
FIN 620

Emp. Methods in Finance

Lecture 6 – Natural Experiment *[P1]*

Professor Todd Gormley

Announcements

- If interested in optional exercises, material in Exercise #3 covers today's stuff but also a bit of IV material
 - **Rough draft of proposal due after next class (at noon)**
 - Should upload to Canvas
 - I'll try to give feedback by next week
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Background readings

- Roberts and Whited
 - *Sections 2.2, 4*
- Angrist and Pischke
 - *Section 5.2*

Outline for Today

- Quick review of IV regressions
 - Discuss natural experiments
 - How do they help?
 - What assumptions are needed?
 - What are their weaknesses?
 - Student presentations of “IV” papers
-

Quick Review *[Part 1]*

- Two necessary conditions for an IV
 - **Relevance condition** – IV explains problematic regressor after conditioning on other x 's
 - **Exclusion restriction** – IV does not explain y after conditioning on other x 's
 - We can only test relevance condition
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Quick Review *[Part 2]*

- Angrist (1990) used randomness of Vietnam draft to study effect of military service on Veterans' earnings
 - Person's draft number (which was random) predicted likelihood of serving in Vietnam
 - He found, using draft # as IV, that serving in military reduced future earnings

Question: What might be a concern about the external validity of his findings, and why?

Quick Review [Part 3]

- **Answer = IV** only identifies effect of serving on those that served because of being drafted
 - E.g., his finding doesn't necessarily tell us what the effect of serving is for people that would serve *regardless* of whether they are drafted or not
 - Must keep this **local average treatment effect** (LATE) in mind when interpreting IV
-

Quick Review *[Part 4]*

- **Question:** Are more instruments necessarily a good thing? If not, why not?
 - **Answer = ...**

Quick Review *[Part 5]*

- **Question:** How can overidentification tests be used to prove the IV is valid?
 - **Answer** = Trick question! They cannot be used in such a way. They rely on the assumption that at least one IV is good. You must provide a convincing economic argument as to why your IVs make sense!
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Natural Experiments – *Outline*

- Motivation and definition
 - Understanding treatment effects
 - Two types of simple differences
 - Difference-in-differences
-

Recall... CMI assumption is key

- A violation of conditional mean independence (CMI), such that $E(u | x) \neq E(u)$ precludes our ability to make causal inferences

$$y = \beta_0 + \beta_1 x + u$$

- $\text{Cov}(x, u) \neq 0$ implies CMI is violated

CMI violation implies non-randomness

- Another way to think about violation is that it indicates that our x is non-random
 - **I.e., the distribution of x (or the distribution of x after controlling for other observable covariates) isn't random**
 - E.g., firms with high x might have higher y (beyond just the effect of x on y) because high x is more likely for firms with some omitted variable contained in u ...

Randomized experiments are great...

- In many of the “hard” sciences, the researcher can simply design experiment to achieve the necessary randomness
 - Ex. #1 – To determine effect of new drug, you randomly give it to certain patients
 - Ex. #2 – To determine effect of certain gene, you modify it in a random sample of mice
-

But we simply can't do them 😞

- We can't do this in corporate finance!
 - E.g., we can't randomly assign a firm's leverage to determine its effect on investment
 - And we can't randomly assign CEOs' # of options to determine their effect on risk-taking
 - Therefore, we need to rely on what we call “Natural experiments”
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Defining a Natural Experiment

- Natural experiment is basically when some event causes a random assignment of (or change in) a variable of interest, x
 - Ex. #1 – Some weather event increases leverage for a random subset of firms
 - Ex. #2 – Some change in regulation reduces usage of options at a random subset of firms
-

Nat. Experiments Provide Randomness

- We can use such “natural” experiments to ensure that randomness (i.e., CMI) holds and make causal inferences!
 - E.g., we use the randomness introduced into x by the natural experiment to uncover the causal effect of x on y

NEs can be used in many ways

- Technically, natural experiments can be used in many ways
 - Use them to construct IV
 - E.g., gender of first child being a boy used in Bannedsen, et al. (2007) is an example NE
 - Use them to construct regression discontinuity
 - E.g., cutoff for securitizing loans at credit score of 620 used in Keys, et al. (2010) is a NE
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And the Difference-in-Differences...

- But admittedly, when most people refer to natural experiment, they are talking about a difference-in-differences (DiD) estimator
 - Basically, compares outcome y for a “treated” group to outcome y for “untreated” group where treatment is randomly assigned by the natural experiment
 - **This is how I’ll use NE in this class**
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Natural Experiments – *Outline*

- Motivation and definition
 - Understanding treatment effects
 - Notation and definitions
 - Selection bias and why randomization matters
 - Regression for treatment effects
 - Two types of simple differences
 - Difference-in-differences
-

Treatment Effects

- Before getting into natural experiments in context of difference-in-differences, it is first helpful to describe “treatment effects”
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Notation and Framework

- Let d equal a treatment indicator from the experiment we will study
 - $d = 0 \rightarrow$ untreated by experiment (*i.e., control group*)
 - $d = 1 \rightarrow$ treated by experiment (*i.e., treated group*)
 - Let y be the potential outcome of interest
 - $y = y(0)$ for untreated group
 - $y = y(1)$ for treated group
 - Easy to show that $y = y(0) + d[y(1) - y(0)]$
-

Example treatments in corp. fin...

- Ex. #1 – Treatment might be that your firm's state passed anti-takeover law
 - $d = 1$ for firms incorporated in those states
 - y could be several things, e.g., ROA
- Ex. #2 – Treatment is that your firm discovers workers exposed to carcinogen
 - $d = 1$ if have exposed workers
 - y could be several things, like M&A

Average Treatment Effect (ATE)

- Can now define some useful things
 - **Average Treatment Effect (ATE)** is given by

$$E[y(1) - y(0)]$$

- What does this mean in words?
 - **Answer:** The expected change in y from being treated by the experiment; this is the causal effect we are typically interested in uncovering!
-

But ATE is unobservable

$$E[y(1) - y(0)]$$

- Why can't we directly observe ATE?
 - **Answer** = We only observe one outcome...
 - If treated, we observe $y(1)$; if untreated, we observe $y(0)$. We never observe both.
 - I.e., we cannot observe the counterfactual of what your y would have been absent treatment
-

Defining ATT

- **Average Treatment Effect if Treated (ATT)**

is given by $E[y(1) - y(0) \mid d = 1]$

- This is the effect of treatment on those that are treated; i.e., change in y we'd expect to find if treated random sample from population of observations that are treated
 - **What don't we observe here?**
 - **Answer** = $y(0) \mid d = 1$
-

Defining ATU

- **Average Treatment Effect if Untreated (ATU)**
is given by $E[y(1) - y(0) \mid d = 0]$
 - This is what the effect of treatment would have been on those that are not treated by the experiment
 - We don't observe $y(1) \mid d = 0$

Uncovering ATE [Part 1]

- How do we estimate ATE, $E[y(1) - y(0)]$?
 - **Answer** = We instead rely on $E[y(1) | d = 1] - E[y(0) | d = 0]$ as our way to *infer* the ATE

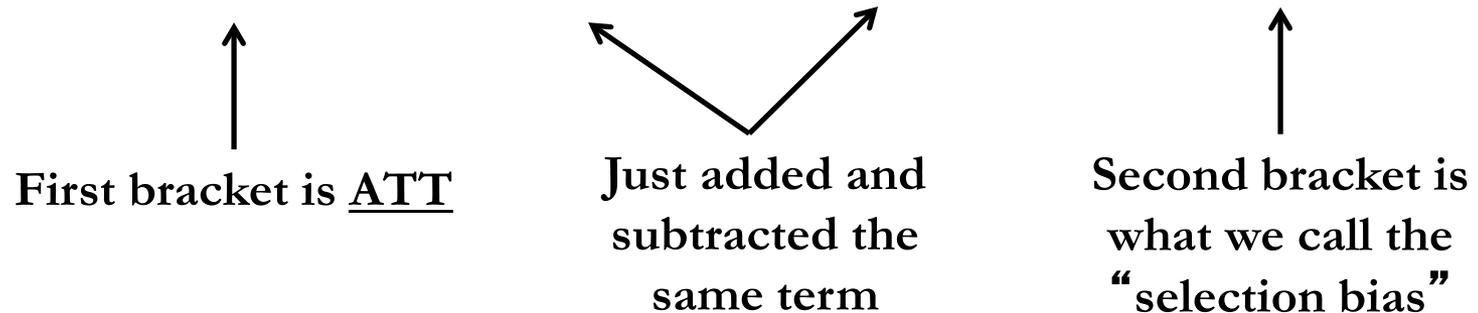
In words, what are we doing
& what are we assuming?

Uncovering ATE [*Part 2*]

- In words, we compare average y of treated to average y of untreated observations
 - If we interpret this as the ATE, we are assuming that absent the treatment, the treated group would, on average, have had same outcome y as the untreated group
 - We can show this formally by simply working out $E[y(1) | d = 1] - E[y(0) | d = 0] \dots$
-

Uncovering ATE [Part 3]

$$\{E[y(1) | d = 1] - E[y(0) | d = 1]\} + \{E[y(0) | d = 1] - E[y(0) | d = 0]\}$$



- Simple comparison doesn't give us the ATE!
In fact, the comparison is rather meaningless!
- What is the “**selection bias**” in words?

Natural Experiments – *Outline*

- Motivation and definition
 - Understanding treatment effects
 - Notation and definitions
 - Selection bias and why randomization matters
 - Regression for treatment effects
 - Two types of simple differences
 - Difference-in-differences
-

Selection bias defined

- Selection bias: $E[y(0) | d = 1] - E[y(0) | d = 0]$
 - **Definition** = What the difference in average y would have been for treated and untreated observations absent any treatment
 - *We do not observe this counterfactual!*
 - Now let's see why randomness is key!
-

Introducing random treatment

- A random treatment, d , implies that d is independent of potential outcomes; i.e.,

$$E[y(0) | d = 1] = E[y(0) | d = 0] = E[y(0)]$$

and

$$E[y(1) | d = 1] = E[y(1) | d = 0] = E[y(1)]$$

In words, the expected value of y is the same for treated and untreated absent treatment

- With this, easy to see that selection bias = 0
- **And** remaining ATT is equal to ATE!

Random treatment makes life easy

- I.e., with random assignment of treatment, our simple comparison gives us the ATE!
 - This is why we like randomness!
 - But, absent randomness, we must worry that any observed difference is driven by selection bias
-

Natural Experiments – *Outline*

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-

ATE in Regression Format *[Part 1]*

- Can re-express everything in regression format

$$y = \beta_0 + \beta_1 d + u$$

$$\beta_0 = E[y(0)]$$

where $\beta_1 = y(1) - y(0)$

$$u = y(0) - E[y(0)]$$

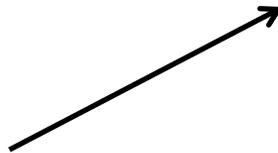
This regression will only give consistent estimate of β_1 if $\text{cov}(d, u) = 0$; i.e., treatment, d , is random, and hence, uncorrelated with $y(0)$!

- If you plug-in, it will get you back to what the true model, $y = y(0) + d[y(1) - y(0)]$

ATE in Regression Format *[Part 2]*

- We are interested in $E[y | d = 1] - E[y | d = 0]$
 - But can easily show that this expression is equal to

$$\beta_1 + E[y(0) | d = 1] - E[y(0) | d = 0]$$



**Our estimate will
equal true effect plus
selection bias term**

**Note: Selection bias
term occurs only if
CMI isn't true!**

Adding additional controls [Part 1]

- Regression format also allows us to easily put in additional controls, X
 - Intuitively, comparison of treated and untreated just becomes $E[y(1) | d = 1, X] - E[y(0) | d = 0, X]$
 - Same selection bias term will appear if treatment, d , isn't random after conditioning on X
 - Regression version just becomes

$$y = \beta_0 + \beta_1 d + \Gamma X + u$$

Why might there still be a selection bias?

Adding additional controls *[Part 2]*

- Selection bias can still be present if treatment is correlated with unobserved variables
 - As we saw earlier, it is what we can't observe (and control for) that can be a problem!

Question: If we had truly randomized experiment, are controls necessary?

Adding additional controls *[Part 3]*

- **Answer:** No, controls are not necessary in truly randomized experiment
 - But they can be helpful in making the estimates more precise by absorbing residual variation... we'll talk more about this later

Treatment effect – *Example*

- Suppose compare leverage of firms with and without a credit rating [or equivalently, regress leverage on indicator for rating]
 - Treatment is having a credit rating
 - Outcome of interest is leverage

Why might our estimate not equal ATE of rating?

Why might controls not help us much?

Treatment effect – *Example Answer*

- **Answer #1:** Having a rating isn't random
 - Firms with rating likely would have had higher leverage anyway because they are larger, more profitable, etc.; selection bias will be positive
 - Selection bias is basically an omitted var.!
 - **Answer #2:** Even adding controls might not help if firms also differ in unobservable ways, like investment opportunities
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Heterogeneous Effects

- Allowing the effect of treatment to vary across individuals doesn't affect much
 - Just introduces additional bias term
 - Will still get ATE if treatment is random...
broadly speaking, randomness is key
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Natural Experiments – *Outline*

- Motivation and definition
 - Understanding treatment effects
 - Two types of simple differences
 - Cross-sectional difference & assumptions
 - Time-series difference & assumptions
 - Miscellaneous issues & advice
 - Difference-in-differences
- We actually just
did this one!
- 

Cross-sectional Simple Difference

- Very intuitive idea
 - Compare post-treatment outcome, y , for treated group to the untreated group
 - I.e., just run following regression...

In regression format...

- Cross-section simple difference

$$y_{i,t} = \beta_0 + \beta_1 d_i + u_{i,t}$$

- $d = 1$ if observation i is in treatment group and equals zero otherwise
- Regression only contains post-treatment time periods

What is needed for β_1 to capture the true (i.e., causal) treatment effect?

Identification Assumption

- **Answer:** $E(u | d) = 0$; i.e., treatment, d , is uncorrelated with the error
 - In words... after accounting for effect of treatment, the expected level of y in post-treatment period isn't related to whether you're in the treated or untreated group
 - *I.e.*, expected y of treated group would have been same as untreated group *absent* treatment
-

Another way to see the assumption...

$$E[y | d = 1] - E[y | d = 0]$$

This is causal interpretation of coefficient on d

$$(\beta_0 + \beta_1 + E[u | d = 1]) - (\beta_0 + E[u | d = 0])$$
$$\beta_1 + E[u | d = 1] - E[u | d = 0]$$

CMI assumption ensures these last two terms cancel such that our interpretation matches causal β_1

- Then, plugging in for $u = y(0) - E[y(0)]$, which is what true error is (see earlier slides)...

$$\beta_1 + E[y(0) | d = 1] - E[y(0) | d = 0]$$

I.e., we must assume no selection bias

Multiple time periods & SEs

- If have multiple post-treatment periods, need to be careful with standard errors
 - Errors $u_{i,t}$ and $u_{i,t+1}$ likely correlated if dependent variable exhibits serial correlation
 - E.g., we observe each firm (treated and untreated) for five years after treatment (e.g., regulatory change), and our post-treatment observations are not independent
-

Multiple time periods & SEs – *Solution*

- Should do one of two things
 - Collapse data to one post-treatment per unit; e.g., for each firm, use average of the firm's post-treatment observations
 - Or cluster standard errors at firm level
[We will come back to clustering in later lecture]
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-

Time-series Simple Difference

- Very intuitive idea
 - Compare pre- and post-treatment outcomes, y , for just the treated group
[i.e., pre-treatment period acts as 'control' group]
 - I.e., run following regression...
-

In Regression Format

- Time-series simple difference

$$y_{i,t} = \beta_0 + \beta_1 p_t + u_{i,t}$$

- $p_t = 1$ if period t occurs after treatment and equals zero otherwise
- Regression contains only observations that are treated by “experiment”

What is needed for β_1 to capture the true (i.e., causal) treatment effect?

Identification Assumption

- **Answer:** $E(u|p) = 0$; i.e., post-treatment indicator, p , is uncorrelated with the error
 - I.e., after accounting for effect of treatment, p , the expected level of y in post-treatment period wouldn't have been any different than expected y in pre-treatment period

Showing the assumption math...

This would be causal interpretation of coefficient on p

$$\begin{aligned} & E[y | p = 1] - E[y | p = 0] \\ & (\beta_0 + \beta_1 + E[u | p = 1]) - (\beta_0 + E[u | p = 0]) \\ & \beta_1 + E[u | p = 1] - E[u | p = 0] \\ & \beta_1 + E[y(0) | p = 1] - E[y(0) | p = 0] \end{aligned}$$

← Same selection bias term... our estimated coefficient on p only matches true causal effect if this is zero

Again, be careful about SEs

- Again, if have multiple pre- and post-treatment periods, need to be careful with standard errors
 - Either cluster SEs at level of each unit
 - Or collapse data down to one pre- and one post-treatment observation for each cross-section
-

Using a First-Difference (FD) Approach

- Could also run regression using first-differences specification

$$y_{i,t} - y_{i,t-1} = \beta_1 (p_t - p_{t-1}) + (u_{i,t} - u_{i,t-1})$$

- If just one pre- and one post-treatment period (i.e., $t-1$ and t), then will get identical results
- But, if more than one pre- and post-treatment period, the results will differ...

FD *versus* Standard Approach [Part 1]

- Why might these two models give different estimates of β_1 when there are more than one pre- and post-treatment periods?

$$y_{i,t} = \beta_0 + \beta_1 p_t + u_{i,t}$$

versus

$$y_{i,t} - y_{i,t-1} = \beta_1 (p_t - p_{t-1}) + (u_{i,t} - u_{i,t-1})$$

FD *versus* Standard Approach [Part 2]

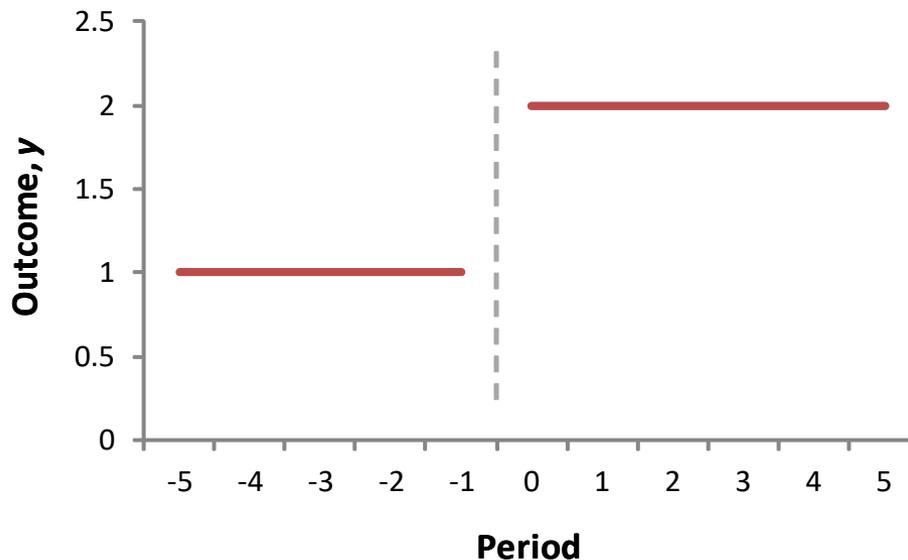
How might this
matter in practice?

■ **Answer:**

- In 1st regression, β_1 captures difference between avg. y pre-treatment *versus* avg. y post-treatment
 - In 2nd regression, β_1 captures difference in Δy immediately after treatment versus Δy in all other pre- and post-treatment periods
 - I.e., the Δp variable equals 1 only in immediate post-treatment period, and 0 for all other periods
-

FD *versus* Standard Approach [Part 3]

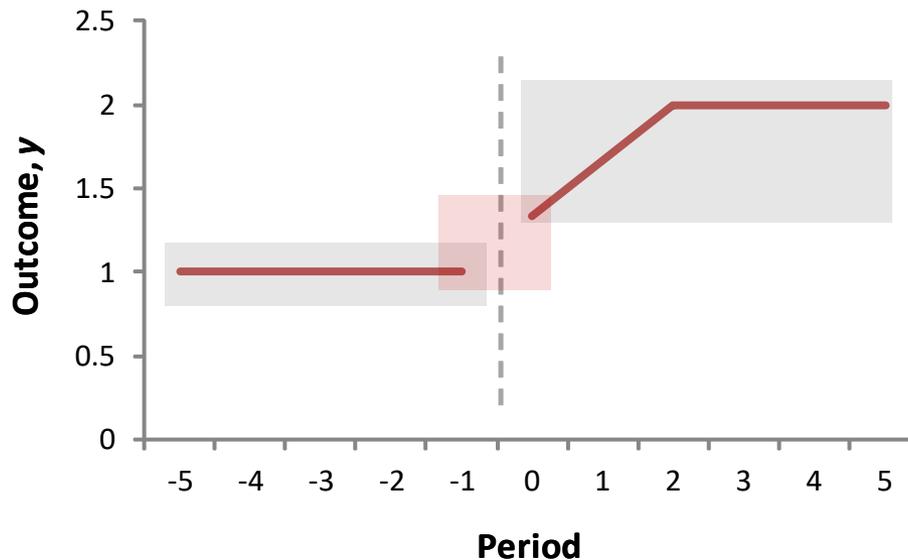
- Both approaches assume the effect of treatment is immediate and persistent, e.g.



**In this scenario,
both approaches
give same estimate**

FD *versus* Standard Approach [Part 4]

- But suppose the following is true...



In this scenario, FD approach gives much smaller estimate

1st approach compares avg. pre- versus post

FD compares Δy from $t=0$ to $t=-1$ against Δy elsewhere (which isn't always zero!)

Correct way to do difference

- Correct way to get a ‘differencing’ approach to match up with the more standard simple diff specification in multi-period setting is to instead use

$$\bar{y}_{i,post} - \bar{y}_{i,pre} = \beta_1 + (\bar{u}_{i,post} - \bar{u}_{i,pre})$$

- This is exactly the same as simple difference

Natural Experiments – *Outline*

- Motivation and definition
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 - Miscellaneous issues & advice
 - Difference-in-differences
-

Treatment effect isn't always immediate

- In prior example, the specification is wrong because the treatment effect only slowly shows up over time
 - Why might such a scenario be plausible?
 - **Answer** = Many reasons. E.g., firms might only slowly respond to change in regulation, or CEO might only slowly change policy in response to compensation shock

Accounting for a delay...

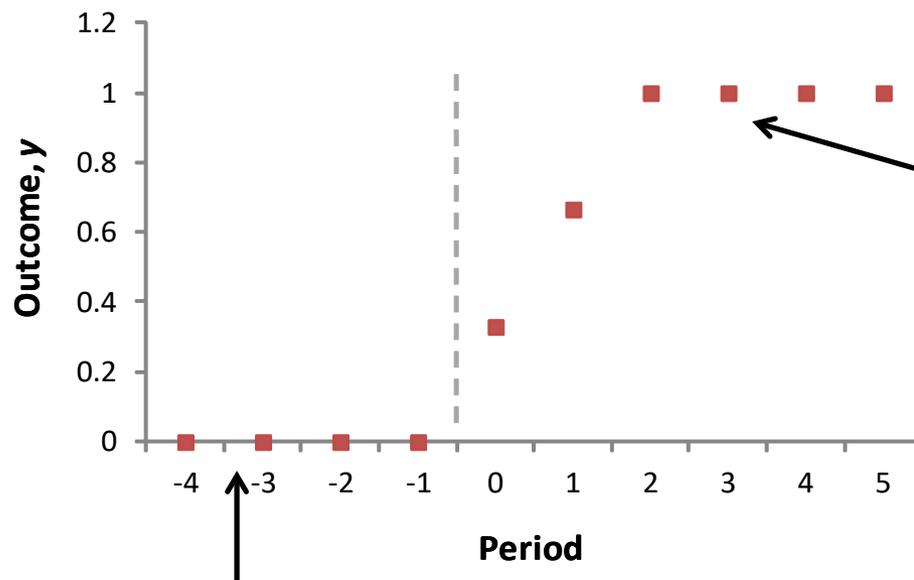
- Simple-difference misses this subtlety; it assumes effect was immediate
 - For this reason, it is always helpful to run regression that allows effect to vary by period
 - **How can you do this?**
 - **Answer** = Insert indicators for each year relative to the treatment year [*see next slide*]
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Non-parametric approach

- If have 5 pre- and 5 post-treatment obs.; could estimate :
$$y_{i,t} = \beta_0 + \sum_{t=-4}^5 \beta_t p_t + u_{i,t}$$
 - p_t is now an indicator that equals 1 if year = t and zero otherwise; e.g.
 - $t = 0$ is the period treatment occurs
 - $t = -1$ is period before treatment
 - β_t estimates change in y relative to excluded periods; you then plot these in graph
-

Non-parametric approach – *Graph*

- Plot estimates to trace out effect of treatment



**Approach allows
effect of treatment
to vary by year!**

Estimates capture
change relative to
excluded period ($t-5$)

Could easily plot
confidence intervals as well

These equal zero because y was
same as y in excluded period ($t-5$)

Simple Differences – *Advice*

- In general, simple differences are not that convincing in practice...
 - Cross-sectional difference requires us to assume the average y of treated and untreated would have been same absent treatment
 - Time-series difference requires us to assume the average y would have been same in post- and pre-treatment periods absent treatment
 - **Is there a better way?**
-

Natural Experiments – *Outline*

- Motivation and definition
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 - Two types of simple differences
 - Difference-in-differences
 - Intuition & implementation
 - “Parallel trends” assumption
-

Difference-in-differences

- Yes, we can do better!
 - We can do a difference-in-differences that combines the two simple differences
 - **Intuition** = compare change in y pre- versus post-treatment for treated group [*1st difference*] to change in y pre- versus post-treatment for untreated group [*2nd difference*]
-

Implementing diff-in-diff

- Difference-in-differences estimator

$$y_{i,t} = \beta_0 + \beta_1 p_t + \beta_2 d_i + \beta_3 (d_i \times p_t) + u_{i,t}$$

- $p_t = 1$ if period t occurs after treatment and equals zero otherwise
- $d_i = 1$ if unit is in treated group and equals zero otherwise

What do β_1 , β_2 , and β_3 capture?

Interpreting the estimates *[Part 1]*

- **Here is how to interpret everything...**
 - β_1 captures the average change in y from the pre- to post-treatment periods that is common to both treated and untreated groups
 - β_2 captures the average difference in level of y between treated and untreated groups that is common to both pre- and post-treatment periods
-

Interpreting the estimates [*Part 2*]

- β_3 captures the average differential change in y from the pre- to post-treatment period for the treatment group *relative* to the change in y for the untreated group
- β_3 is what we call the diff-in-diffs estimate

When does β_3 capture the causal effect of the treatment?

Natural Experiments – *Outline*

- Motivation and definition
 - Understanding treatment effects
 - Two types of simple differences
 - Difference-in-differences
 - Intuition & implementation
 - “Parallel trends” assumption
-

“Parallel trends” assumption

- Identification assumption is what we call the **parallel trends assumption**
 - Absent treatment, the change in y for treated would not have been different than the change in y for the untreated observations
 - To see why this is the underlying identification assumption, it is helpful to re-express the diff-in-diffs...
-

Differences estimation

- Equivalent way to do difference-in-differences is to instead estimate the following:

$$\bar{y}_{i,post} - \bar{y}_{i,pre} = \beta_0 + \beta_1 d_i + (\bar{u}_{i,post} - \bar{u}_{i,pre})$$

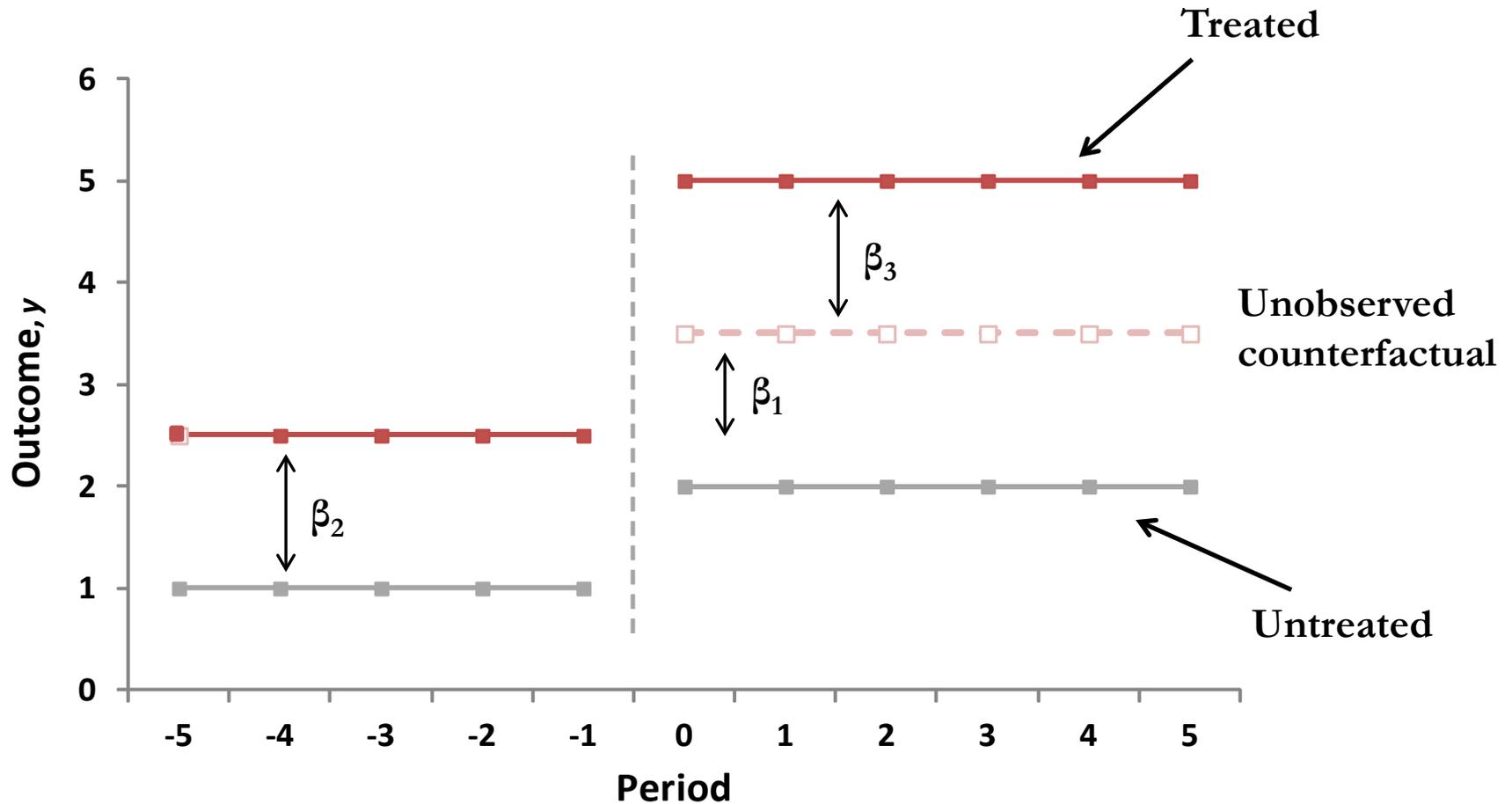
- β_1 gives the difference-in-differences estimate
 - In practice, don't do this because an adjustment to standard errors is necessary to get right t -stat
 - And remember! This is not the same as taking first-differences; FD can give misleading results

Difference-in-differences – *Visually*

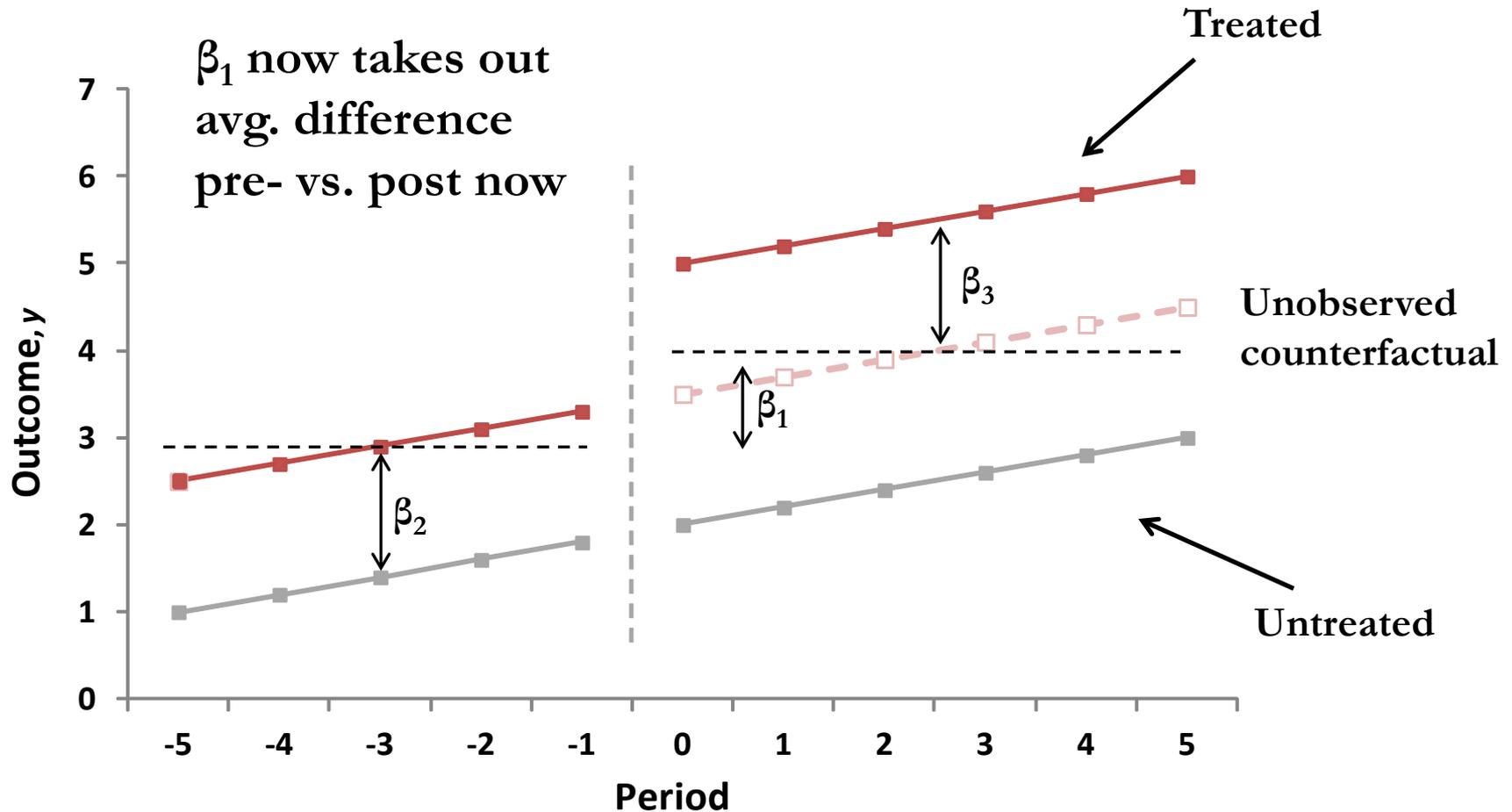
- Looking at what difference-in-differences estimate is doing in graphs will also help you see why the parallel trends assumption is key

$$y_{i,t} = \beta_0 + \beta_1 p_t + \beta_2 d_i + \beta_3 (d_i \times p_t) + u_{i,t}$$

Diff-in-diffs – *Visual Example #1*

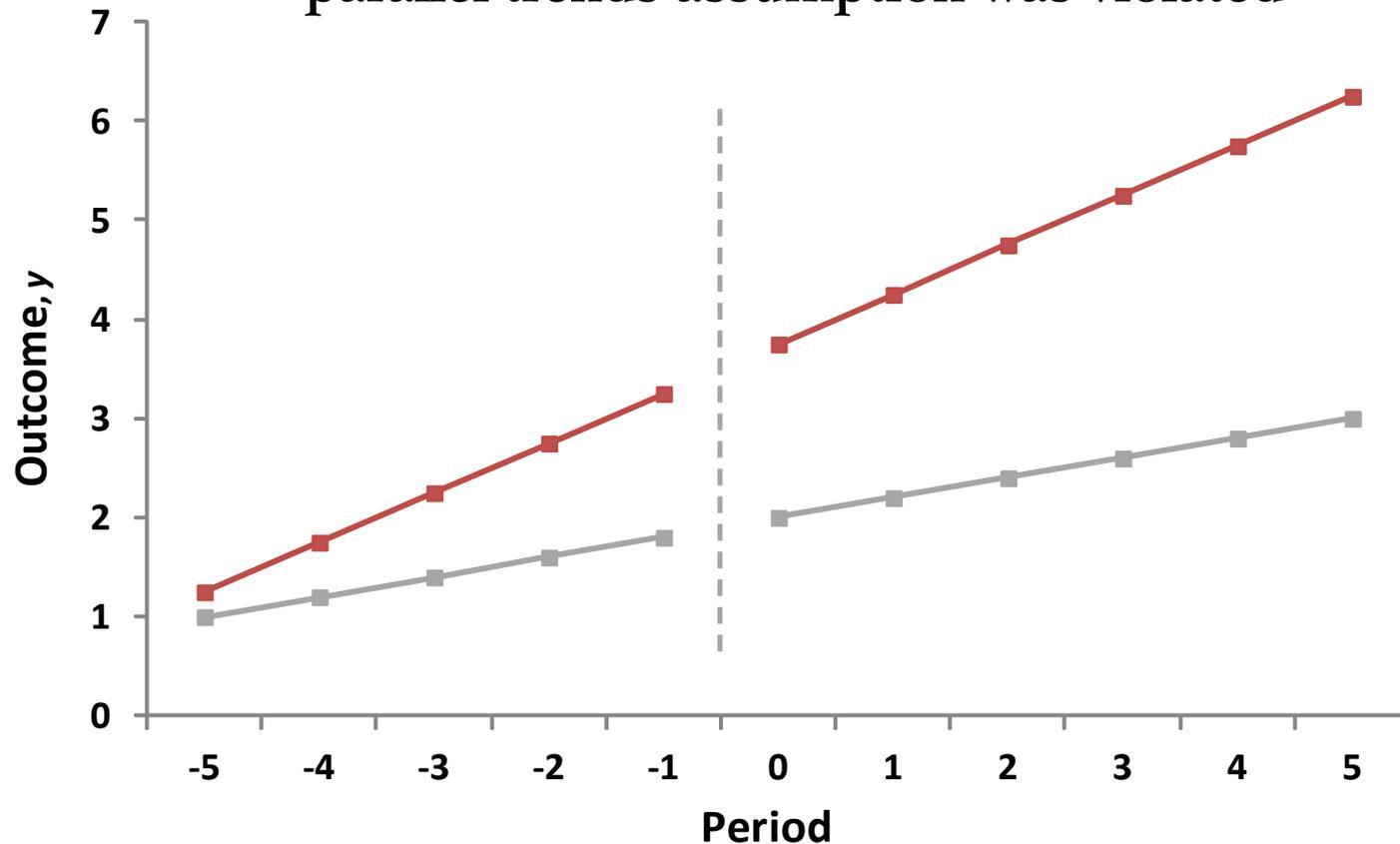


Diff-in-diff – *Visual Example #2*



Violation of parallel trends – *Visual*

There is no effect, but $\beta_3 > 0$ because parallel trends assumption was violated



Why we like diff-in-diff [Part 1]

- With simple difference, any of the below arguments would prevent causal inference
 - Cross-sectional diff – “Treatment and untreated avg. y could be different for reasons a, b, and c, that just happen to be correlated with whether you are treated or not”
 - Time-series diff – “Treatment group’s avg. y could change post- treatment for reasons a, b, and c, that just happen to be correlated with the timing of treatment”
-

Why we like diff-in-diff [Part 2]

- But now the required argument to suggest the estimate isn't causal is...
 - “The change in y for treated observations after treatment would have been different than change in y for untreated observations for reasons a, b, and c, that just happen to be correlated with **both** whether you are treated and when the treatment occurs”

← This is (usually) a much harder story to tell

Example...

- Bertrand & Mullainathan (JPE 2003) uses state-by-state changes in regulations that made it harder for firms to do M&A
 - They compare wages at firms pre- versus post-regulation in treated versus untreated states
 - Are the below valid concerns about their difference-in-differences...
-

Are these concerns for internal validity?

- The regulations were passed during a time period of rapid growth of wages nationally...
 - **Answer = No.** Indicator for post-treatment accounts for common growth in wages
 - States that implement regulation are more likely have unions, and hence, higher wages...
 - **Answer = No.** Indicator for treatment accounts for this average difference in wages
-

Example continued...

- However, ex-ante average differences is troublesome in some regard...
 - Suggests treatment wasn't random
 - And ex-ante differences can be problematic if we think they their effect may vary with time...
 - Time-varying omitted variables **are** problematic because they can cause violation of “parallel trends”
 - E.g., states with more unions were trending differently at that time because of changes in union power
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Summary of Today *[Part 1]*

- Natural experiment provides random variation in x that allows causal inference
 - Can be used in IV, regression discontinuity, but most often associated with “treatment” effects
 - Two types of simple differences
 - Post-treatment comparison of treated & untreated
 - Pre- and post-treatment comparison of treated
-

Summary of Today *[Part 2]*

- Simple differences require strong assumptions; typically, not plausible
 - Difference-in-differences helps with this
 - Compares change in y pre- versus post-treatment for treated to change in y for untreated
 - Requires “parallel trends” assumption
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In First Half of Next Class

- Natural experiments [*Part 2*]
 - How to handle multiple events
 - Triple differences
 - Common robustness tests that can be used to test whether internal validity is likely to hold
 - Related readings... see syllabus
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Assign papers for next week...

- Jayaratne and Strahan (QJE 1996)
 - Bank deregulation and economic growth
 - Bertrand and Mullainathan (JPE 2003)
 - Governance and managerial preferences
 - Hayes, Lemmon, and Qiu (JFE 2012)
 - Stock options and managerial incentives
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Break Time

- Let's take our 10-minute break
- We'll do presentations when we get back

