Causal Inference

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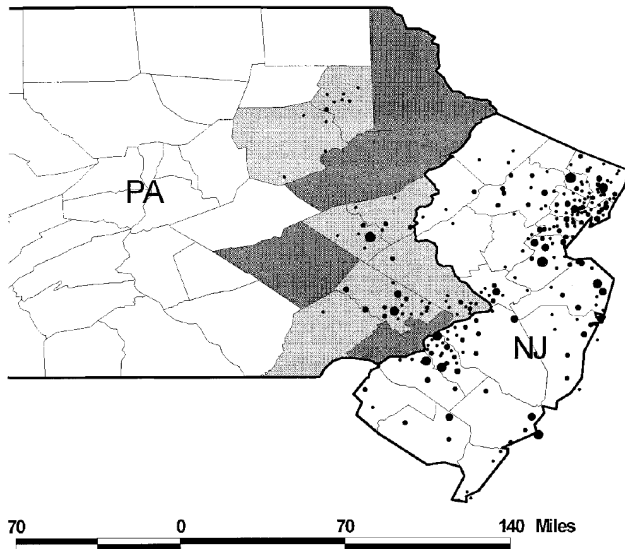
Selection on Unobservables

Problem

Often there are reasons to believe that treated and untreated units differ in unobservable characteristics that are associated with potential outcomes even after controlling for differences in observed characteristics.

In such cases, treated and untreated units are not directly comparable. What can we do then?
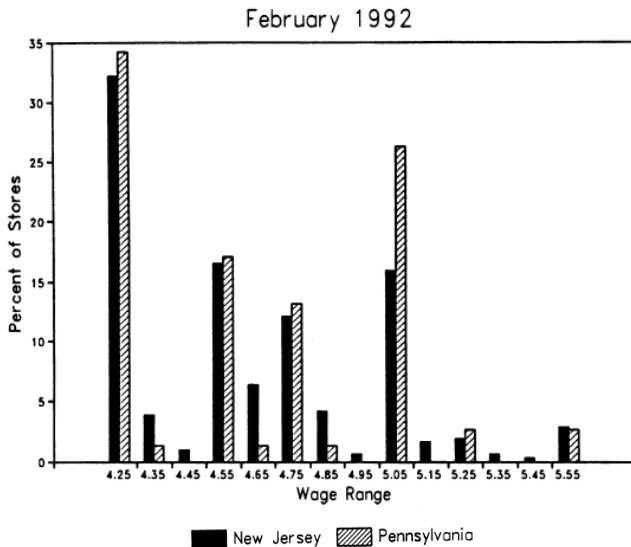
Example: Minimum wage laws and employment

- Do higher minimum wages decrease low-wage employment?

- Card and Krueger (1994) consider impact of New Jersey's 1992 minimum wage increase from \$4.25 to \$5.05 per hour

- Compare employment in 410 fast-food restaurants in New Jersey and eastern Pennsylvania before and after the rise

- Survey data on wages and employment from two waves:
  - Wave 1: March 1992, one month before the minimum wage increase
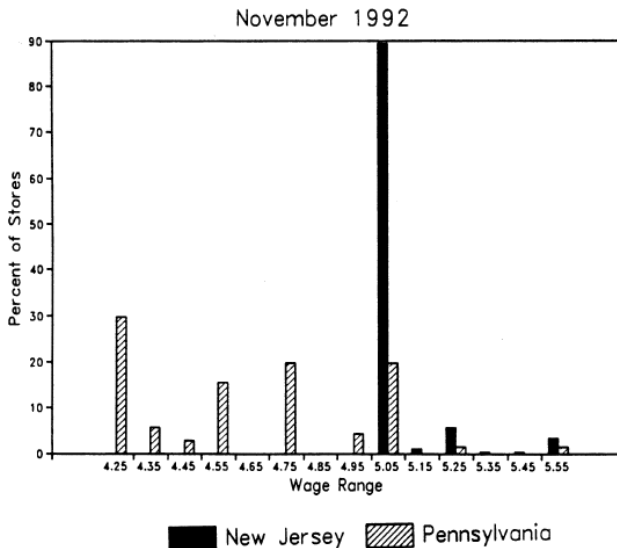 - Wave 2: December 1992, eight months after increase

Locations of Restaurants (Card and Krueger 2000)

Wages Before Rise in Minimum Wage

February 1992

Percent of Stores vs. Wage Range chart comparing New Jersey and Pennsylvania

New Jersey ▪ Pennsylvania ▨

Wages After Rise in Minimum Wage

November 1992

New Jersey   Pennsylvania

Two Groups and Two Periods

Definition

Two groups:

- $D = 1$ Treated units
- $D = 0$ Control units

Two periods:

- $T = 0$ Pre-Treatment period
- $T = 1$ Post-Treatment period

Potential outcomes $Y_d(t)$:

- $Y_{1i}(t)$ potential outcome unit i attains in period t when treated between t and $t - 1$
- $Y_{0i}(t)$ potential outcome unit i attains in period t with control between t and $t - 1$

Two Groups and Two Periods

Definition

Causal effect for unit i at time t is

- $\tau_{it} = Y_{1i}(t) - Y_{0i}(t)$

Observed outcomes $Y_i(t)$ are realized as

- $Y_i(t) = Y_{0i}(t) \cdot (1 - D_i(t)) + Y_{1i}(t) \cdot D_i(t)$

Fundamental problem of causal inference:

- If D occurs only after $t = 0$ ($D_i = D_i(1)$ and $Y_i(0) = Y_{0i}(0)$) we have: $Y_i(1) = Y_{0i}(1) \cdot (1 - D_i) + Y_{1i}(1) \cdot D_i$

Estimand (ATT)

Focus on estimating the average effect of the treatment on the treated:
$\tau_{ATT} = E[Y_1(1) - Y_0(1)|D = 1]$

Two Groups and Two Periods

Estimand (ATT)

$\tau_{ATT} = E[Y_1(1) - Y_0(1)|D = 1]$

	Post-Period (T=1)	**Pre-Period (T=0)**
Treated D=1	$E[Y_1(1)|D = 1]$	$E[Y_0(0)|D = 1]$
Control D=0	$E[Y_0(1)|D = 0]$	$E[Y_0(0)|D = 0]$

Problem

Missing potential outcome: $E[Y_0(1)|D = 1]$, ie. what is the average post-period outcome for the treated in the absence of the treatment?

Two Groups and Two Periods

Estimand (ATT)

$\tau_{ATT} = E[Y_1(1) - Y_0(1)|D = 1]$

	Post-Period (T=1)	Pre-Period (T=0)
Treated D=1	$E[Y_1(1)|D = 1]$	$E[Y_0(0)|D = 1]$
Control D=0	$E[Y_0(1)|D = 0]$	$E[Y_0(0)|D = 0]$

Control Strategy: Before vs. After

- Use: $E[Y(1)|D = 1] - E[Y(0)|D = 1]$

Two Groups and Two Periods

Estimand (ATT)
$\tau_{ATT} = E[Y_1(1) - Y_0(1)|D = 1]$

	Post-Period (T=1)	**Pre-Period (T=0)**
Treated D=1	$E[Y_1(1)|D = 1]$	$E[Y_0(0)|D = 1]$
Control D=0	$E[Y_0(1)|D = 0]$	$E[Y_0(0)|D = 0]$

Control Strategy: Before-After Comparison

- Use: $E[Y(1)|D = 1] - E[Y(0)|D = 1]$
- Assumes: $E[Y_0(1)|D = 1] = E[Y_0(0)|D = 1]$

Two Groups and Two Periods

Estimand (ATT)

$\tau_{ATT} = E[Y_1(1) - Y_0(1)|D = 1]$

	Post-Period (T=1)	**Pre-Period (T=0)**
Treated D=1	$E[Y_1(1)|D = 1]$	$E[Y_0(0)|D = 1]$
Control D=0	$E[Y_0(1)|D = 0]$	$E[Y_0(0)|D = 0]$

Control Strategy: Treated-Control Comparison in Post-Period

- Use: $E[Y(1)|D = 1] - E[Y(1)|D = 0]$

Two Groups and Two Periods

Estimand (ATT)

$\tau_{ATT} = E[Y_1(1) - Y_0(1)|D = 1]$

	Post-Period (T=1)	**Pre-Period (T=0)**
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- Use: $E[Y(1)|D = 1] - E[Y(1)|D = 0]$
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Two Groups and Two Periods

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	Post-Period (T=1)	**Pre-Period (T=0)**
Treated D=1	$E[Y_1(1)|D = 1]$	$E[Y_0(0)|D = 1]$
Control D=0	$E[Y_0(1)|D = 0]$	$E[Y_0(0)|D = 0]$

Control Strategy: Difference-in-Differences (DD)

- Use:
$$\left\{ E[Y(1)|D = 1] - E[Y(1)|D = 0] \right\} -$$
$$\left\{ E[Y(0)|D = 1] - E[Y(0)|D = 0] \right\}$$
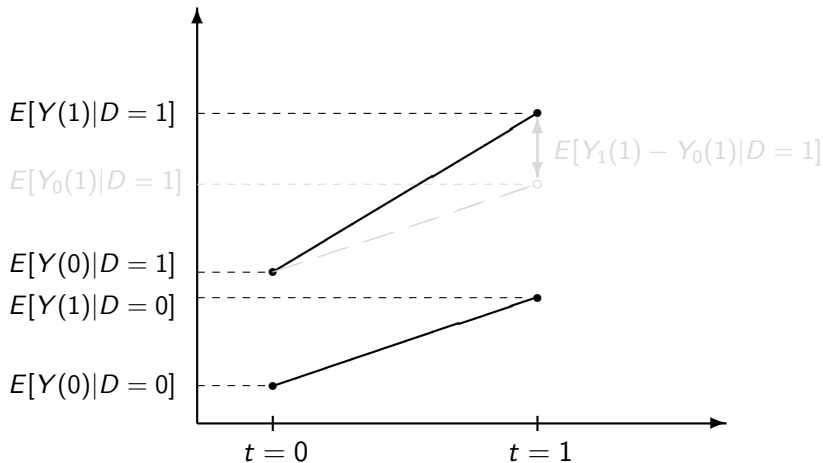
Two Groups and Two Periods

Estimand (ATT)

$\tau_{ATT} = E[Y_1(1) - Y_0(1)|D = 1]$

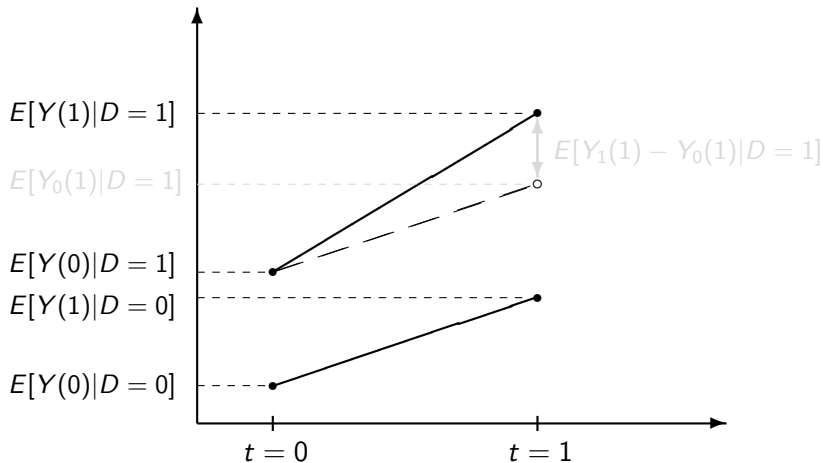|	**Post-Period (T=1)**	**Pre-Period (T=0)**
Treated D=1	$E[Y_1(1)|D = 1]$	$E[Y_0(0)|D = 1]$
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Control Strategy: Difference-in-Differences (DD)

- Use:
$$\left\{ E[Y(1)|D = 1] - E[Y(1)|D = 0] \right\} -$$
$$\left\{ E[Y(0)|D = 1] - E[Y(0)|D = 0] \right\}$$
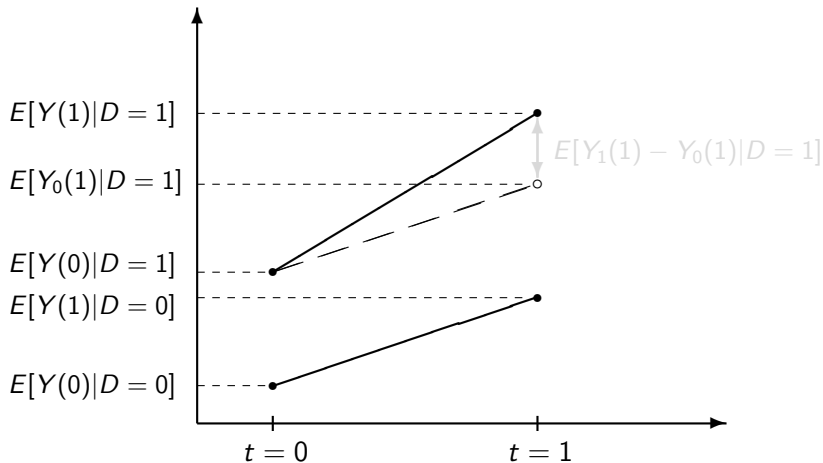- Assumes: $E[Y_0(1) - Y_0(0)|D = 1] = E[Y_0(1) - Y_0(0)|D = 0]$

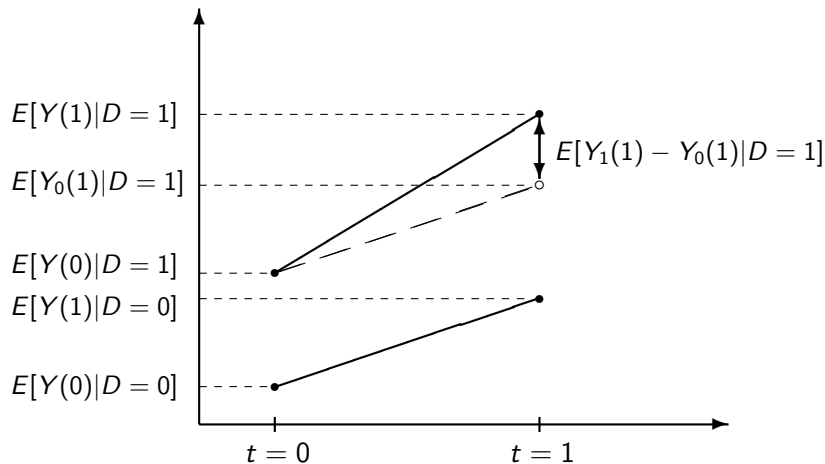# Graphical Representation: Difference-in-Differences

Graphical Representation: Difference-in-Differences

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Graphical Representation: Difference-in-Differences

Identification with Difference-in-Differences

Identification Assumption (parallel trends)

$E[Y_0(1) - Y_0(0)|D = 1] = E[Y_0(1) - Y_0(0)|D = 0]$

Identfication Result

Given parallel trends the ATT is identified as:

$$
\begin{aligned}
E[Y_1(1) - Y_0(1)|D = 1] &= \left\{ E[Y(1)|D = 1] - E[Y(1)|D = 0] \right\} \\
&- \left\{ E[Y(0)|D = 1] - E[Y(0)|D = 0] \right\}
\end{aligned}
$$

Identification with Difference-in-Differences

Identification Assumption (parallel trends)
$$E[Y_0(1) - Y_0(0)|D = 1] = E[Y_0(1) - Y_0(0)|D = 0]$$

Proof.
Note that the identification assumption implies
$$E[Y_0(1)|D = 0] = E[Y_0(1)|D = 1] - E[Y_0(0)|D = 1] + E[Y_0(0)|D = 0]$$
plugging in we get

$$
\begin{aligned}
& \{E[Y(1)|D = 1] - E[Y(1)|D = 0]\} - \{E[Y(0)|D = 1] - E[Y(0)|D = 0]\} \\
= \; & \{E[Y_1(1)|D = 1] - E[Y_0(1)|D = 0]\} - \{E[Y_0(0)|D = 1] - E[Y_0(0)|D = 0]\} \\
= \; & \{E[Y_1(1)|D = 1] - (E[Y_0(1)|D = 1] - E[Y_0(0)|D = 1] + E[Y_0(0)|D = 0])\} \\
- \; & \{E[Y_0(0)|D = 1] - E[Y_0(0)|D = 0]\} \\
= \; & E[Y_1(1) - Y_0(1)|D = 1] + \{E[Y_0(0)|D = 1] - E[Y_0(0)|D = 0]\} \\
- \; & \{E[Y_0(0)|D = 1] - E[Y_0(0)|D = 0]\} \\
= \; & E[Y_1(1) - Y_0(1)|D = 1]
\end{aligned}
$$

$\square$

Difference-in-Differences: Estimators

Estimand (ATT)

$$
\begin{aligned}
E[Y_1(1) - Y_0(1)|D = 1] &= \Big\{ E[Y(1)|D = 1] - E[Y(1)|D = 0] \Big\} \\
&- \Big\{ E[Y(0)|D = 1] - E[Y(0)|D = 0] \Big\}
\end{aligned}
$$

Estimator (Sample Means: Panel)

$$
\begin{aligned}
&\left\{ \frac{1}{N_1} \sum_{D_i=1} Y_i(1) - \frac{1}{N_0} \sum_{D_i=0} Y_i(1) \right\} - \left\{ \frac{1}{N_1} \sum_{D_i=1} Y_i(0) - \frac{1}{N_0} \sum_{D_i=0} Y_i(0) \right\} \\
&= \left\{ \frac{1}{N_1} \sum_{D_i=1} \{Y_i(1) - Y_i(0)\} - \frac{1}{N_0} \sum_{D_i=0} \{Y_i(1) - Y_i(0)\} \right\},
\end{aligned}
$$

where N_1 and N_0 are the number of treated and control units respectively.

Sample Means: Minimum wage laws and employment

| | Stores by state | | |
| | PA (i) | NJ (ii) | Difference, NJ − PA (iii) |
Variable			
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)

Difference-in-Differences: Estimators

Estimator (Sample Means: Repeated Cross-Sections)

Let $\{Y_i, D_i, T_i\}_{i=1}^n$ be the pooled sample (the two different cross-sections merged) where T is a random variable that indicates the period (0 or 1) in which the individual is observed.

The difference-in-differences estimator is given by:

$$\left\{ \frac{\sum D_i \cdot T_i \cdot Y_i}{\sum D_i \cdot T_i} - \frac{\sum (1-D_i) \cdot T_i \cdot Y_i}{\sum (1-D_i) \cdot T_i} \right\}$$
$$- \left\{ \frac{\sum D_i \cdot (1-T_i) \cdot Y_i}{\sum D_i \cdot (1-T_i)} - \frac{\sum (1-D_i) \cdot (1-T_i) \cdot Y_i}{\sum (1-D_i) \cdot (1-T_i)} \right\}$$

Difference-in-Differences: Estimators

Estimator (Regression: Repeated Cross-Sections)

Alternatively, the same estimator can be obtained using regression techniques. Consider the linear model:

$$Y = \mu + \gamma \cdot D + \delta \cdot T + \tau \cdot (D \cdot T) + \varepsilon,$$

where $E[\varepsilon|D, T] = 0$.

Easy to show that τ estimates the DD effect:

$$\tau = \{E[Y|D = 1, T = 1] - E[Y|D = 0, T = 1]\}$$
$$- \{E[Y|D = 1, T = 0] - E[Y|D = 0, T = 0]\}$$

Difference-in-Differences: Estimators

Estimator (Regression: Repeated Cross-Sections)

Alternatively, the same estimator can be obtained using regression techniques. Consider the linear model:

$$Y = \mu + \gamma \cdot D + \delta \cdot T + \tau \cdot (D \cdot T) + \varepsilon,$$

where $E[\varepsilon | D, T] = 0$.

	After (T=1)	**Before (T=0)**	**After - Before**
Treated D=1	$\mu + \gamma + \delta + \tau$	$\mu + \gamma$	$\delta + \tau$
Control D=0	$\mu + \delta$	μ	δ
Treated - Control	$\gamma + \tau$	γ	τ

Regression: Minimum wage laws and employment

```
> d <- read.dta("CK1994_longformat.dta",convert.factors = FALS
> head(d[, c('ID', 'nj', 'postperiod', 'emptot')])
  ID nj postperiod emptot
1  1  0          0  40.50
2  1  0          1  24.00
3  2  0          0  13.75
4  2  0          1  11.50
5  3  0          0   8.50
6  3  0          1  10.50
```

Regression: Minimum wage laws and employment

```
with(d,
 (
  mean(emptot[nj == 1 & postperiod == 1], na.rm = TRUE) -
  mean(emptot[nj == 1 & postperiod == 0], na.rm = TRUE)
  ) -
  (mean(emptot[nj == 0 & postperiod == 1], na.rm = TRUE) -
  mean(emptot[nj == 0 & postperiod == 0], na.rm = TRUE)
  )
 )
[1] 2.753606
```

Regression: Minimum wage laws and employment

```
> ols <- lm(emptot ~ postperiod * nj, data = d)
> coeftest(ols)

t test of coefficients:

             Estimate Std. Error t value Pr(>|t|)
(Intercept)   23.3312     1.0719 21.7668  < 2e-16 ***
postperiod    -2.1656     1.5159 -1.4286  0.15351
nj            -2.8918     1.1935 -2.4229  0.01562 *
postperiod:nj  2.7536     1.6884  1.6309  0.10331
```

Note: Should adjust standard errors to account for temporal dependence

Difference-in-Differences: Estimators

Estimator (Regression: Repeated Cross-Sections)

Can use regression version of the DD estimator to include covariates:

$$Y = \mu + \gamma \cdot D + \delta \cdot T + \tau \cdot (D \cdot T) + X'\beta + \varepsilon.$$

- *introducing time-invariant X's is not helpful (they get differenced-out)*
- *be careful with time-varying X's: they are often affected by the treatment and may introduce endogeneity (e.g. price of meal)*
- *always correct standard errors to account for temporal dependence*

Can interact time-invariant covariates with the time indicator:

$$Y = \mu + \gamma \cdot D + \delta \cdot T + \alpha \cdot (D \cdot T) + X'\beta_0 + (T \cdot X')\beta_1 + \varepsilon$$

$\Rightarrow X$ is used to explain differences in trends.

Difference-in-Differences: Estimators

Estimator (Regression: Panel Data)

With panel data we can estimate the difference-in-differences effect using a fixed effects regression with unit and period fixed effects:

$$Y_{it} = \mu + \gamma_i + \delta T + \tau D_{it} + X_{it}'\beta + \varepsilon_{it}$$

- *One intercept for each unit γ_i*

- *D_{it} coded as 1 for treated in post-period and 0 otherwise*

Or equivalently we can use regression with the dependent variable in first differences:

$$\Delta Y_i = \delta + \tau \cdot D_i + u_i,$$

where $\Delta Y_i = Y_i(1) - Y_i(0)$ and $u_i = \Delta\varepsilon_i$.

Regression: Minimum wage laws and employment

```
library(plm)
library(lmtest)

> d$Dit <- d$nj * d$postperiod

> d <- plm.data(d, indexes = c("ID", "postperiod"))

> did.reg <- plm(emptot ~ postperiod + Dit, data = d,
                         model = "within")

> coeftest(did.reg, vcov=function(x)
                 vcovHC(x, cluster="group", type="HC1"))

t test of coefficients:

            Estimate Std. Error t value Pr(>|t|)
postperiod1  -2.2833     1.2465 -1.8319  0.06775 .
Dit           2.7500     1.3359  2.0585  0.04022 *
```

Regression: Minimum wage laws and employment

```
> firstdiff.mod <- plm(emptot ~ postperiod * nj,
                                 data = d, model = "fd")
> coeftest(firstdiff.mod, vcov=function(x) vcovHC(x, type="HC0"))

t test of coefficients:

              Estimate Std. Error t value Pr(>|t|)
postperiod1    -2.2833     1.2465 -1.8319  0.06775 .
postperiod1:nj  2.7500     1.3359  2.0585  0.04022 *
---
Signif. codes:  0 *** 0.001 ** 0.01 * 0.05 . 0.1   1
```

Difference-in-Differences: Threats to Validity

1 Non-parallel dynamics

2 Compositional differences

3 Long-term effects versus reliability

4 Functional form dependence

Bias is a matter of degree. Small violations of the identification assumptions may not matter much as the bias may be rather small. However, biases can sometimes be so large that the estimates we get are completely wrong, even of the opposite sign of the true treatment effect.

Helpful to avoid overly strong causal claims for difference-in-differences estimates.

Difference-in-Differences: Threats to Validity

1 *Non-parallel dynamics*: Often treatments/programs are targeted based on pre-existing differences in outcomes.

- "Ashenfelter dip": participants in training programs often experience a dip in earnings just before they enter the program (that may be *why* they participate). Since wages have a natural tendency to mean reversion, comparing wages of participants and non-participants using DD leads to an upward biased estimate of the program effect

- Regional targeting: NGOs may target villages that appear most promising (or worst off)
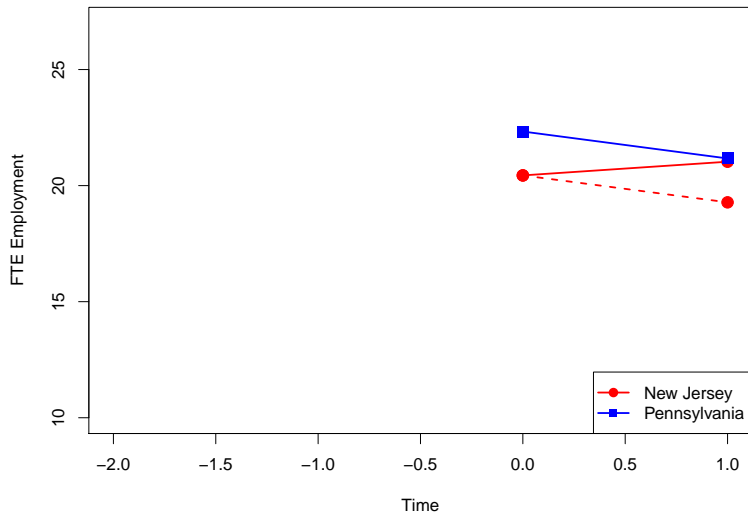
Checks for Difference-in-Differences Design

1. Falsification test using data for prior periods

2. Falsification test using data for alternative control group

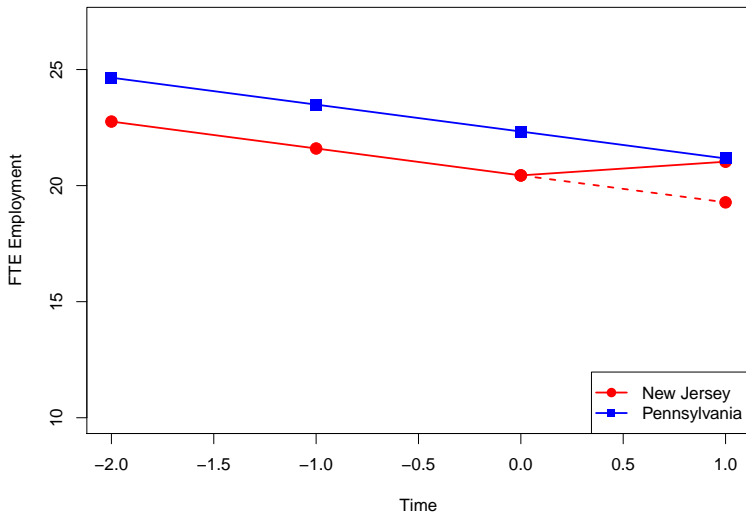3. Falsification test using alternative placebo outcome that is not supposed to be affected by the treatment
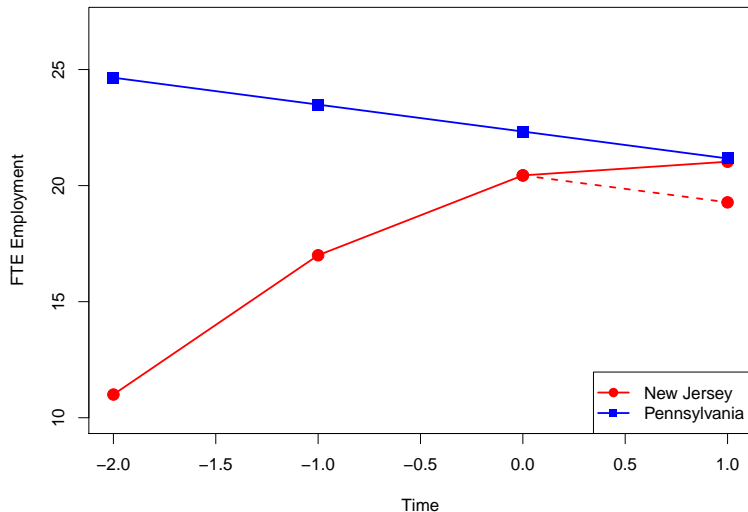
Falsification test: Data for prior periods
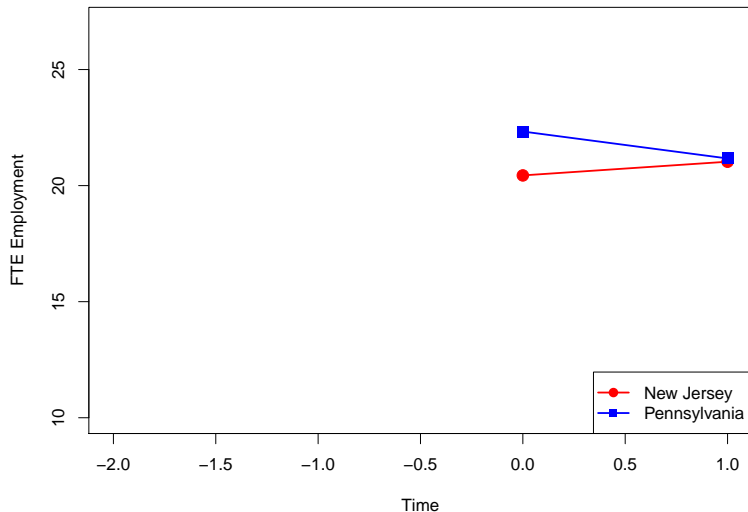
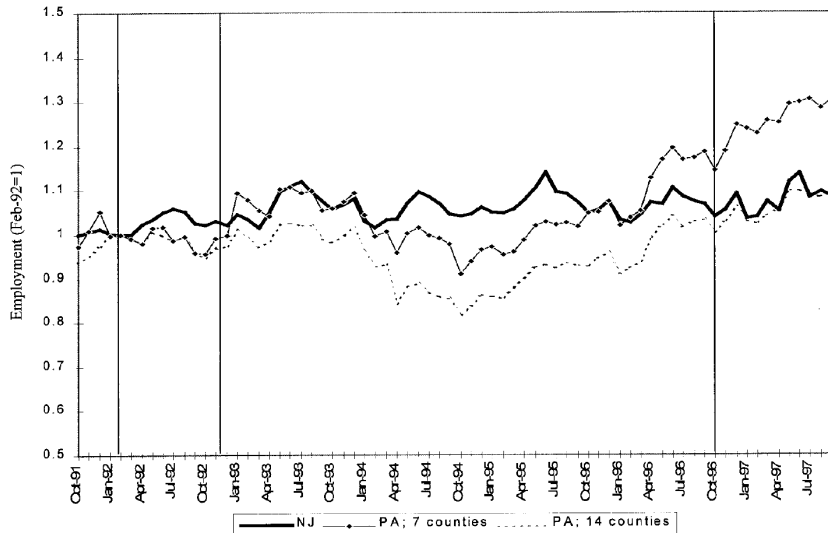# Falsification test: Data for prior periods

Falsification test: Data for prior periods

Falsification test: Data for prior periods

Longer Trends in Employment (Card and Krueger 2000)

Falsification test: Alternative control group

| Variable | Stores by state | | | Stores in New Jersey[a] | | | Differences within NJ[b] | |
	PA (i)	NJ (ii)	Difference, NJ − PA (iii)	Wage = $4.25 (iv)	Wage = $4.26–$4.99 (v)	Wage ≥ $5.00 (vi)	Low–high (vii)	Midrange–high (viii)
1. FTE employment before, all available observations	23.33 (1.35)	20.44 (0.51)	− 2.89 (1.44)	19.56 (0.77)	20.08 (0.84)	22.25 (1.14)	− 2.69 (1.37)	− 2.17 (1.41)
2. FTE employment after, all available observations	21.17 (0.94)	21.03 (0.52)	− 0.14 (1.07)	20.88 (1.01)	20.96 (0.76)	20.21 (1.03)	0.67 (1.44)	0.75 (1.27)
3. Change in mean FTE employment	− 2.16 (1.25)	0.59 (0.54)	2.76 (1.36)	1.32 (0.95)	0.87 (0.84)	− 2.04 (1.14)	3.36 (1.48)	2.91 (1.41)

If placebo DD between original and alternative control group is not zero, then the original DD may be biased

Triple DDD: Mandated Maternity Benefits (Gruber, 1994)

TABLE 3—DDD ESTIMATES OF THE IMPACT OF STATE MANDATES
ON HOURLY WAGES

Location/year	Before law change	After law change	Time difference for location
A. *Treatment Individuals: Married Women, 20–40 Years Old:*			
Experimental states	1.547 (0.012) [1,400]	1.513 (0.012) [1,496]	−0.034 (0.017)
Nonexperimental states	1.369 (0.010) [1,480]	1.397 (0.010) [1,640]	0.028 (0.014)
Location difference at a point in time:	0.178 (0.016)	0.116 (0.015)	
Difference-in-difference:		−0.062 (0.022)	

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B. *Control Group: Over 40 and Single Males 20–40:*			
Experimental states	1.759 (0.007) [5,624]	1.748 (0.007) [5,407]	−0.011 (0.010)
Nonexperimental states	1.630 (0.007) [4,959]	1.627 (0.007) [4,928]	−0.003 (0.010)
Location difference at a point in time:	0.129 (0.010)	0.121 (0.010)	
Difference-in-difference:		−0.008: (0.014)	

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Nonexperimental states	1.630 (0.007) [4,959]	1.627 (0.007) [4,928]	−0.003 (0.010)
Location difference at a point in time:	0.129 (0.010)	0.121 (0.010)	
Difference-in-difference:		−0.008: (0.014)	
DDD:		**−0.054** **(0.026)**	

How useful is the Triple DDD?

- The DDD estimate is the difference between the DD of interest and the placebo DD (that is supposed to be zero)

    - If the placebo DD is non zero, it might be difficult to convince reviewers that the DDD removes all the bias

    - If the placebo DD is zero, then DD and DDD give the same results but DD is preferable because standard errors are smaller for DD than for DDD

Difference-in-Differences: Further Threats to Validity

2 *Compositional differences*

- In repeated cross-sections, we do not want that the composition of the sample changes between periods.
- Example:
    - Hong (2011) uses repeated cross-sectional data from Consumer Expenditure Survey (CEX) containing music expenditures and internet use for random samples of U.S. households
    - Study exploits the emergence of Napster (the first sharing software widely used by Internet users) in June 1999 as a natural experiment.
    - Study compares internet users and internet non-users, before and after emergence of Napster

Compositional differences?

Figure 1: Internet Diffusion and Average Quarterly Music Expenditure in the CEX
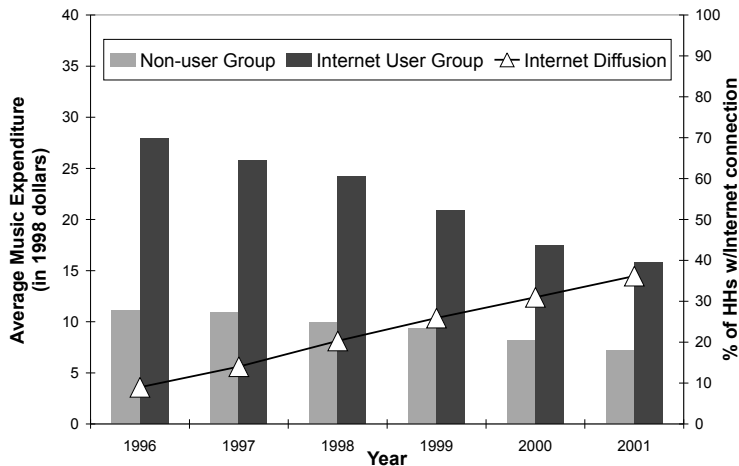
Compositional differences?

Table 1: Descriptive Statistics for Internet User and Non-user Groups[a]

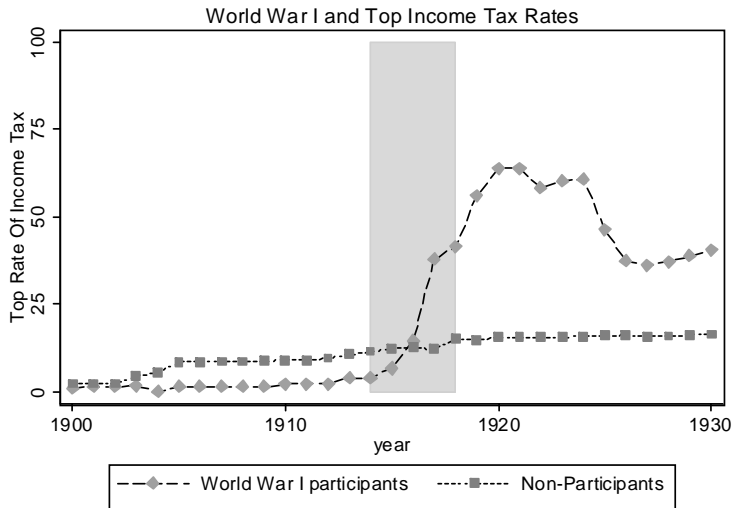| Year	1997		1998		1999	
	Internet User	Non-user	Internet User	Non-user	Internet User	Non-user
Average Expenditure						
Recorded Music	$25.73	$10.90	$24.18	$9.97	$20.92	$9.37
Entertainment	$195.03	$96.71	$193.38	$84.92	$182.42	$80.19
Zero Expenditure						
Recorded Music	.56	.79	.60	.80	.64	.81
Entertainment	.08	.32	.09	.35	.14	.39
Demographics						
Age	40.2	49.0	42.3	49.0	44.1	49.4
Income	$52,887	$30,459	$51,995	$28,169	$49,970	$26,649
High School Grad.	.18	.31	.17	.32	.21	.32
Some College	.37	.28	.35	.27	.34	.27
College Grad.	.43	.21	.45	.21	.42	.20
Manager	.16	.08	.16	.08	.14	.08

Diffusion of the internet changes samples (e.g. younger music fans are early adopters)

Difference-in-Differences: Further Threats to Validity

3. *Long-term effects versus reliability*:

   - Parallel trends assumption for DD is more likely to hold over a shorter time-window

   - In the long-run, many other things may happen that could confound the effect of the treatment

   - Should be cautious to extrapolate short-term effects to long-term effects

Effect of War on Tax Rates (Scheve and Stasavage 2010)

World War I and Top Income Tax Rates

Difference-in-Differences: Further Threats to Validity

4. *Functional form dependence*: Magnitude or even sign of the DD effect may be sensitive to the functional form, when average outcomes for controls and treated are very different at baseline

   - Training program for the young:
     - Employment for the young increases from 20% to 30%
     - Employment for the old increases from 5% to 10%
     - Positive DD effect: $(30 - 20) - (10 - 5) = 5\%$ increase
     - But if you consider log changes in employment, the DD is, $[log(30) - log(20)] - [log(10) - log(5)] = log(1.5) - log(2) < 0$

   - DD estimates may be more reliable if treated and controls are more similar at baseline

   - More similarity may help with parallel trends assumption

Difference-in-Differences: Further Threats to Validity

4 *Functional form dependence*: Magnitude or even sign of the DD effect may be sensitive to the functional form, when average outcomes for controls and treated are very different at baseline

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- DD estimates may be more reliable if treated and controls are more similar at baseline
- More similarity may help with parallel trends assumption

Matching and difference-in-differences

- Combine matching and difference-in-differences:
    - Match on pre-treatment covariates and (lagged) outcomes
    - Run difference-in-differences regression in matched data-set
    - Can also use inverse-propensity score weighting (Hirano, Imbens, and Ridder 2003; Imai and Kim 2012)

- Can also combine difference-in-differences with regression discontinuity design or randomized experiment

Effect of Voter ID Laws

Voter Identification laws: require government ID to vote

- Minority voters: much less likely to hold IDs (Ansolabehere and Hersh 2016)
- What is effect of ID laws on turnout?
- Methods question: assess effect using surveys?

Survey Data and Effects of Election Administration

"Our article evaluates this research and disputes the strength of the statistical arguments used to support findings of an observable negative effect on turnout from voter ID laws. Alternatively, we adjust the models using state samples and difference-indifferences techniques and reanalyze the CPS data for the 2002 and 2006 midterm elections. While we do not conclude that voter ID rules have no effect on turnout, our data and tools are not up to the task of making a compelling statistical argument for an effect " (Erikson and Minnite 2009)

Obstacles to Estimating Voter ID Laws' Effect

Hajnal, Lajevardi, and Nielson (2017) (HLN) $\rightsquigarrow$ Voter ID laws suppress turnout of minority voters, estimate effect using CCES survey data

- General election $\rightsquigarrow$ hispanic voters
- Primary election $\rightsquigarrow$ hispanic, black, and asian voters

Limitations of the design

1) Placebo test: cross sectional designs suffer from selection
2) Difference in Differences in HLN reports positive effect $\rightsquigarrow$ Merge error in Virginia (2006, 2008, and 2010) and other 2006 states
3) Once merge error corrected: data + designs provide positive, negative, or null effects

No reliable inference $\rightsquigarrow$ Administrative data essential to estimate effects

" The analysis shows that strict identification laws have a differentially negative impact on the turnout of racial and ethnic minorities in primaries and general elections"

HLN: Research Design

Data: Cooperative Congressional Election Study (2006-2014)
- Merge: Strict voter ID law in state
- Dependent Variable: General/Primary Election Turnout
- Treatment: Strict Voter ID Law

1) Selection on observables: cross sectional comparion
   - Effect heterogeneity by race, party ID, and ideology
2) Difference-in-Differences: state and year fixed effects
   - Effect heterogeneity by race, party ID, and ideology

HLN Results

Voter ID laws suppress turnout

- General election: increase gap between white and hispanic turnout (general election)
- Primary Election: Increase gap between white and hispanic, black, and asian turnout (primary elections)
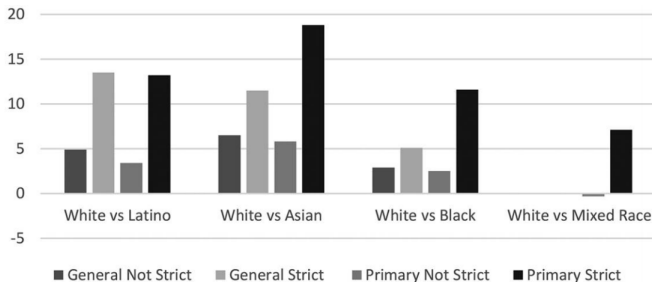

Figure 2. Photo ID laws and predicted racial gaps in turnout. Race-specific effect for white versus Asian and white versus black in general elections and multiracial effect in primaries are not significant at $p < .05$.

Assessing Cross Sectional Design: Placebo Test

- If cross sectional (selection on observables) design works:

Assessing Cross Sectional Design: Placebo Test

- If cross sectional (selection on observables) design works:
    - States that implement voter ID laws are (conditionally) on average similar to states that do not

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    - No "effect" of being a strict voter ID law in the past
- Placebo test: assess "effect" of being future strict voter ID law state on turnout before law implemented

Assessing Cross Sectional Design: Placebo Test

	(1)	(2)	(3)	(4)	(5)	(6)	
			General Elections:				
Include respondents who self-classify as unregistered	No	No	Yes	Yes	Yes	Yes	
Include unmatched respondents as non-voters	No	No	No	No	Yes	Yes	
Number of Observations	93,652	93,652	99,864	99,864	114,230	114,230	
Future Strict Voter ID State	-0.368	-0.385	-0.344	-0.356	-0.253	-0.258	
	(0.117)	(0.141)	(0.092)	(0.116)	(0.077)	(0.097)	
Black X		0.057		0.016		-0.004	
Future Strict Voter ID State		(0.134)		(0.142)		(0.122)	
Hispanic X		0.077		0.050		0.088	
Future Strict Voter ID State		(0.108)		(0.118)		(0.097)	
Asian X		0.398		0.670		0.409	
Future Strict Voter ID State		(0.505)		(0.382)		(0.348)	
Mixed Race X		-0.219		-0.263		-0.406	
Future Strict Voter ID State		(0.141)		(0.128)		(0.103)	

Assessing Cross Sectional Design: Placebo Test

- If cross sectional (selection on observables) design works:
    - States that implement voter ID laws are (conditionally) on average similar to states that do not
    - No "effect" of being a strict voter ID law in the past
- Placebo test: assess "effect" of being future strict voter ID law state on turnout before law implemented
- Hajnal, Kuk, and Lejavardi (2018) suggest placebo test using difference in differences (state and year fixed effects): not possible to estimate this placebo test.
    - Why?: no within state variation on future strict voter ID law status.
    - Coefficients from placebo test in HKL: estimated by statistical routine automatically dropping states to fit model. Strict voter ID law reported coefficient just a state fixed effect, interaction estimated solely from within state racial composition variation
    - Does not provide an assessment of the plausibility of the design

Cross Sectional ⤳ Difference in Differences

Cross Sectional Design Fails ⤳ Difference in Differences Design
HLN: "one of the most rigorous ways to examine panel data"
HKL: " we were aware of concerns related to omitted-variable bias, and we duly noted those concerns in the article. That is exactly why we sought to reconfirm our results with a state fixed effects model" [difference in differences]

Difference in Differences Estimates, HLN

Table A9: The Impact of Strict Voter ID Laws: State Fixed Effects

	(1) General Election Turnout	(2) Primary Election Turnout	(3) General Election Turnout	(4) Primary Election Turnout	(5) Primary Election Turnout
VOTER ID LAW					
Strict Voter ID Law	0.109** (0.00754)	0.0677** (0.0105)	0.100** (0.00884)	0.0309** (0.0118)	0.0108 (0.0118
Strict Voter ID * Black	-0.00497 (0.00841)	-0.0432** (0.00985)			
Strict Voter ID * Latino	-0.0446** (0.0133)	-0.0556** (0.0157)			
Strict Voter ID * Asian	0.0161 (0.0345)	-0.00137 (0.0400)			
Strict Voter ID * Mixed Race	-0.0263 (0.0223)	-0.0367 (0.0258)			
Strict Voter ID * White			0.00799 (0.00661)	0.0359** (0.00775)	
Strict Voter ID * Party ID					0.0115* (0.0013
Strict Voter ID * Ideology					
POLITICAL LEANING					

Difference in Differences Estimates, HLN

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Justin Grimmer (University of Chicago) Causal Inference April 25th, 2018 49 / 47

Estimated Effect of Voter ID Laws from HLN's Diff-in-Diff in General Elections (All Statistically Significant)

	Estimate
White	10.9
African American	10.4
Latinos	6.5
Asian Americans	12.5
Mixed Race	8.3

Results are not credible $\rightsquigarrow$ due to merge error in data

WE ARE NOT ARGUING VOTER ID LAWS INCREASE TURNOUT

What went wrong?

CCES turnout in Virginia shows 0% turnout in control period, plausible turnout levels in treatment period ⤳ "positive effect" due to merge error

Virginia Turnout, HLN Data
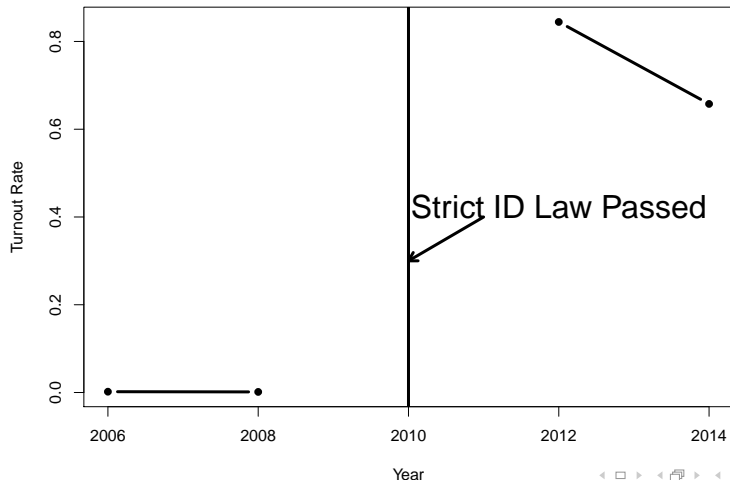
What went wrong?

To see influence of Virginia, we can drop one state at a time and assess the effect on the estimated effect of strict voter ID laws on turnout, estimated using a difference in differences design

Boxplot of Effect Estimates Shows
Virginia Exerts Substantial Influence
on Estimated Effect Voter ID Laws
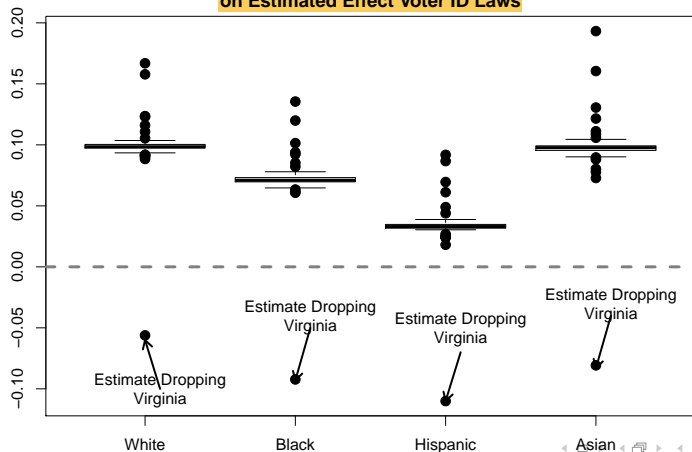
What went wrong?

This is a risk with any fixed effect regression

- Use within unit variation, average across units to calculate effect
- Major Errors in one unit $\rightsquigarrow$ exercise substantial influence over estimates
- Inspect Your Data!

Other Explanations?

- Hajnal, Kuk, and Lejavardi (2018): Argue specification is Missing Political Control Variables (Partisan control of governor, State House, and State Senate)

Other Explanations?

Originally provided political control variables had state-level missingness for states (alphabetically) from Virginia to Wyoming from 2006-2008. Figure below shows percent missing for respondents from each state for Republican governor, 2006-2008 This missingness effectively drops the problematic Virginia years from the analysis. Once corrected, political control variables do not resolve implausible positive effects

Percent Missing Data, Republican Governor HLN/HKL Variable, 2006–200
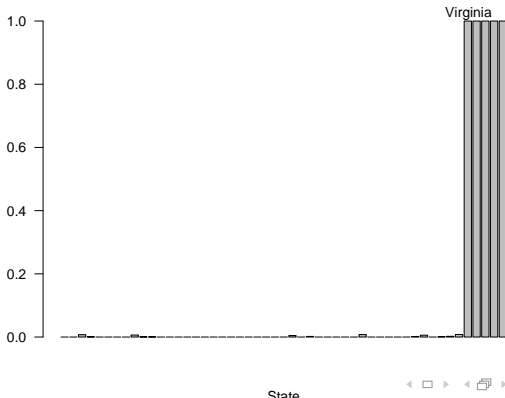
Other Explanations?

HKL (2018) also argue the model in HLN is problematic because:

- No clustering of Standard Errors
- No Survey Weights

Other Explanations?

The top estimate in each figure shows original HLN estimate of strict voter ID laws on general election turnout from difference in differences model, second is the estimate after clustering standard errors, third is estimate after including survey weights, fourth after including political controls, fifth from dropping Virginia, sixth recoding turnout so nonmatches are zero
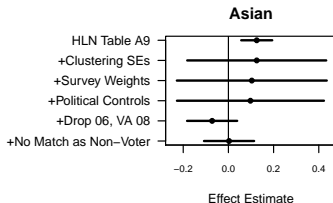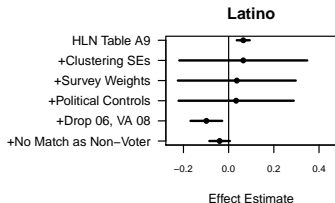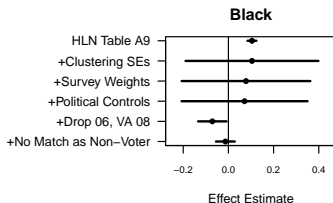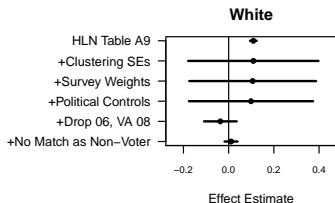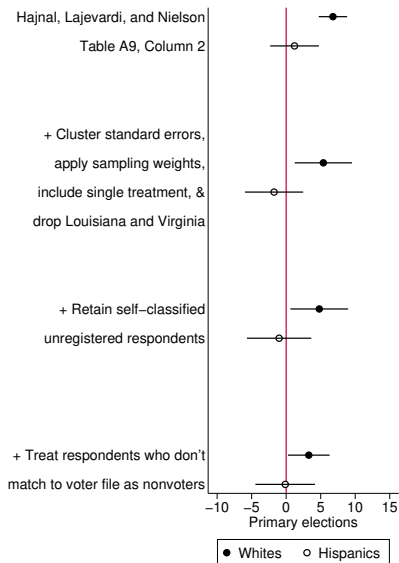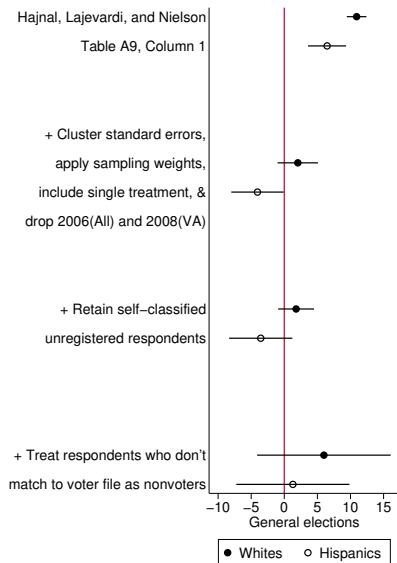
After correcting data, Survey not up to the task (Erikson and Minnite 2009)

Survey Data and Effects of Election Administration
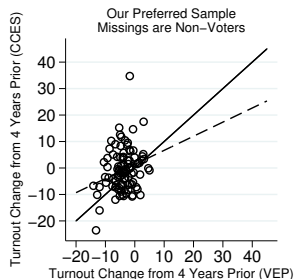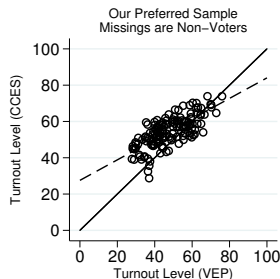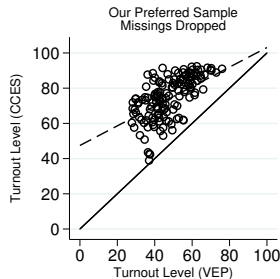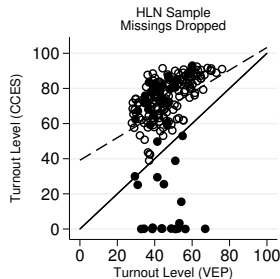
Survey Data and Effects of Election Administration

Δ turnout percentage after strict voter ID implemented

How to Assess Effect of Voter ID Laws?

- Even with large sample CCES unable to inform debate on voter ID laws because small samples in each state
- Placebo tests: useful, but caution must be used because statistical routines drop variables to enable regression to estimate, coefficients may not reflect what you think.
- Fixed effect regression, worry about unit-level errors that exercise substantial influence on estimates