SYSTEMATIC REVIEW/ META ANALYSIS

# Microlearning in Undergraduate Medical Education: A Systematic Review and Meta-Analysis of Impact on Knowledge, Skills, and Retention

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## ABSTRACT

**Background:** Microlearning delivers content in short, focused units and is increasingly used in medical education, but consolidated quantitative evidence for undergraduate training remains limited. This systematic review and meta-analysis evaluated the impact of microlearning on knowledge, skills, and retention among undergraduate medical students.

**Methods:** A comprehensive literature search spanning PubMed, Scopus, Web of Science, and Google Scholar were conducted from database inception through January 2025. The inclusion criteria covered experimental research comparing microlearning methods against conventional instructional techniques among undergraduate medical students. Although the review protocol lacked prospective registration, study quality was appraised using RoB 2 and ROBINS-I tools. Hedges' g standardized mean differences (SMD) were aggregated via a restricted maximum likelihood (REML) random-effects model.

**Results:** A total of seven studies encompassing 1,674 learners comprising two randomized controlled trials and five quasi-experimental or non-randomized investigations met the selection criteria. Microlearning delivered a statistically significant enhancement in learner outcomes compared to traditional education (SMD = 0.90; 95% CI: 0.57-1.22; p <0.001), accompanied by substantial heterogeneity ($I^2$ = 82.0%). These pooled estimates remained stable across leave-one-out sensitivity evaluations and trim-and-fill procedures, which indicated zero imputed studies. Furthermore, publication bias assessments via Egger's (p = 0.76) and Begg's (p = 1.00) tests yielded non-significant outcomes. Based on GRADE criteria, the overall certainty of the evidence was rated as moderate.

**Conclusion:** Microlearning probably enhances knowledge acquisition, skill development, and retention in undergraduate medical education. Further well-designed randomized trials are needed.

**Keywords:** Microlearning, Education, Medical, Undergraduate, Digital learning, Knowledge retention, Systematic review, Meta-Analysis

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## INTRODUCTION

Medical education has undergone substantial transformation as the volume of biomedical information has continued to expand rapidly. In a widely cited projection, Densen P (2011)[1] estimated that the doubling time of medical knowledge, approximately 3.5 years in 2010, would fall to about 73 days by 2020. This rapid growth has intensified cognitive burden among undergraduate medical students, who must integrate increasingly complex content within limited curricular hours. The shift towards competency-based medical education has further emphasized the need for learning strategies that support efficient acquisition, reinforcement, and long-term retention of essential competencies.

Microlearning has emerged as a structured approach in which content is delivered in short, focused segments that target discrete learning outcomes.[2] Typical microlearning units last 5-15 minutes and may incorporate videos, infographics, micro-quizzes, case vignettes, or mobile-based learning objects. Microlearning is grounded in cognitive load theory and spaced repetition principles, both of which have been shown to support better encoding and retrieval of information.[3,4] These features make microlearning particularly relevant for medical education, where learners must process high-density content while managing clinical responsibilities.

Digital access among medical students has increased globally, creating favourable conditions for microlearning adoption. The World Health Organization documented widespread disruption of health services and health-workforce training during the COVID-19 pandemic,[5] and systematic reviews of the educational response have described a rapid, near-universal pivot of medical curricula to online and blended delivery during 2020,[6,7] accelerating the adoption of short digital learning modules. In India, Sagar A et al. (2025)[8] reported that 96% of surveyed MBBS students across four states owned a smart mobile device and 99% used it for study, and Singh S et al. (2025)[9] found, in a five-institution study of 608 undergraduate medical students, that more than 60% considered short video-based learning to improve their understanding, retention and self-directed learning. These findings highlight the readiness of students and institutions to integrate microlearning into routine teaching.

Microlearning has been discussed in the broader context of e-learning and digital pedagogy. A scoping review in health professions education reported increasing use of microlearning components within blended learning, mobile learning, and flipped-classroom models,[10] and reviews in broader educational and self-care contexts suggest potential benefits for knowledge acquisition, skills development, engagement, and learner satisfaction, although the quality of evidence is variable and outcomes are inconsistently measured.[11-13] Many microlearning interventions have been implemented as part of digital ecosystems, yet their specific contribution to learning outcomes remains unclear due to methodological heterogeneity.

Despite its increasing use, important gaps persist. Previous syntheses have been descriptive scoping or narrative reviews spanning multiple health professions;[10,11] none has provided a pooled quantitative estimate of microlearning's effect on knowledge, clinical skills, and retention specifically among undergraduate medical students. The available evidence is dispersed across multiple platforms, disciplines, and intervention formats, and variations in duration, modality, frequency, and assessment tools create uncertainty for curriculum planners. A systematic review and meta-analysis are therefore needed to generate precise pooled estimates of educational impact and to identify characteristics of effective microlearning interventions.

The present review aims to synthesise the available evidence on microlearning in undergraduate medical education, evaluate its effects on immediate learning and retention, and determine whether microlearning offers measurable advantages compared with traditional or blended teaching formats. The findings are intended to guide medical educators, policymakers, and curriculum developers in making evidence-based decisions for integrating microlearning within competency-based education frameworks.

## METHODOLOGY

**Study Design and Protocol Registration:** This systematic review and meta-analysis were conducted according to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA 2020) guidelines.[14] The completed PRISMA 2020 checklist is provided in Appendix S2. The review focused exclusively on experimental research evaluating the effects of microlearning interventions on undergraduate medical students. An internal protocol outlining the population, intervention, comparator, outcomes, and study design criteria was established a priori prior to beginning the search and screening process. Although the protocol was not registered in PROSPERO or any other prospective registry, registration was omitted because the review commenced under an institutional academic schedule that preceded the team's formal registration workflow. We acknowledge this as a limitation, and all protocol elements, including the a priori eligibility criteria and planned analyses, are reported transparently in this manuscript and its supplementary material.

**Eligibility Criteria:** Eligibility was determined using the PICOS framework. Undergraduate medical students enrolled in MBBS or equivalent degree programmes formed the target population. The intervention of interest was microlearning delivered through short, structured instructional units lasting 15 minutes or less. These units included videos, pod-

casts, micro-quizzes, mobile modules, digital cards, and infographics designed to convey discrete learning objectives. The comparator groups consisted of traditional lecture-based teaching, standard e-learning modules, or no-intervention controls. Eligible studies assessed outcomes related to knowledge acquisition, clinical skill performance, and retention of learned material. Additional outcomes included learner satisfaction and engagement metrics when available. Only experimental study designs were included, covering randomized controlled trials, quasi-experimental trials, and controlled pre-post intervention studies. Research involving postgraduate trainees or non-medical health science students was excluded, lacked a comparator group, or presented microlearning as part of a multifaceted intervention whose isolated effect could not be assessed. Conference abstracts, non-English publications, qualitative studies, and observational non-interventional designs were also excluded.

**Information Sources:** A comprehensive search was conducted across major scientific databases PubMed, Scopus, Web of Science, and Google Scholar covering records from each database's inception through January 2025. Supplementary searches were also carried out on ClinicalTrials.gov to locate ongoing or unpublished trials. The search strategies were structured to guarantee sensitivity and reproducibility, with specific adaptations made for each database interface. Detailed, database-specific search strings are available in the supplementary material (Appendix S1).

**Search Strategy:** The search strategy integrated Medical Subject Headings (MeSH), targeted keywords, and Boolean operators. Core search terms encompassed "microlearning," "micro-learning," "bite-sized learning," "digital microlearning," "spaced learning," and "medical students." These were paired with methodological filters related to randomized trials, controlled interventions, and pre-post studies. Searches in Scopus and Web of Science were tailored using equivalent controlled vocabulary and Boolean syntax to optimise retrieval of experimental evidence relevant to undergraduate medical education. The first 200 results from Google Scholar, sorted by relevance, were screened to avoid omission of grey literature.

**Study Selection:** All identified records were imported into Rayyan to facilitate systematic screening and the removal of duplicates. Two independent reviewers screened titles and abstracts against the predefined eligibility criteria, after which the full texts of potentially relevant studies were retrieved and examined in detail. Disagreements were resolved through discussion, with a third reviewer consulted when necessary. Inter-rater agreement for title and abstract screening was almost perfect (Cohen's kappa = 0.82; 95% CI: 0.74-0.90, according to the Landis and Koch classification). The complete study selection process is illustrated in the PRISMA flow diagram (Figure 1).

**Data Extraction:** Data extraction was carried out using a structured template developed specifically for this review. Extracted information captured study characteristics, participant demographics, sample sizes, intervention types and durations, comparator approaches, assessment tools, and measured outcomes. Numerical data necessary for meta-analysis such as means, standard deviations, and sample sizes for each outcome were systematically recorded. When outcome data were incomplete, corresponding authors were contacted to secure missing values.

Data extraction was performed by one reviewer and independently cross-verified in full by a second reviewer against the original source publications (achieving a raw inter-reviewer agreement of 95.0% across extracted numerical fields). Discrepancies were reconciled via joint re-inspection of the source papers, with adjudication by a third reviewer where required. All discrepancies were resolved against the source publications, and the final extraction dataset is available as supplementary material.

**Risk of Bias Assessment:** The risk of bias was appraised independently by two reviewers. The RoB 2.0 tool was utilized for randomized controlled trials, evaluating domains concerning randomization, deviations from intended interventions, missing outcome data, outcome measurement, and selective reporting. Quasi-experimental and controlled pre-post studies were evaluated using the ROBINS-I tool. Each study was categorized as having low, moderate, serious, or critical risk of bias based on predefined criteria, with any discrepancies resolved through consensus.

**Effect Measures:** The primary effect measure for continuous outcomes was the standardized mean difference calculated using Hedges' g. This metric enabled the pooling of results across studies that utilized varying assessment tools for knowledge tests, skill-based evaluations, and retention scores. All effect sizes are presented alongside 95% confidence intervals. For studies employing pre-post designs, change-score standardized mean differences were utilized when sufficient data were available.

**Data Synthesis:** A random-effects model utilizing the restricted maximum likelihood (REML) estimator for between-study variance was pre-specified as the primary analysis, driven by anticipated clinical and methodological heterogeneity across microlearning formats, delivery platforms, course contents, and evaluation methods. Heterogeneity was quantified using the $I^2$ statistic and interpreted against conventional thresholds.

Exploratory subgroup analyses were conducted by study design (randomized versus non-randomized) and outcome domain (knowledge, skills, and retention). Although subgroup analyses by intervention modality and duration were originally planned, they could not be performed because no stratum contained more than two studies a transparently reported deviation necessitated by the small evidence base. Sensitivity analyses were performed by excluding

the retrospective study, applying fixed-effect and DerSimonian-Laird models for comparison, and executing leave-one-out analyses to evaluate the influence of individual studies (including retrospective research) on the pooled effect size.

**Publication Bias:** Publication bias was examined through the visual inspection of funnel plots. Because fewer than ten studies were available, funnel-plot-based methods possess limited power per Cochrane guidance; consequently, Egger's regression and Begg's rank correlation tests were pre-specified as exploratory analyses and interpreted with caution. The trim-and-fill method was also applied to estimate an adjusted effect size regardless of test significance.

**Certainty of Evidence:** The certainty of evidence for each primary outcome was evaluated using the GRADE approach. Key factors including risk of bias, inconsistency of results, indirectness of evidence, imprecision of estimates, and potential publication bias were systematically assessed, classifying outcomes as high, moderate, low, or very low certainty based on these criteria.

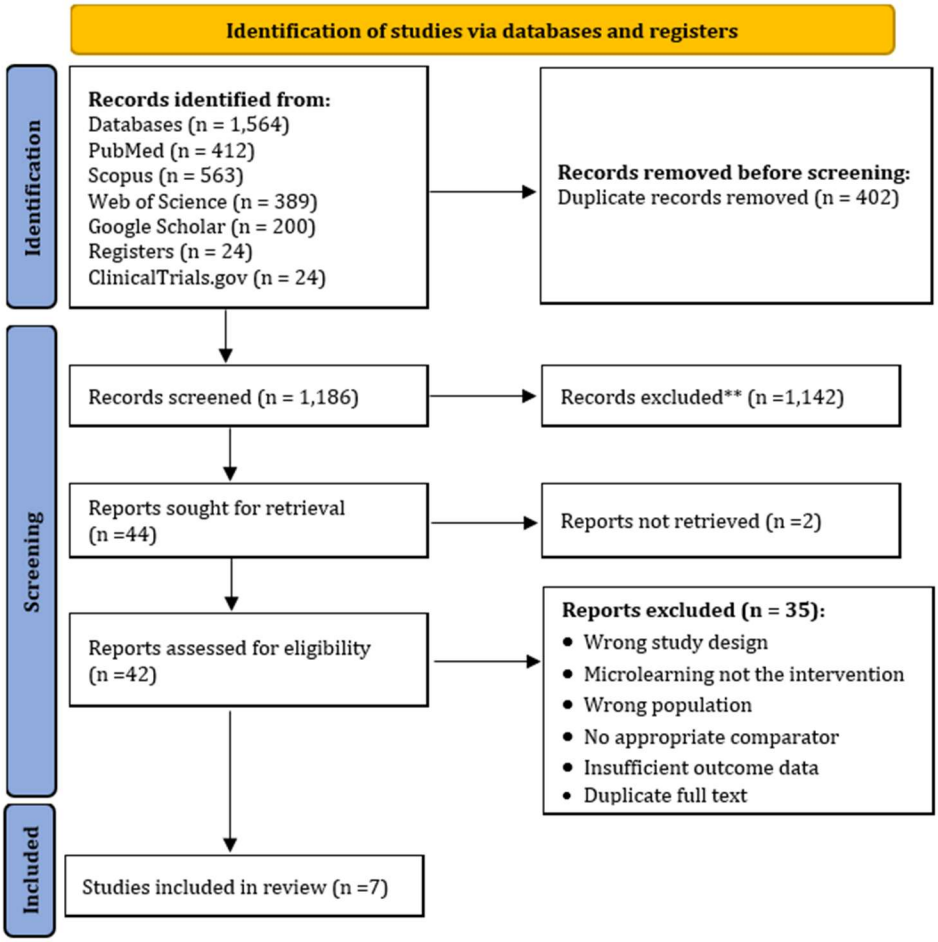


**Figure 1: PRISMA 2020 flow diagram of the study selection process**

## RESULTS

The searches identified 1,588 records (PubMed 412; Scopus 563; Web of Science 389; Google Scholar 200; ClinicalTrials.gov 24). After removal of 402 duplicates, 1,186 records were screened and 44 reports were sought for retrieval, of which 42 were assessed in full text. Thirty-five reports were excluded with reasons (Figure 1; full list with reasons in Appendix S3). Seven studies were therefore included in both the qualitative and quantitative syntheses.

The seven included studies,[15-21] conducted between 2018 and 2025 in India, Syria, Germany, South Africa, Iran, and Bulgaria, are summarized in Table 1. Designs comprised two randomized controlled trials[17,21] and five non-randomized studies (a retrospective comparative study, a nonrandomized clinical trial, two quasi-experimental studies, and a crossover trial).[15,16,18-20] Analysed samples ranged from 39 to 792 learners (total n = 1,674). Interventions were predominantly digital learning-management-system modules, Instagram reels, micro-videos within simulation, WhatsApp videos, SMS questions, and mobile micro-lectures delivered from a single 15-minute session to semester-long programmes; comparators were most often traditional lectures. Outcomes covered knowledge (MCQ or written examinations), skills (OSCE, DOPS), and retention (delayed testing); further details are presented in Table 1 and are not repeated in the text.

**Table 1: Characteristics of Included Studies**

| Study | Design & setting | Participants | Interventions | Comparators | Duration / Frequency | Outcomes Measured |
|---|---|---|---|---|---|---|
| Chaudhuri A, 2025 (India)[15] | Retrospective comparative study across six MBBS batches | First-professional MBBS students; Intervention: 396; Control: 396 (Total: 792) | Microlearning modules (5-10 min) on cardiovascular, neurophysiology, nerve-muscle physiology; delivered via LMS; blended with traditional teaching | Traditional teaching based on NMC curriculum | Six consecutive cohorts (2019-2024); modules accessed on demand | Formative and summative scores (%), pass rates, effect size (Cohen's d) |
| Alsaid B, 2025 (Syria)[16] | Nonrandomized clinical trial | Third-year medical students; Intervention: 84; Control: 83 (Total: 167) | 50 Instagram microlearning reels (90 s each), uploaded daily | Traditional study without videos | Two-month intervention | Pre/post neuroanatomy knowledge, final exam scores, perceptions |
| Gross B, 2019 (Germany)[17] | Prospective randomized controlled trial | Medical students with prior patient exposure; 129 recruited, 120 analysed (Intervention: 63; Control: 57)*; 2-week retention outcome analysed in 39 intervention and 43 control respondents | Microlearning: 5-min CRM instructional video + simulation + debrief | Traditional 5-min videotaped lecture + simulation + debrief | Single structured 15-min session | Team behaviour, immediate knowledge, 2-week retention |
| Enoch LC, 2022 (South Africa)[18] | Non-random quasi-experimental cross-sectional study | Third-year medical students; Intervention: 101 (A1) + 138 (A2); Control: 249 | Asynchronous micro-units (short videos, quizzes) + blended online support | Traditional face-to-face curriculum | One semester (August-November 2021) | OSCE psychomotor, cognitive, affective scores; retention; transfer of learning (reported as medians with interquartile ranges and analysed non-parametrically) |
| Sedaghatkar F, 2023 (Iran)[19] | Quasi-experimental three-group interventional study | Final-year medical students; ML+TBL: 19; TBL only: 20; Routine teaching: 20 | Microlearning videos (7-10 min) on ENT clinical tasks via WhatsApp | Routine teaching; Task-Based Learning alone (the TBL-only arm was used as the comparator in the meta-analysis) | Two-week clerkship rotation | Knowledge (MCQ pre/post); performance (DOPS) |
| Sichani MM, 2018 (Iran)[20] | Semi-experimental crossover intervention | Ninth-semester medical students (n=47) | SMS microlearning: 1 educational micro-question/day | Lecture-based method | One week per method | Pretest, post-test, delayed test, satisfaction |
| Molchovski P, 2024 (Bulgaria)[21] | Controlled educational trial with random assignment to microlearning or traditional teaching | Undergraduate medical students; Intervention: 75; Control: 75 | Mobile micro-lectures (2-5 min each) using Moodle app, push notifications | Traditional lectures + practical exercises | 14-week course | Knowledge retention; mid-semester and final MCQ scores |

Note: * Of 129 recruited participants, 120 were analysed (63 intervention, 57 control); the 2-week retention outcome used in the meta-analysis was assessed in 39 intervention and 43 control respondents (see Table 2). Abbreviations: CRM = crew resource management; DOPS = Direct Observation of Procedural Skills; ENT = ear, nose and throat; LMS = learning management system; MBBS = Bachelor of Medicine, Bachelor of Surgery; MCQ = multiple-choice question; ML = microlearning; NMC = National Medical Commission (India); OSCE = Objective Structured Clinical Examination; SMS = short message service; TBL = task-based learning.

**Table 2: Standardized Outcome Data from Included Studies for Meta-analysis**

| Study (Author, Year) | Intervention Group | | Control Group | | Effect Size |
|---|---|---|---|---|---|
| | N | Mean ± SD | N | Mean ± SD | (SMD,Hedges' g) |
| Chaudhuri A (2025)[15] | 396 | 72.4 ± 8.6 | 396 | 64.8 ± 9.2 | 0.85 |
| Alsaid B (2025)[16] | 84 | 9.9 ± 2.1 | 83 | 7.5 ± 2.4 | 1.06 |
| Gross B (2019)[17] | 39* | 1.87 ± 1.24 | 43* | 0.77 ± 0.92 | 1.01 |
| Enoch LC (2022)[18] | 101 | 90.0† ± 10.8‡ | 249 | 84.0† ± 10.1‡ | 0.58 |
| Sedaghatkar F (2023)[19] | 19 | 11.00 ± 1.73 | 20 | 10.15§ ± 1.38 | 0.53 |
| Sichani MM (2018)[20] | 47 | 16.7 ± 3.23 | 47 | 15.0 ± 4.15 | 0.45 |
| Molchovski P (2024)[21] | 75 | 8.28 ± 0.99‡ | 75 | 6.70 ± 0.80‡ | 1.75 |

Footnote: All SMDs are Hedges' g calculated from the tabulated means, SDs and Ns; the analysis dataset and output log are provided as supplementary material (Appendix S4). * Ns for Gross B et al. (2019) are the respondents to the 2-week retention assessment (39 intervention, 43 control) of the 120 analysed participants. † Enoch LC et al. (2022) report medians with interquartile ranges rather than means with standard deviations, and analysed the data non-parametrically; the tabulated values are the reported medians, expressed as a percentage of the maximum OSCE score, with standard deviations imputed from the reported dispersion; a sensitivity analysis excluding this study is reported in the Results. ‡ SD not reported directly in the source publication and derived from the reported summary statistics; for Molchovski P et al. (2024) the source reports group means only, and SDs were back-calculated from the reported test statistics. § The comparator used for Sedaghatkar F et al. (2023) is the task-based-learning-only control arm (10.15 ± 1.38, n = 20); the routine-teaching arm of that three-group study scored 10.45 ± 1.46 (n = 20). SD = standard deviation; SMD = standardized mean difference; N (I)/N (C) = analysed sample size in intervention/control groups.

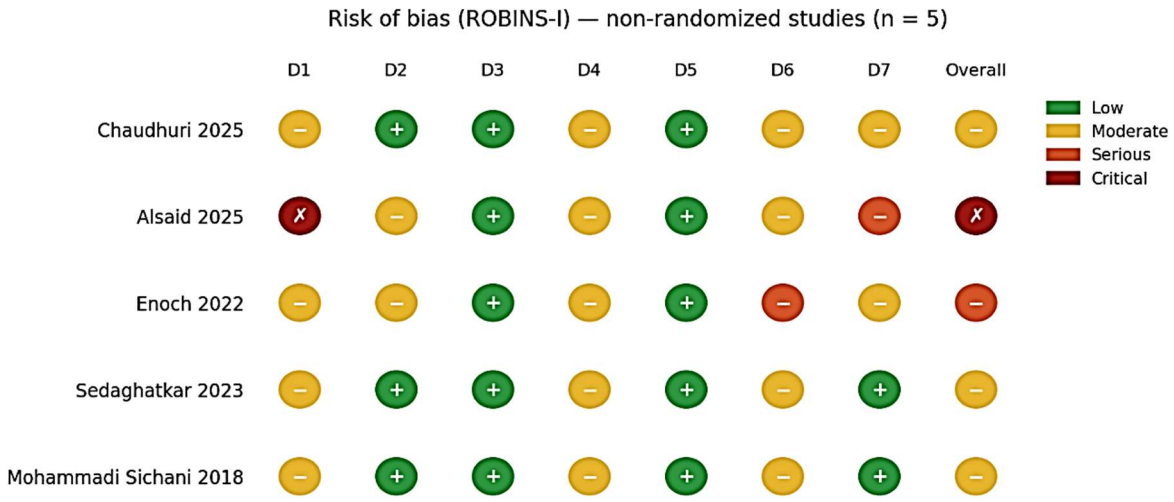


**Figure 2: Risk of Bias Assessment Using ROBINS-I for Included Non-Randomized Studies**

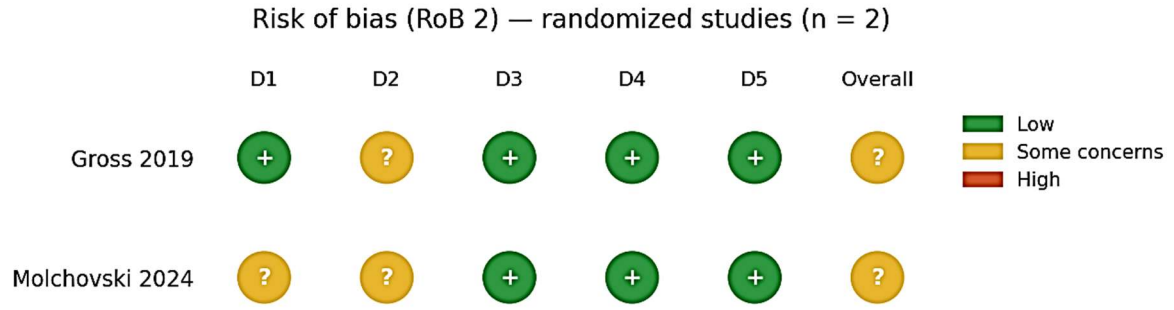


**Figure 3: Risk of Bias Assessment Using RoB 2 for Randomized Studies**

Figure 2 shows the distribution of risk of bias across the seven ROBINS-I domains for the five non-randomized studies. Most studies demonstrated a moderate risk of bias, particularly in domains related to confounding (D1), deviations from intended interventions (D4), and outcome measurement (D6). These patterns are expected in educational research, where allocation is frequently non-random and learning exposure varies between participants.

One study, Alsaid B et al. (2025)[16], showed a critical risk of bias due to confounding and selective reporting. This resulted from voluntary participation, unequal access to social-media content, and limited protocol transparency. Another study, Enoch LC et al.

(2022)[18], showed a serious risk in outcome measurement, which reflected the subjective nature of OSCE-based clinical skill scoring.

Despite these issues, the majority of non-randomized studies were rated as moderate overall, indicating that their findings should be interpreted cautiously but remain valuable for synthesis.

Figure 3 presents the RoB 2 assessment for the two randomized/controlled trials, Gross B et al. (2019)[17] and Molchovski P et al. (2024),[21] both judged to have some concerns overall. The main source of concern was deviations from intended interventions (D2), which is common in educational trials where participant blinding is not feasible.

Bias arising from the randomization process (D1) was judged low for Gross B et al. (2019)[17], while Molchovski P et al. (2024)[21] showed some concerns due to insufficient reporting of the allocation procedure. Outcome measurement (D4) and missing data (D3) were consistently rated as low risk, reflecting the objective nature of assessment scores and minimal attrition. Taken together, no study in either assessment achieved a low overall risk of bias; the evidence base was judged methodologically acceptable for synthesis but warrants cautious interpretation, underscoring the need for more rigorously designed trials in the microlearning literature.

The primary random-effects meta-analysis (REML estimator) of the seven included studies demonstrated a large, statistically significant benefit of microlearning over traditional teaching (SMD = 0.90; 95% CI: 0.57-1.22; z = 5.43; p < 0.001) (Figure 4).

Effects favoured microlearning in all seven studies, with largest effects in Molchovski P et al. (2024)[21] (g = 1.75; 95% CI: 1.37 to 2.12) and Alsaid B et al. (2025)[16] (g = 1.06; 95% CI: 0.74 to 1.38) and the smallest in Sichani MM et al. (2018)[20] (g = 0.45; 95% CI: 0.04 to 0.86); only Sedaghatkar F et al. (2023)[19] (g = 0.53; 95% CI: −0.11 to 1.17) had a confidence interval crossing zero.

Between-study heterogeneity was substantial ($Tau^2$ = 0.155; Q = 33.39, df = 6, p < 0.0001; $I^2$ = 82.0%), reflecting variability in intervention format, delivery platform, duration, learner level, and outcome assessment.

In exploratory subgroup analyses, the pooled effect was larger in the two randomized trials (SMD = 1.39; 95% CI: 0.66-2.11; $I^2$ = 83.3%) than in the five non-randomized studies (SMD = 0.74; 95% CI: 0.52-0.95; $I^2$ = 58.3%). By outcome domain, pooled effects were SMD = 0.79 (95% CI: 0.53-1.04; $I^2$ = 50.8%; k = 4) for knowledge, SMD = 0.58 (95% CI: 0.35-0.82; k = 1) for skills, and SMD = 1.39 (95% CI: 0.66-2.11; k = 2) for retention. These strata are small and the domain-specific estimates should be regarded as hypothesis-generating; they nonetheless suggest that pooling across outcome constructs partly explains the observed heterogeneity.

Visual inspection of the funnel plot (Figure 5) showed approximate symmetry with mild right-sided concentration of positive effects.

However, because the number of included studies is relatively small (n = 7), the ability to reliably detect funnel plot asymmetry is limited. Cochrane guidance notes that funnel plot interpretation is less dependable when fewer than 10 studies are included. Despite the slight imbalance, the distribution of points still approximates an inverted funnel shape, and there is no strong evidence of substantial publication bias.

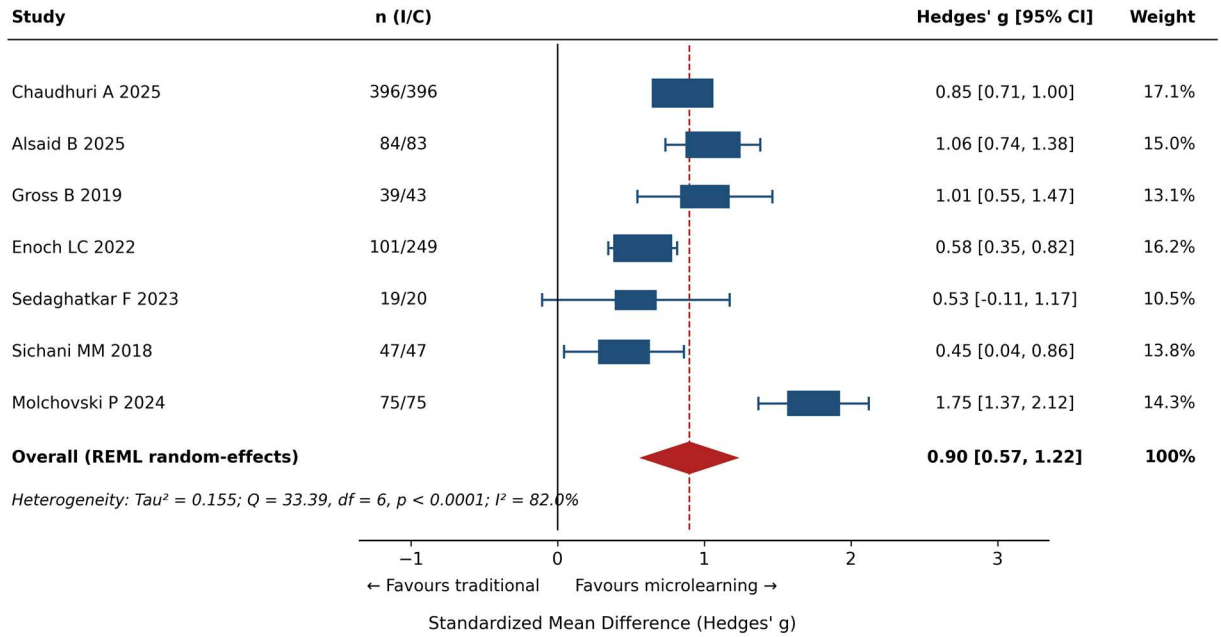


**Figure 4: Standardized Mean Differences for Microlearning versus Traditional Learning Across Included Studies**

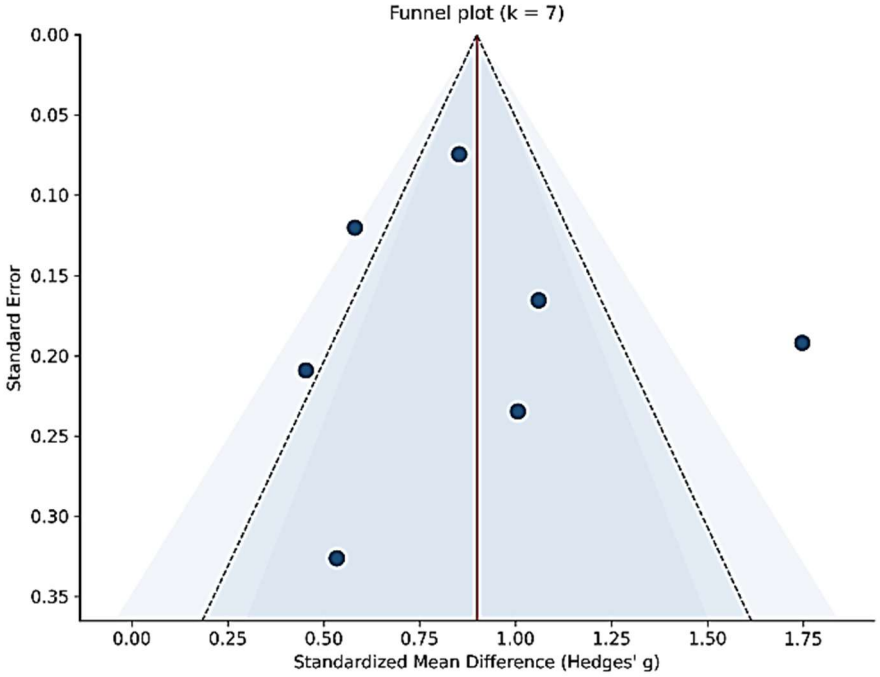


**Figure 5: Funnel Plot Assessing Publication Bias for Studies Comparing Microlearning and Traditional Learning**

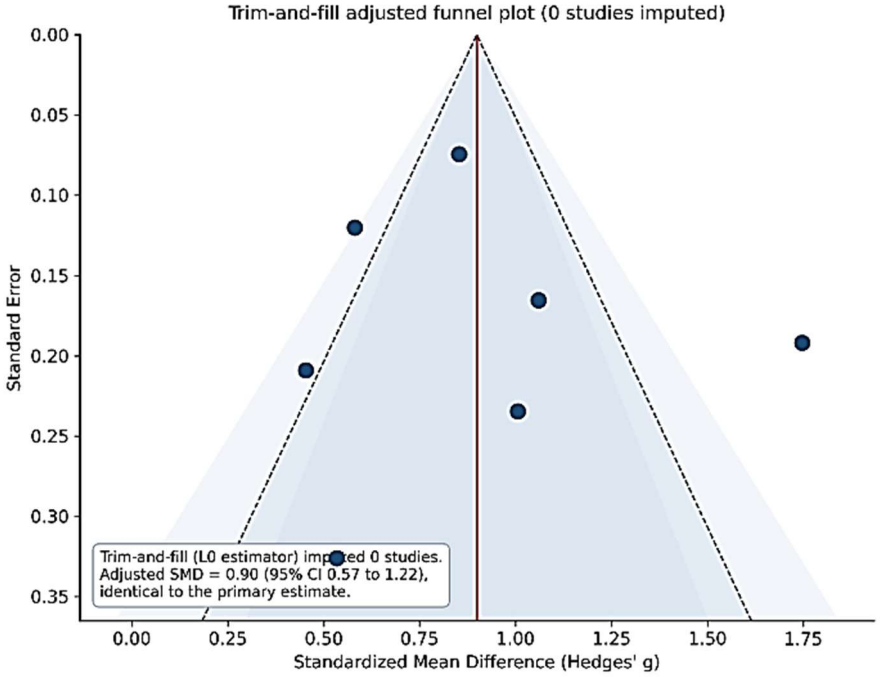


**Figure 6: Funnel Plot with Trim-and-Fill Adjustment for Publication Bias**

**Table 3: Summary of Publication Bias Tests (Egger and Begg)**

| Test | Statistic(s) | p-value | Interpretation |
|---|---|---|---|
| Egger's Regression Test | Intercept b = 0.68 (95% CI: -4.86 - 6.22) | 0.764 | No funnel plot asymmetry detected |
| Begg's Rank Correlation Test | Kendall's τ = 0.048 | 1.000 | No evidence of publication bias |
| Trim-and-fill (REML) | 0 studies imputed; adjusted SMD = 0.90 (95% CI: 0.57 - 1.22) | - | Pooled estimate unchanged after adjustment |

Footnote: Tests conducted on the seven included studies; exploratory given k < 10 (limited power). CI = confidence interval; df = degrees of freedom; REML = restricted maximum likelihood; SMD = standardized mean difference.

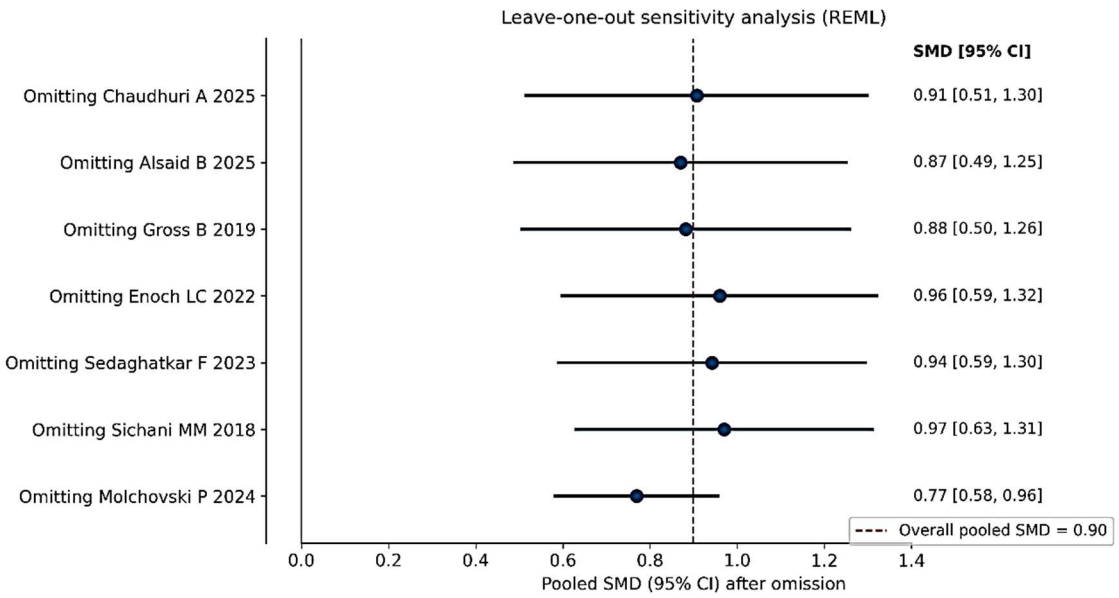


**Figure 7: Leave-One-Out Sensitivity Analysis of the Pooled Standardized Mean Difference**

Formal exploratory statistics are reported in Table 3; based on graphical and statistical assessment together, there is no strong evidence of substantial publication bias, interpreted with caution given the limited number of studies.

With the seven-study dataset, the trim-and-fill procedure imputed no studies, and the adjusted pooled estimate was therefore identical to the primary estimate (SMD = 0.90; 95% CI: 0.57-1.22), indicating that the meta-analytic findings are robust and minimally influenced by missing or unpublished studies (Fig 6).

As summarized in Table 3, both Egger's regression and Begg's rank correlation were non-significant; given their concordance with the largely symmetrical funnel plot, publication bias is unlikely to have meaningfully influenced the pooled estimate, although these tests are underpowered with only 7 studies.

Figure 7 illustrates the results of the leave-one-out sensitivity analysis conducted using a random-effects model. Each point represents the recalculated pooled standardized mean difference (SMD) after the sequential exclusion of one individual study. Pooled estimates ranged from 0.77 (omitting Molchovski P et al. 2024[21], which also reduced $I^2$ to 52.2%) to 0.97 and remained statistically significant throughout, indicating that no single study unduly influenced the overall results.

The pooled estimate was also robust to model specification: the fixed-effect model yielded SMD = 0.86 (95% CI: 0.76-0.97) and the DerSimonian-Laird random-effects model yielded SMD = 0.90 (95% CI: 0.62-1.18). Excluding the retrospective study (Chaudhuri A et al. 2025)[15] yielded SMD = 0.91 (95% CI: 0.51-1.30), indicating that this study did not materially influence the pooled estimate. A further sensitivity analysis excluding Enoch LC et al. (2022)[18] the only included study reporting medians with interquartile ranges rather than means with standard deviations yielded SMD = 0.96 (95% CI: 0.59 to 1.32), again without materially altering the pooled estimate or the direction of effect.

Despite the high heterogeneity, the direction of effect consistently favoured microlearning across studies, and the pooled confidence interval did not cross the null value. These findings suggest that microlearning is associated with meaningful improvements in learner outcomes, although the magnitude of benefit varies depending on study design, intervention format, duration, learner level, and outcome assessment methods.

Given the substantial heterogeneity and mixed methodological quality of the included studies, the results should be interpreted with caution. Accordingly, the certainty of evidence was rated as moderate, indicating that microlearning probably improves knowledge acquisition, skills, and retention in medical education, but further well-designed randomized trials are required to refine effect estimates and reduce unexplained variability.

Although several included studies exhibited a moderate risk of bias and substantial heterogeneity was observed, the effect size remained large, directionally consistent, and robust across models, with no significant evidence of publication bias. The certainty of the evidence was rated as **moderate**: the rating was downgraded by one level due to serious risk of bias (reflecting a mix of randomized and non-randomized designs with no studies at a low overall risk) and by one level for serious inconsistency ($I^2$ = 82.0%). No downgrades were applied for indirectness, imprecision, or publication bias, and the rating was upgraded by one level to account for the large, consistent effect size. Consequently, microlearning probably improves learner performance compared to conventional teaching methods.

**Table 4: GRADE Summary of Findings: Microlearning vs Traditional Learning**

| GRADE Domain | Assessment / Findings |
|---|---|
| Outcome | Learner performance (knowledge, skills, retention) |
| Study Design | 7 studies (2 RCTs, 5 quasi-experimental/non-randomized) |
| Risk of Bias | *Serious* concerns (downgraded by 1 level due to mixed ROB2/ROBINS-I ratings; some studies Moderate/Critical) |
| Inconsistency | Serious (downgraded 1 level; $I^2$ = 82.0%) |
| Indirectness | Not serious |
| Imprecision | Not serious (large effect, narrow CIs) |
| Publication Bias | Not serious (Egger p = 0.76; Begg p = 1.00; trim-and-fill imputed no studies) |
| Effect (SMD) | SMD = 0.90 (95% CI: 0.57 to 1.22) |
| Certainty of Evidence | MODERATE certainty ⊕⊕⊕⊖ |

Footnote: GRADE certainty symbols: ⊕⊕⊕⊕ = high; ⊕⊕⊕⊖ = moderate; ⊕⊕⊖⊖ = low; ⊕⊖⊖⊖ = very low. The rating was downgraded one level for serious risk of bias and one level for serious inconsistency, and rated up one level for the large, consistent effect, yielding MODERATE certainty. CI = confidence interval; RCT = randomized controlled trial; SMD = standardized mean difference.

# DISCUSSION

The findings of this review indicate that microlearning is associated with meaningful enhancements in knowledge acquisition, retention, and clinical skill performance among undergraduate medical students a conclusion that aligns directionally with the broader health professions education literature. The large pooled effect observed in this meta-analysis reinforces the conceptualization of microlearning as a high-yield instructional strategy. However, the moderate certainty of the evidence and substantial heterogeneity necessitate that these estimates be interpreted as probable rather than definitive effects.

Within the included dataset, the randomized trial by Gross B et al. (2019)[17] found that a 5-minute crew-resource-management micro-video improved observed team behaviour and recall of training content immediately and at two weeks compared with a lecture-format video, consistent with the retention benefit observed in our pooled analysis. Beyond the included studies, a scoping review by Taylor AD and Hung W (2022) across academic and industry settings reported predominantly positive effects of microlearning on knowledge and engagement,[11] and a systematic review by Wang C et al. (2020) reported that microlearning improved cognitive self-care capability in most of the studies reviewed, while being largely ineffective at changing actual self-care behaviour.[12] These findings reinforce the notion that microlearning promotes cognitive encoding and long-term memory consolidation by minimizing extraneous cognitive load.[3,4]

Microlearning has also shown consistent benefits in digital and mobile-supported education. The scoping review by De Gagne JC et al. (2019) of microlearning in health professions education reported improvements in knowledge, engagement, and learner satisfaction across nursing and medical contexts, particularly for mobile-delivered, short-segment formats,[10] and Lee YM et al. (2021) demonstrated that a mobile microlearning course in higher education improved knowledge and learner confidence.[22] This parallels the outcomes reported in the present analysis, especially those from digital interventions such as Instagram-based learning in Alsaid B et al. (2025)[16] and mobile micro-lectures in Molchovski P et al. (2024).[21] Both suggest that portability and easy access enhance motivation and repeated exposure.

Microlearning benefits span both knowledge and procedural or skill-based domains. Ichiuji BA et al[23] (2022) found that a microlearning module produced knowledge acquisition comparable to a traditional online module among surgery clerkship students, and in the present review modular short videos positively influenced behavioural performance in the simulation-based assessment of Gross B et al. (2019)[17] and OSCE-based clinical competency in Enoch LC et al. (2022).[18]

Furthermore, microlearning appears effective in postgraduate and continuing medical education, although such evidence lies outside the scope of the present pooled analysis. Yang C et al.[24] (2023) reported that a micro-course combined with a flipped classroom improved emergency-medicine learning among health vocational college students during the COVID-19 pandemic, Huang P et al[25] (2025), in a randomized trial of neurology residents, reported improved theoretical and clinical competency with an integrated microlearning programme, and Ho CM et al[26] (2025) found preclass micro- and nano-video formats feasible within blended medical curricula. The convergence of findings across undergraduate, postgraduate, and continuing education contexts highlights the broad applicability of microlearning, while underscoring that our pooled estimate applies specifically to undergraduate medical education.

Evidence from adjacent digital-education literature should be distinguished from microlearning evidence per se. Buchem I and Hamelmann H[27] (2010) provided an early conceptual framework positioning microlearning within continuing professional development, and Rathbone AL and Prescott J[28] (2017) systematically reviewed mobile-app and SMS-based health interventions; the latter concerns general mobile-health behaviour change rather than medical education and is cited here only as contextual evi-

dence that brief, spaced digital contacts can influence knowledge and behaviour.

Despite these consistent findings, heterogeneity remains common in microlearning research. Differences in instructional design, delivery format, assessment type, and learner characteristics contribute to variations in effect size in this review, $I^2$ was 82.0%, and subgroup analyses suggested larger effects in randomized trials and retention-focused outcomes. The observed heterogeneity is expected in educational intervention research, particularly for microlearning, which is implemented using diverse pedagogical designs ranging from SMS-based prompts to immersive digital modules. Despite this variability, the direction of effect consistently favoured microlearning, supporting the robustness of the pooled estimate. The findings suggest that microlearning functions most effectively as a complementary educational strategy that enhances, rather than replaces, conventional teaching approaches.

This review is limited by substantial heterogeneity across interventions and outcomes, inclusion of non-randomized and retrospective designs, and variability in assessment tools. These methodological constraints reduce certainty in effect magnitude, although consistency in effect direction supports the overall findings. Second, knowledge, skill, and retention outcomes measure related but distinct constructs; pooling them into a single SMD, although pre-specified to maximize statistical power, inflates heterogeneity and complicates interpretation, and the domain-specific subgroup estimates (based on only one to four studies each) should be treated as exploratory. Third, the protocol was not prospectively registered. Fourth, one included study used a retrospective design with historical control cohorts (Chaudhuri A et al., 2025)[15]; a sensitivity analysis excluding it did not materially change the pooled estimate. Fifth, two included studies did not report dispersion statistics in a form directly usable for meta-analysis: Enoch LC et al. (2022)[18] reported medians with interquartile ranges and analysed the data non-parametrically, and Molchovski P et al. (2024)[21] reported group means only, so standard deviations were imputed in both cases; a sensitivity analysis excluding Enoch LC et al. did not materially change the pooled estimate. Sixth, only English-language publications were included, raising the possibility of language bias. Finally, with only seven studies, publication-bias tests are underpowered and subgroup analyses by modality and duration were not feasible.

## Conclusion

Based on moderate-certainty evidence, microlearning probably improves knowledge acquisition, skills, and retention in undergraduate medical education (pooled SMD = 0.90; 95% CI: 0.57-1.22). However, substantial heterogeneity and methodological limitations across studies warrant cautious interpretation. Microlearning should be considered a valuable adjunct rather than a replacement for traditional teaching, and further rigorously designed randomized trials are needed to refine optimal implementation strategies.

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**Individual Author contributions: SR** contributed to the conceptualization and study design, data extraction, and manuscript writing. **VR** contributed to data analysis and full-text screening. **MPS** contributed to manuscript writing and full-text screening. All authors reviewed and approved the final manuscript.

**Availability of Data:** The complete database-specific search strategies (Appendix S1), the PRISMA 2020 checklist (Appendix S2), the list of reports excluded at full text with reasons (Appendix S3), and the full meta-analysis output including study-level effect sizes, sensitivity, subgroup and publication-bias analyses (Appendix S4) accompany this manuscript as supplementary material. The full data-extraction dataset and analysis script underlying all reported statistics are available from the corresponding author on reasonable request.

**Declaration of Non-use of Generative AI tools:** The authors declare that no generative artificial intelligence (AI) tools or AI-assisted technologies were used in the conception, design, literature search, screening, data extraction, statistical analysis, or writing of this manuscript, or in the generation of any text, table, figure, or reference contained in it. The authors alone are responsible for the content, accuracy, and integrity of the work and for the views expressed.

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