

A Short Introduction to Instrumental Variables and discussion of Angrist et al. (1996)

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Outline

- The Econometrician's View
- The Biostatistician's View
- Angrist, Imbens, Rubin (1996)

Part I

The Econometrician's View

Regression

Assume, for $i = 1, \dots, n$, the **structural** relationship

$$Y_i = X_i\beta + u_i$$

where, after stacking,

- Y is $n \times 1$
- X_i is $1 \times p$, X is $n \times p$ (**fixed or random**)
- β is $p \times 1$
- u is $n \times 1$

with $u_1, \dots, u_n$ random samples and

- fixed case: $X_i = x_i, i = 1, \dots, n$.
- random case: $X_1, \dots, X_n$ random sample, wlog assume

$$\mathbb{E}[X_i] = 0$$

and for $i = 1, \dots, n$.

$$X_i \perp u_i$$

Regression: fixed X case

Premultiply by x^T :

$$x^T Y = x^T x \beta + x^T u$$

so

$$\mathbb{E}[x^T Y] = x^T x \beta + x^T \mathbb{E}[u_i]$$

We have $x^T \mathbb{E}[u_i] = 0$ so β solves

$$\mathbb{E}[x^T Y] = x^T x \beta$$

If $x^T x$ is non-singular,

$$\beta = (x^T x)^{-1} \mathbb{E}[x^T Y]$$

and thus the sample moment-based estimate is

$$\hat{\beta} = (x^T x)^{-1} x^T y$$

Regression: random X case

Premultiply by X_i^T ($p \times 1$):

$$X_i^T Y_i = X_i^T X_i \beta + X_i^T u_i$$

and thus

$$\mathbb{E}[X_i^T Y_i] = \mathbb{E}[X_i^T X_i] \beta + \mathbb{E}[X_i^T u_i]$$

We have

- $\mathbb{E}[X_i^T Y_i] = \text{Cov}[X_i^T, Y_i]$
- $\mathbb{E}[X_i^T X_i] = \Psi$, say ($p \times p$, presumed non-singular)
- $\mathbb{E}[X_i^T u_i] = \mathbb{E}[X_i^T] \mathbb{E}[u_i] = 0$

and hence β solves

$$\text{Cov}[X_i^T, Y_i] = \mathbb{E}[X_i^T X_i] \beta$$

that is,

$$\beta = (\mathbb{E}[X_i^T X_i])^{-1} \text{Cov}[X_i^T, Y_i]$$

Regression: random X case

Adapting this argument to the case of the simple linear regression

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

we obtain

$$\beta_1 = \frac{\text{Cov}[X_i, Y_i]}{\text{Var}[X_i]}$$

and the sample moment-based estimate

$$\hat{\beta}_1 = \frac{\frac{1}{n} \sum_{i=1}^n (x_i - \bar{x})(y_i - \bar{y})}{\frac{1}{n} \sum_{i=1}^n (x_i - \bar{x})^2}$$

Regression: random X case

Important note: if in reality

$$\mathbb{E}[X_i^\top u_i] = \text{Cov}[X_i^\top, u_i] \neq 0$$

then in reality β_* solves

$$\text{Cov}[X_i, Y_i] = \mathbb{E}[X_i^\top X_i] \beta_* + \text{Cov}[X_i^\top, u_i]$$

that is

$$\beta_* = (\mathbb{E}[X_i^\top X_i])^{-1} (\text{Cov}[X_i^\top, Y_i] - \text{Cov}[X_i^\top, u_i])$$

so

$$\beta = \beta_* + (\mathbb{E}[X_i^\top X_i])^{-1} \text{Cov}[X_i^\top, u_i]$$

and there is a **bias** if we use β .

Instrumental Variables: regression version

Suppose we have the structural relationship

$$Y_i = W_i\alpha + X_i\beta + u_i$$

where

- W_i is $d \times 1$, with $\text{Cov}[W_i, u_i] = 0$,
- X_i is $p \times 1$, with $\text{Cov}[X_{ij}, u_i] \neq 0, j = 1, \dots, p$.

Now suppose another collection of q variables, Z_i , are such that

$$\text{Cov}[X_{ij}, Z_{ik}] \neq 0 \quad \text{at least one } k \text{ for each } j \quad (1)$$

$$\text{Cov}[Z_{ik}, u_i] = 0 \quad (2)$$

Jargon:

- (1) makes Z_i a set of **instrumental variables** or **instruments** for X_i .
(2) is termed **exogeneity**

Instrumental Variables: simple linear regression

Let $p = q = 1, d = 0$:

$$Y_i = \beta_0 + \beta_1 X_i + u_{1i}$$

whilst in addition

$$X_i = \gamma_0 + \gamma_1 Z_i + u_{2i}$$

where

$$\text{Cov}[X_i, u_{1i}] \neq 0$$

but

$$\text{Cov}[Z_i, u_{1i}] = \text{Cov}[Z_i, u_{2i}] = 0$$

and $\gamma_1 \neq 0$.

Instrumental Variables: simple linear regression

Substituting from one equation into the other:

$$\begin{aligned}Y_i &= \beta_0 + \beta_1(\gamma_0 + \gamma_1 Z_i + u_{2i}) + u_{1i} \\&= \beta_0^* + \beta_1^* Z_i + u_i\end{aligned}$$

where

- $\beta_0^* = \beta_0 + \beta_1 \gamma_0$
- $\beta_1^* = \beta_1 \gamma_1$
- $u_i = u_{1i} + \beta_1 u_{2i}$.

Note that by construction

$$\text{Cov}[Z_i, u_i] = \text{Cov}[Z_i, u_{1i} + \beta_1 u_{2i}] = \mathbb{E}[Z_i(u_{1i} + \beta_1 u_{2i})] = 0$$

Instrumental Variables: simple linear regression

Thus, by previous arguments

$$\beta_1^* = \frac{\text{Cov}[Y_i, Z_i]}{\text{Var}[Z_i]}$$

But also, by assumption,

$$\gamma_1 = \frac{\text{Cov}[X_i, Z_i]}{\text{Var}[Z_i]}$$

Therefore

$$\beta_1 = \frac{\beta_1^*}{\gamma_1} = \frac{\text{Cov}[Y_i, Z_i]}{\text{Cov}[X_i, Z_i]}$$

Instrumental Variables: simple linear regression

To produce an estimate of β_1 : take the ratio of the estimate in the regression of Y on Z to the estimate of the regression of X on Z , that is, the ratio of the sample covariances

$$\hat{\beta}_1 = \frac{\hat{\beta}_1^*}{\hat{\gamma}_1} = \frac{\frac{1}{n} \sum_{i=1}^n (y_i - \bar{y})(z_i - \bar{z})}{\frac{1}{n} \sum_{i=1}^n (x_i - \bar{x})(z_i - \bar{z})}$$

Note: if γ_1 is near zero, then the method is prone to producing highly variable estimates

Jargon: Z is a **weak instrument**

Instrumental Variables: multiple linear regression

For multiple regressors/instruments: can use

two stage least-squares (2SLS).

1. Fit the regression of X on Z to get $\hat{\gamma}$:

$$\hat{\gamma} = (Z^T Z)^{-1} Z^T X$$

2. Obtain fitted-values $\hat{X}$:

$$\hat{X} = Z\hat{\gamma} = (Z^T Z)^{-1} Z^T X$$

3. Fit the regression of Y on W and $\hat{X}$:

$$\mathbb{E}[Y] = [W \ \hat{X}][\alpha \ \beta]^T$$

The resulting estimators of β are consistent under previous assumptions (1) and (2).

Part II

The Biostatistician's View

Let's Draw a DAG !!

Let

- Y be the response
- X be the exposure (treatment)
- $\mathcal{X}$ be unmeasured confounders
- Z be the instrument

such that

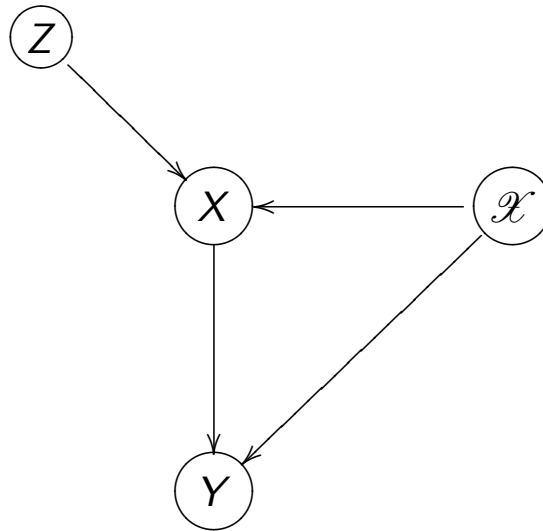
$$X \perp Z$$

$$Y \perp Z \mid X, \mathcal{X}$$

$$\text{Cov}[X, Z] \neq 0$$

We are after the effect of X on Y , but we suspect the effect is confounded by $\mathcal{X}$.

Let's Draw a DAG !!



Notes:

- (i) The influence of X on Y is confounded by $\mathcal{X}$.
- (ii) Note the assumption that Z is independent of $\mathcal{X}$
- (iii) $\mathcal{X}$ could contain some measured components

Model

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

$$X_i = \gamma_0 + \gamma_1 Z_i + u_{2i}$$

where

$$\xi_i = \eta_0 + \eta_1 \mathcal{X}_i + u_{1i}$$

and

$$\text{Cov}[X_i, \xi_i] = \text{Cov}[X_i, \eta_0 + \eta_1 \mathcal{X}_i + u_{1i}] = \eta_1 \mathbb{E}[X_i \mathcal{X}_i] \neq 0$$

but

$$\text{Cov}[Z_i, \xi_i] = \text{Cov}[Z_i, u_{2i}] = 0$$

and $\gamma_1 \neq 0$.

Model

Recalling the earlier formulation, if we write

$$Y_i = \beta_0^* + \beta_1^* Z_i + u_i$$

then

$$\beta_1^* = \gamma_1 \beta_1$$

that is

$$\text{EFFECT}_{Z \rightarrow Y} = \text{EFFECT}_{Z \rightarrow X} \times \text{EFFECT}_{X \rightarrow Y}$$

Example: non-compliance in randomized trials

Greenland (2000):

Three binary variables:

- X is treatment received
- Y is response
- Z is treatment assigned

	$Z = 1$		$Z = 0$	
	$X = 1$	$X = 0$	$X = 1$	$X = 0$
$Y = 1$	12	34	0	74
$Y = 0$	9675	2419	0	11588
Total	9687	2453	0	11662

Observational Analysis

Collapsing over Z :

	$X = 1$	$X = 0$
$Y = 1$	12	108
$Y = 0$	9675	14007
Total	9687	14115

$$RD = 12/9687 - 108/14115 = -6.413(0.815) \quad (\times 10^3)$$

$$RR = \frac{12/9687}{108/14115} = 0.162$$

$$\log RR = -1.821(0.304)$$

$$t = -5.990$$

ITT Analysis

Collapsing over X :

	$Z = 1$	$Z = 0$
$Y = 1$	46	74
$Y = 0$	12094	11588
Total	12140	11662

$$RD = 46/12140 - 74/11662 = -2.556(0.923) \quad (\times 10^3)$$

$$RR = \frac{46/12140}{74/11662} = 0.597$$

$$\log RR = -0.516(0.187)$$

$$t = -2.753$$

IV

Following the above approach for causal **risk difference**:

- Regress Y on Z to get $\hat{\beta}_1^*$
- Regress X on Z to get $\hat{\gamma}_1$
- Report $\hat{\beta}_1 = \hat{\beta}_1^* / \hat{\gamma}_1$

Notes

- (i) Use linear regression
- (ii) Check $\hat{\gamma}_1 \neq 0$
- (iii) Standard errors via analytic formula/bootstrap
- (iv) **Relative risk** more complicated

IV Analysis Comparison

Risk Difference:

```
Obs: (12/9687)-(108/14115)
-0.006412661
```

```
ITT: (46/12140)-(74/11662)
-0.002556268
```

```
IV: coef(lm(Y~factor(Z)))[2]/coef(lm(X~factor(Z)))[2]
-0.003203582
```

```
signif(summary(lm(X~factor(Z)))$coef[2,],digits=4)
Estimate Std.Error t value Pr(>|t|)
7.979e-01 3.718e-03 2.146e+02 0.000e+00
```

IV Analysis Comparison

Risk Difference: $\times 10^{-3}$

	Est.	s. e.	95 % CI
Obs	-6.401	0.811	(-8.021 : -4.837)
ITT	-2.546	0.926	(-4.323 : -0.722)
IV	-3.190	1.161	(-5.420 : -0.904)

5000 bootstrap resamples.

Errors-in-Variables

The model

$$Y_i = \beta_0 + \beta_1 X_i + u_{1i}$$

$$X_i = \gamma_0 + Z_i + u_{2i}$$

with X_i **unobserved** is known as the

(classical) errors-in-variables

model. The assumption

$$Y \perp Z \mid X$$

is still used. Z is a proxy for X .

Part III

Angrist, Imbens, Rubin (1996)

Angrist, Imbens, Rubin (1996)

- **Objective:** To link classical instrumental variables (IV) to the “Rubin Causal Model” (RCM) for the identification of causal effects.
- **Structure:**
 - ▶ Historical perspective
 - ▶ Mathematical framework and assumptions
 - ▶ Link between IV and RCM
 - ▶ Validity of Assumptions
 - ▶ Example
 - ▶ Discussion

Central theme: “Classical IV estimator estimates a causal effect under certain assumptions, and we can study the sensitivity of this estimate to relaxing assumptions”

Structural Equation Models

Essentially “structural” means “causal”.

- Y health outcome
- D observed treatment
- Z instrument

$$Y_i = \beta_0 + \beta_1 D + \epsilon_i$$

$$D_i^* = \alpha_0 + \alpha_1 Z_i + \nu_i$$

and

$$D_i = \mathbb{I}\{D_i^* > 0\}.$$

Assume

$$\mathbb{E}[Z_i \epsilon_i] = \mathbb{E}[Z_i \nu_i] = 0 \quad \mathbb{Cov}[D_i, Z_i] \neq 0$$

Structural Equation Models

- termed the *dummy endogenous variable model*
- D_i is the *endogenous regressor*
- can have $\mathbb{Cov}[D_i, \epsilon_i] \neq 0$, so that D_i is potentially
 - ▶ *not exogenous* (econometrics)
 - ▶ *not ignorable* (biostatistics)

By the previous arguments,

$$\hat{\beta}_1^{IV} = \hat{\gamma}_{YZ} / \hat{\gamma}_{DZ}$$

where

$$\gamma_{YZ} = \mathbb{Cov}[Y, Z] \quad \gamma_{DZ} = \mathbb{Cov}[D, Z]$$

are estimated using the usual moment estimators.

Structural equation models ... have not found widespread use among statisticians

AIR, p446.

The Rubin Causal Model Formulation

The standard model, using *potential outcomes*

- $\mathbf{Y} = (Y_1, \dots, Y_n)$
- $\mathbf{D} = (D_1, \dots, D_n)$
- $\mathbf{Z} = (Z_1, \dots, Z_n)$

Let

- $D_i(\mathbf{Z})$ denote the *potential treatment* for subject i given vector of instruments $\mathbf{Z}$.
- $Y_i(\mathbf{Z}, \mathbf{D})$ denote the *potential outcome* for subject i given $\mathbf{Z}$ and $\mathbf{D}$.

$D_i(\mathbf{Z})$ and $Y_i(\mathbf{Z})$ are fixed but unknown values

AIR, p446.

The Rubin Causal Model Formulation

Stable Unit Value Assumption (SUTVA):

(a)

$$Z_i = Z'_i \implies D_i(\mathbf{Z}) = D_i(\mathbf{Z}')$$

(b)

$$Z_i = Z'_i, D_i = D'_i \implies Y_i(\mathbf{Z}, \mathbf{D}) = Y_i(\mathbf{Z}', \mathbf{D}')$$

i.e. potential outcomes for each unit i are unrelated to the treatment status of other units, so that

$$D_i(\mathbf{Z}) \equiv D_i(Z_i) \quad Y_i(\mathbf{Z}, \mathbf{D}) \equiv Y_i(Z_i, D_i)$$

Causal Effect of Z on D :

$$D_i(1) - D_i(0)$$

Causal Effect of Z on Y :

$$Y_i(1, D_i(1)) - Y_i(0, D_i(0))$$

The Rubin Causal Model Formulation

Average Causal Effect:

- Average causal effect of Z on D

$$\mu_D = \mathbb{E}_D[D_i(1) - D_i(0)]$$

- Average causal effect of Z on Y

$$\mu_{D,Y} = \mathbb{E}_{D,Y}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))]$$

Random assignment assumption: for all $\mathbf{c}, \mathbf{c}'$ with

$$\sum_{i=1}^n c_i = \sum_{i=1}^n c'_i$$

we assume $\Pr[\mathbf{Z} = \mathbf{c}] = \Pr[\mathbf{Z} = \mathbf{c}']$.

The Rubin Causal Model Formulation

Under SUTVA and Random Assignment, unbiased estimators of the average intent to treat effects can be obtained as the differences

$$\begin{aligned}\hat{\gamma}_D &= \frac{\sum D_i Z_i}{\sum Z_i} - \frac{\sum D_i (1 - Z_i)}{\sum (1 - Z_i)} \\ \hat{\gamma}_{D,Y} &= \frac{\sum Y_i Z_i}{\sum Z_i} - \frac{\sum Y_i (1 - Z_i)}{\sum (1 - Z_i)}\end{aligned}$$

and the ratio of these two estimators (second to first) can be written as the ratio of sample covariances between Y and Z and between D and Z .

This ratio, the ratio of average ITT effects, is the usual IV estimator of β_1 in the structural model.

More assumptions

- **Exclusion restriction:**

$$Y(Z, D) = Y(Z', D) \quad \forall Z, Z', D.$$

that is, $Y_i(1, d) = Y_i(0, d)$ for $d = 0, 1$ - any effect of Z on Y must be via an effect of Z on D . As a consequence

$$Y(D) \equiv Y(Z, D) \equiv Y(Z', D) \quad \forall Z, Z', D.$$

The **Causal Effect of D on Y** for subject i is thus

$$Y_i(1) - Y_i(0)$$

The two terms can never be observed **simultaneously**, but in subjects for whom $D_i(0) \neq D_i(1)$, one or other term may be observed through appropriate choice of Z_i (therefore, restrict attention to these subjects)

More assumptions

- **Non-zero Average Causal Effect of Z on D :**

$$\mathbb{E}[D_i(1) - D_i(0)] \neq 0$$

- **Monotonicity:** For all i ,

$$D_i(1) \geq D_i(0)$$

Strong monotonicity if both hold

Z is an instrument for the causal effect of D on Y if all of these assumptions hold.

Interpretation

$$\begin{aligned} Y_i(1, D_i(1)) - Y_i(0, D_i(0)) &= Y_i(D_i(1)) - Y_i(D_i(0)) \quad (\text{Excl. Restr.}) \\ &= [Y_i(1)D_i(1) + Y_i(0)(1 - D_i(1))] \\ &\quad - [Y_i(0)D_i(0) + Y_i(0)(1 - D_i(0))] \\ &= (Y_i(1) - Y_i(0))(D_i(1) - D_i(0)) \end{aligned}$$

Note that

$$Y_i(D_i(1)) = \begin{cases} Y_i(0) & \text{if } D_i(1) = 0 \\ Y_i(1) & \text{if } D_i(1) = 1 \end{cases} = Y_i(1)D_i(1) + Y_i(0)(1 - D_i(1))$$

and so on.

Interpretation

We have

$$Y_i(1, D_i(1)) - Y_i(0, D_i(0)) = (Y_i(1) - Y_i(0))(D_i(1) - D_i(0))$$

so, under the assumptions, the causal effect of Z on Y for subject i is the product of the causal effect of D on Y and the causal effect of Z on D . Thus

$$\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))] = \mathbb{E}[(Y_i(1) - Y_i(0))(D_i(1) - D_i(0))].$$

This expectation can be computed using iterated expectation, conditioning on the three possible values of $\Delta_i = D_i(1) - D_i(0)$, namely -1,0,1. But given

$$(D_i(1) - D_i(0)) = 0$$

(so either $D_i(0) = D_i(1) = 0$ or $D_i(0) = D_i(1) = 1$), the product in the expectation is zero.

Interpretation

Therefore

$$\begin{aligned}\mathbb{E}[(Y_i(1) - Y_i(0))(D_i(1) - D_i(0))] \\&= \mathbb{E}[(Y_i(1) - Y_i(0))\Delta_i | \Delta_i = 1] \Pr(\Delta_i = 1) \\&\quad + \mathbb{E}[(Y_i(1) - Y_i(0))\Delta_i | \Delta_i = -1] \Pr(\Delta_i = -1) \\&= \mathbb{E}[(Y_i(1) - Y_i(0)) | \Delta_i = 1] \Pr(\Delta_i = 1) \\&\quad - \mathbb{E}[(Y_i(1) - Y_i(0)) | \Delta_i = -1] \Pr(\Delta_i = -1)\end{aligned}$$

Monotonicity implies $\Delta_i = D_i(1) - D_i(0)$ is equal to 0 or 1, so under this assumption

$$\begin{aligned}\mathbb{E}[(Y_i(1) - Y_i(0))(D_i(1) - D_i(0))] \\&= \mathbb{E}[(Y_i(1) - Y_i(0)) | \Delta_i = 1] \Pr(\Delta_i = 1)\end{aligned}$$

Interpretation

But, under the assumptions,

$$\Pr(\Delta_i = 1) = \mathbb{E}[D_i(1) - D_i(0)]$$

Therefore, cross-multiplying

$$\frac{\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))]}{\mathbb{E}[D_i(1) - D_i(0)]} = \mathbb{E}[(Y_i(1) - Y_i(0)) | D_i(1) - D_i(0) = 1]$$

Right-hand side: ratio of causal effects of Z on Y and on D .

Left-hand side: average treatment effect in the subgroup of subjects amongst whom $D_i(1) - D_i(0) = 1$ (known as the **Local Average Treatment Effect, LATE**)

Interpretation

Four cases:

1. *Compliers*: $D_i(1) = 1, D_i(0) = 0$
2. *Never takers*: $D_i(1) = 0, D_i(0) = 0$
3. *Always takers*: $D_i(1) = 1, D_i(0) = 1$
4. *Defiers*: $D_i(1) = 0, D_i(0) = 1$.

By the **exclusion restriction**, for 2. and 3., the causal effect of Z on Y is zero as $Y_i(0) = Y_i(1)$.

By **monotonicity**, there are no defiers.

Note: Identifying the individuals in the population who are compliers is not straightforward.

Comparison of Viewpoints

Benefits of structural equation (SE) model formulation over potential outcome (PO) formulation are largely to do with **transparency**.

- Uncorrelatedness of Z with residual errors ϵ and ν separated out in PO framework into the exclusion restriction and ignorability.

For [compliers], the exclusion restriction amounts to attributing the effect of Z on Y to the change in the treatment received D rather than the change in the assignment Z .

- Random assignment of instrument/ignorability needs consideration of the assignment mechanism.
- In PO, monotonicity excludes the possibility of defiers. No counterpart in SE.

Sensitivity

Amending the argument above without the exclusion restriction

$$\begin{aligned} & \mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))] \\ &= \mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0)) | \Delta_i = 1] \Pr(\Delta_i = 1) \\ & \quad + \mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0)) | \Delta_i = 0] \Pr(\Delta_i = 0) \end{aligned}$$

and note that

$$\mathbb{E}[D_i(1) - D_i(0)] = \Pr(\Delta_i = 1) = \Pr(i \text{ is a complier})$$

as $\Pr(\Delta_i \geq 0) = 1$.

Sensitivity

Thus, shifting the first RHS term across, and dividing through

$$\begin{aligned} & \frac{\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))]}{\mathbb{E}[D_i(1) - D_i(0)]} - \mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0)) | \Delta_i = 1] \\ &= \mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0)) | \Delta_i = 0] \\ & \quad \times \frac{\Pr(\Delta_i = 0)}{\Pr(\Delta_i = 1)} \end{aligned}$$

Sensitivity

Relaxing Exclusion Restriction: ... introduces bias.

If i is a non-complier, so that $D_i(0) = D_i(1)$, then the causal effect of Z on Y is $H_i = Y_i(1, d) - Y_i(0, d)$ where $d = 0/1$ for never/always takers.

$$\begin{aligned} \frac{\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))]}{\mathbb{E}[D_i(1) - D_i(0)]} - \mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0)) | i \text{ is a complier}] \\ = \mathbb{E}[H_i | i \text{ is a non-complier}] \frac{P(i \text{ is non-complier})}{P(i \text{ is complier})} \end{aligned}$$

(Under exclusion restriction, for non-compliers, $H_i = 0$.)

Sensitivity

Suppose that for each complier, assignment and treatment have additive effects on the outcome

$$Y_i(1, 0) - Y_i(0, 0) = Y_i(1, 1) - Y_i(0, 1)$$

for all compliers. Under this assumption, define the causal effect of Z on Y (H), and the causal effect of D on Y (G) by

$$H_i = Y_i(1, d) - Y_i(0, d)$$

$$G_i = Y_i(z, 1) - Y_i(z, 0).$$

Sensitivity

Then

$$\frac{\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))]}{\mathbb{E}[D_i(1) - D_i(0)]} = \mathbb{E}[G_i | i \text{ is a complier}] + \frac{\mathbb{E}[H_i]}{\Pr(i \text{ is a complier})}.$$

The second term (the bias relative to the average causal effect of D on Y , G) can be re-expressed

$$\mathbb{E}[H_i | i \text{ is a complier}] + \mathbb{E}[H_i | i \text{ is a non-complier}] \frac{\Pr(i \text{ is a non-complier})}{\Pr(i \text{ is a complier})}$$

Interpretation: bottom of page 451.

Sensitivity

The first term ... has nothing to do with noncompliance, but is the bias due to the direct effect of assignment for those who take the treatment. If compliance were perfect, the second term would be zero but the first term of the bias would still be present. The increased bias in the IV estimand due to noncompliance is directly proportional to the product of the average size of the direct effect of Z for noncompliers and the odds of noncompliance given monotonicity. The higher the correlation between the instrument and the treatment status (i.e., the “stronger” the instrument), the smaller the odds of noncompliance, and consequently the less sensitive the IV estimand is to violations of the exclusion assumption.

AIR, p451

Sensitivity

Without monotonicity, adopting the initial decomposition

$$\begin{aligned}\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))] \\&= \mathbb{E}[(Y_i(1) - Y_i(0))\Delta_i | \Delta_i = 1] \Pr(\Delta_i = 1) \\&\quad + \mathbb{E}[(Y_i(1) - Y_i(0))\Delta_i | \Delta_i = -1] \Pr(\Delta_i = -1) \\&= \mathbb{E}[(Y_i(1) - Y_i(0)) | \Delta_i = 1] \Pr(\Delta_i = 1) \\&\quad - \mathbb{E}[(Y_i(1) - Y_i(0)) | \Delta_i = -1] \Pr(\Delta_i = -1)\end{aligned}$$

Now

$$\begin{aligned}\mathbb{E}[D_i(1) - D_i(0)] &= \Pr(\Delta_i = 1) - \Pr(\Delta_i = -1) \\&= \Pr(i \text{ is a complier}) - \Pr(i \text{ is a defier})\end{aligned}$$

Sensitivity

Therefore

$$\begin{aligned}\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))] \\&= \mathbb{E}[Y_i(1) - Y_i(0) | \Delta_i = 1] (\mathbb{E}[D_i(1) - D_i(0)] + \Pr(\Delta_i = -1)) \\&\quad - \mathbb{E}[Y_i(1) - Y_i(0) | \Delta_i = -1] \Pr(\Delta_i = -1)\end{aligned}$$

Rearranging

$$\begin{aligned}\frac{\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))]}{\mathbb{E}[D_i(1) - D_i(0)]} &= \mathbb{E}[(Y_i(1) - Y_i(0)) | \Delta_i = 1] \\&= \frac{\Pr(\Delta_i = -1)}{\mathbb{E}[D_i(1) - D_i(0)]} \mathbb{E}[Y_i(1) - Y_i(0) | \Delta_i = 1] \\&\quad - \frac{\Pr(\Delta_i = -1)}{\mathbb{E}[D_i(1) - D_i(0)]} \mathbb{E}[Y_i(1) - Y_i(0) | \Delta_i = -1]\end{aligned}$$

Relaxing Monotonicity: ... introduces bias. If

$$\begin{aligned}\frac{\mathbb{E}[Y_i(1, D_i(1)) - Y_i(0, D_i(0))]}{\mathbb{E}[D_i(1) - D_i(0)]} &= \mathbb{E}[(Y_i(1) - Y_i(0)) | i \text{ is a complier}] \\ &= -\lambda \mathbb{E}[(Y_i(1) - Y_i(0)) | i \text{ is a defier}] \\ &\quad + \lambda \mathbb{E}[(Y_i(1) - Y_i(0)) | i \text{ is a complier}]\end{aligned}$$

where

$$\lambda = \frac{\Pr(i \text{ is a defier})}{\Pr(i \text{ is a complier}) - \Pr(i \text{ is a defier})}$$

Interpretation: bottom of page 451.

Application: Effect of Military Service on Mortality

Cohort: US men born in 1950 and 1951

- D - military service (1 - served, 0 - did not serve)
- Z - lottery assignment (1 - low number, 0 - high number)
- Y - civilian death 1974-1983 (1 - died, 0 - did not die)

IV/PO Assumptions : all plausible

Results : not much significant ...

Application: Effect of Military Service on Mortality

Sensitivity:

- *Exclusion Restriction*: relaxing this seems to affect results, so treat IV estimates with care

AIR, top p454

- *Monotonicity*: results not so sensitive

AIR, bottom p454

Discussion

In short:

- **Robins and Greenland**: Ignore IV altogether, and discuss methods for estimating different causal parameters under fewer assumptions.
- **Heckman**: Criticizes extensively: claims that LATE is not a viable parameter of interest, and that econometricians have IV methods for genuine parameters of interest. Alleges that assumptions are unnecessarily strong:

Econometricians make weaker mean independence assumptions rather than the strong independence assumptions made by AIR to identify their parameter.

Heckman, in Discussion of AIR, p 459.

Discussion

In short:

- **Moffitt:** Discusses other interpretations of IV, limitations of assumptions, provides illuminating examples. Questions the example.
- **Rosenbaum:** Examines a nonrandomized artificial example, and a more complicated IV procedure based on the Hodges-Lehmann estimator (a robust location parameter estimator based on ranks)

Rejoinder

In short: an extensive rebuttal of Heckman.