



McCOMBS SCHOOL OF BUSINESS

**Salem Center for Policy**

# Causality

David Puelz



Cause-and-effect

Potential outcomes and Counterfactuals

RCTs and Obs Studies

# Causality and policy analysis?



These two ideas seem completely unrelated.

**But, they are and stealthily show up EVERYWHERE!**



# *The Federal Unemployment Bonus Holds the Recovery Back*

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## **How Often Should You Shower? Celebrities Ignite a Ferocious Debate**

Hollywood types including Jake Gyllenhaal, Mila Kunis, Ashton Kutcher and Dax Shepard take a lax approach to hygiene, stoking a contentious uproar on how often one should bathe. It mirrors a similar discord in the medical community, and among everyday people.

# Cause-and-effect $\iff$ policy impacts



These two paradigms in the title are one and the same! One is a general framework, and one is specific to the policy arena.

**Cause** is a statement of something being manipulated or changed  
**Effect** is a measure of the change in an outcome of interest

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- “Cause” is the same as a policy introduction or change
- “Effect” is the unique, independent measurement of how the cause modulated some other part of our system

## Cause-and-effect: policy decisions as cause



We see **causes** all of the time

→ The federal government increases the minimum wage

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- Sweetgreen decreases its Kale caesar salad price by \$2
- States implement stay-at-home orders during the pandemic

Cause-and-effect: varying features (treatments) as cause



Causes might also just be variation in a population

→ Are dog-lovers nicer people?

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Causes might also just be variation in a population

- Are dog-lovers nicer people?
- Does race affect hiring decisions?
- Does age affect COVID-19 mortality?
- Is there a "gender-gap" in salary?

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- What outcomes do we look at? (**lower class unemployment rate, income, ...**)
- How do we measure them? (**government data, surveys, ...**)
- Are there other variables that might affect the outcomes *and* the causes? (**current economic conditions, differences among states, ...**)





The **difficult** question is, what about the **effects**?

- What outcomes do we look at? (**revenue, count of kale caesars sold, number of daily lunch visitors, ...**)
- How do we measure them? (**financial data, ...**)
- Are there other variables that might affect the outcomes *and* the causes? (**time of year (seasonality), temperature, weather, length of daily wait time, ...**)

# Racial discrimination in hiring?



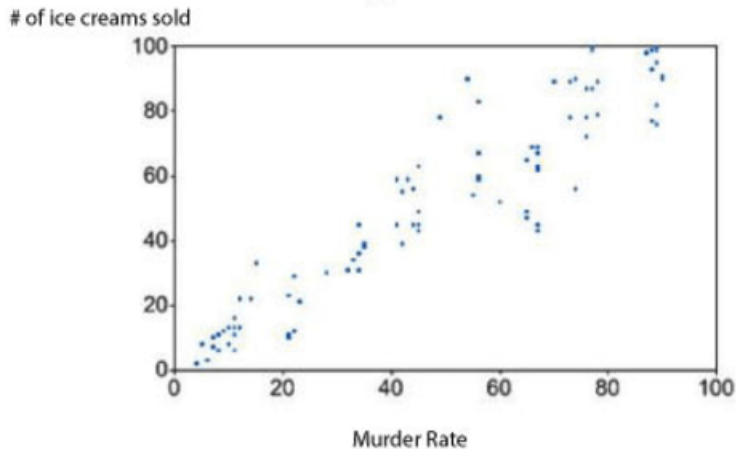
The **difficult** question is, what about the **effects**?

- What outcomes do we look at? (**whether or not a job applicant receives a callback**)
- How do we measure them? (**follows directly from above ...**)
- Are there other variables that might affect the outcomes *and* the causes? (**other resume characteristics, average GPA, brand of university,...**)

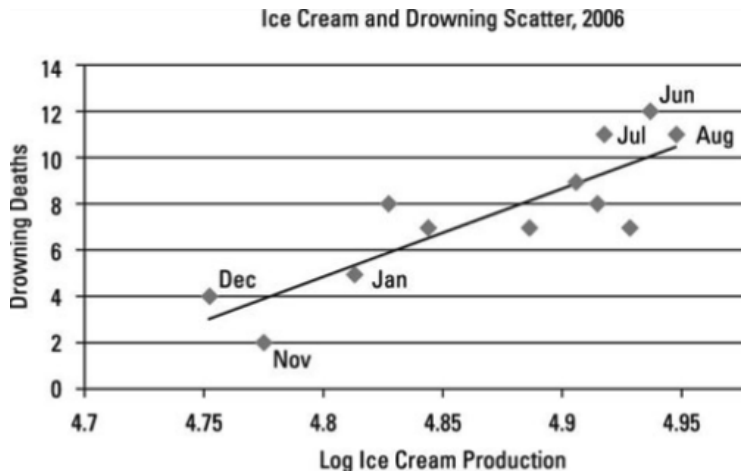
# Ice cream and NYC murder rate



Figure 1



# Ice cream and drownings



# To sum up cause-and-effect



- Challenges are related to both the **system of study** and **ability to gather the right data**.
- With data in hand, you can start to formulate hypotheses and test them.
- There might be lurking variables driving an underlying relationship (**ice cream**). Only an expert (you!) can identify those and take them into account.

**Let's formalize these ideas with some basic notation**



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$$Y_i(z_i)$$





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- with this notation, we have the building blocks to talk about policy **effects**!

## Example: COVID-19 lockdowns



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What are the **potential outcomes**?

## Example: COVID-19 lockdowns



Let's set up the structure of this problem.

Let  $i$  denote a state, so

$$i \in \{\text{New York, California, Florida, Texas, South Dakota, ...}\}$$

First, we define what  $z_i$  is:

$$z_i = \begin{cases} 0 & \text{no lockdown} \\ 1 & \text{lockdown} \end{cases}$$

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Second, we define our outcome:  $Y_i$  : let's choose the cases per capita (in state  $i$ ) after lockdown or no lockdown.



## A brief aside:

Defining exactly what the treatment  $z_i$  is very hard! It could be a combination of many available data.

- masking
- bar and restaurant closures
- school closures
- curfews
- limits to exercise
- retail store closures



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**Q: How would you define  $z_i$ ?**





$i$ (state)	$z_i$ (lockdown)	$Y_i(0)$	$Y_i(1)$
New York			
Florida			
California			
Texas			
South Dakota			
Illinois			
$\vdots$	$\vdots$	$\vdots$	$\vdots$



$i$ (state)	$z_i$ (lockdown)	$Y_i(0)$	$Y_i(1)$
New York	1		
Florida	0		
California	1		
Texas	0		
South Dakota	0		
Illinois	1		
$\vdots$	$\vdots$	$\vdots$	$\vdots$



$i$ (state)	$z_i$ (lockdown)	$Y_i(0)$	$Y_i(1)$
New York	1		.0034
Florida	0	.007	
California	1		.0014
Texas	0	.004	
South Dakota	0	.0028	
Illinois	1		.002
⋮	⋮	⋮	⋮

## Organizing our data: The Science Table (CDC, cases/100k)



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# What is the ideal scenario?



We are able to know both of the potential outcomes for each state!



# Defining a causal effect for NY



We can define the **causal effect** of the “lockdown treatment” as the difference between the two potential outcomes.

$$\tau_{NY} = Y_{NY} \left( \text{Image of busy NYC street} \right) - Y_{NY} \left( \text{Image of quiet NYC street} \right)$$

or written more generally:

$$\tau_i = Y_i(1) - Y_i(0)$$

# The fundamental problem of causal inference



We only observe one of the two potential outcomes for New York and all other states. In general, we always only observe one of two potential outcomes for our units of study.

- **economics of COVID policy**: a state either locks down or doesn't
- **drug trials**: an individual either receives the medicine or the placebo
- **gender wage gap**: a person is either male or female

The unknown outcomes are called the **missing potential outcomes** or **counterfactuals**. This is what makes causality a nontrivial task ... it is a **missing data problem**.

Is all hope lost?





# Is all hope lost?



Definitely not! The potential outcomes will **always** be used as a starting point. Depending on the data and question to be answered, there are several approaches:

- Randomization and the sample average treatment effect
- Observational data – before-and-after and DiD approaches
- Fancier (probabilistic) models to address confounding. Regression, etc. (the “Prediction” part of class).

# The average causal effect across the sample



This is called the **sample average treatment effect**. In stats language, it is called an **estimand**. Let's suppose we have  $N$  units in our data.

$$\begin{aligned}\mathbf{SATE} &= \frac{1}{N} \sum_{i=1}^N \tau_i \\ &= \frac{1}{N} \sum_{i=1}^N \{Y_i(1) - Y_i(0)\}\end{aligned}$$

We still don't know how to calculate this because of the fundamental problem of causal inference.

However, here's an idea ...

# Estimator of the **SATE**



We have the **observed** outcome and treatment. Let's call them:

$$Y_{\text{obs}} = (Y_1, \dots, Y_N)$$

$$Z_{\text{obs}} = (Z_1, \dots, Z_N)$$

Let's define our **estimator** of the **SATE** as the simple **difference-in-means (DiM)** between the treated and control units.

$$\widehat{\text{SATE}} = \frac{1}{\sum_i \mathbb{1}(Z_i = 1)} \sum_i \mathbb{1}(Z_i = 1) Y_i - \frac{1}{\sum_i \mathbb{1}(Z_i = 0)} \sum_i \mathbb{1}(Z_i = 0) Y_i$$

Q: When can this be reasonably interpreted as the average **causal effect**, when can it not?



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- The difference-in-means (DiM) estimator is useful and interpretable for randomized experiments. **Why?**
- The **treatment** and **control** groups that you're computing the "means" over are otherwise equal in all other attributes!
- This eliminates the confounding issue!
- In short, randomizing the **treatment** (cause) is the **gold standard** for understanding causality.

So we have a point estimate of the causal effect ...



**But, what about uncertainty?**



So we have a point estimate of the causal effect ...



## But, what about uncertainty?

- Let's use the known process of randomization to our advantage
- It is common to characterize uncertainty under a particular **null hypothesis**.
- Think of the **null** as the not interesting or exciting causal conclusion. **"There is no causal effect"**.

$$H_0 : Y_i(0) = Y_i(1) \quad \text{for all } i$$

(Then, we use our actual data to probe this statement.)

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$\vdots$	$\vdots$	$\vdots$	$\vdots$

## But where does uncertainty come in?



- Let's go back to our original DiM estimate
- This is given by a single number denoting the difference in average outcome of treated and control units

$$Y_{\text{obs}} = (Y_1, \dots, Y_N)$$

$$Z_{\text{obs}} = (Z_1, \dots, Z_N)$$

$$\text{DiM} = \frac{1}{\sum_i \mathbb{1}(Z_i = 1)} \sum_i \mathbb{1}(Z_i = 1) Y_i - \frac{1}{\sum_i \mathbb{1}(Z_i = 0)} \sum_i \mathbb{1}(Z_i = 0) Y_i$$

= “function of  $Y$  and  $Z$ ”

So, let's write out the estimate explicitly as DiM( $Y, Z$ ).

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- For example, in the simplest case, the randomization could be done by flipping a fair coin for each unit  $i$ . If heads, unit  $i$  is in control, if tails, unit  $i$  is in treatment.
- The point is that I have an experimental design,  $P(Z)$ , from which I can generate many **alternative treatments**!
- We write an alternative treatment as  $Z' \sim P(Z)$

Importantly, under the  $H_0$ , I can now generate a bunch of alternative values of the DiM statistic .... DiM( $Y, Z'$ )

Randomization from the experiment is the uncertainty!



**Q:** Why do we obtain new values of the difference-in-means (DiM) for alternative draws of the treatment/control allocation,  $Z'$ ?

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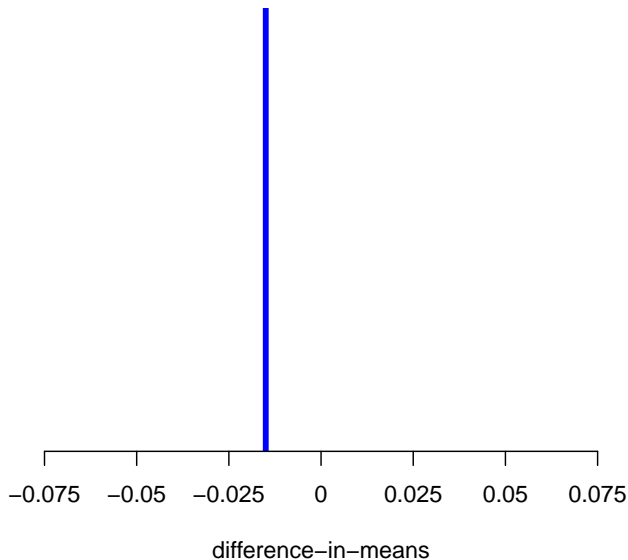


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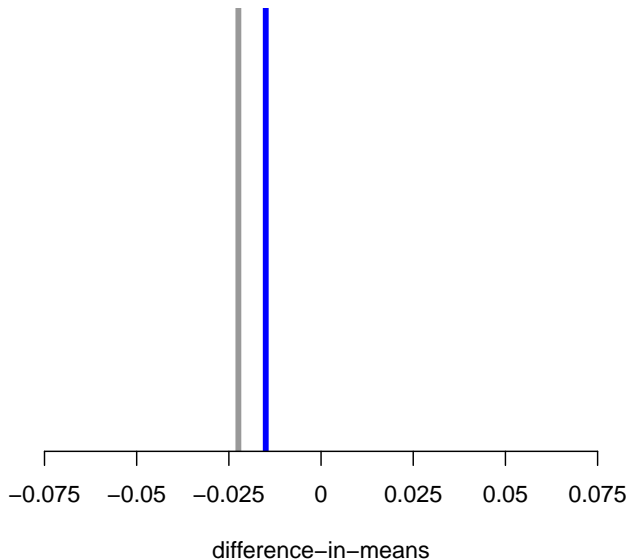
$$\text{DiM}(Y, Z) = \text{average } Y \text{ for treated} - \text{average } Y \text{ for control}$$

For a new  $Z'$ , the groups of treated units and control units are different! So, the averages will be different and thus  $\text{DiM}(Y, Z')$  will be different.

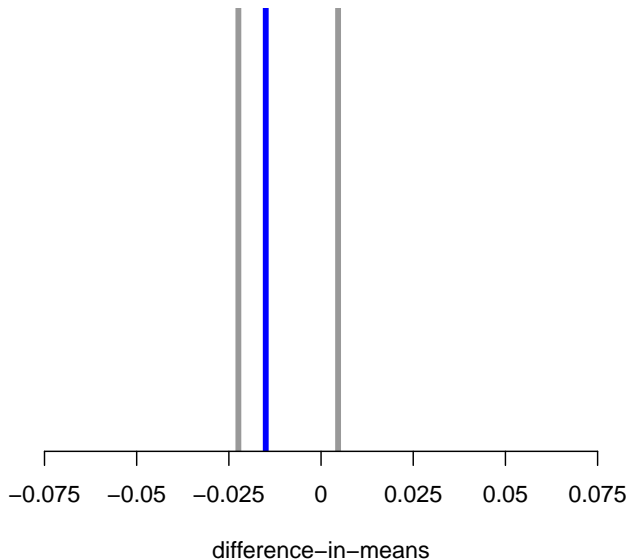
# Observed DiM and its alternative values



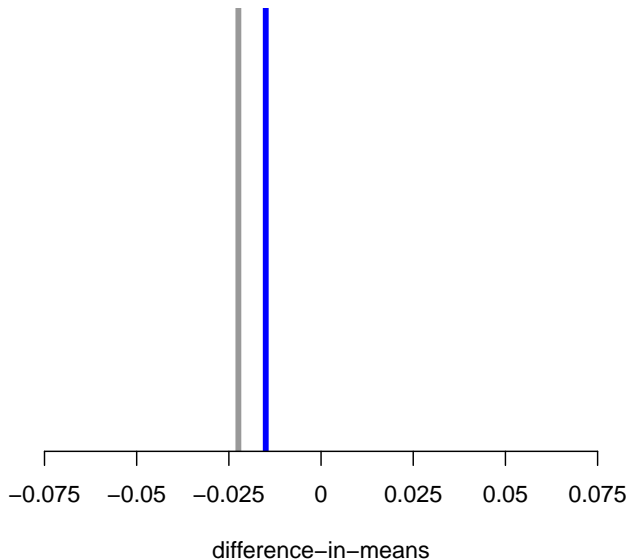
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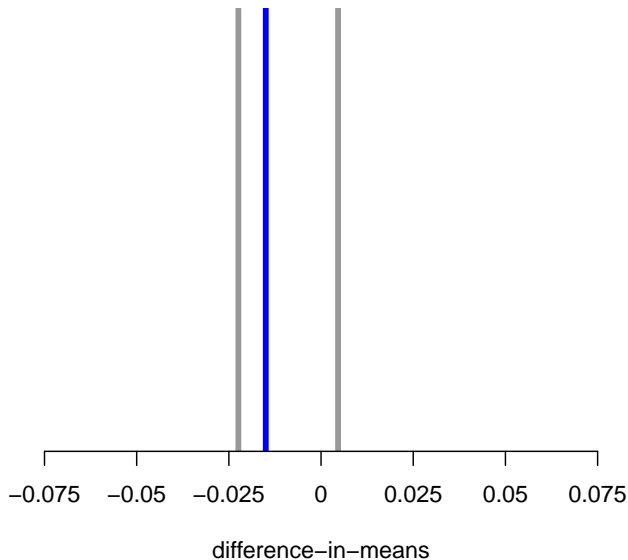
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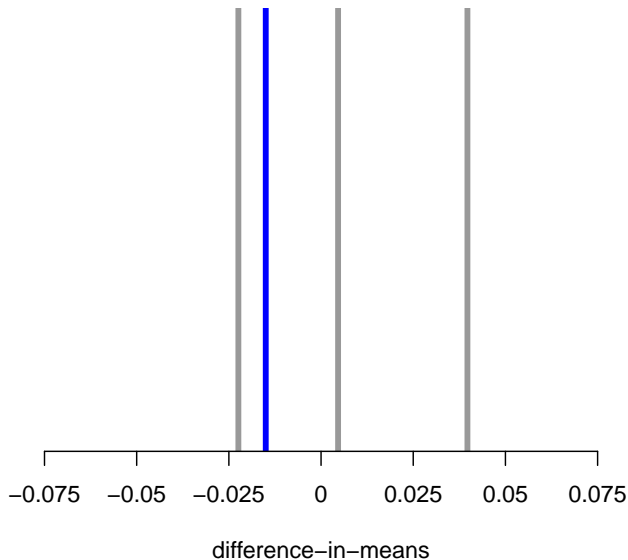


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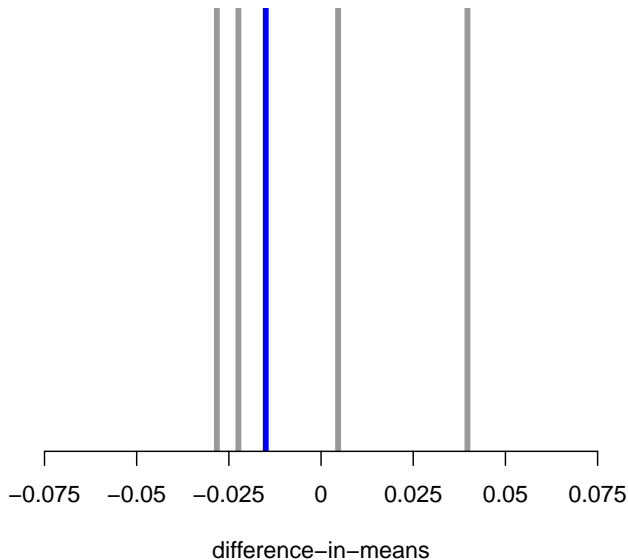




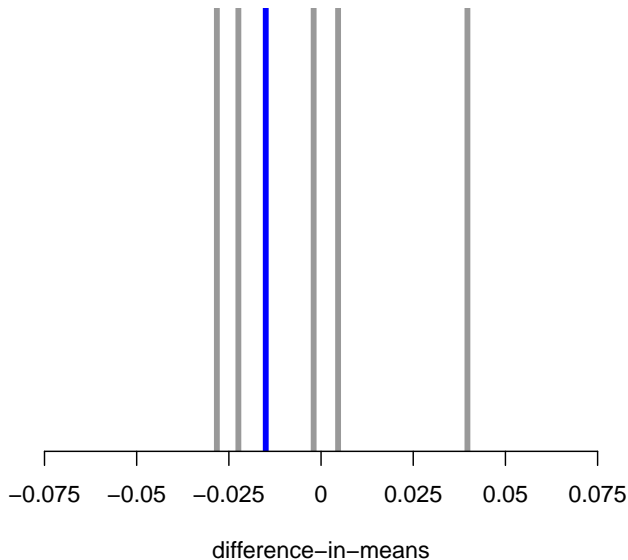
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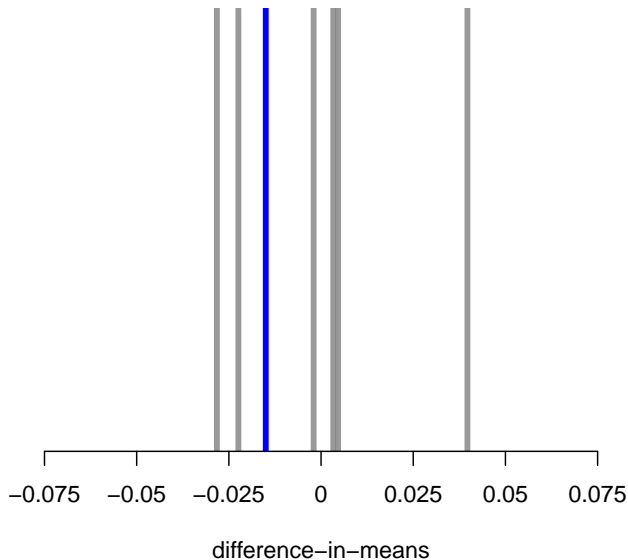
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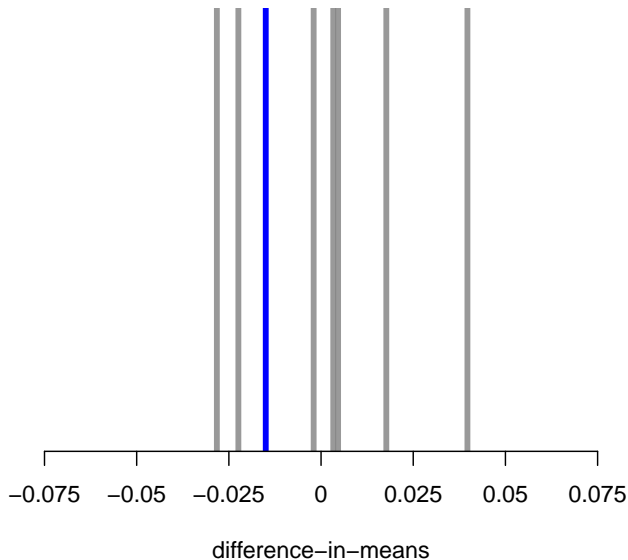
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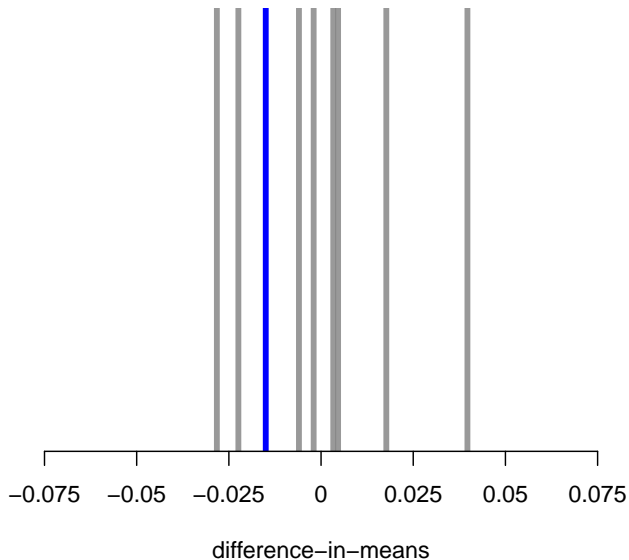
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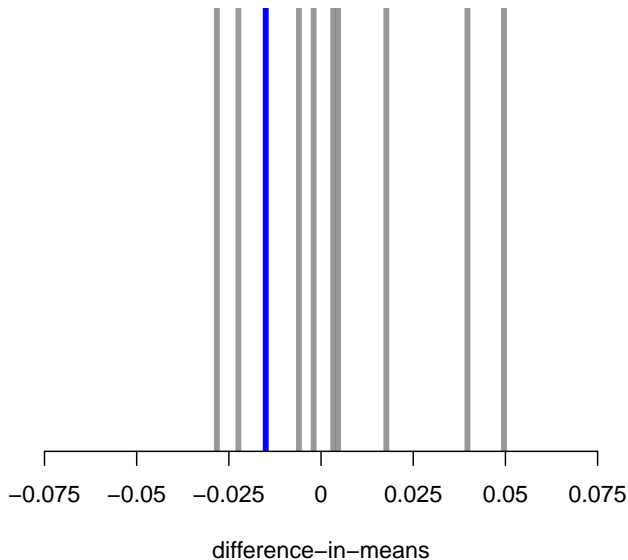
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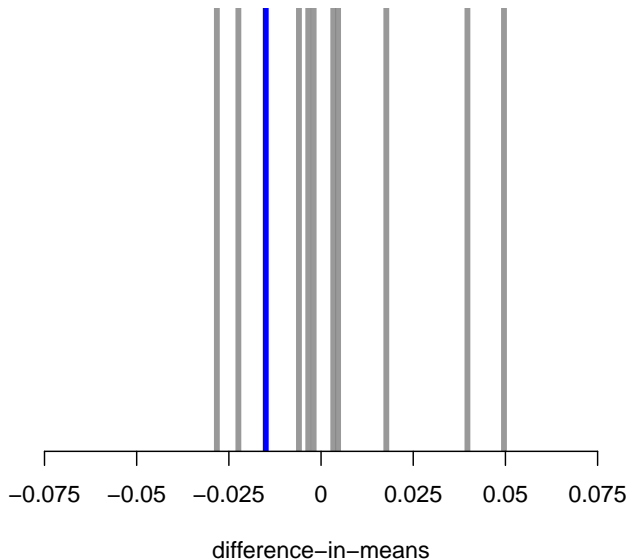
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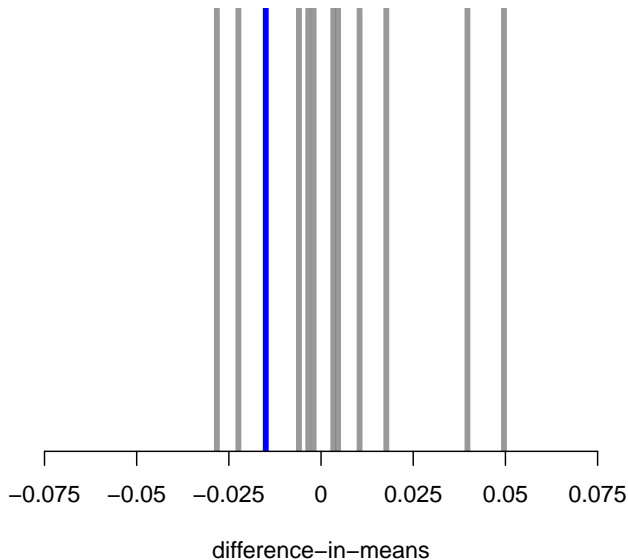


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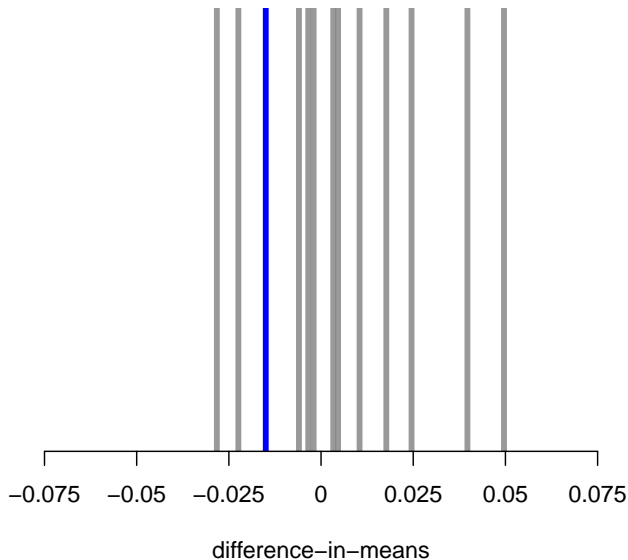




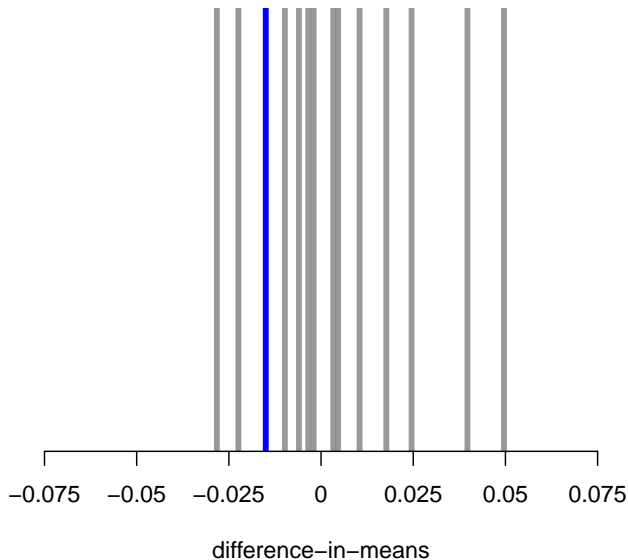
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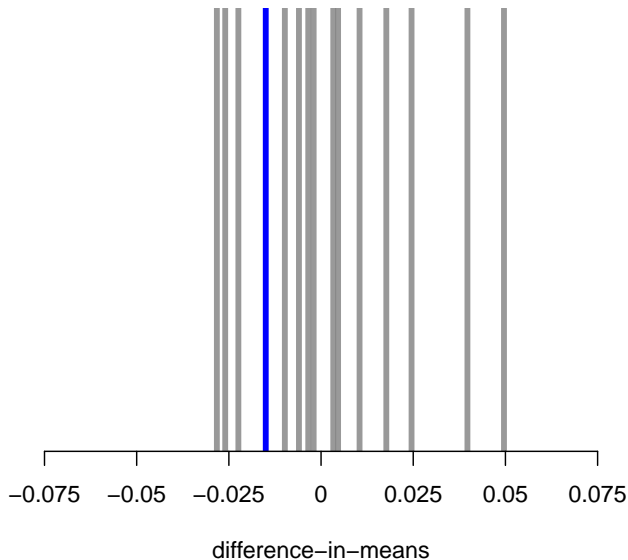
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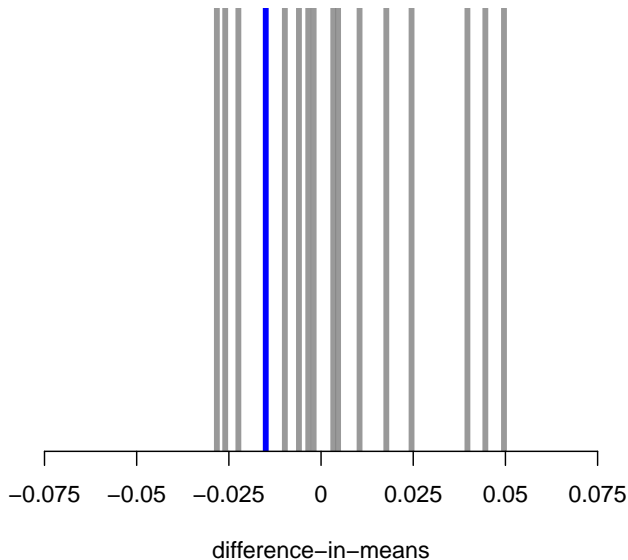
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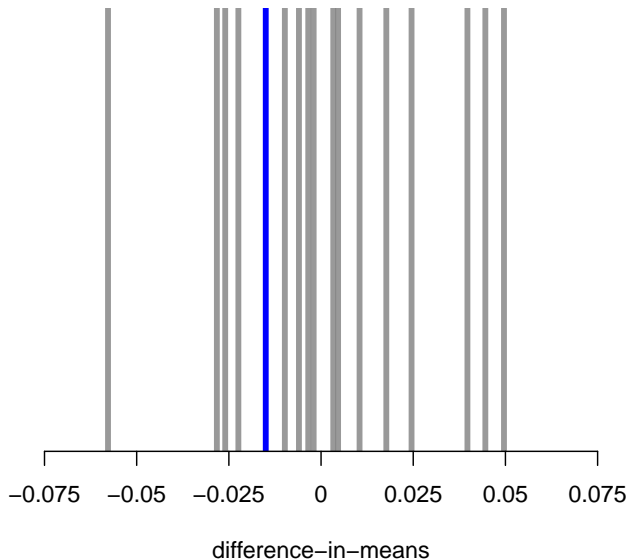
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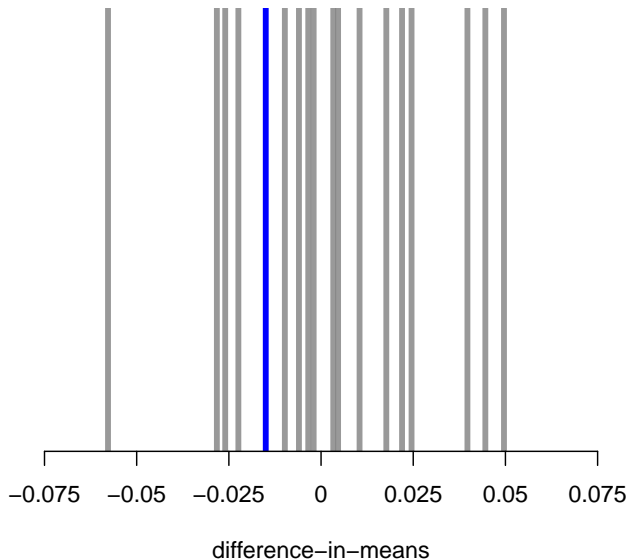
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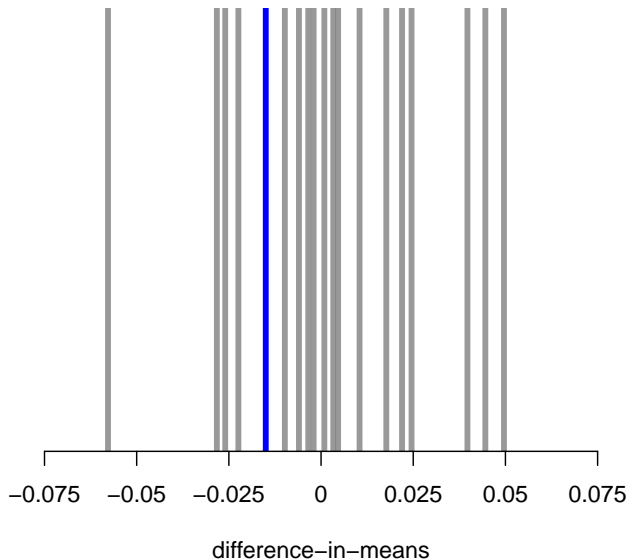
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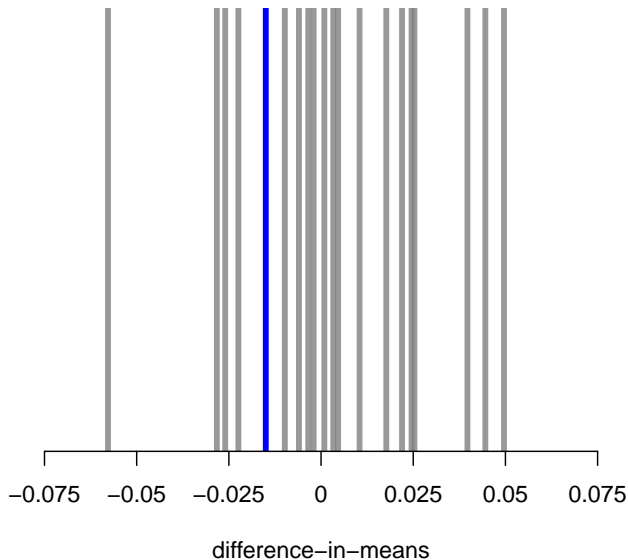


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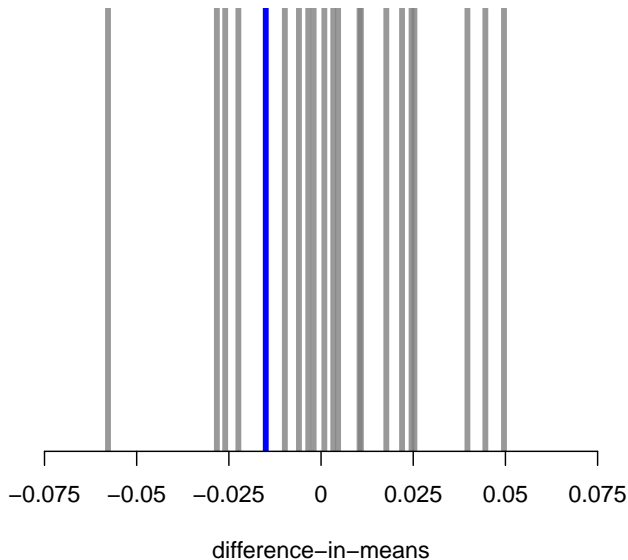




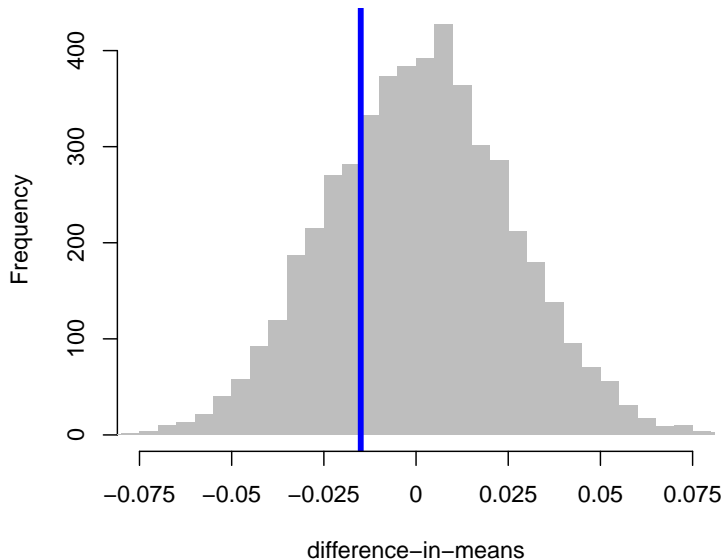
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# Fisher randomization test (1935)



$$H_0 : Y_i(0) = Y_i(1) \quad \text{for all } i$$

## The procedure:

1.  $\text{DiM}_{\text{obs}} = \text{DiM}(Y_{\text{obs}}, Z_{\text{obs}})$ .
2. Draw  $Z' \sim P(Z')$ ,  
store  $\text{DiM}_r = \text{DiM}(Y', Z') \stackrel{H_0}{=} \text{DiM}(Y_{\text{obs}}, Z')$ .
3.  $\text{p-value} = \mathbb{E}[\mathbb{1}\{\text{DiM}_r \geq \text{DiM}_{\text{obs}}\}]$ .

This beautifully simple procedure tells you whether or not your causal effect is “significant” (under the null hypothesis of no effect)!



- First, we defined potential outcomes,  $Y_i(0)$  and  $Y_i(1)$
- Second, we wrote down an **estimand** (SATE) and **estimator** (DiM) of the average causal effect
- Third, we decided that a point estimate isn't enough, we need **uncertainty**!
- Fourth, we discussed the Fisher randomization test (FRT) as a simple and powerful procedure for characterizing uncertainty using the experimental design,  $P(Z)$ .

# From randomized experiments to observational studies



The goal is always the same, estimate the causal effect of some cause/treatment/intervention.

- **Randomized experimental data**: The treatment allocation is **randomized** among study units. This makes treatment and control group comparable and estimating causal effects straightforward.

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- **Randomized experimental data**: The treatment allocation is **randomized** among study units. This makes treatment and control group comparable and estimating causal effects straightforward.
- **Observational study data**: The treatment allocation is *not* randomized, but simply **observed**.
  - \* “I observe states lockdown and not lockdown”
  - \* “I observe minimum wage increases in some places and not others”

We can't control what we observe, but we can still try to use this variation to answer interesting causal questions!



Let's use the minimum wage increase in NJ as our running example.

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  - \* Compare the same group *before* and *after* the intervention: **panel comparison / before-after design**
  - \* Compare before-after difference in NJ to before-after difference in PA: **difference-in-differences**

# Cross-sectional comparison



This one is simple.

Define  $\bar{Y}^{\text{NJ}}$  and  $\bar{Y}^{\text{PA}}$  as the average percentage of full-time employees across fast-food restaurants in New Jersey and Pennsylvania, respectively.

$$\text{causal effect} = \bar{Y}^{\text{NJ}}(\text{treatment}) - \bar{Y}^{\text{PA}}(\text{control})$$

Q: What are some issues with this?

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confounding! NJ and PA might be different in other ways, they have their own economies, political issues, populations, etc.

## Statistical control (**matching**)



This one tries to address the confounding.

Pick covariates  $X$  in the data that might be confounding. Compute the causal effect only on treatment and control units with **identical** values of  $X$ .

$$\text{causal effect} = \overline{Y}_{X=\text{Taco Bell}}^{\text{NJ}} - \overline{Y}_{X=\text{Taco Bell}}^{\text{PA}}$$

It is called **matching** because you match on like values of covariates, i.e., compare apples-to-apples!

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**geographic location, closeness to NJ-PA border, urban or rural area**



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$$Y_{i,\text{before}}^{\text{NJ}} \longrightarrow \text{min wage hike} \longrightarrow Y_{i,\text{after}}^{\text{NJ}}$$

$$Y_{i,\text{before}}^{\text{PA}} \longrightarrow \text{min wage hike} \longrightarrow Y_{i,\text{after}}^{\text{PA}}$$

where  $i$  is a fast-food restaurant.

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where  $i$  is a fast-food restaurant.

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Q: What are some issues with this?

actually, confounding again! time is potentially an issue (remember our ice cream example).

This one tries to remove time-trend confounding by using panel data from both the **treated** state (NJ) and the **control** state (PA).

There are four outcomes of interest now (see previous slide):

$\bar{Y}_{\text{before}}^{\text{NJ}}$ ,  $\bar{Y}_{\text{after}}^{\text{NJ}}$ ,  $\bar{Y}_{\text{before}}^{\text{PA}}$ ,  $\bar{Y}_{\text{after}}^{\text{PA}}$   $\leftarrow$  average full-time employment rates

$$\text{causal effect (DiD)} = (\bar{Y}_{\text{after}}^{\text{NJ}} - \bar{Y}_{\text{before}}^{\text{NJ}}) - (\bar{Y}_{\text{after}}^{\text{PA}} - \bar{Y}_{\text{before}}^{\text{PA}})$$

**Why does this work?**

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## Why does this work?

- answer 1: **picture**
- answer 2: **some math**

## Difference-in-differences ([math explanation](#))



$4 \times N$  potential outcomes, 2 for each state.

$$\{Y_i^{\text{NJ}}(0), Y_i^{\text{NJ}}(1), Y_i^{\text{PA}}(0), Y_i^{\text{PA}}(1)\} \quad \text{for all } i = 1, \dots, N$$

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$$\begin{aligned} \text{causal effect (DiD)} &= (\bar{Y}_{\text{after}}^{\text{NJ}} - \bar{Y}_{\text{before}}^{\text{NJ}}) - (\bar{Y}_{\text{after}}^{\text{PA}} - \bar{Y}_{\text{before}}^{\text{PA}}) \\ &= (\mathbb{E}[Y_i^{\text{NJ}}(1) \mid \text{after}] - \mathbb{E}[Y_i^{\text{NJ}}(0) \mid \text{before}]) \\ &\quad - (\mathbb{E}[Y_i^{\text{PA}}(0) \mid \text{after}] - \mathbb{E}[Y_i^{\text{PA}}(0) \mid \text{before}]) + 0 \\ &= (\mathbb{E}[Y_i^{\text{NJ}}(1) \mid \text{after}] - \mathbb{E}[Y_i^{\text{NJ}}(0) \mid \text{before}]) \\ &\quad - (\mathbb{E}[Y_i^{\text{PA}}(0) \mid \text{after}] - \mathbb{E}[Y_i^{\text{PA}}(0) \mid \text{before}]) \\ &\quad + (\mathbb{E}[Y_i^{\text{NJ}}(0) \mid \text{after}] - \mathbb{E}[Y_i^{\text{NJ}}(0) \mid \text{after}]) \\ &= (\mathbb{E}[Y_i^{\text{NJ}}(1) \mid \text{after}] - \mathbb{E}[Y_i^{\text{NJ}}(0) \mid \text{after}]) \\ &\quad + (\mathbb{E}[Y_i^{\text{NJ}}(0) \mid \text{after}] - \mathbb{E}[Y_i^{\text{NJ}}(0) \mid \text{before}]) \\ &\quad - (\mathbb{E}[Y_i^{\text{PA}}(0) \mid \text{after}] - \mathbb{E}[Y_i^{\text{PA}}(0) \mid \text{before}]) \end{aligned}$$

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parallel trend assumption: **second part** = 0