STA 305/1004 - Class 8

February 1, 2017

Today's Class

- Introduction to causal inference

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- The fundemental problem of causal inference

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- The fundemental problem of causal inference
- The assignment mechanism

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- Getting around the fundemental problem

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- Ignorable treatment assignment

Introduction to causal inference - Bob's headache

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- 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."

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- 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.

$$\text{treat} \rightarrow \text{Aspirin} = \begin{cases} \text{Yes} \\ \text{No} \end{cases}$$

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

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2. The causal effect is the comparison of potential outcomes, for the same unit, at the same moment in time post-treatment.

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Y (Show up at time t .) = divorce

Y (Don't Show up at time t) = Stay married

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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.

Y(Caught train) = Good life

Y (miss train) = poor life

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.

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- 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.

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

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

The assignment mechanism

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- **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

- Suppose that a doctor prescribes surgery (labeled 1) or drug (labeled 0) for a certain condition.

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

2 year Survival advantage with Surgery!

Y is years of post-treatment survival.

The assignment mechanism

- 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.

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patient #1	1	7 ✓	6
patient #2	6 ✓	5	-1
patient #3	1	5 ✓	4
patient #4	8 ✓	7	-1
Average	4	6	2

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.

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**		
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- 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.

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Drug has 1 Year Survival advantage over Surgery.

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- The assignment mechanism depended on the potential outcomes and was therefore nonignorable (implying that it was confounded).

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.

unit	$Y_i(0)$	$Y_i(1)$	p_1	p_0
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

where, $p_1 = P(T_i = 1 | Y_i(0), Y_i(1))$,and $p_0 = P(T_i = 0 | Y_i(0), Y_i(1))$.

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- 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.

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- If $Y(1) \geq Y(0)$ then $P(T_i = 1|Y_i(0), Y_i(1)) = 0.8$.

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- If $Y(1) \geq Y(0)$ then $P(T_i = 1|Y_i(0), Y_i(1)) = 0.8$.
- If $Y(1) < Y(0)$ then $P(T_i = 1|Y_i(0), Y_i(1)) = 0.3$.

unit	$Y_i(0)$	$Y_i(1)$	p_1	p_0
patient #1	1	7	0.8	0.2
patient #2	6	5	0.3	0.7
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where, $p_1 = P(T_i = 1|Y_i(0), Y_i(1))$,and $p_0 = P(T_i = 0|Y_i(0), Y_i(1))$.

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."

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- 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.

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- 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

Questions:

1. What are the units? Students male / Female

Weight gain study

Questions:

1. What are the units?
2. What are the treatments? → dinning hall diet and Control diet which Students would have eaten if not eating dinning hall diet.

Weight gain study

Questions:

1. What are the units?
2. What are the treatments?
3. What is the assignment mechanism?

all Students exposed to dinning hall diet
the assignment mechanism is all Student's
receive treatment with probability 1,
and Control with prob. 0.

Weight gain study

Questions:

1. What are the units?
2. What are the treatments?
3. What is the assignment mechanism?
4. Is the assignment mechanism useful for causal inference?

$Y(1) - Y(0)$ estimated can't be

unit #	Sex	Sept. wt.	T	Y(0)	Y(1)
1	F	x_1	1	?	y_1
2	F	x_2	1	?	y_2

This assignment mechanism is not useful for causal inference.

Weight gain study

e.g.,

Randomize

males → Dinning hall
males → control

Females → Dinning hall
Females → Control

Value of x.

Questions:

1. What are the units?
2. What are the treatments?
3. What is the assignment mechanism?
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?

No. We want to have replication at each value of x – covariate for sex.
→ we want to see some treated and controls at each value of x. Only way to achieve is to have random assignment at each

Weight gain study

Questions:

1. What are the units?
2. What are the treatments?
3. What is the assignment mechanism?
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?

either could be correct depending on how we define $Y(0)$.

Statistician 1: fill in all $Y(0)$ values with Sept. Weight (or June wt.).

To support Statistician 2 that new diet leads to more weight gain for men. Predict $Y(0)$ to be a constant and unit's Sept. weight another constant

Regress $Y(1)$ on vector of ones and Sept. weight). Do the same for females.

Takeaway: - Both Statisticians can be made correct

Lord's Paradox

- everyone in data set is treated.

- to draw causal conclusions

$$0 < p\left(T_i = 1 \mid X_i\right) < 1$$

- need unconfounded assign. mechanism and stochastic assign. mechanism.

Getting around the fundemental problem by using close substitutes

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- Drink tea one night and milk another night then measure the amount of sleep. What has been assumed?

No Systematic diffs. between days that Could effect Sleep.

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- Drink tea one night and milk another night then measure the amount of sleep. What has been assumed?
- Divide a piece of plastic into two parts then expose each piece to a corrosive chemical. What has been assumed?

assumes that the pieces are identical in how they respond to chemical

$y_1^0 = y_2^0 \quad , \quad y_1^1 = y_2^1$

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- Measure the effect of a new diet by comparing your weight before the diet and your weight after. What has been assumed?

Pre-treatment measure acts as a Substitute for potential outcome under Control. $y_i^0 = x_i =$ pre-treat wt.

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- 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.

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- 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.

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- 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

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$H_0: \mu_A = \mu_B$

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- 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)

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- 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.

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.

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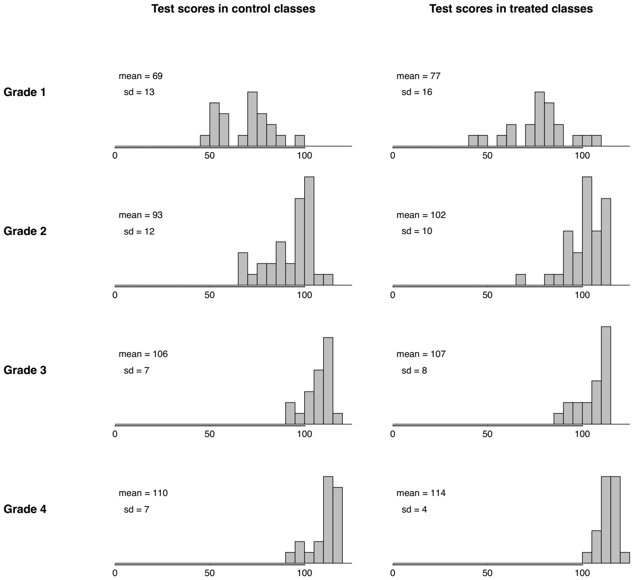
- **Treatment assignment:** Classes were randomized into treated and control groups.
- **Primary study outcome:** At the end of the school year, students in all the classes were given a reading test, and the average test score within each class was recorded.

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.



Electric company study

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

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

for each grade.

Electric company study

95% CIs for β_1

```
Grade: 1

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

$\therefore$ the 95% CI includes 0 $\therefore$ not enough evidence to reject $H_0: \beta_1 = 0$

```
Grade: 2

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

→ does not contain 0 $\therefore$ reject H_0.

```
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

If we reject at level α $H_0: \beta_1 = 0$ vs $H_1: \beta_1 \neq 0$

then p-Value $\leq \alpha$

If the 95% $100(1-\alpha)\%$ CI for β_1 does not Contain 0 (the null value) then p-Value $\leq .05$ (α)

If the p-value ≤ 0.05 then the 95% CI for β_1 will not Contain 0.

Duality between CIs and Two-Sided Hypothesis Tests.

Electric company study

What does the model

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

represents the difference in means and $T_i = 0$ vs $T_i = 1$.

assume about pre-test scores?

Omitted variable bias in regression

Suppose that the following model is correct

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

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?

Omitted variable bias in regression!

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 x_i + \varepsilon_i$$

$$y_i = \beta_0^* + \beta_1^* T_i + \varepsilon_i^*$$

(omit pre-test score)

$$x_i = \gamma_0 + \gamma_1 T_i + v_i$$

plug into correct model

$$y_i = \beta_0 + (\beta_1 + \gamma_1 \beta_2) T_i + (\gamma_0 + v_i)\beta_2 + \varepsilon_i$$

$$\beta_1^* = \beta_1 + \gamma_1 \beta_2 \Big\} \text{ no bias if } \gamma_1 \beta_2 = 0$$

if $\gamma_1 = 0$ then no association between treatment and confounder. If $\beta_2 = 0$ then no assoc. between outcome and confounder

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
```

Sig.

→ Sig.

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Observational Studies

- 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

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- 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.

Main benefit of randomization is that we obtain two treatment groups balanced wrt observed and unobserved covariates.

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.

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- 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

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- 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.

Some Students → Show

→ Show + reg. reading.

at teachers discretion.

Observational Studies

- Let's assume that the choice between the two treatment options—"replace" or "supplement"—to be randomly assigned conditional on pre-test scores.

Observational Studies

- 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.

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
```

— no effect

— evidence of effect

no effect

no effect.

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- 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

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**.

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

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:

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

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Strongly ignorable Treatment Assignment

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- 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.