Econometric Evaluation of Policy Design: Introduction, and Motivating Examples

Edward Vytlacil,
Yale University

Renmin University, Department of Economics
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Treatment Effects

Course focused on identifying and estimating "treatment effects".

Examples:

- What is the effect of taking the medicine on patient's survival?
- What is the effect of individual vaccination for the flu on incidence of the flu?
- What is the effect of attending college on later earnings?
- What is the effect of incarceration on recidivism?
- What is the effect of fertility on labor supply of the mother?

Treatment Effects

Treated unit need not be an individual, for example:

- Treated unit might be a village or city:
  - What is the effect of a measles vaccination campaign implemented at the village level on village-level incidence of measles?
- Treated unit might be a firm:
  - What is the effect of receipt of management consulting on firm productivity?
- Treated unit might be a country:
  - What is the effect of a country's economic growth on incidence of civil conflict?

Fundamental Problem of Casual Inference

“Fundamental Problem of Casual Inference”’ (Holland), problem of “missing counterfactual states” (Heckman)

- Among those who received treatment, we observe what happened to them with treatment but we do not observe what would have happened to them without treatment.
- Among those who did not receive treatment, we observe what happened to them without treatment but we do not observe what would have happened to them with treatment.

Can view as a missing data problem (Heckman).

Fundamental Problem of Casual Inference

"Fundamental Problem of Casual Inference"'

- Cannot observe same person simultaneously treated and not treated.
- This problem motivates redefining parameter of interest to be population level parameter, not individual level effect.
- Identification analysis for treatment parameters seeks to exploit additional assumptions to overcome this problem in order to identify population level parameters.

Classical Issue: Selection Bias

Classical Issue: Selection Bias.

- For example:
  - sickest individuals are the ones who take the medicine.
  - high ability students are the ones who attend college.
  - those who are most inclined to commit crimes are the ones incarcerated.

Classical Solutions:

- Regression with more proxies/covariates.
- Randomized control trials.
- Instrumental variables.

Heterogeneous Effects, Sorting on Gain

Focus of my work (and that of other recent work): Heterogeneous effects and sorting on gain.

- Key issues:
  - do treatment effects vary across individuals?
  - do treatment effects vary across observationally identical individuals?
  - do individuals know and act upon some knowledge of their own idiosyncratic effect?
- For example, maybe those who attend college are the ones who benefit the most from attending college.

Heckman and Vytlacil define "essential heterogeneity" to be when treatment effects vary across observationally identical individuals and individuals select into treatment in a manner systematically related to their idiosyncratic treatment effect.

Essential Heterogeneity

Essential heterogeneity raises important complications:

- How to define parameter of interest?
- A randomized control trial need not address question of interest.
- Classical IV results no longer hold.
- Complicates identification, estimation.

Plan for my lectures:

1. Motivating applied examples,
   (a) Effect of fertility on labor supply (Angrist and Evans, 1998).
   (b) Effect of water filters on health (Berry, Fischer and Guiteras, 2016)
   (c) Effect of college on earnings (Heckamn and Li, 2004)

   Will use these examples to motivate theory/methodology.

Future Lectures:

2. Heterogeneous treatment effects, and alternative treatment effect parameters.
   (a) Counterfactual notation, relation to structural models.
   (b) Treatment effects, heterogeneity in treatment effects, selection on gain.
   (c) Treatment parameters:
      - Average treatment effect parameters,
      - Distributional treatment effect parameters,
      - Treatment effects on the distribution.

Future Lectures:

3. Overview of Alternative Approaches:
   (a) Randomized Control Trials,
   (b) Matching,
   (c) Instrumental Variables / Selection Model Approach,
   (d) Regression Discontinuity,
   (e) Bounding/Set-Identification.

   For each approach, will consider implications of heterogeneous treatment effects, and ask what parameter is actually being identified by the approach. Will include empirical examples to illustrate approaches.

Future Lectures:

4. Marginal Treatment Effect/Selection Model
    - Method developed by Heckman and Vytlacil.
    - Impose selection model with exclusion restrictions (instruments).
    - Approach seeks to identify relationship between selection and treatment effect heterogeneity.
    - Will cover theory, issues of implementation, and empirical examples.

Fertility and Labor Supply (Angrist and Evans, 1998)

Motivating Example:
effect of fertility on female labor supply.

Empirically, women with more children work less, but how much of this relationship is causal versus the result of selection?

- Perhaps having more children causes women to work less.
- Perhaps desire to work influences fertility decisions.
- In theory, expect fertility and labor supply to be jointly determined.

Fertility and Labor Supply (Angrist and Evans, 1998)

- Related to empirical literature on effect of access to birth control and abortion on female education and labor supply decisions.
- Related to empirical literature on "quantity-vs-quality" tradeoff for number of children.
- Understanding womens' fertility and labor supply decisions important for policy analysis.

Fertility and Labor Supply (Angrist and Evans, 1998)

Angrist and Evans (1998), "Children and Their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size"

- Outcome of interest: measure of labor supply of mother
  - Alternatively consider: Work/not work; weeks worked; hours worked per week; labor income.
- Exogenous covariates:
  - Age, Age at first birth, Black, Hispanic, and Other race.
- Treatment variable is indicator variable for having three or more children or remaining at two.

Fertility and Labor Supply (Angrist and Evans, 1998)

- Treatment variable is indicator variable for having three or more children or remaining at two.
- Instruments :
  1. First two children same sex.
     - In some specifications, split to first two children male and first two children female as separate instruments.
  2. Second birth resulted in twins.

Fertility and Labor Supply (Angrist and Evans, 1998)

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In classical instrumental variables (IV) framework with constant coefficients (i.e., constant treatment effect), IV conditions:

- IV Exogeneity?
- IV Relevance?

Are these restrictions plausible?

Fertility and Labor Supply (Angrist and Evans, 1998)

Data:

- From 1980 and 1990 United States Census Public Use Micro Sample (PUMS)
- Restrict sample to women aged 21-35 with 2 or more children.
- Results in sample of 398,835 women in 1980, and 380,007 women in 1990.

Fertility and Labor Supply (Angrist and Evans, 1998)

Sample statistics for 1980,
women aged 21-35 with 2 or more children.

- Fraction of women
  - with 3 or more children: .40.
  - first two births same sex: .51.
  - second birth resulted in twins: 0.009.
  - work for pay: .57.
- Average
  - Weeks worked: 21.
  - Hours per week: 19.
  - Labor income: 7,160.

Fertility and Labor Supply (Angrist and Evans, 1998)

Fraction of Families that Had Another Child by Parity and Sex of Chidlren

Sex of first two children in families with two or more children	1980 PUMS (394,835 observations) Fraction of sample	Fraction that had another child
one boy, one girl	0.494	0.372 (0.001)
two girls	0.242	0.441 (0.002)
two boys	0.264	0.423 (0.002)
(1) one boy, one girl	0.494	0.372 (0.001)
(2) both same sex	0.506	0.432 (0.001)
difference (2) − (1)	—	0.060 (0.002)

Fertility and Labor Supply (Angrist and Evans, 1998)

Wald/LATE Estimates

1980 PUMS

Variable	Mean difference by *Same sex*	Wald estimate using as covariate: *More than 2 children*	Wald estimate using as covariate: *Number of children*
More than 2 children	0.0600 (0.0016)	—	—
Number of children	0.0765 (0.0026)	—	—
Worked for pay	−0.0080 (0.0016)	−0.133 (0.026)	−0.104 (0.021)
Weeks worked	−0.3826 (0.0709)	−6.38 (1.17)	−5.00 (0.92)
Hours/week	−0.3110 (0.0602)	−5.18 (1.00)	−4.07 (0.78)
Labor income	−132.5 (34.4)	−2208.8 (569.2)	−1732.4 (446.3)

Fertility and Labor Supply (Angrist and Evans, 1998)

OLS and TSLS, 1980 PUMS

Estimation method	OLS	2SLS	2SLS
Instrument for *More than 2 children*	—	*Same sex*	*Two boys, Two girls*
Dependent variable:			
Worked for pay	−0.176	−0.120	−0.113
	(0.002)	(0.025)	(0.025)
			[0.013]
Weeks worked	−8.97	−5.66	−5.37
	(0.07)	(1.11)	(1.10)
			[0.017]
Hours/week	−6.66	−4.59	−4.37
	(0.06)	(0.95)	(0.94)
			[0.030]
Labor income	−3768.2	−1960.5	−1870.4
	(35.4)	(541.5)	(538.5)
			[0.126]
ln(*Family income*)	−0.126	−0.038	−0.045
	(0.004)	(0.064)	(0.064)
			[0.319]

Fertility and Labor Supply (Angrist and Evans, 1998)

Instruments: Same-Sex vs Twins, 1980 PUMS

Instrument for *More than 2 children*	*Same sex*	*Twins-2*
Dependent variable:		
Worked for pay	−0.125	−0.079
	(0.026)	(0.013)
Weeks worked	−5.82	−3.64
	(1.15)	(0.60)
Hours/week	−4.76	−3.33
	(0.98)	(0.51)
Labor income	−1961.7	−1262.2
	(560.5)	(292.8)
ln(*Family income*)	−0.021	−0.071
	(0.067)	(0.035)

Fertility and Labor Supply (Angrist and Evans, 1998)

Allow for treatment effect heterogeneity:

- Perhaps effects of fertility on labor supply varies across women.

If treatment effect varies across women:

- What assumptions are required for IV? Are they plausible in this example?
- How to interpret IV estimands?
- How to interpret differences in estimates for different choices of instrument?
- Are the IV results useful for policy analysis?

Will return to these questions in the related context of the quantity-vs-quality tradeoff, following Brinch, Mogstad and Wiswall (2016)

Effect of Clean Water on Health

Motivating Example: Effect of Clean Water on Health

- 52% of rural Africans lack access to clean water.
- Contaminated water linked to diarrheal diseases and other health problems.
- Diarrheal diseases:
  - Worldwide, cause 1.8 million deaths annually.
  - Responsible for 17% of deaths of African children under age five.

Berry, Fischer and Guiteras (2016)

Berry, Fischer, and Guiteras (2015)
"Eliciting and Utilizing Willingness to Pay:
Evidence from Field Trials in Northern Ghana."

Examine effect of purchase of Kosim water filter on diarrhea in Ghanna

- 26% of individuals in rural Ghanna lack access to clean water.
- Kosim water filter highly effective,
  - Does not require electricity or chemicals, but requires effort to use and maintain.
  - Cost of production is about $15 USD.
  - No demand at that price within rural Africa.

Figure: Kosim Water Filter MTE, from Berry et al (2015)

Kosim Water Filter

Figure A1: The *Kosim* filter

Source: Berry et al (2015)
"Eliciting and Utilizing Willingness to Pay:
Evidence from Field Trials in Northern Ghana."

Berry, Fischer and Guiteras (2016)

Run randomized control sample with randomized price of water filter, on two samples:

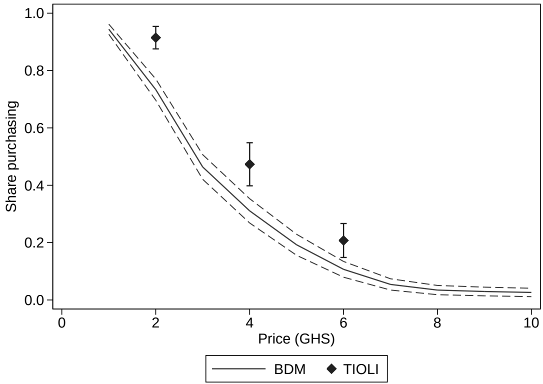
- In one sample, run experiment with a Take-It-Or-Leave-It (TIORLI) design.
- In other sample, run experiment with elicitation of willingness-to-pay for the filter using Becker-DeGroot-Marschak (BDM) mechanism.

Selective Trials: Berry, Fischer and Guiteras

- For sample of 608 individuals, ran experiment with BDM mechanism with randomized price.
- For sample of 658 individuals, ran experiment with TIOLI mechanism with randomized price.
- 24% of households had a child under age 5 with diarrhea in the past two weeks.
- Only 19% of households had access to clean water.
- Two followup surveys of both groups (one at one month and one year), surveyed:
  - If filter was purchased, was it being used and maintained properly.
  - Incidence of diarrhea for children under 5.

Figure: Inverse Demand for Filter MTE, from Berry et al (2015)

Figure 1: Inverse Demand



Notes: This figure plots the BDM demand curve, with a 90% confidence band, and take-it-or-leave-it (TIOLI) demand at three price points (2, 4 and 6 GHS), with 90% confidence intervals. The BDM demand curve indicates the share of respondents with a BDM filter bid greater than or equal to the indicated price. The TIOLI markers indicate the share of respondents who purchased the filter at the corresponding (random) price. Point-wise inference from logit regressions (at prices GHS 1, 2, . . . , 10 for BDM, 2, 4, 6 for TIOLI). Standard errors clustered at the compound (extended family) level. 608 BDM observations. 658 TIOLI observations, of which 246 at a price of 2, 224 at a price of 4, and 188 at a price of 6.

Source: Berry et al (2015)

Table: IV Estimates, from Berry et al (2015)

TABLE 5: INSTRUMENTAL VARIABLES ESTIMATES OF TREATMENT EFFECTS[a]

Dependent variable: Child aged 0-5 has had diarrhea over previous two weeks

	TIOLI Subjects		BDM Subjects		Combined all subjects	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Short-term effects						
Filter Purchase	-0.100 *	-0.110 **	-0.049	-0.057	-0.072 **	-0.079 **
	(0.054)	(0.051)	(0.050)	(0.044)	(0.035)	(0.034)
Controls[b]	NO	YES	NO	YES	NO	YES
Village FEs	NO	YES	NO	YES	NO	YES
Mean Dependent Variable	0.149	0.149	0.142	0.142	0.145	0.145
R-squared		0.068		0.102		0.054
Observations	665	665	579	579	1244	1244
B. One-year effects						
Filter Purchase	0.148	0.226 **	0.090	0.109	0.105	0.130 *
	(0.099)	(0.110)	(0.089)	(0.090)	(0.067)	(0.068)
Controls	NO	YES	NO	YES	NO	YES
Village FEs	NO	YES	NO	YES	NO	YES
Mean Dependent Variable	0.215	0.215	0.262	0.262	0.241	0.241
R-squared	.	0.093	0.006	0.118	0.003	0.067
Observations	266	266	273	273	539	539

Source: Berry et al (2015)
"Eliciting and Utilizing Willingness to Pay: Evidence from Field Trials in Northern Ghana."

Figure: MTE Estimates, from Berry et al (2015)

Figure 3: Kernel IV Estimates of Treatment Effects

(a) Short-term: One-Month Follow-Up

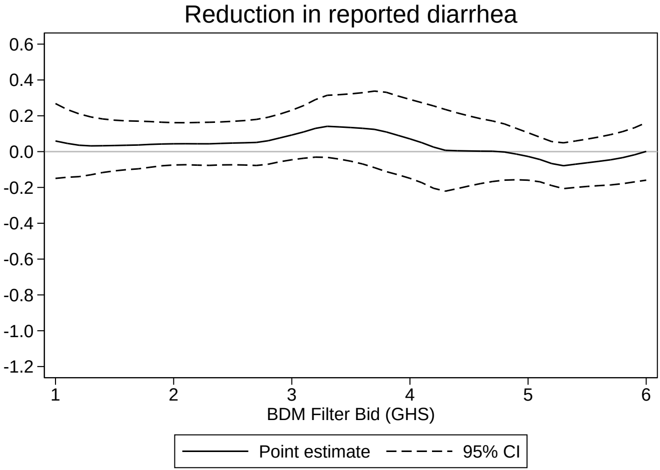


Figure: MTE Estimates, from Berry et al (2015)

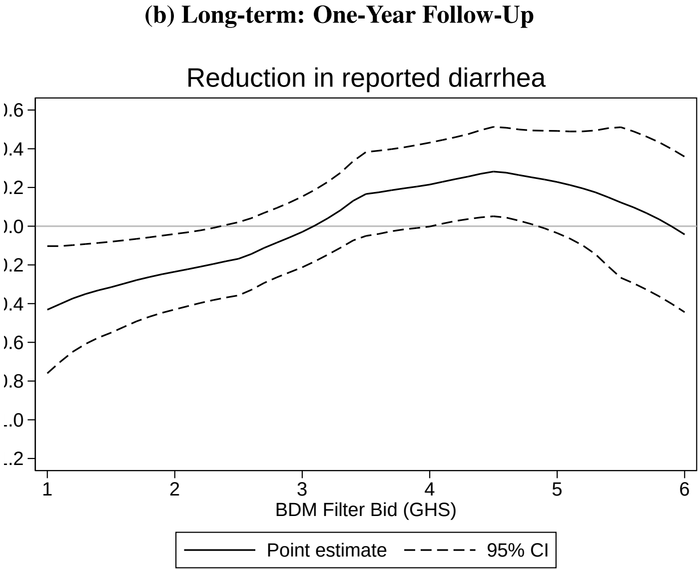
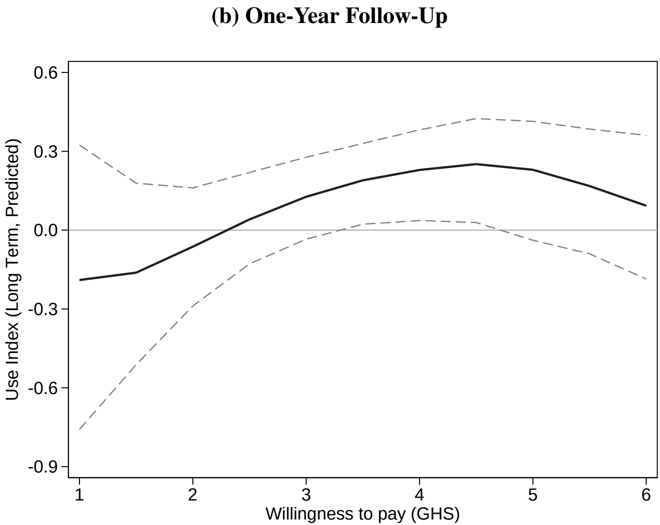


Figure: MTE Estimates, from Berry et al (2015)



Selective Trials: Berry, Fischer and Guiteras

- TSLS estimates positive, suggest water filter is increasing diarrhea among children.
- Using observed WTP shows treatment effect heterogeneity
  - Increases diarrhea of children of those with $WTP < 2.5$.
  - Decreases diarrhea of children of those with $WTP >= 2.5$.
  - Most WTP < 2.5, causing TSLS to be positive.
  - Setting price at 3.5 maximizes average decrease in diarrhea among children of those purchasing.
- How to investigate relationship between desire for treatment and treatment effect heterogeneity without elicitation of WTP?

Effect of Schooling on Earnings

Motivating Example: Effect of Schooling on Earnings.

Classical Framework due to Griliches (1976):

$$Y_i = \beta_0 + \beta_1 S_i + \beta_2 A_i + \epsilon_i, \quad \epsilon_i \perp (S_i, A_i),$$

where

- Y_i is log-wage,
- S_i is years of schooling
  - Alternatively, sometimes taken to be dummy variable for college attendance, as in Willis and Rosen (1979).
- A_i is ability.

Model of absolute advantage.

Effect of Schooling on Earnings

$$Y_i = \beta_0 + \beta_1 S_i + \beta_2 A_i + \epsilon_i.$$

- Suppose ability is not observed, run regression of Y on schooling omitting ability.
- Resulting OLS estimator,

$$\hat{\beta}_1^{OLS} \xrightarrow{p} \beta_1 + \beta_2 \frac{\text{Cov}(S_i, A_i)}{\text{Var}(S_i)}.$$

- If $\beta_2 > 0$ and $\text{Cov}(S_i, A_i) > 0$, then OLS upward biased.
- Called "ability bias" by Griliches
- Caveat: Measurement error might cause some downward bias in OLS.

Table: IQ and Schooling

RELATIONSHIP BETWEEN IQ AND SCHOOLING

	Pooled Sample		Near College		Not Near College	
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient of IQ	0.075	0.068	0.081	0.072	0.059	0.058
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)
Other Controls	No	Yes	No	Yes	No	Yes
R-squared	0.260	0.348	0.249	0.375	0.175	0.299
Number of Observations	2,061	2,061	1,460	1,460	601	601

Note: Table reports coefficient of IQ in a linear regression model for completed education in 1976. Models in odd columns include no other controls. Models in even columns include both parents' education, age and age-squared, indicators for race, family structure at age 14, and region in 1966. Near College subgroup are those whose county of residence in 1966 had a local 4-year college (public or private). Sample includes men in the NLS Young Men sample who were interviewed in 1976 and who have valid education data for their parents and an IQ score obtained from their school records.

[18] Schooling is taken from the 1976 interview, when the men were 24–34 years old. IQ measures were retrieved by NLS staff from the school records of men in the sample, and converted to a standardized basis (with mean 100 and standard deviation 15).

Source: Card (2001)
"Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems."

Effect of Schooling on Earnings

- Expect OLS to be upward biased for effect of schooling on earnings (ability bias).
- One solution: use instrument that is related to schooling but unrelated to ability.
  - Proposed instruments include distance to the nearest college, tuition at the nearest college, quarter of birth, mother's education, . . .
- With valid instrument, expect IV estimator to be consistent for β_1, true effect of schooling on earnings.

Other approaches to address ability bias include using proxies for ability and using fixed effect models with data on twins.

Effect of Schooling on Earnings

- Expect OLS to be upward biased for β_1.
- IV consistently estimates β_1.
- Thus expect IV estimates to be smaller than OLS estimates.
- Paradox from empirical literature: IV estimates of returns to schooling systematically *larger* than OLS estimates (Card, 2001).

Table: OLS vs IV Estimates for Return to Schooling

TABLE II

OLS AND IV ESTIMATES OF THE RETURN TO EDUCATION WITH INSTRUMENTS BASED ON FEATURES OF THE SCHOOL SYSTEM

Author	Sample and Instrument		Schooling Coefficients OLS	Schooling Coefficients IV
1. Angrist and Krueger (1991)	1970 and 1980 Census Data, Men. Instruments are quarter of birth interacted with year of birth. Controls include quadratic in age and indicators for race, marital status, urban residence.	1920–29 cohort in 1970	0.070 (0.000)	0.101 (0.033)
		1930–39 cohort in 1980	0.063 (0.000)	0.060 (0.030)
		1940–49 cohort in 1980	0.052 (0.000)	0.078 (0.030)
2. Staiger and Stock (1997)	1980 Census, Men. Instruments are quarter of birth interacted with state and year of birth. Controls are same as in Angrist and Krueger, plus indicators for state of birth. LIML estimates.	1930–39 cohort in 1980	0.063 (0.000)	0.098 (0.015)
		1940–49 cohort in 1980	0.052 (0.000)	0.088 (0.018)
3. Kane and Rouse (1993)	NLS Class of 1972, Women. Instruments are tuition at 2 and 4-year state colleges and distance to nearest college. Controls include race, part-time status, experience. Note: Schooling measured in units of college credit equivalents.	Models without test score or parental education	0.080 (0.005)	0.091 (0.033)
		Models with test scores and parental education	0.063 (0.005)	0.094 (0.042)
4. Card (1995b)	NLS Young Men (1966 Cohort) Instrument is an indicator for a nearby 4-year college in 1966, or the interaction of this with parental education. Controls include race, experience (treated as endogenous), region, and parental education	Models that use college proximity as instrument (1976 earnings)	0.073 (0.004)	0.132 (0.049)
		Models that use college proximity × family background as instrument	—	0.097 (0.048)

Source: Card (2001)

"Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems."

Table: OLS vs IV Estimates for Return to Schooling

5. Conneely and Uusitalo (1997)	Finnish men who served in the army in 1982, and were working full time in civilian jobs in 1994. Administrative earnings and education data. Instrument is living in university town in 1980. Controls include quadratic in experience and parental education and earnings.	Models that exclude parental education and earnings	0.085 (0.001)	0.110 (0.024)
		Models that include parental education and earnings	0.083 (0.001)	0.098 (0.035)
6. Harmon and Walker (1995)	British Family Expenditure Survey 1978–86 (men). Instruments are indicators for changes in the minimum school leaving age in 1947 and 1973. Controls include quadratic in age, survey year, and region.		0.061 (0.001)	0.153 (0.015)
7. Ichino and Winter-Ebmer (1998)	Austria: 1983 Census, men born before 1946. Germany: 1986 GSOEP for adult men. Instrument is indicator for 1930–35 cohort. (Second German IV also uses dummy for father's veteran status). Controls include age, unemployment rate at age 14, and father's education (Germany only). Education measure is dummy for high school or more.	Austrian Men	0.518 (0.015)	0.947 (0.343)
		German Men	0.289 (0.031)	0.590/0.708 (0.844) (0.279)
8. Lemieux and Card (1998)	Canadian Census, 1971 and 1981: French-speaking men in Quebec and English-speaking in Ontario. Instrument is dummy for Ontario men age 19–22 in 1946. Controls include full set of experience dummies and Quebec-specific cubic experience profile.	1971 Census:	0.070 (0.002)	0.164 (0.053)
		1981 Census:	0.062 (0.001)	0.076 (0.022)
9. Meghir and Palme (1999)	Swedish Level of Living Survey (SLLS) data for men born 1945–55, with earnings in 1991, and Individual Statistics (IS) sample of men born in 1948 and 1953, with earnings in 1993. Instrument is dummy for attending "reformed" school system at age 13. Other controls include cohort, father's education, and county dummies. Models for IS data also include test scores at age 13.	SLLS Data (Years of education)	0.028 (0.007)	0.036 (0.021)
		IS Data (Dummy for 1–2 years of college relative to minimum schooling)	0.222 (0.020)	0.245 (0.082)

Source: Card (2001)

Table: OLS vs IV Estimates for Return to Schooling

Author	Sample and Instrument		Schooling Coefficients OLS	IV
10. Maluccio (1997)	Bicol Multipurpose Survey (rural Philippines): male and female wage earners age 20–44 in 1994, whose families were interviewed in 1978. Instruments are distance to nearest high school and indicator for local private high school. Controls include quadratic in age and indicators for gender and residence in a rural community.	Models that do not control for selection of employment status or location	0.073 (0.011)	0.145 (0.041)
		Models with selection correction for location and employment status	0.063 (0.006)	0.113 (0.033)
11. Duflo (1999)	1995 Intercensal Survey of Indonesia: men born 1950–72. Instruments are interactions of birth year and targeted level of school building activity in region of birth. Other controls are dummies for year and region of birth and interactions of year of birth and child population in region of birth. Second IV adds controls for year of birth interacted with regional enrollment rate and presence of water and sanitation programs in region.	Model for hourly wage	0.078 (0.001)	0.064/0.091 (0.025) (0.023)
		Model for monthly wage with imputation for self-employed.	0.057 (0.003)	0.064/0.049 (0.017) (0.013)

Source: Card (2001)
"Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems."

Effect of Schooling on Earnings

One explanation: treatment effect heterogeneity (equivalently, β_1 a random coefficient).

- See Card (2001) for an analysis of this explanation in the context of a model with continuous S.
- See, e.g., Carneiro, Heckman and Vytlacil (2011) for an analysis of this explanation in the context of a model with binary S (college or not college) using an MTE framework.

Roy/Selection Model for College Attendence

Carneiro, Heckman and Vytlacil (2011)

- Define S to be dummy variable for attending college.
- Allow effect of college on earnings to vary across observationally identical individuals.
- Allow individuals to select whether to go to college based in part on some knowledge of their own idiosyncratic returns from college.
- Allows possibility of comparative advantage, not necessarily absolute advantage.
- Estimate relationship between desire to attend college and return to college.
- We will cover their methodology and results extensively in future lectures.

Heckman and Li: Returns to Education in China

Heckman and Li (2004):
"Selection Bias, Comparative Advantage, and Heterogeneous Returns to Education: Evidence from China in 2000."

- Apply Carneiro, Heckman and Vytlacil MTE methodology to Chinese data.
- Dataset: China Urban Household Investment and Expenditure Survey.
- Data from year 2000 from urban areas of six provinces: Gaungdong, Liaoning, Sichean, Shaanxi, Zhejiang, and Beijing.

Heckman and Li: Returns to Education in China

- For their analysis, they use working individuals who are either college/university graduates or who are senior high school graduates.
- Resulting sample: 273 observations with college/university degrees and 314 observations with only senior high school certificates.

Heckman and Li: Returns to Education in China

- Include as covariates in wage equation: Years of experience, experience-squared, parental income as a proxy of ability, and dummy variables for sex, province of residence, sector of employment, whether the firm in which the individual works is state-owned, collective-owned, or privately owned.
- Include as determinants of college choice that are excluded from wage equation (instruments): father's education, mother's education.

Table: Selection Bias, Sorting Gain, and Comparative Advantage

Parameter	*Estimation*
OLS	0.2929
IV*	0.5609
ATE	0.4336
TT	0.5149
TUT	0.3630
Bias**	–0.1407
Selection Bias***	–0.2220
Sorting Gain****	0.0813

*Using the propensity score as the instrument.

**$Bias = \text{OLS} - \text{ATE}$.

***$Selection\ bias = \text{OLS} - \text{TT}$.

****$Sorting\ gain = \text{TT} - \text{ATE}$.

Source: Heckman and Li (2004)
"Selection Bias, Comparative Advantage, and Heterogeneous Returns to Education: Evidence from China in 2000 ."

Table: Selection Bias, Sorting Gain, and Comparative Advantage

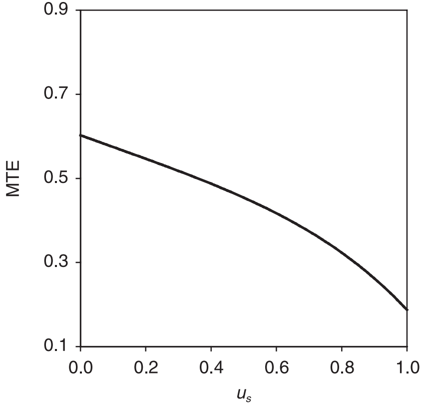


Figure 2. Marginal treatment effect, including parental income as proxy for ability in wage equation, all ownership, and sectoral dummies; bandwidth = 0.4

Source: Heckman and Li (2004)

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