

# The Experimental Ideal

EC 607, Set 02

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Edward Rubin

Spring 2021

# Prologue

# Schedule

## Last time

Research basics, our class, and R

## Today

**Admin:** Zoom recordings on Canvas.

**Material:** The Rubin causal model (not mine), [Chapter 2 MHE](#).

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## Future

**Lab:** Meet Philip and start deepening R knowledge.

**Long run:** Deepen understandings/intuitions for causality and inference.

# Review

*Research fundamentals*

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## Research fundamentals

Angrist and Pischke provide four **fundamental questions for research**:

1. What is the **causal relationship of interest**?
2. How would an **ideal experiment** capture this causal effect of interest?
3. What is your **identification strategy**?
4. What is your **mode of inference**?



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3. What is your **identification strategy**?
4. What is your **mode of inference**?

Seemingly straightforward questions can be fundamentally unanswerable.

# Review

## General research recommendations

More unsolicited advice:

- Be curious.
- Ask questions.
- Attend seminars.
- Meet faculty (UO + visitors).
- Focus on learning—especially intuition.<sup>†</sup>
- **Be kind and constructive.**

<sup>†</sup> *Learning* is not always the same as getting good grades.

# The experimental ideal

# The experimental ideal

## What's so great about experiments?

Science widely regards **experiments as the gold standard** for research.

*But why?* The costs can be substantial.

### **Costs**

- slow and expensive
- heavily regulated by (risk-averse?) review boards
- can abstract away from the actual question/setting

### **Benefits**

So the benefits need to be pretty large, right?

# The experimental ideal

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### Empirical exercise

1. Collect data on *health status* and *hospital visits*.
2. Summarize health status by hospital-visit group.

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Our empirical exercise from the 2005 National Health Interview Survey:

<b>Group</b>	<b>Sample Size</b>	<b>Mean Health Status</b>	<b>Std. Error</b>
Hospital	7,774	3.21	0.014
No hospital	90,049	3.93	0.003



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- Binary treatment variable (*e.g.*, hospitalized):  $D_i = 0, 1$
- Outcome for individual  $i$  (*e.g.*, health):  $Y_i$

This framework has a few names...

- Neyman potential outcomes framework
- Rubin causal model
- Neyman-Rubin `"potential outcome"|"causal" "framework"|"model"`

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The difference between these two outcomes gives us the **causal effect of hospital treatment**, *i.e.*,

$$\tau_i = Y_{1i} - Y_{0i}$$

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This simple equation

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leads us to ***the fundamental problem of causal inference.***

■ We can never simultaneously observe  $Y_{1i}$  and  $Y_{0i}$ .

Most of applied econometrics focuses on addressing this simple problem.

Accordingly, our methods try to address the related question

■ For each  $Y_{1i}$ , what is a (reasonably) good counterfactual?

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**Q** This comparison will return *an* answer, but is it *the* answer we want?

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Now write out our expectation, apply this definition, do creative math.

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$$E[Y_{0i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 0]$$

The difference in the average untreated outcome between the treatment and control groups.

**Selection bias** The extent to which the "control group" provides a bad counterfactual for the treated individuals.

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Angrist and Pischke (MHE, p. 15),

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The goal of most empirical economic research is to overcome selection bias, and therefore to say something about the causal effect of a variable like  $\mathbf{D}_i$ .

**Q** So how do experiments—the gold standard of empirical economic (and scientific) research—accomplish this goal and overcome selection bias?

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## Back to experiments

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*In other words:* Randomly assigning  $D_i$  makes  $D_i$  independent of which outcome we observe (meaning  $Y_{1i}$  or  $Y_{0i}$ ).

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meaning the control group's mean now provides a good counterfactual for the treatment group's mean.

In other words, there is no selection bias, *i.e.*,

$$\text{Selection bias} = E[Y_{0i} | D_i = 1] - E[Y_{0i} | D_i = 0] = 0$$

# The experimental ideal

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Additional benefit of randomization:

The *average treatment effect* is now representative of the *population average*, rather than the *treatment-group average*.

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## Example: Training programs

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**Q** Do these programs have the desired effects (*i.e.*, increase wages)?

**A** Observational studies—comparing wage data from participants and non-participants—often find that people who complete these programs actually make **lower wages**.

**Challenges** Participants self select. + Programs target lower-wage workers.

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## Example: Training programs

How do we formalize these concerns in our framework?



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### Observational program evaluations

$$E[\text{Wage}_i \mid \text{Program}_i = 1] - E[\text{Wage}_i \mid \text{Program}_i = 0] =$$

$$\underbrace{E[\text{Wage}_{1i} \mid \text{Program}_i = 1] - E[\text{Wage}_{0i} \mid \text{Program}_i = 1]}_{\text{Average causal effect of training program on wages for participants, i.e., } \bar{\tau}_1} +$$

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$$\underbrace{E[\text{Wage}_{0i} \mid \text{Program}_i = 1] - E[\text{Wage}_{0i} \mid \text{Program}_i = 0]}_{\text{Selection bias}}$$

If the program attracts/selects individuals who, on average, have lower wages without the program (sort of the point of the program), then we have negative selection bias.

# The experimental ideal

## Example: Training programs

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So even if the program, on average, has a positive wage effect (in the participant group), *i.e.*,  $\bar{\tau}_1 > 0$ , we will detect a lower effect due to the negative selection bias.

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If the bias is sufficiently large (relative to the treatment effect), our estimate will even get the sign of the effect wrong.

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So even if the program, on average, has a positive wage effect (in the participant group), *i.e.*,  $\bar{\tau}_1 > 0$ , we will detect a lower effect due to the negative selection bias.

If the bias is sufficiently large (relative to the treatment effect), our estimate will even get the sign of the effect wrong.

**Related** While observational studies typically found negative program effects, several experiments found positive program effects.

# The experimental ideal

## Example: The STAR experiment

The Tennessee STAR experiment is a famous/popular example of an experiment that allows us to answer an important social/policy question.

**Research question** Do classroom resources affect student performance?

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### Treatments

1. *Small* classes (13–17 students)
2. *Regular* classes (22–35 students) plus part-time teacher's aide
3. *Regular* classes (22–35 students) plus full-time teacher's aide



# The experimental ideal

## Example: The STAR experiment

**First question** Did the randomization balance participants' characteristics across the treatment groups?

# The experimental ideal

## Example: The STAR experiment

**First question** Did the randomization balance participants' characteristics across the treatment groups?

Ideally, we would have pre-experiment data on outcome variable.

Unfortunately, we only have a few demographic attributes.

Table 2.2.1, MHE

<b>Treatment: Class Size</b>				
<b>Variable</b>	<b>Small</b>	<b>Regular</b>	<b>Regular + Aide</b>	<b>P-value</b>
<i>Free lunch</i>	0.47	0.48	0.50	0.09
<i>White/Asian</i>	0.68	0.67	0.66	0.26
<i>Age in 1985</i>	5.44	5.43	5.42	0.32
<i>Attrition rate</i>	0.49	0.52	0.53	0.02
<i>K. class size</i>	15.10	22.40	22.80	0.00
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Demographics appear balanced across the three treatment groups.

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The randomization generated variation in the treatment.

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The small-class treatment significantly increased test scores.

# The experimental ideal

## The STAR experiment

The previous table estimated/compared the treatment effects using simple differences in means.

We can make the same comparisons using regressions.

Specifically, we regress our outcome (test percentile) on dummy variables (binary indicator variables) for each treatment group.



# The experimental ideal

Example of our three treatment dummies.

$i$	$y_i$	$\text{Trt}_{1i}$	$\text{Trt}_{2i}$	$\text{Trt}_{3i}$
1	$y_1$	1	0	0
2	$y_2$	1	0	0
$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$
$\ell$	$y_\ell$	1	0	0
$\ell + 1$	$y_{\ell-1}$	0	1	0
$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$
$p$	$y_p$	0	1	0
$p + 1$	$y_{p+1}$	0	0	1
$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$
$N$	$y_N$	0	0	1

# The experimental ideal

## Regression analysis

Assume for the moment that the treatment effect is constant<sup>†</sup>, *i.e.*,

$$Y_{1i} - Y_{0i} = \rho \quad \forall i$$

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as

$$Y_i = \underbrace{\alpha}_{=E[Y_{0i}]} + D_i \underbrace{\rho}_{Y_{1i} - Y_{0i}} + \underbrace{\eta_i}_{Y_{0i} - E[Y_{0i}]}$$

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Take the difference...

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$$E[Y_i | D_i = 1] - E[Y_i | D_i = 0] = \rho + E[\eta_i | D_i = 1] - E[\eta_i | D_i = 0]$$

Again, our estimate of the **treatment effect** ( $\rho$ ) is only going to be as good as our ability to shut down the **selection bias**.

**Selection bias in regression model:**  $E[\eta_i | D_i = 1] - E[\eta_i | D_i = 0]$

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There is something in our disturbance  $\eta_i$  that is affecting  $Y_i$  and is also correlated with  $D_i$ .

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In other metrics-y words: Our treatment  $D_i$  is endogenous.



# The experimental ideal

## Solutions and covariates

***Selection bias in regression model:***  $E[\eta_i | \mathbf{D}_i = 1] - E[\eta_i | \mathbf{D}_i = 0]$

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Another potential route to identification is to condition on covariates in the hopes that they "take care of" the relationship between  $\mathbf{D}_i$  and whatever is in our disturbance  $\eta_i$ .

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Without very clear reasons explaining how you know you've controlled for the "bad variation", clean and convincing identification on this path is going to be challenging.

# The experimental ideal

## Covariates

That said, covariates can help with two things:

1. Even experiments may need **conditioning/controls**: The STAR experiment was random *within school*—not across schools.
2. Covariates can soak up unexplained variation—**increasing precision**.

# The experimental ideal

## Covariates

That said, covariates can help with two things:

1. Even experiments may need **conditioning/controls**: The STAR experiment was random *within school*—not across schools.
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Now that we've seen regression can analyze experiments, let's estimate the STAR example...

Table 2.2.2, MHE

<b>Explanatory variable</b>	<b>1</b>	<b>2</b>	<b>3</b>
<i>Small class</i>	4.82 (2.19)	5.37 (1.26)	5.36 (1.21)
<i>Regular + aide</i>	0.12 (2.23)	0.29 (1.13)	0.53 (1.09)
<i>White/Asian</i>			8.35 (1.35)
<i>Female</i>			4.48 (0.63)
<i>Free lunch</i>			-13.15 (0.77)
<i>School F.E.</i>	F	T	T

The omitted level is *Regular* (with part-time aide).

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Results without other controls are very similar to the difference in means.



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School FEs enforce the experiment's design and increase precision.

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Additional controls slightly increase precision.

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