Econ 2140, spring 2018, Part Ia
Causality, Identification, Instrumental variables

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Takeaways for this part of class

1. fundamental notions of causal inference:
    - causality
    - structural objects
    - identification
2. identification approaches:
    - randomized experiments
    - instrumental variables
    - conditional independence
    - difference in differences
    - regression discontinuity
3. analog estimators

Roadmap

Basic concepts

Origins: systems of structural equations

Alternative representations of linear IV

Treatment effects

Instrumental variables

Causal and structural objects in economics

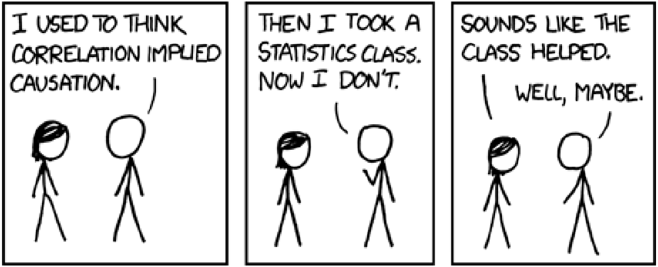
- Returns to schooling
- Elasticity of the tax base with respect to tax rates
- Effect of minimum wage on employment
- Effect of deworming pills on school attendance
- Price elasticity of demand for gasoline
- ...

Correlation and causality

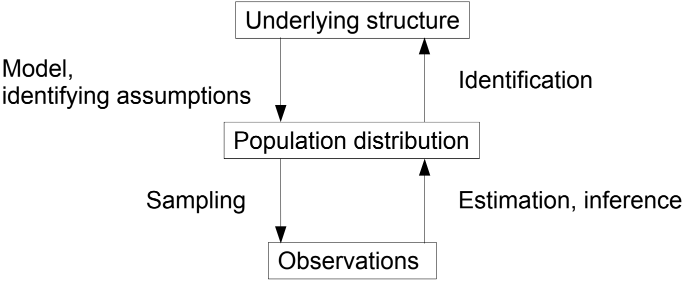
Do observable distributions tell us something about causality?

- College graduates earn x% more than high school graduates
- Countries with higher GDP have higher tax rates on average
- Minimum wage levels seem uncorrelated with unemployment levels across time and space
- Gasoline consumption and gasoline price are negatively correlated over time
- ...

Figure: correlation and causation


I USED TO THINK CORRELATION IMPLIED CAUSATION.
THEN I TOOK A STATISTICS CLASS. NOW I DON'T.
SOUNDS LIKE THE CLASS HELPED.
WELL, MAYBE.

Identification vs inference



Identification vs inference

- goal of econometrics:
  learning interesting things (hopefully)
  about economic phenomena from observations.
- two separate components of econometrics:
  1. identification
  2. estimation and inference

Estimation and Inference

1. learning about a population distribution
2. from a finite number of observations.
3. examples:
    - estimate a difference in expectations using a difference in means
    - perform inference using a t-test.

Identification

1. learning about underlying structures, causal mechanisms
2. from a population distribution.
3. example:
   identify a causal effect
   by a difference in expectations
   if we have a randomized experiment.

- identification inverts the mapping
- from underlying structures to a population distribution
- implied by a model and identifying assumptions.

Causality

Practice problem

How would you define causality?

“Pure” statistics

- causality is meaningless
- observations only tell us about **correlations**
- more generally, joint distributions
- disclaimer: few statisticians today would say this!

Sciences

- Galileo Galilei: one of the first to follow experimental ideal
- full **control of experimental circumstances.**
- do the same thing
  ⇒ same thing happens to the outcomes you measure
- variation in experimental circumstances
  ⇒ difference in observed outcomes ≈ causal effect
- example:
  - dropping a ball from different floors of the tower of Pisa
  - different time till the ball hits the ground
- crucial component:
  external intervention (“exogenous variation”)
  ⇒ allows to interpret correlation as causation

Social and biological sciences

- economics is not physics
- this version of the experimental ideal is not very useful
- reason: many unobserved, and unknown, factors which we cannot hope to control
- ⇒ **can never replicate** experimental circumstances fully
- there is “**unobserved heterogeneity**.”

Social and biological sciences ctd

- not all is lost, however
- can still hope create experimental circumstances which are the same **on average**
- this is the idea of a **randomized experiment**!
- randomly pick treatment and control groups
  $\Rightarrow$ they are identical on average.
- “compare apples with apples.”
- many settings of interest in economics:
  not possible to run experiments
- but: definition of causality is intimately tied to randomized experiment, hypothetical or actual

Recap

- Framework of classic probability theory:
    - Does not allow to talk about causality,
    - only joint distributions.
- Causality in the sciences:
    - Additional concept:
    - External intervention / exogeneous variation
    - $\Rightarrow$ experiments.
- Causality in econometrics, biostatistics,...:
    - Additional concept:
    - Unobserved heterogeneity.
    - $\Rightarrow$ randomized experiments
    - (or “quasi-experiments”)

Structural objects

Practice problem

How would you define “structural?”

My preferred definition

- An object is structural, if it is **invariant** across relevant counterfactuals.
- Example: dropping a ball from tower of Pisa
  - acceleration is the same, no matter which floor you drop it from
  - also the same if you do this on the Eiffel tower
  - time to ground would not be the same
  - acceleration is not the same if you do this on the moon

Possibly structural objects

- “economic primitives”
  - preferences
  - technologies
  - assumed to be invariant across policy changes
- derived objects
  - demand function, as supply, and thus price, varies
  - more generally causal effects, as treatment varies

Another common use for “structural”

- full specification of parametric forms for economic primitives
- assumptions strong enough to identify these primitives
- somewhat misleading terminology:
    1. less ambitious approaches with weaker assumptions
       can also identify structural objects
    2. if assumptions for full identification of economic primitives are violated
       then the objects identified are not structural.

Origins: systems of structural equations

- econometrics pioneered by “Cowles commission” starting in the 1930s
- they were interested in demand (elasticities) for agricultural goods
- introduced systems of simultaneous equations
  - outcomes as equilibria of some structural relationships
  - goal: recover the slopes of structural relationships
  - from observations of equilibrium outcomes and exogenous shifters

System of structural equations

$$Y = A \cdot Y + B \cdot Z + U,$$

- Y: k-dimensional vector of equilibrium outcomes
- Z: l-dimensional vector of exogenous variables
- A: unknown $k \times k$ matrix of coefficients of interest
- B: unknown $k \times l$ matrix
- U: further unobserved factors affecting outcomes

Example: supply and demand

$$
\begin{aligned}
Y &= (P, Q) \\
P &= A_{12} \cdot Q + B_1 \cdot Z + U_1 \text{ demand} \\
Q &= A_{21} \cdot P + B_2 \cdot Z + U_2 \text{ supply}
\end{aligned}
$$

- demand function: relates prices to quantity supplied and shifters Z and U_1 of demand
- supply function relates quantities supplied to prices and shifters Z and U_2 of supply.
- does not really matter which of the equations puts prices on the “left hand side.’
- price and quantity in market equilibrium: solution of this system of equations.

Reduced form

- solve equation $Y = A \cdot Y + B \cdot Z + U$
  for Y as a function of Z and U
- bring $A \cdot Y$ to the left hand side,
  pre-multiply by $(I - A)^{-1} \Rightarrow$

$$Y = C \cdot Z + \eta \text{ “reduced form”}$$
$$C := (I - A)^{-1} \cdot B \text{ reduced form coefficients}$$
$$\eta := (I - A)^{-1} \cdot U$$

- suppose $E[U|Z] = 0$ (ie., Z is randomly assigned)
- then we can **identify** C from

$$E[Y|Z] = C \cdot Z.$$

Exclusion restrictions

- suppose we know C
- what we want is A, possibly B
- problem: $k \times l$ coefficients in $C = (I - A)^{-1} \cdot B$
  $k \times (k + l)$ coefficients in A and B
- $\Rightarrow$ further assumptions needed
- exclusion restrictions: assume that some of the coefficients in B or A are $= 0$.
- Example: rainfall affects grain supply but not grain demand

Supply and demand continued

- suppose Z is (i) random, $E[U|Z]=0$
- and (ii) “excluded” from the demand equation
  $\Rightarrow B_{11}=0$
- by construction, $\operatorname{diag}(A)=0$
- therefore

$$\operatorname{Cov}(Z,P)=\operatorname{Cov}(Z,A_{12}\cdot Q+B_1\cdot Z+U_1)=A_{12}\cdot\operatorname{Cov}(Z,Q),$$

- $\Rightarrow$ the slope of demand is identified by

$$A_{12}=\frac{\operatorname{Cov}(Z,P)}{\operatorname{Cov}(Z,Q)}.$$

- Z is an **instrumental variable**

Alternative ways of writing the linear IV estimand

- Linear triangular system: $Y, D \in \mathbb{R}$,

$$Y = \beta_0 + \beta_1 D + U$$
$$D = \gamma_0 + \gamma_1 Z + V.$$

- Relation to system of structural equations:
  - Y equation: structural equation (say first line in system)
  - D equation: reduced form relationship
- Exogeneity conditions (subsume exclusion, randomization), relevance condition:

$$\mathrm{Cov}(Z, U) = 0 \qquad\qquad \mathrm{Cov}(Z, V) = 0$$
$$\mathrm{Cov}(Z, D) = \gamma_1 \, \mathrm{Var}(Z) \neq 0.$$

- Under these conditions,

$$\beta_1 = \frac{\mathrm{Cov}(Z, Y)}{\mathrm{Cov}(Z, D)}.$$

Two-stage least squares

- Let $\widehat{D}$ be the predicted value from a first stage regression,

$$\widehat{D} = \gamma_0 + \gamma_1 Z,$$

- and regress $Y = \beta_0 + \beta_1 D + U$ on $\widehat{D}$,

$$Y = \alpha_0 + \alpha_1 \widehat{D} + \tilde{U}.$$

- Then, since $\text{Cov}(Z, U) = 0$,

$$\alpha_1 = \frac{\text{Cov}(\widehat{D}, Y)}{\text{Var}(\widehat{D})} = \frac{\gamma_1 \text{Cov}(Z, Y)}{\gamma_1^2 \text{Var}(Z)} = \frac{\text{Cov}(Z, Y)}{\text{Cov}(Z, D)} = \beta_1 .$$

- $\Rightarrow$ two-stage least squares identifies β_1!

Control function

- V is the residual of a first stage regression of D on Z.
- Consider a regression of Y on D and V,

$$Y = \delta_0 + \delta_1 D + \delta_2 V + W$$

- Partial regression formula:
    - δ_1 is the coefficient of a regression of $\tilde{Y}$ on $\tilde{D}$ (or of Y on $\tilde{D}$),
    - where $\tilde{Y}$, $\tilde{D}$ are the residuals of regressions on V.
- By construction:

$$\tilde{D} = \gamma_0 + \gamma_1 Z = \widehat{D}$$
$$\tilde{Y} = \beta_0 + \beta_1 \tilde{D} + \tilde{U}$$

- $\text{Cov}(Z,U) = \text{Cov}(Z,V) = 0$ implies $\text{Cov}(\tilde{D},\tilde{U}) = 0$, and thus

$$\delta_1 = \beta_1.$$

Recap

- Three numerically equivalent estimands:
  1. The slope
  $$\text{Cov}(Z,Y)/\text{Cov}(Z,D).$$
  2. The two-stage least squares slope from the regression
  $$Y=\alpha_0+\alpha_1\widehat{D}+\tilde{U},$$
  where $\tilde{U}=(\beta_1 V+U)$, and $\widehat{D}$ is the first stage predicted value $\widehat{D}=\gamma_0+\gamma_1 Z$.
  3. The slope of the regression with control
  $$Y=\delta_0+\delta_1 D+\delta_2 V+W,$$
  where the control function V is given by the first stage residual, $V=D-\gamma_0-\gamma_1 Z$.

Empirical example

- **Aizer, A. and Doyle, J. J. (2015).** Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. *The Quarterly Journal of Economics*, 130(2):759–803.
- Judges (within days, courts) randomly assigned to cases.
- Use judge-specific incarceration rate as instrument for juvenile incarceration.
- Finding: Juvenile incarceration reduces high school graduation, increases adult crime.

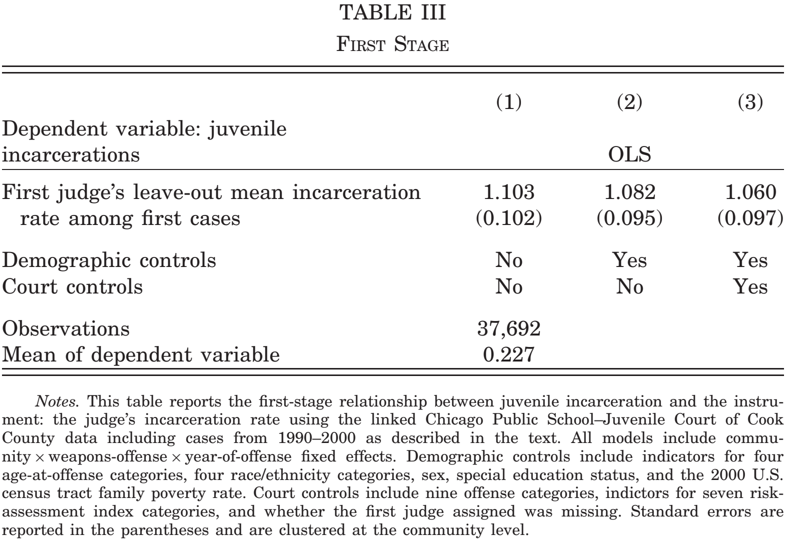


TABLE III
First Stage

	(1)	(2)	(3)
Dependent variable: juvenile incarcerations		OLS	
First judge's leave-out mean incarceration rate among first cases	1.103 (0.102)	1.082 (0.095)	1.060 (0.097)
Demographic controls	No	Yes	Yes
Court controls	No	No	Yes
Observations	37,692		
Mean of dependent variable	0.227		

Notes. This table reports the first-stage relationship between juvenile incarceration and the instrument: the judge's incarceration rate using the linked Chicago Public School–Juvenile Court of Cook County data including cases from 1990–2000 as described in the text. All models include community × weapons-offense × year-of-offense fixed effects. Demographic controls include indicators for four age-at-offense categories, four race/ethnicity categories, sex, special education status, and the 2000 U.S. census tract family poverty rate. Court controls include nine offense categories, indictors for seven risk-assessment index categories, and whether the first judge assigned was missing. Standard errors are reported in the parentheses and are clustered at the community level.

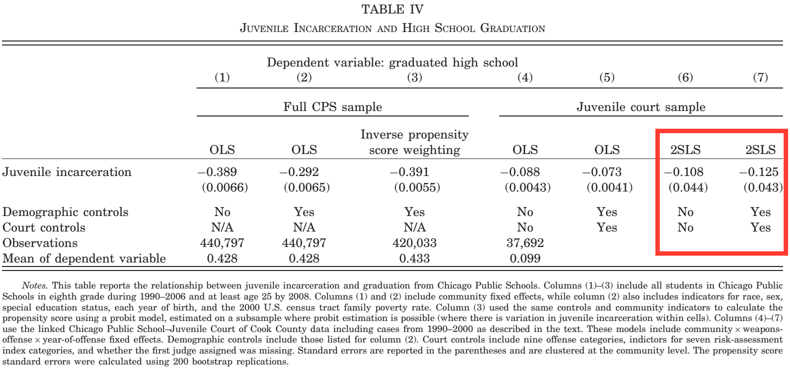


TABLE IV

JUVENILE INCARCERATION AND HIGH SCHOOL GRADUATION

	Dependent variable: graduated high school						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full CPS sample			Juvenile court sample			
	OLS	OLS	Inverse propensity score weighting	OLS	OLS	2SLS	2SLS
Juvenile incarceration	−0.389 (0.0066)	−0.292 (0.0065)	−0.391 (0.0055)	−0.088 (0.0043)	−0.073 (0.0041)	−0.108 (0.044)	−0.125 (0.043)
Demographic controls	No	Yes	Yes	No	Yes	No	Yes
Court controls	N/A	N/A	N/A	No	Yes	No	Yes
Observations	440,797	440,797	420,033	37,692			
Mean of dependent variable	0.428	0.428	0.433	0.099			

Notes. This table reports the relationship between juvenile incarceration and graduation from Chicago Public Schools. Columns (1)–(3) include all students in Chicago Public Schools in eighth grade during 1990–2006 and at least age 25 by 2008. Columns (1) and (2) include community fixed effects, while column (2) also includes indicators for race, sex, special education status, each year of birth, and the 2000 U.S. census tract family poverty rate. Column (3) used the same controls and community indicators to calculate the propensity score using a probit model, estimated on a subsample where probit estimation is possible (where there is variation in juvenile incarceration within cells). Columns (4)–(7) use the linked Chicago Public School–Juvenile Court of Cook County data including cases from 1990–2000 as described in the text. These models include community × weapons-offense × year-of-offense fixed effects. Demographic controls include those listed for column (2). Court controls include nine offense categories, indictors for seven risk-assessment index categories, and whether the first judge assigned was missing. Standard errors are reported in the parentheses and are clustered at the community level. The propensity score standard errors were calculated using 200 bootstrap replications.

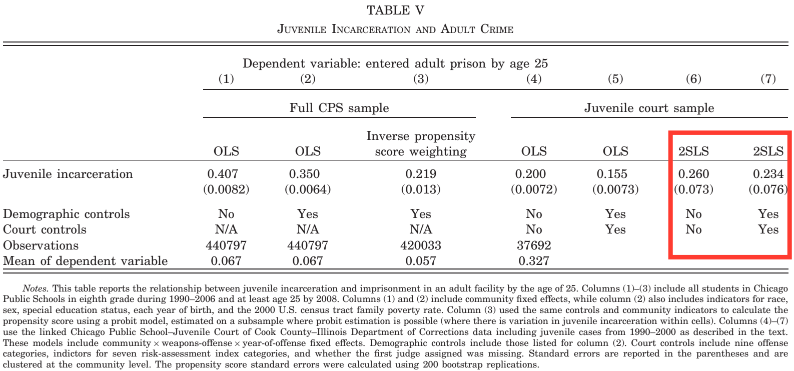


TABLE V

Juvenile Incarceration and Adult Crime

	Dependent variable: entered adult prison by age 25						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full CPS sample			Juvenile court sample			
	OLS	OLS	Inverse propensity score weighting	OLS	OLS	2SLS	2SLS
Juvenile incarceration	0.407 (0.0082)	0.350 (0.0064)	0.219 (0.013)	0.200 (0.0072)	0.155 (0.0073)	0.260 (0.073)	0.234 (0.076)
Demographic controls	No	Yes	Yes	No	Yes	No	Yes
Court controls	N/A	N/A	N/A	No	Yes	No	Yes
Observations	440797	440797	420033	37692			
Mean of dependent variable	0.067	0.067	0.057	0.327			

Notes. This table reports the relationship between juvenile incarceration and imprisonment in an adult facility by the age of 25. Columns (1)–(3) include all students in Chicago Public Schools in eighth grade during 1990–2006 and at least age 25 by 2008. Columns (1) and (2) include community fixed effects, while column (2) also includes indicators for race, sex, special education status, each year of birth, and the 2000 U.S. census tract family poverty rate. Column (3) used the same controls and community indicators to calculate the propensity score using a probit model, estimated on a subsample where probit estimation is possible (where there is variation in juvenile incarceration within cells). Columns (4)–(7) use the linked Chicago Public School–Juvenile Court of Cook County–Illinois Department of Corrections data including juvenile cases from 1990–2000 as described in the text. These models include community × weapons-offense × year-of-offense fixed effects. Demographic controls include those listed for column (2). Court controls include nine offense categories, indictors for seven risk-assessment index categories, and whether the first judge assigned was missing. Standard errors are reported in the parentheses and are clustered at the community level. The propensity score standard errors were calculated using 200 bootstrap replications.

Remarks

- historically, applied researchers have not been very careful about choosing Z for which
  (i) randomization and (ii) exclusion restriction are well justified.
- since the 1980s, more emphasis on credibility of identifying assumptions
- some additional problematic restrictions we imposed:
  1. linearity
  2. constant (non-random) slopes
  3. heterogeneity is k dimensional and enters additively
- $\Rightarrow$ causal effects assumed to be the same for everyone
- next: framework which does not impose this

Treatment effects and potential outcomes

- coming from biostatistics / medical trials
- potential outcome framework: answer to “what if” questions
- two “treatments:” $D = 0$ or $D = 1$
- eg. placebo vs. actual treatment in a medical trial
- Y_i person *i*’s outcome
  eg. survival after 2 years
- potential outcome Y_i^0:
  what if person *i* would have gotten treatment 0
- potential outcome Y_i^1:
  what if person *i* would have gotten treatment 1
- question to you: is this even meaningful?

- causal effect / treatment effect for person i : $Y_i^1 - Y_i^0$.
- average causal effect / average treatment effect:

$$ATE = E[Y^1 - Y^0],$$

- expectation averages over the population of interest

The fundamental problem of causal inference

- **we never observe both Y^0 and Y^1 at the same time**
- one of the potential outcomes is always missing from the data
- treatment D determines which of the two we observe
- formally:

$$Y = D \cdot Y^1 + (1 - D) \cdot Y^0.$$

Selection problem

- distribution of Y^1 among those with $D = 1$
  need not be the same as the distribution of Y^1 among everyone.
- in particular

$$\begin{aligned} E[Y|D=1] &= E[Y^1|D=1] \neq E[Y^1] \\ E[Y|D=0] &= E[Y^0|D=0] \neq E[Y^0] \\ E[Y|D=1] - E[Y|D=0] &\neq E[Y^1 - Y^0] = ATE. \end{aligned}$$

Randomization

- no selection ⇔ D is random

$$(Y^0, Y^1) \perp D.$$

- in this case,

$$\begin{aligned} E[Y|D=1] &= E[Y^1|D=1] = E[Y^1] \\ E[Y|D=0] &= E[Y^0|D=0] = E[Y^0] \\ E[Y|D=1] - E[Y|D=0] &= E[Y^1 - Y^0] = ATE. \end{aligned}$$

- can ensure this by actually randomly assigning D
- independence ⇒ comparing treatment and control actually compares “apples with apples”
- this gives **empirical content to** the “metaphysical” notion of **potential outcomes**!

Empirical example

- **Bertrand, M. and Mullainathan, S. (2004).** Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination. *American Economic Review*, 94(4):991–1013.
- Randomly assign names which are statistically “white” or “black” to resumes which are sent out as job applications.
- Estimate causal effect on likelihood of getting invited to a job interview.

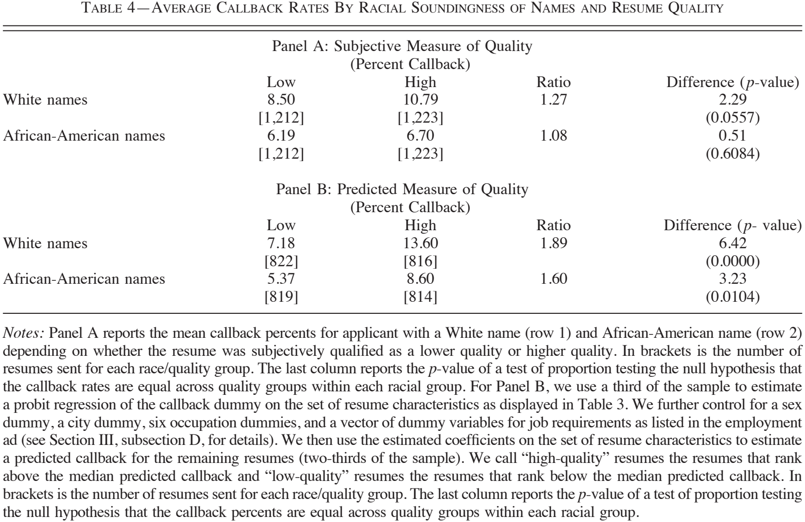


TABLE 4—AVERAGE CALLBACK RATES BY RACIAL SOUNDINGNESS OF NAMES AND RESUME QUALITY

	Low	High	Ratio	Difference (*p*-value)
	Panel A: Subjective Measure of Quality (Percent Callback)			
	Low	High	Ratio	Difference (*p*-value)
White names	8.50	10.79	1.27	2.29
	[1,212]	[1,223]		(0.0557)
African-American names	6.19	6.70	1.08	0.51
	[1,212]	[1,223]		(0.6084)
	Panel B: Predicted Measure of Quality (Percent Callback)			
	Low	High	Ratio	Difference (*p*- value)
White names	7.18	13.60	1.89	6.42
	[822]	[816]		(0.0000)
African-American names	5.37	8.60	1.60	3.23
	[819]	[814]		(0.0104)

Notes: Panel A reports the mean callback percents for applicant with a White name (row 1) and African-American name (row 2) depending on whether the resume was subjectively qualified as a lower quality or higher quality. In brackets is the number of resumes sent for each race/quality group. The last column reports the *p*-value of a test of proportion testing the null hypothesis that the callback rates are equal across quality groups within each racial group. For Panel B, we use a third of the sample to estimate a probit regression of the callback dummy on the set of resume characteristics as displayed in Table 3. We further control for a sex dummy, a city dummy, six occupation dummies, and a vector of dummy variables for job requirements as listed in the employment ad (see Section III, subsection D, for details). We then use the estimated coefficients on the set of resume characteristics to estimate a predicted callback for the remaining resumes (two-thirds of the sample). We call "high-quality" resumes the resumes that rank above the median predicted callback and "low-quality" resumes the resumes that rank below the median predicted callback. In brackets is the number of resumes sent for each race/quality group. The last column reports the *p*-value of a test of proportion testing the null hypothesis that the callback percents are equal across quality groups within each racial group.

Relation to linear structural equations?

- Linear structural equations
  are a special case of treatment effect framework.
- Suppose

$$Y = \alpha + \beta D + U,$$

- where this equation is "structural."
  $\Leftrightarrow$ Coefficients and the residual stay the same
  if treatment is changed.
- Then

$$Y^0 = \alpha + U$$
$$Y^1 = \alpha + \beta + U,$$

- and $ATE = \beta$.
- Note: Causal effects are the same for everyone in the linear framework.

Nonlinear structural equations

- without imposing linearity / restrictions on the dimension of heterogeneity:
$$Y = g(D, U).$$
- equivalent to potential outcomes
- potential outcomes notation: more compact
- structural function notation: more explicit
which determinants are held constant to define causal effects in more complex settings? Eg.

$$Y = g(D, X, W, U)$$

Instrumental variables

- recall: simultaneous equations models with exclusion restrictions
- $\Rightarrow$ instrumental variables

$$\beta = \frac{\text{Cov}(Z, Y)}{\text{Cov}(Z, D)}.$$

- we will now give a new interpretation to β
- using the potential outcomes framework, allowing for heterogeneity of treatment effects
- "Local Average Treatment Effect" (LATE)

6 assumptions

Angrist, J., Imbens, G., and Rubin, D. (1996). Identification of causal effects using instrumental variables. *Journal of the American Statistical Association*, 91(434):444–455.

1. $Z \in \{0,1\}$, $D \in \{0,1\}$
2. $Y = D \cdot Y^1 + (1-D) \cdot Y^0$
3. $D = Z \cdot D^1 + (1-Z) \cdot D^0$
4. $D^1 \geq D^0$
5. $Z \perp (Y^0, Y^1, D^0, D^1)$
6. $\text{Cov}(Z, D) \neq 0$

Discussion of assumptions

Generalization of randomized experiment

- D is "partially randomized"
- instrument Z is randomized
- D depends on Z, but is not fully determined by it

1. **Binary treatment and instrument:**
   both D and Z can only take two values
   results generalize, but things get messier without this
2. **Potential outcome equation for Y:** $Y = D \cdot Y^1 + (1 - D) \cdot Y^0$
   - *exclusion restriction*: Z does not show up in the equation determining the outcome.
   - *"stable unit treatment values assumption"* (SUTVA): outcomes are not affected by the treatment received by other units.
     excludes general equilibrium effects or externalities.

3. **Potential outcome equation for D:** $D = Z \cdot D^1 + (1 - Z) \cdot D^0$
   SUTVA; treatment is not affected by the instrument values of other units
4. **No defiers:** $D^1 \geq D^0$
   - four possible combinations for the potential treatments (D^0, D^1) in the binary setting
   - $D^1 = 0, D^0 = 1$, is excluded
   - $\Leftrightarrow$ monotonicity

Table: No defiers

	D^0	D^1
Never takers (NT)	0	0
Compliers (C)	0	1
Always takers (AT)	1	1
~~Defiers~~	1	0

5. **Randomization:** $Z \perp (Y^0, Y^1, D^0, D^1)$
   - Z is (as if) randomized.
   - in applications, have to justify both exclusion and randomization
   - no reverse causality, common cause!
6. **Instrument relevance:** $\mathrm{Cov}(Z, D) \neq 0$
   - guarantees that the IV estimand is well defined
   - there are at least some compliers
   - testable
   - near-violation: weak instruments

Graphical illustration

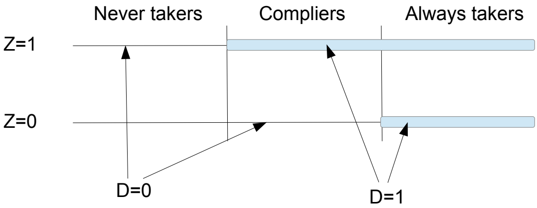


Illustration explained

- 3 groups, never takers, compliers, and always takers
- by randomization of Z:
  each group represented equally given $Z = 0$ / $Z = 1$
- depending on group:
  observe different treatment values and potential outcomes.
- will now take the IV estimand

$$\frac{\text{Cov}(Z, Y)}{\text{Cov}(Z, D)}$$

- interpret it in terms of potential outcomes:
  average causal effects for the subgroup of compliers
- idea of proof:
  take the "top part" of figure 56, and subtract the "bottom part."

Preliminary result:

If Z is binary, then

$$\frac{\text{Cov}(Z,Y)}{\text{Cov}(Z,D)} = \frac{E[Y|Z=1]-E[Y|Z=0]}{E[D|Z=1]-E[D|Z=0]}.$$

Practice problem

Try to prove this!

Proof

- Consider the covariance in the numerator:

$$\begin{aligned} \text{Cov}(Z, Y) &= E[YZ] - E[Y] \cdot E[Z] \\ = E[Y|Z=1] \cdot E[Z] - (E[Y|Z=1] \cdot E[Z] - E[Y|Z=0] \cdot E[1-Z]) \cdot E[Z] \\ &= (E[Y|Z=1] - E[Y|Z=0]) \cdot E[Z] \cdot E[1-Z]. \end{aligned}$$

- Similarly for the denominator:

$$Cov(Z, D) = (E[D|Z=1] - E[D|Z=0]) \cdot E[Z] \cdot E[1-Z].$$

- The $E[Z] \cdot E[1-Z]$ terms cancel when taking a ratio

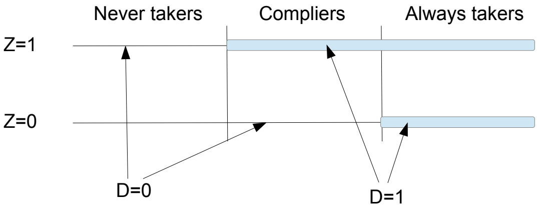
The “LATE” result

$$\frac{E[Y|Z=1]-E[Y|Z=0]}{E[D|Z=1]-E[D|Z=0]} = E[Y^1 - Y^0|D^1 > D^0]$$

Practice problem

Try to prove this!

Hint: decompose $E[Y|Z=1]-E[Y|Z=0]$ in 3 parts corresponding to our illustration


Never takers
Compliers
Always takers
Z=1
Z=0
D=0
D=1

Proof

- "top part" of figure:

$$\begin{aligned} E[Y|Z=1] &= E[Y|Z=1,NT]\cdot P(NT|Z=1) \\ &\quad + E[Y|Z=1,C]\cdot P(C|Z=1) \\ &\quad + E[Y|Z=1,AT]\cdot P(AT|Z=1) \\ &= E[Y^0|NT]\cdot P(NT) + E[Y^1|C]\cdot P(C) + E[Y^1|AT]\cdot P(AT). \end{aligned}$$

  - first equation relies on the no defiers assumption
  - second equation uses the exclusion restriction and randomization assumptions.
- Similarly

$$\begin{aligned} E[Y|Z=0] = E[Y^0|NT]\cdot P(NT) + \\ E[Y^0|C]\cdot P(C) + E[Y^1|AT]\cdot P(AT). \end{aligned}$$

proof continued:

- Taking the difference, only the complier terms remain, the others drop out:

$$E[Y|Z=1]-E[Y|Z=0]=\big(E[Y^1|C]-E[Y^0|C]\big)\cdot P(C).$$

- denominator:

$$\begin{aligned} E[D|Z=1]-E[D|Z=0] &= E[D^1]-E[D^0] \\ &= (P(C)+P(AT))-P(AT)=P(C). \end{aligned}$$

- taking the ratio, the claim follows. □

Recap

LATE result:

- take the **same statistical object** economists estimate a lot
- which used to be interpreted as average treatment effect
- **new interpretation** in a more general framework
- allowing for heterogeneity of treatment effects
- $\Rightarrow$ treatment effect for a subgroup

Practice problem

Is the LATE, $E[Y^1 - Y^0 | D^1 > D^0]$, a structural object?

Empirical example

- **Angrist, J.D. and Krueger, A.B. (1991).** Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, 106(4):979–1014.
- compare individuals born in different quarters of the year
- school start age and structure compulsory schooling laws
- ⇒ people born late in the year have to stay in school longer
- quarter of birth as an instrument for educational attainment in estimates of returns to schooling
- estimates effect for those affected by compulsory schooling laws

Practice problem

Who do you think are the compliers for the quarter of birth instrument?

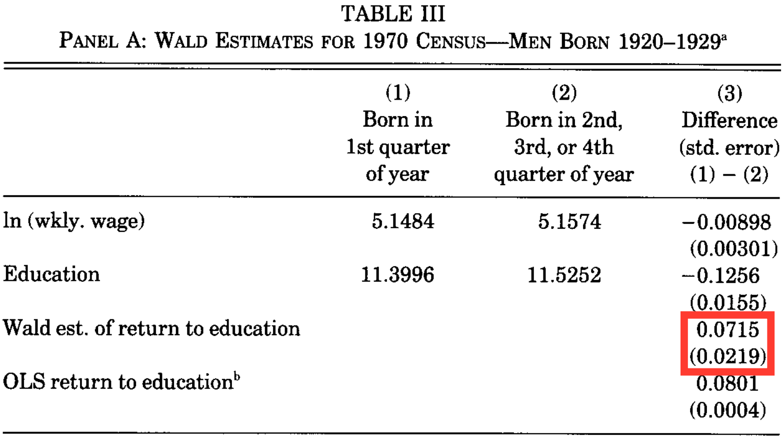
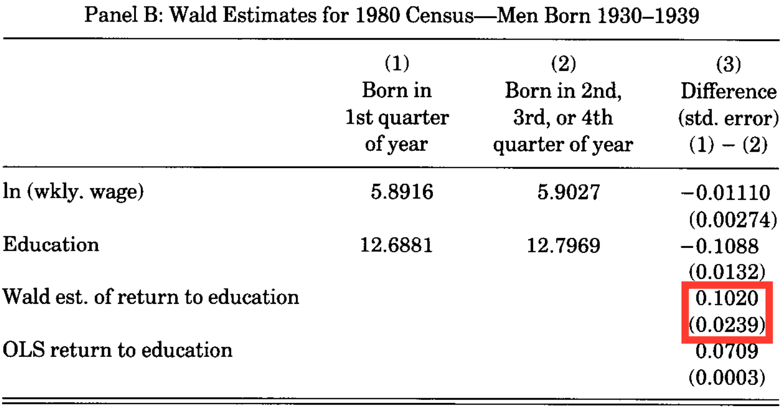


TABLE III
PANEL A: WALD ESTIMATES FOR 1970 CENSUS—MEN BORN 1920–1929[a]

	(1) Born in 1st quarter of year	(2) Born in 2nd, 3rd, or 4th quarter of year	(3) Difference (std. error) (1) − (2)
ln (wkly. wage)	5.1484	5.1574	−0.00898 (0.00301)
Education	11.3996	11.5252	−0.1256 (0.0155)
Wald est. of return to education			0.0715 (0.0219)
OLS return to education[b]			0.0801 (0.0004)



Panel B: Wald Estimates for 1980 Census—Men Born 1930–1939

	(1) Born in 1st quarter of year	(2) Born in 2nd, 3rd, or 4th quarter of year	(3) Difference (std. error) (1) − (2)
ln (wkly. wage)	5.8916	5.9027	−0.01110 (0.00274)
Education	12.6881	12.7969	−0.1088 (0.0132)
Wald est. of return to education			0.1020 (0.0239)
OLS return to education			0.0709 (0.0003)

An alternative approach: Bounds

- keep the **old structural object** of interest: average treatment effect
- but analyze its identification in the more general framework with heterogeneous treatment effects
- in general: we can learn something, not everything
- $\Rightarrow$ bounds / "**partial identification**"

Manski, C. (2003). *Partial identification of probability distributions*. Springer Verlag.

Same assumptions as before

1. $Z \in \{0,1\}$, $D \in \{0,1\}$
2. $Y = D \cdot Y^1 + (1-D) \cdot Y^0$
3. $D = Z \cdot D^1 + (1-Z) \cdot D^0$
4. $D^1 \geq D^0$
5. $Z \perp (Y^0, Y^1, D^0, D^1)$
6. $\text{Cov}(Z, D) \neq 0$

   additionally:

7. Y is bounded, $Y \in [0,1]$

Decomposing ATE in known and unknown components

- decompose $E[Y^1]$:

$$E[Y^1] = E[Y^1|NT] \cdot P(NT) + E[Y^1|C \vee AT] \cdot P(C \vee AT).$$

- terms that are identified:

$$\begin{aligned} E[Y^1|C \vee AT] &= E[Y|Z = 1, D = 1] \\ P(C \vee AT) &= E[D|Z = 1] \\ P(NT) &= E[1 - D|Z = 1] \end{aligned}$$

and thus

$$E[Y^1|C \vee AT] \cdot P(C \vee AT) = E[YD|Z = 1].$$

- Data tell us nothing about is $E[Y^1|NT]$.
  Y^1 is never observed for never takers.
- but we know, since Y is bounded, that

$$E[Y^1|NT] \in [0,1]$$

- Combining these pieces, get upper and lower bounds on $E[Y^1]$:

$$\begin{aligned} E[Y^1] \in [&E[YD|Z=1], \\ &E[YD|Z=1] + E[1-D|Z=1]]. \end{aligned}$$

- For Y^0, similarly

$$\begin{aligned} E[Y^0] \in [&E[Y(1-D)|Z=0], \\ &E[Y(1-D)|Z=0] + E[D|Z=0]]. \end{aligned}$$

- Data are uninformative about $E[Y^0|AT]$.

Practice problem

Show this.

Combining to get bounds on ATE

- lower bound for $E[Y^1]$, upper bound for $E[Y^0]$ ⇒ lower bound on $E[Y^1 - Y^0]$

$$E[Y^1 - Y^0] \geq E[YD|Z=1] - E[Y(1-D)|Z=0] - E[D|Z=0]$$

- upper bound for $E[Y^1]$, lower bound for $E[Y^0]$ ⇒ upper bound on $E[Y^1 - Y^0]$

$$E[Y^1 - Y^0] \leq E[YD|Z=1] - E[Y(1-D)|Z=0] + E[1-D|Z=1]$$

Between randomized experiments and nothing

- bounds on ATE:

$$
\begin{aligned}
E[Y^1 - Y^0] \in [&E[YD|Z=1] - E[Y(1-D)|Z=0] - E[D|Z=0],\\
&E[YD|Z=1] - E[Y(1-D)|Z=0] + E[1-D|Z=1]].
\end{aligned}
$$

- length of this interval:

$$E[1-D|Z=1] + E[D|Z=0] = P(NT) + P(AT) = 1 - P(C)$$

- Share of compliers → 1
    - interval (“identified set”) shrinks to a point
    - In the limit, $D = Z$
    - thus $(Y^1, Y^0) \perp D$ – randomized experiment
- Share of compliers → 0
    - length of the interval goes to 1
    - In the limit the identified set is the same as without instrument

Supplementary readings

- Applied microeconomics perspective:
  Angrist, J. D. and Pischke, J. S. (2009). *Mostly harmless econometrics: an empiricist's companion*. Princeton Univ Press.
- Textbook focusing on binary treatments, experiments and conditional independence:
  Imbens, G. W. and Rubin, D. B. (2015). *Causal inference in statistics, social, and biomedical sciences*. Cambridge University Press.
- Principled treatment of (partial) identification:
  Manski, C. (2003). *Partial identification of probability distributions*. Springer Verlag.
- Theoretical computer scientist on the notion of causality:
  Pearl, J. (2000). *Causality: Models, Reasoning, and Inference*. Cambridge University Press.