Review

Estimated Effects of Behavior Change Components in mHealth Interventions for Physical Activity and Related Psychological Outcomes: Bayesian Component Network Meta-Analysis

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Abstract

Background: Mobile health (mHealth) interventions for physical activity commonly combine several behavior change and digital design components, making it difficult to determine which components contribute to outcomes. Component estimates also depend on how repeated contrasts within trials and departures from additivity are handled.

Objective: This study aimed to estimate component-level contributions of 7 operationalized intervention components and examine whether their contributions differed between physical activity and related psychological outcomes.

Methods: We conducted a systematic review and Bayesian component network meta-analysis of 130 randomized controlled trials involving 25,369 participants after a final study-design eligibility reaudit. Component composition was coded for 310 study arms. The primary target-scope dataset contained 469 component-informative contrast-level observations from 121 trials; a broader 492-observation dataset was analyzed as a sensitivity. A Bayesian additive component metaregression used a multivariate normal likelihood by study, a shared-comparator covariance for multiarm outcome sets, a study-level random effect, and within-study residual heterogeneity. Sensitivity analyses varied working cross-outcome and cross-time correlations, restricted each study to a primary outcome group, excluded cluster-coded trials, compared outcome domains, and explored component interactions.

Results: In the primary hierarchical model, self-monitoring (Hedges g=0.153, 95% credible interval [CrI] 0.053-0.249) and gamification (g=0.102, 95% CrI 0.020-0.183) had CrIs above 0. Feedback retained a positive point estimate (g=0.127), but its 95% CrI included 0 (−0.004 to 0.259) after the final eligibility reaudit; intervals for the other 4 components also included 0. Across the 7 outcome-domain contrasts, social comparison was lower for psychological than physical activity outcomes (psychological-minus-physical activity difference g=−0.189, 95% CrI −0.325 to −0.052), including in the 66-trial same-study analysis (g=−0.201, 95% CrI −0.386 to −0.021), although social comparison was sparsely represented. For feedback, the corresponding differences were 0.084 (95% CrI −0.088 to 0.254) and 0.046 (95% CrI −0.163 to 0.254), respectively. The low-risk subset was rank deficient and did not support stable estimation of all 7 components.

Conclusions: Self-monitoring and gamification had the clearest evidence of positive component contributions. Feedback retained a positive point estimate, but its CrI included 0 in the eligibility-locked analysis, and the data did not support a robust divergence in feedback by outcome domain. The isolated social comparison domain contrast arose from sparse evidence and should be considered hypothesis generating. Component rankings should be interpreted alongside small absolute effects, uncertainty, the pragmatic additivity assumption, uncorrected cluster designs in the source summaries, and limited low-risk evidence.

Trial Registration: PROSPERO CRD420251182885; https://www.crd.york.ac.uk/PROSPERO/view/CRD420251182885

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(J Med Internet Res 2026;28:e88460) doi: 10.2196/88460

KEYWORDS

mobile health; mHealth; physical activity; behavior change components; component network meta-analysis; hierarchical meta-analysis

Introduction

Background and Rationale

Physical inactivity remains a major public health problem [1], and mobile health (mHealth) platforms offer scalable ways to support physical activity. However, intervention effects vary, and most mHealth programs combine several features within a single package. Conventional intervention-vs-control syntheses can estimate an overall package effect but cannot readily identify which features contribute to that effect.

Component network meta-analysis addresses this problem by representing each study arm as a combination of intervention components and estimating component-level contributions from between-arm differences [2]. These estimates are conditional on the specified component set and, in the usual additive model, on the assumption that component contributions combine additively. Dense component co-occurrence can make some effects or interactions difficult to identify, so additivity and network variation require explicit examination.

Outcome-Domain Differences and Study Objectives

A further question is whether a component contributes similarly to physical activity and related psychological outcomes, such as self-efficacy or motivation. Prior mHealth research has questioned whether digitally supported psychological changes reliably translate into behavior [3], but comparisons made across different sets of studies can confound an outcome-domain contrast with study characteristics. A defensible domain comparison therefore needs to account for repeated contrasts within trials and to examine studies that report both domains.

This study addressed three questions: (1) What were the estimated contributions of 7 operationalized behavior change and digital design components in mHealth interventions? (2) Did component contributions differ between physical activity and related psychological outcomes after accounting for study-level clustering and restricting the comparison to studies reporting both domains? (3) How sensitive were the estimates to assumptions about within-study dependence, follow-up duration, outcome scope, cluster randomized designs, additivity, and risk of bias?

Methods

Protocol, Search Strategy, and Eligibility Criteria

The review protocol was registered in PROSPERO (CRD420251182885), and the review was conducted in accordance with the PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) 2020 guidelines [4]. Web of Science, PubMed, Cochrane, and PsycInfo databases were initially searched from January 1, 2005, through October 20, 2025. The database searches were updated on August 8, 2026, using the same search concepts and eligibility framework. The January 1, 2005, lower bound was specified in the registered protocol before screening to focus the review on contemporary mobile and digitally delivered platforms and reduce comparability problems with presmartphone telehealth modalities; it was not chosen after examination of retrieved effects. Full database-specific search strategies are provided in Multimedia Appendix 1.

Eligible reports were peer-reviewed English-language randomized controlled trials (RCTs) of interventions delivered through a mobile or digital platform, including smartphone apps, websites, wearable devices, or exergames, with the primary aim of promoting physical activity or reducing sedentary behavior. Studies had to provide sufficient intervention descriptions for arm-level component coding and postintervention means, SDs, and sample sizes for at least 1 eligible physical activity or psychological outcome. Cluster RCTs were eligible when they otherwise met the inclusion criteria. Nonrandomized studies, crossover or within-participant studies without an eligible parallel randomized contrast, nonhuman studies, interventions without a core digital component, reports without calculable outcome data, protocols, conference abstracts, dissertations, editorials, reviews, and studies targeting unrelated behaviors were excluded.

Data Extraction, Component Framework, and Risk of Bias

Two reviewers independently extracted study characteristics, arm-level intervention content, risk-of-bias judgments, and postintervention outcome data; disagreements were resolved through discussion or consultation with a third reviewer.

The registered protocol anticipated Behavior Change Technique Taxonomy version 1 (BCTTv1)–informed coding but did not enumerate the final analytical component set. During data extraction and before component-effect estimation, the standardized arm-level template operationalized 7 higher-level fields: goal setting, self-monitoring, feedback, social support, social comparison, gamification, and personalization. These 7 fields constituted the standardized arm-level coding framework used during extraction and could be distinguished consistently from published descriptions across heterogeneous digital platforms. The framework was not intended to be an exhaustive coding of all 93 BCTTv1 techniques [5] and was not defined according to observed effect sizes, post hoc recurrence thresholds, or model significance. Because gamification represents a composite digital design feature and personalization represents a delivery attribute rather than a one-to-one BCTTv1 item, we use the broader term “intervention component.” Comparator arms were coded using the same framework. The resulting component-network geometry is presented in Multimedia Appendix 2. Detailed operational definitions and BCTTv1 mappings are provided in Multimedia Appendix 3 [5-8].

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Risk of bias was assessed using the Cochrane Risk of Bias 2 tool [9] across the randomization process, deviations from intended interventions, missing outcome data, outcome measurement, and selection of the reported result. Two reviewers made judgments independently, with disagreements resolved by discussion or consultation with a third reviewer. Overall judgments were classified as low risk, some concerns, or high risk.

Outcome Classification and Effect Size Construction

The primary outcome scope comprised physical activity outcomes and physical activity–related psychological antecedents. General clinical symptoms, general well-being, diet-only measures, and motor or balance outcomes were excluded from the primary analysis and retained only in a broader outcome-scope sensitivity analysis. Exercise or physical activity adherence was classified as a behavioral outcome, whereas app log-in, platform engagement, and intervention use adherence were not treated as physical activity. Outcomes for which lower values indicated benefit, including sedentary time, screen time, barriers, amotivation, autonomy frustration, and negative affect, were direction reversed so that positive Hedges *g* consistently favored the active arm.

Hedges *g* was calculated from postintervention means, SDs, and sample sizes. Duplicate subgroup rows belonging to the same arm and outcome were combined using pooled means and variances. Within each study-outcome-measure-time point group, the reference was defined as the arm with the fewest coded components, with control, usual care, waitlist, or standard care labels prioritized when ties occurred. Each remaining eligible arm was contrasted with that reference. Consequently, the analytical unit was a between-arm contrast rather than a study arm or trial. Multiple contrasts from the same trial were retained as nested observations rather than treated as statistically independent studies. The detailed study-, arm-, outcome-, and contrast-level construction is documented in Multimedia Appendix 3 [5-8].

Statistical Analysis

The primary analysis used a Bayesian additive component metaregression with a multivariate normal likelihood defined at the study level. Each design-matrix row represented the between-arm difference in the 7 binary component indicators, and the corresponding coefficients represented average component contributions conditional on the additive model. Independent normal(0, variance=1000) priors were assigned to component coefficients, and independent half-normal(0, scale=0.5) priors were assigned to the study-level and within-study heterogeneity SDs.

Multiple eligible contrasts could arise from multiarm designs, different outcomes, or repeated follow-up measurements. The covariance structure therefore incorporated shared-comparator sampling covariance within the same outcome and time point group, a study-level random effect linking contrasts from the same trial, and a within-study residual heterogeneity term. Correlations among different outcomes and repeated follow-up measurements from the same participants were generally unavailable and were set to 0 in the primary sampling covariance. Sensitivity analyses imposed working correlations of 0.30 and 0.60 and additionally restricted analyses to a single primary outcome group per study domain or per study; the selected outcome group was based on the largest total effective sample size rather than the observed effect magnitude.

Outcome-domain modification was examined by extending the model to estimate component contributions separately for physical activity and psychological outcomes, together with psychological-minus-physical activity differences and 95% credible intervals (CrIs). To reduce confounding arising from comparisons across different sets of studies, a same-study sensitivity analysis was restricted to trials reporting both outcome domains.

Temporal heterogeneity was examined in an exploratory follow-up duration analysis. Assessment timing was standardized in weeks. Explicit week- or month-based time points were converted to weeks, ranges were represented by their midpoint, and generic postintervention assessments without an explicit numerical time point were assigned the reported intervention duration. Where a follow-up interval was explicitly reported after a separately identified postintervention assessment, the interval was added to the intervention duration. Each component contrast was then interacted with log2-transformed assessment time centered at 12 weeks, such that the interaction coefficient represented the change in estimated component contribution associated with a doubling of assessment time.

To assess whether outcome-domain comparisons could be influenced by differential assessment timing, we also summarized assessment times separately for physical activity and psychological outcomes and conducted a paired study-level timing audit among trials reporting both outcome domains.

Additional sensitivity analyses examined the influence of outcome scope, cluster randomized designs, and risk of bias. Where cluster-adjusted estimates or intraclass correlations were unavailable, analyses used the reported arm-level summaries without imputation of a design effect. The influence of cluster randomized designs was therefore examined by repeating the analysis after excluding cluster randomized trials. This was treated as an influence analysis rather than a formal correction for clustering. For the low-risk-of-bias analysis, the component design matrix was evaluated for identifiability before component-specific estimates were interpreted.

Additivity was examined using an exploratory model containing pairwise component interactions that met a pragmatic minimum-information threshold of at least 20 nonzero interaction-design rows and increased the design-matrix rank. This threshold was used as an exploratory stability screen and was not protocol prespecified. Component pairs that did not contribute independent variation were classified as nonidentifiable rather than interpreted as having no interaction. Additive and interaction models were compared using the widely applicable information criterion (WAIC), with study as the pointwise unit and lower values indicating better predictive fit.

Posterior inference was obtained by numerical integration over the heterogeneity parameters, followed by conditional

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multivariate-normal draws. Numerical stability was evaluated using a denser integration grid and a larger number of posterior draws. Full computational specifications and stability results are reported in Multimedia Appendix 3 [5-8]. Surface under the cumulative ranking curve (SUCRA) values and posterior probabilities of ranking first were calculated as secondary measures of relative ordering and were interpreted alongside absolute component estimates and their 95% CrIs.

A supplementary assessment of bias due to missing evidence and a component-level certainty-of-evidence assessment were also conducted using structured criteria based on ROB-MEN (Risk of Bias due to Missing Evidence in Network Meta-Analysis) and GRADE (Grading of Recommendations Assessment, Development and Evaluation) or CINeMA (Confidence in Network Meta-Analysis), respectively. Detailed methods and decision rules are provided in Multimedia Appendix 4 [4,9-16]; corresponding analytical data are provided in Multimedia Appendix 5 [17-146].

Ethical Considerations

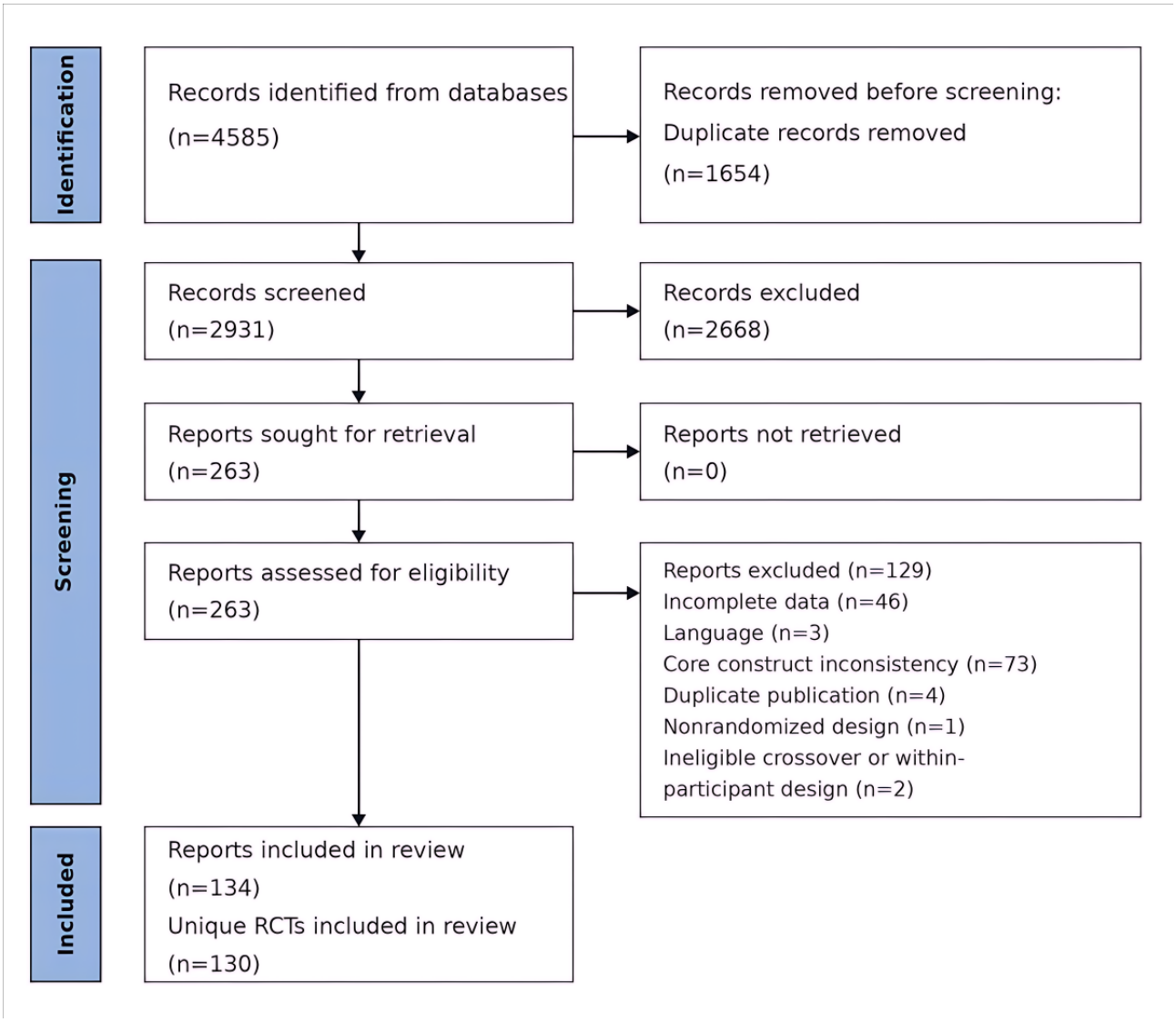
This systematic review analyzed aggregate data from published reports and did not recruit human participants or use identifiable individual-level data; therefore, no new ethics approval or informed consent was required.

Results

Study Selection and Risk of Bias

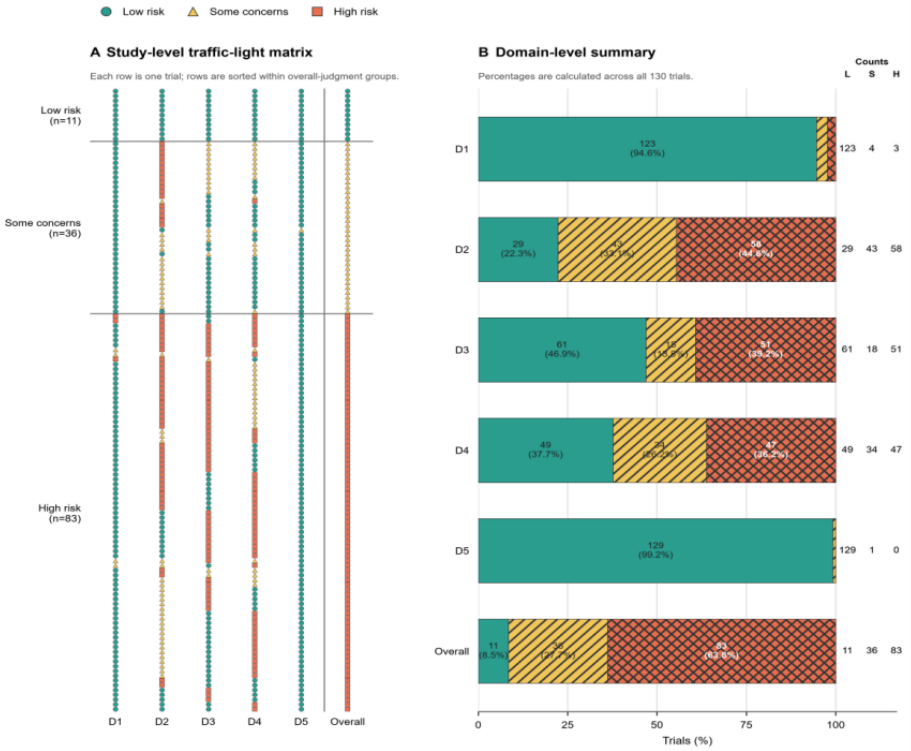
The updated search conducted on August 8, 2026, identified no additional eligible RCTs that altered the final analytical dataset. The final eligibility reaudit excluded the reports by Mo et al [6] (nonrandomized allocation), Glen et al [7] (randomized-order within-participant design), and Durst et al [8] (randomized crossover design). The final systematic review dataset included 130 unique RCTs involving 25,369 participants [17-146]. Of 263 full-text reports assessed for eligibility, 129 (49%) were excluded, leaving 134 (51%) eligible reports corresponding to 130 unique RCTs after report-to-study linkage (Figure 1). The reasons for full-text exclusion are summarized in Figure 1. Of 130 studies, 11 (8.5%) were rated as having a low overall risk of bias, 36 (27.7%) as having some concerns, and 83 (63.8%) as having a high overall risk of bias (Figure 2).

Figure 1. PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) 2020 study selection flow diagram.



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Figure 2. Risk-of-bias summary for the 130 included randomized controlled trials. D: domain; D1: bias arising from the randomization process; D2: bias due to deviations from intended interventions; D3: bias due to missing outcome data; D4: bias in measurement of the outcome; D5: bias in selection of the reported result; H: high risk; L: low risk; S: some concerns.



Analytical Dataset Construction and Component Network

Component composition was coded for 310 study arms (Table 1). Of these, 308 had at least 1 calculable follow-up record, 305 had a broad-scope outcome, and 297 had a target-scope outcome. A total of 123 studies generated at least 1 target-scope Hedges *g* contrast, and 121 contributed component-informative data. The primary dataset comprised 469 observations across 385 study-outcome-measure-time point groups; 107 studies contributed more than 1 observation. The broad-scope sensitivity contained 492 observations from 125 studies.

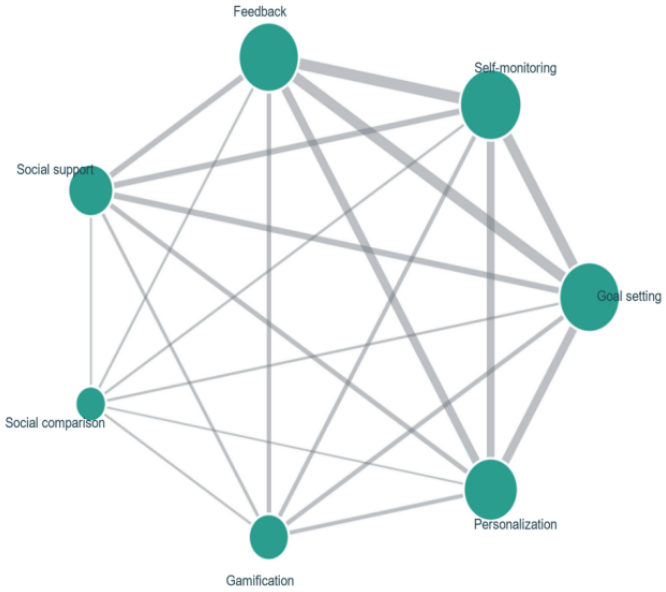
Figure 3 shows that across 310 study arms, self-monitoring (n=211, 68.1%), goal setting (n=203, 65.5%), feedback (n=201, 64.8%), and personalization (n=156, 50.3%) were the most frequently coded components, whereas social comparison was the least frequently represented (n=28, 9%). Several commonly coded components also showed substantial co-occurrence.

Table 1. Frequency of the 7 coded components across study arms (N=310).

Components	Study arms, n (%)
Self-monitoring	211 (68.1)
Goal setting	203 (65.5)
Feedback	201 (64.8)
Personalization	156 (50.3)
Social support	98 (31.6)
Gamification	71 (22.9)
Social comparison	28 (9.0)

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Figure 3. Component co-occurrence across the arm-level coding dataset.



Primary Component Contributions

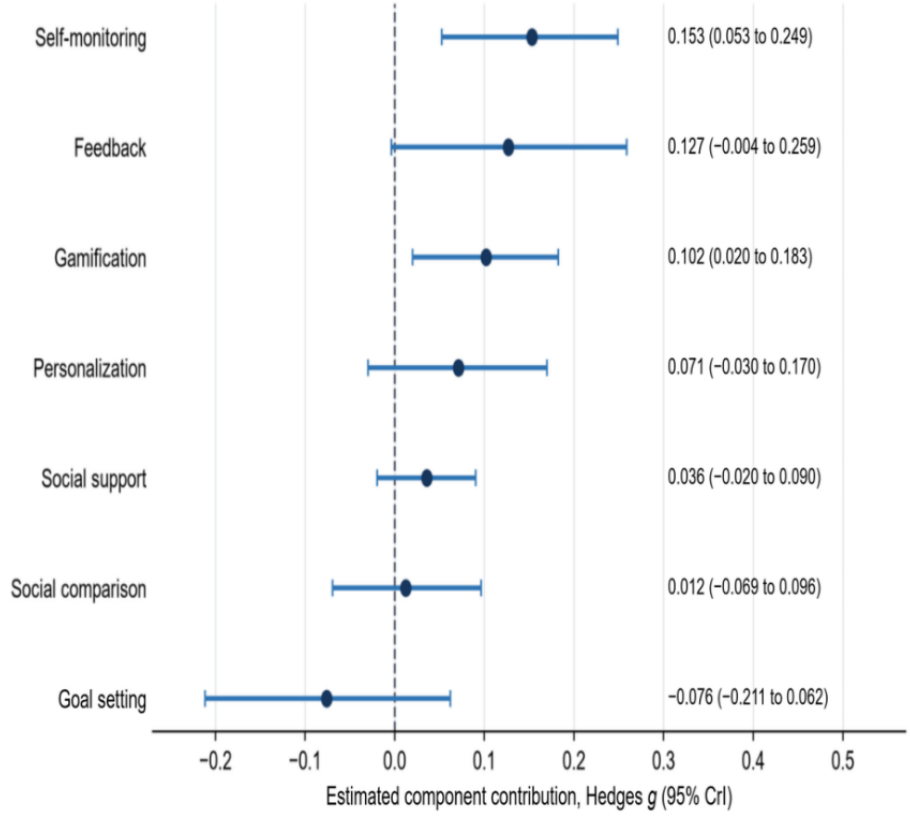
In the additive hierarchical model, self-monitoring had the largest posterior mean contribution (Hedges *g*=0.153, 95% CrI 0.053-0.249), followed by feedback (*g*=0.127, 95% CrI −0.004 to 0.259) and gamification (*g*=0.102, 95% CrI 0.020-0.183). The 95% CrIs for self-monitoring and gamification were above 0, whereas the interval for feedback included 0. Intervals for personalization, social support, social comparison, and goal setting also included 0. These component estimates and their 95% CrIs are displayed in Table 2 and Figure 4. The estimated study-level heterogeneity SD was 0.218 (95% CrI 0.175-0.250), and the within-study residual heterogeneity SD was 0.053 (95% CrI 0.000-0.100).

Table 2. Estimated component contributions in the primary hierarchical additive model.

Components	Contributions, Hedges *g* (95% CrI)
Self-monitoring	0.153 (0.053 to 0.249)
Feedback	0.127 (−0.004 to 0.259)
Gamification	0.102 (0.020 to 0.183)
Personalization	0.071 (−0.030 to 0.170)
Social support	0.036 (−0.020 to 0.090)
Social comparison	0.012 (−0.069 to 0.096)
Goal setting	−0.076 (−0.211 to 0.062)

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Figure 4. Posterior component contributions in the primary additive hierarchical model. Positive values favour the active arm. Estimates are conditional on the additive model.



Relative Ranking

Self-monitoring had the highest SUCRA (0.875) and a 52% posterior probability of ranking first, followed by feedback (SUCRA 0.751; probability of ranking first 34.2%) and gamification (SUCRA 0.677; probability of ranking first 10.3%). Full ranking results are presented in Table 3.

Table 3. Posterior relative ranking of modeled components.

Components	SUCRA[a]	Probability of rank 1, %	Posterior mean rank
Self-monitoring	0.875	52.0	1.75
Feedback	0.751	34.2	2.49
Gamification	0.677	10.3	2.94
Personalization	0.526	3.0	3.85
Social support	0.350	0.1	4.90
Social comparison	0.262	0.1	5.43
Goal setting	0.059	0.2	6.65

[a]SUCRA: surface under the cumulative ranking curve.

Outcome-Domain Analysis

Full outcome-domain estimates for all 7 components are provided in Multimedia Appendix 3 [5-8]. For feedback, the estimated contribution was g=0.149 (95% CrI −0.008 to 0.313) for psychological outcomes and g=0.065 (95% CrI −0.093 to 0.223) for physical activity outcomes (Table 4). The psychological-minus-physical activity difference was g=0.084 (95% CrI −0.088 to 0.254). In the same-study analysis restricted to the 66 trials reporting both domains, the corresponding difference was g=0.046 (95% CrI −0.163 to 0.254). Figure 5 summarizes the feedback estimates across the 2 outcome domains and the same-study sensitivity analysis.

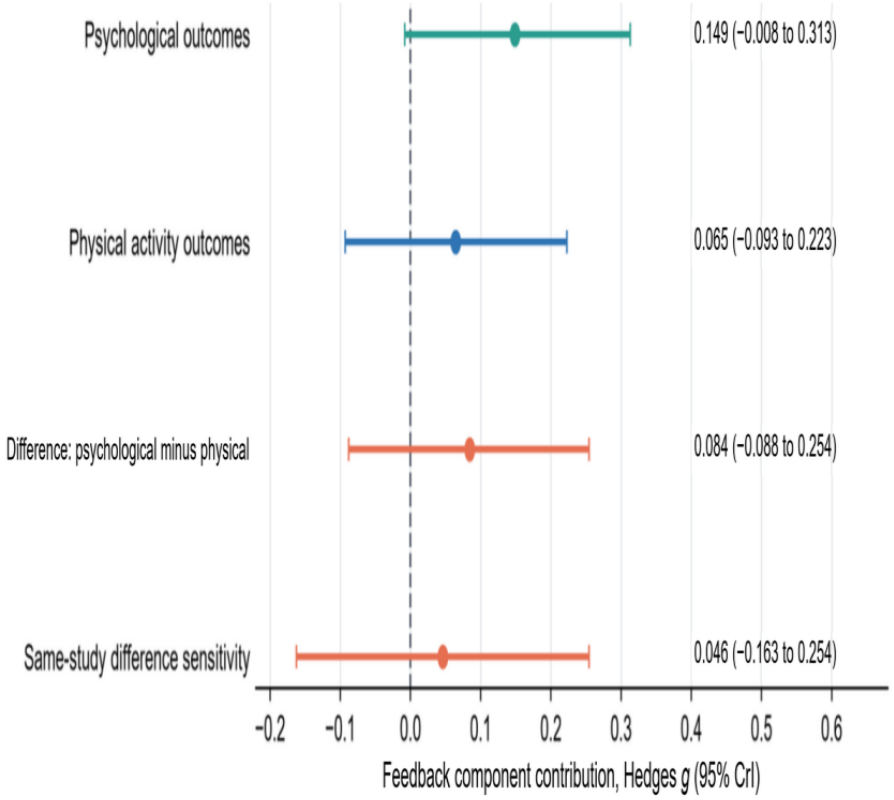
Social comparison was the only component for which the 95% CrI for the outcome-domain difference excluded 0. The psychological-minus-physical activity difference was g=−0.189 (95% CrI −0.325 to −0.052) in the full analysis and g=−0.201 (95% CrI −0.386 to −0.021) in the same-study analysis. The corresponding intervals for the other 5 components included 0.

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Table 4. Outcome-domain estimates for feedback.

Feedback estimate	Contributions, Hedges *g* (95% CrI)
Psychological outcomes	0.149 (−0.008 to 0.313)
Physical activity outcomes	0.065 (−0.093 to 0.223)
Psychological minus physical activity	0.084 (−0.088 to 0.254)
Same-study difference sensitivity	0.046 (−0.163 to 0.254)

Figure 5. Feedback estimates by outcome domain and same-study sensitivity analysis.



Follow-Up Duration and Assessment Timing

Across the 469 primary contrast-level observations, assessment timing ranged from 2 to 52 weeks, with a median of 12 (IQR 8-24) weeks. In the exploratory time-moderated model, none of the 7 component-by-time interaction estimates had a 95% CrI that excluded 0. Estimated changes in component contribution per doubling of assessment time ranged from −0.071 for feedback to 0.078 for self-monitoring (Multimedia Appendix 3 [5-8]). The time-moderated model did not improve predictive fit compared with the primary additive model (WAIC 219.1 vs 209.5).

Across unique study-domain assessment times, the median assessment time was 12 weeks for both physical activity outcomes (IQR 8.3-26.0 weeks) and psychological outcomes (IQR 8.0-17.1 weeks), although the upper quartile was later for physical activity outcomes. Among the 66 trials reporting both outcome domains, the median study-level assessment time was 12 (IQR 8.0-17.7) weeks for physical activity outcomes and 12 (IQR 8.0-16.9) weeks for psychological outcomes, with a median paired difference of 0 (IQR 0-0) weeks. Of 66 trials, 64 (97%) had identical assessment-time sets across domains, and 65 (98.5%) shared at least 1 common assessment time (Table S10 in Multimedia Appendix 3 [5-8]).

Sensitivity Analyses

Sensitivity analyses produced generally similar point estimates for the 3 leading components (Figure 6 and Table 5). For self-monitoring, the 95% CrI excluded 0 in all analyses except the ρ=0.60 working-correlation model. For gamification, the interval excluded 0 in all analyses except the analysis that focused on 1 primary outcome per study. For feedback, all point estimates were positive; the 95% CrI included 0 in the primary, working correlation, primary outcome, and broad-scope analyses and excluded 0 only in the analysis excluding cluster randomized trials.

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Figure 6. Dependence and outcome-scope sensitivity analyses for the 3 leading components in the primary model.

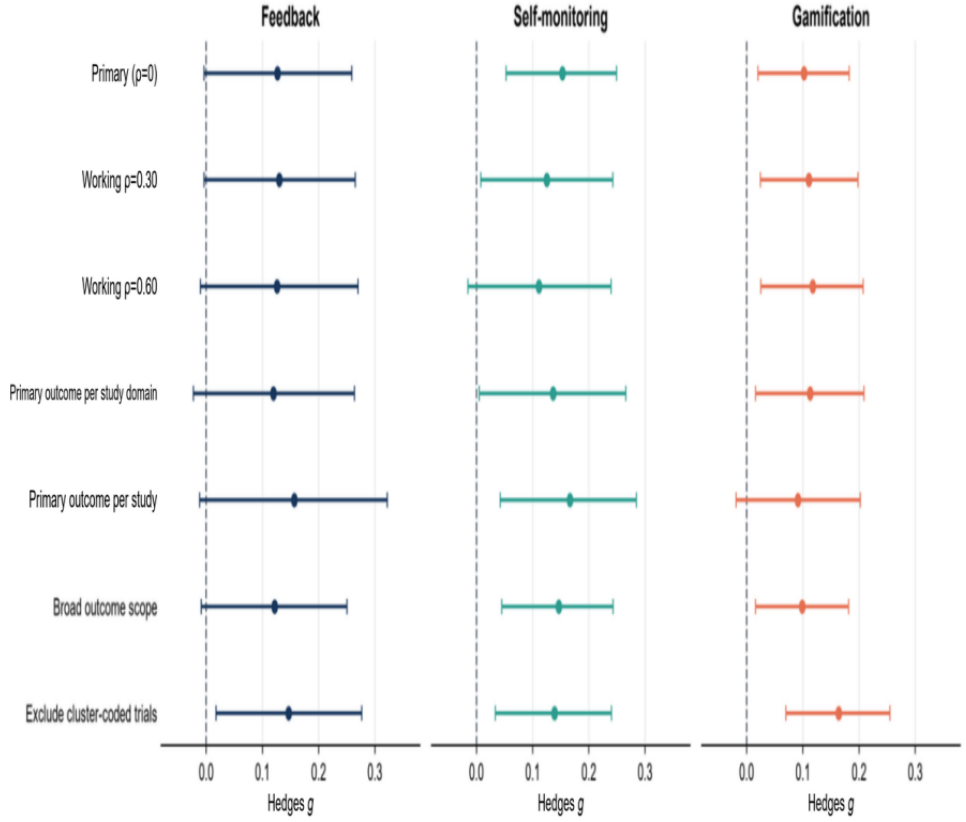


Table 5. Dependence and outcome-scope sensitivity analyses.

Model	Contrast-level observations, n[a]	Feedback, Hedges *g* (95% CrI)	Self-monitoring, Hedges *g* (95% CrI)	Gamification, Hedges *g* (95% CrI)
Primary, ρ=0	469	0.127 (−0.004 to 0.259)	0.153 (0.053 to 0.249)	0.102 (0.020 to 0.183)
Working, ρ=0.30	469	0.130 (−0.004 to 0.265)	0.125 (0.008 to 0.243)	0.111 (0.025 to 0.198)
Working, ρ=0.60	469	0.126 (−0.010 to 0.270)	0.111 (−0.015 to 0.240)	0.118 (0.025 to 0.208)
Primary outcome or study domain	240	0.119 (−0.023 to 0.264)	0.136 (0.005 to 0.266)	0.113 (0.016 to 0.209)
Primary outcome or study	162	0.157 (−0.012 to 0.322)	0.166 (0.042 to 0.285)	0.091 (−0.019 to 0.202)
Broad outcome scope	492	0.122 (−0.009 to 0.250)	0.147 (0.045 to 0.243)	0.099 (0.016, 0.181)
Exclude cluster-coded trials	401	0.146 (0.017 to 0.277)	0.139 (0.034 to 0.240)	0.164 (0.070 to 0.255)

[a]n denotes the number of contrast-level observations included in each analysis. The number varies across models because of differences in outcome scope and sensitivity-analysis restrictions; therefore, a common N and corresponding percentages are not applicable.

Model Diagnostics and Risk-of-Bias Sensitivity

The additive model had a study-level WAIC of 209.5, compared with 236.6 for the exploratory interaction model (ΔWAIC=27.1; paired SE=12.9). A total of 19 pairwise interaction terms were estimable, and all corresponding 95% CrIs included 0. Feedback×gamification and feedback×social comparison were structurally nonidentifiable because the observed component combinations did not provide independent variation.

The low-risk-of-bias subset comprised 42 component-informative contrasts from 8 contributing trials. The 7-component design matrix had rank 6, precluding estimation of the full component model within this subset; component-specific estimates were therefore not reported.

Numerical-stability checks changed posterior means by a maximum of 0.0018 and 95% CrI limits by a maximum of 0.0037 (Multimedia Appendix 3 [5-8]).

The supplementary missing-evidence assessment was judged to indicate “some concerns.” Certainty of evidence was rated low for gamification and very low for the other 6 components (Multimedia Appendix 4 [4,9-16]).

Discussion

Principal Findings

This Bayesian component network meta-analysis found that self-monitoring and gamification had the clearest evidence of

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positive component contributions to physical activity and related psychological outcomes. Feedback also had a positive posterior mean, but its 95% CrI included 0, as did the intervals for personalization, social support, social comparison, and goal setting. The estimated component contributions were small on the standardized mean difference scale and should therefore be interpreted in conjunction with their uncertainty rather than relative rankings alone.

The outcome-domain analysis did not indicate a reliable difference in the contribution of feedback between psychological and physical activity outcomes. Although the estimated contribution of feedback was numerically larger for psychological outcomes, the CrI for the psychological-minus-physical activity difference included 0 in both the full analysis and the analysis restricted to trials reporting both domains. This finding is consistent with the broader concern raised by Lin et al [3] that digitally supported psychological changes may not translate uniformly into behavioral change, but the present results do not establish a component-specific psychological-behavioral divergence or provide a direct test of an intention-behavior gap. The follow-up duration analysis showed no clear variation in component contributions over time. In the 66 trials reporting both outcome domains, physical activity and psychological outcomes were generally assessed at the same time points.

Social comparison was the only component for which the outcome-domain difference had a CrI that excluded 0, with a lower estimated contribution to psychological than to physical activity outcomes in both the full and same-study analyses. However, social comparison was represented in only 28 of 310 coded arms, and 7 component-by-domain contrasts were examined without multiplicity adjustment. This finding should therefore be regarded as exploratory and requires confirmation in studies that provide greater independent variation in social comparison.

Interpretation of Component Contributions

Self-monitoring had the largest posterior mean contribution in the primary model. This finding is broadly consistent with recent evidence suggesting that self-monitoring is a commonly useful feature of digital physical activity interventions [147,148]. However, self-monitoring frequently co-occurred with feedback, goal setting, and other intervention features, and its CrI included 0 under the strongest working-correlation sensitivity analysis. Its estimated contribution should therefore be interpreted as an average component-level association within the observed intervention network rather than as evidence that self-monitoring is independently sufficient to produce behavior change.

Feedback had one of the larger positive posterior means, but its CrI included 0 across most analyses. Recent activity tracker research has identified feedback as a potentially useful behavior change technique [147,148], yet its effects are difficult to isolate because feedback is commonly delivered together with self-monitoring, goals, prompts, or personalized content. The present estimate therefore reflects heterogeneous implementations, populations, comparators, and outcome measures and does not establish a definitive independent contribution of feedback.

Gamification also showed a positive estimated contribution, with its CrI excluding 0 in the primary model and most sensitivity analyses. This pattern is compatible with recent syntheses reporting small and heterogeneous effects of gamified interventions. Mazeas et al [149] observed attenuation of effects after intervention completion, Yang et al [150] emphasized the need to isolate individual or clustered application features, and Nishi et al [151] reported only a small increase in step counts when gamified applications were compared with nongamified applications. Together, these findings suggest that gamification may be a useful design component, but its contribution is likely to depend on implementation and accompanying intervention features.

Additivity and Component Co-Occurrence

The exploratory interaction analysis provided no clear evidence that a more complex interaction model better represented the observed data than the additive model. The interaction model had a higher WAIC, all 19 estimable interaction terms had CrIs that included 0, and 2 component pairs were structurally nonidentifiable because the observed intervention combinations did not provide sufficient independent variation.

These findings do not demonstrate that component effects are strictly additive. Rather, the component coefficients should be interpreted as average contributions under a pragmatic additive representation of the available evidence. Strong co-occurrence among commonly implemented components limits the extent to which individual contributions and interactions can be separated observationally. Factorial trials, microrandomized trials, and other experimental designs that deliberately manipulate individual and combined features would provide stronger evidence regarding synergy, redundancy, or antagonism among digital intervention components.

Strengths and Limitations

This study has several strengths. The analytical framework distinguished report-, study-, arm-, outcome-, and contrast-level units and explicitly modeled dependence among multiple contrasts from the same trial. Shared-comparator covariance was incorporated for multiarm outcome groups, and study-level and within-study heterogeneity were modeled separately. Sensitivity analyses examined alternative working correlations, restrictions to primary outcomes, broader outcome definitions, cluster randomized designs, and outcome-domain comparisons. The outcome-domain analysis was additionally restricted to trials reporting both physical activity and psychological outcomes, reducing confounding arising from comparisons across different sets of studies. Additivity was examined using an exploratory interaction model, and numerical stability was assessed using a denser integration grid and additional posterior draws.

Several limitations should also be considered. First, the component framework comprised 7 higher-level fields derived from the standardized extraction framework rather than an exhaustive mapping of all 93 BCTTv1 techniques. Gamification and personalization are not one-to-one BCTTv1 techniques, and each modeled component encompasses heterogeneous implementations. The estimated coefficients therefore represent

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broad component categories rather than effects of narrowly standardized intervention techniques.

Second, correlations among different outcomes and repeated follow-up measurements were generally unavailable. Study-level random effects and working-correlation sensitivity analyses addressed this dependence only partially. Third, cluster-adjusted estimates and intraclass correlations were not consistently available for cluster randomized trials. The primary analysis therefore relied on reported arm-level summaries, and exclusion of cluster randomized trials served as an influence analysis rather than a formal correction for clustering.

Fourth, the overall evidence base was methodologically limited: most included trials were rated as having high risk of bias, and the low-risk subset was too sparse to identify the complete 7-component model. The low or very low certainty ratings further limit confidence in component-specific estimates. Fifth, some component combinations provided insufficient independent variation to estimate selected interaction terms. Sixth, social comparison was uncommon in the intervention network, and its outcome-domain finding arose from multiple exploratory comparisons without multiplicity adjustment. Finally, although the exploratory follow-up duration analysis did not identify clear temporal modification of component contributions and assessment timing was closely aligned across outcome domains in the paired-study analysis, follow-up duration remained heterogeneous across the evidence base. The absence of a clear moderator signal should not be interpreted as evidence that component effects are temporally invariant. Standardized mean differences also combine different instruments and populations and therefore do not directly establish clinical importance, durability, or the absolute behavioral benefit associated with any individual component.

Implications

The findings identify self-monitoring and gamification as components that warrant further prospective evaluation, while the contribution of feedback remains uncertain despite a consistently positive point estimate. These results should not be interpreted as evidence that any component is sufficient on its own or that relative ranking reflects clinical superiority.

Future studies should be designed to isolate component contributions more directly. Factorial and microrandomized designs could provide greater independent variation in component combinations, while more complete reporting of arm-level intervention content, repeated-outcome correlations, cluster-adjusted estimates, and intraclass correlations would improve the precision and interpretability of future component meta-analyses. Greater standardization of component definitions and outcome measurement would also help distinguish whether observed heterogeneity reflects true differences in component effectiveness or differences in implementation and measurement.

Conclusions

In this Bayesian component network meta-analysis of 130 RCTs, self-monitoring and gamification had the clearest evidence of positive component contributions, although the estimated effects were small. Feedback had a positive posterior mean, but its CrI included 0, and there was no reliable evidence that its contribution differed between psychological and physical activity outcomes. Social comparison showed a lower estimated contribution to psychological than physical activity outcomes, but this finding was based on sparse evidence and should be regarded as exploratory.

The component estimates are conditional on the specified 7-component framework and a pragmatic additive model and are limited by component co-occurrence, incomplete adjustment for cluster randomized designs, and sparse low-risk evidence. More experimentally informative designs are needed to determine which digital behavior change components contribute independently or interactively to sustained physical activity outcomes.

Funding

The authors received no financial support for this research.

Data Availability

The eligibility-locked analytic datasets supporting the study characteristics, arm-level component coding, outcome construction, primary and broad-scope contrast datasets, and risk-of-bias and certainty assessments are provided in Multimedia Appendix 5 [17-146].

Generative AI Use

During manuscript preparation and revision, the authors used ChatGPT (OpenAI) primarily to assist with English-language editing and to improve the clarity and readability of the manuscript and responses to reviewers. All AI-assisted text was reviewed and edited by the authors, who take full responsibility for the final manuscript.

Authors' Contributions

Conceptualization: YG, LC
Methodology: YG, LC
Formal analysis: YG
Investigation: YG, YZ, WX
Data curation: YG, YZ, WX

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Visualization: YG
Supervision: LC
Writing – original draft: YG
Writing – review & editing: YG, YZ, WX, LC
All authors reviewed and approved the final manuscript.

Conflicts of Interest

None declared.

Multimedia Appendix 1

Full electronic search strategies.
[DOCX File , 55 KB-Multimedia Appendix 1]

Multimedia Appendix 2

Component network geometry.
[PNG File , 401 KB-Multimedia Appendix 2]

Multimedia Appendix 3

Analytical dataset construction and statistical-dependence audit.
[DOCX File , 355 KB-Multimedia Appendix 3]

Multimedia Appendix 4

PRISMA 2020 and PRISMA-NMA reporting checklists.
[PDF File (Adobe PDF File), 669 KB-Multimedia Appendix 4]

Multimedia Appendix 5

Eligibility-locked analytical data workbook containing study characteristics, arm-level component coding, cleaned outcome data, primary and broad-scope contrast datasets, and risk-of-bias and certainty tables.
[XLSX File (Microsoft Excel File), 496 KB-Multimedia Appendix 5]

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Abbreviations

BCTTv1: Behavior Change Technique Taxonomy version 1
CINeMA: Confidence in Network Meta-Analysis
CrI: credible interval
GRADE: Grading of Recommendations Assessment, Development and Evaluation
mHealth: mobile health
PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses
RCT: randomized controlled trial
ROB-MEN: Risk of Bias due to Missing Evidence in Network Meta-Analysis
SUCRA: surface under the cumulative ranking curve
WAIC: widely applicable information criterion

Edited by G Greco; submitted 25.Nov.2025; peer-reviewed by A Eisingerich, J Lee; comments to author 07.Aug.2026; revised version received 12.Oct.2026; accepted 30.Oct.2026; published 19.Nov.2026

Please cite as:
Gu Y, Zhao Y, Xi W, Cheng L
Estimated Effects of Behavior Change Components in mHealth Interventions for Physical Activity and Related Psychological Outcomes: Bayesian Component Network Meta-Analysis
J Med Internet Res 2026;28:e88460
URL: https://www.jmir.org/2026/1/e88460
doi: 10.2196/88460
PMID:

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