Experimental Methods: Lecture 1

Causal Inference and Alternative Designs

Raymond Duch

April 26, 2022

DPIR University of Oxford

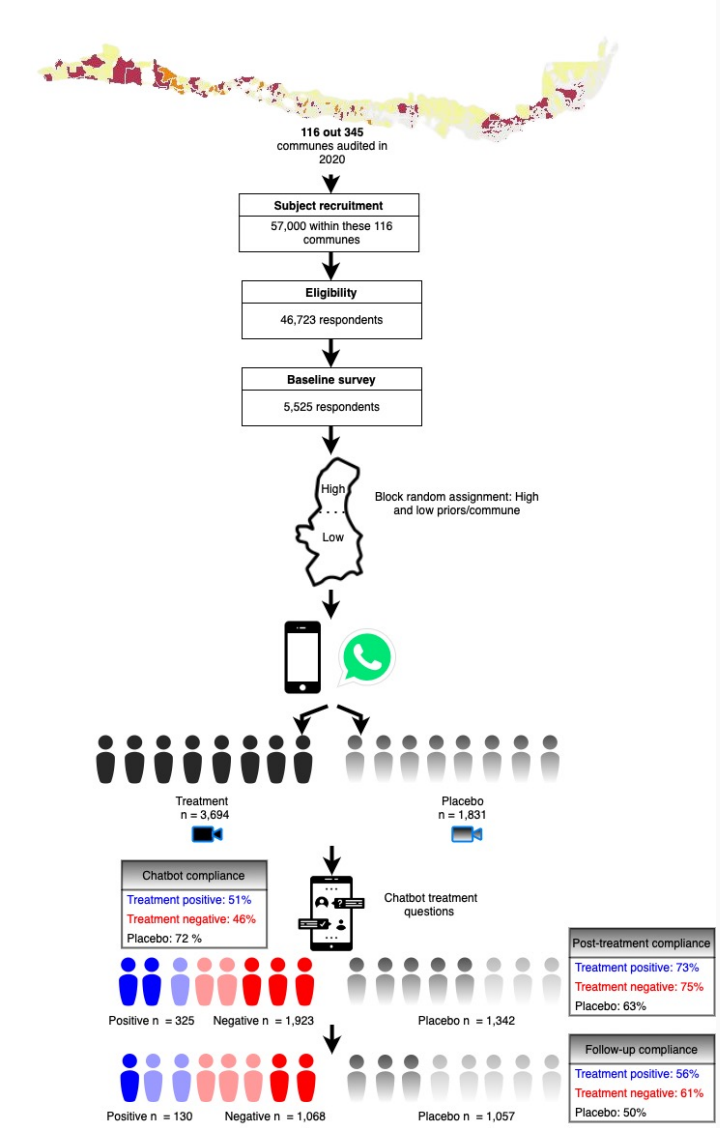
Road Map to Lecture 1

- Experiments illustrated
- Potential outcomes and causal inference
- Average Treatment Effects (ATE)
- Alternative designs
    - Within and Between Subject
    - Multiple Arms

Why Should We Do Experiments? I

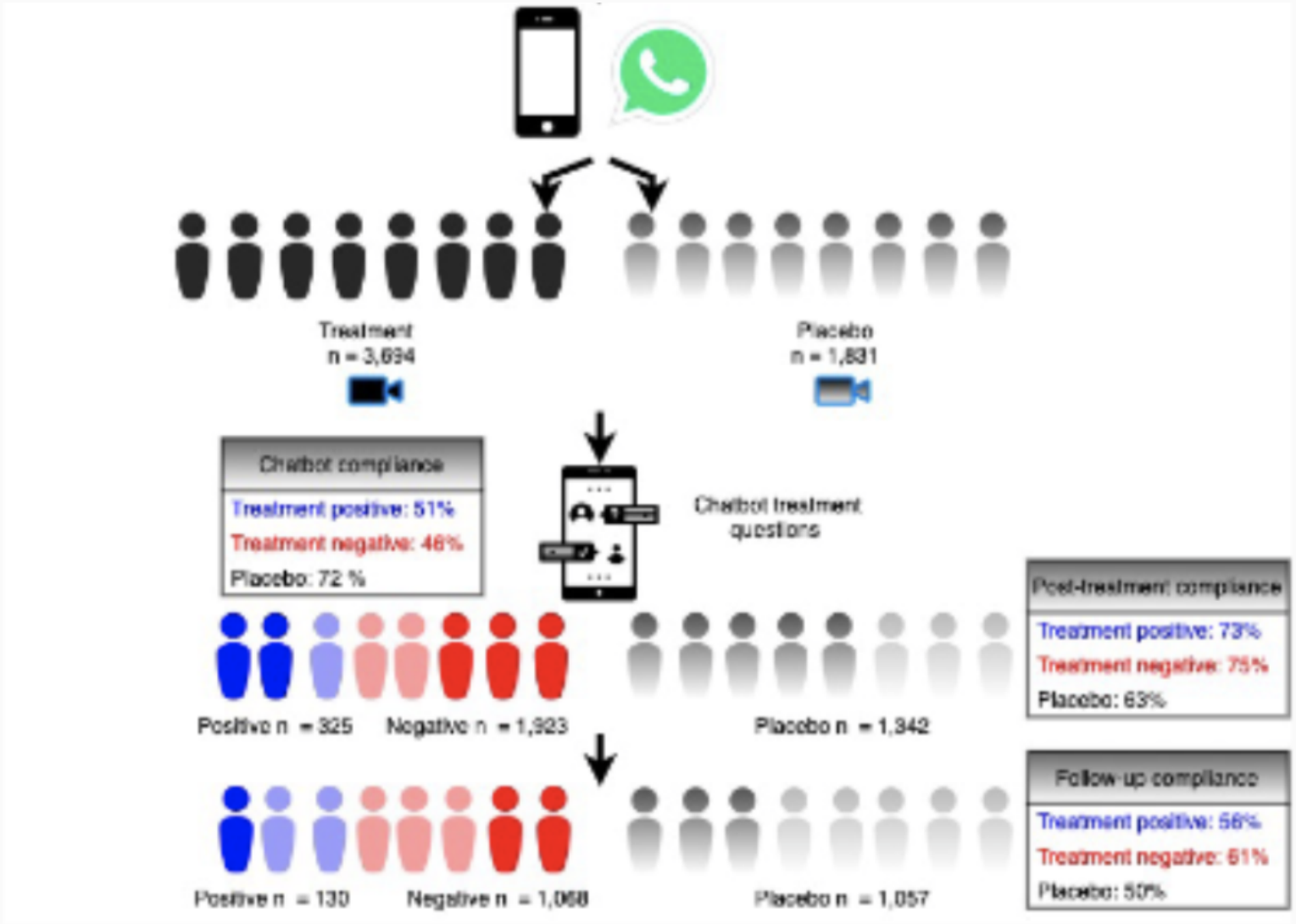
- Duch Torres (2022) Government Audits of Municipal Corruption and Belief Updating.
- Do individuals update their beliefs about corruption when informed about audit results for their local governments?
- Does corruption information cause belief updating?
  - Random assignment to audit information
  - Measure beliefs about corruption in local government.

Duch and Torres 2022

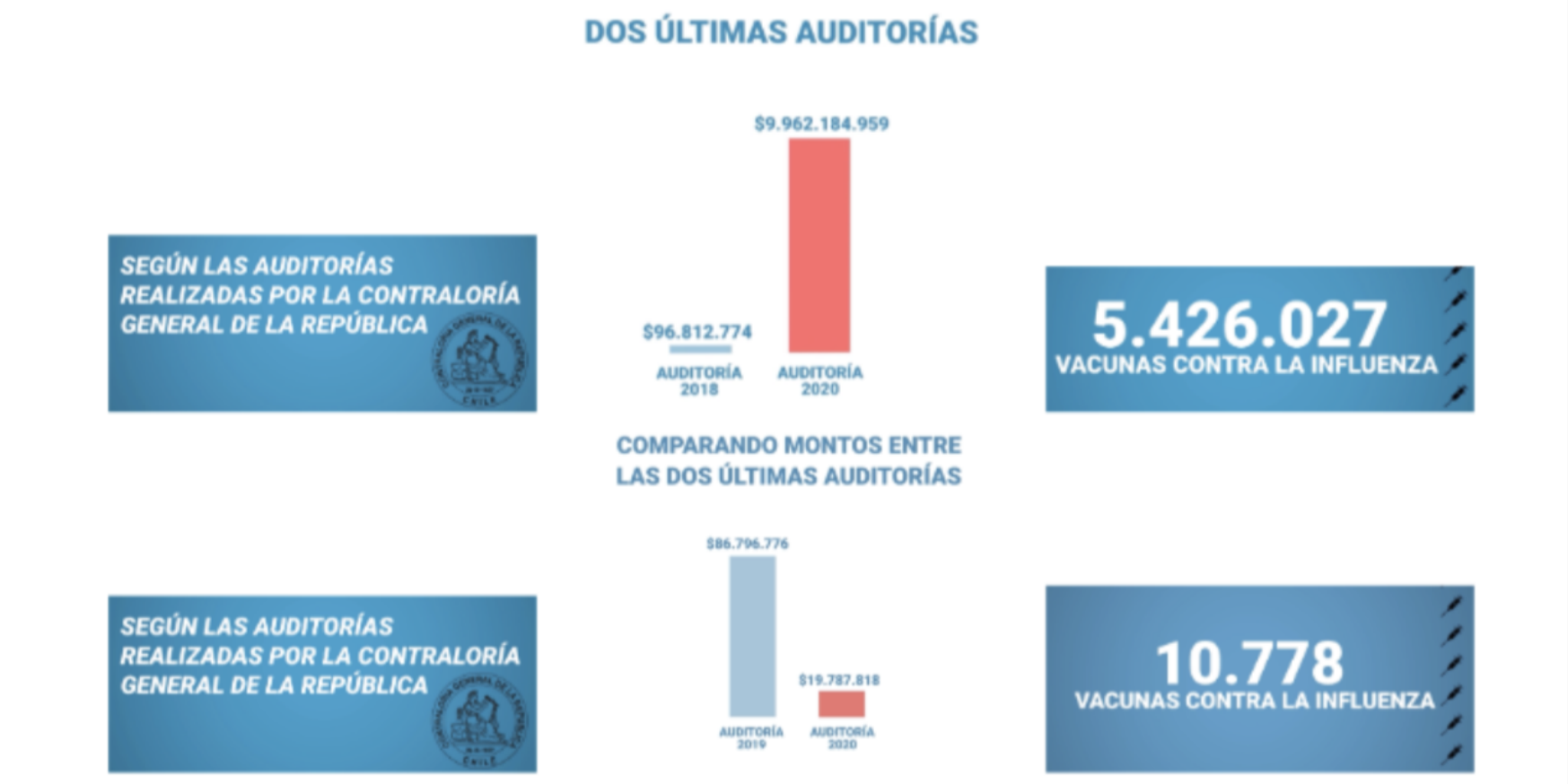


Within Subject Design

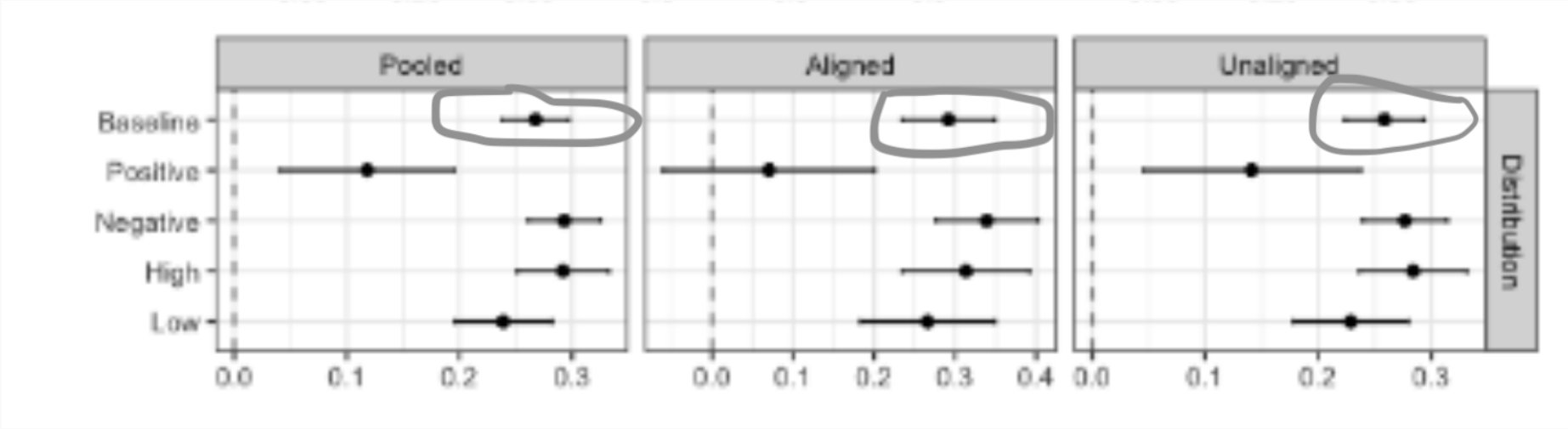
Random Assignment One Treatment Arm



Treatment


DOS ÚLTIMAS AUDITORÍAS
$9.962.184.959
$96.812.774
AUDITORÍA 2018
AUDITORÍA 2020
SEGÚN LAS AUDITORÍAS REALIZADAS POR LA CONTRALORÍA GENERAL DE LA REPÚBLICA
5.426.027
VACUNAS CONTRA LA INFLUENZA
COMPARANDO MONTOS ENTRE LAS DOS ÚLTIMAS AUDITORÍAS
$86.796.776
$19.787.818
AUDITORÍA 2019
AUDITORÍA 2020
SEGÚN LAS AUDITORÍAS REALIZADAS POR LA CONTRALORÍA GENERAL DE LA REPÚBLICA
10.778
VACUNAS CONTRA LA INFLUENZA

Average Within Subject Change



Average Treatment Effect

	Baseline
Intercept	1.122***
	(0.153)
Prior	0.421***
	(0.024)
Treat	0.347***
	(0.028)
Negative	
Log malfeasance	
Corrup diff	
Treat x Negative	
Treat x Log malfeasance	
Treat x Corrup diff	
Covariates	Yes
Num.Obs.	3439
R2	0.203
R2 Adj.	0.196

Why Should We Do Experiments? II

- Lauren E. Young (2019) The Psychology of State Repression: Fear and Dissent Decisions in Zimbabwe. *APSR* 113(1):140–155.
- What is the effect of the emotion of fear on citizen dissent in autocracies?
- How would we demonstrate that the effect of fear on dissent is unconfounded by other variables?
    - Characteristics that induce emotions
    - New information about a threat

Why Should We Do Experiments? II

- Lab-in-the-field experiment in Harare, Masvingo and Manicaland provinces in Zimbabwe
- Random assignment to treatment or control: Affective emotional memory task (AEMT)
- Ethical implications

Why Should We Do Experiments? II

- Sample: 647 participants from six communities in Zimbabwe where the NGO Voice for Democracy (VfD) has a network of mobilizer and informants; and affected by state-sponsored violence
    - Treatment 1: Enumerator asks to describe a situation of fear *around politics and elections* [political fear]
    - Treatment 2: Enumerator asks to describe a situation of *general fears other than politics or elections* [general fear]
    - Control: Enumerator asks to describe a situation that makes them relaxed
- Outcome: Propensity to dissent: hypothetical (via index) and behavioral (via selection of political wristband)

Why Should We Do Experiments? II

- Young (2019)

TABLE 3. The Fear Treatments Reduce Dissent

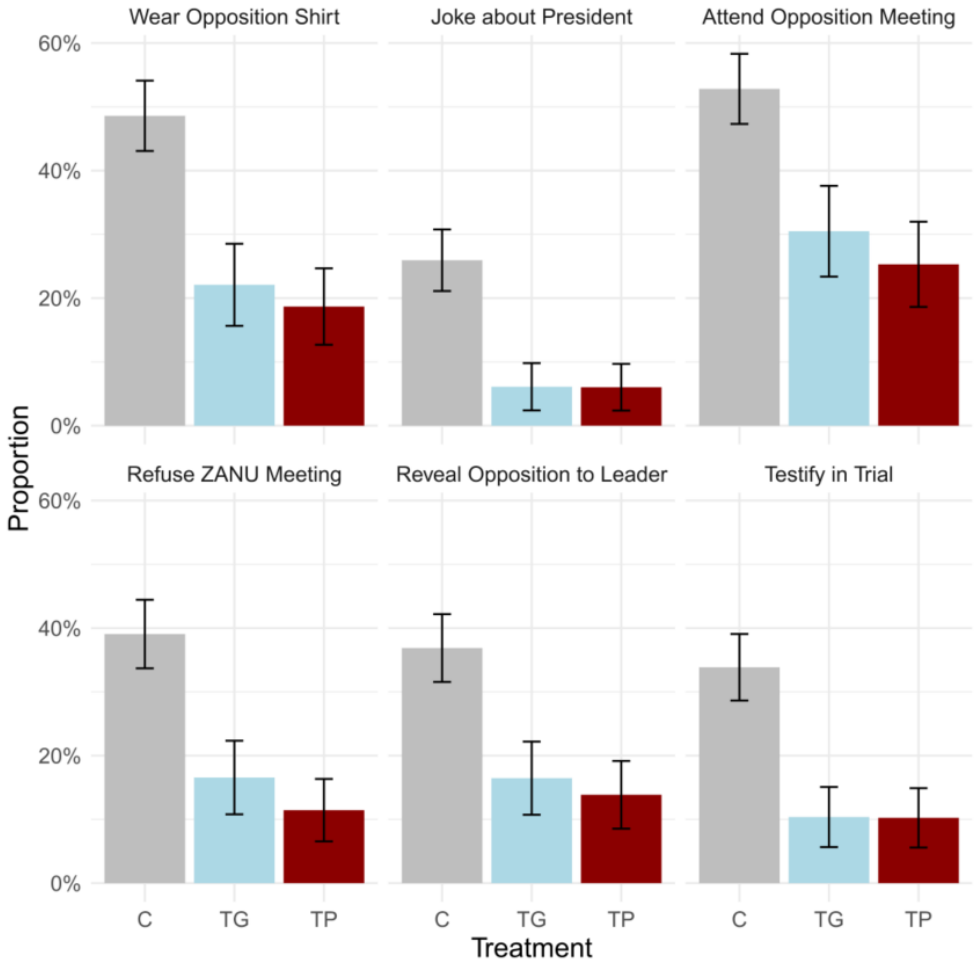
	Hypothetical		Behavioral	
	General fear (1)	Political fear (2)	General fear (3)	Political fear (4)
ATE[1]	−0.545	−0.773	−0.104	−0.189
SE[2]	(0.077)	(0.080)	(0.050)	(0.053)
RI *p*-value[3]	<0.001	<0.001	0.035	<0.001
N	484	486	329	326
Sample	All		Wristband[4]	

[1]The first row presents the estimated average treatment effects (ATEs) of the general and political fear treatments on the hypothetical measure of propensity to dissent in columns 1 and 2, and the behavioral measure in columns 3 and 4. ATEs are calculated based on assignment to treatment and weighted by inverse propensity scores by block.
[2]Robust standard errors (SEs) from linear regression analysis.
[3]The *p*-value is based on a two-tailed test using randomization inference.
[4]The estimate of the treatment effect on the wristband measure comes from the subset of the sample respondents who were offered a choice between two real wristbands. Results are similar for the full sample.

Why Should We Do Experiments? V

- Young (2019)

FIGURE 2. The Fear Treatments Cause Substantively Large Increases in the Proportion of Respondents Who Are Very Likely or Sure to Dissent During an Election Period



Defining Treatment

- The variable d_i indicates whether the ith subject is treated
- In the typical case of binary treatments, $d_i = 1$ means the ith subject receives the treatment
- $d_i = 0$ means the ith subject does not receive the treatment
- It is assumed that d_i is observed for every subject

Potential Outcomes

- Y_i: the potential outcome for subject i
- $Y_i(d_i)$: the outcome for subject i, written as a function of the treatment i received; it is generally the case that we observe only one of the potential outcomes for each i
- For the binary-valued treatment, there are two "potential outcomes":
  - $Y_i(1)$, the potential outcome for i conditional on i being treated
  - $Y_i(0)$, the potential outcome for i conditional on i not being treated

Potential Outcome Schedule

- "Hypothetical"
- Comprehensive list of potential outcomes for all subjects
- Rows of this schedule are indexed by i, and the columns are indexed by d
- Potential outcomes for the fifth subject may be found in adjacent columns of the fifth row

Treatments as random variables

- Note on notation: We distinguish between d_i, the treatment that a given subject actually receives, and D_i, the treatment that could be administered hypothetically.
- D_i is a random variable (the ith subject might be treated in one hypothetical study and not in another).
- $Y_i(0)|D_i = 1$: untreated potential outcome for subjects that would receive the treatment under a hypothetical random assignment.
- We use D_i when talking about the statistical properties of treatments.

Chattopadhyay & Duflo 2004

- Randomized policy experiment in India
- 1990s, one-third of village council heads reserved for women
- women.csv contains subset of data from West Bengal
- Gram Panchayat (GP) = level of government
- Analysis?
    - Was randomization implemented properly?
    - Conjecture: more drinking facilities under women
    - Conjecture: no effect on irrigation

Potential Outcomes Local Budget

	Budget share if village head is male	Budget share if village head is female	Treatment Effect
Village 1	10	15	5
Village 2	15	15	0
Village 3	20	30	10
Village 4	20	15	-5
Village 5	10	20	10
Village 6	15	15	0
Village 7	15	30	15
Average	15	20	5

Potential Outcome Subgroup

- Sometimes useful to refer to potential outcomes for a subset of the subjects
- Expressions of the form $Y_i(d)|X = x$ denote potential outcomes when the condition $X = x$ holds
- For example, $Y_i(0)|d_i = 1$ refers to the untreated potential outcome for a subject who actually receives the treatment

Conditional potential outcomes

- $Y_i(0)|d_i = 1$: untreated potential outcome for subjects that receive treatment
- $Y_i(0)|d_i = 0$: untreated potential outcome for subjects that do not receive treatment
- $Y_i(1)|d_i = 1$: treated potential outcome for subjects that receive treatment
- $Y_i(1)|d_i = 0$: treated potential outcome for subjects that do not receive treatment

Individual Level Causal Effect

- For subject i, the effect of the treatment is conventionally defined as the difference between outcomes across the two potential outcomes:

$$\delta_i = Y_i(1) - Y_i(0)$$

- Alternatively:

$$Y_i = Y_i(0) + (Y_i(1) - Y_i(0))D_i$$

- Often referred to as the Rubin causal model; perhaps more appropriately, the Neyman-Holland-Rubin causal model
- **The Fundamental Problem of Causal Inference** only one of the two potential outcomes is realized, so that δ_i is typically non-operational

Realized Potential Outcomes

- Use lower-case letters for realization of the potential quantities (again, typically only one of the two potential outcomes is realized)
  1. $y_i(1)$, the outcome observed for i conditional on $d_i = 1$ (i is treated)
  2. $y_i(0)$, the outcome observed for i conditional on $d_i = 0$ (i is not treated)

The Fundamental Problem of Causal Inference

Table 1: Table 2.1, p35 Morgan and Winship, *Counterfactuals and Causal Inference*

Group	$Y_i(1)$	$Y_i(0)$
Treatment ($D_i = 1$)	**Observable**	Counterfactual
Treatment ($D_i = 0$)	Counterfactual	**Observable**

Observed Outcomes

- The connection between the observed outcome and the underlying potential outcome is given by the equation $Y_i = d_i Y_i(1) + (1 - d_i) Y_i(0))$
- This equation indicates that the $Y_i(1)$ are observed for subjects who are treated, and the $Y_i(0)$ are observed for subjects who are not treated
- For any given subject, we observe either $Y_i(1)$ or $Y_i(0)$, not both

Observed Outcomes Local Budget

	Budget share if village head is male	Budget share if village head is female
Village 1	?	15
Village 2	15	?
Village 3	20	?
Village 4	20	?
Village 5	10	?
Village 6	15	?
Village 7	?	30

Average Treatment Effect

- Average Treatment Effect:

$$E(\delta) = E[Y(1)] - E[Y(0)] = E[Y(1) - Y(0)]$$

- where the expectation is over a population, and so no subscript i
- This is operational, in that we can compute sample estimates of E[Y(1)] and E[Y(0)]: e.g., the sample averages:

$$\hat{y}(1) = \frac{1}{n_1} \sum_{i:d_i=1} y_i(1) \text{ and } \hat{y}(0) = \frac{1}{n_0} \sum_{i:d_i=0} y_i(0)$$

- where n_1 and n_0 are the number of subjects in groups $d(1)$ and $d(0)$ respectively

Randomization Generates Unbiased Estimates of Average Treatment Effect

- Rubin (1974) calls this:

$$\begin{aligned}\hat{\delta} &= \hat{y}(1) - \hat{y}(0) \\ &= \hat{y}_d\end{aligned}$$

- Under certain circumstances, this is an unbiased estimate of the population average treatment effect δ
- Why? How?
- Nice, informal treatment in "Two Formal Benefits of Randomization"

Properties of Random Assignment

- Under equal probability random assignment, the conditional ATE among the treated is the same as the conditional ATE among the control group, which is therefore the same as the ATE
- The expected $Y_i(0)$ in the treatment group is the same as the expected $Y_i(0)$ in the control group
- When random assignment is not used (i.e., observational research), the unbiasedness of the difference-in-means estimator becomes a matter of conjecture

Potential Outcomes: Core Assumptions

- We assume that each subject has two potential outcomes $Y_i(1)$ if treated and $Y_i(0)$ if not treated
- Each potential outcome depends **solely** on whether the subject **itself** receives the treatment
- Potential outcomes respond only to the treatment and not to some other feature of the experiment - such as assignment or measurement

The Beauty of Randomization: Independence

- Treatment status is statistically independent of potential outcomes and background attributes $\mathbf{X}$

$$Y_i(0), Y_i(1), \mathbf{X} \perp\!\!\!\perp D_i$$

- If a subject is randomly assigned to treatment, knowing wheether a subject is treated provides no information about the subject's potential outcomes, or background attributes.

Exclusion restriction

- Let $Y_i(z, d)$ be the potential outcome when $z_i = z$ and $d_i = d$ for $z \in (0, 1)$ and for $d \in (0, 1)$
- For example, if $z_i = 1$ and $d_i = 1$, the subject is assigned to the treatment group and receives the treatment
- Or $z_i = 1$ and $d_i = 0$ - subject is assigned treatment but does not receive treatment
- The exclusion restriction is that $Y_i(1, d) = Y_i(0, d)$ - subjects only respond to input from d_i
- The excludability assumption cannot be verified empirically because we never observe both and for the same subject

Classic Drug Experiment Example

- Treatment group receives a new drug
- Control group receives nothing
- Experiment confounds this treatment with receipt of a pill
- If patients respond to the pill rather than the pill's ingredients, excludability is violated
- Jeopardizes unbiasedness of the difference-in-means estimator

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

Non-interference violated

- Police patrols displace crime from treated to untreated areas
- Non-interference violated if your estimand is following:
    - Average potential outcome when a block is treated minus average potential outcome when no blocks are treated
- If police patrols displace crime from treated to untreated areas, observed outcomes in control will not be potential outcomes when no treatment administered anywhere
- Estimated ATE will tend to exaggerate the true ATE

Core assumptions violated?

- Public Health: Providing an infectious disease vaccine to some individuals may decrease the probability that nearby individuals become ill
- Politics: Election monitoring at some polling stations may displace fraud to neighboring polling stations
- Economics: Lowering the cost of production for one firm may change the market price faced by other firms
- Marketing: Advertisements displayed to one person may increase product recognition among her work colleagues

Estimation

Difference-in-means is an unbiased estimator of ATE

$$
\begin{aligned}
E\left[\frac{\sum_1^m Y_i}{m} - \frac{\sum_{m+1}^N Y_i}{N-m}\right] &= E\left[\frac{\sum_1^m Y_i}{m}\right] - E\left[\frac{\sum_{m+1}^N Y_i}{N-m}\right] \\
&= E[Y_i(1)] - E[Y_i(0)] \\
&= E[\tau_i] = ATE
\end{aligned}
$$

Estimation

Difference-in-means estimator implemented via OLS

$$
\begin{aligned}
Y_i &= Y_i(0)(1 - d_i) + Y_i(1)d_i \\
&= Y_i(0) + (Y_i(1) - Y_i(0))d_i \\
&= \mu_{Y(0)} + [\mu_{Y(1)} - \mu_{Y(0)}]d_i + Y_i(0) - \mu_{Y(0)} \\
&+ [(Y_i(1) - \mu_{Y(1)}) - (Y_i(0) - \mu_{Y(0)})]d_i \\
&= \alpha + \beta d_i + \epsilon_i,
\end{aligned}
$$

$\alpha = \mu_{Y(0)}$ (average of untreated potential outcomes for all N),
$Y_i(0) =$ untreated potential outcome
$\beta = \mu_{Y(1)} - \mu_{Y(0)}$
and ϵ_i comprises idiosyncratic variation in untreated responses plus idiosyncratic variation in treatment effects.

Chattopadhyay & Duflo 2004

- Randomized policy experiment in India
- 1990s, one-third of village council heads reserved for women
- women.csv contains subset of data from West Bengal
- Gram Panchayat (GP) = level of government
- Analysis?
    - Was randomization implemented properly?
    - Conjecture: more drinking facilities under women
    - Conjecture: no effect on irrigation

Name	Description
GP	An identifier for the Gram Panchayat (GP)
village	identifier for each village
reserved	binary variable indicating whether the GP was reserved for women leaders or not
female	binary variable indicating whether the GP had a female leader or not
irrigation	variable measuring the number of new or repaired irrigation facilities in the village since the reserve policy started
water	drinking-water facilities in the village since the reserve policy started

Table 4.6: *The Variable Names and Descriptions of the Women as Policy Makers Data.*

```
women <- read.csv("women.csv")

##proportion of female politicians in
    reserved GP vs. unreserved GP
mean(women$female[women$reserved] == 1)
[1] 1
mean(women$female[women$reserved == 0])
[1] 0.07476636
```

```
## drinking-water facilities
mean(women$water[women$reserved == 1]) -
    mean(women$water[women$reserved ==
        0])

## [1] 9.25223

## irrigation facilities
mean(women4irrigation[women$reserved ==
    1]) -
    mean(women$irrigation[women$
        reserved == 0])

## [1] -0.3693319
```

Randomizr in R

```
library(randomizr)
# Load built-in dataset
data(HairEyeColor)
HairEyeColor <- data.frame(HairEyeColor)

# Transform so each row is a subject
# Columns describe subject's hair color,
    eye color, and gender
hec <- HairEyeColor[rep(1:nrow(HairEyeColor
    ),
                            times =
                                HairEyeColor$
                                Freq), 1:3]


N <- nrow(hec)
```

```r
# Fix the rownames
rownames(hec) <- NULL

# Set a seed for reproducability
set.seed(343)

# Create untreated and treated outcomes for all
    subjects
hec <- within(hec,{
  Y0 <- rnorm(n = N,mean = (2*as.numeric(Hair) + -4
      *as.numeric(Eye) + -6*as.numeric(Sex)), sd =
      5)
  Y1 <- Y0 + 6*as.numeric(Hair) + 4*as.numeric(Eye)
       + 2*as.numeric(Sex)
})

# Calculate true ATE
with(hec, mean(Y1 - Y0))
#> [1] 25
```

```r
library(randomizr)
Z <- simple_ra(N = N)
table(Z)
Z <- simple_ra(N = N, prob = 0.30)
table(Z)

Z <- simple_ra(N = N, num_arms = 3)
table(Z)

Z <- simple_ra(N = N, prob_each = c(.2, .2, .6))
table(Z)

Z <- simple_ra(N = N, prob_each = c(.2, .2, .6),
               conditions=c("control", "placebo", "
                   treatment"))
table(Z)

Z <- complete_ra(N = N)
table(Z)
```

```
Z <- complete_ra(N = N, num_arms = 3)
table(Z)

Z <- complete_ra(N = N, m_each = c(100, 200, 292))
table(Z)
```

```
sims <- 1000

# Set up empty vectors to collect results
simple_ests <- rep(NA, sims)
complete_ests <- rep(NA, sims)

# Loop through simulation 2000 times
for(i in 1:sims){
  hec <- within(hec,{

    # Conduct both kinds of random assignment
    Z_simple <- simple_ra(N = N)
    Z_complete <- complete_ra(N = N)

    # Reveal observed potential outcomes
    Y_simple <- Y1*Z_simple + Y0*(1-Z_simple)
    Y_complete <- Y1*Z_complete + Y0*(1-Z_complete)
  })
```

```
  # Estimate ATE under both models
  fit_simple <- lm(Y_simple ~ Z_simple, data=hec)
  fit_complete <- lm(Y_complete ~ Z_complete, data=
      hec)

  # Save the estimates
  simple_ests[i] <- coef(fit_simple)[2]
  complete_ests[i] <- coef(fit_complete)[2]
}
```

What is a treatment, after all?

Within-subject designs

- Subject i receives multiple treatments at multiple time points t
- Behavioral games; answering questions

Now potential outcomes can be written as $Y_{t-1,t,t+1}$ as a function of whether treatment is administered in the preceding, current, or next time period

ATE is now $E[Y_{010} - Y_{000}]$. Given no anticipation ($Y_{001} = Y_{000}$) and no persistence ($Y_{100} = Y_{000}$), the within-subject design identifies ATE

Within-subject designs

- Costs
  - Demand effects
  - Confounders from multiple treatments (assumptions stated above)
  - Complexity and validity: willingness to pay in Charness et al 2012
- Benefits
  - Internal validity unrelated to assignment mechanism
  - Statistical power
  - Proximity to theory
- Example: Bellemare and Shearer 2009

Between-subject designs

- Formal setup as all of part 1 of today's lecture
- Each individual exposed to 1 treatment
- ATE as difference in means of *different* groups of individuals
- Costs
  - Natural anchor with respect to (economic) decision-making?
  - Statistical power
- Example: Gneezy and List (2006)
- Overall: confounders $\succ$ power

Within vs Between: Bellemare et al. 2016

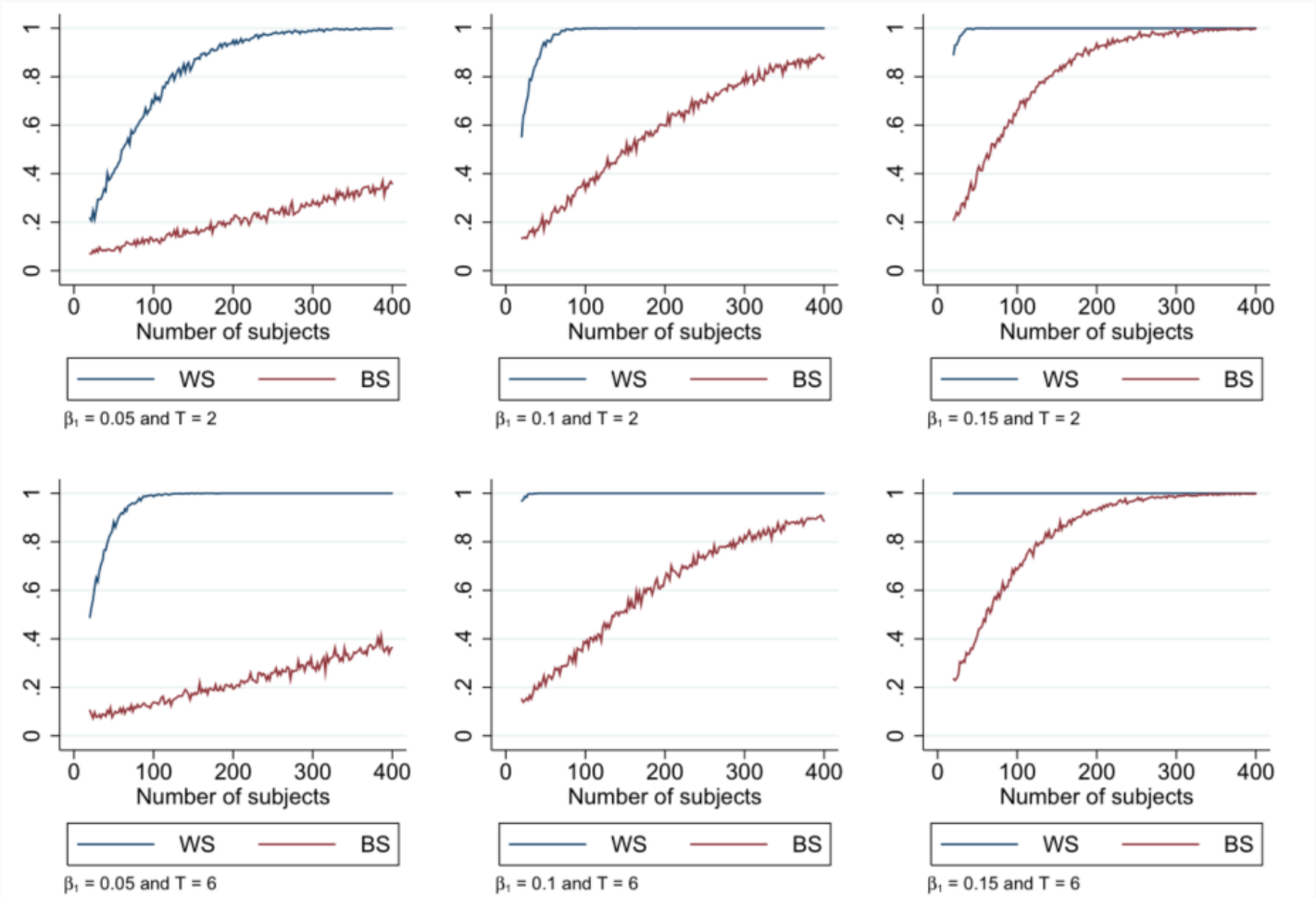


Fig. 2 Simulated statistical power of BS and WS designs with $T = 2$ and $T = 6$ for the high-noise scenario. Simulations based on values $\sigma^2_\mu = 0.09$ and $\sigma^2_\epsilon = 0.02$. Results for the BS design are computed by allocating the same number of subjects to control and treatment conditions for all periods. Results for the WS design are computed by assigning all subjects to the same number of control and treatment periods

Multiple treatment arms

- Define a factorial experiment as an experiment involving factors 1 and 2, with factor 1 conditions being A and B, and factor 2 conditions being C and D and E
- Then, allocate subjects at random to every possible combination of experimental conditions
- $\{AC, AD, AE, BC, BD, BE\}$

Multiple treatment arms

From Rosen 2010

	Colin		Jose	
	Good grammar	Bad grammar	Good grammar	bad grammar
% Received reply	52	29	37	34
(N)	(100)	(100)	(100)	(100)

This design requires us to be especially careful with defining the causal estimand – what quantity are we interested in in this application?

Multiple treatment arms

Quiz: Why would these two models estimate the same quantities from the Rosen 2010 experiment?

$\{CG, JG, CB, JB\}$ are indicator variables for each of the 4 treatment groups

$J_i = 1$ if Jose Ramirez; $G_i = 1$ if good grammar

$$Y_i = b_1 CG + b_2 JG + b_3 CB + b_4 JB + u_i$$
$$Y_i = a + bJ_i + cG_i + d(J_i G_i) + u_i$$

What quantity in the table do each of the coefficients represent?

Lecture 1 Assignment

- Replicate the Randomizr example from this lecture
- Create a hypothetical, although meaningful, political data set
- Generate observed potential outcomes
- Estimate the ATE
- Describe the uncertainty associated with your estimate of your ATE
- Test an hypothesis about your ATE