5. Difference-in-Differences

Econometrics II
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Osaka U

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Summary of the last lecture

Shape RD

Fuzzy RD

Outline

Fixed effects (FE) models

- FE or RE?

Difference-in-differences (DID)

Fixed effects model

Suppose you have monthly individual panel data.

- What is the difference between panel data and pooled cross-section data?

You estimate an effect of union membership on annual income by regressing the following model

$$Y_{it} = \alpha + \beta D_{it} + \varepsilon_{it}, \tag{1}$$

Where Y_{it} is person i's annual income in year t, α is constant, D_{it} is i's union membership status $(0/1)$, and ε_{it} is an idiosyncratic error.

What are potential confounding factors?

- Time invariant factors?
- Time variant factors?

Fixed effects model (cont.)

With panel data, you can control for fixed effects, i.e.,

$$Y_{it} = \delta D_{it} + \phi_i + \tau_t + u_{it}, \tag{2}$$

Where $\phi_i = \alpha + \hat{\phi}_i$ is an individual fixed effect and τ_t is a time fixed effect.

- How can we interpret α in FE models? See, e.g., this page.

What are the effects that fixed effects can absorb?

- Individual FEs: Absorb all effects that are constant across study periods for any particular individual (e.g., gender, education)
- Time FEs: Absorb all effects that are constant across individuals at any given time (e.g., business cycle)

What are the effects that fixed effects *cannot* absorb?

What kind of variation does (2) use to estimate δ?

Individual FEs and deviations from means

Adding individual FEs is like demeaning variables.

To see this, take the average of the model (2) with respect to time for each individual

$$\bar{Y}_i = \delta \bar{D}_i + \phi_i + \bar{\tau} + \bar{u}_i. \tag{3}$$

Subtracting this from (2) yields

$$Y_{it} - \bar{Y}_i = \delta (D_{it} - \bar{D}_i) + (\tau_t - \bar{\tau}) + (u_{it} - \bar{u}_i). \tag{4}$$

Individual FEs and deviations from means (cont.)

That is, we can estimate the same δ using (4) (rather than (2)) without adding individual FEs (ϕ_i).

- What variation does (4) use to estimate δ?
- A good thing about using the demeaned regression is that one can reduce the number of parameters to be estimated. (Imagine that there are millions of individuals in your data.)
- However, standard errors are wrong. The degrees of freedom should be $n(t-1)-k$, rather than $nt-k$, for n individuals, t periods, and k independent variables.

How can we recover ϕ_i?

- One can use (3) to compute $\hat{\phi}_i$

$$\hat{\phi}_i = \bar{Y}_i - \hat{\delta}\bar{D}_i - \bar{\tau}. \tag{5}$$

Individual FEs and deviations from means (cont.)

The pooled OLS estimator from the regression (4) is called the fixed effects (FE) estimator.

- The FE estimator is also called the within estimator because it uses the time variation within each individual.
- Instead, (3) is called the between estimator, because it only uses variation between individuals.

Adjusted R-squared

R-squared from the FE estimation is typically very high.

- Why?

You need to report both R-squared and adjusted R-squared

$$\text{Adjusted R-squared} = 1 - \frac{(1-R^2)(n-1)}{n-k-1}, \tag{6}$$

Where R^2 is R-squared, n is the number of observations, and k is the number of independent variables.

FE or RE?

A related method is random effects estimation.

- For details about random effects estimation, see, e.g., Chapter 14 in Woodbridge.

What is the difference?

If the model assumptions of RE are satisfied, RE is more efficient than FE. However,

- An assumption requires that ϕ_i is uncorrelated with regressors.
- Otherwise, the RE estimator is inconsistent.
- Also, small-sample properties may be worse.

RE still gets a chance if

- The variable of interest is time-invariant. (In that case, FE cannot be used!)

R exercise

Let's estimate the effect of union membership on wages using a fixed effects model.

We use panel data from Vella and Verbeek (1998), which contain 545 men who worked every year between 1980 and 1987.

- Available variables include race, education, marital status, work experience, occupations, etc.

You run a regression

$$\text{wage}_{it} = \delta \text{union}_{it} + X_{it}\beta + \phi_i + \tau_t + u_{it}, \tag{7}$$

Where X_{it} contains control variables.

- What kinds of variables do we need to add?
- What kinds of variables can't we add?

R exercise (cont.)

Launch RStudio.

Type

```
union <- wooldridge::wagepan

reg1 <- lm(lwage ~ union, data=union)
reg2 <- lm(lwage ~ union + educ + black + hisp + exper +
expersq + married, data=union)
reg3 <- felm(lwage ~ union + expersq + married | nr +
year, data=union)

stargazer(reg1, reg2, reg3, type="text")
```

Why can't we add variables such as `educ`, `black`, and `hisp` in `reg3`?

Is there any difference between the OLS estimates and the FE estimate of union membership on wages?

- What could be the potential explanation?

R exercise (cont.)

Next, let's check that demeaned regression gives the same estimates.

Type

```
union_mean <- summaryBy(lwage + union + educ + black +
hisp + exper + expersq + married + d81 + d82 + d83 + d84
+ d85 + d86 + d87 ~ nr, data=union)

union <- merge(union, union_mean, by="nr")

myvec <- c("lwage", "union", "educ", "black", "hisp",
"exper", "expersq", "married", "d81", "d82", "d83",
"d84", "d85", "d86", "d87")

for(i in myvec){
    union[paste0(i,".diff")] <- union[i] -
union[paste0(i,".mean")]
}
```

R exercise (cont.)

Type

```
reg4 <- felm(lwage ~ union + expersq + married +
factor(year) | nr, data=union)
reg5 <- lm(lwage.diff ~ union.diff + expersq.diff +
married.diff + d81.diff + d82.diff + d83.diff + d84.diff
+ d85.diff + d86.diff + d87.diff + 0, data=union)

stargazer(reg1, reg2, reg3, reg4, reg5, type="text")
```

Compare the estimates of the demeaned regression with those of the FE estimates. What do you find?

R exercise (cont.)

Let's recover individual FEs.

Type

```
union_subset <- subset(union, nr <= 150)

n <- factor(union_subset$nr)
nr <- model.matrix(~ n + 0)
union_subset <- cbind(union_subset, nr)

reg6 <- lm(lwage.diff ~ union.diff + expersq.diff +
married.diff + d81.diff + d82.diff + d83.diff + d84.diff
+ d85.diff + d86.diff + d87.diff + 0, data=union_subset)

est <- reg6$coefficients
est
```

R exercise (cont.)

Type

```
union_subset$indiv <- with(union_subset, lwage.mean
- est[1]*union.mean - est[2]*expersq.mean
- est[3]*married.mean - est[4]*d81.mean
- est[5]*d82.mean - est[6]*d83.mean - est[7]*d84.mean
- est[8]*d85.mean - est[9]*d86.mean - est[10]*d87.mean)

reg7 <- lm(lwage ~ union + expersq + married + d81 +d82
+ d83 + d84 + d85 + d86 + d87 + n13 + n17 + n18 + n45 +
n110 + n120 + n126 + n150 + 0, data=union_subset)

View(union_subset)

stargazer(reg7, type="text")
```

Compare the estimates on individual FEs and the values of `indiv` in `union_subset`. What do you find?

Difference-in-differences

Let's move on to difference-in-differences (DID).

- DID often applies fixed effects models.

Education in developing countries

In 2015, 57 million children of primary school age were out of school, according to the United Nations.

> SDG Target 4.1: By 2030, ensure that all girls and boys complete free, equitable and quality primary and secondary education leading to relevant and effective learning outcomes.

The number of schools is typically limited in developing countries.

What is the impact of school construction on educational attainment?

Impact of school construction

Between 1973-78, the Indonesian government engaged in one of the largest school construction programs (aka the INPRES program).

- 61,807 new schools were constructed, which represented more than one school per 500 children aged 5-14 in 1971.
- The stock of schools was doubled over the period.
- An effort to train more teachers paralleled the program.

Impact of school construction (cont.)

Suppose you estimate a model like

$$Y_{ir} = \alpha + \beta X_r + \varepsilon_{ir}, \tag{8}$$

Where Y_{ir} is a school outcome (e.g. years of education) for an adult i who grew up in region r, and X_r is the intensity of the program.

Suppose you find a positive effect.

- Can we say that the school construction program increased years of education?

Is there any endogeneity issue? How can we solve it?

Impact of school construction (cont.)

Indonesian children between ages 7-12 attend primary schools.

- The effect of the program should be zero for children who were 12 years old or older in 1974, when the first INPRES schools were constructed, and should be larger for younger children.
- Children who were 12 years old or older in 1974 can be used as a control group.

Impact of school construction (cont.)

Let $Y_{a,r}$ be the average years of education for individuals who were at age a in region r in 1974, when the first INPRES schools were built.

The identification strategy uses difference-in-differences.

- Difference 1: $Y_{below12,high} - Y_{12orabove,high}$
- Difference 2: $Y_{below12,low} - Y_{12orabove,low}$
- Take a difference in the differences, i.e., Difference 1 - Difference 2.

The difference is a difference-in-differences (DID) estimate.

Difference-in-Differences

TABLE 3—MEANS OF EDUCATION AND LOG(WAGE) BY COHORT AND LEVEL OF PROGRAM CELLS

	Years of education			Log(wages)		
	Level of program in region of birth			Level of program in region of birth		
	High (1)	Low (2)	Difference (3)	High (4)	Low (5)	Difference (6)
Panel A: Experiment of Interest						
Aged 2 to 6 in 1974	8.49	9.76	−1.27	6.61	6.73	−0.12
	(0.043)	(0.037)	(0.057)	(0.0078)	(0.0064)	(0.010)
Aged 12 to 17 in 1974	8.02	9.40	−1.39	6.87	7.02	−0.15
	(0.053)	(0.042)	(0.067)	(0.0085)	(0.0069)	(0.011)
Difference	0.47	0.36	0.12	−0.26	−0.29	0.026
	(0.070)	(0.038)	(0.089)	(0.011)	(0.0096)	(0.015)
Panel B: Control Experiment						
Aged 12 to 17 in 1974	8.02	9.40	−1.39	6.87	7.02	−0.15
	(0.053)	(0.042)	(0.067)	(0.0085)	(0.0069)	(0.011)
Aged 18 to 24 in 1974	7.70	9.12	−1.42	6.92	7.08	−0.16
	(0.059)	(0.044)	(0.072)	(0.0097)	(0.0076)	(0.012)
Difference	0.32	0.28	0.034	0.056	0.063	0.0070
	(0.080)	(0.061)	(0.098)	(0.013)	(0.010)	(0.016)

Notes: The sample is made of the individuals who earn a wage. Standard errors are in parentheses.

Notes: Program intensity is the INPRES schools built per 1,000 children.

Source: Duflo (2001).

Impact of school construction (cont.)

Why do we take a *difference* in *differences*?

- Instead of, say, comparing regions with more school construction with less?

Recall the above equation

$$\begin{aligned} & (Y_{below12,high} - Y_{12orabove,high}) - (Y_{below12,low} - Y_{12orabove,low}) \\ = \quad & (Y_{below12,high} - Y_{below12,low}) - (Y_{12orabove,high} - Y_{12orabove,low}). \end{aligned}$$

Considering the second parentheses captures the systematic differences between two regions, taking a difference in differences removes such differences.

Results (Duflo 2001)

For males, each primary school constructed per 1,000 children led to an average increase of 0.12 to 0.19 years of education, as well as a 1.5 to 2.7 percent increase in wages (based on the 1995 data).

- Estimates of economic returns to education ranged from 6.8 to 10.6 %, which was very similar to what was commonly found in the United States (8-11%).

No average effect on females.

- Another study found that each primary school constructed per 1,000 children led to an average increase of 0.16 years of education for *females from a bride price ethnicity* (Ashraf et al. 2019).

Supporting banks in a recession

During the Great Recession, many banks shut down due to bank runs in the United States.

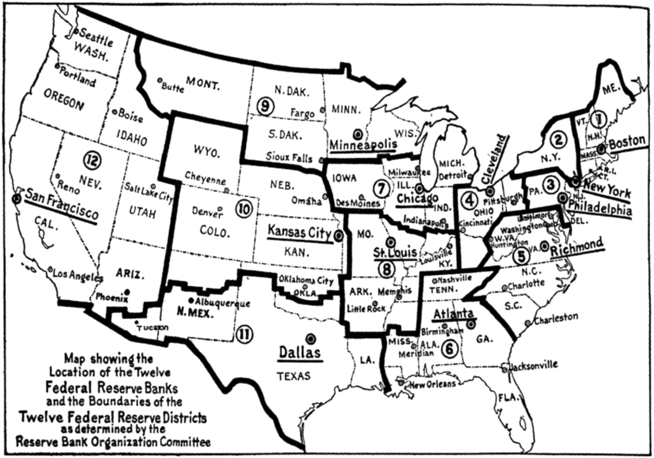
- In Mississippi, this happened in December, 1930.

Facing troubled banks, regional Federal Reserve Banks (FRBs) have decided whether or not to help them.

- Sixth district (Atlanta Fed) favored lending to troubled banks.

Were there any changes in bank failures before and after the shock because of the FRB's policies?

FRBs



Source: Reserve Bank Organisation Committee (1914).

Before-After comparison

Let $Y_{d,y}$ denotes the number of banks open in district d in year y.

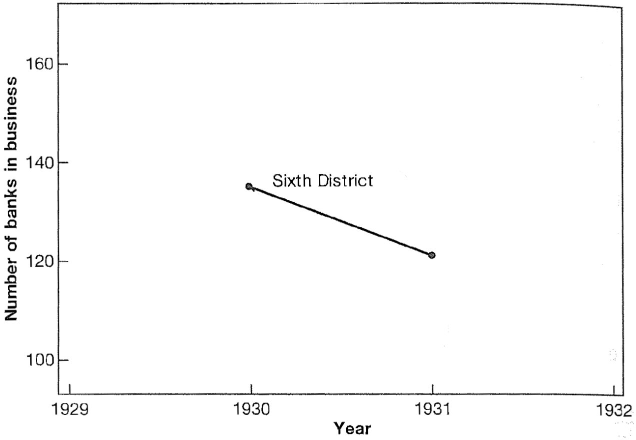
Take numbers from July 1930 (pre-treatment) and July 1931 (post-treatment) for 6th district, we get

$$\begin{aligned} & (Y_{6th,1931} - Y_{6th,1930}) \\ = & \ (121 - 135) \\ = & \ -14. \end{aligned} \tag{9}$$

This is a before-after comparison.

Can we say that the policy worsened the situation?

Graphical explanation



Source: Mastering 'Metrics.

Before-After comparison (cont.)

This kind of naive before-after analysis is commonly observed in practice!

Example 1: Reducing bicycle theft

International

日本經濟新聞

2018年10月4日（木）

トップ　経済・政治　ビジネス　マーケット　テクノロジー　国際・アジア　スポーツ　社会

速報　朝刊・夕刊

自転車盗難防げ、音で警告　兵庫・尼崎で社会実験

2017/9/13 10:40

保存　共有　印刷　その他

兵庫県尼崎市は13日、自転車盗難を減らそうと、市内の駐輪場など3カ所に、振動で警報音が鳴る装置を取り付けた自転車を潜ませ、心理的効果を試す社会実験を始めた。

市によると、盗難対策でこうしたダミー自転車を使う実験は全国の自治体で初めて。約3カ月間で、発生をどの程度減らせるか見極める。

装置は南京錠の形をしており、それぞれの場所に1台ずつの自転車をダミーとして置く。自転車を動かそうとすると、最初は小さめの「ピピピ」という音が鳴り、さらに動かすと、周囲の人も分かるような約10秒の大音量の警報になる。

周辺には「盗もうとするとアラームが鳴ります」との警告文も張り出し、付近の自転車全てで音が出ると思わせ、盗難を防ぐことを狙う。

Example 1: Reducing bicycle theft (cont.)

To reduce bicycle theft, Amagasaki City in Hyogo Prefecture conducted an "experiment."

- They randomly locate bicycles with an alarm in three bicycle stations (which were not randomly selected). There were no "control" bicycle stations.
- If someone tried to steel the bicycle, the alarm started beeping.
- They also displayed a poster alerting that there was an experiment going on in the bicycle stations.

They find that the number of bicycle theft was reduced in these three stations after the experiment.

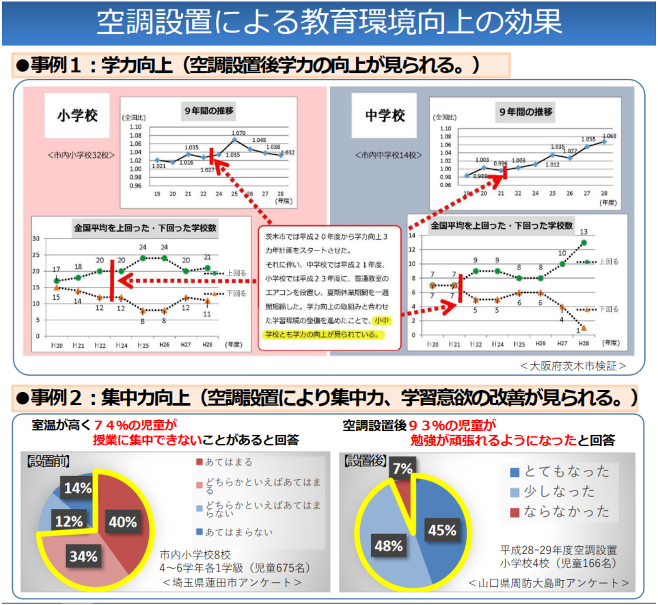
How many problems can you spot in this example?

Example 2: Effect of air-conditioning

- There was some debate on whether or not schools should put air conditioners (ACs) in classrooms.
- Ibaraki City in Osaka Prefecture installed ACs in all 14 public junior-high schools.
- Test scores increased in these schools after the installation of ACs somehow.
- The finding was cited as "evidence" by the Ministry of Education in Japan.

  $\rightarrow$ This might have affected policy making?

Example 2: Effect of air-conditioning (cont.)



Example 2: Effect of air-conditioning (cont.)

Can you spot the problem?

Before-After comparison (cont.)

Why is the simple before-after comparison often misleading?

It does not have a comparison group!

- Bicycle stations were not randomly selected. Stations without such beeping bicycles were not included in the study.
- All public junior-high schools were treated by ACs (hence, no control schools!).

We do not know whether the effect is driven by the treatment, or just by an overall trend.

- Recall the Ice Cream example.

We need a good control group!

Supporting banks in a recession (cont.)

In contrast to the 6th district, a neighboring 8th district (St. Louis Fed) favored restricting credit in a recession.

- Let's use the 8th district as a control group.

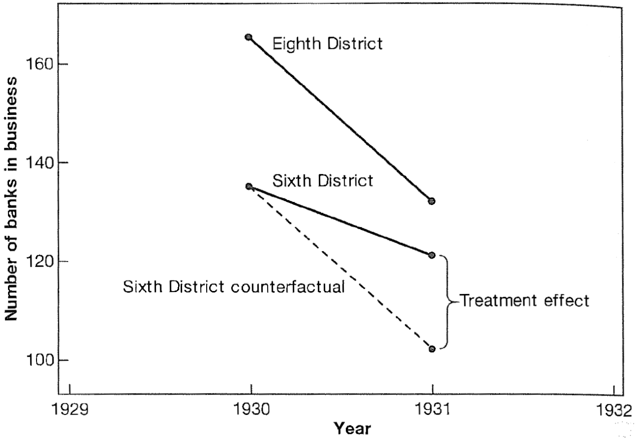
Take numbers from both districts between July 1930 (pre-treatment) and July 1931 (post-treatment), we get

$$
\begin{aligned}
& (Y_{6th,1931} - Y_{6th,1930}) - (Y_{8th,1931} - Y_{8th,1931}) \\
= \; & (121 - 135) - (132 - 165) \\
= \; & 19. \qquad (10)
\end{aligned}
$$

This is a DID estimate.

Banks in the 6th district were less likely to close down compared with banks in the 8th district after the shock!

Graphical explanation (cont.)



Source: *Metrics*.

Deriving ATT using DID

Using DID, one can derive a causal effect.

Consider a potential outcome model

$$Y_{it} = Y_{it}(1)D_{it} - Y_{it}(0)(1 - D_{it}), \tag{11}$$

Where $Y_{it}(1)$ is the potential outcome in period t if treated before t and $Y_{it}(0)$ is the potential outcome in period t if not treated before t.

Assuming that the post-treatment period is $t = 1$, one can write

$$\begin{aligned} Y_{i0} &= Y_{i0}(0) && (12) \\ Y_{i1} &= Y_{i1}(1)D_{i1} - Y_{i1}(0)(1 - D_{i1}). && (13) \end{aligned}$$

Why is the equation (12) written like this?

The common trends (parallel trends) assumption states that

$$E[Y_{i1}(0) - Y_{i0}(0)|D_{i1} = 1] = E[Y_{i1}(0) - Y_{i0}(0)|D_{i1} = 0]. \tag{14}$$

Under the assumption, ATT can be identified.

Deriving ATT using DID (cont.)

Consider the ATT,

$$E[Y_{i1}(1) - Y_{i1}(0)|D_{i1} = 1]. \tag{15}$$

What kind of comparison are we making here?

Taking a difference in differences of the outcome yields,

$$\begin{aligned}
& (E[Y_{i1}|D_{i1} = 1] - E[Y_{i1}|D_{i1} = 0]) \\
& -(E[Y_{i0}|D_{i1} = 1] - E[Y_{i0}|D_{i1} = 0]) \qquad (16) \\
= \; & (E[Y_{i1}(1)|D_{i1} = 1] - E[Y_{i1}(0)|D_{i1} = 0]) \\
& -(E[Y_{i0}(0)|D_{i1} = 1] - E[Y_{i0}(0)|D_{i1} = 0]) \qquad (17) \\
= \; & E[Y_{i1}(1)|D_{i1} = 1] - E[Y_{i0}(1)|D_{i1} = 1] \qquad (18) \\
= \; & E[Y_{i1}(1) - Y_{i1}(0)|D_{i1} = 1], \qquad (19)
\end{aligned}$$

Where I used the common trends assumption to derive (18).

Difference-in-Differences (cont.)

DID captures a change in trend.

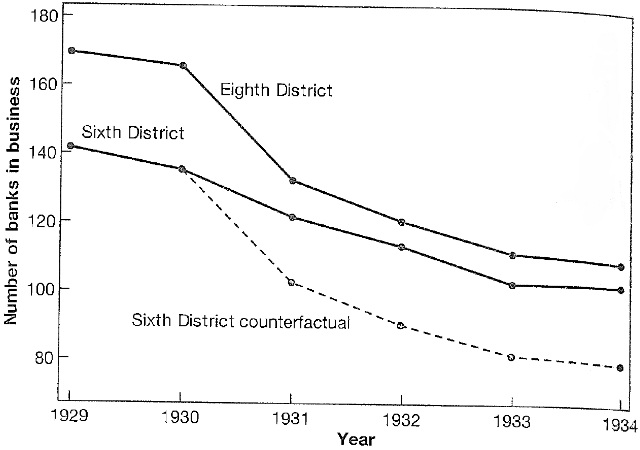
- The trend for the 6th district would have been like the trend for the 8th district if there was no treatment (the common trends assumption).
- However, due to treatment, the trend for the 6th district changed.
- The DID estimate captures this change in the slope of the 6th district, relative to that of the 8th district.

A crucial assumption for DID is the common trends assumption.

We cannot test the assumption due to the fundamental problem of causal inference.

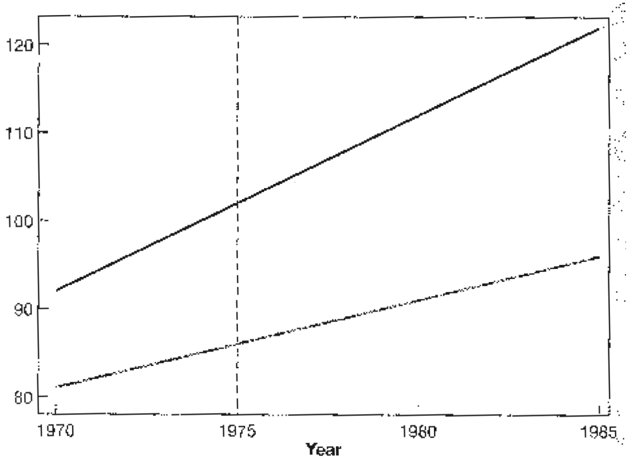
- BUT, we can argue the validity of the assumption.
- At least, you should show trends in a figure.

Eighth (control) vs. sixth (treatment)



Source: *Metrics*.

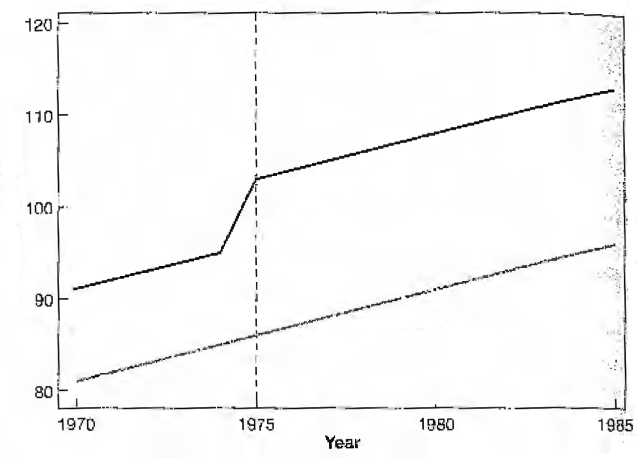
Common trends?



Notes: y-axis: Death rate (per 100,000).

Source: *Metrics*.

Common trends? (cont.)



Notes: y-axis: Death rate (per 100,000).

Source: *Metrics*.

R exercise

Let's compute a DID estimate.

Launch RStudio.

Type

```
banks <- mmdata::banks
banks$date <- as.Date(with(banks, paste(year, month,
day, sep="-")), "%Y-%m-%d")

banks.lmt <- subset(banks,(month==7 & day==1))

diff_sixth <- with(banks.lmt, bib6[year==1931]) -
with(banks.lmt, bib6[year==1930])
diff_eighth <- with(banks.lmt, bib8[year==1931]) -
with(banks.lmt, bib8[year==1930])

diff_sixth
```

R exercise (cont.)

Type

```
diff <- diff_sixth - diff_eighth

diff
```

Regression DID

As mentioned earlier, DID often applies fixed effects estimation. Assume that all the assumptions for the FE model hold.

- See the Appendix.

Consider a FE model for district $i \in \{6th, 8th\}$ and time $t \in \{1930, 1931\}$

$$Y_{it} = \delta D_{it} + \phi_i + \tau_t + u_{it}, \tag{20}$$

Where D_{it} takes value one for $i = 6th$ and $t = 1931$ and zero otherwise, and $E[u_{it}|i,t] = 0$.
Since

$$E[Y_{it}|i,t] = \delta D_{it} + \phi_i + \tau_t, \tag{21}$$

The causal effect is

$$\begin{aligned}
& (E[Y_{it}|i = 6th, t = 1931] - E[Y_{it}|i = 6th, t = 1930]) \\
& -(E[Y_{it}|i = 8th, t = 1931] - E[Y_{it}|i = 8th, t = 1930]) \\
= \ & (\delta + \tau_{1931} - \tau_{1930}) - (\tau_{1931} - \tau_{1930}) \\
= \ & \delta. \qquad (22)
\end{aligned}$$

Omitted variable

The FE estimate is not necessarily a causal effect.

If there is another time-variant variable that is correlated with D_{it} and is omitted, it would introduce a bias in estimates.

- E.g. Another policy targeting districts that are treated by the policy of your interest.

It is important to add variables as a robustness check or argue why this is unlikely the case.

Regression DID (cont.)

Consider a different FE model for district $i \in \{6th, 8th\}$ and time $t \in \{1930, 1931\}$

$$Y_{it} = \alpha + \delta(6\mathrm{d}_i \times \mathrm{Post}_t) + \phi 6\mathrm{d}_i + \tau \mathrm{Post}_t + \varepsilon_{it}, \qquad (23)$$

Where 6d takes value one for the 6th district, and zero otherwise, and Post takes value one for 1931, and zero otherwise.

This is a *saturated model* as there are four possible values for $E[Y_{it}|i,t]$ (6th-1930, 6th-1931, etc.) and there are four parameters.

Regression DID (cont.)

Now we have two different models

$$
\begin{aligned}
Y_{it} &= \delta D_{it} + \phi_i + \tau_t + u_{it}, \\
Y_{it} &= \alpha + \delta(\mathsf{6d}_i \times \mathsf{Post}_t) + \phi \mathsf{6d}_i + \tau \mathsf{Post}_t + \varepsilon_{it}.
\end{aligned}
$$

What is the relationship?

$$
\begin{aligned}
\alpha &= E[Y_{it}|i = 8th, t = 1930] = \phi_{8th} + \tau_{1930} && (24) \\
\delta &= \text{Same } \delta && (25) \\
\phi &= E[Y_{it}|i = 6th, t = 1930] - E[Y_{it}|i = 8th, t = 1930] \\
&= \phi_{6th} - \phi_{8th} && (26) \\
\tau &= E[Y_{it}|i = 8th, t = 1931] - E[Y_{it}|i = 8th, t = 1930] \\
&= \tau_{1931} - \tau_{1930}. && (27)
\end{aligned}
$$

Explain intuitively the meanings of these parameters $(\alpha, \delta, \phi, \tau)$.

Regression DID (cont.)

Often (20) is preferred to (23).

- Why?

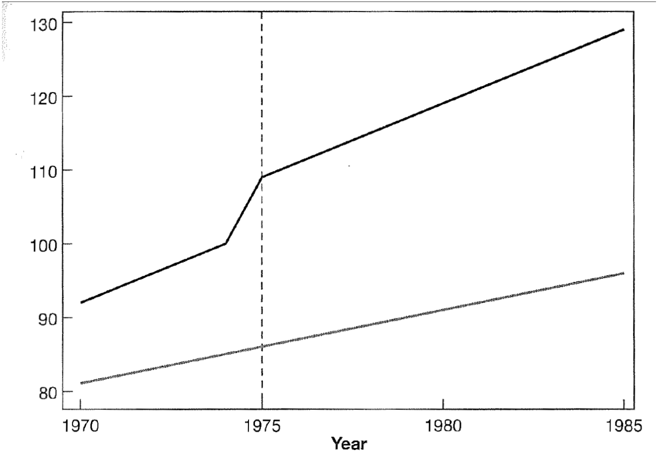
You can even consider a model like

$$Y_{it} = \delta D_{it} + \phi_i + \tau_t + \kappa(\phi_i * t) + u_{it}, \tag{28}$$

Where κ absorbs district-specific linear trends.

- What is the role of such control?

The case of differential trends



Notes: y-axis: Death rate (per 100,000).

Source: *Metrics*.

R exercise

Let's run a DID regression.

Type

```
banks.rshp <- reshape(banks.lmt, direction="long",
varying=c("bib6","bio6","bib8","bio8"),
v.names=c("bib","bio"), timevar="d6", times=c(1,0))

banks.rshp$post <- as.numeric(banks.rshp$year>=1931)
```

R exercise (cont.)

Type

```
reg1 <- lm(bib ~ d6*post, data=banks.rshp)
reg2 <- felm(bib ~ d6*post | year, data=banks.rshp)
reg3 <- felm(bib ~ d6*post + d6*year | year,
data=banks.rshp)

stargazer(reg1, reg2, reg3, type="text")
```

Are these DID estimates stable?

Separating years

We can think of a more flexible model

$$Y_{it} = \alpha + \sum_j \delta_j (6\mathsf{d}_i \times \mathsf{Year}_j) + 6\mathsf{d}_i + \tau \mathsf{Year}_t + \varepsilon_{it}. \tag{29}$$

That is, the coefficients δ_j can take different values across years.

With such a model, one can possibly check

- The common trends assumption (how?)
- Heterogeneous effects across years

But, naturally, we need more observations to run such a model.

- Why?

R exercise (cont.)

Type (I skip the step for reshaping the original banks data.)

```
reg4 <- lm(bib ~ d6*post, data=banks.rshp2)
reg5 <- felm(bib ~ d6*post | year, data=banks.rshp2)
reg6 <- lm(bib ~ d6*factor(year), data=banks.rshp2)

stargazer(reg4, reg5, reg6, type="text")
```

What do you find?

Minimum rules for empiricists: DID

Visualize parallel trends if data contain more than two periods.

- It is better to have at least two periods before the treatment (i.e., at least three periods in total).
- Otherwise, you cannot credibly argue the validity of the common trends assumption.

You also need to argue that the change in slope is not caused by any other shock (e.g. another policy). That is, your treatment variable is not affected by other time-variant variables.

Summary

Fixed effects (FE) models

- FE or RE?

Difference-in-differences (DID)

Appendix: FE assumptions

Assume

$$E[Y_{it}(0)|D_{it},i,t] = E[Y_{it}(0)|i,t]. \tag{30}$$

That is, the value of D_{it} does not matter.

Suppose also that the population average of $Y_i(0)$ is explained solely by fixed effects in a linear model

$$E[Y_{it}(0)|i,t] = \phi_i + \tau_t \tag{31}$$

And that the causal effect δ is constant and additive

$$E[Y_{it}(1)|i,t] = E[Y_{it}(0)|i,t] + \delta. \tag{32}$$

Then we can write a CEF by

$$E[Y_{it}|i,t] = \delta D_{it} + \phi_i + \tau_t, \tag{33}$$

Which implies a fixed effect model,

$$Y_{it} = \delta D_{it} + \phi_i + \tau_t + u_{it}, \tag{34}$$

Where $u_{it} = Y_{it}(0) - E[Y_{it}(0)|i,t]$.