Introduction to causal identification

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IGC Summer School,
New Delhi, July 2015

Outline

1. Micro-empirical methods

2. Rubin causal model

3. More on Instrumental Variables (IV)

Estimating causal effects

- We often want to estimate the causal effect of a program
- What is the effect of a performance incentive system for teachers on student test scores?
- Observe the students scores before and after school adopts new pay scheme
- Need to know counterfactual of what the test scores would have been had the teachers not been incentivized, but *all else had been equal*.

Challenges

- Previously could run a regression and control for variables to isolate the causal effect
  - Control for observable measures of the teacher's quality and student characteristics
  - Regard program/treatment status, conditional on observables, as exogenous
- Program/treatment status might not be exogenous
  - Policy changes are endogenous
  - Take-up of interventions is endogenous
  - Other sources of omitted variable bias or reverse causality
- If program status is endogenous and we do not correct for it, we end up drawing incorrect conclusions

Some micro-methods that address endogeneity

- Randomized experiments
- Natural experiments
- IV
- Regression discontinuity
- Difference-in-differences

Rubin Causal Model

- Framework for thinking about causal effects
- We apply the treatment and control terminology broadly to cases where we want to evaluate a causal effect
- A policy does not need to be binary (you are treated or you are not), but consider this case for simplicity

Notation

- Let T denote treated and C denote comparison or control

- Let Y_i^T be the test score of a student i in a school with teacher incentives

- Y_i^C is test score of the same student i if her school had not had the incentive scheme

Potential Outcomes

- Here treated means your school has the teacher incentive program
- Y_i^T and Y_i^C are “potential outcomes"; they might be actual outcomes or hypothetical outcomes

Causal effect of being treated

- We are interested in the difference:

$$Y_i^T - Y_i^C$$

- This is the effect of the teacher incentive program on student i's test scores
- Problem: we don't observe student i both with and without the teacher incentive program in her school at the same time
- Fundamental Problem of Causal Inference:

It is impossible to observe the value of Y_i^T and Y_i^C on the same unit and, therefore, not clear how to measure the effect of T for i.

Average causal effects in a population

- Statistical solution replaces the impossible-to-observe causal effect of T on a specific unit i with the possible-to-estimate average causal effect of T over a population of units (students)

- We can hope to learn the average effect of the teacher incentive program in a population of students

$$\mathrm{E}[Y_i^T - Y_i^C]$$

- For simplicity, let's assume the treatment effect is constant across individuals: $Y_i^T - Y_i^C = Y_j^T - Y_j^C \; \forall$students i and j

Assumptions

- Treatment status of one person does not affect others' outcomes, that is, there are no spillovers
  - SUTVA: Stable Unit Treatment Value Assumption
- Not insurmountable if it does not hold
- Essentially means we need to think of different treatments for individual i
  - with and without others being treated if there are spillovers
  - intense versus light if treatment type varies

Measuring average effects in a population

- Imagine we have access to data on several individuals in an area
- Some schools have the teacher incentive program, some do not
  - Policy is optional and some headmasters adopt new policy
  - Government chooses some schools in which to pilot the policy
- We can take the average in both groups and calculate the difference

$$E[Y_i|\text{program in place}] - E[Y_i|\text{program not in place}]$$
$$= E[Y_i^T|T] - E[Y_i^C|C]$$

- Subtract and add $E[Y_i^C|T]$

$$E[Y_i^T|T] - E[Y_i^C|T] - E[Y_i^C|C] + E[Y_i^C|T]$$
$$= E[Y_i^T - Y_i^C|T] + E[Y_i^C|T] - E[Y_i^C|C]$$

Comparing the treated to the untreated

$$E[Y_i^T|T] - E[Y_i^C|C] =$$
$$E[Y_i^T - Y_i^C|T] + E[Y_i^C|T] - E[Y_i^C|C]$$

- The first term is the *treatment effect* that we are trying to isolate
- On average, what is the effect of the teacher incentive program on student test scores?
- Then, what is:
  - $E[Y_i^C|T]$?
  - $E[Y_i^C|C]$?
  - The difference $E[Y_i^C|T] - E[Y_i^C|C]$? (*selection bias*)

Selection Bias

- Is the difference $E[Y_i^C|T] - E[Y_i^C|C]$ likely to be positive or negative?
- This gives the *selection bias* a sign
- There may be systematic differences between students in schools with the program and those in schools without it
- How would pre-period data before any school had the program help?

Under what circumstances would it eliminate the selection bias?

  – A method that addresses this: Difference-in-differences

Analogous problem when evaluating other policies

- Suppose a microcredit organization began offering microcredit in some villages but not others

- Can we measure the effect of the program by comparing villages with and without the program?

- T is having microcredit in the village and let Y be average income in the village

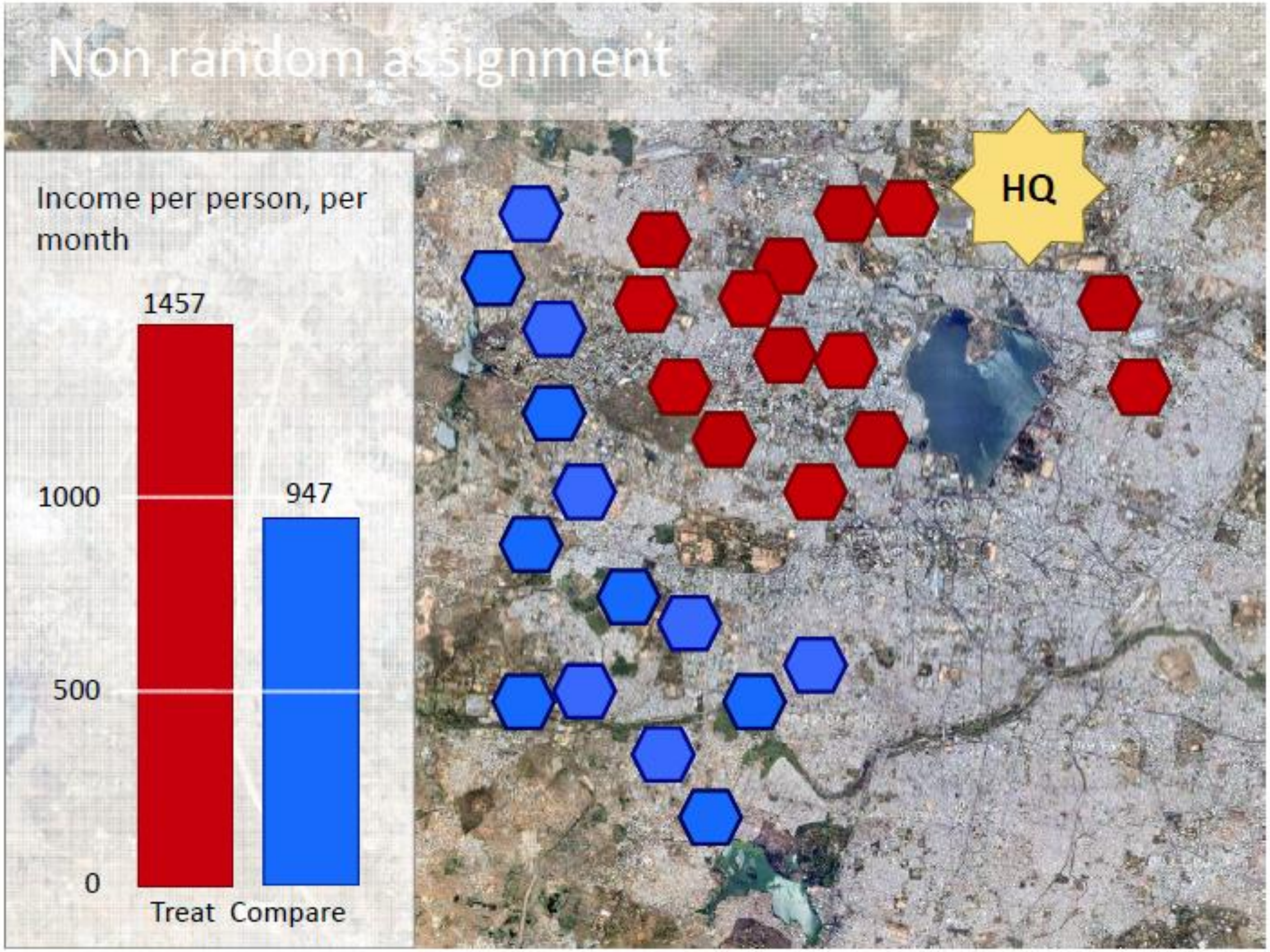
- Is $E[Y^C|T] - E[Y^C|C]$ likely to be positive or negative?

Eliminating the selection bias

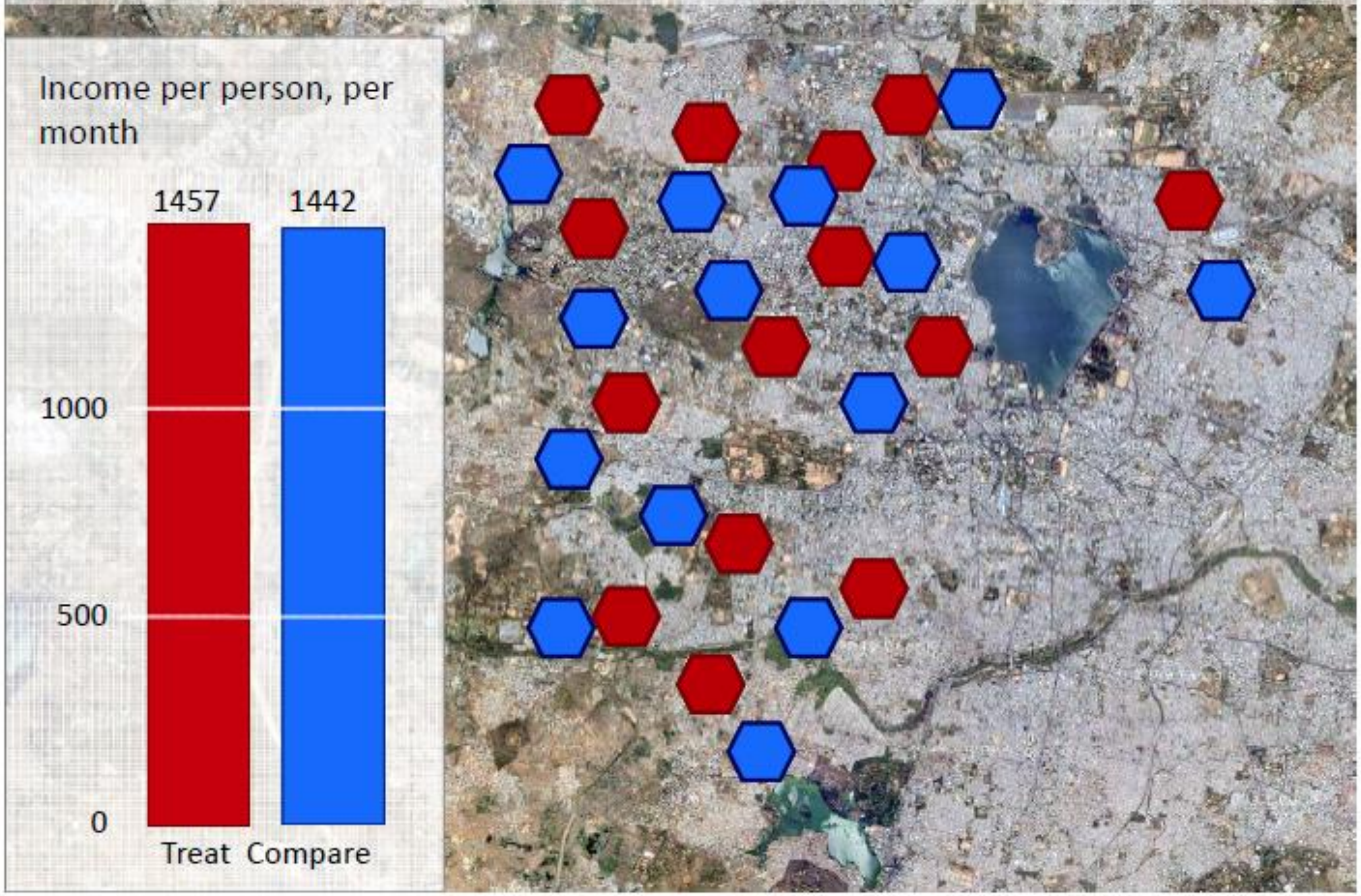
- Much of the challenge in empirical work is eliminating the selection bias
- Choosing an empirical method (“identification strategy” or “research design") that makes selection bias less likely
- Presenting checks on data and results that lend credibility to the assumption of no selection bias
- Fundamental problem of causal inference is that we cannot directly test the assumption of no selection bias

Randomization

- If treatment and control groups are chosen randomly, $E[Y_i^C|T] - E[Y_i^C|C] = 0$
- Eliminates systematic differences between treatment and control because no self-selection or targeting (non-random program placement)
- May still not eliminate selection bias completely if there is sampling error


Non random assignment
Income per person, per month
1457
947
1000
500
0
Treat
Compare
HQ

Random assignment



Another example – Thomas *et al*. 2006

- Effect of taking dietary iron supplements on earnings, health and psycho-social measures

- T being offered is iron supplements

- Dietary Iron → Higher hemoglobin (Hb) level in blood → Less fatigued → More productive worker

- Thomas *et al*. (2006) used an RCT design.

- Use this example to illustrate IV

Program to distribute iron supplements

- Free iron supplement program could be interesting to us as researchers for 2 reasons

  1. We want to measure the program's effectiveness (policy question)

$$Y_i = \alpha + \beta \cdot T_i + \epsilon_i$$

  2. Program enables us to measure the effect of hemoglobin on economic outcomes (more general question)

$$Y_i = \alpha + \beta \cdot Hb_i + \epsilon_i$$

Effect of Hb on earnings

- Suppose we want to estimate the effects of Hb on earnings

$$Y_i = \alpha + \beta \cdot Hb_i + \epsilon_i$$

- Want to regress Y on Hb, but Hb could be an *endogenous regressor* (from omitted variables)

- Use treatment T as an instrumental variable that affects Hb, and only affects Y through its effect on Hb

First and second stage

- Second stage

$$Y_i = \alpha + \beta \cdot \widehat{Hb}_i + \epsilon_i$$

- First stage

$$Hb_i = \alpha' + \beta' \cdot T_i + \epsilon'_i$$

Requirements for IV

1. Instrument is correlated with the endogenous regressor (strong first stage) - testable assumption

$$E[Hb_i \cdot T_i] \neq 0$$

2. Instrument is exogenous to the outcome (random assignment)

$$E[\epsilon_i \cdot T_i] = 0$$

3. Instrument only affects the outcome via its effect on the endogenous regressor (exclusion restriction)

4. Either ∀ individuals the instrument increases the endogenous regressor or ∀ individuals decreases the endogenous regressor (“no defiers" or *monotonicity condition*)

IV estimate

- Intent to treat (ITT) for Hb levels is measured by:

$$E[Hb_i|T_i = 1] - E[Hb_i|T_i = 0] \qquad (1)$$

First stage equation

- Intent to treat (ITT) estimate for labor market outcomes

$$E[Y_i|T_i = 1] - E[Y_i|T_i = 0] \qquad (2)$$

Reduced form equation

IV estimate

- Using our expression for Y_i, we have:

$$E[Y_i|T_i = 1] = \alpha + \beta E[H_i|T_i = 1] + E[\epsilon_i|T_i = 1]$$

and:

$$E[Y_i|T_i = 0] = \alpha + \beta E[H_i|T_i = 0] + E[\epsilon_i|T_i = 0]$$

- Therefore,

$$E[Y_i|T_i = 1] - E[Y_i|T_i = 0] =$$
$$\beta(E[H_i|T_i = 1] - E[H_i|T_i = 0]) + E[\epsilon_i|T_i = 1] - E[\epsilon_i|T_i = 0]$$

IV estimate

- What can we assume about

$$E[\epsilon_i|T_i = 1] - E[\epsilon_i|T_i = 0]?$$

- Strict exogeneity assumption above implies the less strong assumption needed, $E[\epsilon_i \cdot T_i] = 0$

IV estimate

$$E[Y_i|T_i = 1] - E[Y_i|T_i = 0] = \beta(E[H_i|T_i = 1] - E[H_i|T_i = 0])$$

- Wald estimate of $\hat{\beta}$

$$\hat{\beta} = \frac{E[Y_i|T_i=1]-E[Y_i|T_i=0]}{E[H_i|T_i=1]-E[H_i|T_i=0]}$$

- Wald estimate: IV with binary instrument and no covariates
- Estimate the effect of health on labor market outcomes by dividing the effect of the program on labor market outcomes (reduced form, equation (2)) by the effect of the program on health (first stage, equation (1))

Violations of exclusion restriction

- Distinction between exclusion restriction and “random assignment“
- Random assignment: instrument is exogenous in the reduced form and first stage equations
- What if the exclusion restriction was violated but T_i is still random?
- Can still interpret the first stage or reduced form results as causal (treatment on income or treatment on Hb)
- But cannot use the treatment to learn about the relationship between the endogenous regressor and the outcome of interest (Hb on income)

Violations of monotonicity condition

- Could treatment reduce Hb level for some individuals (see Thomas *et al*. 2006)?
- Another example: information on returns to schooling as IV for school attendance
  - Randomized information intervention telling some households what the labor-market returns to secondary school are
  - Use treatment as IV for attending secondary school
  - On average, treatment increases secondary school attendance; households had underestimated returns (first stage)
  - Random assignment of treatment and exclusion restriction probably OK
  - But what if someone had overestimated returns to schooling?

“Identifying variation” with IV?

- For pedagogical reasons, will look at special case where H is also a dummy variable for high Hb
- Notation: $Y_i\big(T_i, H_i(T_i)\big)$ and $Y_i(H_i)$ where T is treatment
- We want $E[Y_i(1) - Y_i(0)]$
- Start by decomposing effect of T on Y for person i

$$
\begin{aligned}
Y_i\big(1, H_i(1)\big) - Y_i\big(0, H_i(0)\big) &= Y_i\big(H_i(1)\big) - Y_i\big(H_i(0)\big) \\
&= \big[Y_i(1) \cdot H_i(1) + Y_i(0) \cdot \big(1 - H_i(1)\big)\big] \\
&\quad - [Y_i(1) \cdot H_i(0) + Y_i(0) \cdot \big(1 - H_i(0)\big)] \\
&= (Y_i(1) - Y_i(0))(H_i(1) - H_i(0))
\end{aligned}
$$

- For the sample average of the estimand, when we use the treatment as our identifying variation in H, the only individuals who contribute to our estimate are those for whom $H_i(1) \neq Hi(0)$

Matrix

	$H_i(0) = 0$	$H_i(0) = 1$
$H_i(1) = 0$	$Y_i(1,0) - Y_i(0,0) = 0$ Never taker	$Y_i(1,0) - Y_i(0,1)$ $= Y_i(0) - Y_i(1)$ Defier
$H_i(1) = 1$	$Y_i(1,1) - Y_i(0,0)$ $= Y_i(1) - Y_i(0)$ Complier	$Y_i(1,1) - Y_i(0,1) = 0$ Always taker

Notation: $Y_i(T_i, H_i)$ and $Y_i(H_i)$ and $H_i(T_i)$

$Y_i(1) - Y_i(0)$ is effect of high Hb on income, the object of interest

IV estimand for the sample

- Taking the sample mean of our individual-level estimand, we get

$$E\left[Y_i\left(1, Hi(1)\right) - Yi\left(0, Hi(0)\right)\right) = E[Y_i(1) - Yi(0))\left(H_i(1) - Hi(0)\right)]$$
$$= E[Y_i(1) - Yi(0)|H_i(1) - Hi(0) = 1] \cdot Prob[Hi(1) - Hi(0) = 1] +$$
$$E[Y_i(0) - Yi(1)|H_i(1) \ - Hi(0) = -1] \cdot Prob[Hi(1) - Hi(0) = -1]$$

First term is compliers

Second term is defiers

Wald estimate with defiers

$$\hat{\beta}w = \frac{E[Y_i|T_i=1]-E[Y_i|T_i=0]}{E[H_i|T_i=1]-E[H_i|T_i=0]} =$$

$$\frac{\hat{\beta}_c \cdot \text{Prob[i is a complier]} - \hat{\beta}_d \cdot \text{Prob[i is a defier]}}{\text{Prob[i is a complier]} - \text{Prob[i is a defier]}}$$

- Let $\lambda \equiv \frac{\text{Prob[i is a defier]}}{\text{Prob[i is a complier]}-\text{Prob[i is a defier]}} \rightarrow \hat{\beta}w = (1+\lambda)\hat{\beta}_c - \lambda\hat{\beta}_d$
- If $\beta_c = \beta_d$, defiers are not problematic
- But with heterogeneity in treatment effects, if monotonicity condition does not hold, IV estimand is a strange weighted average (weights not restricted to [0, 1])

LATE

- IV estimates give us a *local average treatment effect* (LATE)

- $\hat{\beta}$ is estimated for the "compliers" or those induced to have higher Hb by virtue of being assigned to the treatment group

Where do we find IVs?

- Random experiments

- "Natural experiment”: use real-world variation that we think is uncorrelated with important unobserved factors as a way to solve selection bias problem

- Often generated by a policy
  - Draft lottery number affected likelihood of serving in Vietnam

Another IV example

- Want to regress *Income* on *Military*, but those who enter military differ in other ways

- Find a factor LotteryNo (lottery number - IV) that affects *Military*, and only affects *Income* through its effect on *Military*

- Instrument is correlated with the endogenous regressor (first stage)

$$Military_i = \alpha + \beta LotteryNo_i + v_i \qquad (1)$$

- Only channel through which instrument affects outcome is through the endogenous regressor?

- If yes, take predicted value from (1) and run second stage

$$Income_i = \delta + \gamma \widehat{Military}_i + \varepsilon_i \qquad (2)$$