# Psychology 434 In-Class Test 1 Answers

**2026**

## Part 1: Short Questions

### 1. Framing a causal question

**Question.** Suppose an investigator asks, "Does using a mindfulness app reduce anxiety in university students?"

Rewrite this as a clearer causal question by stating:

a) the target population

b) the intervention

c) the comparison condition

d) the outcome

e) the time at which follow-up begins

**Answer.** A strong answer should make each part of the causal contrast explicit:

a) **Target population:** university students enrolled at the start of semester.

b) **Intervention:** beginning a specified mindfulness-app programme at time zero, such as using the app according to a stated schedule.

c) **Comparison condition:** not beginning that programme at time zero, or continuing usual behaviour without that app programme.

d) **Outcome:** anxiety measured at a specified follow-up time, for example at the end of semester.

e) **Time zero:** the start of semester, before app use begins and before follow-up starts.

One defensible causal question is: among university students enrolled at the start of semester, what is the effect on anxiety at the end of semester of beginning a specified mindfulness-app programme at time zero, compared with not beginning that programme at time zero?

A vague question such as "does mindfulness help?" is not enough for causal inference because it leaves the treatment, comparison, outcome timing, and population unclear. Where possible, investigators should randomise students to the app programme or comparison condition. If randomisation is not possible and observational data are available, the causal question should still be framed as the target trial we would like to emulate, with clear eligibility criteria, treatment strategies, time zero, follow-up, and outcome measurement.

### 2. Adjustment in a DAG

**Question.** Suppose the causal structure is:

$$A \leftarrow L \rightarrow Y, \qquad A \rightarrow M \rightarrow Y, \qquad A \rightarrow C \leftarrow U \rightarrow Y.$$

You want to estimate the total causal effect of $A$ on $Y$.

a) Which variable or variables should you condition on?

b) Which variable or variables should you not condition on?

Give a brief reason for each answer.

**Answer.** a) Condition on $L$. It is a common cause of $A$ and $Y$, so it opens the backdoor path $A \leftarrow L \rightarrow Y$ unless adjusted for.

b) Do not condition on $M$ when estimating the total effect, because $M$ is a mediator on the path $A \rightarrow M \rightarrow Y$. Conditioning on $M$ would block part of the effect of $A$. Do not condition on $C$, because $C$ is a collider on the path $A \rightarrow C \leftarrow U \rightarrow Y$. Conditioning on $C$ would open a non-causal path from $A$ to $Y$ through $U$.

### 3. Model fit and causal inference

**Question.** Explain why a regression model with a higher $R^2$ or lower AIC does not necessarily provide a less confounded estimate of a causal effect.

**Answer.** Model fit is a statistical property, not a causal property. A model can fit the observed data well while conditioning on a mediator or collider, omitting a confounder, adjusting for variables measured after treatment, or answering the wrong causal question. Higher $R^2$ or lower AIC can improve prediction while leaving the causal contrast biased.

### 4. Selection bias

**Question.** Briefly distinguish the following two ways in which selection bias can threaten a study:

a) selection as collider conditioning
b) selection through imbalance in effect modifiers between the analytic sample and the target population

For each, explain whether the main threat is to internal validity, external validity, or both.

**Answer. Selection as collider conditioning** occurs when inclusion in the analysed sample depends on treatment and outcome, or on variables causally linked to both. Conditioning on the selected sample opens a non-causal path. The main threat is to internal validity because even the sample-specific causal contrast may be biased. It may also create external-validity problems if the selected sample no longer represents the target population.

**Selection through effect-modifier imbalance** occurs when the analytic sample differs from the target population in the distribution of variables that modify treatment effects. In that case the study may estimate the sample effect correctly while estimating a different effect from the one in the target population. The main threat is to external validity, unless the selection process also creates collider bias or uncontrolled confounding.

### 5. Identification assumptions

**Question.** State the average treatment effect using potential-outcomes notation.

Then explain, in one or two sentences each, why the following assumptions are needed for identification:

a) consistency
b) exchangeability
c) positivity

**Answer.** For a binary treatment in the target population, the average treatment effect is

$$\mathrm{ATE} = \mathbb{E}[Y(1) - Y(0)].$$

a) **Consistency:** consistency links observed outcomes to potential outcomes. If a person receives treatment level $a$, the observed outcome is $Y(a)$. It requires a well-defined intervention so that $Y(1)$ and $Y(0)$ refer to coherent counterfactual outcomes.

b) **Exchangeability:** exchangeability allows observed outcomes in one exposure group to stand in for the missing counterfactual outcomes in the other group. In observational settings this usually means conditional exchangeability given measured pre-treatment covariates, with no unmeasured confounding.

c) **Positivity:** positivity ensures that both sides of the intervention contrast occur with non-zero probability in all relevant covariate strata. Without positivity, the statistical model must extrapolate beyond the observed data, for example by estimating what would happen under treatment among people like those for whom only the comparison condition is observed.

## Part 2: Longer Questions

### 6. Causal workflow

**Question.** An investigator wants to estimate the effect of beginning a structured gratitude-journaling programme at the start of semester on psychological distress at the end of semester in university students.

Set out a brief causal workflow for this study.

Your answer should:

a) state a causal estimand

b) describe the key identification assumptions

c) explain what should be measured before time zero

d) name one plausible estimation strategy

e) explain one way the design could fail if the intervention is left vague

**Answer. Estimand.** Among university students at the start of semester, what is the effect on end-of-semester psychological distress of beginning the structured gratitude-journaling programme at time zero versus not beginning it at time zero? A natural estimand is the average treatment effect,

$$\mathbb{E}[Y(1) - Y(0)].$$

**Identification assumptions.** The intervention must be well-defined, so consistency is plausible. Exchangeability requires that students beginning and not beginning the programme are comparable, at least after conditioning on measured pre-treatment covariates. Positivity requires that both treatment options occur within the covariate strata needed for comparison.

**Pre-time-zero measurement.** Before time zero, investigators should measure plausible common causes of programme uptake and later psychological distress, such as baseline distress, prior journaling habits, help-seeking, age, and study stress.

**Estimation strategy.** In a randomised design, a simple difference in mean outcomes or regression-adjusted contrast could estimate the effect. In an observational design, standardisation, inverse probability weighting, or regression adjustment would be defensible only if the identification assumptions remain credible.

**Vague intervention problem.** The design fails if "gratitude journaling" is left vague, because different treatment versions are bundled under one label. That makes consistency difficult to defend and makes the causal contrast unclear.

### 7. Cross-cultural study design

**Question.** A study compares the effect of a school-based social-belonging intervention on later academic self-efficacy across countries. Recruitment is school-based. Students with weaker attendance are less likely to remain in the study. The self-efficacy scale is validated mainly in one language family.

Discuss the main threats to causal inference in this study.

Your answer should consider:

a) selection bias

b) measurement bias

c) transportability

d) one concrete design or analysis response for each threat

**Answer. Selection bias.** Students with weaker attendance are less likely to remain under follow-up. If retention depends on later self-efficacy, the intervention, or variables affected by the intervention, the analysed sample may be a collider-selected sample.

**Measurement bias.** The self-efficacy scale is validated mainly in one language family. If language background affects how well the outcome is measured, the recorded outcome may not correspond to the same construct across countries.

**Transportability.** Treatment effects may differ across countries, language groups, school systems, or attendance patterns. School-based recruitment may also produce samples that do not represent the target student populations.

**Concrete responses.** For selection, measure predictors of attendance and retention, model loss to follow-up, and use inverse-probability-of-censoring weights or sensitivity analyses for attrition. For measurement, assess measurement invariance, improve translation and cultural adaptation, or use instruments validated in each language group. For transportability, state whether the target is a country-specific effect or a pooled effect across settings, measure plausible effect modifiers, and use standardisation or country-specific estimates when effect modifiers differ across settings.

### 8. CATE and effect modification

**Question.** A researcher studies the effect of a teaching intervention $A$ on exam performance $Y$. Age group $G$ is measured at baseline. After conditioning on baseline covariates $L$, the researcher notes that $G$ is d-separated from $Y$ in the DAG and concludes that age cannot modify the effect of $A$ on $Y$.

Explain why this conclusion does not settle the causal question of whether age modifies the effect of the intervention.

Your answer should:

a) state the relevant causal estimand

b) explain why d-separation does not by itself rule out effect modification

c) explain why $G$ might still matter even without a direct arrow from $G$ to $Y$ after conditioning

d) describe one better follow-up analysis or design strategy

**Answer. Estimand.** The relevant causal estimand is a subgroup causal contrast such as

$$\tau(g) = \mathbb{E}[Y(1) - Y(0) \mid G = g].$$

This is a conditional average treatment effect. It asks whether the effect of $A$ differs across baseline age groups.

**Why d-separation does not settle effect modification.** D-separation describes conditional independence in the observed-data graph. Effect modification concerns whether the causal contrast $Y(1) - Y(0)$ varies across groups. A DAG can show whether a set of variables blocks non-causal paths, but it does not by itself show that the treatment effect is constant across levels of $G$.

**Why $G$ might still matter.** Age groups can differ in the distribution of other baseline variables that modify the effect of $A$. For example, if the effect of $A$ varies across levels of $L$, and the distribution of $L$ differs across age groups, then $\tau(g)$ can differ across $G$ even when $G$ has no direct arrow to $Y$ after conditioning on $L$.

**Better follow-up.** Define the subgroup estimand explicitly and estimate subgroup-specific treatment effects with a design that still addresses confounding. Depending on the setting, that might mean standardised subgroup contrasts, stratified analyses with adequate sample size, or a flexible heterogeneity estimator once the identification assumptions are defended.