Chapter 6. Difference in Differences

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I. Introduction

With data from a randomized experiment, the simple comparison of the mean outcome in treatment and control groups (which we can define here as the "difference" estimator) provides an unbiased and consistent estimate of the average treatment effect, as discussed in Chapter 2. This is so because the randomization ensures there are no systematic differences in any "pre-treatment" variables, and, hence, confounding factors are balanced.

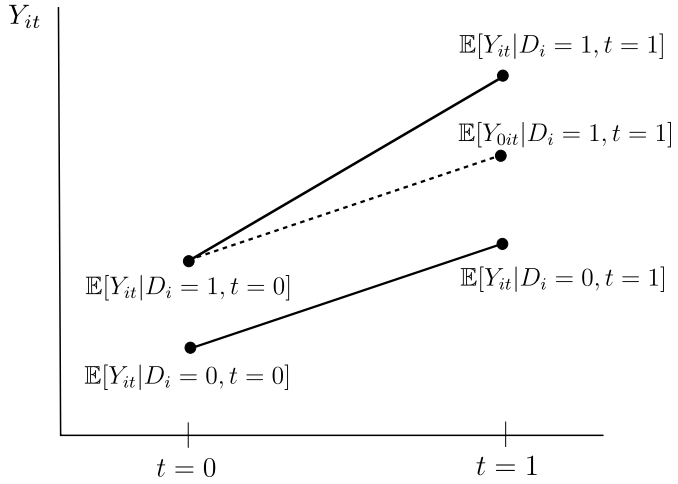
In subsequent chapters we have dealt with deviations from the independence assumption. In Chapter 3, and in sharp RD designs in Chapter 5, we proposed different techniques that balance out systematic differences among treated and control units, creating comparable groups, and, thus, ruling out confounders. In Chapter 4 and fuzzy RD designs in Chapter 5 we tried to get causal effects by using instrumental variables. However, good instruments are hard to find, and we would like to have other techniques to rule out unobserved confounders.

The approach in this chapter follows an approach that is closer to the first of the two broad approaches described in the previous paragraph. This approach proposes an alternative method to eliminate confounders that are fixed over time, using repeated observations over time. We assume that, even though treated and control groups are not comparable, the evolution of the outcome pre- and post-treatment would be the same in the absence of treatment. In other words, we assume that treated and control groups have the same counterfactual *trends*, even if the levels differ. In this case, we use data on treatment and control groups before the treatment to estimate the pre-treatment difference between these groups and then compare this difference with the difference in average outcomes after the treatment group received the treatment. Intuitively, we can use the pre-treatment comparison in outcomes among the two groups (none of them treated yet) to obtain and estimate of the selection bias, and then subtract that estimated selection bias to the difference in outcomes post-treatment to obtain the treatment effect.

II. Difference in Differences Setup

The figure below illustrates this discussion. Let Y_{it} denote the observed outcome for individual i in period $t \in \{0, 1\}$, and let $D_i = 1$ if the individual is in the treated

group, with $D_i = 0$ otherwise. Note that we did not subscript D_i by time in this notation, as $D_{it} = 0$ when $t = 0$ for both treated and untreated individuals. For treated individuals we observe $\mathbb{E}[Y_{it}|D_i = 1, t = 0] = \mathbb{E}[Y_{0it}|D_i = 1, t = 0]$, because at $t = 0$ no observation is treated, and $\mathbb{E}[Y_{it}|D_i = 1, t = 1] = \mathbb{E}[Y_{1it}|D_i = 1, t = 1]$, because these individuals are treated at $t = 1$. Likewise, for controls, we observe $\mathbb{E}[Y_{it}|D_i = 0, t = 0] = \mathbb{E}[Y_{0it}|D_i = 0, t = 0]$ as well, but, in this case, the mean observed in the second period is $\mathbb{E}[Y_{it}|D_i = 0, t = 1] = \mathbb{E}[Y_{0it}|D_i = 0, t = 1]$. What we do not observe is $\mathbb{E}[Y_{0it}|D_i = 1, t = 1]$, which we need to compute the average treatment effect on the treated:



What the figure suggests is to use the same trend observed for untreated individuals to predict the counterfactual trend for treated individuals in the absence of treatment. Thus, our prediction of the counterfactual value $\mathbb{E}[Y_{0it}|D_i = 1, t = 1]$ is:

$$\mathbb{E}[Y_{0it}|D_i = 1, t = 1] = \underbrace{\mathbb{E}[Y_{it}|D_i = 0, t = 1]}_{\text{level for controls at } t=1} + \underbrace{\{\mathbb{E}[Y_{it}|D_i = 1, t = 0] - \mathbb{E}[Y_{it}|D_i = 0, t = 0]\}}_{\text{difference in levels at } t=0 \text{ difference}}, \quad (1)$$

which builds on the fundamental assumption that $\mathbb{E}[Y_{0i1} - Y_{0i0}|D_i = 1] = \mathbb{E}[Y_{0i1} - Y_{0i0}|D_i = 0]$. This assumption is known as *the common trend assumption*, and, where there are multiple periods before treatment, it is typically checked by showing that trends before treatmend coincided. Hence, the difference in differences coefficient (which is an average treatment effect on the treated) is:

$$\begin{aligned}\beta &= \mathbb{E}[Y_{1it}|D_i = 1, t = 1] - \mathbb{E}[Y_{0it}|D_i = 1, t = 1] \\ &= \{\mathbb{E}[Y_{it}|D_i = 1, t = 1] - \mathbb{E}[Y_{it}|D_i = 1, t = 0]\} \\ &- \{\mathbb{E}[Y_{it}|D_i = 0, t = 1] - \mathbb{E}[Y_{it}|D_i = 0, t = 0]\}. \quad (2)\end{aligned}$$

Intuitively, β measures the difference between the increase in average observed outcomes for treated and the increase in average observed outcomes for controls.

III. Difference in Differences in the Regression Context

The difference in differences coefficient can be obtained as the β coefficient in the following regression:

$$Y_{it} = \beta_0 + \beta_D D_i + \beta_T T_{it} + \beta D_i T_{it} + U_{it}, \tag{3}$$

where $T_{it} = 1$ if individual i is treatment period $t = 1$, and $T_{it} = 0$ otherwise. With a proof that is very similar than those done in previous chapters, one can prove that β_0 is $\mathbb{E}[Y_{it}|D_i = 0, t = 0]$, $\beta_0 + \beta_D = \mathbb{E}[Y_{it}|D_i = 1, t = 0]$, $\beta_0 + \beta_T = \mathbb{E}[Y_{it}|D_i = 0, t = 1]$, and β is the difference in differences coefficient.

This regression model can be expanded in several ways. First, by including further periods, both before, and after the treatment. In such case, T_{it} is not a time dummy but, instead, a dummy that equals one in the post-treatment period. One could additionally include time effects, but the interaction term should be with the "post" dummy only. Second, the regression allows for controls, X_{it}. In this context, the difference between the regression coefficient and the difference in differences coefficient (obtained nonparametrically from differences in means) is analogous to the difference between matching and regression coefficients discussed in Chapter 3. Third, actually there is no need for panel data to estimate (3): repeated cross sections should suffice. However, in the repeated cross-section context, the researcher needs to sustain the assumption that the sample composition does not vary over time, which is satisfied by construction with panel data. Finally, some authors use the same regression setup to build *placebo exercises*. A placebo regression is a regression that simulates the difference in differences analysis but for a point in time or group of individuals that resemble the treatment period or group but that was actually not treated. It is a "placebo" in the sense that it looks as if treatment was administered, but it actually was not.

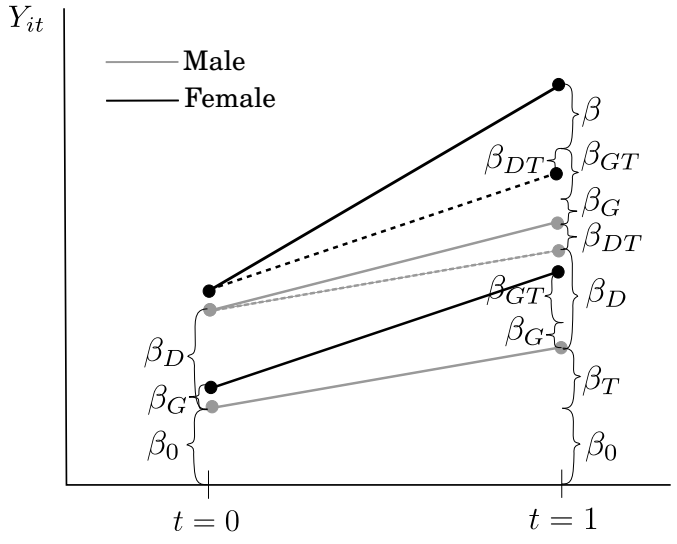
IV. Triple Differences Model

Some authors pose *triple-differences* models, in which the difference in differences assumption does not hold, but the change in trends is assumed to be the same across sub-groups, some of which should be more affected than others. For example, let G_i denote the (say sociodemographic) group to which individual i

belongs. Then, the triple-differences model is:

$$Y_{it} = \beta_0 + \beta_D D_i + \beta_T T_{it} + \beta_G G_i + \beta_{GD} G_i D_i + \beta_{GT} G_i T_{it} + \beta_{DT} D_i T_{it} + \beta G_i D_i T_{it} + U_{it}. \quad (4)$$

For example, consider the analysis of maternity leave policies on labor supply. These policies affect young women but do not affect old women. In this context, even though the labor supply of old women is systematically different than that of young women (level difference), this systematic difference persists before and after the policy, and, therefore, we can use old women as a control group in a differences in differences setting. Now imagine that, at the same time that the maternity leave policy is introduced, a tax reform occurs that particularly affects the labor supply of young workers relative to old workers. This additional policy would constitute a counfounder that would break the common trend assumption, because it affects the treated group only in the "post-reform" period, as the maternity leave policy change. However, we have a different group of people, males, that are equally affected by the tax reform, but not affected by the maternity leave policy. In this context, we can use a difference-in-difference estimation for male to "remove" the effect of taxes from the composite effect on female (taxes plus maternal leave policy). In this case, the key assumption is that taxes affect male of different ages in the same way that they affect female. The triple difference coefficients are easily interpreted in the following figure:



V. Synthetic Control Methods

Consider the case in which we have several periods before treatment is implemented, and, thus, we can check the common trends assumption. For example,

consider the case where one state implements a policy and other states do not. With enough data, we could define as the control the state that has the most similar pre-trend compared to the treated group (or alternatively, all non-treated states). However, often no state is the perfect counterfactual for another.

Synthetic control methods use longitudinal data to build the weighted average of non-treated units that best reproduces the characteristics of the treated unit over time prior to the treatment. Thus, we build an artificial control that has the best possible pre-trend possible, and then we compute the difference in differences estimate using such synthetic control group.

VI. Fixed effects and panel data

As we indicated above, there is no need to have panel data to estimate a difference in differences model. However, in the presence of panel data, one would estimate the following model.

$$Y_{it} = \beta_0 + \eta_i + \delta_t + \beta D_{it} + U_{it}. \tag{5}$$

In this regression model, the individual fixed effect η_i would capture the average outcome for each individual in the absence of treatment. Therefore, it would provide an individual-specific estimate of "$\mathbb{E}[Y_{0it}|i]$", the average Y_{0it} for individual i across time periods t. The time effects δ_t captures the average over-time trend in the absence of treatment. For example, in the two-period model, intuitively it would give us the counterpart of β_T in the simple regression above. Finally, β identifies the average treatment effect on the treated (when the treatment effect is constant), and it is identified from treated individuals, whose treatment status D_{it} changes over the observation period.

Notice that this regression allows us to identify the treatment effect from multiple treatments implemented at different points in time. This would mean that some observations contribute as control group in some treatments and as treatment group in others. As shown in Godman and Bacon (2021), the OLS estimate would provide a weighted average of all possible two-by-two difference in differences estimators for each policy change. The specific weights depend on the absolute size of each subsample (treated and control groups) and also about the timing of the treatments. In particular, weights are larger for treatments in which the sizes of treated and control are similar, and treatments that happen at a closer point to the middle of the time window considered. A direct implication of this is that changing the spacing of time periods changes the weights and therefore the obtained estimate, even if the underling difference-in-differences are themselves

constant. This is so, of course, unless the different treatment effects are indeed constant across treatments, in which case, we obtain the average treatment effect on the treated.

VII. Event studies

A natural implication of the previous paragraph is that, whether we have panel data or not, we can construct event studies in which timing is centred around the first treatment date. Let T_{0i} denote the timing in which individual i becomes treated. Define $S_{it} \equiv t - T_{0i}$ as the time period relative to the event. For untreated units, we may need to define the date when the "event" would start for them. In some cases, this is given by the event study itself (e.g. birth of first child in the child penalty example, discussed in class).

Intuitively, what we do is to transform regression (3), or any of its variants described in the paragraph below that equation, to the following regression:

$$Y_{it} = \beta_0 + \beta_D D_i + \beta_T S_{it} + \beta D_i S_{it} \, \mathbb{1}\{S_{it} \geq 0\} + U_{it}. \tag{6}$$

For example, in the child penalty example, we capture β_D by gender dummies, which capture the wage difference between men and women in the year prior of the birth of their first child. The term β_T is now captured by dummies or trends of period in the event (i.e., years since the birth of the first child, with negative values for years prior the child). Finally, the treatment effect β is identified from the differential change in wage for men and women in the years after the birth of the first child. Once again, even though this can be captured by time trends, this is often done including time dummies, in which case, we can allow for the dummies for $S_{it} < -1$ to be different from zero, as we only need to normalize the one for $S_{it} = -1$ to be equal to zero.