

Big Data and Economics

Difference-in-differences

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Bates College | DCS/ECON 368

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Prologue

Announcement

- Talk on Friday March 15 at 11am by my friend and colleague, Dr. Szymon Sacher
- The talk is entitled "Inference for Regression with Variables Generated from Unstructured Data"
- It is in Pettengill Hall G50 and concerns matters of data science, economics, and machine learning methods
- I'll announce an incentive to attend after I decide on the appropriate incentive

Plan for the day

- By the end of the day you will:
 - Understand how difference-in-differences uses variation in time and treatment to identify causal effects
 - Be able to implement difference-in-differences models in R
 - Create pretty event study plots of your results

Attribution

- These slides are adapted from work by Nick Huntington-Klein, Ed Rubin, and Scott Cunningham
- They're both superb econometric instructors and I highly recommend their work

Questions

- Any questions?
- Anything you want to talk through?
- There are some sneaky data tricks to the problem set that a few of you have started to uncover
- It is due Monday, so get on it if you're behind

Check-in on causality

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- Popcorn style: what do we need for causality?

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- Popcorn style: what do we need for causality?
- We need to account for everything that is correlated with treatment
- There are two extreme strategies:

1. Control variables for everything!

- Did we control for it all? How do we know?

2. Get a treatment that is uncorrelated with everything else

- How do we know we got rid of all the correlation?

And then there's a middle ground...

Causal Inference Review

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$$\tau_i = y_{1i} - y_{0i}$$

Fund. problem of causal inference

We cannot simultaneously know y_{1i} and y_{0i} .

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or we observe i in the control group, *i.e.*,

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but never both at the same time.

So what do we do?

If we want to know τ_i (or at least $\bar{\tau}$), what can we do?

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Idea: Estimate the **average treatment effect** as the difference between the average outcomes in the treatment group and the control group, *i.e.*,

$$E(y_i \mid D_i = 1) - E(y_i \mid D_i = 0)$$

where $D_i = 1$ if i received treatment, and $D_i = 0$ if i is in the control group.

Constant treatment effect

Result: Even when treatment effect is constant (meaning $\tau_i = \tau$ for all i),

$$\begin{aligned} & E(y_i \mid D_i = 1) - E(y_i \mid D_i = 0) \\ &= \tau + \underbrace{E(y_{0,i} \mid D_i = 1) - E(y_{0,i} \mid D_i = 0)}_{\text{Selection bias}} \end{aligned}$$

which says that the difference in the groups' means will give us a **biased estimate** for the causal effect of treatment **if we have selection bias**.

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Specifically, the control group doesn't give us a good **counterfactual** for what our treatment group would have looked like if the members had not received treatment. Basically, the groups are different.

A: (Formal) The *average untreated* outcome for a member of our **treatment group** (which we cannot observe) differs from the *average untreated* outcome for a member of our **control group**, i.e.,

$$E(y_{0,i} \mid D_i = 1) - E(y_{0,i} \mid D_i = 0)$$

Observing selection bias?

Practical problem: Selection bias is also difficult to observe

$$\underbrace{E(y_{0,i} \mid D_i = 1)}_{\text{Unobservable}} - E(y_{0,i} \mid D_i = 0)$$

(back to the *fundamental problem of causal inference*)

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Sounds a bit like omitted-variable bias, right? That's cause they're all forms of **endogeneity!**
Our **treatment** variable is correlated with something that makes the two groups different.

Hypothetical example

Example: Imagine we have two people—Al and Bri—and a single binary treatment, college. We are interested in the effect of college on earnings.

$$\text{Earn}_{1,\text{Al}} = \$60$$

$$\text{Earn}_{0,\text{Al}} = \$30\text{K}$$

$$\text{Earn}_{1,\text{Bri}} = \$80\text{K}$$

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but any real-world estimate would have serious selection issues since $\text{Earn}_{0,\text{Al}} \neq \text{Earn}_{0,\text{Bri}}$.

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What if we saw pre-treatment

- Think in the case of Bri and Al, but we now observe pre-college earnings

$$\text{Earn}_{1,\text{Al}}^{\text{Post}} = \$60\text{K}$$

$$\text{Earn}_{0,\text{Al}}^{\text{Pre}} = \$20\text{K}$$

$$\text{Earn}_{0,\text{Bri}}^{\text{Post}} = \$50\text{K}$$

$$\text{Earn}_{0,\text{Bri}}^{\text{Pre}} = \$40\text{K}$$

Bridge from fixed effects

- If we have individual fixed effects, we adjust for **average** differences between individuals (hometown, family, education, race, etc.)
- If treatment is randomly assigned within the fixed effect, i.e. over time within an individual, we're done!
- But what if Al graduated during a recession? Is the timing of college **exogenous**?

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- No! Recessions drive down earnings and his education is correlated with recession timing
- Can I just use a fixed effect for time to wash out the effect of the recession **on average**?

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- No! Recessions drive down earnings and his education is correlated with recession timing
- Can I just use a fixed effect for time to wash out the effect of the recession **on average**?
- Again, no! That would wash out all the variation in the treatment effect

Compare to Bri

- Let's say Bri did not go to college like Al, we've already seen we cannot just use $Earn_{1,Al} - Earn_{0,Bri}$ due to selection bias
- But we know that before college, their earnings differ by \$20K
- Let's adjust for that difference and see what happens

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$$Earn_{1,Al}^{Post} - Earn_{0,Bri}^{Post} - (Earn_{0,Al}^{Pre} - Earn_{0,Bri}^{Pre}) = \$60K - \$50K - (\$20K - \$40K) = \$30K$$

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- The actual effect was \$30K! (I forced it to work that way, life is more complicated -- sorry.)
- This is a method called difference-in-differences
 1. Difference in the groups' means before treatment
 2. Difference in the groups' means after treatment
 3. Difference in these differences

Difference what with who?

Difference-in-Differences

- Today we will talk about difference-in-differences (DiD), which is a way of using within variation in a more deliberate way in order to identify the effect we want
- At its simplest, we need a treatment that *goes into effect* at a particular time, and we need a group that is *treated* and a group that is *not*
- The untreated group, or control group, is our **counterfactual**
- Then, we compare the within-variation for the treated group vs. the within-variation for the untreated group
- Voila, we have an effect!

Difference-in-Differences

- Because the requirements to use it are so low, DiD is used a *lot*
- Any time a policy is enacted but isn't enacted everywhere at once? DiD!
- Plus, the logic is pretty straightforward
- The question DiD tries to answer is "what was the effect of (some policy) on the people who were affected by it?"
- We have some data on the people who were affected both before the policy went into effect and after
- However, we can't just compare before and after, because things usually change over time for other reasons
- So we compare to people who weren't affected by the policy

An old method

- DiD is an old method with its first documented use by John Snow in 1854 to identify that cholera was transmitted through water¹
- Cholera was a big problem in London in the 1800s. It causes severe diarrhea and vomiting, and eventually death
 - It was spread due to victims' waste finding its way into the Thames, the main water supply
- There were two water companies, Lambeth and Southwark and Vauxhall (SV)
- Lambeth moved its water pump higher upstream, while SV did not
- John Snow went door-to-door collecting info where people got their water in 1849 and counted cholera cases in 1849 and 1854

¹ Unlike Jon Snow, who knows nothing, John Snow knew quite a lot.

Cholera spread

- The analysis was not perfect, but it was convincing

supplier	1849	1854	diff
Non-Lambeth Only	134.9	146.6	11.7
Lambeth + Others	130.1	84.9	-45.2

- Resulting difference was -56.9 per 10K
- He did not calculate it, but he used it to argue that the water was the cause of the cholera

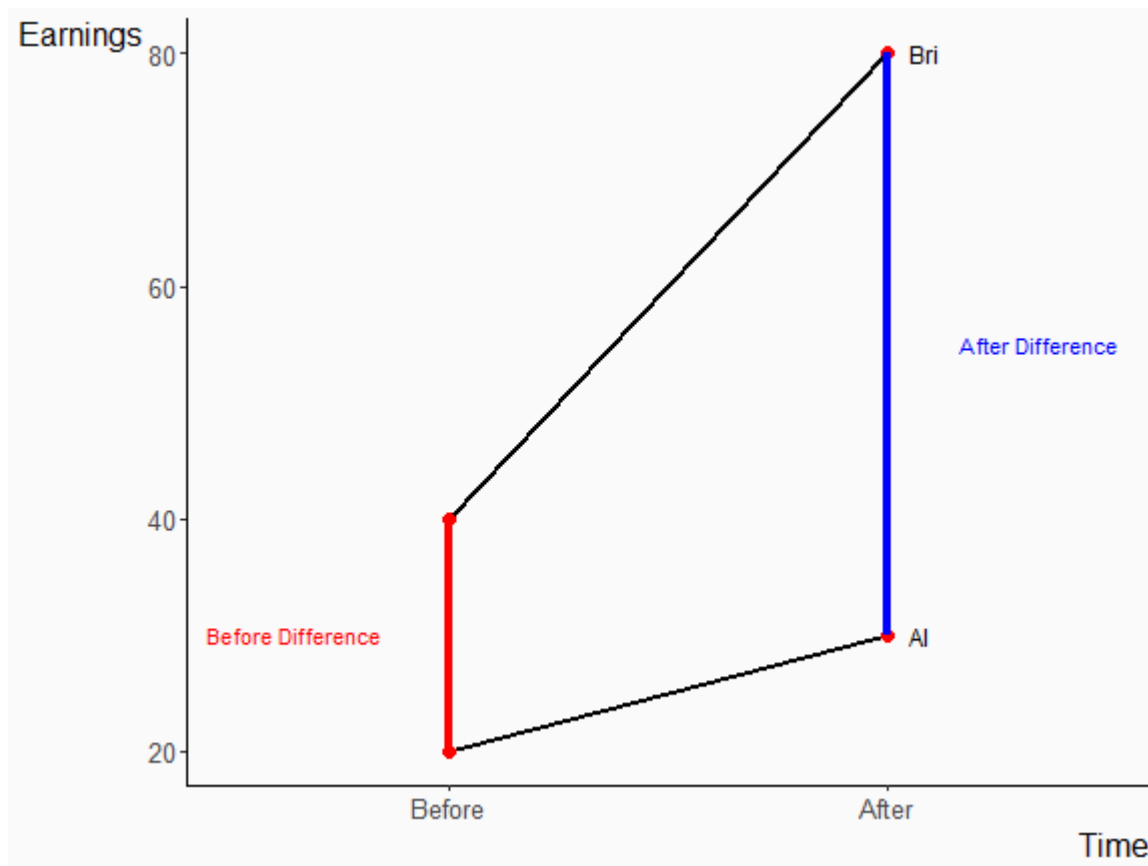
Check back on Bri and Al

$$\text{Earn}_{1,\text{Al}}^{\text{Post}} = \$60\text{K}$$

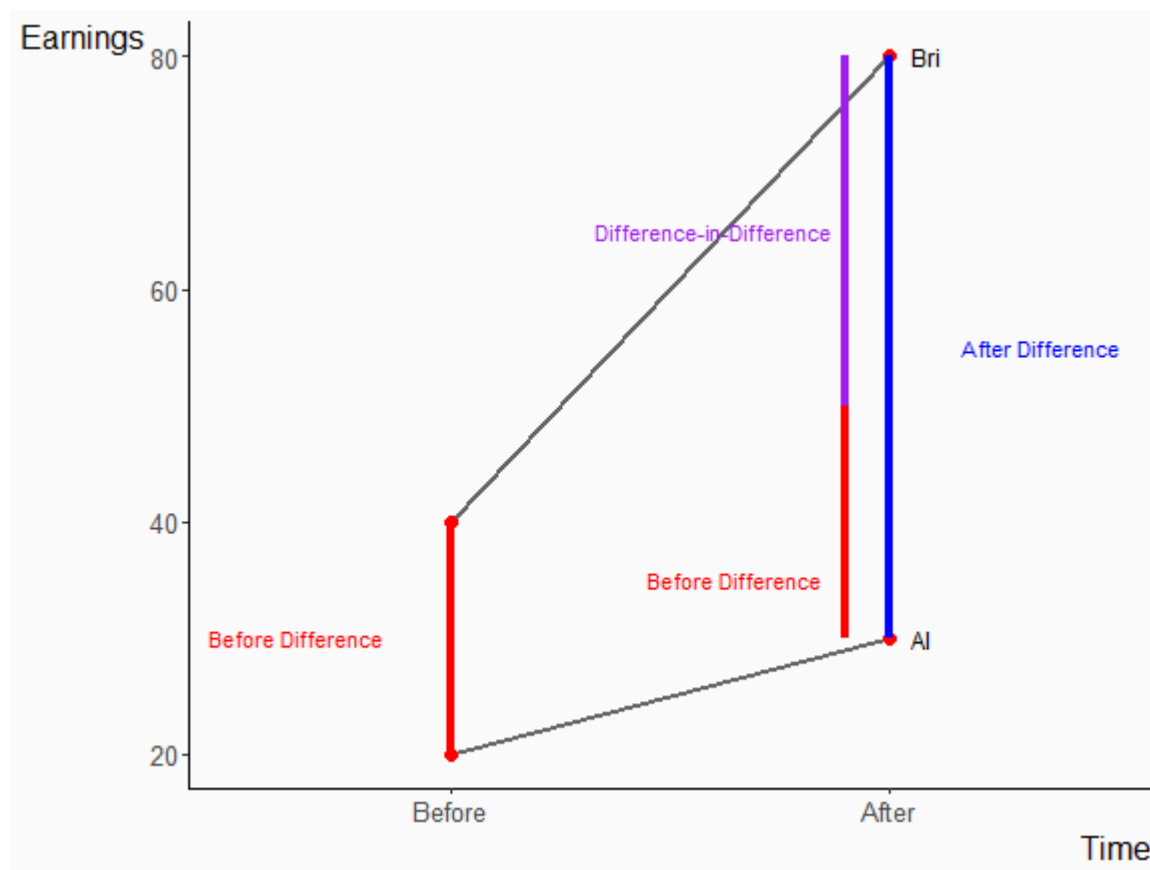
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Example



Difference-in-Differences

What *changes* are included in each value?

- **Untreated Before**: Untreated Group Mean
- **Untreated After**: Untreated Group Mean + Time Effect
- **Treated Before**: Treated Group Mean
- **Treated After**: Treated Group Mean + Time Effect + **Treatment Effect**
- **Untreated After** - **Untreated Before** = Time Effect
- **Treated After** - **Treated Before** = Time Effect + **Treatment Effect**

$$\text{DiD} = (\text{TA} - \text{TB}) - (\text{UA} - \text{UB}) = \text{Treatment Effect}$$

(Abbreviations for Untreated and Treated Before/After to save space)

Concept Checks:

menti.com, code 9338 7492

- Why do we need a control group? What does this let us do?
- What do we need to assume is true about our control group?
- In 2015, a new, higher minimum wage went into effect in Seattle, but this increase did not occur in some of the areas surrounding Seattle.
 - How might you use DiD to estimate the effect of this minimum wage change on employment levels?

Difference-in-Difference

- What if there are more than four data points?
- Usually these four points would be four means from lots of observations, not just two people in two time periods
- How can we do this and get things like standard errors, and perhaps include controls?
- Why, use OLS regression of course, just use binary variables and interaction terms to get a DiD

$$Y_{it} = \beta_0 + \beta_1 \textit{After}_t + \beta_2 \textit{Treated}_i + \beta_3 \textit{After}_t \textit{Treated}_i + \varepsilon_{it}$$

where \textit{After}_t is a binary variable for being in the post-treatment period, and $\textit{Treated}_i$ is a binary variable for being in the treated group

##	Person	Time	Earnings	After	Treated
## 1	Al	Before	20	FALSE	FALSE
## 2	Al	After	30	TRUE	FALSE
## 3	Bri	Before	40	FALSE	TRUE
## 4	Bri	After	80	TRUE	TRUE

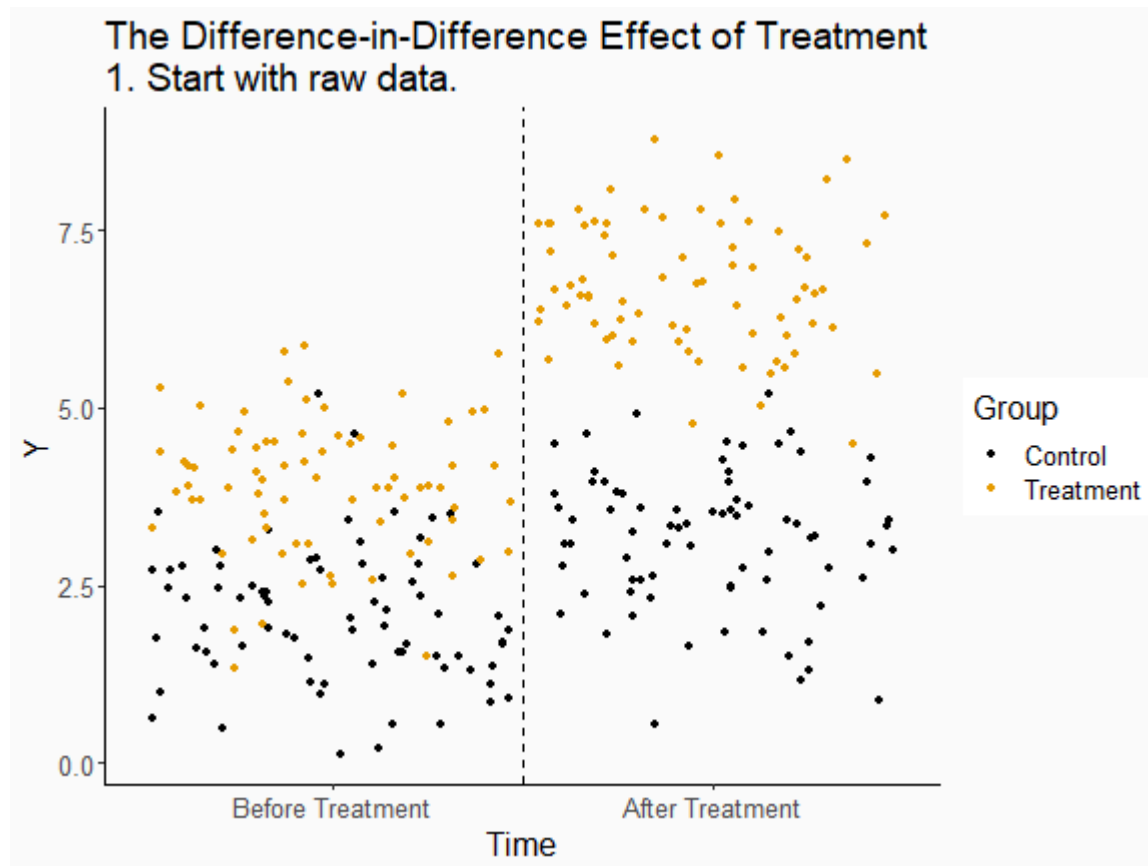
Difference-in-Differences

- How can we interpret this using what we know?

$$Y_{it} = \beta_0 + \beta_1 \textit{After}_t + \beta_2 \textit{Treated}_i + \beta_3 \textit{After}_t \times \textit{Treated}_i + \varepsilon_{it}$$

- β_0 is the prediction when $\textit{Treated}_i = 0$ and $\textit{After}_t = 0 \rightarrow$ the **Untreated Before** mean!
- β_1 is the *time difference* for $\textit{Treated}_i = 0 \rightarrow$ **UA** - **UB**
- β_2 is the *treatment difference* for $\textit{After}_t = 0 \rightarrow$ **BT**-**BU**
- β_3 is how much bigger the *Before-After difference* is for $\textit{Treated}_i = 1$ than for $\textit{Treated}_i = 0 \rightarrow$ (**TA** - **TB**) - (**UA** - **UB**) = DID!

Graphically



Design vs. Regression

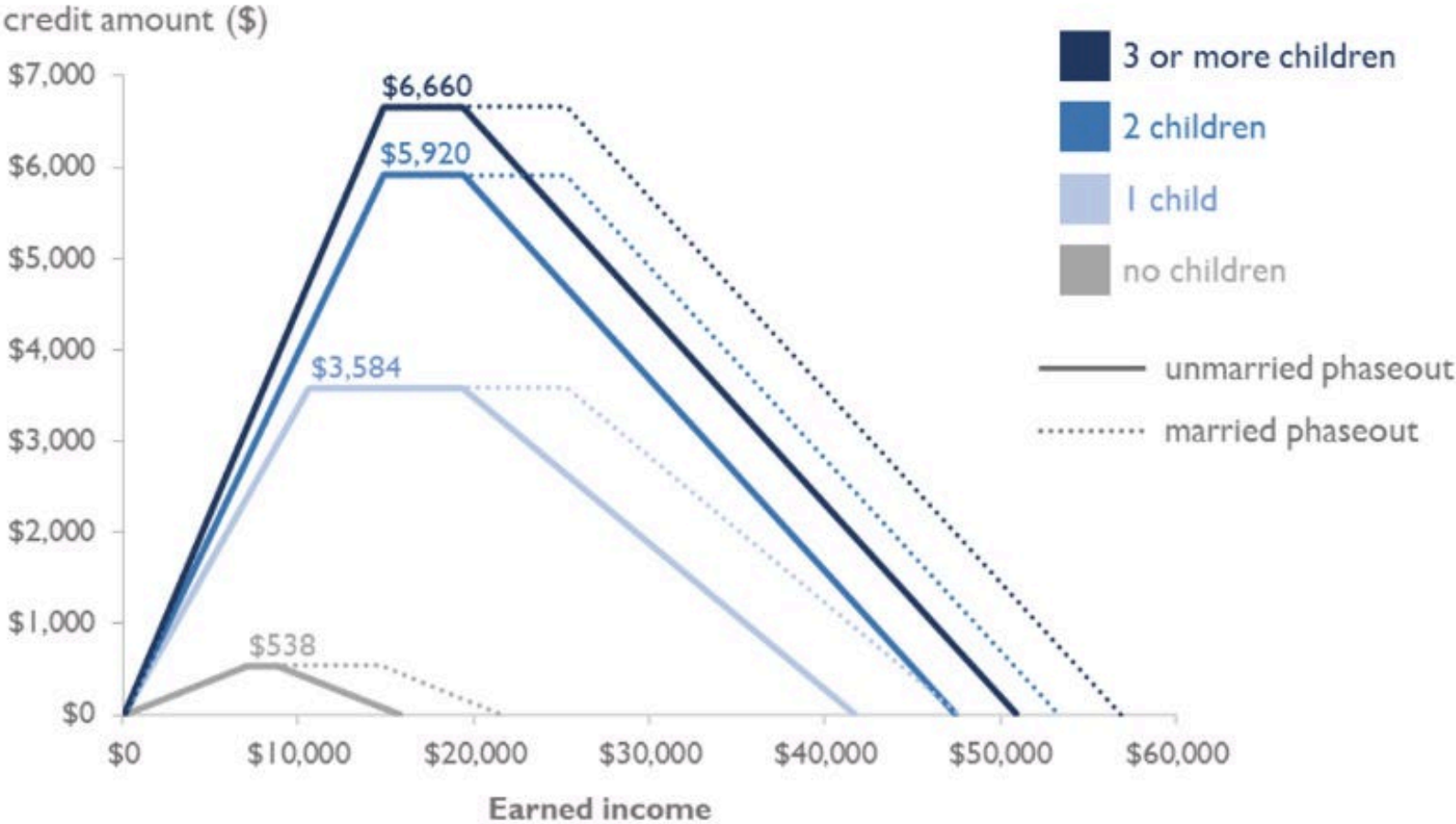
- There is a distinction between *regression model* and *research design*
 - This is also true for fixed effects
- We have a model with an interaction term
- Not all models with interaction terms are DID!
- It's DID because it's an interaction between treated/control and before/after
- If you don't have a before/after, or you don't have a control group, that same setup may tell you something interesting but it won't be DID!

Example: Earned Income Tax Credit

Earned Income Tax Credit

- The Earned Income Tax Credit (EITC) is a refundable tax credit for low-income workers
- Many argue it is the most effective anti-poverty program in the US as it increases the incentive to work
- Key features:
 - Initially increases as earned income increases before leveling off at a threshold, at next threshold it declines as income increases further
 - Increases with number of children
 - Phaseout differs by filing status (single vs. married)
- It was introduced in 1975 and expanded in 1986, 1990, and 1993 (we'll focus on 1993)
- Policy details vary, so we're gonna focus on single mothers vs. single women without kids to keep it simple

EITC Amount by Number of Qualifying Children, Marital Status, and Income, 2020



Earned Income Tax Credit

- EITC was increased in 1993. This may increase chances single mothers (treated) return to work, but likely not affect single non-moms (control)
- Does this program incentivize single mothers to work more?

```
eitc ← read_csv('http://nickchk.com/eitc.csv') %>%  
  mutate(after = year ≥ 1994,  
         treated = children > 0)  
eitc %>%  
  group_by(after, treated) %>%  
  summarize(proportion_working = mean(work))
```

```
## # A tibble: 4 × 3  
## # Groups:   after [2]  
##   after treated proportion_working  
##   <lgl> <lgl>           <dbl>  
## 1 FALSE FALSE         0.575  
## 2 FALSE TRUE          0.446  
## 3 TRUE  FALSE         0.573  
## 4 TRUE  TRUE          0.491
```


Example

- We can do it by just comparing the points, like we did with Al and Bri
- This will give us the DID estimate: The EITC increase increases the probability of working by 4.7 percentage points
- But not standard errors, or the ability to include controls easily

```
means <- eitc %>%  
  group_by(after, treated) %>%  
  summarize(proportion_working = mean(work)) %>%  
  pull(proportion_working)  
(means[4] - means[2]) - (means[3] - means[1])
```

```
## [1] 0.04687313
```

Let's try OLS!

```
etable(feols(work ~ after*treated, data = eitc), digits = 3, fitstat=c('n', 'r2'))
```

```
##                                feols(work ~ af..  
## Dependent Var.:                work  
##  
## Constant                      0.575*** (0.009)  
## afterTRUE                     -0.002 (0.013)  
## treatedTRUE                   -0.129*** (0.012)  
## afterTRUE x treatedTRUE       0.047** (0.017)  
## -----  
## S.E. type                      IID  
## Observations                  13,746  
## R2                            0.01260  
## ---  
## Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

Concept Checks: [menti.com](https://www.menti.com), code 9338 7492

- Interpret the coefficients in a sentence each.
- Why can the *After* \times *Treated* interaction stand in for *Treated*?

What if there are more groups?

- You can recognize $After_t$ and $Treated_i$ as *fixed effects*
- $Treated_i$ is a fixed effect for group - we only need one coefficient for it since there are only two groups
- And $After_t$ is a fixed effect for *time* - one coefficient for two time periods
- You can have more than one set of fixed effects like this! Our interpretation is now within-group *and* within-time
- (i.e. comparing the within-group variation across groups)

Example: Multiple Groups

Multiple Treated and Control Groups

- We can extend DID to having more than two groups, some of which get treated and some of which don't
- And more than two time periods! Multiple before and/or multiple after
- We don't have a full set of interaction terms, we still only need the one, which we can now call *CurrentlyTreated_{it}*
- **If you have more than two groups and/or more than two time periods, then this is what you should be doing**

Multiple treatment groups

- Let's make some quick example data to show this off, with the first treated period being period 7 and the treated groups being 1 and 9, and a true effect of 3

```
did_data <- tibble(group = sort(rep(1:10, 10)),
                    time = rep(1:10, 10)) %>%
  mutate(CurrentlyTreated = group %in% c(1,9) & time ≥ 7) %>%
  mutate(Outcome = group + time + 3*CurrentlyTreated + rnorm(100))
did_data
```

```
## # A tibble: 100 × 4
##   group time CurrentlyTreated Outcome
##   <int> <int> <lgl>          <dbl>
## 1     1     1     FALSE          1.50
## 2     1     2     FALSE          2.43
## 3     1     3     FALSE          3.70
## 4     1     4     FALSE          5.75
## 5     1     5     FALSE          4.57
## 6     1     6     FALSE          6.47
## 7     1     7     TRUE           11.1
## 8     1     8     TRUE           12.6
## 9     1     9     TRUE           12.6
## 10    1    10     TRUE           13.6
## # i 90 more rows
```

Multiple treatment groups

```
# Put group first so the clustering is on group
many_periods_did <- feols(Outcome ~ CurrentlyTreated | group + time, data = did_data)
etable(many_periods_did)
```

```
##                               many_periods_did
## Dependent Var.:                Outcome
##
## CurrentlyTreatedTRUE 3.095*** (0.4991)
## Fixed-Effects:      -----
## group                                Yes
## time                                Yes
## -----
## S.E.: Clustered                by: group
## Observations                    100
## R2                             0.96131
## Within R2                      0.34238
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

Two-way fixed effects model

- We just ran a two-way fixed effects model
- We have a fixed effect for group and a fixed effect for time
- This is often done when we have a panel dataset with individuals over time
- The generic formula is:

$$y_{it} = \underbrace{\alpha_i}_{\text{Individual FE}} + \underbrace{\alpha_t}_{\text{Time FE}} + \beta \textit{CurrentlyTreated}_{it} + \varepsilon_{it}$$

- Where did the treatment and pre-post indicator go?

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- Where did the treatment and pre-post indicator go?
- They're in the fixed effects! We don't need to include them separately

Downers and Assumptions

- So... does this all work?
- That example got pretty close to the truth of 3 but who knows in other cases!¹
- What needs to be true for this to work?

Downers and Assumptions

- So... does this all work?
- That example got pretty close to the truth of 3 but who knows in other cases!¹
- What needs to be true for this to work?
- DID and TWFE give a causal effect as long as *the only reason the gap changed* was the treatment
 - e.g. If Al's earnings were going up \$40K *anyway*, then we'd mistakenly attribute \$30K of that to college
- For TWFE to have a causal effect with panel data, we assume no endogenous variation across time within unit
 - It gets even messier if we have staggered rollout of treatment
- In DID, we need to assume that there's no endogenous variation *across this particular before/after time change*

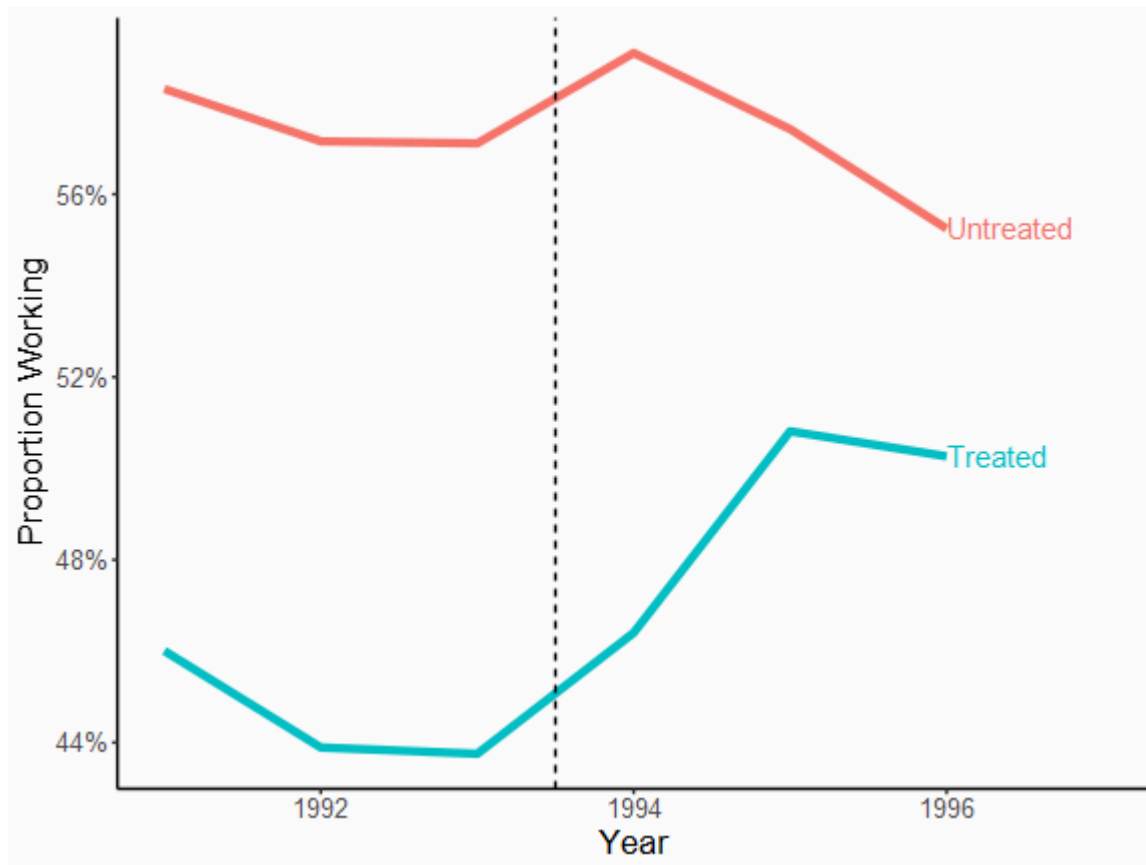
¹ We do know. It falls a lot.

Parallel Trends

- This assumption - that nothing else changes at the same time, is the poorly-named "parallel trends"
- Again, this assumes that, *if the Treatment hadn't happened to anyone*, the gap between the two would have stayed the same
- You can if *prior trends* are the same - if we have multiple pre-treatment periods, was the gap changing a lot during that period?
- There are methods to "adjust for prior trends" to fix parallel trends violations, or use related methods like Synthetic Control
 - These are beyond the scope of this class and also often snake oil

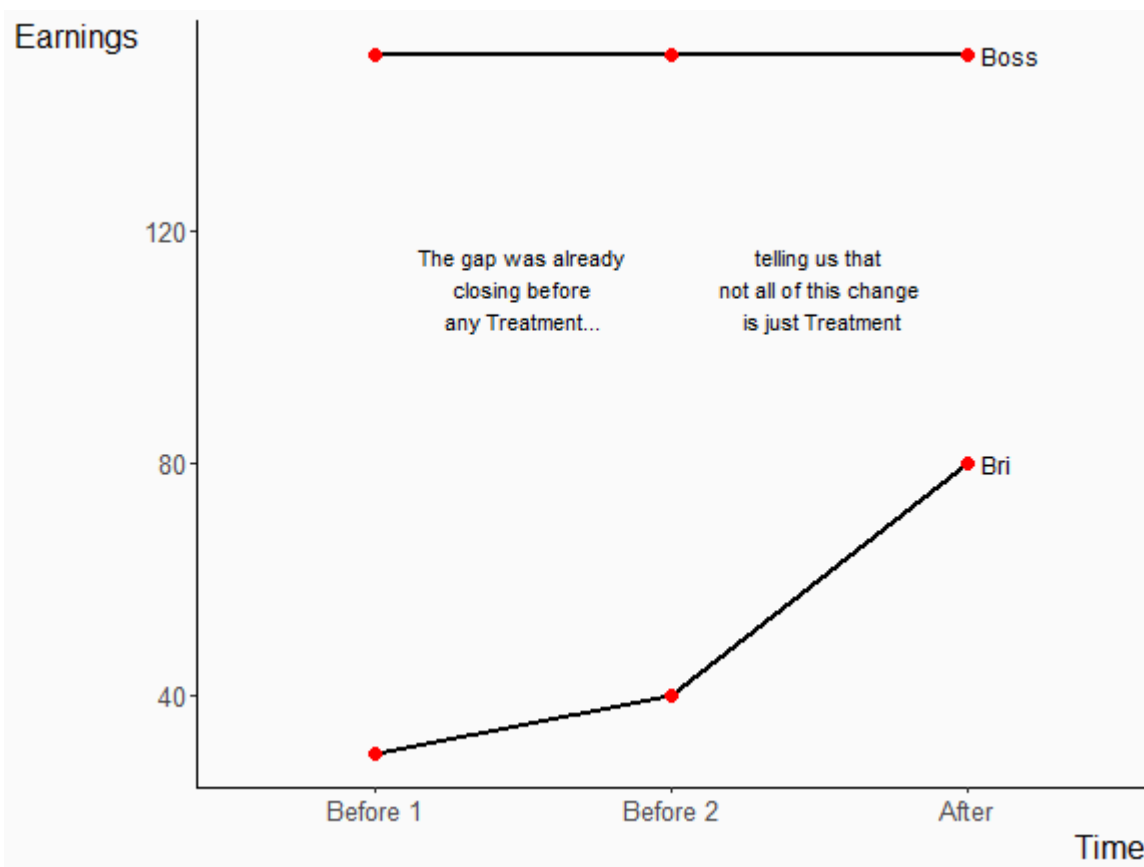
Prior Trends

- Let's see how that EITC example looks in the leadup to 1994
- They look like the gap between them is pretty constant before 1994! They move up and down but the *gap* stays the same. That's good.



Prior Trends

- Formally, prior trends being the same tells us nothing about parallel trends
- But it can be suggestive if the gap was closing *anyway*
 - For example, what if you compare earnings to Bri's boss



Parallel Trends

- Just because *prior trends* are equal doesn't mean that *parallel trends* holds.
- *Parallel trends* is about what the before-after change *would have been* - we can't see that!

Parallel Trends

- Just because *prior trends* are equal doesn't mean that *parallel trends* holds.
- *Parallel trends* is about what the before-after change *would have been* - we can't see that!
- For example, let's say we want to see the effect of online teaching on student test scores, using COVID school shutdowns to get a Before/After
- As of March/April 2020, some schools had gone online (Treated) and others hadn't (Untreated)
- Test score trends were probably pretty similar in the Before periods (Jan/Feb 2020), so prior trends are likely the same
- But LOTS of stuff changed between Jan/Feb and Mar/Apr, like, uh, Coronavirus, lockdowns, etc. not just online teaching! SO *parallel trends* likely wouldn't hold

Concept Checks

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- Go back to the Seattle minimum wage effect example from the first Concept Check slide. Clearly state what the parallel trends assumption means in this context.
- It's possible (although perhaps unlikely) that parallel trends can hold even if it looks like the treatment and control groups were trending apart before treatment went into effect. How is this possible?

Before we finish, a warning!

- DID is so nice and simple that it feels like you can get real flexible with it
- But the stuff we're covering in this class - up to TWFE, *relies very strongly* on the assumptions we made.
- If you break them, the *research design* may hold up, *but the estimator really doesn't* and you may need a different estimator

Context

- TWFE *quickly falls apart* if you have different groups getting treated at different times, called "staggered treatment"
 1. **Forbidden comparison:** Your early treated group becomes a control for your later treated group
 2. If the effect increases/decreases in relative time, your early treated group gives a "bad comparison"
 3. See slides by [Andrew Baker](#) for an example explanation of the problem
- If you need to control for stuff to support parallel trends, *just tossing in controls doesn't work quite so well with TWFE*

Fixes

- There are tons of fixes -- it is a bit of a cottage industry in econometrics and there's often package implementations in R, Stata, etc.
 - A few are even implemented in **fixest**, the package we're using for fixed effects
- The open source implementations are amazing and you can do a lot with them
- That way you just need to get the data and understand the research design
- You can leave the nuanced programming and linear algebra to the original authors
- Asjad Naqvi hosts a [list of tons of these packages](#)

What next?

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- List up to three things you learned today
- Now I want you to go try to implement fixed effects and difference-in-differences models in R
- There are tons of examples given to practice implementing the various R packages used to estimate these models
- Navigate to the lecture activity [11a-panel-twfe](#)

Next lecture: Regression Discontinuity Design
