Difference-in-differences design

Tutorial 5

Stanislav Avdeev

Goal for today's tutorial

1. Discuss and estimate a basic framework of DiD as two-way fixed effects estimator
2. Discuss and estimate an event-study specification
3. Discuss how to make proper inferences about DiD estimates
4. Discuss parallel trend assumption, pre-trend tests, and the connection between the two

DiD as two-way fixed effects

- The basic idea is to take fixed effects and then compare the **within variation** across groups
  - a **treated** group: individuals who get treated
  - a **control** group: individuals who do not get treated
  - we need to observe them both **before** and **after** they (don't) get their treatment
- Eventually, we want to estimate **within variation** for groups
  - control for time effects
  - control for group effects
  - compare within variation across groups
  - sounds like a job for the fixed effects estimator
- The question DiD tries to answer is "what was the effect of some policy on the people who were affected by it?"
  - so, DiD estimates ATET under constant treatment effect assumption with two periods only

DiD as two-way fixed effects

- We can estimate a standard DiD using the following formula

$$Y_{gt} = \alpha_t + \eta_g + \delta D_{gt} + U_{gt}$$

  where

  - α_t is the common time trend
  - η_g is the group specific effect
  - D_{gt} is is an interaction term equal to 1 if you are in the treated group in the post-treatment period
  - δ is the DiD estimate

DiD as two-way fixed effects

- If you have only two groups and two time periods you can present this regression as follows

$$Y_{gt} = \beta_0 + \beta_1 \boldsymbol{Post_t} + \beta_2 \boldsymbol{Treated_g} + \beta_3 \boldsymbol{Post_t} \times \boldsymbol{Treated_g} + U_{gt}$$

  where

  - $\boldsymbol{Post_t}$ is a binary variable equal to 1 if you are in the post-treatment period
  - $\boldsymbol{Treated_g}$ is a binary variable equal to 1 if you are in the treated group
  - $\boldsymbol{Treated_g} \times \boldsymbol{Post_t}$ is an interaction term equal to 1 if you are in the treated group in the post-treatment period

DiD as two-way fixed effects

- How can we interpret the estimated coefficients?

$$Y_{gt} = \beta_0 + \beta_1 Post_t + \beta_2 Treated_g + \beta_3 Post_t \times Treated_g + U_{gt}$$

- β_0 is the prediction when $Post_t = 0$ and $Treated_g = 0$
  - β_0 is the **mean of the control group before**
- β_1 is the prediction when $Post_t = 1$ and $Treated_g = 0$, i.e. difference between periods before and after for the control group
  - $\beta_0 + \beta_1$ is the **mean of the control group after**
- β_2 is the prediction when $Post_t = 0$ and $Treated_g = 1$, i.e. difference between treated and control groups
  - $\beta_0 + \beta_2$ is the **mean of the treated group before**
- β_3 is the prediction when $Post_t = 1$ and $Treated_g = 1$, i.e. is how much bigger the before-after difference for the control and treated groups - DiD
  - $\beta_0 + \beta_1 + \beta_2 + \beta_3$ is the **mean of the treated group after**

2×2 DiD: simulation

- Let us simulate a dataset with 2 groups and 2 time periods

```
set.seed(7)
df <- tibble(year = rep(1:2, 5000),
             group = sort(rep(0:1, 5000)),
             post = ifelse(year == 2, 1, 0),
             treated = ifelse(group == 1, 1, 0),
             D = post*treated,
             Y = 3*D + year + group + rnorm(10000))
```

year	group	post	treated	D	Y
1	0	0	0	0	2.208622
2	0	1	0	0	1.611525
1	1	0	1	0	1.560944
2	1	1	1	1	5.562782

2 × 2 DiD: simulation

- First, let us manually calculate DiD

```r
# The true effect is 3
means <- df %>% group_by(treated, post) %>% summarize(Y = mean(Y))
means
```

```
## # A tibble: 4 × 3
## # Groups:   treated [2]
##   treated  post     Y
##     <dbl> <dbl> <dbl>
## 1       0     0  1.01
## 2       0     1  2.00
## 3       1     0  2.00
## 4       1     1  6.00
```

```r
treated_dif <- means[means$treated == 1 & means$post == 1,]$Y -
               means[means$treated == 1 & means$post == 0,]$Y
control_dif <- means[means$treated == 0 & means$post == 1,]$Y -
               means[means$treated == 0 & means$post == 0,]$Y
did <- treated_dif - control_dif
```

```
## [1] 4.0096336 0.9845777 3.0250559
```

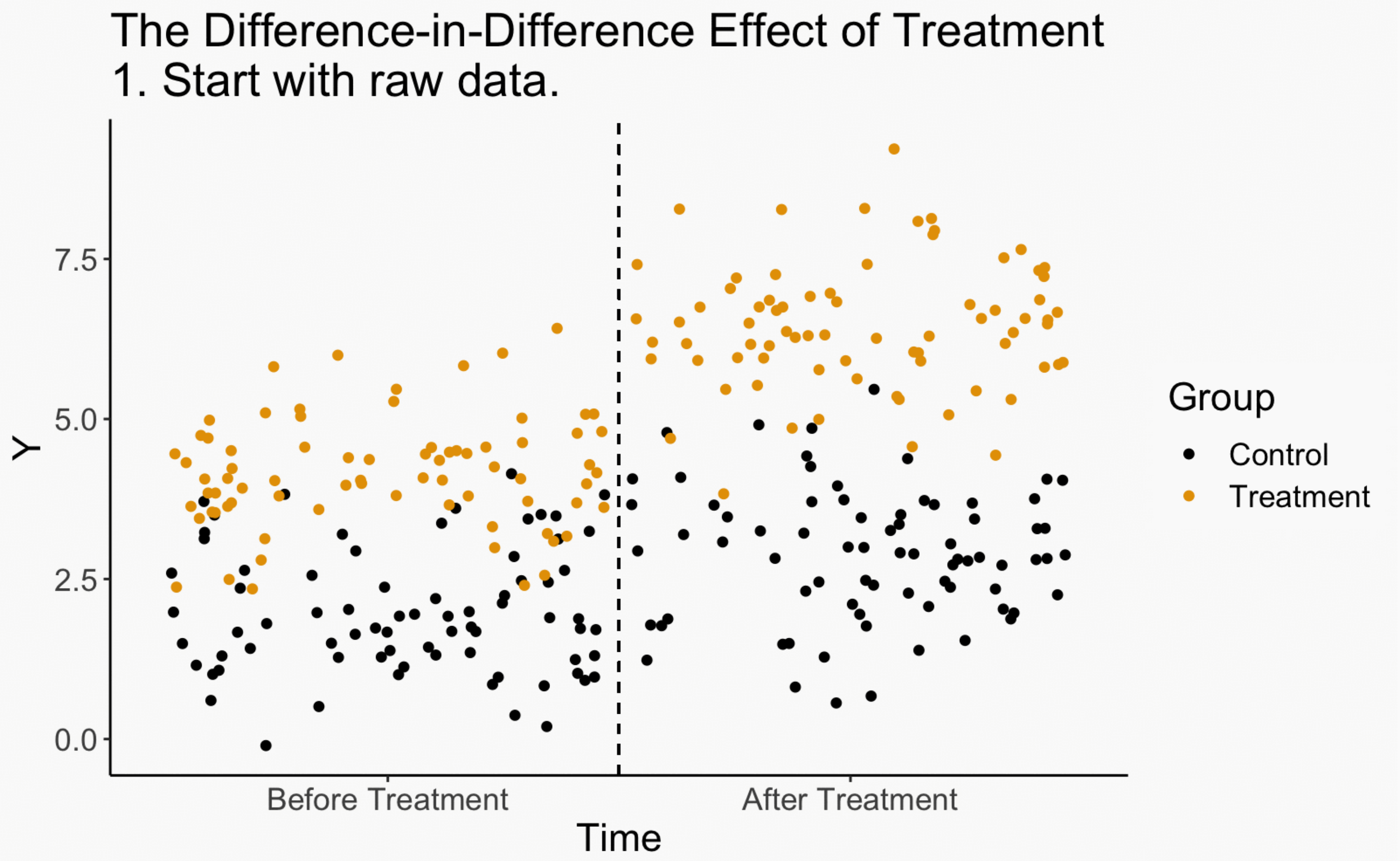
2 × 2 DiD: simulation

- Now let us use `feols()` in `fixest` package as DiD is the FE estimator

```r
# The true effect is 3
library(fixest)
m1 <- feols(Y ~ D | year + group, df,
            se = 'standard') # no need to cluster s.e.
                             # as there are only 2 groups and 2 time periods
# We can estimate DiD using simple lm()
m2 <- lm(Y ~ D + factor(year) + factor(group), df)
```

	Model 1	**Model 2**
D	3.025***	3.025***
	(0.040)	(0.040)
Num.Obs.	10000	10000
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001		

DiD: graphically



DiD: more groups and time periods

- Let us simulate a dataset with 20 groups and 10 time periods
  - with first treated period being period 7
  - and the treated groups being 15 and 20

```
set.seed(7)
df <- tibble(year = rep(1:10, 1000),
             group = sort(rep(1:20, 500)),
             post = ifelse(year >= 7, 1, 0),
             treated = ifelse(group >= 15, 1, 0),
             D = post*treated,
             Y = 3*D + year + group + rnorm(10000))
```

year	group	post	treated	D	Y
5	20	0	1	0	23.71590
6	20	0	1	0	25.20272
7	20	1	1	1	31.16885
8	20	1	1	1	33.84800

DiD: simulation

```r
# The true effect is 3
library(fixest)
m ← feols(Y ~ D | year + group, df)
```

	Model 1
D	2.987***
	(0.033)
Num.Obs.	10000
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001	

DiD: inference

- Always remember what is the level of your treatment
  - is your treatment assigned at the level of state?
  - is your treatment assigned at the level of university?
  - is your treatment assigned at the level of class?
- If you have not done so, read a paper by Abadie et al. 2017
- It's common to cluster s.e. at the level of the fixed effects, since it seems likely that errors would be correlated over time
  - not accounting for clustering leads to incorrect s.e.
  - `feols()` clusters by the first FE by default

DiD: inference

```r
# The true effect is 3
m1 ← feols(Y ~ D | year + group, df, se = 'standard')
m2 ← feols(Y ~ D | year + group, df, cluster = "year")
m3 ← feols(Y ~ D | year + group, df, cluster = "group")
m4 ← feols(Y ~ D | year + group, df, cluster = "year^group")
```

	Model 1	**Model 2**	**Model 3**	**Model 4**
D	2.987***	2.987***	2.987***	2.987***
	(0.045)	(0.033)	(0.040)	(0.046)
Num.Obs.	10000	10000	10000	10000
Std.Errors	IID	by: year	by: group	by: year^group
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001				

- Remember that how you calculate your s.e. **does not** affect point estimates

DiD: event-study specification

- We've limited ourselves to "before" and "after" but this is not all we have
- But that averages out the treatment across the entire "after" period
  - what if an effect takes time to get going? or fades out?
- We can also estimate a **dynamic effect** where we allow the effect to be different at different time periods since the treatment
- To implement an event-study specification
  - interact a binary indicator for being in the treated group with binary indicators for time period
  - impose the normalisation $\delta_{-1} = 0$, which is the coefficient for the last period before the treatment, otherwise you get perfect multicollinearity

$$Y_{gt} = \alpha_t + \eta_g + \delta_{t-\tau_g} D_{gt} + U_{gt}$$

    where

  - τ_g is the moment of the treatment
  - t is time period
  - `feols()` makes this easy with its `i()` interaction function
- Then, just plot these estimates

DiD: event-study specification

- Let us make a more concrete example
- Suppose we have 6 time periods: 3 periods before and 3 periods after
    - so $t = 6$ and $\tau_g = 4$
- Then our model is as follows

$$
\begin{aligned}
Y_{gt} &= \alpha_t + \eta_g + \delta_{t-\tau_g} D_{gt} + U_{gt} \\
&= \alpha_t + \eta_g + \delta_{1-4} D_{g1} + \delta_{2-4} D_{g2} + \delta_{3-4} D_{g3} \\
&+ \delta_{4-4} D_{g4} + \delta_{5-4} D_{g5} + \delta_{6-4} D_{g6} + U_{gt} \\
&= \alpha_t + \eta_g + \delta_{-3} D_{g1} + \delta_{-2} D_{g2} + \delta_{-1} D_{g3} + \delta_0 D_{g4} + \delta_1 D_{g5} + \delta_2 D_{g6} + U_{gt}
\end{aligned}
$$

where

- $\delta_{-3}, \delta_{-2}, \delta_{-1}$ are coefficients of the "effect" before the treatment period
- $\delta_{-1} = 0$ which is the coefficient for the last period before the treatment
- $\delta_0, \delta_1, \delta_2$ are coefficients of the effect after the treatment period

Parallel trends

- For the DiD to work we have to pick the control group
  - we need a control group for which parallel trends holds
  - if there had been no treatment, both treated and control groups would have had the same time effect
- We can't check this directly, since it's counterfactual
  - we can only check whether it is plausible
- DiD gives us a causal effect as long as **the only reason the gap changed** was the treatment
  - this is called **parallel trends assumption**
- The parallel trends assumption means that if the treatment had not happened, the gap between the two groups would have stayed the same
- There are two main ways we can use test the plausibility of parallel trends
  - First, we can check for differences in **prior trends**
  - Second, we can do a **placebo test**

Parallel trends: prior trends

- You can check whether the assumption is plausible by seeing if **prior trends** are the same for treated and control groups
  - if we have multiple pre-treatment periods, was the gap changing a lot during that period?
- If the two groups were already trending towards each other, or away from each other, before treatment, it is hard to believe that parallel trends holds
- They **probably** would have continued trending together/apart, breaking parallel trends
  - in this case we would mix up the continuation of the trend with the effect of treatment
- Sometimes you can adjust for prior trends to fix parallel trends violations
  - by including a time variable directly
  - or by using a synthetic control method
- Just because **prior trends** are equal does not mean that **parallel trends** holds
  - **parallel trends** is about what the before-after change **would have been** and we can't see that
  - but it can be **suggestive**

Parallel trends: prior trends

- Recall the formula we used in an event-study framework

$$
\begin{aligned}
Y_{gt} &= \alpha_t + \eta_g + \delta_{t-\tau_g} D_{gt} + U_{gt} \\
&= \alpha_t + \eta_g + \delta_{-3} D_{g1} + \delta_{-2} D_{g2} + \delta_{-1} D_{g3} + \delta_0 D_{g4} + \delta_1 D_{g5} + \delta_2 D_{g6} + U_{gt}
\end{aligned}
$$

- To check parallel pre-trends, test if δ_{-3}, δ_{-2} are jointly significant
  - you can do so with `wald()` in `fixest` package
- If they are jointly insignificant, there is no evidence of differences in prior trends
  - that doesn't **prove** parallel trends but failing this test would make prior trends **less plausible**
- You can also check more complex time trends by including polynomial terms or other nonlinearities

Parallel trends: placebo

- Many causal inference designs can be tested using **placebo tests**
- To implement a placebo test
  - use only the data that came before the treatment went into effect
  - pick a fake treatment period
  - estimate the same DiD model you used
  - if you find an "effect", that is evidence that there is something wrong with your design, which may imply a violation of parallel trends

Parallel trends: placebo

```r
# Remember the first treated period was period 7
df_fake <- df %>%
  filter(year < 7) %>%
  # pick a fake treatment period
  mutate(post1 = ifelse(year >= 4, 1, 0),
         post2 = ifelse(year >= 5, 1, 0),
         D1 = post1*treated,
         D2 = post2*treated)
```

year	group	post	treated	D	Y	post1	post2	D1	D2
3	20	0	1	0	24.57066	0	0	0	0
4	20	0	1	0	23.15619	1	0	1	0
5	20	0	1	0	23.71590	1	1	1	1
6	20	0	1	0	25.20272	1	1	1	1

Parallel trends: placebo

```r
# The true effect is 3
library(fixest)
m1 <- feols(Y ~ D1 | year + group, df_fake)
m2 <- feols(Y ~ D2 | year + group, df_fake)
```

	Model 1	**Model 2**
D1	-0.022	
	(0.062)	
D2		-0.081
		(0.050)
Num.Obs.	6000	6000
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001		

- There is no "effect" of our fake treatment which is a good sign

Parallel trends: some remarks

- Sometimes you will find significant effects while testing parallel pre-trends or by using a placebo
- However, for both prior trends and placebo tests, we are a little less concerned with **significance** than with **meaningful size** of the violations
    - after all, with enough sample size **anything** is significant
    - and if fake treatment effects are fairly tiny, you can argue these effects away

DiD as two-way fixed effects: problems

- One common variant of difference-in-difference is the **rollout design**, in which there are multiple treated groups, each being treated at a different time
  - rollout designs are possibly the most common form of DiD you see
- As discovered recently, two-way fixed effects **does not** work to estimate DiD when you have a rollout design
  - think about what fixed effects does - it leaves you only with within variation
  - two types of individuals without **any** within variation between periods A and B: the never-treated and the already-treated
  - so the already-treated can end up getting used as controls in a rollout
- This becomes a big problem especially if the effect grows/shrinks over time

DiD as two-way fixed effects: solutions

- There are a few new estimators that deal with rollout designs properly
  - Goodman-Bacon (2021)
  - Callaway and Sant'Anna (2021)
- They take each period of treatment and consider the group treated **on that particular period**
- They explicitly only use untreated groups as controls
- And they also use **matching** to improve the selection of control groups for each period's treated group
- We will not go into these methods, but it is good to know for your future research

References

Books

- Huntington-Klein, N. The Effect: An Introduction to Research Design and Causality, Chapter 18: Difference-in-Differences
- Cunningham, S. Causal Inference: The Mixtape, Chapter 9: Difference-in-Differences
- Cunningham, S. Causal Inference: The Mixtape, Chapter 10: Synthetic Control

Slides

- Huntington-Klein, N. Econometrics Course, Week 07: Difference-in-Difference
- Huntington-Klein, N. Causality Inference Course, Lecture 09: Difference-in-Differences
- Huntington-Klein, N. Causality Inference Course, Lecture 10: Difference-in-Differences

Articles

- Abadie, A., Athey, S., Imbens, G. W., & Wooldridge, J. (2017). When Should You Adjust Standard Errors for Clustering? (No. w24003). National Bureau of Economic Research
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing
- Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods