Causal Inference II

Mixtape Session

Roadmap

- Introduction
  - Managing expectations
  - Introducing difference-in-differences
  - Potential outcomes
  - Estimation
  - Inference

- Parallel trends violations
  - How parallel trends can get violated
  - DiD in Court
  - Falsifications

Introduction

- Welcome to Mixtape Sessions workshop on difference-in-differences and synthetic control (“Causal Inference II”)
- 8:00am to 5:00pm CST, 15 min breaks every hour, 1 hour lunch at noon CST
- Lecture, discussion, exercises, application

What my pedagogy is like

- Long days that don't feel long because it's high energy, with regular breaks including lunch
- Move between the econometrics, history of thought, videos, applications, code, spreadsheets, exercises
- 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

Introduction to DiD basics

- Potential outcomes review and the ATT parameter
- DiD equation (“four averages and three differences”), parallel trends and estimation with OLS
- Evaluating parallel trends with falsifications, event studies
- Compositional changes, triple differences and covariates

Day 2 outline

Differential timing

- TWFE Pathologies in static and dynamic specifications (“event study”)
- Solution 1: Aggregating group-time ATTs
- Solution 2: Imputation estimators
- Solution 3: Stacked regression

Day 3 outline

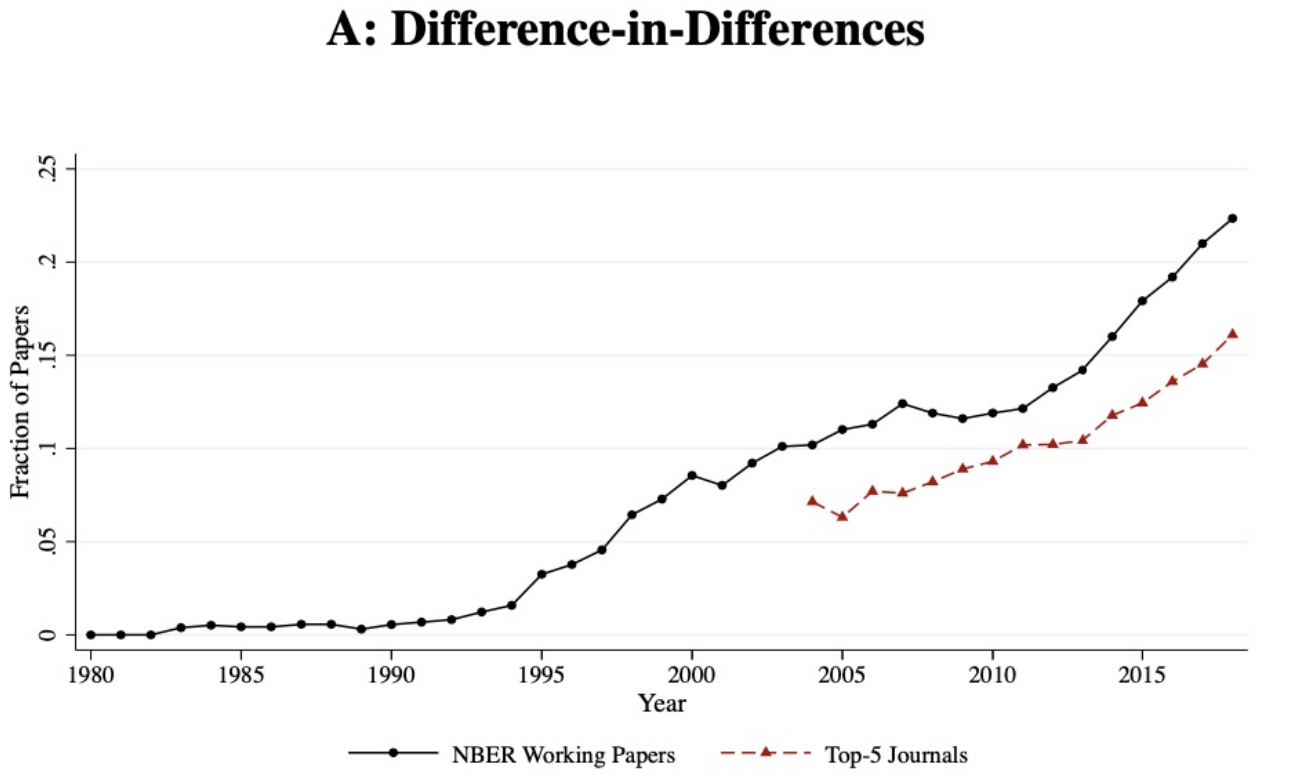
- Canonical synth (Abadie papers)
- Augmented synth (Ben-Michael, et al)
- Matrix completion with nuclear norm regularization (Athey, et al.)
- Exercises and estimation

What is difference-in-differences (DiD)

- DiD is a very old, relatively straightforward, intuitive research design
- A group of units are assigned some treatment and then compared to a group of units that weren't
- One of the most widely used quasi-experimental methods in economics and increasingly in industry
- Mostly associated with "big shocks" happening in space over time

  "A good way to do econometrics is to look for good natural experiments and use statistical methods that can tidy up the confounding factors that nature has not controlled for us." – Daniel McFadden (Nobel Laureate recipient with Heckman 2002)

Figure: Currie, et al. (2020)



Origins of diff-in-diff

- Difference-in-differences (DiD) was quietly and largely unnoticed introduced in the 19th century as a way to convince skeptics in health policy arguments
- Dominant disease theory in 19th century was *miasma* – disease caused by smelly vapor
- Keep in mind – microorganisms would not be identified until much later, partly caused by poor resolution in microscopes (Freedman 2007)

Miasma I: Ignaz Semmelweis and washing hands

- 1840s, Vienna maternity wards had high postpartum infections in one wing compared to other wings
- One division had doctors and trainee doctors, but another had midwives and trainee midwives

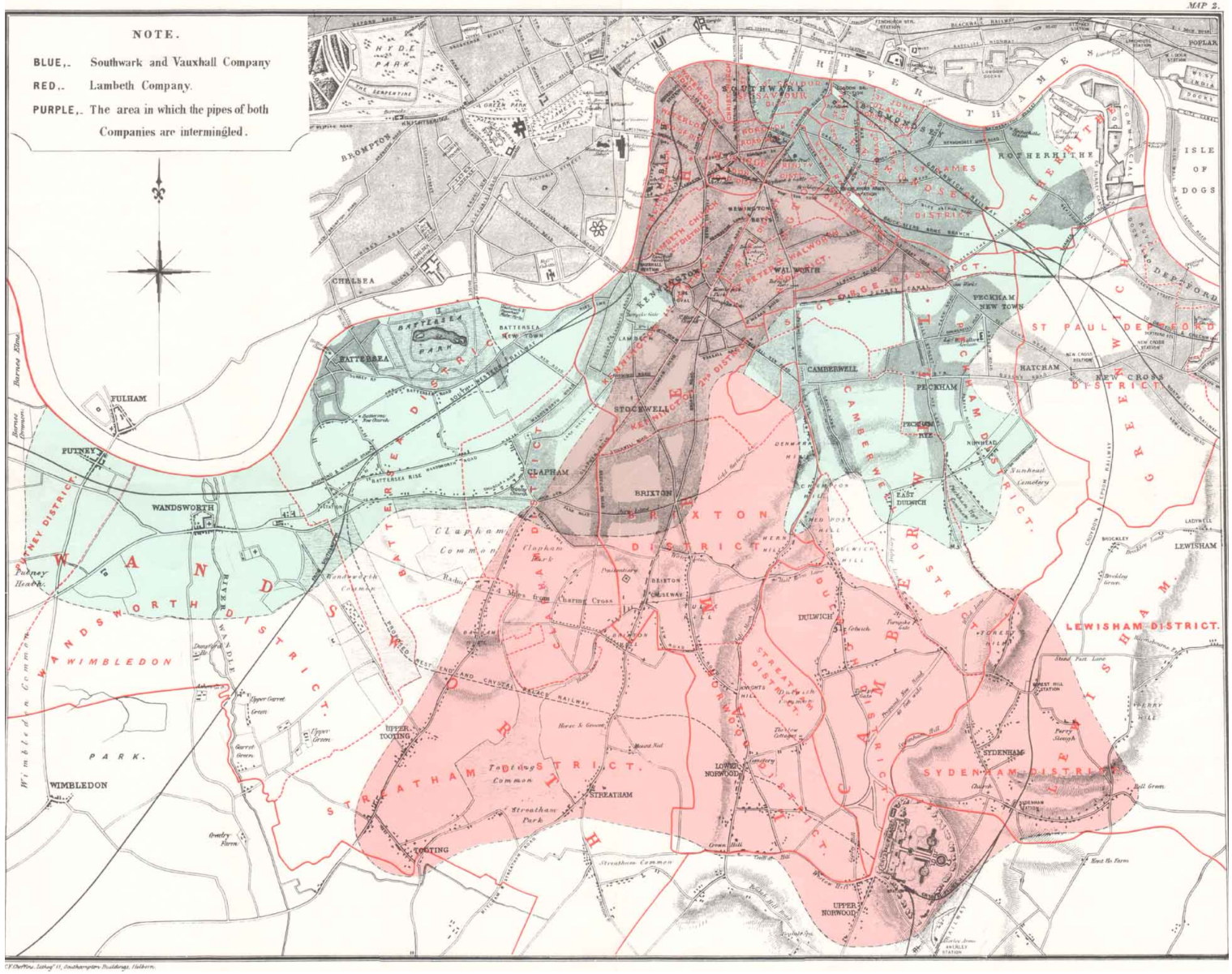
Miasma I: Ignaz Semmelweis and washing hands

- Ignaz Semmelweis notes the difference in 1841 when hospitals moved to “anatomical” training involving cadavers (Pamela Jakeila lecture notes on DiD)
- New training happens to one but not the other and Semmelweis thinks the mortality is caused by working with cadavers
- Proposes washing hands with chlorine in 1847 in the midwives’ wing and uses a DiD design of pre and post

Miasma II: John Snow and cholera

- Three major waves of cholera in the early to mid 1800s in London
- John Snow believed cholera was spread through the Thames water supply which contradicted dominant theory about “dirty air” transmission
- Grand experiment: Lambeth moves its pipe between 1849 and 1854; Southwark and Vauxhall delay
- He can evaluate the effect in three ways (one of which is DiD)

Figure: Two water utility companies in London 1854



1) Simple cross-sectional design

Table: Lambeth and Southwark and Vauxhall, 1854

Company	**Cholera mortality**
Lambeth	$Y = L + D$
Southwark and Vauxhall	$Y = SV$

$$\widehat{\delta}_{cs} = D + (L - SV)$$

What is L and SV?

1) Simple cross-sectional design

Table: Lambeth and Southwark and Vauxhall, 1854

Company	**Cholera mortality**
Lambeth	$Y = L + D$
Southwark and Vauxhall	$Y = SV$

$$\widehat{\delta}_{cs} = D + (L - SV)$$

This is biased if L $\neq SV$ (selection bias). Give an example when we're pretty sure they are equal.

2) Interrupted time series design

Table: Lambeth, 1849 and 1854

Company	Time	Cholera mortality
Lambeth	1849	$Y = L$
	1854	$Y = L + (L_t + D)$

$$\widehat{\delta}_{its} = D + L_t$$

What is required for this estimator to be unbiased?

3) 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 $L_t = SV_t$

Difference-in-differences and empirical crisis in labor economics

- Empirical crisis in empirical labor back in the 1970s (26:31 to 32:00) https://youtu.be/1soLdywFb_Q?t=1579
- Orley Ashenfelter graduated from Princeton in the 1970s, takes a job in Washington DC and begins studying “job trainings programs” where he develops the difference-in-differences design
- 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

Steps of a 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

Most of us 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? Let’s dig into Orley’s comment a little more.

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 OLS 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"

Work together #1

Work together:

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

Assume that the U group is treated in both periods. Replace expectations with potential outcomes and rewrite using the “add zero” trick we did. How is this similar to what we did before? Is parallel trends enough?

Work together #2

Work together:

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

In the pre-period, how important is it that the U group and k group have the same treatment status?

What is parallel trends

- Parallel trends assumes away the selection bias associated with comparisons
- The assumption is thought to be more plausible than simply assuming simple comparisons held equal
  $E[Y^0|D=0] = E[Y^0|D=1]$
- But it is still a strong assumption, and differs from the assumptions have in the RCT which though also untestable, is nearly guaranteed by randomization
- Most of the hard part of the work involves the old fashioned detective work and the work of making good arguments with good exhibits (tables and figures)

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

No Anticipation

- Additional assumption is “no anticipation” – poorly named as it doesn’t require literally no anticipation
- No anticipation means that the treatment effect happens only at the time that the treatment occurs or after, but not before
  - → **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.
- We need NA because we are comparing to a baseline period and it needs to not be treated

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

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 (ATT) of minimum wages on employment
- Let's listen to Card tell its origin
  https://youtu.be/1soLdywFb_Q?t=2680
- Exploited a policy change in New Jersey between February and November in mid-1990s where minimum wage was increased, but neighbor PA did not
- Using DiD, they do not find a negative effect of the minimum wage on employment which is part of its legacy today, but I mainly present it to illustrate the history and the design principles

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

- Was it or was it not controversial? Yes and no
- Orley's opinion about the paper's controversy at the time.
  https://youtu.be/MOtbuRX4eyQ?t=1882
- What Card and Krueger *experienced*
  https://youtu.be/1soLdywFb_Q?t=3076

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."

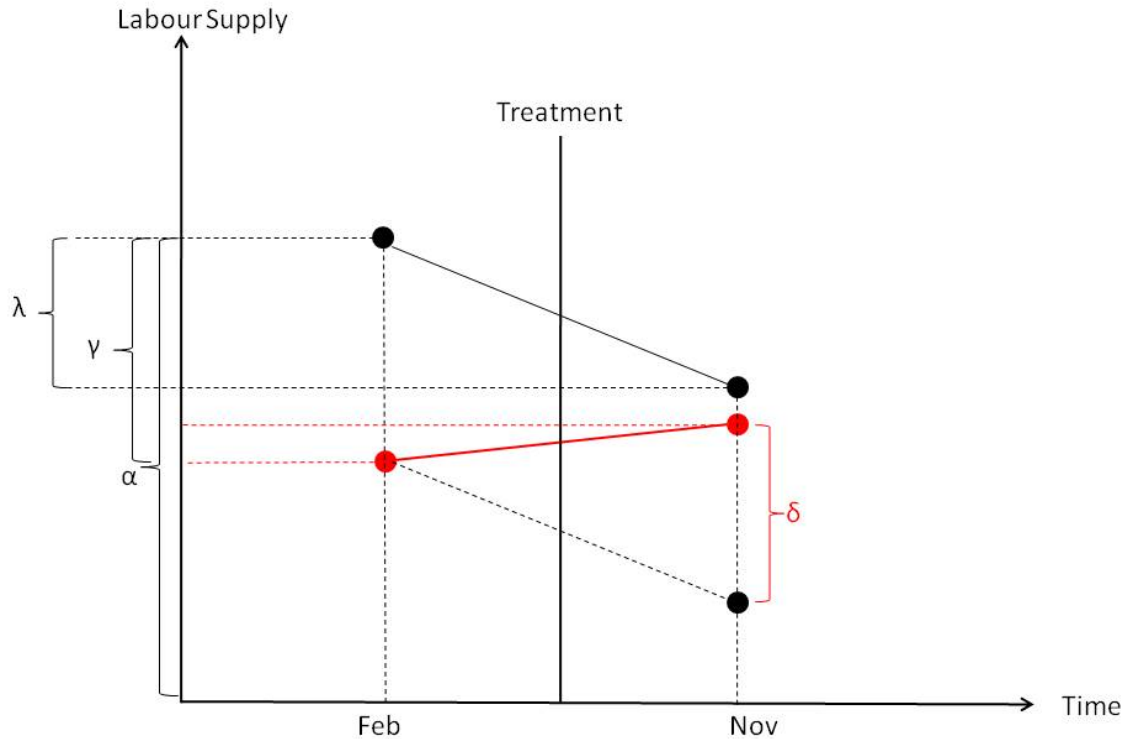
OLS specification of the DiD equation

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

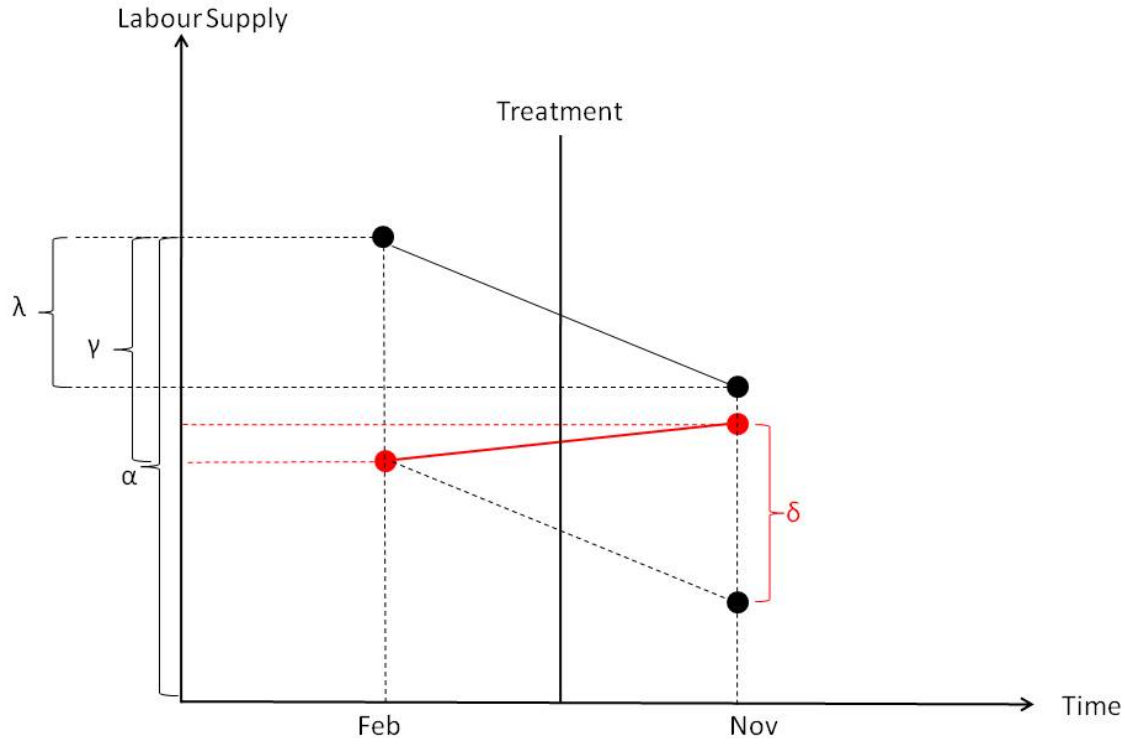
$$Y_{its} = \alpha + \gamma NJ_s + \lambda d_t + \delta(NJ \times d)_{st} + \varepsilon_{its}$$

  - → 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

Introduction to 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 classical OLS assumption of independent errors, leading to inefficient and possibly biased standard errors.
- Clustering standard errors accounts for this within-cluster correlation.

Why Focus on Correlated Errors?

- OLS assumptions are about errors, not the observed variables.
- Failing to account for correlated errors can lead to misleading inference.
- Clustering standard errors at the appropriate level corrects for this, giving more reliable hypothesis tests and confidence intervals.

Serial correlation creates problems

- So errors within a cluster (e.g., same treatment group) might be correlated.
- Even if between clusters, they are assumed to be independent, the within correlation in errors biases standard errors downward
- 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)

Inference

1. Block bootstrapping standard errors (if you analyze states the block should be the states and you would sample whole states with replacement for bootstrapping)
2. Clustering standard errors at the group level (in Stata one would simply add `, cluster(state)` to the regression equation if one analyzes state level variation)

Most people will simply cluster, but there are issues if you have too few clusters. They mention a third way but it's only a curiosity.

Computing Cluster-Robust Standard Errors

1. Run your regression to get u and $\hat{Y}$.
2. Calculate cluster-level residuals $\hat{u}_c$.
3. Calculate the "meat" (as in bread-meat-bread sandwich) M as $\sum_c X_c' \hat{u}_c \hat{u}_c' X_c$.
4. Your standard error covariance matrix is $\hat{V} = (X'X)^{-1} M (X'X)^{-1}$.

Mixtape reference (chapter 2):
https://mixtape.scunning.com/02-probability_and_regression#cluster-robust-standard-errors

Compressing Into a Single Number

- Diagonal elements of $\hat{V}$ contain variances for each coefficient.
- Standard errors are the square root of these diagonal elements.
- Non-diagonal elements are used for hypothesis tests and confidence intervals for combinations of coefficients.

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Parallel trends violations
- How parallel trends can get violated
- DiD in Court
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Violating parallel trends exercise

- Parallel trends are needed so we can impute the missing $E[Y^0|D=1]$ with $E[Y^0|D=0]$ either explicitly or implicitly
- Which means if parallel trends isn't true, then the imputation isn't correct and therefore estimates are biased
- To illustrate this, let's go through the document again

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

Violating parallel trends

- Parallel trends are in expectation only – we don't rely everybody to follow the same trend, just that the group average for Y^0 be approximately the same for treated and control
- Violations are a form of selection bias and there are two straightforward ways that parallel trends will be violated
    1. Compositional differences in samples associated with repeated cross-sections
    2. Policy endogeneity

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

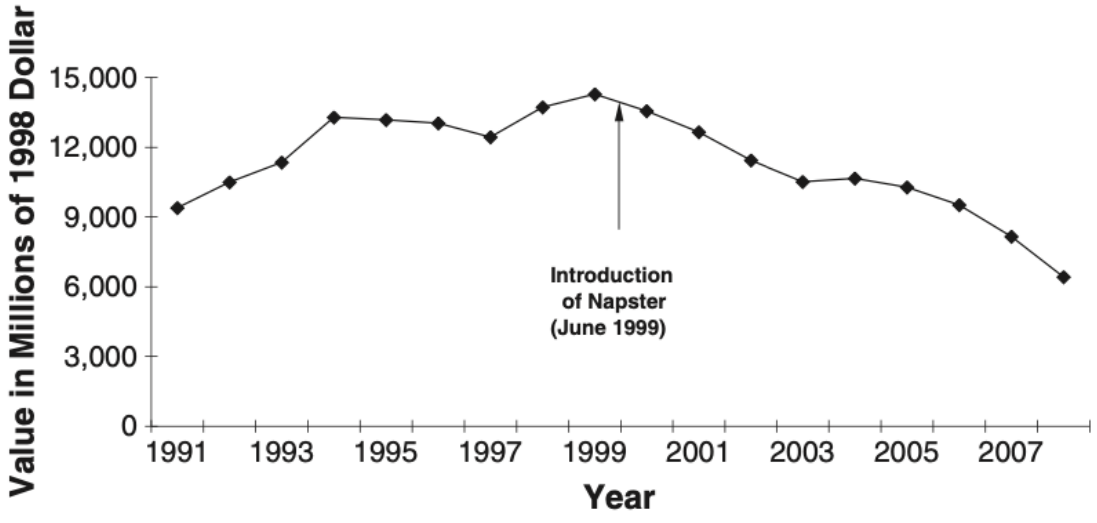


Figure 2. Total real value of record shipments in the USA. Refer to the RIAA's year-end statistics. Total sales include CDs, cassettes, LPs, and music videos. Starting from 2004, total sales also include digital formats such as legitimate download

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

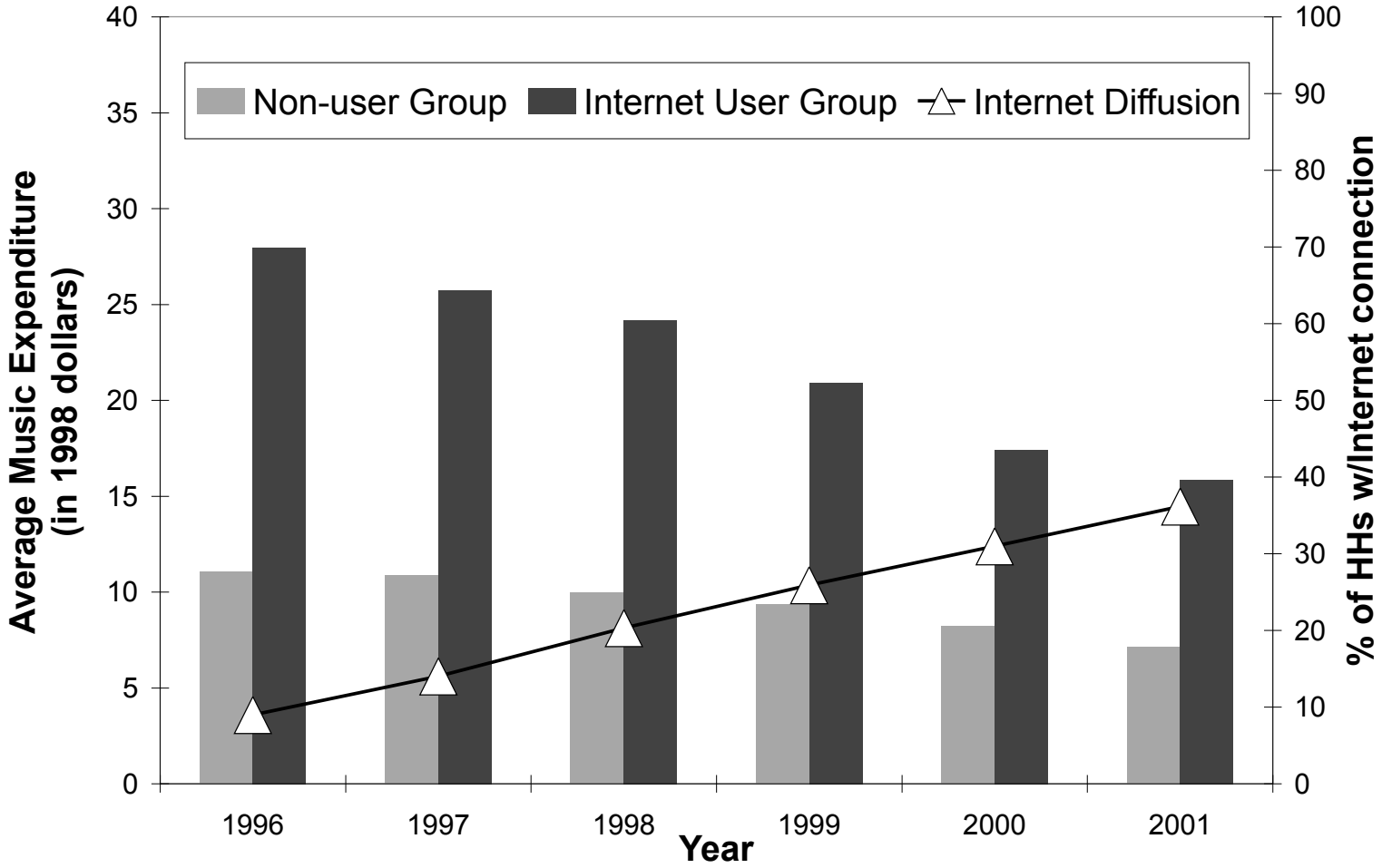


Table 1: Descriptive Statistics for Internet User and Non-user Groups[a]

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)

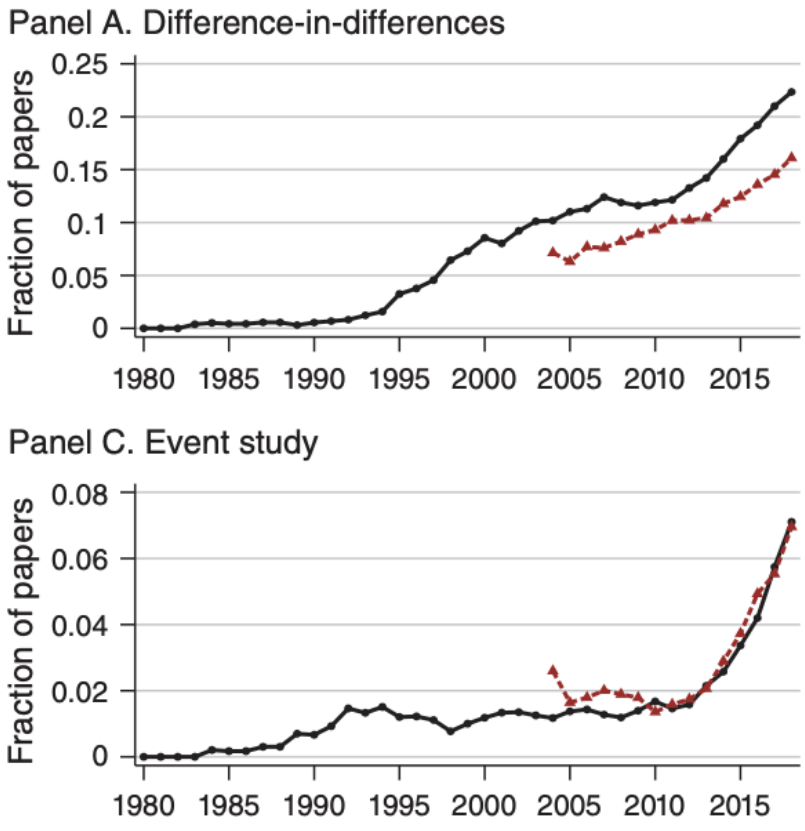
Repeated cross-sections

- Surprisingly underappreciated problem with almost no literature around it
- So what can you do? Check covariate balance by regressing the time-varying covariates instead of the outcome onto the treatment using your OLS specification
- They should be exogenous remember, so this covariate regression can be a helpful test of whether this is a problem
- “Difference-in-differences with Compositional Changes” by Pedro Sant’anna and Qi Xu (not yet released) is the only paper I’ve ever seen to look into it

Types of evidence

- You are building a case, the prosecutor before a judge and jury, always in battle with the defense attorney
- Evidence has particular broadly defined forms that can help you on the front end
- Your goal in my humble opinion should be mixing tight logic based falsifications with particular kinds of data visualization, starting with the event study

Event studies have become mandatory in DiD



Intuition behind event studies

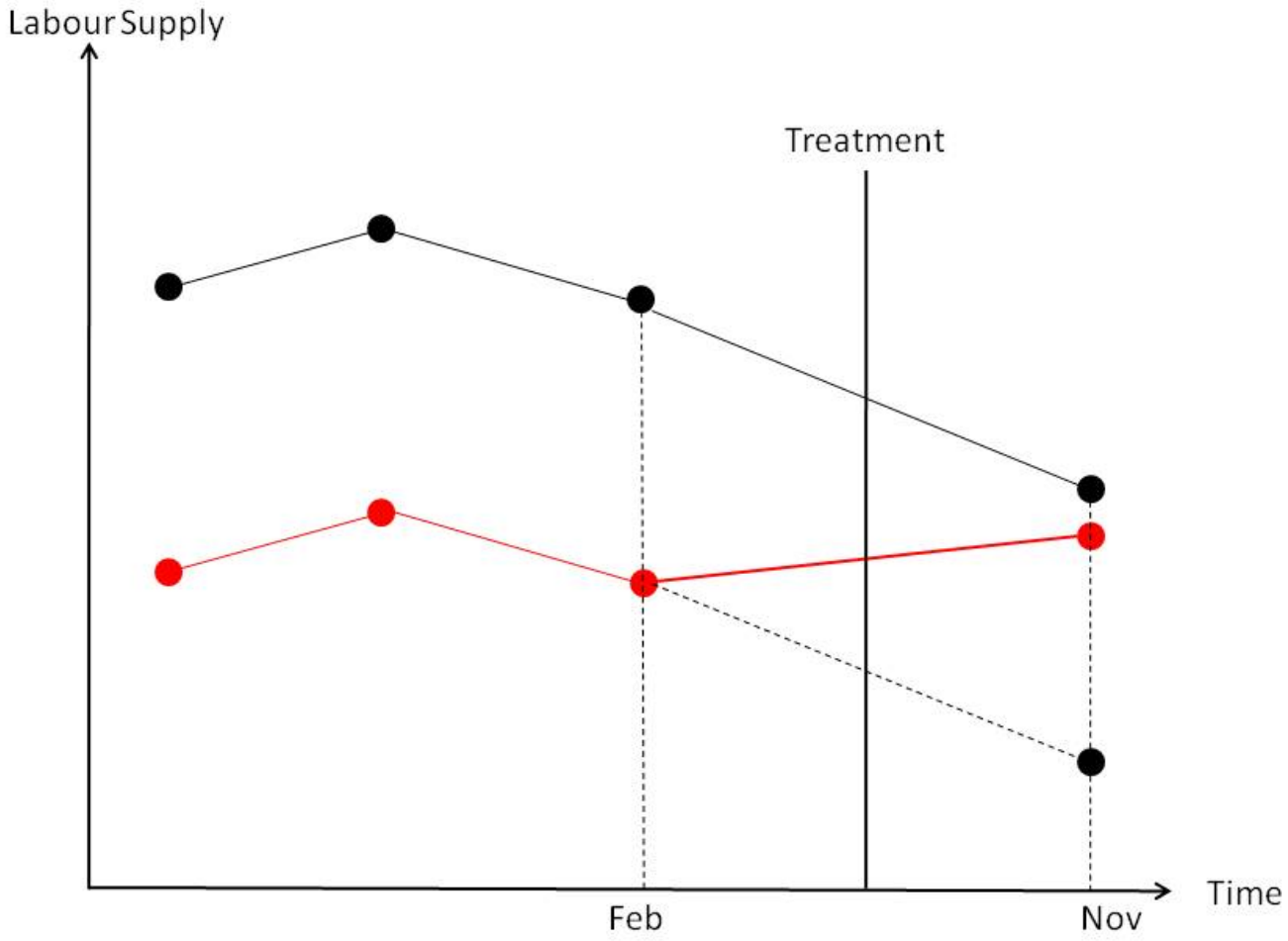
- Princeton Industrial Relations Section seems to be behind this – this intense focus on research design but also verifying assumptions
- The identifying assumption for all DD designs is parallel trends , but since we cannot verify parallel trends, we often look at pre-trends
- It's a type of check for selection bias, but you must understand what it is and what it isn't to see its value but not be naive about it (it is not a silver bullet)
- 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)

Ashenfelter's dip

Orley: https://youtu.be/MOtbuRX4eyQ?t=960

Card: https://youtu.be/1soLdywFb_Q?t=3333

Plot the raw data when there's only two groups



Evidence for parallel trends: pre-trends

Let's do the bonus questions on first and second tab now

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

Beware of naive reasoning

- Parallel pre-trends $\neq$ parallel trends – these are often thought to be the same thing, and they aren't
- Equating them is a kind of *post hoc ergo propter hoc* fallacy
- Parallel pre-trends is more like a smoking gun based on things "looking the same" before
- Similar to checking for covariate balance in RCTs
- But **cannot** substitute for domain knowledge!

Event study regression

- Event studies have a simple OLS specification with only one treatment group and one never-treated group

$$Y_{its} = \alpha + \sum_{\tau=-2}^{-q} \mu_\tau D_{s\tau} + \sum_{\tau=0}^{m} \delta_\tau D_{s\tau} + \varepsilon_{ist}$$

  - → where D is an interaction of the treatment group s with the calendar year τ
  - → Treatment occurs in year 0, no anticipation, drop baseline $t-1$
  - → Includes q leads or anticipatory effects and m lags or post treatment effects

Event study regression

$$Y_{its} = \alpha + \sum_{\tau=-2}^{-q} \mu_\tau D_{s\tau} + \sum_{\tau=0}^{m} \delta_\tau D_{s\tau} + \varepsilon_{ist}$$

Typically you'll plot the coefficients and 95% CI on all leads and lags (binned or not, trimmed or not)

Under no anticipation, then you expect $\widehat{\mu}$ coefficients to be zero, which gives you confidence that parallel trends holds (but is not a guarantee, and there are still specification issues – see Jon Roth's work)

Under parallel trends, $\widehat{\delta}$ are estimates of the ATT at points in time

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

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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

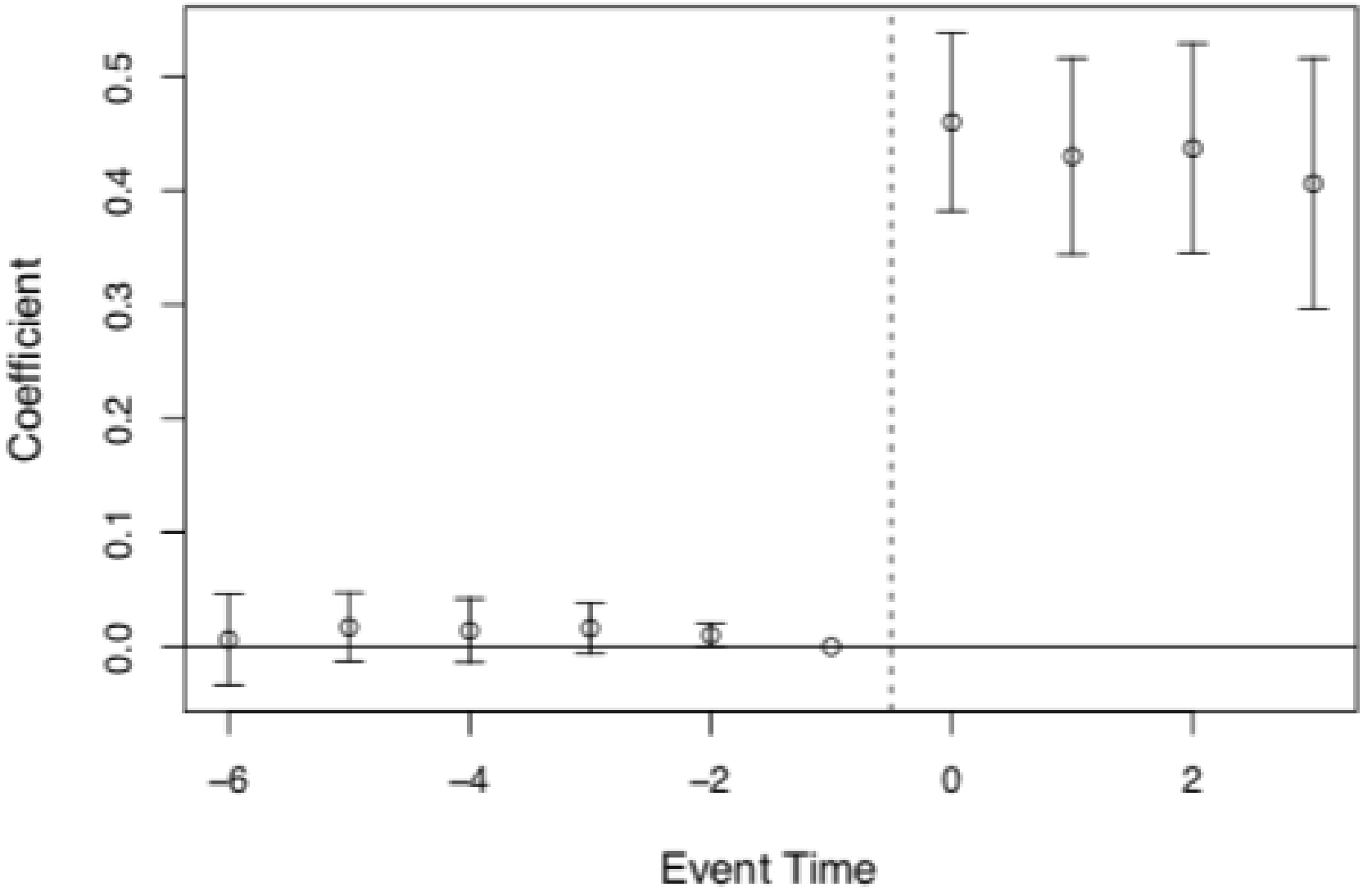
We use large-scale federal survey data linked to administrative death records to investigate the relationship between Medicaid enrollment and mortality. Our analysis compares changes in mortality for near-elderly adults in states with and without Affordable Care Act Medicaid expansions. We identify adults 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

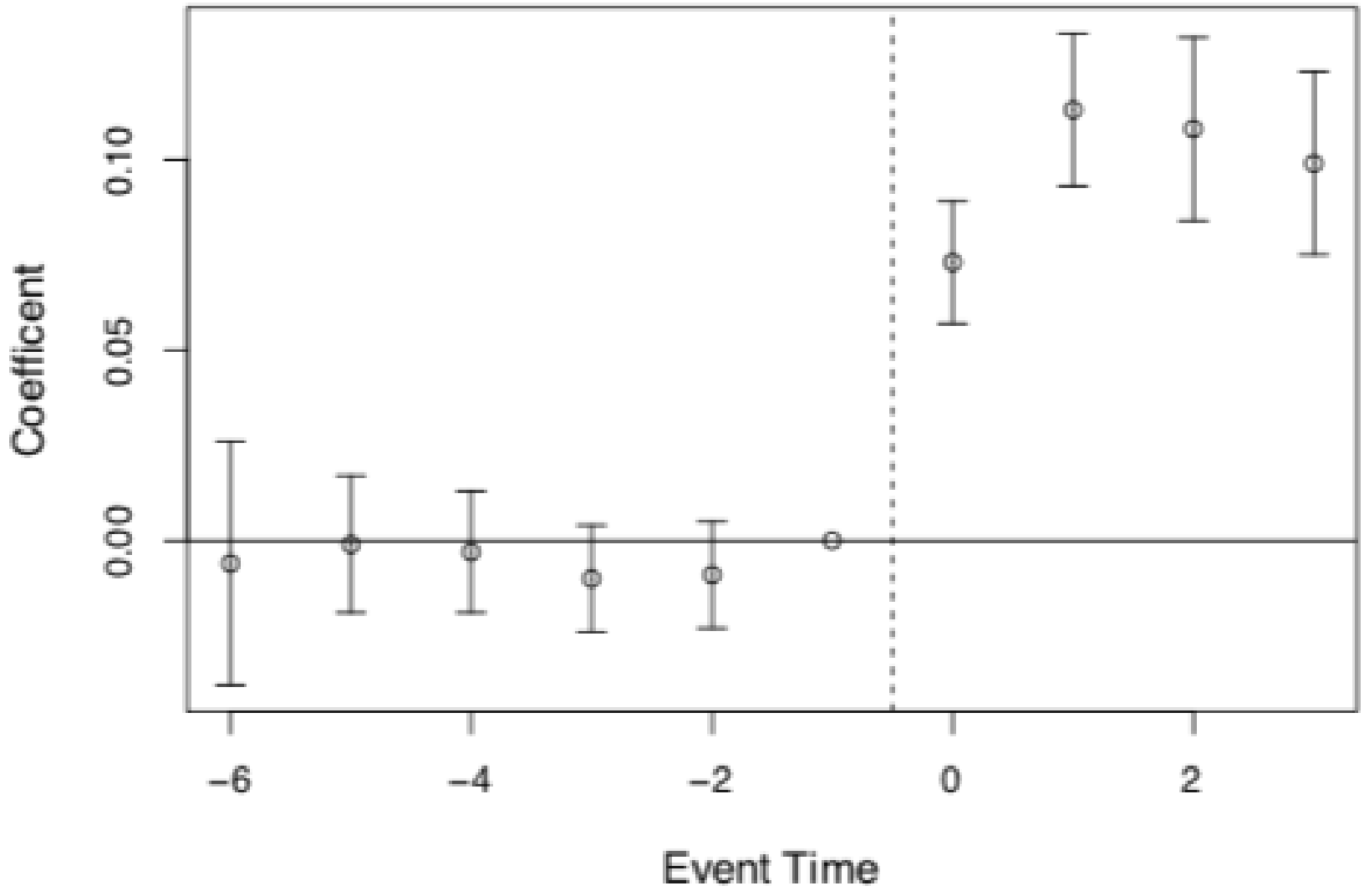
Issue Section: Article

Types of evidence

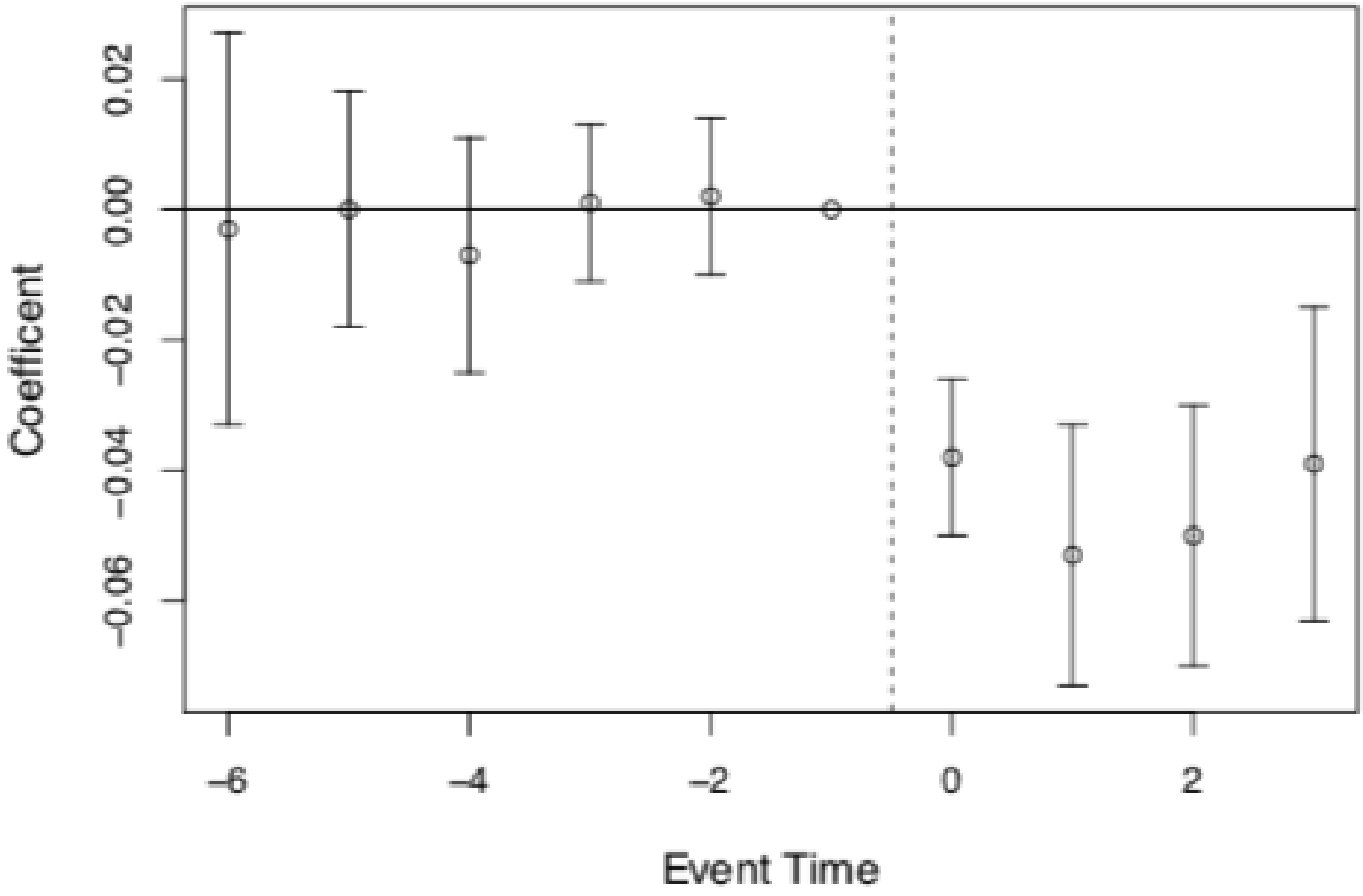
- **Bite** – show that the expansion shifted people into Medicaid and out of uninsured status
- **Main Results** – Show your main results (the point of the paper)
- **Placebos** – Show that there's no effect on mortality for groups it shouldn't be affecting (people 65+)
- **Mechanisms** – Find some reason explaining why the treatment affects the outcome via some "mechanism"
- **Event study** – Show leads and lags on mortality



(a) Medicaid Eligibility



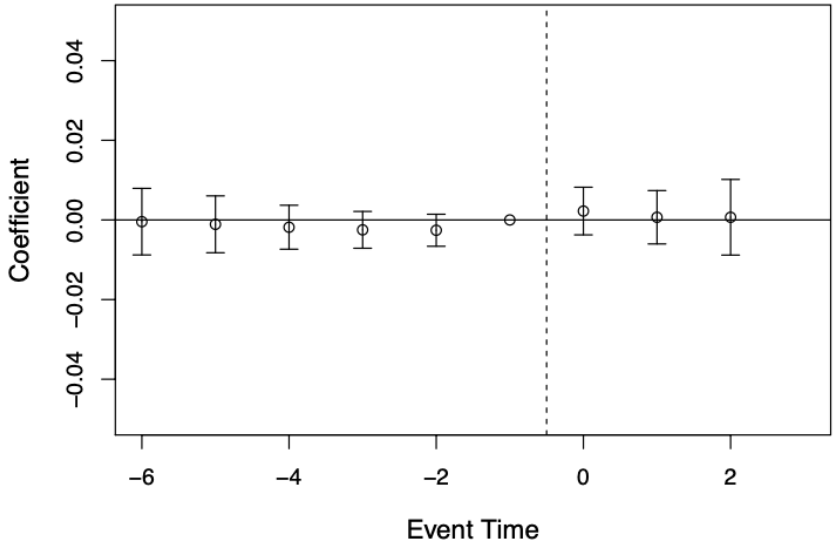
(b) Medicaid Coverage



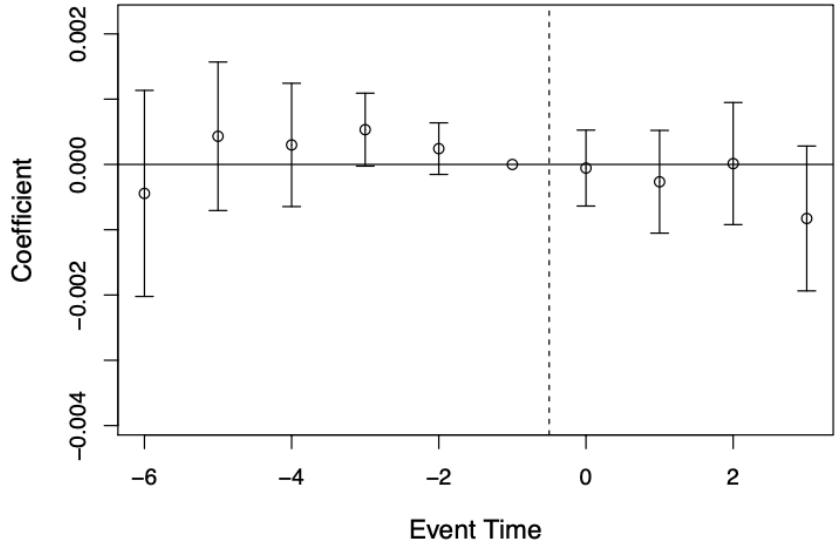
(c) Uninsured

Falsifications on elderly

Age 65+ in 2014



(c) Medicaid Coverage



(d) Annual Mortality

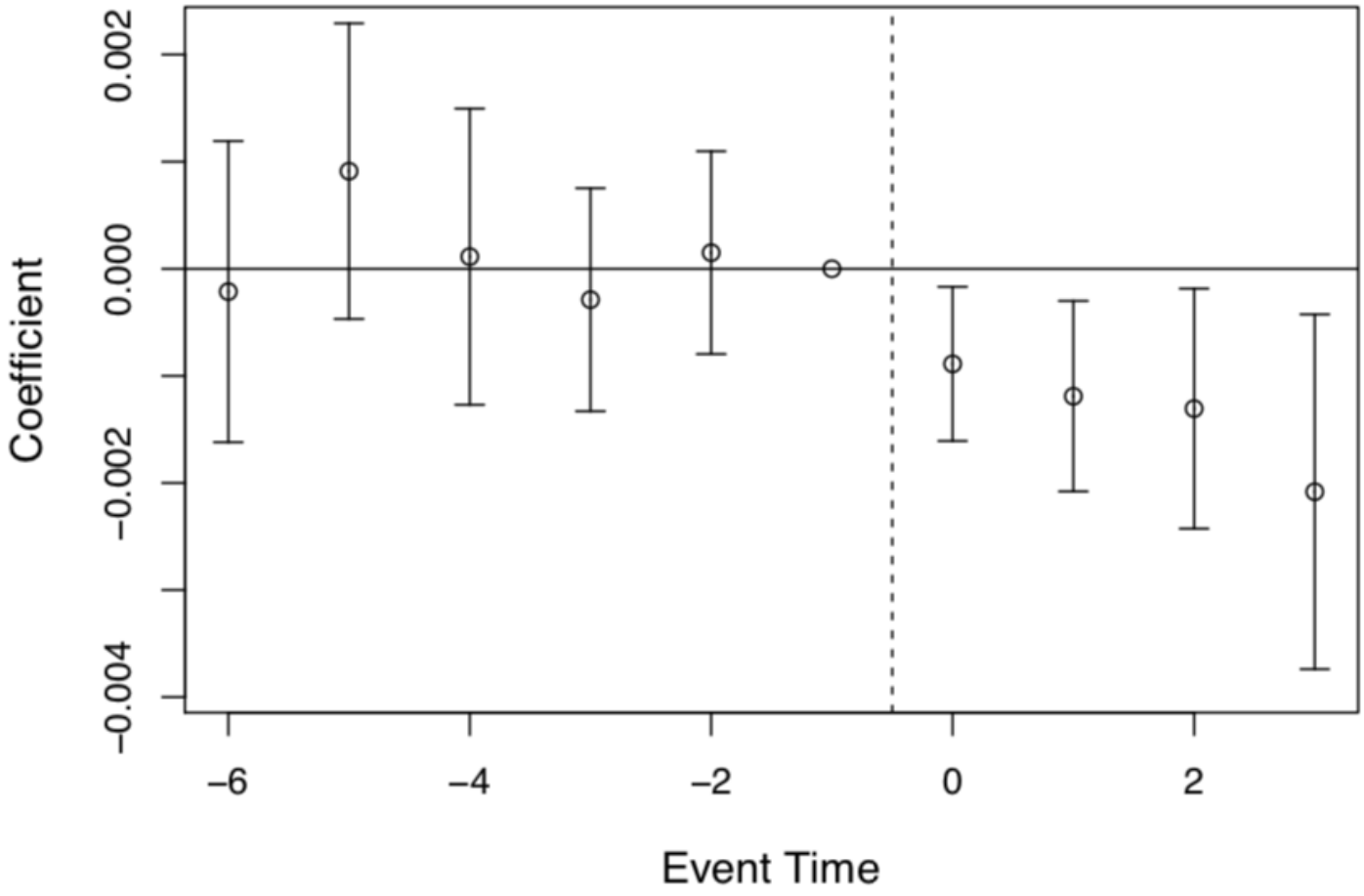


Figure: Miller, et al. (2019) estimates of Medicaid expansion's effects on on annual mortality

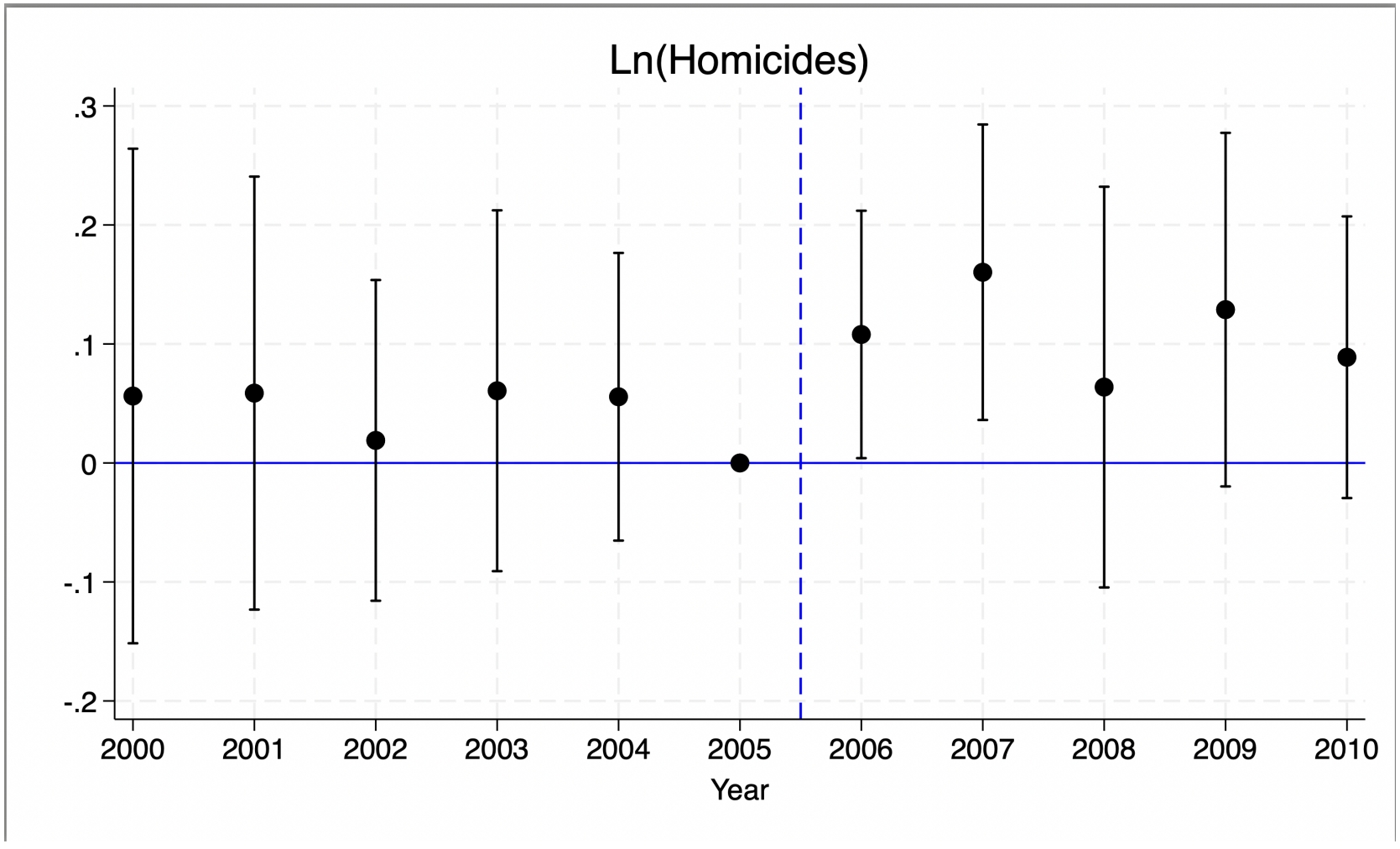
Mechanism

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

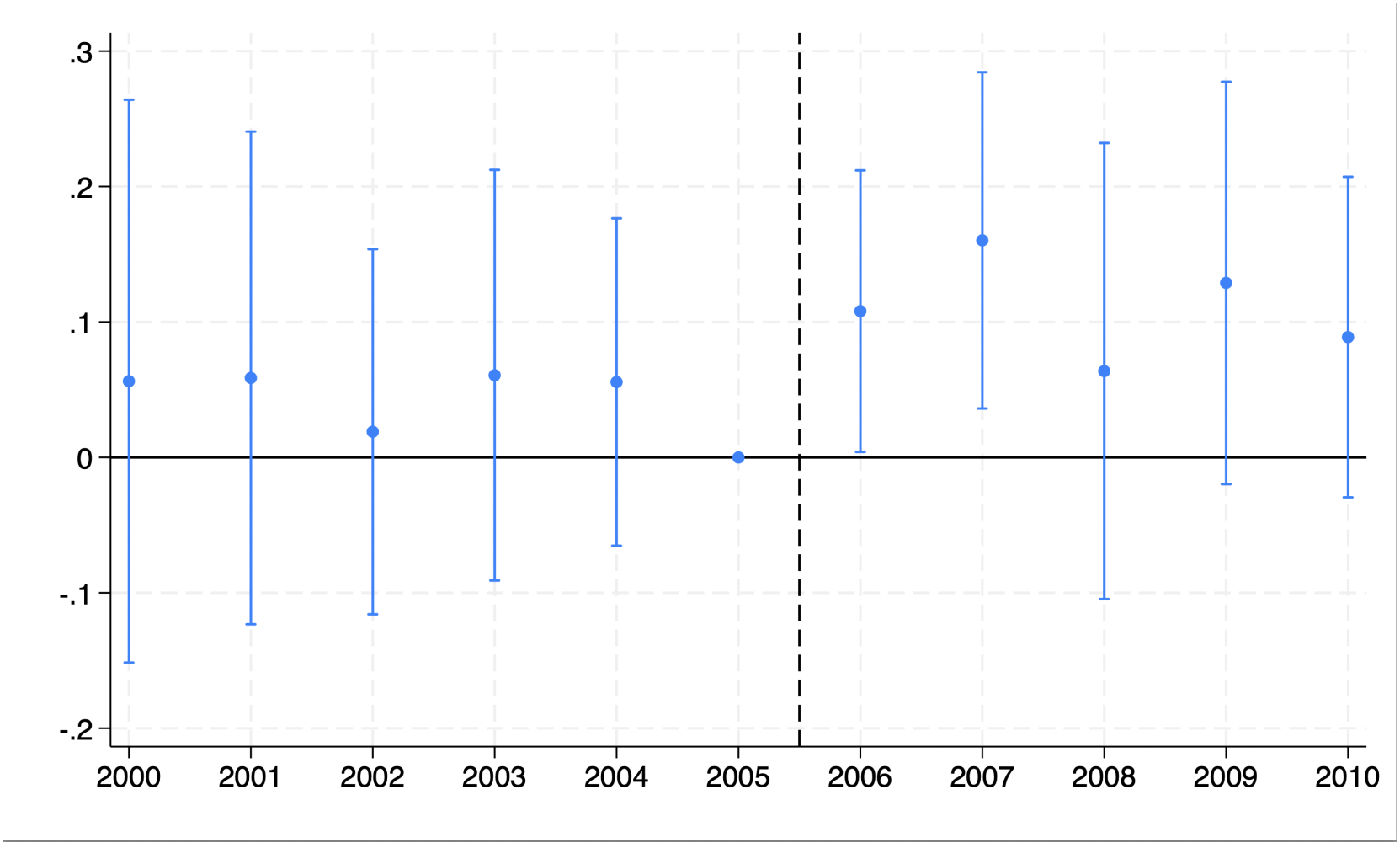
Making event study

- All the simple event study is is an interaction between the treatment group dummy and the calendar year dummies
- You must drop a $t - \tau$ as the baseline (e.g., $t - 1$) must be Y^0 untreated comparisons recall
- I have included in a do file that will do it for you either manually or using coefplot in `simple_eventstudy.do` at github labs

Manually creating the event study



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



Falsifications on more comparison groups

- Very common for readers and others to request a variety of “robustness checks” from a DD design
- We saw some of these just now (e.g., falsification test using data for alternative control group, the Medicare population)
- Triple differences is another way to do that, but it will have a somewhat different parallel trends assumption

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	

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

The ATT is D. Diff-in-diff is an unbiased estimate of D if $NJ_t = PA_t$. Call parallel trends.

Biased diff-in-diff #2

Table: Biased diff-in-diff #2: comparing groups

Groups	**Period**	**Outcomes**	D_1	D_2
Married women	Before	$Y = MW$		
	After	$Y = MW + MW_t + D$	$MW_t + D$	
				$D + (MW_t - SO_t)$
Single men and older women	Before	$Y = SO$		
	After	$Y = SO + SO_t$	SO_t	

$$\widehat{\delta}_{did} = D + (MW_t - SO_t)$$

Also unbiased estimate of D if $MW_t = SO_t$. A different parallel trends assumption.

Two biased diff-in-diffs

- If parallel trends of one doesn't hold, use the other, because both identify the ATT under their respective parallel trends
- But what if both of them are biased – then you can't use diff-in-diff because diff-in-diff requires at least one parallel trends
- Okay, but what if both biases are the same?

$$(NJ_t - PA_t) = (MW_t - SO_t)$$

- Then you can use triple differences, first developed by Gruber (1994)

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

- In Gruber's 1994 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	$NJ + MW + NJ_t + MW_t + D$	$NJ_t + MW_t + D$	$D + NJ_t + MW_t - (PA_t + MW_t)$	
		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	$NJ + SO + NJ_t + SO_t$	$NJ_t + SO_t$	$NJ_t + SO_t - (PA_t + SO_t)$	D
		Before	$NJ + SO$			
	Non-experimental states	After	$PA + SO + PA_t + SO_t$	$PA_t + SO_t$		
		Before	$PA + SO$			

Triple diff assumption

$$\widehat{\delta}_{DDD} = D + \Big((NJ_t + MW_t) - (PA_t + MW_t)\Big) - \Big((NJ_t + SO_t) - (PA_t + SO_t)\Big)$$

Recall that our first DiD was biased by $NJ_t - PA_t$ and our second DiD was biased by $MW_t - SO_t$.

Two biased diff-in-diff

$$
\begin{aligned}
&= [(NJ_t^{MW} + MW_t^{NJ}) - (PA_t^{MW} + MW_t^{PA})] - [(NJ_t^{SO} + SO_t^{NJ}) - (PA_t^{SO} + SO_t^{PA})] \\
&= (NJ_t^{MW} + MW_t^{NJ} - PA_t^{MW} - MW_t^{PA}) - (NJ_t^{SO} + SO_t^{NJ} - PA_t^{SO} - SO_t^{PA}) \\
&= NJ_t^{MW} + MW_t^{NJ} - PA_t^{MW} - MW_t^{PA} - NJ_t^{SO} - SO_t^{NJ} + PA_t^{SO} + SO_t^{PA} \\
&= NJ_t^{MW} - PA_t^{MW} - NJ_t^{SO} + PA_t^{SO} + MW_t^{NJ} - MW_t^{PA} - SO_t^{NJ} + SO_t^{PA} \\
&= (NJ_t^{MW} - PA_t^{MW} - NJ_t^{SO} + PA_t^{SO}) + (MW_t^{NJ} - SO_t^{NJ} - MW_t^{PA} + SO_t^{PA}) \\
&= [(NJ_t^{MW} - PA_t^{MW}) - (NJ_t^{SO} - PA_t^{SO})] - [(SO_t^{NJ} - MW_t^{NJ}) - (SO_t^{PA} - MW_t^{PA})] \\
&= \underbrace{[(NJ_t^{MW} - PA_t^{MW}) - (NJ_t^{SO} - PA_t^{SO})]}_{\text{Equally biased DiD \#1}} - \underbrace{[(SO_t^{NJ} - MW_t^{NJ}) - (SO_t^{PA} - MW_t^{PA})]}_{\text{Equally biased DiD \#2}}
\end{aligned}
$$

Triple differences requires two diff-in-diff, each 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 panel is now a group j state i (e.g., AR high wage worker 1991, AR high wage worker 1992, etc.)
- Assume we drop τ_t but I just want to show it to you for now.
- If the placebo DD is non-zero, it might be difficult to convince the reviewer that the DDD removed all the bias

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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First version received: 14 May 2020; final version accepted: 10 May 2021.

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'.

Falsification on outcomes

- The within-group control group (DDD) is a form of placebo analysis using the same *outcome*
- But there are also placebos using a *different* outcome – but you need a hypothesis of mechanisms to figure out what is in fact a *different outcome*
- Figure out what those are, and test them – finding no effect on placebo outcomes tends to help people your other results interestingly enough
- 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

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
- Ockham’s razor - given social interaction endogeneity (Manski 1993), homophily more likely explanation