# Lecture 7: Quasi-experimental design

## Difference-in-Difference

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# Outline



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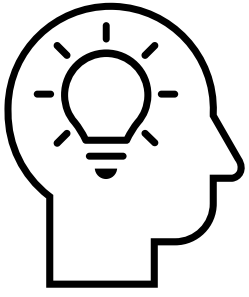
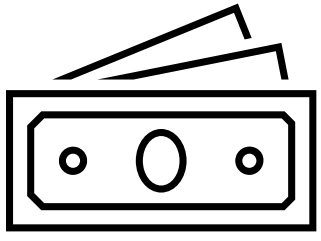
# Randomized experiments are powerful, but...

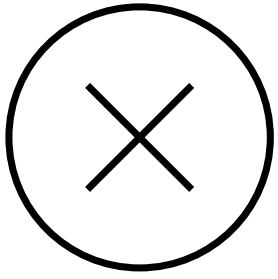
In many scenarios, we cannot do randomized experiments.



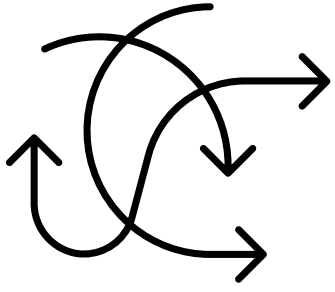
Unethical



Costly



Physically Impossible



Infeasible

# Quasi-experiments

**Definition (Quasi-experiments)**

Research designs that resemble experimental designs but lack full random assignment of participants to groups.

**Different designs of quasi-experiments**

- Interrupted time-series design
- Event studies
- Case-control design
- **Difference-in-difference (DID)**
- **Regression discontinuity design (RDD)**
- ...

# Quasi-experiments

## Definition (Quasi-experiments)

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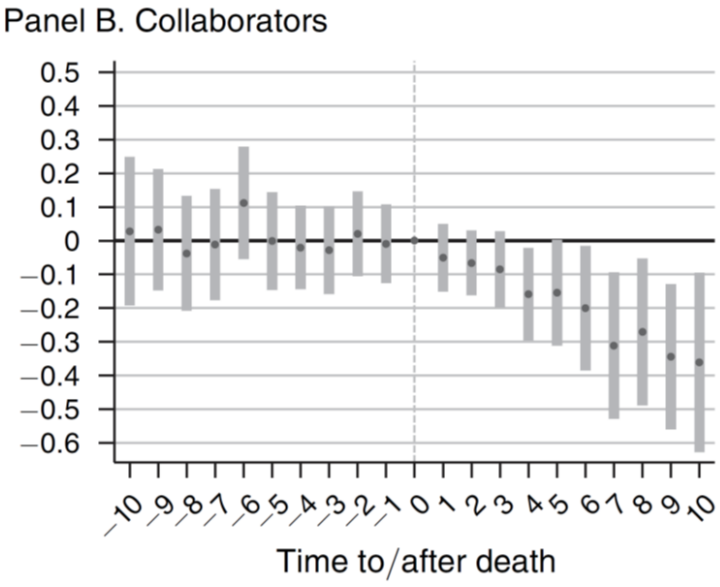
- ...

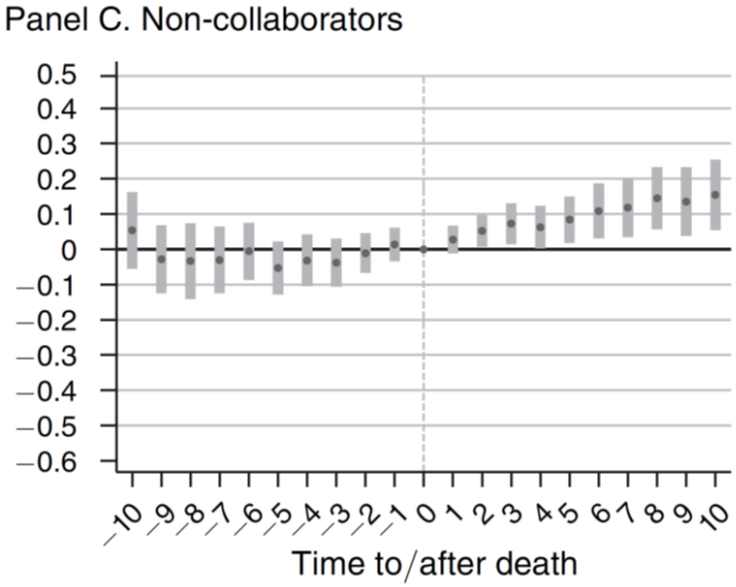
# Numerous examples of quasi-experiments...

Azoulay et al. (2019) investigated how premature death of eminent life scientists alter the vitality of their fields.

- The flow of articles by collaborators into affected fields decreases after the death of a star scientist.
- The flow of articles by non-collaborators increases markedly.




(a) Collaborators

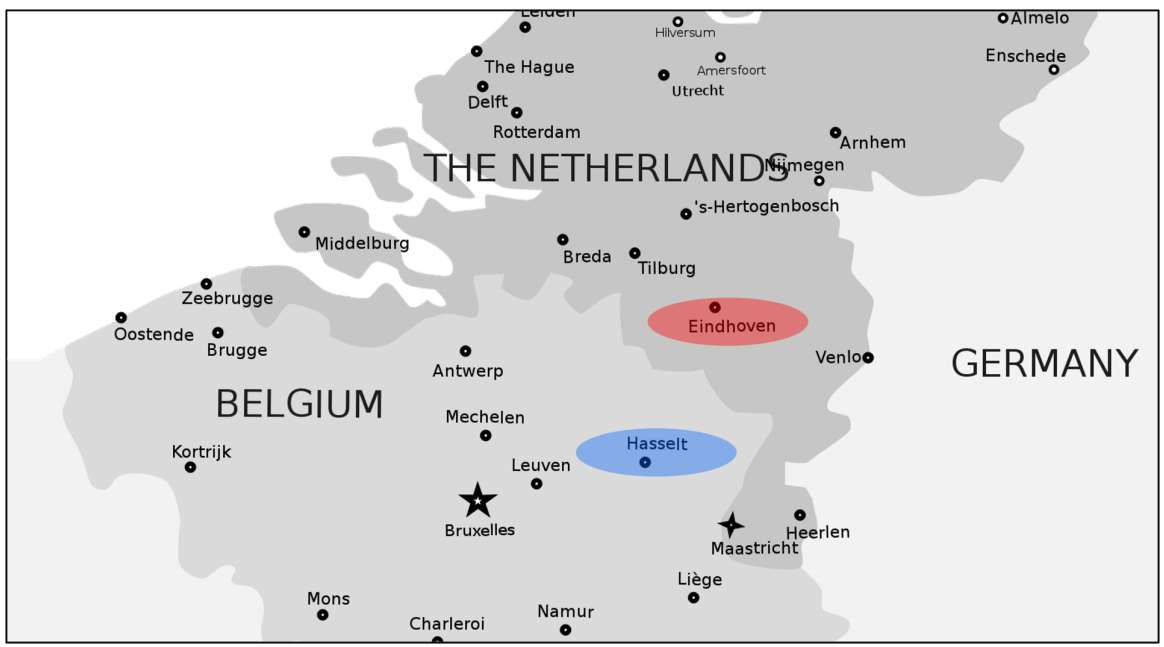



(b) Non-collaborators

# Numerous examples of quasi-experiments...

Bol.com - a Dutch e-retailer of consumer electronics - examined the effect of display ads on sales.

- They compared two cities: one in Holland and one in Belgium.
- They stopped the display ads in the Belgian city.

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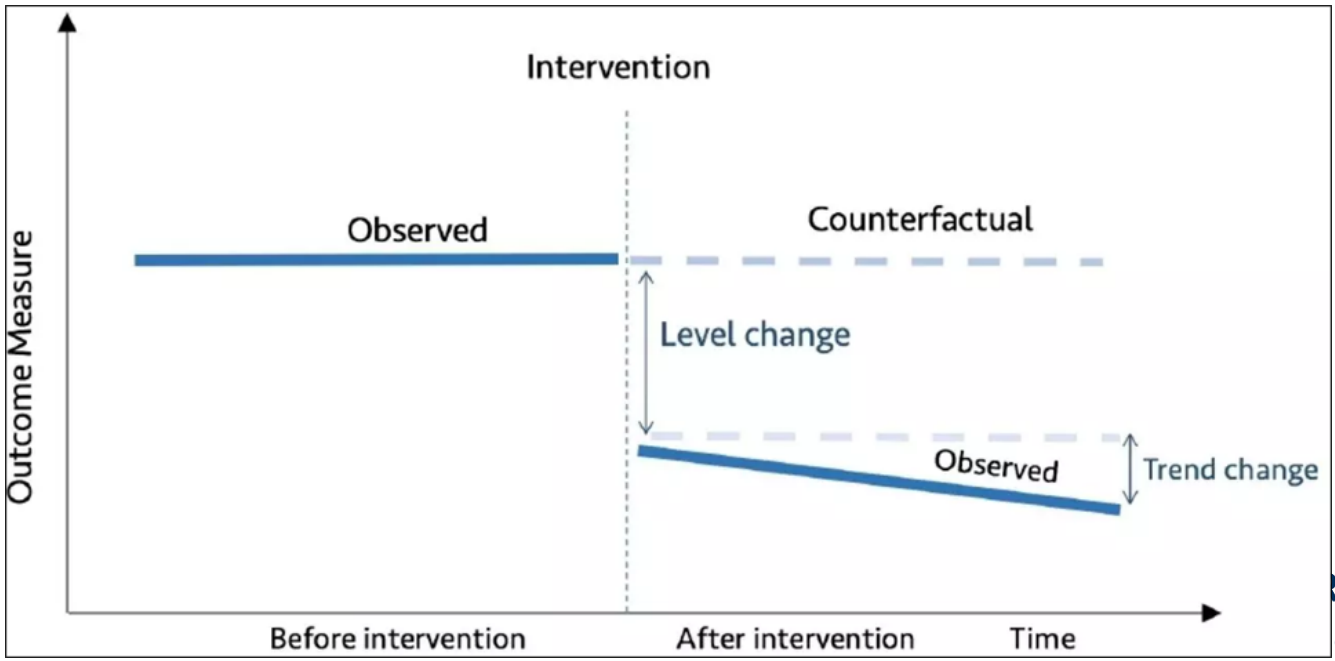
# Before-after design

In practice, we observe an event happens at certain time point.

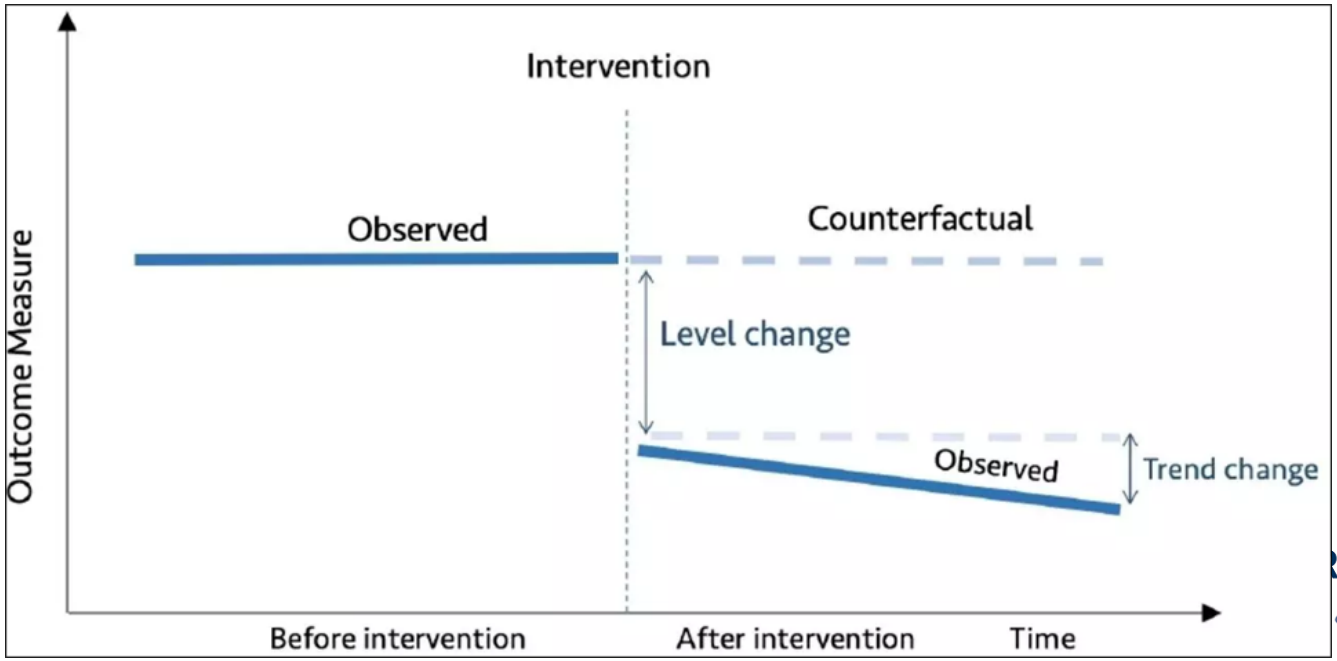
- Predict the "after" outcome based on the before data.
- Compare the "after" outcome with the predicted one.
  - **Level changes**: up and down shift.
  - **Trend changes**: changes in the trajectory.



# Before-after design

In practice, we observe an event happens at certain time point.

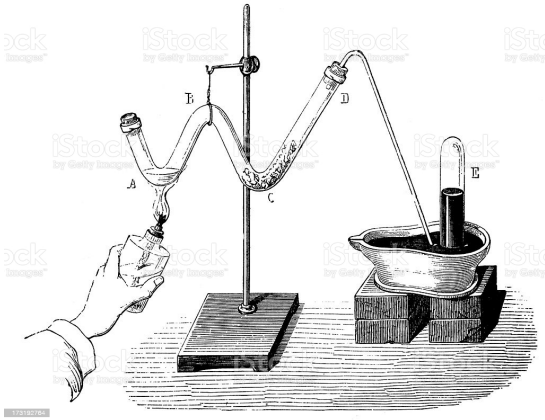
- German reunification
- The implementation of GDPR
- The Great Recession in 2008
- ...



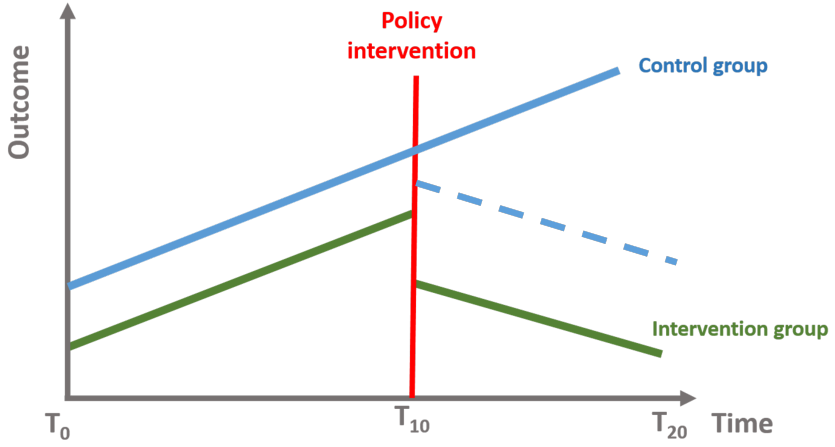
# Before-after design

The use of "before" to predict the counterfactual of "after?"

For some scenarios, it's credible:



What if the counterfactual also goes through a "regime shift?"

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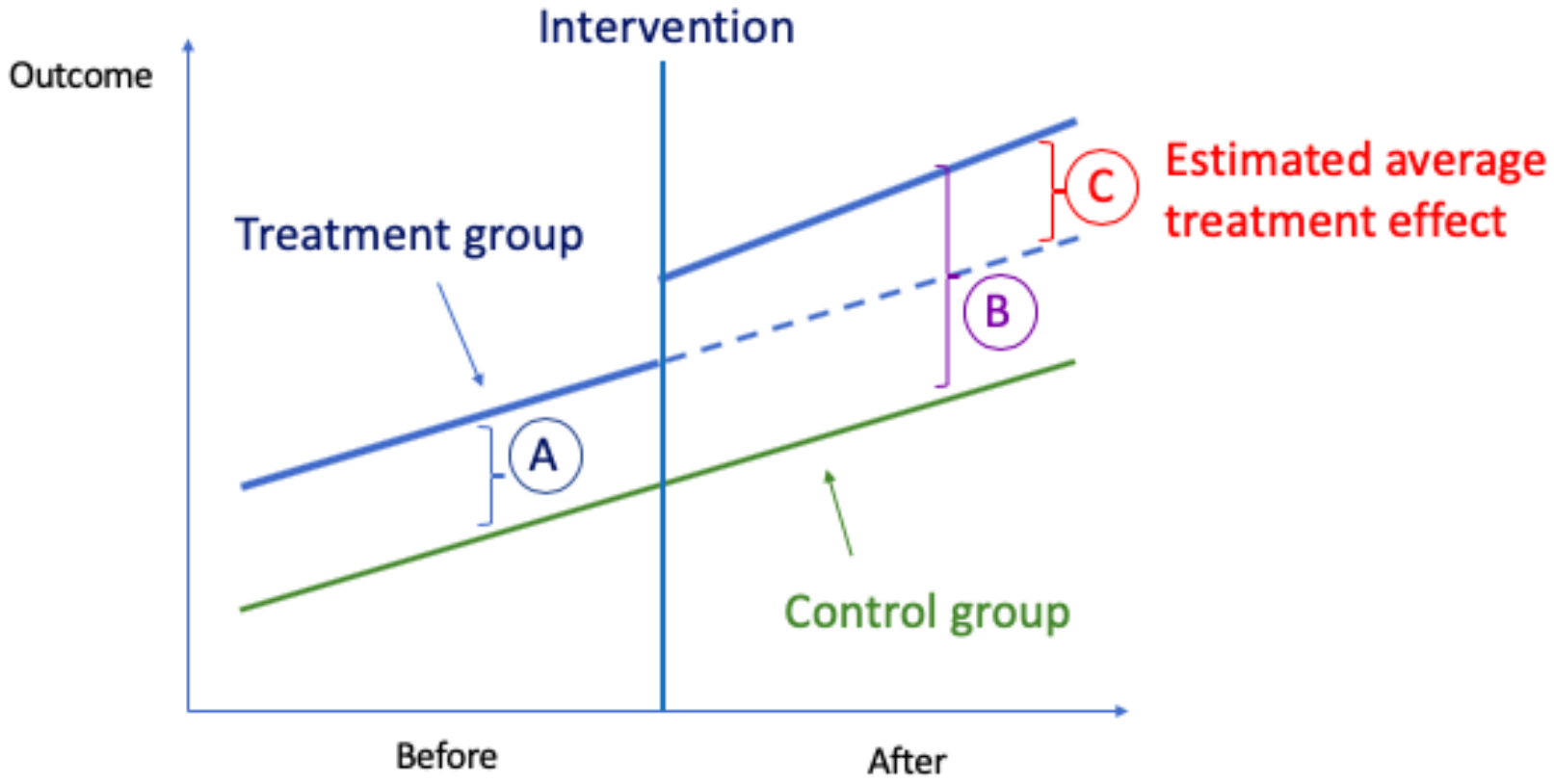
# Two sources of variations




- **Time variation**: Before vs. after the intervention.
- **Treatment variation**: Treatment vs. control group.

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# The canonical DID: basic setup

- $N$ units $(i)$ across $T = 2$ time periods $(t)$.
- A binary treatment $D_{it} = \{0, 1\}$.
- Outcome $Y_{it}$.

- Treatment happens in period 2 $(t = 2)$.
- A group of always untreated units $(D_{it} = 0, \forall t)$

## Potential outcomes

2 time periods $\times$ 2 treatment status $=$ 4 potential outcomes.

| $Y_{it}(D_i)$ | Time 1 | Time 2 |
|---|---|---|
| Untreated | $Y_{i1}(0)$ | $Y_{i2}(0)$ |
| Treated | $Y_{i1}(1)$ | $Y_{i2}(1)$ |

# The canonical DID: potential outcomes

Observed vs. unobserved potential outcomes.

| $Y_{it}(D_i)$ | Time 1 | Time 2 |
|---|---|---|
| Untreated | $Y_{i1}(0)$ | ~~$Y_{i2}(0)$~~ |
| Treated | ~~$Y_{i1}(1)$~~ | $Y_{i2}(1)$ |

(a) Treatment Group

| $Y_{it}(D_i)$ | Time 1 | Time 2 |
|---|---|---|
| Untreated | $Y_{i1}(0)$ | $Y_{i2}(0)$ |
| Treated | ~~$Y_{i1}(1)$~~ | ~~$Y_{i2}(1)$~~ |

(b) Control Group

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# The canonical DID: the target of identification

- **Primary interests**: the treatment effects of the intervention ($t = 2$).

$$\tau_{\text{ATE}} = E\left(Y_{i2}\left(1\right) - Y_{i2}\left(0\right)\right)$$

- But, we only observe:

$$\begin{cases} E\left(Y_{i2}\left(1\right) \mid D_{i2} = 1\right) & \text{For treatment group} \\ E\left(Y_{i2}\left(0\right) \mid D_{i2} = 0\right) & \text{For control group} \end{cases}$$

- Assuming **unconfoundedness**, we can identify $\tau_{\text{ATE}}$:

$$\begin{cases} E\left(Y_{i2}\left(1\right) \mid D_{i2} = 1\right) & = E\left(Y_{i2}\left(1\right)\right) \\ E\left(Y_{i2}\left(0\right) \mid D_{i2} = 0\right) & = E\left(Y_{i2}\left(0\right)\right) \end{cases}$$

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# The canonical DID: the target of identification

- The unconfoundedness assumption is unrealistic for quasi-experiments.
- By definition, the treatment is not randomly assigned!

**The average treatment effect on the treated (ATT)**

Researchers or policymakers may be more interested in ATT, because:

1. It is uncontrollable who will be in the treatment;
2. Participants of the treatment are more relevant for the policy.

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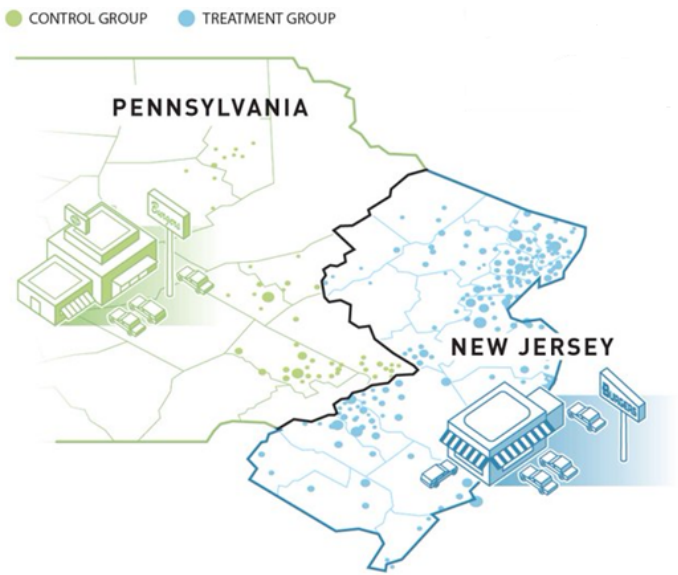
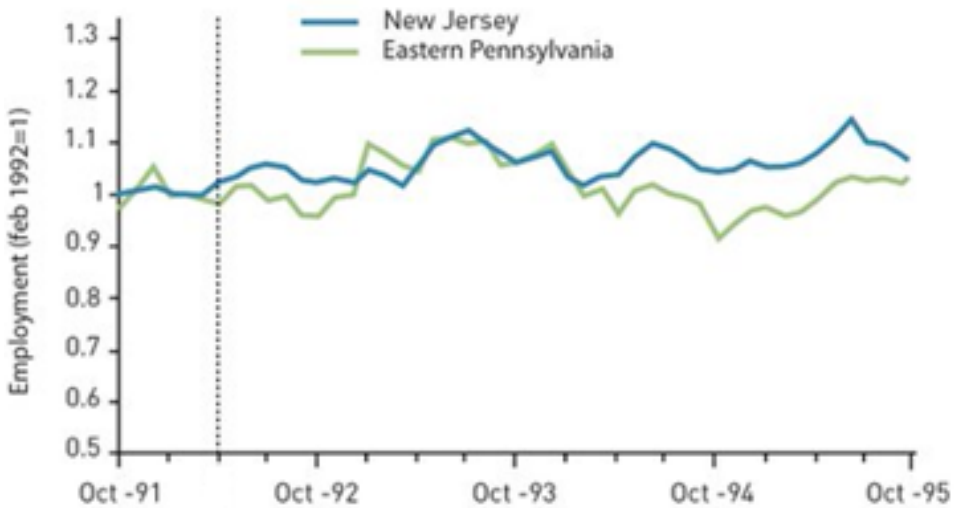
# The canonical DID: examples

The influence of minimum wage on employment from Card and Krueger (1994)[1].




[1] Figures are accredited to Jahan Jarnestad at the Royal Swedish Academy of Sciences.

# The canonical DID: examples

Snapchat did a lot of field experiments of the app features and recommendation algorithms.

- They mainly focused on the US market.
- Always used New Zealand as the control group.

# The canonical DID: identification assumptions

The target is ATT: $\tau_{\text{ATT}} = E\left[Y_{i2}\left(1\right) - Y_{i2}\left(0\right) \mid D_{i2} = 1\right]$

- We observe $E\left[Y_{i2}\left(1\right) \mid D_{i2} = 1\right]$ for the treatment group.
- But, we do not observe $E\left[Y_{i2}\left(0\right) \mid D_{i2} = 1\right]$.

One assumption to make is **ignorability**:

$$E\left[Y_{i2}\left(0\right) \mid D_{i2} = 1\right] = E\left[Y_{i2}\left(0\right) \mid D_{i2} = 0\right]$$

- The treatment status is independent of $Y_{i2}\left(0\right)$.
- Yet, this is still a strong assumption.
- People do not participate because they see little benefits.

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# The canonical DID: identification assumptions

It turns out we need an assumption weaker than ignorability.

## The Parallel Trend Assumption

**The before-after difference of the potential outcomes under no treatment is ignorable**:

$$\underbrace{E\left[Y_{i2}\left(0\right)-Y_{i1}\left(0\right)\mid D_{i2}=1\right]}_{\text{Before-after differences of treatment group}} = \underbrace{E\left[Y_{i2}\left(0\right)-Y_{i1}\left(0\right)\mid D_{i2}=0\right]}_{\text{Before-after differences of control group}}$$

Under the the parallel trend assumption, **we can identify the ATT of the treatment at** $t=2$!

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# The canonical DID: proof of identification results

Technical proof:
If we rearrange the parallel trend assumption, we have:

$$\begin{aligned} &E\left[Y_{i2}\left(0\right) \mid D_{i2}=1\right]-E\left[Y_{i1}\left(0\right) \mid D_{i2}=1\right] \\ &-E\left[Y_{i2}\left(0\right) \mid D_{i2}=0\right]+E\left[Y_{i1}\left(0\right) \mid D_{i2}=0\right]=0 \end{aligned} \tag{1}$$

The ATT is:

$$\tau_{\text{ATT}}=E\left[Y_{i2}\left(1\right) \mid D_{i2}=1\right]-E\left[Y_{i2}\left(0\right) \mid D_{i2}=1\right]$$

If we add equation (1) to the ATT formula, we have:

$$\begin{aligned} \tau_{\text{ATT}}=&\left(E\left[Y_{i2}\left(1\right) \mid D_{i2}=1\right]-E\left[Y_{i1}\left(0\right) \mid D_{i2}=1\right]\right) \\ &-\left(E\left[Y_{i2}\left(0\right) \mid D_{i2}=0\right]-E\left[Y_{i1}\left(0\right) \mid D_{i2}=0\right]\right) \end{aligned} \tag{2}$$

All the terms in equation (2) can be inferred from the observation.

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# Outline



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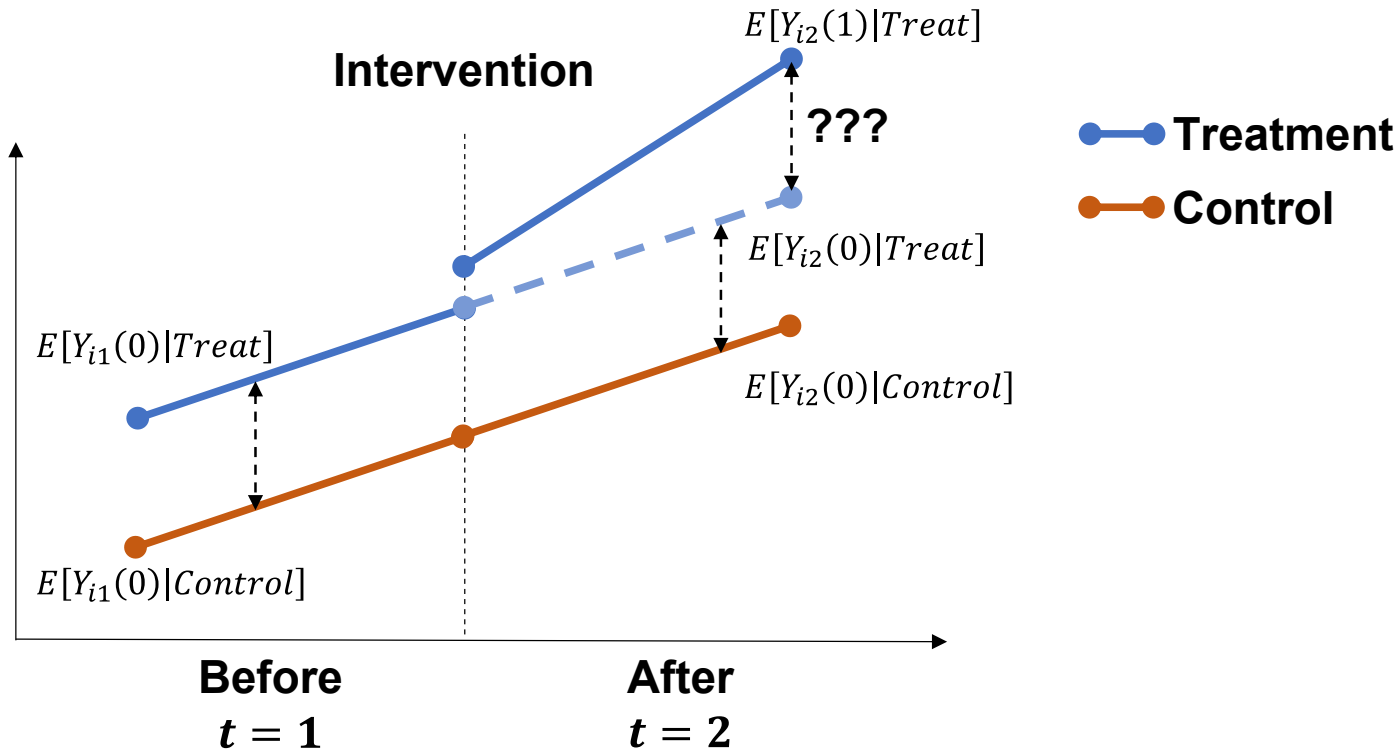
# Understanding the parallel trend




We need to calculate: $\tau_{\text{ATT}} = E\left[Y_{i2}\left(1\right) \mid \text{Treat}\right] - \cancel{E\left[Y_{i2}\left(0\right) \mid \text{Treat}\right]}$.

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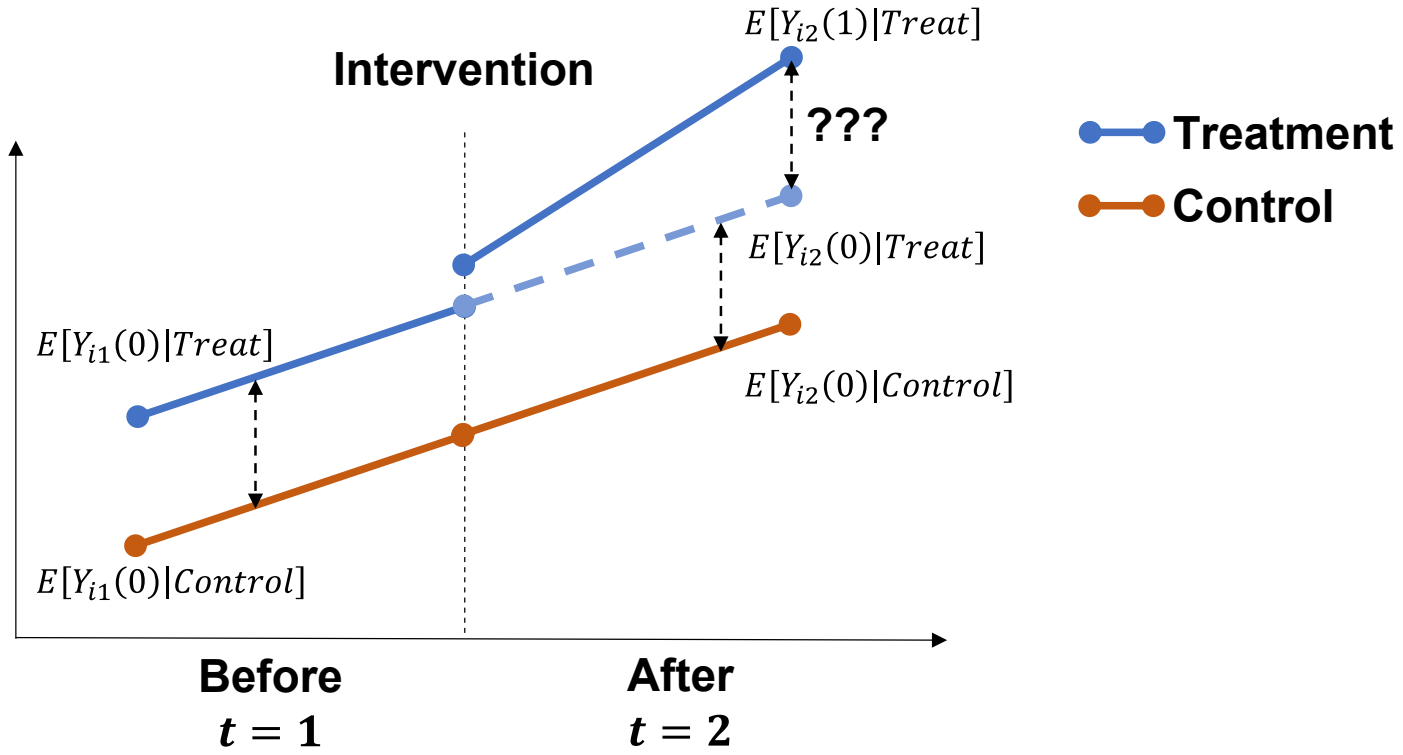
# Understanding the parallel trend




**Counterfactual language**: in the absence of the treatment, the difference between the treated and control group is constant over time.

$$E\left[Y_{i1}\left(0\right) \mid \text{Treat}\right] - E\left[Y_{i1}\left(0\right) \mid \text{Control}\right] = \cancel{E\left[Y_{i2}\left(0\right) \mid \text{Treat}\right]} - E\left[Y_{i2}\left(0\right) \mid \text{Control}\right]$$

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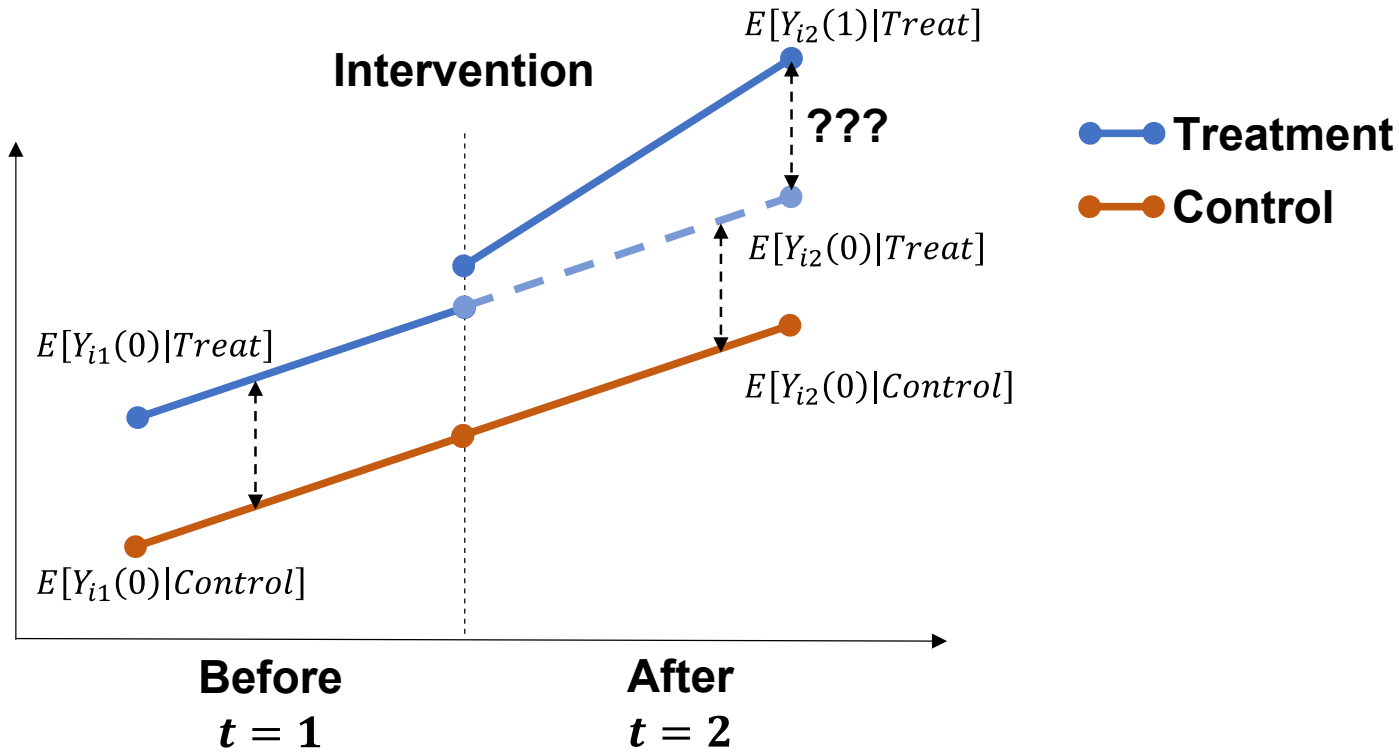
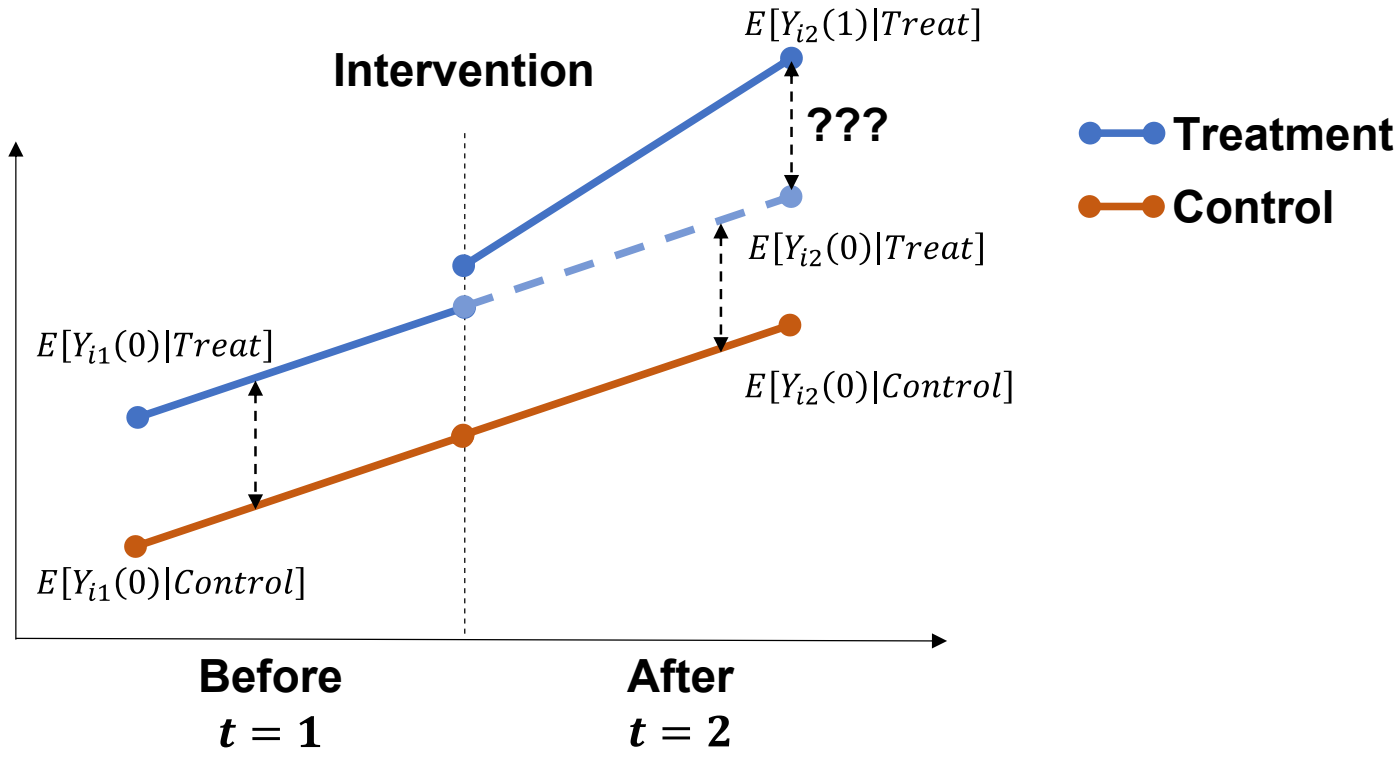
# Understanding the parallel trend




Assume the parallel trend, we can solve for $E[Y_{i2}(0) \mid \text{Treat}]$ with:

$$E[Y_{i1}(0) \mid \text{Treat}] - E[Y_{i1}(0) \mid \text{Control}] = \underbrace{E[Y_{i2}(0) \mid \text{Treat}]}_{\text{Solve for this term}} - E[Y_{i2}(0) \mid \text{Control}]$$

# Parallel trend assumption is untestable




- It's an assumption on an unobserved potential outcome $E[Y_{i2}(0) \mid \text{Treat}]$.
- The fundamental problem of causal inference $\rightarrow$ it's untestable!

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# Parallel trend assumption is scale dependent

The parallel trend holds for $Y$, but may not for a monotonic transformation of $Y$.

| $Y_{it}(D_i)$ | Time 1 | Time 2 |
|---|---|---|
| Untreated | $Y_{i1}(0) = 1$ | $Y_{i2}(0) = 3$ |
| Treated | $Y_{i1}(0) = 2$ | $Y_{i2}(0) = 4$ |

(c) Original Values: $2 - 1 = 4 - 3$

| $Y_{it}(D_i)$ | Time 1 | Time 2 |
|---|---|---|
| Untreated | $log(Y_{i1}(0)) = log(1)$ | $log(Y_{i2}(0)) = log(3)$ |
| Treated | $log(Y_{i1}(0)) = log(2)$ | $log(Y_{i2}(0)) = log(4)$ |

(d) Log-transformed Values: $log(2) \neq log\left(\frac{4}{3}\right)$

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# Non-parametric estimation

From the identification results on page 24

$$\begin{aligned}\tau_{\text{ATT}} = &\left(E\left[Y_{i2}\left(1\right) \mid \text{Treat}\right] - E\left[Y_{i1}\left(0\right) \mid \text{Treat}\right]\right) \\ &- \left(E\left[Y_{i2}\left(0\right) \mid \text{Control}\right] - E\left[Y_{i1}\left(0\right) \mid \text{Control}\right]\right),\end{aligned}$$

A natural non-parametric estimator is

$$\begin{aligned}\hat{\tau}_{\text{ATT}} = &\left(\frac{1}{N_1}\sum_{i\in\text{Treat}} Y_{i2} - \frac{1}{N_1}\sum_{i\in\text{Treat}} Y_{i1}\right), \\ &- \left(\frac{1}{N_0}\sum_{i\in\text{Control}} Y_{i2} - \frac{1}{N_0}\sum_{i\in\text{Control}} Y_{i1}\right)\end{aligned}$$

Two differences $\Rightarrow$ "Difference-in-difference"

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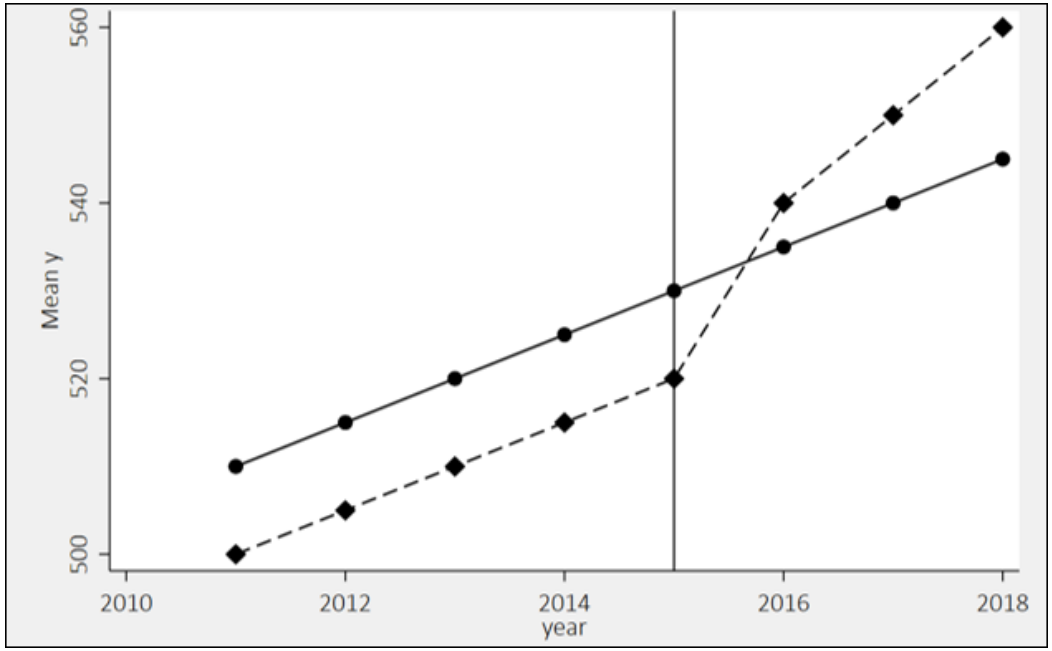
# Multiple time periods

- Average the time periods and get 4 outcomes (treated vs. control and before vs. after).
  - Not efficient as aggregation loses information.
- **Model-based estimation**: two-way fixed effects regression.



# Model-based estimation

A two-way fixed effects regressioin:

$$y_{it} = \alpha_i + \lambda_t + \underbrace{\tau}_{\text{ATT}}(\text{Treated}_i \times \text{After}_t) + \varepsilon_{it}$$

- $\alpha_i$: individual fixed effects.
- $\lambda_t$: time fixed effects.
- $\text{After}_t$: a time dummy of before vs. after treatment.
- $\text{Treated}_i$: an individual dummy of treated vs. control.

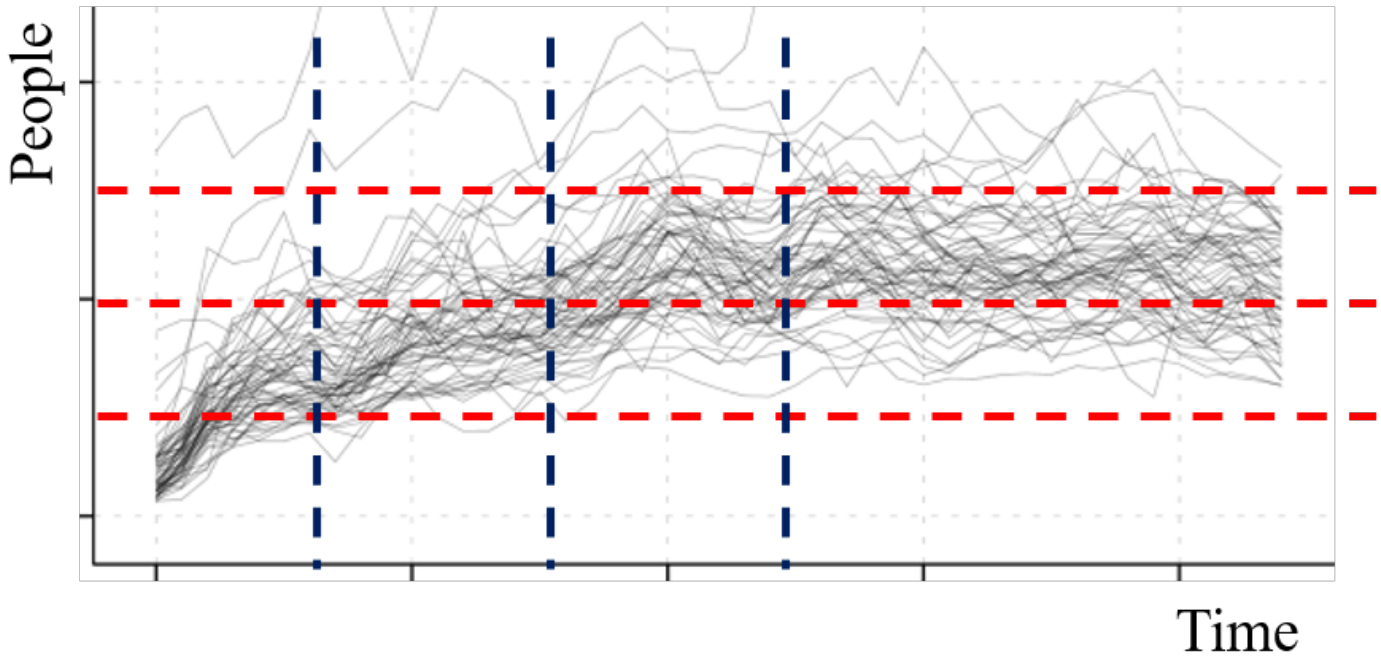
# Model-based estimation

To better understand the model:

$y_{it} = \alpha_i + \lambda_t + \tau\,(\text{Treated}_i \times \text{After}_t) + \varepsilon_{it}$

| | Before $(t = 1)$ | After $(t = 2)$ | Difference |
|---|---|---|---|
| Control $(i = 1)$ | $\alpha_1 + \lambda_1 + \varepsilon_{11}$ | $\alpha_1 + \lambda_2 + \varepsilon_{12}$ | $E(\Delta Y_C) = \lambda_2 - \lambda_1$ |
| Treated $(i = 2)$ | $\alpha_2 + \lambda_1 + \varepsilon_{21}$ | $\alpha_2 + \lambda_2 + \tau + \varepsilon_{22}$ | $E(\Delta Y_T) = \tau + \lambda_2 - \lambda_1$ |
| DID | | | $E(\Delta Y_T) - E(\Delta_C) = \tau$ |

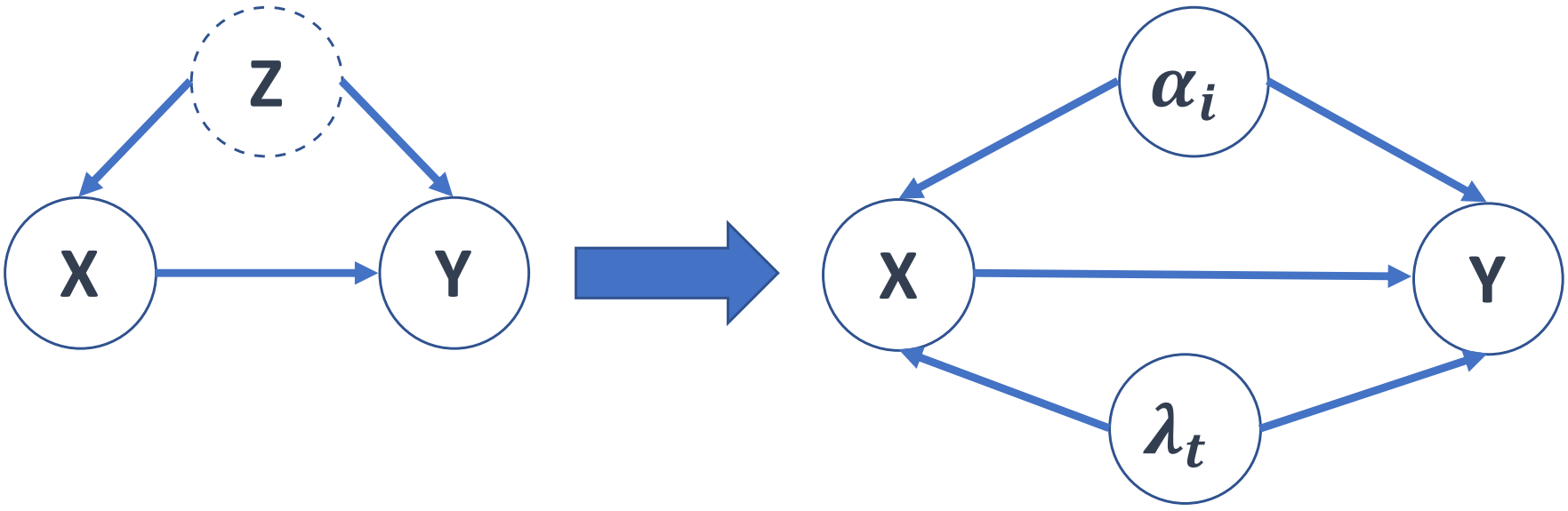
# Model-based estimation: a link to data




With the two dimensional data (time-person), we can "control for"

1. Individual fixed effects ($\alpha_i$) from repeated time periods;
2. Time fixed effects ($\lambda_t$) from multiple people.

# Another look at the identification of DID




- DID approach is by no means safe-proof!
- Threat-to-identification: **confounders that vary across individuals and time**.

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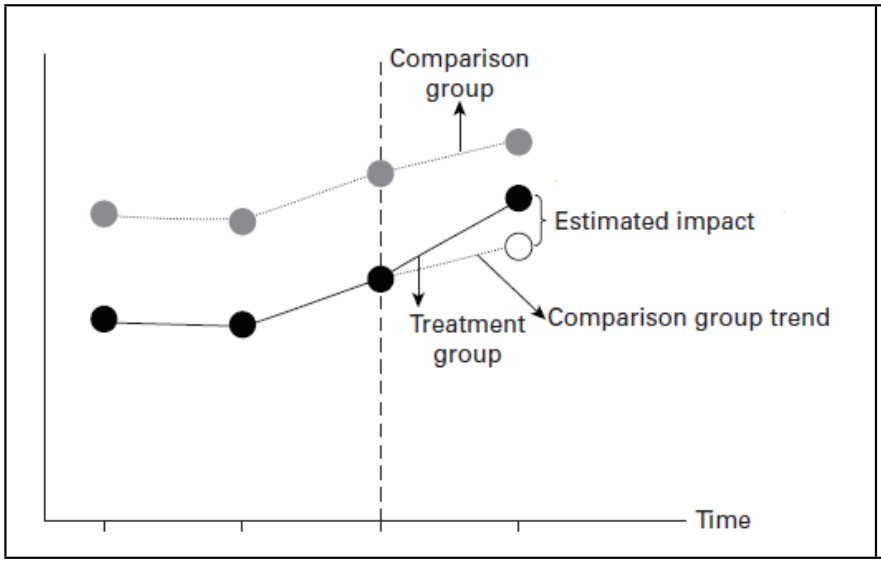
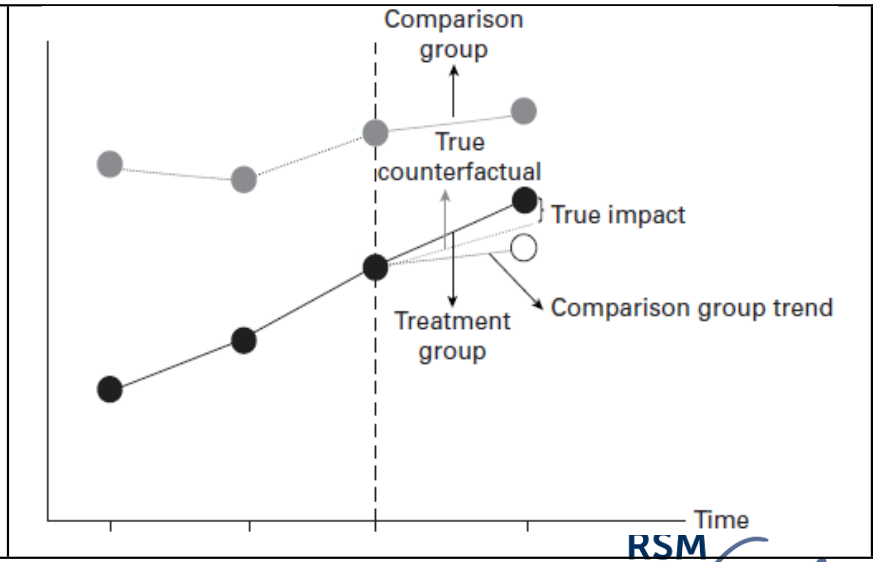
# How to examine the parallel trend assumption?

**The parallel trend assumption is untestable!** What to do then?

- It has testable implications: **the pre-treatment trends should be parallel**.
- Therefore, we can test the pre-treatment trends.




Parallel Pre-trends

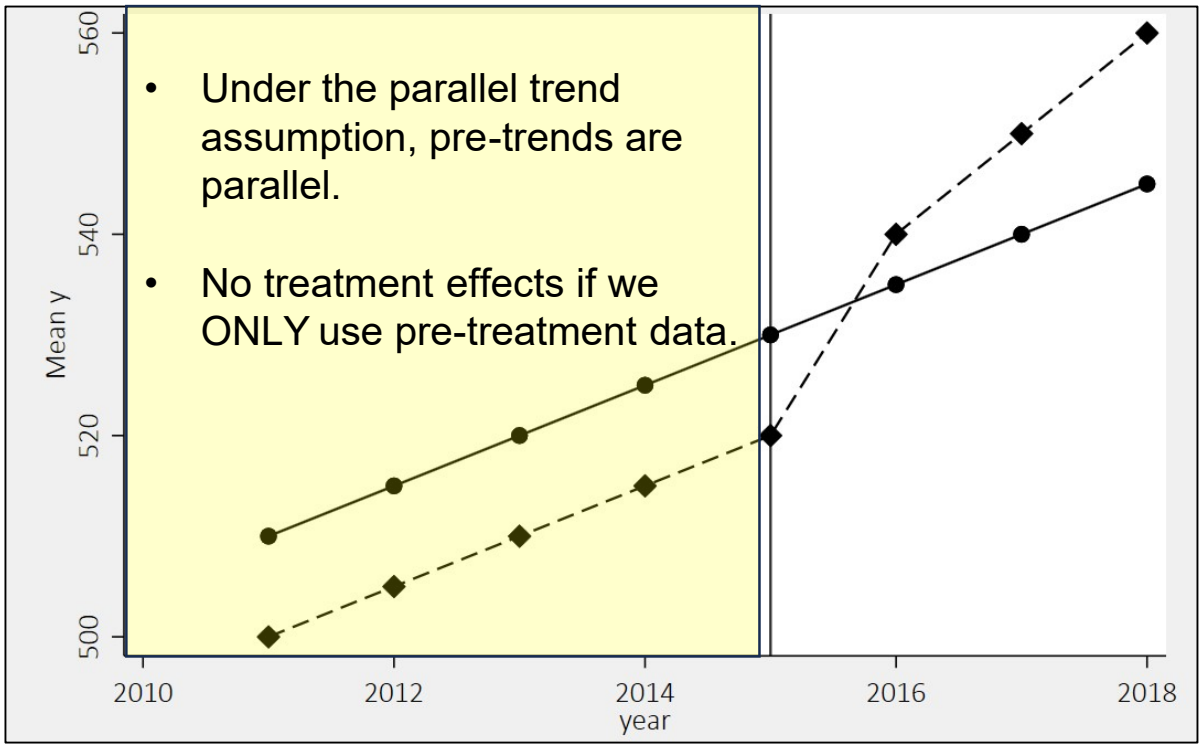



Non-parallel Pre-trends

# Placebo tests in DID

## Definition (Placebo Tests)

A pseudo-treatment (like a placebo in a drug trial) should not or cannot have an effect. Therefore, analyses with the pseudo-treatment should yield null results.

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# Placebo tests in DID

## The procedure of placebo tests in DID

1. Obtain the pre-treatment data.
2. Construct a pseudo treatment $\widetilde{\text{After}}_t$ by assuming the treatment happens at $t$ in the pre-data.
3. Run a two-way fixed effects regression with $\widetilde{\text{After}}_t$ on pre-data.

$$y_{it} = \alpha_i + \lambda_t + \widetilde{\tau}\left(\text{Treated}_i \times \widetilde{\text{After}}_t\right) + \varepsilon_{it}$$

4. Repeat for different times $t$ and collect the pseudo-ATT $\widetilde{\tau}$.

**Under the parallel trend, $\widetilde{\tau}$ should be statistically 0.**

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# Event study graphs

Another way is to look at the dynamics of treatment effects using **event study graphs**.

## Event Study Graphs

- Estimate a two-way fixed effects regression with $q$ leads, $m$ lags and the original treatment timing $\text{After}_t$.

$$y_{it} = \alpha_i + \lambda_t + \sum_{s=-q}^{m} \widetilde{\tau}_s \left( \text{Treated}_i \times \left( \underbrace{L^{-s}}_{\text{Lag}} \widetilde{\text{After}}_t \right) \right) + \varepsilon_{it}$$

- Plot the coefficients $\widetilde{\tau}_s$ and check the patterns.

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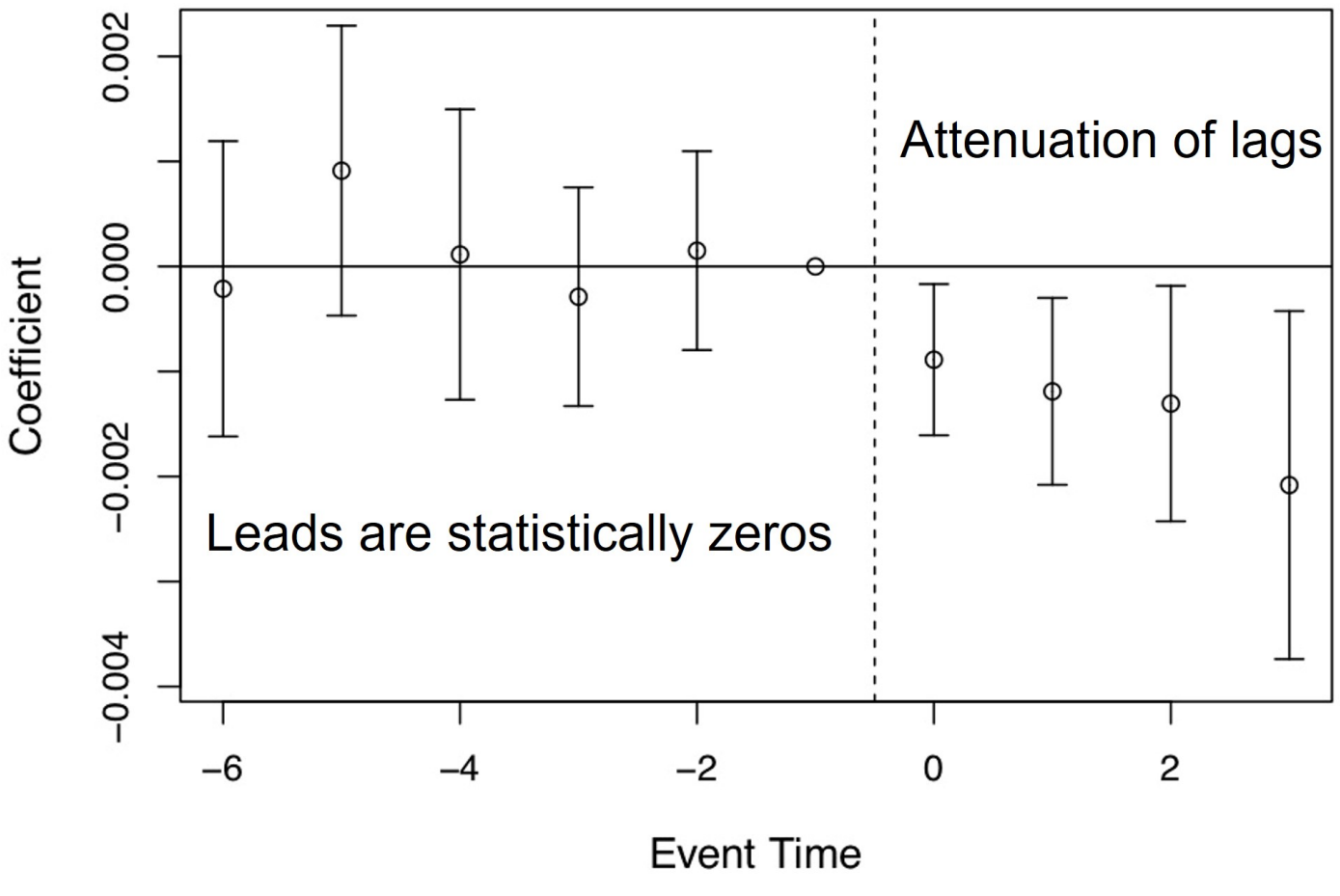
# Event study graphs




The plot is accredited to Miller et al. (2021).

# Sensitivity analysis

**Decomposing the estimated ATT:**

$$\begin{aligned}\hat{\tau}_{\mathbf{ATT}} &= E\left(Y_{i2}^1 - Y_{i1}^0 \mid D_i = 1\right) - E\left(Y_{i2}^0 - Y_{i1}^0 \mid D_i = 1\right) \\ &= \underbrace{E\left(Y_{i2}^1 - Y_{i2}^0 \mid D_i = 1\right)}_{\mathbf{True}\tau_{\mathbf{ATT}}} + \\ &\underbrace{\left[E\left(Y_{i2}^0 - Y_{i1}^0 \mid D_i = 1\right) - E\left(Y_{i2}^0 - Y_{i1}^0 \mid D_i = 0\right)\right]}_{\mathbf{Difference\ in\ Trends}}\end{aligned}$$

From this, we have the estimation bias is:

$$\hat{\tau}_{\text{ATT}} - \tau_{\text{ATT}} = \underbrace{E\left(Y_{i2}^0 - Y_{i1}^0 \mid D_i = 1\right) - E\left(Y_{i2}^0 - Y_{i1}^0 \mid D_i = 0\right)}_{\text{Difference in Trends}}$$

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- Under parallel trend or $\delta_1 = 0$, we have no bias.

- Now assume the parallel trend is violated or $\delta_1 \neq 0$.
- **Question: what extend of the violation ($\delta_1$) would nullify the estimated ATT?**
  - A larger value of $\delta_1$ implies a more credible $\hat{\tau}_{\text{ATT}}$.

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**Problem**: how to set the values of $\delta_1$?

It seems fairly random to find proper values of $\delta_1 \in (-\infty, +\infty)$.
**How to find a good anchor for the values of $\delta_1$?**

**Solution**: using pre-trends as anchors

Assuming $\delta_1$ is X times the values of the difference in pre-trends.
X is interpretable and applicable to different studies.

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# Sensitivity analysis

Consider a DID with 3 time periods with $t = 0, 1, 2$, and the treatment occurs in $t = 2$.

1. The difference in pre-trends $\delta_0$ is:

$$\delta_0 = E\left(Y_{i1}^0 - Y_{i0}^0 \mid D_i = 1\right) - E\left(Y_{i1}^0 - Y_{i0}^0 \mid D_i = 0\right)$$

2. Assume the difference in post-trends $\delta_1$ is bounded as:

$$|\delta_1| < M\,|\delta_0|$$

3. We can vary the value of $M$ and get a critical value $M^*$ that nullifies the ATT.

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# What if pre-trends are not parallel?

You need extra assumptions / information for a more credible identification.

- **Scenario 1**: With pre-treatment characteristics $X_i$.
- **Scenario 2**: With outcomes $Y_{it}$ of many pre-treat periods.

**Parallel pre-trends**

Parallel pre-trends are **neither necessary or sufficient** for the parallel trend assumption!

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# Scenario 1: With pre-treatment variables $X_i$

## Conditional Parallel Trends Assumption

Conditioning on $X_i$, we have the following:

$$E\left[Y_{i2}(0) - Y_{i1}(0) \mid D_i = 1, X_i\right] = E\left[Y_{i2}(0) - Y_{i1}(0) \mid D_i = 0, X_i\right]$$

## Identification results

Given the assumption, we can identify and conditional ATT:

$$\tau_{\text{ATT}}(x) = \underbrace{E\left[Y_{i2} - Y_{i1} \mid D_i = 1, X_i = x\right]}_{\text{Changes for the treated at } x} - \underbrace{E\left[Y_{i2} - Y_{i1} \mid D_i = 0, X_i = x\right]}_{\text{Changes for the control at } x}$$

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# Scenario 1: With pre-treatment variables $X_i$

**Matching** (see Heckman et al. 1997):

$$\hat{\tau}_{\text{ATT}} = \frac{1}{N_1} \sum_{i \in \text{Treated}} \Big( (Y_{i2} - Y_{i1}) - \hat{E}_X \left[ Y_{i2} - Y_{i1} \mid D_i = 0, X_i \right] \Big)$$

- $\hat{E}_X \left[ Y_{i2} - Y_{i1} \mid D_i = 0, X_i \right]$ is the estimated expectation function fitted on the control units.
- It takes in values of $X_i$ and predicts the counterfactual changes at $X_i$ for a treated unit.
- In theory, any function form can be use, e.g., a polynomial function.

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# Scenario 1: With pre-treatment variables $X_i$

**Weighting** (see Abadie 2005):

$$\hat{\tau}_{\text{ATT}} = \frac{1}{N_1} \sum_{i \in \text{Treated}} (Y_{i2} - Y_{i1}) - \frac{1}{N_0} \sum_{i \in \text{Control}} \left( \frac{\hat{e}(X_i)}{1 - \hat{e}(X_i)} \right) (Y_{i2} - Y_{i1})$$

- $\hat{e}(X_i)$ is the estimated propensity score function.
- We first estimate $\hat{e}(X_i)$ and then plug it in.

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# Scenario 1: With pre-treatment variables $X_i$

With multiple time periods, we can run a two-way fixed effects regression with $X_i$ as controls:

$$y_{it} = \alpha_i + \lambda_t + \tau\left(\text{Treated}_i \cdot \text{After}_t\right) + \beta\left(X_i \cdot \text{After}_t\right) + \varepsilon_{it}$$

- Note that this specification may not be consistent – no treatment heterogeneity.
- The coefficient $\beta$ is the same across different levels of $X_i$.
- With too may $X_i$, we can use sub-classification based on $X_i$.

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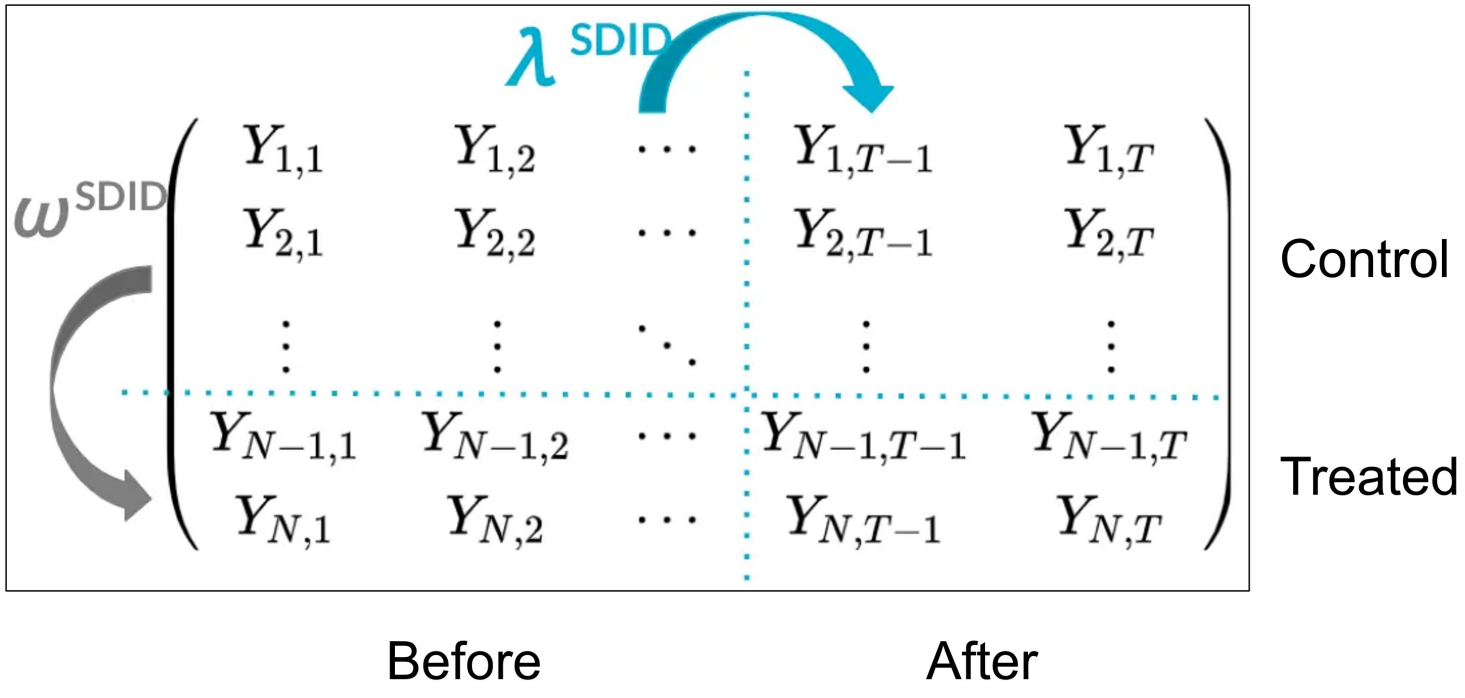
# Scenario 2: With many pre-treatment periods

In weighting approach, we weight the control units for ATT ($\omega^{\text{SDID}}$).

- Inadequate if the pre-trends are non-parallel.

Arkhangelsky 2021 proposes to also weight the time periods ($\lambda^{\text{SDID}}$).

- To calculate $\lambda^{\text{SDID}}$ with long pre-treatment periods.
- The idea is similar to traditional event studies.



# Outline



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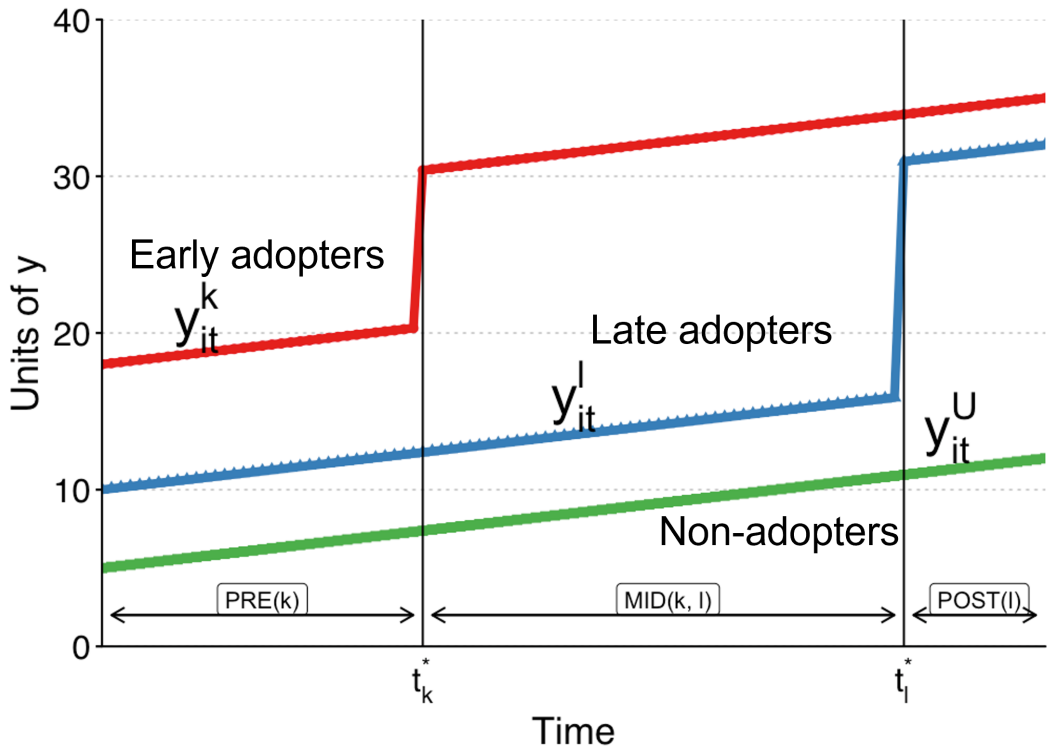
# Differential timing of the treatment

So far, we assume two things about the treatment:

1. Treatment happens at $t$ for all people.
2. Once treated, remain treated[2].

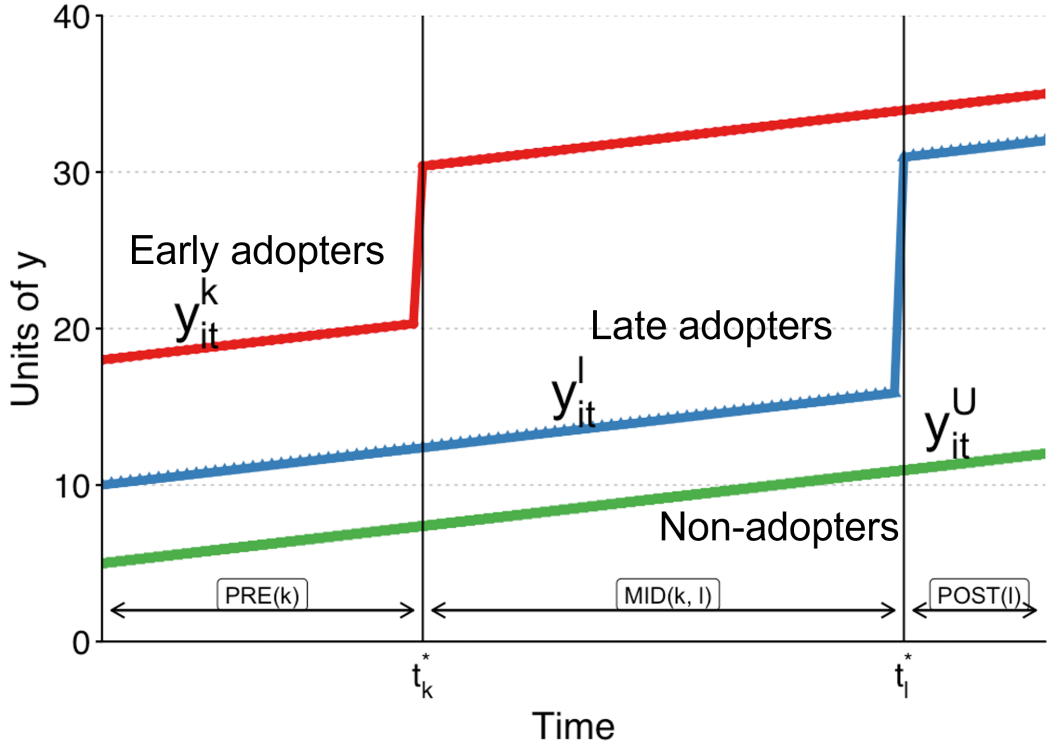



[2] In statistical term, treatment is an absorbing state.

# Differential timing of the treatment

In practice, we have "staggered adoptions:"

- Different groups receive the treatments at different times.
- E.g., roll-out strategies in marketing or policy testing.

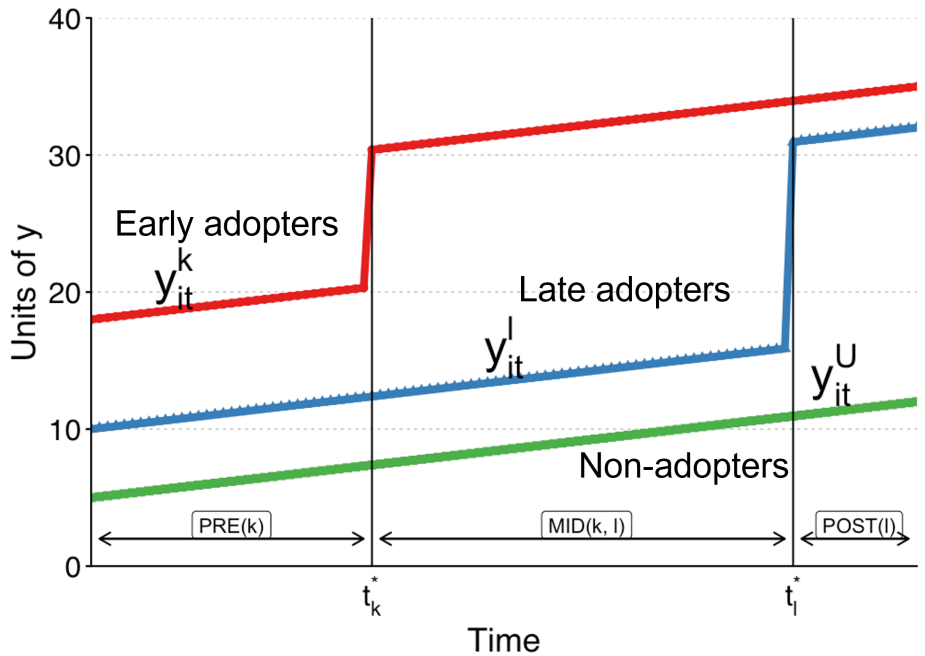


# Differential timing of the treatment

Can we still use the go-to model – two-way fixed effects regression?

$$y_{it} = \alpha_i + \lambda_t + \tau \underbrace{D_{it}}_{\text{Treatment}} + \varepsilon_{it}$$

- Instead of $(\text{Treated}_i \cdot \text{After}_t)$, we now have a variable $D_{it}$.
- The treated people receive the treatment at different times.

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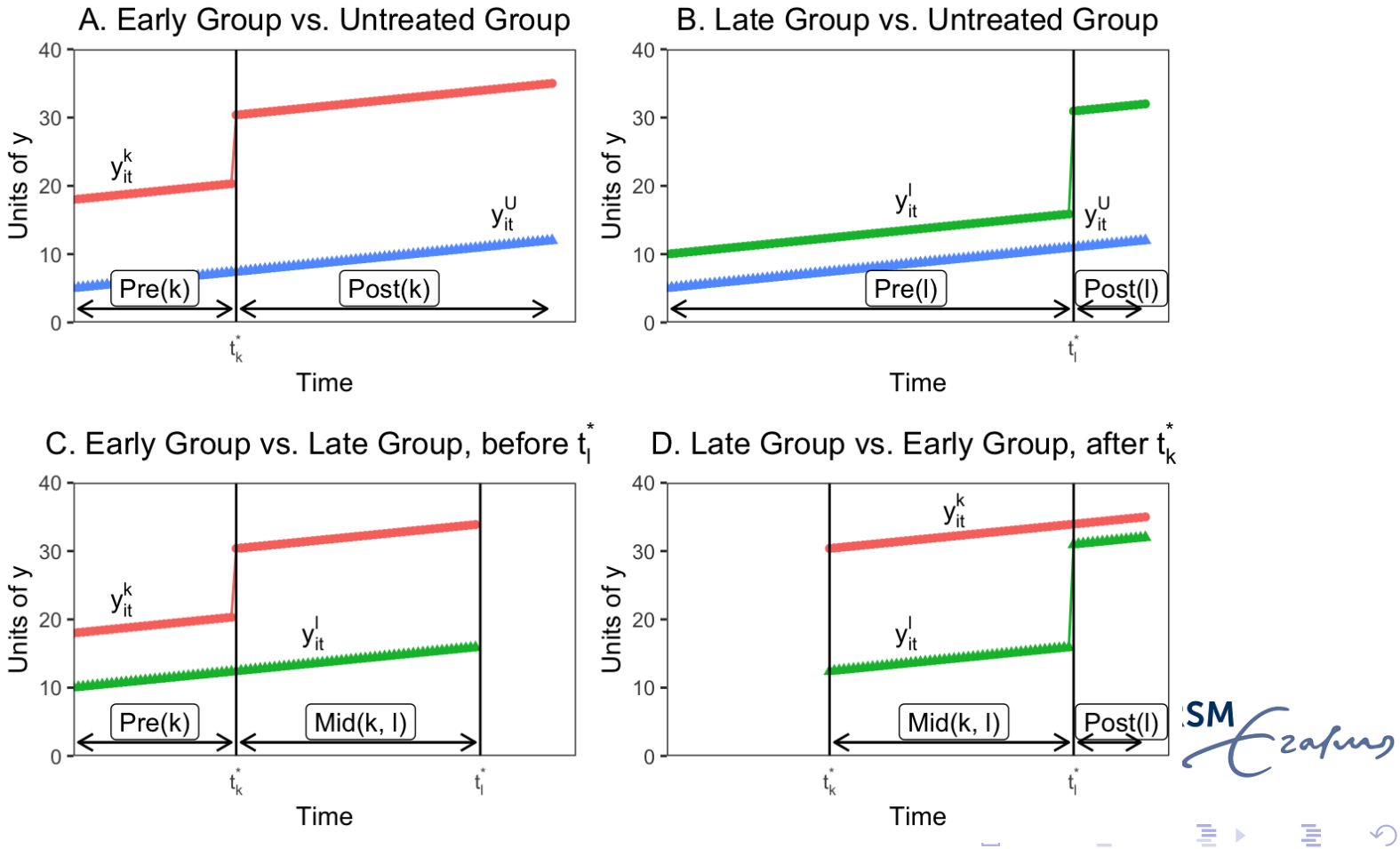
# The two-ways fixed effects regression is biased!

Goodman-Bacon (2021) formalizes this and shows a nice intuition. ATT from the regression is a weighted average of group*time comparisons.



# How to correct for the bias?

A few approaches proposed recently:

- Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. Journal of Econometrics, 225(2), 200-230.
- Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. Journal of Econometrics, 225(2), 175-199.
- Cengiz, D., Dube, A., Lindner, A., & Zipperer, B. (2019). The effect of minimum wages on low-wage jobs. The Quarterly Journal of Economics, 134(3), 1405-1454.

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