Difference-in-Differences Estimation: Staggered Treatment Timing

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Motivation

- The same (or similar) policies can be adopted in different locales (or among different populations) at different times
- If multiple states/countries/individuals adopt the same policy at the same time, we can use our standard approach....but how can we handle the situation when there is not a clear "post" period?

An example

- Kong and Qin (2021)- "China's Anticorruption Campaign and Entrepreneurship"
- Does corruption hinder entrepreneurship?
- Authors want to exploit a series of anticorruption investigations to see if these probes have any effect on new business formation
  - These investigations are all from the same government initiative but occur in multiple years in different states
- The "post" period differs for different treated units

From Simultaneous to Staggered Adoption

For simultaneous adoption:

$$y = \alpha + \beta_1(post) + \beta_2(treatment\ \ group) + \beta_3(post * treatment\ \ group) + \mu$$

For staggered adoption:

$$y_{it} = \alpha_i + \lambda_t + \tau(treated_{it}) + \mu_{it}$$

Relating the two estimators:

$$\alpha_i \approx \beta_2(treatment\ \ group)$$
$$\lambda_t \approx \beta_1(post)$$
$$(treated_{it}) \approx (post * treatment\ \ group)$$

Two way fixed effects functional form

- Vector of unit fixed effects
  - Dummy variables for each unit (state/country/individual/etc)
  - These control for time-invariant unit-specific characteristics
- Vector of time fixed effects
  - Dummy variables for each time period
  - These control for unit-invariant time period-specific characteristics
  - Control for "global" shocks
- A treatment variable that varies within unit and across time
  - "Turns on" when a particular unit receives its initial treatment
  - Coefficient this estimates with be the "average treatment effect on the treated" (ATT)- or the estimated impact of a policy or intervention
- Time-varying covariates can also be included

Back to our Chinese Anticorruption example

- Provinces are our units
- Observations are yearly, with 2012 through 2016 covered
- Treatment indicator is equal to one once a corruption probe has been instigated for a particular province
- Outcome variable is the log of 1 plus the number of new enterprises per 10,000 people

$$Entrepreneurship_{it} = \alpha + \beta(Investigation)_{jt} + \gamma X_{it} + \mu_i + \lambda_t + \epsilon_{it} \quad (1)$$

Back to our Chinese Anticorruption example

Anticorruption Campaign and Entrepreneurship

	Without Controls	With Controls
InvestigationAft	.084**	.092**
	(7.733)	(8.291)
LnGDP		.183**
		(3.565)
GDP2%		.018**
		(4.735)
GDP3%		.015**
		(3.581)
CPI		.033**
		(4.608)
LnPopulation		−.938**
		(−13.184)
Adjusted R^2	.955	.957

Note. The dependent variable is Entrepreneurship. All regressions include year and county fixed effects and control for local economic level and other factors. The *t*-statistics reported in parentheses are based on standard errors clustered at the county level. $N = 18{,}721$.

** $p < .01$.

Assumptions

- Exogeneity of treatment
- Stable unit treatment value assumption (SUTVA)
- Parallel pre-treatment trends

Treatment Exogeneity

- Does some missing variable determine treatment status?
- Is treatment status correlated with the error term?
- Is treatment status effectively as good as random?
- Is the eventual treatment status correlated with the outcome variable in the pre-treatment periods?
- These are BIG QUESTIONS!
  - Validity of any quasi-experimental design in our causal inference world depends on the validity of this assumption

Testing for treatment Endogeneity

- Our Chinese Corruption paper does not address this issue
- Potential tests
  - Point biserial correlation test between error term and treatment variable
  - Correlation between outcome variable in pre-treatment periods and an indicator for treatment selection
    - Looking for selection into treatment
    - Can also be done with an event study (discussed later)
  - Demonstrate balance between control and treated observations in pre-treatment periods
  - Analytical arguments

If your treatment is endogenous.....

- Craft an argument for the direction of bias this introduces?
- Instrument for treatment status?
- Models built on latent-factor/ interactive fixed effects models (like Synthetic Difference-in-Differences) that allow for identification in the presence of unobserved time-variant global shocks or time-invariant unobserved unit characteristics (see Bai 2009, Arkhangelsky et al. 2021, Porreca 2022)

SUTVA

- "Stable Unit Treatment Value Assumption" (Rubin 1980)
- The treatment status of a particular unit is not correlated with that of other units
- The outcome for one unit only depends on it's own treatment status- not that of other
  - Treatment does not spill over to other units
- Typically we are left to analytical arguments here

Addressing SUTVA

- If displacement or spillovers exist, redefine the treatment to capture these displacements...
- Example
  - Porreca (2023) explicitly examines the spillover effects of the redevelopment of neighborhoods on violence in surrounding neighborhoods
  - Treatment and the units of analysis are redefined (into a network in this case) so that units are treated *if they're neighbors are treated*
- Corrections like this are simple, but do take some thought.
- How can we redesign or data and our treatment to capture these spillovers?

Parallel Pre-Treatment Trends

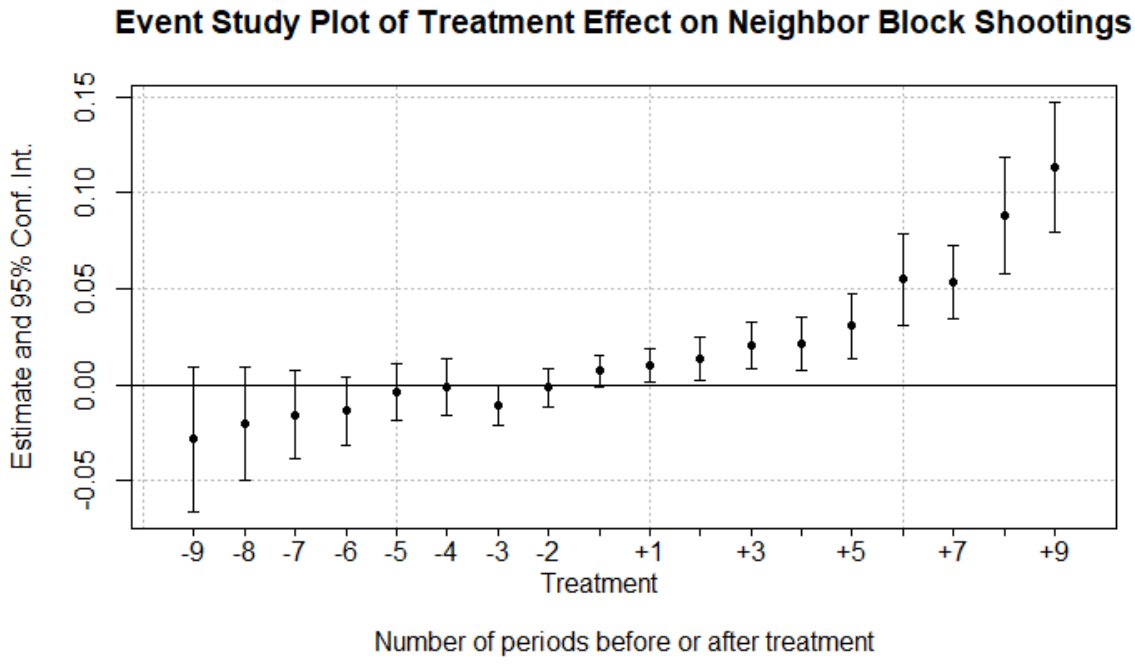
- Perhaps most important of assumptions
- In the absence of treatment, the evolution of both control and treatment group outcomes would be identical
- Harder to visualize in staggered setting
- $E(Y_{g,t}(0) - Y_{g,t-1}(0))$ does not vary across different g (From de Chaistemartin and Haultfoeuille (2022))

Event Studies and Testing for Violations

- Decompose treatment indicator into a series of treatment leads and lags
- Formally:
  - $$Y_{it} = \sum_{k=-K, k\neq -1}^{k=K} D_{it}^{k} \cdot \delta_k + \gamma_t + \psi_i + \epsilon_{it} \quad (2)$$
  - The coefficients of interest are δ_k
  - D_{it}^{k} represents a vector of dummy variables equal to one, if unit i in period t is k periods away from initial treatment
  - $k = 0$ in the initial treatment period
  - As in He and Wang (2017), $k = -1$ is omitted so that post-treatment event study estimators are relative to the period immediately before treatment.

Event Study Graph Example

Our example paper does not include a graph, so here is an example from Porreca (2023)



Possibilities for correction?

- New methods relying on latent factor models/interactive fixed effects like Bai 2009, Arkhangelsky et al. 2021, and Butts and Brown (2022), Porreca 2022 allow for identification with this assumption violated
- Standard OLS based DiD methods will fail to identify ATT with this assumption violated, however

Treatment Effect Heterogeneity

- Does the effect vary between units?
- Does the effect vary over time?
- Does the effect vary among treatment cohorts?

Basic logic of effect variation between units

- Not all units are the same, does the impact of the intervention change with that variation?
- Perhaps these differential effects are the parameter of interest?
- Porreca (2023) looks at effect of urban redevelopment on gun violence- of interest is how does that effect vary between high drug crime blocks and low drug crime blocks
- Decompose treatment effect between various types of units

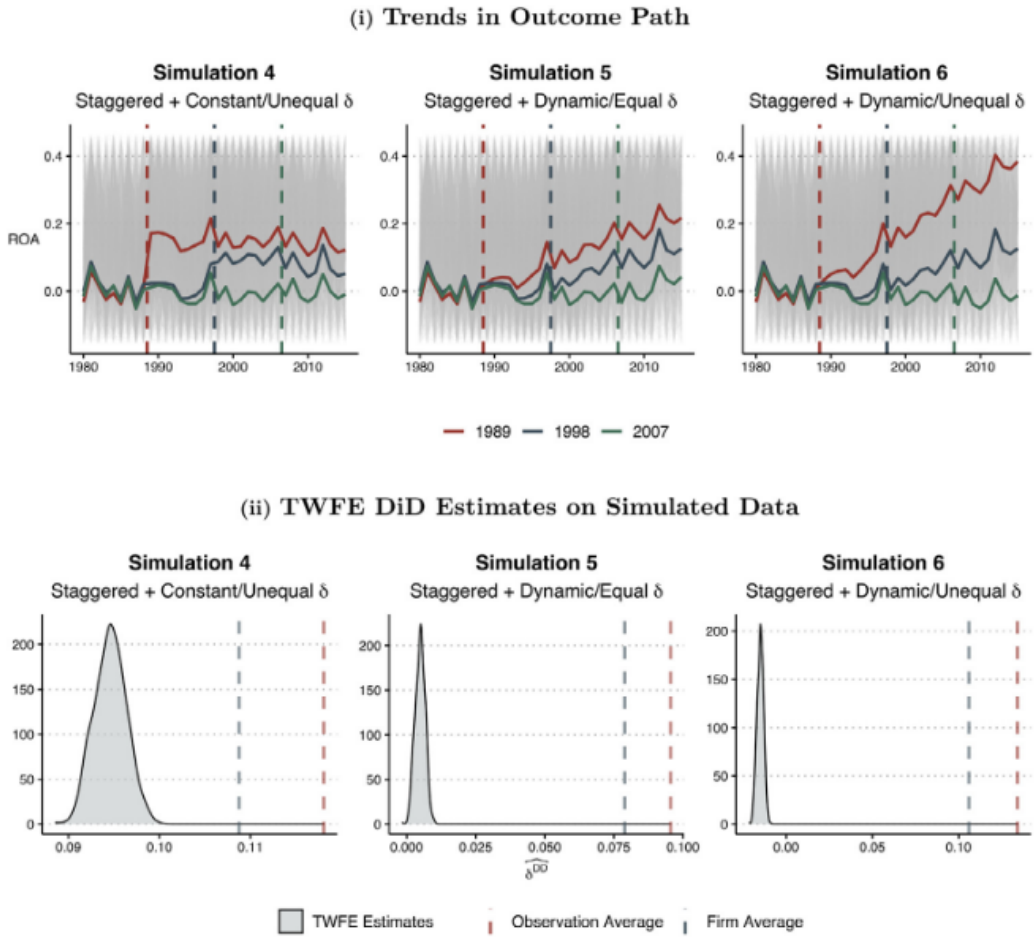
Strategy

- For example: two types of treated units
- $$y_{it} = \alpha_i + \lambda_t + \tau_1(\textit{type 1 treated}_{it}) + \tau_2(\textit{type 2 treated}_{it}) + \mu_{it}$$
- New treatment variables are interactions between treatment status and an indicator for which group of units the observation falls into
- The equality of the τ_i coefficients can be compared with a Wald Chi Square test
- Differences in effect size magnitude, significance, and sign between unit types can provide valuable information
- This same logic can easily be extended to more than two types

Basic Logic of Variation with Time or Cohort

- Great summary in Goodman-Bacon (2019): "So You've Been Told to Do My Difference-in-Differences Thing: A Guide"
- Staggered DiD estimator is a *weighted composite* of various 2x2 DiD estimators (two units, two time periods)
- Those weights come from size of the subgroups and effect size variance
- Treatment effects put units on different trends- This can introduce biases into those 2x2 estimates
- Staggered DiD is a "variance weighted average treatment effect" which is not necessarily the same as the average treatment effect on the treated
- "The dynamics of their treatment can curdle the milk and so we avoid it at all cost."

Illustration of Bias



Bacon Decomposition

- Tool to diagnose *which 2x2 estimates* matter the most in your DiD estimate
- Can show if the bulk of your estimate is being derived from *untreated versus treated units*, or if it is being derived from comparisons between units treated at different time periods
- Also, can provide weights that can be useful in de-biasing the TWFE-DiD estimate with removal of treatment timing comparisons
- Easy implementation in R or Stata

Callaway and Sant'Anna Estimator

- Estimate individual ATT for each treatment cohort- called aggregated ATT
- Each cohort's effect is estimated against groups who are never treated and groups who are *not yet treated*
- Cohort ATT's can be averaged together to provide a single estimate of the ATT (not the VWATT of TWFE)
- Also able to provide estimates of treatment effect variation with length of exposure
- Easy Stata or R implementation

Some Other Issues/Extensions

- Multiple Treatments
- Continuous Treatments
- Lack of valid control group

Continuous Treatment

- Logic is MUCH less intuitive here
  - Concept of dosage of treatment is crucial here
- Scott Cunningham blog link
- Related is Chaisemartin and D'Haultfeuille (2018): *Fuzzy* DiD
  - identification from changes in dosage when all units are partially treated and *treatment* group sees changes in dosage

Multiple Treatments

- What happens when there are multiple *different* treatments?
- Naive estimates suffer from "contamination" bias- other treatments' effects impact estimates of other treatments' effects
- Chaisemartin and D'Haultfeuille (2022)
- Suggested solution is to estimate treatments *separately* with subsamples of the data
  - Simple example: two treatments, multiple groups, staggered adoption, treatment one always precedes treatment two
  - Estimate treatment one's effect on the sub-sample for which treatment two is equal to zero
  - Estimate treatment two's effect on the sub-sample for which treatment one is equal to one

Summary

- Overview of the connection between standard DiD and the TWFE DiD estimator for staggered adoption
- Overview of the assumptions needed for this estimator to identify ATT (VWATT)
- Discussed basic issues with effect heterogeneity
- Outlines several extensions to the basic framework

Articles Referenced- Links

- Chaisemartin and D'Haulfeuille (2018)- Fuzzy Did
- Chaisemartin and D'Haulfeuille (2022)- TWFE Mutiple Treatments
- Callaway and Sant'Anna (2021)- Their estimator
- Goodman-Bacon (2021)- Bacon Decomposition
- Goodman-Bacon (2019)- Decomposition explanation
- Bai (2009)- Interactive Fixed Effects
- Arkhangelsky et al. (2021)-Synthetic Difference-in-Differences
- Porreca (2022)- Staggered SynthDiD
- Kong and Qin (2021)- Chinese anticorruption example
- Porreca (2023)- Staggered paper example

Additional Resources

- Roth et al. (2023)- Review of Recent DiD Literature
- Andrew Baker Youtube Video on DiD Issues and Solutions
- Baker et al. (2022)- Demonstrations of Bias in TWFE DiD

Questions/ Contact Info

Thank you! Please reach out to me via email at zachary.porreca@unibocconi.it or at @zachporreca on Twitter

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