Week 12: Repeated Observations and Panel Data

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Princeton

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[1] These slides are heavily influenced by Matt Blackwell, Adam Glynn, Jens Hainmueller, and Erin Hartman.

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

- Last Week
  - causal inference with unmeasured confounding
- This Week
  - panel data
  - diff-in-diff
  - fixed effects
  - wrap-up
- The Following Week
  - ?
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causality

Motivation

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Michael Ross University of California, Los Angeles

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  - possess some cultural trait correlated with better health outcomes
- If we have data on countries over time, can we make any progress in spite of these problems?

Ross Data

```
##      cty_name year democracy infmort_unicef
## 1 Afghanistan 1965         0            230
## 2 Afghanistan 1966         0             NA
## 3 Afghanistan 1967         0             NA
## 4 Afghanistan 1968         0             NA
## 5 Afghanistan 1969         0             NA
## 6 Afghanistan 1970         0            215
```

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- NB: we won't be using T for treatment today because it is extremely consistently used for time. We will end up using D for treatment which is another common letter for treatment.

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- Panel data: large n, relatively short T
- Time series, cross-sectional (TSCS) data: smaller n, large T
- We are primarily going to focus on similarities today but there are some differences.

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We will start by considering performance of estimators assuming this model is true.

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  1. Heteroskedasticity (see clustering from diagnostics week)
  2. Possible violation of zero conditional mean errors
- Both problems arise out of ignoring the unmeasured heterogeneity inherent in a_i

Pooled OLS with Ross data

```
pooled.mod <- lm(log(kidmort_unicef) ~ democracy + log(GDPcur),
                 data = ross)
summary(pooled.mod)

##
## Coefficients:
##              Estimate Std. Error t value Pr(>|t|)
## (Intercept)  9.76405    0.34491   28.31   <2e-16 ***
## democracy   -0.95525    0.06978  -13.69   <2e-16 ***
## log(GDPcur) -0.22828    0.01548  -14.75   <2e-16 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## Residual standard error: 0.7948 on 646 degrees of freedom
##   (5773 observations deleted due to missingness)
## Multiple R-squared:  0.5044, Adjusted R-squared:  0.5029
## F-statistic: 328.7 on 2 and 646 DF,  p-value: < 2.2e-16
```

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- Pooled OLS will be biased and inconsistent because zero conditional mean error fails for the combined error.

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- Two time periods:

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&= \Delta\mathbf{x}'_i\boldsymbol{\beta} + \Delta u_i
\end{aligned}
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- Due to 'no perfect collinearity': $\mathbf{x}_{it}$ has to change over time for some units. High variance if its slow moving.
- Differencing will reduce the variation in the independent variables and thus increase standard errors.

First Differences in R (via `plm` package)

```
library(plm)

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

## Oneway (individual) effect First-Difference Model
##
## Call:
## plm(formula = log(kidmort_unicef) ~ democracy + log(GDPcur),
##     data = ross, model = "fd", index = c("id", "year"))
##
## Unbalanced Panel: n=166, T=1-7, N=649
##
## Residuals :
##    Min. 1st Qu.  Median 3rd Qu.    Max.
## -0.9060 -0.0956  0.0468  0.1410  0.3950
##
## Coefficients :
##              Estimate Std. Error  t-value Pr(>|t|)
## (intercept) -0.149469   0.011275 -13.2567  < 2e-16 ***
## democracy   -0.044887   0.024206  -1.8544  0.06429 .
## log(GDPcur) -0.171796   0.013756 -12.4886  < 2e-16 ***
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## Total Sum of Squares:    23.545
## Residual Sum of Squares: 17.762
## R-Squared      :  0.24561
##       Adj. R-Squared :  0.24408
## F-statistic: 78.1367 on 2 and 480 DF, p-value: < 2.22e-16
```

We Covered

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Next Time: Difference-in-Differences

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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

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  - Wave 1: March 1992, one month before the minimum wage increased
  - Wave 2: December 1992, eight months after increase

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- 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

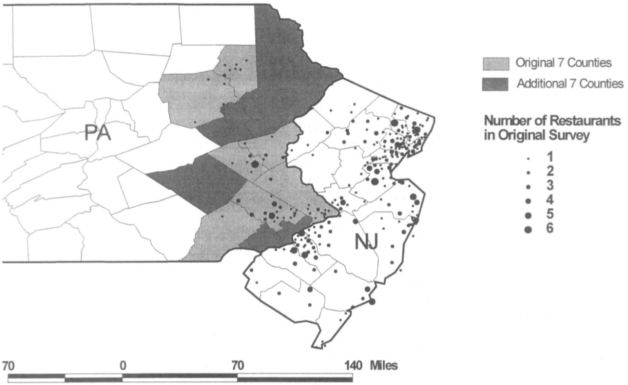

FIGURE 1. AREAS OF NEW JERSEY AND PENNSYLVANIA COVERED BY ORIGINAL SURVEY AND BLS DATA

Source: Card and Krueger 2000

Conversations / *David Card and Alan Krueger*

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

By SYLVIA NASAR

WASHINGTON

A CONTROVERSIAL memorandum from Robert Reich, the Secretary of Labor, has been making the rounds here recently. It urges the President to propose not only a rapid 25-cent increase in the $4.25 Federal minimum wage but automatic annual increases in the minimum pay of everyone from McDonald's kitchen crews to Marriott chambermaids.

That may seem a bit strange from someone who says he is a New Democrat and considers himself a champion of disadvantaged workers. Economists, neoliberals and advocates for the poor have long believed that making it more expensive for business to hire the young, the least-skilled and the least-educated means fewer jobs for people who most need a break.

But the Labor Secretary thinks otherwise — and one reason, it turns out, has a lot to do with research conducted by two young iconoclasts who have made names for themselves in academe and are now being talked about in Washington.

The two, David Card, 37 years old, and Alan Krueger, 32, are economists at Princeton University and pioneers in a new kind of survey research. Sometimes together, sometimes with collaborators like Larry Katz, Mr. Reich's right-hand economist — have found that testing the conventional wisdom on trendy topics like the minimum wage, mandated benefits and immigration can yield a rich harvest of unexpected results. Quite often, they argue, the received wisdom is simply wrong.

"Other economists think we're out here doing cold fusion," said Professor Card, a Canadian who retains many of the hard-working, conservative tenets of the farm community he grew up in. "People assume we know no economics, but we just think the conventional model should be tested."

If the two were merely academic Luddites, smashing the elegant theoretical machinery of conventional economics without creating credible alternatives, nobody in the field would pay much attention to them. But that's not the case. Professor Card ranks third in the country in terms of articles in top economics journals, and edits the profession's most prestigious mathematical journal. Professor Krueger, no slouch either, recently was the object of a bidding war between Princeton and M.I.T.

The New Jersey Test

Their studies of the minimum wage have attracted the most attention. Classic economics assumes that a higher minimum puts more money in the pockets of some low-wage workers, but forces others out of work, dismissed by companies who cannot afford to pay them. Professors Card and Krueger decided to test that when New Jersey, in the middle of a recession, raised the minimum wage in the state to more than $5.05 — 80 cents higher than neighboring Pennsylvania

They surveyed 400 fast food restaurants and found that those in New Jersey actually added 2.5 workers after the minimum wage went up. In Pennsylvania restaurants, meanwhile, payrolls shrank.

Why should bosses hire more workers if it's more expensive to do so? The two economists speculate that any fast-food restaurant typically operates with a couple of vacancies it can't fill because it doesn't pay enough.

"If you raise the minimum a little," said Professor Card, "teens who are sitting home go out looking for jobs."

Professor Krueger said the study shows that the labor market is a social institution as well. "There are lots of concerns about fairness," he said. "The conventional economic model ignores those concerns. It assumes people make decisions about where to work, how hard to work or how to treat customers on purely economic grounds."

Frank C. Dougherty for The New York Times

Princeton economists David Card, left, and Alan Krueger.

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

They also caused a stir challenging the usual thinking about benefits that the law sometimes requires employers to offer their workers — health insurance, workers compensation and the like.

Professor Krueger studied pay and benefits in the trucking industry and concluded that the workers — not the employers — are the ones who ultimately pay for the benefits. When benefit costs are pushed up by either market forces or government mandate, wages tend to go up more slowly. In states with high worker compensation coverage, pay tended to be commensurately lower.

And most economists will say that a surge in immigration depresses the wages and raises unemployment for the American working poor. But Professor Card's study of the impact of the Mariel boatlift in Florida found no effect on unskilled black workers from the influx of over 100,000 Cubans.

Their work does have critics, who say that it's trendy but that its conclusions, like the one about the minimum wage, can't be generalized. "It's really one observation," Professor Card admits. "It's always possible that there's some idiosyncratic thing that you've missed."

But even critics agree that their research is careful and applaud their desire to give economic theories more of the real-world testingthat the natural sciences have.

"Economists have this theoretical structure," Professor Card said. "The big selling point was an elegant framework that let you make logical predictions. But it's like a Greek tragedy — our strength is also our weakness. A large number of people believe that the equations really work and that you don't have to investigate the facts.

'Let's Not Assume'

"The idea used to be that if the data didn't fit the theory, there must be something wrong with your data. Now there's a younger generation that says 'Let's not assume the answer.' "

The methods the two employ, which resemble the ones used by natural scientists, also attract attention. They use control groups, for example. Other economists virtually never do, but rely on statistical methods to isolate cause and effect.

"What attracted us to control groups is that we wanted to design our research scientifically," Professor Card said. "What would a skeptic consider convincing evidence?"

Although they analyze many large government surveys, they think they get better insights by collecting their own data — as they did by telephoning or writing to 400 fast food joints. "There was a time when we thought econometric techniques would solve a lot of the data problems," Professor Krueger said. "Now I think the feeling is that there are a lot of problems for which it is easier to get better data."

They also prize — to a far greater degree than some of their generation who have taken posts in the Clinton administration — their scientific detachment. They insist, for example, that they are not prepared to tell the President, via Mr. Reich, that it's a good idea to put the Federal minimum wage on an escalator. Or that making employers buy insurance for all their workers is a bad idea.

Taking sides would conflict with their main mission — testing whether the labor market really works the way the models say. "I really view what we do as scientific research rather than partisan input into some particular policy," Professor Card said. "Suppose we say we believe that the minimum wage should be higher. A lot of people would forever think our results were rigged."

But they aren't laughing at the idea that someone might be listening.

"Milton Friedman, who never was C.E.A. chairman or Fed chairman or anything like that," Professor Krueger said, "is the most influential economist alive today."

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- β_1 is the quantity of interest: it's the effect of being treated

Diff-in-Diff Mechanics

- Let's take differences:

$$(y_{i2} - y_{i1}) = \delta_0(1-0) + \beta_1(x_{i2} - x_{i1}) + (a_i - a_i) + (u_{i2} - u_{i1})$$

- This represents
  - δ_0: the difference in the average outcome from period 1 to period 2 in the untreated group
  - $(x_{i2} - x_{i1}) = 1$ for the treated group and 0 for the control group
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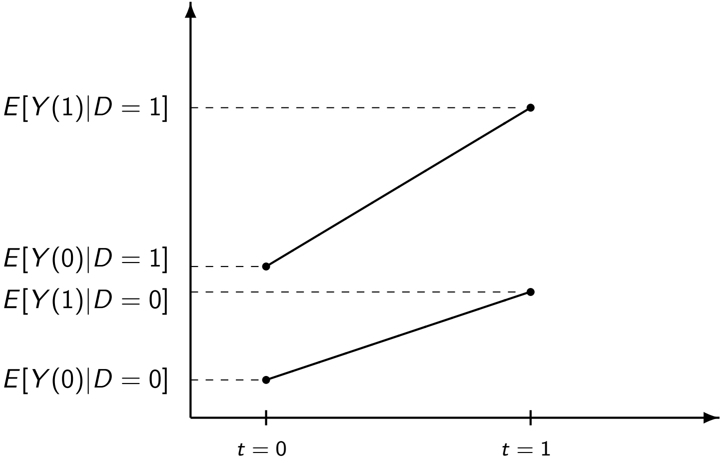
Graphical Representation: Difference-in-Differences

Define $D = 1$ when $x_{i2} - x_{i1} = 1$ and 0 otherwise



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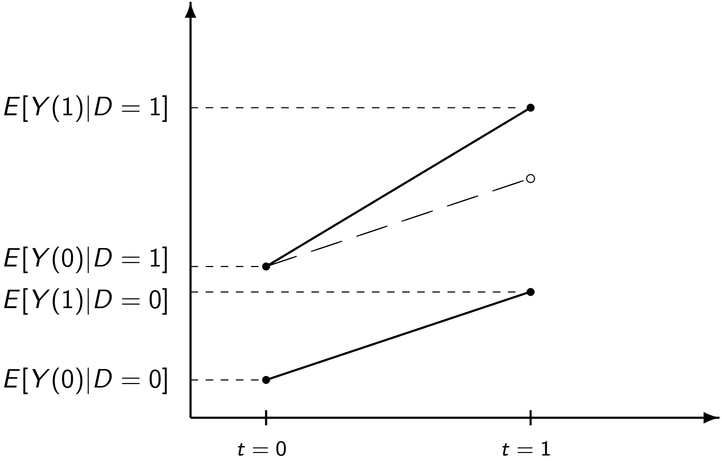

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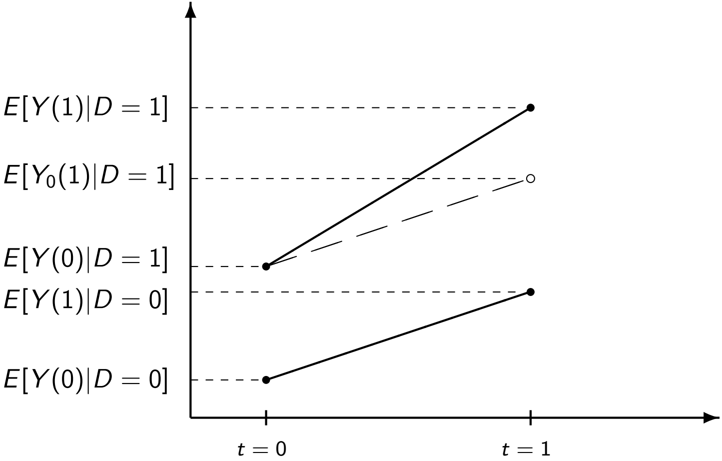

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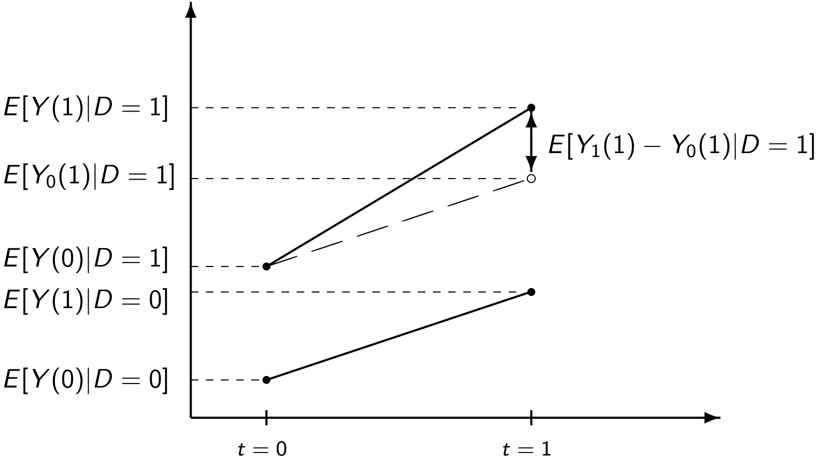

Identification with Difference-in-Differences

Identification Assumption (parallel trends)

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

Identification Result

Given parallel trends the ATT is identified as:

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

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

$$
\begin{aligned}
& \{E[Y(1)|D = 1] - E[Y(1)|D = 0]\} - \{E[Y(0)|D = 1] - E[Y(0)|D = 0]\} \\
= & \{E[Y_1(1)|D = 1] - E[Y_0(1)|D = 0]\} - \{E[Y_0(0)|D = 1] - E[Y_0(0)|D = 0]\} \\
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- & \{E[Y_0(0)|D = 1] - E[Y_0(0)|D = 0]\} \\
= & E[Y_1(1) - Y_0(1)|D = 1] + \{E[Y_0(0)|D = 1] - E[Y_0(0)|D = 0]\} \\
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= & E[Y_1(1) - Y_0(1)|D = 1]
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- DiD works for additive and time-invariant confounding (i.e. satisfies parallel trends)

Example: Lyall (2009)

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Does Indiscriminate Violence Incite Insurgent Attacks?

Evidence from Chechnya

Jason Lyall
Department of Politics and the Woodrow Wilson School
Princeton University, New Jersey

Example: Lyall (2009)

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- Counterintuitive findings: shelled villages experience a 24% reduction in insurgent attacks relative to controls.

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

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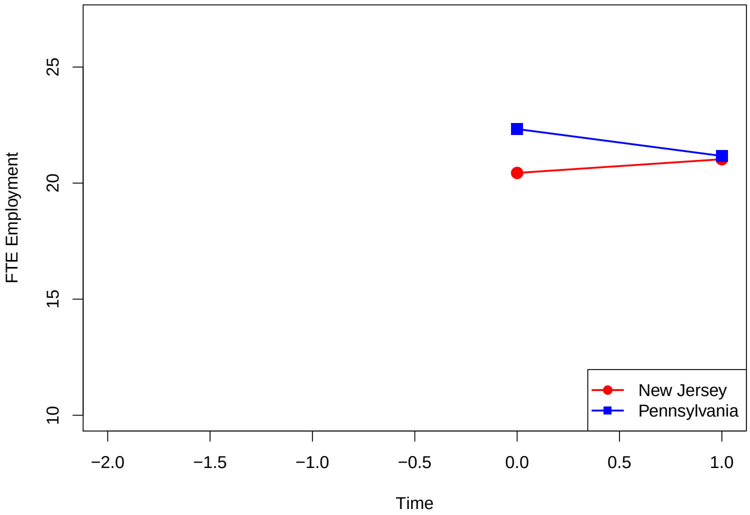

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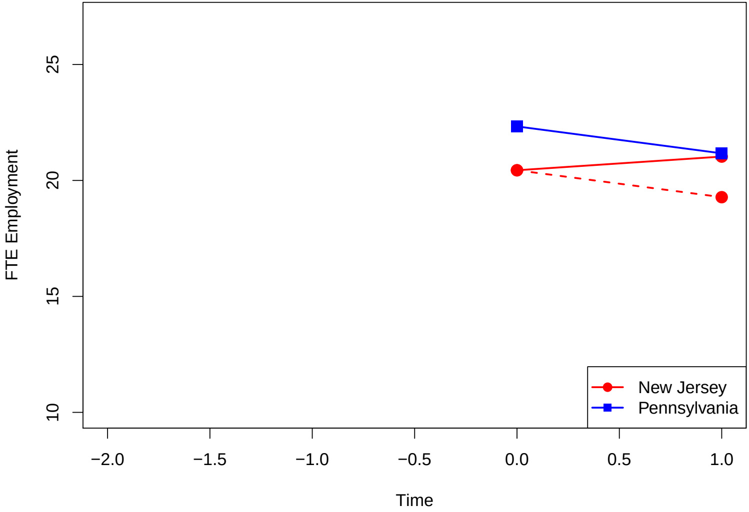

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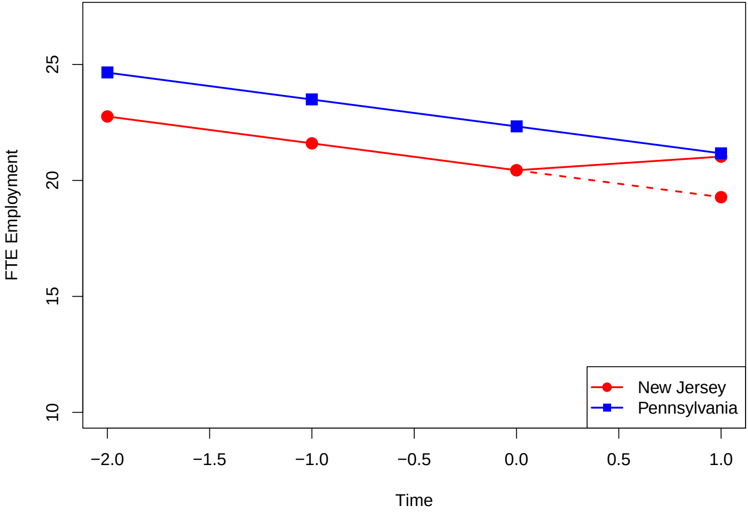

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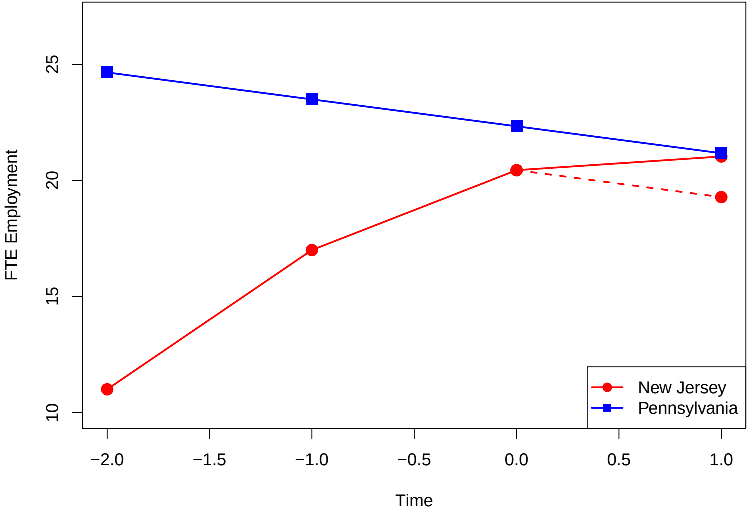
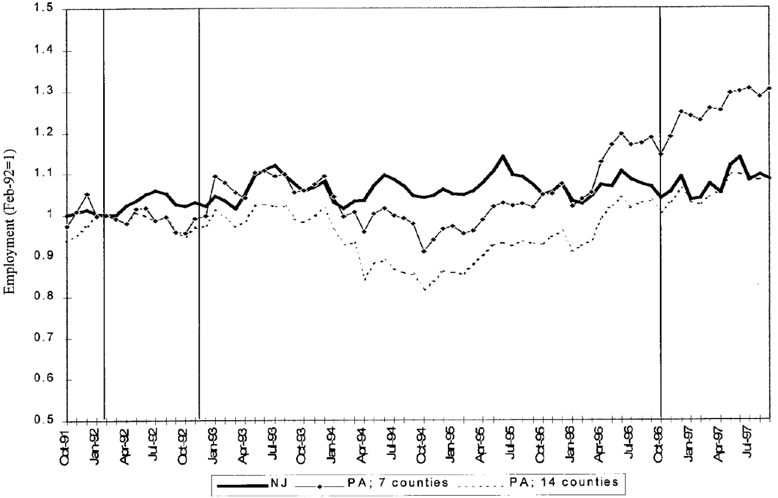


Longer Trends in Employment (Card and Krueger 2000)



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

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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.

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6) Endogenous Control Variables
can add (time-varying) covariates to help with some of above concerns $\rightsquigarrow$ "regression diff-in-diff"

$$y_{i2} - y_{i1} = \delta_0 + \mathbf{z}_i'\tau + \beta(x_{i2} - x_{i1}) + (u_{i2} - u_{i1})$$

but need to be careful that they aren't affected by the treatment.

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What to read next?

- Angrist and Pishke Chapter 5 Parallel Worlds: Fixed Effects, Differences-in-Differences and Panel Data
- Morgan and Winship Chapter 11 Repeated Observations and the Estimation of Causal Effects

We Covered

- Difference-in-Differences
- Parallel Trends Assumption

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- Difference-in-Differences
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Next Time: Fixed Effects

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

- Last Week
  - causal inference with unmeasured confounding
- This Week
  - panel data
  - diff-in-diff
  - fixed effects
  - wrap-up
- The Following Week
  - ?
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causality

Basic Model Review

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

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- We discussed a differencing approach to this model
- The Fixed effects model is an alternative way to remove time-invariant unmeasured confounding
- We will start by assuming the model and discussing properties and in the next section, we will consider non-parametric identification.

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- 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:

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- 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 with Ross data

```
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:

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- 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}.

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- Basic message: any time-constant variable gets "absorbed" by the fixed effect. It has nothing to contribute because the comparison is within the units.

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- 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:

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

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- 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
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    - ★ 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

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- So which one is better when $T > 2$? Which one is more efficient?
  - if u_{it} uncorrelated $\rightsquigarrow$ FE is more efficient
  - if $u_{it} = u_{i,t-1} + e_{it}$ with e_{it} iid (random walk) $\rightsquigarrow$ FD is more efficient.

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- Note that when the second dimension isn't time, fixed effects will be relevant more often.

We Covered

- Fixed Effects!
- Computation for Fixed Effects!

Next Time: Non-parametric Identification and Fixed Effects

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

- Last Week
  - causal inference with unmeasured confounding
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1. Differencing Models
2. Difference-in-Differences
3. Fixed Effects
4. Non-parametric Identification and Fixed Effects
5. Wrap-Up
   - Questions
   - Concluding Thoughts for the Course

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  - 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.

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- 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.

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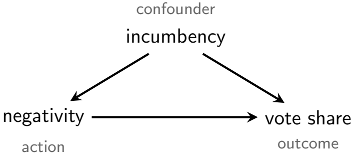

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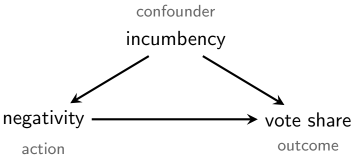

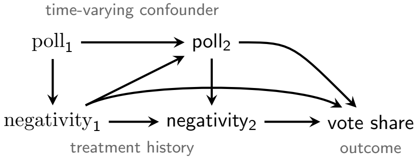

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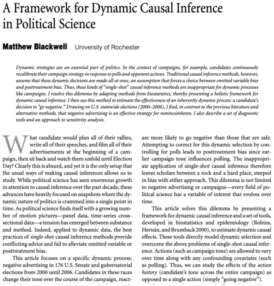

A Framework for Dynamic Causal Inference in Political Science

Matthew Blackwell University of Rochester

Dynamic strategies are an essential part of politics. In the context of campaigns, for example, candidates continuously recalibrate their campaign strategy in response to polls and opponent actions. Traditional causal inference methods, however, assume that these dynamic decisions are made all at once, an assumption that forces a choice between omitted variable bias and posttreatment bias. Thus, these kinds of "single-shot" causal inference methods are inappropriate for dynamic processes like campaigns. I resolve this dilemma by adapting methods from biostatistics, thereby presenting a holistic framework for dynamic causal inference. I then use this method to estimate the effectiveness of an inherently dynamic process: a candidate's decision to "go negative." Drawing on U.S. statewide elections (2000–2006), I find, in contrast to the previous literature and alternative methods, that negative advertising is an effective strategy for nonincumbents. I also describe a set of diagnostic tools and an approach to sensitivity analysis.

What candidate would plan all of their rallies, write all of their speeches, and film all of their advertisements at the beginning of a campaign, then sit back and watch them unfold until Election Day? Clearly this is absurd, and yet it is the only setup that the usual ways of making causal inferences allows us to study. While political science has seen enormous growth in attention to causal inference over the past decade, these advances have heavily focused on snapshots where the dynamic nature of politics is crammed into a single point in time. As political science finds itself with a growing number of motion pictures—panel data, time-series cross-sectional data—a tension has emerged between substance and method. Indeed, applied to dynamic data, the best practices of *single-shot* causal inference methods provide conflicting advice and fail to alleviate omitted variable or posttreatment bias.

This article focuses on a specific dynamic process: negative advertising in 176 U.S. Senate and gubernatorial elections from 2000 until 2006. Candidates in these races change their tone over the course of the campaign, react-

are more likely to go negative than those that are safe. Attempting to correct for this dynamic selection by controlling for polls leads to posttreatment bias since earlier campaign tone influences polling. The inappropriate application of single-shot causal inference therefore leaves scholars between a rock and a hard place, steeped in bias with either approach. This dilemma is not limited to negative advertising or campaigns—every field of political science has a variable of interest that evolves over time.

This article solves this dilemma by presenting a framework for dynamic causal inference and a set of tools, developed in biostatistics and epidemiology (Robins, Hernán, and Brumback 2000), to estimate dynamic causal effects. These tools directly model dynamic selection and overcome the above problems of single-shot causal inference. Actions (such as campaign tone) are allowed to vary over time along with any confounding covariates (such as polling). Thus, we can study the effects of the *action history* (candidate's tone across the entire campaign) as opposed to a single action (simply "going negative").

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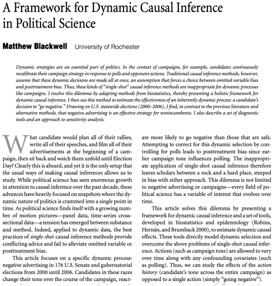

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doi:10.1017/S0003055418000357

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

MATTHEW BLACKWELL *Harvard University*
ADAM N. GLYNN *Emory University*

Repeated measurements of the same countries, people, or groups over time are vital to many fields of political science. These measurements, sometimes called time-series cross-sectional (TSCS) data, allow researchers to estimate a broad set of causal quantities, including contemporaneous effects and direct effects of lagged treatments. Unfortunately, popular methods for TSCS data can only produce valid inferences for lagged effects under some strong assumptions. In this paper, we use potential outcomes to define causal quantities of interest in these settings and clarify how standard models like the autoregressive distributed lag model can produce biased estimates of these quantities due to post-treatment conditioning. We then describe two estimation strategies that avoid these post-treatment biases—inverse probability weighting and structural nested mean models—and show via simulations that they can outperform standard approaches in small sample settings. We illustrate these methods in a study of how welfare spending affects terrorism.

INTRODUCTION

Many inquiries in political science involve the study of repeated measurements of the same countries, people, or groups at several points in time. This type of data, sometimes called time-series cross-sectional (TSCS) data, allows researchers to draw on a larger pool of information when estimating causal effects. TSCS data also give researchers the power to ask a richer set of questions than data with a single measurement for each unit (for example, see Beck and Katz 2011). Using this data, researchers can move past the narrowest contemporaneous questions—what are the effects of a single event?—and instead ask how the *history* of a process affects the political world. Unfortunately, the most common approaches to modeling TSCS data require strict assumptions to estimate the effect of treatment histories without bias and make it difficult to understand the nature of the counterfactual comparisons.

This paper makes three contributions to the study of TSCS data. Our first contribution is to define some

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counterfactual causal effects and discuss the assumptions needed to identify them nonparametrically. We also relate these quantities of interest to common quantities in the TSCS literature, like impulse responses, and show how to derive them from the parameters of a common TSCS model, the autoregressive distributed lag (ADL) model. These treatment effects can be nonparametrically identified under a key selection-on-observables assumption called sequential ignorability; unfortunately, however, many common TSCS approaches rely on more stringent assumptions, including a lack of causal feedback between the treatment and time-varying covariates. This feedback, for example, might involve a country's level of welfare spending affecting the vote share of left wing parties, which in turn might affect future levels of spending. We argue that this type of feedback is common in TSCS settings. While we focus on a selection-on-observables assumption in this paper, we discuss the tradeoffs with this choice compared to standard fixed-effects methods, noting that the latter may also rule out this type of dynamic feedback.

Our second contribution is to provide an introduction to two methods from biostatistics that can estimate the effect of treatment histories without bias and under weaker assumptions than common TSCS models. We focus on two methods: (1) *structural nested mean models* or SNMMs (Robins 1997) and (2) *marginal structural models* (MSMs) *with inverse probability of treatment weighting* (IPTWs) (Robins, Hernán, and

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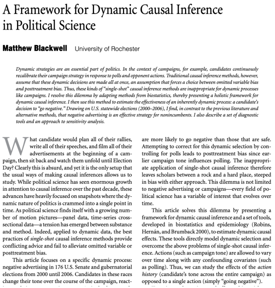
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When Should We Use Unit Fixed Effects Regression Models for Causal Inference with Longitudinal Data?

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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:

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

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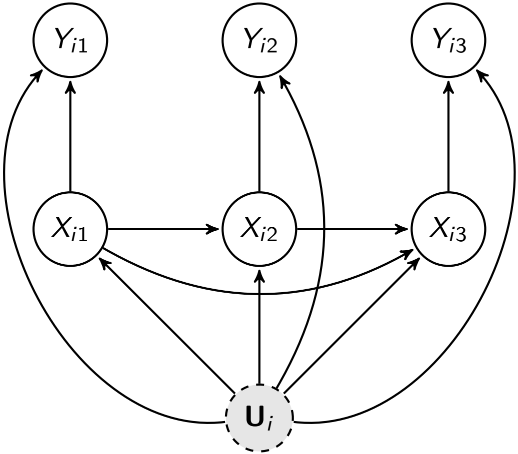

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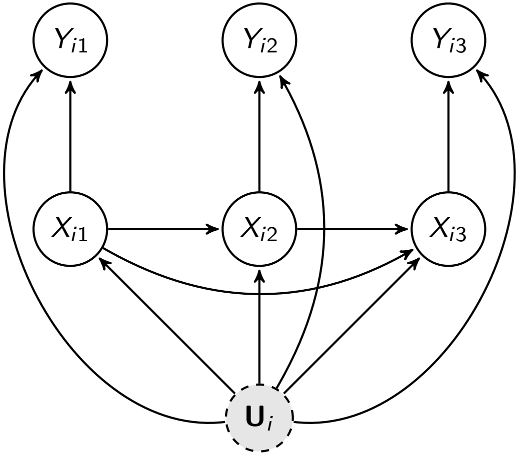

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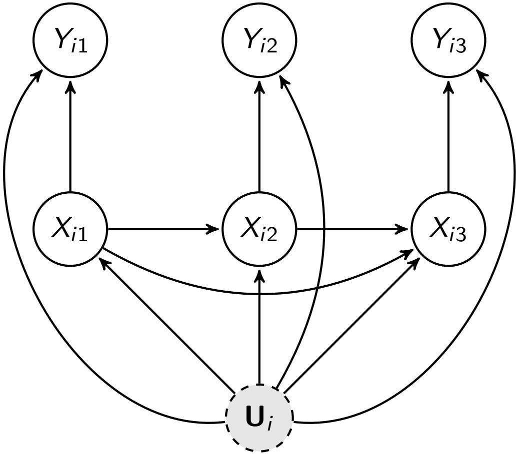

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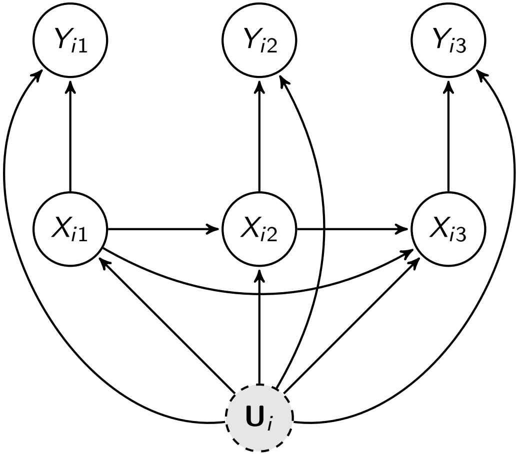

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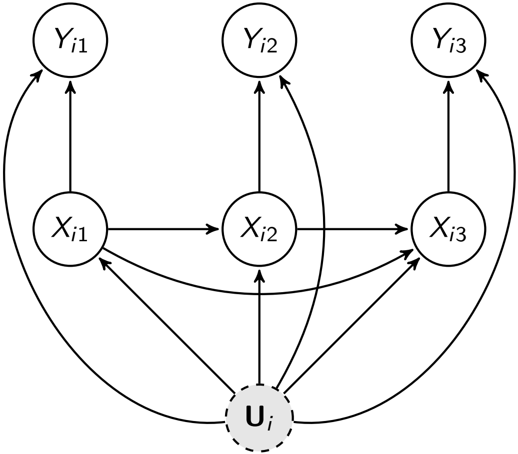

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3. Past outcomes do not directly affect current treatment
4. Past treatments do not directly affect current outcome

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)

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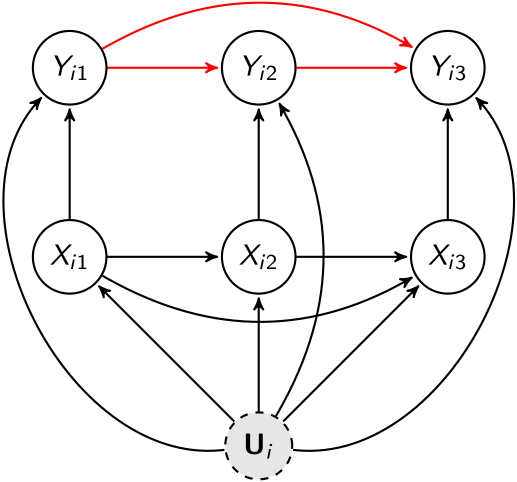
- Now let's consider each assumption in turn.

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



- Strict exogeneity still holds.

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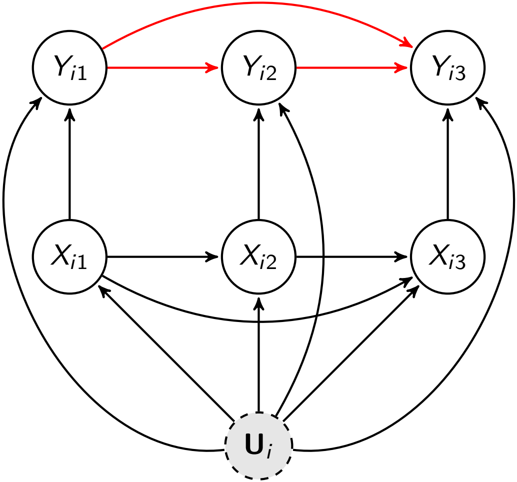

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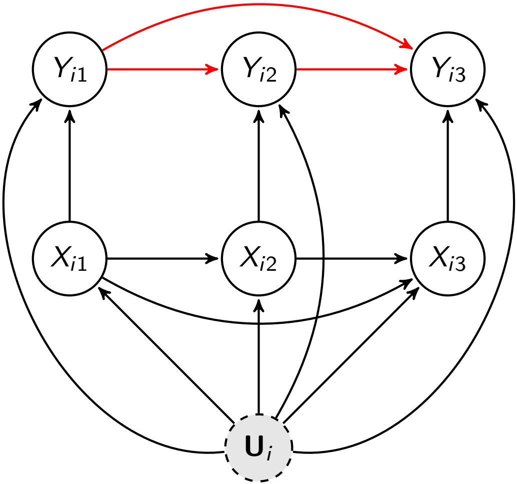

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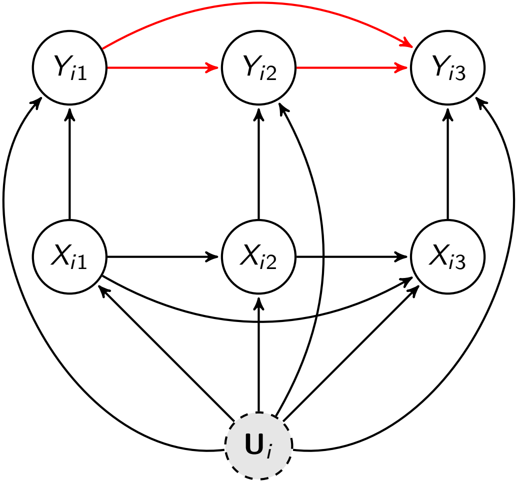

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- Should use cluster robust standard errors for inference.

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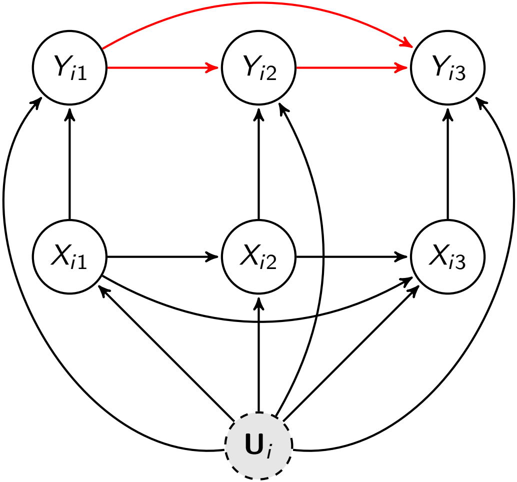

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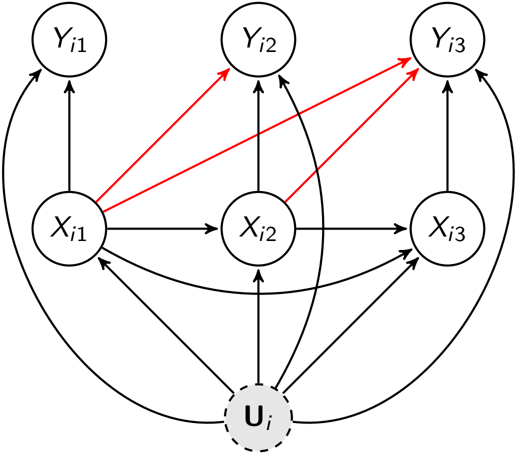
- Past outcomes do not confound $X_{it} \longrightarrow Y_{it}$ given $\mathbf{U}_i$.
- No need to adjust for past outcomes.
- Should use cluster robust standard errors for inference.
- Conclusion: The assumption can be relaxed

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



- Need to adjust for past treatments

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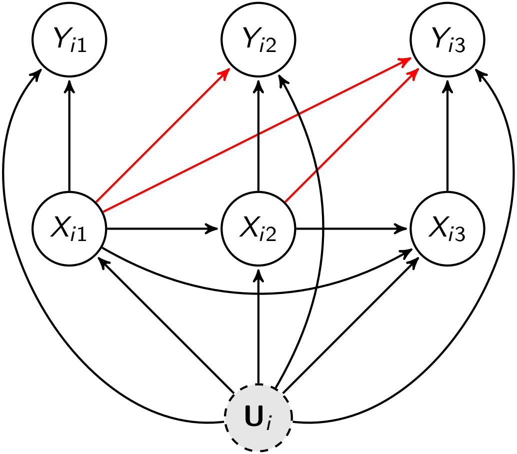

- Need to adjust for past treatments
- Strict exogeneity holds given past treatments and $\mathbf{U}_i$

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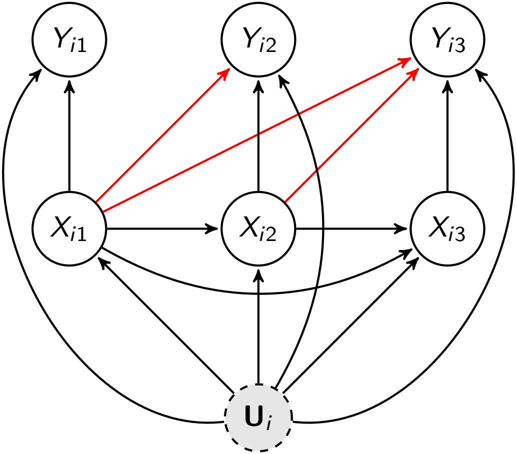

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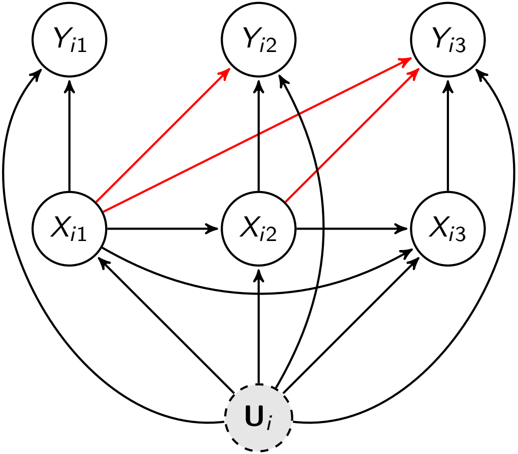

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- Strict exogeneity holds given past treatments and $\mathbf{U}_i$
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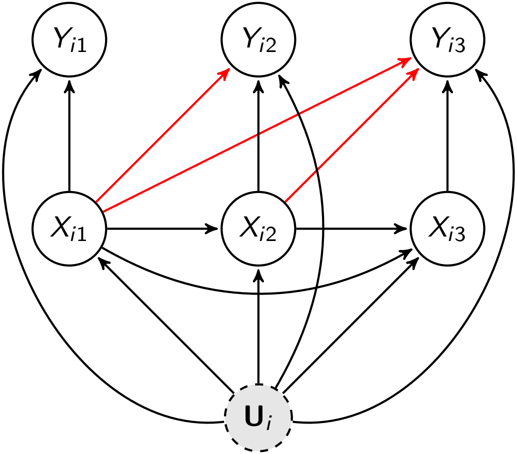

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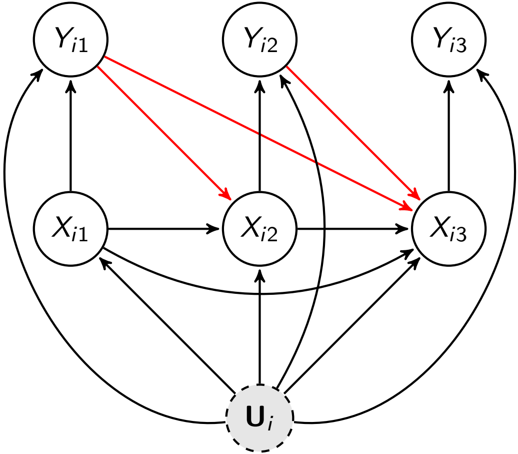
- Impossible to adjust for an entire treatment history and $\mathbf{U}_i$ at the same time
- Adjust for a small number of past treatments $\rightsquigarrow$ often arbitrary
- Conclusion: The assumption can be partially relaxed

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



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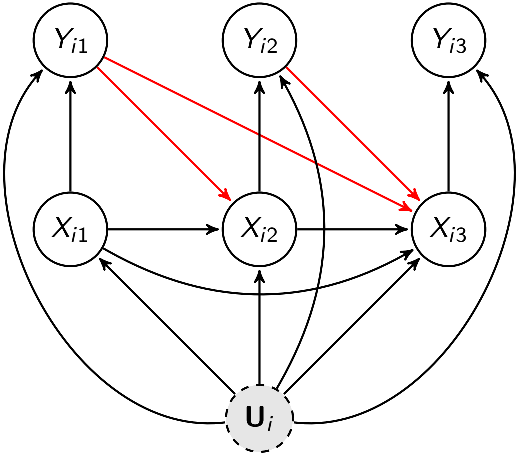
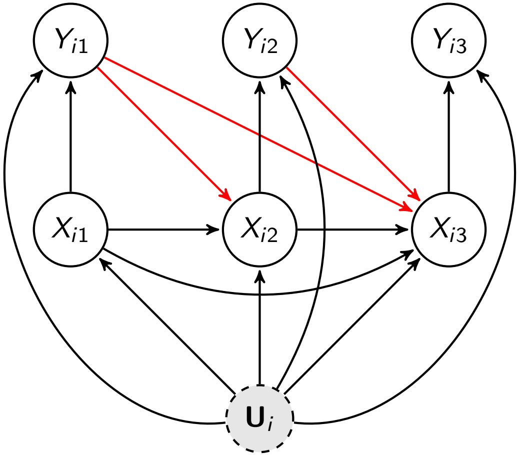


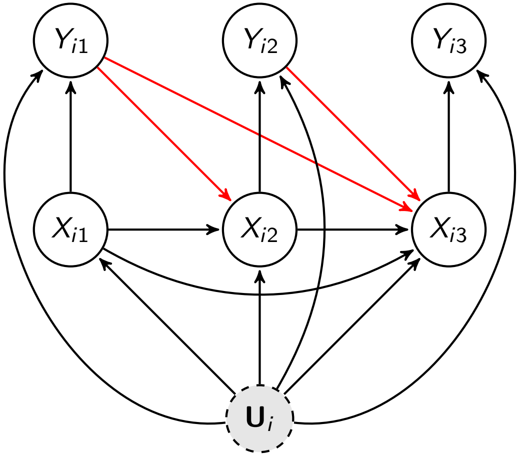
- Correlation between error term and future treatments

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



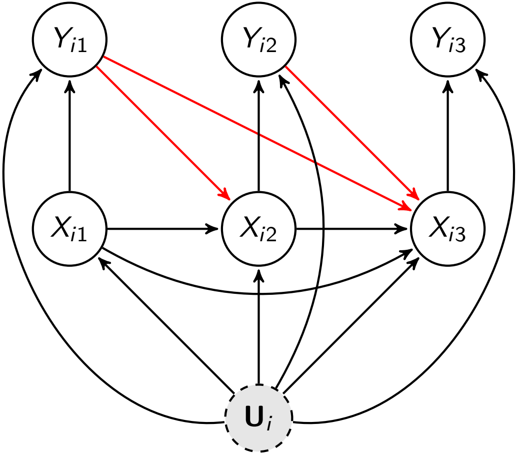
- Correlation between error term and future treatments
- Violation of strict exogeneity

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

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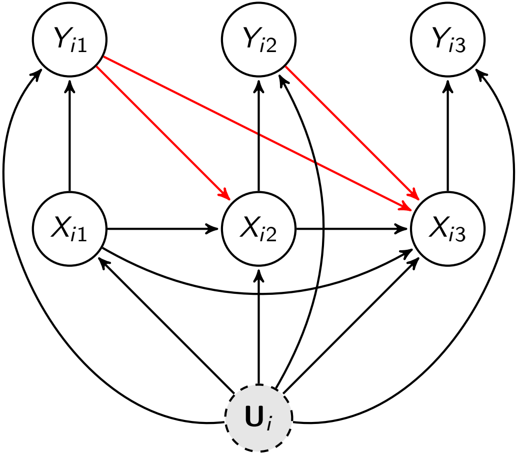

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- Implication: No dynamic causal relationships between treatment and outcome variables
- Conclusion: The assumption cannot be relaxed

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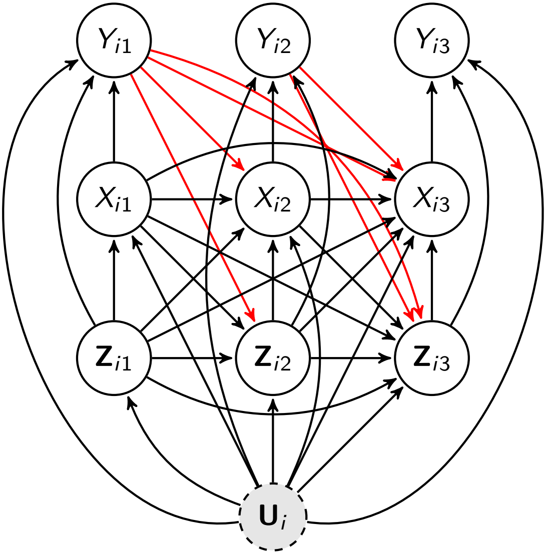

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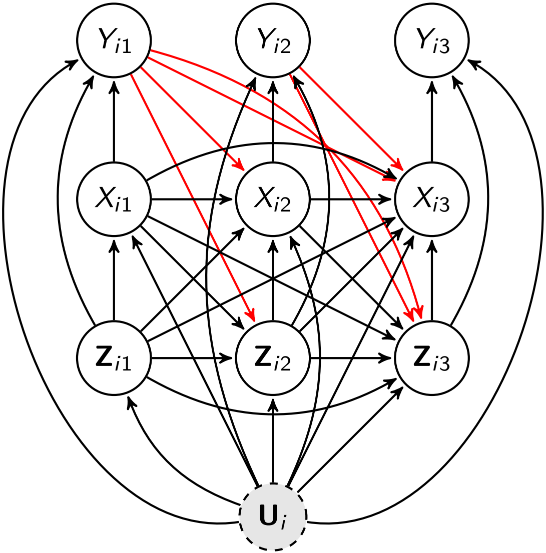

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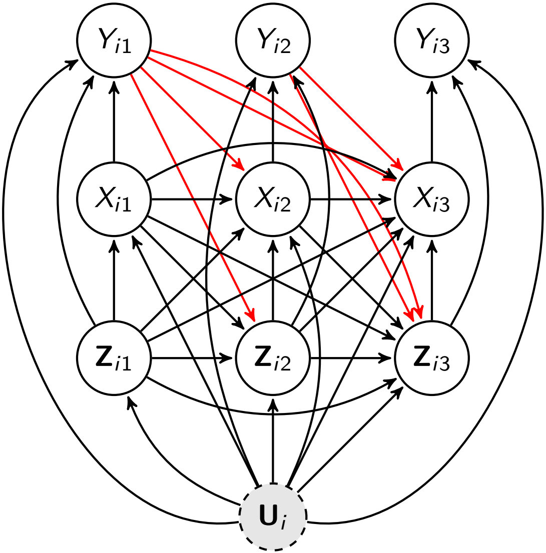

- $Y_{it} = \alpha_i + \beta X_{it} + \gamma^\top \mathbf{Z}_{it} + \epsilon_{it}$
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- past outcomes cannot *indirectly* affect current treatment through $\mathbf{Z}_{it}$

But What If I Have Causal Dynamics?

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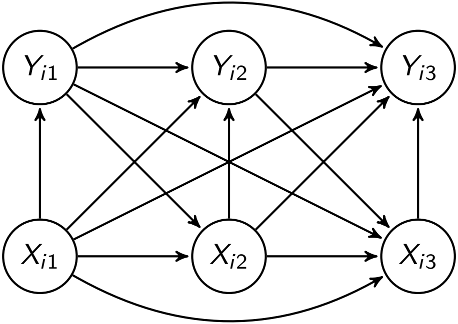

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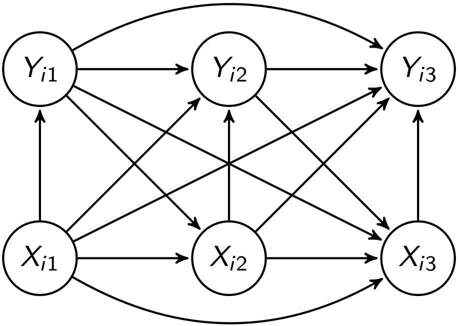

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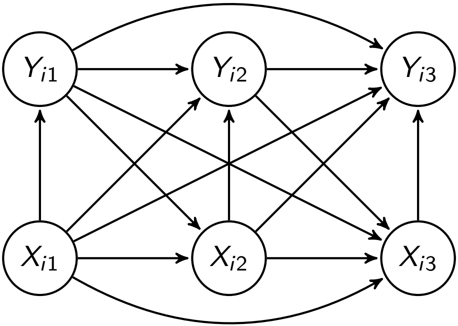

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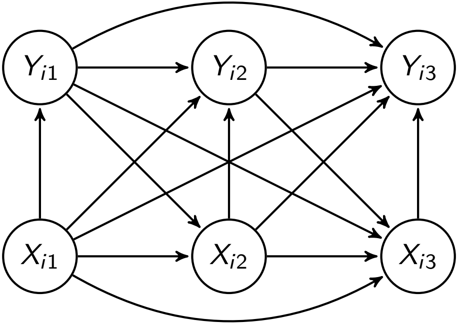

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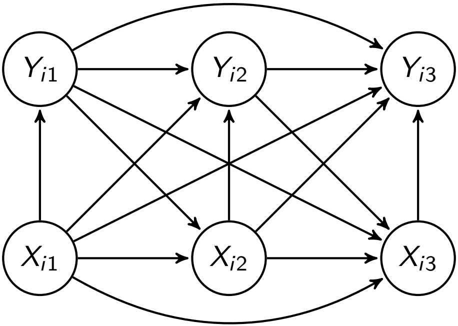

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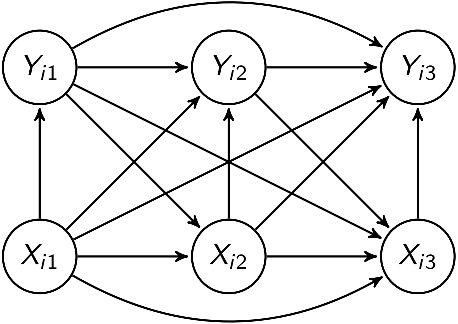

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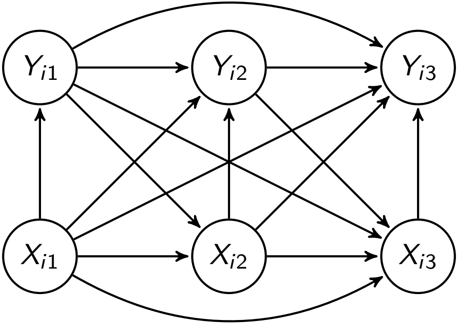

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Summary Table (Imai and Kim 2019)

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
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$Y_{it} = \alpha_i + \beta_1 X_{it} + \beta_2 X_{i,t-1} + \rho Y_{i,t-1} + \epsilon_{it}$	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

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Next Time: Review

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

- Last Week
  - causal inference with unmeasured confounding
- This Week
  - panel data
  - diff-in-diff
  - fixed effects
  - wrap-up
- The Following Week
  - ?
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causality

Q: What conditions do we need to infer causality?

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

Identification Strategies in This Class

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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

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- Stratification

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

Q: Can you review how instrumental variables deal with issues of confounding?

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A: We use only the units whose treatment status was effectively randomized by the instrument (because they are compliers).

Q: What can make standard errors larger or smaller?

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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 $\mathbf{Z}$.

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

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

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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.

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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`

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3. Derive the equations/math
4. Try to explain it to someone else

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A: Let's chat.

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7. On the Ground Decision Making

Where are you?

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You’ve been given a powerful set of tools

Your New Weapons

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- Basic probability theory
  - Probability axioms, random variables, marginal and conditional probability, building a probability model
  - Expected value, variances, independence
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- Univariate Inference
  - Interval estimation (normal and non-normal Population)
  - Confidence intervals, hypothesis tests, p-values
  - Practical versus statistical significance

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  - regression to approximate the conditional expectation function
  - idea of conditioning
  - kernel and loess regressions
  - OLS estimator for bivariate regression
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  - 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

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  - Non-normality
  - Outliers, leverage, and influence points, Robust Regression
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- And you learned how to use `R`: you're not afraid of trying something new!

Using these Tools

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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

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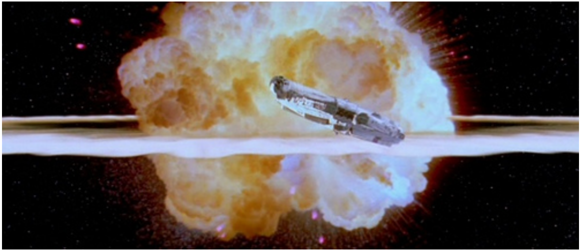
I've tried to offer a perspective on technique in addition to just a list of methods. See Lundberg, Johnson and Stewart "What's Your Estimand? Defining the Target Quantity Connects Statistical Evidence to Theory"

Thanks!

Thanks so much for an amazing semester.



I will see you in the final synchronous session!