

# Causal Inference II

*MIXTAPE SESSION*

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# Roadmap

Imputation DiD

Imputation based robust estimator

2SDiD

Example: Facebook and Mental Health

Concluding remarks

# Background

- The origin of the robust diff-in-diff papers identifying pathologies in TWFE was Borusyak and Jaravel (2016) working paper
- Both problems with static and dynamic specifications were discussed, and the identification of the “already treated” as controls
- Paper remained in working paper until 2021 when Jan Speiss was brought on; the team developed a new estimator
- We will focus primarily on the estimator, to avoid redundancies

# My 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 will be unit level treatment effects aggregated to a higher level like the ATT:

$$\tau_w = \sum_{it \in \Omega_1} w_{it} \tau_{it} = w_1' \tau$$

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

# Standard TWFE Assumptions

1. Parallel trends – expressed as a TWFE model
2. No anticipation
3. Homogenous treatment effects

# A1: Parallel trends

**Assumption 1: Parallel trends.** There exist non-stochastic  $\alpha_i$  and  $\beta_t$  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.



## A2: No anticipation

- Caveat: No Anticipation was never mentioned until recently
- It's crucial for the pre-trends to be zero, far more than parallel trends, as parallel trends is *only* about post-treatment from baseline, whereas event studies are about pre-trends and that's where no anticipation reigns

## 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	$\alpha_A$	$\alpha_B$
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  $\delta_W$ , 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

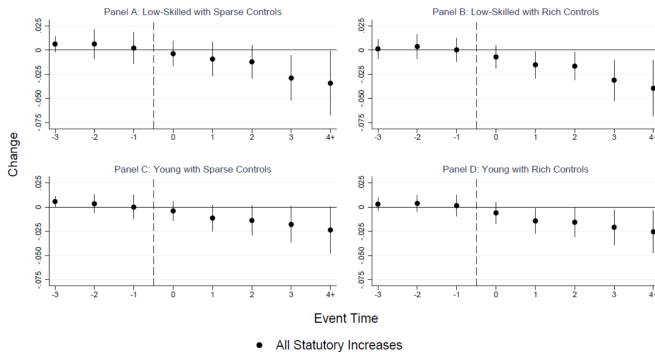
*Notes:* See Section 4.6 for a detailed description of the data-generating processes and reported statistics.

# 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

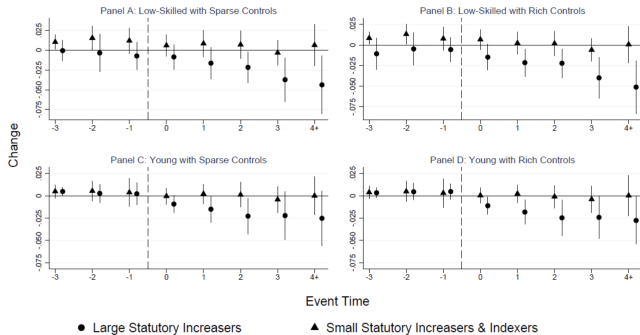


# BJS Results



**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  $w_{gp}$ ? They are variance weights

# 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,  $\lambda_g$  and  $\gamma_p$  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

# Roadmap

Imputation DiD

Imputation based robust estimator

2SDiD

Example: Facebook and Mental Health

Concluding remarks

# Bringing them together

- Examine a paper that is contemporary with respect to all that we've reviewed
- Staggered rollout, important question, high quality data
- Braghieri, Levy and Makarin (2022), "Social Media and Mental Health", *American Economic Review*, 112(11): 3660-3693

# Big picture

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



# Many things to like

- Important question: mental health, suicide, review descriptive stats together
- Strong design: staggered rollout
- Event study is eye popping
- Lots of “applied wisdom” we haven’t discussed such as mechanism
- Very interesting dataset

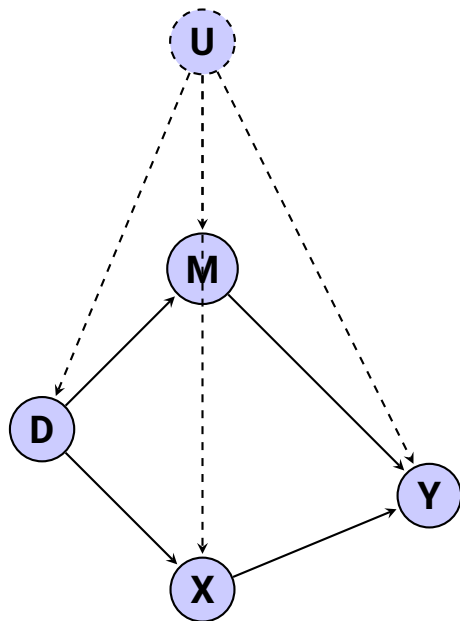
# Read together

- We will read together and discuss most of the introduction now

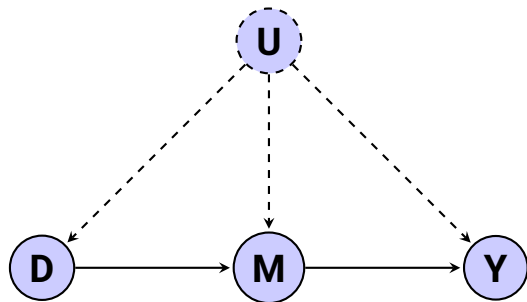
## Fourth part of a strong DiD

1. **Bite**: They cannot really show much here. No data on Facebook usage. More an ITT
2. **Falsifications**: I can't really see very strong falsifications either.
3. **Event studies**: POW. Just wait
4. **Mechanism**: What's that?

# Mechanism



# Mechanism



# Mechanism

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

# Mechanism

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

# Data

- Read Section II together.
- Discuss “shoeleather” concept
- You must go the distance to get the data because if you don’t, someone else will



TWFE

$$Y_{icgt} = \alpha_g + \delta_t + \beta \times Facebook_{gt} + X_i \times \gamma + X_c \times \psi + \varepsilon_{icgt} \quad (1)$$

Read page 3671

TABLE 1—BASELINE RESULTS: INDEX OF POOR MENTAL HEALTH

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

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

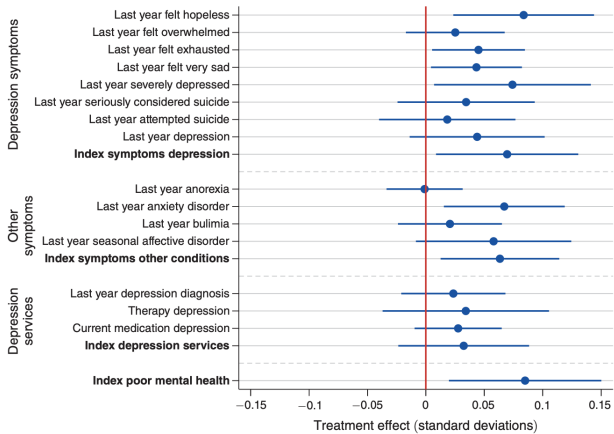


FIGURE 1. EFFECTS OF THE INTRODUCTION OF FACEBOOK ON STUDENT MENTAL HEALTH

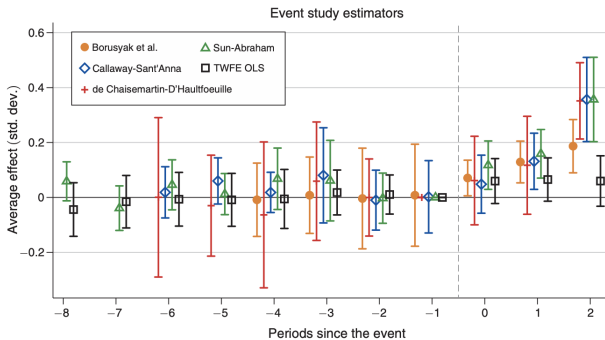


FIGURE 2. EFFECTS OF FACEBOOK ON THE INDEX OF POOR MENTAL HEALTH BASED ON DISTANCE TO/FROM FACEBOOK INTRODUCTION

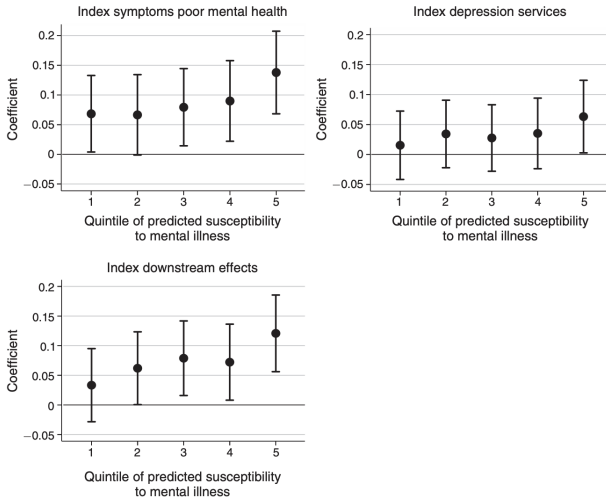


FIGURE 3. HETEROGENEOUS EFFECTS BY PREDICTED SUSCEPTIBILITY TO MENTAL ILLNESS

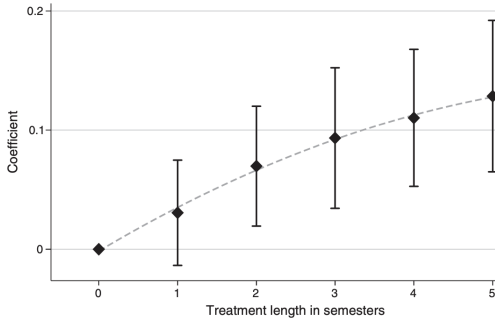


FIGURE 4. EFFECT ON POOR MENTAL HEALTH BY LENGTH OF EXPOSURE TO FACEBOOK

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

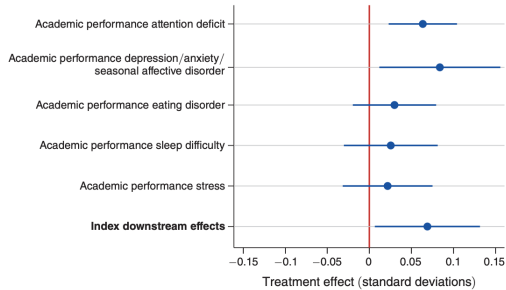


FIGURE 5. DOWNSTREAM EFFECTS ON ACADEMIC PERFORMANCE

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

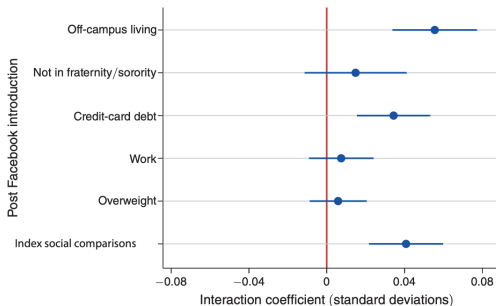


FIGURE 6. HETEROGENEOUS EFFECTS AS EVIDENCE OF UNFAVORABLE SOCIAL COMPARISONS

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



# Roadmap

Imputation DiD

Imputation based robust estimator

2SDiD

Example: Facebook and Mental Health

Concluding remarks

# Summarizing

- Difference-in-differences is very popular; it is often a go-to procedure for studying large policies and demand for staggered adoption will remain
- Canonical panel modeling of TWFE can with dynamic treatment effects flip the sign; will it is another matter, but it can
- Treatment effects can be positive, and yet TWFE reports a negative effect
- Caused by non-transparent use of already treated units as controls

# Summarizing

- Even if in your situation that didn't happen, how would you know without making strong prior statements about heterogeneity
- This requires *a priori* knowledge of opaque production functions that are simply not available to most researchers
- Generalizability is also an issue because the weights that TWFE uses are not the weights most likely what the policymaker needs (“variance weighted ATT”)

# Summarizing

- My recommendation is to eschew TWFE and use more transparent and robust methods when facing staggered adoption, and pay careful attention to how each handles covariates
- Know who your comparison group is, and how each underlying building block aggregates (i.e., weights chosen)

# Summarizing

- Core assumptions of difference-in-differences is, apart from no anticipation and SUTVA, the parallel trends assumption
- It is not testable; people often check for it through falsifications, including the pre-treatment event study which under no anticipation leaves only *that period's* differential trend
- Even under robust diff-in-diff methods, pre-treatment trends can fail, which would simply reflect that the treatment and control group are diverging even before the intervention

# Summarizing

- But what if parallel trends simply does not become believable?
- This may happen because of the event study – the *aggregate* comparison group is simply not a proxy for  $E[\Delta Y^0 | D = 1]$ , the counterfactual
- Then it may be time to shift gears and move into the synthetic control methods as they do not rely on parallel trends