Review

Digital Health Interventions for Pregnancy- and Childbirth-Related Posttraumatic Stress Disorder Symptoms in Postpartum Women: Systematic Review and Meta-Analysis

Ziyin Huang[1*], BSN; Zhengxiu Xiao[2*], BSN; Xiaoli Liao[1*], BSN; Haoran Wu[1], BSN; Junyu Long[1], BSN; Zhiqian Zhu[1], BSN; Jiahao Huang[1], BSN; Zitong Tian[1], BSN; Qianqian Du[1], PhD; Anuttara Devassy[1], MS; Smita Pakhale[3], MD; Ri-hua Xie[1,2*], PhD

[1]Nursing Innovation and Translation Engineering Research Center, School of Nursing, Guangdong Medical University, Dongguan, Guangdong, China

[2]The Affiliated Foshan Women and Children Hospital, Guangdong Medical University, Foshan, Guangdong, China

[3]School of Epidemiology and Public Health, Faculty of Medicine, University of Ottawa, Ottawa, ON, Canada

[*]these authors contributed equally

Corresponding Author:
Ri-hua Xie, PhD
Nursing Innovation and Translation Engineering Research Center, School of Nursing
Guangdong Medical University
No.1 Xincheng Avenue
Songshan Lake High-tech Industrial Development Zone
Dongguan, Guangdong, 523808
China
Phone: 86 18928697126
Email: xierihua928@hotmail.com

Abstract

Background: Pregnancy- or childbirth-related traumatic experiences may lead to posttraumatic stress disorder (PTSD) symptoms during the postpartum period, adversely affecting women and their families. Digital health interventions may improve access to psychological support; however, evidence of their effectiveness for pregnancy- and childbirth-related posttraumatic stress disorder (PCR-PTSD) symptoms remains limited and has not been systematically synthesized.

Objective: This study aimed to evaluate the effects of digital health interventions on PCR-PTSD symptoms in postpartum women, with depressive and anxiety symptoms as secondary outcomes.

Methods: We searched 7 English-language and 4 Chinese-language databases from inception through April 30, 2026, to identify randomized controlled trials (RCTs) evaluating digital health interventions in women who had experienced pregnancy- or childbirth-related traumatic events and were assessed for PTSD symptoms during the postpartum period. Risk of bias was assessed using the Cochrane Risk of Bias 2 tool. Effect estimates were pooled using random-effects models with restricted maximum likelihood estimation and Hartung-Knapp adjustments and reported as standardized mean differences (SMDs) with 95% CIs. Statistical heterogeneity, 95% prediction intervals (PIs), subgroup effects, and the robustness of pooled estimates were also examined.

Results: A total of 10 RCTs involving 1026 participants were included. Of these, 9 trials (809 participants) reported PTSD symptom outcomes immediately after the intervention, and 6 trials (413 participants) reported outcomes at follow-up. Digital health interventions were associated with lower PTSD symptom severity immediately after the intervention (SMD −0.59, 95% CI −0.98 to −0.19, 95% PI −1.72 to 0.54; P=.01), with substantial heterogeneity (I^2=79.86%). However, the pooled effect was not statistically significant at the longest available follow-up (SMD −0.36, 95% CI −1 to 0.27, 95% PI −2.03 to 1.31; P=.20). All 95% PIs crossed the null. No statistically significant subgroup differences were observed by PTSD-related eligibility requirements, delivery modality, intervention duration, or trauma context. A statistically significant subgroup difference was observed by health care provider involvement (Q=14.46; P<.001); however, this exploratory finding should be interpreted cautiously. Sensitivity analyses supported the robustness of the pooled effect on PTSD symptoms immediately after the intervention.

Conclusions: Digital health interventions may be associated with short-term reductions in PTSD, depressive, and anxiety symptoms among women with pregnancy- or childbirth-related trauma. However, substantial heterogeneity and PIs crossing the

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null limit confidence in these effects. Further well-designed trials with clinically interpretable outcomes and longer follow-up are needed.

Trial Registration: PROSPERO CRD420261290528; https://www.crd.york.ac.uk/PROSPERO/view/CRD420261290528

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KEYWORDS

digital health; postpartum posttraumatic stress disorder; depression; anxiety; meta-analysis

Introduction

Pregnancy and childbirth are major life events that may involve potentially traumatic experiences. Pregnancy-related traumatic experiences may include pregnancy loss or termination because of fetal abnormalities [1], whereas childbirth-related traumatic experiences may include operative birth, obstetric complications, perceived threats to the life of the woman or her infant, and preterm birth [2]. Neonatal complications or admission to a neonatal intensive care unit may also contribute to traumatic experiences surrounding childbirth [3]. These experiences may lead to posttraumatic stress disorder (PTSD) symptoms. Additionally, adverse childhood experiences [4] and insufficient social support [5] have also been associated with an increased risk of PTSD symptoms following pregnancy or childbirth. Pregnancy- and childbirth-related posttraumatic stress disorder (PCR-PTSD) symptoms refer to PTSD symptoms experienced by women following pregnancy or childbirth that arise from traumatic experiences specifically related to pregnancy or childbirth. In this review, PCR-PTSD symptoms refer to PTSD symptoms arising from traumatic experiences related to pregnancy or childbirth and assessed during the postpartum period. PCR-PTSD symptoms may be assessed using validated self-report screening instruments. Scores exceeding instrument-specific established cutoffs may indicate probable PTSD but do not independently establish a clinical diagnosis [6]. In contrast, a clinical diagnosis requires a comprehensive assessment according to recognized diagnostic criteria and may involve a structured or clinician-administered diagnostic interview [7].

The burden of PTSD symptoms following pregnancy or childbirth is substantial, although estimates vary across populations and clinical contexts. A systematic review and meta-analysis reported pooled postpartum PTSD prevalence estimates of 4% in community samples and 18.5% in high-risk samples during the first postpartum year [8]. Another meta-analysis reported a pooled PTSD incidence of 19.4% among women who experienced traumatic childbirth [9]. These estimates are not directly comparable because they were derived from populations with different levels of risk and trauma exposure and used different assessment approaches, assessment timings, and outcome definitions. Together, these findings suggest that posttraumatic stress following pregnancy or childbirth represents a clinically important problem, particularly among women exposed to traumatic pregnancy or childbirth experiences. PCR-PTSD symptoms have been associated with difficulties in maternal-infant bonding [10], adverse infant behavioral and developmental outcomes [11], and lower couple relationship satisfaction [12]. Women experiencing PCR-PTSD symptoms may also face barriers to seeking professional support [13]. Therefore, effective, accessible, and scalable interventions are needed to reduce PCR-PTSD symptoms and their associated psychological burden.

Digital health is broadly defined as the field of knowledge and practice associated with the development and use of digital technologies to improve health [14]. In this review, digital health interventions included internet- and web-based programs, mobile applications and platforms, SMS or text messaging, email, instant messaging, telephone counseling, videoconferencing, telehealth, and other digitally delivered interventions [15]. Compared with face-to-face interventions, digital delivery may provide remote and flexible access to psychological support, potentially reducing barriers related to time, travel, and geographic distance [16,17]. Digital delivery may be particularly relevant for women experiencing PTSD symptoms during the postpartum period, for whom PTSD-related avoidance, stigma, concerns about disclosing traumatic experiences, and limited access to trauma-focused treatment may impede help-seeking and engagement with conventional services [18,19].

Digital platforms can also incorporate personalized content, automated reminders, symptom monitoring, and interactive feedback, which may facilitate engagement with psychological interventions [16]. These potential advantages have prompted empirical evaluation of digital health interventions for PTSD symptoms following pregnancy or childbirth in randomized controlled trials (RCTs). However, evidence from RCTs regarding the effectiveness of digital health interventions for PCR-PTSD symptoms has been inconsistent. For example, one RCT reported that a brief online self-compassion intervention significantly improved PTSD symptom scores among mothers who adhered to the intervention protocol (mean difference 2.89; 95% CI 0.31 to 5.47; P=.03) [20].

In contrast, another RCT evaluating an internet-based Developmental Home Care Support Program for mothers following preterm birth found no significant improvement in PTSD symptoms compared with usual care (β −1.24; 95% CI −5.16 to 2.69; P=.54) [21]. These discrepant findings may reflect differences in intervention components, duration, theoretical frameworks, target populations, outcome measures, and delivery modalities. Consequently, the overall effectiveness of digital health interventions for PCR-PTSD symptoms remains uncertain.

Previous reviews have examined psychological interventions for childbirth-related or postpartum PTSD [22,23], including a range of intervention approaches such as face-to-face and, in some studies, online interventions. However, these reviews did not specifically focus on digital health interventions or

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synthesize their effects as a distinct intervention category [22,23]. Other reviews have examined telehealth or mobile interventions among pregnant or postpartum women, but primarily focused on postpartum depression, anxiety, and other common mental health outcomes rather than conducting a PTSD-specific quantitative synthesis [24-26]. Similarly, a review of digital mental health interventions for PTSD synthesized evidence from general PTSD populations and did not specifically examine women during the postpartum period or PTSD symptoms related to pregnancy or childbirth [27]. Thus, the existing literature has addressed important components of the present research question but has not specifically synthesized RCT evidence on the effects of digital health interventions on PCR-PTSD symptoms assessed during the postpartum period. To our knowledge, no previous systematic review has addressed this specific question. Accordingly, this review aimed to evaluate the effects of digital health interventions on PCR-PTSD symptoms in postpartum women, with depressive and anxiety symptoms evaluated as secondary outcomes.

Methods

Research Design

This review was conducted in accordance with the PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) 2020 statement [28]. The completed PRISMA 2020 checklist is provided in Multimedia Appendix 1.

Registration and Protocol Deviations

The review protocol was developed before the literature search began. The review was registered with PROSPERO (International Prospective Register of Systematic Reviews) on March 6, 2026 (CRD420261290528), while the literature search was underway but before data extraction, risk-of-bias assessment, and data synthesis.

The registered record was subsequently amended to reflect changes in the intervention scope, population eligibility criteria, search strategy, end date, and statistical analysis plan. These deviations from the registered protocol, together with their methodological rationale and potential impact, are summarized in Multimedia Appendix 2. Accordingly, this review is reported as a registered review with subsequent major protocol amendments rather than as a review in which the final methods were fully prospectively registered.

Search Strategy

Two reviewers (ZX and XL) independently searched 7 English-language databases (PubMed, Cochrane Library, CINAHL, Scopus, Embase, Web of Science, and PsycINFO) and 4 Chinese-language databases (CNKI, Wanfang, SinoMed, and Weipu Database [VIP]) from inception through April 30, 2026, to identify studies evaluating digital health interventions for women experiencing PCR-PTSD symptoms assessed during the postpartum period. The search strategies combined database-specific controlled vocabulary terms (eg, MeSH and Emtree terms) with free-text terms covering 3 concepts: digital health interventions, pregnancy and childbirth, and PTSD or posttraumatic stress symptoms. Synonymous terms within each concept were combined using the Boolean operator OR, and the 3 concept blocks were combined using AND. Reference lists of relevant studies and systematic reviews were also manually screened to identify additional eligible studies. Complete search strategies for all databases are provided in Multimedia Appendix 3.

Eligibility Criteria

Studies were included if they met the following criteria. Participants: women who were enrolled during pregnancy or after childbirth were eligible. Interventions initiated during pregnancy were included provided that PTSD symptoms were assessed after childbirth. No prespecified upper postpartum time limit was applied to the assessment period. Eligible PTSD symptoms had to be related to pregnancy- or childbirth-related traumatic experiences. A formal clinical diagnosis of PTSD or an elevated baseline PTSD symptom score based on a prespecified cutoff was not required. However, PTSD symptoms had to be assessed using a validated clinician-administered diagnostic interview or a standardized self-report symptom scale. Intervention: Digital health interventions were defined as interventions using digital information and communication technologies to deliver or exchange health-related information and provide substantive therapeutic, counseling, educational, behavioral, or supportive content. Eligible modalities included internet- or web-based programs, mobile applications or platforms, SMS or text messaging, email, instant messaging, telephone counseling, videoconferencing, telehealth, and other digital communication technologies. Both digital-only and blended interventions were eligible, provided that the digital component delivered substantive intervention content. For multicomponent interventions, when the effect of the digital component could not be separated from that of a substantive nondigital component, the effect estimate was interpreted as representing the effect of the complete intervention package rather than the independent effect of the digital component. Such interventions were classified as blended interventions. Comparator: Eligible comparators included routine postpartum care, nondigital standard care or interventions, and wait-list controls. Outcomes: The primary outcome was PCR-PTSD symptom severity assessed after the intervention using a validated clinician-administered diagnostic interview or a standardized self-report symptom scale. Secondary outcomes included depressive and anxiety symptoms. Study design: RCTs published in English or Chinese were eligible. Studies were excluded if they were duplicate publications or if the full text was unavailable.

Study Selection

All records identified through the database searches were imported into Covidence for duplicate removal. Two reviewers (ZX and XL) independently screened the titles and abstracts of all retrieved records, followed by full-text assessment of potentially eligible studies. Disagreements were resolved through discussion, with consultation of a third reviewer (RHX) when consensus could not be reached.

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Data Extraction

Data were extracted independently by 2 reviewers (ZH and ZX) using a standardized data extraction form developed in Microsoft Excel. Before formal data extraction, the form was piloted on 2 included studies and refined to improve clarity and consistency. The extracted information included general study characteristics (author, publication year, country, sample size, study design, study period, and duration of participation); intervention characteristics (delivery modality, health care provider involvement, timing of intervention initiation, duration, and frequency); PTSD-related trauma context; outcome characteristics (outcome measures, assessment instruments, and assessment time points); and information on funding sources, conflicts of interest, trial registration, adherence/compliance or intervention completion, intervention fidelity, and adverse events. Numerical outcome data, including sample sizes, means, SDs, SEs, and effect estimates with 95% CIs when reported, were extracted for each prespecified assessment time point. Information unavailable in the original reports was recorded as "not reported." Disagreements were resolved through discussion, with consultation of a third reviewer (RHX) when necessary.

Study Risk-of-Bias Assessment

Two reviewers (ZH and XL) independently assessed risk of bias (RoB) using the Cochrane Risk of Bias 2 (RoB 2) tool for individually randomized parallel-group trials (version dated August 22, 2019) [29]. RoB was assessed for each included outcome result rather than assigned solely at the study level. The effect of interest was assignment to intervention, and the intention-to-treat framework was applied when assessing deviations from intended interventions. Missing outcome data were evaluated according to their extent and reasons, differences between intervention groups, and the potential dependence of missingness on the true outcome value. Domain-level and overall judgments were classified as low RoB, some concerns, or high RoB. Disagreements were resolved through discussion, with consultation of a third reviewer (RHX) when necessary.

Statistical Analysis

Because clinical and methodological heterogeneity was anticipated across the included studies due to differences in participant populations, PTSD-related eligibility criteria and assessment methods, intervention components, intervention durations, and delivery modalities, all meta-analyses were conducted using random-effects models rather than selecting the model based on statistical heterogeneity alone [30]. Because different validated instruments were used to assess the same outcome, continuous outcomes were pooled using Hedges *g* standardized mean differences (SMDs) with 95% CIs. For PTSD, depression, and anxiety outcomes, a negative SMD indicated an effect favoring the intervention. Between-study variance (τ^2) was estimated using the restricted maximum likelihood method, and Hartung-Knapp adjustments were applied when calculating 95% CIs for the pooled estimates [31]. Statistical heterogeneity was evaluated using the Cochran *Q* test, the I^2 statistic, and τ^2. In addition, 95% prediction intervals (PIs) were calculated for the main random-effects meta-analyses when statistically estimable to indicate the range within which the true effect of a future comparable study might be expected to lie [32]. PIs were not calculated for subgroup analyses because several subgroups contained few studies, resulting in unstable and difficult-to-interpret estimates.

To avoid double-counting participants, each study contributed only one effect estimate to a given meta-analysis. For multiarm trials, one eligible intervention group and one comparator group were included. When more than one comparator group was available, the comparator most consistent with the review question and the control conditions used across the other included studies was selected. Specifically, usual care, no-intervention, or waiting-list controls were prioritized over active comparators. The primary analyses used continuous postintervention scores rather than change-from-baseline scores; when both were available, postintervention scores were preferentially extracted and not combined with change-from-baseline data. When multiple PTSD instruments were reported at the same time point, the study-designated primary measure was selected; if none was specified, a validated PTSD-specific instrument most closely aligned with recognized diagnostic criteria was prioritized. For multiple postintervention time points, the assessment immediately after intervention completion was used for the primary analysis, whereas the longest eligible follow-up assessment was analyzed separately. Thus, each group of participants was included no more than once in each analysis.

The robustness of the primary findings was examined using leave-one-out sensitivity analysis, in which the meta-analysis was repeated after sequentially excluding each study. An additional sensitivity analysis was conducted after excluding studies judged to have high overall RoB.

To assess the robustness of the findings to alternative random-effects model specifications, the primary analysis was also repeated using the DerSimonian-Laird method for estimating between-study variance. Changes in the pooled effect estimate, 95% CI, heterogeneity statistics, and direction of effect were examined to evaluate the stability of the findings. Because fewer than 10 studies were available for each meta-analysis, funnel plots and Egger regression tests were not performed due to their limited power to detect funnel plot asymmetry and small-study effects in this setting [33]. All statistical analyses were conducted using Stata (version 18; StataCorp).

Prespecified subgroup analyses examined PTSD-related eligibility criteria (required vs not required), intervention delivery modality (digital-only vs blended), intervention duration (≤1 week, 4-6 weeks, or 8-12 weeks), PTSD-related trauma context (pregnancy loss vs childbirth-related trauma), and health care provider involvement (nurse- or midwife-led, other clinician-guided, or self-guided). Differences between subgroups were evaluated using formal tests for subgroup interaction. If subgroups included few studies, the subgroup findings were considered exploratory and interpreted cautiously.

Results

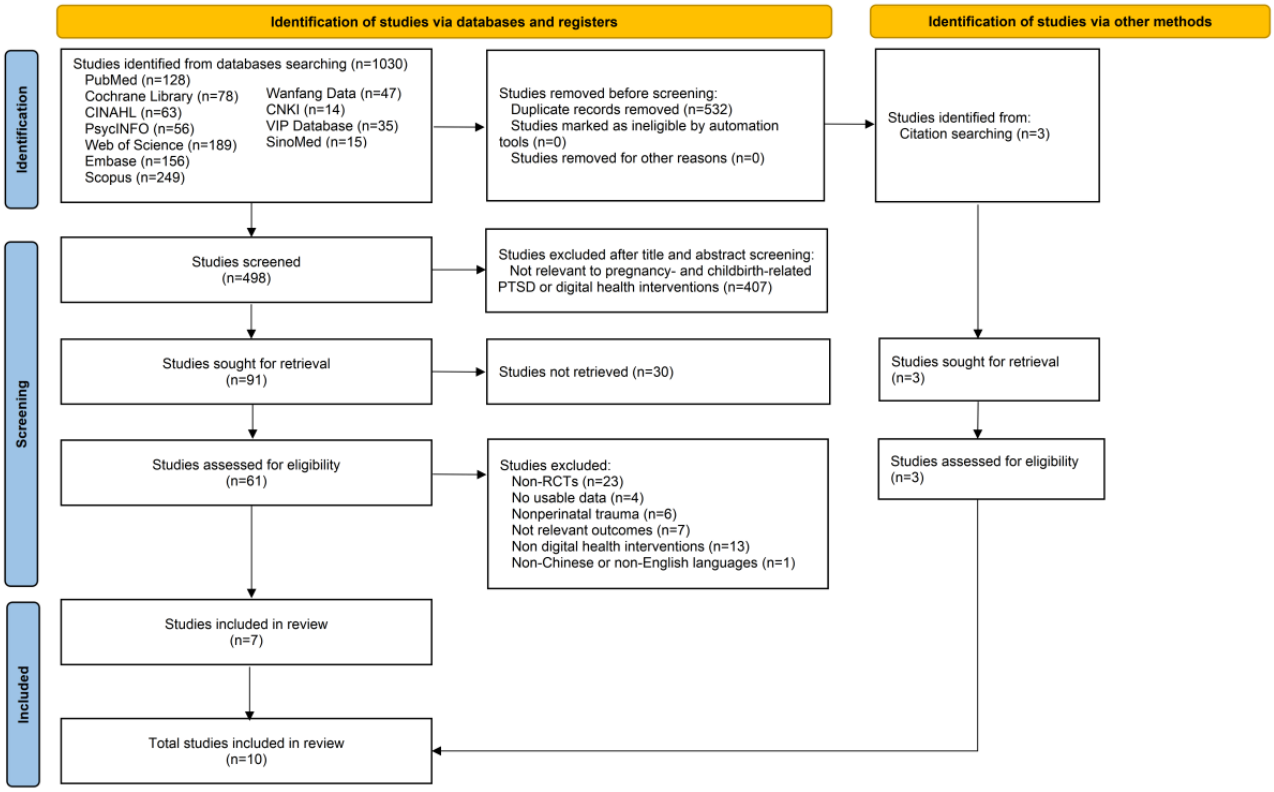
Study Selection

The study selection process is summarized in the PRISMA flow diagram (Figure 1). Database searches identified 1030 records,

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of which 532 duplicates were removed. After title and abstract screening, 91 reports were sought for retrieval; 30 could not be retrieved, and 61 were assessed for eligibility. Of these, 54 were excluded because of non-RCT study designs, unusable data, trauma unrelated to pregnancy or childbirth, irrelevant outcomes, nondigital health interventions, or language restrictions. The database searches identified 7 eligible studies [21,34-39], and citation searching identified 3 additional studies [40-42], resulting in a total of 10 included studies [21,34-42].

Figure 1. PRISMA flowchart of this systematic review. PTSD: posttraumatic stress disorder; RCT: randomized controlled trial.



Characteristics of Included Studies

A total of 10 RCTs published between 2005 and 2025 were included, involving 1026 randomized participants [21,34-42]. Sample sizes ranged from 35 [21] to 228 [36] participants. The studies were conducted in 5 countries: China (n=3) [21,34,39], Australia (n=3) [35,37,42], Iran (n=2) [38,40], Germany (n=1) [36], and Sweden (n=1) [41]. A total of 9 studies [21,34-36,38-42] used a 2-arm parallel-group design, whereas 1 [37] used a 3-arm parallel-group design.

A total of 3 studies [34,37,39] focused on pregnancy-related traumatic experiences, whereas 7 studies [21,35,36,38,40-42] addressed childbirth-related traumatic experiences. Four studies [21,36,37,41] used predominantly digital delivery, including websites, online courses, online writing, WeChat (Tencent), online consultation, and digital learning platforms. The remaining 6 studies [34,35,38-40,42] used blended delivery, combining digital or telephone components with face-to-face counseling, education, printed materials, or clinician contact.

Of the 10 studies [21,34-42], 3 studies [34,36,39] initiated the intervention during pregnancy or before pregnancy termination, whereas the remaining 7 studies [21,35,37,38,40-42] initiated the intervention after pregnancy loss or childbirth. Intervention duration ranged from 1 week [34] to 12 months [42]. Intervention frequency varied from a single self-paced online course or continuous on-demand digital support to structured programs involving multiple sessions or clinician contacts. Outcome assessments ranged from immediately postintervention to 24 months after childbirth or treatment. Detailed study characteristics are presented in Multimedia Appendix 4. Information on study funding, conflicts of interest, trial registration, adherence or intervention completion, intervention fidelity, and adverse events is summarized in Multimedia Appendix 5. Of the 10 included studies [21,34-42], 9 studies [21,34-39,41,42] reported funding sources, 9 studies [21,34,36-42] provided conflict-of-interest statements, and 6 studies [21,34,37,38,40,42] reported trial registration information. Adherence or compliance was reported in 9 studies [21,34-41], whereas intervention fidelity was reported in 9 studies [21,34-36,38-42]. Adverse events were reported in 3 studies [34,35,37]; the remaining 7 studies [21,36,38-42] did not report adverse events.

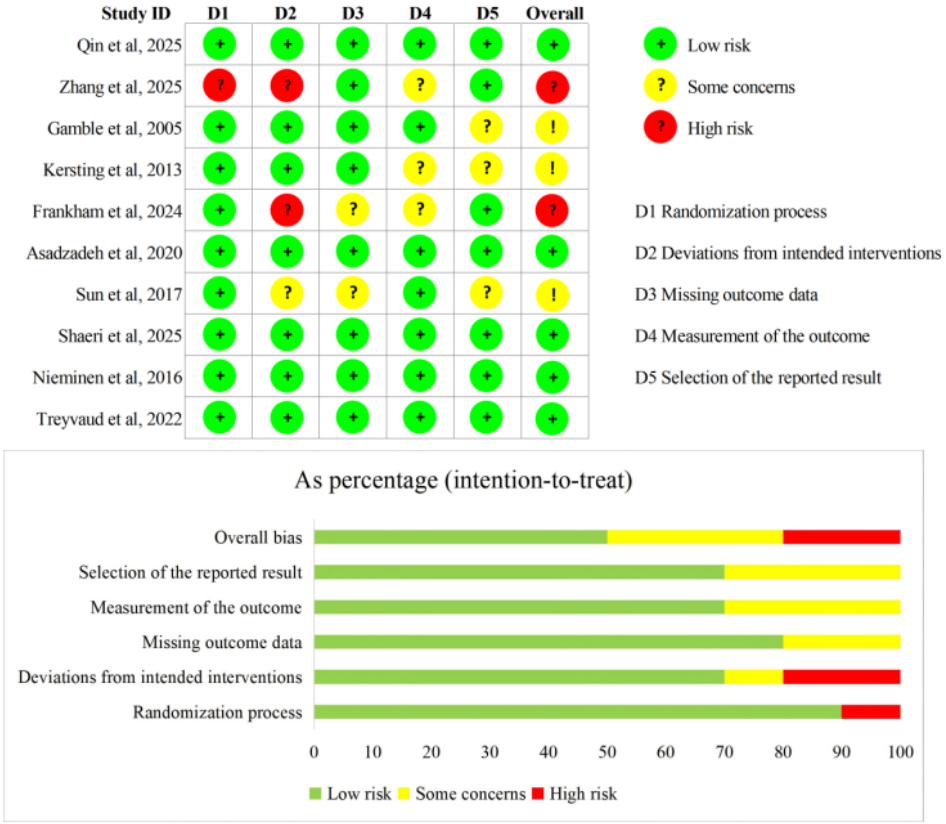
RoB in the Included Studies

RoB was assessed using the RoB 2. Overall, 5 (50%) studies [34,38,40-42] were judged to be at low RoB, 3 (30%) studies [35,36,39] raised some concerns, and 2 (20%) studies [21,37] were judged to be at high RoB. The main concerns related to deviations from intended interventions, outcome measurement, and selection of the reported results. Blinding of participants and intervention providers was generally difficult because of the nature of digital and blended psychological interventions. Moreover, because PTSD symptoms were predominantly

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assessed using participant-reported instruments, awareness of group allocation may have influenced participants' expectations or outcome reporting. The 2 studies [21,37] judged to be at high RoB were primarily affected by concerns regarding the randomization process and/or deviations from intended interventions. Detailed domain-level judgments are presented in Figure 2 [21,34-42].

Figure 2. Risk of bias judgments for the included studies [21,34-42].



Effect of Digital Health Interventions

Postpartum PTSD Symptoms

Of the 10 included studies [21,34-42], 9 studies [21,34-41] involving 809 participants reported postpartum PTSD outcomes immediately after the intervention, and 6 studies [21,34,35,37,38,42] involving 413 participants reported outcomes at follow-up. The longest available follow-up assessments occurred at 3 months postintervention or postpartum [34,35,38], a corrected age of 3 months [21], 6 months postpartum [37], and a corrected age of 24 months [42]. Immediately after the intervention, digital health interventions significantly reduced postpartum PTSD symptom severity compared with control conditions (SMD −0.59, 95% CI −0.98 to −0.19, 95% PI −1.72 to 0.54; P=.01). Heterogeneity was substantial (I^2=79.86%, τ^2=0.20; P<.001; Figure 3) [21,34-41]. Although the 95% CI excluded the null, the 95% PI included zero, indicating that the intervention effect may vary across comparable settings and that a null or smaller effect may be observed in some settings. At the longest available follow-up, the pooled effect was not statistically significant (SMD −0.36, 95% CI −1.00 to 0.27, 95% PI −2.03 to 1.31; P=.20), with substantial heterogeneity (I^2=82.61%, τ^2=0.30; P<.001; Figure 4)[21,34,35,37,38,42]. Both the 95% CI and PI included zero, indicating uncertainty regarding the persistence of treatment effects over follow-up.

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Figure 3. Forest plot of the effects of digital health interventions on postpartum posttraumatic stress disorder symptoms immediately after the intervention [21,34-41]. REML: restricted maximum likelihood.

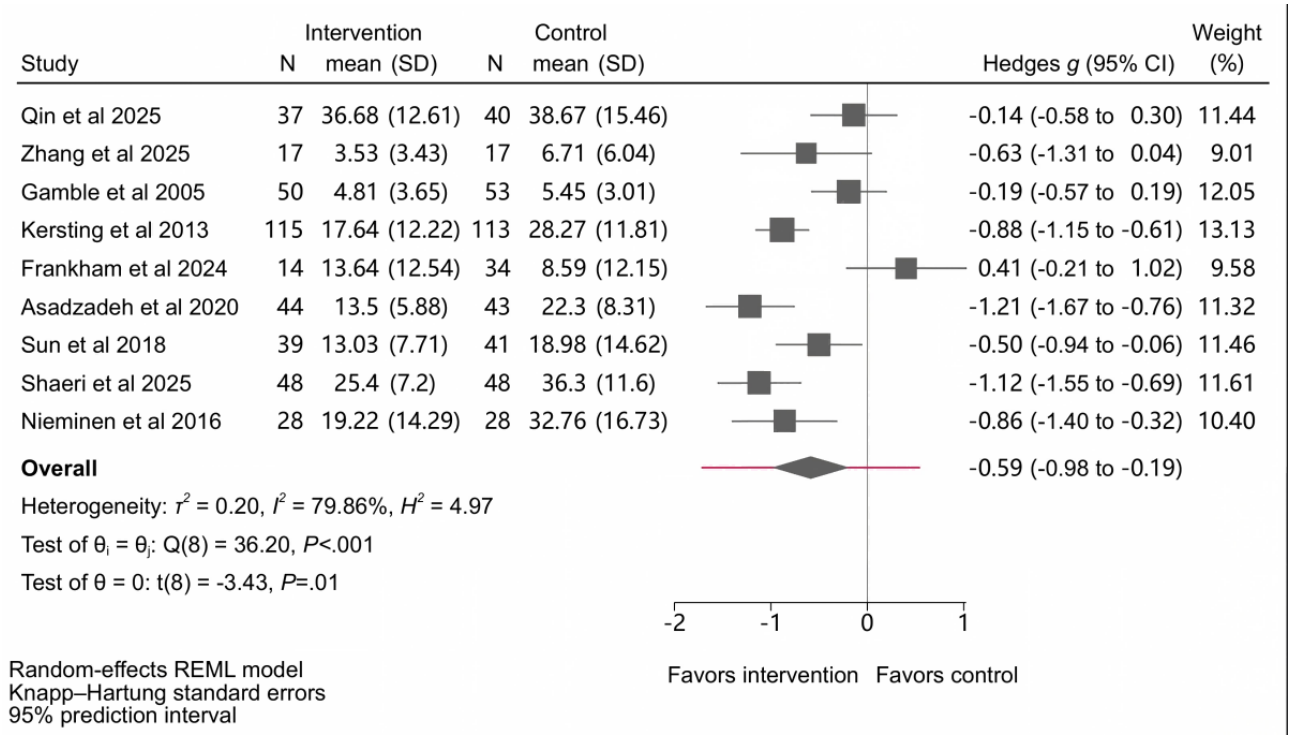
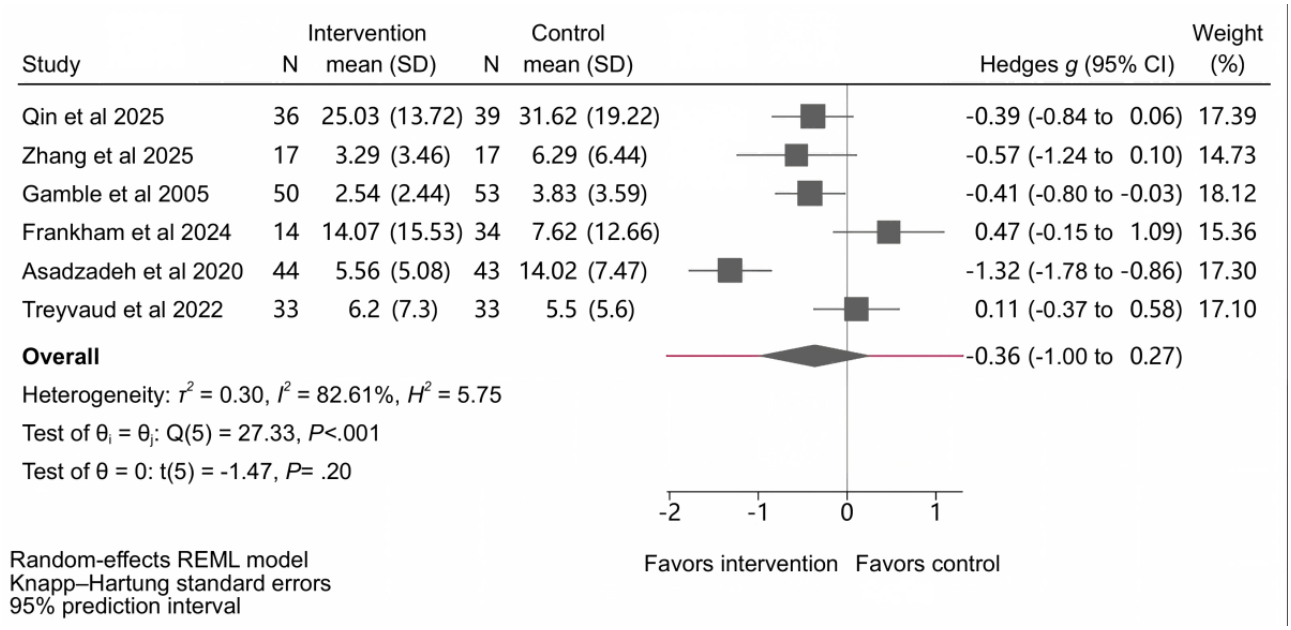


Figure 4. Forest plot of the effects of digital health interventions on postpartum posttraumatic stress disorder symptoms at follow-up [21,34,35,37,38,42]. REML: restricted maximum likelihood.



Depression Symptoms

Of the 10 included studies [21,34-42], 7 studies [21,34,36,38,39,41,42] involving 628 participants reported depressive symptom outcomes immediately after the intervention, and 4 studies [21,34,38,42] involving 262 participants reported depressive symptom outcomes at follow-up. The longest available follow-up assessments were conducted at 3 months after the intervention or 3 months postpartum [34,38], a corrected age of 3 months [21], and a corrected age of 24 months [42]. Immediately after the intervention, digital health interventions significantly reduced postpartum depressive symptoms compared with control conditions (SMD −0.58, 95% CI −0.94 to −0.21, 95% PI −1.48 to 0.32; *P*=.01; Figure 5) [21,34,36,38,39,41,42]. Heterogeneity was substantial (I^2=66.09%, τ^2=0.10; *P*=.01). Although the 95% CI excluded the null, the 95% PI included zero, indicating that the true effect in a comparable setting could range from a beneficial effect to little or no effect. At the longest available follow-up, the pooled effect was not statistically significant (SMD −0.74, 95% CI −2.12 to 0.64, 95% PI −4.77 to 3.29; *P*=.19; Figure 6) [21,34,38,42], with substantial heterogeneity

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(I^2=90.86%, τ^2=0.69; P<.001). Both the 95% CI and PI included zero, indicating uncertainty regarding the sustained effect of digital health interventions on postpartum depressive symptoms.

Figure 5. Forest plot of the effects of digital health interventions on postpartum depression symptoms immediately after the intervention [21,34,36,38,39,41,42]. REML: restricted maximum likelihood.

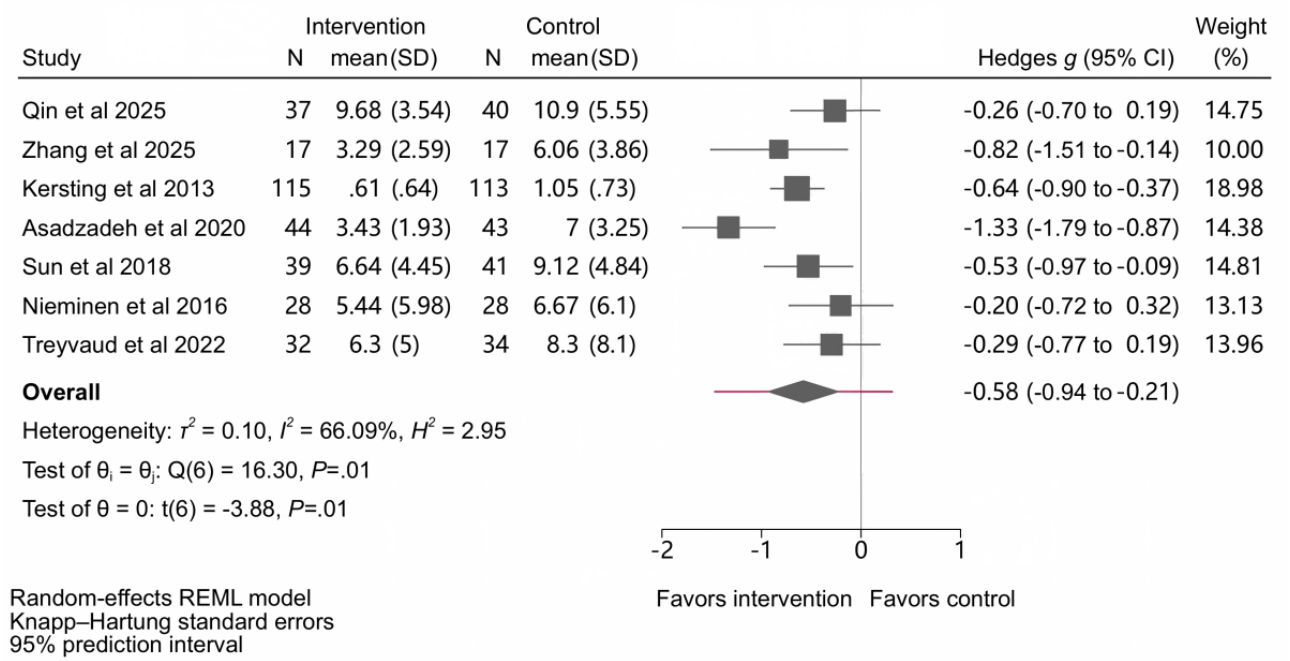
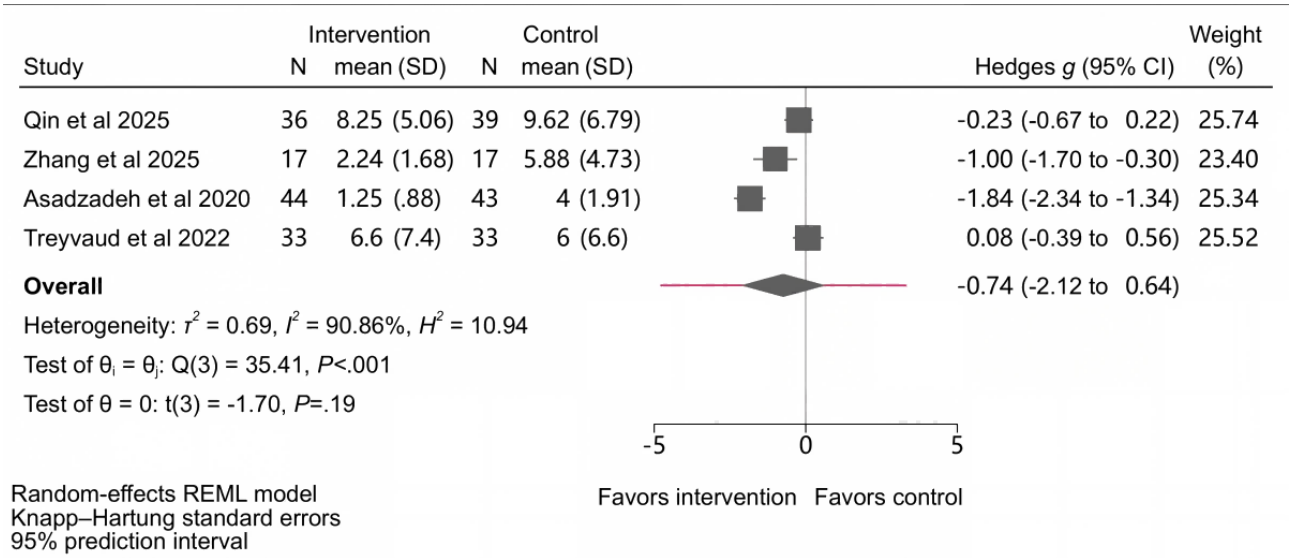


Figure 6. Forest plot of the effects of digital health interventions on postpartum depression symptoms at follow-up [21,34,38,42]. REML: restricted maximum likelihood.

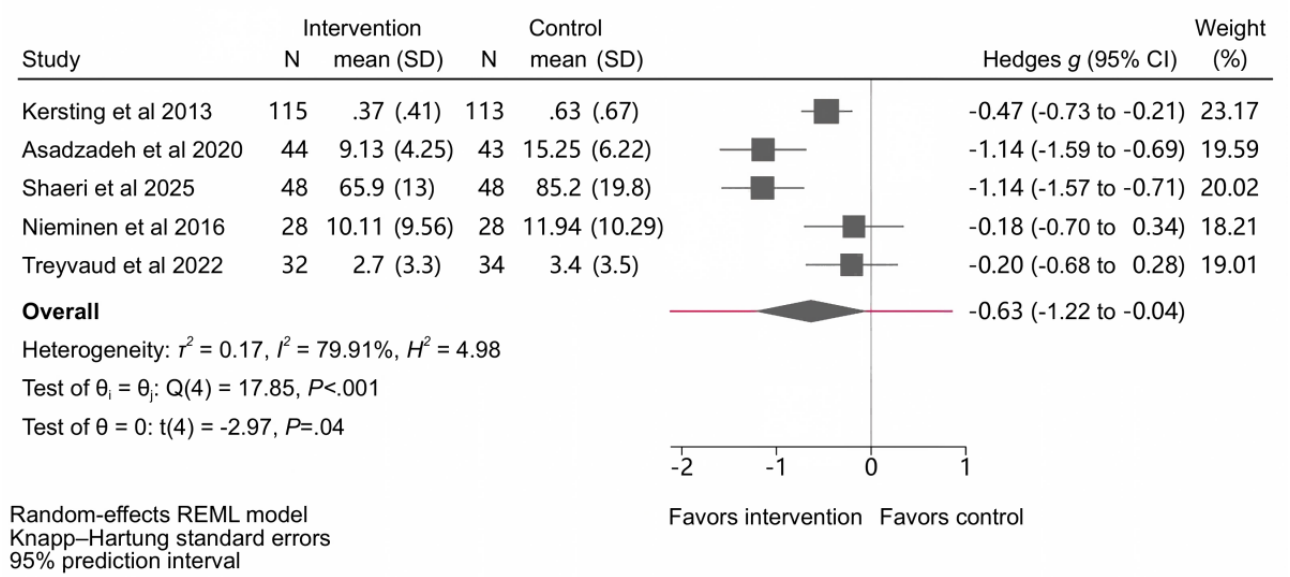


Anxiety Symptoms

Of the 10 included studies [21,34-42], 5 studies [36,38,40-42] involving 533 participants reported anxiety outcomes immediately after the intervention. Digital health interventions significantly reduced postpartum anxiety symptoms compared with control conditions (SMD −0.63, 95% CI −1.22 to −0.04, 95% PI −2.11 to 0.85; P=.04; Figure 7) [36,38,40-42]. Heterogeneity was substantial (I^2=79.91%, τ^2=0.17; P=.001). Although the 95% CI excluded the null, the 95% PI included zero, indicating that the true effect in a comparable setting could range from a beneficial effect to little or no effect.

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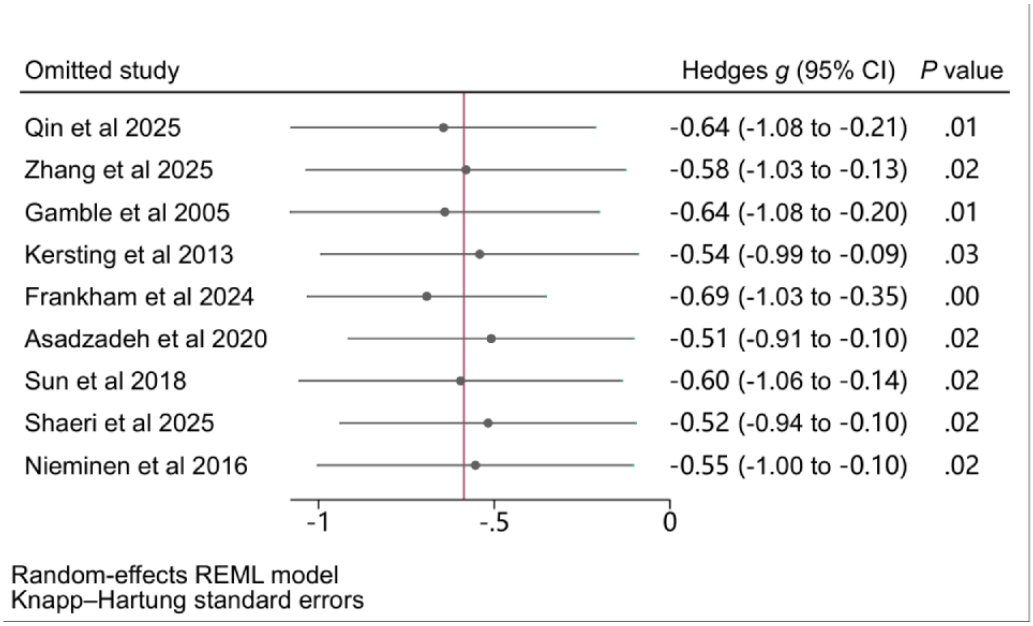
Figure 7. Forest plot of the effects of digital health interventions on postpartum anxiety symptoms immediately after the intervention [36,38,40-42]. REML: restricted maximum likelihood.



Sensitivity Analysis

The leave-one-out sensitivity analysis showed that the pooled effect remained statistically significant after the sequential exclusion of individual studies, with SMDs ranging from −0.69 to −0.51 (Figure 8) [21,34-41]. The direction and statistical significance of the pooled effect remained unchanged, indicating that the immediate postintervention findings were not driven by any single study. In an additional sensitivity analysis excluding 2 studies [21,37] judged to be at high overall RoB, 7 studies [34-36,38-41] involving 727 participants were included. The pooled effect remained statistically significant and favored digital health interventions for postpartum PTSD symptom severity (SMD −0.70, 95% CI −1.10 to −0.30, 95% PI −1.74 to 0.34; *P*=.01; Figure S1 in Multimedia Appendix 6). Heterogeneity remained substantial (I^2=76.34%, τ^2=0.14; *P*<.001). Furthermore, an alternative analysis using the DerSimonian-Laird random-effects model produced a similar pooled effect (SMD −0.59, 95% CI −0.90 to −0.27; *P*<.001; Figure S2 in Multimedia Appendix 6), although substantial heterogeneity remained (I^2=77.90%, τ^2=0.18; *P*<.001). Overall, the consistency in the direction and statistical significance of the estimates supported the stability of the pooled average effect, although between-study heterogeneity persisted.

Figure 8. Leave-one-out sensitivity analysis of the effect of digital health interventions on postpartum posttraumatic stress disorder symptoms immediately after the intervention [21,34-41]. REML: restricted maximum likelihood.



Subgroup Analysis

There was no statistically significant subgroup difference according to PTSD-related eligibility (*Q*=1.93; *P*=.16; Table 1). The pooled estimates were SMD −0.84 (95% CI −1.59 to −0.08; τ^2=0.18) [35,38,40,41] among studies requiring PTSD-related eligibility and SMD −0.38 (95% CI −0.99 to 0.23;

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τ^2=0.18) [21,34,36,37,39] among studies without this requirement.

There was no statistically significant subgroup difference by delivery (Q=0.08; P=.77). The pooled estimates were SMD −0.63 (95% CI −1.26 to 0.00; τ^2=0.21) [34,35,38-40] for blended interventions and SMD −0.52 (95% CI −1.47 to 0.43; τ^2=0.28) [21,36,37,41] for digital-only interventions. There was no statistically significant subgroup difference by intervention duration (Q=3.29; P=.19). The pooled estimates were SMD −0.14 (95% CI −0.58 to 0.30) [34] for interventions lasting ≤1 week, SMD −0.70 (95% CI −1.40 to 0.01; τ^2=0.15) [35,36,38,39] for those lasting 4-6 weeks, and SMD −0.57 (95% CI −1.63 to 0.49; τ^2=0.36) [21,37,40,41] for those lasting 8–12 weeks. Several subgroup estimates were imprecise, with confidence intervals spanning the null.

There was no statistically significant subgroup difference by trauma context (Q=0.06; P=.81). The pooled estimates were SMD −0.62 (95% CI −1.26 to 0.02; τ^2=0.30) [21,35,37,38,40,41] for childbirth-related trauma and SMD −0.54 (95% CI −1.47 to 0.40; τ^2=0.11) [34,36,39] for pregnancy loss. A statistically significant subgroup difference was observed according to health care provider involvement (Q=14.46, P<.001). The pooled effect was SMD −0.63 (95% CI −1.11 to −0.14) [21,34,35,38-40] for nurse- or midwife-led interventions and SMD −0.88 (95% CI −1 to −0.76) [36,41] for interventions guided by other clinicians. The single self-guided study had an effect estimate of SMD 0.41 (95% CI −0.21 to 1.02) [37]. Because some subgroups included few studies, these subgroup findings were considered exploratory and interpreted cautiously.

Table 1. Subgroup analyses of the effects of digital health interventions on postpartum PTSD[a] symptoms immediately after the intervention.

Subgroup analyses	Studies, n	N[b]	SMD[c] (95% CI)	P[d] values	τ^{2c}	I^{2c} (%)
PTSD eligibility criteria				.16		
PTSD-related eligibility required	4	342	–0.84 (–1.59 to –0.08)	.04	0.18	77.79
PTSD-related eligibility not required	5	467	–0.38 (–0.99 to 0.23)	.16	0.18	77.40
Delivery modality				.77		
Digital-only	4	366	–0.52 (–1.47 to 0.43)	.18	0.28	81.42
Blended	5	443	–0.63 (–1.26 to 0)	.05	0.21	81.39
Intervention duration				.19		
≤1 week[e]	1	77	−0.14 (−0.58 to 0.30)	—[f]	—	—
4-6 weeks	4	498	−0.70 (−1.40 to 0.01)	.05	0.15	80.24
8-12 weeks	4	234	−0.57 (−1.63 to 0.49)	.19	0.36	82
PTSD trauma context				.81		
Pregnancy loss	3	385	−0.54 (−1.47 to 0.40)	.13	0.11	74.40
Childbirth-related	6	424	−0.62 (−1.26 to 0.02)	.06	0.30	82.54
Health care provider involvement				<.001		
Nurse- or midwife-led	6	477	−0.63 (−1.11 to −0.14)	.02	0.17	75.64
Other clinician-guided	2	284	−0.88 (−1.00 to −0.76)	.007	0	0
Self-guided[d]	1	48	0.41 (−0.21 to 1.02)	—	—	—

[a]PTSD: posttraumatic stress disorder.

[b]N indicates the number of participants included in each subgroup analysis.

[c]I^2 indicates the percentage of total variability attributable to between-study heterogeneity; SMD, standardized mean difference; τ^2, between-study variance. Negative SMDs favor the intervention.

[d]For the bolded subgroup characteristics, P values represent tests for subgroup differences; for individual subgroup categories, P values represent tests of the pooled effect.

[e]The P value for the pooled effect and heterogeneity statistics could not be estimated because the subgroup contained only 1 study.

[f]Not available.

Discussion

Principal Findings

This systematic review and meta-analysis synthesized evidence from 10 RCTs evaluating digital health interventions for PCR-PTSD symptoms and related mental health outcomes. Digital health interventions were associated with statistically significant average reductions in postpartum PTSD, depressive, and anxiety symptoms immediately after the intervention. The findings for depressive and anxiety symptoms are broadly consistent with previous systematic reviews and meta-analyses reporting small to moderate beneficial effects of digital interventions on perinatal or postpartum depression and anxiety

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[24,26,43]. However, the 95% PIs for the immediate postintervention effects on all 3 outcomes included the null, indicating that the true intervention effects may vary considerably across comparable settings and may not consistently favor digital health interventions [44]. The pooled effects on PTSD and depressive symptoms were not statistically significant at follow-up, while follow-up data for anxiety symptoms were insufficient to evaluate sustained effects.

The pooled immediate postintervention effect on PTSD symptoms was SMD −0.59. However, statistical significance and conventional thresholds for interpreting effect size classifications do not necessarily indicate clinical importance. PTSD symptoms were assessed using the Impact of Event Scale–Revised [34,36,37,39-41], PTSD Checklist for *Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition* (*DSM-5*). [38], PTSD Checklist-Civilian Version [40], Perinatal Post-Traumatic Stress Questionnaire [21], Traumatic Event Scale [41], and PTSD module of the Mini-International Neuropsychiatric Interview [35], which differ in content, scoring, and alignment with diagnostic criteria. Because no common validated minimally important difference or symptom threshold was applicable across these measures and populations, the pooled SMD could not be translated into a common absolute change in symptoms. Moreover, the included studies mainly reported continuous symptom scores rather than clinically interpretable outcomes such as remission, reliable improvement, or transition below a probable PTSD cutoff. Therefore, the clinical importance of the observed reduction remains uncertain.

The point estimate was more favorable for 4 to 6-week interventions, but the subgroup-difference test for intervention duration was not statistically significant (P=.19). Several 4- to 6-week interventions included structured professional support, such as therapist-guided expressive writing [36], midwife-led counseling [35,38], or nurse-led family support [39], making it difficult to separate the effect of intervention duration from those of intervention content, intensity, and professional support. Thus, these findings should be considered exploratory and should not be interpreted as evidence of an optimal intervention duration.

Health care provider involvement may also have contributed to heterogeneity. Although a statistically significant subgroup difference was observed by health care provider involvement, this finding should be interpreted cautiously because the other clinician-guided subgroup included only 2 studies [36,41] and the self-guided subgroup was represented by only 1 study [37]. Moreover, the form and intensity of provider involvement varied across studies and were closely intertwined with intervention content and duration, making it difficult to isolate the independent contribution of professional support. Thus, these exploratory findings do not establish whether health care provider involvement independently influenced intervention effects or whether professionally guided interventions were superior to self-guided interventions.

Similarly, the larger point estimates observed in some blended interventions were primarily observed in structured midwife-led counseling trials [38,40], whereas other blended interventions showed smaller effects [34,35]. Previous evidence indicates that technology-based postpartum mental health interventions commonly last approximately 4 to 8 weeks [43] and that human support may improve engagement and therapeutic alliance in some digital mental health interventions; however, its added value remains uncertain, particularly for PTSD outcomes [45,46]. The duration-specific estimates had wide CIs, and the ≤1-week subgroup included only one study. The absence of a statistically significant effect in some longer-duration interventions may reflect differences in intervention content or intensity, potentially lower engagement over time, or limited statistical power rather than evidence that longer interventions are ineffective. Although adherence was reported as generally high in several studies, inconsistent definitions and limited reporting of postintervention engagement mean that these proposed explanations remain hypothetical [47]. Thus, the available evidence does not establish an optimal intervention duration or the superiority of blended delivery.

Heterogeneity may also reflect differences in trauma context, baseline PTSD risk, and PTSD-related eligibility criteria. Among the 9 trials in the primary analysis, 3 [35,38,40] required participants to meet the relevant DSM Criterion A following traumatic childbirth, whereas 1 [41] required self-reported childbirth-related posttraumatic stress symptoms and a Traumatic Event Scale score of at least 30. The other 5 trials did not require elevated PTSD symptoms, probable PTSD, or clinically diagnosed PTSD at enrollment [21,34,36,37,39]. The pooled estimate therefore reflects an average across populations with different PTSD risk profiles rather than an effect in a clinically homogeneous population. PTSD-related eligibility did not significantly moderate the effect (P=.16). However, PTSD-related eligibility criteria do not directly capture baseline symptom severity. Because baseline PTSD symptoms were assessed using different instruments and were not consistently reported in a comparable form, the contribution of baseline symptom severity to heterogeneity could not be evaluated directly. Trauma context also did not significantly moderate the effect (P=.81), although these subgroup analyses had limited power. Differences in control conditions and in the cultural contexts and health care systems across the 5 countries may also have contributed to heterogeneity. Meta-regression was not conducted because only 9 studies [21,34-41] contributed to the primary analysis, and no single source of heterogeneity could be identified.

At the longest available follow-up, the pooled estimates for PTSD and depressive symptoms were not statistically significant and were imprecise, while findings across individual studies were inconsistent [21,34,35,37,38,42]. In a broader evidence base, Brocklehurst et al [43] reported small but statistically significant follow-up effects of technology-based postpartum interventions on depression and anxiety. Another review identified follow-up timing as an important source of variation in the effects of telehealth interventions on postpartum depression [24]. Similarly, a recent postpartum SMS trial found that the improvement in anxiety observed immediately after a 6-week intervention was not maintained at 6 months [48]. In this review, few studies provided follow-up data, and assessments ranged from 3 months postintervention or postpartum to 24 months of corrected age, using different

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reference points across studies. Thus, the pooled estimates do not represent effects at a uniformly defined follow-up time point. Among studies reporting adherence, adherence during the active intervention period was generally high (85.7%-100%); however, definitions of adherence varied, and postintervention engagement was rarely reported. Given the recognized importance of engagement in digital mental health interventions [49,50], its role in maintaining treatment effects could not be determined. Follow-up data for anxiety were also insufficient to evaluate sustained effects. Overall, the sustained effects of digital health interventions on PTSD, depressive, and anxiety symptoms remain uncertain.

Risk-of-bias findings warrant caution when interpreting the pooled estimates. Blinding of participants and intervention providers was generally infeasible, and the predominantly self-reported outcomes may have been influenced by participants' awareness of group allocation. Attrition and missing outcome data may also have affected the estimates, particularly if dropout was related to symptom severity or intervention response. Incomplete reporting of allocation concealment and potential selective reporting may also have introduced additional uncertainty. Nevertheless, the immediate PTSD effect remained statistically significant in both the leave-one-out analysis and the analysis excluding the 2 studies [21,37] at high overall RoB (SMD −0.70, 95% CI −1.10 to −0.30), indicating that the direction and statistical significance of the pooled effect were unchanged across these sensitivity analyses. However, substantial heterogeneity persisted (I^2=76.34%), and the 95% PI included the null (−1.74 to 0.34), indicating continued uncertainty.

Clinical Implications

Digital health interventions may provide flexible and accessible psychological support for women with pregnancy- or childbirth-related traumatic experiences, particularly when face-to-face care is difficult to access [43]. However, given the limited evidence base, substantial heterogeneity, PIs that included the null, and uncertainty regarding sustained effects, recommendations for implementation should remain conditional. Digital health interventions may currently be considered potential adjuncts to postpartum mental health care, supporting rather than replacing existing health services [51]. Women with clinically significant or persistent PTSD symptoms should continue to receive appropriate professional assessment and evidence-based treatment [52].

Strengths and Limitations

This review searched 11 databases covering English- and Chinese-language literature and included only RCTs. RoB was assessed using RoB 2, and the analyses used random-effects models with restricted maximum likelihood estimation and Hartung-Knapp adjustments to the 95% CIs. PIs and sensitivity analyses, including leave-one-out analysis, exclusion of studies at high overall RoB, and use of the DerSimonian-Laird model, were conducted to assess uncertainty and the robustness of the pooled estimates. The inclusion of studies from 5 countries also enhances the geographic diversity of the evidence base.

Several limitations should be acknowledged. Substantial clinical and methodological heterogeneity existed in participant risk profiles, trauma contexts, PTSD-related eligibility criteria and assessment methods, intervention characteristics, and control conditions; therefore, the pooled estimates may not apply consistently across settings. For blended or multicomponent interventions, the observed effects represented the complete intervention package when the independent effect of the digital component could not be isolated. Subgroup analyses included few studies and were exploratory, and meta-regression was not conducted because only 9 studies contributed to the primary analysis. The use of different PTSD instruments prevented comparison with a common minimally important difference, while clinically interpretable outcomes such as remission were rarely reported. Participant blinding was generally infeasible, and missing outcome data, incomplete reporting of allocation concealment, and potential selective reporting may have affected the estimates. Finally, because fewer than 10 studies were included in each meta-analysis, potential small-study effects could not be reliably assessed using funnel plots or Egger's regression test. Consequently, small-study effects, including those potentially arising from publication bias, cannot be ruled out.

Conclusions

Digital health interventions may be associated with short-term reductions in posttraumatic stress, depressive, and anxiety symptoms among women experiencing pregnancy- or childbirth-related trauma. However, substantial heterogeneity and PIs that included the null indicate uncertainty regarding the consistency of these effects. Evidence of sustained benefit remains limited. Further well-designed trials with clinically interpretable outcomes and longer, consistently defined follow-up are needed.

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Data Availability

The data extracted and analyzed in this systematic review are available from the corresponding author upon reasonable request.

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Generative AI Use

The authors declare that generative AI was used in the creation of this manuscript. Generative AI (ChatGPT) was used for proofreading, editing, or language polishing. The authors verified all outputs of the tool(s) used in the preparation of this manuscript and take full responsibility for the final content of the manuscript.

Authors' Contributions

Conceptualization: RHX, ZH, XL, ZX
Data curation: ZH, ZX, XL, HW, ZZ, JL, JH, ZT, QD
Formal analysis: ZH, ZX
Investigation: ZX, XL, ZH, HW, QD
Methodology: ZRH, ZX, ZH, XL, QD, SP
Supervision: RHX, ZX, SP
Validation: ZH, ZX, XL, RHX, ZZ, JL, JH, ZT, QD
Visualization: ZH, ZX, XL
Writing-original draft: ZH, ZX, XL
Writing-review & editing: RHX, ZX, ZH, XL, HW, ZZ, JL, JH, ZT, QD, AD, SP

Conflicts of Interest

None declared.

Multimedia Appendix 1

PRISMA 2020 checklist.
[DOCX File , 34 KB-Multimedia Appendix 1]

Multimedia Appendix 2

Protocol deviations, rationale, potential implications, and corrective actions.
[DOCX File , 35 KB-Multimedia Appendix 2]

Multimedia Appendix 3

Search strategy.
[DOCX File , 18 KB-Multimedia Appendix 3]

Multimedia Appendix 4

Characteristics of the 10 included randomized controlled trials.
[DOCX File , 42 KB-Multimedia Appendix 4]

Multimedia Appendix 5

Implementation and reporting characteristics of the included randomized controlled trials.
[DOCX File , 2721 KB-Multimedia Appendix 5]

Multimedia Appendix 6

Sensitivity analysis.
[DOCX File , 494 KB-Multimedia Appendix 6]

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Abbreviations

DSM-5: Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition
PCR-PTSD: pregnancy- and childbirth-related posttraumatic stress disorder
PI: prediction interval
PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses
PROSPERO: International Prospective Register of Systematic Reviews
PTSD: posttraumatic stress disorder
RCT: randomized controlled trial
RoB: risk of bias
RoB 2: Cochrane Risk of Bias 2
SMD: standardized mean difference

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