Causality 2

http://users.ox.ac.uk/~sfos0015

1. **The idea of potential outcomes.**
2. **Treatment, exposure, mediators, confounders, moderators, covariates.**
3. **Types of Treatment Effects.**
4. **Directed Acyclical Graphs (Introduction)**

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Effects of causes

Potential Outcomes

1. Define potential outcome random variables Y^1 and Y^0.
2. Y^1 is the PO in the treatment condition and Y^0 is the PO in the control condition.
3. Realised values are y_i^1 and y_i^0 for individual i.
4. The individual-level causal effect of the treatment is $\delta_i = y_i^1 - y_i^0$.

Effects of causes

Potential Outcomes

1. Define a causal exposure variable D.

2. D = 1 for those i exposed to the treatment state and D = 0 for those i exposed to the control state.

3. The observable outcome variable is defined as:

$$Y = Y^1 \text{ if } D = 1$$

$$Y = Y^0 \text{ if } D = 0$$

Effects of causes

Potential Outcomes

Fundamental problem of causal inference

Group	Y^1	Y^0
Treatment group (D=1)	Observable as Y	Counterfactual
Control group (D=0)	Counterfactual	Observable as Y

Effects of causes

Average Treatment Effect

1. In practice we study the **average treatment effect** (ATE).

$$E[\delta_i] = E[Y^1 - Y^0]$$

$$= E[Y^1] - E[Y^0]$$

Effects of causes

Treatment assignment

1. Consider a randomized experiment.

2. Subject i is allocated either to D=1 or to D=0 by a lottery.

3. Imagine we use a random number generator to create for each subject i a value in the 1-100 interval of a variable R.

4. Subjects are allocated to treatments according to a rule:

    1. If R <= 50 D=0; if R > 50 D=1.

Effects of causes

Treatment assignment

I	R	D
1	23	0
2	31	0
3	56	1
4	4	0

Effects of causes

Treatment assignment

1. By definition R cannot causally influence Y because R is just a random number.

2. Whatever relationship R may appear to have with Y must be because R controls allocation to D.

R → D → Y

In an RCT we know how treatment allocation is assigned in an observational study we usually don’t.

Effects of causes

Treatment assignment

1. If assignment to D is by randomisation then the assignment process is **ignorable**.

2. Another circumstance in which the treatment assignment mechanism is ignorable is when assignment to D depends **only on observed variables** S.

3. In this case we say that the potential outcomes are independent of D **conditional on S.**

    1. So correct inference means the variables in S must be controlled for (including interactions).

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Effects of causes

Treatment assignment

1. Ignorability through randomization under investigator's control leads to experiments, RCTs .

2. Ignorability through randomization via a suitable "instrument" leads to "natural experiments", instrumental variable estimation with observational data.

3. Ignorability through knowing the variables that select units into treatment conditions leads to observational data analysed by rigorous **conditioning**, **matching**, propensity score analysis.

4. **Take home**. To make sense of a causal claim you need to know (or make assumptions about) how units are assigned to treatments.

5. Human subjects make choices….often based on expected outcomes.

Stable Unit Treatment Value Assumption (SUTVA)

SUTVA is the assumption that the value of Y for individual i exposed to treatment d **does not depend on the way the individuals are assigned to treatments**.

Table 2.2: A Hypothetical Example in Which SUTVA is Violated

Treatment assignment patterns					Potential outcomes	
$\begin{bmatrix} d_1 = 1 \\ d_2 = 0 \\ d_3 = 0 \end{bmatrix}$	or	$\begin{bmatrix} d_1 = 0 \\ d_2 = 1 \\ d_3 = 0 \end{bmatrix}$	or	$\begin{bmatrix} d_1 = 0 \\ d_2 = 0 \\ d_3 = 1 \end{bmatrix}$	$y_1^1 = 3$ $y_2^1 = 3$ $y_3^1 = 3$	$y_1^0 = 1$ $y_2^0 = 1$ $y_3^0 = 1$
$\begin{bmatrix} d_1 = 1 \\ d_2 = 1 \\ d_3 = 0 \end{bmatrix}$	or	$\begin{bmatrix} d_1 = 0 \\ d_2 = 1 \\ d_3 = 1 \end{bmatrix}$	or	$\begin{bmatrix} d_1 = 1 \\ d_2 = 0 \\ d_3 = 1 \end{bmatrix}$	$y_1^1 = 2$ $y_2^1 = 2$ $y_3^1 = 2$	$y_1^0 = 1$ $y_2^0 = 1$ $y_3^0 = 1$

Treatment, exposure, mediators, confounders, moderators, covariates

1. Treatment (exposure)

T ⟶ Y

Divorce ⟶ Failure in school

2. Mediation

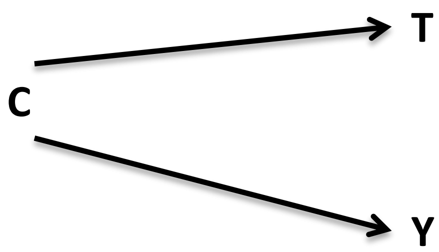
T ⟶ M ⟶ Y

Divorce ⟶ Low income ⟶ Failure in school

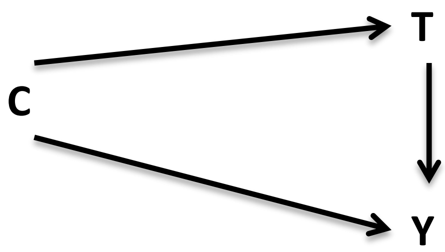
Treatment, exposure, mediators, confounders, moderators, covariates

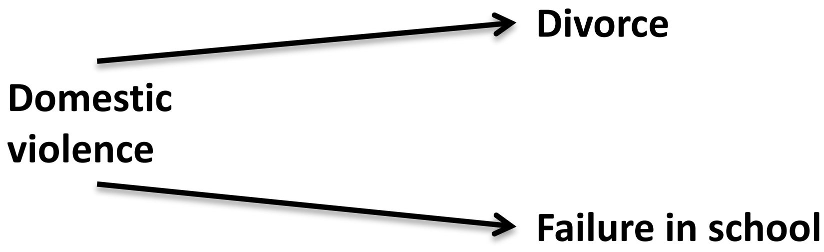
3. Confounders

a)



b)





Treatment, exposure, mediators, confounders, moderators, covariates

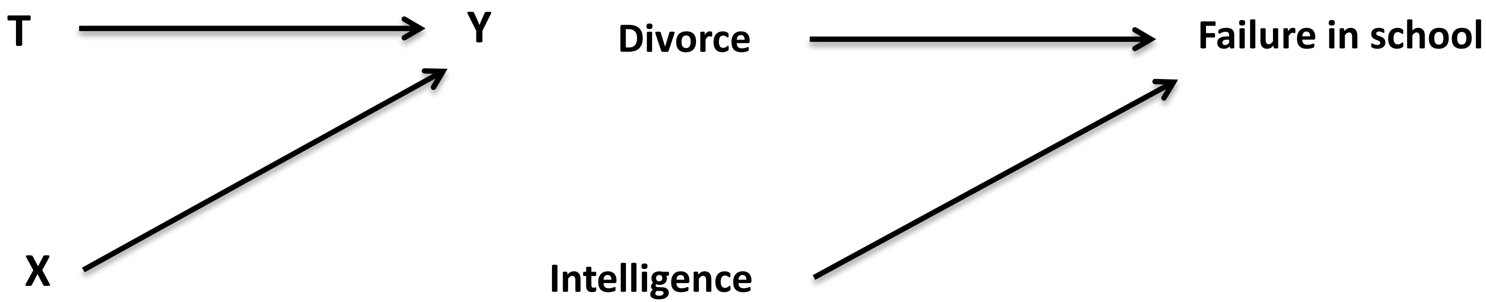
4. Moderators

C_1 T → Y **Violence** **Divorce → Failure in school**

C_2 T Y **No Violence** **Divorce** **Failure in school**

Treatment, exposure, mediators, confounders, moderators, covariates

5. Covariates



Effects of causes

Randomisation

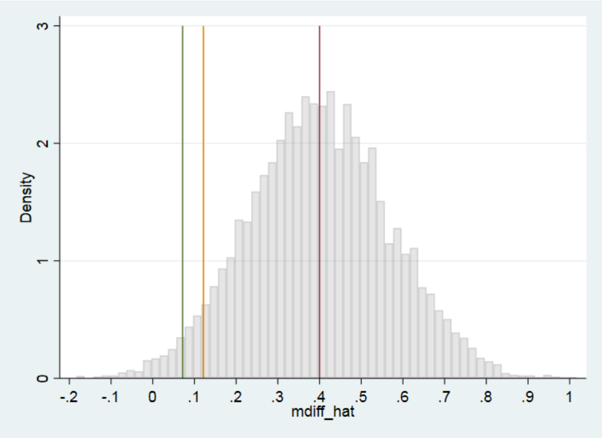
Simple simulation example

1. Create 200 cases.
2. Assign them a sex: 100 female, 100 male.
3. Generate a random number and sort the cases.
4. First 100 get the treatment, second 100 get the control.
5. Generate the outcome Y = .4 · treatment + .4 · sex + ε.
6. Calculate $\bar{Y}_t - \bar{Y}_c$ and save the result.
7. Go back to 3. and repeat until you have 10000 replications.
8. Calculate some summary statistics for the distribution of $\bar{Y}_t - \bar{Y}_c$ and draw a histogram.

2a. Create covariate_1. Random number drawn from N(0,1).

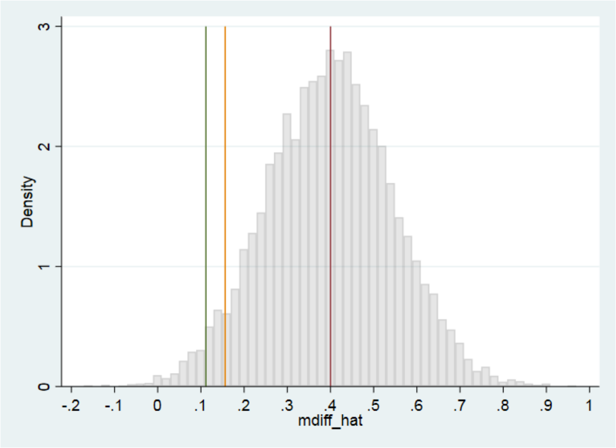
5a. Substitute for 5. Generate the outcome Y = .4 · treatment + .4 · sex + .6 · covariate_1 + ε.

6a. Estimate i) $\hat{Y} = \alpha + \beta_1$Treatment and ii) $\hat{Y} = \alpha + \beta_1$Treatment $+ \beta_2$Covariate_1



Without covariate_1:

Mean = 0.4
SD = 0.170
2.5th percentile = 0.72
5th percentile = 0.122



With covariate_1:

Mean = 0.4
SD = 0.147
2.5th percentile = 0.112
5th percentile = 0.156

Treatment Effects

Average treatment effect (ATE) $E[\delta] = E[Y^1 - Y^0]$

Average treatment effect for the treated (ATT) $E[\delta|D = 1] = E[Y^1 - Y^0|D = 1]$

Average treatment effect for the controls (ATC) $E[\delta|D = 0] = E[Y^1 - Y^0|D = 0]$

ATT = average treatment effect for those that typically are (choose to be) treated based on counterfactual comparison.

In a well designed experiment, ATE should (over many replications) be the same for those randomized to the treatment and those randomized to the controls.

Q. Is there any reason to expect this in observational data?

Treatment Effects

Sources of bias in the estimation of the ATE

(Details of estimation from sample data skipped here (see Morgan & Winship , 2007: 44-46)

Turns out that:

$$E[Y^1|D=1]-E[Y^0|D=0]=$$

$$E[\delta]$$ The true average treatment effect

$$+\{E[Y^0|D=1]-E[Y^0|D=0]\}$$ A "baseline" bias

$$+(1-\pi)\{E[\delta|D=1]-E[\delta|D=0]\}$$ A differential treatment effect bias

Where π is the proportion receiving the treatment

So, much depends on whether the second and third component can be either shown or assumed to = 0.

Numerical example – discussed in Morgan & Winship pp 47

Table 2.3: An Example of the Bias of the Naïve Estimator of the ATE

Group	$E[Y^1 \mid .]$	$E[Y^0 \mid .]$
Treatment group (D=1)	**10**	**6**
Control group (D=0)	**8**	**5**

Effect of college degree on a labour market outcome. Assume π = 0.3

Average PO under treatment for treated = 10 and average PO under control for controls = 5. This is what is observed. Q. Is the ATE = 5?

What would have happened in the control state? Those in D=1 would have done better, 6 versus 5. **Baseline difference**.
What would have happened in the treatment state? Those in D=0 would have done worse, 8 versus 10.

ATT = 4; ATC = 3 therefore ATE = 0.3(10-6) + 0.7(8-5) = 3.3 NOT 5!

Treatment Effects

Example: Church Schools

We observe (hypothetically) that children attending religious schools do better in exams.

Why?

1. Religious schools do a better job of teaching kids (there is a causal impact).

2. Kids that entered religious schools were different (smarter, came from more advantaged homes) right from the start.

3. Kids that actually entered religious schools flourish more in religious schools than would the kids whose parents (actually) chose a secular school for them.

What we want to know about is 1. But 2. & 3. get in the way. 2. is a problem of heterogeneity; 3. could be a problem of self-selection on the basis of the anticipated outcome (parents select schools they think will suit their kids).

Treatment Effects

Example: Church Schools

Random assignment to treatment and control solves (**on average**) 2. and 3.

But randomization is in practice impossible in this case. Many of the problems sociologists are interested in share these characteristics:

Treatment and control groups are heterogeneous. Normal move is to try to deal with heterogeneity by :

1. Matching on observables
2. Conditioning on observables (ie introducing relevant control variables)
3. Or both.

No guarantee this will work!

Units self-select themselves into or are selected into treatment and control on the basis of **anticipated outcomes**. If we notice that kids who read for an hour a day for pleasure do better in school, would we expect the same result if we forced reading on kids that wouldn't normally choose it as a leisure pursuit?

Treatment Effects

Implications for the target to estimate

Average causal effect?

Effect of Catholic school on Catholics and others?

Causal effect of treatment on the treated?

Effect of Catholic school on those kids that would choose a Catholic school

Effect of a training programme on those that would choose to take it

Causal effect of treatment on those that would choose the control?

Effect of Catholic school on Northern Ireland Protestants

Effect of marriage on those who prefer cohabitation

Ni Bhrolcháin, M. (2001) " 'Divorce Effects' and Causality in the Social Sciences" *European Sociological Review*, 17,1, 33-57.

Directed Acyclical Graphs (DAGS)

1. A way of representing a set of causal assumptions.
2. A set of rules for identifying a causal effect.
    1. Tells you what should and should not be conditioned on.
3. Formal equivalence to POT framework.
4. No causes out without putting causes in!

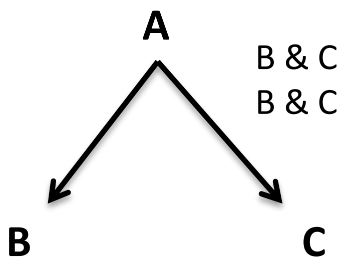
DAGs

Building blocks

Conditional Independence

1. Chain of mediation

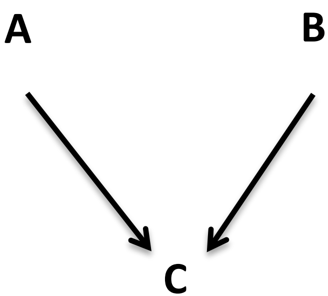
A & C are marginally dependent;
A & C are conditionally independent.



B & C are marginally dependent;
B & C are conditionally independent.

2. Mutual dependence

3. Collider (Mutual Causation)



A & B are marginally independent;
A & B are conditionally dependent.