Difference-in-Differences

Mixtape Session

Roadmap

- Differential timing
  - Bacon decomposition
  - Simulation
  - Castle doctrine reform
- Implicit imputation
  - CS
  - SA
  - dCH

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

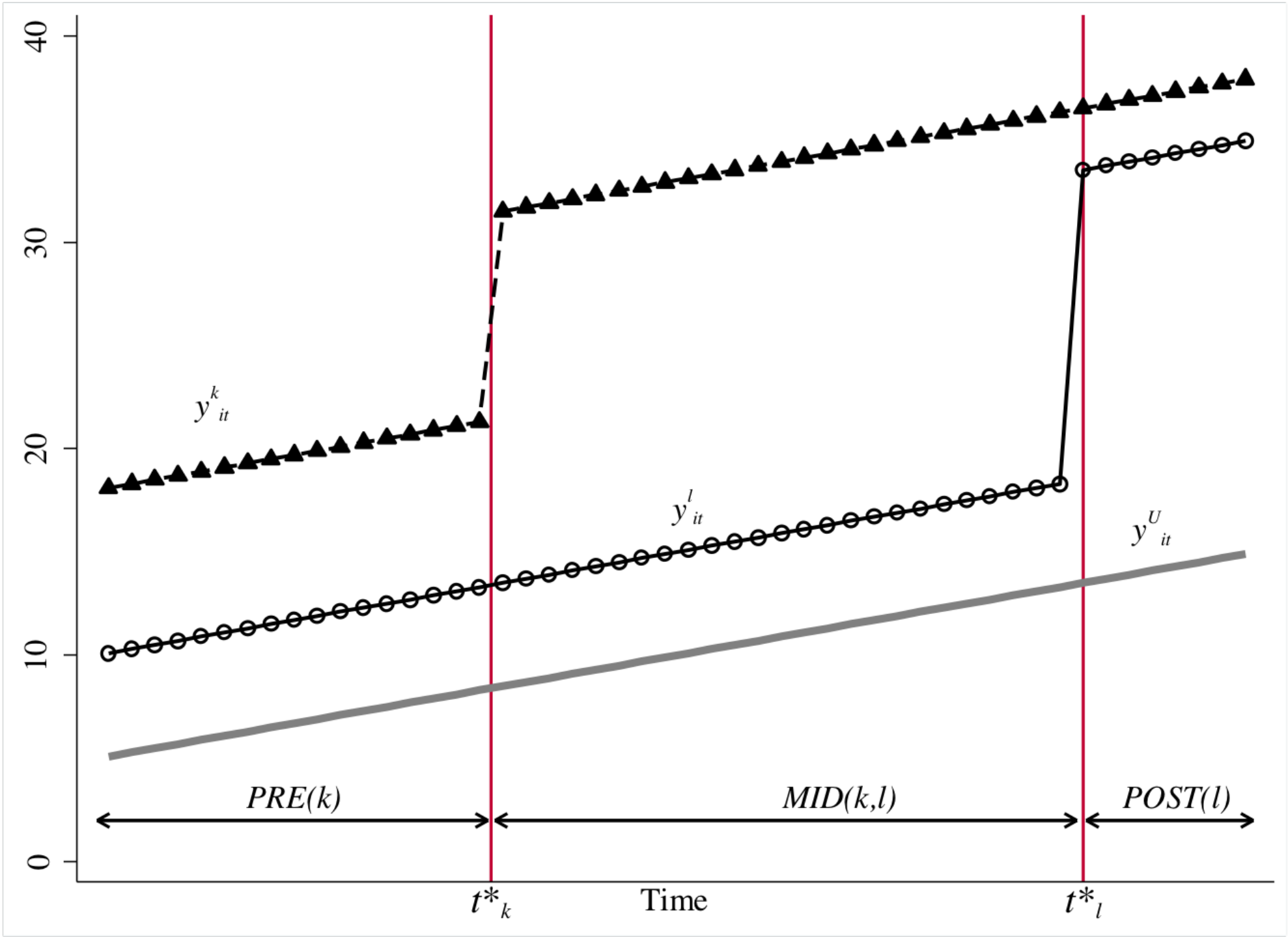
Let's look at 3 timing groups (a, b and c) and one untreated group (U). With 3 timing groups, there are 9 2x2 DDs. Here they are:

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

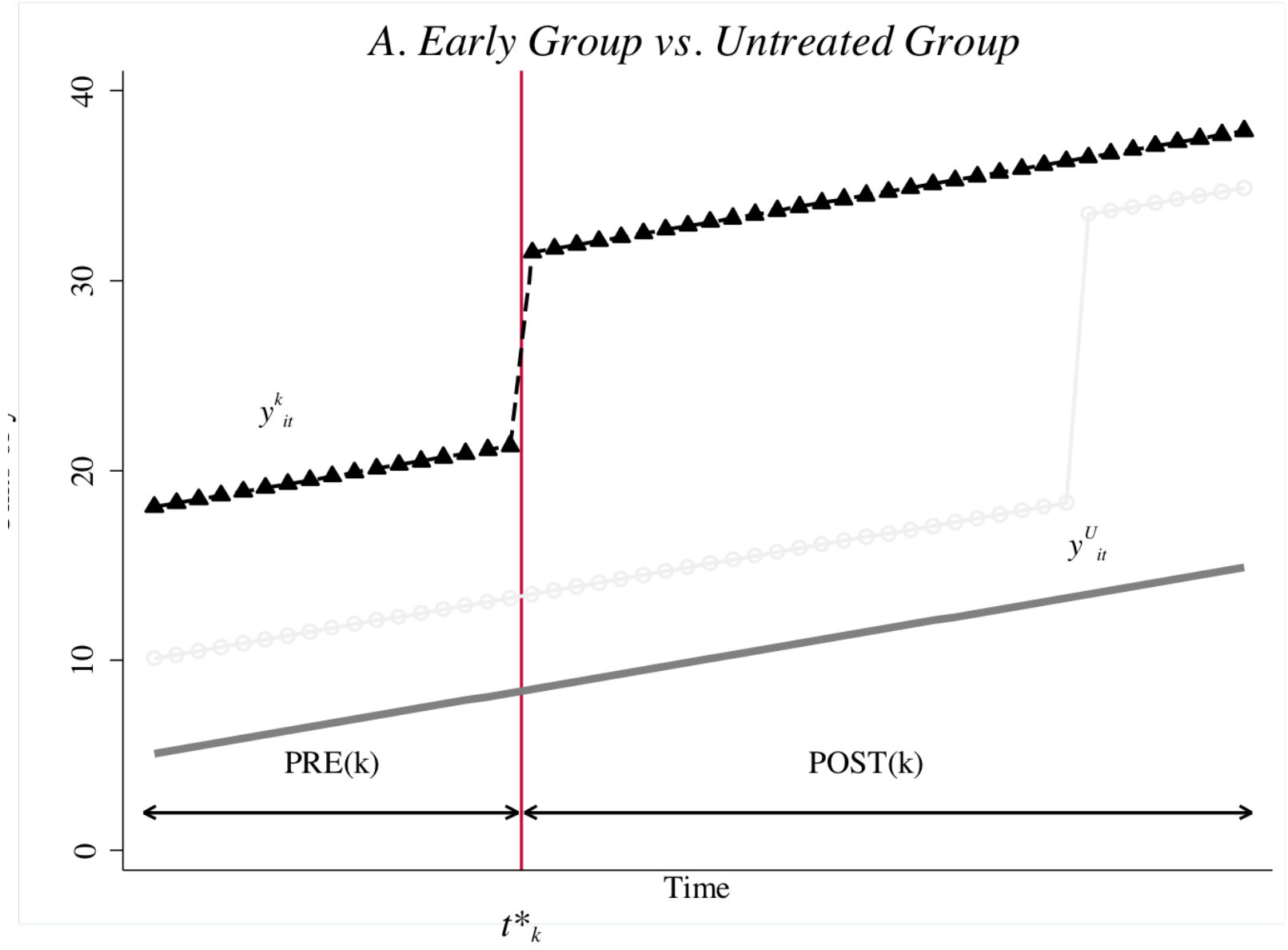
Let's return to a simpler example with only two groups – a k group treated at t_k^* and an l treated at t_l^* plus an never-treated group called the U untreated group

Terms and notation

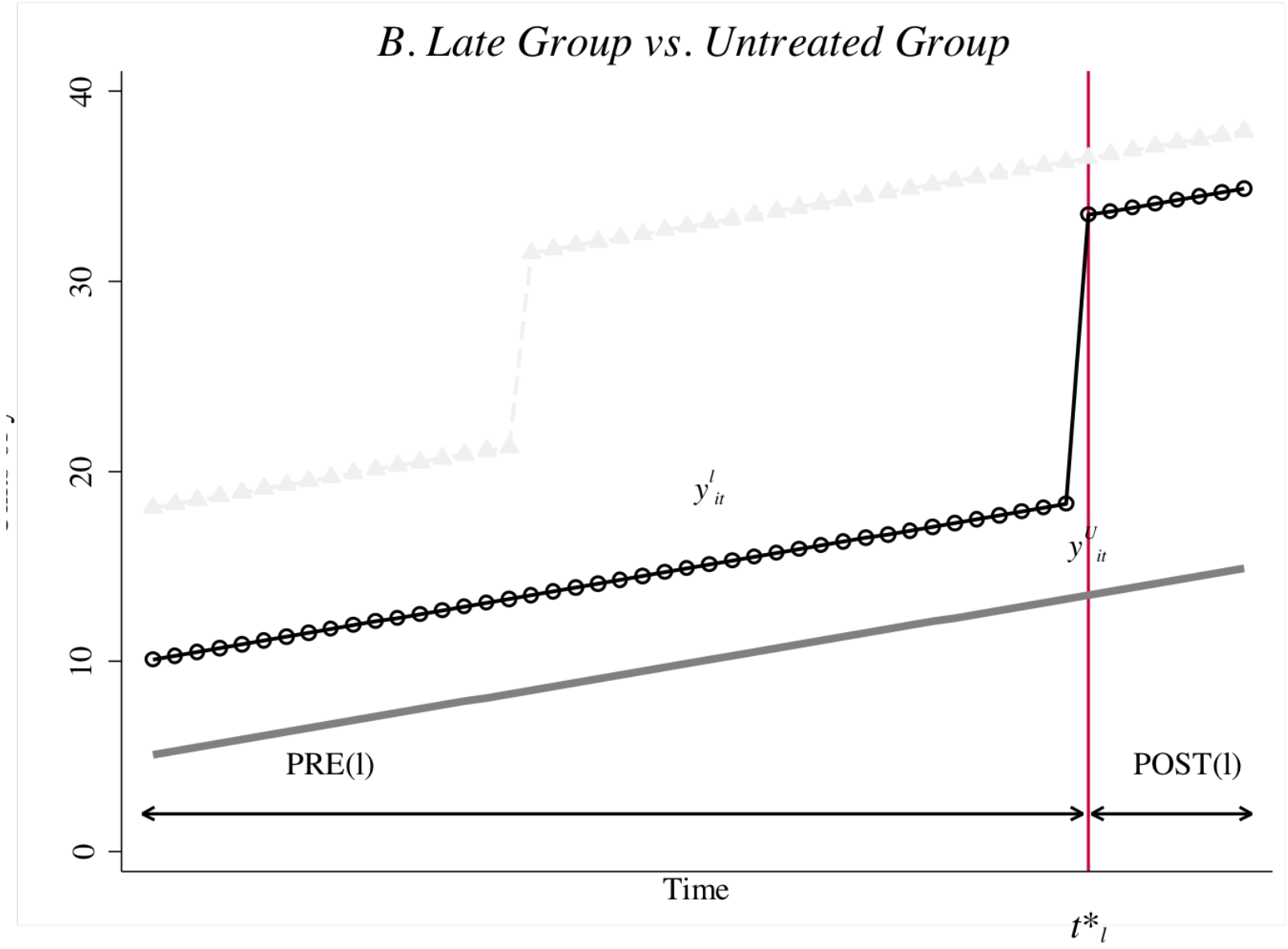
- Let there be two treatment groups (k,l) and one untreated group (U)
- k,l define the groups based on when they receive treatment (differently in time) with k receiving it earlier than l
- Denote $\overline{D}_k$ as the share of time each group spends in treatment status
- Denote $\widehat{\delta}_{jb}^{2x2}$ 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}

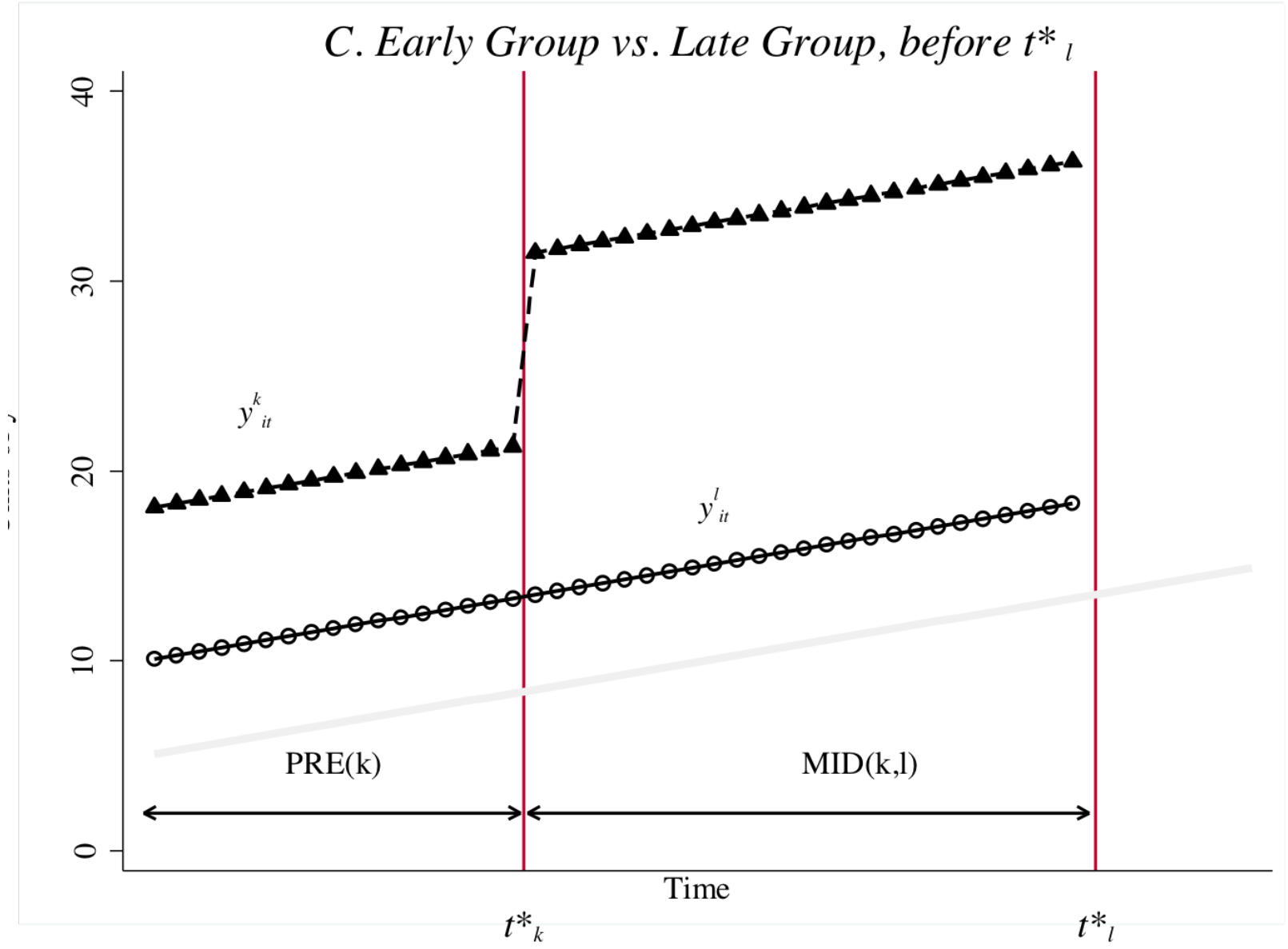
$$\widehat{\delta}_{kU}^{2x2} = \left(\overline{y}_k^{post(k)} - \overline{y}_k^{pre(k)} \right) - \left(\overline{y}_U^{post(k)} - \overline{y}_U^{pre(k)} \right)$$



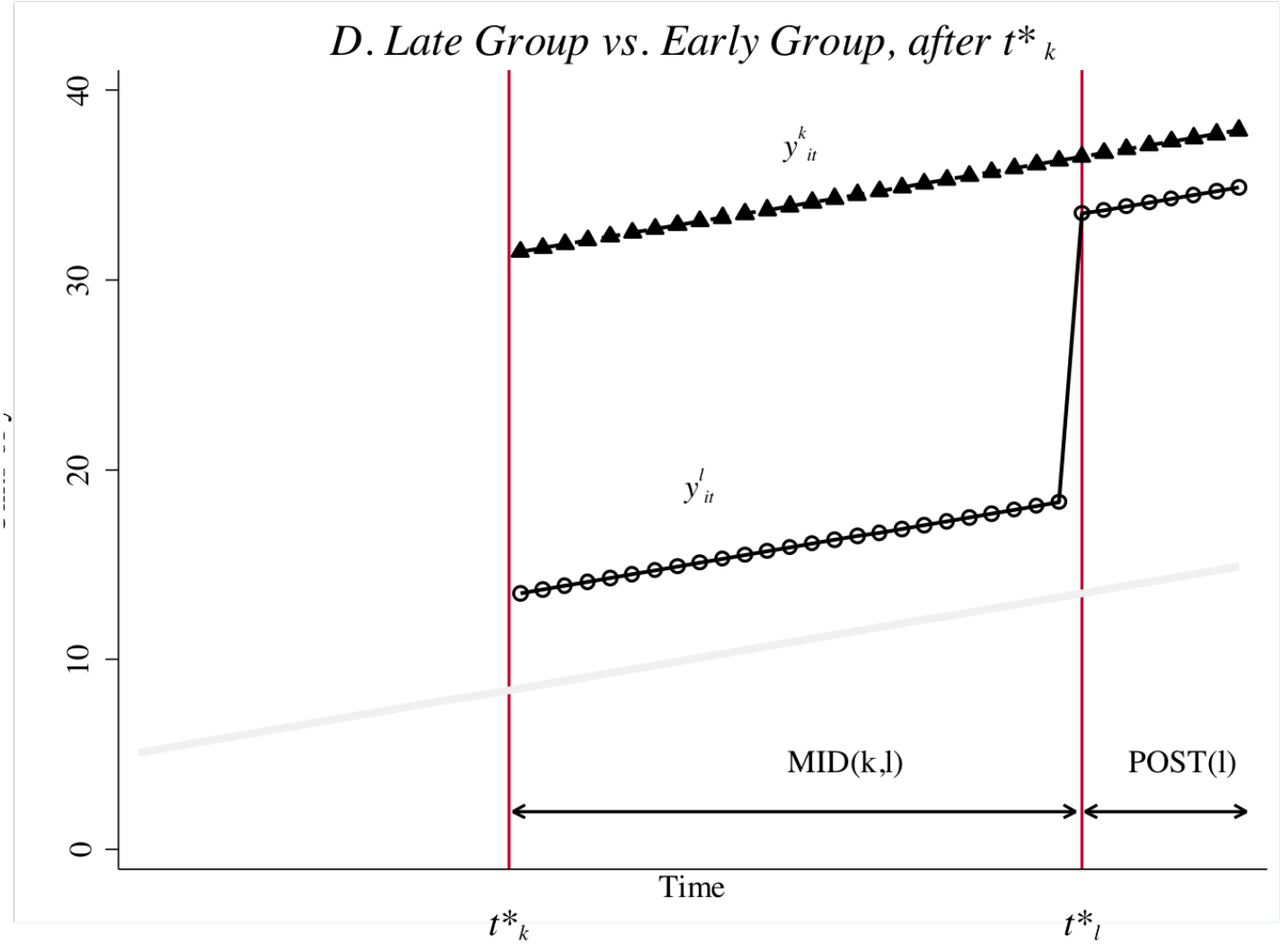
$$\widehat{\delta}_{lU}^{2x2} = \left(\overline{y}_{l}^{post(l)} - \overline{y}_{l}^{pre(l)}\right) - \left(\overline{y}_{U}^{post(l)} - \overline{y}_{U}^{pre(l)}\right)$$



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



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



Bacon decomposition

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

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

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

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

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

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

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

JHR
The Journal of Human Resources

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 $\rightarrow$ 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 $\rightarrow$ 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 Castle Doctrine Law				-0.0201						-0.0154		
				(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 Castle Doctrine Law				-0.0156						-0.0115		
				(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 Castle Doctrine Law				-0.00343						-0.0150		
				(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

- Bacon decomposition
- 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")
  - $\rightarrow$ 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 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

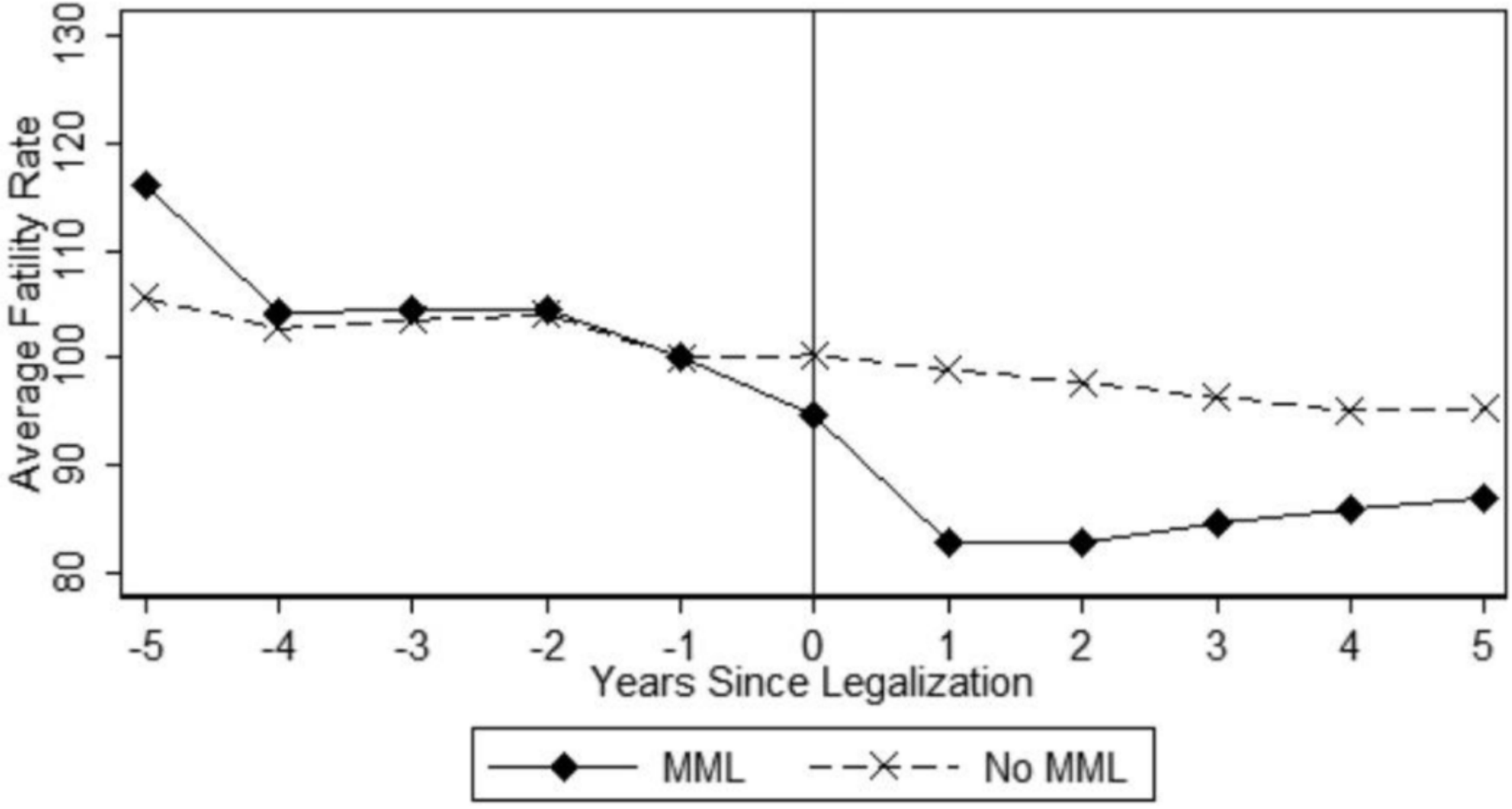


Figure: Anderson, et al. (2013) display of raw traffic fatality rates for re-centered treatment states and control states with randomized treatment dates

Replicated from a project of mine

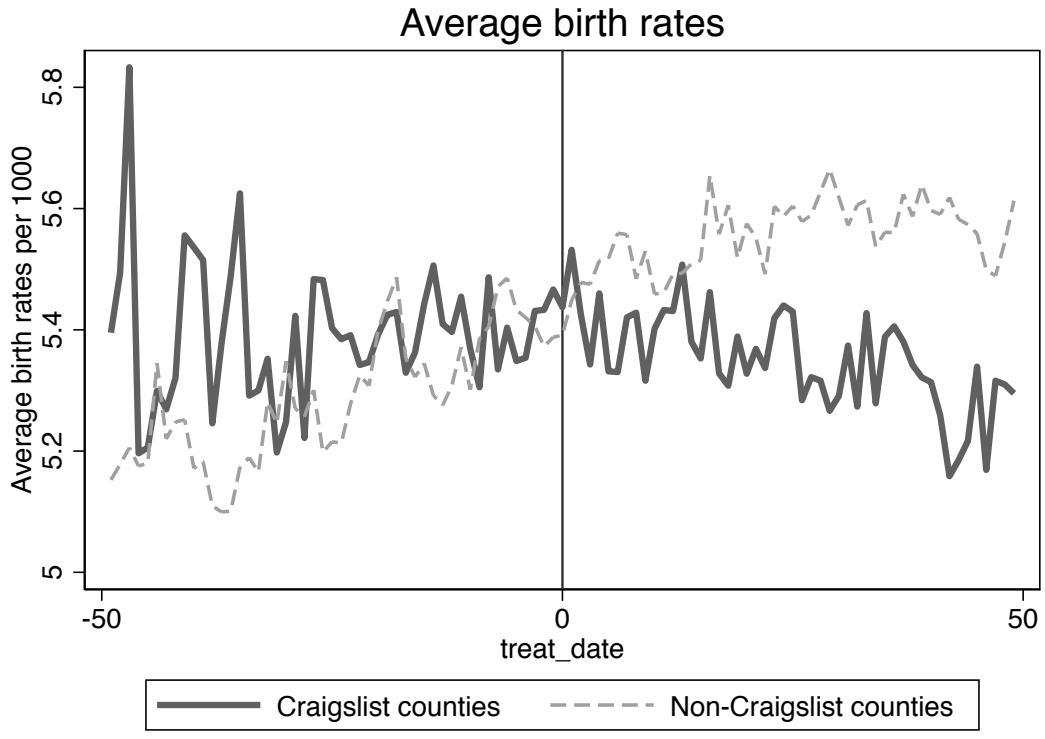
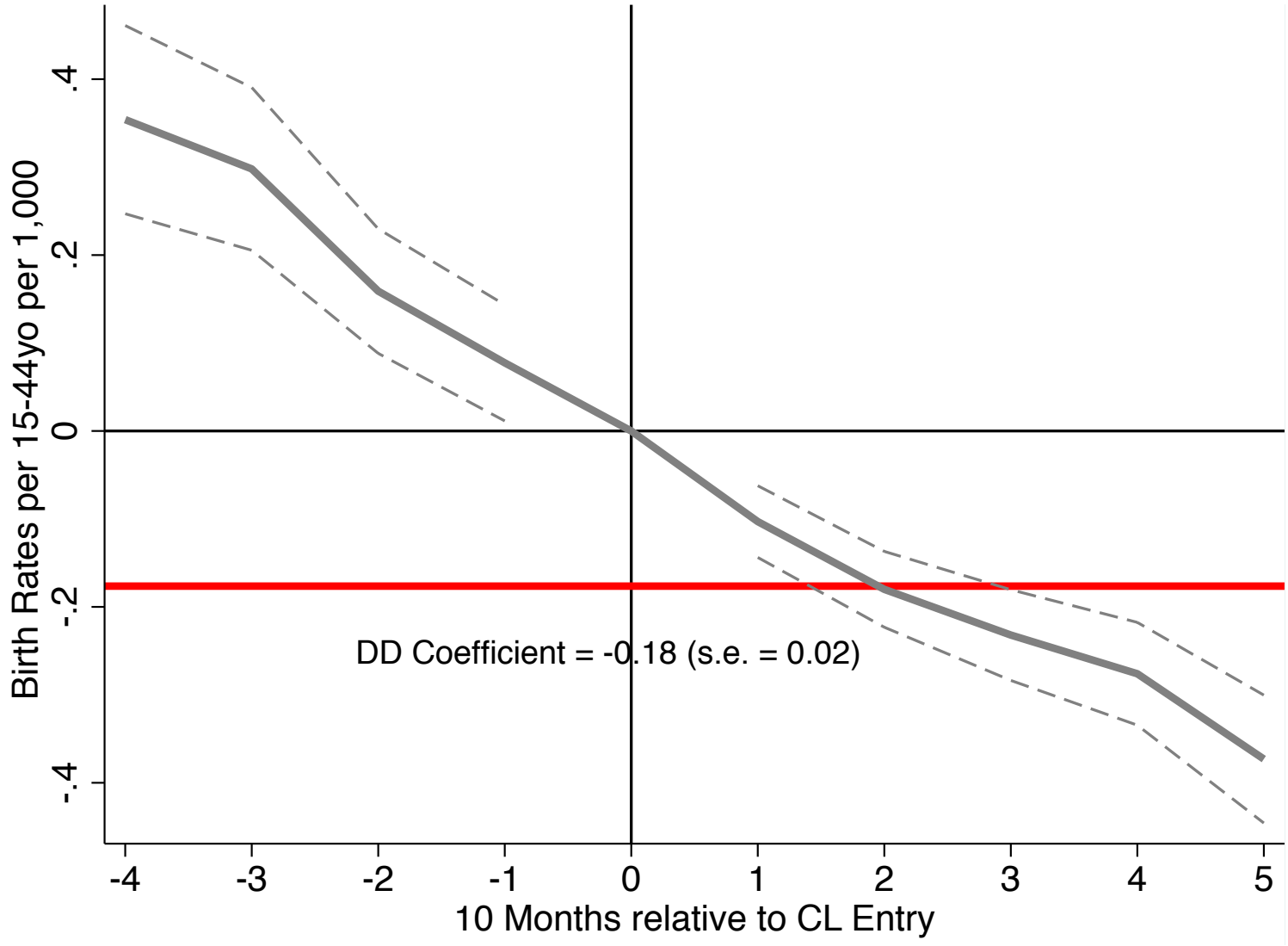


Figure: From one of my studies. Looks decent right?

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

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

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

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

- 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

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

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

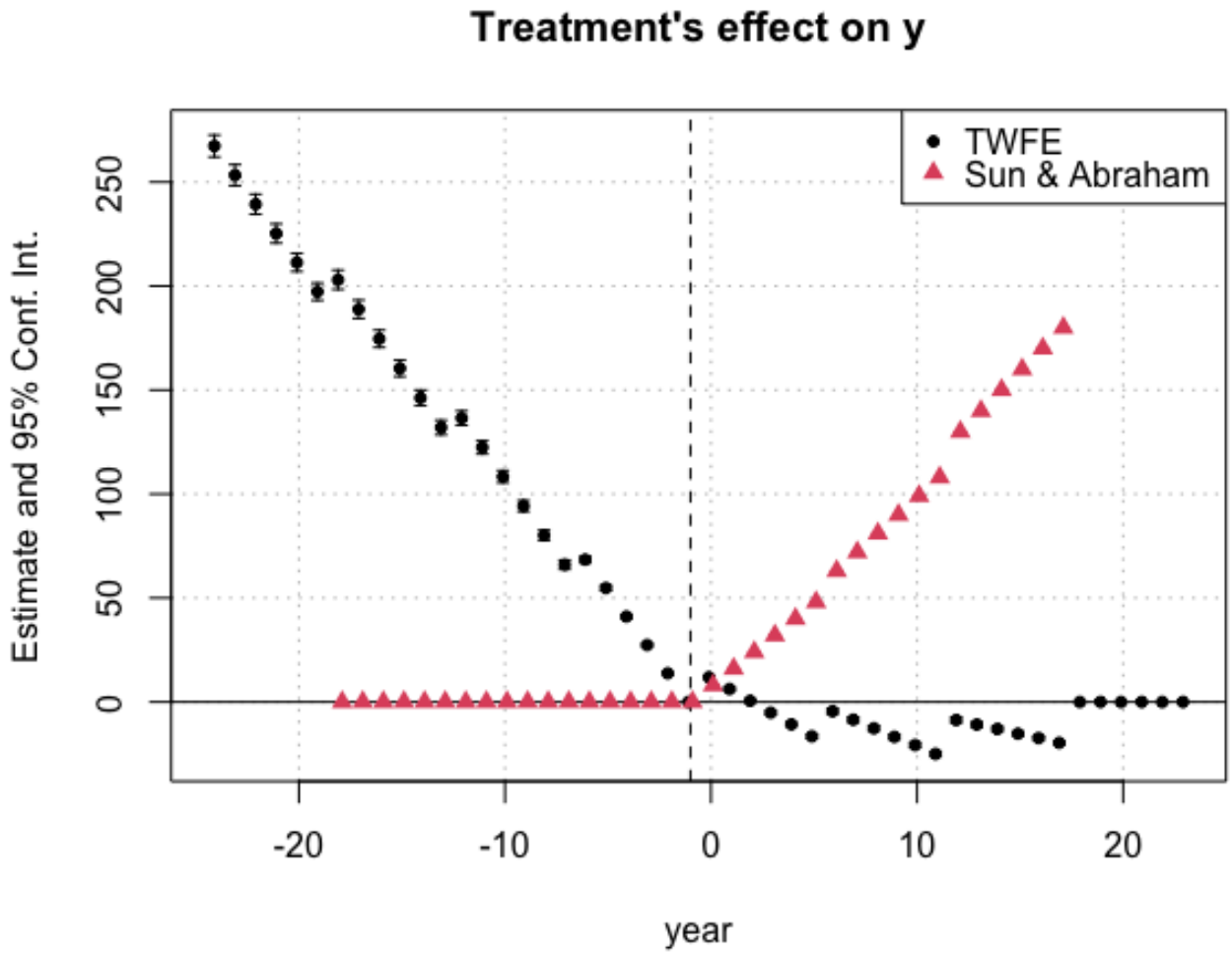
	(Truth)	**(TWFE)**	**(CS)**	**(SA)**	**(BJS)**
$\widehat{Feasible\ ATT}$	68.33	26.81***	68.34***	68.33***	

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:

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

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

- 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

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

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