# 14.661 Recitation 2: DD, SC

Andrea Manera

September 23, 2021

# Potential Outcome: the What and How of Causality

- $X_i$: treatment actually administered to a unit $i$.
  - $X_i = 1$ administered, $\forall i \in \mathrm{T}$, treatment;
  - $X_i = 0$, $\forall i \in \mathrm{C}$, control.
- $Y_{ij}$ the outcomes of some unit $i$, after receiving treatment $j$
- $T_i = (Y_i|X_i = 1) - (Y_i|X_i = 0)$: treatment effect for unit $i$
- Two objects of interest:

$$ATT \equiv E_{i \in \mathrm{T}}\left[Y_{i1} - Y_{i0}\right]$$

$$\begin{aligned} ATE &\equiv E_{i \in \mathrm{pop}}\left[Y_{i1} - Y_{i0}\right] \\ &= Pr\left\{i \in \mathrm{C}\right\} E_{i \in \mathrm{C}}\left[Y_{i1} - Y_{i0}\right] + Pr\left\{i \in \mathrm{T}\right\} E_{i \in \mathrm{T}}\left[Y_{i1} - Y_{i0}\right] \end{aligned}$$

- ATT: causal effect of intervention on treated units
- ATE: causal effect of intervention if scaled up to both treatment and control

# Selection Bias

- Almost all papers estimate the ATT as:

$$\begin{aligned}\hat{ATT} &= E_{i\in \mathrm{T}}[Y_{i1}] - E_{i\in \mathrm{C}}[Y_{i0}] \\ &= E_{i\in \mathrm{T}}[Y_{i1}] \pm E_{i\in \mathrm{T}}[Y_{i0}] - E_{i\in \mathrm{C}}[Y_{i0}] \\ &= ATT + E_{i\in \mathrm{T}}[Y_{i0}] - E_{i\in \mathrm{C}}[Y_{i0}]\end{aligned}$$

- The rightmost term is selection bias, difference between treatment and control *in the absence of treatment* (counterfactual!)
- $E_{i\in \mathrm{T}}[Y_{i0}] - E_{i\in \mathrm{C}}[Y_{i0}]$ is treatment and control balance in the counterfactual world where $T$ are not treated
- Literally *almost every empirical issue is about selection bias!*
- Caveat: Even if $\hat{ATT} = ATT$, it might be that $ATE \neq ATT$, since the latter requires:

$$E_{i\in \mathrm{T}}[Y_{i1}] - E_{i\in \mathrm{C}}[Y_{i1}]!$$

## Example: Parallel Trends

- In Rubin's Notation diff-in-diff has:

$$\begin{aligned} A\hat{T}T &= E_{i\in \mathrm{T}}\left[\Delta Y_{i1}\right] - E_{i\in \mathrm{C}}\left[\Delta Y_{i0}\right] \\ &= ATT + E_{i\in \mathrm{T}}\left[\Delta Y_{i0}\right] - E_{i\in \mathrm{C}}\left[\Delta Y_{i0}\right] \end{aligned}$$

- Selection bias is now called "Parallel Trends"

# Many States, Treated Variably

- Card (1992) makes the *federal min* into a DD experiment using an equation like

$$y_{ist} = \gamma_s + \lambda_t + \delta(\mathrm{fa}_s \cdot d_t) + \varepsilon_{ist}, \quad (1)$$

where $\mathrm{fa}_s$ is *fraction affected* in each state (pre-increase proportion of teen labor force earning $< 3.80\$$) and $d_t$ is a dummy for observations in 1990, after increase.

- Card (1992) used two periods, before and after and 51 states
- Double-Diff: $\gamma_s$ differences across states, $\lambda_t$ across periods
- Two periods: levels w/fixed (state) effects = first differences:

$$\Delta \bar{y}_s = \lambda^* + \delta \mathrm{fa}_s + \Delta \bar{\varepsilon}_s, \quad (2)$$

where $\Delta \bar{y}_s$ is the change in teen employment in state $s$ and $\Delta \bar{\varepsilon}_s$ is the differenced error

# "Event Studies": Design and Diagnostics

$$y_{st} = \gamma_s + \lambda_t + \sum_{\tau=-T_{\text{pre}},\tau\neq 1}^{T_{\text{post}}} \delta_\tau d_{s,t,\tau} + X'_{st}\beta + \varepsilon_{st},$$

$$d_{s,t,\tau} \equiv 1\{s \text{ received treatment } \tau \text{ periods ago}\}$$

- If $\tau$ is negative, $\delta_\tau$ gives the *pre-trend*, or *anticipatory effects*. If significant, trouble for parallel trends!
- Usually omit $\tau = -1$, normalize by period just before treatment.
- Beware: if treatment period is not the same for all treated units, $\delta_\tau$, $\tau < 0$ are spurious
- Use Sun and Abraham (2020): Interaction-weighted estimator!
- Unit/covariate time trends (3 periods min)
- Randomization/exact p-values

# Nice Graphs!

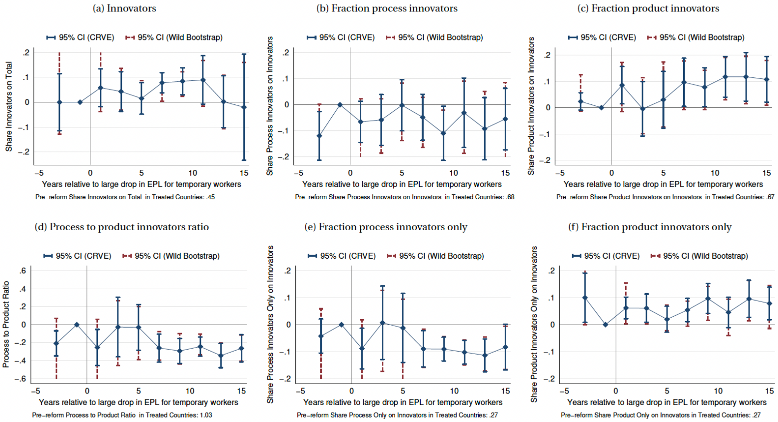
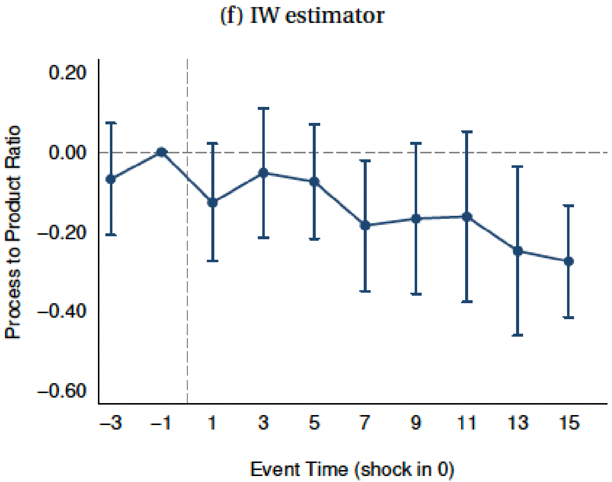


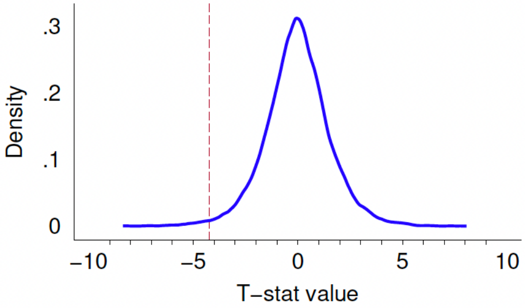

Figure 3: Main Results: effect of large EPL drop on innovators and product/process innovation

# Interaction-Weighted



# Randomization

(c) Total randomization




T–stat est.: –4.225. Rand. p–value: 0.016.
Coeff. est.: –0.251. Rand. Avg.: –0.0014.

Figure 5: Process on product ratio: Permutation tests

# Synthetic Control (Abadie et al., JEL 2021)

Abadie et al. (2003, 2010):

- Control group is called "donor pool"
- Covariates are called "predictors"
- Synthetic controls use a weighted average of comparison units to match lagged predictors:
    - Idea: if you are similar on observables you are also on unobservables
    - Not necessarily, but often, matches also pre-treatment outcomes

# Notation In Abadie Case

- The data consist of observations on regions $i$ at time $t$ for $i = 1, \ldots, J+1$, and $t = 1, \ldots, T$, where $1 \leq T_0 < T$ is the intervention date
- $\alpha_{it} = Y_{it}^I - Y_{it}^N$ is the effect of the intervention for unit $i$ at time $t > T_0$. The first unit is treated and the aim is to estimate $(\alpha_{1T_0+1}, \ldots, \alpha_{1T})$. For $t > T_0$,

$$\alpha_{1t} = Y_{1t}^I - Y_{1t}^N = Y_{1t} - Y_{1t}^N.$$

  $Y_{1t}^I$ is observed in post-intervention periods. *Counterfactual* $Y_{1t}^N$:

$$Y_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt}^N,$$

- Weighted average of untreated units, effectively a vastly more general DD!

# Choice of SC

The *Synthetic Control* is defined as the vector of weights $\boldsymbol{W}$. How to choose it?

Abadie, Diamond, and Hainmuller (2010):

- Choose a set of covariates ("predictors" for the dep. variable), $h \in \mathcal{H}$.
- Choose a set of importance weights $v_h$
- Synthetic control solves:

$$\boldsymbol{W}(\boldsymbol{V}) = \arg\min_{\boldsymbol{w} \geq 0} \left( \sum_{h \in \mathcal{H}} v_h \left[ X_{1,h} - \sum_{j=2}^{J+1} w_j X_{j,h} \right]^2 \right)^{\frac{1}{2}}$$

- But how to choose $v_h$?

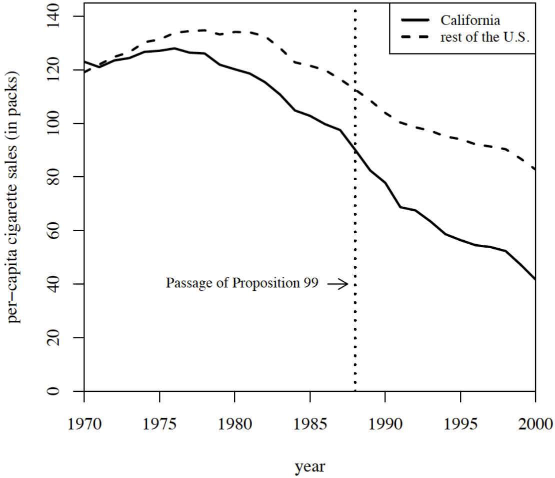
## Choosing weights

- Choose $\boldsymbol{V}$ such that:

$$\boldsymbol{V} = \arg\min_{\boldsymbol{v}} \mathrm{MSPE}(\boldsymbol{v}) \equiv \sum_{t<T_0} \left( Y_{1t} - \sum_{j=2}^{J+1} w_j(\boldsymbol{v}) Y_{jt} \right)^2$$

- Minimizes the mean square prediction error (MSPE) of outcome in the pre-period.
- A more sophisticated way is using *out-of-sample validation* (see JEL and Abadie et al, 2015).
- Final result:
    - Synthetic control is a set of nonnegative weights, which you can report in a table
    - Also regression creates weights for each observation, but nobody sees them! (Cunningham, 2021)
    - Estimator bias decreases with length of pre-period.

# All Data



California
rest of the U.S.
Passage of Proposition 99
per−capita cigarette sales (in packs)
year
0
20
40
60
80
100
120
140
1970
1975
1980
1985
1990
1995
2000

# Matching Table

Table 1: Cigarette Sales Predictor Means

| Variables | California Real | California Synthetic | Average of 38 control states |
|---|---|---|---|
| Ln(GDP per capita) | 10.08 | 9.86 | 9.86 |
| Percent aged 15-24 | 17.40 | 17.40 | 17.29 |
| Retail price | 89.42 | 89.41 | 87.27 |
| Beer consumption per capita | 24.28 | 24.20 | 23.75 |
| Cigarette sales per capita 1988 | 90.10 | 91.62 | 114.20 |
| Cigarette sales per capita 1980 | 120.20 | 120.43 | 136.58 |
| Cigarette sales per capita 1975 | 127.10 | 126.99 | 132.81 |

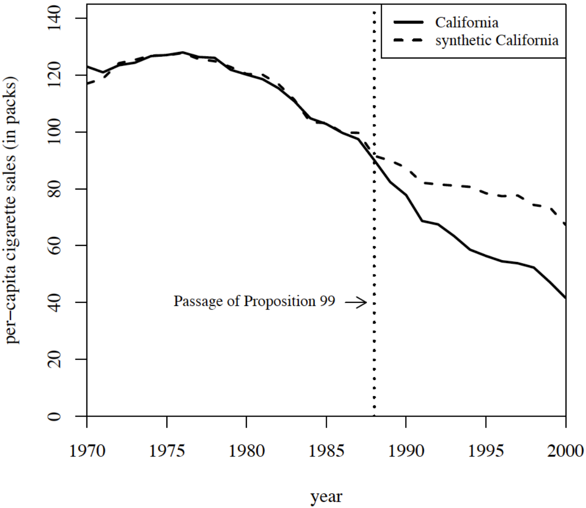
*Note:* All variables except lagged cigarette sales are averaged for the 1980-1988 period (beer consumption is averaged 1984-1988). Cigarette sales are measured in packs.

# Matching Weights (?)

Table 2: State Weights in the Synthetic California

| State | Weight | State | Weight |
|---|---|---|---|
| Alabama | 0 | Montana | 0.199 |
| Alaska | - | Nebraska | 0 |
| Arizona | - | Nevada | 0.234 |
| Arkansas | 0 | New Hampshire | 0 |
| Colorado | 0.164 | New Jersey | - |
| Connecticut | 0.069 | New Mexico | 0 |
| Delaware | 0 | New York | - |
| District of Columbia | - | North Carolina | 0 |
| Florida | - | North Dakota | 0 |
| Georgia | 0 | Ohio | 0 |
| Hawaii | - | Oklahoma | 0 |
| Idaho | 0 | Oregon | - |
| Illinois | 0 | Pennsylvania | 0 |
| Indiana | 0 | Rhode Island | 0 |
| Iowa | 0 | South Carolina | 0 |
| Kansas | 0 | South Dakota | 0 |
| Kentucky | 0 | Tennessee | 0 |
| Louisiana | 0 | Texas | 0 |
| Maine | 0 | Utah | 0.334 |
| Maryland | - | Vermont | 0 |
| Massachusetts | - | Virginia | 0 |
| Michigan | - | Washington | - |
| Minnesota | 0 | West Virginia | 0 |
| Mississippi | 0 | Wisconsin | 0 |
| Missouri | 0 | Wyoming | 0 |

# Result


California
synthetic California
Passage of Proposition 99
per−capita cigarette sales (in packs)
0
20
40
60
80
100
120
140
1970
1975
1980
1985
1990
1995
2000
year

# What About Inference? Randomize!

- Empirical CDF of *real* treatment post-outcome relative to *random* treatment ("*exact p-value*")
- Abadie et al. (2010) compute a synthetic control $W_j$ for each unit in the sample
- Then compute:

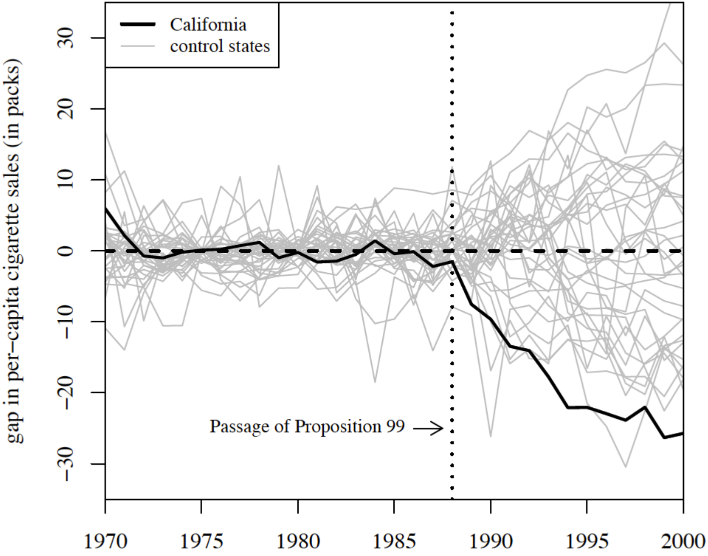
$$\text{RMSPE}_{j,t_1,t_2} = \left( \frac{1}{t_2 - t_1 + 1} \sum_{t \in [t_1, t_2]} \left( Y_{1t} - \sum_{j=2}^{J+1} w_j(\upsilon) Y_{jt} \right)^2 \right)^{\frac{1}{2}} .$$

- Compute the ratio of RMSPE post versus pre:

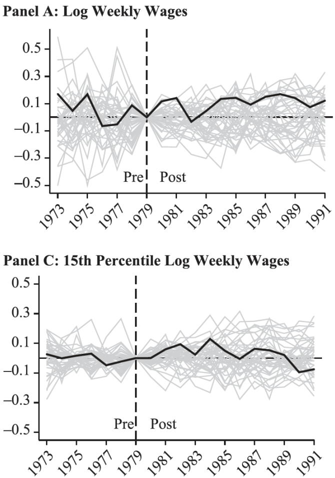
$$r_j = \frac{\text{RMSPE}_{j,T_0+1,T}}{\text{RMSPE}_{j,1,T_0}}$$

- Show the *permutation distribution* of $r_j$ or compute the p-value as the empirical inverse CDF of $r_j$

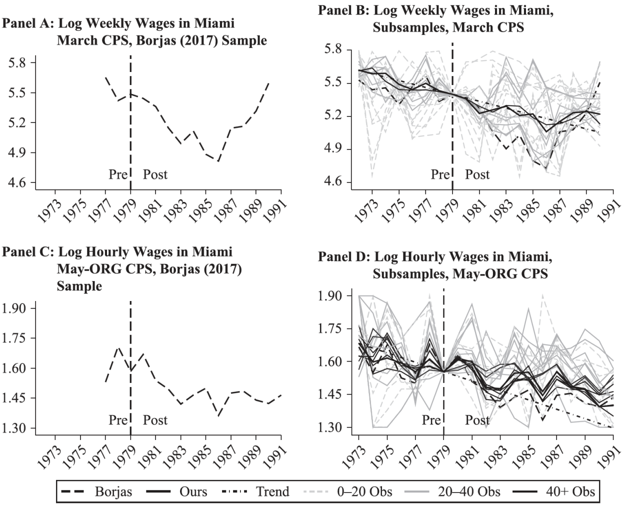
# Show all Placebo Gaps, $Y_{j,t} - \hat{Y}^N_{j,t}$



# Last Words on Mariel? Peri and Yasenov (2018)



# Peri and Yasenov v. Borjas



# Peri and Yasenov vs. Josh and Krueger