STA 305/1004 - Class 8

February 3, 2016

Today's Class

- Read class notes & try the problems.
- Next HW by end of this week.

- Introduction to causal inference
- The fundemental problem of causal inference
- The assignment mechanism
- Getting around the fundemental problem
- SUTVA Stable unit treatment value assumption
- Omitted variable bias in regression
- Observational studies
- Ignorable treatment assignment

- Suppose Bob, at a particular point in time, is contemplating whether or not to take an aspirin for a headache.
- There are two treatment levels, taking an aspirin, and not taking an aspirin.
- If Bob takes the aspirin, his headache may be gone, or it may remain, say, an hour later; we denote this outcome, which can be either "Headache" or "No Headache," by $Y(\text{Aspirin})$. → Indicator
- Similarly, if Bob does not take the aspirin, his headache may remain an hour later, or it may not; we denote this potential outcome by $Y(\text{No Aspirin})$, which also can be either "Headache," or "No Headache."
- There are therefore two potential outcomes, $Y(\text{Aspirin})$ and $Y(\text{No Aspirin})$, one for each level of the treatment. The causal effect of the treatment involves the comparison of these two potential outcomes.

causal effect: $Y(\text{aspirin}) - Y(\text{no aspirin})$

(But impossible to estimate)

Introduction to causal inference - Bob's headache

Because in this example each potential outcome can take on only two values, the unit- level causal effect – the comparison of these two outcomes for the same unit – involves one of four (two by two) possibilities:

1. Headache gone only with aspirin: Y(Aspirin) = No Headache, Y(No Aspirin) = Headache
2. No effect of aspirin, with a headache in both cases: Y(Aspirin) = Headache, Y(No Aspirin) = Headache
3. No effect of aspirin, with the headache gone in both cases: Y(Aspirin) = No Headache, Y(No Aspirin) = No Headache
4. Headache gone only without aspirin: Y(Aspirin) = Headache, Y(No Aspirin) = No Headache

There are two important aspects of this definition of a causal effect.

"Treatment does not affect potential outcome"

1. The definition of the causal effect depends on the potential outcomes, but it does not depend on which outcome is actually observed.
2. The causal effect is the comparison of potential outcomes, for the same unit, at the same moment in time post-treatment.

- The causal effect is not defined in terms of comparisons of outcomes at different times, as in a before-and-after comparison of my headache before and after deciding to take or not to take the aspirin.

The fundemental problem of causal inference

"The fundamental problem of causal inference" (Holland, 1986, p. 947) is the problem that at most one of the potential outcomes can be realized and thus observed.

- If the action you take is Aspirin, you observe Y(Aspirin) and will never know the value of Y(No Aspirin) because you cannot go back in time.
- Similarly, if your action is No Aspirin, you observe Y(No Aspirin) but cannot know the value of Y(Aspirin).
- In general, therefore, even though the unit-level causal effect (the comparison of the two potential outcomes) may be well defined, by definition we cannot learn its value from just the single realized potential outcome.

The fundemental problem of causal inference

The outcomes that would be observed under control and treatment conditions are often called **counterfactuals** or **potential outcomes**.

- If Bob took asprin for his headache then he would be assigned to the treatment condition so $T_i = 1$. treatment indicator
- Then Y(Aspirin) is observed and Y(No Aspirin) is the unobserved counterfactual outcome—it represents what would have happened to Bob if he had not taken aspirin.
- Conversely, if Bob had not taken aspirin then Y(No Aspirin) is observed and Y(Aspirin) is counterfactual.
- In either case, a simple treatment effect for Bob can be defined as

$$\text{treatment effect for Bob} = Y(\text{Aspirin}) - Y(\text{No Aspirin}).$$

only observe 1 of the treatment on Bob.

- The problem is that we can only observe one outcome.

The assignment mechanism

- **Assignment mechanism:** The process for deciding which units receive treatment and which receive control.
- **Ignorable Assignment Mechanism:** The assignment of treatment or control for all units is independent of the unobserved potential outcomes ("nonignorable" means not ignorable)
- **Unconfounded Assignment Mechanism:** The assignment of treatment or control for all units is independent of all potential outcomes, observed or unobserved ("confounded" means not unconfounded)

The assignment mechanism

perfect doctor example:

- Suppose that a doctor prescribes surgery (labeled 1) or drug (labeled 0) for a certain condition.
- The doctor knows enough about the potential outcomes of the patients so assigns each patient the treatment that is more beneficial to that patient.

unit	$Y_i(0)$	$Y_i(1)$	$Y_i(1) - Y_i(0)$
patient #1	1	7	6
patient #2	6	5	-1
patient #3	1	5	4
patient #4	8	7	-1
Average	4	6	2

→ indicates that surgery is better

Y is years of post-treatment survival.

The assignment mechanism

- Patients 1 and 3 will receive surgery and patients 2 and 4 will receive drug treatment.
- The observed treatments and outcomes are in this table.

unit	T_i	Y_i^{obs}	$Y_i(1)$	$Y_i(0)$
patient #1	1	7		
patient #2	0	6		
patient #3	1	5		
patient #4	0	8		
Average Drug		**7**		
Average Surg		**6**		

No, does not agree.
Drug has longer
life expect vs. drug

- This shows that we can reach invalid conclusions if we look at the observed values of potential outcomes without considering how the treatments were assigned.
- The assignment mechanism depended on the potential outcomes and was therefore nonignorable (implying that it was confounded).

NOT independent of obs. potential outcomes

The assignment mechanism

The observed difference in means is entirely misleading in this situation. The biggest problem when using the difference of sample means here is that we have effectively pretended that we had an unconfounded treatment assignment when in fact we did not. This example demonstrates the importance of finding a statistic that is appropriate for the actual assignment mechanism.

The assignment mechanism

Is the treatment assignment ignorable?

- The doctor knows enough about the potential outcomes of the patients so assigns each patient the treatment that is more beneficial to that patient.
- Suppose that a doctor prescribes surgery (labeled 1) or drug (labeled 0) for a certain condition by tossing a biased coin that depends on $Y(0)$ and $Y(1)$, where Y is years of post-treatment survival.
- If $Y(1) \geq Y(0)$ then $P(T_i = 1 | Y_i(0), Y_i(1)) = 0.8$. "biased coin"
- If $Y(1) < Y(0)$ then $P(T_i = 1 | Y_i(0), Y_i(1)) = 0.3$.

we assign these prob.

unit	$Y_i(0)$	$Y_i(1)$	$P(T_i = 1 \| Y_i(0), Y_i(1))$	$P(T_i = 0 \| Y_i(0), Y_i(1))$
patient #1	1	7	0.8	0.2
patient #2	6	5	0.3	0.7
patient #3	1	5	0.8	0.2
patient #4	8	7	0.3	0.7

randomization is built in.

still not ignorable even though some (still depends on potential outcomes)

Weight gain study

From Holland and Rubin (1983).

"A large university is interested in investigating the effects on the students of the diet provided in the university dining halls and any sex differences in these effects. Various types of data are gathered. In particular, the weight of each student at the time of his [or her] arrival in September and his [or her] weight the following June are recorded."

- The average weight for Males was 180 in both September and June. Thus, the average weight gain for Males was zero.
- The average weight for Females was 130 in both September and June. Thus, the average weight gain for Females was zero.
- Question: What is the differential causal effect of the diet on male weights and on female weights?
- Statistician 1: Look at gain scores: No effect of diet on weight for either males or females, and no evidence of differential effect of the two sexes, because no group shows any systematic change.
- Statistician 2: Compare June weight for males and females with the same weight in September: On average, for a given September weight, men weigh more in June than women. Thus, the new diet leads to more weight gain for men.
- Is Statistician 1 correct? Statistician 2? Neither? Both?

Weight gain study

4. No, we observe treated outcome for all the units & control outcome for none.
. Assign-mech should involve some replication for each treatment.

5. No. We want replication at each value of covariates X.

another treatment is "not eating in the dining hall" (but we don't have this?)

Questions:

1. What are the units? 1 Students M/F in September
2. What are the treatments? 2 eating @ university dining hall
3. What is the assignment mechanism? 3 All students are exposed to dining hall diet, so assignment mechanism is all students receive treatment with prob 1 & all students receive control with prob 0
4. Is the assignment mechanism useful for causal inference?
5. Would it have helped if all males received the dining hall diet and all females received the control diet?
6. Is Statistician 1 or Statistician 2 correct?

The assignment mechanism is unconfounded b/c prob of treatment not related to potential outcomes

6 Either could be correct depending on how we fill in values:
- To support Statistician 1 fill in all units Y(0) values with Sept. weight (or June weight)

- To support Stat 2 predict $Y(0)$ in a linear regression on vector
- Take away
  - both Stats can be made correct
  - everyone in dataset is treated

unit #	sex	Sept wt	T_i	$Y(0)$	$Y(1)$
1	F	x_1	1	?	92
2	F	x_2	1	?	95
⋮					
N	M	x_N	1	?	100

$P(X_i = 1 \mid x_i)$
$= 1$ for all $i = 1, \ldots, N$

- Would you assess the effectness of a pill by giving everyone the pill?
- to draw causal inferences need

$$0 < P(T_i = 1 \mid X_i) < 1 \quad \forall i$$

must be stochastic

- need unconfounded assignment
  - ↳ ass-mech does not depend on potential outcome

Getting around the fundemental problem by using close substitutes

- Are there situations where you can measure both Y_i^0 and Y_i^1 on the same unit?
- Drink tea one night and milk another night then measure the amount of sleep. What has been assumed? no systematic diff b/w days ; same person
- Divide a piece of plastic into two parts then expose each piece to a corrosive chemical. What has been assumed? assume pieces are the same

$$y_1^0 = y_2^0$$
$$y_1^1 = y_2^1$$

- Measure the effect of a new diet by comparing your weight before the diet and your weight after. What has been assumed?
- There are strong assumptions implicit in these types of strategies.

$$y_i^0 = x_i = \text{pretreatment weight}$$

Getting around the fundemental problem by using randomization and experimentation

- The "statistical" idea of using the outcomes observed on a sample of units to learn about the distribution of outcomes in the population.
- The basic idea is that since we cannot compare treatment and control outcomes for the same units, we try to compare them on similar units.
- Similarity can be attained by using randomization to decide which units are assigned to the treatment group and which units are assigned to the control group.

Getting around the fundemental problem by using randomization and experimentation

- It is not always possible to achieve close similarity between the treated and control groups in a causal study.
- In observational studies, units often end up treated or not based on characteristics that are predictive of the outcome of interest (for example, men enter a job training program because they have low earnings and future earnings is the outcome of interest).
- Randomized experiments can be impractical or unethical.
- When treatment and control groups are not similar, modeling or other forms of statistical adjustment can be used to fill in the gap.

Fisherian Randomization Test

- The randomization test is related to a stochastic proof by contradiction giving the plausability of the null hypothesis of no treatment effect.
- The null hypothesis is $Y_i^0 = Y_i^1$, for all units.
- Under the null hypothesis all potential outcomes are known from Y^{obs} since $Y^{obs} = Y^1 = Y^0$.
- Under the null hypothesis, the observed value of any statistic, such as $\bar{y}^1 - \bar{y}^0$ is known for all possible treatment assignments.
- The randomization distribution of $\bar{y}^1 - \bar{y}^0$ can then be obtained.
- Unless the data suggest that the null hypothesis of no treatment effect is false then it's difficult to claim evidence that the treatments are different.

Stable Unit Treatment Value Assumption (SUTVA)

To learn about causal effects through multiple experimental units assumptions are required.

The assumption. The potential outcomes for any unit do not vary with the treatments assigned to other units, and, for each unit, there are no different forms or versions of each treatment level, which lead to different potential outcomes.

Stable Unit Treatment Value Assumption (SUTVA)

- Involves assuming that treatments applied to one unit do not affect the outcome for another unit.
- For example, if Adam and Oliver are in different locations and have no contact with each other, it would appear reasonable to assume that if Oliver takes an an aspirin for his headache then his behaviour has no effect on the status of Adam's headache.
- This assumption might not hold if Adam and Oliver are in the same location, and Adam's behavior, affects Oliver's behaviour.
- SUTVA incorporates the idea that Adam and Oliver do not interfere with one another and the idea that for each unit there is only a single version of each treatment level (e.g., there is only one dose of aspirin). (Imbens and Rubin, 2015)

Electric company study

The following example is from Gelman and Hill (2007).

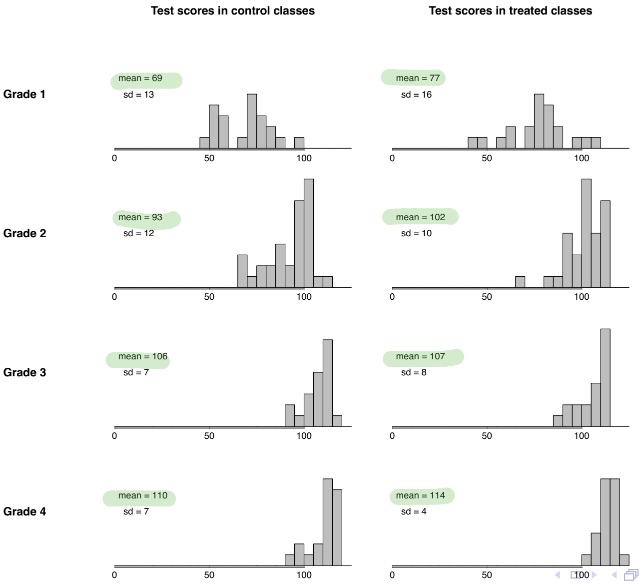
An educational experiment performed on four elementary school classes (grades 1-4) was conducted.

- **Treatment:** Exposure to a new educational television show called The Electric Company.
- **Treatment assignment:** Classes were randomized into treated and control groups.
- **Primary study outcome:** At the end of the school year, students in all the classes were given a reading test, and the average test score within each class was recorded.

A pre-test was given at the beginning of the school year before the treatment. For grade 1 the pre-test was a subset of the longer post-test and in grades 2-4 the pre-test was the same as the post-test.

Electric company study

The distribution of scores are shown for each treatment and control groups for each grade.



No diff

No diff

Electric company study

The average causal effect of the treatment is the coefficient in the regression

$$y_i = \beta_0 + \beta_1 T_i + \epsilon_i,$$

$T_i = \begin{cases} 1 & \text{if class } i \text{ matches show} \\ 0 & \text{o.w.} \end{cases}$

for each grade.

```
attach(electric)
post.test <- c (treated.Posttest, control.Posttest)
grade <- rep (Grade, 2)
treatment <- rep (c(1,0), rep(length(treated.Posttest),2))

for (k in 1:4){
  cat("Grade:",k, "\n")
  print(summary((lm (post.test ~ treatment, subset=(grade==k)))))
  print(confint(lm (post.test ~ treatment, subset=(grade==k))))
}
```

Electric company study

pretest not included !

```
Grade: 1

                2.5 %   97.5 %
(Intercept) 62.184754 75.3962
treatment   -1.041902 17.6419
```

H_0: no diff $\beta_1 = 0$

Fail to reject since 0 is contained in 95% CI for β_1.

```
Grade: 2

               2.5 %   97.5 %
(Intercept) 89.40453 97.01900
treatment    2.97458 13.74307
```

Reject: since 0 ∉ 95% CI

```
Grade: 3

                 2.5 %     97.5 %
(Intercept) 102.809071 109.540929
treatment    -4.425143   5.095143
```

Fail to reject

```
Grade: 4

                   2.5 %     97.5 %
(Intercept) 107.732487230 112.981798
treatment    -0.002299775   7.421347
```

Fail to reject

Electric company study

What does the model

$$y_i = \beta_0 + \beta_1 T_i + \epsilon_i,$$

assume about pre-test scores?

Omitted variable bias in regression

a confounder is associated with treatment & outcome (hard to disentangle)

Suppose that the following model is correct

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 x_i + \epsilon_i,$$

includes pretest score

if $\beta_2 = 0$ then no assoc. b/w outcome & confounder(x_i)

where x_i is the i^{th} pre-test score.

Assume that the confounding covariate, x_i, is ignored and we fit the model

$$y_i = \beta_0^* + \beta_1^* T_i + \epsilon_i^*.$$

What is the relation between these models?

$x_i = \gamma_0 + \gamma_1 T_i + v_i \longrightarrow$ plug into correct model

$\Rightarrow \beta_1^* = \beta_1 + \gamma_1 \beta_2 \longrightarrow$ if $\gamma_1 = 0$, then there's no association between treatment & pretest

Electric company study

The pre-test can be controlled by fitting the model:

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 x_i + \epsilon_i,$$

where x_i is the pre-test.

```
attach(electric)
post.test <- c (treated.Posttest, control.Posttest)
pre.test <- c (treated.Pretest, control.Pretest)
grade <- rep (Grade, 2)
treatment <- rep (c(1,0), rep(length(treated.Posttest),2))

for (k in 1:4){
  cat("Grade:",k, "\n")
  print(summary((lm (post.test ~ treatment+pre.test, subset=(grade==k)))))
  print(confint(lm (post.test ~ treatment+pre.test, subset=(grade==k))))
}
```

Electric company study

```
Grade:  1
                  2.5 %     97.5 %
(Intercept) -28.794124  6.748422
treatment     3.503741 14.069296
pre.test      3.996396  6.220489
Grade:  2
                  2.5 %     97.5 %
(Intercept) 29.4582320 45.3993009
treatment    1.5517446  6.9797863
pre.test     0.6793567  0.8984661
Grade:  3
                  2.5 %     97.5 %
(Intercept) 33.0310748 48.1374132
treatment    0.3370718  3.4823799
pre.test     0.6066683  0.7626586
Grade:  4
                  2.5 %     97.5 %
(Intercept) 33.3343230 50.6551288
treatment    0.3151918  3.0876810
pre.test     0.5732627  0.7383963
```

By omitting pretest score, we introduce a bias into the estimate of the causal effect (which est. by coeff. of reg. model)

Observational Studies

assumed in for example t-test

- A solution to the fundamental problem of causal inference is, as we have described, to randomly sample a different set of units for each treatment group assignment from a common population, and then apply the appropriate treatments to each group; or
- Randomly assign the treatment conditions among a selected set of units. Both approaches are supposed to guaruntee that, on average, the different treatment groups are balanced. In other words, $\bar{y}^0$ and $\bar{y}^1$ from the sample are estimating the average outcomes under control and treatment for the same population.

Randomization test

Observational Studies

- In observational studies treatments are observed rather than assigned (e.g., comparison of lung cancer rates in smokers to nonsmokers), and the observed data under different treatments is usually not a random sample from a common population.
- In an observational study there might be systematic differences between groups of units, outside the control of the experimenter, that receive different treatments that can affect the outcome, y.

Observational Studies

- The educational experiment described above had an embedded observational study.
- Once the treatments had been assigned, the teacher for each class assigned to the Electric Company treatment chose to either replace or supplement the regular reading program with the Electric Company television show.
- All the classes in the treatment group watched the show, but some watched it instead of the regular reading program and others got it in addition.

- Let's assume that the choice between the two treatment options—"replace" or "supplement"—to be randomly assigned conditional on pre-test scores.
- This is a strong assumption but we use it simply as a starting point. We can then estimate the treatment effect by regression, as with an actual experiment.

```
attach(electric)
supp <- c(as.numeric(electric[,"Supplement."])-1, rep(NA,nrow(electric)))
# supp=0 for replace, 1 for supplement, NA for control

est1 <- rep(NA,4)
se1 <- rep(NA,4)
for (k in 1:4){
  ok <- (grade==k) & (!is.na(supp))
  cat("Grade:",k, "\n")
  print(summary(lm (post.test ~ supp + pre.test, subset=ok)))
  print(confint(lm (post.test ~ supp + pre.test, subset=ok)))
}
```

Observational Studies

```
Grade:  1
                2.5 %    97.5 %
(Intercept) -25.732934 25.301026
supp         -3.413773 15.259775
pre.test      3.075675  6.372427
Grade:  2
                2.5 %     97.5 %
(Intercept) 26.1746504 52.7975109
supp         0.9463903  8.4273952
pre.test     0.6090171  0.9543507
Grade:  3
                2.5 %     97.5 %
(Intercept) 30.3113901 56.6268005
supp        -4.4901749  1.8944099
pre.test     0.5528908  0.8163994
Grade:  4
                2.5 %     97.5 %
(Intercept) 33.4755555 75.9830111
supp        -2.7842683  2.1365878
pre.test     0.3542762  0.7551821
```

only effective in grade 2

Ignorable Treatment Assignment

- The observational study of replace or supplement was treated as a completely randomized experiment and an estimate of the treatment effect was calculated.
- What has been assumed?
- The distribution of classes across the treatment variable (replace/supplement) is random with respect to the potential outcomes.
- This is almost correct, since the regression adjusts for pre-test scores.
- So, the distribution of classes with respect to potential outcomes is random across the treatment groups *conditional* on the confounding covariate pre-test.

Strongly ignorable Treatment Assignment

potential outcome under control

potential outcome under treatment

If treatment assignment T is conditionally independent of y^0, y^1 given confounding covariates X and and $0 < P(T = 1|y^0, y^1, X) < 1$ then the treatment assignment is said to be **strongly ignorable**.

$$y^0, y^1 \perp T|X.$$

independent

This means that the conditional distribution of potential responses is the same across levels of the treatment variable once we condition on the confounding covariates X.

Another way to view ignorability is:

$$P(T = 1|y^0, y^1, X) = P(T = 1|X).$$

Statistical indep. A is indep. of B iff $P(A|B)=P(A)$

Strongly ignorable Treatment Assignment

- Imagine that two grade 1 classes have the same average pre-test scores.
- A coin toss might determine which of these two classes would get replacement or supplement.
- This doesn't mean that any two classes will have the same probability of receiving the suplement.
- The ignorability assumption means that if we want to interpret the regression coefficient for treatment as an average causal effect then all the counfounding covariates should be controlled for in the regression model.

- Then there could be other covariates.
- In design stage this can be controlled in an expt.
- One can never prove treatment assign. in an observational study is ignorable.

END