Data and Causation

EC 350: Labor Economics

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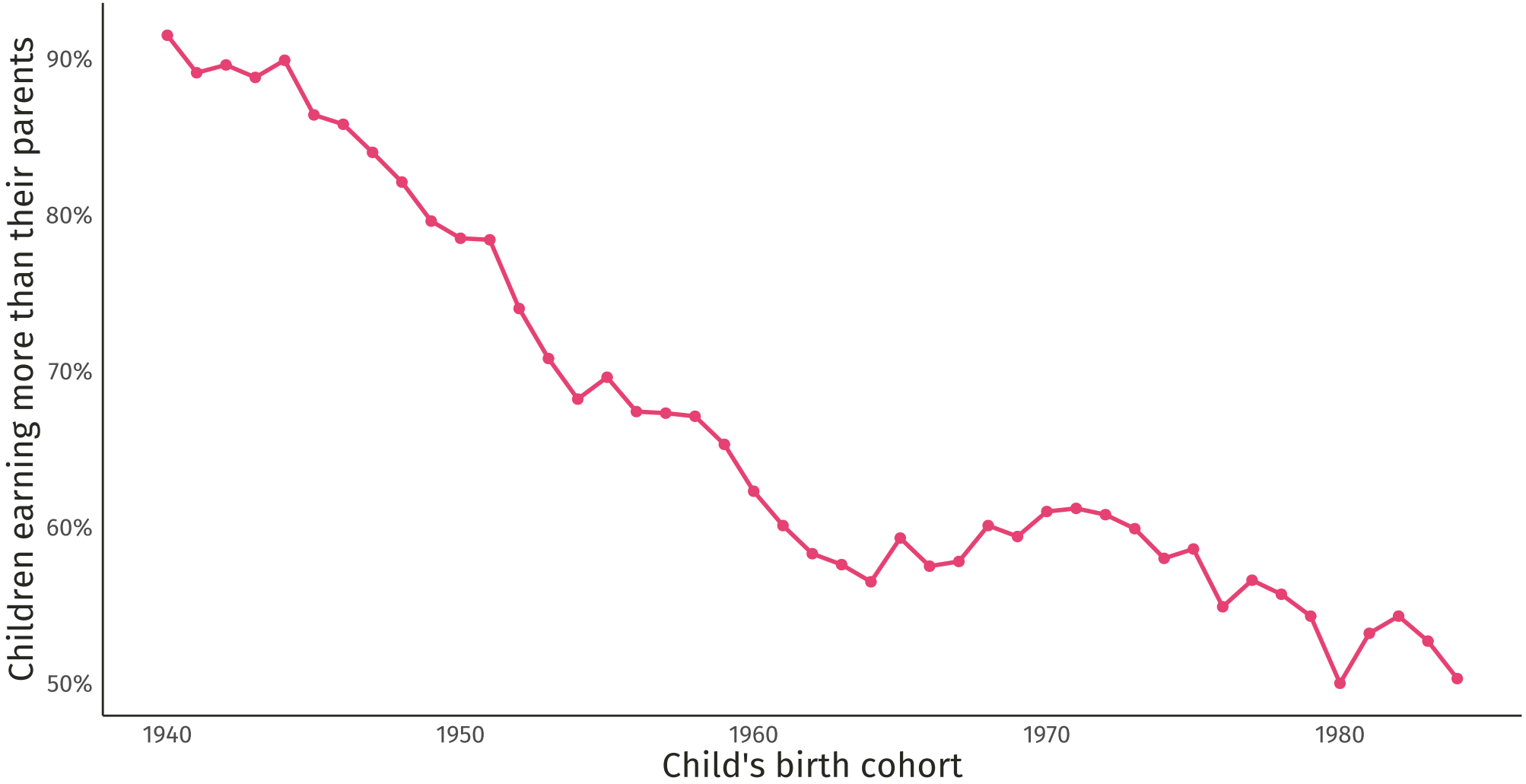
Data and Causation

1. The rise of empirical evidence
2. Making *other-things-equal* comparisons
3. Causal identification
    - Average treatment effects
    - Selection bias
4. Randomized control trials
5. *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

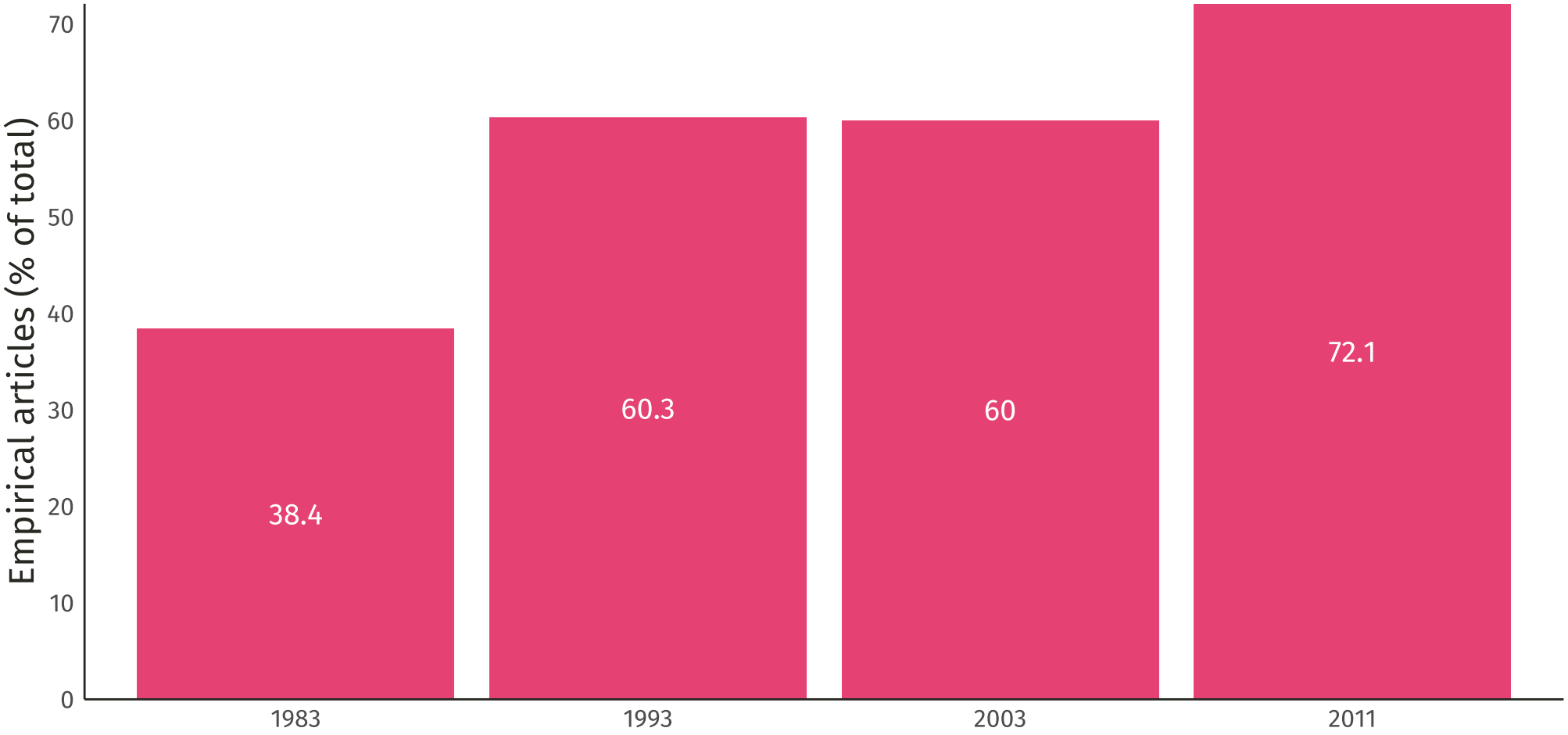
- Some researchers developed mathematical models
- Some developed qualitative theories
- Both used their theories to make policy recommendations (*e.g.*, to improve upward mobility)

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.

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
```

The mean of these individual treatment effects = 2.82

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

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.

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 $i \in \{1, \ldots, 5\}$

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

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** $\longrightarrow$ in expectation, observed differences will deviate *systematically* from the ATE!

Randomized control trials

Example: Effect of de-worming on attendance

Solution: Run an experiment.

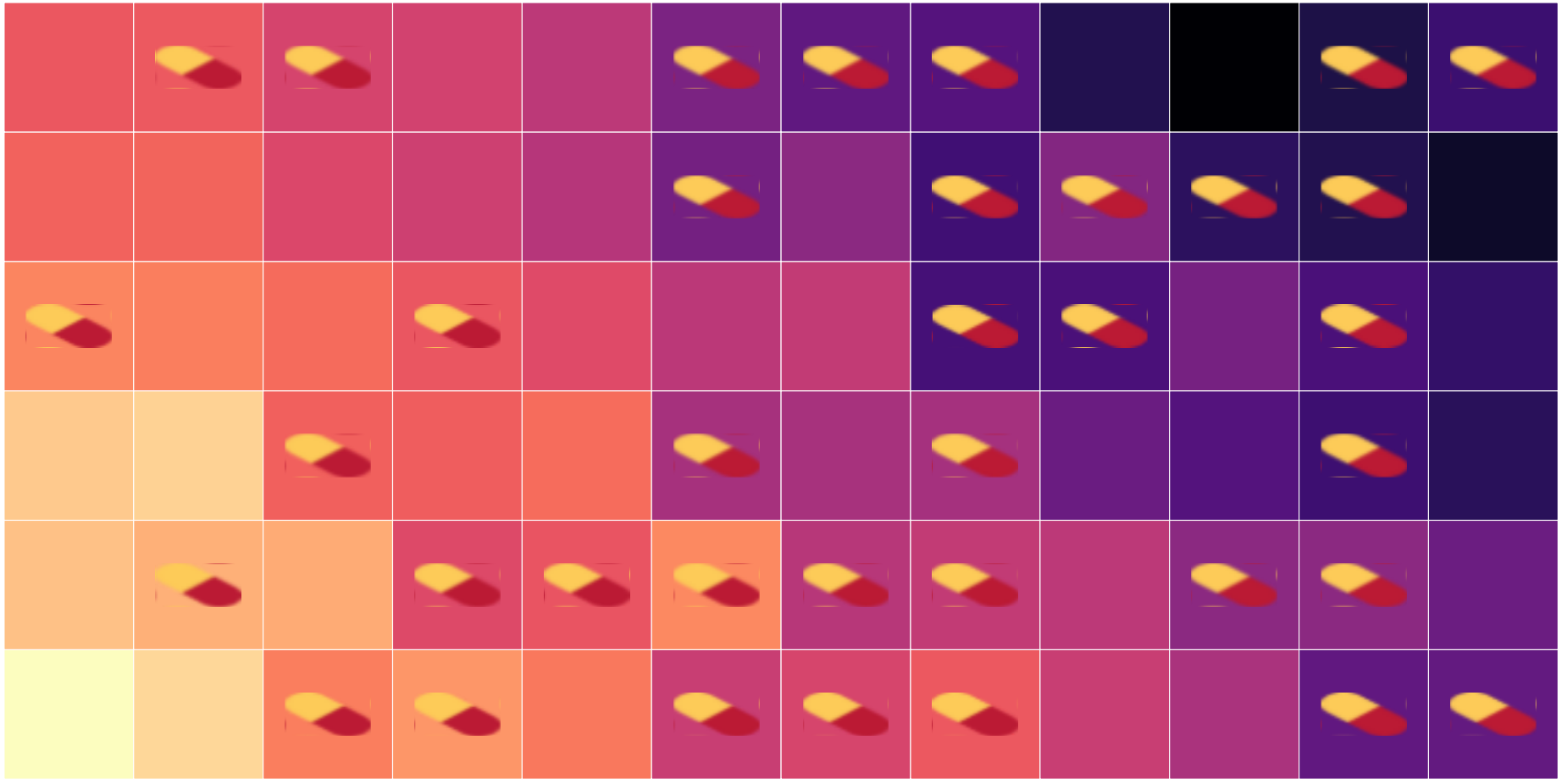
Imagine an RCT where we have two groups:

- **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 💊):

Treatment group attendance rate – **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

Source: Deworming to increase school attendance, *Abdul Latif Jameel Poverty Action Lab.*

Randomized control trials

Example: Effect of de-worming on attendance

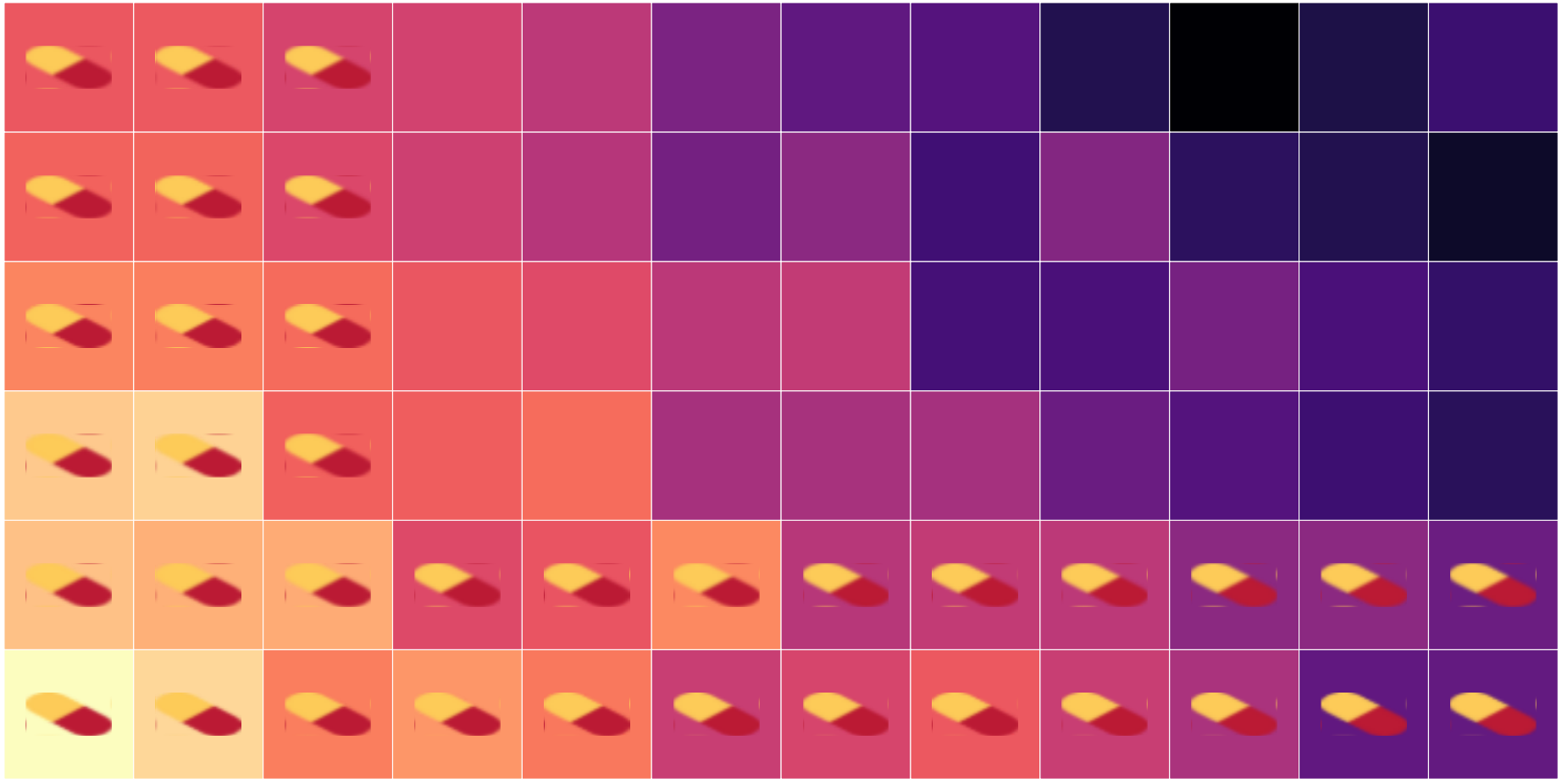
We can estimate the **causal effect** of de-worming on school attendance by **comparing the average attendance rates** in the **treatment group** (💊) with those in the **control group** (no 💊):

Treatment group attendance rate – **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
School engagement index	0	0.04	0.04	0.088
			(0.016)	(0.034)
Total arrests per youth per year	0.603	0.53	-0.073	-0.161
			(0.031)	(0.068)
Violent	0.136	0.109	-0.027	-0.06
			(0.011)	(0.024)
Property	0.069	0.072	0.003	0.006
			(0.008)	(0.018)
Drug	0.132	0.127	-0.005	-0.011
			(0.012)	(0.027)
Other	0.266	0.222	-0.044	-0.097
			(0.019)	(0.040)

Notes: 4804 observations. Standard errors in parentheses.