Causality

EC 421, Set 10

Luciana Etcheverry
November 14, 2019

Prologue

Schedule

Last Time

- Nonstationarity, 'in-class' analysis
- **Follow up:** `EC422` (time series) is offered in the winter! 😃
- **Follow up:** `EC410` (computational economics) in the spring! 😃

Today

- Return to our in-class examples
- Causality

Upcoming

Assignment due Sunday. Another one coming soon, due before Thanksgiving.

Problems and strategies

Step 1: Define the problem.

Q: What was the problem/goal/objective for the analysis?

A: For y_1 and y_2 each, find the **true** model.

Clarification:

Q: What does the *true model* for y_1 mean?

- (**A**) The variables that best explain/predict y_1.
- (**B**) The variables that are statistically significant.
- (**C**) The variables that actually generated y_1.
- (**D**) Something else?

A: (C) We want to know variables and coefficients generated y_1.

The **true data-generating process** (DGP).

Problems and strategies

Step 2: Define your strategy

How did you approach this problem?

A few options:

1. Find the combination of variables that **maximize R^2** or **adjusted R^2**.
2. First **include all** variables. Keep statistically **significant variables**.
3. Iterate with (2.): **Drop non-significant variables** until nothing changes.
4. **Add variables one by one**. Keep statistically **significant variables**.
5. **Plot** variables' (or residuals') relationships with y.

```r
# Load the data
fun_df ← read_csv("fun_data.csv")
# Separate into two datasets
y1_df ← fun_df %>% select(-y2)
y2_df ← fun_df %>% select(-y1)
# Peak at the data
y1_df
```

```
#> # A tibble: 100 x 10
#>          y1     x1    x2     x3    x4     x5    x6        x7    x8    x9
#>       <dbl>  <dbl> <dbl>  <dbl> <dbl>  <dbl> <dbl>     <dbl> <dbl> <dbl>
#>  1    1.09  -0.777 0.405  1.23  0.762 -0.232  1.17  0.111        1 1.98
#>  2    3.69   0.473 1.59   0.584 1.53   0.349  1.52 -0.00994      2 0.511
#>  3    4.08   2.30  3.52  -0.976 3.32   0.581  1.50  0.974        3 0.936
#>  4    0.926  2.46  5.33  -1.77  4.64  -0.576  1.92  2.53         4 2.88
#>  5   -4.22   0.313 2.09  -2.59  1.37  -0.717  3.76  2.14         5 2.20
#>  6   -2.01   1.37  1.23   2.34  2.21  -1.40   3.55  1.17         6 1.83
#>  7    0.875  1.73  3.46   0.584 2.24  -1.31   3.77  1.92         7 1.75
#>  8  -18.6    2.60  4.09  -4.15  4.13  -2.57   4.60  0.886        8 1.14
#>  9   -7.06   0.877 3.96   2.08  1.42  -2.89   3.68  1.32         9 2.23
#> 10   -1.27  -0.197 0.875 -0.760 0.697 -1.92   1.90  1.85        10 1.90
#> # … with 90 more rows
```

`gather`ing data

Let's plot y_1 against the nine potential explanatory variables, x_1 to x_9.

We'll use two new functions to streamline this process.

- `gather()` (from `dplyr`): *Stacks* variables (names and values).
- `facet_wrap()`: Creates multiple plots grouped by a variable.

R showcase

`gather`ing data

Example: `gather` all variables in our dataset.

```
data.frame(w = 0:1, x = 2:3, y = 4:5, z = 6:7) %>%
  gather(key = "var", value = "value")
```

```
#>   var value
#> 1   w     0
#> 2   w     1
#> 3   x     2
#> 4   x     3
#> 5   y     4
#> 6   y     5
#> 7   z     6
#> 8   z     7
```

`gather`ing data

Example: `gather` all variables in our dataset except **`w`**.

```
data.frame(w = 0:1, x = 2:3, y = 4:5, z = 6:7) %>%
  gather(-w, key = "var", value = "value")
```

```
#>   w var value
#> 1 0   x     2
#> 2 1   x     3
#> 3 0   y     4
#> 4 1   y     5
#> 5 0   z     6
#> 6 1   z     7
```

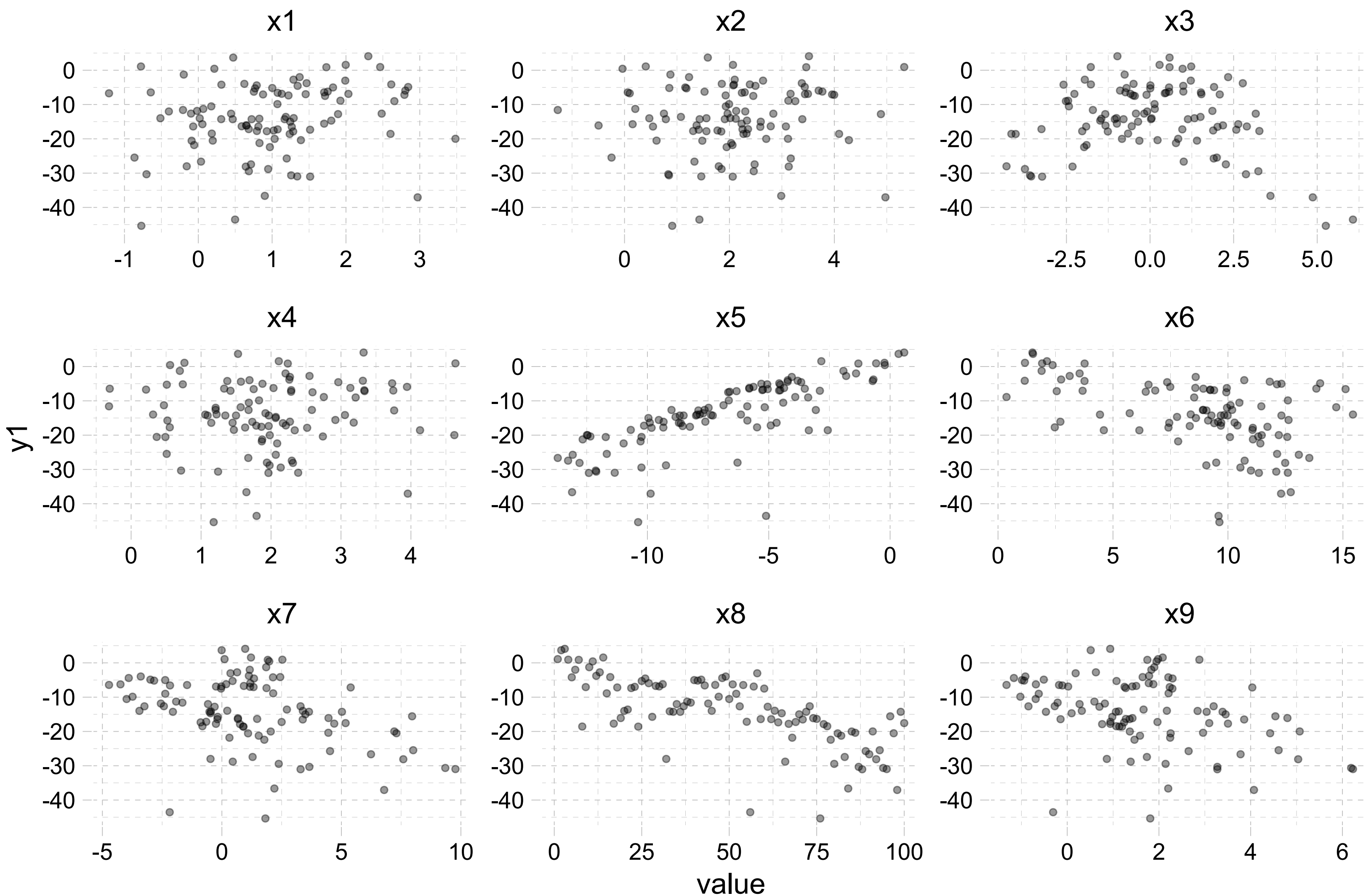
R showcase

`gather`ing data

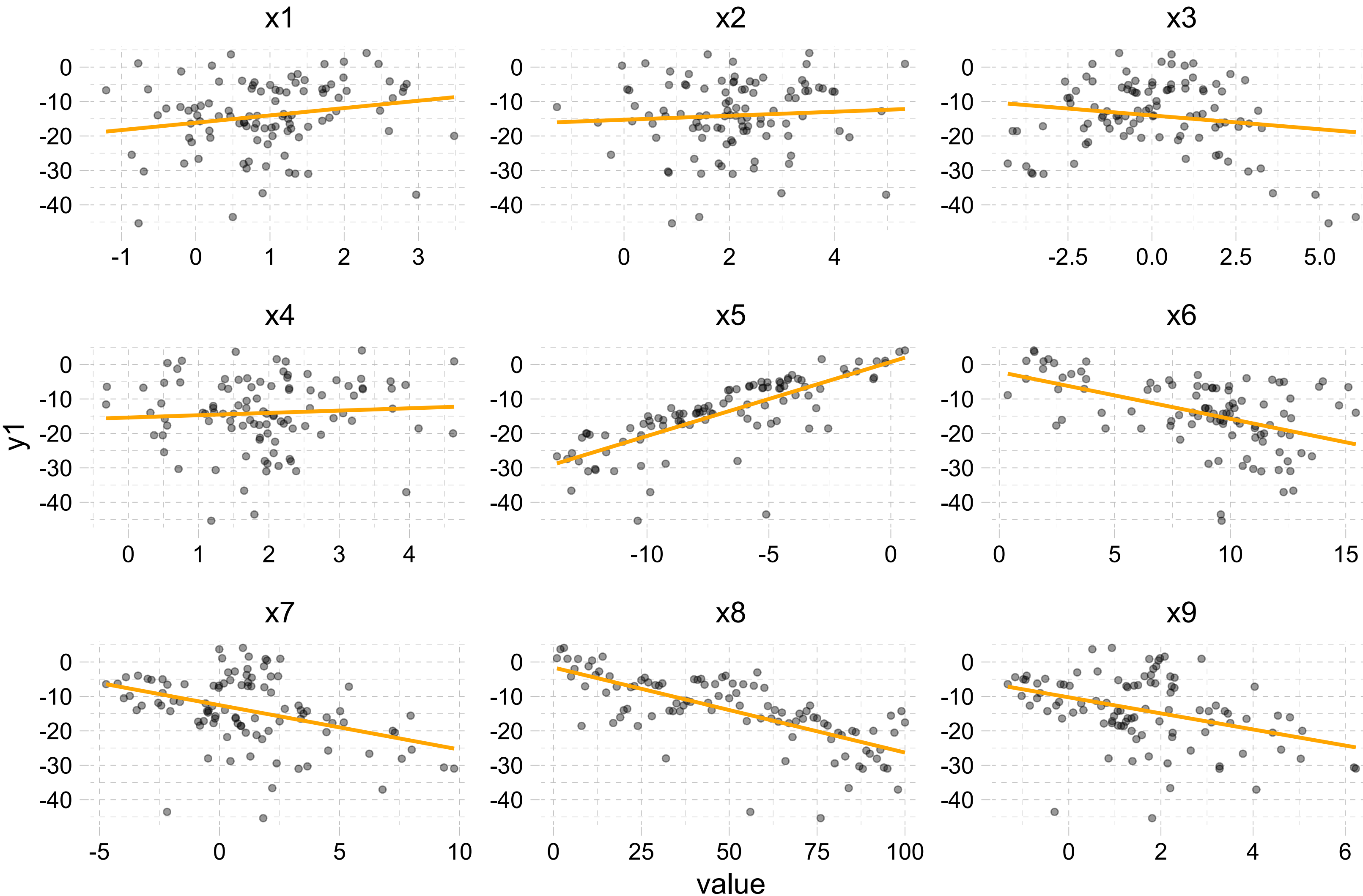
Adding these new functions to our previous `ggplot2` work...

```
y1_df %>% gather(-y1, key = "var", value = "value") %>%
  ggplot(aes(x = value, y = y1)) +
  geom_point(alpha = 0.4, size = 1.5) +
  facet_wrap(~ var, scales = "free") +
  theme_pander(base_size = 16)
```

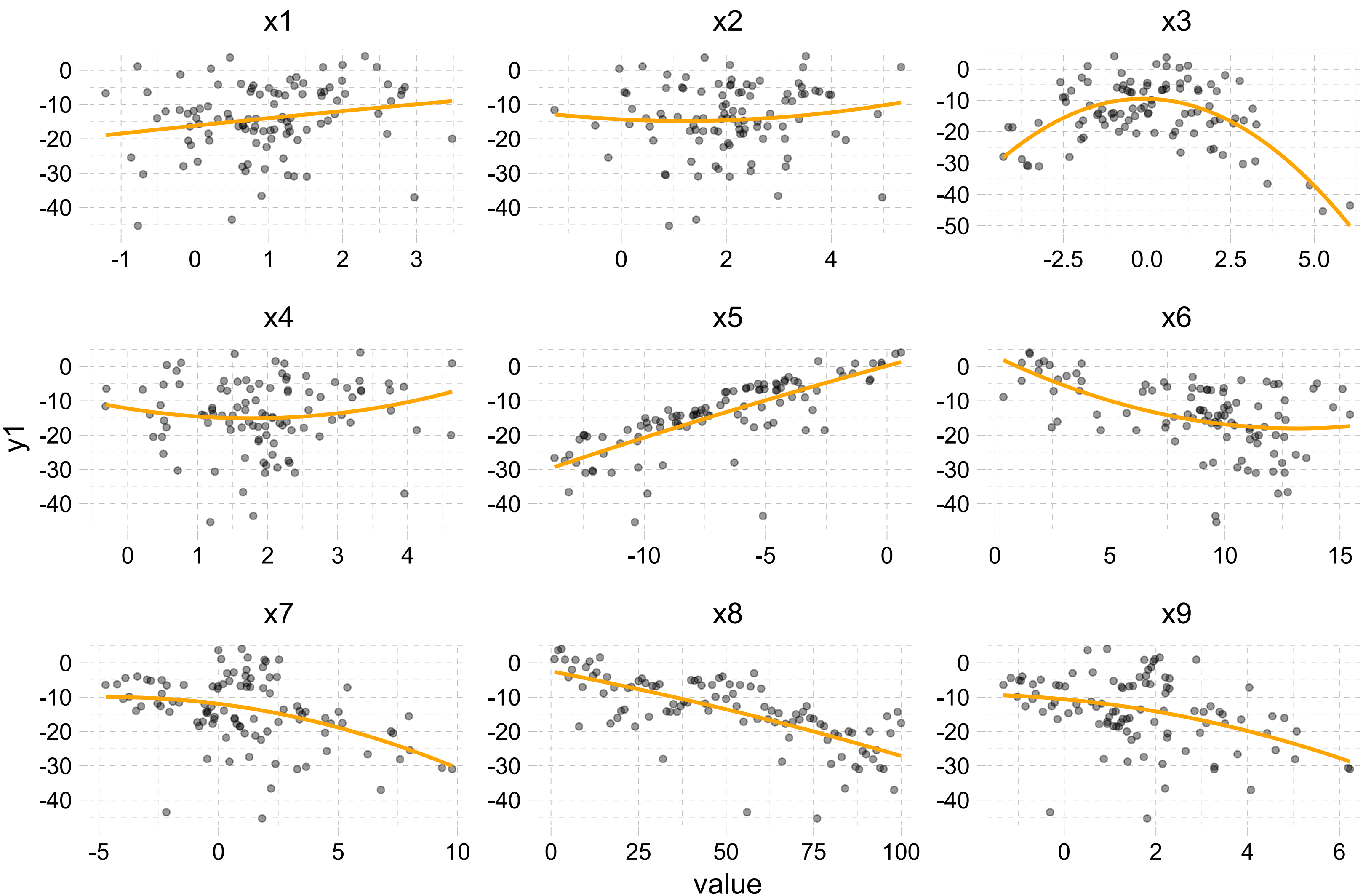
Plot: y_1 against x_1 through x_9



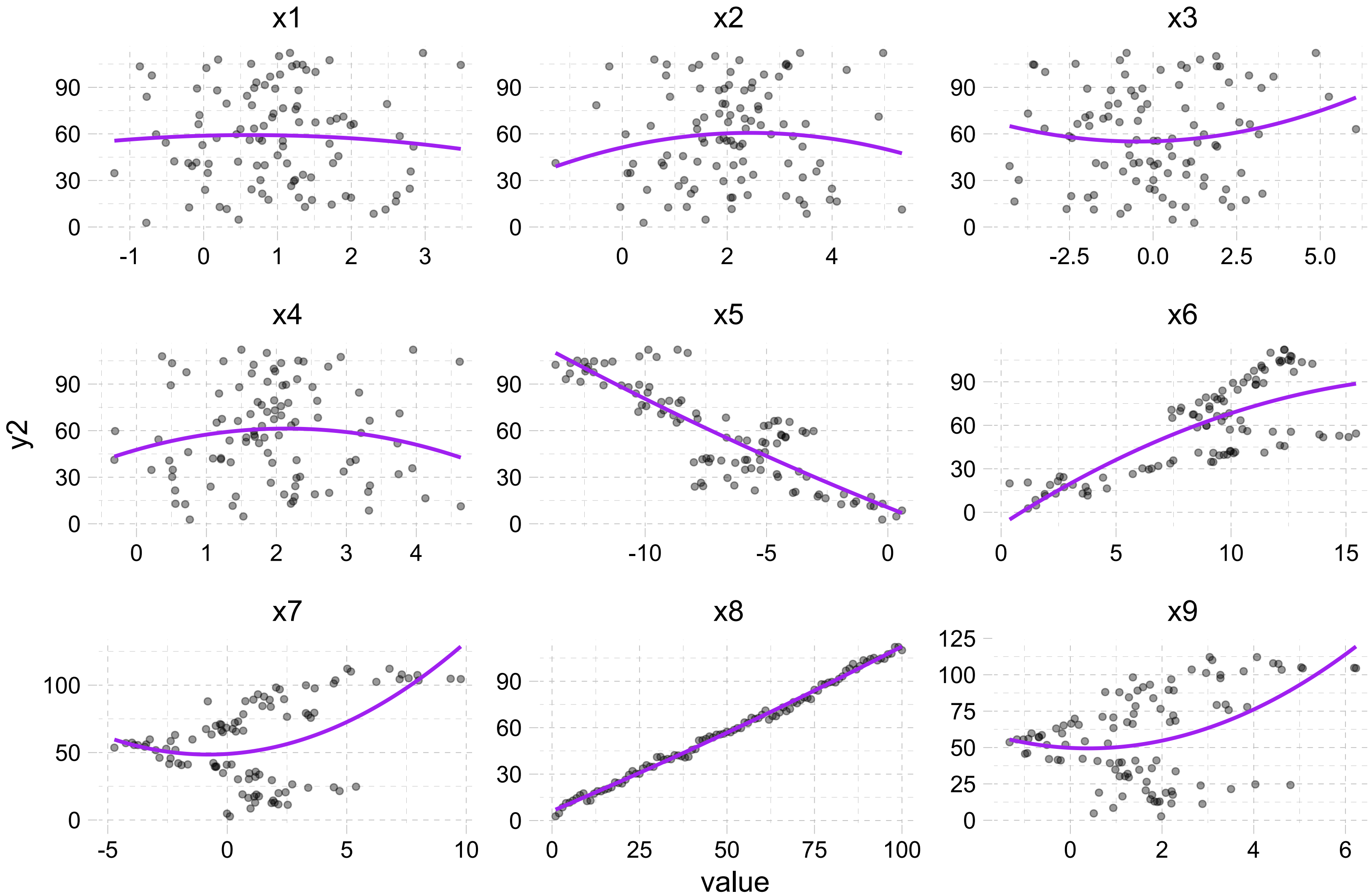
Simple linear regressions: y_1 against x_1 through x_9



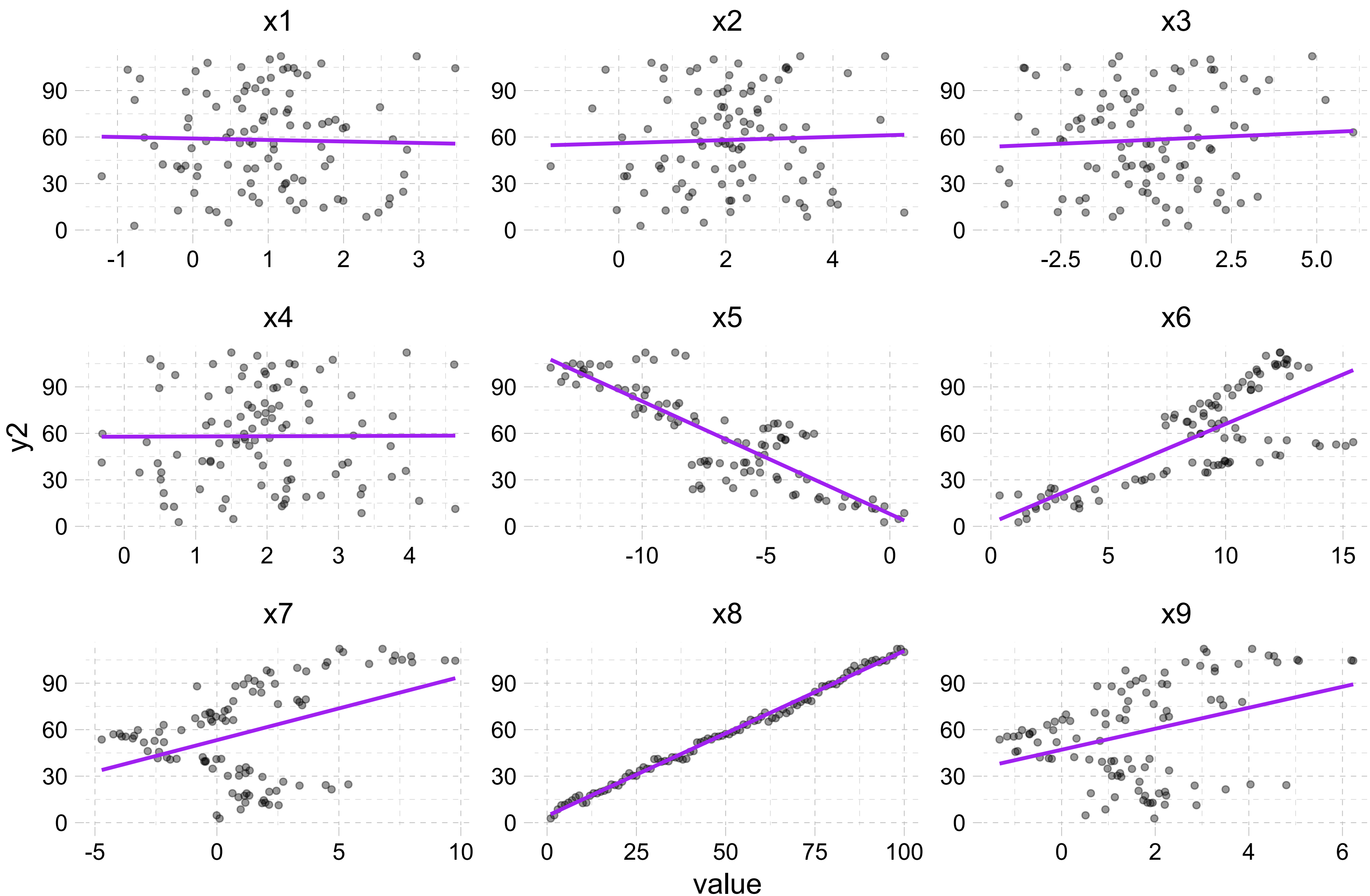
Linear regressions with quadratic RHS: y_1 against x_1 through x_9



Linear regressions with quadratic RHS: y_2 against x_1 through x_9



Simple linear regressions: y_2 against x_1 through x_9



Searching for the unknown model

Results

Your responses: Percentage who said TRUE (74 responses)

	X1	X2	X3	X4	X5	X6	X7	X8	X9
y1	85	8	73	12	91	24	22	23	16
y2	16	72	8	7	28	86	34	96	32

Truth: The true data-generating processes

$$y_1 = 3 + x_1 - x_3^2 + 2x_5 + u$$
$$y_2 = 1 + x_2 + x_6 + x_8 + v$$

Q: Is it worse include an incorrect variable or exlcude a correct variable?

Causality

Causality

Intro

Most tasks in econometrics boil down to one of two goals:

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \cdots + \beta_k x_k + u$$

1. **Prediction:** Accurately and dependably predict/forecast y using on some set of explanatory variables—doesn't need to be x_1 through x_k. Focuses on $\hat{y}$. β_j doesn't really matter.

2. **Causal estimation:**† Estimate the actual data-generating process—learning about the true, population model that explains how y changes when we change x_j—focuses on β_j. Accuracy of $\hat{y}$ is not important.

For the rest of the term, we will focus on **causally estimating** β_j.

† Often called *causal identification*.

Causality

The challenges

As you saw in the data-analysis exercise, determining and estimating the true model can be pretty difficult—both practically and econometrically.

Practical challenges

- Which variables?
- Which functional form(s)?
- Do data exist? How much?
- Is the sample representative?

Econometric challenges

- Omitted-variable bias
- Reverse causality
- Measurement error
- How precise can/must we be?

Many of these challenges relate to **exogeneity**, *i.e.*, $\boldsymbol{E}[u_i|X] = 0$.
Causality requires us to **hold all else constant** (*ceterus paribus*).

Causality

It's complicated

Occasionally, ***causal*** relationships are simply/easily understood, *e.g.*,

- What caused the forest fire?
- How did this baby get here?

Generally, ***causal*** relationships are complex and challenging to answer, *e.g.*,

- What causes some countries to grow and others to decline?
- What caused President Trump's 2016 election?
- How does the number of police officers affect crime?
- What is the effect of better air quality on test scores?
- Do longer prison sentences decrease crime?
- How did cannabis legalization affect mental health/opioid addiction?

Causality

Correlation ≠ Causation

You've likely heard the saying

> Correlation is not causation.

The saying is just pointing out that there are violations of exogeneity.

Although correlation is not causation, **causation *requires* correlation**.

New saying:

> Correlation plus exogeneity is causation.

Let's work through a few examples.

Causation

Example: The causal effect of fertilizer[†]

Suppose we want to know the causal effect of fertilizer on corn yield.

Q: Could we simply regress yield on fertilizer?
A: Probably not (if we want the causal effect).

Q: Why not?
A: Omitted-variable bias: Farmers may apply less fertilizer in areas that are already worse on other dimensions that affect yield (soil, slope, water).
Violates *all else equal* (exogeneity). Biased and/or spurious results.

Q: So what *should* we do?
A: **Run an experiment!** 💩

† Many of the early statistical and econometric studies involved agricultural field trials.

Example: The causal effect of fertilizer

Randomized experiments help us maintain *all else equal* (exogeneity).

We often call these experiments ***randomized control trials*** (RCTs).†

Imagine an RCT where we have two groups:

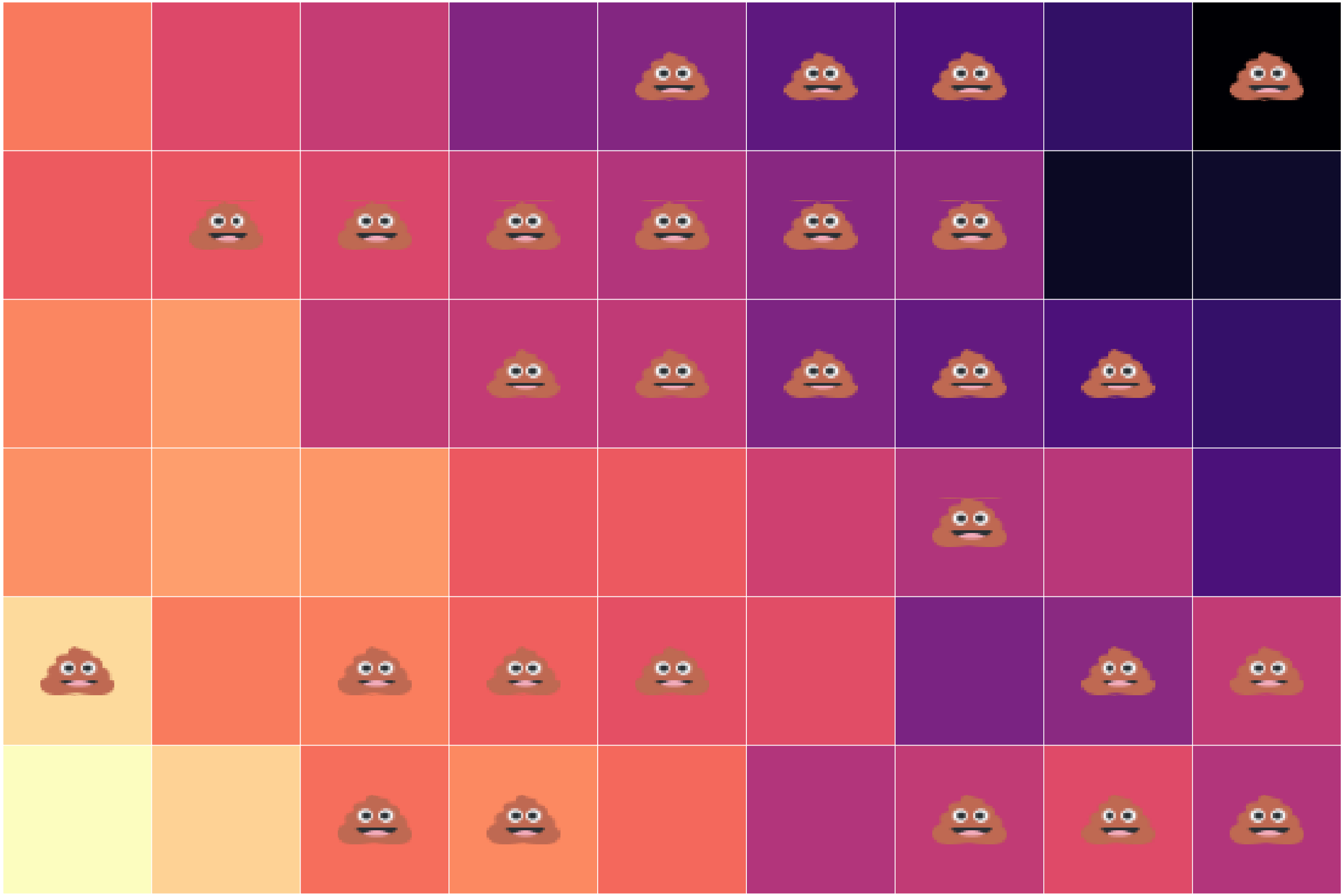
- **Treatment:** We apply fertilizer.
- **Control:** We do not apply fertilizer.

By randomizing plots of land into **treatment** or **control**, we will, on average, include all kinds of land (soild, slope, water, *etc.*) in both groups.

All else equal!

† Econometrics (and statistics) borrows this language from biostatistics and pharmaceutical trials.

54 equal-sized plots of varying quality plus randomly assigned treatment



Causation

Example: The causal effect of fertilizer

We can estimate the **causal effect** of fertilizer on crop yield by comparing the average yield in the treatment group (💩) with the control group (no 💩).

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

Alternatively, we can use the regression

$$\text{Yield}_i = \beta_0 + \beta_1 \text{Trt}_i + u_i \tag{1}$$

where Trt_i is a binary variable (=1 if plot i received the fertilizer treatment).

Q: Should we expect (1) to satisfy exogeneity? Why?
A: On average, **randomly assigning treatment should balance** trt. and control across the other dimensions that affect yield (soil, slope, water).

Example: Returns to education

Labor economists, policy makers, parents, and students are all interested in the (monetary) *return to education*.

Thought experiment:

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

This change in earnings gives the **causal effect** of education on earnings.

Causality

Example: Returns to education

Q: Could we simply regress earnings on education?

A: Again, probably not if we want the true, causal effect.

1. People *choose* education based upon many factors, *e.g.*, ability.
2. Education likely reduces experience (time out of the workforce).
3. Education is ***endogenous*** (violates *exogeneity*).

The point (2) above also illustrates the difficulty in learning about educations while *holding all else constant*.

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

Causality

Example: Returns to education

Q: So how can we estimate the returns to education?

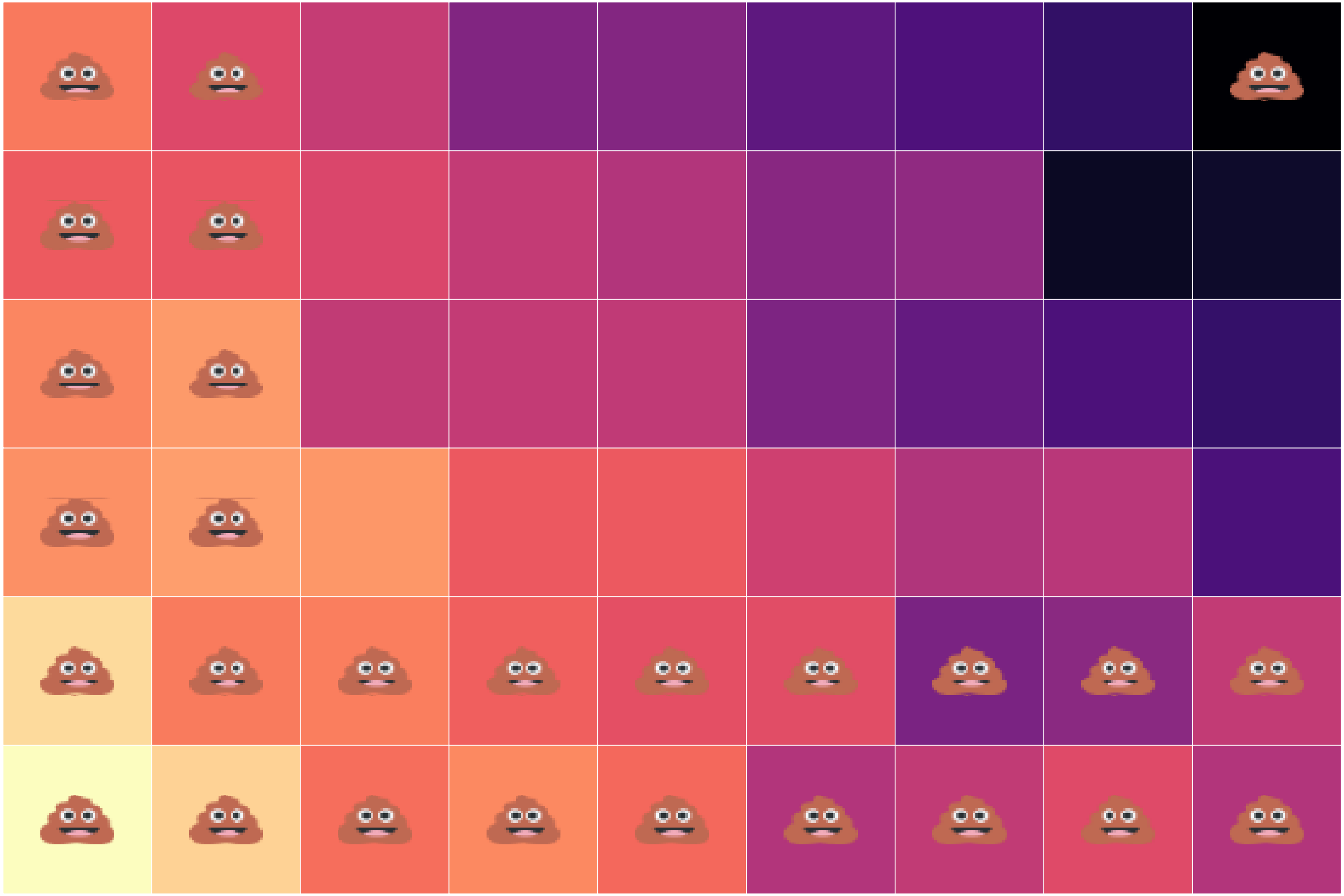
Option 1: Run an **experiment**.

- 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

Unfortunate randomization



Causality

The ideal experiment

The **ideal experiment** would be subtly different.

Rather than comparing units randomized as treatment vs. control, the ideal experiment would compare treatment and control **for the same, exact unit**.

$$y_{\text{Treatment},i} - y_{\text{Control},i}$$

which we will write (for simplicity) as

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

This *ideal experiment* is clearly infeasible[†], but it creates nice notation for causality (the Rubin causal model/Neyman potential outcomes framework).

† Without (1) God-like abilities and multiple universes or (2) a time machine.

The ideal experiment

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 trt.

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

for each individual i.

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

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

The ideal experiment

This model highlights the fundamental problem of causal inference.

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

The challenge:

If we observe $y_{1,i}$, then we cannot observe $y_{0,i}$.
If we observe $y_{0,i}$, then we cannot observe $y_{1,i}$.

Causality

The ideal experiment

So a dataset that we actually observe for 6 people will look 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 $\overline{\tau}$?

Causally estimating the treatment effect

Notation: Let D_i be 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 $\overline{\tau}$ using only $(y_{1,i}|D_i = 1)$ and $(y_{0,i}|D_i = 0)$?

Causally estimating the treatment effect

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 this simple difference in groups' 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 $\overline{\tau}$?

Time for math!

Causally estimating the treatment effect

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

This assumption says that the treatment effect is equal (constant) across all individuals i.

Note: We defined

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

which implies

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

$\mathbf{Q_{3.0}}$: Is $Avg(y_i \mid D_i = 1) - Avg(y_i \mid D_i = 0)$ a *good* estimator for τ?

Difference in groups' means

$$
\begin{aligned}
&= Avg(y_i \mid D_i = 1) - Avg(y_i \mid D_i = 0) \\
&= Avg(y_{1,i} \mid D_i = 1) - Avg(y_{0,i} \mid D_i = 0) \\
&= Avg(\tau + y_{0,i} \mid D_i = 1) - Avg(y_{0,i} \mid D_i = 0) \\
&= \tau + Avg(y_{0,i} \mid D_i = 1) - Avg(y_{0,i} \mid D_i = 0) \\
&= \text{Average causal effect} + \text{Selection bias}
\end{aligned}
$$

So our proposed group-difference estimator give us the sum of

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

Next time: Solving selection bias.

Table of contents

Admin

Causality