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Randomization inference and causal inference

- “In randomization-based inference, uncertainty in estimates arises naturally from the random assignment of the treatments, rather than from hypothesized sampling from a large population.” (Athey and Imbens 2017)
- Athey and Imbens is part of growing trend of economists using randomization-based methods for doing causal inference

Lady tasting tea experiment

- Ronald Aylmer Fisher (1890-1962)
  - Two classic books on statistics: *Statistical Methods for Research Workers* (1925) and *The Design of Experiments* (1935), as well as a famous work in genetics, *The Genetical Theory of Natural Science*
  - Developed many fundamental notions of modern statistics including the theory of randomized experimental design.

Lady tasting tea

- Muriel Bristol (1888-1950)
    - A PhD scientist back in the days when women weren't PhD scientists
    - Worked with Fisher at the Rothamsted Experiment Station (which she established) in 1919
    - During afternoon tea, Muriel claimed she could tell from taste whether the milk was added to the cup before or after the tea
    - Scientists were incredulous, but Fisher was inspired by her strong claim
    - He devised a way to test her claim which she passed using randomization inference

Description of the tea-tasting experiment

- Original claim: Given a cup of tea with milk, Bristol claims she can discriminate the order in which the milk and tea were added to the cup
- Experiment: To test her claim, Fisher prepares 8 cups of tea – 4 **milk then tea** and 4 **tea then milk** – and presents each cup to Bristol for a taste test
- Question: How many cups must Bristol correctly identify to convince us of her unusual ability to identify the order in which the milk was poured?
- Fisher's sharp null: Assume she can't discriminate. Then what's the likelihood that random chance was responsible for her answers?

Choosing subsets

- The lady performs the experiment by selecting 4 cups, say, the ones she claims to have had the tea poured first.

$$\binom{n}{k} = \frac{n!}{k!\,(n-k)!}$$

- "8 choose 4" – $\binom{8}{4}$ – ways to choose 4 cups out of 8
  - Numerator is $8 \times 7 \times 6 \times 5 = 1,680$ ways to choose a first cup, a second cup, a third cup, and a fourth cup, in order.
  - Denominator is $4 \times 3 \times 2 \times 1 = 24$ ways to order 4 cups.

Choosing subsets

- There are 70 ways to choose 4 cups out of 8, and therefore a 1.4% probability of producing the correct answer by chance

$$\frac{24}{1680} = 1/70 = 0.014.$$

- For example, the probability that she would correctly identify all 4 cups is $\frac{1}{70}$

Statistical significance

- Suppose the lady correctly identifies all 4 cups. Then ...
  1. Either she has no ability, and has chosen the correct 4 cups purely by chance, or
  2. She has the discriminatory ability she claims.
- Since choosing correctly is highly unlikely in the first case (one chance in 70), the second seems plausible.
  1. Fisher is the originator of the convention that a result is considered "statistically significant" if the probability of its occurrence by chance is < 0.05, or, less than 1 out of 20.
- Bristol actually got all four correct

Replication

Let's look at tea.do and tea.R to see this experiment

Null hypothesis

- In this example, the null hypothesis is the hypothesis that the lady has no special ability to discriminate between the cups of tea.
- We can never prove the null hypothesis, but the data may provide evidence to reject it.
- In most situations, rejecting the null hypothesis is what we hope to do.

Null hypothesis of no effect

- Randomization inference allows us to make probability calculations revealing whether the treatment assignment was “unusual”
- Fisher’s sharp null is when entertain the possibility that no unit has a treatment effect
- This allows us to make “exact” p-values which do not depend on large sample approximations
- It also means the inference is not dependent on any particular distribution (e.g., Gaussian); sometimes called nonparametric

Sidebar: bootstrapping is different

- Sometimes people confuse randomization inference with bootstrapping
- Bootstrapping randomly draws a percent of the total observations for estimation; “uncertainty over the sample”
- Randomization inference randomly reassigns the treatment; “uncertainty over treatment assignment”

6-step guide to randomization inference

1. Choose a sharp null hypothesis (e.g., no treatment effects)
2. Calculate a test statistic (T is a scalar based on D and Y)
3. Then pick a randomized treatment vector $\tilde{D}_1$
4. Calculate the test statistic associated with $(\tilde{D}, Y)$
5. Repeat steps 3 and 4 for all possible combinations to get $\tilde{T} = \{\tilde{T}_1, \dots, \tilde{T}_K\}$
6. Calculate exact p-value as $p = \frac{1}{K}\sum_{k=1}^{K} I(\tilde{T}_k \geq T)$

Pretend experiment

Table: Pretend DBT intervention for some homeless population

Name	D	Y	Y^0	Y^1
Andy	1	10	.	10
Ben	1	5	.	5
Chad	1	16	.	16
Daniel	1	3	.	3
Edith	0	5	5	.
Frank	0	7	7	.
George	0	8	8	.
Hank	0	10	10	.

For concreteness, assume a program where we pay homeless people $15 to take dialectical behavioral therapy. Outcomes are some measure of mental health 0-20 with higher scores being better.

Step 1: Sharp null of no effect

Fisher's Sharp Null Hypothesis

$H_0 : \delta_i = Y_i^1 - Y_i^0 = 0 \; \forall i$

- Assuming no effect means any test statistic is due to chance
- Neyman and Fisher test statistics were different – Fisher was exact, Neyman was not
- Neyman's null was no average treatment effect (ATE=0). If you have a treatment effect of 5 and I have a treatment effect of -5, our ATE is zero. This is not the sharp null even though it also implies a zero ATE

More sharp null

- Since under the Fisher sharp null $\delta_i = 0$, it means each unit's potential outcomes under both states of the world are the same
- We therefore know each unit's missing counterfactual
- The randomization we will perform will cycle through all treatment assignments under a null well treatment assignment doesn't matter because all treatment assignments are associated with a null of zero unit treatment effects
- We are looking for evidence *against* the null

Step 1: Fisher's sharp null and missing potential outcomes

Table: Missing potential outcomes are no longer missing

Name	D	Y	Y^0	Y^1
Andy	1	10	**10**	10
Ben	1	5	**5**	5
Chad	1	16	**16**	16
Daniel	1	3	**3**	3
Edith	0	5	5	**5**
Frank	0	7	7	**7**
George	0	8	8	**8**
Hank	0	10	10	**10**

Fisher sharp null allows us to fill in the missing counterfactuals bc under the null there's zero treatment effect at the unit level. This guarantees zero ATE, but is different in formulation than Neyman's null effect of no ATE.

Step 2: Choosing a test statistic

Test Statistic

A test statistic $T(D, Y)$ is a scalar quantity calculated from the treatment assignments D and the observed outcomes Y

- By scalar, I just mean it's a number (vs. a function) measuring some relationship between D and Y
- Ultimately there are many tests to choose from; I'll review a few later
- If you want a test statistic with high statistical power, you need large values when the null is false, and small values when the null is true (i.e., *extreme*)

Simple difference in means

- Consider the absolute SDO from earlier

$$\delta_{SDO} = \left| \frac{1}{N_T} \sum_{i=1}^{N} D_i Y_i - \frac{1}{N_C} \sum_{i=1}^{N} (1 - D_i) Y_i \right|$$

- Larger values of δ_{SDO} are evidence *against* the sharp null
- Good estimator for constant, additive treatment effects and relatively few outliers in the potential outcomes

Step 2: Calculate test statistic, $T(D, Y)$

Table: Calculate T using D and Y

Name	D	Y	Y^0	Y^1	δ_i
Andy	**1**	**10**	10	10	0
Ben	**1**	**5**	5	5	0
Chad	**1**	**16**	16	16	0
Daniel	**1**	**3**	3	3	0
Edith	**0**	**5**	5	5	0
Frank	**0**	**7**	7	7	0
George	**0**	**8**	8	8	0
Hank	**0**	**10**	10	10	0

We'll start with this simple the simple difference in means test statistic, $T(D, Y)$: $\delta_{SDO} = 34/4 - 30/4 = 1$

Steps 3-5: Null randomization distribution

- Randomization steps reassign treatment assignment for every combination, calculating test statistics each time, to obtain the entire distribution of counterfactual test statistics
- The key insight of randomization inference is that under Fisher's sharp null, the treatment assignment shouldn't matter
- Ask yourself:
    - if there is no unit level treatment effect, can you picture a distribution of counterfactual test statistics?
    - and if there is no unit level treatment effect, what must average counterfactual test statistics equal?

Step 6: Calculate “exact” p-values

- Question: how often would we get a test statistic as big or bigger as our “real” one if Fisher’s sharp null was true?
- This can be calculated “easily” (sometimes) once we have the randomization distribution from steps 3-5
    - The number of test statistics ($t(D, Y)$) bigger than the observed divided by total number of randomizations

$$Pr(T(D,Y) \geq T(\tilde{D}, Y|\delta = 0)) = \frac{\sum_{D \in \Omega} I(T(D,Y) \leq T(\tilde{D}, Y)}{K}$$

Approximate p-values

These have been “exact” tests when they use every possible combination of D

- When you can’t use every combination, then you can get *approximate* p-values from a simulation (TBD)
- With a rejection threshold of α (e.g., 0.05), randomization inference test will falsely reject less than $100 \times \alpha\%$ of the time

First permutation (holding N_T fixed)

Name	$\tilde{D}_2$	Y	Y^0	Y^1
Andy	1	10	10	10
Ben	0	5	5	5
Chad	1	16	16	16
Daniel	1	3	3	3
Edith	0	5	5	5
Frank	1	7	7	7
George	0	8	8	8
Hank	0	10	10	10

$$\tilde{T}_1 = |36/4 - 28/4| = 9 - 7 = 2$$

Second permutation (again holding N_T fixed)

Name	$\tilde{D}_3$	Y	Y^0	Y^1
Andy	1	10	10	10
Ben	0	5	5	5
Chad	1	16	16	16
Daniel	1	3	3	3
Edith	0	5	5	5
Frank	0	7	7	7
George	1	8	8	8
Hank	0	10	10	10

$$T_{rank} = |36/4 - 27/4| = 9 - 6.75 = 2.25$$

Sidebar: Should it be 4 treatment groups each time?

- In this experiment, I've been using the same N_T *under the assumption* that N_T had been fixed when the experiment was drawn.
- But if the original treatment assignment had been generated by something like a Bernoulli distribution (e.g., coin flips over every unit), then you should be doing a complete permutation that is also random in this way
- This means that for 8 units, sometimes you'd have 1 treated, or even 8
- Correct inference requires you know the original data generating process

Randomization distribution

Assignment	D_1	D_2	D_3	D_4	D_5	D_6	D_7	D_8	$\lvert T_i \rvert$
True D	1	1	1	1	0	0	0	0	1
$\tilde{D}_2$	1	0	1	1	0	1	0	0	2
$\tilde{D}_3$	1	0	1	1	0	0	1	0	2.25
...									

Step 2: Other test statistics

- The simple difference in means is fine when effects are additive, and there are few outliers in the data
- But outliers create more variation in the randomization distribution
- What are some alternative test statistics?

Transformations

- What if there was a constant multiplicative effect: $Y_i^1/Y_i^0 = C$?
- Difference in means will have low power to detect this alternative hypothesis
- So we transform the observed outcome using the natural log:

$$T_{log} = \left| \frac{1}{N_T} \sum_{i=1}^{N} D_i ln(Y_i) - \frac{1}{N_C} \sum_{i=1}^{N} (1 - D_i) ln(Y_i) \right|$$

- This is useful for skewed distributions of outcomes

Difference in medians/quantiles

- We can protect against outliers using other test statistics such as the difference in quantiles
- Difference in medians:

$$T_{median} = |median(Y_T) - median(Y_C)|$$

- We could also estimate the difference in quantiles at any point in the distribution (e.g., 25th or 75th quantile)

Rank test statistics

- Basic idea is rank the outcomes (higher values of Y_i are assigned higher ranks)
- Then calculate a test statistic based on the transformed ranked outcome (e.g., mean rank)
- Useful with continuous outcomes, small datasets and/or many outliers

Rank statistics formally

- Rank is the domination of others (including oneself):

$$\tilde{R} = \tilde{R}_i(Y_1, \ldots, Y_N) = \sum_{j=1}^{N} I(Y_j \leq Y_i)$$

- Normalize the ranks to have mean 0

$$\tilde{R}_i = \tilde{R}_i(Y_1, \ldots, Y_N) = \sum_{j=1}^{N} I(Y_j \leq Y_i) - \frac{N+1}{2}$$

- Calculate the absolute difference in average ranks:

$$T_{rank} = |\overline{R}_T - \overline{R}_C| = \left| \frac{\sum_{i:D_i=1} R_i}{N_T} - \frac{\sum_{i:D_i=0} R_i}{N_C} \right|$$

- Minor adjustment (averages) for ties

Randomization distribution

Name	D	Y	Y^0	Y^1	Rank	R_i
Andy	1	10	**10**	10	6.5	2
Ben	1	5	**5**	5	2.5	-2
Chad	1	16	**16**	16	8	3.5
Daniel	1	3	**3**	3	1	-3.5
Edith	0	5	5	**5**	2.5	-1
Frank	0	7	7	**7**	4	-0.5
George	0	8	8	**8**	5	0.5
Hank	0	10	10	**10**	6.5	2

$$T_{rank} = |0 - 1/4| = 1/4$$

Effects on outcome distributions

- Focused so far on "average" differences between groups.
- Kolmogorov-Smirnov test statistics is based on the difference in the distribution of outcomes
- Empirical cumulative distribution function (eCDF):

$$\begin{aligned} \widehat{F}_C(Y) &= \frac{1}{N_C} \sum_{i:D_i=0} 1(Y_i \leq Y) \\ \widehat{F}_T(Y) &= \frac{1}{N_T} \sum_{i:D_i=1} 1(Y_i \leq Y) \end{aligned}$$

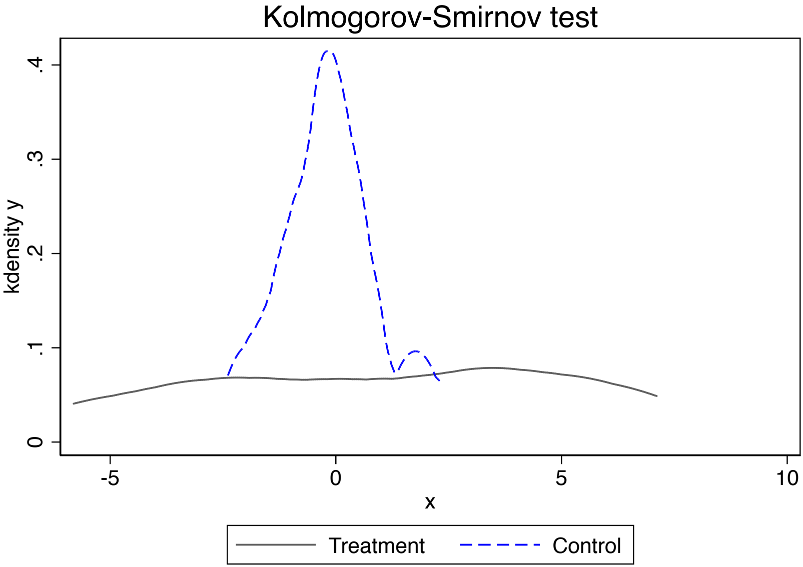
- Proportion of observed outcomes below a chosen value for treated and control separately
- If two distributions are the same, then $\widehat{F}_C(Y) = \widehat{F}_T(Y)$

Kolmogorov-Smirnov statistic

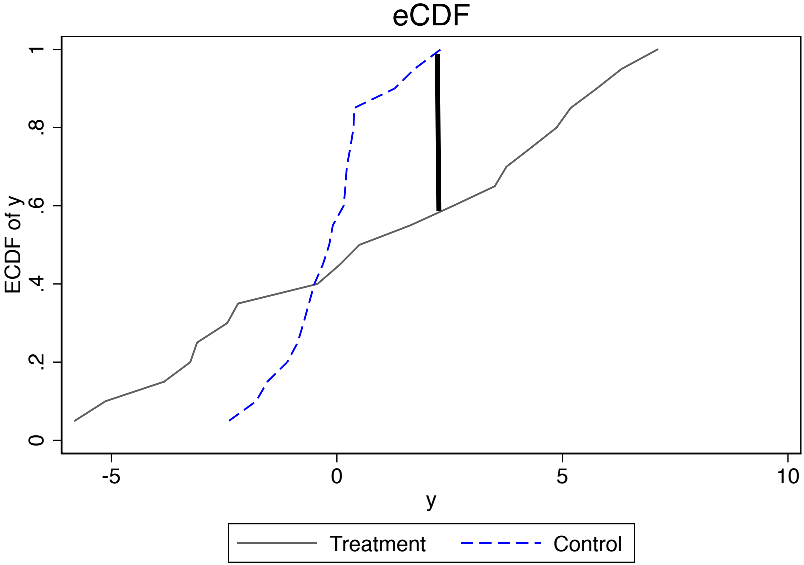
- Test statistics are scalars not functions
- eCDFs are functions, not scalars
- Solution: use the maximum discrepancy between the two eCDFs:

$$T_{KS} = max|\widehat{F}_T(Y_i) - \widehat{F}_C(Y_i)|$$

Kernel density by group status



eCDFs by treatment status and test statistic



KS Test Statistic

Treatment	D	Exact P-value
K-S	0.4500	0.034

Max distance is 0.45. Exact *p* is 0.034.

“Which bear is best?” – Jim Halpert

A good test statistic is the one that best fits your data. Some test statistics will have weird properties in the randomization as we’ll see in synthetic control.

One-sided or two-sided?

- So far, we have defined all test statistics as absolute values
- We are testing against a two-sided alternative hypothesis

$$
\begin{aligned}
H_0 &: \delta_i = 0 \ \forall i \\
H_1 &: \delta_i \neq 0 \ \texttt{for some} \ i
\end{aligned}
$$

- What about a one-sided alternative

$$
\begin{aligned}
H_0 &: \delta_i = 0 \ \forall i \\
H_1 &: \delta_i > 0 \ \texttt{for some} \ i
\end{aligned}
$$

- For these, use a test statistic that is bigger under the alternative:

$$T_{diff} * = \overline{Y}_T - \overline{Y}_C$$

Small vs. Modest Sample Sizes are non-trivial

Computing the exact randomization distribution is not always feasible (Wolfram Alpha)

- $N = 6$ and $N_T = 3$ gives us 20 assignment vectors
- $N = 8$ and $N_T = 4$ gives us 70 assignment vectors
- $N = 10$ and $N_T = 5$ gives us 252 assignment vectors
- $N = 20$ and $N_T = 10$ gives us 184,756 assignment vectors
- $N = 50$ and $N_T = 25$ gives us 1.2641061×10^{14} assignment vectors

Exact p calculations are not realistic bc the number of assignments explodes at even modest size

Approximate p values

- Use simulation to get approximate p-values
  - Take K samples from the treatment assignment space
  - Calculate the randomization distribution in the K samples
  - Tests no longer exact, but bias is under your control (increase K)
- Imbens and Rubin show that p values converge to stable p values pretty quickly (in their example after 1000 replications)

Sample dataset

Let's do this now with Thornton's data. You can replicate that using thorton_ri.do or thornton_ri.R

Thornton's experiment

ATE	Iteration	Rank	p	no. trials
0.45	1	1	0.01	100
0.45	1	1	0.002	500
0.45	1	1	0.001	1000

Table: Estimated p-value using different number of trials.

Including covariate information

- Let X_i be a pretreatment measure of the outcome
- One way is to use this as a gain score: $Y^{d'} = Y_i^d - X_i$
- Causal effects are the same $Y^{1i} - Y^{0i} = Y_i^1 - Y_i^0$
- But the test statistic is different:

$$T_{gain} = \left| (\overline{Y}_T - \overline{Y}_C) - (\overline{X}_T - \overline{X}_C) \right|$$

- If X_i is strongly predictive of Y_i^0, then this could have higher power
    - Y_{gain} will have lower variance under the null
    - This makes it easier to detect smaller effects

Regression in RI

- We can extend this to use covariates in more complicated ways
- For instance, we can use an OLS regression:

$$Y_i = \alpha + \delta D_i + \beta X_i + \varepsilon$$

- Then our test statistic could be $T_{OLS} = \widehat{\delta}$
- RI is justified even if the model is wrong
  - OLS is just another way to generate a test statistic
  - The more the model is "right" (read: predictive of Y_i^0), the higher the power T_{OLS} will have
- See if you can do this in Thornton's dataset using the loops and saving the OLS coefficient (or just use `ritest`)