Micro Econometrics

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Lecture 1: Regression

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1 Regression

[L1 Regression]

Regression Fundamentals

As an empiricist, differences in economic fortune are hard to explain, as applied econometricians, we believe we can summarise and interpret 'randomness' in a useful way.

Whilst expectation is a population concept, in practice we have samples which rarely consist of the entire population. We use these to make inferences about he population, so the sample CEF is used to learn about the population CEF.

Law of Iterated Expectations An unconditional expectation can be written as the unconditional average of the CEF. Or,

$$E[Y_i] = E\{E[Y_i|X_i]\}$$

Where the outer expectation uses the distribution of X_i

> **Theorem 1 :**
> 3.1.1 CEF Decomposition property
> $$Y_i = E[Y_i|X_i] + \varepsilon_i$$
> Where ε_i is mean independent of X_i, that is $E[\varepsilon_i|X_i] = 0$ and therefore ε_i is uncorrelated with any function of X_i

Regression

Studying the relationship between one (dependent) variable y and k other independent variables x_j, $(j = 1, \ldots, k)$

1. Does the Covid vaccine work
2. What are the returns to schooling
3. What is the effect of having internet at home on student's grades
4. Does a job training program decrease the time of getting out of unemployment

We are often interested in a single variable, including other regressors as controls

1.1 Classical Linear Model

Linear regression

- Relies on 5 main Gauss-Markov assumptions
- In small samples is unbiased and BLUE
- In large samples is consistent and asymptotically normal. There is no need for the normality assumption to establish asymptotic distribution

The key assumption for OLS to consistently estimate β is the **population orthogonality condition**

If x contains years of schooling and experience, and the main component of u is innate ability then this assumption implies that ability is uncorrelated with education and experience in the population.

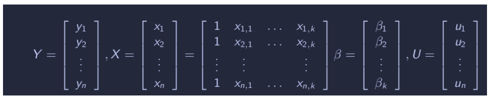
By the LIE, sufficient for this assumption is $E[u^2|x] = \sigma^2$ which is the same as $\text{Var}(u|x) = \sigma^2$ when $E[u|x] = 0$

1.2 Multiple Regression

Classical Linear Model

$$\begin{aligned} Y_i = \beta_0 + \beta_1 x_{i,1} + \beta_2 x_{i,2} + \ldots + \beta_k x_{i,k} + u_i, i = 1, \ldots, n \\ = x_i \beta + u_i \quad \text{vector notation} \\ Y = X\beta + U \quad \text{matrix notation.} \end{aligned}$$

1. Where β_0 is the intercept, β_j is the parameter (slope) associated with x_j
2. A is the unobserved error term : containing factors other than x_j 's explaining y
3. n is the number of observations



$$Y = \begin{bmatrix} y_1 \\ y_2 \\ \vdots \\ y_n \end{bmatrix}, X = \begin{bmatrix} x_1 \\ x_2 \\ \vdots \\ x_n \end{bmatrix} = \begin{bmatrix} 1 & x_{1,1} & \ldots & x_{1,k} \\ 1 & x_{2,1} & \ldots & x_{2,k} \\ \vdots & \vdots & & \vdots \\ 1 & x_{n,1} & \ldots & x_{n,k} \end{bmatrix} \beta = \begin{bmatrix} \beta_1 \\ \beta_2 \\ \vdots \\ \beta_k \end{bmatrix}, U = \begin{bmatrix} u_1 \\ u_2 \\ \vdots \\ u_n \end{bmatrix}$$

Least Squares Estimator **Objective**: to estimate the effect of x_j on y, we need to estimate the population parameters $\beta_0, \ldots, \beta_k$

Ordinary Least squares estimates β by minimising the sum of squared residuals :

$$\hat{\beta} = argmin \sum_{i=1}^{n} (y_i - \beta_0 - \beta_1 x_{i,1} - \ldots - \beta_k x_{i,k})^2 = \|Y - X\beta\|^2$$

Taking first order conditions

$$\hat{\beta} = (X'X)^{-1} X'Y$$

It can be shown that

1. Residuals : $\hat{u}_i = y_i - x_i \beta$ with $\sum_{i=1}^{n} \hat{u}_i = 0$
2. Fitted values : $\hat{y}_i = x_i \beta$

It can be shown that in the single regressor model where

$y = \beta_0 + \beta_1 x_1 + u$

$$\begin{aligned}\hat{\beta}_1 &= \frac{\sum_{i=1}^{n}(x_{i,1} - \overline{x}_1(y_i - \overline{y})}{\sum_{i=1}^{n}(x_{i,1} - \overline{x}_1)^2} \\ &= cov\frac{x_1, y}{\hat{v}(x_1)}\hat{\beta}_0 = \overline{y} - \beta_1\hat{\overline{x}}_1\end{aligned}$$

1.3 Gauss Markov Assumptions

Assumption 1 (Linear in parameters (*MLR.1*)) The model in the population can be written as

$$Y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \ldots + B_k x_k + u$$

Where $\beta_0, \beta_1, \ldots \beta_k$ are the unknown parameters (constants) of interest and u is an unobservable random error or disturbance term

Assumption 2 (Random Sampling (*MLR.2*)) We have a random sample of n observations, following the population model assumption in MLR.1.

- Often referred to as IID assumption
- Ensures that our sample's representative for the population
- Would fail if we observed only part of the population in our sample

Assumption 3 (No perfect colinearity *MLR.3*) In the sample (thus pop too), none of the independent variables are constant, and there are no exact linear relationships between the independent variables

- Often referred to as full rank assumption
- Dummy variable trap - not to include a binary for both male and female
- Not to confuse with highly but not perfectly correlated variables (multicollinearity)

Assumption 4 (zero conditional mean *MLR.4*) The error u has an expected value of zero given any value of the explanatory variable,

$$E[u|x_1, x_2, \ldots, x_k] = 0$$

- Key to deriving unbiasedness
- If it holds for variable x_j, the variable is exogenous
- Requires at a minimum : all factors in the observed error term must be uncorrelated with the explanatory variables
- Any problem that causes u to be correlated with any of the x_j 's causes this assumption to fail and OLS to be biased !
- Examples for endogeneity : misspecified functional form, omitting important variables, measurement error and any x_j being jointly determined with y

Assumption 5 (Homoskedacity *MLR.5*) The error u has the same variance given any value of explanatory variables, in other words

$$V[u|x_1, \ldots x_k] = \sigma^2$$

- The variance of the unobserved error u conditional on the explanatory variables is the same for all combinations of the outcomes of the explanatory variables
- If this assumption fails, we speak of heteroskedatic errors
- This assumption is not needed for unbiased/ consistency but for efficiency of OLS
- This also means that $V[y|x] = \sigma^2$

1.4 Small Sample Properties

Unbiasedness of Ols

Under assumption 1-4, the OLS estimator is unbiased

$$E[\hat{\beta}_j] = \beta_j \text{for } j = 0, \ldots, k$$

For any values of the population parameter β_j.

Or, we say the OLS estimators are unbiased estimators of the population parameters

However,

- Might not exactly be the population value
- Deviations from the population value are not systematic
- If we were to repeat the estimation on several random samples the deviations should average out to zero

Variance

Sampling variance of the OLS slope estimators

Under assumptions 1-5, conditional on the sample values of the independent variables the variance is

$$V[\hat{\beta}_j] = \frac{\sigma^2}{SST_j(1 - R_j^2} \text{for} j = 0, \ldots, k$$

Where $SST_j = \sum_{i=1}^{n}(x_{ij} - \overline{x}_j)^2$ is the sum of total sample variation in x_j and R_j^2 is the R-squared from regressing x_j on all other independent variables (including an intercept)

- The standard error formulas make it apparent that we need variation in the regress ors to increase precision
- The R_j^2 representation makes it also apparent that a high multicollinearity increases the variance of the estimator

Matrix Representation General formula in matrix form (including the intercept)

$$V[\hat{B}_j] = \sigma^2(X'X)^{-1}$$

The variance of the j-the parameter estimate

$$\sigma^2(X'X)^{-1}_{[j+1,j+1}$$

Gauss Markov Theorem

Theorem 2 : Gauss Markov Theorem
Under assumptions 1-5 $\hat{\beta}_0, \hat{\beta}_1, \ldots, \hat{\beta}_k$ are the *best linear unbiased estimators* (BLUE)s of

$$\beta_0, \beta_1, \ldots, \beta_k$$

- If the assumptions hold, we do not need to look for another unbiased estimator since this is the best
- Best meaning the most efficient, with smallest variance

A more general (and asymptotic) version of the GM theorem ([**JW-CS- 14**]). While stating that OLS has the smallest variance in the class of linear, unbiased estimators, it does not allow us to compare OLS to unbiased estimators that are not linear in the vector of observations on the dependent variable

Theorem 3 : (Efficinency in a Class of Estimators)
Let $\hat{\theta_\tau} : \tau \in \mathcal{F}$ be a class of $\sqrt{N}$ -asymptotically normal estimators with variances matrix's of the form $V = A^{-1}E[s(w)s(w)'](A')^{-1}$

If for some $\tau* \in \mathcal{F}$ and $\rho > 0$

$$E[s_\tau(w)s_{\tau*}(w)'] = \rho A_\tau \qquad \text{all} \quad t \in \mathcal{F} \tag{1}$$

Then $\hat{\theta_{\tau*}}$ is asymptotically relatively efficient in the class $\{\hat{\theta_\tau} : \tau \in \mathcal{F}\}$

That is, if we specify a class of estimators by defining the index set $\mathcal{F}$ then the estimator $\hat{\theta_\tau}$ is more efficient than all other estimators in the class if we can show this

Small Sample Inference Error Variance Estimation

Since we are interested in performing inference, we need : variance (standard error) and the distribution of parameter estimator

Firstly Estimation of the error variance : $\sigma^2 = \frac{1}{n-k-1}\sum_{i=1}^{n}\hat{u}_i^2 = \frac{SSR}{n-k-1}$

We can show this estimator is unbiased under

Theorem 4 : unbiased estimation of σ^2
Under the GM assumptions (1-5),

$$E[\hat{\sigma^2}] = \sigma^2$$

Standard Errors The $\sqrt{\text{variance}}$

$$Sd(\hat{\beta}_j) = \sqrt{\frac{\sigma^2}{SST_j(1 - r_j^2}}$$

$$Se(\hat{\beta}_j) = \sqrt{\frac{\sigma^2}{SST_j(1 - R_j^2}} = \frac{\hat{\sigma}}{\sqrt{n} sd(x_j)\sqrt{1 - R_j^2}} \quad \text{where} sd(x_j) = \sqrt{n^{-1}\sum_i (x_{ij} - \overline{x}_j)^2}$$

Standard errors shrink to zero at the rate $\frac{1}{\sqrt{n}}$ (since in denominator)

Assumption 6 (Normality MLR.6) The population error u is independent of the explanatory variables $x_1, x_2, \ldots, x_k$ and is normally distributed with zero mean and variance $\sigma^2 : u \sim \mathcal{N}(0, \sigma^2)$

This is a stronger assumption than 1-5 and means we are necessarily assuming zero conditional mean (4) and homoskedacity (5).

Theorem 5 : Normal Sampling Distributions
Under assumptions 1-6, conditional on the sample values of the independent variables

$$\hat{\beta}_j \sim \mathcal{N}(\beta_j, V(\hat{\beta}_j)$$

Where $V[\hat{B}_j] = \sigma^2(X'X)^{-1}$

Therefore,

$$(\hat{\beta}_j - \beta_j)/sd(\hat{\beta}_j) \sim \mathcal{N}(0, 1)$$

Or, $\hat{\beta}|X \sim MVN(\beta, \sigma^2(X'X)^{-1}$ (matrix notation)

1.5 Asymptotic Properties

Consistency

Assumption 7 (Zero Mean and Zero correlation *(MLR.4')*)

$$E[u] = 0 \;\; and \;\; Cov[x_j, u] = 0, \quad \text{for} \;\; j = 1, 2, \ldots, k.$$

- If we are only interested in consistency : this replace zero conditional mean (MLR.4)
- However, zero conditional mean important for finite sample and to ensure that we have properly modelled the population regression function $E[y|x] = \beta_0 + \beta_1 x_1 + \ldots + \beta_k x_k$
- This gives us the average or partial effects of x_j on the expected value of y.

Theorem 6 : Consistency of OLS
Under assumptions MLR.1 - MLR.4, (or replacing 4 with 7), the OLS estimator $\hat{\beta}_j$ is consistent for β_j for all $j = 1, 2, \ldots, k$

Consistency means that when n goes to ∞, the estimator will recover the population value in probability :

$$\operatorname*{plim}_{n\to\infty} \hat{\beta}_j = \beta_j$$

Essentially, the asymptotic bias shrinks to 0.

For the simple model with one regressor : $y_i = \beta_0 + \beta_1 x_{i,1} + u_i$, to show consistency :

1. Write down the formula for $\hat{\beta}_1$ and plug in y_i :

$$\hat{\beta}_1 = (\sum_{i=1}^{n}(x_{i,1} - \overline{x})(y_i - \overline{y})/(\sum_{i=1}^{n}(x_{i,1} - \overline{x}_1)^2)$$
$$= \beta_1 + (\frac{1}{n}\sum_{i=1}^{n}(x_{i,1} - \overline{x}_1)(u_i - \overline{u})/(\frac{1}{n}\sum_{i=1}^{n}(x_{i,1} - \overline{x}_1)^2)$$

2. Apply the LLN :

$$\underset{n\to\infty}{plim}\hat{\beta}_j = \beta_1 + \frac{cov[u, x_1]}{V(x_1)} = \beta_1$$

Since $Cov[u, x_1] = 0$ (previous assumption)

However, we need to assume finite moments for the LLN to hold. Since it assumes iid observations

LLN : $\bar{X}_n \xrightarrow{P} X$ as $n \to \infty$

Remarks

- In a single regressor model : $\beta_1 = \frac{Cov(y,x_1)}{V(x_1)}$
- Including more regressor changes this expression for the population estimate β_j but since the effect of the other covariates is partialled out, we still recover β_j
- Multicollinearity only affects the variance of the estimator but not consistency

Theorem 7 : Asymptotic Normality of OLS
Under the Gauss-Markov assumptions (1-5),

1. $\sqrt{n}(\hat{\beta}_j - \beta_j) \sim \mathcal{N}(\beta_j, \frac{\sigma^2}{a_j^2})$ where $(\frac{\sigma^2}{a_j^2} > 0$ is the asymptotic variance of $\sqrt{n}(\hat{\beta}_j - \beta_j)$: for the slope coefficients, $a_j^2 = plim\frac{1}{n}\sum_{i=1}^{n}\hat{r}_{ij}^2$ where the $\hat{r}_{ij}^2$ are the residuals from regressing x_{ji} on the other independent variables. And we can say that $\hat{\beta}_j$ is asymptotically normally distributed
2. $hat\sigma^2$ is a consistent estimator of $\sigma^2 = V(u)$
3. For each j, $(\frac{\hat{\beta}_j - \beta_j)}{sd(\hat{\beta}_j} \sim \mathcal{N}(0, 1)$ (where sd unobserved)
4. For each j, $(\frac{\hat{\beta}_j - \beta_j)}{se(\hat{\beta}_j} \sim \mathcal{N}(0, 1)$ (where se estimated)

Where $se(\hat{\beta}_j)$ is the usual OLS estimator

Matrix Form

$$\sqrt{n}(\beta \hat{-} \beta \sim \mathcal{N}[0, \sigma^2(plim\frac{X'X}{n})^{-1}]$$

With $plim\frac{X'X}{n} = E[x'x]$

- For convergence, one needs the asymptotic normalisation $\sqrt{n}$
- However we are interested in the variance of $\hat{\beta}$. For estimation, we use the sample analog of the variance covariance and remove the asymptotic normalisation again by dividing by n
- We obtain the asymptotic variance : $\hat{AV} = \hat{\beta} = \sigma^2(X'X)^{-1}$

This is a very important result for inference. The normality assumption is not needed in large sample. Therefore, regardless of the error distribution, if properly standardised, we have approximate normal standard distributions. We can use the (unobserved) $sd(\hat{\beta}_j$ or the observed $se(\beta_j)$ to achieve this result, where we can estimate the latter since it depends on $\hat{\sigma}^2$

Then because the t distribution approaches the normal distribution for large df, we can also say that $(\widehat{\beta_j - \beta_j})/se(\hat{\beta_j} \sim t_{(n-k-1)}$. But we still need homoskedacity, and with large sample, all the testing issues still apply.

Partialling Out

Intuitively, $\hat{\beta_1}$ measures the sample relationship between y and x_1 after the other regressors have been partialled out

1. Model :
$$Y = \beta_0 + \beta_1 x_1 + \ldots + \beta_k x_k + u, \quad E[u, x_1, \ldots, x_k] = 0$$
2. Regress $x_1 \sim x_1 + x_2 + \ldots + x_k$ and compute the residual $\hat{r_{i1}}$
3. Regress $y \sim \hat{r_1}$ which yields the OLS estimate $\hat{\beta_1}$
4. One can show that the resulting OLS estimator from the regression in 3, equals the OLS estimator for β_1 from a regression based on the model in 1

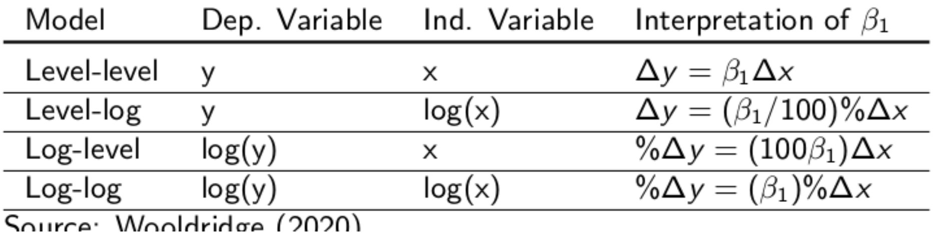
In general form, this is called the **Frisch-Waugh Theorem**

Also giving us the regression anatomy formula :

$$\beta_j = \frac{Cov(y_i, \hat{r}_{i,j}}{V(\hat{r}_{i,j}}$$

1.6 Interpretation And Modelling

1. $y = \beta_0 + \beta_1 x_1 + u$
2. $y = \beta_0 + \beta_1 \log(x) + u$
3. $\log(y) = \beta_0 + \beta_1 x + u$
4. $\log(y) = \beta_0 + \beta_1 \log(x) + u$



Model	Dep. Variable	Ind. Variable	Interpretation of β_1
Level-level	y	x	$\Delta y = \beta_1 \Delta x$
Level-log	y	log(x)	$\Delta y = (\beta_1/100)\%\Delta x$
Log-level	log(y)	x	$\%\Delta y = (100\beta_1)\Delta x$
Log-log	log(y)	log(x)	$\%\Delta y = (\beta_1)\%\Delta x$

Source: Wooldridge (2020)

Figure 1: Interpretation And Log Transformation Table

Linear Probability Model

Model $y = \beta_0 + \beta_1 x + u$ such that $y = \{0, 1\}$ is a binary dependent variable

- Since y is binary

$$E[y|x] = P(y = 1|x) = \beta_0 + \beta_1 x$$
$$1 - E[y|x] = P(y = 0|x) = 1 - \beta_0 - \beta_1 x$$

With marginal effects

$$\frac{\partial E[y|x]}{\partial x} = \beta_1$$

- The predicted values are probabilities of the outcome being equal to 1
- Interpretation : β_1 is the change in the probability that $y = 1$ for a 1 unit increase in x_1 (percentage points)

Aside

- Pros are the estimation and interpretation is straight forward
- Cons are for prediction, the predicted probabilities can be outside the interval $[0, 1]$
- Another con is the errors are Heteroskedacity and hence violate the gauss Markov assumption

$$V(y|x) = P(y = 1|x)(1 - P(y = 1|x))$$

Note other binary dependent variable models are probit and logit

Binary Regressors

Model

$$Y + \beta_0 + \beta_1 x + u \quad E[x|u] = 0$$

Where

- $x \in \{0, 1\}$ is binary variable
- Often also referred to as 'dummy' variable
- Example : effect of gender on hourly wage. Let x = 1 if the individual is a woman (x = 0 man)
- Often used to evaluate a treatment effect such as the effect of an intervention, a policy, a program
- OLS results in comparing group averages

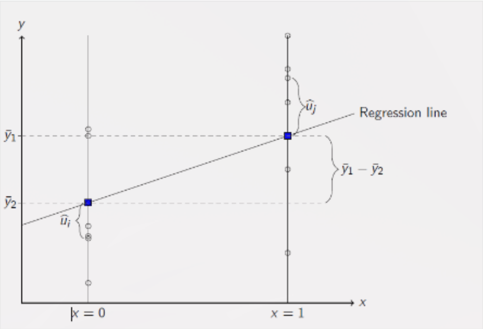


Figure 2: Relationship between y and x when x is binary

The white circles represent typical datapoints and the blue rectangles represent sample averages.

The average effect on y is the difference between the averages of both groups

Population Raw Differential

The CEF for $x = 1$ and $x = 0$:

$$\begin{aligned} \mu_1 \equiv E[y|x=1] &= \beta_0 + \beta_1 + E[u|x=1] \\ \mu_0 \equiv E[y|x=0] &= \beta_0 + E[u|x=0] \\ &\text{under zero conditional mean:} \\ E[y|x=1] &= \beta_0 + \beta_1 \\ E[y|x=0] &= \beta_0 \\ &\text{hence} \\ \boldsymbol{\beta_1} \mathbf{E[y|x=1] - E[y|x=0]} \end{aligned}$$

Where the parameter to be estimated by OLS is the **population raw differential**

Sample Raw Differential Now we can replace the conditional expectations by their sample analogue, that the conditional expectation of y for women is the mean outcome of women and the conditional expectation of y for men is the mean outcome of men.

- Replacing the population means by their sample averages, we obtain the OLS estimators
- $\hat{\beta}_1$ is the **sample raw differential**

$$\hat{\beta}_1 = \frac{1}{n_1} \sum_{i:x_i=1} y_i - \frac{1}{n_0} \sum_{i:x_i=1} y_i = \overline{y}_1 - \overline{y}_0 \quad , \quad \hat{\beta}_0 = \overline{y}_0$$

Where n_1 (n_0) is the number of observations with x = 1 (x = 0)

Let $\widehat{\beta}_0 = 7$ and $\widehat{\beta}_1 = -2.5$ then men earn on average 7 GBP per hour, and women earn on average 2.5 GBP per hour less than the *average* man

1.7 Timeout: Testing Equal Means

To test whether the population means for 2 sub-samples are the same

$$H_0 = E[y|x=1] = E[y|x=0] = \equiv \beta_1 = 0$$

With Test stat :

$$\hat{\beta}_1 / se(\hat{\beta}_1 = \frac{\overline{y}_1 - \overline{y}_0}{\sqrt{\hat{\sigma_1^2}/n_1 + \hat{\sigma_0^2}/n_0}}$$

Where $\hat{\sigma^2}$ are the estimated group specific error variances

(y) And Binary Regressors

Model : $\log(y) = \beta_0 + \beta_1 x$ for $x \in \{0, 1\}$

$$\log(y) = \begin{cases} \log(y_1) = \beta_0 + \beta_1 + u & \text{if } x = 1 \\ \log(y_0) = \beta_0 + u & \text{if } x = 0 \end{cases}$$

Log Point Interpretation If the resulting $\%\Delta y/100$ is small :

$$\begin{aligned}\beta_1 = \log(y_1) - \log(y_0) = \log(\frac{y_1}{y_0}) &= \log\left(1 + \frac{y_1 - y_0}{y_0}\right) \\ &= \log(1 + \frac{\%\Delta y}{100}) \approx \%\Delta y/100 \\ 100\beta_1 &\approx \%\Delta y\end{aligned}$$

If $\%\Delta y/100$ is small, one can interpret β_1 as the *raw differential*

Exact Interpretation - Percentage Change

$$\frac{\Delta y}{y_0} = \frac{y_1 - y_0}{y_0} = \frac{y_1}{y_0} - 1 = frac \exp beta_0 + \beta_1 + u \exp(\beta_0 + u - 1 = \exp(\beta_1) - 1$$

Then, plugging the estimate into the equation

$$\%\Delta y = 100[\exp(\hat{\beta_1}) - 1]$$

interpretation - $\frac{\delta y}{y_0} = -0.26$ means that a woman's wage is 26% below that of a comparable man's wage

Categorical Regressors

- Some characteristics such as regions are originally categorical
- We can render categorical variables based on an originally continuous variable, say bins based on firm size
- The solution is to create multi-category dummies d_k to account for differential effects
- Say the effect of law school ranking on median starting salaries, $\ln(y_i)$ where they found better ranked schools result in higher wages

In order to estimate whether there is a differential effect for the different ranks include the categories as dummies, say

$$D_{i,1} = \begin{cases} 1, \; if \; 1 \leqslant rank \leqslant 10 \\ 0, \; otherwise \end{cases} \quad \vdots D_{i,6} = \begin{cases} 1, \; if \; rank > 100 \\ 0, \; otherwise \end{cases}$$

The model

$$\ln(y_i) = \beta_0 + \beta_1 d_{i,1} + \ldots \beta_5 d_{i,5} + x\gamma + u_i$$

Where we exclude one dummy $d_{i,6}$ to avoid perfect-collinearity (dummy var trap)

Interpretation

- Usual interpretation for a binary regressor wrt base category
- $\beta_j = E[\ln(y)|d_j = 1] - E[\ln(y_i)|d_6 = 1]$ for $j = 1, \ldots, 5$
- β_0 : log median starting salary for the omitted (base) category, the largest rank category
- Estimated percentage change for the frist rank bin compared to the largest bin 101.29%

Interaction Terms

Model : $y + \beta_0 + \beta_1 x_1 + \beta_2 d + \beta_3 x_1 \times d + u$

- Where x_1 is continuous
- Then the effect of x_1 is different for each group : $\frac{\partial E[y|x_1,d]}{\partial x_1} = \beta_1 + \beta_3 \times d$
- If d = 1, then $\frac{\partial E[y|x_1,d]}{\partial x_1} = \beta_1 + \beta_3$
- If d = 0, then $\frac{\partial E[y|x_1,d]}{\partial x_1} = \beta_1$
- If d=1 for women, then a unit increase in x_1 leads to a $\beta_1 + \beta_3$ increase for women and an increase for men of β_1. That is, the returns to x_1 are for women β_3 higher.
- If the independent variable is in log, we can interpret the coefficient as 1oo*[parameter]%

Slightly different model, where both regressors are binary, $y = \beta_0 + \beta_1 d_1 + \beta_2 d_2 + \beta_3 d_1 \times d_2 + u$

- We can compute expectation for each case:

$$\begin{aligned} E[y|d_1 = 0, d_2 = 0] &= \beta_0 \\ E[y|d_1 = 0, d_2 = 1] &= \beta_0 + \beta_2 \\ E[y|d_1 = 1, d_2 = 0] &= \beta_0 + \beta_1 \\ E[y|d_1 = 1, d_2 = 1] &= \beta_0 + \beta_1 + \beta_2 + \beta_3 \end{aligned}$$

- We can now interpret the obtained regression coefficients according to these differentials
- For example, if $d_1 = 1$ for female and $d_2 = 1$ for being married, then the outcome is on average for married women by $\beta_1 + \beta_2 + \beta_3$ higher than for single men

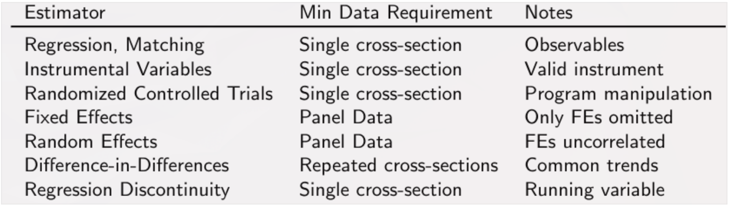
Polynomials

We often use to model non-linear relationships such as the diminishing returns to experience

- Model : $y = \beta_0 + \beta_1 x + \beta_2 x^2$
- Interpretation : $\frac{\partial E[y|x]}{\partial x} = \beta_1 + 2 \times \beta_2 x$
- We can compute the average change in y by a one unit change in x for a specific point in time (say for 0, 1, 2 years of experience)

Econometrics Techniques

- Nonlinear relationships : EG modelling, non-parametric regression
- Standard errors: robust, clustered and bootstrap standard errors
- Addressing homogeneity :



Estimator	Min Data Requirement	Notes
Regression, Matching	Single cross-section	Observables
Instrumental Variables	Single cross-section	Valid instrument
Randomized Controlled Trials	Single cross-section	Program manipulation
Fixed Effects	Panel Data	Only FEs omitted
Random Effects	Panel Data	FEs uncorrelated
Difference-in-Differences	Repeated cross-sections	Common trends
Regression Discontinuity	Single cross-section	Running variable

Figure 3

Lecture 2: Standard Errors

Sun 04 Feb 17:54

[**L2**]

2 Standard Errors

2.1 Introduction

- After a point estimate, we want to know the statistical significance to draw conclusions
- This typically requires the standard error and the distribution
- If the standard errors are wrong we cannot use the usual t / F statistics for drawing inference
- We either have too large SEs,
  - Zero might be included in the CI when it should not be
  - There is a risk of not detecting an effect even there was
- Or, too small SEs
  - Zero might not be in the CI when it should be
  - We may claim the existence of an effect when in reality there is none
  - Of course, this is worse - a **wrong** SE can lead to a **wrong** conclusion!

Robust SE

- Traditionally, inference assumes homoskedacity
- But the variance of error terms might be different for different observations depending on their characteristics
- Heteroskedacity robust SE accounts for this

Standard Errors

- Traditional estimation relied on random sampling
- In the case of data with a group structure, the error terms might be correlated
- To account we use clustered SE

Bootstrap

- Bootstrap is a re sampling method that offers an alternative to inference based on asymptotic formulas convenient in cases where the sampling distribution is unknown

2.2 Heteroskedacity - Robust Standard Errors

Heteroskedacity Problems

- Traditional inference assumes homoskedatic errors $V(u|x) = \sigma^2$
- This implies that the variance of the unobserved error a, is constant for all possible values of all the regressor x's
- Since the proofs for unbiasedness and consistency do not depend on this assumption we still obtain unbiased and consistent OLS estimates
- However, if this is not true (σ_i^2) then the errors are called **heteroskedatic** and traditional variance estimators are biased
- Heteroskedacity robust SE specifically in the CS case
- If the degree of heteroskedacity is low ,the traditional variance estimator might be less biased

> **Example** (Returns to education)**.** Regressing $wage \sim educ$
>
> It is reasonable to believe that the variance is unobserved factors hidden in the error term differers by educational attainment
>
> Individuals with higher education : potentially more diverse interests and more job opportunities affecting their wage
>
> Individuals with very low education : fewer opportunities and often must work at the minimum wage, the error variance is typically lower

Variance Estimation With Heteroskedacity

Simple regression : $y = \beta_0 + \beta_1 x + u$

We know $\hat{b}_1 = \beta_1 + \frac{\sum_{i=1}^{n}(x_i - \overline{x}u_i)}{\sum_{i=1}^{n}(x_i - \overline{x})^2}$

Which is a function of the error terms

Therefore :

$$V(\hat{\beta}_1) = \frac{\sum_{i=1}^{n}(x_i - \overline{x})^2 \sigma_i^2}{SST_x^2}$$

Where $SST_x = \sum_{i=1}^{n}(x_i - \overline{x})^2$

- Where σ_i^2 is conditional variance of error term (depending on each individual)
- If $\sigma_i^2 = \sigma^2$ the formula reduces to the traditional (OLS variance) formula : $V(\hat{\beta}_1) = \frac{\sigma^2}{SST_x}$
- We have to estimate the conditional variance of the error, we do this by tang the residuals of OLS, squaring them and replacing them in the following formula for the error variance
- This leads to the following heteroskedacity robust estimator (simple regression model) :

$$\hat{V}(\hat{\beta}_1) = \frac{\sum_{i=1}^{n}(x_i - \overline{x})^2 \overline{u}_i^2}{SST_x^2}$$

Where u_i^2 are the OLS residuals

Generalisation

The formula generalises to

$$\hat{V}(\hat{\beta}_j) = \frac{\sum_{i=1}^{n} \hat{r}_{ij}^2 \hat{u}_i^2}{SSR_j^2}$$

Where the σ_i^2 are replaced by residuals soured from OG regression and the $\hat{r_{ij}}$ are the residuals from regressing x_j on all other independent variables.

Where $\hat{r_{ij}}$ is the i-th residual from regressing x_j on all other independent variables and SSR_j the sum of squared residuals from this regression

- Robust to heteroskedacity **of any form** (inc homoskedacity)
- Often also called white, huber, eicker SE
- Sometimes degrees of freedom adjustment by multiplying $\frac{n}{n-k-1}$
- But with **drawback** that it only has asymptotic justification (need large sample for it to be valid)

Matrix Representation - Asymptotic Variance

Model : $y = X\beta + U$

We know $\sqrt{n}(\hat{\beta} - \beta)\overrightarrow{d}\mathcal{N}(0, V)$ Where

$$\begin{aligned} V &= E[E[X'X]]^{-1}[E[X'Xu^2]][E[X'X]]^{-1} \\ &\text{with fixed regressors(replace with sample analog)} \\ &= [\frac{1}{n}X'X]^{-1}[\frac{1}{n}X'\psi X][\frac{1}{n}X'X]^{-1} \\ &\text{And the variance-covaraince matrix}\psi \end{aligned}$$

$$\psi = \begin{bmatrix} V(u_1|x) & 0 & \dots & 0 \\ 0 & V[u_2|x] & \dots & 0 \\ \dots & \dots & \ddots & \vdots \\ 0 & 0 & \dots & V[u_n|X] \end{bmatrix}$$

Eventually, $\frac{V}{n} = AV(\hat{\beta})$

Matrix Representation - Estimation

- We can then find an estimate the middle term by : $\frac{1}{n}\sum_{i=1}^{n} u_i^2 \hat{x_i'} x_i = \frac{1}{n}X\hat{\psi}X$
- Where $\hat{\psi} = diag[\hat{u_i^2}, \dots,]$

$$\hat{V} = [\frac{1}{n}X'X]^{-1}\frac{1}{nX'\hat{\psi}}X[\frac{1}{n}X'X]^{-1}$$

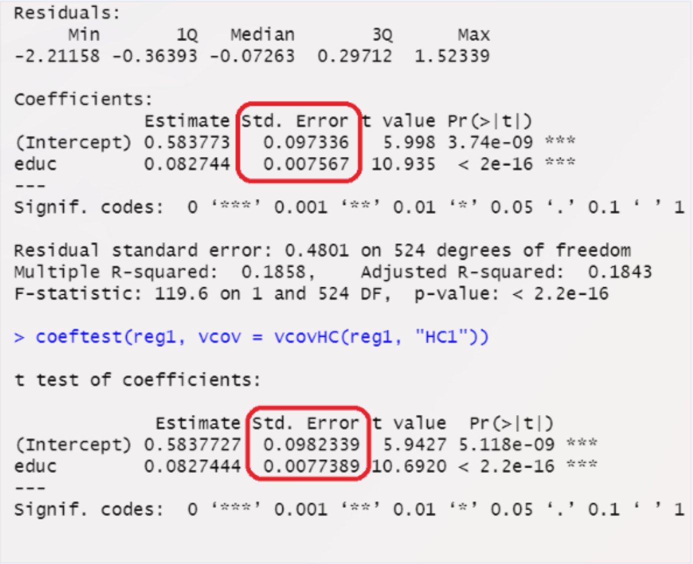
- In order to estimate the Asymptotic Variance (AV) $\hat{\beta}_j$, we need to remove the asymptotic normalisation by dividing by n
- Resulting Estimator :

$$\hat{AV} = n[X'X]^{-1}\frac{\sum_{i=1}^{n} \hat{u}_i^2 x_i' x_i}{n}[X'X]^{-1}$$

- Sometimes corrected by the degrees of freedom $n/n-k-1$ to improve finite sample properties

- SEs : square root of the diagonal elements
- Recall that under homoskedacity, we obtain $\sigma^2(X'X)^{-1}$

Example. Returns to Education Reg1 = lm(wage ∼ educ, data =wage1)



```
Residuals:
     Min       1Q   Median       3Q      Max
-2.21158 -0.36393 -0.07263  0.29712  1.52339

Coefficients:
            Estimate Std. Error t value Pr(>|t|)
(Intercept) 0.583773   0.097336   5.998 3.74e-09 ***
educ        0.082744   0.007567  10.935  < 2e-16 ***
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 0.4801 on 524 degrees of freedom
Multiple R-squared:  0.1858,	Adjusted R-squared:  0.1843
F-statistic: 119.6 on 1 and 524 DF,  p-value: < 2.2e-16

> coeftest(reg1, vcov = vcovHC(reg1, "HC1"))

t test of coefficients:

             Estimate Std. Error t value  Pr(>|t|)
(Intercept) 0.5837727  0.0982339  5.9427 5.118e-09 ***
educ        0.0827444  0.0077389 10.6920 < 2.2e-16 ***
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1
```

Figure 4: R regression output

In which we have used coeftest and vcovHC HC1 variance covariance matrix for one form of the robust one. We have obtained the estimates in both cases.

Comparing, we have the **same estimate**, however the **SE** in the robust case are slightly bigger. This isn't a great example since it doesn't change significance however it shows both estimators can give different SE, but the estimate from OLS remains the same.

Breusch-pagan Test For Heteroskedacity

- Testing hypothesis
$$H_0 : V(u|x_1, \ldots, x_k) = E(u^2|x_1, \ldots, x_k) = \sigma^2$$
Where $V[u|x] = E[u^2|x] - \underbrace{0}\, E[u|x]$
- Assume a linear relationship :
$$U^2 = \delta_0 + \delta_1 x_1 + \ldots + \delta_k x_k + v, E[v|x_1, \ldots, x_k] = 0$$
- Since we cannot observe the errors (u^2), we replace them with the residuals and estimate the regression

1. Estimate
$$\hat{u}^2 = \delta_0 + \delta_1 x_1 + \ldots + error$$
Recover the $R^2_{\hat{u}}$

2. Hypotheses : $\delta_1 = \ldots = \delta_k = 0$
3. Test stat : $F = \frac{R_{\hat{u}}^2/k}{(1-R_{\hat{u}}^2)/(n-k-1)} \sim^{H_0} \mathcal{F}_{k,n-k-1}$ Or $LM = nR^2_{\hat{u}^2} \sim^{H_0} \chi^2_k$ where k dof, F follows fisher dist, LM follows chi squared dist.
4. Decision : if the p-value is small enough (typically < 0.05), we **reject** the null of homoskedacity

Exercise 1 (Heteroskedacity with 2 Categories). Model $y_i = \beta_0 + \beta_1 d_i + u_i$, $i = 1, \ldots, n$ where d_i is a binary variable

Let $n_1 = \sum_i d_i$, $n_0 = \sum_i (1-d_i)$, $n = n_1 + n_0$ and $p = \frac{n_1}{n}$ (probability of being trated, share of treated ind in samp / n)

We have seen that $\hat{\beta}_1 = \overline{y}_1 - \overline{y_0}$ and $\hat{\beta}_0 = \overline{y_0}$ (differences in group mean outcomes) ($\hat{\beta}_0$ is intercept, mean of untreated)

Under homoskedacity in small sample conventional t statistic has a t-distribution

Heteroskedacity here means that the variances in the $d_i = 1$ and $d_i = 0$ population are different: the exact small sample distribution for this problem is unknwon

Differences in the standard error formulae depend on how the variance in d_i is modelled (residual as difference between outcome and group mean outcome)

- Note $\hat{u_i} = y_i = \overline{y_i}$ for $d_i = I$, $I \in \{0, 1\}$
- Define $s_I^2 = \sum_{i:d=I} (y_i - \overline{y_I})^2$ (which is the estimated sum of squared residuals in each group)
- Under conventional SEs: $\hat{\sigma^2}(X'X)^{-1}$ with estimate of $\hat{\sigma^2} = \frac{1}{n-k-1}\sum_{i=1}^n \hat{u_i}^2$
- Where $\sum_{i=1}^n \hat{u_i}^2 = \sum_{i:d=1} \hat{u_i}^2 + \sum_{i:d=0} \hat{u_i}^2 = s_1^2 + s_0^2$ (sum od squared resid = sum of residuals squared for treated and untreated ind)
- Hence $\hat{\sigma}^2 = \frac{s_1^2 + s_0^2}{n-2}$ (is equal to n-2 since have single regressor and intercept)
- Now, $(X'X)^{-1}_{[2,2]} = \frac{n}{nn_1 - n_1^2}$ (if interested in slope, take X and 2,2 element equal to this expression, using this we can take estimator for variance)
- Hence $\hat{V}(\hat{\beta}_1)_c = \frac{n}{n_1 n_0} \frac{s_1^2 + s_0^2}{n-2}$ (conventional variance estimator if replace elements by percentage shares)
- It can be shown that $\hat{V}(\hat{\beta}_1)_c = \frac{1}{np(1-p)} \frac{s_1^2 + s_0^2}{n-2}$
- For robust SEs : $\hat{\sigma^2}(X'X)^{-1}(X\hat{\psi}Z)(X'X)^{-1} \to \hat{V}(\hat{\beta}_1)_r = \frac{s_1^2}{n_1^2} + \frac{s_0^2}{n_0^2}$
- When $\frac{s_1^2}{n_1} = \frac{s_0^2}{n_0}$, both estimates coincide (for large n)
- When $n_1 = n_0 = \frac{n}{2}$ they also coincide, when the data are balanced, the robust SE won't differ much from the traditional one under heteroskedacity
- If both groups variances are the same, then both estimates coincide, because then we have homoskedacity

- Also if we have the same indiviudals for treated and untreated groups, then they also coincide, so if we have very balanced data (2 cat) the robust SE won't differ much from the traditional one

BP test

- Interpretation of the BP test
- Recall the regression $\hat{u_i}^2 = \delta_0 + \delta_1 d_i + v$

$$\hat{\delta}_0 = \frac{\sum_{i:d=0} \hat{u_i}^2}{n_0} = \frac{s_0^2}{n_0}\hat{\delta}_1 = \frac{\sum_{i:d=1} \hat{u_i}^2}{n_1} - \frac{\sum_{i:d=0}}{\hat{u_i}^2} n_0 = \frac{s_1^2}{/} n_1 - \frac{s_0^2}{n_0}$$

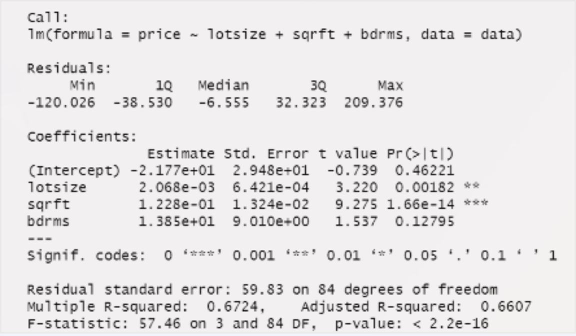
- Testing $H_0 : \delta_1 = 0$ is equivalent to testing $\sigma_1^2 = \sigma_0^2$

Example (Housing Price Equation). Log is sometimes used to get rid of heteroskedacity

Model : price $= \beta_0 + \beta_1$lotsize $+ \beta_2$sqrft $+ \beta_3$bdrms $+ u$

Where price is the housing price, lotsize the size of the lot, size size of house in sq ft

We want to estimate teh above regression and test for heteroskedacity and see whether using logs in the dependent variable changes our conclusion

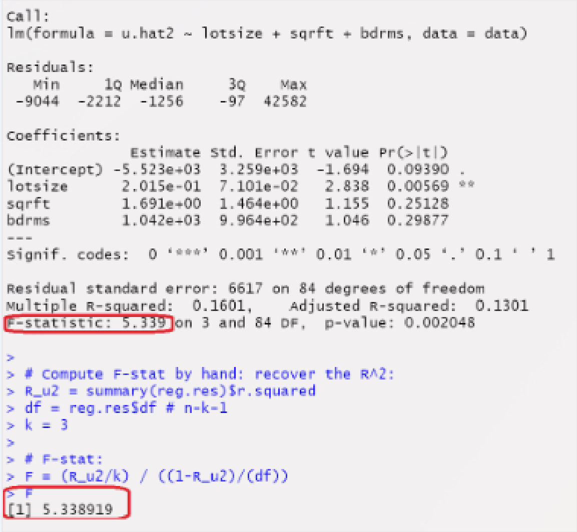


```
Call:
lm(formula = price ~ lotsize + sqrft + bdrms, data = data)

Residuals:
     Min       1Q   Median       3Q      Max 
-120.026  -38.530   -6.555   32.323  209.376 

Coefficients:
              Estimate Std. Error t value Pr(>|t|)    
(Intercept) -2.177e+01  2.948e+01  -0.739  0.46221    
lotsize      2.068e-03  6.421e-04   3.220  0.00182 ** 
sqrft        1.228e-01  1.324e-02   9.275 1.66e-14 ***
bdrms        1.385e+01  9.010e+00   1.537  0.12795    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 59.83 on 84 degrees of freedom
Multiple R-squared:  0.6724,	Adjusted R-squared:  0.6607 
F-statistic: 57.46 on 3 and 84 DF,  p-value: < 2.2e-16
```

Figure 5: Housing Price Equation Output 1

We cannot really learn much about heteroskedacity, although lot and size is statistically significant

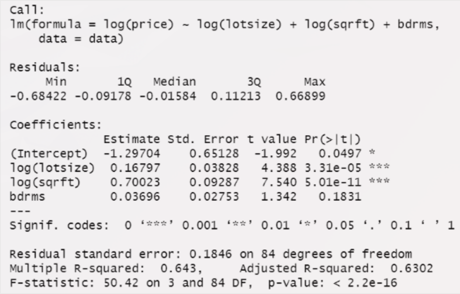
Testing for heteroskedacity using BP test, predicting residuals from previous regression and squared them, then we take the squared residuals and regress on independent variables



```
Call:
lm(formula = u.hat2 ~ lotsize + sqrft + bdrms, data = data)

Residuals:
   Min     1Q Median     3Q    Max 
 -9044  -2212  -1256    -97  42582 

Coefficients:
              Estimate Std. Error t value Pr(>|t|)   
(Intercept) -5.523e+03  3.259e+03  -1.694  0.09390 . 
lotsize      2.015e-01  7.101e-02   2.838  0.00569 **
sqrft        1.691e+00  1.464e+00   1.155  0.25128   
bdrms        1.042e+03  9.964e+02   1.046  0.29877   
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 6617 on 84 degrees of freedom
Multiple R-squared:  0.1601,	Adjusted R-squared:  0.1301 
F-statistic: 5.339 on 3 and 84 DF,  p-value: 0.002048

> 
> # Compute F-stat by hand: recover the R^2:
> R_u2 = summary(reg.res)$r.squared
> df = reg.res$df # n-k-1
> k = 3
> 
> # F-stat:
> F = (R_u2/k) / ((1-R_u2)/(df))
> F
[1] 5.338919
```

Figure 6: Housing Price equation output 2

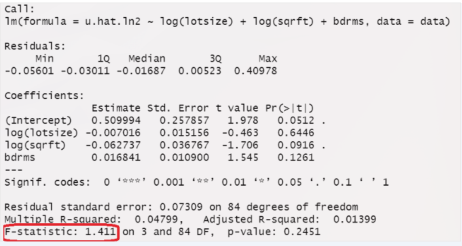
We obtain the f stat, testing for joint normality of parameter estimate, 5.3 with p value <0.05, testing for heteroskedacity using BP test leads us to reject the null of homoskedacity



```
Call:
lm(formula = log(price) ~ log(lotsize) + log(sqrft) + bdrms, 
    data = data)

Residuals:
     Min       1Q   Median       3Q      Max 
-0.68422 -0.09178 -0.01584  0.11213  0.66899 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)  -1.29704    0.65128  -1.992   0.0497 *  
log(lotsize)  0.16797    0.03828   4.388 3.31e-05 ***
log(sqrft)    0.70023    0.09287   7.540 5.01e-11 ***
bdrms         0.03696    0.02753   1.342   0.1831    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 0.1846 on 84 degrees of freedom
Multiple R-squared:  0.643,	Adjusted R-squared:  0.6302 
F-statistic: 50.42 on 3 and 84 DF,  p-value: < 2.2e-16
```

Figure 7

Does our question change if we use logs? Running the regression we obtain the above, not telling us much again, but helps us to predict residuals based on this regression, then we can test for heteroskedacity



```
Call:
lm(formula = u.hat.ln2 ~ log(lotsize) + log(sqrft) + bdrms, data = data)

Residuals:
     Min       1Q   Median       3Q      Max 
-0.05601 -0.03011 -0.01687  0.00523  0.40978 

Coefficients:
              Estimate Std. Error t value Pr(>|t|)  
(Intercept)   0.509994   0.257857   1.978   0.0512 .
log(lotsize) -0.007016   0.015156  -0.463   0.6446  
log(sqrft)   -0.062737   0.036767  -1.706   0.0916 .
bdrms         0.016841   0.010900   1.545   0.1261  
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 0.07309 on 84 degrees of freedom
Multiple R-squared:  0.04799,	Adjusted R-squared:  0.01399 
F-statistic: 1.411 on 3 and 84 DF,  p-value: 0.2451
```

Figure 8

Doing the same as before (without logs) we take our residuals, square the, then regress on individual variables. Giving us f stat of 1.4, which given the p val of 0.2 leads us to *failing to reject the null of homoskedacity* . Thus our initial SE werent very useful, but using the logs we can assume homoskedacity.

Heteroskedacity Conclusion

- Use robust SE when heteroskedatic errors
- But there is a danger of small sample bias from robust SE (arising from asymptotic justification)
- Under homoskedacity or little heteroskedacity, it might be preferable to use the traditional OLS variance estimator
- It is recommended to report both the robust and conventional standard error and suggest to take the maximum of both for inference
- White test for heteroskedacity includes the squares and cross-products of the independent variables
- LPM : built in heteroskedacity → need to compute robust SEs
- Using logs in the dependent variables has been seen to improve in terms of heteroskedacity in many applications

2.3 Clustered Standard Errors

Illustration of Moulton Problem

The Moulton Problem - biased standard errors where observations are not independent within groups, but the regression model incorrectly specifies that they are.

The clustering of data can lead to an underestimation of standard errors → statistical tests likely overly optimistic about the significance.

Closely related to correlation over time in DiD, state average employment rates are correlated over time[**ME-AP**]

- Pillar assumption is random sampling
- There is potential dependence of data within a group structure
  - Exam grades of children from same class or school : grades are correlated because of the same school, teacher and background / class environment

  - Health outcomes in the same village, Errors are correalted because of the same medical and food supply and similar cultural background
  - Earning in the same region might be correlated because of the same industrial structure
  - Analysing workers in firms (earnings, tenure, promotion) will suffer from common firm effects

The Moulton Problem

- Illustration using a simple model with a group structure
- Intuitively, effect of a macro variable on an individual level outcomes
  - Effect of school-type on exam-grades
  - Effect of regional unemployment on individuals' wages
- Model

$$Y_{ig} = \beta_0 + \beta_1 x_g + e_{ig}$$

  - With $g = 1, \dots, G$ and $i = 1, \dots, n$
  - y_{ig} is the outcome for individual I in group g
  - Here x_g varies only at the group level

> **Note.** Lecture : if we estimate a model parameter consistently, why do we care about inference?
>
> - We would like to investigate a problem
> - Policymaker would like to know whether to implement school building program
> - But what is decision rule? Typically, think about Statistical significance and sufficient magnitude then the policymaker wants to adopt the program, if not then not adoptable.
> - We need CI or at least a statistical test. For this we need SE and distribution
> - Need to estimate SE correctly to get correct CI, if we have too large CI (SE wrong), the implication/ error is that we risk not detecting an effect, when there is
> - But the other way around too small SE (forgot to cluster), might think building schools help and invest a lot of money, but the effect is 0
> - This is a danger and the problem is *incorrect standard errors lead to incorrect confidence intervals*

> **Note.** Lecture : Once we have accounted for clustering using the Moulton approach compared to the standard errors, is it more likely that the clustered standard errors are larger or smaller than the OLS
>
> > **Note.** Larger

> **Note.** Lecture : what solutions exist to account for clustering
>
> - Group averages (only valid for regressors that don't vary within each individual within a group)

> - Parametric - estimate the Moulton factor
> - Clustering SE
> - Block bootstrap

Data Structure

- Recall $E[e_{ig}] = 0$ & $v(E_{ig}] = \sigma_e^2$
- Recall correlation: $\rho_e = \frac{Cov(e_{ig},e_{ig})}{sd(e_{ig})sd(e_{ig})}$
- Likely : for individual I and j from the same group g :

$$Cov[e_{ig}, e_{jg}] = \rho_E \sigma_e^2 > 0$$

Additive Random Effects

- Group correlation often modelled using additive random effects, assume $e_{ig} = v$
- v_g : group specific error term which captures all the within-group correlation with $E[v_g] = 0$ & $V(v_g] = \sigma_b^2$
- n_{ig} : individual level specific error term with $E[n_{ig}] = 0$ & $V(n_{ig}) = \sigma_n^2$
- Assuming v_g and n_{ig} are uncorrelated
- We note that n_{ig} and n_{jg} are uncorrelated

Data Structure

- Recall $E[e_{ig}] = 0$ & $v(E_{ig}] = \sigma_e^2$
- Recall correlation: $\rho_e = \frac{Cov(e_{ig},e_{ig})}{sd(e_{ig})sd(e_{ig})}$
- Likely : for individual I and j from the same group g :

$$Cov[e_{ig}, e_{jg}] = \rho_E \sigma_e^2 > 0$$

Additive Random Effects

- Group correlation often modelled using additive random effects, assume $e_{ig} = v$
- v_g : group specific error term which captures all the within-group correlation with $E[v_g] = 0$ & $V(v_g] = \sigma_b^2$
- n_{ig} : individual level specific error term with $E[n_{ig}] = 0$ & $V(n_{ig}) = \sigma_n^2$
- Assuming v_g and n_{ig} are uncorrelated
- We note that n_{ig} and n_{jg} are uncorrelated

$$\begin{aligned} Cov(e_{ig}, e_{jg}) &= E[(v_g, n_{ig})(v_g + n_{jg})] = E[v_g^2] = \sigma_v^2 \\ V[e_{ig}] &= E[(v_g + n_{ig})^2] = E(v_g^2 + n_{ig}^2) = \sigma_v^2 + \sigma_n^2 \end{aligned}$$

Intraclass Correlation Coefficient

- The intraclass correlation coefficient as the proportion of variation in $(v+n)$ due to v :

$$\rho_e = \frac{Cov(e_{ig}, e_{jg})}{sd(e_{ig})sd(e_{jg})} = \frac{\sigma_v^2}{\sigma_v^2 + \sigma_n^2}$$

- When the regressor of interest varies only at group level, then this error structure can increase standard errors sharply
- By how much is the conventional variance of the OLS estimate inflated?
- Let $V_c(\hat{\beta}_1)$ denote the conventional OLS variance expression and $V(\hat{\beta}_1)$ be the correct sampling variance with this error structure
- Depending on the data structure. There are various versions to quantify $\frac{V(\hat{\beta}_1)}{V_c[\hat{\beta}_{s1:1}]}$

For the following data structure :

- Nonstochastic regressors fixed at the group level (that is, all regressors are the same for each individual in a group)
- Equal group sizes $N = n_1 = \ldots = n_G$ with total sample size $n = G * N$

$$\frac{V[\hat{\beta}_{s1:1}]}{V_c[\hat{\beta}_{s1:1}]} = 1 + (N-1)\rho_e$$

$$\text{Moulton Factor} \quad \sqrt{1 + (N-1)\rho_e}$$

Which quantifies how much we over estimate precision by ignoring intraclass correlation

> **Remark.**
>
> - Conventional standard errors become increasingly misleading as group size N and / or ρ_e increase
> - If there is no error correlation $(\rho_e = 0)$, there is no overestimation
> - If $\rho_e = 1$ (or $n_{ig} = 0$), then within a group, all 's are the same : the conventional variance is scaled up by (N-1) since we copy each information N times without generating new information
> - With the total sample size fixed, increasing the group sizes N just decreases the number of clusters which leads to less independent information
> - The Moulton factor can be very big even with a small correlation. Assume 100 observations per group and a $\rho_e = 0.1$ leads to a Moulton factor of 3.3. The conventional standard errors are only roughly $\frac{1}{3}$ of what they should be

Generalisations

The most general form where x varies by g and I with variations in g :

$$\frac{V[\hat{\beta}_{s1:1}]}{V_c[\hat{\beta}_{s1:1}]} = 1 + [\frac{V(N_g)}{\overline{n}_g} + \overline{n}_g - 1]\rho_r\rho_x$$

where $\;rho_x\;$ is the within cluster correlation coefficient for x:

$$\rho_x = \frac{\sum_g \sum_j \sum_{i \neq j}(x_{ig} - \overline{x})(x_{jg} - \overline{x})}{V[x_g]\sum_g n_g(n_g - 1)}$$

- ρ_x is a generic measure of the correlation of the regressors within the group. If this correlation is zero, the Moulton effect disappears
- Clustering has a bigger impact on standard errors with variable group sizes and when ρ_x is large
- If the group size is fixed but x varies by g and I, the Moulton factor becomes the square root of $1 + (N-1)\rho_E \rho_x$

Moulton Problem - Solutions

Model $y = \beta_0 + \beta_1 x_{ig} + e_{ig}$ with $g = 1, \ldots, G$

1. Parametric approach
   - Fix the conventional standard errors using the general formula for the Moulton factor by estimating the intraclass correlations ρ_e and ρ_x
2. Cluster standard errors
   (a) Generalisation of white's robust covariance matrix

   $$\hat{AV}(\hat{\beta}_{s1:1}) = (X'X)^{-1}(a\sum_{g=1}^{G} X'_g \hat{e}_g \hat{e}'_g X_g)(X'X)^{-1}$$

   (b) Where $\hat{e}_g$ is a n_g x 1 vector of redials for observations in the g-the cluster and X_g is a n_g x k matrix of regressors for observations in the g-the cluster

   (c) Typically, there is a degrees of freedom adjustment $a = \frac{G(n-1)}{(G-1)(n-k)}$

   (d) Consistent if number of cluster is large but not consistent with fixed number of groups (even when group sizes tend to ∞)

   (e) No assumptions about within-group correlation structure (not just parametric such as in the additive error structure)

   (f) If each individual is his own group (I = g and G = n) then the formula collapses back the robust estimator
3. Use group averages instead of microdata
   (a) Model : $y_{ig} = \beta_0 + \beta_1 x_g + e_{ig}$, $g = 1, \ldots, G$

   (b) We estimate $\overline{y}_g = \beta_0 + \beta_1 x_g + \overline{e}_g$ by weighted least squares using n_g as weights

   (c) However, neglecting heteroskedacity unless the group sizes are equal

   (d) Relying on asymptotics for group number, not group sizes

   (e) With modest group sizes, it is expected to have good finite sample properties of regressions with normal errors

   (f) And is likely to be more reliable than clustered standard errors with few clusters

   (g) But does not work if x varies within groups and ignores any other micro-level covariates

   (h) But there exists a 2 step approach to include micro level covariates (A & P)
4. Block bootstrap

(a) To be discussed

5. GLS or Max Likelihood approaches

GLS :

(a) In some cases is possible to estimate GLS or maximum likelihood model

(b) Requires a model for error structure

Example (Star Experiment). Krueger (1999) uses IV to estimate the effect of class size on students' achievements y_{ig} is the test score of student I in class g and class size x_g

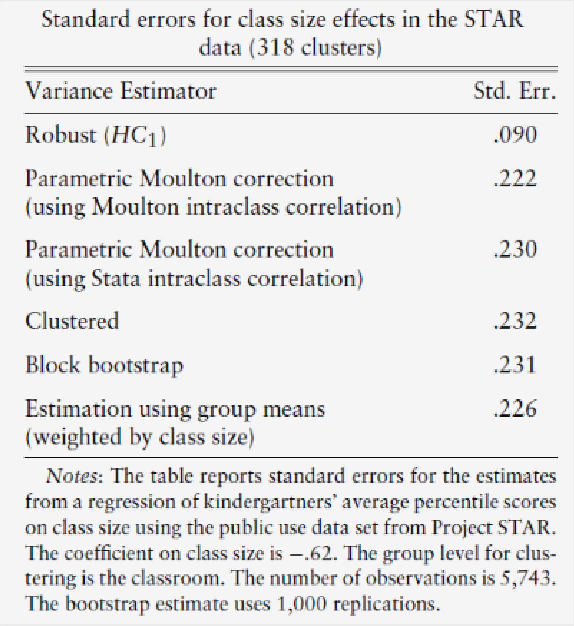
Students were randomly assigned to each class but data are unlikely to be independent across observations.

Test scores in the same classes are correlated because students in the same class share background characteristics and are exposed to the same teacher and classroom environment

It is likely for students I and j from the same class g :

$$E[e_{ig}, e_{jg}] = \rho_r \sigma_e^2 > 0$$

The estimation strategy is for now not in our focus, thought we can compare the different standard error estimates



Standard errors for class size effects in the STAR data (318 clusters)

Variance Estimator	Std. Err.
Robust (HC_1)	.090
Parametric Moulton correction (using Moulton intraclass correlation)	.222
Parametric Moulton correction (using Stata intraclass correlation)	.230
Clustered	.232
Block bootstrap	.231
Estimation using group means (weighted by class size)	.226

Notes: The table reports standard errors for the estimates from a regression of kindergartners' average percentile scores on class size using the public use data set from Project STAR. The coefficient on class size is −.62. The group level for clustering is the classroom. The number of observations is 5,743. The bootstrap estimate uses 1,000 replications.

Figure 9: Robust standard errors after correcting for clustering

[Moulton Derivation]

Lecture 3: Bootstrap and IV

Tue 13 Feb 15:20

[**L3**]

2.4 Basic Intro to Bootstrap

Based on the data we have we simulate and pretend we many more 'made-up' datasets we didn't have previously. Runs into issues when estimating min or max, rather than mean and under non-Gaussian distribution. When asymptotically normal or ..., bootstrap good choice.

OvB only creates bias if correlation with regressors, if going to argue variable is non-correlated, it is fine. But including too many regressors may be problematic too, end up including too many variables correlated with regressor, on top of fact it creates noise.

Attenuation Bias - if measurement error, nothing can do about it. As long as variance and this measurement error, it exists. But if less variance in measurement error, then it disappears. Of course, provided error isn't systematic.

- Another method for estimating variance, CI and dist on statistic
- Often used when exact distribution is unknown
- Different versions but non parametric most common

Non-parametric Bootstrap

- X is distributed according to some distribution F: $X \sim F$
- $x = (x_1, \ldots, x_n)$ represents an iid sample from this variable
- Suppose we want to estimate the variance and the distribution of a statistic $T_n = g(x_1, \ldots, x_n)$
- Ultimately interested in variance of distribution of this statistic $T_n = g(x_1, \ldots, x_n)$

NP Bootstrap - Variance

- Let V_F denote the variance of T_n where the subscript F indicates that the variance is a function of F
- If we knew F, we could compute the variance
- For example for $T_n = \frac{1}{n}\sum_{i=1}^{n} x_i$,

  $$V_F(T_n) = \frac{V(x)}{n} = \frac{\int x^2 dF(x) - (\int x dF(x))^2}{n}$$

  Where $dF(x)$ is the pdf in integral form (2nd term)
- Which is a function of F
- Idea is to estimate $V_F(T_n)$ with $V_{\hat{F}}(T_n)$
- Or, *use a plug in estimator* of the variance
- Since $V_{\hat{F}}(T_n)$ may be difficult to compute, we approximate it with a simulation estimate denoted by v_{boot}

Key Idea

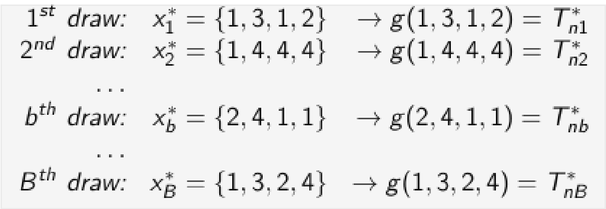
Put the initial sample $x = (x_1, \dots, x_n)$ into an urn

1. Draw n observations from x with replacement
   - Each observation has the probability of $\frac{1}{n}$ of being drawn
   - Gives each bootstrap sample $x^*_1 = (x^*_{11}, \dots, x^*_{n_1})$
2. Based on the single bootstrap sample, we estimate (compute bootstrap statistic)
$$T^*_{n_1} = g(x^*_{11}, \dots, x^*_{n_1})$$
3. Repeat steps 1 and 2 B times to get $T^*_{n_1}, \dots, T^*_{nB}$ where :
$$T^*_{nb} = g(x*_{1b}, \dots, x^*_{nb})\ for\ b = 1, \dots, B$$
   Where B is the number of bootstrap replications
$$V_{boot} = \frac{1}{B}\sum_{b=1}^{B}(T^*_{nb} - \frac{1}{B}\sum_{r=1}^{B} T^*_{nr})$$
   (then take sample analog of variance)

Then by the law of large numbers $v_{boot} \xrightarrow{a} V_{\hat{F}(T_n)}$ as $B \to \infty$ (bootstrap variance tends to variance of stat we were after)

Need to reiterate quite often, in real world we have initial sample from true distribution F which gies us stat T_n in bootstrap world we have bootstrap sample which comes from resampling our initial sample which gives us our bootstrap stat : T^*_n

Imagine in real world, initial sample with 4 obs, giving us stat which is a function of these 4 obs (say the average over these 4 obs). In order to get into boostrap world, we place sample in urn, we draw b times 4 observations each time with replacement



$$\begin{array}{rll}
1^{st}\ draw: & x^*_1 = \{1,3,1,2\} & \to g(1,3,1,2) = T^*_{n1} \\
2^{nd}\ draw: & x^*_2 = \{1,4,4,4\} & \to g(1,4,4,4) = T^*_{n2} \\
\dots & & \\
b^{th}\ draw: & x^*_b = \{2,4,1,1\} & \to g(2,4,1,1) = T^*_{nb} \\
\dots & & \\
B^{th}\ draw: & x^*_B = \{1,3,2,4\} & \to g(1,3,2,4) = T^*_{nB}
\end{array}$$

Figure 10: Boostrap World

When we do the 1-st draw we get 1, then since draw with replacement, it could happen we draw this again, second is 3, we also put this back, we do this even further then we got again observation with 1.

Then eventually we get the observation with 2 then we can compute the bootstrap statistic b times to obtain bootstrap samples

Use of the Bootstrap

- The empirical distribution of the B bootstrap samples gives us the approximated distribution / moments of T_n

- EEG standard Errors : $\hat{se} = \sqrt{v_{boot}}$
- Approximate the CDF of T_n. Let $G_n(t) = (T_n < t)$ be the CDF of T_n
- The bootstrap appropriate to G_n is

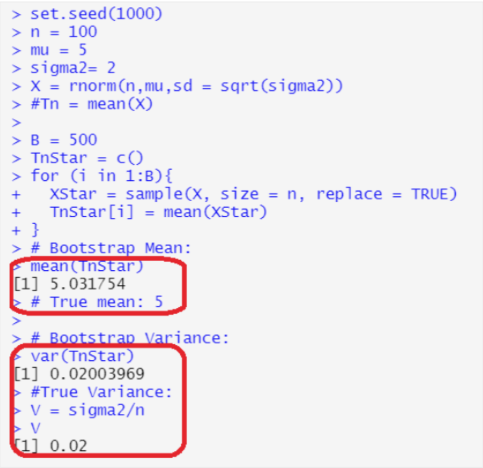
$$\hat{G}_n^*(t) = \frac{1}{B}\sum_{b=1}^{B} 1_{\{T_{nb}^* \leqslant t\}}$$

Where the binary variable obtains probability

- Confidence intervals based on SE or quantiles
- Normal interval :

$$T_n \pm z_{\frac{a}{2}} \hat{se}_{boot}$$

- Where $\hat{se}_{boot}$ is the bootstrap estimate for the SE
- Where $z_{\frac{a}{2}}$ is the $\frac{a}{2}$ quantile of the standard normal distribution
- The interval is not accurate unless the distribution of T_n is close to normal



```
> set.seed(1000)
> n = 100
> mu = 5
> sigma2= 2
> X = rnorm(n,mu,sd = sqrt(sigma2))
> #Tn = mean(X)
>
> B = 500
> TnStar = c()
> for (i in 1:B){
+   XStar = sample(X, size = n, replace = TRUE)
+   TnStar[i] = mean(XStar)
+ }
> # Bootstrap Mean:
> mean(TnStar)
[1] 5.031754
> # True mean: 5
>
> # Bootstrap Variance:
> var(TnStar)
[1] 0.02003969
> #True Variance:
> V = sigma2/n
> V
[1] 0.02
```

Figure 11: Boostrap Code

Set seed to ensure RV is same on diff computers, then 100 obs, mean = 5, variance = 2

We want 500 bootstrap replications, then we initiate an empty vector t_n^* to collect bootstrap replications, then iterate over 500 i's. For each I in 1:500 we sample from our initial vector, with replacement 100 observations, giving us bootstrap sample, then we take mean to obtain bootstrap mean

T_n^* has 500 bootstrap means, then we take mean over these 500 and compare to true expectation. 5.03 is very close to the true mean,

We proceed the same to estimate variance based on bootstrap replications, we are also close to variance also.

Practically, it depends on the situation to normalise test stat (demean or standardise in order to ensure normal distribution)

Regression Estimates

Procedure quite similar, but with at least 2 characteristics for each individual parameters

Instead of drawing directly from RV, we Draw pairs of $\{y_i, x_i\}$ to

- Sometimes called the *pairs bootstrap*
- Instead of drawing directly from the random variable, you would sample the indices of the observations

Empirical, non parametric, standard, pairs.

Wild Bootstrap

relies on assumption that error term at disposal

- Model $y_i = \beta_0 + \beta_1 x_i + u_i$ (one regressor)
- Preserves heteroskedatic behaviour since don't destroy link between x's and error terms
- Initial sample $z = [(y_1, x_1) \dots (y_{n,x_n})]$ with outcome and regressors for each individual

Methodology

Quite similar but main difference that it is residual bootstrap but keep regressors fixed

1. Estimate $\hat{u}_i = y_i - \hat{\beta}_0 - \hat{\beta}_1 x_i$ for all $i = 1, \dots, n$
2. Randomly create a bootstrap residual (weights)

$$\text{weights:} \quad w_i = \begin{cases} 1\ , & \text{with probability } \frac{1}{2} \\ -1\ , & \text{with probability } \frac{1}{2} \end{cases}$$

   Bootstrap residuals $\hat{u}_i^* - w_i \hat{u}_i$, for all $i = 1, \dots, n$

3. Compute the bootstrap dependent variables (essentially changed sign of original residual)

$$Y_i^* = \hat{\beta}_0 + \hat{\beta}_1 x_i + \hat{u}_i$$

   For all $i = 1, \dots, n$ Gives : a single bootstrap sample : $z_1^* = [(y_{11}^*, x_1), (y_{n1}^*, x_n)]$

Repeat 1-3 B times to obtain B wild bootstrap samples : $z_b^*[(y_{1b}^*, x_1), \dots, (y_{nb}^*, x_n)]$ for $b = 1, \dots, B$

Clustered Bootstrap

Under similar randomisation, you do not preserve the dependence (unobserved factors relating to microdata) structure in the data

To fix this, we draw blocks of data defined by the groups g. Say block bootstrap by re sampling entire classes instead of individual students, to keep structure of correlation intact.

Can also have cluster 1 bootstrap, maybe you have stratified sampling such that while you sampled you made sure have say gender quota or certain subset, we would need to do bootstrap for this.

The way you sample data structure, try to mimic through the bootstrap exactly this structure. That is, replicate DGP as close as you can (provided we know about it)

Exercise 2 (Algorithm to obtain SE using clustered bootstrap)**.**

Set the seed

If we have 1 village, should we have randomly different households in village 1? No

Village 1, 2, 3 and households a, b, c (1), def, (2), ghi (3). Everything within the villages is kept fixed

We then draw 1 (abc) , 3(ghi) , 3 (ghi) for 1-st bootstrap

Then 2, (ghi) , 1 (abc) , 1 (abc) for 2-nd bootstrap

Need large sample.

Based on practice not theory

Advantages - as opposed to random draw, forget about cluster, end up doing randomly a,d,f,c and estimate OLS $\hat{\beta}_1$ and g,h,c,f and estimate $\hat{\beta}_2$, you have no attachment to group and lose correlation within each group

Need to re merge together 'blocks' into single data set since we have individually sampled blocks

Have to store estimator so on

Exercise 3 (Effect of schooling on wages, use father educ as instrument for years of educ)**.**

Conditions of good instrument?

Exclusion restriction (orthogonality - instrument cannot be correlated with error term), Relevance restriction ($Cov(x_1, z) = 0$)

Maybe since there is push to education, maybe with time this effect is fading, but likely still relevant, but maybe in other countries this is deterministic and is something we can test

Exclusion restriction - 1. We can control for this, if this is not part of the model. 2. Might be violated if we can argue ability for singers, parents can sing, inherit singing talent so opera hires, this might be correlated with number of years taking singing lessons but choosing to take singing lessons due to natural talent suggests violation of exclusion criteria

Exercise 4 (Is month of birth good instrument for years of education)**.**

Exclusion Restriction - it is pretty random when you are born, there is no reason to believe the error term left over when explaining wages is correlated with when you are born

Some spikes of the births (seasonality etc) though, could this be an issue?

Relevance condition - Structure of Education System : cutoff for year of schooling in the year, therefore matters for when say leave school at 16

Usa : start in September, able to leave when 16, people born earlier get extra months of schooling

Does extra month of education have strong effect? No

F stat - very low in this case, but we wont a large F-stat

> If we have a small f stat we have very low relevance, but also $\beta = cov(y, x)/cov(x, z)$
>
> Bias end up having depends on the covariance between y and u and x and z
>
> If low relevance, then $\frac{cov(z,u)}{cov(x,z)}$ 'explodes'
>
> To test, there is no proper overall applicable test for exclusion restriction, it is something you have to argue for.

3 Instrumental Variables

[L3-IV]

Motivation

Let's say we are interested in identifying the causal effect of years of schooling on wage, we estimate the model $y = \beta_0 + \beta_1 x_1 + \ldots + \beta_k x_k + u$

- One of the key assumptions for unbiasedness is the *homogeneity of regressors* : $E[u|x_1, \ldots, x_k] = 0$
- Indeed, the problem arises when the regression error is correlated with a regressor : ie $E[u|x_k] \neq 0$
- There are three broad reasons for *Endogeneity* :
    1. Omitted variable bias
    2. Measurement error
    3. Simultaneous equations
- In our example, x_k is said to be endogenous, meaning the years of schooling might be correlated with innate and unobserved ability
- The OLS estimator β_k is *biased* and *inconsistent*
- One approach to deal with this issue is to use instrumental variables

3.1 Forms of Endogeneity

- Omitted variables
    1. Arises in cases when one fails to control for a regressor that is correlated with other regressors
    2. Often due to self selection : if an agent chooses the value of the regressor, this might depend on factors that we cannot observe
    3. That is, *unobserved heterogeneity*
- Measurement Error
    1. Occurs when we can only observe an imperfect measure of a variable
    2. Depending on how the observed and true variable are related, we might have endogeneity
- Simultaneity
    1. Occurs when dependent and independent variables are simultaneously determined
    2. If x is partially determined by y, then the error might be correlated with x

Though this is not to say there exist sharp distinctions

> **Example.** Effect of alcohol consumption on worker productivity (measured by wages)
>
> Alcohol usage correlated wuth unobserved factors such as family background, which may also have an effect on wage. Leading to an Omitted variable problem
>
> Alcohol demand can depend on income, leading to the simultaneity problem
>
> There also exists possibility of mismeasurement of alcohol consumption

Omitted Variable Bias (ovb)

- Long regression - true model is : $y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + e$
- Then, assuming we cannot observe x_2 but only x_1
- Or, in short : $y = \delta_0 + \delta_1 x_1 + u$ with $u = \beta_2 x_2 + e$
- We know the population parameter can be expressed as :

$$\delta_1 = \frac{cov(y, x_1)}{v[x_1]}$$

$$\begin{aligned}&\text{replacing y from the true model:}\\ &= \frac{Cov(\beta_0 + \beta_1 x_1 + \beta_2 x_2 + e, x_1)}{V[x_1]}\\ &= \beta_1 v[x_1] + \beta_2 Cov(x_2, x_1) + cov(e, x_1)/V[x_1]\\ &= \beta_1 + \beta_2 \frac{cov(x_2, x_1)}{v(x_1)}\end{aligned}$$

Defining τ_1 as the parameter in the population model that relates x_1 to x_2 :

$$X_2 = \tau_0 + \tau_1 x_1 +' error'$$

We therefore estimate $\delta_1 = \beta_1 + \beta_2 \tau_1$

However, our OLS estimate is *inconsistent* (asymptotically biased)

$$\underset{n\to\infty}{plim}\hat{\delta_1} - \beta_1 = \beta_2 \frac{Cov(x_1, x_2)}{v(x_1)} = \beta\tau_1$$

With

$$Bias(\hat{\delta_1}) = E[\hat{\delta_1}] - \beta_1 = \beta_2 \hat{\tau_1}$$

Where thinking about the direction of the correlation helps us think about the direction of the bias

Essentially, if the omitted variable is related to the included regressor, then the parameter in the short regression will not identify the parameter in the long regression.

With more regressors, the formula changes but the principle remains the same

Example. omitted variable bias

Let y be the wages, x_1 years of education and x_2 ability

Regressing wages on years of education alone delivers a biased estimate $\hat{\delta_1}$

We would expect both years of education and ability to have a positive impact on average earnings (that is $\{\beta_{1/2} > 0, \}$)

But we also expect both regressors to be *positively correlated* as individuals with more *innate ability* tend to choose / acquire more education ($\tau_1 > 0$)

Therefore, $\hat{\tau_1}$ likely overestimates the value of education, since in our education regressor we have not controlled for the correlation with ability and thus include more than the effect of education in this estimate, here thinking about the direction of the correlation has helped us to identify the sign of the bias

Measurement Error in y

Situation 1 : Measurement error in the dependent variable (y)

True model $y = \beta_0 + \beta_1 x_1 + \varepsilon$, $E[\varepsilon|x_1] = 0$

We can only observe $\tilde{y}$ which measures the unobserved y with an error $\tilde{y} = y + e$

We regress $\tilde{y} = \tilde{\beta_0} + \tilde{\beta_1} x_1 + \tilde{\varepsilon}$

$$\tilde{\beta_1} = \frac{Cov(\tilde{y}, x_1)}{V[x_1]} = \frac{Cov(y + e, x_1)}{V[x_1]} = \beta_1 + \frac{Cov(e, x_1)}{V(x_1)}$$
$$\tilde{\beta_0} = E(\tilde{y}) - \beta_1 E(x_1) = E[y] + E[e] - \beta_1 E[x_1] = \beta_0 + E(e)$$

However, this can cause bias and inconsistency. Although it vanishes if the measurement error is statistically independent of each explanatory variable. We note the usual OLS inference procedures are asymptotically valid.

Situation 2 : Measurement error in the regressor (x)

True model $y = \beta_0 + \beta_1 x_1 + \varepsilon$, $E(\varepsilon|x_1) = 0$

Where we can only observe $\tilde{x_1}$, a measure of the unobserved x_1 with an error $\tilde{x_1} = x_1 + \varepsilon$

We then regress $y = \tilde{\beta_0} + \tilde{\beta_1}\tilde{x_1} + \tilde{\varepsilon}$

$$\tilde{\beta_1} = \frac{Cov(y, \tilde{x_1})}{V[x_1]} = \frac{Cov(\beta_0 + \beta_1 x_1 + e, x_1 + e)}{V[\tilde{x_1}]} = \beta_1 \frac{V(x_1)}{V(\tilde{x_1})}$$
$$\tilde{\beta_0} = E(\tilde{y}) - \beta_1 E(x_1) = E[y] + E[e] - \beta_1 E[x_1] = \beta_1 \frac{V(x_1)}{V(x_1) + V(e)} = \beta_1 \lambda$$

With the key assumptions that $Cov(e, x_1) = 0$, $cov(e, \varepsilon) = 0$, and $E[e] = 0$

In which we can show $plim_{n \to \infty} \hat{\tilde{\beta}}_1 = \beta_1 \lambda$, where $\lambda \in \{0, 1\}$: $\hat{\tilde{\beta}}_1$ underestimates β_1, this is attenuation bias. Though as $V(e)$ shrinks relative to $V(x_1)$, the attenuation bias disappears.

In the general model, it is not the variance of the true regressor that affects the consistency but the variance in the true regressor after netting out the other explanatory variables

Simultaneity / Reverse Causality

Problem : y_1 and y_2 are simultaneously determined.

$$Y_1 = \alpha_1 y_2 + \beta_1 z_1 + u_1, E[z_1|u_1] = 0$$
$$Y_2 = \alpha_2 y_1 + \beta_2 z_2 + u_2, E[z_2|u_2] = 0$$

A classic example is when y_1 is price and y_2 is the quantity and both equations are demand and supply. But note the intercept is suppressed for simplicity.

Focusing on the 1-st equation, to show that $Cov(y_2, u_1) \neq 0$:

$$\begin{aligned} Y_2 &= \alpha_2[\alpha_1 y_2 + \beta_1 z_1 + u_1] + \beta_2 z_2 + u_2 \\ &= \frac{\beta_1\alpha_2}{1-\alpha_1\alpha_2} z_2 + \frac{\alpha_2}{1-\alpha_1\alpha_2} u_1 + \frac{1}{1-\alpha_1\alpha_2} u_2 \end{aligned}$$

Assuming that $\alpha_1\alpha_2 \neq 0$, $Cov(u_1, u_2) = 0$ and $cov(z_2, u_1) = cov(z_2, u_2) = 0$, thus violating exogeneity

$$Cov(y_2, u_1) = \frac{\alpha_2}{1-\alpha_1\alpha_2} V(u_1 \neq 0)$$

Although, without additional controls (z_1), it can be shown that the consistency has the same sign as $\frac{\alpha_2}{1-\alpha_1\alpha_2}$

3.2 IV Estimator

Single Regressor Model

Properties of an instrument to overcome Endogeneity Model : $y = \beta_0 + \beta_1 x_1 + u$ with $E[u|x_1] \neq 0$

We need an instrumental variable (IV) z with the following properties

- Exclusion Restriction : $Cov(u, z) = 0$
- Relevance : $Cov(x_1, z) \neq 0$

The instrument cannot be correlated with any of the omitted variables for egg, but it does need to be correlated with the endogenous regressor

1. We can test the relevance assumption
2. However we cannot generally test the exclusion restriction. One needs to carefully apply common sense and economic theory to convince the audience about the validity of the instrument

Testing for Relevance First stage regression

$$X_1 = \pi_0 + \pi_1 z + v$$

Where we regress endogenous regressor on instrument, π_1 is equal to covariance of instrument and endow regressor / variance of endow regressor

Recall, $\pi_1 = \frac{Cov(z,x_1)}{V(x_1)}$

Test

$$H_0 = \pi_1 = 0 \ \ vs \ \ H_0 :\neq 0$$

To understand whether covariance is zero or not

Where we should be able to reject at a small significance level. In this case we can be confident that the relevance condition holds

How to find IVs?

- It might be hard to think of a valid instrument and or to have data on them
- Instruments come from
  - Economic theory
  - Exogenous sources of variation in the endogenous regressor arising from a random phenomenon such as whether events, or exogenous policies (cutting class sizes to increase grades)

> **Example.** Wages Where ability causes the regressor of years of educating to be endogenous One could think of
>
> - Family background variables
> - Proximity to school / college
> - Month of birth
>
> As potential Ifs. Whether these would work requires scrutiny

Identification

Model : $y = \beta_0 + \beta_1 x_1 + u$, with $E(u|x_1) \neq 0$ Instrument : $Cov(x_1, z) \neq 0$ and $Cov(z, u) = 0$ (satisfying exclusion and relevance)

Identification in this context : we can write β_1 (parameter of interest) in terms of population moments that can be estimated

We write β_1 in terms of population covariances

$$\begin{aligned} Cov(z,y) &= \beta_1 Cov(z,x_1) + Civ(z,u) \\ Since\ cov(z,u) = 0 \ \ and \ \ cov(z,x_1) \neq 0\ (assumptions) \\ \beta_1 &= \frac{Cov(z,y)}{cov(z,x_1)} \end{aligned}$$

However, this fails if $Cov(z, x_1) = 0$, that is the relevance condition doesn't hold. This is an expression we can estimate using a random sample.

IV Estimator

- Given random sampling, we estimate the moments by the sample analogs :

$$\widehat{\beta}_1^{IV} = \frac{\sum_{i=1}^n (z_i - \overline{z})(y_i - \overline{y})}{\sum_{i=1}^n (z_i - \overline{z})(x_{i,1} - \overline{x}_1)}, \text{ and } \widehat{\beta}_0^{IV} = \overline{y} - \widehat{\beta}_1^{IV} \overline{x}_1$$

- When $z = x_1$ then the IV estimator reduces to the OLS estimator
- Using the Law of Large Numbers, we can show that $\widehat{\beta}_1^{IV}$ is consistent under the assumptions :

$$\text{plim}_{n\to\infty}\widehat{\beta}_1^{IV} = \beta_1$$

- However, the IV estimator is biased
- Requiring large samples

We note that if we divide the denominator and numerator by V(z) :

$$\beta_1 = \frac{\text{Cov}(z,y)/V(z)}{\text{Cov}(z,x_1)/V(z)}$$

Where β_1 is the ratio of the population regression of the reduced form over the first stage.

Wald Estimator - Binary Instrument

Recalling that for a regression on a binary variable, the resulting slope estimate is the difference between both groups averages. Then, under the IV assumptions, the β_1 can be represented as the ratio of the two OLS estimands, in case of a binary IV:

$$\beta_1 = \frac{\text{Cov}(y,z)V(z)}{\text{Cov}(x_1,z)/V(z)} = \frac{E\left[y|z=1]\right] - E\left[y|z=0]\right]}{E\left[x_1|z=1]\right] - E\left[x_1|z=0]\right]}$$

Then, taking the sample analog : $\widehat{\beta}_1^{IV} = \frac{\overline{y_1}-\overline{y_0}}{\overline{x}_{1,1}-\overline{x}_{1,0}}$ Where $\overline{y_1}$ and $\overline{x}_{1,I}$

Lecture 4: IV continued

Tue 20 Feb 16:07

[**L4-IV**] Model $y = x\beta + u$ with x a vector of k exogenous and endogenous regressors and z a vector of m IV's (including the exogenous variable)

1. M = k : the model is just identiified ,we have an instrument for each endogenous variable ⇒ use IV
2. M < k : the model is not identified, we do not have enough IVs
3. M > k : the model is over-identified → we have too many IVs. Use GIVE / 2SLS

Case : Length(z) = Length(x)

Model : $y = x\beta + u$, $x = (q, x_2, \ldots, x_k)$ and $z = (1, x_2, \ldots, x_{k-1}, z_1)$. We know $Cov(x_j, u) = 0$ for $j = 2, \ldots, k-1$ and $Cov(x_k, u \neq 0)$

We have an instrument for x_k :

- Exogenous $Cov(z_1, u) = 0$
- Partial Correlation : $\theta_1 \neq 0$ in $x_k = \delta_1 + \delta_2 x_2 + \ldots + \delta_{k-1}x_{k-1} + \theta_1 z_1 + r_k$

Where the moment conditions imply :

$$E[z'u] = E[z'(y - x\beta)] = 0$$

We have one instrument at our disposal for this endogenous regressor, we include the constant and all the exogenous regressors because they can be used for instruments for themselves.

Partial correlation best seen by regressing endogenous regressor x_k on all exogenous variables plus the instrument for x_k and we need the parameter on the instrument θ_1 not to be 0, thus partial correlation, the correlation cannot be 0 after the other effects have been 'netted' out. Different to simple case where sufficient to have covariance between endogenous regressor and instrument $\neq 0$

Exogeneity leads to above expression, plugging in expression for u.

Multiplying the model through with z', taking expectation and using the moment condition :

$$\begin{aligned} E[z'y] &= E[z'x]\beta \\ \text{if rank } \ E[z'x] &= k \\ \beta &= [E[z'x]]^{-1}E[z'y] \end{aligned}$$

There is a *unique solution only under full rank* and it can be shown that if we rule out perfect collinearity in z, full rank holds iff $\theta_1 \neq 0$

Given a random sample, we can estimate consistently :

$$\hat{\beta}^{IV} = \left(\frac{1}{n}\sum_{i=1}^{n} z_i' x_i\right))^{-1}\left(\frac{1}{n}\sum_{i=1}^{n} z_i' y_i\right)) = (Z'X)^{-1}Z'Y$$

Where Z and X are $n \times k$ data matrices and y is $N \times 1$

Given the assumptions, this estimator is consistent

3.3 2sls

Case: Length(z) $>$ Length(x):

The idea is to used the fitted values from the first stage regression of the endogenous regressor on all the exogenous variables (including the instruments) and use them as "instruments" in the IV estimator

$$Z = (1, x_1, \ldots, x_{k-1}, z_1, \ldots, z_l) - m = k + I \ \text{ vector for } \ x_k$$

1. Fitted values from the first stage $\hat{x_i} = (1, x_1, \ldots, x_{k-1}, \hat{x}_k)$

$$\begin{aligned} \hat{x}_{ik} = \hat{\delta}_0 + \hat{\delta}_1 x_{i1} + \ldots + \hat{\delta}_{k-1} x_{i,k-1} + \hat{\theta}_1 z_{i1} + \ldots + \hat{\theta}_I z_{il} \\ \hat{x_i} = z_i \left(\sum z_i' z_i\right)^{-1} z_i' x_i \\ \hat{X} = Z(Z'Z)^{-1}Z'X \end{aligned}$$

For this endogenous regressor you have several potential instruments at your disposal, you would then regress on exogenous variables from initial model and instruments. That gives you a vector of instruments that is equal to (including all potential instruments).

Then you start with obtaining fitted values from First Stage (FS) regressing x_k on exogenous regressors δ and instruments z

Using the fitted values as instruments :

$$\hat{\beta}^{IV} = \left(\frac{1}{n}\sum_{i=1}^{n}\hat{x_i}'x_i\right)^{-1}\left(\frac{1}{n}\sum_{i=1}^{n}\hat{x}_i y_i\right) = (\hat{X}'X)^{-1}\hat{X}'Y$$

Then, using calculus, we can show that $\hat{X}'X = \hat{X}'\hat{X}$ and hence

$$\hat{\beta}^{IV} = (\hat{X}'\hat{X})^{-1}\hat{X}'Y$$

Which is the GIVE / 2SLS estimator since it equals the OLS estimator on the fitted values from the first stage

Where we have simply replaced with fitted values, and then replaced in matrix form. We can essentially show this is the OLS estimator on the FS using the fitted values. Then we use this to plug in the IV estimator to obtain the OLS estimator on the fitted values

To Obtain The β

1. First Stage: Obtain the fitted values $\hat{x}_k$ from the regression x_k on $1, x_1, \dots, x_{k-1}, z_1, \dots, z_l$
2. Second Stage: Run the OLS regression: y on $1 + x_1, \dots, x_{k-1} + \hat{x_k}$

- However, omitting the exogenous regressors in the first stage is easily done and will lead to inconsistency
- And, SE obtained from the second step are incorrect

Testing for rank condition : $H_0 : \theta_1 = \ldots = \theta_l = 0$ vs at least one θ_s for $s - 1, \dots, l$ is non zero.

3.4 Properties

Here x is 1 x k and generally includes unity, several elements of x may be endogenous, while z includes any exogenous variable

Assumption 8 2SLS.1
For some 1 x m-vector z, $E\left[z'u\right] = 0$

Assumption 9 2SLS.2

1. rank $E\left[z'z\right] = m$
2. rank $E\left[z'x\right] = k$

Under the above 2 assumptions, the 2SLS estimator obtained from a random sample is *consistent* for β

Group Mean Estimator

[L4-GM] In some situations, have instruments that can be changed into 2 groups, water (of birth/ financial year). 'Chop instrument into groups' like Moulton problem/structure.

It can be shown that group mean estimator is IV, a weighted least squares regression, where it is sufficient to know size of groups and means, do regression and obtain estimator that is equivalent to an IV estimator, that is consistent despite the fact we have an endogenous variable.

- Where x_{ig} is endogenous

- We have g *moment conditions*, if this is IV, we know exogeneity must hold, whether group 1 or 2, the error term conditional on this group needs to be equal to 0, this must hold for all groups. Essentially, we have g different groups this is really $E[y_{ig}|z_g = I] = \beta_0 + \beta_1 E[x_{ig}|i : z_g = I]$
- To estimate an expectation, we replace with an average, since this is conditional, to estimate the expectation for the first group (born in the 1-st quarter), we take the average for the first group (conditional average by restricting to the first group) - taking the means of all the groups
- We do the same for $\overline{x}$ and intuitively obtain

$$\overline{y_g} = \beta_0 + \beta_1 \overline{x_g} + \overline{u}_g$$

Doing this is the same as using dummy variables for quarter of birth in 2SLS regression, **thus** group means are consistent.

> **Exercise 5.** Group mean estimator - what happens if the number of groups = 2 If we have dummy variable, we obtain the Wald estimator (last week). Our instrument, we obtain the same expression In order to derive,
>
> 1. Regress x on dummies, x can only belong to 1, so fitted values are sample means of dependent variable x_{ig}. Fitted values $\hat{x_{ig}}$ are means $\overline{x}_g$
> 2. Apply OLS on this after we have found fitted values, the predicted values are our sample means

Angrist and Krueger

- Does compulsory school attendance affect schooling and earnings
- Using *quarter of birth* as an instrument
  1. Exclusion : Season of birth is a natural experiment and hence unrelated to innate ability, motivation or family connections
  2. Relevance : In the US, children were allowed to drop out at 16. Since the age of starting school differs, children have different lengths of schooling when they turn 16
- Potentially weak instrument and potential reasons why quarter of birth might be somewhat correlated with the error

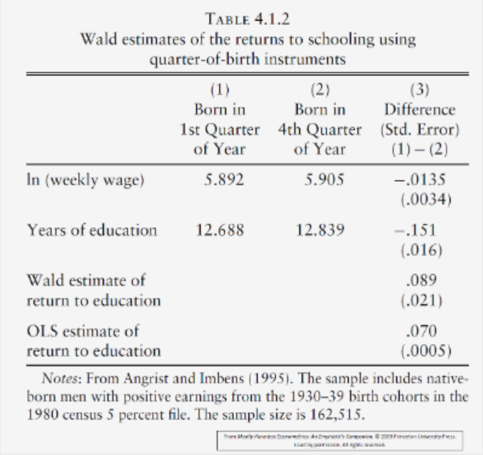


TABLE 4.1.2
Wald estimates of the returns to schooling using quarter-of-birth instruments

	(1) Born in 1st Quarter of Year	(2) Born in 4th Quarter of Year	(3) Difference (Std. Error) (1) – (2)
ln (weekly wage)	5.892	5.905	−.0135 (.0034)
Years of education	12.688	12.839	−.151 (.016)
Wald estimate of return to education			.089 (.021)
OLS estimate of return to education			.070 (.0005)

Notes: From Angrist and Imbens (1995). The sample includes native-born men with positive earnings from the 1930–39 birth cohorts in the 1980 census 5 percent file. The sample size is 162,515.

Figure 12: Wald estimates of IV - weak/exclusion violated?

Can't test by how much IV exclusion is violated, it might be best to use OLS, but in the same sense it may be incorrect - can we search for better instrument? Or,

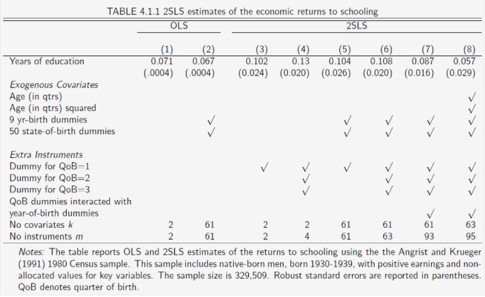


TABLE 4.1.1 2SLS estimates of the economic returns to schooling

	OLS		2SLS					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of education	0.071 (.0004)	0.067 (.0004)	0.102 (0.024)	0.13 (0.020)	0.104 (0.026)	0.108 (0.020)	0.087 (0.016)	0.057 (0.029)
Exogenous Covariates								
Age (in qtrs)								√
Age (in qtrs) squared								√
9 yr-birth dummies		√			√	√	√	√
50 state-of-birth dummies		√			√	√	√	√
Extra Instruments								
Dummy for QoB=1			√	√	√	√	√	√
Dummy for QoB=2				√		√	√	√
Dummy for QoB=3				√		√	√	√
QoB dummies interacted with year-of-birth dummies							√	√
No covariates *k*	2	61	2	2	61	61	61	63
No instruments *m*	2	61	2	4	61	63	93	95

Notes: The table reports OLS and 2SLS estimates of the returns to schooling using the the Angrist and Krueger (1991) 1980 Census sample. This sample includes native-born men, born 1930-1939, with positive earnings and non-allocated values for key variables. The sample size is 329,509. Robust standard errors are reported in parentheses. QoB denotes quarter of birth.

Figure 13: 2SLS estimates of economic returns to schooling

Inflated standard errors : $0.\frac{0.021}{0.0005} = 42$, even though the estimate is significant due to a large sample size, the 9% CI is large.

Problem also of a small $R^2_{x,z}$: the *instruments are weak*, it might be better to use OLS instead of IV

Including more instruments and covariates

- Reduces SE but comes to the cost of potentially having a weak instrument
- Col 3 : just identified 1 instrument
- Col 4 : over identified (3 QoB instruments)
- Col 5/6 : + 59 covariates to 3/4 : m - k = 0 (or 2 resp)
- Col 7 : + 30 Ifs (m-k =32)
- Col 8 : + age and age^2 to x and z ($m - k = 32$)

But there is potential to test for over identifying restrictions, using the Sargon test

Why β larger? Asymptotic variance depends on error variance, depends on the $R^2_{x,z}$ from first stage regression (x on instruments, will be higher if instruments highly relevant and vice versa).

Exercise 6. Consequences of weak instruments

1. High SE
2. Slight violation of exclusion restrictions leads to large bias

Exercise 7. Discuss intuition behind Hausman Test Exogenous regressor then both (testing for whether x endogenous) OLS and IV consistent (if we have valid instrument) If we have an endogenous regressor and OLS is not consistent, difference does not converge to 0 any more, test start follows chi-squared distribution with k degrees of freedom, to test for endogeneity 2 main assumptions, 1 test for, 1 assume

- Relies on valid instrument, otherwise $\hat{\beta}$ would not converge at all, this is almost critical assumption in Hausman test

Tutorial 1. IV and simultaneity bias We have expression for y_1 and y_2, we are going to replace this equation, since our asymptotic bias will sum to .? Asymptotic bias $= \frac{Cov(u_1,y_2)}{V(y_2)}$ Then we plug long covariance into $Cov(u_1, y_2)$ We know $cov(u_1, u_2) = 0$ and $cov(u_1, z_2) = 0$, but the problem is the variance is typically positive But depending on assumptions we can determine direction of bias based upon α_2 We show that in the formula we replace by y_1

Tutorial 2. IV is asymptotically unbiased That is $plim\ \tilde{\alpha_1} = \alpha_1$ (the IV estimator)

Tutorial 3. why can z_2 not be used as an instrument for y_1 to estimate α_2 the slope of the supply curve It is under-identified, we don't have an endogenous variable at our disposal, we don't have a shock for y_1 → we don't have an instrument, two endogenous variables require 2 instruments

Tutorial 4. Exercise 1 (tut3) Regression of log wage on education, estimate using OLS, internet, do you expect OLS to be trustworthy? Education on wage includes ability and motivation etc explaining the wages, that are correlate with education → OVB. We expect a positive omitted variable bias, since ability is likely correlated with log wages Testing relevance condition by FS regression : running education on number of siblings, this is significantly different from 0 and f-stat >10 Then running IV regression, we find the instrument has strong enough F-stat, but it could be that the exclusion restriction is violated, before we found coefficient of 0.059, with IV we find 0.122 (12%), which is higher than OLS, revealing inconsistency already, perhaps our assumption about *exogeneity* is not fulfilled.

Tutorial 5. Exercise 2 Using sibs as iv is not same as plugging sibs into education (as in proxy), we find very different result from our IV estimator, that is big diff from 0 controlling for. Education and birth quarter negatively correlated? B) C) again, we get an increase than the OLS estimator, and larger than when we used siblings as IV. But do we have similar concerns now using birth order Is birth order endogenous? Like the number of siblings? The decision to have children might be

> related to budget constraints etc. D) identification assumption $\log(wage) = \beta_0 + \beta_1$ Test whether π_2 is significantly different from 0, if we estimate our IV, we need to include all exogenous variables as instruments, we estimate a different coefficient.

Lecture 5: RCT

Tue 27 Feb 16:02

[L5-RCT]

4 Randomised Experiments

4.1 Introduction

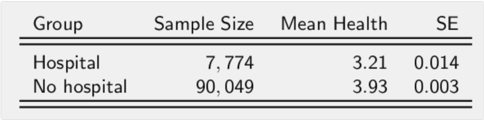
Motivation : Program Evaluation

Binary treatment on a set of outcomes, lets say the effect of having internet at home on school grades, however this would of course run into selection bias since the decision to have internet at home might depend on other unobserved factors (income etc).

Thus, program evaluation is often about how to overcome the problem of selection bias, using the potential outcomes framework allows us to illustrate this.

Example Do hospitals make people healthier? Q : Do hospitals make people healthier? If we have data on the following questions :

1. In the last 12 months have you spent a night in hospital?
2. What would you rate you health 1-5 (being excellent)



Group	Sample Size	Mean Health	SE
Hospital	7, 774	3.21	0.014
No hospital	90, 049	3.93	0.003

Figure 14: Naive Hospital Comparison

This naive comparison of individuals hospitalised and not, a difference of 0.72 suggest that non hospitalised people are healthier Thus, can we ask does going to the hospital make people sick? Maybe in some cases, but the main problem is *self selection*

- People who decide to go to the hospital are less healthy to begin with
- Even if the treatment works, such individuals won't be healthier than those who do not go to the hospital

We can formalise this with the *Potential Outcomes framework*

4.2 Potential Outcomes Framework

Treatment Allocation And Outcomes

- Start with single unit I

- Denote the outcome of interest by Y and treatment variable D
  - D = 1 the individual is *treated*
  - D= 0 the individual is *not treated* (control)
- Typical assumption is that one individual can have 2 states
  1. Y(1) - the potential outcome if I receives treatment
  2. Y(0) - the potential outcome if I 'would not' recieve the treatment (control)
- Individual Causal Effect of the treatment for observation I:

$$Y(1) - Y(0)$$

- The *problem of causal inference* - is that it is impossible to observe **both** potential outcomes at the same time, only one is realised → thus is is impossible to observe the causal effect

Stable Unit Treatment Value Assumption

- Generalisation to n units $i = 1, 2, \dots, n$
- Let D_i be the treatment for unit i
- Each unit can be exposed to the two treatments : *the problem is that in principle the potential outcomes can depend on the treatment of all units*
- *thus we make the assumption* that the potential outcome for unit i depends only on the treatment received by unit i and not on the allocation of other individuals
- Denote $D_{-i} = (D_j) : j \neq i$ treatment status of all other individuals in the population. Then SUTVA states

$$Y_i(1), Y_i(0) \perp D_{-i}$$

- Aka the 'no interference assumption'
- However, this might be violated if individuals intact
- There cannot be contagion between individuals

4.3 Imperfect Compliance

- In some cases, its impossible to enforce compliance to the randomisation
- If we were to randomise the offer to have internet at home, students who are randomised to receive a voucher for internet
- Students now can decide on both randomisation arms whether to get internet using a voucher or not (if not, can use other sources)
- Imperfect compliance can typically be subsumed under two forms
  - Encouragement design : individuals in both the treatment and control groups can decide to take up the treatment
  - Eligibility design : the control group can be prevented from taking up the treatment

Setup

- Individuals $i = 1, \ldots, n$ receive a randomised offer Z_i to take up a program
- $Z_i = 1$ if the individual is randomised into the treatment group or is offered the treatment and Z_i otherwise
- Denote the actual program participation or receipt of the treatment by D_i
- $D_i = 1$ if the individual chooses to participate and $D_i = 0$ otherwise
- If $D_i = Z_i$, we have perfect compliance. Otherwise we call the setup imperfect compliance

Intention to Treat

- Comparing individuals who are randomised in with those randomised *out* identifies the **intention to treat** effect (ITT):
$$\tau_{ITT} = E\left[Y_i|Z_i = 1\right] - E\left[Y_i|Z_i = 0\right]$$
- Since Z_i is randomised, we obtain a causal interpretation
- The effect of the randomised offer of treatment (not of the treatment itself)
- Can be a parameter of interest
- However, we are often interested in the effect of the treatment

Encouragement Design

- Individuals can choose to participate in both randomisation arms
- The participation probability conditional on the randomisation arm is in both cases non zero:
$$P(D_i = 1|Z_i = z) > 0 \quad , \quad z \in \{0, 1\}$$
- Denote the potential participation given the randomisation status by $D_i(z)$
- Observed treatment is therefore
$$D_i = D_i(0) + (D_i(1) - D_i(0))Z_i$$

Individuals receive a voucher for internet at home and can decide to get internet or not. They can also decide to get internet if they do not receive the voucher. Take-up can happen in both groups.

Subpopulations We can split the individuals in 4 different groups

1. Always Takers (AT) : individuals who will always take up the treatment regardless of their randomisation status ($D_i(1) = D_i(0) = 1$)
2. Never takers (NT) : individuals who will never take up the treatment regardless of their randomisation status ($D_i(1) = D_i(0) = 0$)
3. Compliers (C) : individuals who will do as the experiment induces them to do. They take up the treatment when they are randomised and do not when they are randomised out. $D_i(1) = 1, D_i(0) = 0$ which is equivalent to $D_i(1) - D_i(0) = 1$

4. Defiers (D) : individuals who do the opposite of what the experiment induces them to do: they do not take up the treatment when they are randomised in and they do take up the treatment when they are randomised out $(D_i(1) = 0, D_i(0) = 1)$

Late

The local average treatment effect

Let $Y_i(z, d)$ be the potential outcome for individual i with treatment status $D_i = d$ and the assignment $Z_i - z, z, d, \in \{0, 1\}$

1. Independence $[Y_i(z, d)\forall d, z, D_i(1), D_i(0)] \perp Z_i$
2. Exclusion Restriction $Y_i(d, 0) = Y_i(d, 1) = Y_i(d)$
3. First Stage $\mathrm{P}(D_i = 1|Z_i = 1) - \mathrm{P}(D_i = 1|Z_i = 0) > 0$
4. Monotonicity $D_1(1) - D_i(0) \geqslant 0$ for all I

- Independence should be satisfied by good random assignment
- Exclusion need to be discussed, not justified by random assignment since it can be violated
- The first stage ensures that compliers exists, which is equivalent to the Wald estimator in IV, giving the share of compliers under monotonicity. We assume everybody reacts to the treatment in the same way (thus ruling out the existence of defiers)
- *Monotonicity* implies that we cannot have *any defiers*. It needs to be examined but often plausible in this setting when we assume that assigning someone to the active treatment increases the incentive to take the active treatment

LATE Estimand Under the LATE assumptions, the ITT divided by the share of compliers recovers the LATE

$$E\left[Y_i(1) - Y_i(0)|D_i(1) - D_i(0)\right] = \frac{E\left[Y_i|Z_i = 1\right] - E\left[Y_i|Z_i = 0\right]}{P(D_i = 1|Z_i = 1) - P(D_i = 1|Z_i = 0)}$$

This is the average treatment effect on the subgroup of individuals whose treatment status has been affected by the assignment. In this setting, we cannot identify the ATE or AT or NT without additional assumptions

Late And Bloom Result

> **Exercise 8.** [**Late-bloom**] Starting from monotonicity, we can write total variation of binary variable, condition this equal to 1 plus same thing conditional on 0, then this term doesn't exist any more, we get rid and our numerator is equal to The switching function exists only due to the exclusion restriction, otherwise we would need to write y as a function observed by both variables **The denominator** - same thing but for FS, replace now here
>
> IV estimator provides more meaningful interpretation, provides average treatment effect for average treated people We take the numerator again Replace the observed outcome with switching equation via the switching equation, then replace switching equation since the exclusion restriction holds We also know only the non-treated PO is realised, By independence Since D_i is binary

This is a general framework for a binary IV estimator too. The LATE/ATT is obtained.

Eligibility Design

- In this setting, the individuals who are randomised in can chose to participate but those who are randomised out cannot participate
- Since the take-up on the control group arm is zero, the Wald estimand recovers the ATT
- Hence, the ITT divided by the share of participants recovers the ATT

Bloom Result Suppose the assumptions of the LATE theorem hold and $E\left[D_i|Z_i=0\right]=P\left[D_i=1|Z_i=0\right]=0$ The bloom result is

$$E\left[Y_i(1)-Y_i(0)|D_i=1\right]=\frac{E\left[Y_i|Z_i=1\right]-E\left[Y_i|Z_i=0\right]}{P\left[D_i=1|Z_i=1\right]} \tag{2}$$

The randomisation design recovers the ATT Which, we can notice, the LATE looks just like the Walt estimand from the IV for a binary instrument, which we estimate both designs via 2SLS, instrumenting D_i with Z_i

4.4 Going Further

Stratification

- Use baseline information to stratify / block the sample in order to improve precision
- Divide samples into groups to obtain an equal proportion of treated and untreated within each block
- Can decrease the variance
- Can be used to perform subgroup analysis
- Include the strata in the regression as dummies

Clustering

- If individuals can interact with each outer, the treatment status of one individual can influence the potential outcome of another
- This is a violation of SUTVA
- Randomisation at the group / cluster level where individual cannot interact
- Compute cluster-robust SE
- Often less costly to implement but increases the variance of estimator which means less power

Discussion

- Internal validity : ability to estimate causal effects with the study population
- External validity : ability to general's the results from a specific setting, to other settings (population / outcomes / contexts)
- RCTs can have a strong internal validity but are often criticised for a lack of external validity

4.5 Application

Gerber et al (2009) "Does the media matter? A field experiment measuring the effect of newspapers on voting behavior and political opinions"

- Randomised experiment to measure the effect of political news content on political behaviour and opinions
- The Washington DC are is served by the Washington times (more right leaning) and the Washington post (more left leaning)
- One month before Virginia election in November 2005, the authors run a short survey to a random selection of households
- 3347 households reported not to receive the post or the times and responded to all questions. Authors then randomly assigned these to get subscriptions to either paper for 10 weeks
- Control group DiD not get either paper in the framework of this experiment
- Households have been drawn from a voters register and consumers list
- Stratified randomisation
- One week after the election, the authors conducted a follow-up survey about political behaviour and opinions
- Authors had further access to state administrative dataset including voter turnout data for the November 2005 and 2006 elections

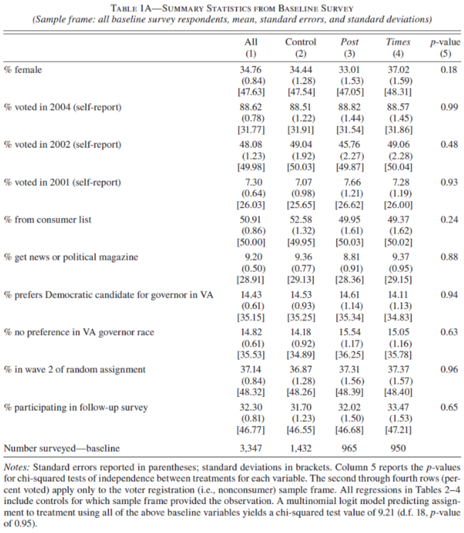


TABLE 1A—SUMMARY STATISTICS FROM BASELINE SURVEY
(Sample frame: all baseline survey respondents, mean, standard errors, and standard deviations)

	All (1)	Control (2)	*Post* (3)	*Times* (4)	*p*-value (5)
% female	34.76 (0.84) [47.63]	34.44 (1.28) [47.54]	33.01 (1.53) [47.05]	37.02 (1.59) [48.31]	0.18
% voted in 2004 (self-report)	88.62 (0.78) [31.77]	88.51 (1.22) [31.91]	88.82 (1.44) [31.54]	88.57 (1.45) [31.86]	0.99
% voted in 2002 (self-report)	48.08 (1.23) [49.98]	49.04 (1.92) [50.03]	45.76 (2.27) [49.87]	49.06 (2.28) [50.04]	0.48
% voted in 2001 (self-report)	7.30 (0.64) [26.03]	7.07 (0.98) [25.65]	7.66 (1.21) [26.62]	7.28 (1.19) [26.00]	0.93
% from consumer list	50.91 (0.86) [50.00]	52.58 (1.32) [49.95]	49.95 (1.61) [50.03]	49.37 (1.62) [50.02]	0.24
% get news or political magazine	9.20 (0.50) [28.91]	9.36 (0.77) [29.13]	8.81 (0.91) [28.36]	9.37 (0.95) [29.15]	0.88
% prefers Democratic candidate for governor in VA	14.43 (0.61) [35.15]	14.53 (0.93) [35.25]	14.61 (1.14) [35.34]	14.11 (1.13) [34.83]	0.94
% no preference in VA governor race	14.82 (0.61) [35.53]	14.18 (0.92) [34.89]	15.54 (1.17) [36.25]	15.05 (1.16) [35.78]	0.63
% in wave 2 of random assignment	37.14 (0.84) [48.32]	36.87 (1.28) [48.26]	37.31 (1.56) [48.39]	37.37 (1.57) [48.40]	0.96
% participating in follow-up survey	32.30 (0.81) [46.77]	31.70 (1.23) [46.55]	32.02 (1.50) [46.68]	33.47 (1.53) [47.21]	0.65
Number surveyed—baseline	3,347	1,432	965	950	

Notes: Standard errors reported in parentheses; standard deviations in brackets. Column 5 reports the *p*-values for chi-squared tests of independence between treatments for each variable. The second through fourth rows (percent voted) apply only to the voter registration (i.e., nonconsumer) sample frame. All regressions in Tables 2–4 include controls for which sample frame provided the observation. A multinomial logit model predicting assignment to treatment using all of the above baseline variables yields a chi-squared test value of 9.21 (d.f. 18, *p*-value of 0.95).

Figure 15: Comparison of baseline characteristics lets the authors conclude that there are not significant differences

Attrition

- Failure to collect outcome data from some individuals who were part of the initial sample used for the randomisation

- If attrition is random : reduces power
- If attrition is correlated with the treatment, may bias estimates
- Authors argue that covariates appear to be orthogonal

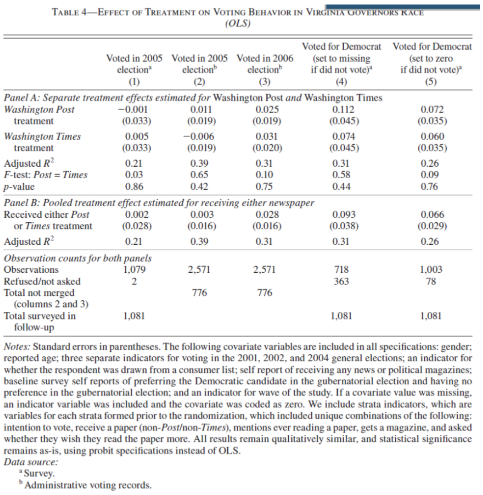


Table 4—Effect of Treatment on Voting Behavior in Virginia Governors Race
(OLS)

	Voted in 2005 election[a] (1)	Voted in 2005 election[b] (2)	Voted in 2006 election[b] (3)	Voted for Democrat (set to missing if did not vote)[a] (4)	Voted for Democrat (set to zero if did not vote)[a] (5)
Panel A: Separate treatment effects estimated for Washington Post *and* Washington Times					
Washington Post treatment	−0.001 (0.033)	0.011 (0.019)	0.025 (0.019)	0.112 (0.045)	0.072 (0.035)
Washington Times treatment	0.005 (0.033)	−0.006 (0.019)	0.031 (0.020)	0.074 (0.045)	0.060 (0.035)
Adjusted R^2	0.21	0.39	0.31	0.31	0.26
F-test: *Post* = *Times*	0.03	0.65	0.10	0.58	0.09
p-value	0.86	0.42	0.75	0.44	0.76
Panel B: Pooled treatment effect estimated for receiving either newspaper					
Received either *Post* or *Times* treatment	0.002 (0.028)	0.003 (0.016)	0.028 (0.016)	0.093 (0.038)	0.066 (0.029)
Adjusted R^2	0.21	0.39	0.31	0.31	0.26
Observation counts for both panels					
Observations	1,079	2,571	2,571	718	1,003
Refused/not asked	2			363	78
Total not merged (columns 2 and 3)		776	776		
Total surveyed in follow-up	1,081			1,081	1,081

Notes: Standard errors in parentheses. The following covariate variables are included in all specifications: gender; reported age; three separate indicators for voting in the 2001, 2002, and 2004 general elections; an indicator for whether the respondent was drawn from a consumer list; self report of receiving any news or political magazines; baseline survey self reports of preferring the Democratic candidate in the gubernatorial election and having no preference in the gubernatorial election; and an indicator for wave of the study. If a covariate value was missing, an indicator variable was included and the covariate was coded as zero. We include strata indicators, which are variables for each strata formed prior to the randomization, which included unique combinations of the following: intention to vote, receive a paper (non-*Post*/non-*Times*), mentions ever reading a paper, gets a magazine, and asked whether they wish they read the paper more. All results remain qualitatively similar, and statistical significance remains as-is, using probit specifications instead of OLS.
Data source:
[a] Survey.
[b] Administrative voting records.

Figure 16: Outcomes

Admin data : 2.8pp higher voter turnout in 2006 if received either paper

Lecture 6: Panel Data

Wed 06 Mar 12:07

[**L6**]

5 Panel Data Methods

Panel Data methods are another way of dealing with the problem of endogeneity

Panel Data

So far we have only seen a cross section of individuals (i)

Now we introduce panel which combines individual and time dimension (t).

We assume the cross section model (t=1) as :

$$Y_{i,1} = \beta_0 + x_{i,1}\beta + \alpha_i + u_{i,1} \tag{3}$$

Where we have previously estimated the impact of education on wages where *ability* is typically an unobserved omitted variable, our solutions so far have been to (a) find an IV and (b) randomise

The additional time dimension gives us new tools.

There are essentially 3 ways of getting rid of a_i, they are

1. First differences

2. Fixed effects estimator
3. Dummy variable regression

Then, assuming the FE is uncorrelated with the regressor we can use random effects approach or pooled OLS.

Intuition FD Assuming that ability is constant over time and does not change, and that we observe the same individual (i) from eq. (3) (t=2)

$$y_{i,2} = \beta_0 + x_{i,2}\beta + a_i + u_{i,2} \tag{4}$$

The idea is to take the difference between both periods of time, to get rid of the *unobserved* constant effect

Taking the difference between both periods

$$y_{i,2} - y_{i,1} = (x_{i,2} - x_{i,1})\beta + u_{i,2} - u_{i,1} \tag{5}$$

Then, if unobserved ability is constant over time, we can get rid of it.

5.1 First Differences

Typical panel data structure for T=2 (two time periods)

Assuming random sample of individuals that we observe twice at $t = 1$ and $t = 2$

We also observe an outcome at both time periods, the outcome of individual 1 in both periods and so on. We also have an intercept, and also 2 binary variables that indicate the relevant time periods. Switching "on" for each period, exactly the opposite of each other.

We also have fixed effects a_i that only vary with i, so $a_1, a_2, \ldots, a_n$.

It is important to note we do not observe all of the *fixed effects.*

$$y_{i,t} = \beta_0 + \beta_1 x_{i,t} + \delta_0 d_t^2 + a_i + u_{i,t}$$

Where

1. y_{it} is the outcome of interest, varies over i and t
2. x_{it} is the observed regressor, varying over i and t
3. M d_t^1 (resp d_t^2) is a period 1 (2) dummy varying over t (but only one enters regression - dummy var trap)
4. The unobserved fixed effect a_i only varies over i
5. $u_{i,t}$ is an unobserved idiosyncratic error
6. The time dummy ensures a time varying intercept

Pooled OLS is to estimate a composite error term since we don't observe a_i

$$y_{i,t} = \beta_0 + \beta_1 x_{i,t}\delta_0 d_t^2 + \underbrace{v_{i,t}}_{a_i+u_{i,t}},\ t = 1, 2$$

Then, *taking first differences* to difference out a_i

$$y_{i,2} - y_{i.1} = \delta_0 + \beta_1(x_{i,2} - x_{i,1}) + u_{i,2} - u_{i,1}$$
$$\Delta y_i = \delta_0 + \beta_1 \Delta x_i + \Delta u_i$$

The change in $y_{i,t}$ between period 1 and 2 is regressed on the change in the regressor(s) and a constant using n observations [**L6-LaggedDV**]

$$\Delta y_i = \delta_0 + \beta_1 \Delta x_i + \Delta u_i$$

- New intercept δ_0 : change in the intercept from $t = 1$ to $t = 2$
- We can use the normal framework provided the assumptions are fulfilled. Most importantly that Δx_i and Δu_i are uncorrelated
- This means strict exogeneity : the idiosyncratic error at each time t, u_{it} is uncorrelated with the explanatory variable in both periods
- Allows however x_{it} to be correlated with unobservable s that are constant over time
- If strict exogeneity does not hold, FD is biased and inconsistent
- Need to assume homoskedasticity of ΔU_{it} which means that we can use the normal OLS inference procedures
- Can be extended to more regressors

Variation in Covariates Over t

- Need variation in Δx_i across i
- Even if Δx_i varies only a little : leads to large SE
- Assume we have several regressors : splits covariates into 2 blocks : some change with time and some not

$$\begin{aligned} Y_{i,t} &= x_{i,t}\beta + \delta_0 d_t^2 + a_i + u_{i,t} \\ &= \underbrace{x_{i,t}^{(1)}}_{\text{changes overt}} + \beta_1 + \underbrace{x_i^{(2)}}_{\text{does not change over t}} \beta_2 + \delta_0 d_t^2 + a_i + u_{i,t} \end{aligned}$$

- Taking the first difference:

$$y_{i,2} - y_{i,1} = \delta_0 + (x_{i,2}^{(1)} - x_{i,2}^{(1)})\beta_1 + u_{i,2} - u_{i,1}$$

- The not changing variables disappear
- Only interpretation for variables changing over time

More Time Periods : T=3

$$y_{it} = \delta_0 + \delta_1 d_t^2 + \delta_3 d_t^3 + \beta_1 x_{it1} + \ldots + \beta_k x_{itk} + a_i + u_{it}\, t = 1, 2, 3$$

Estimate by pooled OLS

$$\Delta y_{it} = \delta_1 \Delta d_t^2 + \delta_3 \Delta d_t^3 + \beta_1 \Delta x_{it1} + \ldots + \beta_k \Delta x_{itk} + \Delta u_{it}\, t = 2, 3$$

- Key assumption Δu_{it} is uncorrelated with Δx_{itj} for all $j = 1, \ldots, k$ and $t = 2, 3$
- We only have $T - 1$ time periods and $T - 1$ differenced time dummies
- Constant is dropped, to include an intercept, include the time dummies starting with d^3 instead of the differenced time dummies
-

Serial Correlation

- If $T > 2$ we must assume Δu_{it} to be uncorrelated over time for inference to be valid
- Estimtated by pooled OLS :
$$\hat{\Delta} U_{i,t} = \rho \hat{\delta} u_{i,t-1} + \varepsilon_{i,t}$$
- Test $H_0 : \rho = 0$
- Potentially make the test robust to heteroskedacity
- If serial correlation suspected : cluster at the cross sectional identifier ("id") level

Drawbacks of FD

- Can be hard to e=collect panel data
- Regression only on n observations : variables are not defined for period 1
- First differencing can reduce variation in covariates
- Only solves *unobserved variable problem* if those are constant over time
- Measurement error can get worse with FD which leads to endogeneity

5.2 Fixed Effects / Within Estimator

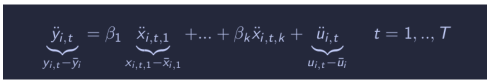
Consider the unobserved effects model:

$$y_{i,t} = \beta_1 x_{i,t,1} + \ldots + \beta_k x_{i,t,k} + a_i + u_{i,t} \quad t = 1, \ldots, T$$

Averaging for each i over time, $\hat{s}_i = \frac{1}{T}\sum_{t=1}^{T} s_{i,t}$:

$$\overline{y}_i = \beta_1 \overline{x_{i,1}} + \ldots \beta_k \overline{x_{i,k}} + a_i + \overline{u}_i \tag{6}$$

Then eliminating a_i by demeaning the variables



$$\underbrace{\ddot{y}_{i,t}}_{y_{i,t}-\bar{y}_i} = \beta_1 \underbrace{\ddot{x}_{i,t,1}}_{x_{i,t,1}-\bar{x}_{i,1}} + \ldots + \beta_k \ddot{x}_{i,t,k} + \underbrace{\ddot{u}_{i,t}}_{u_{i,t}-\bar{u}_i} \quad t = 1, .., T$$

Within transformation based on time demeaned data: uses the time variation within each cross-sectional observation Estimate by pooled OLS : Fixed Effects estimator

FE - Assumptions **FE.1** for each i in the model is

$$y_{i,t} = \beta_1 x_{i,t,1} + \ldots + \beta_k x_{i,t,k} + a_i + u_{i,t} \quad t = 1, \ldots, T$$

Where the β_j are the parameters to estimated and a_i is the unobserved effect

FE.2 we have a random sample from the cross section

FE.3 each explanatory variable changes over time (for at least some i) and no perfect linear relationship exist among the explanatory variables

FE.4 For each t, the expected value of the idiosyncratic error given the explantory variables in all time periods and the unobserved effect is zero : $E\left[u_{i,t}|X_i\,, a_i\right] = 0$

- Where X_i contains all X_{itj} for $j = 1, \ldots, k$ and $t = 1, \ldots, T$
- Under **FE.1-4** FE estimator is unbiased and consistent with fixed T and $n \longrightarrow \infty$
- Key is strict **endogeneity** (FE.4)

FE.5 $V\left[u_{i,t}|X_i, a_i\right] = V\left[u_{i,t}\right] = \sigma_u^2$ for all $t = 1, \ldots, T$

FE.6 For all $t \neq s$, the idiosyncratic errors are uncorrelated (conditional on all explanatory variables and a_i) : $\text{Cov}(u_{i,t}, u_{i,s}|X_i, a_i) = 0$

- Under FE.1-6 : FE estimator of β_j is the best linear unbiased estimator (BLUE)
- Implication : since FD is linear and unbiased, FE is more efficient
- FE.6 implies that the errors are serially uncorrelated

FE.7 conditional on X_i and a_i, the $u_{i,t}$ are iid as $\mathcal{N}(0,\,\sigma_u^2)$

- This assumption implies FE.4-6 but is stronger as it assumes normality
- This assumption implies that the FE estimator is normally distributed and t and F statistics have exact t and F distributions
- Without FE.7 : rely on asymptotics : large n, small T

In **summary**, to be unbiased under strict exogeneity, the idiosyncratic error should be uncorrelated with each explanatory variable across all time periods.

If the fixed effects are constant over time, they can be correlated with the regressors in any period, but we must be careful since constant regressors are swept away.

For inference, we need homoskedatic and serially uncorrelated error terms.

And, there is a DOF adjustment : for each i, we lose one df because few demeaning and have no intercept. Therefore we have $df = nT - n - k = n(T-1) - k$

Least Squares Dummy Variables Estimator FE estimator by dummy variable regression:

- Traditionally, consider a_i to be estimated for each i
- This represents the intercept for person i
- Appraoch : add a dummy variable for each observation I
- Without intercept, since $\Sigma_i$individual dummies = 1

By variable regression

- Gives exactly he same estimates of the β_j as regression on time demeaned data
- SEs and most stats identical
- Properly computes dof
- One can compute the estimated "intercepts" $\hat{a}_i$

Estimate the FE:

$$\hat{a}_i = \overline{y}_i - \widehat{\beta}_1 \hat{x}_{i,1} - \ldots - \widehat{\beta}_k \hat{x}_{i,k}$$

- Where the $\widehat{\beta}_j$ $j = 1, \ldots, k$ are the FE estimates
- Directly available from the dummy variable regression
- Inconsistent with fixed T but unbiased

FE vs FD

- T = 2
  - FE and FD are identical if the same model
  - For FD easy to compute heteroskedacity robust statistics
- T> 2
  - Both unbiased and consistent for T fixed and $n \longrightarrow \infty$. Therefore compare efficiency.
  - No serial correlation of $u_{i,t}$: FE is more efficient
  - If the FD error is serially uncorrelated (but not $u_{i,t}$), then FD is more efficient
  - In-between : not always easily comparable efficiency
  - Good idea to try both and check

Between Estimator Pooled estimator based on the averaged equation 6 including a constant

$$\overline{y}_i = \beta_0 + \beta_1 \overline{x}_i + a_i + \overline{u}_i$$

5.3 Random Effects

5.4 FE vs RE

5.5 Application

Tutorial

[Tutorial 5]

1. Interpretation of coefficient, 0.38% log-log

2.

3.

4. Use fixed effects since . . . Demean

Lecture 7: Differences-in-Differences

Thu 07 Mar 15:03

[L7-DID]

6 Differences-in-differences

To compare the difference between two groups before and after a change to establish causality

> **Example.** John Snow Cholera London
>
> Cholera epidemic in London, he wanted to establish that Cholera was transmitted through contaminated drinking water Since districts were served by two water companies, To compare the difference between 2 groups before and after a change to establish causality In 1849, both companies obtained their water supply from the dirty Thames In 1852 Lambeth company moved its water works upriver to an area less contaminated with sewage Death rates fell in districts supplied by Lambeth compared to the change in death rates in districts supplied by Southwark and vauxhall

Canonical DiD

- Data with a time dimension, at least 2 repeated cross sections
- An exogenous treatment : no self selection into the event and no change of behaviour in anticipation of the event
- Measure the outcomes of interest before and after the event
- Two groups, one impacted by the event and the other not

One can introduce more groups, time periods and covariates to the *canonical* 'simple 2x2' model.

6.1 Differences-in-differences - Simple Case

Following frolick and sperling (2019)

Considering the arrival of a large number of refugees in one city. The idea is to *estimate the impacts of this increase in refugees on local markets*, employment Supposing we have data on an outcome variable Y for a time period t after the influx of refugees for a time period $t-1$, before the influx of refugees. The immigrants arrive some time between $t-1$ and t. Thus the before-after-difference is $Y_t - Y_{t-1}$

Then, if the time periods are far apart, it may be that other changes have an impact during this time. Then, we can subtract the time trend that *would have happened* if no influx of refugees had occurred. With unaffected neighbouring regions helping us to identify this *unobserved trend.*

We have data for city A (arrival) and B (no arrival)

We compare the differences:

$$\begin{aligned}\Delta Y_{t,A} - \Delta Y_{t,B} &= \underbrace{(Y_{t,A} - Y_{t-1,A})}_{\text{diff over time}} - \underbrace{(Y_{t,B} - Y_{t-1,B})}_{\text{diff over time}} \\ &= \underbrace{(Y_{t,A} - Y_{t,B})}_{\text{diff between cities}} - \underbrace{(Y_{t-1,A} - Y_{t-1,B})}_{\text{diff between cities}}\end{aligned} \tag{7}$$

Where taking the differences in the 'differences' over time is the same as taking the differences in the differences between cities

The idea is to use the changes of the outcomes in the control groups to construct the counterfactual outcome for the treated

We assume the *common trends* and *SUTVA* assumptions.

Potential Outcomes Define $D = 1$, if city A and $D = 0$ if city B. Let $d_t = 1$ if $t = 1$ and $d_t = 0$ if $t = 0$. We denote the potential outcomes $Y_t(1)$ or $Y_t(0)$, where an observation is treated if $D = 1$ and $d_t = 1$. In $t = 0$, both groups do not receive treatment.

The observed outcome is a linear function of t, D and the Potential Outcomes:

$$Y_t = Y_t(1) \cdot d_t + Y_t(0)\,(1 - D \cdot d_t) \tag{8}$$

$$= \begin{cases} y_{t=1} = Y(1)_{t=1}D + Y(0)_{t=1}(1 - D), & \text{if } t = 1 \\ Y_{t=0} = Y(0)_{t=0} \quad \text{if } t = 0 \end{cases} \tag{9}$$

Assumption 10 Common Trends Assumption (CT)

During the period $[t-1, t]$, the potential non-treatment outcomes $Y(0)$ followed the same linear trend in the treatment group as in the control group:

$$E[Y(0)_{t=1} - Y(0)_{t-0}|D = 1] = E[Y(0)_{t=1} - Y(0)_{t=0}|D = 0]$$

or the *Parallel trend* or *Parallel Path*

Proof. DiD identifies ATT

Recalling that we have only treated individuals in $t = 1$ therefore in $t = 1$,

$$\tau = E\left[Y(1)_{t=1} - Y(0)_{t=1}|D = 1\right]$$

while we can also identify $E\left[Y(1)_{t=1}|D = 1\right] = E\left[Y_{t=1}|D = 1\right]$, the CT assumption helps us to identify $E\left[Y(0)_{t=1}|D = 1\right]$ Then, rearranging CT:

$$E\left[Y(0)_{t=1}|D = 1\right] = E\left[Y(0)_{t=0}|D = 1\right] + E\left[Y(0)_{t=1} - Y(0)_{t=0}|D = 0\right]$$

then, by equation 1

$$= E\left[Y_{t=0}|D = 1\right] + E\left[Y_{t=1} - Y_{t=0}|D = 0\right]$$

hence,

$$\tau = E\left[Y_{t=1} - Y_{t=0}|D = 1\right] - E\left[Y_{t=1} - Y_{t=0}|D = 0\right]$$

[Slide] □

Toy proof?

Then, taking the sample analog of the conditional expectations:

$$\hat{\tau} = \hat{E}\left[Y_{t=1} - Y_{t=0}|D = 1\right] - \hat{E}\left[Y_{t=1} - Y_{t=0}|D = 0\right]$$

$$= \hat{E}\left[Y|D = 1, t = 1\right] - \hat{E}\left[Y|D = 1, t = 0\right] - \Big\{\hat{E}\left[Y|D = 0, t = 1\right] - \hat{E}\left[Y\right.$$

That is, to estimate the Did estimator, we only need 4 data points.

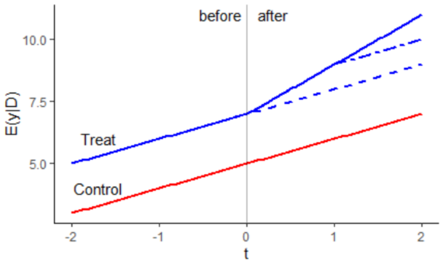


Figure 17: Illustration of Common Trends

Common Trends Extension

- The CT assumption might sometimes be hard to argue for
- In some cases more credible to hold, conditional on confounders
- That is, matching DiD or conditional DiD
- Main assumption:

For confounders not affected by the treatment, ie $X(0) = X(1) = X$, we have

$$E\left[Y(0)_{t=1} - Y(0)_{t=0}|X, D = 1\right] = E\left[Y(0)_{t=1} - Y(0)_{t=0}|X, D = 0\right]$$

6.2 Regression

We can obtain the AT estimator by linear regression, including the interaction term

$$Y_{i,g,t} = \beta_0 + \gamma D_g + \delta d_t + \tau D_g \cdot d_t + u_{i,g,t}$$

Where i would be a person, family, firm, school. Belonging to a pair (g, t), that could represent a city, state, county

Alternative Representation

There is an alternative way of writing to represent the potential non-treatment outcome $Y(0)$ as

$$Y_{i,g,t}(0) = \beta_0 + \delta d_t + \gamma D_g + u_{i,g,t}(0)$$

Where treatment status is defined as $W)_{g,t} = D_g \cdot d_t$

Then, if we are interested in the ATT, then we do not need a model for $Y(1)$ because

$$E\left[Y(1) - Y(0)|W_i = 1\right] = E\left[Y|D = 1, t = 1\right] - E\left[Y|D = 1, t = 0\right] - \{E\left[Y|D = 0, t = 1\right] - E\left[Y|D = 0, t = 0\right]\}$$

Including covariates

$$Y_{i,g,t} = \beta_0 + \gamma D_g + \delta d_t + \tau D_g \times d_t + X_{i,g,t}\theta + u_{i,g,t}$$

Where $X_{i,g,t}$ can include individual level characteristics as well as time varying variables at the group level And, individual level covariates can increase precision

6.3 Multiple Groups And Time Periods

General framework :

- Policy intervention at group level
- I belongs to a pair (g,t)
- There should be a before and after period for at least some of the groups
- Switch treatment definition. Define treatment now as $W_{g,t}$:

$$W_{g,t} = \begin{cases} 1 & \text{if group g in year t is subject to intervention} \\ 0, & \text{otherwise} \end{cases}$$

We estimate this by pooled OLS

$$\begin{aligned} y_{i,g,t} &= \delta_t + \gamma_g + \beta W_{g,t} + X_{i,g,t}\theta + u_{i,g,t} \\ & g = 1, \ldots, G \ ; \ t = 1, \ldots, T \end{aligned}$$

- Outcome and covariates measured at unit level
- δ_t is the aggregate time effect, include time dummies d_t for each t
- γ_g is the group effects. Include dummies for each group d_g
- In practice, intercept is included and one of the time and group dummies are excluded

6.4 Application

Card and Krueger (1994) Effect on minimum wage on employment

- Classical microeconomic theory predicts that higher minimum wage reduces employment in a competitive market
- 2 states : New Jersey and Pennsylvania with minimum wage at \$ 4.25
- Policy change in April 1992, NJ raised the min wage from 4.25 to 5.05
- 2 periods : Feb 1992 and Nov 1992
- Data in fast food restaurants in each period in both states
- The outcome : employment at restaurant i in state g in year t
- Analysis : compare the difference November - Feb change of employment in NJ to the difference in Pennsylvania
- Under adequate assumption, this can recover the causal effect of the policy change

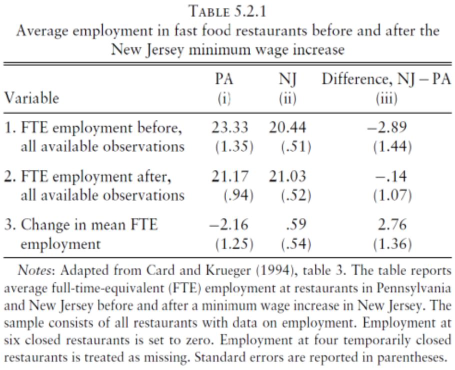


Table 5.2.1
Average employment in fast food restaurants before and after the New Jersey minimum wage increase

Variable	PA (i)	NJ (ii)	Difference, NJ – PA (iii)
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (.51)	−2.89 (1.44)
2. FTE employment after, all available observations	21.17 (.94)	21.03 (.52)	−.14 (1.07)
3. Change in mean FTE employment	−2.16 (1.25)	.59 (.54)	2.76 (1.36)

Notes: Adapted from Card and Krueger (1994), table 3. The table reports average full-time-equivalent (FTE) employment at restaurants in Pennsylvania and New Jersey before and after a minimum wage increase in New Jersey. The sample consists of all restaurants with data on employment. Employment at six closed restaurants is set to zero. Employment at four temporarily closed restaurants is treated as missing. Standard errors are reported in parentheses.

Figure 18: Source: table from Angrist and Pischke (2009), chapter 5.

- The contradiction with economic theory seems to have led to further investigation
- In a follow up study, they obtained additional payroll data and included more periods before the treatment
- In 1996, the federal minimum wage increased to $4.75 while the min wage in NJ stayed at 5.05
- A new policy experiment

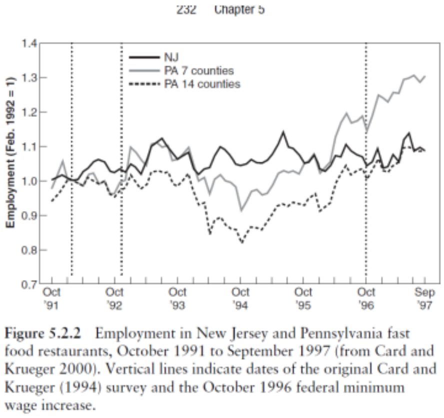


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Figure 5.2.2 Employment in New Jersey and Pennsylvania fast food restaurants, October 1991 to September 1997 (from Card and Krueger 2000). Vertical lines indicate dates of the original Card and Krueger (1994) survey and the October 1996 federal minimum wage increase.

Figure 19: Figure: Source: table from Angrist and Pischke (2009), chapter 5.

Leads And Lags If we have several pre-treatment periods, it is common to use an estimation strategy to include leads and lags

- Autor 2003 - whether increased employment protection affects firm's use of temporary help
- US labour law usually allows workers to be hired and fired at will
- Some states have allowed exceptions leading to lawsuits for unjust dismissal
- Autor wants to understand whether fear of employee law suits makes firms more likely to use temporary workers instead of hiring in workforce

- Identification: uses dummy variables to indicate state court rulings that allow exceptions to the employment-at-will doctrine and then assesses their effect on the use of temporary workers
- Includes leads and lags: 2 year ahead and 4 years behind

$$y_{i,g,t} = \delta_t + \gamma_g + \sum_{\tau=0}^{m} \beta_{-\tau} W_{g,ti\tau} + \sum_{\tau=1}^{q} \beta_{+\tau} W_{g,t+\tau} + X_{i,g,t}\theta + u_{i,g,t}$$

Where sums allow for m-lags, posttreatment effects or q leads anticipatory effects

Example

Policy where poorly performing schools are given additional financial resources

DiD compares average school outcomes between treated and control schools before and after the intervention, the school outcomes are mewasurer eint eh same hgreaf before and after the intervention (different pupils), all schools that are below a threshold are selected according to the average performance of their pupils

positive or negative treatment effect? Positive, Control group, upwards sloping students get better, we have observation at t=0, extrapolating the trend until t=1 where measured. We also know that the treatment group had the same treatment, but just before the treatment average treatment dropped. But after the treatment they should be similar on average, drawing the line would mis-identify the treatment effect

here our parallel trends assumption is violated just before the treatment,

is the common trend assumption likely to hold?

Lecture 8: Regression Discontinuity Designs

Tue 12 Mar 16:19

7 Regression Discontinuity Design

[L8-RDD] [cont]

Motivation

Regression discontinuity exploits precise knowledge of the rules determining treatment

Example such as the minimum legal drinking age, to determine the **cause effect** of legal access to alcohol on death rates (Angrist and Pischke 2014)

Suppose X_i is the individuals drinking age, x_0 the threshold (the MCDA) and D_i is legal drinking

$$D_i = \begin{cases} 1 & , \text{ if } X_i \geqslant 21 \\ 0 & , \text{ if} X_i < 21 \end{cases}$$

3 Requirements

- A score or running variable (e.g. age)
- A cutoff or threshold (e.g. 21)
- A treatment (e.g. Legal drinking)

2 different styles Sharp

- Everyone whose score is above (or below) the threshold receives treatment
- Selection on observable threshold / score

Fuzzy

- Imperfect compliance with the treatment assignment
- Leads to an IV type setup

Essentially, there are two ways of looking at the mechanism,

1. The threshold acts like a random assignment mechanism: an individual is by chance right above or below x_0
2. The threshold creates a local instrumental variable: an instrument only valid at or around the threshold

RD only provides identification around the threshold x_0

7.1 Sharp Design

Treatment status D_i is a deterministic and discontinuous function of a covariate (score) X_i Where x_0 is a known threshold

$$D_i = \begin{cases} 1 \ , & if X_i > x_0 \\ 0 \ , & if X_i < x_0 \end{cases}$$

Once we know X_i, we know D_i. With potential outcomes:

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

Treatment is a discontinuous function of X because no matter how close $X = x$ gets to x_0, treatment is unchanged until $X = x_0$

$$\lim_{\varepsilon \to 0} E\left[D|X = x_0 + \varepsilon\right] - \lim_{\varepsilon \to 0} E\left[D|X = x_0 + \varepsilon\right] = 1$$

RDD Assumption $E\left[Y(d)|X = x\right]$ is continuous in x at x_0 for $d \in 0, 1$ Potential outcomes are essentially the same on both sides of the threshold and can be violated if other things happen at the threshold. Sometimes, one finds a stronger assumption

$$Y_i(d) \perp X_i \ , \ i near x_0$$

Based on this assumption, we can identify the potential outcomes as follows:

$$E\left[Y(1)|X = x_0\right] = \lim_{\varepsilon \to 0} E\left[Y(1)|X = x_0 + \varepsilon\right] = \lim_{\varepsilon \to 0} E\left[Y|X = x_0 + \varepsilon\right]$$

similarly

$$E\left[Y(0)|X = x_0\right] = \lim_{\varepsilon \to 0} E\left[Y(0)|X = x_0 - \varepsilon\right] = \lim_{\varepsilon \to 0} E\left[Y|X = x_0 - \varepsilon\right]$$

Hence, sharp RD identifies the ATE = ATT at (or near) x_0 :

$$\tau(x_0) = E\left[Y(1) - Y(0)|X = x_0\right] = \lim_{\varepsilon \to 0} E\left[Y|X = x_0 + \varepsilon\right] - \lim_{\varepsilon \to 0} E\left[Y|X = x_0 - \varepsilon\right]$$

Estimation - Ols

Assuming linearity of the regression functions, we can use OLS To do so, we estiamte the regression to the left ($X < x_0$) and to the right of the threshold ($X \geqslant x_{00}$):

$$Y = \beta_{0,l} = \beta_{1,l}(X - x_0) + u$$
$$Y = \beta_{0,r} = \beta_{1,r}(X - x_0) + u$$

Centre the running variable around the cut-off. Taking the difference between the intercepts gives the treatment effect $\tau = \beta_{0,l} - \beta_{0,r}$

Pooled OLS The pooled regression yields the same τ:

$$Y = \beta_{0,l} + \tau D + \beta_1(X - x_0) + u$$

Near the cutoff (switching D on and off) :

$$\lim_{\varepsilon\to 0} E\left[Y|X = x_0 + \varepsilon\right] = \beta_{0,l} + \tau \text{ and} \lim_{\varepsilon\to 0} E\left[Y|X = x_0 - \varepsilon\right] = \beta_{0,l}$$

The effect of the policy is the jump at the cutoff:

$$\tau = \lim_{\varepsilon\to 0} E\left[Y|X = x_0 + \varepsilon\right] - \lim_{\varepsilon\to 0} E\left[Y|X = c_0 - \varepsilon\right]$$

This assumes the same slope above and below the cutoff

Functional Forms

- There is no value of the score at which you observe both treatment and control observations
- RD relies on extrapolation across covariate values
- Consequently, we cannot be agnostic about the functional form of the regression. Possible generalisation
  - Allow for the slopes on the right and left of the threshold to differ
  - Use more flexible specifications including non linear relationships
  - Use non-parametric estimation

Sharp And Linear Include interaction terms

$$Y = \beta_0 + \tau D + \beta_1(X - x_0) + \gamma_1 D \cdot (X - x_0) + u$$

Effect far away from cutoff

$$E\left[Y|X \geqslant x_0\right] = \beta_0 + \tau + (\beta_1 + \gamma_1)(X - x_0)$$
$$E\left[Y|X < x_0\right] = \beta_0 + \beta_1(X - x_0)$$

Near the cutoff:

$$\lim_{\varepsilon\to 0} E\left[Y|X = x_0 + \varepsilon\right] = \beta_0 + \tau \text{ and } \lim_{\varepsilon\to 0} E\left[Y|X = x_0 + \varepsilon\right] = \beta_0$$

The effect at the cut-off is still τ:

$$\tau = \lim_{\varepsilon\to 0} E\left[Y|X = x_0 + \varepsilon\right] - \lim_{\varepsilon\to 0} E\left[Y|X = x_0 + \varepsilon\right]$$

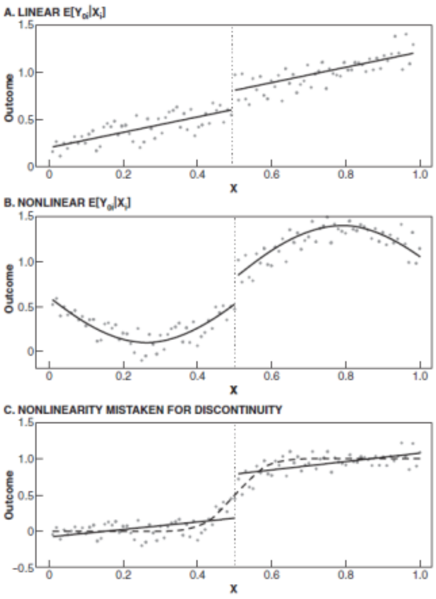

Figure 6.1.1 The sharp regression discontinuity design.

Figure 20: Non Linearities

One must be careful not to confuse a non-linear relationship with a discontinuity

Parametric RD Include higher order polynomials to account for non linearities, eg cubic polynomials

And we can allow for different slopes, e.g. a quadratic polynomial such that τ the treatment effect near the cutoff is still identified.

Non-parametric RD

- Focus only on observations near the cutoff:

$$Y = \beta_0 + \tau D + \beta_1(X - x_0) + \beta_2 D \cdot (X - x_0) + u$$

  in a sample such that $x_0 - h \leqslant X \leqslant x_0 + h$

- Where h is called the bandwidth of the window
- e.g. Is the cutoff is at 21, then one could decide to only include observations between 20 to 22 which represent a bandwidth of 1

Bandwidth Choice

- However, one has to choose a bandwidth
- There is a tradeoff
  - A small bandwidth will include observations near the cut-off and decrease a potential extrapolation bias
  - But we lose information, less observations increase the standard errors
  - This is a variance bias tradeoff

7.2 Fuzzy And Mixed Designs

Fuzzy RD

- Imperfect compliance at the threshold
- The running variable determines eligibility / encouragement Z of the treatment but individuals still can choose to take up the treatment D or not:

$$Z_i = \begin{cases} 1, & \text{if} X_i \geqslant x_0 \\ 0, & \text{if} X_i < x_0 \end{cases}$$

- The discontinuity lies now in the probability of treatment receipt but does not jump from 0 to 1 as in the sharp design

Assumption RDD - 2

$$\lim_{\varepsilon \to 0} p\left(D = 1 | X = x_0 + \varepsilon\right) \neq \lim_{\varepsilon \to 0} P\left(D = 1 | X = x_0 - \varepsilon\right)$$

Local Compliers Concept

- Let $D(x)$ be the treatment status of individual i if X was exogenously set to x
- Moving $X = x$ a bit around the threshold, leads to four different types of people
  1. Local always - takers
  2. Local never takers
  3. Local compliers
  4. Local defiers

Assumption RDD - 3

$$\{Y_i(1) - Y_i(0), D_i(x)\} \perp X_i \text{near } x_0$$

and there exists $\varepsilon > 0$ such that for all $0 < \varepsilon < e$

$$D_i(x_0 + \varepsilon) \geqslant D_i(x_0 - \varepsilon)$$

The first line : similar to an instrument exclusion restriction The second line : local monotonicity restriction which assumes away the existence of local defiers in a neighbourhood of x_0

Local LATE Under assumptions RDD - 1,2,3. Fuzzy RD identifies the local LATE:

$$\tau_{LATE}(x_0) = \lim_{\varepsilon \to 0} E\left[Y(1) - Y(0) | D(x_0 + \varepsilon) > D(x_0 - \varepsilon), X = x_0\right]$$

It can be shown that the ATE on the local compliers can be identified as:

$$\tau_{LATE}(x_0) = \frac{\lim\limits_{\varepsilon \to 0} E\left[Y | x_0 + \varepsilon\right] - \lim\limits_{\varepsilon \to 0} E\left[Y | x_0 - \varepsilon\right]}{\lim\limits_{\varepsilon \to 0} E\left[D | x_0 + \varepsilon\right] - \lim\limits_{\varepsilon \to 0} E\left[D | x_0 - \varepsilon\right]}$$

The $\tau_{LATE}(x_0)$ is local twice : for $X = x_0$ and for compliers

Mixed Design - Identification

Mixed RD recovers $ATT(x_0)$ One sided non compliance : when treatment eligibility depends strictly on the threshold Participation is voluntary when the threshold is passed. Non eligible implies no participation

$$\lim_{\varepsilon \to 0}$$

In a mixed design, under RDD - 1 and RDD - 2, we can show that the LATE and the ATT are the same at the threshold

$$\tau_t(x_0) = E\left[Y(1) - Y(0)|D = 1, X = x_0\right]$$

The main assumption needed is the mean of $Y(0)$ is continuous at the threshold (RDD-2)

Estimation

The LATE at the threshold looks just like the IV estimator at the threshold for a binary instrument. We can use standard instrumental variable techniques Recall:

$$Z_i = \begin{cases} 1, & if X_i \geqslant x_0 \\ 0, & \text{if} X_i < x_0 \end{cases}$$

The estimation follows a similar reasoning as for the sharp RD, with this time instrumenting D with Z

Thus, fuzzy RD can be described by the two equations system

$$\begin{aligned} \text{first stage} : D &= \gamma + \delta Z + g(X - x_0) + \nu \\ \text{Second stage} : Y &= \beta_0 + \tau D + f(X - x_0) + u \end{aligned}$$

Where $f(X - x_0)$ and $g(X - x_0)$ are again p-order polynomials 2LS : Instrument D with Z

Note that substituting the treatment determining equation into the outcome equation yields the reduced form

$$Y = \beta_{0,r} + \tau_r Z + f_r(X - x_0) + u_r$$

With $\tau_r = \tau\delta$. Then, if the same polynomial is used for $f(.)$ and $g(\cdot)$, 2SLS is numerically identical to $\tau = \frac{\tau_r}{\delta}$

Non-parametric Estimation

Instead of assuming the shape of regression function, assume that $E\left[Y|X = x\right] - \mu(x)$ is any function of the running variable The idea is to compare people just before and just after the threshold.

We have seen the naive non-parametric estimator: define a window (or bandwidth) around the threshold and estimate e.g. the mean for all observations above / below the cutoff. The difference in mean outcomes gives you the causal effect.

The implicit weighting function of each individual is very basic : each individual gets the same weight. However, intuitively, we would want to give higher weights to observations closer to the cut-off and less weights to those further away from the cut-off. In order to estimate, we must select a kernel and bandwidth

Kernels The weighting function is called a Kernel $K(u)$ and determines which observations are included in the estimation and how. The word kernel refers to any smooth function K such that $K(u) \geqslant 0$ and

$\int K(u)du = 1$, $\int uK(u)du = 0$ and $\sigma_K^2 = \int u^2 K(u)du > 0$ A *uniform* or *boxcar* kernel will allocate the same weight to everyone as in the naive approach: $K(u) = \frac{1}{2}1_{|u|\leqslant 1}$ Or other popular kernel choice are the Gaussian and Epanechinkov

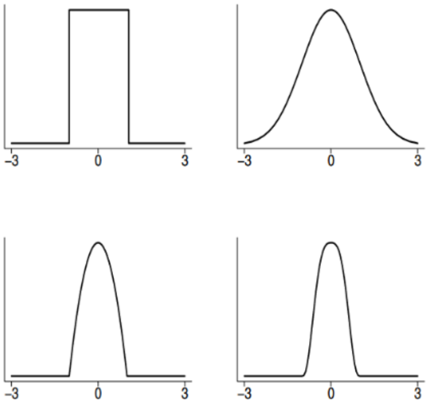

Figure 21: igure: Source: Wasserman (2006), chapter 4

Local constant kernel estimator Estiamte $\tau_r = \lim_{\varepsilon \to 0} E\left[Y|x_0 + @E\right]$ and $\tau_i = \lim_{\varepsilon \to 0} E\left[Y|x_0 - \varepsilon\right]$:

$$\hat{\tau}_r(x_0) = \frac{\Sigma_{X_i \geqslant x_0} Y_i K(\frac{X_i - x_0}{h}}{\Sigma_{X_i \geqslant x_0} K(\frac{X_i - x_0}{h})}$$

$$\hat{\tau}_l(x_0) = \frac{\Sigma_{X_i \leqslant x_0} Y_i K(\frac{X_i - x_0}{h}}{\Sigma_{X_i \leqslant x_0} K(\frac{X_i - x_0}{h})}$$

The outcomes are weighted by the kernel function. The causal effect is then estimated as follows:

$$ATE(x_0) = \hat{\tau}_r(x_0) - \hat{\tau}(x_0)$$

And it is recommended to use boundary correction kernels for the local constant kernel estimator since the estimator is biased at the boundary (cutoff)

However, since the estimation involves boundaries (cutoffs), local linear regression estimators are often preffered since they are expected to have better boundary properties than many other estimators

Non-parametric Local Linear Regression Intuition is to use a weighted regression where the weights are determined by the kernel function.

The causal effect is then estimated as follows:

$$ATE(x_0) = \hat{a}_r - \hat{a}_l$$

Note that the bandwidths h_r and h_l do not have to be the same on each side of the threshold.