

Difference-in-Differences

MIXTAPE SESSION



Roadmap

Imputation DiD

Imputation based robust estimator

2SDiD

Background

- First “new diff-in-diff” paper was Borusyak and Jaravel (2016) – a lot of what was simultaneously discovered elsewhere was in that paper
- We will discuss its successor – Borusyak, Jaravel and Spiess (2021)
- As a lot of this has already been elaborated on, I’m going to focus primarily on the estimator

My Outline (versus their outline)

1. Discussion of their interpretation of “basic” DiD assumptions
2. Critique of TWFE OLS when strong assumptions don't hold
3. Introduction of new assumptions
4. Robust efficient imputation estimator

ATT parameter

Estimation target:

$$\tau_w = \sum_{it \in \Omega_1} w_{it} \tau_{it} = w'_1 \tau$$

Weights need not add up to one. Weights could be $\frac{1}{N}$ for all $it \in \Omega_1$. We have a number of options.

A1: Parallel trends

Assumption 1: Parallel trends. There exist non-stochastic α_i and β_t such that:

$$Y_{it}(0) = \alpha_i + \beta_t + \varepsilon_{it}$$

with

$$E[\varepsilon_{it}] = 0$$

for all $it \in \Omega$. Can be extended (e.g., unit-specific trends). Only imposes restrictions on $Y(0)$, not treatment effects themselves. Notice how it is a TWFE assumption – it's actually the same data generating process as in baker.do.

A2: No anticipation

- No anticipation rules out anticipatory behavior that would cause treatment effects to materialize even before the treatment occurred:

$$Y_{it} = Y_{it}(0)$$

for all $it \in \Omega_0$.

- Notice how as an assumption, it literally imposes $\tau = 0$ for all pre-treatment periods.
- Even before Goodman-Bacon (2021), Sun and Abraham (2020) and Borusyak and Jaravel (2016), I had seen a million applied papers and only seen references to PT, not NA

A3: Restricted causal effects

This is the one that places restrictions on what treatment effects can and cannot be (i.e., homogenous treatment effects). Notice the very detailed expression:

Assumption 3 (Restricted causal effects): $B\tau=0$ for a known $M \times N_1$ matrix B of full row rank.

If we can assume something like homogenous treatment effects, then TWFE actually is best because its ability to *correctly* extrapolate will increase efficiency. But it's when A3 is not tenable or not really ex ante justified by theory that we should be worried. There's an A3' that is a slight modification.

Critique of Common Practice

1. Under-identification in event studies
2. Negative weighting
3. Spurious identification of long-run casual effects

Critique: Underidentification problem

Lemma 1: If there are no never-treated units, the path of [pre-treatment lead population regression coefficients] is not point identified in the fully dynamic OLS specification. In particular, adding a linear trend to this path $\{\tau_h + k(h + 1)\}$ for any $k \in R$ fits the data equally well with the fixed effects coefficients appropriately modified.

In english, it means you're going to have a multicollinearity problem even worse than you thought when estimating the fully dynamic event study model (i.e., dropping only one lead for all base comparisons)

Underidentification of lead coefficients

Under-identification problem

Formally the problem arises because a linear time trend t and a linear term in the cohort E_i (subsumed by the unit FEs) can perfectly reproduce a linear term in relative time $K_{it} = t - E_i$. Therefore a complete set of treatment leads and lags, which is equivalent to the FE of relative time, is collinear with the unit and period FEs.

Just one additional normalization is needed – drop $\tau_{-a} = 0$ and $\tau_{-1} = 0$. This will break the multicollinearity. We saw this in SA also. So multiple people saw this at the same time.

Under-identification and theoretical justifications

- Imposing any $-a$ lead and -1 lead to equal zero is somewhat ad hoc. Why those two and not some other two?
- Recall with SA – it mattered which ones you dropped because otherwise leads were contaminated
- This is again about NA – if you chose $-a$ and -1 , then you had some theoretical reason to assume NA held for them and not some other periods

Negative weighting and violations of A3

Assume some simple static model with a single dummy for treatment. Then they lay out a second lemma

Lemma 2: If A1 and A2 hold, then the estimand of the static OLS specification satisfies $\tau^{static} = \sum_{it \in \Omega_1} w_{it}^{OLS} \tau_{it}$ for some weights w_{it}^{OLS} that do not depend on the outcome realizations and add up to one $\sum_{it \in \Omega_1} = 1$.

The static OLS estimand cannot be interpreted as a “proper” weighted average, as some weights can be negative.

Simple illustration

Table: TWFE dynamics

$E(y_{it})$	$i = A$	$i = B$
t=1	α_A	α_B
t=2	$\alpha_A + \beta_2 + \delta_{A2}$	$\alpha_B + \beta_2$
t=3	$\alpha_A + \beta_3 + \delta_{A3}$	$\alpha_B + \beta_3 + \delta_{B3}$
Event date	$E_i = 2$	$E_i = 3$

Static: $\delta = \delta_{A2} + \frac{1}{2}\delta_{B3} - \frac{1}{2}\delta_{A3}$.

Notice the negative weight on the furthest lag. This is what you get when A3 is not satisfied..

Short-run bias of TWFE

- TWFE OLS has a severe short-run bias
- the long-run causal effect, corresponding to the early treated unit A and the late period 3, enters with a negative weight ($-1/2$)
- The larger the effects in the long-run, the smaller the coefficient will be
- It's caused by “forbidden comparisons” (late to early treated) – we saw this with Goodman-Bacon (2021)
- Forbidden comparisons create downward bias on long-run effects with treatment effect heterogeneity, *but not with treatment effect homogeneity* – so it really is an A3 violation

Spurious Long-Run Causal Effects

More A3 problems, this time finding long-run effects where there are none. Basically, you need to impose a lot of pre-trend restrictions to get estimates of long-run population regression coefficients. Even then you can't get them all.

OLS estimates are fully driven by unwarranted extrapolations of treatment effects across observations and may not be trusted unless strong ex ante justifications for A3 exist

Lemma 4: Suppose there are no never-treated units and let $H = \max_i E_i - \min_i E_i$. Then for any non-negative weights w_{it} defined over the set of observations with $K_{it} \geq \overline{H}$ (that are not identically zero), the weighted sum of causal effects $\sum_{it: K_{it} \geq \overline{H}} w_{it} \tau_{it}$ is not identified by A1 and A2.

Modifications of general model

Modification of A1 to A1':

$$Y_{it}(0) = A'_{it}\lambda_i + X'_{it}\delta + \varepsilon_{it}$$

Assumption 4 is introduced (homoskedastic residuals). This is key, because they will be building an “efficient estimator” with BLUE like OLS properties.

Using A1' to A4, we get the “efficient estimator” which is for all linear unbiased estimates of δ_W , the unique efficient estimator $\widehat{\delta_W^*}$ can be obtained with 3 steps

Role of the untreated observations

"At some level, all methods for causal inference can be viewed as imputation methods, although some more explicitly than others." – Imbens and Rubin (2015)

"The idea is to estimate the model of Y_{it}^0 using the untreated observations and extrapolate it to impute Y_{it}^0 for treated observations."

Steps

1. Estimate expected potential outcomes using OLS and only the untreated observations (this is similar to Gardner 2021)
2. Then calculate $\hat{\delta}_{it} = Y_{it}^1 - \hat{Y}_{it}^0$
3. Then estimate target parameters as weighted sums

$$\hat{\delta}_W = \sum_{it} w_{it} \hat{\delta}_{it}$$

Why is this working?

- Think back to that original statement of the PT assumption – you're modeling $Y(0)_{it}$.
- That is, without treatment – so the potential outcomes do not depend on any treatment effect
- Hence where we get treatment heterogeneity
- We obtain consistent estimates of the fixed effects which are then used to extrapolate to the counterfactual units for all $Y(0)_{it \in \Omega_1}$
- I think this is a very cool trick personally, and as it is still OLS, it's computationally fast and flexible to unit-trends, triple diff, covariates and so forth (though remember what we said about covariates)

Comparisons to other estimators

Table 3: Efficiency and Bias of Alternative Estimators

Horizon	Estimator	Baseline simulation		More pre-periods	Heterosk. residuals	AR(1) residuals	Anticipation effects
		Variance (1)	Coverage (2)	Variance (3)	Variance (4)	Variance (5)	Bias (6)
$h = 0$	Imputation	0.0099	0.942	0.0080	0.0347	0.0072	-0.0569
	DCDH	0.0140	0.938	0.0140	0.0526	0.0070	-0.0915
	SA	0.0115	0.938	0.0115	0.0404	0.0066	-0.0753
$h = 1$	Imputation	0.0145	0.936	0.0111	0.0532	0.0143	-0.0719
	DCDH	0.0185	0.948	0.0185	0.0703	0.0151	-0.0972
	SA	0.0177	0.948	0.0177	0.0643	0.0165	-0.0812
$h = 2$	Imputation	0.0222	0.956	0.0161	0.0813	0.0240	-0.0886
	DCDH	0.0262	0.958	0.0262	0.0952	0.0257	-0.1020
	SA	0.0317	0.950	0.0317	0.1108	0.0341	-0.0850
$h = 3$	Imputation	0.0366	0.928	0.0255	0.1379	0.0394	-0.1101
	DCDH	0.0422	0.930	0.0422	0.1488	0.0446	-0.1087
	SA	0.0479	0.952	0.0479	0.1659	0.0543	-0.0932
$h = 4$	Imputation	0.0800	0.942	0.0546	0.3197	0.0773	-0.1487
	DCDH	0.0932	0.950	0.0932	0.3263	0.0903	-0.1265
	SA	0.0932	0.954	0.0932	0.3263	0.0903	-0.1265

Notes: See Section 4.6 for a detailed description of the data-generating processes and reported statistics.

Returning to the minimum wage

- Clemens and Strain (2021) implemented the BJS imputation estimator to estimate the effect of the minimum wage (post Great Recession) on employment
- One comment abt the following graphics: BJS procedure does not have a “base” period in the same sense as the regression models do because it is not contrasting each period relative to some omitted group
- Since it is imputing counterfactuals, we can calculate each period’s effect

BJS Results

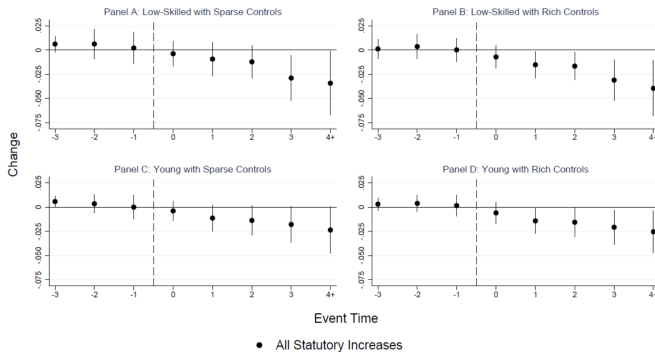


Figure 11. Event Studies of Changes in Employment Following Statutory Minimum Wage Increases Using the BJS Imputation Estimator: This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel and Spiess (2021) (BJS). For the BJS estimator, we code the first treatment year as the year in which a state's first statutory minimum wage increase took effect. Note that this appears graphically as "year 0" in the BJS figures, but corresponds with year 1 in the stacked event study figures. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with "sparse controls" include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with "rich controls" include all controls in the base controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.

BJS Results

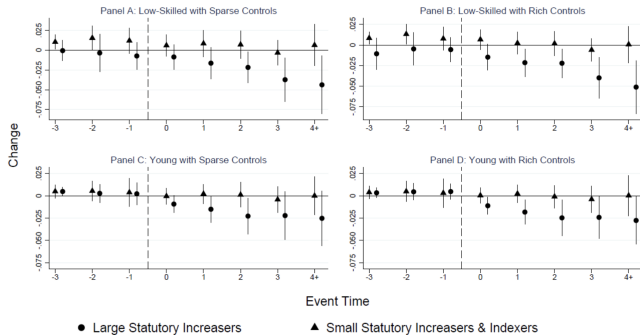


Figure 12. Event Studies of Changes in Employment Following Large and Small Statutory Minimum Wage Increases Using the BJS Imputation Estimator: This figure displays coefficients obtained using the imputation estimator proposed by Borusyak, Jaravel and Spiess (2021) (BJS). For the BJS estimator, we code the first treatment year as the year in which a state's first statutory minimum wage increase took effect. Note that this appears graphically as "year 0" in the BJS figures but corresponds with year 1 in the stacked event study figures. We compare estimates for large vs. small increases as defined in the main text. Panels A and B plot coefficients for low-skilled individuals defined as individuals ages 16–25 without a completed high school education. Panels C and D plot coefficients for young individuals defined as all individuals ages 16–21. The samples are from the ACS. Regressions with "sparse controls" include state and year fixed effects, as well as the log of annual average *per capita* income and the annual average state house price index used in our main regressions. Regressions with "rich controls" include all controls in the base controls regressions plus the three-year lag of log *per capita* income and the house price index, as well as a dummy variable for each education group and age. Error bars denote 95 percent confidence intervals around each estimated coefficient. Standard errors are clustered by state.

Comments abt the minimum wage study

- Elasticity of employment with respect to minimum wage is -0.124 and -0.082 for those without high school and the young, respectively
- Differences by size of minimum wage increase:
 - Large increases (around \$2.90): own-wage elasticity is -1.01 for 16-25yo with less than HS and -0.41 for 16 to 21yo (large effects)
 - Small increases (around \$1.90): own-wage elasticity is 0.46 (i.e., no employment effects)
 - Inflation-index increases (around \$0.90): own-wage elasticity is 0.16 (no effect) and -0.17 (no effect)

Two Stage DiD

"It seems natural that TWFE should identify the ATT" – Gardner (2021)

It just seems like TWFE with a DiD will estimate the ATT with weights that we'll find intuitive. Was this just a conjecture and was never true? Why isn't this working?

Two Stage DiD

- Why does TWFE fail under differential timing? Violates strict exogeneity under heterogeneity
- The logic of the failure suggests an obvious, but previously unknown, solution which is the 2SDiD
- I'll explain 2SDiD, focus on the parallel trends implications, and show we can get a consistent and unbiased estimate of group and relative time fixed effects
- If you can get consistent and unbiased estimates of group and relative time fixed effects, then you can delete them and run normal analysis

Background

- By now, we all agree that TWFE just doesn't handle heterogeneity under differential timing very well
- We've seen in the Goodman-Bacon decomposition why – it's caused by TWFE implicitly calculating late to early 2x2s, which are a source of bias
- But some of you are coming straight from a panel econometrics course that maybe didn't use potential outcomes notation
- Isn't strict exogeneity enough for consistent estimates? What then does strict exogeneity have to do with heterogeneity and differential timing?

High level discussion

- TWFE identifies the ATT when the heterogeneous effects are distributed equally across all groups and periods, but since that is a knife-edge situation, it is likely that TWFE will not in our applications meet this special scenario
- In the two group case, that is what happens though which is why TWFE worked fine there
- Metaphorically, the two group case that we always used to pin our intuition of what DiD was doing was the exception not the rule
- Goodman-Bacon (2021) shows the problem is caused by late-to-early comparisons; Gardner (2021) will show that the problem is misspecification
- Think of these as different perspectives on the same problem

Model misspecification

“Misspecified DiD regression models project heterogenous treatment effects onto group and period fixed effects rather than the treatment status itself”

Spoiler: This analysis of the problem suggests solution – why don't we remove those?

2SDiD

- First stage – estimate the group and relative time fixed effects using only the $D = 0$ observations
- Second stage – using predicted values based off those fixed effect coefficients, run your model off the transformed outcome
- Get the standard errors right just like 2SLS by taking the first stage into account (uses GMM)

More high level

- The second step recovers the average difference in outcomes between treated and untreated units after removing group and period fixed effects
- Strong parallel trends assumption compared to CS and SA, but unclear if this is a big deal in general

Notation

i : panel units

t : calendar time – think of real dates

$g \in \{0, 1, \dots, G\}$ – groups

$p \in \{0, 1, \dots, P\}$ – relative time or “periods”

Periods are successive. Group 0 – never treated. Group 1 – treated in period 1, 2, and on. Group 2 – treated in period 2, etc.

Parameters

$$\beta_{gp} = E \left[Y_{gpit}^1 - Y_{gpit}^0 | g, p \right]$$

It's a group-time ATT but expressed in a more traditional econometric notation that you could easily find in Wooldridge or some such

Modeling basics

Under parallel trends, mean outcomes will satisfy the following equation

$$E\left[Y_{gpit}|g, p, D_{gp}\right] = \lambda_g + \gamma_p + \beta_{gp}D_{gp}$$

In two-group, group and period effects are eliminated with dummies because TWFE uses dummies to demean across multiple dimensions. Then TWFE identifies ATT. But this does not hold when average effects vary across group and period. There are many ways to express a treatment effect's across group and time, but Gardner presented it as a weighted average of the coefficients for only that group-period situation:

$$E\left(\beta_{gp}|D_{gp} = 1\right) = E\left(Y_{gpit}^1 - Y_{gpit}^0|D_{gp} = 1\right)$$

Strict exogeneity violation

Rewriting the above we get:

$$E\left[Y_{gpit}|g, p, D_{gp}\right] = \lambda_g + \gamma_p + E\left[\beta_{gp}|D_{gp} = 1\right]D_{gp} \\ \left[\beta_{gp} - E(\beta_{gp}|D_{gp} = 1)\right]D_{gp}$$

The problem is there's this weird new error term and it isn't mean zero under heterogenous treatment effects spread across group and period. Unlike the two group case, the coefficient on D_{gp} from TWFE doesn't identify the average $E(\beta_{gp}|D_{gp} = 1)$

So let's see Gardner's solution, but note – his solution was suggested by the problem itself. Gardner is thoughtful and observant.

DiD regression estimand

- So if TWFE isn't recovering $E(\beta_{gp}|D_{gp} = 1)$, then what is it recovering?
- He shows that under PT, the coefficient on D_{gp} is:

$$\beta^* = \sum_{g=1}^G \sum_{p=g}^P w_{gp} \beta_{gp}$$

- So then – what are the weights w_{gp} ?
- Groan – It's a huge mess, and I hate even showing it to you because I find the weights almost impossible to decipher, but maybe you'll have a better go at it than me

Weights

$$w_{gp} = \frac{\left\{ [1 - P(D_{gp} = 1|g)] - [P(D_{gp} = 1|p) - P(D_{gp} = 1)] \right\} P(g, p)}{\sum_{g=1}^G \sum_{p=g}^P \left\{ [1 - P(D_{gp} = 1|g)] - [P(D_{gp} = 1|p) - P(D_{gp} = 1)] \right\} P(g, p)}$$

Terms:

- $P(D_{gp} = 1|p)$: share of units treated in period p
- $P(D_{gp} = 1|g)$: share of periods in which g is treated
- $P(D_{gp} = 1)$: share of unit \times time treated
- $P(g, p)$: population share of observation corresponding to group g and period p

I thought about changing all those probabilities into means, but honestly, it really didn't help me at all. But Gardner notes that this is from theorem 1 of deChaisemartin and D'Haultfoeiller (2020) and his Appendix A

Estimation

$$Y_{gpit} = \lambda_g + \gamma_p + \beta D_{gp} + \varepsilon_{gpit}$$

This specification assumes a conditional expectation function that is linear in group, period and treatment status. But when the model is misspecified, it will attribute some of the heterogeneity impacts of the treatment to group and period fixed effects. The longer the treatment, the greater \overline{D} is, the more that group's treatment effects will be absorbed by group fixed effects. When misspecified, TWFE doesn't recover $E[\beta|D = 1]$.

Statistical issues

- Common support: “as long as there are untreated and treated observations for each group and period, λ_g and γ_p are identified from the subpopulation of untreated groups and periods.”
- Identification: “the overall group \times period ATT is identified from a comparison of mean outcomes between treated and untreated groups after removing group and period effects.”

Estimation: First stage

First stage:

$$Y_{gpit} = \lambda_g + \gamma_p + \varepsilon_{gpit}$$

using only $D_{gp} = 0$, retaining the fixed effects. Collect the $\widehat{\lambda}_g$ and $\widehat{\gamma}_p$.

Estimation: Second stage

Second stage:

$$\begin{aligned}\hat{y}_{gpit} &= y_{gpit} - \hat{\lambda}_g - \hat{\gamma}_p \\ \hat{y}_{gpit} &= \alpha + \beta D_{gp} + \psi_{gpit}\end{aligned}$$

Why does this work? Parallel trends assumption implies:

$$E(y_{gpit}|g, p, D_{gp}) - \lambda_g - \gamma_p = E\left[\beta_{gp}|D_{gp} = 1\right]D_{gp} + \left[\beta_{gp} - E(\beta_{gp}|D_{gp} = 1)\right]D_{gp}$$

But because

$$E\left\{[\beta_{gp} - E(\beta_{gp}|D_{gp} = 1)]D_{gp}|D_{gp}\right\} = 0$$

Estimand

Then this procedure will identify $E(\beta_{gp}|D_{gp} = 1)$. Consistency and unbiasedness proofs.

This is $E(\beta_{gp}|D_{gp} = 1) = \sum^G \sum^P \beta_{gp} P(g, p|D_{gp} = 1)$. It will tend to put more weight, by definition, on groups earlier into their treatment. But this isn't the same as the negative weighting that BJS say occurs of the long lags. It just means there are more of them.

Event studies are:

$$y_{gpit} = \lambda_g + \gamma_p + \sum_{r=-R}^P \beta_r D_{rgp} + \varepsilon_{gpit}$$

Just change the second stage with the transformed outcome.

Inference

- Standard errors are wrong on the second stage because the dependent variable uses estimates obtained from the first stage.
- The asymptotic distribution of the second stage can be obtained by interpreting the two-stage procedure as a joint GMM

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