Econometrics II

Lecture 4: Experiments

Konrad Burchardi

Stockholm University

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Literature

1. **”Mostly Harmless Econometrics”**, Angrist and Pischke
   Chapter 2 [introduction]; Chapter 3.2.3 [bad controls]
2. **”Causal Inference for Statistics, Social and Biomedical Sciences: An Introduction”**, Imbens and Rubin
   Chapter 4 [introduction]; Chapter 5 [Fisher inference]; Chapter 7.5 [controls]

All mistakes are mine.

Plan for Today

Unbiased Estimation

Why randomize?

$\Rightarrow$ Balanced distribution of potential outcomes.

Randomized experiment guarantees, by design, *ex ante*:

$$\mathbb{E}[Y_i(0)|D_i = 1] - \mathbb{E}[Y_i(0)|D_i = 0] = 0$$

Therefore:

$$\begin{aligned}
&\mathbb{E}[Y_i(1)|D_i = 1] - \mathbb{E}[Y_i(0)|D_i = 0] \\
&= \mathbb{E}[Y_i(1)|D_i = 1] - \mathbb{E}[Y_i(0)|D_i = 1] + \mathbb{E}[Y_i(0)|D_i = 1] - \mathbb{E}[Y_i(0)|D_i = 0] \\
&= \mathbb{E}[Y_i(1) - Y_i(0)|D_i = 1] \equiv ATT
\end{aligned}$$

and in fact:

$$= \mathbb{E}[Y_i(1) - Y_i(0)] \equiv ATE(\text{'average treatment effect'})$$

How to Randomize?

Why should we think about how to randomise?

Purely random treatment assignment:

1. Suboptimal fractions of T/C.
    1. Ex-ante random assignment: unnecessarily low **power**.
2. Imbalanced distribution of potential outcomes across T/C.
    1. Ex-post random assignment: problematic **causal inference**.
    2. Ex-ante random assignment: unnecessarily low **power**.

How should we randomize optimally then?

Set Fraction of T and C

Assignment Mechanism: Randomisation conditional on N_1 and N_0.

Under this assignment mechanism: $V(\hat{\beta}|N_0, N_1) = \frac{\sigma_1^2}{N_1} + \frac{\sigma_0^2}{N-N_1}$.

How to minimise $V(\hat{\beta})$?

With $\sigma_0^2 = \sigma_1^2$:	$N_1^* = N/2$.
With $\sigma_0^2 \neq \sigma_1^2$:	More observations for noisy outcome.
Budget & Costs:	With costs c_0 and c_1, and a fixed budget B, then $\min_{N,N_1} V(\hat{\beta})$ s.s. $(N - N_1)c_0 + N_1 c_1 \leq B$. More observations for cheap outcome.

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How should we randomize optimally then?

Plan for Today

Balance Tests: How to judge imbalances ex-post?

In practice, treatment arms are unlikely "balanced".

Imbalances in potential outcomes show up in covariates!

How to detect imbalances?

- Often t-test for each covariate shown.
  - Conceptually problematic.
  - Statistical significance is not what matters.[1]
- What is useful then?
  - Focus on **size of differences** for **covariates that impact outcome**![2]
  - Estimate size of difference using same specification as for differences in outcomes.
  - To check randomisation was implemented correctly, use omnibus test.

[1] Altman (1985) notes that such tests amount to assessing the probability of something having occurred by chance when you know that it did occur by chance. "Such a procedure is clearly absurd".

[2] Imbens and Rubin, 2015

Forcing Balance

Without further information:

Randomisation conditional on N_1 is best we can do to achieve $(Y(0), Y(1)) \perp D$ in sample.

But generally have more information:

$$\mathbb{E}[Y_i(D_i)] = f(X_i^+, X_i^-, D_i) \tag{1}$$

where X_i^+ are observable and X_i^- non-observable covariates.

Cannot force balance of potential outcomes, but...
...can try to force balance of X_i^+. Reduce $V(\hat{\beta})$.

Plan for Today

1. Unbiased Estimation
2. Balance
3. Stratification/Paired Experiments
4. Power
5. Control/Bad Control
6. Attrition
7. Canonical Experimental Designs

Stratification: Simple Example

Idea: Do not leave imbalance of important covariates to chance.

Formally: Restrict assignment mechanisms.

Example 1: Potential outcomes determines as $Y_i(D_i) = g_i + 1 \times D_i$, where $g_i \in \{1, 2\}$ is only covariate, $N = 4$, $N_1 = 2$ and $i = 1, 2$ only have $g_i = 1$. Note: true $\beta = 1$.

Assignment	d_1	d_2	d_3	d_4	$\hat{\beta}$
#1	0	1	0	1	1
#2	0	1	1	0	1
#3	1	0	0	1	1
#4	1	0	1	0	1
#5	1	1	0	0	0
#6	0	0	1	1	2

$\Rightarrow V(\hat{\beta})$ lower by excluding last two assignments! Stratification.

Stratification: Practicalities

Extends to several categorical variables. **How?**

$\Rightarrow$ In Problem Set 2 you are asked to simulate effect of stratification on the variance of the estimator.

Implementation in STATA:

```
set seed 20230323
gen random = runiform()
sort cat_var1 cat_var2 ...  random
gen treatment = mod(_n,2)
```

Stratification: Practicalities

Question 1: Which variables should we stratify on?

General recommendation: covariates strongly related to the outcome.[3] Why?

Question 2: And when covariates are continuous, or cells sparse?

[3] See Bruhn and McKenzie, 2009; Glennerster and Takavarasha, 2013.

Stratification: Implementations

What do people do? (Bruhn and McKenzie, 2009)

1. Pure Randomisation.
2. Re-Randomisation.
   - Subjectively decide whether to make another draw; or
   - Re-randomise until some statistic of balance is achieved: or
   - Choose assignment with best balance amongst N draws.
3. Matched-Pair ('blocking'): stratified randomisation with 2 units in each stratum.

   Recent insight:

   Matched-Pair designs optimal (under some conditions, in some sense).

Stratification: Matched-Pair Designs

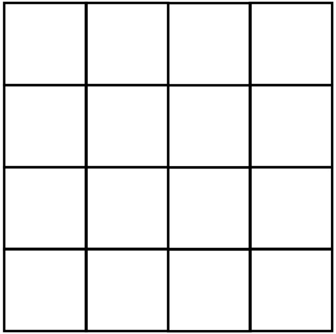
Matched-Pair Design is stratified randomisation with two units in each stratum.

1. Bai (AER, 2022):
   "Optimality of Matched-Pair Designs in Randomized Controlled Trials"
   - Shows optimality (in MSE sense) of a specific matched-pair design: Calculate $\mathbb{E}[Y_i(1)|X_i] + \mathbb{E}[Y_i(0)|X_i]$, match adjacent units on that "simple" scalar function.
   - Problem: we do not know that sum! Can be estimated using pilot data with large class of estimators, including machine learning techniques.
2. Barrios (2015) – special case:
   "Optimal Stratification in Randomized Experiments"
   - With homogeneous treatment effects, best way to choose matched-pairs is to match on $\mathbb{E}[Y_i(0)|X_i]$.
   - Baseline, but no pilot data needed.

Stratification: Taking it to the Extreme

Next level: No randomization.

Consider this example, a 'field'. *What is optimal?*



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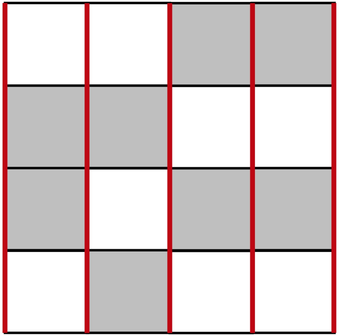

Stratification by x_1? Potentially unbalanced marginal distribution of x_2.

Stratification: Taking it to the Extreme

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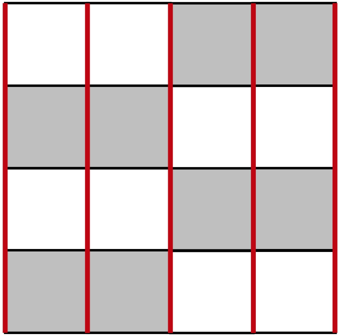

Something like this? Not maximizing power.

Stratification: Taking it to the Extreme

Next level: No randomization.

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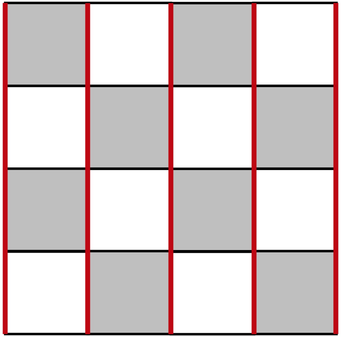

Balance joint distribution of x_1, x_2.

But only two assignments achieve that! Randomization?

Stratification: Taking it to the Extreme

Given any prior on how potential outcomes are generated...

...argument is very general! (Kasy, 2016)

- For any assignment **D**, calculate loss function (MSE, V, ...).
- Expected loss is average across potential assignments.
- Find subset of assignments such that loss is minimised.
- Typically those are two.

Controlled Trial, not Randomized Controlled Trial

- Stratification special case where subset larger than 2.

Benefits

Notice: Stratification has benefits ex-post, and ex-ante!

After Kasy: *Why do we randomize then?*

- Optimal solution might be hard to find.
- Might set up the decision problem differently.
- Randomization Inference!

Plan for Today

1. Unbiased Estimation
2. Balance
3. Stratification/Paired Experiments
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5. Control/Bad Control
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7. Canonical Experimental Designs

Power: The Concept

Talked about experimental design choices that lead to lower variance of the estimator. Closely related concept: Power.

1. Eventually want to test hypothesis H_0 vs alternative H_1.
2. Two types of errors we can make:
   1. Type I error: reject H_0 when H_0 is true.
      Probability of type I error chosen by setting α.
   2. Type II error: fail to reject H_0 when H_0 is false.
      Probability of type II error depends on true effect size, experimental set-up and estimator/test statistics..

Power: How to increase it?

How to increase power?

1. Reduce the variance of the estimator:
   - Stratification (see before).
   - Baseline Controls (see later).
   - Sample Size.[4]
   - Measurement!
2. Choice of test statistic.

[4] When starting a project, make sure it is 'powered' to detect expected effects with reasonably high probability. Funders want to see this in grant applications.

Plan for Today

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Analysis: Control Variables

- Control variables: pre-determined, observed variables.
- If simple random assignment: no need to include control to guarantee **unbiasedness**...
- ... but might make sense to increase **power**.
- If data is generated by *conditional random assignment*, need to hold variables you conditioned on in randomisation constant in the analysis. (Only *conditional* independence assumption will surely hold, will come back to this.)

Analysis: Bad Control

Control variables can not be outcomes themselves!

Mathematical reason:

$$\mathbb{E}[Y_i(0)|D_i = 1] = \mathbb{E}[Y_i(0)|D_i = 0]$$

does not imply

$$\mathbb{E}[Y_i(0)|D_i = 1, X_i = x] = \mathbb{E}[Y_i(0)|D_i = 0, X_i = x]$$

where X_i refers to the (observed) outcomes for the covariate for unit i. Such an X_i would be a bad control.

Analysis: Bad Control

Example: Think of Y as achievement, D as class size (0 if large/1 if small), and X as parental help (1 if help/0 if not). D_i assigned by classical randomized experiment.
Plausibly parents' help responds to variation in D, so X is an outcome. Define X_i as potential outcome analogous to how we defined Y_i.

Suppose 'control for X' in difference in outcomes:

$$
\begin{aligned}
&\mathbb{E}[Y_i|D_i = 1, X_i = 1] - \mathbb{E}[Y_i|D_i = 0, X_i = 1] \\
&= \mathbb{E}[Y_i(1)|D_i = 1, X_i = 1] - \mathbb{E}[Y_i(0)|D_i = 0, X_i = 1] \\
&= \mathbb{E}[Y_i(1)|X_i(1) = 1] - \mathbb{E}[Y_i(0)|X_i(0) = 1] \quad | \quad \text{by Random Assignment} \\
&= \underbrace{\mathbb{E}[Y_i(1) - Y_i(0)|X_i(1) = 1]}_{\text{'some' causal effect}} + \underbrace{\mathbb{E}[Y_i(0)|X_i(1) = 1] - \mathbb{E}[Y_i(0)|X_i(0) = 1]}_{\text{selection bias}}
\end{aligned}
$$

Analysis: Bad Control

$$\underbrace{\mathbb{E}[Y_i(1) - Y_i(0)|X_i(1) = 1]}_{\text{'some' causal effect}} + \underbrace{\mathbb{E}[Y_i(0)|X_i(1) = 1] - \mathbb{E}[Y_i(0)|X_i(0) = 1]}_{\text{selection bias}}$$

Causal effect: for population that helps when class size is small.

Compositional bias: control sample *shouls be* those that help when class size is small, but *it is* those that help when class size is large - who are probably more than those who help when size is small. Probably special group!

Same argument applies *generally* when restricting to subgroup with some outcome. Example: effect of training on wages – which are only observed for employed.

Extensive margin: easy; intensive margin: infeasible.

Analysis: Estimate only reduced form of effects

Example: Educational production function $Y = f(D, X, \theta)$. Suppose that X responds to D.

Comparing outcomes across treatment arms, we estimate:

$$\Delta Y = \frac{\partial f}{\partial D}\Delta D + \frac{\partial f}{\partial X}\frac{\partial X}{\partial D}\Delta D \tag{2}$$

- Sometimes that is what you are interested in.
- But for other questions you might need $\frac{\partial f}{\partial D}$ or $\frac{\partial f}{\partial X}$.

P. Fredriksson: “One reason for the slight dismay in certain quarters over the experimental approach.”

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Attrition: Practical Advise

Attrition is common in experiments:

cannot obtain follow-up data for some treated and some un-treated observations.

- Generally good to avoid!
- Unproblematic when attrition is unrelated to potential outcomes.

  Unfortunately impossible to know!
- Commonly accepted test: attrition rates unrelated to treatment.

  Might still be that attrition is selected differently in T and C.
- When attrition rates differ across T and C:

  Some bounding exercise, commonly Lee (2009) bounds.

Attrition: Lee (2009) Bounds

Suppose the attrition rate is higher in C than in T.

Worry: Those with 'best' $Y_i(0)$ cannot be observed in Control. Comparison of T with C exaggerates ATE.

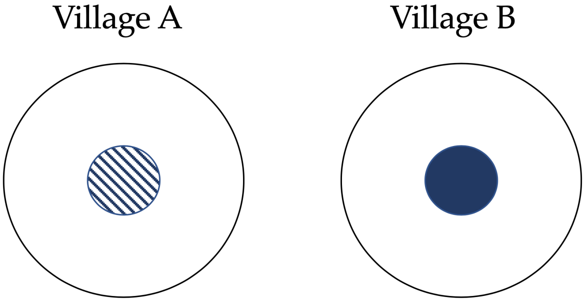
Lee Bounds are an extreme bounding exercise:

- Drop the 'best' from T, s.t. T and C have same attr. rate, and rerun analysis.
- Drop the 'worst' from T, s.t. T and C have same attr. rate, and rerun analysis.
- Report results from both exercises as bounds.

Plan for Today

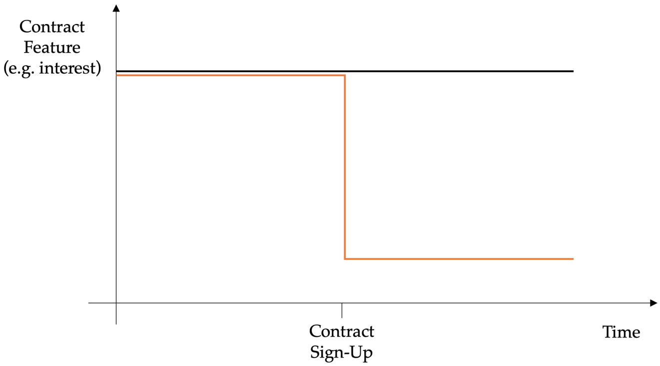
Spill-Overs: *Miguel and Kremer* (2004)

Suppose you believe your treatment might affect other units, i.e. has **spill-over effects.** *How can you estimate those?*



Moral Hazard and Adverse Selection: *Karlan and Zinman* (2009)

Distinguishing Moral Hazard and Adverse Selection *empirically* is generally hard.
Karlan and Zinman figured out one way:



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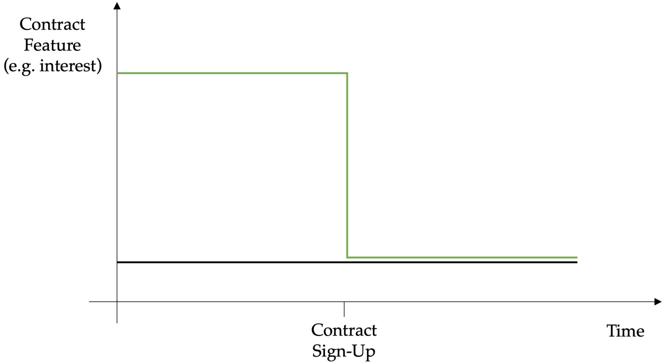

Randomized Experiments

- Experiments important to understand on their own right.

  Today just a small introduction.
- Modern econometric approaches approximate that ideal.
- The experimental ideal will often be very useful to think in *observational studies* whether you estimate causal effects.
- Issues we discussed will come up, more or less explicitly: assignment mechanism, conditional independence, balance, biases, reduced form estimation, average treatment effects for subpopulations...

Questions?

References

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