Week 10: Causality with Measured Confounding

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Princeton

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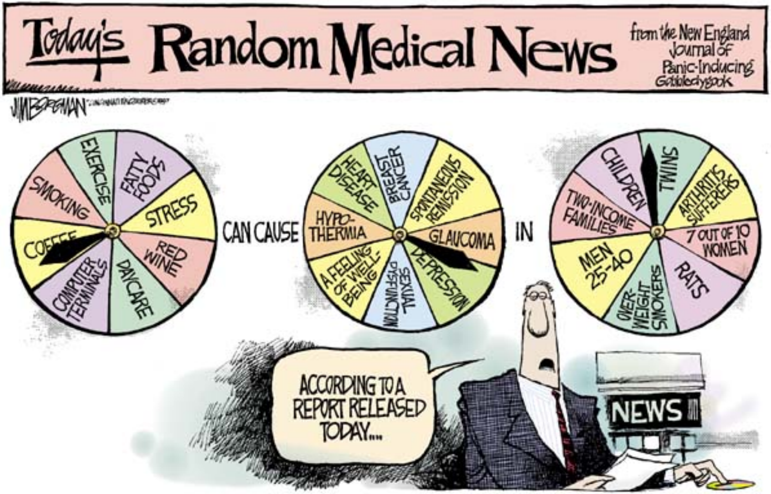
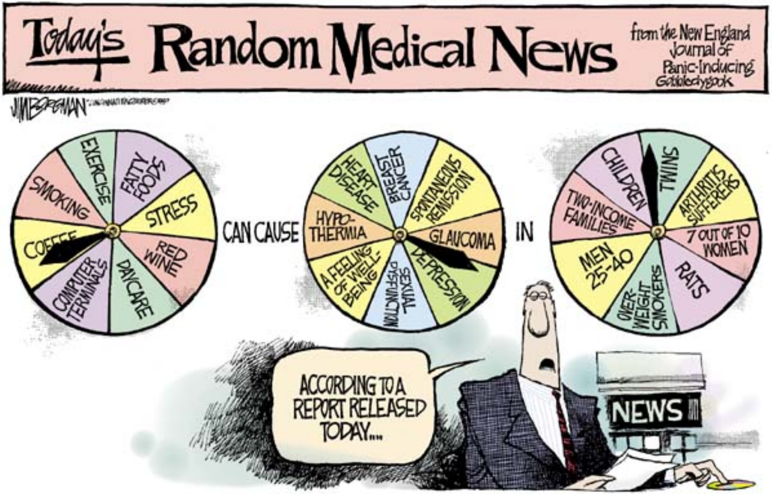
[1]These slides are heavily influenced by Matt Blackwell, Jens Hainmueller, Erin Hartman, Kosuke Imai, Gary King, and Ian Lundberg.

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

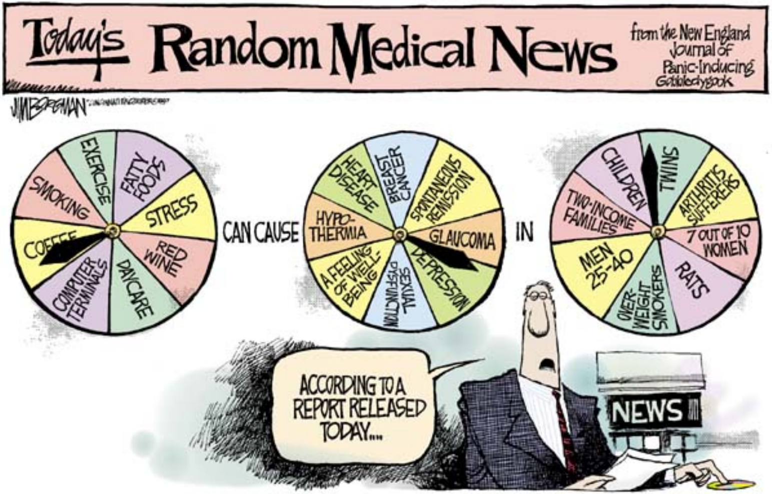
- Last Week
  - frameworks for causal inference
- This Week
  - experimental ideal
  - identification with measured confounding
  - estimation via stratification, matching and regression
- Next Week
  - approaches with unmeasured confounding
- Long Run
  - causal frameworks → inference → regression → causal inference



Lancet 2001: negative correlation between coronary heart disease mortality and level of vitamin C in bloodstream (controlling for age, gender, blood pressure, diabetes, and smoking)



Lancet 2002: no effect of vitamin C on mortality in controlled placebo trial (controlling for nothing)



Lancet 2003: comparing among individuals with the same age, gender, blood pressure, diabetes, and smoking, those with higher vitamin C levels have lower levels of obesity, lower levels of alcohol consumption, are less likely to grow up in working class, etc.

Why So Much Variation?

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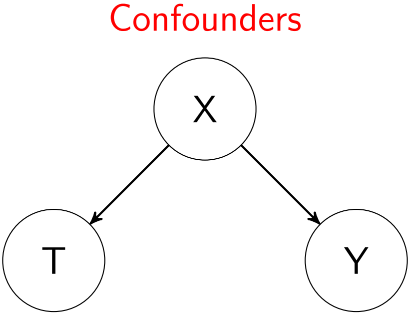

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- Cannot always randomize so we do observational studies, where we adjust for the observed covariates and hope that unobservables are balanced
- Better than hoping: design observational study to approximate an experiment
    - "The planner of an observational study should always ask himself [sic]: How would the study be conducted if it were possible to do it by controlled experimentation" (Cochran 1965)

Experiment review

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     - Treatment assignment does not depend on any potential outcomes.
     - Sometimes written as $T_i \perp\!\!\!\perp (\mathbf{Y}(1), \mathbf{Y}(0))$

Why do Experiments Help?

Remember selection bias?

$$
\begin{aligned}
& E[Y_i|T_i = 1] - E[Y_i|T_i = 0] \\
& \quad = E[Y_i(1)|T_i = 1] - E[Y_i(0)|T_i = 0] \\
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& \quad = \underbrace{E[Y_i(1) - Y_i(0)|T_i = 1]}_{\text{Average Treatment Effect on Treated}} + \underbrace{E[Y_i(0)|T_i = 1] - E[Y_i(0)|T_i = 0]}_{\text{selection bias}}
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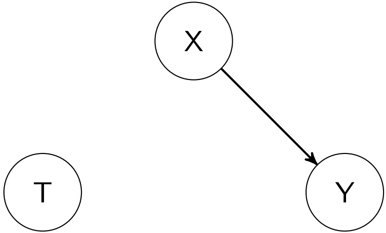
When all goes well, an experiment eliminates selection bias.

Randomization Removes the Arrows into the Treatment



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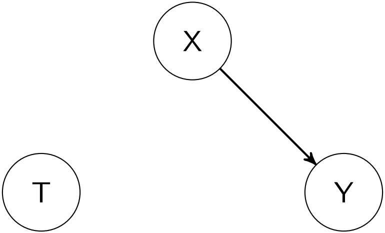

This ensures any observed or unobserved pretreatment covariates have the same distribution in the treatment and the control group. That is, they are balanced.

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- Generally, stratified designs mean that the probability of treatment depends on a covariate, X_i: $P(T_i = 1 | X_i = x)$.
- Conditional randomization assumptions:
  - Positivity: $0 < P(T_i = 1 | X_i = x) < 1$ for all i and x.
  - Unconfoundedness: $P(T_i = 1 | \mathbf{X}, \mathbf{Y}(1), \mathbf{Y}(0)) = P(T_i = 1 | X_i)$

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- ATE is just the weighted average of the within-strata differences in means.
- Identified because the last line is a function of observables.
- The averaging is over the distribution of the strata $\rightsquigarrow$ size of the blocks.

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- Of course, in real experiments sometimes people don't always do what we say (compliance problems).

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Next Time: Identification with Measured Confounding!

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  - Analyze this as a stratified randomized experiment with this estimated procedure.
- This is the perspective on observational design that comes out of the potential outcomes framework.
- Core task from this perspective is to reverse engineer the treatment assignment mechanism to find the experiment.

Identification Under Selection on Observables

Identification Assumption

1. $(Y(1), Y(0)) \perp\!\!\!\perp T | X$ *(selection on observables)*
2. $0 < P(T = 1|X) < 1$ *with probability one (common support)*

Identification Result

Given selection on observables we have

$$\begin{aligned} E[Y(1) - Y(0)|X] &= E[Y(1) - Y(0)|X, T = 1] \\ &= E[Y|X, T = 1] - E[Y|X, T = 0] \end{aligned}$$

Therefore, under the positivity assumption:

$$\begin{aligned} \tau_{ATE} &= E[Y(1) - Y(0)] = \int E[Y(1) - Y(0)|X]\, dP(X) \\ &= \int \big(E[Y|X, T = 1] - E[Y|X, T = 0]\big)\, dP(X) \end{aligned}$$

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Identification Result

Similarly, for the Average Treatment Effect on the Treated (ATT),

$$\begin{aligned}\tau_{ATT} &= E[Y(1) - Y(0)|T = 1] \\ &= \int \left(E[Y|X, T = 1] - E[Y|X, T = 0]\right) dP(X|T = 1)\end{aligned}$$

To identify τ_{ATT} the selection on observables and common support conditions can be relaxed to:

- $Y(0) \perp\!\!\!\perp T | X$ *(Selection on Observables for Controls)*
- $P(T = 1|X) < 1$ *(Weak Positivity)*

Identification Under Selection on Observables

unit	Potential Outcome under Treatment	Potential Outcome under Control		
i	$Y_i(1)$	$Y_i(0)$	T_i	X_i
1	$E[Y(1) \mid X=0, T=1]$	$E[Y(0) \mid X=0, T=1]$	1	0
2			1	0
3	$E[Y(1) \mid X=0, T=0]$	$E[Y(0) \mid X=0, T=0]$	0	0
4			0	0
5	$E[Y(1) \mid X=1, T=1]$	$E[Y(0) \mid X=1, T=1]$	1	1
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$(Y(1),Y(0)) \perp\!\!\!\perp T|X$ implies that we conditioned on all confounders. The treatment is randomly assigned within each stratum of X:

$$
\begin{aligned}
E[Y(0)|X=0,T=1] &= E[Y(0)|X=0,T=0] \text{ and} \\
E[Y(0)|X=1,T=1] &= E[Y(0)|X=1,T=0]
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- Another way: use DAGs and look at back-door paths.

Backdoor paths and blocking paths

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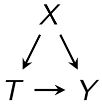

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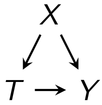
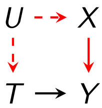


- Here there is a backdoor path $T \leftarrow X \rightarrow Y$, where X is a common cause for the treatment and the outcome.

Other types of confounding



- T is enrolling in a job training program.

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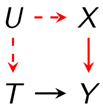

- T is enrolling in a job training program.
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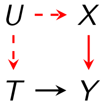

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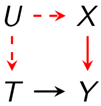

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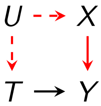

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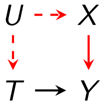

- T is exercise.
- Y is having a disease.
- U is lifestyle.
- X is smoking
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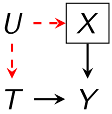
What's the problem with backdoor paths?



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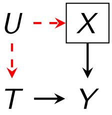

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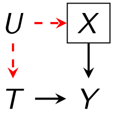

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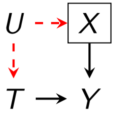

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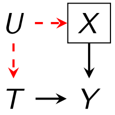

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- In the DAG here, if we condition on X, then the backdoor path is blocked.

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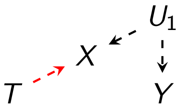

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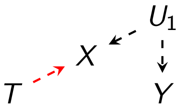

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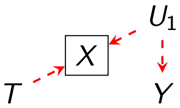

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Don't condition on post-treatment variables

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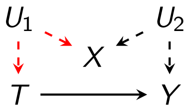
Every time you do, a puppy cries.

(just kidding. but seriously, this is one of the easiest ways to mess up your analysis if you don't know what you are doing.)

M-bias



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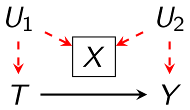

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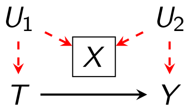

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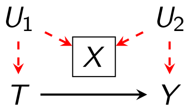

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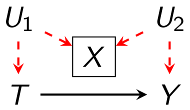

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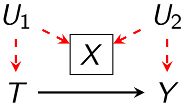

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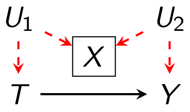

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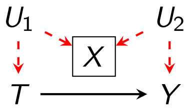

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  - See the Elwert and Winship piece for more!

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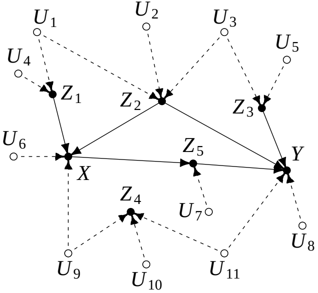
  - what variables to condition on to eliminate the confounding.

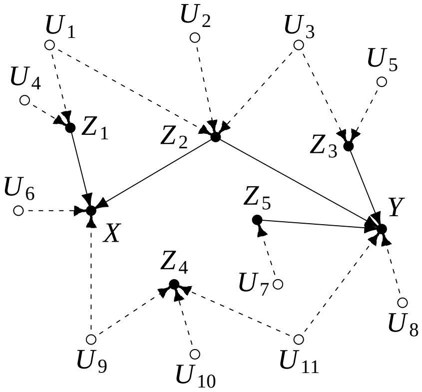
Example: Sufficient Conditioning Sets

We want to estimate the effect of X on Y for this DAG.



Remove arrows out of X.

Example: Sufficient Conditioning Sets



Recall that paths are blocked by "unconditioned colliders" or conditioned non-colliders

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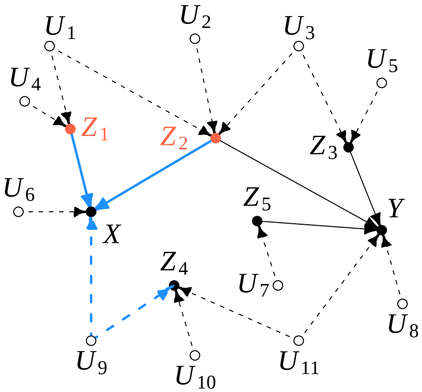

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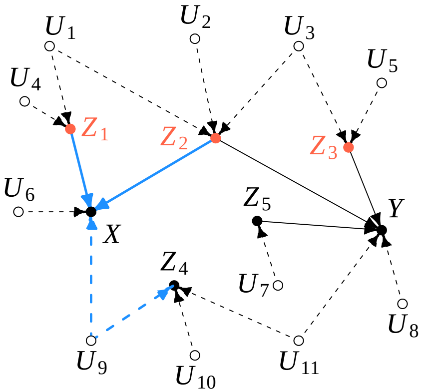

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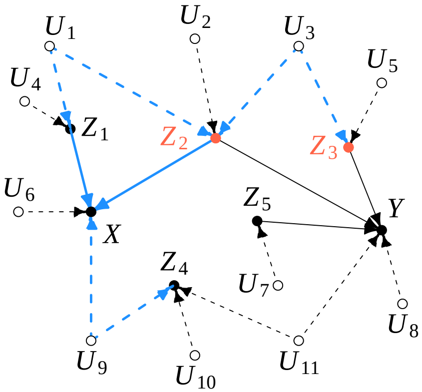
Example: Sufficient Conditioning Sets



No unblocked backdoor paths if we condition on Z_2 and Z_3

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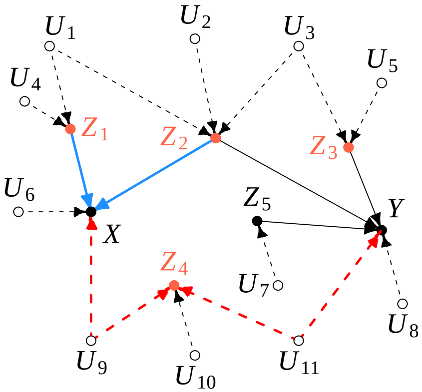
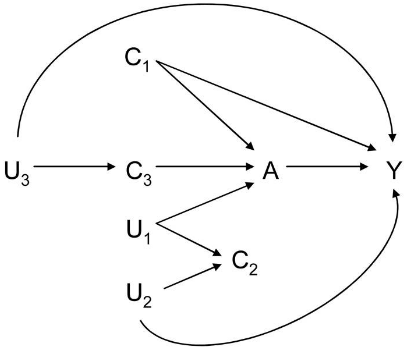


There are unblocked paths if we condition on Z_1, Z_2, Z_3, Z_4

Recall that paths are blocked by "unconditioned colliders" or conditioned non-colliders

More examples in Morgan and Winship

Implications (via Vanderweele and Shpitser 2011)



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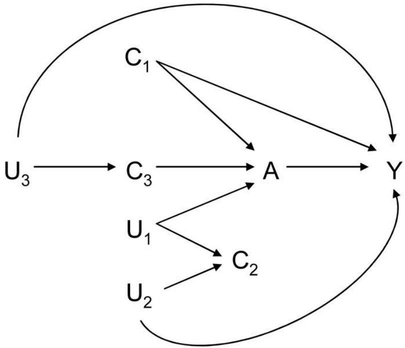

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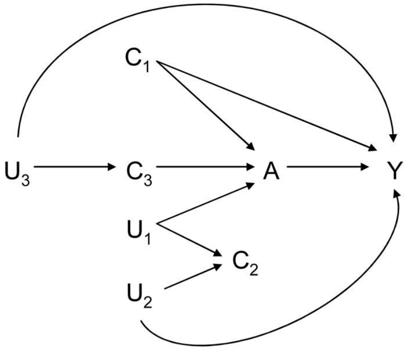

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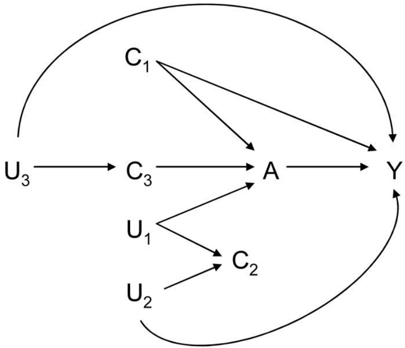

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No unmeasured confounders is not testable

- No unmeasured confounding places no restrictions on the observed data.

$$\underbrace{\left(Y_i(0) \middle| T_i = 1, X_i\right)}_{\text{unobserved}} \stackrel{d}{=} \underbrace{\left(Y_i(0) \middle| T_i = 0, X_i\right)}_{\text{observed}}$$

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Assessing no unmeasured confounders

TABLE VI
THE FOX NEWS EFFECT: INTERACTIONS AND PLACEBO SPECIFICATIONS

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Dep. var.	(1)	(2)	(3)	(4)	(5)
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- Unconfoundedness could still be violated even if you pass this test!

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- No unmeasured confounders $\approx$ randomized experiment.
- These variables are those that block backdoor paths and we want to be *really* sure we know what we are doing if we condition on a post-treatment variable.

So Where Does That Leave Us?

- If we can identify the right covariates $\mathbf{X}$ to get conditional ignorability we can do selection on observables.
- No unmeasured confounders $\approx$ randomized experiment.
- These variables are those that block backdoor paths and we want to be *really* sure we know what we are doing if we condition on a post-treatment variable.
- This still leaves us with a tricky estimation problem as we now need to work with distributions conditional on $\mathbf{X}$.

We Covered

- Identification under selection on observables.
- DAGs return to help us choose what to condition on.

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Next Time: Stratification!

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

- Last Week
  - frameworks for causal inference
- This Week
  - experimental ideal
  - identification with measured confounding
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  - approaches with unmeasured confounding
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  - causal frameworks → inference → regression → causal inference

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- Stratification is great because it makes no assumptions about parametric form.

Stratification Example: Smoking and Mortality (Cochran, 1968)

TABLE 1
DEATH RATES PER 1,000 PERSON-YEARS

Smoking group	Canada	U.K.	U.S.
Non-smokers	20.2	11.3	13.5
Cigarettes	20.5	14.1	13.5
Cigars/pipes	35.5	20.7	17.4

Stratification Example: Smoking and Mortality (Cochran, 1968)

TABLE 2

MEAN AGES, YEARS

Smoking group	Canada	U.K.	U.S.
Non-smokers	54.9	49.1	57.0
Cigarettes	50.5	49.8	53.2
Cigars/pipes	65.9	55.7	59.7

Stratification

To control for differences in age, we would like to compare different smoking-habit groups with the same age distribution

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- for each country, divide each group into different age subgroups
- calculate death rates within age subgroups
- average within age subgroup death rates using fixed weights (e.g. number of cigarette smokers)

Stratification: Example

	Death Rates Pipe Smokers	# Pipe-Smokers	# Non-Smokers
Age 20 - 50	15	11	29
Age 50 - 70	35	13	9
Age + 70	50	16	2
Total		40	40

What is the average death rate for Pipe Smokers?

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	Death Rates Pipe Smokers	# Pipe-Smokers	# Non-Smokers
Age 20 - 50	15	11	29
Age 50 - 70	35	13	9
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What is the average death rate for Pipe Smokers?
$15 \cdot (11/40) + 35 \cdot (13/40) + 50 \cdot (16/40) = 35.5$

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What is the average death rate for Pipe Smokers if they had same age distribution as Non-Smokers?

$15 \cdot (29/40) + 35 \cdot (9/40) + 50 \cdot (2/40) = 21.2$

Smoking and Mortality (Cochran, 1968)

Table 3

Adjusted Death Rates using 3 Age groups

Smoking group	Canada	U.K.	U.S.
Non-smokers	20.2	11.3	13.5
Cigarettes	28.3	12.8	17.7
Cigars/pipes	21.2	12.0	14.2

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- Practically, this is massively important: almost always have data with unique values.
- One option is to discretize (as we did with age) but that doesn't work if there are patterns within the bins.
- Note that this is the problem of approximating conditional expectations and that's the machinery we've been building all semester!

We Covered

- Stratification!

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Next Time: Matching

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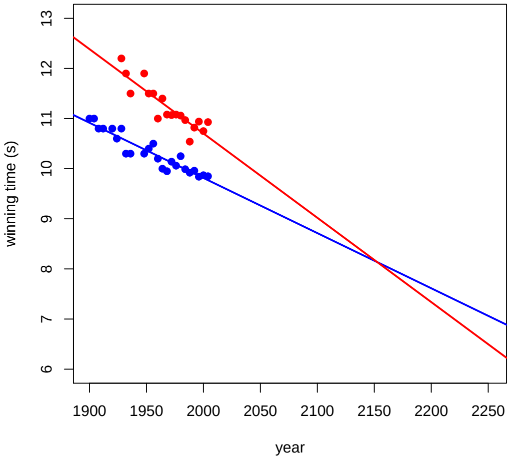

Tatem et al.'s predictions of sprint times with alternate models. Men's times are in blue, women's times are in red.

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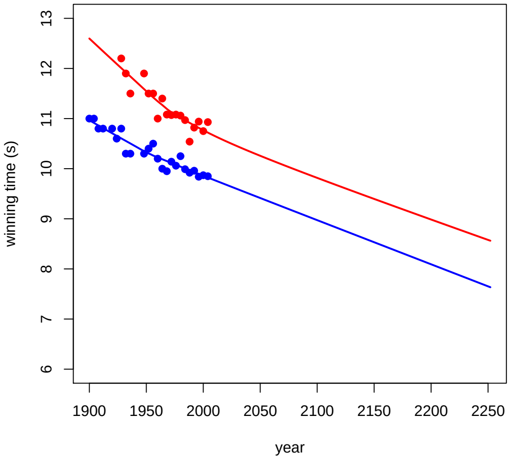

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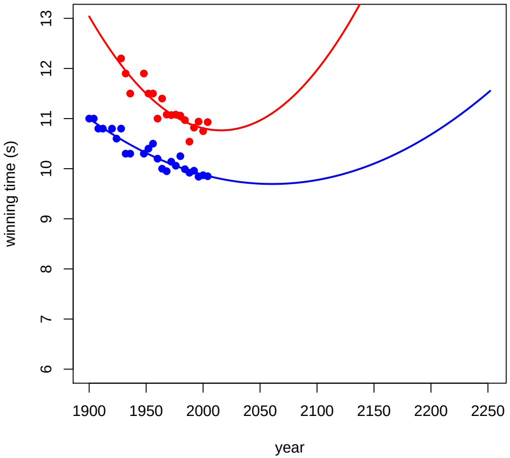

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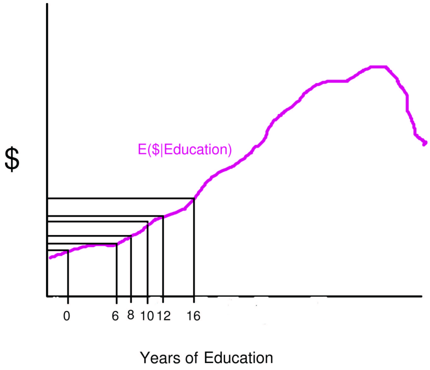
Result

The maximum degree of model dependence: solely a function of the distance from the counterfactual to the data

What Inferences Would You Be Willing to Make?



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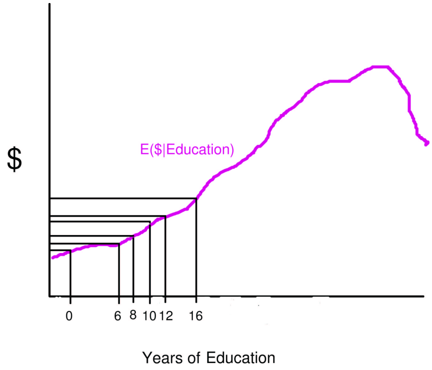

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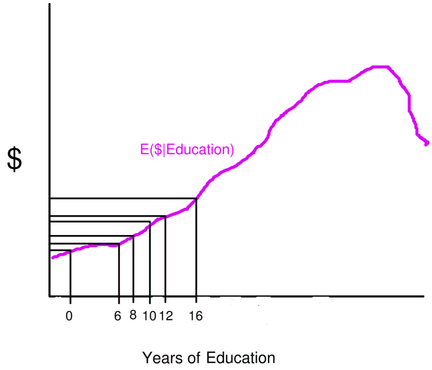

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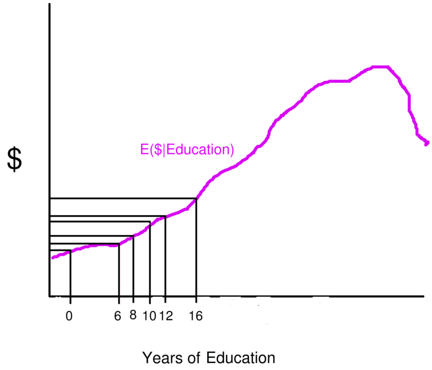

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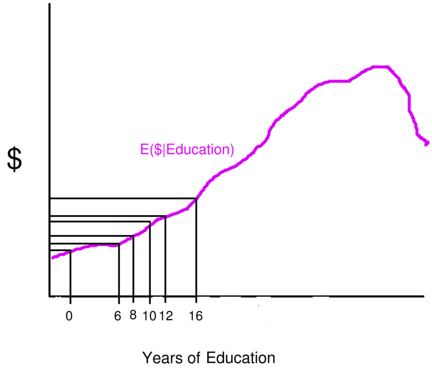
Counterfactual Inferences with Interpolation



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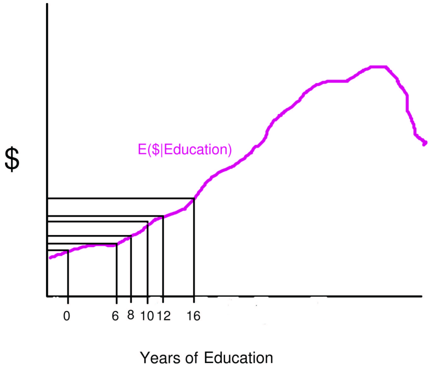

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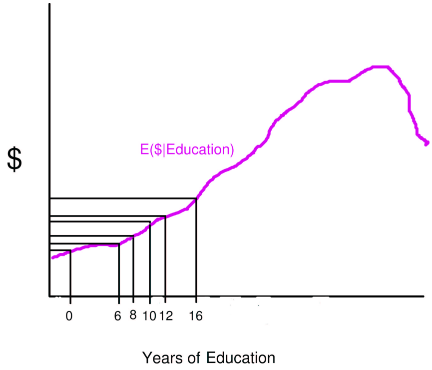

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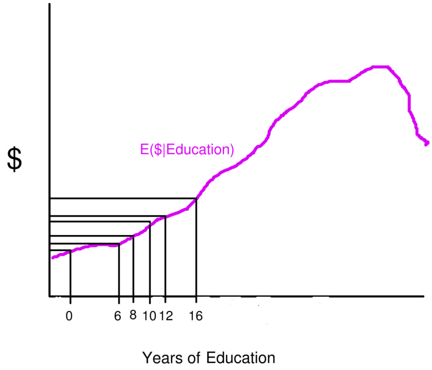

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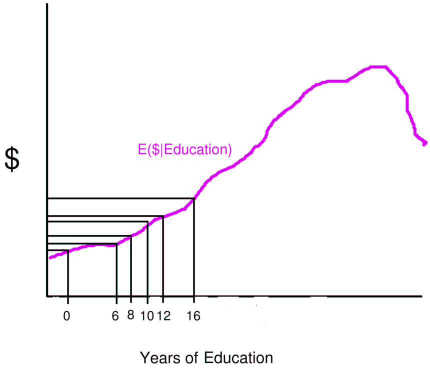

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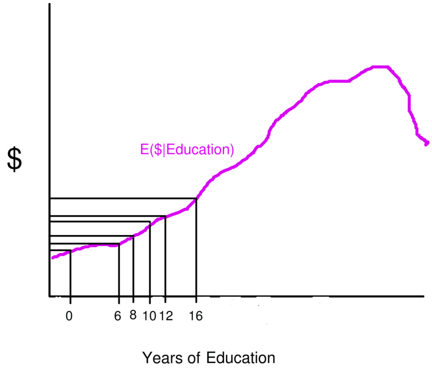

- How much salary would someone receive with 24 years of education (a Ph.D.)?

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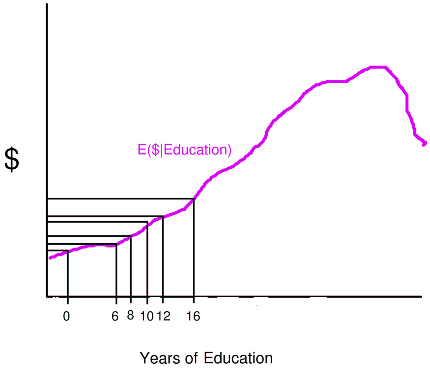

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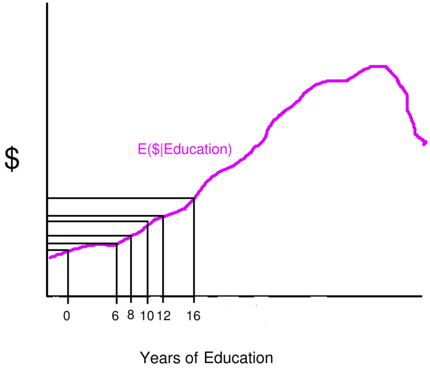
Another Counterfactual Inference with Extrapolation



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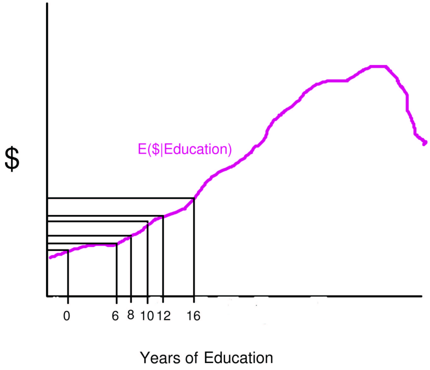

- How much salary would someone receive with 53 years of education?

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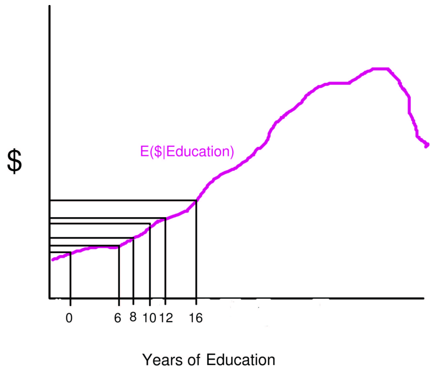

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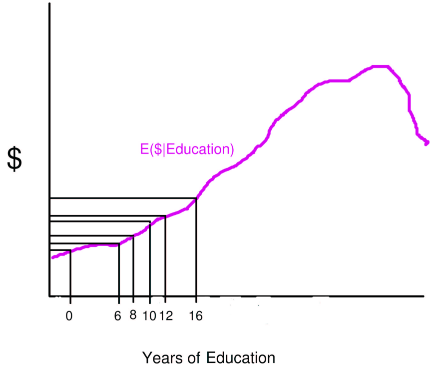

- How much salary would someone receive with 53 years of education?
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- What's changed? How would we recognize it when the example is less extreme or multidimensional?

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- (If X were continuous, we would be reducing ∞ to 2, also by assumption)

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- The difference: an enormous assumption based on convenience, not evidence or theory.

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  - Yet, with a few simple assumptions, we can still run a regression and estimate only 81 parameters.

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  - Yet, with a few simple assumptions, we can still run a regression and estimate only 81 parameters.
- The curse of dimensionality introduces huge assumptions, often unrecognized.

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  - If treated and control groups are better balanced than when you started, due to pruning, model dependence is reduced

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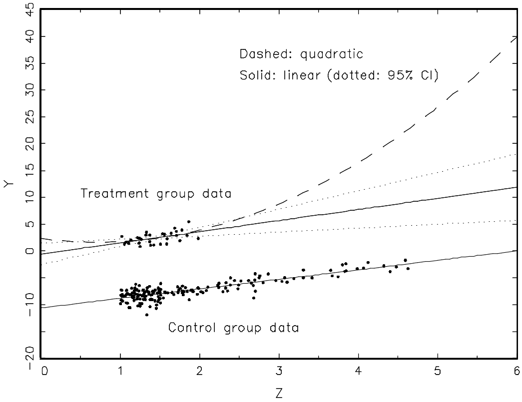

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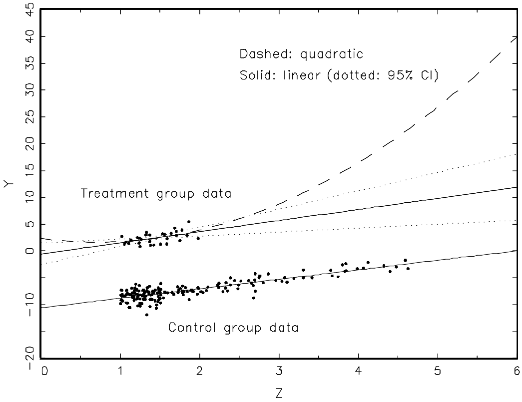

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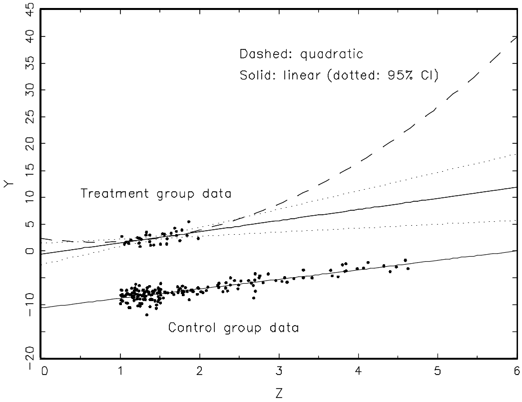

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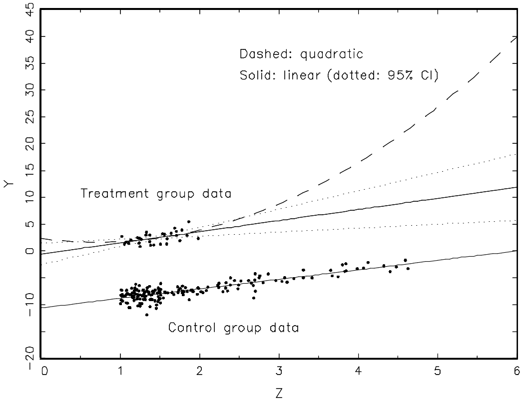

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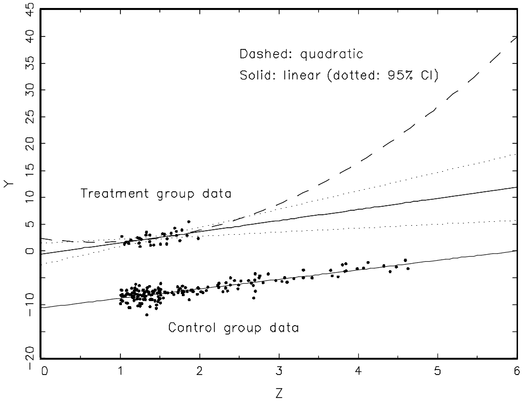

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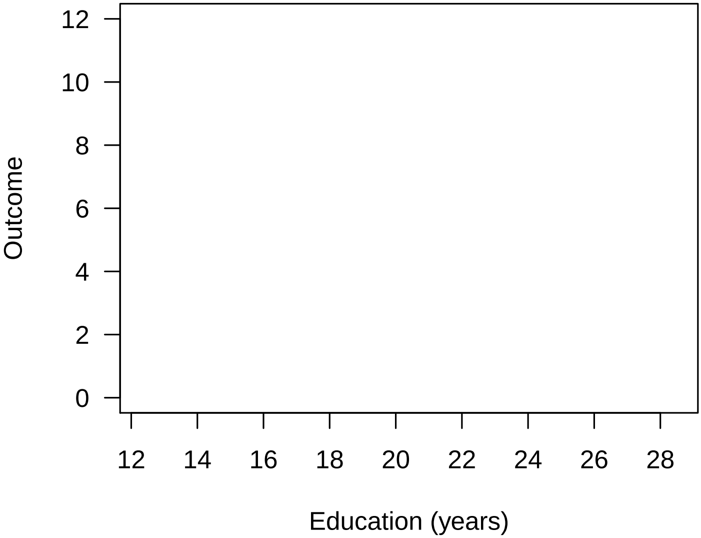

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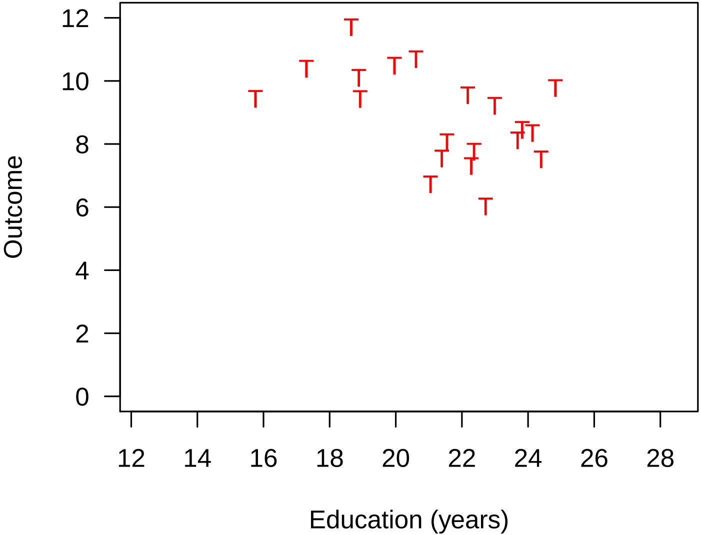

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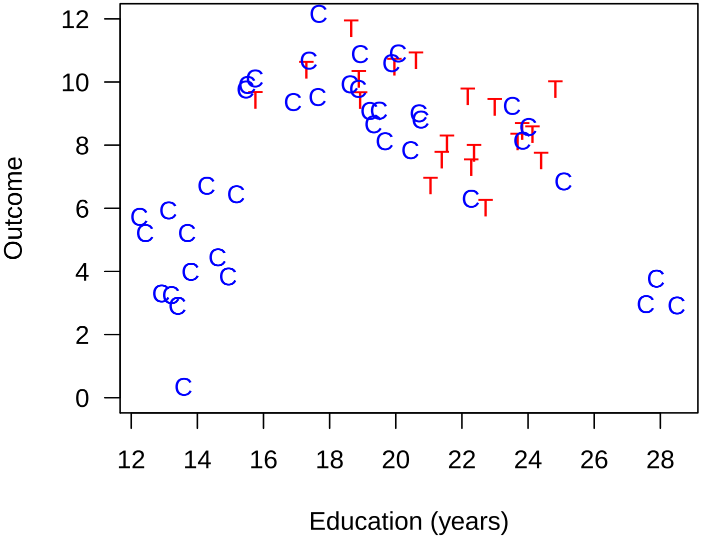

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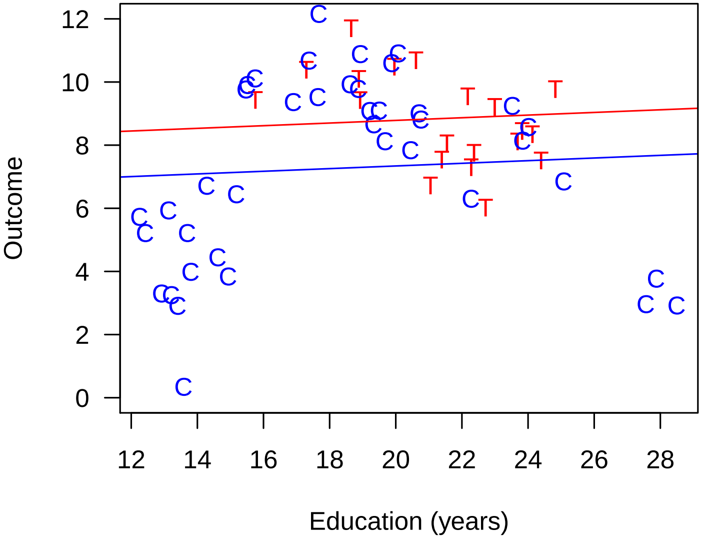

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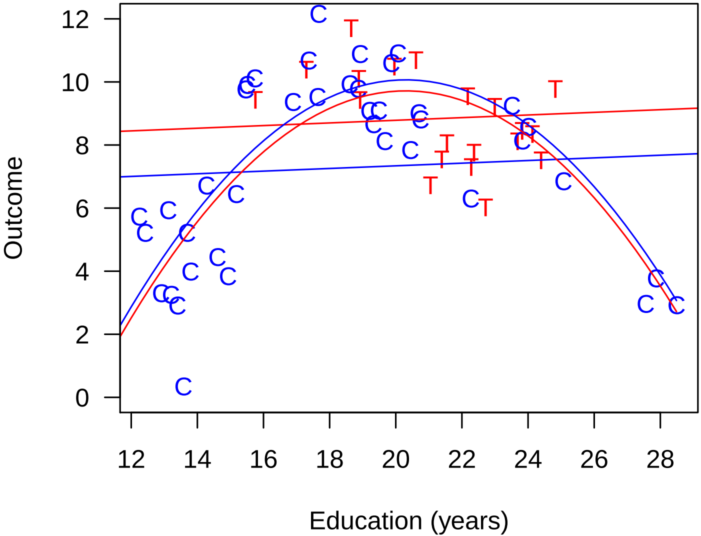

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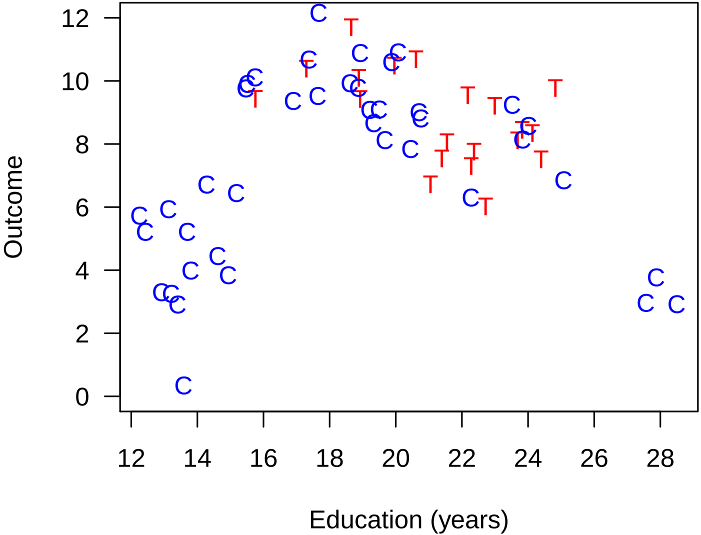

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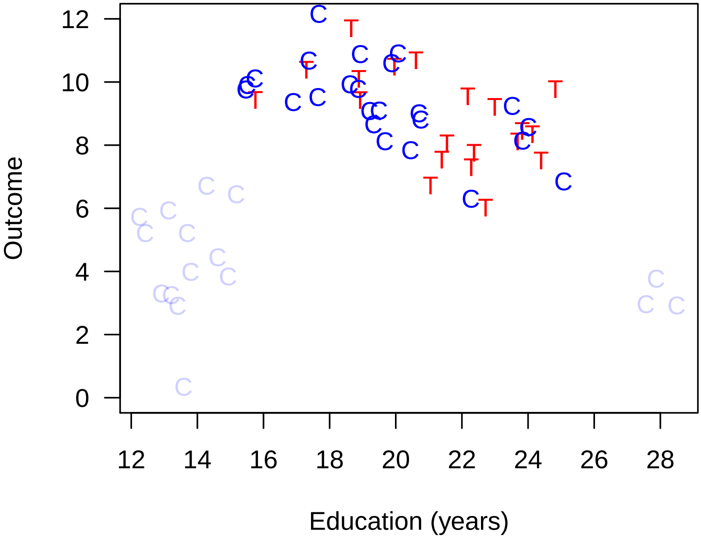

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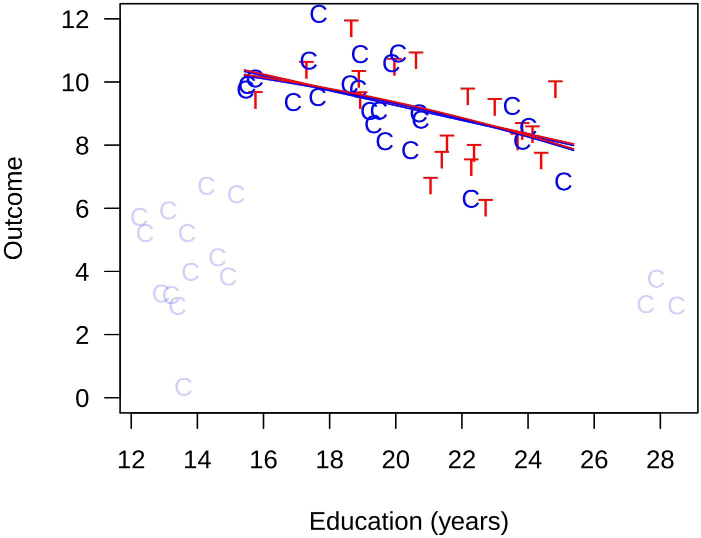

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- 18 control variables (clinical factors, firm characteristics, media variables, etc.)

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- (Normal applications would only use one or a few specifications.)

Reducing Model Dependence

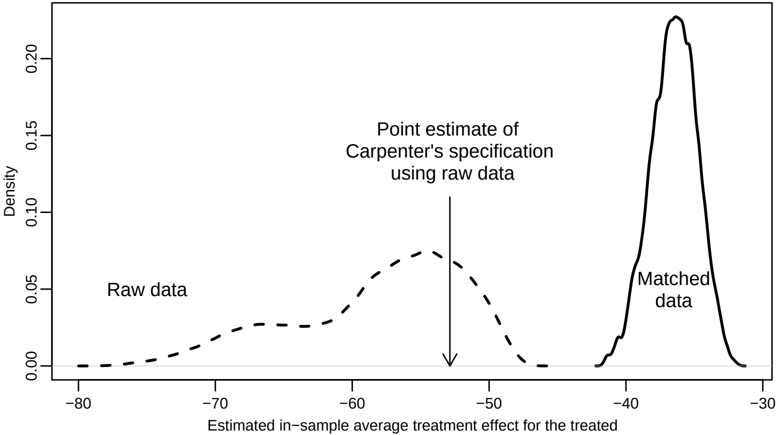

Figure: SATT Histogram: Effect of Democratic Senate majority on FDA drug approval time, across 262, 143 specifications.

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- Last, discard all unmatched control units.
- The distribution of X_i will be exactly the same for treated and matched control:

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- $\rightsquigarrow$ average of the within matched-pair differences.

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- Balance is checkable $\rightsquigarrow$ are T_i and X_i related in the matched data?

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$$\widehat{Y}_i(0) = \frac{1}{M} \sum_{j \in J_M(i)} Y_j$$

- New estimator for the effect:

$$\widehat{\tau}_m = \frac{1}{N_t} \sum_{i=1}^{N_t} (Y_i - \widehat{Y}_i(0))$$

More than 1 control match

- What if we have enough controls to have M matched controls per treated?
  - $P(X_i = x | T_i = 1) = P(X_i = x | T_i = 0, \mathbb{I}_c)$ because M is constant across treated units.
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- Under no unmeasured confounding, $\widehat{Y}_i(0)$ is a good predictor of the true potential outcome under control, Y_i.

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- If M varies by treated unit, need to weight observations to ensure balance.

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  - Choice of distance metric will lead to different matches.

Exact distance metric

- Exact: only match units to other units that have the same exact values of X_i.

$$D_{ij} = \begin{cases} 0 & \text{if } X_i = X_j \\ \infty & \text{if } X_i \neq X_j \end{cases}$$

Euclidean distance

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- Here, $\widehat{\sigma}_k^2$ is the variance of the kth variable:

$$\widehat{\sigma}_k^2 = \frac{1}{N-1} \sum_{i=1}^{N} (X_{ik} - \bar{X}_k)$$

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- $\widehat{\Sigma}$ is the estimated variance-covariance matrix of the observations:

$$\widehat{\Sigma} = \frac{1}{N}\sum_{i=1}^{N}(X_i - \bar{X})(X_i - \bar{X})^T$$

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- Implementation: a caliper, which is the maximum distance we would accept

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- Estimated:

$$\widehat{\tau} = \widehat{\tau}_{ATT} \left(\frac{N_t}{N} \right) + \widehat{\tau}_{ATC} \left(\frac{N_c}{N} \right)$$

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  - Good to be clear about this.

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- Note: in nearest neighbor without replacement the order matters!

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  - L_1: multivariate histogram.

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- Which is the best method? The one that produces the best balance!

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(Approximates Fully Blocked Experiment)

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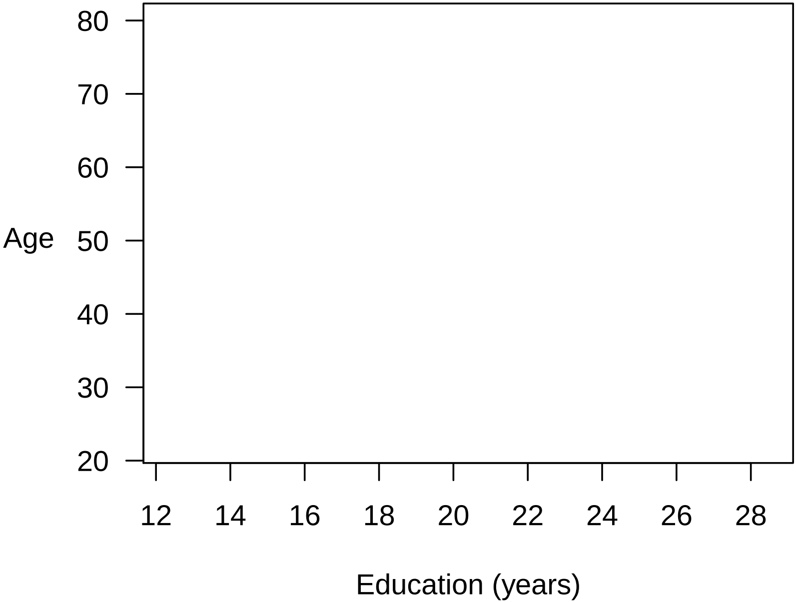

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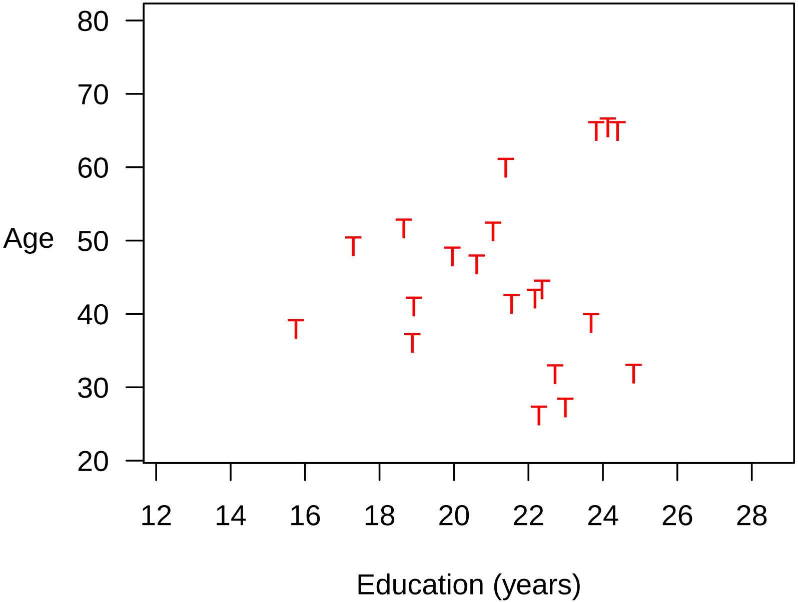

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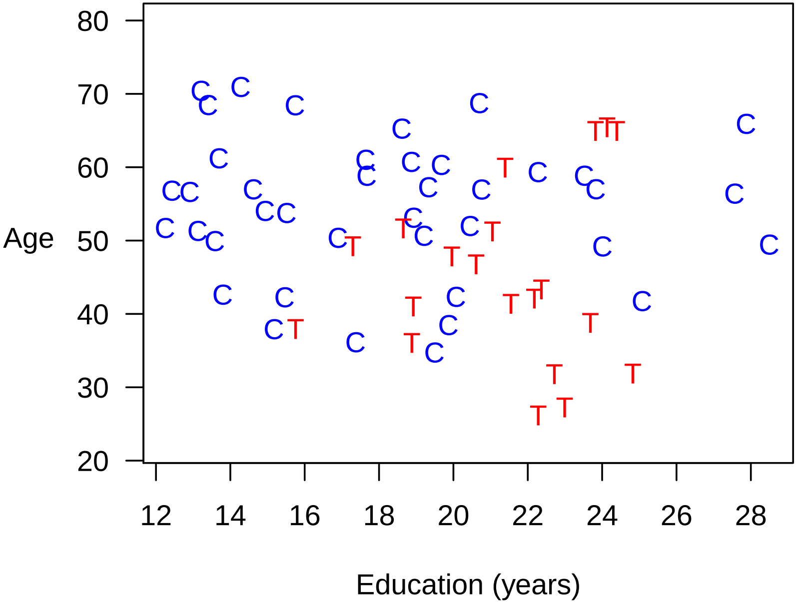

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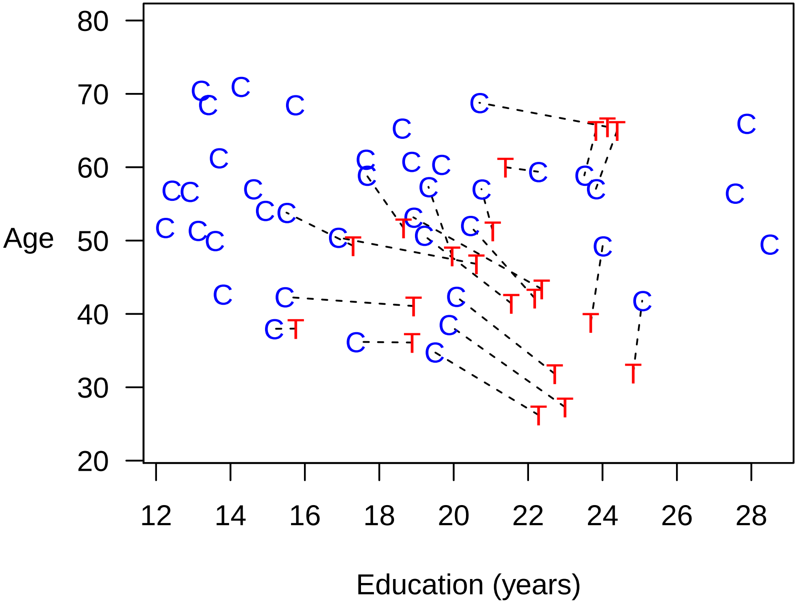

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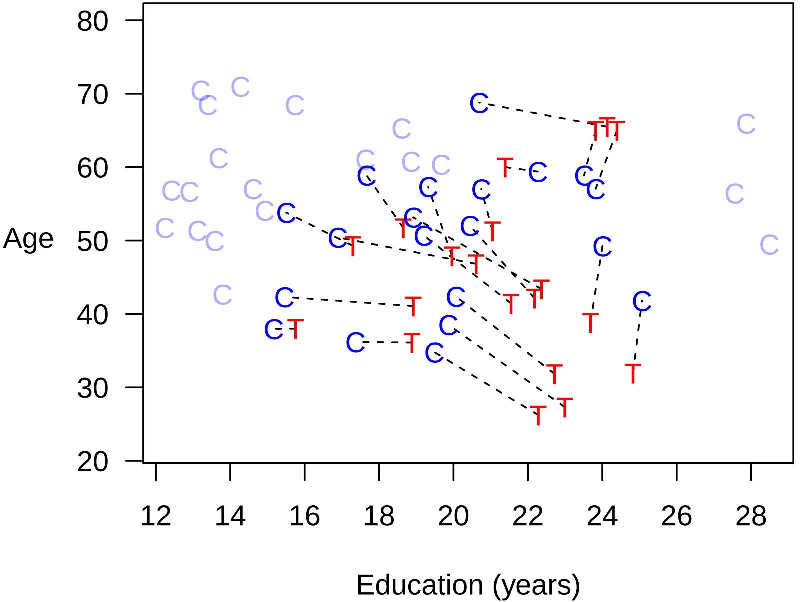

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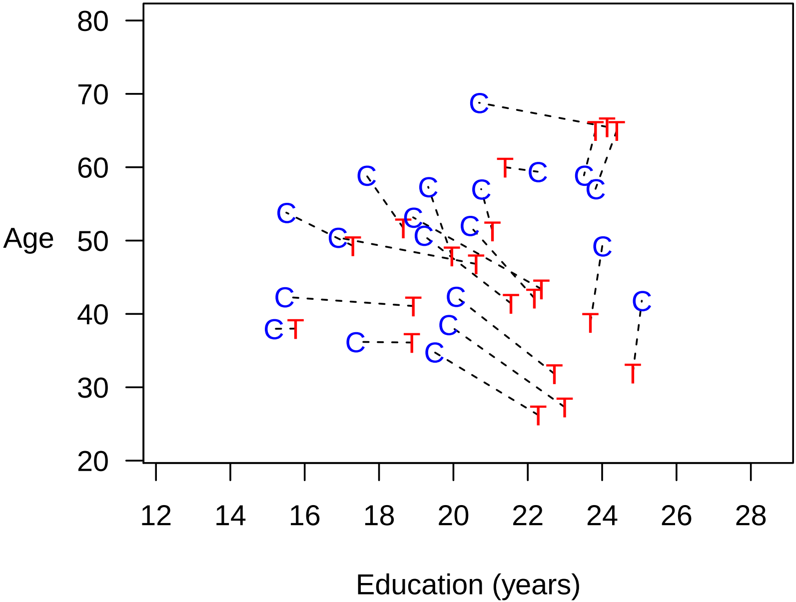

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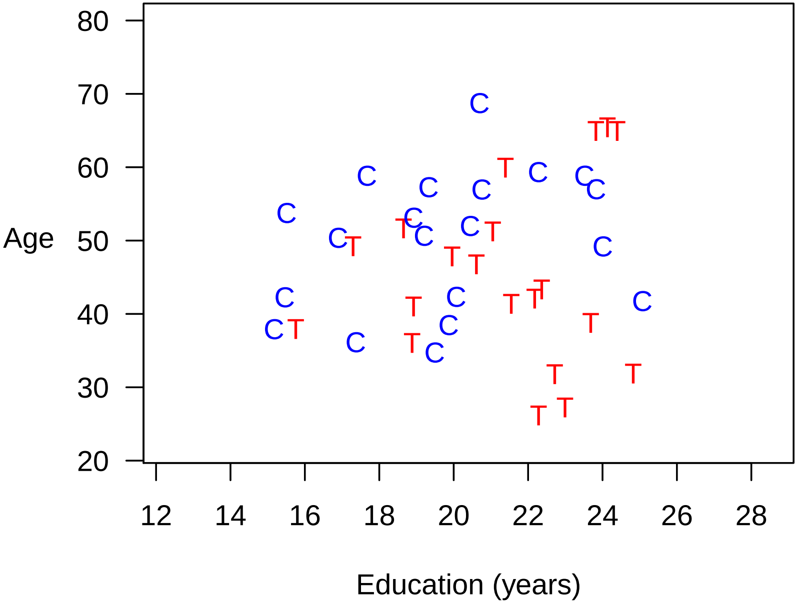

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(Approximates Fully Blocked Experiment)

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Coarsened Exact Matching

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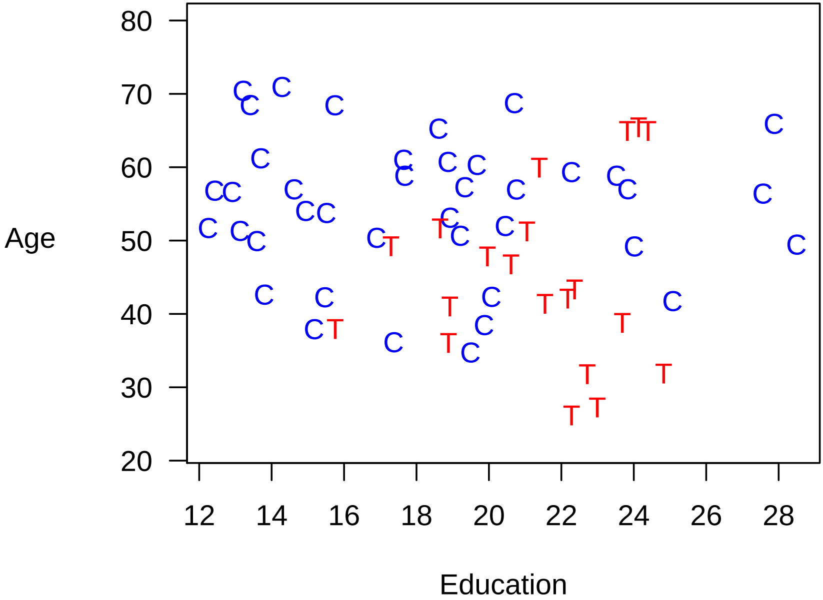

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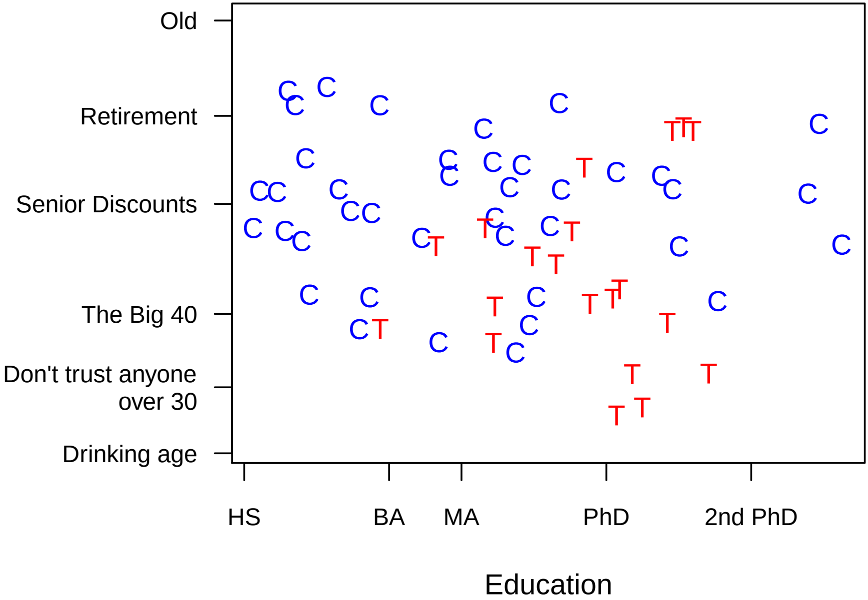

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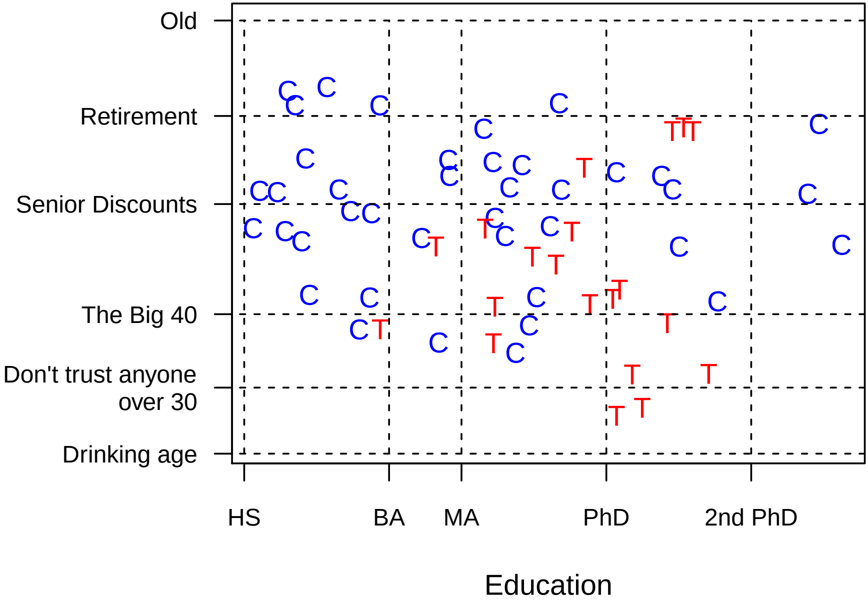

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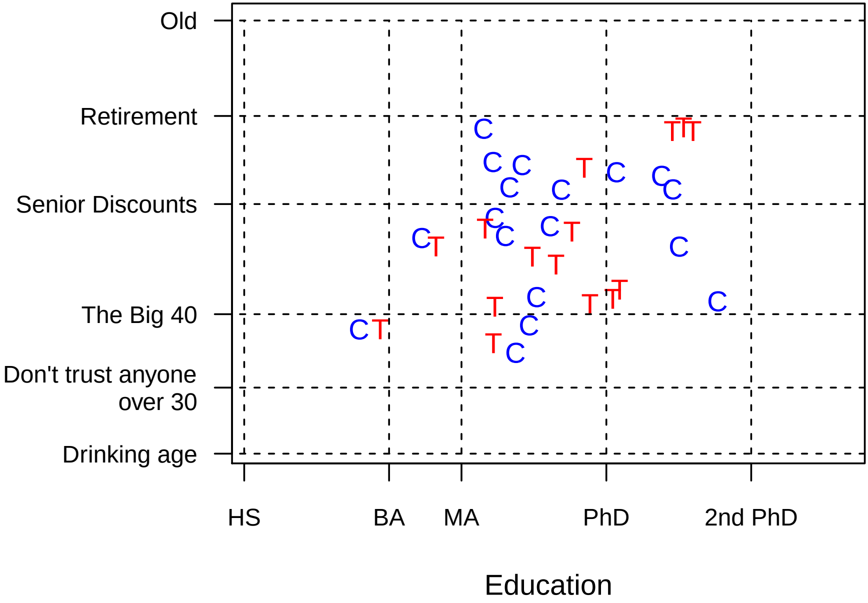

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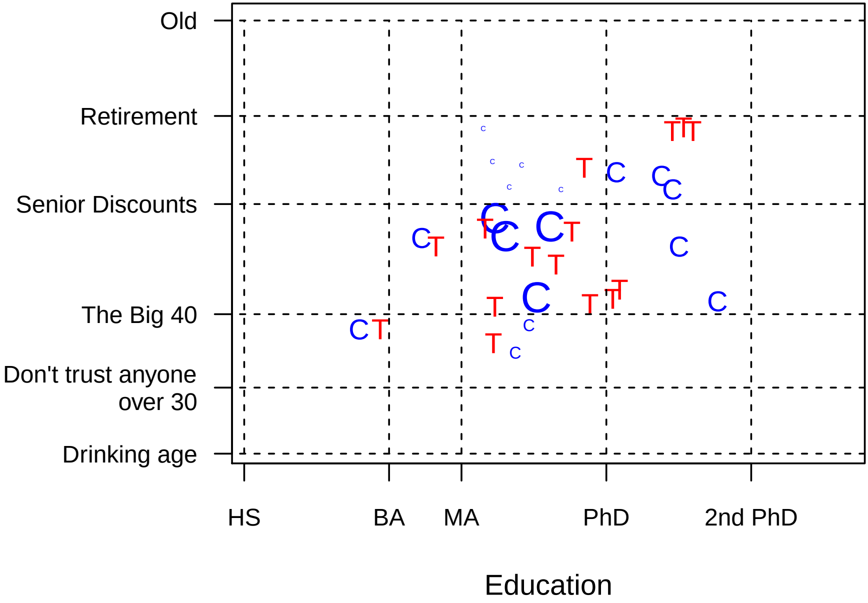

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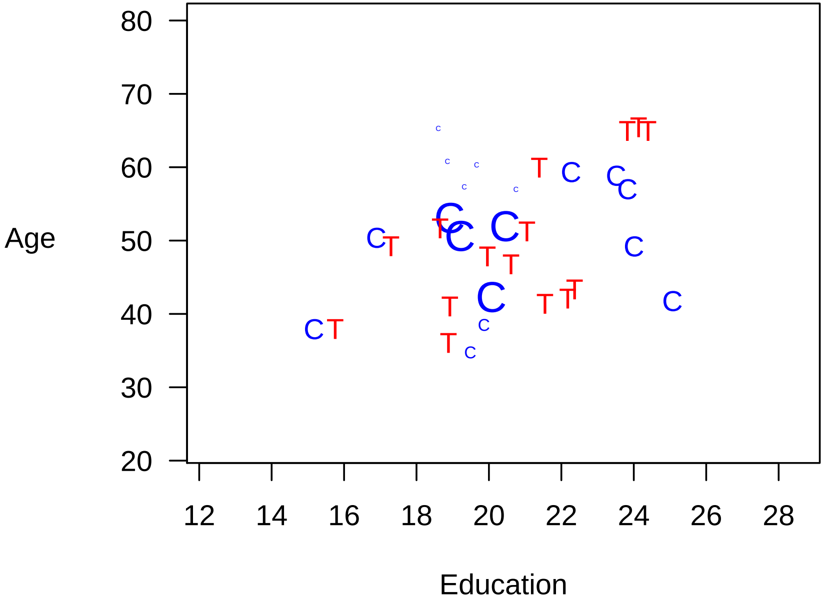

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- Technically the thing it is buying you relative to regression methods we will talk about next is that it limits extrapolation.
- But there is no need for these techniques to compete—we can match and then use regression!
- Importantly, there is nothing magic about matching, it is just another way of conditioning.

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

- Last Week
  - frameworks for causal inference
- This Week
  - experimental ideal
  - identification with measured confounding
  - estimation via stratification, matching and regression
- Next Week
  - approaches with unmeasured confounding
- Long Run
  - causal frameworks → inference → regression → causal inference

Regression and Causality

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- When is regression causal? When the CEF is causal.
- This means that the question of whether regression has a causal interpretation is a question about identification

Identification under Selection on Observables: Regression

Consider the linear regression of $Y_i = \beta_0 + \tau T_i + X_i'\beta + u_i$.

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3. Heterogeneous treatment effects (τ differs for different values of X)
   - even If outcomes are linear in X, τ converges to the conditional-variance-weighted average of the underlying causal effects rather than the ATE.

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Assume a data generating process with constant effects and linearly separable confounding:

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- Thus, a regression where T_i and X_i are entered linearly can recover the ATE. (The regression model matches the data generating process)

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- Thus, OLS estimates the ATE with no covariates.

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- What about the regression estimand, τ_R? How does it relate to the ATE/ATT?

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- Let's investigate this under a saturated regression model:

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- Linear in X_i by construction!

Investigating the regression coefficient

- How can we investigate τ_R? Well, we can rely on the regression anatomy:

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- $\sigma_t^2(x) = \text{Var}[T_i|X_i = x]$ is the conditional variance of treatment assignment.

ATE versus OLS

$$\tau_R = E[\tau(X_i)W_i] = \sum_x \tau(x)\frac{\sigma_t^2(x)}{E[\sigma_t^2(X_i)]}P[X_i = x]$$

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- Maximum variance with $P[T_i = 1|X_i = x] = 1/2$.

OLS weighting example

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Group 1	Group 2
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- Implies the ATE is $\tau = 0.5$

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$\sigma_t^2(1) = 0.09$	$\sigma_t^2(0) = 0.25$
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- Implies the ATE is $\tau = 0.5$
- Average conditional variance: $E[\sigma_t^2(X_i)] = 0.13$

OLS weighting example

- Binary covariate:

$$\begin{aligned} &\text{Group 1} & &\text{Group 2} \\ P[X_i = 1] &= 0.75 & P[X_i = 0] &= 0.25 \\ P[T_i = 1|X_i = 1] &= 0.9 & P[T_i = 1|X_i = 0] &= 0.5 \\ \sigma^2_t(1) &= 0.09 & \sigma^2_t(0) &= 0.25 \\ \tau(1) &= 1 & \tau(0) &= -1 \end{aligned}$$

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\end{aligned}
$$

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- Constant probability of treatment: $e(x) = P[T_i = 1 | X_i = x] = e$.
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- Incorrect linearity assumption in X_i will lead to more bias.

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- How can we use this?

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- Impute the treated potential outcomes with $\widehat{Y}_i(1) = \hat{\mu}_1(X_i)$!

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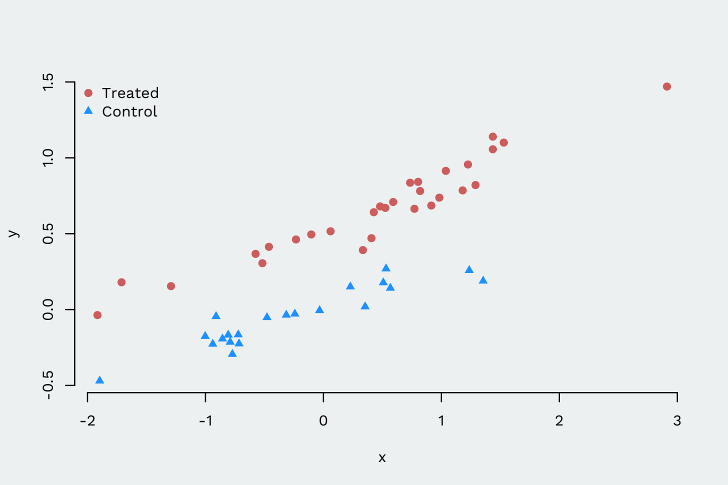
- Sometimes called an **imputation estimator**.

Imputation estimator visualization



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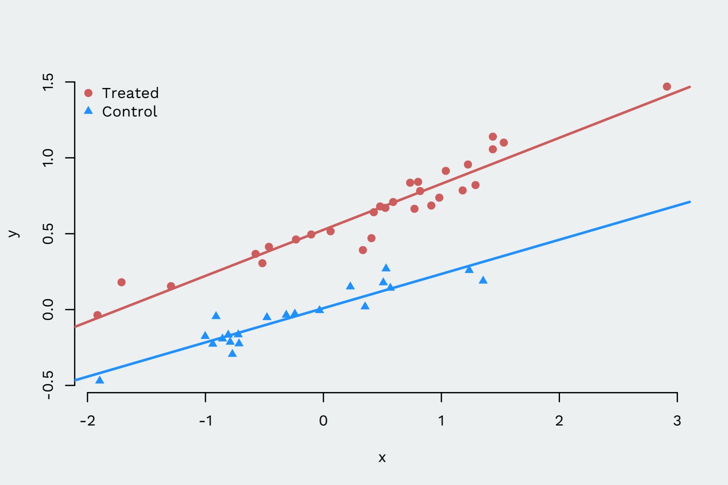

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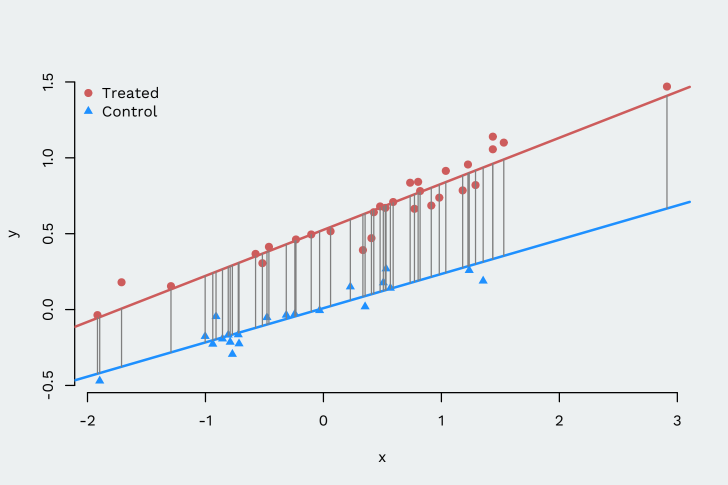

Nonlinear relationships

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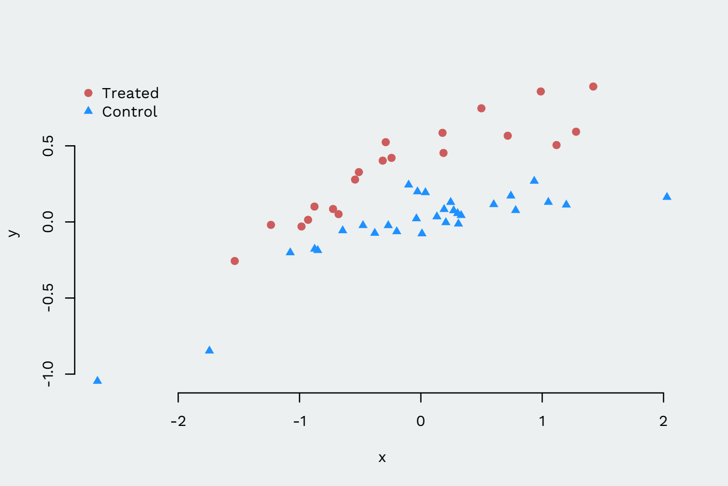

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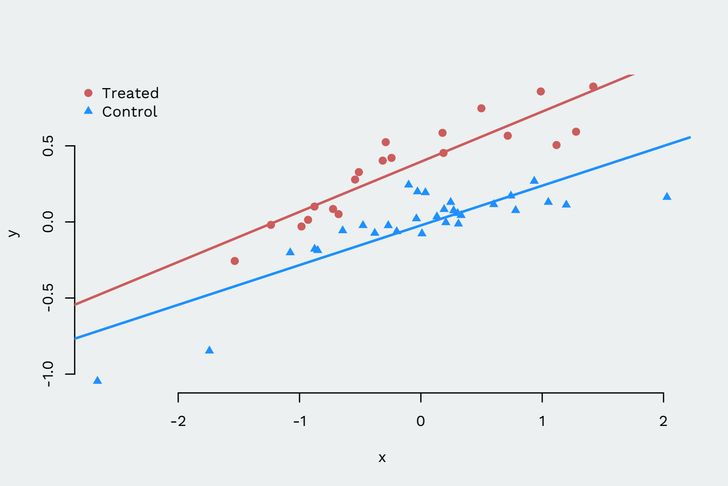

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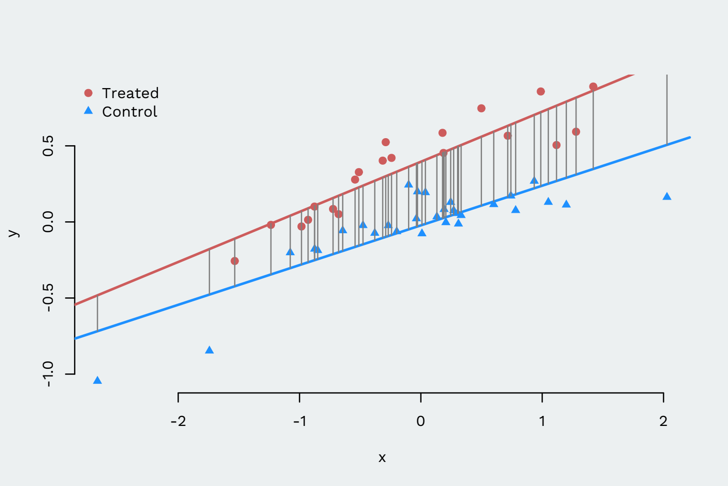

Using semiparametric regression

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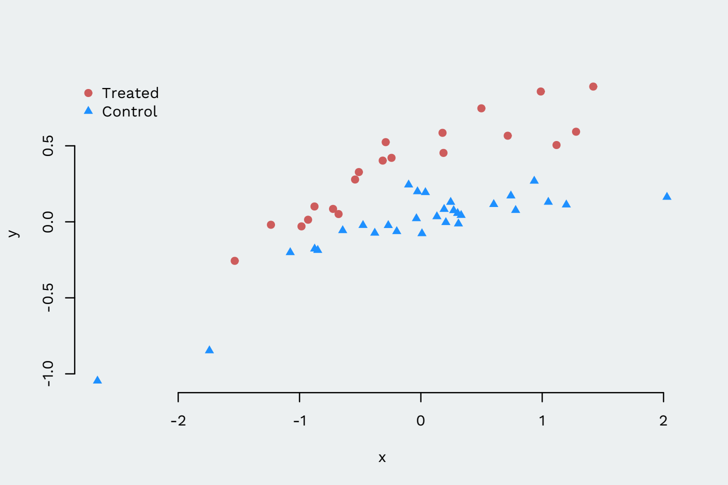
- Here, CEFs are nonlinear, but we don't know their form.
- We can use GAMs from the `mgcv` package for flexible estimate.

Using GAMs



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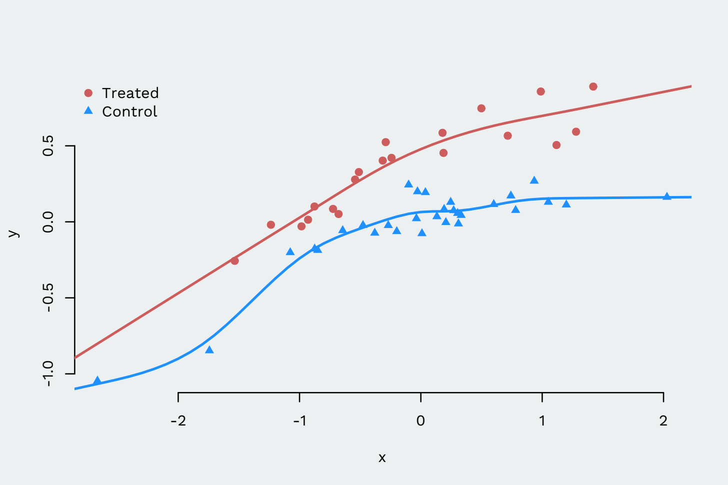

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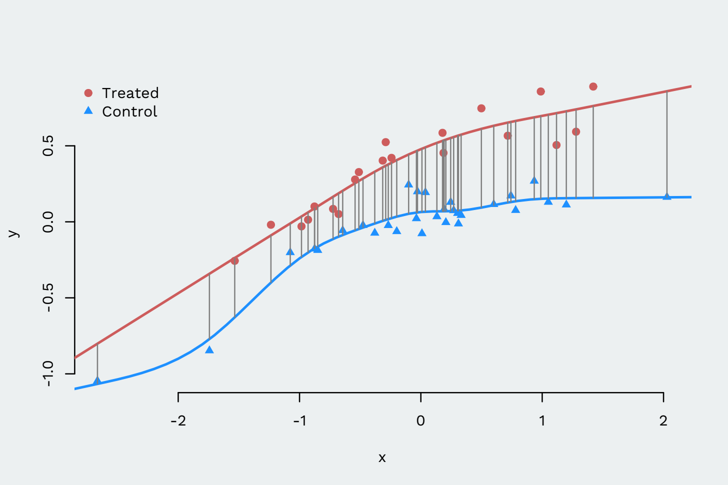

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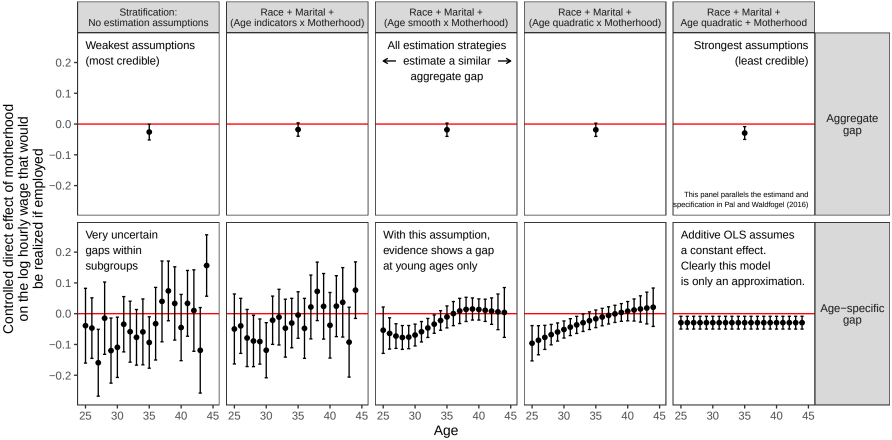
- To be flexible, people are increasingly using machine learning techniques like: kernel regression, neural networks, regression trees, etc.
- As we just saw, GAMs are a nice trade-off of the ease vs. flexibility side.
- These kinds of things will tend to matter a lot more for conditional treatment effects than the overall aggregate treatment effect, but you also don't know for sure until you try.

Example from Lundberg, Johnson and Stewart



All the Steps Together

1) Set the target. Define a theoretical estimand. Requires substantive **argument**.

Average difference in the **potential outcome** each woman i would realize

	if she were an employed mother	versus	if she were an employed non-mother	
$\tau = \frac{1}{n}\sum_{i=1}^{n}\Bigg($	Y_i(Mother, Employed)	$-$	Y_i(Non-mother, Employed)	$\Bigg)$

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Average difference in the **realized outcomes** of women with the covariates $\vec{x}_i$ of women i who

	actually are mothers	versus	actually are not mothers	
$\theta = \frac{1}{n}\sum_{i=1}^{n}\Bigg($	$E(Y \mid \vec{X} = \vec{x}_i, \text{Motherhood} = \text{Mother})$	$-$	$E(Y \mid \vec{X} = \vec{x}_i, \text{Motherhood} = \text{Non-mother})$	$\Bigg)$

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3) Learn from data. Select an estimation strategy. Requires statistical **evidence**.

Average difference in the **regression prediction** at the covariates $\vec{x}_i$ of women i if we

recode as a mother versus recode as not a mother

$$\hat{\theta} = \frac{1}{n}\sum_{i=1}^{n}\left(\underbrace{\hat{E}(Y \mid \vec{X} = \vec{x}_i, \text{Motherhood} = \text{Mother})}_{\text{estimated } \hat{Y}_i(\text{Mother})} - \underbrace{\hat{E}(Y \mid \vec{X} = \vec{x}_i, \text{Motherhood} = \text{Non-mother})}_{\text{estimated } \hat{Y}_i(\text{Non-mother})} \right)$$

↑ estimate of the estimand ($\hat{\theta}$)

↑ estimated $\hat{Y}_i(\text{Mother})$

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Connections to the Coefficient In Regression

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$$\hat{E}(Y \mid \vec{X} = \vec{x}_i, \text{Motherhood})$$

$$= \begin{cases} \hat{\alpha} \qquad + \vec{x}_i' \hat{\vec{\gamma}} & \text{if Motherhood} = \text{Non-Mother} \\ \hat{\alpha} + \hat{\beta} + \vec{x}_i' \hat{\vec{\gamma}} & \text{if Motherhood} = \text{Mother} \end{cases}$$

By parametric approximation

Intercept

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$$= \hat{\beta} \quad \leftarrow \text{coefficient on motherhood}$$

Fun With Weights

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- Imagine we care about the possibly heterogeneous causal effect of a treatment T and we control for some covariates X?
- We can express the regression as a weighting over individual observation treatment effects where the weight depends only on X.

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Fun With Weights

Aronow, Peter M., and Cyrus Samii. "Does Regression Produce Representative Estimates of Causal Effects?." *American Journal of Political Science* (2015).[2]

- Imagine we care about the possibly heterogeneous causal effect of a treatment T and we control for some covariates X?
- We can express the regression as a weighting over individual observation treatment effects where the weight depends only on X.
- Useful technology for understanding what our models are identifying off of by showing us our effective sample.

[2] I'm grateful to Peter Aronow for sharing his slides, several of which are used here.

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$$\hat{\beta} \xrightarrow{p} \frac{E[w_i \tau_i]}{E[w_i]} \text{ where } w_i = (T_i - E[T_i|X])^2 \,,$$

so that $\hat{\beta}$ converges to a reweighted causal effect. As $E[w_i|X_i] = \text{Var}[T_i|X_i]$, we obtain an average causal effect reweighted by conditional variance of the treatment.

Estimation

A simple, consistent plug-in estimator of w_i is available: $\hat{w}_i = \tilde{T}_i^2$ where $\tilde{T}_i$ is the residualized treatment. (the proof is connected to the partialing out strategy)

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Easily implemented in R:

```
wts <- (t - predict(lm(t~x)))^2
```

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- When some observations have no weight, this means that the covariates completely explain their treatment condition.
- This is a feature, not a bug, of regression: we can't learn anything from those cases anyway (i.e. it is automatically handling issues of common support).
- The downside is that we have to be aware of what happened!

Application

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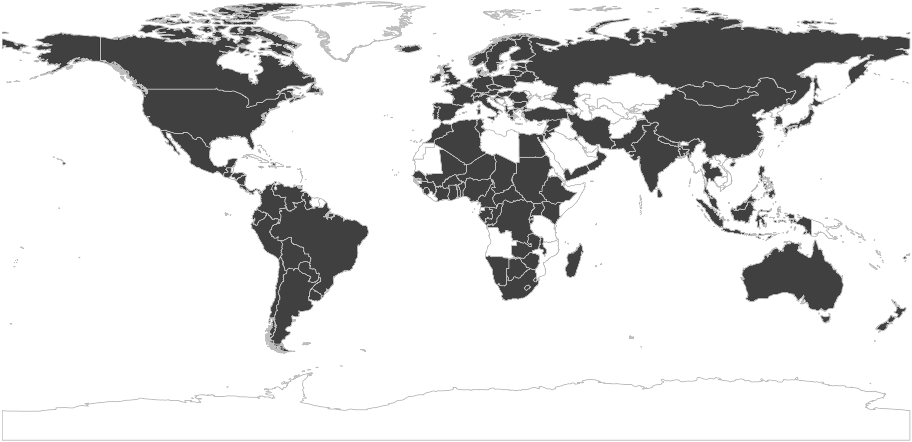
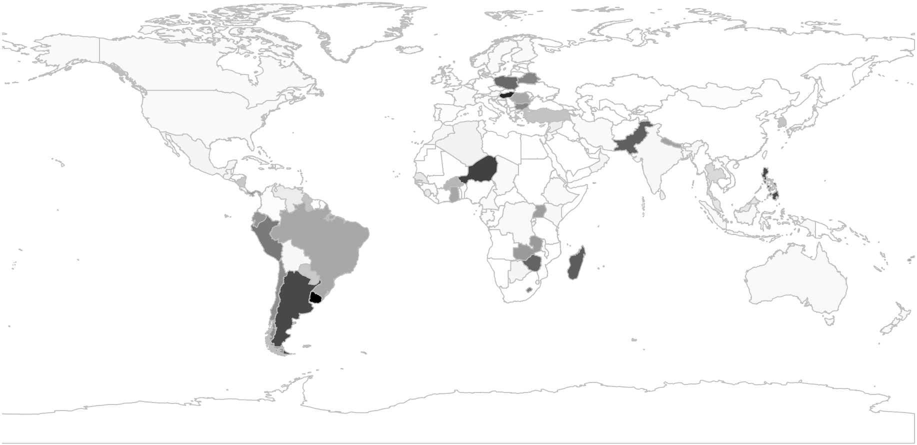
Jensen estimates that a 1 unit increase in polity score corresponds to a 0.020 increase in net FDI inflows as a percentage of GDP ($p < 0.001$).

Nominal and Effective Samples



Nominal and Effective Samples



Nominal and Effective Samples

Over 50% of the weight goes to just 12 (out of 114) countries.

Broader Implications

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  - randomized (lab, field, survey) experiments, instrumental variables, regression discontinuity designs, other natural experiments
- "Externally valid": perhaps unreliable estimates of ATEs, but for the population of interest
  - large-N analyses, representative surveys

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- The effective sample (upon which causal effects are estimated) may have radically different properties than the nominal sample.
- When there is an underlying natural experiment in the data, a properly specified regression model may reproduce the internally valid estimate associated with the natural experiment.

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Next Time: Estimands

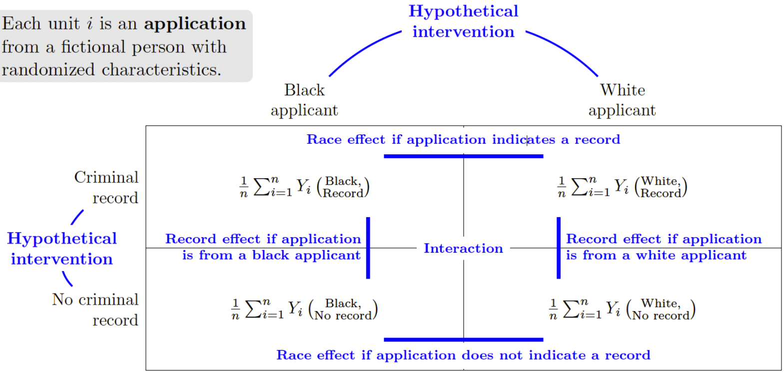
Where We've Been and Where We're Going...

- Last Week
  - frameworks for causal inference
- This Week
  - experimental ideal
  - identification with measured confounding
  - estimation via stratification, matching and regression
- Next Week
  - approaches with unmeasured confounding
- Long Run
  - causal frameworks → inference → regression → causal inference

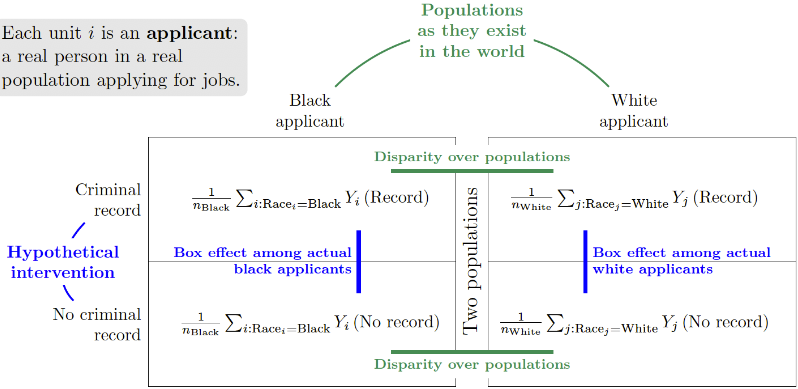
Estimands

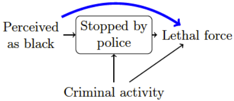
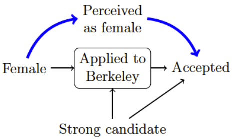
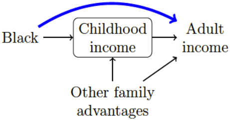
Estimand name	Mathematical statement	DAG	Reference	Colloquial terms
Average treatment effect	$\frac{1}{n}\sum_i Y_i(d') - Y_i(d)$	$D \rightarrow Y$	Morgan and Winship (2015)	Effect
Conditional average treatment effect	$\frac{1}{n_x}\sum_{i:X_i=x}(Y_i(d') - Y_i(d))$	$X \rightarrow D \rightarrow Y$, $X \rightarrow Y$	Athey and Imbens (2016)	Effect heterogeneity or moderation
Causal interaction	$\frac{1}{n}\sum_i \left(\left(Y_i(a',d') - Y_i(a',d)\right) - \left(Y_i(a,d') - Y_i(a,d)\right)\right)$	$A \rightarrow Y$, $D \rightarrow Y$	Vanderweele 2015	Joint treatment effect
Controlled direct effect	$\frac{1}{n}\sum_i \left(Y_i(d',m) - Y_i(d,m)\right)$	$D \rightarrow M \rightarrow Y$, $D \rightarrow Y$	Acharya Blackwell and Sen (2016)	Mediation
Natural direct effect	$\frac{1}{n}\sum_i \left(Y_i(d',M_i(d)) - Y_i(d,M_i(d))\right)$	$D \rightarrow M \rightarrow Y$, $D \rightarrow Y$	Imai et al 2011	Mediation
Effect of dynamic treatment regime	$\frac{1}{n}\sum_i Y_i(d_1',d_2') - Y_i(d_1,d_2)$	$D_1 \rightarrow D_2 \rightarrow Y$, $D_1 \rightarrow Y$	Wodtke et al 2011	Cumulative effect

Each unit i is an **application** from a fictional person with randomized characteristics.



Each unit i is an **applicant**: a real person in a real population applying for jobs.



Study	Empirical regularity	Misleading conclusion	Directed Acyclic Graph
Fryer (2019)	Among those they stop, police shoot the same proportion of black individuals as white individuals.	Police do not discriminate against black individuals when using lethal force.	
Bickel et al. (1975)	Among those who apply, Berkeley departments admit a higher proportion of women than of men.	Admissions committees do not discriminate against women.	
Chetty et al. (2020)	Among those with equal childhood incomes, black and white women earn similar amounts as adults.	Equalizing childhood incomes would eliminate the racial gap in women's adult incomes.	

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- Despite a common framework, there are still disagreements between experimental and observational design approaches
- Mostly related to the debate between internal and external validity of estimates
- Most researchers are inherently interested in Population Average Treatment Effects (PATE)

Decomposition of Causal Effect Estimation Error

- Difference in means estimator:

$$D \equiv \left(\frac{1}{n/2}\sum_{i\in\{I_i=1,T_i=1\}} Y_i\right) - \left(\frac{1}{n/2}\sum_{i\in\{I_i=1,T_i=0\}} Y_i\right).$$

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- Decomposition:

$$\begin{aligned}\Delta &= \Delta_S + \Delta_T \\ &= (\Delta_{S_X} + \Delta_{S_U}) + (\Delta_{T_X} + \Delta_{T_U})\end{aligned}$$

Error due to Δ_S (sample selection), Δ_T (treatment imbalance), and each due to observed (X_i) and unobserved (U_i) covariates

Note: Analogous decompositions hold for other estimands of interest.

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where NATE is the nonsample average treatment effect.

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- Δ_S vanishes if:
  1. The sample is a census ($I_i = 1$ for all observations and $n = N$);
  2. SATE = NATE; or
  3. Switch quantity of interest from PATE to SATE

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- conditions are unverifiable: X is observed only in sample and U is not observed at all.
- Δ_{S_X} vanishes if weighting on X
- Δ_{S_U} cannot be corrected after the fact

Decomposing Treatment Imbalance

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- Decomposition:

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- $\Delta_{T_X} = 0$ when X is balanced between treateds and controls:

$$\widetilde{F}(X \mid T = 1, I = 1) \;\; = \;\; \widetilde{F}(X \mid T = 0, I = 1).$$

  Verifiable from data; can be generated ex ante by blocking or enforced ex post via matching or parametric adjustment

Decomposing Treatment Imbalance

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- $\Delta_{T_U} = 0$ when U is balanced between treateds and controls:

$$\widetilde{F}(U \mid T = 1, I = 1) \;=\; \widetilde{F}(U \mid T = 0, I = 1).$$

  Unverifiable. Achieved only by assumption or, on average, by random treatment assignment

Effects of Design Components on Estimation Error

Design Choice	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Complete stratified random sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling	$= 0$	$=?$		
Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$=?$
Exact matching			$= 0$	$=?$
By Assumption				
No selection bias	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Ignorability				$\stackrel{\text{avg}}{=} 0$
No omitted variables				$= 0$

The Benefits of Major Research Designs: Overview

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Ideal experiment	$\to 0$	$\to 0$	$= 0$	$\to 0$
Randomized trials (Limited or no blocking)	$\neq 0$	$\neq 0$	$\overset{\text{avg}}{=} 0$	$\overset{\text{avg}}{=} 0$
Randomized trials (Full blocking)	$\neq 0$	$\neq 0$	$= 0$	$\overset{\text{avg}}{=} 0$
Survey Experiment (Limited or no blocking)	$\to ?$	$\to ?$	$\to 0$	$\to 0$
Observational Study (Representative data set, Well-matched)	≈ 0	≈ 0	≈ 0	$\neq 0$
Observational Study (Unrepresentative but partially, correctable data, well-matched)	≈ 0	$\neq 0$	≈ 0	$\neq 0$
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Randomized trials (Full blocking)	$\neq 0$	$\neq 0$	$= 0$	$\stackrel{\text{avg}}{=} 0$
Survey Experiment (Limited or no blocking, no non-response)	$\to 0$	$\to 0$	$\to 0$	$\to 0$
Observational Study (Representative data set, Well-matched)	≈ 0	≈ 0	≈ 0	$\neq 0$
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Next week: Selection with Unmeasured Confounding!