

STA 235H - Observational Studies

Fall 2022

McCombs School of Business, UT Austin

Announcements

- **Grades for Homework 2** posted on Friday
 - Review the Answer Key and re-grading requests until this Friday.
 - **Slides for the review session** on the course website (Resources)
 - Many opportunities for students to get support. **Reach out!**
 - Check out the schedule for **final presentations**:
 - Syllabus > Grading > Project (at the end)

Last week



- Continued discussing **randomized controlled trials**.
- Introduced **stratification** in RCTs:
 - Divide sample in homogeneous groups (by stratifying variables) and randomizing within groups.

Today

- Discuss about **limitations of RCTs**:
 - Generalizability
 - Spillover/General equilibrium effects.
- What **selection on observables**?:
 - Types of bias
 - Matching



Limitations of RCTs

Recap

- In RCTs, **ignorability assumption holds by design**
 - No systematic differences in observed or unobserved characteristics.
- Under the ignorability assumption, we can just **compare the means of both groups** to obtain a causal effect
 - $\bar{Y}_T - \bar{Y}_C$ or `lm(y ~ treat)`
- If we want to ensure balance on certain characteristics, we can **stratify**
 - At the design stage!

Potential issues to have in mind

Generalizability of our estimated effects

- Where did we get our sample for our study from? Is it representative of a larger population?

Spillover effects

- Can an individual in the control group be affected by the treatment?

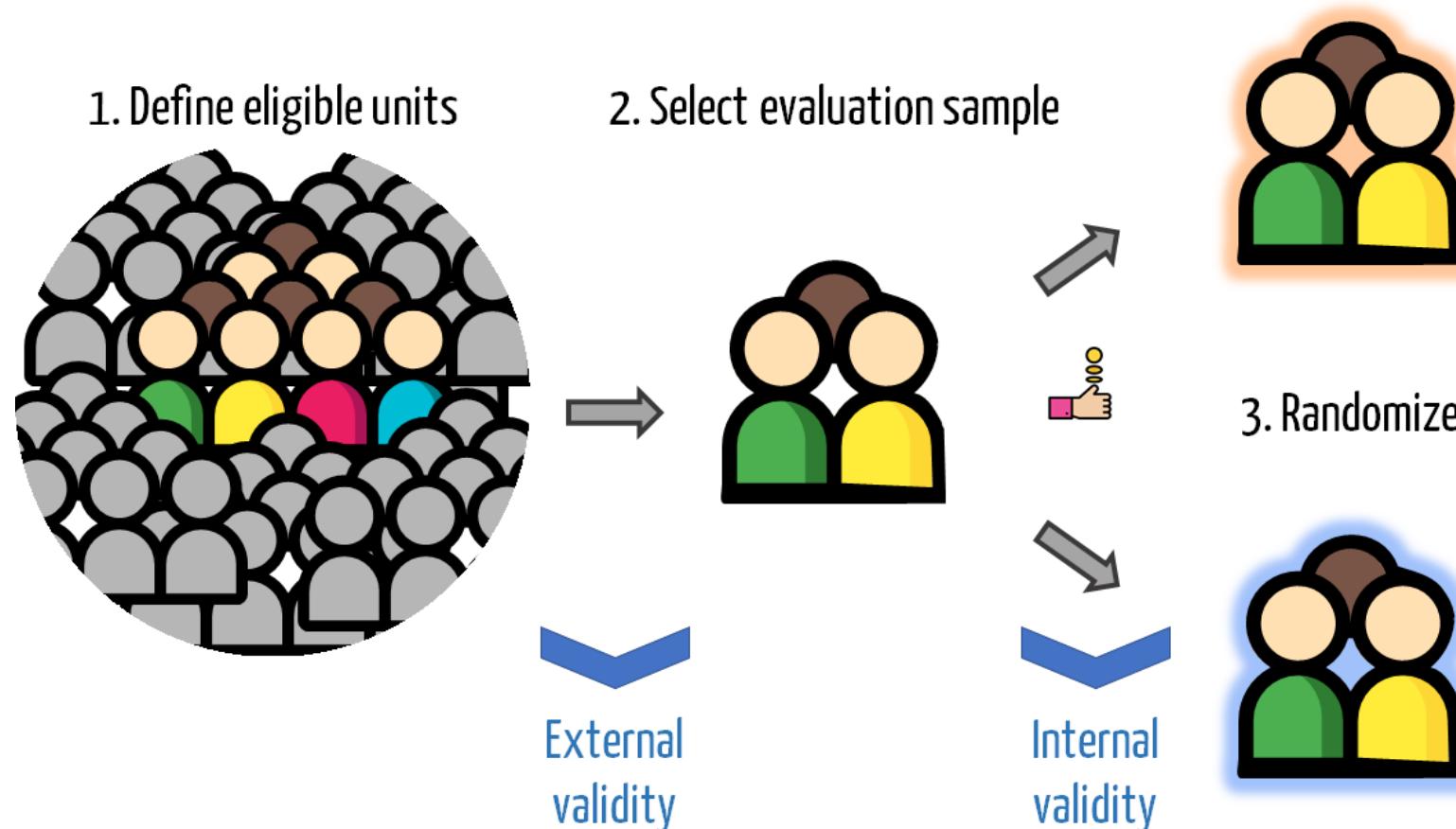
General equilibrium effects

- What happens if we scale up an intervention? Will the effect be the same?

Generalizability of RCTs

- External Validity vs Internal Validity:
 - **External validity:** "The extent to which results can be generalized to other contexts or populations."
 - **Internal validity:** "[T]he extent to which the observed results represent the truth in the population we are studying."

External vs Internal Validity



- Many times, RCTs use **convenience samples**

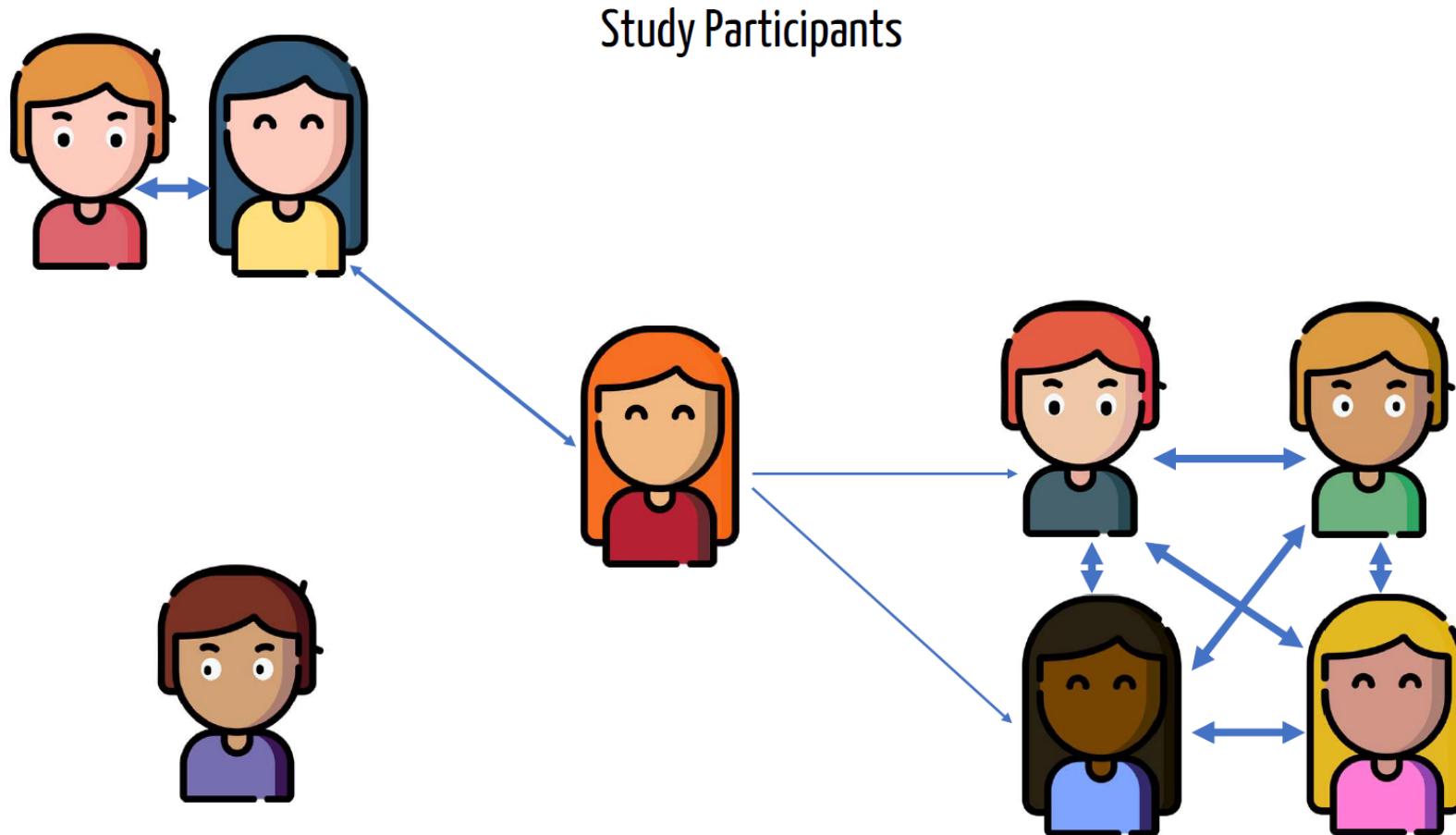
SUTVA: No interference

- Aside from **ignorability**, RCTs rely on the **Stable Unit Treatment Value Assumption (SUTVA)**

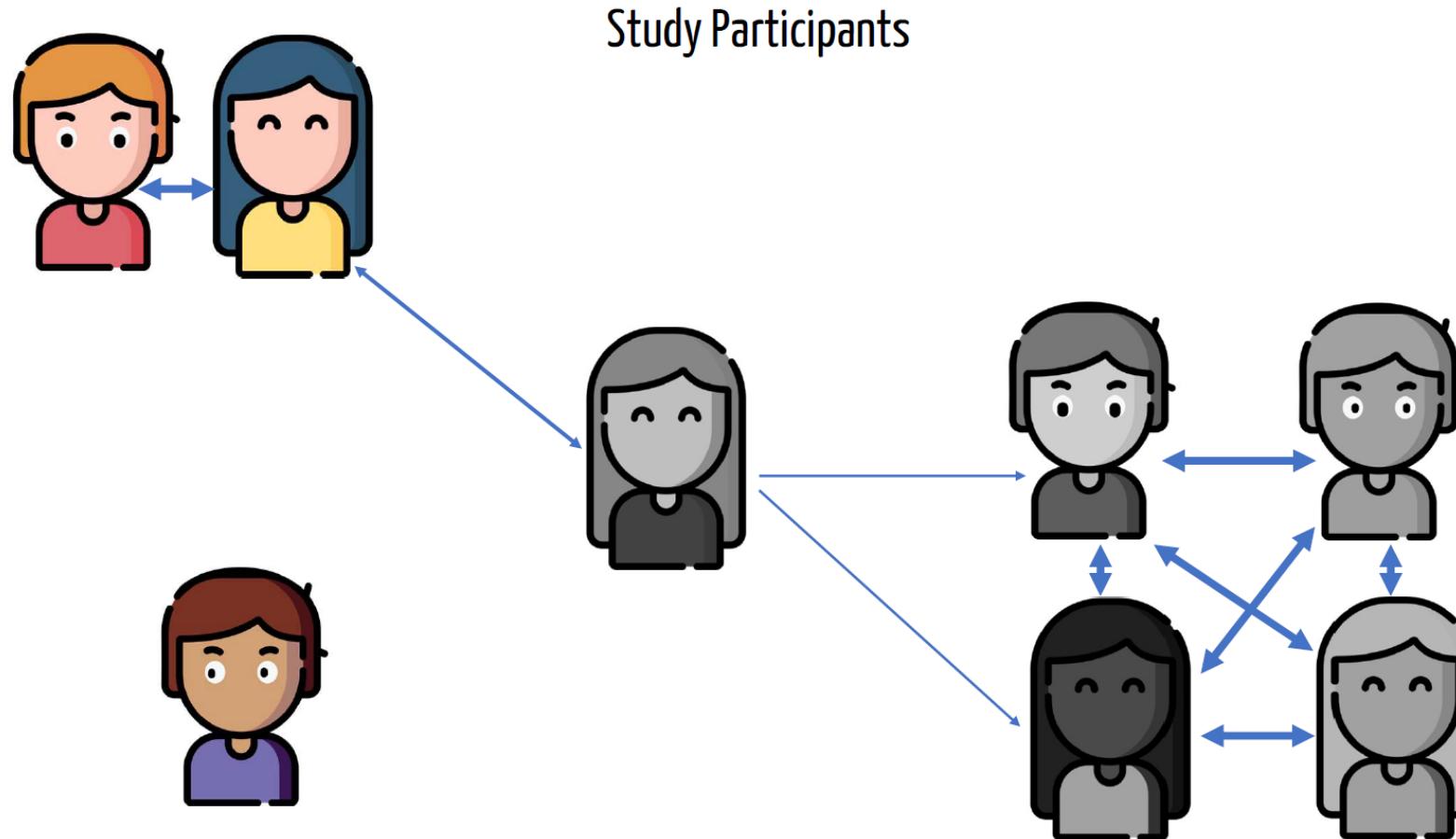
"The treatment applied to one unit does not affect the outcome for other units"

- No **spillovers**
- No **general equilibrium effects**

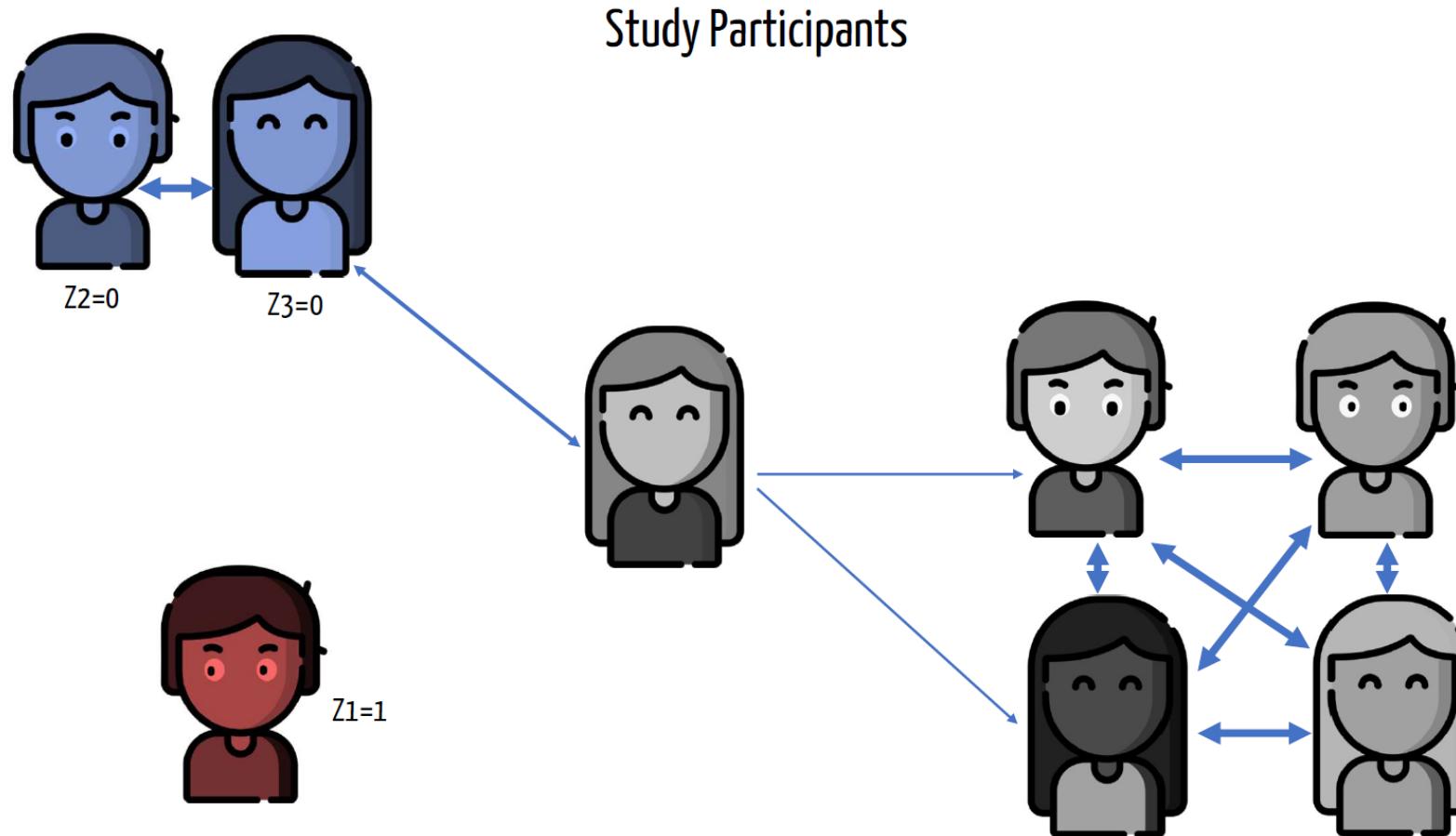
Network effects



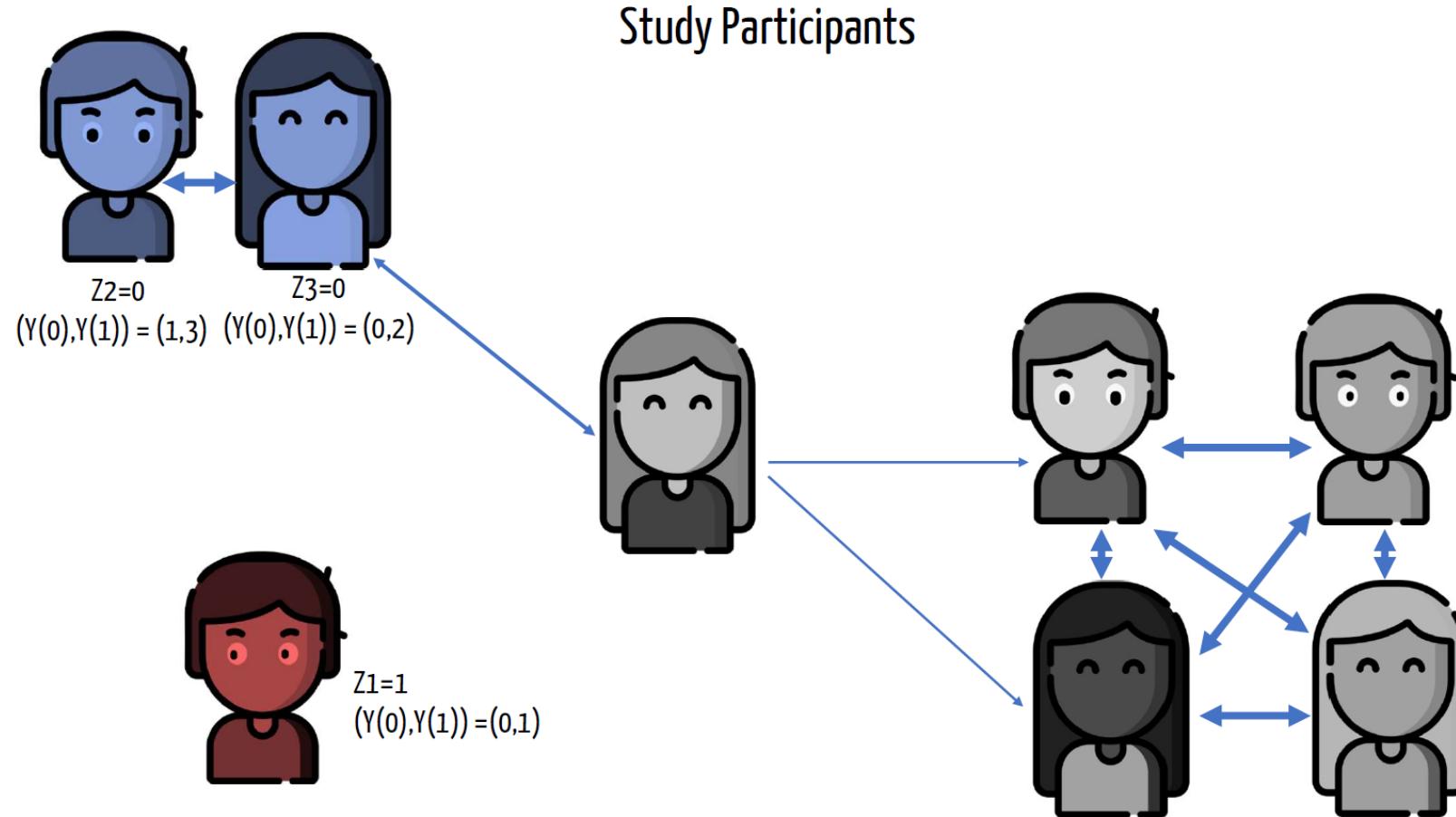
Network effects



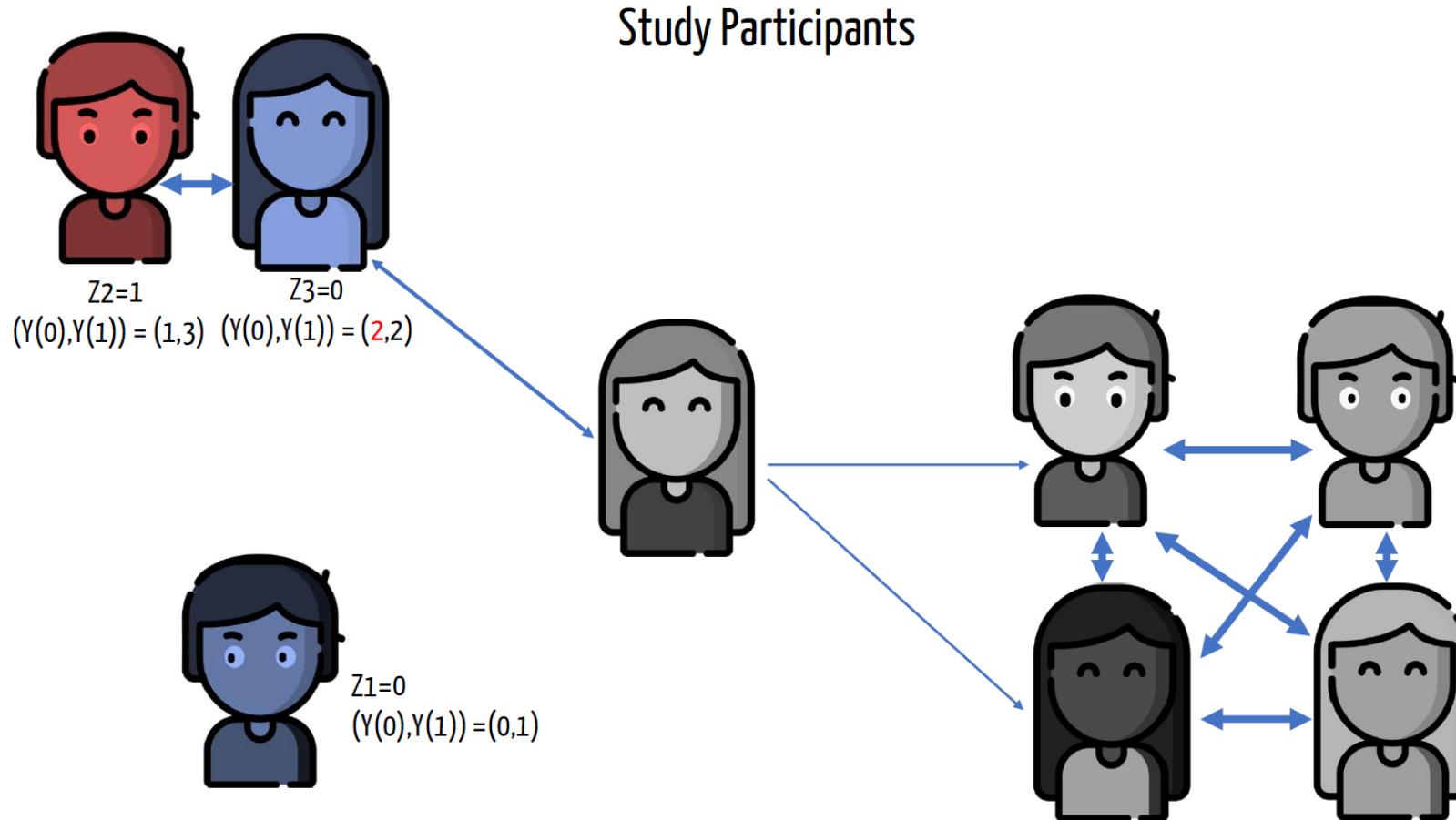
Network effects



Network effects



Network effects



Examples where the treatment of one person can affect another one?

Network effects

Can we do something about this?

1. Randomize at a higher level (e.g. neighborhood, school, etc. instead of at the individual level)
2. Model the network!

General Equilibrium Effects

- Usually arise when you **scale up** a program or intervention.
- Imagine you want to test the effect of providing information about employment and expected income to students to see whether it affect their choice of university and/or major.

What could happen if you offer it to everyone?

Wrapping things up

- Randomized controlled trials are great... **but not for everything!**
- Randomization buys us **no systematic selection on observables or unobservables**
 - But things can go wrong, too!

Check your assumptions and look out for potential issues!

What if we can't randomize?

What happens when the ignorability assumption doesn't hold?

- Imagine we can't randomize

Observational Study

Can we compare two groups to get a causal effect?

What happens when the ignorability assumption doesn't hold?

- If the ignorability assumption doesn't hold, then $(Y(0), Y(1)) \not\perp\!\!\!\perp Z$

$$\tau = E[Y_i(1)] - E[Y_i(0)] \neq E[Y_i|Z=1] - E[Y_i|Z=0]$$

Correlation does not imply causation

What happens when the ignorability assumption doesn't hold?

- Now, let's assume $(Y(0), Y(1)) \not\perp\!\!\!\perp Z$

$$\begin{aligned}\tau &= E[Y_i(1) - Y_i(0)] = \\ &= E[Y_i(1) - Y_i(0)|Z = 1]Pr(Z = 1) + E[Y_i(1) - Y_i(0)|Z = 0](1 - Pr(Z = 1))\end{aligned}$$

What happens if the ignorability assumption doesn't hold?

- Now, let's assume $(Y(0), Y(1)) \not\perp\!\!\!\perp Z$

$$\begin{aligned}\tau &= E[Y_i(1) - Y_i(0)] = ATE = \\ &= \underbrace{E[Y_i(1) - Y_i(0)|Z = 1]}_{ATT} Pr(Z = 1) + \overbrace{E[Y_i(1) - Y_i(0)|Z = 0]}^{\text{ATC}} (1 - Pr(Z = 1))\end{aligned}$$

- Weighted average of the Average Treatment on the treated (ATT) and the average treatment on the control (ATC).

What happens if the ignorability assumption doesn't hold?

- After some simple math, you can get to:

$$\tau = E[Y_i(1) - Y_i(0)] = ATE$$

$$ATE = E[Y_i|Z=1] - E[Y_i|Z=0]$$

$$- (E[Y_i(0)|Z=1] - E[Y_i(0)|Z=0])$$

$$- (1 - Pr(Z=1))(ATT - ATC)$$

Check out Scott Cunningham's "Causal Inference: The Mixtape" (Ch. 4.1.3) for the decomposition

What happens if the ignorability assumption doesn't hold?

- After some simple math, you can get to:

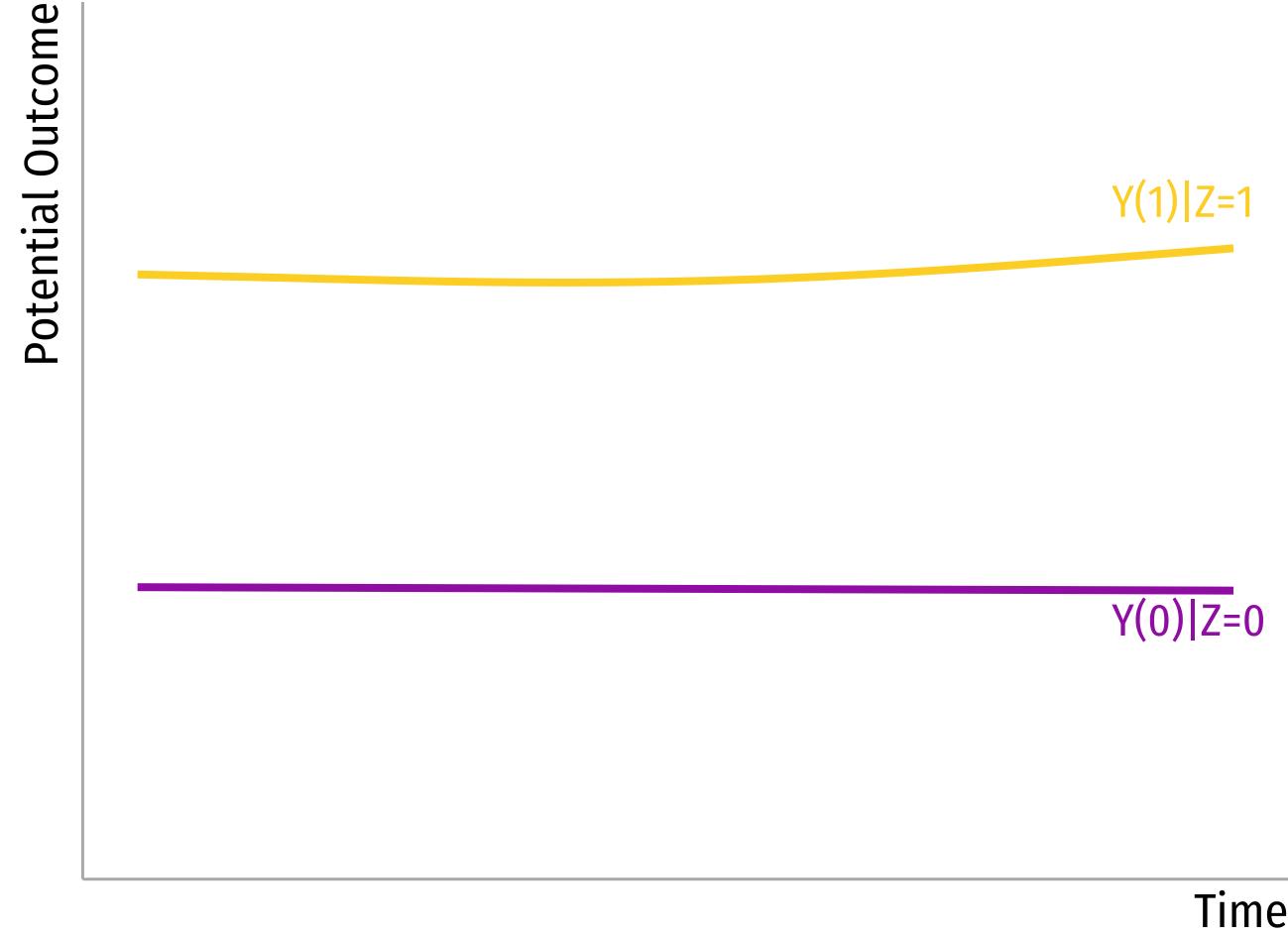
$$\tau = E[Y_i(1) - Y_i(0)] = ATE$$

$$ATE = \underbrace{E[Y_i|Z=1] - E[Y_i|Z=0]}_{\text{Obs diff in means}} \\ - \underbrace{(E[Y_i(0)|Z=1] - E[Y_i(0)|Z=0])}_{\text{Selection bias}} \\ - \underbrace{(1 - Pr(Z=1))(ATT - ATC)}_{\text{Heterogeneous treat. effect bias}}$$

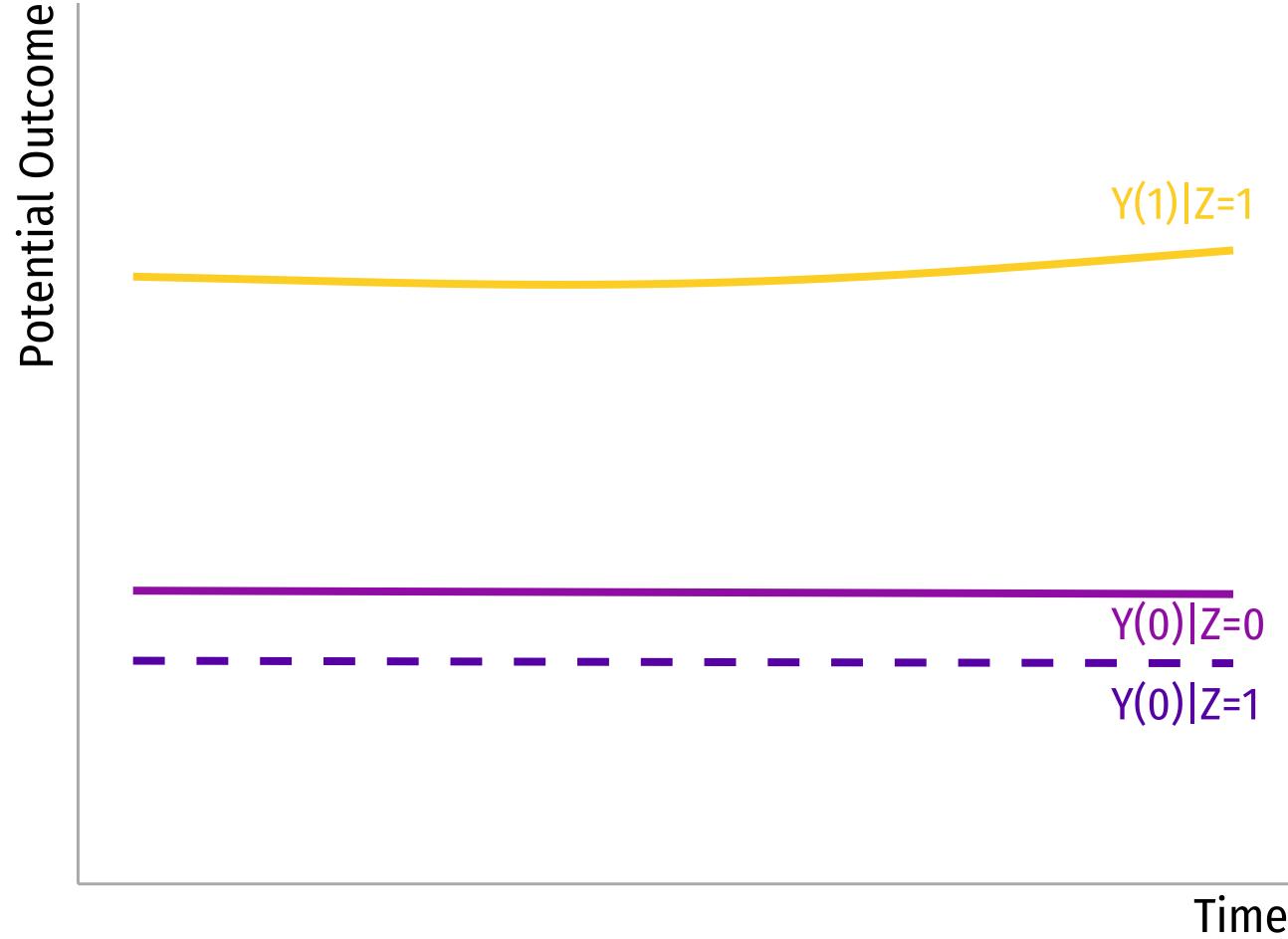
- **Selection Bias:** Difference between groups if they both were under control (e.g. baseline differences).
- **Heterogeneous Treatment Effect Bias:** Difference in returns to treatment for the two groups (weighted by the control population).

Check out Scott Cunningham's "Causal Inference: The Mixtape" (Ch. 4.1.3) for the decomposition

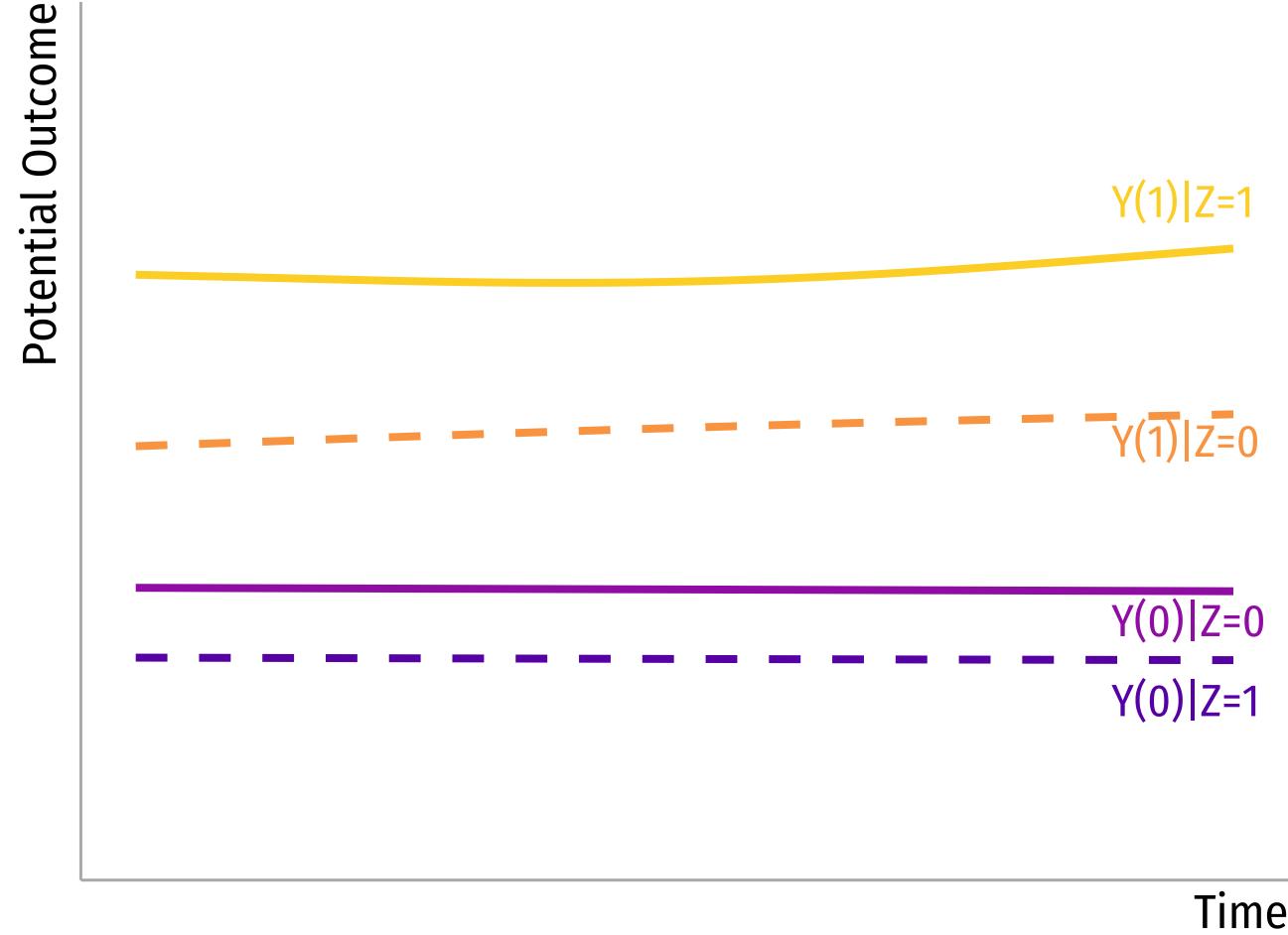
How would bias look like?



How would bias look like?



How would bias look like?



Example: Effect of types of advertising on sales

You want to know whether it is more convenient to **e-mail** or **physically mail** potential customers to increase your subscribers.

- FD sends flyers to potential customers that started the subscription process but never finished.
- FD sends emails to a list of contacts they purchased.



- Can we compare subscriptions between both groups and establish a causal effect?

Example: Going to Office Hours

An important question could be: **Does going to office hours increase our GPA?**

- What could be an example of **selection bias**?
 - Remember that selection bias means differences in $Y(0)$ for people that go to office hours vs those that don't.
- What could be an example of **heterogeneous return to treatment bias**?
 - Remember that heterogeneous return to treatment bias means that $Y(1) - Y(0)$ is different for those that go vs those that don't go to office hours.

How do confounders affect our causal estimate?

confounding variable



What happens if we control by our confounders?

adjusting for confounders



Controlling by Confounders

Controlling by Confounders

- We can control by a confounder by **including it in our regression**:
 - After we control for it, we are doing a fair comparison (e.g. "*holding X constant*")

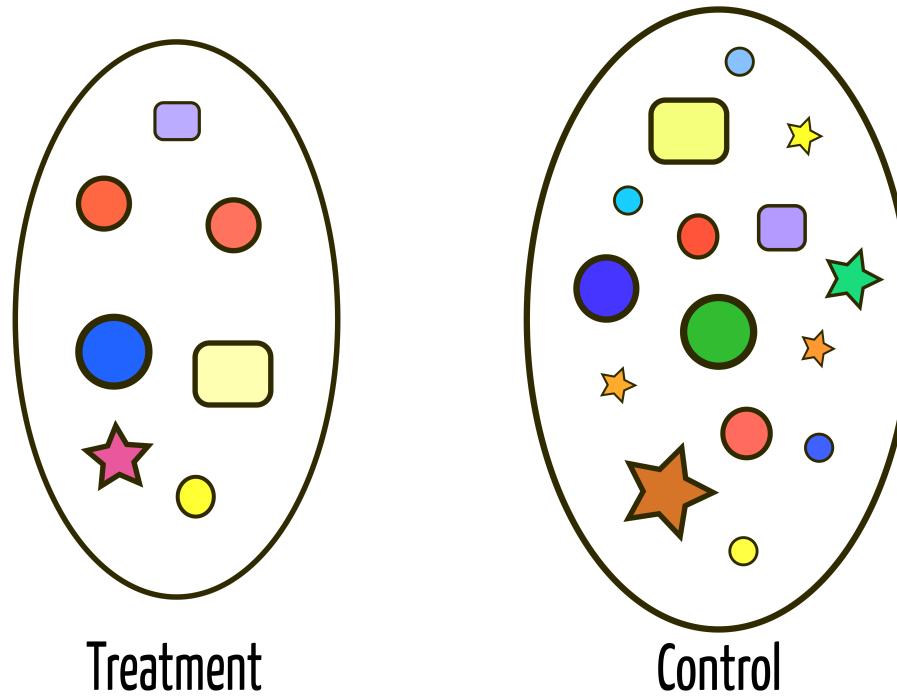
Conditional Independence Assumption (CIA)

- But is there another way to control for confounders?

Matching

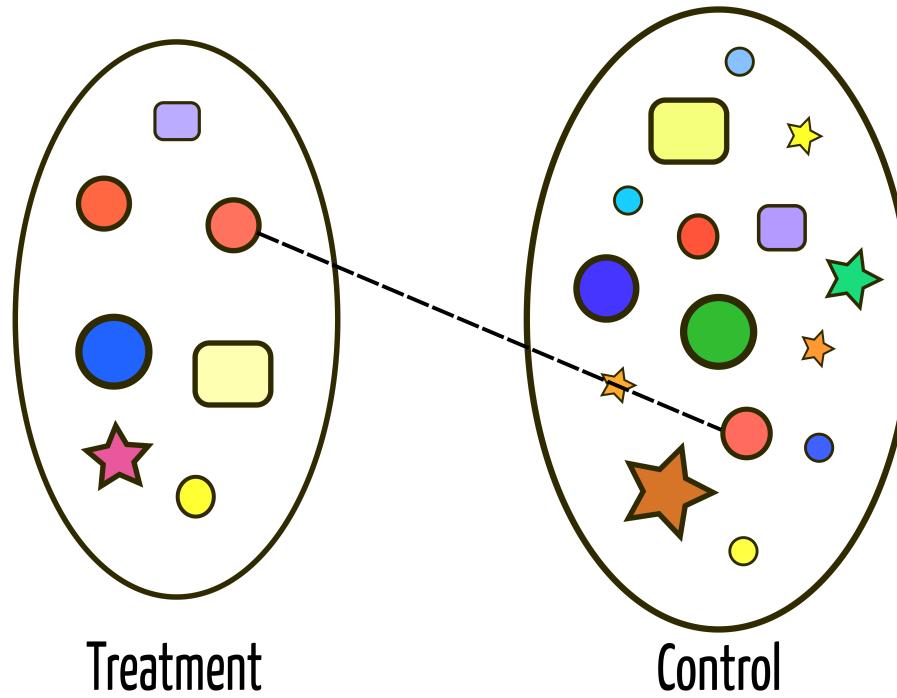
Matching

Start with two groups: A treatment and a control group



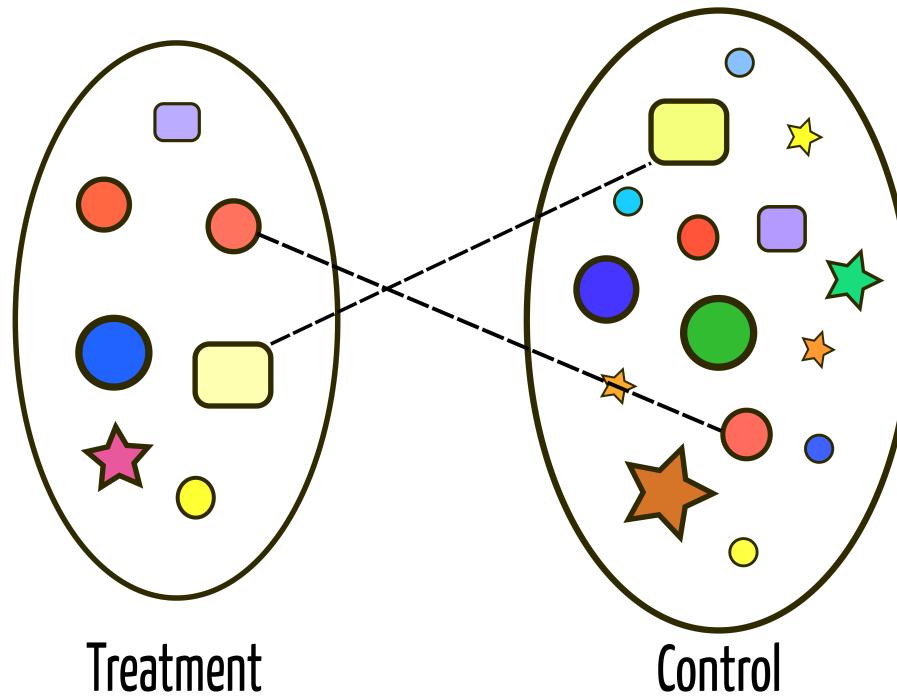
Matching

For each unit in the treatment group, let's find a similar unit in the control group



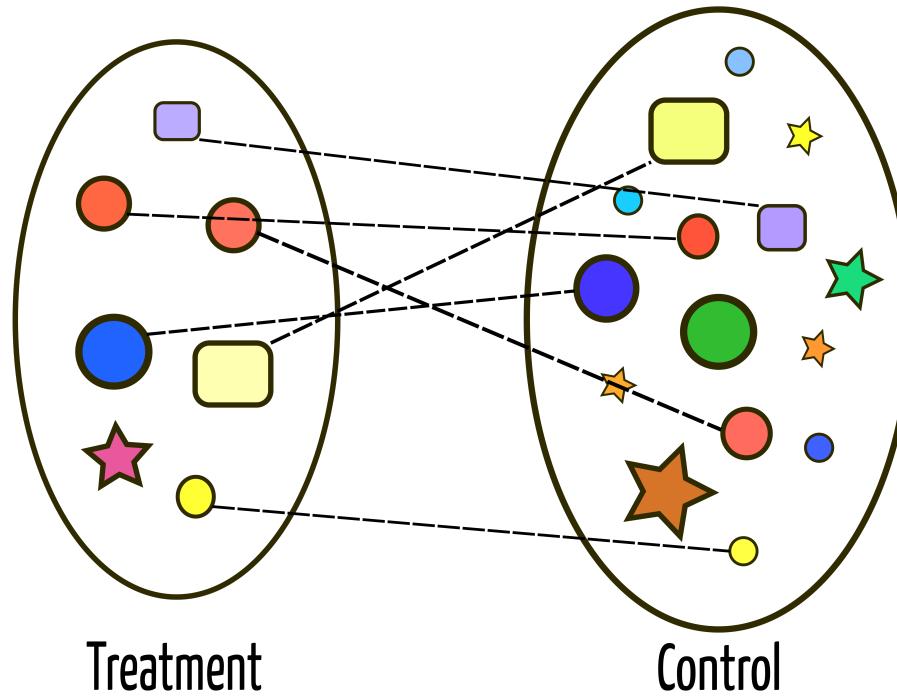
Matching

And we do this for all units



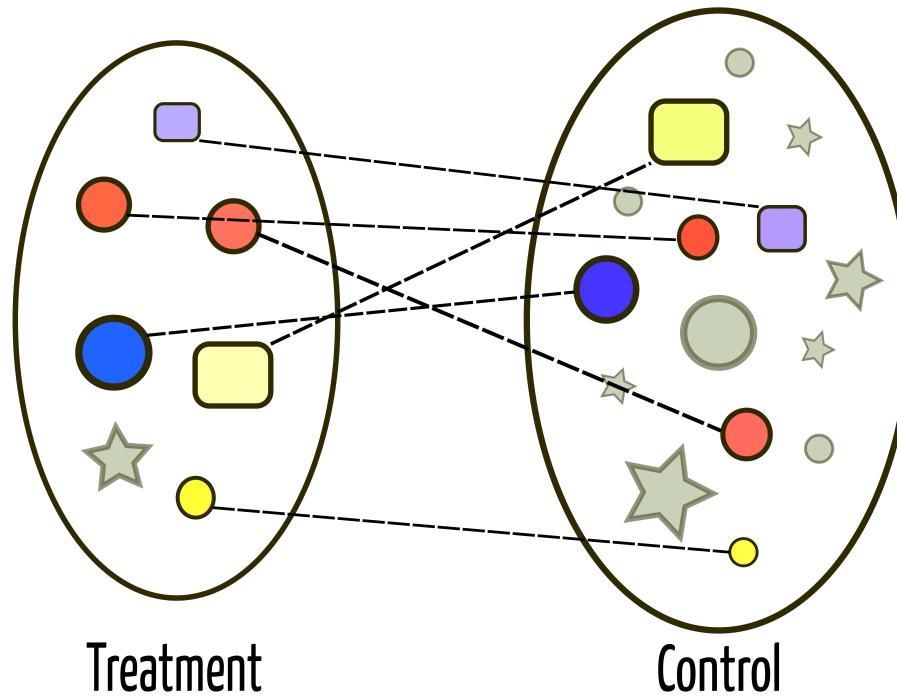
Matching

Note that we might not be able to find similar units for everyone!



Matching

The we just compare our matched groups



Propensity Score Matching

- It is **difficult (impossible)** to match on all the variables we want (potential confounders)
 - The curse of dimensionality
- **Propensity score:** Probability of being in the treatment group given the individuals characteristics.

$$p = Pr(Z = 1) = \hat{\beta}_0 + \hat{\beta}_1 X_1 + \hat{\beta}_2 X_2 + \dots + \hat{\beta}_k X_k$$

- Don't need to calculate this by hand; we will use the MatchIt package.

Let's go to R

Wrapping things up

- If the **ignorability assumption doesn't hold**, I can potentially control by all my confounders.
 - Conditional Independence Assumption.
- **Unlikely to hold**
- Do we have other alternatives?
 - Let's see next class!

