Econometrics of Evaluation
Advanced Instrumental Variables (IV)

Jean-Noël Senne

Université Paris-Saclay

Problématiques Contemporaines du Développement
M2EIED
Université de Pau et des Pays de l'Adour
2023 - 2024

IV as an evaluation tool

- The use of **instrumental variables** to deal with endogeneity issues in econometrics is very old (Wright (1928))

→ Useful tool when **treatment is endogenous** due to (self)-selection

- **Basic idea : use an exogenous source of variation** Z in individuals' environment that affects (the probability of) treatment T, but not *directly* the outcome Y

→ **"Natural" or "Quasi" experiments**

- **Assumptions** for the identification of causal effects :

$$Y_1, Y_0 \perp Z$$
$$cov(Z, T) \neq 0$$

- However, the recent literature in the econometrics of evaluation has led to reconsider usual IV estimators...

⇒ **How does IV work ?**

⇒ **What is a (really) good instrument ?**

⇒ **What causal effect do we (exactly) identify with IV ?**

Plan

Endogeneity bias in OLS estimation

- Let's consider the following equation where individuals (self-)select into treatment due to both observable and **unobservable characteristics :**

$$Y_i = T_i\beta + \epsilon_i$$

$\Rightarrow$ **Selection** $\Leftrightarrow$ T **is endogenous :** $cov(\epsilon, T) \neq 0$

- If T is endogenous, the OLS estimator is **biased** :

$$\beta_{OLS} = \frac{cov(Y,T)}{V(T)} = \frac{cov(T\beta+\epsilon,T)}{V(T)} = \beta + \overbrace{\frac{cov(\epsilon,T)}{V(T)}}^{Bias}$$

$\Rightarrow$ We want to **eliminate this bias** (due to the correlation between treatment and unobservables)

$\Rightarrow$ We need to find **an exogenous source** Z of variation in treatment status

Note – *Empirically, with matrix notation :* $\hat{\beta}_{OLS} = (T'T)^{-1}T'Y$.

The idea behind IV

- **Intuition :** Use (at least some) variability in treatment induced by an exogenous variable Z = **instrument**
  - When the instrument moves, the probability of treatment changes (some individuals will switch treatment status)
  - We can measure the change in outcomes associated with this change in treatment status
  - Since this change in treatment status is uncorrelated with unobservables that might also make the outcome move, we can conclude that the change in outcome is due to the change in treatment status

- The IV idea is to **model selection into treatment** in a first stage :

$$T_i = Z_i\pi + \nu_i$$

→ **Issue :** ν_i is correlated to ϵ_i (endogenous selection)

→ **First stage :** disentangle the endogenous and exogenous variations in T_i

→ **Second stage :** Use the exogenous variation in T_i to estimate the causal effect of treatment on the outcome

⇒ We need **3 assumptions** to make this work

The assumptions behind IV

The 3 assumptions of IV :

1. **Assumption 1 (relevance) :** Z is correlated to treatment T

$$cov(T, Z) \neq 0$$

$\rightarrow$ Z affects the probability of treatment

2. **Assumption 2 (exogeneity) :** Z is uncorrelated to unobservables (ν, ϵ)

$$cov(Z, \nu) = 0$$
$$cov(Z, \epsilon) = 0$$

$\rightarrow$ Z is exogenous in the treatment model (first stage)

$\rightarrow$ Z does not directly influence the outcome (second stage)

3. **Assumption 3 (exclusion) :** Z is not a deterministic function of X

$\rightarrow$ Assumption (1) and (2) may hold conditional on X (exogenous) variables

$\rightarrow$ X can be included in Z ('included' instruments)...

$\rightarrow$ ... but we need something else in Z ('excluded' instruments)

IV estimation in a simple model

- If there exists one variable Z such that :

$$\begin{array}{ll} (1) & cov(T, Z) \neq 0 \\ (2) & cov(Z, \epsilon) = cov(Z, \nu) = 0 \end{array}$$

- We can write :

$$\begin{aligned} cov(Y, Z) &= cov(T\beta + \epsilon, Z) \\ &= \underbrace{cov(T, Z)}_{\neq 0}\beta + \underbrace{cov(\epsilon, Z)}_{=0} \end{aligned}$$

- Then, the **IV estimator** writes :

$$\beta_{IV} = \frac{cov(Y, Z)}{cov(T, Z)}$$

$\rightarrow$ Z is used to **identify the causal effect of** T **on** Y (eliminates the bias)

Note – *Empirically, with matrix notation :* $\hat{\beta}_{IV} = (Z'T)^{-1}Z'Y$.

IV estimation in a simple model

- Let's write this as **two-stage model :**

$$\begin{cases} Y = T\beta + \epsilon & \text{(second stage)} \\ T = Z\pi + \nu & \text{(first stage)} \end{cases}$$

- The **reduced form** writes :

$$Y = Z\underbrace{(\overbrace{\pi}^{Z \text{ on } T} \times \overbrace{\beta}^{T \text{ on } Y})}_{\theta} + (\nu\beta + \epsilon)$$

→ Z has **no direct effect** on Y ($cov(Z, \epsilon) = 0$)

→ Z has an **indirect effect** on Y ($cov(Y, Z) = \theta$) through :

- The direct effect of Z on T
- And the direct effect of T on Y

→ Z identifies **("reveals")** the causal effect of T on Y

→ To recover **("quantify")** this causal effect β, we need to divide $\theta = cov(Y, Z)$ (reduced form) by $\pi = cov(T, Z)$ (first stage)

A particular case : the Wald estimator

- Particular case when the instrumental variable Z is a **dummy (0/1)**
- The **reduced form** writes :

$$\begin{aligned} & Y & = Z\theta + u \\ \Rightarrow \quad & \theta & = E(Y|Z=1) - E(Y|Z=0) \end{aligned}$$

- The **first stage** writes :

$$\begin{aligned} & T & = Z\pi + \nu \\ \Rightarrow \quad & \pi & = E(T|Z=1) - E(T|Z=0) \end{aligned}$$

- The **IV estimator** writes :

$$\beta_{IV} = \frac{E(Y|Z=1) - E(Y|Z=0)}{E(T|Z=1) - E(T|Z=0)} = \frac{E(Y|Z=1) - E(Y|Z=0)}{P(T=1|Z=1) - P(T=1|Z=0)}$$

$\Rightarrow$ **The Wald estimator** use an exogenous change in treatment probability to recover the causal effect of treatment

2SLS estimation in a general model

- Let's write a more general **two-stage model**, with X variables and (possibly multiple) Z excluded instruments :

$$\begin{cases} Y = T\beta + X\gamma + \epsilon & \text{(second stage)} \\ T = Z\pi + X\delta + \nu & \text{(first stage)} \end{cases}$$

  - If Z is a (single) dummy, use the **Wald estimator** conditional on X :

$$\beta_{IV} = \frac{E(Y|X, Z = 1) - E(Y|X, Z = 0)}{E(T|X, Z = 1) - E(T|X, Z = 0)}$$

  - Otherwise, use **two-stage least squares (2SLS)** :
    1. Estimate the first stage model :

$$E(T|X, Z) = P(T = 1|X, Z)$$

    2. Estimate the second stage model by replacing T with :

$$\widehat{T} = \widehat{P}(T = 1|X, Z)$$

    3. Caution with standard errors ! (use standard software packages)

$$\beta_{IV} = \frac{cov(Y, \widehat{T})}{V(\widehat{T})}$$

Note – *Empirically, with matrix notation :* $\hat{\beta}_{IV} = (\widehat{T}'\widehat{T})^{-1}\widehat{T}'Y$.

Where do we find good instruments ?

A credible instrument is a variable Z that does affect the treatment T but not *directly* the outcome Y

- **Experimental design :** the most credible instruments usually come from RCTs with an **encouragement design**
  - Z is an incentive to take-up the treatment $\Rightarrow cov(T, Z) \neq 0$
  - Z is randomly allocated $\Rightarrow cov(\epsilon, Z) = 0$

$\Rightarrow$ Z is an **'ideal' (but costly) IV** by construction ! *(see Lecture 2)*

$\Rightarrow$ Can we find good instrument elsewhere ?

Where do we find good instruments ?

- **Quasi-experimental design :** instruments come from an exogenous source of variation in individuals' environment that affects the probability of treatment

  - **Natural randomness**

  - Angrist & Krueger (1991) : quarter of birth affects school duration
    $\rightarrow$ Used to estimate the impact of education on wages
  - Chort & Senne (2018) : local rainfall variation affects migration decisions
    $\rightarrow$ Used to estimate the impact of migration on income at destination
  - Angrist & Evans (1998) : Having same-sex children increases the number of children
    $\rightarrow$ Used to estimate the impact of fertility on women labor supply

  - **Institutional rules or laws**

  - Angrist (1990) : Viet Nam veterans designated based on birth day
    $\rightarrow$ Used to estimate the impact of experience on wages
  - Duflo (2001) : School building program to increase education
    $\rightarrow$ Used to estimate returns to schooling

Example : Angrist & Evans (1998) using same-sex children

TABLE 5—WALD ESTIMATES OF LABOR-SUPPLY MODELS

	1980 PUMS			1990 PUMS			1980 PUMS		
		Wald estimate using as covariate:			Wald estimate using as covariate:			Wald estimate using as covariate:	
Variable	Mean difference by *Same sex*	*More than 2 children*	*Number of children*	Mean difference by *Same sex*	*More than 2 children*	*Number of children*	Mean difference by *Twins-2*	*More than 2 children*	*Number of children*
More than 2 children	0.0600 (0.0016)	—	—	0.0628 (0.0016)	—	—	0.6031 (0.0084)	—	—
Number of children	0.0765 (0.0026)	—	—	0.0836 (0.0025)	—	—	0.8094 (0.0139)	—	—
Worked for pay	−0.0080 (0.0016)	−0.133 (0.026)	−0.104 (0.021)	−0.0053 (0.0015)	−0.084 (0.024)	−0.063 (0.018)	−0.0459 (0.0086)	−0.076 (0.014)	−0.057 (0.011)
Weeks worked	−0.3826 (0.0709)	−6.38 (1.17)	−5.00 (0.92)	−0.3233 (0.0743)	−5.15 (1.17)	−3.87 (0.88)	−1.982 (0.386)	−3.28 (0.63)	−2.45 (0.47)
Hours/week	−0.3110 (0.0602)	−5.18 (1.00)	−4.07 (0.78)	−0.2363 (0.0620)	−3.76 (0.98)	−2.83 (0.73)	−1.979 (0.327)	−3.28 (0.54)	−2.44 (0.40)
Labor income	−132.5 (34.4)	−2208.8 (569.2)	−1732.4 (446.3)	−119.4 (42.4)	−1901.4 (670.3)	−1428.0 (502.6)	−570.8 (186.9)	−946.4 (308.6)	−705.2 (229.8)
ln(*Family income*)	−0.0018 (0.0041)	−0.029 (0.068)	−0.023 (0.054)	−0.0085 (0.0047)	−0.136 (0.074)	−0.102 (0.056)	−0.0341 (0.0223)	−0.057 (0.037)	−0.042 (0.027)

Example : Angrist & Evans (1998) using same-sex children

TABLE 5—WALD ESTIMATES OF LABOR-SUPPLY MODELS

	1980 PUMS			1990 PUMS			1980 PUMS		
		Wald estimate using as covariate:			Wald estimate using as covariate:			Wald estimate using as covariate:	
Variable	Mean difference by *Same sex*	*More than 2 children*	*Number of children*	Mean difference by *Same sex*	*More than 2 children*	*Number of children*	Mean difference by *Twins-2*	*More than 2 children*	*Number of children*
More than 2 children	0.0600 (0.0016)	—	—	0.0628 (0.0016)	—	—	0.6031 (0.0084)	—	—
Number of children	0.0765 (0.0026)	—	—	0.0836 (0.0025)	—	—	0.8094 (0.0139)	—	—
Worked for pay	−0.0080 (0.0016)	−0.133 (0.026)	−0.104 (0.021)	−0.0053 (0.0015)	−0.084 (0.024)	−0.063 (0.018)	−0.0459 (0.0086)	−0.076 (0.014)	−0.057 (0.011)
Weeks worked	−0.3826 (0.0709)	−6.38 (1.17)	−5.00 (0.92)	−0.3233 (0.0743)	−5.15 (1.17)	−3.87 (0.88)	−1.982 (0.386)	−3.28 (0.63)	−2.45 (0.47)
Hours/week	−0.3110 (0.0602)	−5.18 (1.00)	−4.07 (0.78)	−0.2363 (0.0620)	−3.76 (0.98)	−2.83 (0.73)	−1.979 (0.327)	−3.28 (0.54)	−2.44 (0.40)
Labor income	−132.5 (34.4)	−2208.8 (569.2)	−1732.4 (446.3)	−119.4 (42.4)	−1901.4 (670.3)	−1428.0 (502.6)	−570.8 (186.9)	−946.4 (308.6)	−705.2 (229.8)
ln(*Family income*)	−0.0018 (0.0041)	−0.029 (0.068)	−0.023 (0.054)	−0.0085 (0.0047)	−0.136 (0.074)	−0.102 (0.056)	−0.0341 (0.0223)	−0.057 (0.037)	−0.042 (0.027)

Example : Angrist & Evans (1998) using same-sex children

TABLE 5—WALD ESTIMATES OF LABOR-SUPPLY MODELS

	1980 PUMS			1990 PUMS			1980 PUMS		
		Wald estimate using as covariate:			Wald estimate using as covariate:			Wald estimate using as covariate:	
Variable	Mean difference by *Same sex*	*More than 2 children*	*Number of children*	Mean difference by *Same sex*	*More than 2 children*	*Number of children*	Mean difference by *Twins-2*	*More than 2 children*	*Number of children*
More than 2 children	0.0600 (0.0016)	—	—	0.0628 (0.0016)	—	—	0.6031 (0.0084)	—	—
Number of children	0.0765 (0.0026)	—	—	0.0836 (0.0025)	—	—	0.8094 (0.0139)	—	—
Worked for pay	−0.0080 (0.0016)	−0.133 (0.026)	−0.104 (0.021)	−0.0053 (0.0015)	−0.084 (0.024)	−0.063 (0.018)	−0.0459 (0.0086)	−0.076 (0.014)	−0.057 (0.011)
Weeks worked	−0.3826 (0.0709)	−6.38 (1.17)	−5.00 (0.92)	−0.3233 (0.0743)	−5.15 (1.17)	−3.87 (0.88)	−1.982 (0.386)	−3.28 (0.63)	−2.45 (0.47)
Hours/week	−0.3110 (0.0602)	−5.18 (1.00)	−4.07 (0.78)	−0.2363 (0.0620)	−3.76 (0.98)	−2.83 (0.73)	−1.979 (0.327)	−3.28 (0.54)	−2.44 (0.40)
Labor income	−132.5 (34.4)	−2208.8 (569.2)	−1732.4 (446.3)	−119.4 (42.4)	−1901.4 (670.3)	−1428.0 (502.6)	−570.8 (186.9)	−946.4 (308.6)	−705.2 (229.8)
ln(*Family income*)	−0.0018 (0.0041)	−0.029 (0.068)	−0.023 (0.054)	−0.0085 (0.0047)	−0.136 (0.074)	−0.102 (0.056)	−0.0341 (0.0223)	−0.057 (0.037)	−0.042 (0.027)

Example : Angrist & Evans (1998) using same-sex children

TABLE 7—OLS AND 2SLS ESTIMATES OF LABOR-SUPPLY MODELS USING 1980 CENSUS DATA

	All women			Married women			Husbands of married women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Estimation method	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
Instrument for *More than 2 children*	—	*Same sex*	*Two boys, Two girls*	—	*Same sex*	*Two boys, Two girls*	—	*Same sex*	*Two boys, Two girls*
Dependent variable:									
Worked for pay	−0.176	−0.120	−0.113	−0.167	−0.120	−0.113	−0.008	0.004	0.001
	(0.002)	(0.025)	(0.025)	(0.002)	(0.028)	(0.028)	(0.001)	(0.009)	(0.008)
			[0.013]			[0.013]			[0.013]
Weeks worked	−8.97	−5.66	−5.37	−8.05	−5.40	−5.16	−0.82	0.59	0.45
	(0.07)	(1.11)	(1.10)	(0.09)	(1.20)	(1.20)	(0.04)	(0.60)	(0.59)
			[0.017]			[0.071]			[0.030]
Hours/week	−6.66	−4.59	−4.37	−6.02	−4.83	−4.61	0.25	0.56	0.50
	(0.06)	(0.95)	(0.94)	(0.08)	(1.02)	(1.01)	(0.05)	(0.70)	(0.69)
			[0.030]			[0.049]			[0.71]
Labor income	−3768.2	−1960.5	−1870.4	−3165.7	−1344.8	−1321.2	−1505.5	−1248.1	−1382.3
	(35.4)	(541.5)	(538.5)	(42.0)	(569.2)	(565.9)	(103.5)	(1397.8)	(1388.9)
			[0.126]			[0.703]			(0.549)
ln(*Family income*)	−0.126	−0.038	−0.045	−0.132	−0.051	−0.053	—	—	—
	(0.004)	(0.064)	(0.064)	(0.004)	(0.056)	(0.056)			
			[0.319]			[0.743]			
ln(*Non-wife income*)	—	—	—	−0.053	0.023	0.016	—	—	—
				(0.005)	(0.066)	(0.066)			
						[0.297]			

Notes: The table reports estimates of the coefficient on the *More than 2 children* variable in equations (4) and (6) in the text. Other covariates in the models are *Age, Age at first birth,* plus indicators for *Boy 1st, Boy 2nd, Black, Hispanic,* and *Other race.* The variable *Boy 2nd* is excluded from equation (6). The *p*-value for the test of overidentifying restrictions associated with equation (6) is shown in brackets. Standard errors are reported in parentheses.

So why don't we always use IV estimation ?

- If IV works, why don't we use it all the time ?
  - **Key issue : Finding an instrument...**
    - Take exogeneity (very) seriously (credible instruments often 'mimic' experiments)
    - If exogeneity is convincing, instruments do not derive from an explicit model (interpretation issues) and may be anecdotal (poorly reproductible)
  - **Additional (serious) econometric issues : practice and interpretation of IV estimations**
    - Credible IV may happen to be **weak instruments** (i.e. explain little of the variation in treatment)
    - If treatment effect is heterogenous, IV only estimates a **local average treatment effect (LATE)**

Plan

Testing the IV assumptions

Can we test the IV assumptions ?

- **Assumption 2 (exogeneity) :** Z is uncorrelated to unobservables (ν, ϵ)
  - With 1 instrument (just-identified model), there is nothing we can do...
  - With more than 1 instrument (over-identified model), we can run an **over-identification test**, i.e test the hypothesis that additional instruments are exogenous (if other are)
    - With homoskedasticity : run a **Sargan test**
    - With heteroskedasticity : run a **Hansen J-test**

  $\rightarrow$ Yet, they only test the overall consistency of the instruments...

$\Rightarrow$ In the end, assumption 2 is **rigorously untestable**

$\Rightarrow$ Be convincing about exogeneity ! It will bring much debate...

Testing the IV assumptions

Can we test the IV assumptions ?

- **Assumption 3 (exclusion) :** Z is not a deterministic function of X
  - Usually easy to justify economically
  - If doubts, regress your Z instruments on your X variables
    - If the R^2 is close to 1, there is a problem...
    - If the R^2 is close to 0, unlikely that there is a perfectly deterministic relation between X and Z

$\Rightarrow$ Assumption 3 is quite **easily testable**

- **Assumption 1 (relevance) :** Z is correlated to treatment T
  - You need to have a close look at the **first stage**
  - This is where the **weak instruments** issues come up !

The weak instrument issues

- **What is a weak instrument ?** An instrument Z which has **limited explanatory power** over endogenous treatment T
  - $\rightarrow$ The $R^2_{T,Z}$ or the F-stat of the first-stage is low
  - $\rightarrow$ May happen if Z is really exogenous

- **Why is this an issue ?** IV estimators are **imprecise** and may be **more biased** than OLS !
  - $\rightarrow$ Remember that the IV estimator is biased in finite samples but is **asymptotically consistent** (converge to the true parameter)
  - $\rightarrow$ Usually, samples are large enough to rely on asymptotic properties...
  - $\rightarrow$ ... but with weak instruments, asymptotic properties fail even with large samples

- **3 distinct issues** with a weak instrument :
  1. IV is (much) more **imprecise** than OLS
  2. If Z **is not** strictly exogenous, $\hat{\beta}_{IV}$ is **not consistent** and can be **more biased** than $\hat{\beta}_{OLS}$
  3. If Z **is** strictly exogenous, $\hat{\beta}_{IV}$ is consistent but **biased towards OLS in finite samples**

The imprecision of IV

- Let's write the **(asymptotic) variance** of the OLS and 2SLS estimators :

$$Avar(\hat{\beta}_{OLS}) = \sigma_\epsilon^2 Var(T)^{-1}$$
$$Avar(\hat{\beta}_{2SLS}) = \sigma_\epsilon^2 Var(\hat{T})^{-1}$$

$\Rightarrow$ **Intuition :** If $\hat{T}$ is small (weak instrument), $Avar(\hat{\beta}_{IV})$ tends to $+\infty$

- Formally :

$$\begin{aligned} Avar(\hat{\beta}_{2SLS}) &= \sigma_\epsilon^2 Var(\hat{T})^{-1} = \sigma_\epsilon^2 var(\hat{\pi} Z)^{-1} = \sigma_\epsilon^2 [\hat{\pi}^2 var(Z)]^{-1} \\ &= \sigma_\epsilon^2 [\tfrac{cov(T,Z)^2}{var(Z)^2} var(Z)]^{-1} \\ &= \sigma_\epsilon^2 [\tfrac{cov(T,Z)^2}{var(Z)}]^{-1} \\ &= \sigma_\epsilon^2 [var(T) \tfrac{cov(T,Z)^2}{var(T)var(Z)}]^{-1} \\ &= \sigma_\epsilon^2 [var(T)]^{-1} [corr(T,Z)^2]^{-1} \\ &= Avar(\hat{\beta}_{OLS}) \times [R^2_{T,Z}]^{-1} \end{aligned}$$

$\Rightarrow$ The 2SLS variance is **always larger** than the OLS variance

$\Rightarrow$ When the instrument Z is **weak (i.e low $R^2_{T,Z}$)**, the variance of 2SLS can be **(very) much larger** than the variance of OLS

Note – *Publication bias ! IV estimations need larger point estimates to be significant (Ashenfelter, Harmon & Oosterbeek (1999)).*

The inconsistency of IV

- Let's write the **(asymptotic) bias** of the OLS and 2SLS estimators :

$$plim(\hat{\beta}_{OLS}) = \beta + \frac{cov(\epsilon, T)}{V(T)}$$

$$plim(\hat{\beta}_{2SLS}) = \beta + \frac{cov(\epsilon, \hat{T})}{V(\hat{T})}$$

$\Rightarrow$ If Z **is not strictly** exogenous ($cov(\epsilon, \hat{T}) \neq 0$) , $\hat{\beta}_{2SLS}$ is **not consistent**

- Moreover, the **relative bias** writes :

$$\frac{plim(\hat{\beta}_{2SLS}) - \beta}{plim(\hat{\beta}_{OLS}) - \beta} = \frac{cov(\epsilon, \hat{T})/cov(\epsilon, T)}{V(\hat{T})/V(T)} = \frac{cov(\epsilon, \hat{T})/cov(\epsilon, T)}{R^2_{T,Z}}$$

$\Rightarrow$ Even if exogeneity is only **slightly questionable** ($cov(\epsilon, \hat{T}) << cov(\epsilon, T)$), $\hat{\beta}_{2SLS}$ can be **less consistent (more biased)** than $\hat{\beta}_{OLS}$ if the instrument Z is **weak (i.e low $R^2_{T,Z}$)**

The bias of IV

- If Z **is strictly** exogenous, the 2SLS estimator is **consistent...**

$$plim(\hat{\beta}_{2SLS}) = \beta$$

- ... but still **biased in finite samples** :

$$Bias(\hat{\beta}_{2SLS}) \cong \frac{cov(\epsilon, \nu)}{V(\nu)} \frac{(1 - R^2_{T,Z})}{R^2_{T,Z}} \frac{(k-2)}{(n-k)} \cong \frac{cov(\epsilon, \nu)}{V(\nu)} \frac{1}{F}$$

where k is the number of 'excluded' instruments Z and n the sample size

$\Rightarrow$ If the instrument Z is **weak (i.e low $R^2_{T,Z}$)**, even large samples cannot impede **large bias**

$\Rightarrow$ **Several weak instruments** make it even worse (increase k but not $R^2_{T,Z}$)

- The bias of 2SLS is inversely proportionnal to the **F-stat for the test of joint significance of 'excluded' instruments** ($\pi = 0$)

$\Rightarrow$ The Staiger & Stock (1997) rule of thumb :

$F > 10$ **to rule out weak instruments**

Note – *Influential paper by Stock & Yogo (2002).*

Plan

The four instrumental populations

What causal effect do we identify with IV ?

- Any individual may respond differently to the instrument Z
- Let's define an individual **response function** $T_i(Z_i)$ that assigns the value of the treatment T_i $(0/1)$ to the value of the instrument Z_i $(0/1)$
- We can the define **4 (instrumental) populations :**

Compliers :	$T_i(0) = 0$	$T_i(1) = 1$
Never-takers :	$T_i(0) = 0$	$T_i(1) = 0$
Always-takers :	$T_i(0) = 1$	$T_i(1) = 1$
Defiers :	$T_i(0) = 1$	$T_i(1) = 0$

	$T = 1 \vert Z = 1$	$T = 0 \vert Z = 1$
$T = 1 \vert Z = 0$	Always Takers	Defiers
$T = 0 \vert Z = 0$	Compliers	Never Takers

$\Rightarrow$ Angrist, Imbens & Rubin (1996) have shown that IV only estimate **the effect of the treatment on the compliers** (those who change their treatment status in reaction to the instrument)

$\Rightarrow$ IV estimate a **Local Average Treatment Effect (LATE)**

Note – *Can be generalized to continuous treatments and instruments.*

Two fundamental assumptions

1. **Assumption 1 : Independence**

$$Z \perp Y_{i1}, Y_{i0}$$
$$Z \perp T_i(0), T_i(1)$$

$\Rightarrow$ Individuals with specific reaction to the instrument $(T(0), T(1))$ are not likely to draw a specific value of the instrument Z

$\Rightarrow$ Z is **randomly allocated** across the 4 instrumental populations

2. **Assumption 2 : Monotonicity**

$$T_i(1) \geq T_i(0) \quad \forall i$$

$\Rightarrow$ All individuals' response to the instrument goes in the same direction

$\Rightarrow$ There are **no defiers**

Example – *A mother with one boy and one girl who has a third child would also have a third child if she had two boys or two girls, i.e. same-sex children never reduce fertility.*

The reduced form

- Due to the **independence assumption**, we can write :

$$\begin{aligned} E(Y_i|Z_i=1) &= E[Y_{i0}+T_i(Y_{i1}-Y_{i0})|Z_i=1] \\ &= E[Y_{i0}+T_i(1)(Y_{i1}-Y_{i0})] \qquad (\textit{Assumption 1}) \end{aligned}$$

$$\begin{aligned} E(Y_i|Z_i=0) &= E[Y_{i0}+T_i(Y_{i1}-Y_{i0})|Z_i=0] \\ &= E[Y_{i0}+T_i(0)(Y_{i1}-Y_{i0})] \qquad (\textit{Assumption 1}) \end{aligned}$$

- Thus, the **reduced form** writes :

$$\begin{aligned} & E(Y_i|Z_i=1)-E(Y_i|Z_i=0) \\ = \;& E[(T_i(1)-T_i(0))(Y_{i1}-Y_{i0})] \\ = \;& \underbrace{E[0\times(Y_{i1}-Y_{i0})|\,T_i(1)-T_i(0)=0]P[T_i(1)-T_i(0)=0]}_{\text{never/always-takers}} \\ + \;& \underbrace{E[-1\times(Y_{i1}-Y_{i0})|\,T_i(1)-T_i(0)=-1]P[T_i(1)-T_i(0)=-1]}_{\text{defiers}} \\ + \;& \underbrace{E[1\times(Y_{i1}-Y_{i0})|\,T_i(1)-T_i(0)=1]P[T_i(1)-T_i(0)=1]}_{\text{compliers}} \end{aligned}$$

The first stage

- Due to the **monotonicity assumption (no defiers)**, the reduced form equals :

$$\begin{aligned} & E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0) \\ = \quad & E[(Y_{i1} - Y_{i0})|T_i(1) - T_i(0) = 1]P[T_i(1) - T_i(0) = 1] \end{aligned}$$

$\Rightarrow$ **Only the compliers** contributes to the estimation !

- The **first stage** writes :

$$\begin{aligned} & P(T_i = 1|Z_i = 1) - P(T_i = 1|Z_i = 0) \\ = \quad & E(T_i|Z_i = 1) - E(T_i|Z_i = 0) \\ = \quad & E(T_i(1)|Z_i = 1) - E(T_i(0)|Z_i = 0) \\ = \quad & E(T_i(1) - T_i(0)) \qquad (\textit{Assumption 1}) \\ = \quad & P(T_i(1) - T_i(0) = 1) \end{aligned}$$

$\Rightarrow$ The first stage estimates **the share of compliers !**

IV is a LATE

- Under the independence and monotonicity assumptions, **the IV (Wald) estimator** writes :

$$\hat{\beta}_{IV} = \frac{E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0)}{P(T_i = 1|Z_i = 1) - P(T_i = 1|Z_i = 0)}$$

$$\Leftrightarrow \hat{\beta}_{IV} = E[(Y_{i1} - Y_{i0})|T_i(1) - T_i(0) = 1]$$

$\Rightarrow$ Treatment effect is estimated **on the compliers only**

$\Rightarrow$ This IV estimator identifies a **Local Average Treatment Effect (LATE)** (This is the intention-to-treat effect (ITT) divided ("rescaled") by the share of compliers, i.e. individuals who are indeed sensitive to the instrument)

- **Is this an issue ?** Yes, if there is **essential heterogeneity** in treatment effects :
  1. Treatment effects are heterogenous
  2. This heterogeneity is related to the treatment response to the instrument

$$E(Y_{i1} - Y_{i0}|Compliers) \neq E(Y_{i1} - Y_{i0})$$

$$LATE \neq ATE$$

$\Rightarrow$ **Can we characterize the compliers ?**

Who are the compliers ?

- Unfortunately, we only observe the treatment response $T_i(Z_i)$ for a given value of the instrument (same missing data issue as with potential outcomes)

$\Rightarrow$ **We can never know who is a complier...**

	$Z = 0$	$Z = 1$
$T = 0$	Never-Takers / Compliers	Never-Takers (/ Defiers)
$T = 1$	Always-Takers (/ Defiers)	Compliers / Always-Takers

- However, if the monotonicity and independence assumptions hold, we can identify some **always-takers and never-takers** (and their shares in the population)
  - $P(T_i = 1|Z_i = 0) = \pi_a$ (share of always-takers in the population)
  - $P(T_i = 0|Z_i = 1) = \pi_n$ (share of never-takers in the population)

$\Rightarrow$ **We can then estimate the share of compliers !**

$$\pi_c = P(T_i = 1|Z_i = 1) - \underbrace{P(T_i = 1|Z_i = 0)}_{\pi_a}$$

(This is exactly what the first stage does !)

A simple example

- We want to estimate the wage impact of **college** *versus* **high school** education
- Education choices are obviously endogenous
- We use as an instrument for college education T a **randomized college scholarship** Z (among 200 students)

A simple example

Perfect compliance

Z=0 (no scholarship)	Z=1 (scholarship)
100 High school	0 High school
0 College	100 College
Average wage : 100	Average wage : 130

- With perfect compliance : $T = Z$

$\Rightarrow$ ATE = E(wage|Z=1) - E(wage|Z=0) = 30

A simple example

Imperfect compliance

Z=0 (no scholarship)	Z=1 (scholarship)
80 High school	0 High school
20 College	100 College
Average wage : **105**	Average wage : **121**

- With imperfect compliance : $T \neq Z$
- We have 20 always-takers (reasonable to assume that there is no defiers)
- T is endogenous, we use Z as an instrument

$\Rightarrow LATE = \frac{E(wage|Z=1)-E(wage|Z=0)}{E(college|Z=1)-E(college|Z=0)} = 16/0.8 = 20$

A simple example

Let's assume that we have separate information on parents' educational background (college *versus* high-school)

Z=0 (no scholarship)	Z=1 (scholarship)
HIGH SCHOOL PARENTS	
80 High school	0 High school
0 College	80 College
Average wage : 100	Average wage : 120
COLLEGE PARENTS	
0 High school	0 High school
20 College	20 College
Average wage : 125	Average wage : 125

- College parents are always-takers : the effect is not identified
- High-school parents are compliers : the effect is equal to 20

$\Rightarrow$ College parents do not contribute to the estimation !

- The +16 change in wage (reduced form) comes from high-school parents
- They represent a +80% (share of compliers) change in college participation

$\Rightarrow$ The (L)ATE ($16/0.8 = 20$) is only identified on compliers !

$\Rightarrow$ **Are compliers similar to always-takers ? LATE = ATE ?**

Implications for IV estimations

This setting has a number of implications for the interpretation of IV estimations :

- The IV estimator has **no interpretation** if treatment effects are heterogenous and there are **defiers**
- If treatment effects are heterogenous, the IV estimator only identifies a **local treatment effect** (issue of external validity, especially of the share of compliers is small)
- Different IVs can identify **different treatment effects** (i.e. different LATEs) since they estimate treatments effects on different subpopulations (of compliers)
- 2SLS estimations with multiple instruments are to be interpreted as a **weighted sum of the LATEs** (where weights depend on the share of compliers for each instrument)
- Therefore, the gap between OLS and IV estimations mix both the **bias correction** and the **change in the population** that contributes to the estimation

To sum up

Steps for implementing IV estimations :

1. **Find a good instrument**, i.e. an exogenous variable that does affect (the probability) of treatment but not directly the outcome
2. **Check the exogeneity of your instrument.** The exogeneity of the instrument is not formally testable so be serious (and convincing) !
3. **Check the relevance of your instrument.** Have a close look at your first stage and check the value of the F-stat to rule out weak instruments !
4. **Estimate the second stage using 2SLS.** Use software packages to get the correct standard errors (and adapt your estimation method if your outcome is binary or discrete).
5. **Interpret your results as a local average treatment effect (LATE).** The external validity of your results maybe discussed (especially if the share of compliers is small).

References

Angrist, J. D. (1990). Lifetime earnings and the Vietnam era draft lottery : evidence from social security administrative records. The american economic review, 313-336.

Angrist, J., & Evans, W. N. (1998). Children and their parents' labor supply : Evidence from exogenous variation in family size. The American Economic Review (88)

Angrist, J. D., Imbens, G. W., & Rubin, D. B. (1996). Identification of causal effects using instrumental variables. Journal of the American statistical Association, 91(434), 444-455. Angrist,

J. D., & Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings ?. The Quarterly Journal of Economics, 106(4), 979-1014.

Ashenfelter, O., Harmon, C., & Oosterbeek, H. (1999). A review of estimates of the schooling/earnings relationship, with tests for publication bias. Labour economics, 6(4), 453-470.

Bound, J., Jaeger, D. A., & Baker, R. M. (1995). Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variable is weak. Journal of the American statistical association, 90(430), 443-450.

Chort, I., & Senne, J. N. (2018). You'll be a migrant, my son : Accounting for migrant selection within the household. Economic Development and Cultural Change, 66(2), 217-263.

Duflo, E. (2001). Schooling and labor market consequences of school construction in Indonesia : Evidence from an unusual policy experiment. American economic review, 91(4), 795-813.

Stock, J. H., & Trebbi, F. (2003). Retrospectives : Who invented instrumental variable regression ?. Journal of Economic Perspectives, 17(3), 177-194.

Stock, J. H., & Yogo, M. (2002). Testing for weak instruments in linear IV regression.