Applied Quantitative Methods II

Lecture 7: Endogeneity and IVs

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Outline

OLS assumptions: short reminder

- The regression model is linear in the coefficients, is correctly specified, and has an additive error term
- The error term has a zero population mean
- Observations of the error term are uncorrelated with each other
- The error term has a constant variance
- **All explanatory variables are uncorrelated with the error term**
- No explanatory variable is a perfect linear function of any other explanatory variable(s)
- (The error term is normally distributed)

Does the OLS estimate treatment effect?

- If the OLS assumptions hold, OLS is "BLUE"
  - and estimates the treatment effect correctly
- assumption of no correlation b/w explanatory variables and error term crucial!
- Variables correlated with error term are "endogenous" variables
  - coefficients are inconsistent and biased
- If an x and the error term correlated, OLS estimate attributes to x some of the variation in y that actually comes form the error term
- If the treatment variable is endogenous, OLS does not estimate the treatment effect

Outline

Typical cases of endogeneity

1. Omitted variables
   - explanatory variable is not in equation
   - makes part of the error term
2. Selection
   - unobservable characteristic influences both dependent and explanatory variables
   - most important and widespread problem in treatment effect estimation
3. Simultaneity - reversed causation
   - relationship between dependent variable and explanatory variable in both directions
4. Measurement error
   - Some of independent variables measured with error

- **Goal**: Estimate effect of class size on educational attainment od children
- Problems:

1. Omitted variables
    - Unobserved factors influencing test scores that are correlated with class size
    - e.g. funding, teachers' ability, students' motivations...
2. Reverse causality
    - Students with low test scores might be put into a smaller classes
3. Measurement error
    - test-scores done with pen-and-paper, which brings errors on

1. Regression with Omitted variables

Classroom size and test scores

- Suppose we estimate a model

$$\textit{test scores} = \alpha + \beta \textit{class size} + \delta \textit{funding} + \omega$$

- But we don't include *funding*

$$\textit{test scores} = \alpha + \beta \textit{class size} + \epsilon$$

  - now residuals include funding: $\epsilon = \delta \textit{funding} + \omega$
  - funding correlated with class-size and test-scores: $E[\epsilon | \textit{class size}] \neq 0$
  - richer schools may pay more teachers $\rightarrow$smaller classes $\rightarrow$negative corr.

- BIAS: We expect $\beta < 0$, and OLS estimate will overstate the true value: $\beta^{OLS} = -10$
  - part of this negative correlation is due to negative corr of funding
  - we **overstate** the true effect: $\beta^{TRUE} = -5$

Example

IQ in Africa

- Do people in Africa have genetically lower IQ scores?

$$test\ scores = \alpha + \beta Africa + \epsilon$$

- Forgetting education!
- Education - much less in Africa
- Q: What bias will we have, if we assume $\beta^{TRUE} = 0$?
- A: Negative bias: e.g.$\beta^{LS} = -10$ which can lead to wrong policy conclusions!

2. Regression with reversed causality

Eli Berman - Can Hearts and Minds be bought?

- **Goal**: Effect of development spending on violence in Afghanistan

$$violence = \alpha + \beta spending + \epsilon$$

- We expect $\beta < 0$ but we get $\beta > 0$: Q: Why?
- A: regions with more violence get more money
    - two-way relationship
    - the reversed effect dominates
    - result - **upward bias** of OLS estimate

3. Measurement error- attenuation bias

ME in Independent variable!

- **Goal**: Does spending on advertising increase sales?

$$sales_t = \alpha + \beta A_t + \epsilon$$

- Assume e.g. $\beta^{TRUE} = 10$
- What if advertising measured with error? $M_t = A_t + v_t$
- correlation with sales will be weaker: $\beta^{OLS} = 7$
- If really high measurement error, we may not find any sig. effect at all
- Measurement error in *dependent* variable leads to inefficiency, not bias or inconsistency
- Results in **downward bias**

$$sales_t = \alpha + \beta(M_t - v_t) + \epsilon$$

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$$sales_t = \alpha + \beta M_t + (\beta v + \epsilon)$$

4. Self-selection

Angrist (1991): Vietnam war

- **Goal**: Estimate effect of being in army (0/1) on lifetime earnings

$$LI = \alpha + \beta MP + ... + \epsilon$$

- Idea: Military participation should decrease lifetime earnings $\beta < 0$
- Suppose we observe many factors
- MP depends on many unobserved factors in error term that are crucial for LI
  - Who goes voluntarily into army?
  - Likes office work? Value money? Academic attitudes? ...
- OLS estimates biased: it will we **too low** (bigger in abs. value)

Outline

Ways to deal with endogeneity

1. Controlled (social) experiment
   - Direct randomization of treated and untreated
2. Natural experiment
   - Finding naturally occurring treated and untreated group that are as similar as possible
3. Discontinuity design
   - Probability of treatment is changing discontinuously with a characteristic (i.e. age)
4. Propensity score matching
   - comparing "similar" people
5. Constructing proxy variables
   - e.g. IQ score proxy of abilities, reduces omitted variable bias
6. **Instrumental variable (IV)**
   - Finding variable that assigns into treatment but does not affect outcome

4. Self-selection - dealing with

Angrist (1991): Vietnam war

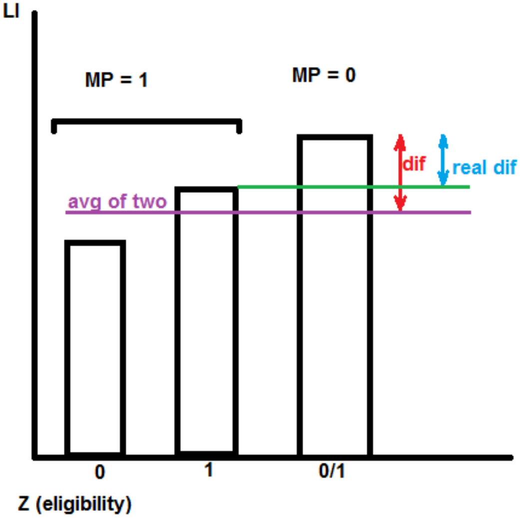
- **Goal**: Estimate effect of being in army (0/1) on lifetime earnings

$$LI = \alpha + \beta MP + ... + \epsilon$$

- **Story**: some soldiers volunteered, but then still not enough
  - lottery: days of year - all men of suff. age born that day as a group
  - lottery assigns a random number to each day of year
  - treshold value: all groups with a number above this treshold eligible into army ($Z_i = 1$)
- lottery - random and exogenous selection of **eligibility** (not of selection)
  - some were eligible but not participated (health reasons ...)
  - some were not eligible but volunteered
  - still, most according to a rule

4. Self-selection - dealing with
Angrist (1991): Vietnam war

- **Individual level:**



4. Self-selection - dealing with
Angrist (1991): Vietnam war

- **Aggregate level:**
- eligibility highly correlated with participation
- If participant eligible, by 16% higher probability of being selected
- eligibility not connected with any unobserved factors
- comparing those who were eligible with those who were not, the effect will solely be due to military participation
- eligibility as an instrument for participation
    - variability in LI then solely due to one-way relationship

1. Omitted variables - dealing with

Classroom size and test scores (Angrist and Lavy, 1999)

- We estimate $\textit{test scores} = \alpha + \beta \textit{class size} + \omega$
- Maimonides rule (from Talmud): classes should have max 40 students
  - school with 40 students $\rightarrow$1 class
  - school with 41 students $\rightarrow$2 classes (20 and 21)
  - above classical RD design: multiple discontinuities
- predicted class size in school with enrolment e_s is $m_s = \frac{e_s}{int(\frac{e_s-1}{40})+1}$
- discontinuities: 40-41, 80-81, ...
- **Instrument**: rule-calculated class-room size for the real CS
  - does not affect test-scores directly but through class-size

- Rule is not followed strictly => fuzzy design
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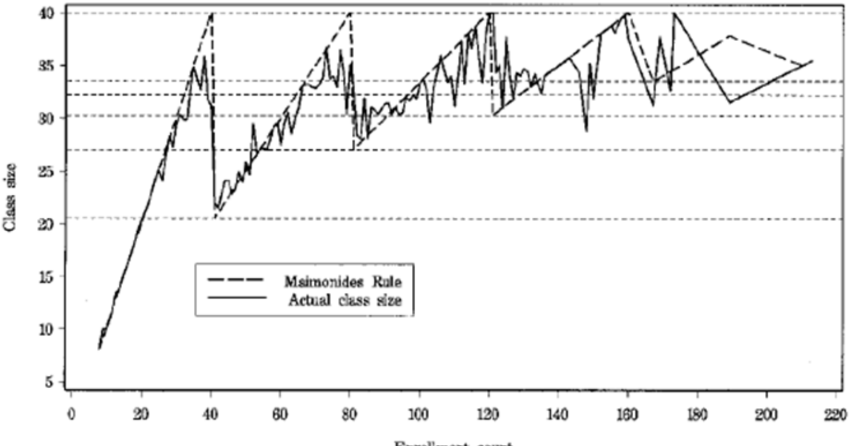
b. Fourth Grade



1. Omitted variables - dealing with

Reduced form – test scores vs. enrollment

- Average test scores partly mirror predicted class size
- Students in schools with bigger enrollment do better on average

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- Better socioeconomic background -> push for smaller classes
- Weaker students might be put in smaller classes

	5th Grade					
	Reading comprehension			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean score		74.3			67.3	
(s.d.)		(8.1)			(9.9)	
Regressors						
Class size	.221	−.031	−.025	.322	.076	.019
	(.031)	(.026)	(.031)	(.039)	(.036)	(.044)
Percent disadvantaged		−.350	−.351		−.340	−.332
		(.012)	(.013)		(.018)	(.018)
Enrollment			−.002			.017
			(.006)			(.009)
Root MSE	7.54	6.10	6.10	9.36	8.32	8.30
R^2	.036	.369	.369	.048	.249	.252
N		2,019			2,018	

1. Omitted variables - dealing with

RD results

- 1.stage – regress class size on a predicted class size + other determinants
- 2.stage – regress test scores on fitted class size
- Effect of class size is now significantly negative

	Full sample				+/− 5 Discontinuity sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Mean score			74.4		74.5	
(s.d.)			(7.7)		(8.2)	
Regressors						
Class size	−.158	−.275	−.260	−.186	−.410	−.582
	(.040)	(.066)	(.081)	(.104)	(.113)	(.181)
Percent disadvantaged	−.372	−.369	−.369		−.477	−.461
	(.014)	(.014)	(.013)		(.037)	(.037)
Enrollment		.022	.012			.053
		(.009)	(.026)			(.028)
Enrollment squared/100			.005			
			(.011)			
Piecewise linear trend				.136		
				(.032)		
Root MSE	6.15	6.23	6.22	7.71	6.79	7.15
N		2019		1961	471	

4. Measurement error - dealing with

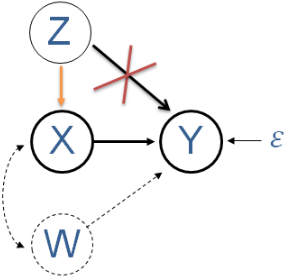
Classroom size and test scores (Angrist and Lavy, 1999)

- get better data
- do not transform data - problem will magnify
- reverse regression does not solve problem, but may create bounds for true value
- if we know severity of error, may adjust bias by that
    - if we know true variance of x from e.g. a validation study
- find a proper instrument

Outline

IV estimation is a viable approach

- An "instrumental variable" (IV) for X is one solution to the problem of omitted variables bias
- Requirements for Z to be a valid instrument for X:
  - **Relevant** = Correlated with X
  - **Exogenous** = Not correlated with Y in other way *except through* its correlation with X



Important point about IV models

- Is it true that *"a good instrument should not be correlated with the dependent variable"*? NO!
- Z **has to be** correlated with Y, otherwise it is useless as an instrument
  - It can only be correlated with Y through X
- A good instrument **must not be** correlated with the unobserved determinants of Y
  - otherwise inconsistent

Problem of weak IV

- Bound, Jaeger and Baker (1995)
  - "the cure can be worse than disease": if the excluded instruments are only weakly correlated with the endogenous variables
  - IV estimates will be biased in same direction as OLS
  - Weak IV estimates may not be consistent
  - Tests of significance have incorrect size and confidence intervals are wrong
- Strong and Young (2005)
  - "rule of thumb"
  - Previously, F-stat from first stage >10 + look at partial R2 from the first stage regression
  - Now – compare F-statistics with Table1/2 from Strong and Young
  - http://www.stata.com/meeting/5nasug/wiv.pdf

Bad instruments 1

Wage and education

- We estimate $wage_i = \alpha + \beta educ_i + u_i$ with an instrument *parent educ$_i$*
- Conditions:

1. corr *parent educ$_i$* and *educ$_i$* should not be 0: is it?
    - is it not likely that educated parents have educated children?
    - educated parents may provide motivation, role models, better start, ... OK
2. corr *parent educ$_i$*and unobserved factors should be 0: is it?
    - what about *parental income*? correlated with par.education?
    - par.income positively related to children's wage - not OK

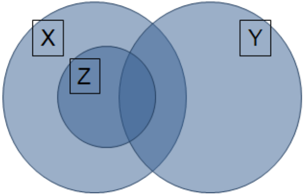
Bad instruments 2

Classroom attendance and test scores

- We estimate *test scores* $= \alpha + \beta$ *class attendance* $+ u_i$
  - unobserved: ability, interest
  - more interested/able in class more likely to attend
  - Instrument: being *pregnant*
- Conditions:
  1. corr b/w pregnancy and class attendance should not be 0: is it?
     - pregnant will miss classes often - negative correlation - OK
  2. corr b/w pregnancy and unobserved factors should be 0: is it?
     - teenage pregnancies prob. associated with lower interest/ability
     - those less interested more likely to get pregnant - not OK

Important point about IV models

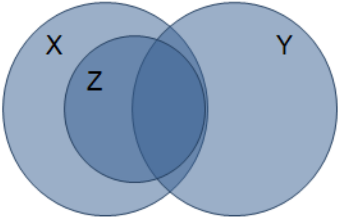
- Not all of the available variation in X is used
    - Only that portion of variation in X ,which is "explained" by Z, is used to explain Y



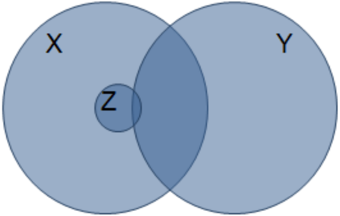
X = Endogenous variable
Y = Dependent variable
Z = Instrumental variable

Important point about IV models

- Reality:



Best-case scenario: A lot of *X* is explained by Z, and most of the overlap between *X* and *Y* is accounted for



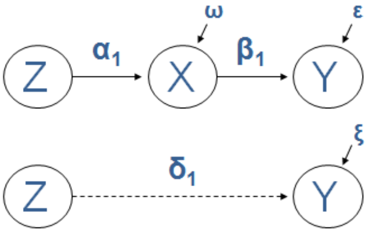
Realistic scenario: Very little of X is explained by Z, or what is explained does not overlap much with Y

Important point about IV models

- The IV estimator is biased
  - $E(b_{IV}) \neq \beta$
  - but with endogeneity: $E(b_{LS}) \neq \beta$, $plim(b_{IV}) \neq \beta$ anyway
- The good thing is, IV estimator is consistent
  - $E(b_{IV}) \rightarrow \beta \; as \; N \rightarrow \infty$
  - IV studies thus need large samples
- Asymptotic behavior or IV
  - $plim(b_{IV}) = \beta + Cov(Z, e)/Cov(Z, X)$
    - if Z truly exogenous, then $Cov(Z, e) = 0$

IV terminology

- First stage: $X = \alpha_0 + \alpha_1 Z + \omega$
- Structural model: $Y = \beta_0 + \beta_1 X + \epsilon$
- Reduced form: $Y = \delta_0 + \delta_1 Z + \xi$
- an interesting equality follows:
  - $\delta_1 = \alpha_1 \times \beta_1$ so $\beta_1 = \delta_1 / \alpha_1$



Outline

Two-stage least squares (2SLS or TSLS)

- Step 1: $X = a_0 + a_1 Z_1 + a_2 Z_2 + \ldots + a_k Z_k + u$
- Step 2: $Y = b_0 + b_1 \tilde{X} + e$
  - Substitute the fitted $\tilde{X}$ in place of original X
  - Note: if done manually in two stages, standard errors are based on wrong residuals
  - $e = Y - b_0 - b_1 \tilde{X}$ when it should be $e = Y - b_0 - b_1 X$
- Let software do it for you ;-)

Control variables in 2SLS model

- Control variables should be in both stages
  - Stage 1: $X = a_0 + a_1 Z + a_2 W + u$
  - Stage 2: $Y = b_0 + b_1 \tilde{X} + b_2 W + e$
- control vars considered "instruments", they are just not "excluded instruments"
  - they are their own instrument

Technical conditions required for model identification

1. At least as many instruments as endogenous variables
   - (order condition)
2. At least one instrument for each endogenous variable must be "strong"
   - i.e. sufficiently correlated with the endogenous variable

- Detection of weak IVs:
  - F-statistics from the first stage >10
- Weak IV estimates are not consistent!

Example

Classroom attendance and test scores

- We estimate $\textit{test scores} = \alpha + \beta \textit{class attendance} + \gamma SAT + u_i$
  - unobserved: ability, interest
  - more interested/able in class more likely to attend
  - Instrument: distance, public transport quality
- Procedure:
  1. Run $\textit{class attendance} = \delta_0 + \delta_1 SAT + \delta_2 dist + \delta_3 pubtrans + v_i$
  2. Obtain fitted values:
     $\widehat{\textit{class attendance}} = \hat{\delta}_0 + \hat{\delta}_1 SAT + \hat{\delta}_2 dist + \hat{\delta}_3 pubtrans$
  3. Run $\textit{test scores} = \alpha + \beta \widehat{\textit{class attendance}} + \gamma SAT + u_i$

Finding instrument: an art

- Geography as an instrument
  - distance, rivers, small area variation
- Legal/political institutions as an instrument
  - laws, election dynamics
- Administrative rules as an instrument
  - wage/staffing rules, reimbursement rules, eligibility rules
- Naturally occurring randomization
  - draft, birth date, lottery, roommate assignment, weather

What IV estimation measures?

- The average treatment effect for individuals "who can be induced to change [treatment] status by a change in the instrument"
  - Imbens and Angrist (1994)
- **Local Average Treatment Effect** (LATE)
- LATE is instrument-dependent, in contrast to the population ATE
- LATE is the average causal effect of X on Y for "compliers"
  - people affected by instrument (as opposed to "always takers" or "never takers")

Proximity to college study and LATE

- Who are the compliers?
  - Proximity to college means lower costs of college education
  - Low-income individuals might base their decision to go to college on proximity of college
- Who are never-takers?
  - Individual that never go to college, no matter how close they live
- Who are always-takers?
  - Individuals that always go to college, no matter how close they live (mart and/or rich)

Heterogenous treatment effects

- Changes in instrumental variable Z drive into treatment people with higher expected gains
- Example: as IV for education we use costs of education – e.g. distance to college
  - Exclusion restriction: proximity to college should not have any effect on wage other than through education
  - Result: observed distribution of ability of college students is different for low-costs and high costs students
  - Q1: what differences in distribution would you expect?
  - Q2: if I randomly assign costs to individuals, do I solve the baseline problem?
- LATE: We only measure the effect of treatment on the people whose switch from untreated to treated is triggered by the instrumental variable

Examples of IV Studies

Y (outcome)	X (determinant)	Z (instrument)	paper
Earnings, happiness	College education	Proximity to college	Card (1993)
Earnings	Years of education	Quarter of birth	Angrist & Krueger (1991)
Earnings	Military service	Vietnam era draft lottery	Angrist (1990)
Test scores	Class size	Threshold for maximum class size	Angrist & Lavy (1999)
Crime rate	Police force	Timing of elections	Levitt (1997)
Economic performance (GDP)	Institutions	Type of colonization policy	Acemoglu, Johnson & Robinson (2001)

Example: Police Hiring

- **Levitt (1997)**
- **Goal**: Estimate effect of police force on crime rate at regional level

$$\textit{crime} = \alpha + \beta \textit{police} + \epsilon$$

- Problems:

1. Measurement error
   - Mobilization of police officers (M.E. in X) as well as differential crime reporting (M.E. in Y)
2. Simultaneity
   - More police might be hired during a crime wave
3. Omitted variables
   - Unobserved characteristics of regions that affect crime rate
   - e.g. severity of punishment, activity of police

Example: Police Hiring

- Exogenous variation:
  - additional hiring of policy officers in the election years
- Relevance:
  - 2.1% change in sworn police offcers in election years, 0% in non-election years
- Exclusion restriction:
  - Is there any other channel by which election affect crime rate?
  - Yes, e.g. public welfare programs => need to include them into estimation regression

Example: Police Hiring

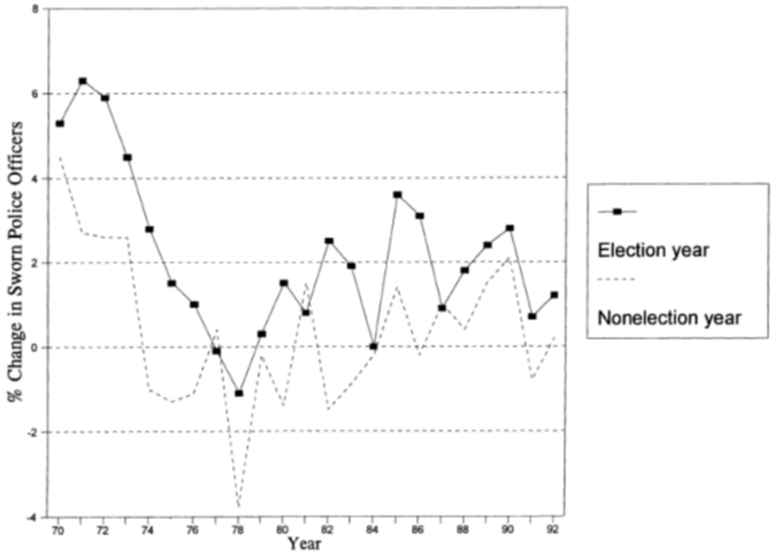


FIGURE 2. YEARLY CHANGES IN SWORN POLICE (ELECTION YEARS VERSUS NONELECTION YEARS)

Example: Police Hiring

TABLE 2—THE ELECTION CYCLE AS A PREDICTOR OF CHANGES IN THE POLICE FORCE

Variable	(1) Δln Sworn officers	(2) Δln Sworn officers	(3) Δln Sworn officers	(4) Violent crime	(5) Property crime
Mayoral election year	0.013	0.011	0.012	−0.008	−0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Gubernatorial election year	0.025	0.024	0.024	−0.006	−0.009
	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)
Δln Public welfare spending per capita	—	−0.012	−0.013	−0.010	0.008
		(0.009)	(0.009)	(0.013)	(0.011)
Δln Education spending per capita	—	0.088	0.094	0.054	−0.022
		(0.044)	(0.045)	(0.043)	(0.035)
Δ State unemployment rate	—	−0.323	−0.319	−0.286	0.645
		(0.256)	(0.258)	(0.224)	(0.182)
Δ(Percent ages 15–24 in SMSA)	—	2.41	4.69	−4.03	4.24
		(2.24)	(2.56)	(2.76)	(2.24)
Δ(Percent black)	—	0.001	−0.007	−0.018	−0.012
		(0.007)	(0.010)	(0.009)	(0.007)
Δ(Percent female-headed households)	—	−0.003	0.012	−0.002	0.013
		(0.014)	(0.019)	(0.018)	(0.014)
Year indicators?	Yes	Yes	Yes	Yes	Yes
City-size indicators?	No	Yes	Yes	Yes	Yes
City-fixed effects?	No	No	Yes	Yes	Yes
P-value: Joint significance of election years?	<0.001	<0.001	<0.01	0.082	0.062
R^2	0.06	0.09	0.11	0.21	0.37

Example: Police Hiring

TABLE 5—CRIME-SPECIFIC ESTIMATES OF THE EFFECT OF CHANGES IN SWORN OFFICERS

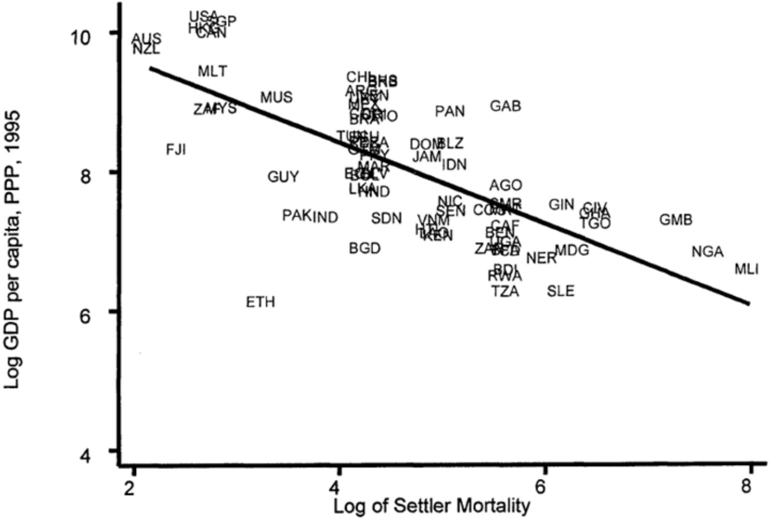
	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
OLS (levels)	0.27 (0.06)	−0.07 (0.05)	0.64 (0.05)	0.34 (0.06)	0.08 (0.05)	0.14 (0.05)	0.38 (0.06)
OLS (differences)	−0.60 (0.19)	−0.06 (0.13)	−0.31 (0.10)	0.11 (0.13)	−0.25 (0.08)	−0.10 (0.06)	−0.29 (0.10)
2SLS (elections as instruments)	−3.05 (0.91)	0.67 (1.22)	−1.20 (1.31)	−0.82 (1.20)	−0.58 (1.55)	0.26 (1.66)	−0.61 (1.31)
2SLS (election*city-size interactions as instruments)	−2.09 (0.64)	0.08 (0.84)	−0.38 (0.89)	−0.36 (0.81)	−0.39 (1.06)	0.06 (1.20)	0.14 (0.89)
2SLS (election*region interactions as instruments)	−1.18 (0.39)	−0.11 (0.49)	−0.49 (0.53)	−0.41 (0.50)	−0.11 (0.62)	−0.21 (0.67)	−0.34 (0.53)
LIML (election*region interactions as instruments)	−1.98 (0.59)	−0.27 (0.77)	−0.79 (0.79)	−1.09 (0.73)	−0.05 (0.90)	−0.43 (1.01)	−0.50 (0.80)

- Why differences across crime types?

Example: Institutions and economic development

- **Acemoglu, Johnson & Robinson (2001)**
- Basic question: How do institutions affect economic performance?
- Estimate the effect of contemporary institutional quality (protection against appropriation) on the log GDP per capita
- Identification issues:
  - Rich economies can afford better institutions
  - Omitted variable problem (other variable affecting both growth and quality of institutions)

Example: Institutions and economic development



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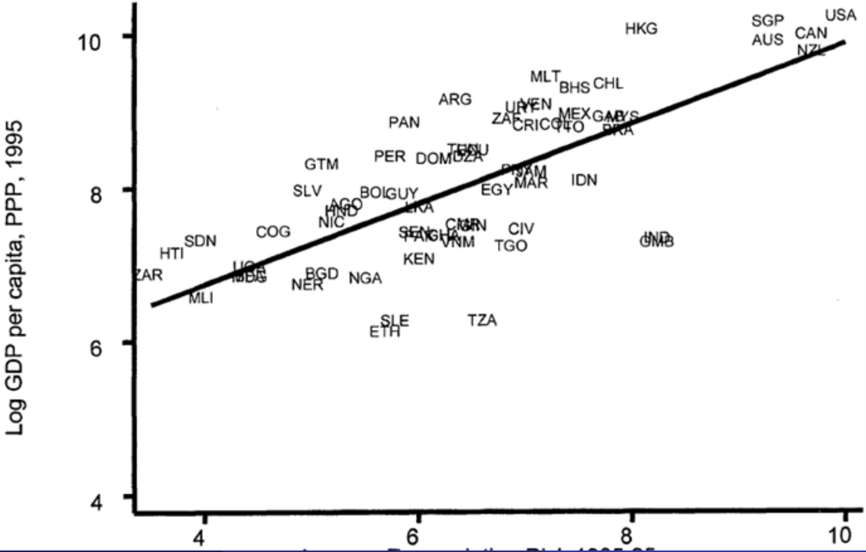
- Colonial Origins of Development

Example: Institutions and economic development

- Exogenous variation: 2 types of colonization policies:
  - "extractive" – transfer resources from colony, no strong institutions (e.g. Congo)
  - "Neo-Europes" – intention to settle down, replication of domestic institutions, strong emphasis on private property
- Colonisation strategy affected by feasibility of settlement – prevalence disease
- Ass.: Colonial institutions persisted even after independence

Example: Institutions and economic development

- 

Example: Institutions and economic development

- Instrument:
  - historical settler mortality
- Relevance (strength of IV):
  - lower mortality => creation of institutions => persist until today
- Exclusion restriction:
  - there is no link between mortality rates and current income per capita other than via correlation with institutions
- Data:
  - 75 colonized nations, 64 with complete data
  - IV: Soldier, bishop and sailor mortality rates
  - Institutions: Index of protection against appropriation (1985-95)
  - Economic performance: GDP per capita (1995)

Example: Institutions and economic development

	Base sample (1)	Base sample (2)	Base sample without Neo-Europes (3)	Base sample without Neo-Europes (4)	Base sample without Africa (5)	Base sample without Africa (6)	Base sample with continent dummies (7)	Base sample with continent dummies (8)	Base sample, dependent variable is log output per worker (9)
Panel A: Two-Stage Least Squares									
Average protection against expropriation risk 1985–1995	0.94	1.00	1.28	1.21	0.58	0.58	0.98	1.10	0.98
	(0.16)	(0.22)	(0.36)	(0.35)	(0.10)	(0.12)	(0.30)	(0.46)	(0.17)
Latitude		−0.65		0.94		0.04		−1.20	
		(1.34)		(1.46)		(0.84)		(1.8)	
Asia dummy							−0.92	−1.10	
							(0.40)	(0.52)	
Africa dummy							−0.46	−0.44	
							(0.36)	(0.42)	
"Other" continent dummy							−0.94	−0.99	
							(0.85)	(1.0)	
Panel B: First Stage for Average Protection Against Expropriation Risk in 1985–1995									
Log European settler mortality	−0.61	−0.51	−0.39	−0.39	−1.20	−1.10	−0.43	−0.34	−0.63
	(0.13)	(0.14)	(0.13)	(0.14)	(0.22)	(0.24)	(0.17)	(0.18)	(0.13)
Latitude		2.00		−0.11		0.99		2.00	
		(1.34)		(1.50)		(1.43)		(1.40)	
Asia dummy							0.33	0.47	
							(0.49)	(0.50)	
Africa dummy							−0.27	−0.26	
							(0.41)	(0.41)	
"Other" continent dummy							1.24	1.1	
							(0.84)	(0.84)	
R^2	0.27	0.30	0.13	0.13	0.47	0.47	0.30	0.33	0.28
Panel C: Ordinary Least Squares									
Average protection against expropriation risk 1985–1995	0.52	0.47	0.49	0.47	0.48	0.47	0.42	0.40	0.46
	(0.06)	(0.06)	(0.08)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)
Number of observations	64	64	60	60	37	37	64	64	61

Conclusion

- When can we use IVs?
- What is the underlying idea?
- What are the basic conditions?

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