Chapter 6: Panel Data and Difference-in-Differences

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Motivation

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- But often we're worried that there are unobservable characteristics we haven't properly accounted for (i.e. confounding variables)
- Next we'll think about how we can deal with certain types of unobserved confounding variables when we have **panel data**

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- Let's see how this works in an example of **difference-in-differences**, which is the most common panel data method used in applied microeconomic research

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- Hastings attempted to answer this question empirically using data on gas prices by neighborhood in CA
  - Data contains info on neighborhoods both with/without Thrifty stations

- Suppose first that we only had data on gas prices from after the merger occurred.
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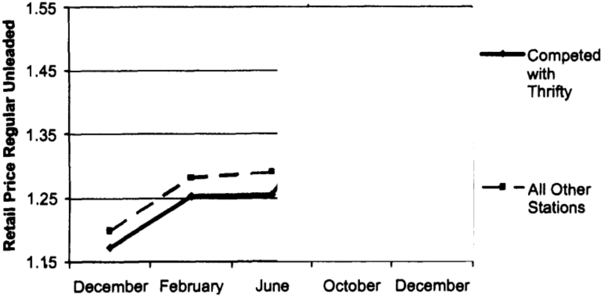
- With panel data, we can test this empirically by looking at prices before the merger!



(a) Los Angeles

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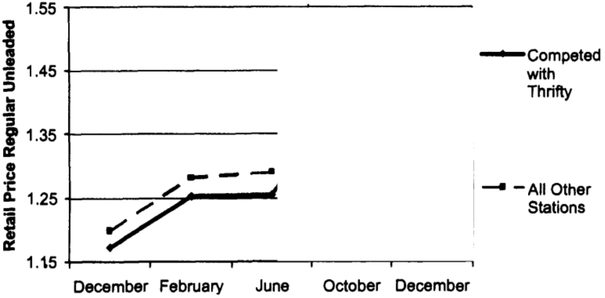

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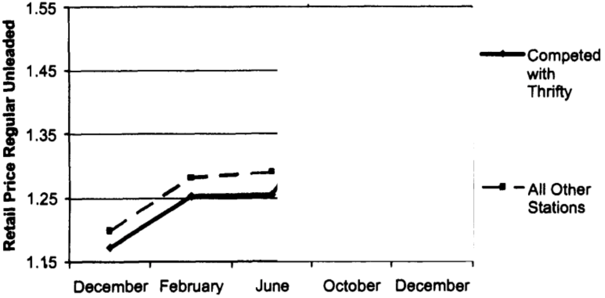

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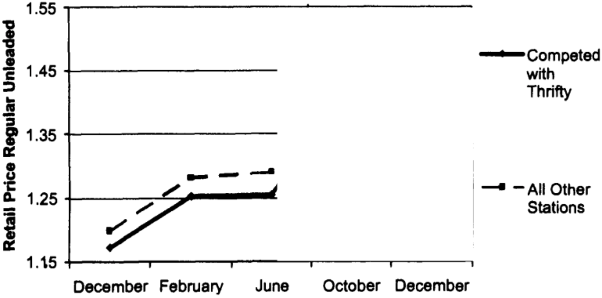

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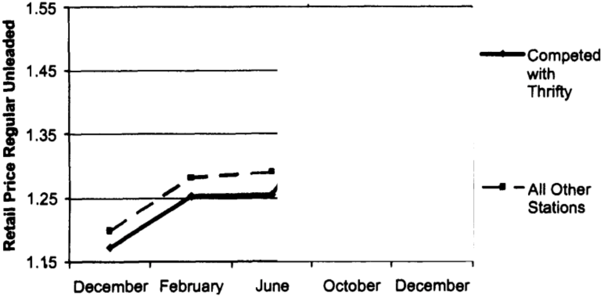

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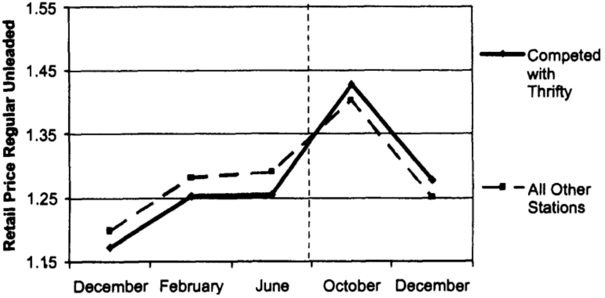

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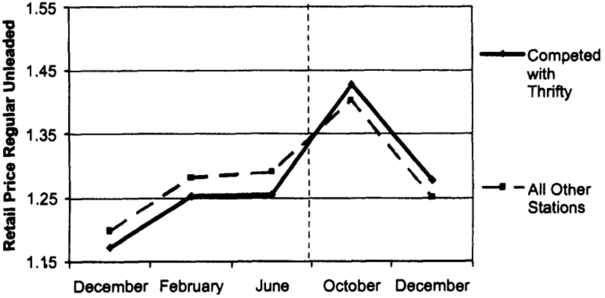

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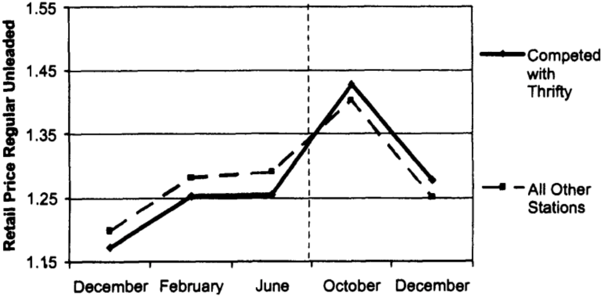

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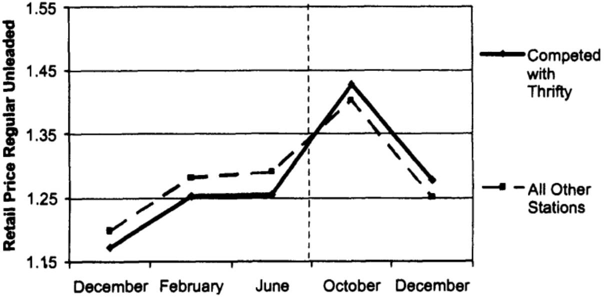

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- After the merger, stations in areas with a Thrifty had *higher prices* by about 2c
- If we assume that they would have had *lower* prices by 3c (as before the merger), then this implies a treatment effect of $2-(-3)=5$
- This is the post-treatment difference (2) between treatment & control minus the pre-treatment difference (-3), i.e. a *difference-in-differences*

Formalizing the Assumptions of DiD

- Assume there are 2 periods, $t = 1, 2$. Treated units ($D_i = 1$) are treated in period 2; control units never-treated.

- Let Y_{it} be the observed outcome for unit i in period t.
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- **No anticipation assumption**: $Y_{i1}(0) = Y_{i1}(1)$
  - Your treatment in period 2 doesn't affect your outcome in period 1

- **Parallel trends assumption**:

$$\underbrace{E[Y_{i2}(0) - Y_{i1}(0) | D_i = 1]}_{\text{Change in } Y(0) \text{ for treated}} = \underbrace{E[Y_{i2}(0) - Y_{i1}(0) | D_i = 0]}_{\text{Change in } Y(0) \text{ for control}}$$

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Equivalently,

$$\underbrace{E[Y_{i2}(0) | D_i = 1] - E[Y_{i2}(0) | D_i = 0]}_{\text{Selection bias in period 2}} = \underbrace{E[Y_{i1}(0) | D_i = 1] - E[Y_{i1}(0) | D_i = 0]}_{\text{Selection bias in period 1}}$$

- Under these assumptions, we have

$$\underbrace{E[Y_{i2} - Y_{i1}|D_i = 1]}_{\text{Observed change for treated}} - \underbrace{E[Y_{i2} - Y_{i1}|D_i = 0]}_{\text{Observed change for control}} =$$

$$= E[Y_{i2}(1) - Y_{i1}(1)|D_i = 1] - E[Y_{i2}(0) - Y_{i1}(0)|D_i = 0] \text{ (Observed data rule)}$$

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- Thus, the difference-in-difference of sample means identifies $\tau_{ATT} = E[Y_{i2}(1) - Y_{i2}(0)|D_i = 1]$.

- This is called the **average treatment effect on the treated** (ATT). It is the average effect in period 2 for treated units.

Estimating the ATT

- We've shown that under the DiD assumptions (parallel trends and no anticipation), the ATT is identified as

$$\tau_{ATT} = \underbrace{E[Y_{i2} - Y_{i1} | D_i = 1]}_{\text{Change in pop mean for treated}} - \underbrace{E[Y_{i2} - Y_{i1} | D_i = 0]}_{\text{Change in pop mean for control}}$$

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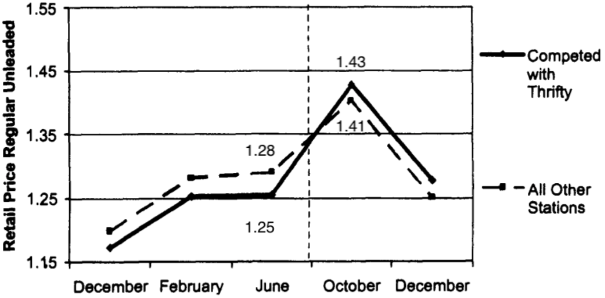
- Our estimate is:

$$\hat{\tau}_{ATT} = \underbrace{\bar{Y}_{12} - \bar{Y}_{11}}_{\text{Change in sample mean for treated}} - \underbrace{\bar{Y}_{02} - \bar{Y}_{01}}_{\text{Change in sample mean for control}},$$

where $\bar{Y}_{dt}$ is the sample mean for units with $D_i = d$ in period t.

Example

- Consider Hasting's example, comparing June (period 1) to October



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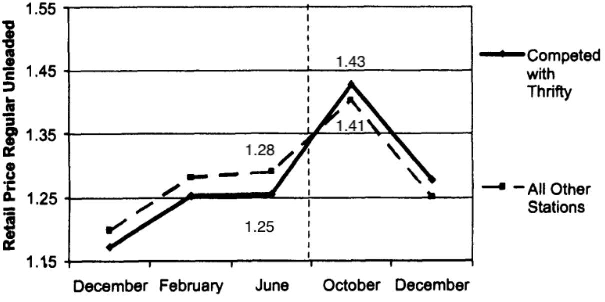

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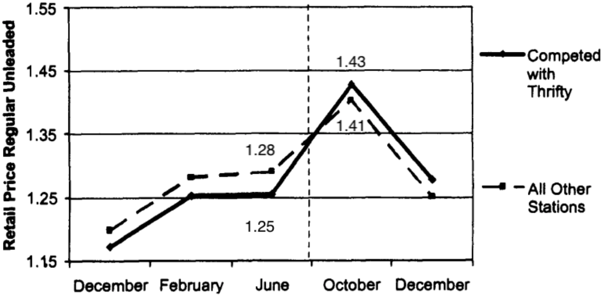

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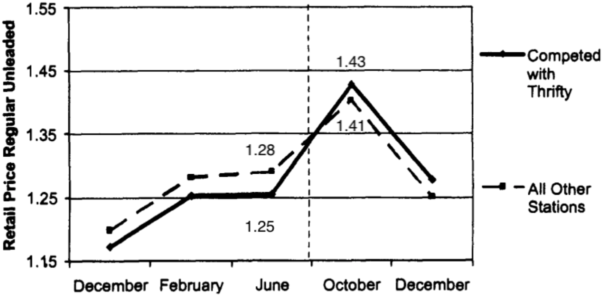

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DiD as Regression

- Consider the regression

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$$\begin{aligned} E[Y_{it}|D_i = 0, Post_t = 0] &= \beta_0 \\ E[Y_{it}|D_i = 0, Post_t = 1] &= \beta_0 + \beta_1 \\ E[Y_{it}|D_i = 1, Post_t = 0] &= \beta_0 + \beta_2 \\ E[Y_{it}|D_i = 1, Post_t = 1] &= \beta_0 + \beta_1 + \beta_2 + \beta_3 \end{aligned}$$

- Thus,

$$\begin{aligned} \beta_3 =& (E[Y_{it}|D_i = 1, Post_t = 1] - E[Y_{it}|D_i = 1, Post_t = 0]) - \\ & (E[Y_{it}|D_i = 0, Post_t = 1] - E[Y_{it}|D_i = 0, Post_t = 0]) = \tau_{ATT} \end{aligned}$$

DiD as Regression

- Consider the regression

$$Y_{it} = \beta_0 + \beta_1 \times Post_t + \beta_2 D_i + \beta_3 D_i \times Post_t + \varepsilon_{it},$$

  where $Post_t = 1[t = 2]$.
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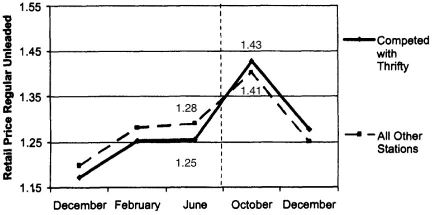
- Analogously, $\hat{\beta}_3 = (\bar{Y}_{12} - \bar{Y}_{11}) - (\bar{Y}_{02} - \bar{Y}_{01}) = \hat{\tau}_{ATT}$

Example



(a) LOS ANGELES

- Suppose we take the Hastings data from June/October and estimate

$$Y_{it} = \beta_0 + \beta_1 \times Post_t + \beta_2 D_i + \beta_3 D_i \times Post_t + \varepsilon_{it},$$

via OLS, where $Post_t$ is 1 for October and 0 for June.

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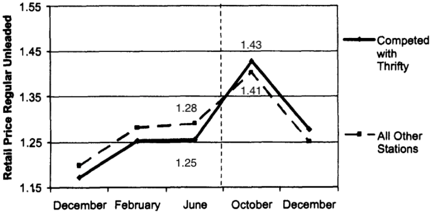

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via OLS, where $Post_t$ is 1 for October and 0 for June.

- We get the regression coefficients:

Constant ($\hat{\beta}_0$)	1.28
Post ($\hat{\beta}_1$)	0.13
Treated ($\hat{\beta}_2$)	-0.03
Treated $\times$ Post ($\hat{\beta}_3$)	0.05

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- You can also replace $D_i\gamma$ with a unit fixed effect λ_i and you get the exact same $\hat{\beta}_s$.

Example - Medicaid Expansion

- The Affordable Care Act (ACA, aka Obamacare) expanded Medicaid coverage to people with income up to 138% of the federal poverty line
- Medicaid expansion went into effect in 2014. However, some Republican-leaning states opted out of expanded coverage.
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- By 2015, 24 states had expanded Medicaid (more have done so since)
- Carey, Miller, and Wherry (2020) study the impacts of Medicaid expansion using a DiD design comparing early-adopting states to non-adopters.

Example - Medicaid Expansion

- A slightly simplified version of their regression specification is

$$Y_{its} = \phi_t + \lambda_s + \sum_{r \neq -1} D_i \times 1[t = 2014 + r] \times \beta_r + \varepsilon_{it}$$

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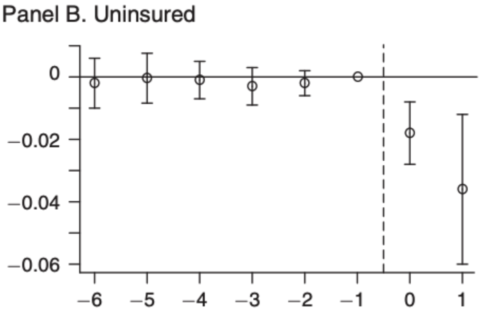

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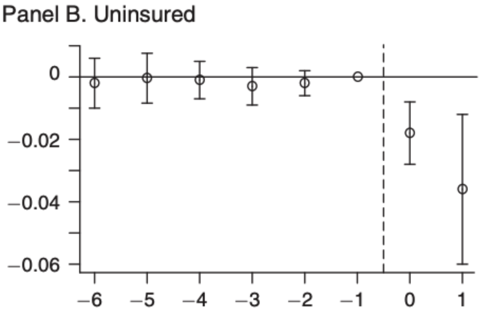
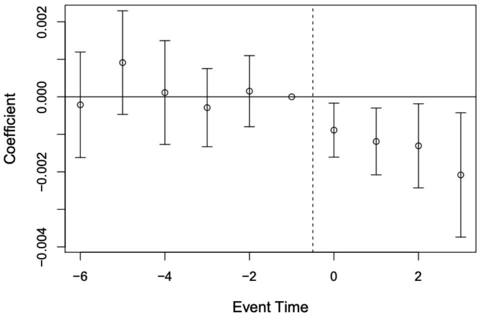


- Results show similar "pre-trends" but negative effects after treatment

In a related paper, some of the same authors used a similar research design to estimate the impacts on mortality

Figure 2: Effect of the ACA Medicaid Expansions on Annual Mortality



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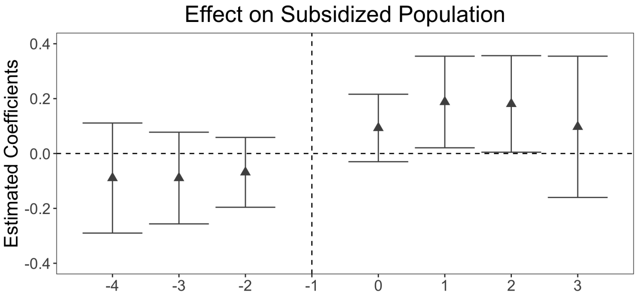

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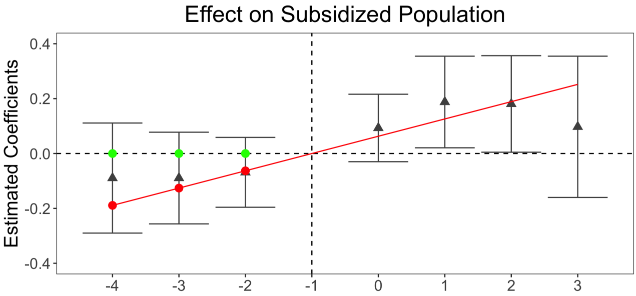

- Are you convinced there's an effect here? Maybe not!

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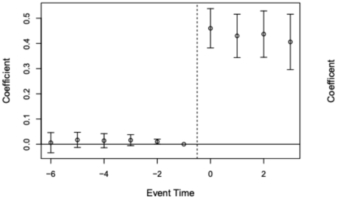

(a) Medicaid Eligibility

- What about here?

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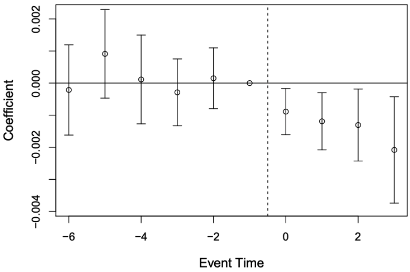

- And here?

Standard Errors for Panel Regressions

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  1. We expect Y_{i1} to be correlated with Y_{i2}, e.g., people with high earnings in 2010 also tend to have higher earnings in 2011. This is called *serial autocorrelation*
  2. More subtlely, if treatment is assigned at the state level, all people in a given state will have the same value of D_{it} (which is included in $\boldsymbol{X}_{it}$)

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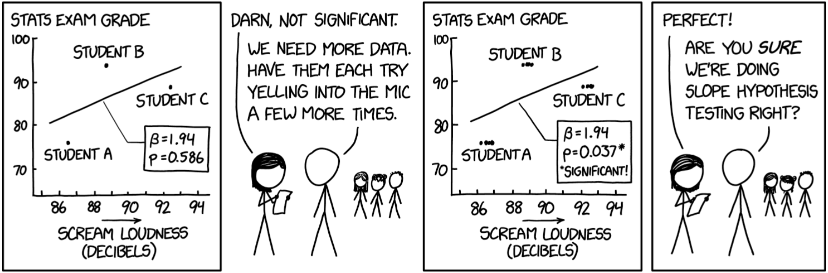
- In panel analyses, you should at minimum cluster at the individual level to allow for autocorrelation.
- If treatment is assigned at a more aggregate level, it is best to cluster at the level where treatment is assigned.
- Keep in mind: the number of "effective observations" (used for CLT) is the number of clusters
  - Clustered SEs will not be reliable when the number of clusters is very small (e.g. < 20)

XKCD



STATS EXAM GRADE
100
90
80
70
STUDENT B
STUDENT C
STUDENT A
β=1.94
p=0.586
86 88 90 92 94
SCREAM LOUDNESS (DECIBELS)
DARN, NOT SIGNIFICANT.
WE NEED MORE DATA. HAVE THEM EACH TRY YELLING INTO THE MIC A FEW MORE TIMES.
STATS EXAM GRADE
100
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STUDENT B
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β=1.94
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86 88 90 92 94
SCREAM LOUDNESS (DECIBELS)
PERFECT!
ARE YOU SURE WE'RE DOING SLOPE HYPOTHESIS TESTING RIGHT?

Implementing Clustered SEs

- Implementing clustered SEs in Stata is very easy
- Just replace
  reg y x, robust

  with
  reg y x, cluster(clustervar)

```
. reg vehicle_fatality_rate beertax i.state i.year, r

Linear regression                               Number of obs     =        336
                                                F(54, 281)        =     128.89
                                                Prob > F          =     0.0000
                                                R-squared         =     0.9089
                                                Root MSE          =     1.9e-05

------------------------------------------------------------------------------
             |               Robust
vehicle_fa~e | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
     beertax |   -.000064   .0000255    -2.51   0.013    -.0001141   -.0000139
             |
       state |
         AZ  |  -.0000547   .0000357    -1.53   0.127    -.0001249    .0000156
         AR  |  -.0000639   .0000293    -2.18   0.030    -.0001214   -6.26e-06
         CA  |  -.0001485   .0000412    -3.60   0.000    -.0002296   -.0000674
```

```
. reg vehicle_fatality_rate beertax i.state i.year, cluster(state)

Linear regression                               Number of obs     =        336
                                                F(6, 47)          =          .
                                                Prob > F          =          .
                                                R-squared         =     0.9089
                                                Root MSE          =     1.9e-05

                                 (Std. err. adjusted for 48 clusters in state)
------------------------------------------------------------------------------
             |               Robust
vehicle_fa~e | Coefficient  std. err.      t    P>|t|     [95% conf. interval]
-------------+----------------------------------------------------------------
     beertax |   -.000064   .0000386    -1.66   0.104    -.0001416    .0000136
             |
       state |
         AZ  |  -.0000547   .0000506    -1.08   0.286    -.0001566    .0000472
         AR  |  -.0000639   .0000399    -1.60   0.116     -.000144    .0000163
         CA  |  -.0001485   .0000589    -2.52   0.015    -.0002671     -.00003
```

The Quarterly Journal of Economics, February 2004

HOW MUCH SHOULD WE TRUST DIFFERENCES-IN-DIFFERENCES ESTIMATES?*

MARIANNE BERTRAND
ESTHER DUFLO
SENDHIL MULLAINATHAN

Most papers that employ Differences-in-Differences estimation (DD) use many years of data and focus on serially correlated outcomes but ignore that the resulting standard errors are inconsistent. To illustrate the severity of this issue, we randomly generate placebo laws in state-level data on female wages from the Current Population Survey. For each law, we use OLS to compute the DD estimate of its "effect" as well as the standard error of this estimate. These conventional DD standard errors severely understate the standard deviation of the estimators: we find an "effect" significant at the 5 percent level for up to 45 percent of the placebo interventions. We use Monte Carlo simulations to investi-

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  - In a competitive market, a floor on wages (i.e. the price of labor), should induce a decrease in demand
- To study this, CK study an episode in 1992 where NJ raised its minimum wage from \$4.25 to \$5.05
- They use a DiD comparing change in employment in fast food restaurants in NJ to that in neighboring PA , where the MW was flat at \$4.25

Variable		PA (i)	NJ (ii)	Difference, NJ-PA (iii)
1.	FTE employment before,	23.33	20.44	-2.89
	all available observations	(1.35)	(0.51)	(1.44)
2.	FTE employment after,	21.17	21.03	-0.14
	all available observations	(0.94)	(0.52)	(1.07)
3.	Change in mean FTE	-2.16	0.59	2.76
	employment	(1.25)	(0.54)	(1.36)

Notes: Adapted from Card and Krueger (1994), Table 3. The table reports average full-time equivalent (FTE) employment at restaurants in Pennsylvania and New Jersey before and after a minimum wage increase in New Jersey. The sample consists of all stores with data on employment. Employment at six closed stores is set to zero. Employment at four temporarily closed stores is treated as missing. Standard errors are reported in parentheses

Variable	PA (i)	NJ (ii)	Difference, NJ-PA (iii)
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (0.51)	-2.89 (1.44)
2. FTE employment after, all available observations	21.17 (0.94)	21.03 (0.52)	-0.14 (1.07)
3. Change in mean FTE employment	-2.16 (1.25)	0.59 (0.54)	2.76 (1.36)

Notes: Adapted from Card and Krueger (1994), Table 3. The table reports average full-time equivalent (FTE) employment at restaurants in Pennsylvania and New Jersey before and after a minimum wage increase in New Jersey. The sample consists of all stores with data on employment. Employment at six closed stores is set to zero. Employment at four temporarily closed stores is treated as missing. Standard errors are reported in parentheses

- Point estimates suggest an *increase* in employment of 2.76 FTEs, but not statisticially signfiicant.

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    - However, if the MW is raised to \$8/hour, then the firm has to pay the first 100 workers \$8 anyway, and would gladly hire the 101st worker at \$8/hour since this brings \$10 of profit.

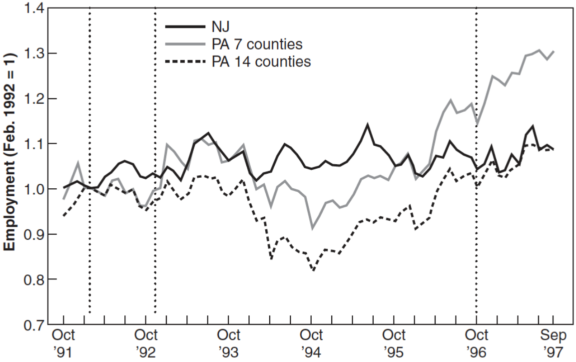

Figure 5.2.2 Employment in New Jersey and Pennsylvania fast food restaurants, October 1991 to September 1997 (from Card and Krueger 2000). Vertical lines indicate dates of the original Card and Krueger (1994) survey and the October 1996 federal minimum wage increase.

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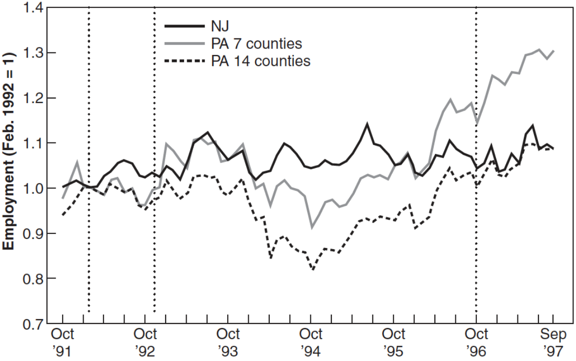

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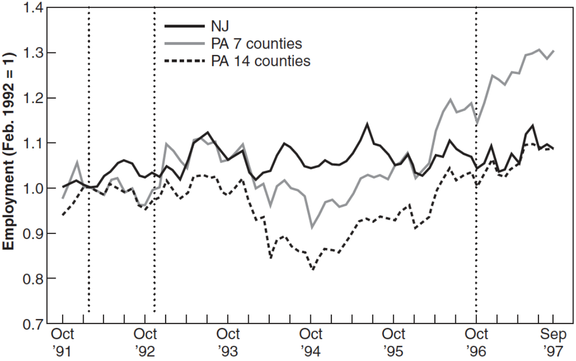

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- The two states do not move exactly in parallel even before the policy change in April 1992. We also only have 2 states!

Staggered Timing

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- But first we need to discuss DiD when treatment timing is staggered – e.g., states pass minimum wages in different years

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- In the two-period model, this corresponds to the diff-in-diff in sample means between treatment and control
- Unfortunately, it turns out that this estimator is *not* an average of DiDs between treated and untreated units in the staggered case.
  - See Borusyak and Jaravel (2016), de Chaisemartin and D'Haultfoeuille (2020), Goodman-Bacon (2021)

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- There are many implementations of this and related approaches, including Callaway and Sant'Anna (2020), Sun and Abraham (2020), Borusyak, Jaravel & Spiess (2021)

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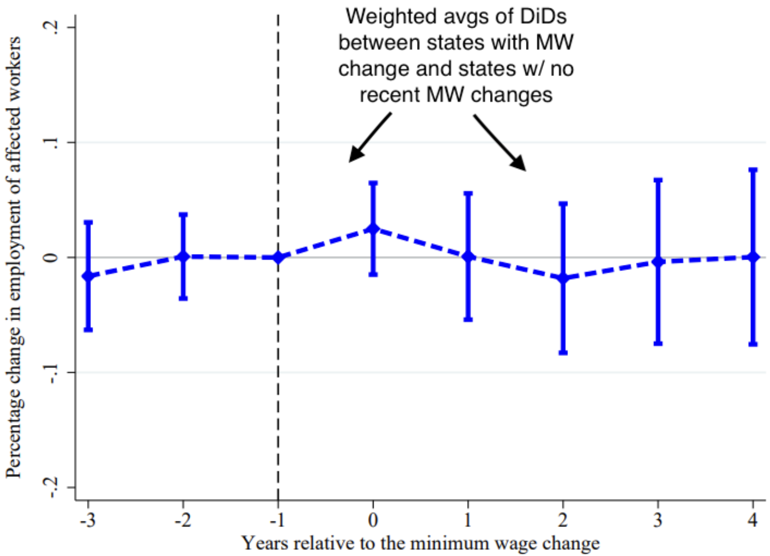
- They compute a DiD between the treated state and the matched control states
- They then take a weighted average of these DiDs to get an overall average effect

(a) Evolution of the missing and excess jobs



(b) Evolution of the employment of the affected workers

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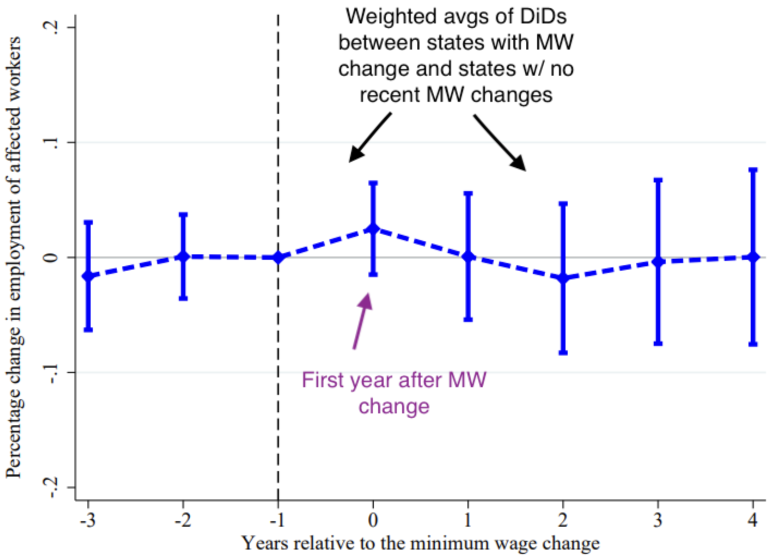

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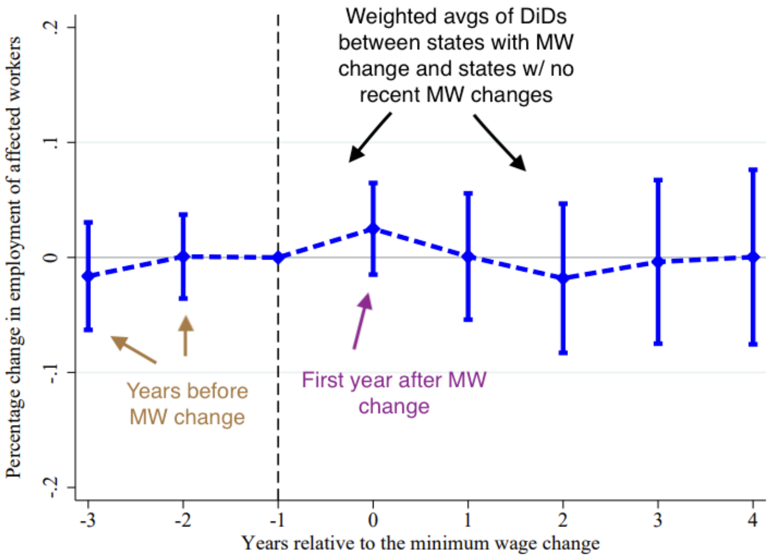

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Important Considerations/Caveats

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  - Not clear that changes in MW from \$4.25 to \$5.05 are informative abour raises from \$7.25 to \$15!

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- Historical analyses of MW are typically relatively short-run
  - Over long-run, MW increases may induce shifts in technology that replace workers
- There is still some debate among economists over whether MWs reduce employment!

Other Panel Data Methods

- We've focused on DiD, which is the most commonly-used panel data method in applied micro-economics
- But there are many others:
    - Controls for lagged dependent variables
    - Synthetic control
    - Matrix completion
- We won't have time to cover these, but if you're interested, I suggest taking more econometrics classes :)