Randomization: helps ensure that there are no differences, on average, between those who received treatment and those who do not.

* Bias = E[Y0|T=1] – E[Y0|T=0] – control group vs. treatment group had they not gotten the treatment
* Randomization eliminates confounding – have no bias – can get unconfounded ATE
* If we randomize and control the exposure, we don’t need to worry about confounding, reverse causality, and selection on DV

Standard Error Formula:

* Larger sample 🡪 Lower standard error 🡪 More precision
* Lower variance in outcomes 🡪 More precision (samples with less natural variance (noise), then more precise inferences.)

Confidence Intervals: a range representing our uncertainty about our point estimate due to random error/noise- 95%Cl = Estimate – 2 \* SE, Estimate + 2 \* SE

* Central Limit Theorem 🡪 if we repeated a study a zillion times, the value of many of the statistics we get will follow a normal distribution – provide important information about population parameters
* ATE is positive when 0 is not in interval and large magnitude when all the values are positive

Null hypothesis = hypothesis that there is no effect

* Reject the null hypothesis = p<0.05 (statistically significant/difference); fail to reject the null hypothesis = p > 0.05 (ATE^ is due to noise)
* P-values = odds of getting the same result if the true effect is zero

Internal Validity

* Fundamental Principle of Controlled Experiment: actual outcomes among the control group can stand in for counterfactual outcomes of the treated group – E[Y0|T=1] = E[Y0|T=0]
* Erode the validity – 1. Chance imbalance – diff between covariates of treatment and control group; 2. Lack of stat power – study is too small to detect minor causal effects; 3. Non-compliance – subjects decide if they want to take treatment of not; 4. Placebo effects – subjects take the pill bc they think they should; 5. Attrition – subjects drop out of study after randomization; 6. Interference – control and treatment groups influence each other 🡪 contamination (treatment become control and vice versa) & spillover (treatment affects those in control)

External Validity: How well does the results from our experiment generalize to reality?

* Experimental effects: subjects in the study are responding in a way to please the experimenter; Heterogeneity: Population in experiment is not representative of population in interest

Regression

* Regression equation – Yi = alpha + beta(Ti) + ri
  + Alpha = intercept, value of the outcome when X’s are 0
  + Beta = slope, how much on average the y changes for a 1 unit change
  + Ri = error
* Long regression equation –
  + Controlled because it contains confounders
  + Alpha = the mean outcome among t=1 when confounder is 0; beta = change in mean outcome among t=1 compared to t=0 when confounder is manipulated
* Short regression equation –
  + More biased prediction of outcome because of confounder
* OVB = Bs – BL – omitting the confounder major led to a positive bias of 1; negative = underestimation; positive = overestimation

Differences in Differences: measuring trends in outcome before and after the policy changes

* Parallel trends = average outcome would have been the same had everyone remained untreated; outcomes must change at similar rates (have similar slopes)
* DiD is unbiased if the treated and untreated units experience the same trends in Y0 (before treatment) – Bias = DiD – ATT
* DiD = (Y:treatment, before – Y:treatment, after) – (Y:control, after – Y: control, before)

Regression Discontinuity: inferring causality from observational studies

* There are assumptions that many treatments vary discontinuity at thresholds
* Running variable: continuous variable that determines whether or not the unit receives the treatment (T = 1 receives scholarship)
* Local Average Treatment Effect (LATE)
* A screenshot of a graph

  Description automatically generatedUse regressions instead of means for a better estimate
  + Less bias and less noise
  + Use you reduce the width of the bin size, the sample size gets smaller (less bias is shown)
* Continuity assumption: points must have the same potential outcomes at threshold; this allows us to use our untreated groups as a counterfactual proxy at the threshold, without introducing bias

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Instrumental Variable

Assumptions: No defiers, some compilers, exclusion (effect of Z on Y only through T), exogeneity (random or as-if random)

Always Takers: T1 = T0 = 1; Never Takers: T1 = T0 = 0; Compliers: T1 = 1 and T0 = 0; Defiers: T1 = 0 and T0 = 1

\*\*COMPLIERS CAN BE Z=0, T=1, T0 = 0\*\*

First Stage (Compliers) = Avg[T|Z=1] – Avg[T|Z=0] – Pr(Compliers) - Pr(Compliers) + Pr(Always Takers) + Pr(Never Takers) = 1

* Avg(T|Z=1) is unbiased for E[T1|Z=1]

Intent to Treat (ITT) = Avg [Y|Z=1] – Avg[Y|Z=0]

* ITT = CATE\*Pr(Complier)
* Effect of Z|Always Takers)\*Pr(Always) + (Effect of Z|Nevers)\*Pr(Nevers) + (Effect of Z|Compliers)\*Pr(Compliers)
  + Complier Average Treatment Effect: The effect of Z among compliers is the effect of treatment among compliers (under some assumptions)
* ITT underestimates CATE

What if our population was 100% Always Takers or 100% Never Takers? No effect of Z on Y (Avg [Y|Z=1] – Avg[Y|Z=0] = Y1/Y0 – Y1/Y0 = 0

Wald Estimate = (Avg [Y|Z=1] – Avg[Y|Z=0] / Avg[T|Z=1] – Avg[T|Z=0])

* Gives an unbiased estimate of the average effect of T on Y for compliers