# Epidemiological study designs

Descriptive, analytic and experimental studies — sampling, time, comparison and threats to validity

**Practical Class 10 · Dental Public Health · 2026/2027**

Chief Assistant Professor Kostadin Kostadinov, MD, PhD

Department of Social Medicine and Public Health · Medical University of Plovdiv

# What you will be able to do

1. Identify a design from **assignment, sampling and time**.
2. Distinguish cross-sectional, ecological, case–control and cohort studies.
3. Interpret RR, risk difference and OR in a design-specific example.
4. Explain randomisation, concealment, blinding and cluster trials.
5. Detect selection and information bias; explain confounding with stratification.

**Plan · 90 minutes**

1. Question and design — 8 min
2. Descriptive studies — 10 min
3. Analytic studies — 25 min
4. Experimental studies — 15 min
5. Errors, causation, evidence — 18 min
6. Exercises and recap — 14 min

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

CONTENTS

1. **From question to design**
2. Descriptive and cross-sectional studies
3. Analytic observational studies
4. Experimental studies
5. Errors, confounding and causal interpretation
6. Exercises and recap

# Purpose, assignment and unit are different axes

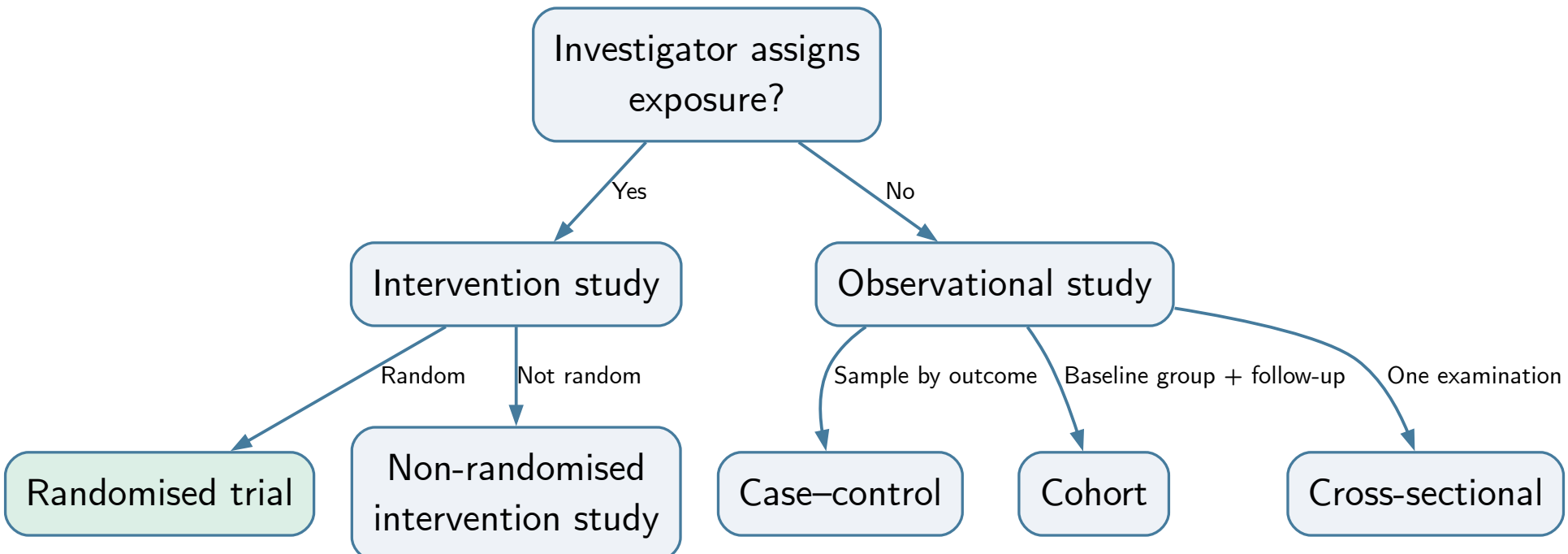
| Axis | Question | Categories |
|---|---|---|
| Purpose | Describe a pattern or test a specified comparison? | **Descriptive** / **analytic** |
| Assignment | Does the investigator assign the intervention? | **Observational** / **interventional** |
| Unit of analysis | Are data linked within each person or only within groups? | **Individual** / **ecological** |

**One design can serve different purposes**

A cross-sectional examination can describe caries frequency, or compare caries between exposure groups. Having two groups alone does not establish causality.

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# Identify the design from how the study is assembled




Simplified guide to the designs in this class. First check the unit: group-level exposure and outcome → ecological study.

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# A protocol fixes the question before the results are known

## Define in advance

Population and eligibility · exposure/intervention · comparator · outcome and timing · sampling/allocation · analysis and missing-data plan.

## A concrete dental question

Among eligible children, does a **new varnish versus standard varnish** reduce **new cavitated lesions over 3 years**? Specify criteria, follow-up and the intended comparison.

**The question, ethical safeguards and practical constraints determine the design. A design label does not guarantee a valid study.**

Ethics review, appropriate consent/assent, data protection and a justified comparator belong in the protocol.

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

CONTENTS

1. From question to design
2. **Descriptive and cross-sectional studies**
3. Analytic observational studies
4. Experimental studies
5. Errors, confounding and causal interpretation
6. Exercises and recap

# A case series can reveal a signal, but not its cause

**Case report / case series**

One unusual patient / several patients with a shared clinical feature. Describe the condition, timing, course and possible exposures.

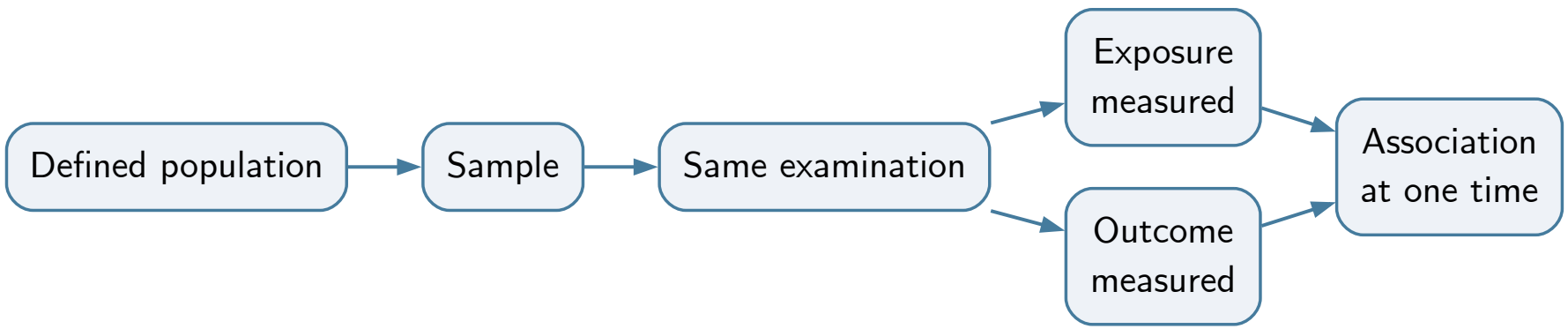
**Hypothetical signal**

A dentist sees three patients with mucosal irritation after starting a new mouthrinse. Record the details and investigate; three observations do not establish the product as the cause.

| Useful for | Cannot establish alone |
|---|---|
| Rare or unexpected events; detailed clinical description; generating a hypothesis | Population frequency without a denominator; an exposure effect without a valid comparison |

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# Cross-sectional: exposure and outcome at one examination




**What it does well**

Describes existing burden; can compare exposure groups; often quicker than follow-up.

**What requires caution**

Current exposure may follow disease. Coverage and non-response affect generalisability; random sampling alone does not guarantee representativeness.

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# Cross-sectional example: an association at one time

Hypothetical: 300 children aged 12, examined and interviewed on the same day.

| | Untreated caries | No untreated caries | Total |
|---|---|---|---|
| Sugary drinks daily | 50 | 50 | 100 |
| Not daily | 40 | 160 | 200 |

**Comparison**

Daily: 50/100 = 50%; others: 40/200 = 20%. **Prevalence ratio = 2.5**. This is not a risk ratio for new disease.

**Alternative explanations**

Diet may have changed after pain or advice. Socioeconomic conditions may influence both diet and dental care.

Hypothetical teaching data

# Ecological studies use group-level data

**Unit = town, region or country**

Compare average exposure or policy with a group outcome. Individuals are not linked to their own exposure and outcome.

**Question**

Do regions with greater sugar sales per resident have more untreated caries? Regional sales are not the measured intake of each resident.

| Useful when | Main concerns |
|---|---|
| The exposure is a policy or contextual property; existing aggregate data are available | Confounding between groups; measurement differences; drawing an individual conclusion from group results |

**Ecological describes the unit of analysis. A group-level question can be legitimate; its interpretation must stay at that level.**

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# A group pattern does not reveal the individual pattern

Hypothetical towns, 100 residents each; only totals are available.

| Town | Daily mouthrinse users | People with tooth loss |
|---|---|---|
| A | 80 | 20 |
| B | 20 | 40 |

**What the totals do not tell us**

In town A, all 20 people with tooth loss could be **users**, or all 20 could be **non-users**. Both allocations fit the same totals.

**We cannot calculate the individual exposure–outcome association from these totals. This is the ecological-fallacy problem.**

Hypothetical teaching data

CONTENTS

1. From question to design
2. Descriptive and cross-sectional studies
3. **Analytic observational studies**
4. Experimental studies
5. Errors, confounding and causal interpretation
6. Exercises and recap

# The same 2 × 2 layout has different sampling meanings

| | **Outcome +** | **Outcome −** | **Total** |
|---|---|---|---|
| Exposed | $a$ | $b$ | $a + b$ |
| Unexposed | $c$ | $d$ | $c + d$ |

**Cohort: row totals are denominators**

Risk ratio (RR) = [a/(a+b)] / [c/(c+d)]
Risk difference = a/(a+b) − c/(c+d).

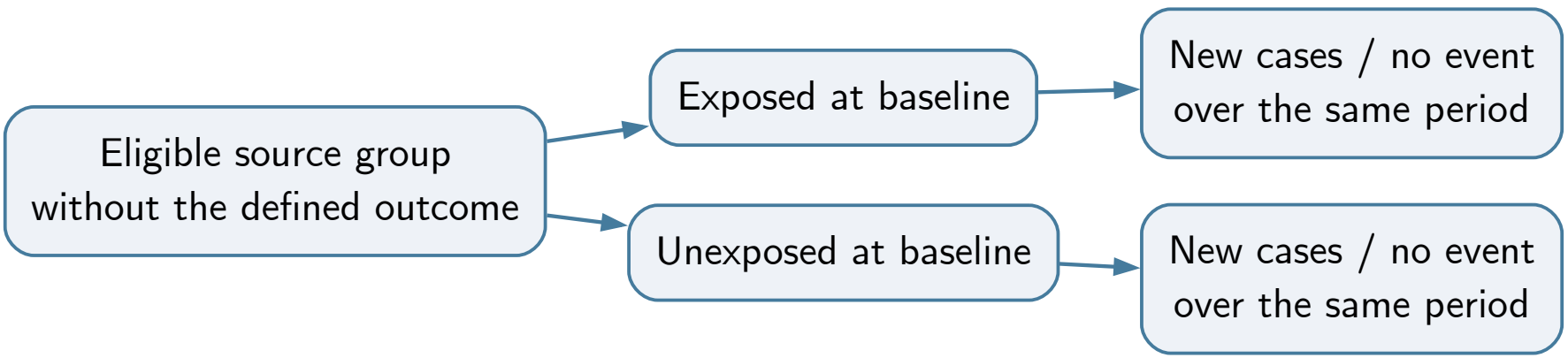
**Case–control: outcomes guide sampling**

Exposure odds in cases = a/c; in controls = b/d. **Odds ratio (OR) = ad/bc**. Ordinary sampled row totals do not give population risks.

1 = no association on the ratio scale; below/above 1 describes direction. A causal interpretation also needs valid design and assumptions.

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# A cohort follows exposure groups towards outcomes




**What is fixed at the start?**

Define eligibility, the baseline exposure and the outcome not yet experienced. Follow the same outcome in both groups, recording changes and losses. Exposure is **observed**, not assigned.

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# Prospective and retrospective cohorts share one time direction

| | **Prospective cohort** | **Retrospective cohort** |
|---|---|---|
| Investigator starts | 2026: enrol a defined group | 2026: reconstruct a defined group from 2016 records |
| Exposure precedes outcome | Baseline 2026 $\longrightarrow$ outcomes 2026–2031 | Baseline 2016 $\longrightarrow$ recorded outcomes 2016–2026 |
| Data collection | Future follow-up | Existing records; no need to wait another decade |
| Main challenge | Retention and changing exposure | Completeness, timing and validity of historical records |

**Retrospective does not mean case–control. A historical cohort is still assembled from a baseline group, not by selecting cases and controls.**

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# Cohort example: relative and absolute comparisons

Hypothetical: 1000 adults without periodontitis, complete 5-year follow-up.

| | New periodontitis | No periodontitis | Total |
|---|---|---|---|
| Smokers | 60 | 340 | 400 |
| Non-smokers | 30 | 570 | 600 |

**Calculate**

$60/400 = 15\%$; $30/600 = 5\%$.

**RR = 3.0**.

**Risk difference = 10 percentage points** over 5 years.

**Interpret without overclaiming**

An excess is associated with smoking in these data. Calling it preventable by removing smoking requires causal assumptions and control of other explanations.

Hypothetical teaching data

# Dunedin: a real prospective cohort example

## Design and timing

Birth cohort in New Zealand, born 1972–1973. Cannabis exposure assessed at ages 18, 21, 26 and 32; dental examinations at 26 and 32. Complete analysis: **903 participants**.

## Defined result

High versus no cannabis exposure: adjusted RR for **incident attachment loss from age 26 to 32 = 2.2 (95% CI 1.2–3.9)**. Adjusted for tobacco, sex, dental attendance and plaque.

**Repeated measurements clarify the time sequence. Adjustment addresses measured factors; it does not guarantee the absence of confounding.**

Thomson et al. JAMA 2008;299:525–531. doi:10.1001/jama.299.5.525

# Cohorts: strong temporal information, real practical limits

**Strengths**

Incidence comparisons · multiple outcomes · useful for rare exposures when an exposed group can be identified · exposure records may precede diagnosis.

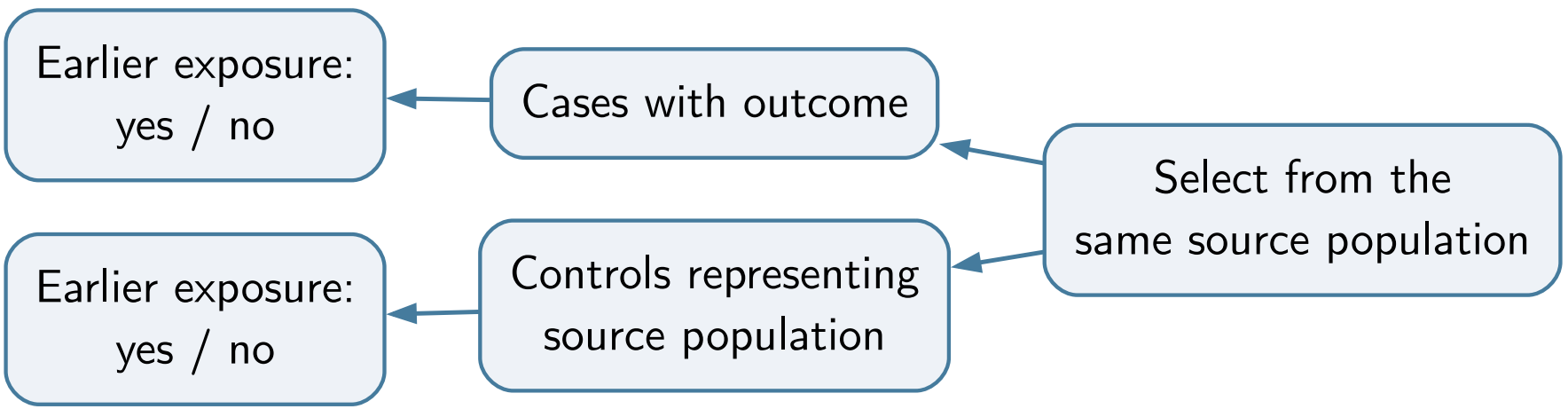
**Limitations**

Prospective follow-up may be long and costly · rare outcomes need large populations · differential losses · changing exposure · historical record quality.

**Chronology helps causal reasoning; it does not remove confounding, measurement error or selection bias.**

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# Case–control: sampling begins with the outcome




**Read the logic from right to left**

Select cases and suitable controls, then ascertain earlier exposure in both. Existing records or stored samples can measure exposure; recall is not required by the design.

Doll & Hill (1950), doi:10.1136/bmj.2.4682.739; Vandenbroucke & Pearce (2012), doi:10.1093/ije/dys147

# Good controls represent the source of the cases

## The key question

If a control developed the outcome, would that person have been eligible to be included as a case in this study? Apply the same catchment area, eligibility and time window.

## A hospital is not enough

Controls treated for smoking-related conditions can have unusually high smoking exposure. "Same hospital" alone does not make the comparison valid.

## Matching is a design choice

Age/sex matching can aid comparability and efficiency, but needs analysis appropriate to matching. Do not match on the exposure being studied; avoid unnecessary matching on its close proxies.

Vandenbroucke & Pearce (2012), doi:10.1093/ije/dys147; CDC Field Epidemiology Manual

# Case–control example: interpret exposure odds

Hypothetical unmatched sample: 100 oral-cancer cases and 200 suitable controls from the source population.

| | Cases | Controls | Total |
|---|---|---|---|
| Ever smokers | 60 | 50 | 110 |
| Never smokers | 40 | 150 | 190 |

**Calculate**

Exposure odds: cases 60/40 = 1.5; controls 50/150 = 0.333.

**OR = 60 × 150 / (50 × 40) = 4.5.**

**The wrong denominator**

60/(60+50) is not the population cancer risk among smokers. The case/control sample sizes were chosen by the investigator.

Hypothetical unmatched teaching data

# When is OR close to a risk ratio?

Two hypothetical complete-population examples; risks refer to the same follow-up interval.

| Outcome frequency | Risk, exposed | Risk, unexposed | RR | Outcome OR |
|---|---|---|---|---|
| Rare in both groups | 2% | 1% | 2.00 | **2.02** |
| Common | 40% | 20% | 2.00 | **2.67** |

**Why the difference?**

Odds = risk / (1 – risk). Thus OR = RR × (1 – $\text{risk}_{\text{unexposed}}$) / (1 – $\text{risk}_{\text{exposed}}$). With small risks both correction terms are close to 1.

**Rarity refers to the source-population risks, not the chosen fraction of cases in a case–control sample. OR 2.67 is not automatically "2.67 times the risk".**

Own arithmetic; Vandenbroucke & Pearce. Int J Epidemiol 2012;41:1480–1489. doi:10.1093/ije/dys147

# Compare observational designs by sampling, not just dates

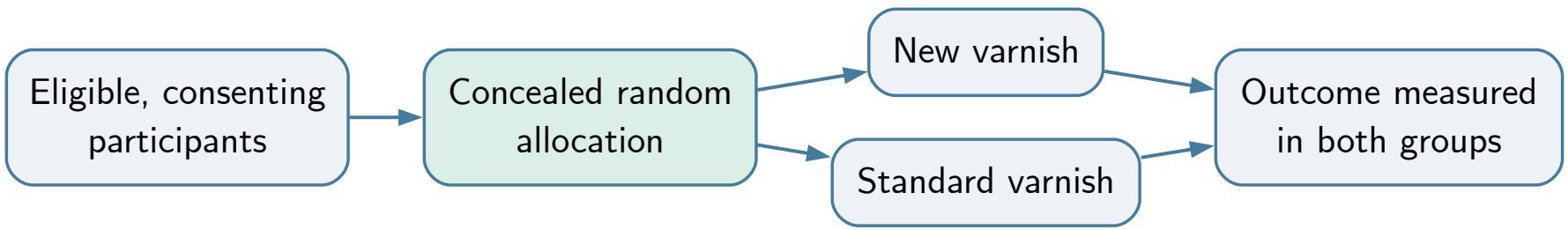
| | Cross-sectional | Case–control | Cohort |
|---|---|---|---|
| Selected from | Population at a time | Cases + source-population controls | Defined baseline group |
| Exposure–outcome link | Same examination | Prior exposure ascertained after sampling | Exposure → later outcome, prospective or historical |
| Typical comparison | Prevalence ratio | Exposure OR | RR, rate ratio, risk difference |
| Useful for | Burden and service planning | Rare outcomes; many exposures | Rare exposures; many outcomes |
| Key threat | Unclear ordering | Control selection and exposure quality | Losses, confounding and exposure changes |

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

CONTENTS

1. From question to design
2. Descriptive and cross-sectional studies
3. Analytic observational studies
4. **Experimental studies**
5. Errors, confounding and causal interpretation
6. Exercises and recap

# In a randomised trial the investigator assigns the intervention




**Dental example**

Randomly allocate eligible children to a new varnish or standard varnish. Use the same follow-up, outcome criteria and background care. Compare outcomes by assigned group.

An intention-to-treat approach retains the assigned comparison; missing outcomes still require a justified plan.

Cochrane Handbook, ch. 8; Burt & Eklund (2021), ch. 12

# Randomisation and allocation concealment solve different problems

| Step | Purpose | Example |
|---|---|---|
| Random sequence | Avoid systematic assignment by prognosis | Computer-generated sequence; 1:1 or a justified unequal ratio |
| Allocation concealment | Prevent prediction of the next assignment during recruitment | Central allocation after eligibility and enrolment are confirmed |
| Stratified randomisation | Improve balance on selected prognostic factors | Separate random sequences within baseline-risk strata |

**Randomisation balances prognostic factors in expectation, not perfectly in every sample. Alternating patients or letting them choose is not randomisation.**

Cochrane Handbook, ch. 8 (randomisation process and allocation concealment)

# A control need not be placebo, and blinding must name who

## Comparator choices

**Active control**: standard varnish or usual care.
**Placebo**: similar product without the tested active ingredient, when scientifically and ethically justified. Both arms may receive background standard prevention.

## Who is unaware?

Participants · care providers · outcome assessors · analysts. State the roles; "single" and "double" blind can be ambiguous.

## When participant blinding is difficult

A school brushing programme is visible. Outcome assessors may still be kept unaware of allocation; use standard criteria and equal follow-up.

Cochrane Handbook, ch. 8; Burt & Eklund (2021), ch. 12

# Cluster trials randomise groups, not each child

**Parallel cluster trial**

20 schools → randomise 10 to supervised brushing and 10 to standard school practice. The **school** is the allocation unit; child outcomes are measured within schools.

**Stepped-wedge cluster trial**

Clusters switch from control to intervention in a **randomised order of periods**. Eventually all switch. Calendar-time trends must be handled in design and analysis.

**Children within a school are correlated. Analysing them as independent observations can overstate precision.**

A community intervention need not be randomised. A field trial is not a synonym for a cluster trial.

Hemming et al. BMJ 2015;350:h391. doi:10.1136/bmj.h391; Cochrane Handbook, ch. 23

# A before–after change is not automatically an intervention effect

Hypothetical repeated school examinations, same age and outcome definition.

| Group | Before | After | Change |
|---|---|---|---|
| Programme school | 40% | 30% | –10 percentage points |
| Comparison school | 35% | 30% | –5 percentage points |

**The extra change**

(30 – 40) – (30 – 35) = **–5 percentage points**. The programme school improved by 10 points, but a background improvement also occurred.

Causal interpretation needs assumptions: comparable background trends and no differential concurrent changes. One pre-period cannot establish those trends.

Hypothetical data; CDC fluoridation timeline (Grand Rapids–Muskegon, 1945–1951)

# Crossover trials need reversible effects and appropriate timing

| Random sequence | Period 1 | Washout if needed | Period 2 |
|---|---|---|---|
| AB | Product A | Sufficient time | Product B |
| BA | Product B | Sufficient time | Product A |

**Each person is their own control**

Can suit short-term effects on stable, reversible conditions. Analyse paired outcomes and period effects. A and B may be two active products.

**Carry-over**

The first treatment can influence the second period. Irreversible outcomes, such as an extracted tooth, cannot be reset by a washout period.

Cochrane Handbook, ch. 23 (crossover trials)

# Trial purpose differs from trial phase

## Efficacy and effectiveness

**Efficacy**: benefit under controlled conditions.
**Effectiveness**: benefit in routine practice, with real adherence, staffing and access.
These are aims on a continuum, not separate allocation methods.

## Drug-development phases

**I**: safety, dose and tolerability.
**II**: preliminary efficacy and safety.
**III**: confirmatory benefit–risk evaluation.
**IV**: studies after approval.

Phase labels do not apply to every public-health or dental-service study. Sample sizes and comparators depend on the question; a phase III trial does not have to demonstrate superiority.

Burt & Eklund (2021), ch. 12; FDA, Step 3: Clinical Research

CONTENTS

1. From question to design
2. Descriptive and cross-sectional studies
3. Analytic observational studies
4. Experimental studies
5. **Errors, confounding and causal interpretation**
6. Exercises and recap

# More participants improve precision, not necessarily validity

| Problem | What happens? | Does a larger sample solve it? |
|---|---|---|
| Random error | An estimate fluctuates by chance | Usually improves precision, other things equal |
| Systematic error (bias) | Selection or measurement systematically distorts the estimate | No; a narrow interval can surround a biased value |
| Confounding | A common cause mixes the exposure–outcome comparison | No; more observations do not create comparability |

**A confidence interval quantifies statistical uncertainty under the analysis assumptions. It is not a certificate that bias and confounding are absent.**

CDC Field Epidemiology Manual, Analyzing and Interpreting Data

# Selection bias can arise at recruitment or during follow-up

| Mechanism | Dental example | Prevention / response |
|---|---|---|
| Inappropriate controls | Controls with smoking-related admissions | Define the source population and eligible comparison |
| Non-response | People with poor oral health respond differently by exposure | Improve participation; assess who is missing |
| Differential loss | Exposed participants with disease leave more often | Retain participants; examine missingness; sensitivity analysis |

**How a real difference can disappear — hypothetical**

Complete outcomes: exposed 20/100 vs unexposed 10/100 $\longrightarrow$ **RR 2**.

If only 5 cases and 45 non-cases remain in the exposed group: 5/50 vs 10/100 $\longrightarrow$ **observed RR 1**.

CDC Field Epidemiology Manual; hypothetical selection example

# Information bias distorts what is recorded

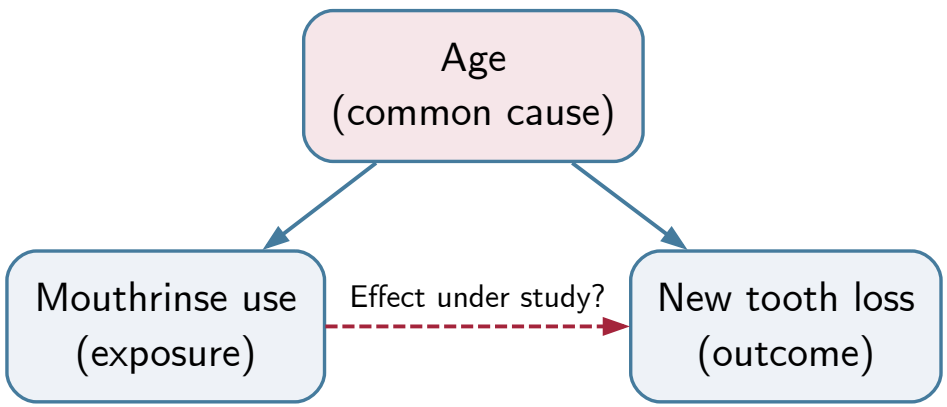
| Type | Example | Safeguard |
|---|---|---|
| Recall bias | Cases recall a past habit differently from controls | Prior records; identical structured questions |
| Interviewer / examiner bias | Known allocation changes questioning or lesion scoring | Blinded assessment where feasible; calibration |
| Misclassification | Exposure or disease is assigned to the wrong category | Validated definitions; equal measurement protocols |

**Hypothetical recall distortion**

True exposure: 40% of 100 cases and 40% of 100 controls.
Reported: 60% of cases but 40% of controls $\longrightarrow$ OR $= (60\times60)/(40\times40) =$ **2.25**, despite true exposure OR 1.

CDC Field Epidemiology Manual; hypothetical misclassification example

# Confounding: a common cause can create an apparent effect




**Hypothetical causal model**

Older adults may use mouthrinse more often and also have greater tooth-loss risk. A crude comparison can mix the effect of age with the exposure of interest.

**A confounder is not merely any variable associated with both. In the simple model it is a common cause, outside the exposure → outcome pathway.**

CDC Field Epidemiology Manual; own hypothetical causal diagram

# Stratification reveals the source of the crude association

Hypothetical: all participants have natural teeth at baseline; outcome = any new tooth loss over 5 years; complete follow-up.

| Age | Mouthrinse users: cases / total | Non-users: cases / total | RR |
|---|---|---|---|
| Under 50 | 10/100 = 10% | 40/400 = 10% | **1.00** |
| 50+ | 160/400 = 40% | 40/100 = 40% | **1.00** |
| All ages | 170/500 = 34% | 80/500 = 16% | **2.125** |

**Compare like with like**

Within each age group there is no exposure association. With equal age weights: both groups have 0.5×10% + 0.5×40% = **25%** → adjusted contrast RR = **1**.

**Users include many more older adults. The crude excess here is entirely explained by the age composition built into the example.**

Own hypothetical confounding example and arithmetic

# Control confounding at design and analysis stages

| Method | What it does | Limit |
|---|---|---|
| Randomisation | Separates assignment from prognostic factors in expectation | Chance imbalance; post-allocation problems remain |
| Restriction | Includes only a defined level, e.g. one age band | Narrower target population |
| Matching | Selects comparable distributions on chosen factors | Needs appropriate matched/adjusted analysis |
| Stratification | Compares exposure within levels of a factor | Sparse data; few factors at once |
| Model adjustment | Accounts for measured factors under model assumptions | Residual/unmeasured confounding; wrong model |

If the exposure effect differs by stratum, consider effect modification and report the differences; do not automatically average them away.

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

# Hill's viewpoints guide reasoning; they are not a scoring rule

| Viewpoints (1965) | What to ask |
|---|---|
| Temporality | Did the relevant exposure precede the outcome? |
| Strength; consistency | How large is the association, and is it found in different settings? |
| Specificity; analogy | How specific is the link, and are comparable causal examples known? |
| Biological gradient | Does the association vary with exposure? |
| Plausibility; coherence | Does the explanation fit mechanisms and the wider evidence? |
| Experiment | Does changing the exposure change the outcome? |

**Temporal ordering is logically necessary for a cause. Neither a large RR nor a count of satisfied viewpoints proves causation; lack of a gradient does not rule it out.**

Hill AB. Proc R Soc Med 1965;58:295–300. doi:10.1177/003591576505800503

# Evidence strength depends on the question and execution

## For prevention or treatment effects

A well-conducted randomised comparison usually offers stronger protection from confounding than a non-randomised one. Systematic reviews depend on the validity and relevance of their included studies.

## For other questions

**Burden**: a representative examination.
**Rare harms**: a well-designed case–control or cohort study.
**Diagnostic accuracy**: compare the test with an appropriate reference standard.

**Assess risk of bias, precision, consistency and applicability. A large cohort or a meta-analysis is not automatically high-certainty evidence.**

Burt & Eklund (2021), ch. 10 and 12; Cochrane Handbook, ch. 14

# Choose a feasible design for the actual question

| Question | One suitable option |
|---|---|
| How common is untreated caries among 12-year-olds? | Representative cross-sectional examination |
| What preceded a rare oral cancer? | Case–control with suitable source-population controls |
| Did a recorded occupational exposure precede new disease? | Historical cohort with valid baseline records |
| Does new varnish outperform standard varnish? | RCT with an active comparator |
| Does supervised brushing help at school level? | Cluster RCT if feasible and ethical |
| Do regional policies relate to regional outcomes? | Ecological analysis with group-level interpretation |

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

CONTENTS

1. From question to design
2. Descriptive and cross-sectional studies
3. Analytic observational studies
4. Experimental studies
5. Errors, confounding and causal interpretation
6. **Exercises and recap**

# Exercises 1–4: identify the design and interpret the estimate

**1.** Examine 800 adults once and measure smoking and periodontitis. Name the design and a limitation of the time sequence.

**2.** 150 oral-cancer cases and 300 controls: 90 cases and 100 controls report heavy alcohol use. Calculate the unmatched OR.

**3.** Parents choose sealants. Follow 500 children with sealants and 500 without for 3 years; 25 and 100 develop new occlusal caries. Name the design, calculate RR and risk difference, and name a possible confounder.

**4.** Twenty schools are randomly allocated to supervised brushing or usual practice. Name the allocation unit and a required feature of the analysis.

Hypothetical teaching exercises

# Exercises 5–7: diagnose the interpretation error

**5.** A 2026 team uses a complete 2016 employee roster, baseline exposure records and outcomes recorded to 2026. Is this case–control because the records are old?

**6.** In the mouthrinse example the crude RR is 2.125; both age-specific RRs are 1. What explains the change?

**7.** Two population risks are 40% and 20%. A report states: "OR = 2.67, so the risk is 2.67 times higher." Correct it.

**Discuss:** A town comparison links more dentists with less tooth loss. What extra data would be needed to make an individual-level comparison?

# Answers: explain why, not just the design name

| No. | Answer |
|---|---|
| 1 | Analytic cross-sectional study; current smoking may not establish the pre-disease exposure. |
| 2 | Cells: 90, 100, 60, 200. OR = 90×200/(100×60) = **3.0**. |
| 3 | Prospective observational cohort; risks 5% vs 20%; **RR 0.25**; difference **–15 percentage points** over 3 years. Health behaviour, income or care access may confound. |
| 4 | Cluster RCT; school is the allocation unit; account for within-school correlation. |
| 5 | Historical cohort: sampling comes from a baseline roster, not outcome status. |
| 6 | Age confounding in this constructed example: users contain more older adults. |
| 7 | RR = 40/20 = **2**. OR = (0.4/0.6)/(0.2/0.8) = **2.67**. Odds are not risks. |

**For every answer state: assignment, sampling, time order, comparison and the most plausible threat to validity.**

Answers checked arithmetically

# Take home: the recruitment rule determines what can be inferred

## Recognise the design

**Cross-sectional**: one examination.
**Ecological**: group-level data.
**Cohort**: baseline group → outcomes.
**Case–control**: outcome-based sampling → earlier exposure.
**Trial**: investigator assigns intervention.

## Judge the comparison

Check time order, suitable controls and equal measurement.
Distinguish odds from risks.
Examine selection, information bias and confounding.
Randomisation helps; execution and missing data still matter.

**Name the design, justify the sampling logic and state what the result does — and does not — establish.**

Burt & Eklund (2021), ch. 12; CDC Field Epidemiology Manual

Thank you for your attention!