4. Difference-in-Differences

LPO 8852: Regression II

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Difference-in-differences

Difference-in-differences is a design that—in its most common (but not only) application—contrasts *changes over time* for treated and untreated groups. DD is often used with **natural experiments**, settings in which an external force "naturally" assigns units into treatment and control groups.

Figure: Scott Cunningham's (of *Mixtape* fame) bumper sticker

DD models are typically estimated with *panel* or *repeated cross-section* data. But they can also work with other data structures.

Natural experiments

Examples of natural experiments:

- John Snow's cholera study (1855)
- Natural and other disasters (hurricanes, earthquakes, COVID, 9/11)
- Policy implementation (e.g., Medicaid expansion, EZ Pass)
- Targeted investments (e.g., school construction, ed finance reform)
- Idiosyncratic policy rules (e.g., class size maximum)
- Idiosyncratic differences in location (opposite sides of boundaries)
- Date of birth and eligibility rules

Many natural experiments are analyzed using DD, others are better suited to tools we'll see later.

High-stakes testing in Chicago

Do test-based "high-stakes" accountability policies improve student academic performance?

- A potential "natural experiment": in Chicago, the Iowa Test of Basic Skills (ITBS) became "high stakes" for students and schools in 1997. The test was administered—but was "low stakes"—prior to that year. The test is given in grades 3, 6, and 8.
- Many other districts in Illinois also regularly administered the ITBS to these grades, but the test was low stakes.

Note: this is a simplified example inspired by Jacob (2005).

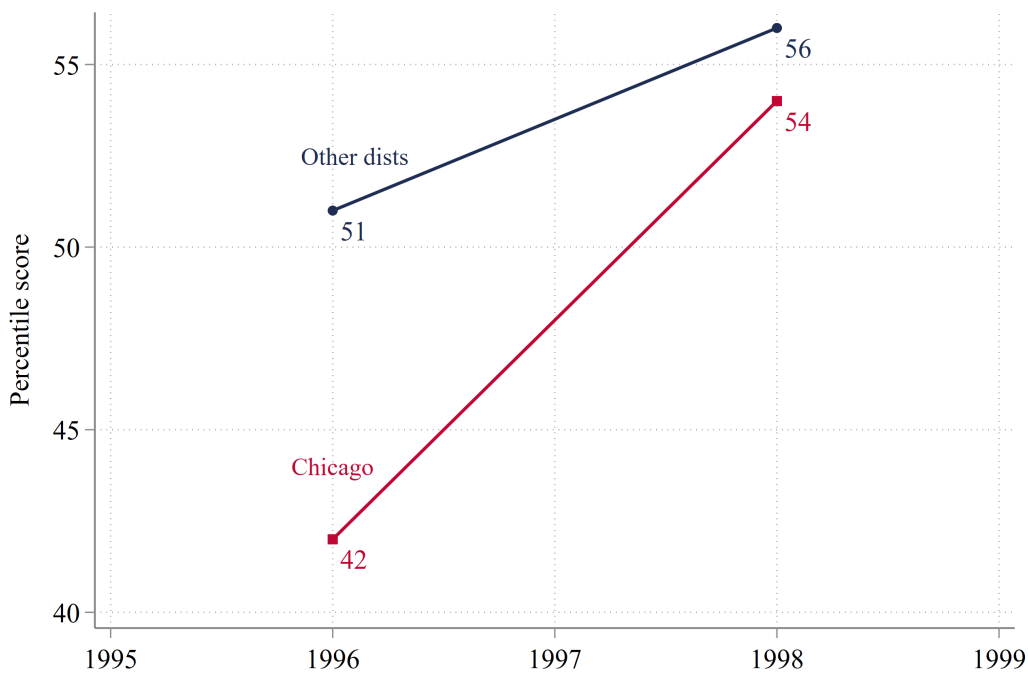
High-stakes testing in Chicago

Consider two types of comparisons:

- "Cross-sectional": the mean scores of Chicago 6th graders in 1998 (treated) vs. other Illinois 6th graders in 1998 (untreated).
- First difference or "interrupted time series (ITS)": the pre-to-post change in mean scores of Chicago 6th graders between 1996 and 1998.

An ITS design would be greatly improved by more time periods—to better establish a trend—but this is just for illustration!

High-stakes testing in Chicago



High-stakes testing in Chicago

The cross sectional comparison suggests *worse* outcomes for Chicago:

$$Y_{Chicago,1998} - Y_{Other,1998} = 54 - 56 = -2$$

The first difference or ITS for Chicago suggests a large *improvement*:

$$Y_{Chicago,1998} - Y_{Chicago,1996} = 54 - 42 = +12$$

Conflicting conclusions!

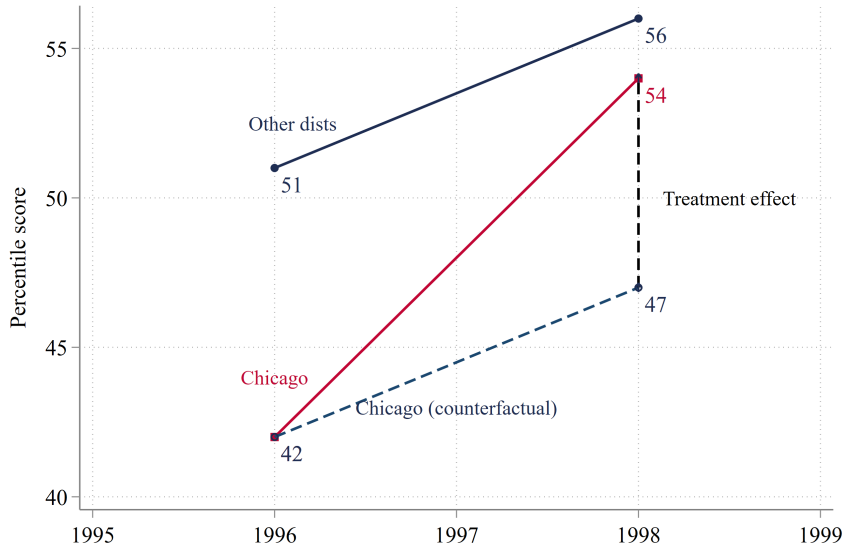
High-stakes testing in Chicago

Problems:

- The cross sectional comparison fails to recognize that Chicago 6th graders performed worse in 1996 than 6th graders in other districts did (i.e., baseline differences between treated and untreated).
- The first difference is unable to differentiate between a treatment effect for Chicago (if any) and gains between 1996 and 1998 that were common to all districts.

High-stakes testing in Chicago

Under the assumption that the change over time in other (untreated) districts represents what *would have happened* in Chicago (treated) in the absence of treatment, we can contrast *changes* in the two, or the **difference-in-differences**:



High-stakes testing in Chicago

The difference-in-differences:

$$\delta_{DD} = \underbrace{(Y_{Chicago,1998} - Y_{Chicago,1996})}_{\text{Change in Chicago}} - \underbrace{(Y_{Other,1998} - Y_{Other,1996})}_{\text{Change in other districts}}$$

$$\delta_{DD} = (54 - 42) - (56 - 51) = +7$$

The differencing of the two "first differences" represents the **second difference**. There was a "counterfactual" gain of 5 implied by the other districts.

High-stakes testing in Chicago

An equivalent way to write δ_{DD}:

$$\delta_{DD} = \underbrace{(Y_{Chicago,1998} - Y_{Other,1998})}_{\text{Difference "post"}} - \underbrace{(Y_{Chicago,1996} - Y_{Other,1996})}_{\text{Difference "pre"}}$$

Writing δ_{DD} this way makes it clear we are "netting out" pre-existing differences between the two groups.

Note in this example δ_{DD} was calculated using only four numbers (mean scores in Chicago and other districts for 1996 and 1998).

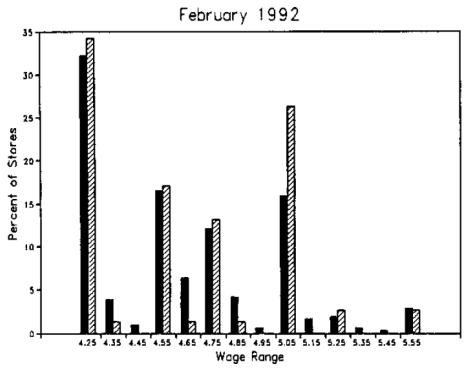
Card & Krueger (1994)

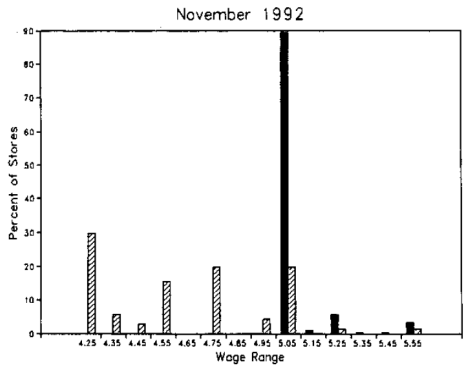
A classic DD study of the impact of the minimum wage on fast food employment (an industry likely to be affected by the minimum wage).

- NJ increased its minimum wage in April 1992, PA did not.
- Card & Krueger collected employment data at fast food restaurants in NJ and Eastern PA before and after the minimum wage increase.

Next figure: the minimum wage increase had a "first stage." That is, it led to higher starting wages in NJ. (This is important—if the minimum wage were not binding, it wouldn't make for a very interesting study. These kinds of checks are often important in DD studies).

Card & Krueger (1994)





New Jersey Pennsylvania

FIGURE 1. DISTRIBUTION OF STARTING WAGE RATES

Card & Krueger (1994)

Main result (portion of Table 3 in C&K):

	Stores by State		
	PA	*NJ*	*NJ* − *PA*
FTE before	23.3	20.44	-2.89
	(1.35)	(-0.51)	(1.44)
FTE after	21.15	21.03	-0.14
	(0.94)	(0.52)	(1.07)
Change in mean FTE	-2.16	+0.59	**2.76**
	(1.25)	(0.54)	**(1.36)**

Standard errors in parentheses. FTE=full time equivalent employees.

Mean employment fell in PA and *rose* in NJ, for $\delta_{DD} = 2.76$. A surprising result to many economists who expected to see a reduction in employment following an increase in the minimum wage.

2x2 difference-in-differences

The two examples thus far are the simplest form of difference-in-differences (2x2):

- Two groups: a treated and an untreated comparison group
- Two time periods: pre and post, before and after treatment occurs
- Treated units are all treated at the same time

The DD design can accommodate much more complicated setups, as we will see later.

Causal interpretation of difference-in-differences

Causal interpretation of difference-in-differences

Under what conditions might the difference-in-differences design estimate a *causal parameter*? And what causal parameter is it estimating?

Let's apply the potential outcomes framework to a 2x2 setup:

n individuals	indexed i
2 groups	$G_i = 0$ never treated $G_i = 1$ eventually treated
2 time periods	$T_t = 0$ "pre" $T_t = 1$ "post"

Note that G is not subscripted with an t. It is a time-invariant group indicator. In the simple 2x2, the treatment indicator is $D_{it} = G_i \times T_t$. Groups could be states, counties, schools, etc.

Causal interpretation of difference-in-differences

Suppose potential outcomes for individual i at time t are given by:

$$Y_{it}(0) = \beta_0 + \beta_1 G_i + \beta_2 T_t + u_{it}$$
$$Y_{it}(1) = \beta_0 + \beta_1 G_i + \beta_2 T_t + \delta + u_{it}$$

A few things to notice here:

- There are **time-invariant** group differences in mean potential outcomes represented by β_1
- There is a **group-invariant**, common time trend represented by β_2
- The impact of the treatment δ is assumed to be the same for all i, and does not vary over time (constant treatment effect)

$$Y_{it}(1) - Y_{it}(0) = \delta \quad \forall i, t$$

Causal interpretation of difference-in-differences

If it helps, think of β_1 as standing in for the combined effects of *all unmeasured covariates* that differ systematically between groups and do not vary over the study period.

Likewise, think of β_2 as standing in for the combined effects of *all unmeasured covariates* that change between periods but affect outcomes in the same way in both groups.

In the DD framework, treatment status D_{it} can be related to G_i (i.e., "self-selection").

Causal interpretation of difference-in-differences

A natural causal parameter of interest is the ATT:

$$ATT = \underbrace{E[Y(1)|G=1,T=1]}_{\text{observed}} - \underbrace{E[Y(0)|G=1,T=1]}_{\textit{unobserved}}$$

$$= (\beta_0 + \beta_1 + \beta_2 + \delta) - (\beta_0 + \beta_1 + \beta_2)$$

$$= \delta$$

The ATT of interest is the difference in mean potential outcomes $Y(1)$ and $Y(0)$ in time period 1 ("post") *among those who are actually treated* (the $G = 1$ group).

Causal interpretation of difference-in-differences

Of course, we can't observe the same i in two different states (0 and 1) in the same period t. The outcome we do observe is:

$$Y_{it} = Y_{it}(0) + D_{it}\left[Y_{it}(1) - Y_{it}(0)\right]$$

$$= \beta_0 + \beta_1 G_i + \beta_2 T_t + D_{it}\delta + u_{it}$$

where $D_{it} = G_i \times T_t$ as defined above.

Causal interpretation of difference-in-differences

Suppose we compare the mean observed outcomes Y_{it} of the $G = 1$ and $G = 0$ groups in time period 1 (post):

$$\underbrace{E[Y|G=1,T=1]}_{\beta_0+\beta_1+\beta_2+\delta} - \underbrace{E[Y|G=0,T=1]}_{\beta_0+\beta_2}$$

$$= \delta + \underbrace{\beta_1}_{\text{selection bias}}$$

This difference does not identify δ since there are baseline differences between the $G = 1$ and $G = 0$ groups..

Causal interpretation of difference-in-differences

Alternatively, we might restrict our attention to the $G = 1$ group and do a pre-post comparison of mean observed outcomes Y_{it}:

$$\underbrace{E[Y|G=1,T=1]}_{\beta_0+\beta_1+\beta_2+\delta} - \underbrace{E[Y|G=1,T=0]}_{\beta_0+\beta_1}$$

$$= \delta + \underbrace{\beta_2}_{\text{common time trend}}$$

This is the first difference or interrupted time series (ITS). Unfortunately, this difference does not identify δ since there is an unaccounted-for time trend.

Causal interpretation of difference-in-differences

Now consider the pre-post comparison for the $G = 0$ group:

$$\underbrace{E[Y|G=0,T=1]}_{\beta_0+\beta_2} - \underbrace{E[Y|G=0,T=0]}_{\beta_0}$$

$$= \beta_2$$

The comparison group allows us to estimate the time trend!

Causal interpretation of difference-in-differences

Subtract the pre-post comparison for the *untreated* group from the pre-post comparison for the *treated* group:

$$\underbrace{E[Y|G=1,T=1]}_{\beta_0+\beta_1+\beta_2+\delta} - \underbrace{E[Y|G=1,T=0]}_{\beta_0+\beta_1} -$$

$$(\underbrace{E[Y|G=0,T=1]}_{\beta_0+\beta_2} - \underbrace{E[Y|G=0,T=0]}_{\beta_0})$$

$$= (\beta_2 + \delta) - \beta_2$$

$$= \delta$$

The difference-in-differences recovers the ATT. The **parallel trends** or **common trends** assumption is critical here.

Causal interpretation of difference-in-differences

The above example in table form:

	Pre ($T=0$)	Post ($T=1$)	Diff
Never treated ($G=0$)	β_0	$\beta_0+\beta_2$	β_2
Eventually treated ($G=1$)	$\beta_0+\beta_1$	$\beta_0+\beta_1+\beta_2+\delta$	$\beta_2+\delta$
Diff	β_1	$\beta_1+\delta$	δ

2x2 DD is effectively a comparison of four cell-level means.

Parallel trends assumption

The ATT is again:

$$ATT = \underbrace{E[Y(1)|G=1,T=1]}_{\text{observed}} - \underbrace{E[Y(0)|G=1,T=1]}_{\textit{unobserved}}$$

The DD estimates:

$$\underbrace{E[Y(1)|G=1,T=1] - E[Y(0)|G=1,T=0]}_{\text{change over time for treated group}}$$

$$-\underbrace{(E[Y(0)|G=0,T=1] - E[Y(0)|G=0,T=0])}_{\text{change over time for untreated group}}$$

From this, subtract and add the *unobserved* term from above right:

Parallel trends assumption

$$E[Y(1)|G=1,T=1] - E[Y(0)|G=1,T=0] - \underbrace{E[Y(0)|G=1,T=1]}_{\text{unobserved}}$$

$$-(E[Y(0)|G=0,T=1] - E[Y(0)|G=0,T=0]) + \underbrace{E[Y(0)|G=1,T=1]}_{\text{unobserved}}$$

Gathering terms, this equals:

$$ATT + \underbrace{(E[Y(0)|G=1,T=1] - E[Y(0)|G=1,T=0])}_{\text{pre to post change in Y(0) for G=1 group}}$$

$$-\underbrace{(E[Y(0)|G=0,T=1] - E[Y(0)|G=0,T=0])}_{\text{pre to post change in Y(0) for G=0 group}}$$

The second term is counterfactual (unobserved). However if **parallel trends** holds, the second and third terms cancel each other out.

Parallel trends assumption

The parallel trends assumption means that the pre-to-post change in $Y(0)$ for the $G = 0$ group represents what *would have happened* to the $G = 1$ group had they not been treated.

$$\underbrace{(E[Y(0)|G = 1, T = 1] - E[Y(0)|G = 1, T = 0])}_{\text{pre to post change in Y(0) for G=1 group}}$$
$$- \underbrace{(E[Y(0)|G = 0, T = 1] - E[Y(0)|G = 0, T = 0])}_{\text{pre to post change in Y(0) for G=0 group}} = 0$$

These canceled out in our case because of how we specified potential outcomes. Keep in mind this is a model we posited for potential outcomes! It may not correspond to reality in a particular case. More on this later.

Difference-in-differences: summary thus far

To summarize:

- Changes over time in the $G = 0$ group provide the counterfactual.
- Selection into treatment related to fixed (time invariant) unobserved differences is OK.
- The outcome *levels* themselves are not important and can vary systematically by group. Only within-group *differences* are used in estimation.
- DD can provide a consistent estimate of the ATT if the parallel trends assumption holds.

Difference-in-differences: summary thus far

DD is probably the most commonly used quasi-experimental design in the social sciences and in education policy research.

- Its use precedes the RCT (see Snow cholera example, 1855)
- DD is sometimes called a "comparative interrupted time series" (CITS) or nonequivalent control group pretest design. However, the CITS is usually thought of as a more general model than DD. See Section 3 of the MDRC paper by Somers et al. (2013) for a distinction between the two in the context of an educational intervention.

Regression difference-in-differences

Estimation using regression

With many units (i) in two groups observed in "pre" and "post" periods, we can use regression to estimate δ:

$$Y_{it} = \beta_0 + \beta_1 G_i + \beta_2 T_t + \delta(G_i \times T_t) + u_{it}$$

where $G_i = 1$ for units i who are ultimately treated (and 0 otherwise), and $T_t = 1$ for observations in the "post" period. Note the "post" period is the same for all units.

Very easy to implement in Stata, especially with factor variable notation: `reg y i.evertreated##i.post`

Estimation using regression

You will recognize this as a **two-way fixed effects regression** where group (G_i) is the fixed effect and T_t is the time trend. It could be estimated using the fixed effects "within" estimator: `xtreg, fe` or `areg`.

Example (2x2)

Some NYC schools adopted a breakfast in the classroom program in 2010. What was the impact of this program on average daily participation in breakfast?

```
. reg bkfast_part i.everbic##i.post
```

Source	SS	df	MS		
Model	6.66627777	3	2.22209259	Number of obs	= 6,160
Residual	111.439598	6,156	.018102599	F(3, 6156)	= 122.75
				Prob > F	= 0.0000
Total	118.105875	6,159	.019176145	R-squared	= 0.0564
				Adj R-squared	= 0.0560
				Root MSE	= .13455

bkfast_part	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
1.everbic	.0364431	.011215	3.25	0.001	.0144578	.0584285
1.post	.0004512	.0035743	0.13	0.900	-.0065557	.0074581
everbic#post						
1 1	.2219777	.0177852	12.48	0.000	.1871125	.256843
_cons	.2494476	.0022566	110.54	0.000	.2450239	.2538713

Estimation using regression

Equivalently, with two time periods we could estimate a regression using first differences for each observation i, subtracting Y_{i0} from Y_{i1}:

$$Y_{i1} = \beta_0 + \beta_1 G_i + \beta_2 + \delta G_i + u_{i1}$$
$$Y_{i0} = \beta_0 + \beta_1 G_i + u_{i0}$$

$$Y_{i1} - Y_{i0} = \beta_2 + \delta G_i + \epsilon_{it}$$

$$\Delta Y_i = \beta_2 + \delta G_i + \epsilon_{it}$$

The intercept here represents the common time trend β_2, and δ is the DD. The baseline differences wash out.

Estimation using regression

The above model can easily accommodate more than two time periods. Continue to assume two groups and a common "post" period ($POST_t = 1$):

$$Y_{it} = \beta_0 + \beta_1 G_i + \gamma_t + \delta(G_i \times POST_t) + u_{it}$$

The γ_t are time effects—a common intercept shift in each period. The variable $POST_t$ indicates post-treatment time periods. One could put more structure on the time trend and assume linearity:

$$Y_{it} = \beta_0 + \beta_1 G_i + \beta_2 t + \delta(G_i \times POST_t) + u_{it}$$

where t is a linear time trend. This only makes sense, though, if the time trend is really (or approximately) linear.

Estimation using regression

The above regression models can also be extended to include time-varying covariates $\mathbf{X_{it}}$:

$$Y_{it} = \beta_0 + \beta_1 G_i + \beta_2 T_t + \delta(G_i \times T_t) + \mathbf{X_{it}}\eta + u_{it}$$

Careful thought should be put into the implications of including covariates in the model (more on this later). For example, does the parallel trends assumption hold conditional on covariates? Or unconditionally?

In-class exercise 1

What is the effect of financial assistance on college enrollment? Replicating a simple difference-in-differences result from Murnane & Willett chapter 8 based on Dynarski (2003).

- Data: high school seniors in the National Longitudinal Surveys of Youth (NLSY) 1979-1983 (five cohorts). Includes information on college enrollment and total years of schooling.
- The data include seniors with a deceased parent. Before 1982, college students with a deceased parent were eligible for the Social Security Student Benefit Program. The program was discontinued in 1982.

How would you estimate the effect of the SSBP using a 2x2 DD design?

Generalized difference-in-differences

The examples thus far assumed a common "post" period. In practice, "treatment" often occurs for different groups at different times (e.g., policy adoption). The DD framework easily adapts: this is sometimes referred to as the "generalized difference-in-differences" model.

Suppose potential outcomes for individual i at time t are given by:

$$Y_{it}(0) = \alpha_g + \gamma_t + u_{it}$$
$$Y_{it}(1) = \alpha_g + \gamma_t + \delta + u_{it}$$

The α_g are group effects (e.g., states), γ_t captures the time trend, and δ is a constant treatment effect. Think of there being a unique intercept for every group α_g and a yearly deviation from that intercept common to every group γ_t.

Generalized difference-in-differences

δ can again be estimated using a two-way fixed effects regression with group and time effects:

$$Y_{it} = \alpha_g + \gamma_t + \delta D_{it} + u_{it}$$

where $D_{it} = 1$ in time periods in which i's group g is treated. There is no longer a common "post" period, so the timing may vary for different groups.

Intuitively, under the parallel trends assumption that changes within states over time would be the same in the absence of treatment, we can interpret δ as the *differential* change over time associated with treatment.

Generalized difference-in-differences

Implementing in Stata: can be done in multiple ways, including `xtreg`:

```
xtreg y x i.year i.evertreated#i.post, i(group) fe
```

Alternatively, user-written `reghdfe` which accommodates multiple fixed effects:

```
reghdfe y i.evertreated#i.post, absorb(group year)
```

In Stata 17+ can use `did` commands. For example, with panel data for states, where *treated* is a time-varying treatment variable (like the interaction of *evertreated* and *post*):

```
xtdidregress (y x) (treated), group(group) time(year)
```

Continuous treatments

All examples thus far have used a binary treatment (e.g., $D_{it} = G_i \times T_t$). It is common in DD studies to operationalize treatment as a continuous "intensity," "dosage," or "coverage" measure. For example:

- $D_{it} = 0$ if untreated and $D_{it} > 0$ if treated
- D_{it} could be an index of treatment *intensity* (e.g., law strength, new school construction per capita)
- Or, a measure of *coverage* like the proportion of the population affected by a policy change

Swap in this continuous measure for the D_{it} above and the interpretation of δ becomes the effect of a 1-unit change in this treatment intensity measure.

In-class exercise 2

What is the effect of a lower Minimum Legal Drinking Age (MLDA) on traffic fatalities among young adults? Example from *Mastering 'Metrics* chapter 5 based on Carpenter & Dobkin (2011).

- Following the 26th Amendment (1971) some states lowered the MLDA to 18.
- In 1984, federal legislation pressured states to increase MLDA to 21. Between 1971-1984, there was a lot of variation across states and years in the MLDA.
- Was a lower MLDA associated with more traffic fatalities among 18-20 year olds?

We will use panel (*state* $\times$ *year*) data and generalized DD to address this question.

In-class exercise 2

One could use a binary "treatment" variable year (e.g., $D_{st} = 1$ if the MLDA is below 21 in state s and year t and zero otherwise). However, this neglects some potentially interesting variation in MLDA.

They use a continuous "dosage" measure of treatment, $LEGAL_{st}$, defined as the proportion of adults aged 18-20 who could legally drink in state s in year t. This measure also takes into account within-year changes in the MLDA (e.g., if a state raises its MLDA from 18 to 21 mid-year, $LEGAL_{st} = 0.5$).

In-class exercise 2

The TWFE (generalized DD) model for mortality rates by motor vehicle accidents (Y_{st}) in state s and year t:

$$Y_{st} = \alpha_g + \gamma_t + \delta LEGAL_{st} + u_{st}$$

The coefficient δ represents how much, on average, mortality rates differ when the MLDA is 18 (relative to 21), conditional on state and year. (In other words, beyond that predicted by the state and year effects).

Defending the parallel trends assumption

Parallel trends assumption

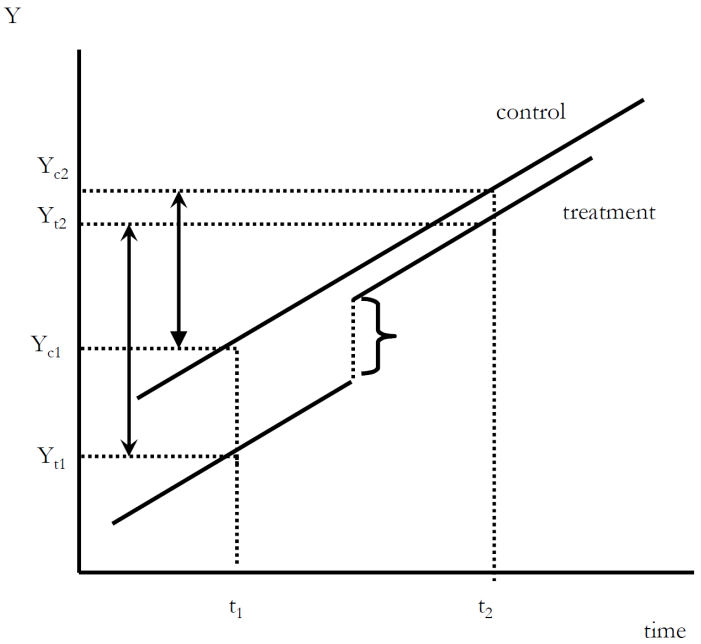
The DD design leans heavily on the parallel trends assumption:

- Confounding factors varying across groups are time-invariant
- Time-varying confounding factors are group-invariant

i.e., there are no group-specific, unobserved, time varying factors that would lead to groups to follow different time paths.

Parallel trends assumption

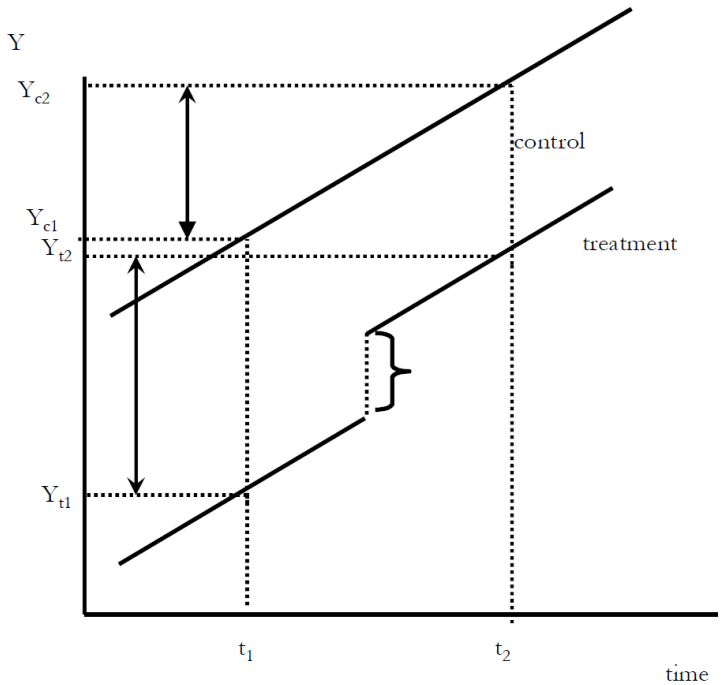
Parallel trends implies the time trend in the absence of treatment would be the same in both groups:



Estimated Treatment effect= $(Y_{t2}-Y_{t1}) - (Y_{c2}-Y_{c1})$

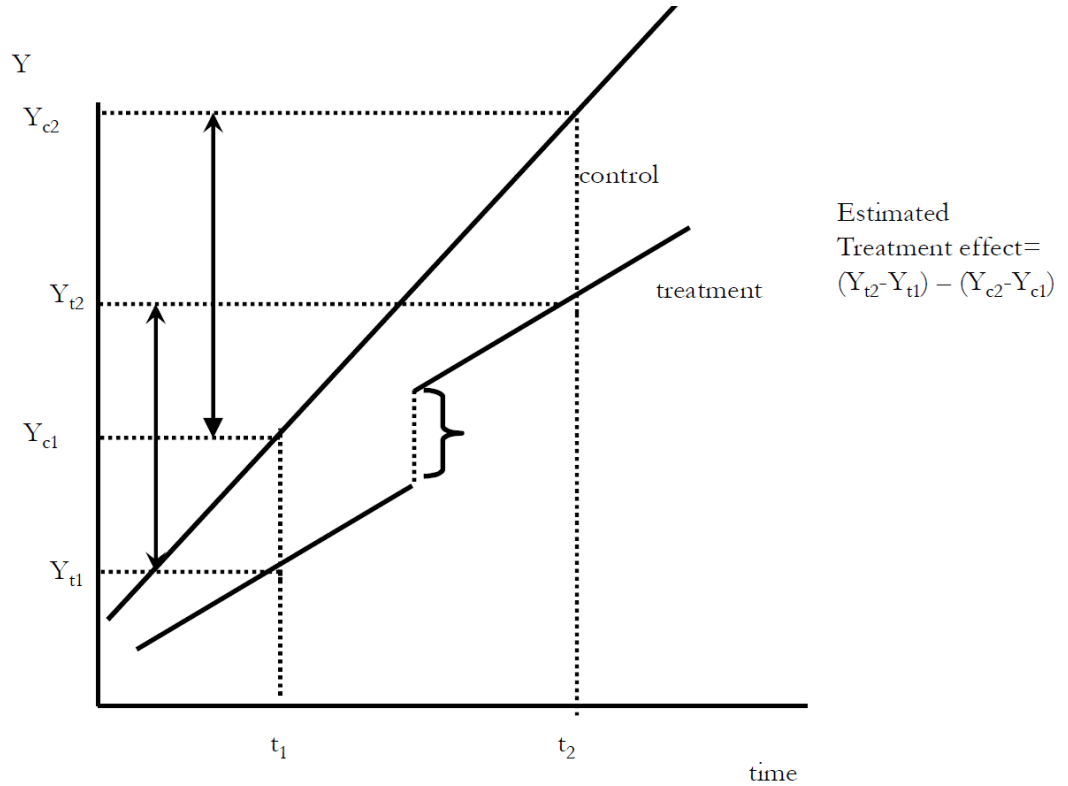
Parallel trends assumption

Size of baseline differences in treated and untreated groups doesn't matter.



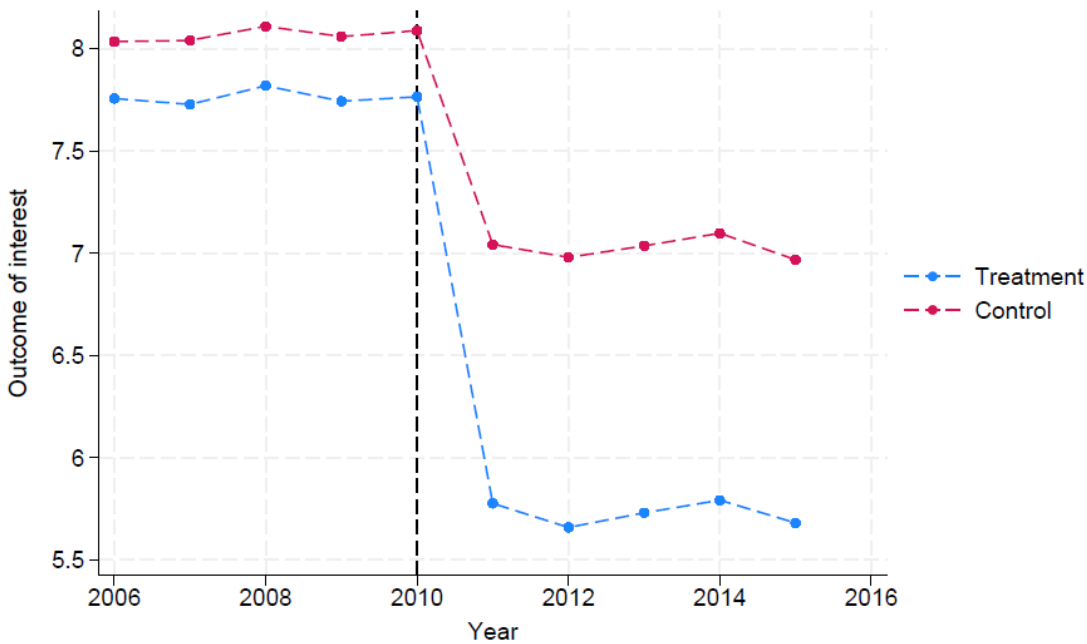
Estimated Treatment effect= $(Y_{t2}-Y_{t1}) - (Y_{c2}-Y_{c1})$

Violation of parallel trends assumption



Parallel trends assumption

Time trends need not be linear—here, individual year effects:



Common violations of parallel trends assumption

Common scenarios that would violate the parallel trends assumption:

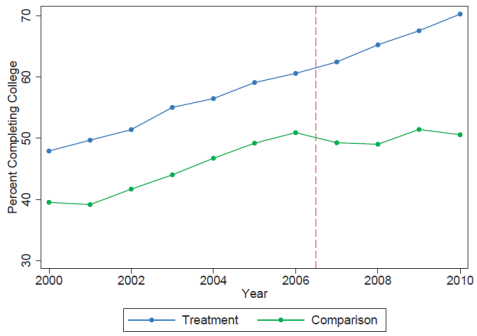
- **Targeted treatments**: often programs are targeted at subjects who are most likely to benefit from it. In many cases, the fact that a subject was on a different trajectory is what made them a good candidate for the program (e.g., a struggling student).
- **Ashenfelter's dip**: treated cases may experience a "dip" just prior to treatment that results in a reversion to the mean after treatment (e.g, job training).
- **Anticipation**: behavior (and outcomes) change prior to treatment due to anticipation effects.

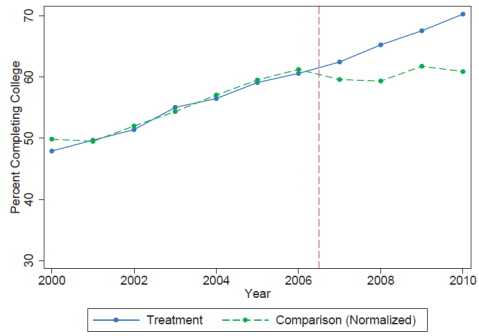
Parallel trends assumption

We can't verify the parallel trends assumption directly, but researchers typically defend it in a variety of ways:

- A compelling graph: point to similar trends *prior to* treatment. Note: parallel trends *prior* to treatment are neither necessary nor sufficient for the parallel trends assumption, which is about the *post* period! However, they help the case.
- Event study regression and graph (Lecture 5)
- Statistical tests for differences in pre-treatment trends
- A placebo / falsification test
- Controlling for time trends directly (leans heavily on functional form)
- Triple-differences model
- Probably most important: understanding the context of your study! Ruling out reasons for non parallel-trends.

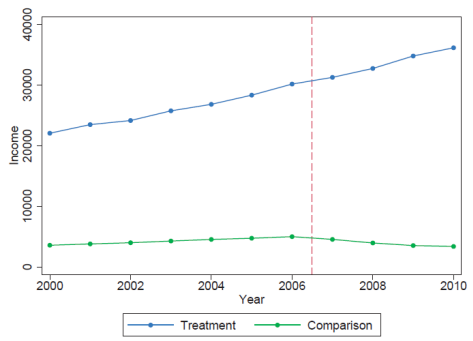
Graphical assessment of parallel trends assumption

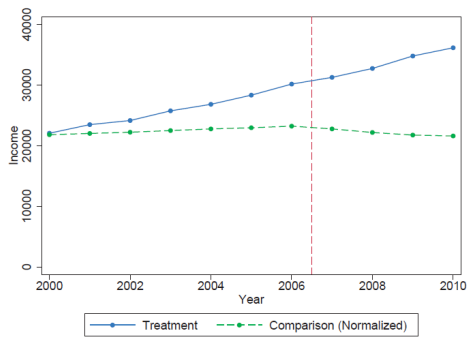




The graph on the right ("normalized") subtracts baseline difference between a treated and untreated comparison group to better visualize the trend differences. In Stata 17+ see `didregress` post-estimation command `estat trendplots`. Also easy to do yourself.

Graphical assessment of parallel trends assumption

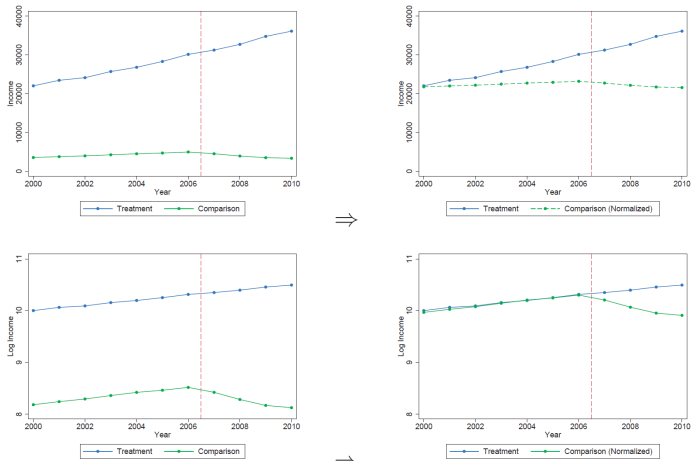




The graph on the right makes the lack of a parallel trend more visually apparent than the graph on the left.

Graphical assessment of parallel trends assumption

Note: if trends are parallel in levels they will *not* be parallel in logs, and vice versa!



If your outcome variable is in levels and does not satisfy parallel trends, a log transformation may help (if appropriate for your outcome). Above, the top panels are in levels; the bottom panels are in logs.

Covariates and the parallel trends assumption

When covariates are included in the model, the parallel trends assumption is *conditional* on the covariates. It is possible that the unconditional outcomes do not follow a parallel trend, but the conditional outcomes do.

Put another way, controlling for covariates allows you to account for factors that might produce different time trends.

Statistical tests for differences in pre-treatment trends

Two tests (easily implemented in Stata 17+ `did` commands, though not hard to code):

1. **Differential linear trend for the treated**: add to the DD model separate linear time trends for the ever-treated group, pre- and post-treatment. Conduct an F-test for significance of the pre-treatment linear trend. This assesses whether the treated group was on a differential trend prior to treatment. See `estat ptrends`.

2. **Granger-type test**: add to the DD model a full set of interactions between pre-treatment years and ever-treated. Conduct an F-test for the joint significance of these interactions. This assesses "anticipatory" effects. See `estat granger`.

In practice, event studies are more common than these tests.

Placebo/falsification tests

The DD design assumes that any change over time beyond that predicted by the untreated group is the ATT, and not some other time-varying factor specific to the treated group.

If there is an unobserved time-varying factor specific to the treated group, one might see its effects show up on *other* outcomes that shouldn't have been affected by the treatment.

- Card & Krueger: employment in higher-wage firms
- Miller et al.: mortality of populations not eligible for Medicaid
- Cheng & Hoekstra (2013): effects of Stand Your Ground laws on other non-homicide crimes (see *Mixtape*)

Estimate the same DD model for these outcomes. If there is an "effect", this may indicate an unobserved, time-varying confounder specific to the treated group.

Placebo/falsification tests

Another approach is to apply the same treatment assignment to an earlier period, before the treatment actually occurred, and re-estimate the DD model on this earlier data. If there is an apparent treatment "effect" in these untreated years, there may well be unobserved, group-specific trends driving the result.

There are lots of ways to do this, including picking your own period for the "fake" treatment, or trying lots of alternatives.