MY457/MY557: Causal Inference for Observational and Experimental Studies

Week 10: Difference-in-Differences 1

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Course Outline

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

Difference-in-Differences

Thus far, we have reasoned about cases where D varies between units, and we assert some belief about the assignment of D.

But sometimes D may vary over two dimensions. This is actually very common, e.g. variation over time and between units.

Enter difference-in-differences (DiD).

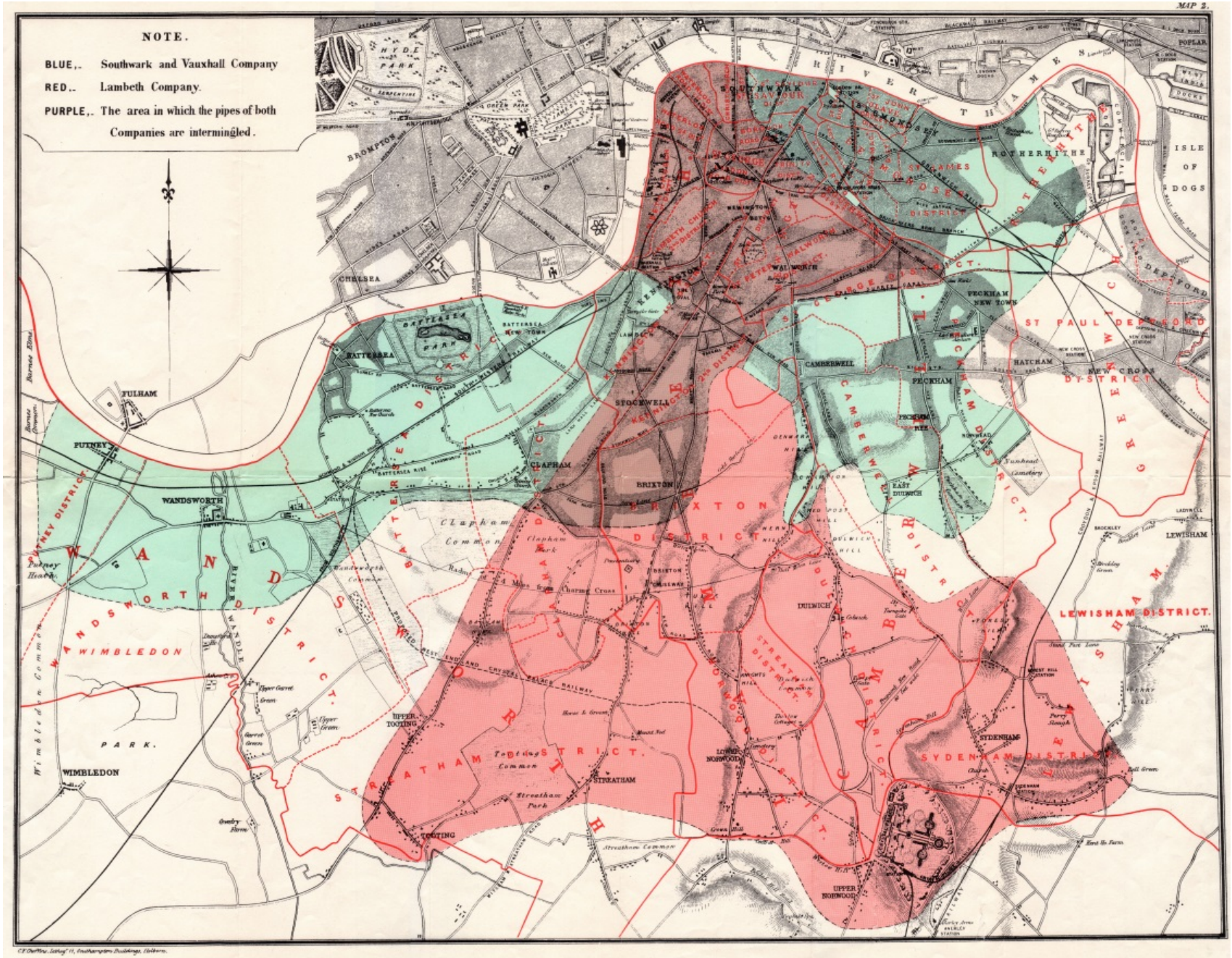
Popular in both academia and industry, DiD is a widely applicable research design that has received renewed attention in recent years.

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.


JOHN SNOW
JOHN SNOW
ITALIAN BEAR CHOCOLATE
SALOON
SNUG
TRADITIONAL FOOD
SERVED HERE


SALUS POPULI SUPREMA LEX.
Time. Mid-day.
Tide. Low Water at London Bridge.
Give us Clean Water!
Give us pure water!
We shall all have the Cholera
What torrents of filth come from that Walbrook sewer!!
Sewer! why there are 130 such!
Oh! never mind any nastiness goes down here in the Borough!
What do they drink that!
Prodigious!
"Tell it not in Gath"
Give us clean water
Give us clean water
Give us clean water
Devilish thick!
yes, here I stick!
It makes me Sick!
March of cleanliness!
Walbrook
Horse Shoe Alley
SOURCE OF THE SOUTHWARK WATER WORKS
George Cruikshank fec.
Hail long-expected days
That Thames's glory to the stars shall raise Pope


MAP 2.
NOTE.
BLUE,. Southwark and Vauxhall Company
RED,. Lambeth Company.
PURPLE,. The area in which the pipes of both Companies are intermingled.
RIVER THAMES
CHELSEA
BATTERSEA
FULHAM
PUTNEY
WANDSWORTH
CLAPHAM
BRIXTON
STOCKWELL
CAMBERWELL
PECKHAM
DULWICH
EAST DULWICH
LEWISHAM
SYDENHAM
STREATHAM
TOOTING
WIMBLEDON
ISLE OF DOGS
DEPTFORD
LEWISHAM DISTRICT.
WANDSWORTH DISTRICT.
PARK.

Week ending	Deaths from Cholera.	Water Supply.				
		Southwk. and Vauxhall.	Lambeth.	Kent Company.	Pumps, and other sources.	Not ascer-tained.
September 2.	670	399	45	38	72	116
„ 9.	972	580	72	45	62	213
„ 16.	856	524	66	48	44	174
„ 23.	724	432	72	28	62	130
„ 30.	383	228	25	19	24	87
October 7.	200	121	14	10	9	46
„ 14.	115	69	8	3	6	29
	3920	2353	302	191	279	795

Table 1 John Snow's data on mortality from cholera in areas served by only one of the Southwark & Vauxhall or Lambeth Water Companies before and after the change in water source for the Lambeth Water Company. Rates for all time points were calculated based on 1851 population census

Water supply	Cholera deaths, 1849, rate per 100,000	Cholera deaths, 1854, rate per 100,000	Difference in rates comparing 1854 to 1849, rate per 100,000
Southwark & Vauxhall Company only	1349	1466	118
Lambeth Company Only	847	193	−653
Difference-in-difference, Lambeth versus Southwark & Vauxhall	502	1273	−771

Treatment: Change of Lambeth Company's water source in 1852

Difference 1: Pre-Period - Post-Period

Difference 2: Lambeth - Southwark & Vauxhall

Leveraging both gives us difference-in-differences!

Note: We can compute this across either dimension.

Groups, Periods, and Treatment

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

Note: Pay attention to the notation change above!

Individual 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 ($t = 1$), we can also write

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

Identification Challenge

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

What would the average post-period outcome for the treated group have been in the absence of treatment?

Possible Comparisons

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

Comparison: Post-Period vs. Pre-Period, for Treated

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

Possible Comparisons

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

Comparison: Treated vs. Control, in Post-Period

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

Possible Comparisons

Estimand: ATT in the post-treatment period

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

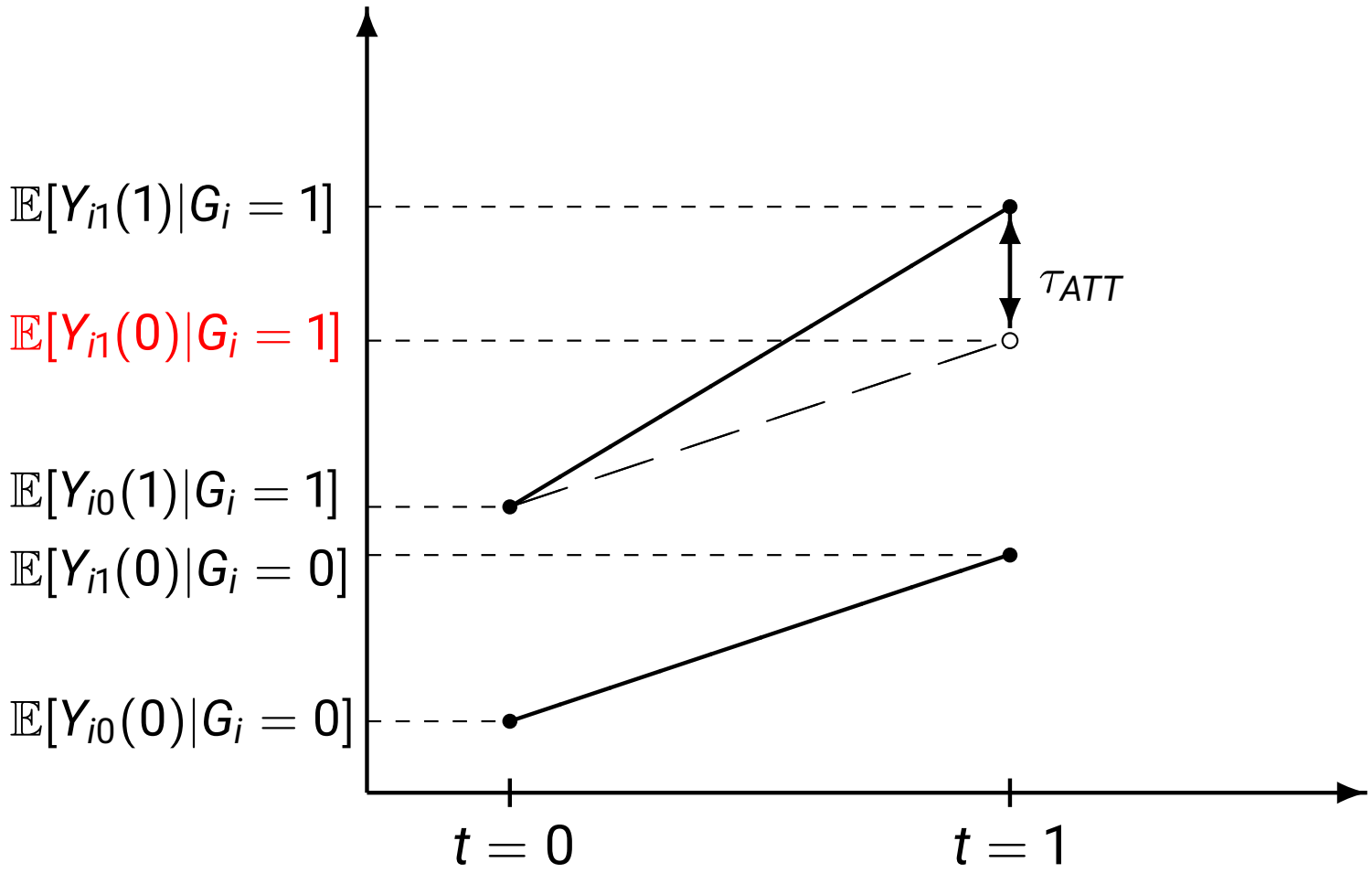
Observed quantities:

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

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

Graphical Representation: Difference-in-Differences



Identification Result for 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:

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

Proof:

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

Conditional Parallel Trends

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

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

Read: Within each stratum of X, potential outcomes are parallel.

Under this conditional parallel trends assumption, the ATT is identified as

$$\tau_{ATT} \;=\; \sum_x \tau_{DID,x} \Pr(X_i = x \mid G_i = 1)$$

where: $\tau_{DID,x} = \big[\mathbb{E}[Y_{i1}|G_i = 1, X_i = x] - \mathbb{E}[Y_{i1}|G_i = 0, X_i = x]\big] - \big[\mathbb{E}[Y_{i0}|G_i = 1, X_i = x] - \mathbb{E}[Y_{i0}|G_i = 0, X_i = x]\big]$

Read: Conditional (stratum-specific) ATT, weighted by prevalence of $X = x$ among the treated ($G = 1$).

No Anticipation

We have actually slipped in an extra assumption:

- We asserted that $\mathbb{E}[Y_{i0}|G=1]$ is equal to $\mathbb{E}[Y_{i0}(0)|G=1]$.
- That is, the observed Y in the pre-period is the realised potential outcome under control. This is an assumption!
- No anticipation: treated units do not react to treatment prior to it occurring.

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

Our proof then changes:

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

Brief Aside: About What Are We Making Assumptions?

In randomized experiments, selection on observables, IV, and local randomization RDD we knew, measured, and accounted for the assignment mechanism of D.

That is, our assumptions were about the assignment of D in relation to potential outcomes.

In DiD (and continuity RDD) we are instead making assumptions about potential outcomes in relation to treatment.

These are subtly different approaches in terms of what is "knowable" (and testable) as researchers.

While we can "know" (and reason about, test) the assignment mechanism for D, can we ever "know" potential outcomes?

Common Weaknesses of Difference-in-Differences

Violations of parallel trends:

- Time-varying confounders
- Bundled treatments (related to the above)
- Spillovers that induce changes in control group

Scale and transformations of Y:

- Parallel trends is not invariant to nonlinear transformations of the outcome
- E.g. parallel trends in $Y_{it}(z)$ implies non-parallel trends in $\log(Y_{it}(z))$ and *vice versa*

Violations of no anticipation:

- Treatment or control group strategically change behaviour prior to treatment

Panel vs. Cross-Sectional Data

Consider two data structures: panel or repeated cross sectional.

For panel data, what does this look like in practice?

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

Consider two data structures: panel or repeated cross sectional.

For panel data, what does this look like in practice?

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	$x_{n,1}$

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"):

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

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

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)

Panel vs. Cross-Sectional Data

Now consider the data structure 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

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

The plug-in estimator is then written as:

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

Note: 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	$\hat{\gamma} + \hat{\tau}$	$\hat{\gamma}$	$\hat{\tau}$

Note: 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!

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 know two things:

- $\tau = \tau_{ATE} = \tau_{ATT}$ (homogeneous treatment effects)
- $\mathbb{E}[\varepsilon_{i1} - \varepsilon_{i0} \mid G_i = d] = 0 \quad \text{for} \quad d \in \{0, 1\}$ (parallel trends)

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

Notice that panel data allow for *unit-level* unobserved confounding beyond *group-level* unobserved confounding, but it must be additive and time-invariant

Note: Covariate adjustment can be considered, as before.

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 water companies in London)
- 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)

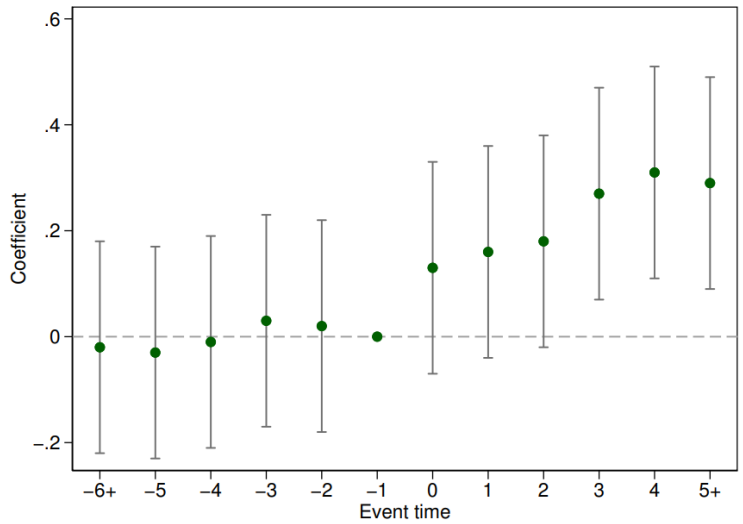
If you have a small number of clusters (fewer than $\approx$ **30**), consider a bootstrapped alternative (see Cameron & Miller, 2015 for guidance)

Testing Parallel Trends

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

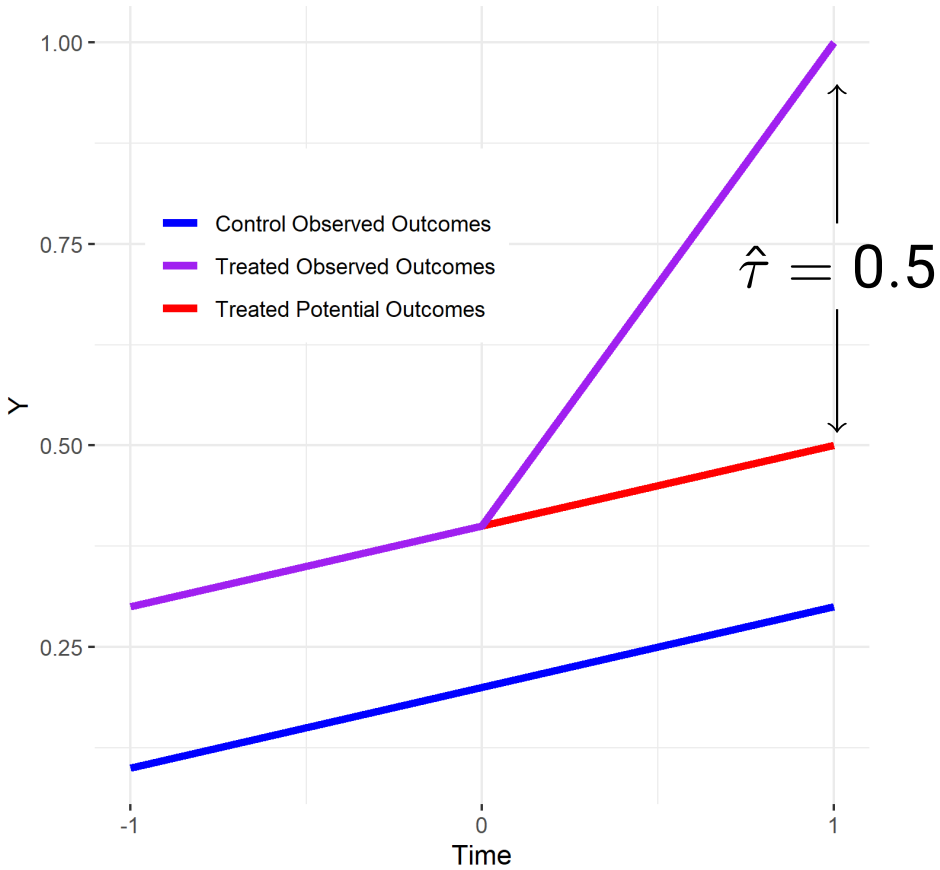
But assuming $T > 2$ in the pre-period, we can test for parallel pre-trends:

- 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

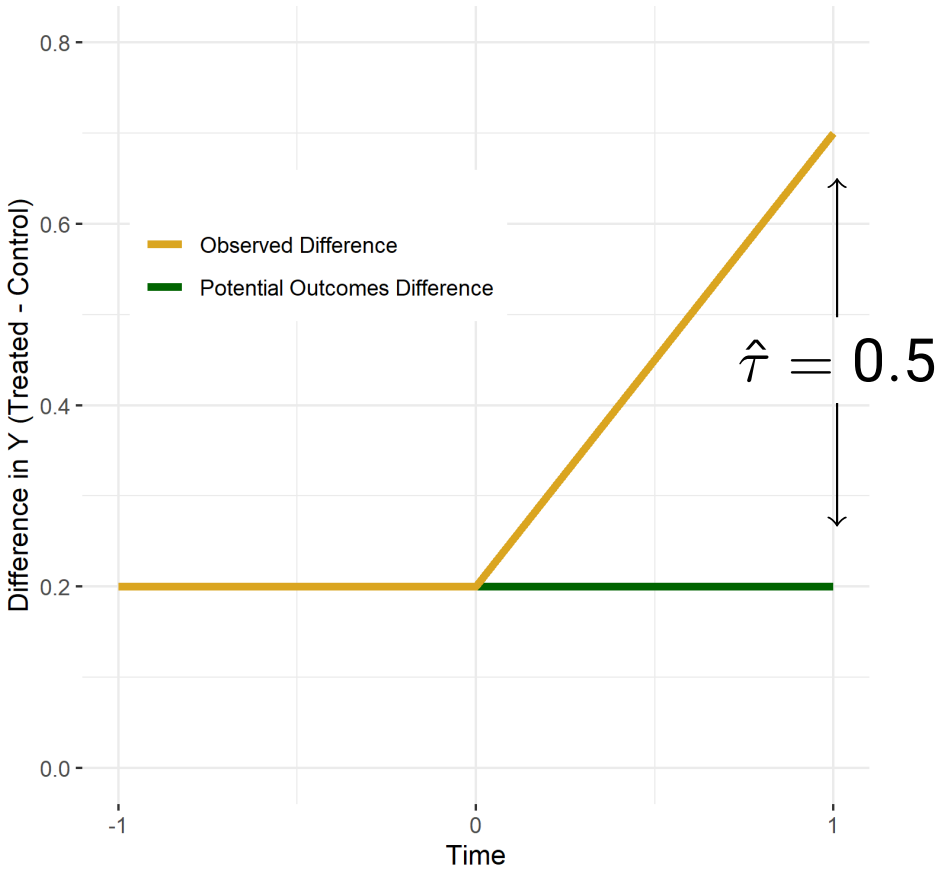


Consider a case where $\tau = 0.5$ and where parallel trends in potential outcomes holds.

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



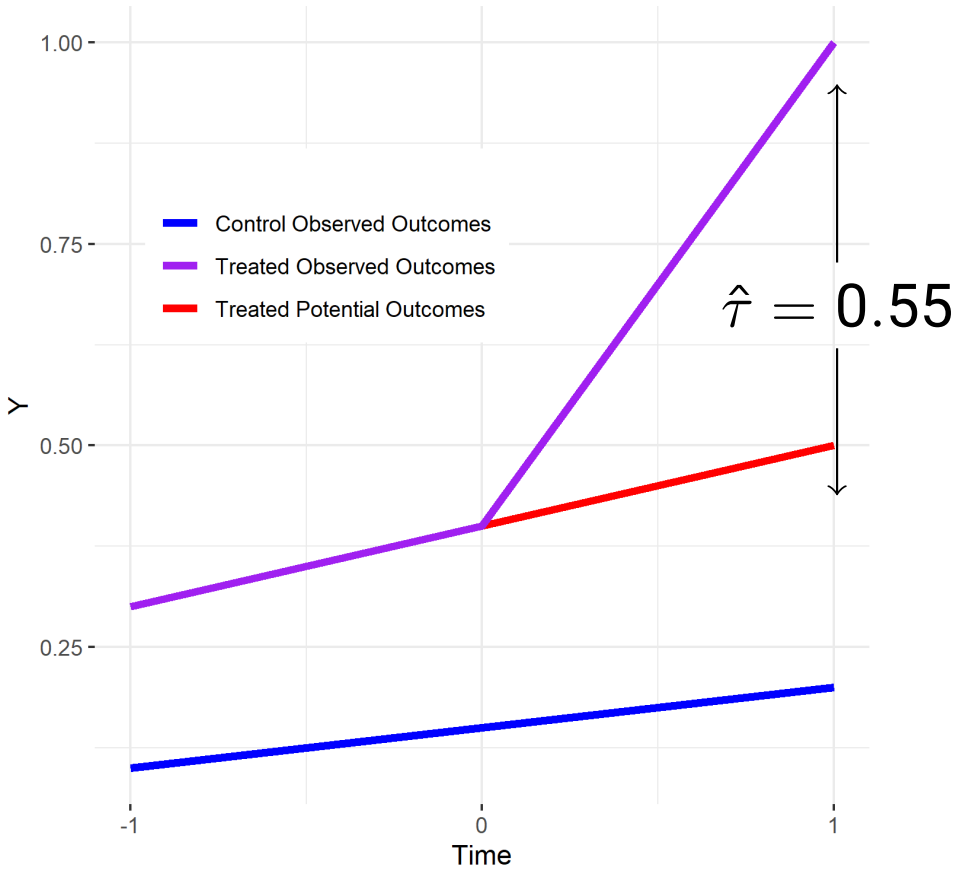
Consider a case where $\tau = 0.5$ and where parallel trends in potential outcomes holds.

Looking at differences between units at each time point, the difference in potential outcomes between treated and control is constant.

If we assessed the pre-trend we would conclude that parallel trends is plausible.

The pre-trend estimate would be ≈ 0.

Pre-Trend Test with Caution

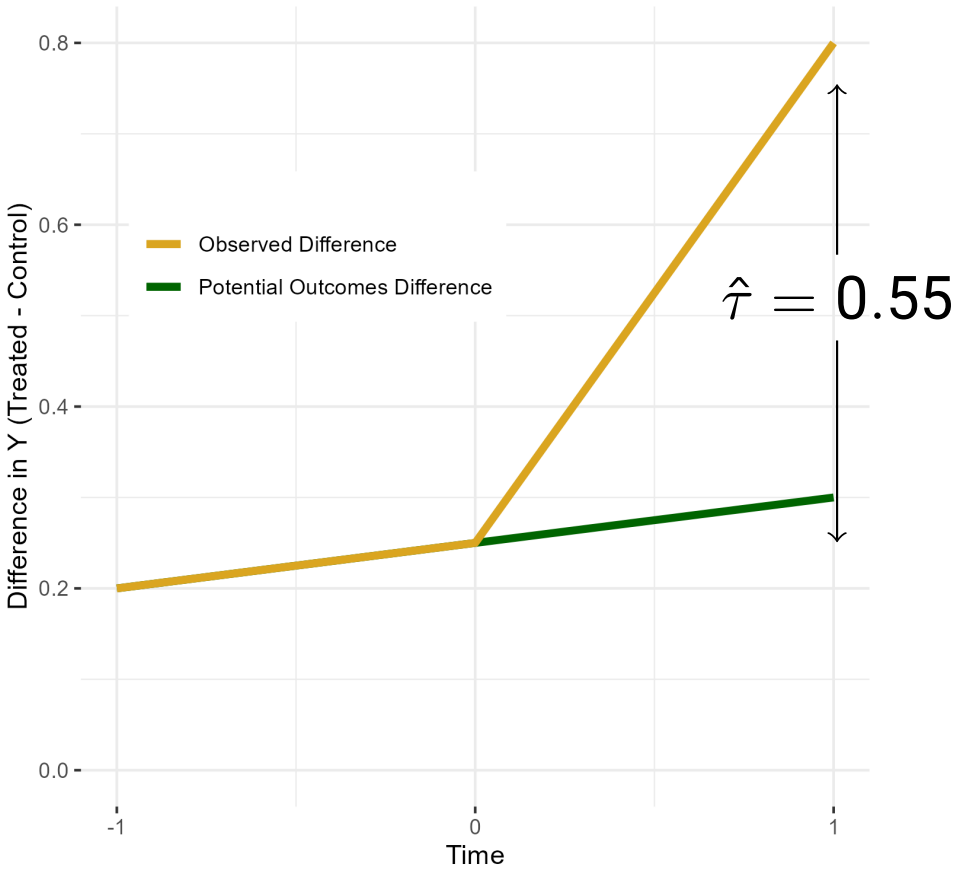


Now consider a case with $\tau = 0.5$ and a non-statistically significant linear violation of parallel trends.

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

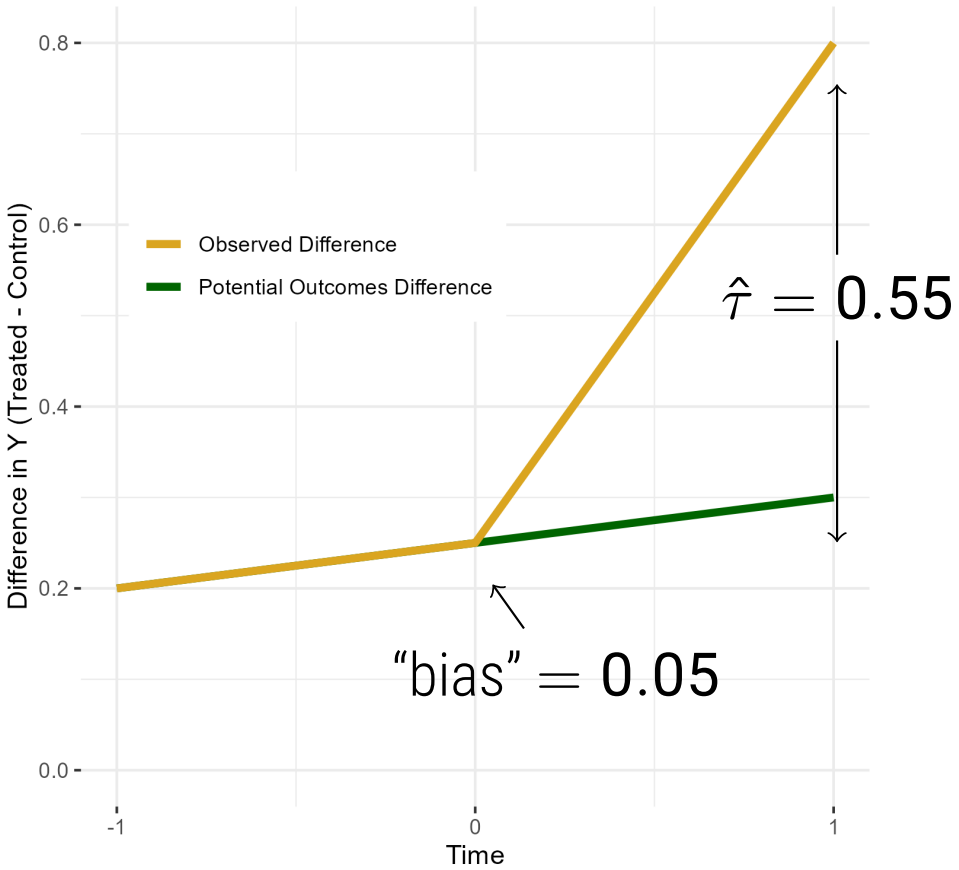


Now consider a case with $\tau = 0.5$ and a non-statistically significant linear violation of parallel trends.

The difference in parallel trends is now increasing in time.

This increasing deviation in potential outcomes misleads us in the post-period.

Pre-Trend Test with Caution



So, Reviewer 2 suggests, we should test for pre-trends!

The magnitude of the pre-trend violation will be $\approx = \mathbf{0.05}$

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 and no anticipation assumptions:

- Treated units, in the absence of treatment, would trend exactly as the control did
- No pre-treatment shifts in outcome as a result of treatment.
- 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.