Lecture 04:
Randomized controlled trials II – Noncompliance

PPHA 34600
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From last time: randomization solves our selection issue

Recall that the ATE is just:

$$\tau^{ATE} = E[Y_i(1)] - E[Y_i(0)]$$

Since we have random assignment, we can estimate this as:

$$\hat{\tau}^{ATE} = \overline{Y(1)} - \overline{Y(0)}$$

Regression is a convenient way to do this:

$$Y_i = \alpha + \tau D_i + \varepsilon_i$$

Generalizing our approach to randomization

Define $R_i \in \{0, 1\}$ as an indicator for being **selected for treatment**

- $R_i = 1$ is people assigned to treatment
- $R_i = 0$ is people assigned to control
- This is distinct from D_i, an indicator for **being treated**
- We can then write:

$$\tau^{\text{experiment}} = E[Y_i | R_i = 1] - E[Y_i | R_i = 0]$$

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Let's unpack the perfect compliance piece.

Why do we need perfect compliance?

$$\tau^{\text{experiment}} = E[Y_i|R_i = 1] - E[Y_i|R_i = 0]$$
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- Recall that we define $Y_i(D_i)$, **not** $Y_i(R_i)$!
- $E[Y_i|\mathbf{D_i} = 1]$ and $E[Y_i(1)|\mathbf{D_i} = 1]$ are equivalent

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→ This is not automatic for R_i

To get $E[Y_i|R_i = 1] = E[Y_i(1)|R_i = 1]$, we need $D_i = R_i$ for all i...
aka **perfect compliance**

What generates imperfect compliance?

Noncompliance has two sources:

1. Treated units *don't* get treated
2. Control units *do* get treated
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Why might this happen?

- Treatment is costly (money, time, effort)
- Treatment is desirable
- Program implementers are imperfect

What's wrong with imperfect compliance?

The whole point of the RCT is that

$$E[Y_i(1)|D_i = 1] = E[Y_i(1)|D_i = 0] = E[Y_i(1)] \approx \overline{Y}(1)$$

and

$$E[Y_i(0)|D_i = 0] = E[Y_i(0)|D_i = 1] = E[Y_i(0)] \approx \overline{Y}(0)$$

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Non-compliance breaks this result

With non-compliance, our means don't estimate the ATE

With noncompliance:

$$E[Y_i|R_i = 1] - E[Y_i|R_i = 0]$$

$$\neq$$

$$E[Y_i(1)|R_i = 1] - E[Y_i(0)|R_i = 0]$$

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As a result, we can't just estimate

$$\tau^{ATE} = \overline{Y}(1) - \overline{Y}(0)$$

A simple example of non-compliance

The mean outcome for $R_i = 1$ is not equal to the mean for $D_i = 1$:

- Consider an intervention to get people into factories
- Without treatment, nobody works (hours $= 0$)
- Treatment causes a 10 hour increase in hours worked
- Of 100 treatment group units, only 50 are actually treated (comply)

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$$\bar{Y}(D_i = 1) = 10, \text{ but } \bar{Y}(\mathbf{R_i} = 1) = 10 \times 0.5 + 0 \times 0.5 = 5$$

We can tell the same story for control units

The mean outcome for $R_i = 0$ is not equal to the mean for $D_i = 0$:

- Consider an intervention to get people into factories
- Without treatment, nobody works (hours $= 0$)
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If these both happened in the same experiment, we'd estimate an "ATE" of 0!

What do we learn from the experimental estimate?

We're no longer measuring the ATE, but:

- This measure may still be meaningful (when?)
- We are now measuring the ATE of *assigning* treatment
- Another name for this: the **intent to treat (ITT)** estimate
- We can get an unbiased estimate of the ITT thanks to random assignment

An example: Electricity pricing in Sacramento

Policy issue:

- The cost of providing electricity is time-varying
- Prices typically aren't
- This causes large welfare losses

Program:

- SMUD (randomly) implemented time-varying pricing
- Experimental run: 2011-2013
- Two flavors: "time-of-use" (TOU) and "critical peak pricing" (CPP)
- Both **opt-in** and **opt-out** versions

Randomized encouragement design: just an RCT w/ noncompliance!

Estimating the SMUD ITT

A very simple estimating equation:

$$y_{it} = \alpha + \beta_{ITT} Z_{it} + \gamma_i + \tau_t + \varepsilon_{it}$$

where:

- y_{it} is electricity use for unit i in time t
- Z_{it} is the assignment-to-treatment indicator (R_i in our world)
- γ_i and δ_t are "fixed effects" (more on these later)
- ε_{it} is an error term

The authors estimate this for each treatment (same control group)

Note that in our standard notation, the piece that matters is:

$$y_{it} = \alpha + \tau^{ITT} R_{it} + \varepsilon_{it}$$

What do we find?

Table 3: Intent to treat effects

	Critical event		Non-event peak	
	Opt-in	Opt-out	Opt-in	Opt-out
Encouragement (CPP)	−0.129***	−0.305***	−0.029***	−0.094***
	(0.010)	(0.037)	(0.006)	(0.020)
Mean usage (kW)	2.49	2.5	1.8	1.8
Customers	55,028	46,684	55,028	46,684
Customer-hours	4,832,874	4,104,263	31,198,201	26,495,612
Encouragement (TOU)	−0.091***	−0.130***	−0.054***	−0.100***
	(0.008)	(0.019)	(0.006)	(0.013)
Mean usage (kW)	2.49	2.5	1.8	1.8
Customers	55,028	46,684	55,028	46,684
Customer-hours	4,832,874	4,104,263	31,198,201	26,495,612

Getting beyond the ITT requires extra assumptions

- Let's assume some treated units don't get treated...
- ... and no control units do
- Define D_i the usual way: 1 if treated, 0 if not treated
- Define D_i^* as a **latent (unseen) variable**:

$$D_i^* = \begin{cases} 1 & \text{if control unit } i \text{ would've been treated} \\ 0 & \text{if control unit } i \text{ wouldn't have been treated} \end{cases}$$

How can we handle noncompliance?

Now we can look at people in both groups:

Treatment group ($R_i = 1$):

- $E[Y_i|R_i = 1, D_i = 1]$: units who took up treatment
- $E[Y_i|R_i = 1, D_i = 0]$: units who **did not** take up treatment

Control group ($R_i = 0$):

- $E[Y_i|R_i = 0, D_i^* = 1]$: units who would have taken up treatment
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Control group ($R_i = 0$):

- $E[Y_i | R_i = 0, D_i^* = 1]$: units who would have taken up treatment
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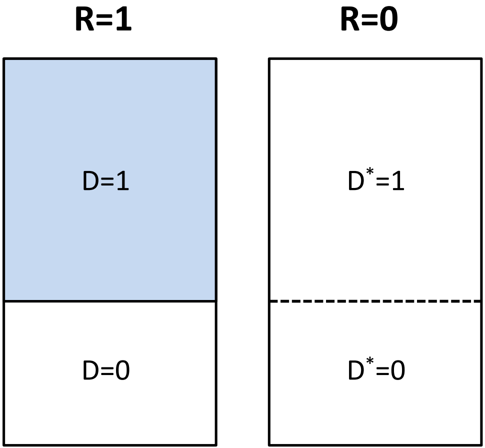
Fundamental problem: we can see who is treated in the treatment group, but can't see who **would've been** treated in the control group!

A cute visual depiction



We can do better with an additional assumption

In order for us to get an unbiased estimate, we need:

$$E[Y_i|R_i = 1, D_i = 0] = E[Y_i|R_i = 0, D_i^* = 0]$$

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In words:

Units randomized into the **treatment** group who **chose not to get treated** have the same mean outcome as those in the **control** group who **would have chosen not to be treated**

In other words:

Assignment to treatment didn't affect the likelihood of non-compliance

In other other words:

No selection into non-treatment

Under this assumption we can get back to...something

Suppose we **know** impacts on the treated (τ^T) and nontreated (τ^N):

- $\tau^T = E[Y_i|R_i = 1, D_i = 1] - E[Y_i|R_i = 0, D_i^* = 1]$
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$$\tau^{\text{Experiment}} = \underbrace{Pr(D_i = 1 | R_i = 1)}_{\text{\% of T group units treated}} \times \tau^T + \underbrace{(1 - Pr(D_i = 1 | R_i = 1))}_{\text{\% of T group units not}} \times \tau^N$$

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$$\hat{\tau}^T = \underbrace{\frac{\overline{Y}(R = 1) - \overline{Y}(R = 0)}{P^{D_i=1}_{R_i=1}}}_{\text{new definition}}$$

where $P^{D_i=1}_{R_i=1}$ is the fraction of treatment group units receiving treatment

What is τ^T?

If units who took up treatment are **not selected**:

$$\tau^T = \tau^{ATT} = \tau^{ATE}$$

- Non-selection is equivalent to **treatment effect homogeneity**

$\rightarrow$ Why?

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$\rightarrow$ Why?

If units who took up treatment **are** selected:

$$\tau^T = \tau^{ATT} \neq \tau^{ATE}$$

- Selection is equivalent to **treatment effect heterogeneity**

$\rightarrow$ Why?

What if control units can get treated?

- Suppose now control units can obtain treatment
- Just like before, $\overline{Y}(R_i = 1) - \overline{Y}(R_i = 0) = \tau^{ITT}$
- And like before, getting from the ITT to something else requires assumptions

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- **Compliers:** Treated in treatment group, untreated in control group

  $$(D_i|R_i = 1) = 1; (D_i|R_i = 0) = 0$$

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- **Compliers:** Treated in treatment group, untreated in control group

  $$(D_i|R_i = 1) = 1; (D_i|R_i = 0) = 0$$

- **Defiers:** Untreated in treatment group, treated in control group

  $$(D_i|R_i = 1) = 0; (D_i|R_i = 0) = 1$$

  $\rightarrow$ We typically assume **no defiers**

Estimating treatment effects

We observe outcomes from $R_i = 0$ and $R_i = 1$ groups:

Define $\pi^G = \frac{\text{total units of type G}}{\text{total units of all types}}$

$$E[Y_i|R_i = 1] = \pi^{NT} E[Y_i(0)|NT] + \pi^C E[Y(1)|C] + \pi^{AT} E[Y_i(1)|AT]$$

and

$$E[Y_i|R_i = 0] = \pi^{NT} E[Y_i(0)|NT] + \pi^C E[Y(0)|C] + \pi^{AT} E[Y_i(1)|AT]$$

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When we compare these groups, we get:

$$E[Y_i|R_i = 1] - E[Y_i|R_i = 0] = \pi^C (E[Y_i = 1|C] - E[Y_i = 0|C])$$

Important assumption: No defiers

If we have defiers, we end up with:

$$
\begin{aligned}
E[Y_i|R_i = 1] \\
&= \pi^{NT} E[Y_i(0)|NT] + \pi^{C} E[Y(1)|C] + \pi^{AT} E[Y_i(1)|AT] \\
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and

$$E[Y_i|R_i = 0]$$
$$= \pi^{NT} E[Y_i(0)|NT] + \pi^{C} E[Y(0)|C] + \pi^{AT} E[Y_i(1)|AT] + \pi^{DE} E[Y(1)|DE]$$

so

$$E[Y_i|R_i = 1] - E[Y_i|R_i = 0] = \pi^{C}(E[Y(1)|C] - E[Y(0)|C]) + \pi^{DE}(E[Y(0)|DE] - E[Y(1)|DE])$$

We don't know how to interpret this gross thing

Estimating the Local Average Treatment Effect

Without defiers, we can get to something interesting:

$$E[Y_i|R_i = 1] - E[Y_i|R_i = 0] = \pi^C(E[Y_i = 1|C] - E[Y_i = 0|C])$$

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$$\frac{E[Y_i|R_i = 1] - E[Y_i|R_i = 0]}{E[D_i|R_i = 1] - E[D_i|R_i = 0]} = \underbrace{\frac{\pi^C(E[Y_i = 1|C] - E[Y_i = 0|C])}{E[D_i|R_i = 1] - E[D_i|R_i = 0]}}_{\text{divide by effect of } R_i \text{ on } D_i}$$

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$$= \underbrace{\frac{\pi^C (E[Y_i(1)|C] - E[Y_i(0)|C])}{\pi^C}}_{\text{by def'n of } \pi^C}$$

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Without defiers, we can get to something interesting:

$$E[Y_i|R_i = 1] - E[Y_i|R_i = 0] = \pi^C(E[Y_i = 1|C] - E[Y_i = 0|C])$$

$$\frac{E[Y_i|R_i = 1] - E[Y_i|R_i = 0]}{E[D_i|R_i = 1] - E[D_i|R_i = 0]} = \underbrace{\frac{\pi^C(E[Y_i = 1|C] - E[Y_i = 0|C])}{E[D_i|R_i = 1] - E[D_i|R_i = 0]}}_{\text{divide by effect of } R_i \text{ on } D_i}$$

$$= \underbrace{\frac{\pi^C(E[Y_i(1)|C] - E[Y_i(0)|C])}{\pi^C}}_{\text{by def'n of } \pi^C}$$

$$= \underbrace{E[Y_i(1)|C] - E[Y_i(0)|C]}_{\text{thanks to division}}$$

Estimating the Local Average Treatment Effect

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$$= \underbrace{\frac{\pi^C(E[Y_i(1)|C] - E[Y_i(0)|C])}{\pi^C}}_{\text{by def'n of } \pi^C}$$

$$= \underbrace{E[Y_i(1)|C] - E[Y_i(0)|C]}_{\text{thanks to division}}$$

$$= \tau^{LATE}$$

The LATE is the effect of treatment for the compliers

From τ^T to τ^{LATE}

To bring us full circle, notice that:

$$\tau^T = \underbrace{\frac{\bar{Y}(R=1) - \bar{Y}(R=0)}{P^{D_i=1}_{R_i=1} - P^{D_i=1}_{R_i=0}}}_{P^{D_i=1}_{R_i=1} - P^{D_i=1}_{R_i=0} = \pi^C \text{ when both groups non-comply}}$$

From τ^T to τ^{LATE}

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In other words:

$$\tau^T = \tau^{LATE}$$

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In other words:

$$\tau^T = \tau^{LATE}$$

<u>To estimate the LATE:</u>

1. Regress Y_i on R_i to recover $\hat{\tau}^{ITT}$
2. Regress D_i on R_i to recover $\hat{\pi}^C$
3. $\hat{\tau}^{LATE} = \frac{\hat{\tau}^{ITT}}{\hat{\pi}^C}$

Back to treatment parameters

What can we estimate with non-compliance?

- **ITT**: $\bar{Y}(R_i = 1) - \bar{Y}(R_i = 0)$

Back to treatment parameters

What can we estimate with non-compliance?

- **ITT**: $\bar{Y}(R_i = 1) - \bar{Y}(R_i = 0)$
- **LATE**: $\frac{\bar{Y}(R_i=1)-\bar{Y}(R_i=0)}{\pi^C}$
  - Under constant treatment effects: equal to ATE, ATT
  - With heterogeneous treatment effects: equal to ATT
  - With defiers: equal to ☠

Estimating the SMUD τ^T

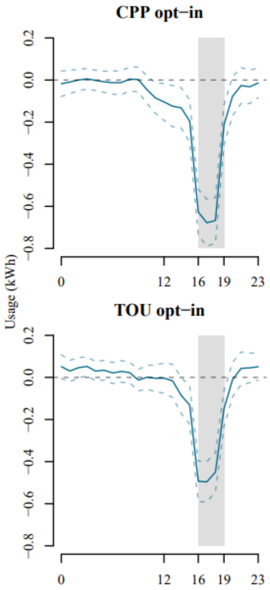
All they need to do is estimate:

$$\hat{\tau}^T = \frac{\overline{Y}(R_i = 1) - \overline{Y}(R_i = 0)}{\hat{\pi}^C}$$

$$= \hat{\tau}_{ITT} / \hat{\pi}^C$$

They do this with an "instrumental variable" (more on this in a few weeks)

What do they find?



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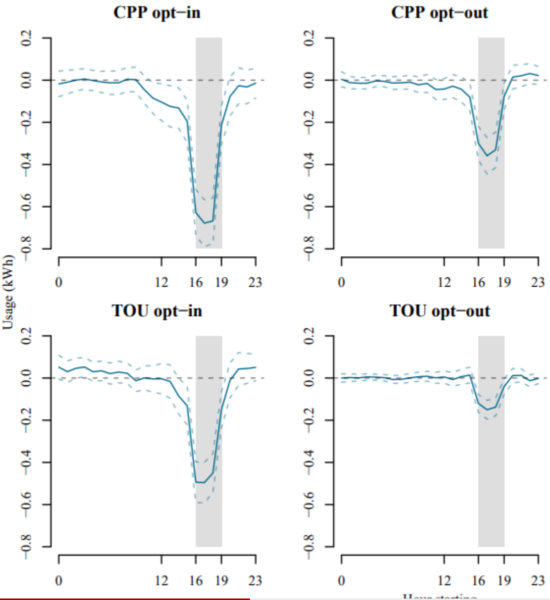

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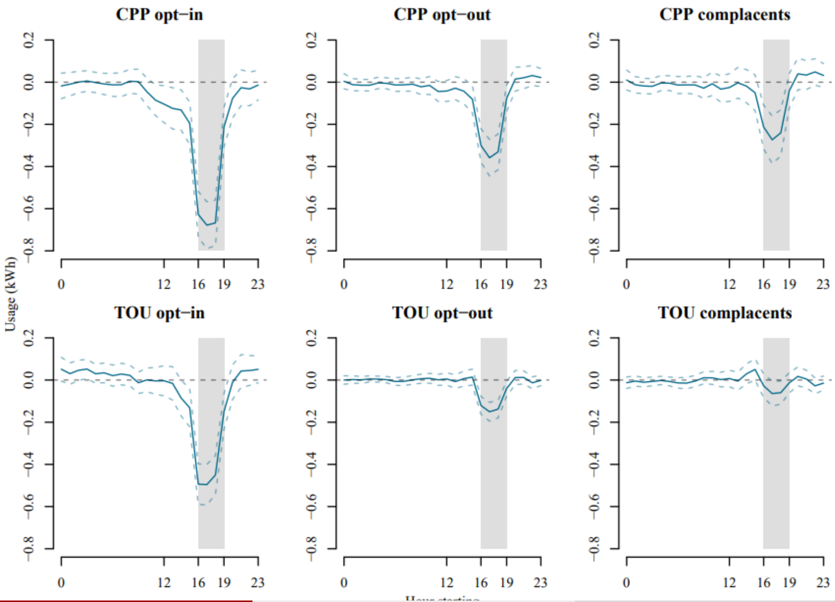

Recap

TL;DR:

1. RCTs are (still) great!
2. Non-compliance makes things more complicated
3. We can still make progress on (some) treatment parameters