Randomized Experiment

Tadao Hoshino （星野匡郎）

ver. 2018 Fall Semester

Review of the Previous Lecture

Review of the Previous Lecture

- Y: Observed outcome variable
- T: Treatment variable (dummy variable: $T = 1$ for treated and $T = 0$ for control)
- We are interested in estimating the causal effect of T on Y.
- Rubin's causal model:

$$Y_i = \begin{cases} Y_{1i} & \text{if } T_i = 1 \\ Y_{0i} & \text{if } T_i = 0 \end{cases}$$

or equivalently

$$Y_i = T_i \cdot Y_{1i} + (1 - T_i) \cdot Y_{0i},$$

where Y_{ti} is the "potential" outcome when $T_i = t$.

Review of the Previous Lecture

Treatment Effect

We define the causal effect of T on Y for individual i as

$$Y_{1i} - Y_{0i},$$

which is called the (individual) treatment effect.

For each individual i, it is impossible to observe both potential outcomes Y_{1i} and Y_{0i} at the same time.
$=>$ We cannot estimate the individual treatment effect.

Average Treatment Effect (ATE)

The treatment effect averaged over the population

$$E[Y_{1i}] - E[Y_{0i}]$$

is called the average treatment effect (ATE).

Estimation of ATE

- We have a random sample of n observations $\{(Y_i, T_i) : i = 1, ..., n\}$.
- Treatment group $= \{i : T_i = 1\}$, Control group $= \{i : T_i = 0\}$.
- A simple difference estimator of ATE comparing average outcomes between the two groups:

$$\frac{\sum_{i=1}^{n} T_i Y_i}{\sum_{i=1}^{n} T_i} - \frac{\sum_{i=1}^{n} (1 - T_i) Y_i}{\sum_{i=1}^{n} (1 - T_i)}$$

  This estimator is biased in general, except for the case when T is exogenous.

- For expositional simplicity, assume that the treatment effect is homogeneous $Y_{1i} - Y_{0i} = \beta$. (Clearly, we have ATE $= \beta$ in this case.)
- Note that the difference estimator is numerically equivalent to the OLS estimator of β in the following simple regression model:

$$Y_i = \alpha + T_i\beta + \varepsilon_i, \;\; i = 1, ..., n$$

- If T_i is an endogenous treatment variable ($E[T_i\varepsilon_i] \neq 0$), the difference estimator is biased for the true causal effect β (endogeneity bias).
- Treatment variables are often endogenous due to the presence of omitted variables (unobserved spurious factors) and simultaneity.

Review of the Previous Lecture

Econometric methods to circumvent the endogeneity problem:

- Pure-experimental approach
  - Randomized experiment
- Quasi-experimental approaches
  - Instrumental variable method
  - Matching method
  - etc

Selection Bias

Class attendance and academic performance

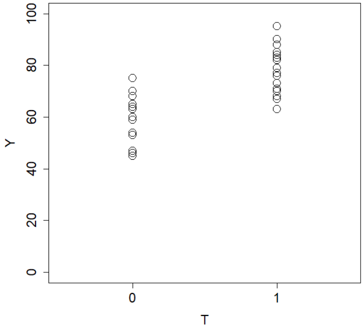
- When class attendance is not compulsory, it would be expected that students with lower attendance rates achieve lower academic performances than students with higher attendance rates.
- Is it possible to interpret this correlation btwn class attendance and academic performance as a causal relationship?

Let

- Y_i: academic test score
- T_i: treatment variable ($T_i = 1$ if student i's attendance rate is higher than 50%, $T_i = 0$ otherwise.)

Class attendance and academic performance

[Plot of (T, Y)[1]]



	# students	Average test score
$T = 1$ (attendance $> 50\%$)	19	77.21
$T = 0$ (attendance $\leq 50\%$)	21	57.05

[1] This is simulated data created by myself.

Class attendance and academic performance

- The difference btwn the average outcome for the treatment group and that for the control group is about 20.16.
- To what extent does this figure reflect the causal effect of class attendance on students' performance?
  - For a student who is reluctant to attend school, is it possible to increase her test score by 20.16 point by making class attendance compulsory?
- Spurious correlation :
  - Potential existence of omitted variables that affect both class attendance and academic performance.
  - For example, students who have strong <u>motivation for studying</u> tend to be less absent in school, and would achieve higher performance.
- Thus, 20.16 is likely to be an overestimation of the true causal effect.

An empirical evidence by Romer (1993)[2]

- Romer took attendance at six meetings of his intermediate (undergraduate-level) macroeconomics course at UC Berkeley.
- Student performance is measured as the overall score on the three exams in the course.

[2] Romer, D. (1993) Do students go to class? Should they? The Journal of Economic Perspectives, 7(3), 167-174.

An empirical evidence by Romer (1993)

- The first column shows the result of the simple regression with Fraction of Lectures Attended as explanatory variable.
  - We can observe a strong positive correlation btwn the test score and the attendance rate.
- The second column shows the result of the multiple regression with an additional regressor Prior GPA. The prior GPA may be regarded as an index of academic motivation.
  - The coefficient of attendance is two-thirds as large as it is in the baseline model, implying that mere correlation between the two variables overestimates the causal effect.

Results of regression analysis: extracted from Romer (1993) Table 2

Constant	1.25 (0.27)	-0.67 (0.32)
Fraction of Lectures Attended	2.19 (0.35)	1.52 (0.32)
Prior GPA		0.78 (0.12)
Sample size	195	195

(Standard errors in the parentheses.)

Selection Bias

- We summarize the above discussion in terms of Rubin's causal model.
- Each student i's academic performance is

$$Y_i = T_i \cdot Y_{1i} + (1 - T_i) \cdot Y_{0i}.$$

- Consider the difference between the expected value of the outcome for the treatment group and that for the control group:

$$E\left[Y_i | T_i = 1\right] - E\left[Y_i | T_i = 0\right].$$

In the above example, the sample estimate of this quantity is 20.16.

Selection Bias

- Here, observe that

$$\begin{aligned} E\left[Y_i | T_i = 1\right] &= E\left[T_i \cdot Y_{1i} + (1 - T_i) \cdot Y_{0i} | T_i = 1\right] \\ &= E\left[1 \cdot Y_{1i} + 0 \cdot Y_{0i} | T_i = 1\right] \\ &= E\left[Y_{1i} | T_i = 1\right]. \end{aligned}$$

- Similarly,

$$\begin{aligned} E\left[Y_i | T_i = 0\right] &= E\left[T_i \cdot Y_{1i} + (1 - T_i) \cdot Y_{0i} | T_i = 0\right] \\ &= E\left[0 \cdot Y_{1i} + 1 \cdot Y_{0i} | T_i = 0\right] \\ &= E\left[Y_{0i} | T_i = 0\right]. \end{aligned}$$

- Hence,

$$\underbrace{E\left[Y_i|T_i=1\right]-E\left[Y_i|T_i=0\right]}_{\text{(A)}} \tag{1}$$

$$= E\left[Y_{1i}|T_i=1\right]-E\left[Y_{0i}|T_i=0\right]$$

$$= \underbrace{E\left[Y_{1i}-Y_{0i}|T_i=1\right]}_{\text{(B)}} + \underbrace{E\left[Y_{0i}|T_i=1\right]-E\left[Y_{0i}|T_i=0\right]}_{\text{(C)}},$$

where the second equality is obtained by adding and subtracting $E\left[Y_{0i}|T_i=1\right]$.

- The term (B) is interpreted as the average causal effect of class attendance on academic performance for the students whose attendance rate is higher than 50%.
- This average treatment effect for the treatment group is called the Average Treatment Effect on the Treated (ATET or ATT) :

$$\text{ATET: } E\left[Y_{1i} - Y_{0i} | T_i = 1\right]$$

Selection Bias

- When the treatment effect for the treatment group and that for the control group are the same on average, i.e.,

$$E\left[Y_{1i} - Y_{0i} | T_i = 1\right] = E\left[Y_{1i} - Y_{0i} | T_i = 0\right],$$

by the law of iterated expectations,

$$\begin{aligned}
\underbrace{E\left[Y_{1i} - Y_{0i}\right]}_{ATE} &= E[E\left[Y_{1i} - Y_{0i} | T_i\right]] \\
&= E\left[Y_{1i} - Y_{0i} | T_i = 1\right] \Pr(T_i = 1) + E\left[Y_{1i} - Y_{0i} | T_i = 0\right] \Pr(T_i = 0) \\
&= E\left[Y_{1i} - Y_{0i} | T_i = 1\right] \left\{\Pr(T_i = 1) + \Pr(T_i = 0)\right\} \\
&= \underbrace{E\left[Y_{1i} - Y_{0i} | T_i = 1\right]}_{ATET}.
\end{aligned}$$

- Hence, in this case ATE is equal to ATET.

Selection Bias

- The term (C)

$$E\left[Y_{0i}|T_i = 1\right] - E\left[Y_{0i}|T_i = 0\right]$$

  is interpreted as

  the average difference between

  > the counterfactual academic performance among students whose attendance rate is higher than 50% had they not attended class

  and

  > the actual academic performance among students whose attendance rate is less than 50%.

- This term is called the selection bias.

Selection Bias

- As in equation (1), if the selection bias (C) is zero, ATET (B) can be obtained simply by computing (A); the resulting estimated value of (A) is 20.16 in the above example.
- However, when comparing the potential outcome Y_0 for students with higher academic motivation $(T = 1)$ and the actual outcome Y_0 for less-motivated students $(T = 0)$, the former would be larger than the latter:
$$E\left[Y_{0i}|T_i = 1\right] \geq E\left[Y_{0i}|T_i = 0\right].$$
- Thus, we can expect that the selection bias term is positive, and the value 20.16 would be an overestimation of the causal effect ATET:

$$\underbrace{E\left[Y_i|T_i = 1\right] - E\left[Y_i|T_i = 0\right]}_{\approx 20.16} = \text{ATET} + \underbrace{\text{Selection bias}}_{\geq 0}$$

Selection Bias

- The selection bias can be viewed as a special case of endogeneity bias.
- When a selection bias exists, we have

$$E\left[Y_{0i} | T_i = 1\right] \neq E\left[Y_{0i} | T_i = 0\right],$$

which suggests that Y_{0i} and T_i are not independent.

- Recall that when the treatment effect is homogeneous $\beta = Y_{1i} - Y_{0i}$, we have $Y_i = Y_{0i} + T_i\beta$. Then, letting $\alpha = E[Y_{0i}]$ and $\varepsilon_i = Y_{0i} - E[Y_{0i}]$, we can consider the following simple regression model:

$$Y_i = \alpha + T_i\beta + \varepsilon_i$$

- In order to consistently estimate β, T_i needs to satisfy $E[T_i\varepsilon_i] = 0$ (exogeneity).

Selection Bias

- By the definition of ε_i,

$$\begin{aligned} E[T_i \varepsilon_i] &= E[(Y_{0i} - E[Y_{0i}])T_i] \\ &= E[Y_{0i}T_i] - E[Y_{0i}]E[T_i] \\ &= Cov(Y_{0i}, T_i), \end{aligned}$$

  which is not zero in general since Y_{0i} and T_i are not independent.
- Thus, the selection bias can be viewed as a source of endogeneity bias.

Selection Bias

- In general, selection bias arises when individuals can "select" their own treatment status by themselves.
- The selection bias problem does not exist in laboratory experiments, where the treatment status of each subject can be "exogenously" determined by researcher.
  - In the example of class attendance and academic performance, the source of selection bias is that students can choose whether to attend class or not.
- What if we randomly assign individuals to the treatment and control groups?
  $=>$ Average characteristics of individuals will be likely to be equal between the two groups, and thus the selection bias would disappear.
  $=>$ Randomized Experiment (also called Randomized Controlled Trial: RCT).

Randomized Experiment

Randomized Experiment

Randomized experiment

A randomized experiment is an experiment with the following properties:

- Assignment to the treatment or control groups is random:

$$0 < \Pr(T = 1) < 1.$$

- Treatment status is independent of the two potential outcomes:

$$(Y_1, Y_0) \perp\!\!\!\perp T.$$

- This is conceptually equivalent to determine assignment to the treatment group by a lottery.
- Since the value of T_i (whether i wins or loses in the lottery) is irrelevant to her potential achievement, it must hold that

$$E\left[Y_{1i}|T_i = 1\right] = E\left[Y_{1i}|T_i = 0\right]$$
$$E\left[Y_{0i}|T_i = 1\right] = E\left[Y_{0i}|T_i = 0\right]$$

Randomized Experiment

- This implies that the selection bias is zero.
- Furthermore, since

$$E\left[Y_{1i} - Y_{0i} | T_i = 1\right] = E\left[Y_{1i} - Y_{0i} | T_i = 0\right],$$

ATET and ATE will coincide under randomization (ATET = ATE).

- Hence, we have

$$E\left[Y_i | T_i = 1\right] - E\left[Y_i | T_i = 0\right] = \underbrace{E\left[Y_{1i} - Y_{0i} | T_i = 1\right]}_{ATET} = \underbrace{E\left[Y_{1i} - Y_{0i}\right]}_{ATE}.$$

- In randomized experiments, we can estimate ATE(T) by simply taking the difference between the sample averages for the treatment and control groups (or, equivalently, by a simple regression of Y on T).

Limitations of randomized experiments

Feasibility

- Cost of implementation:
  - In empirical studies in economics, treatments of which we are interested in estimating their causal effects are, for example, the implementation of a new economic policy, the introduction of a new transportation service, increasing/decreasing the regional minimum wages, etc.
  - The costs of implementing such treatments are enormous, not only financially but also politically.
- Ethical problem:
  - An experiment in which whether students can attend class or not is randomly determined in a lottery (only for the purpose of estimating the treatment effect) is ethically problematic in terms of fairness of educational opportunity.

Limitations of randomized experiments

Attrition of participants

- When the duration of experiment is long, there is a risk that some participants may drop out.
- There is no problem if the attrition of participants is at random.
- If particular types of participants are more likely to drop out of one group than the other, the two groups will no longer be similar.
  - For example, consider a job training program designed to reduce unemployment. If a participant finds a job while in training, she would exit from the training program before it ends.

Hawthorne effect (Experimenter demand effect)

- The Hawthorne factory of the Western Electric Company conducted an experiment in 1924-32 to investigate whether their workers would become more productive in higher/lower levels of light.

Women in the Relay Assembly Test Room at the Hawthorne factory. This picture is taken from: http://www.lmmiller.com/lean-lessons-from-the-hawthorne-studies

Limitations of randomized experiments

Hawthorne effect (Experimenter demand effect)

- Surprisingly, the study found that regardless of whether the level of lighting was increased or decreased, the productivity of workers increased.
- The researchers concluded that it was not the changes in the level of lighting that were affecting the workers' productivity.
- Rather, the increase in productivity was because of the fact that the workers worked harder as they felt they were being watched.
- Hawthorne effect : People behave differently when they know they are participating in an experiment.

Examples of Randomized Experiments

Project STAR

- It is often believed that smaller class size is more beneficial to students and their academic achievement than larger class size.
- However, smaller class size is more expensive. More classrooms and more teachers are needed. It is crucial to clarify to what extent small class size is actually effective for learning.
- Tennessee Student/Teacher Achievement Ratio (STAR) experiment:
  - Large-scale randomized experiment to evaluate the causal effect of class-size reduction on reading and mathematics achievement.
  - More than 11,000 students and 80 schools in the state participated in this experiment, which became known Project STAR.

Experimental design of Project STAR

- Children entering kindergarten in the fall of 1985 were randomly assigned to one of the following types of classes:

  Small class a small-size class with 13 - 17 students.

  Regular class a regular-size class with 22 - 25 students.

  Regular/aide class a regular-size class with a full-time teachers' aide.
- Teachers in each school were also randomly assigned.
- The students stayed in their randomly assigned category from kindergarten until third grade.

Results

- The following table shows the average SAT score for each category at each grade. The test scores are scaled in percentile points.
- Students in small-size classes tend to achieve higher SAT scores than those in regular-size classes.
- Teacher aides have only a subtle effect on the test score.

Percentile score : extracted from Krueger (1999)[3] Table I

	Average percentile score			
	Kindergarten	1st grade	2nd grade	3rd grade
Small class	54.7	49.2	46.4	47.6
Regular class	49.9	42.6	45.3	44.2
Regular/aide class	50.0	47.7	41.7	41.3

[3] Krueger, A. (1999) Experimental estimates of education production functions. The Quarterly Journal of Economics, 114(2), 497-532.

Results (cont')

- The next table presents the regression results of the test score on the class-size dummies (Regular class is omitted as a baseline).
- Students in small classes tend to perform better than those in regular classes; in particular, the gap in average performance is large (about 8.6 points) in first grade.
- Again, teacher aides have only a small effect.

Regression analysis : extracted from Krueger (1999) Table V

	Kindergarten	1st grade	2nd grade	3rd grade
Small class	4.82 (2.19)	8.57 (1.97)	5.93 (1.97)	5.32 (1.91)
Regular/aide class	0.12 (2.23)	3.44 (2.05)	1.97 (2.05)	-0.22 (1.95)
Sample size	5861	6452	5950	6109

(The estimates of the intercept are omitted. Standard errors in the parentheses.)

Moving to Opportunity

- Neighborhood effects : The economic and social environment of neighborhoods have direct/indirect on individual behaviors.
- The existence of neighborhood effects has been demonstrated in many fields of research including education, health, crime, juvenile delinquency, voting, etc.
- MTO (Moving to Opportunity) : a social experiment designed to evaluate the neighborhood effects on economic and social outcomes of low-income families in the United States.

Experimental design of MTO

- A total of 4,600 low-income households, mostly African American or Hispanic, participated in the MTO experiment between September 1994 and August 1998.
- They were randomly assigned to one of the following three groups by a lottery:

  Low poverty voucher (LPV) gourp, which received Section 8 certificates[4] or vouchers usable only in low-poverty areas.

  Traditional voucher group, which received regular Section 8 certificates or vouchers (geographically unrestricted).

  Control group, which received no certificates or vouchers.
- Among the households assigned to the LPV group, 47 percent used the voucher to relocate to a low-poverty neighborhood.

[4] Eligibility to receive rent subsidy from the government.

Moving to Opportunity

Chetty, Henderson and Katz (2016)[5]

- "*We find that moving to a lower-poverty neighborhood significantly improves college attendance rates and earnings for children who were young (below age 13) when their families moved.*"
- "*The treatment effects are substantial: children whose families take up an experimental voucher to move to a lower-poverty area when they are less than 13 years old have an annual income that is 3,477 (31%) USD higher on average relative to a mean of 11,270 USD in the control group in their mid-twenties.*"

[5] Chetty, R., Hendren, N. and Katz, L. F. (2016) The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. American Economic Review, 106(4), 855-902.

Glossary I

ATET: Average treatment effect on the treated, 17
Hawthorne effext, 30
MTO: Moving to Opportunity, 36
neighborhood effects, 36
Project STAR, 32
randomized experiment, 23, 25
selection bias, 19