Lecture 7: Quasi-experimental design

Difference-in-Difference

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Outline

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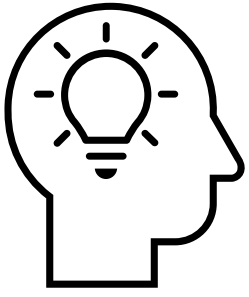
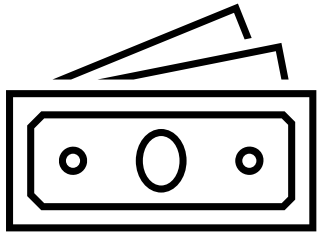
Randomized experiments are powerful, but...

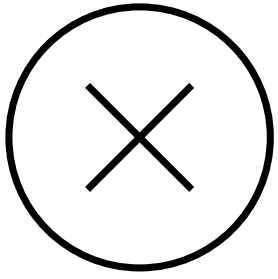
In many scenarios, we cannot do randomized experiments.



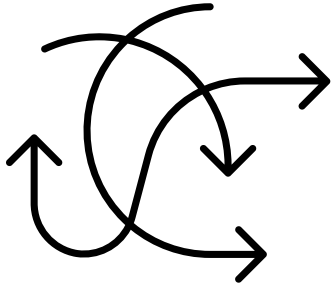
Unethical



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

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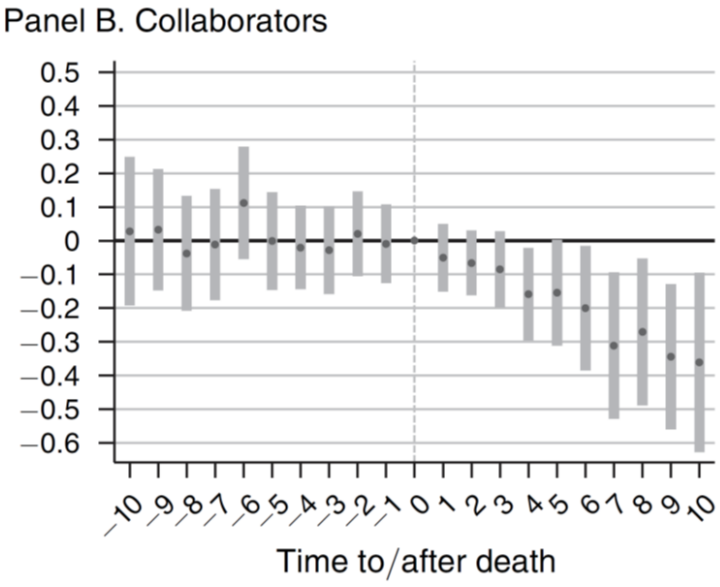
- ...

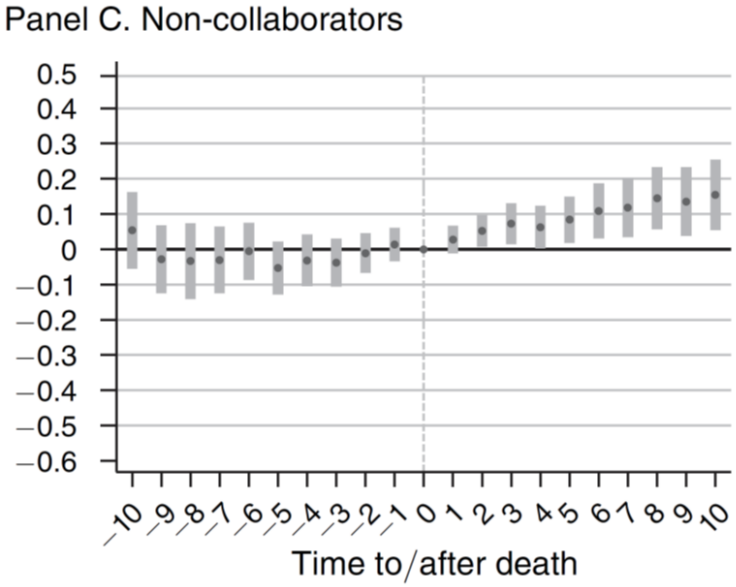
Numerous examples of quasi-experiments...

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

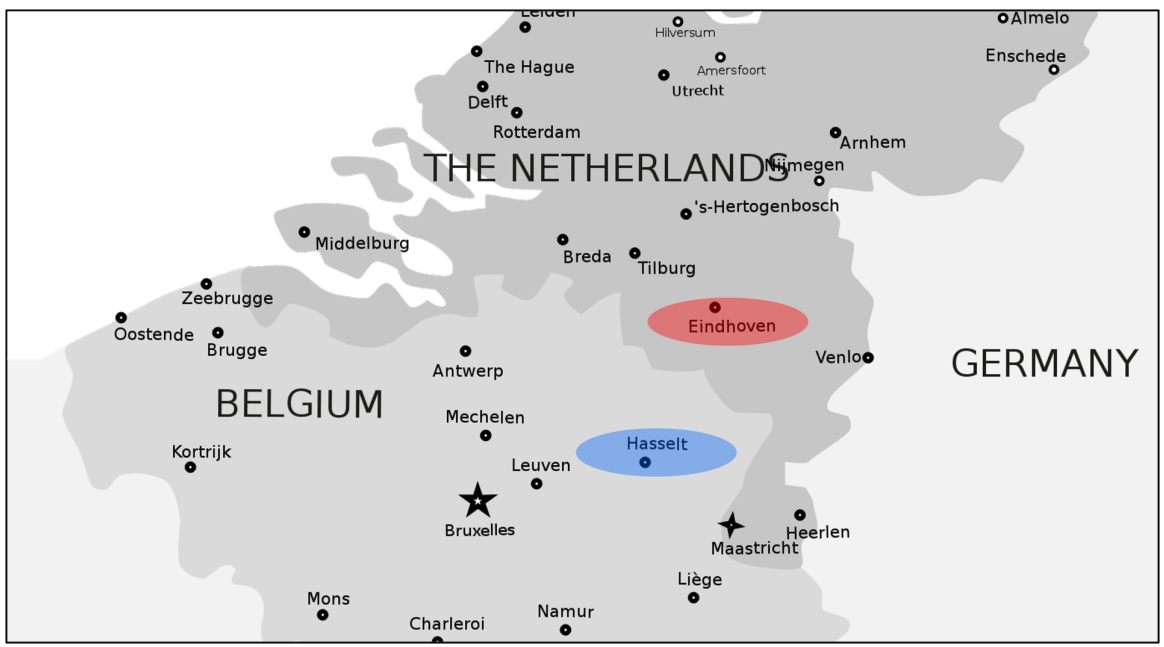


(b) Non-collaborators

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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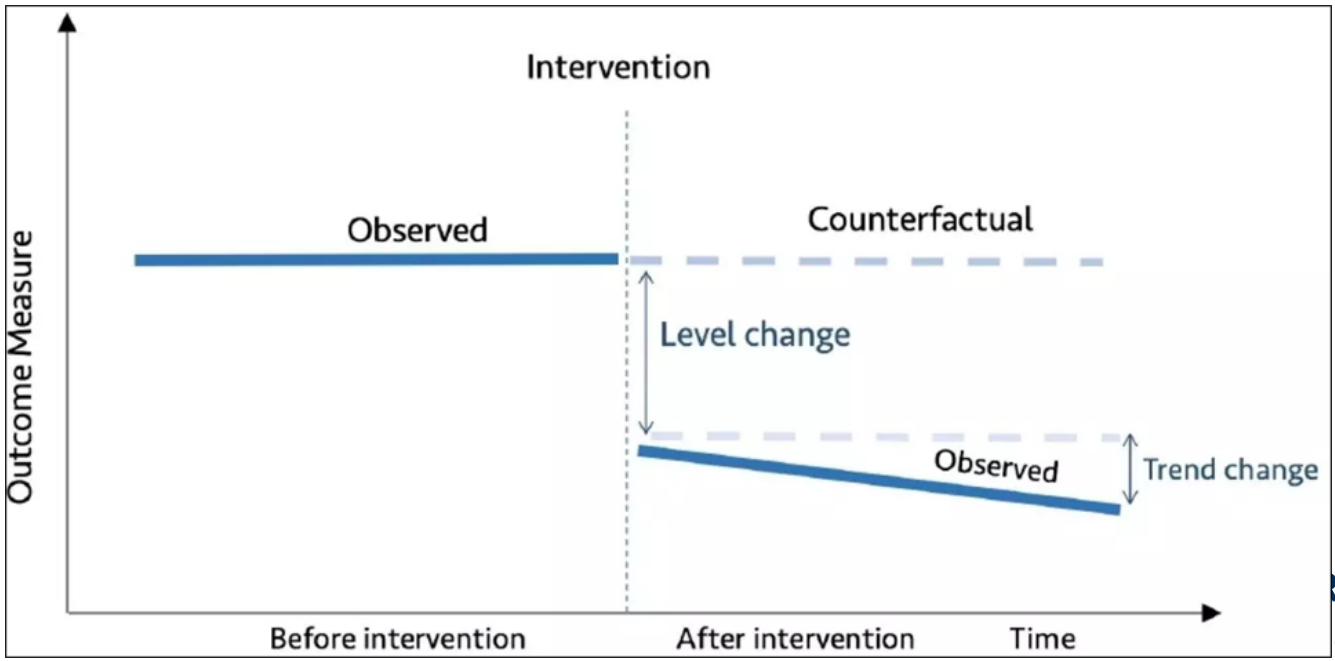
Before-after design

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

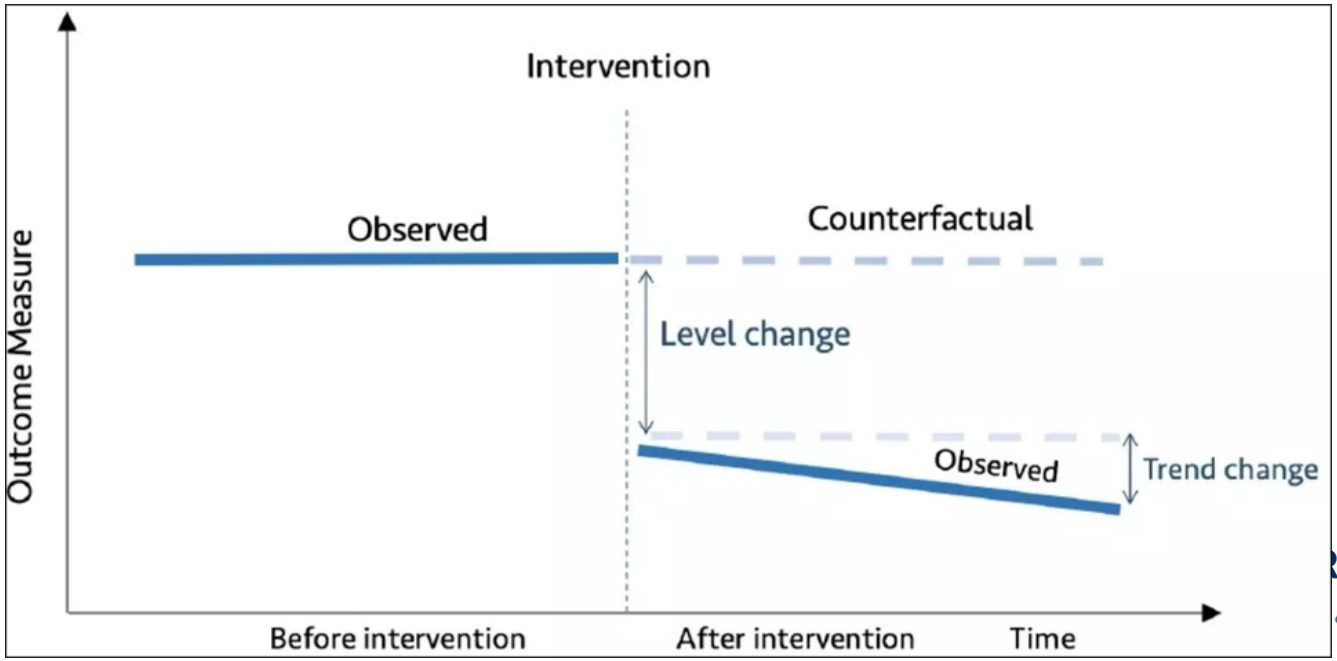
- Predict the "after" outcome based on the before data.
- Compare the "after" outcome with the predicted one.
  - **Level changes**: up and down shift.
  - **Trend changes**: changes in the trajectory.



Before-after design

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

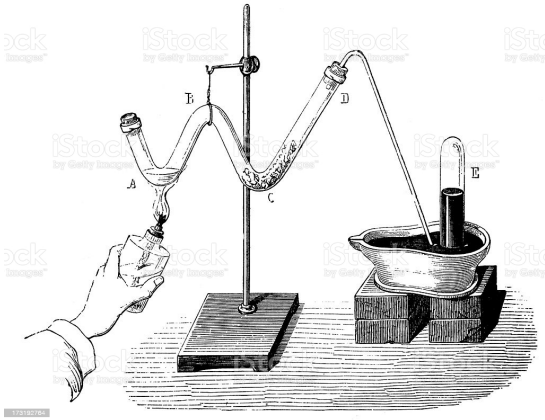
- German reunification
- The implementation of GDPR
- The Great Recession in 2008
- ...



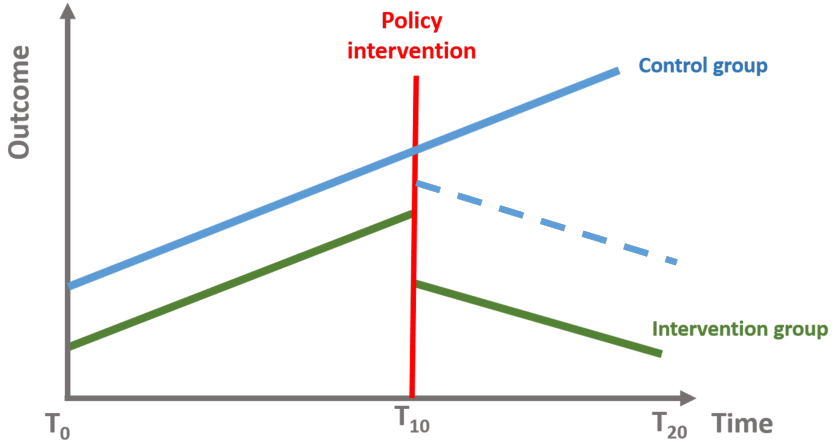
Before-after design

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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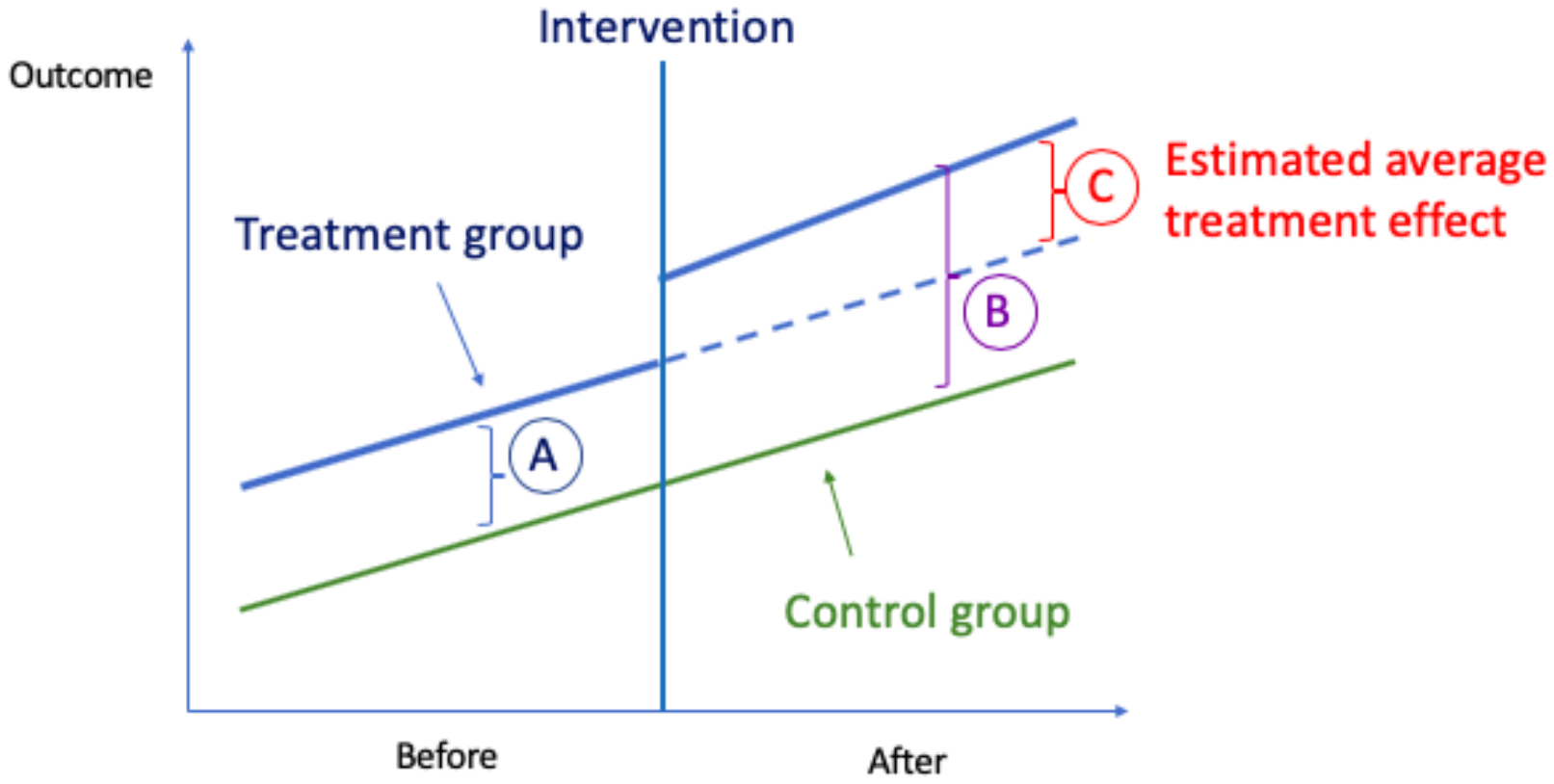
Two sources of variations



- **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 τ_{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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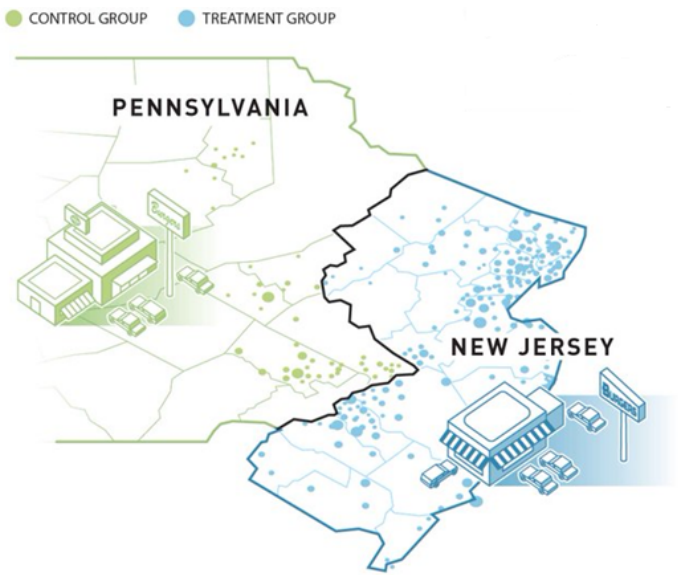
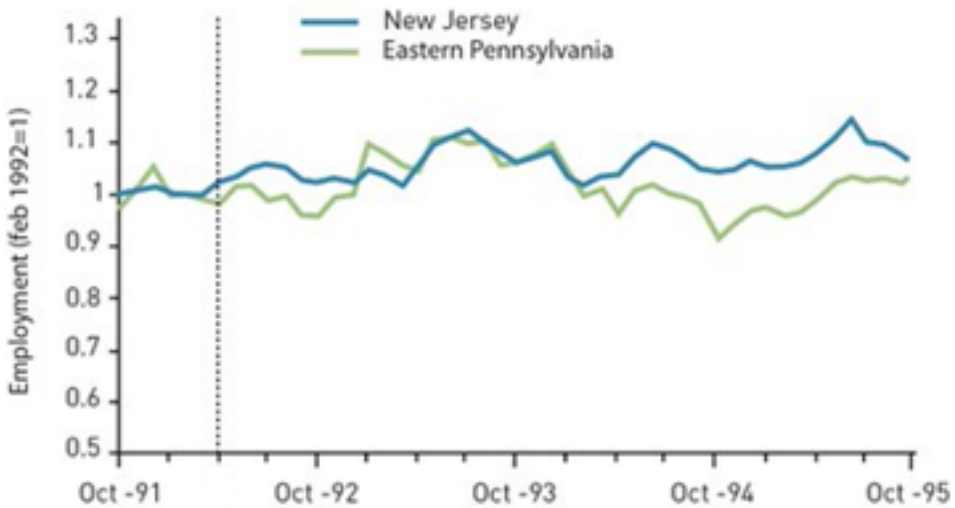
The canonical DID: examples

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 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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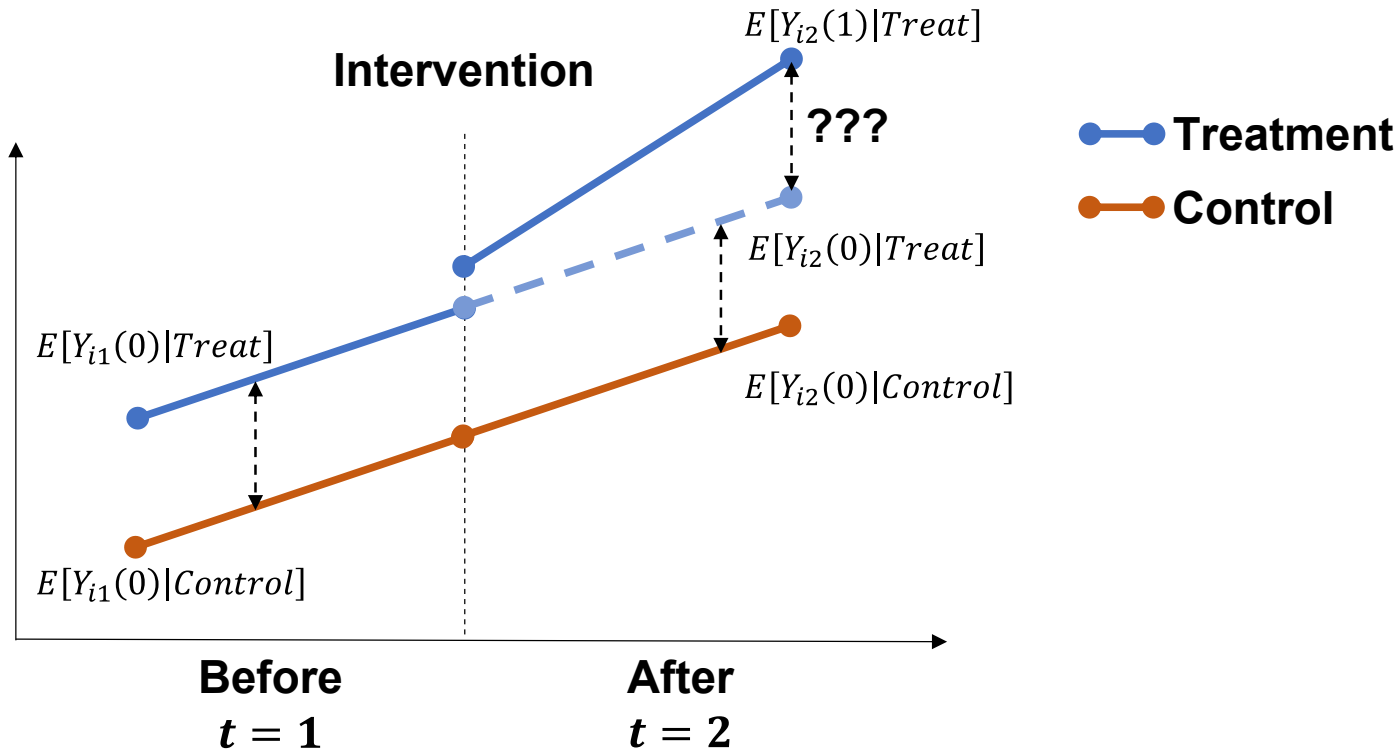
Understanding the parallel trend



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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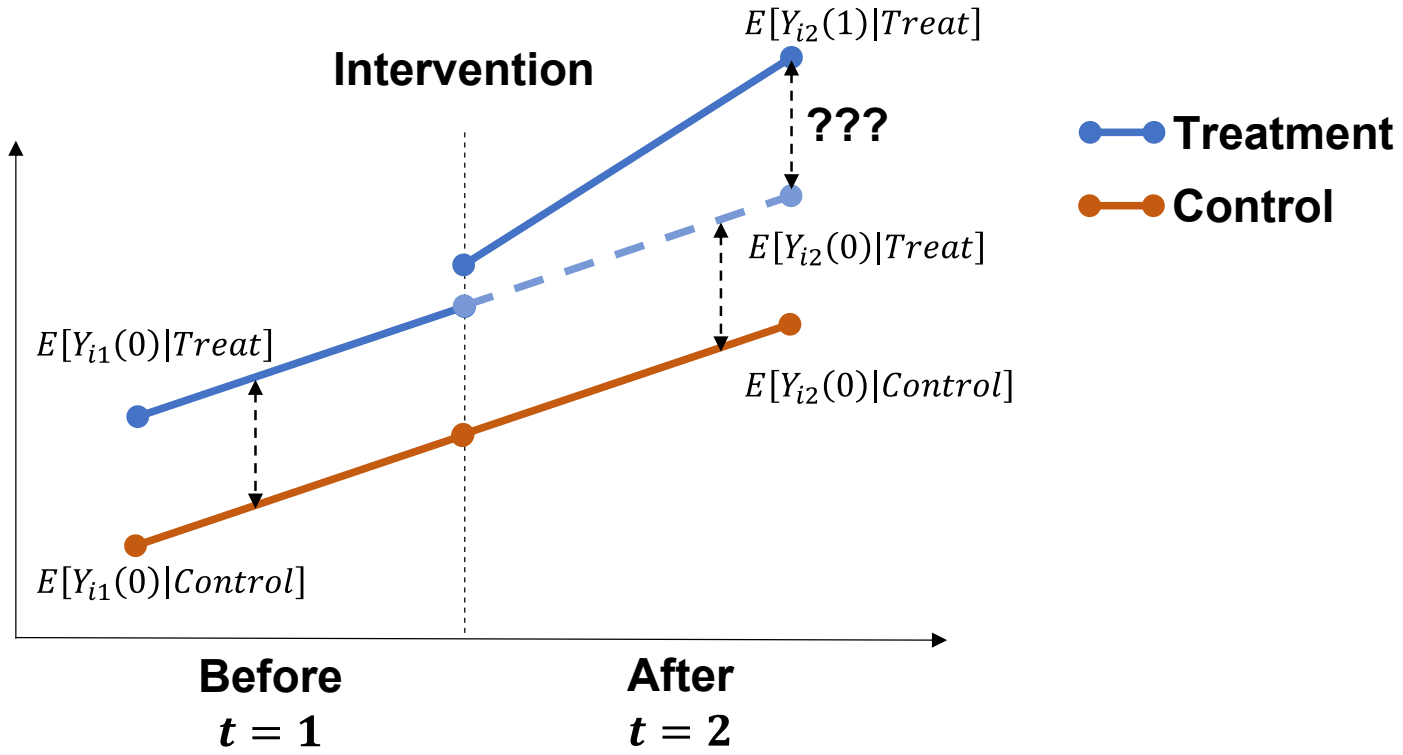
Understanding the parallel trend



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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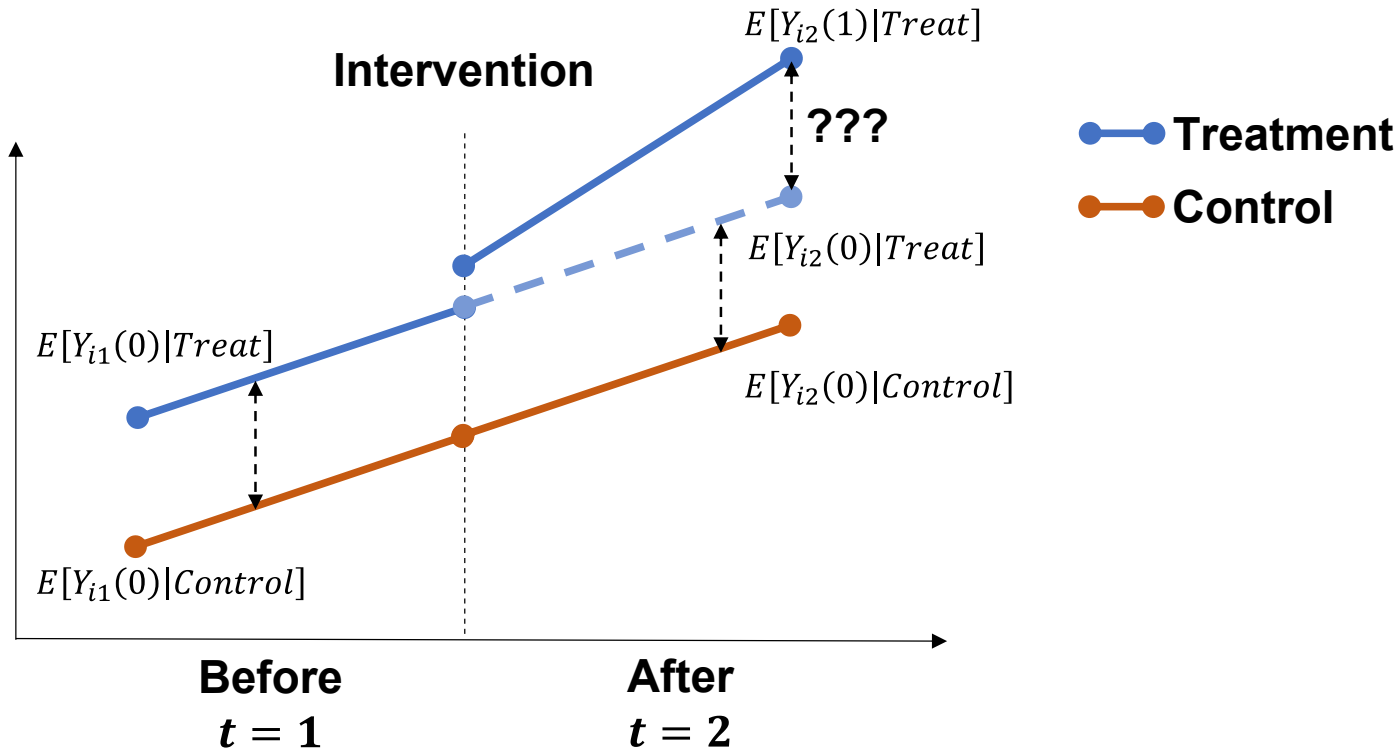
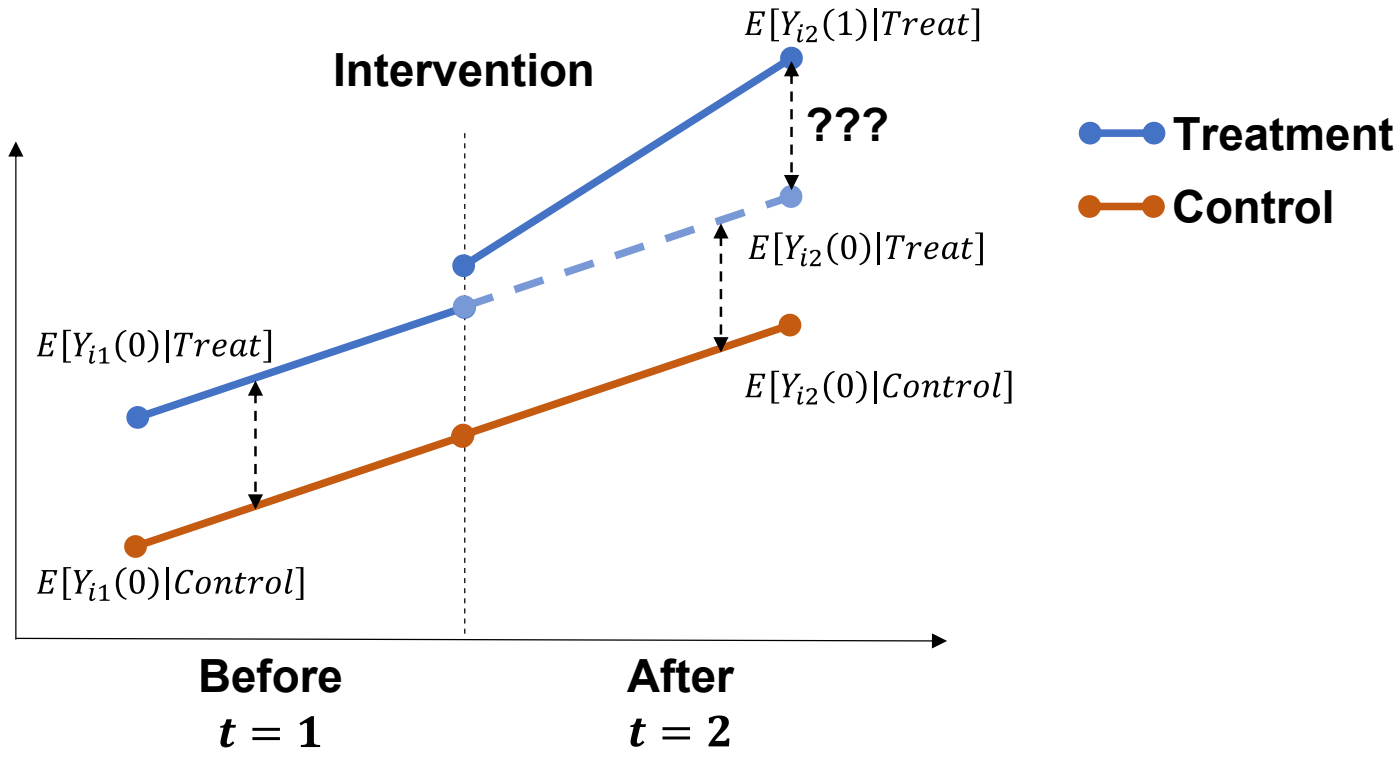
Understanding the parallel trend



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!

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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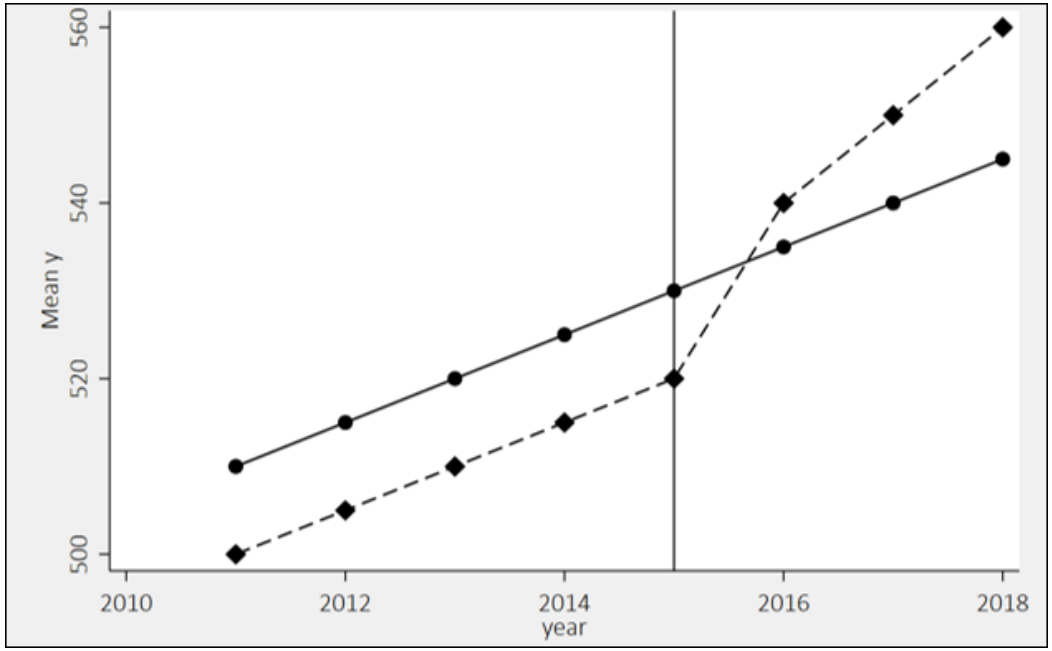
Multiple time periods

- 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:

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

- α_i: individual fixed effects.
- λ_t: time fixed effects.
- After_t: a time dummy of before vs. after treatment.
- Treated_i: an individual dummy of treated vs. control.

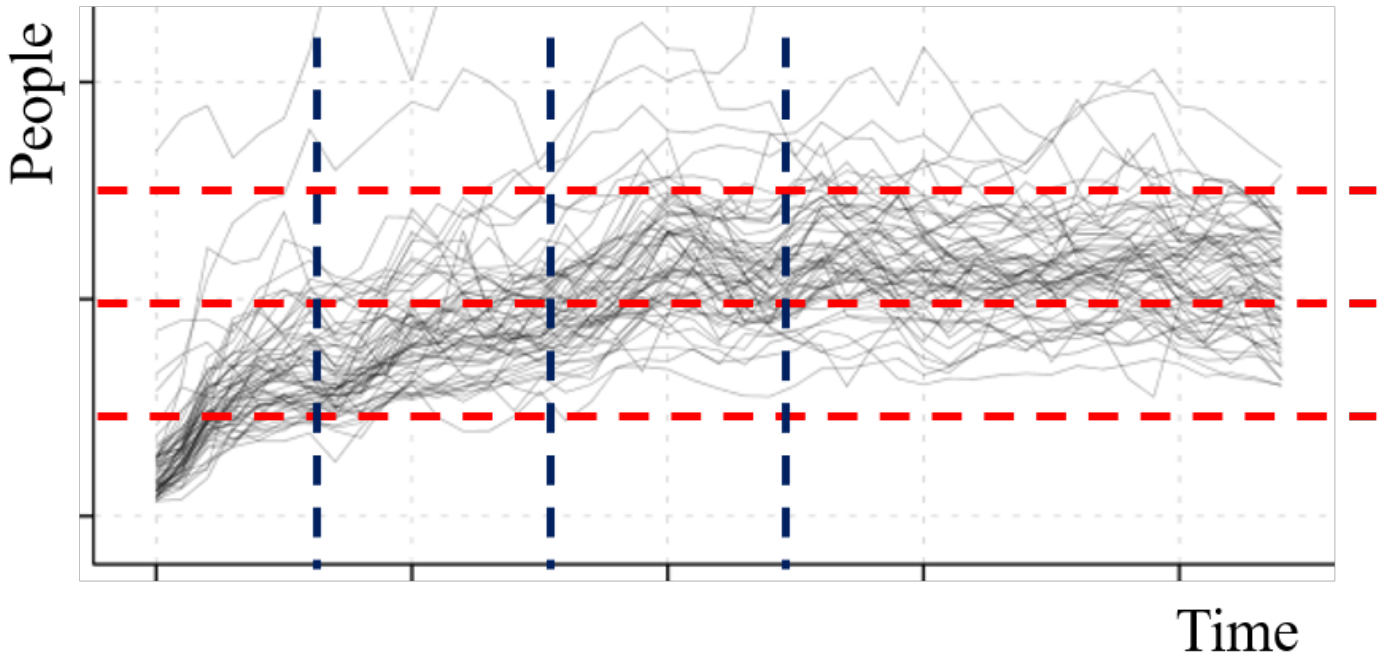
Model-based estimation

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			$E(\Delta Y_T) - E(\Delta_C) = \tau$

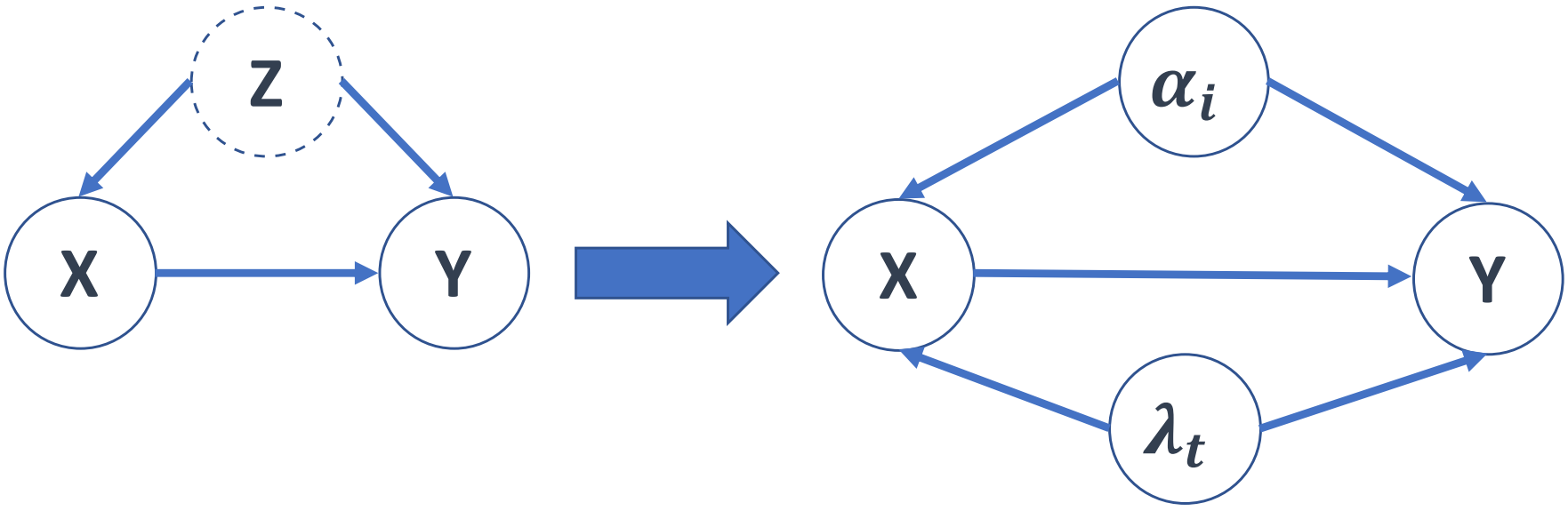
Model-based estimation: a link to data



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

1. Individual fixed effects (α_i) from repeated time periods;
2. Time fixed effects (λ_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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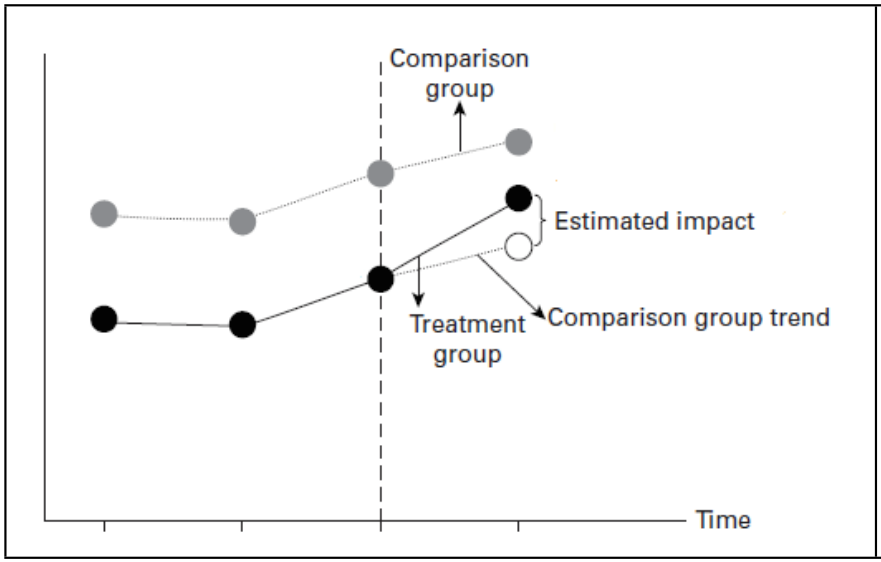
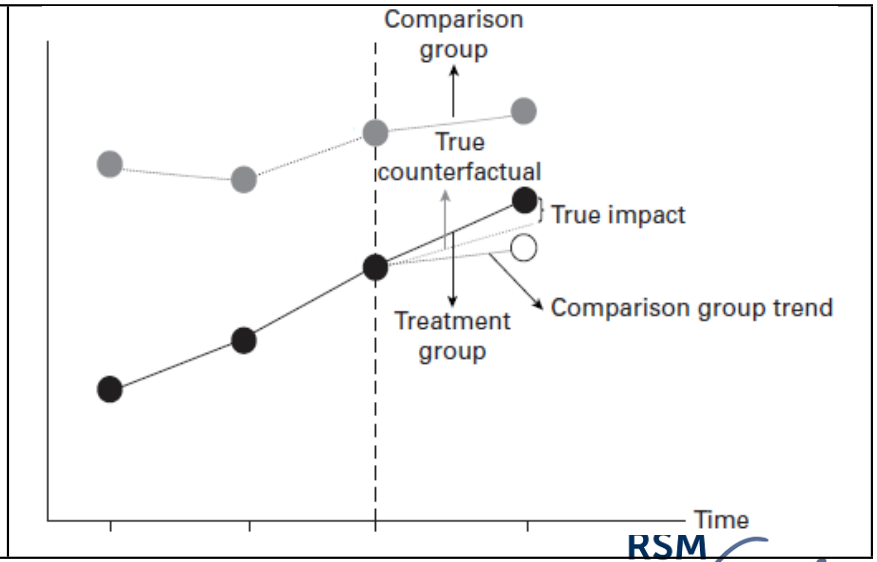
How to examine the parallel trend assumption?

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

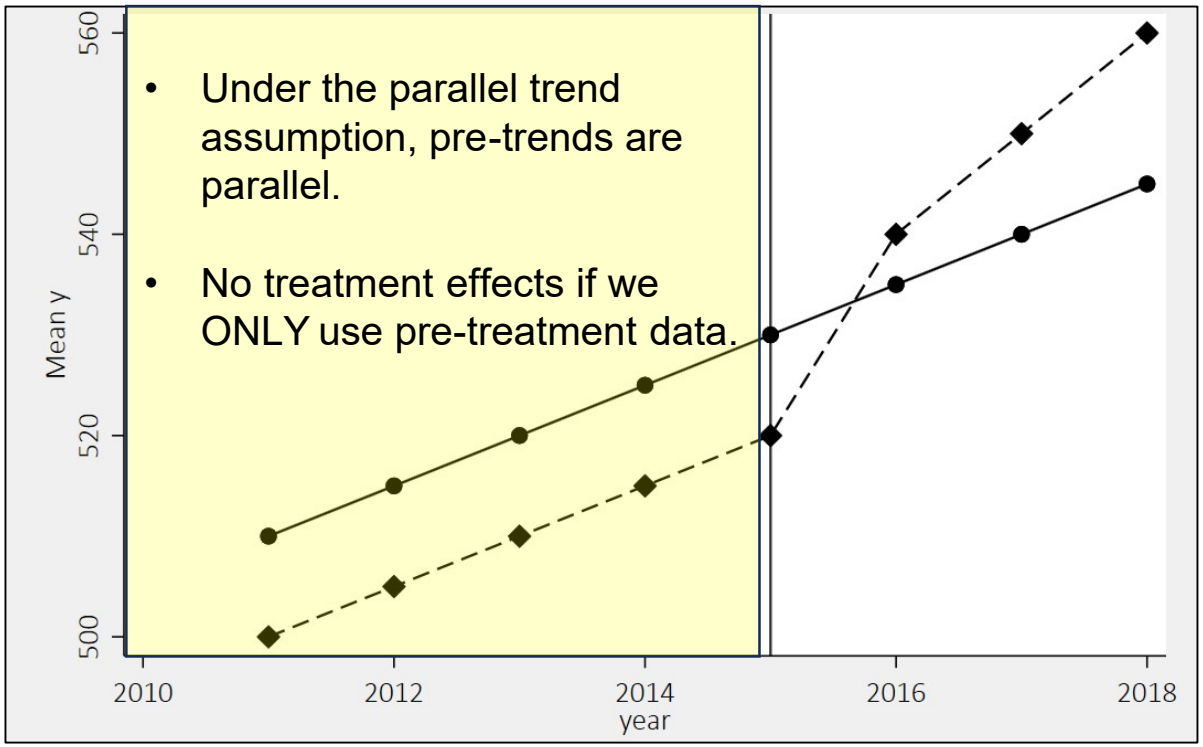


Non-parallel Pre-trends

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 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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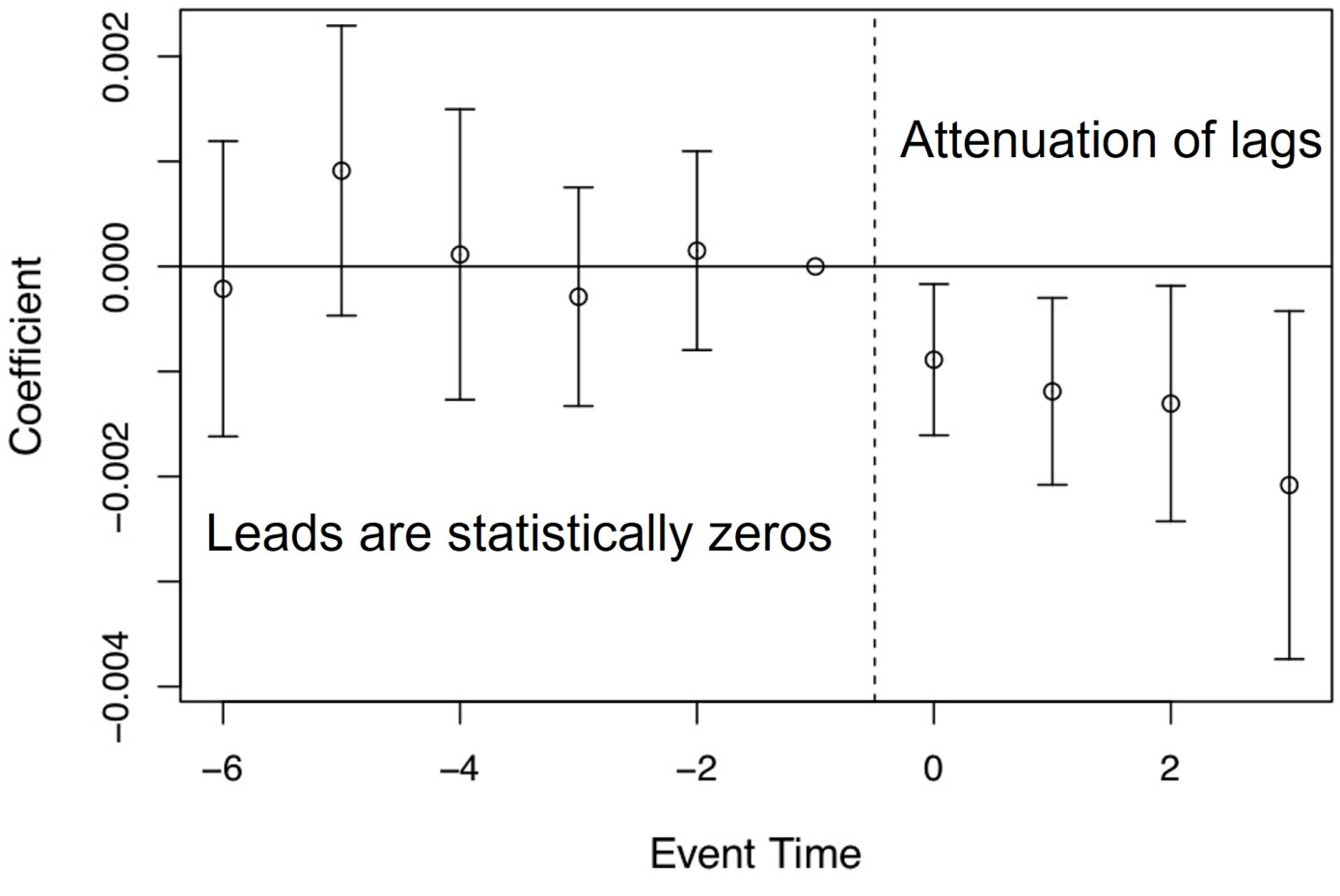
Event study graphs



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

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:

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

Scenario 1: With pre-treatment variables X_i

Conditional Parallel Trends Assumption

Conditioning on X_i, we have the following:

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

Identification results

Given the assumption, we can identify and conditional ATT:

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

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):

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

- $\hat{e}(X_i)$ 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 β 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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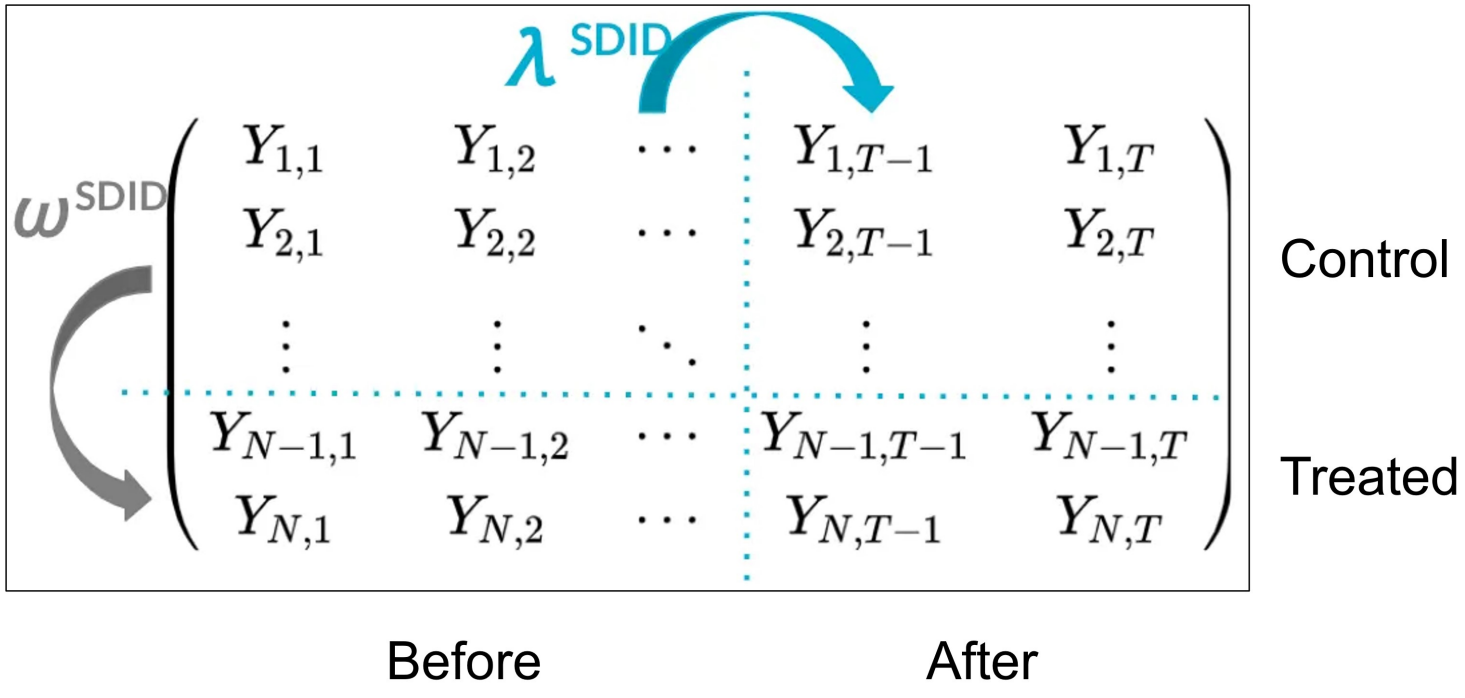
Scenario 2: With many pre-treatment periods

In weighting approach, we weight the control units for ATT (ω^{SDID}).

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

Arkhangelsky 2021 proposes to also weight the time periods (λ^{SDID}).

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



Outline

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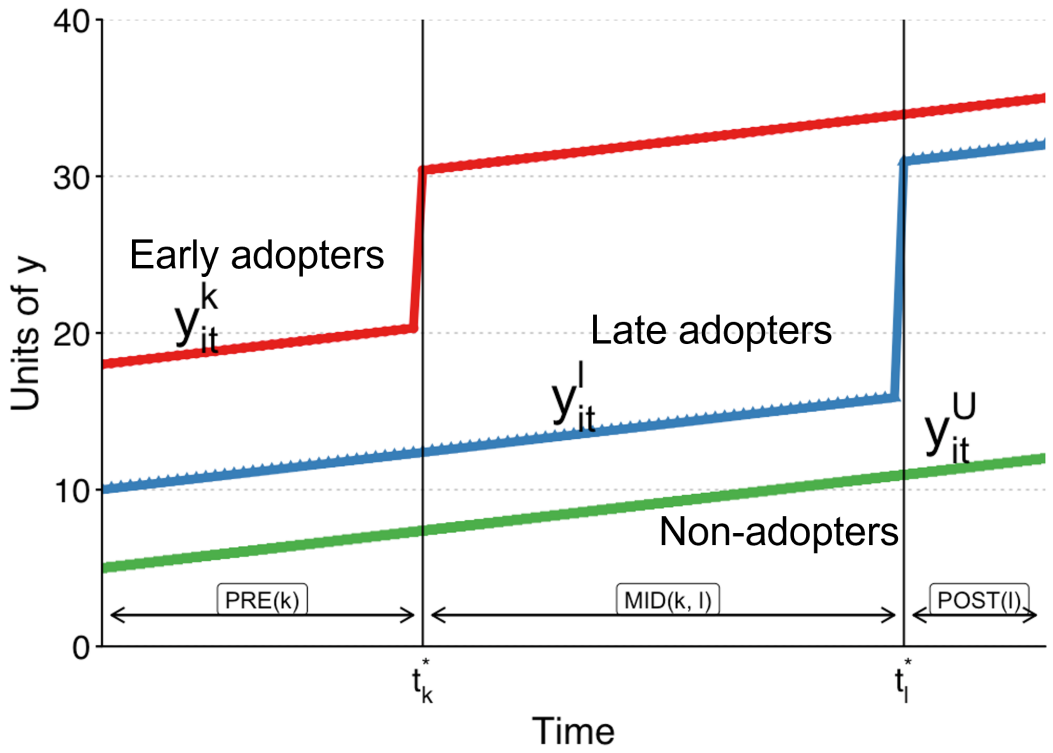
Differential timing of the treatment

So far, we assume two things about the treatment:

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

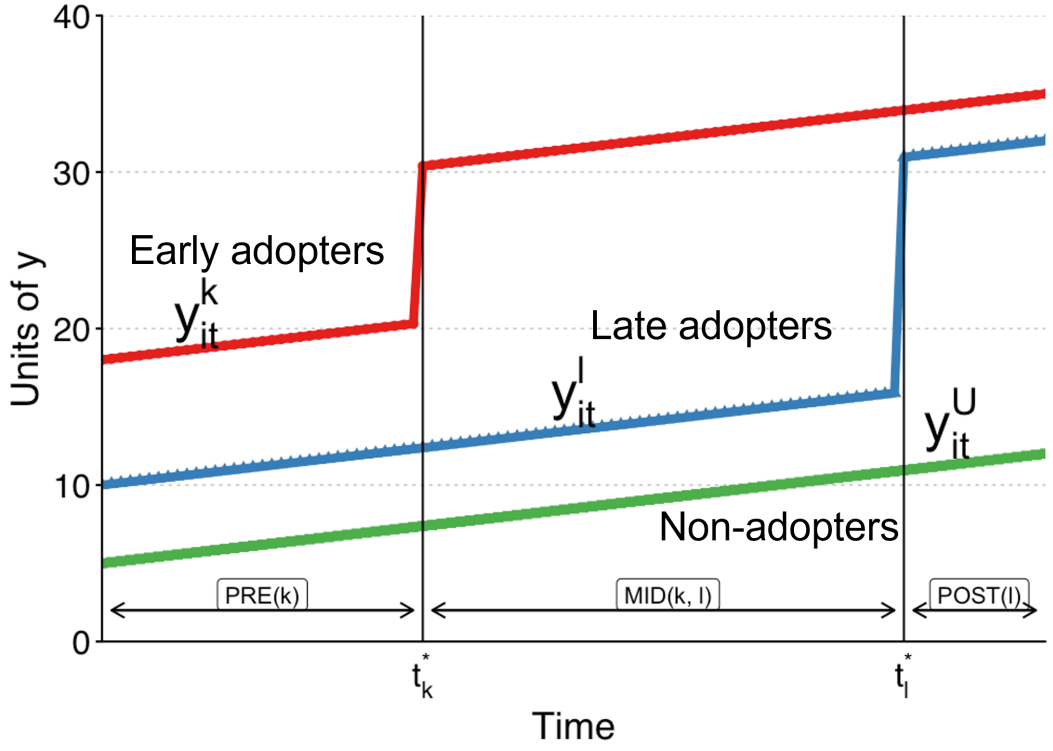


[2] In statistical term, treatment is an absorbing state.

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.

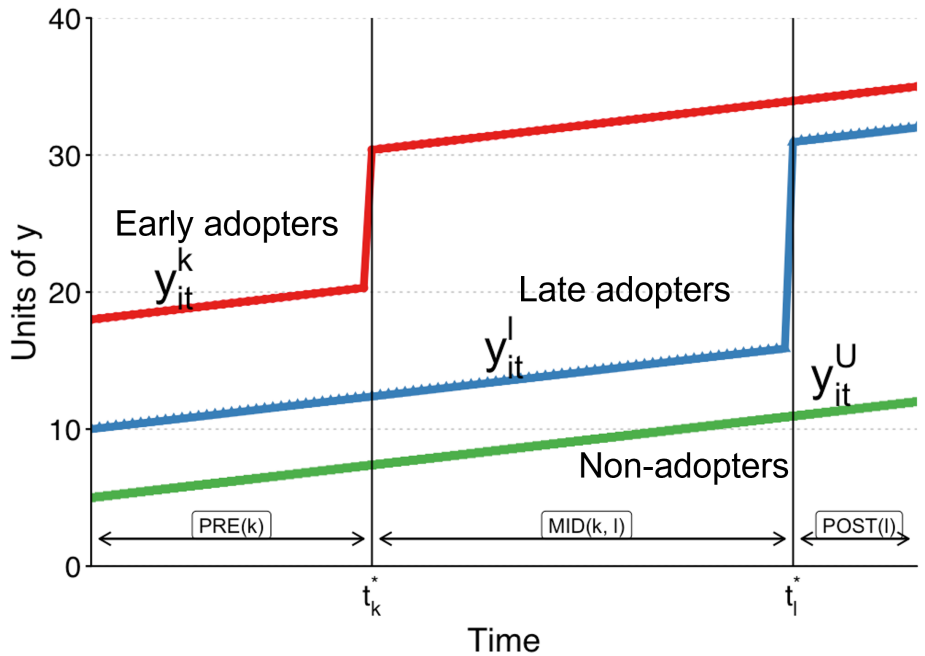


Differential timing of the treatment

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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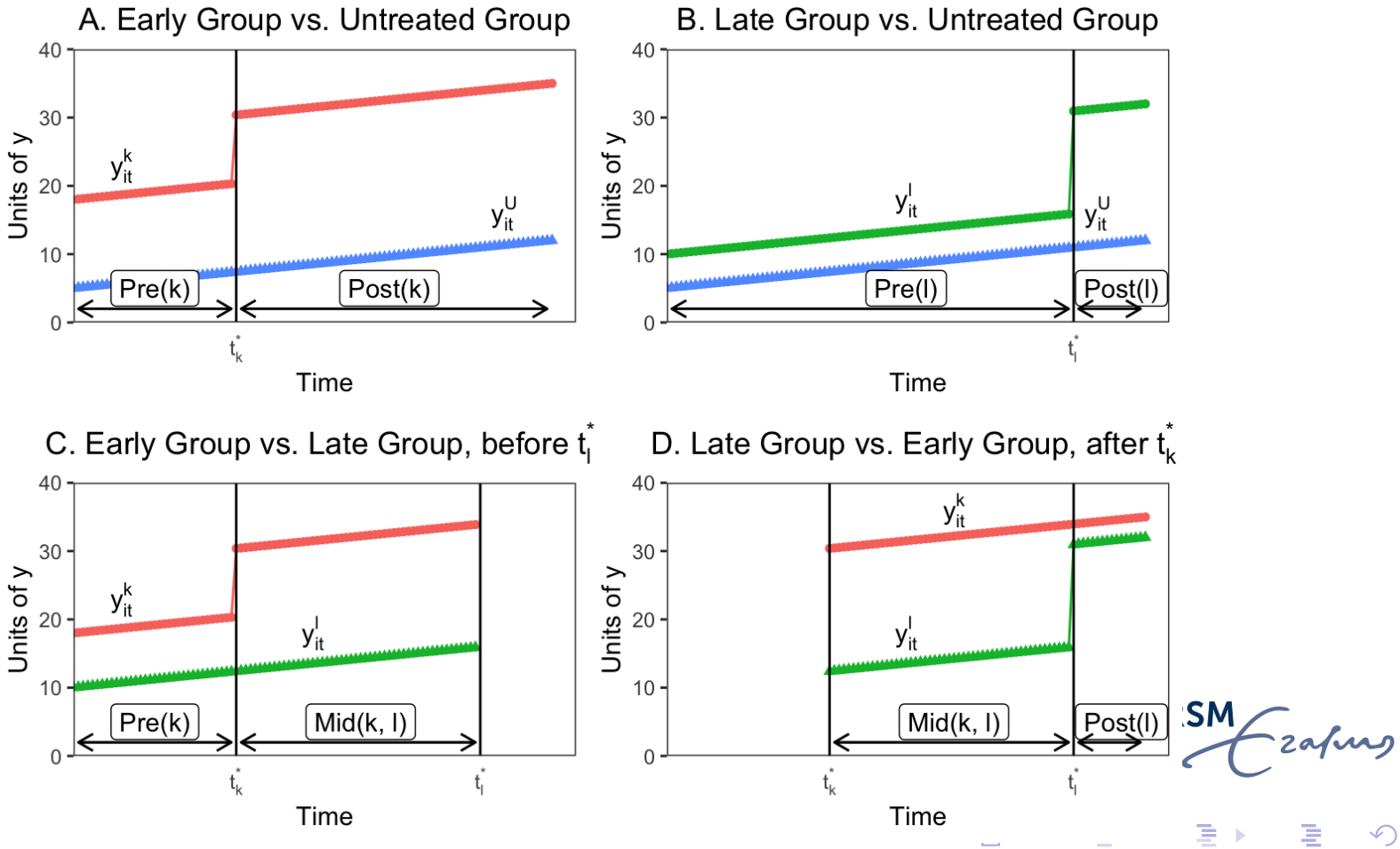
The two-ways fixed effects regression is biased!

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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- Miller, S., Johnson, N., & Wherry, L. R. (2021). Medicaid and mortality: new evidence from linked survey and administrative data. The Quarterly Journal of Economics, 136(3), 1783-1829.

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- Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., & Wager, S. (2021). Synthetic difference-in-differences. American Economic Review, 111(12), 4088-4118.
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. Journal of Econometrics, 225(2), 254-277.

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