

Causal Inference II

MIXTAPE SESSION



Roadmap

Background material

Introduction

Two-way fixed effects

TWFE Pathologies

Bacon decomposition

Simulation

Two solutions and a new decomposition

CS

SA

dCH

Overview

- Brief review of TWFE and idea of strict exogeneity
- Brief review of potential outcomes, ATT, and the parallel trends assumption
- Discussion of standard “constant treatment effect” TWFE pathologies using Goodman-Bacon
- Discussion of two solutions and if we have time a third

Beaver dam and diff-in-diff credibility crisis

- Differential timing literature is like a stick that struck a beaver's dam
- Stick made a hole causing a leak
- Gradually that hole got larger and the leak got bigger
- Eventually the dam collapsed
- That's now



Difference-in-differences credibility crisis

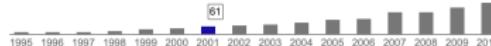
- Series of important papers starting in 2016, born independent of one another, by grad students and assistant professors found critical pathologies with TWFE and developed solutions
- Can't cover all of them, but they have a lot in common, so I'm going to cover the basic problem and some basic solutions
- Extreme meteoric rise, unusual for econometrics

Compare with LATE paper

- Compare what happened with Imbens and Angrist 1995 LATE in *Econometrica*
- 61 annual cites the year Imbens is denied tenure at Harvard for what would later win him a Nobel Prize
- Gradual rise; many famous econometrics papers like this because of the extreme firewall between econometrics and practices
- Diff-in-diff is unusual for some reason (following is only mid-2022 cites)

Identification and estimation of local average treatment effects

| | |
|------------------|--|
| Authors | Guido W Imbens, Joshua D Angrist |
| Publication date | 1994/3/1 |
| Journal | <i>Econometrica: journal of the Econometric Society</i> |
| Pages | 467-475 |
| Publisher | Econometric Society |
| Description | RANDOM ASSIGNMENT OF TREATMENT and concurrent data collection on treatment and control groups is the norm in medical evaluation research. In contrast, the use of random assignment to evaluate social programs remains controversial. Following criticism of parametric evaluation models (eg, Lalonde (1986)), econometric research has been geared towards establishing conditions that guarantee nonparametric identification of treatment effects in observational studies, ie identification without relying on functional form restrictions or distributional assumptions. The focus has been on identification of average treatment effects in a population of interest, or on the average effect for the subpopulation that is treated. The conditions required to nonparametrically identify these parameters can be restrictive, however, and the derived identification results fragile. In particular, results in Chamberlain (1986), Manski (1990) ... |
| Total citations | Cited by 5586 |



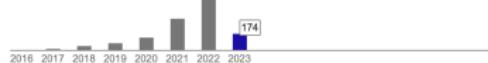
Borusyak et al

- Starts it all; written as grad students at Harvard
- Goes through many revisions, posted as working paper
- Returned to a few years ago with a third coauthor, Speiss, now R\$R at REstud

Revisiting event study designs: Robust and efficient estimation

Authors Kirill Borusyak, Xavier Jaravel, Jann Spiess
Publication date 2021/8/27
Journal arXiv preprint arXiv:2108.12419
Description We develop a framework for difference-in-differences designs with staggered treatment adoption and heterogeneous causal effects. We show that conventional regression-based estimators fail to provide unbiased estimates of relevant estimands absent strong restrictions on treatment-effect homogeneity. We then derive the efficient estimator addressing this challenge, which takes an intuitive "imputation" form when treatment-effect heterogeneity is unrestricted. We characterize the asymptotic behavior of the estimator, propose tools for inference, and develop tests for identifying assumptions. Extensions include time-varying controls, triple-differences, and certain non-binary treatments. We show the practical relevance of these insights in a simulation study and an application. Studying the consumption response to tax rebates in the United States, we find that the notional marginal propensity to consume is between 8 and 11 percent in the first quarter—about half as large as benchmark estimates used to calibrate macroeconomic models—and predominantly occurs in the first month after the rebate.

Total citations Cited by 1399



"dCdH"

- First major hit in AER
- Very thorough decomposition of the TWFE pathology, very general solution, included code
- Very active and talented young team (assistant profs when this was done)

Two-way fixed effects estimators with heterogeneous treatment effects

| | |
|------------------|---|
| Authors | Clément De Chaisemartin, Xavier d'Haultfoeuille |
| Publication date | 2020/9/1 |
| Journal | American Economic Review |
| Volume | 110 |
| Issue | 9 |
| Pages | 2964-2996 |
| Publisher | American Economic Association |
| Description | Linear regressions with period and group fixed effects are widely used to estimate treatment effects. We show that they estimate weighted sums of the average treatment effects (ATE) in each group and period, with weights that may be negative. Due to the negative weights, the linear regression coefficient may for instance be negative while all the ATEs are positive. We propose another estimator that solves this issue. In the two applications we revisit, it is significantly different from the linear regression estimator. (JEL C21, C23, D72, J31, J51, L82) |

Total citations [Cited by 2019](#)



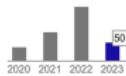
Goodman-Bacon

- Arguably the most influential in terms of bringing attention to the problem
- Begun while grad student at Michigan, published last of the crop
- Probably Twitter network had a role as he was very active, also not an econometrician

Difference-in-differences with variation in treatment timing

| | |
|------------------|--|
| Authors | Andrew Goodman-Bacon |
| Publication date | 2021/12/1 |
| Journal | Journal of Econometrics |
| Volume | 225 |
| Issue | 2 |
| Pages | 254-277 |
| Publisher | North-Holland |
| Description | The canonical difference-in-differences (DD) estimator contains two time periods, "pre" and "post", and two groups, "treatment" and "control". Most DD applications, however, exploit variation across groups of units that receive treatment at different times. This paper shows that the two-way fixed effects estimator equals a weighted average of all possible two-grouptwo-period DD estimators in the data. A causal interpretation of two-way fixed effects DD estimates requires both a parallel trends assumption and treatment effects that are constant over time. I show how to decompose the difference between two specifications, and provide a new analysis of models that include time-varying controls. |

Total citations Cited by 3307



"CS"

- Second broad solution to the problem, written while assistant professors at Vanderbilt and Ole Miss
- Colleague with Andrew Goodman-Bacon
- Introduced new terms like group-time ATT
- Seems to be in the lead

Difference-in-differences with multiple time periods

| | |
|------------------|--|
| Authors | Brantly Callaway, Pedro HC Sant'Anna |
| Publication date | 2021/12/1 |
| Journal | Journal of Econometrics |
| Volume | 225 |
| Issue | 2 |
| Pages | 200-230 |
| Publisher | North-Holland |
| Description | In this article, we consider identification, estimation, and inference procedures for treatment effect parameters using Difference-in-Differences (DiD) with (i) multiple time periods, (ii) variation in treatment timing, and (iii) when the "parallel trends assumption" holds potentially only after conditioning on observed covariates. We show that a family of causal effect parameters are identified in staggered DiD setups, even if differences in observed characteristics create non-parallel outcome dynamics between groups. Our identification results allow one to use outcome regression, inverse probability weighting, or doubly-robust estimands. We also propose different aggregation schemes that can be used to highlight treatment effect heterogeneity across different dimensions as well as to summarize the overall effect of participating in the treatment. We establish the asymptotic properties of the proposed estimators and prove the ... |

Total citations [Cited by 2378](#)



"CS"

- Third broad solution to the problem, very similar to CS
- Focus was on decomposing the event study
- Written while grad students at MIT

Estimating dynamic treatment effects in event studies with heterogeneous treatment effects

Authors Liyang Sun, Sarah Abraham

Publication date 2021/12/1

Journal Journal of Econometrics

Volume 225

Issue 2

Pages 175-199

Publisher North-Holland

Description To estimate the dynamic effects of an absorbing treatment, researchers often use two-way fixed effects regressions that include leads and lags of the treatment. We show that in settings with variation in treatment timing across units, the coefficient on a given lead or lag can be contaminated by effects from other periods, and apparent pretrends can arise solely from treatment effects heterogeneity. We propose an alternative estimator that is free of contamination, and illustrate the relative shortcomings of two-way fixed effects regressions with leads and lags through an empirical application.

Total citations Cited by 1828



Just a drop in the bucket

- Gardner, Wooldridge, John Roth, and on and on
- Too many people to name at this point
- Given the large cites, we are likely to keep seeing more on this
- Probably shifting applied practice for the better but there are some growing pains

Comments

- We once thought difference-in-differences was the simplest of all designs
- That's because we had an incorrect understanding of what strict exogeneity implied in a panel fixed effects estimator
- Strict exogeneity, as it turned out, had functional form assumptions and constant treatment effects assumptions buried in it
- There are ways around it, but the dominant model used for decades (which probably all of us have published using) was more assumption laden than we knew
- Hence why you are seeing so much about differential timing these days – take heart! It's solveable

Two-way fixed effects

- When working with panel data, the so-called “two-way fixed effects” (TWFE) estimator was the workhorse estimator
- It was at some point adopted for difference-in-differences designs when treatments are adopted at different points in time
- It's easy to implement, handles time-varying treatments, and has a relatively straightforward interpretation under constant treatment effects
- Turns out its interpretation is more complicated with heterogeneous treatment effects

Panel estimators

- Panel estimators estimate causal effects in situations where there are unobserved factors associated with the treatment variable creating endogeneity problems
- Less about identification under parallel trends and more about modeling unobservables as unchanging over time ("time invariant")
- Fixed effects estimation eliminate the unobserved confounder through a demeaning process while retaining the identification of the treatment parameter under constant treatment effects

When to use TWFE

- Traditionally, this was used for estimating constant treatment effects with unobserved time-invariant heterogeneity
- And this also made it appealing for diff-in-diff – allowed you to “control for” many unobservables, like differences in questionnaires, differences in sites, etc.
- It’s a linear model, so you’ll be estimating conditional mean treatment effects – if you want the median, you can’t use this
- Once you enter into a world with dynamic treatment effects and differential timing, standard specifications became perverse

When not to use it

- Simultaneous equations: cannot estimate demand curves with fixed effects
- Reverse causality: Becker predicted police reduce crime, but when you regress crime onto police, it's usually positive
- Time-varying unobserved heterogeneity

Notation

- Let y and $x \equiv (x_1, x_2, \dots, x_k)$ be observable random variables and c be an unobservable random variable
- We are interested in the partial effects of variable x_j in the population regression function

$$E[y|x_1, x_2, \dots, x_k, c]$$

Notation

- We observe a sample of $i = 1, 2, \dots, N$ cross-sectional units for $t = 1, 2, \dots, T$ time periods (a balanced panel)
 - For each unit i , we denote the observable variables for all time periods as $\{(y_{it}, x_{it}) : t = 1, 2, \dots, T\}$
 - $x_{it} \equiv (x_{it1}, x_{it2}, \dots, x_{itk})$ is a $1 \times K$ vector
- Typically assume that cross-sectional units are i.i.d. draws from the population: $\{y_i, x_i, c_i\}_{i=1}^N \sim i.i.d.$ (cross-sectional independence)
 - $y_i \equiv (y_{i1}, y_{i2}, \dots, y_{iT})'$ and $x_i \equiv (x_{i1}, x_{i2}, \dots, x_{iT})$
 - Consider asymptotic properties with T fixed and $N \rightarrow \infty$

Notation

Single unit:

$$y_i = \begin{pmatrix} y_{i1} \\ \vdots \\ y_{it} \\ \vdots \\ y_{iT} \end{pmatrix}_{T \times 1} \quad X_i = \begin{pmatrix} X_{i,1,1} & X_{i,1,2} & X_{i,1,j} & \dots & X_{i,1,K} \\ \vdots & \vdots & \vdots & & \vdots \\ X_{i,t,1} & X_{i,t,2} & X_{i,t,j} & \dots & X_{i,t,K} \\ \vdots & \vdots & \vdots & & \vdots \\ X_{i,T,1} & X_{i,T,2} & X_{i,T,j} & \dots & X_{i,T,K} \end{pmatrix}_{T \times K}$$

Panel with all units:

$$y = \begin{pmatrix} y_1 \\ \vdots \\ y_i \\ \vdots \\ y_N \end{pmatrix}_{NT \times 1} \quad X = \begin{pmatrix} X_1 \\ \vdots \\ X_i \\ \vdots \\ X_N \end{pmatrix}_{NT \times K}$$

Unobserved heterogeneity

- For a randomly drawn cross-sectional unit i , the model is given by

$$y_{it} = x_{it}\beta + c_i + \varepsilon_{it}, \quad t = 1, 2, \dots, T$$

- When we ignore the panel structure and regress y_{it} on x_{it} we get

$$y_{it} = x_{it}\beta + v_{it}; \quad t = 1, 2, \dots, T$$

with composite error $v_{it} \equiv c_i + \varepsilon_{it}$

- What happens when we regress y_{it} on x_{it} if x is correlated with c_i ?
- Then x ends up correlated with v , the composite error term.
- Somehow we need to eliminate this bias, but how?

Fixed effects

- Our unobserved effects model is:

$$y_{it} = x_{it}\beta + c_i + \varepsilon_{it}; t = 1, 2, \dots, T$$

- If we have data on multiple time periods, we can think of c_i as **fixed effects** to be estimated
- OLS estimation with fixed effects yields

$$(\hat{\beta}, \hat{c}_1, \dots, \hat{c}_N) = \underset{b, m_1, \dots, m_N}{\operatorname{argmin}} \sum_{i=1}^N \sum_{t=1}^T (y_{it} - x_{it}b - m_i)^2$$

this amounts to including N individual dummies in regression of y_{it} on x_{it}

Fixed effects

Running a regression with the time-demeaned variables $\ddot{y}_{it} \equiv y_{it} - \bar{y}_i$ and $\ddot{x}_{it} \equiv x_{it} - \bar{x}$ is numerically equivalent to a regression of y_{it} on x_{it} and unit specific dummy variables.

Even better, the regression with the time demeaned variables is consistent for β even when $Cov[x_{it}, c_i] \neq 0$ because time-demeaning eliminates the unobserved effects

$$y_{it} = x_{it}\beta + c_i + \varepsilon_{it}$$

$$\bar{y}_i = \bar{x}_i\beta + c_i + \bar{\varepsilon}_i$$

$$(y_{it} - \bar{y}_i) = (x_{it} - \bar{x})\beta + (c_i - \bar{c}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i)$$

$$\ddot{y}_{it} = \ddot{x}_{it}\beta + \ddot{\varepsilon}_{it}$$

Fixed effects

- Identification assumptions:

1. $E[\varepsilon_{it}|x_{i1}, x_{i2}, \dots, x_{iT}, c_i] = 0; t = 1, 2, \dots, T$
 - regressors are strictly exogenous conditional on the unobserved effect
 - allows x_{it} to be arbitrarily related to c_i
2. $\text{rank}\left(\sum_{t=1}^T E[\ddot{x}'_{it} \ddot{x}_{it}]\right) = K$
 - regressors vary over time for at least some i and not collinear

Fixed effects

- Fixed effects estimator
 1. Demean and regress \ddot{y}_{it} on \ddot{x}_{it} (need to correct degrees of freedom)
 2. Regress y_{it} on x_{it} and unit dummies (dummy variable regression)
 3. Regress y_{it} on x_{it} with canned fixed effects routine
 - Stata: `xtreg y x, fe i(PanelID)`

Fixed effects

- Properties (under assumptions 1-2):
 - $\hat{\beta}_{FE}$ is consistent: $\underset{N \rightarrow \infty}{plim} \hat{\beta}_{FE,N} = \beta$
 - $\hat{\beta}_{FE}$ is unbiased conditional on \mathbf{X}

Fixed effects

- Inference:
 - Standard errors have to be “clustered” by panel unit (e.g., farm) to allow correlation in the ε_{it} ’s for the same i .
 - Yields valid inference as long as number of clusters is reasonably large

Application: Survey for Adult Service Providers

- From 2008-2009, I fielded a survey of Internet sex workers (685 respondents, 5% response rate)
- I asked two types of questions: static provider-specific information (e.g., age, weight) and dynamic session information over last 5 sessions
- Let's look at the panel aspect of this analysis together

Returns to risk

$$\begin{aligned} Y_{is} &= \beta X_i + \delta D_{is} + \gamma_{is} Z_{is} + c_i + \varepsilon_{is} \\ \ddot{Y}_{is} &= \delta \ddot{D}_{is} + \gamma_{is} \ddot{Z}_{is} + \ddot{\eta}_{is} \end{aligned}$$

where Y is log hourly price (i.e., gross price divided by session length in minutes times 60), D is unprotected sex with a client in session s , X are time invariant observable worker i characteristics, Z are time varying session s characteristics, and c_i is unobserved worker heterogeneity unchanging over time that is correlated with D_{is} .

Table: POLS, FE and Demeaned OLS Estimates of the Determinants of Log Hourly Price for a Panel of Sex Workers

| Depvar: | POLS | FE | Demeaned OLS |
|--|----------------------|----------------------|----------------------|
| Unprotected sex with client of any kind | 0.013 (0.028) | 0.051* (0.028) | 0.051* (0.026) |
| Ln(Length) | -0.308*** (0.028) | -0.435*** (0.024) | -0.435*** (0.019) |
| Client was a Regular | -0.047* (0.028) | -0.037** (0.019) | -0.037** (0.017) |
| Age of Client | -0.001 (0.009) | 0.002 (0.007) | 0.002 (0.006) |
| Age of Client Squared | 0.000 (0.000) | -0.000 (0.000) | -0.000 (0.000) |
| Client Attractiveness (Scale of 1 to 10) | 0.020*** (0.007) | 0.006 (0.006) | 0.006 (0.005) |
| Second Provider Involved | 0.055 (0.067) | 0.113* (0.060) | 0.113* (0.048) |
| Asian Client | -0.014 (0.049) | -0.010 (0.034) | -0.010 (0.030) |
| Black Client | 0.092 (0.073) | 0.027 (0.042) | 0.027 (0.037) |
| Hispanic Client | 0.052 (0.080) | -0.062 (0.052) | -0.062 (0.045) |
| Other Ethnicity Client | 0.156** (0.068) | 0.142*** (0.049) | 0.142*** (0.045) |
| Met Client in Hotel | 0.133*** (0.029) | 0.052* (0.027) | 0.052* (0.024) |
| Gave Client a Massage | -0.134*** (0.029) | -0.001 (0.028) | -0.001 (0.024) |
| Age of provider | 0.003 (0.012) | 0.000 (.) | 0.000 (.) |

Table: POLS, FE and Demeaned OLS Estimates of the Determinants of Log Hourly Price for a Panel of Sex Workers

| Depvar: | POLS | FE | Demeaned OLS |
|--|----------------------|--------------|---------------------|
| Body Mass Index | -0.022*** (0.002) | 0.000 (.) | 0.000 (.) |
| Hispanic | -0.226*** (0.082) | 0.000 (.) | 0.000 (.) |
| Black | 0.028 (0.064) | 0.000 (.) | 0.000 (.) |
| Other | -0.112 (0.077) | 0.000 (.) | 0.000 (.) |
| Asian | 0.086 (0.158) | 0.000 (.) | 0.000 (.) |
| Imputed Years of Schooling | 0.020** (0.010) | 0.000 (.) | 0.000 (.) |
| Cohabitating (living with a partner) but unmarried | -0.054 (0.036) | 0.000 (.) | 0.000 (.) |
| Currently married and living with your spouse | 0.005 (0.043) | 0.000 (.) | 0.000 (.) |
| Divorced and not remarried | -0.021 (0.038) | 0.000 (.) | 0.000 (.) |
| Married but not currently living with your spouse | -0.056 (0.059) | 0.000 (.) | 0.000 (.) |
| N | 1,028 | 1,028 | 1,028 |
| Mean of dependent variable | 5.57 | 5.57 | 0.00 |

Heteroskedastic robust standard errors in parenthesis clustered at the provider level. * p<0.10, ** p<0.05, *** p<0.01

Strict exogeneity

- So fixed effects estimates the parameter of interest under strict exogeneity, and since it eliminates time invariant heterogeneity in panel settings, the appeal was widespread as more panel data came online
- Strict exogeneity assumed constant treatment effects, though, and only most sophisticated instructors (i.e., not me) seemed to know and teach this
- Gardner (2021) shows that under treatment effect heterogeneity, the group and period fixed effects became correlated with a composite error term under differential timing, but we are going to need to first dive into potential outcomes to fully understand this

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Differential timing

- In the two group case, we can estimate the ATT under parallel trends using OLS with unit and time fixed effects
- If we have covariates, then we can use TWFE under restrictive assumptions, or we have other options (OR, IPW, DR)
- Now let's move to a more common scenario where we have more than two groups who get treated at various times

2x2 versus differential timing

- For this next part, we will decompose TWFE to understand what it needs for unbiasedness under differential timing
- All of this is from Goodman-Bacon (2022) though the expression of the weights is from 2018 for personal preference
- Goodman-Bacon (2022) shows that parallel trends is **not enough** for TWFE to be unbiased when treatment adoption is described by differential timing
- TWFE with differential timing uses treated groups as controls – not all estimators do – and this can introduce bias

Decomposition Preview

- TWFE estimates a parameter that is a weighted average over all 2x2 in your sample
- TWFE assigns weights that are a function of sample sizes of each “group” and the variance of the treatment dummies for those groups

Decomposition (cont.)

- TWFE needs two assumptions: that the variance weighted parallel trends are zero (far more parallel trends iow) and no dynamic treatment effects (not the case with 2x2)
- Under those assumptions, TWFE estimator estimates the variance weighted ATT as a weighted average of all possible ATTs

K^2 distinct DDs

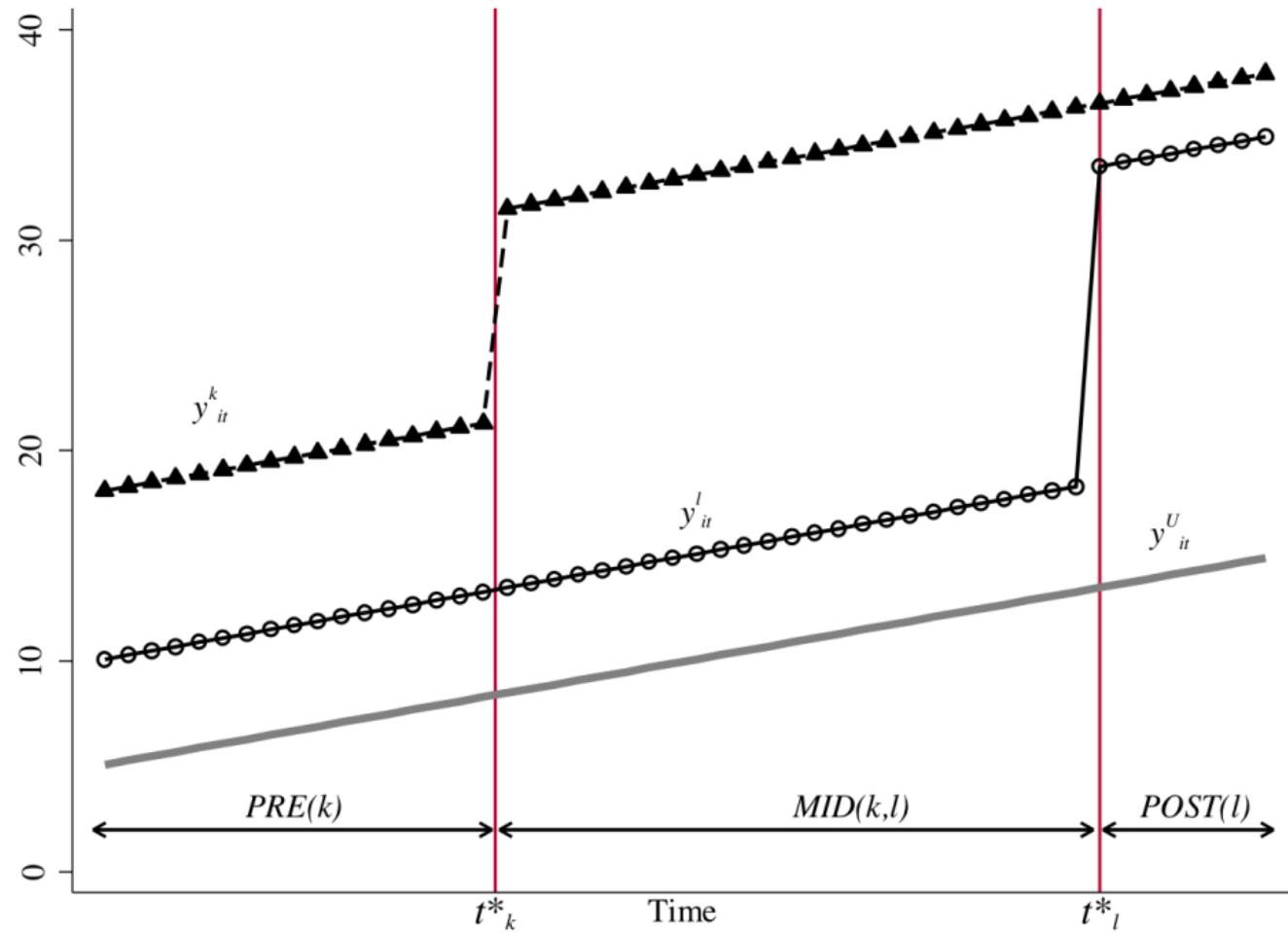
Let's look at 3 timing groups (a, b and c) and one untreated group (U).
With 3 timing groups, there are 9 2x2 DDs. Here they are:

| | | |
|--------|--------|--------|
| a to b | b to a | c to a |
| a to c | b to c | c to b |
| a to U | b to U | c to U |

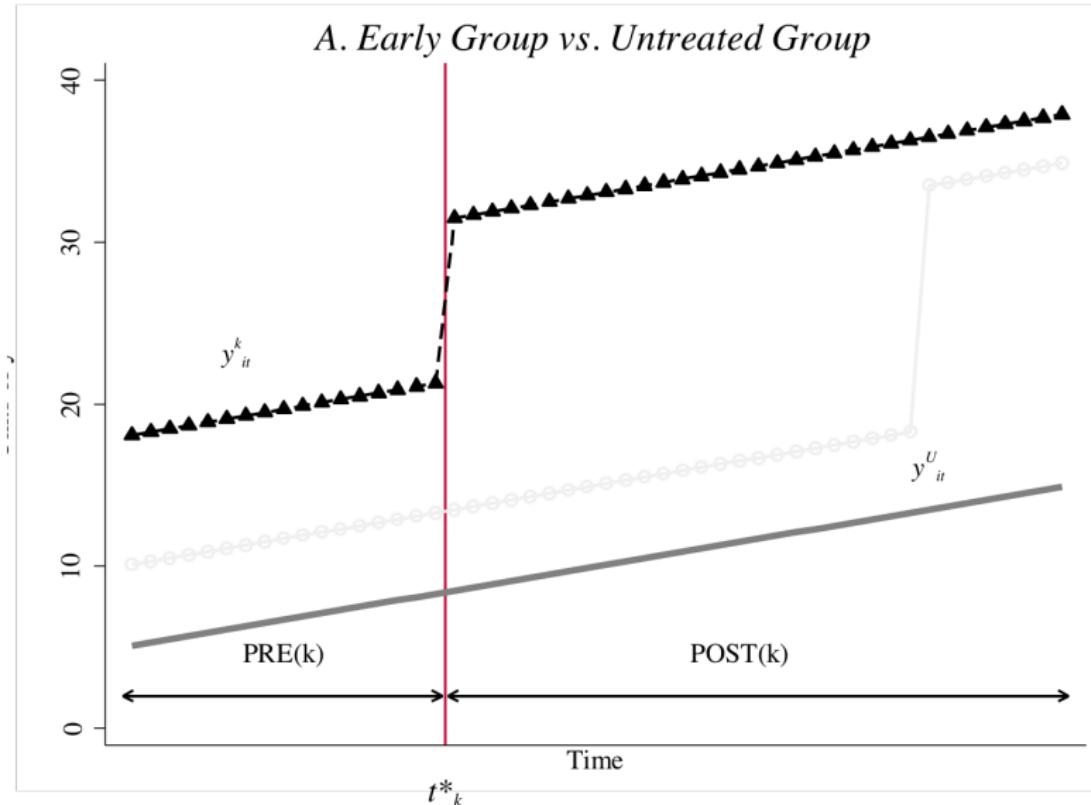
Let's return to a simpler example with only two groups – a k group treated at t_k^* and an l treated at t_l^* plus an never-treated group called the U untreated group

Terms and notation

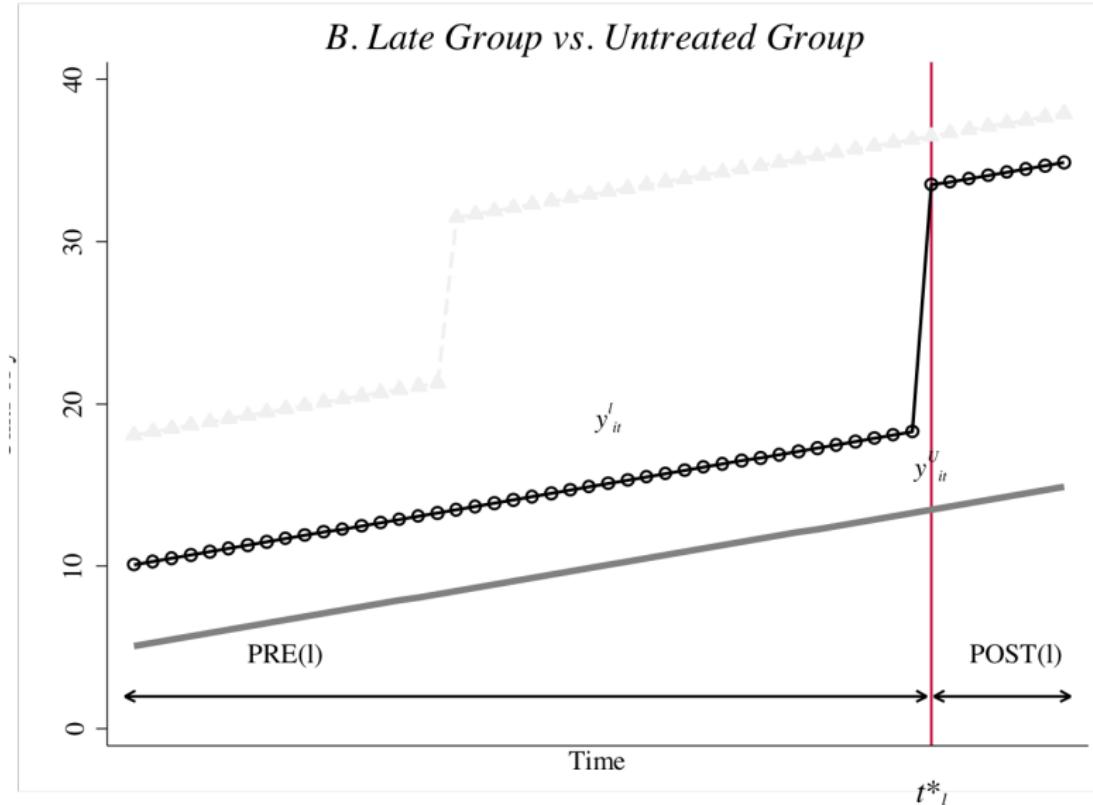
- Let there be two treatment groups (k, l) and one untreated group (U)
- k, l define the groups based on when they receive treatment (differently in time) with k receiving it earlier than l
- Denote \bar{D}_k as the share of time each group spends in treatment status
- Denote $\hat{\delta}_{jb}^{2x2}$ as the canonical 2×2 DD estimator for groups j and b where j is the treatment group and b is the comparison group



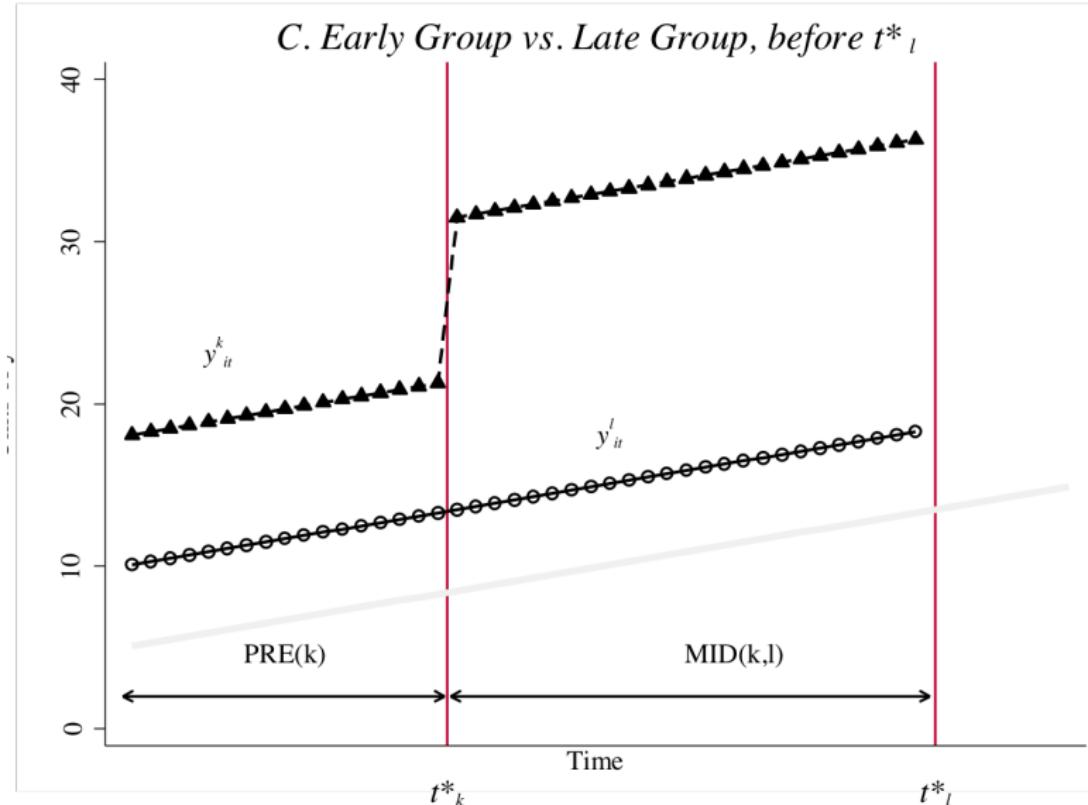
$$\widehat{\delta}_{kU}^{2x2} = \left(\overline{y}_k^{post(k)} - \overline{y}_k^{pre(k)} \right) - \left(\overline{y}_U^{post(k)} - \overline{y}_U^{pre(k)} \right)$$



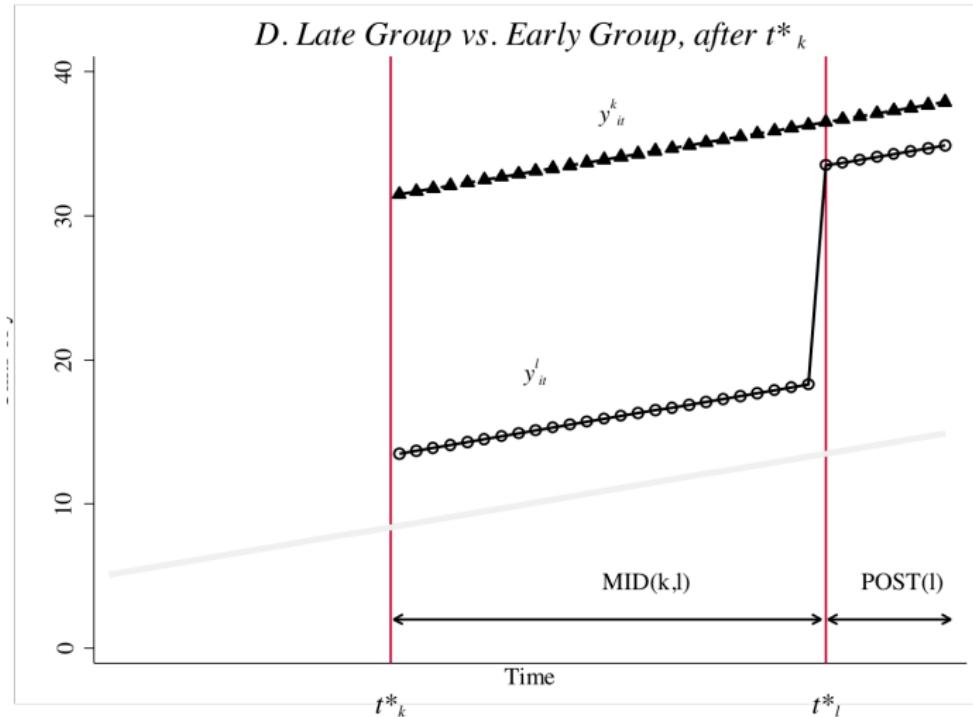
$$\widehat{\delta}_{lU}^{2x2} = \left(\overline{y}_l^{post(l)} - \overline{y}_l^{pre(l)} \right) - \left(\overline{y}_U^{post(l)} - \overline{y}_U^{pre(l)} \right)$$



$$\delta_{kl}^{2x2,k} = \left(\bar{y}_k^{MID(k,l)} - \bar{y}_k^{Pre(k,l)} \right) - \left(\bar{y}_l^{MID(k,l)} - \bar{y}_l^{PRE(k,l)} \right)$$



$$\delta_{lk}^{2x2,l} = \left(\bar{y}_l^{POST(k,l)} - \bar{y}_l^{MID(k,l)} \right) - \left(\bar{y}_k^{POST(k,l)} - \bar{y}_k^{MID(k,l)} \right)$$



Bacon decomposition

TWFE estimate yields a weighted combination of each groups' respective 2x2 (of which there are 4 in this example)

$$\widehat{\delta}^{DD} = \sum_{k \neq U} s_{kU} \widehat{\delta}_{kU}^{2x2} + \sum_{k \neq U} \sum_{l > k} s_{kl} \left[\mu_{kl} \widehat{\delta}_{kl}^{2x2,k} + (1 - \mu_{kl}) \widehat{\delta}_{lk}^{2x2,l} \right]$$

where that first 2x2 combines the k compared to U and the l to U
(combined to make the equation shorter)

Third, the Weights

$$\begin{aligned}s_{ku} &= \frac{n_k n_u \bar{D}_k (1 - \bar{D}_k)}{\widehat{Var}(\tilde{D}_{it})} \\ s_{kl} &= \frac{n_k n_l (\bar{D}_k - \bar{D}_l) (1 - (\bar{D}_k - \bar{D}_l))}{\widehat{Var}(\tilde{D}_{it})} \\ \mu_{kl} &= \frac{1 - \bar{D}_k}{1 - (\bar{D}_k - \bar{D}_l)}\end{aligned}$$

where n refer to sample sizes, $\bar{D}_k(1 - \bar{D}_k)$ ($\bar{D}_k - \bar{D}_l$) $(1 - (\bar{D}_k - \bar{D}_l))$ expressions refer to variance of treatment, and the final equation is the same for two timing groups.

Weights discussion

- Two things to note:
 - More units in a group, the bigger its 2x2 weight is
 - Group treatment variance weights up or down a group's 2x2
- Think about what causes the treatment variance to be as big as possible. Let's think about the s_{ku} weights.
 - $\bar{D} = 0.1$. Then $0.1 \times 0.9 = 0.09$
 - $\bar{D} = 0.4$. Then $0.4 \times 0.6 = 0.24$
 - $\bar{D} = 0.5$. Then $0.5 \times 0.5 = 0.25$
 - $\bar{D} = 0.6$. Then $0.6 \times 0.4 = 0.24$
- This means the weight on treatment variance is maximized for *groups treated in middle of the panel*

More weights discussion

- But what about the “treated on treated” weights (i.e., $\bar{D}_k - \bar{D}_l$)
- Same principle as before - when the difference between treatment variance is close to 0.5, those 2x2s are given the greatest weight
- For instance, say $t_k^* = 0.15$ and $t_l^* = 0.67$. Then $\bar{D}_k - \bar{D}_l = 0.52$. And thus $0.52 \times 0.48 = 0.2496$.

Summarizing TWFE centralities

- Groups in the middle of the panel weight up their respective 2x2s via the variance weighting
- Decomposition highlights the strange role of panel length when using TWFE
- Different choices about panel length change both the 2x2 and the weights based on variance of treatment

Moving from 2x2s to causal effects and bias terms

Let's start breaking down these estimators into their corresponding estimation objects expressed in causal effects and biases

$$\begin{aligned}\hat{\delta}_{kU}^{2x2} &= ATT_k Post + \Delta Y_k^0(Post(k), Pre(k)) - \Delta Y_U^0(Post(k), Pre) \\ \hat{\delta}_{kl}^{2x2} &= ATT_k(MID) + \Delta Y_k^0(MID, Pre) - \Delta Y_l^0(MID, Pre)\end{aligned}$$

These look the same because you're always comparing the treated unit with an untreated unit (though in the second case it's just that they haven't been treated yet).

The dangerous 2x2

But what about the 2x2 that compared the late groups to the already-treated earlier groups? With a lot of substitutions we get:

$$\widehat{\delta}_{lk}^{2x2} = ATT_{l,Post(l)} + \underbrace{\Delta Y_l^0(Post(l), MID) - \Delta Y_k^0(Post(l), MID)}_{\text{Parallel trends bias}} - \underbrace{(ATT_k(Post) - ATT_k(Mid))}_{\text{Heterogeneity bias!}}$$

Substitute all this stuff into the decomposition formula

$$\widehat{\delta}^{DD} = \sum_{k \neq U} s_{kU} \widehat{\delta}_{kU}^{2x2} + \sum_{k \neq U} \sum_{l > k} s_{kl} \left[\mu_{kl} \widehat{\delta}_{kl}^{2x2,k} + (1 - \mu_{kl}) \widehat{\delta}_{kl}^{2x2,l} \right]$$

where we will make these substitutions

$$\begin{aligned}\widehat{\delta}_{kU}^{2x2} &= ATT_k(Post) + \Delta Y_l^0(Post, Pre) - \Delta Y_U^0(Post, Pre) \\ \widehat{\delta}_{kl}^{2x2,k} &= ATT_k(Mid) + \Delta Y_l^0(Mid, Pre) - \Delta Y_l^0(Mid, Pre) \\ \widehat{\delta}_{lk}^{2x2,l} &= ATT_l Post(l) + \Delta Y_l^0(Post(l), MID) - \Delta Y_k^0(Post(l), MID) \\ &\quad - (ATT_k(Post) - ATT_k(Mid))\end{aligned}$$

Notice all those potential sources of biases!

Potential Outcome Notation

$$p \lim_{n \rightarrow \infty} \hat{\delta}_{n \rightarrow \infty}^{TWFE} = VWATT + VWPT - \Delta ATT$$

- Notice the number of assumptions needed even to estimate this very strange weighted ATT (which is a function of how you drew the panel in the first place).
- With dynamics, it attenuates the estimate (bias) and can even reverse sign depending on the magnitudes of what is otherwise effects in the sign in a reinforcing direction!
- Model can flip signs (does not satisfy a “no sign flip property”)

Simulated data

- 1000 firms, 40 states, 25 firms per states, 1980 to 2009 or 30 years, 30,000 observations, four groups
- $E[Y^0]$ satisfies “strong parallel trends” (stronger than necessary)

$$Y_{ist}^0 = \alpha_i + \gamma_t + \varepsilon_{ist}$$

- Also no anticipation of treatment effects until treatment occurs but does *not* guarantee homogenous treatment effects

Group-time ATT

| Year | ATT(1986,t) | ATT(1992,t) | ATT(1998,t) | ATT(2004,t) |
|------|-------------|-------------|-------------|-------------|
| 1980 | 0 | 0 | 0 | 0 |
| 1986 | 10 | 0 | 0 | 0 |
| 1987 | 20 | 0 | 0 | 0 |
| 1988 | 30 | 0 | 0 | 0 |
| 1989 | 40 | 0 | 0 | 0 |
| 1990 | 50 | 0 | 0 | 0 |
| 1991 | 60 | 0 | 0 | 0 |
| 1992 | 70 | 8 | 0 | 0 |
| 1993 | 80 | 16 | 0 | 0 |
| 1994 | 90 | 24 | 0 | 0 |
| 1995 | 100 | 32 | 0 | 0 |
| 1996 | 110 | 40 | 0 | 0 |
| 1997 | 120 | 48 | 0 | 0 |
| 1998 | 130 | 56 | 6 | 0 |
| 1999 | 140 | 64 | 12 | 0 |
| 2000 | 150 | 72 | 18 | 0 |
| 2001 | 160 | 80 | 24 | 0 |
| 2002 | 170 | 88 | 30 | 0 |
| 2003 | 180 | 96 | 36 | 0 |
| 2004 | 190 | 104 | 42 | 4 |
| 2005 | 200 | 112 | 48 | 8 |
| 2006 | 210 | 120 | 54 | 12 |
| 2007 | 220 | 128 | 60 | 16 |
| 2008 | 230 | 136 | 66 | 20 |
| 2009 | 240 | 144 | 72 | 24 |
| ATT | 82 | | | |

- Heterogenous treatment effects across time and across groups
- Cells are called “group-time ATT” (Callaway and Sant’anna 2020) or “cohort ATT” (Sun and Abraham 2020)
- ATT is weighted average of all cells and +82 with uniform weights 1/60

Estimation

Estimate the following equation using OLS:

$$Y_{ist} = \alpha_i + \gamma_t + \delta D_{it} + \varepsilon_{ist}$$

Table: Estimating ATT with different models

| Truth | (TWFE) | (CS) | (SA) | (BJS) |
|-----------------|--------|----------|------|-------|
| \widehat{ATT} | 82 | -6.69*** | | |

The sign flipped. Why? Because of extreme dynamics (i.e., $-\Delta ATT$)

Bacon decomposition

Table: Bacon Decomposition (TWFE = -6.69)

| DD Comparison | Weight | Avg DD Est |
|-----------------------|--------|------------|
| Earlier T vs. Later C | 0.500 | 51.800 |
| Later T vs. Earlier C | 0.500 | -65.180 |

T = Treatment; C= Comparison

$$(0.5 * 51.8) + (0.5 * -65.180) = -6.69$$

While large weight on the “late to early 2x2” is suggestive of an issue, these would appear even if we had constant treatment effects

Roadmap

Background material

Introduction

Two-way fixed effects

TWFE Pathologies

Bacon decomposition

Simulation

Two solutions and a new decomposition

CS

SA

dCH

Causal inference is imputation

"At some level, all methods for causal inference can be viewed as imputation methods, although some more explicitly than others." – Imbens and Rubin (2015)

Causal inference involves imputation

- Causal inference is a missing data problem – we are missing counterfactuals
- And recall that estimating the ATT necessarily involved correctly imputing the counterfactual using parallel trends
- OLS, therefore, is *implicitly* imputing counterfactuals for estimating the ATT

Callaway and Sant'Anna 2020

CS is a DiD model used for estimating ATT parameters under differential timing and conditional parallel trends

Difference-in-differences with multiple time periods

| | |
|------------------|--|
| Authors | Brantly Callaway, Pedro HC Sant'Anna |
| Publication date | 2021/12/1 |
| Journal | Journal of Econometrics |
| Volume | 225 |
| Issue | 2 |
| Pages | 200-230 |
| Publisher | North-Holland |
| Description | In this article, we consider identification, estimation, and inference procedures for treatment effect parameters using Difference-in-Differences (DiD) with (i) multiple time periods, (ii) variation in treatment timing, and (iii) when the “parallel trends assumption” holds potentially only after conditioning on observed covariates. We show that a family of causal effect parameters are identified in staggered DiD setups, even if differences in observed characteristics create non-parallel outcome dynamics between groups. Our identification results allow one to use outcome regression, inverse probability weighting, or doubly-robust estimands. We also propose different aggregation schemes that can be used to highlight treatment effect heterogeneity across different dimensions as well as to summarize the overall effect of participating in the treatment. We establish the asymptotic properties of the proposed estimators and prove the ... |

Total citations [Cited by 2378](#)



When is CS used

Just some examples of when you'd want to consider it:

1. When treatment effects differ depending on when it was adopted
2. When treatment effects change over time
3. When shortrun treatment effects more pronounced than longrun effects
4. When treatment effect dynamics differ if people are first treated in a recession relative to expansion years

In other words – CS is used to identify and aggregate heterogeneous treatment effects

Group-time ATT

| Year | ATT(1986,t) | ATT(1992,t) | ATT(1998,t) | ATT(2004,t) |
|------|-------------|-------------|-------------|-------------|
| 1980 | 0 | 0 | 0 | 0 |
| 1986 | 10 | 0 | 0 | 0 |
| 1987 | 20 | 0 | 0 | 0 |
| 1988 | 30 | 0 | 0 | 0 |
| 1989 | 40 | 0 | 0 | 0 |
| 1990 | 50 | 0 | 0 | 0 |
| 1991 | 60 | 0 | 0 | 0 |
| 1992 | 70 | 8 | 0 | 0 |
| 1993 | 80 | 16 | 0 | 0 |
| 1994 | 90 | 24 | 0 | 0 |
| 1995 | 100 | 32 | 0 | 0 |
| 1996 | 110 | 40 | 0 | 0 |
| 1997 | 120 | 48 | 0 | 0 |
| 1998 | 130 | 56 | 6 | 0 |
| 1999 | 140 | 64 | 12 | 0 |
| 2000 | 150 | 72 | 18 | 0 |
| 2001 | 160 | 80 | 24 | 0 |
| 2002 | 170 | 88 | 30 | 0 |
| 2003 | 180 | 96 | 36 | 0 |
| 2004 | 190 | 104 | 42 | 4 |
| 2005 | 200 | 112 | 48 | 8 |
| 2006 | 210 | 120 | 54 | 12 |
| 2007 | 220 | 128 | 60 | 16 |
| 2008 | 230 | 136 | 66 | 20 |
| 2009 | 240 | 144 | 72 | 24 |
| ATT | 82 | | | |

Each cell contains that group's ATT(g,t)

$$ATT(g, t) = E[Y_t^1 - Y_t^0 | G_g = 1]$$

CS identifies all feasible ATT(g,t)

Group-time ATT

Group-time ATT is the ATT for a specific group and time

- Groups are basically cohorts of units treated at the same time
- Group-time ATT estimates are simple (weighted) differences in means
- Does not directly restrict heterogeneity with respect to observed covariates, timing or the evolution of treatment effects over time
- Allows us ways to choose our aggregations
- Inference is the bootstrap

Notation

- T periods going from $t = 1, \dots, T$
- Units are either treated ($D_t = 1$) or untreated ($D_t = 0$) but once treated cannot revert to untreated state
- G_g signifies a group and is binary. Equals one if individual units are treated at time period t .
- C is also binary and indicates a control group unit equalling one if “never treated” (can be relaxed though to “not yet treated”) → Recall the problem with TWFE on using treatment units as controls
- Generalized propensity score enters into the estimator as a weight:

$$\widehat{p(X)} = \Pr(G_g = 1 | X, G_c + C = 1)$$

Assumptions

Assumption 1: Sampling is iid (panel data)

Assumption 2: Conditional parallel trends (for either never treated or not yet treated)

$$E[Y_t^0 - Y_{t-1}^0 | X, G_g = 1] = [Y_t^0 - Y_{t-1}^0 | X, C = 1]$$

Assumption 3: Irreversible treatment

Assumption 4: Common support (propensity score)

Assumption 5: Limited treatment anticipation (i.e., treatment effects are zero pre-treatment)

CS Estimator (the IPW version)

$$ATT(g, t) = E \left[\left(\frac{G_g}{E[G_g]} - \frac{\frac{\hat{p}(X)C}{1-\hat{p}(X)}}{E \left[\frac{\hat{p}(X)C}{1-\hat{p}(X)} \right]} \right) (Y_t - Y_{g-1}) \right]$$

This is the inverse probability weighting estimator. Alternatively, there is an outcome regression approach and a doubly robust. Sant'Anna recommends DR. Notice how CS doesn't use already-treated as controls.

Staggered adoption (i.e., universal coverage)

Proof.

Remark 1: In some applications, eventually all units are treated, implying that C is never equal to one. In such cases one can consider the “not yet treated” ($D_t = 0$) as a control group instead of the “never treated?” ($C = 1$). □

Aggregated vs single year/group ATT

- The method they propose is really just identifying very narrow ATT per group time.
- But we are often interested in more aggregate parameters, like the ATT across all groups and all times
- They present two alternative methods for building “interesting parameters”
- Inference from a bootstrap

Group-time ATT

| Truth | | | | | CS estimates | | | | |
|--------------|-------------|-------------|-------------|-------------|--------------|-------------|-------------|-------------|-------------|
| Year | ATT(1986,t) | ATT(1992,t) | ATT(1998,t) | ATT(2004,t) | Year | ATT(1986,t) | ATT(1992,t) | ATT(1998,t) | ATT(2004,t) |
| 1980 | 0 | 0 | 0 | 0 | 1981 | -0.0548 | 0.0191 | 0.0578 | 0 |
| 1986 | 10 | 0 | 0 | 0 | 1986 | 10.0258 | -0.0128 | -0.0382 | 0 |
| 1987 | 20 | 0 | 0 | 0 | 1987 | 20.0439 | 0.0349 | -0.0105 | 0 |
| 1988 | 30 | 0 | 0 | 0 | 1988 | 30.0028 | -0.0516 | -0.0055 | 0 |
| 1989 | 40 | 0 | 0 | 0 | 1989 | 40.0201 | 0.0257 | 0.0313 | 0 |
| 1990 | 50 | 0 | 0 | 0 | 1990 | 50.0249 | 0.0285 | -0.0284 | 0 |
| 1991 | 60 | 0 | 0 | 0 | 1991 | 60.0172 | -0.0395 | 0.0335 | 0 |
| 1992 | 70 | 8 | 0 | 0 | 1992 | 69.9961 | 8.013 | 0 | 0 |
| 1993 | 80 | 16 | 0 | 0 | 1993 | 80.0155 | 16.0117 | 0.0105 | 0 |
| 1994 | 90 | 24 | 0 | 0 | 1994 | 89.9912 | 24.0149 | 0.0185 | 0 |
| 1995 | 100 | 32 | 0 | 0 | 1995 | 99.9757 | 32.0219 | -0.0505 | 0 |
| 1996 | 110 | 40 | 0 | 0 | 1996 | 110.0465 | 40.0186 | 0.0344 | 0 |
| 1997 | 120 | 48 | 0 | 0 | 1997 | 120.0222 | 48.0338 | -0.0101 | 0 |
| 1998 | 130 | 56 | 6 | 0 | 1998 | 129.9164 | 56.0051 | 6.027 | 0 |
| 1999 | 140 | 64 | 12 | 0 | 1999 | 139.9235 | 63.9884 | 11.969 | 0 |
| 2000 | 150 | 72 | 18 | 0 | 2000 | 150.0087 | 71.9924 | 18.0152 | 0 |
| 2001 | 160 | 80 | 24 | 0 | 2001 | 159.9702 | 80.0152 | 23.9656 | 0 |
| 2002 | 170 | 88 | 30 | 0 | 2002 | 169.9857 | 88.0745 | 29.9757 | 0 |
| 2003 | 180 | 96 | 36 | 0 | 2003 | 179.981 | 96.0161 | 36.013 | 0 |
| 2004 | 190 | 104 | 42 | 4 | 2004 | | | | |
| 2005 | 200 | 112 | 48 | 8 | 2005 | | | | |
| 2006 | 210 | 120 | 54 | 12 | 2006 | | | | |
| 2007 | 220 | 128 | 60 | 16 | 2007 | | | | |
| 2008 | 230 | 136 | 66 | 20 | 2008 | | | | |
| 2009 | 240 | 144 | 72 | 24 | 2009 | | | | |
| ATT | 82 | | | | Total ATT | n/a | | | |
| Feasible ATT | 68.3333333 | | | | Feasible ATT | 68.33718056 | | | |

Question: Why didn't CS estimate all $\text{ATT}(g,t)$? What is "feasible ATT"?

Reporting results

Table: Estimating ATT using only pre-2004 data

| | (Truth) | (TWFE) | (CS) | (SA) | (BJS) |
|---------------------|----------------|---------------|-------------|-------------|--------------|
| <i>Feasible ATT</i> | 68.33 | 26.81 *** | 68.34*** | | |

TWFE is no longer negative, interestingly, once we eliminate the last group (giving us a never-treated group), but is still suffering from attenuation bias.

Event study and differential timing

- Event studies with one treatment group and one untreated group were relatively straightforward
- Interact treatment group with calendar date to get a series of leads and lags
- But when there are more than one treatment group, specification challenges emerge

Differential timing complicates plotting sample averages

- New Jersey treated in late 1992, New York in late 1993, Pennsylvania never treated
- What years are each state's post-treatment?
 - New Jersey: post-1992
 - New York: post-1993
 - Pennsylvania: ?
- How did people go about event studies then?

Early efforts at event studies

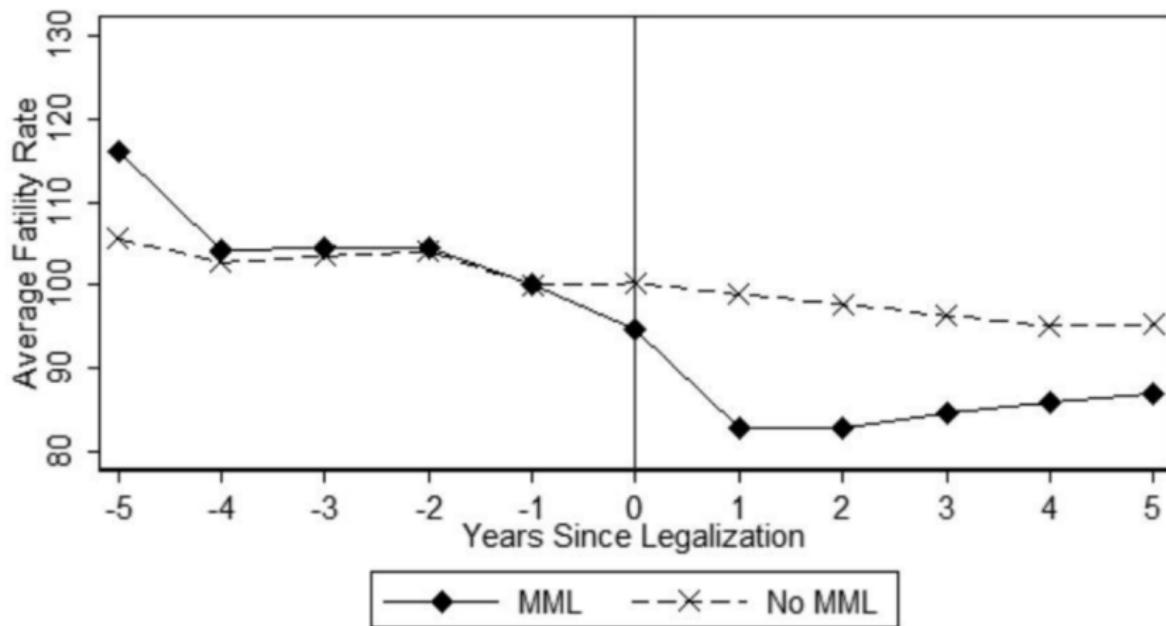


Figure: Anderson, et al. (2013) display of raw traffic fatality rates for re-centered treatment states and control states with randomized treatment dates

Replicated from a project of mine

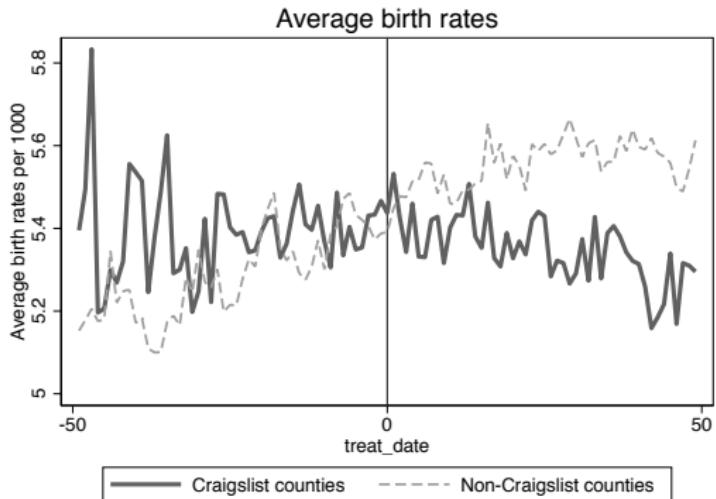
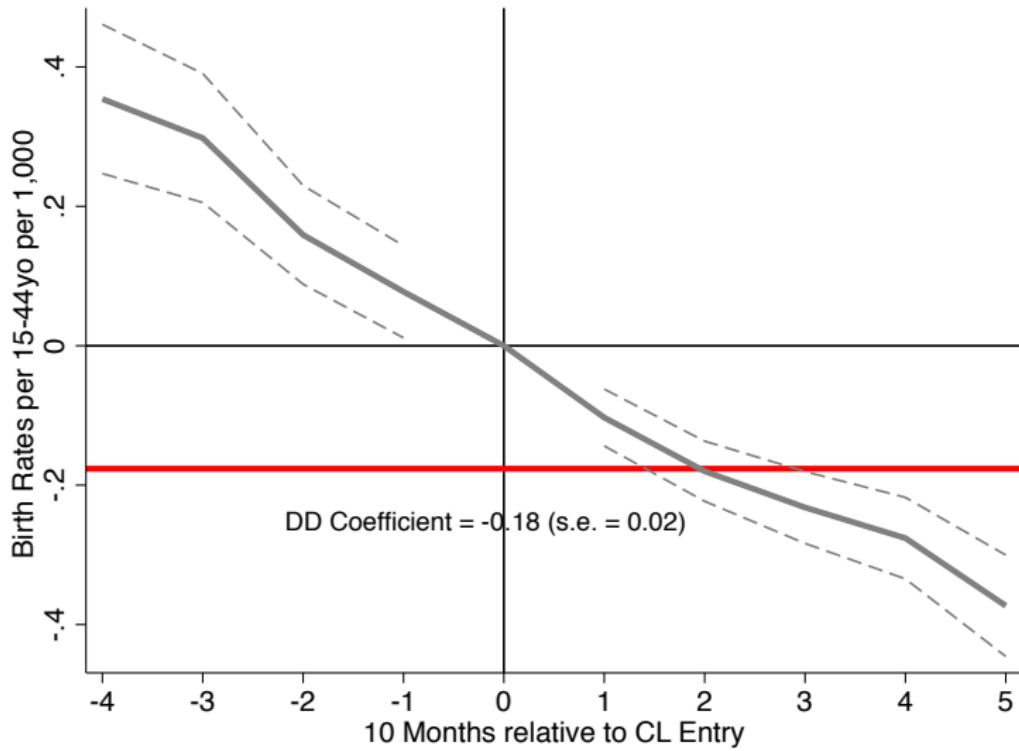


Figure: From one of my studies. Looks decent right?

Canonical event study specification with TWFE

$$Y_{i,t} = \alpha_i + \delta_t + \sum_{g \in G} \mu_g \mathbf{1}\{t - E_i \in g\} + \varepsilon_{i,t}$$

Coefficient μ_g on a dummy measuring the number of years prior to or after that unit was treated. This model, it turned out, suffered from model misspecification.



Same data as a couple slides ago, leads don't look good, so I abandoned the project.

Sun and Abraham 2020

- Now that we know about the biases of the constant treatment effect model estimated with TWFE, let's revisit event studies under differential timing
- Goodman-Bacon (2021, forthcoming) focused on decomposition of TWFE to show bias under differential timing
- Callaway and Sant'anna (2020) presents alternative estimator that yields unbiased estimates of group-time ATTs which can be aggregated or put into event study plots
- Sun and Abraham (SA) is like a combination of the two papers

Summarizing (cont.)

1. SA is a decomposition of the population regression coefficient on event study leads and lags with differential timing estimated with TWFE
2. They show that the population regression coefficient is "contaminated" by information from other leads and lags
3. SA presents an alternative estimator that is a version of CS only using the "last cohort" as the treatment group (not the not-yet-treated)

Summarizing (cont.)

- Under homogenous treatment profiles, weights sum to zero and “cancel out” the treatment effects from other periods
- Under treatment effect heterogeneity, they do not cancel out and leads and lags are biased
- They present a 3-step TWFE based alternative estimator which addresses the problems that they find

Some notation and terms

- As people often **bin** the data, we allow a lead or lag l to appear in bin g so sometimes they use g instead of l or $l \in g$
- Building block is the “cohort-specific ATT” or $CATT_{e,l}$ – same as $ATT(g,t)$
- Our goal is to estimate $CATT_{e,l}$ with population regression coefficient μ_l
- They focus on irreversible treatment where treatment status is non-decreasing sequence of zeroes and ones

Difficult notation (cont.)

- The ∞ symbol is used to either describe the group ($E_i = \infty$) or the potential outcome (Y^∞)
- $Y_{i,t}^\infty$ is the potential outcome for unit i if it had never received treatment (versus received it later), also called the baseline outcome
- Other counterfactuals are possible – maybe unit i isn't "never treated" but treated later in counterfactual

More difficult notation (cont.)

- Treatment effects are the difference between the observed outcome relative to the never-treated counterfactual outcome: $Y_{i,t} - Y_{i,t}^{\infty}$
- We can take the average of treatment effects at a given relative time period across units first treated at time $E_i = e$ (same cohort) which is what we mean by $CATT_{e,l}$
- Doesn't use t index time ("calendar time"), rather uses l which is time until or time after treatment date e ("relative time")
- Think of it as $l = \text{year} - \text{treatment date}$

Relative vs calendar event time

```
. list state-treat time_til in 1/10
```

| | state | firms | year | n | id | group | treat_~e | treat | time_til |
|-----|-------|----------|------|----|----|-------|----------|-------|----------|
| 1. | 1 | .3257218 | 1980 | 1 | 1 | 1 | 1986 | 0 | -6 |
| 2. | 1 | .3257218 | 1981 | 2 | 1 | 1 | 1986 | 0 | -5 |
| 3. | 1 | .3257218 | 1982 | 3 | 1 | 1 | 1986 | 0 | -4 |
| 4. | 1 | .3257218 | 1983 | 4 | 1 | 1 | 1986 | 0 | -3 |
| 5. | 1 | .3257218 | 1984 | 5 | 1 | 1 | 1986 | 0 | -2 |
| 6. | 1 | .3257218 | 1985 | 6 | 1 | 1 | 1986 | 0 | -1 |
| 7. | 1 | .3257218 | 1986 | 7 | 1 | 1 | 1986 | 1 | 0 |
| 8. | 1 | .3257218 | 1987 | 8 | 1 | 1 | 1986 | 1 | 1 |
| 9. | 1 | .3257218 | 1988 | 9 | 1 | 1 | 1986 | 1 | 2 |
| 10. | 1 | .3257218 | 1989 | 10 | 1 | 1 | 1986 | 1 | 3 |

Definition 1

Definition 1: The cohort-specific ATT l periods from initial treatment date e is:

$$CATT_{e,l} = E[Y_{i,e+l} - Y_{i,e+l}^{\infty} | E_i = e]$$

Fill out the second part of the Group-time ATT exercise together.

TWFE assumptions

- For consistent estimates of the coefficient leads and lags using TWFE model, we need three assumptions
- For SA and CS, we only need two
- Let's look then at the three

Assumption 1: Parallel trends

Assumption 1: Parallel trends in baseline outcomes:

$E[Y_{i,t}^\infty - Y_{i,s}^\infty | E_i = e]$ is the same for all $e \in supp(E_i)$ and for all s, t and is equal to $E[Y_{i,t}^\infty - Y_{i,s}^\infty]$

Lead and lag coefficients are DiD equations but once we invoke parallel trends they can become causal parameters. This reminds us again how crucial it is to have appropriate controls

Assumption 2: No anticipation

Assumption 2: No anticipator behavior in pre-treatment periods:

There is a set of pre-treatment periods such that

$$E[Y_{i,e+l}^e - Y_{i,e+l}^\infty | E_i = e] = 0 \text{ for all possible leads.}$$

Essentially means that pre-treatment, the causal effect is zero. Most plausible if no one sees the treatment coming, but even if they see it coming, they may not be able to make adjustments that affect outcomes

Assumption 3: Homogeneity

Assumption 3: Treatment effect profile homogeneity: For each relative time period l , the $CATT_{e,l}$ doesn't depend on the cohort and is equal to $CATT_l$.

Treatment effect heterogeneity

- Assumption 3 is violated when different cohorts experience different paths of treatment effects
- Cohorts may differ in their covariates which affect how they respond to treatment (e.g., if treatment effects vary with age, and there is variation in age across units first treated at different times, then there will be heterogeneous treatment effects)
- Doesn't rule out parallel trends

Event study model

$$Y_{i,t} = \alpha_i + \delta_t + \sum_{g \in G} \mu_g 1\{t - E_i \in g\} + \varepsilon_{i,t}$$

We are interested in the properties of μ_g under differential timing as well as whether there are any never-treated units

Specifying the leads and lags

How will we specify the $1\{t - E_i \in g\}$ term? SA considers a couple:

1. Static specification:

$$Y_{i,t} = \alpha_i + \delta_t + \mu_g \sum_{l \geq 0} D_{i,t}^l + \varepsilon_{i,t}$$

2. Dynamic specification:

$$Y_{i,t} = \alpha_i + \delta_t + \sum_{l=-K}^{-2} \mu_l D_{i,t}^l + \sum_{l=0}^L \mu_l D_{i,t}^l + \varepsilon_{i,t}$$

Dropping, trimming and binning

- Dynamic specification with differential timing requires dropping two leads:
 1. Drop the baseline to avoid multicollinearity in the relative time indicators
 2. Drop a second one because of the multicollinearity coming from the linear relationship between TWFE and the relative period indicators.
- Binning means placing all “distant” relative time indicators into a single one due to imbalance in relative event time
- Trimming is done for the same reason but drops any relative time period for which you do not have balance

Interpreting $\widehat{\mu}_g$ under no to all assumptions

Proposition 1 (no assumptions): The population regression coefficient on relative period bin g is a linear combination of differences in trends from its own relative period $l \in g$, from relative periods $l \in g'$ of other bins $g' \neq g$, and from relative periods excluded from the specification (e.g., trimming).

$$\begin{aligned} \mu_g = & \underbrace{\sum_{l \in g} \sum_e w_{e,l}^g (E[Y_{i,e+l} - Y_{i,0}^\infty | E_i = e] - E[Y_{i,e+l}^\infty - Y_{i,0}^\infty])}_{\text{Targets}} \\ & + \underbrace{\sum_{g' \neq g} \sum_{l \in g'} \sum_e w_{e,l}^g (E[Y_{i,e+l} - Y_{i,0}^\infty | E_i = e] - E[Y_{i,e+l}^\infty - Y_{i,0}^\infty])}_{\text{Contamination from other leads and lags}} \\ & + \underbrace{\sum_{l \in g^{excl}} \sum_e w_{e,l}^g (E[Y_{i,e+l} - Y_{i,0}^\infty | E_i = e] - E[Y_{i,e+l}^\infty - Y_{i,0}^\infty])}_{\text{Contamination from dropped periods}} \end{aligned}$$

Weight ($w_{e,l}^g$) summation cheat sheet

1. For relative periods of μ_g own $l \in g$, $\sum_{l \in g} \sum_e w_{e,l}^g = 1$
2. For relative periods belonging to some other bin $l \in g'$ and $g' \neq g$,
 $\sum_{l \in g'} \sum_e w_{e,l}^g = 0$
3. For relative periods not included in G , $\sum_{l \in g^{excl}} \sum_e w_{e,l}^g = -1$

Estimating the weights

Regress $D_{i,t}^l \times 1\{E_i = e\}$ on:

1. all bin indicators included in the main TWFE regression,
2. $\{1\{t - E_i \in g\}\}_{g \in G}$ (i.e., leads and lags) and
3. the unit and time fixed effects

Still biased under parallel trends

Proposition 2: Under the parallel trends only, the population regression coefficient on the indicator for relative period bin g is a linear combination of $CATT_{e,l \in g}$ as well as $CATT_{d,l'}$ from other relative periods $l' \notin g$ with the same weights stated in Proposition 1:

$$\begin{aligned}\mu_g = & \underbrace{\sum_{l \in g} \sum_e w_{e,l}^g CATT_{e,l}}_{\text{Desirable}} \\ & + \underbrace{\sum_{g' \neq g, g' \in G} \sum_{l' \in g'} \sum_e w_{e,l'}^g CATT_{e,l'}}_{\text{Bias from other specified bins}} \\ & + \underbrace{\sum_{l' \in g^{excl}} \sum_e w_{e,l'}^g CATT_{e,l'}}_{\text{Bias from dropped relative time indicators}}\end{aligned}$$

Still biased under parallel trends and no anticipation

Proposition 3: If parallel trends holds and no anticipation holds for all $l < 0$ (i.e., no anticipatory behavior pre-treatment), then the population regression coefficient μ_g for g is a linear combination of post-treatment $CATT_{e,l'}$ for all $l' \geq 0$.

$$\begin{aligned}\mu_g = & \sum_{l' \in g, l' \geq 0} \sum_e w_{e,l'}^g CATT_{e,l'} \\ & + \sum_{g' \neq g, g' \in G} \sum_{l' \in g', l' \geq 0} \sum_e w_{e,l'}^g CATT_{e,l'} \\ & + \sum_{l' \in g^{excl}, l' \geq 0} \sum_e w_{w,l'}^g CATT_{e,l'}\end{aligned}$$

Proposition 3 comment

Notice how once we impose zero pre-treatment treatment effects, those terms are gone (i.e., no $l \in g, l < 0$). But the second term remains unless we impose treatment effect homogeneity (homogeneity causes terms due to weights summing to zero to cancel out). Thus μ_g may be non-zero for pre-treatment periods even *though parallel trends hold in the pre period.*

Proposition 4

Proposition 4: If parallel trends and treatment effect homogeneity, then $CATT_{e,l} = ATT_l$ is constant across e for a given l , and the population regression coefficient μ_g is equal to a linear combination of $ATT_{l \in g}$, as well as $ATT_{l' \notin g}$ from other relative periods

$$\begin{aligned}\mu_g &= \sum_{l \in g} w_l^g ATT_l \\ &+ \sum_{g' \neq g} \sum_{l' \in g'} w_{l'}^g ATT_{l'} \\ &+ \sum_{l' \in g^{excl}} w_{l'}^g ATT_{l'}\end{aligned}$$

Simple example

Balanced panel $T = 2$ with cohorts $E_i \in \{1, 2\}$. For illustrative purposes, we will include bins $\{-2, 0\}$ in our calculations but drop $\{-1, 1\}$.

Simple example

$$\begin{aligned}\mu_{-2} = & \underbrace{CATT_{2,-2}}_{\text{own period}} + \underbrace{\frac{1}{2}CATT_{1,0} - \frac{1}{2}CATT_{2,0}}_{\text{other included bins}} \\ & + \underbrace{\frac{1}{2}CATT_{1,1} - CATT_{1,-1} - \frac{1}{2}CATT_{2,-1}}_{\text{Excluded bins}}\end{aligned}$$

- Parallel trends gets us to all of the $CATT$
- No anticipation makes $CATT = 0$ for all $l < 0$ (all $l < 0$ cancel out)
- Homogeneity cancels second and third terms
- Still leaves $\frac{1}{2}CATT_{1,1}$ – you chose to exclude a group with a treatment effect

Lesson: drop the relative time indicators on the left, not things on the right, bc lagged effects will contaminate through the excluded bins

Robust event study estimation

- All the robust estimators under differential timing have solutions and they all skip over forbidden contrasts.
- Sun and Abraham (2020) propose a 3-step interacted weighted estimator (IW) using last treated group as control group
- Callaway and Sant'anna (2020) estimate group-time ATT which can be a weighted average over relative time periods too but uses "not-yet-treated" as control

Interaction-weighted estimator

- **Step one:** Do this DD regression and hold on to $\widehat{\delta}_{e,l}$

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{e \notin C} \sum_{l \neq -1} \delta_{e,l} (1\{E_i = e\} \cdot D_{i,t}^l) + \varepsilon_{i,t}$$

Can use never-treated or last-treated cohort. Drop always treated. The $\delta_{e,l}$ is a DD estimator for $CATT_{e,l}$ with particular choices for pre-period and cohort controls

Interaction-weighted estimator

- **Step two:** Estimate weights using sample shares of each cohort in the relevant periods:

$$Pr(E_i = e | E_i \in [-l, T - l])$$

Interaction-weighted estimator

- **Step three:** Take a weighted average of estimates for $CATT_{e,l}$ from Step 1 with weight estimates from step 2

$$\hat{v}_g = \frac{1}{|g|} \sum_{l \in g} \sum_e \hat{\delta}_{e,l} \widehat{Pr}\{E_i = e | E_i \in [-l, T - l]\}$$

Consistency and Inference

- Under parallel trends and no anticipation, $\hat{\delta}_{e,l}$ is consistent, and sample shares are also consistent estimators for population shares.
- Thus IW estimator is consistent for a weighted average of $CATT_{e,l}$ with weights equal to the share of each cohort in the relevant period(s).
- They show that each IW estimator is asymptotically normal and derive its asymptotic variance. Doesn't rely on bootstrap like CS.

DD Estimator of CATT

Definition 2: DD estimator with pre-period s and control cohorts C estimates $CATT_{e,l}$ as:

$$\widehat{\delta}_{e,l} = \frac{E_N[(Y_{i,e+l} - Y_{i,s}) \times 1\{E_i = e\}]}{E_N[1\{E_i = e\}]} - \frac{E_N[(Y_{i,e+l} \times 1\{E_i \in C\})]}{E_N[1\{E_i \in C\}]}$$

Proposition 5: If parallel trends and no anticipation both hold for all pre-periods, then the DD estimator using any pre-period and non-empty control cohorts (never-treated or not-yet-treated) is an unbiased estimate for $CATT_{e,l}$.

Software

- **Stata:** eventstudyinteract (can be installed from ssc)
- **R:** fixest with subab() option (see
<https://lrberge.github.io/fixest/reference/sunab.html/>)

Reporting results

Table: Estimating ATT

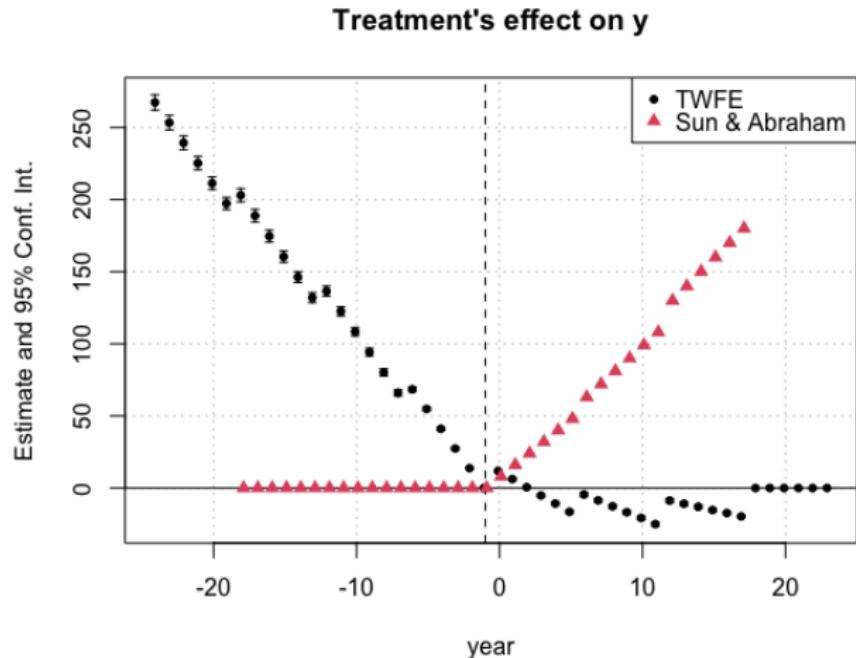
| | (Truth) | (TWFE) | (CS) | (SA) | (BJS) |
|---------------------------------|----------------|---------------|-------------|-------------|--------------|
| <i>Feasible</i> \widehat{ATT} | 68.33 | 26.81*** | 68.34*** | 68.33*** | |

Computing relative event time leads and lags

| Year | Truth | | | | Relative time coefficients | | |
|------|-------------|-------------|-------------|-------------|----------------------------|-------|--------|
| | ATT(1986,t) | ATT(1992,t) | ATT(1998,t) | ATT(2004,t) | Leads | Truth | SA |
| 1980 | 0 | 0 | 0 | 0 | t-2 | 0 | 0.02 |
| 1986 | 10 | 0 | 0 | 0 | t | 8 | 8.01 |
| 1987 | 20 | 0 | 0 | 0 | t+1 | 16 | 16.00 |
| 1988 | 30 | 0 | 0 | 0 | t+2 | 24 | 24.00 |
| 1989 | 40 | 0 | 0 | 0 | t+3 | 32 | 31.99 |
| 1990 | 50 | 0 | 0 | 0 | t+4 | 40 | 40.00 |
| 1991 | 60 | 0 | 0 | 0 | t+5 | 48 | 48.01 |
| 1992 | 70 | 8 | 0 | 0 | t+6 | 63 | 62.99 |
| 1993 | 80 | 16 | 0 | 0 | t+7 | 72 | 72.00 |
| 1994 | 90 | 24 | 0 | 0 | t+8 | 81 | 80.99 |
| 1995 | 100 | 32 | 0 | 0 | t+9 | 90 | 89.98 |
| 1996 | 110 | 40 | 0 | 0 | t+10 | 99 | 99.06 |
| 1997 | 120 | 48 | 0 | 0 | t+11 | 108 | 108.01 |
| 1998 | 130 | 56 | 6 | 0 | t+12 | 130 | 129.92 |
| 1999 | 140 | 64 | 12 | 0 | t+13 | 140 | 139.92 |
| 2000 | 150 | 72 | 18 | 0 | t+14 | 150 | 150.01 |
| 2001 | 160 | 80 | 24 | 0 | t+15 | 160 | 159.97 |
| 2002 | 170 | 88 | 30 | 0 | t+16 | 170 | 169.99 |
| 2003 | 180 | 96 | 36 | 0 | t+17 | 180 | 179.98 |
| 2004 | 190 | 104 | 42 | 4 | | | |
| 2005 | 200 | 112 | 48 | 8 | | | |
| 2006 | 210 | 120 | 54 | 12 | | | |
| 2007 | 220 | 128 | 60 | 16 | | | |
| 2008 | 230 | 136 | 66 | 20 | | | |
| 2009 | 240 | 144 | 72 | 24 | | | |

Two things to notice: (1) there only 17 lags with robust models but will be 24 with TWFE; (2) changing colors mean what?

Comparing TWFE and SA



Question: why is TWFE *falling* pre-treatment? Why is SA rising, but jagged, post-treatment?

de Chaisemartin and D'Haultfoeuille 2020

de Chaisemartin and D'Haultfouelle 2020 (dCdH) is different from the other papers in several ways

- Like SA, it's a diagnosis and a cure
- TWFE decomposition shows coefficient a weighted average of underlying treatment effects, but weights can be negative negating causal interpretation
- Propose a solution for both static and dynamic specification which does not use already treated as controls
- Treatment can turn on and off

Comment on Bacon

- Recall the Bacon decomposition – TWFE coefficients are decomposed into weighted average of all underlying 2x2s. Weights were non-negative and summed to one.
- But this decomposition was more a numerical decomposition – what exactly adds up to equal the TWFE coefficient using the data we observe?
- Bacon's decomposition is not “theoretical” – not in the way that other decompositions are. He is just explaining what OLS “does” when it calculates $\hat{\delta}$
- Just explains what comparisons OLS is using to calculate the TWFE coefficient – just peels back the curtain.

Negative weights

- dCdH impose causal assumptions and try a different decomposition strategy
- Uses as its building block the unit-specific treatment effects
- Their decomposition will reveal negative weights on the underlying treatment effects (similar to negative weight on dynamics with Bacon)
- Remember though: the Bacon decomposition weights were *always* positive, because they were numerical weights (not theoretical weights) on the underlying 2x2s (not the treatment effects)

Turning on and off

- CS and SA both require interventions to turn on and stay on
- dCdH allows for “switching” on and off
- Before we move quickly into that, please note that the researcher bears the burden of knowing whether in fact you want to impose symmetry on turning on and off
- Roe v Wade “turned on” legalized abortion and 2022 it was “turned off” – do we want to treat these as simply a single policy flipping of the switch or two separate policies?

dCdH notation

- Individual treatment effects (iow, not the group-time ATT):

$$\Delta_{i,t}^g = Y_{i,t}^1 - Y_{i,t}^\infty$$

but where the treatment is in time period g . Notice –it's not the ATT
(it's i individual treatment effect)

- with defined error term as $\varepsilon_{i,t}$:

$$D_{i,t} = \alpha_i + \alpha_t + \varepsilon_{i,t}$$

- Weights:

$$w_{i,t} = \frac{\varepsilon_{i,t}}{\frac{1}{N^T} \sum_{i,t:D_{i,t}=1} \varepsilon_{i,t}}$$

Parallel trend assumption

Strong unconditional PT

Assume that for every time period t and every group g, g' ,

$$E[Y_t^\infty - Y_{t-1}^\infty | G = g] = E[Y_t^\infty - Y_{t-1}^\infty | G = g']$$

Assume parallel trends for every unit in every cohort in every time period.

What then does TWFE estimate with differential timing?

dCdH Theorem

Theorem – dCdH decomposition

Assuming SUTVA, no anticipation and the strong PT, then let δ be the TWFE estimand associated with

$$Y_{i,t} = \alpha_i + \alpha_t + \delta D_{i,t} + \varepsilon_{i,t}$$

Then it follows that

$$\delta = E \left[\sum_{i,t:D_{i,t}=1} \frac{1}{N^T} w_{i,t} \cdot \Delta_{i,t}^g \right]$$

where $\sum_{i,t:D_{i,t}=1} \frac{w_{i,t}}{N^T} = 1$ but $w_{i,t}$ can be negative

Origins

- So once you run that specification, $\hat{\delta}$ is going to recover a “non-convex average” over all unit level treatment effects (weights can be negative, more on this).
- Not sure who came first, because there were working papers before publications, but my understanding is dCdH was the first to prove this
- Very important theorem – established the “no sign flip property” for OLS with differential timing in the canonical static specification

Negative weights

- Very common now to hear about negative weights, and furthermore, that negative weights wipe out any causal interpretation, but why?
- Thought experiment: imagine every unit gained from the treatment, but their treatment effect when estimated was multiplied by a negative number
- It's possible it could flip the sign, but it would definitely at least pull the estimate away from the true effect
- This is dangerous – and it's caused by the forbidden contrasts (comparing treated to already treated) which is what the canonical TWFE static specification is doing (for many of us unknowingly)

Negative weights

- Doesn't always pose a problem, but no proofs for this intuition known yet
- A large number of never-treated seems to make this less an issue
- Shrinking the spacing between treatment dates also can drive it down
- But does that mean that TWFE works, and what does it mean to work?
- TWFE still even when all the weights are positive the weighted average may not aggregate to what we think it does

Weighting

- The weights in OLS all come out of the model itself, *not the economic question*
- The economic question is “what parameter do you want? What does it look like? Who is in it?”
- And when you define the parameter up front, you’ve more or less defined the economic question you’re asking
- But OLS sort of ignores your question and just gives you what it wants

Weighting

- What makes something a good vs a bad weight?
- Not being negative is the absolute minimal requirement
- But it's also not a good sign if you can't really explain the weights

dCdH Solution

- dCdH propose an alternative that doesn't have the problems of TWFE
 - both avoiding negative weights and improving interpretability
- Recall, their model can handle reversible treatments