ECON 626: Applied Microeconomics

Lecture 1:

Selection Bias and the Experimental Ideal

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Potential Outcomes

Do Hospitals Make People Healthier?

Your health status is: excellent, very good, good, fair, or poor?

	Hospital	**No Hospital**	**Difference**
Health status	3.21	3.93	-0.72^{***}
	(0.014)	(0.003)	
Observations	7,774	90,049	

A simple comparison of means suggests that going to the hospital makes people worse off: those who had a hospital stay in the last 6 months are, on average, less healthy than those that were not admitted to the hospital

- What's wrong with this picture?

Potential Outcomes

We are interested in the relationship between "**treatment**" and some outcome that may be impacted by the treatment (eg. health)

Outcome of interest:

- Y = outcome we are interested in studying (e.g. health)
- Y_i = value of outcome of interest *for individual i*

For each individual, there are two **potential outcomes**:

- $Y_{0,i}$ = i's outcome if she **doesn't** receive treatment
- $Y_{1,i}$ = i's outcome if she **does** receive treatment

Potential Outcomes

Alejandro has a broken leg.

- $Y_{0,a}$ = If he doesn't go to the hospital, his leg doesn't heal properly
- $Y_{1,a}$ = If he goes to the hospital, his leg heals completely

Benicio doesn't have any broken bones. His health is fine.

- $Y_{0,b}$ = If he doesn't go to the hospital, his health is still fine
- $Y_{1,b}$ = If he goes to the hospital, his health is still fine

The fundamental problem of causal inference:

We never observe both potential outcomes for the same individual

⇒ Creates a missing data problem if we compare treated to untreated

Potential Outcomes

For any individual, we can only observe one potential outcome:

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

where D_i is a treatment indicator (equal to 1 if i was treated)

- Each individual either participates in the program or not
- The causal impact of program (D) on i is: $Y_{1i} - Y_{0i}$

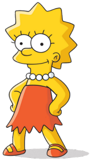
We only observe i's actual outcome:

$$Y_i = Y_{0i} + \underbrace{(Y_{1i} - Y_{0i})}_{impact} D_i$$

Example: Alejandro goes to the hospital, Benicio does not

Establishing Causality

In an ideal world (research-wise), we could clone each treated individual and observe the impacts of treatment on the outcomes of interest



vs.

What is the impact of giving Lisa a textbook on her test score?

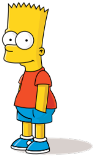
- Impact = Lisa's score with a book - Lisa's score without a book

In the real world, we either observe Lisa with a textbook or without

- We never observe the **counterfactual**

Establishing Causality

To measure the causal impact of giving Lisa a book on her test score, we need to find a similar child that did not receive a book



vs.

Our estimate of the impact of the book is then the difference in test scores between the **treatment group** and the **comparison group**

- Impact = Lisa's score with a book - Bart's score without a book

As this example illustrates, finding a good comparison group is hard

- In applied micro, your research design **is** your counterfactual

Average Causal Effects

What we actually want to know is the **average causal effect**, but that is not what we get from a difference in means comparison

Difference in group means

$= \text{average causal effect of program on participants} + \text{selection bias}$

Even in a large sample:

- People will choose to participate in a program when they expect the program to make them better off (i.e. when $Y_{1,i} - Y_{0,i} > 0$)
- The people who choose to participate are likely yo be different than those who choose not to... *even in the absence of the program*

Selection Bias

When we compare (many) participants to (many) non-participants:

$$\textbf{Difference in group means} = E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$$

$$= E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 0]$$

Adding in $\underbrace{-E[Y_{0,i}|D_i = 1] + E[Y_{0,i}|D_i = 1]}_{=0}$, we get:

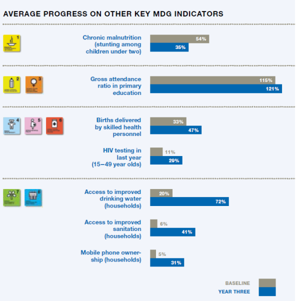
Difference in group means

$$= \underbrace{E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 1]}_{\text{average causal effect on participants}} + \underbrace{E[Y_{0,i}|D_i = 1] - E[Y_{0,i}|D_i = 0]}_{\text{selection bias}}$$

How Can We Estimate Causal Impacts?

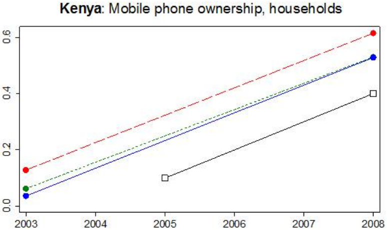
Another approach: comparing pre-treatment vs. post-treatment

The perils of pre vs. post analysis should be obvious. . .
But sometimes pre vs. post analysis still happens to smart people



Data on pre-treatment and post-treatment outcomes in Bar Sauri, Kenya, comes from an early evaluation of the Millenium Villages Project

How Can We Estimate Causal Impacts?



Clemens-Demombynes (2010) compare changes in phone ownership in Bar Sauri (rectangles) to trends in Kenya (in red), rural Kenya (in green), and rural areas in Nyanza Province (in blue)

- The problem is obvious: before vs. after analysis assumes that there is no time trend in mobile phone ownership in Kenya

How Can We Estimate Causal Impacts?

Two types of **false counterfactuals**:

- Pre-treatment vs. Post-treatment Comparisons
- Participant vs. Non-Participant Comparisons

Extremely strong (read: often completely unreasonable) assumptions are required to make either of these impact evaluation approaches credible

How Can We Estimate Causal Impacts?

Quasi-experimental approaches:

- Difference-in-difference estimation
  - Idea: combine pre/post + treated/untreated designs
  - Requirement: common trends in treatment, comparison groups
- Instrumental variables
  - Idea: find a source of **exogenous variation** in treatment
  - Requirement: a valid instrument (satisfying the exclusion restriction)
- Regression discontinuity
  - Idea: exploit explicit rules (cutoffs) for assigning treatment
  - Requirement: the existence of discontinuity

How Can We Estimate Causal Impacts?

Alternatives approaches:

- Conditional Independence Assumption (CIA) approaches
  - "$\hat{\theta}_{hfb}$" – Associate Professor Bryan Graham, UC Berkeley
  - Matching estimators (i.e. propensity score matching)
  - Coefficient stability (robustness to controls)
  - Explicit models (structural or not) of selection into treatment
- Natural experiments (when treatment is as-good-as-random)
  - Example: rainfall shocks in childhood (Maccini and Yang 2009)
  - Closely related to instrumental variables approach

The Experimental Ideal

How Can We Estimate Causal Impacts?

Experimental approach:

- **Random assignment to treatment:** eligibility for program is literally determined at random, e.g. via pulling names out of hat

The **law of large numbers** tells us that a sample average can be brought as close as we like to the population average just by enlarging the sample

When treatment is randomly assigned,
the treatment, control groups are random samples of a single population (e.g. the population of all eligible applicants for the program)

$$\Rightarrow E[Y_{0,i}|D_i = 1] = E[Y_{0,i}|D_i = 0] = E[Y_{0,i}]$$

Expected outcomes are the same in the absence of the program

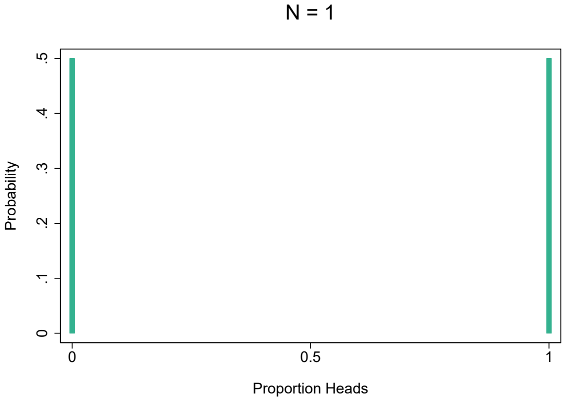
Random Assignment Solves the Selection Problem

Example: imagine that I want to evaluate the impact of Stata 16 so I randomly choose which of my two RAs should receive a copy

Omitted variables still likely to matter — by chance — in small samples

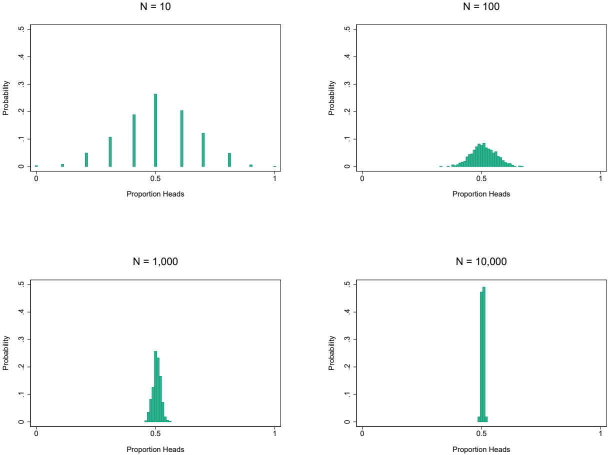
> *"Randomization works not by eliminating individual difference but rather by ensuring that the mix of individuals being compared is the same. Think of this as comparing barrels that include equal proportions of apples and oranges."*

Random Assignment & the Law of Large Numbers



The **law of large numbers** tells us that a sample average can be brought as close as we like to the population average just by enlarging the sample

Random Assignment & the Law of Large Numbers


N = 10

N = 100

N = 1,000

N = 10,000

The **law of large numbers** tells us that a sample average can be brought as close as we like to the population average just by enlarging the sample

Random Assignment Eliminates Selection Bias

If treatment is random and $E[Y_{0,i}|D_i = 1] = E[Y_{0,i}|D_i = 0] = E[Y_{0,i}]$

The difference in means estimator gives us the average treatment effect:

Difference in group means

$$= E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$$

$$= E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 0]$$

$$= E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 1] + E[Y_{0,i}|D_i = 1] - E[Y_{0,i}|D_i = 0]$$

$$= \underbrace{E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 1]}_{\text{average treatment effect on participants}} + \underbrace{E[Y_{0,i}] - E[Y_{0,i}]}_{=0}$$

$$= \underbrace{E[Y_{1,i}] - E[Y_{0,i}]}_{\text{ATE}}$$

Internal Validity

Excellent news: random assignment eliminates selection bias*

*Some restrictions apply

The **Stable Unit Treatment Value Assumption (SUTVA)**:

- "The potential outcomes for any unit do not vary with the treatments assigned to other units."

  Source: Imbens and Rubin (2015)

When is SUTVA likely to be violated?

Causal Effects in the Presence of Spillovers

- What is the appropriate unit of randomization?
  - Cluster-randomized trials make sense when spillovers are anticipated
- When can we use additional assumptions to measure the direct and indirect effects of treatment (e.g. via multi-level randomization)?
- When can we anticipate the direction of bias?

Internal Validity: Additional Assumptions?

Imbens and Rubin include a second component of SUTVA:

- “There are no different forms or versions of each treatment level which lead to different potential outcomes.”

This terminology is not standard, and the assumption is often violated

- Treatments often vary across locations or strata
- Cox (1958) proposes an alternative: “either only average treatment effects are required, or that the treatment effects are constant”
  - In other words, we’ll always have internal validity
  - External validity is another matter

Gerber and Green (2012) highlight an add’l assumption, **excludability**: the treatment shouldn’t be confounded (well, duh, right?)

Randomization: A History of Thought

Randomized Experiments in Theory

Petrarch (1364):

> *"If a hundred thousand men of the same age, same temperament and habits, together with the same surroundings, were attacked at the same time by the same disease, that if one half followed the prescriptions of the doctors of the variety of those practicing at the present day, and that the other half took no medicine but relied on nature's instincts, I have no doubt as to which half would escape."*

van Helmont (who died in 1644):

> *"Let us take out of the Hospitals, pit of the Camps, or from elsewhere, 200 or 500 poor People, that have Fevers, Pleurisies, etc. Let us divide them in halfes, let us cast lots, that one half of them may fall to my share, and the other to yours; I will cure them without bloodletting... we shall see how many Funerals both of us shall have."*

Source: Jamison (2019)

Randomization: A Timeline (Part I)

1885 Pierce and Jastrow use randomization in a psychology experiment (varying order in which different stimuli are presented to subjects)

1898 Johannes Fibiger conducts a trial of an anti-diphtheria serum in which every other subject is assigned to treatment (or control)

1923 Neyman suggests the idea of potential outcomes

1925 **Fisher suggests the explicit randomization of treatments (in the context of agriculture experiments)**

1926 Amberson *et al* study of sanocrysin treatments for TB: coin flipped to determine which group receives treatment, which group serves as controls

1948 Randomized trial of streptomycin treatment for TB conducted by the Medical Research Council of Great Britain

⇒ Randomized evaluations become the norm in medicine

The Lady Tasting Tea

Chapter II of Fisher's *The Design of Experiments* begins:

> *"A lady declares that by tasting a cup of tea made with milk she can discriminate whether the milk or the tea infusion was first added to the cup."*

Critical lesson to take away from this anecdote:
Caffeine breaks with colleagues are critical to the advancement of science

- The lady in question was biologist Muriel Bristol, who worked with Fisher at the Rothamsted Experimental Station in Harpenden, UK
- H_0: Fisher believes that Dr. Bristol cannot taste the difference
- A test of the hypothesis: *"Our experiment consists in mixing eight cups of tea, four in one way and four in the other, and presenting them to the subject for judgment in a random order."*

The Lady Tasting Tea: Experimental Design

Rule #1: do not confound your own treatment

- Critical assumption: if Dr. Bristol is unable to detect whether the milk was poured in first, then she will choose 4 cups at random
- Fisher points out that the experimenter could screw this up:

> *"If all those cups made with the milk first had sugar added,*
> *while those made with the tea first had none,*
> *a very obvious difference in flavour would have been introduced*
> *which might well ensure that all those made with sugar*
> *should be classed alike."*

- Gerber and Green refer to this as **excludability**

The Lady Tasting Tea: Experimental Design

Rule #1B: do not accidentally confound your own treatment

- Fisher, in (perhaps) the earliest known scientific subtweet:

  > *"It is not sufficient remedy to insist that 'all the cups must be exactly alike' in every respect except that to be tested. For this is a totally impossible requirement."*

- To minimize the likelihood of accidentally confounding your treatment, the best approach is to constrain yourself by randomizing
  - Randomization minimizes the likelihood of unfortunate coincidences
  - This was a highly controversial position at the time, and it is still debated in some circles; the alternative is to force balance (on observables, and then just hope that unobservables don't matter)

The Lady Tasting Tea: a Hypothesis Test

How should we interpret data from this experiment?

Suppose Dr. Bristol correctly identified all 4 “treated” cups

- How likely is it that this outcome could have occurred by chance?
  - There are $\binom{8}{4} = 70$ possible ways to choose 4 of 8 cups
  - Only one is correct; a subject with no ability to discriminate between treated and untreated cups would have a $1/70$ chance of success
  - The p-value associated with this outcome is $1/70 \approx 0.014$, which is less than the cutoff for the “standard level of significance” of 0.05

The Lady Tasting Tea: a Hypothesis Test

How should we interpret data from this experiment?

Suppose Dr. Bristol correctly identified 3 “treated” cups

- How likely is it that this outcome could have occurred by chance?
  - There are $\binom{4}{3} \times \binom{4}{1} = 16$ possible ways to choose 3 of 8 cups
    - There are 17 ways to choose **at least** 3 correct cups
  - The p-value associated with this outcome is $17/70 \approx 0.243$
  - We should not reject the null hypothesis

The only experimental result that would lead to the rejection of the null hypothesis was correct identification of all 4 treated cups

- In the actual experiment, the null hypothesis was rejected

Fisher's Exact Test

	Identified by Dr. Bristol?	
	Yes	**No**
Milk poured first	a	b
Tea poured first	c	d

Is Dr. Bristol more likely to select cups where the milk was poured first?

$$\text{probability} = \frac{\binom{a+b}{a}\binom{c+d}{c}}{\binom{a+b+c+d}{a+c}} = \frac{(a+b)!(c+d)!(a+c)!(b+d)!}{a!b!c!d!(a+b+c+d)!}$$

The p-value is the sum of the probabilities of outcomes that are at least as extreme (i.e. contrary to H_0) as the observed outcome

The Lady Tasting Tea: Size and Power

The size of a test is the likelihood of rejecting a true null

- Fisher asserts that tests of size 0.05 are typical

Alternative experiment: what if we had treated 3 out of 6 cups of tea?

- There are $\binom{6}{3} = 20$ possible ways to choose 3 of 6 cups
- Best possible p-value is therefore 0.05

Alternative experiment: what if we had treated 3 out of 8 cups of tea?

- There are $\binom{8}{3} = 56$ possible ways to choose 3 of 8 cups
- Best possible p-value is therefore 0.017

$\Rightarrow$ **Optimal to have equal numbers of treated, untreated cups**

The Lady Tasting Tea: Size and Power

An alternate experiment: an unknown number of treated cups

- Under the null, the probability of getting 8 right is 1 in 2^8
- Probability of getting 7 right is $8/256 = 0.03125$

This design would achieve higher power with the same number of trials

- Possible to reject the hypothesis that the lady tasting tea cannot tell the difference even when her ability to discriminate is imperfect

Ronald Fisher's Contributions to Statistics

1. Introduced the modern randomized trial
2. Introduced the idea of permutation tests
3. Reminded us of the importance of caffeine

Fisher's permutation-based approach to inference is not the norm in economics; our default is regression analysis and classical statistics

Randomization: A Timeline (Part II)

1942 Launch of Cambridge-Somerville Youth Study of at-risk boys

1962 Perry Preschool (Ypsilanti) and Early Training Project (Murfreesboro) experiments randomize assignment of at-risk children to preschools

1967 New Jersey Income Maintenance Experiment (proposed by Heather Ross)
Four other negative income tax experiments between 1971 and 1982

1972 Abecedarian Project randomized early intervention for at-risk infants (NC)

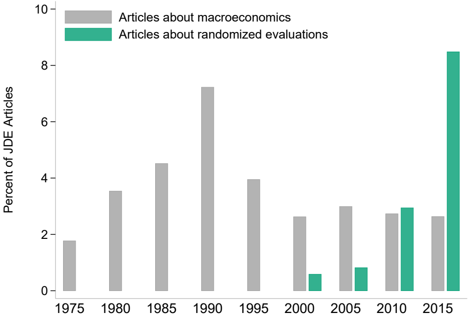
1974 Rubin introduces the concept of potential outcomes (as we know it)

1994 National Job Corps Study (done by Mathematica for US Dept. of Labor)

1995 PROGRESA evaluation launched by Mexican government

1998 Dutch NGO ICS begins cluster-randomized trial of mass deworming in 75 Kenyan primary schools... in partnership with Harvard's Michael Kremer

RCTs in Development Economics



Search of abstracts of 2,695 *Journal of Development Economics* articles

What Do We Learn from Randomized Experiments?

Constant Treatment Effects? Really?

Consider the hospitalization example?

- Is it reasonable to assume that treatment effects are homogeneous?
- **No.** Clearly, people go to the hospital when they are sick

A more interesting thought experiment:

- $z =$ i's health if she doesn't get sick
- $s =$ the reduction in health associated with sickness
- $b =$ benefit a **sick** person receives from treatment
- $c =$ the reduction in health from going to the hospital

Reasonable to assume that $b > c > 0$

Potential Outcomes: Hospital Example

	$Y_{0,i}$	$Y_{1,i}$
Sick	$z - s$	$z - s + b - c$
Not sick	z	$z - c$

What happens without random assignment?

- Do healthy people go to the hospital?
- Do sick people go to the hospital?

Life without Random Assignment

Let S_i be an indicator for being sick

- $E[S_i|D_i = 1] = ?$
- $E[S_i|D_i = 0] = ?$

What do we learn from a comparison of means?

$$\begin{aligned}\textbf{difference in means} &= E[Y_i|D_i = 1] - E[Y_i|D_i = 0] \\ &= E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 0] \\ &= z - s + b - c - z \\ &= b - c - s\end{aligned}$$

Difference in means is the treatment effect on those who choose to take up the treatment (i.e. on the sick) plus selection bias

Random Assignment: Entire Population

Suppose, absurdly, we randomize who goes to the hospital such that:

$$\lambda = E[S_i|D_i = 1] = E[S_i|D_i = 0] = E[S_i]$$

Randomization breaks the link between illness and going to the hospital

What does the difference in means tell us?

$$\begin{aligned}
\textbf{difference in means} &= E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 0] \\
&= \underbrace{z - E[S_i|D_i = 1]\,(s - b) - c}_{=E[Y_{1,i}|D_i=1]} - \underbrace{\{z - E[S_i|D_i = 0]s\}}_{=E[Y_{0,i}|D_i=0]} \\
&= z - \lambda s + \lambda b - c - (z - \lambda s) \\
&= \lambda b - c
\end{aligned}$$

Difference in means = ATE of hospitalization on the population

Random Assignment: Sick People

Suppose we randomize treatment assignment among the sick:

$$E[S_i|D_i = 1] = E[S_i|D_i = 0] = 1$$

What does the difference in means tell us?

$$\begin{aligned}\textbf{difference in means} &= E[Y_{1,i}|D_i = 1] - E[Y_{0,i}|D_i = 0] \\ &= \underbrace{z - s + b - c}_{=E[Y_{1,i}|D_i=1]} - \underbrace{\{z - s\}}_{=E[Y_{0,i}|D_i=0]} \\ &= b - c\end{aligned}$$

Difference in means = ATE of hospitalization on the sick

Is this the ideal experiment? Why or why not?

Random Assignment: Endogenous Take-Up

We might consider randomizing **access** to treatment:

- Let T_i be an indicator for random assignment to a treatment group that is allowed to choose whether or not to go to the hospital
- Those in the control group cannot use the hospital

Q: Who will choose to go to the hospital?

- **A:** People who get sick during the study
- $E[D_i|T_i = 1] = ?$

When take-up is endogenous, we (usually) have **imperfect compliance**

- With one-sided non-compliance: compliers vs. never-takers

Random Assignment: Endogenous Take-Up

What does the difference in means tell us in this case?

$$\begin{aligned}
\textbf{difference in means} &= E[Y_{1,i}|T_i = 1] - E[Y_{0,i}|T_i = 0] \\
&= \underbrace{z + E[S_i|T_i = 1](-s + b - c)}_{=E[Y_{1,i}|T_i=1]} - \underbrace{\{z - E[S_i|T_i = 0]s\}}_{=E[Y_{0,i}|T_i=0]} \\
&= z - \lambda s + \lambda b - \lambda c - (z - \lambda s) \\
&= \lambda (b - c)
\end{aligned}$$

Difference in means = ATE of access to hospitalization

- The ATE is the **intent-to-treat** effect
- ITT = compliance × effect of **treatment on the treated**

External Validity

Three randomized evaluations, three average treatment effects

- How much can we learn from a single study?
- How much can we learn without a model?

A more realistic evaluation scenario would have considered:

- A broader range of heterogeneous treatment effects
- Two-sided non-compliance
  - Encouragement designs may increase take-up among the healthy

None of these problems is specific to randomized evaluations

External Validity

In many early randomized evaluations, the ATE of interest was clear

- The impact of new seed varieties on crop yields
- The impact of medical treatments on patients with specific ailments

Economists consider a very broad range of “treatments”

- The impact of access to credit
- The impact of having two children of the same gender
- The impact of going on the Hajj
- The impact of sunshine on the 4^{th} of July during childhood

A good research idea requires (1) identification and (2) a model