Instrumental Variables

SW Chapter 12

ECON3500: Econometrics and Applications

Spring 2026

Learning Objectives

Understand **why** we need instrumental variables to address endogeneity

Identify the three key characteristics of a valid instrument

Explain the mechanics of **two-stage least squares** (2SLS)

Test for **weak instruments** using first-stage F-statistics

Combine IV with **panel data** and fixed effects

Textbook Coverage

12.1: IV estimator with single regressor and single instrument

- *We won't manually compute standard errors*

12.2: General IV regression model

12.3: Checking instrument validity

- *Weak instruments and exogeneity*
- **Exclude** *overidentifying restrictions test*

12.4/12.5: Interesting examples!

Why Instrumental Variables?

The Endogeneity Problem

Three threats to internal validity all produce **endogeneity**—the regression X_i is correlated with the error term. That is, $E(u_i|X_i) \neq 0$:

Omitted variable bias: An unobserved variable correlated with X is left out

Simultaneous causality: X causes Y, but Y also causes X

Errors-in-variables bias: X is measured with error

What We've Tried So Far

Solutions to endogeneity considered previously:

Difference-in-differences

- Requires a clear before/after treatment

Fixed effects (Ch. 10)

- Requires panel data
- Endogeneity source must be time-constant
- Regressors must not be time-constant

Today: **Instrumental variables (IV)**

Widely-used method for addressing endogeneity

IV can **eliminate bias** when $E(u_i|X_i) \neq 0$ if we have a valid **instrumental variable** Z

Wages and Schooling

$$\log(wage_i) = \beta_0 + \beta_1 schooling_i + \delta V_i + u_i$$

β_1 measures the returns to schooling

One omitted variable V : innate ability as a worker

- Innate ability positively affects *wages* ($\delta > 0$)
- Likely that innate ability is positively correlated with *schooling*: $\mathrm{corr}(schooling, V) > 0$

OLS estimator of β_1 may have **omitted variable bias**

If this is the only omitted variable, bias is **positive**

- $\hat{\beta_1}$ overestimates the financial returns to schooling

Can We Fix This with Multiple Regression?

$$\log(wage_i) = \beta_0 + \beta_1 schooling_i + \delta V_i + u_i$$

How do we measure innate ability?

IQ tests may capture *some* part of ability; hard to get IQ data for large samples

IQ is not a perfect measure of innate ability in the workplace

- Example: IQ tests wouldn't measure social skills
- Note: you *should* include IQ or equivalent if available

Since IQ tests are imperfect, *schooling* likely remains correlated with the omitted part of innate ability

Multiple regression doesn't fully solve this problem!

Can We Fix This with Panel Data?

$$\log(wage_i) = \beta_0 + \beta_1 schooling_i + \delta V_i + a_i + u_i$$

Innate ability might be reasonably constant over a career, captured by a_i

But *schooling* is also typically constant for adult workers

Adults who go back to school after working are a non-representative group

Panel data do not provide convincing variation in schooling over a worker's career

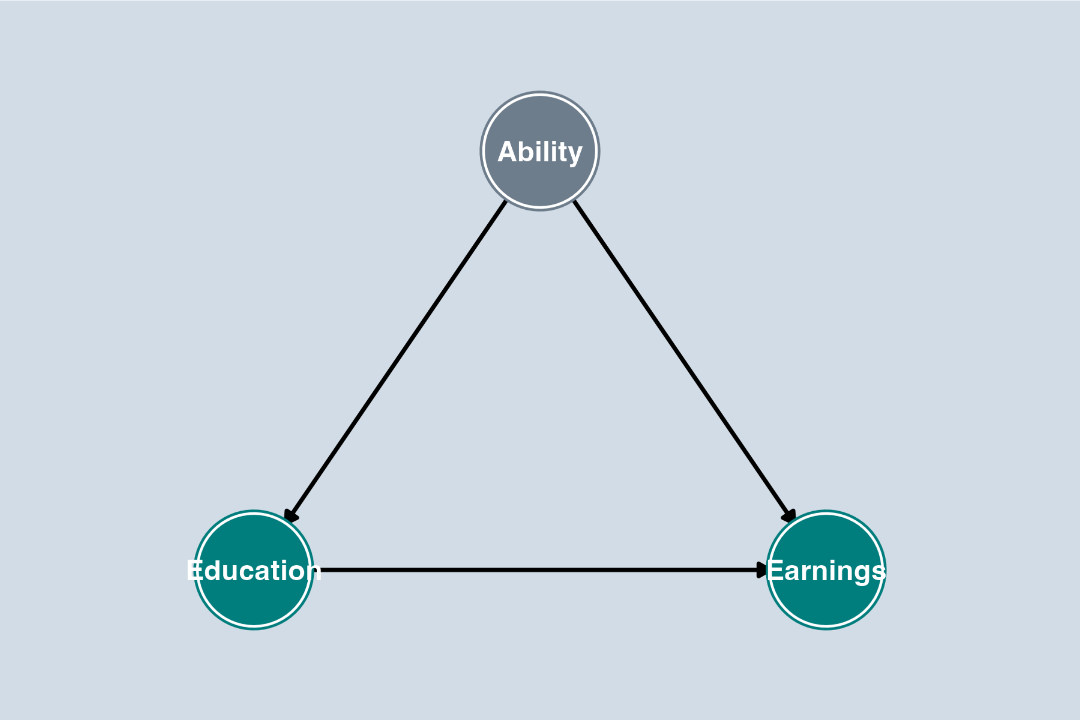
Fixed effects won't work here! (and if they do, won't capture the type of variation we care about)

> ⚠ **Bottom Line**
>
> Neither multiple regression nor panel data convincingly addresses the ability bias in estimating returns to schooling. We need a different approach.

Endogeneity as a DAG

The Problem in a DAG

To understand how IV solves this, let's visualize the problem using causal diagrams from Ch 8b.



Gray = unobserved

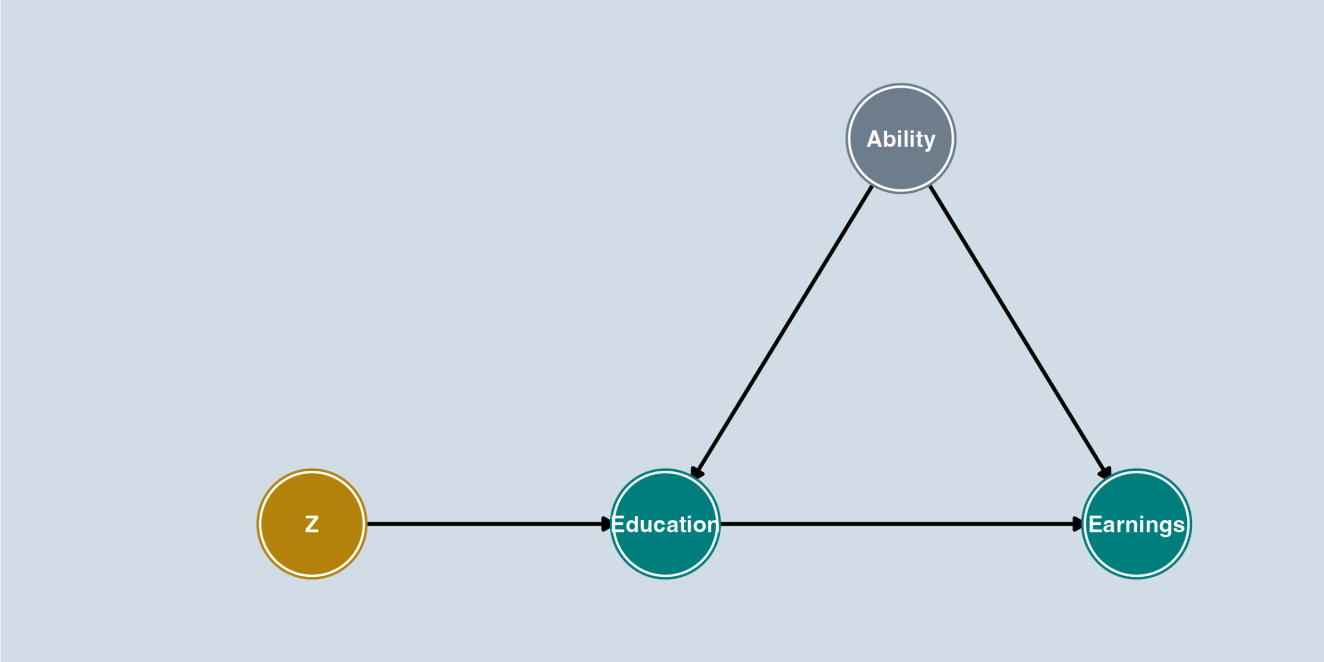
From Ch 8b, we know this DAG:

- **Causal path**: Education → Earnings ✓
- **Backdoor path**: Education ← **Ability** → Earnings ✗

The backdoor path is **open** and Ability is **unobserved**.

- Can't control for it (Ch 6/7)
- Can't difference it out (Ch 10)
- **We need a new tool to close this path.**

The IV Solution in a DAG



Gold = instrument, **Gray** = unobserved

Add an **instrument** Z to the DAG. A valid instrument must satisfy:

1. **Z $\rightarrow$ Education**: Z affects X (**relevance**)
2. **No Z $\leftarrow$ Ability**: Z is not connected to unobserved confounders (**exogeneity**)
3. **No Z $\rightarrow$ Earnings**: Z does not directly affect Y (**exclusion**)

Key Insight

IV works by isolating the variation in Education that comes **only from** Z. Since Z has no connection to Ability and no direct effect on Earnings, this variation is "clean" — free of confounding.

Cigarette Demand: A Running Example

Demand for Cigarettes

Broad public policy interest in reducing cigarette consumption.

$$sales_i = \beta_0 + \beta_1 price_i + u_i$$

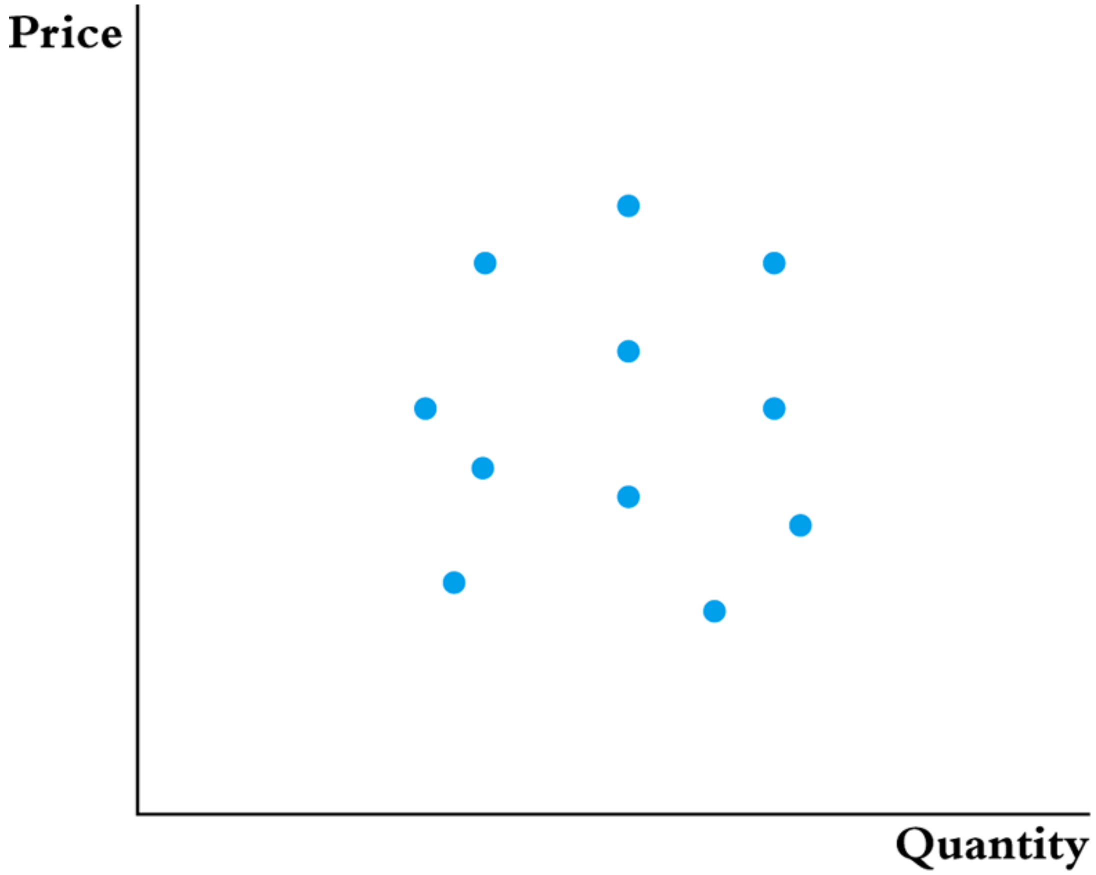
Price may be correlated with omitted variables in u: firms set prices based on demand conditions

When state i has unusually high demand (u_i high), prices are higher too

This is **simultaneous causality**: sales depend on prices, but prices also depend on sales

The identification problem: When both supply and demand shift, equilibrium data trace out **neither** curve — just a scatter of equilibrium points.

We need something that shifts supply **without** shifting demand.



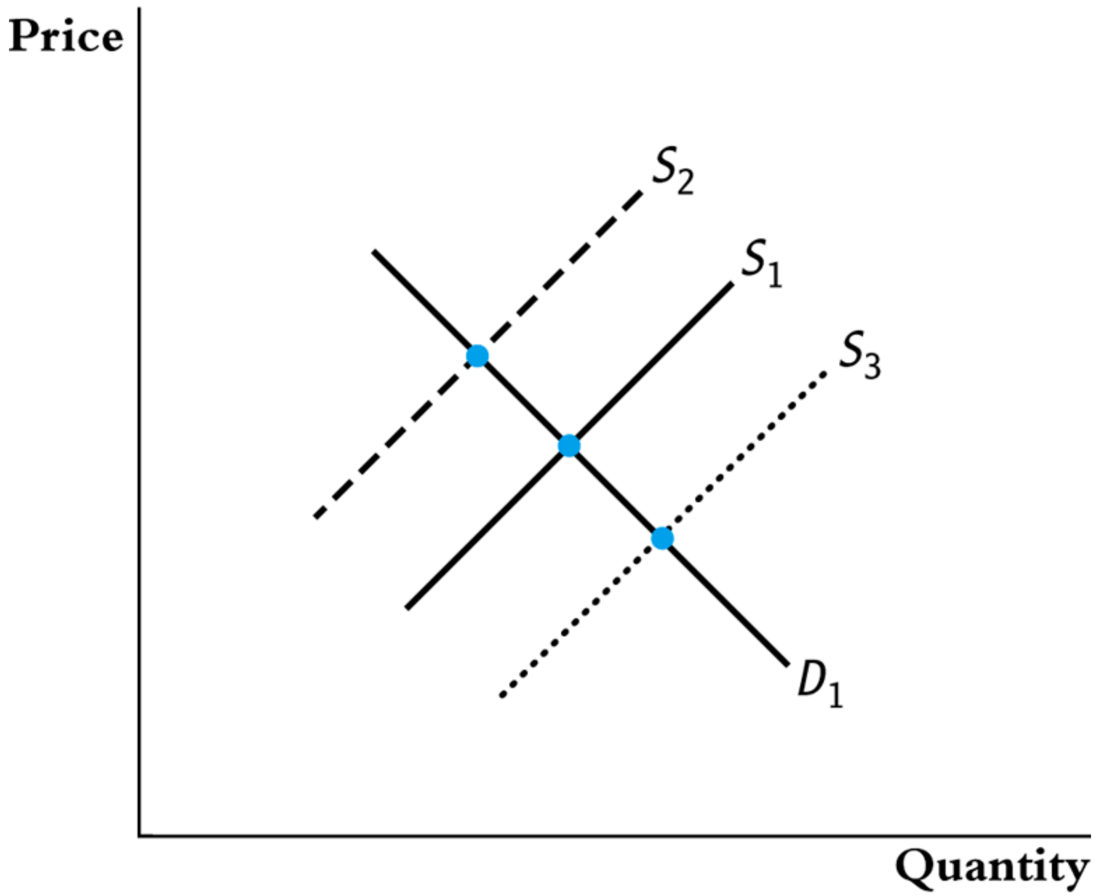
(b) Equilibrium price and quantity for 11 time periods

The IV Intuition

If we can **hold demand fixed** and only observe supply shifting:

The equilibrium points trace out the **demand curve**

The **instrument** shifts supply without directly affecting demand — it moves X without directly affecting Y



(c) Equilibrium price and quantity when only the supply curve shifts

Why Simultaneous Causality Is Especially Problematic

$$sales_i = 164.4 - 0.38 \cdot price_i + u_i$$

Simultaneous causality is especially problematic because X_i will generally be correlated with **all** omitted variables in u_i

Hard to remove OVB by measuring the omitted variables

Would need to measure *every single* omitted variable

Simultaneous causality would *disappear* if we could **randomly assign** prices to the different states

In that experiment, there would be no correlation between *price* and u

IV provides a way to mimic this

The IV Solution: Formal Definitions

Instrumental Variables: Three Assumptions

We introduced these conditions visually using DAGs. Now let's state them formally.

An **instrumental variable** is an additional variable Z_i that satisfies three assumptions:

Z_i **is correlated** with X_i

- $\text{Corr}(Z, X) \neq 0$
- Z is a **powerful** or **relevant** instrument

Z_i **is not correlated** with the omitted variable, u_i

- $\text{Corr}(Z, u) = 0$
- Z is an **exogenous** instrument

Z_i **does not** *directly affect* (cause) Y_i

- It can only affect Y_i through its effect on X_i
- Z is an **excluded** instrument
- Z_i does not enter the equation $Y_i = \beta_0 + \beta_1 X_i + u_i$

Two vs. Three Assumptions

Some textbooks combine (2) and (3) into a single **validity** or **exogeneity** condition: $\text{Corr}(Z, u) = 0$. Others (including SW) separate them: **exogeneity** (Z uncorrelated with u) and **exclusion** (Z affects Y only through X). The practical question is the same either way: *can Z affect Y through any channel other than X?*

The Wald Estimator

With a **binary instrument** $Z_i \in \{0, 1\}$, there is a simple way to see how IV works:

First stage: How does Z move X?

$$\text{First stage} = E[X_i \mid Z_i = 1] - E[X_i \mid Z_i = 0]$$

Reduced form: How does Z move Y?

$$\text{Reduced form} = E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0]$$

$$\boxed{\beta_1^{Wald} = \frac{\text{Reduced form}}{\text{First stage}}}$$

Intuition

The reduced form tells us how the instrument changes the outcome. The first stage tells us how the instrument changes treatment. Their ratio tells us the effect of treatment **induced by the instrument**.

Identification

Start with the population model:

$$Y = \beta_0 + \beta_1 X + u$$

Take the covariance of both sides with Z:

$$\text{Cov}(Y, Z) = \text{Cov}(\beta_0 + \beta_1 X + u, Z)$$

$$= \text{Cov}(\beta_0, Z) + \text{Cov}(\beta_1 X, Z) + \text{Cov}(u, Z)$$

$$= 0 + \beta_1 \text{Cov}(X, Z) + 0 \quad \text{by } \text{Cov}(u, Z) = 0$$

Solving for β_1:

$$\boxed{\beta_1 = \frac{\text{Cov}(Y, Z)}{\text{Cov}(X, Z)}}$$

Which Assumptions Did We Use?

$$\beta_1 = \frac{\text{Cov}(Y, Z)}{\text{Cov}(X, Z)}$$

$\text{Cov}(Z_i, u_i) = 0$ — explicitly used in the derivation

$\text{Cov}(X_i, Z_i) \neq 0$ — used to divide by $\text{Cov}(X, Z)$; can't divide by zero!

Z_i does not affect Y_i directly — used to write the population model as $Y_i = \beta_0 + \beta_1 X_i + u_i$

Key Feature of IV

Notice we **never** assumed $\text{Cov}(X_i, u_i) = 0$. IV explicitly allows for endogeneity — that's the whole point!

Intuition for the IV Formula

$$\beta_1 = \frac{\mathrm{Cov}(Y, Z)}{\mathrm{Cov}(X, Z)}$$

Goal: estimate β_1, how X affects Y

Problem: We think X is correlated with u

Solution: Don't compare Y (which contains u) and X directly

- $\mathrm{Cov}(X, Y)$ is explicitly *not* in our formula

Instead, see how Y moves with a third variable Z, and how X moves with Z

Z is exogenous: uncorrelated with u; Z also does not affect Y directly

If Y and X are both correlated with Z, the only explanation under our assumptions is that X causes Y according to β_1

Applying the Formula: Distance to College

Back to our wages/schooling example. Instrument: distance from childhood home to nearest college.

$$\beta_1 = \frac{\mathrm{Cov}(\log \mathrm{wage}, \mathrm{distance})}{\mathrm{Cov}(\mathrm{schooling}, \mathrm{distance})}$$

Denominator is positive (closer to college → more schooling)

Numerator is positive if people near a college earn higher wages as adults

Note: *not* because the distance causes the higher wage

Key Feature

$\mathrm{Cov}(X, Y)$ does not appear in the formula — we never directly compare someone's wage to their schooling. Instead, we use **only the variation in schooling driven by distance**.

Two-Stage Least Squares

The 2SLS Estimator

For a dataset with n observations, replace population covariances with sample covariances:

$$\hat{\beta}_1^{2SLS} = \frac{\widehat{\text{Cov}}(Y, Z)}{\widehat{\text{Cov}}(X, Z)} = \frac{\frac{1}{n}\sum_{i=1}^{n}(Y_i - \bar{Y})(Z_i - \bar{Z})}{\frac{1}{n}\sum_{i=1}^{n}(X_i - \bar{X})(Z_i - \bar{Z})}$$

$$\hat{\beta}_0^{2SLS} = \bar{Y} - \hat{\beta}_1^{2SLS}\bar{X}$$

Called **two-stage least squares** — why will become apparent shortly.

What Are the Two Stages?

Stage 1: Regress X on Z (the “first stage”)

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

Form the predicted values:

$$\hat{X_i} = \hat{\pi_0} + \hat{\pi_1} Z_i$$

Note: $X_i = \hat{X_i} + \hat{v_i}$

Stage 2: Regress Y on $\hat{X}$ (the “second stage”)

$$Y_i = \beta_0 + \beta_1 \hat{X_i} + u_i$$

Intuition: Why 2SLS Works

First stage regresses X on Z

- Forms a "best guess" of X using only data on Z

The predicted $\hat{X}$ is **not** correlated with omitted variables in the second stage

- If we predict price using sales tax, predicted prices can't be correlated with unmeasured factors that affect demand
- We assumed exogeneity: sales tax is uncorrelated with omitted variables

Second stage regresses Y on $\hat{X}$

- $\hat{X}$ is "cleansed" of any correlation with omitted variables
- No more OVB or simultaneous causality bias

Key Insight

The instrument isolates the variation in X that is exogenous. 2SLS uses only this "clean" variation to estimate β_1.

The Local Average Treatment Effect

IV uses only the variation in X driven by the instrument — that's the whole point.

But this also means we can only observe the effect among people for whom the instrument **actually affects their treatment**.

Suppose a treatment improves *your* outcome by 2, but *my* outcome by only 1

And the instrument strongly affects whether *you* get treatment, but barely affects *me*

Then the IV estimate will be much closer to **2** than to 1

> **Local Average Treatment Effect (LATE)**
>
> The IV estimate is **local** to the people whose treatment status is changed by the instrument, weighted by how much the instrument affects them. It is **not** the average effect for all units, or even for all treated units.

Better LATE Than Never?

Implication: The IV estimate may not represent the average effect for everyone — or even for those actually treated.

Compared to a randomized experiment, IV may be **less informative** about what would happen if treatment were expanded to a broader population

But IV still provides a **valid causal estimate** for the subgroup whose behavior is changed by the instrument

When exogenous variation is limited, LATE may be the best causal estimate available

> **Practical Implication**
>
> When interpreting IV results, ask: *For whom does the instrument induce treatment?* The IV estimate applies to that group — not necessarily to all units in the sample.

Cigarette Demand: IV in Practice

The Instrument: Sales Tax

$$sales_i = \beta_0 + \beta_1 \, price_i + u_i$$

Instrument for *price* of cigarettes? Need a Z_i that is:

- **Powerful**: Correlated with price
- **Exogenous**: Uncorrelated with u_i (unobserved demand factors)
- **Excluded**: Does not directly impact cigarette demand

Sales tax in state i?

- **Powerful**: Sales tax should be positively correlated with price (price measured inclusive of taxes)
- **Exogenous**: Plausible, but contestable: states with stronger anti-smoking preferences may both set higher taxes and have lower cigarette demand
- **Excluded**: Plausible, but contestable: the tax affects demand primarily through price, not through other channels

2SLS in Stata

Stata's `ivregress` command handles both stages automatically:

```
ivregress 2sls packpc (avgprs=tax), robust
```

`ivregress 2sls Y (X=Z), robust`

Y = dependent variable (first after `2sls`)

X = endogenous regressor (in parentheses before `=`)

Z = excluded instrument (after `=`)

`robust` = heteroskedasticity-robust standard errors

Stata: 2SLS Results

```
. ivregress 2sls packpc (avgprs=tax), robust

Instrumental variables (2SLS) regression       Number of obs     =         96
                                                Wald chi2(1)      =      88.46
                                                Prob > chi2       =     0.0000
                                                R-squared         =     0.4219
                                                Root MSE          =     19.567

------------------------------------------------------------------------------
             |               Robust
      packpc | Coefficient  std. err.      z    P>|z|     [95% conf. interval]
-------------+----------------------------------------------------------------
      avgprs |  -.4208748   .0447474    -9.41   0.000    -.5085781   -.3331715
       _cons |    169.556   7.516482    22.56   0.000      154.824    184.2881
------------------------------------------------------------------------------
Instrumented:  avgprs
Instruments:   tax
```

2SLS estimates that a 1-unit increase in price **leads to** a decrease of 0.42 packs per capita.

Viewing Both Stages with `first`

`ivregress 2sls packpc (avgprs=tax), robust first`

The `first` option displays the first-stage regression alongside the second stage.

```
. ivregress 2sls packpc (avgprs=tax), robust first

First-stage regressions
-----------------------

                                                Number of obs     =         96
                                                F(   1,      94)  =     391.06
                                                Prob > F          =     0.0000
                                                R-squared         =     0.8089
                                                Adj R-squared     =     0.8068
                                                Root MSE          =    19.2886

------------------------------------------------------------------------------
             |               Robust
      avgprs |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
         tax |   2.445839   .1236823    19.78   0.000     2.200265    2.691413
       _cons |   39.04966   5.940047     6.57   0.000     27.25556    50.84376
------------------------------------------------------------------------------


Instrumental variables (2SLS) regression          Number of obs     =         96
                                                  Wald chi2(1)      =      88.46
                                                  Prob > chi2       =     0.0000
                                                  R-squared         =     0.4219
                                                  Root MSE          =     19.567

------------------------------------------------------------------------------
             |               Robust
      packpc |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
      avgprs |  -.4208748   .0447474    -9.41   0.000    -.5085781   -.3331715
       _cons |    169.556   7.516482    22.56   0.000      154.824    184.2881
------------------------------------------------------------------------------
Instrumented:  avgprs
Instruments:   tax
```

Reporting the First Stage

First stage shows how X and Z are related

It provides a **statistical test** of the relevance assumption: $\text{Corr}(X, Z) \neq 0$

Rule of Thumb: F > 10

A common rule of thumb is that the first-stage F-statistic should be **greater than 10**. If so, instruments are likely **powerful** (relevant). This threshold is a guide, not a bright line.

Checking Instrument Validity

Weak Instruments

What if the first-stage F-test is less than 10?

May have a **weak instrument**

Sample covariance of X and Z may be close to 0

$$\hat{\beta}_1^{2SLS} = \frac{\widehat{\text{Cov}}(Y, Z)}{\widehat{\text{Cov}}(X, Z)}$$

Intuition: dividing by something close to zero **blows up** your estimate

Large standard errors and unreliable point estimates

⚠ **Consequences of Weak Instruments**

If the first-stage F-statistic is less than 10, the 2SLS estimates may be biased and confidence intervals may have incorrect coverage. Do not trust the results.

Which Assumptions Can Be Tested?

Assumption	Testable?	How?
Relevance ($\text{Corr}(Z, X) \neq 0$)	Yes	First-stage F-test (rule of thumb: > 10)
Exogeneity ($\text{Corr}(Z, u) = 0$)	No	Must argue based on institutional knowledge
Exclusion (Z doesn't directly cause Y)	No	Must argue based on theory

Exogeneity and Exclusion Cannot Be Tested

You lack data on the omitted variable, so you cannot test whether Z is correlated with it. **Both** exogeneity *and* exclusion must be **defended with reasoning** — relevance is the only one with a formal test.

IV with Multiple Regressors

Adding Exogenous Controls

$$sales_i = \beta_0 + \beta_1 price_i + \beta_2 income_i + u_i$$

Income per person at the state level may affect cigarette sales

Income is **not** determined simultaneously with cigarette demand

- We assume income is uncorrelated with the composite error u

2SLS can handle variables **not treated as endogenous**

Income enters **both** stages:

- First stage: helps predict price
- Second stage: controls for income's direct effect on sales

```
ivregress 2sls packpc (avgprs=tax) incomepop, robust first
```

```
. ivregress 2sls packpc (avgprs=tax) incomepop, robust first

First-stage regressions
-----------------------

                                                  Number of obs   =         96
                                                  F(   2,     93) =     477.80
                                                  Prob > F        =     0.0000
                                                  R-squared       =     0.9041
                                                  Adj R-squared   =     0.9020
                                                  Root MSE        =    13.7372

------------------------------------------------------------------------------
             |               Robust
      avgprs |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   incomepop |   4.128513   .4434336     9.31   0.000     3.247941    5.009084
         tax |   1.467367   .1281227    11.45   0.000     1.212941    1.721793
       _cons |   5.821362   4.996161     1.17   0.247    -4.100024    15.74275
------------------------------------------------------------------------------


Instrumental variables (2SLS) regression               Number of obs =      96
                                                       Wald chi2(2)  =   68.04
                                                       Prob > chi2   =  0.0000
                                                       R-squared     =  0.4600
                                                       Root MSE      =  18.913

------------------------------------------------------------------------------
             |               Robust
```

Need an *Excluded* Instrument

We need at least one instrument for **each** regressor treated as endogenous in the outcome equation

Even if we have income as a regressor, we still need `tax` as the excluded instrument

Stata will give you an error message if there is no excluded instrument

> **Terminology**
>
> **Included instruments**: Exogenous regressors (like income) that appear in both stages
>
> **Excluded instruments**: Variables (like sales tax) that appear in the first stage but *not* the second stage
>
> The "excluded" instrument is what gives IV its identifying power

Panel Data, Fixed Effects, and IV

Combining IV with Panel Data

$$sales_{it} = \alpha_i + \lambda_t + \beta_1 \, price_{it} + \beta_2 income_{it} + \delta V_i + \omega_{it}$$

Instrument for *price* is *sales tax* (`taxs`) — the panel-data sales-tax variable; distinct from the per-pack excise `tax` used earlier.

Panel data with fixed effects **and** instrumental variables; data from 1985 & 1995

State fixed effects (α_i): Control for time-invariant omitted factors (e.g., a state's attitude towards smoking)

Time fixed effects (λ_t): Control for factors affecting all states in one year (e.g., national anti-smoking campaigns)

Instrument (sales tax): Addresses simultaneous causality between demand factors and price

Income is assumed exogenous

```
xtivreg packpc (avgprs = taxs) incomepop y1995, fe vce(cluster state)
```

Comparing All Specifications

VARIABLES	(1) OLS	(2) 2SLS	(3) 2SLS	(4) State FE	(5) State and ear FE	(6) 2SLS panel	(7) 2SLS log panel
avgprs	-0.385***	-0.421***	-0.687***	-0.355***	-0.416***	-0.409***	
	(0.0412)	(0.0412)	(0.119)	(0.0579)	(0.0665)	(0.0683)	
incomepop			2.816***	0.232	-1.642*	-1.681**	
			(1.002)	(0.596)	(0.833)	(0.850)	
1995.year					21.49***	21.24***	0.251
					(6.211)	(6.234)	(0.190)
lavgprs							-1.269***
							(0.197)
lincomepop							0.446
							(0.300)
Constant	164.4***	169.6***	156.6***	155.8***	187.9***	187.7***	9.509***
	(6.700)	(7.025)	(7.256)	(3.351)	(9.783)	(9.784)	(1.270)
Observations	96	96	96	96	96	96	96
R-squared	0.426	0.422	0.463	0.909	0.923		
Number of stateID				48	48	48	48

Best Elasticity Estimate

Column 7 adds logs so β_1 is an **elasticity**: when price rises 1%, sales fall **1.3%**. The CI of $(-1.53, -1.00)$ lies entirely below -1, so we can reject that demand is inelastic.

What Changes Across Specifications?

Adding state + year FEs and an IV for price (sales tax) moves the coefficient from OLS's biased estimate toward a credible causal effect.

Applied Example: Returns to Schooling Revisited

A Different Instrument for the Same Problem

We've been using wages/schooling throughout. We used *distance to college* as one instrument. Here's another famous approach to the same question:

Instrument: Quarter of Birth (Angrist and Krueger, 1991)

Many states do not let you drop out of school until age 16 (some places 17)

High school students turn 16 at different times during the year

Children born **earlier** in the year can drop out **earlier**

So, children born earlier in the year get **less** total schooling

Check the Three Assumptions

1. **Relevant?** Yes — quarter of birth is correlated with schooling attainment through compulsory schooling laws
2. **Exogenous?** Debatable — is quarter of birth correlated with innate ability or other wage determinants? (Bound, Jaeger, and Baker, 1995)
3. **Excludable?** Debatable — does birth quarter directly affect wages, e.g., through age-at-hire effects?

This instrument has been extensively debated — it illustrates how the assumptions can be **challenged** even when the research design seems clever.

AK (1991): First Stage

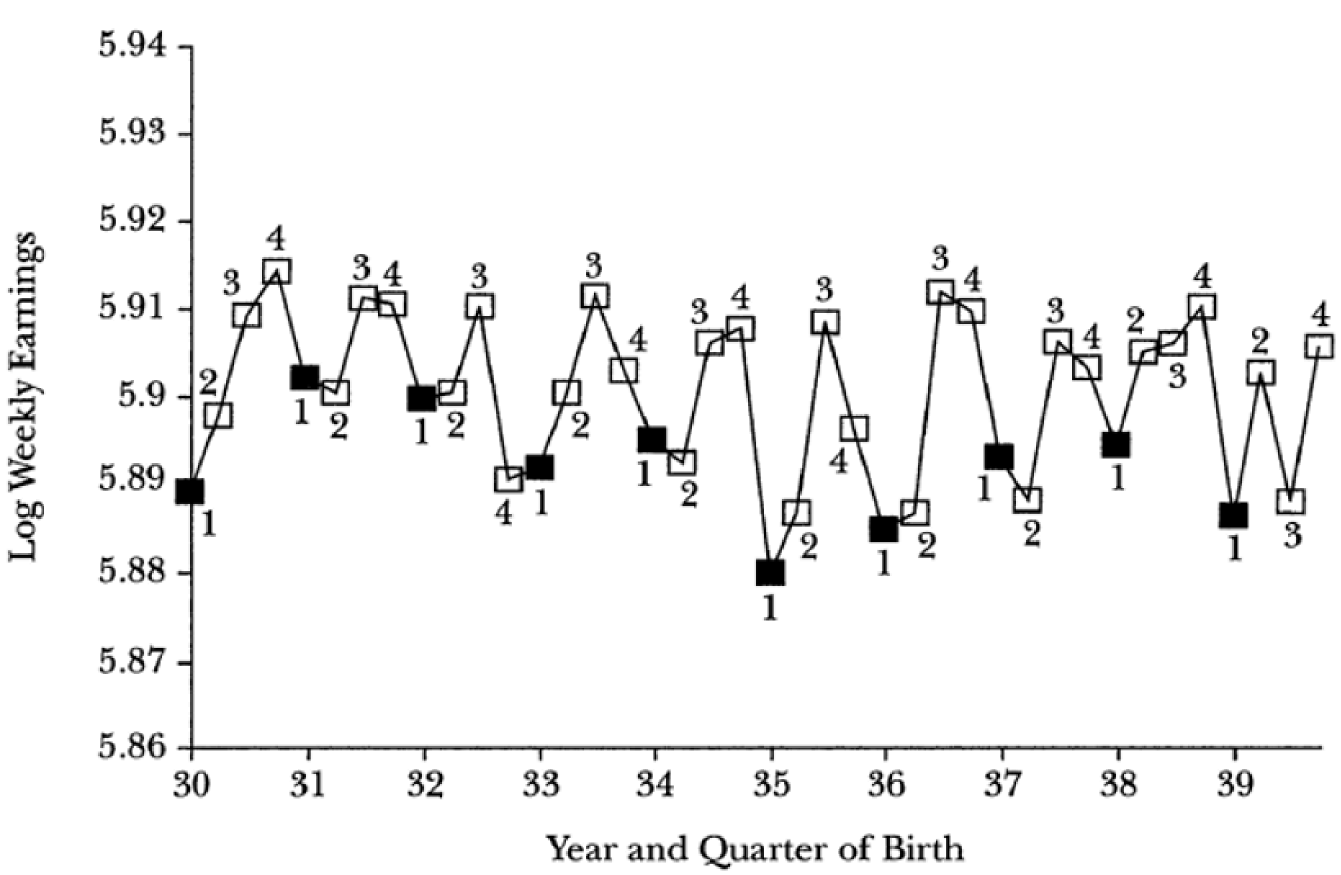
Does quarter of birth actually predict education?

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

Born in Q1 → significantly **less** education. F-tests are strong for total years.

AK (1991): The Earnings Pattern

Figure 2
Mean Log Weekly Earnings, by Quarter of Birth



Source: Authors' calculations from the 1980 Census.

Mean log weekly earnings by quarter of birth (1980 Census). The sawtooth pattern mirrors the education pattern — Q1 births earn less.

AK (1991): OLS vs. TSLS Estimates

TABLE IV

OLS AND TSLS ESTIMATES OF THE RETURN TO EDUCATION FOR MEN BORN 1920–1929: 1970 CENSUS[a]

Independent variable	(1) OLS	(2) TSLS	(3) OLS	(4) TSLS	(5) OLS	(6) TSLS	(7) OLS	(8) TSLS
Years of education	0.0802	0.0769	0.0802	0.1310	0.0701	0.0669	0.0701	0.1007
	(0.0004)	(0.0150)	(0.0004)	(0.0334)	(0.0004)	(0.0151)	(0.0004)	(0.0334)
Race (1 = black)	—	—	—	—	0.2980	−0.3055	−0.2980	−0.2271
					(0.0043)	(0.0353)	(0.0043)	(0.0776)
SMSA (1 = center city)	—	—	—	—	0.1343	0.1362	0.1343	0.1163
					(0.0026)	(0.0092)	(0.0026)	(0.0198)
Married (1 = married)	—	—	—	—	0.2928	0.2941	0.2928	0.2804
					(0.0037)	(0.0072)	(0.0037)	(0.0141)
9 Year-of-birth dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
8 Region of residence dummies	No	No	No	No	Yes	Yes	Yes	Yes
Age	—	—	0.1446	0.1409	—	—	0.1162	0.1170
			(0.0676)	(0.0704)			(0.0652)	(0.0662)
Age-squared	—	—	−0.0015	−0.0014	—	—	−0.0013	−0.0012
			(0.0007)	(0.0008)			(0.0007)	(0.0007)
χ^2 [dof]	—	36.0 [29]	—	25.6 [27]	—	34.2 [29]	—	28.8 [27]

a. Standard errors are in parentheses. Sample size is 247,199. Instruments are a full set of quarter-of-birth times year-of-birth interactions. The sample consists of males born in the United States. The sample is drawn from the State, County, and Neighborhoods 1 percent samples of the 1970 Census (15 percent form). The dependent variable is the log of weekly earnings. Age and age-squared are measured in quarters of years. Each equation also includes an intercept.

OLS and TSLS estimates of returns to education, men born 1920–1929 (1970 Census, $n = 247{,}199$). Instruments: quarter-of-birth × year-of-birth interactions.

Puzzle: IV > OLS?

If ability bias is the main problem, we'd expect OLS to *over*estimate the return to schooling — so IV should be *smaller* than OLS. But here IV is **larger**. Two possible reasons:

Measurement error in self-reported years of education attenuates OLS toward zero; IV corrects it

LATE ≠ ATE: compliers here are marginal dropouts, who may have *higher* returns to each extra year than the average student

Also note: IV standard errors are much larger than OLS — we're using less variation, so estimates are noisier.

BJB (1995) Problem 1: Inconsistency When the First Stage is Weak

Core insight: When the first stage is weak, even a *tiny* direct effect of Z on Y biases IV *more* than OLS.

$$\frac{\text{plim}\,\hat{\beta}_{IV} - \beta}{\text{plim}\,\hat{\beta}_{OLS} - \beta} = \frac{\rho_{Z,\varepsilon}/\rho_{X,\varepsilon}}{\rho_{X,Z}}$$

Does QOB have a direct effect on wages? BJB's quantitative argument:

Kids born Q1 come from families with **0.024 lower mean log family income** (1980 Census)

Intergenerational income correlation $\approx 0.4 \Rightarrow$ predicts Q1 wage gap of $\approx 0.95\%$ from family background alone

Actual Q1-vs-rest wage gap in AK's sample: $\approx 1.1\%$

> ⚠ **BJB's conclusion**
>
> Differences in family income at birth *"would seem to account for virtually all of the association between quarter of birth and wages."*

Plus other QOB correlates (school attendance, behavioral problems, reading/writing/math, IQ) — any could open a direct $Z \to Y$ channel.

BJB (1995) Problem 2: Finite-Sample Bias

Even a *legitimate* IV is biased toward OLS in finite samples. The magnitude is approximately $1/F$ — proportional to the first-stage F-stat on the excluded instruments.

For AK's Q1 × year-of-birth specification with within-year age controls (28 excluded instruments): $F = 1.6$, partial $R^2 = 0.014\%$. Quantitatively important bias *despite* $n = 329{,}509$.

BJB's smoking gun: replaced AK's real QOB with **randomly-generated** "QOB" and re-ran the 2SLS.

	Real QOB	Random noise
Mean 2SLS coef. on educ	0.060	0.061
Mean SE	0.029	0.039
First-stage F	1.61	≈ 1
OLS coef (for reference)	0.063	0.063

The lesson

Even with *purely random* "instruments," the 2SLS output looks reasonable — point estimate near OLS, plausible SEs. **You cannot detect the problem from second-stage coefficients and standard errors alone.** Only the first-stage F reveals it. Always report it.

Why Did AK Use So Many Instruments?

AK's preferred specification used up to **180 excluded instruments**: QOB (3) $\times$ year of birth (10) $\times$ state of birth (50) interactions.

The logic (reasonable): Compulsory-attendance laws vary across states (different age cutoffs, different enforcement). So QOB $\times$ state interactions capture *real institutional variation* that plain QOB misses — and should also tighten the standard errors.

Why it backfires (BJB's critique):

Going from 30 to 178 instruments barely lifts the first-stage F (4.7 $\rightarrow$ 1.9) and partial R^2 hardly moves — most of the new instruments are *weak*

Finite-sample bias scales with $K/\tau^2 \approx 1/F$ — **more weak instruments make the bias worse**, not better

So the extra interactions buy precision without buying identification

Moral

You can't instrument your way out of a weak first stage by piling on interactions. *Quality* of instrument variation matters more than *quantity* of instruments.

IV Recipe Card

IV Estimation: Step by Step

Recipe Card: Instrumental Variables

1. Identify the endogeneity problem

Why do you think $\text{Corr}(X, u) \neq 0$?

OVB, simultaneous causality, or measurement error?

2. Propose an instrument Z and argue it is valid

Powerful: $\text{Corr}(Z, X) \neq 0$ (testable)

Exogenous: $\text{Corr}(Z, u) = 0$ (not testable — must argue)

Excluded: Z does not directly cause Y (not testable — must argue)

3. Estimate via 2SLS

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ivregress 2sls Y (X=Z) controls, robust
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4. Report and check

Report first-stage F-stat (rule of thumb: > 10)

Discuss threats to exogeneity and exclusion

Compare IV estimates to OLS — if they differ, explain why

Common Pitfalls

Mistakes to avoid

Using a **weak instrument** (F < 10)

Failing to argue **exogeneity**

Forgetting to report the **first stage**

Using manual 2SLS standard errors

Treating IV as a “magic fix” for endogeneity

Good IV practice

- Provide institutional justification for the instrument
- Report the first-stage F-statistic
- Use `ivregress` (not manual two-step)
- Discuss threats to exclusion
- Compare IV to OLS and explain differences

Key Takeaways

Endogeneity ($E(u|X) \neq 0$) makes OLS biased and inconsistent

IV addresses this by finding a variable Z that:

- Affects X (powerful)
- Is uncorrelated with u (exogenous)
- Does not directly affect Y (excluded)

2SLS isolates the exogenous variation in X using Z

First-stage F > 10 is a common rule-of-thumb diagnostic for instrument strength

Exogeneity cannot be tested — it must be argued

IV can be combined with **panel data** and fixed effects

Always use `ivregress` or `xtivreg` — never manual two-step

Appendix: Additional Examples

Effect of Studying on Grades

What is the effect on grades of studying an additional hour per day?

Y = GPA

X = study time (hours per day)

Would you expect the OLS estimator of β_1 *to be unbiased? Why or why not?*

Stinebrickner and Stinebrickner (2008), "The Causal Effect of Studying on Academic Performance," *The B.E. Journal of Economic Analysis & Policy*

$n = 210$ freshmen at Berea College (Kentucky) in 2001

Y = first-semester GPA

X = average study hours per day (time use survey)

Roommates were **randomly assigned**

Z = 1 if roommate brought a video game, = 0 otherwise

Is the Video Game Instrument Valid?

Do you think Z_i (whether a roommate brought a video game) is a valid instrument?

Is the instrument **powerful**?

- If your roommate brought a video game, you might study less
- First stage: video game treatment reduces study hours by 0.67 hours/day (significant)

Is the instrument **exogenous**?

- Roommates were **randomly assigned** — so Z is uncorrelated with unobserved student ability
- Random assignment makes this a strong argument

Is the instrument **excludable**?

- Does having a roommate with a video game directly affect your GPA, other than through study time?
- Plausible: the video game mainly affects grades by reducing study hours

Stinebrickner and Stinebrickner (2008): Results

Table 2
First Stage Regressions
The effect of instruments (and other variables) on study hours

Independent Variable	estimate (std error) n=210	estimate (std error) n=176
INSTRUMENTS		
video game TREATMENT	-.668 (.252)**	-.658 (.268)**
RSTUDYHS		.028 (.013)**
REXSTUDY		.049 (.074)

Table 4
Estimates of the effect of studying on grade performance:
Ordinary Least Squares, Instrumental Variables, Fixed Effects

Independent Variable	OLS n=210 estimate (std. error)	IV instrument: video game TREATMENT n=210 estimate (std. error)	IV instruments: video game TREATMENT, RSTUDYHS, REXSTUDY n=176 estimate (std. error)
CONSTANT	.719 (.408)*	-.073 (.709)	-.062 (.638)
STUDY	.038 (.025)	.360 (.183)**	.291 (.121)**
SEX	-.132 (.084)	-.023 (.129)	-.010 (.126)

OLS coefficient on study = 0.038; IV coefficient = **0.36** (much larger). Why? **Selection bias** — students who study more tend to be **weaker** students compensating with extra effort (**negative selection**). IV corrects this by using only the variation in study time driven by the random video-game assignment.