Week 12: Repeated Observations and Panel Data

Brandon Stewart[1]

Princeton

December 5–7, 2022

[1]Many slides drawn from Matt Blackwell, Adam Glynn, Jens Hainmueller, Erin Hartman, and Ian Lundberg.

Where We've Been and Where We're Going...

- Last Week
  - causal inference with unmeasured confounding
- This Week
  - difference-in-differences (DID/'diff-in-diff')
  - synthetic controls
  - fixed effects
  - wrap-up
- The Following Week
  - reading period and review session
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causality

Motivation: Studying the Minimum Wage

Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania

By David Card and Alan B. Krueger*

On April 1, 1992, New Jersey's minimum wage rose from $4.25 to $5.05 per hour. To evaluate the impact of the law we surveyed 410 fast-food restaurants in New Jersey and eastern Pennsylvania before and after the rise. Comparisons of employment growth at stores in New Jersey and Pennsylvania (where the minimum wage was constant) provide simple estimates of the effect of the higher minimum wage. We also compare employment changes at stores in New Jersey that were initially paying high wages (above $5) to the changes at lower-wage stores. We find no indication that the rise in the minimum wage reduced employment. (*JEL* J30, J23)

https://www.jstor.org/stable/2118030

Motivation: Studying the Minimum Wage

- Economics conventional wisdom: higher minimum wages decrease low-wage jobs.

Motivation: Studying the Minimum Wage

- Economics conventional wisdom: higher minimum wages decrease low-wage jobs.
- Card and Krueger (1994) study a 1992 NJ minimum wage increase ($4.25 to $5.05).

Motivation: Studying the Minimum Wage

- Economics conventional wisdom: higher minimum wages decrease low-wage jobs.
- Card and Krueger (1994) study a 1992 NJ minimum wage increase ($4.25 to $5.05).
- Idea: compare employment rates in 410 fast-food restaurants in NJ and eastern PA (where there wasn't a wage increase) both before and after the change.

Motivation: Studying the Minimum Wage

- Economics conventional wisdom: higher minimum wages decrease low-wage jobs.
- Card and Krueger (1994) study a 1992 NJ minimum wage increase (\$4.25 to \$5.05).
- Idea: compare employment rates in 410 fast-food restaurants in NJ and eastern PA (where there wasn't a wage increase) both before and after the change.
- Based on survey data:
  - Wave 1: March 1992, one month before the minimum wage increased
  - Wave 2: December 1992, eight months after increase

Motivation: Studying the Minimum Wage

- Economics conventional wisdom: higher minimum wages decrease low-wage jobs.
- Card and Krueger (1994) study a 1992 NJ minimum wage increase ($4.25 to $5.05).
- Idea: compare employment rates in 410 fast-food restaurants in NJ and eastern PA (where there wasn't a wage increase) both before and after the change.
- Based on survey data:
  - Wave 1: March 1992, one month before the minimum wage increased
  - Wave 2: December 1992, eight months after increase
- "What would a skeptic consider convincing evidence?" David Card
- "There was a time when we thought econometric techniques would solve a lot of the data problems. Now I think the feeling is that there are a lot of problems for which it is easier to get better data." Alan Krueger

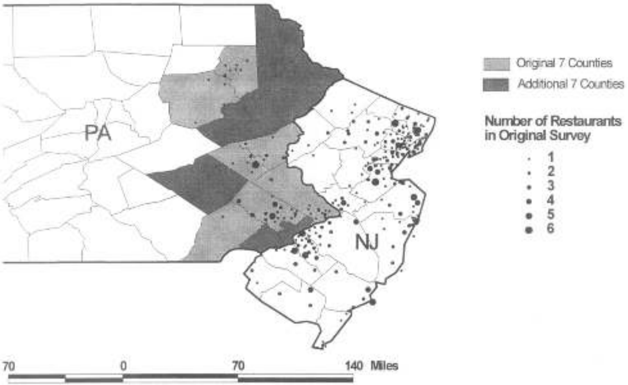


FIGURE 1. AREAS OF NEW JERSEY AND PENNSYLVANIA COVERED BY ORIGINAL SURVEY AND BLS DATA

Source: Card and Krueger 2000

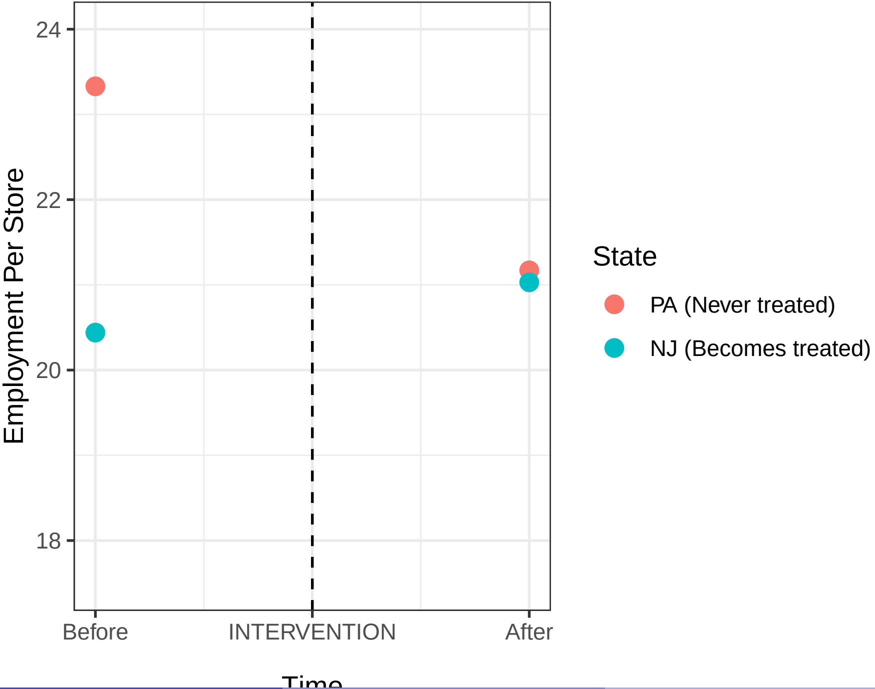
Conversations / *David Card and Alan Krueger*

Two Economists Catch Clinton's Eye By Bucking the Common Wisdom

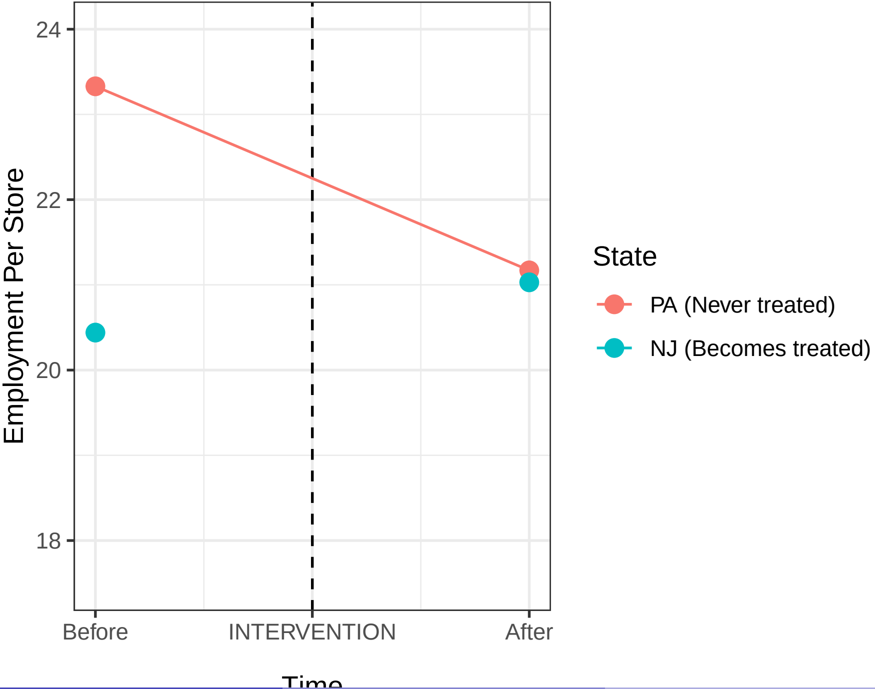
They use control groups in their research, and test minimum-wage theories by surveying the managers of fast-food restaurants.

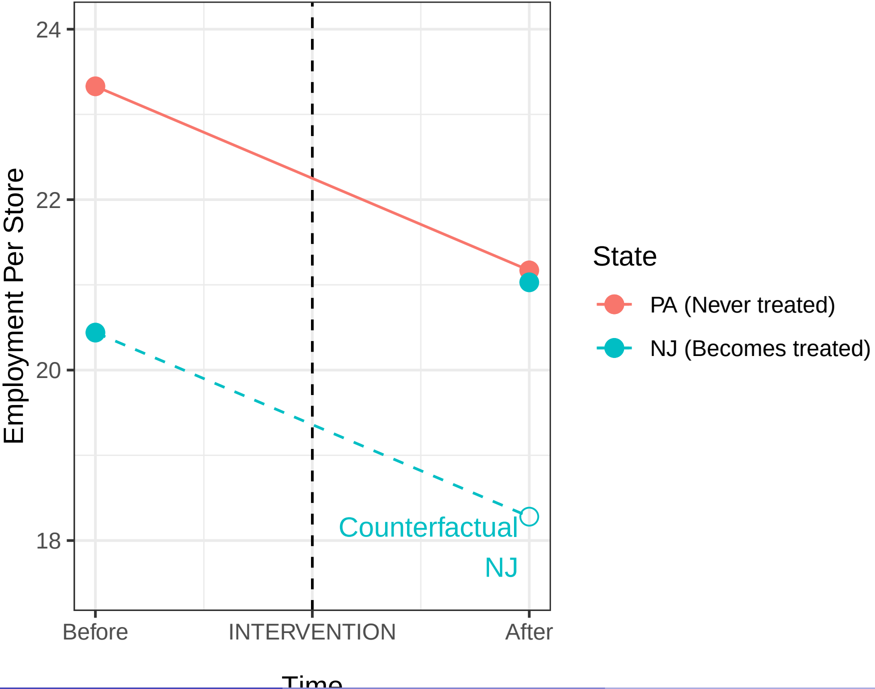
The New Jersey Test

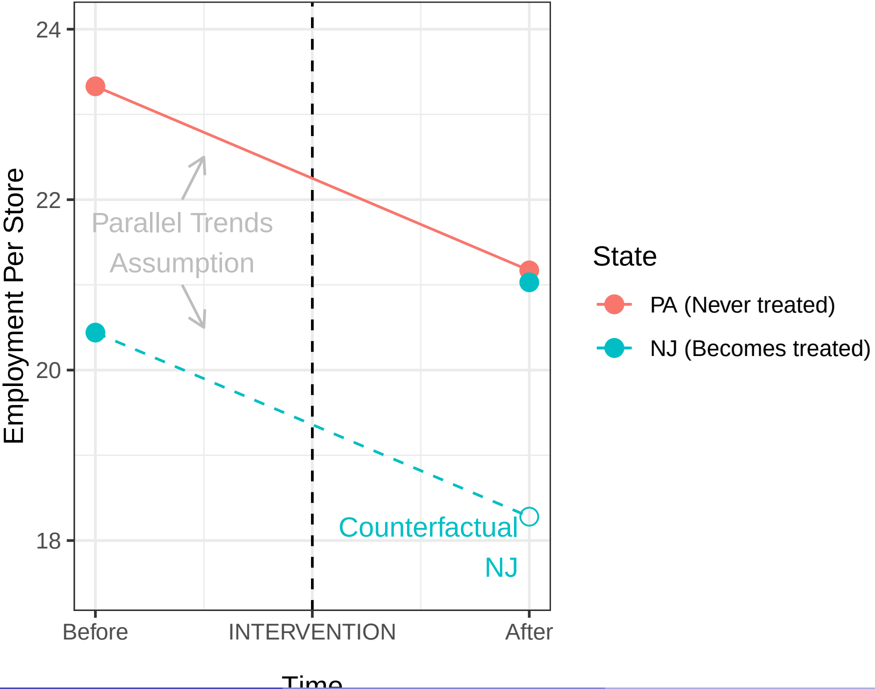
'Let's Not Assume'

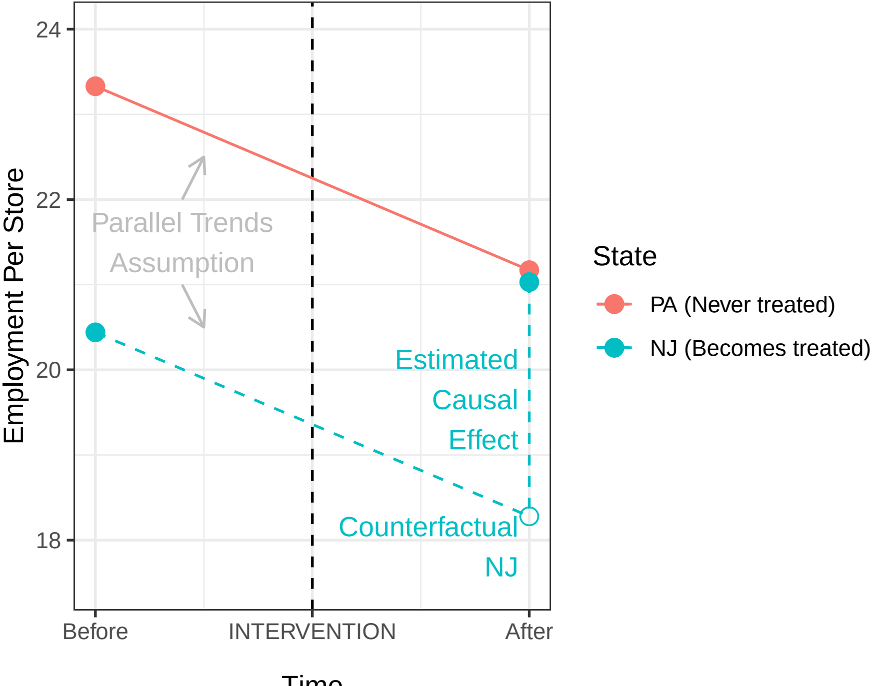


Employment Per Store
24
22
20
18
Before
INTERVENTION
After
Time
State
PA (Never treated)
NJ (Becomes treated)


Employment Per Store
24
22
20
18
Before
INTERVENTION
After
Time
State
PA (Never treated)
NJ (Becomes treated)


24
22
20
18
Employment Per Store
Before
INTERVENTION
After
Time
Counterfactual
NJ
State
PA (Never treated)
NJ (Becomes treated)


24
22
20
18
Employment Per Store
Parallel Trends
Assumption
Counterfactual
NJ
Before
INTERVENTION
After
Time
State
PA (Never treated)
NJ (Becomes treated)


24
22
20
18
Employment Per Store
Parallel Trends
Assumption
Estimated
Causal
Effect
Counterfactual
NJ
Before
INTERVENTION
After
Time
State
PA (Never treated)
NJ (Becomes treated)

Example: Card and Krueger (2000)

- Do increases to the minimum wage depress employment at fast-food restaurants?

$$\text{employment}_{it} = \beta_0 + \beta_1 \text{minimum wage}_{it} + a_i + u_{it}$$

Example: Card and Krueger (2000)

- Do increases to the minimum wage depress employment at fast-food restaurants?

$$\text{employment}_{it} = \beta_0 + \beta_1 \text{minimum wage}_{it} + a_i + u_{it}$$

- Each i here is a different fast food restaurant in either New Jersey or Pennsylvania

Example: Card and Krueger (2000)

- Do increases to the minimum wage depress employment at fast-food restaurants?

$$\text{employment}_{it} = \beta_0 + \beta_1 \text{minimum wage}_{it} + a_i + u_{it}$$

- Each i here is a different fast food restaurant in either New Jersey or Pennsylvania
- Between $t = 1$ and $t = 2$ NJ raised its minimum wage

Example: Card and Krueger (2000)

- Do increases to the minimum wage depress employment at fast-food restaurants?

$$\text{employment}_{it} = \beta_0 + \beta_1 \text{minimum wage}_{it} + a_i + u_{it}$$

- Each i here is a different fast food restaurant in either New Jersey or Pennsylvania
- Between $t = 1$ and $t = 2$ NJ raised its minimum wage
- Employment in fast food might be driven by other state-level policies correlated with minimum wage

Example: Card and Krueger (2000)

- Do increases to the minimum wage depress employment at fast-food restaurants?

$$\text{employment}_{it} = \beta_0 + \beta_1 \text{minimum wage}_{it} + a_i + u_{it}$$

- Each i here is a different fast food restaurant in either New Jersey or Pennsylvania
- Between $t = 1$ and $t = 2$ NJ raised its minimum wage
- Employment in fast food might be driven by other state-level policies correlated with minimum wage
- Diff-in-diff approach: regress changes in employment on store being in NJ

$$\Delta\text{employment}_i = \beta_0 + \beta_1 NJ_i + \Delta u_i$$

Example: Card and Krueger (2000)

- Do increases to the minimum wage depress employment at fast-food restaurants?

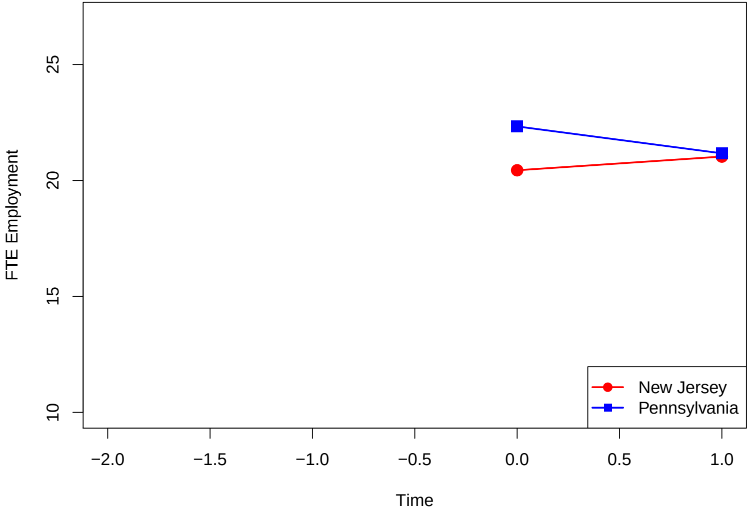
$$\text{employment}_{it} = \beta_0 + \beta_1 \text{minimum wage}_{it} + a_i + u_{it}$$

- Each i here is a different fast food restaurant in either New Jersey or Pennsylvania
- Between $t = 1$ and $t = 2$ NJ raised its minimum wage
- Employment in fast food might be driven by other state-level policies correlated with minimum wage
- Diff-in-diff approach: regress changes in employment on store being in NJ

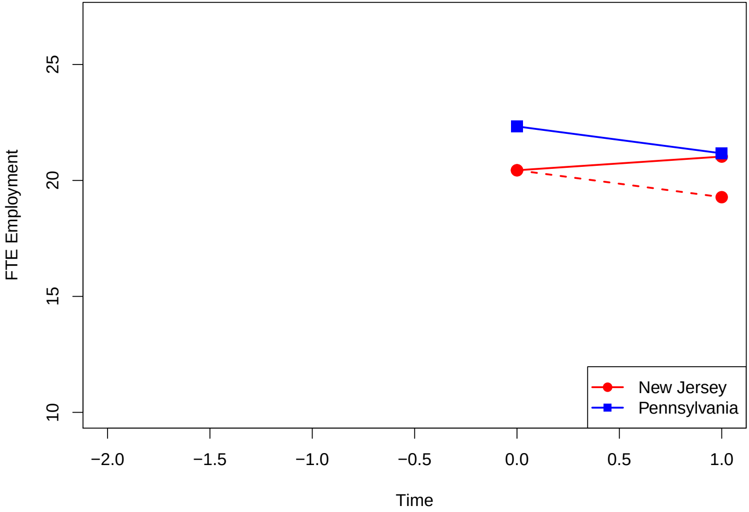
$$\Delta\text{employment}_i = \beta_0 + \beta_1 NJ_i + \Delta u_i$$

- NJ_i indicates which stores received the treatment of a higher minimum wage at time period $t = 2$

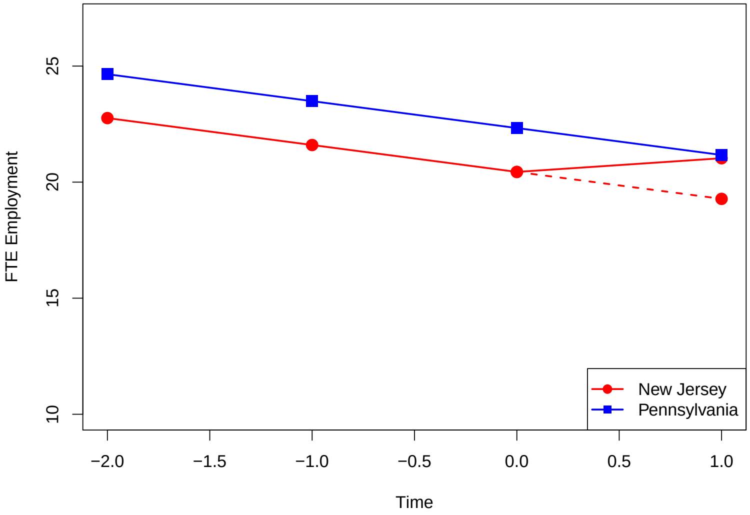
Parallel Trends?



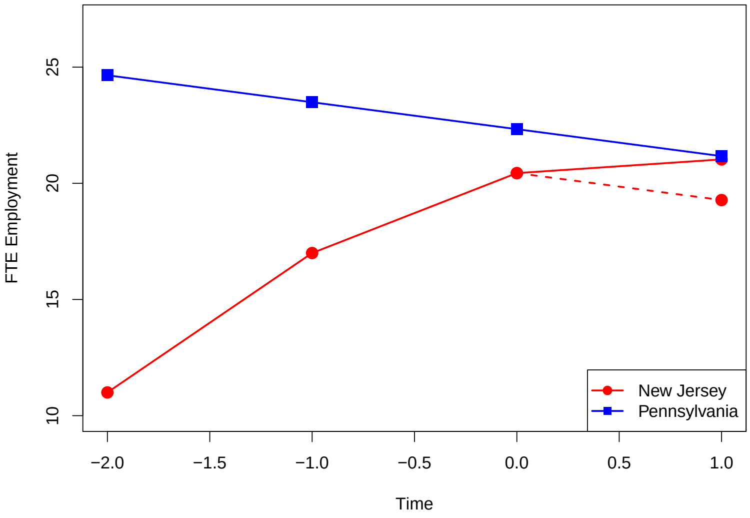
Parallel Trends?



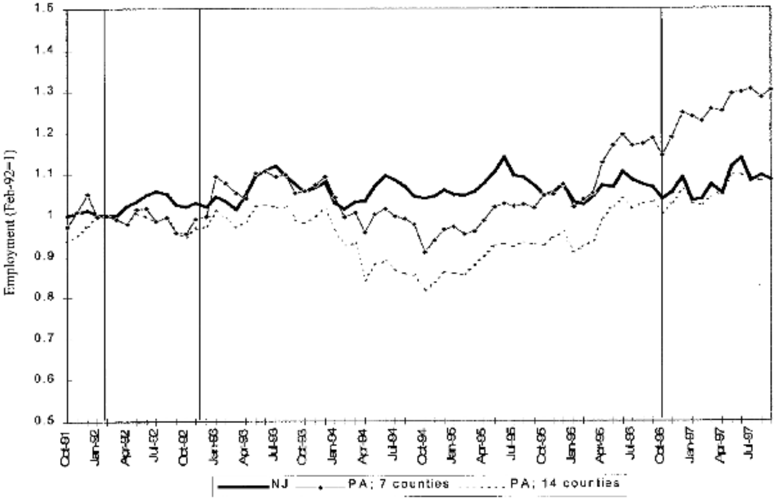
Parallel Trends?



Parallel Trends?



Longer Trends in Employment (Card and Krueger 2000)



First two vertical lines indicate the dates of the Card-Krueger survey. October 1996 line is the federal minimum wage hike which was binding in PA but not NJ

Threats to Identification

Threats to Identification

1) Failure of Exogeneity
   Treatment needs to be independent of the idiosyncratic shocks

Threats to Identification

1) Failure of Exogeneity
   Treatment needs to be independent of the idiosyncratic shocks

2) Non-parallel dynamics
   variation in the outcome over time is the same for the treated and control groups (i.e. no omitted time-varying confounders). e.g. Ashenfelter's dip: people who enroll in job training programs see their earnings decline prior to that training (presumably why they are entering)

Threats to Identification

1) Failure of Exogeneity
   Treatment needs to be independent of the idiosyncratic shocks
2) Non-parallel dynamics
   variation in the outcome over time is the same for the treated and control groups (i.e. no omitted time-varying confounders). e.g. Ashenfelter's dip: people who enroll in job training programs see their earnings decline prior to that training (presumably why they are entering)
3) Changes in Composition of Treatment/Control Groups
   we don't want composition of sample to change between periods. what if workers move from eastern PA to NJ in search of higher paying jobs?

Threats to Identification

1) Failure of Exogeneity
   Treatment needs to be independent of the idiosyncratic shocks
2) Non-parallel dynamics
   variation in the outcome over time is the same for the treated and control groups (i.e. no omitted time-varying confounders). e.g. Ashenfelter's dip: people who enroll in job training programs see their earnings decline prior to that training (presumably why they are entering)
3) Changes in Composition of Treatment/Control Groups
   we don't want composition of sample to change between periods. what if workers move from eastern PA to NJ in search of higher paying jobs?
4) Long-term vs. Short-term Effects
   parallel trends are less credible over a long time horizon, but many policies need time to take effect.

Threats to Identification

Threats to Identification

5) Functional Form Dependence
   difference in levels and difference in logs can be quite different
   (example via Justin Grimmer)

Threats to Identification

5) Functional Form Dependence
   difference in levels and difference in logs can be quite different
   (example via Justin Grimmer)

   - imagine a training program for the young

Threats to Identification

5) Functional Form Dependence
   difference in levels and difference in logs can be quite different (example via Justin Grimmer)

   - imagine a training program for the young
   - employment for the young increases from 20% to 30%
   - employment for the old increases from 5% to 10%

Threats to Identification

5) Functional Form Dependence
   difference in levels and difference in logs can be quite different
   (example via Justin Grimmer)

   - imagine a training program for the young
   - employment for the young increases from 20% to 30%
   - employment for the old increases from 5% to 10%
   - positive DiD effect: $(30 - 20) - (10 - 5) = 5\%$

Threats to Identification

5) Functional Form Dependence
   difference in levels and difference in logs can be quite different
   (example via Justin Grimmer)

   - imagine a training program for the young
   - employment for the young increases from 20% to 30%
   - employment for the old increases from 5% to 10%
   - positive DiD effect: $(30 - 20) - (10 - 5) = 5\%$
   - but if you consider chanes in the log:
     $[log(30) - log(20)] - [log(10) - log(5)] = log(1.5) - log(2) < 0$

Threats to Identification

5) Functional Form Dependence
difference in levels and difference in logs can be quite different
(example via Justin Grimmer)

   - imagine a training program for the young
   - employment for the young increases from 20% to 30%
   - employment for the old increases from 5% to 10%
   - positive DiD effect: $(30 - 20) - (10 - 5) = 5\%$
   - but if you consider chanes in the log:
     $[log(30) - log(20)] - [log(10) - log(5)] = log(1.5) - log(2) < 0$
   - how do we tell which (if either) yields parallel trends?

Threats to Identification

5) Functional Form Dependence
difference in levels and difference in logs can be quite different
(example via Justin Grimmer)

   - imagine a training program for the young
   - employment for the young increases from 20% to 30%
   - employment for the old increases from 5% to 10%
   - positive DiD effect: $(30 - 20) - (10 - 5) = 5\%$
   - but if you consider chanes in the log:
     $[log(30) - log(20)] - [log(10) - log(5)] = log(1.5) - log(2) < 0$
   - how do we tell which (if either) yields parallel trends?

1. Difference-in-Differences

2. Synthetic Controls

3. Fixed Effects

4. Non-parametric Identification and Fixed Effects

5. Wrap-Up
   - Questions
   - Concluding Thoughts for the Course

Synthetic control

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association, 105(490), 493-505.

Synthetic control

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association, 105(490), 493-505.

In 1988, California implemented a tobacco control program

Synthetic control

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association, 105(490), 493-505.

In 1988, California implemented a tobacco control program

- New tax: 25 cents per pack

Synthetic control

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association, 105(490), 493-505.

In 1988, California implemented a tobacco control program

- New tax: 25 cents per pack
- Money earmarked for smoking-reduction programs

Synthetic control

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association, 105(490), 493-505.

In 1988, California implemented a tobacco control program

- New tax: 25 cents per pack
- Money earmarked for smoking-reduction programs

How much did it reduce CA cigarette sales in 1990? 1995? 2000?

Synthetic control

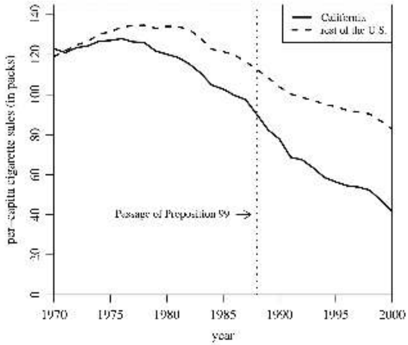


Figure 1. Trends in per-capita cigarette sales: California vs. the rest of the United States.

Can't use RDD
— Effect at 1988 not of interest

Can't use ITS
— Hard to extrapolate Y^0 trend

Can't use DID
— No other state like CA

Idea: Create a **synthetic CA** to estimate $Y^0_{\text{CA,t}}$ for $t \geq 1988$

A Synthetic California

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). GDP per capita is measured in 1997 dollars, retail prices are measured in cents, beer consumption is measured in gallons, and cigarette sales are measured in packs.

A Synthetic California

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

Synthetic control

Theoretical Estimand: $\tau(t) = Y^1_{\text{CA},t} - Y^0_{\text{CA},t} \qquad t \geq 1988$

Synthetic control

Theoretical Estimand: $\tau(t) = Y^1_{\text{CA},t} - Y^0_{\text{CA},t} \qquad t \geq 1988$

Empirical Estimand: $\theta(t) = Y^1_{\text{CA},t} - Y^0_{\text{SyntheticCA},t} \qquad t \geq 1988$

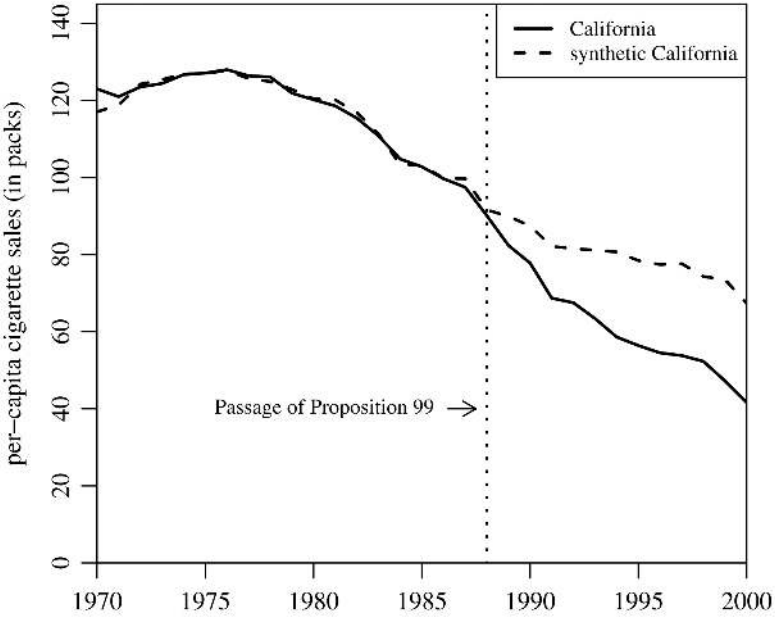
Synthetic control

$$\text{Theoretical Estimand:} \quad \tau(t) = Y^1_{\text{CA},t} - Y^0_{\text{CA},t} \qquad t \geq 1988$$

$$\text{Empirical Estimand:} \quad \theta(t) = Y^1_{\text{CA},t} - Y^0_{\text{SyntheticCA},t} \qquad t \geq 1988$$

$$\text{Identifying Assumption:} \quad \underbrace{Y^0_{\text{CA},t}}_{\text{Counterfactual}} = \underbrace{Y^0_{\text{SyntheticCA},t}}_{\text{Factual}} \qquad t \geq 1988$$

Synthetic control



Justifications of Synthetic Control

Justifications of Synthetic Control

- Two model-based justifications for synthetic control.

Justifications of Synthetic Control

- Two model-based justifications for synthetic control.
  - Model 1: structured, unobserved confounding with interactive fixed effects

Justifications of Synthetic Control

- Two model-based justifications for synthetic control.
  - Model 1: structured, unobserved confounding with interactive fixed effects
  - Model 2: autoregressive model without fixed effects

Justifications of Synthetic Control

- Two model-based justifications for synthetic control.
  - Model 1: structured, unobserved confounding with interactive fixed effects
  - Model 2: autoregressive model without fixed effects
  - Works for fixed effects or lagged dependent variables, but not both.

Justifications of Synthetic Control

- Two model-based justifications for synthetic control.
  - Model 1: structured, unobserved confounding with interactive fixed effects
  - Model 2: autoregressive model without fixed effects
  - Works for fixed effects or lagged dependent variables, but not both.
- Strategies that generalize to more treated units:
  - interactive fixed effects (e.g. Xu 2017)
  - matrix completion (e.g. Athey et al 2021)

Justifications of Synthetic Control

- Two model-based justifications for synthetic control.
  - Model 1: structured, unobserved confounding with interactive fixed effects
  - Model 2: autoregressive model without fixed effects
  - Works for fixed effects or lagged dependent variables, but not both.
- Strategies that generalize to more treated units:
  - interactive fixed effects (e.g. Xu 2017)
  - matrix completion (e.g. Athey et al 2021)

To read more about core SCM: Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. "Comparative politics and the synthetic control method." *American Journal of Political Science* 59.2 (2015): 495-510.

Basic Model

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}$$

- Consider the above basic model where a_i is an intercept associated with unit i and u_{it} satisfy strict exogeneity.

Basic Model

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}$$

- Consider the above basic model where a_i is an intercept associated with unit i and u_{it} satisfy strict exogeneity.
- The fixed effects model is a way to remove time-invariant unmeasured confounding from a_i

Basic Model

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}$$

- Consider the above basic model where a_i is an intercept associated with unit i and u_{it} satisfy strict exogeneity.
- The fixed effects model is a way to remove time-invariant unmeasured confounding from a_i
- We will start by assuming the model and discussing properties and in the next section, we will consider non-parametric identification.

Fixed Effects Models

- Core idea is to focus on within-unit comparisons: changes in y_{it} and x_{it} relative to their within-group means

Fixed Effects Models

- Core idea is to focus on within-unit comparisons: changes in y_{it} and x_{it} relative to their within-group means
- First note that taking the average of the y's over time for a given unit leaves us with a very similar model:

$$\overline{y}_i = \frac{1}{T}\sum_{t=1}^{T}\left[\mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}\right]$$

Fixed Effects Models

- Core idea is to focus on within-unit comparisons: changes in y_{it} and x_{it} relative to their within-group means
- First note that taking the average of the y's over time for a given unit leaves us with a very similar model:

$$
\begin{aligned}
\overline{y}_i &= \frac{1}{T}\sum_{t=1}^{T}\left[\mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}\right] \\
&= \left(\frac{1}{T}\sum_{t=1}^{T}\mathbf{x}_{it}'\right)\boldsymbol{\beta} + \frac{1}{T}\sum_{t=1}^{T} a_i + \frac{1}{T}\sum_{t=1}^{T} u_{it}
\end{aligned}
$$

Fixed Effects Models

- Core idea is to focus on within-unit comparisons: changes in y_{it} and x_{it} relative to their within-group means
- First note that taking the average of the y's over time for a given unit leaves us with a very similar model:

$$
\begin{aligned}
\overline{y}_i &= \frac{1}{T}\sum_{t=1}^{T}\left[\mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}\right] \\
&= \left(\frac{1}{T}\sum_{t=1}^{T}\mathbf{x}_{it}'\right)\boldsymbol{\beta} + \frac{1}{T}\sum_{t=1}^{T} a_i + \frac{1}{T}\sum_{t=1}^{T} u_{it} \\
&= \overline{\mathbf{x}}_i'\boldsymbol{\beta} + a_i + \overline{u}_i
\end{aligned}
$$

Fixed Effects Models

- Core idea is to focus on within-unit comparisons: changes in y_{it} and x_{it} relative to their within-group means
- First note that taking the average of the y's over time for a given unit leaves us with a very similar model:

$$
\begin{aligned}
\overline{y}_i &= \frac{1}{T}\sum_{t=1}^{T}\left[\mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}\right] \\
&= \left(\frac{1}{T}\sum_{t=1}^{T}\mathbf{x}_{it}'\right)\boldsymbol{\beta} + \frac{1}{T}\sum_{t=1}^{T}a_i + \frac{1}{T}\sum_{t=1}^{T}u_{it} \\
&= \overline{\mathbf{x}}_i'\boldsymbol{\beta} + a_i + \overline{u}_i
\end{aligned}
$$

- Key fact: because it is time-constant the mean of a_i is just a_i

Fixed Effects Models

- Core idea is to focus on within-unit comparisons: changes in y_{it} and x_{it} relative to their within-group means
- First note that taking the average of the y's over time for a given unit leaves us with a very similar model:

$$
\begin{aligned}
\overline{y}_i &= \frac{1}{T}\sum_{t=1}^{T}\left[\mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}\right] \\
&= \left(\frac{1}{T}\sum_{t=1}^{T}\mathbf{x}_{it}'\right)\boldsymbol{\beta} + \frac{1}{T}\sum_{t=1}^{T} a_i + \frac{1}{T}\sum_{t=1}^{T} u_{it} \\
&= \overline{\mathbf{x}}_i'\boldsymbol{\beta} + a_i + \overline{u}_i
\end{aligned}
$$

- Key fact: because it is time-constant the mean of a_i is just a_i
- This regression is sometimes called the "between regression"

Within Transformation

Within Transformation

- The "fixed effects," "within," or "time-demeaning" transformation is when we subtract off the over-time means from the original data:

$$(y_{it} - \overline{y}_i) = (\mathbf{x}'_{it} - \overline{\mathbf{x}}'_i)\boldsymbol{\beta} + (u_{it} - \overline{u}_i)$$

Within Transformation

- The "fixed effects," "within," or "time-demeaning" transformation is when we subtract off the over-time means from the original data:

$$(y_{it} - \overline{y}_i) = (\mathbf{x}'_{it} - \overline{\mathbf{x}}'_i)\boldsymbol{\beta} + (u_{it} - \overline{u}_i)$$

- If we write $\ddot{y}_{it} = y_{it} - \overline{y}_i$, then we can write this more compactly as:

$$\ddot{y}_{it} = \ddot{\mathbf{x}}'_{it}\boldsymbol{\beta} + \ddot{u}_{it}$$

Within Transformation

- The "fixed effects," "within," or "time-demeaning" transformation is when we subtract off the over-time means from the original data:

$$(y_{it} - \overline{y}_i) = (\mathbf{x}'_{it} - \overline{\mathbf{x}}'_i)\boldsymbol{\beta} + (u_{it} - \overline{u}_i)$$

- If we write $\ddot{y}_{it} = y_{it} - \overline{y}_i$, then we can write this more compactly as:

$$\ddot{y}_{it} = \ddot{\mathbf{x}}'_{it}\boldsymbol{\beta} + \ddot{u}_{it}$$

- Degrees of freedom: $nT - n - k - 1$, which accounts for within transformation (i.e. either use a package like `plm` or adjust the degrees of freedom manually).

Within Transformation

- The "fixed effects," "within," or "time-demeaning" transformation is when we subtract off the over-time means from the original data:

$$(y_{it} - \overline{y}_i) = (\mathbf{x}'_{it} - \overline{\mathbf{x}}'_i)\boldsymbol{\beta} + (u_{it} - \overline{u}_i)$$

- If we write $\ddot{y}_{it} = y_{it} - \overline{y}_i$, then we can write this more compactly as:

$$\ddot{y}_{it} = \ddot{\mathbf{x}}'_{it}\boldsymbol{\beta} + \ddot{u}_{it}$$

- Degrees of freedom: $nT - n - k - 1$, which accounts for within transformation (i.e. either use a package like `plm` or adjust the degrees of freedom manually).
- We are now modeling observations as deviation from their group mean.

Within Transformation

- The "fixed effects," "within," or "time-demeaning" transformation is when we subtract off the over-time means from the original data:

$$(y_{it} - \overline{y}_i) = (\mathbf{x}'_{it} - \overline{\mathbf{x}}'_i)\boldsymbol{\beta} + (u_{it} - \overline{u}_i)$$

- If we write $\ddot{y}_{it} = y_{it} - \overline{y}_i$, then we can write this more compactly as:

$$\ddot{y}_{it} = \ddot{\mathbf{x}}'_{it}\boldsymbol{\beta} + \ddot{u}_{it}$$

- Degrees of freedom: $nT - n - k - 1$, which accounts for within transformation (i.e. either use a package like `plm` or adjust the degrees of freedom manually).
- We are now modeling observations as deviation from their group mean.
- NB: you must demean the X variables not just the Y variables.

Fixed Effects Example with data from Ross

```
fe.mod <- plm(log(kidmort_unicef) ~ democracy + log(GDPcur), data = ross, index = c("id", "year"),
 model = "within")
summary(fe.mod)
```

```
## Oneway (individual) effect Within Model
##
## Call:
## plm(formula = log(kidmort_unicef) ~ democracy + log(GDPcur),
##     data = ross, model = "within", index = c("id", "year"))
##
## Unbalanced Panel: n=166, T=1-7, N=649
##
## Residuals :
##     Min.  1st Qu.   Median  3rd Qu.     Max.
## -0.70500 -0.11700  0.00628  0.12200  0.75700
##
## Coefficients :
##              Estimate Std. Error  t-value  Pr(>|t|)
## democracy   -0.143233   0.033500  -4.2756 2.299e-05 ***
## log(GDPcur) -0.375203   0.011328 -33.1226 < 2.2e-16 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## Total Sum of Squares:    81.711
## Residual Sum of Squares: 23.012
## R-Squared      :  0.71838
##       Adj. R-Squared :  0.53242
## F-statistic: 613.481 on 2 and 481 DF, p-value: < 2.22e-16
```

Strict Exogeneity

- FE models are valid if $E[\mathbf{u}|\mathbf{X}] = 0$: all errors are uncorrelated with covariates in every period:

$$E[\ddot{u}_{it}|\ddot{\mathbf{X}}] = E[u_{it}|\ddot{\mathbf{X}}] - E[\overline{u}_i|\ddot{\mathbf{X}}] = 0 - 0 = 0$$

Strict Exogeneity

- FE models are valid if $E[\mathbf{u}|\mathbf{X}] = 0$: all errors are uncorrelated with covariates in every period:

$$E[\ddot{u}_{it}|\ddot{\mathbf{X}}] = E[u_{it}|\ddot{\mathbf{X}}] - E[\overline{u}_i|\ddot{\mathbf{X}}] = 0 - 0 = 0$$

- This is because the composite errors, $\ddot{u}_{it}$ are function of the errors in every time period through the average, $\overline{u}_i$

Strict Exogeneity

- FE models are valid if $E[\mathbf{u}|\mathbf{X}] = 0$: all errors are uncorrelated with covariates in every period:

$$E[\ddot{u}_{it}|\ddot{\mathbf{X}}] = E[u_{it}|\ddot{\mathbf{X}}] - E[\overline{u}_i|\ddot{\mathbf{X}}] = 0 - 0 = 0$$

- This is because the composite errors, $\ddot{u}_{it}$ are function of the errors in every time period through the average, $\overline{u}_i$
- This rules out, for instance, lagged dependent variables, since $y_{i,t-1}$ has to be correlated with $u_{i,t-1}$. Thus it can't be a covariate for y_{it}.

Fixed Effects and Time-Invariant Covariates

- What if there is a covariate that doesn't vary over time?

Fixed Effects and Time-Invariant Covariates

- What if there is a covariate that doesn't vary over time?
- Then $x_{it} = \overline{x}_i$ and $\ddot{x}_{it} = 0$ for all periods t.

Fixed Effects and Time-Invariant Covariates

- What if there is a covariate that doesn't vary over time?
- Then $x_{it} = \overline{x}_i$ and $\ddot{x}_{it} = 0$ for all periods t.
- If the time-demeaned covariate is always 0, then it will be perfectly collinear with the intercept and will violate full rank. R/Stata will drop it from the regression.

Fixed Effects and Time-Invariant Covariates

- What if there is a covariate that doesn't vary over time?
- Then $x_{it} = \overline{x}_i$ and $\ddot{x}_{it} = 0$ for all periods t.
- If the time-demeaned covariate is always 0, then it will be perfectly collinear with the intercept and will violate full rank. R/Stata will drop it from the regression.
- Basic message: any time-constant variable gets "absorbed" by the fixed effect. It has nothing to contribute because the comparison is within the units.

Fixed Effects and Time-Invariant Covariates

- What if there is a covariate that doesn't vary over time?
- Then $x_{it} = \overline{x}_i$ and $\ddot{x}_{it} = 0$ for all periods t.
- If the time-demeaned covariate is always 0, then it will be perfectly collinear with the intercept and will violate full rank. R/Stata will drop it from the regression.
- Basic message: any time-constant variable gets "absorbed" by the fixed effect. It has nothing to contribute because the comparison is within the units.
- Can include interactions between time-constant and time-varying variables, but lower order term of the time-constant variables get absorbed by fixed effects too

Time-constant variables

- Pooled model with a time-constant variable, proportion Islamic:

```
library(lmtest)
p.mod <- plm(log(kidmort_unicef) ~ democracy + log(GDPcur) + islam,
             data = ross, index = c("id", "year"), model = "pooling")
coeftest(p.mod)
```

```
##
## t test of coefficients:
##
##                Estimate  Std. Error  t value  Pr(>|t|)
## (Intercept) 10.30607817  0.35951939  28.6663 < 2.2e-16 ***
## democracy   -0.80233845  0.07766814 -10.3303 < 2.2e-16 ***
## log(GDPcur) -0.25497406  0.01607061 -15.8659 < 2.2e-16 ***
## islam        0.00343325  0.00091045   3.7709 0.0001794 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

Time-constant variables

- FE model, where the islam variable drops out, along with the intercept:

```
fe.mod2 <- plm(log(kidmort_unicef) ~ democracy + log(GDPcur) + islam,
               data = ross, index = c("id", "year"), model = "within")
coeftest(fe.mod2)

##
## t test of coefficients:
##
##              Estimate Std. Error  t value  Pr(>|t|)
## democracy   -0.129693   0.035865  -3.6162 0.0003332 ***
## log(GDPcur) -0.379997   0.011849 -32.0707 < 2.2e-16 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

Alternate Computation: Least Squares Dummy Variable

- As an alternative to the within transformation, we can also include a series of $n - 1$ dummy variables for each unit:

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + d_i^{(1)}\alpha_1 + d_i^{(2)}\alpha_2 + \cdots + d_i^{(n)}\alpha_n + u_{it}$$

Alternate Computation: Least Squares Dummy Variable

- As an alternative to the within transformation, we can also include a series of $n-1$ dummy variables for each unit:

$$y_{it} = \mathbf{x}_{it}'\beta + d_i^{(1)}\alpha_1 + d_i^{(2)}\alpha_2 + \cdots + d_i^{(n)}\alpha_n + u_{it}$$

- Here, $d_i^{(1)}$ is a binary variable which is 1 if $i = 1$ and 0 otherwise—just a unit dummy.

Alternate Computation: Least Squares Dummy Variable

- As an alternative to the within transformation, we can also include a series of $n-1$ dummy variables for each unit:

$$y_{it} = \mathbf{x}'_{it}\beta + d_i^{(1)}\alpha_1 + d_i^{(2)}\alpha_2 + \cdots + d_i^{(n)}\alpha_n + u_{it}$$

- Here, $d_i^{(1)}$ is a binary variable which is 1 if $i = 1$ and 0 otherwise—just a unit dummy.
- Gives the exact same estimates/standard errors as with time-demeaning

Alternate Computation: Least Squares Dummy Variable

- As an alternative to the within transformation, we can also include a series of $n-1$ dummy variables for each unit:

$$y_{it} = \mathbf{x}_{it}'\beta + d_i^{(1)}\alpha_1 + d_i^{(2)}\alpha_2 + \cdots + d_i^{(n)}\alpha_n + u_{it}$$

- Here, $d_i^{(1)}$ is a binary variable which is 1 if $i = 1$ and 0 otherwise—just a unit dummy.
- Gives the exact same estimates/standard errors as with time-demeaning
  - Advantage: easy to implement in base `R` (so is the de-meaning but you have to recompute standard errors by changing the degrees of freedom manually).

Alternate Computation: Least Squares Dummy Variable

- As an alternative to the within transformation, we can also include a series of $n-1$ dummy variables for each unit:

$$y_{it} = \mathbf{x}'_{it}\boldsymbol{\beta} + d_i^{(1)}\alpha_1 + d_i^{(2)}\alpha_2 + \cdots + d_i^{(n)}\alpha_n + u_{it}$$

- Here, $d_i^{(1)}$ is a binary variable which is 1 if $i = 1$ and 0 otherwise—just a unit dummy.
- Gives the exact same estimates/standard errors as with time-demeaning
  - Advantage: easy to implement in base `R` (so is the de-meaning but you have to recompute standard errors by changing the degrees of freedom manually).
  - Disadvantage: computationally difficult with large data sets, since we have to run a regression with $n + k$ variables.

Alternate Computation: Least Squares Dummy Variable

- As an alternative to the within transformation, we can also include a series of $n-1$ dummy variables for each unit:

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + d_i^{(1)}\alpha_1 + d_i^{(2)}\alpha_2 + \cdots + d_i^{(n)}\alpha_n + u_{it}$$

- Here, $d_i^{(1)}$ is a binary variable which is 1 if $i = 1$ and 0 otherwise—just a unit dummy.
- Gives the exact same estimates/standard errors as with time-demeaning
  - Advantage: easy to implement in base `R` (so is the de-meaning but you have to recompute standard errors by changing the degrees of freedom manually).
  - Disadvantage: computationally difficult with large data sets, since we have to run a regression with $n + k$ variables.
- Why are these equivalent? (remember partialing out strategy and Frisch-Waugh-Lovell theorem)

Example with Ross data

```
library(lmtest)
lsdv.mod <- lm(log(kidmort_unicef) ~ democracy + log(GDPcur) +
               as.factor(id), data = ross)
coeftest(lsdv.mod)[1:6,]
coeftest(fe.mod)[1:2,]
```

```
##                     Estimate Std. Error    t value      Pr(>|t|)
## (Intercept)       13.7644887 0.26597312  51.751427 1.008329e-198
## democracy         -0.1432331 0.03349977  -4.275644  2.299393e-05
## log(GDPcur)       -0.3752030 0.01132772 -33.122568 3.494887e-126
## as.factor(id)AGO   0.2997206 0.16767730   1.787485  7.448861e-02
## as.factor(id)ALB  -1.9309618 0.19013955 -10.155498  4.392512e-22
## as.factor(id)ARE  -1.8762909 0.17020738 -11.023558  2.386557e-25
```

```
##                Estimate Std. Error    t value      Pr(>|t|)
## democracy   -0.1432331 0.03349977  -4.275644  2.299393e-05
## log(GDPcur) -0.3752030 0.01132772 -33.122568 3.494887e-126
```

Applying Fixed Effects

- We can use fixed effects for other data structures to restrict comparisons to within unit variation

Applying Fixed Effects

- We can use fixed effects for other data structures to restrict comparisons to within unit variation
  - Matched pairs
    - Twin fixed effects to control for unobserved effects of family background

Applying Fixed Effects

- We can use fixed effects for other data structures to restrict comparisons to within unit variation
  - Matched pairs
    - ★ Twin fixed effects to control for unobserved effects of family background
  - Cluster fixed effects in hierarchical data
    - ★ School fixed effects to control for unobserved effects of school

Moving Beyond Linear Separable Confounding

Moving Beyond Linear Separable Confounding

- One reason we like DAGs is that the identification results don't have to start with a statement like, assume the following linear model:

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}$$

Moving Beyond Linear Separable Confounding

- One reason we like DAGs is that the identification results don't have to start with a statement like, assume the following linear model:

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}$$

- What assumptions have we made so far?

Moving Beyond Linear Separable Confounding

- One reason we like DAGs is that the identification results don't have to start with a statement like, assume the following linear model:

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + a_i + u_{it}$$

- What assumptions have we made so far?
    - constant effects
    - linearity
    - strict exogeneity

Moving Beyond Linear Separable Confounding

- One reason we like DAGs is that the identification results don't have to start with a statement like, assume the following linear model:

$$y_{it} = \mathbf{x}'_{it}\boldsymbol{\beta} + a_i + u_{it}$$

- What assumptions have we made so far?
  - constant effects
  - linearity
  - strict exogeneity
- We've seen the trouble with constant effects before, it goes back to Lecture 10 and results on regression with heterogenous treatment effects more generally.

Contemporaneous, Cumulative and Dynamic Effects

- Another assumption we have been making is that our interest is in a single contemporaneous effect: $\mathbf{x}'_{it}\boldsymbol{\beta}$

Contemporaneous, Cumulative and Dynamic Effects

- Another assumption we have been making is that our interest is in a single contemporaneous effect: $\mathbf{x}_{it}'\boldsymbol{\beta}$
- What if we want to consider the history of a treatment or the effect of a treatment regime (i.e. a treatment that varies over time)?

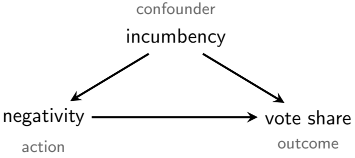
Contemporaneous, Cumulative and Dynamic Effects

- Another assumption we have been making is that our interest is in a single contemporaneous effect: $\mathbf{x}_{it}'\boldsymbol{\beta}$
- What if we want to consider the history of a treatment or the effect of a treatment regime (i.e. a treatment that varies over time)?
- Opens up new estimands, but have to think about how time-varying confounders affect treatment assignment.

Contemporaneous, Cumulative and Dynamic Effects

- Another assumption we have been making is that our interest is in a single contemporaneous effect: $\mathbf{x}_{it}'\boldsymbol{\beta}$
- What if we want to consider the history of a treatment or the effect of a treatment regime (i.e. a treatment that varies over time)?
- Opens up new estimands, but have to think about how time-varying confounders affect treatment assignment.

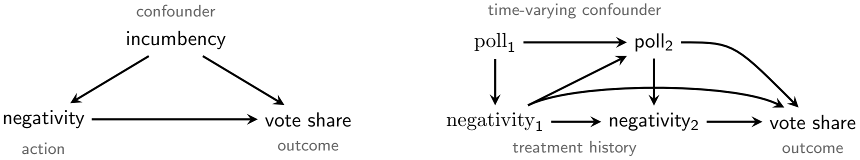
Examples of static and dynamic causal inference problems:



Contemporaneous, Cumulative and Dynamic Effects

- Another assumption we have been making is that our interest is in a single contemporaneous effect: $\mathbf{x}'_{it}\boldsymbol{\beta}$
- What if we want to consider the history of a treatment or the effect of a treatment regime (i.e. a treatment that varies over time)?
- Opens up new estimands, but have to think about how time-varying confounders affect treatment assignment.

Examples of static and dynamic causal inference problems:

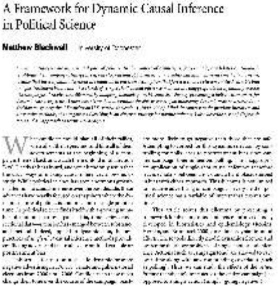


Core Conundrum

There is a (possibly irresolvable) tension: modeling causal dynamics between treatment and outcomes OR addressing unobserved time-invariant confounders.

Core Conundrum

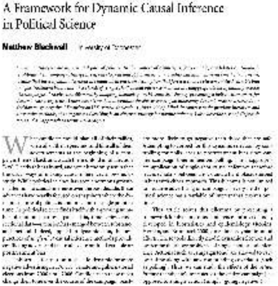
There is a (possibly irresolvable) tension: modeling causal dynamics between treatment and outcomes OR addressing unobserved time-invariant confounders. Three great recent papers:


A Framework for Dynamic Causal Inference in Political Science

Matthew Blackwell

Core Conundrum

There is a (possibly irresolvable) tension: modeling causal dynamics between treatment and outcomes OR addressing unobserved time-invariant confounders. Three great recent papers:



A Framework for Dynamic Causal Inference in Political Science

Matthew Blackwell

How to Make Causal Inferences with Time-Series Cross-Sectional Data under Selection on Observables

MATTHEW BLACKWELL Harvard University

ADAM N. GLYNN Emory University

INTRODUCTION

Core Conundrum

There is a (possibly irresolvable) tension: modeling causal dynamics between treatment and outcomes OR addressing unobserved time-invariant confounders. Three great recent papers:


A Framework for Dynamic Causal Inference in Political Science

Matthew Blackwell


How to Make Causal Inferences with Time-Series Cross-Sectional Data under Selection on Observables

MATTHEW BLACKWELL Harvard University
ADAM N. GLYNN Emory University

INTRODUCTION


AJPS

When Should We Use Unit Fixed Effects Regression Models for Causal Inference with Longitudinal Data?

Core Conundrum

There is a (possibly irresolvable) tension: modeling causal dynamics between treatment and outcomes OR addressing unobserved time-invariant confounders. Three great recent papers:

We are going to focus on addressing unobserved time-invariant confounders using the last paper.

Next several slides are based on slides graciously provided by In Song Kim and Kosuke Imai.

Directed Acyclic Graph (DAG)

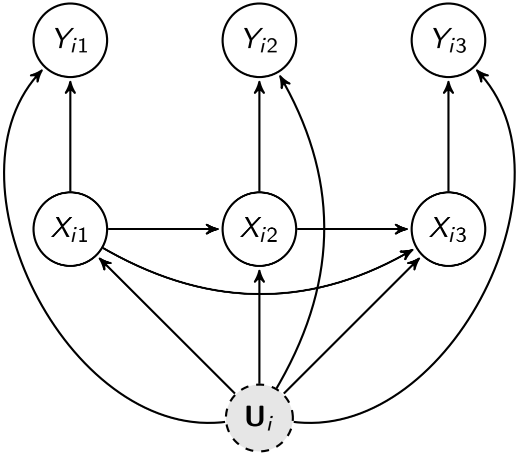
Non-parametric identification assumptions for fixed effects:

$$Y_{it} \quad = \quad g(X_{it}, \mathbf{U}_i, \epsilon_{it}) \quad \text{and} \quad \epsilon_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{U}_i\}$$

Directed Acyclic Graph (DAG)

Non-parametric identification assumptions for fixed effects:

$$Y_{it} \quad = \quad g(X_{it}, \mathbf{U}_i, \epsilon_{it}) \quad \text{and} \quad \epsilon_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{U}_i\}$$

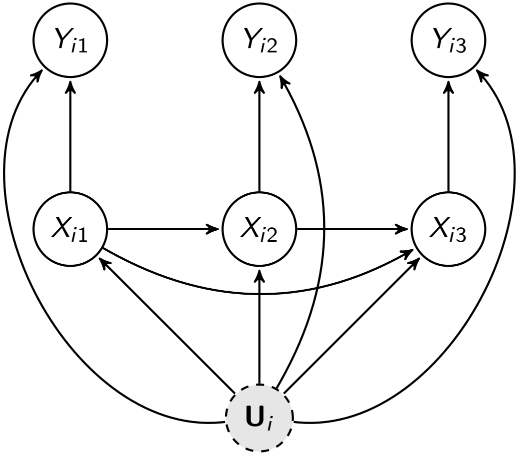


Assumptions:

Directed Acyclic Graph (DAG)

Non-parametric identification assumptions for fixed effects:

$$Y_{it} \quad = \quad g(X_{it}, \mathbf{U}_i, \epsilon_{it}) \quad \text{and} \quad \epsilon_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{U}_i\}$$



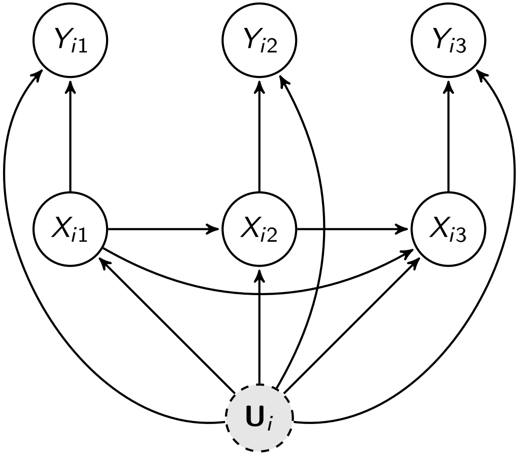
Assumptions:

1. No unobserved time-varying confounders

Directed Acyclic Graph (DAG)

Non-parametric identification assumptions for fixed effects:

$$Y_{it} \quad = \quad g(X_{it}, \mathbf{U}_i, \epsilon_{it}) \quad \text{and} \quad \epsilon_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{U}_i\}$$



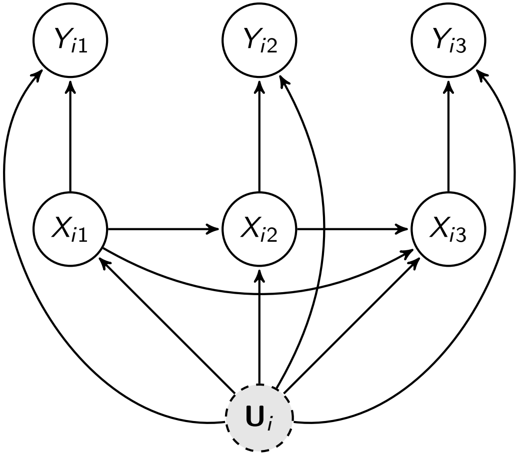
Assumptions:

1. No unobserved time-varying confounders
2. Past outcomes do not directly affect current outcome

Directed Acyclic Graph (DAG)

Non-parametric identification assumptions for fixed effects:

$$Y_{it} \quad = \quad g(X_{it}, \mathbf{U}_i, \epsilon_{it}) \quad \text{and} \quad \epsilon_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{U}_i\}$$



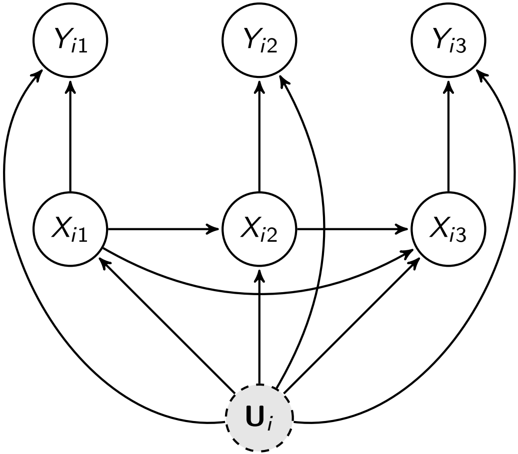
Assumptions:

1. No unobserved time-varying confounders
2. Past outcomes do not directly affect current outcome
3. Past outcomes do not directly affect current treatment

Directed Acyclic Graph (DAG)

Non-parametric identification assumptions for fixed effects:

$$Y_{it} = g(X_{it}, \mathbf{U}_i, \epsilon_{it}) \quad \text{and} \quad \epsilon_{it} \perp\!\!\!\perp \{\mathbf{X}_i, \mathbf{U}_i\}$$



Assumptions:

1. No unobserved time-varying confounders
2. Past outcomes do not directly affect current outcome
3. Past outcomes do not directly affect current treatment
4. Past treatments do not directly affect current outcome

the result implies that the counterfactual outcome for a treated observation in a given time period is estimated using the observed outcomes of different time periods of the same unit. Since such a comparison is valid only when no causal dynamics exist, this finding underscores the important limitation of linear regression models with unit fixed effects.

- Imai and Kim (2019)

What Ideal Experiment Corresponds to the Fixed Effects Model?

- Experiment that satisfies the model assumptions:

What Ideal Experiment Corresponds to the Fixed Effects Model?

- Experiment that satisfies the model assumptions:
  1. randomize X_{i1} given $\mathbf{U}_i$

What Ideal Experiment Corresponds to the Fixed Effects Model?

- Experiment that satisfies the model assumptions:
  1. randomize X_{i1} given $\mathbf{U}_i$
  2. randomize X_{i2} given X_{i1} and $\mathbf{U}_i$

What Ideal Experiment Corresponds to the Fixed Effects Model?

- Experiment that satisfies the model assumptions:
  1. randomize X_{i1} given $\mathbf{U}_i$
  2. randomize X_{i2} given X_{i1} and $\mathbf{U}_i$
  3. randomize X_{i3} given X_{i2}, X_{i1}, and $\mathbf{U}_i$
  4. and so on

What Ideal Experiment Corresponds to the Fixed Effects Model?

- Experiment that satisfies the model assumptions:
  1. randomize X_{i1} given $\mathbf{U}_i$
  2. randomize X_{i2} given X_{i1} and $\mathbf{U}_i$
  3. randomize X_{i3} given X_{i2}, X_{i1}, and $\mathbf{U}_i$
  4. and so on
- Experiment that does not satisfy the model assumptions:

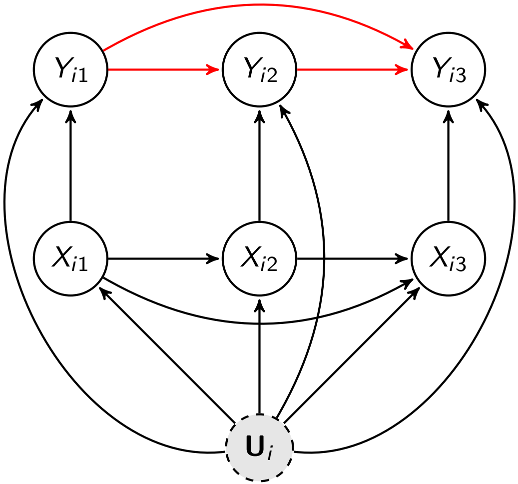
What Ideal Experiment Corresponds to the Fixed Effects Model?

- Experiment that satisfies the model assumptions:
  1. randomize X_{i1} given $\mathbf{U}_i$
  2. randomize X_{i2} given X_{i1} and $\mathbf{U}_i$
  3. randomize X_{i3} given X_{i2}, X_{i1}, and $\mathbf{U}_i$
  4. and so on
- Experiment that does not satisfy the model assumptions:
  1. randomize X_{i1}
  2. randomize X_{i2} given X_{i1} and Y_{i1}
  3. randomize X_{i3} given X_{i2}, X_{i1}, Y_{i1}, and Y_{i2}
  4. and so on

What Ideal Experiment Corresponds to the Fixed Effects Model?

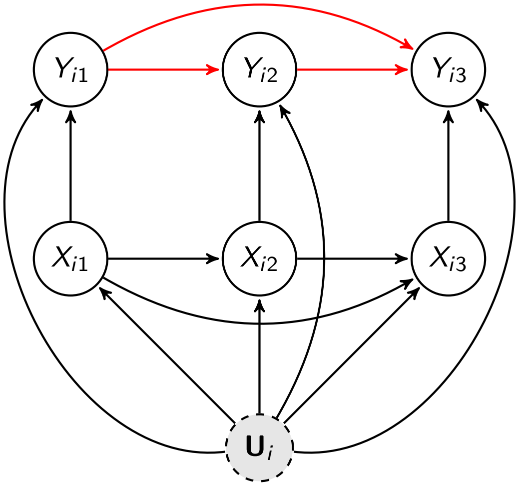
- Experiment that satisfies the model assumptions:
  1. randomize X_{i1} given $\mathbf{U}_i$
  2. randomize X_{i2} given X_{i1} and $\mathbf{U}_i$
  3. randomize X_{i3} given X_{i2}, X_{i1}, and $\mathbf{U}_i$
  4. and so on
- Experiment that does not satisfy the model assumptions:
  1. randomize X_{i1}
  2. randomize X_{i2} given X_{i1} and Y_{i1}
  3. randomize X_{i3} given X_{i2}, X_{i1}, Y_{i1}, and Y_{i2}
  4. and so on
- Now let's consider each assumption in turn.

Past Outcomes Don't Directly Affect Current Outcome (A2)



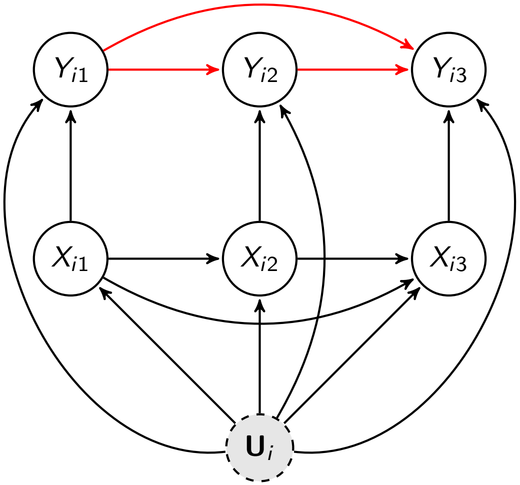
- Strict exogeneity still holds.

Past Outcomes Don't Directly Affect Current Outcome (A2)



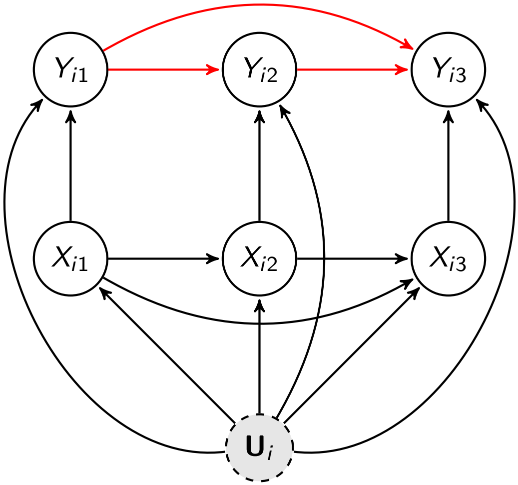
- Strict exogeneity still holds.
- Past outcomes do not confound $X_{it} \longrightarrow Y_{it}$ given $\mathbf{U}_i$.

Past Outcomes Don't Directly Affect Current Outcome (A2)



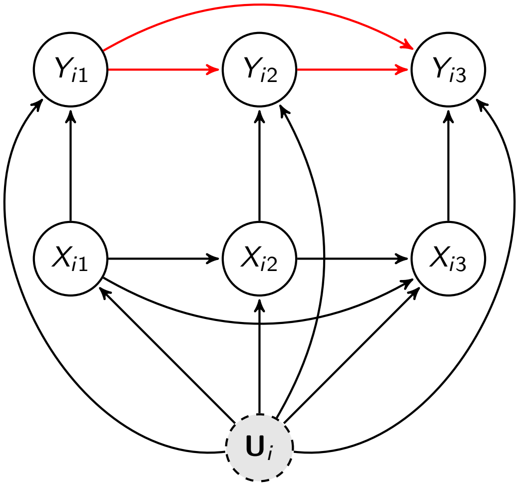
- Strict exogeneity still holds.
- Past outcomes do not confound $X_{it} \longrightarrow Y_{it}$ given $\mathbf{U}_i$.
- No need to adjust for past outcomes.

Past Outcomes Don't Directly Affect Current Outcome (A2)



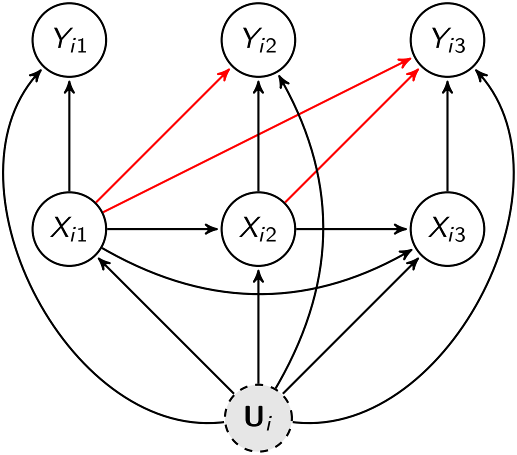
- Strict exogeneity still holds.
- Past outcomes do not confound $X_{it} \longrightarrow Y_{it}$ given $\mathbf{U}_i$.
- No need to adjust for past outcomes.
- Should use cluster robust standard errors for inference.

Past Outcomes Don't Directly Affect Current Outcome (A2)



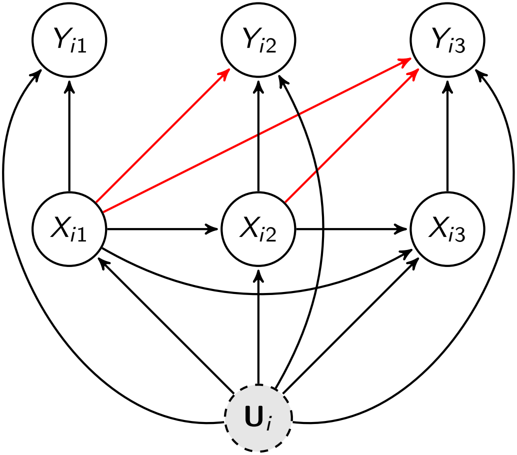
- Strict exogeneity still holds.
- Past outcomes do not confound $X_{it} \longrightarrow Y_{it}$ given $\mathbf{U}_i$.
- No need to adjust for past outcomes.
- Should use cluster robust standard errors for inference.
- Conclusion: The assumption can be relaxed

Past Treatments Don't Directly Affect Current Outcome (A4)



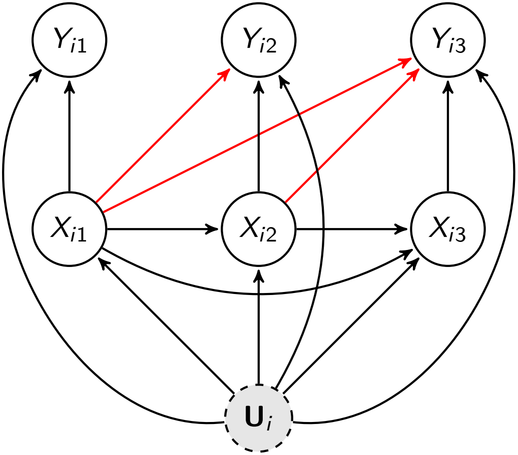
- Need to adjust for past treatments

Past Treatments Don't Directly Affect Current Outcome (A4)



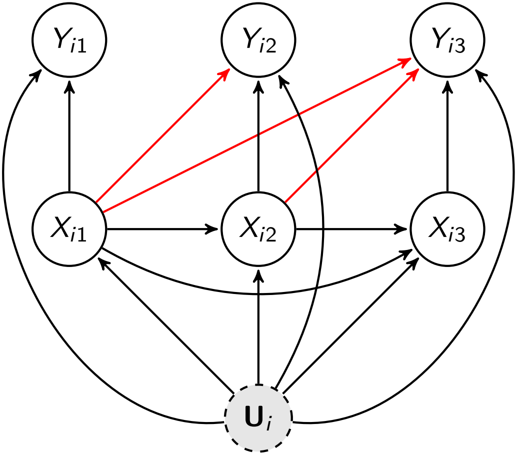
- Need to adjust for past treatments
- Strict exogeneity holds given past treatments and $\mathbf{U}_i$

Past Treatments Don't Directly Affect Current Outcome (A4)



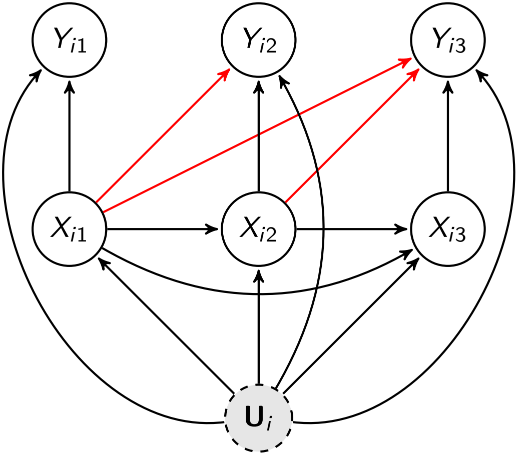
- Need to adjust for past treatments
- Strict exogeneity holds given past treatments and $\mathbf{U}_i$
- Impossible to adjust for an entire treatment history and $\mathbf{U}_i$ at the same time

Past Treatments Don't Directly Affect Current Outcome (A4)



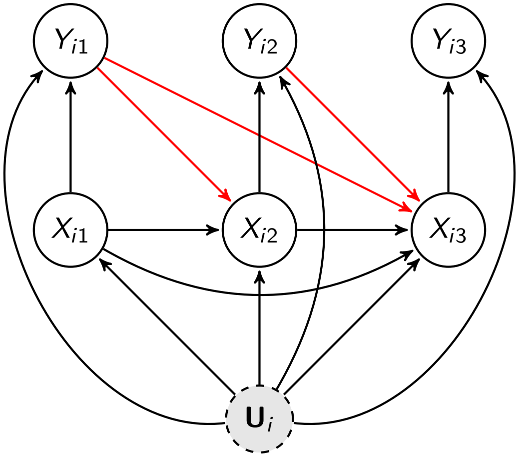
- Need to adjust for past treatments
- Strict exogeneity holds given past treatments and $\mathbf{U}_i$
- Impossible to adjust for an entire treatment history and $\mathbf{U}_i$ at the same time
- Adjust for a small number of past treatments $\rightsquigarrow$ often arbitrary

Past Treatments Don't Directly Affect Current Outcome (A4)

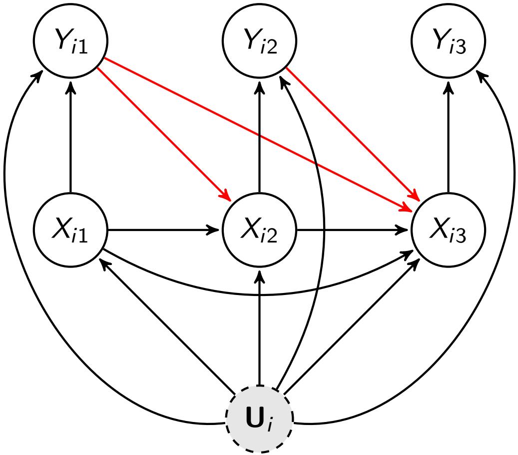


- Need to adjust for past treatments
- Strict exogeneity holds given past treatments and $\mathbf{U}_i$
- Impossible to adjust for an entire treatment history and $\mathbf{U}_i$ at the same time
- Adjust for a small number of past treatments $\rightsquigarrow$ often arbitrary
- Conclusion: The assumption can be partially relaxed

Past Outcomes Don't Directly Affect Current Treatment (A3)

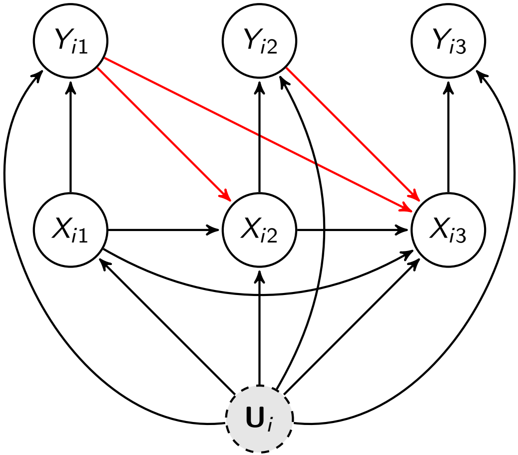


Past Outcomes Don't Directly Affect Current Treatment (A3)



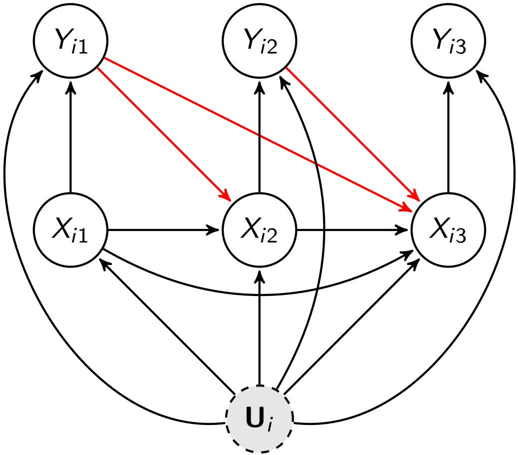
- Correlation between error term and future treatments

Past Outcomes Don't Directly Affect Current Treatment (A3)



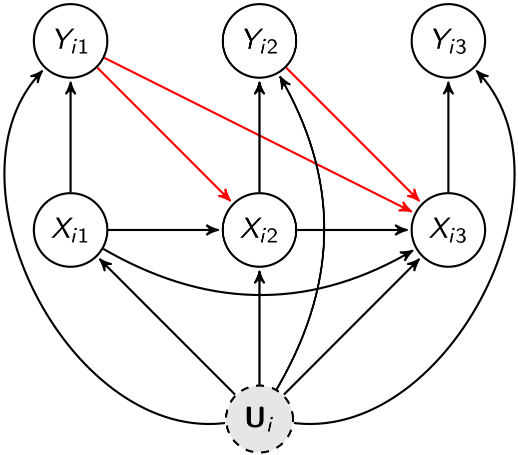
- Correlation between error term and future treatments
- Violation of strict exogeneity

Past Outcomes Don't Directly Affect Current Treatment (A3)



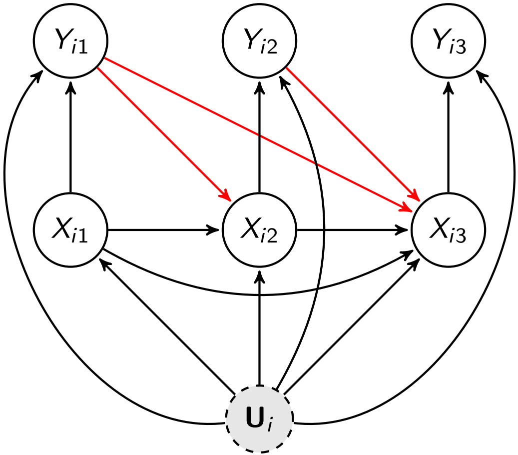
- Correlation between error term and future treatments
- Violation of strict exogeneity
- No adjustment is sufficient

Past Outcomes Don't Directly Affect Current Treatment (A3)



- Correlation between error term and future treatments
- Violation of strict exogeneity
- No adjustment is sufficient
- Implication: No dynamic causal relationships between treatment and outcome variables

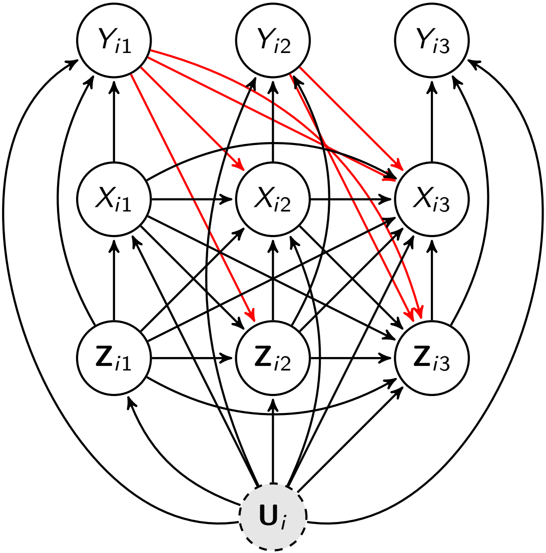
Past Outcomes Don't Directly Affect Current Treatment (A3)



- Correlation between error term and future treatments
- Violation of strict exogeneity
- No adjustment is sufficient
- Implication: No dynamic causal relationships between treatment and outcome variables
- Conclusion: The assumption cannot be relaxed

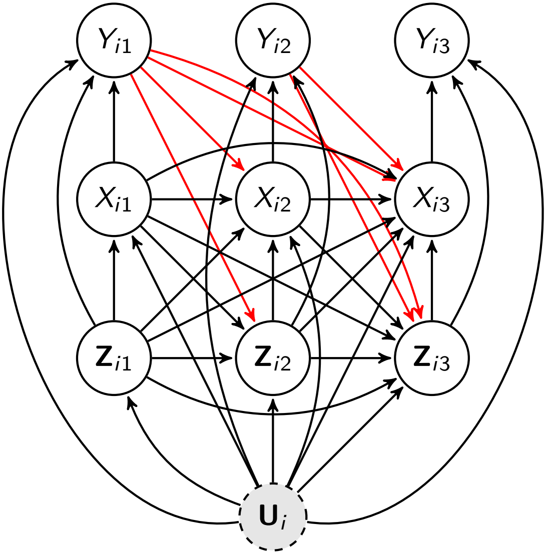
Can't We Just Adjust for Time-Varying Confounders?

Can't We Just Adjust for Time-Varying Confounders?



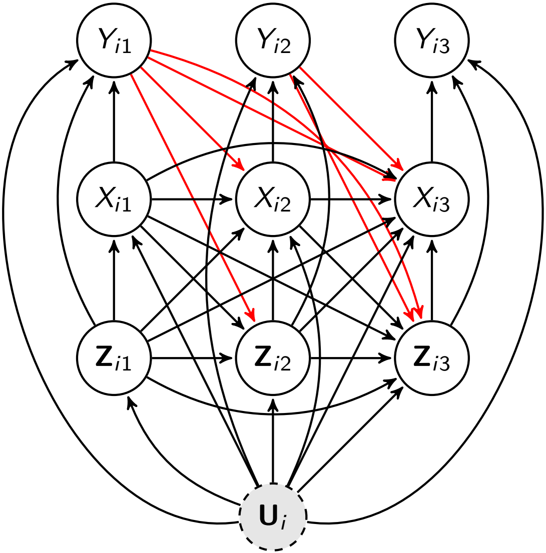
- $Y_{it} = \alpha_i + \beta X_{it} + \gamma^\top \mathbf{Z}_{it} + \epsilon_{it}$

Can't We Just Adjust for Time-Varying Confounders?



- $Y_{it} = \alpha_i + \beta X_{it} + \gamma^\top \mathbf{Z}_{it} + \epsilon_{it}$
- past outcomes cannot directly affect current treatment

Can't We Just Adjust for Time-Varying Confounders?



- $Y_{it} = \alpha_i + \beta X_{it} + \gamma^\top \mathbf{Z}_{it} + \epsilon_{it}$
- past outcomes cannot directly affect current treatment
- past outcomes cannot *indirectly* affect current treatment through $\mathbf{Z}_{it}$

But What If I Have Causal Dynamics?

But What If I Have Causal Dynamics?

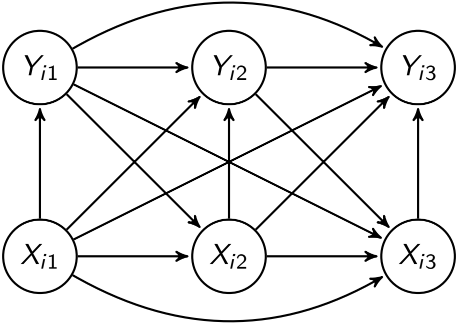
Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000)

But What If I Have Causal Dynamics?

Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.

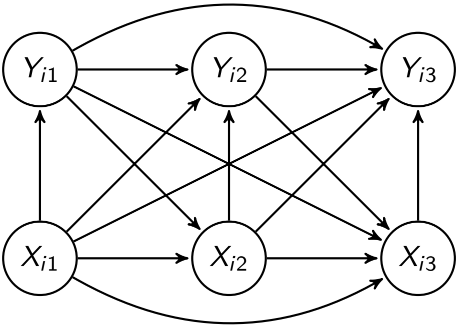
But What If I Have Causal Dynamics?

Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.



But What If I Have Causal Dynamics?

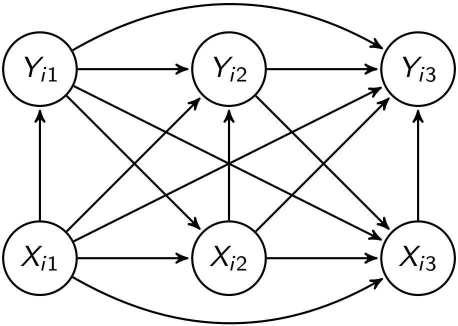
Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.



- Absence of unobserved time-invariant confounders $\mathbf{U}_i$

But What If I Have Causal Dynamics?

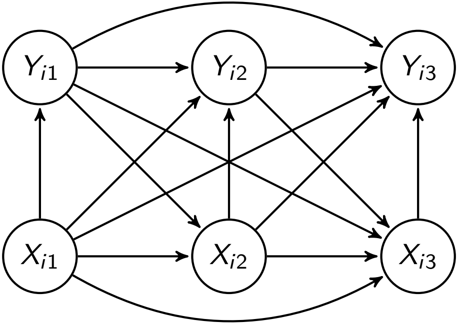
Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.



- Absence of unobserved time-invariant confounders $\mathbf{U}_i$
- past treatments can directly affect current outcome

But What If I Have Causal Dynamics?

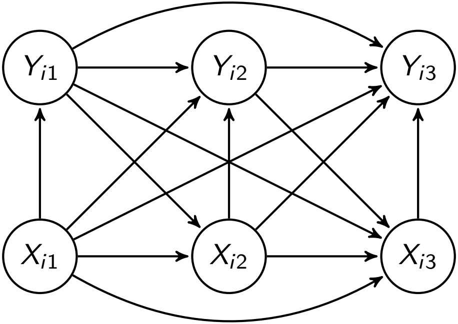
Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.



- Absence of unobserved time-invariant confounders $\mathbf{U}_i$
- past treatments can directly affect current outcome
- past outcomes can directly affect current treatment

But What If I Have Causal Dynamics?

Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.

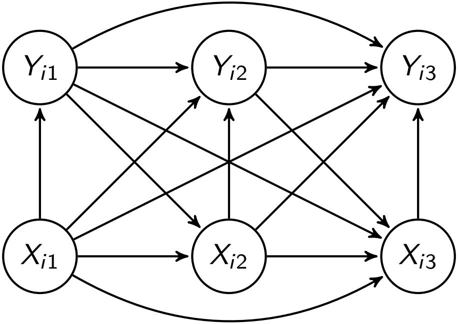


- Absence of unobserved time-invariant confounders $\mathbf{U}_i$
- past treatments can directly affect current outcome
- past outcomes can directly affect current treatment

- Comparison across units within the same time rather than across different time periods within the same unit

But What If I Have Causal Dynamics?

Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.

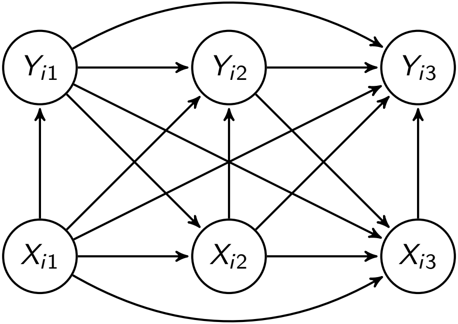


- Absence of unobserved time-invariant confounders $\mathbf{U}_i$
- past treatments can directly affect current outcome
- past outcomes can directly affect current treatment

- Comparison across units within the same time rather than across different time periods within the same unit
- Can identify the average effect of an entire treatment sequence

But What If I Have Causal Dynamics?

Alternative: Marginal Structural Models (Robins, Hernán and Brumback, 2000) — see Blackwell 2013 and Blackwell and Glynn 2018 for accessible introductions.



- Absence of unobserved time-invariant confounders $\mathbf{U}_i$
- past treatments can directly affect current outcome
- past outcomes can directly affect current treatment

- Comparison across units within the same time rather than across different time periods within the same unit
- Can identify the average effect of an entire treatment sequence
- Trade-off $\rightsquigarrow$ no free lunch

Conclusions and Nonparametric Estimation

Conclusions and Nonparametric Estimation

- Imai and Kim (2019) offer a matching framework for fixed effects models which exploits an equivalence to weighted unit fixed effects estimators (see `wfe` package in `R` as well).

Conclusions and Nonparametric Estimation

- Imai and Kim (2019) offer a matching framework for fixed effects models which exploits an equivalence to weighted unit fixed effects estimators (see `wfe` package in `R` as well).
- The paper clarifies assumptions for fixed effects and first difference estimators.

Conclusions and Nonparametric Estimation

- Imai and Kim (2019) offer a matching framework for fixed effects models which exploits an equivalence to weighted unit fixed effects estimators (see `wfe` package in `R` as well).
- The paper clarifies assumptions for fixed effects and first difference estimators.
- Follow-up papers Imai and Kim (2021) and Imai, Kim and Wang (2022+) extends to two-way fixed effects estimator.

Conclusions and Nonparametric Estimation

- Imai and Kim (2019) offer a matching framework for fixed effects models which exploits an equivalence to weighted unit fixed effects estimators (see `wfe` package in `R` as well).
- The paper clarifies assumptions for fixed effects and first difference estimators.
- Follow-up papers Imai and Kim (2021) and Imai, Kim and Wang (2022+) extends to two-way fixed effects estimator.
- Tradeoff:
  1) unobserved time-invariant confounders $\rightsquigarrow$ fixed effects

Conclusions and Nonparametric Estimation

- Imai and Kim (2019) offer a matching framework for fixed effects models which exploits an equivalence to weighted unit fixed effects estimators (see `wfe` package in `R` as well).
- The paper clarifies assumptions for fixed effects and first difference estimators.
- Follow-up papers Imai and Kim (2021) and Imai, Kim and Wang (2022+) extends to two-way fixed effects estimator.
- Tradeoff:
  1) unobserved time-invariant confounders $\rightsquigarrow$ fixed effects
  2) causal dynamics between treatment and outcome $\rightsquigarrow$ selection-on-observables

Summary Table (Imai and Kim 2019)

TABLE 1 Identification Assumptions of Various Estimators

	Linearity	Time-Invariant Unobservables	Past Outcomes Affect Current Treatment	Past Treatments Affect Current Outcome
$Y_{it} = \alpha_i + \beta X_{it} + \epsilon_{it}$	Yes	Allowed	Not allowed	Not allowed
$Y_{it} = \alpha_i + \beta X_{it} + \rho Y_{i,t-1} + \epsilon_{it}$	Yes	Allowed	Allowed	Not allowed
$Y_{it} = \alpha_i + \beta_1 X_{it} + \beta_2 X_{i,t-1} + \epsilon_{it}$	Yes	Allowed	Not allowed	Allowed
$Y_{it} = \alpha_i + \beta_1 X_{it} + \beta_2 X_{i,t-1} + \rho Y_{i,t-1} + \epsilon_{it}$	Yes	Allowed	Partially allowed	Partially allowed
Marginal structural models	No	Not allowed	Allowed	Allowed

What to read next?

- Morgan and Winship Chapter 11 Repeated Observations and the Estimation of Causal Effects
- Imai and Kim (2019) "When Should We Use Unit Fixed Effects Regression Models for Causal Inference with Longitudinal Data?" *American Journal of Political Science*, `http://dx.doi.org/10.1111/ajps.12417`
- Liu, Wang and Xu "A Practical Guide to Counterfactual Estimators for Causal Inference with Time-Series Cross-Sectional Data" *American Journal of Political Science*.

Q: What conditions do we need to infer causality?

Q: What conditions do we need to infer causality?

A: A clear estimand, an identification strategy and an estimation strategy.

Identification Strategies in This Class

Identification Strategies in This Class

- Experiments (ignorability via randomization)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)
- Natural Experiments (ignorability via quasi-randomization)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)
- Natural Experiments (ignorability via quasi-randomization)
- Interrupted Time Series (forecasting)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)
- Natural Experiments (ignorability via quasi-randomization)
- Interrupted Time Series (forecasting)
- Instrumental Variables (instrument + exclusion restriction)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)
- Natural Experiments (ignorability via quasi-randomization)
- Interrupted Time Series (forecasting)
- Instrumental Variables (instrument + exclusion restriction)
- Regression Discontinuity (continuity assumption)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)
- Natural Experiments (ignorability via quasi-randomization)
- Interrupted Time Series (forecasting)
- Instrumental Variables (instrument + exclusion restriction)
- Regression Discontinuity (continuity assumption)
- Difference-in-Differences / Synthetic Controls (parallel trends)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)
- Natural Experiments (ignorability via quasi-randomization)
- Interrupted Time Series (forecasting)
- Instrumental Variables (instrument + exclusion restriction)
- Regression Discontinuity (continuity assumption)
- Difference-in-Differences / Synthetic Controls (parallel trends)
- Fixed Effects (time-invariant unobserved heterogeneity, strict ignorability)

Identification Strategies in This Class

- Experiments (ignorability via randomization)
- Selection on Observables (conditional ignorability)
- Natural Experiments (ignorability via quasi-randomization)
- Interrupted Time Series (forecasting)
- Instrumental Variables (instrument + exclusion restriction)
- Regression Discontinuity (continuity assumption)
- Difference-in-Differences / Synthetic Controls (parallel trends)
- Fixed Effects (time-invariant unobserved heterogeneity, strict ignorability)

Almost everything assumes: consistency/SUTVA (no interference between units, variation in the treatment is irrelevant) and positivity (there is some chance of all getting treatment)

Some Estimation Strategies

Some Estimation Strategies

- Stratification

Some Estimation Strategies

- Stratification
- Regression (and relatives)

Some Estimation Strategies

- Stratification
- Regression (and relatives)
- Matching (not covered)
- Weighting (not covered)

Q: What perspective do you want us to take away from the class?

Q: What perspective do you want us to take away from the class?

- State your target estimand before getting into estimation.

Q: What perspective do you want us to take away from the class?

- State your target estimand before getting into estimation.
- Make explicit how your evidence relates to your argument both in terms of what the unit-specific quantity it is and which units it applies to.

Q: What perspective do you want us to take away from the class?

- State your target estimand before getting into estimation.
- Make explicit how your evidence relates to your argument both in terms of what the unit-specific quantity it is and which units it applies to.
- Know what you can defend with data and what you have to argue for by assumption and don't confuse the two!

Q: What perspective do you want us to take away from the class?

- State your target estimand before getting into estimation.
- Make explicit how your evidence relates to your argument both in terms of what the unit-specific quantity it is and which units it applies to.
- Know what you can defend with data and what you have to argue for by assumption and don't confuse the two!
- Be at least a bit skeptical of model-based inference: think about the non-parametric object you are approximating.

Q: What perspective do you want us to take away from the class?

- State your target estimand before getting into estimation.
- Make explicit how your evidence relates to your argument both in terms of what the unit-specific quantity it is and which units it applies to.
- Know what you can defend with data and what you have to argue for by assumption and don't confuse the two!
- Be at least a bit skeptical of model-based inference: think about the non-parametric object you are approximating.
- You can look under the hood for why/under what conditions things work, rules of thumb or textbook guidance do not universally: there is generally no single right answer to how you should analyze some data.

Q: What perspective do you want us to take away from the class?

- State your target estimand before getting into estimation.
- Make explicit how your evidence relates to your argument both in terms of what the unit-specific quantity it is and which units it applies to.
- Know what you can defend with data and what you have to argue for by assumption and don't confuse the two!
- Be at least a bit skeptical of model-based inference: think about the non-parametric object you are approximating.
- You can look under the hood for why/under what conditions things work, rules of thumb or textbook guidance do not universally: there is generally no single right answer to how you should analyze some data.

See also 'What is Your Estimand?' with Lundberg and Johnson *ASR*, 2021.

Q: What can make standard errors larger or smaller?

Q: What can make standard errors larger or smaller?

A: Let's consider the anatomy of a standard error.

Anatomy of the Standard Error

Imagine we have a regression of Y on a variable of interest X and a vector of other variables $\mathbf{Z}$. Let's consider the homoskedastic case for simplicity:

$$\widehat{\text{Var}}(\widehat{\beta}_X) = \frac{\frac{1}{(n-k-1)}\sum_{i=1}^{n}\hat{u}_i^2}{(1 - R^2_{X\sim\mathbf{Z}})\sum_{i=1}^{n}(X_i - \overline{X})^2}$$

Anatomy of the Standard Error

Imagine we have a regression of Y on a variable of interest X and a vector of other variables $\mathbf{Z}$. Let's consider the homoskedastic case for simplicity:

$$\widehat{\text{Var}}(\widehat{\beta}_X) = \frac{\frac{1}{(n-k-1)} \sum_{i=1}^{n} \hat{u}_i^2}{(1 - R^2_{X\sim\mathbf{Z}}) \sum_{i=1}^{n}(X_i - \overline{X})^2}$$

- the numerator is our estimator for σ_u^2 the unknown error variance. It is formed by the degrees of freedom correction times the sum of the squared residuals.

Anatomy of the Standard Error

Imagine we have a regression of Y on a variable of interest X and a vector of other variables $\mathbf{Z}$. Let's consider the homoskedastic case for simplicity:

$$\widehat{\text{Var}}(\widehat{\beta}_X) = \frac{\frac{1}{(n-k-1)}\sum_{i=1}^{n}\hat{u}_i^2}{(1 - R^2_{X\sim\mathbf{Z}})\sum_{i=1}^{n}(X_i - \overline{X})^2}$$

- the numerator is our estimator for σ_u^2 the unknown error variance. It is formed by the degrees of freedom correction times the sum of the squared residuals.
- the denominator includes one minus the R^2 from the regression of X_i on $\mathbf{Z}_i$

Anatomy of the Standard Error

Imagine we have a regression of Y on a variable of interest X and a vector of other variables $\mathbf{Z}$. Let's consider the homoskedastic case for simplicity:

$$\widehat{\text{Var}}(\widehat{\beta}_X) = \frac{\frac{1}{(n-k-1)}\sum_{i=1}^{n}\hat{u}_i^2}{(1-R^2_{X\sim\mathbf{Z}})\sum_{i=1}^{n}(X_i-\overline{X})^2}$$

- the numerator is our estimator for σ_u^2 the unknown error variance. It is formed by the degrees of freedom correction times the sum of the squared residuals.
- the denominator includes one minus the R^2 from the regression of X_i on $\mathbf{Z}_i$
- we complete the denominator by multiplying a measure of the variation in X_i, the sum of squared deviations from the mean.

Anatomy of the Standard Error

Imagine we have a regression of Y on a variable of interest X and a vector of other variables $\mathbf{Z}$. Let's consider the homoskedastic case for simplicity:

$$\widehat{\text{Var}}(\widehat{\beta}_X) = \frac{\frac{1}{(n-k-1)}\sum_{i=1}^{n}\hat{u}_i^2}{(1-R^2_{X\sim\mathbf{Z}})\sum_{i=1}^{n}(X_i-\overline{X})^2}$$

- the numerator is our estimator for σ_u^2 the unknown error variance. It is formed by the degrees of freedom correction times the sum of the squared residuals.
- the denominator includes one minus the R^2 from the regression of X_i on $\mathbf{Z}_i$
- we complete the denominator by multiplying a measure of the variation in X_i, the sum of squared deviations from the mean.

$$\widehat{\text{SE}}(\widehat{\beta}_X) = \sqrt{\widehat{\text{Var}}(\widehat{\beta}_X)}$$

Q: When conducting an experiment, should we avoid OLS and always go for difference in means?

Q: When conducting an experiment, should we avoid OLS and always go for difference in means?

A: Regression adjustment of experiments can be helpful for improving precision. We don't need it for confounding, but it can improve our standard errors to adjust for pre-treatment covariates that are highly predictive of the output. If done correctly and in moderate-to-large samples, this can dramatically improve your standard errors. Even better though is blocking which is adjustment by design.

Q: When conducting an experiment, should we avoid OLS and always go for difference in means?

A: Regression adjustment of experiments can be helpful for improving precision. We don't need it for confounding, but it can improve our standard errors to adjust for pre-treatment covariates that are highly predictive of the output. If done correctly and in moderate-to-large samples, this can dramatically improve your standard errors. Even better though is blocking which is adjustment by design.

Further Reading:

- Lin, W., 2013. 'Agnostic notes on regression adjustments to experimental data: Reexamining Freedman's critique.' *The Annals of Applied Statistics*
- Athey, S. and Imbens, G.W., 2017. 'The Econometrics of Randomized Experiments.' In *Handbook of Economic Field Experiments* (Vol. 1, pp. 73-140).
- Egap Methods Guide: 10 things you need to know about covariate adjustment. `https://egap.org/methods-guides/10-things-know-about-covariate-adjustment`

Q: What are your favorite resources for learning tricky concepts?

Q: What are your favorite resources for learning tricky concepts?

I've used the following procedure many times:

Q: What are your favorite resources for learning tricky concepts?

I've used the following procedure many times:

1. Identify approx. the best textbook (often can do this via syllabi hunting)

Q: What are your favorite resources for learning tricky concepts?

I've used the following procedure many times:

1. Identify approx. the best textbook (often can do this via syllabi hunting)
2. Read the relevant textbook material

Q: What are your favorite resources for learning tricky concepts?

I've used the following procedure many times:

1. Identify approx. the best textbook (often can do this via syllabi hunting)
2. Read the relevant textbook material
3. Derive the equations/math

Q: What are your favorite resources for learning tricky concepts?

I've used the following procedure many times:

1. Identify approx. the best textbook (often can do this via syllabi hunting)
2. Read the relevant textbook material
3. Derive the equations/math
4. Try to explain it to someone else

Q: In the real world of research, when you have your data, how do you know which method to use?

Q: In the real world of research, when you have your data, how do you know which method to use?

A: Let's chat.

Q: What would you have covered with more time?

Q: What would you have covered with more time?

1. Missing Data

Q: What would you have covered with more time?

1. Missing Data
2. Sensitivity Analysis

Q: What would you have covered with more time?

1. Missing Data
2. Sensitivity Analysis
3. Mediation (and Front Door Adjustment)

Q: What would you have covered with more time?

1. Missing Data
2. Sensitivity Analysis
3. Mediation (and Front Door Adjustment)
4. Bounds and Partial Identification

Q: What would you have covered with more time?

1. Missing Data
2. Sensitivity Analysis
3. Mediation (and Front Door Adjustment)
4. Bounds and Partial Identification
5. Weighting/Matching Approaches

Q: What would you have covered with more time?

1. Missing Data
2. Sensitivity Analysis
3. Mediation (and Front Door Adjustment)
4. Bounds and Partial Identification
5. Weighting/Matching Approaches
6. Quantile Regression

Q: What would you have covered with more time?

1. Missing Data
2. Sensitivity Analysis
3. Mediation (and Front Door Adjustment)
4. Bounds and Partial Identification
5. Weighting/Matching Approaches
6. Quantile Regression
7. Machine Learning and Generalizations of Regression

Q: What would you have covered with more time?

1. Missing Data
2. Sensitivity Analysis
3. Mediation (and Front Door Adjustment)
4. Bounds and Partial Identification
5. Weighting/Matching Approaches
6. Quantile Regression
7. Machine Learning and Generalizations of Regression
8. On the Ground Decision Making

1 Difference-in-Differences

2 Synthetic Controls

3 Fixed Effects

4 Non-parametric Identification and Fixed Effects

5 Wrap-Up
- Questions
- Concluding Thoughts for the Course

Where are you?

Where are you?

You've been given a powerful set of tools

Your New Weapons

Your New Weapons

- Basic probability theory
  - Probability axioms, random variables, marginal and conditional probability, building a probability model
  - Expected value, variances, independence
  - CDF and PDF (discrete and continuous)

Your New Weapons

- Basic probability theory
  - Probability axioms, random variables, marginal and conditional probability, building a probability model
  - Expected value, variances, independence
  - CDF and PDF (discrete and continuous)

- Properties of Estimators
  - Bias, Efficiency, Consistency
  - Central limit theorem

Your New Weapons

- Basic probability theory
  - Probability axioms, random variables, marginal and conditional probability, building a probability model
  - Expected value, variances, independence
  - CDF and PDF (discrete and continuous)
- Properties of Estimators
  - Bias, Efficiency, Consistency
  - Central limit theorem
- Univariate Inference
  - Interval estimation (normal and non-normal Population)
  - Confidence intervals, hypothesis tests, p-values
  - Practical versus statistical significance

Your New Weapons

Your New Weapons

- Simple Regression
  - regression to approximate the conditional expectation function
  - idea of conditioning
  - kernel and loess regressions
  - OLS estimator for bivariate regression
  - Variance decomposition, goodness of fit, interpretation of estimates, transformations

Your New Weapons

- Simple Regression
  - regression to approximate the conditional expectation function
  - idea of conditioning
  - kernel and loess regressions
  - OLS estimator for bivariate regression
  - Variance decomposition, goodness of fit, interpretation of estimates, transformations
- Multiple Regression
  - OLS estimator for multiple regression in matrix notation
  - Regression assumptions (classical and agnostic)
  - Properties: Bias, Efficiency, Consistency
  - Standard errors, testing, p-values, and confidence intervals
  - Polynomials, Interactions, Dummy Variables
  - F-tests

Your New Weapons

Your New Weapons

- Diagnosing and Fixing Regression Problems
  - Non-linear transformations
  - Classical diagnostics
  - QQ-plots
  - Clustering

Your New Weapons

- Diagnosing and Fixing Regression Problems
  - Non-linear transformations
  - Classical diagnostics
  - QQ-plots
  - Clustering
- Causal Inference
  - Frameworks: potential outcomes and DAGs
  - Measured Confounding
  - Unmeasured Confounding
  - Methods for repeated data

Your New Weapons

- Diagnosing and Fixing Regression Problems
  - Non-linear transformations
  - Classical diagnostics
  - QQ-plots
  - Clustering
- Causal Inference
  - Frameworks: potential outcomes and DAGs
  - Measured Confounding
  - Unmeasured Confounding
  - Methods for repeated data
- And you learned how to use `R`: you're not afraid of trying something new!

Using these Tools

Using these Tools

So, Admiral Ackbar, now that you've learned how to run these regressions we can just use them blindly, right?


IT'S A TRAP!

Beyond Linear Regressions

You need more training

Beyond Linear Regressions

Beyond Linear Regressions

There is so much more to learn! Take classes, read books!

Beyond Linear Regressions

There is so much more to learn! Take classes, read books!

- Take SOC 504 or the POL sequence!

Beyond Linear Regressions

There is so much more to learn! Take classes, read books!

- Take SOC 504 or the POL sequence!
- Pursue the SML certificate!

Beyond Linear Regressions

There is so much more to learn! Take classes, read books!

- Take SOC 504 or the POL sequence!
- Pursue the SML certificate!
- Go to workshops and seminars etc.!

Beyond Linear Regressions

There is so much more to learn! Take classes, read books!

- Take SOC 504 or the POL sequence!
- Pursue the SML certificate!
- Go to workshops and seminars etc.!
- Read the suggested books in the syllabus!

Beyond Linear Regressions

There is so much more to learn! Take classes, read books!

- Take SOC 504 or the POL sequence!
- Pursue the SML certificate!
- Go to workshops and seminars etc.!
- Read the suggested books in the syllabus!

Thanks!

Thanks so much for an amazing semester.