Causal Inference II

Mixtape Session

MIXTAPE SESSIONS

Roadmap

- Differential timing
  - Twoway fixed effects vs Pooled OLS
  - Diff-in-diff wars
  - TWFE Pathologies
  - Simulation
  - Castle doctrine reform
- Implicit imputation
  - CS
  - SA
  - dCH

Twoway fixed effects

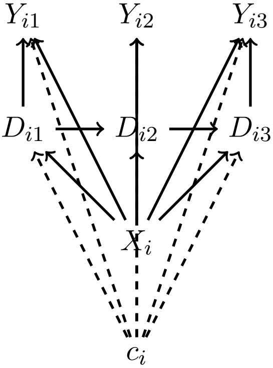
- When working with panel data, the so-called “twoway fixed effects” (TWFE) estimator is the workhorse estimator
- It’s easy to run, a version of OLS, and many people are just interested in mean effects anyway
- It’s the most common model for estimating treatment effects in a difference-in-differences, and so for all these reasons, we need to spend some time understanding what it is

Panel Data

- Panel data: we observe the same units (individuals, firms, countries, schools, etc.) over several time periods
- Often our outcome variable depends on unobserved factors which are also correlated with our explanatory variable of interest
- If these omitted variables are constant over time, we can use panel data estimators to consistently estimate the effect of our explanatory variable

What I will cover

- I will cover pooled OLS and twoway fixed effects
- But I won't be covering random effects, Arrelano and Bond and any number of important panel estimators because the purpose here is to present the modal regression model used in difference-in-differences



Sorry - drawing the DAG for a simple panel model is somewhat messy!

When to use this

- Traditionally, this was used for estimating constant treatment effects with unobserved time-invariant heterogeneity – recall the c_i was constant across all time periods
- 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

Problems that fixed effects cannot solve

- Reverse causality: Becker predicted police reduce crime, but when you regress crime onto police, it's usually positive
  - → $\widehat{\beta}_{FE}$ inconsistent unless strict exogeneity conditional on c_i holds
    - ■ $E[\varepsilon_{it}|x_{i1},x_{i2},\dots,x_{iT},c_i]=0; t=1,2,\dots,T$
    - ■ implies ε_{it} uncorrelated with past, current and future regressors
- Time-varying unobserved heterogeneity
  - → It's the time-varying unobservables you have to worry about in fixed effects
  - → Can include time-varying controls, but as always, don't condition on a collider

Formal panel notation

- Let y and $x \equiv (x_1, x_2, \ldots, x_k)$ 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

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

Formal panel notation cont.

- We observe a sample of $i = 1, 2, \ldots, N$ cross-sectional units for $t = 1, 2, \ldots, T$ time periods (a balanced panel)
  - → For each unit i, we denote the observable variables for all time periods as $\{(y_{it}, x_{it}) : t = 1, 2, \ldots, T\}$
  - → $x_{it} \equiv (x_{it1}, x_{it2}, \ldots, x_{itk})$ 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)
  - → $y_i \equiv (y_{i1}, y_{i2}, \ldots, y_{iT})'$ and $x_i \equiv (x_{i1}, x_{i2}, \ldots, x_{iT})$
  - → Consider asymptotic properties with T fixed and $N \to \infty$

Formal panel notation

Single unit:

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

Panel with all units:

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

Unobserved heterogeneity

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

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

→ y_{it}: log wages i in year t

→ $x_{it} : 1 \times K$ vector of variable events for person i in year t, such as education, marriage, etc. plus an intercept

→ $\beta : K \times 1$ vector of marginal effects of events

→ c_i: sum of all time-invariant inputs known to people i (but unobserved for the researcher), e.g., ability, beauty, grit, etc., often called unobserved heterogeneity or fixed effect

→ ε_{it}: time-varying unobserved factors, such as a recession, unknown to the farmer at the time the decision on the events x_{it} are made, sometimes called idiosyncratic error

Pooled OLS

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

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

  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?

Pooled OLS

- Main assumption to obtain consistent estimates for β is:
  - → $E[v_{it}|x_{i1}, x_{i2}, \ldots, x_{iT}] = E[v_{it}|x_{it}] = 0$ for $t = 1, 2, \ldots, T$
    - ■ x_{it} are strictly exogenous: the composite error v_{it} in each time period is uncorrelated with the past, current and future regressors
    - ■ But: education x_{it} likely depends on grit and ability c_i and so we have omitted variable bias and $\widehat{\beta}$ is not consistent
  - → No correlation between x_{it} and v_{it} implies no correlation between unobserved effect c_i and x_{it} for all t
    - ■ Violations are common: whenever we omit a time-constant variable that is correlated with the regressors (heterogeneity bias)
  - → Additional problem: v_{it} are serially correlated for same i since c_i is present in each t and thus pooled OLS standard errors are invalid

Pooled OLS

- Always ask: is there a time-constant unobserved variable (c_i) that is correlated with the regressors?
- If yes, then pooled OLS is problematic
- This is how we motivate a fixed effects model: because we believe unobserved heterogeneity is the main driving force making the treatment variable endogenous

Fixed effect regression

- Our unobserved effects model is:

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

- 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

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

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

Derivation: fixed effects regression

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

The first-order conditions (FOC) for this minimization problem are:

$$\sum_{i=1}^{N} \sum_{t=1}^{T} x_{it}' (y_{it} - x_{it} \widehat{\beta} - \widehat{c}_i) = 0$$

and

$$\sum_{t=1}^{T} (y_{it} - x_{it} \widehat{\beta} - \widehat{c}_i) = 0$$

for $i = 1, \ldots, N$.

Derivation: fixed effects regression

Therefore, for $i = 1, \ldots, N$,

$$\widehat{c}_i = \frac{1}{T}\sum_{t=1}^{T}(y_{it} - x_{it}\widehat{\beta}) = \bar{y}_i - \bar{x}_i\widehat{\beta},$$

where

$$\bar{x}_i \equiv \frac{1}{T}\sum_{t=1}^{T} x_{it}; \bar{y}_i \equiv \frac{1}{T}\sum_{t=1}^{T} y_{it}$$

Plug this result into the first FOC to obtain:

$$\widehat{\beta} = \left(\sum_{i=1}^{N}\sum_{t=1}^{T}(x_{it} - \bar{x}_i)'(x_{it} - \bar{x}_i)\right)^{-1}\left(\sum_{i=1}^{N}\sum_{t=1}^{T}(x_{it} - \bar{x}_i)'(y_{it} - \bar{y})\right)$$

$$\widehat{\beta} = \left(\sum_{i=1}^{N}\sum_{t=1}^{T}\ddot{x}_{it}'\ddot{x}_{it}\right)^{-1}\left(\sum_{i=1}^{N}\sum_{t=1}^{T}\ddot{x}_{it}'\ddot{y}_{it}\right)$$

with time-demeaned variables $\ddot{x}_{it} \equiv x_{it} - \bar{x}, \ddot{y}_{it} \equiv y_{it} - \bar{y}_i$

Fixed effects regression

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

$$\begin{array}{rcl} y_{it} & = & x_{it}\beta + c_i + \varepsilon_{it} \\ \bar{y}_i & = & \bar{x}_i\beta + c_i + \bar{\varepsilon}_i \\ \hline (y_{it} - \bar{y}_i) & = & (x_{it} - \bar{x})\beta + (c_i - c_i) + (\varepsilon_{it} - \bar{\varepsilon}_i) \\ \ddot{y}_{it} & = & \ddot{x}_{it}\beta + \ddot{\varepsilon}_{it} \end{array}$$

Fixed effects regression: main results

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

FE main results

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

Fixed effects regression: main issues

- 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
- Typically we care about β, but unit fixed effects c_i could be of interest
  - → $\widehat{c}_i$ from dummy variable regression is unbiased but not consistent for c_i (based on fixed T and $N \to \infty$)

Application: SASP

- 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

Risk premium equation

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

where Y is log price, D is unprotected sex with a client in a session, X are client and session characteristics, Z is unobserved heterogeneity, and u_i is both unobserved and correlated with Z_{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	0.051*	0.051*
	(0.028)	(0.028)	(0.026)
Ln(Length)	-0.308***	-0.435***	-0.435***
	(0.028)	(0.024)	(0.019)
Client was a Regular	-0.047*	-0.037**	-0.037**
	(0.028)	(0.019)	(0.017)
Age of Client	-0.001	0.002	0.002
	(0.009)	(0.007)	(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***	0.006	0.006
	(0.007)	(0.006)	(0.005)
Second Provider Involved	0.055	0.113*	0.113*
	(0.067)	(0.060)	(0.048)
Asian Client	-0.014	-0.010	-0.010
	(0.049)	(0.034)	(0.030)
Black Client	0.092	0.027	0.027
	(0.073)	(0.042)	(0.037)
Hispanic Client	0.052	-0.062	-0.062
	(0.080)	(0.052)	(0.045)
Other Ethnicity Client	0.156**	0.142***	0.142***
	(0.068)	(0.049)	(0.045)
Met Client in Hotel	0.133***	0.052*	0.052*
	(0.029)	(0.027)	(0.024)
Gave Client a Massage	-0.134***	-0.001	-0.001
	(0.029)	(0.028)	(0.024)
Age of provider	0.003	0.000	0.000
	(0.012)	(.)	(.)

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***	0.000	0.000
	(0.002)	(.)	(.)
Hispanic	-0.226***	0.000	0.000
	(0.082)	(.)	(.)
Black	0.028	0.000	0.000
	(0.064)	(.)	(.)
Other	-0.112	0.000	0.000
	(0.077)	(.)	(.)
Asian	0.086	0.000	0.000
	(0.158)	(.)	(.)
Imputed Years of Schooling	0.020**	0.000	0.000
	(0.010)	(.)	(.)
Cohabitating (living with a partner) but unmarried	-0.054	0.000	0.000
	(0.036)	(.)	(.)
Currently married and living with your spouse	0.005	0.000	0.000
	(0.043)	(.)	(.)
Divorced and not remarried	-0.021	0.000	0.000
	(0.038)	(.)	(.)
Married but not currently living with your spouse	-0.056	0.000	0.000
	(0.059)	(.)	(.)
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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Unit specific time trends often eliminate “results”

Table: Demeaned OLS Estimates of the Determinants of Log Hourly Price for a Panel of Sex Workers with provider specific trends

Depvar:	**FE w/provider trends**
Unprotected sex with client of any kind	0.004
	(0.046)
Ln(Length)	-0.450***
	(0.020)
Client was a Regular	-0.071**
	(0.023)
Age of Client	0.008
	(0.005)
Age of Client Squared	-0.000
	(0.000)
Client Attractiveness (Scale of 1 to 10)	0.003
	(0.003)
Second Provider Involved	0.126*
	(0.055)
Asian Client	-0.048***
	(0.007)
Black Client	0.017
	(0.043)
Hispanic Client	-0.015
	(0.022)
Other Ethnicity Client	0.135***
	(0.031)
Met Client in Hotel	0.073***
	(0.019)
Gave Client a Massage	0.022

Concluding remarks

- This was not a review of panel econometrics; for that see Wooldridge and other excellent options
- We reviewed POLS and TWFE because they are commonly used with individual level panel data and difference-in-differences
- Their main value is how they control for unobserved heterogeneity through a simple demeaning while still incorporating time varying covariates
- Now let’s discuss difference-in-differences which will at various times use the TWFE model

Difference-in-differences

- Keep in mind that yesterday, we had reviewed OLS used for diff-in-diff with two groups and two time periods

$$Y_{ist} = \alpha + \lambda NJ_s + \gamma d_t + \delta(NJ_s \times d_t) + \varepsilon_{ist}$$

- But what if there are more than two treatment groups?
- Unclear exactly when it was used, but at some point economists simply began using TWFE with state and year fixed effects and treatment dummy

$$Y_{ist} = \alpha + \delta D_{st} + \sigma_s + \tau_t + \varepsilon_{ist}$$

- The hope was that $\widehat{\delta}$ equaled a “reasonably weighted average” over all underlying treatment effects and therefore was the ATT

Diff-in-diff wars

- 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
- We're going to basically go in order, but I wanted to show you a little of their influence by focusing on their google cites
- Extreme meteoric rise, unusual for econometrics

Kirill Borusyak

Revisiting event study designs: Robust and efficient estimation

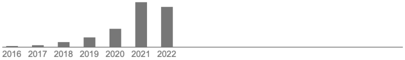
Authors Kirill Borusyak, Xavier Jaravel, Jann Spiess

Publication date 2021/8/27

Journal arXiv preprint arXiv:2108.12419

Description A broad empirical literature uses "event study," or "difference-in-differences with staggered rollout," research designs for treatment effect estimation: settings in which units in the panel receive treatment at different times. We show a series of problems with conventional regression-based two-way fixed effects estimators, both static and dynamic. These problems arise when researchers conflate the identifying assumptions of parallel trends and no anticipatory effects, implicit assumptions that restrict treatment effect heterogeneity, and the specification of the estimand as a weighted average of treatment effects. We then derive the efficient estimator robust to treatment effect heterogeneity for this setting, show that it has a particularly intuitive "imputation" form when treatment-effect heterogeneity is unrestricted, characterize its asymptotic behavior, provide tools for inference, and illustrate its attractive properties in simulations. We further discuss appropriate tests for parallel trends, and show how our estimation approach extends to many settings beyond standard event studies.

Total citations Cited by 944



Scholar articles Revisiting event study designs *
K Borusyak, X Jaravel - Available at SSRN 2826228, 2017
Cited by 663 Related articles All 4 versions

Revisiting event study designs: Robust and efficient estimation
K Borusyak, X Jaravel, J Spiess - arXiv preprint arXiv:2108.12419, 2021
Cited by 298 Related articles All 11 versions

Revisiting Event Study Designs. SSRN Scholarly Paper ID 2826228 *
K Borusyak, X Jaravel - Social Science Research Network, Rochester, NY, 2016
Cited by 6 Related articles

Revisiting Event Study Designs, With an Application to the Estimation of the Marginal Propensity to Consume *
B Kirill, J Xavier - 2017
Cited by 3 Related articles

Arindrajit Dube

The effect of minimum wages on low-wage jobs

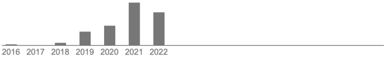
[HTML] from oup.com

Authors	Doruk Cengiz, Arindrajit Dube, Attila Lindner, Ben Zipperer
Publication date	2019/8/1
Journal	The Quarterly Journal of Economics
Volume	134
Issue	3
Pages	1405-1454
Publisher	Oxford Academic

Description

We estimate the effect of minimum wages on low-wage jobs using 138 prominent state-level minimum wage changes between 1979 and 2016 in the United States using a difference-in-differences approach. We first estimate the effect of the minimum wage increase on employment changes by wage bins throughout the hourly wage distribution. We then focus on the bottom part of the wage distribution and compare the number of excess jobs paying at or slightly above the new minimum wage to the missing jobs paying below it to infer the employment effect. We find that the overall number of low-wage jobs remained essentially unchanged over the five years following the increase. At the same time, the direct effect of the minimum wage on average earnings was amplified by modest wage spillovers at the bottom of the wage distribution. Our estimates by detailed demographic groups show that the lack of job loss is …

Total citations Cited by 648

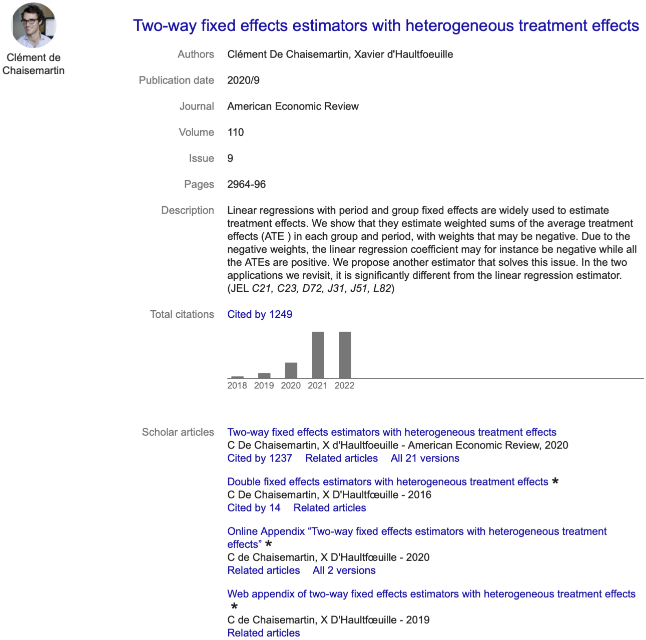


Scholar articles

The effect of minimum wages on low-wage jobs
D Cengiz, A Dube, A Lindner, B Zipperer - The Quarterly Journal of Economics, 2019
Cited by 586 Related articles All 19 versions

The effect of minimum wages on low-wage jobs: Evidence from the United States using a bunching estimator *
D Cengiz, A Dube, A Lindner, B Zipperer - 2018
Cited by 39 Related articles All 7 versions

The effect of minimum wages on the total number of jobs: Evidence from the United States using a bunching estimator *
D Cengiz, A Dube, A Lindner, B Zipperer - Unpublished paper, http://sole-jole. org/17722. pdf …, 2017
Cited by 29 Related articles All 4 versions



Clément de Chaisemartin

Two-way fixed effects estimators with heterogeneous treatment effects

Authors	Clément De Chaisemartin, Xavier d'Haultfoeuille
Publication date	2020/9
Journal	American Economic Review
Volume	110
Issue	9
Pages	2964-96
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 1249

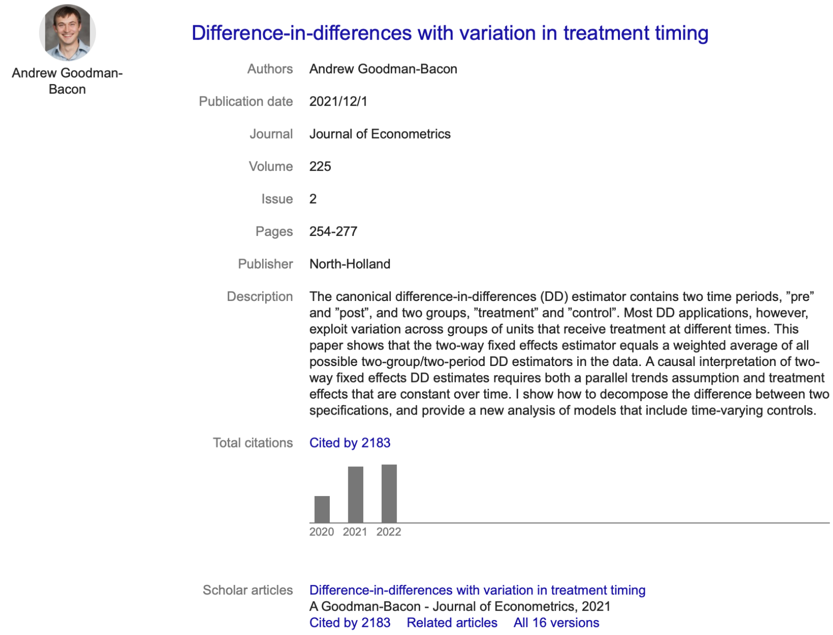
Scholar articles

Two-way fixed effects estimators with heterogeneous treatment effects
C De Chaisemartin, X d'Haultfoeuille - American Economic Review, 2020
Cited by 1237 Related articles All 21 versions

Double fixed effects estimators with heterogeneous treatment effects *
C De Chaisemartin, X D'Haultfœuille - 2016
Cited by 14 Related articles

Online Appendix “Two-way fixed effects estimators with heterogeneous treatment effects” *
C de Chaisemartin, X D'Haultfœuille - 2020
Related articles All 2 versions

Web appendix of two-way fixed effects estimators with heterogeneous treatment effects *
C de Chaisemartin, X D'Haultfœuille - 2019
Related articles



Andrew Goodman-Bacon

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-group/two-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 2183

Scholar articles
Difference-in-differences with variation in treatment timing
A Goodman-Bacon - Journal of Econometrics, 2021
Cited by 2183   Related articles   All 16 versions

Pedro H.C. Sant'Anna

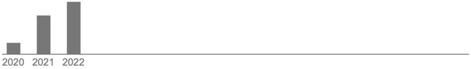
Difference-in-differences with multiple time periods

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 1377



Scholar articles

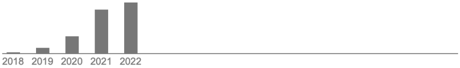
Difference-in-differences with multiple time periods
B Callaway, PHC Sant'Anna - Journal of Econometrics, 2021
Cited by 1377 Related articles All 18 versions

Supplementary Appendix: Difference-in-Differences with Multiple Time Periods *
B Callaway, PHC Sant'Anna - 2019
Related articles All 3 versions

Liyang Sun

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 1118



Scholar articles

Estimating dynamic treatment effects in event studies with heterogeneous treatment effects
L Sun, S Abraham - Journal of Econometrics, 2021
Cited by 861 Related articles All 13 versions

Estimating dynamic treatment effects in event studies with heterogeneous treatment effects *
S Abraham, L Sun - Available at SSRN, 2018
Cited by 279 Related articles

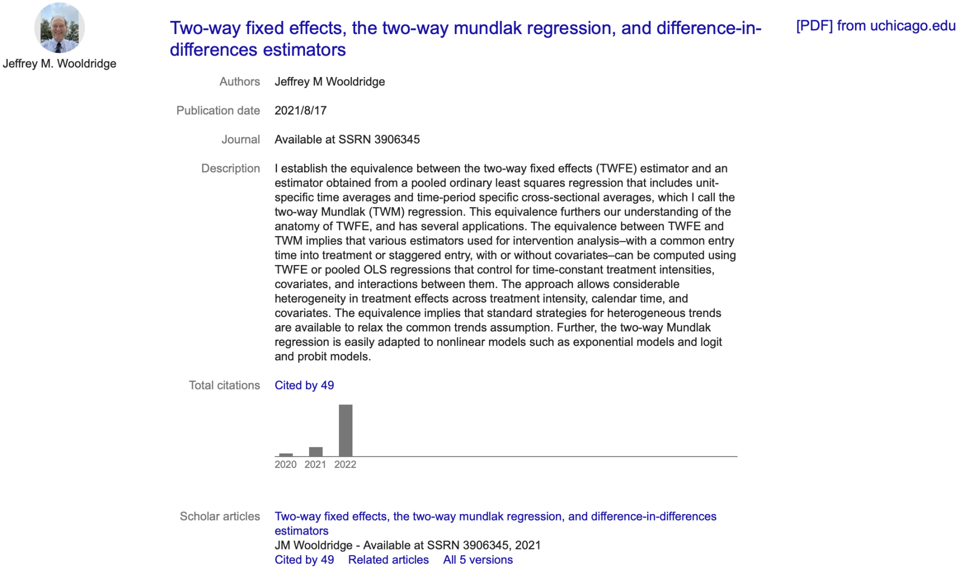
Two-stage differences in differences

John Gardner*

This version: April 2021

Abstract

A recent literature has shown that when adoption of a treatment is staggered and average treatment effects vary across groups and over time, difference-in-differences regression does not identify an easily interpretable measure of the typical effect of the treatment. In this paper, I extend this literature in two ways. First, I provide some simple underlying intuition for why difference-in-differences regression does not identify a group×period average treatment effect. Second, I propose an alternative two-



Jeffrey M. Wooldridge

Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators

[PDF] from uchicago.edu

Authors	Jeffrey M Wooldridge
Publication date	2021/8/17
Journal	Available at SSRN 3906345
Description	I establish the equivalence between the two-way fixed effects (TWFE) estimator and an estimator obtained from a pooled ordinary least squares regression that includes unit-specific time averages and time-period specific cross-sectional averages, which I call the two-way Mundlak (TWM) regression. This equivalence furthers our understanding of the anatomy of TWFE, and has several applications. The equivalence between TWFE and TWM implies that various estimators used for intervention analysis–with a common entry time into treatment or staggered entry, with or without covariates–can be computed using TWFE or pooled OLS regressions that control for time-constant treatment intensities, covariates, and interactions between them. The approach allows considerable heterogeneity in treatment effects across treatment intensity, calendar time, and covariates. The equivalence implies that standard strategies for heterogeneous trends are available to relax the common trends assumption. Further, the two-way Mundlak regression is easily adapted to nonlinear models such as exponential models and logit and probit models.
Total citations	Cited by 49

Scholar articles	Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators JM Wooldridge - Available at SSRN 3906345, 2021 Cited by 49 Related articles All 5 versions

Overview

1. Review TWFE pathologies using Bacon decomposition
2. Discuss solutions (CS)
3. Review event study pathology and solution (CS and SA)
4. Review turning on and off (dCdH)
5. Stacking (Cengiz, et al. 2019)
6. Imputation estimators (BJS and 2SDID)

Differential timing

- We covered mostly the simple two group case
- 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, similar to how we did with Sant'Anna and Zhao (2020), we will decompose TWFE to understand what it needs for unbiasedness under differential timing
- All of this is from Goodman-Bacon (2021, forthcoming) though the expression of the weights is from 2018 for personal preference
- Goodman-Bacon (2021, forthcoming) 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

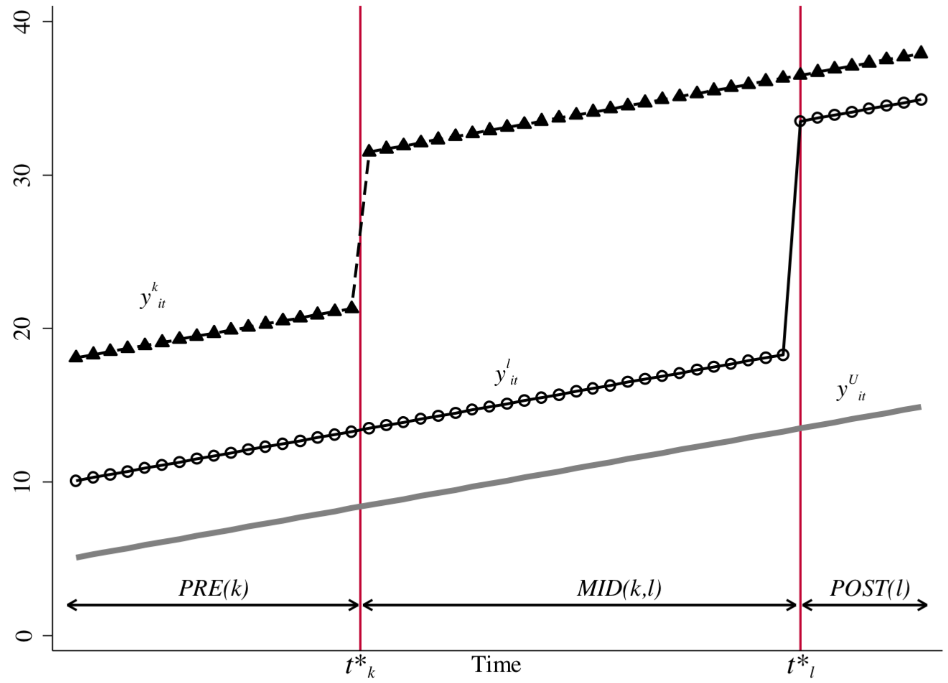
Let’s look at 3 timing groups (a, b and c) and one untreated group (U). With 3 timing groups, there are 9 2x2 DDs. Here they are:

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

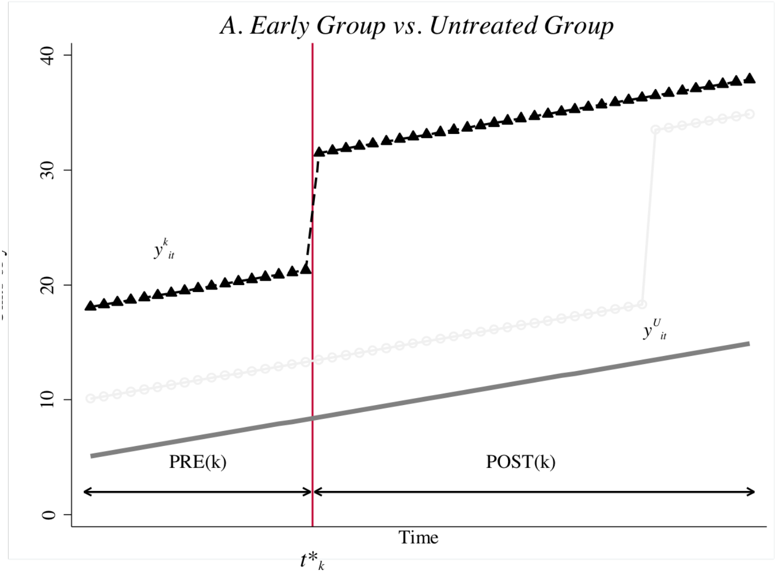
Let’s return to a simpler example with only two groups – a k group treated at t_k^* and an l treated at t_l^* plus an never-treated group called the U untreated group

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 $\overline{D}_k$ as the share of time each group spends in treatment status
- Denote $\widehat{\delta}_{jb}^{2x2}$ as the canonical 2×2 DD estimator for groups j and b where j is the treatment group and b is the comparison group

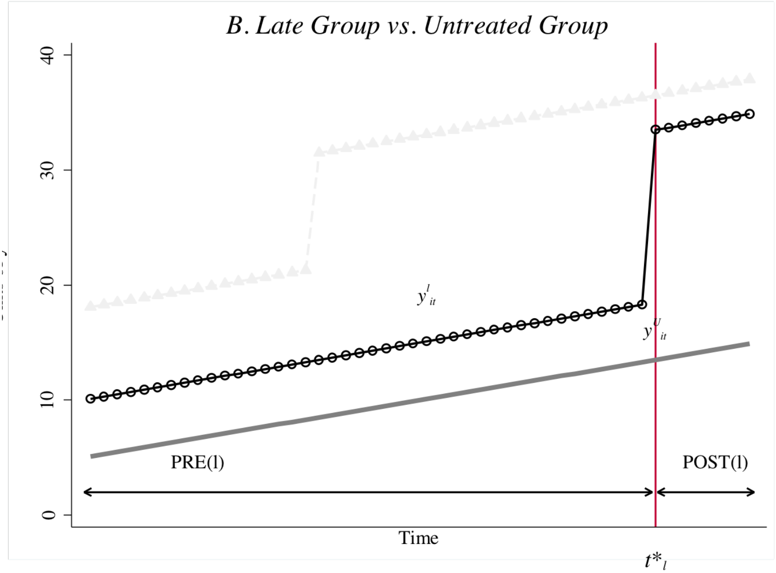

40
30
20
10
0
y^k_{it}
y^l_{it}
y^U_{it}
PRE(k)
MID(k,l)
POST(l)
t^*_k
Time
t^*_l

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

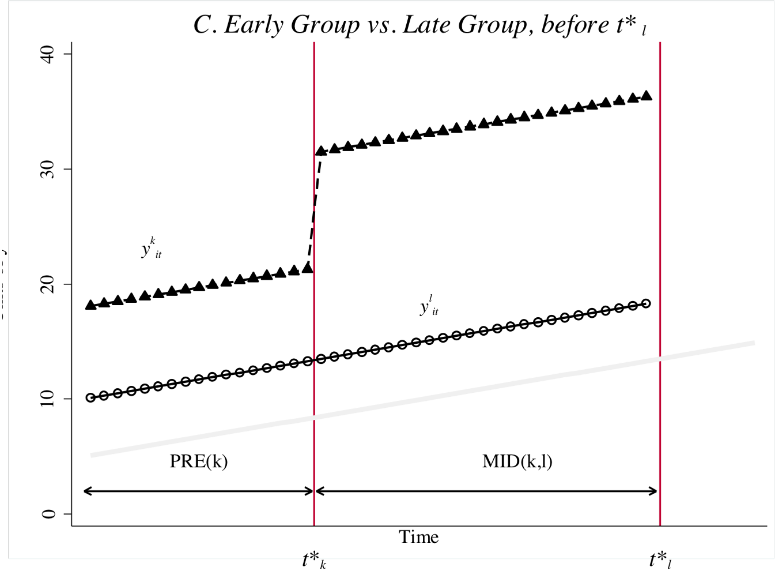


A. Early Group vs. Untreated Group

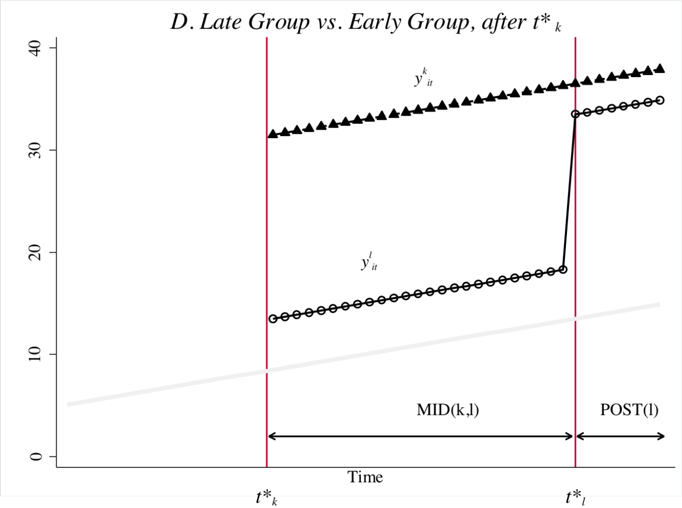
$$\widehat{\delta}_{lU}^{2x2} = \left(\overline{y}_l^{post(l)} - \overline{y}_l^{pre(l)}\right) - \left(\overline{y}_U^{post(l)} - \overline{y}_U^{pre(l)}\right)$$



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



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



D. Late Group vs. Early Group, after t^*_k

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

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

where n refer to sample sizes, $\overline{D}_k(1-\overline{D}_k)$ $(\overline{D}_k - \overline{D}_l)(1-(\overline{D}_k - \overline{D}_l))$ 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.
  - → $\overline{D} = 0.1$. Then $0.1 \times 0.9 = 0.09$
  - → $\overline{D} = 0.4$. Then $0.4 \times 0.6 = 0.24$
  - → $\overline{D} = 0.5$. Then $0.5 \times 0.5 = 0.25$
  - → $\overline{D} = 0.6$. 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., $\overline{D}_k - \overline{D}_l$)
- 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 $\overline{D}_k - \overline{D}_l = 0.52$. 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

$$\begin{aligned}
\widehat{\delta}^{2x2}_{kU} &= ATT_k Post + \Delta Y^0_k(Post(k), Pre(k)) - \Delta Y^0_U(Post(k), Pre) \\
\widehat{\delta}^{2x2}_{kl} &= ATT_k(MID) + \Delta Y^0_k(MID, Pre) - \Delta Y^0_l(MID, Pre)
\end{aligned}$$

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:

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

Substitute all this stuff into the decomposition formula

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

where we will make these substitutions

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

Notice all those potential sources of biases!

Potential Outcome Notation

$$p\, lim\, \widehat{\delta}^{TWFE}_{n\to\infty} \quad = \quad VWATT + VWPT - \Delta ATT$$

- 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”)

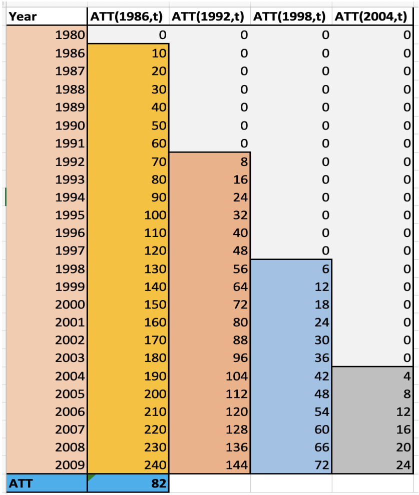
Simulated data

- 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

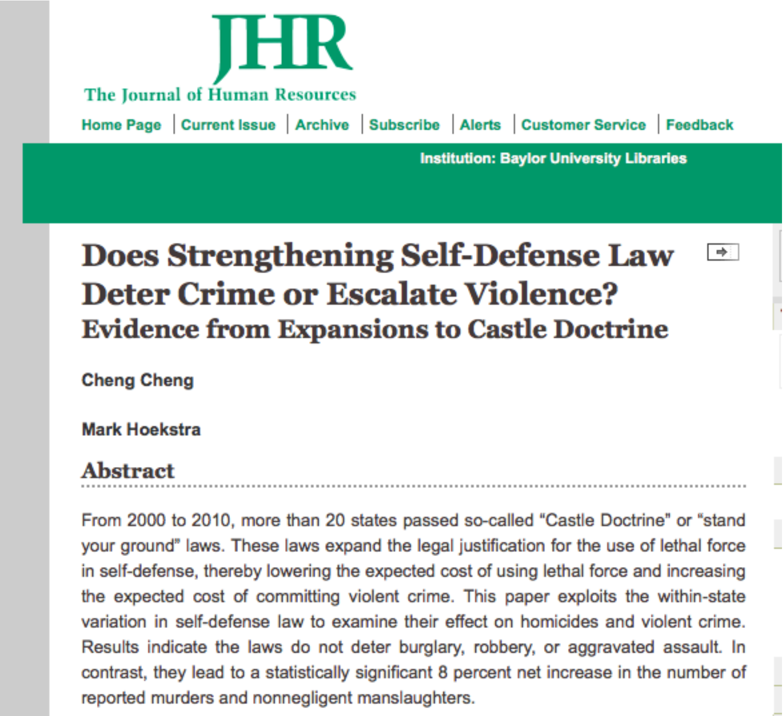
Table: Bacon Decomposition (TWFE $= -6.69$)

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



Does Strengthening Self-Defense Law Deter Crime or Escalate Violence?

Evidence from Expansions to Castle Doctrine

Cheng Cheng

Mark Hoekstra

Abstract

From 2000 to 2010, more than 20 states passed so-called "Castle Doctrine" or "stand your ground" laws. These laws expand the legal justification for the use of lethal force in self-defense, thereby lowering the expected cost of using lethal force and increasing the expected cost of committing violent crime. This paper exploits the within-state variation in self-defense law to examine their effect on homicides and violent crime. Results indicate the laws do not deter burglary, robbery, or aggravated assault. In contrast, they lead to a statistically significant 8 percent net increase in the number of reported murders and nonnegligent manslaughters.

Case study: Castle doctrine reforms

- Cheng and Hoekstra (2013) is a good, clean example of a differential timing for us to practice on
- In 2005, Florida passed a law called Stand Your Ground that expanded self-defense protections beyond the house
- More “castle doctrine” reforms followed from 2006 to 2009

Description

Details of castle doctrine reforms

- “Duty to retreat” is removed versus castle doctrine reforms; expanded where you can use lethal force
- Presumption of reasonable fear is added
- Civil liability for those acting under the law is removed

Ambiguous predictions

Castle reforms → homicides: Increase by removing homicide penalties and increasing opportunities

- Castle doctrine expansions lowered the (expected) cost of killing someone in self-defense
- Lowering the price of lethal self-defense should increase lethal homicides

Castle reforms → homicides: decrease through deterrence

Cheng and Hoekstra's estimation model

- TWFE model

$$Y_{it} = \beta_1 D_i + \beta_2 T_t + \beta_3 (CDL_{it}) + \alpha_1 X_{it} + c_i + u_t + \varepsilon_{it}$$

- CDL is a fraction between 0 and 1 depending on the percent of the year the state has a castle doctrine law
- Preferred specifications includes "region-by-year fixed effects" (see next slide)
- Estimation with TWFE and Poisson with and without population weights
- Models will include covariates (e.g., police, imprisonment, race shares, state spending on public assistance)

Publicly available crime data

Main data: FBI Uniform Crime Reports Part 1 Offenses (2000-2010)

- Main outcomes: log homicides
- Falsification outcomes: motor vehicle theft and larceny
- Deterrence outcomes: burglary, robbery, assault

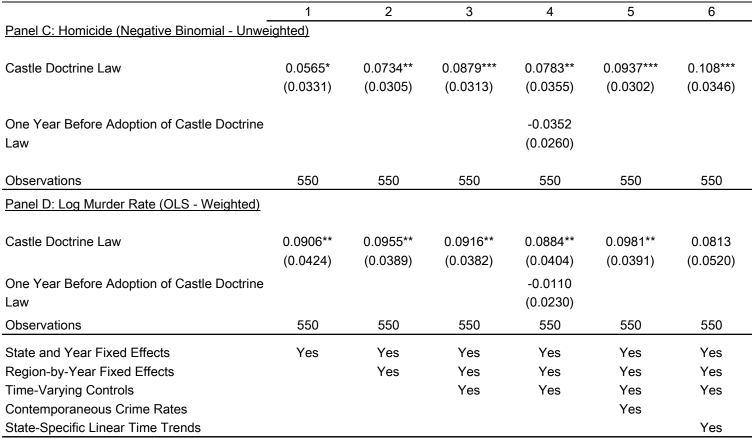
Region-by-year fixed effects

- **Parallel trends assumption**: imposed structurally with region-by-year dummies
- **Argument**: unobserved changes in crime are running “parallel” to the treatment states within region over time
- **SUTVA** and **No Anticipation**: No spillovers, no hidden variation in treatment, no behavioral change today in response to tomorrow’s law

Results – Deterrence

	OLS - Weighted by State Population						OLS - Unweighted					
	1	2	3	4	5	6	7	8	9	10	11	12
Panel A: Burglary	Log (Burglary Rate)						Log (Burglary Rate)					
Castle Doctrine Law	0.0780***	0.0290	0.0223	0.0164	0.0327*	0.0237	0.0572**	0.00961	0.00663	0.00277	0.00683	0.0207
	(0.0255)	(0.0236)	(0.0223)	(0.0247)	(0.0165)	(0.0207)	(0.0272)	(0.0291)	(0.0268)	(0.0304)	(0.0222)	(0.0259)
One Year Before Adoption of				-0.0201						-0.0154		
Castle Doctrine Law				(0.0139)						(0.0214)		
Panel B: Robbery	Log (Robbery Rate)						Log (Robbery Rate)					
Castle Doctrine Law	0.0408	0.0344	0.0262	0.0216	0.0376**	0.0515*	0.0448	0.0320	0.00839	0.00552	0.00874	0.0267
	(0.0254)	(0.0224)	(0.0229)	(0.0246)	(0.0181)	(0.0274)	(0.0331)	(0.0421)	(0.0387)	(0.0437)	(0.0339)	(0.0299)
One Year Before Adoption of				-0.0156						-0.0115		
Castle Doctrine Law				(0.0167)						(0.0283)		
Panel C: Aggravated Assault	Log (Aggravated Assault Rate)						Log (Aggravated Assault Rate)					
Castle Doctrine Law	0.0434	0.0397	0.0372	0.0362	0.0424	0.0414	0.0555	0.0698	0.0343	0.0305	0.0341	0.0317
	(0.0387)	(0.0407)	(0.0319)	(0.0349)	(0.0291)	(0.0285)	(0.0604)	(0.0630)	(0.0433)	(0.0478)	(0.0405)	(0.0380)
One Year Before Adoption of				-0.00343						-0.0150		
Castle Doctrine Law				(0.0161)						(0.0251)		
Observations	550	550	550	550	550	550	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes						Yes	
State-Specific Linear Time Trends						Yes						Yes

Results – Homicides



	1	2	3	4	5	6
Panel C: Homicide (Negative Binomial - Unweighted)						
Castle Doctrine Law	0.0565*	0.0734**	0.0879***	0.0783**	0.0937***	0.108***
	(0.0331)	(0.0305)	(0.0313)	(0.0355)	(0.0302)	(0.0346)
One Year Before Adoption of Castle Doctrine Law				-0.0352		
				(0.0260)		
Observations	550	550	550	550	550	550
Panel D: Log Murder Rate (OLS - Weighted)						
Castle Doctrine Law	0.0906**	0.0955**	0.0916**	0.0884**	0.0981**	0.0813
	(0.0424)	(0.0389)	(0.0382)	(0.0404)	(0.0391)	(0.0520)
One Year Before Adoption of Castle Doctrine Law				-0.0110		
				(0.0230)		
Observations	550	550	550	550	550	550
State and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Year Fixed Effects		Yes	Yes	Yes	Yes	Yes
Time-Varying Controls			Yes	Yes	Yes	Yes
Contemporaneous Crime Rates					Yes	
State-Specific Linear Time Trends						Yes

Interpretation

- Series of robustness checks (falsifications on larceny and motor vehicle theft; deterrence; many different specifications)
- Castle doctrine reforms are associated with an 8% net increase in homicide rates per year across the 21 adopting states
- Interpretation is these would not have occurred without castle doctrine reforms
- But is this robust to alternative models? Today we will check

Roadmap

Differential timing

- Twoway fixed effects vs Pooled OLS
- Diff-in-diff wars
- TWFE Pathologies
- Simulation
- Castle doctrine reform

Implicit imputation

- CS
- SA
- dCH

Causal inference is imputation

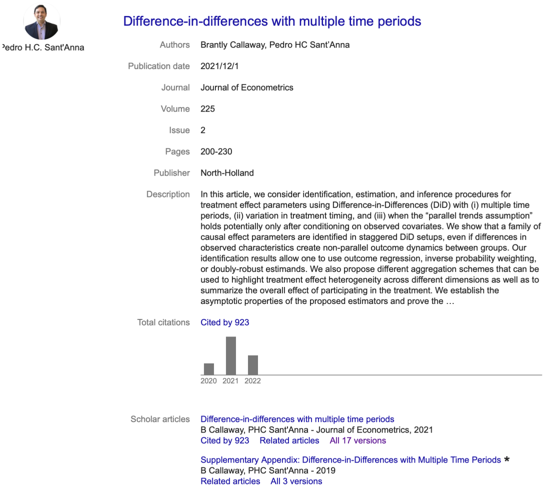
“At some level, all methods for causal inference can be viewed as imputation methods, although some more explicitly than others.” – Imbens and Rubin (2015)

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



Pedro H.C. Sant'Anna

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 923

Scholar articles

Difference-in-differences with multiple time periods
B Callaway, PHC Sant'Anna - Journal of Econometrics, 2021
Cited by 923 Related articles All 17 versions

Supplementary Appendix: Difference-in-Differences with Multiple Time Periods *
B Callaway, PHC Sant'Anna - 2019
Related articles All 3 versions

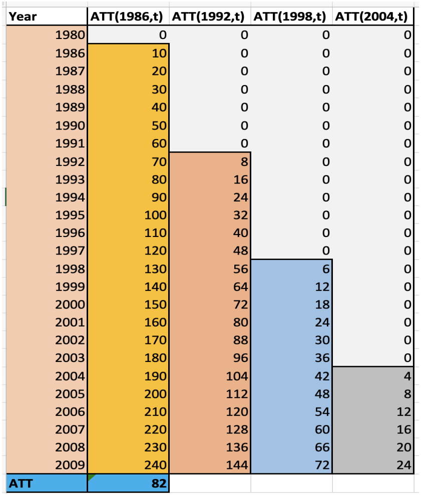
When is CS used

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 identity and aggregate heterogenous 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 $t = 1, \ldots, T$
- 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)

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

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

Staggered adoption (i.e., universal coverage)

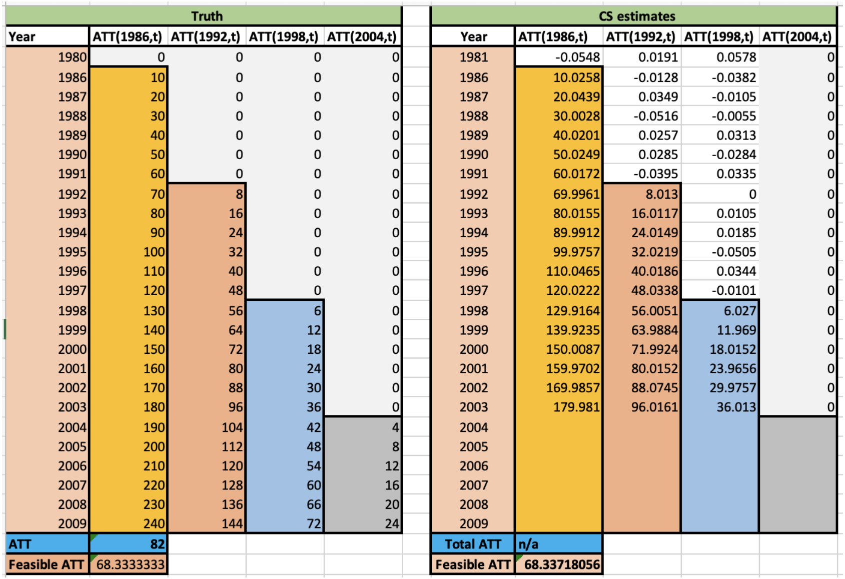
Proof.

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				
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			
Feasible ATT	68.3333333			

CS estimates				
Year	**ATT(1986,t)**	**ATT(1992,t)**	**ATT(1998,t)**	**ATT(2004,t)**
1981	-0.0548	0.0191	0.0578	0
1986	10.0258	-0.0128	-0.0382	0
1987	20.0439	0.0349	-0.0105	0
1988	30.0028	-0.0516	-0.0055	0
1989	40.0201	0.0257	0.0313	0
1990	50.0249	0.0285	-0.0284	0
1991	60.0172	-0.0395	0.0335	0
1992	69.9961	8.013	0	0
1993	80.0155	16.0117	0.0105	0
1994	89.9912	24.0149	0.0185	0
1995	99.9757	32.0219	-0.0505	0
1996	110.0465	40.0186	0.0344	0
1997	120.0222	48.0338	-0.0101	0
1998	129.9164	56.0051	6.027	0
1999	139.9235	63.9884	11.969	0
2000	150.0087	71.9924	18.0152	0
2001	159.9702	80.0152	23.9656	0
2002	169.9857	88.0745	29.9757	0
2003	179.981	96.0161	36.013	0
2004				
2005				
2006				
2007				
2008				
2009				
Total ATT	n/a			
Feasible ATT	68.33718056			

Question: Why didn't CS estimate all ATT(g,t)? What is "feasible ATT"?

Reporting results

Table: Estimating ATT using only pre-2004 data

	(Truth)	**(TWFE)**	**(CS)**	**(SA)**	**(BJS)**
$\widehat{Feasible\ ATT}$	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

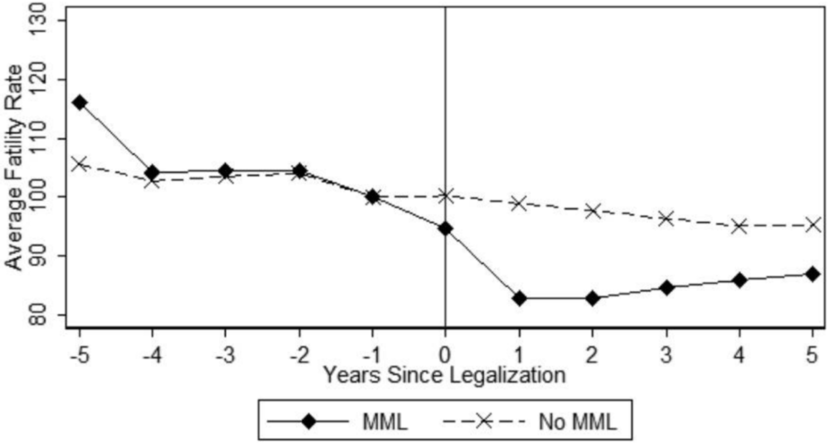


Figure: Anderson, et al. (2013) display of raw traffic fatality rates for re-centered treatment states and control states with randomized treatment dates

Replicated from a project of mine

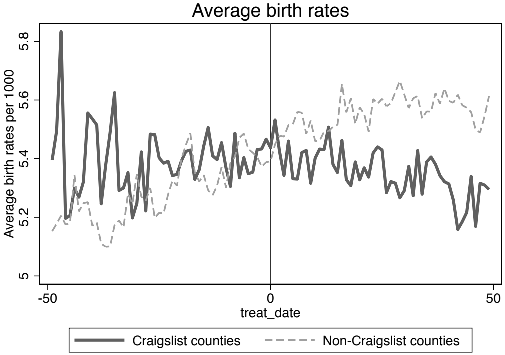
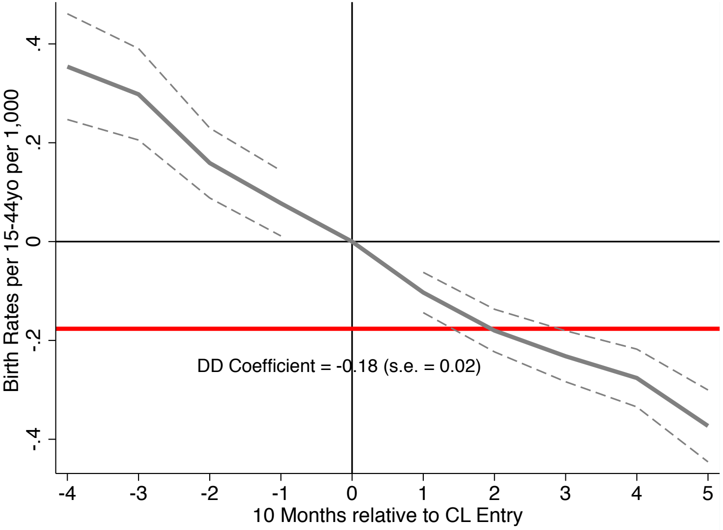


Figure: From one of my studies. Looks decent right?

Canonical event study specification with TWFE

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

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

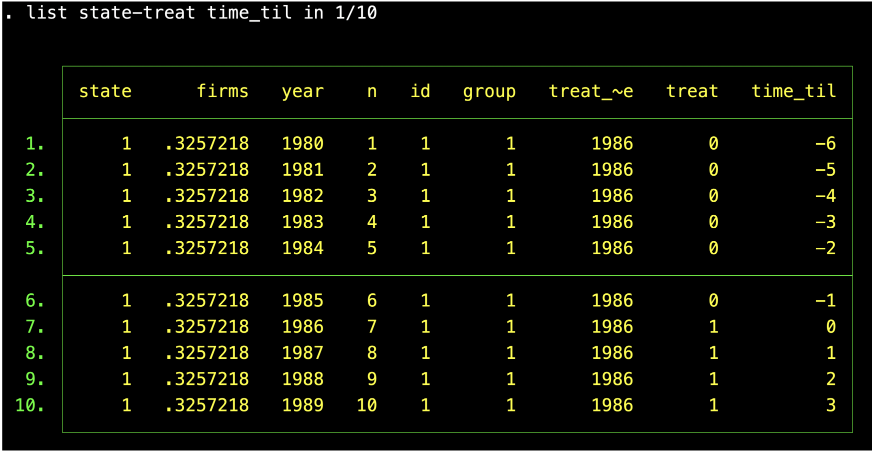
Difficult notation (cont.)

- The ∞ symbol is used to either describe the group ($E_i = \infty$) or the potential outcome (Y^∞)
- $Y_{i,t}^\infty$ is 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 $l =$year - 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:

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

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 $E[Y^e_{i,e+l} - Y^\infty_{i,e+l} | E_i = e] = 0$ 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).

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

Weight ($w^g_{e,l}$) summation cheat sheet

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

Estimating the weights

Regress $D^l_{i,t} \times 1\{E_i = e\}$ 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 bing 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:

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

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 $l' \geq 0$.

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

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

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

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

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

- 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 $\widehat{\delta}_{e,l}$

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

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

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

Consistency and Inference

- Under parallel trends and no anticipation, $\widehat{\delta}_{e,l}$ is consistent, and sample shares are also consistent estimators for population shares.
- Thus IV 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

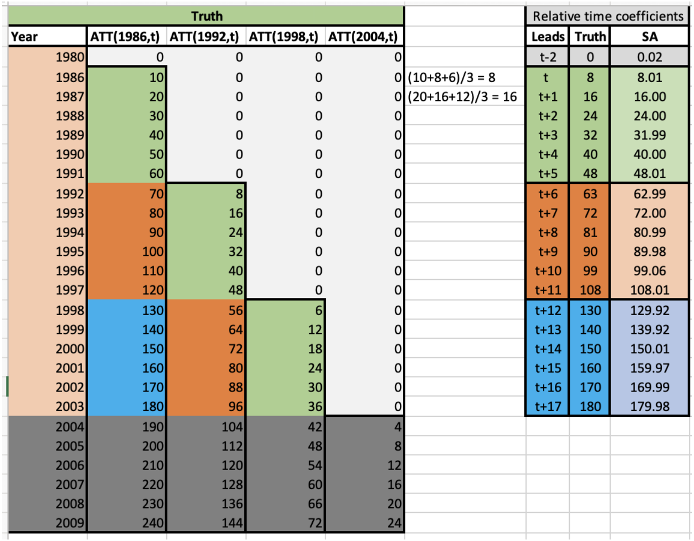
- **Stata**: eventstudyinteract (can be installed from ssc)
- **R**: fixest with subab() option (see `https://lrberge.github.io/fixest/reference/sunab.html/`)

Reporting results

Table: Estimating ATT

	(Truth)	**(TWFE)**	**(CS)**	**(SA)**	**(BJS)**
$\widehat{Feasible\ ATT}$	68.33	26.81***	68.34***	68.33***	

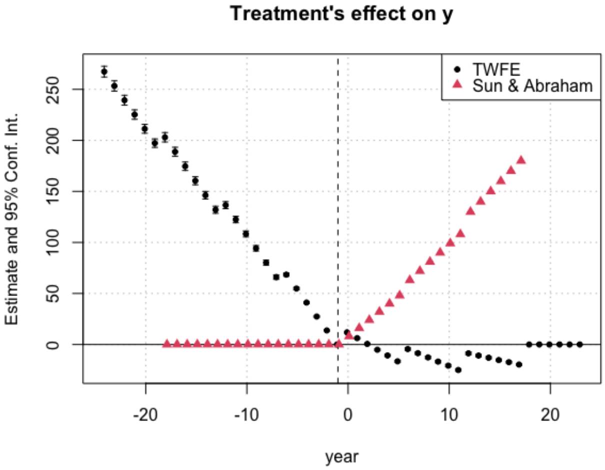
Computing relative event time leads and lags



Truth						Relative time coefficients		
Year	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	(10+8+6)/3 = 8	t	8	8.01
1987	20	0	0	0	(20+16+12)/3 = 16	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’Haultfoeulle 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 $\widehat{\delta}$
- Just explains what comparisons OLS is using to calculate the TWFE coefficient – just peels back the curtain.

Causal effects

- dCdH impose causal assumptions and try a different decomposition strategy
- Uses as its building block the unit-specific treatment effects
- This is hopefully going to help us better understand where these negative weights are coming from
- Note that their model is very general in that the treatment is reversible (meaning you can turn it on and off)

Terms

- Target parameter:

  $$\Delta^g_{i,t} = Y^1_{i,t} - Y^\infty_{i,t}$$

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

- TWFE terms. Define the error term as $\varepsilon_{i,t}$:

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

- Weights:

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

  Basically divide the error by the average of the error for all treated units.

Assumptions

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.

dCdH Theorem

Theorem – dCdH decomposition

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

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

Then it follows that

$$\beta = E\Bigg[\sum_{i,t:D_{i,t}=1} \frac{1}{N^T} w_{i,t} \cdot \Delta^g_{i,t}\Bigg]$$

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

So once you run that specification, β is going to recover a non-convex average over all unit level treatment effects (weights can be negative). dCdH was the first I think.

Negative weights

- Very common now to hear about negative weights, and furthermore, that negative weights wipe out any causal interpretation, but why?
- What if every unit treatment effect was positive, but some of the weights were negative?
- 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 (treated to already treated)

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