Econometric Analysis of Cross Section and Panel Data

Lecture 9: Difference-in-Differences

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This lecture

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  - → Simplest form: a group of units is treated at the same time
  - → Staggered form: groups of units are treated at different points in time

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  - → Simplest form: a group of units is treated at the same time
  - → Staggered form: groups of units are treated at different points in time
- This lecture focuses on
  - → Identifying assumption
  - → Estimation and inference
  - → Recent development

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  - → But methods like quarantining the sick were strangely ineffective at slowing down this plague.
- John Snow asked: Is it possible that Cholera was transmitted by water?
- If Snow was a dictator with unlimited wealth and power, how could he test his theory that cholera is waterborne?

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- In the 1800s, neighborhoods were served by water companies, which took water from the Thames (polluted by victims’ evacuations via runoff).
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- A DiD estimate: [19-85]-[147-135]=-78

The simplest case

- We will start a description of DiD in the simplest "canonical" case
- Why? Because recent DiD literature can be viewed as relaxing various components of the canonical model while preserving others

The simplest case

In the canonical DiD model, we have:

- 2 periods: treatment occurs (for some units) in period 2
- Identification of the ATT from parallel trends and no anticipation
- Estimation using sample analogs, equivalent to OLS with TWFE
- A large number of independent observations (or clusters)

Canonical DiD – with math

- Panel data on Y_{it} for $t = 1, 2$ and $i = 1, ..., N$
- **Treatment timing:** Some units $(D_i = 1)$ are treated in period 2; everyone else is untreated $(D_i = 0)$

Canonical DiD – with math

- Panel data on Y_{it} for $t = 1, 2$ and $i = 1, ..., N$
- **Treatment timing:** Some units $(D_i = 1)$ are treated in period 2; everyone else is untreated $(D_i = 0)$
- **Potential outcomes:** Observe $Y_{it}(1) \equiv Y_{it}(0, 1)$ for treated units; and $Y_{it}(0) \equiv Y_{it}(0, 0)$ for comparison

Key identifying assumptions

- **Parallel trends:**

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

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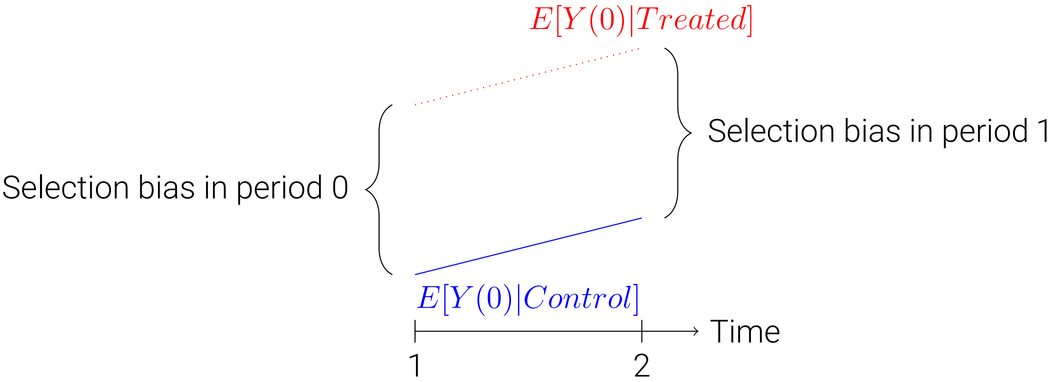
- **No anticipation:** $Y_{i1}(1) = Y_{i1}(0)$

  $\rightarrow$ Intuitively, outcome in period 1 isn't affected by treatment status in period 2

  $\rightarrow$ Often left implicit in notation, but important for interpreting DiD estimand as a causal effect in period 2

Visualizing PT



Identification

- **Target parameter:** Average treatment effect on the treated (ATT) in period 2

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- Under parallel trends and no anticipation, can show that

$$\tau_{ATT} = \underbrace{(E[Y_{i2}|D_i = 1] - E[Y_{i1}|D_i = 1])}_{\text{Change for treated}} - \underbrace{(E[Y_{i2}|D_i = 0] - E[Y_{i1}|D_i = 0])}_{\text{Change for control}},$$

  a "difference-in-differences" of population means

Proof of identification argument

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- Add and subtract $E[Y_{i2}(0)|D_i = 1]$ to obtain:

$$E[Y_{i2}(1) - Y_{i2}(0)|D_i = 1] +$$
$$[(E[Y_{i2}(0)|D_i = 1] - E[Y_{i1}(0)|D_i = 1]) - (E[Y_{i2}(0)|D_i = 0] - E[Y_{i1}(0)|D_i = 0])$$

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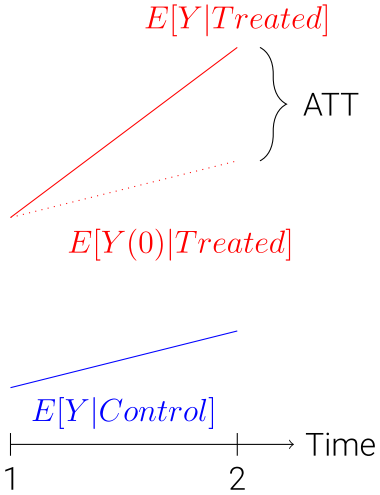
- Cancel the last terms using PT to get $E[Y_{i2}(1) - Y_{i2}(0) | D_i = 1] = \tau_{ATT}$

Visualizing identification



Estimation and inference

- The most conceptually simple estimator replaces population means with sample analogs:

$$\hat{\tau}_{DiD} = (\bar{Y}_{12} - \bar{Y}_{11}) - (\bar{Y}_{02} - \bar{Y}_{01})$$

where $\bar{Y}_{dt}$ is sample mean for group d in period t

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- Conveniently, $\hat{\tau}_{DID}$ is algebraically equal to OLS coefficient $\hat{\beta}$ from

$$Y_{it} = \alpha_i + \phi_t + D_{it}\beta + \epsilon_{it}, \tag{2}$$

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- **Inference:** And clustered standard errors are valid as number of clusters grows large

Support PT

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- Given multiple periods, you can support parallel trend by showing parallel pre-trend
  - → If they had been similar before, then why wouldn't they continue to be in the absence of treatment?

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  - → If they had been similar before, then why wouldn't they continue to be in the absence of treatment?
- One way is to simply show the raw data and just visually inspect whether the pre-treatment dynamics of the treatment group differed from that of the control group units

Support PT

- Galiani et al. (2005)

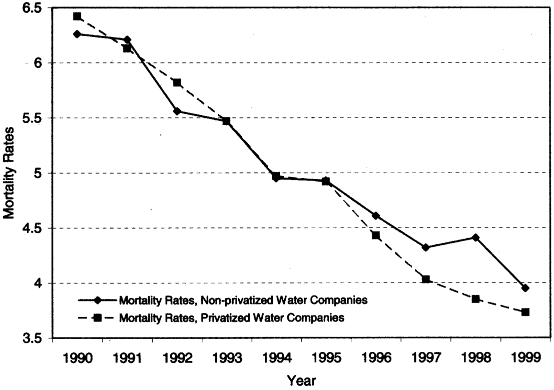


Fig. 1.—Evolution of mortality rates for municipalities with privatized vs. nonprivatized water services

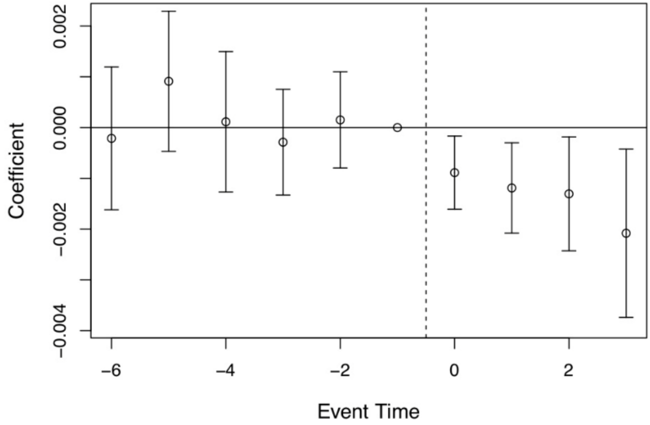
Support PT

- Or you can estimate the differences in each period using an event study

$$Y_{it} = \alpha_i + \phi_t + \sum_{\tau \neq -1} \beta_\tau D_i * T_\tau + \epsilon_{it}, \tag{3}$$

where $T_\tau = 1[t - t^* = \tau]$ and t^* indicates the treatment period

Support PT



Characterizing the recent literature

We can group the recent innovations in DiD lit by which elements of the canonical model they relax:

- **Multiple periods and staggered treatment timing**
- **Relaxing or allowing PT to be violated**
- **Inference with a small number of clusters**

Staggered timing

- Remember that in the canonical DiD model we had:
  - $\rightarrow$ Two periods and a common treatment date
  - $\rightarrow$ Identification from parallel trends and no anticipation
  - $\rightarrow$ A large number of clusters for inference
- A very active recent literature has focused on relaxing the first assumption: **what if there are multiple periods and units adopt treatment at different times?**
- This literature typically maintains the remaining ingredients: parallel trends and many clusters

Overview of staggered timing literature

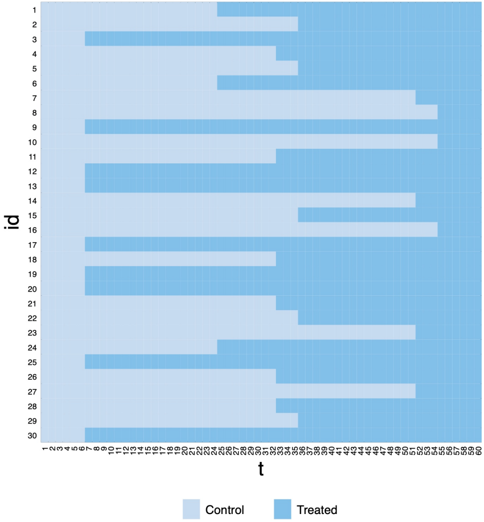
1. Negative results: TWFE OLS doesn't give us what we want with treatment effect heterogeneity
2. New estimators: perform better under treatment effect heterogeneity

Staggered timing set-up

- Panel of observations for periods $t = 1, ..., T$
- Suppose units adopt a binary treatment at different dates $G_i \in \{1, ..., T\} \cup \infty$ (where $G_i = \infty$ means "never-treated")
  - $\rightarrow$ Literature is now starting to consider cases with continuous treatment & treatments that turn on/off – that lit is still developing
- Potential outcomes $Y_{it}(g)$ – depend on time and time you were first-treated

Staggered timing set-up

panelview



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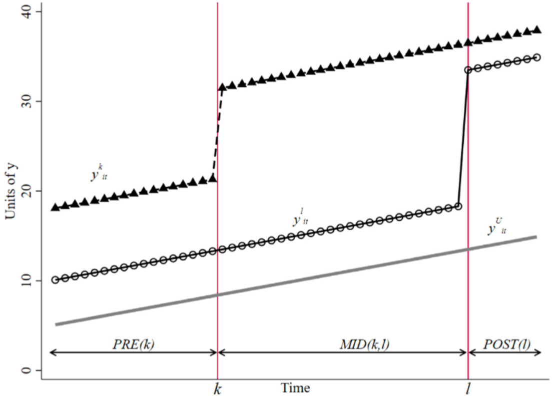

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- **No anticipation:** Intuitively, says that treatment has no impact before it is implemented

$$Y_{it}(g) = Y_{it}(\infty) \text{ for all } t < g$$

Negative results

- Suppose we again run the regression

$$Y_{it} = \alpha_i + \phi_t + D_{it}\beta + \epsilon_{it},$$

  where $D_{it} = 1[t \geq G_i]$ is a treatment indicator.
- Suppose we're willing to assume no anticipation and parallel trends across all adoption cohorts as described above

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异质性处理效应双向固定效应模型存在偏误

- Bad news: if treatment effects are heterogeneous, then β may put negative weights on treatment effects for some units and time periods
  - $\rightarrow$ E.g., if treatment effect depends on time since treatment, $Y_{it}(t-r) - Y_{it}(\infty) = \tau_r$, then some τ_rs may get negative weight

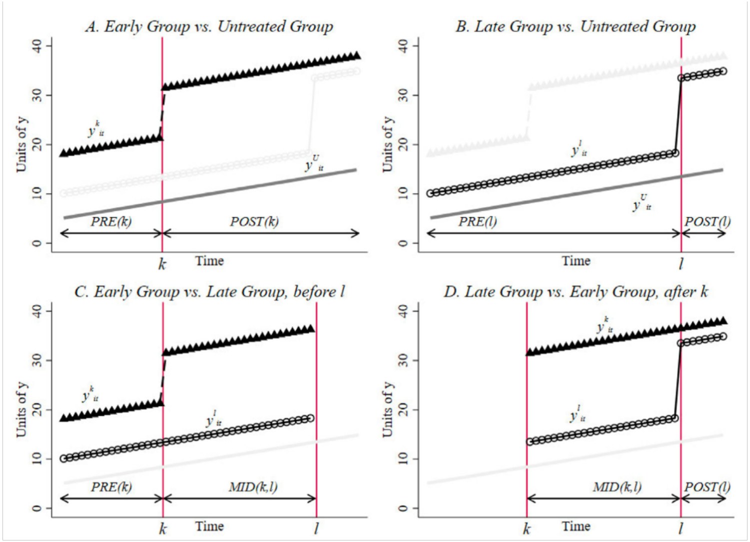
Where do these negative results come from?

- The intuition for these negative results is that the TWFE OLS specification combines two sources of comparisons:

    1. **Clean comparisons:** DiD's between treated and not-yet-treated units
    2. **Forbidden comparisons:** DiD's between two sets of already-treated units (who began treatment at different times)

- These forbidden comparisons can lead to negative weights: the "control group" is already treated, so we run into problems if their treatment effects change over time

Bacon decomposition



forbidden

Some intuition for forbidden comparisons

- Consider the two period model, except suppose now that our two groups are **always-treated** units (treated in both periods) and **switchers** (treated only in period 2)
- With two periods, the coefficient β from $Y_{it} = \alpha_i + \phi_t + D_{it}\beta + \epsilon_{it}$ is the same as from the first-differenced regression $\Delta Y_i = \alpha + \Delta D_i \beta + u_i$

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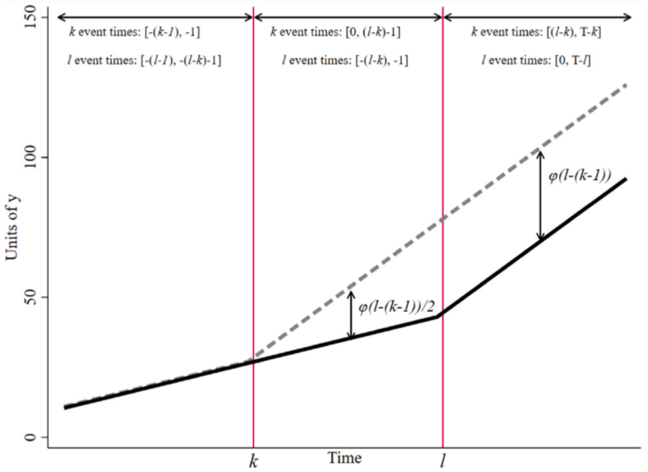
- Observe that ΔD_i is one for switchers and zero for stayers.
  That is, the stayers are the control group! Thus,

$$\hat{\beta} = \underbrace{\left(\bar{Y}_{Switchers,2} - \bar{Y}_{Switchers,1}\right)}_{\text{Change for switchers}} - \underbrace{\left(\bar{Y}_{AT,2} - \bar{Y}_{AT,1}\right)}_{\text{Change for always treated}}$$

- Problem: if the treatment effect for the always-treated grows over time, that will enter $\hat{\beta}$ negatively!

Some intuition for forbidden comparisons



Second intuition for negative weights

- The Frisch-Waugh-Lovell theorem says that we can obtain the coefficient β in $Y_{it} = \alpha_i + \phi_t + D_{it}\beta + \epsilon_{it}$ by the following two-step procedure.

Second intuition for negative weights

- The Frisch-Waugh-Lovell theorem says that we can obtain the coefficient β in $Y_{it} = \alpha_i + \phi_t + D_{it}\beta + \epsilon_{it}$ by the following two-step procedure.

- First, regress the treatment indicator D_{it} on the FEs (a linear probability model): $D_{it} = \tilde{\alpha}_i + \tilde{\phi}_t + \tilde{\epsilon_{it}}$

- Then run a univariate regression of Y_{it} on $D_{it} - \hat{D}_{it}$ to obtain β.
  $\rightarrow$ Thus, $\beta = \frac{Cov(Y_{it}, D_{it} - \hat{D}_{it})}{Var(D_{it} - \hat{D}_{it})} = \frac{E(Y_{it}(D_{it} - \hat{D}_{it}))}{Var(D_{it} - \hat{D}_{it})}$

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- However, it's well known that the linear probability model for D_{it} may have predictions outside the unit interval. If $\hat{D}_{it} > 1$ even though unit i is treated in period t, then $D_{it} - \hat{D}_{it} < 0$, and thus Y_{it} gets negative weight.

Not just negative but weird...

The literature has placed a lot of emphasis on the fact that some treatment effects may get negative weights

- But even if the weights are non-negative, they might not give us the most intuitive parameter

- For example, suppose each unit i has treatment effect τ_i in every period if they are treated (no dynamics). Then β gives a weighted average of the τ_i where the weights are largest for units treated closest to the middle of the panel

- It is not obvious that these weights are relevant for policy, even if they are all non-negative!

Issues with dynamic TWFE

- Sun and Abraham (2021) show that similar issues arise with dynamic TWFE specifications:

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{k \neq -1} \gamma_k D_{i,t}^k + \varepsilon_{i,t},$$

where $D_{i,t}^k = 1\{t - G_i = k\}$ are "event-time" dummies.

- Like for the static spec, γ_k may put negative weight on treatment effects after k periods for some units

- SA also show that γ_k may be "contaminated" by treatment effects at lags $k' \neq k$

Dynamic TWFE - Continued

- The results in SA suggest that interpreting the $\hat{\gamma}_k$ for $k = 1, 2, ...$ as estimates of the dynamic effects of treatment may be misleading
- These results also imply that pre-trends tests of the γ_k for $k < 0$ may be misleading – could be non-zero even if parallel trends holds, since they may be "contaminated" by post-treatment effects!

Dynamic TWFE - Continued

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- The issues discussed in SA arise if dynamic path of treatment effects is heterogeneous across adoption cohorts
  - $\rightarrow$ Biases may be less severe than for "static" specs if dynamic patterns are similar across cohorts

New estimators (and estimands!)

- Several new (closely-related) estimators have been proposed to try to address these negative weighting issues
- The key components of all of these are:
  1. Be precise about the target parameter (estimand) – i.e., how do we want to aggregate treatment effects across time/units
  2. Estimate the target parameter using only "clean-comparisons"

Example – Callaway and Sant'Anna (2020)

- Define $ATT(g, t)$ to be ATT in period t for units first treated at period g,

$$ATT(g, t) = E[Y_{it}(g) - Y_{it}(\infty) | G_i = g]$$

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- Under PT and No Anticipation, $ATT(g,t)$ is identified as

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- Why?

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- Why? This is a two-group two-period comparison, so the argument is the same as in the canonical case!

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- Use No Anticipation to substitute $Y_{i,g-1}(\infty)$ for $Y_{i,g-1}(g)$:
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$$E[Y_{it} - Y_{i,g-1}|G_i = g] - E[Y_{it} - Y_{i,g-1}|G_i = \infty]$$
- Apply definition of POs to obtain:
$$E[Y_{it}(g) - Y_{i,g-1}(g)|G_i = g] - E[Y_{ig}(\infty) - Y_{i,g-1}(\infty)|G_i = \infty]$$
- Use No Anticipation to substitute $Y_{i,g-1}(\infty)$ for $Y_{i,g-1}(g)$:
$$E[Y_{it}(g) - Y_{i,g-1}(\infty)|G_i = g] - E[Y_{ig}(\infty) - Y_{i,g-1}(\infty)|G_i = \infty]$$
- Add and subtract $E[Y_{it}(\infty)|G_i = g]$ to obtain:

$$E[Y_{it}(g) - Y_{it}(\infty)|G_i = g] +$$
$$[E[Y_{it}(\infty) - Y_{i,g-1}(\infty)|G_i = g] - E[Y_{ig}(\infty) - Y_{i,g-1}(\infty)|G_i = \infty]]$$

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- Cancel the last term using PT to get $E[Y_{it}(g) - Y_{it}(\infty)|G_i = g] = ATT(g,t)$

Example – Callaway and Sant'Anna (2020)

- Define $ATT(g,t)$ to be ATT in period t for units first treated at period g,

$$ATT(g,t) = E[Y_{it}(g) - Y_{it}(\infty)|G_i = g]$$

- Under PT and No Anticipation,

$$ATT(g,t) = \underbrace{E[Y_{it} - Y_{i,g-1}|G_i = g]}_{\text{Change for cohort g}} - \underbrace{E[Y_{it} - Y_{i,g-1}|G_i = \infty]}_{\text{Change for never-treated}}$$

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- We can then estimate this with sample analogs:

$$\widehat{ATT}(g,t) = \underbrace{\widehat{E}[Y_{it} - Y_{i,g-1} | G_i = g]}_{\text{Sample change for cohort g}} - \underbrace{\widehat{E}[Y_{it} - Y_{i,g-1} | G_i = \infty]}_{\text{Sample change for never-treated}}$$

Aggregation schemes

- If have a large number of observations and relatively few groups/periods, can report $\widehat{ATT}(g,t)$'s directly.
- If there are many groups/periods, the $\widehat{ATT}(g,t)$ may be very imprecisely estimated and/or too numerous to report concisely

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  - $\rightarrow$ E.g. $\hat{\theta}_k = \sum_g \widehat{ATT}(g, g+k)$ aggregates effects for cohorts in the kth period after treatment
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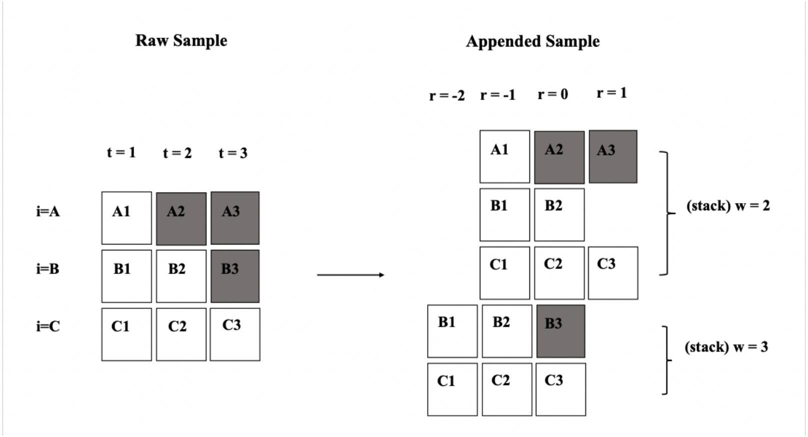
  - → Can also construct for $k < 0$ to estimate "pre-trends"
- C&S discuss other sensible aggregations too – e.g., if interested in whether treatment effects differ across good/bad economies, may want to "calendar averages" that pool the $\widehat{ATT}(g,t)$ for the same year

Comparisons of new estimators

- Callaway and Sant'Anna also propose an analogous estimator using *not-yet-treated* rather than never-treated units.
- Sun and Abraham (2021) propose a similar estimator but with different comparisons groups (e.g. using last-to-be treated rather than not-yet-treated)
- Borusyak et al. (2024), Wooldridge (2021), Gardner (2021) propose "imputation" estimators that estimate the counterfactual $\hat{Y}_{it}(0)$ using a TWFE model that is fit using only pre-treatment data
  - → Main difference from C&S is that this uses more pre-treatment periods, not just period $g-1$
  - → This can sometimes be more efficient (if outcome not too serially correlated), but also relies on a stronger PT assumption that may be more susceptible to bias

Stacked DiD



Raw Sample
Appended Sample
r = -2
r = -1
r = 0
r = 1
t = 1
t = 2
t = 3
i=A
i=B
i=C
A1
A2
A3
B1
B2
B3
C1
C2
C3
(stack) w = 2
(stack) w = 3

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- The most important thing is to be precise about who you want the comparison group to be and to choose a method that only uses these "clean comparisons"
- In my experience, the difference between the new estimators is typically not that large – can report multiple new methods for robustness (to make your referees happy!)

References I

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