

Data and Causation

EC 350: Labor Economics

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Spring 2021

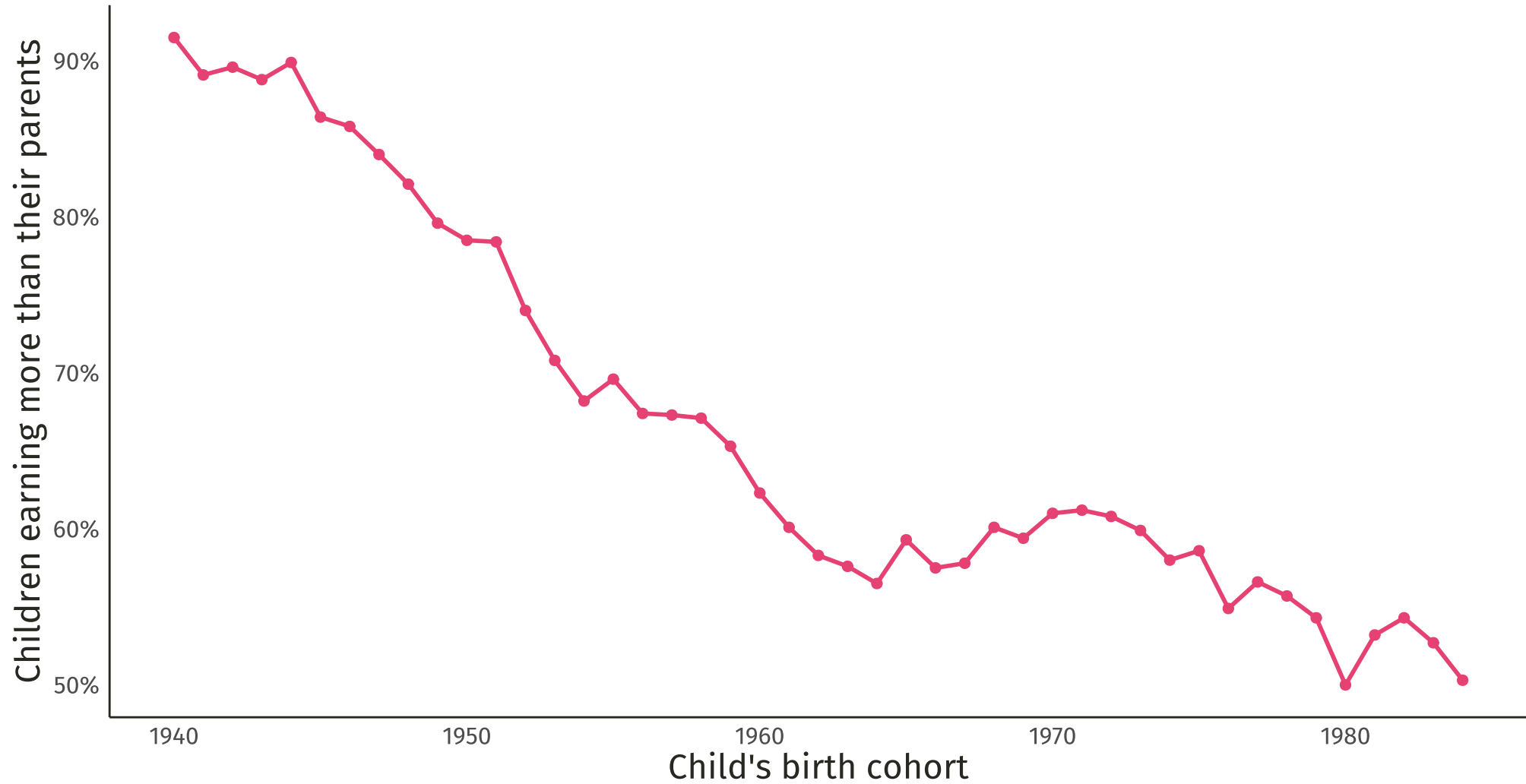
Prologue

Data and Causation

1. The rise of empirical evidence
2. Making *other-things-equal* comparisons
3. What do "data" look like?
4. Causal identification
 - Average treatment effects
 - Selection bias
5. Randomized control trials
6. *Thinking Fast and Slow*, Chicago edition

The rise of empirical evidence

The fading *American dream*



Source: Raj Chetty et al. (2017), [The fading American dream: Trends in absolute income mobility since 1940](#), *Science*.

Why is the *American dream* fading?

Policy Question: Why is a child's chance of climbing the income ladder decreasing in the United States?

- What can we do to reverse this trend?

Difficult to answer with historical data on macroeconomic trends.

- **That other things change over time makes it difficult** to separately identify the roles of alternative explanations

Theoretical social science

Historically, the social sciences had **limited data** to study policy questions.

The result? Social sciences were **theoretical** fields

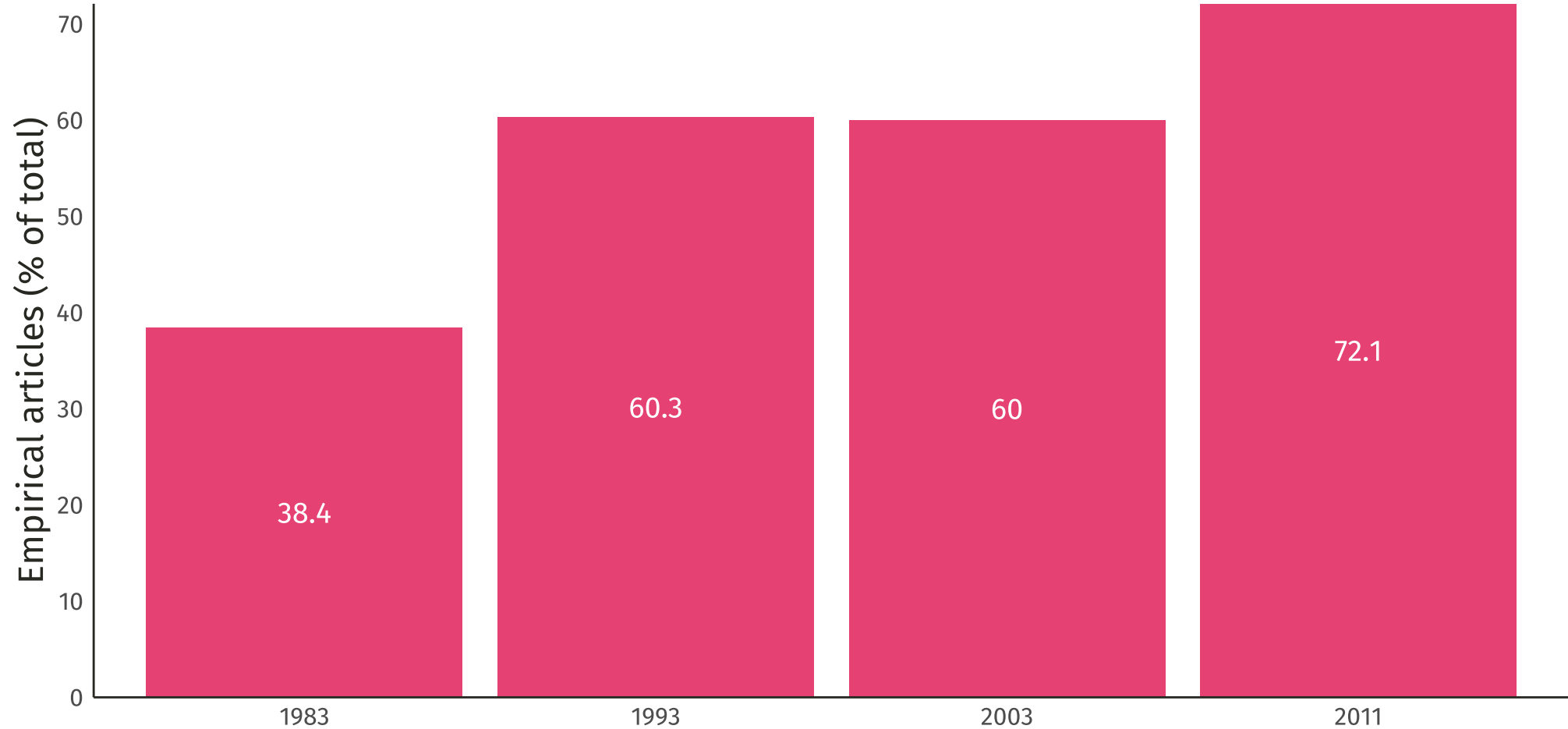
- Some researchers developed **mathematical models**
- Some developed **qualitative theories**
- Both used their theories to make policy recommendations (e.g., to improve upward mobility)

The problem? Without data, even falsifiable theories are never tested!

- Five researchers could have five different answers to the same question
- Can lead to a politicization of questions that, in principle, have scientific answers (e.g., do minimum wage laws reduce employment?)

Economics is becoming more data-driven

Empirical articles in the top three economics journals over time



Source: Daniel S. Hamermesh (2013), [Six Decades of Top Economics Publishing: Who and How?](#) *Journal of Economic Literature*.

The rise of empirical evidence

Today, the social sciences are increasingly **empirical** thanks to the growing availability of data and computational power.

- Gives us **the ability to test** existing theories
- Gives us **the ability to refine** theory to (i) better explain decision making and (ii) better fit real-world data

The social sciences have caught up to the natural sciences in terms of scientific rigor, arguably surpassing the natural sciences in sophistication.

- Given the complexity of human decision making, inability to experiment in controlled environments, *etc.?*

Making *other-things-equal* comparisons

Other-things-equal comparisons

The policy? In 2017, the University of Oregon started requiring first-year students to live on campus.

The rationale? First-year students who live on campus outperform those who live off campus.

- Average 2nd-year retention rate *5 percentage points higher*
- *80 percent more likely to graduate* in four years
- GPA *0.13 points higher*

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

Q: Do they describe the effect of living on campus?

Q: Do they describe *something else*?

Other-things-equal comparisons

Healthy skepticism should leave us questioning the UO's interpretation.

- The **decision** to live on campus is likely related to family wealth and interest in school.
- Family wealth and interest in school are also related to academic achievement.

The difference in outcomes between those on and off campus **does not offer an other-things-equal comparison.**

- Without further evidence, one should not attribute the difference in outcomes to living on campus.
 - Not without considering those things that both (i) correlate with living on campus (e.g., family wealth) and (ii) correlate with outcomes (e.g., graduation)

Other-things-equal comparisons

Statistical comparisons can only identify causal relationships between variables **when all other factors are "held constant."**

- *Causal* relationship = How a change in one variable *induces* a change in another

Economists have developed a *comparative advantage*[†] in understanding where **other-things-equal** comparisons can (and cannot) be made.

- Anyone can retort "*correlation doesn't imply causation!*"
- Understanding why it doesn't? The conditions under which it actually does imply causality?
 - Difficult, but necessary for learning from data!

[†] *Comparative advantage* = Ability of an individual or group to perform an activity at lower cost relative to another individual or group.

What do "data" look like?

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Sample of US workers (1976)

	Wage ↕	Education ↕	Female? ↕
1	3.1	11	1
2	3.24	12	1
3	3	11	0
4	6	8	0
5	5.3	12	0

Showing 1 to 5 of 526 entries

Previous 1 2 3 4 5 ... 106

Next

Rows represent **observations**.

Columns represent **variables**.

Each value is associated with an **observation** and a **variable**.

Causal identification

Causal identification

The objective

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

The ideal comparison

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

Causal identification

The **ideal data** for 10 people

```
#>      i treat Y_1i Y_0i effect_i
#> 1     1     1 5.01 4.56      0.45
#> 2     2     1 8.85 4.53      4.32
#> 3     3     1 6.31 4.67      1.64
#> 4     4     1 5.97 4.79      1.18
#> 5     5     1 7.61 6.34      1.27
#> 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
```

We could calculate the treatment effect for each individual i ,

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

and we would be inclined to think of it as the causal effect.

The mean of these individual treatment effects = 2.82

- We call this the **average treatment effect** (ATE)

Causal identification

The fundamental problem of causal inference

While the ideal comparison is

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

this comparison is fundamentally challenged!

- If we observe Y_1 for i , then we cannot observe Y_0 for i
- If we observe Y_0 for i , then we cannot observe Y_1 for i
- We only observe what *actually* happened—we cannot observe the **counterfactual**

The implication? **ALL** causal inference is **by assumption!**

Causal identification

The data we *actually* see for these 10 people?

```
#>      i treat Y_1i Y_0i
#> 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 only observe Y_1 for $i \in \{1, \dots, 5\}$

We only observe Y_0 for $i \in \{6, \dots, 10\}$

We do not observe both $Y_{1,i}$ and $Y_{0,i}$ for anyone

Q: How can we estimate the average treatment effect when we cannot observe individual treatment effects?

Causal identification

Can we **compare the mean outcomes** of each group?

- Take the average of Y_1 for those who received the treatment (*i.e.*, the **treatment-group mean**)
- Take the average of Y_0 for those who didn't receive the treatment (*i.e.*, the **control-group mean**)

Q: Does **treatment-group mean** – **control-group mean** isolate the causal effect of the treatment?

Causal identification

```
#>      i treat Y_1i Y_0i
#> 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
```

Treatment group mean = 6.75

Control group mean = 2.82

Difference-in-means = 3.93

Difference-in-means = **average treatment effect** + **selection bias**
= 2.82 + (3.93 - 2.82) = 2.82 + 1.11

Selection bias $\neq 0 \implies$ people who "select into" treatment are different

Randomized control trials

Randomized control trials

Overcoming selection bias

The problem? The existence of selection bias precludes making *other-things-equal* comparisons.

- To make valid comparisons that identify causal effects, we need to shut down the bias coming from selection.

The solution? Conduct an experiment!

- How? Assign treatment **randomly**
- 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. (Why not?)

Selection bias: Families with access to de-worming medication probably have healthier children for other reasons, too (wealth, access to clean drinking water, etc.).

- **We can't make an *all-else-equal* comparison** → in expectation, observed differences will deviate *systematically* from the ATE!

Randomized control trials

Example: Effect of de-worming on attendance

Solution: Run an experiment.

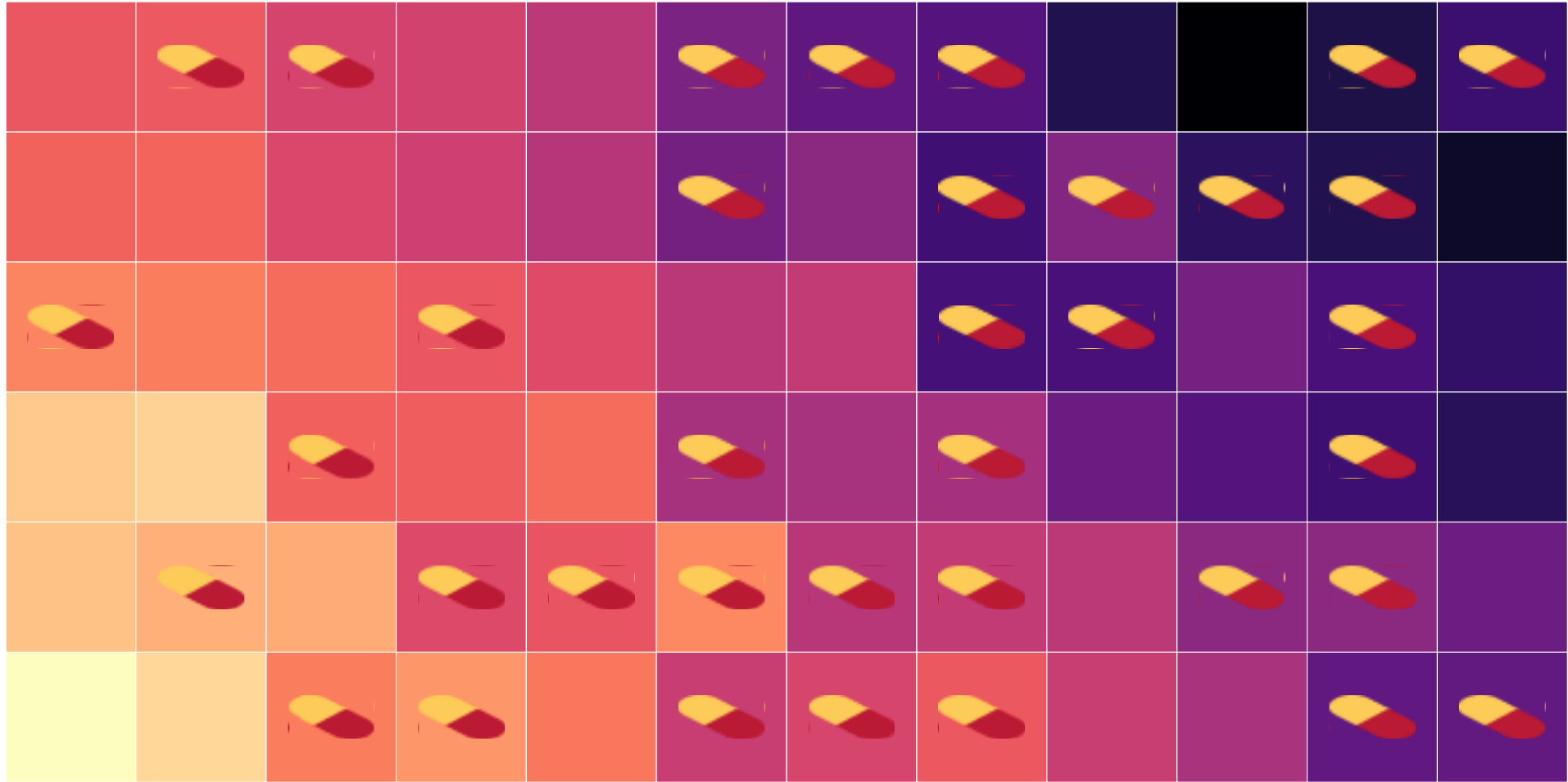
Imagine an RCT where we have two groups:

- **Treatment:** Villages where children get de-worming medication in school.
- **Control:** Villages 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!

72 villages of varying levels of development + randomly assigned treatment



Randomized control trials

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 💊):

$$\text{Treatment group attendance rate} - \text{Control group attendance rate}$$

Result: This was done in Kenya, where **attendance increased** with the random assignment of treatment.

- 25-percent decrease in absenteeism at a cost of \$0.60 per child
- Long term cost effectiveness: Additional 11.91 years of schooling per \$100 spent on de-worming

Randomized control trials

Example: Effect of de-worming on attendance

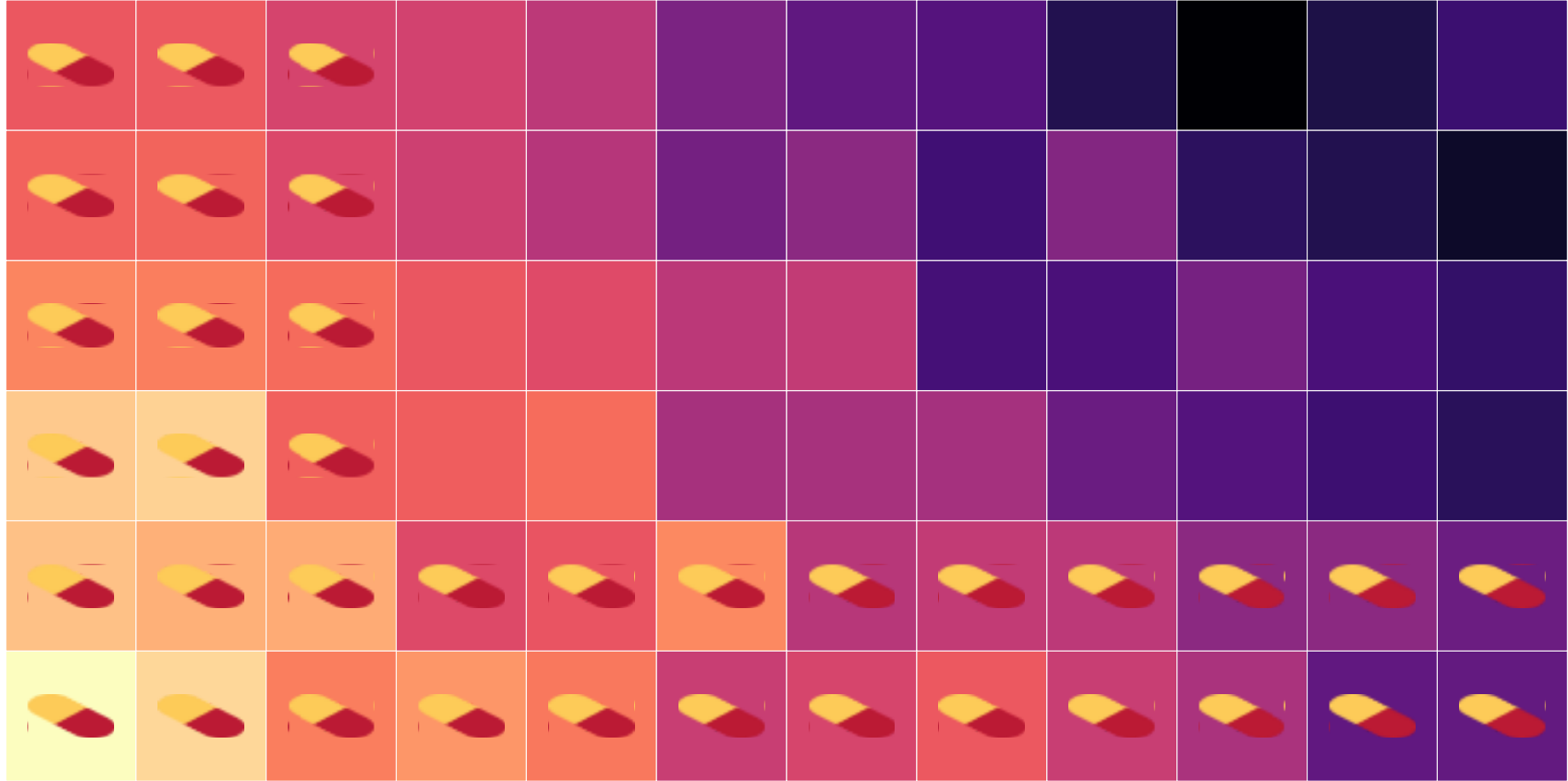
We can estimate the **causal effect** of de-worming on school attendance by **comparing the average attendance rates** in the **treatment group** (💊) with those in the **control group** (no 💊):

$$\text{Treatment group attendance rate} - \text{Control group attendance rate}$$

Q: Should we trust the results of the comparison?

A: Even with healthy skepticism, we probably should? On average, randomly assigning treatment balances the treatment and control groups across other dimensions that could explain school attendance.

Balance *on average* \neq Balance *every time*



Interpreting results

Internal validity

Addresses the question, ***should we believe the study?***

A study has high **internal validity** if, within the context of the study, we are confident that one variable has a **causal** influence on the outcome of interest (e.g., there's **no selection bias**).

External validity

Addresses the question, ***how far can we generalize the results of the study?***

A study has high **external validity** to the extent that the results **apply to other contexts** (not just the local environment that generated the results).

Thinking Fast and Slow, Chicago edition

Thinking Fast and Slow, Chicago edition

Background

Policy question: How can we reduce violent crime among young men?

Research agenda: What factors influence an individual's proclivity toward violent crime?

- Self control? Social skills? Grit?
- Economic hardship?
- Police presesnce?
- Early chilhood education?
- Something else?

Thinking Fast and Slow, Chicago edition

Research question: Can cognitive-behavioral therapy keep young men in school and out of trouble?

- Proposed mechanism: Automaticity.

Experiment: *Becoming a Man*

4804 young men in Chicago Public Schools randomly assigned to one of two groups:

- **Treatment group:** Group cognitive-behavioral therapy program during school (once per week for 1-2 school years)
- **Control group:** No intervention

A similar experiment was also conducted in the Cook County Juvenile Temporary Detention Center.

Source: Sara B Heller et al. (2017), *Thinking, Fast and Slow? Some Field Experiments to Reduce Crime and Dropout in Chicago*, *The Quarterly Journal of Economics*.

Becoming a Man: Experimental results

Outcome	Control mean	Treatment mean	Effect of treatment assignment	Effect of participation
<i>School engagement index</i>	0	0.04	0.04	0.088
			(0.016)	(0.034)
<i>Total arrests per youth per year</i>	0.603	0.53	-0.073	-0.161
			(0.031)	(0.068)
<i>Violent</i>	0.136	0.109	-0.027	-0.06
			(0.011)	(0.024)
<i>Property</i>	0.069	0.072	0.003	0.006
			(0.008)	(0.018)
<i>Drug</i>	0.132	0.127	-0.005	-0.011
			(0.012)	(0.027)
<i>Other</i>	0.266	0.222	-0.044	-0.097
			(0.019)	(0.040)

Notes: 4804 observations. Standard errors in parentheses.

Housekeeping

Assigned reading for ~~Wednesday~~ Friday: Snapping back: Food stamp bans and criminal recidivism by Cody Tuttle (2019).

- I will post the paper on Canvas tonight!
- Reading Quiz 3 will be due this Friday.
- Details to follow!