EEP/IAS 118 - Introductory Applied Econometrics, Section 12b

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Agenda

1. Difference in Differences
2. Regression Discontinuity Recap (from Section 11)

Difference-in-Difference: Intro

- Big question: how to recover causal estimates without randomization
- Big concern: OVB
- One approach: *Regression Discontinuity* (recap at end if time allows)
- But: there isn't always a clear "threshold" for inclusion in treatment over a continuous variable
- With panel data, *unit fixed effects* remove any possible OVB from time-invariant variables. But could still have OVB from time-varying unit characteristics.
- Another way to use panel data: even if treatment and control groups are different, we can sometimes exploit the timing of treatment to see how outcomes in treatment and control group *change* after treatment is implemented.

Diff-in-Diff: Calculation

Assume we have two groups (T and C) and two time periods (0 and 1). The program was implemented for the treatment group in time period 1: 0 is the "baseline" or "pre"-treatment period, 1 is the "post"-treatment period

1. Calculate difference in the outcome variable Y in the control group across the two time periods:

$$\bar{Y}_{C1} - \bar{Y}_{C0} = \Delta\bar{Y}_C$$

2. Do the same for treatment:

$$\bar{Y}_{T1} - \bar{Y}_{T0} = \Delta\bar{Y}_T$$

3. The impact of the program is measured by the difference in the differences:

$$(\bar{Y}_{T1} - \bar{Y}_{T0}) - (\bar{Y}_{C1} - \bar{Y}_{C0}) = (\Delta\bar{Y}_T - \Delta\bar{Y}_C)$$

DIff-in-Diff: Regression

We can also find the impact of the program through regression:

$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Post_t \times Treat_i + u_{it}$$

- $Post_t$ is a dummy that indicates time period 1
- $Treat_i$ is a dummy that indicates being in the treatment group

	Pre	Post
Control	Not Treated	Not Treated
Treatment	Not Treated	Treated

This has the benefit of giving us standard errors for the $\hat{\beta}$ so we can run hypothesis tests

- **Question:** Interpret what each of the β above indicates

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- β_0 gives you the baseline (pre-treatment) mean of Y in the Control group. $\bar{Y}_{C0}$
- β_1 gives you the average change in Y from the pre- to the post-treatment period for the Control group. $\bar{Y}_{C1} - \bar{Y}_{C0}$
  - This captures "secular change" that we allow to occur in the outcome due to factors other than the treatment. A key assumption is that this change is the same for everyone.
- β_2 is the difference in the baseline mean for the Treatment group, relative to Control. $\bar{Y}_{T0} - \bar{Y}_{C0}$
- β_3 is the difference in change over time in Y for the Treatment group relative to the control. $[\bar{Y}_{T1} - \bar{Y}_{T_0}] - [\bar{Y}_{C1} - \bar{Y}_{C0}]$

Diff-in-Diff: Assumption

Key assumption for this to work:

- *The difference between before and after in the control group is a good counterfactual for the treatment group*

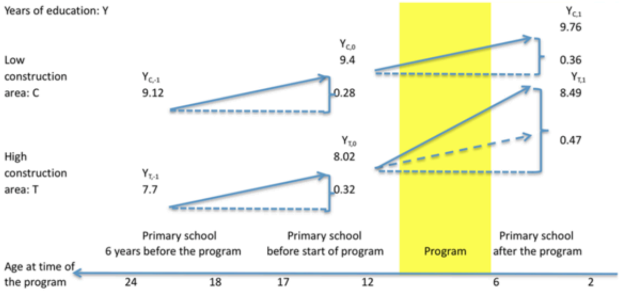
In other words, the change in outcomes for the control group is what we would have observed in the treatment group absent the policy. In the model

$$Y_{it} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Post_t \times Treat_i + u_{it}$$

It means that if the treatment never happened in period 1, we would have $\beta_3 = 0$: no difference in the change over time between the two groups.

Diff-in-Diff: Testing Assumption

- Assumption is fundamentally untestable
- Best we can do is analyze pre-trends (or baseline balance, though this is weak evidence)
- In order for DD to be valid, we want to see **parallel trends**
- Example from DD analysis of impact of primary school construction program on years of education in Indonesia:



Diff-in-Diff: Testing Assumption

To test for parallel trends, we need at least one more year of data before the intervention (*pre-pre-period*)

Then in a regression framework we can run the following estimation using data only from **BEFORE** treatment:

$$Y_{it} = \beta_0 + \beta_1 Pre_t + \beta_2 Treat_i + \beta_3 Pre_t \times Treat_i + u_{it}$$

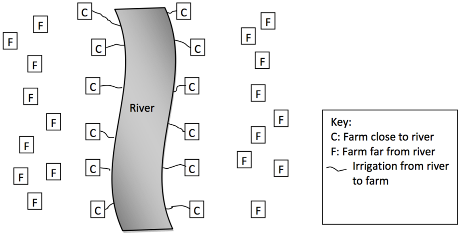
- Pre_t is a dummy that indicates being in the pre-period (as opposed to the pre-pre-period)
- $Treat_i$ is a dummy that indicates being in the treatment group

If parallel trends holds, we expect the coefficient on the interaction term to be statistically insignificant (can't reject $\beta_3 = 0$)

Example: Irrigation

Want to evaluate the effect of irrigation on farm yields.

- Naive estimation would just compare yields for farms close enough to the river to get irrigation to those who were not:



$$yield_i = \beta_0 + \beta_1 irrigation_i + u_i$$

- **What's the problem with this approach?**

Example: Irrigation

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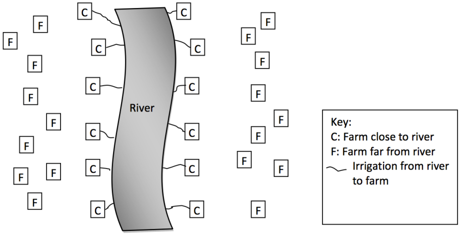
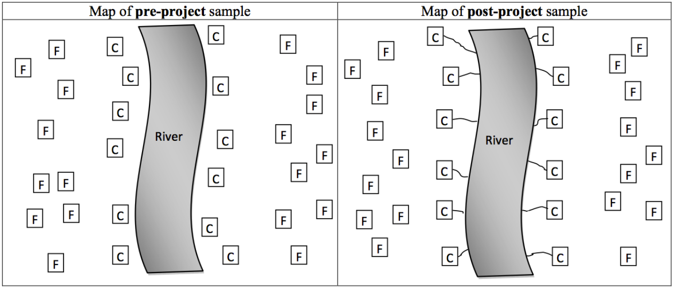


$$yield_i = \beta_0 + \beta_1 irrigation_i + u_i$$

- **Problem:** The farms that got irrigation are the farms that are close to the river! There are probably a lot of things that vary between C and F farms besides irrigation

Example: Irrigation

A better design would use two waves of data, one before the project was started and one after it was completed:



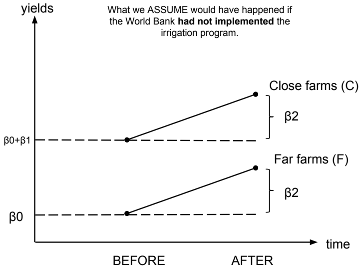
$$yield_{it} = \beta_0 + \beta_1 irrigation_i + \beta_2 post_t + \beta_3 (irrigation_i * post_t) + u_{it}$$

- $irrigation_i$ accounts for differences (some of which we can't observe) between the C and F farms, getting around the fact that the irrigation was not randomly assigned across farms.

Example: Irrigation

What is the identifying assumption for this estimation strategy?

- The difference between before and after in the comparison group is a good counterfactual for the treatment group
- We can also draw a picture to understand the diff in diff assumptions and strategy:



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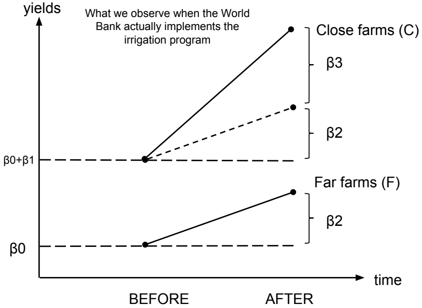

- The diff-in-diff strategy assumes that the entire difference in the slope of these two lines is due to the treatment (because we are assuming that the slopes *would* have been the same without the program).

Example: Irrigation

Exercise: Using these four data points for average yield, calculate what values for $\hat{\beta}_0, \hat{\beta}_1, \hat{\beta}_2$, and $\hat{\beta}_3$ we would obtain from a diff-in-diff regression:

Yields in Kg/acre

	Pre-Period	Post-Period
Far (Not Irrigated)	40	60
Close (Irrigated)	70	95

$$yield_{it} = \beta_0 + \beta_1 irrigation_i + \beta_2 post_t + \beta_3 (irrigation_i * post_t) + u_{it}$$

Example: Irrigation

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$$yield_{it} = \beta_0 + \beta_1 irrigation_i + \beta_2 post_t + \beta_3 (irrigation_i * post_t) + u_{it}$$

- $\hat{\beta}_0 = 40$
- $\hat{\beta}_1 = 30$
- $\hat{\beta}_2 = 20$
- $\hat{\beta}_3 = 5$

Example: Irrigation

What does β_3 give us? What if some close farms didn't get irrigated?

How do we test the validity of the diff-in-diff assumption?

Example: Irrigation

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- It gives us the average treatment effect (ATE)!
- If compliance is not perfect, it would give us the intention to treat effect (ITT).

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- It gives us the average treatment effect (ATE)!
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How do we test the validity of the diff-in-diff assumption?

- We want to test if parallel trends hold.
- If we had pre-treatment data, we could see whether the slope, or trends, in yields were the same for both groups leading up to the introduction of the irrigation.
- If the slopes are similar in the pre-period, then is more reasonable to assume they would have *continued* to have similar slopes

Example: Irrigation

What might we worried about even if parallel trends does hold?

Example: Irrigation

What might we worried about even if parallel trends does hold?

- If anything else is going on between the Pre and Post periods that affects close and far farms differently, this will be incorrectly attributed to the effect of irrigation
  - For example, if the irrigation project involved damming the river so that traders could no longer reach the farmers close to the river, this would affect the trade costs of farmers close to the river more than those far away and cause them to farm less intensively
  - In other words, anything changing differently between treatment and control groups can only be affecting outcomes through the treatment (where else have we seen something similar?)

Regression Discontinuity: Intro

In an RD design, we take advantage of policy quirks where treatment was assigned based on some threshold value of a "running variable." Examples include:

- Age
- Test scores
- Poverty line
- Number of students in a classroom

Basic idea of an RD is to compare the outcome variable for observations just below and just above the threshold.

- The expectation is that people just below and above the threshold are identical in all observable and non-observable characteristics, except for program participation.

Regression Discontinuity: Estimation

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 (RunningVar_i - threshold)$$
$$+\beta_3 T_i \times (RunningVar_i - threshold) + u_i$$

- $RunningVar_i$ is the running variable
- $threshold$ is the threshold value for being treated or not treated
- T_i is a dummy variable indicating whether the observation has a value of the running variable such that it received treatment

Question: What coefficient tells us the effect of the treatment?

Regression Discontinuity: Estimation

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 (RunningVar_i - threshold)$$
$$+\beta_3 T_i \times (RunningVar_i - threshold) + u_i$$

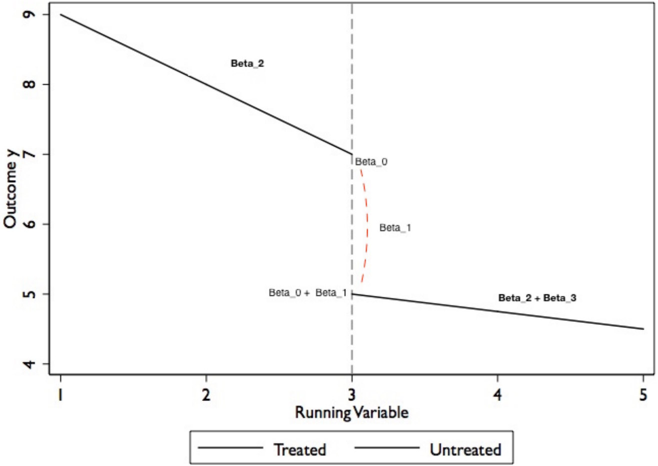
- $RunningVar_i$ is the running variable
- $threshold$ is the threshold value for being treated or not treated
- T_i is a dummy variable indicating whether the observation has a value of the running variable such that it received treatment

$\hat{\beta}_1$ **captures the effect of of the treatment**

We call it a **Local Average Treatment Effect (LATE)** around the threshold

Regression Discontinuity: Estimation

$$y_i = \beta_0 + \beta_1 T_i + \beta_2 (RunningVar_i - threshold)$$
$$+\beta_3 T_i \times (RunningVar_i - threshold) + u_i$$



Regression Discontinuity: Assumptions

Key Assumption:

- Relationship between outcome and running variable would be continuous around the threshold if it were not for the treatment
- In other words we need to assume that the treatment is the only reason why there would be a discontinuity

This assumption might be violated if:

- Participants in the program can manipulate the value of their running variable (e.g. mis-report income to receive subsidy).
- Characteristics of observations just above and just below threshold likely to be different (in ways that might be correlated with the outcome) if there is manipulation of the running variable.

Regression Discontinuity: Test Assumption

- Check for bunching of the running variable around the threshold - a sign of possible manipulation
- Test there are no discontinuities around the running variable threshold for relevant variables **other** than the treatment and the outcome variables
- Look at the averages of observable characteristics of household just above and below the threshold and make sure they're similar (kind of like in RCT)

$$x_i = \beta_0 + \beta_1 T_i + \beta_2 (RunningVar_i - threshold)$$
$$+\beta_3 T_i \times (RunningVar_i - threshold) + u_i$$

Here we want to find a coefficient of zero for our estimated $\hat{\beta}_1$, to encourage us that observations just above and below the threshold look similar on observables.

Regression Discontinuity: Example

If a youth who is less than 18 years old commits an offense, the case is sent to the more lenient juvenile courts. However, if a youth commits an offense after his/her 18th birthday, the case is sent to the much harsher adult criminal court. You have a cross-sectional data set of youths of ages 16-20 in Florida in 2005. This data set includes the birthday, gender, family income, and whether or not the youth had been arrested for committing an offense in the past year.

1. Write a regression to estimate the causal effect of harsher punishments on the probability of committing a crime. Which coefficient will give you the estimated causal effect?
2. What key assumption do you need to make for your regression to estimate the causal effect of harsher punishments on the probability of committing a crime?
3. What test can we conduct in support of our assumption?

Regression Discontinuity: Example

1. Write a regression to estimate the causal effect of harsher punishments on the probability of committing a crime (assume likelihood of being arrested for crime doesn't vary with age). Which coefficient will give you the estimated causal effect?

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$$arrest_i = \beta_0 + \beta_1 Over18_i + \beta_2(age_i - 6574) + \beta_3 Over18_i \times (age_i - 6574) + u_i$$

Where $arrest_i$ is a dummy variable equal to 1 if youth i was arrested for a criminal offense, age_i is age in days, and $Over18_i$ is a dummy variable equal to 1 if youth i is 18 or older. Note that 6574 is the number of days in 18 years. We could equivalently have set this up using age in months or weeks as the running variable rather than age in days. Age in years would likely be too broad of a range. The coefficient $\hat{\beta}_1$ will give us the LATE estimate of the effect of harsher punishments on the probability of committing a criminal offense.

Regression Discontinuity: Example

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Regression Discontinuity: Example

2. What key assumption do you need to make for your regression to estimate the causal effect of harsher punishments on the probability of committing a crime?

We have to assume that without the “treatment” of harsher punishments after age 18, the probability of committing an offense is a continuous function of age. That is, we have to assume that there are no other discontinuities in observable or unobservable characteristics around age 18, making the group just under the threshold on average similar to the group just above the threshold—except for the treatment. This makes those about to turn 18 a suitable counterfactual for those who just turned 18.

Regression Discontinuity: Example

3. What test can we conduct in support of our assumption?

Regression Discontinuity: Example

3. What test can we conduct in support of our assumption?

A test of the validity of the approach is that there are no discontinuities around the threshold for relevant variables other than the treatment and the outcome variable. With the data we have, we could run the same regression as in part 1) for family income.

Sharp vs Fuzzy RD

- Compliance with treatment assignments under a threshold eligibility rule may not be perfect.
  - In the above example, compliance would be perfect if once a youth turns 18 they are sent to the adult instead of the juvenile court without exceptions.
- We call situations of perfect compliance "Sharp" RD. We can use our RD approach to estimate the LATE.
- Imperfect compliance with a threshold-based treatment is sometimes called a "Fuzzy" RD. We can estimate a local ITT and TOT.
  - In Professor Magruder's study of irrigation canals on hillsides in Rwanda, about 5% of plots above the canal used irrigation, while only about 23% of plots under the canal used it.
- We can check whether we do in fact have a discontinuity around the threshold for treatment by running the RD regression with treatment take-up (in this example, use of irrigation) as the outcome variable.

What do We Need to Worry About With RD?

1. Is the effect really there? Should be demonstrable with a figure: look for a discontinuity around the threshold.
2. Does the cutoff matter? Run the RD specification with treatment takeup as the outcome variable.
3. Can the running variable be manipulated? Look for "bunching" around the threshold.
4. Does anything else change sharply at the cutoff?
5. Who are we identifying the effect for? We know that we identify a local ATE (LATE). This might not be the same as the effect globally. For example, estimates of the impact of the harshness of punishment on crime among youths around age 18 might not apply for older adults.