Experimental Methods: Lecture 3

Noncompliance, Power

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Road Map

- Noncompliance
- Statistical power

Noncompliance

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
Intervention contents						
Non-judgmental exchange of narratives…						
○ From participants (voter and canvasser)	YES	YES	NO	YES	NO	YES
○ In video	YES	NO	NO	YES	YES	NO
Address concerns and deliver talking points	YES	YES	YES	YES	YES	YES
Results						
ITT[a]	Positive effects ($d = 0.16$, $p < 0.001$)	Positive effects ($d = 0.08$, $p < 0.001$)	Null effects ($d = 0.02$, $p = 0.27$), statistically distinguishable from Full Intervention ($d = 0.06$, $p < 0.01$)	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[b]	$d = 0.22$	$d = 0.12$	$d = 0.03$ (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.

[a]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.

[b]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 $\rightarrow$ 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 ATT, 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

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

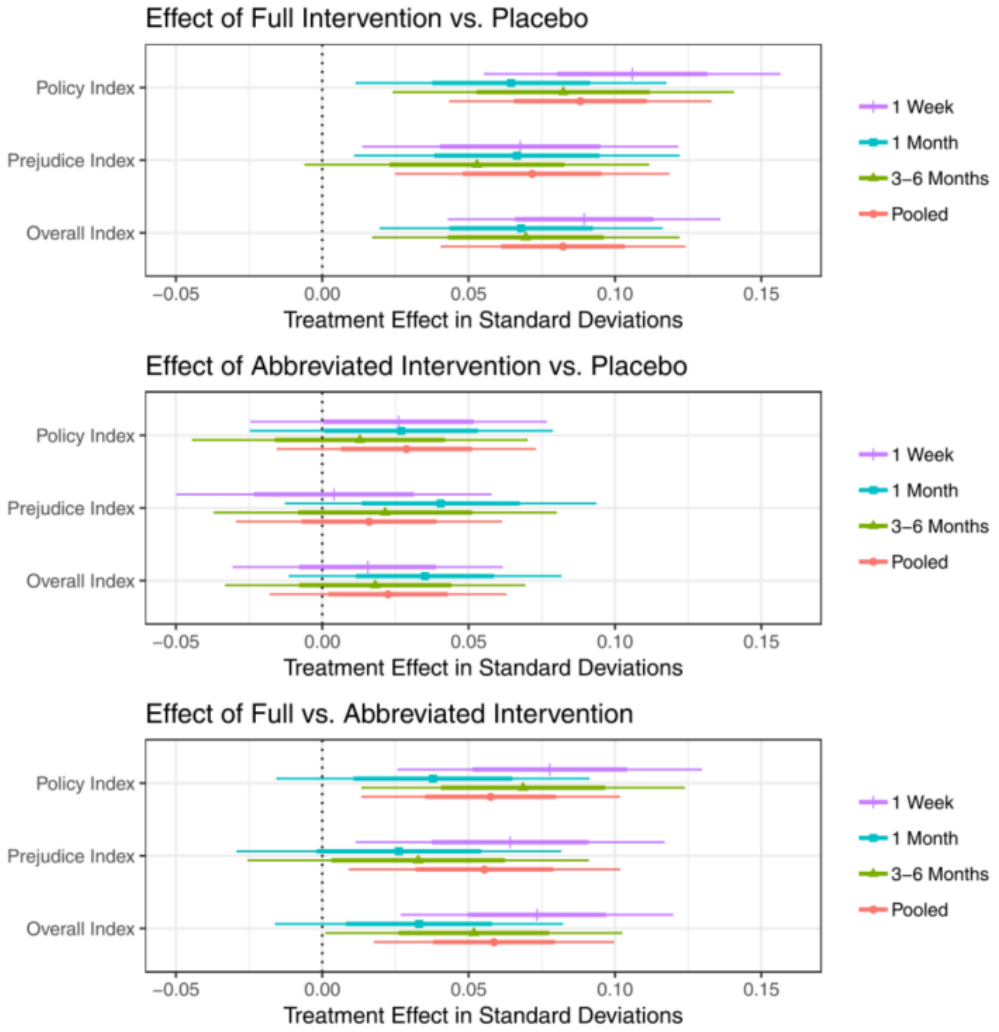
New Haven Voter Mobilization

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)

- ITT $=$ 41.38 - 37.54 $=$ 3.84
- $\pi_C = 395/1445 = 0.273$
- CACE $=$ 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

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

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

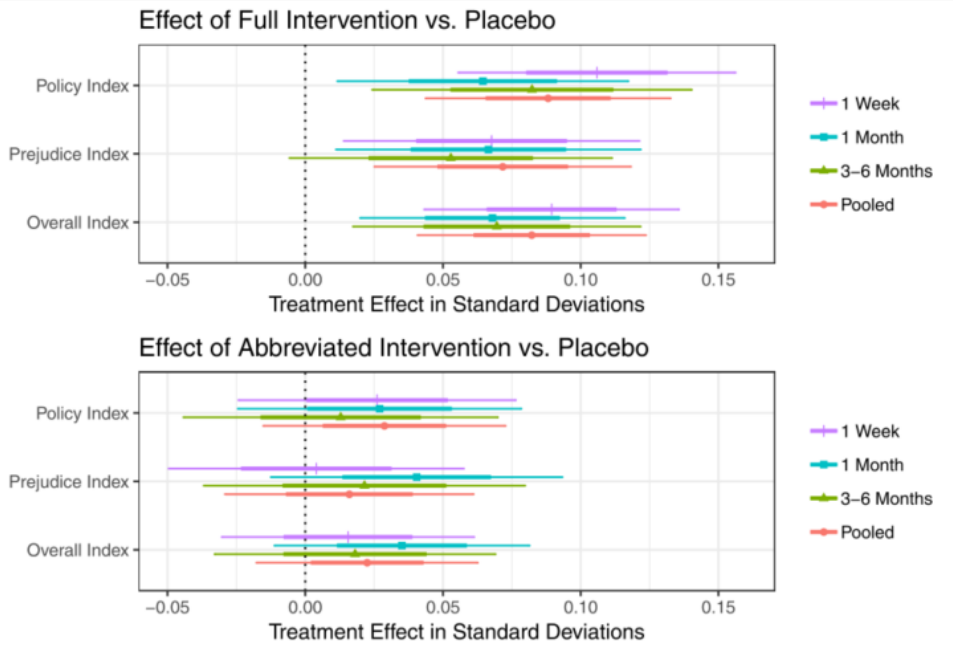
- Increase π_C; rule out defiers
- 1-sided noncompliance: Placebo design

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 ($d = 0.06$, $p < 0.01$)	
$d = 0.12$	$d = 0.03$ (Abbreviated vs. Placebo)	



Placebo Design

- 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

- 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

Voter Turnout Example

- Gerber, Green, and Larimer (2008) interested in the effect of communication on turnout
- U.S. has voters files, anyone know what they are?
- 180,000 Michigan households in experiment
- 100,000 in control group (no postcards), other groups 20,000 each
- **Civic duty**: "It's your civic duty to vote"
- **Hawthorne**: "It's your civic duty to vote, we're doing a study and will check public records"
- **Self**: "You should vote, here's your recent voting record"
- **Neighbors**: "You should vote, here's your neighbors' voting records and your own"

Results

	Control	Civic	Hawthorne	Self	Neighbors
Pct Voting	29.7%	31.5%	32.2%	34.5%	37.8%
N	191,243	38,218	38,204	38,218	38,201

Anyone here know how Gerber followed up on this study?

Power Analysis

Statistical Power

- What is the power of a statistical test? H_0: null hypothesis
- Apply estimator to test some alternative H_A
- Type I error: False positive
  - If the null is true, how likely does the estimated effect (or greater) occur by chance?
  - Our tolerance for these errors is set by α
  - When $\alpha = 0.05$, 95% of the CIs we construct from repeated sampling will contain the true parameter

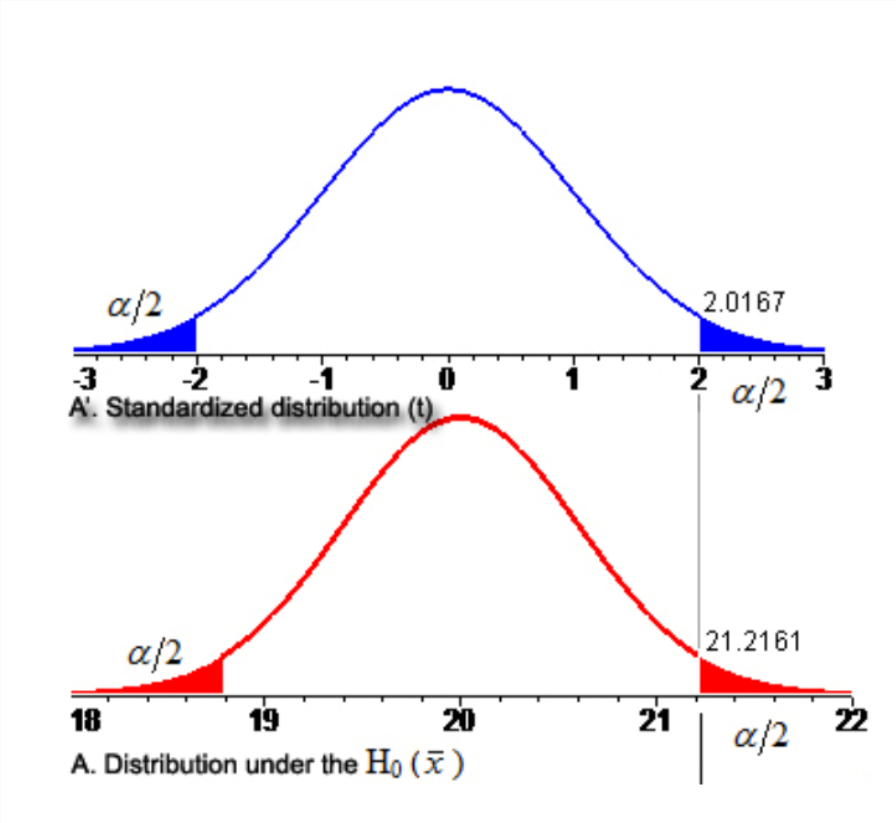
Statistical Power

- Type II error: False negative
  - If the null is not true, how often can we reject the null successfully?
  - Probability or rate of Type II error, β
- Power of a test: probability that the test rejects H_0, $1 - \beta$

Basic Inference Revisited

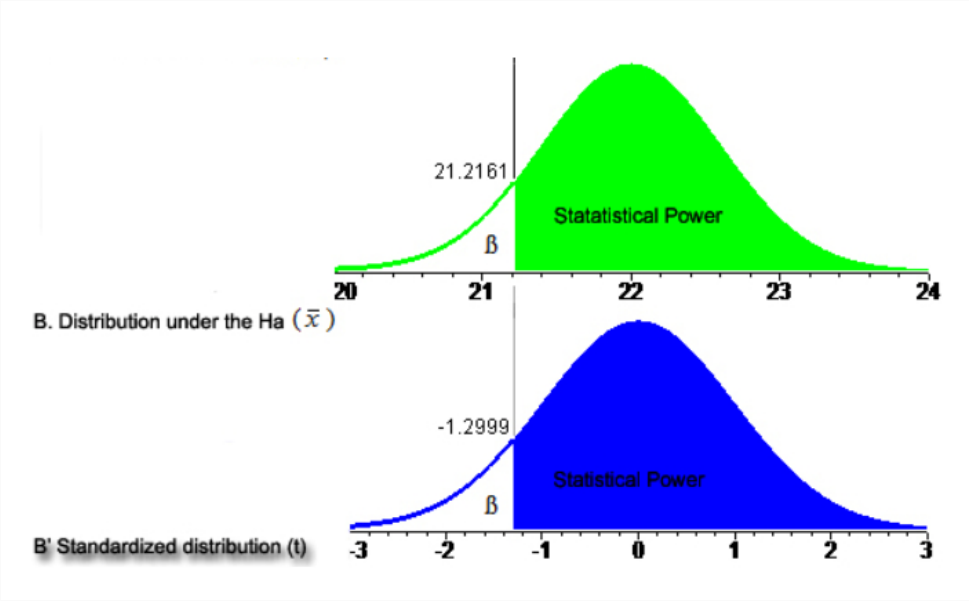
- What is the effect of losing Medicaid on infant mortality?
- $H_0 = 20$ deaths per 1,000 live births (assumed known without uncertainty here)
- True effect is an increase of 2 deaths per 1,000 live births
- Standard deviation in population is 4, we have N=44 observations; sampling distribution yields a standard error of 0.60
- $\hat{x}$ is our estimate of the new infant mortality rate
- Let's say we get an estimate right at the true estimate, $\hat{x} = 22$
- How unlikely is it we get this estimate, if the null is actually true?

Sampling Distribution Under Null



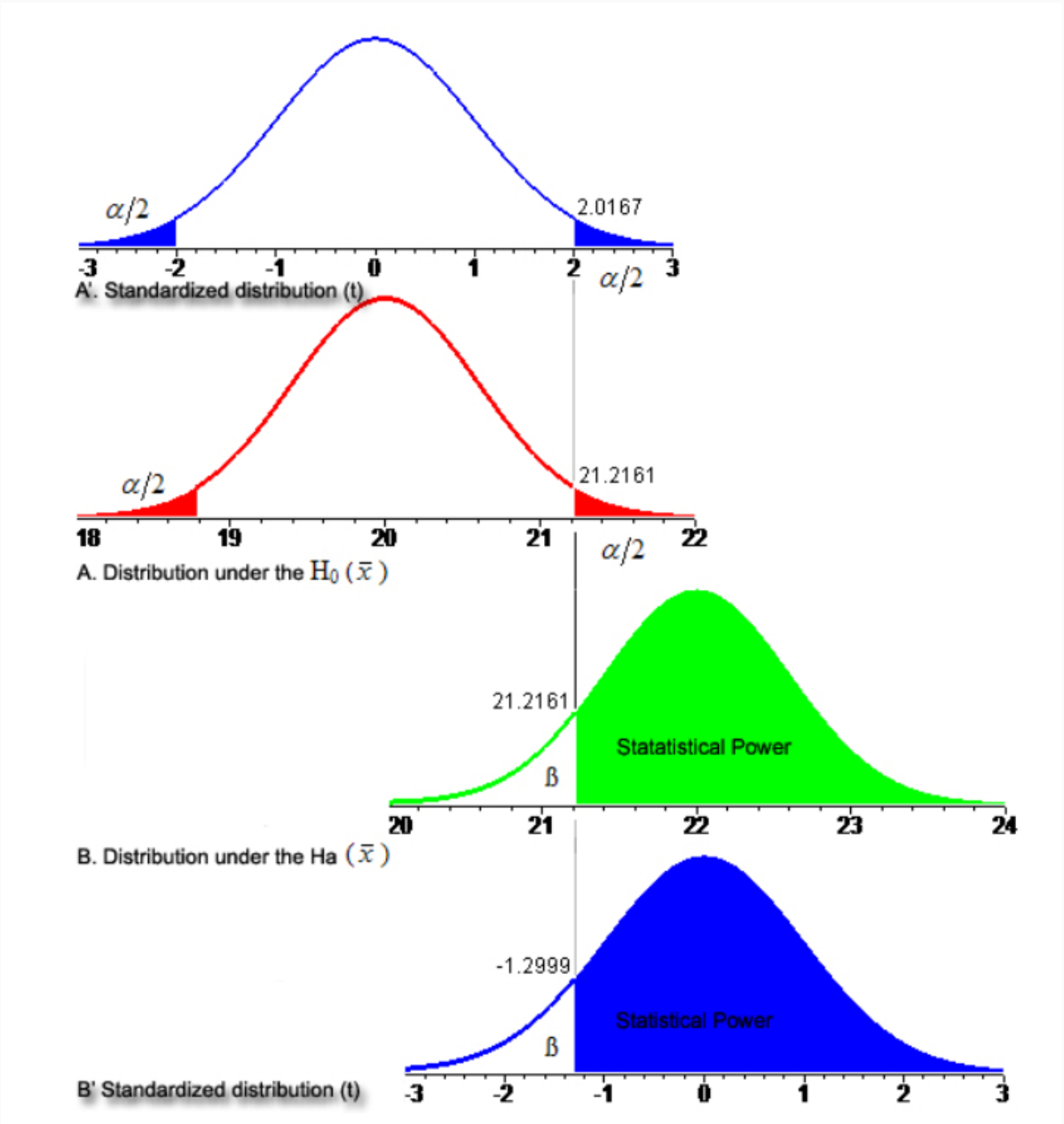
- Say for our test $\alpha = 0.05$
- Can rescale via Z-transformation
- What does this graphic mean?
- For $\hat{x} = 22$,
- t-stat=3.32, $p < 0.01$

Sampling Distribution of $\hat{x}$



- Interpret this graphic
- $1 - \beta$ is fraction of estimates that reject null hypothesis
- Power of the test
- What $x_t rue$ yields $1 - \beta = 0.5$?
- What parameters are needed?

The Relationship Between α and β



Sample Size Increases Power

- Of primary interest because it can be manipulated
- Law of large numbers: for independent data, statistical precision of estimates increases with the square root of the sample size, $\sqrt{n}$
- Test statistics often have the form $T = \hat{\theta} / \sqrt{\hat{V}(\hat{\theta})}$
- Example: Mean of normal distribution θ, data $y = (y_1, ..., y_n)$, iid

$$\hat{\theta} = n^{-1} \sum_{i=1}^{n} y_i = \bar{y}$$

$$\hat{V}(\hat{\theta}) = V(y)/n \text{ and } \sqrt{\hat{V}(\hat{\theta})} = s_y/\sqrt{n}$$

$$T = \bar{y}/(s_y/\sqrt{n})$$

- This logic extends to two-sample case (e.g., treated vs control in an experiment), regression, logistic regression, etc.

Reverse Engineer T to Determine Sample Size

- How much sample do I need to give myself a "reasonable" chance of rejecting H_0, given expectations as to the magnitude of the "effect"
- Example:

  A proportion $\theta \in [0, 1]$ estimated as $\hat{\theta}$

  Variance is $\theta(1-\theta)/n$, maxes at 0.5

  A 95% CI at $\theta = 0.5$ is $0.5 \pm 2\sqrt{0.25/n}$

  Width of that interval is $W = 4\sqrt{0.25/n} \rightarrow n = 4/W^2$

- Typical use: how big must a poll be to get reasonable MOE?
- For researchers, how big must a poll be to detect a campaign effect?
  - Answer depends on beliefs about likely magnitude of campaign effects

Example 2: campaign effect

- In R, power.prop.test()
- Researcher thinks effects that move a proportion (i.e. vote support) from 50% to 52% are likely
- Would like to be able to detect effects of this size at conventional levels of statistical significance
- ($p = 0.05$; 95% confidence interval for the effect excludes zero), with power $(1 - \beta)$ equal to 0.50
- $H_0 : \delta = \theta_1 - \theta_2 = 0$; $H_A : \delta \neq 0$ (two-sided alternative)

Power Estimate for 2 Point Effect

Two-sided alternative at conventional levels of significance

```
>power.prop.test(p1 = 0.5, p2 = 0.52, power
    = 0.5)
```

Two-sample comparison of proportions power calculation

n = 4799.903

p1 = 0.5

p2 = 0.52

sig.level = 0.05

power = 0.5

alternative = two.sided

NOTE: n is number in *each* group

Power Estimate for 2 Point Effect

One-sided alternative at conventional levels of significance

```
> power.prop.test(p1 = 0.5, p2 = 0.52,
    power = 0.5,
alternative=  one.sided")
```

Two-sample comparison of proportions power calculation

n = 3380.577

p1 = 0.5

p2 = 0.52

sig.level = 0.05

power = 0.5

alternative = one.sided

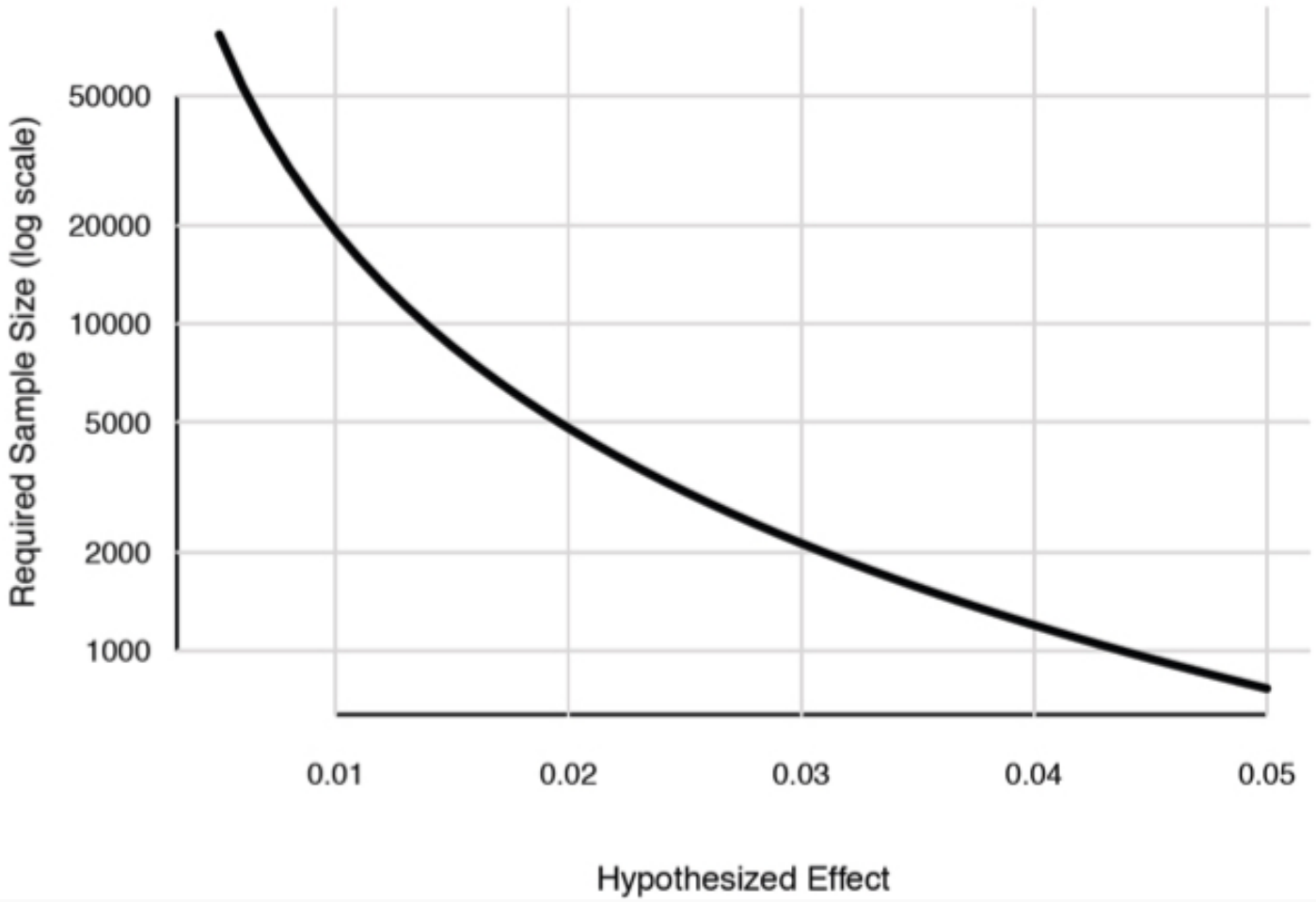
NOTE: n is number in *each* group

Power Curves

```
effects <- seq(0.005, 0.05, by = 0.001)

base <- 0.5
m <- length(effects)
n <- rep(NA, m)
for (i in 1:m) {
    n[i] <- power.prop.test(p1 = base, p2 =
        base + effects[i],power = 0.5)$n}
```

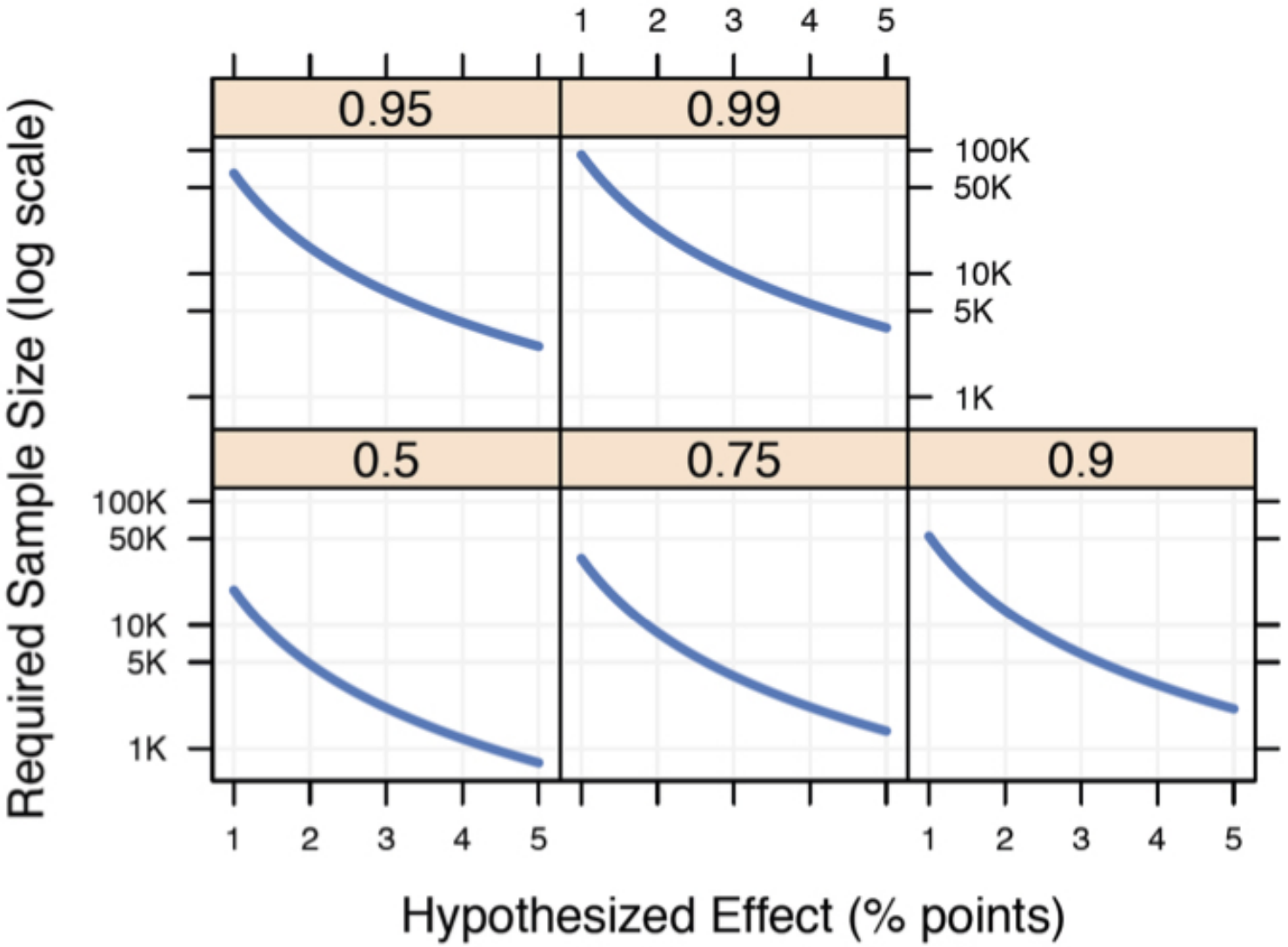
Power Curves



Looping over Power Curves

```
> power <- c(0.5, 0.75, 0.9, 0.95, 0.99)
> effects <- seq(0.01, 0.05, by = 0.001)
> base <- 0.5
> m <- c(length(power), length(effects))
> n <- matrix(NA, m[1], m[2])
> for (i in 1:(m[1])) {
+ for (j in 1:(m[2])) {
+ n[i, j] <- power.prop.test(p1 = base, p2
    = base + effects[j],
+ power = power[i])$n
+ }
+ }
```

Power Curves: different power levels



Practical Advice on Power

- What is "typical" size for effects, and how might we guess?
  - Some thoughts on later example
- Generally, experiments require $1 - \beta > 0.8$ to get funding
- Zaller's maxim: "Do your power analysis, figure out your sample size, then double it"

Practical Advice on Power

- Cost considerations: Gerber and Green turnout experiment
  - One component involved canvassing
  - $40 per hour for a pair of students, 6,000 treated
  - If 6 houses an hour, need 1000 hours, so $40k right there alone
  - Implications based on power curve slide
- In particular costs high for general population experiments
- Anyone have guesses how much surveys cost?
- How much value?