

## **Announcements:**

- Midterm Monday. Bring calculator and one sheet of notes. No calculator = cell phone!
- Assigned seats, random ID check.
- Review Friday. Review sheet posted on website.
- Mon discussion *is* for credit (3<sup>rd</sup> of 7 for credit).
- Week 3 quiz starts at 1pm today, ends Fri.
- After midterm, a “week” will be Wed, Fri, Mon, so quizzes start on *Mondays*, homework due *Weds*. See website for specific days and dates.

## **Homework** (due Friday)

Chapter 4: #13, 21, 36

**Today:** Chapter 4 and finish Ch 3 lecture.

# Chapter 4

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Gathering Useful Data for Examining  
Relationships

# Research Studies to Detect Relationships

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## **Observational Study:**

Researchers *observe* or question participants about opinions, behaviors, or outcomes. Participants are not asked to do anything differently.

## **Experiment:**

Researchers *manipulate* something and measure the effect of the manipulation on some outcome of interest.

**Randomized experiment:** The participants are *randomly assigned* to participate in one condition or another, or if they do all conditions the *order* is randomly assigned.

# Examples (details given in class)

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Are these experiments or observational studies?

1. Mozart and IQ
2. Drinking tea and conception (p. 721)
3. Autistic spectrum disorder and mercury  
<http://www.jpands.org/vol8no3/geier.pdf>
4. Aspirin and heart attacks (Case study 1.6)

# Who is Measured: Units, Subjects, Participants

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- **Unit:** a single individual or object being measured once.
- If an experiment, then called an **experimental unit**.
- When units are people, often called **subjects** or **participants**.

Units for the 4 examples:

Students, women, autistic children, physicians

# Explanatory and Response Variables

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**Explanatory variable** (or *independent* variable) is one that may explain or may cause differences in a **response variable** (or *outcome* or *dependent* variable).

## Explanatory

Mozart, etc.

Drank tea or not

Mercury level?

Aspirin or placebo

## Response

IQ

Conceived or not

Autistic or not?

Heart attack or not



# Confounding variables

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A **confounding variable** is a variable that:

1. *Affects the response variable and also*
2. *is related to the explanatory variable.*

A potential confounding variable not measured in the study is called a **lurking variable**.

# Confounding variables and causation

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- **Randomized experiments:**

Confounding variables probably average out over the different treatment groups, so we *can* conclude change in explanatory variable *causes* change in response variable.

- **Observational studies:**

Confounding variables may explain an observed relationship between the explanatory and response variables, so we *cannot* conclude that a change in the explanatory variable *causes* a change in the response variable.



## Examples: Confounding variable *affects* response, is *related* to explanatory variable

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- Tea and conception: Possible confounding variable is drinking coffee:
  - It might *affect* probability of conception, and
  - It differs for tea drinkers and non-tea drinkers
- Autism and mercury: Possible confounding variable is genetic ability to shed mercury:
  - Same genetic pool may be more prone to autism (genetic makeup affects response of autism)
  - It would result in different mercury levels (related to explanatory variable)

# Designing a good experiment

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- Who participates? Can results be extended to a population?
- How are the units randomized to treatments?
- What controls are used?
- Should pairs, blocks, and/or repeated measures be used?

# Who Participates in Randomized Experiments?

Participants are often **volunteers**.

Recall **Fundamental Rule for Inference:**

Available data can be used to make inferences about a much larger group *if the data can be considered to be representative with regard to the question(s) of interest.*

Volunteer group often meets this criterion.

Example: Students listening to Mozart.

Example: Male physicians taking aspirin?

# Randomization: Used to Rule out Confounding Variables

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## Randomizing the *Type* of Treatment:

Randomly assigning the treatments to the experimental units keeps the researchers from making assignments favorable to their hypotheses and also helps protect against hidden or unknown biases.

Ex: Physicians were randomly assigned to take aspirin or placebo.

## Randomizing the *Order* of Treatments:

If all treatments are applied to each unit, randomization should be used to determine the *order*.

Ex: Order of listening conditions randomly assigned.

# Control Groups and Placebos

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## **Control group and/or control condition:**

Treated identically in all respects except they don't receive the active treatment. Sometimes they receive a *dummy* treatment or a standard or existing treatment. Ex: Silent condition

## **Placebo:**

Looks like real drug but has no active ingredient. Ex: Placebo looked just like aspirin

***Placebo effect*** = people respond to placebos.

# Blind, double blind; Double dummy

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- **Blinding:**

- **Single-blind** = participants do not know which treatment they have received.
- **Double-blind** = neither participant nor researcher making measurements knows who had which treatment.

- **Double Dummy: When treatments can't be blind**

- Each group given two “treatments”...
  - Group 1 = real treatment 1 and placebo treatment 2
  - Group 2 = placebo treatment 1 and real treatment 2

**Example:** Compare nicotine patches and nicotine gum to quit smoking

Group 1: Nicotine patch + placebo gum

Group 2: Placebo patch + nicotine gum

# Examples:

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- Aspirin and heart attacks
  - Double blind. Neither the physicians participating nor their health assessors knew who had aspirin.
- Mozart and IQ
  - Single blind at best. Obviously students knew which condition they just had. Hopefully the person administering the IQ test didn't know.

# “Designing” Studies with Pairing, Blocking and Repeated Measures

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- **Block Designs – More efficient if units quite variable**

Experimental units divided into homogeneous groups called **blocks**, each treatment randomly assigned to one or more units in each block. Goal: *Small* natural variability within blocks.

- **Matched-Pair Designs**

Two matched individuals, or same individual, receives each of two treatments. Special case of a *block design*. Important to randomize order of two treatments and use blinding if possible.

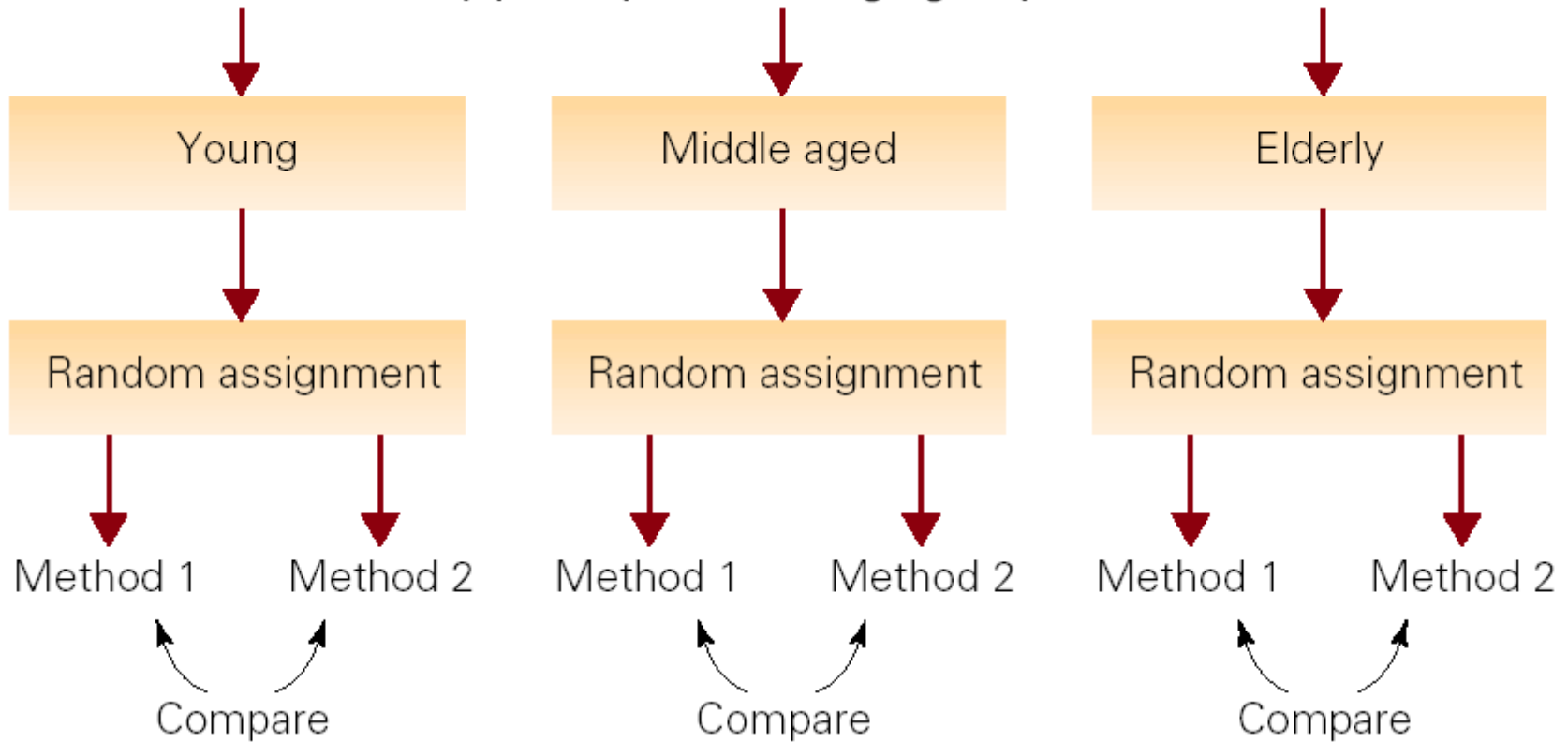
- **Repeated-measures designs**

*Blocks* = individuals and *units* = repeated time periods in which they receive varying treatments (Mozart example)



# Example from book: Compare two memorization methods, block by age

Classify participants into age groups (blocks)



# Terminology for various designs

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## ■ **Completely randomized experiment**

- No blocks, no matched pairs, no repeated measures. Randomly assign a certain number of units to receive each treatment. Aspirin example.

## ■ **Randomized block design**

- Divide units into groups (blocks) of similar units; randomly assign treatments within each block. Ideal is one unit per block gets each treatment.

Special cases:

- Repeated measures: Each individual is his/her own block
- Matched-pairs design: Two units per block, same individual or matched to be similar (e.g. twins, same IQ, etc.)

# Nicotine patch example:

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- Who were the participants?
- Completely randomized experiment?  
Randomized block experiment? Repeated measures experiment? Matched pairs?
- Single blind, double blind, or neither?
- Control group, placebo, both, neither?

## 4.3 Designing a Good Observational Study

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- **Disadvantage:** more difficult to establish causal links; possible confounding variables.
- **Advantage:** more likely to measure participants in their natural setting.
- It isn't always possible to do an experiment, for ethical or practical reasons.

# Types of Observational Studies:

## Retrospective/ Prospective

## Case-control/ Cross sectional

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**Retrospective:** Participants are asked to recall past events.  
Example: Myopia study asked parents to recall infant night-light.

**Prospective:** Participants are followed into the future and events are recorded.  
Example: Tea-drinking study, women kept food diaries for a year.

**Case-Control Studies:** A sample of “Cases” who have a particular attribute or condition are compared to “controls” who do not, to see how they differ on an explanatory variable of interest. The “case-control” variable is *usually* the *response* variable. (Example: Autism or not is the *response* variable.)

**Cross-sectional Studies:** Sample taken, then classified.

# Advantages of Case-control Studies compared to “cross-sectional” studies

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- Efficiency – may not get enough cases otherwise
  - Autism and mercury example. If they had chosen a sample of kids (cross-sectional) and measured mercury and whether they had autism, they would have had few autism cases.
- Reduction of potential confounding variables
  - Controls often chosen to be as similar as possible to cases in all other ways. For example, for cancer studies, possibly use a sibling or close friend of the cancer case (matched pairs). Idea is to have similar genetics and lifestyle.

# Case Study 4.4 *Baldness and Heart Attacks*

“Men with typical male pattern baldness ... are anywhere from 30 to 300 percent more likely to suffer a heart attack than men with little or no hair loss at all.” *Newsweek*, March 9, 1993, p. 62

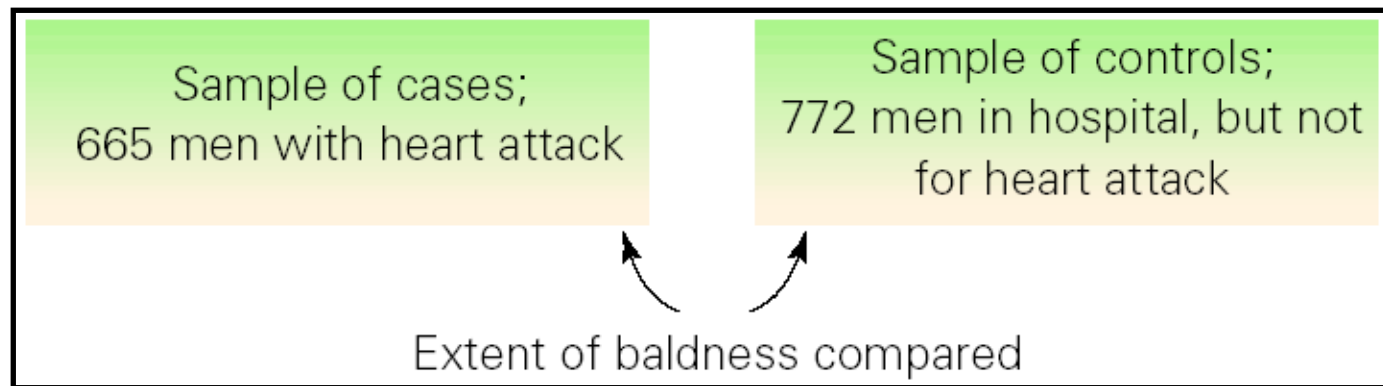
**Case-control study** (Case/Control is *response variable*)

Cases = men admitted to hospital with heart attack

Controls = men admitted for other reasons.

**Case/control (response) variable:** heart attack status (yes/no)

**Explanatory variable:** degree of baldness



# Why relative risk often doesn't make sense, and must use odds ratio instead

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	Heart attack	No heart attack	Total
Baldness	279	263	542
No baldness	386	509	895
Total	665	772	1437

The *column totals* were chosen to be about equal, so about equal numbers with and without heart attacks. Risk of heart attack if bald is *not* estimated by  $279/542 = .515$  (over half!). But we *can* compare odds of heart attack to no heart attack for bald and not bald.



## 4.4 Difficulties and Disasters in Experiments and Observational Studies

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### **Confounding Variables and the Implication of Causation in Observational Studies**

Big misinterpretation = reporting *cause-and-effect* relationship based on an observational study. No way to separate the role of confounding variables from the role of explanatory variables in producing the outcome variable if randomization is not used.

### **Extending Results Inappropriately**

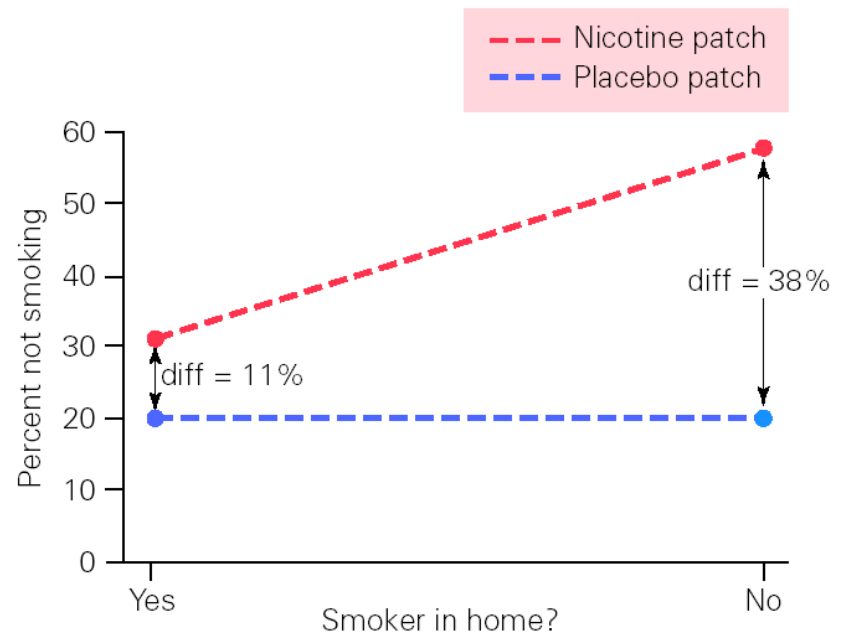
Many studies use convenience samples or volunteers. Need to assess if the results can be extended to any larger group for the question of interest.

# Interacting Variables – not the same as confounding variables!

Another variable can *interact* with the explanatory variable in its relationship with the outcome variable. Results should be reported taking the interaction into account.

## Example:

The difference between the nicotine and placebo patches is greater when there are no smokers in the home than when there are smokers in the home.



# Different from confounding variable

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- If this had been an *observational study* asking about using nicotine patches, “other smokers at home” would have been a confounding variable
  - Affects response of quitting or not
  - Related to explanatory – using nicotine patches or not
- However, as a randomized experiment, proportion with other smokers at home should be similar for nicotine and placebo patch groups, so not related to explanatory variable.

# Hawthorne Effect and Experimenter Bias

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## **Hawthorne effect**

Participants in an experiment respond differently than they otherwise would, just because they are in the experiment. Many treatments have higher success rate in clinical trials than in actual practice.

## **Experimenter effects**

Experimenters do subtle things unintentionally that help results match desired outcome, such as recording errors in their favor, treating subjects differently, etc. Mostly can be overcome by blinding and control groups.

(See example 4.5 – even mice responded to cues!)

# Ecological Validity and Generalizability

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When variables have been removed from their natural setting and are measured in the laboratory or in some other artificial setting, the results may not reflect the impact of the variable in the real world. Less of a problem in observational studies.

## Example:

Women in the tea-drinking study may have altered their diets because they knew they were being monitored by the experimenters.

# Relying on Memory or Secondhand Sources

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- **Can be a problem in retrospective observational studies.**
- **Try to use authoritative sources such as medical records rather than rely on memory.**
- **If possible, use prospective observational studies.**

Example 4.7 on whether left-handers die young.

# If statistically significant relationship is found, what can be concluded?

	Sample represents population for question of interest	Sample doesn't represent population
Randomized Experiment	Causal relationship, and can extend results to population	Causal relationship, but cannot extend results to population
Observational Study	Can't conclude causal relationship, but can extend results to population	Cannot conclude causal relationship, and cannot extend results to a population

# Examples:

	Sample represents population for question of interest	Sample doesn't represent population
Randomized Experiment	Mozart and IQ Nicotine patches	Aspirin and heart attacks: male physicians represent limited population
Observational Study	Autism and mercury Tea and conception	Website surveys, e.g. CNN "Quick Vote"



# Finishing Chapter 3 from Monday

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If time, go over remainder of lecture from Friday. If not, read those slides on your own, starting with slide #48 on Confidentiality and anonymity. Similar material in book on pages 98 to 103.