w241: Experiments and Causality

Incomplete Control Over Treatment Delivery

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Introduction to Noncompliance

Introduction

Noncompliance in randomized experiments

- Sometimes units assigned to treatment do not actually receive treatment
- This week, we will learn how to deal with noncompliance
  - How to analyze data correctly without inducing bias
  - How to design experiments to improve precision in the presence of noncompliance
- Leading example: Advertising experiments from earlier in the course
  - Randomly assign a person to be targeted with advertising
  - But, they don't browse to the site and so *choose* to receive zero ads
  - Or, some other advertiser non-randomly targets the person so our ad campaign cannot reach them.

Reading

Gerber and Green: Chapter 5, Introduction

- This is about 4 pages of reading.

Example: Get Out To Vote

Introduction to Chapter 5

Get out the vote (GOTV) example

- Treatment assigned at random
  - Canvasser knock on the doors of 1,000 treatment houses
  - Canvassers skip knocking on the doors of 1,000 control houses

In the treatment Group

- 250 subjects answer the door
- The other 750 subjects did not receive treatment, *even though they were assigned to*.

Three groups of individuals

With three groups of individuals, who should we compare to whom when estimating the treatment effect?

1. **Group A**: 250 people who answered the doors?
2. **Group B**: 750 people who didn't answer their doors?
3. **Group C**: 1,000 people in the control group (on whose doors no one knocked)

Introduction to Chapter 5 (cont'd)

Field Experiments considers three options:

1. Compare group *A* to *C*: treatment individuals vs. control individuals
2. Compare group *A* to groups *B* and *C*: treated individuals vs. untreated individuals
3. Compare group *A* and *B* to group *C*: whole treatment group to whole control group

Example: Yahoo! Ad-Effectiveness

Yahoo! Ad Experiemnt

Group A

- Assigned to treatment and received treatment
- 64% of the treatment group
- Purchased $1.81 per person

Group B

- Assigned to treatment, *but did not receive* treatment
- 36% of the treatment group
- Purchased $2.04 per person

Group C

- Assigned to control, *but did not receive* treatment
- 100% of the control group
- Purchased $1.84 per person

Yahoo! Ad Experiemnt (cont'd)

- The correct apples to apples comparison is {*A* & *B*} to *C*
- Treatment effect of

$$(\$1.81 \times 0.64) + (\$2.04 \times 0.36) - \$1.84 = \mathbf{\$0.05}$$

But, what is this effect?

- Treatment effect is *diluted* but the 36% of consumers in the treatment group who did not receive ads and therefore could not have had any treatment effect
- This $0.04 estimate is called the **intent-to-treat (ITT)** treatment effect
- The treatment effect on those who were *actually* treated must have been larger than $0.05
- Producing an unbiased estimate of the treatment effect on those actually treated requires *reweighing this ITT*.

Reading

Read *Field Experiments*, Section 5.1

- Section 5.1 introduces new notation about treatment since assignment to treatment no longer guarantees that a subject will (or did) receive treatment
- As before, $d = 1$ indicates that someone received a dose of treatment, and $d = 0$ indicates that someone received a dose of control
- Section 5.1 introduces new notation, z, that indicates whether someone was assigned ("azzigned" mnemonically) to treatment

Assignment and receiving treatment are distinct events

- Someone might be assigned to treatment, $z = 1$, but choose to take a dose of control, $d = 0$
- In the future, we'll let subjects be assigned to control, $z = 0$, but choose to take a dose of treatment, $d = 1$

Example: Blood Pressure

Example: Blood Pressure

Suppose our goal is to assess the effect of a new blood pressure medicine

- 100 control individuals are giving nothing, $z = 0$
- 100 treatment individuals are provided with blood pressure medicine
  - 60 individuals take their pills, $z = 1, d = 1$
  - 40 individuals do not take their pills, $z = 1, d = 0$

Who are the compliers, and who are the never takers?

Example: Blood Pressure (cont'd)

- 100 control group units did not take the pill, $z = 0, d = 0$, and have a **mean BP = 140**
- 60 compliers who took the pill, $z = 1, d = 1$, have a **mean BP = 150**.
- 40 never-takers who did not take the pill, $z = 1, d = 0$, have a **mean BP of 100**

Don't be tempted!

- Tempting to naïvely conclude that pills increase blood pressure
  - People who take the pill have higher blood pressure than either of the groups that did not take the pill
- But, a more careful analysis would show that the ATE is, in fact, a *reduction* in blood pressure

Reading: Skip *FE*, Section 5.2

Key Takeaway #1

- The **ITT** is the "intent-to-treat" effect
  - The ITT is the difference of the average outcomes in the group assigned to receive treatment and the group assigned to receive control
  - The $ITT = E[Y|Z = 1] - E[Y|Z = 0]$
  - This is a correct, *apples-to-apples* comparison
  - But, this estimate will be diluted compared to the actual treatment effect for the people who received the treatment
  - In other words, the ITT is the treatment effect of the *intention to treat*, z, on the outcome variable Y.

Reading: Skip *FE*, Section 5.2

Key Takeaway #2

- The ITT_D is the effect of being assigned to treatment, on *receiving a dose of treatment*
- Because receiving a dose happens **after** random treatment assignment, it meets all the requirements of a causal effect
- $ITT_D = E[d_i|z_i = 1] - E[d_i|z_i = 0]$

Blood Pressure Example

- 60% of the treatment group received treatment, $E[d_i|z_i = 1] = 0.6$
- 0% of the control group received the treatment, $E[d_i|z_i = 0] = 0.0$.
- And so, $ITT_D = E[d_i|z_i = 1] - E[d_i|z_i = 0] = 0.6 - 0.0 = \mathbf{0.6}$

Alternative Terms

- *Take up rate*
- Or, alternative symbol, α

Reading: *FE*, Section 5.3

Read *Field Experiments*, Section 5.3

- Gerber and Green draw an important distinction between the ATE and the CACE in this section
  - The ATE is the average treatment effect for the whole population
  - The CACE is the average treatment effect for the population who comply with their assignment
- When there is non-compliance, we cannot measure the potential outcomes to treatment for non-compliers -- none of them receive treatment!
- Thus, when there is non-compliance, there is no guarantee that the ATE = CACE.

Example: Computing the CACE for Yahoo!

Context for Causal Effects

Context shapes which which quantity is more important to estimate

- The *ATE* for everyone
- The *CACE* for the people we can actually treat
- In advertising, the advertiser only pays for ads that were delivered to compliers
- When computing the rate of return to advertising, all we care about is the cost of the ads and marginal increase in sales for people who received the ads

Context for Causal Effects (cont'd)

Advertising

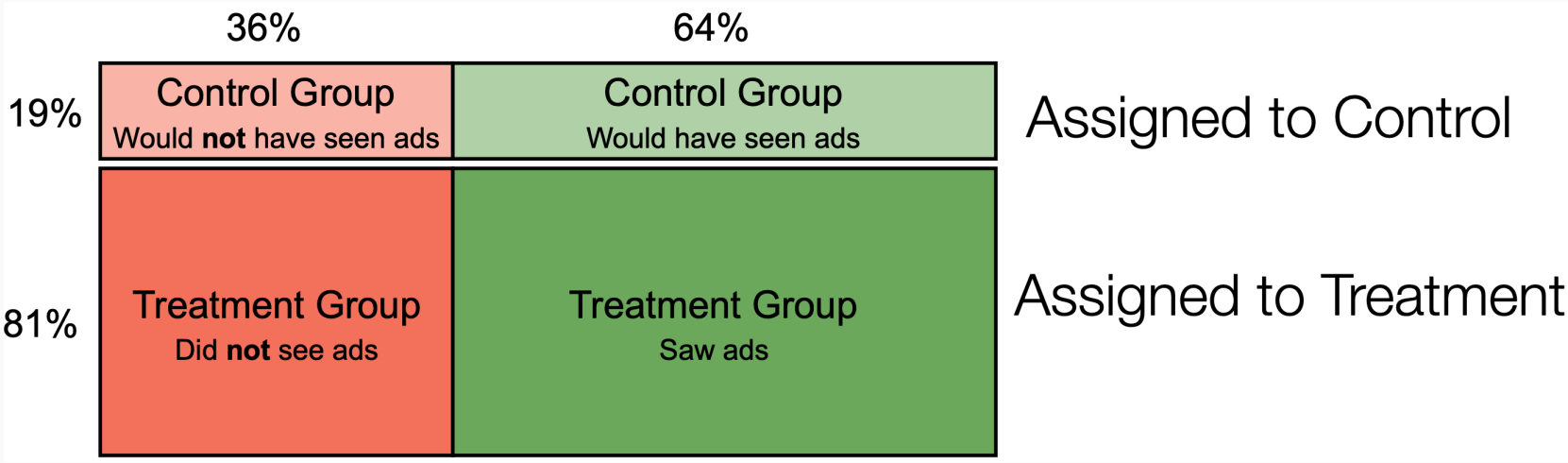
- How much **more** would never takers purchase if we had given them ads?
- An advertising team might want to know, but cannot *possibly* learn this because they cannot actually reach them!
- (A targeting team; or product team might change its product to reach these previously unreachable leads)

Other Examples

- GOTV canvassing
- Job-training programs
- Blood pressure drug trials
- If people will not take the treatment they are assigned, what should we try to do to reach them?
- If we cannot reach them, why do we care about the effect if we *could* reach them?

Example: Advertising Effectiveness

- 36% of the treatment group did not receive any ads



- The un-exposable 36% of never-takers do not produce any treatment effect, *because they do not receive treatment*.
- Include them in estimate to maintain an apples-to-apples comparison

Computing CACE for Yahoo!

- $ITT = 0.05$, difference between treatment and control, including never-takers
- $\alpha = ITT_D = 0.64$, the fraction of compliers that were reached with ads
- If the exclusion restriction is true, never takers should have zero treatment effect
- Therefore, any treatment effect is generated from the 64% who received ads
- To estimate treatment, divide the ITT by the compliance rate

$$CACE = \frac{ITT}{\alpha} = \$0.08$$

CACE Standard Errors

- In the Yahoo! example, we estimate an ITT of $0.05 \pm 0.07 = (0.02, 0.12)$
- Among the people who complied with their assignment, their *CACE* is larger, 0.08. Is this *now* statistically significant? Is the confidence interval 0.08 ± 0.07?
- No! Equation 5.29 points out that scaling up our estimate also means scaling up our uncertainty?
- We estimate a $CACE = 0.08 \pm 0.11$ and still have a non-significant result
- Equation 5.29 provides this statement of CACE standard errors

(Optional) Footnote 5.4

- We do not actually know $\alpha = 0.64$ with certainty; this is,
- α is, itself a statistic with sampling variation
- Estimating $CACE = \frac{ITT}{\alpha}$ will introduce some bias in samples if α is estimated with uncertainty
- Bias has the *opposite* sign as the correlation between ITT and α
- But, it is hard to know what the sign of this correlation is!
- With large samples, α is estimated without much sampling variability, and $\lim_{n \to \infty} \widehat{CACE} \xrightarrow{p} CACE$.

Reading

Read *Field Experiments*, Section 5.9

But First!

- Think about the advertising-effectiveness example:
  - 36% of subjects in that experiment are never-takers that cannot provide information about a treatment effect
  - But, we cannot exclude them, because we cannot identify who, in the control group, *would have been* a non-complier if they were in treatment.
- Section 5.9 introduces the **placebo design** that distinguishes between subject *types* in without giving treatment to them.
- As a result, it is possible to exclude never-takers from the CACE estimate *while still* maintaining an apples-to-apples comparison

Two-Stage Least Squares

- The acronym **2SLS** stands for two-stage least squares
- Don't worry too much about equations 5.30 and 5.31

Applying a Placebo Design

- Canvassing example shows that using a placebo to identify individuals in the *control group* would could receive treatment (i.e. they are compliers) can increase *efficiency*
- Apply this concept to the Yahoo! example
  - Yahoo! ad effectiveness experiment randomly assigned 80% of the population to be in the treatment group
  - Of those, 64% that were targeted for treatment actually received it
  - If we could find the 36% in the control group that *would not have taken treatment*, then we could exclude them and increase the precision of our estimates

Think for a moment

- What was placebo in the canvassing example?
- How could one make a similar placebo in the Yahoo! example?

Applying a Placebo Design (cont'd)

Run placebo ads!

In the control group, run an ad on an unrelated topic -- maybe the American Red Cross -- to the control group

Johnson, Lewis and Reiley (2016)

- JLR (2016) use this idea for a follow-up experiment on advertising effectiveness
- Run placebo ads ("Do your searches on Yahoo!") to the control group with *exactly* the same campaign parameters as the treatment campaign for the retail store
- Added benefit: **Two advertising experiments for the price of one!**
- By making the placebo campaign exactly mirror the treatment campaign, guaranteed that those receiving ads would be *exactly* the same population of compliers in treatment and control

Benefits of a Placebo Design

Benefits of a Placebo Design

- With a treatment-placebo or treatment-control design we produce an unbiased estimate of the CACE
- **Treatment-Placebo Design**
  - Compare complier in treatment to compliers in control.
  - Directly compute the *average treatment effect on the treated* individuals (sometimes, called the "ATET")
- **Treatment-Control Design**
  - Compute the ITT over all subjects
  - Compute $CACE = \frac{ITT}{\alpha}$, scaling up the estimate, but also the errors from the estimate
- The placebo design does not change the estimated treatment effect, ATET and CACE are unbiased estimates of the same quantity

Placebo design produces precision $\longrightarrow$ **Power**

How Much Benefit for Placebo Design?

How much does the placebo design shrink standard errors?

- Suppose σ_t^2 and σ_u^2 represent the variances of Y for compliers and never-takers respectively
- The index in t and u are for *treated* and *untreated* individuals respectively.

$$\frac{V[\tau_{CACE}]}{V[\tau_{ATET}]} = 1 + \frac{\left(\frac{\sigma_t^2}{\sigma_u^2}\right) \times (1+\alpha)}{\alpha}$$

- So long as the variance of Y for the never-takers and compliers are approximately equal, this converges in probability to

$$\frac{V[\tau_{CACE}]}{V[\tau_{ATET}]} = \frac{1}{\alpha}$$

How Much Benefit for Placebo Design?

- The standard error from using a treatment-control design will be larger than the standard error from a placebo design by a factor of $\frac{1}{\sqrt{\alpha}}$
- The take-up rate, α is always between $[0, 1]$, so this variance inflation rate will always be larger than 1.
  - If the take-up rate is only 1%, then a placebo-design will shrink standard errors by a factor of ten
  - If the take up rate is 25%, a placebo design will shrink standard errors by a factor of two
  - If the take up rate is 90%, a placebo design will shrink standard errors by only five percent

Technology to Apply Placebo

Johnson, Lewis, Nubbemeyer (2017): **Ghost Ads**!

- With a correctly designed server, it is not necessary to pay for placebo ads to get a placebo design
- Instead, log the counterfactual ad impressions that would have occurred in the control group

Ghost Ads at Pandora

- Suppose that Pandora is ready to serve an audio ad; and that the marketplace clears a *Home Depot* ad.
- If that user has been assigned to the control group, the system instead plays the second-highest bidding ad in the marketplace. Suppose this is for a Toyota truck.
  - (This example really lands home with Alex while he's making the slides...)
- The user *hears* a Toyota ad that has be *possessed* (spooky!) by a Home Depot ghost ad.
- In practice, the key is to log the fact that the listener *would have* received a Home Depot ad if they were in the control group
- So long as the server is set up correctly, and doesn't change any *other* features of the ad, the group no longer has to pay for American Red Cross placebo ads

Ghost Ads: Lower Cost, Moar Precision

Ghost ads produce a more accurate counterfactual

- In a real Home Depot campaign, sometimes the treatment ad will displace a competitors ad (Lowe's?)
- With a placebo ad, a control group would not get the placebo Red Cross ad, but instead the Lowe's ad.
- *This doesn't match with the ideal of either the treatment-control or treatment-placebo design*.
- Perhaps this is a small effect, but it is still better to give the listener exactly the ad that they would have received in the absence of the campaign

More Neat Technology

Smart pill bottles

- Return to the blood pressure case
  - Suppose you give placebo (sugar) pills to the control group and everybody has a bottle that is wirelessly connected to a recording computer
  - The chip transmits information to the researcher to record every time the bottle has been opened
  - The researcher knows exactly who has and has not opened the bottle (is this the same as taking the pill?) and who has not
- Implemented correctly, this study can now discard the data from never-takers, and produce a more efficient estimate from the compliers

What Can Go Wrong with Placebos?

Did the placebo work as required?

Is the take-up rate the same in treatment and control?

- Do we see covariate balance between compliers in treatment and control?
- A (bad) idea: save money on placebo ads by putting a frequency cap on the placebo campaign.
  - Each individual hears at most one placebo ad.
  - Money saving, since we're not subsidizing an advertising campaign for the Red Cross (they're a great organization!)
  - What could go wrong?
- When we conduct this experiment, what if we learn that the take-up rate was different in control than treatment?
- *For example*: suppose we observe that 60% of the treatment group receives treatment ads; and 82% of placebo receive placebo ads
  - Covariates show that compliers in treatment browse more than compliers in placebo
  - **Biased estimate!**

What *else* could go wrong?

- What if the placebo has a treatment effect on the outcome we're interested in? This would be an **exclusion restriction** violation

Examples

- Play Red Cross ads to the control group of a Home Depot advertising campaign. **We're probably in the clear.**
- But, what if we had played Habitat for Humanity? Might people go purchase supplies when they were preparing to volunteer?

Two-Sided Noncompliance

Reading

Read *Field Experiments*, Introduction to Chapter 6 and section 6.1

Reading Tips

- Be certain to read box 6.1; it provides concepts and notation for this section
- Remember that when we are using binary variables, we can multiply them to get a boolean AND.

$$\pi_c \equiv \frac{1}{N} \sum_{i=1}^{N} d_i(1)(1 - d_i(0))$$

- $d_i(1) = 1$ if, when assigned to treatment, the individual receives the treatment
- $d_i(0) = 0$ if, when assigned to control, the individual receives control
- If $d_i(1)(1 - d_i(0)) = 1$ then the person is a complier

Two-sided Noncompliance

- One-sided noncompliance occurs when treatment units receive control, but all control units correctly receive control
- If control-group subjects can get treated, then we must consider four types of individuals

1. **Compliers** who do exactly as they are told:
   - $z = 1 \rightarrow d = 1$
   - $z = 0 \rightarrow d = 0$
2. **Never-takers** who never take the treatment, no matter their assignment.
3. **Always-takers** who always receive the treatment, no matter their assignment.
4. **Defiers**, the 4 year-old kids of experiments, who do the opposite of what they are told:
   - $z = 1 \rightarrow d = 0$
   - $z = 0 \rightarrow d = 1$

Key assumption: No defiers

- Also known as *monotonicity assumption* the dosage is increasing in assignment
- Without this assumption, we cannot produce an estimator. Is this assumption ever violated?

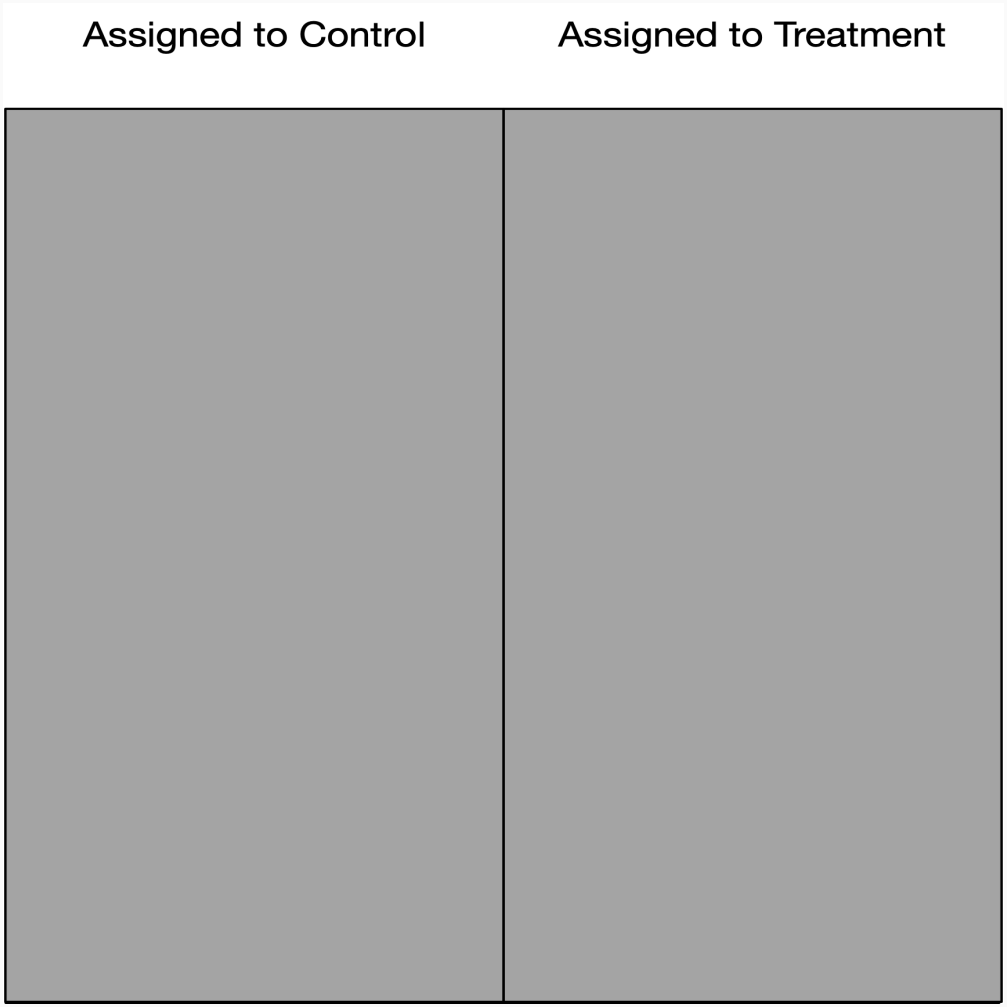
Estimating Treatment Effects

Estimating treatment effects in a two-sided noncompliance case is an extension of the method developed and presented for one-sided noncompliance.

- Treatment effect is estimated *only* for the compliers:
  - Both always-takers and never-takers can demonstrate no treatment effect because their dosage is the same in treatment as control. Always $D = 1$ for always takers; always $D = 0$ for never takers.
  - Only compliers are affected by treatment assignment
  - Once again, estimate the ITT across all individuals, and then re-scale by the share of compliers.

Key Question

What is the share of compliers?



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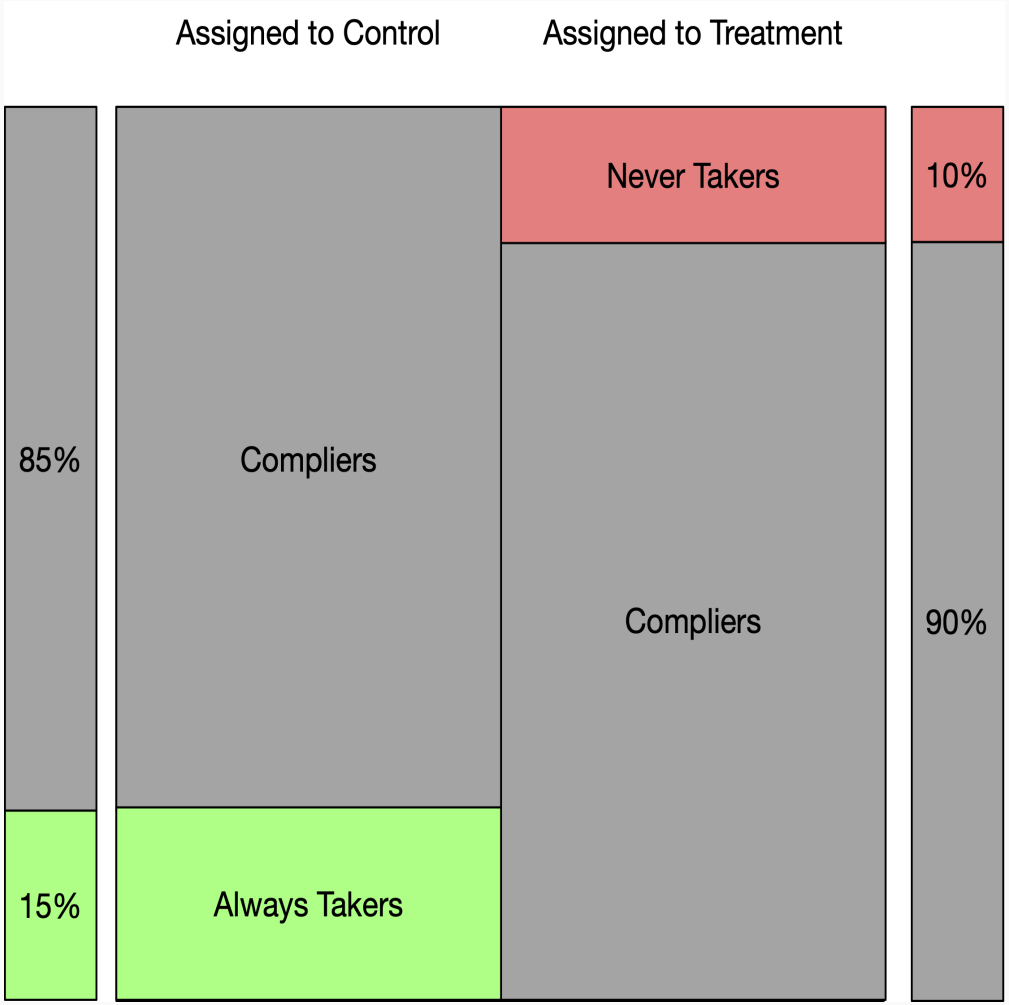

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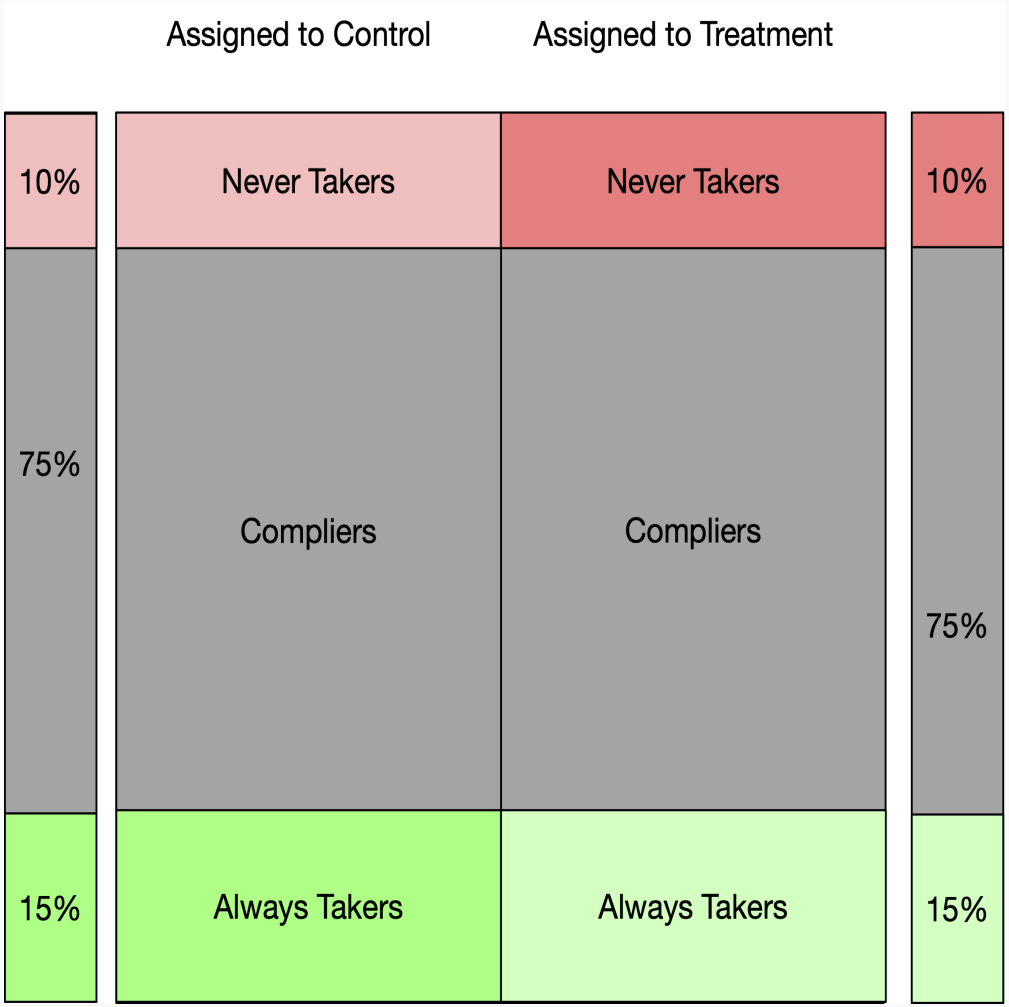

Key Question

What is the share of compliers?

- ITT_D is more sensible in two-sided than one-sided noncompliance. It is the effect of treatment assignment, Z, on dosage, D.
- The treatment effect of assignment on dosage raises the *fraction* of people getting dosed:
  - From some quantity greater than zero (because some assigned to control get treatment)
  - To some quantity lesser than one (because some assigned to treatment get control)
  - $ITT_D = E[D|Z = 1] - D[D|Z = 0]$
  - From the last slide, $ITT_D = 0.90 - 0.15 = 0.75$ of the subjects are compliers
  - So, divide ITT by this 0.75.
- **One-sided noncompliance**: $ITT_D \equiv$ the take up rate
- **Two-sided noncompliance**: $ITT_D \equiv$ the *difference* in take up rates

Estimating CACE

- In two-sided, like one-sided noncompliance, $CACE = \frac{ITT}{ITT_D}$.
- Theorem 6.2 in *Field Experiments* gives the full, technical description

Reading: The KIPP Lottery

Reading: The KIPP Lottery

Reading: Please read *Mastering Metrics* pages 98 - 105

- A copy of the PDF of this chapter is in the `./readings/` folder of the course.

Two Sided Non-compliance

- Charter schools in many areas have more demand from parents than supply in the schools--they are oversubscribed
- In an attempt to be fair, *Knowledge Is Power Program* (KIPP) schools in Lynn, MA allocated seats via a lottery
- In the reading that you're going to do, watch for two kinds of non-compliance, never-takers and always-takers.

Stop when you get to the section heading "LATE for Charter School"

Discussion of MM Table 3.1

Table 3.1, Panel A

TABLE 3.1
Analysis of KIPP lotteries

		KIPP applicants			
	Lynn public fifth graders (1)	KIPP Lynn lottery winners (2)	Winners vs. losers (3)	Attended KIPP (4)	Attended KIPP vs. others (5)
Panel A. Baseline characteristics					
Baseline (4th grade) math score	−.307	−.290	.102	−.289	.069
			(.120)		(.109)
Baseline (4th grade) verbal score	−.356	−.386	.063	−.368	.088
			(.125)		(.114)

Table 3.1, Panel B

TABLE 3.1
Analysis of KIPP lotteries

		KIPP applicants			
	Lynn public fifth graders (1)	KIPP Lynn lottery winners (2)	Winners vs. losers (3)	Attended KIPP (4)	Attended KIPP vs. others (5)
Panel B. Outcomes					
Attended KIPP	.000	.787	.741 (.037)	1.000	1.000 —
Math score	−.363	−.003	.355 (.115)	.095	.467 (.103)
Verbal score	−.417	−.262	.113 (.122)	−.211	.211 (.109)
Sample size	3,964	253	371	204	371

Features to Notice in Table 3.1

- Panel A is a covariate balance check
- Panel B gives several different outcomes, one on each row
- Columns (1), (2), and (4) give mean levels of the variables
- Columns (3) and (5) report differences, or treatment effects
- Look, for example, at the math-score outcome
  - Column (2) shows us that the math score for lottery winners was 0.003 standard deviations below the state mean
  - Column (3) shows us that the difference between lottery winners and lottery losers was 0.355 standard deviations in math score. **This is a substantial ITT**.

Table 3.1, cont'd

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Noncompliance in KIPP

- **Never-takers**: didn't attend KIPP even though the won the lottery (see Figure 3.1: there are 82 never-takers out of 303 lottery winners)
- **Always-takers**: figures out how to attend KIPP even though they lost the lottery (5 students out of 143 lottery losers)
- **Compliers**: attended KIPP if, and only if, they won the lottery (about 74% of the students)
- **Defiers**: by assumption, there are are none

Table 3.2: Mnemonic for Types

		Lottery losers $Z_i = 0$	
		Doesn't attend KIPP $D_i = 0$	Attends KIPP $D_i = 1$
Lottery winners $Z_i = 1$	Doesn't attend KIPP $D_i = 0$	Never-takers (*Normando*)	Defiers
	Attends KIPP $D_i = 1$	Compliers (*Camila*)	Always-takers (*Alvaro*)

CACE is Estimated Using Instrumental

There are two kinds of attendance in KIPP attendance

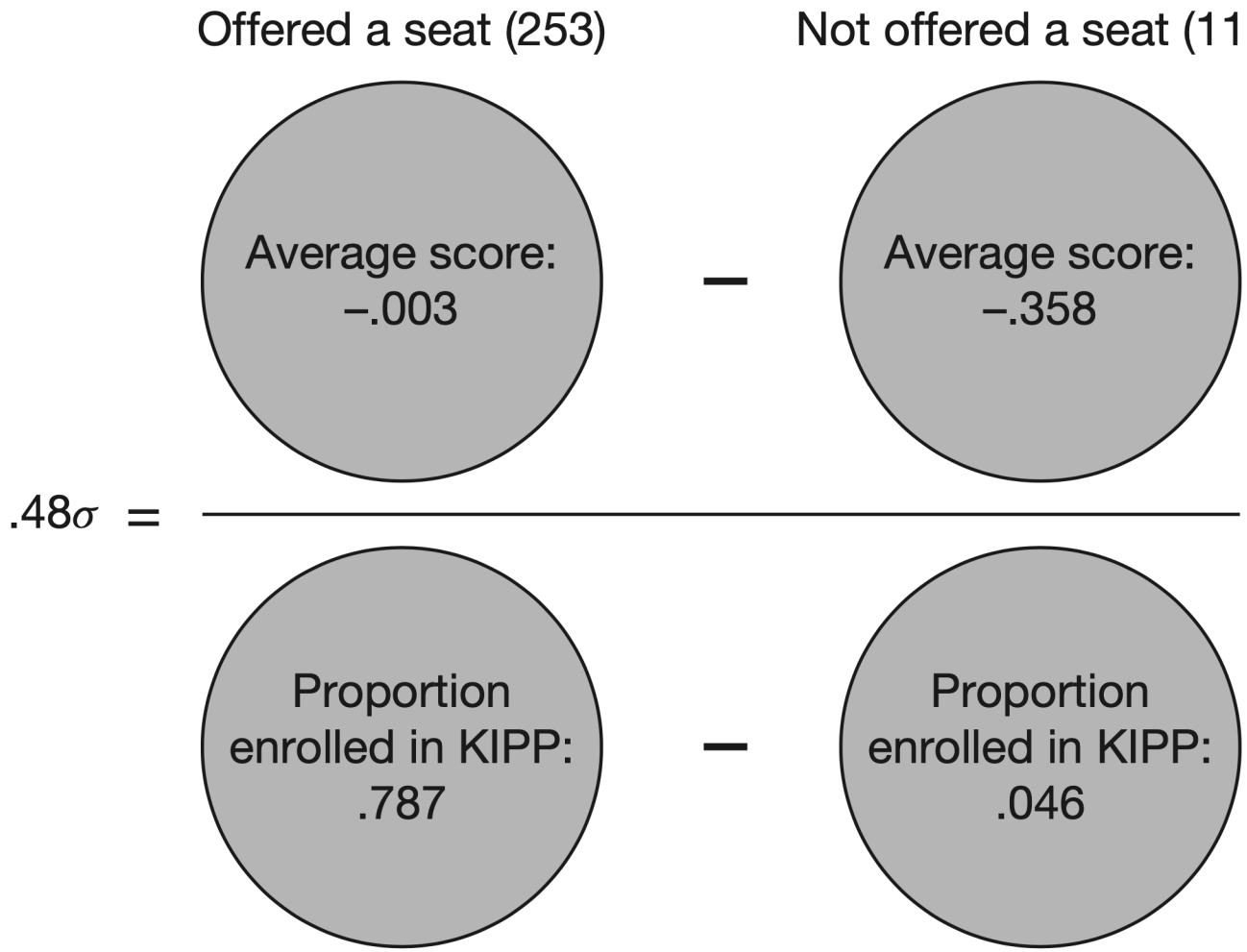
1. **Clean Variation**: random variation generated by the lottery
   - Used for measuring causal effects
   - Provides apples-to-apples comparisons
2. **Dirty Variation**: endogenous variation generated by heterogeneous student characteristics
   - This can contaminate causal effects with spurious correlation
   - Might cause an apples-to-oranges comparison
   - Perhaps always takers are more motivated than compliers?

Instrumental Variables can "purge" dirty variation

Estimating CACE in KIPP

Figure 3.2: IV in School

FIGURE 3.2
IV in school: the effect of KIPP attendance on math scores



Reading: *Field Experiments*

Reading: *Field Experiments*, Section 6.5.4

- Please read about *encouragement designs*
  - *Optional*: If you are interested, Section 6.4 is another very readable example of computing treatment effects with two-sided non-compliance

Encouragement Designs

What is an "Encouragement Design"?

- Sometimes, ethical reasons prohibit us from requiring people to either take, or not-take a treatment
- But, we might *encourage* subjects to follow our guidance.
- This encouragement might cause some to take (or not) the treatment
- But, there might be considerable two-sided non-compliance
- Estimation will require reliable estimators

Examples of Encouragement Designs

Calling Voters

- Phone individuals and encourage them to watch a mayoral debate
- Check to see if the individual watched, and whether opinions of candidates changes as a result of watching
- Control group is encouraged to watch a non-political TV show in a placebo design

Incentivising Gym Attendance

- Offer $100 to individuals who visit the gym three times per week for a month
- Monitor gym visits after the month of encouragement to observe whether a month's encouragement can cause a "habit"

Incentivising "Healthy" Eating

- Offer subjects $1000 to follow a keto diet for six months (monsters!)
- Ask subjects to wear blood-sugar monitors so we can monitor compliance
- Only pay those who keep their blood-sugar under 140 mg/dL
- Measure how lower blood-sugar affects cardiovascular health

Reading: *Field Experiments*, Section 6.6

Reading: *Field Experiments*, Section 6.6

- Please read the first 2.5 pages on "downstream experimentation"
- Stop when you get to the paragraph that begins with, "Let's now consider"
- The rest of section 6.6 is optional, and interesting.

Downstream Experiments

What is a Downstream Experiment?

Upstream Experiments

- Class size on graduation rates

Downstream Experiment

- "Do increases in graduation rates cause people to be more likely to vote?"
  - Once we've observed that there are large effects in the upstream experiment, one can use this as if the class-size experiment were an encouragement design
  - Does reducing class sizes increase graduation rates?
  - Do randomly generate increases in graduation rates make people more likely to vote?

Rapid Decay

- Typically, effects of treatment **very rapidly** decay.
- Downstream experiments will only work if there are strong upstream experimental effects

Examples of Downstream Experiments

Smoking

What is the effect of smoking a first cigarette at age 21 on the likelihood of being a regular smoker at age 25?

- If we're lucky, someone has already done an experiment on cigarette prices to estimate cigarette demand as a function of price
- Z: Cigarette-price treatment
- D: Trying one's first cigarette at age 21
- Y: Being a regular smoker at age 25

Sentence Length on Recidivism

What is the effect of a longer incarceration on the likelihood of being incarcerated in the future?

- Z: Being assigned to a tough judge who gives longer sentences
- D: Getting a sentence of a year or more in prison
- Y: Probability of being convicted of another crime within 10 years of the first

Noncompliance in Review

Noncompliance in Review

- Sometimes, we cannot deliver the assigned treatment to every unit
- Maintaining experimental comparability requires comparing all units we **intended** to give treatment to all units we **intended** to give control (ITT)
- Tempting, but incorrect, to compare those in the treatment group who received treatment against the control group
    - Without a placebo design, we cannot know who in the control group *would have complied*
    - Placebo designs *always* show a difference in baseline outcomes for compliers than never-takers
    - Apples-to-oranges comparisons are not a theoretical problem -- it is a *real* problem

Noncompliance in Review (cont'd)

- With two-sided noncompliance, there are three categories of subjects
  - Never-takers
  - Always-takers
  - Compliers
- We assume there are no defiers.
- CACE estimates are specific to compliers.
- ITT estimates are the treatment effect of treatment assignment, not receiving treatment, on outcomes
- $CACE = \frac{ITT}{\Delta ITT_D}$
- Placebo designs can increase precision in CACE by "deleting data" on never-takers in both treatment and control/placebo groups
  - Increases the signal-to-noise ratio while maintaining apples-to-apples comparability