Causal Inference II

Mixtape Session

Roadmap

- Background material
  - Introduction
- TWFE Pathologies
  - Historical links
  - Potential outcomes
  - Bacon decomposition
  - Simulation
- Two solutions and a new decomposition
  - CS
  - SA
- Some opinions and an application

Why diff-in-diff and differential timing?

- We will be discussing some of the newer material on difference-in-differences with differential timing
- Fairly rapid advances over last six years in methodologies
- Arguably the most recent wave of updates to our understanding of a technique that we thought was probably pretty straightforward
- Difference-in-differences is as we'll see very straightforward, but *regression* is less straightforward

Differential timing outline

We will cover some of the properties of twoway fixed effects (TWFE), some solutions and my personal opinions

1. Brief review of potential outcomes and the ATT
2. Difference-in-differences equation ("four averages and three differences") and the parallel trends assumption
3. TWFE Pathologies in static specification
   - → Goodman-Bacon decomposition as diagnosis of the problem
   - → Callaway and Sant'Anna estimator as a cure
4. TWFE Pathologies in event study specification
   - → Sun and Abraham as both a diagnosis and a cure
5. Application, practical advice and code

Beaver dam and diff-in-diff credibility crisis

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

Difference-in-differences credibility crisis

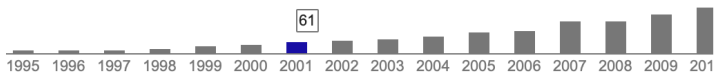
- I'll start with circa 2016 onward – several grad students and assistant professors found critical pathologies with TWFE and developed solutions
- Many simultaneous discoveries, some redundancies, and **sudden** awareness of the issues started happening around 2017, eventually became a massive thing
- Extreme meteoric rise, unusual for econometrics

Compare with LATE paper

- Compare with Imbens and Angrist 1995 LATE in *Econometrica*
- 61 annual cites the year Imbens is denied tenure at Harvard for what would later win him a Nobel Prize

Identification and estimation of local average treatment effects

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

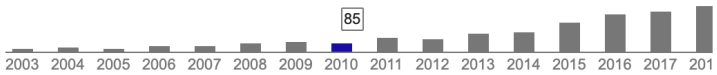


Compare with synth paper

- Athey and Imbens called synth the most important innovation in causal inference of the last two decades
- Most econometrics papers, even influential ones, show slow growth
- Something was different about diff-in-diff even before the econometricians recently shifted their attention to it

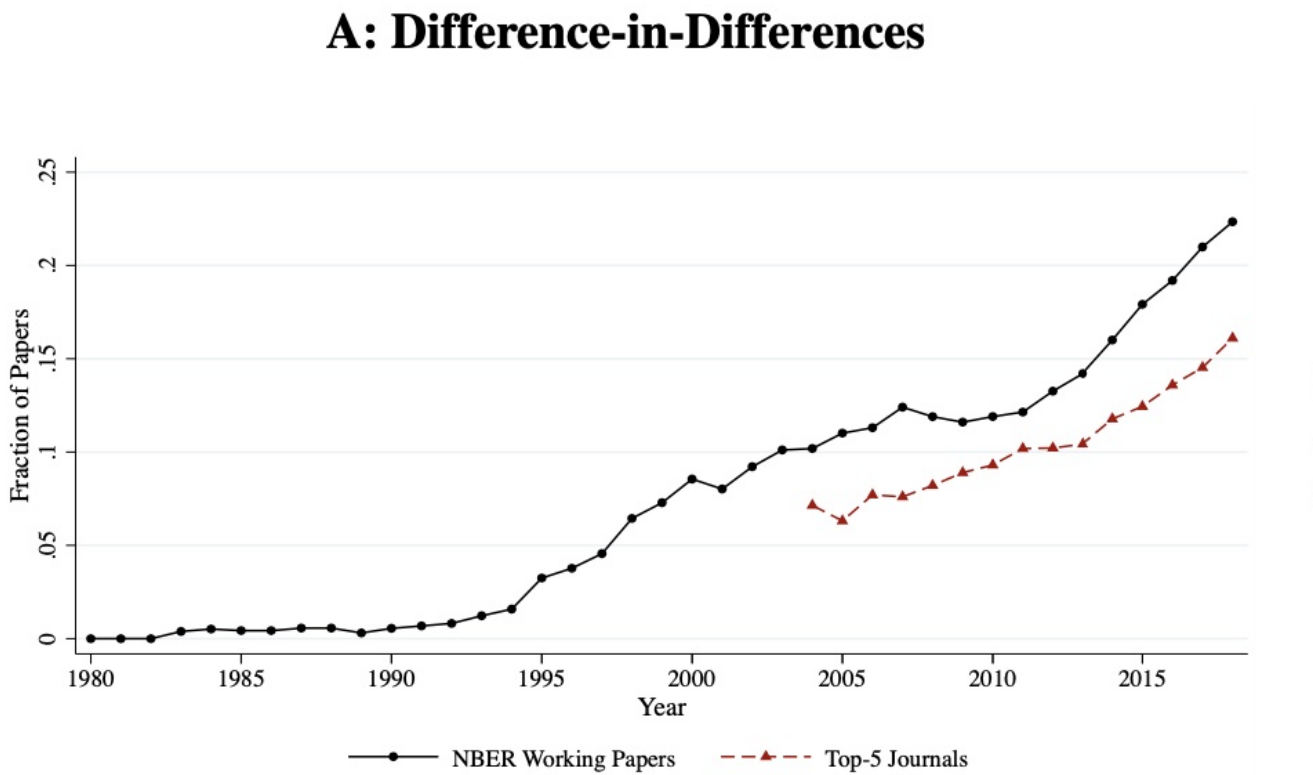
The economic costs of conflict: A case study of the Basque Country

Authors	Alberto Abadie, Javier Gardeazabal
Publication date	2003/3/1
Journal	American economic review
Volume	93
Issue	1
Pages	113-132
Publisher	American Economic Association
Description	This article investigates the economic effects of conflict, using the terrorist conflict in the Basque Country as a case study. We find that, after the outbreak of terrorism in the late 1960's, per capita GDP in the Basque Country declined about 10 percentage points relative to a synthetic control region without terrorism. In addition, we use the 1998–1999 truce as a natural experiment. We find that stocks of firms with a significant part of their business in the Basque Country showed a positive relative performance when truce became credible, and a negative relative performance at the end of the cease-fire.
Total citations	Cited by 5368



Diff-in-diff had belonged to the empiricists

Figure: Currie, et al. (2020)



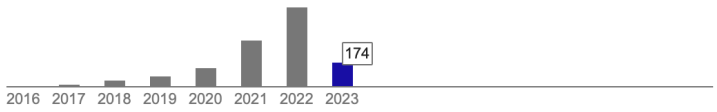
With some exception (e.g., Heckman, Ichimura and Todd 1997; Abadie 2005; Bertrand, Duflo and Mullainthan 2004), econometricians had not given it much notice

Borusyak et al

- Starts it all; written as grad students at Harvard
- Goes through many revisions, posted as working paper
- Returned to a few years ago with a third coauthor, Jahn Spiess, now R&R at Restud

Revisiting event study designs: Robust and efficient estimation

Authors	Kirill Borusyak, Xavier Jaravel, Jann Spiess
Publication date	2021/8/27
Journal	arXiv preprint arXiv:2108.12419
Description	We develop a framework for difference-in-differences designs with staggered treatment adoption and heterogeneous causal effects. We show that conventional regression-based estimators fail to provide unbiased estimates of relevant estimands absent strong restrictions on treatment-effect homogeneity. We then derive the efficient estimator addressing this challenge, which takes an intuitive "imputation" form when treatment-effect heterogeneity is unrestricted. We characterize the asymptotic behavior of the estimator, propose tools for inference, and develop tests for identifying assumptions. Extensions include time-varying controls, triple-differences, and certain non-binary treatments. We show the practical relevance of these insights in a simulation study and an application. Studying the consumption response to tax rebates in the United States, we find that the notional marginal propensity to consume is between 8 and 11 percent in the first quarter—about half as large as benchmark estimates used to calibrate macroeconomic models—and predominantly occurs in the first month after the rebate.
Total citations	Cited by 1399



“dCdH”

- First major hit (in AER), may have been in working paper in 2017 (at least 2018)
- Very thorough decomposition of the TWFE pathology, very general solution, included Stata code
- Very active and talented young team (assistant profs when this was done)

Two-way fixed effects estimators with heterogeneous treatment effects

Authors	Clément De Chaisemartin, Xavier d'Haultfoeuille
Publication date	2020/9/1
Journal	American Economic Review
Volume	110
Issue	9
Pages	2964-2996
Publisher	American Economic Association
Description	Linear regressions with period and group fixed effects are widely used to estimate treatment effects. We show that they estimate weighted sums of the average treatment effects (ATE) in each group and period, with weights that may be negative. Due to the negative weights, the linear regression coefficient may for instance be negative while all the ATEs are positive. We propose another estimator that solves this issue. In the two applications we revisit, it is significantly different from the linear regression estimator. (JEL *C21, C23, D72, J31, J51, L82*)
Total citations	Cited by 2019

Goodman-Bacon

- Arguably the most influential in terms of bringing attention to the problem (but no solution)
- Begun while grad student at Michigan, published last of the crop
- Probably Twitter network had a role as he was very active, also not an econometrician

Difference-in-differences with variation in treatment timing

Authors	Andrew Goodman-Bacon
Publication date	2021/12/1
Journal	Journal of Econometrics
Volume	225
Issue	2
Pages	254-277
Publisher	North-Holland
Description	The canonical difference-in-differences (DD) estimator contains two time periods, "pre" and "post", and two groups, "treatment" and "control". Most DD applications, however, exploit variation across groups of units that receive treatment at different times. This paper shows that the two-way fixed effects estimator equals a weighted average of all possible two-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 3307

“CS”

- Second published solution to the problem, written while assistant professors at Vanderbilt and Ole Miss,
- Pedro is a UC3M alum (2015 grad) and Brantly is a Vanderbilt grad
- Both are now coauthors with Andrew Goodman-Bacon
- Introduced new terms like group-time ATT, released very tight R code (“did”)

Difference-in-differences with multiple time periods

Authors	Brantly Callaway, Pedro HC Sant'Anna
Publication date	2021/12/1
Journal	Journal of Econometrics
Volume	225
Issue	2
Pages	200-230
Publisher	North-Holland
Description	In this article, we consider identification, estimation, and inference procedures for treatment effect parameters using Difference-in-Differences (DiD) with (i) multiple time periods, (ii) variation in treatment timing, and (iii) when the “parallel trends assumption” holds potentially only after conditioning on observed covariates. We show that a family of causal effect parameters are identified in staggered DiD setups, even if differences in observed characteristics create non-parallel outcome dynamics between groups. Our identification results allow one to use outcome regression, inverse probability weighting, or doubly-robust estimands. We also propose different aggregation schemes that can be used to highlight treatment effect heterogeneity across different dimensions as well as to summarize the overall effect of participating in the treatment. We establish the asymptotic properties of the proposed estimators and prove the …
Total citations	Cited by 2378

“SA”

- Third published solution to the problem, very similar to CS
- Focus was on decomposing the event study
- Written while grad students at MIT but Sophie Sun is now an assistant professor at CEMFI!

Estimating dynamic treatment effects in event studies with heterogeneous treatment effects

Authors	Liyang Sun, Sarah Abraham
Publication date	2021/12/1
Journal	Journal of Econometrics
Volume	225
Issue	2
Pages	175-199
Publisher	North-Holland
Description	To estimate the dynamic effects of an absorbing treatment, researchers often use two-way fixed effects regressions that include leads and lags of the treatment. We show that in settings with variation in treatment timing across units, the coefficient on a given lead or lag can be contaminated by effects from other periods, and apparent pretrends can arise solely from treatment effects heterogeneity. We propose an alternative estimator that is free of contamination, and illustrate the relative shortcomings of two-way fixed effects regressions with leads and lags through an empirical application.
Total citations	Cited by 1828

There’s even more and more coming

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

Roadmap

Background material
- Introduction

TWFE Pathologies
- Historical links
- Potential outcomes
- Bacon decomposition
- Simulation

Two solutions and a new decomposition
- CS
- SA

Some opinions and an application

Twoway fixed effects

- When working with panel data, the so-called “twoway fixed effects” (TWFE) estimator was the workhorse estimator
- And from the start, it was used with diff-in-diff
- But at the start, it wasn’t staggered adoption – it was a much simpler design in which a group was treated in one year, and a comparison group wasn’t

Two OLS Models

$$Y_{ist} = \alpha_0 + \alpha_1 Treat_{is} + \alpha_2 Post_t + \delta(Treat_{is} \times Post_t) + \varepsilon_{ist} \quad (1)$$

$$Y_{ist} = \beta_0 + \delta D_{ist} + \tau_t + \sigma_s + \varepsilon_{ist} \quad (2)$$

First equation is used for simple designs when everyone is treated at once; second equation was used when different groups were treated at different times (“differential timing”)

First equation works; second one only sometimes works

Orley goes to Washington

- Orley Ashenfelter graduated from Princeton in the 1970s, takes a job in Washington DC and begins studying “job trainings programs”
- Empirical crisis in empirical macro and empirical labor back in the 1970s – Orley, David Card, Bob Lalonde, Alan Krueger at Princeton all helped bring attention to it and began pushing for solutions, one of which was RCTs in labor but also diff-in-diff as well as better instruments
- Listen to Orley explain the connection he made between twoway fixed effects and difference-in-differences; it was born out of a need to explain OLS to an American bureaucrat

https://youtu.be/WnB3EJ8K7lg?t=126

Equivalence

$$Y_{ist} = \alpha_0 + \alpha_1 Treat_{is} + \alpha_2 Post_t + \delta(Treat_{is} \times Post_t) + \varepsilon_{ist}$$

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

- Orley claims that the TWFE estimator of δ and the "four averages and three subtractions" are the same thing numerically
- And they are – they are numerically *identical*
- And under a particular assumption, they are also unbiased estimates of an aggregate causal parameter
- But to see this we need new notation – potential outcomes

Potential outcomes notation

- Let the treatment be a binary variable:

$$D_{i,t} = \begin{cases} 1 \text{ if in job training program } t \\ 0 \text{ if not in job training program at time } t \end{cases}$$

where i indexes an individual observation, such as a person

Potential outcomes notation

- Potential outcomes:

$$Y_{i,t}^{j} = \begin{cases} 1\text{: wages at time } t \text{ if trained} \\ 0\text{: wages at time } t \text{ if not trained} \end{cases}$$

where j indexes a counterfactual state of the world

Treatment effect definitions

Individual treatment effect

The individual treatment effect, δ_i, equals $Y_i^1 - Y_i^0$

Missing data problem: I don't know my own counterfactual

Conditional Average Treatment Effects

Average Treatment Effect on the Treated (ATT)

The average treatment effect on the treatment group is equal to the average treatment effect conditional on being a treatment group member:

$$\begin{aligned} E[\delta|D=1] &= E[Y^1 - Y^0|D=1] \\ &= E[Y^1|D=1] - E[Y^0|D=1] \end{aligned}$$

This is one of the most important policy parameters, if not the most important, and coincidentally it's also the parameter you get with diff-in-diff (even with heterogeneity)

Potential outcomes vs data

- ATT is expressed in terms of potential outcomes, but we do not use potential outcomes for estimation; we use data
- Potential outcomes are unknown and *hypothetical* possibilities describing states of the world but our data are realized outcomes, or "data", that actually occurred
- Potential outcomes become realized under treatment assignment

$$Y_{it} = D_{it}Y_{it}^1 + (1 - D_{it})Y_{it}^0$$

- Depending on how the treatment is assigned really dictates whether correlations reveal causal effects or bias

DiD equation

Orley's "four averages and three subtractions", or what Bacon will call the 2x2

$$\widehat{\delta} = \Big(E[Y_k|Post] - E[Y_k|Pre] \Big) - \Big(E[Y_U|Post] - E[Y_U|Pre] \Big)$$

k are the people in the job training program, U are the untreated people not in the program, $Post$ is after the trainees took the class, Pre is the period just before they took the class, and $E[y]$ is mean earnings.

Does $\widehat{\delta}$ equal the ATT? If so when? If not why not?

Potential outcomes and the switching equation

$$\widehat{\delta} = \underbrace{\left(E[Y_k^1|Post] - E[Y_k^0|Pre] \right) - \left(E[Y_U^0|Post] - E[Y_U^0|Pre] \right)}_{\text{Switching equation}}$$

$$+ \underbrace{E[Y_k^0|Post] - E[Y_k^0|Post]}_{\text{Adding zero}}$$

Parallel trends bias

$$\widehat{\delta} = \underbrace{E[Y_k^1|Post] - E[Y_k^0|Post]}_{\text{ATT}} + \underbrace{\Big[E[Y_k^0|Post] - E[Y_k^0|Pre]\Big] - \Big[E[Y_U^0|Post] - E[Y_U^0|Pre]\Big]}_{\text{Non-parallel trends bias in 2x2 case}}$$

Identification through parallel trends

Parallel trends

Assume two groups, treated and comparison group, then we define parallel trends as:

$$E(\Delta Y_k^0) = E(\Delta Y_U^0)$$

In words: "The evolution of earnings for our trainees *had they not trained* is the same as the evolution of mean earnings for non-trainees".

It's in red because parallel trends is untestable and critically important to estimation of the ATT using any method, OLS or "four averages and three subtractions"

Discussion of estimate

$$Y_{ist} = \beta_0 + \delta D_{ist} + \tau_t + \sigma_s + \varepsilon_{ist}$$

- So that's the simple case; what about the differential timing case?
- If you estimate with OLS with differential timing, what does $\widehat{\delta}$ correspond to?
- It also corresponds to the previous "four averages and three subtractions" – but it's numerous of them, not just one

Decomposition Preview

- Andrew Goodman-Bacon decomposed $\widehat{\delta}$ and showed it is numerically identical to a weighted average of all “four averages and three subtractions”
- But, even before we get to causality there are unusual features
- TWFE model assigns its own weights which are a function of the size of a “group” and the variance of group treatment dummies

K^2 distinct DDs

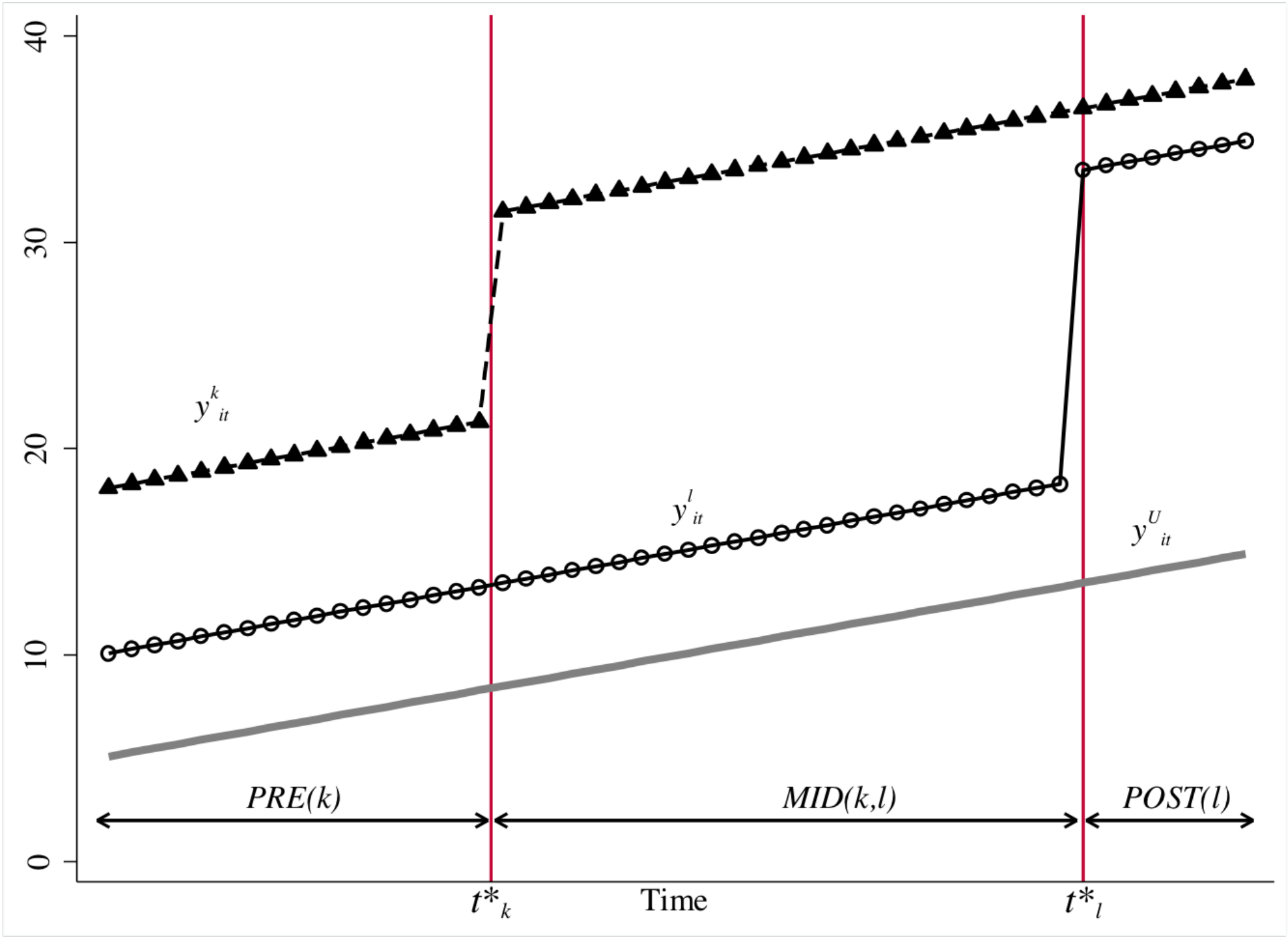
Let's look at 3 timing groups (a, b and c) and one untreated group (U). With 3 timing groups, there are 9 2x2 DDs. Here they are:

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

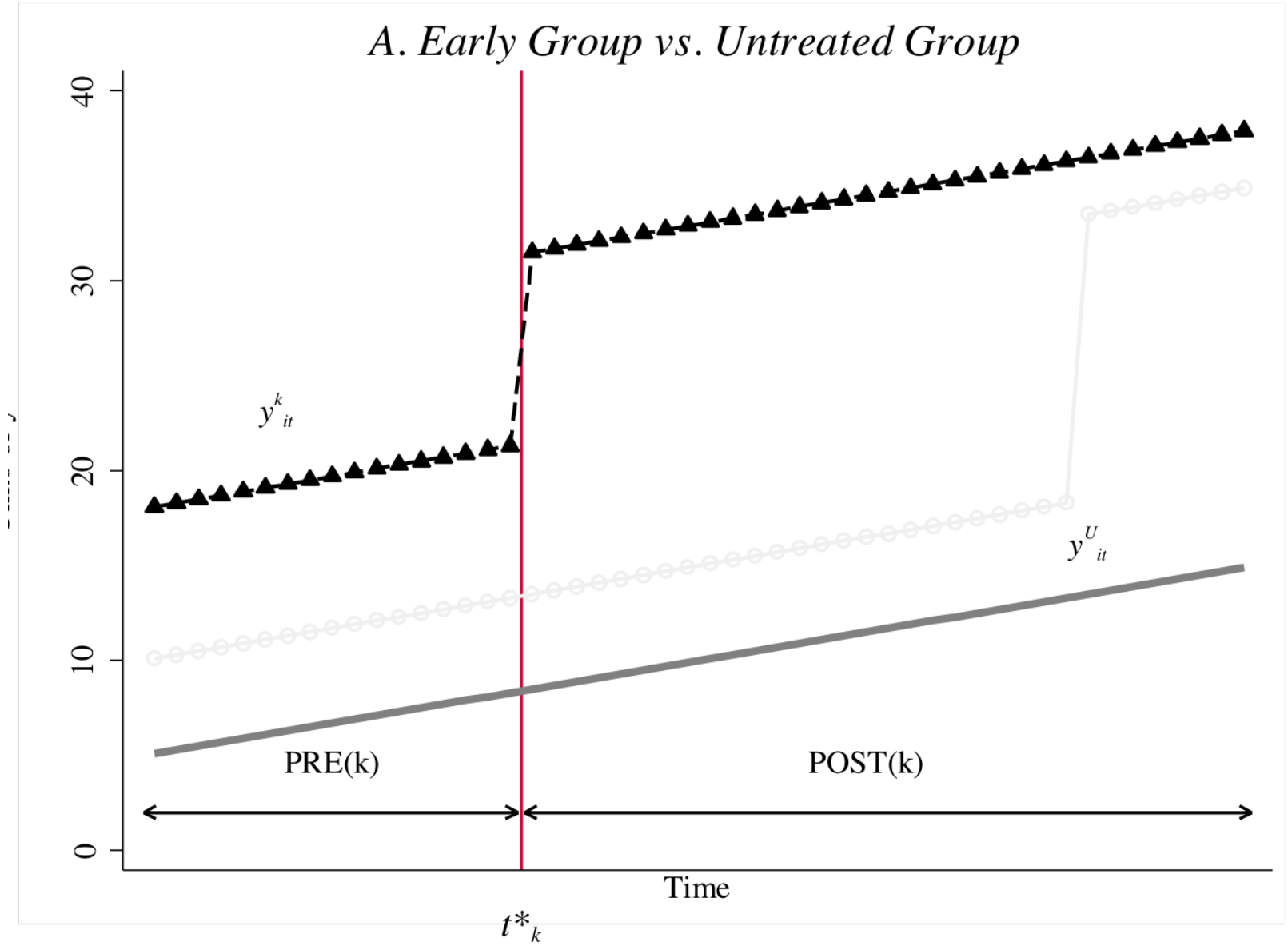
Let's return to a simpler example with only two groups – a k group treated at t_k^* and an l treated at t_l^* plus an never-treated group called the U untreated group

Terms and notation

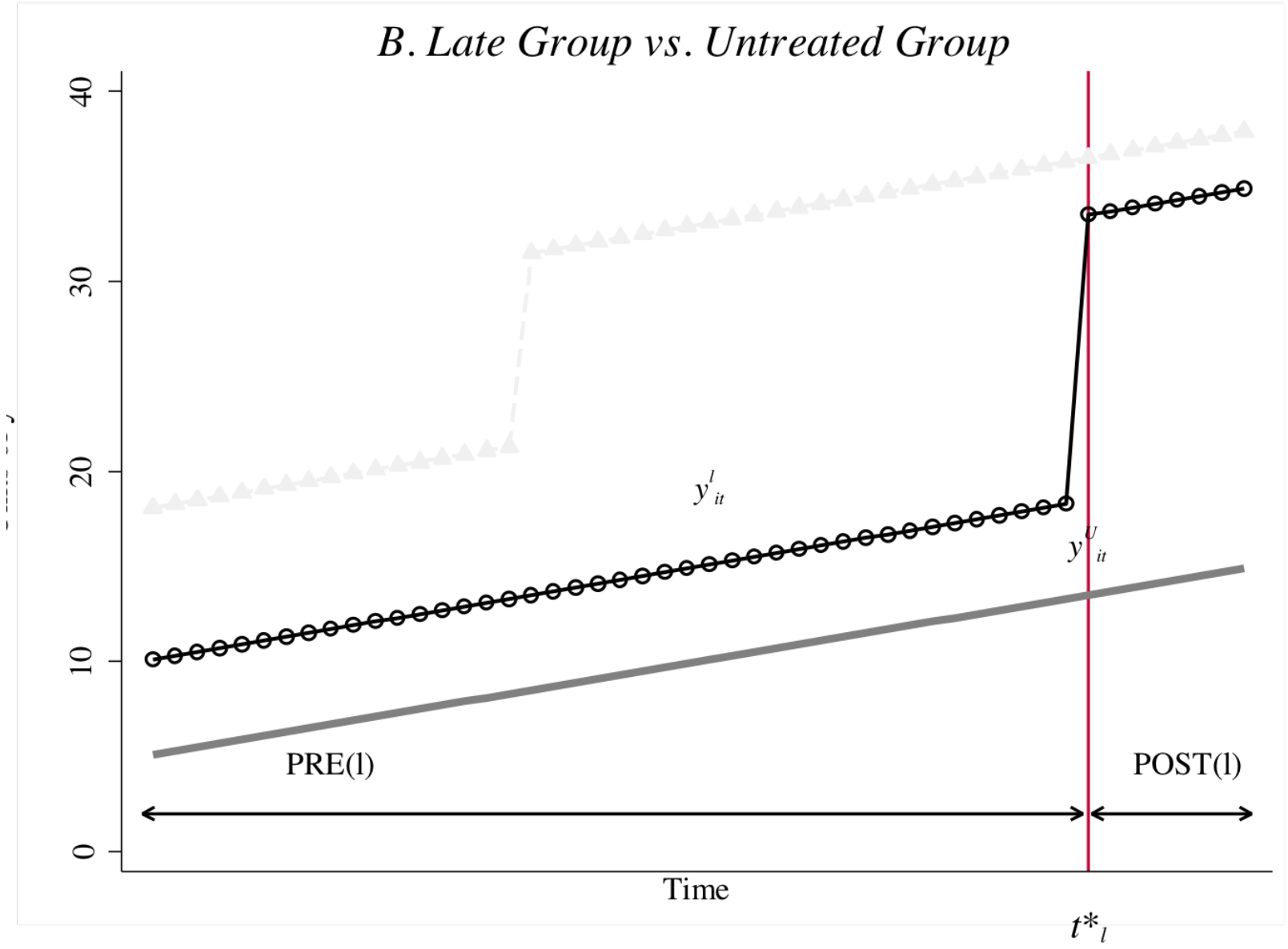
- Let there be two treatment groups (k,l) and one untreated group (U)
- k,l define the groups based on when they receive treatment (differently in time) with k receiving it earlier than l
- Denote $\overline{D}_k$ as the share of time each group spends in treatment status
- Denote $\widehat{\delta}_{jb}^{2x2}$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

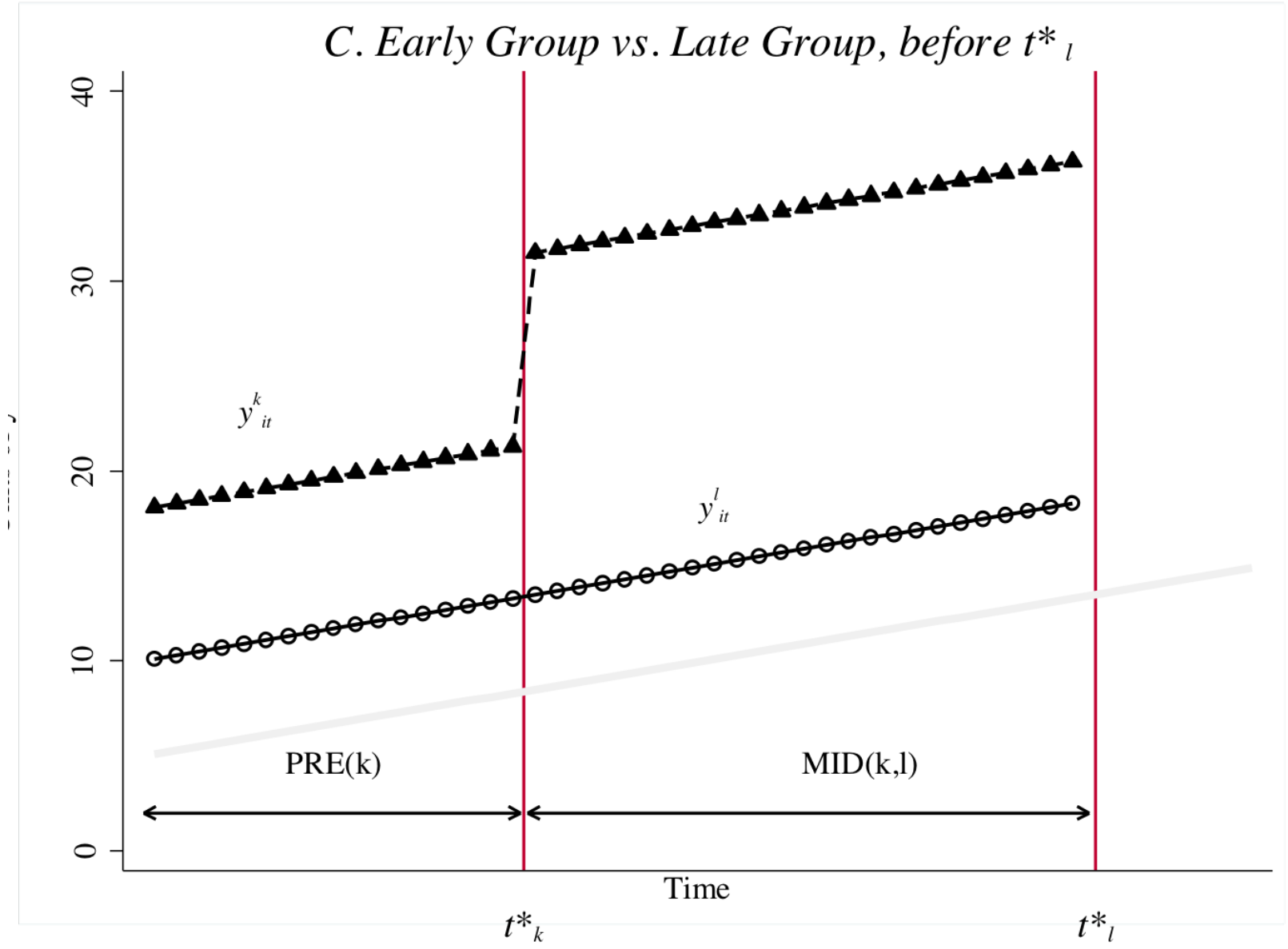
$$\widehat{\delta}_{kU}^{2x2} = \left(\overline{y}_k^{post(k)} - \overline{y}_k^{pre(k)} \right) - \left(\overline{y}_U^{post(k)} - \overline{y}_U^{pre(k)} \right)$$



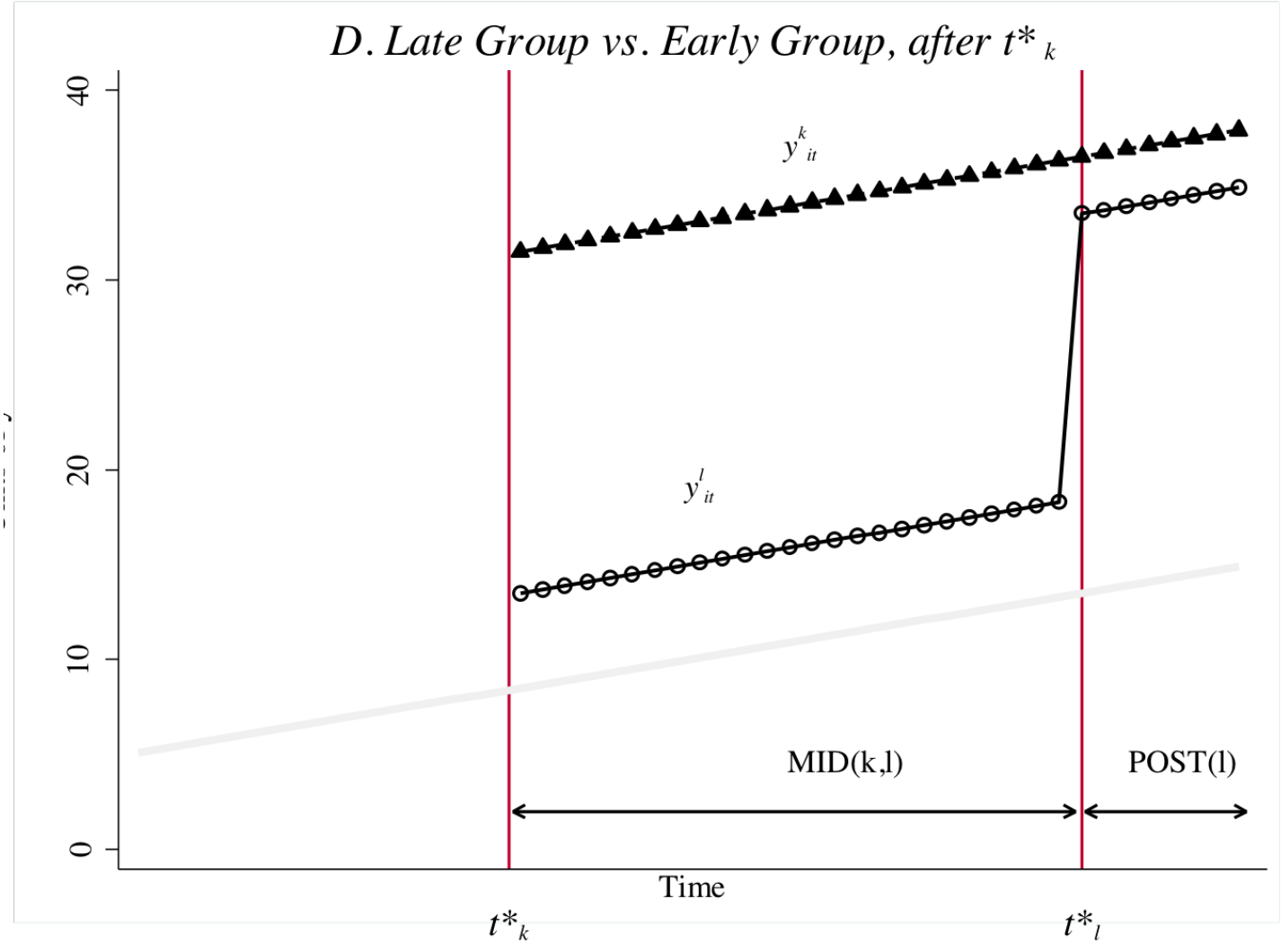
$$\widehat{\delta}_{lU}^{2x2} = \left(\overline{y}_{l}^{post(l)} - \overline{y}_{l}^{pre(l)}\right) - \left(\overline{y}_{U}^{post(l)} - \overline{y}_{U}^{pre(l)}\right)$$



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



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



Bacon decomposition

$$Y_{ist} = \beta_0 + \delta D_{ist} + \tau_t + \sigma_s + \varepsilon_{ist}$$

TWFE estimate of $\widehat{\delta}$ is equal to a weighted average over all group 2x2 (of which there are 4 in this example)

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

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

Back to TWFE

$$Y_{ist} = \beta_0 + \delta D_{ist} + \tau_t + \sigma_s + \varepsilon_{ist}$$

- So we know that the estimate is a weighted average over all "four averages and three subtractions" but is that good or bad?
- It's good if it's unbiased; it's bad if it isn't, and the decomposition doesn't tell us which unless we replace realized outcomes with potential outcomes
- Bacon shows that TWFE estimate of δ needs two assumptions for unbiasedness:
  1. variance weighted parallel trends are zero and
  2. 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 (not just weighted average of DiDs)

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
- I'll impose "unit level parallel trends", which is much stronger than we need (we only need average parallel trends)
- Also no anticipation of treatment effects until treatment occurs but does *not* guarantee homogenous treatment effects
- Two types of situations: constant versus dynamic treatment effects

Constant vs Dynamic Treatment Effects

Calendar Time	ATT(1986,t)	ATT(1992,t)	ATT(1998,t)	ATT(2004,t)
1980	0	0	0	0
1981	0	0	0	0
1982	0	0	0	0
1983	0	0	0	0
1984	0	0	0	0
1985	0	0	0	0
1986	10	0	0	0
1987	10	0	0	0
1988	10	0	0	0
1989	10	0	0	0
1990	10	0	0	0
1991	10	0	0	0
1992	10	8	0	0
1993	10	8	0	0
1994	10	8	0	0
1995	10	8	0	0
1996	10	8	0	0
1997	10	8	0	0
1998	10	8	6	0
1999	10	8	6	0
2000	10	8	6	0
2001	10	8	6	0
2002	10	8	6	0

Calendar Time	ATT(1986,t)	ATT(1992,t)	ATT(1998,t)	ATT(2004,t)
1980	0	0	0	0
1981	0	0	0	0
1982	0	0	0	0
1983	0	0	0	0
1984	0	0	0	0
1985	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

Group-time ATT

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

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

Estimation

Estimate the following equation using OLS:

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

Table: Estimating ATT with different models

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

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

Bacon decomposition

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

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

T $=$ Treatment; C$=$ Comparison

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

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

Roadmap

Background material
- Introduction

TWFE Pathologies
- Historical links
- Potential outcomes
- Bacon decomposition
- Simulation

Two solutions and a new decomposition
- CS
- SA

Some opinions and an application

Callaway and Sant'Anna 2020

CS is a DiD estimator used for estimating and then summarizing smaller ATT parameters under differential timing and conditional parallel trends into more policy relevant ATT parameters (either dynamic or static)

Difference-in-differences with multiple time periods

Authors	Brantly Callaway, Pedro HC Sant'Anna
Publication date	2021/12/1
Journal	Journal of Econometrics
Volume	225
Issue	2
Pages	200-230
Publisher	North-Holland
Description	In this article, we consider identification, estimation, and inference procedures for treatment effect parameters using Difference-in-Differences (DiD) with (i) multiple time periods, (ii) variation in treatment timing, and (iii) when the "parallel trends assumption" holds potentially only after conditioning on observed covariates. We show that a family of causal effect parameters are identified in staggered DiD setups, even if differences in observed characteristics create non-parallel outcome dynamics between groups. Our identification results allow one to use outcome regression, inverse probability weighting, or doubly-robust estimands. We also propose different aggregation schemes that can be used to highlight treatment effect heterogeneity across different dimensions as well as to summarize the overall effect of participating in the treatment. We establish the asymptotic properties of the proposed estimators and prove the …
Total citations	Cited by 2378

When is CS used

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

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

CS estimates the ATT by identifying smaller causal effects and aggregating them using non-negative weights

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:

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

Assumptions

Assumption 1: Sampling is iid (panel data, but repeated cross-sections are possible)

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. CS uses the never-treated or the not-yet-treated as controls but never the already-treated

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

- Sometimes we care about a simple summary, and sometimes we care about separating it out in time and sometimes in even more interesting ways
- 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

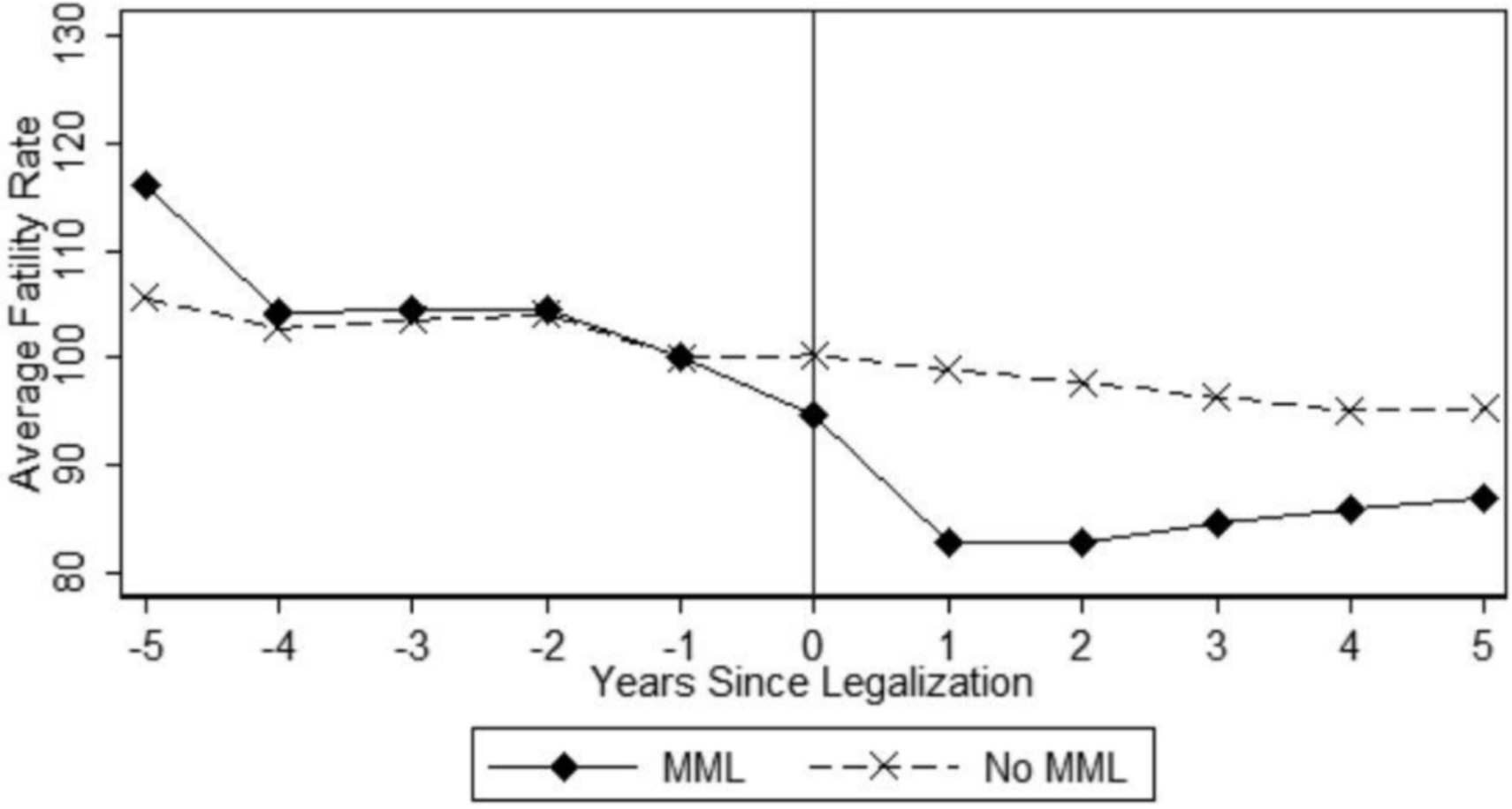


Figure: Anderson, et al. (2013) display of raw traffic fatality rates for re-centered treatment states and control states with randomized treatment dates

Replicated from a project of mine

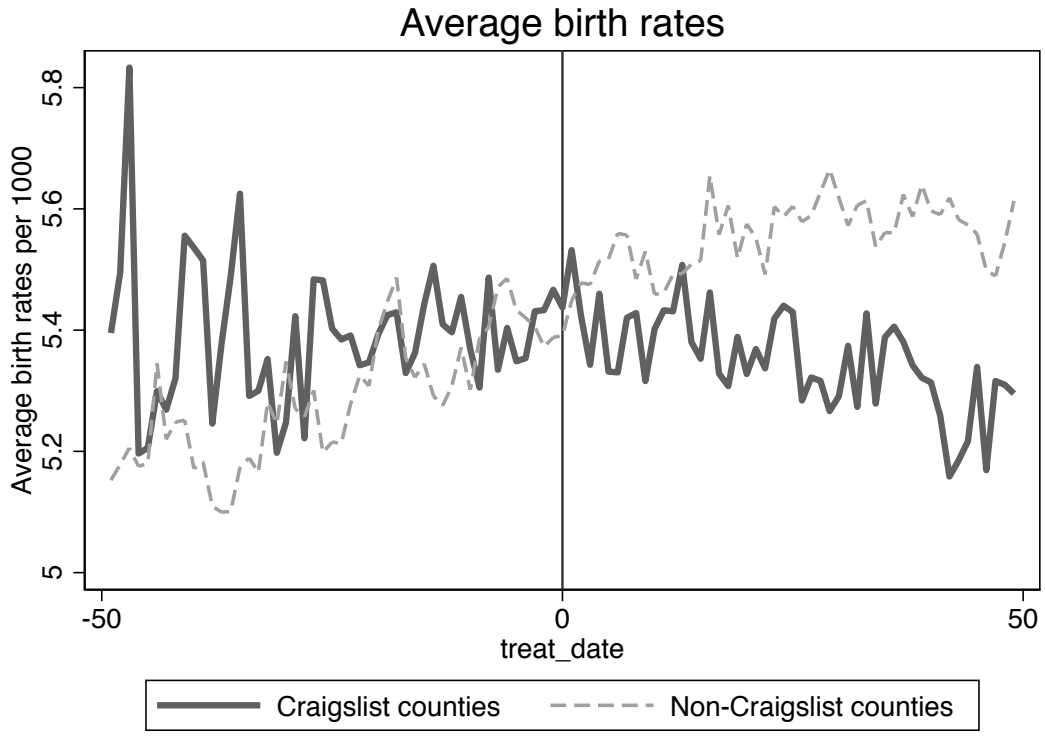
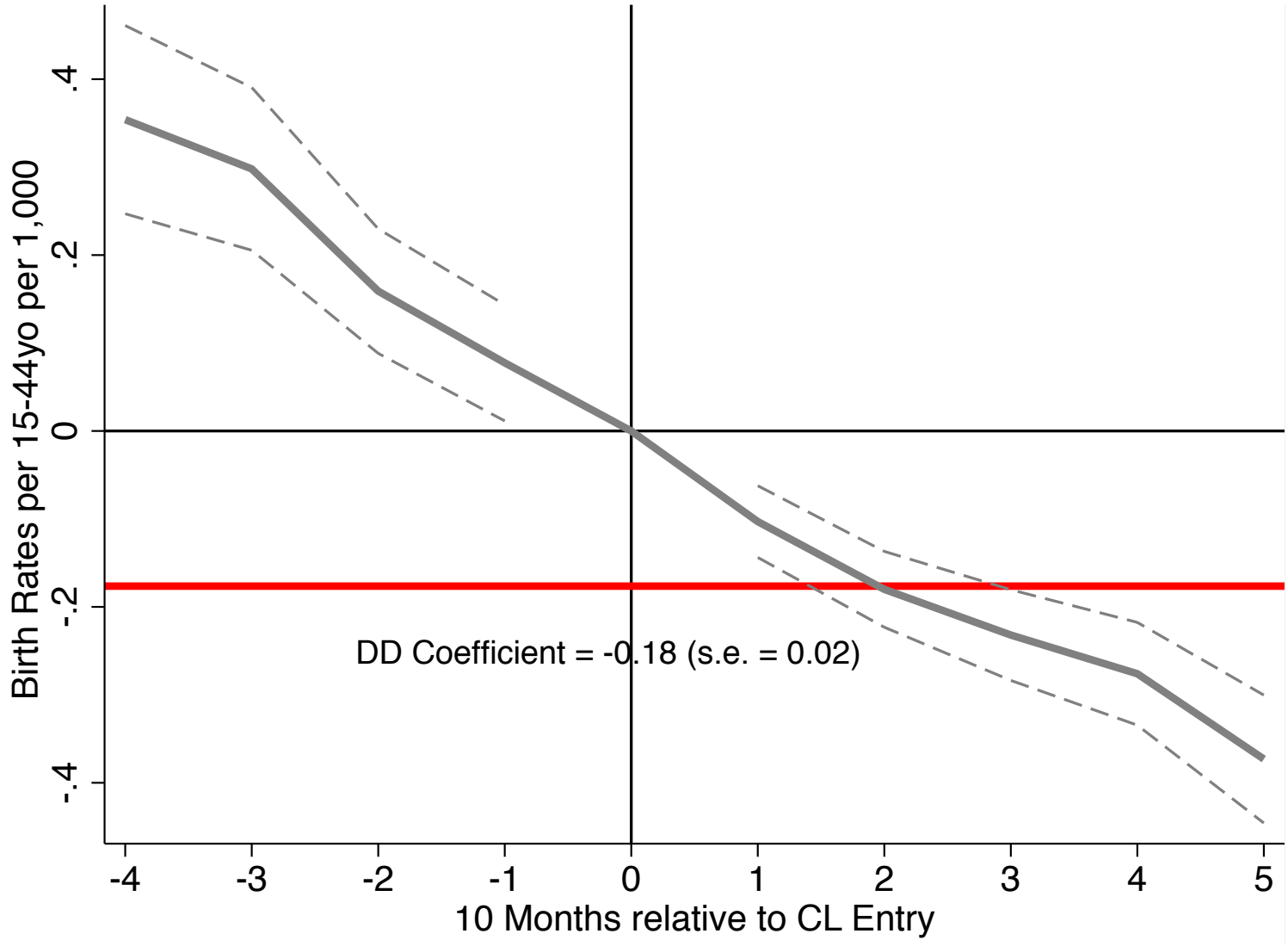


Figure: Roll out of Craigslist "personal ads" for casual intimate encounters and birth rates using the "randomized treatment assignment" approach for visualization

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.



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

Bias of TWFE Event Study Specificaiton

- Bacon only focused on the static specification, and that's where the biases due to dynamics revealed itself
- He was unable to get into the leads and lags using the FWL method he was using ("it's hard!" - Bacon)
- Sophie Sun and Sarah Abraham did though – prompted by a stray comment by their professor
- But they also unlike Bacon present a solution (which is like CS, but discovered independently)

Sun and Abraham 2020

1. SA shows 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 (which is then later generalized by Goldsmith-Pinkham, Hull and Kolsar 2022)
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)
4. Derives the variance of the estimator instead of bootstrapping, handles covariates differently than CS, but otherwise identical

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

Dynamic TWFE 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

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 IW estimator is consistent for a weighted average of $CATT_{e,l}$ with weights equal to the share of each cohort in the relevant period(s).
- They show that each IW estimator is asymptotically normal and derive its asymptotic variance. Doesn't rely on bootstrap like CS.

DD Estimator of CATT

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

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

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

Software

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

Reporting results

Table: Estimating ATT

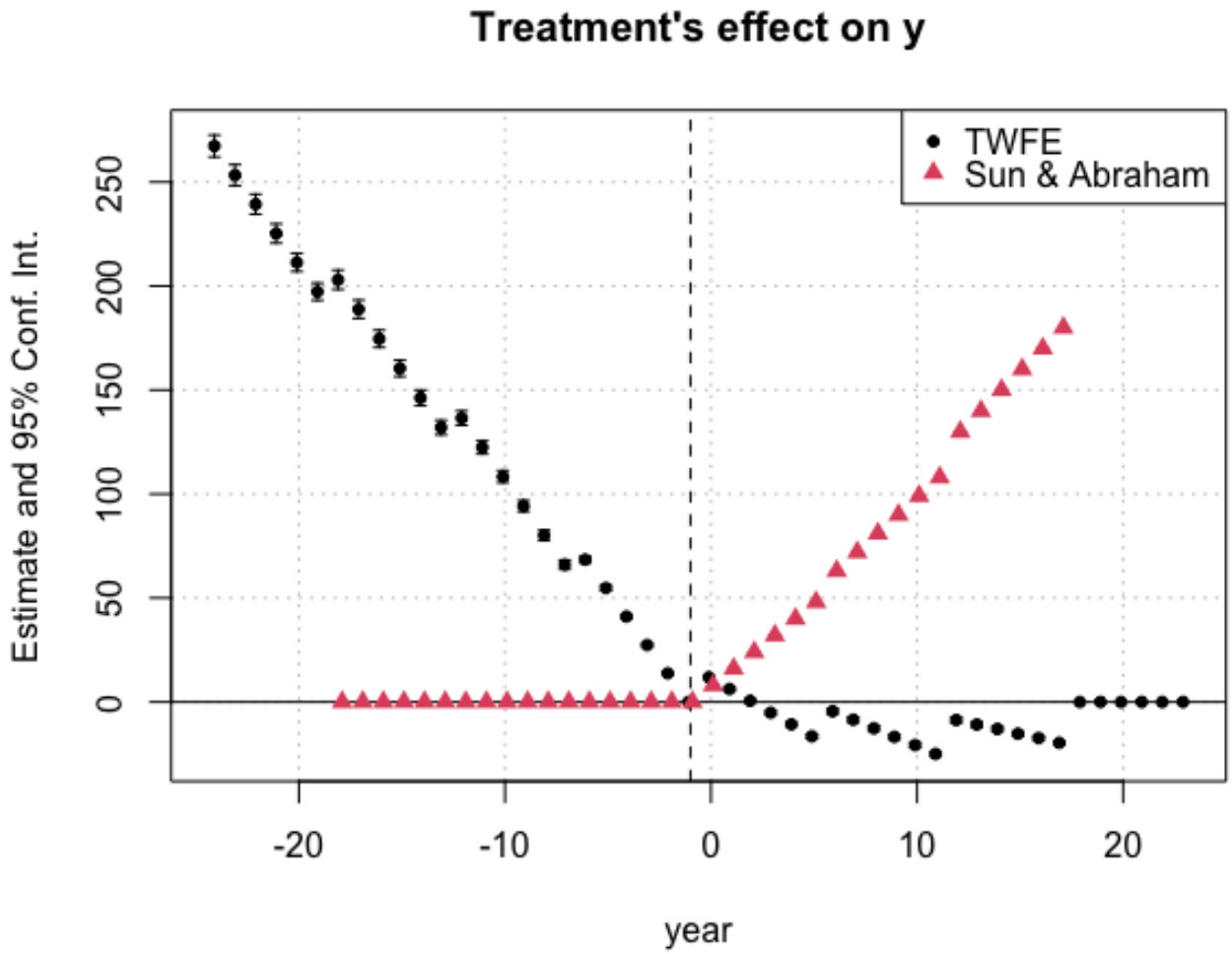
	(Truth)	**(TWFE)**	**(CS)**	**(SA)**	**(BJS)**
$\widehat{Feasible\ ATT}$	68.33	26.81***	68.34***	68.33***	

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?

Roadmap

Background material
- Introduction

TWFE Pathologies
- Historical links
- Potential outcomes
- Bacon decomposition
- Simulation

Two solutions and a new decomposition
- CS
- SA

Some opinions and an application

Advice

- DiD will remain popular for a while, and if anything all this new DiD has brought even more attention to it
- But now things are changing – how do we write the papers? Not just how do we estimate parameters
- Papers are a combination of science and rhetoric – let's look at a new one
- Braghieri, Levy and Makarin (2022), "Social Media and Mental Health", *American Economic Review*, 112(11): 3660-3693

Big picture

- Widely cited that social media causes mental health problems in youth
- Anecdotal, documentaries, but no causal evidence (“slim to none”)
- Study will use staggered rollout of Facebook platform to college campuses from 2004 to 2006 to estimate the effect on aggregate mental health scores from a survey
- You be the judge, but they present what in most cases would be strong evidence that Facebook harmed college students mental health

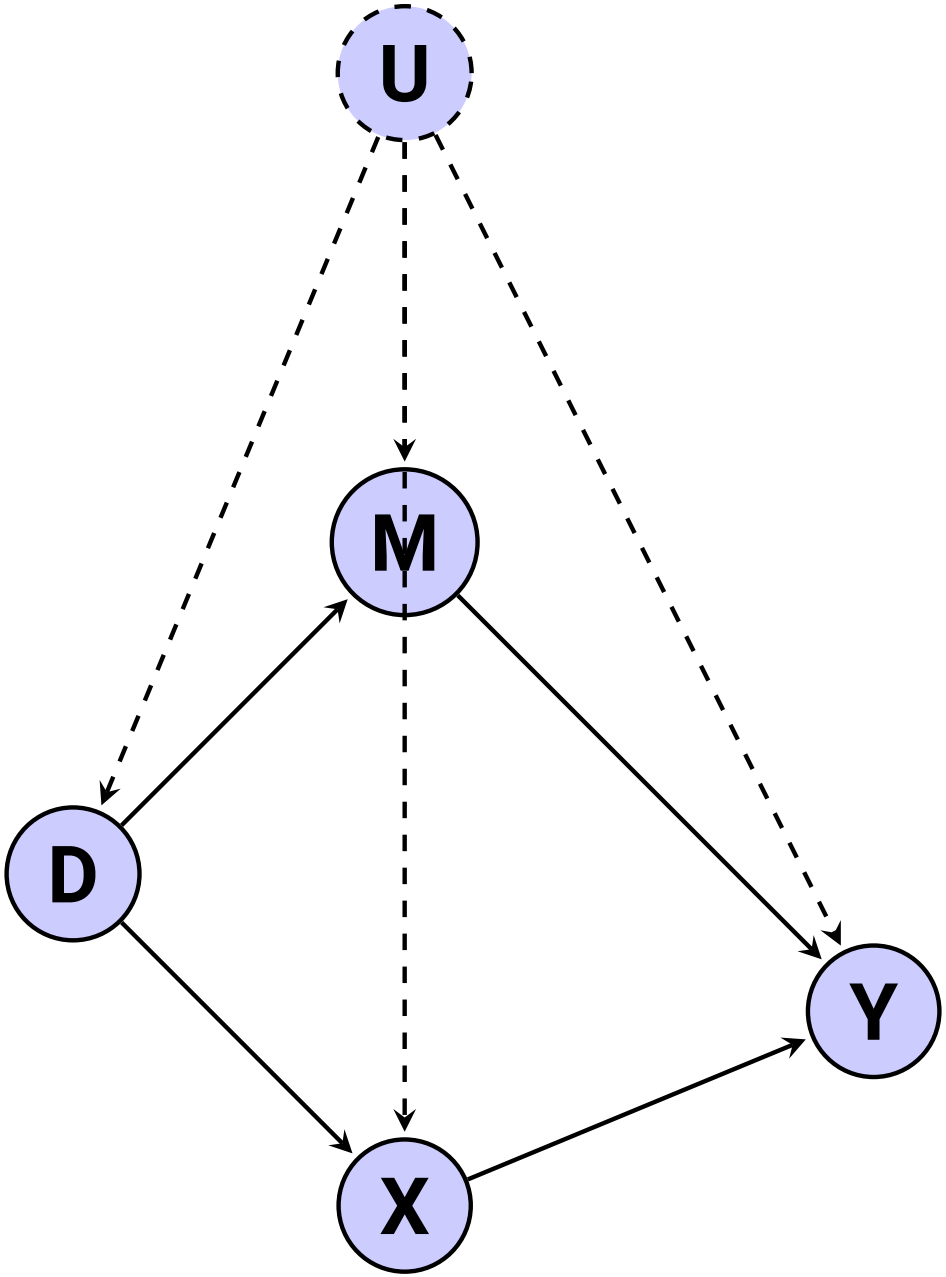
Many things to like

- Important question: mental health, suicide, review descriptive stats together
- Strong design: staggered rollout
- Event study is eye popping
- Mechanism and main results
- Very interesting dataset

Fives parts of a strong DiD

1. **Bite**: They cannot really show much here. No data on Facebook usage. More an ITT
2. **Main Results**: Estimated effect on mental health measures
3. Mechanism: Speculative
4. **Falsifications**: I can't really see very strong falsifications either.
5. **Event studies**: POW. Just wait

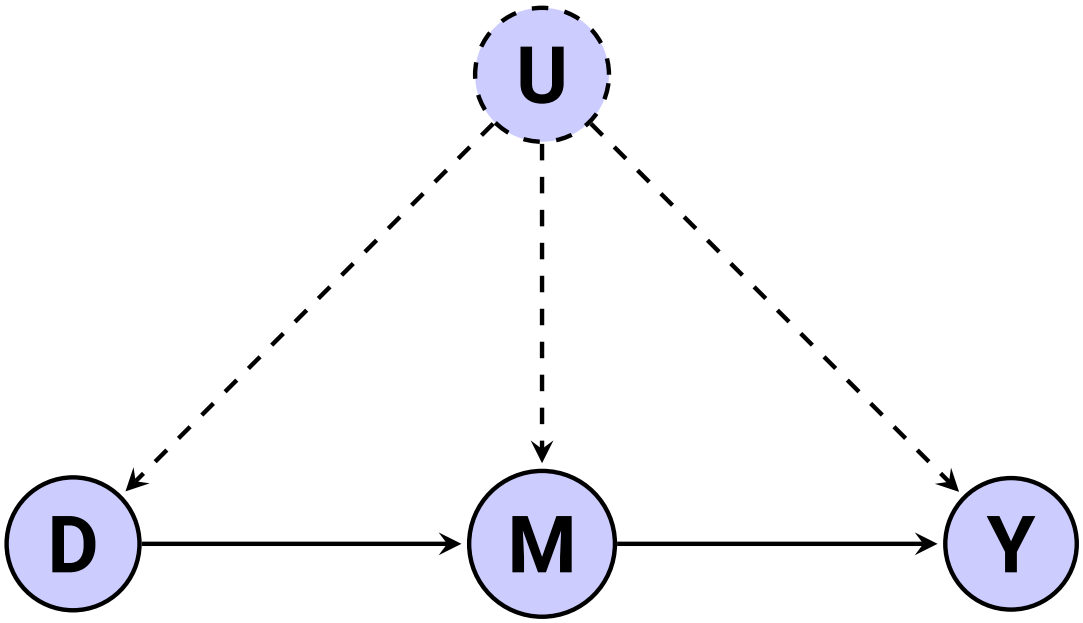
Mechanism



Mechanism

- D is the treatment variable, and the ATT is over all possible channels, but what if you want to think M is the mechanism
- When you can't rule out competing theories with falsifications, you have to try and build the case that the effect is coming through a channel
- Rule out X and provide evidence for M
- Goal here is to try and present evidence (not proof) that it's probably the story you're saying

Ruling out alternative mechanism



Mechanism

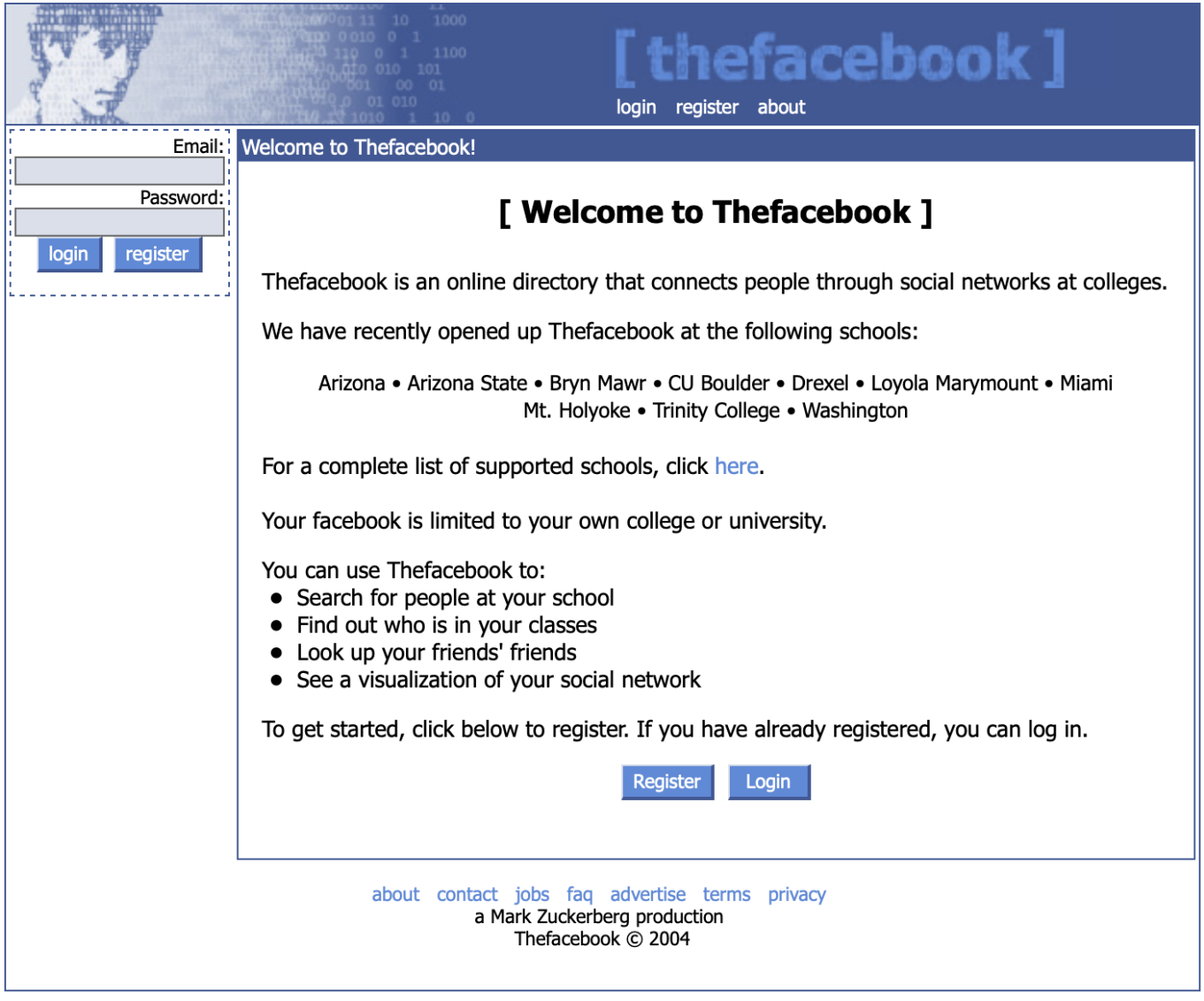
- Story is interpersonal comparisons which they try to show
- We can discuss how plausible we found it, but ask yourself at the end – did the event study help you believe it? Why/why not?

Data on Facebook

- Ingenious use of the Wayback Time Machine
- Looked at over 700 schools using Facebook screen shots
- When Facebook first mentions a school on its front page, that school is marked as having gotten Facebook

New schools being adopted

New schools being adopted



Data on college students

- NCHA Data is survey administered to college students on a semi-annual basis by American College Health Assoc
- Inquires about demographics, physical health, mental health, alcohol and drug use, sexual behaviors, and perception of these behaviors by peers
- ACHA merged a treatment indicator to each respondent based on Facebook dataset provided to them so that privacy could be maintained

Mental health

- Self-reported symptoms are standard medical practice in mental health – DSM-5 relies on self-reports such as difficulty sleeping, fatigue, feelings of guilt, suicidal ideation
- No data on Facebook or social media usage so this is ITT version of the ATT
- Respondent answers to the questions are aggregated into indices such as *poor mental health* where larger numbers are worse

Main TWFE Model

$$Y_{icgt} = \alpha_g + \delta_t + \beta \times Facebook_{gt} + X_i \times \gamma + X_c \times \psi + \varepsilon_{icgt} \quad (3)$$

Y_{icgt} is an outcome for person i in wave t attending college c in expansion group g; α_g is expansion-group or college fixed effects; δ_t are survey-wave fixed effects; $Facebook_{gt}$ indicates the respondents' campus has Facebook by time t at expansion group g; X_i and X_c are individual and college-level controls; and standard errors are clustered at college level.

$\widehat{\beta}$ identifies the ATT under parallel trends in the robust models

Robustness

- Main static results will all be in TWFE, but appendix shows other methods like CS and SA
- Event studies will show all models including some we haven't reviewed
- Growing popularity to show "all the robust DiD" models so that readers can see you aren't cherry picking

TABLE 1—BASELINE RESULTS: INDEX OF POOR MENTAL HEALTH

	Index of poor mental health			
	(1)	(2)	(3)	(4)
Post-Facebook introduction	0.137	0.124	0.085	0.077
	(0.040)	(0.022)	(0.033)	(0.032)
Observations	374,805	359,827	359,827	359,827
Survey-wave fixed effects	✓	✓	✓	✓
Facebook-expansion-group fixed effects	✓	✓		
Controls		✓	✓	✓
College fixed effects			✓	✓
FB-expansion-group linear time trends				✓

Notes: This table explores the effect of the introduction of Facebook at a college on student mental health. Specifically, it presents estimates of coefficient β from equation (1) with our index of poor mental health as the outcome variable. The index is standardized so that, in the preperiod, it has a mean of zero and a standard deviation of one. Column 1 estimates equation (1) without including controls; column 2 estimates equation (1) including controls; column 3, our preferred specification, replaces Facebook-expansion-group fixed effects with college fixed effects; column 4 includes linear time trends estimated at the Facebook-expansion-group level. Our controls consist of age, age squared, gender, indicators for year in school (freshman, sophomore, junior, senior), indicators for race (White, Black, Hispanic, Asian, Indian, and other), and an indicator for international student. Column 2 also includes indicators for geographic region of college (Northeast, Midwest, West, South); such indicators are omitted in columns 3 and 4 because they are collinear with the college fixed effects. For a detailed description of the outcome, treatment, and control variables, see online Appendix Table A.31. Standard errors in parentheses are clustered at the college level.

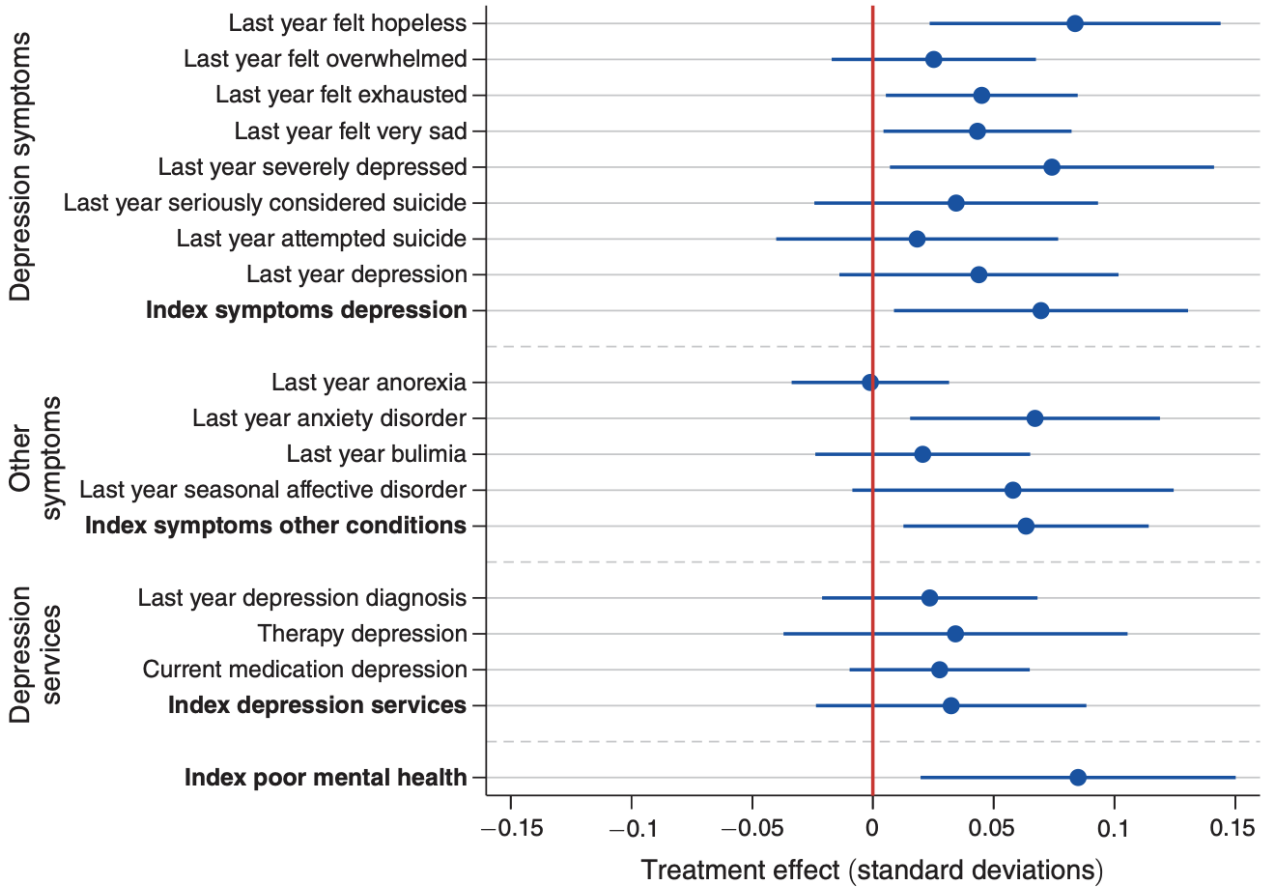


FIGURE 1. EFFECTS OF THE INTRODUCTION OF FACEBOOK ON STUDENT MENTAL HEALTH

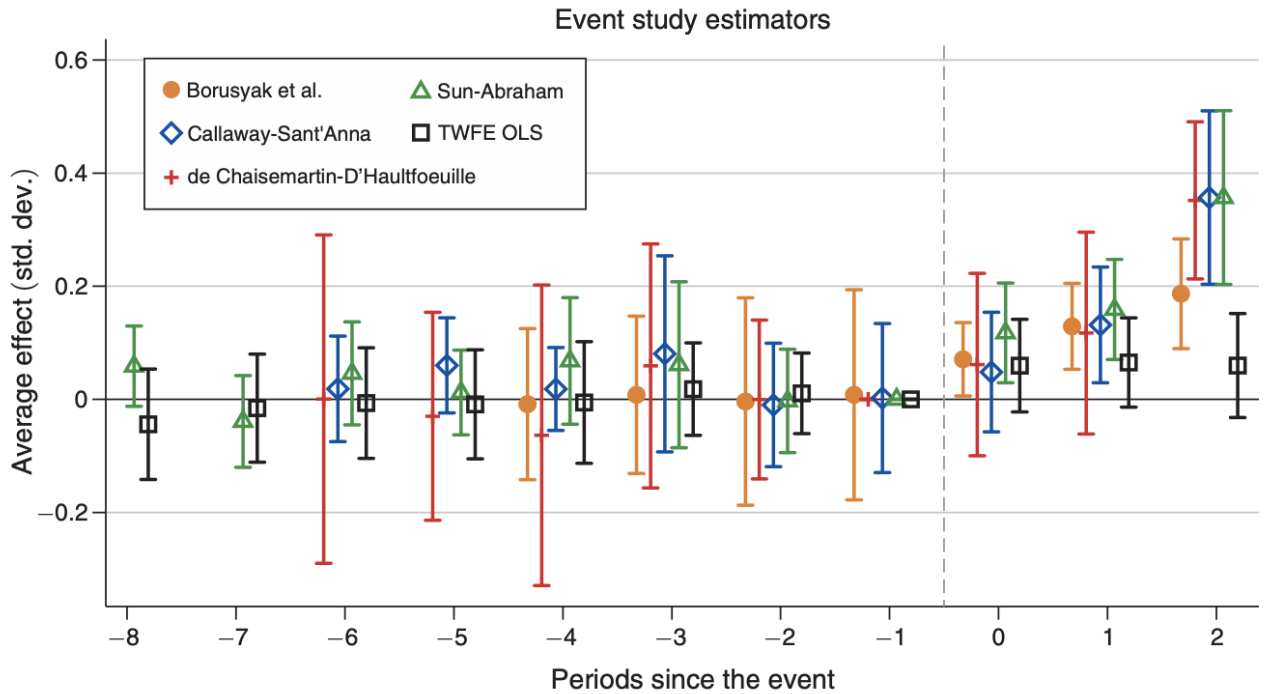


FIGURE 2. EFFECTS OF FACEBOOK ON THE INDEX OF POOR MENTAL HEALTH BASED ON DISTANCE TO/FROM FACEBOOK INTRODUCTION

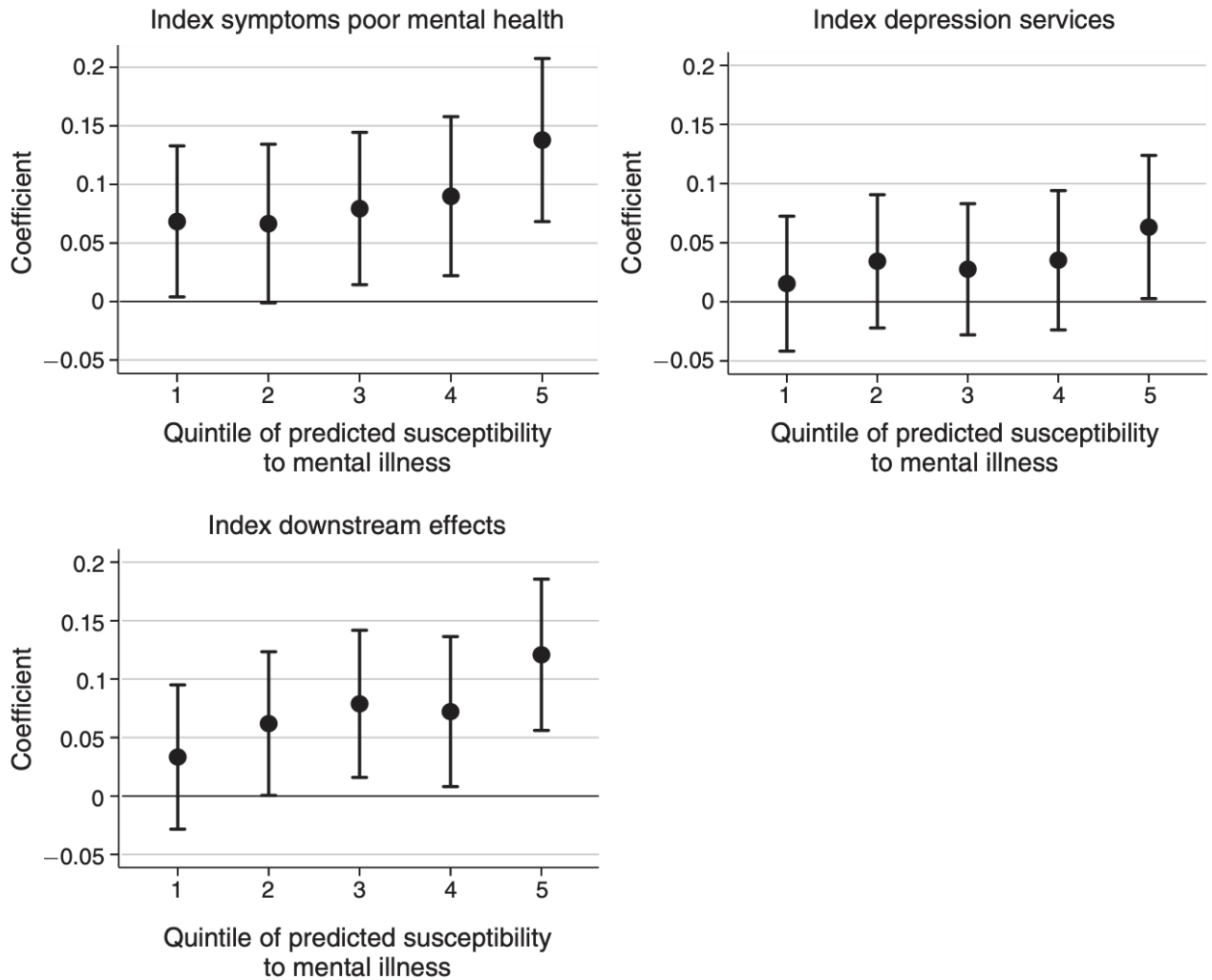


FIGURE 3. HETEROGENEOUS EFFECTS BY PREDICTED SUSCEPTIBILITY TO MENTAL ILLNESS

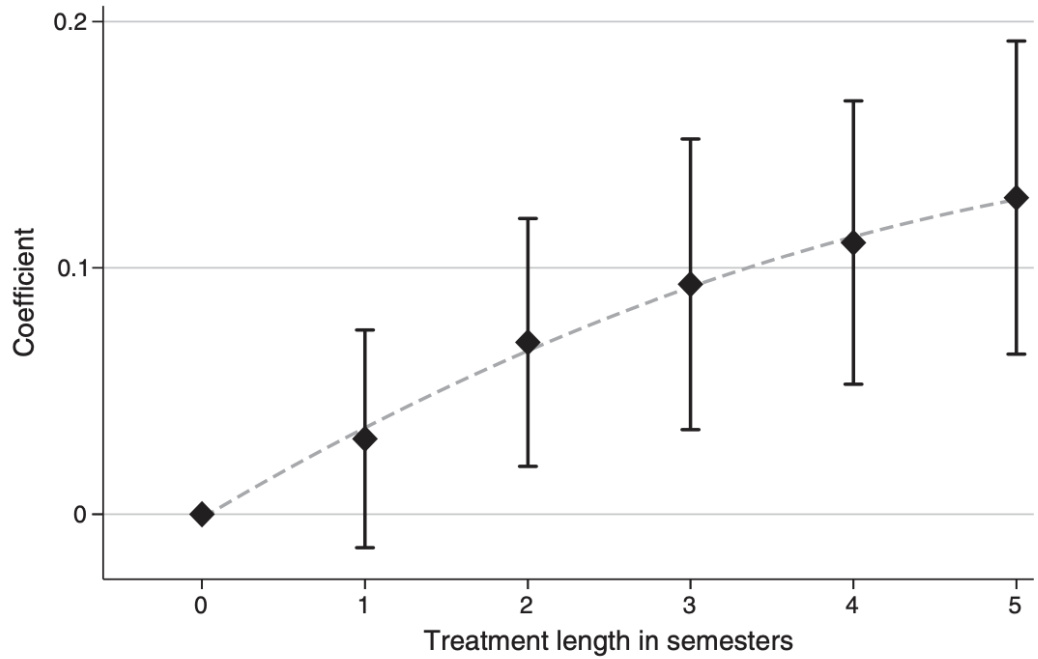


FIGURE 4. EFFECT ON POOR MENTAL HEALTH BY LENGTH OF EXPOSURE TO FACEBOOK

Notes: This figure explores the effects of length of exposure to Facebook on our index of poor mental health by presenting estimates of equation (4). The index is standardized so that, in the preperiod, it has a mean of zero and a standard deviation of one. The dashed curve is the quadratic curve of best fit. Our controls consist of age, age squared, gender, indicators for year in school (freshman, sophomore, junior, senior), indicators for race (White, Black, Hispanic, Asian, Indian, and other), and an indicator for international student. Students who entered college in 2006 might have been exposed to Facebook already in high school, because, starting in September 2005, college students with Facebook access could invite high school students to join the platform. Such students are excluded from the regression. For a detailed description of the outcome, treatment, and control variables, see online Appendix Table A.31. The bars represent 95 percent confidence intervals. Standard errors are clustered at the college level.

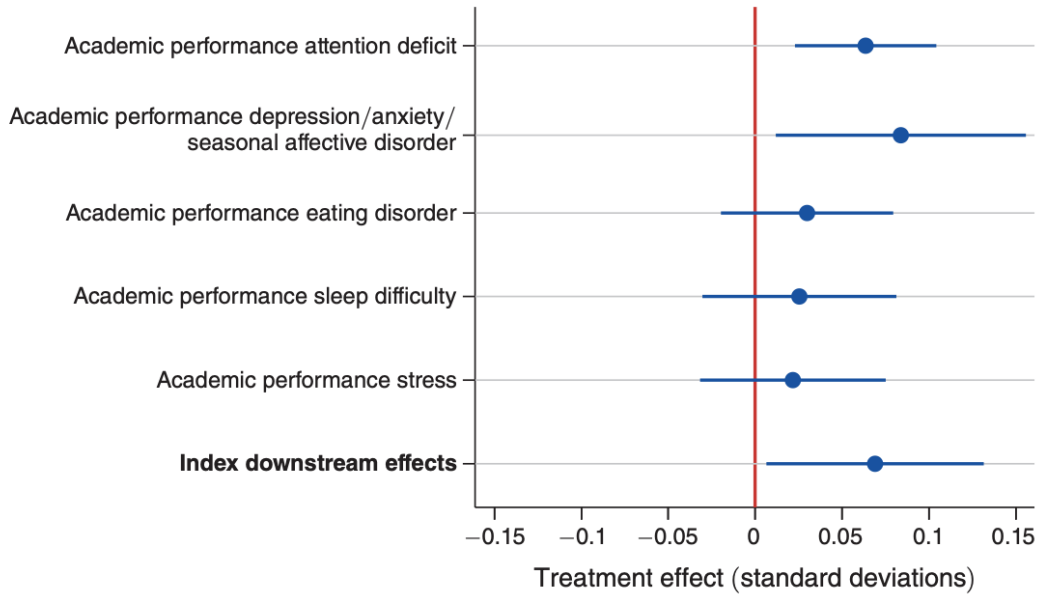


FIGURE 5. DOWNSTREAM EFFECTS ON ACADEMIC PERFORMANCE

Notes: This figure explores downstream effects of the introduction of Facebook on the students' academic performance. It presents estimates of coefficient β from equation (1) using our preferred specification, including survey-wave fixed effects, college fixed effects, and controls. The outcome variables are answers to questions inquiring as to whether various mental health conditions affected the students' academic performance and our index of downstream effects. All outcomes are standardized so that, in the preperiod, they have a mean of zero and a standard deviation of one. For a detailed description of the outcome, treatment, and control variables, see online Appendix Table A.31. The bars represent 95 percent confidence intervals. Standard errors are clustered at the college level.

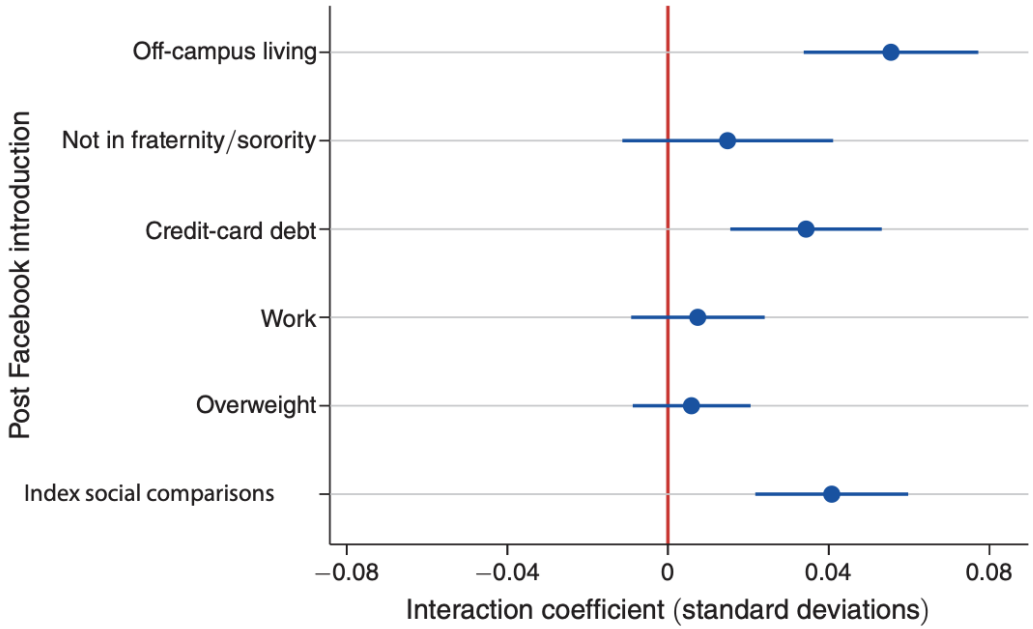


FIGURE 6. HETEROGENEOUS EFFECTS AS EVIDENCE OF UNFAVORABLE SOCIAL COMPARISONS

Notes: This figure explores the mechanisms behind the effects of Facebook on mental health. It presents estimates from a version of equation (1) in which our treatment indicator is interacted with a set of indicators for belonging to a certain subpopulation of students. The outcome variable is our overall index of poor mental health. The estimates are obtained using our preferred specification, namely the one including survey-wave fixed effects, college fixed effects, and controls. For a detailed description of the outcome, treatment, interaction, and control variables, see online Appendix Table A.31. The bars represent 95 percent confidence intervals. Standard errors are clustered at the college level.

Comments

- Can’t lose sight of the big picture – you still have to write a great paper, not just pass your exams, and going from estimation to publishing is a different but related skill
- Some of the old exhibits may not carry forward (TWFE with many columns)
- Increasingly, people are presenting a single event study graph with “all the DiD” against TWFE so as to avoid cherry picking estimators
- You should use the tool for the job, but these differences are subtle (“which parallel trends?”, “which comparison group?”)

Conclusion

- Good question, good data, and you can publish well with DiD
- Hardly definitive, but the staggered design is a solution to our inability to run the RCT
- Remember – many questions can be randomized in theory but not practice (e.g., smoking)
- Learn as much as you can and don’t stop learning