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
  - Monday:
    - panel data
    - diff-in-diff
    - fixed effects
  - Wednesday:
    - spillover of material
    - Q&A
    - wrap-up
- The Following Week
  - break!
- Long Run
  - probability $\rightarrow$ inference $\rightarrow$ regression $\rightarrow$ causality

Questions?

Motivation

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  - provide benefits more efficiently
  - 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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  - The main difference is what level of analysis we care about (individual, city, county, state, country, etc).
- Time is a typical application, but applies to other groupings:
  - counties within states
  - states within countries
  - people within professions

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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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- Has two problems:
  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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- Ignore the heterogeneity $\rightsquigarrow$ correlation between the combined error and the independent variables:

$$\mathbb{E}[v_{it}|\mathbf{X}] = \mathbb{E}[a_i + u_{it}|\mathbf{X}] \neq 0$$

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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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- 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
```

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)

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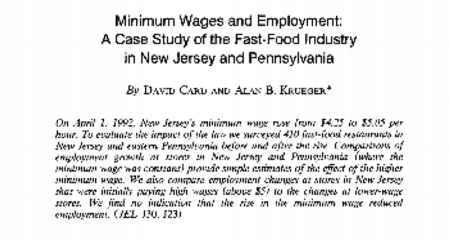

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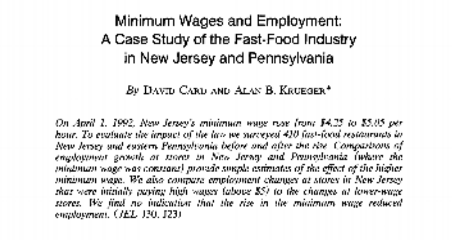

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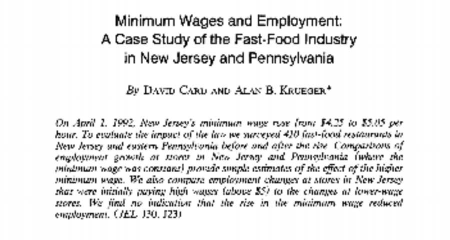

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- Idea: compare employment rates in 410 fast-food restauarants in New Jersey and eastern Pennsylvania (where there wasn't a wage increase) both before and after the change.
- Based on survey data:
  - Wave 1: March 1992, one month before the minimum wage increased
  - Wave 2: December 1992, eight months after increase

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  - $d_2 = 1$ and $d_1 = 0$

Difference-in-Differences

- Often called "diff-in-diff" (DiD), it is a special kind of FD model
- Let x_{it} be an indicator of a unit being "treated" at time t.
- Focus on two-periods where:
  - $x_{i1} = 0$ for all i
  - $x_{i2} = 1$ for the "treated group"
- Assume the model:

$$y_{it} = \beta_0 + \delta_0 d_t + \beta_1 x_{it} + a_i + u_{it}$$

- d_t is a dummy variable for the second time period
  - $d_2 = 1$ and $d_1 = 0$
- 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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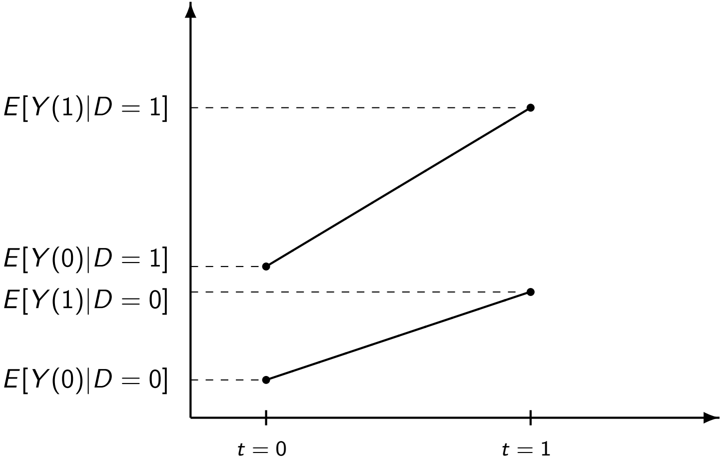
Graphical Representation: Difference-in-Differences

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



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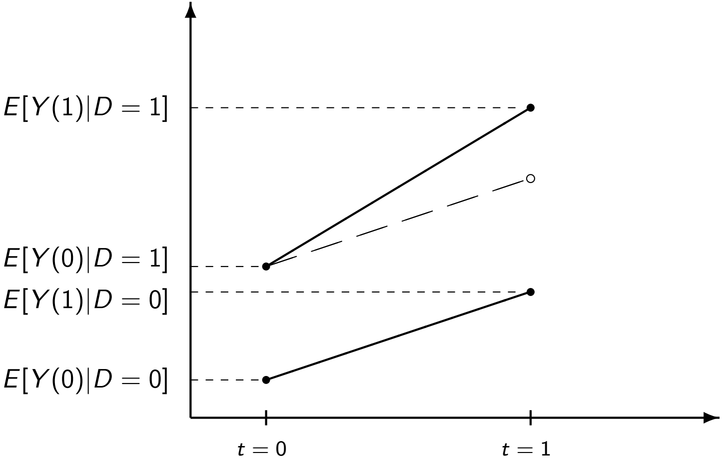

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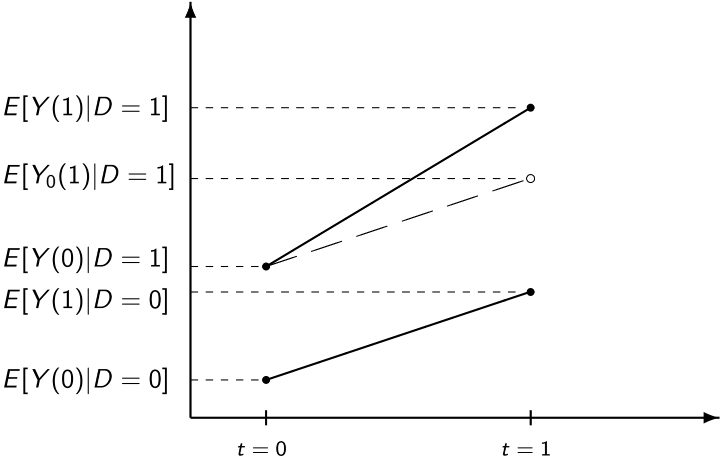

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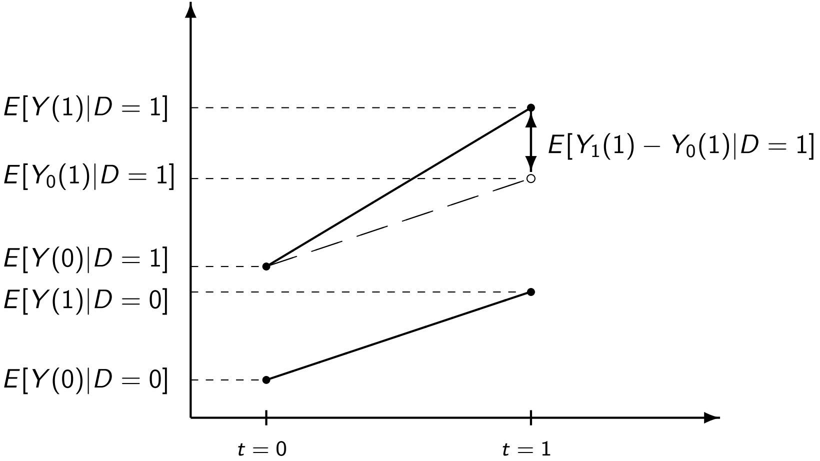

Identification with Difference-in-Differences

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:

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

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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}
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= & \{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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$\square$

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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

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

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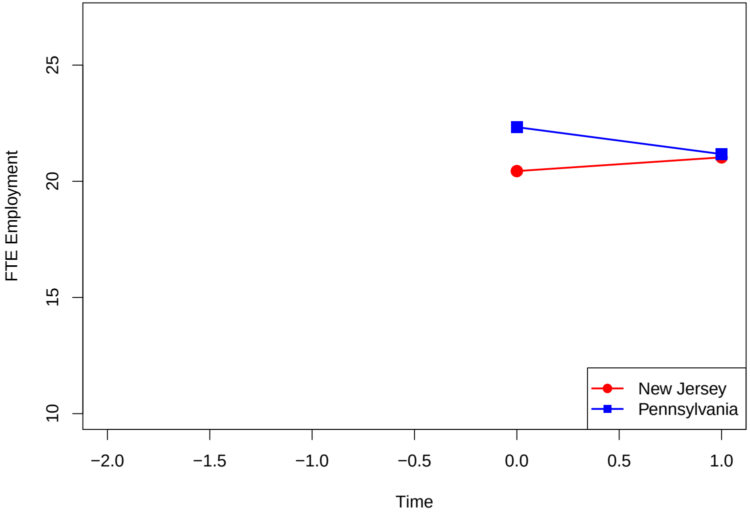
- NJ_i indicates which stores received the treatment of a higher minimum wage at time period $t = 2$

Parallel Trends?



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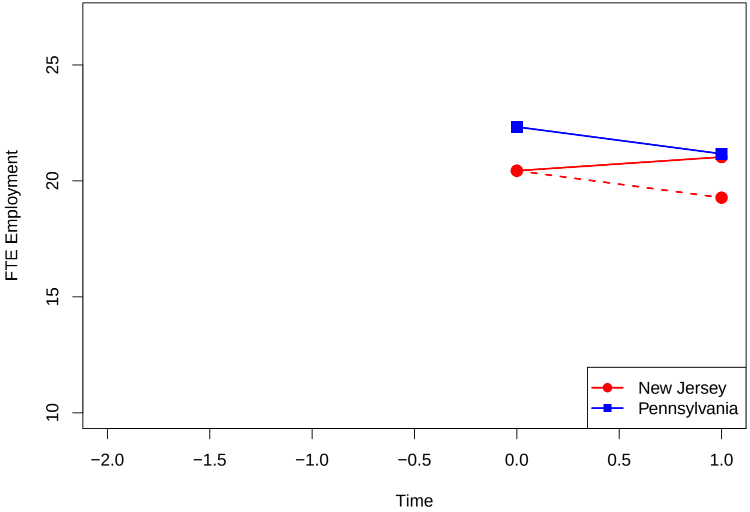

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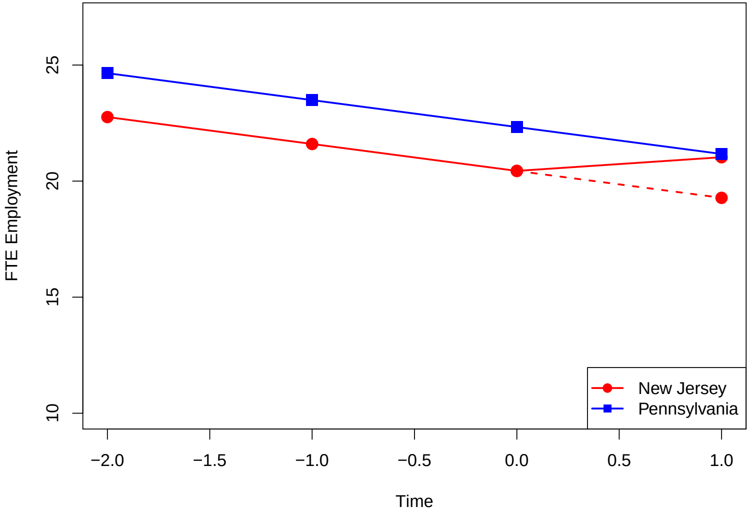

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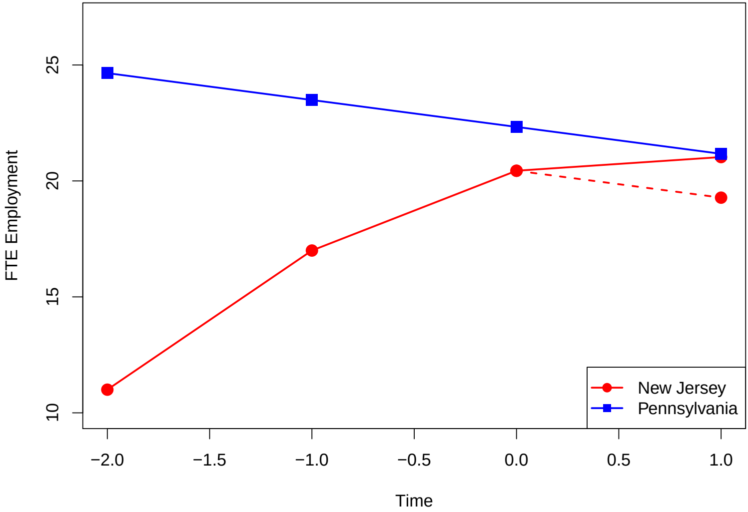
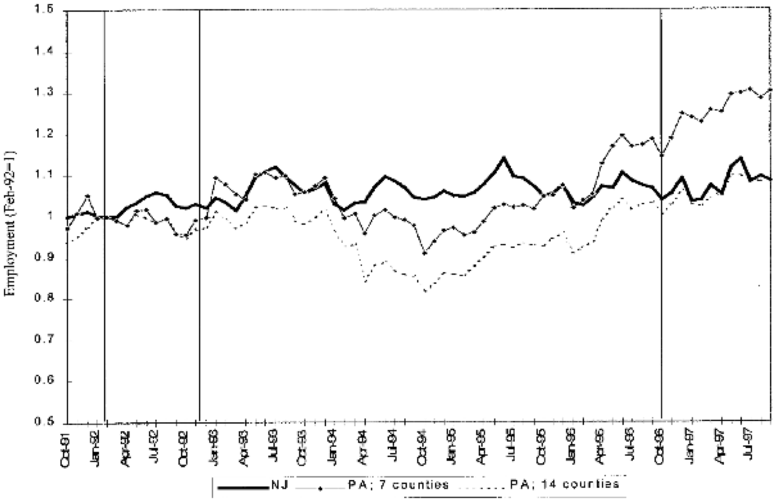


Longer Trends in Employment (Card and Krueger 2000)



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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- Two questions to ask:
  1. 'what is the counterfactual?' or
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- Personal Gripe: 'Two-way Fixed Effects' models often called a DiD or Generalized-DiD design but the parallel trend assumptions are different in important ways.

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- 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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&= \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:

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

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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 $\mathbb{E}[\mathbf{u}|\mathbf{X}] = 0$: all errors are uncorrelated with covariates in every period:

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

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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}.

Fixed Effects and Time-Invariant Covariates

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

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  - 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.

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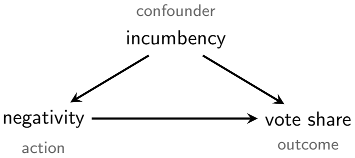

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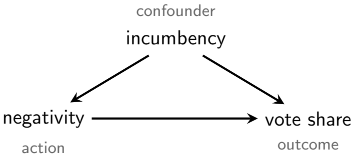

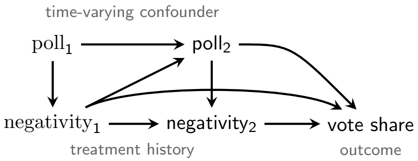

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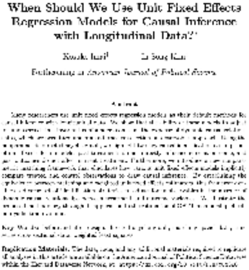
INTRODUCTION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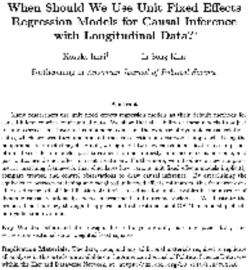
When Should We Use Unit Fixed Effects Regression Models for Causal Inference with Longitudinal Data?

Kosuke Imai

In Song Kim

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:

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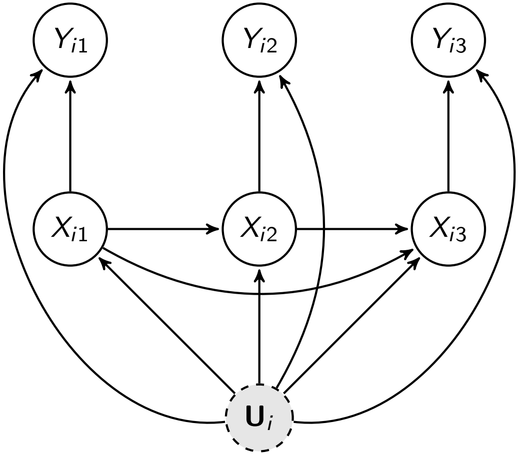

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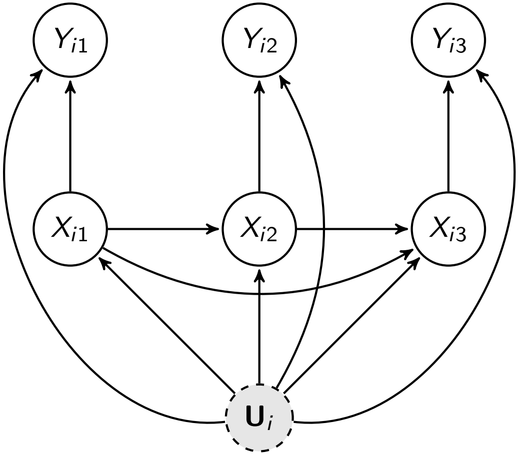

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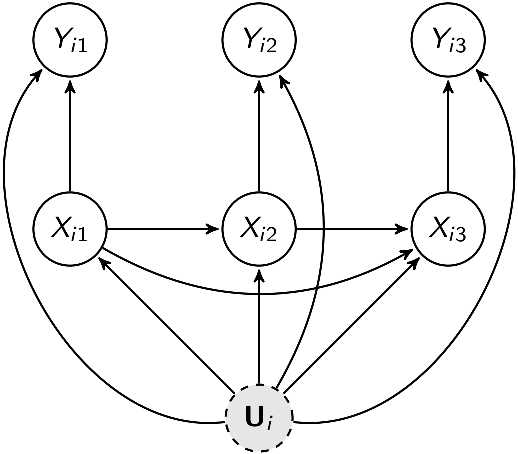

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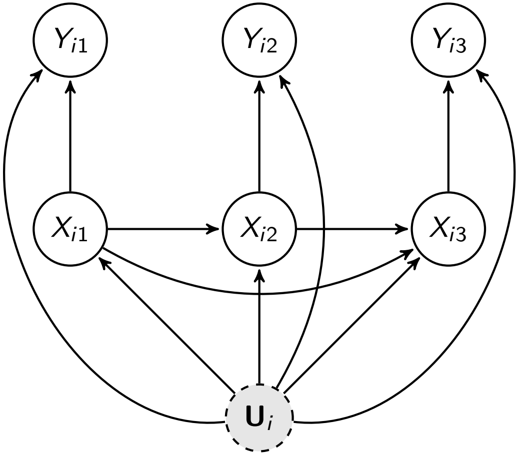

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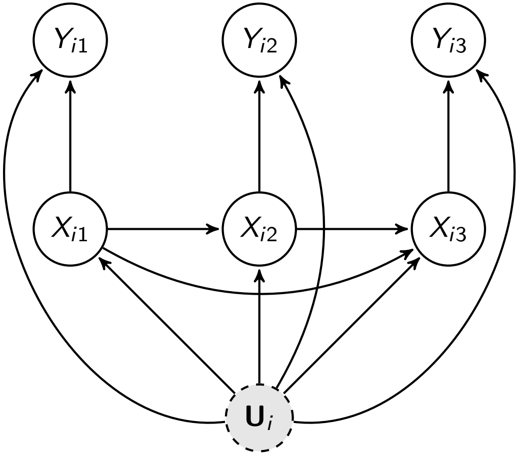

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the result implies that the counterfactual outcome for a treated observation in a given time period is estimated using the observed outcomes of different time periods of the same unit. Since such a comparison is valid only when no causal dynamics exist,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

- Imai and Kim (Forthcoming)

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- Now let's consider each assumption in turn.

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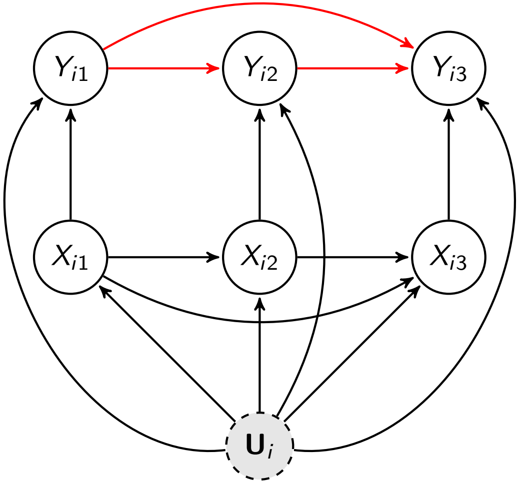

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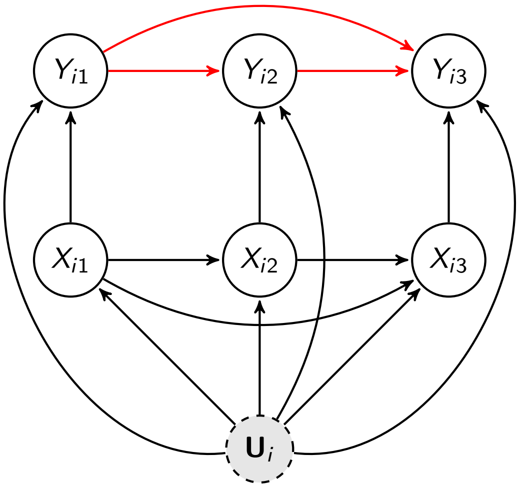

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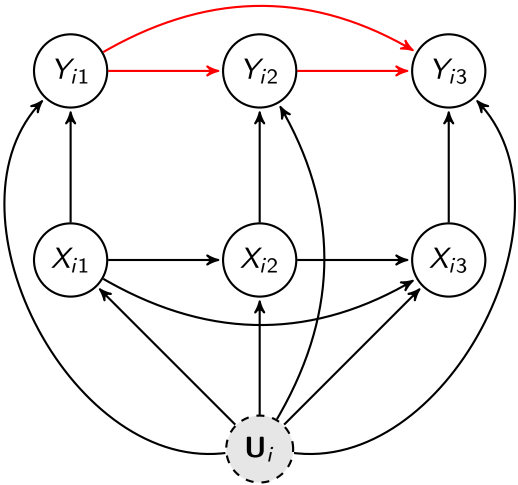

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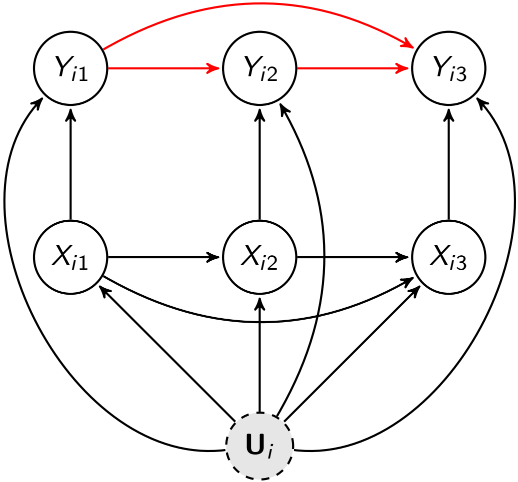

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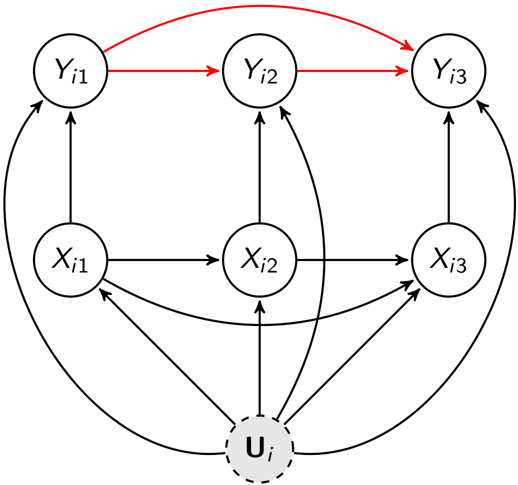

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- Conclusion: The assumption can be relaxed

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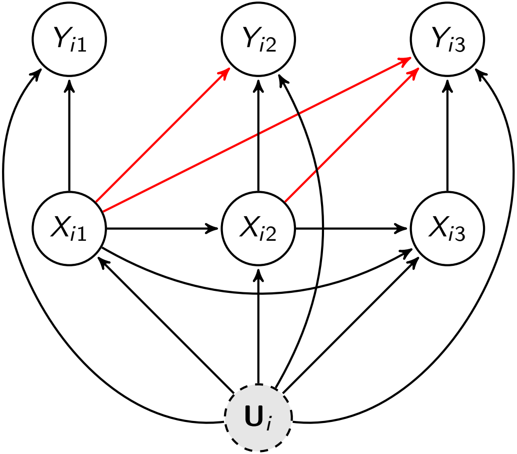

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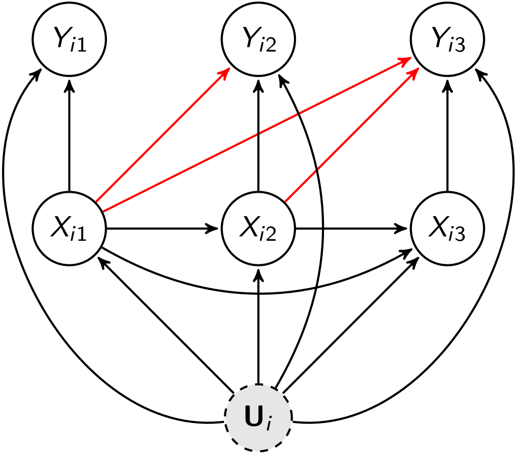

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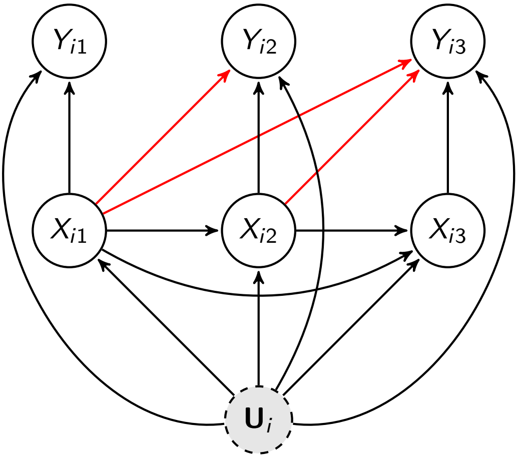

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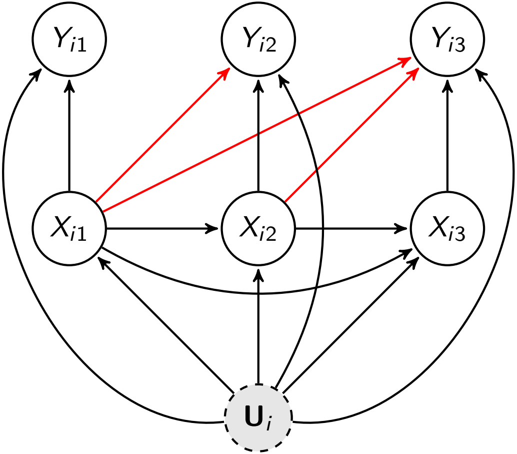

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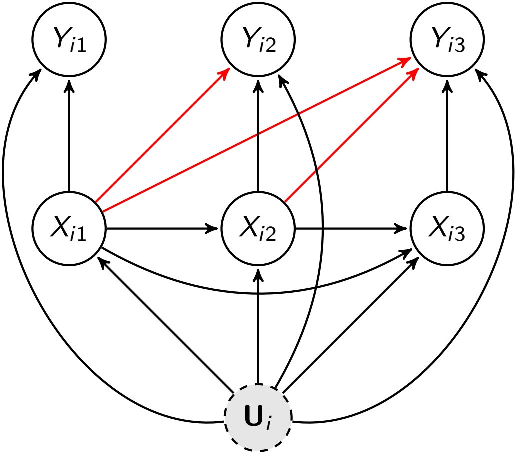

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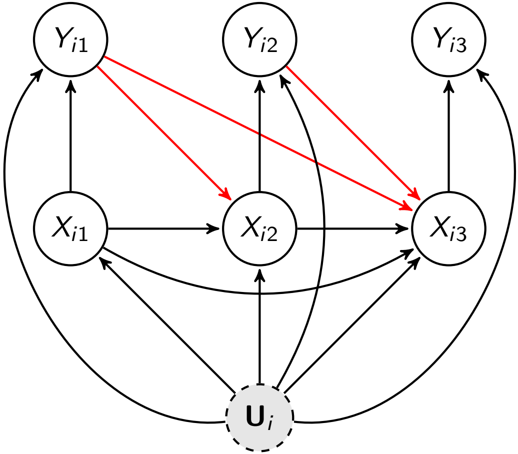

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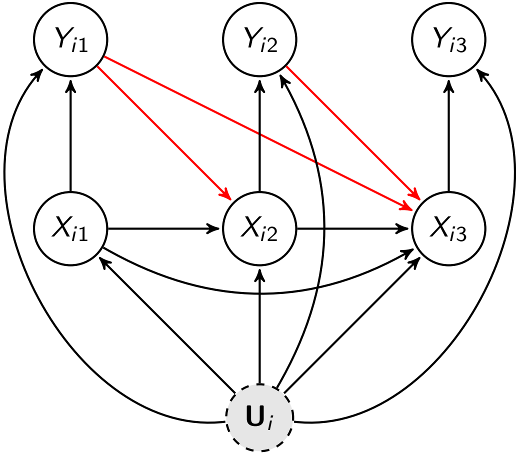

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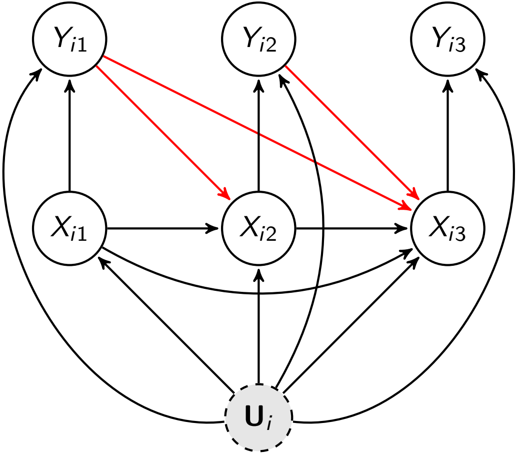

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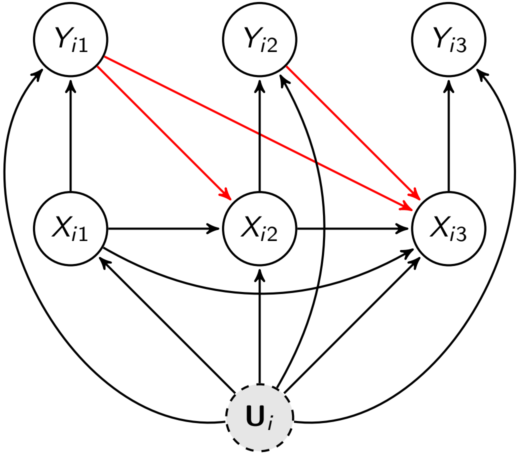

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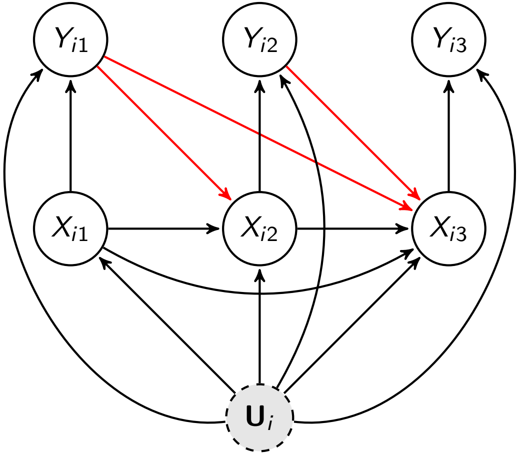

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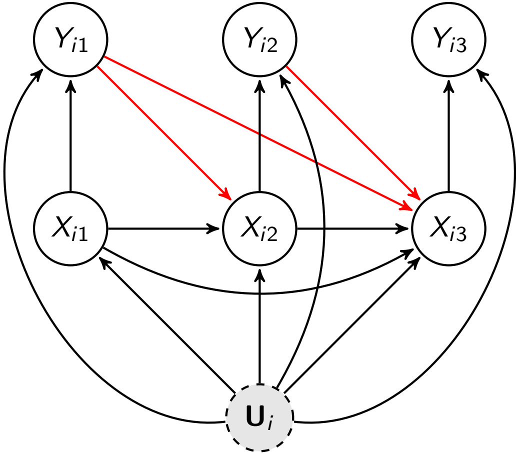

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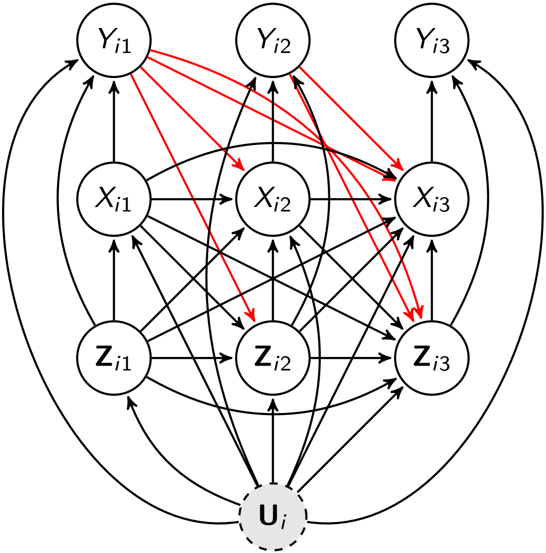

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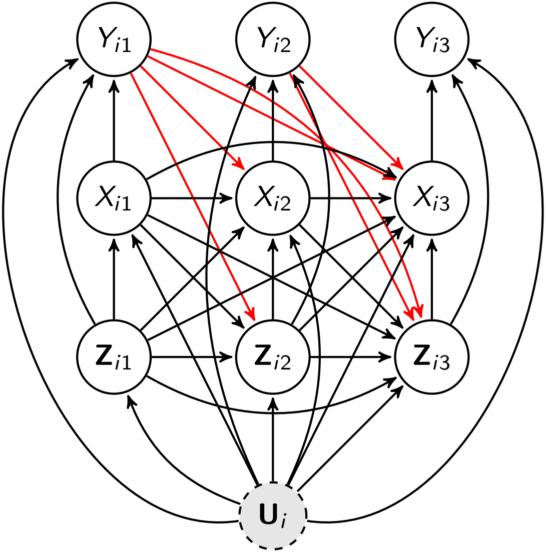

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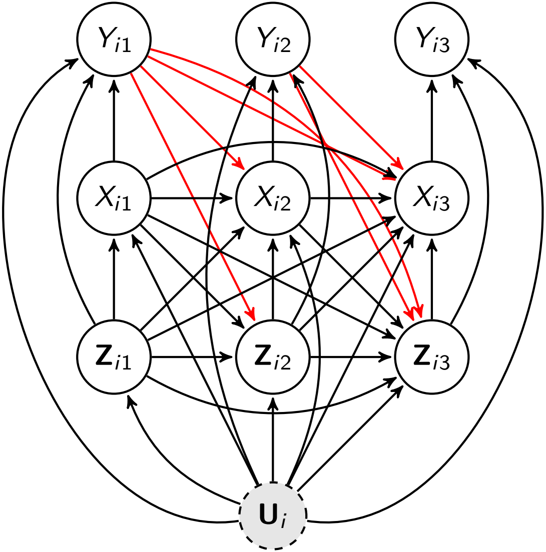

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- past outcomes cannot *indirectly* affect current treatment through $\mathbf{Z}_{it}$

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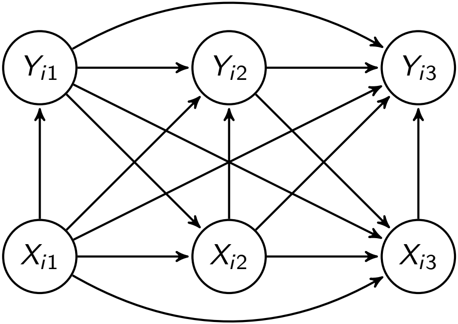

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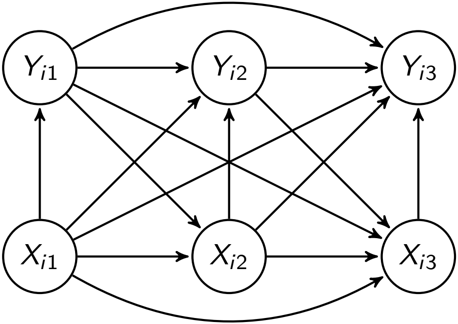

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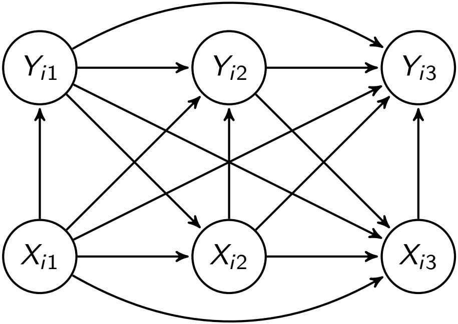

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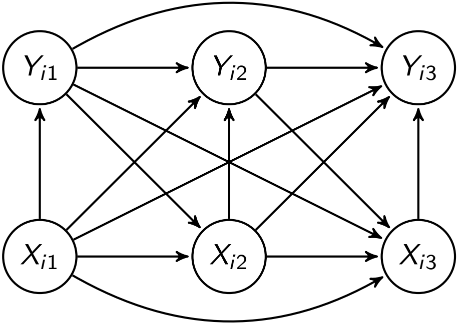

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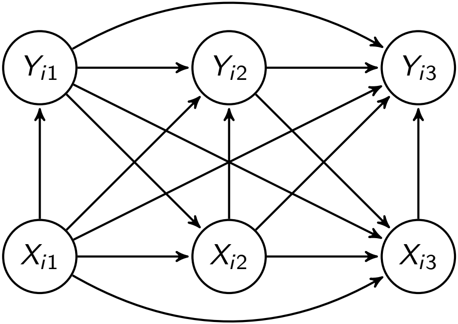

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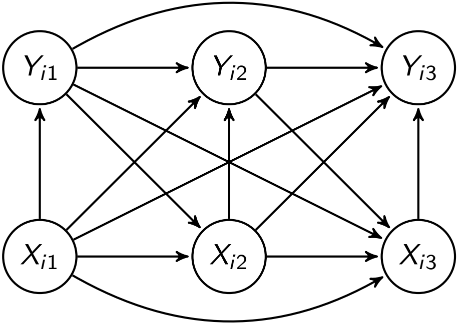

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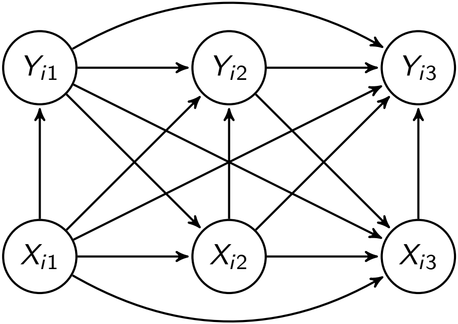

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    2) causal dynamics between treatment and outcome $\rightsquigarrow$ selection-on-observables

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

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Essentially 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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Some Estimation Strategies

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- Matching (not covered)
- Weighting (not covered)

Q: Can you review how to read DAGs?

[2] Courtesy of Erin Hartman's slides for this.

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A: Sure[2]

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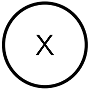
Notation

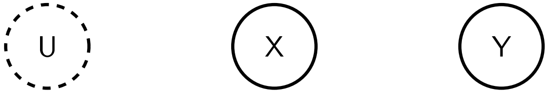


Node – A random Variable. Sometimes drawn as a solid circle $\overset{X}{\bullet}$.

Notation

Dashed line means its unobserved. Sometimes drawn as a hollow circle $\overset{U}{\circ}$.

Notation



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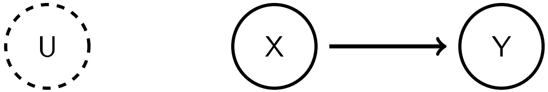
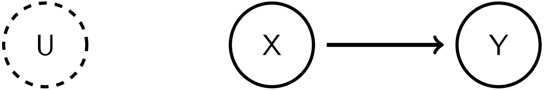


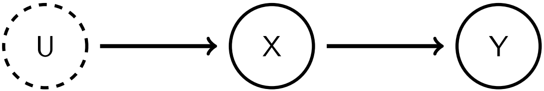
Arrow means “X causes Y”.

Notation



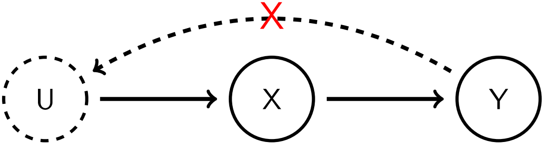
A parent is a directly cause of a child, a child is directly caused by a parent.

Notation



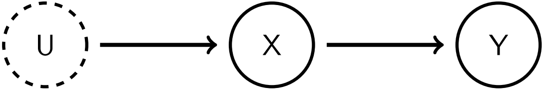
An ancestor is a direct or indirect cause, a descendant is caused, directly or indirectly, by an ancestor.

Notation



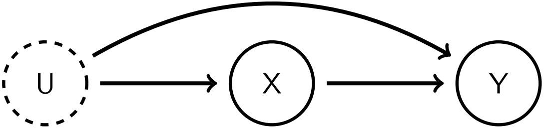
Acyclic implies there are no paths from a variable back to itself.

Notation



A lack of arrows implies no causal relationship.

Notation



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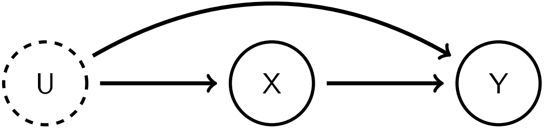
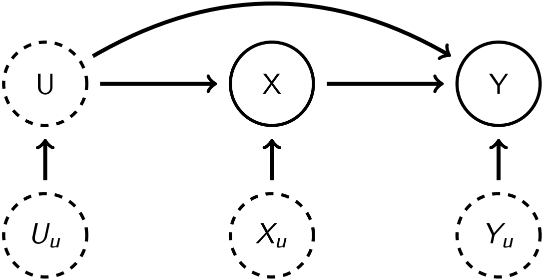


A lack of variables indicates a lack of common causes in the DGP.

Notation



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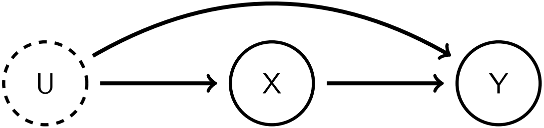
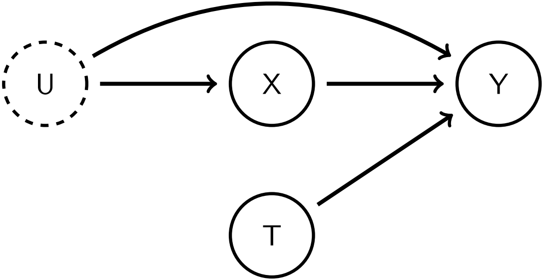


DAGs encode non-parametric structural models.

$$X = f_X(U)$$

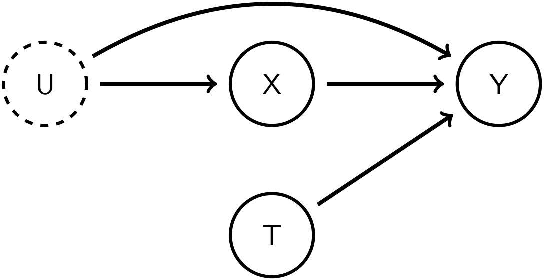
$$Y = f_Y(X, U)$$

Notation



A collider is when a node receives edges from two, or more, other nodes.

Notation

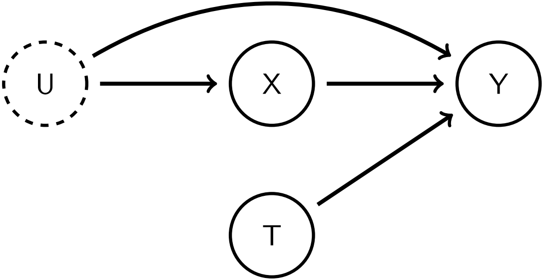


A causal effect can be defined using the *do* operator.

$$P(Y = y \mid do(X = x)) = \sum_z P(Y = y \mid X = x, PA = z) P(PA = z)$$

where PA are parents of X, and z ranges of all the combinations of values that the variables in PA can take.

Notation



Then, if T is binary,

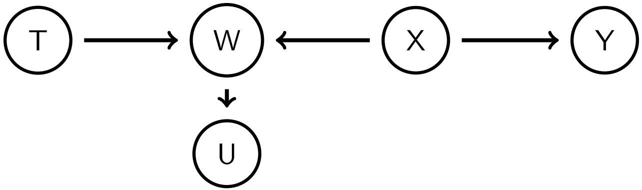
$$ACE = P(Y = 1 \mid do(T = 1)) - P(Y = 1 \mid do(T = 0))$$

and if T is randomized, then:

$$ACE = P(Y = 1 \mid T = 1) - P(Y = 1 \mid T = 0)$$

because there are no parents of T.

d-separation



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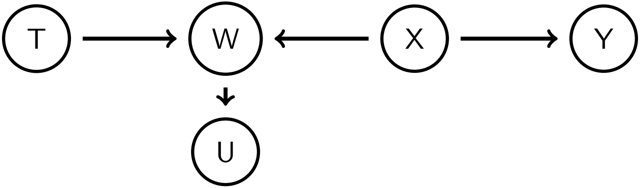


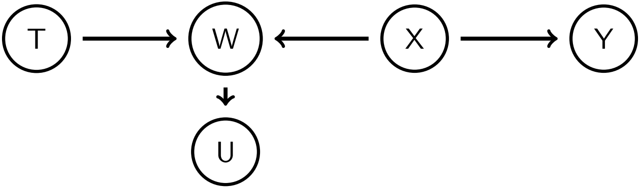
A path p is blocked by a set of nodes Z if and only if:

(1) p contains a chain of nodes $A \rightarrow B \rightarrow C$ or a fork $A \leftarrow B \rightarrow C$ such that the middle node B is in Z or

(2) p contains a collider $A \rightarrow B \leftarrow C$ such that the collision node B is not in Z and no descendant of B is in Z

If Z blocks every path between two nodes X and Y, then X and Y are d-separated, conditional on Z, and thus are conditionally independent given Z.

d-separation



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T and Y are d-separated conditional on $\{\}$, because they are blocked by the collider W, meets (2)

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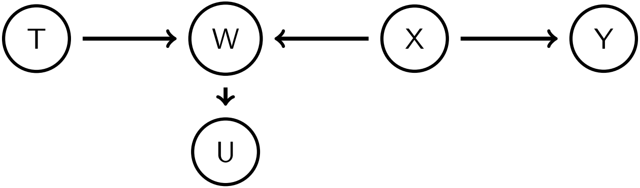

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T and Y are d-connected conditional on $\{W\}$, violates (2).

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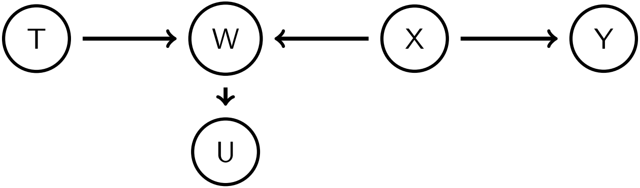

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T and Y are d-separated conditional on $\{W, X\}$, because X blocks the path by criterion (1).

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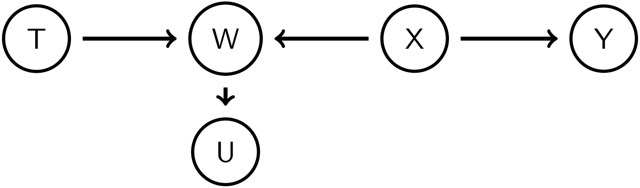

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We can use d-separation to do calculate causal effects via the "back-door" criterion, so long as Z does not contain descendants of our treatment of interest.

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 are degrees of freedom and how do they play into standard errors?

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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 Freedmans 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: Can you discuss the difference between having an instrument and having a mediator?

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A: If we think of the treatment as the mediator of the instrument, it is by the exclusion restriction a total mediator (the direct effect is 0).

Q: How do propensity scores and matching fit into all of this?

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A: They are different ways of conditioning on variables in a selection on observables strategy. Importantly: they are tools for estimation not tools for identification.

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- $\rightsquigarrow$ stratifying on e_i is the same in expectation as stratifying on the full X_i.
- The true propensity score is actually a balancing score, which means that $D_i \perp\!\!\!\perp X_i \mid e(X_i)$

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  - NB: propensity scores only achieve balance in expectation

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(Ho, Imai, King, Stuart, 2007: fig.1, Political Analysis)

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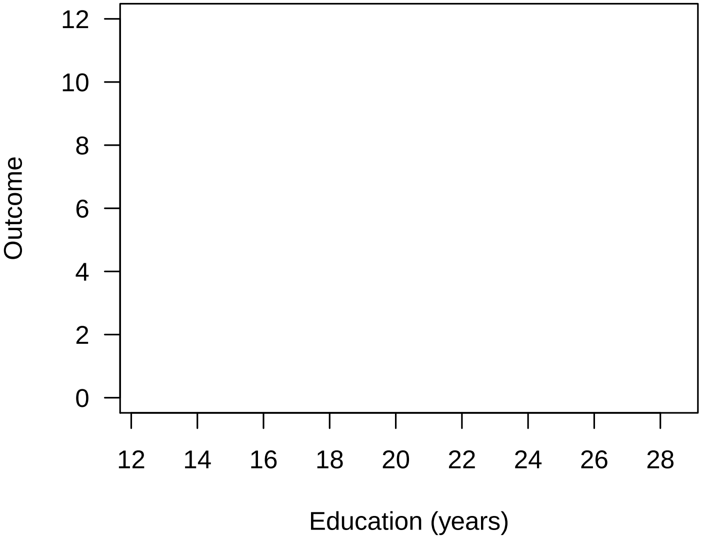

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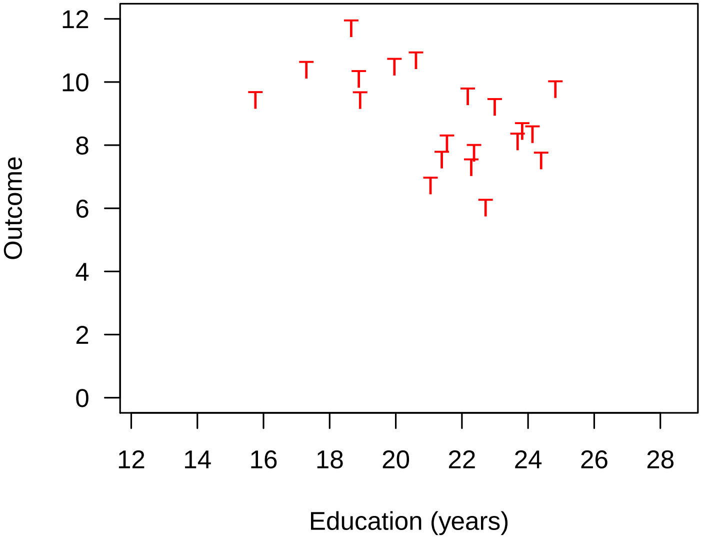

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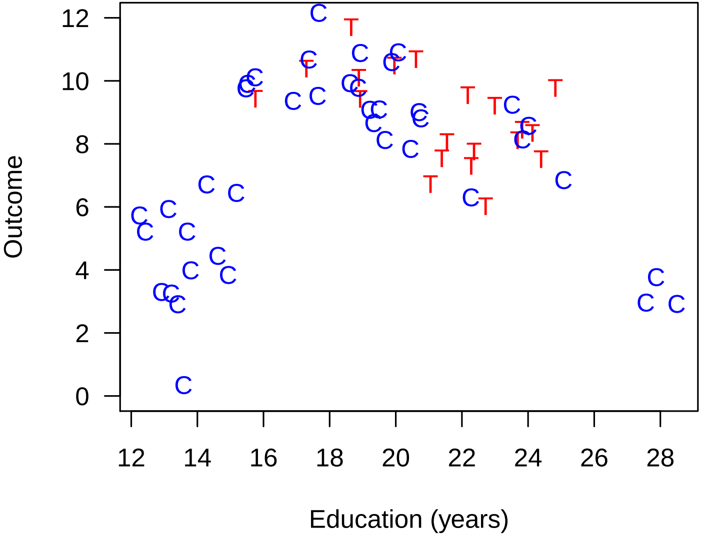

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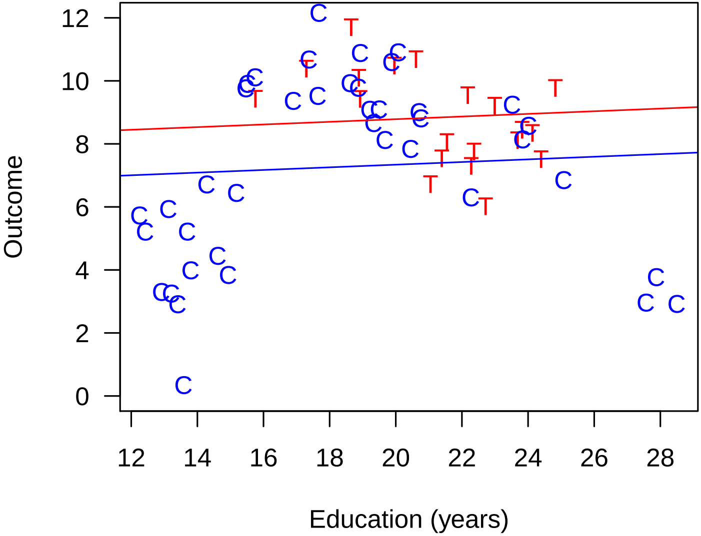

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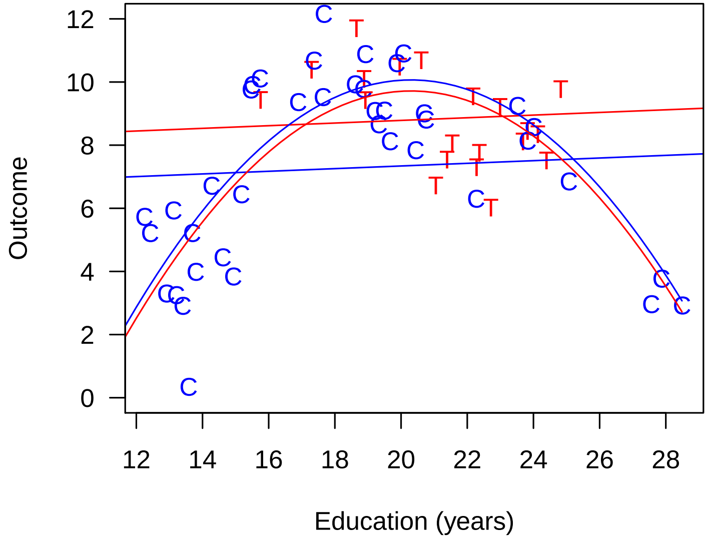

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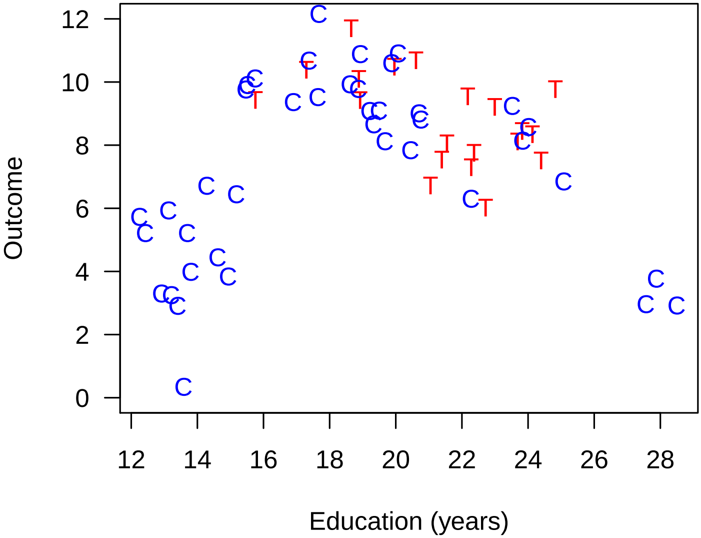

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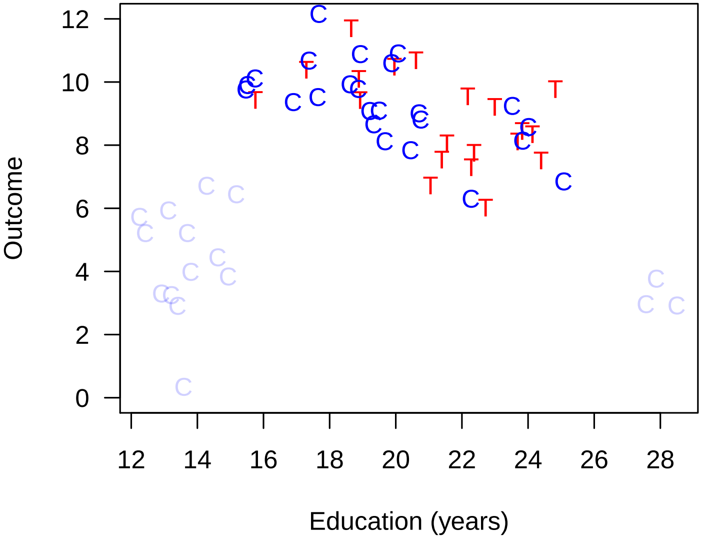

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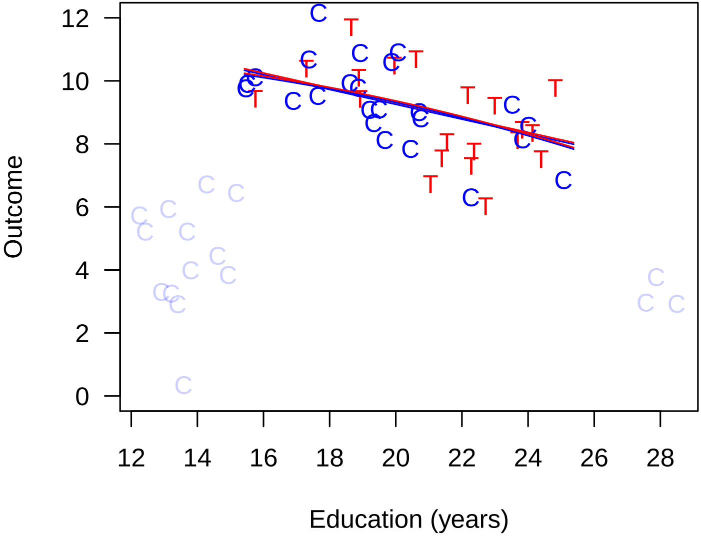

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- There are many approaches to matching. We will cover just three for the sake of time.
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- Which is the best method? The one that produces the best balance!

Next few slides based on slides by Gary King and Rich Nielsen

Method 1: Mahalanobis Distance Matching

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(Approximates Fully Blocked Experiment)

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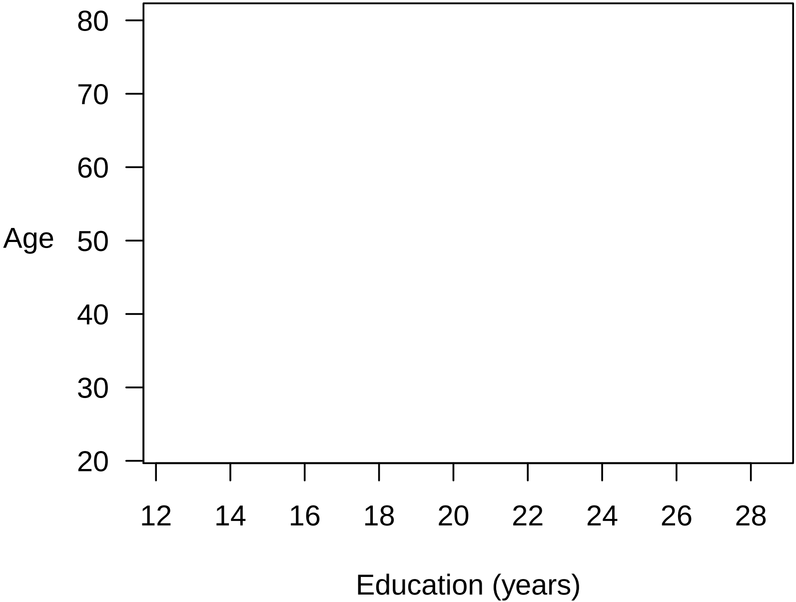

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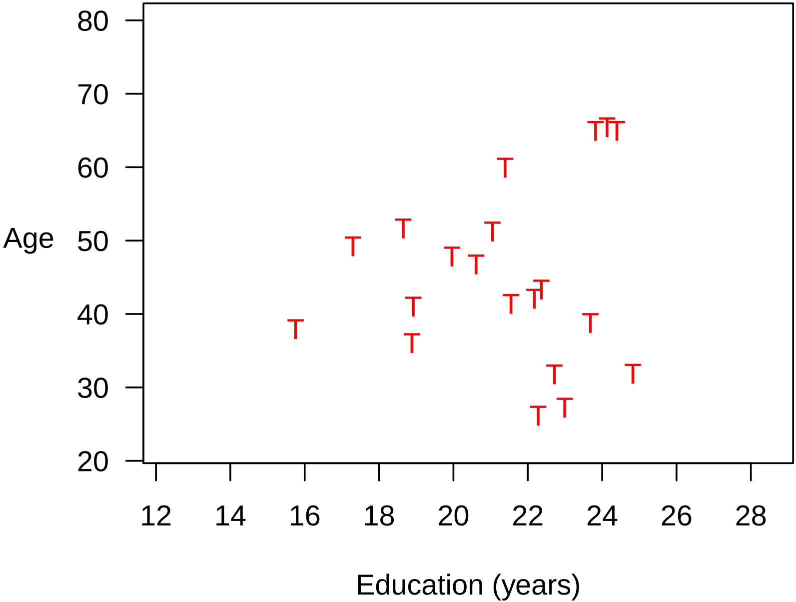

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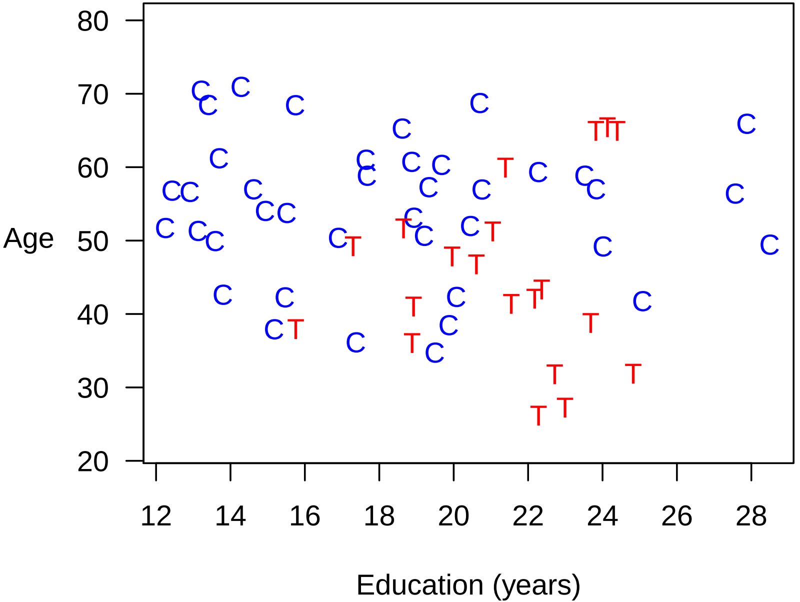

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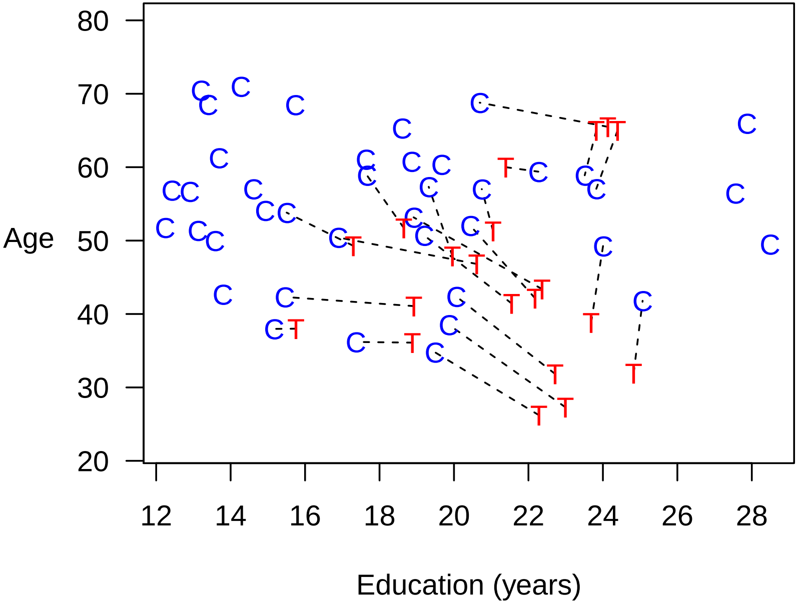

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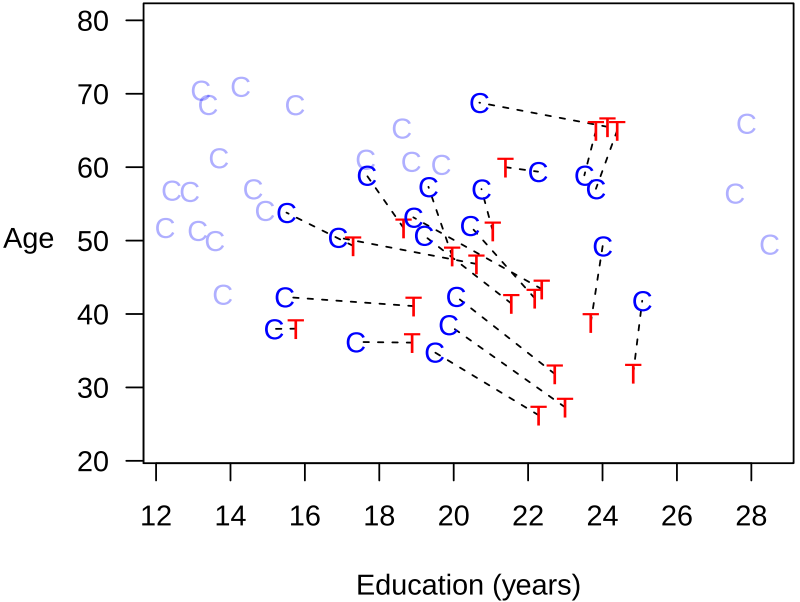

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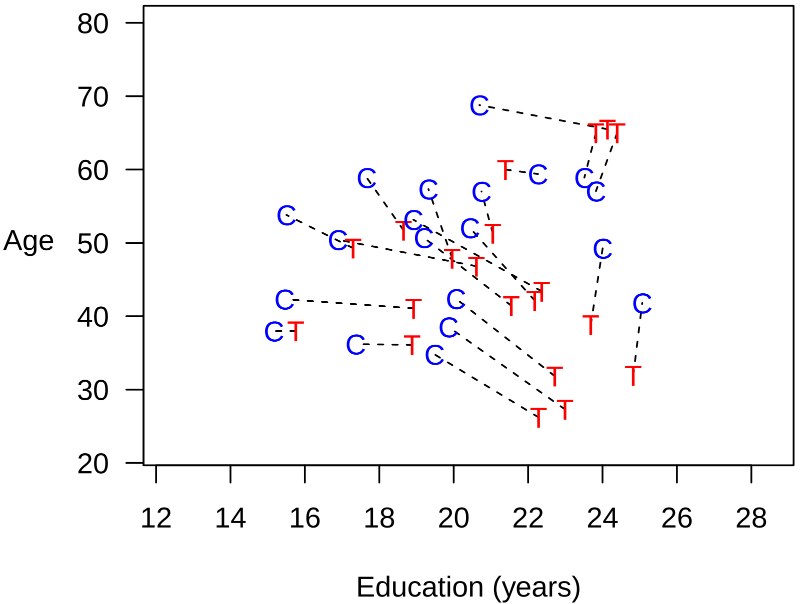

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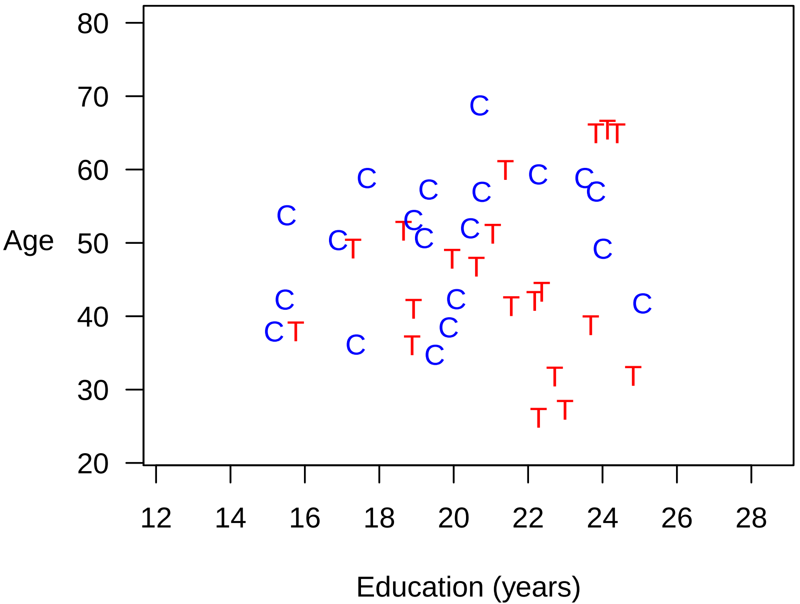

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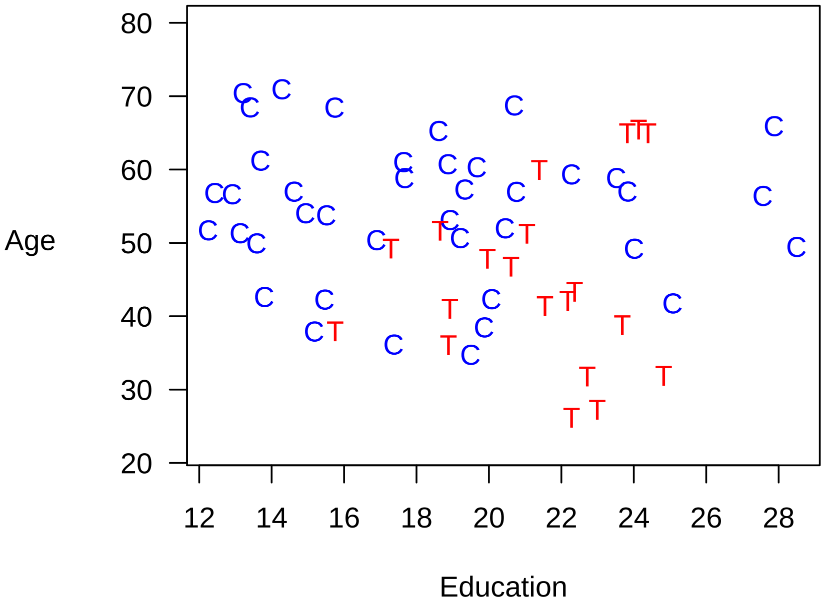

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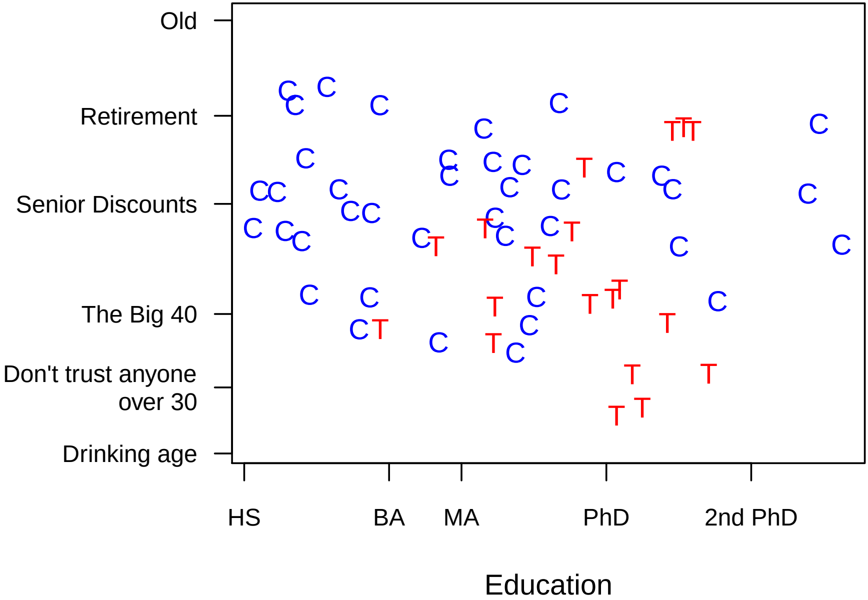

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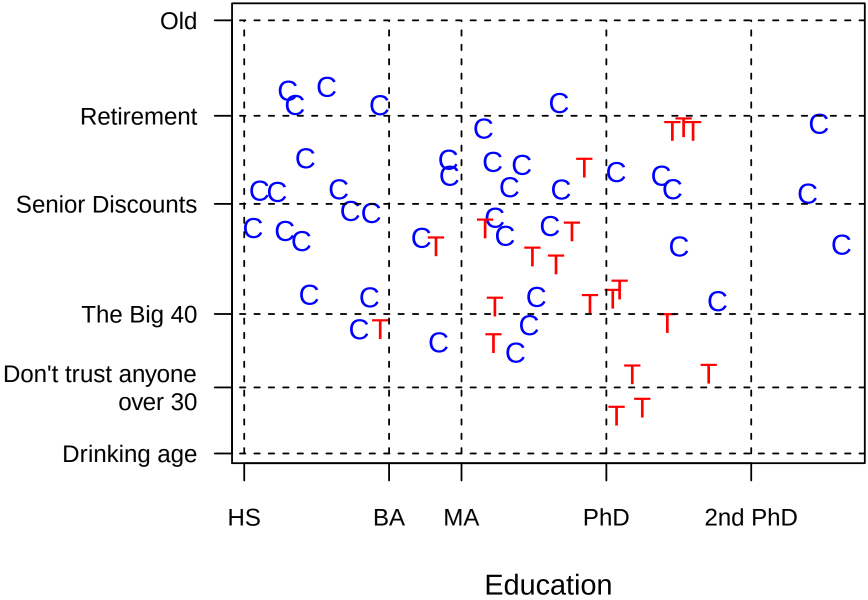

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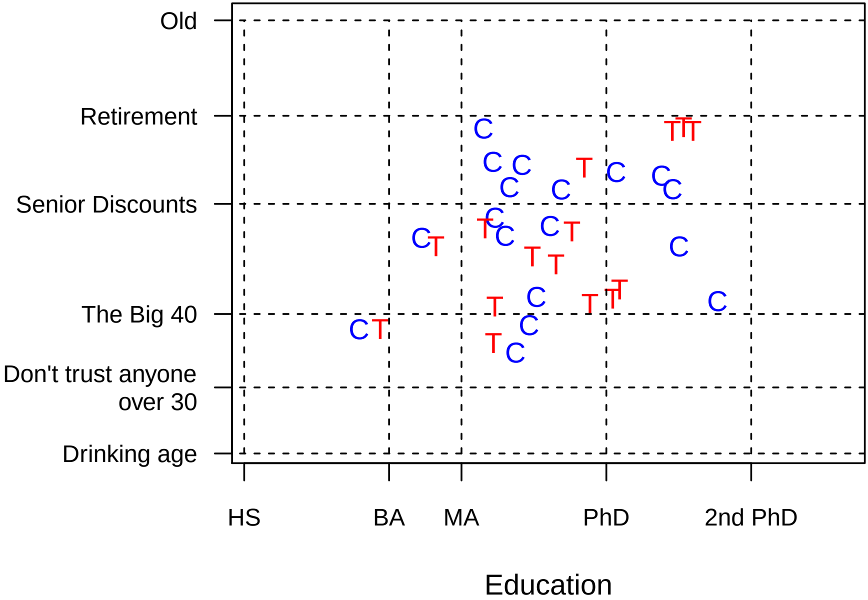

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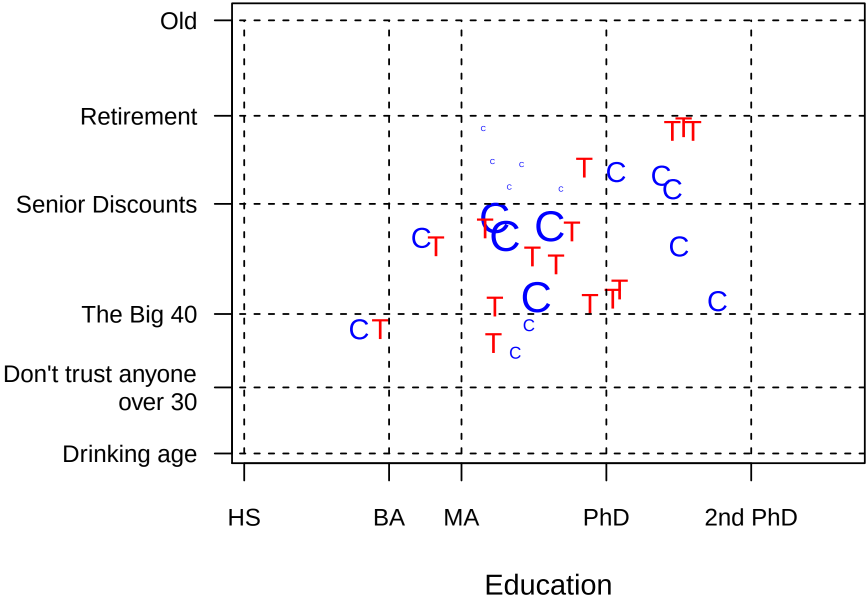

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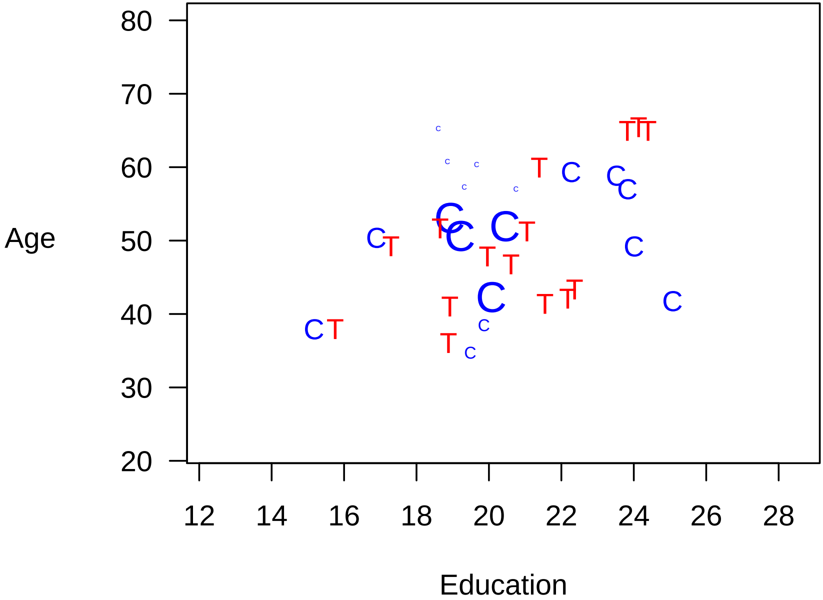

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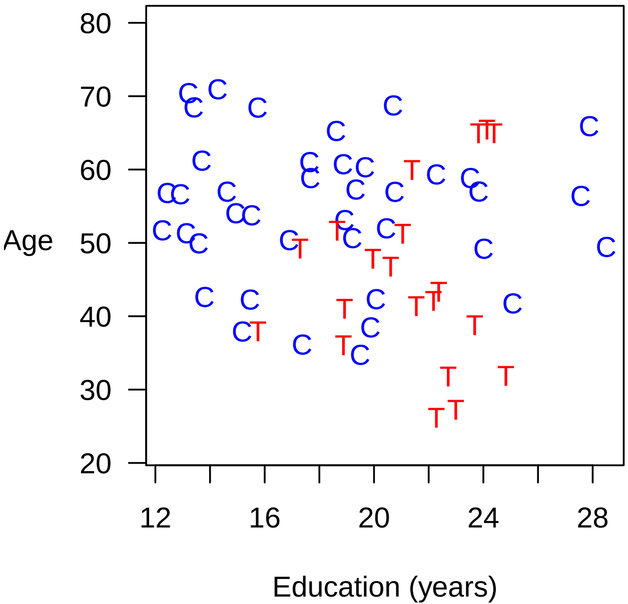

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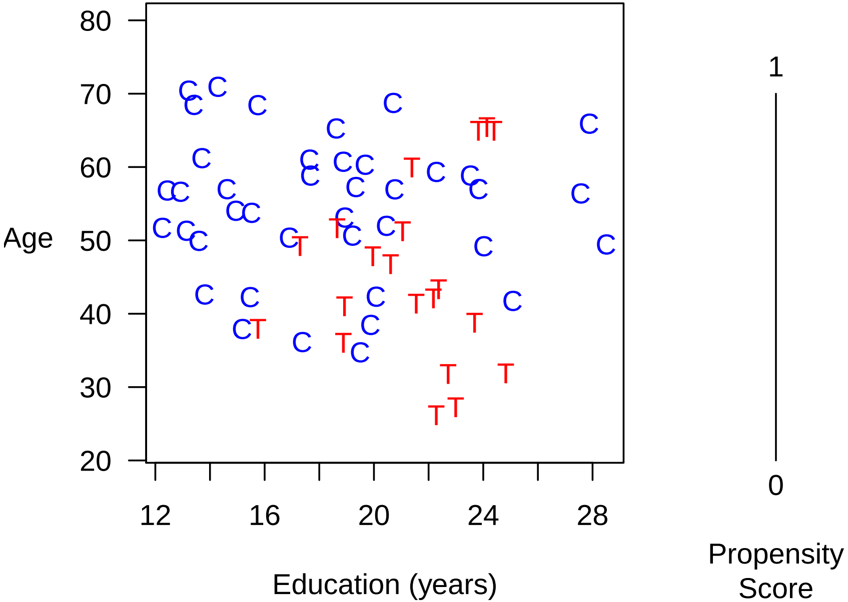

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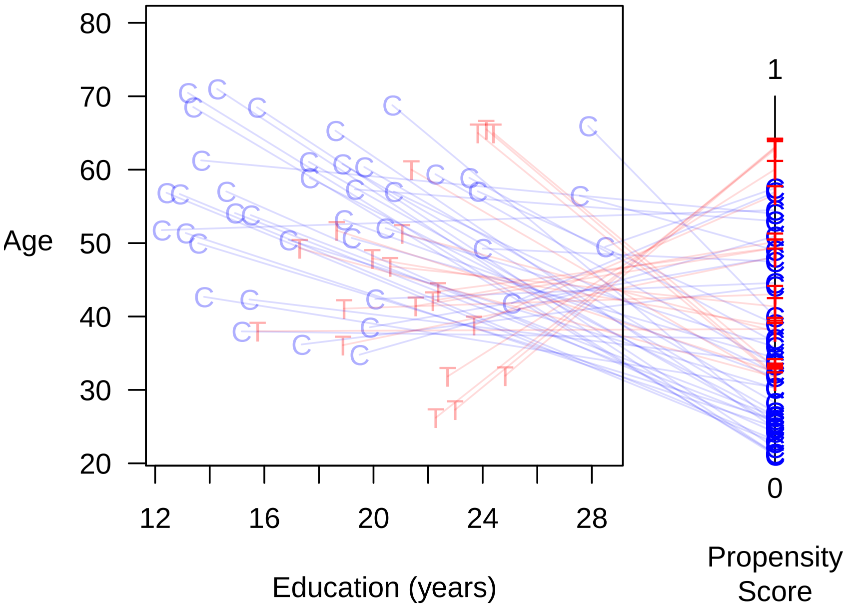

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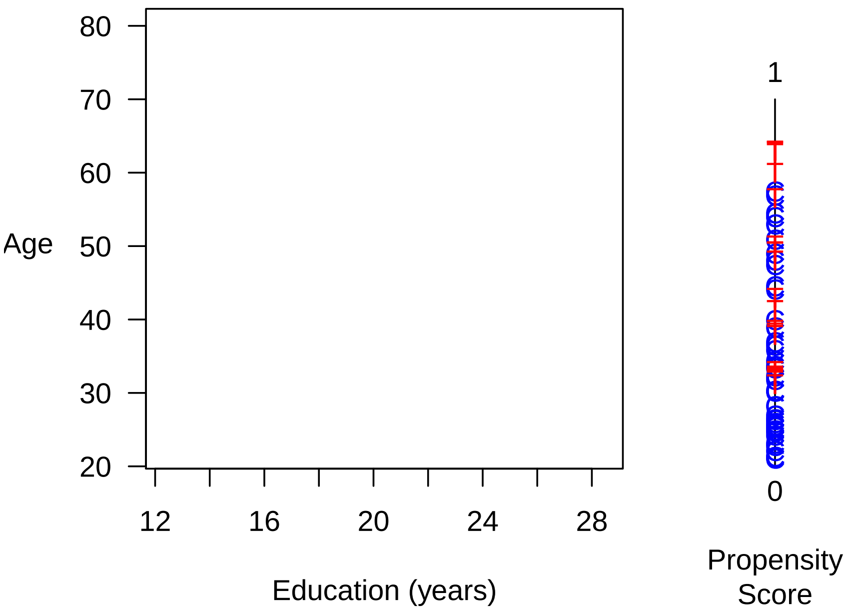

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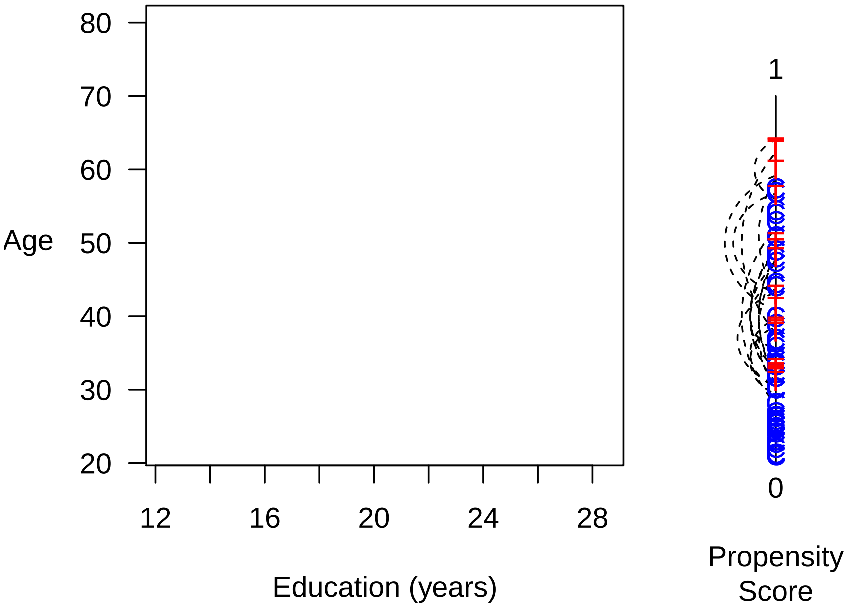

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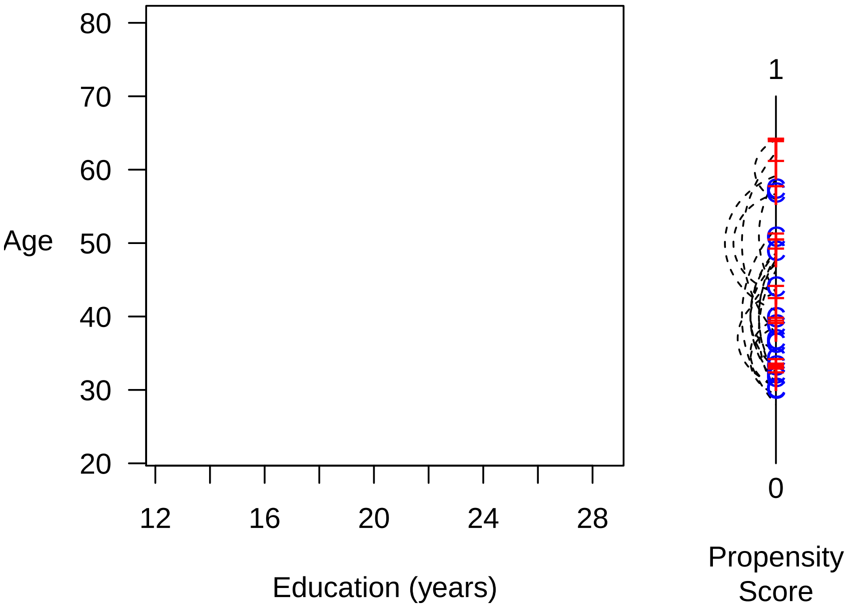

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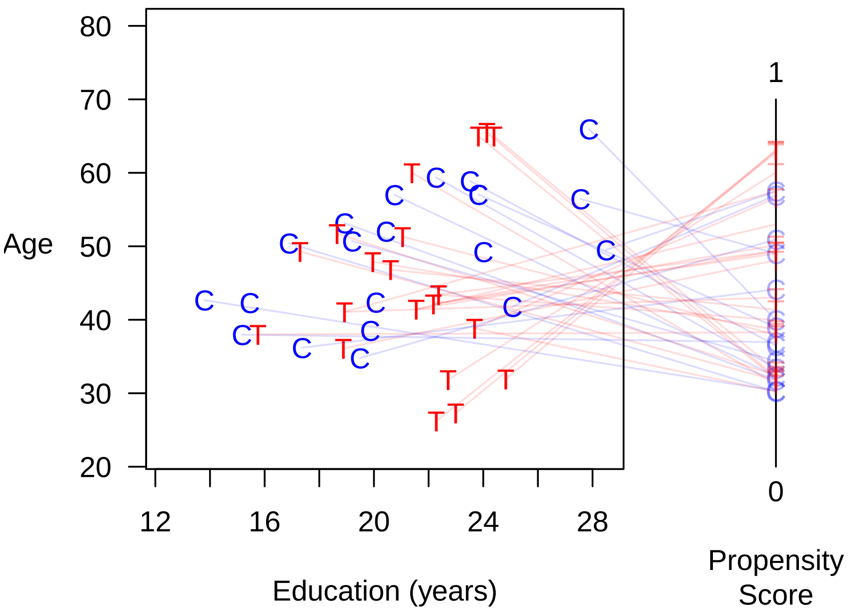

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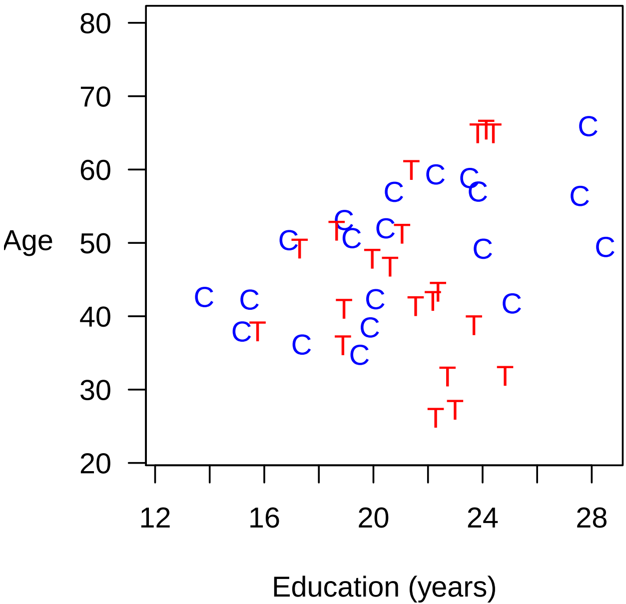

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A: Sure. Generally speaking, they are a way of borrowing information.

Eight Schools Data

School	Est. Effect	SE
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Policy Question: What is the effect size in School A?

Eight Schools Background

- ETS analyzes special coaching program on test scores
- 8 separate parallel experiments in different high schools
- Outcome was the score on a special administration of SAT-V with scores varying between 200 and 800 ($\mu = 500, \sigma = 100$)
- SAT is designed to be resistant to short-term efforts intended to boost performance, but each school thought it was a success.
- No prior reason to believe that one program would be more effective than the others
- Treatment effects estimated controlling for PSAT-M and PSAT-V scores
- A bit over the 30 students in each school
- For the sake of scale: an 8-point increase in the score indicates about 1 more test item was answered correctly.
- (Analysis is from Rubin 1981, treatment via Gelman et al 2015)

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There are two clear options:

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   - the pooled estimate is 7.7 with standard error of 4.1. Thus the confidence interval is $[-.5, 15.9]$

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- This model contains both the separate and pooled estimates as limiting special cases. If we force the standard deviation of the true effects to be zero, then all school get the same estimate, if we let it go to infinity we get the separate estimates

The Model

$$\bar{y}_j|\theta_j \sim \text{Normal}(\theta_j, \sigma_j^2)$$
$$\theta_j|\mu, \tau \sim \text{Normal}(\mu, \tau^2)$$
$$p(\mu, \tau) = p(\mu|\tau)p(\tau) \propto p(\tau)$$

Known: $\bar{y}_j, \sigma_j^2$
Unknown: τ, μ, θ

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$$\theta_j | \mu, \tau, y \sim \mathrm{N}(\hat{\theta}_j, V_j)$$
$$\hat{\theta}_j = \frac{\frac{1}{\sigma_j^2}\bar{y}_j + \frac{1}{\tau^2}\mu}{\frac{1}{\sigma_j^2} + \frac{1}{\tau^2}}$$
$$V_j = \frac{1}{\frac{1}{\sigma_j^2} + \frac{1}{\tau^2}}$$

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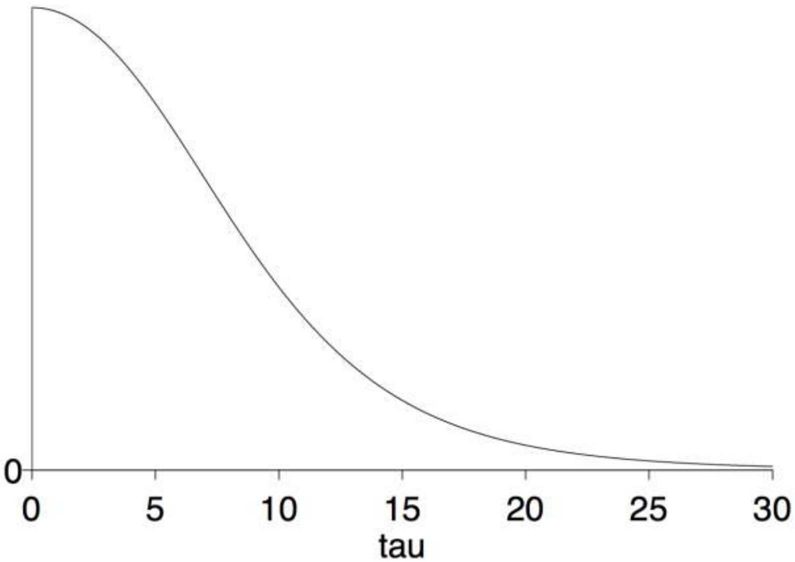
- The form show us that the amount of shrinkage is relative to our certainty about the estimate and how much we believe the individual effects matter
- Our final guess is that the median effect for school A is about 10 points with 50% probability between 7 and 16

Results



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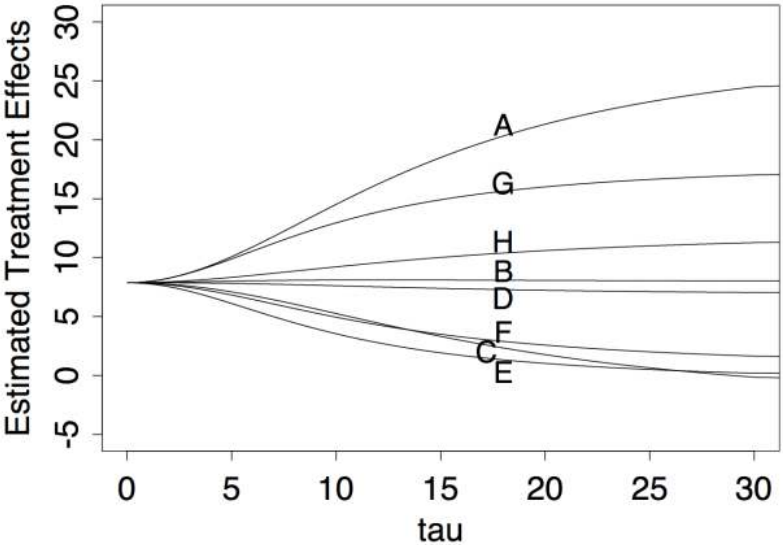

Estimated Treatment Effects
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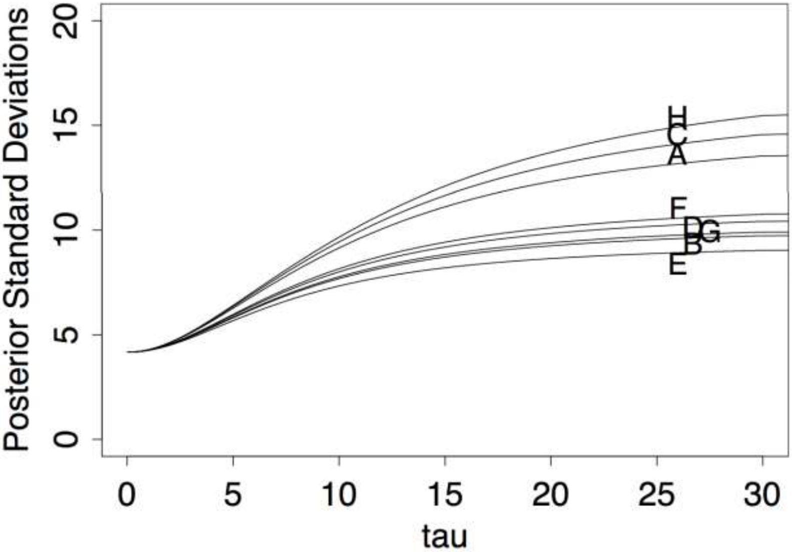
0
5
10
15
20
25
30
A
G
H
B
D
F
C
E
0
5
10
15
20
25
30
tau

Results


Posterior Standard Deviations
0
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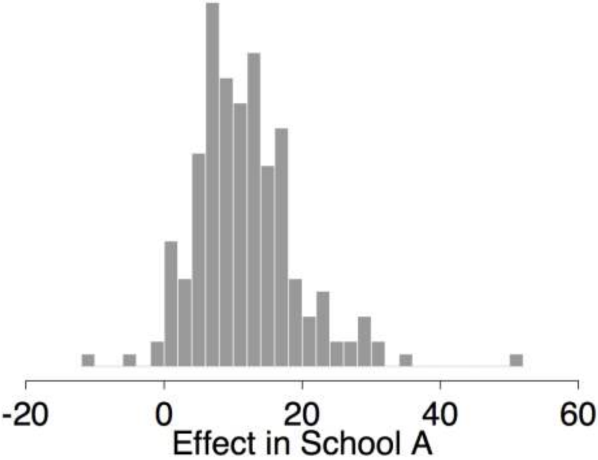

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- Allows us to capture variability in our treatment effects, variances etc.
- Allows us to model dependence in our error terms

Q: How do we determine power?

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A: A combination of the effect size, the variance and the sample size. Unfortunately, only one of which we know. See the `DeclareDesign` suite of packages for this and so much more!

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A: I think the biggest and most frequent mistakes I see are:

- not being clear about the estimand
- mistaking not significant results for a finding of zero effect (need equivalence tests)
- lack of clarity about the counterfactual and common support

Q: When should you pick your statistical strategy? How do you balance pre-planning research / literature reviews with potential problems with data/causal assumptions? How much data exploration should you do up front compared to exploration throughout the question? If you have a causal question or idea but arent sure of data, how should you go about searching for potential data and making sure assumptions are reasonable?

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A: Let's chat.

Q: What do you believe will be the biggest applications for social statistics in the future?

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2. Read the relevant textbook material
3. Derive the equations/math
4. Try to explain it to someone else

Where are you?

Where are you?

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
  - CDF and PDF (discrete and continuous)

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

Your New Weapons

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- Diagnosing and Fixing Regression Problems
  - Non-normality
  - Outliers, leverage, and influence points, Robust Regression
  - Non-linearities and GAMs
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- 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

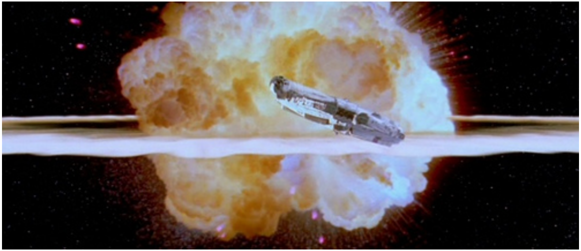
Beyond Linear Regressions

Beyond Linear Regressions

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

Thanks!

Thanks so much for an amazing semester.



Fill out your evaluations!

Weighting with the Propensity Score

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- Treated and control samples are unrepresentative of the overall population.
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- Reweight them to be more representative.

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- Sample mean is biased:

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- Reweights the sample to be representative of the population.

Back to causal effects

- With a completely randomized experiment, we can just use the simple differences in means:

$$\mathbb{E}[Y_i|D_i = 1] - \mathbb{E}[Y_i|D_i = 0] = \mathbb{E}[Y_i(1)] - \mathbb{E}[Y_i(0)]$$

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\end{aligned}$$

- With subclassification, we binned X_i, calclulated within-bin differences and then averaged across the bins, just like this.

Searching for the weights

$$\mathbb{E}[Y_i(d)] = \sum_{x \in \mathcal{X}} \mathbb{E}[Y_i | D_i = d, X_i = x]\mathbb{P}(X_i = x)$$

- Compare this to the the within treatment group average:

$$\mathbb{E}[Y_i | D_i = d] = \sum_{x \in \mathcal{X}} \mathbb{E}[Y_i | D_i = d, X_i = x]\mathbb{P}(X_i = x | D_i = d)$$

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- How should we reweight the data from an observational study?

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- How should we reweight the data from an observational study?
- If we were to reweight the data by $W_i = 1/\mathbb{P}(D_i = d | X_i)$, then we would break the relationship between D_i and X_i.

Weights

- Single binary covariate. Define the weight function:

$$w(d, x) = \frac{1}{e(x)^d (1 - e(x))^{1-d}}$$

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- If $(D_i, X_i) = (0, 0)$:

$$W_i = \frac{1}{1 - e(0)} = \frac{1}{\mathbb{P}(D_i = 0|X_i = 0)}$$

Example

	$X_i = 0$	$X_i = 1$
$D_i = 0$	4	3
$D_i = 1$	4	9

- $\mathbb{P}(D_i = 1 | X_i = 0) = 0.5$

Example

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$D_i = 0$	$1/0.5$	$1/0.25$
$D_i = 1$	$1/0.5$	$1/0.75$

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$D_i = 0$	2	4
$D_i = 1$	2	$4/3$

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- Weighted data (the pseudo-population):

	$X_i = 0$	$X_i = 1$
$D_i = 0$	8	12
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- Important point: $\mathbb{P}_W(D_i = 1|X_i = 1) = \mathbb{P}_W(D_i = 1|X_i = 0) = \frac{1}{\omega^*}$
- $\rightsquigarrow$ D_i independent of X_i in the reweighted data.

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- What is the weighted mean for the treated group?

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- We want to see what the conditional weighted mean identifies:

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- This is sometimes called the Horvitz-Thompson estimator due to the close connection to the survey sampling estimator.