More on instrumental variables and "Natural Experiments"

Throughout, we consider the simplest case of a linear outcome equation, and homogeneous effects:

$$y = \beta x + \epsilon \tag{1}$$

where y is some outcome, x is an explanatory variable, and ϵ is an unobservable which represent unobserved determinants of y not accounted for in x. Consider simplest case: just one explanatory variable. Here β measures the *causal effect* of a unitary change in x[1] on the outcome y. Examples: (y is wages, x is yrs of schooling), (y is quantity demanded, x is price), (y is price, x is market concentration), (y is test scores, x is class size), etc.

You want to estimate β. But if x is endogenous (in the sense that $E(\epsilon x) \neq 0$) then OLS estimate is biased. Two ways out:

1. Find an IV for x: roughly speaking, correlated with x, uncorrelated with ϵ, and (of course) excluded from the equation of interest (1)
2. Find an *experimental* (or quasi-experimental) situation where one could plausibly claim that x is exogenous (in the sense of being uncorrelated with the unobservable ϵ).

Basically speaking, a *natural experiment* is a *discrete* (usually binary) variable z which fulfills one of the functions above.

1 Natural experiments as IV

Estimation of β using **Wald estimator** $(z'x)^{-1}(z'y)$. Justification is because population analog is

$$\frac{cov(y,z)}{cov(x,z)} = \frac{cov(x,z)\beta + cov(\epsilon,z)}{cov(x,z)} = \beta$$

when z is valid instrument.

For a binary z, Wald estimator becomes

$$\frac{\frac{\sum_{i=1}^n y_i z_i}{\sum_{i=1}^n z_i} - \frac{\sum_{i=1}^n y_i(1-z_i)}{\sum_{i=1}^n (1-z_i)}}{\frac{\sum_{i=1}^n x_i z_i}{\sum_{i=1}^n z_i} - \frac{\sum_{i=1}^n x_i(1-z_i)}{\sum_{i=1}^n (1-z_i)}}.$$

Examples:

Angrist and Krueger (1991) y is wages, x is yrs of schooling, z is quarter of birth (0=Jan-March, 1=April-Dec of previous year).[2] Exploits two institutional features:

[1] If x is a continuous variable, then β is a *marginal* effect.

[2] Angrist and Krueger do the analysis for each birth cohort separately.

(i) can only enter school when you are 5 yrs old by Dec. 31; (ii) must remain in school until age 16 $\Longrightarrow$ people with $z = 1$ forced to complete more yrs of schooling before they can drop out.[3]

Angrist (1990) y is lifetime income, x is years of experience in the (civilian) workforce, and z is draft eligibility. Intuition: that draft eligibility led to exogenous shift in years of experience.

Angrist, Graddy, and Imbens (2000) y is quantity demanded, x is price, and z is weather variable.

Angrist and Evans (1990) y is parents' labor supply, x is number of children, z is indicator of sex composition of children (i.e., whether first two births were females)

When β varies across individuals ("random coefficients") then, given the additional **monotonicity assumption**, the Wald estimator measures the *average* effect of x on y for those for whom a change in z from 0 to 1 would have affected the treatment x. This additional monotonicity assumption says, roughly, that $x(z)$, which expresses the treatment x as a function of the instrument z, is *nondecreasing* in z. That is, when z moves from 0 to 1, then the treatment x either stays unchanged, or also moves from 0 to 1. This is an assumption on the unobserved counterfactuals: Let's say you observe an individual with $z = 0$ and $x = 1$. If his z were to change to $z = 1$, then the monotonicity assumption says that his x must stay at 1; cannot go down to 0. See Angrist and Imbens (1994) for more details.

For example, in the schooling/wages example, Wald estimator measures effect of an extra year of schooling on those (dropout) students for whom an earlier birth (ie. change z from 0 to 1) would have been forced to complete an extra year of schooling before dropping out. This insight is known by several terms, including *local IV* and *local average treatment effect (LATE)*.

2 Natural experiments as exogenous change in x

In these situations, you find a setting where x is exogenous. Here you could just estimate equation (1) directly. Usually, a more sophisticated approach is taken.

In many cases, x is across-time (but only affects a subset of the population), with a panel dataset. For example, x is often a policy change, such as rise in the minimum wage, which affects some states but not others. Estimation of (1) usually proceeds via the regression:

$$y_{it} = \alpha_i + \beta x_{it} + \gamma_t + \epsilon_{it}$$

Let $\hat{\beta}$ denote our estimate of β from this equation. Given the exogeneity and mean-zero assumptions on ϵ, we can interpret $\hat{\beta}$ in the following manner.

[3]Note that if compulsory schooling were described in terms of *years of schooling*, then identification strategy fails.

For simplicity, assume two periods, t and t'. Assume the policy is enacted in period t', and let x be a binary variable, which turns to 1 when the policy is enacted. Let i denote a cross-sectional unit which experienced the shift in x, and j denote a unit which did not experience such a shift. If we subtract (or "difference") the expected outcome equations for units i and j between periods t and t', we get rid of the unit-specific fixed effects:

$$\begin{aligned} E\left(y_{it'} - y_{it} | x_{it}, x_{it'}\right) &= \hat{\beta} + \left(\hat{\gamma}_t - \hat{\gamma}_{t'}\right) \\ E\left(y_{jt'} - y_{jt} | x_{jt}, x_{jt'}\right) &= \left(\hat{\gamma}_t - \hat{\gamma}_{t'}\right). \end{aligned} \tag{2}$$

Now difference the two equations in (2):

$$E\left[\left(y_{it'} - y_{it}\right) - \left(y_{jt'} - y_{jt}\right) | \cdots\right] = \hat{\beta} \tag{3}$$

so that $\hat{\beta}$ is a **difference-in-difference** estimate of the effect of a change in x on y.

In practice, you construct the diff-in-diff estimator by replacing the expectations with sample averages:

$$\hat{\beta}_n = \left(\bar{y}_{1t'} - \bar{y}_{1t}\right) - \left(\bar{y}_{0t'} - \bar{y}_{0t}\right)$$

where the bars denote sample averages, and the subscript '1" denotes the cross-sectional units who experienced the policy shift, and "0" the units that did not. Sample averages are, eg. $\bar{y}_{1t} = \sum_{i \in \mathcal{I}_1} y_{it}/|\mathcal{I}_1|$.

There are numerous examples of this. Two examples are:

Card and Krueger (1994) y is employment, x is minimum wage (look for evidence of general equilibrium effects of minimum wage). Exploit policy shift which resulted in rise of minimum wage in New Jersey, but not in Pennsylvania. Sample is fast food restaurants on the NJ/Pennsylvania border.

Kim and Singal (1993) y is price, x is concentration of particular flight market. Exploit merger of Northwest and Republic airlines, which affected only markets (so we hope) in which Northwest or Republic offered flights.

A related idea is exploiting a *regression discontinuity* for exogenous variation in the treatment. The idea is that, in many institutional settings, whether an individual undergoes a treatment is determined by some "scoring" variable Z, and the treatment rule is a threshold one:

$$\text{treatment} = \begin{cases} 0 & \text{if } Z \leq \bar{Z} \\ 1 & \text{if } Z > \bar{Z} \end{cases}$$

The idea is to compare the outcome variable Y for individuals with Z "just under" $\bar{Z}$, to those with Z just above $\bar{Z}$. The difference $E[Y|Z+] - E[Y|Z-]$ is an estimate of the treatment effect.

Examples:

Angrist and Lavy (1999) y is test scores, z is class size, and $\bar{Z}$ is multiple of 40. Maimonides' rules states (roughly) that no class size should exceed forty, so that if enrollment (treated as exogenous) is "just below" 40, class sizes will be bigger, whereas if enrollment is "just above" 40, class sizes will be smaller.

They restrict their sample to all (school-cohorts) where total enrollment was within +/- 5 of a multiple of 40.

David Lee y is number of crimes, z is < 18 vs > 18. Look at the deterrence effect of punishment on crime.

Van der Klaauw y is college performance, x is whether you for financial aid, z is SAT score

More formally: Consider a binary treatment case $X \in 0, 1$. Define Y_0 as the outcome under $X = 0$, and Y_1 as outcome under $X = 1$. Hence observed Y is equal to

$$Y = X * Y_1 + (1 - X) * Y_0.$$

Discontinuity assumption implies

$$P(X = 1|Z) = \begin{cases} 0 & \text{if } Z < \bar{Z} \\ 1 & \text{if } Z \geq \bar{Z}. \end{cases}$$

Assumption (*): for $i = 0, 1$, $E[Y_i|Z]$ is continuous at $\bar{Z}$.

Then

$$\begin{aligned} & E[Y|Z \in (\bar{Z}, \bar{Z} + \epsilon)] - E[Y|Z \in (\bar{Z} - \epsilon, \bar{Z})] \\ & = E[Y_1|Z \in (\bar{Z}, \bar{Z} + \epsilon)] - E[Y_0|Z \in (\bar{Z} - \epsilon, \bar{Z})] \\ & \longrightarrow_{\epsilon \to 0} E[Y_1 - Y_0|\bar{Z}] \end{aligned}$$

Interpretation of assumption (*): the Z variable is exogenous, in the sense that individuals which very high values of Y_1 cannot cause themselves to have high values of Z (so that treatment is chosen).

Note: measured treatment effect is very "local" (only at $\bar{Z}$). Effect could be very different, away from $\bar{Z}$.

Regression discontinuity idea can be interpreted "geographically", where $\bar{Z}$ denotes some geographical border. (eg. state boundary, city boundary, river).

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