = \frac{\sigma_{YX}}{\sigma_X^2}$$

Method of moments estimation of β_1

We estimate β_1 using the sample analogs of σ_{YX} and σ_X^2 , yielding the traditional OLS slope estimator:

$$\hat{\beta}_1 = \frac{\sum_{i=1}^n (X_i - \bar{X})(Y_i - \bar{Y})}{\sum_{i=1}^n (X_i - \bar{X})^2}$$

This is called the **method of moments** derivation of β_1 since it relies on moments of Y and X (variance and covariance).

The OLS derivation of β_1 leads you to the same formula.

IV introduction

Now consider the same simple regression of Y on X , but where $\text{Cov}(X_i, u_i) \neq 0$ (X_i is endogenous).

$$Y_i = \beta_0 + \beta_1 X_i + u_i$$

Suppose a third variable Z_i is available, where:

- ① $\text{Cov}(Z_i, X_i) \neq 0$
- ② $\text{Cov}(Z_i, u_i) = 0$

Z_i is an *instrumental variable* or *instrument*.

IV introduction

We cannot test the second of these conditions, since u is unobserved. We must rely on theory or introspection to rationalize this.

We *can* offer evidence for the first condition, by estimating this regression and testing for significance of $\hat{\pi}_1$:

$$X_i = \pi_0 + \pi_1 * Z_i + v_i$$

IV introduction

When might the second condition be violated? Suppose one were interested in the following simple regression for college students:

$$\text{final grade} = \beta_0 + \beta_1(\text{days absent}) + u$$

We can hypothesize that $\beta_1 < 0$, but one might worry that the coefficient in the above regression is not causal. There may be factors associated with class attendance that are also associated with the final grade (e.g., general motivation, subject interest, family resources).

IV introduction

A potential instrument is the student's distance from home to school. It is plausible that students who live further from school miss more classes due to weather, traffic, etc. This would imply $\text{Cov}(Z, X) \neq 0$. One could also argue that unanticipated weather and traffic are uncorrelated with u in the population model of interest.

However, what if less engaged students choose to live further (on average) from school? Or if housing prices are high near school such that distance to school is related to ability to pay? Then distance to school may be correlated with the unobserved component u in the population model of interest, or $\text{Cov}(Z, u) \neq 0$.

More on this later.

Method of moments estimation of $\beta_{1,IV}$

What can a (valid) instrument do for us? Begin with:

$$Y_i = \beta_0 + \beta_1 X_i + u_i$$

Applying a rule of covariance:

$$\text{Cov}(Z_i, Y_i) = \beta_1 \text{Cov}(Z_i, X_i) + \text{Cov}(Z_i, u_i)$$

$$\beta_{1,IV} = \frac{\text{Cov}(Z_i, Y_i)}{\text{Cov}(Z_i, X_i)} = \frac{\sigma_{ZY}}{\sigma_{ZX}}$$

as long as the two *identifying assumptions* hold: $\text{Cov}(Z, u) = 0$ and $\text{Cov}(Z, X) \neq 0$.

Method of moments estimation of $\beta_{1,IV}$

Can be estimated using sample analogs:

$$\hat{\beta}_{1,IV} = \frac{\sum_{i=1}^n (Z_i - \bar{Z})(Y_i - \bar{Y})}{\sum_{i=1}^n (Z_i - \bar{Z})(X_i - \bar{X})}$$

Compare this to the OLS simple regression slope estimator, below. If X itself satisfies the two identifying assumptions (i.e., it is exogenous), this simplifies to the traditional OLS estimator.

$$\hat{\beta}_{1,OLS} = \frac{\sum_{i=1}^n (X_i - \bar{X})(Y_i - \bar{Y})}{\sum_{i=1}^n (X_i - \bar{X})^2}$$

In this case we could say that X is itself an “instrument.”

Application to charter school lottery

We are interested in the following *structural equation*, where Y_i = a student test score and D_i = attendance at a charter school:

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

In practice, attendance at charter schools is non-random. OLS estimation of the above may suffer from omitted variables bias: $E(u_i|D_i) \neq 0$.

Fortunately, at over-subscribed charters, schools hold random lotteries which determine offers of admission. Let $Z_i = 1$ if a student receives a random offer to attend a charter school and $Z_i = 0$ otherwise.

Application to charter school lottery

Z_i is a valid instrumental variable for D_i since $\text{Cov}(Z_i, D_i) > 0$ and $\text{Cov}(Z_i, u_i) = 0$. We can use instrumental variables to estimate the population slope coefficient β_1 :

$$\beta_{1,IV} = \frac{\text{Cov}(Z_i, Y_i)}{\text{Cov}(Z_i, D_i)}$$

$$\hat{\beta}_{1,IV} = \frac{\sum_{i=1}^n (Z_i - \bar{Z})(Y_i - \bar{Y})}{\sum_{i=1}^n (Z_i - \bar{Z})(D_i - \bar{D})}$$

Application to charter school lottery

Alternatively, consider the following two simple regressions:

$$Y_i = \alpha + \rho Z_i + w_i$$

$$D_i = \gamma + \phi Z_i + v_i$$

The former equation is called the *reduced form*, the latter is the *first stage*. Z_i is randomly assigned, so there is no OVB in either case. The population slope coefficients can be written as:

$$\rho = \frac{\text{Cov}(Z_i, Y_i)}{\text{Var}(Z_i)}$$

$$\phi = \frac{\text{Cov}(Z_i, D_i)}{\text{Var}(Z_i)}$$

Application to charter school lottery

Note that:

$$\frac{\rho}{\phi} = \frac{\text{Cov}(Z_i, Y_i)}{\text{Cov}(Z_i, D_i)} \times \frac{\text{Var}(Z_i)}{\text{Var}(Z_i)} = \frac{\text{Cov}(Z_i, Y_i)}{\text{Cov}(Z_i, D_i)}$$

That is, the reduced form slope coefficient divided by the first stage coefficient is the instrumental variables estimator of β_1 , the causal effect of charter school attendance.

One can use the sample analogs $\hat{\rho}$ and $\hat{\phi}$ to estimate β_1 .

Application to charter school lottery

Both Z_i and D_i are binary variables in this example. Another way to write the slope coefficients in the reduced form and first stage are:

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

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

ρ is the difference in the mean of Y for those randomly assigned an offer and those not—the **ITT**. ϕ is the difference in the proportion treated for those randomly assigned an offer and those not. The ratio is $\beta_{1,IV}$:

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

KIPP example (Angrist & Pischke, ch. 3)

KIPP randomizes the *offer* of a seat at its schools. Angrist et al. (2010, 2012) examined the impact attending KIPP Lynn (MA).

- 629 applicants between 2005 and 2008
- 446 assigned via the lottery and had complete data
- 303 (68%) were offered a seat
- 221 of the 303 enrolled in KIPP (73%)
- 3.5% of lottery losers enrolled in KIPP

The lottery should yield a treatment and control group that are on average equivalent at baseline, including pre-treatment outcomes. See Table 3.1, column 3 panel A.

KIPP example (Angrist & Pischke, ch. 3)

TABLE 3.1
Analysis of KIPP lotteries

	KIPP applicants				
	Lynn public fifth graders	KIPP Lynn lottery winners	Winners vs. losers	Attended KIPP	Attended KIP vs. other
	(1)	(2)	(3)	(4)	(5)
Panel A. Baseline characteristics					
Hispanic	.418	.510	-.058 (.058)	.539	.012 (.054)
Black	.173	.257	.026 (.047)	.240	-.001 (.043)
Female	.480	.494	-.008 (.059)	.495	-.009 (.055)
Free/Reduced price lunch	.770	.814	-.032 (.046)	.828	.011 (.043)
Baseline math score	-.307	-.290	.102 (.120)	-.289	.069 (.109)
Baseline verbal score	-.356	-.386	.063 (.125)	-.368	.088 (.114)
Panel B. Outcomes					
Attended KIPP	.000	.787	.741 (.037)	1.000	1.000
Math score	-.363	-.003	.355 (.115)	.095	.467 (.103)
Verbal score	-.417	-.262	.113 (.122)	-.211	.213 (.109)
Sample size	3,964	253	371	204	371

Notes: This table describes baseline characteristics of Lynn fifth graders and reports estimated effect for Knowledge In Power Program (KIPP) Lynn applicants. Means appear in column (1), (2), and (4). Column (3) shows differences between lottery winners and losers. These are coefficients from regressions that control for risk sets, namely, dummies for year and grade of application and the presence of a sibling applicant. Column (5) shows differences between KIPP students and applicants who did not attend KIPP. Standard errors are reported in parentheses.

KIPP example (Angrist & Pischke, ch. 3)

Using *math score* estimates from Panel B of Table 3.1:

Reduced form:

$$\hat{\rho} = \text{Avg}[Y_i | Z_i = 1] - \text{Avg}[Y_i | Z_i = 0] = 0.355$$

First stage:

$$\hat{\phi} = \text{Avg}[D_i | Z_i = 1] - \text{Avg}[D_i | Z_i = 0] = 0.741$$

Instrumental variables estimate:

$$\frac{\hat{\rho}}{\hat{\phi}} = \frac{0.355}{0.741} = 0.479$$

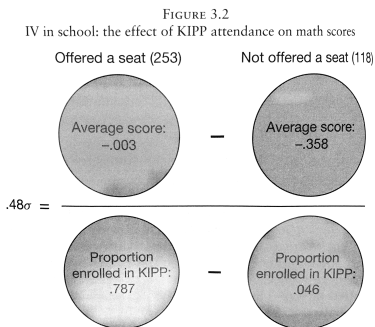
KIPP example (Angrist & Pischke, ch. 3)

“The IV estimator converts KIPP *offer* estimates into KIPP *attendance* effects.” The logic:

- The instrument (Z) has a causal effect on KIPP enrollment (D)—the *first stage*
- The instrument (Z) is randomly assigned (or is as good as randomly assigned) and is thus unrelated to omitted variables in the main equation: the *independence assumption*
- The instrument (Z) affects Y only through D : the *exclusion restriction*

The exclusion restriction implies that the 0.355 difference in Y is attributable only to the 0.741 increase in KIPP attendance.

KIPP example (Angrist & Pischke, ch. 3)



Note: The effect of Knowledge Is Power Program (KIPP) enrollment described by this figure is $.48\sigma = .355\sigma / .741$.

KIPP example (Angrist & Pischke, ch. 3)

The quantity estimated by IV here is a *local average treatment effect* (LATE). Why?

- The ratio tells us the ATE for those induced into treatment by the instrument (the lottery offer): the *compliers*
- The first stage is driven by compliers.
- We don't learn anything about *always-takers* or *never-takers*.
- We assume there are no *defiers*. This is called a *monotonicity* assumption—the instrument pushes treatment in one direction only.

If the treated population includes always-takers, the LATE and TOT are not generally the same. (Always-takers are treated, and may have different potential outcomes from the compliers).

KIPP example (Angrist & Pischke, ch. 3)

Example: when there are always-takers and never-takers

	Instrument Z_i (assignment to treatment)					
	$Z_i = 1$	$Z_i = 0$	$Z_i = 1$ Pr(D=1)	$Z_i = 0$ Pr(D=1)	$Z_i = 1$ Share	$Z_i = 0$ Share
Compliers	$Z_i = 1$ $D_i = 1$	$Z_i = 0$ $D_i = 0$	100	0	25	25
Always takers	$Z_i = 1$ $D_i = 1$	$Z_i = 0$ $D_i = 1$	100	100	15	15
Never takers	$Z_i = 1$ $D_i = 0$	$Z_i = 0$ $D_i = 0$	0	0	10	10
Defiers	$Z_i = 1$ $D_i = 0$	$Z_i = 0$ $D_i = 1$	0	100	0	0

Consider:

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

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

KIPP example (Angrist & Pischke, ch. 3)

Consider:

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

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

The $Z_i = 1$ and $Z_i = 0$ groups include the same proportions of always-takers and never-takers. The population under study includes some proportion of always-takers and never-takers. If treatment is randomly assigned, they should be evenly represented in the $Z_i = 1$ and $Z_i = 0$ groups.

IV bivariate regression—estimator properties

- The IV estimator is *consistent* as long as the identification assumptions hold:
 - ▶ $\text{Cov}(Z_i, X_i) \neq 0$
 - ▶ $\text{Cov}(Z_i, u_i) = 0$
- The IV estimator is *biased* in finite samples (as long as X and u are in fact correlated). More on this later.
- The IV estimator is *less efficient* than OLS.

IV bivariate regression—estimator properties

Assuming homoskedasticity, the asymptotic variance of $\hat{\beta}_{1,IV}$ is:

$$\text{Var}(\hat{\beta}_{1,IV}) = \frac{\sigma^2}{n\sigma_x^2\rho_{x,z}^2}$$

- σ_x^2 is the variance of x (can estimate with s_x^2)
- $\rho_{x,z}^2$ is the squared correlation between X and Z (can estimate with R^2 from a regression of X on Z)
- σ^2 is the variance of the residuals, conditional on Z (can estimate with residuals from the IV model)

The presence of $\rho_{x,z}^2 \leq 1$ in the denominator implies that the variance of the IV estimator will be *greater* than that of the OLS estimator. This is intuitive: we are using a portion of the variation in X .

MDVE example (Angrist & Pischke, ch. 3)

MDVE is the Minneapolis Domestic Violence Experiment (1980s), with random assignment to arrest, separation, or counseling (advice). Adherence to random assignment was not strict. (See Table 3.3).

TABLE 3.3
Assigned and delivered treatments in the MDVE

Assigned treatment	Delivered treatment			Total
	Arrest	Coddled		
		Advise	Separate	
Arrest	98.9 (91)	0.0 (0)	1.1 (1)	29.3 (92)
Advise	17.6 (19)	77.8 (84)	4.6 (5)	34.4 (108)
Separate	22.8 (26)	4.4 (5)	72.8 (83)	36.3 (114)
Total	43.4 (136)	28.3 (89)	28.3 (89)	100.0 (314)

Notes: This table shows percentages and counts for the distribution of assigned and delivered treatments in the Minneapolis Domestic Violence Experiment (MDVE). The first three columns show row percentages. The last column reports column percentages. The number of cases appears in parentheses.

MDVE example (Angrist & Pischke, ch. 3)

Define treatment assignment (Z) as assignment to a non-arrest category. Define $D = 1$ as non-arrest treatment delivered. The outcome of interest is recidivism (21.1% for those assigned to the non-arrest intervention, and 9.7% for those assigned to the arrest intervention).

Reduced form:

$$\hat{\rho} = \text{Avg}[Y_i|Z_i = 1] - \text{Avg}[Y_i|Z_i = 0] = 0.211 - 0.097 = 0.114$$

First stage:

$$\hat{\phi} = \text{Avg}[D_i|Z_i = 1] - \text{Avg}[D_i|Z_i = 0] = 0.797 - 0.011 = 0.786$$

$$\hat{\beta}_{1,IV} = \hat{\rho}/\hat{\phi} = 0.114/0.786 = 0.145$$

MDVE example (Angrist & Pischke, ch. 3)

The point estimate of 0.145 is a LATE: the impact of a non-arrest intervention on the population induced into this intervention by the random assignment.

- It is not informative about never-takers: those who would receive the arrest intervention in any case.
- It is not informative about always-takers: those who would receive the *non-arrest* intervention in any case.
- Table 3.3 suggests the population of always-takers is small—there aren't many assigned to *arrest* who were delivered the non-arrest intervention. With no always-takers, the LATE is also the TOT.

Instruments for continuous variables

The examples we have seen thus far might be called “broken experiments,” where assignment to treatment was random, but treatment delivery (or participation) was not. Both Z and D were binary variables.

Instrumental variables are also used in *natural experiments* and other designs where the IV is used to isolate exogenous variation in X , where X is a continuous variable.

Instruments for continuous variables

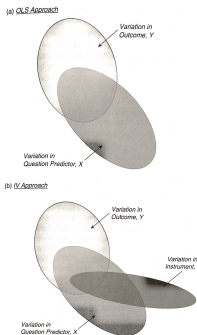


Figure 10.1. Graphical analog for the population variation and covariance among outcome Y , potentially endogenous question predictor X , and instrument Z , used for distinguishing the OLS and IV approaches (a) OLS Approach: bivariate relationship between Y and X ; (b) IV Approach: trivariate relationship among Y , X , and Z .

Instrument for family size

Consider the following regression intended to estimate the effect of family size on a child's years of schooling (the "quantity-quality tradeoff"):

$$educ = \beta_0 + \beta_1 famsize + u$$

Family size is likely endogenous, correlation with omitted variables related to both family size and educational attainment. Can we identify an instrumental variable Z that satisfies the identification assumptions?

- $Cov(Z, famsize) \neq 0$
- $Cov(Z, u) = 0$

Instrument for family size

Some potential candidates:

- Twins: an "exogenous shock" to family size (Angrist, Lavy, & Schlosser, 2010)
- Sibling sex composition: families with two boys or two girls are more likely to have a third child (Angrist & Evans, 1998)

Instrument for family size

Issues to consider:

- First stage: both have a positive association with family size. See Table 3.4 (later).
- Exclusion restriction: can we assume the presence of twins or two siblings of the same sex has no effect on educational attainment, except through family size? Consider effects of gender mix on educational inputs.
- Independence: can we assume that the presence of twins or two siblings of the same sex is uncorrelated with omitted variables in u ? Consider in-vitro fertilization.

Instrument for family size

If the identification assumptions hold, we again have two regressions to estimate:

$$educ_i = \alpha + \rho Z_i + w_i$$

$$famsize_i = \gamma + \phi Z_i + v_i$$

These are the reduced form and first stage equations. The IV estimate of β_1 in the original model can be obtained as $\hat{\rho}/\hat{\phi}$.

Angrist et al. find an insignificant $\hat{\rho}$. If there is no reduced form effect, there isn't a LATE.

Precision and IV

Consider again the asymptotic variance of $\hat{\beta}_{1,IV}$:

$$\text{Var}(\hat{\beta}_{1,IV}) = \frac{\sigma^2}{n\sigma_x^2\rho_{x,z}^2}$$

- A weak first stage (small $\rho_{x,z}^2$) can yield large standard errors
- A large sample size (n) can counteract this.

In the family size example, twins have a larger first stage, but there are fewer cases of twins. The gender mix first stage is smaller, but the number of treated cases is much larger. Again, see Table 3.4 (later).

Weak instruments

We cannot verify the exclusion restriction $\text{Cov}(Z, u) = 0$ in a simple regression, so there always remains some possibility this is violated. For large sample bias, we *hope* this correlation is small:

$$\text{plim}\hat{\beta}_{1,IV} = \beta_1 + \frac{\text{Corr}(Z, u)}{\text{Corr}(Z, X)} \times \frac{\sigma_u}{\sigma_x}$$

Even when $\text{Corr}(X, u)$ is small, the bias can be large if $\text{Corr}(Z, X)$ is small. A large sample does *not* help in this case. More on this later.

Two stage least squares

In the family size example, we considered two excluded instruments: twin births and the gender mix of the first two children. If both instruments are valid, can we use them together to estimate the causal effect of *famsize*?

Yes: using **two-stage least squares** (2SLS)

- Combines multiple instrumental variables efficiently
- Can include covariates when the instrument is imperfect (i.e., where the assumption $Cov(Z, u) = 0$ does not hold without conditioning on the covariates).

Two stage least squares—single instrument

Stage 1: get predicted values from first stage regression with single instrument Z_i

$$\widehat{X}_i = \widehat{\gamma} + \widehat{\phi}Z_i$$

Stage 2: use predicted values in the structural equation

$$Y_i = \beta_0 + \beta_1\widehat{X}_i + u_i$$

In this example, the 2SLS estimator of β_1 is equivalent to the IV estimator (the ratio of the reduced form slope to the first stage slope, or $\widehat{\rho}/\widehat{\phi}$)

Two stage least squares—single instrument

Can also include covariates:

Stage 1: get predicted values

$$\widehat{X_{1i}} = \hat{\gamma} + \hat{\phi}Z_i + \hat{\alpha}_1X_{2i}$$

Stage 2: use predicted values in the structural equation

$$Y_i = \beta_0 + \beta_1\widehat{X_{1i}} + \alpha_2X_{2i} + u_i$$

X_{1i} is the endogenous explanatory variable X_{2i} is an exogenous explanatory variable. Note the covariate(s) need to be included in both stages.

Two stage least squares—multiple instruments

Stage 1: get predicted values

$$\widehat{X_{1i}} = \hat{\gamma} + \hat{\phi}_1Z_{1i} + \hat{\phi}_2Z_{2i} + \hat{\alpha}_1X_{2i}$$

Stage 2: use predicted values in the structural equation

$$Y_i = \beta_0 + \beta_1\widehat{X_{1i}} + \alpha_2X_{2i} + u_i$$

Note the covariate(s) needs to be included in both stages. The two instruments Z_{1i} and Z_{2i} are only in the first stage.

With multiple instruments, the estimator of β_1 in Stage 2 is a weighted average of estimators that use each instrument individually.

Multiple instruments and multiple endogenous variables

Stage 1: get predicted values *for each endogenous variable*

$$\widehat{X}_{1i} = \widehat{\gamma}_1 + \widehat{\phi}_{11}Z_{1i} + \widehat{\phi}_{12}Z_{2i} + \widehat{\alpha}_{11}X_{3i}$$

$$\widehat{X}_{2i} = \widehat{\gamma}_2 + \widehat{\phi}_{21}Z_{1i} + \widehat{\phi}_{22}Z_{2i} + \widehat{\alpha}_{21}X_{3i}$$

Stage 2: use predicted values in the structural equation

$$Y_i = \beta_0 + \beta_1\widehat{X}_{1i} + \beta_2\widehat{X}_{2i} + \alpha X_{3i} + u_i$$

Here X_{1i} and X_{2i} are endogenous explanatory variables. X_{3i} is exogenous.

Two stage least squares: family size example

TABLE 3.4
Quantity-quality first stages

	Twins instruments		Same-sex instruments		Twins and same-sex instruments
	(1)	(2)	(3)	(4)	(5)
Second-born twins	.320 (.052)	.437 (.050)			.449 (.050)
Same-sex sibships			.079 (.012)	.073 (.010)	.076 (.010)
Male		-.018 (.010)		-.020 (.010)	-.020 (.010)
Controls	No	Yes	No	Yes	Yes

Notes: This table reports coefficients from a regression of the number of children on instruments and covariates. The sample size is 89,445. Standard errors are reported in parentheses.

Two stage least squares: family size example

TABLE 3.5
OLS and 2SLS estimates of the quantity-quality trade-off

Dependent variable	OLS estimates (1)	2SLS estimates		
		Twins instruments (2)	Same-sex instruments (3)	Twins and same-sex instruments (4)
Years of schooling	-.145 (.005)	.174 (.166)	.318 (.210)	.237 (.128)
High school graduate	-.029 (.001)	.030 (.028)	.001 (.033)	.017 (.021)
Some college (for age ≥ 24)	-.023 (.001)	.017 (.052)	.078 (.054)	.048 (.037)
College graduate (for age ≥ 24)	-.015 (.001)	-.021 (.045)	.125 (.053)	.052 (.032)

Notes: This table reports OLS and 2SLS estimates of the effect of family size on schooling. OLS estimates appear in column (1). Columns (2), (3), and (4) show 2SLS estimates constructed using the instruments indicated in column headings. Sample sizes are 89,445 for rows (1) and (2); 50,561 for row (3); and 50,535 for row (4). Standard errors are reported in parentheses.

Two stage least squares: family size example

Some things to note:

- Change in sign from OLS to IV estimates
- Lack of precision in IV estimates
- Some improvement in precision when multiple instruments are used

Stata example 1

Implementing 2SLS in Stata:

```
ivregress 2sls y1 x1 x2 x3 (y2 = z1), first
```

- 2sls is the estimation method (ivregress has others)
- y1 is the dependent variable
- x1-x3 are the exogenous explanatory variables
- y2 is the endogenous explanatory variable
- z1 is the excluded instrument
- This example has only one endogenous explanatory variable and one excluded instrument
- first option displays first-stage results

See in-class example 1 using *Card.dta*

Some notes on terminology

- A *structural equation* typically has endogenous explanatory variables.
- *Reduced form equations* only have exogenous explanatory variables on the RHS. We referred to the equation with Y on the LHS and Z on the RHS as the “reduced form,” but the first-stage equation is technically also “reduced form.”
- The *Wald estimator* is the name given to estimation with one endogenous variable and one excluded instrument, in which the reduced form slope coefficient is divided by the first stage slope coefficient.

Some notes on terminology

An IV model is:

- *just identified* if the number of excluded instruments in the vector Z_i equals the number of endogenous explanatory variables.
- *under-identified* if the number of excluded instruments in the vector Z_i is less than the number of endogenous explanatory variables.
- *over-identified* if the number of excluded instruments in the vector Z_i is more than the number of endogenous explanatory variables.

If the model is under-identified, no consistent IV estimator exists. In other words, you need at least as many excluded instruments as endogenous explanatory variables.

Testing IV assumptions

“Anyone using an IV estimator should conduct and report tests of the following:” (Nichols, 2007)

- Instrument validity (*overid* tests)
- Tests for endogeneity
- Identification
- Presence of weak instruments
- Misspecification of functional form

Over-identification test

It is impossible to test the identification assumption $\text{Cov}(Z, u) = 0$ in the just-identified case. But one can test for the validity of instruments in the over-identified case.

- After `ivregress` can use `estat overid` command
- If this follows 2SLS, the Sargan (1958) chi-squared test statistic is reported (df = number of over-identified restrictions)
- H_0 : all instruments are valid
- Rejection can be interpreted as one or more instruments are not valid (or, model was mis-specified to begin with). Note a lack of rejection should not be interpreted as confirmation of validity.

Over-identification test

If you have instruments that are truly exogenous, then it is also true that the squares and cross-products of these instruments are exogenous. Nichols (2007) recommends an overid test in which these are added to the list of overid restrictions.

Endogeneity test

We are typically using IV because we believe an explanatory variable in the structural model is endogenous. If that variable is in fact *exogenous*, IV is *consistent* but *less efficient*. Would prefer OLS.

- After `ivregress` can use `estat endogenous` command
- If this follows 2SLS, the Durbin-Wu-Hausman test statistic is reported.
- H_0 : explanatory variable(s) is exogenous
- Rejection suggests explanatory variable(s) is endogenous and IV is appropriate.

See in-class example 1 using *Card.dta*

Identification and weak instruments

The strength of association between the excluded instrument(s) Z and the endogenous explanatory variables X is important for identification. When there are multiple endogenous variables and multiple instruments, it is important that the instruments explain variation in all of the endogenous variables, not just a subset.

- After `ivregress` can use `estat firststage` command
- The identification tests look at the *least* partial correlation, or the minimum value of the Cragg-Donald statistic
- When there is one endogenous explanatory variable, this statistic is the F statistic for significance of the instruments from the first stage. $F > 10$ is the usual rule-of-thumb for rejecting a weak instrument.

Identification and weak instruments

The `estat firststage` output includes a table with “rule-of-thumb” critical values that can be used. These values (measures of the predictive power of the excluded instruments) imply some limit of the bias to a percent of OLS. See in-class example 1 using *Card.dta*

Note: a new paper by Lee et al. (2020) has called the rule of thumb $F = 10$ into question, and suggests F should be more than 10 times this. See <https://arxiv.org/abs/2010.05058>

More on weak instruments

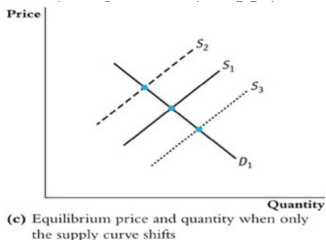
What to do in the case of weak instruments?

- Find better instruments, duh! A transformation of your existing instrument(s) may help.
- Can use LIML estimation (vs 2SLS) which has more desirable finite sample properties, especially if instruments are weak.
- Also see `condivreg` command for robust inference with weak instruments.

Applications of IV

Early applications: identifying parameters of the demand curve. Observed prices reflect simultaneous effects of supply and demand. Need instrument Z that affects price that is unrelated to other demand-side factors in u .

$$D = \alpha_0 + \alpha_1 P + u$$



Applications of IV

Angrist and Krueger (1991) - compulsory schooling

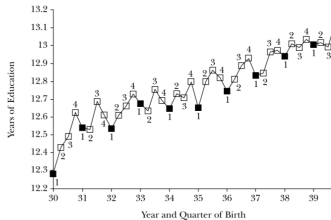
- Let X be years of schooling and Y be (log) earnings. Can we find an instrument Z that affects years of schooling but not earnings (except through years of schooling)?
- Consider a child born in Q4: starts school before turning age 6. At age 16, has completed 10+ years of school
- Consider a child born in Q1: starts school the following year. At age 16, has completed 9+ years of school

Prediction: if kids drop out at 16, those born in Q1 have less completed schooling.

Applications of IV

Angrist and Krueger (1991) - compulsory schooling

Figure 1
Mean Years of Completed Education, by Quarter of Birth

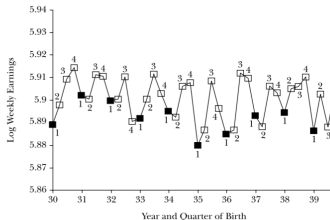


Source: Authors' calculations from the 1980 Census.

Applications of IV

Angrist and Krueger (1991) - compulsory schooling

Figure 2
Mean Log Weekly Earnings, by Quarter of Birth



Source: Authors' calculations from the 1980 Census.

Applications of IV

- Vietnam draft lottery number (Angrist & Krueger 1992)
- Proximity to colleges (Card 1993)
- Weather: rainfall, snow, temperature
- Election cycles and effect on policing
- Terrorist attacks
- Dorm room assignments
- Immigration (e.g., Mariel Boat Lift)
- Topological features (Hoxby 2000)
- “Shift-share” designs: applying aggregate shocks to baseline measures

COVID-19 as an instrument?

Instrumental variables often emerge from “natural experiments” in which an exogenous force changes behavior or exposure to “treatment” in unanticipated ways. Might COVID-19 be used as an instrument for, say, the use of online instruction or some other policy change?

Most likely not (Bacher-Hicks & Goodman, 2020).

COVID-19 as an instrument?

Consider the following regression:

$$Y_i = \beta_0 + \beta_1 \text{online}_i + u_i$$

where $\text{online}_i = 1$ if student i is receiving remote-only instruction, or their district is requiring remote-only instruction. Usually we are concerned about self-selection in remote instruction and OVB. However, COVID-19 forced many learners into remote instruction. One might even argue there was some idiosyncratic variation in this change, given variation in district decisions. Perhaps local variation in COVID severity could be used as an IV for remote instruction?

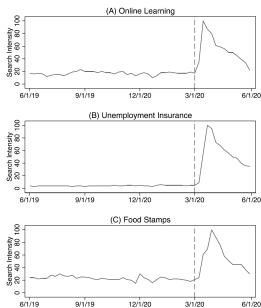
COVID-19 as an instrument?

Problems:

- Exclusion restriction: the instrument should be unrelated to other contemporaneous changes that might affect Y . Unlikely for lots of reasons! Negative effect of the pandemic, compensating behavior by parents.
- External validity: even if we could identify the causal effect of remote instruction via the pandemic, the LATE estimates are unlikely to translate to other times and places.

COVID-19 as an instrument?

Figure 2. Search Intensity for Online Learning and Economic Indicators



Notes: This figure presents the weekly popularity for the keywords “Online Learning”, “Unemployment Insurance”, and “Food Stamps” in the United States from June 1, 2019 through June 1, 2020. Keyword trend data come from Google Trends. Popularity is indexed relative to the week of March 29, 2020, the week in which “Online Learning” was most popular across the weeks in this sample.