The fundamental problem of causal inference

EC 320, Set 03

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Spring 2023

Housekeeping

PS02:

- Posted earlier today
- Due next Tuesday at 11:59p
- Covers remaining topics from the review and from lecture today

LA02:

- Due Friday at 5:00p

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:

- ***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 the policy improves 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 related to academic achievement.

Why might I be worried?

> The difference in outcomes between those on and off campus is not an *all else equal* comparison.

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

Living on campus (**D**) increases student welfare (**Y**)

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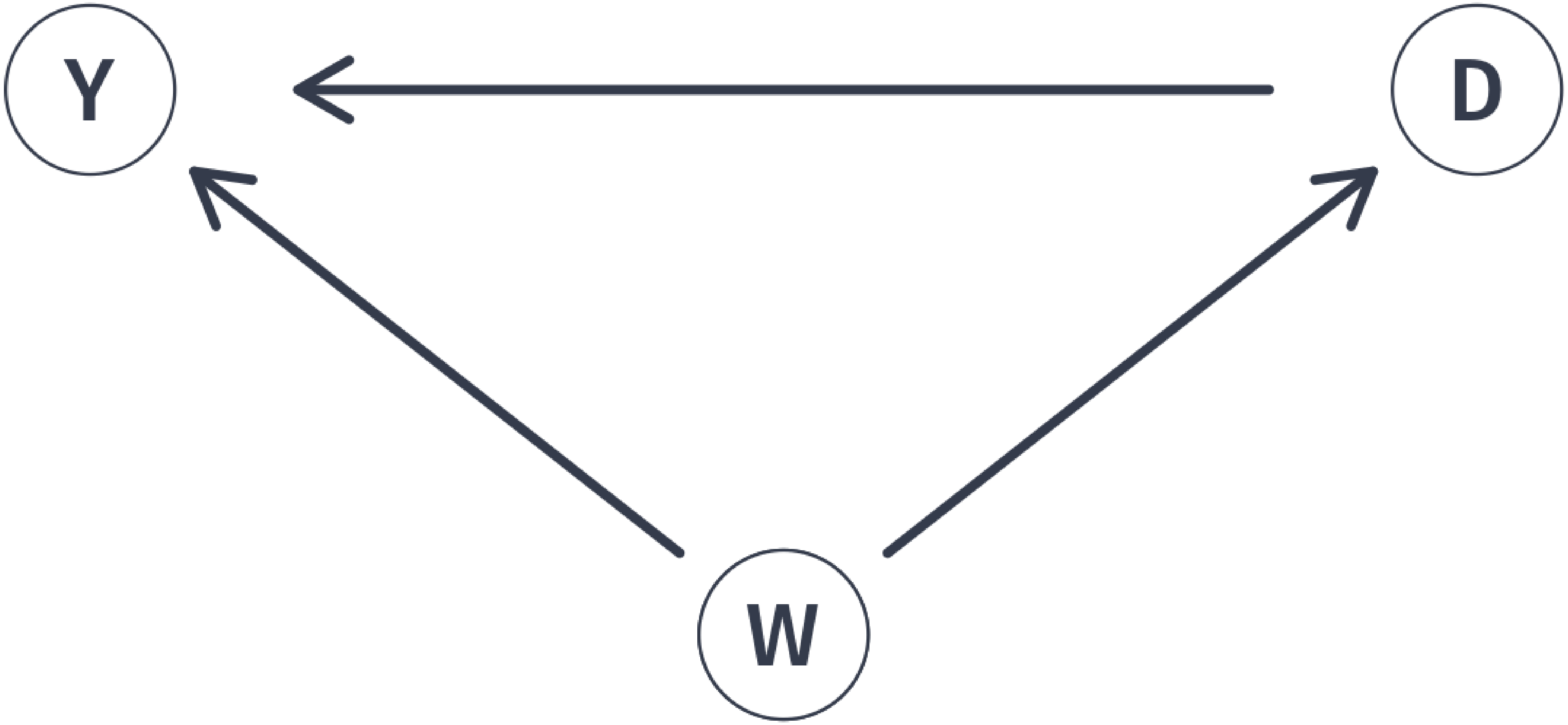

But parental income (**W**) impacts both student welfare (**Y**) and living on campus (**D**). Failing to account for parental income will bias comparisons

All else equal

> *Ceteris paribus, all else held constant, etc.*

A high bar

When *all else equal*, statistical comparisons detect causal relationships.

(Micro)economics has developed a comparative advantage in understanding where *all else 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.

Fundamental problem of causal inference

Causal identification

Goal:

> Identify the effect of a treatment, D_i, on individual i's outcome, $\text{Y}_{\text{D,i}}$.

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

$$\text{Y}_{1,\text{i}} - \text{Y}_{0,\text{i}}$$

- $\text{Y}_{1,\text{i}}$: the outcome for person i when she receives the treatment.
- $\text{Y}_{0,\text{i}}$: the outcome for person i when she doesn't receive the treatment.

Referred to as *potential outcomes*

Ideal data

The *ideal* data for 10 people

	i	trt	y1i	y0i
1	1	1	5.01	2.56
2	2	1	8.85	2.53
3	3	1	6.31	2.67
4	4	1	5.97	2.79
5	5	1	7.61	4.34
6	6	0	7.63	4.15
7	7	0	4.75	0.56
8	8	0	5.77	3.52
9	9	0	7.47	4.49
10	10	0	7.79	1.40

Causal effect of treatment.

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

for each individual i.

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
```

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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
```

Causal effect of treatment.

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

for each individual i.

Define the mean of τ_i as the *average treatment effect* (ATE)

$$\overline{\tau} = 3.82$$

Notice the assignment of treatment is irrelevant in this setting.

Ideal comparison

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

Highlights the fundamental problem of econometrics, much like when a traveler assesses options down two separate roads.

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 $y_{1,i}$.
- We do not observe the *counterfactual*.

Counterfactual

> Hypothetical scenario representing the unobserved outcome for an individual or unit if they had experienced the alternative treatment or condition

The traveler’s alternative outcome is forever unknown to them.

Fundamental problem of causal inference

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

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

Q: What is a good *counterfactual* for the missing data?

Estimating causal effects

Notation: $\mathrm{D_i}$ is a binary indicator variable such that

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

Then, rephrasing the previous slide,

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

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

Estimating causal effects

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

Idea: What if we compare the group of n peoples' means? *I.e.*,

$$
\begin{aligned}
&= \text{Avg}_n(y_i \mid D_i = 1) - \text{Avg}_n(y_i \mid D_i = 0) \\
&= \text{Avg}_n(y_{1i} \mid D_i = 1) - \text{Avg}_n(y_{0i} \mid D_i = 0)
\end{aligned}
$$

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

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

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

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

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

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

\quad \color{#ffffff}{\Bigg|}= \color{#434C5E}\text{Average causal effect} + \color{#B

Estimating causal effects

Our proposed difference-in-means estimator

\color{#434C5E}\text{Average causal effect} + \color{#B48EAD}{\text{Selection bias}}

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.

Mastering metrics video on selection bias

Randomized controlled trials

Selection Bias

Problem: Selection bias precludes *all else equal* comparisons.

- To make causal statements, 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).

Groups will need to be large enough

- Following the LLN, as we increase n of both groups, our randomly assigned treatment estimate is *more likely* to be representative of the population mean.

Randomized control trials

Ex. **Effect of de-worming on attendance**

Motivation: Intestinal worms are common among children in less-developed countries. The parasitic symptoms disrupt human capital acquisition by keeping children home.

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

Randomized control trials

Ex. **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.

Randomized control trials

Ex. **Effect of de-worming on attendance**

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

Q: Why not?

A: Selection bias – Families with access to de-worming medication probably have healthier children for other reasons (*e.g. wealth, access to clean drinking water*).[1]

1. *Can't make an all else equal comparison. Biased and/or spurious results*

Randomized Control Trials

Ex. **Effect of de-worming on attendance**

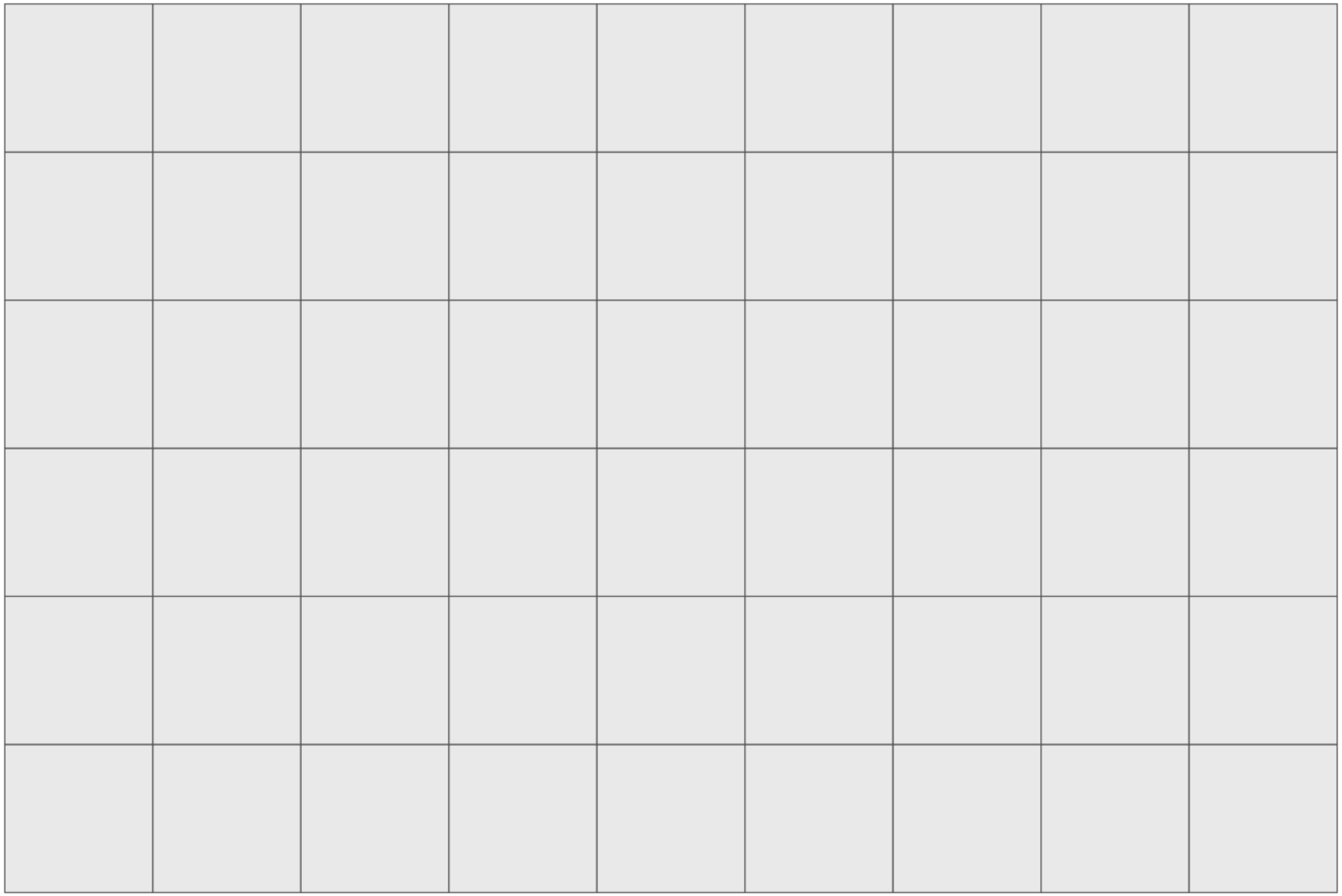
Imagine an *RCT* where we have **two** groups of villages:

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

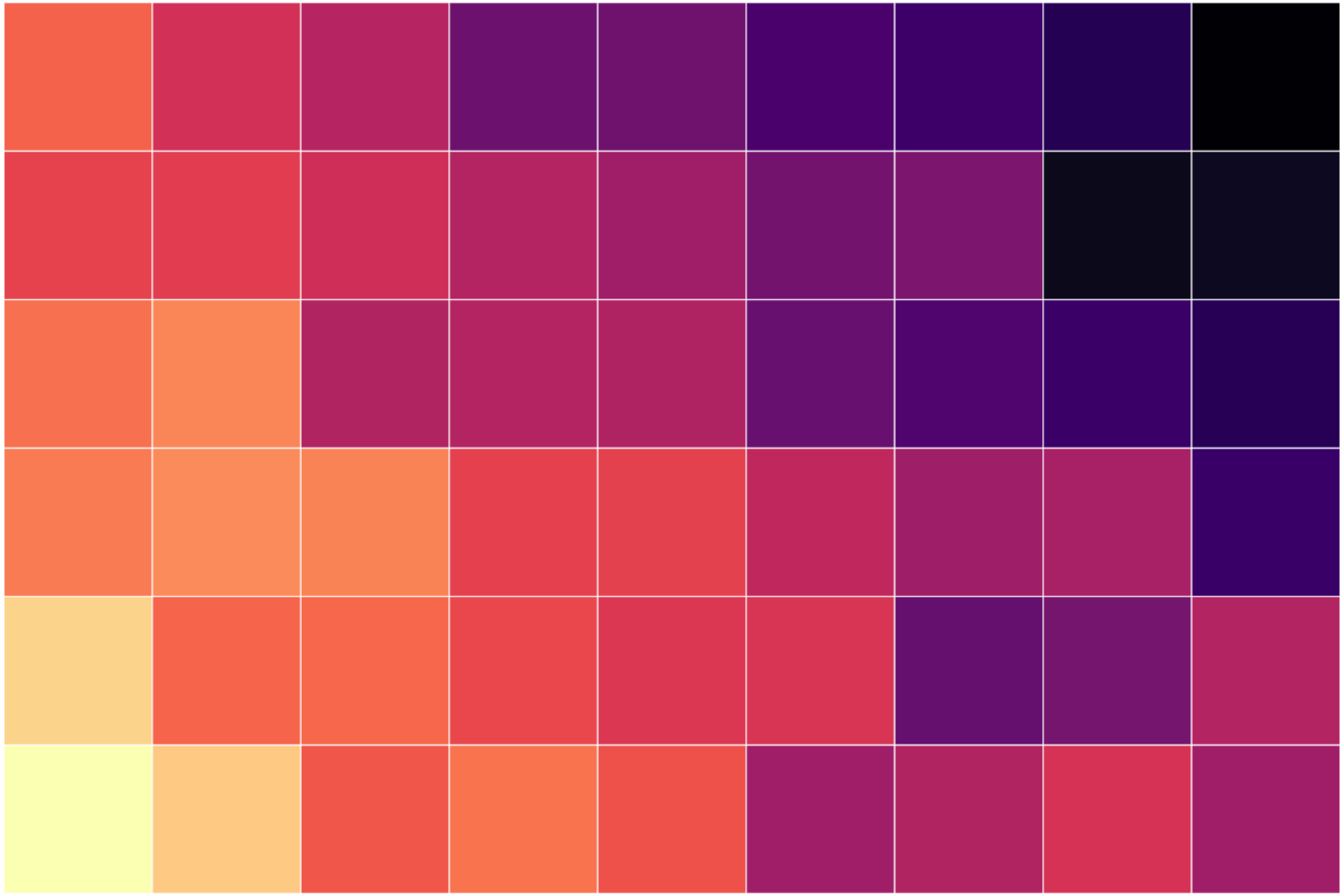
By randomizing, we will, on average, include all kinds of villages in both groups

- poor *vs.* less poor
- access to clean water *vs.* contaminated water
- hospital *vs.* no hospital

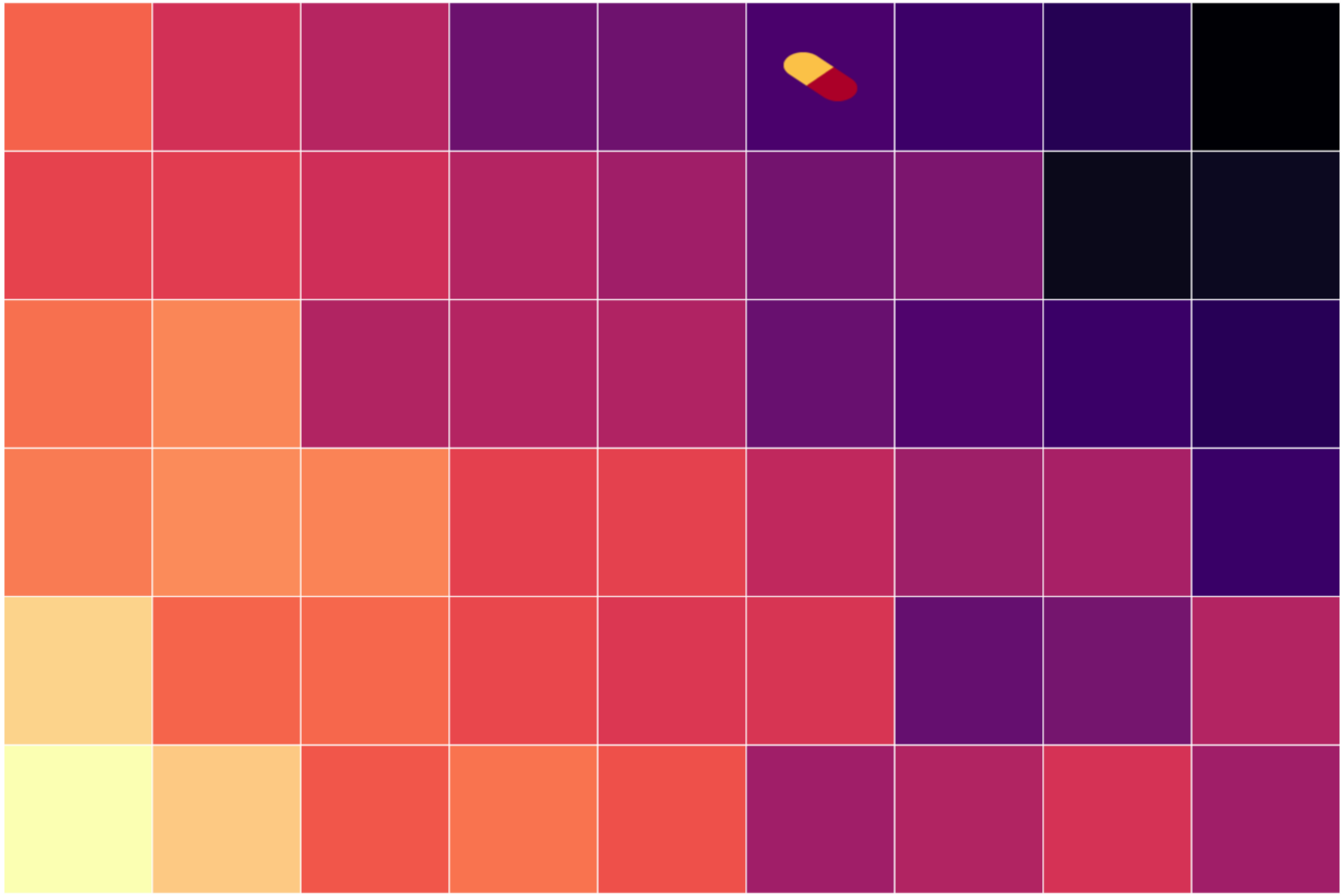
54 villages



54 villages *of varying levels of development*



54 villages *of varying levels of development* **plus randomly assigned treatment**



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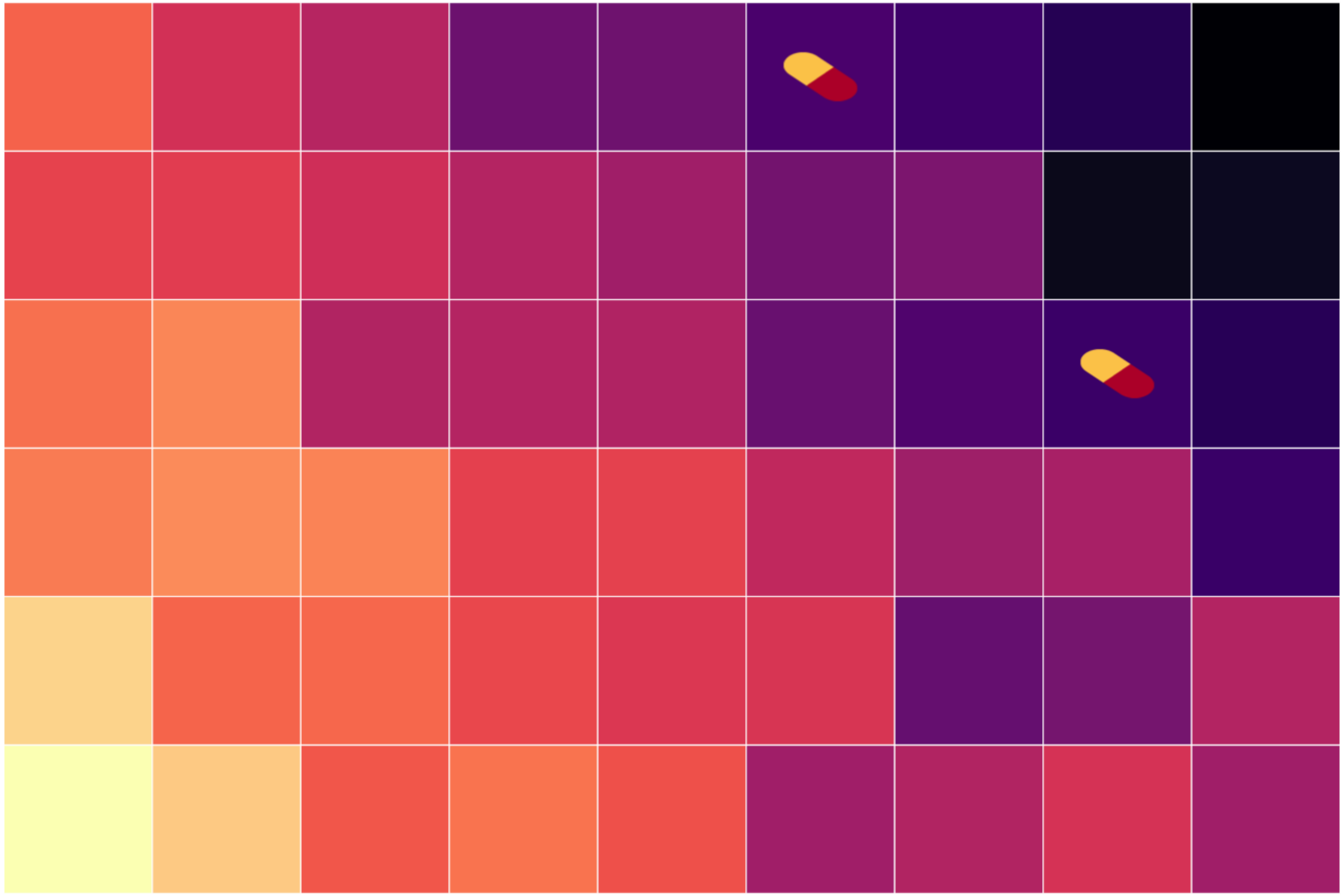

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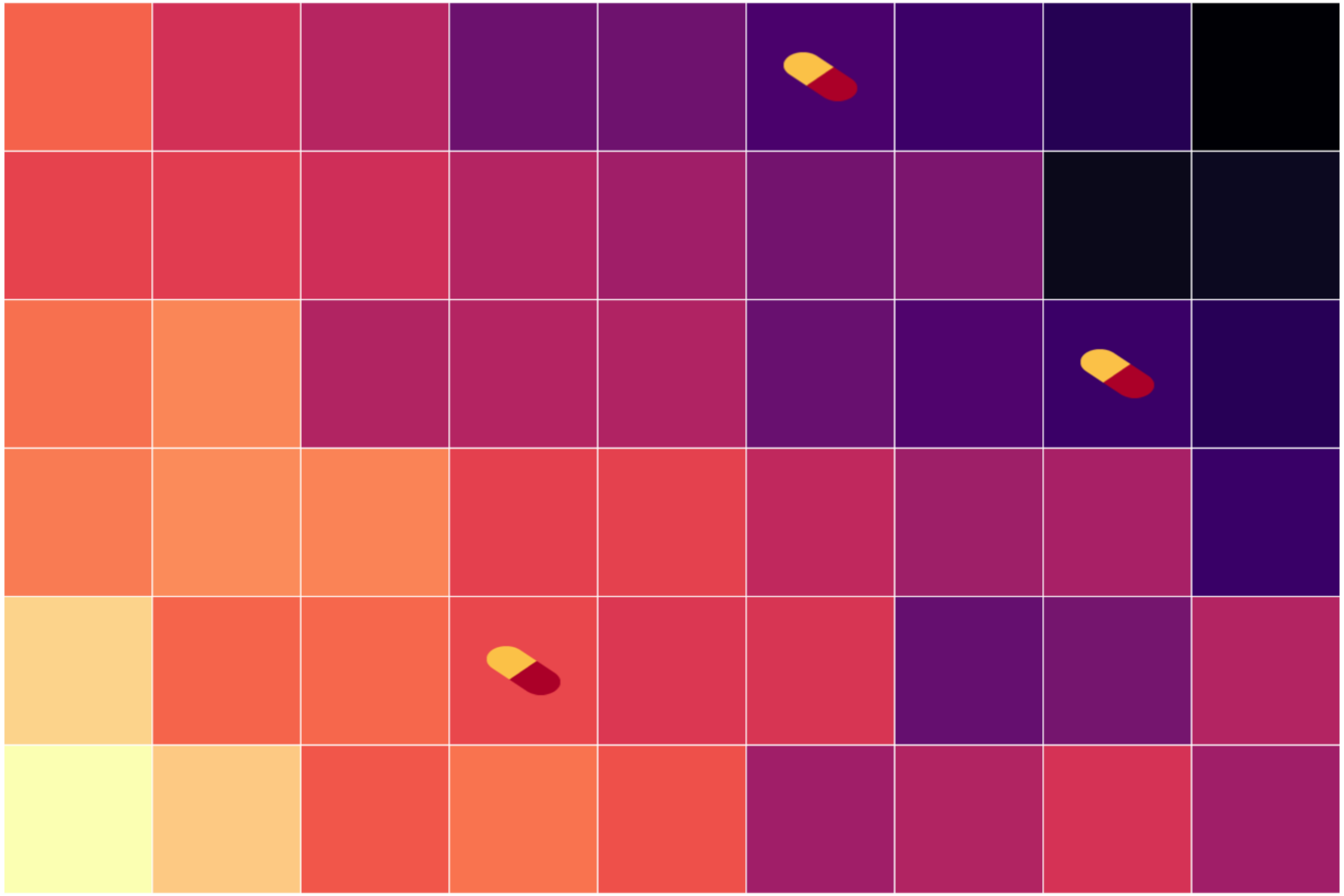

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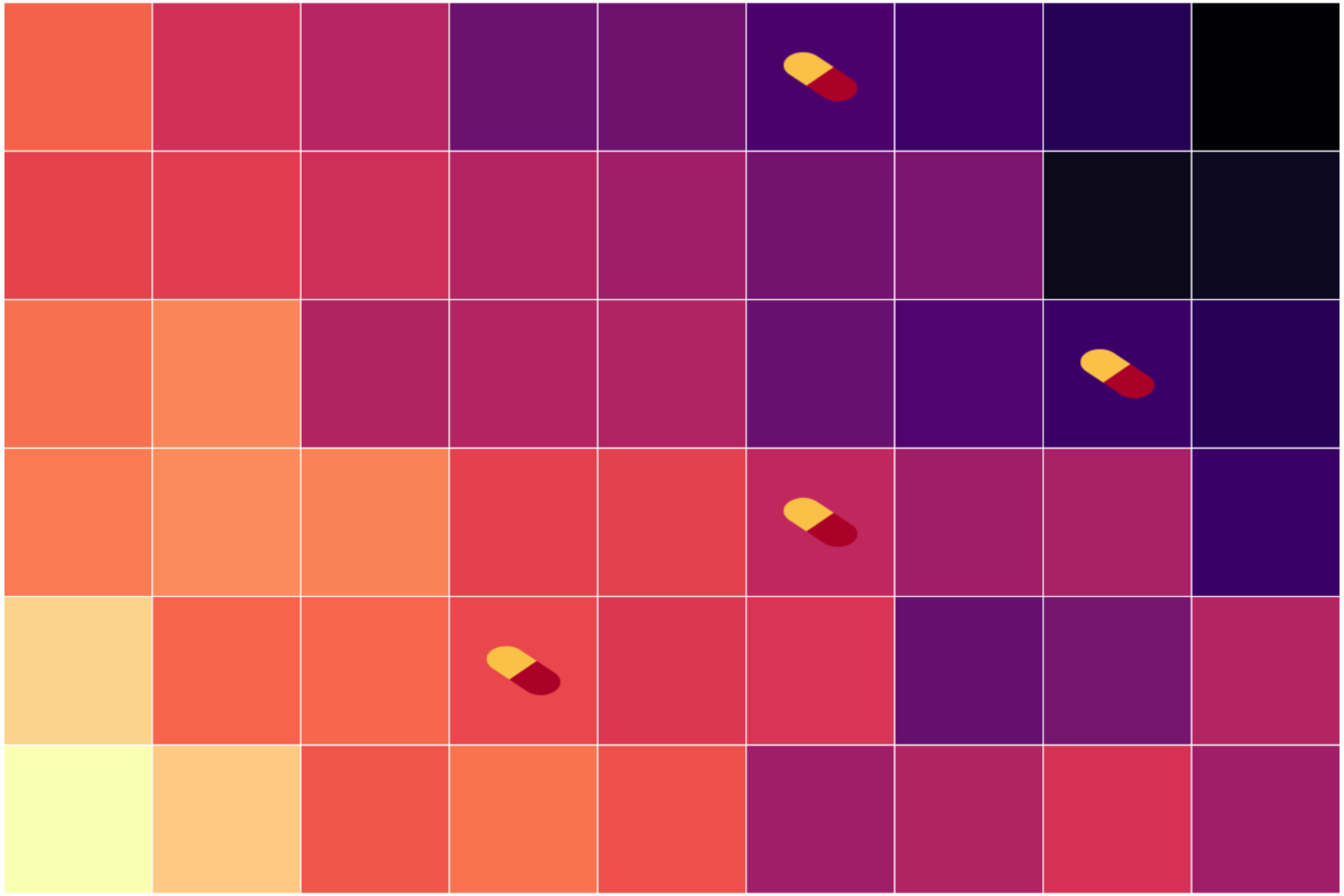

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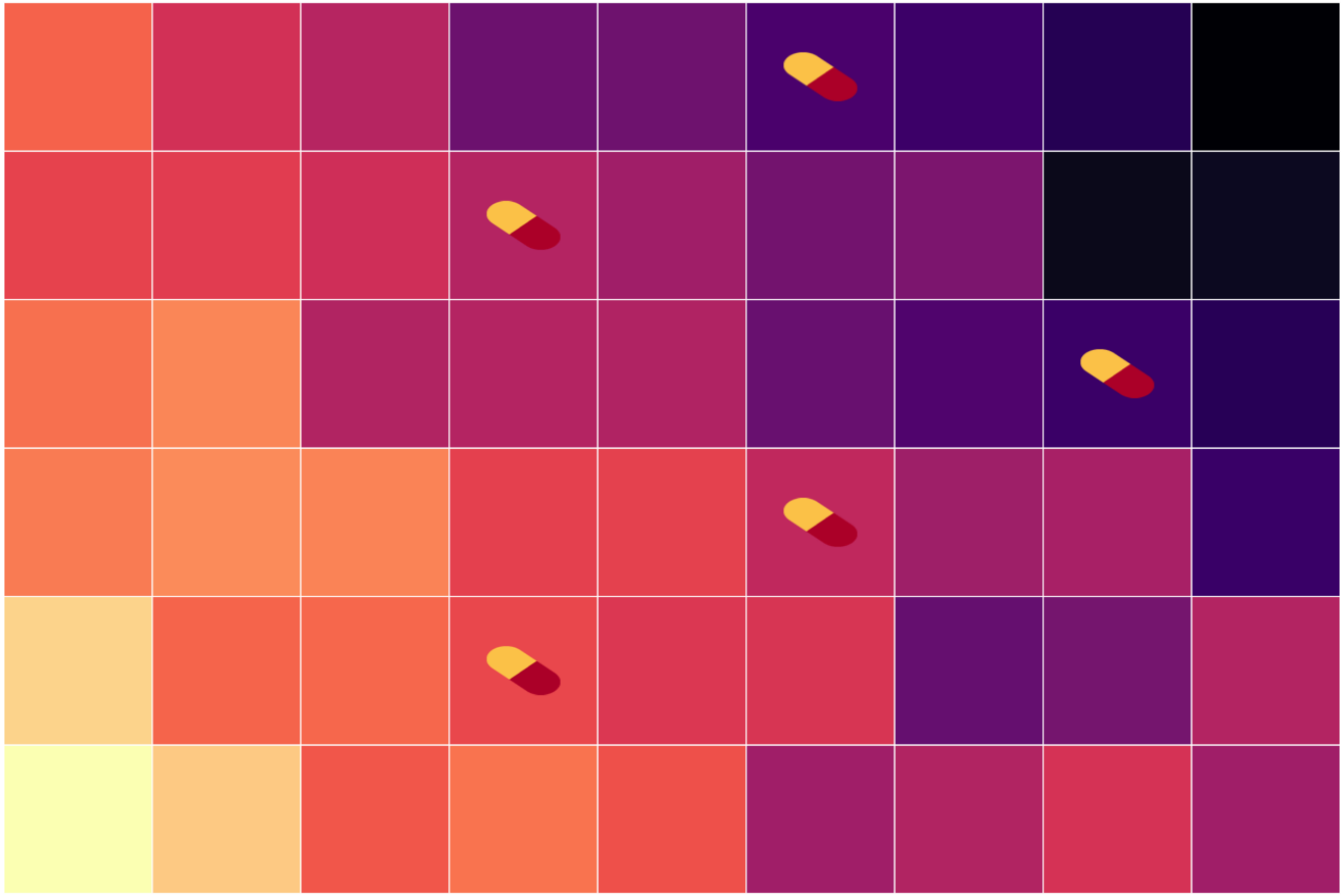

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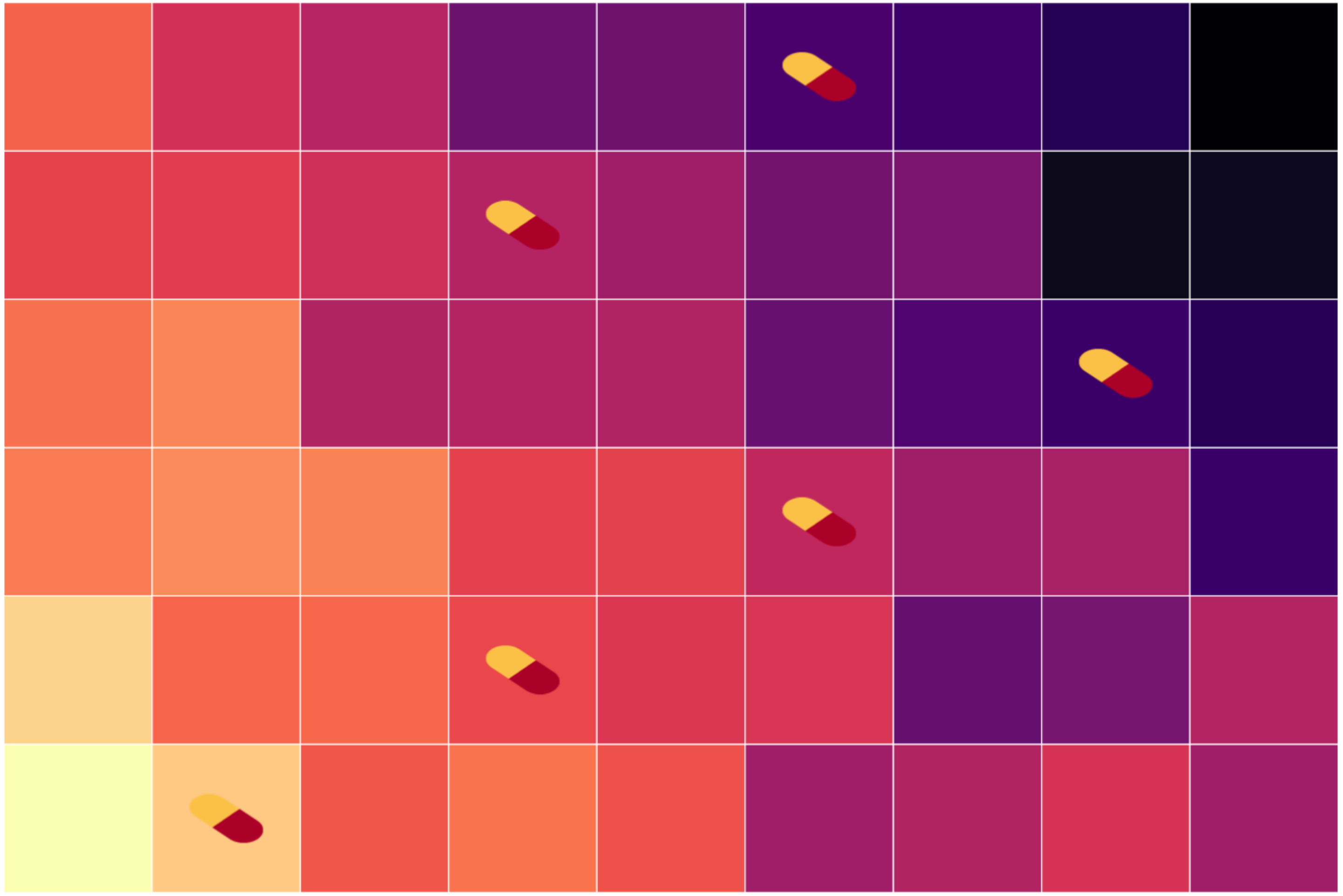

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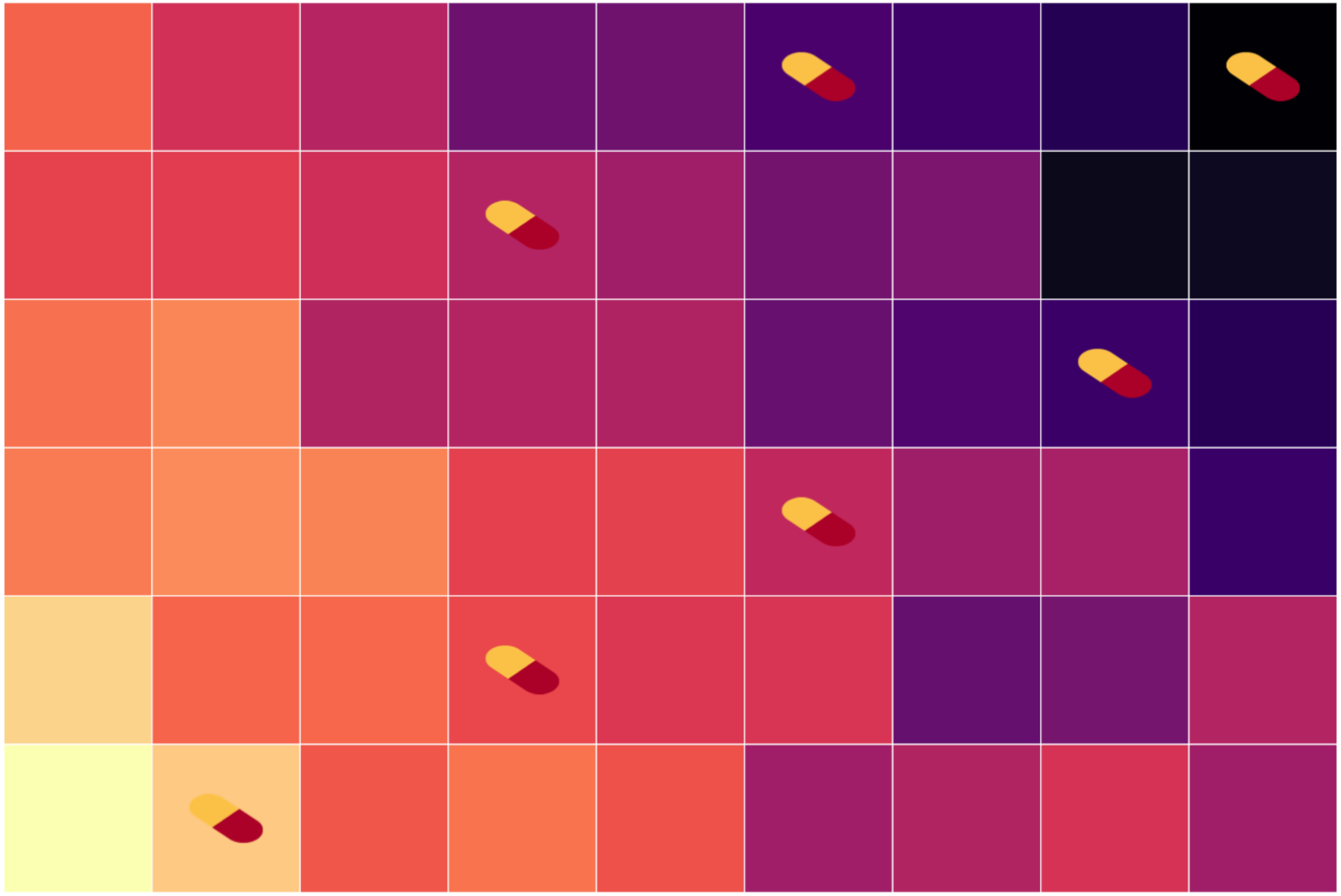

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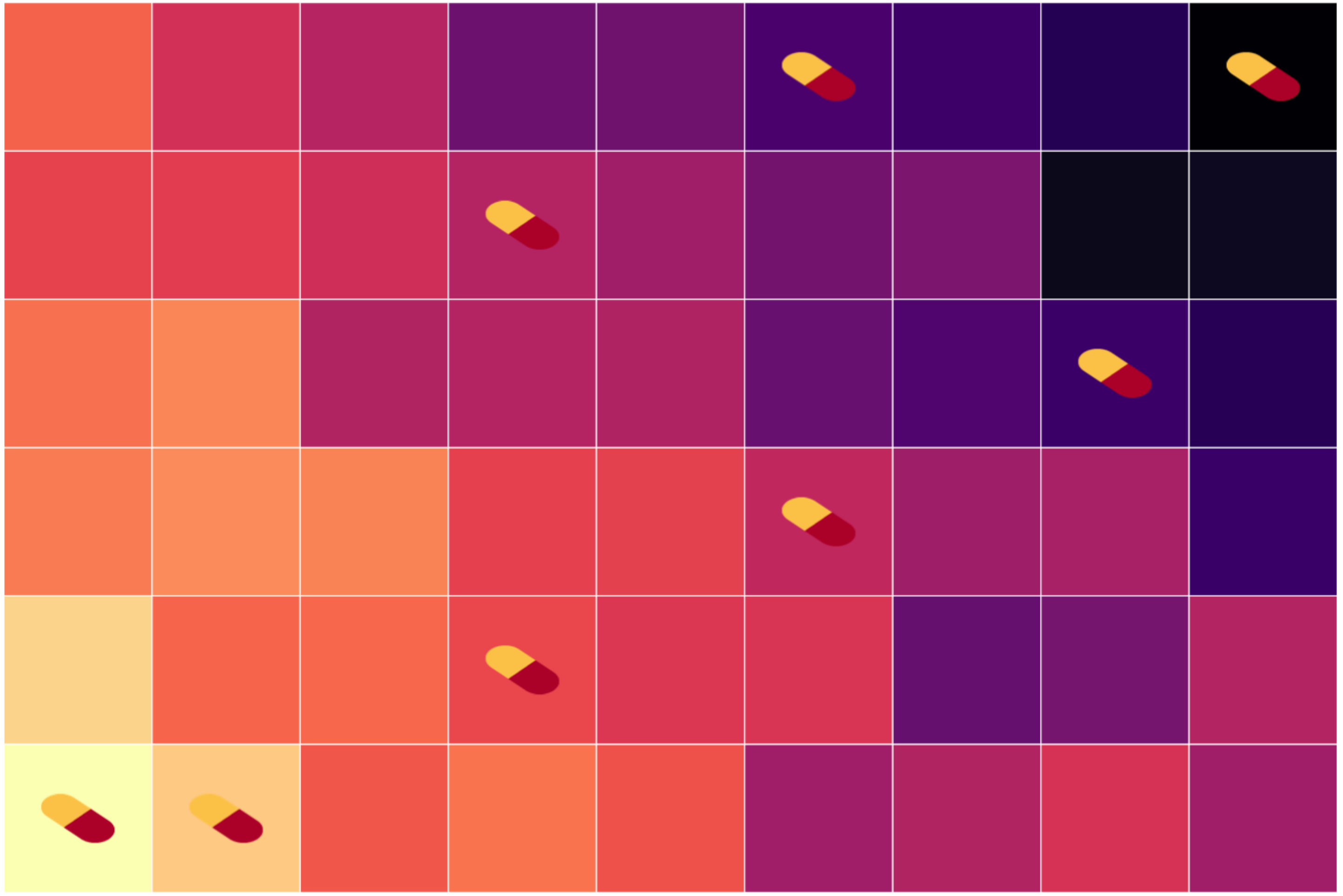

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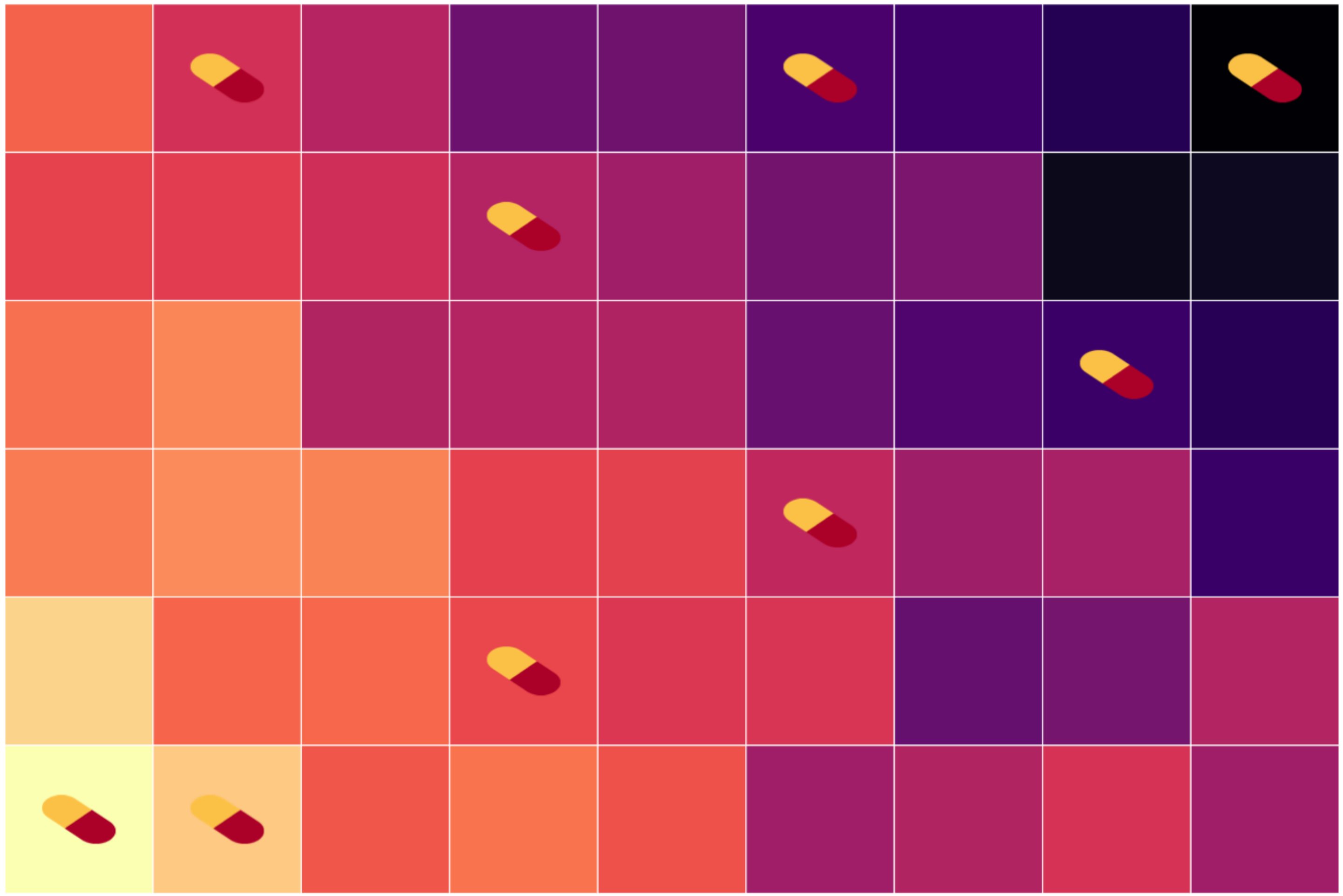

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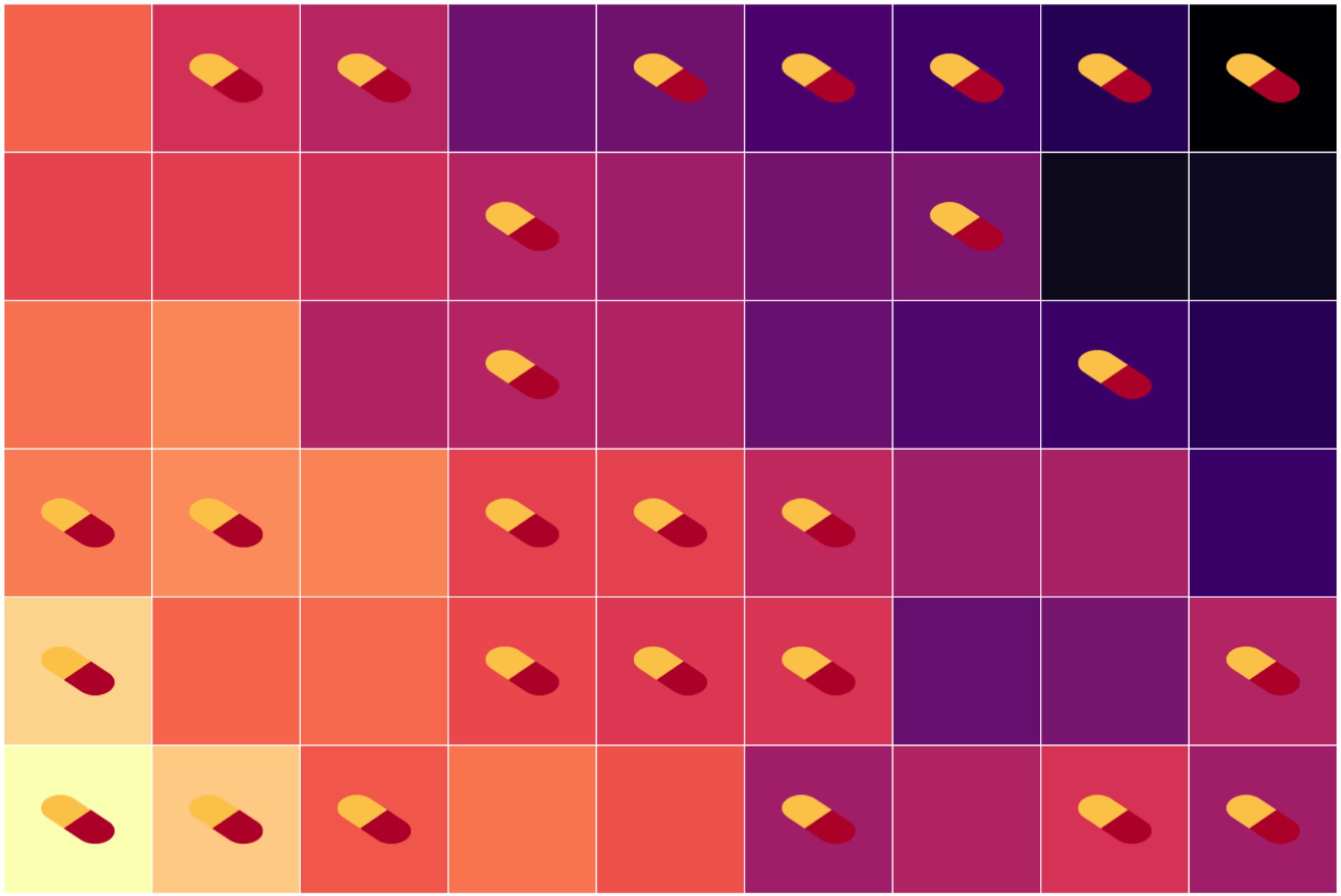

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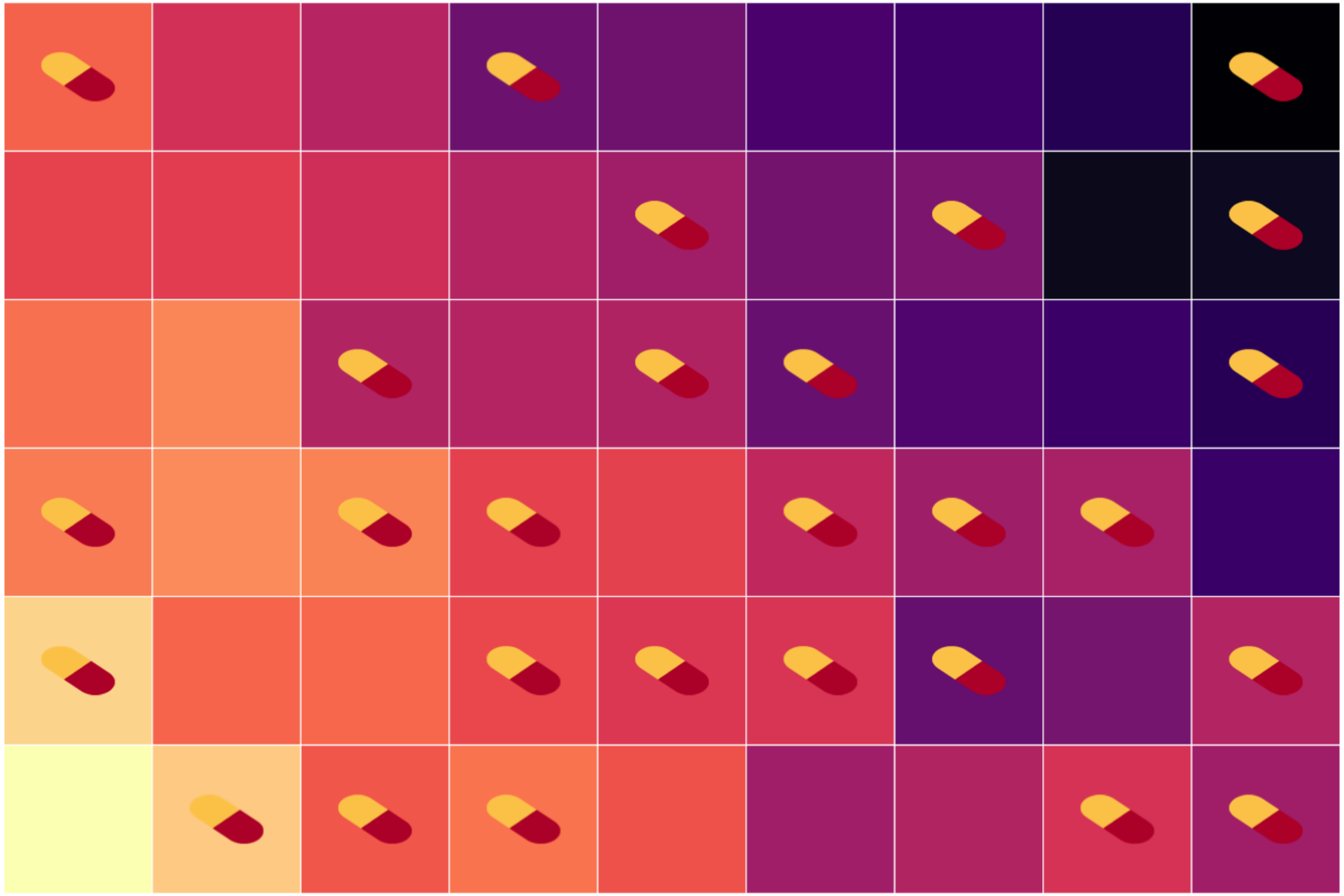

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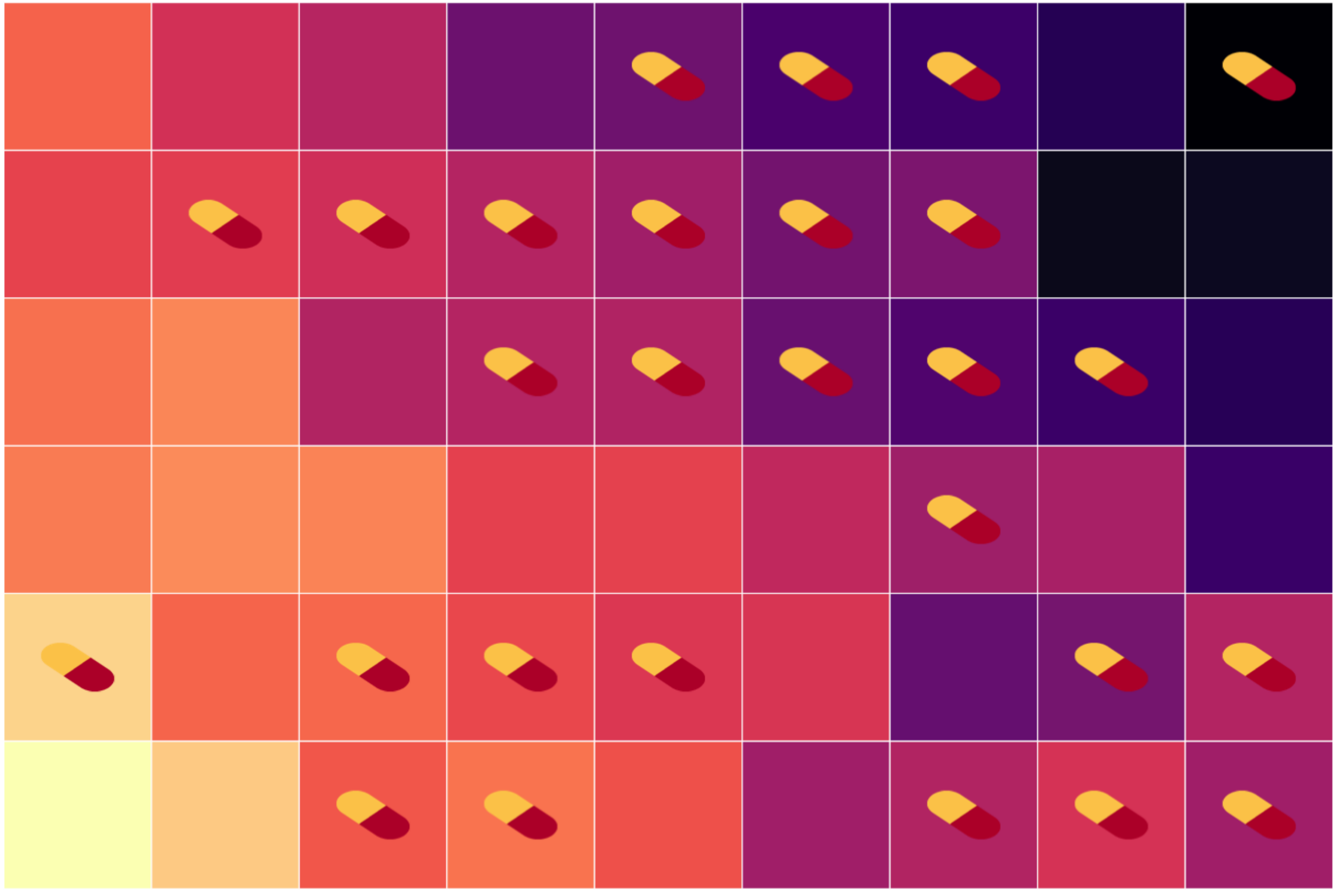


Randomized Control Trials

Ex. **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).

Randomized Control Trials

Ex. **Effect of de-worming on attendance**

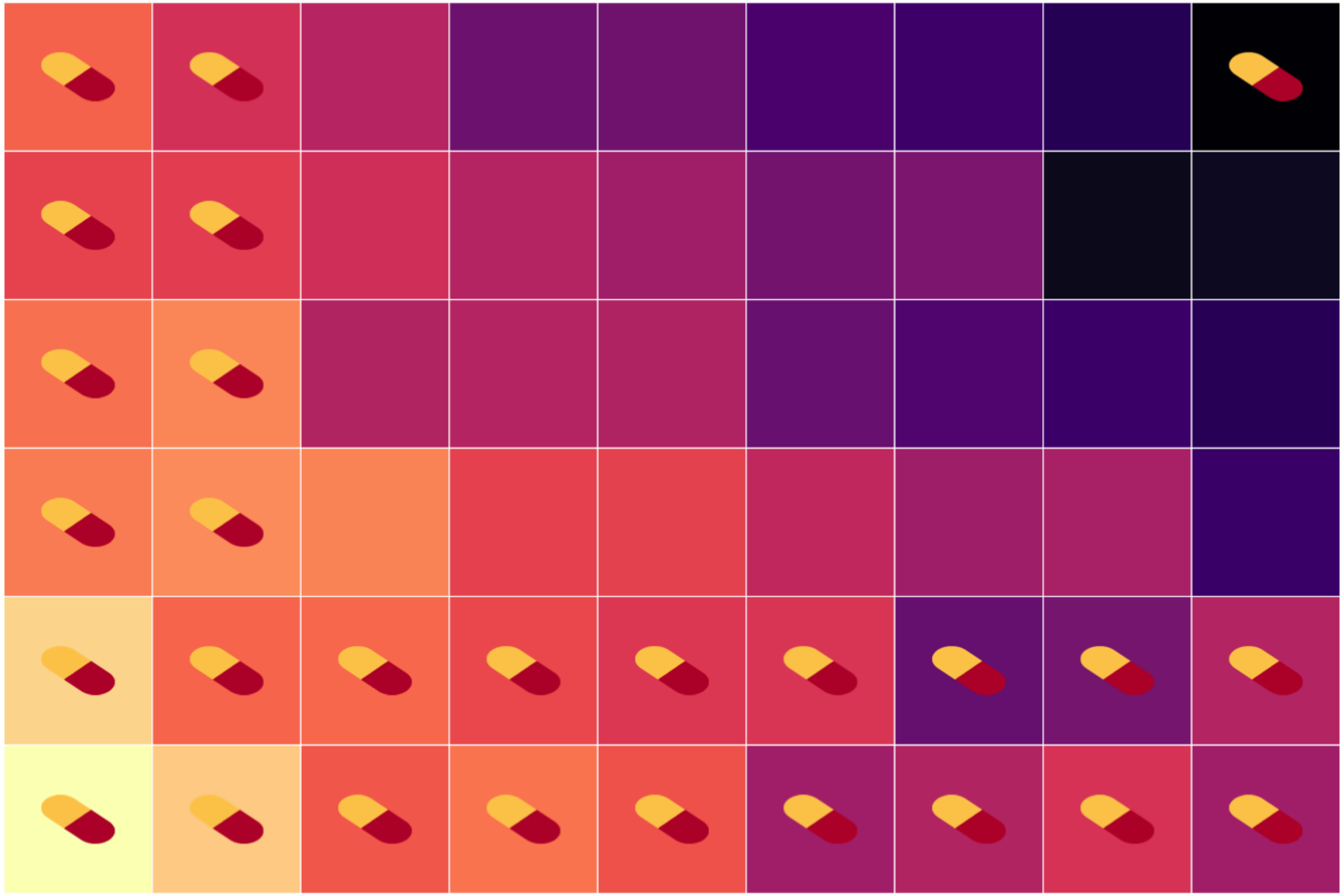
$$\text{Attendance}_i = \beta_0 + \beta_1 \text{Treatment}_i + u_i \tag{1}$$

where Treatment_i is a binary variable (=1 if village i received the de-worming treatment).

Q: Should trust the results of Eq. (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!



Causality Example.

Causality

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 of 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).

Causality

Example: **Returns to education**

Q: Could we simply compare the earnings of those with more education to those with less?

A: If we want to measure the causal effect, probably not.

Point 3. 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

Causality

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
- Lottery enrollment and/or capacity constraints

Summary

Takeaway

- The fundamental problem of causal inference is our inability to observe the both outcomes of a particular unit in parallel
- This introduces challenges in finding a way to construct a valid counterfactual using group means
- This quasi-experimental approach is valid if there is no selection bias or if we control for selection bias such that it is eliminated
- RCTs and natural experiments are one way to avoid selection bias.
- Since these events are often rare or infeasible, we will explore many other ways to approach this challenge