MY457/MY557: Causal Inference for Observational and Experimental Studies

Week 7: Difference-in-Differences 1

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Course Outline

- **Week 1:** The potential outcomes framework
- **Week 2:** Randomized experiments
- **Week 3:** Estimation under selection on observables I
- **Week 4:** Estimation under selection on observables II
- **Week 5:** Estimation under selection on observables III
- *Week 6: Reading week*
- **Week 7:** Difference-in-differences I
- **Week 8:** Difference-in-differences II
- **Week 9:** Synthetic control methods
- **Week 10:** Instrumental variables
- **Week 11:** Regression discontinuity

Today

Beyond Selection on Observables

Previously we assumed selection on observables: We have measured and accounted for all confounding variables.

This is a very strong assumption, and rarely defensible or plausible.

Often, treated and untreated units will differ in unobservable characteristics that are associated with potential outcomes, even after controlling for observables.

With variation in treatment over two dimensions – between units and over time – we may be able to account for certain unobservables, even without measuring them explicitly.

This is difference-in-differences. We will require new assumptions that are often more (and *very rarely less*) defensible.

Difference-in-Differences

Difference-in-differences is a powerful, widely applicable design that has received renewed attention in recent years.

Used in both social science and industry – wherever there is variation in treatment over both time and one other dimension. This is very common!

Today we will focus on the canonical 2-period difference-in-differences design, with a brief extension to two pre-treatment periods.

Next week we will extend this design to multiple time periods with (potentially) staggered treatment roll-out and (potentially) heterogeneous treatment effects.

Example: Minimum Wage and Employment

Do higher minimum wages decrease employment?

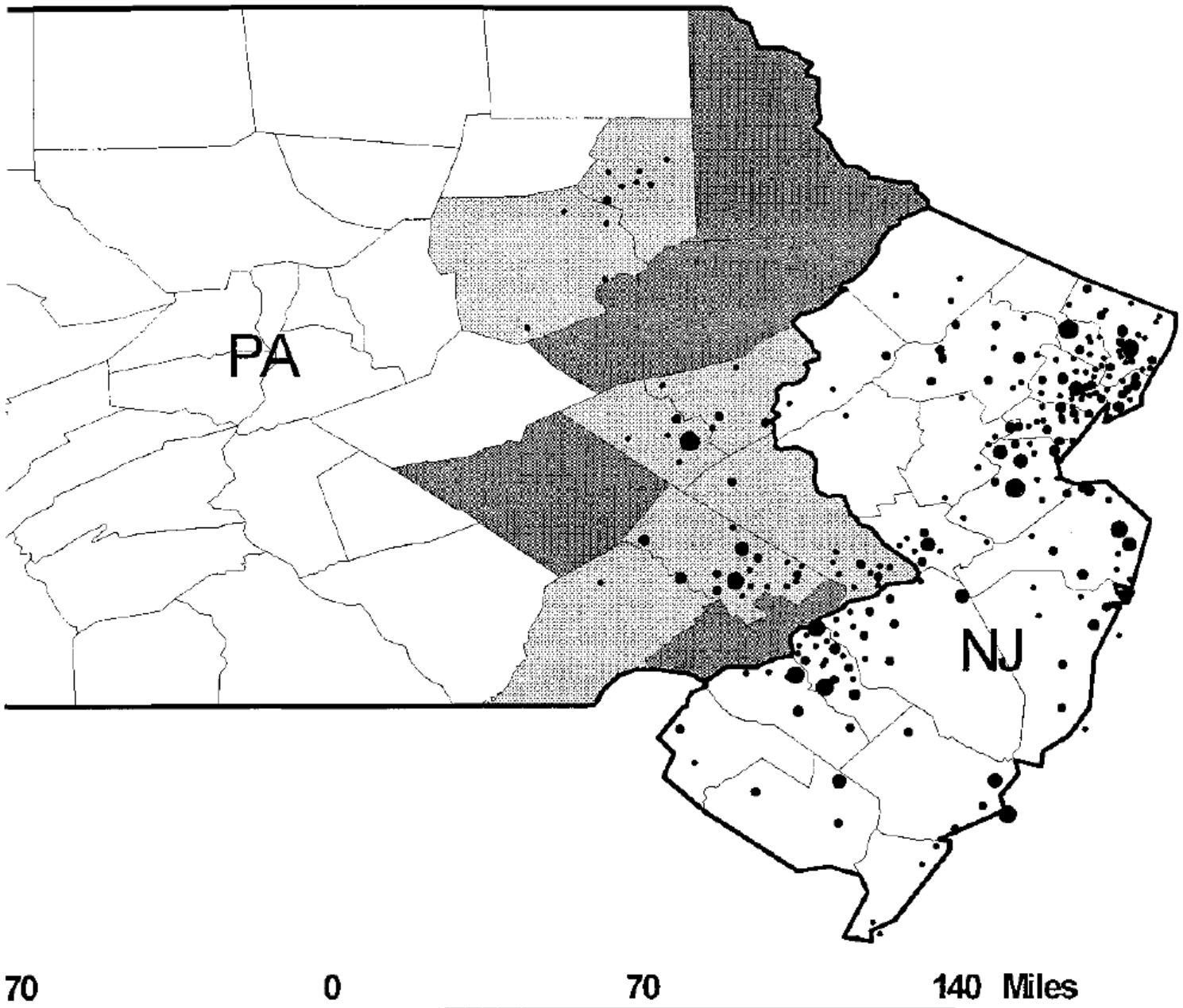
Card and Krueger (1994) consider impact of New Jersey's 1992 minimum wage increase from $4.25 to $5.05 per hour

Compare employment in 410 fast-food restaurants in New Jersey and eastern Pennsylvania before and after the rise

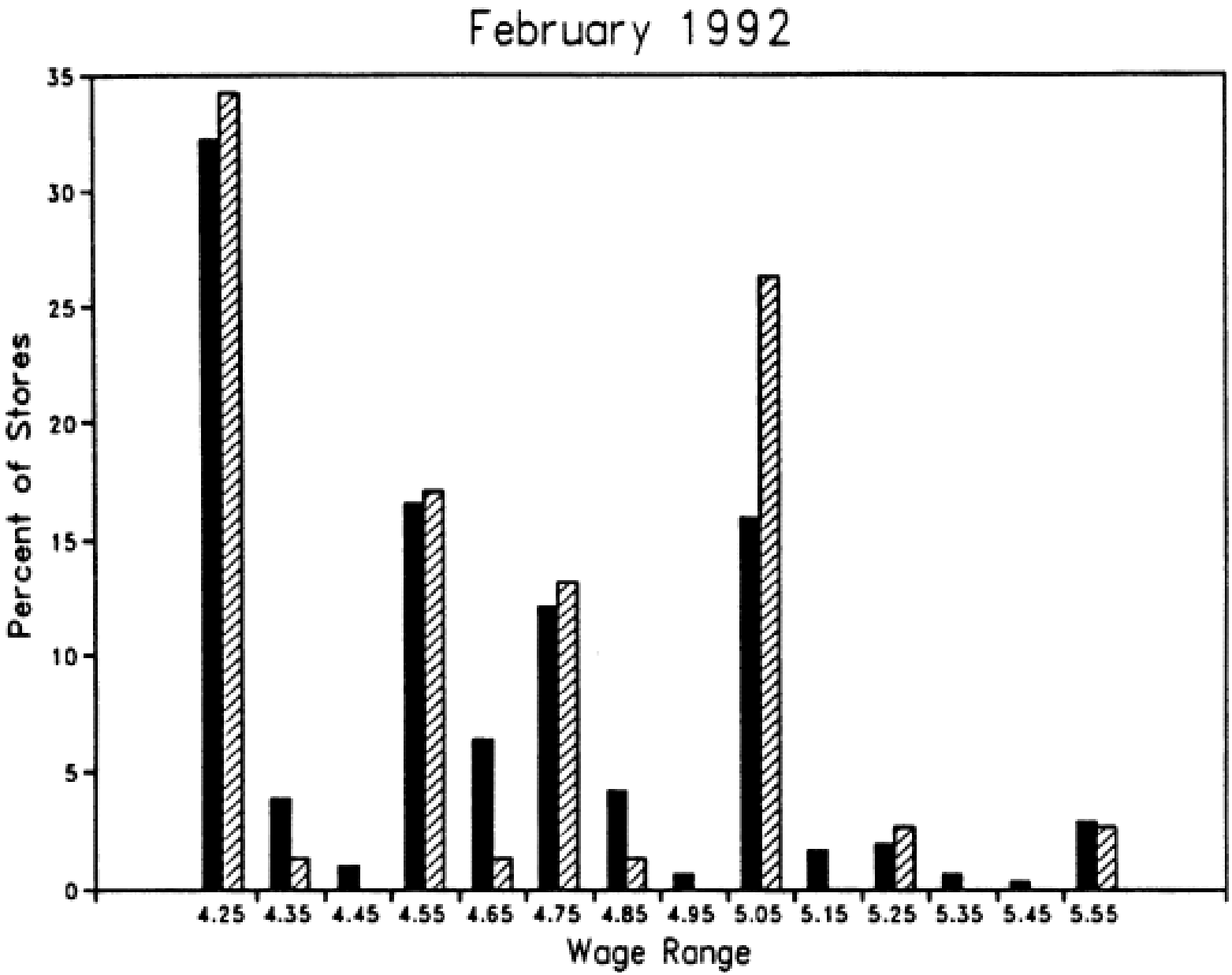
Survey data on wages and employment from two waves:

- Wave 1: March 1992, one month before the minimum wage increase
- Wave 2: December 1992, eight month after increase

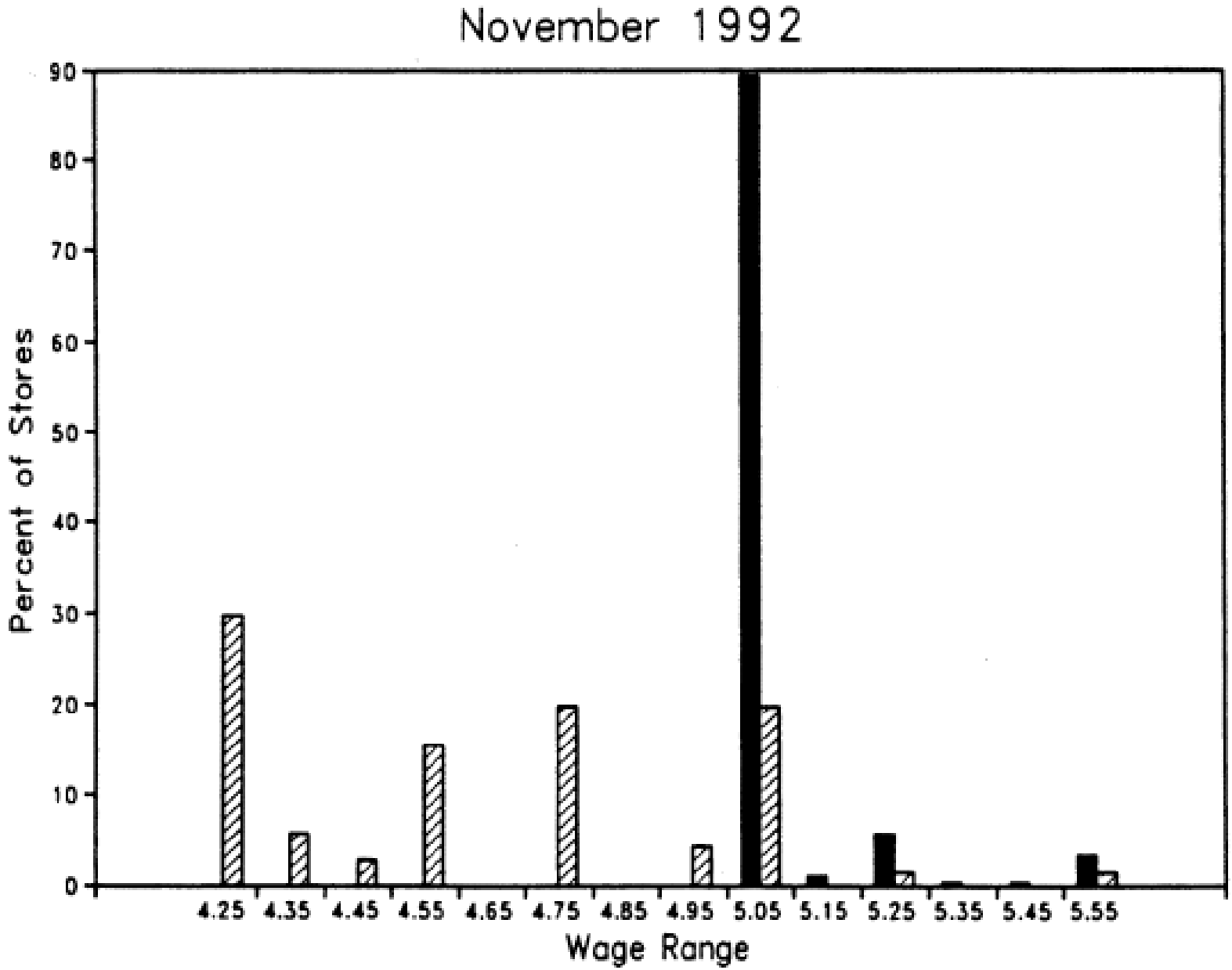
Location of Restaurants



Wages Before Rise in Minimum Wage



Wages After Rise in Minimum Wage



Wages After Rise in Minimum Wage

Variable	Stores by state: PA (i)	NJ (ii)	Difference, NJ − PA (iii)
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (0.51)	−2.89 (1.44)
2. FTE employment after, all available observations	21.17 (0.94)	21.03 (0.52)	−0.14 (1.07)
3. Change in mean FTE employment	−2.16 (1.25)	0.59 (0.54)	2.76 (1.36)

Difference 1: Pre-minimum wage - Post-minimum wage

Difference 2: NJ - PA

Leveraging both gives us **difference**-in-**differences**!

"Now **this** is causal inference!"

Groups, Periods and Treatments

Data structure for a 2-period difference-in-differences generally takes one of two forms:

1. "Panel data": A sample of units, where each unit is observed at two time points.
2. "Repeated cross-sections": Two random cross-sectional samples of units, collected at two time points, where each unit is observed only once.

Units: $i \in \{1, ..., N\}$

Time periods: $t \in \{0 \text{ (pre-treatment)}, 1 \text{ (post-treatment)}\}$

Group indicator: $G_i = \begin{cases} 1 & \text{(treatment group)} \\ 0 & \text{(control group)} \end{cases}$

Units in the treatment group receive treatment in $t = 1$, so:

Treatment indicator: $Z_{it} \in \{0, 1\}$

	Time Period	
Group	$t = 0$	$t = 1$
$G_i = 1$ (treatment group)	$Z_{i0} = 0$ (untreated)	$Z_{i1} = 1$ (treated)
$G_i = 0$ (control group)	$Z_{i0} = 0$ (untreated)	$Z_{i0} = 0$ (untreated)

Outcomes – Potential and Observed

Define potential outcomes $Y_{it}(z)$ as:

- $Y_{it}(0)$: potential outcome for unit i in period t when not treated
- $Y_{it}(1)$: potential outcome for unit i in period t when treated

Causal effect for unit i at time t is

$$\tau_{it} = Y_{it}(1) - Y_{it}(0)$$

Observed outcomes Y_{it} are realized as

$$Y_{it} = Y_{it}(0)(1 - Z_{it}) + Y_{it}(1)Z_{it}$$

Because $Z_{i1} = G_i$ in the post-treatment period, we can also write

$$Y_{i1} = Y_{i1}(0)(1 - G_i) + Y_{i1}(1)G_i$$

Identification Strategies

Estimand: ATT in the post-treatment period

$$\begin{aligned} \tau_{ATT} &= \mathbb{E}[Y_{i1}(1) - Y_{i1}(0) | G_i = 1] \\ &= \mathbb{E}[Y_{i1}(1) | G_i = 1] - \mathbb{E}[Y_{i1}(0) | G_i = 1] \end{aligned}$$

Observed quantities:

	Pre-Period ($t = 0$)	Post-Period ($t = 1$)
Treatment Group ($G_i = 1$)	$\mathbb{E}[Y_{i0}(0) \mid G_i = 1]$	$\mathbb{E}[Y_{i1}(1) \mid G_i = 1]$
Control Group ($G_i = 0$)	$\mathbb{E}[Y_{i0}(0) \mid G_i = 0]$	$\mathbb{E}[Y_{i1}(0) \mid G_i = 0]$

Problem: Missing potential outcome: $\mathbb{E}[Y_{i1}(0) | G_i = 1]$, i.e. what is the average post-period outcome for the treated group in the absence of the treatment?

Identification Strategies

Estimand: ATT in the post-treatment period

$$\begin{aligned} \tau_{ATT} &= \mathbb{E}[Y_{i1}(1) - Y_{i1}(0) | G_i = 1] \\ &= \mathbb{E}[Y_{i1}(1) | G_i = 1] - \mathbb{E}[Y_{i1}(0) | G_i = 1] \end{aligned}$$

Observed quantities:

	Pre-Period ($t = 0$)	Post-Period ($t = 1$)
Treatment Group ($G_i = 1$)	$\mathbb{E}[Y_{i0}(0) \mid G_i = 1]$	$\mathbb{E}[Y_{i1}(1) \mid G_i = 1]$
Control Group ($G_i = 0$)	$\mathbb{E}[Y_{i0}(0) \mid G_i = 0]$	$\mathbb{E}[Y_{i1}(0) \mid G_i = 0]$

Control Strategy: Before vs. After

- Use $\mathbb{E}[Y_{i1} | G_i = 1] - \mathbb{E}[Y_{i0} | G_i = 1]$ for τ_{ATT}
- Assumes $\mathbb{E}[Y_{i1}(0) | G_i = 1]$ = $\mathbb{E}[Y_{i0}(0) | G_i = 1]$
  (No change in average potential outcome over time)

Identification Strategies

Estimand: ATT in the post-treatment period

$$\begin{aligned} \tau_{ATT} &= \mathbb{E}[Y_{i1}(1) - Y_{i1}(0)|G_i = 1] \\ &= \mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 1] \end{aligned}$$

Observed quantities:

	Pre-Period ($t = 0$)	Post-Period ($t = 1$)
Treatment Group ($G_i = 1$)	$\mathbb{E}[Y_{i0}(0)\|G_i = 1]$	$\mathbb{E}[Y_{i1}(1)\|G_i = 1]$
Control Group ($G_i = 0$)	$\mathbb{E}[Y_{i0}(0)\|G_i = 0]$	$\mathbb{E}[Y_{i1}(0)\|G_i = 0]$

Control Strategy: Treated vs. Control in Post-Period

- Use $\mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i1}|G_i = 0]$ for τ_{ATT}
- Assumes $\mathbb{E}[Y_{i1}(0)|G_i = 1] = \mathbb{E}[Y_{i1}(0)|G_i = 0]$
  (Mean ignorability of treatment assignment)

Identification Strategies

Estimand: ATT in the post-treatment period

$$\begin{aligned} \tau_{ATT} &= \mathbb{E}[Y_{i1}(1) - Y_{i1}(0)|G_i = 1] \\ &= \mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 1] \end{aligned}$$

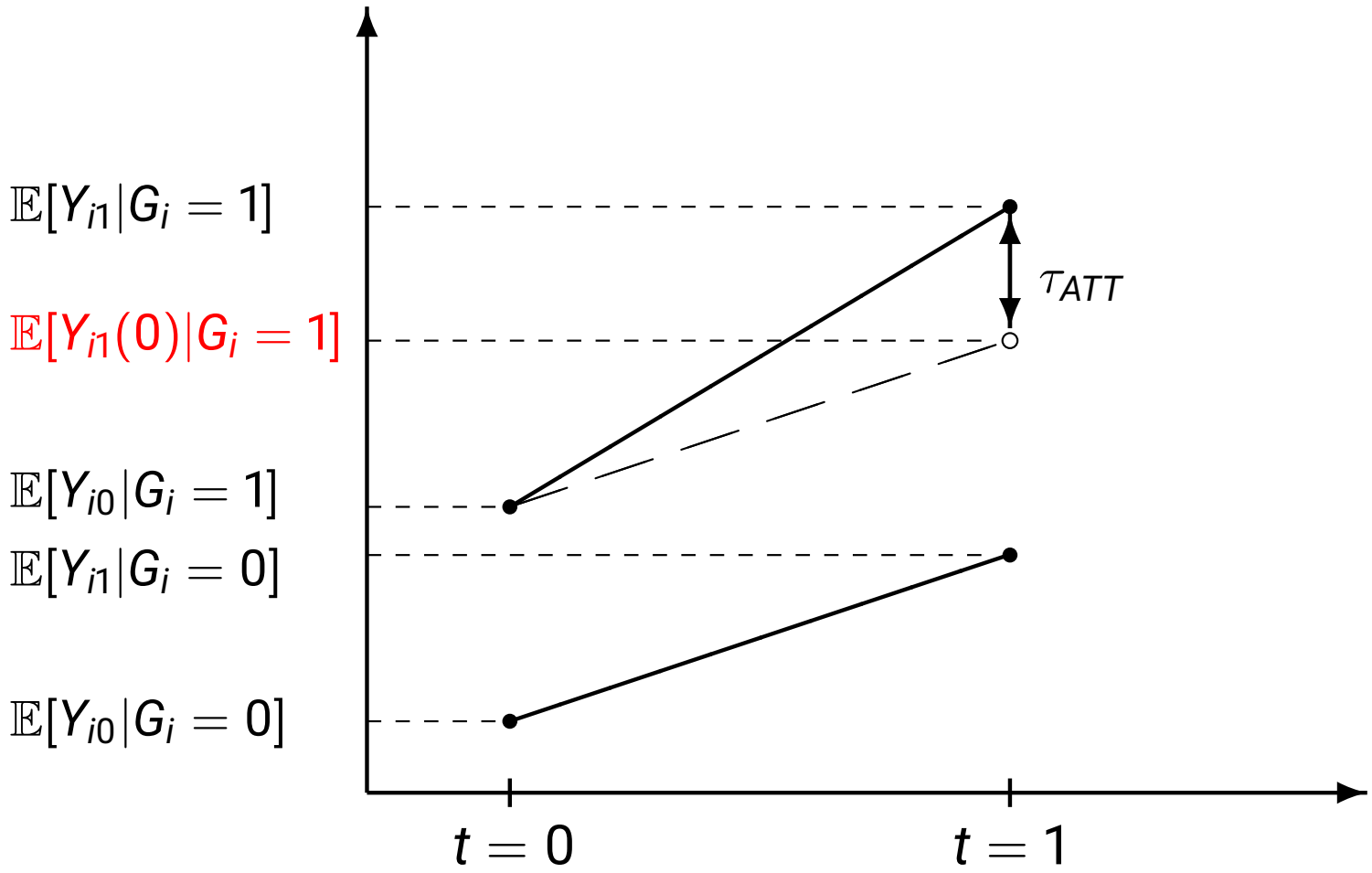
Observed quantities:

	Pre-Period ($t = 0$)	Post-Period ($t = 1$)
Treatment Group ($G_i = 1$)	$\mathbb{E}[Y_{i0}(0)\|G_i = 1]$	$\mathbb{E}[Y_{i1}(1)\|G_i = 1]$
Control Group ($G_i = 0$)	$\mathbb{E}[Y_{i0}(0)\|G_i = 0]$	$\mathbb{E}[Y_{i1}(0)\|G_i = 0]$

Control Strategy: Difference-in-Differences (DD)

- Use: $\Big\{\mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i1}|G_i = 0]\Big\} - \Big\{\mathbb{E}[Y_{i0}|G_i = 1] - \mathbb{E}[Y_{i0}|G_i = 0]\Big\}$
- Assumes: $\mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 1] = \mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 0]$ (Parallel trends)

Graphical Representation: Difference-in-Differences



Identification with Difference-in-Differences

Under the parallel trends assumption:

$$\mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 1] = \mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 0]$$

The ATT can be nonparametrically identified as:

$$\begin{aligned}\tau_{ATT} &= \{\mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i1}|G_i = 0]\} \\ &\quad - \{\mathbb{E}[Y_{i0}|G_i = 1] - \mathbb{E}[Y_{i0}|G_i = 0]\}\end{aligned}$$

Proof:

$$\begin{aligned}
& \{\mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i1}|G_i = 0]\} - \{\mathbb{E}[Y_{i0}|G_i = 1] - \mathbb{E}[Y_{i0}|G_i = 0]\} \\
= \; & \{\mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 0]\} - \{\mathbb{E}[Y_{i0}(0)|G_i = 1] - \mathbb{E}[Y_{i0}(0)|G_i = 0]\} \\
= \; & \underbrace{\mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 1]}_{= \tau_{ATT}} + \mathbb{E}[Y_{i1}(0)|G_i = 1] \\
& \qquad - \mathbb{E}[Y_{i1}(0)|G_i = 0] - \mathbb{E}[Y_{i0}(0)|G_i = 1] + \mathbb{E}[Y_{i0}(0)|G_i = 0] \\
= \; & \tau_{ATT} + \underbrace{\{\mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 1] - \mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 0]\}}_{= 0 \text{ under parallel trends}} \\
= \; & \tau_{ATT}
\end{aligned}$$

Notes on the Parallel Trends Assumption

What type of confounding does diff-in-diff make us robust to?

- Unobserved factors that are unit-varying, time-invariant and additive? Yes!
- Unobserved factors that are time-varying and unit-invariant? Yes!
- Unobserved factors that are time-varying and unit-varying? No!

Parallel trends may be more plausible with pre-treatment covariates:

$$\mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 1, X_i = x] = \mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 0, X_i = x]$$

This assumes parallel trends within strata $\rightarrow$ conditional parallel trends

Notes on the Parallel Trends Assumption

Under conditional parallel trends assumption, the ATT is identified as

$$\tau_{ATT} = \sum_x [\{\mathbb{E}[Y_{i1}|G_i = 1, X_i = x] - \mathbb{E}[Y_{i1}|G_i = 0, X_i = x]\}$$
$$- \{\mathbb{E}[Y_{i0}|G_i = 1, X_i = x] - \mathbb{E}[Y_{i0}|G_i = 0, X_i = x]\}] \Pr(X_i = x \mid G_i = 1)$$

Note the parallel trends assumption is not invariant to nonlinear transformations of the outcome scale...

For example, parallel trends in $Y_{it}(z)$ implies non-parallel trends in $\log(Y_{it}(z))$ and *vice versa*!

No Anticipation

So far we have slipped in an extra assumption:

- We asserted that $\mathbb{E}[Y_{i1}|G=1]$ is equal to $\mathbb{E}[Y_{i1}(0)|G=1]$.
- That is, the observed Y in the pre-period is the realised potential outcome under control. This is an assumption!
- Typically referred to as no anticipation: treated units do not react to treatment prior to it occurring.

Define $Y^*_{i0}(0)$ as the potential outcome under control in $t=0$ for the treated group ($G=1$) if they anticipate treatment. Our proof then changes:

$$
\begin{aligned}
& \{\mathbb{E}[Y_{i1}|G_i=1]-\mathbb{E}[Y_{i1}|G_i=0]\}-\{\mathbb{E}[Y_{i0}|G_i=1]-\mathbb{E}[Y_{i0}|G_i=0]\} \\
= \quad & \tau_{ATT}+\underbrace{\{\mathbb{E}[Y_{i1}(0)-Y_{i0}(0)|G_i=1]-\mathbb{E}[Y_{i1}(0)-Y_{i0}(0)|G_i=0]\}}_{\text{non-parallel trends bias}} \\
& +\underbrace{\{\mathbb{E}[Y_{i0}(0)-Y^*_{i0}(0)|G_i=1]\}}_{\text{anticipation bias}}
\end{aligned}
$$

Panel vs. Cross-Sectional Data

Our data structure is either panel or repeated cross sectional.

In practice, what does this look like?

For panel data:

Unit	Time	Y_{it}	G_i	Z_{it}	X_{it}
1	0	$y_{1,0}$	g_1	$z_{1,0}$	$x_{1,0}$
1	1	$y_{1,1}$	g_1	$z_{1,1}$	$x_{1,1}$
2	0	$y_{2,0}$	g_2	$z_{2,0}$	$x_{2,0}$
2	1	$y_{2,1}$	g_2	$z_{2,1}$	$x_{2,1}$
...	...	...	...	...	...
n	0	$y_{n,0}$	g_n	$z_{n,0}$	$x_{n,0}$
n	1	$y_{n,1}$	g_n	$z_{n,1}$	$x_{n,1}$

Panel vs. Cross-Sectional Data

Our data structure is either panel or repeated cross sectional.

In practice, what does this look like?

For panel data, a particular realisation might be:

Unit	Time	Y_{it}	G_i	Z_{it}	X_{it}
1	0	$y_{1,0}$	1	0	$x_{1,0}$
1	1	$y_{1,1}$	1	1	$x_{1,1}$
2	0	$y_{2,0}$	0	0	$x_{2,0}$
2	1	$y_{2,1}$	0	0	$x_{2,1}$
...	...	...	...	...	...
n	0	$y_{n,0}$	1	0	$x_{n,0}$
n	1	$y_{n,1}$	1	1	$x_{n,1}$

Panel vs. Cross-Sectional Data

Our data structure is either panel or repeated cross sectional.

In practice, what does this look like?

For repeated cross-sections:

Unit	Time	Y_i	G_i	Z_i	X_i
1	0	y_1	g_1	$z_{1,0}$	x_1
2	1	y_2	g_2	$z_{2,1}$	x_2
3	0	y_3	g_3	$z_{3,0}$	x_3
4	1	y_4	g_4	$z_{4,1}$	x_4
...	...	...	...	...	...
$n-1$	0	y_{n-1}	g_{n-1}	z_{n-1}	x_{n-1}
n	1	y_n	g_n	z_n	x_n

Panel vs. Cross-Sectional Data

Our data structure is either panel or repeated cross sectional.

In practice, what does this look like?

For repeated cross-sections, a particular realisation might be:

Unit	Time	Y_i	G_i	Z_i	X_i
1	0	y_1	1	0	x_1
2	1	y_2	1	1	x_2
3	0	y_3	0	0	x_3
4	1	y_4	0	0	x_4
...	...	...	...	...	...
$n-1$	0	y_{n-1}	1	0	x_{n-1}
n	1	y_n	1	1	x_n

Plug-in Estimation for Panel Data

Estimand:

$$\tau_{ATT} = \Big\{\mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i1}|G_i = 0]\Big\} - \Big\{\mathbb{E}[Y_{i0}|G_i = 1] - \mathbb{E}[Y_{i0}|G_i = 0]\Big\}$$

A plug-in **estimator** ("difference in difference-in-means"):

$$\left\{\frac{1}{N_1}\sum_{i=1}^{N} G_i Y_{i1} - \frac{1}{N_0}\sum_{i=1}^{N}(1-G_i)Y_{i1}\right\} - \left\{\frac{1}{N_1}\sum_{i=1}^{N} G_i Y_{i0} - \frac{1}{N_0}\sum_{i=1}^{N}(1-G_i)Y_{i0}\right\}$$

$$= \left\{\frac{1}{N_1}\sum_{i=1}^{N} G_i\{Y_{i1} - Y_{i0}\} - \frac{1}{N_0}\sum_{i=1}^{N}(1-G_i)\{Y_{i1} - Y_{i0}\}\right\},$$

where N_1 and N_0 are the number of treated and control units respectively

Standard errors can be estimated by extending the diff-in-means variance formula using the same logic (assuming no clustering – more on this later)

Plug-in Estimation for Repeated Cross Sections

Repeated cross-sectional data requires a slight change in notation.

Estimand: $\tau_{ATT} = \mathbb{E}[Y_i(1) - Y_i(0) \mid G_i = 1, T_i = 1]$

- Identified as: $\tau_{ATT} = \mathbb{E}[Y_i \mid G_i = 1, T_i = 1] - \mathbb{E}[Y_i \mid G_i = 0, T_i = 1] - \{\mathbb{E}[Y_i \mid G_i = 1, T_i = 0] - \mathbb{E}[Y_i \mid G_i = 0, T_i = 0]\}$
- Note that N now refers to the size of the pooled sample

The plug-in **estimator** is then written as:

$$\hat{\tau}_{ATT} = \left\{ \frac{\sum_{i=1}^{N} G_i T_i Y_i}{\sum_{i=1}^{N} G_i T_i} - \frac{\sum_{i=1}^{N} (1 - G_i) T_i Y_i}{\sum_{i=1}^{N} (1 - G_i) T_i} \right\} - \left\{ \frac{\sum_{i=1}^{N} G_i (1 - T_i) Y_i}{\sum_{i=1}^{N} G_i (1 - T_i)} - \frac{\sum_{i=1}^{N} (1 - G_i)(1 - T_i) Y_i}{\sum_{i=1}^{N} (1 - G_i)(1 - T_i)} \right\}$$

Covariates X_i can be incorporated via subclassification

Regression Estimator for Repeated Cross Sections

Because G_i and T_i are both binary, the same estimator can be calculated via regression:

$$\hat{Y}_i = \hat{\mu} + \hat{\gamma} G_i + \hat{\delta} T_i + \hat{\tau} G_i T_i$$

where $\hat{\mu}$, $\hat{\gamma}$, $\hat{\delta}$ and $\hat{\tau}$ are estimated with OLS regression.

Easy to show that $\hat{\tau} = \hat{\tau}_{ATT}$:

	After ($T_i = 1$)	Before ($T_i = 0$)	After - Before
Treated $G_i = 1$	$\hat{\mu} + \hat{\gamma} + \hat{\delta} + \hat{\tau}$	$\hat{\mu} + \hat{\gamma}$	$\hat{\delta} + \hat{\tau}$
Control $G_i = 0$	$\hat{\mu} + \hat{\delta}$	$\hat{\mu}$	$\hat{\delta}$
Treated - Control	$\hat{\gamma} + \hat{\tau}$	$\hat{\gamma}$	$\hat{\tau}$

- Covariates (X_i) can be added to the right-hand side, with the risk of possible misspecification bias
- Don't include X_i that can be affected by the treatment! (post-treatment bias)

Regression Estimator for Panel Data

For panel data, consider an additive linear model for potential outcomes:

$$Y_{it}(z) = \alpha_i + \gamma t + \tau z + \varepsilon_{it}$$

where α_i is a time-invariant unobserved parameter for unit i.

We can show:

- $\tau = \tau_{ATE} = \tau_{ATT}$
- Parallel trends imply:

$$\mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 1] = \mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 0]$$

$$\iff \mathbb{E}[\varepsilon_{i1} - \varepsilon_{i0} \mid G_i = d] = 0 \quad \text{for} \quad d \in \{0, 1\}$$

Therefore, the first-differenced regression of $Y_{i1} - Y_{i0}$ on G_i can unbiasedly estimate $\tau_{ATT} = \tau_{ATE}$

Notice that panel data allow for *unit-level* unobserved confounding unlike the repeated cross-section case (*group-level* unobserved confounding), but it must be additive and time-invariant

Can include time-varying covariates (X_{it}) with possible risk of post-treatment bias

A Note on Inference

Treatment assignment may or may not be assigned at the unit level. In some cases, it is assigned in a clustered fashion:

- Geographic assignment (e.g. the NJ minimum wage)
- Grouped assignment (e.g. school interventions)

Standard errors should account for the level of assignment – if clustered assignment, cluster SEs (see Abadie et al., 2023)

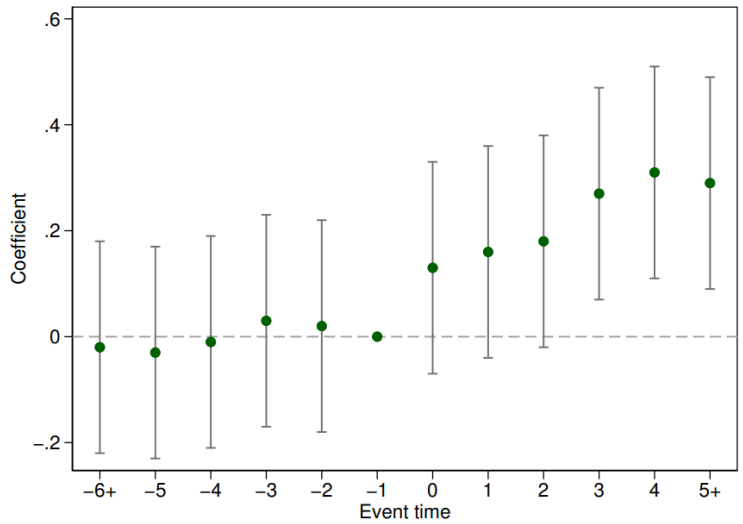
If you have a small number of clusters (fewer than $\approx$ **30**), consider a bootstrapped alternative (see Cameron & Miller, 2015 for guidance)

Testing Parallel Trends

Can we directly test parallel trends? No! We never observe potential outcomes.

But assuming $T > 2$ in the pre-period, we can test for parallel pre-trends:

- Sometimes done through discrete analyses: Generate placebo treatment indicators before true treatment occurs, and test for "effect."
- Often done through an event-study model: Fully interact treatment with time period dummies (generalising the above idea).

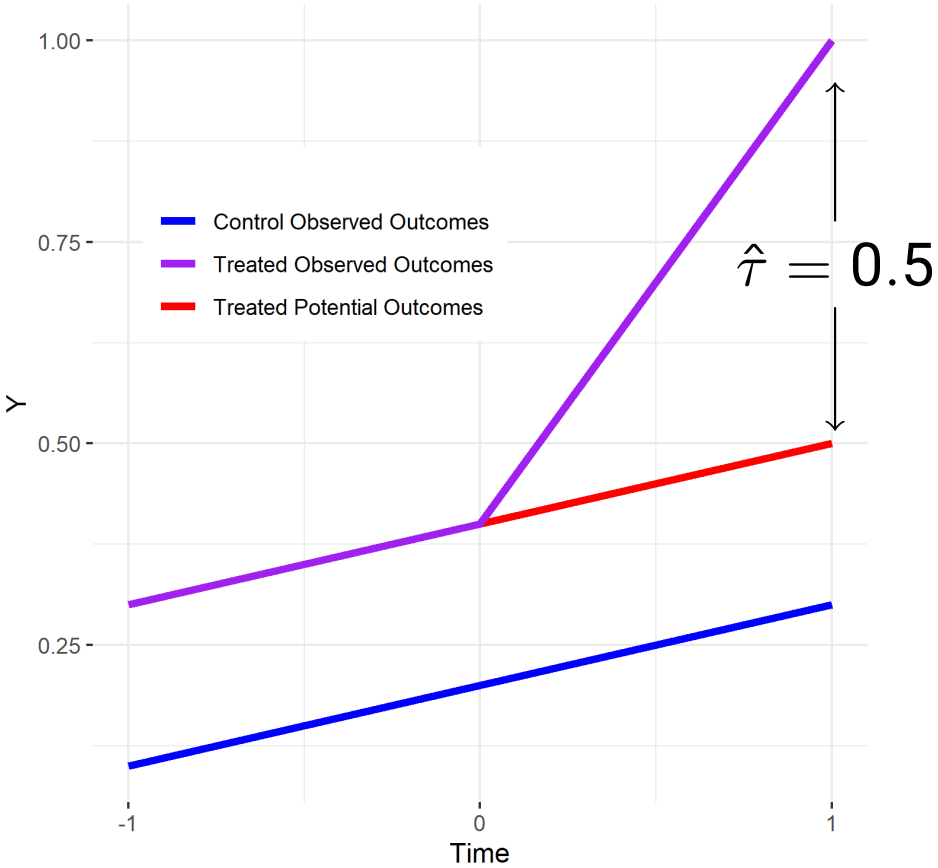


(from Freyaldenhoven et al, 2021)

Pre-Trend Test with Caution

Imagine we test pre-trends, and find no statistically significant difference.

Is this good evidence for the parallel trends assumption?



Consider a case where $\tau = \mathbf{0.5}$ and where parallel trends in potential outcomes holds.

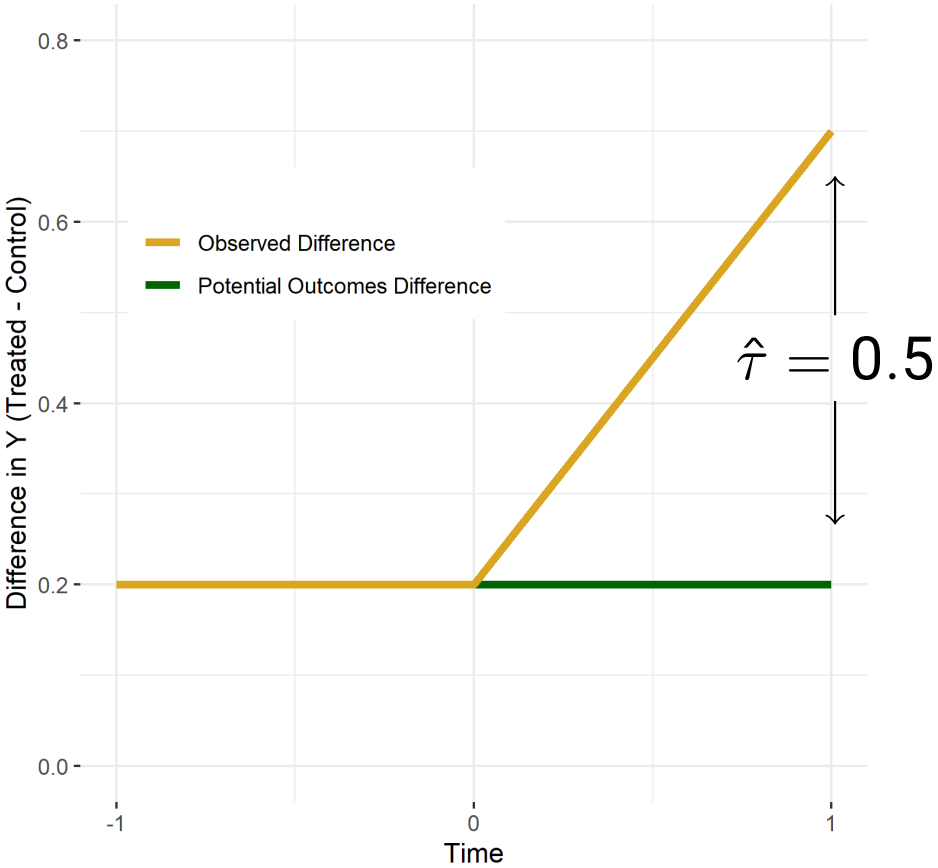
Here, we can safely use the trend in the control as a "stand-in" for the trend in the treated.

Our diff-in-diff estimator is an unbiased estimator of τ.

Pre-Trend Test with Caution

Imagine we test pre-trends, and find no statistically significant difference.

Is this good evidence for the parallel trends assumption?



Consider a case where $\tau = \mathbf{0.5}$ and where parallel trends in potential outcomes holds.

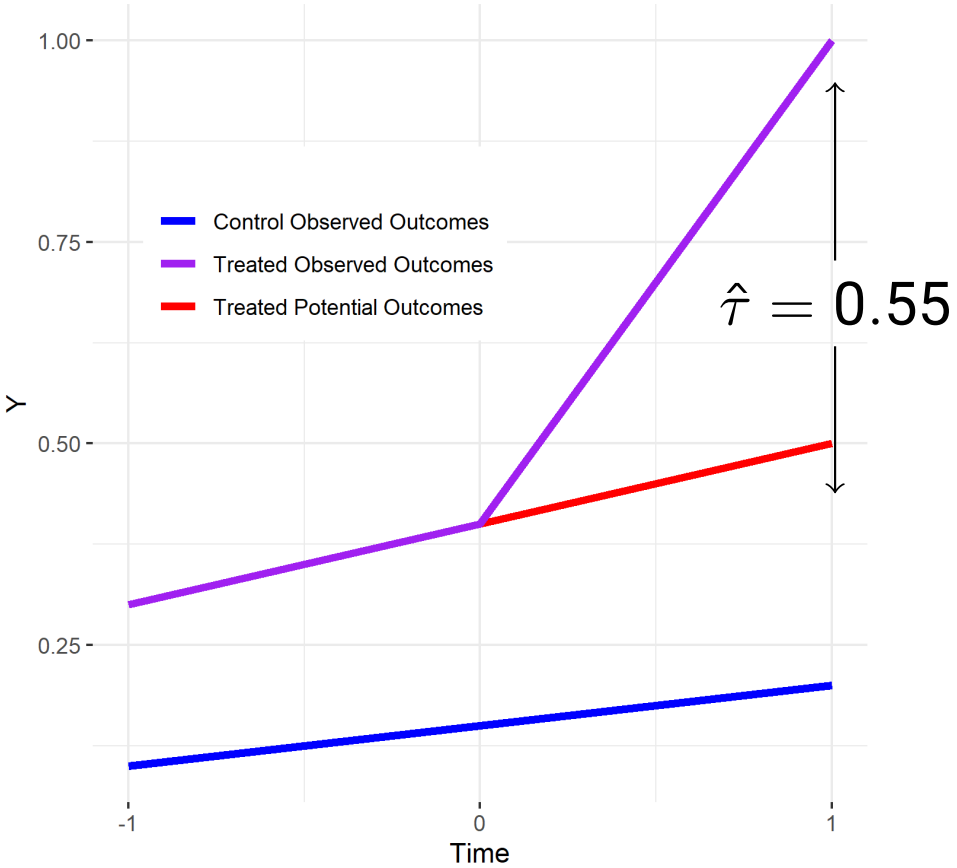
Looking at differences between units over time, the difference in potential outcomes is zero, pre and post.

We can simply assess the diff-in-diffs for observed outcomes.

Pre-Trend Test with Caution

Imagine we test pre-trends, and find no statistically significant difference.

Is this good evidence for the parallel trends assumption?



Now consider a case where $\tau = 0.5$ and a non-statistically significant linear violation of parallel trends.

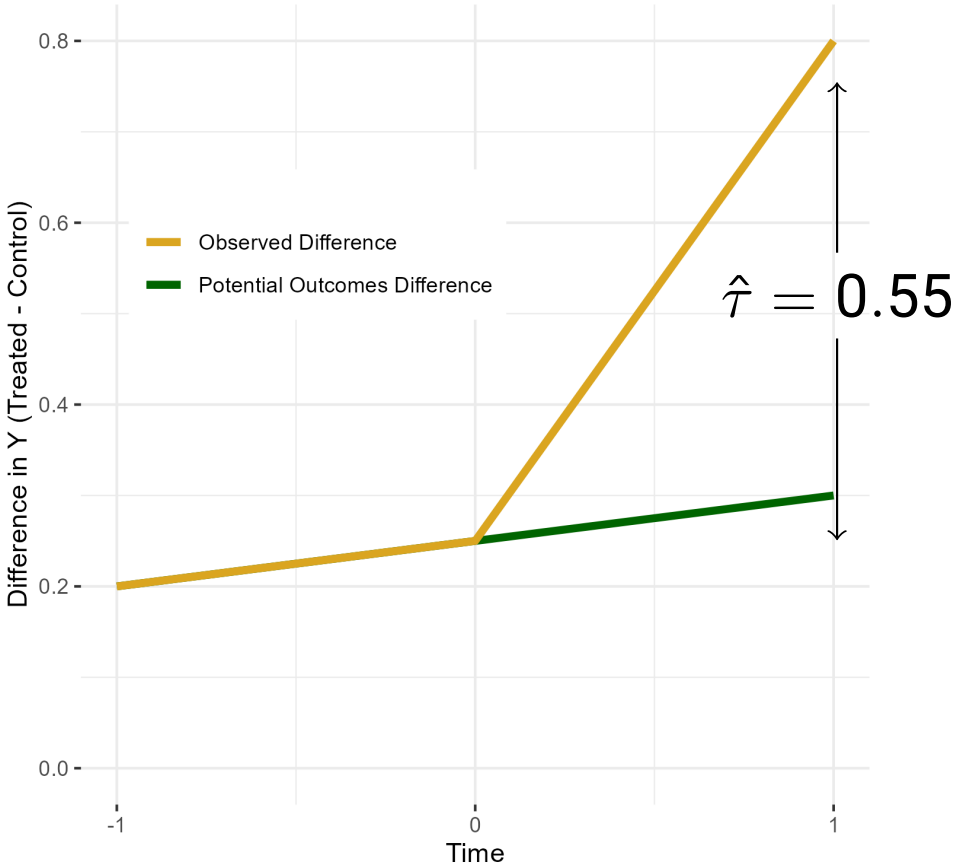
Here, the trend in the control is a less good "stand-in" for the trend in the treated.

Our diff-in-diff estimator ends up "missing" τ (by $\approx$ linear violation).

Pre-Trend Test with Caution

Imagine we test pre-trends, and find no statistically significant difference.

Is this good evidence for the parallel trends assumption?



Now consider a case where $\tau = 0.5$ and a non-statistically significant linear violation of parallel trends.

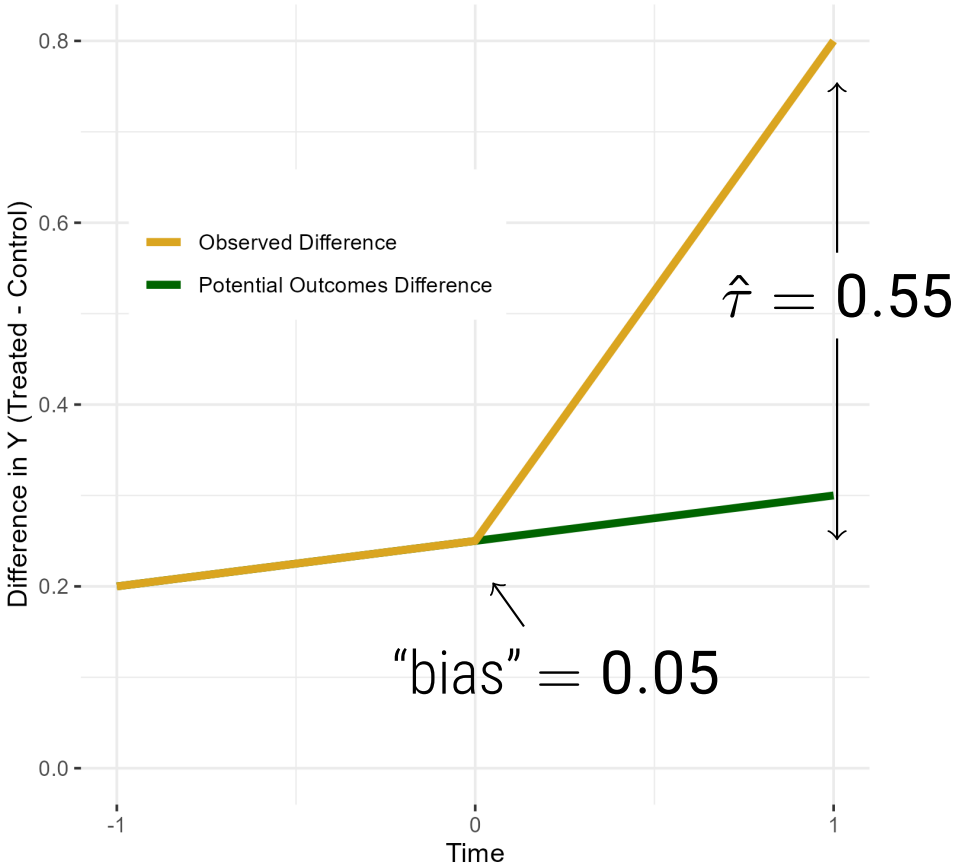
The difference in parallel trends is now non-zero pre and post, and increasing in post-period time.

The deviation in potential outcomes misleads us in the post-period.

Pre-Trend Test with Caution

Imagine we test pre-trends, and find no statistically significant difference.

Is this good evidence for the parallel trends assumption?



Okay, okay, you say, we already know this! A violation of parallel trends = bad! We tested for pre-trends!

But for which difference are you more likely to reject the null of no difference from zero?

And if we condition on a non-significant pre-trend, are we targeting the ATT?

Pre-Trend Test with Caution

Tests of pre-trend deviations are typically less well-powered than tests for the main effect $\rightsquigarrow$ we may (often) incorrectly "pass" a pre-trend test.

Further, by conditioning our analyses on only cases that "pass" the pre-trend test, we bias our estimator, possibly quite badly (see Roth, 2020).

Point-wise tests for statistical significance alone are insufficient:

- Pay attention to substantive significance (how big is any estimated deviation), and calculate the minimum detectable effect (MDE) for the pre-test.
- Correct your pre-trend inferences using uniform confidence bands (Freyaldenhoven et al, 2021), or tests of joint significance (Liu et al, 2022)
- Estimate any pre-trend difference, then assess the sensitivity of your results to variation in that (linear) trend violation (Rambachan and Roth, 2022)

Summary

Difference-in-differences can be applied to settings in which:

1. we have data for units in at least two time periods,
2. between which an intervention has occurred where some units are treated and some not

We need to satisfy the parallel trends assumption:

- Treated units, in the absence of treatment, would trend exactly as the control units did
- Often supported by the treatment being "exogenous" – no anticipation, no selection.

But parallel trends is untestable!

- Testing pre-trends can give us indirect evidence, but be careful!
- Plausibility will often come down to detailed qualitative case knowledge.