Technical Track Session II: Randomized Experiments

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Randomized Trials

- How do researchers learn about counterfactual states of the world in practice?

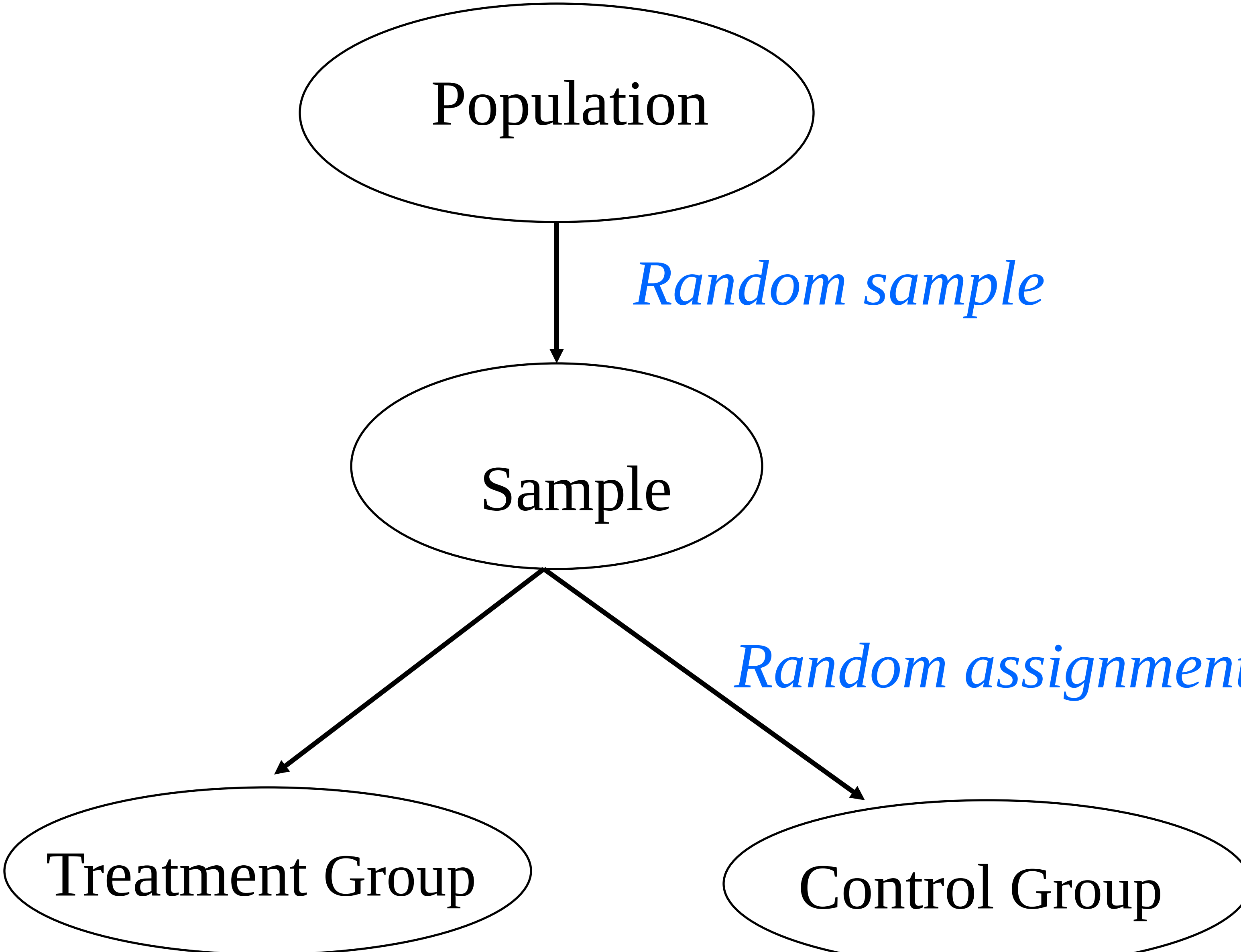
- In many fields, and especially in medical research, evidence about counterfactuals is generated by randomized trials.

- Under certain conditions, randomized trials ensure that outcomes in the control group really do capture the **counterfactual** for a treatment group.

Randomization for causal inference

Statisticians recommend a formal two-stage randomization model:

- **First stage:** a random sample of units is selected from a defined population.
- **Second stage:** this sample of units is randomly assigned to treatment and control groups.


Population
Random sample
Sample
Random assignment
Treatment Group
Control Group

Why 2 stages of randomization?

- **First Stage: for External Validity**
  - I.e. ensure that the results in the sample will represent the results in the population within a defined level of sampling error

- **Second Stage: for Internal Validity**
  - I.e. ensure that the observed effect on the dependent variable is due to the treatment rather than to other confounding factors

Two-Stage Randomized Trials

In large samples, two-stage randomized trials ensure that:

$$[\bar{Y}_1 \mid D=1] = [\bar{Y}_1 \mid D=0] \text{ and } [\bar{Y}_0 \mid D=1] = [\bar{Y}_0 \mid D=0]$$

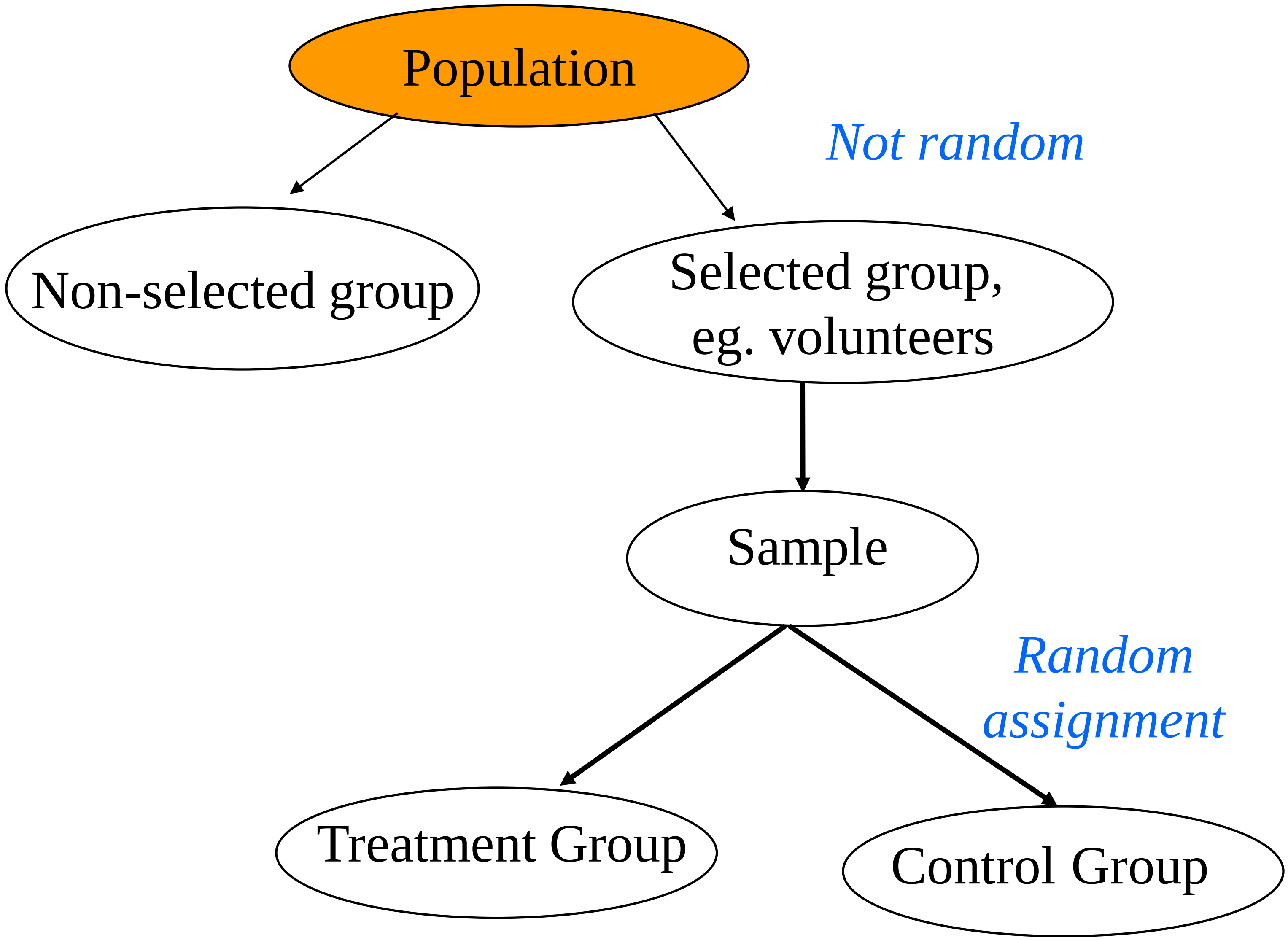
Why is this true....

Thus, the estimator

$$\hat{\bar{\delta}} = [\hat{\bar{Y}}_1 \mid D=1] - [\hat{\bar{Y}}_0 \mid D=0]$$

consistently estimates the Average Treatment Effect *ATE*

Population vs. selected group?



Population vs. selected group?

- If the randomization takes place on a selected group of units
- we’ll be estimating ????
- The treatment effect on that selected group of units!

Randomized Trials: Caveats

- **Non-compliance**
  - Not all treatment units will receive the treatment (non-compliance)
  - Some control units may receive treatment (non-compliance)
- **Attrition:** We may not be able to observe what happens to all units
- **Hawthorne effect:** just observing units makes them behave differently
- **John Henry effect**: the “controls” work harder to compensate

Randomized vs. Non-Randomized Trials

- **Randomized experiments**
  - Assumptions play a minor role
  - Or no role at all when testing the hypothesis of “no treatment effect”
  - In the absence of difficulties such as noncompliance or attrition….
- **Non-randomized methods**
  - Requires strong assumptions in order to validate the counterfactual
  - Attrition is equally a problem as in randomized trials

Example 1: Randomized Trial of "Computers for Education", Colombia

- Program activities:
  - re-furbish computers donated by private firms and installs them in public schools.
  - train teachers in the pedagogic uses of computers with the help of a local university.
- 2006: 100 schools were subject to a randomization:
  - 50 of them received computers
  - 50 did not receive computers

Appendix B. BASE LINE, COLOMBIA: SOME SELECTED STATISTICS

	Mean		Difference	Mean	Difference
	Urban	Rural	Urban/Rural	Sample	Treatment/Control
SCHOOL INFORMATION					
Number of teaches	16	7.55	8.45***	9.64	0.26
	(12.25)	(5.86)	(1.84)	(8.67)	(1.34)
Student/Teach. ratio	14.1	13.53	0.57	13.67	-0.73
	(13.53)	(11.26)	(2.87)	(11.79)	(1.77)
More than 10 years as teacher	0.88	0.64	0.24***	0.7	0.03
	(0.13)	(0.25)	(0.02)	(0.25)	(0.03)
Total number of students	200.08	105.48	94.60***	128.89	-26.17
	(217.21)	(140.87)	(24.21)	(166.94)	(21.17)
Number of repeating grade	14.08	4.96	9.12***	7.22	5.14***
	(24.88)	(6.22)	(3.04)	(13.89)	(0.59)
Number of drop-outs	17.21	12.3	4.91**	13.52	-1.23
	(24.37)	(15.15)	(2.42)	(17.86)	(1.47)
Classrooms	10.83	6.48	4.35***	7.56	0.38
	(7.04)	(3.54)	(1.10)	(4.99)	(0.90)
Libraries	0.54	0.6	-0.06	0.59	-0.01
	(0.51)	(0.55)	(0.06)	(0.54)	(0.06)
Central component of school	0.83	0.86	-0.03	0.86	-0.17**
	(0.38)	(0.35)	(0.06)	(0.35)	(0.08)
Usefulness	1.08	1.04	0.04	1.05	-0.02
	(0.28)	(0.26)	(0.05)	(0.27)	(0.03)

INDIVIDUAL INFORMATION

Gender	0.55	0.52	0.03	0.54	-0.04
	(0.0067)	(0.0056)	(0.05)	(0.0043)	(0.04)
Age	11	12.54	-1.54***	11.91	0.17
	(0.0330)	(0.0311)	(0.54)	(0.0237)	(0.51)
Number of siblings	3.2	4.16	-0.96***	3.77	-0.12
	(0.0337)	(0.0322)	(0.25)	(0.0239)	(0.28)
Work	0.16	0.23	-0.07***	0.2	-0.01
	(0.0050)	(0.0047)	(0.02)	(0.0035)	(0.02)
Attend school last year	0.97	0.98	-0.0075	0.97	-0.0013
	(0.1757)	(0.1544)	(0.0049)	(0.1635)	(0.0048)
Repeated grade last year	0.29	0.38	-0.0920***	0.35	0.0011
	(0.4542)	(0.4861)	(0.0251)	(0.4755)	(0.0276)
Did not attend school last week	0.21	0.24	-0.0222	0.23	-0.0097
	(0.4104)	(0.4250)	(0.0231)	(0.4193)	(0.0264)
How many days	2.15	1.9	0.2571*	2	0.1003
	(2.9484)	(1.6404)	(0.1500)	(2.2373)	(0.1523)
Like the school	0.98	0.98	0.0033	0.98	-0.0022
	(0.1313)	(0.1427)	(0.0038)	(0.1382)	(0.0039)
Know internet	0.48	0.35	0.1314**	0.4	-0.0009
	(0.4996)	(0.4758)	(0.0517)	(0.4900)	(0.0592)
Uses internet (if yes)	0.8	0.65	0.1464***	0.73	-0.0077
	(0.3993)	(0.4756)	(0.0422)	(0.4459)	(0.0514)
Hours of study outside school	1.47	1.31	0.1565***	1.38	0.0812
	(1.0064)	(0.9610)	(0.0559)	(0.9826)	(0.0542)
Test scores: Language pool	0.45	0.4	0.0520**	0.42	0.0072
	(0.2667)	(0.2579)	(0.0207)	(0.2627)	(0.0229)
Test scores: Mathematics pool	0.33	0.31	0.0239	0.31	-0.0077
	(0.2574)	(0.2437)	(0.0235)	(0.2495)	(0.0231)

Example 2: Does reducing class size improve elementary school education?

- Project STAR (Student-Teacher Achievement Ratio)
- 4-year experiment to evaluate the effect of small class sizes on learning, in kindergarten through 3rd grade.
- Treatment levels:
  1. <u>Regular class size</u>: 22-25 students and a single teacher.
  2. <u>Small class</u>: 13-17 students and a single teacher.
  3. <u>Teacher's aide</u>: regular-sized class plus a teacher's aide.

Example 2: Does reducing class size improve elementary school education?

- Each school had at least one class of each type.
- Students entering kindergarten in a participating school were randomly assigned to one of these three groups.
- Teachers were also assigned randomly.

Estimates of Effect of Treatments on Standardized Test Scores

$$Y_i = \beta_0 + \beta_1 I_i^{SmallClass} + \beta_2 I_i^{Aide} + u_i$$

	Grade			
Regressor	**Kinder**	**1st**	**2nd**	**3rd**
Intercept	918.0*** (1.6)	1,039.4*** (1.8)	1,157.8*** (1.8)	1,228.5*** (1.7)
Small Class	13.9*** (2.5)	29.8*** (2.8)	19.4** (2.7)	15.6*** (2.4)
Regular Size with aide	0.3 (2.3)	12.0*** (2.7)	3.5 (2.5)	-0.3 (2.3)
Number of Observations	*5,786*	*6,379*	*6,049*	*5,967*

Example 2: Does reducing class size improve elementary school education?

- **Findings**:
  - Reducing class size has an effect on test performance,
  - Adding a teacher's aide to a regular sized class has a much smaller effect, possible zero.

- **Caveat:**
  - These estimates ignore both attrition and non-compliance.
  - These two nuisances were high => the results might be biased.

Non-Compliance and Attrition: Solutions?

- **Non-compliance:**

  1. Intention to Treat Analysis
  2. Instrumental Variables Analysis (Local Average Treatment Effect)

- **Attrition** (Hidden bias)

  1. Make sure that there is no difference in attrition between treatment and comparison groups
  2. Use Instrumental Variables & Matching Methods

Example 3:

Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment

Angrist et al. (2002)
AER

Programa de Apliacion de Cobertura de la Educacion Secundaria (PACES)

- **Program benefits**
  - School voucher programs for attendance of private secondary schools
  - Vouchers covering somewhat more than half the cost of private secondary school
  - Vouchers were renewable if students maintained satisfactory academic performance

- **Beneficiaries**
  - 125,000 “treated” pupils
  - Beneficiaries were chosen by lottery from a pool of eligible applicants

Design of the PACES evaluation

- Interviewed 1,600 PACES applicants in 1998
  - Similar numbers of lottery winners and losers
  - 1995 and 1997 applicant cohorts from Bogota
  - 1993 applicant cohort from Jamundi

- Interview method: through telephone
  - Response rate: +/- 60%
  - Response is independent of treatment assignment (i.e. same for lottery winners and loosers)

Personal Characteristics and Voucher Status for Bogotá 1995

Dependent variable	Loser means	Won voucher
Age at time of survey	15.0	-0.013
	(1.4)	(0.078)
Male	0.501	0.004
		(0.029)
Mother’s highest grade completed	5.9	-0.079
	(2.7)	(0.166)
Mother’s age	40.7	-0.027
	(7.3)	(0.426)
Father’s age	44.4	0.567
	(8.1)	(0.533)
Father’s wage (>2 min wage)	0.100	0.005
		(0.021)
N	583	1,176

Notes: The table reports voucher losers’ means and the estimated effect of wining a voucher. Numbers in parentheses are standard deviations in columns of means and standard errors in columns of estimated voucher effects.

Educational Outcomes and Voucher Status, Bogotá 1995

Dependent variable	Loser means (1)	No controls (2)	Basic controls (3)	Full controls (4)
Finished 6th grade	0.943 (0.232)	0.026** (0.012)	0.023* (0.012)	0.021* (0.011)
Finished 8th grade	0.632 (0.483)	0.112*** (0.027)	0.100*** (0.027)	0.094*** (0.027)
Repetitions of 6th grade	0.194 (0.454)	-0.066*** (0.024)	-0.059** (0.024)	-0.059** (0.024)
Ever repeated after lottery	0.224 (0.417)	-0.060*** (0.023)	-0.055** (0.023)	-0.051** (0.023)
Years in school since lottery	3.7 (0.951)	0.058 (0.052)	0.034 (0.050)	0.031 (0.050)
Sample size	562		1,147	

Notes: The table reports voucher losers' means and the estimated effect of wining a voucher. Numbers in parentheses are standard deviations in columns of means and standard errors in columns of estimated voucher effects. *** significant at 1% ** significant at 5% * significant at 10%

References

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- Cochran, W. G. (1965): “The planning of observational studies of human populations”, *Journal of the Royal Statistics Association Series A 128*, pp. 134-155, with discussion.
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