Basics of random assignment

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Average Treatment Effect

- Consider a program (T) that induces two binary "potential outcomes" for each individual i
  - The untreated outcome Y_{0i}
  - The treated outcome Y_{1i}
- The observe outcome

$$\begin{aligned} Y_i &= \begin{cases} Y_{1i} & \text{if } T_i = 1 \\ Y_{0i} & \text{if } T_i = 0 \end{cases} \\ &= Y_{0i} + (Y_{1i} - Y_{0i})T_i \end{aligned}$$

- The impact for any individual is $\delta_i = Y_{1i} - Y_{0i}$
- If we were able to observe both of these outcomes for every individual, then program evaluation would be straightforward
- The Average Treatment Effect (ATE) is

$$ATE = E(\delta_i) = E(Y_{1i} - Y_{0i})$$

Average Treatment Effect

- But we only observe Y_i

Average Treatment Effect

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- If we compare the outcomes of treated and untreated individuals:

$$\underbrace{E(Y_i|T_i=1)-E(Y_i|T_i=0)}_{\text{Observed difference}} = \underbrace{E(Y_{1i}|T_i=1)-E(Y_{0i}|T_i=1)}_{\text{average treatment effect on the treated}} + \underbrace{E(Y_{0i}|T_i=1)-E(Y_{0i}|T_i=0)}_{\text{Selection bias}}$$

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- Since $E(Y_{1i}-Y_{0i}|T_i=1)=E(Y_{1i}-Y_{0i})$, can drop the "on the treated" qualifier

$$\begin{aligned} E(Y_i|T_i=1)-E(Y_i|T_i=0) &= E(Y_{1i}|T_i=1)-E(Y_{0i}|T_i=1) \\ &= E(Y_{1i}-Y_{0i}|T_i=1) \\ &= \underbrace{E(Y_{1i}-Y_{0i})}_{ATE} \end{aligned}$$

Regression analysis of experiments

- Suppose (for now) the treatment effect is the same for everyone, then:

$$Y_i = \underbrace{\alpha}_{E(Y_{0i})} + \underbrace{\delta}_{Y_{1i}-Y_{0i}} T_i + \underbrace{\varepsilon_i}_{Y_{0i}-E(Y_{0i})}$$

Regression analysis of experiments

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- Then

$$\begin{aligned} E(Y_i|T_i=1) &= \alpha + \delta + E(\varepsilon_i|T_i=1) \\ E(Y_i|T_i=0) &= \alpha + E(\varepsilon_i|T_i=0) \end{aligned}$$

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- Thus,

$$E(Y_i|T_i=1) - E(Y_i|T_i=0) = \underbrace{\delta}_{ATE} + \underbrace{E(\varepsilon_i|T_i=1) - E(\varepsilon_i|T_i=0)}_{\text{Selection bias}}$$

Regression analysis of experiments

$$E(Y_i|T_i=1) - E(Y_i|T_i=0) = \underbrace{\delta}_{ATE} + \underbrace{E(\varepsilon_i|T_i=1) - E(\varepsilon_i|T_i=0)}_{\text{Selection bias}}$$

- $E(\varepsilon_i|T_i=1) - E(\varepsilon_i|T_i=0) = E(Y_{i0}|T_i=1) - E(Y_{i0}|T_i=0)$
- Selection bias amounts to correlation between the error and the treatment status
- Or correlation between Y_{i0} and the treatment status

Regression analysis of experiments

- In a simple experiment the average treatment effect is the difference in sample means between the treatment and the control group

- This is the OLS coefficient of β in the regression

$$Y_i = \alpha + \delta T_i + \varepsilon_i$$

Regression analysis of experiments

- We know that $\begin{pmatrix} \widehat{\alpha} \\ \widehat{\delta} \end{pmatrix} = (X'X)^{-1}X'y$
- In an RCT (or any binary treatment) context with pN units treated
  - Regress the outcome on a constant and the treatment indicator $X_i = \begin{pmatrix} 1 & T_i \end{pmatrix}$

$$X = \begin{pmatrix} 1 & T_1 \\ 1 & T_2 \\ 1 & T_3 \\ \vdots & \vdots \\ 1 & T_{pN} \\ 1 & T_{pN+1} \\ 1 & T_{pN+2} \\ \vdots & \vdots \\ 1 & T_N \end{pmatrix} = \begin{pmatrix} 1 & 1 \\ 1 & 1 \\ 1 & 1 \\ \vdots & \vdots \\ 1 & 1 \\ 1 & 0 \\ 1 & 0 \\ \vdots & \vdots \\ 1 & 0 \end{pmatrix}; \qquad Y = \begin{pmatrix} Y_1 \\ Y_2 \\ Y_3 \\ \vdots \\ Y_{pN} \\ Y_{pN+1} \\ Y_{pN+2} \\ \vdots \\ Y_N \end{pmatrix}$$

Regression analysis of experiments

$$X'X = \begin{pmatrix} 1 & 1 & 1 & \cdots & 1 & 1 & 1 & \cdots 1 \\ 1 & 1 & 1 & \cdots & 1 & 0 & 0 & \cdots 0 \end{pmatrix} \begin{pmatrix} 1 & 1 \\ 1 & 1 \\ 1 & 1 \\ \vdots & \vdots \\ 1 & 1 \\ 1 & 0 \\ 1 & 0 \\ \vdots & \vdots \\ 1 & 0 \end{pmatrix} = \begin{pmatrix} N & pN \\ pN & pN \end{pmatrix} = pN \begin{pmatrix} \frac{1}{p} & 1 \\ 1 & 1 \end{pmatrix}$$

The formula for inverting a 2×2 matrix:

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix}^{-1} = \frac{1}{ad - bc} \begin{pmatrix} d & -b \\ -c & a \end{pmatrix}$$

Hence:

$$\left(pN \begin{pmatrix} 1 & 1 \\ 1 & \frac{1}{p} \end{pmatrix} \right)^{-1} = \frac{1}{pN} \begin{pmatrix} \frac{1}{p} & 1 \\ 1 & 1 \end{pmatrix}^{-1} = \frac{1}{(pN)\left(\frac{1}{p} - 1\right)} \begin{pmatrix} 1 & -1 \\ -1 & \frac{1}{p} \end{pmatrix} = \frac{1}{N(1-p)} \begin{pmatrix} 1 & -1 \\ -1 & \frac{1}{p} \end{pmatrix}$$

$$X'y = \begin{pmatrix} 1 & 1 & 1 & \cdots & 1 & 1 & 1 & \cdots 1 \\ 1 & 1 & 1 & \cdots & 1 & 0 & 0 & \cdots 0 \end{pmatrix} \begin{pmatrix} Y_1 & \\ Y_2 & \\ Y_3 & \\ \vdots & \vdots \\ Y_{pN} & \\ Y_{pN+1} & \\ Y_{pN+2} & \\ \vdots & \vdots \\ Y_N & \end{pmatrix} = \begin{pmatrix} \sum_{i=1}^{N} Y_i \\ \sum_{i=1}^{pN} Y_i \end{pmatrix}$$

Regression analysis of OLS

$$(X'X)^{-1}X'y = \frac{1}{N(1-p)}\begin{pmatrix} 1 & -1 \\ -1 & \frac{1}{p} \end{pmatrix}\begin{pmatrix} \sum_{i=1}^{N} Y_i \\ \sum_{i=1}^{pN} Y_i \end{pmatrix}$$

Regression analysis of OLS

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\begin{aligned}
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&= \begin{pmatrix} \frac{\sum_{i=1}^{N} Y_i}{N(1-p)} - \frac{p}{pN(1-p)} \sum_{i=1}^{pN} Y_i \\ \frac{1}{N(1-p)}\left(-\sum_{i=1}^{N} Y_i + \frac{1}{p}\sum_{i=1}^{pN} Y_i \right) \end{pmatrix}
\end{aligned}
$$

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&= \begin{pmatrix} \frac{\overline{Y}}{(1-p)} - \frac{p}{(1-p)}\overline{Y_T} \\ -\frac{1}{(1-p)}\overline{Y} + \frac{1}{(1-p)}\overline{Y_T} \end{pmatrix}
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&= \begin{pmatrix} \frac{1}{1-p}\left(p\overline{Y_T} + (1-p)\overline{Y_C} - p\overline{Y_T}\right) \\ \frac{1}{(1-p)}\left(-p\overline{Y_T} - (1-p)\overline{Y_C} + \overline{Y_T}\right) \end{pmatrix}
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&= \begin{pmatrix} \overline{Y_C} \\ \overline{Y_T} - \overline{Y_C} \end{pmatrix}
\end{aligned}
$$

Regression analysis of experiments

- Typically add controls

$$Y_i = \alpha + \beta T_i + \gamma X_i + \varepsilon_i$$

- Good controls
  - Dummies for randomization strata (more on this tomorrow)
  - Baseline covariates that predict the outcome
  - Baseline values of outcome variables are (sometimes) most important control
- Bad controls We do not want to include:
  - Controls that could be impacted by treatment

Basics of random assignment

Compliance

- Most RCTs examine programs that will not be universally adopted when offered
- **Compliance**: Whether someone takes the treatment when they are offered
  - In the case of a new type of farming practice there will be farmers offered the practice who do not adopt it
  - In the case of a job training program there will be entrepreneurs who choose not to attend the training
  - These are the non-compliers
- Let C_i be the compliance status of individual i
  - If she chooses to accept the program then $C_i = 1$
  - If not then $C_i = 0$
- Two impacts: intention-to-treat (ITT) and treatment-on-the-treated (ToT)

Intention to Treat Effect

- The Intention to Treat Effect

$$ITT = E(Y_{1i}|T_i = 1) - E(Y_{0i}|T_i = 0)$$

- The ITT essentially ignores non-compliance: estimates the effect of 'intending to treat' some units, regardless of how many take up the treatment
- Partially sidesteps compliance issues: Focus on treatment/ignore compliance
- It cannot completely escape compliance because a decreasing compliance rate will push the ITT towards zero

Treatment Effect on the Treated

- The Treatment Effect on the Treated

$$ToT = E(Y_{1i}|T_i = 1; C_i = 1) - E(Y_{0i}|T_i = 0; C_i = 1)$$

- This is the treatment effect on those who actually choose to accept the treatment
- **The counterfactual is those who would have accepted the treatment if they had been offered it**
- Non-compliance drives down the ITT relative to the ToT
- If a program has no spillover effect (i.e., non-compliers in the treatment area receive no indirect effect from the treatment taking place around them), the treatment effect on the non-compliers is 0

Treatment Effect on the Treated

There is a mechanical relationship between the ITT and ToT

$$ITT = E(Y_{1i}|T_i = 1) - E(Y_{0i}|T_i = 0)$$

Treatment Effect on the Treated

There is a mechanical relationship between the ITT and ToT

$$\begin{aligned} ITT &= E(Y_{1i}|T_i = 1) - E(Y_{0i}|T_i = 0) \\ &= cE(Y_{1i}|T_i = 1; C_i = 1) + (1-c)E(Y_{1i}|T_i = 1; C_i = 0) - \\ &\quad cE(Y_{1i}|T_i = 0; C_i = 1) - (1-c)E(Y_{1i}|T_i = 0; C_i = 0) \end{aligned}$$

Treatment Effect on the Treated

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&= cE(Y_{1i}|T_i = 1; C_i = 1) + (1-c)E(Y_{1i}|T_i = 1; C_i = 0) - \\
&\quad cE(Y_{1i}|T_i = 0; C_i = 1) - (1-c)E(Y_{1i}|T_i = 0; C_i = 0) \\
&= c\left[E(Y_{1i}|T_i = 1; C_i = 1) - E(Y_{0i}|T_i = 0; C_i = 1)\right] + \\
&\quad (1-c)\underbrace{\left[E(Y_{1i}|T_i = 1; C_i = 0) - E(Y_{1i}|T_i = 0; C_i = 0)\right]}_{\text{zero because of no spillovers (i.e., exclusion restriction)}}
\end{aligned}
$$

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ITT &= E(Y_{1i}|T_i = 1) - E(Y_{0i}|T_i = 0) \\
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&\quad cE(Y_{1i}|T_i = 0; C_i = 1) - (1-c)E(Y_{1i}|T_i = 0; C_i = 0) \\
&= c\left[E(Y_{1i}|T_i = 1; C_i = 1) - E(Y_{0i}|T_i = 0; C_i = 1)\right] + \\
&\quad (1-c)\underbrace{\left[E(Y_{1i}|T_i = 1; C_i = 0) - E(Y_{1i}|T_i = 0; C_i = 0)\right]}_{\text{zero because of no spillovers (i.e., exclusion restriction)}} \\
&= ToT * c
\end{aligned}
$$

where c is the compliance rate

Treatment Effect on the Treated

- Compliance is not typically observed in the control group!
- Estimator for the ToT given above cannot be estimated with standard data
- If we are willing to assume that there is no interference with the control group then we can back out the ToT as ITT/c
- Standard empirical way of estimating ToT effects is to instrument actual receipt of treatment with being offered treatment
  - Run a regression with compliance as the endogeneous variable
  - Being in the treatment group is the instrument
- In a regression without any other control variables, this instrumented ToT will be exactly the ITT blown up by the inverse of the compliance rate.

Treatment Effect on the Treated

- Think of the compliance as an endogenous variable with an IV (treatment)

$$\begin{aligned} C_i &= \gamma_0 + \gamma_1 T_i + \mu_i \\ Y_i &= \beta_0 + \beta_1 \widehat{C}_i + \varepsilon \end{aligned}$$

- $\widehat{C}_i = c$ (i.e, the compliance rate in the treatment group)
- Regressing the outcome on the treated yields the ITT
- Thus, $\widehat{\beta}_1 = \frac{ITT}{c} = ToT$

Two-sided non-compliance

- Two-sided non-compliance:
  - Individuals in the control who get treated
  - Individuals in the treatment who do not comply
- In this context it is natural to think of the treatment simply as something that boosts compliance, and not as the actual receipt of the treatment itself
  - Email encouraging people to do something
  - Facebook/Twitter/Google ads with information
- We can of course continue to estimate a kind of ITT in this context (difference between the group offered the "treatment" and the group not offered)
- What we are estimating with the ITT is the **impact of the intervention that changes compliance and not the impact of the treatment itself**
- If the compliance rate in the treatment and control groups is the same, we have no experiment at all!

Local Average Treatment Effects

- The treatment and compliance possibilities define four possible cells (assuming that the purpose of the treatment is to increase compliance):

1. Always takers: $c_i(T_i = 0) = c_i(T_i = 1) = 1$
2. Never takers: $c_i(T_i = 0) = c_i(T_i = 1) = 0$
3. Compliers: $c_i(T_i = 0) = 0$ and $c_i(T_i = 1) = 1$
4. Defiers: $c_i(T_i = 0) = 1$ and $c_i(T_i = 1) = 0$

Local Average Treatment Effects

- We are not typically interested in the impact of the intervention to boost compliance, but rather the impact of the treatment itself
- We can instrument compliance with offering the treatment: In some ways this is simply a standard implementation of the instrumented TOT
- Abadie and Imbens: Cannot use this instrument to understand the effect of the promotion on "always compliers" nor on of the "defiers"
- In other words, the instrument has no first-stage for groups that were going to comply or not comply in the absence of the promotion
- What we estimate with this technique is the Local Average Treatment Effect
- This is the impact of the actual treatment (rather than the promotion), estimated only upon those types who were induced to comply by the promotion

Local Average Treatment Effects

- The analogy to the estimation of the ToT with one-sided non-compliance would be to inflate the ITT estimated from a Randomized Promotion design by the differential compliance between the treatment and control
- But in order to do this we must make an assumption stronger than the "Non-Interference and Exclusion Restriction" assumptions already laid out
- We must add a Monotonicity assumption known as the "No Defiers" assumption
- The reason we need this assumption is precisely the heterogeneity of impacts
- Under homogeneous impacts and the exclusion restriction, if the fraction of Defiers and Compliers in the sample were equal, we could not have a treatment effect

"No Defiers" assumption

- We set the fraction of defiers to zero: $\pi_D = 0$
- Then
  - Fraction of always takers: $\pi_{AT} = E[C_i(T_i = 0)]$
  - Fraction of never takers: $\pi_{NT} = 1 - E[C_i(T_i = 1)]$
  - Fraction of compliers: $\pi_C = E[C_i(T_i = 1) - C_i(T_i = 0)]$

Local Average Treatment Effects

- We can write the ITT as a weighted average of outcomes in the four cells:

$$
\begin{aligned}
ITT \;=\; & \pi_C E[Y_i(T_i=1;C_i=1) - Y_i(T_i=0;C_i=1)] + \\
& \pi_{AT} \underbrace{E[Y_i(T_i=1;C_i=1) - Y_i(T_i=0;C_i=1)]}_{\text{zero by the exclusion restriction (no spillovers)}} + \\
& \pi_{NT} \underbrace{E[Y_i(T_i=1;C_i=0) - Y_i(T_i=0;C_i=0)]}_{\text{zero by the exclusion restriction (no spillovers)}} + \\
& \underbrace{\pi_D E[Y(T_i=1;C_i=0) - Y_i(T_i=1;C_i=0)]}_{\text{zero by the no defier assumption}}
\end{aligned}
$$

Local Average Treatment Effects

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\begin{aligned}
ITT &= \pi_C E[Y_i(T_i = 1; C_i = 1) - Y_i(T_i = 0; C_i = 1)] + \\
&\quad \pi_{AT} \underbrace{E[Y_i(T_i = 1; C_i = 1) - Y_i(T_i = 0; C_i = 1)]}_{\text{zero by the exclusion restriction (no spillovers)}} + \\
&\quad \pi_{NT} \underbrace{E[Y_i(T_i = 1; C_i = 0) - Y_i(T_i = 0; C_i = 0)]}_{\text{zero by the exclusion restriction (no spillovers)}} + \\
&\quad \underbrace{\pi_D E[Y(T_i = 1; C_i = 0) - Y_i(T_i = 1; C_i = 0)]}_{\text{zero by the no defier assumption}} \\
&= \pi_C E[Y_i(T_i = 1; C_i = 1) - Y_i(T_i = 0; C_i = 1)]
\end{aligned}
$$

Local Average Treatment Effects

- Problem: I can tell the fraction of compliers in the population (by comparing uptake rates in treatment relative to the control) but I cannot tell which individuals were induced to comply by the promotion
- This LATE may be interesting (if the promotion is a real policy that is being considered, or is based off of price variation that we will really observe)
- ...Or may be completely artificial (if the promotion induces a group to comply that would never comply in the native implementation of the program)

Local Average Treatment Effects

Imbens: Reporting the local average treatment effect, solely, or in combination with bounds or point estimates for the overall average based on additional assumptions, is thus emphatically not motivated by a claim that the local average treatment effect is the sole or primary effect of interest. Rather, it is motivated by a sober assessment that estimates for other subpopulations do not have the same internal validity, and by an attempt to clarify what can be learned from the data in the absence of identification of the population average effect

When are LATEs what we want to measure?

- A price randomization where the implementer is considering a range of different prices/subsidies and the experiment includes the relevant range (this design still only gives the marginal impact and not the impact on the whole client pool)
- Eligibility randomization 'on the bubble' where the question answered is the impact of extending access on the eligibility margin
- A randomized promotion intervention where a technology is universally available but not widely adopted, so the relevant policy question is the effect of expanding uptake through adoption-enhancing interventions

When are LATEs **NOT** what we want to measure?

- A price incentive randomization in a context where the product is provided by the private market and the prices in the study will never be observed in reality
- Randomized promotion campaign where the promotion is so expensive that it yields a group of beneficiaries who would never take the product in reality
- LATE or IV based on variation that selects an odd sample that is not representative of the implementation population (e.g., Deaton's earthquakes example)

Example: Thornton (2008), The Demand for, and Impact of, Learning HIV Status

- Even with widespread HIV testing, many people choose not to learn their status
- Obvious and potentially cost-effective solution is to pay them to learn their results
- Two-sided non-compliance: 33% of those without incentives learned their HIV status, and 21% of those with incentives did not learn their HIV status
- More complicated than a standard LATE:
  - Incentives become increasingly important to compliance as distance grows
  - As distance increases the two-sided LATE closer to a one-sided ToT