Threats to Inference

EGAP LEARNING DAYS

SANTIAGO

DAY 4


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1. Get your question. **Identify X and Y.**
2. Form **partnerships**, engage in "scoping"
3. Figure out **randomization** and **measurement** strategies
4. IRB (update later)
5. Gather **pre-existing data** and conduct **power calculations**
6. Seek **peer review** of draft design
7. **Register** design (update later)
8. **Pilot** Baseline (sampling)
9. Run **Baseline**
10. Assign **Treatment**
11. Take any **intermediate** measures and **CHECK** that treatment is going OK
12. **End** of treatment
13. Gather **endline measures** (prepare instruments; train enumerators; pilot instrument)
14. **Run** analyses
15. **Check** analyses (better: have someone else check)
16. Generate **key tables and circulate** policy relevant material immediately.
17. Make data and instruments **available** to others.
18. Complete **writeup** and submit for publication.
19. **Revise and resubmit**.

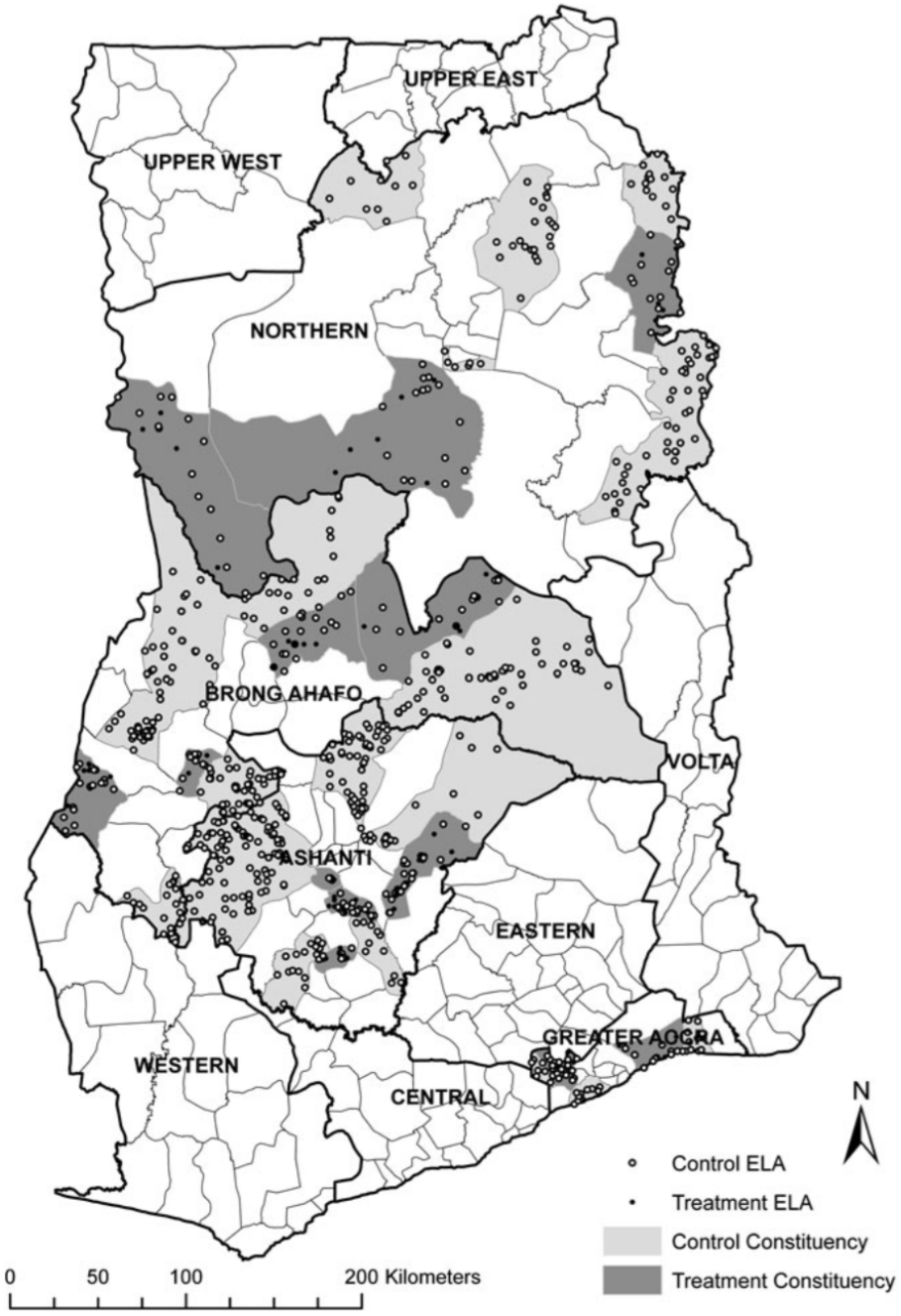
Nine Limitation of Randomization (?)

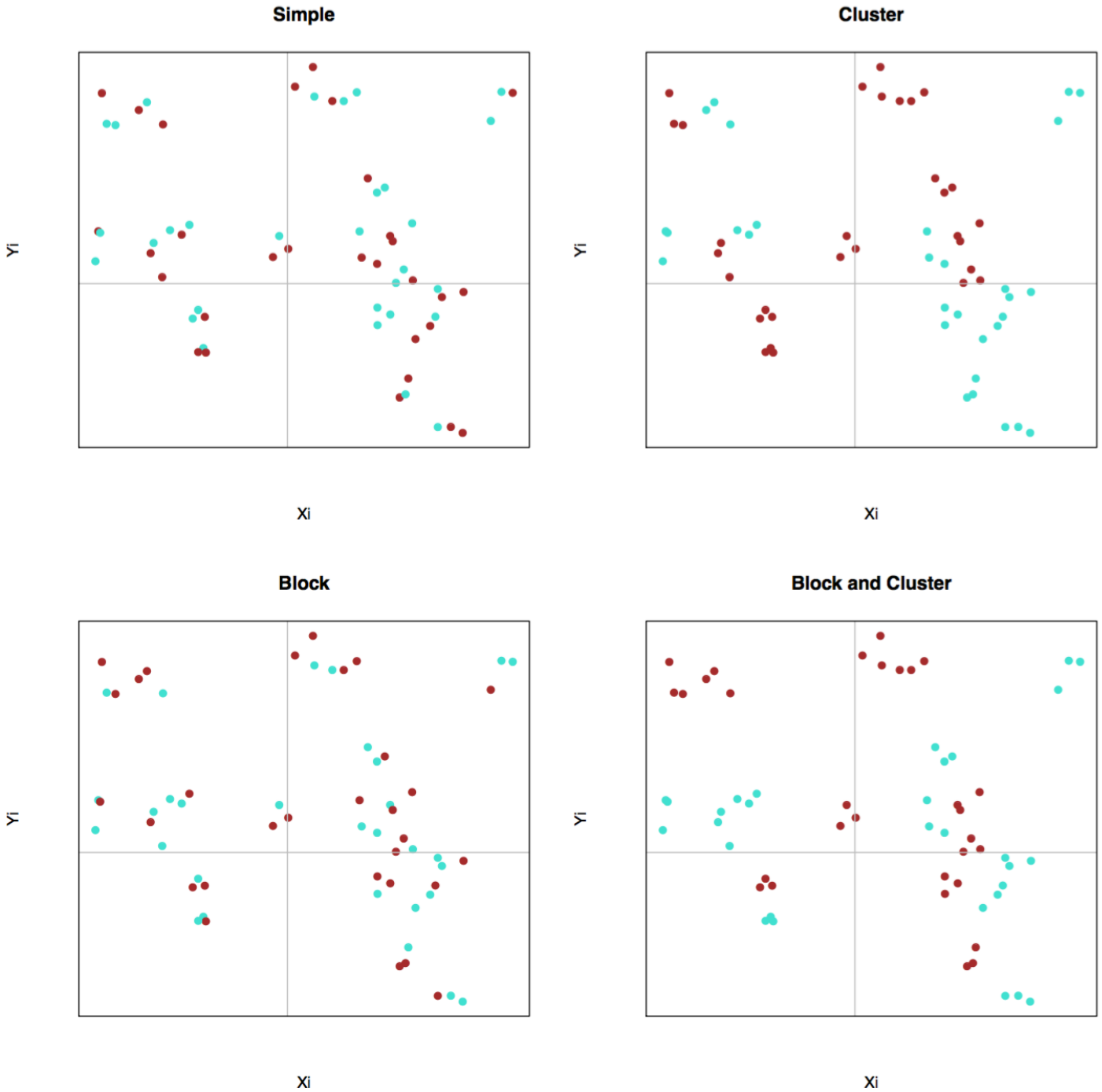
1. Ethics – is this sort of manipulation ethical? Sometimes not (parachutes)
2. The *real time* constraint. Sometimes to slow. Not much good to help understand history
3. History has happened
4. The problem of cost (sometimes; but possible very low)
5. The power constraint. You need a lot of units
   (actually: a problem for any statistical approaches)
6. External validity (problem for any evaluation)
7. The problem of spillovers, attrition, compliance, demand (problem for any evaluation)
8. The *variables as attributes* constraint (gender, ethnicity, problem for any evaluation)
9. The *assignment to treatment* constraint.
10. Reduced Flexibility for organization (problem for any prospective evaluation)

Block and cluster

- GHANA MAP

Ghana, with Treatment and Control Constituencies and Electoral Areas




Simple
Yi
Xi
Cluster
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Block and Cluster
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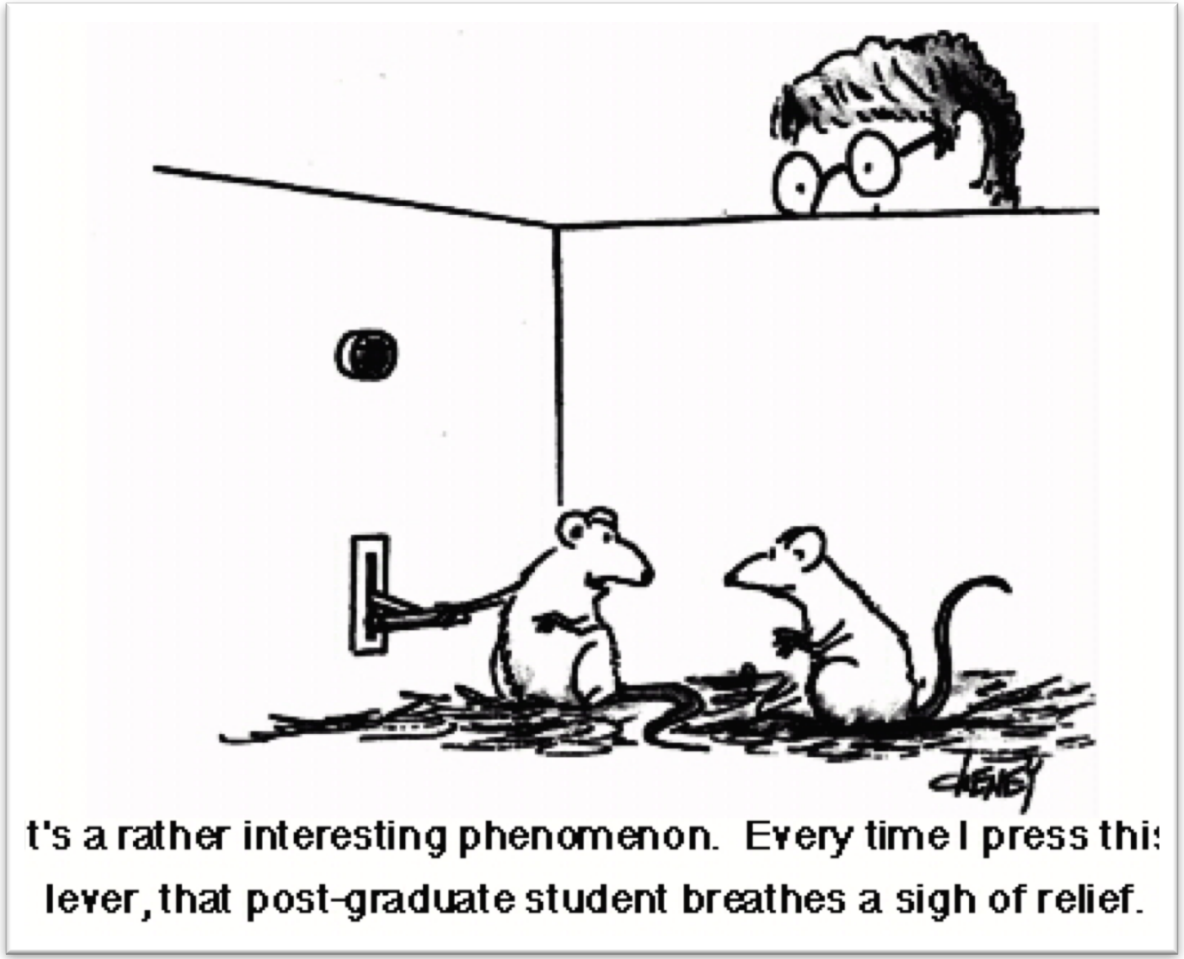
Overview

- Hawthorne effects
- Spillovers
- Noncompliance
- Attrition

Hawthorne Effects & Related



Hawthorne Effects & Related

- **The problem**: perhaps the experimental effects you are measuring are due to the implementation of the experiment itself rather than due to the treatment.

- Examples?
- Possible also of effects associated with being in control?

- **Principles**:
- Make interventions as natural as possible
- Also, remember that treatment effects are always differences between treatment and control, so if the control condition makes things worse this does not necessarily mean that the treatment condition makes things better!

Noncompliance

- ..

Noncompliance

- The treated do not get treatment
  - ie. The treatment villages in Sierra Leone sample **do not** get aid
    - They refused
    - The implementing partner make a mistake, or deliberate action
- The control get treated
  - Ie. The control villages **did** get aid (from us, or someone else)
    - *"Goal came and built us a fake toilet"*
- Importance of monitoring

Noncompliance

Table 1

	Assigned to treatment	Assigned to control
Compliers	Treated	Not treated
Always-Takers	Treated	Treated
Never-Takers	Not treated	Not treated
Defiers	Not treated	Treated

Noncompliance

- Example, n =200
- We find that only 80 people are actually treated.
- What is the impact of the treatment?
- ATE? Not really
- Compare Yt vs Yc on all units, this is the intention to treat effect (ITT).
- Not give a measure of the effect of the treatment itself.
- Compare the 120 untreated and 80 treated subjects? Unbiased?

Local Average Treatment Effect (LATE)

- Treatment effect for the Compliers.

Table 2

	Assigned to treatment Average outcome = 50	Assigned to control Average outcome = 10
Never-Takers	20 people	20 people
Compliers	80 people	80 people

- ITT = ?
- LATE = ?
- Assumption: outcome for a Never-Taker is the same regardless of whether they are assigned to the treatment or control (exclusion restriction)

Two-sided non-compliance.

Table 3

	Assigned to treatment Average outcome = 50	Assigned to control Average outcome = 10
Defiers	0 people	0 people
Never-Takers	10 people	10 people
Compliers	80 people	80 people
Always-Takers	10 people	10 people

- Assume sample contains no Defiers (monotonicity assumption)
- ITT = 40
- Share of Compliers = ?
- There are no Defiers, so Never-Takers in treatment and control are the same
- LATE = 40/0.8 = 50
- See Nolen and Hudgens 2011 RI with two sided non compliance

Nigeria example

- El Grande

Spillovers

- Violation of **non-interference assumption or** Stable Unit Treatment Value Assumption (SUTVA)
  - We have been talking about treatment (control) units as if the expected *Y* for unit *i only depends upon whether or not the unit gets the treatment*
- We assume there are no spillovers

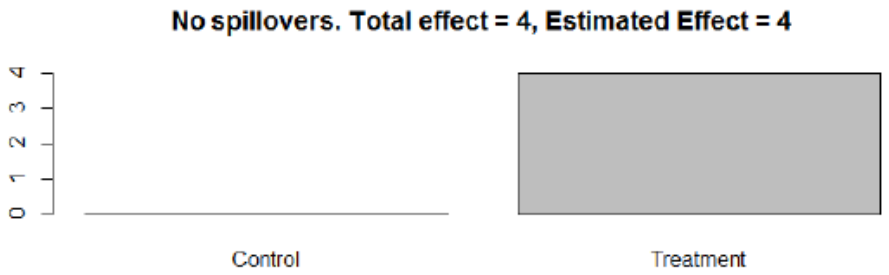
- Spillovers may produce biased estimates
- The sign and magnitude of the bias depend on the way in which treatment effects spill over across observations
  - Spillovers can result in the estimation of **weaker** effects in cases where effects are actually **stronger**.

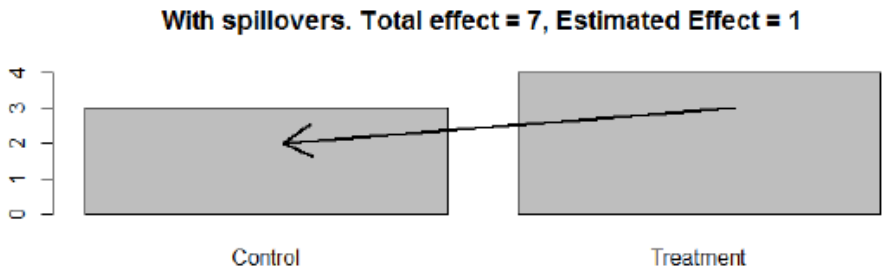
Spillovers

- The key is to think through the **structure** of spillovers.
  - Physical (malaria, worms, tvs)
  - Behavioral (imitation)
  - Informational (social learning, enthusiasm)
  - Markets (changes in demand change prices, vv)

Spillovers

- The key problem is that in these cases "Y(1) and Y(0)" are not sufficient to describe potential outcomes





- Underestimate effect (if positive spillovers) and vv

Spillovers

- In example immediate neighbors are exposed
- Anticipate: **what** is spilling over and **to whom**?
  - Positive: maximize them!
  - Negative: minimize
- Adjust level and design
- Measure spillovers!

- Randomization for Spillovers
- Two level designs
  - Control,
  - Spillover control
  - Treatment

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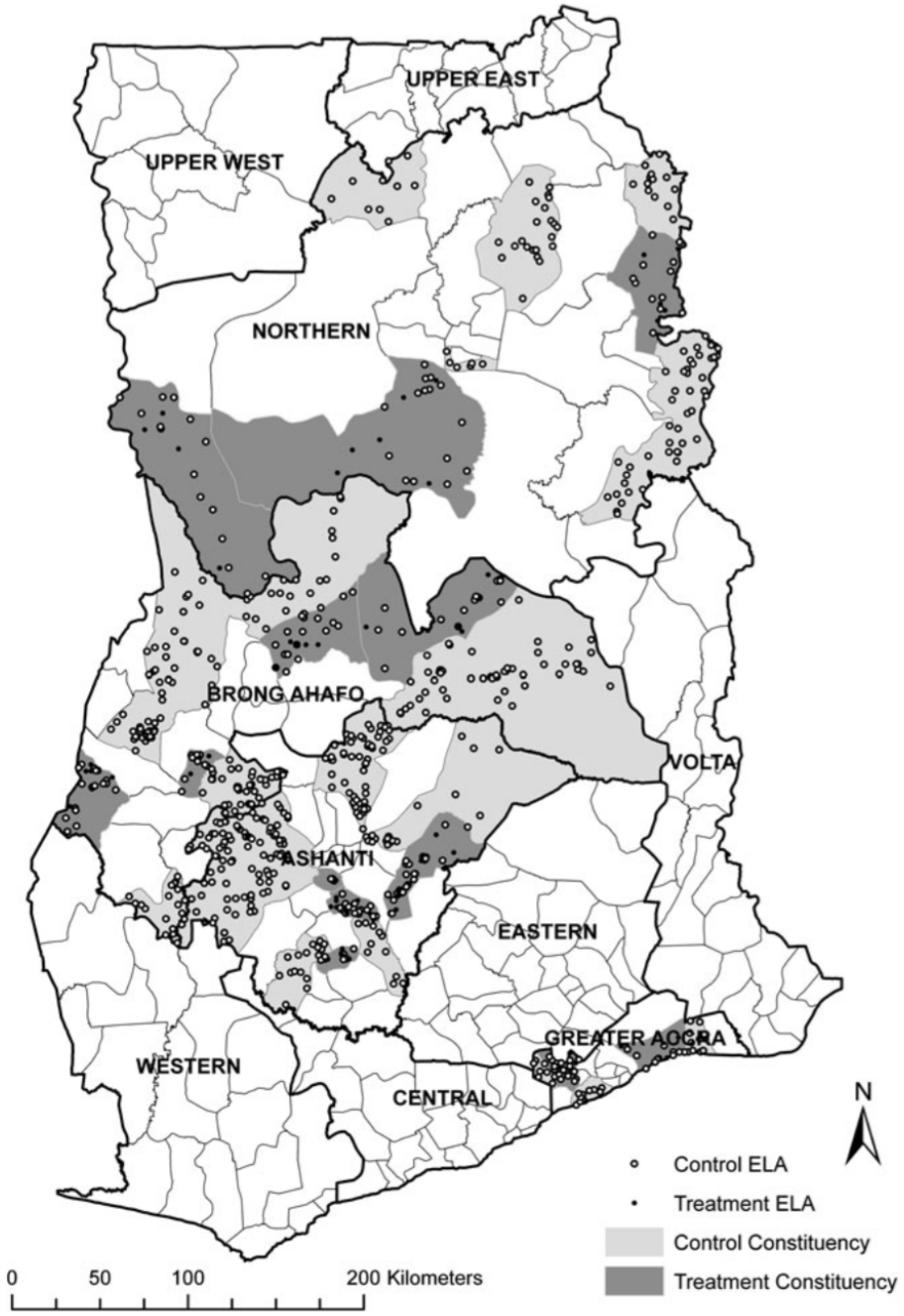

Attrition

- Missing data problem
  - People die/move
  - People cant be located
  - People refuse to answer
  - RA problems…

Attrition

	Originals		Drop out		Replacements	
BL	Villages	92				
	Households	2379				
	Individuals	2379				
ML	Villages	90	Villages	2	Villages	-
	Households	2108	Households	271 (11%)	Households	143
	Individuals	1514	Individuals	865 (33%)	Individuals	143
EL	Villages	92	Villages	0	Villages	-
	Households	1599	Households	780 (33%)	Households	652
	Individuals	1077	Individuals	1302 (56%)	Individuals	652

Attrition

- Missing data problem
- Is it systematic?
  - Difference rates across treatment and control?
- Loss of data -> power
- Preventing?
  - Level of measurement (Hawthorne effects)
  - Data collection effort (admin, tracking, etc)

- Adjusting your analysis?
  - Ignore (dropping observations) bias vs power
  - Bounds (Manski, Blattman et al 2015)
  - Sensitivity analysis
  - Double sampling (Aranow et al 2015) bias vs power