3. Difference-in-Differences

PhD Applied Methods

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Spring 2026

Quick IV review: Compliance types are **fixed** attributes

Recall: an individual's compliance type is defined by the pair of **potential treatments**:

	$T_i(0) = 0$	$T_i(0) = 1$
$T_i(1) = 0$	Never-taker	Defier
$T_i(1) = 1$	**Complier**	Always-taker

- This is a **fixed characteristic** of each person — does not depend on realized Z_i
- Avoid "$T_i = 1$ when $Z_i = 1$" — this is a *realized outcome*, not a *type*
- Correct: $T_i(1) = 1$ and $T_i(0) = 0 \implies$ Complier

Also: when computing type shares from a joint distribution table, **condition**:

$$P(T=1 \mid Z=1) = \frac{P(T=1,\, Z=1)}{P(Z=1)} \neq P(T=1,\, Z=1)$$

Defining a parameter $\neq$ identifying it from data

A common pattern in causal inference:

- It is often easy to *define* the causal parameter you want
- It is much harder to show you can *identify* it from observable data

Example: Suppose you want to estimate a treatment spillover effect on the untreated.

- You might try comparing untreated individuals in treated vs. control groups
- But **who are the untreated?** In each group, the composition differs:
  - In a treated group: untreated = never-takers only (compliers took up treatment)
  - In a control group: untreated = never-takers + compliers
- So the naive comparison confounds the spillover with a **composition effect**

Lesson: Always ask: *who is in each comparison group?* If the type composition differs, your estimator may not identify your target parameter — even if it is well-defined.

Different instruments $\Longrightarrow$ different IV estimates

Angrist & Imbens (1995) show that with **multi-valued treatment**, IV identifies a weighted average of margin-specific LATEs — and the **weights depend on the instrument**.

Concrete example: Two instruments for years of schooling

Instrument	Compliers	Margin
Compulsory schooling laws	Students pushed from dropping out to staying	$8 \to 9$, $9 \to 10$ years
College proximity	Students induced to attend college	$12 \to 13$, $13 \to 16$ years

- Both are valid instruments for "the effect of education on earnings"
- But they identify effects for **different complier populations** at **different margins**
- Different IV estimates $\neq$ one instrument is invalid

Different instruments $\Longrightarrow$ different IV estimates (cont.)

Why does this happen?

- With heterogeneous treatment effects, the return to an extra year of schooling differs across individuals and across margins (e.g., $8 \to 9$ vs. $15 \to 16$)
- Each instrument affects a *different set of margins* — so it weights different LATEs
- The IV estimate is a **weighted average** of these margin-specific effects, with weights determined by how much the instrument shifts treatment at each margin

Implication: IV estimates are *instrument-dependent*. When comparing IV results across studies, always ask: who are the compliers, and which treatment margin is being affected?

Why difference-in-differences?

So far in this course:

- **RCTs** give us clean causal effects — but often infeasible or unethical
- **Instrumental variables** give us causal effects without randomizing treatment directly — but good instruments are hard to find, and IV only identifies the **LATE**, not the ATE

Today: **Difference-in-Differences (DiD)**

- Applicable when there is both **temporal** and **cross-sectional** variation in treatment exposure
- Identifies causal effects under a **stronger assumption** (parallel trends) — but one that is often plausible in policy settings
- One of the most widely used methods in applied economics

The fundamental problem

Simple comparison of treated vs. untreated gives us:

$$\mathbb{E}[Y_i|D_i = 1] - \mathbb{E}[Y_i|D_i = 0] \tag{1}$$

But this is **not** the causal effect! Why not?

$$\mathbb{E}[Y_i|D_i = 1] - \mathbb{E}[Y_i|D_i = 0] \tag{2}$$

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

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

Enter: Difference-in-Differences

- **Difference-in-Differences (DiD)** is one of the most widely used methods in applied econometrics
- **Key insight**: Even if treated and control groups differ in levels, we can still identify causal effects if they share common trends
- 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

Naive before-after estimator:

$$\hat{\tau}^{BA} = \mathbb{E}[Y_{i2}|D_i = 1] - \mathbb{E}[Y_{i1}|D_i = 1] \tag{4}$$

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

The naive before-after estimator

Recall the ATT is:

$$\tau_2^{ATT} = \mathbb{E}[Y_{i,2}(1) - Y_{i,2}(0)|D_i = 1] \tag{5}$$

The before-after estimator gives us:

$$\hat{\tau}^{BA} = \mathbb{E}[Y_{i2}|D_i = 1] - \mathbb{E}[Y_{i1}|D_i = 1] \tag{6}$$

$$= \mathbb{E}[Y_{i,2}(1)|D_i = 1] - \mathbb{E}[Y_{i,1}(0)|D_i = 1] \tag{7}$$

This equals the ATT if and only if:

$$\mathbb{E}[Y_{i,2}(0)|D_i = 1] = \mathbb{E}[Y_{i,1}(0)|D_i = 1] \tag{8}$$

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!

It rules out:

- **General time trends**: economic cycles, inflation, technological progress
- **Life-cycle effects**: aging, experience accumulation, depreciation
- **Seasonality**: quarterly or monthly patterns
- **Mean reversion**: regression to the mean

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

The common trends assumption

Key insight: We can relax the before-after assumption by using a control group

Instead of assuming treated outcomes are constant over time, we assume:

> **Common Trends (Parallel Trends) Assumption**:
> In the absence of treatment, the average outcomes for the treated and control groups would have evolved in parallel

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**
  - $\mathbb{E}[Y_{i,1}(0)|D_i = 1] \neq \mathbb{E}[Y_{i,1}(0)|D_i = 0]$ ✓
- But they must have the same **change over time** (absent treatment)
  - $\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 1] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 0]$
- 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:

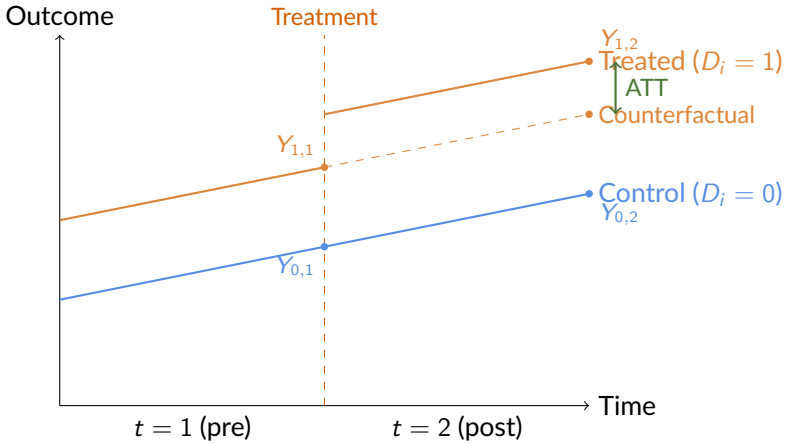
$$\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 1] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 0] \tag{10}$$

2. Selection bias is constant over time:

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

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

Graphical intuition: Difference-in-Differences



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

The 2×2 difference-in-differences estimator

We observe four group-time averages:

	Pre-treatment ($t = 1$)	**Post-treatment** ($t = 2$)
Treated ($D_i = 1$)	$\bar{Y}_{1,1}$	$\bar{Y}_{1,2}$
Control ($D_i = 0$)	$\bar{Y}_{0,1}$	$\bar{Y}_{0,2}$

The **difference-in-differences estimator** is:

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

- 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:

$$\hat{\tau}^{DiD} = (\bar{Y}_{1,2} - \bar{Y}_{1,1}) - (\bar{Y}_{0,2} - \bar{Y}_{0,1}) \quad (13)$$

$$= (\bar{Y}_{1,2} - \bar{Y}_{0,2}) - (\bar{Y}_{1,1} - \bar{Y}_{0,1}) \quad (14)$$

Interpretation:

- First difference: post-treatment difference between treated and control
- 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:

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

the DiD estimator identifies the ATT:

$$\mathbb{E}[\hat{\tau}^{DiD}] = \mathbb{E}[Y_{i,2}(1) - Y_{i,2}(0)|D_i = 1] = \tau_2^{ATT} \tag{16}$$

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)

$$\mathbb{E}[\hat{\tau}^{DiD}] = \mathbb{E}[Y_{i,2}(1)|D_i = 1] - \mathbb{E}[Y_{i,1}(0)|D_i = 1] \tag{19}$$
$$- (\mathbb{E}[Y_{i,2}(0)|D_i = 0] - \mathbb{E}[Y_{i,1}(0)|D_i = 0]) \tag{20}$$

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

$$= \underbrace{\mathbb{E}[Y_{i,2}(1) - Y_{i,2}(0)|D_i = 1]}_{\tau_2^{ATT}} \tag{21}$$
$$+ \underbrace{\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 1] - \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_i = 0]}_{=0 \text{ under parallel trends}} \tag{22}$$

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:

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

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

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

Understanding the regression coefficients

$$Y_{it} = \alpha + \gamma \cdot \mathbb{1}(D_i = 1) + \lambda \cdot \mathbb{1}(t = 2) + \beta \cdot \mathbb{1}(D_i = 1) \times \mathbb{1}(t = 2) + \varepsilon_{it} \tag{25}$$

What do the parameters represent?

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

- 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:

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

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:

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

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!)

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:

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

where now:

- δ_t: separate time fixed effect for each period $t \in \{1, 2, \ldots, T\}$
- $D_{it} = \mathbb{1}(\text{unit } i \text{ is treated at time } t)$

Key advantages with multiple periods:

1. Can test parallel trends using pre-treatment data
2. Can study dynamic effects (how β changes over time since treatment)
3. More robust identification (not reliant on single time comparison)

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)

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	

DiD estimate:

$$\hat{\tau}^{DiD} = 0.59 - (-2.16) = 2.75 \text{ FTE workers} \tag{33}$$

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 $t \in \{1, 2\}$

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

With $T > 2$ periods, the TWFE specification becomes:

$$Y_{it} = \alpha_i + \delta_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{34}$$

where now:

- α_i: unit fixed effects (as before)
- δ_t: time fixed effects for $t = 1, 2, \ldots, T$
- $D_{it} = \mathbb{1}(i \text{ treated at time } t)$

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

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:

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

where:

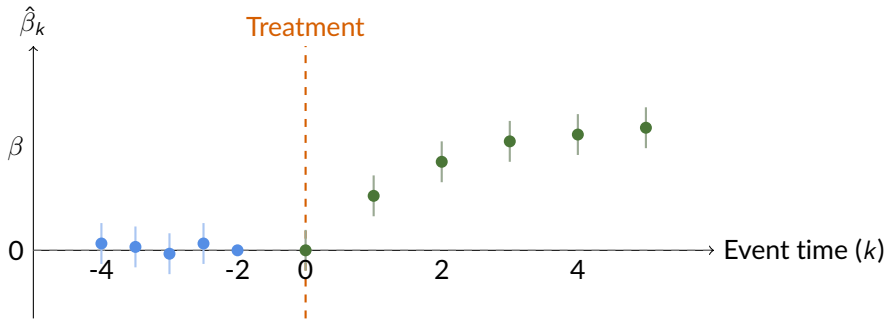
- t_0^i: time when unit i is treated
- $k = t - t_0^i$: time relative to treatment ("event time")
- β_k: treatment effect k periods after treatment
- $k = -1$ is the omitted reference period (normalization)

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)

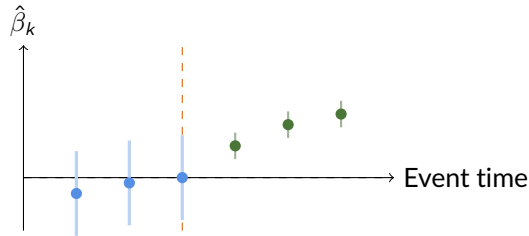
Pre-trends: What to look for

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 $k < 0$
   - Test jointly: $H_0 : \beta_{-K} = \ldots = \beta_{-2} = 0$
2. **Economic significance**: Even if not statistically significant, are they economically large?
   - Compare magnitude of pre-trends to post-treatment effects
   - Large pre-trends (even if imprecise) are concerning
3. **Precision**: How precisely estimated are the pre-trends? (Roth 2022)
   - Wide confidence intervals $\implies$ can't rule out large violations
   - 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:

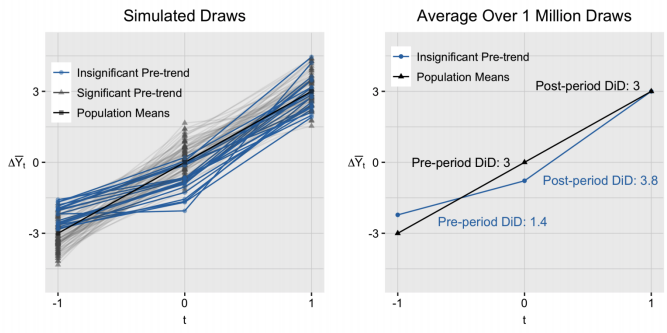


Issue: Pre-trends not significant, but:

- Clear upward trajectory before treatment
- Confidence intervals VERY wide in pre-period
- Cannot reject a pre-trend of size 0.8 (two-thirds the treatment effect!)

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:



By selecting on flat pre-trends, we systematically choose realizations that make the treatment effect look larger

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)

- **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

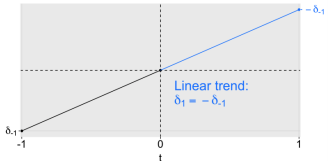
Rambachan and Roth (2023) suggestion

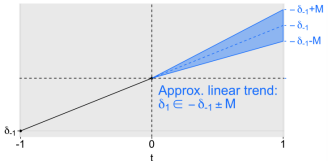
Intuitive proposed solution for robustness. Note the post and pre effects:

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

$$\mathbb{E}[\hat{\beta}_{-1}] = \underbrace{\mathbb{E}[Y_{t,-1}(0) - Y_{t,0}(0)|D_i=1] - \mathbb{E}[Y_{t,-1}(0) - Y_{t,0}(0)|D_i=0]}_{=:\delta_{-1}}$$

Parallel trends: $\delta = 0$. R&R: use pre-trends to **bound** the post-trend via smoothness:





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
- Standard OLS standard errors are severely downward biased
- Leads to massive over-rejection of null hypotheses

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: `feols(Y ~ D | unit + time, cluster = "unit", data = df)` from `fixest`

Outline

Main threats to DiD identification

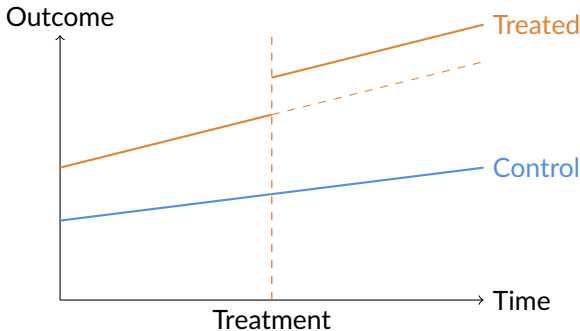
The parallel trends assumption can be violated in several ways:

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 $\implies$ DiD overestimates treatment effect

Dealing with differential trends

Solutions:

1. Group-specific linear trends:

$$Y_{it} = \alpha_i + \delta_t + \gamma_i \cdot t + \beta \cdot D_{it} + \varepsilon_{it} \tag{36}$$

- γ_i: unit-specific linear time trend
- 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

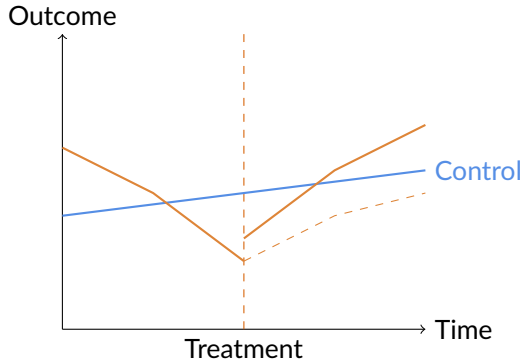
- NJ raises minimum wage; PA does not
- But NJ also experiences a state-specific recession
- Employment falls in NJ for two reasons: minimum wage + recession
- DiD incorrectly attributes recession effect to minimum wage

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



Example: Workers enroll in job training after earnings drop $\implies$ DiD attributes mean reversion to treatment effect

Ashenfelter's dip: Evidence

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

Ashenfelter's dip: The deeper problem

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

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
- DiD underestimates true revenue effect

Detection:

- Event study: look for effects in periods immediately before treatment
- "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)

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

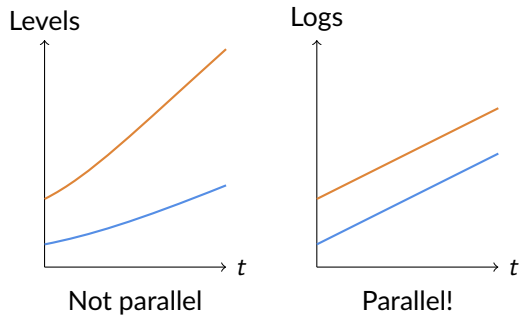
- High-wage workers migrate to treated region after treatment
- Average wage appears to increase
- But this is composition, not causal effect on incumbent workers

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)

Solution: Use $\log(Y_{it})$ — interprets β as percentage effect, often more natural

Functional form and DiD assumptions

Key insight: Every DiD design embeds a strong functional form assumption

The parallel trends assumption $\mathbb{E}[Y_{it}(0) - Y_{it'}(0)|D_i = 1] = \mathbb{E}[Y_{it}(0) - Y_{it'}(0)|D_i = 0]$ is scale-dependent:

- If it holds in levels, it typically **does not hold** in logs
- If it holds in logs, it typically **does not hold** in levels
- Cannot be true in both scales simultaneously (unless constant trends)

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 ($Y_{it}(1) = Y_{it}(0) \times (1 + \tau)$)

Beyond linear DiD: Change-in-changes

Alternative: Athey & Imbens (2006) **Changes-in-Changes** estimator

Key idea: Don't assume parallel trends in levels or logs. Instead:

- Allow for heterogeneous effects across the outcome distribution
- Make distributional assumptions rather than mean assumptions
- More flexible functional form

Assumption:

$$Y_{it}(0) = h_t(U_i) \tag{37}$$

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

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?
   - Tax policy: proportional (use logs)
   - Cash transfer: additive (use levels)
2. **Pre-trends analysis**: Which scale shows flatter pre-trends?
   - If parallel in logs pre-treatment, assume parallel in logs post-treatment
   - But remember: not a perfect test (Roth 2022)
3. **Robustness**: Report results in multiple specifications
   - Levels, logs, changes-in-changes
   - If conclusions are robust, more credible
   - If sensitive, discuss why one specification is preferred
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)

Motivation: What if we're worried about differential shocks to treated vs. control?

Idea: Add a third dimension of differencing using an "unaffected" group

Example: Health insurance program for women

- Treatment: Some states introduce health insurance program for women only
- Control states: No program
- Concern: Treated states may have different macro trends (differential shocks)

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)

DiD for women:

$$DiD_F = (\bar{Y}_{treated,female,post} - \bar{Y}_{treated,female,pre}) - (\bar{Y}_{control,female,post} - \bar{Y}_{control,female,pre}) \quad (38)$$

DiD for men:

$$DiD_M = (\bar{Y}_{treated,male,post} - \bar{Y}_{treated,male,pre}) - (\bar{Y}_{control,male,post} - \bar{Y}_{control,male,pre}) \quad (39)$$

Triple difference:

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

Triple differences: Regression

Can implement via regression:

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

Key coefficient: β_7 is the DDD estimator

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
- More credible when worried about differential macro trends
- Provides robustness check even if not primary specification

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

In reality: Treatment often rolls out at different times

- States adopt policies in different years
- Firms receive treatment based on phased rollout
- Individuals age into eligibility at different times

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
- All cuts happen in one year (1992)
- Control: States without cuts

Problem: What if 1992 is special?

- Maybe nationwide recession affects treated/control states differently
- Or tech boom affects entrepreneurship independent of taxes
- Hard to separate policy effect from concurrent macro shocks

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

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

where $D_{it} = \mathbb{1}(i \text{ has been treated by time } t)$

Seemed reasonable:

- α_i controls for unit fixed effects
- δ_t controls for common time shocks
- β measures average treatment effect

Recent discovery: This doesn't work with:

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

Goodman-Bacon decomposition (2 treatment times)

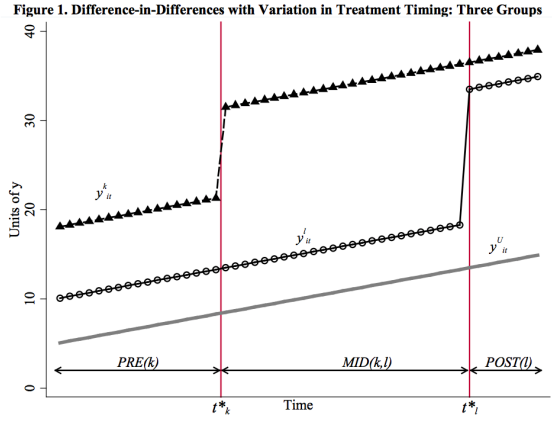
Goodman-Bacon (2021): Special case with 2 treatment times

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

1. **Earlier-treated vs. never-treated**
   - Weight: variance share of this comparison
   - Sign: positive (good comparison)
2. **Later-treated vs. never-treated**
   - Weight: variance share
   - Sign: positive (good comparison)
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



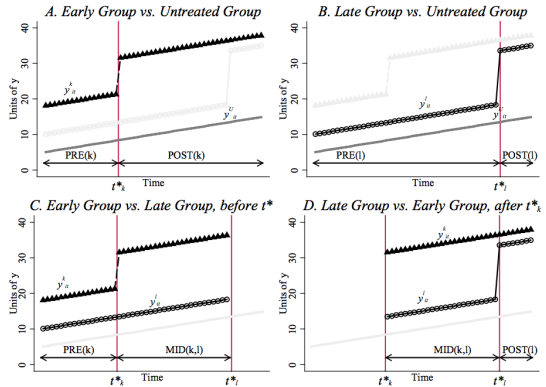
Goodman-Bacon 2×2 comparisons

- 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



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

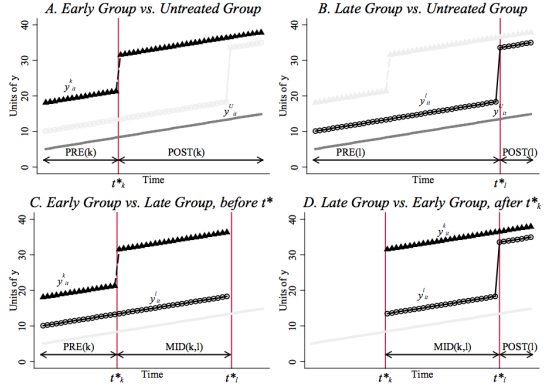
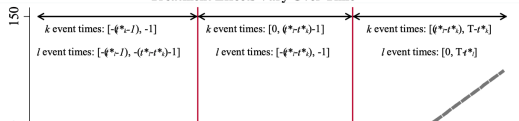


Figure 3. Difference-in-Differences Estimates with Variation in Timing Are Biased When Treatment Effects Vary Over Time



Heterogeneous treatment effects: 3-group example

Setup: Three groups with parallel trends, 3 time periods ($t = 1, 2, 3$)

Group E: Treated early at $t = T_E = 2$

- Untreated outcome: $Y_{Et}(0) = \alpha_E + g(t)$

Group L: Treated late at $t = T_L = 3 > T_E$

- Untreated outcome: $Y_{Lt}(0) = \alpha_L + g(t)$ (parallel trends: same $g(t)$)

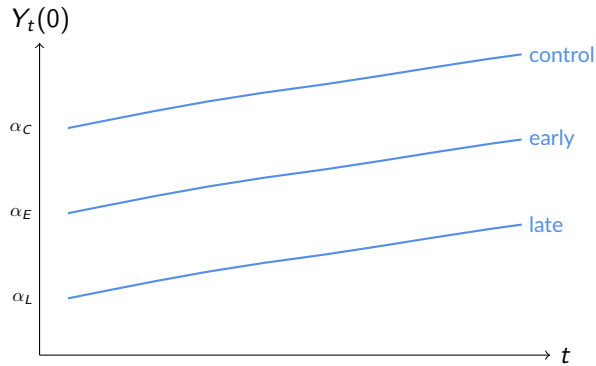
Group C: Never treated (control)

- Untreated outcome: $Y_{Ct}(0) = \alpha_C + g(t)$ (parallel trends)

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

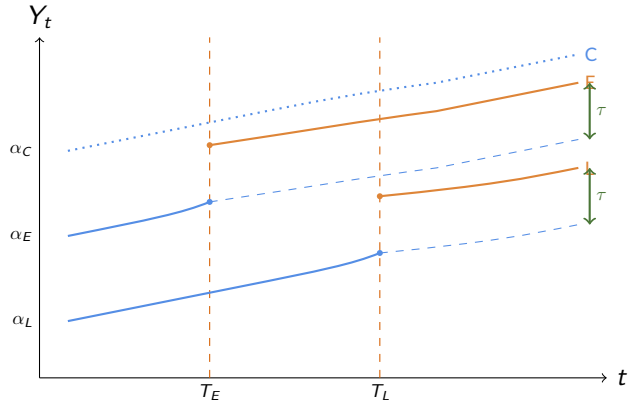
- Effect is τ_l in first period after treatment
- Effect grows to τ_h in second period after treatment (where $\tau_h > \tau_l$)
- Problem: Group E has been treated longer at $t = 3$ than Group L

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)



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

Why constant effects are not a problem

Special case: Suppose treatment effect is constant τ (no dynamics)

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

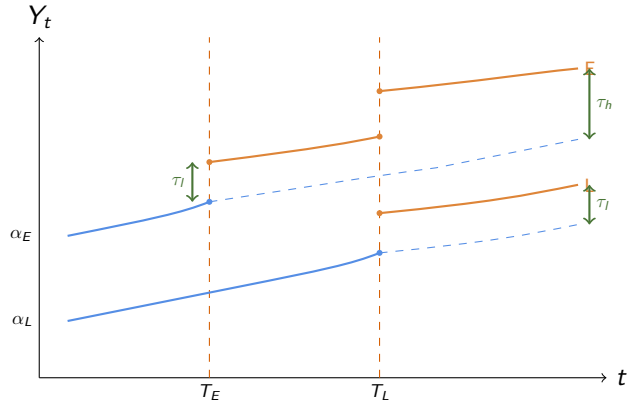
The forbidden comparison now gives:

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

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!)

What TWFE estimates with dynamic effects

The TWFE estimator uses Group E as a control for Group L at $t = T_L$

But at $t = T_L$:

- Group E has been treated for 2 periods $\Rightarrow$ effect is τ_h
- Group L just got treated $\Rightarrow$ effect is τ_l

TWFE's implicit estimate from this comparison:

$$\begin{aligned}
\hat{\tau}^{TWFE}_{L \text{ vs } E} &= [Y_{L,T_L} - Y_{L,T_E}] - [Y_{E,T_L} - Y_{E,T_E}] \\
&= [\alpha_L + g(T_L) + \tau_l - \alpha_L - g(T_E)] \\
&\quad - [\alpha_E + g(T_L) + \tau_h - \alpha_E - g(T_E) - \tau_l] \\
&= \tau_l - (\tau_h - \tau_l) \\
&= 2\tau_l - \tau_h
\end{aligned}$$

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!

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
- $w_{g,t}$: weight on this effect (depends on treatment variance)

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)
- 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!

Alternative estimators:

1. **Callaway & Sant'Anna (2021)**:
   - Compute clean 2×2 DiDs for each cohort-time pair
   - Only use never-treated or not-yet-treated as controls
   - Aggregate using explicit weights
2. **de Chaisemartin & D'Haultfoeuille (2020)**:
   - Similar approach: avoid forbidden comparisons
   - Provides diagnostic for negative weights in your data
   - R package: `DIDmultiplegt`
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
   - $g = \infty$ if never treated
2. For each cohort g and time $t \geq g$, compute:

$$ATT(g,t) = \mathbb{E}[Y_t - Y_{g-1} | G_i = g] - \mathbb{E}[Y_t - Y_{g-1} | G_i = C_t] \tag{46}$$

   where C_t is comparison group (never-treated or not-yet-treated at t)
3. Aggregate across cohorts and times:

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

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

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**

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

Why this matters:

- If control group has small treatment effect $\tau_C > 0$ and treatment group has $\tau_T > \tau_C$
- Standard DiD estimates $\tau_T - \tau_C$, not τ_T
- Same issue as forbidden comparisons: comparing “more treated” vs. “less treated”

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
2. **Use diagnostic tools:**
   - `bacon` package (Goodman-Bacon decomposition)
   - `DIDmultiplegt` (de Chaisemartin & D'Haultfoeuille)
   - Check for negative weights in your data
3. **Use robust estimators:**
   - Callaway & Sant'Anna: R package `did`
   - Sun & Abraham: Stata package `eventstudyinteract`
   - Report both TWFE and robust estimator for comparison
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?

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

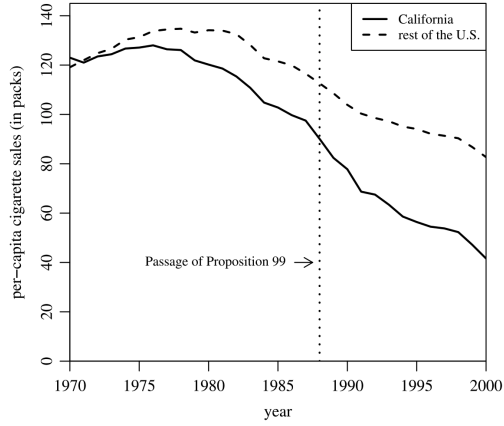


Figure 1. Trends in per-capita cigarette sales: California vs. the rest of the United States.

Synthetic control: basic method

Method (Abadie et al., 2010):

- Control (weighted): $\sum_j \omega_j = 1$

- 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 (Abadi [illegible] (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)

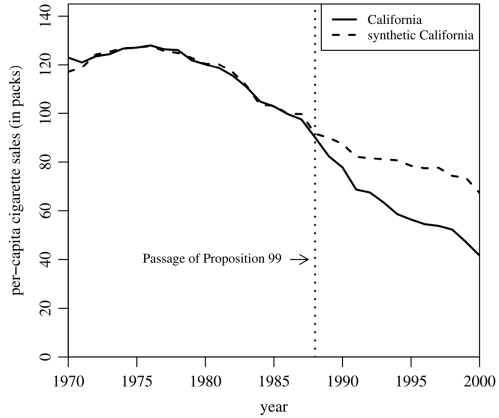


Figure 2. Trends in per-capita cigarette sales: California vs. synthetic California.

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

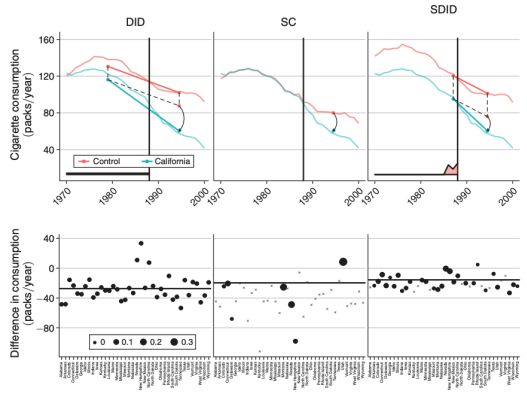


FIGURE 1. A COMPARISON BETWEEN DID, SC, AND SDID ESTIMATES FOR THE EFFECT OF CALIFORNIA PROPOSITION 99 ON PER-CAPITA ANNUAL CIGARETTE CONSUMPTION (IN PACKS/YEAR)

Summary: The DiD toolkit

Basic DiD (2×2 designs):

- 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: $Y_{it} = \alpha_i + \delta_t + \beta \cdot D_{it} + \varepsilon_{it}$

Key threats:

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

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)

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

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
2. **Be honest about threats**:
   - Discuss potential violations of identifying assumptions
   - Show robustness checks (functional form, sample restrictions, etc.)
   - Consider alternative explanations
3. **With staggered timing**:
   - Check for heterogeneity across cohorts/time
   - Use diagnostic tools (Goodman-Bacon decomposition)
   - 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:

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

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

Synthetic DiD:

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

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: $\mathbf{X}$ can include lagged outcomes ($Y_{i,t-1}$, $Y_{i,t-2}$, ...), time-invariant covariates, and time-varying covariates.

This is fundamentally a **matching problem**: find control units whose pre-treatment characteristics best predict the treated unit's trajectory.

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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

- Staggered adoption isn't natural for synthetic control

Issue: Usual synthetic control/DiD focused on single adoption period

- 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, control units, weighting — 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"), good pre-period match
- If no good match exists, stop or use Ben-Michael et al. (2021) augmented SC

When to use synthetic DiD:

- Promising for multiple treated units; show results with **both** DiD and synthetic DiD

Software: `augsynth` (augmented SC), `synthdid` (Arkhangelsky et al.), `tidysynth` (user-friendly SC)

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