Quantitative Economics for the Evaluation of the European Policy

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Instrumental Variables and Causal Inference

Another way to measure the impact of a program when treatment has not been randomly assigned is by using the instrumental variable (IV) method.

- The IV estimation ragards the treatment variable as **endogenous**.
- The idea: to find an observable exogenous variable or variables (instruments) that affect the participation variable but do not influence the outcome of the program if participating.
- Thus, one would want at leats one instrument that is not in the covariates and that satisifies tha preceding requirements.
- IV estimation is a two steps process:
  1. The treatment variable is run against all covariates, including the instruments;
  2. The predicted value of the treatment, instead of the actual value, is used in the second stage.

Issues with OLS

- We assume we run the following regression:

$$Y_i = \alpha + \beta_1 D_i + \beta_2 X_i + \epsilon_i \tag{1}$$

  where D represents the treatment variable, and X is the vector of control variables (exogenous and observed).

- Why is this not working? Two potential reasons:
  1. The true data generating process is $Y_i = \gamma_0 + \gamma_1 D_i + \gamma_2 X_i + \gamma_3 M_{1i} + \eta_i \Longrightarrow$ Omitted variable bias.
  2. Decision to participate in training is endogenous.

- In these cases $\hat{\beta}_1$ is not asymptotically consistent: the data does not allow telling to what extent the second story is true.

Problem 1: omitted variables

Even if we try to control for "everything", we'll miss:

- Characteristics that we did not know they mattered, and
- Characteristics that are too complicated to measure (not observables or not observed): talent, motivation, level of information and access to services, opportunity cost of participation.
- Full model would be:

$$Y_i = \gamma_0 + \gamma_1 D_i + \gamma_2 X_i + \gamma_3 M_{1i} + \eta_i \tag{2}$$

- But we cannot observe M_1 , the "missing" and unobserved variables.

Omitted variables

- True model is: $Y_i = \gamma_0 + \gamma_1 D_i + \gamma_2 X_i + \gamma_3 M_{1i} + \eta_i$
- But we estimate: $Y_i = \alpha + \beta_1 D_i + \beta_2 X_i + \epsilon_i$
- If there is a correlation between M_1 and D, then the OLS estimator of β_1 will not be a consistent estimator of γ_1, the true impact of D.
- Why? When M_1 is missing from the regression, the coefficient of D will "pick up" some of the effect of M_1.

Problem 2: Endogenous Decision to Participate

- True model is: $Y_i = \gamma_0 + \gamma_1 D_i + \gamma_2 X_i + \gamma_3 M_{1i} + \eta_i$, with

$$D_i = \pi_0 + \pi_1 X_i + \pi_2 M_{1i} + \xi_i \tag{3}$$

  where M_{2i} is the vector of unobserved/missing characteristics (i.e. we do not fully know why people decide to participate).

- Since we do not observe M_{2i}, we can only estimate the simplified model in Eq.(1).

- Is $\beta_{1,OLS}$ an unbiased estimator of γ_1?

Endogenous Decision to Participate

Consider the correlation between the treatment variable and the error term:

$$
\begin{aligned}
Corr(\epsilon, D) &= corr(\epsilon, \pi_0 + \pi_1 X + \pi_2 M_2 + \xi) \\
&= \pi_1 corr(\epsilon, X) + \pi_2 corr(\epsilon, M_2) \\
&= \pi_2 corr(\epsilon, M_2)
\end{aligned}
\tag{4}
$$

- If there is a correlation between the missing variables, that determine participation, and outcomes not explained by observed characteristics, then the OLS estimator will be biased.

What can we do to solve this problem?

- We estimate:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + \epsilon_i \tag{5}$$

- So the problem is the correlation between D and ϵ.
- How about we replace D with "something else", call it **Z**:
  - **Z** needs to be similar to D (correlated to D),
  - But is not correlated with ϵ

$\Longrightarrow$ **Z** can be used as an instrumental variable to measure the effect of D on Y.

Example 1: Vietnam veterans and civilian earnings

- Did military service in Vietnam have a negative effect on earnings? (Angrist, 1990).
- Here we have:
  - Instrumental variable: draft lottery eligibility.
  - Treatment variable: Veteran status.
  - Outcome variable: Log earnings.
  - Data: N=11,637 white men born 1950 – 1953.
  - March Population Surveys of 1979 and 1981 – 1985.
- This lottery was conducted annually during 1970-1974. It assigned numbers (from 1 to 365) to dates of birth in the cohorts being drafted. Men with lowest numbers were called to serve up to a ceiling determined every year by the Department of Defense.
- Abadie (2002) uses as instrument an indicator for lottery numbers lower than 100.

Vietnam veterans and civilian earnings

- The fact that draft eligibility affected the probability of enrollment along with its random nature makes this variable a good candidate to instrument "veteran status".
- There was a strong selection process in the military during the Vietnam period. Presumably, enrollment was influenced by future potential earnings.

Example 2: voluntary job training program

- $D =$ participation;
- $\epsilon =$ that part of outcomes that is not explained by program participation or by observed characteristics;
- We are looking for a variable **Z** that is:
  - Closely related to participation D,
  - but does not directly affect people's outcomes Y, other than through its effect on participation.

Voluntary job training program

- Suppose that a social worker visits unemployed persons to encourage them to participate.
- She/he only visits 50% of persons on her/his roster, and she/he randomly chooses whom she/he will visit.
- If she/he is effective, many people she/he visits will enroll $\Longrightarrow$ There will be a correlation between receiving a visit and enrolling.
- But visit does not have direct effect on outcomes (e.g. income) apart from its effect through enrollment in the training program.
- Randomized "encouragement" or "promotion" visits are an **Instrumental Variable**.

Characteristics of an instrumental variable

- Define a new variable Z equals to 1 if person was randomly chosen to receive the encouragement visit from the social worker; equals to 0 if person was randomly chosen not to receive the encouragement visit from the social worker.
- $Corr(Z, D) > 0$ People who receive the encouragement visit are more likely to participate than those who do not;
- $Corr(Z, \epsilon) = 0$ No correlation between receiving a visit and benefit to the program apart from the effect of the visit on participation.

Two-stage least squares (2SLS)

- Remember the original model with endogenous D:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + \epsilon_i \tag{6}$$

STEP 1

Regress the endogenous variable D on the instrumental variable(s) Z and other exogenous variables:

$$D_i = \delta_0 + \delta_1 X_i + \delta_2 Z_i + \tau_i \tag{7}$$

- Calculate the predicted value of D for each observation;
- Since Z and X are not correlated with ϵ, neither will be D.
- You will need one instrumental variable for each potentially endogenous regressor.

Two-stage least squares (2SLS)

STEP 2

Regress Y on the predicted variable D and the other exogenous variables:

$$Y_i = \beta_0 + \beta_1 \hat{D}_i + \beta_2 X_i + \epsilon_i \tag{8}$$

- Note: The standard errors of the second stage OLS need to be corrected because D is not a fixed regressor.;
- **Intuition**: By using Z for D, we cleaned D of its correlation with η.
- It can be shown that (under certain conditions) $\beta_{1,IV}$ yields a consistent estimator of γ_1 (large sample theory).

Summary of findings on Vietnam draft lottery

- **First stage results**:
  Having a low lottery number (being eligible for the draft) increases veteran status by about 16 percentage points (the mean of veteran status is about 27 percent).

- **Second stage results:**
  Serving in the army lowers earnings by between $2,050$\$ and $2,741$\$ per year.

Identification of causal effects in IV settings

The question is whether the availability of an instrumental variable identifies causal effects. To answer it, we consider a binary Z, and abstract from conditioning.

Homogeneous effects

- If the causal effect is the same for every individual:

$$Y_{1i} - Y_{0i} = \rho$$

- The availability of an IV allows us to identify ρ. This is the traditional situation in econometric models with endogenous explanatory variables.
- In the homogeneous case:

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})D_i = Y_{0i} + \rho D_i$$

- Also, taking into account that $Y_{0i} \perp Z_i$:

$$E(Y_i|Z_i = 1) = E(Y_{0i}) + \rho E(D_i|Z_i = 1)$$
$$E(Y_i|Z_i = 0) = E(Y_{0i}) + \rho E(D_i|Z_i = 0)$$

Identification of causal effects in IV settings

- Subtracting both equations we obtain:

$$\rho = \frac{E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0)}{E(D_i|Z_i = 1) - E(D_i|Z_i = 0)}$$

- which determines ρ as long as:

$$E(D_i|Z_i = 1) \neq E(D_i|Z_i = 0)$$

- Get the effect of D on Y through the effect of Z because Z only affects Y through D.

IV with heterogeneous treatment effects

- Up to this point we only considered models where the causal effect was the same for all individuals (homogenous treatment effects): $Y_{1i} - Y_{0i} = \rho$ for all i
- We now try to understand what IV estimates if treatment effects are heterogeneous.
- This will inform us about two types of validity:
  - Internal validity: Does the design successfully uncover causal effects for the population studied?
  - External validity: Do the studys results inform us about different populations?

Heterogeneous effects

Summary:

- In the heterogeneous case the availability of IVs is not sufficient to identify a causal effect.
- An additional assumption that helps identification of causal effects is the following "**monotonicity**" condition: Any person that was willing to treat if assigned to the control group, would also be prepared to treat if assigned to the treatment group.
- The plausibility of this assumption depends on the context of application.
- Under monotonicity, the IV coefficient coincides with the average treatment effect for those whose value of D would change when changing the value of Z (**local average treatment effe**ct or **LATE**).

Indicator of potential treatment status

- In preparation for the discussion below let us introduce the following notation:

$$D = \begin{cases} D_0, & \text{if Z=0} \\ D_1, & \text{if Z=1} \end{cases}$$

- Given data on (Y ,D) there are 4 observable groups but 8 underlying groups, which can be classified as never-takers, compliers, defiers, and always-takers.

Example

- Consider two levels of schooling ($D = 0, 1$, high school and college) with associated potential wages (Y_0, Y_1), so that individual returns are $Y_1 - Y_0$.
- Also consider an exogenous determinant of schooling Z with associated potential schooling levels (D_0, D_1).

Indicator of potential treatment status

Example con't

- The IV Z is exogenous in the sense that it is independent of (Y_0, Y_1, D_0, D_1).
- An example of Z is proximity to college:
- $Z = 0$ college far away
- $Z = 1$ college nearby
- Defier with D=1, Z=0 (i.e. $D_1 = 0$): Person who goes to college when is far but would not go if it was near.
- Defier with D=0, Z=1 (i.e. $D_0 = 1$): Person does not go to college when it is near but would go if it was far.

Tabella: Observable and Latent Types

	Z	D	D_0	D_1		
Type 1	0	0	0	0	Type 1A	Never-taker
				1	Type 1B	Complier
Type 2	0	1	1	0	Type 2A	Defier
				1	Type 2B	Always-Taker
Type 3	1	0	0	0	Type 3A	Never-taker
			1		Type 3B	Defier
Type 4	1	1	0	1	Type 4A	Complier
			1		Type 4B	Always-Taker

Availability of IV is not sufficient by itself to identify causal effects

- Note that since:

$$E(Y|Z=1) = E(Y_0) + E[(Y_1 - Y_0)D_1]$$
$$E(Y|Z=0) = E(Y_0) + E[(Y_1 - Y_0)D_0]$$

  we have

$$E(Y|Z=1) - E(Y|Z=0) = E[(Y_1 - Y_0)(D_1 - D_0)]$$
$$= E(Y_1 - Y_0|D_1 - D_0 = 1)Pr(D_1 - D_0 = 1)$$
$$-E(Y_1 - Y_0|D_1 - D_0 = -1)Pr(D_1 - D_0 = -1)$$

- $E(Y|Z=1) - E(Y|Z=0)$ could be negative and yet the causal effect be positive for everyone, as long as the probability of defiers is sufficiently large.

IV measure a local average treatment effect

- Under the assumption that there are no defiers (no people who will not attend the training program because of the letter), you can measure a causal effect through randomized experiments even when compliance is imperfect.
- However, this is not the ATET on the whole population that is measured, only the ATE on a given part of the population: the compliers. Hence the name **local average treatment effect** (LATE).

Local average treatment effects (LATE)

Monotonicity and LATEs

- If we rule out defiers i.e. $Pr(D_1 - D_0 = -1) = 0$, we have
$E(Y|Z = 1) - E(Y|Z = 0) = E(Y_1 - Y_0|D_1 - D_0 = 1)Pr(D_1 - D_0 = 1)$
and
$$E(Y|Z = 1) - E(Y|Z = 0) = E(D_1) - E(D_0) = Pr(D_1 - D_0 = 1)$$

- Therefore:
$$E(Y_1 - Y_0|D_1 - D_0 = 1) = \frac{E(Y|Z = 1) - E(Y|Z = 0)}{E(D|Z = 1) - E(D|Z = 0)}$$

- Imbens and Angrist called this parameter "local average treatment effects" (LATE).

Local average treatment effects (LATE)

- Different IV's lead to different parameters, even under instrument validity, which is counter to standard GMM thinking.
- Policy relevance of a LATE parameter depends on the subpopulation of compliers defined by the instrument. Most relevant LATE's are those based on instruments that are policy variables (e.g. college fee policies or college creation).
- What happens if there are no compliers? In the absence of defiers, the probability of compliers satisfies:

$$Pr(D_1 - D_0 = 1) = E(D|Z = 1) - E(D|Z = 0)$$

So, lack of compliers implies lack of instrument relevance, hence underidentification.

Distributions of potential wages for compliers

- Imbens and Rubin (1997) showed that under monotonicity not only the average treatment effect for compliers is identified but also the entire marginal distributions of Y_0 and Y_1 for compliers.
- Abadie (2002) gives a simple proof that suggests a Wald calculation.

Conditional estimation with instrumental variables

- So far we abstracted from the fact that the validity of the instrument may only be conditional on X: It may be that $(Y_0, Y_1) \perp Z$ does not hold, but the following does:

$$(Y_0, Y_1) \perp Z|X \text{ (conditional independence)}$$
$$Z \not\perp D|X \text{ (conditional relevance)}$$

- For example, in the analysis of returns to college where Z is an indicator of proximity to college. The problem is that Z is not randomly assigned but chosen by parents, and this choice may depend on characteristics that subsequently affect wages. The validity of Z may be more credible given family background variables X.

Conditional estimation with instrumental variables

- In a linear version of the problem:
  - First stage: Regress D on Z and X $\Longrightarrow$ get $\hat{D}$.
  - Second stage: Regress Y on $\hat{D}$ and X .
- In general we now have conditional LATE given X
- On the other hand, we have conditional IV estimands:

$$\rho(X) = \frac{E(Y|Z=1,X) - E(Y|Z=0,X)}{E(D|Z=1,X) - E(D|Z=0,X)}$$

IV in Randomized Trials

- The use of IV methods can also be useful when evaluating a randomized trial.
- In many randomized trials, participation is voluntary among those randomly assigned to treatment.
- On the other hand people in the control group usually do not have access to treatment.
  - only those who are particularly likely to benefit from treatment will actually take up treatment (leads almost always to positive selection bias).
  - if you just compare means between treated and untreated individuals (using OLS) you will obtain biased treatment effects.
- **Solution**: Instrument for treatment with whether you were offered treatment. $\Longrightarrow$ you estimate LATE.

Weak Instruments

- IV is consistent but not unbiased.
- For a long time researchers estimating IV models never cared much about the small sample bias.
- In the early 1990s a number of papers, however, highlighted that IV can be severely biased in particular if instruments are weak (i.e. the first stage relationship is weak) and if you use many instruments to instrument for one endogenous variable (i.e. there are many overidentifying restrictions).
- In the worst case, if the instruments are so weak that there is no first stage the 2SLS sampling distribution is centered on the probability limit of OLS.

Weak Instruments - Adding More Instruments

- Adding more weak instruments will increase the bias of 2SLS. By adding further instruments without predictive power the first stage F-statistic goes towards 0 and the bias increases.

What Can You Do If You Have Weak Instruments?

- Use a just identified model with your strongest IV. If the instrument is very weak, however, your standard errors will probably be very large.
- Use a limited information maximum likelihood estimator (LIML). This is approximately median unbiased for overidentied constant effects models. It provides the same asymptotic distribution as 2SLS (under constant effects) but provides a finite-sample bias reduction.
- Find stronger instruments.

Practical Tips For IV Papers

- *Report the first stage.*
  - Does it make sense?
  - Do the coefficients have the right magnitude and sign?
- *Report the F-statistic on the excluded instrument(s).*
  - Stock, Wright, and Yogo (2002) suggest that F-statistics above 10 indicate that you do not have a weak instrument problem (but this is of course not a proof).
  - If you have more than one endogenous regressor for which you want to instrument, reporting the first stage F-statistic is not enough (because 1 instrument could affect both endogenous variables and the other could have no effect - the model would be underidentified).

Practical Tips For IV Papers

- If you have many IVs *pick your best instrument* and report the just identified model (weak instrument problem is much less problematic).
- *Check overidentified 2SLS models with LIML.*
- *Look at the Reduced Form.*
  - The reduced form is estimated with OLS and is therefore unbiased.
  - If you ca not see the causal relationship of interest in the reduced form it is probably not there.

Summary

- Causal relationship of interest:

$$Y = \alpha + \rho D_i + \eta_i$$

- First-Stage regression:

$$D_i = \alpha + \gamma Z_i + \zeta_i$$

- Second-Stage regression:

$$Y_i = \alpha + \rho \hat{D}_i + \nu_i$$

- Reduced form:

$$Y_i = \alpha + \delta Z_i + \epsilon_i$$