Experimental Research in Legislative Studies

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Government Department
London School of Economics and Political Science

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Activity!

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1. Ask you to guess a number

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2. Number off 1 and 2 across the room

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3. Group 2, close your eyes

Activity!

Group 1

Think about whether the population of Chicago is more or less than 500,000 people. What do you think the population of Chicago is?

Activity!

1. Ask you to guess a number
2. Number off 1 and 2 across the room
3. Group 2, close your eyes
4. Group 1, close your eyes

Activity!

Group 2

Think about whether the population of Chicago is more or less than 10,000,000 people. What do you think the population of Chicago is?

Enter your data

- Go here: `http://bit.ly/297vEdd`
- Enter your guess and your group number

Results

- True population: 2.79 million

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- What did you guess? (See Responses)

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- What did you guess? (See Responses)
- What's going on here?
  - An experiment!
  - Demonstrates "anchoring" heuristic

Results

- True population: 2.79 million
- What did you guess? (See Responses)
- What's going on here?
  - An experiment!
  - Demonstrates "anchoring" heuristic
- Experiments are easy to analyze and generate causal inferences, but only if designed and implemented well

Who am I?

- Thomas Leeper
- Associate Professor in Political Behaviour at London School of Economics
    - 2013–15: Aarhus University (Denmark)
    - 2008–12: PhD from Northwestern University (Chicago, USA)
    - Birth–2008: Minnesota, USA
- Interested in survey and experimental methods and political psychology
- Email: t.leeper@lse.ac.uk

Who are you?

- What's your name?
- Where are you from?
- Have you designed and/or analyzed an experiment before?

Course Materials

All material for this workshop, including required and suggested readings, are available at:

`http://www.thomasleeper.com/legexpcourse/`

Learning Outcomes

By the end of the day, you should be able to...

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2. Explain how to design experiments that speak to relevant research questions and theories.
3. Evaluate the uses and limitations of three common legislative experimental paradigms: survey experiments, field experiments, and simulations.

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By the end of the day, you should be able to...

1. Explain how to analyze experiments quantitatively.
2. Explain how to design experiments that speak to relevant research questions and theories.
3. Evaluate the uses and limitations of three common legislative experimental paradigms: survey experiments, field experiments, and simulations.
4. Identify practical issues that arise in the implementation of experiments and evaluate how to anticipate and respond to them.

Questions?

Experiments: Definition

Oxford English Dictionary defines "experiment" as:

1. A scientific procedure undertaken to make a discovery, test a hypothesis, or demonstrate a known fact
2. A course of action tentatively adopted without being sure of the outcome

Experiments have a long history

- Origins in agricultural and biostatistical research in the 19th century (Fisher, Neyman, Pearson, etc.)
- First randomized, controlled trial (RCT) by Peirce and Jastrow in 1884
- First polisci experiment by Gosnell (1924)
- Survey experiments have been common since 1930s
- Gerber and Green (2000) first *major*, *modern*, *field* experiment

Legislative Experiments

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- **Field experiments**
  - Really only emerged in the past decade
- **Survey Experiments**
  - Much more sparsely used for reasons that will become obvious

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- Forward causal questions
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- Forward causal questions
  - Can X cause Y?
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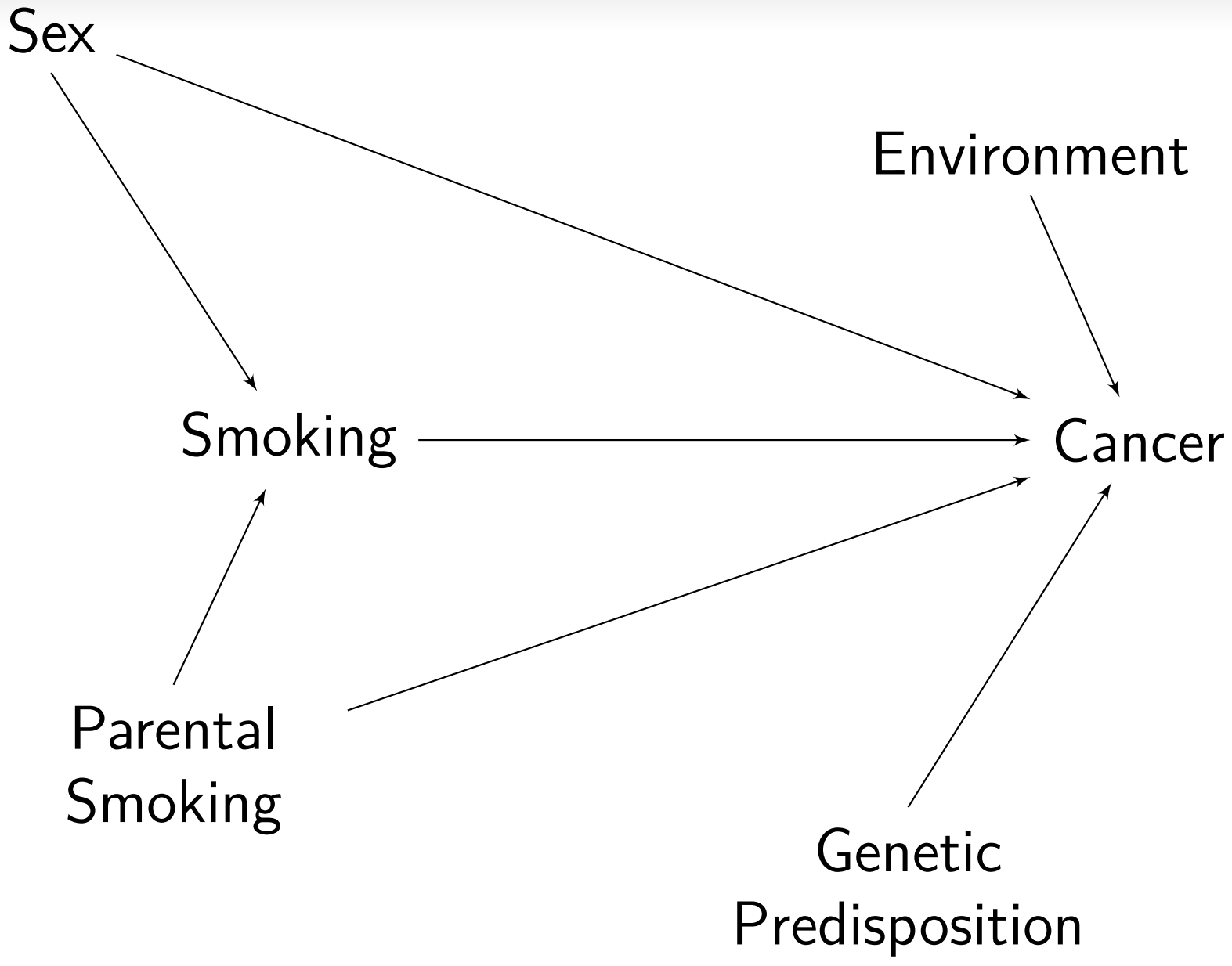
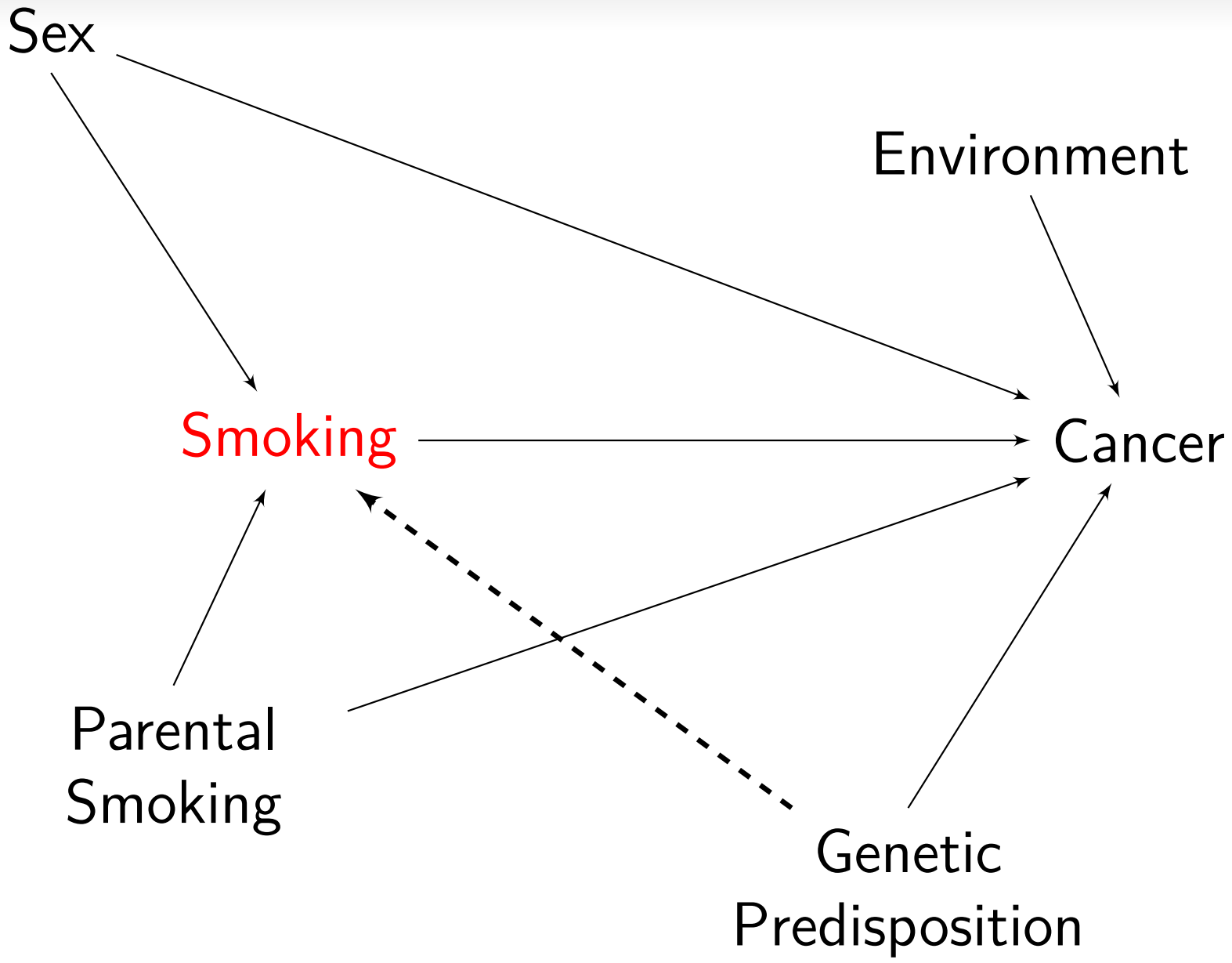
- Backward causal questions
  - What causes Y?
  - How much of Y is attributable to X?
- Even though answering "forward" causal question, we start with an outcome concept


Sex
Environment
Smoking
Cancer
Parental Smoking
Genetic Predisposition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Principles of causality

1. Correlation/Relationship
2. Nonconfounding
3. Direction ("temporal precedence")
4. Mechanism
5. Appropriate level of analysis

Principles of causality

1. **Correlation/Relationship**
2. **Nonconfounding**
3. **Direction ("temporal precedence")**
4. Mechanism
5. Appropriate level of analysis

Establishing Relationship

- This is fairly trivial
- Simply find value of $Corr(X, Y)$
- In causal inference we often talk about correlations in terms of *differences*
  - Difference in values of Y across values of X
  - The presence of a difference indicates a correlation

Addressing Confounding

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1. Correlating a "putative" cause (X) and an outcome (Y)
2. Identifying all possible confounds ($\mathbf{Z}$)
3. "Conditioning" on all confounds
   - Calculating correlation between X and Y at each combination of levels of $\mathbf{Z}$

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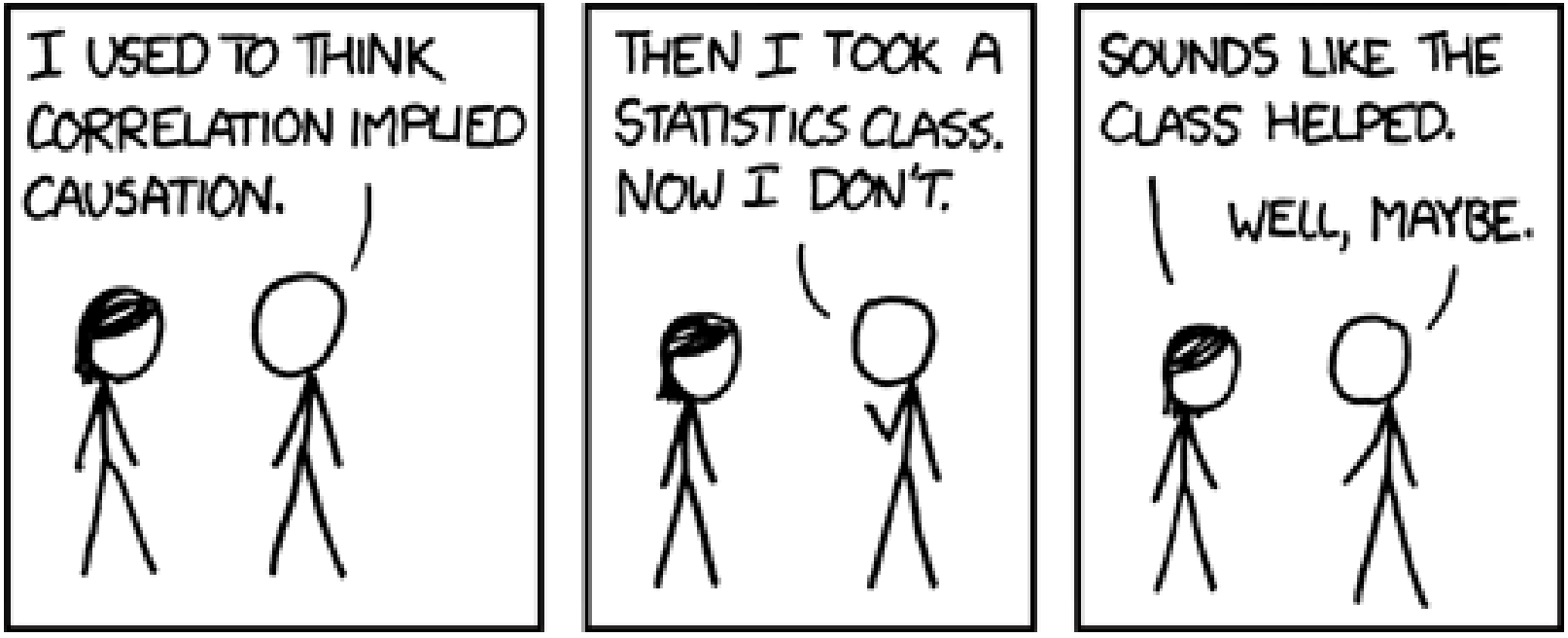
  - Sequentially reinforcing causality between X and Y
- Causation is strictly forward moving in time
- X must precede Y in time for X to cause Y
  - X can be *measured* after Y as long as it comes before it


I USED TO THINK CORRELATION IMPLIED CAUSATION.
THEN I TOOK A STATISTICS CLASS. NOW I DON'T.
SOUNDS LIKE THE CLASS HELPED.
WELL, MAYBE.

Experiments!

The observation of units after, and possibly before, a randomly assigned intervention in a controlled setting, which tests one or more precise causal expectations

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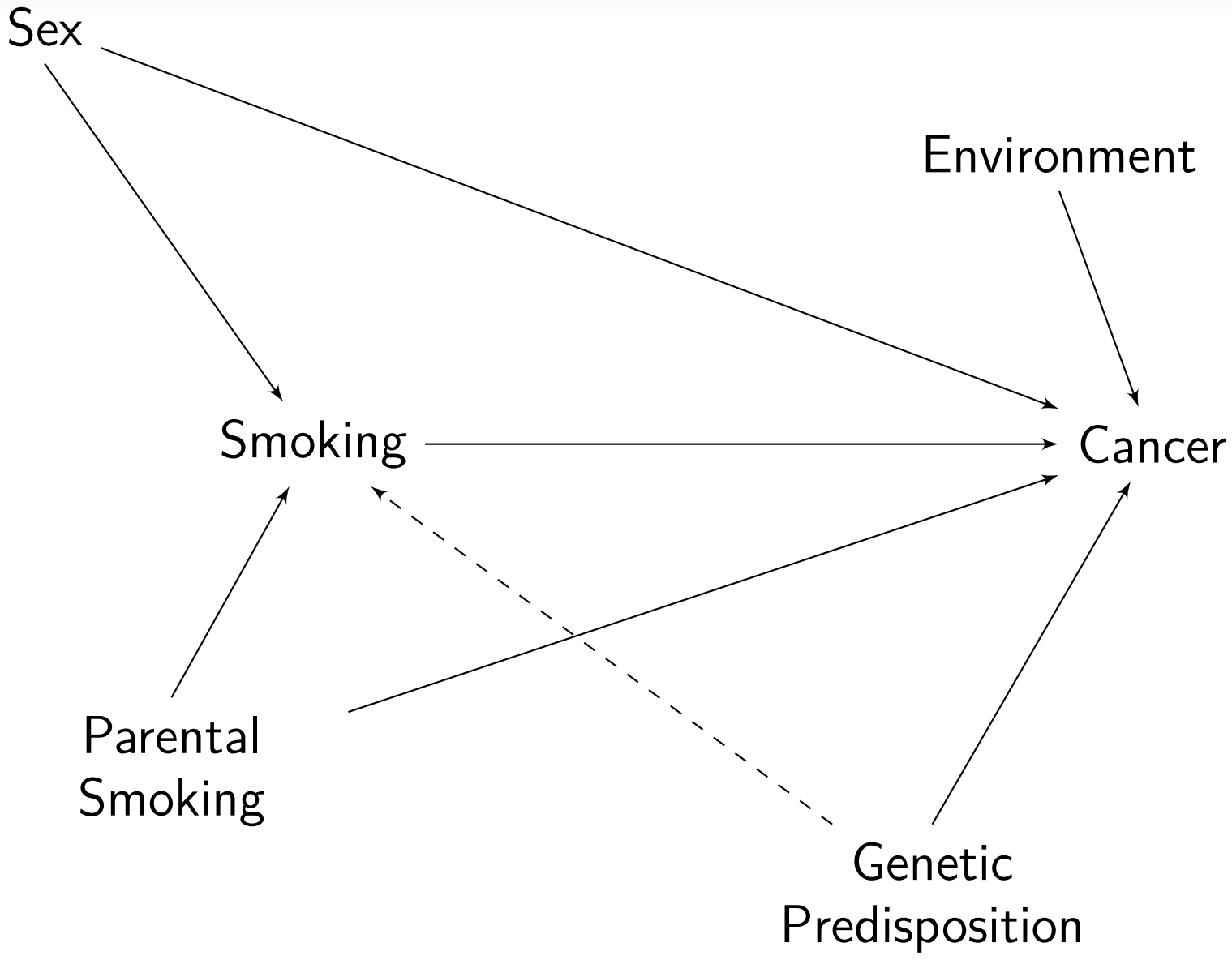
- If we manipulate the thing we want to know the effect of (X), and control (i.e., hold constant) everything we do not want to know the effect of (Z), the only thing that can affect the outcome (Y) is X.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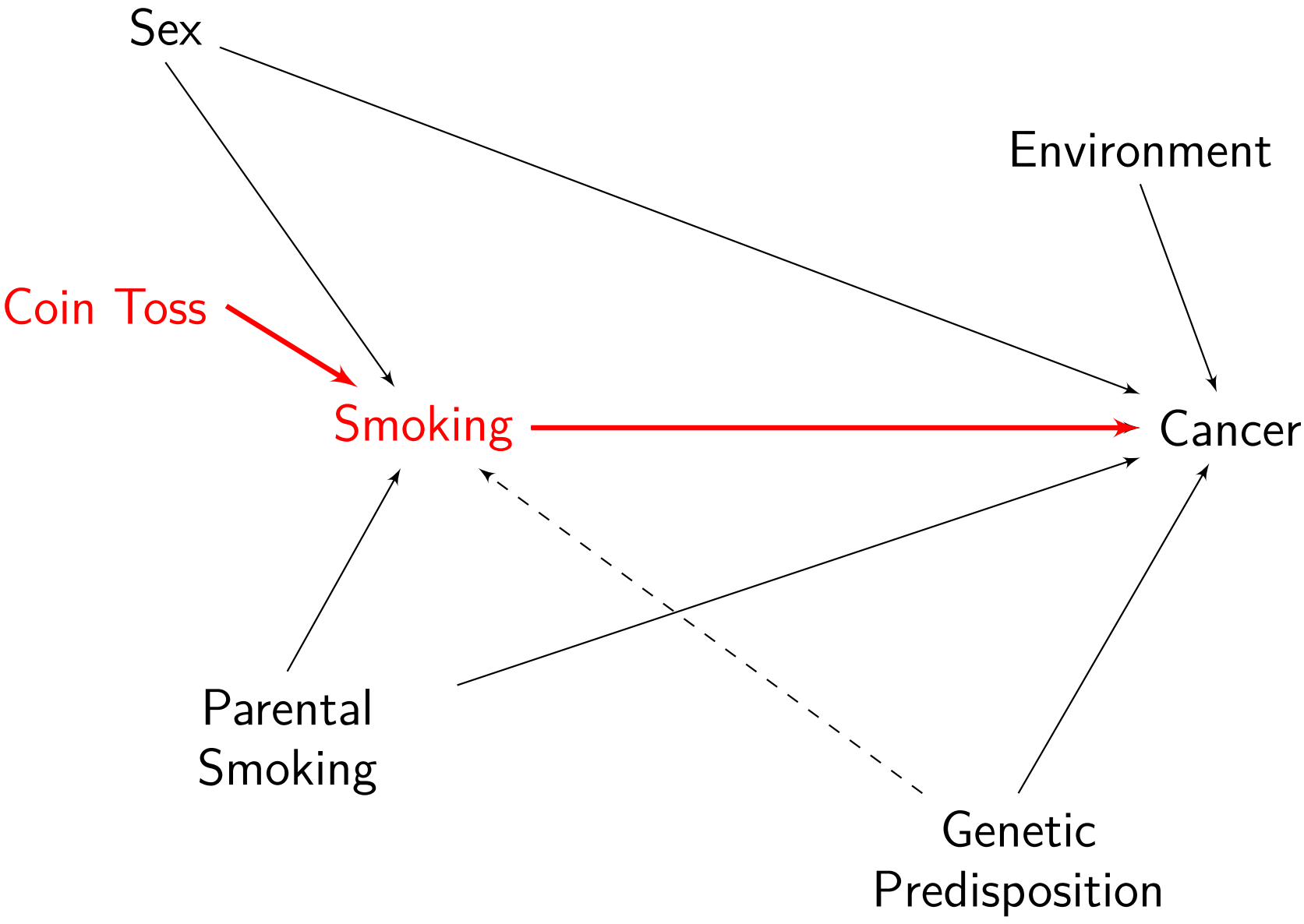
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Questions?

Definitions

Definitions

Unit: A physical object at a particular point in time

Definitions

Treatment: An intervention, whose effect(s) we wish to assess relative to some other (non-)intervention

Definitions

Outcome: The variable we are trying to explain

Definitions

Potential outcomes: The outcome value for each unit that we *would observe* if that unit received each treatment

Multiple potential outcomes for each unit, but we only observe one of them

Definitions

Causal effect: The comparisons between the unit-level potential outcomes under each intervention

This is what we want to know!

"The Perfect Doctor"

Unit	Y_0	Y_1
1	?	?
2	?	?
3	?	?
4	?	?
5	?	?
6	?	?
7	?	?
8	?	?
Mean	**?**	**?**

"The Perfect Doctor"

Unit	Y_0	Y_1
1	?	14
2	6	?
3	4	?
4	5	?
5	6	?
6	6	?
7	?	10
8	?	9
Mean	**5.4**	**11**

"The Perfect Doctor"

Unit	Y_0	Y_1
1	13	14
2	6	0
3	4	1
4	5	2
5	6	3
6	6	1
7	8	10
8	8	9
Mean	**7**	**5**

Experimental Inference I

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  $ATE = E[Y_{1i} - Y_{0i}] = E[Y_{1i}] - E[Y_{0i}]$
- But we still only see one potential outcome for each unit:

  $ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0]$

Experimental Inference II

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  $ATE = E[Y_{1i} - Y_{0i}] = E[Y_{1i}] - E[Y_{0i}]$
- But we still only see one potential outcome for each unit:

  $ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0]$
- Is this what we want to know?

Experimental Inference III

- What we want and what we have:

$$ATE = E[Y_{1i}] - E[Y_{0i}] \tag{1}$$

$$ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0] \tag{2}$$

Experimental Inference III

- What we want and what we have:

$$ATE = E[Y_{1i}] - E[Y_{0i}] \tag{1}$$

$$ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0] \tag{2}$$

- Are the following statements true?
  - $E[Y_{1i}] = E[Y_{1i}|X = 1]$
  - $E[Y_{0i}] = E[Y_{0i}|X = 0]$

Experimental Inference III

- What we want and what we have:

$$ATE = E[Y_{1i}] - E[Y_{0i}] \tag{1}$$

$$ATE_{naive} = E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0] \tag{2}$$

- Are the following statements true?
  - $E[Y_{1i}] = E[Y_{1i}|X = 1]$
  - $E[Y_{0i}] = E[Y_{0i}|X = 0]$
- Not in general!

Experimental Inference IV

- Only true when both of the following hold:

$$E[Y_{1i}] = E[Y_{1i}|X = 1] = E[Y_{1i}|X = 0] \quad (3)$$
$$E[Y_{0i}] = E[Y_{0i}|X = 1] = E[Y_{0i}|X = 0] \quad (4)$$

- In that case, potential outcomes are *independent* of treatment assignment
- If true, then:

$$\begin{aligned} ATE_{naive} &= E[Y_{1i}|X = 1] - E[Y_{0i}|X = 0] \quad (5) \\ &= E[Y_{1i}] - E[Y_{0i}] \\ &= ATE \end{aligned}$$

Experimental Inference V

- This holds in experiments because of randomization, which is a special, physical process of unpredictable sorting[1]
  - Units differ only in what side of coin was up
  - Experiments randomly reveal potential outcomes
  - Randomization balances Z *in expectation*

[1] Not "random" in the casual, everyday sense of the word

Experimental Inference V

- This holds in experiments because of randomization, which is a special, physical process of unpredictable sorting[1]
  - Units differ only in what side of coin was up
  - Experiments randomly reveal potential outcomes
  - Randomization balances Z *in expectation*
- Matching/regression/etc. attempts to eliminate those confounds, such that:

$$E[Y_{1i}|Z] = E[Y_{1i}|X = 1, Z] = E[Y_{1i}|X = 0, Z]$$
$$E[Y_{0i}|Z] = E[Y_{0i}|X = 1, Z] = E[Y_{0i}|X = 0, Z]$$

[1] Not "random" in the casual, everyday sense of the word

Why an 'Experimental Ideal'?

- It solves both the temporal ordering and confounding problems
  - Treatment (X) is applied by the researcher before outcome (Y)
  - Randomization means there are no confounding (Z) variables
- Thus experiments are sometimes called a "gold standard" or "ideal" design for causal inference

Questions?

Experimental Analysis I

- The statistic of interest in an experiment is the *sample average treatment effect* (SATE)
- This boils down to being a mean-difference between two groups:

$$SATE = \frac{1}{n_1} \sum Y_{1i} - \frac{1}{n_0} \sum Y_{0i} \tag{5}$$

- In practice we often estimate this using:
  - t-tests
  - OLS regression

Why use regression?

1. Coefficient estimates are directly interpretable as estimated SATEs
2. Basically no functional form or specification assumptions involved
3. Flexibly accommodates experiments with > 2 conditions
   - *n*-condition experiments
   - *Factorial* designs

Two ways to *parameterize* factorial designs

Dummy variable regression (i.e., treatment–control CATEs):

$$Y = \beta_0 + \beta_1 X_{0,1} + \beta_2 X_{1,0} + \beta_3 X_{1,1} + \epsilon$$

Interaction effects (i.e., treatment–treatment CATEs):

$$Y = \beta_0 + \beta_1 X1_1 + \beta_2 X2_1 + \beta_3 X1_1 * X2_1 + \epsilon$$

Use `margins` to extract marginal effects

Computation of Effects in Stata/R

Stata:

```
ttest outcome, by(treatment)
reg outcome i.treatment
```

R:

```
t.test(outcome ~ treatment, data = data)
lm(outcome ~ factor(treatment), data = data)
```

Experimental Data Structures

An experimental data structure looks like:

unit	treatment	outcome
1	0	13
2	0	6
3	0	4
4	0	5
5	1	3
6	1	1
7	1	10
8	1	9

Experimental Data Structures

Sometimes it looks like this instead, which is bad:

unit	treatment	outcome0	outcome1
1	0	13	NA
2	0	6	NA
3	0	4	NA
4	0	5	NA
5	1	NA	3
6	1	NA	1
7	1	NA	10
8	1	NA	9

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Experimental Analysis II

- We don't just care about the size of the SATE. We also want to know whether it is significantly different from zero (i.e., different from no effect/difference)
- To know that, we need to estimate the *variance* of the SATE
- The variance is influenced by:
  - Total sample size
  - Variance of the outcome, Y
  - Relative size of each treatment group

Experimental Analysis III

- Formula for the variance of the SATE is:

$$\widehat{Var}(SATE) = \frac{\widehat{Var}(Y_0)}{N_0} + \frac{\widehat{Var}(Y_1)}{N_1}$$

  - $\widehat{Var}(Y_0)$ is control group variance
  - $\widehat{Var}(Y_1)$ is treatment group variance

- We often express this as the *standard error* of the estimate:

$$\widehat{SE}_{SATE} = \sqrt{\frac{\widehat{Var}(Y_0)}{N_0} + \frac{\widehat{Var}(Y_1)}{N_1}}$$

Intuition about Variance

- Bigger sample $\rightarrow$ smaller SEs
- Smaller variance $\rightarrow$ smaller SEs
- Efficient use of sample size:
  - When treatment group variances equal, equal sample sizes are most efficient
  - When variances differ, sample units are better allocated to the group with higher variance in Y

Statistical Power

- Power analysis to determine sample size
- Type I and Type II Errors
  - True positive rate is power
  - False negative rate is the significance threshold (α)

	H_0 True	H_0 False
Reject H_0	Type 1 Error	**True positive**
Accept H_0	False negative	Type II error

Doing a Power Analysis

- μ, Treatment group mean outcomes
- N, Sample size
- σ, Outcome variance
- α Statistical significance threshold
- ϕ, a sampling distribution

$$Power = \phi\left(\frac{|\mu_1 - \mu_0|\sqrt{N}}{2\sigma} - \phi^{-1}\left(1 - \frac{\alpha}{2}\right)\right)$$

Intuition about Power

Minimum detectable effect is the smallest effect we could detect given sample size, "true" effect size, variance of outcome, power, and α.

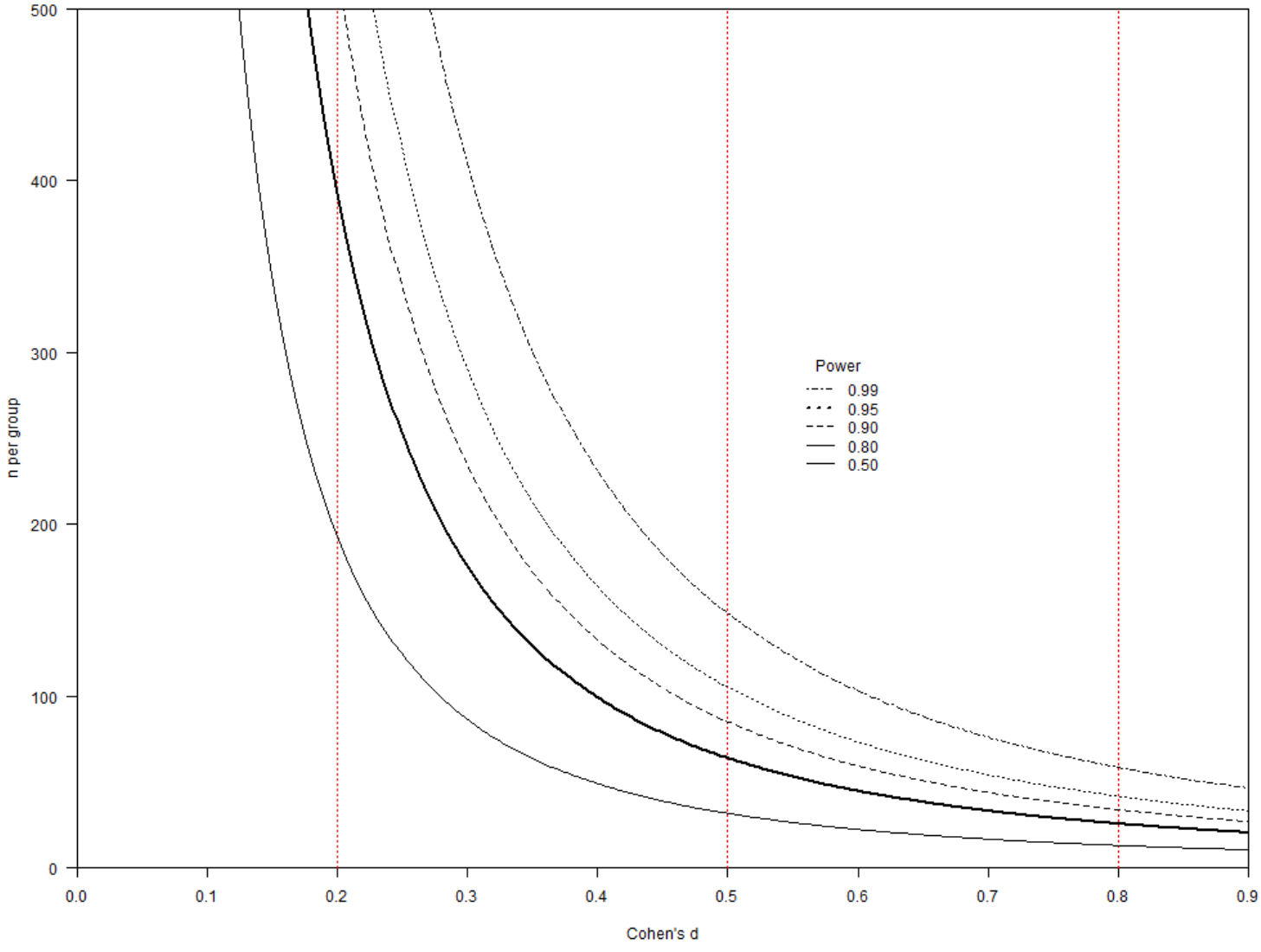
In essence: some non-zero effect sizes are not detectable by a study of a given sample size.[2]

[2] Gelman, A. and Weakliem, D. 2009. "Of Beauty, Sex and Power." *American Scientist* 97(4): 310–16

Intuition about Power

- It can help to think in terms of "standardized effect sizes"
- Cohen's d:
  $d = \frac{\bar{x}_1 - \bar{x}_0}{s}$, where $s = \sqrt{\frac{(n_1-1)s_1^2+(n_0-1)s_0^2}{n_1+n_0-2}}$
- Intuition: How large is the effect in standard deviations of the outcome?
  - Know if effects are large or small
  - Compare effects across studies
- Small: 0.2; Medium: 0.5; Large: 0.8

Intuition about Power



Power in Legislative Experiments

Power in Legislative Experiments

- Legislatures are small!!!

Power in Legislative Experiments

- Legislatures are small!!!
- Because N is fixed, limited capacity to increase n, so power has to be maximized by:
  - Reducing item variance in outcome measures
  - Studying treatments with bigger effects
  - Expanding scope of studies
  - Creative research design

Questions?

Experimental Hypothesis Testing

- From theory, we derive testable hypotheses
  - Hypotheses are expectations about differences in outcomes across levels of a putatively causal variable
  - In an experiment, an hypothesis must be testable by an SATE
- The experimental manipulations induce variation in the causal variable that enable tests of the hypotheses

Example: News Framing

- Theory: Presentation of news affects opinion
- Hypotheses:
  - News emphasizing free speech increases support for a hate group rally
  - News emphasizing public safety decreases support for a hate group rally
- Manipulation:
  - Control group: no information
  - Free speech group: article emphasizing rights
  - Public safety group: article emphasizing safety

Example: Partisan Identity

- Theory: Strength of partisan identity affects tendency to accept party position
- Hypotheses:
  - Strong partisans are more likely to accept their party's position on an issue
- Manipulation:
  - Control group: no manipulation
  - "Univalent" condition
  - "Ambivalent" condition

Univalent

These days, Democrats and Republicans differ from one another considerably. The two groups seem to be growing further and further apart, not only in terms of their opinions but also their lifestyles. Earlier in the survey, you said you tend to identify as a *Democrat/ Republican*. Please take a few minutes to think about what you like about *Democrats/ Republicans* compared to the *Republicans/ Democrats*. Think of 2 to 3 things you especially like best about **your party**. Then think of 2 to 3 things you especially dislike about **the other party**. Now please write those thoughts in the space below.

Ambivalent

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Treatments Test Hypotheses!

- Derive experimental design from hypotheses
- Experimental "factors" are expressions of hypotheses as randomized groups
- What intervention each group receives depends on hypotheses
  - presence/absence
  - levels/doses
  - qualitative variations

Ex.: Presence/Absence

- Theory: Negative campaigning reduces support for the party described negatively.
- Hypothesis: Exposure to a negative advertisement criticizing a party reduces support for that party.
- Manipulation:
  - Control group receives no advertisement.
  - Treatment group watches a video containing a negative ad describing a party.

Ex.: Levels/doses

- Theory: Negative campaigning reduces support for the party described negatively.
- Hypothesis: Exposure to higher levels of negative advertising criticizing a party reduces support for that party.
- Manipulation:
  - Control group receives no advertisement.
  - Treatment group 1 watches a video containing 1 negative ad describing a party.
  - Treatment group 2 watches a video containing 2 negative ads describing a party.
  - Treatment group 3 watches a video containing 3 negative ads describing a party.
  - etc.

Ex.: Qualitative variation

- Theory: Negative campaigning reduces support for the party described negatively.
- Hypothesis: Exposure to a negative advertisement criticizing a party reduces support for that party, while a positive advertisement has no effect.
- Manipulation:
  - Control group receives no advertisement.
  - Negative treatment group watches a video containing a negative ad describing a party.
  - Positive treatment group watches a video containing a positive ad describing a party.

Questions?

Three Major Paradigms

1. Field Experiments
   - Ex. Broockman (2013) – "Black Politicians Are More Intrinsically Motivated to Advance Blacks' Interests"
2. Survey Experiments
   - Ex. Renshon, Yarhi-Milo, and Kertzer (2016) – "Democratic Leaders, Crises and War: Paired Experiments on the Israeli Knesset and Public"
3. Simulations
   - Ex. Frechette, Kagel, Lehrer (2003) – "Bargaining in Legislatures: An Experimental Investigation of Open versus Closed Amendment Rules"

Paradigm 1: Field Experiments

- Basic idea: randomly expose legislators *in situ* to some experience and measure an outcome that might be affected by it
- Two "flavours"
  1. Orchestrated by the researcher(s)
  2. "Natural" experiments not orchestrated by the researcher(s)
- Tend to be simple in terms of design due to practical difficulty of exposing legislators' to treatment and measuring outcomes
- "Natural" experiments are limited to the fact

Paradigm 1: Field Experiments

- Example of Flavour A
  - Broockman (2013)
  - Treatment: Form of contact from a prospective constituent
  - Outcome: Whether a response is received
  - Effect: Difference in response rates by treatment
- Example of Flavour B
  - Kellermann, Shepsle (2009)
  - Treatment: Freshmen legislators are randomly ordered in determining committee assignments
  - Outcome: Various metrics of leadership and legislative activity
  - Effect: Difference in those outcomes between higher- and lower-ranked legislators

Paradigm 2: Survey experiments

- Basic idea: conduct interviews with legislators (in-person or through another mode), where features of questionnaire are randomized
- Recruiting legislators into interviews tends to be extremely difficult, thus:
  - Almost unavoidably underpowered
  - Can only study legislators who agree to participate
  - Necessarily simplistic designs with treatment and outcome measured in a single interview[3]
  - Survey experiments on legislators tend to be rare

[3] Can be generalized to allow field treatments with survey measures, or survey treatments with field measures

Paradigm 2: Survey Experiments

Example:

- Butler and Dynes (2016)
- Treatment: State legislators completing a survey read a hypothetical constituent letter with varying stated opinions
- Outcome: Measures of perceptions of constituent characteristics (e.g., knowledge)
- Effect: Difference in perceptions b/w constituents with similar/dissimilar views to

Paradigm 3: Simulations

- Basic idea: Derive theoretical expectations about legislative behavior and test those predictions in a *stylized* legislative context using non-legislators as participants
- These are historically much more common than paradigms 1 or 2
- Unique considerations:
  - Tend to be based in formal theories of legislatures
  - Sample sizes limited by resources
  - Historically in labs, but increasingly common online
  - Tend to lack face validity given context and participants

Paradigm 3: Simulations

Example:

- Wilson (1986)
- Treatment: "Legislators" vote under open or closed amendment rules
- Outcome: The final "policy" adopted by the "legislature"
- Effect: Difference in "policy" adopted by the legislature

Questions?

15-minute Activity!

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   - Group 3: Frechette, Kagel, Lehrer (2003)

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   - Group 3: Frechette, Kagel, Lehrer (2003)
3. Discuss:
   - What is the experiment? How does it work?
   - What do the authors find? What is the effect?
   - What are the practical challenges/issues raised?

Many Challenges, Too Little Time

1. Nonresponse and Noncompliance
2. Spillover
3. What can be randomized?
4. Ethics

Treatment Noncompliance

- Definition:

  "when subjects who were assigned to receive the treatment go untreated or when subjects assigned to the control group are treated" [4]

[4] Gerber & Green. 2012. *Field Experiments*, p.132.

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  "when subjects who were assigned to receive the treatment go untreated or when subjects assigned to the control group are treated" [4]

- Several strategies
  - "As treated" analysis
  - "Intention to treat" analysis
  - Estimate a LATE

[4] Gerber & Green. 2012. *Field Experiments*, p.132.

Analyzing Noncompliance

- If noncompliance only occurs in one group, it is *asymmetric* or *one-sided*
- We can ignore non-compliance and analyze the "intention to treat" effect, which will underestimate our effects because some people were not treated as assigned: $ITT = \overline{Y}_1 - \overline{Y}_0$

Analyzing Noncompliance

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- We can ignore non-compliance and analyze the "intention to treat" effect, which will underestimate our effects because some people were not treated as assigned: $ITT = \overline{Y}_1 - \overline{Y}_0$
- We can use "instrumental variables" to estimate the "local average treatment effect" (LATE) for those that complied with treatment: $LATE = \frac{ITT}{\%Compliant}$

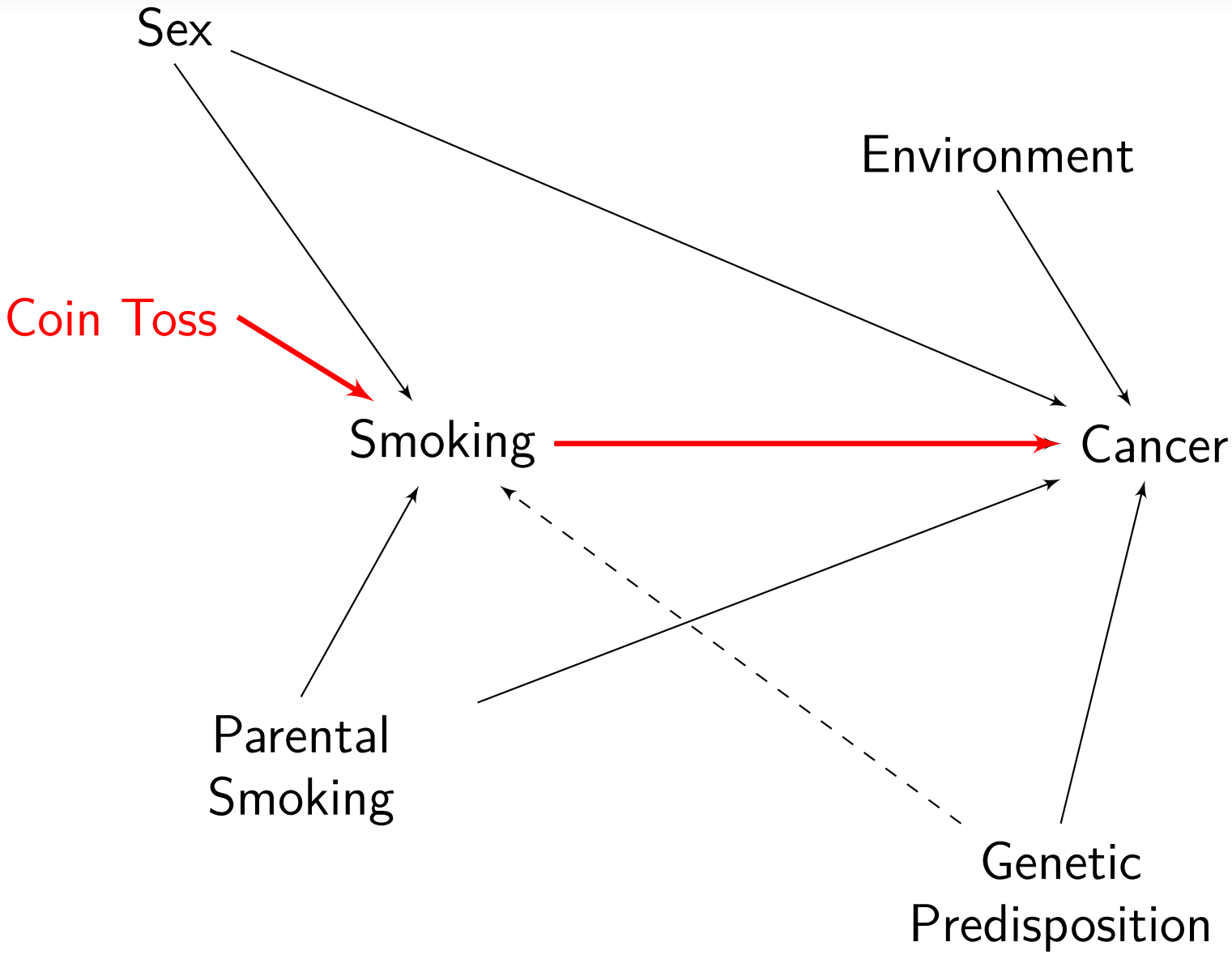
Local Average Treatment Effect

- IV estimate is *local* to the variation in X that is due to variation in D
- This matters if effects are *heterogeneous*
- LATE is effect for those who *comply*
- Four subpopulations:
  - Compliers: $X = 1$ only if $D = 1$
  - Always-takers: $X = 1$ regardless of D
  - Never-takers: $X = 0$ regardless of D
  - Defiers: $X = 1$ only if $D = 0$
- Exclusion restriction! Monotonicity!

Questions?

Compliance

- Compliance is when individuals receive and accept the treatment to which they are assigned:
  - Receive the wrong treatment (cross-over)
  - Fail to receive any treatment
- This causes problems for our analysis because factors other than randomization explain why individuals receive their treatment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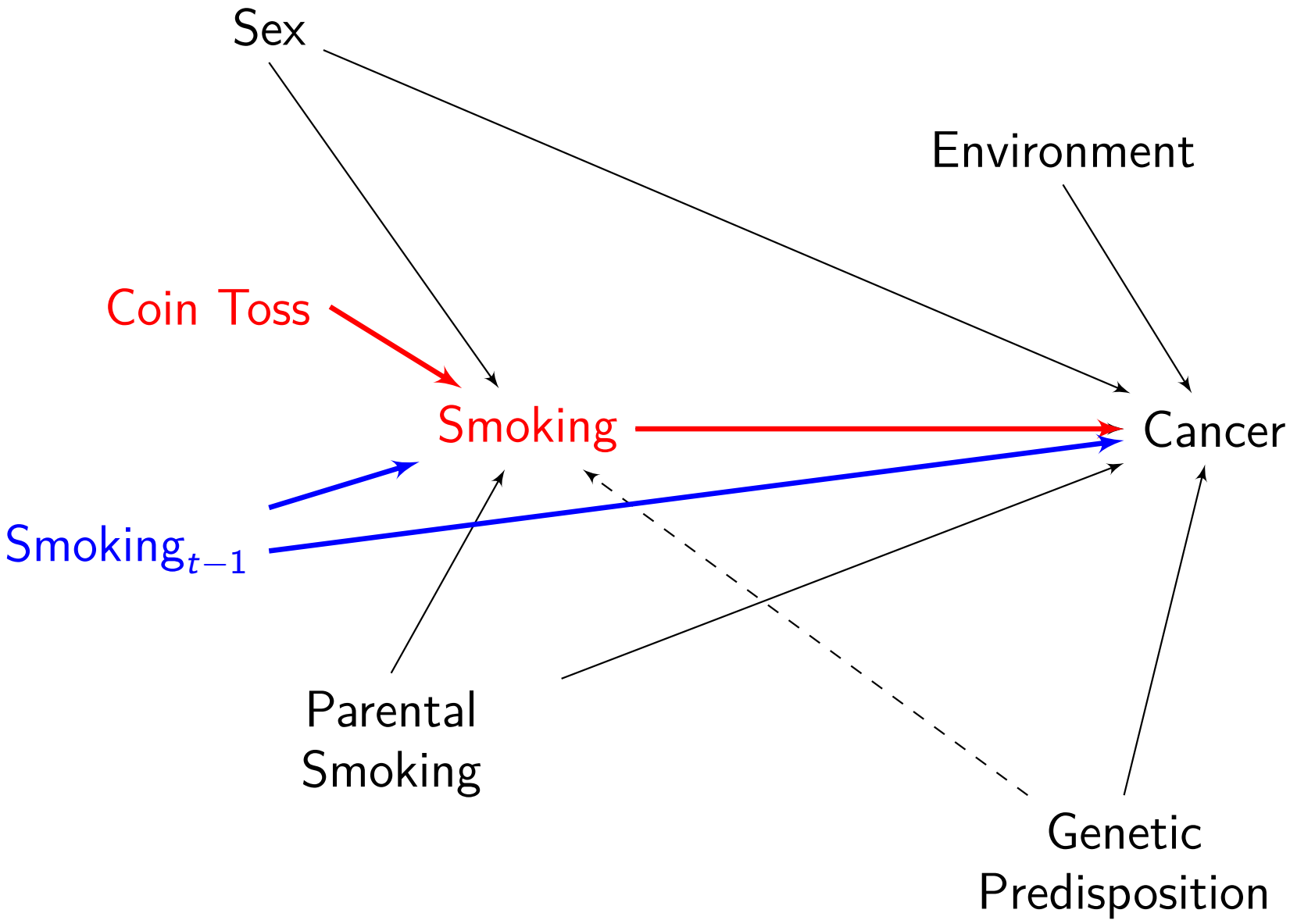
Genetic
Predisposition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Smoking
Cancer
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Parental Smoking
Genetic Predisposition

What can be randomized?

Activity!

- How do we know if an experiment is any good?
- Talk with a partner for about 3 minutes
- Try to develop some criteria that allow you to evaluate "what makes for a good experiment?"

Some possible criteria

- Significant results
- Face validity
- Coherent for respondents
- Non-obvious to respondents
- Simple
- Indirect/unobtrusive
- Validated by prior work
- Innovative/creative
- ...

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–Thomas J. Leeper (18 January 2017)

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- Before the study using *pilot testing* (or *pretesting*)
- During the study, using *manipulation checks*
- During the study, using *placebos*
- During the study, using *non-equivalent outcomes*

I. Outcomes Affected

- Follows a circular logic!
- Doesn't tell us anything if we hypothesize null effects

II. Pilot Testing

- Goal: establish construct validity of manipulation
- Assess whether a set of possible manipulations affect a measure of the *independent* variable

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- Goal: establish construct validity of manipulation
- Assess whether a set of possible manipulations affect a measure of the *independent* variable
- Example:
  - Goal: Manipulate the "strength" of an argument
  - Write several arguments
  - Ask pilot test respondents to report how strong each one was

III. Manipulation Checks

- Manipulation checks are items added post-treatment, post-outcome that assess whether the *independent* variable was affected by treatment
- We typically talk about manipulations as directly setting the value of X, but in practice we are typically manipulating something *that we think* strongly modifies X

III. Manipulation Checks

- Manipulation checks are items added post-treatment, post-outcome that assess whether the *independent* variable was affected by treatment
- We typically talk about manipulations as directly setting the value of X, but in practice we are typically manipulating something *that we think* strongly modifies X
- Example: information manipulations aim to modify knowledge or beliefs, but are necessarily imperfect at doing so

Manipulation check example[5]

1. Treatment 1: Supply Information
2. Manipulation check 1: measure beliefs
3. Treatment 2: Prime a set of considerations
4. Outcome: Measure opinion
5. Manipulation check 2: measure dimension salience

[5] Leeper & Slothuus. n.d. "Can Citizens Be Framed?" Available from: `http://thomasleeper.com/research.html`.

Some Best Practices

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- Manipulation checks should be innocuous
  - Shouldn't modify independent variable
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- Manipulation checks should be innocuous
  - Shouldn't modify independent variable
  - Shouldn't modify outcome variable
- Generally, measure post-outcome
- Measure both what you wanted to manipulate *and* what you didn't want to manipulate
  - Most treatments are *compound*!

IV. Placebos

- Include an experimental condition that *does not* manipulate the variable of interest (but might affect the outcome)

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- Include an experimental condition that *does not* manipulate the variable of interest (but might affect the outcome)
- Example:
  - Study whether risk-related arguments about climate change increase support for a climate change policy
  - Placebo condition: control article with risk-related arguments about non-environmental issue (e.g., terrorism)

V. Non-equivalent outcomes

- Measures an outcome that *should not* be affected by independent variable

V. Non-equivalent outcomes

- Measures an outcome that *should not* be affected by independent variable
- Example:
  - Assess effect of some treatment on attitudes toward group A
  - Focal outcome: attitudes toward group A
  - Non-equivalent outcome: attitudes toward group B

Aside: Demand Characteristics

- "Demand characteristics" are features of experiments that (unintentionally) imply the purpose of the study and thereby change respondents' behavior (to be consistent with theory)

[6] But, consider the ethics of not doing so (more Friday)

Aside: Demand Characteristics

- "Demand characteristics" are features of experiments that (unintentionally) imply the purpose of the study and thereby change respondents' behavior (to be consistent with theory)
- Implications:
  - Design experimental treatments that are non-obvious
  - Do not disclose the purpose of the study up front[6]

[6] But, consider the ethics of not doing so (more Friday)

Student presentations!

Learning Outcomes

By the end of the day, you should be able to...

1. Explain how to analyze experiments quantitatively.
2. Explain how to design experiments that speak to relevant research questions and theories.
3. Evaluate the uses and limitations of three common legislative experimental paradigms: survey experiments, field experiments, and simulations.
4. Identify practical issues that arise in the implementation of experiments and evaluate how to anticipate and respond to them.

In Conclusion

- Experiments are mostly about design, not analysis
- Experiments are underutilized in legislative contexts, in part because conducting them effectively is extremely difficult
- This means that careful but often simple design can generate potentially powerful and novel insights into legislative behavior

Quiz

Quiz time!

Compliance

1. What is compliance?

Compliance

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2. How can we analyze experimental data when there is noncompliance?

Balance testing

1. What does randomization ensure about the composition of treatment groups?

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2. What can we do if we find a covariate imbalance between groups?

Balance testing

1. What does randomization ensure about the composition of treatment groups?
2. What can we do if we find a covariate imbalance between groups?
3. How can we avoid this problem entirely?

Nonresponse and Attrition

1. Do we care about outcome nonresponse in experiments?

Nonresponse and Attrition

1. Do we care about outcome nonresponse in experiments?
2. How can we analyze experimental data when there is outcome nonresponse or post-treatment attrition?

Manipulation checks

1. What is a manipulation check? What can we do with it?

Manipulation checks

1. What is a manipulation check? What can we do with it?
2. What do we do if some respondents "fail" a manipulation check?

Null effects

1. What should we do if we find our estimated $\widehat{SATE} = 0$?

Null effects

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2. What does it mean for an experiment to be *underpowered*?

Null effects

1. What should we do if we find our estimated $\widehat{SATE} = 0$?
2. What does it mean for an experiment to be *underpowered*?
3. What can we do to reduce the probability of obtaining an (unwanted) "null effect"?

Effect heterogeneity

1. What should we do if, post-hoc, we find evidence of effect heterogeneity?

Effect heterogeneity

1. What should we do if, post-hoc, we find evidence of effect heterogeneity?
2. What can we do pre-implementation to address possible heterogeneity?

Representativeness

1. Under what conditions is a design-based, probability sample necessary for experimental inference?

Representativeness

1. Under what conditions is a design-based, probability sample necessary for experimental inference?
2. What kind of causal inferences can we draw from an experiment on a descriptively unrepresentative sample?

Peer Review

1. What should we do if a peer reviewer asks us to “control” for covariates in the analysis?

Peer Review

1. What should we do if a peer reviewer asks us to "control" for covariates in the analysis?
2. What should we do if a peer reviewer asks us to include or exclude particular respondents from the analysis?