Causal Inference: Selection on Observables, Multiple Regression and Matching

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Part 4

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Yesterday

- How randomization can solve the selection bias issue
- Important challenges to RCTs

1. Translating your research question to a treatment and measurable outcome
2. Randomization of the treatment
3. Power of the experiment
4. Validity of the design
5. Correct treatment and mechanisms
6. Non-Compliance (in field experiments)

Now

- What happens if we cannot randomize?
- Selection on observables and the prospects for causal inference
- Multivariate Regression (less)
- Matching (more)

Experimental vs Observational Studies

Definition: Observational Study

An observational study is an empirical investigation of the effects of exposure to different treatment regimes, in which the investigator cannot control the assignment of treatment.

- This means that control and treatment units are not automatically exchangeable.
- Does this mean we can only work with experimental data?
- Of course not. This is why we add controls to our regression models. To adjust for the observed covariates.
- Is that good enough? What about the unobservables?
- Well, we want to make sure they are **as-if random**. That's where we will start next week!
- Is balance -or exchangeability- testable? NO!

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Comparing Randomization to Selection on Observables

Q1: What is our empirical expectation when we randomize?
Q2: How could we know?

Example: Balance Table

Covariates	Control	Treatment	Difference	Sig.
Age (years)	41.6	41.2	.40	.40
% Democrat	.76	.78	.02	.33
% Republican	.04	.03	.01	.21
% Female	.68	.70	.02	.58
% Living in Des Moines	.53	.55	.02	.46
% Voted 2008 primary	.32	.32	.00	.93
% Voted 2008 general	.93	.93	.00	.94
% Voted 2010 primary	.22	.22	.00	.87

Randomization Check (control vs treatment groups *in pre-treatment outcomes)*

Comparing Randomization to Selection on Observables

- The randomization check is a good indication that there is `balance` in the control and treatment group
- In other words, the distribution of pre-treatment variables (let's call them X) in the control and treatment group is very similar.
- In cases where the causal factor is not randomly assigned, we seek to limit our analysis to exchangeable units. i.e. units that are very similar in the distributions of the Xs in the control and treatment group.
- Under -strict- assumptions, we remove bias by conditioning

Definitions

Covariates

A covariate is a variable that is predetermined with respect to treatment D_i: $X_0 = X_1$, i.e. its value does not depend on the value of D_i.

- Does not imply that X and D are independent
- Predetermined variables are often time invariant (YoB, race, etc.), but time invariance is not necessary

Outcomes

The variables, Y, that are (possibly) not predetermined are called outcomes (for some individual i , $Y_{0i} \neq Y_{1i}$)

In general, one should not condition on outcomes, because this may induce `post-treatment bias`.

Removing Bias by Conditioning

We need to satisfy: $P(D_i = 1) \perp\!\!\!\perp Y_0, Y_1$

Experiments

- Randomization ensures unconfoundedness without selection on observables: $P(D_i = 1) \perp\!\!\!\perp Y_0, Y_1$ which translates into:
- $E(Y_1|D = 1) = E(Y_1|D = 0)$ &
- $E(Y_0|D = 1) = E(Y_0|D = 0)$

Typical Observational Studies

- Unconfoundedness can be *assumed* to hold only *after* conditioning on a set of pre-treatment variables: $P(D_i = 1|X_i) \perp\!\!\!\perp Y_0, Y_1$ which translates into:
- $E(Y_1|D = 1, X) = E(Y_1|D = 0, X)$ &
- $E(Y_0|D = 1, X) = E(Y_0|D = 0, X)$

Conditioning on Observables

- Regression
- Matching

Balancing

- All studies have a common goal: to balance the distributions of covariates for units which are treated and units untreated.
- They differ in how they try to achieve balance. In some instances estimation of causal effects happens (seemingly) simultaneously with the attempt to maximise balance on the observables (e.g. regression).
- In others, these two steps are clearly distinguished, with the design stage being the first stage in which balance is attempted and estimation follows after balance is achieved (e.g. matching).

Multivariate Regression and Causality

Recall the bivariate regression:

$$y_i = \alpha + \beta D_i + \epsilon_i$$

- If we believe that the treatment of D_i is as good as randomly assigned, the β coefficient will have a causal interpretation of the regression

Let's imagine we are interested in the effects of reading the Daily Mail on political preferences (voting Conservative):

$$\text{Conservative}_i = \alpha + \beta \text{Daily Mail}_i + \epsilon_i$$

- Is reading the Daily Mail as good as randomly assigned?
- What other variables affect both reading the Daily Mail and voting Conservative for an individual i?

Connecting Multivariate Regression to Potential Outcomes Framework

We would like to compare:

- An individual i that reads the Daily Mail
- to the **same** individual i that does not read the Daily Mail

Multivariate regression allows us to compare:

- An individual i that reads the Daily Mail with income I, education levels E, occupation O, living in region R and Age A
- to an individual j with same income I, same education levels E, same occupation O, living in same region R and same Age A that does not read the Daily Mail

Multivariate Regression: Other Variables Leading to Selection Bias?

Let's imagine we are interested in the effects of reading the Daily Mail on political preferences (voting Conservative):

$$\text{Conservative}_i = \alpha + \beta \text{Daily Mail}_i + \epsilon_i$$

Imagine though the true model specification is:

$$\text{Conservative}_i = \alpha + \beta \text{Daily Mail}_i + \gamma Age_i + u_i$$

- Adding another variable we suspect may correlate with both the treatment and the outcome is called 'conditioning on X_i' in non-experimental data ('controlling for').
- The idea is 'holding X_i constant' - we eliminate the effect of X_i on the treatment before comparing treated and control outcomes

Notations

Recall the bivariate regression:

$$y_i = \alpha + \beta^S D_i + \epsilon_i \tag{1}$$

Imagine though the true model specification is:

$$y_i = \alpha + \beta^L D_i + \gamma X_i + \epsilon_i \tag{2}$$

Will the estimated $\hat{\beta^S}$ from Equation 1 be biased if we don't condition on X, i.e. Age?

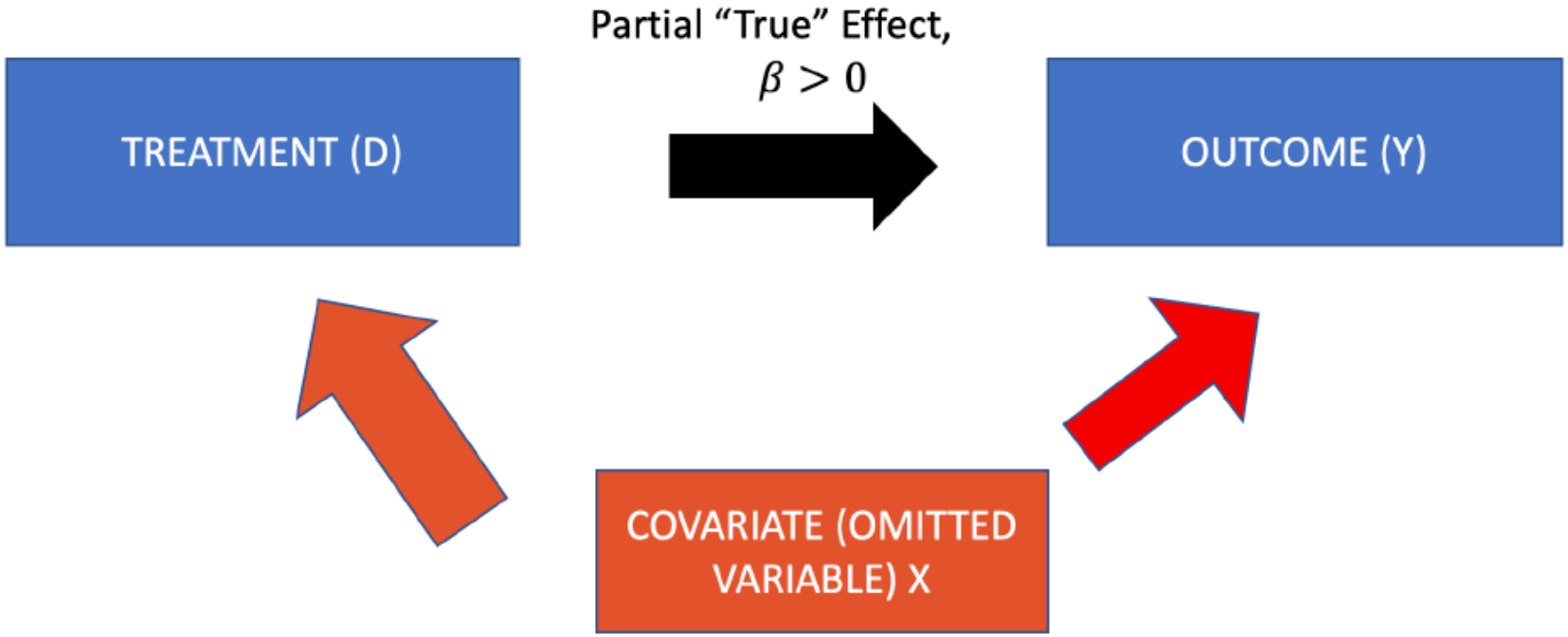
$$\beta^S = \frac{cov(Y_i, D_i)}{var(D_i)} = \beta^L + \gamma \times \delta(D_i, X_i)$$

where $\delta(D_i, X_i)$ is the correlation between the treatment and covariate (or the regression coefficient from a regression of X_i on D_i).

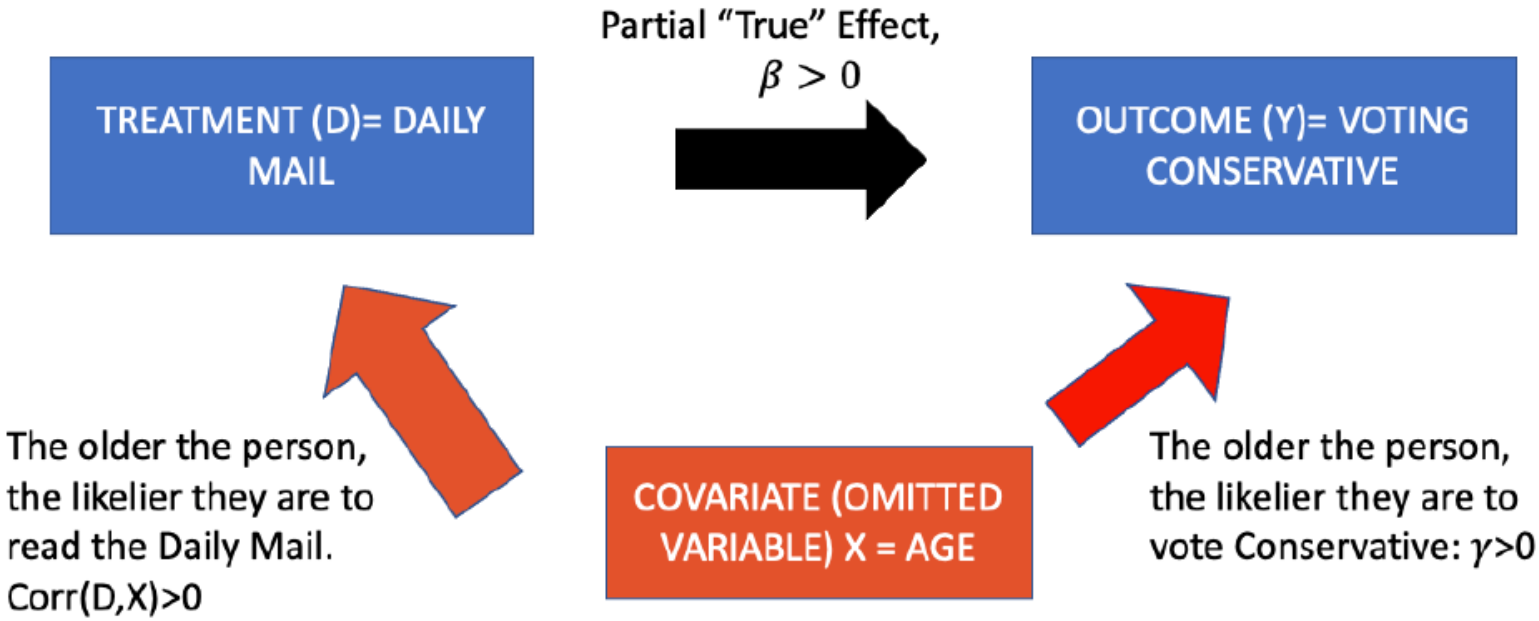
The more the covariates the better? Can we remove every omitted variable bias?

- If $\gamma \neq 0$ but $\delta(D_i, X_i) = 0$, we do not need to include the covariate for OVB purposes. What about precision of the estimate?
- We should not include covariates that are an outcome of the treatment themselves (bad controls), as they reintroduce selection bias.
- Think about correlation across different covariates
- Think about the direction of omitted variable bias and changes in estimates as we introduce more covariates

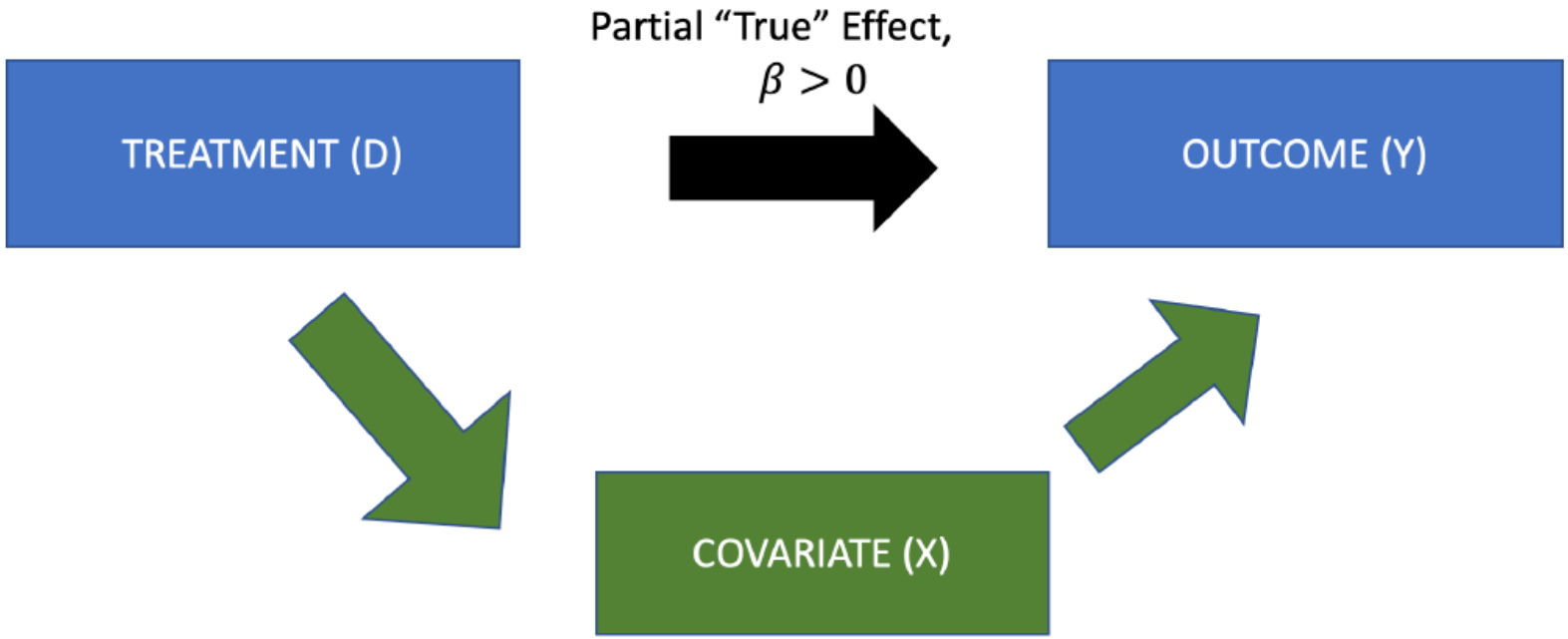
Good Control/Covariate



Good Control/Covariate: Example



Bad Control/Covariate



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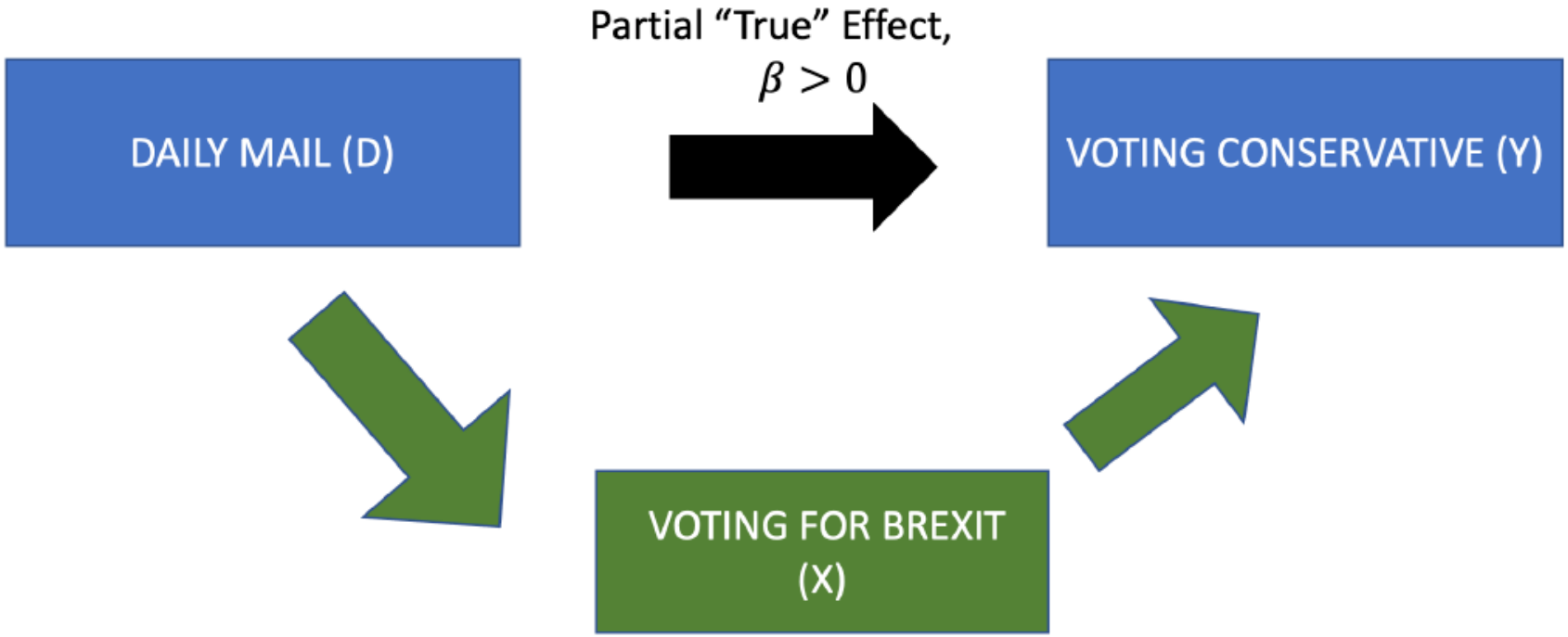

Intuition: Bad Controls Reintroduce Selection Bias

- Imagine that indeed there was an RCT that randomly assigned Daily Mail readership to individuals.
- However, when we estimate the effects of Daily Mail readership, we add 'Voting for Brexit' as a covariate as we suspect it could predict voting for the Conservative party.
- Issue: voting for Brexit is itself an outcome of Daily Mail readership
- That means we are comparing two things: i) a person that does read the Daily Mail and voted for Brexit to a person who does not read the Daily Mail and voted for Brexit, and ii) a person that does read the Daily Mail and did not vote for Brexit to a person who does not read the Daily Mail and did not vote for Brexit
- But in this comparison, these are definitely not the right counterfactuals

Goal of Conditioning

Aim: Once we condition on X_i, we believe that the treatment of D_i becomes as good as randomly assigned. Then the β coefficient will have a causal interpretation of the regression.
Is this feasible with conditioning on a few covariates?

Imagine a model specification:

$$\text{Political Preferences}_i = \alpha + \beta \mathit{Daughters}_i + u_i$$

Can we interpret β as a causal estimate? Is there anything we can condition on to make **the number of daughters** as good as randomly assigned?

Goal of Conditioning

Aim: Once we condition on X_i, we believe that the treatment of D_i becomes as good as randomly assigned. Then the β coefficient will have a causal interpretation of the regression.
Is this feasible with conditioning on a few covariates?

Is there anything we can condition on to make **the number of daughters** as good as randomly assigned?
Imagine a model specification:

$$\text{Political Preferences}_i = \alpha + \beta_1 Daughters_i + \beta_2 Children + \epsilon_i$$

Can we interpret β_1 as a causal estimate?

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Matching

The Underlying Logic

Think of matching as a way to address the missing data problem, by "imputing" missing observations for potential outcomes, using observed outcomes from units chosen on the basis of information about a set of $X's$, which—we believe—drive subjects into their treatment status.

So, if X denotes a set of pre-treatment characteristics for subjects, matching is based on the following assumption:

Unconfoundedness

$Y_1, Y_0 \perp\!\!\!\perp D|X$

Matching

- We seek to find treated and untreated units that are exchangeable
- In Matching, we search across "covariates" for similar units that only differ in terms of D_i.
- Recall JS Mill's idea that across all potential causes we observe similar patterns, while only the true cause is dissimilar.
- Imagine a dataset with characteristics such as height, hair colour, and instrument choice: You are looking for the tall, blonde units who plays the guitar and was treated and the tall blonde guitarist who was untreated.
- Setting aside other possible combinations of variables, you could estimate the effect D_i on Y_i
- A better example might serve us well!

Matching: A Running Example,MPs for Sale?

Research Question

What is the effect of serving in Parliament on politicians' wealth?

Definition: Treatment

D_i : Indicator of treatment status for politician i

$$D_i = \begin{cases} 1 & \text{if } i \text{ was elected into Parliament} \\ 0 & \text{if } i \text{ was not elected into Parliament.} \end{cases}$$

Definition: Observed Outcome

Y_i : Observed wealth at death for politician i

Definition: Potential Outcomes

Y_{0i} and Y_{1i}: Potential Outcomes for politician i

What we Observe

An Example with 10 candidates:

	D_i=Won?
Candidate 1	Yes=1
Candidate 2	Yes=1
Candidate 3	No=0
Candidate 4	No=0
Candidate 5	No=0
Candidate 6	Yes=1
Candidate 7	No=0
Candidate 8	No=0
Candidate 9	Yes=1
Candidate 10	Yes=1

Selection bias?

(Eggers & Hainmueller 2009) Are there returns to wealth in politics? For which ideology?

TABLE 1. Gross Wealth at Death (Real 2007 GBP) for Competitive Candidates Who Ran for House of Commons Between 1950 and 1970 (Estimation Sample)

	Mean	Min.	1st Qtr.	Median	3rd Qtr.	Max.	Obs.
Both Parties							
All candidates	599,385	4,597	186,311	257,948	487,857	12,133,626	427
Winning candidates	828,379	12,111	236,118	315,089	722,944	12,133,626	165
Losing candidates	455,172	4,597	179,200	249,808	329,103	8,338,986	262
Conservative Party							
All candidates	836,934	4,597	192,387	301,386	743,342	12,133,626	223
Winning candidates	1,126,307	34,861	252,825	483,448	1,150,453	12,133,626	104
Losing candidates	584,037	4,597	179,259	250,699	485,832	8,338,986	119
Labour Party							
All candidates	339,712	12,111	179,288	250,329	298,817	7,926,246	204
Winning candidates	320,437	12,111	193,421	254,763	340,313	1,036,062	61
Losing candidates	347,934	40,604	177,203	243,526	295,953	7,926,246	143

Additional Covariates:

- Education
- Aristocrat
- Gender
- Schooling

Selection bias?

(Eggers & Hainmueller 2009) Are there returns to wealth in politics? For which ideology?

Additional Covariates:

- Education
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Back to the Problem

$E(Y_{1i}|D = 1) - E(Y_{0i}|D = 0)$ leads us to the problem of selection bias that we have already seen.

Randomization would solve the problem, but you cannot randomize who gets elected and who does not.

Instead, we try to find direct comparisons: `matches` for each treated unit.

Exact Matching

An Example with 10 candidates:

	Observed Outcome: Wealth at Death	D_i	Male?
Candidate 1	855,557	1	1
Candidate 2	912,331	1	1
Candidate 3	566,271	0	1
Candidate 4	319,838	0	1
Candidate 5	612,233	0	0
Candidate 6	601,222	1	0
Candidate 7	485,709	0	1
Candidate 8	102,509	0	1
Candidate 9	991,511	1	1
Candidate 10	757,972	1	1

What do we do next?

Exact Matching: Counterfactuals for Observed Outcome, Ordered According to Covariate Values

	Potential Outcome Under Treatment	Potential Outcome Under Control	D_i	Male?
Candidate 1	855,557	?	1	1
Candidate 2	912,331	?	1	1
Candidate 9	991,511	?	1	1
Candidate 10	757,972	?	1	1
Candidate 3	?	566,271	0	1
Candidate 4	?	319,838	0	1
Candidate 7	?	485,709	0	1
Candidate 8	?	102,509	0	1
	Potential Outcome Under Treatment	Potential Outcome Under Control	D_i	Male?
Candidate 5	?	612,233	0	0
Candidate 6	601,222	?	1	0

Counterfactuals for Observed Outcome

	Potential Outcome Under Treatment	Potential Outcome Under Control	D_i	Male?
Candidate 1	855,557	?	1	1
Candidate 2	912,331	?	1	1
Candidate 9	991,511	?	1	1
Candidate 10	757,972	?	1	1
Candidate 3	?	566,271	0	1
Candidate 4	?	319,838	0	1
Candidate 7	?	485,709	0	1
Candidate 8	?	102,509	0	1

	Potential Outcome Under Treatment	Potential Outcome Under Control	D_i	Male?
Candidate 5	601,222	612,233	0	0
Candidate 6	601,222	612,233	1	0

Imputing the Missing Outcomes

	Potential Outcome Under Treatment	Potential Outcome Under Control	D_i	Male?
Candidate 1	855,557	368,581.75	1	1
Candidate 2	912,331	368,581.75	1	1
Candidate 9	991,511	368,581.75	1	1
Candidate 10	757,972	368,581.75	1	1
Candidate 3	879,342.75	566,271	0	1
Candidate 4	879,342.75	319,838	0	1
Candidate 7	879,342.75	485,709	0	1
Candidate 8	879,342.75	102,509	0	1
	Potential Outcome Under Treatment	Potential Outcome Under Control	D_i	Male?
Candidate 5	601,222	612,233	0	0
Candidate 6	601,222	612,233	1	0

Estimate Treatment Effects

ATT

- $E[Y_{1i} - Y_{0i}|D = 1, X = 1]$ for each $i = 1, 2, 9, 10$
- $E[Y_{1,6} - Y_{0,6}|D = 1, X = 0]$
- And take weighted average

ATC

- $E[Y_{1i} - Y_{0i}|D = 0, X = 1]$ for each $i = 3, 4, 7, 8$
- $E[Y_{1,5} - Y_{0,5}|D = 0, X = 0]$
- And take the weighted average.

ATE

- $E[Y_{1i} - Y_{0i}|X = 1]$ for each $i = 1, 2, 3, 4, 7, 8, 9, 10$
- $E[Y_{1i} - Y_{0i}|X = 0]$ for each $i = 5, 6$
- And take the weighted average.

A Complication: Adding Covariates

	D_i	Male?	Oxbridge?
Candidate 1	1	1	0
Candidate 2	1	1	1
Candidate 3	0	1	0
Candidate 4	0	1	0
Candidate 5	0	0	0
Candidate 6	1	0	1
Candidate 7	0	1	0
Candidate 8	0	1	1
Candidate 9	1	1	1
Candidate 10	1	1	0

Rearrange with respect to values of X_1

	D_i	Male?	Oxbridge?
Candidate 1	1	1	0
Candidate 2	1	1	1
Candidate 3	0	1	0
Candidate 4	0	1	0
Candidate 7	0	1	0
Candidate 8	0	1	1
Candidate 9	1	1	1
Candidate 10	1	1	0
	D_i	Male?	Oxbridge?
Candidate 5	0	0	0
Candidate 6	1	0	1

Is there a match for Candidate 6?

Is Candidate 5 good enough for any match?

So, we are left with:

	D_i	Male?	Oxbridge?
Candidate 1	1	1	0
Candidate 10	1	1	0
Candidate 3	0	1	0
Candidate 4	0	1	0
Candidate 7	0	1	0
	D_i	Male?	Oxbridge?
Candidate 2	1	1	1
Candidate 9	1	1	1
Candidate 8	0	1	1

Even More Covariates

What are we looking for?

- Units differing in their D_i values while at the same time: Having the exact same values in all other columns ($X's$)
- Any chance?

	D_i	Male?	Oxbridge?	Aristocrat?	Public schooling?
Candidate 1	1	1	0	0	1
Candidate 2	1	1	1	1	1
Candidate 6	1	0	1	1	1
Candidate 9	1	1	1	1	1
Candidate 10	1	1	0	1	1
Candidate 3	0	1	0	0	0
Candidate 4	0	1	0	0	0
Candidate 5	0	0	0	0	0
Candidate 7	0	1	0	0	0
Candidate 8	0	1	1	0	1

- **How about a multivariate regression here?**

Extrapolations

- The "else equal" principle is often satisfied only through extrapolations beyond the range of the available data.
- Such extrapolations are in turn based on assumptions, which are typically untestable and 'invisible' within the regression framework.
- Matching, thus makes the stage of making units similar with regard to covariates more transparent.
- Imagine candidate 7 also went to a public school; then, comparing Candidate 1 and 7 would provide an estimate of ATT.
- This process requires attaching greater weight to most similar units. *Regressions?*

Dimensionality

- In the original study, there are more than 400 observations available.
- But: many more covariates are taken into account
- As the number of covariates used to "match" units increases, it becomes exponentially more difficult to find perfect matches.
- Exact matching fails in finite samples if the dimensionality of X is large: not enough information. Far too demanding for the vast majority of research questions and data available.
- With more than one continuous variable, it is also sub-optimal (Abadie & Imbens, 2006).

Matching in Multidimensional Space

"Else being similar"

With many X's and typically also with continuous X's, estimation of ATT is based on the detection of the closest possible control unit to match every treated unit:
$\hat{\tau} = \frac{1}{N_1} \sum_{D_i=1} (Y_i - Y_{j(i)})$
where $Y_{j(i)}$ is the outcome of an untreated observation such that $X_{j(i)}$ is the closest value to X_i among the untreated observations.

Problem

- How to decide which control is closest?

Defining Closensess

Think of it as a distance metric. Let $X_i = (X_{i1}, X_{i2}, \ldots, X_{ik})$ and $X_j = (X_{j1}, X_{j2}, \ldots, X_{jk})$ be covariate vectors for i and j. We want to find ways to link rows according to their similarities in their values in each of these vectors. This is done with distance metrics.

The Propensity Score

Another way to reduce dimensionality: `match` on the Propensity Score

Definition

The probabity to receive treatment (also known as the selection probability) conditional on the set of pre-treatment covariates: $p(X) = P(D = 1|X)$

Identification Assumptions

1. $(Y_1, Y_0) \perp D|X$ (Selection on Observables)
2. $0 < Pr(D = 1|X) < 1$ (common support)

Propensity Score Properties

Balancing: Balancing of pre-treatment variables given the propensity score: $D \perp X|p(X)$
Unconfoundedness: If $Y_1, Y_0 \perp D|X$, then $Y_1, Y_0 \perp D|p(X)$.

How to Estimate the Propensity Score

- Regress D_i on the set of $X's$ using a logit or probit function to estimate the score.
- Take the predicted values of D_i. These predicted values represent the probability of being assigned to treatment, given X(the Propensity Score).
- Choose closest control on $p(X_i)$ (Call this the Nearest Neighbor (NN))
- Test for balance: If not satisfactory: redo by changing matching criteria.
- Repeat until balance is satisfactory:
  Estimate PrScore $\rightarrow$ Check Balance $\rightarrow$ Re-Estimate $\rightarrow$ Check Balance

Checklist

- Always establish balance before you even look at the estimate
- Look for balance not only at the characteristics included in matching but higher polynomials and on other covariates. Balance should extend beyond X, if X is correctly specified.
- Do not simply think of matching as an alternative or final resort when design-based identification is not provided. Conversely, use it when there is some design that allows you to make the conditional-on-observables assumption more credible.

Wrap-up–Matching

Prince Charles

Male
Born in 1948
Raised in the UK
Married Twice
Lives in a castle
Wealthy and Famous

Ozzy Osbourne

Male
Born in 1948
Raised in the UK
Married Twice
Lives in a castle
Wealthy and Famous

Wrap-up–Matching

- Useful method to create exchangeable units
- (Too) many alternative algorithms
- (Too) many options
- Sensitivity
- Do we really take care of the unobservability problem (inherent in regression models)?
- **Q for discussion**: Can we draw causal inferences by selecting on observables?