Review

New Insights and Effectiveness of Digital Interventions for Health Anxiety: Systematic Review and Meta-Analysis

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Abstract

Background: Health anxiety (HA), classified in the *Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition (DSM-5)* under illness anxiety disorder (IAD) or somatic symptom disorder, is an increasingly prevalent condition associated with substantial psychological and health care burdens. Digital mental health interventions (DMHIs) offer scalable and accessible approaches to care, with digital behavioral interventions (DBIs), particularly internet-delivered cognitive and behavioral therapies, among the most frequently studied. However, their effectiveness in reducing HA has not been comprehensively synthesized.

Objective: This study aimed to systematically evaluate the effectiveness of DBIs for reducing HA in adults and to examine their effects on related psychological outcomes.

Methods: This systematic review and meta-analysis followed the PRISMA 2020 statement and Cochrane guidance. PubMed, Embase, Web of Science, ScienceDirect, PsycINFO, and the Cochrane Library were searched from database inception to November 1, 2025. Eligible studies included randomized controlled trials (RCTs), quasi-experimental studies, and cohort studies involving adults with HA. HA severity was the primary outcome. Random-effects meta-analyses were restricted to RCTs. Methodological quality was appraised using Joanna Briggs Institute tools, risk of bias in RCTs using Risk of Bias 2 (RoB 2), and certainty of evidence using Grading of Recommendations Assessment, Development, and Evaluation (GRADE).

Results: Sixteen studies involving 2581 participants were included, comprising 9 RCTs, 6 quasi-experimental studies, and 1 cohort study. Six RCTs involving 585 participants contributed to the primary meta-analysis. Digital interventions significantly reduced HA compared with control conditions (Hedges g=–0.77, 95% CI –1.25 to –0.30; P=.002), although between-study heterogeneity was substantial (I^2=86.7%). Leave-one-out analyses showed that this effect remained significant after exclusion of any individual trial. By contrast, pooled effects on illness-related attitudes, general anxiety, anxiety sensitivity, and depressive symptoms were not statistically significant and were characterized by substantial heterogeneity. Among the 9 RCTs, 4 were judged as having low overall risk of bias, 2 had some concerns, and 3 were at high risk. GRADE certainty was low for HA and very low for the secondary outcomes.

Conclusions: Digital interventions show potential for reducing core health-anxiety symptoms, but confidence in the magnitude and generalizability of the effect is limited by substantial heterogeneity, risk-of-bias concerns, and low certainty of evidence. Evidence for broader psychological outcomes remains inconclusive. Adequately powered, well-reported RCTs using rigorous outcome assessment, appropriate active comparators, and longer follow-up are needed to confirm effectiveness and clarify which patients benefit most and under what delivery conditions.

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Keywords: health anxiety; anxiety disorder; hypochondria; digital intervention; digital behavioral intervention.

Introduction

Health anxiety (HA), classified in the *Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition (DSM-5)* under illness anxiety disorder (IAD) or somatic symptom disorder (SSD), is a prevalent and distressing condition characterized by persistent preoccupation with having or acquiring a serious illness despite appropriate medical reassurance, a presentation historically referred to as hypochondriasis [1]. Although the *DSM-5* classifies these presentations under somatic symptom and related disorders, HA is widely conceptualized as part of the anxiety spectrum. It shares the core features of anxiety disorders, including excessive worry, anticipatory anxiety, and avoidance behaviors [2]. In HA, these processes are directed toward illness-related threats and manifest as fear of serious disease, hypervigilance to bodily sensations, and repeated reassurance seeking. Consistent with this overlap, HA frequently co-occurs with generalized anxiety disorder, which has an estimated lifetime prevalence of 6.2%, and may represent an early or prodromal manifestation of anxiety pathology [3]. Epidemiological evidence indicates that HA affects approximately 3.4% of the general population [4], with early UK studies reporting rates of 4%-8% [5,6], and its prevalence may reach 20% among patients attending primary care and other medical settings [7]. In the wake of recent global infectious disease outbreaks, HA has become an increasingly prominent public mental health concern [8]. The pandemic and its aftermath have also reshaped the online dissemination of health information. Compared with the general population, individuals with elevated HA are more likely to respond to such information with heightened worry, fear, and negative expectations, thereby reinforcing and worsening their symptoms [9,10]. Persistent fear of serious illness and repeated monitoring of bodily symptoms can cause substantial psychological distress and functional impairment, reducing quality of life [11]. At the system level, excessive health care use, unnecessary diagnostic testing, and productivity losses place a considerable burden on health care systems [12]. Importantly, elevated levels of HA have also been observed among health care providers [13], with prevalence rates reported to be as high as 30% in medical professionals [14]. Consequently, increasing clinical and public health attention has been directed toward these populations, highlighting the urgent need for scalable and accessible interventions to mitigate the growing impact of disease-related anxiety on public health.

A critical characteristic that must be addressed in the prevention and intervention of HA is patients' anxiety sensitivity (AS) and the involuntary emergence of negative mental imagery when processing health-related information [15]. Many individuals, driven by fear of illness, frequently search online for information about diseases and available treatments; however, excessive reliance on internet-based self-diagnosis does not alleviate anxiety and may instead exacerbate it [16]. Continuous exposure to heterogeneous and often unreliable online health information environments can further contribute to psychological dysregulation. Currently, cognitive behavioral interventions have been widely used to address disease-related anxiety at the population level [17], partially reducing anxiety sensitivity and alleviating psychological distress by optimizing daily behaviors and redirecting attentional focus. In recent years, the integration of digital technologies into behavioral therapies has emerged as a promising approach to expanding the reach of mental health services. Digital mental health interventions (DMHIs) [18], including internet-based cognitive behavioral therapy (ICBT) [19], mobile health apps [20], and digitally delivered interactive exposure therapies [21], have demonstrated efficacy comparable to face-to-face interventions in treating anxiety disorders, while offering notable advantages such as cost-effectiveness [22], anonymity, and geographic flexibility [23]. Additionally, questionnaire-based evidence suggests that improving health literacy and promoting health-related behaviors play a critical role in emotional regulation among individuals with HA [24]. Within the domain of behavioral interventions, behavioral activation (BA) has been shown to effectively reduce anxiety symptoms by disrupting avoidance cycles [25] and enhancing engagement in meaningful activities, either actively or passively [26]. When implemented through digital platforms, these behavioral strategies exhibit substantial potential for HA management by enabling continuous monitoring, remote participation, and timely support during acute health-related distress [27].

Although substantial evidence supports the effectiveness of digital interventions for general anxiety and depressive disorders, a comprehensive evaluation of digital behavioral interventions (DBIs) specifically targeting HA remains lacking. Recent reviews have provided an extensive synthesis of intervention outcomes for HA among adolescents and their parents [28], and randomized controlled trials (RCTs) have examined a range of digital technological approaches. However, the evidence remains fragmented. No systematic review has yet synthesized the overall effectiveness of DBIs for reducing HA or examined whether their effects vary across populations and study designs.

This systematic review and meta-analysis addresses these gaps by evaluating the clinical effectiveness of DBIs for individuals with HA and their effects on related psychological outcomes, including depression and stress. It also examines the emerging role of DBIs as an adjunctive approach and synthesizes evidence to inform integrated health management strategies beyond conventional cognitive behavioral therapy (CBT).

Methods

Overview

This systematic review was designed in strict accordance with the PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) 2020 statement [29] (Checklist 1) and the most recent guidance of the Cochrane Handbook for Systematic Reviews of Interventions [30]. To ensure transparency and reproducibility, the review protocol was prospectively registered in the PROSPERO (International Prospective Register of Systematic Reviews; registration number CRD420251238746). The research questions were formulated using the PICOS (Population, Intervention, Comparator, Outcomes, and Study design) framework [31], which provided a structured approach to clearly define the scope and focus of the review and to ensure comprehensive and systematic coverage of the topic.

Eligibility Criteria

Inclusion Criteria

Inclusion Criteria: (1) The study designs included RCTs, quasi-experimental studies (QESs), and cohort studies (CSs). Both controlled (ie, with a separate comparator group) and uncontrolled single-arm pre-post designs were considered eligible for inclusion. All studies were required to provide original outcome data and complete procedural descriptions; (2) the participants meeting diagnostic criteria for HA, including DSM-IV hypochondriasis, *DSM-5* SSD with predominant HA features, or IAD, or participants with elevated HA confirmed using validated scales (eg, HAI ≥30, SHAI ≥18, WI-7 >14). Studies were eligible when HA was assessed as a primary target outcome; (3) digital health interventions targeting HA, defined as structured psychological interventions delivered wholly or mainly through digital technology. Eligible approaches included internet-based CBT (guided or unguided), internet-based ACT, cognitive bias modification, and other digital self-help programs, delivered via web-based platforms, mobile apps, e-books or bibliotherapy, video programs, or interactive formats such as chatbots and serious games. Comparators included waitlist, psychoeducation, usual care, or face-to-face therapy; (4) primary outcomes: severity of HA assessed using validated instruments (eg, HAI, SHAI, and Illness Attitude Scales [IASs]). Secondary outcomes: comorbid symptoms (depression and generalized anxiety), functional impairment (Sheehan Disability Scale [SDS] and World Health Organization Disability Assessment Schedule II [WHODAS-II]), quality of life, and treatment adherence/satisfaction; (5) peer-reviewed original articles published in English or Chinese, from database inception to November 1, 2025.

Exclusion Criteria

(1) Noninterventional studies (cross-sectional studies, retrospective analyses, reviews, meta-analyses), no clear definition of groups without sufficient outcome data, duplicate publications, or studies with overlapping data; (2) Studies involving minors or participants without confirmed HA (no DSM diagnosis or subclinical scale scores), or participants with severe psychiatric comorbidities, including psychotic disorders, bipolar disorder, acute suicidal ideation or behavior, or substance use disorders (active within the past 6 months). Studies were also excluded if participants had severe physical illnesses requiring urgent medical intervention or unstable medication use (antidepressants or anxiolytics adjusted within 2 months), or if concurrent psychotherapy targeting HA (eg, face-to-face CBT) was provided during the study period; (3) Intervention types: nondigital interventions (eg, behavioral therapies without digital components or pharmacotherapy alone), or interventions with unclear core components or implementation procedures; (4) Outcomes: absence of primary outcomes related to HA or IAD, incomplete or nonextractable data, or outcome measures lacking psychometric validation; (5) Non-peer-reviewed literature, including conference abstracts, dissertations, and gray literature.

Electronic Databases and Search Strategy

This review aimed to identify studies examining the effects of digital therapeutics (DTs) and digitally assisted behavioral therapies on the mental health outcomes of individuals with HA. Systematic searches were conducted in PubMed, Embase, Web of Science, ScienceDirect, PsycINFO, and the Cochrane Library from database inception to November 1, 2025. The primary search terms for the research question included health anxiety and illness anxiety (ie, “health anxiety” [All Fields] OR “hypochondriasis” [MeSH] OR “hypochondria” [All Fields]). Intervention-related terms included DT, behavioral activation (BA), and DBIs. Gray literature was explicitly excluded (Table 1). The search strategy was independently implemented by 2 reviewers, who initially screened titles and abstracts. To avoid duplication, all retrieved records were imported into the Zotero reference management software [32]. In addition to database searches, supplementary strategies were applied, including snowballing of reference lists, to enhance the comprehensiveness and depth of retrieval. In accordance with the PRISMA guidelines [33], this review primarily included RCTs, supplemented by QES and CS, to systematically evaluate the effectiveness and mechanisms of digital interventions for HA in adults. Highly similar or overlapping studies were excluded (details in Multimedia Appendix 1), and reference lists of relevant prior reviews were also screened to ensure comprehensive inclusion.

Table 1. Search strategies for English and Chinese databases.

Number	Search terms
1	Health anxiety (MeSH)
2	Health disorder
3	Health anxiety disorder (MeSH)
4	Health-related behavior
5	Fear of illness
6	Illness anxiety disorder (MeSH)
7	Hypochondriasis
8	Hypochondria (MeSH)
9	Somatic symptom disorder (MeSH)
10	1 OR 2 OR 3 OR 4 OR 5 OR 6 OR 7 OR 8 OR 9
11	Digital assistant (MeSH)
12	Digital interventions
13	Digital therapy (MeSH)
14	Online intervention
15	Internet-based
16	Remote intervention
17	11 OR 12 OR 13
18	Behavioral activation (MeSH)
19	Behavior activation (MeSH)
20	Behavioral activation therapy
21	Behavioral activation therapies
22	Digital behavioral interventions
23	Digital behavior therapy (MeSH)
24	Digital behavior therapies
25	18 OR 19 OR 20 OR 21 OR 22 OR 23 OR 24
26	10 AND 17 AND 25

Selection of Studies

Duplicate records were systematically identified and removed using Zotero. Following deduplication, all unique records underwent a structured screening process conducted by 2 reviewers. Records that were clearly unrelated to the scope of the review were initially excluded on the basis of their titles. Subsequently, the remaining publications were assessed using their titles, abstracts, and keywords. At this point, any record considered potentially relevant by at least one reviewer, or for which eligibility remained uncertain, was retained for full-text assessment.

The full texts of these potentially eligible reports were then examined in detail against the predefined inclusion and exclusion criteria. In particular, eligibility was determined according to the types of anxiety addressed, the forms and characteristics of the interventions, and the measurement scales used to evaluate the core outcomes. During this process, JX sequentially assessed each article to determine whether it met the eligibility requirements for inclusion. Where disagreements concerning full-text inclusion arose, they were resolved through discussion. However, when consensus could not be reached, GH and JW made the final decision. Overall, this sequential screening procedure enabled the systematic identification and evaluation of relevant studies, while the complete study selection process was documented and reported in accordance with the PRISMA guidelines.

Data Extraction

This review used a structured PICOS-based approach for preliminary data extraction from the included studies. Data were extracted independently by 3 reviewers. Approximately 30% of the extracted data were randomly selected and cross-checked by a fourth reviewer to assess consistency and accuracy. This multireviewer extraction and random verification process was used to assess the reliability of the extracted data. A standardized Excel-based extraction form was developed by the research team based on the PICOS framework to systematically collect core study information.

Population (P): individuals diagnosed with hypochondriasis or IAD, as well as individuals exhibiting moderate levels of HA.

Intervention (I): specific categories of DBIs, alongside detailed descriptions of their implementation, as well as forms of intervention deliverable via the internet.

Comparator (C): characteristics and descriptions of control or comparison interventions.

Outcomes (O): the primary outcome must be directly related to HA, while secondary outcome measures include, but are not limited to, other functional impairments resulting

from health anxiety, as well as treatment adherence and satisfaction.

Study design (S): study type, including RCTs, QES, or CS.

Additionally, during the data extraction process, if any information was unclear or missing, the research team attempted to contact the original authors for additional data to ensure the completeness of extracted information. All extracted data were systematically organized in a standardized Excel sheet for subsequent quality assessment, data synthesis, and analysis to ensure data reliability and consistency.

Statistical Analysis

When studies were sufficiently comparable in terms of interventions, outcomes, and study designs, quantitative synthesis was undertaken. Following Cochrane methodological guidance, multiple reports of the same trial, including secondary analyses, were linked and treated as a single study [34]. When reports provided overlapping data for the same comparison, outcome, and time point, data from the primary trial report were preferentially used. Secondary reports were consulted to supplement eligible data unavailable in the primary report. Each participant was counted only once within each outcome-specific meta-analysis. All analyses were performed using custom-written code in Python (version 3.12; Python Software Foundation) [35]. For each eligible intervention–control comparison, we extracted postintervention means, SDs, and analyzed sample sizes for both groups. End point values were used consistently, as both end point and change scores are acceptable for continuous-outcome meta-analysis when appropriately analyzed [30].

The effect measure was selected according to the outcome construct and measurement scale. Health-anxiety outcomes assessed using different instruments or different versions of the same instrument, including the Health Anxiety Inventory (HAI) and its short form, the Short Health Anxiety Inventory (SHAI), were pooled using standardized mean differences (SMDs), because these measures assess the same underlying construct but have different scoring ranges [30,36]. SMDs were estimated as Hedges g, which applies a small-sample correction to Cohen d at the comparison level [37]. Mean differences (MDs) were used when the same outcome was assessed using the same instrument and scoring metric. Score directions were harmonized before analysis. Other prespecified outcomes, including illness attitudes, general anxiety, anxiety sensitivity, and depressive symptoms, were analyzed separately and were not combined with the primary health-anxiety outcome.

For comparison i, the MD and its sampling variance were calculated as follows:

$$MD_i = \bar{X}_{I,i} - \bar{X}_{C,i}$$

$$V_{MD,i} = \frac{SD_{I,i}^2}{n_{I,i}} + \frac{SD_{C,i}^2}{n_{C,i}}$$

For SMD analyses, the pooled SD across the 2 groups, Hedges g, and its sampling variance were calculated using the RevMan algorithm as follows [38]:

$$S_{D_p,i} = \sqrt{\frac{(n_{I,i}-1)S_{I,i}^2 + (n_{C,i}-1)S_{C,i}^2}{n_{I,i}+n_{C,i}-2}}$$

$$g_i = \left|1 - \frac{3}{4(n_{I,i}+n_{C,i})-9}\frac{\dot{X}_{I,i} - \dot{X}_{C,i}}{SD_{p,i}}\right|$$

$$V_{g,i} = \frac{n_{I,i}+n_{C,i}}{n_{I,i}n_{C,i}} + \frac{g_i^2}{2(n_{I,i}+n_{C,i}-3.94)}$$

Here, I and C denote the digital intervention and control groups, respectively; $\bar{X}$, SD, and n denote the postintervention mean (SD), and number of participants analyzed; and VMD,i and Vg,i denote the corresponding sampling variances. Before pooling, all outcome scales were oriented so that lower scores represented better outcomes; consequently, negative effect estimates indicated effects favoring the digital intervention.

Because the underlying intervention effects were expected to vary across populations, intervention formats, treatment durations, and comparator conditions, an inverse-variance random-effects model was prespecified [38,39]. The between-study variance was estimated using the DerSimonian–Laird method, and the random-effects weight assigned to comparison i was calculated as

$$w_i = \frac{1}{V_i + \tau^2}$$

where V_i denotes the sampling variance of the corresponding MD or SMD. Pooled effects were reported with 2-sided Wald 95% CIs, and $P<.05$ was considered statistically significant. Model selection was not based on an I^2 threshold, because current guidance recommends selecting the statistical model according to assumptions about the underlying effects rather than solely according to a statistical test or threshold for heterogeneity [38].

Robustness was examined using a leave-one-out sensitivity analysis. Within each outcome meta-analysis, each eligible trial-specific comparison was omitted in turn, and the random-effects meta-analysis was refitted using the remaining comparisons. The pooled effect, 95% CI, τ^2, Cochran Q, I^2 and comparison weights were recalculated after each omission. A trial-specific comparison was considered potentially influential if its omission materially changed the magnitude or direction of the pooled effect, caused the CI to cross the null value, or substantially altered the estimated heterogeneity [40].

Quality Appraisal of the Included Studies

The methodological quality of the included studies was appraised with the Joanna Briggs Institute (JBI) critical appraisal checklists matched to each study design [41]. The 13-item checklist for RCTs covers randomization and allocation concealment, baseline comparability, blinding of participants, treatment providers, and outcome assessors, equal treatment of the groups apart from the intervention, completeness of follow-up, analysis in the randomized groups, consistent and reliable outcome measurement, and appropriate statistical analysis and trial design. The 9-item checklist for QESs addresses clarity of the cause-and-effect relationship, the presence of a control group, comparability of participants and of the care they received, repeated pre- and postintervention measurements, consistent and reliable outcome measurement, completeness of follow-up, and appropriate statistical analysis. The 11-item checklist for CSs addresses similarity and recruitment of the comparison groups, valid and reliable exposure measurement, identification and handling of confounders, absence of the outcome at baseline, valid and reliable outcome measurement, duration and completeness of follow-up, and appropriate statistical analysis. Following thresholds commonly applied in previous systematic reviews [42] using the JBI checklists, studies meeting ≥70% of applicable items were rated high quality, 50%-69% moderate, and <50% as low quality. For fully applicable checklists, these correspond to total scores of 10-13, 7-9, and ≤6 (RCTs); 7-9, 5-6, and ≤4 (quasi-experimental); and 8-11, 6-7, and ≤5 (Cohort). The full wording of each item is given in the footnotes to Table 2. Each item was rated as yes, no, unclear, or not applicable. One point was assigned for each yes response and zero points; otherwise, items rated not applicable were excluded from the denominator, and overall quality was expressed as the percentage of applicable items meeting the criteria. Item-level ratings, total scores, and overall classifications for all 16 studies are presented in Table 2. Two reviewers (JX and JS) performed the appraisal independently, interrater agreement was quantified with Cohen kappa, and disagreements were resolved through discussion with a third reviewer (JW). No study was excluded on the basis of its quality rating [43]. The risk of bias of the RCTs and the certainty of the pooled evidence were further assessed.

Table 2. Quality of evidence in the included 16 reports based on JBI[a]'s critical appraisal tools.

	Evaluation for evidence reported													
Study design and year	1	2	3	4	5	6	7	8	9	10	11	12	13	Total score
RCT[b]														
Hedman et al (2016) [44]	Y[c]	Y	Y	N[d]	N	N	Y	Y	Y	Y	Y	Y	Y	10
Hedman et al (2011) [45]	Y	Y	Y	N	N	Y	Y	Y	Y	Y	Y	Y	Y	11
Newby et al (2018) [46]	Y	Y	Y	Y	N	N	Y	Y	Y	Y	Y	Y	Y	11
Axelsson et al (2020) [47]	Y	Y	Y	N	N	N	Y	Y	Y	Y	Y	Y	Y	10
Newby and McElroy (2020) [48]	Y	Y	Y	N	N	N	Y	Y	Y	Y	Y	Y	Y	10
Hoffmann et al (2021) [49]	Y	Y	Y	N	N	Y	Y	Y	Y	Y	Y	Y	Y	11
Du et al (2024) [50]	Y	Y	Y	N	N	N	Y	Y	Y	Y	Y	Y	Y	10
O'Bryan et al (2021) [51]	Y	N	Y	Y	N	N	Y	Y	Y	Y	Y	Y	Y	10
Hedman et al (2014) [52]	Y	Y	Y	N	N	N	Y	Y	Y	Y	Y	Y	Y	10
n/N (%)	(9/9) 100	(8/9) 89	(9/9) 100	(2/9) 22	(0/9) 0	(2/9) 22	(9/9) 100	(9/9) 100	(9/9) 100	(9/9) 100	(9/9) 100	(9/9) 100	(9/9) 100	—
QES[e]														
Ingeman et al (2025) [53]	Y	N	Y	Y	Y	Y	Y	N	Y	—[f]	—	—	—	7
Shor et al (2023) [54]	Y	N	Y	Y	Y	Y	Y	—[f]	Y	—	—	—	—	7
Sharrock et al (2021) [55]	Y	N	Y	Y	Y	Y	Y	—	Y	—	—	—	—	7

Study design and year	Evaluation for evidence reported													Total score
	1	2	3	4	5	6	7	8	9	10	11	12	13	
Goonesekera and Donkin (2022) [56]	Y	N	Y	Y	Y	Y	Y	N	Y	—	—	—	—	7
Hoffmann et al (2018) [57]	Y	N	Y	Y	Y	Y	Y	N	Y	—	—	—	—	7
Frydendal et al (2025) [58]	Y	N	Y	Y	Y	Y	Y	—	Y	—	—	—	—	7
n/N (%)	(6/6) 100	(0/6) 0	(6/6) 100	(6/6) 100	(6/6) 100	(6/6) 100	(6/6) 100	(6/6) 0	(6/6) 100	—	—	—	—	—
CS[g]														
Österman et al (2024) [59]	—	—	Y	Y	Y	Y	Y	Y	N	Y	Y	—	—	8
n/N (%)	0	0	100	100	100	100	100	100	0	100	100	—	—	—

[a]JBI: Joanna Briggs Institute.
[b]RCT: randomized controlled trial.
[c]Y: yes.
[d]N: no.
[e]QES: quasi-experimental studies.
[f]Not applicable.
[g]CS: cohort study.

Quality Assessment of Evidence and Risk of Bias

The risk of bias of the included RCTs was assessed with the revised Cochrane Risk of Bias 2 tool for randomized trials (RoB 2) [60]. The tool covers 5 domains. These are bias arising from the randomization process, bias due to deviations from intended interventions, bias due to missing outcome data, bias in measurement of the outcome, and bias in selection of the reported result. Each domain was judged as low risk of bias, some concerns, or high risk of bias. Because RoB 2 is applied to a specific result rather than to a trial as a whole, the assessment addressed the primary outcome of this review, namely HA symptoms at postintervention as measured with the HAI, the SHAI, or the IADs in each trial. An overall judgment was then derived for each trial following the RoB 2 algorithm. A trial was rated as having a low risk when all 5 domains were judged low risk, as some concerns when at least one domain raised some concerns without any high risk judgment, and as high risk when any domain was judged high risk or when several domains raised some concerns to a degree that substantially reduced confidence in the result. Two reviewers independently conducted all assessments in accordance with the Cochrane Handbook for Systematic Reviews of Interventions, and disagreements were resolved through discussion or consultation with a third reviewer. The methodological quality of quasi-experimental and CSs was appraised with the JBI checklists, as described in section "Quality Appraisal of the Included Studies."

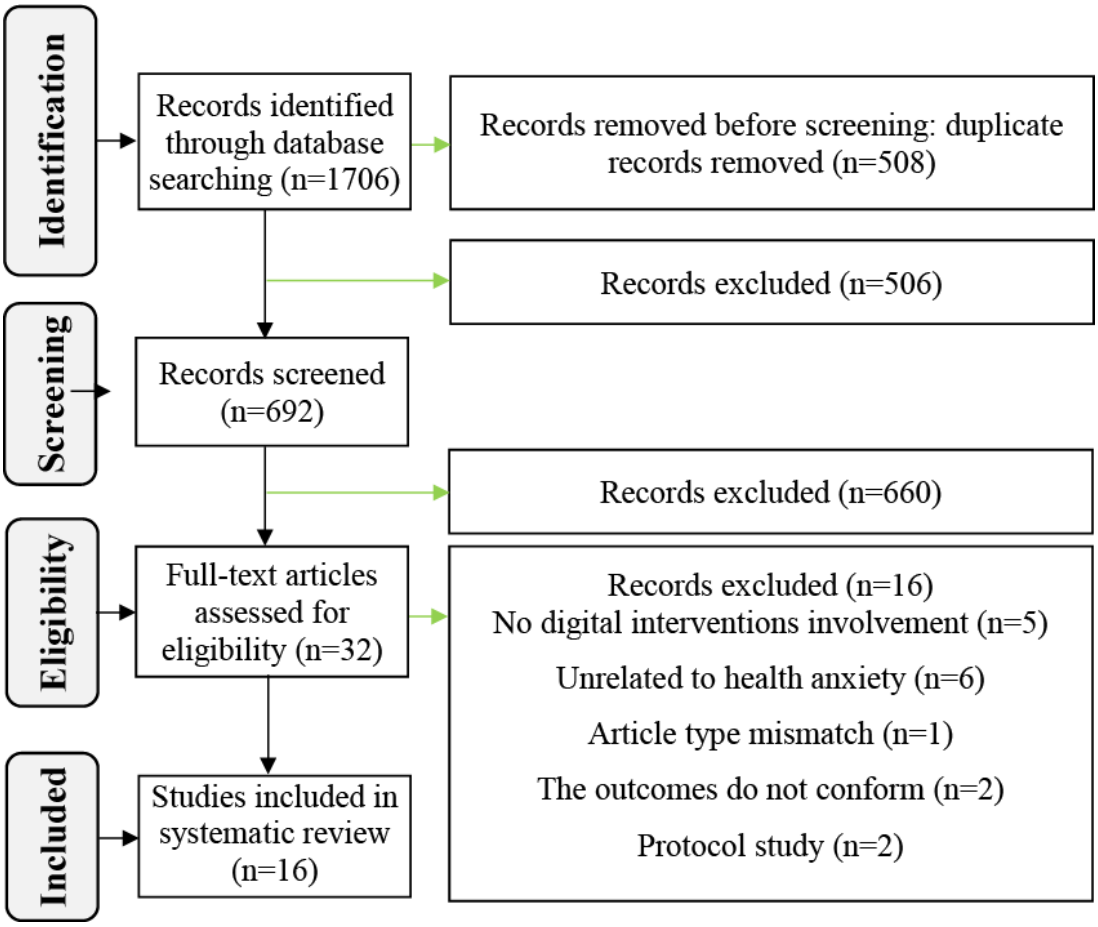
Result

Search Results and Study Characteristics

Overview

A comprehensive search was conducted across 6 electronic databases (PubMed, Embase, Web of Science, ScienceDirect, PsycINFO, and the Cochrane Central Register of Controlled Trials [CENTRAL]). A total of 1706 records were identified through the database searches. After 508 duplicate records were removed, 1198 unique records remained. Title screening excluded 506 records, leaving 692 records for abstract screening. Of these, 660 records were excluded, and the full texts of 32 articles were assessed for eligibility. Sixteen full-text articles were subsequently excluded because they involved nondigital interventions (n=6), did not target HA (n=5), had an ineligible article type (n=1), did not report eligible outcome measures (n=2), or were study protocols (n=2). Ultimately, 16 studies met the predefined eligibility criteria and were included in the systematic review. The study-selection process is presented in Figure 1.

Figure 1. The PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) 2020 flow diagram for the included and excluded articles.



The 16 included studies were published between 2011 and 2025 and comprised 9 RCTs, 6 QESs, and one CS. Five studies were conducted in Sweden [44,45,47,52,59], 4 in Denmark [49,53,57,58], 3 in Australia [46,48,55], and 1 each in New Zealand [56], China [50], Japan [54], and the United States [51]. A total of 2581 participants were included, with individual study sample sizes ranging from 4 to 904. Intervention duration was most commonly 12 weeks (n=10) [44-49,52,57-59]. The remaining interventions comprised an 8-week program (n=1) [58], a single-session lasting 12 minutes (n=1) [54], a 14-day chatbot program (n=1) [56], a 12-day intervention (n=1) [50], a 1-week computerized intervention (n=1) [51], and a flexible program that allowed participants up to 90 days for completion (n=1) [55]. By study design, the evidence base included RCTs (n=9) [44-52], QESs (n=6) [53-58] and one CS (n=1) [59]. Study populations included adults with SSD/IAD or hypochondriasis (n=4) [44,51,52,59], severe HA (n=5) [29,45,46,52,57], HA without a specific diagnostic label (n=4) [47,50,55,56], COVID-19-related HA (n=1) [58], HA with cyberchondria (n=1) [48], and HA associated with inflammatory bowel disease (n=1) [54], as well as parents with HA by proxy (n=1) [53]. These population categories were not mutually exclusive.

Delivery mode and intervention approach were considered separately. Regarding delivery support, guided digital interventions were evaluated in 11 studies, whereas unguided interventions were evaluated in 6 studies. One study included both guided and unguided intervention arms. Therefore, these categories were not mutually exclusive. Regarding intervention approach, 8 studies evaluated internet-delivered CBT, including exposure-based CBT. Three studies evaluated internet-delivered acceptance and commitment therapy. One study each evaluated an integrated CBT and acceptance and commitment therapy (ACT) program, a CBT-based chatbot, internet-based cognitive bias modification of interpretations, a video-testimonial intervention, and a brief computerized intervention combining psychoeducation with interpretation-bias modification.

The primary or co-primary outcomes across the included studies included core HA, illness-related attitudes, generalized anxiety, anxiety sensitivity, and depressive symptoms. Core HA was most commonly assessed using the SHAI (n=8) [45,46,48,50,51,55,56,59] and the HAI (n=4) [44,45,47,52]. Illness-related attitudes were assessed using the IAS (n=4) [44,45,47,52], generalized anxiety using the Beck Anxiety Inventory (BAI; n=4) [44,45,47,52], anxiety sensitivity using the Anxiety Sensitivity Index (ASI; n=4) [44,45,47,52], and depressive symptoms using the Montgomery–Åsberg Depression Rating Scale–Self-report (MADRS-S; n=5) [44,45,47,52,59]. Because several studies assessed more than one primary or co-primary outcome, these categories were not mutually exclusive. Detailed outcome measures and study characteristics are presented in Multimedia Appendix 2.

JBI Critical Appraisal Checklist for RCT

The methodological quality of the included randomized controlled trials was assessed using the JBI Critical Appraisal Checklist for Randomized Controlled Trials, which comprises 13 items addressing randomization, allocation concealment, blinding, group comparability, follow-up, outcome measurement, statistical analysis, and trial design. (1) Was true randomization used for assignment of participants to treatment groups? (2) Was allocation to treatment groups concealed? (3) Were treatment groups similar at the baseline? (4) Were participants blind to treatment assignment? (5) Were those delivering treatment blind to treatment assignment? (6) Were outcomes assessors blind to treatment assignment? (7) Were treatment groups treated identically other than the intervention of interest? (8) Was follow-up complete and if not, were differences between groups in terms of their follow-up adequately described and analyzed? (9) Were participants analyzed in the groups to which they were randomized? (10) Were outcomes measured in the same way for treatment groups? (11) Were outcomes measured in a reliable way? (12) Was appropriate statistical analysis used? (13) Was the trial design appropriate, and any deviations from the standard RCT design (individual randomization, parallel groups) accounted for in the conduct and analysis of the trial?

JBI Critical Appraisal Checklist for QESs

The JBI critical appraisal checklist for QESs consists of the following questions: (1) Is it clear in the study what the "cause" is and what the "effect" is (ie, there is no confusion about which variable comes first)? (2) Was there a control group? (3) Were participants included in any comparisons similar? (4) Were the participants included in any comparisons receiving similar treatment/care, other than the exposure or intervention of interest? (5) Were there multiple measurements of the outcome, both pre and post the intervention/exposure? (6) Were the outcomes of participants included in any comparisons measured in the same way? (7) Were outcomes measured in a reliable way? (8) Was follow-up complete and if not, were differences between groups in terms of their follow-up adequately described and analyzed? (9) Was appropriate statistical analysis used?

JBI Critical Appraisal Checklist for CSs

The JBI critical appraisal checklist for CSs consists of the following questions: (1) Were the 2 groups similar and recruited from the same population? (2) Were the exposures measured similarly to assign people to both exposed and unexposed groups? (3) Was the exposure measured in a valid and reliable way? (4) Were confounding factors identified? (5) Were strategies to deal with confounding factors stated? (6) Were the groups/participants free of the outcome at the start of the study (or at the moment of exposure)? (7) Were the outcomes measured in a valid and reliable way? (8) Was the follow-up time reported and sufficient to be long enough for outcomes to occur? (9) Was follow-up complete, and if not, were the reasons for loss to follow-up described and explored? (10) Were strategies to address incomplete follow-up used? (11) Was appropriate statistical analysis used?

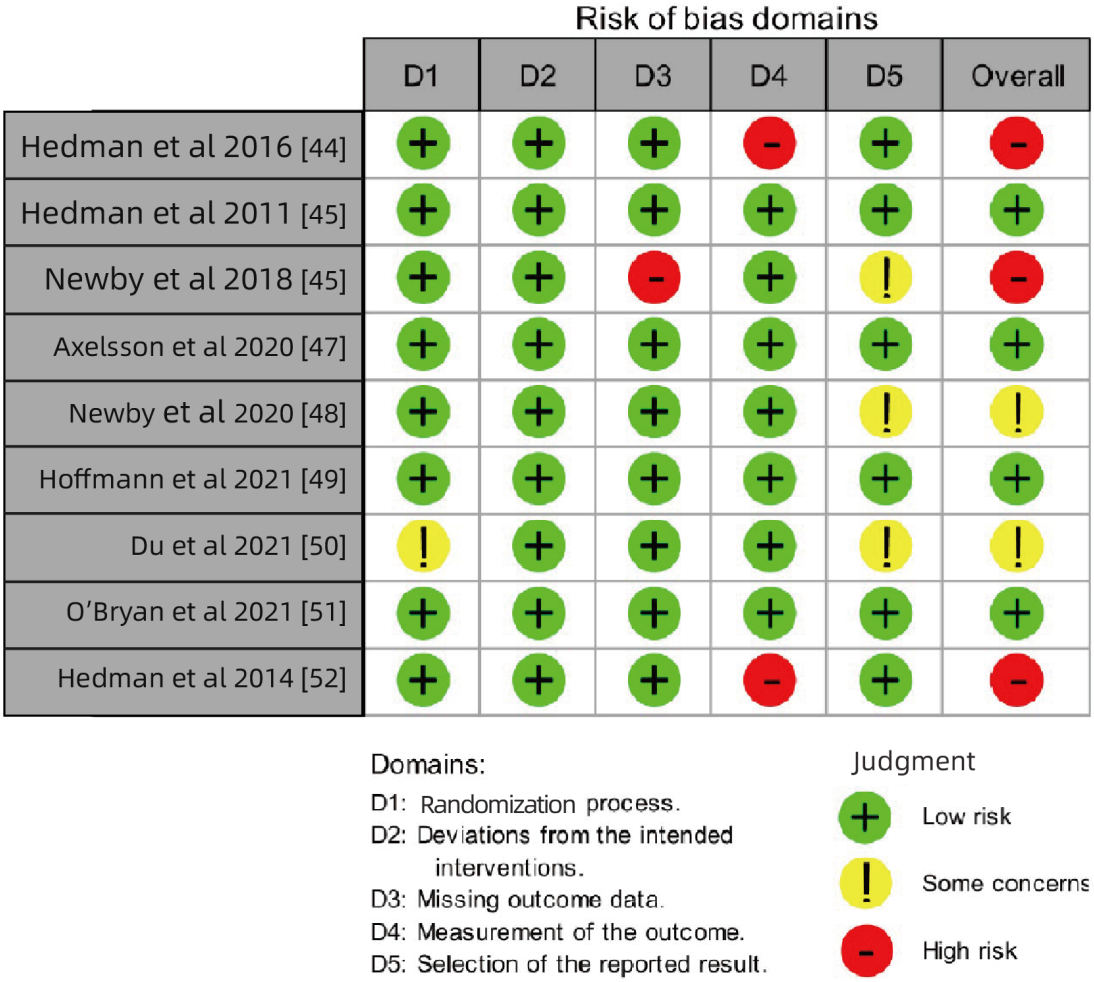
Methodological Quality and Risk of Bias

Two reviewers independently assessed the quality of the included studies (see Table 2), and the results showed an interrater agreement of 87.58% (κ=0.81). The 9 RCTs achieved total scores of 10 to 11, and all were classified as high quality. The trials by Hedman et al [45], Newby et al [46], and Hoffmann et al [49] received the highest score of 11, while the remaining studies each scored 10 [44,47,48,50-52]. Across individual assessment domains, RCTs generally demonstrated strong methodological performance in terms of randomization, baseline comparability, completeness of follow-up, consistency and reliability of outcome measurement, and appropriateness of study design. The principal weakness concerned blinding: participants were blinded in 22% of trials, reflecting the inherent difficulty of blinding in digital intervention research. Most studies implemented blinding only at specific stages rather than across the entire trial process. For QES, all 6 studies [53-58] achieved favorable quality scores, meeting high-quality standards regarding causal clarity, participant comparability, consistency and reliability of outcome measurement, and appropriateness of statistical analyses. Nonetheless, incomplete data reporting and lack of a control group remained common limitations. Quality assessment of the single CS [59] rated it as high quality (8 of 9 applicable items); items 1 and 2 were not applicable because a single treated cohort was enrolled without an unexposed comparison group and follow-up was incomplete, which should be considered when interpreting its findings.

This review focused on HA outcomes and assessed all included RCTs using 5 criteria of the Cochrane Collaboration risk-of-bias assessment tool [61]. Of the 16 included studies, the 9 RCTs were assessed for the HA outcome using the 5-domain Cochrane RoB 2 tool (Figure 2). Overall, 4 trials were rated as having a low risk of bias, 2 as having some concerns, and 3 as having a high risk of bias. For the randomization process (D1), 8 trials were rated as low risk and one as having some concerns. All 9 trials were rated as low risk for deviations from intended interventions (D2). For missing outcome data (D3), 8 trials were rated as low risk and one as high risk. For measurement of the outcome (D4), 7 trials were rated as low risk and 2 as high risk. For selection of the reported result (D5), 6 trials were rated as low risk and 3 as having some concerns. High-risk judgments were recorded for Newby et al [46] in D3 and for Hedman et al [44] and Hedman et al [52] in D4. Some concerns were identified for Du et al [50] in D1 and for Newby et al [46], Newby and McElroy [48], and Du et al [50] in D5. Newby et al [46] were judged to be at high risk in D3 because 16 of 102 randomized participants were excluded before baseline assessment and only 69 provided posttreatment data. The authors also acknowledged that the outcomes of participants with missing data were unknown and that treatment effects might therefore have been overestimated. Hedman et al [44] was rated as having a high risk of bias in D4 because the primary outcome was assessed using

an unblinded, participant-reported measure, and comparison with a waiting-list control could have influenced symptom reporting. The D4 judgment for Hedman et al [52] was similarly based on the use of an unblinded self-reported HA measure, although both groups received active interventions with comparable treatment credibility.

Figure 2. Cochrane risk of bias ratings for included randomized controlled trials (n=9) [44-52].



Results of Meta-Analysis

All 6 meta-analyses reported in this section were restricted to RCTs. The 3 quasi-experimental trials, the single CS, and the replicated single-case experimental design are summarized narratively in Table 3 and were not pooled with the RCT effect estimates. All outcomes were synthesized using prespecified random-effects models with the DerSimonian-Laird estimator (Figure 3). Because HA was measured using both the 64-item HAI (HAI-64 [44,45,52]) and the 18-item SHAI (SHAI-18 [46,50,51]), posttreatment effects were standardized and pooled using Hedges *g*. Across 6 trials involving 585 participants, digital interventions produced a statistically significant, moderate-to-large reduction in HA relative to control conditions (Hedges g=–0.77; 95% CI –1.25 to –0.30; z=–3.17; P=.002) [44-46,50-52]. All trial-level point estimates favored digital interventions and ranged from –1.60 to –0.21, although the CIs of 3 individual trials included zero. Considerable between-trial heterogeneity was observed (τ^2=0.30; Q_5=37.55; P<.001; I^2=86.7%) indicating substantial variation in the magnitude of the intervention effects.

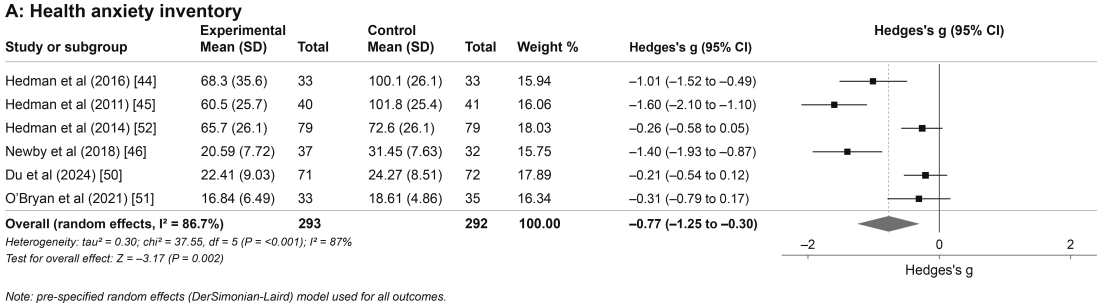
Table 3. Different digital interventions for health anxiety participants.

Guide type and study	Country	Interventions participants	Period of interventions	Primary outcome	Research type	Follow-up	Result
Guided digital therapy							
Hedman et al (2016)[a] [44]	Sweden	32 (female: 24; male: 8)	12-week	HAI[b]	RCT[c]	Yes, 6-month	Significant decrease
Hedman et al (2011) [45]	Sweden	40 (female: 28; male:12)	12-week	HAI/SHAI[d]	RCT	Yes, 6-month	Significant decrease

Guide type and study	Country	Interventions participants	Period of interventions	Primary outcome	Research type	Follow-up	Result
Hedman et al (2014) [52]	Sweden	64 (female: 64; male: 15)	12-week	HAI	RCT	Yes, 6-month	Significant decrease
Newby et al (2018) [46]	Australia	45 (female: 40; male: 5)	12-week	SHAI	RCT	Yes, 3-month	Significant decrease
Axelsson et al (2020) [47]	Sweden	102 (female: 72; male: 30)	12-week	HAI-18[e]	RCT	Yes, 12-month	Significant decrease
Newby Jand McElroy (2020) [48]	Australia	45 (female: 40; male: 5)	12-week	SHAI/CSS[f]	RCT	Yes, 3-mouth	Significant decrease
Österman et al (2024) [59]	Sweden	447 (female: 305; male: 142)	12-week	CSS	CS[g]	Yes, 6-mouth	Significant decrease
Ingeman et al 2022 [53]	Denmark	4 (female: 3; male: 1)	8-week	SHAI/HAI	Single-case experimental design	Yes, 14-33 days	2 patients showed significant improvement in main outcomes, while one did not; the anxiety symptoms of 2 patients were reduced
Hoffmann et al (2018) [57]	Denmark	15 (female: 12; male: 3)	12-week	WI-7[h]	CS	Yes, 3-month	Significant decrease
Frydendal et al (2025) [58]	Denmark	105 (female: 92; male: 13)	12-week	WI-6R[i]	Pragmatic study	No	Significant decrease
Hoffmann et al (2021) [49]	Denmark	101 (iAC: 53; female: 34; male: 19); iFORUM 48 (female: 32; male: 16)	12-week	WI-7	RCT	Yes, 6-month	Significant decrease
Unguided digital therapy							
Hedman et al (2016) [44]	Sweden	67 (U-ICBT: 33; female: 24; male: 9); bibliotherapy: 34 (female: 24; male: 10)	12-week	HAI	RCT	Yes, 6-month	Significant decrease
Sharrock et al (2021) [55]	Australia	904 (female: 607; male: 220; unknown: 32)	90-day	SHAI	Multiple sets of pre- and posttests	No	Significant decrease
Goonesekera and Donkin (2022) [56]	New Zealand	29 (female: 25; male: 4)	14-day	SHAI	Pilot study	Yes, 12-week	Not significant
Shor et al (2023) [54]	Japan	100 (female: 37; male: 63)	Single session (12 minutes)	VAS-A[j]	CS	No	Significant decrease
Du et al (2023) [50]	China	228 (female: 142; male: 86)	12 days	SHAI	RCT	Yes,1 mouth	Significant decrease
O'Bryan et al (2021) [51]	United States	68 (male 14; female: 54)	7-day (single session)	SHAI/ASI-3[k]	RCT	Yes, 1 week and a month	Not significant

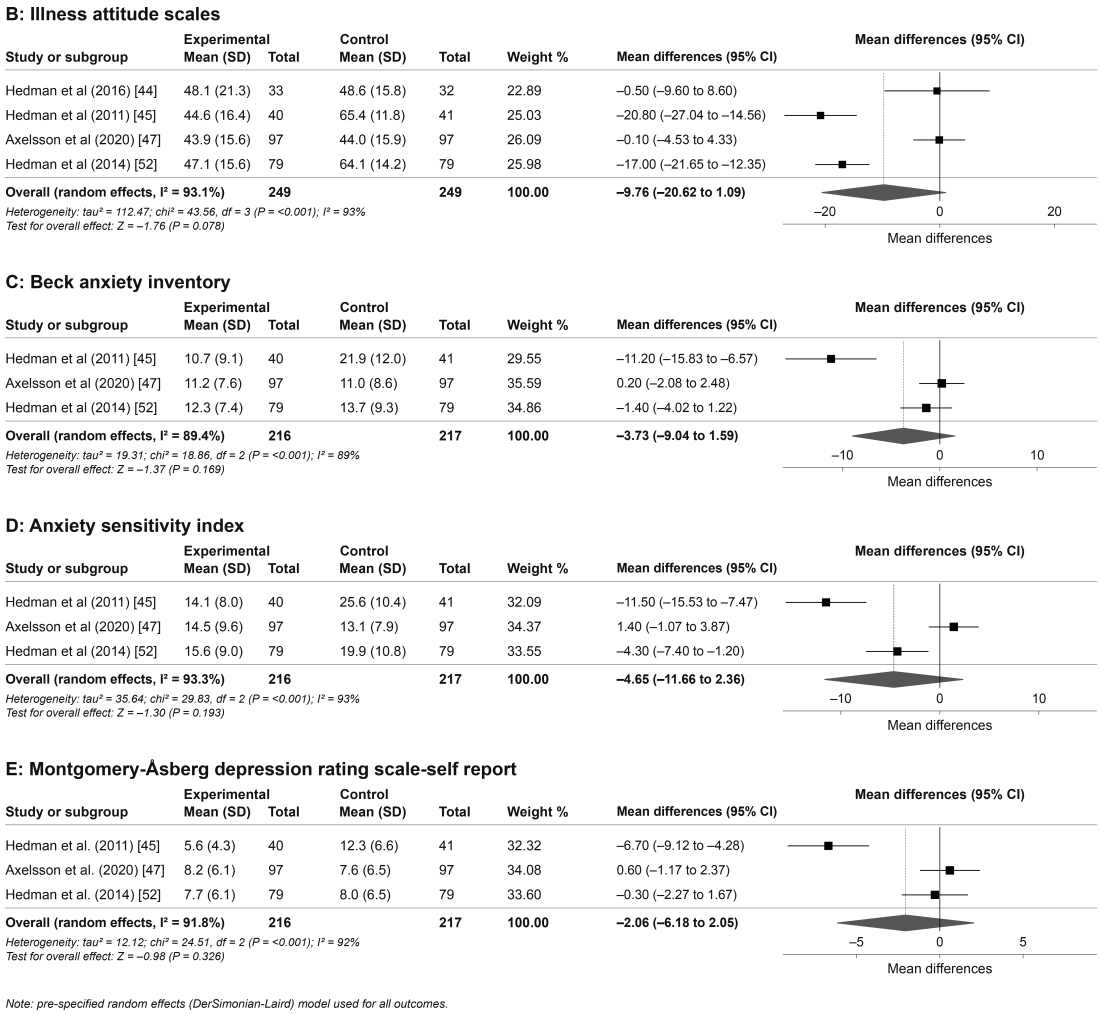
[a]Article "Exposure-based cognitive–behavioural therapy via the internet and as bibliotherapy for somatic symptom disorder and illness anxiety disorder: the randomized controlled trial" discussed 3 intervention groups: ICBT, U-ICBT, and Bibliotherapy. Since both guided and unguided intervention formats are involved for each group, they appear twice in the article.
[b]HAI: Health Anxiety Inventory.
[c]RCT: randomized controlled trial.
[d]SHAI: Short Health Anxiety Inventory.
[e]HAI-18: Health Anxiety Inventory-18 item.
[f]CSS: Cyberchondria Severity Scale.
[g]CS:
[h]WI-7: Whiteley-7 Index.
[i]WI-6R: Whiteley 6-item revised.
[j]VAS-A: visual analog scale for anxiety.
[k]ASI-3: Anxiety Sensitivity Index-3.

Figure 3. Effects of digital behavior interventions for health anxiety outcomes (standardized mean differences [SMDs]) [44-46,50-52].



For outcomes assessed using the same measurement instrument, mean differences (MDs) were pooled. Four trials involving 498 participants assessed illness-related attitudes using the IAS (Figure 4). Although the pooled estimate favored digital interventions, the effect was not statistically significant (MD –9.76, 95% CI –20.62 to 1.09; z=–1.76; *P*=.08) [44,45,47,52]. Similarly, no statistically significant posttreatment effect was observed for general anxiety measured using the Beck Anxiety Inventory (BAI; 3 trials, n=433; MD –3.73, 95% CI –9.04 to 1.59; z=–1.37; *P*=.17) [44,45,47,52]. Anxiety sensitivity measured using the ASI (3 trials, n=433) also did not differ significantly between digital interventions and control conditions (MD –4.65, 95% CI –11.66 to 2.36; z=–1.30; *P*=.20) [45,47,52]. Likewise, the pooled effect on depressive symptoms measured using the MADRS-S (3 trials, n=433) was not statistically significant (MD –2.06, 95% CI –6.18 to 2.05; z=-0.98; *P*=.33) [45, 47,52]. Thus, although all pooled point estimates were in the direction of lower symptom scores following digital interventions, the corresponding CIs included zero.

Figure 4. Effects of digital behavioral interventions on secondary psychological outcomes (mean differences [MDs]) [44,45,47,52].



Substantial heterogeneity was present for the IAS: τ^2=112.47; Q_3=43.56; *P*<.001; I^2=93.1% [44,45,47,52], BAI: τ^2=19.31; Q_2=18.86; *P*<.001; I^2=89.4% [45,47,52], ASI: τ^2=35.64; Q_2=29.83; *P*<.001; I^2=93.3% [45,47,52], and MADRS-S: τ^2=12.12; Q_2=24.51; *P*<.001; I^2=91.8% [45,47,52].

Leave-one-out sensitivity analyses were performed by sequentially removing each trial and recalculating the pooled effects using the same random-effects model. For the primary health-anxiety outcome, the pooled effect remained statistically significant after the exclusion of any individual trial. The recalculated effect sizes ranged from Hedges g=–0.90 to Hedges g=–0.60, and all 95% CIs excluded zero, supporting the robustness of the overall treatment effect [44-46,50-52]. The smallest pooled effect was obtained after excluding the trial by Hedman et al [45], Hedges g=–0.60, 95% CI –1.02 to –0.18, whereas the largest effect was obtained after excluding the trial by Du et al [50], Hedges g=–0.90, 95% CI –1.46 to –0.34. However, heterogeneity remained substantial across all leave-one-out iterations, with I^2 values ranging from 80.4% to 88.9%, indicating that no single trial fully explained the observed between-trial variability.

The sensitivity analyses produced less stable findings for some secondary outcomes. For the IAS, leave-one-out estimates ranged from MD –13.44 to MD –6.08, with I^2 values ranging from 85.3% to 94.9%. The pooled effect generally remained nonsignificant; however, after the exclusion of the trial by Axelsson et al [47], the effect became statistically significant, MD –13.44, 95% CI –23.14 to –3.74; I^2=85.3%. This result indicates that the pooled IAS finding was sensitive to the inclusion of the Axelsson et al [47] trial, although substantial residual heterogeneity remained.

For the BAI, all leave-one-out estimates remained statistically nonsignificant and ranged from MD –6.11 to MD –0.49, supporting the stability of the primary null finding [45,47,52]. Notably, excluding the trial by Hedman et al [45] reduced the pooled effect to MD –0.49, 95% CI –2.21 to 1.23, and eliminated the observed heterogeneity, I^2=0%. In contrast, heterogeneity remained high when either the Axelsson et al [47] or Hedman et al [52] trial was excluded, I^2=92.3% and 94.7%, respectively. These findings suggest that the Hedman et al [45] trial accounted for a substantial proportion of the heterogeneity in the BAI analysis.

For the ASI, leave-one-out estimates ranged from MD –7.78 to MD –1.37, with residual heterogeneity remaining substantial in all analyses, I^2=87.0%-96.5% [45,47,52]. Exclusion of the Axelsson et al [47] trial resulted in a statistically significant pooled effect, MD –7.78, 95% CI –14.83 to –0.73; I^2=87.0%, whereas the effect remained nonsignificant following the exclusion of either the Hedman et al [45] or Hedman et al [52] trial. Therefore, similar to the IAS finding, the ASI result was sensitive to the inclusion of the Axelsson et al [47] trial.

For the MADRS-S, the pooled estimates remained nonsignificant regardless of which trial was removed and ranged from MD –3.46 to MD 0.20 [45,47,52]. Excluding the trial by Hedman et al [45] shifted the pooled estimate toward the null, MD 0.20, 95% CI –1.12 to 1.51, and reduced heterogeneity from I^2=91.8% to 0%. By comparison, heterogeneity remained high after excluding the Axelsson et al [47] or Hedman et al [52] trial, I^2=93.8% and 95.6%, respectively. This pattern suggests that the Hedman et al [45] trial exerted a substantial influence on both the direction of the pooled MADRS-S estimate and the observed heterogeneity, although its exclusion did not alter the conclusion regarding statistical nonsignificance.

Overall, the leave-one-out analyses supported the robustness of the significant posttreatment effect on HA and the nonsignificant findings for the BAI and MADRS-S. In contrast, the IAS and ASI findings were less stable because the exclusion of the Axelsson et al [47] trial changed their pooled effects from nonsignificant to statistically significant. These secondary-outcome results should therefore be interpreted cautiously, particularly given the small number of contributing trials and the persistently high heterogeneity.

Although the consistently high values indicated marked between-trial heterogeneity, they should not be interpreted as evidence of moderation. No formal subgroup-difference tests or meta-regression analyses were conducted. Differences in scoring metrics between the HAI-64 and SHAI-18 were addressed by pooling standardized effect estimates, whereas the leave-one-out analyses evaluated the influence of individual trials; neither procedure constituted a moderator analysis.

Grading of Recommendations Assessment, Development, and Evaluation Assessment

This study used the Grading of Recommendations Assessment, Development, and Evaluation (GRADE) approach to assess the certainty of evidence from the meta-analysis results (Table 4). Overall, the certainty of evidence for the 5 outcomes ranged from low to very low, and all 5 outcomes were informed exclusively by RCTs. Specifically, the meta-analysis of 6 RCTs regarding HA was rated as low-certainty evidence. Although the pooled estimate favored the intervention and the 95% CI did not include the null value, so that the estimate was not downgraded for imprecision, the rating was lowered because of risk-of-bias concerns across the included trials and substantial unexplained heterogeneity (I^2=86.7%). The meta-analysis for illness attitudes (IAS) was based on 4 RCTs and was rated as very low-certainty evidence, owing to risk-of-bias concerns, materially different point estimates across trials with considerable heterogeneity, and imprecision of the pooled estimate. General anxiety symptoms (BAI), anxiety sensitivity (ASI), and depressive symptoms (MADRS-S) were each assessed on the basis of 3 RCTs, and each was rated as very low-certainty evidence for the same 3 reasons: risk of bias, inconsistency, and imprecision, with the 95% CI in each case including both no effect and a potentially important benefit. No outcome was downgraded for indirectness, and publication bias was not formally assessed because fewer than 10 trials contributed to each outcome. Overall, the GRADE assessment indicates that the current evidence for digital interventions in HA remains limited. Although the pooled estimate for health anxiety favored the intervention, confidence in the effects across outcomes is constrained by methodological limitations of the contributing trials, between-study heterogeneity, and limited precision. Overall, the available evidence suggests that digital interventions may reduce HA symptoms; however, substantial

uncertainty remains regarding the magnitude of the true effect and their effects on other psychological outcomes.

Table 4. GRADE[a] assessment of the certainty of evidence for the effects of digital interventions for health anxiety compared with control conditions.

Outcome	Number of studies	Study design	Risk of bias	Inconsistency	Indirectness	Imprecision	Publication bias	Participants, intervention/control (n)	Effect estimate (95% CI)	Certainty
Health Anxiety Inventory	6	Randomized trials	Serious[b]	Serious[c]	Not serious[d]	Not serious[e]	Undetected[f]	293/292 (n=585)	Hedges *g*=–0.77 (–1.25 to –0.30)	Low
Illness Attitude Scales	4	Randomized trials	Serious[b]	Serious[g]	Not serious[d]	Serious[h]	Undetected[f]	249/249 (n=498)	MD[i]=–9.76 (–20.62 to 1.09)	Very low
Beck Anxiety Inventory	3	Randomized trials	Serious[j]	Serious[g]	Not serious[d]	Serious[h]	Undetected[f]	216/217 (n=433)	MD=–3.73 (–9.04 to 1.59)	Very low
Anxiety Sensitivity Index	3	Randomized trials	Serious[j]	Serious[g]	Not serious[d]	Serious[h]	Undetected[f]	216/217 (n=433)	MD=–4.65 (–11.66 to 2.36)	Very low
Montgomery-Åsberg Depression Rating Scale-Self report	3	Randomized trials	Serious[j]	Serious[g]	Not serious[d]	Serious[h]	Undetected[f]	216/217 (n=433)	MD=–2.06 (–6.18 to 2.05)	Very low

[a]GRADE: Grading of Recommendations Assessment, Development, and Evaluation.
[b]Downgraded one level for risk of bias because a substantial proportion of the evidence was contributed by studies rated as high risk or having some concerns in the overall Risk of Bias 2 assessment.
[c]Downgraded one level for inconsistency because of substantial unexplained heterogeneity (I^2=86.7%), although the direction of effect was generally consistent.
[d]Not downgraded for indirectness because the populations, interventions, comparators, and outcomes were considered consistent with the review question.
[e]Not downgraded for imprecision because the 95% CI did not include the null value and consistently favored the intervention.
[f]Formal statistical assessment of publication bias was not performed because fewer than 10 studies contributed to each outcome; no downgrade was applied for publication bias.
[g]Downgraded one level for serious inconsistency because the point estimates differed materially across studies and considerable heterogeneity was present (I^2=89.4%-93.3%).
[h]Downgraded one level for imprecision because the 95% CI included both no effect and a potentially important benefit.
[i]MD: mean difference.
[j]Downgraded one level for risk of bias because one of the 3 contributing studies was rated as having a high overall risk of bias.

Additionally, the digital interventions for HA can be categorized into guided and unguided DTs, each with distinct characteristics. Table 3 summarizes the analytical results of digital interventions for HA, categorized primarily by guidance type into guided and unguided digital therapies. In the guided digital therapy subgroup, the majority of studies (n=10) [44-52,59] used RCT or CS designs, with intervention durations to 12 weeks. These studies were predominantly conducted in Sweden (n=5) [44,45,47,52,59], Denmark (n=4) [49,53,57,58], and Australia (n=2) [46,48], using HAI or SHAI as primary outcomes, often in combination with the Whiteley-7 Index. A key finding was that, except for one small single-case study, all guided interventions reported significant reductions or improvements in primary outcomes, with most including follow-up periods ranging from 3 to 12 months, indicating the durability of digital intervention effects on HA.

In contrast, studies of unguided digital therapies exhibited greater heterogeneity, with intervention durations ranging from 12-week RCTs to single-session interventions (eg, 12 minutes or 7 days). These studies were conducted across a broader geographic distribution, including Sweden (n=1) [44], New Zealand (n=1) [56], the United States (n=1) [51], Australia (n=1) [55], China (n=1) [50], and Japan (n=1) [54]. Although most nonguided interventions have reported significant efficacy, some short-term pilot studies (eg, 14 days) or single-session interventions failed to show significant results. These approaches encompass formats such as unguided ICBT, self-help reading materials, video testimonials, and mobile-based iCBM-I. Additionally, 2 studies [51,56] (involving chatbots and computerized anxiety sensitivity interventions) found no significant effects. These descriptive patterns suggest that the guidance model and intervention dosage might be important factors.

The RCT included 3 intervention groups, including ICBT, unguided ICBT, and bibliotherapy. Since both guided and unguided intervention formats are involved for each group, they appear twice in the article.

Discussion

Principal Findings

This systematic review and meta-analysis examined the effectiveness of digital interventions for health anxiety across 16 studies, including 9 RCTs. Six RCTs involving 585 participants contributed to the primary meta-analysis and showed a significant reduction in health-anxiety symptoms (Hedges g=–0.77, 95% CI –1.25 to –0.30; P=.002). By contrast, pooled effects for IAS, BAI, ASI, and MADRS-S, although generally favoring digital interventions, did not reach statistical significance. Because no formal comparisons between outcomes were conducted and the analyses used different effect measures, this pattern should not be interpreted as proof that the effect on HA was statistically larger than the effects on broader psychological outcomes. Considerable heterogeneity was observed across all outcomes (I^2=86.7%-93.3%), while GRADE certainty was low for HA and very low for the secondary outcomes. Importantly, leave-one-out analyses showed that the health-anxiety effect remained significant after exclusion of any individual trial, with estimates ranging from Hedges g=–0.90 to –0.60, despite persistently high heterogeneity. In comparison, IAS and ASI became significant after exclusion of Axelsson et al [47], whereas BAI and MADRS-S remained nonsignificant throughout. Taken together, these findings indicate a statistically stable effect on health-anxiety-specific symptoms across leave-one-out analyses, whereas evidence for broader psychological outcomes remains less stable and more uncertain.

This pattern is broadly consistent with the mechanisms targeted by the included interventions. HA is characterized by catastrophic interpretations of benign bodily sensations and is maintained by reassurance seeking, repeated checking, and avoidance [62,63]. CBT directly targets these processes through exposure and cognitive restructuring [64], whereas ACT seeks to reduce behavioral restriction associated with illness-related thoughts and promote engagement in meaningful activities [65]. Digital delivery may further support structured practice, symptom monitoring, and repeated therapeutic exercises [66], while its accessibility and privacy may reduce barriers to conventional psychological care [67,68]. The observation that the pooled effect was significant for HA but not for the broader psychological outcomes may also reflect measurement specificity. HAI and SHAI directly assess illness-related worry, bodily vigilance, and fear of illness and therefore closely correspond to illness-focused treatment targets, whereas BAI, ASI, and MADRS-S capture broader anxiety-related or depressive symptoms. Although these constructs overlap with HA, they are not necessarily primary treatment targets [69,70]. Accordingly, reductions in illness-related catastrophic interpretations may occur without parallel improvements in general anxiety or depressive symptoms [71]. Nevertheless, the present meta-analysis cannot determine which therapeutic components or measurement characteristics account for this differential pattern.

Beyond outcome specificity, variation in intervention intensity and clinical presentation may also have contributed to the heterogeneous findings. Intervention duration ranged from a single 12-minute session to programs allowing completion within 90 days, although structured 12-week programs were most common. From a descriptive standpoint, positive outcomes were more consistent in interventions that were longer in duration and guided; this may be linked to the greater willingness of anxious individuals to engage in "in-person social interactions" or "real-world activities" that serve to shift attention. In contrast, several brief or unguided interventions failed to yield significant results. However, duration was not formally evaluated as a moderator, and treatment length was intertwined with differences in intervention content, population, comparator, and outcome assessment. Therefore, this pattern should be regarded as hypothesis-generating rather than evidence of a dose-response relationship. Clinical heterogeneity adds further uncertainty. The included populations encompassed SSD, IAD, hypochondriasis, severe or subclinical HA, COVID-19-related HA, cyberchondria, HA associated with physical illness, and HA by proxy. Differences in baseline severity, comorbidity, and the nature of the perceived health threat may therefore have contributed to between-study variability and limit the extent to which a single pooled estimate can represent all presentations of HA.

At a broader conceptual level, this clinical diversity is relevant to the continuing debate concerning the nosological position of HA. Although *DSM-5* classifies IAD and SSD within "Somatic Symptom and Related Disorders," HA shares several cognitive and behavioral features with generalized anxiety disorder, panic disorder, and obsessive-compulsive disorder, including threat hypervigilance, catastrophic interpretation, and excessive safety behaviors [72]. HA has therefore been proposed to fall, at least partly, within an anxiety-spectrum framework [73]. The present findings are compatible with, but do not establish, this interpretation. In particular, the favorable response of health-anxiety symptoms to interventions largely derived from established anxiety treatments, especially CBT, is consistent with previous evidence [74]. However, the absence of significant pooled effects on BAI and ASI indicates that treatment response alone cannot resolve its nosological classification. A related distinction may also exist between persistent HA and more context-dependent disease-specific fear. The included COVID-19-related study suggests that digital interventions may be applicable to both presentations [58], while longer-term evidence indicates that health-anxiety and general-anxiety trajectories may remain related over time [75]. Thus, future studies may benefit from examining whether more state-like versus trait-like characteristics influence treatment response and maintenance rather than assuming that HA represents a clinically uniform construct.

The distinction between guided and unguided interventions should likewise be interpreted cautiously. Narratively, favorable outcomes appeared relatively consistent across many guided ICBT- and internet-based acceptance and commitment therapy (iACT)–based interventions, whereas

unguided interventions showed greater variability. Therapist support may facilitate adherence, identify avoidance of therapeutic exercises, and strengthen engagement [66,76]. Nevertheless, the current evidence does not establish the superiority of guided interventions: some unguided or brief approaches also produced favorable outcomes, and direct comparisons have not consistently demonstrated significant between-group differences [44]. Moreover, guidance was confounded with intervention duration, therapeutic approach, and population characteristics. Its apparent advantage should therefore be viewed as a descriptive pattern requiring confirmation in adequately powered RCTs that directly compare otherwise equivalent guided and unguided programs. From a clinical perspective, digital interventions may consequently represent a potentially useful low-intensity or adjunctive option for HA, particularly where access to face-to-face psychological care is limited, such as in primary care, general hospital, geographically remote, or resource-constrained settings [12,77]. Their effects on health care use, long-term outcomes, and cost-effectiveness remain to be established.

Finally, implementation should consider digital health equity alongside effectiveness. Although remote delivery may reduce some barriers to psychological care, disparities in device access, internet connectivity, digital literacy, language, and socioeconomic resources may restrict participation among some populations. Accessible interfaces, culturally and linguistically adapted content, appropriate user support, and alternative routes to care should accompany future implementation. Overall, the present review provides evidence that digital interventions can reduce core health-anxiety symptoms, with the primary effect remaining robust across sensitivity analyses. The pooled effects on broader psychological outcomes were nonsignificant, while guided delivery, intervention intensity, and clinical phenotype remain plausible but unconfirmed explanations for between-study variation. Future RCTs should clarify which patients benefit most and under what delivery conditions.

Limitations and Potential of the Study

Several methodological limitations constrain confidence in these findings. First, the quantitative evidence base was limited. Although 9 RCTs were included, 6 contributed to the primary meta-analysis, and most secondary-outcome meta-analyses contained only 3 or 4 trials, restricting precision and the ability to evaluate the robustness of pooled findings. This protocol deviation should be considered when interpreting the robustness of the findings. Second, among the 9 RCTs, 4 had low overall RoB, 2 had some concerns, and 3 had high RoB. Measurement of the outcome was the most frequent source of high-risk judgments, particularly where participant-reported outcomes were used without blinding. Because participant blinding is inherently difficult in digital psychological interventions [78,79], expectancy effects, response bias, and outcome-measurement bias cannot be excluded. Future trials may strengthen assessment by combining self-report measures with blinded diagnostic evaluation and, where appropriate, behavioral, physiological, or sensor-derived indicators [44,80], while carefully designed active or sham digital comparators may help reduce differences in treatment expectancy [81].

Third, substantial statistical heterogeneity was observed, with I^2 values exceeding 80% for every pooled outcome and remaining high in most sensitivity analyses. No subgroup-difference tests or meta-regression analyses were undertaken; consequently, intervention type, guidance, duration, diagnostic status, and baseline severity cannot be regarded as established moderators. Clinical and methodological diversity may have contributed to this variability. The included populations represented several diagnostic and contextual presentations, and the interventions differed in therapeutic components, including exposure, cognitive restructuring, acceptance-based strategies, and bias modification. Differences in duration, professional guidance, adherence, comparators, and assessment timing may have further contributed to variation in the effect estimates.

Fourth, where follow-up was reported, most periods were 6 months or less, leaving the longer-term durability of digital interventions uncertain. In addition, pooled RCT estimates did not demonstrate statistically significant effects on IAS, BAI, ASI, or MADRS-S. This finding should not be generalized to every individual study and remains uncertain because of the small number of contributing trials, substantial heterogeneity, and limited precision. Publication bias could not be formally assessed because fewer than 10 studies contributed to each meta-analysis, and unpublished null findings cannot be excluded. Overall, the low GRADE certainty for the primary outcome and very low certainty for the secondary outcomes preclude strong clinical recommendations.

Despite these limitations, digital interventions remain promising accessible and scalable options for HA, particularly where face-to-face care is limited. To translate this potential into reliable clinical benefit, future research should prioritize several directions.

First, RCTs should directly compare the long-term effectiveness and cost-effectiveness of internet-delivered and face-to-face CBT. Second, predictors such as baseline severity, comorbidity, digital literacy, and treatment preference should be examined to improve matching between guided and unguided interventions. Third, rapid diagnostic tools should distinguish SSD, IAD, and nondiagnostic presentations in primary care and evaluate the added value of health-literacy education [82]. Fourth, studies should test whether somatosensory technologies improve engagement and adherence. AI should follow principles of transparency, data minimization, and clinical oversight [83]. Because severe HA may involve distress during exposure or unintended interpretations of digital content, evidence is needed on the suitability of self-guided interventions for patients with suicidal ideation, psychotic disorders, or severe physical illness. Future systems should include risk monitoring and clear escalation pathways [84] to support safe and trustworthy digital mental health care. Finally, alongside these clinical and methodological priorities, future implementation

and research must consider digital health equity, including access, literacy, and cultural adaptation, to ensure that these interventions benefit diverse populations

Conclusions

The available evidence suggests that digital interventions can reduce core health-anxiety symptoms, with the pooled effect remaining significant across sensitivity analyses. However, substantial between-study heterogeneity, risk-of-bias concerns, and low certainty of evidence limit confidence in the magnitude and generalizability of this effect. Evidence for illness-related attitudes, general anxiety, anxiety sensitivity, and depressive symptoms remains inconclusive, and the current data do not establish which intervention formats, levels of guidance, or patient characteristics account for differences in treatment response. Digital interventions may therefore represent a potentially useful low-intensity or adjunctive option for HA, particularly where access to conventional psychological care is limited, but firm conclusions regarding broader psychological benefits and long-term effectiveness cannot yet be drawn. Adequately powered, well-reported RCTs using rigorous outcome assessment, appropriate active comparators, and longer follow-up are needed to confirm these findings and clarify which patients benefit most and under what delivery conditions.

Generative AI Use

AI was only used to refine language expression and check grammatical errors in this manuscript.

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Data Availability

Data are presented in the main manuscript

Authors' Contributions

JX and JS developed the concept of the study and led the development of the protocol in collaboration with other team members. JX performed the online searches. JS and JW screened the articles for inclusion. GH and JX extracted data from the included articles. JW and JS critically appraised the studies. JX led the JBI assessment and presentation of the figures, in collaboration with JS, and other team members, who helped inform the meta-analysis, interpretation of the results, and formulation of recommendations. All coauthors reviewed and approved the manuscript.

Conflicts of Interest

None declared.

Multimedia Appendix 1

Studies excluded with reason (N=16).
[DOCX File (Microsoft Word File), 24 KB-Multimedia Appendix 1]

Multimedia Appendix 2

Characteristics of the included studies on digital interventions in health anxiety (n=16).
[DOCX File (Microsoft Word File), 88 KB-Multimedia Appendix 2]

Checklist 1

PRISMA 2020 checklist.
[PDF File (Adobe File), 101 KB-Checklist 1]

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Abbreviations

ACT: acceptance and commitment therapy
AS: anxiety sensitivity
ASI: Anxiety Sensitivity Index
BA: behavioral activation
BAI: Beck Anxiety Inventory
CBT: cognitive behavioral therapy
CENTRAL: Cochrane Central Register of Controlled Trials
CS: cohort study
DBI: digital behavioral intervention
DMHI: digital mental health intervention
***DSM-5*:** *Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition*
DT: digital therapeutic
GRADE: Grading of Recommendations Assessment, Development, and Evaluation
HAI-64: 64-item Health Anxiety Inventory
iACT: internet-based acceptance and commitment therapy
IAD: illness anxiety disorder
IAS: Illness Attitude Scale
ICBT: internet-based cognitive behavioral therapy
JBI: Joanna Briggs Institute

MADRS-S: Montgomery-Åsberg Depression Rating Scale–Self
MD: mean difference
PICOS: Population, Intervention, Comparator, Outcomes, and Study design
PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses
PROSPERO: International Prospective Register of Systematic Reviews
QES: quasi-experimental study
RCT: randomized controlled trial.
RoB 2: Risk of Bias 2
SDS: Sheehan Disability Scale
SHAI-18: 18-item Short Health Anxiety Inventory
SMD: standardized mean difference
SSD: somatic symptom disorder
WHODAS-II: World Health Organization Disability Assessment Schedule II

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