Topic 5: Instrumental Variables

ECON 5783 — University of Arkansas

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Introducing Instrumental Variables

What makes a good instrument?

Two-stage least squares estimator

IV with Heterogeneous Effects

Extensions

Instrumental Variables

Instrumental Variables are one of the oldest ‘causal’ identification strategies

- Their roots go back to the early 1900s where economics was primarily the study of particular markets
- Demand and supply curves were a new phenomenon and economists wanted to try and estimate them

Supply and Demand Curves

Consider a simple supply and demand curve setup

$$\text{quantity}_d = \alpha_d + \text{price}\gamma_d + u_d \tag{1}$$

$$\text{quantity}_s = \alpha_s + \text{price}\gamma_s + u_s \tag{2}$$

Here, this is the theoretical relationship between prices and quantity *at the same point in time* (potential outcomes)

- Equilibrium is determined by the price such that $\text{quantity}_d = \text{quantity}_s$

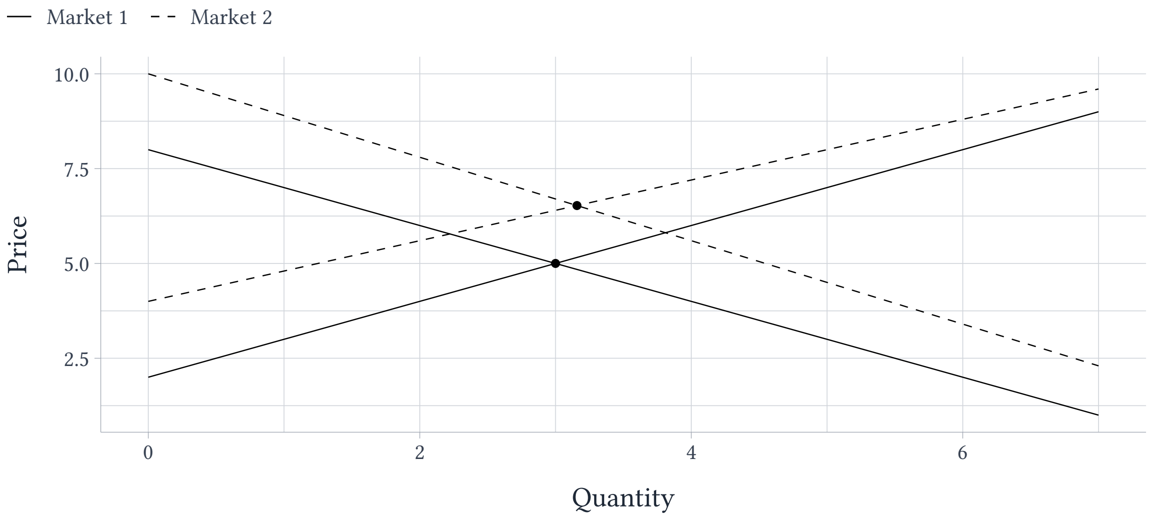
Note that ‘market conditions’ (e.g. preferences, production technologies, etc.) are implicitly embedded in the coefficients

Observed market outcomes

What we observe is a set of markets t with their corresponding price and quantities (p_t, q_t)

The problem here is that market-shocks could move both the demand curve and the supply curve

$\rightarrow$ The data is not tracing 'along' the demand curve or the supply curve



Market 1
Market 2
Price
10.0
7.5
5.0
2.5
0
2
4
6
Quantity

Tracing out the demand curve

Phillip Wright wrote a book in 1928 about ‘animal and vegetable oils’

- he was trying to argue that recent tariffs had negatively impacted the market

In this book, there is a now famous “Appendix B” proposing the first IV estimator

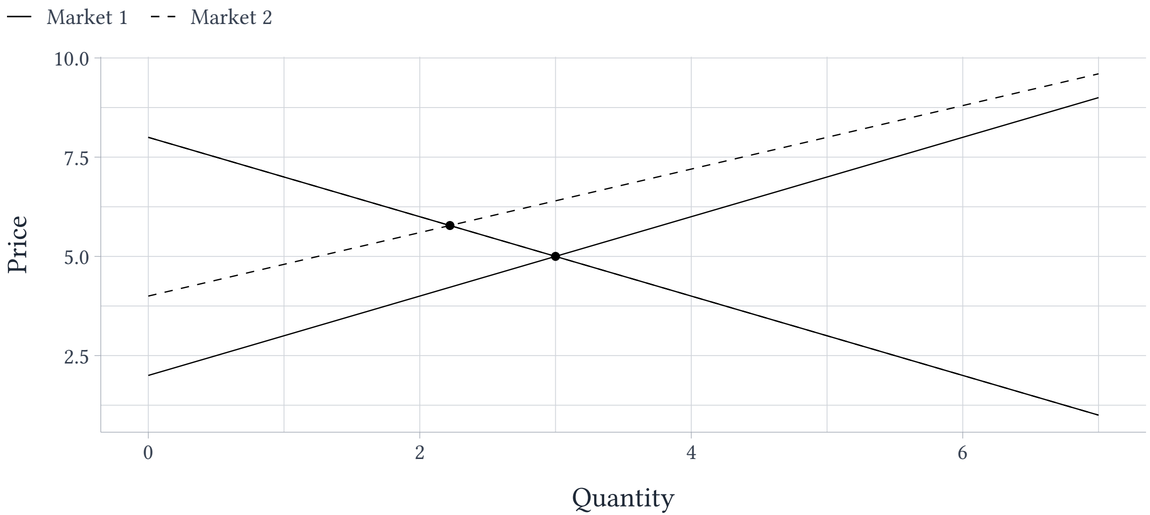
- There’s debate who wrote this chapter: father or son.
- It was likely the son; see Stock and Trebbi, 2003

Identifying movement along one curve

As we saw in the previous graph, we can not take two points (p_1, q_1) and (p_2, q_2) and know if these fall on the same demand curve or the same supply curve

Say somehow we *know* that one of the curves did not change from market 1 to market 2 (say from one day to the next)

- Then our two points both must fall on the curve that did not shift (equilibrium conditions)



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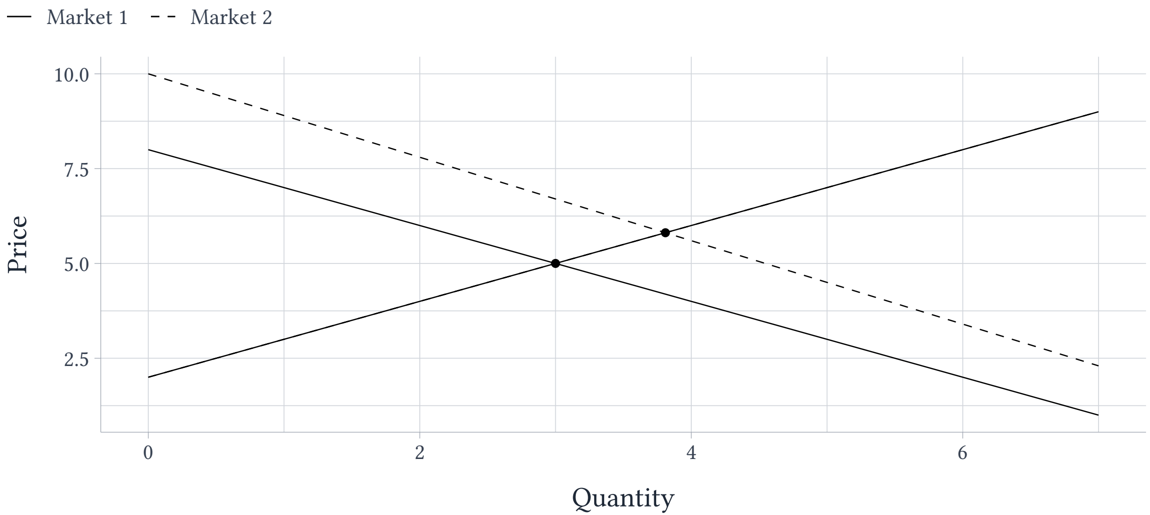

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“Demand Shifters” and “Supply Shifters”

Wright proposed to use a variable that shifts only one of the curves and not the other, what we now call instruments, to estimate the demand/supply curve:

- E.g. a demand shifter being the change in the price of a substitute good
- E.g. a supply shifter being a change in rain from one year to the next

“Demand Shifters” and “Supply Shifters”

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- E.g. a demand shifter being the change in the price of a substitute good
- E.g. a supply shifter being a change in rain from one year to the next

Since these ‘shifters’ only affect one curve and not the other, then we can leverage these to estimate the other curve (e.g. demand shifter to estimate supply)

Fulton Fish Market

Angrist, Graddy, and Imbens (2000) give a great example of this.

Graddy, then a graduate student, woke up every morning before sunrise and traveled from New Jersey to the Fulton Fish Market in downtown Manhattan

- Recorded data on the price and quantity sold of fish
- Also recorded each day's ocean-weather, an important supply curve shifter


24 LA ROCCA FISH
ARROW 22

Fulton Fish Market IV Estimator

Let Z_t denote the weather observed in market t (e.g. Z_t is the wind speeds on the ocean).

The idea of the IV estimator we will present is to do two things:

1. First, see how weather, Z_t, impacts the (log) market price, $\log(p_t)$
2. Second, see how weather, Z_t, impacts the (log) market quantity, $\log(q_t)$

Since the demand elasticity is $\frac{\partial \log(q_t)}{\partial \log(p_t)}$, we can take the ratio of these two quantities to estimate the demand elasticity

Fulton Fish Market IV Estimator

Our IV estimator can be written as

$$\hat{\tau}_{\texttt{IV}} = \frac{\text{Cov}\left(Z_t, q_t\right)}{\text{Cov}\left(Z_t, p_t\right)}$$

- Weather causes a shift in q_t, $\text{Cov}\left(Z_t, q_t\right)$, and a shift in p_t, $\text{Cov}\left(Z_t, p_t\right)$

Our IV estimate compares how much weather changes the quantity sold in equilibrium to how much weather changes the price to estimate the demand curve

Fulton Fish Market IV Estimator

$$\hat{\tau}_{\texttt{IV}} = \frac{\textsf{Cov}\left(Z_t, q_t\right)}{\textsf{Cov}\left(Z_t, p_t\right)}$$

Ideally these shifts come from only the supply curve moving; i.e. Z_t has no effect on quantity$_t^d$ except from the price

- E.g. this requires fish demand not change with the weather, otherwise both curves would be moving

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Generalizing the IV Estimator

What are the “essential” ideas of this IV estimator? Say you want to know the causal effect of X_i on some variable y_i. We are concerned there are some other variables (in the error term, ε_i) that determine both X_i and y_i.
That is:

$$y_i = X_i \beta + \varepsilon_i,$$

where $\mathbb{E}[\varepsilon_i \mid X_i] \neq 0$

Generalizing the IV Estimator

$$y_i = X_i\beta + \varepsilon_i \text{ where } \mathbb{E}[\varepsilon_i \mid X_i] \neq 0$$

Our IV Estimator compares the change in y_i induced by Z_i to the change in X_i induced by Z_i to estimate the slope parameter:

$$\frac{\Delta y \quad \text{induced by Z}}{\Delta X \quad \text{induced by Z}}$$

IV Requirements

$$y_i = X_i\beta + \varepsilon_i \text{ where } \mathbb{E}[\varepsilon_i \mid X_i] \neq 0$$

We want an instrument Z_i that does two things:

1. (Relevancy Condition) The instrument should cause a change in X_i, so that we could trace out the subsequence impact on y_i

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We want an instrument Z_i that does two things:

1. (Relevancy Condition) The instrument should cause a change in X_i, so that we could trace out the subsequence impact on y_i

2. (Exclusion Restriction) The instrument Z_i should change y_i *only through* changing X_i

IV Estimand

$$\hat{\beta}_{\texttt{IV}} = \frac{\textsf{Cov}\left(Z_i, y_i\right)}{\textsf{Cov}\left(Z_i, X_i\right)}$$

IV Estimand

$$
\begin{aligned}
\hat{\beta}_{\texttt{IV}} &= \frac{\textsf{Cov}\left(Z_i, y_i\right)}{\textsf{Cov}\left(Z_i, X_i\right)} \\
&= \frac{\textsf{Cov}\left(Z_i, X_i\beta + \varepsilon_i\right)}{\textsf{Cov}\left(Z_i, X_i\right)}
\end{aligned}
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IV Estimand

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&= \beta + \frac{\textsf{Cov}\,(Z_i, \varepsilon_i)}{\textsf{Cov}\,(Z_i, X_i)}
\end{aligned}
$$

IV Requirements

$$\hat{\beta}_{\texttt{IV}} = \frac{\text{Cov}\,(Z_i, X_i)\beta + \overbrace{\boxed{\text{Cov}\,(Z_i, \varepsilon_i)}}^{\substack{\textit{Exclusion Restriction} \\ = 0}}}{\underbrace{\boxed{\text{Cov}\,(Z_i, X_i)}}_{\substack{\textit{Relevancy Condition} \\ \text{not dividing by } 0}}}$$

1. Relevancy Condition requires Z_i to actually shift X_i
2. Exclusion Restriction requires Z_i to be uncorrelated with other drivers of y_i

IV Requirements

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1. Relevancy Condition requires Z_i to actually shift X_i
2. Exclusion Restriction requires Z_i to be uncorrelated with other drivers of y_i

The first tends to be the easier of the two to satisfy: we need a 'shifter' of X_i. The second is the hard part...

Rainfall IV Example

For example, consider trying to estimate on family income on the years of schooling children receive in the developing world. Papers have used rainfall as an instrument for income

- The idea is that rainfall is ‘random’ year over year and so that creates ‘good variation’ in family income

Rainfall IV Example

In essence, the IV estimator will compare families that had a good rainfall year (and hence more income than normal) to families with a bad rainfall year (and hence less money than expected)

- If high-rainfall families go to school at higher rates, the IV estimator attributes this to higher income

Rainfall IV Exclusion Restriction failure

The exclusion restriction assumes that rainfall only affects the school attendance rate *only through* increasing family incomes

- Is this plausible?

Rainfall IV Exclusion Restriction failure

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- Is this plausible?

Sarsons (2015, JDE) finds that lower rainfall in developing countries increases conflict between villages

- This means we can't say if the higher school attendance in high rain areas is due to more income from better crop-yields *or* due to lower likelihood of conflict

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Canonical IV Setup

The canonical IV setup is as follows:

$$y_i = X_i\beta + \varepsilon_i$$
$$X_i = Z_i\pi + u_i,$$

where Z_i is our instrument and X_i is the variable of interest

Canonical IV setup

$$y_i = X_i \beta + \varepsilon_i \text{ and } X_i = Z_i \pi + u_i$$

In terms of this model, our problem is that $\mathbb{E}[X_i \varepsilon_i] \neq 0$, i.e. that there are unobservables that are correlated with X_i and have an impact on y_i

Canonical IV setup

$$y_i = X_i\beta + \varepsilon_i \text{ and } X_i = Z_i\pi + u_i$$

We instead use an instrument Z_i. Our two requirements for the instrument can be written as follows:

1. (Relevance) $\pi \neq 0$
   - $\rightarrow$ This is testable with t-test that $\pi = 0$
   - $\rightarrow$ Later, problems with 'weak' instruments where $\pi \approx 0$ (relative to noise)
2. (Exclusion) $\mathbb{E}[\varepsilon_i Z_i] = 0$
   - $\rightarrow$ Fundamentally untestable (it is an assumption)

Canonical IV setup

Exclusion Restriction

$$y_i = X_i \beta + \varepsilon_i \text{ and } X_i = Z_i \pi + u_i$$

For example, say there is some variable μ_i that is part of the error term $\varepsilon_i = \mu_i + \upsilon_i$

- If Z_i is correlated with μ_i (or affects μ_i), then our exclusion restriction fails

Two-stage least squares (2SLS)

$$y_i = X_i\beta + \varepsilon_i \text{ and } X_i = Z_i\pi + u_i$$

The previous estimator is identical to the two-stage least squares estimator:

$$\hat{\beta}_{\text{2SLS}} = \frac{(X_i P_Z)' P_Z y_i}{(X_i P_Z)'(P_Z X_i)} = \frac{\hat{X}_i' \hat{y}_i}{\hat{X}_i' \hat{X}_i},$$

where $P_Z = Z(Z'Z)^{-1}Z'$ is the projection-matrix from ordinary least squares regression

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where $P_Z = Z(Z'Z)^{-1}Z'$ is the projection-matrix from ordinary least squares regression

- This is numerically identical when Z is a single instrument to our previous estimator $\text{Cov}\,(Z_i, y_i) / \text{Cov}\,(Z_i, X_i)$

Two-stage least squares Estimator (2SLS)

$$\hat{\beta}_{\texttt{IV}} = \frac{\hat{X}_i' \hat{y}_i}{\hat{X}_i' \hat{X}_i},$$

Can think of this as being done in two-stages (hence the name):

- Predict y_i and X_i using the instrument Z_i via separate regressions
- Regress $\hat{y}_i$ on $\hat{X}_i$

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Mostly Harmless Econometrics: *"Intuitively, conditional on covariates, 2SLS retains only the variation in X that is generated by quasi-experimental variation; that is generated by the instrument Z"*

OLS with Controls vs. IV

The Frisch-Waugh-Lovell (FWL) theorem helped us understand what control variables do in a regression of y_i on a varaible X_i and controls W_i

- Use W_i to *predict* X_i and y_i and remove that predicted variation
- Regress $y_i - \hat{y}_i$ on $X_i - \hat{X}_i$

OLS with Controls vs. IV

Both estimators wants to use variation in X_i that is 'plausibly exogenous'; sometimes called 'quasi-experimental variation'.

The IV estimator and OLS with controls try to get at 'good' variation in X_i in different ways:

1. OLS 'removes bad variation'

   $\rightarrow$ Use controls that you think pick up on variables (in ε_i) that are correlated with X_i

2. IV 'isolates good variation'

   $\rightarrow$ Use an instrument that you think is *not* correlated with ε_i but that shift X_i

Weak IV

$$y_i = X_i\beta + \varepsilon_i \text{ and } X_i = Z_i\pi + u_i$$

When our estimated $\hat{\pi} \approx 0$ we have a ‘weak instruments’ problem

- I.e. close to zero relative to the noise

Weak IV

$$y_i = X_i\beta + \varepsilon_i \text{ and } X_i = Z_i\pi + u_i$$

When our estimated $\hat{\pi} \approx 0$ we have a ‘weak instruments’ problem

- I.e. close to zero relative to the noise

This means there is a small covariance between X_i and Z_i:

- Since $\hat{\beta}_{\texttt{IV}}$ divides by Cov (X_i, Z_i), the estimate is very noisy
  - $\rightarrow$ $1/0.0001 = 10000$ vs. $1/0.00005 = 20000$

Weak IV

The rule of thumb is to use an instrument when the F-stat on the first-stage is ≥ 10

- First-stage is $X_i = Z_i \pi + u_i$
- In case of single IV Z_i, equivalent to t-stat on Z_i to be $\geq \sqrt{10} = 3.16$

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Some recent work shows that 10 might be too small of a lower bound and you may prefer something like $F \geq 50$ (t-stat ≥ 7.07), but it is unsettled debate

Many Instruments

Another problem comes in when you have a large number of instruments (relative to sample size)

Our 2SLS estimator regresses $P_Z y_i$ on $P_Z X_i$

- Increasing the dimension of Z means we are going to necessarily predict X_i and y_i better and better
- Our $P_Z y_i \to y_i$ and $P_Z X_i \to X_i$

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$\implies$ that with too many instruments we $\hat{\beta}_{\text{2SLS}} \approx \hat{\beta}_{\text{0LS}}$

Many Instruments Example

One common IV strategy is the 'judge-leniency design' setting:

- Defendents are randomly assigned to different judges
- Judges vary in how lenient they are in sentencing

$\Longrightarrow$ people with similar backgrounds will be randomly convicted/not-convicted

- Use as an instrument a set of dummy variables for each assigned judge (judge 1, judge 2, $\ldots$)

Many Instruments

The best way to fix the issue of many-instruments is using a JIVE estimator

- For observation i, predict X_i using a leave-out regression of X_j on Z_j using all observations besides i: $\{1, \dots, n\} \setminus \{i\}$

Many Instruments

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- For observation i, predict X_i using a leave-out regression of X_j on Z_j using all observations besides i: $\{1, \dots, n\} \setminus \{i\}$

Call the predicted values of the leave-out regressions as $\hat{X}_{i,\texttt{lo}}$. Then we have

$$\hat{\beta}_{\texttt{JIVE}} \equiv \frac{\hat{X}'_{i,\texttt{lo}} y_i}{\hat{X}'_{i,\texttt{lo}} \hat{X}_{i,\texttt{lo}}}$$

- $\hat{\beta}_{\texttt{JIVE}}$ avoids the problem of over-fitting on the instruments
- See Kolesar (2013, Working Paper)

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Heterogeneous Effects

Up until this slide we have (implicitly) assumed that the marginal causal effect of increasing X_i was given by β

- Assumed to be the same across individuals

When we allow for heterogeneous effects, what our 2SLS estimator finds is more complicated to understand

- Hopefully, it is some 'reasonable' weighted average of heterogeneous effects, $\hat{\beta}_{\texttt{IV}} \to \sum_i w_i \beta_i$

Angrist-Imbens-Rubin Causal Model

Let's see if we can make progress when D_i is a binary variable, Z_i is a binary variable, but allow the treatment effect $y_i(1) - y_i(0)$ to be heterogeneous.

In Angrist, Imbens, and Rubin (1996, JASA), they study military service (D_i) on future earnings (y_i)

- The first problem we have is that military service is not randomly assigned

[†] This has been questioned because the bingo balls were not shuffled enough before drawing

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They use the Vietnam-Draft lottery as an instrument (Z_i)

- Z_i is randomly assigned by birthday[†]

[†] This has been questioned because the bingo balls were not shuffled enough before drawing

Imperfect Compliance

The Vietnam draft lottery is definitely a shifter: those drafted by lottery were more likely to serve

- But we don't have a real RCT. Some with $Z_i = 0$ serve $D_i = 1$ and some with $Z_i = 1$ do not serve $D_i = 0$

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- But we don't have a real RCT. Some with $Z_i = 0$ serve $D_i = 1$ and some with $Z_i = 1$ do not serve $D_i = 0$

This is what we call "imperfect compliance", i.e. treatment is not a perfect function of the instrument

Potential Outcome Model with Imperfect Compliance

To accomodate this, Angrist-Imbens-Rubin framework defines $D_i(Z_i)$ to be the potential outcomes of treatment under both states of the instrument. In our example,

- $D_i(0)$ is whether the person would serve in the world where they *are not* drafted
- $D_i(1)$ is whether the person would serve in the world where they *are* drafted

Potential Outcome Model with Imperfect Compliance

Outcomes are now a potential outcome of both Z_i and D_i: $y_i(D_i, Z_i)$

- Outcomes depend on both whether you were assigned $Z_i = 0, 1$ and whether you are under treatment $D_i = 0, 1$

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If we assume the *exclusion restriction* that Z_i *only* impacts y_i via it's effect on D_i, then we can return to $y_i(D_i)$

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If we assume the *exclusion restriction* that Z_i *only* impacts y_i via it's effect on D_i, then we can return to $y_i(D_i)$

- In the case of the Vietnam Draft lottery, this is plausible assuming that the draft only is correlated with outcomes via causing people to serve

Potential Outcome Model with Imperfect Compliance

Our potential outcomes are therefore

$$D_i(Z_i) \text{ and } y_i(D_i)$$

Our causal model implicitly says:

1. Z_i might impact D_i which impacts y_i
2. Z_i only impacts y_i via D_i

Characterizing People

There are four kinds of people in this model:

1. **Compliers**: people who react to the instrument as expected

   $\rightarrow$ $D_i(1) = 1$ and $D_i(0) = 0$

2. **Always-takers**: people who always take the treatment regardless of Z

   $\rightarrow$ $D_i(1) = 1$ and $D_i(0) = 1$

3. **Never-takers**: people who never take the treatment regardless of Z

   $\rightarrow$ $D_i(1) = 0$ and $D_i(0) = 0$

4. **Defiers**: people who react to the instrument in the wrong direction

   $\rightarrow$ $D_i(1) = 0$ and $D_i(0) = 1$

Defiers

Defiers are often ruled out as implausible:

- “I would have served but not if I was drafted”

But, be careful, they may not be implausible in other settings!

Characterizing People

Ruling out defiers, three types remain:

1. **Compliers**: people who react to the instrument as expected

   $\rightarrow$ $D_i(1) = 1$ and $D_i(0) = 0$

2. **Always-takers**: people who always take the treatment regardless of Z

   $\rightarrow$ $D_i(1) = 1$ and $D_i(0) = 1$

3. **Never-takers**: people who never take the treatment regardless of Z

   $\rightarrow$ $D_i(1) = 0$ and $D_i(0) = 0$

It is impossible to know who is which in the data

First-stage

Our first-stage consists of regressing D_i on Z_i and an intercept:

$$D_i = \alpha + Z_i \pi + u_i$$

From regression mechanics and since Z_i and D_i are indicators, we know

- $\hat{\alpha}$ is the share of people with $D_i = 1$ with $Z_i = 0$
- $\hat{\pi}$ is the difference in share of people with $D_i = 1$ with $Z_i = 1$

First-stage

$$D_i = \alpha + Z_i \pi + u_i$$

In the $Z_i = 0$ group, people with $D_i = 1$ must be always-takers

- $\hat{\alpha}$ is the share of always-takers in the population

First-stage

$$D_i = \alpha + Z_i \pi + u_i$$

In the $Z_i = 0$ group, people with $D_i = 1$ must be always-takers

- $\hat{\alpha}$ is the share of always-takers in the population

In the $Z_i = 1$ group, people with $D_i = 1$ are always-takers *or* compliers

- $\hat{\alpha} + \hat{\pi}$ is the share of always-takers or compliers in the population

First-stage

$$D_i = \alpha + Z_i\pi + u_i$$

In the $Z_i = 0$ group, people with $D_i = 1$ must be always-takers

- $\hat{\alpha}$ is the share of always-takers in the population

In the $Z_i = 1$ group, people with $D_i = 1$ are always-takers *or* compliers

- $\hat{\alpha} + \hat{\pi}$ is the share of always-takers or compliers in the population

$\hat{\pi}$ is our estimated share of compliers

- Share of people who are ‘pushed’ into treatment

Reduced-form

The reduced-form is the regression of

$$y_i = \gamma + Z_i \delta + u_i$$

Since Z_i is an indicator variable

- $\hat{\gamma} = \hat{\mathbb{E}}[y_i \mid Z_i = 0]$
- $\hat{\delta} = \hat{\mathbb{E}}[y_i \mid Z_i = 1] - \hat{\mathbb{E}}[y_i \mid Z_i = 0]$

Reduced-form

We can use the law of iterated expectations to write our estimate $\hat{\delta}$ as a weighted-sum of the three groups:

$$
\begin{aligned}
\hat{\delta} = \ & \mathbb{E}[y_i \mid Z_i = 1] - \mathbb{E}[y_i \mid Z_i = 0] \\
= \ & \mathbb{P}(\text{Always}_i) \left(\mathbb{E}[y_i \mid Z_i = 1, \text{Always}_i] - \mathbb{E}[y_i \mid Z_i = 0, \text{Always}_i] \right) \\
& + \mathbb{P}(\text{Never}_i) \left(\mathbb{E}[y_i \mid Z_i = 1, \text{Never}_i] - \mathbb{E}[y_i \mid Z_i = 0, \text{Never}_i] \right) \\
& + \mathbb{P}(\text{Complier}_i) \left(\mathbb{E}[y_i \mid Z_i = 1, \text{Complier}_i] - \mathbb{E}[y_i \mid Z_i = 0, \text{Complier}_i] \right)
\end{aligned}
$$

Reduced-form

Switching y_i in each to case to the corresponding potential outcome $y_i(d)$:

$$
\begin{aligned}
\hat{\delta} = \ & \mathbb{E}[y_i \mid Z_i = 1] - \mathbb{E}[y_i \mid Z_i = 0] \\
= \ & \mathbb{P}(\text{Always}_i)\left(\mathbb{E}[y_i(1) \mid Z_i = 1, \text{Always}_i] - \mathbb{E}[y_i(1) \mid Z_i = 0, \text{Always}_i]\right) \\
& + \mathbb{P}(\text{Never}_i)\left(\mathbb{E}[y_i(0) \mid Z_i = 1, \text{Never}_i] - \mathbb{E}[y_i(0) \mid Z_i = 0, \text{Never}_i]\right) \\
& + \mathbb{P}(\text{Complier}_i)\left(\mathbb{E}[y_i(1) \mid Z_i = 1, \text{Complier}_i] - \mathbb{E}[y_i(0) \mid Z_i = 0, \text{Complier}_i]\right)
\end{aligned}
$$

Reduced-form

From random assignment of Z_i, we have:

$$\begin{aligned}\hat{\delta} = &\ \mathbb{E}[y_i \mid Z_i = 1] - \mathbb{E}[y_i \mid Z_i = 0] \\ = &\ \mathbb{P}(\text{Always}_i)\left(\mathbb{E}[y_i(1) \mid \text{Always}_i] - \mathbb{E}[y_i(1) \mid \text{Always}_i]\right) \\ &+ \mathbb{P}(\text{Never}_i)\left(\mathbb{E}[y_i(0) \mid \text{Never}_i] - \mathbb{E}[y_i(0) \mid \text{Never}_i]\right) \\ &+ \mathbb{P}(\text{Complier}_i)\left(\mathbb{E}[y_i(1) \mid \text{Complier}_i] - \mathbb{E}[y_i(0) \mid \text{Complier}_i]\right)\end{aligned}$$

Reduced-form

The always-taker and never-taker terms equal 0, so:

$$\begin{aligned}\hat{\delta} &= \mathbb{E}[y_i \mid Z_i = 1] - \mathbb{E}[y_i \mid Z_i = 0] \\ &= \mathbb{P}(\text{Complier}_i)\left(\mathbb{E}[y_i(1) \mid \text{Complier}_i] - \mathbb{E}[y_i(0) \mid \text{Complier}_i]\right)\end{aligned}$$

Reduced-form

$$\hat{\delta} = \hat{\mathbb{E}}[y_i \mid Z_i = 1] - \hat{\mathbb{E}}[y_i \mid Z_i = 0]$$

- For always-takers, $y_i = y_i(1)$ when $Z_i = 1$ and $Z_i = 0$
- For never-takers, $y_i = y_i(0)$ when $Z_i = 1$ and $Z_i = 0$

Therefore, for all but compliers, the difference equals 0. So,

$$\hat{\delta} = \mathbb{P}(\text{Complier}_i)\, \mathbb{E}[y_i(1) - y_i(0) \mid \text{Complier}_i]$$

IV Estimand

Our IV Estimand therefore is the ratio

$$\hat{\beta}_{\text{2SLS}} = \frac{\hat{\delta}}{\hat{\pi}}$$

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Our IV Estimand therefore is the ratio

$$\begin{aligned}\hat{\beta}_{\text{2SLS}} &= \frac{\hat{\delta}}{\hat{\pi}} \\ &= \frac{\mathbb{P}(\text{Complier}_i)\,\mathbb{E}[y_i(1) - y_i(0) \mid \text{Complier}_i]}{\mathbb{P}(\text{Complier}_i)} \\ &= \mathbb{E}[y_i(1) - y_i(0) \mid \text{Complier}_i]\end{aligned}$$

We estimate the average treatment effect among the compliers

- E.g. the average treatment effect among those induced to serve in the military by the lottery

The 'local' average treatment effect

$$\hat{\beta}_{\text{2SLS}} = \mathbb{E}[y_i(1) - y_i(0) \mid \text{Complier}_i]$$

Angrist, Imbens, and Rubin called this the Local Average Treatment Effect

- In applications where you have some 'shifter' of treatment, but imperfect compliance, you want to always think about *who your compliers are*

Example LATEs

Angrist and Evans (1998, AER) study the effect of family size on the parent's labor supply

- They use as an instrument for family size whether or not the first two kids are of the same gender (both male / both female)
- The idea being the baby's genders are random and induces people to try for a third kid

Who are the compliers here?

Example LATEs

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- They use as an instrument for family size whether or not the first two kids are of the same gender (both male / both female)
- The idea being the baby's genders are random and induces people to try for a third kid

Who are the compliers here?

- People who want baby's of both genders and do not mind having three kids

Example LATEs

Frimmel, Halla, and Winter-Ebmer (2024, JPubE) study the impact of parental divorce on chidlrens’ outcomes

- Their instrument is the number of ‘women in [the father’s] relevant age-occupation-group at work are more likely to divorce’

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Example LATEs

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- Their instrument is the number of ‘women in [the father’s] relevant age-occupation-group at work are more likely to divorce’

Who are the compliers here?

- Men who would not have been unfaithful absent the opportunity of meeting more women at work
- This example is from Anna Stansbury on twitter

Better LATE than Nothing

The fact that in a world with imperfect compliance, IV estimates a Local ATE creates a problem

- How do you compare estimates across studies?

In general this is hard to do since the compliers in different papers might look different

Comparing LATEs example

Consider two different papers estimating returns to education:

1. Card (1995) uses distance to local college as an instrument (find 13% increase)
   - $\rightarrow$ Compliers are those that only attend college if they can be close to home

2. Zimmerman (2014, JOLE) uses a GPA threshold for for admissions at a large public university (find 22% increase)
   - $\rightarrow$ Compliers are those near the GPA threshhold who would otherwise not go to college

Do the differences in estimates indicate one of the papers is biased? Or are the complier groups different?

Characterizing Compliers

Since we can only estimate a LATE, we might not be able to apply our estimates to new settings

- This is called a question of “external validity”, i.e. whether our estimates could reasonably be applied to other settings

To see which settings our estimates might reasonably apply to, we want to describe who the compliers are

- Of course, we can not identify compliers since we only see $D_i(1)$ or $D_i(0)$, not both

Characterizing Compliers

It turns out we can still characterize compliers by their potential outcomes ($Y_i(0)$ and $Y_i(1)$) and other observables X_i

- Comparing $\mathbb{E}[\cdot \mid \text{Complier}_i]$ to $\mathbb{E}[\cdot]$ can maybe shed light on how LATE compares to overall ATE

We can do so using a special trick of multiplying a variable with D_i and using that as our outcome variable in 2SLS

- $Z_i D_i$ equals 0 for untreated units ($D_i = 0$) and equals Z_i for treated units

Characterizing Compliers

Three results:

1. 2SLS of $X_i D_i$ on D_i with Z_i as an instrument identifies

$$\mathbb{E}[X_i \mid \text{Complier}_i]$$

2. 2SLS of $Y_i D_i$ on D_i with Z_i as an instrument identifies

$$\mathbb{E}[Y_i(1) \mid \text{Complier}_i]$$

3. 2SLS of $Y_i(1 - D_i)$ on $(1 - D_i)$ with Z_i as an instrument identifies

$$\mathbb{E}[Y_i(0) \mid \text{Complier}_i]$$

Introducing Instrumental Variables

What makes a good instrument?

Two-stage least squares estimator

IV with Heterogeneous Effects

Extensions

Multi-valued instruments

We have discussed the identification of LATEs when we have one single binary instrument Z_i

We needed to rule out 'defiers', i.e. people who had $D_i(1) = 0$ but $D_i(0) = 1$.

- This is sometimes called monotonicity, i.e. the instrument only moves treatment in one direction (compliers)
- Angrist and Krueger show that monotonicity and instrument exclusion imply that we identify a LATE

Multi-valued Instruments

What about when $Z_i \in \{1, \dots, J\}$ is a multi-valued treatment?

- E.g. judge leniency designs (random assignment of judges)

What does the 2SLS estimate of Y_i on D_i using the set of judge indicator variables, $\mathbb{1}[Z_i = j]$, as instruments identify?

- Under monotonicity, the 2SLS estimate will a weighted average of each $Z_{i,k}$'s LATE

Judge IV and LATE

In the judge IV case, think of D_i as being a potential outcome for each judge

- Monotonicity requires that random assignment of judges can only move D_i in one direction

If Judge A is more ‘strict’ than judge B, monotonicity requires:

- Everyone judge B would sentence, judge A would also sentence them (and more people)

Judge IV and LATE

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If Judge A is more 'strict' than judge B, monotonicity requires:

- Everyone judge B would sentence, judge A would also sentence them (and more people)

Montonicity: For all judges, j and j', either $D_i(j) \geq D_i(j')$ or $D_i(j) \leq D_i(j)$ for all i

Judge IV and LATE

If judge A and judge B disagree on who they think is risky, then monotonicity would fail

- $D_i(A) > D_i(B)$ for some defendents, but $D_i(B) > D_i(A)$ for other defendents

If monotonicity fails, we might not have a weighted avearge of LATEs

Intent-to-treat Effect

Say you do not observe the treatment dummy D_i, but only the shifter Z_i. You can still estimate the reduced-form $\hat{\delta}$, but not the first-stage $\hat{\pi}$.

Intent-to-treat Effect

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The reduced-form estimates

$$\hat{\delta} = \mathbb{P}(\text{Complier}_i)\, \mathbb{E}[y_i(1) - y_i(0) \mid \text{Complier}_i]$$

You have the LATE times a proportion between 0 and 1. This is called the Intent-to-treat effect

Intent-to-treat Effect

$$\hat{\delta} = \mathbb{P}(\text{Complier}_i)\, \mathbb{E}[y_i(1) - y_i(0) \mid \text{Complier}_i]$$

If you put bounds on $\mathbb{P}(\text{Complier}_i)$ (e.g. 10-20% of people would be compliers), you can give a range of the LATE (between $10 * \hat{\delta}$ and $20 * \hat{\delta}$)

- This is *very ad-hoc*, but something you could do to put ITT into context
- Be careful with low compliance (i.e. weak instrument) your estimate can be very sensitive