3. Difference-in-Differences

PhD Applied Methods

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Why difference-in-differences?

- From last lecture: we learned how **randomization** allows us to make causal claims
  - Random assignment $\implies \mathbb{E}[Y_i(0)|D_i = 1] = \mathbb{E}[Y_i(0)|D_i = 0]$
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  - Selection bias is very common
  - Assignment is **not** random

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  - Assignment is **not** random

- **Key question**: Can we still estimate causal effects without randomization?

The fundamental problem

Simple comparison of treated vs. untreated gives us:

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$$= \underbrace{\mathbb{E}[Y_i(1) - Y_i(0)|D_i = 1]}_{\text{ATT}} + \underbrace{\mathbb{E}[Y_i(0)|D_i = 1] - \mathbb{E}[Y_i(0)|D_i = 0]}_{\text{Selection bias}} \tag{3}$$

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Selection bias: treated and untreated groups differ systematically, even in the absence of treatment

Examples of selection bias

- **Minimum wage and employment**:
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- **Job training programs**:
  - Those who enroll may be more motivated or have lower outside options
  - Comparing participants to non-participants conflates selection with treatment
- **New school construction**:
  - Areas with new schools may have higher population growth or income
  - Cannot attribute all differences to the schools themselves

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- Requires observing both groups **before and after** treatment
  - Use the change in the control group to construct the counterfactual for the treated group

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- Requires observing both groups **before and after** treatment
  - Use the change in the control group to construct the counterfactual for the treated group
- **Today's goal**: Understand the mechanics, assumptions, and extensions of DiD

Outline

The naive before-after estimator

One approach: compare the treated group before and after treatment

Suppose we have:

- Period 1 (before treatment): $t = 1$
- Period 2 (after treatment): $t = 2$
- Treatment happens between periods 1 and 2

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Naive before-after estimator:

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Question: What assumption is needed for this to identify the ATT?

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Interpretation: The treated group's outcome (absent treatment) would have been the same in both periods

Problems with before-after

The assumption $\mathbb{E}[Y_{i,2}(0)|D_i = 1] = \mathbb{E}[Y_{i,1}(0)|D_i = 1]$ is very strong!

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- **General time trends**: economic cycles, inflation, technological progress
- **Life-cycle effects**: aging, experience accumulation, depreciation
- **Seasonality**: quarterly or monthly patterns
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Example: NJ minimum wage increase in 1992

- Employment in fast-food rises from 20.4 to 21.0
- If economy is booming $\implies$ underestimates negative effect (or overestimates positive effect)
- If economy is in recession $\implies$ overestimates negative effect

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> **Common Trends (Parallel Trends) Assumption**:
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Formally:

$$\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 1] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 0] \tag{9}$$

Common trends: Intuition

The common trends assumption says:

- Treated and control groups can differ in **levels**
  - $\mathbb{E}[Y_{i,1}(0)|D_i = 1] \neq \mathbb{E}[Y_{i,1}(0)|D_i = 0]$ ✓

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- This allows for:
  - Permanent differences between groups
  - Common time shocks that affect everyone
- This rules out:
  - Group-specific time trends
  - Differential exposure to time-varying shocks

Equivalence: Two ways to state parallel trends

The parallel trends assumption can be stated in two equivalent ways:

1. Changes are equal:

$$\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 1] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 0] \tag{10}$$

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2. Selection bias is constant over time:

$$\underbrace{\mathbb{E}[Y_{i,1}(0)|D_i = 1] - \mathbb{E}[Y_{i,1}(0)|D_i = 0]}_{\text{Selection bias in } t=1} = \underbrace{\mathbb{E}[Y_{i,2}(0)|D_i = 1] - \mathbb{E}[Y_{i,2}(0)|D_i = 0]}_{\text{Selection bias in } t=2} \tag{11}$$

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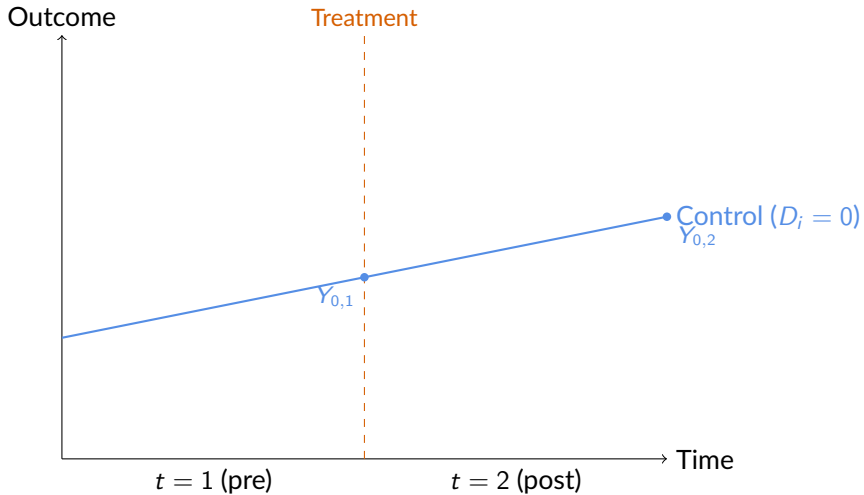
Interpretation: The ”gap” between treated and control (absent treatment) stays constant

Graphical intuition: Difference-in-Differences



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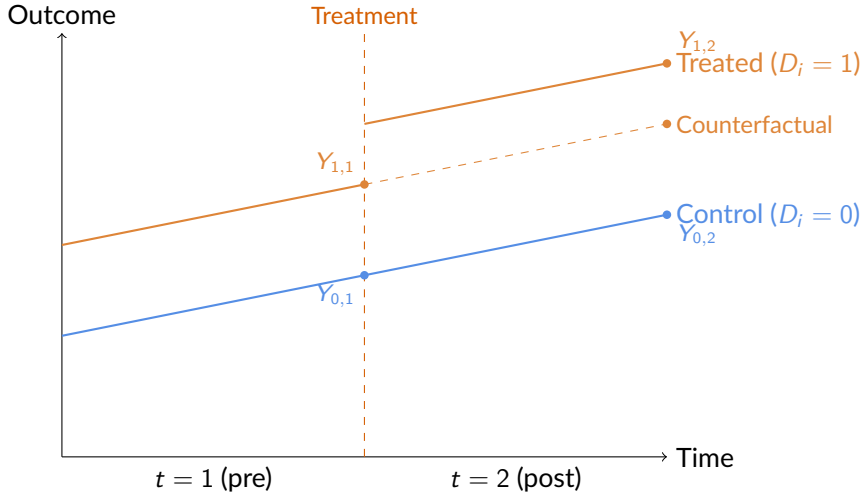

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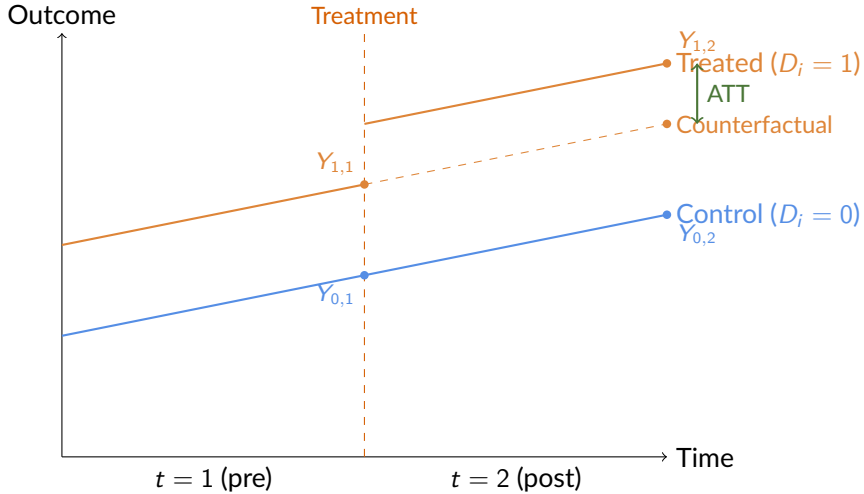

Key insight: The change in the control group gives us the counterfactual trend for the

The 2×2 difference-in-differences estimator

We observe four group-time averages:

	Pre-treatment ($t = 1$)	**Post-treatment** ($t = 2$)
Treated ($D_i = 1$)	$\bar{Y}_{1,1}$	$\bar{Y}_{1,2}$
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The **difference-in-differences estimator** is:

$$\hat{\tau}^{DiD} = (\bar{Y}_{1,2} - \bar{Y}_{1,1}) - (\bar{Y}_{0,2} - \bar{Y}_{0,1}) \tag{12}$$

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- First difference: change in treated group over time
- Second difference: change in control group over time
- DiD: difference between these two changes

Alternative formulation

The DiD estimator can equivalently be written as:

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- First difference: post-treatment difference between treated and control
- Second difference: pre-treatment difference between treated and control
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- Second difference: pre-treatment difference between treated and control
- DiD: how much the gap changed

This formulation makes clear that DiD **differences out** time-invariant differences between groups

What does DiD identify?

Under the parallel trends assumption:

$$\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 1] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 0] \tag{15}$$

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Proof sketch:

$$\mathbb{E}[\hat{\tau}^{DiD}] = \mathbb{E}[Y_{i,2}(1)|D_i = 1] - \mathbb{E}[Y_{i,1}(0)|D_i = 1] \tag{17}$$

$$- (\mathbb{E}[Y_{i,2}(0)|D_i = 0] - \mathbb{E}[Y_{i,1}(0)|D_i = 0]) \tag{18}$$

Proof (continued)

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Add and subtract $\mathbb{E}[Y_{i,2}(0)|D_i = 1]$:

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Under parallel trends, the second term equals zero, so:

$$\mathbb{E}[\hat{\tau}^{DiD}] = \tau_2^{ATT} \tag{23}$$

DiD as a regression

The 2×2 DiD estimator can be implemented via regression:

$$Y_{it} = \alpha + \gamma \cdot \mathbb{1}(D_i = 1) + \lambda \cdot \mathbb{1}(t = 2) + \beta \cdot \mathbb{1}(D_i = 1) \times \mathbb{1}(t = 2) + \varepsilon_{it} \tag{24}$$

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where:

- $\mathbb{1}(D_i = 1)$: dummy for being in treated group
- $\mathbb{1}(t = 2)$: dummy for post-treatment period
- $\mathbb{1}(D_i = 1) \times \mathbb{1}(t = 2)$: interaction (treatment indicator)
- β: the DiD coefficient

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Key result: $\hat{\beta}_{OLS} = \hat{\tau}^{DiD}$

Understanding the regression coefficients

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What do the parameters represent?

	$t = 1$	$t = 2$
$D_i = 0$	α	$\alpha + \lambda$
$D_i = 1$	$\alpha + \gamma$	$\alpha + \gamma + \lambda + \beta$

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- α: baseline outcome (control, pre-period)
- γ: pre-treatment difference between groups
- λ: time trend (common to both groups)
- β: treatment effect (DiD estimator)

Verifying $\hat{\beta} = \hat{\tau}^{DiD}$

From the regression:

$$\beta = \mathbb{E}[Y_{it}|D_i = 1, t = 2] - \mathbb{E}[Y_{it}|D_i = 0, t = 2] \tag{26}$$
$$- (\mathbb{E}[Y_{it}|D_i = 1, t = 1] - \mathbb{E}[Y_{it}|D_i = 0, t = 1]) \tag{27}$$

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Rearranging:

$$\beta = (\mathbb{E}[Y_{it}|D_i = 1, t = 2] - \mathbb{E}[Y_{it}|D_i = 1, t = 1]) \tag{28}$$
$$- (\mathbb{E}[Y_{it}|D_i = 0, t = 2] - \mathbb{E}[Y_{it}|D_i = 0, t = 1]) \tag{29}$$
$$= \hat{\tau}^{DiD} \tag{30}$$

Two-way fixed effects (TWFE) formulation

With panel data, we can rewrite the DiD regression more compactly using fixed effects:

$$Y_{it} = \alpha_i + \delta_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{31}$$

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$$Y_{it} = \alpha_i + \delta_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{31}$$

where:

- α_i: unit fixed effects (captures $\alpha + \gamma \cdot \mathbb{1}(D_i = 1)$ from before)
- δ_t: time fixed effects (captures $\lambda \cdot \mathbb{1}(t = 2)$ from before)
- $D_{it} = \mathbb{1}(D_i = 1) \times \mathbb{1}(t = 2)$: treatment indicator
- β: treatment effect (same as before!)

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Equivalence: This is just a reparameterization of the dummy variable regression

- α_i absorbs all time-invariant unit characteristics
- δ_t absorbs all time-varying shocks common to all units

TWFE: Extending to $T > 2$ periods

The TWFE formulation naturally extends to multiple time periods:

$$Y_{it} = \alpha_i + \delta_t + \beta \cdot D_{it} + \varepsilon_{it}, \quad t = 1, 2, \ldots, T \tag{32}$$

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3. More robust identification (not reliant on single time comparison)

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Parallel trends assumption: $Y_{it}(0) = \alpha_i + \delta_t + \varepsilon_{it}$ for all t

Example: Card & Krueger (1994)

Question: What is the effect of minimum wage on employment?

Setting:

- New Jersey raised minimum wage from $4.25 to $5.05 in April 1992
- Pennsylvania (neighboring state) did not change minimum wage
- Focus on fast-food restaurants (low-wage sector)

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- Focus on fast-food restaurants (low-wage sector)

Data:

- Survey of fast-food restaurants in NJ and PA
- Before (February 1992) and after (November 1992) treatment
- Outcome: full-time equivalent (FTE) employment

Card & Krueger: Results

	Before (Feb 1992)	**After** (Nov 1992)	**Change**
NJ (treated)	20.44	21.03	$+0.59$
PA (control)	23.33	21.17	-2.16
Difference	-2.89	-0.14	

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Interpretation: Minimum wage increase led to a *relative* increase of 2.75 FTE workers in NJ restaurants (contrary to standard theory prediction)

Outline

Extending to multiple time periods

So far: basic 2×2 setup with $t \in \{1, 2\}$

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- Multiple pre-treatment periods: $t < t_0$
- Multiple post-treatment periods: $t \geq t_0$
- Treatment occurs at $t = t_0$

Extending to multiple time periods

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In practice, we often have:

- Multiple pre-treatment periods: $t < t_0$
- Multiple post-treatment periods: $t \geq t_0$
- Treatment occurs at $t = t_0$

Benefits of multiple time periods:

1. Can test the parallel trends assumption using pre-treatment data
2. Can study dynamic treatment effects (how effects evolve over time)
3. Can incorporate more flexible specifications

TWFE with multiple periods

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Interpretation of β:

- Average treatment effect across all treated units and time periods
- Assumes treatment effect is constant over time (homogeneous effects)
- We'll see later this can be problematic with staggered treatment timing

Testing the parallel trends assumption

The fundamental problem:

- Parallel trends is an assumption about **counterfactual** outcomes
- We can never directly observe $Y_{it}(0)$ for treated units after treatment
- So we can never definitively test whether trends would have been parallel

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But: We can check whether trends were parallel **before** treatment!

Pre-trends test:

- If parallel trends holds, we should see no pre-treatment differences in trends
- If we find differential pre-trends, this casts doubt on the assumption
- Not a perfect test, but provides evidence on plausibility

Event study specification

To test for pre-trends and examine dynamic effects, use an **event study** design:

$$Y_{it} = \alpha_i + \delta_t + \sum_{\substack{k=-K \\ k \neq -1}}^{L} \beta_k \cdot \mathbb{1}(t - t_0^i = k) + \varepsilon_{it} \tag{35}$$

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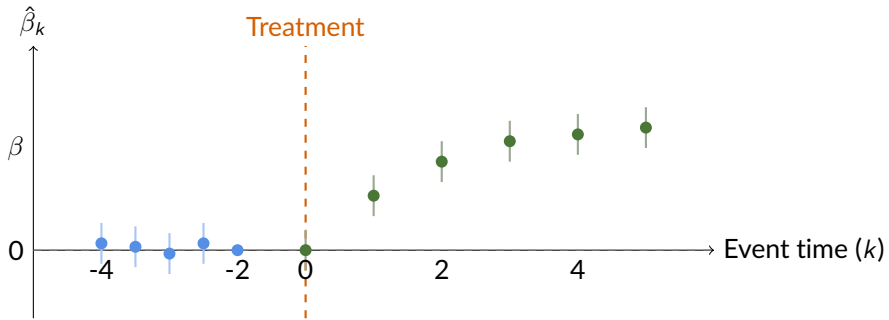
Coefficients:

- β_k for $k < 0$: pre-treatment "effects" (should be zero if parallel trends holds)
- β_k for $k \geq 0$: post-treatment effects (dynamic treatment effects)

Event study: Graphical display

Typical event study plot:



Good pre-trends: Flat, close to zero before treatment (blue dots)
Treatment effects: Jump and evolution after treatment (green dots)

Pre-trends: What to look for

When examining pre-trends, check:

1. **Statistical significance**: Are pre-treatment $\hat{\beta}_k$ significantly different from zero?
   - Test individually: $H_0 : \beta_k = 0$ for each $k < 0$
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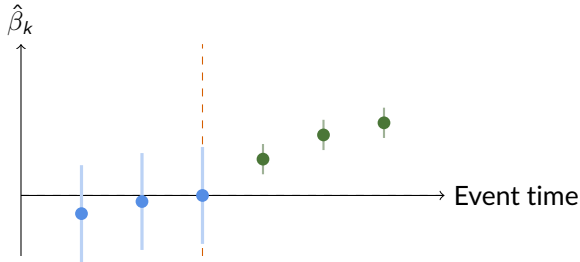
   - Should be able to reject pre-trends as large as the treatment effect
4. **Number of pre-periods**: More pre-periods $\implies$ more power to detect violations
   - With few pre-periods, tests have low power
   - Ideally want multiple pre-periods to credibly test parallel trends

Pre-testing concern 1: **Power** (Roth, 2022)

Problem: Standard pre-trends tests have low power

Consider this event study:



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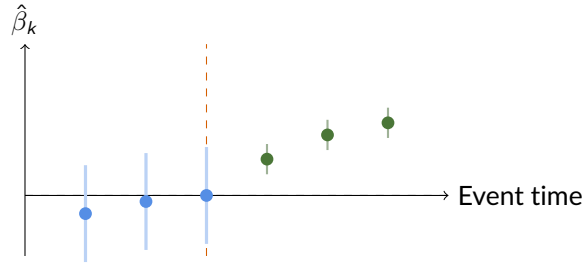


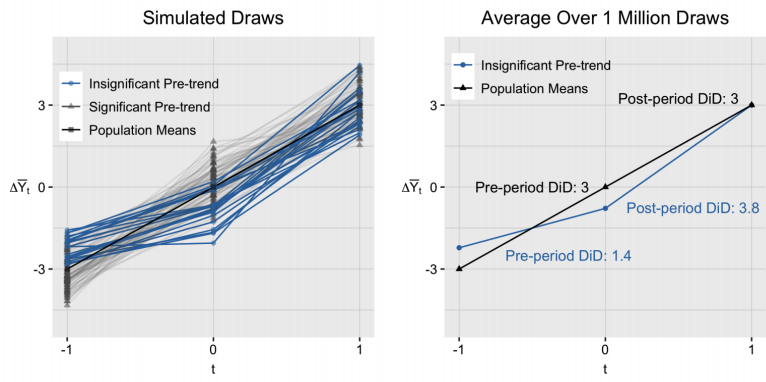
Issue: Pre-trends not significant, but:

- Clear upward trajectory before treatment
- Confidence intervals VERY wide in pre-period

Pre-testing concern 2: **Inference** (Roth, 2022)

The bias from pre-testing:

When researchers select designs based on ”passing” pre-trends tests, this can induce bias:



Ways of dealing with imprecise or differential pre-trends

- **Report pre-trends** - report the size of the pre-trend that can be rejected at conventional levels, and discuss how this compares to the estimated treatment effect (should ideally be able to reject that pre-trend is smaller than the treatment effect)

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- **Bounding estimators** (Rambachan & Roth ReStud 2023) - use information from pre-trends to bound post-trend using an assumption on smooth changes in trends over time

- **Control for linear pre-trends** - you can also just include estimates of linear differential pre-trends in your DiD regression

- Intuitive proposed solution for robustness. Note the post and pre effects:

Rambachan and Roth (2023) suggestion

$$\mathbb{E}[\hat{\beta}_1] = \tau_{ATT} + \underbrace{\mathbb{E}[Y_{t,1}(0) - Y_{t,0}(0)|D_i = 1] - \mathbb{E}[Y_{t,1}(0) - Y_{t,0}(0)|D_i = 0]}_{\text{Post-period differential trend} =: \delta_1}$$

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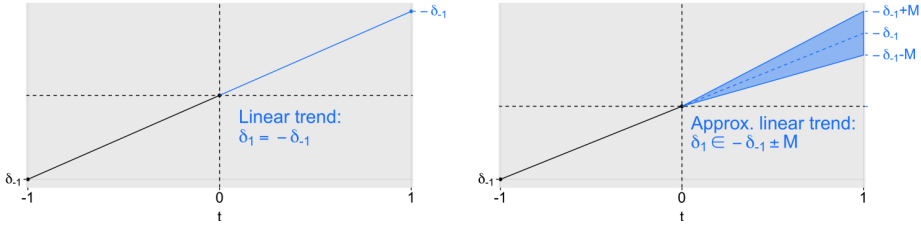
- parallel trends assumes these δ are zero. But pre-trends may not be zero.
  - R&R say: we can use the info from our pre-trends to bound post-trend
  - Use a smoothness assumption, M, on the second derivative. E.g. simple case:



Standard errors in panel DiD

Important: With panel data, standard errors must account for correlation

Problem: Bertrand, Duflo & Mullainathan (2004)

- Outcomes for same unit are serially correlated over time
- ε_{it} and $\varepsilon_{it'}$ are correlated for $t \neq t'$
- Standard OLS standard errors are severely downward biased
- Leads to massive over-rejection of null hypotheses

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Solution: Cluster standard errors at the unit level

- Allows arbitrary correlation within units over time
- Conservative: only assumes independence across units
- In Stata: `reg Y X, cluster(unit_id)`
- In R: `lm_robust(Y ~ X, clusters = unit_id)`
- With fixed effects in R: `feols(Y ~ D | unit + time, cluster = "unit", data = df)`
  from the `fixest` package (fast and flexible clustering specification)

Outline

Main threats to DiD identification

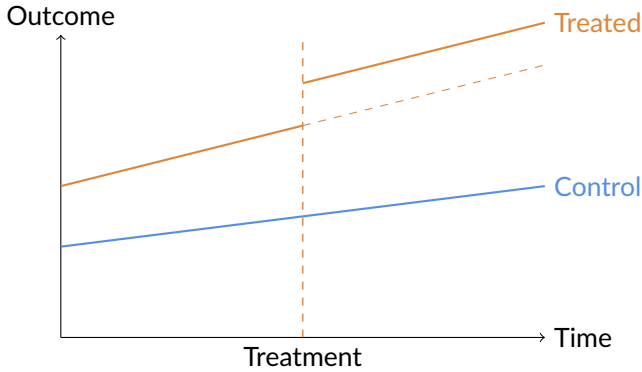
The parallel trends assumption can be violated in several ways:

1. **Differential trends**: Treated and control groups on different trajectories
2. **Differential shocks**: Time-varying shocks that affect groups differently
3. **Selection into treatment** (Ashenfelter's dip)
4. **Anticipation effects**: Behavioral responses before treatment
5. **Spillover effects**: Treatment affects control group
6. **Composition changes**: Different units in cross-sectional DiD
7. **Functional form**: Parallel trends in logs vs. levels

Let's discuss each in turn...

Threat 1: Differential trends

Problem: Treated and control groups on systematically different trajectories



Example: Regions with strong economic growth more likely to get infrastructure investment

Dealing with differential trends

Solutions:

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- γ_i: unit-specific linear time trend
- Allows for different slopes across units
- But: mechanically reduces post-treatment differences

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- Allows for different slopes across units
- But: mechanically reduces post-treatment differences

2. Rambachan & Roth (2023) sensitivity analysis:

- Bound treatment effects under violations of parallel trends
- Assume trend violations can't be "too large"
- Provides robust confidence intervals

Threat 2: Differential shocks

Problem: Time-varying shock affects treated and control groups differently

Example: Minimum wage study

- NJ raises minimum wage; PA does not
- But NJ also experiences a state-specific recession
- Employment falls in NJ for two reasons: minimum wage + recession
- DiD incorrectly attributes recession effect to minimum wage

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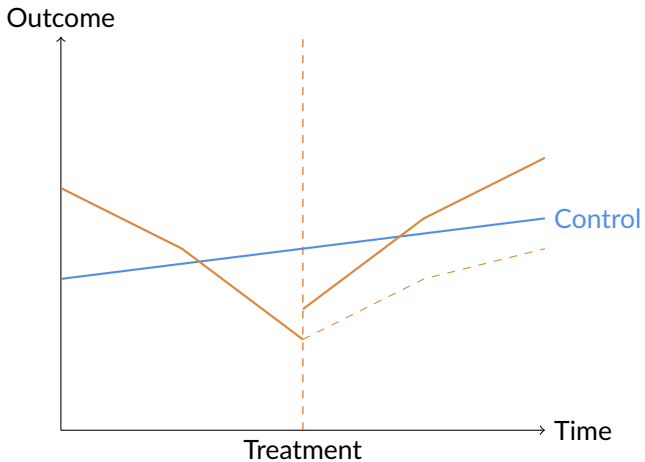
- Employment falls in NJ for two reasons: minimum wage + recession
- DiD incorrectly attributes recession effect to minimum wage

Solutions:

- Find better control groups (similar in all dimensions)
- Use multiple control groups to test robustness
- Look for placebo outcomes (unaffected by treatment)
- Triple differences (if another dimension available)

Threat 3: Ashenfelter's dip

Problem: Units select into treatment precisely because they're on a downward trajectory



Ashenfelter's dip: Evidence and solutions

Named after Ashenfelter (1978):

- Study of job training programs
- Found workers' earnings decline sharply before enrollment
- Then recover after training
- But hard to tell if recovery is due to training or mean reversion

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Critical point: Ashenfelter's dip can be on **unobservables**!

- Even if pre-trends in observed outcomes look parallel, unobserved factors may differ
- Example: Workers enroll when motivation/health declines (unobservable)
- **Fundamentally untestable** — can't see unobservables in pre-period
- Clean pre-trends are reassuring but not definitive proof
- Threatens any setting where units select into treatment

Solutions to Ashenfelter's dip

Solutions:

1. Look for the dip in pre-treatment data (event study)
   - If present in observables, likely worse in unobservables
2. If present, focus on longer pre-treatment differences
3. Match treated units to controls experiencing similar pre-treatment trajectory
4. Use alternative control groups (e.g., future trainees)
5. **Best solution**: Find settings where treatment timing is plausibly exogenous
   - Randomization, policy changes, discontinuities
   - Removes selection-into-treatment concern

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Takeaway: Be skeptical of DiD when treatment is chosen precisely when units need it most

Threat 4: Anticipation effects

Problem: Units change behavior in anticipation of treatment

Example: Tax policy announced in advance

- Firms know corporate tax will increase next year
- Shift profits to current year to avoid higher future tax
- Pre-treatment profits artificially high
- DiD underestimates true revenue effect

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Detection:

- Event study: look for effects in periods immediately before treatment
- "Leads" ($k < 0$) should be zero under no anticipation

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Detection:

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- "Leads" ($k < 0$) should be zero under no anticipation

Solutions:

- Use earlier pre-period as baseline (before announcement)
- Model anticipation explicitly if timing is known

Threat 5: Spillover effects

Problem: Treatment of one group affects outcomes in control group

Example: Job training program

- Treated workers become more productive
- Firms substitute away from untrained workers
- Control group employment falls
- DiD overestimates treatment effect (includes spillover)

Threat 5: Spillover effects

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- Treated workers become more productive
- Firms substitute away from untrained workers
- Control group employment falls
- DiD overestimates treatment effect (includes spillover)

Solutions:

- Choose geographically distant control groups
- Look for evidence of spillovers in untreated outcomes
- Model equilibrium effects explicitly (general equilibrium)
- Acknowledge limitation in interpretation

Threat 6: Composition changes

Problem: In repeated cross-sections, different individuals in each period

Example: Regional minimum wage study

- High-wage workers migrate to treated region after treatment
- Average wage appears to increase
- But this is composition, not causal effect on incumbent workers

Threat 6: Composition changes

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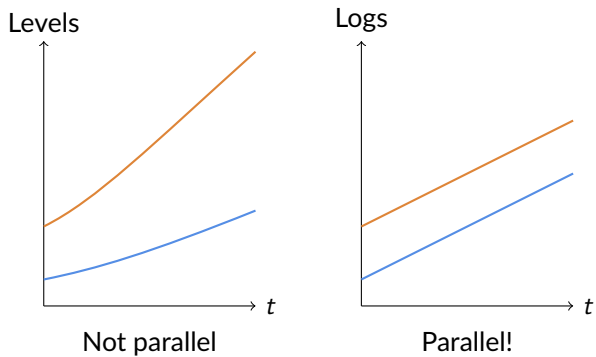
- Average wage appears to increase
- But this is composition, not causal effect on incumbent workers

Solutions:

1. Use panel data (follow same individuals)
2. Test whether observable characteristics change
3. Control for composition using reweighting
4. Focus on intensive margin (hours) not extensive (employment)

Threat 7: Functional form

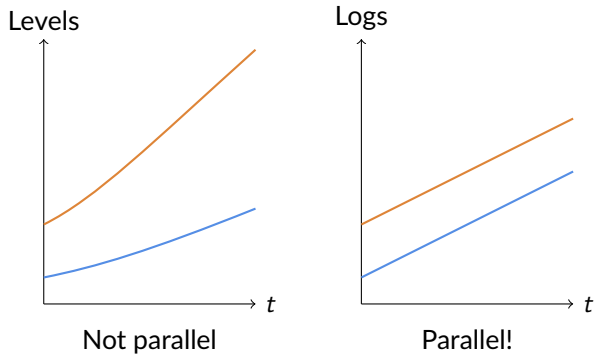
Problem: Parallel trends may hold in one scale but not another



Example: Income growing at constant rates (parallel in logs, not levels)

Threat 7: Functional form

Problem: Parallel trends may hold in one scale but not another



Example: Income growing at constant rates (parallel in logs, not levels)

Functional form and DiD assumptions

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The question: Which functional form assumption is the right one for your setting?

- **Levels**: Additive treatment effects ($Y_{it}(1) = Y_{it}(0) + \tau$)
- **Logs**: Proportional treatment effects ($Y_{it}(1) = Y_{it}(0) \times (1 + \tau)$)

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Assumption:

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where U_i is a time-invariant unobserved heterogeneity term, and $h_t(\cdot)$ is a strictly increasing function that can vary over time

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Intuition: Use the change in the distribution of control group outcomes to construct counterfactual distribution for treated group

Why CIC is not commonly used in practice

1. **Sample requirements**
   - Quantile estimation is noisier than mean estimation
   - Need larger samples for stable distributional estimates

2. **Still quite strong assumptions in practice**
   - The rank invariance assumption (individuals maintain their position in the distribution over time) is quite restrictive
   - May be violated if there's genuine mobility in the outcome distribution
   - Not obviously weaker than parallel trends in all applications

Choosing the right functional form

How to decide?

1. **Economic theory**: Does the treatment have additive or multiplicative effects?
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   - Cash transfer: additive (use levels)

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   - If sensitive, discuss why one specification is preferred

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4. **Be explicit**: State which functional form you assume and why
   - Don't pretend it's a minor technical detail
   - Acknowledge this is a maintained assumption

Outline

Triple differences (DDD)

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- Control states: No program
- Concern: Treated states may have different macro trends (differential shocks)

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Solution: Use men as an additional control group

- Men are not affected by the program (neither in treated nor control states)
- DiD on men captures differential macro shocks between states
- DiD on women captures differential shocks + treatment effect
- Triple difference = DiD_{women} - DiD_{men} isolates treatment effect

Triple differences: Formula

Let:

- $D_s = 1$ for treated state, $= 0$ for control state
- $F_i = 1$ for female, $= 0$ for male
- $t = 1$ (pre), $t = 2$ (post)

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Triple difference:

$$DDD = DiD_F - DiD_M \quad (40)$$

Triple differences: Regression

Can implement via regression:

$$\begin{aligned} Y_{ist} =& \alpha + \beta_1 D_s + \beta_2 F_i + \beta_3 Post_t && (41)\\ &+ \beta_4 (D_s \times F_i) + \beta_5 (D_s \times Post_t) + \beta_6 (F_i \times Post_t) && (42)\\ &+ \beta_7 (D_s \times F_i \times Post_t) + \varepsilon_{ist} && (43) \end{aligned}$$

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Assumption required:

- Men and women in the same state are subject to the same differential shocks
- Common trends for men and women would have been parallel (in differences)

Triple differences: When to use

Advantages:

- Differences out state-specific shocks that affect both genders
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Disadvantages:

- Requires finding a truly ”unaffected” group
- Stronger assumptions (parallel trends for the difference-in-trends)
- Less precise (more differences = more noise)
- Spillovers to ”unaffected” group would bias results

Outline

Moving beyond 2×2: Staggered adoption

So far: single treatment period (t_0), all treated units adopt simultaneously

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Benefits of staggered rollout:

1. More robust to macro shocks
   - Units treated at different times face different macro conditions
   - Differential shocks less likely to confound all comparisons
2. Can use earlier-treated as controls for later-treated (and vice versa)

Example: Yagan vs. Goodman-Bacon

Yagan (2015): Effect of capital gains tax cuts on entrepreneurship

- Treatment: State-level capital gains tax cuts
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Better design: Staggered rollout across years

- Some states cut in 1990, others in 1992, others in 1995...
- Macro shocks in different years unlikely to align with treatment
- More credible parallel trends assumption

The problem with TWFE and staggered timing

Historically: Researchers used TWFE for staggered DiD

$$Y_{it} = \alpha_i + \delta_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{44}$$

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Recent discovery: This doesn't work with:

1. Staggered treatment timing, AND
2. Heterogeneous treatment effects

Goodman-Bacon decomposition (2 treatment times)

Goodman-Bacon (2021): Special case with 2 treatment times

With two treatment cohorts (early and late) plus never-treated, TWFE is a weighted average of three 2×2 comparisons:

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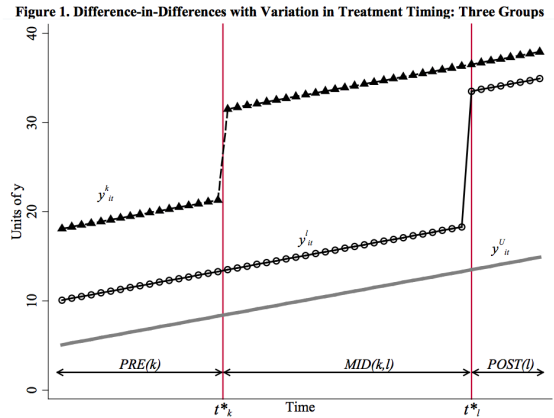
3. **Later-treated vs. earlier-treated**
   - Weight: variance share
   - Sign: can be negative (forbidden comparison!)
   - Earlier-treated serves as ”control” for later-treated
   - But earlier-treated is already experiencing treatment effects

Goodman-Bacon 2×2 comparisons

- Consider two staggered treatments and a never-treated group
- What does the TWFE estimator estimate?
- TWFE decomposes into all possible 2×2 comparisons



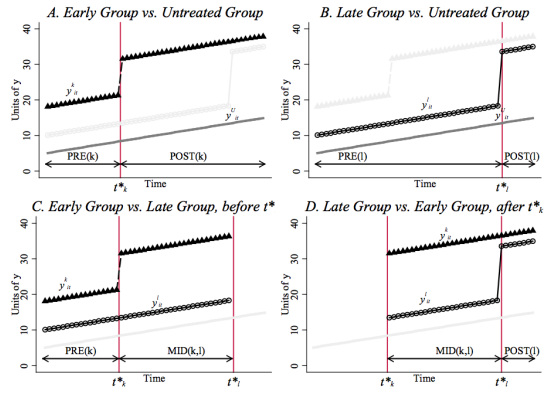
Goodman-Bacon 2×2 comparisons

- Four potential comparisons:
  1. Early vs. never
  2. Late vs. never
  3. Early vs. late (pre-treatment)
  4. Late vs. early (post-treatment) **forbidden!**

- TWFE is weighted average of all 2×2 comparisons

- Weights depend on variance in treatment (so maximised when time in treated vs control is closest to 0.5; i.e. when treatment occurs close to middle of the data's time period)

Figure 2. The Four Simple (2x2) Difference-in-Differences Estimates from the Three Group Case



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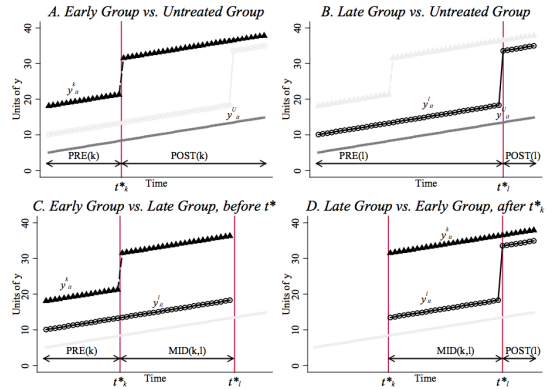
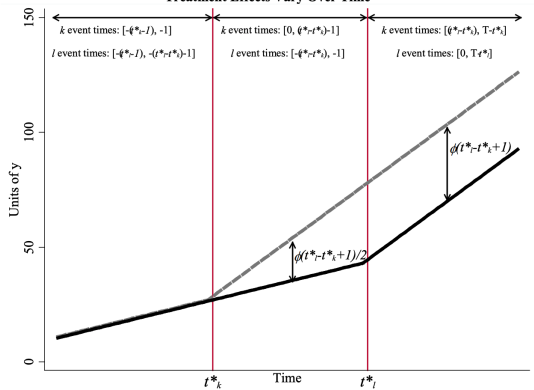


The forbidden comparison problem

- Weighting becomes problematic if effects vary over time
- With time-varying effects, already-treated units are bad controls
- This creates negative weights on some treatment effects
- Goodman-Bacon decomposition reveals how much weight is problematic

Figure 3. Difference-in-Differences Estimates with Variation in Timing Are Biased When Treatment Effects Vary Over Time



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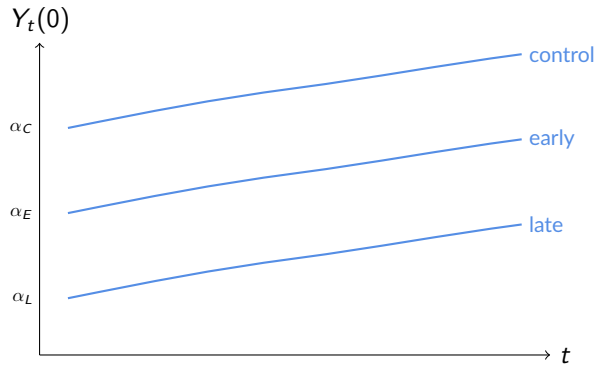
Potential source of problems: Treatment effects are **dynamic** (grow with exposure):

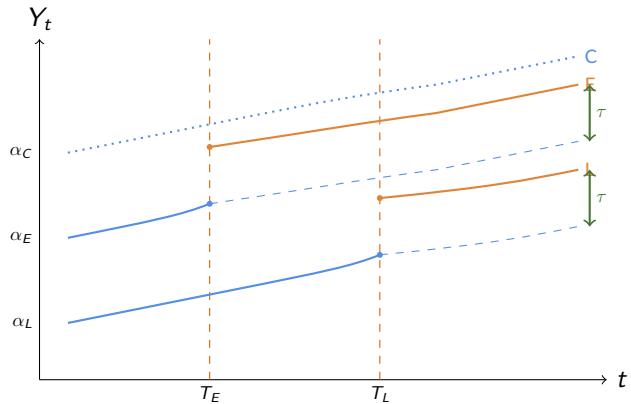
- Effect is τ_l in first period after treatment
- Effect grows to τ_h in second period after treatment (where $\tau_h > \tau_l$)
- Problem: Group E has been treated longer at $t = 3$ than Group L

Parallel trends: All groups share common $g(t)$



All three curves have same shape (parallel) but different levels ($\alpha_C > \alpha_E > \alpha_L$)

With constant treatment effects: τ (no dynamics)



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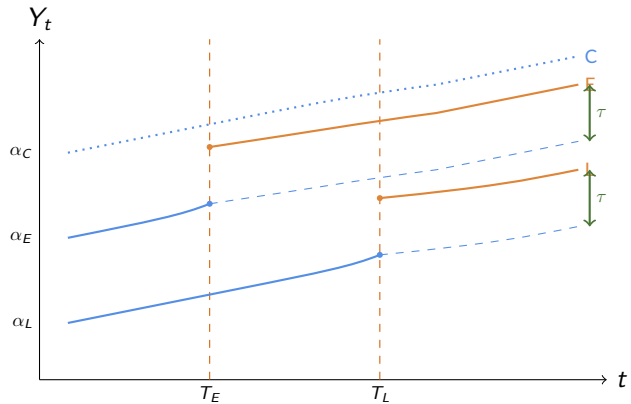

Constant treatment effects: τ remains the same over time (no problem for TWFE)

Why constant effects are not a problem

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With constant effects, Group E has effect τ at both T_E and T_L.

The forbidden comparison now gives:

$$\begin{aligned}
\hat{\tau}^{DID}_{L \text{ vs } E} &= [Y_{L,T_L} - Y_{L,T_E}] - [Y_{E,T_L} - Y_{E,T_E}] \\
&= [(\alpha_L + g(T_L) + \tau) - (\alpha_L + g(T_E))] \\
&\quad - [(\alpha_E + g(T_L) + \tau) - (\alpha_E + g(T_E) + \tau)] \\
&= [g(T_L) - g(T_E) + \tau] - [g(T_L) - g(T_E)] \\
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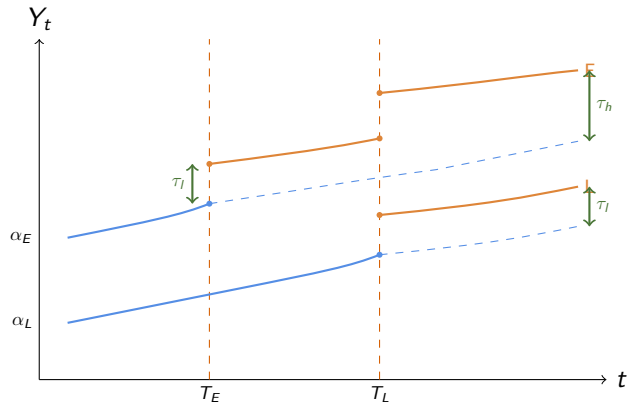
&= \tau
\end{aligned}$$

Key insight: With constant effects, all valid comparisons give the same answer τ

- No contamination from using already-treated units as controls
- TWFE works fine when treatment effects don't change over time

With dynamic treatment effects: $\tau_l < \tau_h$



Dynamic treatment effects: Effects grow from τ_l to τ_h with exposure (problem for TWFE!)

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TWFE's implicit estimate from this comparison:

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&\quad - [\alpha_E + g(T_L) + \tau_h - \alpha_E - g(T_E) - \tau_l] \\
&= \tau_l - (\tau_h - \tau_l) \\
&= 2\tau_l - \tau_h
\end{aligned}$$

What TWFE estimates with dynamic effects

$$\hat{\tau}^{TWFE}_{L \text{ vs } E} = 2\tau_l - \tau_h$$

Problem: If $\tau_l < \tau_h$, this can give a **negative** estimate!

- Even though treatment has a positive effect at all horizons
- TWFE uses already-treated Group E (with large effect) as ”control”
- Contaminates the estimate with **heterogeneity over time**: $2\tau_l - \tau_h$
- Really we care about the ATT or ATE $\approx (\tau_l + \tau_h)/2$. At the very least, τ_h should not be counting negatively towards our estimate of treatment effects!

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Problem: Some weights $w_{g,t}$ can be **negative**!

General case: Many groups, many time periods

de Chaisemartin & D'Haultfoeuille (2020): Extends to general staggered timing

With many treatment cohorts and many time periods, TWFE estimates:

$$\hat{\beta}^{TWFE} = \sum_{g,t} w_{g,t} \cdot ATT_{g,t} \tag{45}$$

- $ATT_{g,t}$: average treatment effect for cohort g at time t
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Problem: Some weights $w_{g,t}$ can be **negative**!

Implication:

- $\hat{\beta}^{TWFE}$ can be negative even if all $ATT_{g,t} > 0$
- Or vice versa: $\hat{\beta}^{TWFE} > 0$ even if all $ATT_{g,t} < 0$
- Cannot interpret $\hat{\beta}^{TWFE}$ as a meaningful average
- Their diagnostic tool shows how much negative weight in your data

Solutions to staggered timing problem

Don’t use standard TWFE with staggered timing + heterogeneous effects!

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   - Compute clean 2×2 DiDs for each cohort-time pair
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3. **Sun & Abraham (2021)**:
   - Event-study approach with interaction-weighted estimator
   - Clean estimates of dynamic effects by cohort

Callaway & Sant'Anna (2021) estimator

Basic approach: Explicitly exclude any forbidden comparisons.

1. Define cohorts by treatment timing: $g \in \{2, 3, \ldots, T, \infty\}$
   - $g = t$ if unit first treated at time t
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$$ATT(g, t) = \mathbb{E}[Y_t - Y_{g-1} | G_i = g] - \mathbb{E}[Y_t - Y_{g-1} | G_i = C_t] \tag{46}$$

where C_t is comparison group (never-treated or not-yet-treated at t)

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$$ATT_{overall} = \sum_{g,t} w_{g,t} \cdot ATT(g,t) \tag{47}$$

   with explicit, non-negative weights $w_{g,t}$. These could be e.g. proportional to the number of units in each cell.

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Key advantage: Transparent about what's being compared and weighted.

Note: Fuzzy DiD and partial treatment

Important: The staggered timing problem also applies to **fuzzy DiD**

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Fuzzy DiD: When the ”control” group is partially treated

- Treatment occurs in the treatment group, but also (to lesser extent) in control group
- Example: Policy rollout affects neighboring regions
- Example: Media coverage spills over to control areas

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Why this matters:

- If control group has small treatment effect $\tau_C > 0$
- And treatment group has larger effect $\tau_T > \tau_C$
- Standard DiD estimates $\tau_T - \tau_C$, not τ_T
- This is the same issue as forbidden comparisons!
- Comparing ”more treated” vs. ”less treated” rather than ”treated” vs. ”untreated”

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Takeaway: Be careful about control group contamination and partial treatment

Practical recommendations

If you have staggered treatment timing:

1. **Check for heterogeneity**:
   - Run event study: do effects vary across cohorts or over time?
   - If yes, standard TWFE is problematic

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4. **Be transparent**:
   - Document which comparisons are being made
   - Show event studies by cohort if heterogeneity is present
   - Discuss sensitivity to choice of comparison group

Outline

Synthetic control methods

Motivation: What if parallel trends doesn't hold for all control units?

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Key idea: Construct a **weighted combination** of control units that best matches the treated unit pre-treatment:

$$\tau = \underbrace{Y_{post}(1)}_{\text{Fully observed}} - \underbrace{\hat{Y}_{post}(0)}_{\text{Constructed}} \tag{48}$$

Synthetic Control example (Abadie et al., 2010))

- Consider following problem: California bans smoking in 1989. What does that do to smoking?
  - Define estimand: $\tau_{ban,CA} = Y_{california,post}(1) - Y_{california,post}(0)$
  - This is the effect of the California smoking ban
  - How can we get at it?
- We need a "synthetic California" as our control
- In an ideal world, the average of the other states would work – however, not clear empirically that they are a good counterfactual

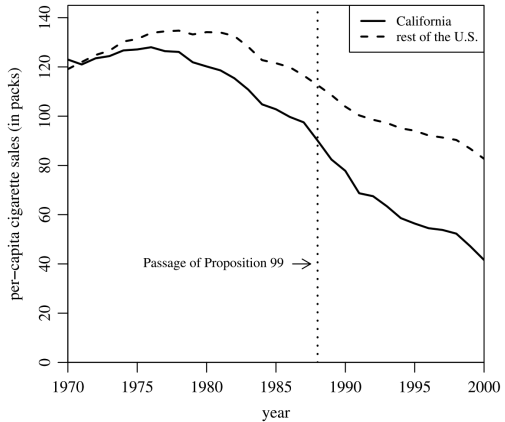

Figure 1. Trends in per-capita cigarette sales: California vs. the rest of the United States.

Synthetic control: basic method

Method (Abadie et al. 2010):

- Choose weights $\omega_j \geq 0$, $\sum_j \omega_j = 1$
- Estimate counterfactual untreated California using a weighted sum of other states that "look like" California (the synthetic California)

$$\hat{Y}_{\text{post,treated}}(0) = \sum_j \omega_j Y_{\text{post,control}}$$

- Select weights to make minimize the distance in terms of pre-treatment covariates:

$$\{\hat{\omega}\}_i = \arg\min_{\boldsymbol{W}} ||\boldsymbol{X}_{\text{treat}} - \boldsymbol{X}_{\text{control}}\boldsymbol{W}||$$

The synthetic control method (Abadie et al. (2010)

- This approach can be incredibly successful

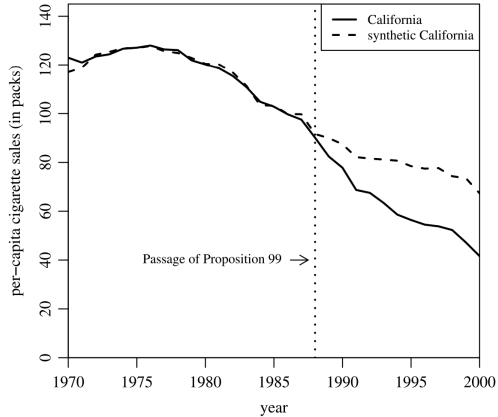

Figure 2. Trends in per-capita cigarette sales: California vs. synthetic California.

The synthetic control method (Abadie et al. (2010)

- This approach can be incredibly successful
- By careful construction of a synthetic control, can calculate counterfactual impacts due to policy
- Still subject to same caveats from DinD – not invariant to some transformations (e.g. log and linear)

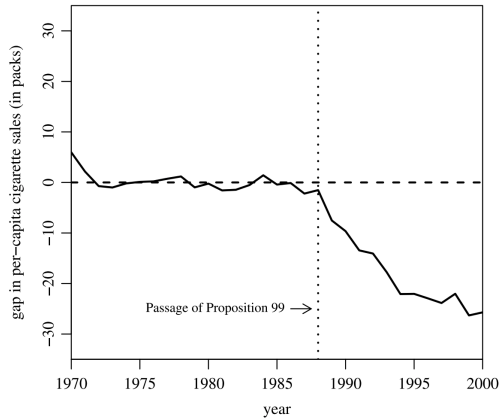

Figure 3. Per-capita cigarette sales gap between California and synthetic California.

Extension to synthetic DiD

Arkhangelsky et al. (2021): Combine unit weights and time weights

- **Unit weights (ω_i)**: reweight controls so that their pre-outcomes match the treated units' pre-outcomes (same as synthetic control)
- **Time weights (λ_t)**: reweight time so that for the controls, the pre-period looks like the post period
  - This soaks up aggregate trends
  - E.g., if there was a macroeconomic recession in the post-period, want to weight pre-periods more if there was a recession in those periods

Then do a **DiD** on this reweighted setup.

▸ More details on synthetic methods

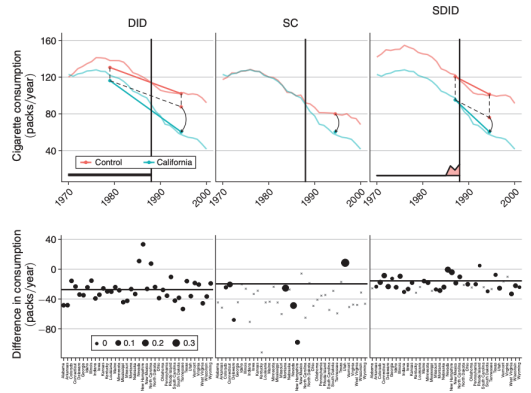

FIGURE 1. A COMPARISON BETWEEN DID, SC, AND SDID ESTIMATES FOR THE EFFECT OF CALIFORNIA PROPOSITION 99 ON PER-CAPITA ANNUAL CIGARETTE CONSUMPTION (IN PACKS/YEAR)

Summary: The DiD toolkit

Basic DiD (2×2 design):

- Parallel trends assumption: $\mathbb{E}[Y_{it}(0) - Y_{it'}(0)|D_i = 1] = \mathbb{E}[Y_{it}(0) - Y_{it'}(0)|D_i = 0]$
- Identifies ATT under parallel trends
- Can implement via regression: $Y_{it} = \alpha_i + \delta_t + \beta \cdot D_{it} + \varepsilon_{it}$

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Key threats:

- Differential trends, differential shocks, Ashenfelter's dip
- Anticipation, spillovers, composition, functional form

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Extensions:

- Triple differences for additional robustness
- Event studies for dynamic effects (with caution on pre-trends)

Summary: Recent developments

Staggered treatment timing:

- Standard TWFE can give **negative weights** with heterogeneous effects
- “Forbidden comparisons”: later-treated vs. already-treated
- **Solutions**: Callaway & Sant’Anna (2021), de Chaisemartin & D’Haultfoeuille (2020), Sun & Abraham (2021)

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Synthetic control methods:

- When parallel trends may not hold for all controls
- Construct weighted combination matching pre-treatment characteristics
- Transparent, data-driven approach to control group construction
- Best for few treated units with rich pre-treatment data

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Synthetic control methods:

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- Construct weighted combination matching pre-treatment characteristics
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Key takeaway: Choice of method depends on your setting, data structure, and assumptions you're willing to make

Practical advice

1. **Always visualize your data**:
   - Plot trends for treatment and control groups
   - Show event studies (but interpret pre-trends carefully)
   - Make the parallel trends assumption transparent

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   - Report both TWFE and robust estimators
4. **Document your choices**:
   - Which comparison groups are being used
   - How you handle standard errors (clustering level)
   - Sensitivity to key decisions

Thank you!

Questions?

Generalized panel setup

Consider a panel with T time periods and $N + 1$ units. Intervention D_{it} at time T_0 for one unit (unit $i = 0$).

Let $\mathbf{Y}_{a,b}$ denote outcomes for $a \in \{\text{treated}, \text{control}\}$ and $b \in \{\text{pre}, \text{post}\}$:

$$\mathbf{Y} = \begin{pmatrix} \mathbf{Y}_{t,\text{post}} & \mathbf{Y}_{c,\text{post}} \\ \mathbf{Y}_{t,\text{pre}} & \mathbf{Y}_{c,\text{pre}} \end{pmatrix} = \begin{pmatrix} \mathbf{Y}_{t,\text{post}}(1) & \mathbf{Y}_{c,\text{post}}(0) \\ \mathbf{Y}_{t,\text{pre}}(0) & \mathbf{Y}_{c,\text{pre}}(0) \end{pmatrix}$$

Key insight: We need to estimate $\mathbf{Y}_{t,\text{post}}(0)$, the counterfactual for the treated unit(s) in the post period.

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Synthetic DiD: The estimator

Standard DiD:

$$(\hat{\alpha}, \hat{\gamma}, \hat{\tau}) = \arg\min \sum_{i,t} (Y_{it} - \alpha_i - \gamma_t - D_{it}\tau)^2$$

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Synthetic Control:

$$(\hat{\gamma}, \hat{\tau}) = \arg\min \sum_{i,t} (Y_{it} - \gamma_t - D_{it}\tau)^2 \hat{\omega}_i$$

where $\hat{\omega}_i$ chosen to match pre-treatment characteristics

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$$(\hat{\alpha}, \hat{\gamma}, \hat{\tau}) = \arg\min \sum_{i,t} (Y_{it} - \alpha_i - \gamma_t - D_{it}\tau)^2 \hat{\omega}_i \hat{\lambda}_t$$

where both $\hat{\omega}_i$ (unit weights) and $\hat{\lambda}_t$ (time weights) are data-driven

Generalized estimator form

Consider estimators of the form:

$$\hat{Y}_{t,\text{post}}(0) = \mu + \sum_{j \in \text{controls}} \omega_j Y_{j,T}$$

Components:

- μ: Constant allowing for level differences (common in DiD)
- ω_j: Weights that vary across control units
  - Simple average would be standard DiD
  - Different weights allow more flexibility

Question: How should we choose the weights ω_j?

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Synthetic control weight restrictions

Abadie, Diamond, Hainmueller (2010) impose three restrictions:

1. $\mu = 0$ (no intercept)
2. $\sum_j \omega_j = 1$ (weights sum to one)
3. $\omega_j \geq 0 \; \forall j$ (non-negative weights)

Interpretation:

- These create a counterfactual whose outcomes are **within the convex hull** of control units
- Treated unit is a weighted average of a subset of control states
- More transparent than allowing negative weights or extrapolation

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Formal weight estimation

Weights ω_j are chosen by minimizing distance between covariates in pre-period:

$$\{\hat{\omega}_j\}_j = \arg\min_{\mathbf{W}} ||\mathbf{X}_{\text{treat}} - \mathbf{X}_{\text{control}}\mathbf{W}||$$

subject to $\sum_j \omega_j = 1$ and $\omega_j \geq 0$.

Crucial feature: **X** can include:

- Lagged outcomes: $Y_{i,t-1}, Y_{i,t-2}, \ldots$
- Time-invariant covariates: demographics, geography, etc.
- Time-varying covariates

Re-envision the panel:

- **Observed outcomes**: $\mathbf{Y}_{t,\text{post}}(1)$, $\mathbf{Y}_{c,\text{post}}(0)$
- **Covariates/predictors**: $\mathbf{Y}_{t,\text{pre}}(0)$, $\mathbf{Y}_{c,\text{pre}}(0)$, $\mathbf{X}_t$, $\mathbf{X}_c$

This is fundamentally a **matching problem** using many characteristics!

Inference with synthetic control

Challenge: With only one treated unit, standard large-sample asymptotics don't apply.

Standard approach: Placebo/permutation tests

- Apply synthetic control method to **each potential control unit**
- Compute "placebo effects" for untreated units
- Compare actual treatment effect to distribution of placebo effects
- Similar to randomization inference

Interpretation:

- If treatment effect is large relative to placebos $\Rightarrow$ evidence of real effect
- If treatment effect is in middle of distribution $\Rightarrow$ could be noise

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Staggered adoption with synthetic DiD

Issue: So far, synthetic control/DiD focused on single adoption period

- Staggered adoption isn't as natural for synthetic control
- How can we adapt it?

Solution (following Callaway & Sant'Anna approach):

- Split up adoption timings by cohort
- Estimate synthetic DiD separately for each (g, t) pair
  - g = adoption cohort
  - t = time period
- Aggregate cohort-time effects

Advantage: Allows for heterogeneous treatment effects across cohorts and time while maintaining synthetic control benefits

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Practical considerations and skepticism

Why limited adoption despite being "cool"?

Challenges:

- Strong structural assumptions
  - Not clear we have good tests yet
  - Pre-trends in DiD felt more testable/transparent
- Researcher degrees of freedom
  - Choice of covariates to match on
  - Which control units to include
  - How to weight different matching variables
  - True in DiD too, but perhaps less transparent?

Alternative interpretation:

- Maybe DiD is equally problematic, but we're not aware of it
- If we accept DiD is sensitive to functional form, then ML methods that construct counterfactuals are natural

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Practical recommendations

When to use synthetic control:

- **Ideal**: Single treatment event ("big bang")
- Get a good synthetic control for treatment unit
- If no good match exists in pre-period, stop (or adjust)
- **Better approach**: Ben-Michael, Feller & Rothstein (2021) adjust for imperfect pre-match

When to use synthetic DiD:

- Promising generalization for multiple treated units
- Key challenge: Convince readers why this works better than traditional DiD
- Recommendation: Show results with **both** DiD and synthetic DiD

Software packages:

- `augsynth`: Augmented synthetic control (Ben-Michael et al.)
- `synthdid`: Synthetic DiD (Arkhangelsky et al.)
- `tidysynth`: User-friendly synthetic control
- Original `synth` package is tougher to use

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Temporary page!

LaTeX was unable to guess the total number of pages correctly. As there was some unprocessed data that should have been added to the final page this extra page has bee added to receive it.

If you rerun the document (without altering it) this surplus page will go away, because LaT now knows how many pages to expect for this document.