Causal Inference II

Mixtape Session

Roadmap

Welcome to CodeChella

Diff-in-Diff Core
- Origins of diff-in-diff
- Potential outcomes
- Identification, Estimation and Inference

Parallel Trends Violations
- Results versus Evidence
- Compositional Changes and Cross Sections
- Event Studies
- Triple differences
- Falsifications

Introductions

- Thank you coming to Mixtape Sessions and CUNEF first annual CodeChella Madrid workshop
- Organizers and Speakers are:
  - → Scott Cunningham, Baylor University
  - → Kyle Butts, University of Arkansas
  - → Dan Rees, Universidad Carlos III de Madrid, IZA, NBER
  - → Mark Anderson, Montana State University, IZA, NBER
  - → Agustin Casa, CUNEF
- Thank you to beautiful Madrid, CUNEF, UC3M and all of you!

Our schedule

Coffee and Food

Each morning of the workshop at 10:30am, fresh coffee and pastries will be provided outside the auditorium. Lunch will take place from 1 — 2:30pm each day. You grab a bite either at the campus cafeteria or head to the surrounding area. On Wednesday, food is generously being provided by CUNEF Universidad for us. A lot of restaurants are to the south on Av. de la Reina Victoria and to the East.

Schedule

This is our tentative schedule. Note that on Wednesday, May 29th, we will be going to a different lecture hall. The reason is that on this day, lunch will be provided for attendees by CUNEF Universidad.

	Day 1 Monday, May 27 CUNEF Auditorium	**Day 2** Tuesday, May 28 CUNEF Auditorium	**Day 3** Wednesday, May 29 **Aula Magna**	**Day 4** Thursday, May 30 CUNEF Auditorium
9 — 1pm	*Scott Cunningham* Core DID	*Scott Cunningham* Covariates	*Scott Cunningham* Callaway and Sant'anna & Sun and Abraham	*Kyle Butts* Imputation DID & Synth
1 — 2:30pm	Lunch	Lunch	*CUNEF* Provides Lunch	Lunch
2:30 — 3:30pm	*Scott Cunningham* Core DID	*Scott Cunningham* Bacon Decomposition & Callaway and Sant'anna	*Scott Cunningham* Callaway and Sant'anna & Sun and Abraham	*Kyle Butts* Imputation DID & Synth
3:30 — 5pm	Coding Lab	Coding Lab	*Dan Rees & Mark Anderson* Doing Applied Research Workshop	*Dan Rees & Mark Anderson* Doing Applied Research Workshop

What my pedagogy is like

- High energy, eclectic approach to teaching
- Move between the econometrics, history of thought, videos, applications, code, spreadsheets, exercises
- Workshop is intended to take someone from knowing nothing about difference-in-differences to nearly the cutting edge
- Ask questions at any point; I'll do my best to answer them

Class goals

Pedagogical goal is to break down the procedures into plain English, rebuilding it into something you can and want to use, but also:

1. **Confidence**: You will feel like you have a good enough understanding of diff-in-diff and synthetic control, both in its basics and some more contemporary issues, so that by the end of the week it a very intuitive, friendly, and useful tool
2. **Comprehension**: You will have learned a lot both conceptually and in the specifics, particularly with regards to issues around identification and estimation in the diff-in-diff and synth context
3. **Competency**: You will have more knowledge of programming syntax in Stata and R so that later you can apply this in your own work

Day 1 outline: the Core

The core of difference-in-differences

- Potential outcomes review and the ATT parameter
- DiD equation (“four averages and three differences”), parallel trends and estimation with OLS specification and inference
- Simple exercises illustrating potential outcomes, parameters, diff-in-diff, and core assumptions
- Parallel trends, pre-trends (event studies) and falsifications

Day 2 outline: Covariates

Violations of parallel trends that can be fixed with covariates

- Incorporating covariates
  - → Outcome regressions: Heckman, Ichimura and Todd (1997)
  - → Weighting: Abadie (2005)
  - → Doubly robust: Sant'Anna and Zhao (2020)
  - → Twoway fixed effects with time varying controls
- Lalonde coding exercise

Day 3 outline: Differential Timing

Pathologies of and Solutions to Twoway Fixed Effects with Differential Timing

- Bacon decomposition (Goodman-Bacon 2021)
- Aggregate ATT(g,t) method (Callaway and Sant'Anna 2021)
- Decomposition event study leads and lags (Sun and Abraham 2021)
- Discussion of two contemporary papers using these methods and the presentation of results
- Simple illustrations in simulations
- Walk through R shiny app

Day 4 outline: Imputation DiD and Synthetic Control

Imputation estimators

- Imputation estimators
  - → Borusyak, et al. (2023) “robust efficient imputation estimator”
  - → Gardner, et al. (2023) two stage difference in differences
- Synthetic control methods
- Continuous treatment difference-in-differences if there is time (Callaway, Goodman-Bacon and Sant’Anna 2024)

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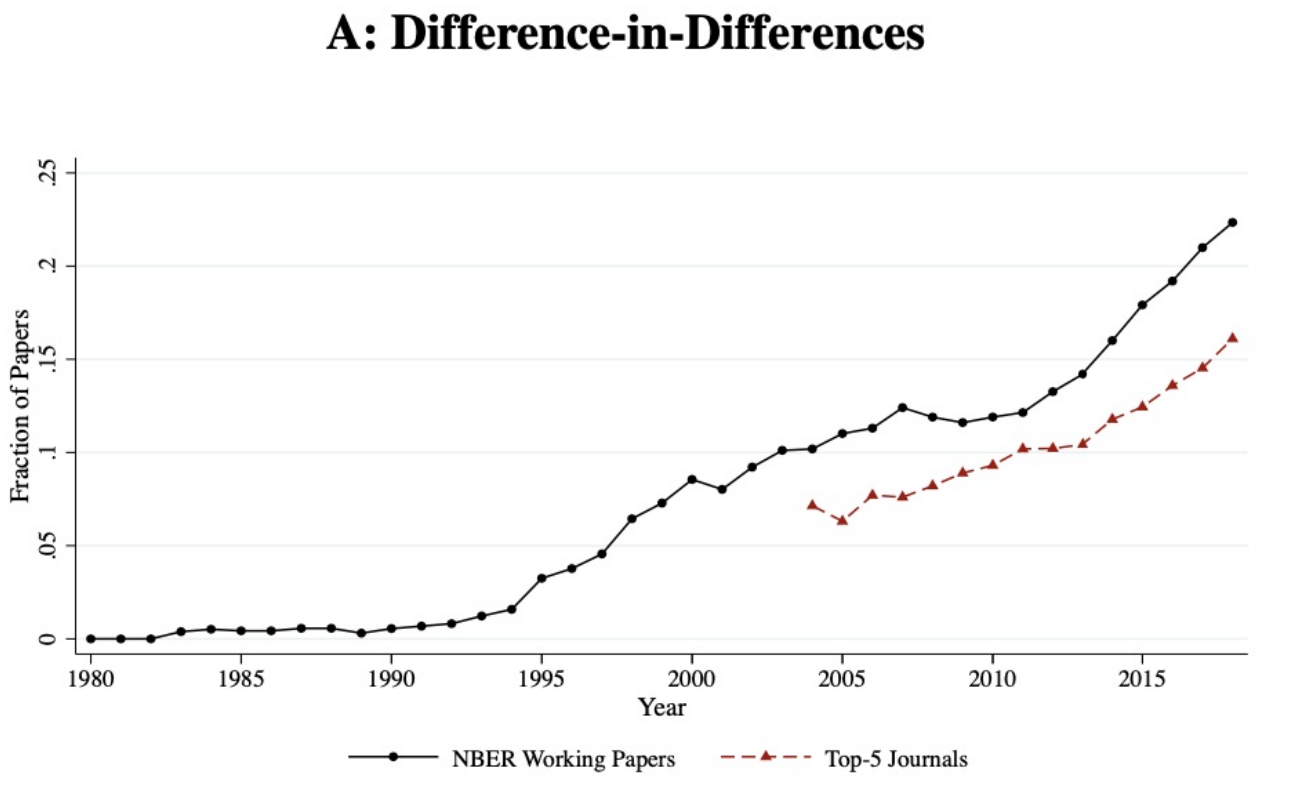
Growing popularity in economics

Figure: Currie, et al. (2020)



Origins of diff-in-diff)

- Its modern usage originates with Orley Ashenfelter and David Card in late 1970s and early 1980s work on job training programs
- The phrase “difference-in-differences” is coined in a 1985 article by Ashenfelter and Card, but it was used before then
- It seems to be the first design – it predates the RCT by over 80 years in fact
- It was also used in two famous public health debates in Vienna and London in the early to mid 19th century

What is difference-in-differences (DiD)

- DiD is when a group of units are assigned some treatment and then compared to a group of units that weren't before and after
- One of the most widely used quasi-experimental methods in economics and increasingly in industry
- Predates the randomized experiment by 80 years, but uses basic experimental ideas about treatment and control groups (just not randomized)
- Uses panel or repeated cross section datasets, binary treatments usually, and often covariates
- Does not require a linear regression model, but for reasons we will see, linear regression accommodates it (just not all specifications equally)

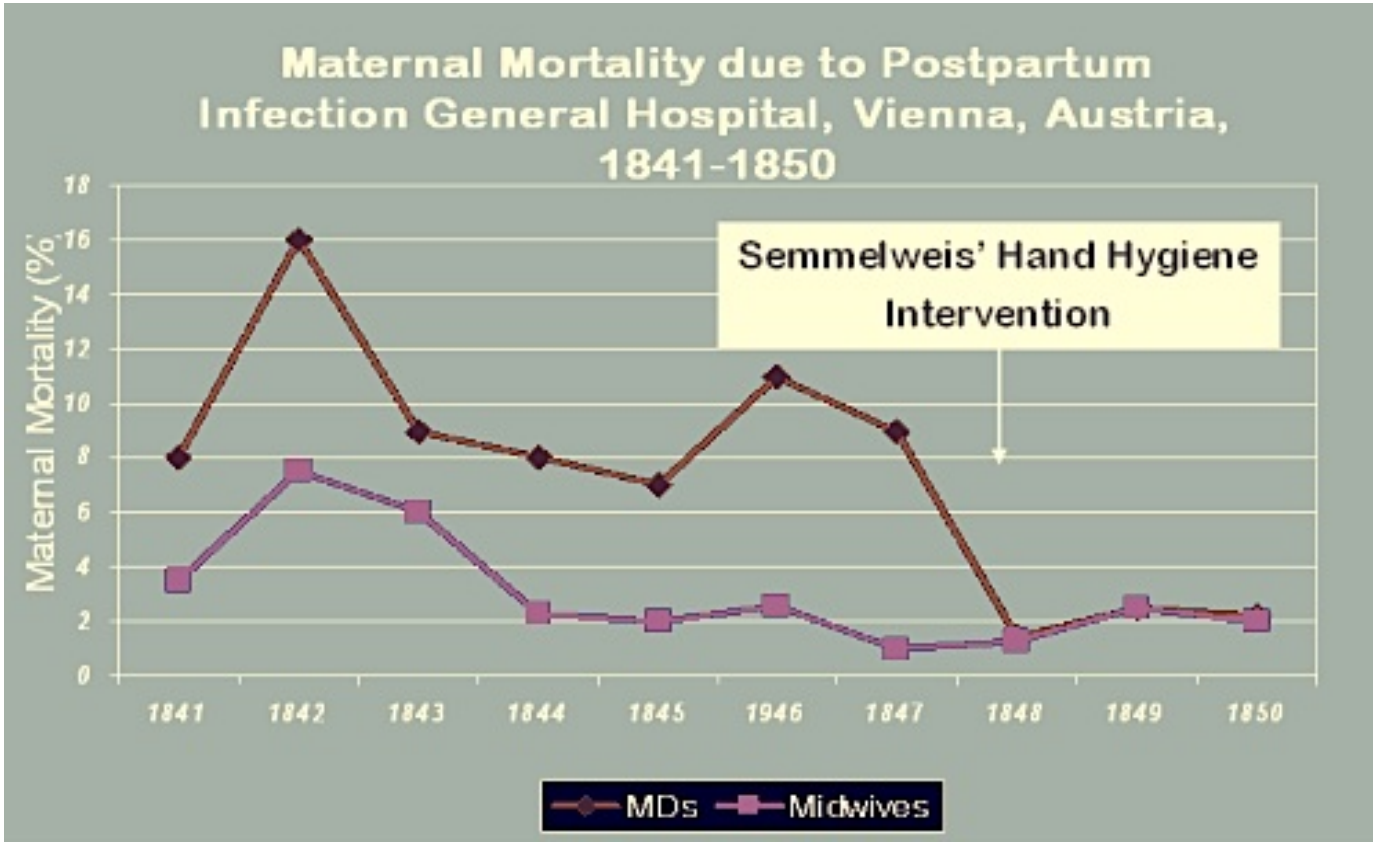
Ignaz Semmelweis and washing hands

- Early 1820s, Vienna passed legislation requiring that if a pregnant women giving birth went to a public hospital (free care), then depending on the day of week and time of day, she would be routed to either the midwife wing or the physician wing (most likely resulting in random assignment)
- But by the 1840s, Ignaz Semmelweis noticed that pregnant women died after delivery in the (male) wing at a rate of 13-18%, but only 3% in the (female) midwife wing – cause was puerperal or “childbed” fever
- Somehow this was also we known – women would give birth in the street rather than go to the physician if they were unlucky enough to have their water break on the wrong day and time

Ignaz Semmelweis and washing hands

- Ignaz Semmelweis conjectures after a lot of observation that the cause is the teaching faculty teaching anatomy using cadavers and then delivering babies *without washing hands*
- New training happens to one but not the other and Semmelweis thinks the mortality is caused by working with cadavers
- Convinced the hospital to have physicians wash their hands in chlorine but not the midwives, creating a type of difference-in-differences design

Semmelweis diff-in-diff evidence



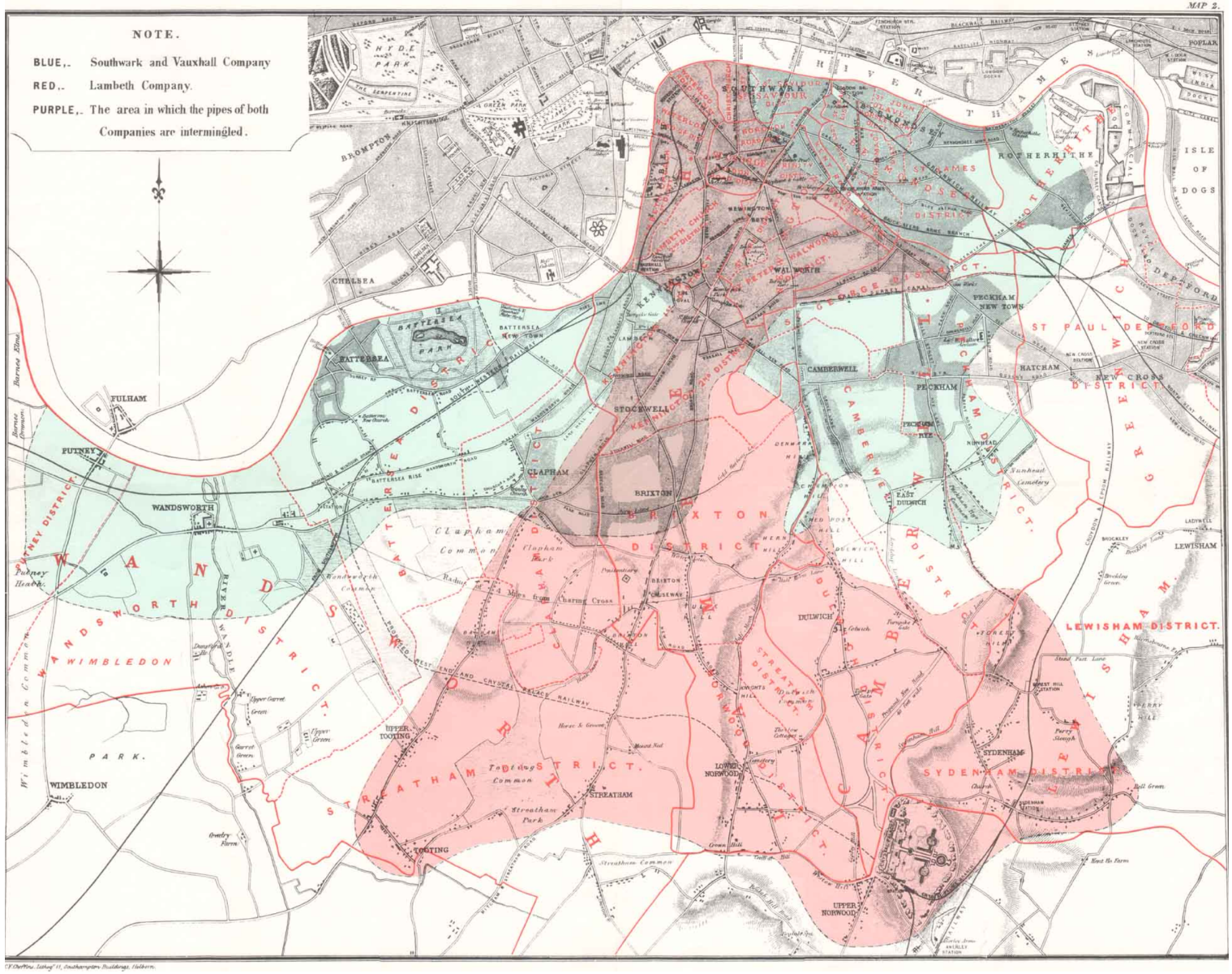
Evidence Rejected

- Diff-in-diff evidence was rejected by Semmelweis' superiors claiming it was the hospital's new ventilation system
- Dominant theory of disease spread was caused by "odors" or miasma or "humors"
- Semmelweis began showing signs of irritability, perhaps onset of dementia, became publicly abusive, was committed to a mental hospital and within two weeks died from wounds he received while in residence
- Despite the strength of evidence, difference-in-differences was rejected – a theme we will see continue
- Let's look at an illustration using a table and another story

John Snow and cholera

- Three major waves of cholera in the early to mid 1800s in London, largely thought to be spread by miasma (“dirty air”)
- John Snow believed cholera was spread through the Thames water supply through an invisible creature that entered the body through food and drink, caused the body to expel water, placing the creature back in the Thames and causing epidemic waves
- London passes ordinance requiring water utility companies to move inlet pipe further up the Thames, above the city center, but not everyone complies
- Natural experiment: Lambeth water company moves its pipe between 1849 and 1854; Southwark and Vauxhall water company delayed

Figure: Two water utility companies in London 1854



Difference-in-differences

Table: Lambeth and Southwark and Vauxhall, 1849 and 1854

Companies	**Time**	**Outcome**	D_1	D_2
Lambeth	Before	$Y = L$		
	After	$Y = L + L_t + D$	$L_t + D$	
				$D + (L_t - SV_t)$
Southwark and Vauxhall	Before	$Y = SV$		
	After	$Y = SV + SV_t$	SV_t	

$$\widehat{\delta}_{did} = D + (L_t - SV_t)$$

This method yields an unbiased estimate of D if $L_t = SV_t$, but note that L_t is a counterfactual trend and therefore not known

Traditional econometric steps to any research project

1. Convert research question into causal parameter
2. Deduce beliefs needed to estimate that causal parameter with data
3. Create a calculator that will use data and estimate the causal parameter

But many skip (1) and maybe even (2) and instead simply “run regressions” and cross our fingers that that coefficient is causal, but is it? And why is it? And what is it?

OLS Measures Four Averages and Three Subtractions

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

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

- Orley claims that the OLS estimator of δ and the "four averages and three subtractions" calculation are numerically identical
  https://youtu.be/WnB3EJ8K7lg?t=126
- Review these two calculations using `equivalence.do` in Stata to illustrate the point

Introducing Potential Outcomes to DiD

- We want to know when does the DiD equation identify a causal parameter and which one (there are several)?
- We need causality concepts that can be linked to DiD if we are to answer this question
- Potential outcomes notation is the main language of modern causal inference and is rooted in the early experimental design writers like Ronald Fisher and Jerzey Neyman, as well as modern statisticians like Don Rubin

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: No ones the counterfactual (no matter how confident they are)

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:

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

It's the average causal effect but only for the people exposed to some intervention; notice we can't calculate it, also, because we are missing the red term

Potential outcomes vs realized data

- Potential outcomes are *a priori* real but unknown descriptions of states of the world under different treatment exposures
- Realized data are selected from the potential outcomes based on one's treatment assignment which we can show using the switching equation

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

- How those treatment get assigned is called the treatment assignment mechanism

Simple spreadsheet exercise

Let’s review basic concepts here at ”WEIGHTS” tab:

https://docs.google.com/spreadsheets/d/10DuQqGtH_Ewea7zQoLTFYHbnvqaTVDhn2GDzq3Oa6EQ/edit?usp=sharing

DiD equation is the 2x2

Orley's "four averages and three subtractions" uses two groups, two time periods, or 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.

When will $\widehat{\delta}$ equal the ATT? When will it not?

Replace with potential outcomes and add a zero

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

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

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"

What is and is not parallel trends?

- Parallel trends does *not* mean treatments were randomly assigned (though random assignment guarantees parallel trends)
- Parallel trends does *not* require that the groups be similar at baseline on outcomes (though random assignment guarantees that would be)
- Parallel trends *does* require that the comparison group follows a trend in outcomes that is approximately the same as the counterfactual trend of the treatment group (what would have had happened had the treatment not occurred)

Homework #1

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

What if the U group had always been treated in both periods? Is parallel trends enough to identify the ATT?

Replace realized outcomes with potential outcomes and rewrite using the “add zero” trick we did. I’ll review the answer tomorrow.

Understanding parallel trends through worksheets

Before we move into regression, let's go through a simple exercise to really pin down these core ideas with simple calculations

https://docs.google.com/spreadsheets/d/1onabpc14JdrGo6NFv0zCWo-nuWDLLV2L1qNogDT9SBw/edit?usp=sharing

Three main DiD assumptions

- Parallel trends is the most common one and most well known
- But parallel trends is nested within a bundle of assumptions, and all of them are need for traditional difference-in-differences
- Other two lesser known assumptions are ”No anticipation” (or NA) and Stable Unit Treatment Value Assumption (SUTVA)

No Anticipation

- "No anticipation" simply means that the unit is not treated until it is treated (and that can be violated with rational forward looking agents but not always)
  - → **Example 1**: Tomorrow I win the lottery, but don't get paid yet. I decide to buy a new house today. That violates NA
  - → **Example 2**: Next year, a state lets you drive without a driver license and you know it. But you can't drive without a driver license today. This satisfies NA.
- Violations in simple 2x2 where baseline is treated creates problems that I discuss here:

https://causalinf.substack.com/p/difference-in-differences-no-anticipation

https://causalinf.substack.com/p/mixtape-mailbag-7-what-happens-in

Second homework assignment

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

What if the k group had been treated at the baseline "pre" period in our 2x2?

Replace realized outcomes with potential outcomes and rewrite using the "add zero" trick we did. I'll review the answer tomorrow, but let's skip to the result here

No Anticipation Violation

If the baseline period is treated, then the simple 2x2 identifies the following three terms:

$$\begin{aligned} \delta &= ATT_k(Post) \\ &\quad + \text{Non PT bias} \\ &\quad - ATT_k(Pre) \end{aligned}$$

First row is the ATT in the post period; middle row is parallel trends; third row subtracts the baseline ATT from the calculation. If treatment effects are constant, then the DiD coefficient will be zero despite positive treatment effects. Let's look in `na.do`.

SUTVA

- Stable Unit Treatment Value Assumption (Imbens and Rubin 2015) focuses on what happens when in our analysis we are combining units (versus defining treatment effects)
    1. **No Interference**: a treated unit cannot impact a control unit such that their potential outcomes change (unstable treatment value)
    2. **No hidden variation in treatment**: When units are indexed to receive a treatment, their dose is the same as someone else with that same index
    3. **Scale**: If scaling causes interference or changes inputs in production process, then #1 or #2 are violated
- Shifts from defining treatment effects to estimating them, which means being careful about who is the control group, how you define treatments and what questions can and cannot be answered with this method

Setup for SUTVA

$$
\begin{aligned}
\widehat{\delta} &= \underbrace{\Big(E[Y_k|Post] - E[Y_k|Pre]\Big) - \Big(E[Y_U|Post] - E[Y_U|Pre]\Big)}_{\text{Diff-in-diff}} \\
&= \underbrace{\Big(E[Y_k^1|Post] - E[Y_k^0|Pre]\Big) - \Big(E[\color{blue}{Y_U^0}|Post] - E[Y_U^0|Pre]\Big)}_{\text{Switching equation with SUTVA in blue}} \\
&\quad + \underbrace{\color{red}{E[Y_k^0|Post] - E[Y_k^0|Post]}}_{\text{Adding zero}}
\end{aligned}
$$

Illustrate the role of SUTVA

Rearrange

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

Notice how NA and SUTVA are key for making DiD be this simple expression.

Summarizing

- Lots of restrictions placed on difference-in-differences
  - → NA: you chose a baseline that is not treated
  - → SUTVA: your comparison group is never treated during the course of the calculations
  - → PT: your comparison group has a trend in $E[Y^0]$ that is the same as the counterfactual
- Only when you have NA and SUTVA does DiD equal ATT + PT
- But it's crucial to remember: DiD and ATT are not the same thing

OLS Specification

- Simple DiD equation will identify ATT under parallel trends
- But so will a particular OLS specification (two groups and no covariates)
- OLS was historically preferred because
  - → OLS estimates the ATT under parallel trends
  - → Easy to calculate the standard errors
  - → Easy to include multiple periods
- People liked it also because of differential timing, continuous treatments and covariates, but those are more complex so we address them later

Minimum wages

- Card and Krueger (1994) have a famous study estimating causal effect of minimum wages on employment
- New Jersey raises its minimum wage in April 1992 (between February and November) but neighboring Pennsylvania does not
- Using DiD, they do not find a negative effect of the minimum wage on employment leading to complex reactions from economists
- Orley's describes his understanding of people's reaction to the paper.
  https://youtu.be/MOtbuRX4eyQ?t=1882

Binyamin Appelbaum
@BCAppelbaum

Replying to @BCAppelbaum

The Nobel laureate James Buchanan wrote in the Wall Street Journal that Card and Krueger were undermining the credibility of economics as a discipline. He called them and their allies "a bevy of camp-following whores."

3:49 PM · Mar 18, 2019

179 Reply Share

Read 18 replies

Reaction to the paper

Lots of anecdotes in this interview with Card, but here are just two. First, Card and Krueger received a lot of personal hostility from their peers (1:07 to 1:10)

https://youtu.be/1soLdywFb_Q?si=laAVYf_E2KBZKywG&t=4020

Later Card says Sherwin Rosen accused them of having an agenda. But the worst is what happens to Alan Krueger maybe (1:16 to 1:17)

https://youtu.be/1soLdywFb_Q?si=jsb8h50ZosGDnKrv&t=4556

Card on that study

“I’ve subsequently stayed away from the minimum wage literature for a number of reasons. First, it cost me a lot of friends. People that I had known for many years, for instance, some of the ones I met at my first job at the University of Chicago, became very angry or disappointed. They thought that in publishing our work we were being traitors to the cause of economics as a whole.”

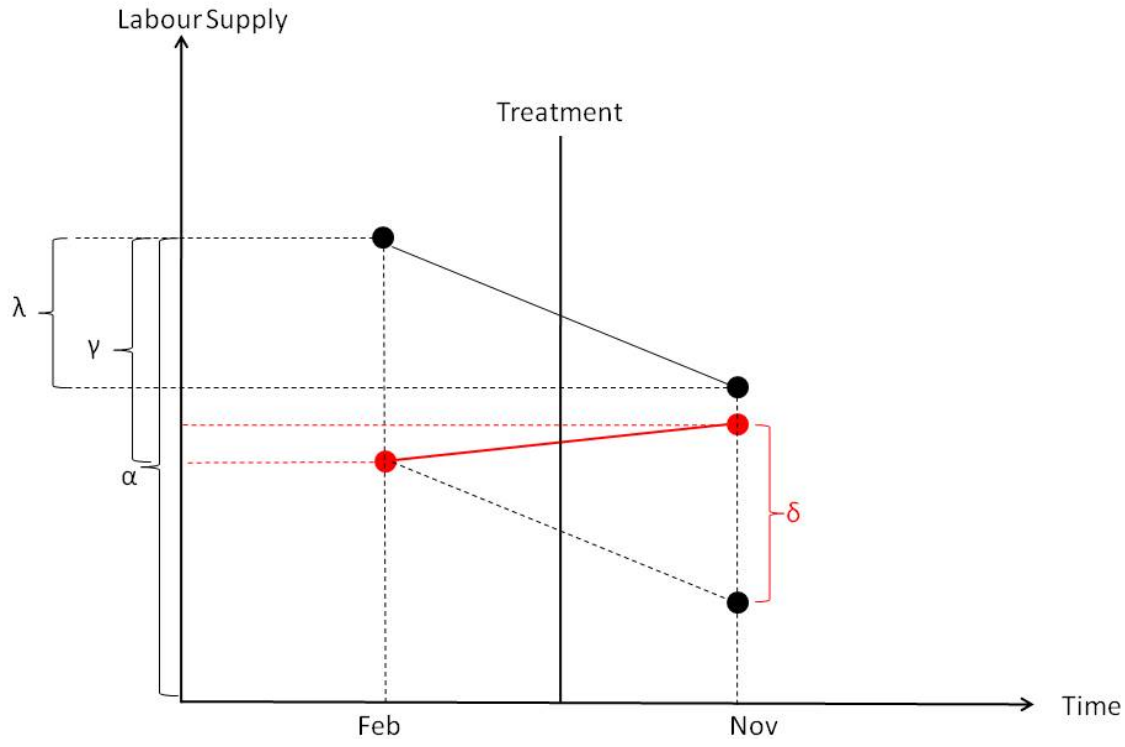
OLS specification of the DiD equation

- The correctly specified OLS regression is an interaction with time and group fixed effects:

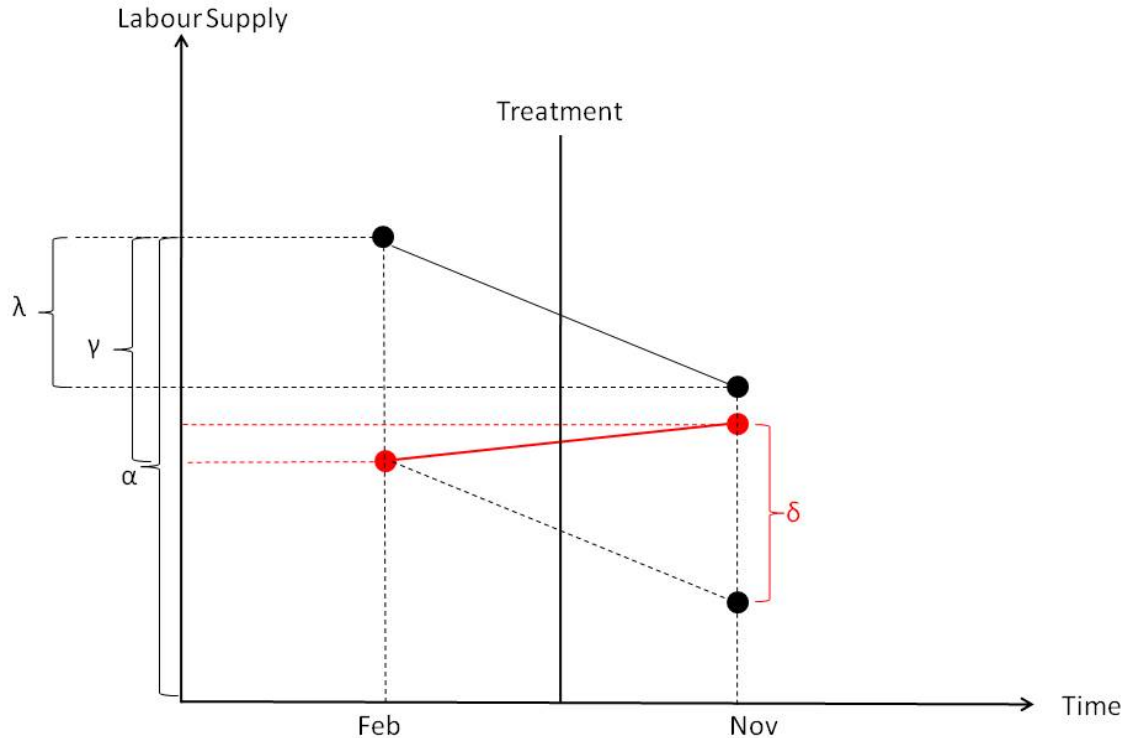
$$Y_{its} = \alpha + \gamma NJ_s + \lambda d_t + \delta(NJ \times d)_{st} + \varepsilon_{its}$$

  - → NJ is a dummy equal to 1 if the observation is from NJ
  - → d is a dummy equal to 1 if the observation is from November (the post period)
- This equation takes the following values
  - → PA Pre: α
  - → PA Post: $\alpha + \lambda$
  - → NJ Pre: $\alpha + \gamma$
  - → NJ Post: $\alpha + \gamma + \lambda + \delta$
- DiD equation: (NJ Post - NJ Pre) - (PA Post - PA Pre) $= \delta$

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



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



Notice how OLS is "imputing" $E[Y^0|D=1, Post]$ for the treatment group in the post period? It is only "correct", though, if parallel trends is a good approximation

Inference in DID

When dealing with clustered data, a crucial concept is the difference between correlated observations and correlated errors. While they may seem similar, they are distinct, and it’s essential to focus on the errors when clustering standard errors.

Correlated Observations

- Correlated observations occur when the observed variables themselves are correlated within a cluster.
- For instance, incomes within a specific region might be positively correlated.
- Correlated observations do not necessarily violate OLS assumptions.

Correlated Errors

- Correlated errors occur when the unobserved errors are correlated within a cluster.
- This violates the assumption of independent errors, leading to possibly biased standard errors and higher over rejection rates
- Failing to account for correlated errors can lead to misleading inference.

Serial correlation creates problems

- Bertrand, Duflo and Mullainathan (2004) show that conventional standard errors will often severely understate the standard deviation of the estimators
- They proposed three solutions, but most only use one of them (clustering)
- Clustering standard errors accounts for this within-cluster correlation and is a more conservative approach
- Clustering is typically recommended at the aggregate unit where the entire treatment occurred

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Court metaphor

- Think a prosecutor arguing against a defense attorney to convince a judge and jury
- The claim the defendant is guilty but the claim is not the evidence – it's more like an assertion
- The evidence is the smoking gun, the fingerprints, the eye witnesses, the footprints in the mud outside the house
- If your claim is supported by weak evidence, then no one *should* convict – it would be borderline corruption if they did

Evidence versus the Main Result

- Causal inference is about *warranted beliefs* – should you or should you not believe the *causal claim*?
- Your DiD *results* are like the claim of guilt, but your DiD results are *not* the smoking gun
- You need to provide evidence for parallel trends against several well known vulnerabilities
- Evidence will be bite, falsifications, mechanisms and event study data visualization
- We will mix the parallel trends violations with the evidence concept before getting into advanced estimators

Three classic parallel trends violations

1. Compositional change with repeated cross-sections
2. Policy endogeneity
3. Covariates

Repeated cross-sections and compositional change

- One of the risks of a repeated cross-section is that the composition of the sample may have changed between the pre and post period in ways that are correlated with treatment
- Hong (2013) uses repeated cross-sectional data from the Consumer Expenditure Survey (CEX) containing music expenditure and internet use for a random sample of households
- Study exploits the emergence of Napster (first file sharing software widely used by Internet users) in June 1999 as a natural experiment
- Study compares internet users and internet non-users before and after emergence of Napster

Introduction of Napster and spending on music

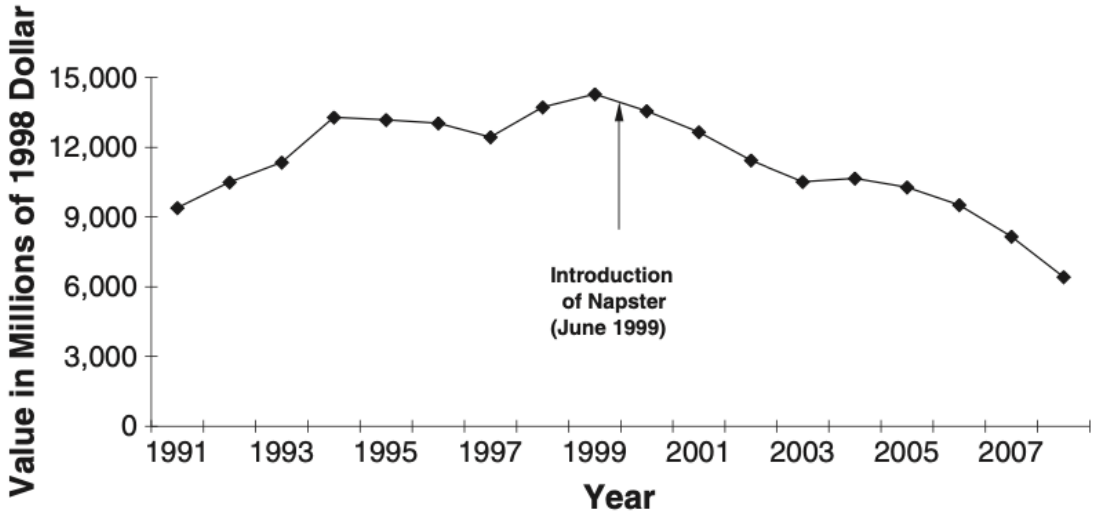


Figure 2. Total real value of record shipments in the USA. Refer to the RIAA's year-end statistics. Total sales include CDs, cassettes, LPs, and music videos. Starting from 2004, total sales also include digital formats such as legitimate download

Figure 1: Internet Diffusion and Average Quarterly Music Expenditure in the CEX

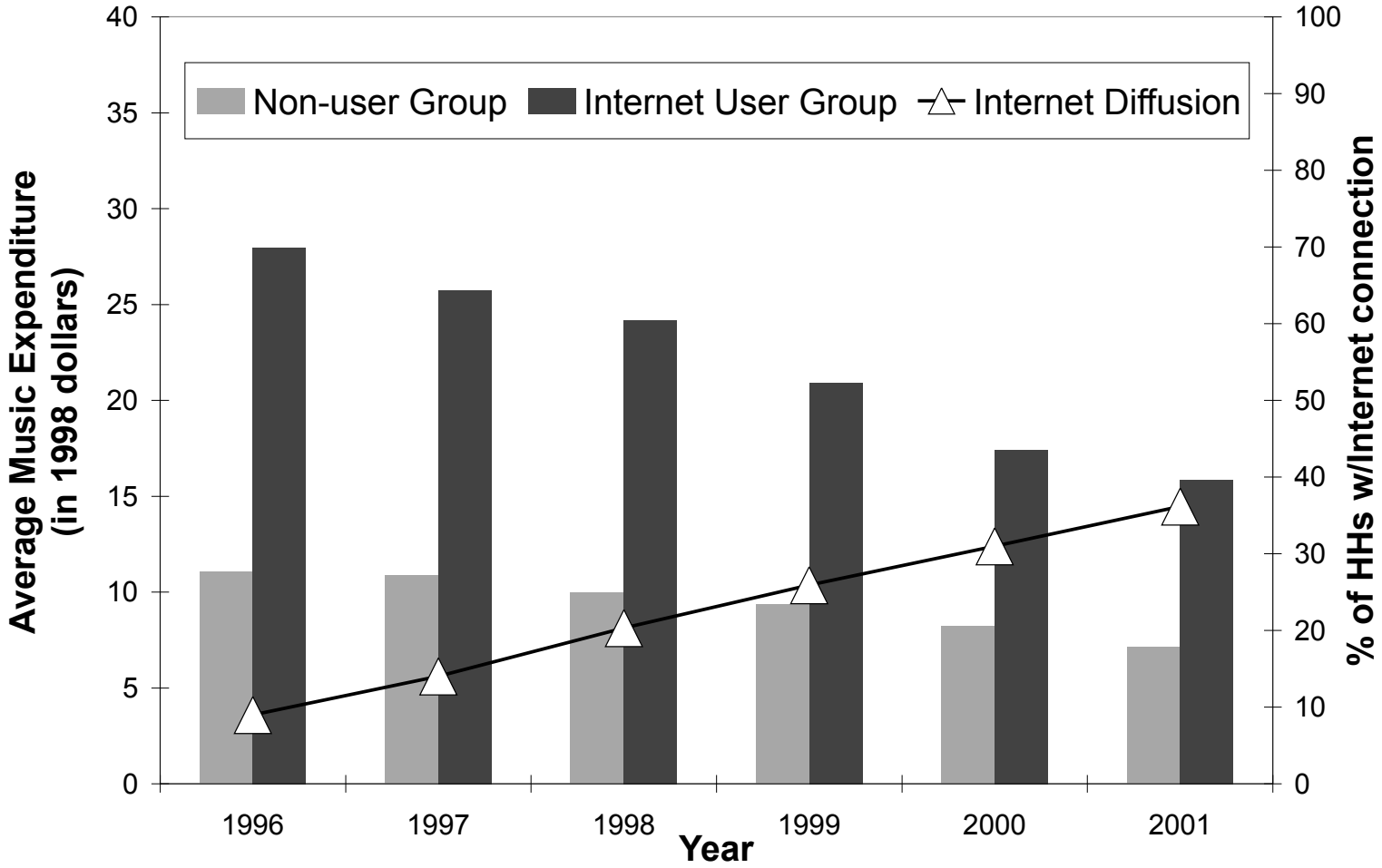


Table 1: Descriptive Statistics for Internet User and Non-user Groups[a]

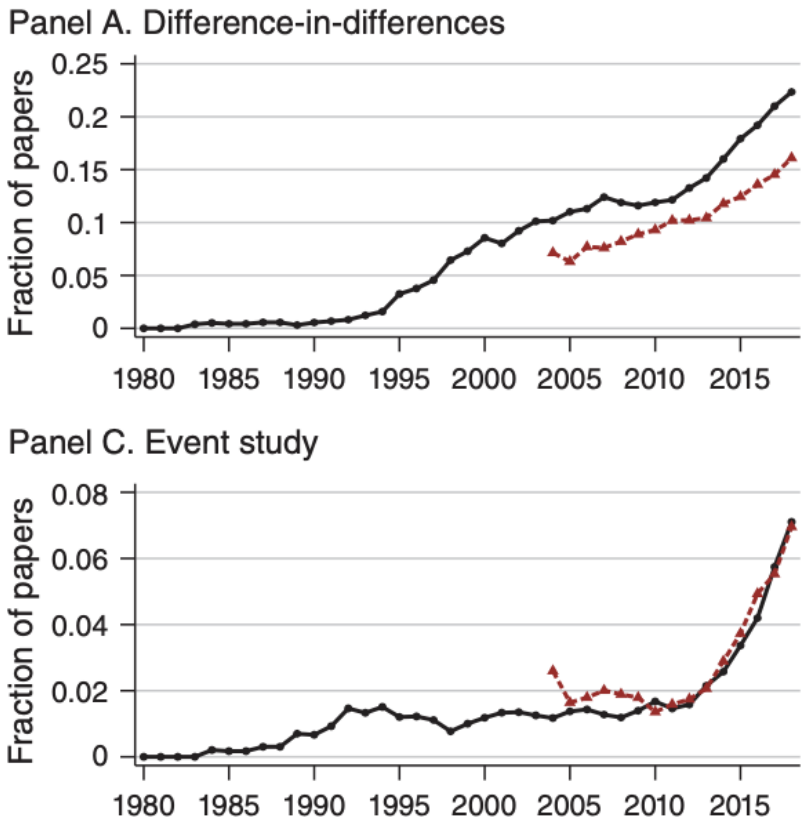
Year	1997		1998		1999	
	Internet User	Non-user	Internet User	Non-user	Internet User	Non-user
Average Expenditure						
Recorded Music	$25.73	$10.90	$24.18	$9.97	$20.92	$9.37
Entertainment	$195.03	$96.71	$193.38	$84.92	$182.42	$80.19
Zero Expenditure						
Recorded Music	.56	.79	.60	.80	.64	.81
Entertainment	.08	.32	.09	.35	.14	.39
Demographics						
Age	40.2	49.0	42.3	49.0	44.1	49.4
Income	$52,887	$30,459	$51,995	$28,169	$49,970	$26,649
High School Grad.	.18	.31	.17	.32	.21	.32
Some College	.37	.28	.35	.27	.34	.27
College Grad.	.43	.21	.45	.21	.42	.20
Manager	.16	.08	.16	.08	.14	.08

Diffusion of the Internet changes samples (e.g., younger music fans are early adopters)

Repeated cross-sections

- Surprisingly underappreciated problem with almost no literature around it
- Replace the outcome with your time-varying covariates and estimate your DiD model
- Use covariates highly predictive of the missing $E[Y^0|D=1]$ for this exercise
- “Difference-in-differences with Compositional Changes” by Pedro Sant’Anna and Qi Xu is an update to Hong (2013)

Event studies have become mandatory in DiD



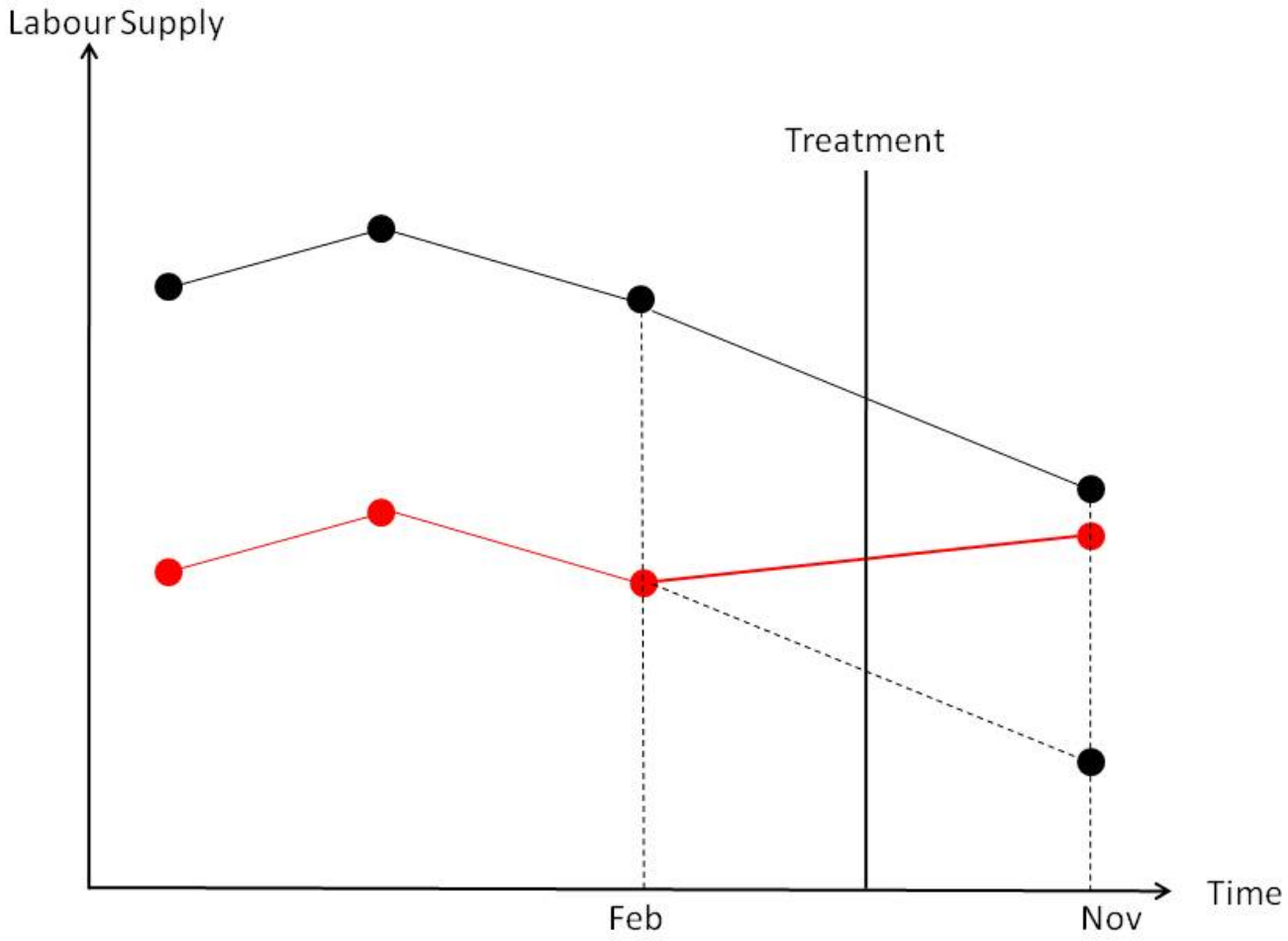
Intuition behind event studies

- We cannot directly verify parallel trends, so for a long time researchers have focused on the pre-trends (Ashenfelter's Dip)
- Parallel pre-trends not required for parallel trends and vice versa, but this is the smoking gun we typically look for nonetheless
- Think of it as a type of check for selection bias, but imperfect with false positives and false negatives
- Even if pre-trends are the same one still has to worry about other policies changing at the same time (omitted variable bias is a parallel trends violation)

Creating event studies

- Originally, there were no event studies (as we saw in the First Wave)
- Economists pulled from finance and took the event study concept and changed it to suit Ashenfelter Dip reasoning
- Always presented graphically, but there were different ways people went about it so we will review them and make suggestions

Plot the raw data when there's only two groups



Event study regression

- Alternatively, present estimated coefficients from a dynamic regression specification:

$$Y_{its} = \alpha + \sum_{\tau=-2}^{-q} \mu_\tau (D_s \times \tau_t) + \sum_{\tau=0}^{m} \delta_\tau (D_s \times \tau_t) + \tau_t + D_s + \varepsilon_{ist}$$

  - → With a simple 2x2, you are interacting treatment indicator with calendar year dummies
  - → Includes q leads or anticipatory effects and m lags or post treatment effects
- Estimated $\widehat{\delta}$ coefficients are estimated ATT parameters assuming parallel trends and $\widehat{\mu}$ is part of your evidence for that

Event study regression

- Typically you'll plot the coefficients and 95% CI on all leads and lags (binned or not, trimmed or not)
- Under No Anticipation, SUTVA and parallel pre-trends, then mechanically $\widehat{\mu_\tau}$ will be zero (does not guarantee the same about post-trends)
- There are still specification and power issues that Jon Roth has written about, but I will skip that
- But also under NA, SUTVA and parallel trends (post trends), then $\widehat{\delta}$ are estimates of the ATT at points in time

Normal DiD coefficient

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

But this was *post*-treatment. Still, put that aside – diff-in-diff equations *always* identify the sum of those terms, even in the pre-period

Pre-treatment DiD coefficient

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

Under NA, then the $t-1$ period is untreated. But then so are the other pre-periods so the ATT is implicitly zero and the *only* thing that you can be measuring with pre-trend DiD coefficients is differential trends.

Event study coefficients

- Remember that the OLS specification we discuss collapses to ATT plus parallel trends bias
- This is *always* true because it's an identity and holds even in the pre-period as much in the post
- It's just in the pre period, you do not have the missing $E[Y^0|D=1]$ term as no one and nothing is treated in pre-period under NA
- This means pre-period is basically an opportunity to directly verify parallel pre-trends – but it's the past's pre-trends, not the counterfactual pre-trend of the present/future
- And that's how people use the pre-period – they use the pre-period to evaluate whether they think this is a good control group

Event study example

- The notion is really simple: if PT held then, you'll argue that it's reasonable it would've still held
- But this is an assertion, and you need to build the case as we said
- At this point, it's a lot easier to show you what I'm talking about – where the art and the science meet – with a great paper

Medicaid and Affordable Care Act example

Volume 136, Issue 3
August 2021

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Medicaid and Mortality: New Evidence From Linked Survey and Administrative Data

Get access >

Sarah Miller, Norman Johnson, Laura R Wherry

The Quarterly Journal of Economics, Volume 136, Issue 3, August 2021, Pages 1783–1829,
https://doi.org/10.1093/qje/qjab004
Published: 30 January 2021

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Abstract

We use large-scale federal survey data linked to administrative death records to investigate the relationship between Medicaid enrollment and mortality. Our analysis compares changes in mortality for near-elderly adults in states with and without Affordable Care Act Medicaid expansions. We identify adults most likely to benefit using survey information on socioeconomic status, citizenship status, and public program participation. We find that prior to the ACA expansions, mortality rates across expansion and nonexpansion states trended similarly, but beginning in the first year of the policy, there were significant reductions in mortality in states that opted to expand relative to nonexpanders. Individuals in expansion states experienced a 0.132 percentage point decline in annual mortality, a 9.4% reduction over the sample mean, as a result of the Medicaid expansions. The effect is driven by a reduction in disease-related deaths and grows over time. A variety of alternative specifications, methods of inference, placebo tests, and sample definitions confirm our main result.

JEL: H75 - State and Local Government: Health; Education; Welfare; Public Pensions, I13 - Health Insurance, Public and Private, I18 - Government Policy; Regulation; Public Health

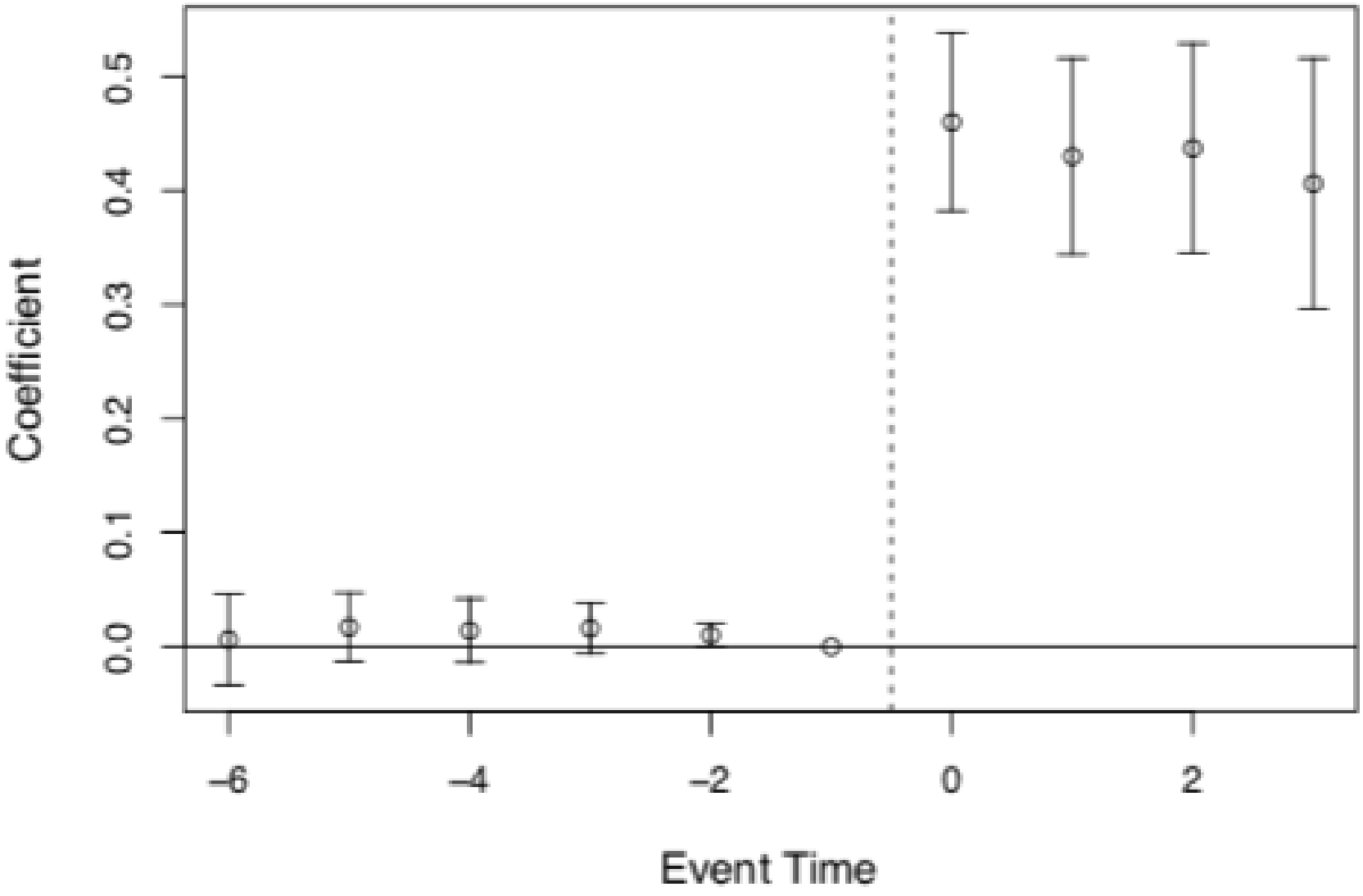
Issue Section: Article

Their Evidence versus Their Result

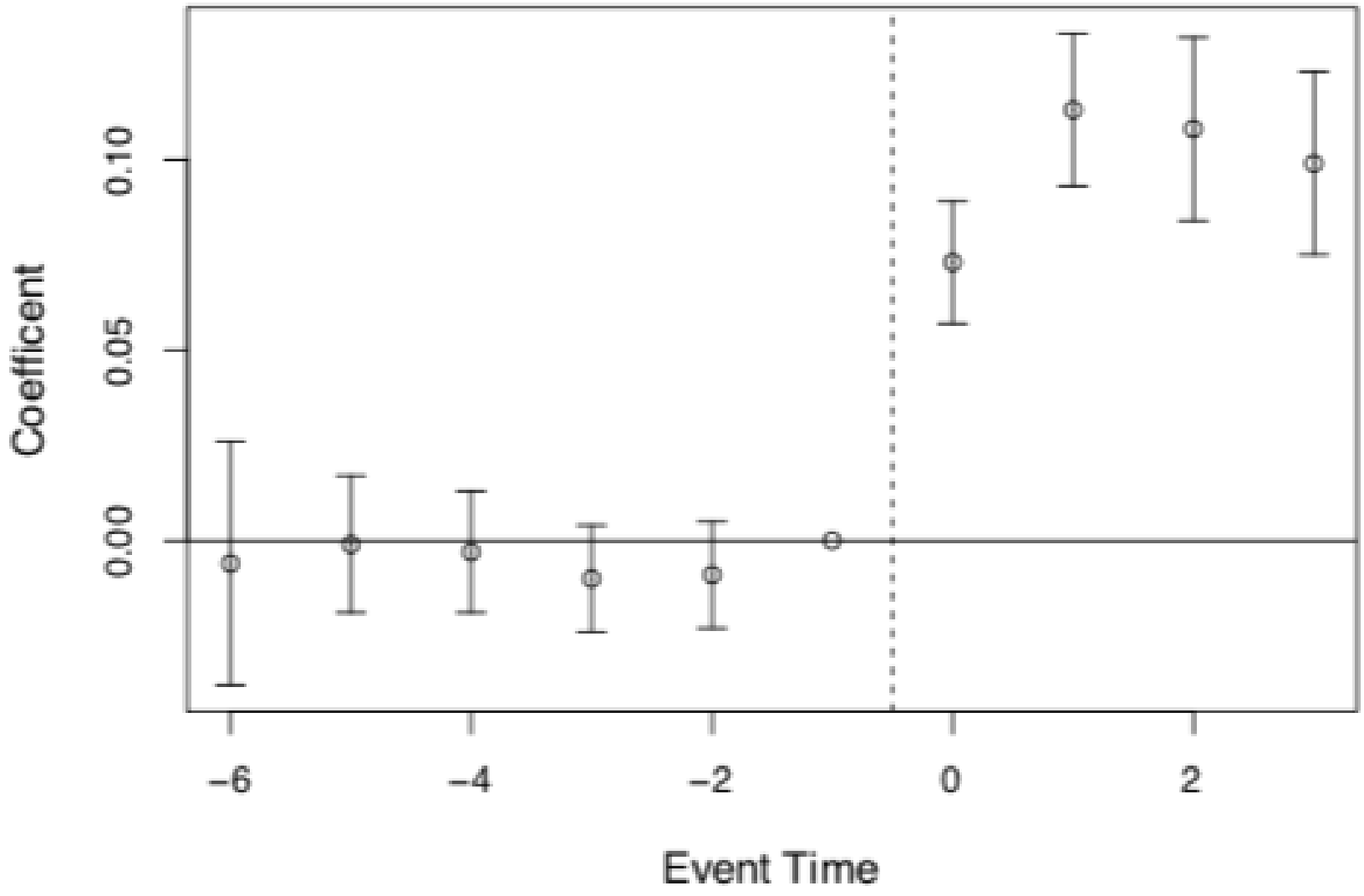
- **Bite** – they will show that the expansion shifted people into Medicaid and out of uninsured status
- **Placebos** – they show that there's no effect of Medicaid on a similar group that didn't enroll
- **Event study** – they will lean hard on those dynamic plots
- **Main results** – with all of this, they will show Medicaid expansion caused near elderly mortality to fall
- **Mechanisms** – they think they can show it's coming from people treating diseases causing mortality declines to compound over time

Bite

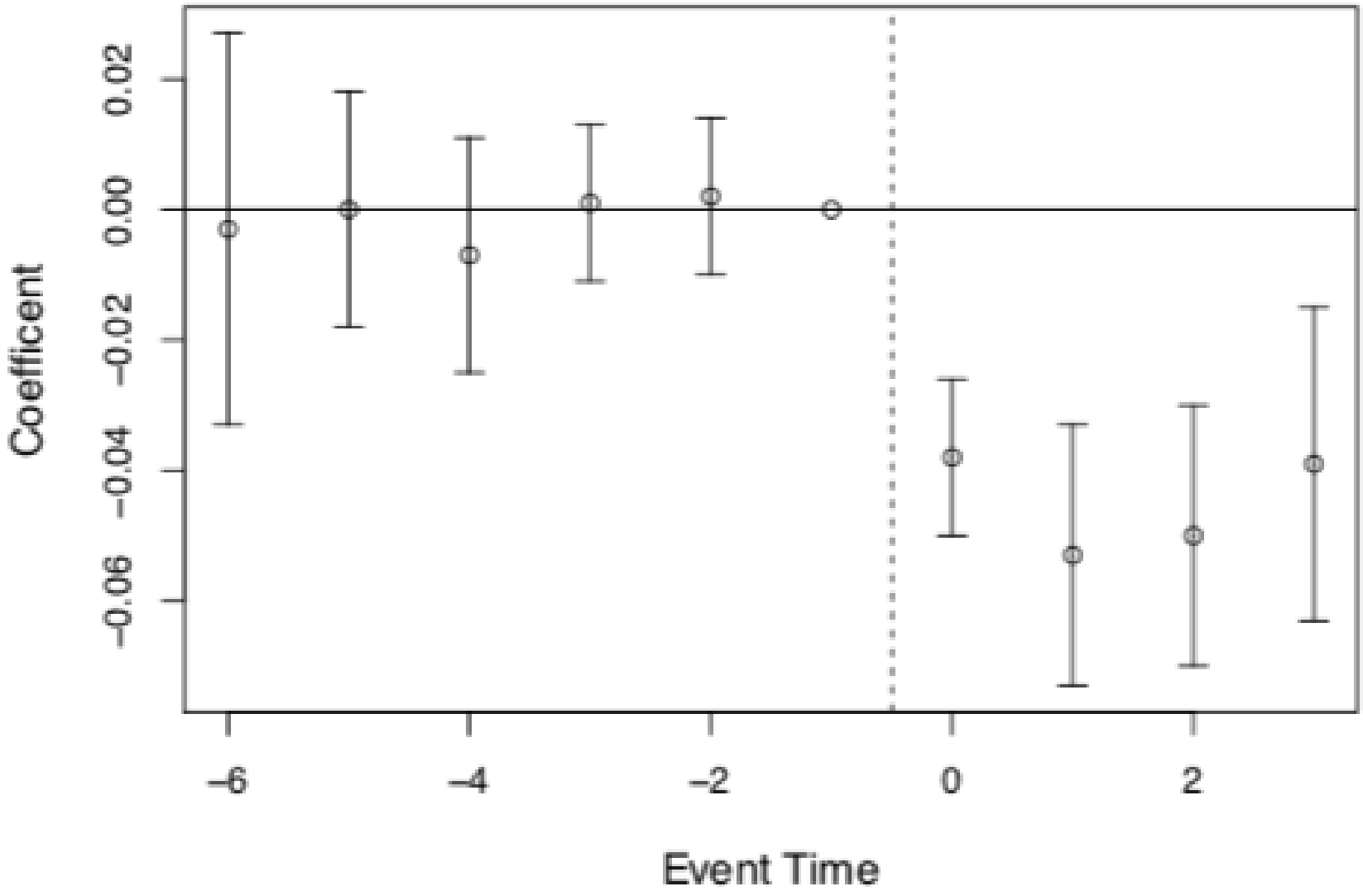
- Bite is a labor economist's phrase, often used with the minimum wage, to say that the minimum wage actually was binding in the first place
- Here it means when US states made Medicaid more generous, people got on Medicaid who would not have been on it otherwise
- And as a bonus, would not have been insured at all without it
- Not the most exciting result, but imagine if the main results on mortality were shown but there was no evidence for bite – is it believable?



(a) Medicaid Eligibility



(b) Medicaid Coverage



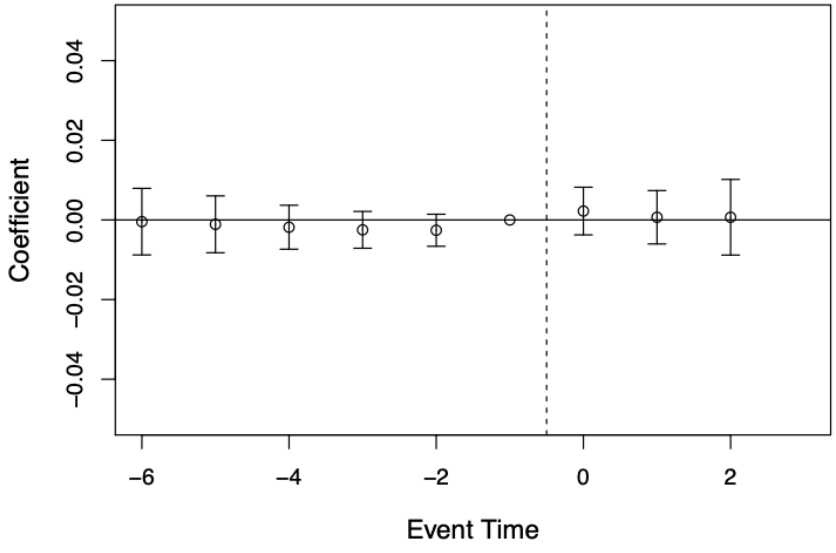
(c) Uninsured

Falsification

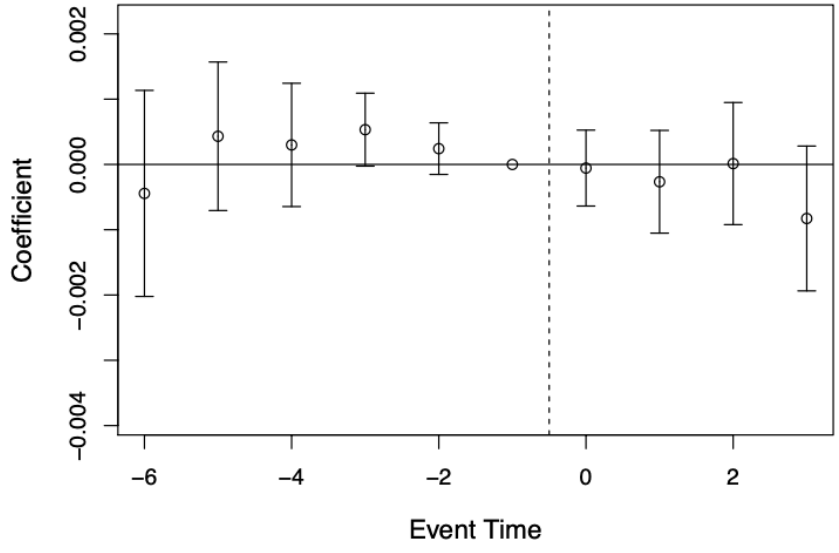
- Their study focuses on “near elderly”, which means just under 65
- They choose just under 65 because in the US, 65 and older are eligible for Medicare so more generous Medicaid is irrelevant
- *But* probably the near elderly and the elderly are equally susceptible to unobserved factors correlated with the treatment
- So they painstakingly examine the effects on elderly as a falsification as this will strengthen the parallel trends assumption on the near elderly

Falsifications on elderly

Age 65+ in 2014



(c) Medicaid Coverage

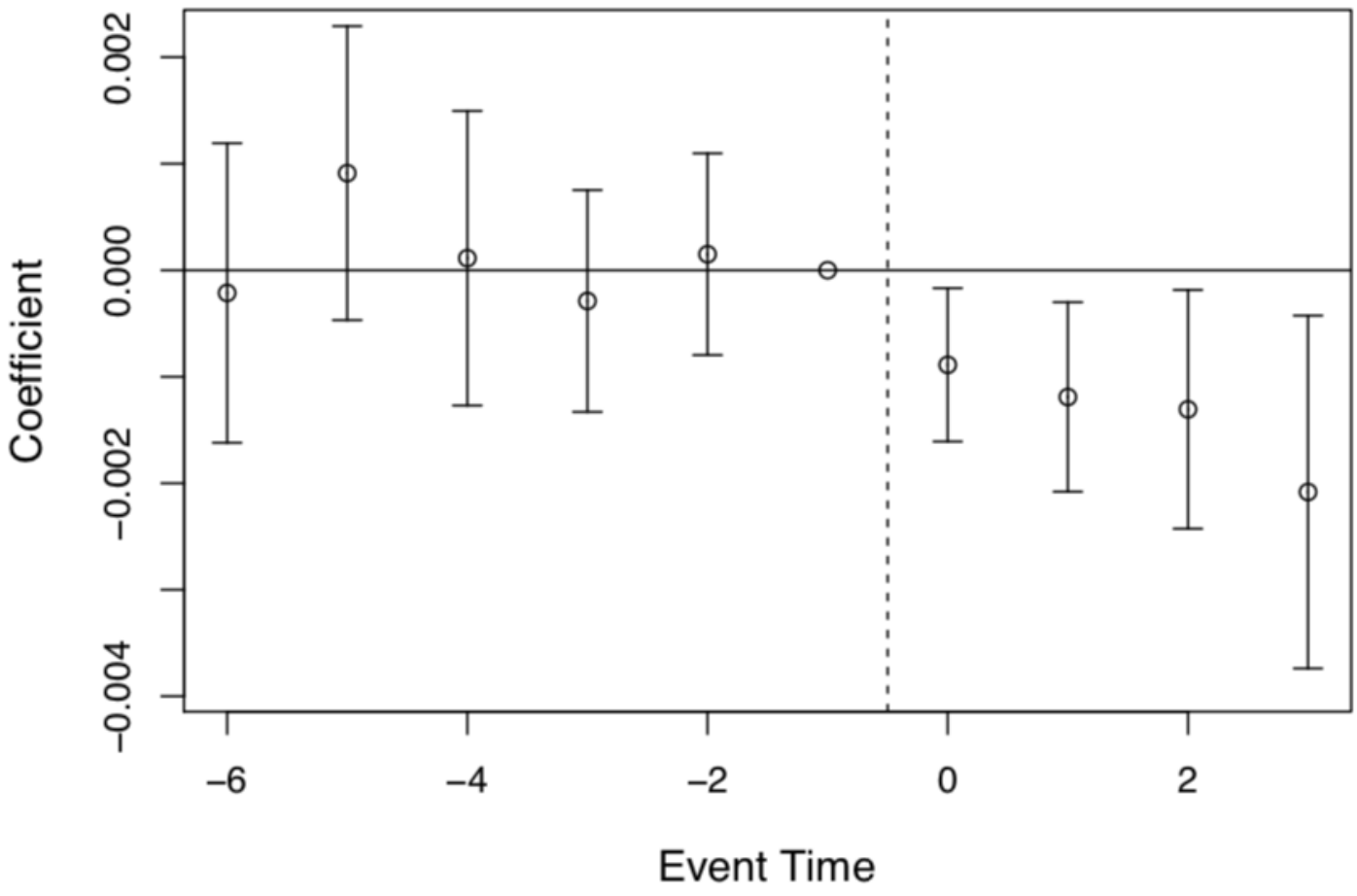


(d) Annual Mortality

Main result

- Finally they focus on the main result – and there's more in the paper than I'm showing
- Event study plots with same specification as the rest allowing us to look at the pre-trends and the post-treatment coefficients
- If parallel trends holds, then the post-treatment coefficients are interpreted as ATT parameter estimates for each time period
- The result alone isn't nearly as strong the result in combination with the rest, but it could still be wrong as parallel trends is ultimately not verfiable

Near elderly mortality and Medicaid expansion



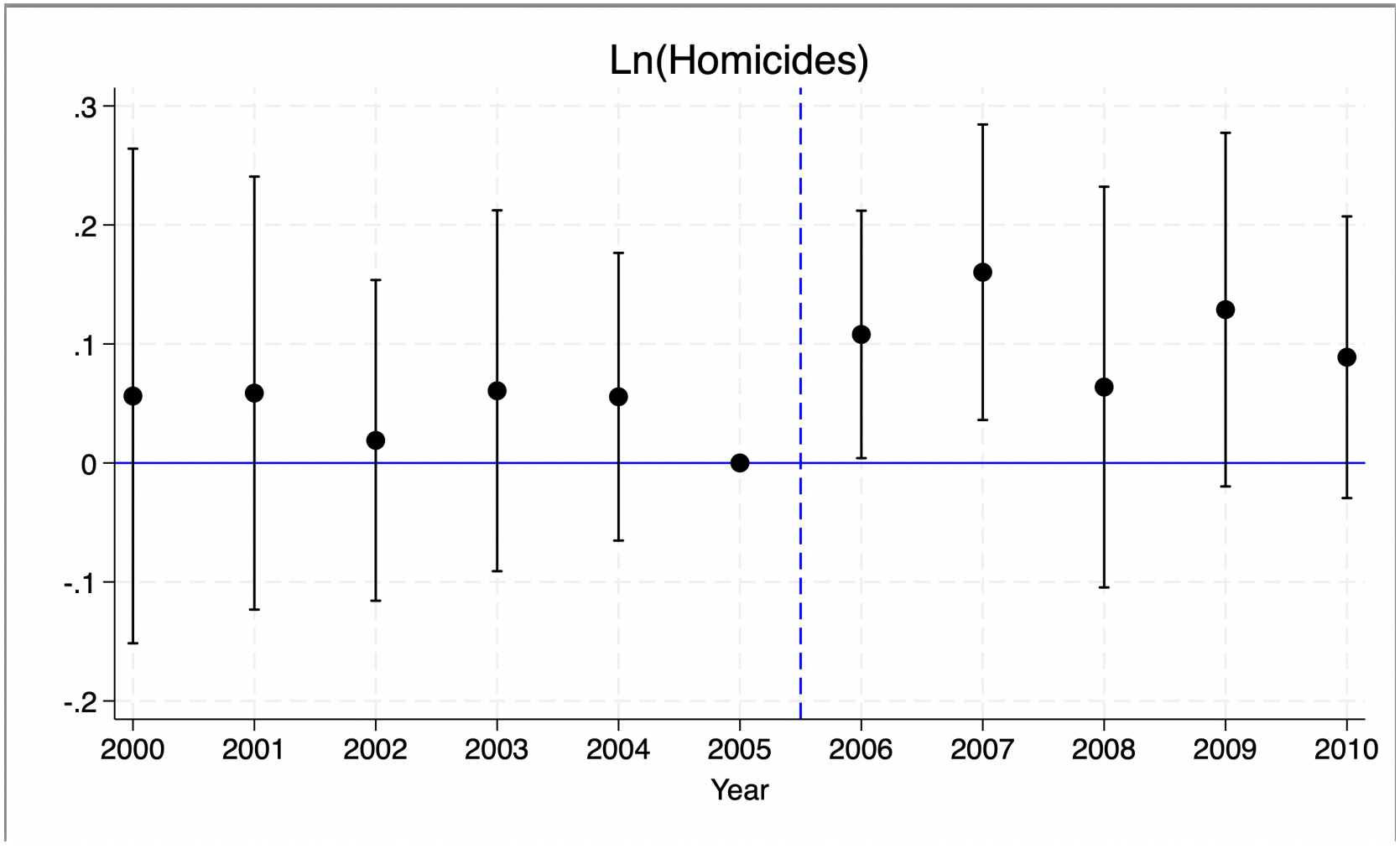
Summarizing evidence and results

- **Bite**: Increases in enrollment and reductions in uninsured support that there is adoption of the treatment
- **Event studies**: Compelling graphics showing similarities between treatment and control
- **Falsifications**: no effect on a similar group who isn't eligible
- Main results: 9.2% reduction in mortality among the near-elderly
- Mechanism: "The effect is driven by a reduction in disease-related deaths and grows over time."

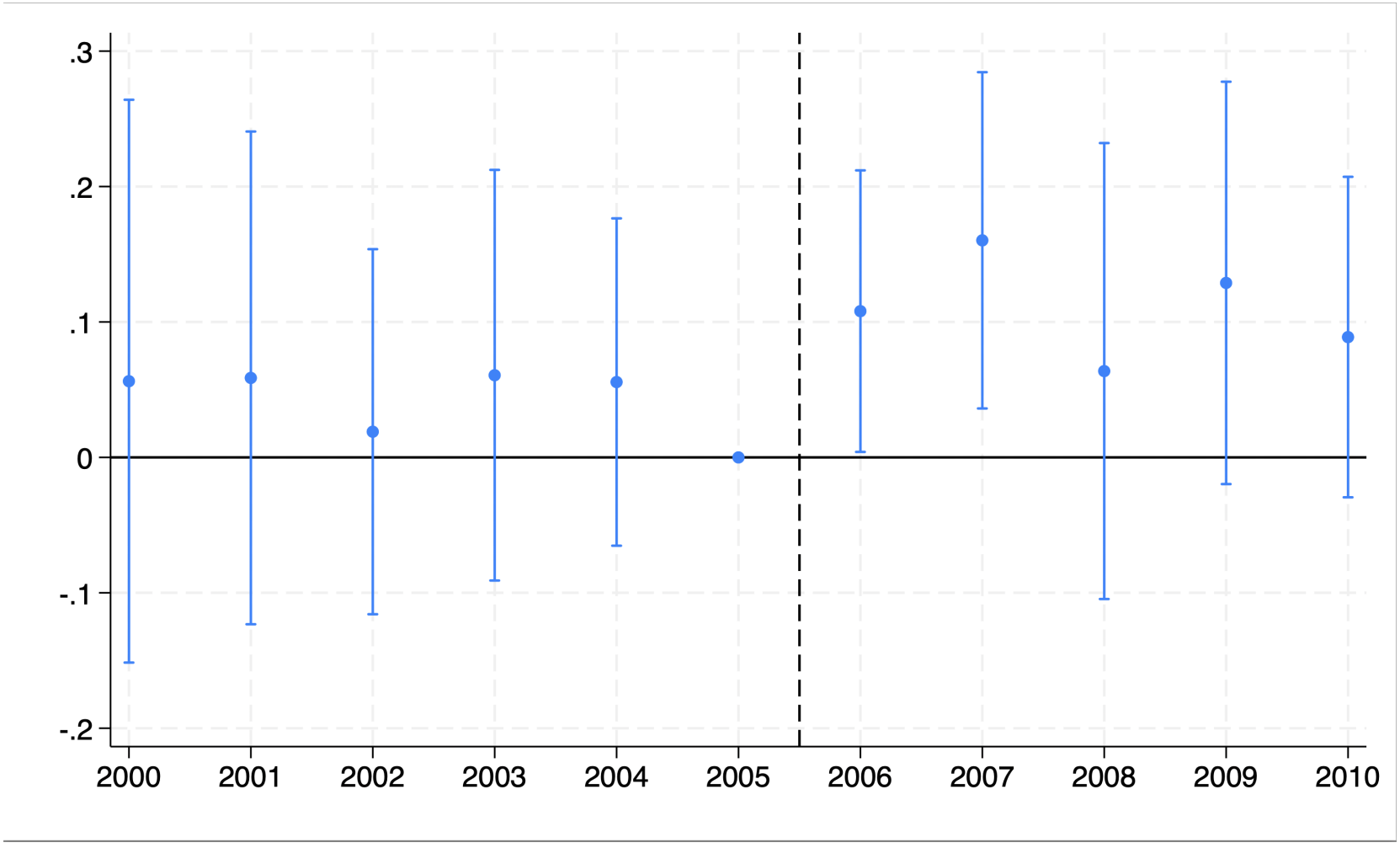
Making event study

- When there is only one treatment group and one comparison group, then you run a regression with an interaction of the treatment group dummy and the calendar year dummies (plus both separately)
- You must drop $t - \tau$ as the baseline (e.g., $t - 1$) and it must be Y^0 untreated comparisons (No Anticipation)
- I have included in a do file that will do it for you either manually or using coefplot in `simple_eventstudy.do` at the shared github labs directory

Manually creating the event study



Creating the event study with Ben Jann's `coefplot`



Biased diff-in-diff

- Many people equate triple differences with falsification exercise, but actually it isn't that – it is it's own design
- You use triple differences when diff-if-diff is biased
- It's used, in other words, when you have a parallel trends violation
- Miller, Johnson and Wherry (2021) didn't use it because they didn't think they had a parallel trends violation – so instead they used falsification

Biased diff-in-diff #1

Table: Biased diff-in-diff #1: comparing states

States	**Period**	**Outcomes**	D_1	D_2
Experimental states	Before	$Y = NJ$		
	After	$Y = NJ + NJ_t + D$	$NJ_t + D$	
				$D + (NJ_t - PA_t)$
Non-experimental states	Before	$Y = PA$		
	After	$Y = PA + PA_t$	PA_t	

$$\widehat{\delta}_{did}^{true} = D + (NJ_t - PA_t)$$

The ATT is D. Assume, though, that parallel trends does not hold, $(NJ_t \neq PA_t)$

Biased Placebo diff-in-diff

Table: Biased placebo diff-in-diff: comparing states but single men and older women

States	**Period**	**Outcomes**	D_1	D_2
Experimental states	Before	$Y = NJ$		
	After	$Y = NJ + NJ_t$	NJ_t	
				$(NJ_t - PA_t)$
Non-experimental states	Before	$Y = PA$		
	After	$Y = PA + PA_t$	PA_t	

$$\widehat{\delta}_{did}^{placebo} = (NJ_t - PA_t)$$

Assume that parallel trends does not hold, $(NJ_t \neq PA_t)$

Two biased diff-in-diffs

- Parallel trends does not hold, ($NJ_t \neq PA_t$), but what if that's the same bias in our placebo DiD?
- Then we can subtract the second from the first:

$$\widehat{\delta}_{ddd} = \widehat{\delta}_{did}^{true} - \widehat{\delta}_{did}^{placebo}$$

- Triple differences is a "real design" with one parallel trends assumption:

$$(NJ_t{}^{true} - PA_t^{true}) = (NJ_t{}^{placebo} - PA_t^{placebo})$$

Triple differences by Gruber (1995)

TABLE 3—DDD ESTIMATES OF THE IMPACT OF STATE MANDATES ON HOURLY WAGES

Location/year	Before law change	After law change	Time difference for location
A. *Treatment Individuals: Married Women, 20–40 Years Old:*			
Experimental states	1.547 (0.012) [1,400]	1.513 (0.012) [1,496]	−0.034 (0.017)
Nonexperimental states	1.369 (0.010) [1,480]	1.397 (0.010) [1,640]	0.028 (0.014)
Location difference at a point in time:	0.178 (0.016)	0.116 (0.015)	
Difference-in-difference:	−0.062 (0.022)		
B. *Control Group: Over 40 and Single Males 20–40:*			
Experimental states	1.759 (0.007) [5,624]	1.748 (0.007) [5,407]	−0.011 (0.010)
Nonexperimental states	1.630 (0.007) [4,959]	1.627 (0.007) [4,928]	−0.003 (0.010)
Location difference at a point in time:	0.129 (0.010)	0.121 (0.010)	
Difference-in-difference:	−0.008: (0.014)		
DDD:	**−0.054** **(0.026)**		

Triple differences commentary

- Some people think that it requires that the placebo DiD be zero, but that's incorrect
- In Gruber's 1995 article, it isn't clear why he needed triple differences in the first place – his triple differences yielded -0.054 which is almost the same as what he found with his first diff-in-diff (-0.062)
- The main value of triple differences is that you use it when you believe the parallel trends assumption doesn't hold

Table: Difference-in-Difference-in-Differences (Gruber version)

Groups	States	Period	Outcomes	D_1	D_2	D_3
Married women 20-40	Experimental states	After	$NJ + MW + NJ_t + MW_t + D$	$NJ_t + MW_t + D$	$D + NJ_t - PA_t$	
		Before	$NJ + MW$			
	Non-experimental states	After	$PA + MW + PA_t + MW_t$	$PA_t + MW_t$		
		Before	$PA + MW$			
Single men Older women	Experimental states	After	$NJ + SO + NJ_t + SO_t$	$NJ_t + SO_t$	$NJ_t - PA_t$	D
		Before	$NJ + SO$			
	Non-experimental states	After	$PA + SO + PA_t + SO_t$	$PA_t + SO_t$		
		Before	$PA + SO$			

Triple diff assumption

$$\widehat{\delta}_{DDD} = D + \underbrace{[(NJ_t^{MW} - PA_t^{MW}) - (NJ_t^{SO} - PA_t^{SO})]}_{\text{Equally biased DiD \#1 and \# 2}}$$

Triple differences requires two diff-in-diff, from different groups, with the same bias. Parallel bias

DDD in Regression

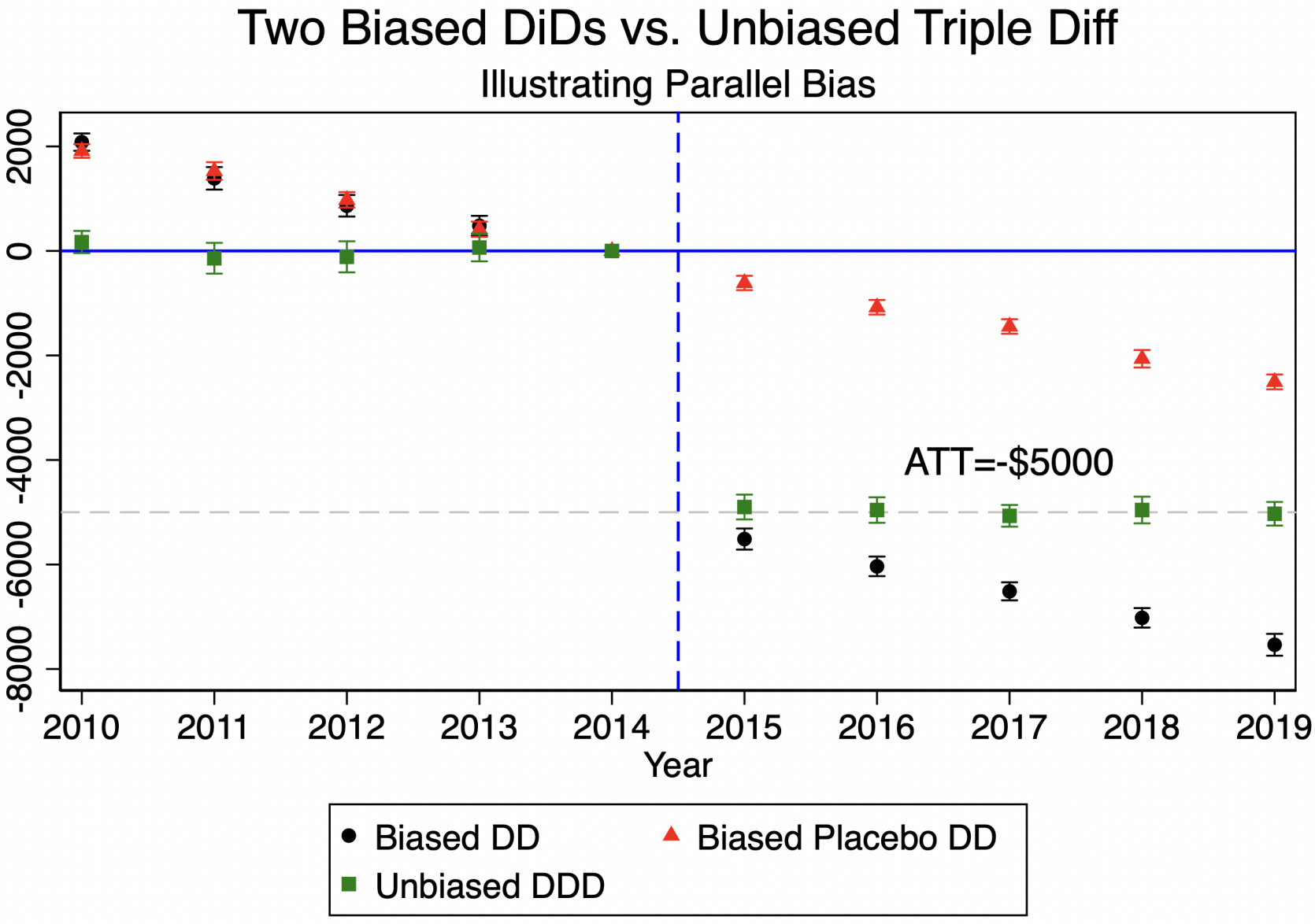
$$\begin{aligned} Y_{ijt} &= \alpha + \beta_2 \tau_t + \beta_3 \delta_j + \beta_4 D_i + \beta_5 (\delta \times \tau)_{jt} \\ &\quad + \beta_6 (\tau \times D)_{ti} + \beta_7 (\delta \times D)_{ij} + \beta_8 (\delta \times \tau \times D)_{ijt} + \varepsilon_{ijt} \end{aligned}$$

- Your dataset will be stacked by group j and state i
- $\widehat{\beta_8}$ estimates the ATT
- Parallel bias, NA and SUTVA necessary and sufficient for identification

Simulation

In /Labs/DDD I have a simulation to illustrate this for us called ddd2.do. The ATT is -$5,000 but the biased DiD is -$7487. The non-parallel trends bias is -$2,487. So I replicate Gruber (with simulated data) where the placebo DiD is close (-$2,507). I then present a triple differences which gives us -$4,972. Let's look at the final product.

Triple differences event study



Great new paper to learn more

The Econometrics Journal

Econometrics Journal (2022), volume **00**, pp. 1–23.
https://doi.org/10.1093/ectj/utac010

The triple difference estimator

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Summary: Triple difference has become a widely used estimator in empirical work. A close reading of articles in top economics journals reveals that the use of the estimator to a large extent rests on intuition. The identifying assumptions are neither formally derived nor generally agreed on. We give a complete presentation of the triple difference estimator, and show that even though the estimator can be computed as the difference between two difference-in-differences estimators, it does not require two parallel trend assumptions to have a causal interpretation. The reason is that the difference between two biased difference-in-differences estimators will be unbiased as long as the bias is the same in both estimators. This requires only one parallel trend assumption to hold.

Keywords: *DD*, *DDD*, *DID*, *DiDiD*, *difference-in-difference-in-differences*, *difference-in-differences*, *parallel trend assumption*, *triple difference*.

JEL Codes: *C10*, *C18*, *C21*.

1. INTRODUCTION

The triple difference estimator is widely used, either under the name 'triple difference' (TD) or the name 'difference-in-difference-in-differences' (DDD), or with minor variations of these spellings. Triple difference is an extension of double differences and was introduced by Gruber (1994). Even though Gruber's paper is well cited, very few modern users of triple difference credit him for his methodological contribution. One reason may be that the properties of the triple difference estimator are considered obvious. Another reason may be that triple difference was little more than a curiosity in the first ten years after Gruber's paper. On Google Scholar, the annual number of references to triple difference did not pass one hundred until year 2007. Since then, the use of the estimator has grown rapidly and reached 928 unique works referencing it in the year 2017.[1]

Looking only at the core economics journals *American Economic Review* (AER), *Journal of Political Economy* (JPE), and *Quarterly Journal of Economics* (QJE), we have found 32 articles using triple difference between 2010 and 2017, see Table A1 in Appendix A. A close reading of these articles reveals that the use of the triple difference estimator to a large extent rests on

[1] More details on the historical development of the use of the triple difference estimator can be found in the working paper version of Olden and Møen (2020, fig. 1). In the working paper, we also analyse naming conventions and suggest that there is a need to unify terminology. We recommend the terms 'triple difference' and 'difference-in-difference-in-differences'.

Summarizing DDD

- Used to be people thought DDD required two parallel trends assumptions but it does not – it is a real design and requires one parallel trends assumption
- Parallel trends assumption is “parallel bias” – that the bias of the true DiD is the same as the bias of the placebo DiD
- The ladder of evidence still holds – you’ll want to present the event study plot, and my code provides it for you, because you need to evaluate the parallel bias assumption
- Given the lack of triple diff literacy, you may have to write this anticipating reader and maybe editor confusion and so ”educate” as you go – overlaying all three plots could be help

Falsification on outcomes

- Miller, Johnson and Wherry (2021) used the same outcome, but a placebo untreated treatment group (elderly) as a falsification to provide evidence for parallel trends
- But you can also use the same group but different outcomes
- Cheng and Hoekstra (2013) examine the effect of castle doctrine gun laws on non-gun related offenses like grand theft auto and find no evidence of an effect
- You just want to pick things that make logical sense in that they are a similar enough group (near elderly versus elderly) or an outcome that would be susceptible to some omitted variable you're worried about

Rational addiction as a placebo critique

Sometimes, an empirical literature may be criticized using nothing more than placebo analysis

> *"A majority of [our] respondents believe the literature is a success story that demonstrates the power of economic reasoning. At the same time, they also believe the empirical evidence is weak, and they disagree both on the type of evidence that would validate the theory and the policy implications. Taken together, this points to an interesting gap. On the one hand, most of the respondents claim that the theory has valuable real world implications. On the other hand, they do not believe the theory has received empirical support."*

Placebo as critique of empirical rational addiction

- Auld and Grootendorst (2004) estimated standard “rational addiction” models (Becker and Murphy 1988) on data with milk, eggs, oranges and apples.
- They find these plausibly non-addictive goods are addictive, which casts doubt on the empirical rational addiction models.

Placebo as critique of peer effects

- Several studies found evidence for “peer effects” involving inter-peer transmission of smoking, alcohol use and happiness tendencies
- Christakis and Fowler (2007) found significant network effects on outcomes like obesity
- Cohen-Cole and Fletcher (2008) use similar models and data and find similar network “effects” for things that *aren’t* contagious like acne, height and headaches
- Homophily (sorting) is probably just as likely an explanation

Concluding the basics

- That concludes the basics of diff-in-diff
- A lot of what we just went through is pretty standard and common to any diff-in-diff, but some of it even other studies too
- Notice how we really didn't address any parallel trends violation issues with much more than just looking for evidence that it probably held
- But what if actually doesn't hold and we can't fix it with triple differences?
- Next we look at one very common example – the use of covariates to fix parallel trends violations