MY457/MY557: Causal Inference for Observational and Experimental Studies

Week 2: Randomized Experiments

Daniel de Kadt

Department of Methodology
LSE

Winter Term 2024

Lecture Roadmap

1. The experimental ideal
2. The 'magic' of randomization
3. Estimation
4. Inference
5. Example experiment: JTPA
6. Beyond simple randomized experiments

(Randomized) Experiments

Definition (Randomized Experiment)

An experiment is a research design where the assignment mechanism is individualistic, probabilistic, uncounfounded, and controlled by the researcher.

In a (classical) randomized experiment ('randomized controlled trial' or RCT) treatment values are assigned to N units at random, with known and positive assignment probabilities for each treatment to each unit.

We consider the 'completely randomized experiment': a random subset of N_1 units assigned to treatment ($D = 1$) and remaining $N_0 = N - N_1$ to control.

- Note the slight difference to simple randomization (Bernoulli trials).
- Extension to cases with more than two treatment levels is reasonably straightforward.
- Other randomized designs are introduced briefly at the end of this lecture.

"It's an illusion, Michael"

The Problem

Recall our basic problem:

$$E[Y|D=1] - E[Y|D=0] = E[Y_1|D=1] - E[Y_0|D=0]$$
$$= \underbrace{E[Y_1|D=1] - E[Y_0|D=1]}_{\text{ATT}} + \underbrace{\{E[Y_0|D=1] - E[Y_0|D=0]\}}_{\text{Selection bias}}$$

Randomization

Our goal is to find conditions under which we can identify our unobservable causal estimand with only observational data.

Randomization implies that assignment probabilities do not depend on potential outcomes (in expectation):

$$P(D|Y_0, Y_1) \quad = \quad P(D)$$

Or, said another way:

$$(Y_1, Y_0) \perp\!\!\!\perp D$$

(Note: $\perp\!\!\!\perp$ means "is independent of".)

To check understanding, does randomization imply $Y \perp\!\!\!\perp D$? No!

$(Y_1, Y_0) \perp\!\!\!\perp D$ means that (in expectation) Y_0 is the same for those with $D = 1$ and for those with $D = 0$ (and similarly for Y_1), says nothing about equivalence of Y between these groups.

Randomization Eliminates Selection Bias

Back to the problem at hand:

$$E[Y|D=1] - E[Y|D=0] = E[Y_1|D=1] - E[Y_0|D=0]$$
$$= \underbrace{E[Y_1|D=1] - E[Y_0|D=1]}_{\text{ATT}} + \underbrace{\{E[Y_0|D=1] - E[Y_0|D=0]\}}_{\text{Selection bias}}$$

Under independence from randomized treatment assignment, we have

$$E[Y_0|D=1] = E[Y_0|D=0] = E[Y_0]$$

thus selection bias equals zero (in expectation).

We also have $E[Y_1|D=1] = E[Y_1|D=0] = E[Y_1]$, thus

$$\tau_{ATT} = E[Y_1|D=1] - E[Y_0|D=1] = E[Y_1] - E[Y_0] = \tau_{ATE}$$

Randomization: Key Identification Result

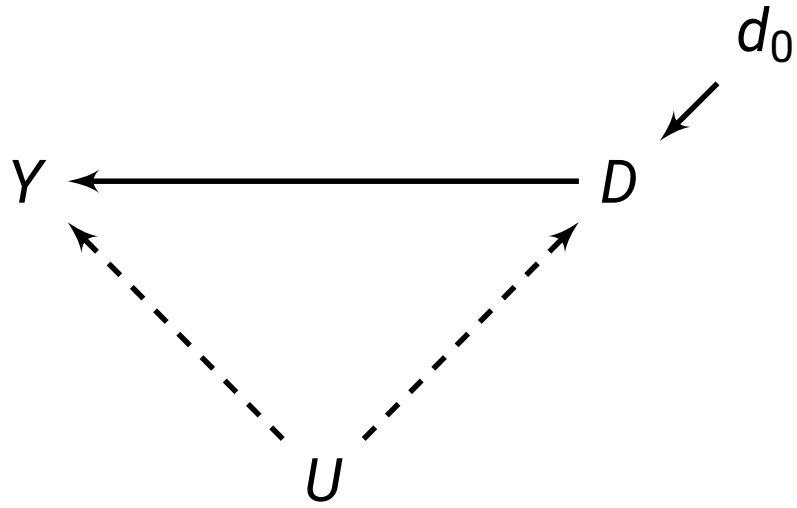
Independence implied by complete randomization gives us:

$$E[Y|D = 1] - E[Y|D = 0] = E[Y_1] - E[Y_0] = \tau_{ATE}$$

The observed means difference between the treatment group identifies the causal average treatment effect ATE (as well as ATT and ATU, which both equal the ATE in this case).

Note: We can also identify most other population-level causal effects, since they are comparisons of some features of the distributions of Y_0 and Y_1 and we can now estimate both of these distributions.

Graphical Representation



Consider a setting in which $D \leftarrow U \rightarrow Y$ is a back-door path connecting D and Y through unobserved U.

This is canonical confounding with the unobserved U confounding $D \rightarrow Y$

Randomization is equivalent to imposing $do(d_0)$ or $do(d_1)$, eliminating $U \rightarrow D$

There are now no back-door paths, so $D \rightarrow Y$ is identified.

Randomization and the Balancing Property

In expectation, complete randomization balances all observed and unobserved pre-treatment characteristics between treatment and control.

Why? For units with the same probability of treatment, X_i is independent of treatment assignment $\rightsquigarrow$ the balancing property.

(Note: We will dive deeper into this next week, when we introduce propensity scores.)

In a given experimental sample, we can empirically check for balance in observed pre-treatment covariate X using so called 'balance tests' (e.g., t-tests or equivalence tests) to see if the distributions $p(X|D = 1)$ and $p(X|D = 0)$ are not meaningfully different:

- In any one sample and treatment regime we might expect some chance imbalance.
- You could 'control' for imbalanced covariates, but don't 'have' to (more later).
- Stratified randomization can guarantee exact balance in some observed X.
- Even more aggressive randomization procedures exist (e.g. pair-matching).

Complications and Limitations in Randomized Experiments

Randomization (and thus internal validity) can be complicated by:

- Missing data (e.g. dropout/attrition) – outcome is unobserved for some units in a way that is associated with D or potential outcomes.
- Non-compliance – some units receive a different treatment than the one they were assigned to.

Randomization does not help with external validity: How well do causal effects for this sample apply to broader population, or other populations?

- Can differentiate Sample ATE (SATE) from Population ATE (PATE) – randomization identifies SATE, but PATE also requires random sampling.
- Moving to a different population entirely would require other (often heroic) assumptions.

Randomized experiments can be weak in construct validity: How well do treatment and outcome in the experiment match the concept we are substantively interested in?

Estimation vs. Inference

Estimation:

- Choosing the right function to apply to our observed data.
- We can use the distributions $p(Y|D = 1)$ and $p(Y|D = 0)$ in the observed data to estimate the distributions of Y_1 and Y_0 in the population, and thus population causal effects.
- Typically quite simple and familiar methods are sufficient for experiments.

Statistical inference:

- Characterizing uncertainty around our estimates.
- Hypothesis tests and confidence intervals tend to be based on the "source of identifying variation" (i.e., what is random?)
- See the discussion in Chapters 5–8 of Imbens & Rubin for more on this, if you are interested.

Estimating ATE

$$\tau_{ATE} = E[Y_1] - E[Y_0]$$

An obvious estimator of this is the sample difference-in-means:

$$\widehat{\tau} = \bar{Y}_1 - \bar{Y}_0$$

where

$$\bar{Y}_1 = \frac{\sum Y_i \cdot D_i}{\sum D_i} = \frac{1}{N_1} \sum_{D_i=1} Y_i$$

$$\bar{Y}_0 = \frac{\sum Y_i \cdot (1 - D_i)}{\sum (1 - D_i)} = \frac{1}{N_0} \sum_{D_i=0} Y_i$$

with $N_1 = \sum_i D_i$
and $N_0 = \sum_i (1 - D_i) = N - N_1$

Have already proven that $\widehat{\tau}$ is an unbiased estimator of τ_{ATE} under randomization!

Estimating ATE: Regression

The same τ_{ATE} can also be estimated using a linear regression model

$$Y_i = \hat{\gamma} + \hat{\tau} D_i + \hat{\varepsilon}_i$$

(Recall: $\hat{\tau}$ from a bivariate regression with a binary independent variable is equivalent to the diff-in-means.)

It is not necessary to include covariates X in this model. Why?

But pre-treatment covariates are sometimes included:

- Can increase precision (reduce standard error) by modeling residual variation in Y
- Control for observable imbalance (generated by random chance)
- Allow for estimation of heterogeneous treatment effects by X (by including interactions in the model)
- There is a risk of inducing small-sample bias (Freedman, 2008) – more in a few weeks when we introduce the 'fully-interacted estimator' (Lin, 2013)
- Note: do not include post-treatment covariates. (Montgomery et al., 2018; Cinelli et al., 2022)

Two Sample T-Test for Inference

From statistical theory, we know that under H_0: $\tau_{ATE} = 0$,

$$t = \frac{\widehat{\tau}}{\sqrt{\frac{\widehat{\sigma}_1^2}{N_1} + \frac{\widehat{\sigma}_0^2}{N_0}}} \xrightarrow{d} N(0,1),$$

where $\widehat{\sigma}_d^2 = \sum_{D_i=d}(Y_i - \bar{Y}_d)^2/N_d$ for $d \in \{0,1\}$.

We reject the null hypothesis H_0: $\tau_{ATE} = 0$ against the alternative H_1: $\tau_{ATE} \neq 0$ at the asymptotic 5% significance level if $|t| > 1.96$.

Randomization Inference

For the two-sample t-test, the null hypothesis was that the average treatment effect τ_{ATE} is zero, i.e.

$$H_0 : E[Y_1] = E[Y_0], \quad H_A : E[Y_1] \neq E[Y_0]$$

Consider now instead the sharp null hypothesis (and alternative)

$$H_0^s : Y_1 = Y_0, \quad H_A^s : Y_1 \neq Y_0$$

i.e. that all individual causal effects are zero.

Assuming H_0^s, then $Y_i = Y_{0i} = Y_{1i}$ for every unit. We can thus construct the full population distributions of Y_{0i} and Y_{1i}, under the null hypothesis!

Why? Under the sharp null the observed data Y_i for every unit would have been exactly the same, no matter the value of D_i

This is called randomization inference, permutation test, or Fisher's exact test

Randomization Inference

Procedure for randomization inference with complete randomization:

1. **Permute** the values of D_i (N_1 1s and N_0 0s) differently across the N units, keeping Y_i unchanged.
2. Calculate and store the value of $\widehat{\tau}_j$ (or any other appropriate statistic, such as the t-test statistic) for each of these permuted datasets j.
3. Calculate p-value as the proportion of $\widehat{\tau}_j$ that are as or more extreme than the actually observed $\widehat{\tau}$

With small N, we can consider *all* the permutations of D_i

- There are $\binom{N}{N_1} = N!/(N_1!N_0!)$ of them
- With larger N, use a random sample of all the permutations

Randomization Inference Example

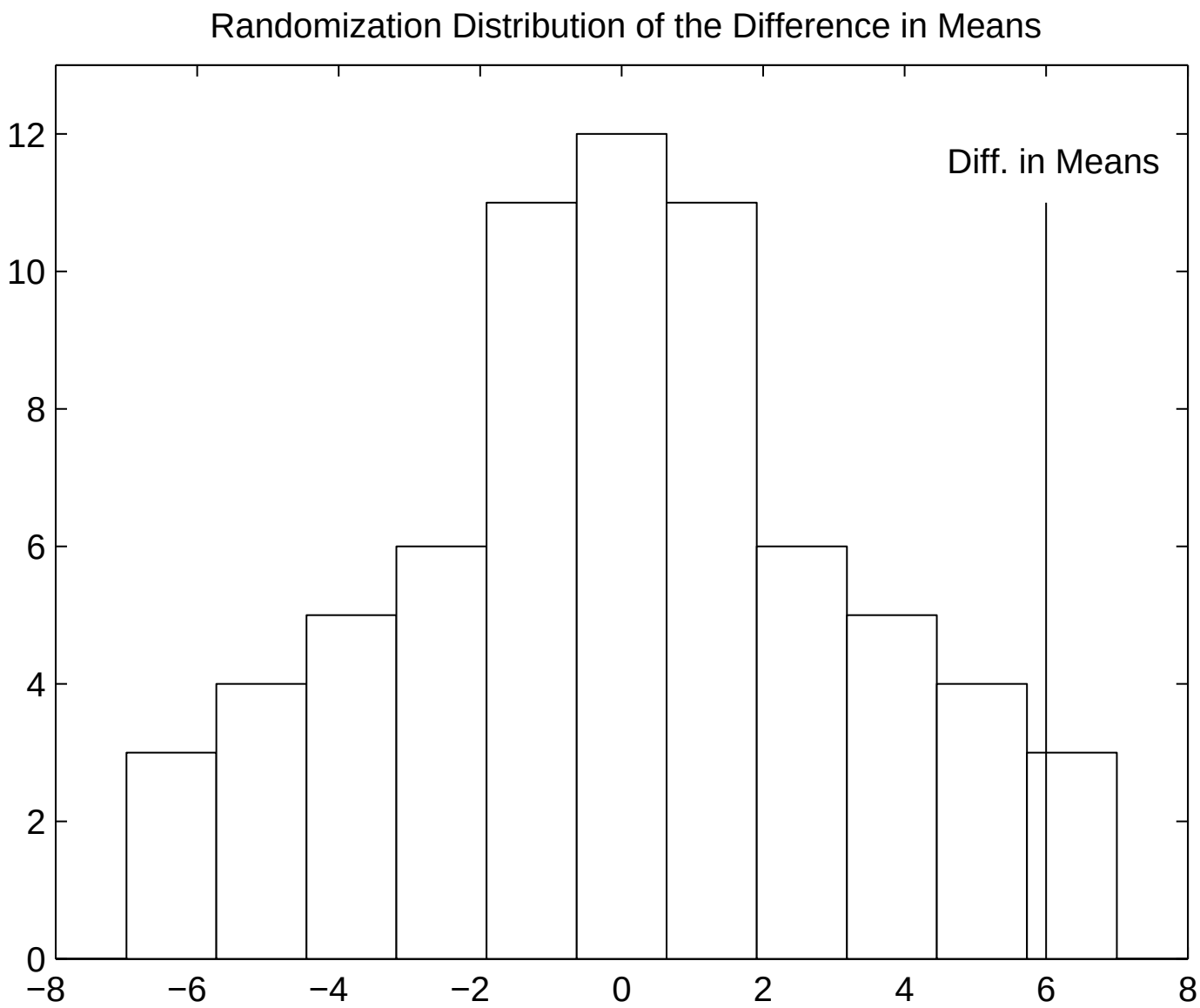
Consider an experiment with 8 units, 4 randomly assigned to treatment.

We can permute all $\binom{8}{4} = 70$ possible assignments.

We can then calculate the sample mean differences that would have been obtained for each of them if the sharp null hypothesis were true.

Y_i	12	4	6	10	6	0	1	1	
D_i	1	1	1	1	0	0	0	0	$\hat{\tau} = 6$
									$\hat{\tau}_j$
$j = 1$	1	1	1	1	0	0	0	0	6
$j = 2$	1	1	1	0	1	0	0	0	4
$j = 3$	1	1	1	0	0	1	0	0	1
$j = 4$	1	1	1	0	0	0	1	0	1.5
				...					
$j = 70$	0	0	0	0	1	1	1	1	-6

Randomization Inference Example



$$p = \Pr(|\hat{\tau}_j| \geq 6) = 0.0857$$

The Bootstrap

Another common method for uncertainty estimation is bootstrapping

The basic idea: Simulate the sampling distribution of a statistic via resampling with replacement

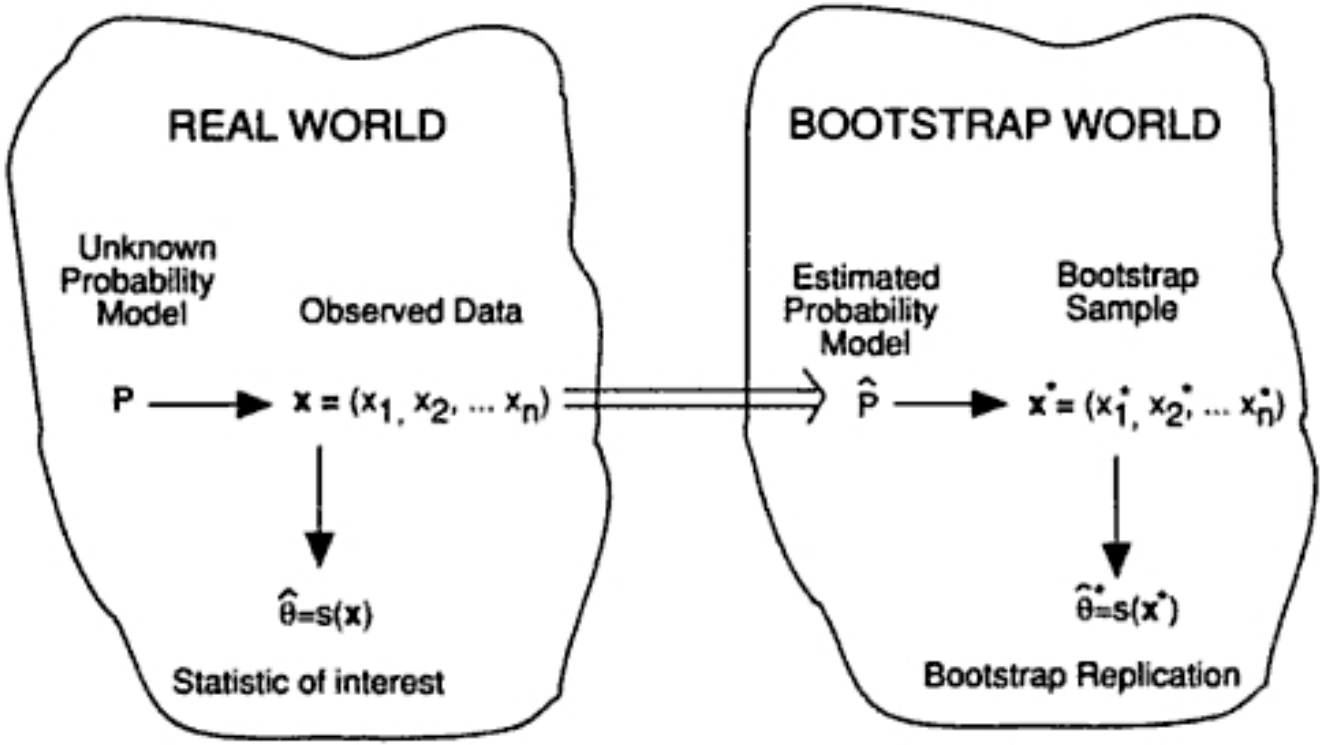
Useful when:

- Statistic is so complicated that analytically deriving its sampling variance is too difficult or cumbersome
- Data are so skewed that inference based on asymptotic normality is unlikely to perform well
- Statistic is of a form that makes CLT kick in only slowly, so normal approximation does not work well

Weakness: Computationally costly, sometimes prohibitively so

Not a general solution for small samples (a common misunderstanding!)

Bootstrap World



Goal: Estimate uncertainty in $\hat{\theta}$ (any statistic or parameter of interest) without making any assumption about P

Idea: If n is sufficiently large, the sample X should be a good approximation of P

$\longrightarrow$ Think of X as an estimated population probability model P,and just like X is a realization from P, let's draw a resample X^* from X

Nonparametric Bootstrap and Parametric Bootstrap

Nonparametric bootstrap:

1. Draw B resamples of size n from X **with replacement**
2. For each X_b^*, compute $\hat{\theta}_b^*$, where $b = 1, ..., B$

3a. To estimate s.e. of $\hat{\theta}$, use the sample standard deviation of $\hat{\theta}^* = \{\hat{\theta}_1^*, ..., \hat{\theta}_B^*\}$ (bootstrap standard errors)

3b. To compute 95% CI, use 2.5/97.5 percentiles of $\hat{\theta}^* = \{\hat{\theta}_1^*, ..., \hat{\theta}_B^*\}$ as the lower/upper bounds (bootstrap percentile CI)

3c. If you know that $\hat{\theta} \overset{\text{approx.}}{\sim} \text{N}$, you can use 3a. and compute the bootstrap normal CI

Not only can you do this without any assumption about P, you can use this for any function of data $\hat{\theta} = f(X)$

Block bootstrap: When observations are clustered, resample clusters with replacement instead of individual units

Google images result for “stock photo of people upskilling in a business setting”

Example: Job Training Partnership Act (JTPA)

Largest randomized training evaluation ever undertaken in the U.S.; started in 1983 at 649 sites throughout the country

Sample: "Underskilled" and "economically disadvantaged" persons in the labor market (previously unemployed or low earnings)

D: (Invitation) to one of three general service strategies:

- classroom training in occupational skills
- on-the-job training and/or job search assistance
- other services (eg. probationary employment)

Y: earnings 30 month following assignment

X: Characteristics measured before assignment (age, gender, previous earnings, race, etc.)

Means and Standard Deviations for JTPA Experiment

	Entire Sample	Assignment		Difference (t-stat.)
		Treatment	Control	
A. Men				
Number of observations	5,102	3,399	1,703	
Treatment				
Training	.42	.62	.01	.61
	[.49]	[.48]	[.11]	(70.34)
Outcome variable				
30 month earnings	19,147	19,520	18,404	1,116
	[19,540]	[19,912]	[18,760]	(1.96)
Baseline Characteristics				
Age	32.91	32.85	33.04	-.19
	[9.46]	[9.46]	[9.45]	(-.67)
High school or GED	.69	.69	.69	-.00
	[.45]	[.45]	[.45]	(-.12)
Married	.35	.36	.34	.02
	[.47]	[.47]	[.46]	(1.64)
Black	.25	.25	.25	.00
	[.44]	[.44]	[.44]	(.04)
Hispanic	.10	.10	.09	.01
	[.30]	[.30]	[.29]	(.70)
Worked less than 13 weeks in past year	.40	.40	.40	.00
	[.47]	[.47]	[.47]	(.56)

JTPA Experiment: Estimated effects separately by group

Exhibit 5 *Impacts on Total 30-Month Earnings: Assignees and Enrollees, by Target Group*

	Mean earnings		*Impact per assignee*		
	Treatment group (1)	*Control group (2)*	*In dollars (3)*	*As a percent of (2)*	*Impact per enrollee in dollars*
Adult women	$ 13,417	$ 12,241	$ 1,176***	9.6%	$ 1,837***
Adult men	19,474	18,496	978*	5.3	1,599*
Female youths	10,241	10,106	135	1.3	210
Male youth non-arrestees	15,786	16,375	-589	-3.6	-868
Male youth arrestees					
Using survey data	14,633	18,842	-4,209**	-22.3	-6,804**
Using scaled UI data	14,148	14,152	-4	0.0	-6

Some Other Randomization Schemes

The completely randomized design is only one option:

- Stratified (conditional, blocked) randomized experiment are randomized separately within levels of some covariate(s) X
  - e.g. separately for men and women
  - An extreme version is a *pairwise randomized experiment*: Each stratum (block) contains 2 units, one assigned to treatment, the other to control.
  - Stratification will be very important from next week, when we move on to observational assignment mechanisms.
- Cluster randomized experiments randomize units in clusters. Every unit within a cluster gets the same treatment level.
  - e.g. randomizing whole villages of people or whole classrooms of pupils.
- Cross-over experiments have units switch treatment status over time.
  - e.g. varying treatments for sick patients over time