Module 9: Panel Data

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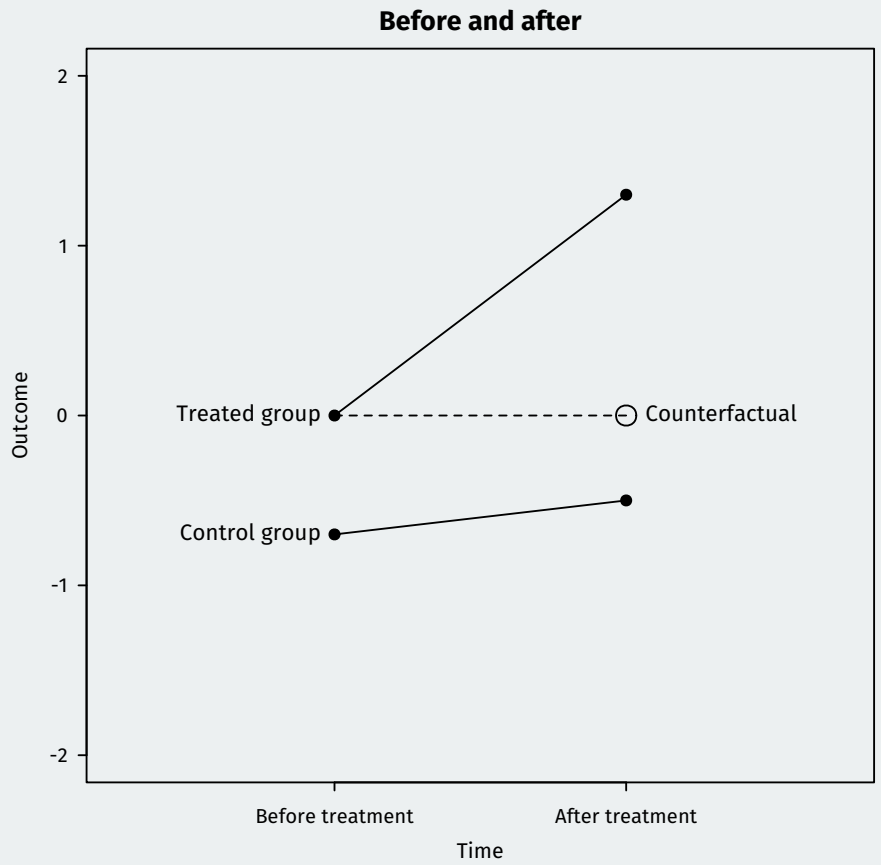
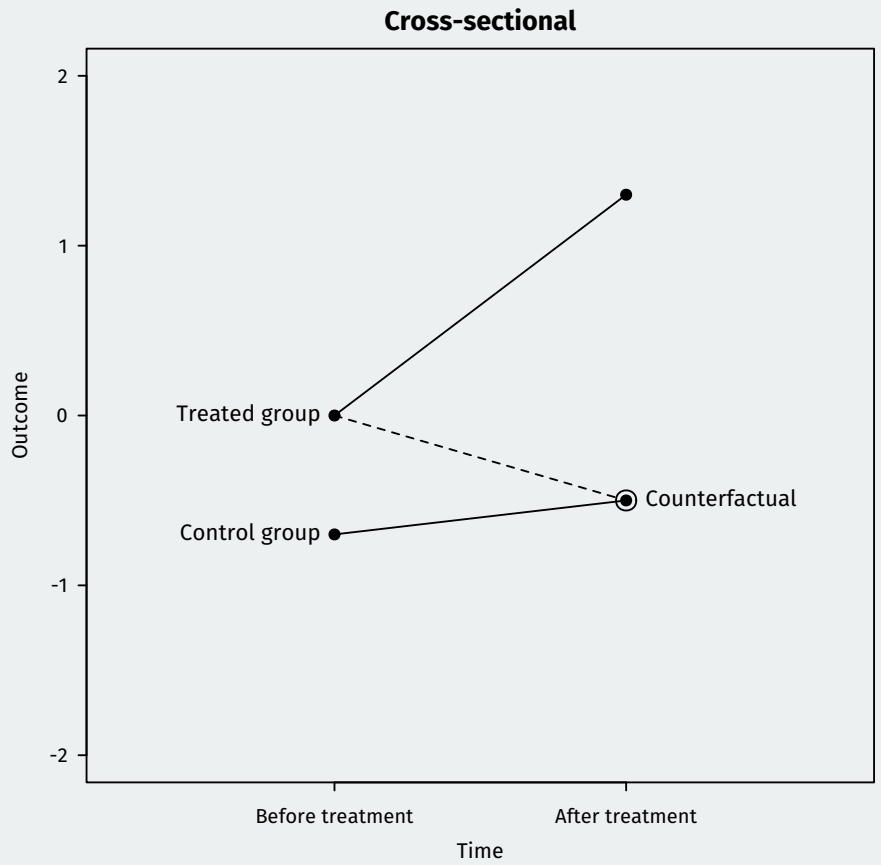
Gov 2003 (Harvard)

Where are we? Where are we going?

- Where we have found good controls:
    - Units randomized to receive control
    - Units with similar values of covariates
    - Units with opposite value of some instrument
    - At a discontinuity in treatment assignment
- What if we have repeated measurements of the same units?
- Now there are two possible sources of variation to exploit:
    - Exploit **cross-sectional** variation in treatment.
    - Exploit variation in treatment **within a unit over time** (before/after)

Cross-sectional vs before/after





1/ Difference in differences

Minimum wages (Card & Krueger, 1994)

- Does increasing the minimum wage affect employment?
- Classical economic theory tends to point to negative effects.
- But difficult to randomize changes to the minimum wage.
- In 1992, NJ minimum wage increased from $4.25 to $5.05
    - Neighboring PA stays at $4.25
    - We observe employment in both states before and after increase
- Look at eastern PA and NJ fast food restaurants.
    - Similar prices, wages, products, etc.
    - Most likely to be affected by the change.

Differences-in-differences design

- Basic setup: two groups, two time periods.
  - Pre-period ($t = 0$): neither group is treated.
  - Post-period ($t = 1$): one group is treated, other remains untreated.
- Groups defined by treatment status in post-period:
  - $G_i = 1$ are those that are treated at $t = 1$
  - $G_i = 0$ for those that are always untreated
- Treatment status in each period:
  - No treatment in the first period for either group: $D_{i0} = 0$
  - In treated group, $G_i = 1 \rightsquigarrow D_{i1} = 1$
  - In control group, $G_i = 0 \rightsquigarrow D_{i1} = 0$

	Time period	
	Pre-period $(t = 0)$	Post-period $(t = 1)$
Control group $(G_i = 0)$	$D_{i0} = 0$	$D_{i1} = 0$
Treated group $(G_i = 1)$	$D_{i0} = 0$	$D_{i1} = 1$

Potential outcomes approach to DID

- $Y_{it}(d)$ is the potential outcome under treatment d at time t.
- Again, the individual causal effect is just $Y_{it}(1) - Y_{it}(0)$.
- **Consistency**: $Y_{it} = D_{it} Y_{it}(1) + (1 - D_{it}) Y_{it}(0)$
    - Observe control p.o. for all units in first period: $Y_{i0}(0) = Y_{i0}$
    - In treated group: $G_i = 1 \rightsquigarrow Y_{i1} = Y_{i1}(1)$
    - In control group: $G_i = 0 \rightsquigarrow Y_{i1} = Y_{i1}(0)$

Identification problem

- Average treatment effect on the treated:

$$\begin{aligned}\tau_{ATT} &= \mathbb{E}[Y_{i1}(1) - Y_{i1}(0)|G_i = 1] \\ &= \mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 1] \\ &= \underbrace{\mathbb{E}[Y_{i1}|G_i = 1]}_{\text{(a)}} - \underbrace{\mathbb{E}[Y_{i1}(0)|G_i = 1]}_{\text{(b)}}\end{aligned}$$

- Part (a) is just a conditional average of observed data $\rightsquigarrow$ identified.
- Part (b) is a counterfactual: what would the average outcome in the treated group have been if it have been in control?

Three control strategies

$$\tau_{ATT} = \mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 1]$$

	Time period	
	Pre-period $(t = 0)$	Post-period $(t = 1)$
Control group $(G_i = 0)$	$\mathbb{E}[Y_{i0}(0)\|G_i = 0]$	$\mathbb{E}[Y_{i1}(0)\|G_i = 0]$
Treated group $(G_i = 1)$	$\mathbb{E}[Y_{i0}(0)\|G_i = 1]$	$\mathbb{E}[Y_{i1}(1)\|G_i = 1]$

- **Cross-sectional design**
  - Assumption: mean independence of treatment

$$\mathbb{E}[Y_{i1}(0)|G_i = 1] = \mathbb{E}[Y_{i1}(0)|G_i = 0]$$

  - Use post-treatment control group:

$$\tau_{ATT} = \mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i1}|G_i = 0]$$

Three control strategies

$$\tau_{ATT} = \mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 1]$$

	Time period	
	Pre-period $(t = 0)$	Post-period $(t = 1)$
Control group $(G_i = 0)$	$\mathbb{E}[Y_{i0}(0)\|G_i = 0]$	$\mathbb{E}[Y_{i1}(0)\|G_i = 0]$
Treated group $(G_i = 1)$	$\mathbb{E}[Y_{i0}(0)\|G_i = 1]$	$\mathbb{E}[Y_{i1}(1)\|G_i = 1]$

- **Before-and-after design**
  - Assumption: no trends

$$\mathbb{E}[Y_{i1}(0)|G_i = 1] = \mathbb{E}[Y_{i0}(0)|G_i = 1]$$

  - Use pre-period outcome in treated group:

$$\tau_{ATT} = \mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i0}|G_i = 1]$$

Three control strategies

$$\tau_{ATT} = \mathbb{E}[Y_{i1}(1)|G_i = 1] - \mathbb{E}[Y_{i1}(0)|G_i = 1]$$

	Time period	
	Pre-period $(t = 0)$	Post-period $(t = 1)$
Control group $(G_i = 0)$	$\mathbb{E}[Y_{i0}(0)\|G_i = 0]$	$\mathbb{E}[Y_{i1}(0)\|G_i = 0]$
Treated group $(G_i = 1)$	$\mathbb{E}[Y_{i0}(0)\|G_i = 1]$	$\mathbb{E}[Y_{i1}(1)\|G_i = 1]$

- **Difference-in-differences**:
  - Assumption: parallel trends

$$\mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 0] = \mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 1]$$

  - Use pre-period treated outcome plus trend in control group:

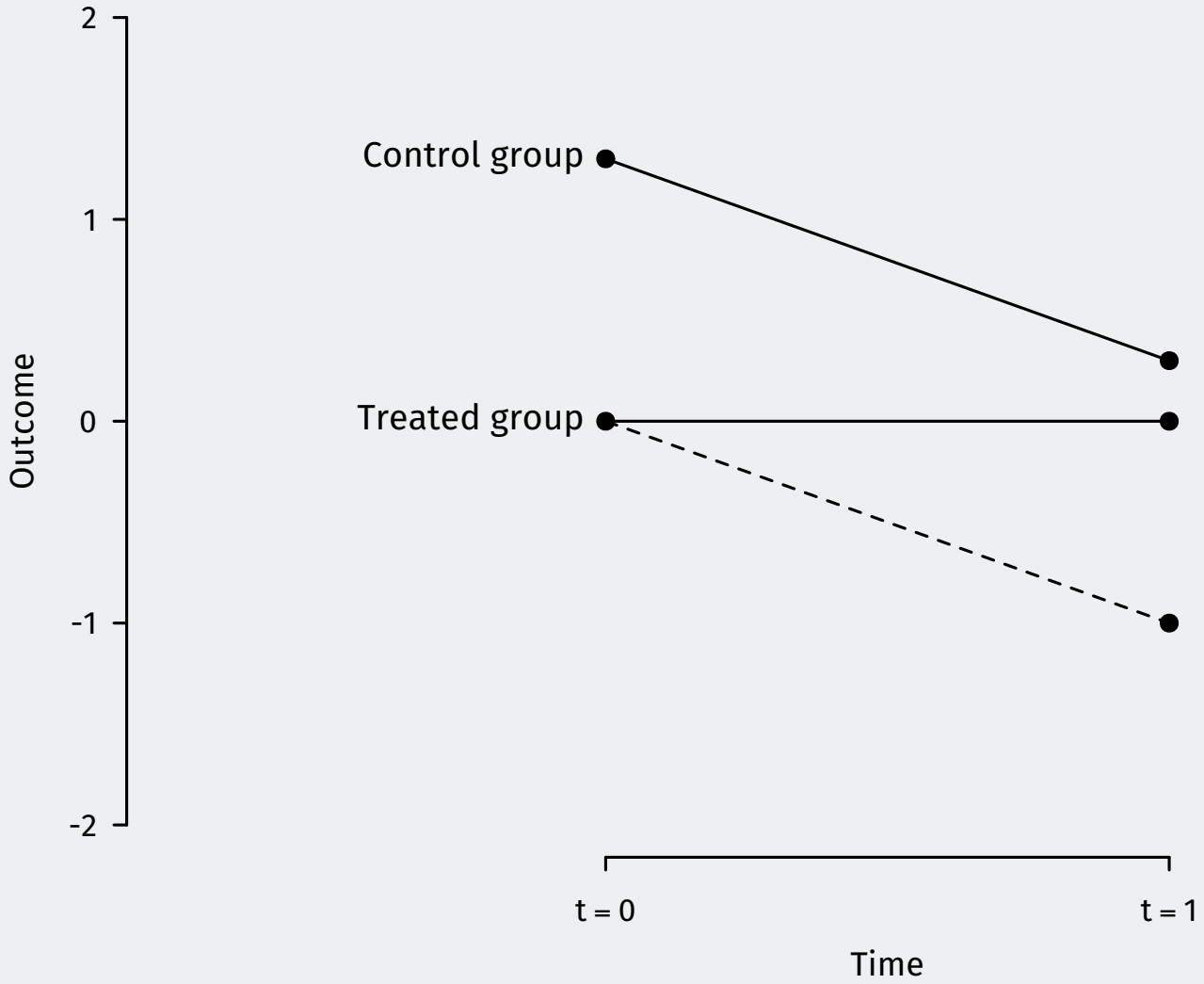
$$\begin{aligned}\tau_{ATT} =& (\mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i0}|G_i = 1]) \\ &- (\mathbb{E}[Y_{i1}|G_i = 0] - \mathbb{E}[Y_{i0}|G_i = 0])\end{aligned}$$

Parallel trends

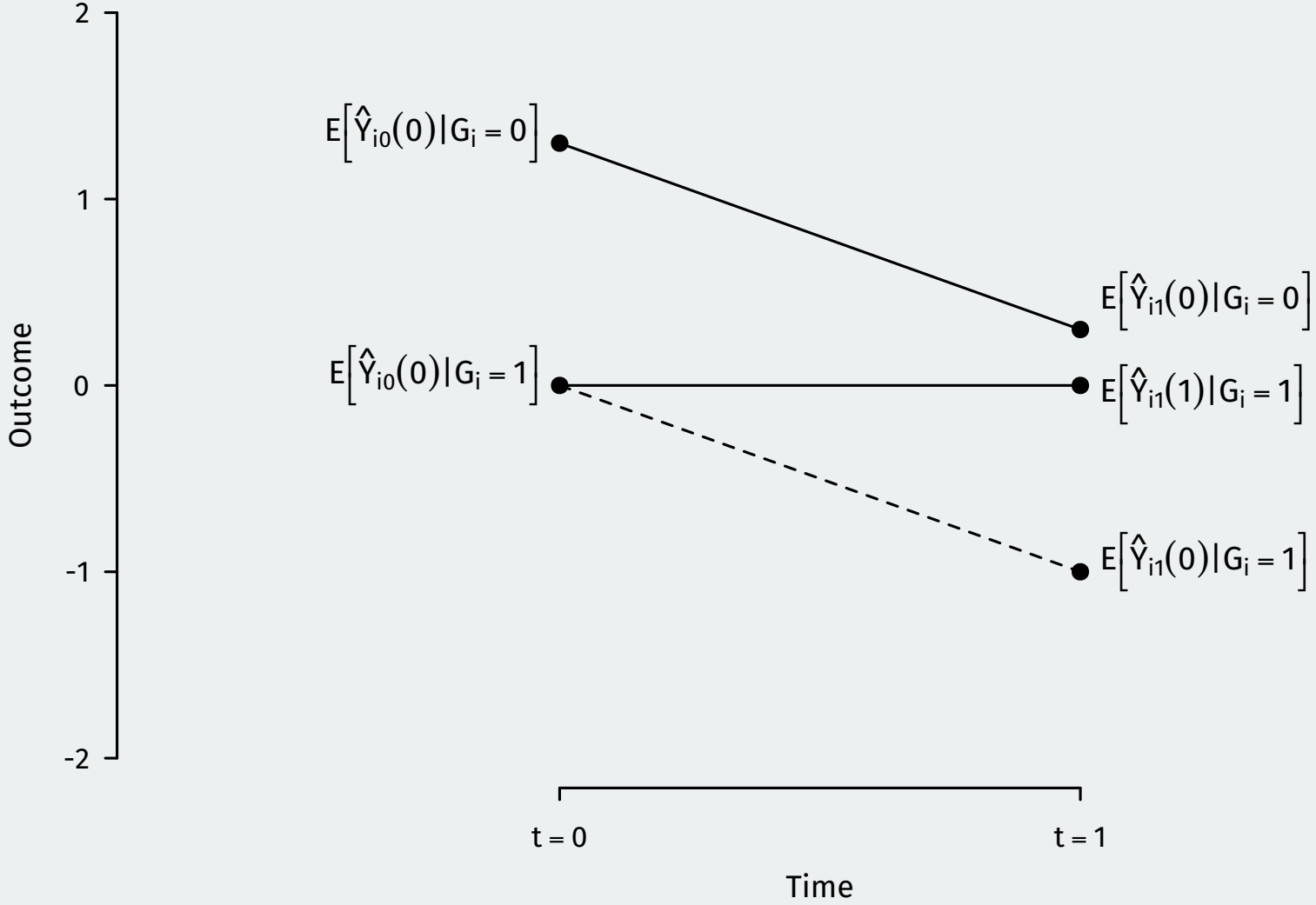
$$\mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 0] = \mathbb{E}[Y_{i1}(0) - Y_{i0}(0)|G_i = 1]$$

- Key assumption of differences-in-differences: **parallel trends**
- Interpretation:
  - Secular trend in the control group is a good proxy how the treated group would have changed over time without treatment.
- Why is this weaker than other assumption?
  - Allows for time-constant unmeasured confounding between Y_{it} and G_i
  - Allows for (common) secular trends in the outcome over time (unlike FE).
- Not invariant to nonlinear transformations!
  - Parallel trends for Y_{it} implies non-parallel trends for $\log(Y_{it})$ and vice versa.

Parallel trends in a graph



Parallel trends in a graph



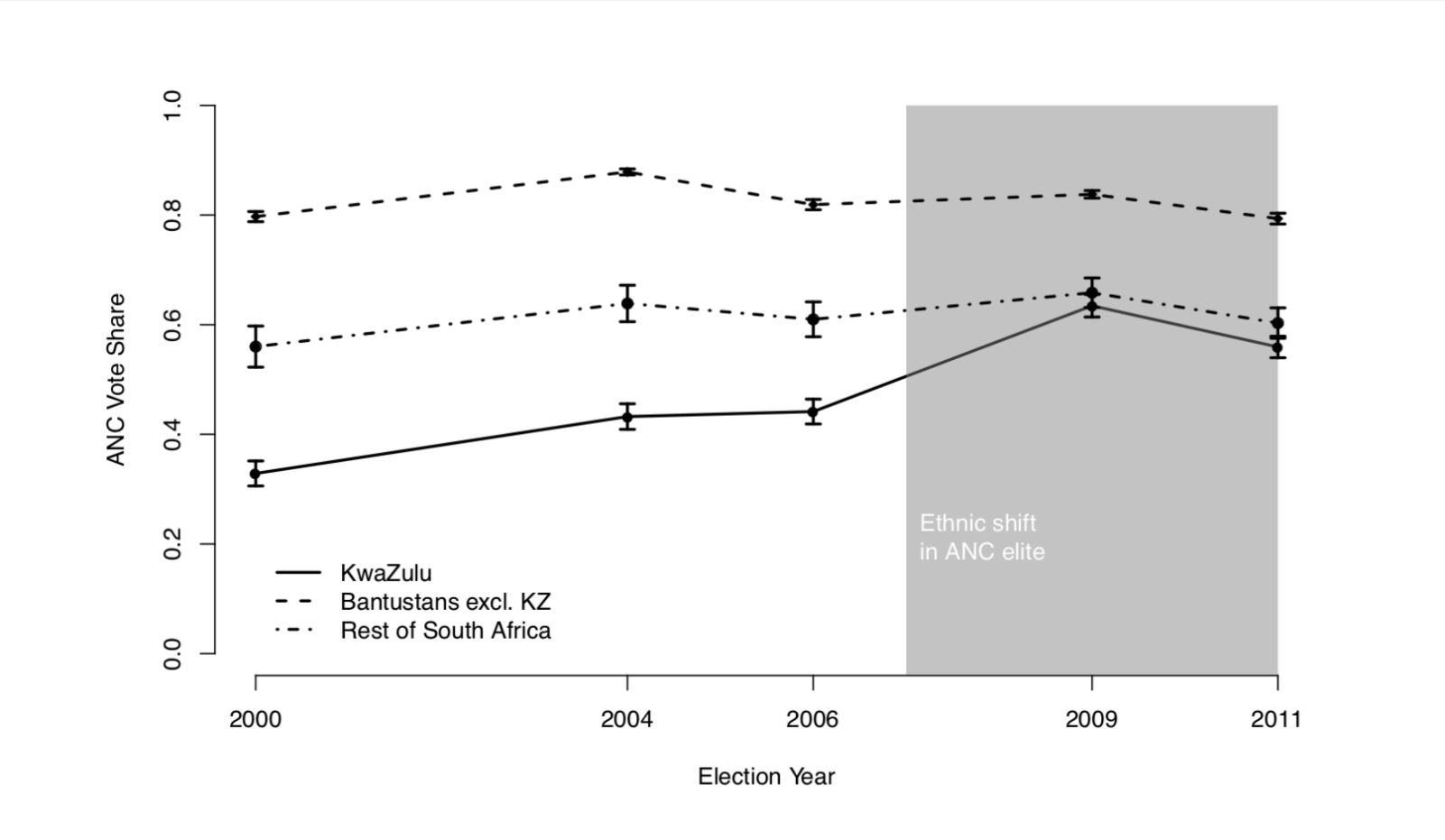
Identification

- Identification result:

$$\begin{aligned}\tau_{ATT} = &(\mathbb{E}[Y_{i1}|G_i = 1] - \mathbb{E}[Y_{i0}|G_i = 1]) \\ &- (\mathbb{E}[Y_{i1}|G_i = 0] - \mathbb{E}[Y_{i0}|G_i = 0])\end{aligned}$$

- Threat to identification: non-parallel trends
  - **unmeasured time-varying confounding**
  - **Ashenfelter's dip**: empirical finding that people who enroll in job training programs see their earnings decline prior to that training.
- Falsification test: check pre-treatment parallel trends.
  - Doesn't imply parallel trends hold for the post-period however!

Checking parallel trends (de Kadt/Larreguy, 2018)



Estimation

- Estimation with panel data:

$$\widehat{\tau}_{\text{ATT}} = \underbrace{\frac{1}{n_1}\sum_{i=1}^{n} G_i \{Y_{i1} - Y_{i0}\}}_{\text{average trend in treated group}} - \underbrace{\frac{1}{n_0}\sum_{i=1}^{n}(1 - G_i)\{Y_{i1} - Y_{i0}\}}_{\text{average trend in the control group}}$$

  - Standard errors from standard difference in means.

- Regression implementation:

  - Regress $\Delta Y_i = Y_{i1} - Y_{i0}$ on G_i.
  - Use (cluster) robust SEs

- Also possible to use DID on repeated cross sections.

DID and linear two-way fixed effects

- Linear two-way (group and time) fixed effect model:

$$Y_{it} = \alpha + \gamma G_i + \beta t + \tau D_{it} + \varepsilon_{it}$$

  - Fixed effect for group and time.
  - Be sure to cluster by unit (or level of treatment assignment)

- Coefficient on D_{it} equivalent to DID estimation.

- Only holds for the 2 group, 2 period case!

  - Large new literature on interpretation of TWFE in more general cases.
  - Basically, TWFE is an odd weighted average of DID effects with sometimes negative weights.

DID vs lagged dependent variable

- Alternative identification assumption:

$$Y_{i1}(0) \perp\!\!\!\perp G_i \mid Y_{i0}$$

  - Doesn't imply and isn't implied by parallel trends.
  - Benefit over parallel trends: it is scale-free.
  - Equivalent to parallel trends if $\mathbb{E}[Y_{i0} \mid G_i = 1] = \mathbb{E}[Y_{i0} \mid G_i = 0]$

- Different ideas about why there is imbalance on the LDV:

  - DID: time-constant unmeasured confounder creates imbalance.
  - LDV: previous outcome directly affects treatment assignment.

DID/LDV bracketing

- Estimator: estimate CEF $\mathbb{E}[Y_{i1} \mid Y_{i0}, G_i] = \alpha + \rho Y_{i0} + \tau G_i$

$$\widehat{\tau}_{LDV} = \underbrace{\frac{1}{n_1}\sum_{i=1}^{n} G_i Y_{i1} - \frac{1}{n_0}\sum_{i=1}^{n}(1-G_i)Y_{i1}}_{\text{difference in post period}}$$
$$- \hat{\rho}_{LDV} \underbrace{\left\{ \frac{1}{n_1}\sum_{i=1}^{n} G_i Y_{i0} - \frac{1}{n_0}\sum_{i=1}^{n}(1-G_i)Y_{i0} \right\}}_{\text{difference in pre period}}$$

- If $\hat{\rho}_{LDV} = 1$ then $\widehat{\tau}_{DID} = \widehat{\tau}_{LDV}$ and if $0 \leq \hat{\rho}_{LDV} < 1$:
  - If $G_i = 1$ has higher baseline outcomes $\rightsquigarrow \widehat{\tau}_{LDV} > \widehat{\tau}_{DID}$.
  - If $G_i = 1$ has lower baseline outcomes $\rightsquigarrow \widehat{\tau}_{DID} > \widehat{\tau}_{LDV}$.
- Bracketing relationship: if you willing to assume parallel trends or LDV,

$$\mathbb{E}[\widehat{\tau}_{LDV}] \geq \tau_{\text{att}} \geq \mathbb{E}[\widehat{\tau}_{DID}]$$

  - Holds nonparametrically as well.

Nonparametric identification

- Up until now, we assumed unconditional parallel trends. What if this doesn't hold?
- Alternative identification: **conditional parallel trends**

$$E[Y_{i1}(0) - Y_{i0}(0) \mid \mathbf{X}_i, G_i = 1] = E[Y_{i1}(0) - Y_{i0}(0) \mid \mathbf{X}_i, G_i = 0]$$

- What does this assumption say? It says that the potential trend under control is the same for the control and treated groups, conditional on covariates.
    - Units that are similar at baseline will follow similar paths under no treatment.
- **Matching**: conduct DID analysis on units with similar values of $\mathbf{X}_i$

Semiparametric estimation with repeated outcomes

- How to estimate regression DID without strong linearity assumptions?
- Abadie (2005) derives **weighting estimators** in this setting:

$$\mathbb{E}[Y_{i1}(1) - Y_{i1}(0) \mid G_i = 1] = \mathbb{E}\left[\frac{(Y_{i1} - Y_{i0})}{\mathbb{P}(G_i = 1)} \cdot \frac{G_i - \mathbb{P}(G_i = 1 \mid \mathbf{X}_i)}{1 - \mathbb{P}(G_i = 1 \mid \mathbf{X}_i)}\right]$$

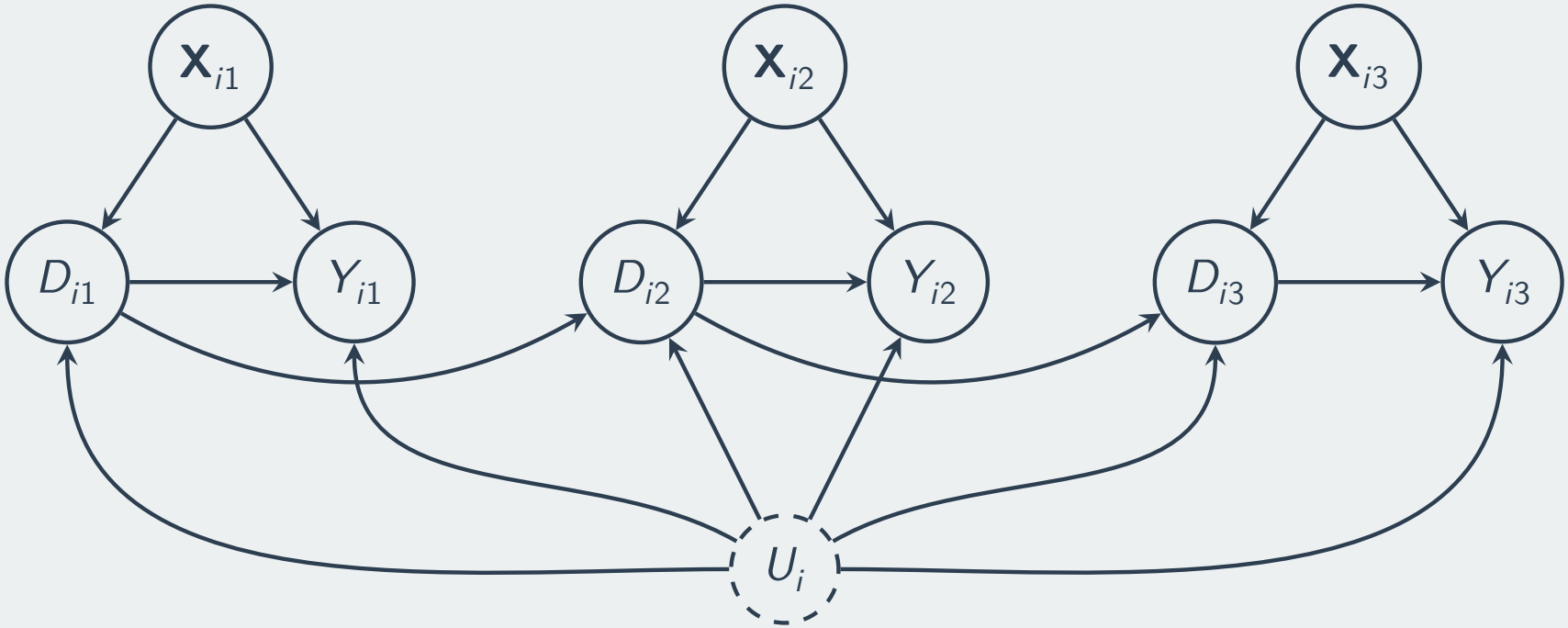
- Reweights control group to have the same distribution of $\mathbf{X}_i$ as treated group.
- Have to estimate the **propensity score** $\mathbb{P}(G_i = 1 \mid \mathbf{X}_i)$
    - Possible model misspecification!

2/ Fixed effects

Basic idea of fixed effects

- "One way" fixed effects generalizes the before/after design.
  - Arbitrary treatment timing, covariates, etc.
  - Units: $i = 1, \ldots, n$
  - Causal ordering with time: covariates $\mathbf{X}_{it}$, treatment D_{it}, outcome Y_{it}
  - History of a variable: $\overline{D}_{it} = (D_{i1}, \ldots, D_{it})$ and $\overline{D}_i \equiv \overline{D}_{iT}$
- Linear fixed effects model: $Y_{it} = \alpha_i + \tau D_{it} + \mathbf{X}_i'\beta + \varepsilon_{it}$
  - Key assumption: **strict exogeneity** $\mathbb{E}[\varepsilon_{it} \mid \overline{\mathbf{X}}_i, \overline{D}_i, \alpha_i] = 0$
  - Implies **no feedback** between outcome and treatment ($Y_{it} \not\mapsto D_{i,t+1}$)
  - $\rightsquigarrow$ LDV cannot be a confounder!
  - Imai and Kim (2019, AJPS) give clarification on these identification issues.
- Implicit assumption of **no carryover**? $Y_{it}(d_1, \ldots, d_t) = Y_{it}(d_t)$
  - More a choice of estimand: focuses on **contemporaneous** effect.
  - Treatment history follows observed path through $t-1$: $Y_{it}(d_t) = Y_{it}(D_{i1}, \ldots, D_{i,t-1}, d_t)$
  - $\rightsquigarrow$ lags of treatments become part of time-varying confounders.

Strict exogeneity DAG



Strict exogeneity implied by strict ignorability $Y_{it}(d) \perp\!\!\!\perp \overline{D}_i \mid \overline{X}_i, U_i$

FE estimation

- With linear models, two transformations can purge the fixed effects.
- **Within/FE transformation**: $\ddot{Z}_{it} = Z_{it} - T^{-1} \sum_{s=1}^{T} Z_{is}$

$$\ddot{Y}_{it} = \ddot{X}'_{it}\beta + \tau \ddot{D}_{it} + \ddot{\varepsilon}_{it}$$

  - Time-demeaning Y_{it} purges the time constant fixed effect.
  - But they retain the same coefficients as the original model.

- **First differences**: $\Delta Z_{it} = Z_{it} - Z_{i,t-1}$

$$\Delta Y_{it} = \Delta \mathbf{X}'_{it}\beta + \tau \Delta D_{it} + \Delta \varepsilon_{it}$$

- Estimation: pooled OLS of either specification, $\widehat{\tau}_{\text{fe}}$, $\widehat{\tau}_{\text{fd}}$
  - Both consistent under strict exogeneity.
  - FE more efficient if original errors, ε_{it}, are serially uncorrelated.
  - FD more efficient if differences, $\Delta\varepsilon_{it}$, are serially uncorrelated.
  - Latter allows for substantial serial dependence in the original errors.

Estimation notes

- Within estimator can be implemented by adding unit dummy variables.

$$\underset{\alpha,\beta,\tau,\gamma}{\arg\max} \sum_{i=1}^{n}\sum_{t=1}^{T}\left(Y_{it} - \alpha - \mathbf{X}_{it}'\beta - \tau D_{it} - \sum_{k=2}^{n}\gamma_k \mathbb{1}(i=k)\right)^2$$

  - **Least squares dummy variable** estimator reasonable for moderate n
  - Computationally inefficient for large n (number of dummies grows with n)

- Best practice: cluster variances at the unit level.

  - With CR variance estimators, LSDV "double counts" degrees of freedom
  - Better to use within estimator in that case.

- Best choice: use canned packages.

  - `{fixest}` in R, `-reghdfe-` in Stata

Non-constant treatment effects

- LFE models assume constant treatment effects. What happens if not?
  - OLS typically biased because nonconstant effects induce correlation between treatment and error.
- With no covariates and no only treated/control units:

$$\widehat{\tau}_{\text{fe}} \xrightarrow{p} \frac{\mathbb{E}\left[\left(\frac{\sum_t D_{it} Y_{it}}{\sum_t D_{it}} - \frac{\sum_t (1-D_{it}) Y_{it}}{\sum_t (1-D_{it})}\right) S_i^2\right]}{\mathbb{E}[S_i^2]} \neq \tau$$

  - S_i^2 is the within-unit treatment variance.
  - Units with even treatment/control split upweighted.
- Imai, Kim & Wang (2019, AJPS): use a matching to target the ATE.
  - Match treated and control periods within units (also weakens linearity).
  - `{PanelMatch}` R package.

Strict vs. sequential exogeneity/ignorability

- Strict exogeneity/ignorability is **very strong**.
    - Remember: rules out all outcome-treatment feedback.
- Weaker assumption: **Sequential ignorability**:

$$Y_{it}(d) \perp\!\!\!\perp D_{it} \mid \overline{\mathbf{X}}_{it}, \overline{D}_{i,t-1}, \alpha_i$$

    - Allow Y_{it} to be related to future $D_{i,t+s}$
- This implies **sequential exogeneity** of the errors: $\mathbb{E}[\varepsilon_{it} \mid \overline{\mathbf{X}}_{it}, \overline{D}_{it}, \alpha_i] = 0$.
- Estimation to these **dynamic panel models**:
    - instrumental variables (Arellano and Bond) using lagged difference and levels as instruments (only valid for linear models).
    - bias correction: estimate the bias and subtract it off (valid for nonlinear models too).

Effect of lagged treatments

- Focused on the contemporaneous effect of D_{it}.
- What about treatment histories $Y_{it}(d_{t-1}, d_t)$?
- Very difficult, if not impossible with fixed effects models.
    - Complicated by the effect of treatment on time-varying confounders.
    - Pathways involving $\mathbf{X}_{it}(d_{t-1})$ difficult to identify.
- Possible approach: **propensity score FEs** (Blackwell & Yamauchi, 2021)
    - Include unit dummies in propensity score model.
    - Bias from incidental parameters, but disappears as $T \to \infty$

3/ Synthetic control methods

Synthetic controls

- Abadie and Gardeazabal (2003) use a DID approach for "quantitative case studies."
- Application: effect of an intervention in a single country/state at one point in time.
- Basic idea: 1 treated group, many controls.
    - Compare the time-series outcomes in the treated group to the control.
    - But which control group should you use?
    - Many possible choices and they may not be comparable to the treated.
- **Synthetic control**: use a convex combination of the controls to create a synthetic control.
    - Choose the weights that minimize the pretreatment differences between treated and synthetic control.

Intervention study

	Time period						
	1	2	$\cdots$	T_0	T_0+1	$\cdots$	T
Treated unit $(i=1)$	0	0	0	0	1	1	1
Control group $(i=2,\ldots,J+1)$	0	0	0	0	0	0	0

- Treatment:
  - All units untreated for T_0 periods.
  - Unit 1 starts treatment at T_0, continues until T.
- Potential outcomes:
  - $Y_{it}(1)$: potential outcome at time t if i had been in the treated group.
  - $Y_{it}(0)$: potential outcome at time t if i had been in the control group.
  - No pre-intervention impacts: $Y_{it}(1) = Y_{it}(0)$ for all $t \leq T_0$.
- $\mathbf{X}_i$ is an $r \times 1$ vector of (pretreatment) covariates.
- Treatment effects: $\tau_{it} = Y_{it}(1) - Y_{it}(0)$
- Goal: estimate $\left(\tau_{1,T_0+1}, \ldots, \tau_{1,T}\right)$.

Missing counterfactuals

- By consistency, for $t > T_0$:

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

- Need to impute missing potential outcomes, $Y_{1t}(0)$.
- **Synthetic control**: Choose weights $(w_2, \dots, w_{J+1})'$ such that:
  - $w_j \geq 0$ and $\sum_j w_j = 1$.
  - for all $t \leq T_0$ minimize

$$\left| Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{jt} \right|, \qquad \left| \mathbf{Z}_1 - \sum_{j=2}^{J+1} w_j \mathbf{Z}_j \right|$$

  - Can also add a penalty for how dispersed the weights are.
- We hope this implies for $t > T_0$: $\sum_{j=2}^{J+1} w_j Y_{jt} \approx Y_{1t}(0)$

Without synthetic controls

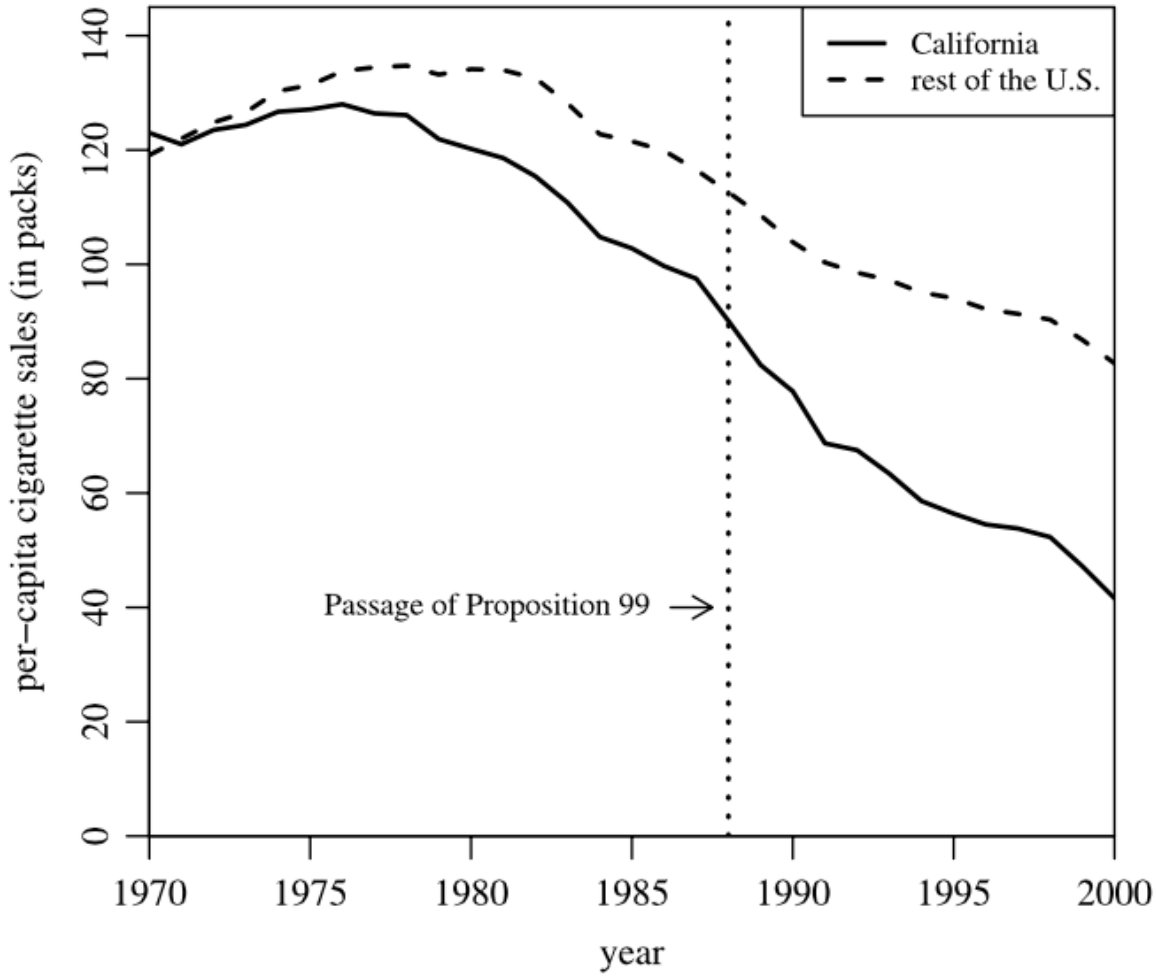


Figure 1. Trends in per-capita cigarette sales: California vs. the rest of the United States.

With synthetic controls

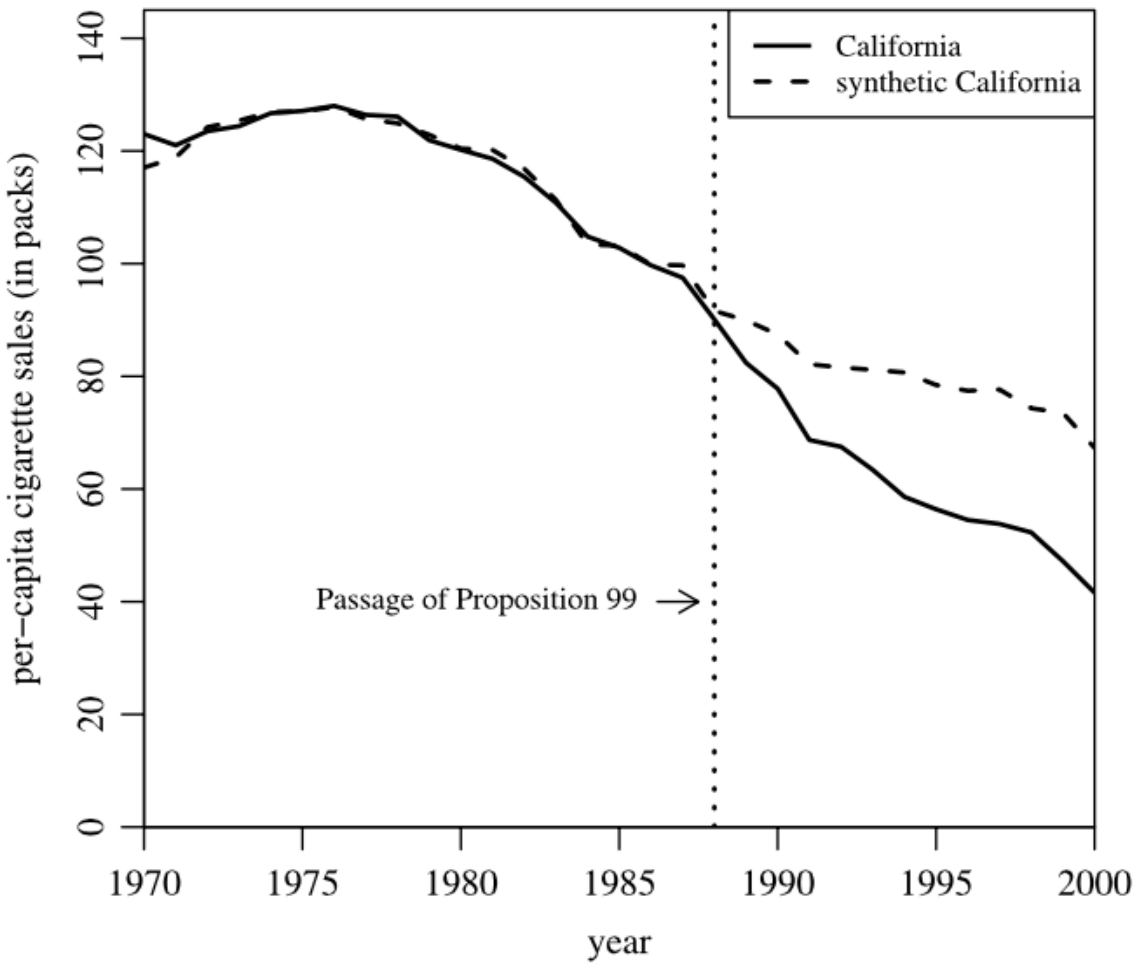


Figure 2. Trends in per-capita cigarette sales: California vs. synthetic California.

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

Inference

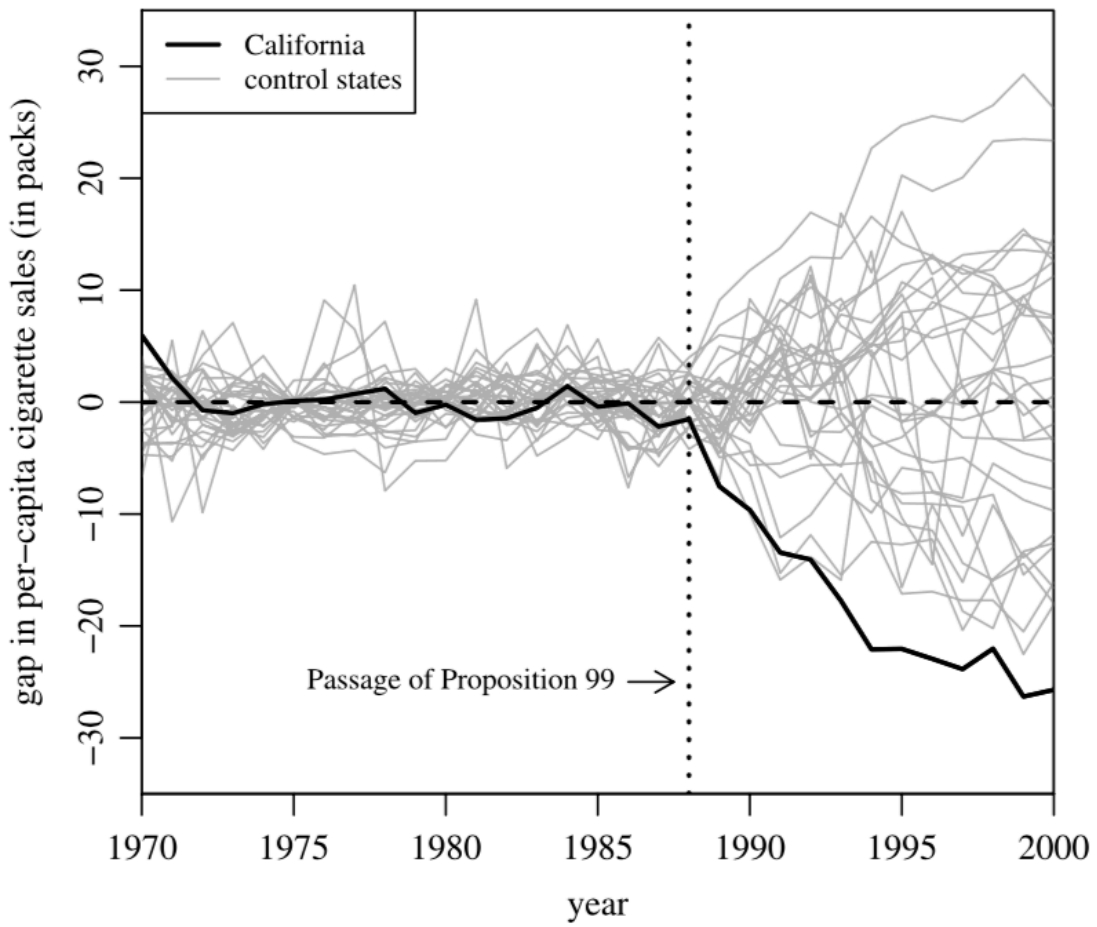


Figure 6. Per-capita cigarette sales gaps in California and placebo gaps in 29 control states (discards states with pre-Proposition 99 MSPE five times higher than California's).

Synthetic control justification

- ADH provide two **model-based** justifications for SC.
- **Model 1**: Interacted factor model

$$Y_{it}(0) = \mathbf{X}_i'\boldsymbol{\beta}_t + \alpha_i + \delta_t + \boldsymbol{\lambda}_t\boldsymbol{\mu}_i + \varepsilon_{it}$$

  - $\boldsymbol{\beta}_t$ are time-varying coefficients on covariates.
  - $\boldsymbol{\lambda}_t$ is a $1 \times F$ vector of common factors
  - $\boldsymbol{\mu}_i$ is a $F \times 1$ vector of factor loadings
  - $\boldsymbol{\lambda}_t\boldsymbol{\mu}_i$ allows time-varying confounding in a structured way.
  - Common time shocks affect each unit in a time-constant way.
- **Model 2**: autoregressive model without fixed effects

$$Y_{i,t+1}(0) = \alpha_t Y_{it}(0) + \boldsymbol{\beta}_{t+1}\mathbf{X}_{i,t+1} + u_{i,t+1}$$
$$\mathbf{X}_{i,t+1} = \gamma_t Y_{it}(0) + \boldsymbol{\Pi}_t\mathbf{X}_{it} + \mathbf{v}_{i,t+1}$$

- Either fixed effects OR lagged dependent variables, not both.

SCM properties

- Suppose perfect balancing weights exist $(w_2^*, \dots, w_{J+1}^*)$ such that:

$$\sum_{j=2}^{J+1} w_j^* Y_{jt} = Y_{1t} \qquad \sum_{j=2}^{J+1} w_j^* \mathbf{X}_j = \mathbf{X}_i$$

- Let $\widehat{Y}_{1t}(0) = \sum_{j=2}^{J+1} w_j^* Y_{jt}$ for post-intervention periods.
- Under Model 1, $\widehat{Y}_{1t}(0) \to Y_{1t}(0)$ as $T_0 \to \infty$
    - As length of pre-intervention period grows, estimates get better.
- Under Model 2, $\mathbb{E}\left[\widehat{Y}_{1t}(0)\right] = \mathbb{E}[Y_{1t}(0)]$
    - Unbiased only based on one pre-treatment periods.
    - But it assumes away unmeasured confounding!
- Outside of those models: ?????

Bias correction

- When pre-treatment fit is imperfect $\rightsquigarrow$ significant bias in SCM
- **Augmented SCM**: use regression models to correct for bias
  - Let $\widehat{m}_{it} = \widehat{m}_{it}(\overline{Y}_{i,t-1})$ be predicted values for a regression of post-treatment outcomes on pre-treatment outcomes.
  - Augment estimator (Ben-Michael, et al, 2021, JASA):

$$\widehat{Y}_{1t}^{\text{aug}}(0) = \sum_{j=2}^{J+1} w_j Y_{jt} + \left(\widehat{m}_{1t} - \sum_{j=2}^{J+1} w_j \widehat{m}_{jt} \right)$$

  - Can add covariates fairly easily.
- Very similar to bias correction in matching.

Generalizing to more treated units

- Two estimation methods to generalize to any number of treated units.
- **Interactive fixed effects**: $Y_{it}(0) = \mathbf{X}_{it}'\beta + \alpha_i + \delta_t + \boldsymbol{\lambda}_t\boldsymbol{\mu}_i$
    - Instead of weights, directly estimate IFE using iterative procedure:
        1. Treat IFE terms as fixed and fit parametric part on untreated units to get new $\hat{\beta}$
        2. Treat covariate coefficients as fixed and use factor analysis to estimate IFE terms.
        3. Repeat until convergence.
- **Matrix completion** methods (Athey et al, 2021)
    - Treat matrix of control POs, $\mathbf{Y}(\mathbf{0})$ as missing data problem.
    - Estimate lower-rank matrix $\mathbf{L}$ as best approximation to observed parts of $\mathbf{Y}(\mathbf{0})$ subject to regularization.