



RU RMT 22 — Research & Regulations

Lecture 05 — Sampling, Randomization, Blinding and Bias

Populations and samples · Random selection and allocation · Control groups and placebo · Blinding · The families of bias · Research ethics in practice

Course structure: Lectures 1–6 Research · Lectures 7–12 Regulation of Massage Therapy in Ontario · 8 instructional days · 56 contact hours · 7 contact hours per day Delivered in person at Ruhani College and online via Zoom in the same live session, in alignment with CMTCA Accreditation Standards (2024) and the FOMTRAC Inter-Jurisdictional Practice Competencies and Performance Indicators (2016) Research strand, Lecture 5 of 6 — how a study protects itself from itself



Where We Are in the Cycle

Recap

Lecture 3: the anatomy of an article and reading the methods first
Lecture 4: variables, operational definitions, hypotheses, designs, validity and reliability
You can now describe what a study did. This lecture asks whether you can believe it

Today

How a sample is drawn from a population, and what that costs
Why random selection and random allocation are different things
Control groups, placebo and the particular difficulty of sham massage
Blinding: single, double, triple, and what is achievable in manual therapy
The families of bias and how each distorts a result
The practical steps taken to reduce bias, and the ethical framework around all of it



Learning Objectives — Lecture 5

You will be able to

Distinguish a population from a sample and a sampling frame
Describe probability and non-probability sampling methods and their consequences
Explain why a sample must be randomly selected and randomly allocated
Distinguish the intervention group from the control group and describe the types of control
Define placebo and the placebo effect, and explain why placebos are difficult in massage research

You will also be able to

Describe single, double and triple blinding and identify what can be blinded in a massage trial
Define bias and research bias
Identify selection, channelling, performance, detection, attrition, reporting, recall, confirmation, funding and publication bias
List the basic steps taken to avoid bias
Apply a risk-of-bias judgement to a published study



Competency and Accreditation Linkage

FOMTRAC Practice Competencies (2016)

1.2 s.4 — Evaluate evidence
1.2 n.2 and n.4 — Identify all relevant information; defend rationale
1.2 o — Apply ethical considerations in decision making
1.2 v — Utilize self-reflection to identify personal strengths and areas for improvement
1.2 t — Maintain currency with developments in the profession

CMTCA Accreditation Standards (2024)

2.4 — Teaching methods and materials promote critical thinking and problem-solving skills
1.1 / 1.2 — Competencies taught and assessed to the designated level of complexity
Bias is the concept that connects the research strand to the regulatory strand: the same self-serving reasoning that distorts a study distorts a treatment decision.

PART 1

Populations and Samples

Almost every research conclusion is a statement about a group nobody studied, based on a group somebody did.

The three levels

Target population: everyone the conclusion is meant to apply to — for example, all Ontario adults with chronic low back pain
Accessible population: the part of it the researcher could actually reach — patients at three Toronto clinics
Sampling frame: the list from which the sample is drawn
Sample: the participants who were actually studied
Every step downward narrows who the findings apply to

Key ideas

Representativeness: does the sample resemble the population in the ways that matter?
Sample size: larger samples give more precise estimates, with narrower confidence intervals
A sample size calculation, done before recruitment, states how many participants are needed to detect a specified effect
An underpowered study can miss a real effect — and its non-significant result must not be read as evidence of no effect
Ask on every paper: how many, drawn from where, and who was left out?

Method	How it works	Consequence
Simple random	Every member of the frame has an equal chance of selection	Best representativeness; often impractical
Systematic	Every nth member of the frame is selected	Simple; fails if the list has a hidden periodic pattern
Stratified	The frame is divided into strata and sampled within each	Ensures subgroups are represented
Cluster	Whole groups (clinics, classes) are selected	Practical; participants within a cluster resemble each other
Convenience	Whoever is available and willing	Fast, cheap, and the most vulnerable to selection bias
Purposive	Deliberately chosen for a characteristic	Standard in qualitative research; not for estimating prevalence
Snowball	Participants recruit further participants	Reaches hidden populations; produces a networked, non-random sample

The first four are probability methods; the last three are non-probability methods. Most massage therapy research uses convenience sampling, which is a real and acknowledgeable limitation, not a disqualification.



Random Selection Is Not Random Allocation

Random selection

Concerns who gets into the study Every member of the population has a known, non-zero chance of being chosen It protects external validity — generalizability to the population It is rare in clinical research and almost unknown in massage therapy trials

Random allocation

Concerns which group a participant joins once they are in the study Assignment is by chance: computer-generated sequence, sealed opaque envelopes, or a central randomization service It protects internal validity by balancing known and unknown confounders between groups It is what makes a trial a randomized controlled trial A study can have random allocation without random selection — and most good trials do. The reverse is far less useful.



Allocation Concealment and Sequence Generation

Sequence generation: how the random order was produced. Acceptable methods include computer random number generators and random number tables. Unacceptable methods: alternation, allocation by date of birth, by hospital number, or by day of the week — these are quasi-random and predictable.

Allocation concealment: whether the person enrolling participants could know, in advance, which group the next participant would receive.

Concealment matters because a recruiter who knows the next allocation may consciously or unconsciously steer a sicker patient toward one group.

Adequate concealment: central randomization by telephone or web; sequentially numbered, opaque, sealed envelopes.

Concealment is distinct from blinding. Concealment happens before allocation; blinding happens after.

Block randomization keeps group sizes balanced; stratified randomization balances an important prognostic factor.

Empirical work in trial methodology shows that inadequate allocation concealment exaggerates treatment effects. This is one of the highest-value checks you can make on a trial.

PART 2

Control, Placebo and Blinding

What you compare against determines what your result means.



Why a Control Group Is Necessary

What happens without treatment

Natural history: most acute musculoskeletal conditions improve over time regardless of intervention
Regression to the mean: clients present when symptoms are at their worst, so the next measurement is likely to be better whatever you do
Hawthorne effect: being observed and attended to changes behaviour and reporting
Placebo response: expectation of benefit produces real, measurable change
Reporting effects: clients tend to report improvement to a therapist they like

What the control group tells you

It shows how much change would have occurred anyway
The treatment effect is the difference between the groups, not the change within the treated group
A before-and-after study without a control cannot separate the intervention from any of the five processes above
This is why a case series showing 80% improvement is not evidence of effectiveness. Eighty per cent might have improved regardless.



Types of Control Condition

Control	What participants receive	What the comparison shows
No treatment	Nothing	Natural history plus regression to the mean
Waitlist	Nothing, but with a promised later treatment	Similar; participants know they are waiting
Usual care	Whatever they would normally receive	Whether the intervention adds anything to current practice
Attention control	Equivalent therapist time without the active technique	Separates the technique from the therapeutic relationship
Sham / placebo	A credible but non-therapeutic procedure	Attempts to isolate the specific effect of the technique
Active comparator	A different real treatment	Which of two real options performs better

The choice of control is a value judgement about which question matters, and it should be justified in the methods section. For a client deciding what to book, an active comparator or usual-care comparison is usually the most informative.



Placebo and the Placebo Effect

Definitions

A placebo is an inert or sham intervention given in place of an active one. The placebo effect is a genuine, measurable change produced by expectation, context and meaning rather than by the specific intervention. It is real physiology, not imagination: it has measurable effects on pain, autonomic activity and reported function. The nocebo effect is its mirror: expectation of harm produces symptoms. Contextual factors that drive it include the practitioner's manner, the setting, the ritual of treatment and the client's expectation.

Why massage is a hard case

There is no agreed sham massage. Light touch is not inert — it produces effects of its own. Participants generally know whether they have been touched, and for how long. The therapeutic relationship is part of the intervention, not a nuisance to be eliminated. Some argue that in manual therapy the contextual effect should be measured rather than subtracted. Ethically and clinically, contextual effects are not something to exploit deceptively. The CMTO Code of Ethics requires that clients receive the information they need to make informed decisions.

Blinding

The levels

Single blind: the participant does not know their allocation
Double blind: neither the participant nor the person delivering the intervention knows
Triple blind: participant, provider and outcome assessor are all unaware; sometimes the data analyst as well
Open label: everybody knows — common and unavoidable in manual therapy
Terminology is used inconsistently in the literature; a well-reported paper says exactly who was blinded rather than using the label

What is achievable in massage research

The therapist can never be blinded — they know what their hands are doing
The participant may be partially blinded if the comparison is a credible sham and they are naïve to massage
The outcome assessor CAN and SHOULD be blinded — this is the single most feasible protection in a massage trial
The data analyst can be blinded to group labels
When a trial reports "assessor-blinded", that is a genuine methodological strength in this field, not a consolation prize.

PART 3

Bias

Bias is not dishonesty. It is systematic error — and it operates just as strongly in careful, well-intentioned people.

Bias is any systematic error that moves a result away from the truth in a consistent direction. It is different from random error, which scatters results in both directions and can be reduced by increasing the sample size. Increasing the sample size does not reduce bias — a large biased study is a precisely wrong study. Bias can be introduced at any stage: designing, recruiting, delivering, measuring, analysing, reporting and publishing. It is rarely deliberate. Most bias comes from ordinary human tendencies operating without a countermeasure. The defence against bias is procedural, not personal: you cannot resolve to be unbiased, you can only design procedures that do not depend on your being unbiased. This is also true clinically. You will not out-will your own expectations about a client. You will control them by measuring, charting and reassessing consistently.



The Families of Bias — Part 1

Selection and allocation

Selection bias: the sample differs systematically from the population — volunteers who expect benefit; only clients who could afford to attend
Channelling bias: clinicians steer particular kinds of patients toward particular treatments, so the groups differ before treatment begins
Allocation bias: group assignment is predictable, allowing conscious or unconscious steering
Berkson bias: studying only people who sought care, who differ from those who did not

Conduct and measurement

Performance bias: the groups are treated differently in ways beyond the intervention — more attention, longer appointments, more encouragement
Detection (assessment) bias: an unblinded assessor measures more generously in the group they expect to improve
Instrumentation bias: the measurement tool changes, drifts or is applied differently over time
Attrition bias: dropout is uneven between groups, so the groups compared at the end are not the groups randomized



The Families of Bias — Part 2

Reporting and analysis

Reporting bias: outcomes that reached significance are reported and those that did not are omitted
Selective outcome reporting: the primary outcome is quietly changed after the results are seen — detectable by comparing the paper with the trial registration
Data-dredging: many analyses are run and only the significant ones are presented
Funding bias: studies funded by a party with a commercial interest report favourable results more often

Human and systemic

Recall bias: participants with an outcome remember exposures differently — the core weakness of case-control designs
Confirmation bias: we notice, remember and weigh evidence that supports what we already believe
Observer expectancy: the researcher's expectation influences how they interact and what they record
Publication bias: positive results are submitted and published more readily than negative ones, so the published literature is more favourable than reality
Language and citation bias: English-language and frequently cited studies are found more easily, whether or not they are the best



Bias in the Treatment Room

How the same errors appear clinically

Confirmation bias: you find the tight muscle you expected to find
Selection bias in your own experience: you see the clients who return, not those who quietly stopped booking
Detection bias: you measure range of motion more generously after the technique you believe in
Recall bias: you remember your successes in far more detail than your failures
Anchoring: the first hypothesis you form about a presentation shapes everything you assess afterwards

What to do about it

Measure with a tool and record the number rather than the impression
Assess in the same position, with the same instruction, each time
Reassess after treatment and chart both values
Chart what did not change, not only what did
Review your own records periodically — this is the practice evaluation the Standard of Practice: Client-centred Care requires
Seek feedback and use self-reflection: FOMTRAC competency 1.2 v names both as entry-to-practice requirements.



Steps Taken to Reduce Bias

Step	What it protects against	When it happens
Prospective registration of the protocol	Selective outcome reporting, post-hoc hypotheses	Before recruitment
Pre-specified primary outcome and analysis plan	Data-dredging	Before recruitment
Sample size calculation	Underpowered studies and misread negative results	Before recruitment
Random sequence generation	Confounding by known and unknown factors	At allocation
Allocation concealment	Allocation and channelling bias	At allocation
Blinding of assessors and analysts	Detection bias	During and after data collection
Standardized protocol with adherence checks	Performance bias	During delivery
Intention-to-treat analysis	Attrition bias	At analysis
Full reporting including negative results	Reporting and publication bias	At publication
Declaration of funding and competing interests	Funding bias	At publication

Reporting guidelines

CONSORT — the checklist for reporting randomized controlled trials
PRISMA — for systematic reviews and meta-analyses
STROBE — for observational studies
CARE — for case reports; directly useful if you write one
COREQ — for qualitative research
SPIRIT — for trial protocols

Appraisal and risk-of-bias tools

Cochrane Risk of Bias 2 — assesses randomization, deviations from intended intervention, missing data, measurement and selective reporting
PEDro scale — a 0–10 methodological quality rating widely applied to physiotherapy and manual therapy trials
CASP checklists — plain-language critical appraisal tools by study type; a good starting point for students
Jadad scale — an older, simpler score based on randomization, blinding and dropout
You are not expected to memorize these. You are expected to know they exist, and to use one when appraising formally.



Research Ethics — Applied

What TCPS 2 requires

Respect for persons: free, informed, ongoing consent, and the right to withdraw without penalty
Concern for welfare: risks must be minimized and justified by the potential benefit
Justice: burdens and benefits distributed fairly; no group carries the risk while another receives the benefit
Research Ethics Board review before recruitment begins
Additional safeguards for participants who may be vulnerable

Ethical tensions in trial design

Withholding an effective treatment from a control group may be unethical — hence waitlist and usual-care designs
A sham that deceives participants requires careful justification and full debriefing
Recruiting your own clients creates a dual relationship and a power imbalance — the same problem the Standard of Practice: Professional Boundaries addresses clinically
Students recruited by their own instructors may not feel free to decline
The ethics of research and the ethics of practice are the same ethics: consent, welfare, fairness, and honesty about what you do not know.



Putting It Together — Judging a Trial

A practical sequence you can apply to any trial in about ten minutes.

Was the allocation genuinely random, and was the sequence concealed from the person enrolling participants? Were the groups similar at baseline? Check the baseline table. Apart from the intervention, were the groups treated equally? Who was blinded — participants, providers, assessors, analysts? Assessor blinding is the key one here. How many participants were lost to follow-up, from which group, and were they analysed by intention to treat? Was the primary outcome pre-specified, and does the paper report it prominently whether or not it was significant? Is the effect large enough to matter clinically, not merely statistically? Lecture 6 answers this. Who funded it, and were competing interests declared? If the answers to the first five are satisfactory, you are reading a trial worth taking seriously — even if it is small.

The task (35 minutes, pairs)

Using your own article, work through the eight questions on the previous slide. For each, record: answer, the page and line where you found it, and whether it raises low, unclear or high concern. Where the information is absent, record "not reported" — this is a finding, not a gap in your reading. Give the study an overall judgement of low, some concerns, or high risk of bias, and defend it in two sentences.

Debrief prompts

How many items were not reported at all? Which single design feature, if it had been present, would most have strengthened the study? Would you now describe this study to a client differently than you would have after reading only the abstract? Not reported is the commonest answer in this literature, and it is why reporting guidelines exist.



Randomization Methods in Detail

Methods

Simple randomization: each participant is allocated independently, like a coin toss. Can produce unequal group sizes in small trials
Block randomization: allocation occurs within blocks of fixed size, keeping group numbers close to equal throughout recruitment
Stratified randomization: separate sequences are run within levels of an important prognostic factor, such as symptom duration or age band
Minimization: each new participant is allocated to whichever group best balances the factors already recorded
Cluster randomization: whole clinics or classes are allocated rather than individuals

What can go wrong

Blocks of a fixed, known size become predictable near the end of each block; varying the block size prevents this
Stratifying on too many factors in a small trial defeats the purpose
Quasi-random methods — alternation, birth date, hospital number, day of the week — are predictable and are not randomization
Cluster trials need analysis that accounts for the clustering; ignoring it produces falsely narrow confidence intervals
A trial that says only "participants were randomized" and gives no method should be recorded as unclear risk of bias, not low.



Attrition — Dropout, Withdrawal and Loss to Follow-Up

The distinctions

Withdrawal: the participant actively chooses to leave the study
Loss to follow-up: the participant cannot be contacted for outcome measurement
Protocol deviation: the participant stays in but does not receive the intervention as intended
Exclusion from analysis: the researcher removes the participant from the analysis after randomization
Each is reported separately in a CONSORT flow diagram, with reasons

Why it threatens validity

Randomization balances the groups at the moment of allocation. Attrition can unbalance them again
Dropout is rarely random: people who are not improving, or who experience adverse effects, leave more often
Uneven dropout between groups is the more serious signal — it suggests the intervention itself drove the difference
Above roughly 20% attrition, most appraisal tools raise concern; the direction of the loss matters more than the raw figure
Read the reasons for dropout. "Adverse effect" and "moved away" are not equivalent findings.



Intention-to-Treat and Per-Protocol — Worked Example

A trial randomizes 100 participants to massage and 100 to usual care. Twenty in the massage group attend fewer than half the sessions.

Analysis	Who is included	What it estimates
Intention-to-treat	All 100 in each group, analysed in the group they were randomized to, whatever they actually received	The effect of being offered the treatment, under real-world adherence
Per-protocol	Only the 80 who adhered, compared with the 100 controls	The effect of the treatment when it is fully received
As-treated	Participants grouped by what they actually received, not what they were allocated	Neither cleanly; it discards the randomization entirely

Intention-to-treat is the conservative default and preserves the benefit of randomization; per-protocol tends to flatter the intervention. The 20 who did not adhere may well be those who were not improving — removing them removes the failures from one group only. A well-reported trial gives the intention-to-treat result as primary and may add per-protocol as a sensitivity analysis. If a paper reports only a per-protocol analysis, ask what happened to the participants who are missing.



Publication Bias and the Funnel Plot

The mechanism

Studies with positive, significant results are submitted, accepted and published more readily than negative ones. Negative results are often never written up at all — the file-drawer problem. Small studies are affected most: a small positive result gets published, a small negative one does not. The consequence is that the published literature overstates the true effect, sometimes substantially. Prospective trial registration exists partly so that unpublished trials can be identified.

How reviewers detect it

A funnel plot charts each study's effect estimate against its precision. With no publication bias the plot is a symmetrical inverted funnel: small studies scatter widely, large ones cluster near the centre. Asymmetry — a missing corner where small negative studies should be — suggests publication bias. Funnel plot asymmetry has other causes too, so it is a signal rather than a proof. Searching grey literature and trial registries is the practical defence, which is why good systematic reviews do both.



Sham and Control Conditions Tried in Massage Research

A survey of what the literature has actually used, and what each costs.

Light or superficial touch: credible to participants, but light touch has documented physiological effects of its own, so it is not inert
Detuned or inactive equipment, such as switched-off ultrasound: credible where equipment is expected, irrelevant to hands-on technique
Non-therapeutic contact to an unrelated body region: controls for touch and attention, but participants may notice the mismatch with their complaint
Attention control — equivalent therapist time in conversation: separates the technique from the relationship, but participants know they were not treated
Relaxation instruction or rest on the table: controls for position and time, not for touch
Waitlist: controls for time and natural history only; participants know they are waiting, and expectation effects run in the opposite direction
Usual care: the most clinically informative comparison, but "usual" differs between settings and must be described
There is no consensus sham for massage therapy, and this is a structural limitation of the field rather than a failing of any one study. Say so when you describe the evidence.



Adverse Events in Manual Therapy Research

What should be reported

A pre-stated definition of what counts as an adverse event
The method used to collect them — spontaneous report, systematic questioning at each visit, or diary
The number of events in each group, not only in the intervention group
Severity and whether they were judged related to the intervention
Any withdrawals caused by an adverse event

What usually happens instead

Many manual therapy trials do not mention adverse events at all
Silence is not evidence of safety; it is absence of data
Commonly reported minor events include transient soreness, fatigue and short-lived symptom increase
Serious events are rare and are therefore better captured by case reports and cohort studies than by small trials
Clinically, you have the parallel obligation: the Standard of Practice: Safety and Risk Management requires you to respond effectively to safety incidents, document them, and disclose them to relevant authorities.

A trial registry is a public database in which a study is recorded before recruitment begins. The main registries are ClinicalTrials.gov, the ISRCTN registry and the WHO International Clinical Trials Registry Platform. Registration fixes the primary outcome, the sample size and the analysis plan in advance, on a public and time-stamped record. This makes two common problems detectable: outcome switching, and hypotheses invented after the results. Most major journals now require prospective registration as a condition of publishing a trial. You can look up the registration number printed in the abstract and compare the registered primary outcome with the published one. Some journals also publish the full protocol as a separate paper or as supplementary material. Comparing a paper against its registration takes about three minutes and is one of the highest-value checks available to a reader.



Bias in Qualitative Research

The concepts are different

Qualitative research does not aim to eliminate the researcher's influence, because it cannot. Instead it requires reflexivity: the researcher states their position, assumptions and relationship to the participants. Rigour is judged by credibility, transferability, dependability and confirmability rather than by internal and external validity. An audit trail documents how the analysis moved from raw data to themes.

Techniques used

Member checking: returning findings to participants to test whether they recognize them. Independent coding by more than one researcher, with disagreements discussed. Triangulation: comparing findings across methods, sources or investigators. Deliberate search for disconfirming cases rather than only supportive quotations. Thick description, so that readers can judge transferability to their own setting. A qualitative paper that reports only quotations supporting the authors' thesis, with no reflexivity statement, has the same problem as a trial with selective outcome reporting.



Conflicts of Interest in This Profession

Where they arise in research

A technique's originator studies their own technique
A study funded by a manufacturer of tables, tools, creams or devices
A continuing-education provider publishing evidence for the course they sell
An author whose certification programme depends on the finding
None of these invalidates a study, and all of them should be declared

Where they arise in practice

The Standard of Practice: Conflict of Interest requires the RMT to avoid practising while in a conflict, and if it cannot be avoided, to manage and disclose it
Named examples in the standard: receiving or paying a referral fee; recommending a product in which you have an interest without disclosing it and noting the discussion in the health record; sharing fees with an unregulated associate without a written agreement; renting premises at a rent set by referral volume
The reasoning is identical in both settings: an undisclosed interest distorts a judgement that another person is relying on. Lecture 9 develops this fully.



A Bias Checklist You Can Carry

Ten questions, in reading order. Answer each low, unclear or high.

#	Question	Bias it addresses
1	How was the random sequence generated?	Selection bias
2	Was allocation concealed from the enroller?	Allocation and channelling bias
3	Were the groups comparable at baseline?	Confounding
4	Were participants blinded, and was the sham credible?	Performance bias
5	Were providers blinded, or is the trial necessarily open?	Performance bias
6	Was the outcome assessor blinded?	Detection bias
7	How much attrition, from which group, and for what reasons?	Attrition bias
8	Was the analysis intention-to-treat?	Attrition bias
9	Does the paper report every pre-registered outcome?	Reporting bias
10	Who funded it and what interests were declared?	Funding bias



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In-Class Activity 2 — Design Around the Bias

The task (25 minutes, small groups)

Take the hypothesis your group wrote in Lecture 4 Specify: the target population, the sampling method, and how you would recruit Specify: how you would generate the random sequence and conceal allocation Choose your control condition and justify the choice State exactly who would be blinded and who could not be Name the two biases your design still cannot eliminate, and say how you would report them honestly

Debrief prompts

Which control condition did most groups choose, and why? Did anyone find a workable sham? What did it cost in credibility? What would your study be able to claim at the end, and what would it not? Every design is a set of compromises. Being able to name yours is the mark of a competent appraiser.



Summary — Lecture 5

Sampling and allocation

Target population, accessible population, sampling frame and sample are four different things. Probability sampling supports generalization; convenience sampling is common and limiting. Random selection concerns who enters the study; random allocation concerns which group they join. Allocation concealment is distinct from blinding and independently affects results. A control group is what separates the treatment effect from natural history, regression to the mean and the placebo response.

Blinding and bias

Massage trials cannot blind the therapist, but they can and should blind the assessor. Placebo effects are genuine physiology, and in manual therapy they may deserve measurement rather than subtraction. Bias is systematic error; a larger sample does not fix it. Know the families: selection, channelling, performance, detection, attrition, reporting, recall, confirmation, funding and publication. Protection is procedural: registration, pre-specification, randomization, concealment, blinding, intention-to-treat and full reporting. The same biases operate in your own clinical reasoning, and the same discipline of measurement answers them.

How this lecture is assessed

Lecture 5 is examinable on the second course quiz and on the final examination. Expect items requiring you to name a bias from a described scenario, to distinguish random selection from random allocation, and to identify which parties could be blinded in a described mass trial. The bias taxonomy is the most heavily weighted single topic in the research strand. Approved assessment scheme: Quiz 1 20%, Quiz 2 20%, Assignment 1 10%, Assignment 2 10%, the Final Examination 30% and Professional Practice and Participation 10%. Pass: 70% overall.

Before Lecture 6

Bring your completed risk-of-bias appraisal. From your article's results section, copy out the primary outcome result exactly as reported, including any p-value, confidence interval or effect size. Note any statistical term in that sentence you cannot define — Lecture 6 works through them. Bring a calculator.



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