Various Design Issues

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13 June 2019

Some Additional Issues for Design

1. Review: Different types of randomization
   - Simple and complete
   - Block
   - Cluster
2. Encouragement design
   - ITT and LATE
   - One-sided non-compliance
   - Two-sided non-compliance
3. Attrition
4. Spillovers
5. Lack of symmetry between treatment and control groups

Different types of randomization

Simple Each unit is assigned treatment with m/N probability by a coin flip.

- Not guaranteed to have exactly m treated units.
- Difficult to budget for the treatment.

Complete m out of N units are assigned to treatment with known probability.

- This is what we did in our experiment yesterday.
- Easier to budget for the treatment.

Different types of randomization

Block You create subgroups, and units are randomly assigned to treatment or control within each of those subgroups.

- We often do complete randomization within each block.
- We usually like this.

Clustered The subgroups themselves are randomly assigned into treatment conditions, so all units within the same subgroup always have the same treatment assignment.

- We don't like this. We generally have it when we can't avoid it.

Different types of randomization

Block, then Clustered – Within blocks (subgroups composed of clusters), we have clustered randomization.

- Nahomi's Ghana study from yesterday's quiz – within each region, I randomize communities to treatment or control, so all members of a given community have the same treatment assignment.
- Prof. Wantchekon's Mexico study – within each municipality, voting precincts were randomized into the different treatment arms, so all households within a given precinct had the same treatment assignment.

What is the ATE of academic coaching on reading skills?

Let's say we do a survey of students, ask whether the subject has gotten academic coaching, and then test their reading skills.

- We compare the average reading skills of students who have had academic coaching (6) to the average reading skills of students who had no academic coaching (4). $6 - 4 = 2$
- Should we believe that the ATE is 2?

Probably we should be skeptical because...

The students who got coaching are probably systematically different from those who didn't in lots of other ways that suggest that they likely have different potential outcomes.

- Motivation
- Family resources
- Parental support

	avg $Y_i(1)$	avg $Y_i(0)$
Had coaching	6	5?
Not had coaching	3? 4? 5? 6?	4

Sometimes we can't directly randomize D (had coaching), the thing we're interested in learning the effect of.

Encouragement Design

In this case, we can try to randomize something else Z to **encourage** subject to have D.

- I can randomize an offer of free academic coaching (Z). The treatment assignment (Z) is binary.
- Some students offered the coaching will accept ($Z = 1, D = 1$) but others will not ($Z = 1, D = 0$). Taking the treatment (D) is also binary.
- Let's assume that coaching is not otherwise available, so if $Z = 0$, then $D = 0$ for everyone.

The idea is $Z \rightarrow D$ and $D \rightarrow Y$.

Type	$D_i(1)$	$D_i(0)$
Compliers	1	0
Never-takers	0	0

Questions: endogenous subgroups

Type	$D_i(1)$	$D_i(0)$
Compliers	1	0
Never-takers	0	0

Let's create subgroups by D. These are *endogenous* subgroups, because D is affected by Z.

1. What type(s) of students are in the $D = 1$ subgroup?
2. What type(s) of students are in the $D = 0$ subgroup?
3. Can we get the ATE of D on Y by taking the difference in the average outcomes of these two subgroups?

Don't turn your randomized study into an observational one by analyzing your data this way!

Questions: randomly-created subgroups

Type	$D_i(1)$	$D_i(0)$
Compliers	1	0
Never-takers	0	0

Now let's create subgroups by Z, as we do in a randomized experiment.

1. If we have randomized Z, what type(s) of students are in the $Z = 1$ subgroup?
2. If we have randomized Z, what type(s) of students are in the $Z = 0$ subgroup?
3. Can we get the ATE of Z on Y? This is known as the **intent to treat effect (ITT)**.

Does this mean there's no hope for getting the ATE of D on Y? Not quite...

1. We may be able to assume that Z only affects Y through D. This is known as an **exclusion restriction** or **excludability**. What does that mean in our example?

2. If this exclusion restriction holds, there is no effect for Never-Takers.

Type	$D_i(1)$	$D_i(0)$	
Compliers	1	0	$Z = D$
Never-takers	0	0	$D = 0$

3. So we know that the difference in average outcomes between the $Z = 1$ and $Z = 0$ groups is due to the effect of Z on Y for Compliers.

We're getting somewhere...

Let's try to fill this in:

Type	$D_i(1)$	$D_i(0)$	avg $Y_i(1)$	avg $Y_i(0)$	avg τ_i
Compliers ($Z = D$)	1	0	?		?
Never-takers ($D = 0$)	0	0			?

- D is taking the coaching.
- Y is reading skills.
- Inside the () is Z, assignment to treatment.

We're getting somewhere...

Type	$D_i(1)$	$D_i(0)$	avg $Y_i(1)$	avg $Y_i(0)$	avg τ_i
Compliers ($Z = D$)	1	0	6		$ATE_{complier}$
Never-takers ($D = 0$)	0	0			0

We're getting somewhere...

Type	$D_i(1)$	$D_i(0)$	avg $Y_i(1)$	avg $Y_i(0)$	avg τ_i
Compliers ($Z = D$)	1	0	6	?	$ATE_{complier}$
Never-takers ($D = 0$)	0	0	?	?	0

We're getting somewhere...

Type	$D_i(1)$	$D_i(0)$	avg $Y_i(1)$	avg $Y_i(0)$	avg τ_i
Compliers ($Z = D$)	1	0	6		$ATE_{complier}$
Never-takers ($D = 0$)	0	0			0

We're getting somewhere...

Type	$D_i(1)$	$D_i(0)$	avg $Y_i(1)$	avg $Y_i(0)$	avg τ_i
Compliers ($Z = D$)	1	0	6	6-$ATE_{complier}$	$ATE_{complier}$
Never-takers ($D = 0$)	0	0	$\bar{y}_{NT}$	$\bar{y}_{NT}$	0

Say that our study has α Compliers and $1 - \alpha$ Never-Takers, then:

- $E[Y_i(0)] = \alpha(6 - ATE_{complier}) + (1 - \alpha)\bar{y}_{NT}$
- $E[Y_i(1)] = \alpha(6) + (1 - \alpha)\bar{y}_{NT}$
- $E[Y_i(1)] - E[Y_i(0)] = \alpha ATE_{complier}$

$$ATE_{complier} = \text{ITT}/\text{proportion of Compliers}$$

We're getting very close...

$$ATE_{complier} = \text{ITT}/\text{proportion of Compliers}$$

1. We can reweight the ITT (ATE of Z on Y) by the proportion of compliers in our sample and get the ATE of Z on Y for the Compliers.
2. For Compliers, $Z = D$, so ATE of Z on Y is the same as ATE of D on Y for this group.
3. How do we get this proportion of Compliers?
4. This proportion is the ATE of Z on D. And we can get this because we randomized Z!

ATE of D on Y for Compliers (LATE)

This is known as the **local average treatment effect** or the **complier average causal effect**. This is another estimand.

$$\text{LATE} = ATE_{complier} = \text{ITT} \ / \ \text{ATE of } Z \text{ on } D$$

We can estimate this by:

- using differences in means in each part separately and then dividing,
- OR using instrumental variables regression where Z is the instrument for D.

This doesn't work very well when the ATE of Z on D is small.

DANCE BREAK!
DANCE BREAK!
DANCE BREAK!
DANCE BREAK!
DANCE BREAK!

One-sided and Two-sided Non-Compliance

Our first example had **one-sided non-compliance**. Everyone assigned to control would take the control condition, but we had issues with people assigned to treatment.

If we also have compliance problems with the $Z = 1$ group, then we have **two-sided non-compliance**. Now we have four possible types.

Type	$D_i(1)$	$D_i(0)$	
Always-Takers	1	1	$D = 1$
Compliers	1	0	$D = Z$
Never-takers	0	0	$D = 0$
Defiers	0	1	$D = 1 - Z$

Think of Defiers as teenagers.

Let's assume we have no Defiers

Type	$D_i(1)$	$D_i(0)$		Proportion
Always-Takers	1	1	$D = 1$	β
Compliers	1	0	$D = Z$	α
Never-takers	0	0	$D = 0$	$1 - \beta - \alpha$

1. If we have randomized Z, what type(s) of people are in the $Z = 1$ subgroup?
2. If we have randomized Z, what type(s) of people are in the $Z = 0$ subgroup?
3. With the exclusion restriction as before, the ITT has to be due to the Compliers and the ATE of Z on D is the proportion of Compliers. So we can get to LATE in the same way!

Keep in mind about LATE

1. You'll have to define what counts as compliance ($D = 1$) and you have to measure D for everyone.
2. You won't know exactly who is a Complier.
3. If you have different Ds, then you will have different Compliers.
4. Do you want ITT (ATE of Z on Y for your whole sample) or do you want LATE (ATE of D on Y for Compliers only)?
5. You'll generally need a larger sample size for LATE.

Attrition

Attrition is missing outcome data for units in our study.

- Records might be lost.
- You can't find survey repondents.

If treatment assignment (Z) can affect whether a unit's outcome is observed (R), then:

Type	$R_i(1)$	$R_i(0)$	
Always-Reporters	1	1	$R = 1$
If-Treated-Reporters	1	0	$R = Z$
Never-Reporters	0	0	$R = 0$
If-Not-Treated-Reporters	0	1	$R = 1 - Z$

Attrition

Type	$R_i(1)$	$R_i(0)$	
Always-Reporters	1	1	$R = 1$
If-Treated-Reporters	1	0	$R = Z$
Never-Reporters	0	0	$R = 0$
If-Not-Treated-Reporters	0	1	$R = 1 - Z$

We randomized Z but only have Y for the $R = 1$ people.

1. What type of people are in the $Z = 1$ group?
2. What type of people are in the $Z = 0$ group?

Attrition

Type	$R_i(1)$	$R_i(0)$	
Always-Reporters	1	1	$R = 1$
If-Treated-Reporters	1	0	$R = Z$
Never-Reporters	0	0	$R = 0$
If-Not-Treated-Reporters	0	1	$R = 1 - Z$

- We can get the ATE for Always-Reporters if we only have Always-Reporters ($R = 1$ always) and Never-Reporters ($R = 0$ always).
- But maybe people drop out if they're in the control group because they think they won't benefit or do well. We have a problem if we have subjects who report only when assigned to one treatment condition but not the other. Now the $Z = 1$ and $Z = 0$ groups that we have data for aren't similar.

What can we do?

1. Try to avoid the problem:
    - Blind people to their treatment status.
    - Promise to make the treatment available to the control group after the research is completed.
2. Try to diagnose whether we have the problem:
    - Do we have similar attrition rates in $Z = 1$ and $Z = 0$ groups?
    - Do the $Z = 1$ and $Z = 0$ groups have similar covariate profiles?

If we have attrition, what can we do?

1. Do intensive effort for data collection for a random sample for the $R = 0$ group.
2. Make best-case and worst-case assumptions about the missing Y, and calculate bounds for the estimate.
3. If attrition seems to be unrelated to Z, maybe ok to drop the attrited units from the study.
   - Be careful if it's something like elections results...
4. Generally, be very careful about dropping units and avoid the problem. Fixes aren't very good, or even if available, aren't cheap.

Spillovers

1. This may not be a problem if you're interested in spillovers and you design your study for it!
2. But if unit i's potential outcomes depend on other unit's treatment assignments, and we don't consider it, then we have violated one of the three core assumptions. And we have a problem.

Other lack of symmetry between Treatment and Control groups

We want to make sure that what we mean by the difference between $Z = 1$ and $Z = 0$ is actually what we want it to be.

Be careful to avoid:

- Hawthorne Effect
- Different data collection for one group than the other