Effectiveness of Single-Session Therapy Based on Cognitive-Behavioral Stress Management Principles on Health Anxiety and Quality of Life in Patients with Irritable Bowel Syndrome

Abstract

Aims: Irritable bowel syndrome is a disorder of gut–brain interaction in which health anxiety and impaired quality of life are prominent. To evaluate the effectiveness of a single-session therapy based on cognitive-behavioral stress management principles on health anxiety and quality of life in patients with IBS.

Materials & Methods: In a quasi-experimental pretest–posttest–follow-up design, 30 patients with IBS were purposively sampled from gastroenterology and psychology clinics in Gilan and Rasht, Iran (January–March 2026) and randomly assigned to an intervention or control group. The sample comprised 30 patients; 27 participants completed all study assessments (intervention n = 13, control n = 14). The intervention group received one 90–120-minute session. Measures were the Short Health Anxiety Inventory (SHAI) and **36-Item Short Form Health Survey (SF-36)**. All statistical analyses were performed using SPSS (Version 27) and JASP (Version 0.19.0).

Findings: The time × group interaction was significant for health anxiety ($F = 14.887$, $p < .001$, $\eta^2 = .383$): SHAI scores in the intervention group declined from 26.69 (pretest) to 23.77 (posttest) and 21.23 (follow-up). Significant interactions were also found for SF-36 vitality ($F = 30.592$, $p < .001$, $\eta^2 = .560$) and social functioning ($F = 14.589$, $p < .001$, $\eta^2 = .378$), with intervention participants improving at posttest and follow-up while controls remained unchanged.

Conclusion: The findings suggest that a single-session therapy based on cognitive-behavioral stress management principles may reduce health anxiety and improve selected aspects of quality of life in patients with IBS.

Keywords: Irritable bowel syndrome; Single-session therapy; Cognitive-behavioral stress management; Health anxiety; Quality of life.

Introduction

Irritable bowel syndrome (IBS) is one of the most prevalent disorders of gut–brain interaction (DGBI) and is characterized by recurrent abdominal pain, altered bowel habits, abdominal distension, and bloating [1, 2]. Although IBS is not typically associated with distinct or progressive structural pathology, its relapsing and unpredictable nature imposes a significant psychological, social, and economic burden on affected individuals and healthcare systems [3, 4]. Contemporary biopsychosocial models indicate that dysregulation of the brain–gut axis is not solely driven by gastrointestinal motility and secretory alterations; rather, psychological factors play an important role in the persistence and severity of symptoms and the associated functional disability [5, 6].

Among the most prominent psychological correlates of IBS are health anxiety and attentional bias toward bodily sensations [7, 8]. Owing to visceral hypersensitivity and gastrointestinal-specific anxiety, individuals with IBS may persistently appraise and hyper-monitor their bodily sensations, frequently misinterpreting benign or ambiguous bowel sensations as catastrophic indicators of serious illness [9, 10]. This vicious cycle of negative cognitive appraisals and excessive hypervigilance may trigger the release of stress mediators and increase autonomic nervous system reactivity, thereby exacerbating physical symptom severity [11, 12]. The chronicity of these processes can impair daily functioning, interpersonal relationships, dietary habits, and health-related quality of life, contributing to persistent functional impairment [13–15].

Given the multifaceted pathophysiology of IBS, conventional medical and pharmacological treatments alone may not fully address the psychological and behavioral aspects of symptom management. Accordingly, psychological interventions centered on stress management and cognitive reappraisal have become important components of comprehensive IBS care [16, 17]. Nonetheless, traditional multi-session psychological protocols may be limited by structural barriers, including substantial financial costs, prolonged treatment duration, elevated attrition rates, and limited accessibility [18, 19]. In response to these constraints, Single-Session Therapy (SST) has gained increasing attention as a pragmatic and structured intervention approach [20–22].

The foundational rationale of SST is that both the therapist and the client approach the encounter as if it were the only available session. Accordingly, the intervention focuses rapidly on core concerns, cognitive

restructuring, the mobilization of internal and external strengths, and the development of actionable coping strategies [23–25]. Recent empirical investigations have reported promising effects of single-session interventions across diverse clinical and psychosomatic outcomes, including reductions in anxiety symptoms and improvements in emotion regulation [26–29]. Integrating Cognitive-Behavioral Stress Management (CBSM) principles into the focused framework of SST provides a structured approach that incorporates cognitive restructuring, relaxation training, and the correction of misattributions related to visceral sensations. This integrated approach may help disrupt the stress–health anxiety cycle in patients with IBS within a focused treatment format [7, 30].

Despite the growing emphasis on personalized and accessible psychological care for functional gastrointestinal disorders [1], a gap remains in the empirical literature regarding the application of CBSM-informed SST specifically tailored to this clinical population. Therefore, the present study aimed to examine the effectiveness of an SST based on CBSM principles in reducing health anxiety and improving quality of life among patients with IBS.

3. Methods

3.1. Study Design

The study was a two-arm, parallel-group randomized controlled trial with a waitlist control group and assessments at baseline, immediately after the intervention, **and one month after** the intervention. Participants were recruited from the Gastroenterology Clinic of Gilan Province and the Psychology Center **of Rasht, Iran, between January and March 2026**. **Participants were recruited using purposive sampling and subsequently randomly allocated to either the intervention group or the waitlist control group.**

3.2. Participants, Sampling, and Sample Size

A total of 35 individuals were screened for eligibility. Five did not proceed to enrollment because they declined participation or did not provide written informed consent, their IBS diagnosis was not confirmed by a gastroenterologist, or unforeseen personal circumstances prevented participation. The remaining 30 participants were enrolled and randomly allocated to the intervention or control group (15 per group). Patients diagnosed with irritable bowel syndrome (IBS) according to the Rome IV criteria were recruited using purposive sampling. An a priori power analysis was conducted based on an assumed medium effect size **. The initial recruitment target was 30 participants (15 per group). The original rationale specified a significance level of α = .05 and an assumed medium effect size of Cohen's d = 0.50, consistent with Cohen's** [31**] conventional benchmark rather than an estimate derived from a previous IBS trial. However, the target statistical power, the statistical model used for sample-size determination, and a prospective allowance for attrition were not documented. The outcomes were analyzed using baseline-adjusted repeated-measures ANCOVA. Three participants withdrew before the post-intervention assessment because of unforeseen changes in their personal circumstances: two in the intervention group and one in the control group. This represented 10% overall attrition, leaving 27 participants for analysis (13 intervention and 14 control).**

Inclusion and Exclusion Criteria

Eligibility Criteria: Participants were eligible if they met the following criteria before randomization: (a) a confirmed diagnosis of IBS by a board-certified gastroenterologist; (b) a minimum educational attainment of lower secondary education (middle school diploma) to support comprehension of the self-report instruments; © age between 30 and 45 years; (d) no documented history of severe psychiatric disorders or psychiatric hospitalization; (e) no active substance or alcohol use disorders; and (f) provision of written informed consent. Individuals who did not meet these criteria were excluded before randomization**.**

Intervention Non-completion and Discontinuation Criteria: Non-attendance at the scheduled single-session intervention, premature departure, or incomplete participation constituted intervention non-completion. Acute medical conditions arising during the study that prevented safe participation were grounds for discontinuing the intervention. These post-randomization events were distinct from eligibility exclusions and did not, by themselves, constitute grounds for exclusion from the analysis.

3.3. Ethical Considerations and Procedure

The study protocol was reviewed and approved by the Biomedical Research Ethics Committee of the Faculty of Psychology and Education, University of Tehran (Approval ID: IR.UT.PSYEDU.REC.1405.129) and prospectively registered with the Iranian Registry of Clinical Trials (IRCT ID: IRCT20260811070617N1).

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The research was conducted through the following sequential phases:

Inter-Institutional Coordination and Diagnostic Screening

Initial coordination was established with the Guilan Gastrointestinal and Liver Specialty Clinic and the Rasht Psychology and Health Center. Medical records of patients with confirmed IBS were pre-screened. Eligible individuals meeting primary criteria (age 30–45 and required educational level) were referred by medical staff to the research team for psychological screening.

Eligibility Verification and Pretest Assessment

Researchers verified secondary eligibility criteria through brief, structured clinical interviews conducted at the Rasht Psychology and Health Center. The study aims; procedures, voluntary participation, and data confidentiality were explained. Upon obtaining written informed consent, all 30 eligible participants completed the baseline battery (pretest).

Random Allocation and Intervention Delivery

Participants were randomly allocated to the intervention or control group in a 1:1 ratio using a computer-generated randomization sequence prepared by an individual who was not involved in recruitment or outcome assessment. Allocation was concealed using sequentially numbered, opaque, sealed envelopes. The first author enrolled participants and implemented group allocation by opening the envelopes sequentially only after eligibility had been confirmed, written informed consent obtained, and baseline assessments completed.

The intervention was delivered individually in a single 90–120-minute session at the Rasht Psychology and Health Center by the first author, who holds a PhD in Health Psychology, under the supervision of the second author. The session was based on established Cognitive-Behavioral Stress Management principles and included psychoeducation and case conceptualization, cognitive restructuring, behavioral modification, relaxation training, exposure to feared bodily sensations, and relapse-prevention planning. The intervention was delivered individually to participants according to a structured protocol.

No formal treatment fidelity monitoring was conducted, and no home practice was assigned. Concomitant medication use, dietary treatments, and other usual care were not systematically recorded.

Posttest Assessment and Ethical Debriefing

Immediately following the intervention, posttest questionnaires were administered to both groups under identical conditions. Follow-up assessments were conducted one month after the intervention. **The waitlist control group received the intervention materials only after completion of the one-month follow-up assessments. No blinding procedures were implemented.**

3.4. Intervention Protocol: Single-Session Cognitive-Behavioral Therapy (SS-CBT)

The intervention protocol was based on the cognitive-behavioral approach described in reference [23] and was delivered in a single structured session lasting 90–120 minutes. The primary objective of this intervention was to interrupt the vicious cycle between *catastrophizing thoughts regarding somatic sensations*, *autonomic nervous system hyperarousal*, and *gastrointestinal symptom exacerbation*.

The protocol was delivered through four sequential, structured steps:

- **Step 1: Psychoeducation and Conceptualization of the Brain-Gut Axis:**

The bidirectional nature of the brain-gut interaction was delineated for participants. Particular emphasis was placed on elucidating how the Enteric Nervous System (ENS) responds to emotional stress and attentional biases, and how heightened health anxiety and somatic hypervigilance substantially lower the threshold for visceral pain perception.

- **Step 2: Cognitive Restructuring and Maladaptive Thought Modification:**

This step focused on identifying prevalent cognitive distortions, such as somatic catastrophizing, overgeneralization, and misattributing benign bodily sensations to severe medical pathology. Utilizing Socratic dialogue and evidence-gathering techniques (examining evidence for and against automatic thoughts), participants were guided to substitute maladaptive assumptions regarding health anxiety and somatosensory amplification with realistic, balanced, and non-threatening appraisals.

- **Step 3: Behavioral Modification and Desensitization:**

Maladaptive safety-seeking behaviors (e.g., compulsive abdominal checking, excessive health-related internet searches, and repeated medical reassurance-seeking) as well as avoidance behaviors were systematically targeted for reduction. Additionally, rapid physiological down-regulation techniques—including diaphragmatic breathing retraining and brief progressive muscle relaxation (PMR)—were taught to mitigate autonomic hyperarousal.

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- **Step 4: Consolidation, Action Planning, and Relapse Prevention:**

A personalized written action plan was developed collaboratively with each participant to facilitate adaptive coping during symptom flare-ups in daily life. The session concluded with the review of self-management assignments and a comprehensive synthesis of key intervention takeaways.

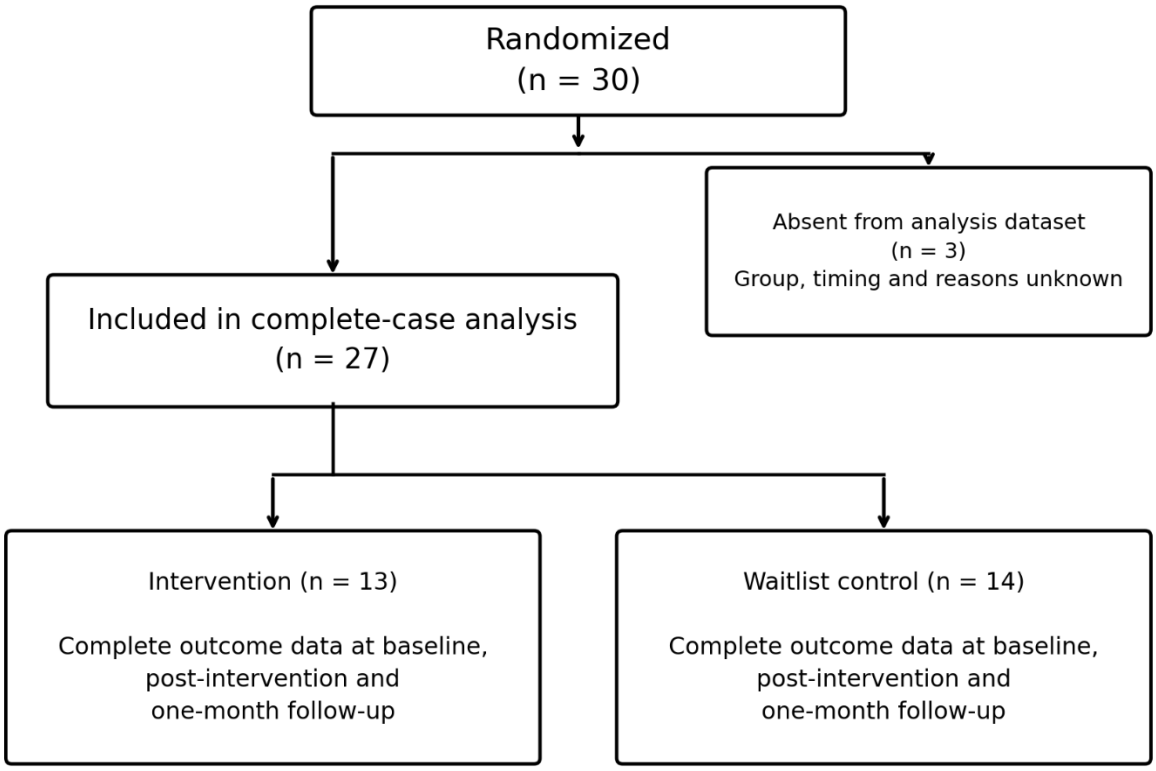


Figure 1. Participant flow based on the available analysis dataset.

3.5. Measures

Short Health Anxiety Inventory (SHAI): Health anxiety was assessed using the 18-item Short Health Anxiety Inventory developed to measure health-related anxiety, hypochondriacal concerns, and the catastrophic misinterpretation of bodily sensations independently of actual physical health status [32]. This self-report instrument encompasses two distinct subscales, comprising the perceived likelihood of illness (items 1–14) and the perceived negative consequences of contracting a severe medical condition (items 15–18), with each item presenting four progressive statements scored on a 4-point scale ranging from 0 to 3, yielding total scores between 0 and 54 where scores equal to or exceeding 18 reflect clinically significant health anxiety [32]. In the Iranian psychometric adaptation, confirmatory factor analysis supported the construct validity and two-factor structure of the scale, demonstrating acceptable internal consistency with Cronbach's alpha coefficients ranging from .88 to .91, test-retest reliability of .82 across a three-week interval, and significant convergent validity with established measures of general anxiety and psychological distress [33].

36-Item Short Form Health Survey (SF-36): Health-related quality of life was assessed using the Persian version of the SF-36. This 36-item questionnaire assesses eight domains: physical functioning, role limitations due to physical health problems, bodily pain, general health, vitality, social functioning, role limitations due to emotional problems, and mental health [34]. Under standard scoring procedures, domain scores range from 0 to 100, with higher scores indicating better health-related quality of life. The Persian version demonstrated

satisfactory validity and internal consistency, with Cronbach's alpha coefficients ranging from 0.77 to 0.90 across seven domains and 0.65 for vitality [35].

3.6. Data Analysis

Statistical analyses were conducted using SPSS (Version 27) and JASP (Version 0.19.0), with additional endpoint-specific ANCOVA estimates and confidence intervals computed in Python using statsmodels. The statistical significance threshold was set at $\alpha = .05$. Analyses included the 27 participants with complete outcome data at baseline, post-intervention, and one-month follow-up: 13 in the intervention group and 14 in the waitlist control group. No missing outcome values were imputed. This was a complete-case analysis rather than an intention-to-treat analysis of all 30 randomized participants. Baseline demographic characteristics were summarized descriptively using frequencies and percentages, and outcome scores were summarized using means and standard deviations. No inferential claims of baseline equivalence were made.

Model assumptions were assessed using the Shapiro–Wilk test for residual normality, Levene's test for homogeneity of variances, and group × baseline interactions to examine homogeneity of regression slopes. Because the within-subject factor comprised only two assessments, sphericity was automatically satisfied, and neither Mauchly's test nor sphericity corrections were required.

Separate baseline-adjusted repeated-measures analyses of covariance (RM-ANCOVA) were used to examine health anxiety and each quality-of-life domain. Group (intervention versus waitlist control) was the between-subjects factor, and time (post-intervention versus one-month follow-up) was the within-subjects factor. The corresponding baseline outcome score was included as a covariate rather than as a third repeated assessment. Models included group, time, baseline, time × group, and time × baseline terms. The group effect represented the baseline-adjusted between-group difference averaged across the two post-baseline assessments, whereas the time × group interaction evaluated whether changes from post-intervention to one-month follow-up differed between groups. Model effects were reported using F statistics with numerator and denominator degrees of freedom, p-values, and partial eta squared (ηp^2). Baseline-adjusted pairwise comparisons for health anxiety were reported separately with their associated multiplicity-adjusted p-values.

For quality-of-life outcomes, additional endpoint-specific ANCOVA models were fitted at post-intervention and one-month follow-up, with group and the corresponding baseline score as predictors. Between-group differences were expressed as intervention minus waitlist control and reported with pointwise 95% confidence intervals based on conventional model standard errors. Quality-of-life analyses were exploratory; their p-values and confidence intervals were not adjusted for multiplicity. Statistical significance was not interpreted as evidence of clinical importance.

Partial eta squared (partial η^2) was reported for model effects. Two-sided 95% confidence intervals were obtained by inverting the noncentral F distribution and transforming the noncentrality limits using $\lambda/(\lambda + df_error)$, with limits constrained to the admissible parameter range. These intervals were not adjusted for multiplicity.

4. Results

Table 1 summarizes the baseline demographic characteristics of the 27 participants included in the analysis: 13 in the intervention group and 14 in the waitlist control group. Overall, 66.7% were female, and 74.1% were married. The most common educational category was high school (29.6%). The largest age category was 30–35 years (44.4%), and the most commonly reported IBS duration was 1–3 years (37.0%). Baseline characteristics are presented descriptively rather than interpreted as evidence of equivalence between groups.

Table 1. Baseline demographic characteristics of participants included in the analysis (N = 27)

Variable	Levels	Total (N=27) n (%)	Intervention (n=13) n (%)	Waitlist control (n=14) n (%)
Age	30–35 years	12 (44.4%)	7 (53.8%)	5 (35.7%)
	36–40 years	7 (25.9%)	3 (23.1%)	4 (28.6%)
	41–45 years	8 (29.6%)	3 (23.1%)	5 (35.7%)
Gender	Female	18 (66.7%)	8 (61.5%)	10 (71.4%)
	Male	9 (33.3%)	5 (38.5%)	4 (28.6%)
Education	Middle School	7 (25.9%)	4 (30.8%)	3 (21.4%)
	High School	8 (29.6%)	4 (30.8%)	4 (28.6%)
	Bachelor's	6 (22.2%)	2 (15.4%)	4 (28.6%)
	Master's	6 (22.2%)	3 (23.1%)	3 (21.4%)

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Marital Status	Married	20 (74.1%)	9 (69.2%)	11 (78.6%)
	Single	7 (25.9%)	4 (30.8%)	3 (21.4%)
Employment	Homemaker	15 (55.6%)	8 (61.5%)	7 (50.0%)
	Employed	6 (22.2%)	4 (30.8%)	2 (14.3%)
	Unemployed	6 (22.2%)	1 (7.7%)	5 (35.7%)
IBS Duration	< 1 year	7 (25.9%)	3 (23.1%)	4 (28.6%)
	1–3 years	10 (37.0%)	5 (38.5%)	5 (35.7%)
	4–6 years	6 (22.2%)	3 (23.1%)	3 (21.4%)
	> 6 years	4 (14.8%)	2 (15.4%)	2 (14.3%)

Descriptive statistics for health anxiety and quality-of-life domains at baseline, post-intervention, and one-month follow-up are presented in Table 2.

Table 2. Health anxiety and quality-of-life scores at baseline, post-intervention, and one-month follow-up (N = 27)

Outcome	Assessment	Intervention (n = 13), Mean (SD)	Waitlist control (n = 14), Mean (SD)
Health anxiety	Baseline	26.692 (1.750)	26.286 (1.773)
	Post-intervention	23.769 (0.832)	26.786 (1.718)
	One-month follow-up	21.231 (1.013)	26.929 (1.774)
Physical functioning	Baseline	55.231 (1.739)	55.286 (1.637)
	Post-intervention	55.231 (1.739)	55.286 (1.684)
	One-month follow-up	54.308 (1.601)	55.286 (1.684)
Role limitations due to physical health	Baseline	55.077 (1.188)	55.357 (1.499)
	Post-intervention	54.692 (1.109)	54.786 (1.251)
	One-month follow-up	54.923 (1.038)	54.786 (1.122)
Role limitations due to emotional problems	Baseline	47.308 (1.251)	47.143 (1.460)
	Post-intervention	47.308 (1.377)	47.071 (1.328)
	One-month follow-up	47.077 (1.605)	47.000 (1.519)
Vitality	Baseline	60.077 (1.605)	61.357 (1.865)
	Post-intervention	66.769 (0.725)	59.857 (1.406)
	One-month follow-up	69.154 (0.899)	59.786 (1.477)
Mental health	Baseline	54.769 (1.301)	54.786 (0.975)
	Post-intervention	54.769 (1.166)	54.714 (1.069)
	One-month follow-up	54.231 (1.166)	54.857 (1.027)
Social functioning	Baseline	61.462 (1.050)	62.500 (1.345)
	Post-intervention	68.923 (0.641)	61.571 (1.089)
	One-month follow-up	70.769 (0.832)	61.571 (1.089)
Bodily pain	Baseline	54.923 (1.115)	54.857 (1.292)
	Post-intervention	55.077 (1.115)	54.857 (1.027)
	One-month follow-up	55.077 (1.256)	54.857 (1.406)
General health	Baseline	54.769 (1.301)	55.000 (1.177)
	Post-intervention	54.923 (1.188)	54.429 (1.222)

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	One-month follow-up	54.385 (1.261)	54.286 (1.267)

Note. Values are unadjusted means (standard deviations). Descriptive statistics refer to the complete-case analysis sample: 13 intervention participants and 14 waitlist control participants with complete outcome data at all three assessments. Baseline-adjusted model effects and between-group comparisons are reported separately in Tables 3–6. SD = standard deviation.

In the intervention group, mean health anxiety decreased from 26.692 at baseline to 23.769 at post-intervention and 21.231 at one-month follow-up. Corresponding means in the waitlist control group were 26.286, 26.786, and 26.929, respectively. Mean vitality and social functioning increased across assessments in the intervention group, whereas no corresponding increases were observed in the waitlist control group. The remaining quality-of-life domains showed relatively small descriptive changes. These unadjusted descriptive patterns alone do not establish statistical significance or clinical importance.

Table 3. Baseline-adjusted repeated-measures analysis of health anxiety

Effect	F(1, 24)	p value	Partial η^2	95% CI for partial η^2
Time	0.630	.435	.026	[.000, .240]
Time × baseline	0.333	.569	.014	[.000, .209]
Time × group	14.887	< .001	.383	[.095, .605]
Baseline	2.819	.106	.105	[.000, .361]
Group	118.171	< .001	.831	[.684, .897]

Note. Analyses included 27 participants (13 intervention and 14 control). The repeated factor comprised post-intervention and follow-up assessments; baseline health anxiety was included as a covariate, not as a repeated assessment. The group effect represents the baseline-adjusted group difference averaged across the two post-baseline assessments. The time × group interaction tests whether change from post-intervention to follow-up differed between groups. Baseline was entered uncentered to reproduce the original analysis; consequently, the time main effect is conditional on a baseline score of zero and should not be interpreted as the average change in the sample. Confidence intervals for partial η^2 were calculated by inversion of the noncentral F distribution. All p-values and confidence intervals are unadjusted for multiplicity.

The baseline-adjusted analysis showed a group effect on health anxiety averaged across post-intervention and follow-up, F(1, 24) = 118.171, p < .001, partial η^2 = .831, 95% CI [.684, .897]. The time × group interaction was also statistically significant, F(1, 24) = 14.887, p < .001, partial η^2 = .383, 95% CI [.095, .605], indicating that change from post-intervention to follow-up differed between groups. Adjusted between-group differences at each assessment are reported in Table 4.

Table 4. Baseline-adjusted pairwise comparisons of health anxiety between group–assessment combinations

Group and assessment (I)	Group and assessment (J)	Mean difference (I – J)	Standard error	Bonferroni-adjusted 95% CI	t (df = 24)	Adjusted p
Intervention, post-intervention	Control, post-intervention	−3.074	0.532	−4.603 to −1.544	−5.779	< .001
Intervention, post-intervention	Intervention, one-month follow-up	2.564	0.508	1.104 to 4.023	5.049	< .001
Intervention, post-intervention	Control, one-month follow-up	−3.240	0.538	−4.788 to −1.692	−6.019	< .001
Control, post-intervention	Intervention, one-month follow-up	5.637	0.539	4.088 to 7.187	10.461	< .001
Control, post-intervention	Control, one-month follow-up	−0.166	0.489	−1.573 to 1.240	−0.340	1.000
Intervention, one-month follow-up	Control, one-month follow-up	−5.804	0.548	−7.381 to −4.227	−10.582	< .001

Note. Analyses included 27 participants with complete data (13 participants in the intervention group and 14 in the waitlist control group) .Post-intervention and one-month follow-up health anxiety scores were analyzed jointly using a multivariate linear model with group and mean-centered baseline health anxiety as predictors,

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allowing assessment-specific baseline slopes and an unstructured within-participant residual covariance matrix. Estimated marginal means were evaluated at the overall mean baseline score. Mean differences represent combination (I) minus combination (J); negative values indicate lower health anxiety in combination (I). All tests used 24 residual degrees of freedom. Two-sided p-values and confidence intervals were Bonferroni-adjusted across all six pairwise comparisons, providing at least 95% simultaneous confidence coverage for this family of comparisons. CI = confidence interval.

As shown in Table 4, baseline-adjusted health anxiety was significantly lower in the intervention group than in the waitlist control group at post-intervention (mean difference = -3.074, Bonferroni-adjusted 95% CI [-4.603, -1.544], adjusted p < .001) and one-month follow-up (mean difference = -5.804, Bonferroni-adjusted 95% CI [-7.381, -4.227], adjusted p < .001). Within the intervention group, health anxiety decreased further between post-intervention and one-month follow-up (post-intervention minus follow-up difference = 2.564, Bonferroni-adjusted 95% CI [1.104, 4.023], adjusted p < .001). No statistically significant change was detected in the control group over the same period (difference = -0.166, Bonferroni-adjusted 95% CI [-1.573, 1.240], adjusted p = 1.000).

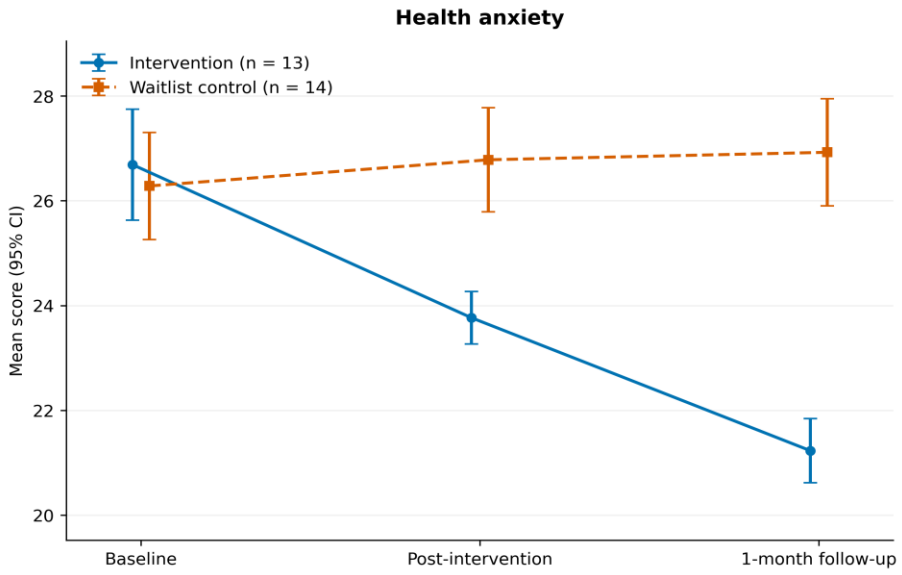


Figure 2. Changes in health-anxiety scores across assessments by treatment group.

Points in figure 2 represent observed means, and error bars indicate pointwise 95% confidence intervals calculated using the t distribution. Data are shown for the 27 participants with complete assessments (intervention, n = 13; waitlist control, n = 14). Lower scores indicate less health anxiety. Values are unadjusted and should be distinguished from the baseline-adjusted model estimates. Confidence intervals are not adjusted for multiple comparisons.

Table 5. Baseline-adjusted group and time × group effects for quality-of-life domains

Quality-of-life domains	Effect	F(1, 24)	p value	Partial η^2	95% CI for partial η^2
Physical functioning	Group	1.446	.241	.057	[.000, .296]
	Time × group	1.292	.267	.051	[.000, .287]
Role limitations due to physical health	Group	0.007	.934	< .001	[.000, .075]
	Time × group	0.037	.849	.002	[.000, .132]
Role limitations due to emotional problems	Group	0.180	.675	.007	[.000, .185]
	Time × group	0.067	.797	.003	[.000, .151]
Vitality	Group	365.526	< .001	.938	[.882, .962]

	Time × group	30.592	< .001	.560	[.272, .727]
Mental health	Group	0.675	.419	.027	[.000, .244]
	Time × group	2.204	.151	.084	[.000, .335]
Social functioning	Group	676.354	< .001	.966	[.934, .979]
	Time × group	14.589	< .001	.378	[.091, .602]
Bodily pain	Group	0.321	.576	.013	[.000, .208]
	Time × group	0.004	.948	< .001	[.000, .057]
General health	Group	0.888	.356	.036	[.000, .261]
	Time × group	0.407	.530	.017	[.000, .218]

Note. Separate baseline-adjusted repeated-measures analyses were conducted for each quality-of-life domain using 27 complete records (13 intervention and 14 control). The repeated factor comprised post-intervention and follow-up assessments, with the corresponding baseline domain score included as a covariate. The group effect represents the adjusted group difference averaged across the two post-baseline assessments; the time × group interaction tests whether change between these assessments differed between groups. Confidence intervals for partial η^2 were calculated by inversion of the noncentral F distribution. All p values and confidence intervals are unadjusted for multiplicity; these secondary-outcome analyses should be interpreted as exploratory.

Baseline-adjusted group effects were statistically significant for vitality, $F(1, 24) = 365.526$, $p < .001$, partial $\eta^2 = .938$, 95% CI [.882, .962], and social functioning, $F(1, 24) = 676.354$, $p < .001$, partial $\eta^2 = .966$, 95% CI [.934, .979]. Time × group interactions were also significant for vitality, $F(1, 24) = 30.592$, $p < .001$, partial $\eta^2 = .560$, 95% CI [.272, .727], and social functioning, $F(1, 24) = 14.589$, $p < .001$, partial $\eta^2 = .378$, 95% CI [.091, .602]. No statistically significant group or time × group effects were detected for the remaining domains. These findings are exploratory because the analyses were not adjusted for multiple testing. Adjusted between-group differences at each post-baseline assessment are presented in Table 6.

Table 6. Baseline-adjusted between-group differences in quality-of-life scores at post-intervention and one-month follow-up

Quality-of-life domains	**Assessment**	**Adjusted mean difference**	**95% CI**	**Unadjusted p**
Physical functioning	Post-intervention	−0.021	[−1.119, 1.077]	.969
	One-month follow-up	−0.967	[−2.275, 0.340]	.140
Role limitations due to physical health	Post-intervention	−0.030	[−0.965, 0.904]	.947
	One-month follow-up	0.084	[−0.773, 0.941]	.842
Role limitations due to emotional problems	Post-intervention	0.273	[−0.798, 1.344]	.604
	One-month follow-up	0.060	[−1.204, 1.324]	.922
Vitality	Post-intervention	6.844	[5.865, 7.823]	< .001
	One-month follow-up	9.535	[8.482, 10.588]	< .001
Mental health	Post-intervention	0.062	[−0.760, 0.883]	.878
	One-month follow-up	−0.627	[−1.516, 0.262]	.159

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Social functioning	Post-intervention	7.506	[6.721, 8.291]	< .001
	One-month follow-up	9.293	[8.434, 10.153]	< .001
Bodily pain	Post-intervention	0.229	[−0.628, 1.087]	.586
	One-month follow-up	0.195	[−0.825, 1.215]	.697
General health	Post-intervention	0.541	[−0.420, 1.503]	.257
	One-month follow-up	0.124	[−0.900, 1.148]	.805

Note. Differences are intervention minus waitlist control. Estimates were obtained from separate ordinary least-squares ANCOVA models at each post-baseline assessment, including group and the corresponding baseline outcome score as predictors. Each model included 27 participants and had 24 residual degrees of freedom. Confidence intervals are pointwise 95% intervals based on the conventional model standard errors; neither confidence intervals nor p-values in this table are adjusted for multiple comparisons. These exploratory estimates should not be interpreted as establishing clinical importance.

In the endpoint-specific ANCOVA analyses, the intervention group had higher baseline-adjusted vitality scores than the control group at post-intervention (adjusted mean difference = 6.844, 95% CI [5.865, 7.823]) and one-month follow-up (9.535, 95% CI [8.482, 10.588]). Corresponding differences in social functioning were 7.506 (95% CI [6.721, 8.291]) and 9.293 (95% CI [8.434, 10.153]), respectively. Confidence intervals for the remaining quality-of-life domains included zero at both assessments. These exploratory findings describe statistical differences in this small sample and do not establish clinically meaningful improvement.

Discussion

The present study aimed to examine the effectiveness of single-session therapy (SST) based on cognitive-behavioral stress-management principles in reducing health anxiety and improving quality of life among patients with irritable bowel syndrome (IBS). Baseline-adjusted analyses identified a significant group effect and a significant time-by-group interaction for health anxiety across the post-intervention and one-month follow-up assessments. Pairwise comparisons showed lower health anxiety in the intervention group than in the waitlist control group at both assessments, with a further reduction from post-intervention to follow-up in the intervention group. For quality of life, significant time-by-group interactions were observed for vitality and social functioning, and endpoint-specific adjusted comparisons favored the intervention group in these domains at both assessments. Comparable evidence of benefit was not observed across the remaining domains. These findings provide preliminary evidence of benefits for health anxiety and selected aspects of quality of life, rather than evidence of comprehensive or clinically established improvement.

The reduction in health anxiety is consistent with the findings of Golshani et al. [7], who examined CBT in 36 women with IBS and reported reductions in health-anxiety components and perceived stress, alongside increased pain self-efficacy. Their intervention comprised eight 90-minute sessions, whereas the present intervention was delivered in a single session. The agreement in the direction of the health-anxiety findings supports further investigation of cognitive-behavioral approaches targeting illness-related concerns in IBS. However, differences in treatment intensity, participant characteristics, and outcome analysis prevent conclusions about comparable efficacy. In particular, the present study does not establish that one session achieves the same benefits as a multi-session CBT program [7].

The findings also fit within a broader, but heterogeneous, literature on single-session interventions. In a primary-care observational study, Perreault et al. [19] reported reductions in perceived problem intensity and psychological distress following a single-session intervention. These findings are broadly compatible with the psychological improvements observed here, although their study lacked a control group and did not specifically concern IBS. Importantly, the evidence is not uniformly positive. The systematic review by Kim et al. [20] identified a trial of single-session behavioral activation in carers that found no significant group-by-time effects for depression or anxiety, and another trial in which a between-group difference in depression was present at two weeks but not at four weeks. These findings differ from the continued reduction in health anxiety observed in the present study. However, differences in populations, therapeutic approaches, and outcomes mean that they represent evidence of variability in SST outcomes rather than direct contradictions of the present IBS

findings. They also caution against assuming that a single session is sufficient across problems or patient groups [20].

The findings for vitality and social functioning should be considered in relation to the multidimensional burden of IBS. Bhinder et al. [14] documented limitations in social activities, activities at home, and participation in work or school among respondents with IBS. Melchior et al. [15] further demonstrated that different aspects of disease-specific quality of life were associated with different combinations of gastrointestinal symptoms, psychological factors, and somatic symptom burden; gastrointestinal-specific anxiety was independently associated with poorer social-role quality of life. These observations provide a rationale for examining functional outcomes alongside health anxiety. However, gastrointestinal-specific anxiety and general health anxiety are related but distinct constructs, and disease-specific quality-of-life domains are not interchangeable with the domains assessed here. Accordingly, these studies offer a conceptual context for the present findings, not direct evidence that reduced health anxiety caused improvements in vitality or social functioning [14,15].

The absence of statistically significant findings across the remaining quality-of-life domains should not be interpreted as proof that these domains cannot benefit from SST. Their response may depend on factors not directly addressed by a focused psychological intervention, consistent with the differing correlates of quality-of-life domains reported by Melchior et al. [15]. Nevertheless, the small sample also limits statistical precision, and the present study cannot determine whether the observed pattern reflects genuinely domain-specific effects or uncertainty in the estimates. Moreover, testing multiple quality-of-life outcomes increases the possibility of chance-positive findings. Bonferroni adjustment of the six health-anxiety comparisons does not control multiplicity across all outcomes and analyses. The vitality and social-functioning findings should therefore be considered exploratory and require confirmation in studies with prespecified outcome priorities and an appropriate multiplicity strategy.

Several psychological processes could plausibly contribute to the observed changes. Cognitive-behavioral stress management may help patients reconsider threatening interpretations of symptoms, develop alternative coping responses, and feel better equipped to manage illness-related concerns. The concurrent changes in health anxiety, perceived stress, and pain self-efficacy reported in the multi-session CBT study by Golshani et al. [7] make these processes relevant candidates for further investigation. Similarly, the association between gastrointestinal-specific anxiety and functional quality-of-life domains reported by Melchior et al. [15] suggests that symptom-related concerns deserve attention when studying functional outcomes. However, neither these comparisons nor the present outcome analyses establish mediation. Changes in catastrophic thinking, avoidance, self-efficacy, or physiological regulation remain proposed mechanisms rather than demonstrated explanations for the intervention effects. Likewise, improvement between post-intervention and follow-up cannot be attributed to continued skills practice because adherence was not directly measured.

The single-session format may warrant further evaluation as an option within stepped psychological care. Schleider and Beidas [18] discuss single-session approaches as a potential strategy for expanding the implementation of evidence-based practices, while Perreault et al. [19] observed increased access to psychological consultations after introducing a single-session service model in primary care. These findings support investigating delivery formats that reduce the commitment required to begin treatment. However, the present trial did not assess service accessibility, cost-effectiveness, acceptability relative to other treatments, or the need for subsequent sessions. Its results therefore support further evaluation of SST as a potentially useful targeted intervention, not a recommendation to replace established multi-session treatment [18–20].

These findings should be considered preliminary and interpreted in light of the methodological limitations outlined below. Statistical significance alone does not establish clinically meaningful improvement, and larger, rigorously designed trials are needed to confirm the findings and assess their durability.

Limitations

This study had several limitations. The small complete-case sample (n = 27) limited statistical power and precision; purposive clinic-based recruitment within a restricted geographical area introduced potential selection bias and limited generalizability despite randomized allocation. Three participants withdrew before post-intervention assessment, and their exclusion from analysis created a risk of attrition bias. The waitlist comparator, absence of blinding, and self-reported outcomes prevented separation of intervention-specific effects from therapeutic attention, expectancy, and reporting biases. Although treatment was structured and supervised, fidelity was not formally monitored. No home practice was assigned, and its potential contribution was not evaluated. Medication use, dietary treatments, and other usual care were not systematically recorded, leaving potential co-intervention effects unaccounted for. The one-month follow-up precluded conclusions about long-term durability. Multiple testing increased false-positive risk: Bonferroni adjustment covered only

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the six health-anxiety comparisons in Table 4, whereas quality-of-life analyses were exploratory and Table 6 comparisons were unadjusted. The original sample-size rationale lacked documented target power, calculation model, and attrition allowance, preventing verification of its adequacy. Future trials should use larger, more diverse samples, active comparators, blinded assessment where feasible, fidelity monitoring, and systematic co-intervention recording; evaluate home practice and booster sessions; extend follow-up to 3, 6, and 12 months; and prospectively specify sample-size calculations, outcomes, missing-data methods, and multiplicity control.

Conclusion

This small randomized trial provides preliminary evidence that single-session cognitive-behavioral stress management may reduce health anxiety in patients with IBS compared with a waitlist condition over a one-month follow-up. Findings for vitality and social functioning were exploratory and do not establish improvement across quality of life as a whole. Clinical significance, longer-term durability, and comparative effectiveness against active or multi-session treatment remain uncertain. The intervention warrants further investigation rather than adoption as a proven substitute for more comprehensive psychological care.

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