Homework for Chapter 18: Difference-in-Differences

How Does It Work?

1. In the Event Studies chapter we estimated the effect of something that occurs at a specific time by just comparing before-event to after-event, without really using a control group. What assumption is made by no-control-group event studies that we *don't* have to make with difference-in-differences?

Without a control group, you make the assumption that the counterfactual change in the outcome before and after treatment is 0. With DinD instead, you assume that the counterfactual change in the outcome for the treated is equal to the change for a set of control units.

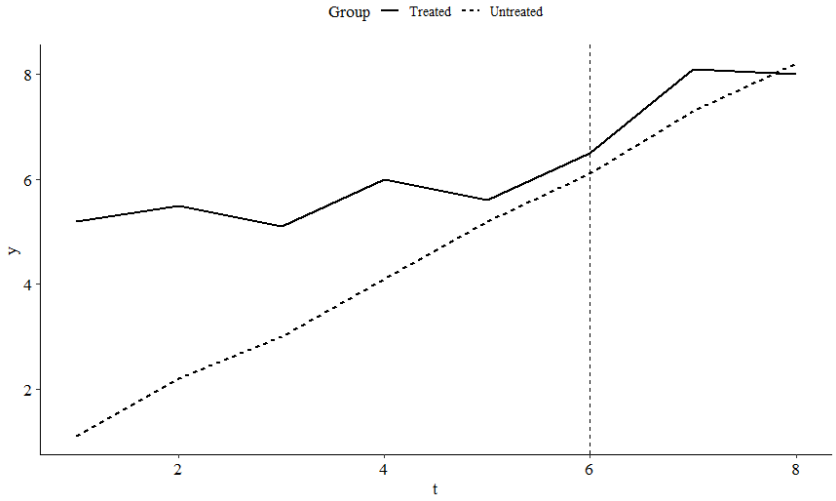
2. Which of the following potential back doors is controlled for by comparing the treated group to a control group?
    a. The treated group may be following a trend, unique to the group, that would make the outcome change from before-treatment to after-treatment anyway
    b. There may be events affecting everyone that would change the outcome from before treatment to after treatment anyway
    c. There may be differences in typical outcome levels between the treated group and the untreated group
    d. The decision to treat the treated group, rather than some other group, may be based on factors that are related to the outcome
3. Consider a treatment and control group. Looking only at the pre-treatment period, they have exactly the same outcomes (zero gap between them in each period).
    a. Despite having exactly the same outcomes pre-treatment, it happens to be the case that parallel trends is violated for these two groups. How is this possible? Explain what it means for parallel trends to be violated in this case, or give an example of how it could be violated.

The parallel trends assumption is the assumption that the gap in outcomes between counterfactual treated units and observed controls does not change after the treatment. The gap in outcomes can be 0 before treatment, but the control units be on a downward trend, while the treated units are not. This might be observable if we compare gaps in earlier time periods, but it need not be (because it's an assumption about the counterfactual and therefore unobservable). In this case, the parallel trends assumption would be violated.

    b. If we estimate the causal effect, in this case, using difference-in-differences, even though parallel trends is violated, how much would our effect be off by? (note you won't be able to give a specific number)

The difference between the true treatment effect and the estimated one in this scenario will be the difference between the counterfactual value for treated units after treatment and the observed outcome value for the controls after treatment.

4. Consider the below graph showing the average outcome for treated and control groups in the leadup to treatment (indicated by the dashed line), and also after treatment.



   a. Based on the prior trend, does it seem likely that parallel trends holds in this instance?

Although the trends are parallel between two time points (t5 and t6), the broader picture tells us that they are not (if we look at t1 - t6). Whether we think this smaller period of parallel trends is enough or not will depend on the scale of the time period and when we measure the outcome after treatment. If the x-axis refers to centuries and we can measure the outcome one year after treatment, two centuries of parallel trends is probably good enough for most outcomes. If the x-axis is seconds and we measure the outcome one second after treatment, then probably not (for any social science application at least).

   b. If we estimate difference-in-differences anyway, are we likely to overestimate the actual causal effect, underestimate it, or get it right on average?

Because the slope for the controls is steeper than for the treated, it is likely that we are biasing down the treatment effect (smaller than the real one).

5. In mid-2020, during the COVID-19 pandemic, different countries pursued different courses of action. Some locked down fully, imposing harsh penalties to most people for leaving the house outside certain proscribed times. Some were looser and only suggested staying at home, and some had hardly any restrictions at all. You notice that COVID rates tend to spike dramatically in different countries at seemingly-random times, and want to know if certain restrictions helped.

   From March through May 2020, US and Canada COVID case rates followed similar trends (US rates were higher, but the trends were similar). You want to look at the effect of COVID restrictions enacted in Canada in late May 2020 on case rates. Is DID, with the US as a control group, a good way to estimate this effect? If not, what concerns would you have about this research design?

Although DID can be a decent approximation, there are potential issues with this strategy. One assumption of the DID model is that the process that we are trying to estimate is a Markov process. In

Markov chains, the future states are only dependent on present states. In other words, the process is memoryless. In DID we take a dynamic process and split it into two time points, before and after, and we study how the outcome, for treatment and controls, moves from the before to the after the treatment period. So (even in dynamic DID) we assume that all the information relevant to estimate the counterfactual for the treated is contained in the time period before the treatment.

The problem is that most complex dynamics, infectious disease transmission being one of them, are not a Markov process. In the most simple model of infectious disease transmission, the number of infected people at time t will depend on the number infected at t-1, some constant rate of transmission, and the total number of people infected up until that moment (if immunity is permanent) or in the past X periods of time (with X being the period of immunity). If more people are susceptible to being infected, the transmission will be larger than if most or all people are already immune. So assuming only the measure before treatment is relevant for the measure after the treatment ignores the mechanics of this process. Similarly, people might react to the number of infections in a way that violates the markovicity of the process. For instance, people might look at the whole trend and make decisions about their health based on that (to stay home, for instance).

6. Consider the below table of mean outcomes, and calculate the difference-in-difference effect of treatment. Write out the equation you used to calculate it (i.e. show how the four numbers in the table are combined to get the estimate)

   (After_treated - Before_treated) - (After_untreated - After_untreated) = (9 - 5) - (7.5 - 6) = 4 - 1.5 = 2.5

	Before	AFTER
Treated	5	9
Untreated	6	7.5

How is it Performed?

7. You are planning to estimate whether voter-protection laws increase voter turnout. You note that, in 2015, a lot of new voter-protection laws were enacted in some provinces but not in others. Conveniently, no new laws were enacted in 2012, 2014, or 2016, so you decide to use 2012 and 2014 as your “before” periods and 2016 as “after”.
   a. Which of the following best describes what you’d want to regress state-and-year level “voter turnout” measures on?
      i. An indicator for whether the state is treated, and an indicator for whether the year is 2016.
      ii. A set of fixed effects for state, and a set of fixed effects for year.

iii. An indicator for whether the state is treated, a set of fixed effects for year, and an indicator for whether the state is currently treated.
iv. A set of fixed effects for state, and for year, and an interaction between "is 2016" and "is a treated state".
v. This design should not be estimated using a regression.

b. Unless you chose the final option in the previous question, specify which coefficient in that regression would give you the DID estimate.

The interaction term between "is 2016" and "is a treated state".

8. You are looking at a difference-in-difference design to estimate the effect of providing laptops to school children on their test scores. Look at the below regression output, in which "Treated" is an indicator that the school received laptops in 2008 as part of a new program (the untreated group did not receive any laptops until years after the sample window for this study ended), and "After" is an indicator for being after the year 2008.

Using the table, fill in the blanks in the sentence "Assuming that _____, the effect of laptops on test scores was _____, and this effect (was/was not) statistically significant at the 95% level."

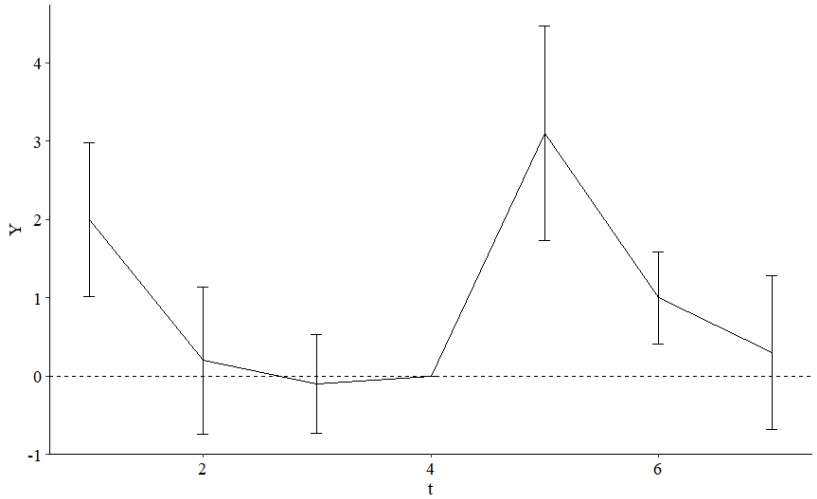
Assuming that the treated schools would have followed parallel trends in test scores to the control units, had they been untreated, the effect of laptops on test scores was an increase of 5 points, and this effect was statistically significant at the 95% level.

	Test Scores
(Intercept)	80.342***
	(0.501)
After	3.369***
	(0.696)
Treated	4.116***
	(0.718)
After× Treated	5.034***
	(0.993)
Num.Obs.	1523
R^2	0.188
Standard errors in parentheses. *+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001*	

9. A standard "prior trends" test might estimate a regression using the model $Y = \beta_0 + \beta_1 t + \beta_2 Treated + \beta_3 t \times Treated + \varepsilon$ (only using data from before-treatment), where t is a time variable, $Treated$ is an indicator for being in the treated group, and Y is an outcome variable, and look for a large/significant estimate of β_3. Explain why this test is performed, and specifically what it shows.

Beta_2 measures the average differences between treatment and control groups in Y across time. B_3 measures whether these differences change over time. If the parallel trends assumption is true in the period before treatment then B3 should be 0 because the differences between control and treated units are constant over time, independently of how large they are (B2).

10. Consider the below graph with estimates from a dynamic difference-in-differences model for a treatment that occurs between periods 4 and 5, with 95% confidence intervals shown.



c. What about this graph might make us concerned about our identification assumptions?
The fact that there seems to be a positive effect in time 1. This is physically impossible if the treatment only acts forward in time.

d. Ignoring any concerns we have, what would we say is the effect of treatment on Y in this case? (note the height of the line in period 5 is about 3, in period 6 is about 1, and in period 7 is about .5).
We can say that there is a positive effect of the treatment, increasing Y by three points at time 5, by 1 at time 6 and going back to around 0 at time 7. We can then say that the treatment effect is observable for two or three (depending on how much we care about the CIs) time periods.

11. Chapter 18.2.5 points out a problem with two-way fixed effects in cases where treatment is not all assigned at the same time, but rather different groups get treated at different times (a "rollout" design). In these designs, two-way fixed effects treats "already-treated" units, who were treated in earlier periods, as "control" units, as though they hadn't gotten treated at all. However, there's nothing theoretically wrong about using an already-treated unit as a control; the DID assumptions don't require that the control group be untreated, just that the gap between treated and control doesn't change when the treated group's treatment goes into effect. Why are we so concerned, then, about using an already-treated group as a control? You can answer generally, or use as an example a DID with only two groups – an already-treated group and a newly-treated group. (hint: to do the example, try assuming the treatment only has an effect for the single period after treatment, and the already-treated group is treated exactly one period before the treated group)

We care about this because treatment effects can be dynamic. Thus, the outcome for treated units might change as a consequence of the treatment in periods subsequent to the first period after treatment. This breaks the parallel trend assumption in a way that is difficult to notice. In the example that Nick proposes the outcome for the control group will be X + treatment effect in t0 and X in t1. For the treated it will be X in t0 and X + treatment effect in t1. Thus the DiD estimate will be 2X, when in fact the true effect is X.

More generally, with rollout treatments the effect will be overestimated if already treated units are used as controls of units treated within Z time periods of their treatment, Z being the number of time periods for which the treatment has an effect.