The Fundamental Problem of Econometrics

EC 320: Introduction to Econometrics

Winter 2022

Prologue

Statistics Inform Policy

Policy: In 2017, the University of Oregon started requiring first-year students to live on campus.

Rationale: First-year students who live on campus fare better than those who live off campus.

- *80 percent more likely* to graduate in four years.
- Second-year retention rate *5 percentage points higher*.
- GPAs *0.13 points higher*, on average.

Do these comparisons suggest that the policy will improve student outcomes?

Do they describe the effect of living on campus?

Do they describe ***something else?***

Other Things Equal

The UO's interpretation of those comparisons warrants skepticism.

- The decision to live on campus is probably related to family wealth and interest in school.

- Family wealth and interest in school are also related to academic achievement.

Why? The difference in outcomes between those on and off campus is not an ***other things equal***[*] comparison.

Upshot: We can't attribute the difference in outcomes solely to living on campus.

[*] *Other things equal = ceteris paribus, all else held constant, etc.*

Other Things Equal

A high bar

When all other factors are held constant, statistical comparisons detect causal relationships.

(Micro)economics has developed a comparative advantage in understanding where ***other things equal*** comparisons can and cannot be made.

- Anyone can retort "*correlation doesn't necessarily imply causation*."
- Understanding *why* is difficult, but useful for learning from data.

The Fundamental Problem of Econometrics

Causal Identification

Goal

Identify the effect of a **treatment** on an **outcome**.

Ideal data

Ideally, we could calculate the **treatment effect** *for each individual* as

$$Y_{1,i} - Y_{0,i}$$

- $Y_{1,i}$ is the outcome for person i when she receives the treatment.
- $Y_{0,i}$ is the outcome for person i when she does not receive the treatment.
- Known as potential outcomes.

Causal Identification

Ideal data

The *ideal* data for 10 people

```
#>     i trt  y1i  y0i effect_i
#> 1   1   1 5.01 2.56     2.45
#> 2   2   1 8.85 2.53     6.32
#> 3   3   1 6.31 2.67     3.64
#> 4   4   1 5.97 2.79     3.18
#> 5   5   1 7.61 4.34     3.27
#> 6   6   0 7.63 4.15     3.48
#> 7   7   0 4.75 0.56     4.19
#> 8   8   0 5.77 3.52     2.25
#> 9   9   0 7.47 4.49     2.98
#> 10 10   0 7.79 1.40     6.39
```

Calculate the causal effect of treatment.

$$\tau_i = y_{1,i} - y_{0,i}$$

for each individual i.

The mean of τ_i is the **average treatment effect** (ATE).

Thus, $\bar{\tau} = 3.82$

Fundamental Problem of Econometrics

Ideal comparison

$$\tau_i = y_{1,i} - y_{0,i}$$

Highlights the fundamental problem of econometrics.

The problem

- If we observe $y_{1,i}$, then we cannot observe $y_{0,i}$.
- If we observe $y_{0,i}$, then we cannot observe $y_{1,i}$.
- Can only observe what actually happened; cannot observe the **counterfactual**.

Fundamental Problem of Econometrics

A dataset that we can observe for 10 people looks something like

```
#>     i trt  y1i  y0i
#> 1   1   1 5.01   NA
#> 2   2   1 8.85   NA
#> 3   3   1 6.31   NA
#> 4   4   1 5.97   NA
#> 5   5   1 7.61   NA
#> 6   6   0   NA 4.15
#> 7   7   0   NA 0.56
#> 8   8   0   NA 3.52
#> 9   9   0   NA 4.49
#> 10 10   0   NA 1.40
```

We can't observe $y_{1,i}$ and $y_{0,i}$.

But, we do observe

- $y_{1,i}$ for i in 1, 2, 3, 4, 5
- $y_{0,j}$ for j in 6, 7, 8, 9, 10

Q: How do we "fill in" the `NA`s and estimate $\overline{\tau}$?

Estimating Causal Effects

Notation: D_i is a binary indicator variable such that

- $D_i = 1$ if individual i is treated.
- $D_i = 0$ if individual i is not treated (*control* group).

Then, rephrasing the previous slide,

- We only observe $y_{1,i}$ when $D_i = 1$.
- We only observe $y_{0,i}$ when $D_i = 0$.

Q: How can we estimate $\bar{\tau}$ using only $(y_{1,i}|D_i = 1)$ and $(y_{0,i}|D_i = 0)$?

Estimating Causal Effects

Q: How can we estimate $\overline{\tau}$ using only $(y_{1,i}|D_i = 1)$ and $(y_{0,i}|D_i = 0)$?

Idea: What if we compare the groups' means? *I.e.*,

$$Avg(y_i \mid D_i = 1) - Avg(y_i \mid D_i = 0)$$

Q: When does a simple difference-in-means provide information on the **causal effect** of the treatment?

Q$_{2.0}$: Is $Avg(y_i \mid D_i = 1) - Avg(y_i \mid D_i = 0)$ a *good* estimator for $\overline{\tau}$?

Estimating Causal Effects

Assumption: Let $\tau_i = \tau$ for all i.

- The treatment effect is equal (constant) across all individuals i.

Note: We defined

$$\tau_i = \tau = y_{1,i} - y_{0,i}$$

which implies

$$y_{1,i} = y_{0,i} + \tau$$

Q: Is $Avg(y_i \mid D_i = 1) - Avg(y_i \mid D_i = 0)$ a *good* estimator for τ?

Difference-in-means

$$= Avg(y_i \mid D_i = 1) - Avg(y_i \mid D_i = 0)$$

$$= Avg(y_{1,i} \mid D_i = 1) - Avg(y_{0,i} \mid D_i = 0)$$

$$= Avg(\tau + y_{0,i} \mid D_i = 1) - Avg(y_{0,i} \mid D_i = 0)$$

$$= \tau + Avg(y_{0,i} \mid D_i = 1) - Avg(y_{0,i} \mid D_i = 0)$$

$$= \text{Average causal effect} + \text{Selection bias}$$

Our proposed difference-in-means estimator gives us the sum of

1. τ, the **causal, average treatment effect** that we want.
2. **Selection bias:** How much treatment and control groups differ, on average.

Randomized Control Trials

Selection Bias

Problem: Existence of selection bias precludes *all else equal* comparisons.

- To make valid comparisons that yield causal effects, we need to shut down the bias term.

Potential solution: Conduct an experiment.

- How? **Random assignment of treatment**.
- Hence the name, ***randomized*** **control trial** (RCT).

Randomized Control Trials

Example: Effect of de-worming on attendance

Motivation: Intestinal worms are common among children in less-developed countries. The symptoms of these parasites can keep school-aged children at home, disrupting human capital accumulation.

Policy Question: Do school-based de-worming interventions provide a cost-effective way to increase school attendance?

Randomized Control Trials

Example: Effect of de-worming on attendance

Research Question: How much do de-worming interventions increase school attendance?

Q: Could we simply compare average attendance among children with and without access to de-worming medication?
A: If we're after the causal effect, probably not.

Q: Why not?
A: Selection bias: Families with access to de-worming medication probably have healthier children for other reasons, too (wealth, access to clean drinking water, *etc.*).
Can't make an *all else equal* comparison. Biased and/or spurious results.

Randomized Control Trials

Example: Effect of de-worming on attendance

Solution: Run an experiment.

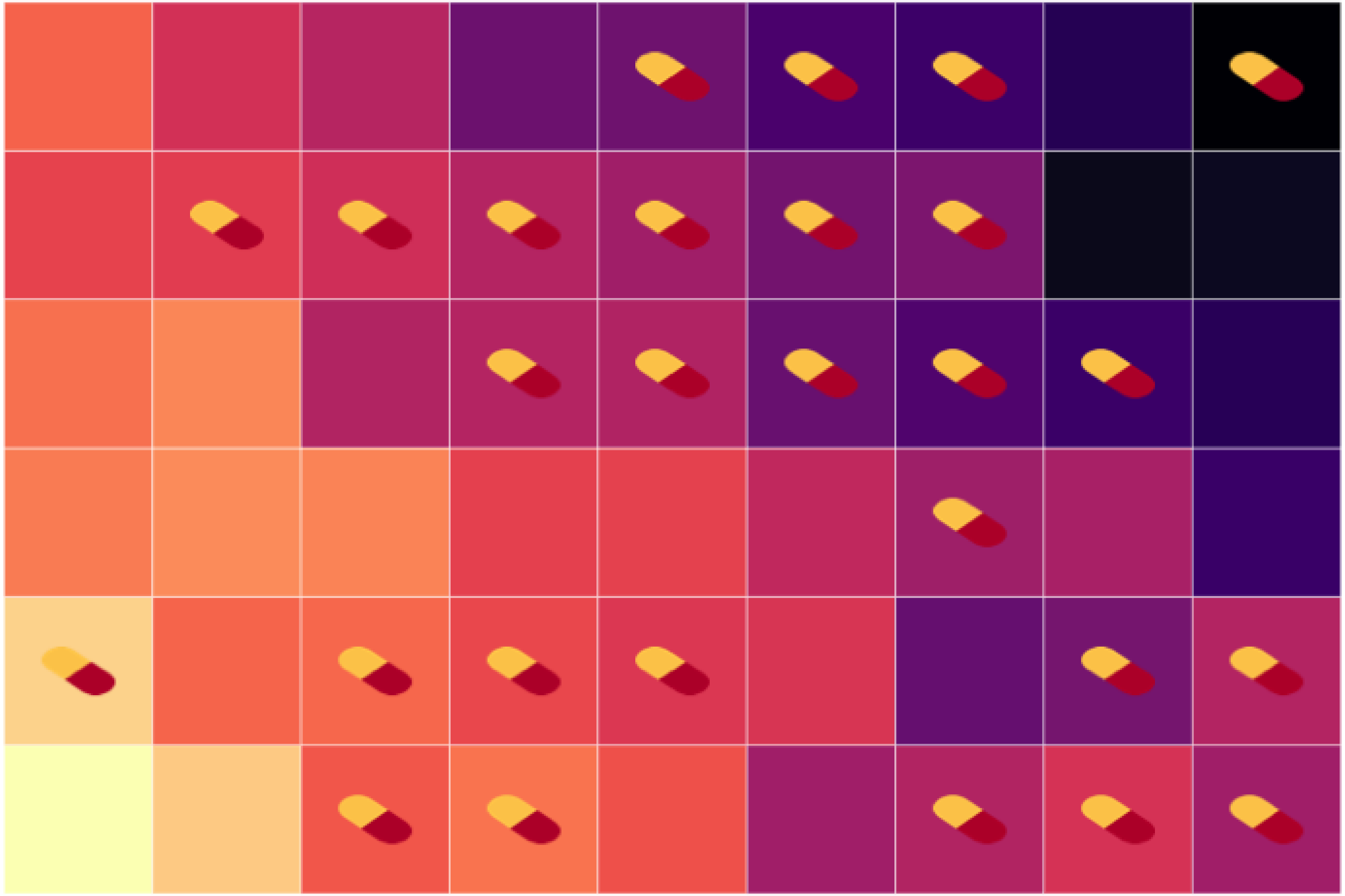
Imagine an RCT where we have two groups:

- **Treatment:** Villages that where children get de-worming medication in school.
- **Control:** Villages that where children don't get de-worming medication in school (status quo).

By randomizing villages into **treatment** or **control**, we will, on average, include all kinds of villages (poor *vs.* less poor, access to clean water *vs.* contaminated water, hospital *vs.* no hospital, *etc.*) in both groups.

All else equal!

54 villages of varying levels of development plus randomly assigned treatment



Example: Effect of de-worming on attendance

We can estimate the **causal effect** of de-worming on school attendance by comparing the average attendance rates in the treatment group (💊) with those in the control group (no 💊).

$$\overline{\text{Attendance}}_{\text{Treatment}} - \overline{\text{Attendance}}_{\text{Control}}$$

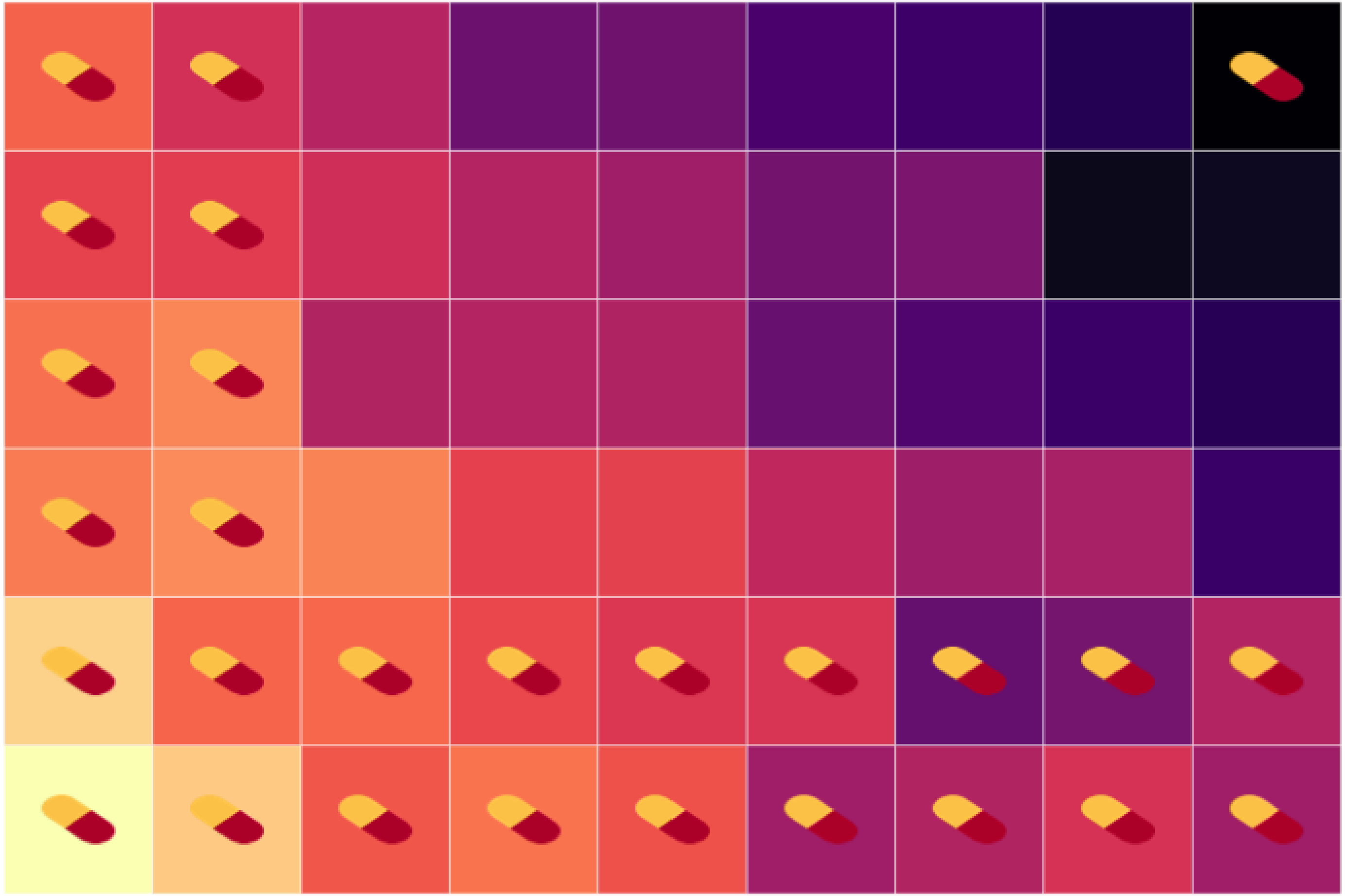
Alternatively, we can use the regression

$$\text{Attendance}_i = \beta_0 + \beta_1 \text{Treatment}_i + u_i \tag{1}$$

where Treatment_i is a binary variable (=1 if village i received the de-worming treatment). **Q:** Should trust the results of (1)? Why?

A: On average, **randomly assigning treatment should balance** treatment and control across the other dimensions that affect school attendance.

Randomization can go wrong!



Example: Returns to education

The optimal investment in education by students, parents, and legislators depends in part on the monetary *return to education*.

Thought experiment:

- Randomly select an individual.
- Give her an additional year of education.
- How much do her earnings increase?

The change in her earnings describes the **causal effect** of education on earnings.

Causality

Example: Returns to education

Q: Could we simply compare the earnings those with more education to those with less?
A: If we want to measure the causal effect, probably not.

1. People *choose* education based on their ability and other factors.
2. High-ability people tend to earn more *and* stay in school longer.
3. Education likely reduces experience (time out of the workforce).

Point (3) also illustrates the difficulty in learning about the effect of education while *holding all else constant*.

Many important variables have the same challenge: gender, race, income.

Example: Returns to education

Q: How can we estimate the returns to education?

Option 1: Run an **experiment**.

- Randomly assign education (might be difficult).
- Randomly encourage education (might work).
- Randomly assign programs that affect education (*e.g.*, mentoring).

Option 2: Look for a ***natural experiment*** (a policy or accident in society that arbitrarily increased education for one subset of people).

- Admissions cutoffs