

# DPIR Experimental Methods: Lecture 3

Non-Compliance, Attrition, Spillovers

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# Road Map to Lecture 3

- Non-compliance
- Attrition
  - RAND case study
  - Combating Attrition
  - MIPO
  - MIPO—X and IPW
- Spillovers
- Design Case Studies
  - Uganda film festivals (Wilke et al 2020)
  - Vaccination in Ghana (Duch et al 2023)
  - Exclusionary Attitudes (Kala and Broockman 2020)
  - Rand Health Insurance (Aron-Dine et al 2013)

# Noncompliance

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# Intuition

- If  $ATE = E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$ , how can we ever know that subjects were actually treated?
- More importantly, *what does it mean to be “treated”?*
- Let us distinguish between
  - Assignment of treatment ( $Z$ )
  - Receipt of treatment ( $D$ )
- Yes, by the exclusion restriction,  $Y_i(z, d) = Y_i(d)$
- However, in many applications,  $z_i \neq d_i$
- Noncompliance with treatment assignment = subjects do not receive the treatment to which they were assigned

## Kalla and Broockman 2020

- Kalla and Broockman (2020): Reducing exclusionary attitudes through interpersonal conversation (APSR)
- 230 canvassers *are assigned to* have face-to-face conversations with 6,869 voters deploying non-judgmental exchange of narratives on a range of topics
- Outcome: Exclusionary immigration policy and prejudicial attitudes
- What can go wrong?
- In this example, when does a subject comply with the treatment assignment?

**TABLE 1. Summary of Differences Between Conditions and Results in Previous Study and Experiments 1–3**

Study	Broockman and Kalla (2016)		Experiment 1		Experiment 2		Experiment 3
Topic	Transphobia		Unauthorized immigrants		Transphobia		Transphobia
Condition name	Full Intervention	Full Intervention	Abbreviated Intervention	Participants' and Video Narratives	Video Narratives Only	Participants' Narratives by Phone	
<b>Intervention contents</b>							
Non-judgmental exchange of narratives...							
<input type="radio"/> From participants (voter and canvasser)	YES	YES	NO	YES	NO	YES	
<input type="radio"/> In video	YES	NO	NO	YES	YES	NO	
Address concerns and deliver talking points	YES	YES	YES	YES	YES	YES	
<b>Results</b>							
Null effects ( $d = 0.02, p = 0.27$ ), statistically distinguishable from Full Intervention							
ITT <sup>a</sup>	Positive effects ( $d = 0.16, p < 0.001$ )	Positive effects ( $d = 0.08, p < 0.001$ )	Positive effects ( $d = 0.06, p < 0.01$ )	Positive effects ( $d = 0.08, p < 0.001$ )	Positive effects ( $d = 0.08, p < 0.001$ )	Positive effects ( $d = 0.04, p < 0.001$ )	
CACE <sup>b</sup>	$d = 0.22$		$d = 0.12$ (Abbreviated vs. Placebo)		$d = 0.10$	$d = 0.10$	$d = 0.08$

Notes: Each Experiment also contained a Placebo condition not shown in the table. These Placebo conditions contained no persuasive content on the topics but are used as a baseline for comparison when estimating the effect sizes shown in the table.

<sup>a</sup>To summarize the results of each study, we first average the pre-specified Overall Index in each study across survey waves to compute a pooled Overall Index. We then report intent-to-treat (ITT) effects on this pooled Overall Index, which represents the mean difference between individuals assigned to each condition among all individuals who identified themselves at their doors, regardless of whether the conversation continued after that point. The ITT estimates represent the average causal effect of attempting to treat people who open their doors, even if they refuse to converse soon after. This means the ITT estimates are “diluted” by the presence of individuals who open the door but do not enter into the conversation.

<sup>b</sup>To estimate the implied Complier Average Causal effect (CACE), or the effect among those who received the intervention, we estimate compliance under a conservative definition of compliance: whether participants got to the “first rating” part of the conversation where they initially told canvassers how they felt about the policy. The CACE estimates represent the average causal effect of treating the people who do

# Definition and formalization

- Where is the *ATE* row? What are *ITT* and *CACE*?
- Let  $d_i(z)$  denote whether subject  $i$  is actually treated when treatment assignment is  $z$
- There are different types of compliance and noncompliance with the treatment
- **Compliers:**  $d_i(1) = 1$  and  $d_i(0) = 0$  or  $d_i(1) > d_i(0)$
- **Never-Takers:**  $d_i(1) = 0$  and  $d_i(0) = 0$
- **Always-Takers:**  $d_i(1) = 1$  and  $d_i(0) = 1$
- **Defiers:**  $d_i(1) = 0$  and  $d_i(0) = 1$  or  $d_i(1) < d_i(0)$

# Definition and formalization

- These groups are formed *after* random assignment, not formed *by* random assignment → they might differ systematically in ways that bias *ATE* estimator
- 2 types of noncompliance
  - One-sided:  $d_i(1) = 0$  for some  $i$  but  $d_i(0) = 0 \forall i$  (only compliers and never-takers)
  - Two-sided: additionally,  $d_i(0) = 1$  for some  $i$  (these can be defiers or always-takers)
- In any experiment facing noncompliance, which subjects *could* make up the treatment group, and which the control group? How might that look like in Kalla and Broockman (2020)?
- What is the problem of naively comparing treated and untreated subjects, i.e. estimate *ATE*?

# Estimation of treatment effects under noncompliance

- What groups *could* we compare to unbiasedly estimate a treatment effect?
- 2 estimands
  - Intent-to-treat effect (*ITT*)
  - Complier average causal effect (*CACE*)
- Choice of estimands depends, of course, on your research question and goal of causal inference

## Intent-to-Treat Effect

$$\begin{aligned}\text{ITT} &\equiv E[Y_i(z = 1)] - E[Y_i(z = 0)] \\ &= E[Y_i(z = 1, d(1))] - E[Y_i(z = 0, d(0))]\end{aligned}$$

- ITT captures the average effect of being assigned to the treatment group regardless of the proportion of the treatment group actually treated
- Which causal inference method does this setup remind you of?

# Complier Average Causal Effect

$$\text{CACE} \equiv \underbrace{E[(Y_i(d=1) - Y_i(d=0))]}_{\text{average treatment effect}} \mid \underbrace{d_i(1) - d_i(0) = 1}_{\text{among Compliers}}$$

Let

$$\pi_C = E[d_i(z=1) - d_i(z=0)]$$

be the proportion of compliers in the sample.

Then, the sample analog of the *CACE* estimand is

$$\text{CACE} = \frac{\text{ITT}}{\pi_C}$$

- Assumptions: Non-interference, excludability, and, under 2-sided noncompliance, monotonicity (no defiers, i.e.  $d_i(1) \geq d_i(0)$ )
- CACE also referred to as Local Average Treatment Effect (LATE) and, under one-sided noncompliance, Treatment on Treated (TOT)
- ATE among Compliers

# Potential Outcomes

Obs	$Y_i(0)$	$Y_i(1)$	$D_i(0)$	$D_i(1)$	Type
1	4	6	0	1	Complier
2	2	8	0	0	Never-Taker
3	1	5	0	1	Complier
4	5	7	0	1	Complier
5	6	10	0	1	Complier
6	2	10	0	0	Never-Taker
7	6	9	0	1	Complier
8	2	5	0	1	Complier
9	5	9	0	0	Never-Taker

## Compare ITT, ATE, and CACE

- ATE does not consider noncompliance:

$$\text{ATE} = \frac{2 + 6 + 4 + 2 + 4 + 8 + 3 + 3 + 4}{9} = 4$$

- ITT accounts for the fact that never-takers will not receive the treatment (always-takers will receive the treatment):

$$\text{ITT} = \frac{2 + 0 + 4 + 2 + 4 + 0 + 3 + 3 + 0}{9} = 2$$

- CACE is based on the subset of Compliers:

$$\text{CACE} = \frac{2 + 4 + 2 + 4 + 3 + 3}{6} = 3$$

# Personal Canvass & Voting

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- Gerber and Green New Haven study APSR 2000
- Randomly assign voters different GOVT tactics
  - Personal canvassing contact?
  - Mail?
  - Telephone?
  - Control?

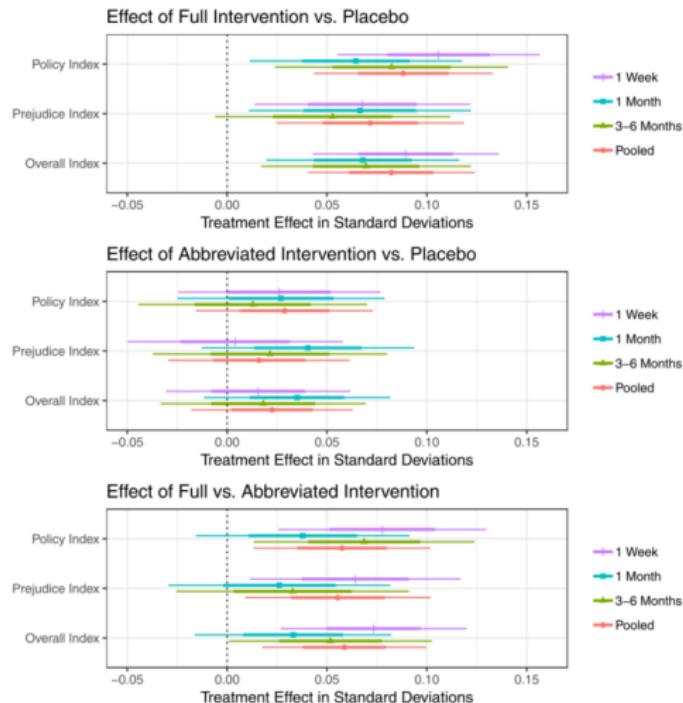
# New Haven Voter Mobilization

Turnout Rate	Treatment Group	Control Group
Among those contacted	54.43 (395)	
Among those not contacted	36.48 (1050)	37.54 (5645)
Overall	41.38 (1445)	37.45 (5645)

- $\text{ITT} = 41.38 - 37.54 = 3.84$
- $\pi_C = 395/1445 = 0.273$
- $\text{CACE} = \text{ITT}/\pi_C = 3.84/0.273 = 14.1$

# Kalla and Broockman (2020)

FIGURE 1. Experiment 1 Results: Intent-to-Treat Effects



Notes: Each panel shows the estimated intent-to-treat effects when comparing the two experimental conditions described in the panel title (e.g., the top panel compares the Full Intervention condition to the Placebo condition). Within each panel, we show treatment effects on the pre-specified primary outcome indices. Results are average treatment effects with 1 standard error (thick) and 95% confidence intervals (thin). To form each pooled index, we average each respondent's values for the corresponding index across all post-treatment survey waves. See Online Appendix Tables OA.9–11 for numerical point estimates and standard errors.

## Broader takeaways

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1. Carefully define the treatment itself
2. Carefully define treatment assignment and treatment receipt
3. Carefully define and try to identify compliant and non-compliant subgroups of subjects

## Design implications

Bear in mind that

$$\text{SE}(\widehat{\text{CACE}}) \approx \frac{\text{SE}(\widehat{\text{ITT}})}{\pi_C}$$

- Increase  $\pi_C$ ; rule out defiers
- 1-sided noncompliance: Placebo design

# Placebo Design

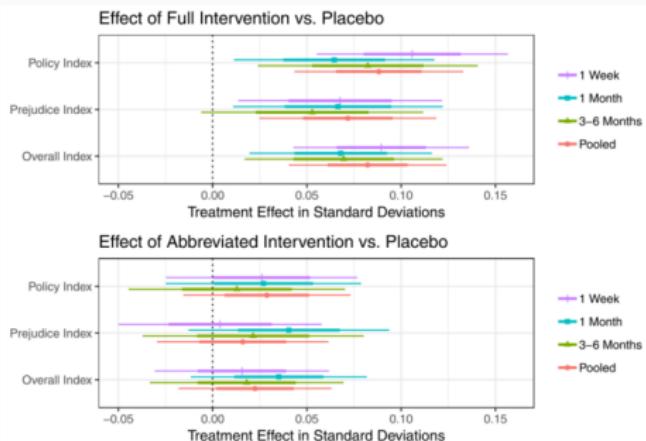
- Researchers attempt to contact individuals assigned to receive the treatment
- Those reached are then randomly allocated to two different groups
  - Treatment group
  - Placebo group receiving a "non-treatment"
- Kalla and Broockman (2020) canvassing experiment
  - Narratives (treatment)
  - Housing in Orange County (placebo)
- CACE estimated by comparing the outcomes for those in the treatment group to those in the placebo group
  - Random sample of Compliers whose untreated potential outcomes can be measured

# Kalla and Broockman (2020)

Experiment 1	
Unauthorized immigrants	
Full Intervention	Abbreviated Intervention
YES	NO
NO	NO
YES	YES

Positive effects ( $d = 0.08$ , $p < 0.001$ )	Null effects ( $d = 0.02$ , $p = 0.27$ ), statistically distinguishable from Full Intervention ( $d = 0.06$ , $p < 0.01$ )
	$d = 0.03$ (Abbreviated vs. Placebo) $d = 0.12$



# Placebo Design

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- Logic is that placebo design screens out Never-Takers (since they are, in addition to compliers, part of the control group under 1-sided noncompliance)
- Compliers in the treatment group are compared directly to Compliers in the untreated group
- Reduces noise from Never-Takers in both treatment and control groups
- Moves us to a world of "full compliance"

# Placebo Design

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- Downside is that not all Compliers receive the treatment
- Resources are wasted on those receiving the placebo
- Opportunity to collaborate with someone studying an unrelated topic

# Placebo Design

- The placebo and conventional design both allow estimation of the CACE
- Choice depends on the budget and compliance rate
- Under a fixed budget, the conventional design is preferable if compliance rate  $> 50\% (\pi_C > 1/2)$
- Canvassing studies often have a lower rate
- A pilot study may give a better idea of the expected compliance rate

## Nickerson 2008

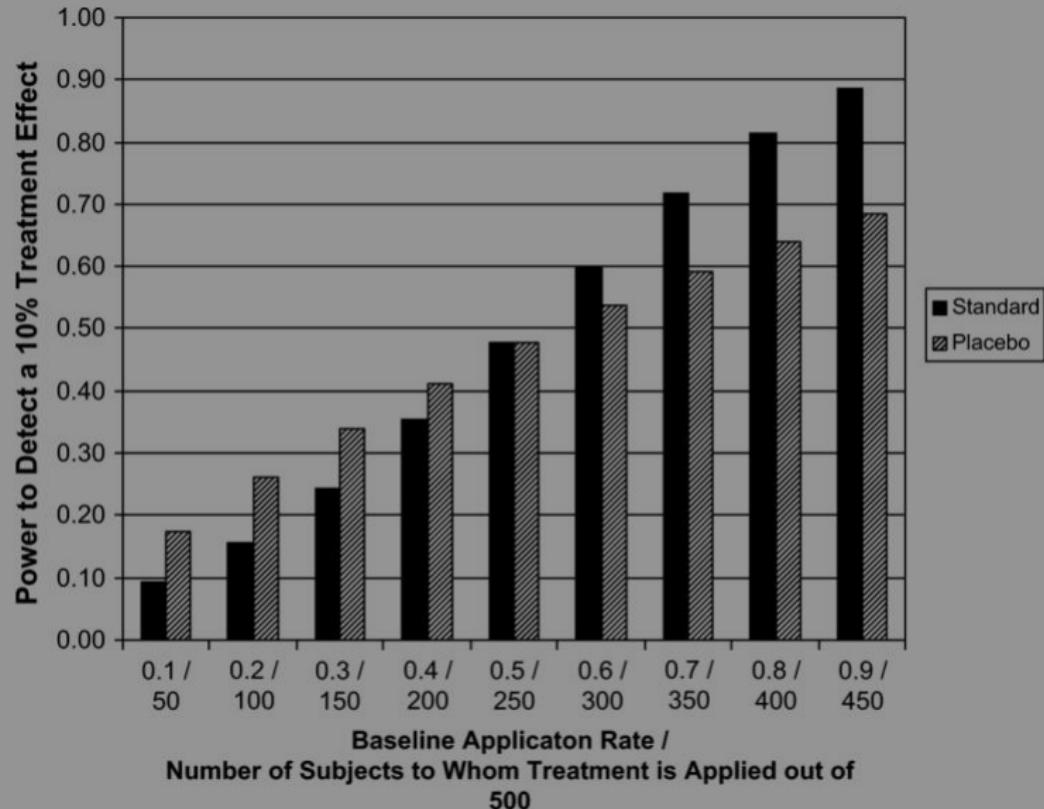
- Researchers attempt to contact individuals assigned to receive the treatment
- Those reached are then randomly allocated to two different groups
  - Treatment group
  - Placebo group receiving a "non-treatment"
- Nickerson (2008) canvassing experiment
  - GOTV (treatment)
  - Recycling (placebo)
- CACE estimated by comparing the outcomes for those in the treatment group to those in the placebo group
  - Random sample of Compliers whose untreated potential outcomes can be measured

# Nickerson 2008

	Denver		Minneapolis		Pooled	
	Direct	Secondary	Direct	Secondary	Direct	Secondary
Percent Voting in GOTV Group	47.7% (3.0)	42.4% (2.9)	27.1% (3.1)	23.6% (3.0)		
Percent Voting in Recycling Group	39.1% (2.9)	36.9% (2.9)	16.2% (2.7)	17.3% (2.7)		
Estimated Treatment Effect	<b>8.6%</b> <b>(4.2)</b>	<b>5.5%</b> <b>(4.1)</b>	<b>10.9%</b> <b>(4.1)</b>	<b>6.4%</b> <b>(4.1)</b>	<b>9.8%</b> <b>(2.9)</b>	<b>6.0%</b> <b>(2.9)</b>
P-Value	0.02	0.09	<0.01	0.06	<0.01	0.02

Note. Numbers in parentheses represent standard errors. P-values test the one-tailed hypothesis. Pooled estimates are weighted averages of results for both cities.

# Nickerson 2008



# Attrition

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## Non-interference

$$\text{ATE} = E [Y_i(1)_j D_i = 1] - E [Y_i(0)_j D_i = 0]$$

- Thus, average outcomes in the control and treatment groups in the sample are unbiased estimators of  $E [Y_i (1)]$  and  $E [Y_i (0)]$
- But implicit in this description is the assumption that the researcher observes  $Y_i$  for all subjects assigned
- Attrition occurs when outcome data are missing
- When attrition systematically related to potential outcomes, remaining subjects are no longer random samples of original group of subjects
- Comparing remaining group averages may no longer be an unbiased estimator of ATE

# How Might Attrition Occur

- Subjects refuse to cooperate with researchers
  - Respondents refuse post-treatment questionnaire
- Researchers lose track of experimental subjects
  - Subjects change address or name
- Firms, organizations, or governments block researchers' access to outcomes
  - Common with experiments on sensitive topics, e.g. corruption
- Outcome variable unavailable for some subjects
  - For a job training program treatment that wants to measure wages six months later, how do you measure this outcome for subjects without jobs?
- Researchers deliberately discard observations
  - Subjects not understanding instructions get discarded

## Rand Health Insurance Experiment: Setup

- Famous study illustrating the dangers of attrition for causal inference Cost \$80 million (1974 USD), or about \$300 million today
- Examines how copayment schemes in health insurance affect health service consumption and outcomes
- Four groups: 5%, 50% 75%, and 100% cost coverage treatments
- Costs over \$1,000 for last three groups fully insured
- Incentives so you “cannot lose financially by participating”
- As a related aside, Finkelstein et al’s Oregon Health Insurance experiment

## Rand Health Insurance Experiment: Setup

- Evaluation 3-5 years after treatment
- Those paying larger share made fewer physician visits and had lower rates of hospital admissions
- 100% group consumed 46% more in health services than 5% group
- 100% group no healthier on average than others, based on a wide array of health assessments
- Traditional interpretation of result: Requiring large co-pays does not have an adverse impact on health incomes for the average person
- But what about attrition? (Newhouse 1989)
- Any thoughts on how attrition might happen in a way that is systematically related to potential outcomes?

# Rand Health Insurance Experiment: Critique

- 8% in free group refused to enroll
  - Military service, institutionalization, death, incomplete data collection
- 25% refusal rate in 5
  - 6.7% in free plan left voluntarily after enrolling
  - 0.4% in 5% treatment group left after enrolling
- One interpretation: Those who chose to remain in copay plans anticipated lower health costs
  - Those anticipating illness drop out to pick up cheaper private insurance
- Alternative: Those who dropped out have same unmeasured health outcomes and health service consumption than those remaining
- How would we know? How would we deal with it?

## Four Strategies to Combat Attrition

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- Assume missingness is independent of potential outcomes
  - Unbiased inferences but lose sample size
  - Cannot directly assess this
  - Newhouse points out average pre-treatment health outcomes and health service utilization are similar across experimental groups among subjects who do not drop out of the study
  - If one assumes prior health outcomes are indicative of subsequent unmeasured potential outcomes, one could interpret this evidence to mean missingness is independent of potential outcomes

## Four Strategies to Combat Attrition

- Assume missingness independent of potential outcomes within subgroups defined by background attributes
  - Newhouse reports refusal to enroll is correlated with age/education
  - We could recover unbiased ATE by re-weighting the data to “fill in” age/education cells depleted by attrition
- Place bounds by guessing missing values (Manski 1995, 2007)
  - Assume those who disappear are extremely healthy or extremely ill
  - Alternatively, “trim” healthiest observations in free care group until attrition rate as high as cost-sharing groups
- Gather more data from missing subjects (Cochran 1977)
  - RAND eventually gathered health outcomes from 77

## Special Forms of Attrition: MIPO

- If  $R_i(z) \perp\!\!\!\perp Y_i(z)$ , attrition is (relatively) innocuous
- Missing Independent of Potential Outcomes or MIPO
- Can only gather circumstantial evidence about plausibility
  - If result of random procedure, no systematic relationship between  $r_i$  and subject background attributes or experimental assignment should exist (regr F-test)
  - But do covariates at one's disposal include the systematic sources of missingness?
- MIPO assumption more convincing if subjects have little discretion over whether their outcomes will be reported
  - Tutoring: Students miss for midyear exam
  - Voter mobilization: Town clerk fails to record data in timely fashion
  - Arguments for/against MIPO in these cases?

## Special Forms of Attrition: MIPO

- Alternative assumption: Attrition is unrelated to potential outcomes conditional on pre-treatment covariates  $X_i$
- Not surprisingly, this is referred to as Missing Independent of Potential Outcomes given X, or MIPO | X
- Example: In student test example, we condition on attendance record prior to the treatment intervention as  $X_i$
- Intuition: Assume  $N_1 = 30$  Oxford,  $N_2 = 10$  Cambridge students treated with tutoring vouchers
  - Let's say 15 Oxford students were missing at random
  - How might you recover an unbiased ATE in this example?

# Weighting under MIPO | X

- Define  $\pi_i(z = 1; x)$  as the share of non-missing subjects among those who are treated and have covariate profile  $X_i = x$
- When MIPO | X holds,

$$E[Y_i(1)] = \frac{1}{N} \sum_{i=1}^N \frac{Y_i(1)r_i(1)}{\pi_i(z = 1, x)} \quad (1)$$

- Subjects with missing outcomes do not appear in formula
- Produces accurate estimates of ATE when non-missing observations are good substitutes for missing outcomes
- Also known as inverse-probability weighting because subjects weighted by  $\frac{1}{\pi_i(z=1,x)}$
- Typically, logistic regression is used to estimate  $\hat{\pi}(z, x)$  weighted regression is then run

# Weighting under MIPO | X

- Primary drawback is the possibility that missingness remains related to potential outcomes
- As before, this is not directly testable or observable
  - Maybe those missing Oxford students weren't randomly missing!
- If attrition biases  $\hat{ATE}$  for a subgroup, it may be given more weight in estimates and actually worsen bias
- Also increases sampling variability because more weight placed on subsamples with large share of missing observations

## Simple Example

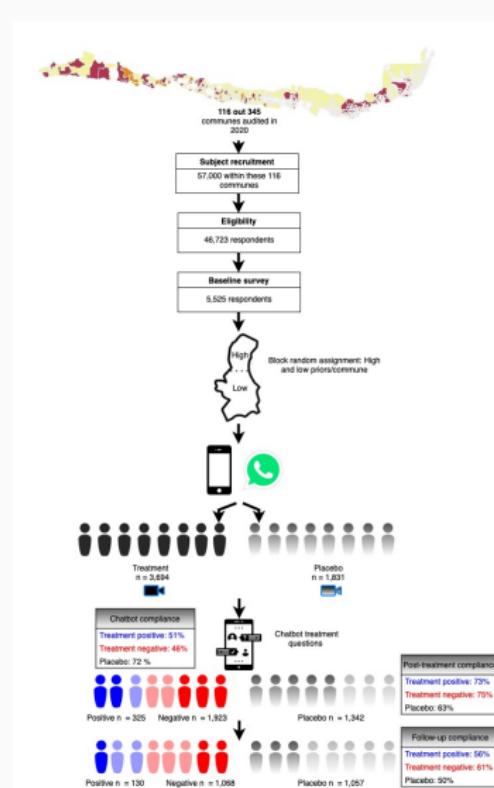
Obs	$Y_i(0)$	$Y_i(1)$	$R_i(0)$	$R_i(1)$	$Y_i(0) R_i(0)$	$Y_i(1)R_i(1)$	$X_i$
1	3	4	1	1	3	4	1
2	4	7	1	1	4	7	1
3	3	4	1	1	3	4	1
4	4	7	1	1	4	7	1
5	10	14	0	0	Missing	Missing	0
6	12	18	0	0	Missing	Missing	0
7	10	14	1	1	10	14	0
8	12	18	1	1	12	18	0

## Simple Example

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- Does MIPO hold? Implications?
- Does MIPO | X hold? Implications?
- What weights?
- Is unweighted expectation measuring something useful?

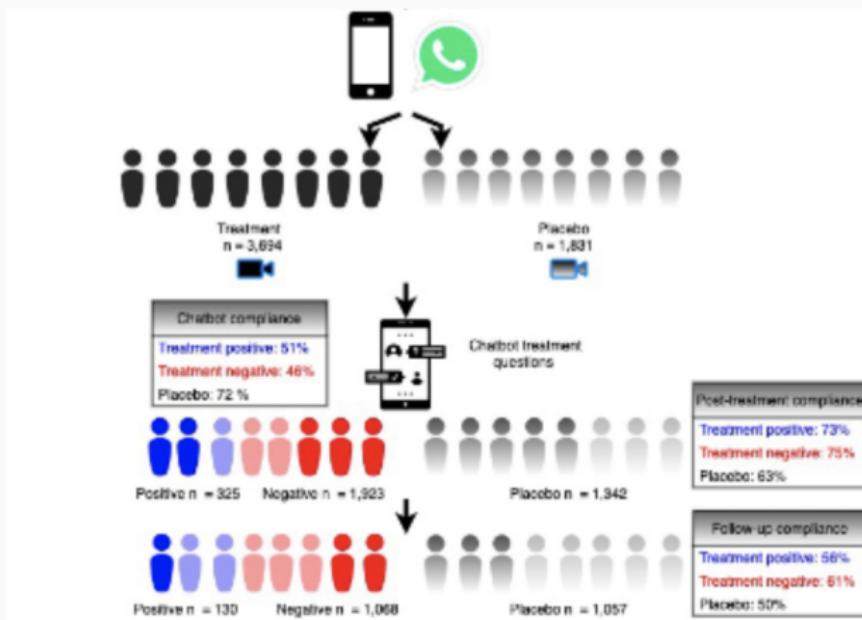
# Duch and Torres



# Duch and Torres



# Duch and Torres



# Duch and Torres IPW

```
## TABLE A5 -Regression results of Malfeasance Beliefs - Unweighed and IPW
##
data$id <- 1:nrow(data)
data$cens <- as.numeric(is.na(data$b.corrup.post)) # turns TRUEs into 1 and
0 if not missing
table(data$cens)

denom.cens <- glm(cens ~as.factor(treat)*age+as.factor(treat)*inv.income +
as.factor(treat)*civic.know + as.factor(treat)*partisanship.scale +
as.factor(treat)*educ.attain + as.factor(treat)*political.com, data=data,
family=binomial)
denom.p.cens <- predict(denom.cens, type = "response") # gets the predicted
probabilities
data$wt <- ifelse(data$cens == 0, (1/(1 - denom.p.cens)),1) # for not
missing values, if gets the predicted probabilities, for missing values it
gives 1

summary(data$wt[data$treat=="Control"])
summary(data$wt[data$treat=="Treatment"])

data$sel_valid <- data$cens == 0 # subsets for those observations with
reported values
```

# Duch and Torres IPW

TABLE A5—INTENTION-TO-TREAT: BELIEFS ABOUT MALFEASANCE - UNWEIGHTED AND IPW

	Qualitative		Resources		Distribution	
	Unweighted	Unstable	Unweighted	Unstable	Unweighted	Unstable
Intercept	2.446*** (0.085)	1.344*** (0.024)	2.004*** (0.086)	2.168*** (0.010)	1.126*** (0.038)	1.152*** (0.015)
Prior	0.478*** (0.014)	0.827*** (0.012)	0.463*** (0.016)	0.828*** (0.009)	0.426*** (0.016)	0.846*** (0.015)
Treat	0.780*** (0.071)	1.859*** (0.060)	0.779*** (0.078)	0.373*** (0.073)	0.362*** (0.027)	0.307*** (0.030)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Num.Obs.	3592	3592	3592	3592	3592	3592
R2	0.258	0.997	0.213	0.998	0.193	0.995
R2 Adj.	0.257	0.997	0.212	0.998	0.192	0.995

Note: This table reports the unweighted and weighted....

Source: All waves data

# Spillovers

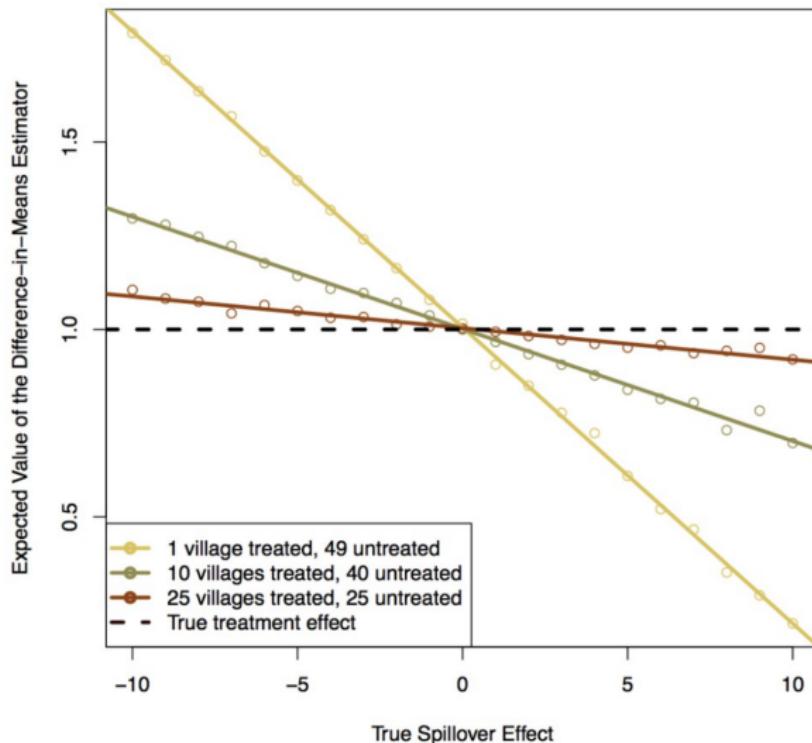
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## Non-interference

- Permits us to ignore the potential outcomes that would arise if subject  $i$  were affected by the treatment of other subjects
- Formally, we reduce the schedule of potential outcomes  $Y_i(\mathbf{d})$ , where  $\mathbf{d}$  describes all of the treatments administered to all subjects, to a much simpler schedule  $Y_i(d)$ , where  $d$  refers to the treatment administered to subject  $i$ .
- Implies that so long as a subject's treatment status remains constant, that subject's outcome is unaffected by the particular way in which treatments happened to be assigned to other subjects

# Spillover

Bias Introduced by Spillovers

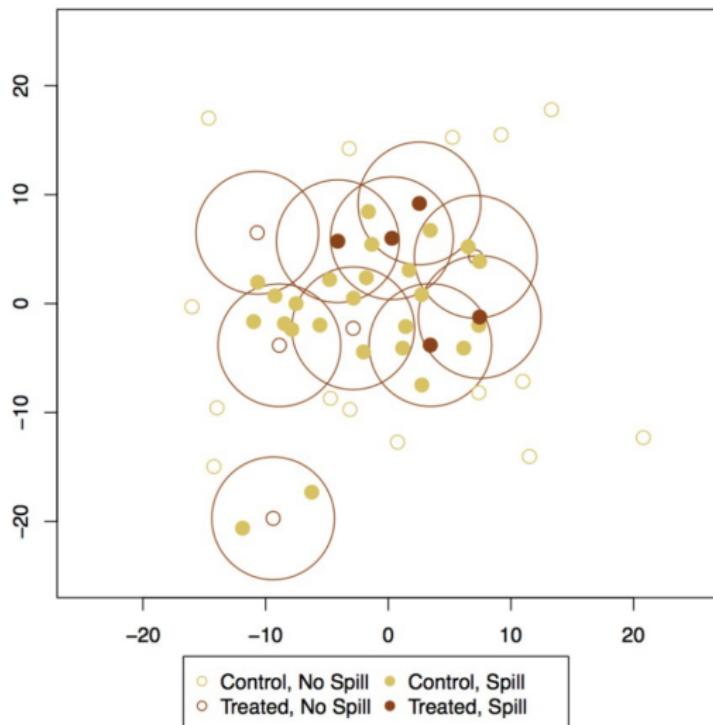


# Estimating Spillover Effects

- $Y_{00} \equiv Y(Z_i = 0, Z_j = 0)$ : Pure Control
- $Y_{10} \equiv Y(Z_i = 1, Z_j = 0)$ : Directly treated, no spillover
- $Y_{01} \equiv Y(Z_i = 0, Z_j = 1)$ : Untreated, with spillover
- $Y_{11} \equiv Y(Z_i = 1, Z_j = 1)$ : Directly treated, with spillover
- We assume...
  - treatment assignments of non-neighboring units do not alter a unit's potential outcomes
  - model spillovers as a binary event: either some neighboring unit is treated, or not

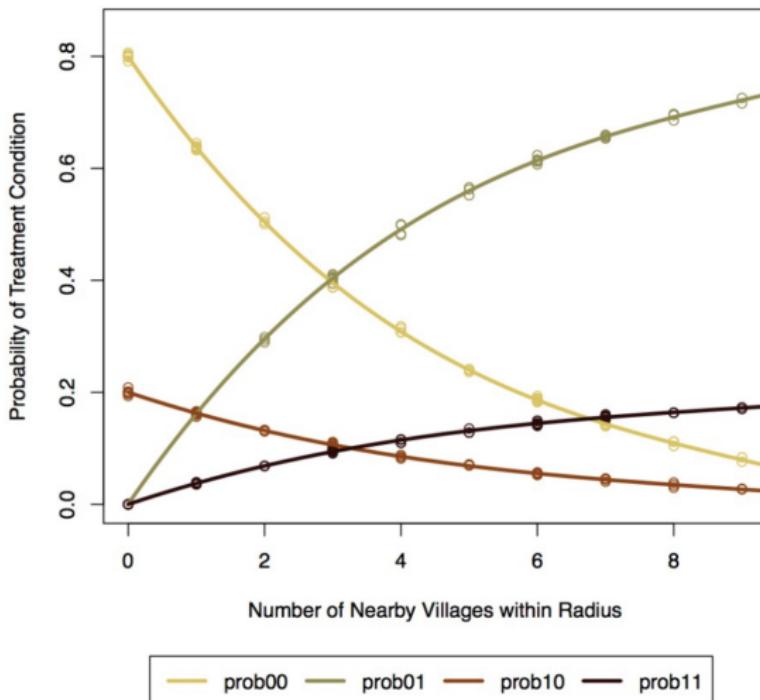
# Spillover

Geographic Spillovers with Radius = 5km



# Spillover

Probabilities of Assignment Vary with Geographic Location  
10 of 50 villages are treated



```
require(ggplot2)

# Define two helper functions
complete_ra <- function(N,m){
  assign <- ifelse(1:N %in% sample(1:N,m),1,0)
  return(assign)
}

get_condition <- function(assign , adjmat){
  exposure <- adjmat %*% assign
  condition <- rep("00" , length(assign))
  condition [assign==1 & exposure==0] <- "10"
  condition [assign==0 & exposure>0] <- "01"
  condition [assign==1 & exposure>0] <- "11"
  return(condition)
}
```

```

N <- 50 # total units
m <- 10 # Number to be treated
# Generate adjacency matrix
set.seed(343)
coords <- matrix(rnorm(N*2)*10, ncol = 2)
distmat <- as.matrix(dist(coords))
true_adjmat <- 1 * (distmat<=5) # true radius = 5
diag(true_adjmat) <- 0

# Run simulation 10000 times
Z_mat <- replicate(10000, complete_ra(N = N, m = m)
)
cond_mat <- apply(Z_mat, 2, get_condition, adjmat=
    true_adjmat)
# Calculate assignment probabilities
prob00 <- rowMeans(cond_mat=="00")
prob01 <- rowMeans(cond_mat=="01")
prob10 <- rowMeans(cond_mat=="10")
prob11 <- rowMeans(cond_mat=="11")

```

```
# calculate number of villages
vil_sum = rowSums(true_adjmat)
probs = cbind(prob00, prob01, prob10, prob11, vil_
sum)
probs2 = as.data.frame(probs)
probs2 = as.matrix(probs2)
probs3 = rbind(probs2[,c(1,5)], probs2[,c(2,5)],
probs2[,c(3,5)], probs2[,c(4,5)])
probs3 = as.data.frame(probs3)
probs3$ProbCat = c(rep("prob00",50), rep("prob01",
50), rep("prob10",50), rep("prob11",50))
probs3$ProbCat = factor(probs3$ProbCat)
p = ggplot(probs3, aes(y = prob00, x=vil_sum))
p
p = p + geom_point()
p
p = p + geom_point(aes(colour = ProbCat))
p
p = p + labs(x = "Number_of_Nearby_Villages_within_ 51
Radius")
```

## Social processes that imply spillovers (from Green)

- *Diffusion*: Interventions that convey information about commercial products or political causes may spread from individuals who receive the treatment to others who are nominally untreated.
- *Displacement*: Police interventions designed to suppress crime in one location may displace criminal activity to nearby locations.
- *Social comparison*: An intervention that offers housing assistance to a treatment group may change how the control group evaluates their own housing conditions.
- *Persistence and memory*: Within-subjects experiments, in which outcomes for a given unit are tracked over time, may involve “carryover” or “anticipation.”

# Why should we care about spillovers?

- Biased estimation of causal effects
  - Example: Understatement of average causal effects when an information treatment spreads to the control group
  - Example: Exaggeration of average causal effects when a crime prevention program causes displacement
- Mistaken policy inferences: Amplification of cumulative policy impact if the behavior encouraged by an intervention causes other individuals to change their behavior.
  - Example: Vaccine incentives to one individual encourages others to get vaccinated

## Designs to detect spillovers

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- Simply measure outcomes for non-experimental units within social networks
  - Example: Spouses of those who receive health interventions (Fletcher and Marksteiner 2017 AEJ)
  - Extensions to placebo-control designs for media interventions involving noncompliance (Green et al. 2020 JRSS-A)
- Target specific nodes in a social network
  - Example: Bullying in 56 high schools (Paluck et al. 2016 PNAS)
  - Example: Encouraging subjects' neighbors or housemates to vote (Sinclair et al. 2012 AJPS)

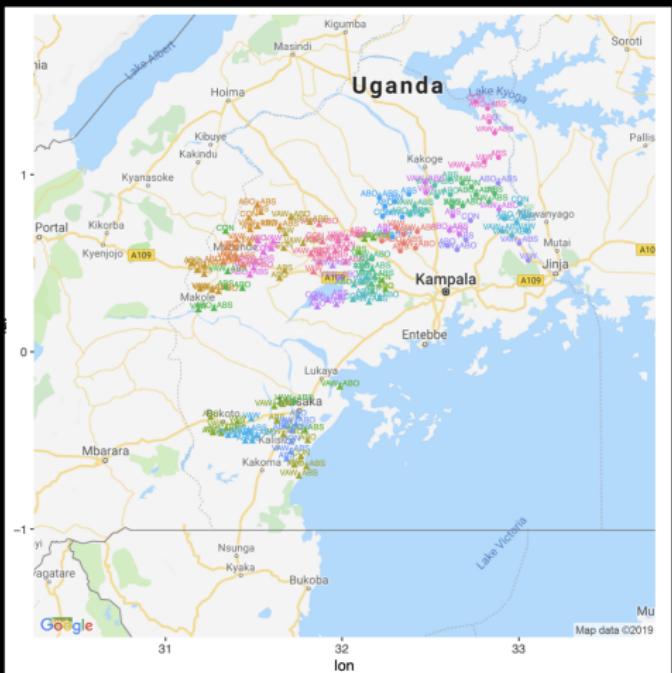
## Design 1: Measure outcomes for non-experimental units within social networks

---

- Uganda film festival experiment: placebo-controlled messages during commercial breaks deployed in 168 rural villages
- Messages concerned violence against women (VAM), teacher absenteeism, or abortion stigma
- Surveys of random samples of villagers conducted 2 months later to measure outcomes
- Compare nonviewers in treatment or placebo villages in order to assess spillovers

## Treatment assignment, by RCT and block

- Round 1
- ▲ Round 2



## Storylines from the three three-part vignettes



Violence against  
women (two versions)

Abortion stigma



However so many of us continue to harshly judge  
the girls.



We parents will make sure to do what we can  
to resolve this.

Teacher absenteeism

# Uganda Wilke et al 2020

<i>Dependent variable:</i>				
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
VAW	0.046** (0.015)	0.004 (0.012)	-0.0002 (0.014)	0.003 (0.010)
Control Mean	0.38	0.38	0.38	0.38
Vill. Means	0.38	0.37	0.37	0.38
Vill. SD	0.09	0.07	0.09	0.06
N Vill.	110	110	110	110
Block FE	Yes	Yes	Yes	Yes
Observations	1,154	2,441	1,918	4,359
Adjusted R <sup>2</sup>	0.011	0.005	0.013	0.006

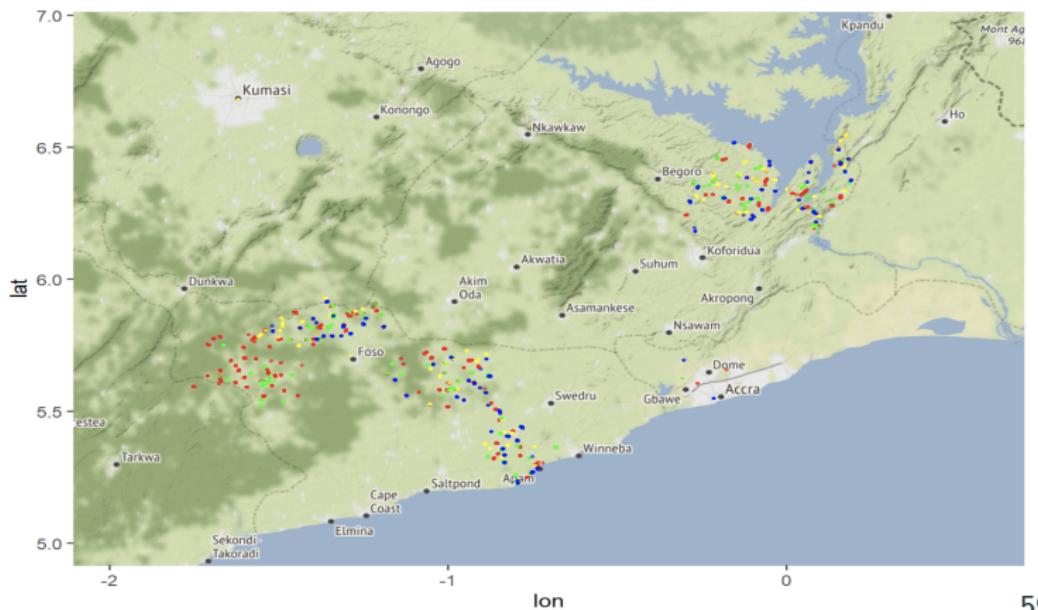
Notes:

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

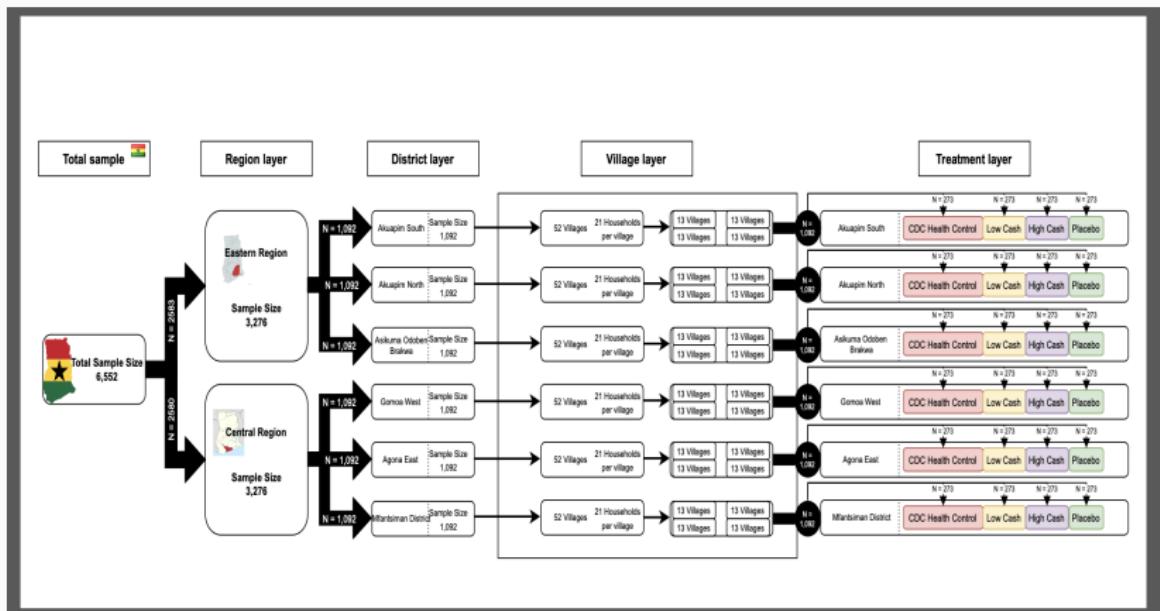
Table 5: Direct effects and spillovers from anti-VAW messages among all respondents in endline surveys following 2016 festival.

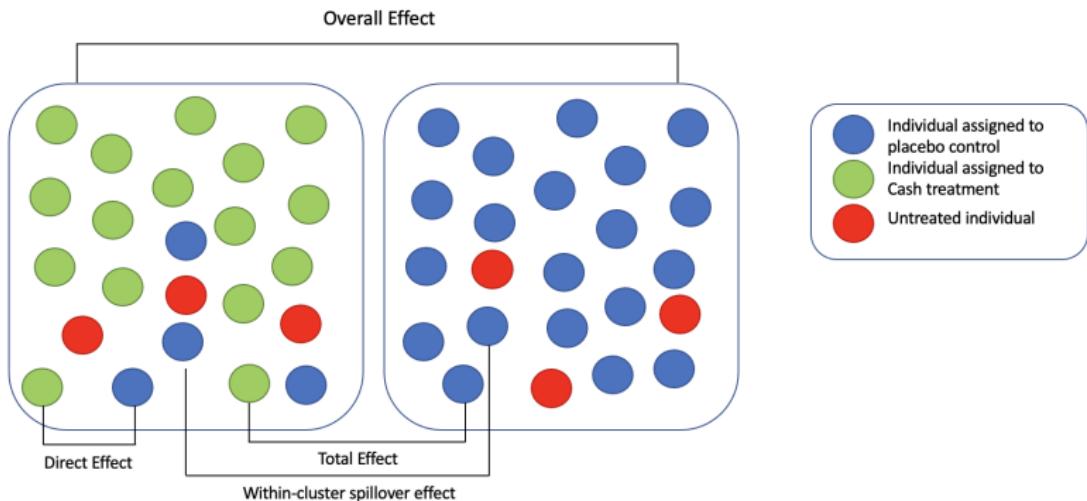
Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed *p*-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

# Geographic Treatment Mapping



# Duch et al 2023 Ghana

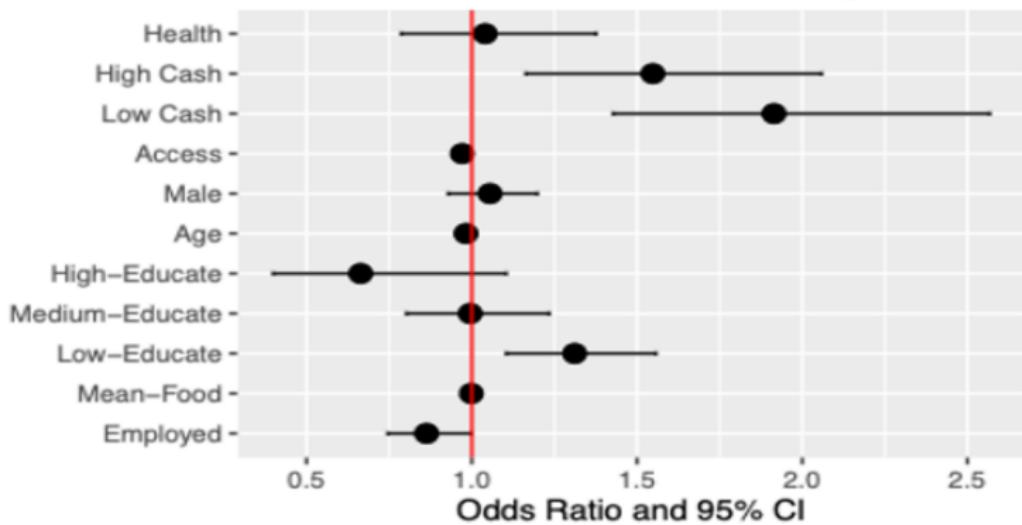




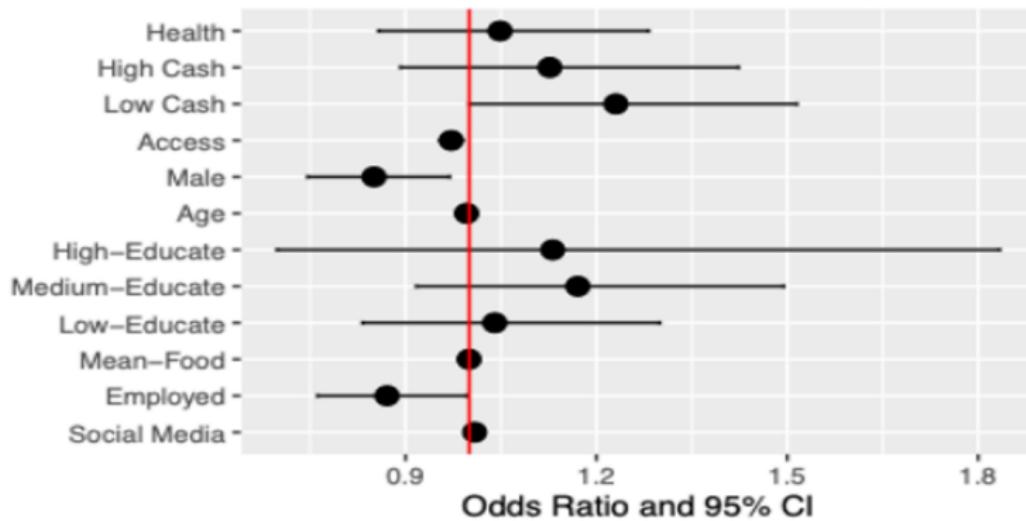
- two “embedded placebo” dummy variables:  
HealthPlacebo and CashPlacebo
- test whether the 25% of Placebo subjects in villages assigned the Health or Cash treatments have vaccination rates that are different than those for subjects in the villages assigned to the Placebo treatment
- expectation: overall average vaccination rates will be depressed for the 25% in the Cash treated villages – hence the expectation that  $\beta_4 < 0$

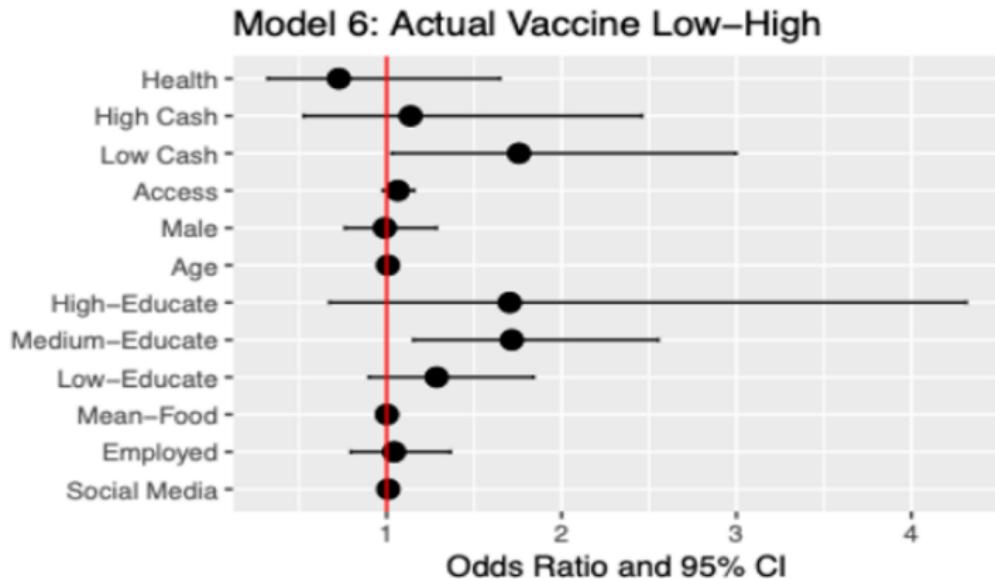
$$\text{Vaccinated}_{ic} = \beta_0 + \beta_1 \text{Health}_{ic} + \beta_2 \text{HealthPlacebo}_{ic} + \\ \beta_3 \text{Cash}_{ic} + \beta_4 \text{CashPlacebo}_{ic} + \omega \mathbf{X}_{ic} + \epsilon_{ic}(2)$$

## Model 2: Vaccine Intention Low–High



## Model 4: Reported Vaccine Low–High





	Vaccine Intention	Vaccine Reported	Actual Vaccine
Cash	1.82 (1.38, 2.39)	1.18 (0.95, 1.47)	1.64 (0.79, 3.41)
Health	1.10 (0.79, 1.52)	1.05 (0.83, 1.34)	0.81 (0.29, 2.26)
Cash_Placebo	1.19 (0.86, 1.64)	1.10 (0.84, 1.44)	1.28 (0.60, 2.70)
Health_Placebo	1.00 (0.66, 1.51)	0.83 (0.59, 1.19)	1.02 (0.45, 2.30)

	Vaccine Intention	Vaccine Reported	Actual Vaccine	Vaccine Reported SP	Actual Vaccine SP
Cash	1.82 (1.38, 2.39)	1.18 (0.95, 1.47)	1.64 (0.79, 3.41)		
Health	1.10 (0.79, 1.52)	1.05 (0.83, 1.34)	0.81 (0.29, 2.26)		
Cash_Placebo	1.19 (0.86, 1.64)	1.10 (0.84, 1.44)	1.28 (0.60, 2.70)		
Health_Placebo	1.00 (0.66, 1.51)	0.83 (0.59, 1.19)	1.02 (0.45, 2.30)		
VCash_cash				1.16 (0.79, 1.69)	1.39 (0.62, 3.11)
VCash_Health				0.96 (0.63, 1.46)	0.61 (0.23, 1.61)

# Duch et al 2023 Ghana

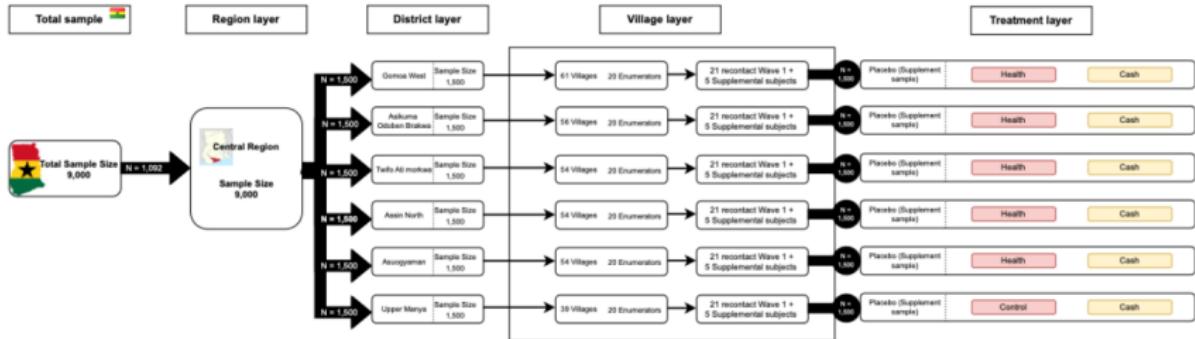
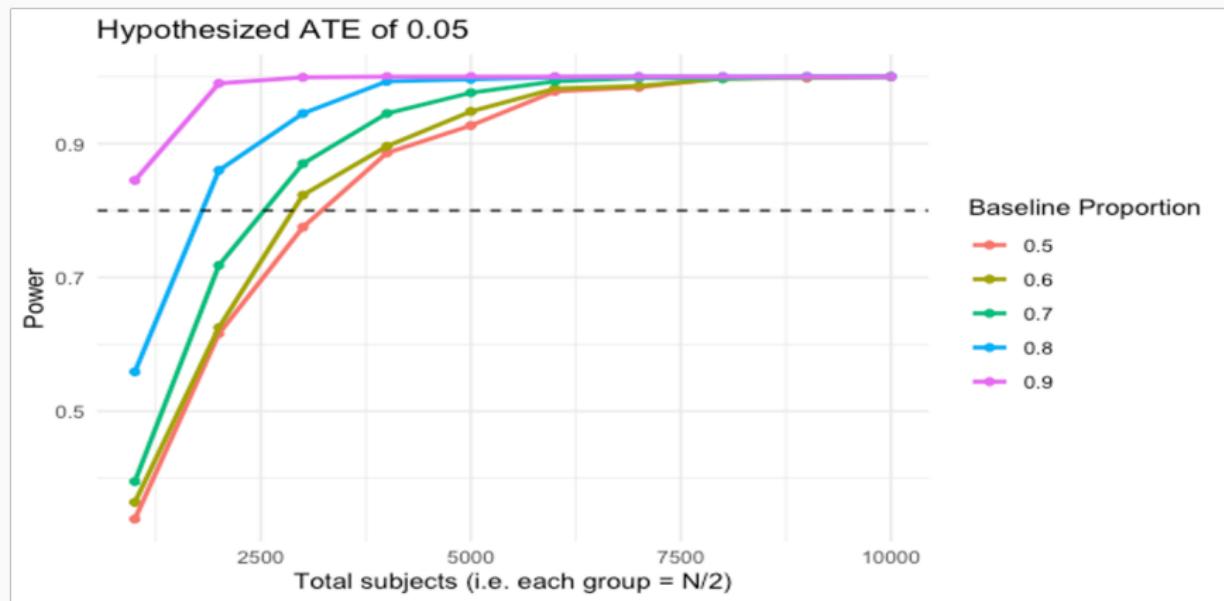


Figure 1: Experiment Flow Chart

Table 1: Wave I and Wave II Sample Description

	Wave I Treated	Wave I Post-Treatment	Wave II Health II	Wave II Cash II	Wave II Placebo	Wave II Total
Placebo I	2,737	1,894	947	947		1,894
Health I	1,063	765	383	382		765
Cash I	2,326	1,589	794	795		1,589
Spillover I		1,101	550	551		1,101
Placebo II					1,650	1,650
Total	6,126	4,248	2,674	2,675	1,650	6,999



# Duch et al 2023 Ghana

Table 1: Financial Incentive Treatment Effects on Vaccination Intentions

	<i>Dependent variable:</i>		
	Intention to Vaccinate		
	(1)	(2)	(3)
Village Health	0.009 (-0.022, 0.040)		0.006 (-0.039, 0.050)
Village High Cash	0.054*** (0.023, 0.085)		0.031 (-0.015, 0.077)
Village Low Cash	0.064*** (0.032, 0.095)		-0.025 (-0.065, 0.015)
Subject Health		0.015 (-0.015, 0.046)	0.010 (-0.036, 0.057)
Subject Low Cash		0.119*** (0.090, 0.148)	0.139*** (0.101, 0.177)
Subject High Cash		0.069*** (0.039, 0.100)	0.039 (-0.009, 0.087)
Constant	0.720*** (0.698, 0.742)	0.712*** (0.696, 0.728)	0.711*** (0.689, 0.733)
Observations	5,938	5,947	5,938
R <sup>2</sup>	0.004	0.012	0.013
Adjusted R <sup>2</sup>	0.004	0.011	0.012
Residual Std. Error	0.432 (df = 5934)	0.430 (df = 5943)	0.430 (df = 5931)
F Statistic	8.009*** (df = 3; 5934)	23.933*** (df = 3; 5943)	12.602*** (df = 6; 5931)

Note:

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

# Homework Exercise Lecture 3

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Prepare a simple R script illustrating how to incorporate the distance between control and treatment units in the estimation of treatment effects.