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ORIGINAL PAPER

Efficacy of Online Trial-Based Cognitive Therapy, Mindfulness-Based Health Promotion, and Positive Psychotherapy for PTSD During COVID-19: A Randomized Clinical Trial

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Abstract

Objectives To evaluate the comparative efficacy of Trial-Based Cognitive Therapy (TBCT), Mindfulness-Based Health Promotion (MBHP), and Positive Psychotherapy (PP) for COVID-19-related PTSD. This was the first randomized clinical trial to compare these three distinct approaches in a fully online format during a global health crisis.

Method This single-blind, three-arm randomized controlled trial was conducted online. Fifty-seven patients (94.7% women; mean age = 43.09 years) with COVID-19-related PTSD received 14 weekly individual, therapist-guided online sessions. Assessments occurred at baseline, mid-treatment, and post-treatment. The primary outcome was PTSD symptom reduction (CAPS-5); secondary outcomes included anxiety, depression, and well-being.

Results All three treatments yielded significant within-group reductions in PTSD symptoms (large effect sizes, $d > 0.8$). However, no significant between-group differences emerged after correction for multiple comparisons, indicating equivalent efficacy. Reductions in trauma-related guilt and dysfunctional beliefs mediated symptom improvement across all groups.

Conclusions TBCT, MBHP, and PP may all be effective individually delivered online PTSD interventions, with no clear evidence of superiority for any single approach. Findings support the feasibility individual virtual care, suggesting treatment selection could be guided by patient preference and clinician expertise rather than assumed differential efficacy. However, the study's reduced sample size and high dropout rate (57.9%) significantly limit statistical power and generalizability. Adequately powered replication studies are essential to establish definitive comparative effectiveness.

Preregistration ClinicalTrials.gov; NCT04852770.

Keywords COVID-19 · Mindfulness-based health promotion · Positive psychotherapy · Post-traumatic stress disorder · Trial-based cognitive therapy

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Since its emergence in 2019, the COVID-19 pandemic has posed unprecedented challenges to public health, extending far beyond physical illness and significantly impacting global mental health (World Health Organization, 2020). The psychological burden has been considerable, with widespread reports of fear, anxiety, grief, insomnia, and persistent trauma-related symptoms (Brooks et al., 2020). As populations faced prolonged social isolation, economic instability, and continuous exposure to distressing news, a context of collective trauma emerged, substantially contributing to the increase in cases of Post-Traumatic Stress Disorder (PTSD) (Salari et al., 2020; Xiong et al., 2020).

PTSD is a debilitating condition characterized by symptoms of re-experiencing, avoidance, negative alterations in cognition and mood, and hyperarousal (American Psychiatric Association, 2013). The pandemic created an unprecedented environment of shared trauma, with studies estimating the prevalence of COVID-19-related PTSD to range from 7% (Liu et al., 2020) to 53.8% (Wang et al., 2020) in the general population and up to 73.4% among healthcare professionals (Andhavarapu et al., 2022). In response to this urgent clinical need—and amid strict social distancing measures—psychological care had to rapidly adapt to remote formats. Although online psychotherapy had already been expanding, the pandemic accelerated its adoption and established it as an essential mode of service delivery. Previous studies have demonstrated that online interventions for PTSD show moderate effectiveness, with meta-analytic evidence indicating effect sizes of $g = 0.71$ when compared to passive controls and $g = 0.28$ when compared to active treatments, particularly for guided cognitive-behavioral approaches (Sijbrandij et al., 2016). Overall, internet-based psychotherapeutic interventions demonstrate effects comparable to face-to-face therapy, with an average effect size of 0.53 across various mental health conditions (Barak et al., 2008). Nonetheless, remote therapy presents challenges such as technological barriers, limited privacy, and the need to adapt techniques for digital platforms.

Against this backdrop, investigating the efficacy of various psychotherapeutic approaches delivered online has become both timely and necessary to inform evidence-based clinical protocols in crisis contexts. This study evaluated and compared three distinct psychotherapeutic modalities: Trial-Based Cognitive Therapy (TBCT), Mindfulness-Based Health Promotion (MBHP), and Positive Psychotherapy (PP). These interventions were selected based on their distinct theoretical frameworks and emerging evidence supporting their potential therapeutic utility in trauma treatment. TBCT, developed by de Oliveira (2015), employs structured cognitive restructuring procedures that systematically target dysfunctional core beliefs and incorporate specific techniques for addressing trauma-related guilt and self-blame; preliminary research suggests promise for PTSD treatment, though definitive effect sizes remain to be established through rigorous controlled trials (Duran et al., 2021). MBHP integrates mindfulness practices to enhance emotional regulation and psychological acceptance, with meta-analytic evidence demonstrating moderate effectiveness in reducing trauma-related symptomatology (Hedges' $g = 0.46$; Liu et al., 2022). PP adopts a strengths-based approach that fosters post-traumatic growth through the cultivation of positive emotions, identification of personal strengths, and enhancement of meaning-making processes; existing evidence from clinical populations indicates small to moderate beneficial effects on psychological well-being and affective symptoms (Hedges' $g = 0.24 - 0.28$; Chakhssi et al., 2018). Collectively, these therapeutic modalities represent theoretically diverse yet complementary approaches to addressing the multifaceted nature of trauma-related psychological distress in digital healthcare delivery contexts.

Despite promising evidence for each modality when delivered individually, few studies have directly compared their efficacy, particularly in fully online formats and within the context of public health emergencies. Understanding their differential mechanisms of change and patient response profiles is critical for tailoring treatment approaches. To address this gap, we conducted a three-arm randomized clinical trial directly comparing individually delivered online TBCT, MBHP, and PP for COVID-19-related PTSD.

We hypothesized that: (1) TBCT and MBHP would demonstrate superior efficacy in reducing PTSD symptoms compared to PP, based on prior evidence supporting cognitive restructuring and emotional regulation approaches for core PTSD symptoms; (2) TBCT and MBHP would be more effective than PP in reducing trauma-related guilt; (3) all three approaches would lead to improvements in anxiety, depression, and well-being; and (4) symptom improvement would be mediated by changes in trauma-related guilt and dysfunctional core beliefs, with differential mediation patterns across interventions.

By comparing these three interventions in a fully virtual format, this study offers novel evidence on the feasibility, efficacy, and mechanisms of action of distinct psychotherapeutic approaches for PTSD treatment in the context of collective trauma. The findings have important clinical implications for personalized care planning and for the scalable delivery of evidence-based psychological interventions during and beyond global health crises.

This three-arm randomized controlled trial ($n = 57$), conducted entirely online over 14 weekly individual sessions, assessed outcomes at baseline, mid-treatment, and post-treatment, with PTSD symptom severity (CAPS-5) as the primary outcome and anxiety, depression, and psychological well-being as secondary outcomes. By

evaluating these three individually delivered interventions in a fully virtual format during a period of collective trauma, this study provides novel evidence to inform personalized treatment planning and the scalable implementation of evidence-based PTSD care during and beyond global health crises.

Method

Participants

Eligible participants were adult patients (18—60 years) who scored 45 or higher on the PTSD Checklist for DSM-5 (PCL-5), with symptoms originating from or exacerbated by the COVID-19 pandemic. Inclusion criteria were PTSD diagnosis, literacy, comprehension ability, and internet access. Exclusion criteria were severe suicide risk, self-harm behavior, ongoing psychotherapy, psychotic symptoms, and substance abuse within the past year. We calculated the required sample size using G*Power (Faul et al., 2007) considering a mixed design. Parameters considered for statistical analysis included comparison of repeated measures between groups, targeting a Cohen's d effect size of 0.25, α criterion of 0.05, power (1-β) of 0.80, three time points (pre, mid, and post), and a priori correlation between repeated measures of 0.50. Thus, a sample of 108 participants was obtained. Considering a dropout rate of 25%, a total of 135 participants were estimated. However, due to recruitment challenges during the COVID-19 pandemic, including reduced willingness to participate and logistical limitations for online assessments (van Dorn, 2020), the final sample comprised 57 participants who met all inclusion criteria and completed baseline assessments. Post-hoc power analysis with the achieved sample size ($n = 57$) indicated a statistical power of 0.65 for detecting the targeted effect size of 0.25, representing a moderate reduction from the planned power of 0.80.

Procedure

This was a single-blind randomized controlled trial (RCT) with three parallel groups (NCT04852770; April 06, 2020). Further details of the study design can be found in a previously published study protocol. The study adheres to CONSORT guidelines for comprehensive reporting of RCTs (Barbour et al., 2017). Participants (from various Brazilian regions to ensure comprehensive geographical representation) were recruited through media platforms and screened using the PCL-5 (Blevins et al., 2015; De Lima Osório et al., 2017; Pereira-Lima et al., 2019). Trained assessors conducted interviews with the Structured Clinical Interview for DSM-5 (SCID-5) to confirm diagnoses. Participants provided informed consent, including authorization for audio recording of sessions. After completing self-assessment, patients were randomly allocated to TBCT, MBHP, or PP groups using simple randomization. An independent researcher external to the study team assigned participants to groups using random sequence generation software (Random.org Random Integer Generator) with a 1:1:1 allocation ratio. After obtaining consent and conducting initial assessments, the study coordinator received a randomized list from the external researcher. This list, delivered by the external assessor, was kept hidden from both assessors and patients until allocation, ensuring the integrity of the study blinding process through sealed opaque envelopes. The trial coordinator informed therapists about new patients based on this randomized list. All interventions were conducted online through secure platforms (Skype or Google Meet).

Each patient received 14 individual weekly 1-hr sessions over 14—17 weeks. To ensure intervention fidelity, all sessions were audio-recorded for subsequent review by clinical supervisors who assessed adherence to treatment protocols using standardized checklists specific to each intervention.

Trial-Based Cognitive Therapy (TBCT) focuses on altering core cognitions through unique techniques including: (a) psychoeducation about trauma and PTSD symptoms; (b) cognitive restructuring in a theatrical therapeutic environment where patients examine evidence for and against their negative beliefs; (c) addressing trauma-related guilt through specific guilt modification techniques; and (d) working with ambivalence toward recovery. Sessions follow a structured format with homework assignments and practice exercises between sessions (de Oliveira, 2015). Mindfulness-Based Health Promotion (MBHP) teaches mindfulness techniques to promote present-moment awareness without directly confronting cognitive distortions. The intervention includes: (a) daily meditation practices focusing on breath awareness and bodily sensations; (b) specific postures and attention-focusing techniques; (c) non-judgmental awareness exercises; and (d) integration of mindfulness principles into daily activities. Each session includes guided meditation, discussion of home practice, and instruction in new techniques (Demarzo et al., 2015). Positive Psychology (PP) emphasizes cultivating positive core beliefs and strengths rather than restructuring negative beliefs. The intervention includes: (a) identification and development of personal strengths and values; (b) gratitude exercises and cultivation of positive emotions; (c) meaning-building activities; (d) goal setting focused on growth and flourishing; and (e) exercises to build hope and optimism. We used the positive psychotherapy protocol developed by Rashid and Seligman (2018). The study was approved by the Ethics Committee of Climério de Oliveira Maternity, Federal

University of Bahia (CAAE: 30,769,420.0.0000.5543)—April 6, 2020.

Measures

The primary outcome was the reduction of PTSD symptoms measured by the Clinician-Administered PTSD Scale for DSM-5 (CAPS-5). Secondary outcomes included anxiety, depression, and mental well-being. Potential mechanistic variables included guilt and negative core beliefs. Outcome assessments were conducted at three time points: baseline (before the first session), mid-treatment (before the seventh session), and post-treatment (after the fourteenth session). All assessments were conducted by trained assessors blinded to treatment allocation via videoconference. PTSD diagnosis was confirmed using the SCID-5 (First et al., 2015) administered by trained clinicians. Initial screening used the PCL-5 (Blevins et al., 2015), a 20-item self-report measure assessing PTSD symptoms over the past month. CAPS-5 (Weathers et al., 2018) measured PTSD symptom severity through clinician-administered structured interview covering all DSM-5 PTSD criteria. The Hospital Anxiety and Depression Scale (HADS—Zigmond and Snaith, 1983) assessed anxiety and depression symptoms, while the World Health Organization Well-Being Index (WHO-5—Topp et al., 2015) measured subjective well-being. The Trauma-Related Guilt Inventory (TRGI—Kubany et al., 1996) assessed trauma-related guilt cognitions, and the Negative Core Beliefs Inventory (NCBI—Osmo et al., 2018) measured dysfunctional core beliefs about self and others. The California Psychotherapy Alliance Scale—Patient Version (CALPAS-P—Gaston, 1991) assessed therapeutic alliance to ensure equivalent working relationships across treatment groups. For participants who discontinued treatment, we attempted to collect outcome data at scheduled assessment points when possible. Multiple imputation procedures were used for missing data in sensitivity analyses.

Data Analyses

Internal consistency coefficients (e.g., McDonald's ω) were not calculated for the current sample due to the limited sample size ($n = 57$), which precludes the robust factor analysis required for stable estimates. Instead, the study relied on the psychometric properties and validity established in the original and Brazilian validation studies of the utilized instruments. Baseline sociodemographic and clinical characteristics of study participants were described using means (SDs) or frequencies (percentages) as appropriate.

The efficacy of intervention groups on the primary outcome (PTSD symptoms measured by CAPS-5 total score) was assessed using mixed-effects regression models with maximum likelihood estimation, following intention-to-treat (ITT) principles. All randomized participants were included regardless of treatment completion. Models included fixed effects for group, time, and Group × Time interaction, with random intercepts for participants to account for repeated measures. Secondary outcomes (anxiety, depression, and well-being) were analyzed using the same linear mixed-effects modeling approach. Given multiple secondary outcomes, p-values were adjusted using the False Discovery Rate (FDR) method (Benjamini & Hochberg, 1995) to control for Type-I error inflation, with adjusted p-values reported in Online Resource Table S5.

Effect sizes were estimated within the multilevel modeling framework following Rights and Sterba (2019) and Hox et al. (2010), which accounts for both within-subject and between-subject variance in longitudinal data. Conventional Cohen's d values are also reported for comparative purposes but should be interpreted with caution due to their limitations in multilevel contexts.

Three analytic approaches were employed as sensitivity analyses: (1) intention-to-treat with complete cases (ITT—CC) as the primary analysis, (2) per-protocol analysis including participants who completed at least 10 of 14 sessions, and (3) ITT with multiple imputation using chained equations for missing data. The preregistered protocol originally specified per-protocol as the primary analysis; however, we modified this to adopt ITT as the primary strategy and added multiple imputation procedures to enhance methodological rigor and better accommodate the actual sample structure.

All analyses used a two-tailed alpha of 0.05 and were performed using SPSS v24 and Stata v18. Unless otherwise specified, reported estimates are based on ITT—CC analyses, with sensitivity analyses available in Tables 2 and Online Resource Tables S3 – S5. Further methodological details can be found in the published protocol (Duran et al., 2021).

Results

Baseline Sociodemographic and Clinical Characteristics

The baseline sociodemographic and clinical characteristics of participants by group are outlined in Table 1. Participants were middle-aged (M (SD) = 43.09 (9.73) years), female (94.7%), and highly educated. Most participants lived with a partner, had children, and followed Catholic or Evangelical faiths. More than 75% of the participants had pre-existing health issues, and the majority used medication and experienced pre-COVID-19 traumatic events.

Table 1 Baseline characteristics of participants by group

	PP ($n=23$)	MBHP ($n=21$)	TBCT ($n=13$)
Sociodemographic characteristics			
Age, *M* (*SD*)	43.94 (9.50)	42.24 (9.26)	43.00 (11.51)
Sex, females, *n* (%)	22 (95.7%)	20 (95.2%)	12 (92.3%)
Civil status, *n* (%)			
Single	4 (22.2%)	3 (18.8%)	3 (33.3%)
Married/with partner	11 (61.1%)	8 (50.1%)	3 (33.3%)
Separated/divorced	3 (16.7%)	3 (18.8%)	3 (33.3%)
Widower	0 (0%)	2 (12.5%)	0 (0%)
Children, yes, *n* (%)	14 (77.8%)	12 (75.0%)	5 (55.6%)
Religion, *n* (%)			
Catholic	8 (44.4%)	8 (50.0%)	2 (22.2%)
Evangelic	5 (27.8%)	5 (31.3%)	4 (44.4%)
Buddhist	0 (0%)	0 (0%)	1 (11.1%)
Atheist	0 (0%)	0 (0%)	1 (11.1%)
Other	5 (27.8%)	3 (18.8%)	1 (11.1%)
Level of studies, *n* (%)			
Primary	1 (5.6%)	2 (12.5%)	0 (0%))
Secondary	3 (16.7%)	4 (25.1%)	4 (44.4%)
University	7 (38.9%)	2 (12.5%)	2 (22.2%)
Master/PhD	7 (38.9%)	8 (50.1%)	3 (33.3%)
Health problems diagnosed, yes, *n* (%)	13 (76.5%)	14 (87.5%)	7 (77.8%)
Use of medication, yes, *n* (%)	15 (83.3%)	12 (75.0%)	6 (66.7%)
Number of traumatic events, *Md* (*IQR*)	1 (1, 2)	1 (0, 1)	1 (1, 2)
Clinical characteristics			
PTD screening, *M* (*SD*)	67.39 (10.05)	65.14 (11.11)	65.62 (12.39)
CAPS Total, *M* (*SD*)	42.96 (12.26)	43.32 (12.36)	38.85 (10.16)
CAPS re-experiment, *M* (*SD*)	12.48 (2.95)	10.00 (5.07)	10.08 (2.69)
CAPS Avoidance, *M* (*SD*)	5.35 (1.61)	4.24 (2.75)	4.08 (1.89)
CAPS Numbing, *M* (*SD*)	14.91 (5.89)	14.57 (6.63)	13.69 (4.91)
CAPS Hyperarousal, *M* (*SD*)	10.22 (4.13)	10.38 (5.51)	11.00 (3.56)
HADS-A, *M* (*SD*)	13.80 (3.56)	14.65 (4.09)	13.33 (4.01)
HADS-D, *M* (*SD*)	11.35 (3.42)	10.88 (4.39)	11.08 (4.87)
WHO, *M* (*SD*)	6.60 (3.28)	7.00 (4.58)	7.20 (5.22)
TRGI Cognitive, *M* (*SD*)	1.42 (0.64)	1.75 (0.94)	2.01 (0.72)
TRGI Distress, *M* (*SD*)	3.46 (0.58)	3.17 (0.74)	3.22 (0.99)
TRGI Global, *M* (*SD*)	2.08 (0.25)	1.97 (0.37)	1.93 (0.33)
NCBI Others, *M* (*SD*)	2.44 (0.94)	2.52 (0.88)	4.44 (0.92)
NCBI Self, *M* (*SD*)	2.36 (0.68)	2.53 (0.77)	2.48 (0.79)
NCBI Inferiority, *M* (*SD*)	2.53 (0.75)	2.40 (0.80)	2.55 (0.23)
NCBI Unlovability, *M* (*SD*)	3.10 (0.92)	2.95 (1.01)	3.00 (0.93)
NCBI Helplessness, *M* (*SD*)	1.97 (0.96)	2.58 (1.03)	2.22 (1.22)
NCBI Worthlessness, *M* (*SD*)	2.11 (0.81)	2.46 (0.98)	2.12 (1.04)

M = mean. *SD* = standard deviation. *Md* = median. *IQR* = interquartile range; *n* = frequencies. % = percentages; PP = positive psychotherapy; MBHP = mindfulness-based health promotion; TBCT = trial-based cognitive therapy. "Prefer not to say" was an allowed option in all sociodemographic data surveys. Consequently, in the PP group, 5 participants did not report on age, 5 participants did not report on civil status, 5 participants did not report on children, 5 participants did not report on religion, 5 participants did not report on level of studies, 6 participants did not report on health problems, 5 participants did not report on use of medication, 3 participants did not report on HADS-A, 3 participants did not report on HADS-D, 3 participants did not report on HADS-A, 3 participants did not report on WHO, 4 participants did not report on TRGI (cognitive, distress, global), 4 participants did not report on NCBI (others, self, inferiority, Unlovability, helplessness, Worthlessness). In the MBHP group, four participants did not report on age, 5 participants did not report on civil status, 5 participants did not report

Table 1 (continued)

on children, 5 participants did not report on religion, 5 participants did not report on level of studies, 5 participants did not report on health problems, 5 participants did not report on use of medication, 4 participants did not report on HADS-A, 4 participants did not report on HADS-D, 5 participants did not report on WHO, 4 participants did not report on TRGI (cognitive, distress, global), 4 participants did not report on NCBI (others, self, inferiority, Unlovability, helplessness, Worthlessness). In the TBCT group, two participants did not report on age, 4 participants did not report on civil status, 4 participants did not report on children, 4 participants did not report on religion, 4 participants did not report on level of studies, 4 participants did not report on health problems, 4 participants did not report on use of medication, one participant did not report on HADS-A, one participant did not report on HADS-D, 3 participants did not report on WHO, 3 participants did not report on TRGI (cognitive, distress, global), 3 participants did not report on NCBI (others, self, inferiority, Unlovability, helplessness, Worthlessness)

Baseline comparisons between groups were conducted using chi-square tests for categorical variables and one-way ANOVA for continuous variables. The average total CAPS-5 score was M (SD) = 42.11 (11.77), with no significant initial differences among the three groups ($F(2,54) = 1.23$, $p = 0.301$). Traumatic experiences varied, encompassing frontline healthcare work, severe domestic violence during quarantine, kidnappings, severe COVID-19 cases, and loss of close relatives. No significant baseline differences were observed across groups for demographic variables (all $p > 0.05$), ensuring comparability at study initiation.

Program Participation, Retention, and Alliance by Group

The recruitment process for the clinical trial was conducted over approximately three years, from April 6, 2020, to August 18, 2023. Four research centers participated in the study, with various professionals mobilized nationally to refer patients. A total of 562 individuals underwent the PCL-5 as part of initial screening. Among these, 201 individuals were identified as having significant symptoms of PTSD. However, during detailed screening using the CAPS-5, 144 participants were excluded: 139 did not meet diagnostic criteria for PTSD and 5 opted not to continue with the proposed treatment.

After screening and excluding patients who did not meet inclusion criteria, 57 patients were deemed eligible and randomized: 23 to the PP group, 21 to the MBHP group, and 13 to the TBCT group. Session completion rates varied among groups: 73.9% of participants in PP, 47.6% in MBHP, and 69.2% in TBCT completed all proposed sessions (Fig. 1 and Online Resource S1).

Post-treatment retention rates were 52.2% for PP, 23.8% for MBHP, and 53.8% for TBCT. Age differences emerged between participants who lost follow-up and those who remained in the study. Younger participants showed significantly higher dropout rates across all groups ($t(55) = 2.34$, $p = 0.023$). This differential attrition may have influenced results, as younger participants typically present with more severe symptomatology and may require modified intervention approaches. Several psychological measures, including TRGI and various NCBI subscales, also showed disparities, as detailed in Online Resource S2.

The CALPAS-P scores (therapeutic alliance strength) did not differ significantly between groups ($F(2,54) = 0.87$, $p = 0.425$). Consistent scores over time suggest a stable therapeutic alliance for all treatment modalities (Online Resource S3).

Effects on Primary Outcome

Within-Group Changes

Mixed-effects models revealed significant main effects of Time ($F(2,108) = 47.32$, $p < 0.001$) and Group × Time interactions ($F(4,108) = 3.41$, $p = 0.012$) for CAPS-5 scores. Both PP and MBHP showed significant within-group reductions in PTSD symptoms using ITT with complete cases at mid-treatment (PP: $B = -12.80$, $SE = 5.12$, $p = 0.016$; MBHP: $B = -21.05$, $SE = 4.89$, $p < 0.001$) and post-treatment (PP: $B = -30.66$, $SE = 6.23$, $p < 0.001$; MBHP: $B = -25.98$, $SE = 5.87$, $p < 0.001$). TBCT exhibited significant within-group reduction only at post-treatment ($B = -15.28$, $SE = 6.15$, $p = 0.017$).

Between-Group Comparisons

significant between-group differences were detected at post-treatment after correction for multiple comparison in the complete case analysis.

The intention-to-treat analysis with multiple imputations (ITT-MI), revealed some differences between-group at mid- and port-treatment. However, after correction for multiple comparisons using the False Discovery Rate (FDR), none of these between-group contrast on primary outcome remained statistically significant. Therefore, no between-group differences in CAPS-5 score met the threshold for statistical significance after adjustment.

Comprehensive results are presented in Table 2 and illustrated in which displays CAPS-5 score trajectories for each treatment group alongside individual participant trajectories.

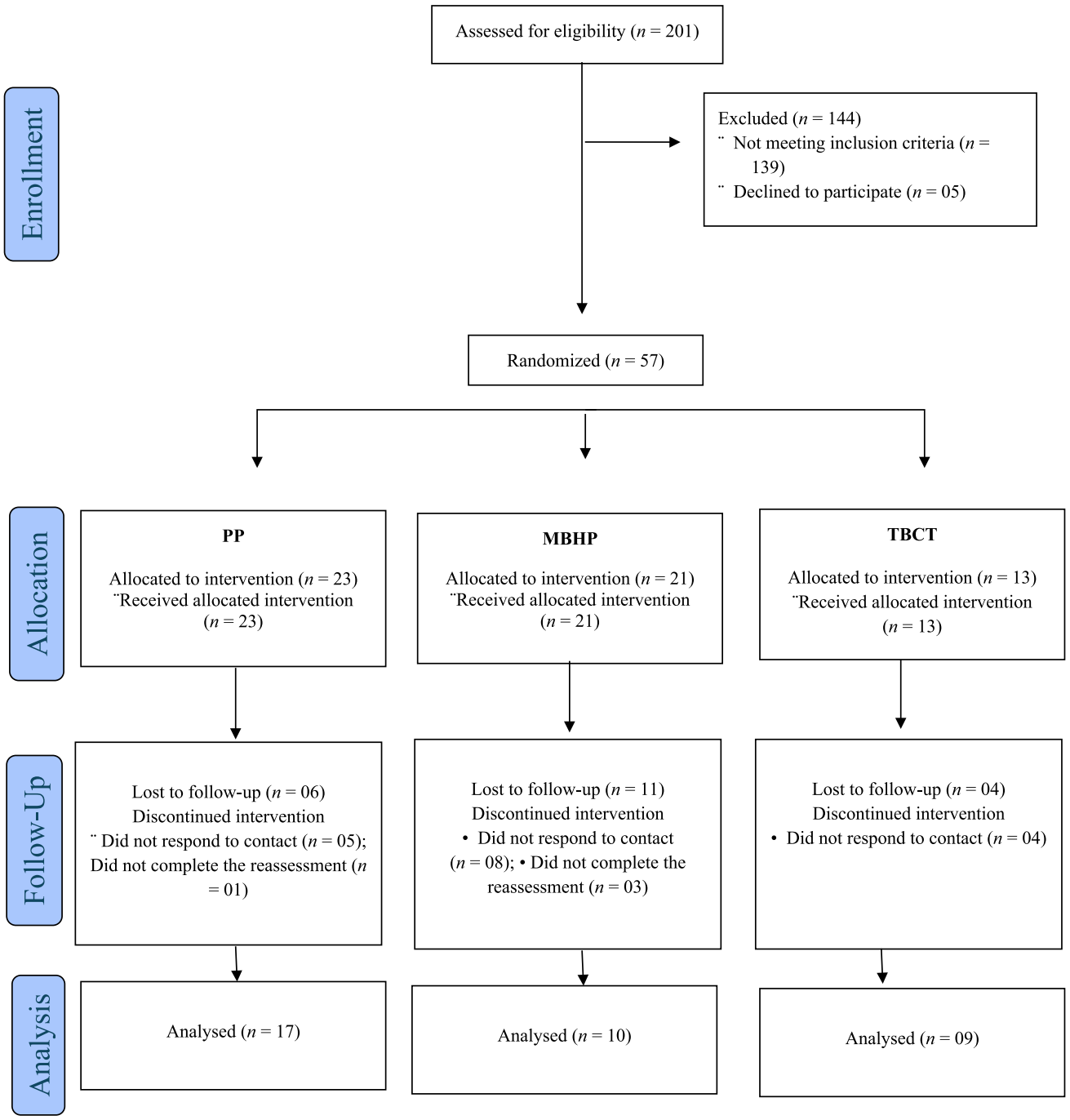

Fig. 1 CONSORT 2010 Flow Diagram

Effects on Secondary Outcomes

Mixed-effects models revealed significant main effects of Time for anxiety (HADS-A: $F(2,98) = 23.45$, $p < 0.001$), depression (HADS-D: $F(2,98) = 18.92$, $p < 0.001$), and well-being (WHO-5: $F(2,96) = 15.67$, $p < 0.001$), indicating within-group improvements across all treatment conditions. However, no significant Group × Time interaction was observed for any of the secondary outcomes: HADS-A ($F(4,98) = 1.23$, $p = 0.345$), HADS-D ($F(4,98) = 0.04$, $p = 0.857$), and WHO-5 ($F(4,96) = 0.55$, $p = 0.554$) (Table 3).

To account for multiple comparisons, p-values for all Group × Time interactions were adjusted using the Benjamini and Hochberg (1995) False Discovery Rate (FDR) method. As shown in Online Resource Table S5,

Table 2 Within- and between-group analysis for reduction in PTSD symptoms (CAPS total score)

		PP	*within*	MBHP	*within*	TBCT	*within*	*PP vs. MBHP*	*PP vs. TBCT*	*MBHP versus TBCT*
		M (SD)	*B (95% CI) d (p)*	*M (SD)*	*B (95% CI) d (p)*	*M (SD)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*
ITT-CC	pre	42.16 (13.14)		40.57 (12.38)		37.25 (11.93)				
	mid	30.16 (21.61)	−12.80 (−23.20, −2.40)	21.71 (9.93)	−21.05 (−30.61, −11.50)	28.50 (17.05)	−10.32 (−21.82, 1.17)	−8.27 (−23.42, 6.89)	2.45 (−13.36, 18.26)	10.71 (−4.05, 25.48)
			−0.61 (**0.016**)		−1.07 (**<.001**)		−0.52 (0.078)	−0.51 (0.285)	0.25 (0.761)	0.78 (0.155)
	post	11.92 (8.10)	−30.66 (−37.38, −23.95)	15.40 (9.92)	−25.98 (−36.90, −15.07)	23.57 (18.84)	−15.28 (−27.85, −2.70)	5.06 (−8.78, 18.89)	15.77 (1.92, 29.61)	10.71 (−5.80, 27.21)
			−1.98 (**< 0.001**)		−1.12 (**< 0.001**)		−0.72 (**0.017**)	0.37 (0.474)	1.24 (**0.026**)	0.88 (0.204)
ITT-MI	mid		−11.08 (−13.23, −8.92)		−34.60 (−36.37, −32.82)		2.77 (−0.41, 5.94)	−23.32 (−26.91, −19.73)	14.05 (10.77, 17.33)	37.30 (33.65, 40.95)
			(**< 0.001**)		(**< 0.001**)		(.088)	(**< 0.001**)	(**< 0.001**)	(**< 0.001**)
	post		−23.36 (−28.54, −18.19)		−37.49 (−42.44, −32.54)		−7.37 (−12.79, −1.95)	−13.96 (−21.95, −5.98)	16.10 (8.95, 23.24)	30.08 (22.59, 37.57)
			(**< 0.001**)		(**< 0.001**)		(**0.008**)	(**0.001**)	(**< 0.001**)	(**< 0.001**)

ITT-CC = intention-to-treat using complete cases. ITT-MI = intention-to-treat using multiple imputations. Multiple imputations were calculated assuming random missing data. Missing outcome data at post-intervention were imputed using chained equations based on linear regressions. The imputation model included the primary and secondary outcomes (i.e., CAPS-total, HADS-Anxiety, HADS-Depression, WHO, TRGI-global, NCBI-others, NCBI-self, NCBI-inferiority, NCBI-Unlovability, ICC-helplessness, NCBI-Worthlessness) at baseline, pre-intervention sociodemographic variables (i.e., age, sex), group allocation (i.e., PP, MBHP, TBCT), and program participation (i.e., number of sessions), which generated 20 imputed datasets. *M (SD)* = mean (standard deviation). *B* = coefficient for the mixed effects regression model. 95% *CI* = 95% confidence interval. *d* = Cohen's *d*. *p* = *p*-value associated with the mixed effects regression model. Mid-treatment: PP ($n = 19$), MBHP ($n = 7$), and TBCT ($n = 8$). Post-treatment: PP ($n = 12$), MBHP ($n = 5$), and TBCT ($n = 7$)

Table 3 Within- and between-group analyses of the secondary outcomes

		PP ($n=19$)	*within*	MBHP ($n=8$)	*within*	TBCT ($n=8$)	*within*	*PP vs. MBHP*	*PP vs. TBCT*	*MBHP versus TBCT*
		M (SD)	*B (95% CI) d (p)*	*M (SD)*	*B (95% CI) d (p)*	*M (SD)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*
CAPS Re-experiencing	pre	12.58 (3.10)		9.75 (4.23)		9.50 (2.67)				
	mid	8.37 (4.21)	−4.17 (−5.64, −2.69)	5.75 (3.11)	−4.25 (−8.03, −0.47)	6.75 (3.66)	−3.31 (−8.03, −0.47)	−0.03 (−3.46, 3.40)	0.99 (−2.56, 4.54)	0.92 (−3.96, 5.81)
			−1.54 (**< 0.001**)		−0.57 (**0.028**)		−0.74 (**0.016**)	−0.06 (0.985)	0.48 (0.585)	0.57 (0.711)
	post	3.42 (2.31)	−9.15 (−10.81, −7.48)	4.33 (2.42)	−5.67 (−9.90, −1.44)	5.71 (5.25)	−4.36 (−7.80, −0.92)	3.40 (−0.85, 7.64)	4.70 (0.42, 8.98)	1.30 (−4.25, 6.86)
			−2.65 (**< 0.001**)		−0.80 (**0.009**)		−0.87 (**0.013**)	1.02 (0.117)	1.74 (**0.031**)	0.44 (0.645)
CAPS Avoidance	pre	5.37 (1.64)		4.13 (2.80)		3.63 (2.13)				
	mid	3.21 (1.62)	−2.14 (−2.98, −1.31)	2.50 (1.31)	−1.74 (−3.74, 0.27)	3.00 (2.07)	−1.08 (−2.80, 0.65)	0.41 (−1.59, 2.40)	1.08 (−1.02, 3.18)	0.66 (−2.06, 3.38)
			−1.11 (**< 0.001**)		−0.37 (0.089)		−0.19 (0.221)	0.42 (0.691)	0.83 (0.312)	0.38 (0.634)
	post	1.71 (1.82)	−3.51 (−4.26, −2.76)	0.57 (1.16)	−3.67 (−5.20, −2.14)	2.11 (1.69)	−1.97 (−3.51, −0.42)	−0.08 (−1.82, 1.66)	1.62 (−0.14, 3.37)	1.70 (−0.59, 3.99)
			−2.67 (**< 0.001**)		−0.79 (**< 0.001**)		−0.40 (**0.012**)	−0.04 (0.930)	1.12 (0.071)	0.77 (0.145)
CAPS Numbing	pre	14.83 (5.85)		13.00 (7.45)		13.13 (4.82)				
	mid	7.83 (5.82)	−7.04 (−9.73, −4.36)	7.50 (4.54)	−6.94 (−11.82, −2.05)	10.37 (5.90)	−3.22 (−7.55, 1.11)	0.49 (−4.68, 5.65)	3.92 (−1.41, 9.25)	3.63 (−3.03, 10.29)
			−1.17 (**< 0.001**)		−0.55 (**0.005**)		−0.45 (0.145)	0.23 (0.853)	0.74 (0.150)	0.41 (0.286)
	post	2.92 (2.39)	−11.93 (−15.35, −8.52)	6.00 (3.90)	−8.57 (−14.18, −2.96)	6.71 (6.70)	−6.98 (−12.10, −1.86)	3.42 (−2.92, 9.77)	5.02 (−1.38, 11.42)	1.59 (−6.07, 9.25)
			−1.49 (**< 0.001**)		−0.55 (**0.003**)		−0.93 (**0.008**)	0.73 (0.290)	0.95 (0.124)	0.09 (0.684)
CAPS Hyperarousal	pre	9.95 (4.30)		8.63 (5.81)		11.00 (3.89)				
	mid	5.95 (4.03)	−4.12 (−5.77, −2.46)	6.50 (2.45)	−3.67 (−7.51, 0.18)	8.38 (6.70)	−2.63 (−6.87, 1.62)	0.87 (−3.13, 4.87)	1.56 (−2.57, 5.69)	1.11 (−4.62, 6.83)
			−1.03 (**< 0.001**)		−0.28 (0.062)		−0.50 (0.225)	0.38 (0.669)	0.32 (0.460)	0.09 (0.705)
	post	3.58 (2.97)	−6.36 (−8.51, −4.21)	5.67 (3.62)	−4.71 (−9.42, −0.01)	8.43 (6.00)	−2.60 (−6.69, 1.50)	1.91 (−3.19, 7.03)	4.06 (−1.09, 9.21)	2.14 (−4.21, 8.49)
			−1.31 (**< 0.001**)		−0.28 (**0.049**)		−0.48 (0.214)	0.67 (0.461)	0.87 (0.122)	0.08 (0.508)
HADS-A	pre	14.38 (3.10)		14.29 (3.86)		11.80 (3.49)				
	post	9.23 (4.92)	−4.62 (−7.41, −1.83)	7.86 (1.95)	−6.66 (−9.44, −3.89)	7.20 (5.59)	−5.09 (−8.04, −2.13)	−1.96 (−6.02, 2.11)	−0.98 (−5.53, 3.57)	1.31 (−2.73, 5.36)
			−1.23 (**0.001**)		−1.40 (**< 0.001**)		−1.99 (**0.001**)	−0.36 (0.345)	−0.16 (0.674)	0.46 (0.524)
HADS-D	pre	11.15 (2.85)		11.57 (3.51)		9.00 (5.00)				
	post	6.00 (4.95)	−5.25 (−7.37, −3.13)	6.29 (4.23)	−5.05 (−7.64, −2.45)	5.80 (4.32)	−5.14 (−9.96, −0.31)	0.36 (−3.53, 4.25)	0.91 (−3.45, 5.27)	0.33 (−4.94, 5.60)

Table 3 (continued)

		PP ($n=19$)	*within*	MBHP ($n=8$)	*within*	TBCT ($n=8$)	*within*	*PP vs. MBHP*	*PP vs. TBCT*	*MBHP versus TBCT*
		M (SD)	*B (95% CI) d (p)*	*M (SD)*	*B (95% CI) d (p)*	*M (SD)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*	*B (95% CI) d (p)*
			−1.95 (**< 0.001**)		−1.64 (**< 0.001**)		−0.47 (**0.037**)	0.04 (0.857)	0.53 (0.683)	0.46 (0.902)
WHO	pre	6.83 (3.13)		7.40 (3.58)		6.75 (5.25)				
	post	14.50 (5.42)	7.83 (5.22, 10.45)	13.20 (2.39)	6.67 (2.78, 10.55)	14.25 (4.99)	6.81 (3.62, 10.00)	−1.37 (−5.93, 3.18)	−2.20 (−7.15, 2.75)	−0.27 (−5.55, 5.01)
			2.17 (**< 0.001**)		1.03 (**0.001**)		2.20 (**< 0.001**)	−0.55 (0.554)	−0.04 (0.384)	−0.35(0.920)

M (SD) = mean (standard deviation). *B* = coefficient for the mixed effects regression model (intention-to-treat using complete cases). 95% *CI* = 95% confidence interval. *d* = Cohen's *d*. *p* = *p*-value associated with the mixed effects regression model. HADS-A: PP ($n=13$), MBHP ($n=7$), TBCT ($n=5$). HADS-D: PP ($n=13$), MBHP ($n=7$), TBCT ($n=5$). WHO: PP ($n=12$), MBHP ($n=5$), TBCT ($n=4$). CAPS re-experiencing (post): PP ($n=12$), MBHP ($n=6$), TBCT ($n=7$). CAPS avoidance (post): PP ($n=14$), MBHP ($n=14$), TBCT ($n=9$). CAPS numbing (post): PP ($n=12$), MBHP ($n=6$), TBCT ($n=7$). CAPS hyperarousal (post): PP ($n=12$), MBHP ($n=6$), TBCT ($n=7$)

none of the interactions remained statistically significant after correction.

Descriptively, all three groups showed meaningful within-group reductions in anxiety and depression scores. For HADS-A, pre-treatment mean scores were 14.38 ($SD=3.10$) for PP, 14.29 ($SD=3.86$) for MBHP, and 11.80 ($SD=3.49$) for TBCT, decreasing post-treatment to 9.23 ($SD=4.92$), 7.86 ($SD=1.95$), and 7.20 ($SD=5.59$), respectively. Similarly, for HADS-D, baseline scores were 11.15 ($SD=2.85$) for PP, 11.57 ($SD=3.51$) for MBHP, and 9.00 ($SD=5.00$) for TBCT, reducing to 6.00 ($SD=4.95$), 6.29 ($SD=4.23$), and 5.80 ($SD=4.32$) respectively. A detailed analysis of the weekly trajectory for these outcomes is presented in Online Resource S4.

Quality of life (WHO-5) also improved significantly over time across all groups ($F(2,96)=15.67$, $p<0.001$), with no significant differences between groups ($F(4,96)=0.55$, $p=0.554$).

Comprehensive descriptive statistics are provided in Online Resource Table S3A, with inferential results presented in Table S3B. After applying FDR correction (Table S5), none of the Group × Time effects for secondary outcomes reached statistical significance. These findings underscore the exploratory nature of the secondary analyses and warrant cautious interpretation, particularly given the limited statistical power of the study.

Effects on Mechanistic Variables

Group × Time interaction effects were examined for all mechanistic variables. None of the interactions reached statistical significance after correction for multiple comparisons using the False Discovery Rate (FDR). For example, TRGI Cognitive showed $F=2.11$, $p=0.117$ (FDR-adjusted $p=0.600$), and NCBI Inferiority had $F=0.46$, $p=0.634$ (FDR-adjusted $p=0.780$). Complete model results for all mechanistic variables are presented in Online Resource Tables S3B and S5.

Despite the lack of statistically significant interaction effects, significant within-group improvements were observed across all groups in both TRGI and NCBI subscales. A between-group difference emerged in TRGI Cognitive, with MBHP showing superior improvement compared to PP at mid-treatment ($B=-0.60$, 95% $CI=-1.16$ to -0.04, $p=0.05$). However, this difference was not maintained statistical significant after correction for multiple comparisons using the FDR and was not maintained at post-treatment ($B=-0.29$, 95% $CI=-1.00$ to 0.41, *ns*). Detailed data are presented in Table 4.

Table 4 Within- and between-group analyses of the TRGI and NCBI mechanistic variables

		PP	*within*	MBHP	*within*	TBCT	*within*	*PP vs. MBHP*	*PP vs. TBCT*	*MBHP versus TBCT*
		M (SD)	*B (95% CI)*	*M (SD)*	*B (95% CI)*	*M (SD)*	*B (95% CI)*	*B (95% CI)*	*B (95% CI)*	*B (95% CI)*
TRGI Global	pre	2.02 (0.18)		2.07 (0.45)		1.81 (0.24)				
	mid	1.78 (0.56)	−0.26 (−0.52, 0.01)	1.75 (0.85)	−0.19 (−0.58, 0.20)	1.88 (0.25)	−0.13 (−0.47, 0.22)	0.08 (−0.37, 0.53)	0.14 (−0.41, 0.68)	0.09 (−0.50, 0.67)
	post	1.96 (0.30)	−0.12 (−0.34, 0.10)	2.05 (0.11)	0.02 (−0.25, 0.30)	1.88 (0.43)	−0.03 (−0.40, 0.35)	0.19 (−0.19, 0.57)	0.10 (−0.32, 0.51)	−0.08 (−0.56, 0.39)
TRGI Cognitive	pre	1.34 (0.66)		1.60 (0.97)		1.94 (0.84)				
	mid	1.15 (0.44)	−0.20 (−0.45, 0.05)	0.94 (0.25)	−0.82 (−1.40, −0.24)†	1.15 (0.52)	−0.86 (−1.50, −0.21)†	−0.60 (−1.16, −0.04)*	−0.64 (−1.29, 0.02)	−0.01 (−0.92, 0.89)
	post	0.94 (0.42)	−0.43 (−0.77, −0.09)*	0.89 (0.39)	−0.89 (−1.67, −0.11)*	1.22 (0.42)	−0.72 (−1.32, −0.13)*	−0.29 (−1.00, 0.41)	−0.28 (−1.05, 0.49)	0.05 (−0.98, 1.08)
TRGI Distress	pre	3.37 (0.65)		3.39 (0.39)		2.75 (1.32)				
	mid	2.36 (1.08)	−1.02 (−1.53, −0.50)‡	2.36 (1.23)	−1.09 (−1.79, −0.39)†	1.88 (1.32)	−1.12 (−2.02, −0.22)*	−0.08 (−0.95, 0.80)	−0.17 (−1.20, 0.86)	−0.04 (−1.17, 1.09)
	post	1.72 (1.27)	−1.72 (−2.34, −1.09)‡	1.93 (0.96)	−1.15 (−1.85, −0.45)†	2.58 (0.57)	−0.82 (−1.78, 0.14)	0.59 (−0.40, 1.59)	0.90 (−0.18, 2.00)	0.30 (−0.88, 1.47)
NCBI Others	pre	2.22 (0.97)		2.58 (0.86)		2.09 (0.80)				
	mid	2.22 (0.71)	−0.12 (−0.49, 0.25)	1.89 (0.67)	−0.70 (−1.35, −0.06)*	1.67 (0.49)	−0.51 (−1.04, 0.02)	−0.56 (−1.23, 0.11)	−0.40 (−1.14, 0.34)	0.13 (−0.77, 1.03)
	post	2.11 (0.86)	−0.24 (−0.66, 0.18)	1.69 (0.63)	−0.81 (−1.57, −0.04)*	1.36 (0.39)	−0.32 (−0.70, 0.07)	−0.51 (−1.28, 0.27)	−0.29 (−1.14, 0.56)	0.11 (−0.93, 1.16)
NCBI Self	pre	2.36 (0.68)		2.53 (0.77)		2.48 (0.79)				
	mid	3.66 (0.96)	1.30 (0.70, 1.90)‡	3.84 (1.12)	1.31 (0.45, 2.17)†	3.52 (1.03)	1.04 (0.24, 1.83)*	-0.14 (−1.13, 0.84)	0.17 (−0.64, 0.98)	0.31 (−0.79, 1.40)
	post	1.95 (0.49)	−0.41 (−0.78, −0.05)*	1.87 (0.49)	−0.66 (−1.13, −0.19)†	0.09 (0.01)	−0.592 (−1,13, 0.05)*	0.10 (−0.28, 0.47)	0.08 (−0.36, 0.52)	−0.02 (−0.54, 0.49)
NCBI Inferiority	pre	2.48 (0.74)		2.71 (0.43)		2.60 (0.29)				
	mid	2.54 (0.46)	0.01 (−0.35, 0.37)	2.58 (0.41)	−0.07 (−0.41, 0.27)	2.20 (0.45)	−0.38 (−0.69, −0.07)*	0.04 (−0.54, 0.61)	−0.40 (−1.04, 0.25)	−0.43 (−1.11, 0.26)
	post	2.58 (0.47)	0.04 (−0.35, 0.42)	2.60 (0.45)	−0.09 (−0.28, 0.11)	2.38 (0.32)	−0.14 (−0.38, 0.10)	0.03 (−0.55, 0.61)	−0.17 (−0.80, 0.47)	−0.06 (−0.41, 0.29)
NCBI Unlovability	pre	3.01 (1.02)		2.90 (0.99)		2.64 (0.62)				
	mid	2.53 (0.62)	−0.56 (−0.94, −0.18)†	2.57 (0.90)	−0.63 (−1.45, 0.20)	2.08 (0.52)	−0.63 (−1.28, 0.02)	0.02 (−0.79, 0.83)	−0.08 (−0.98, 0.83)	−0.11 (−1.33, 1.12)
	post	2.58 (1.00)	−0.52 (−1.06, 0.02)	2.72 (0.58)	−0.29 (−1.00, 0.43)	2.45 (0.66)	−0.43 (−1.25, 0.39)	0.22 (−0.69, 1.13)	0.17 (−0.82, 1.16)	−0.05 (−1.14, 1.04)
NCBI Helplessness	pre	1.83 (0.91)		2.08 (0.95)		1.40 (0.56)				
	mid	1.56 (0.67)	−0.33 (−0.67, 0.01)	1.94 (0.88)	−0.48 (−1.08, 0.12)	1.13 (0.14)	−0.41 (−0.95, 0.13)	−0.10 (−0.73, 0.53)	−0.17 (−0.86, 0.52)	−0.08 (−0.90, 0.75)

Table 4 (continued)

		PP	*within*	MBHP	*within*	TBCT	*within*	*PP vs. MBHP*	*PP vs. TBCT*	*MBHP versus TBCT*
		M (SD)	*B (95% CI)*	*M (SD)*	*B (95% CI)*	*M (SD)*	*B (95% CI)*	*B (95% CI)*	*B (95% CI)*	*B (95% CI)*
	post	1.47 (0.51)	−0.28 (−0.65, 0.09)	1.70 (0.82)	−0.59 (−1.32, 0.13)	1.21 (0.16)	−0.28 (−0.87, 0.30)	−0.15 (−0.86, 0.55)	−0.11 (−0.87, 0.66)	0.05 (−0.92, 1.02)
NCBI Worthlessness	pre	1.92 (0.78)		2.20 (1.14)		1.40 (0.30)				
	mid	1.44 (0.66)	−0.63 (−1.02, −0.24)†	1.86 (0.62)	−0.78 (−1.51, −0.06)*	1.07 (0.15)	−0.37 (−0.64, −0.09)†	−0.05 (−0.79, 0.69)	−0.05 (−0.87, 0.77)	−0.01 (−1.01, 1.00)
	post	1.44 (0.48)	−0.50 (−0.90, −0.10)*	1.57 (0.55)	−0.99 (−1.82, −0.15)*	1.29 (0.16)	−0.10 (−0.31, 0.11)	−0.20 (−0.97, 0.58)	0.12 (−0.73, 0.96)	0.32 (−0.79, 1.43)

M (SD) = mean (standard deviation). *B* = coefficient for the mixed effects regression model (intention-to-treat using complete cases). 95% *CI* = 95% confidence interval. ‡ $p < 0.001$. † $p < 0.01$. * $p < 0.05$. (All the p-values are associated with the corresponding mixed effects regression model). TRGI global: PP ($n = 15$), MBHP ($n = 7$), TBCT ($n = 4$). TRGI cognitive: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 4$). TRGI distress: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 4$). NCBI others: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 5$). NCBI self: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 5$). NCBI inferiority: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 5$). NCBI Unlovability: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 5$). NCBI helplessness: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 5$). NCBI Worthlessness: PP ($n = 14$), MBHP ($n = 6$), TBCT ($n = 5$)

Dropout Analysis by Age Groups

Attrition analyses revealed significant age-related disparities in retention rates. Younger participants (18—35 years, $n = 28$) exhibited significantly higher dropout rates (41.2%) compared to older participants (> 35 years, $n = 29$; 22.1%), $\chi^2(1) = 3.94$, $p = 0.048$. Furthermore, the timing of attrition differed between groups: the median time to dropout was 6.2 weeks (*IQR*: 4.1–9.3) for younger participants, whereas older participants who withdrew remained in treatment longer, with a median time of 8.7 weeks (*IQR*: 6.2–11.4).

Clinical Significance and Implications

The findings demonstrate that all three interventions produced clinically meaningful reductions in PTSD symptoms. PP and MBHP showed the most consistent effects, achieving significant symptom reduction by mid-treatment, whereas TBCT demonstrated significant benefits primarily at post-treatment. The high dropout rates, particularly among younger participants, highlight the need for age-adapted intervention strategies and enhanced retention protocols in future studies. These results suggest that the approaches tested here may offer alternatives to cognitive-behavioral interventions for the online treatment of PTSD, broadening the spectrum of evidence-based treatment options.

Discussion

This randomized controlled trial aimed to compare the efficacy of three online psychotherapeutic interventions, TBCT, MBHP and PP, for treating PTSD symptoms during the COVID-19 pandemic. The most important finding was that all three interventions produced significant within-group improvements in PTSD symptoms, anxiety, depression, and well-being, with no statistically robust differences emerging between treatment groups after correction for multiple comparisons. This pattern of equivalent efficacy across diverse therapeutic approaches has critical implications for expanding access to evidence-based PTSD treatment through digital platforms, particularly when traditional face-to-face therapy is not feasible.

Contrary to our initial hypotheses that TBCT and MBHP would demonstrate superior outcomes compared to PP, the results revealed comparable effectiveness across all three modalities. While some pairwise comparisons suggested potential advantages for specific interventions under certain analytic conditions, these differences did not withstand correction for multiple comparisons and should be considered exploratory. This equivalence in treatment outcomes aligns with recent meta-analyses suggesting that

common therapeutic factors, such as therapeutic alliance, patient engagement, and session completion, may play a more substantial role than specific therapeutic techniques in determining treatment success (Flückiger et al., 2018; Saxler et al., 2024). Supporting this interpretation, our analysis revealed no significant differences in therapeutic alliance scores (CALPAS-P) across treatment groups, reinforcing the centrality of non-specific elements in digital therapeutic formats.

Our results align with the broader literature indicating that multiple therapeutic approaches can be effective in the treatment of PTSD. Prior studies have supported the efficacy of cognitive-behavioral therapies (Gaudiano & Miller, 2013; Öst et al., 2023), mindfulness-based interventions (Dumarkaite et al., 2022; Liu et al., 2022), and positive psychology approaches (Rashid & Seligman, 2018) for reducing trauma-related symptoms. The COVID-19 pandemic, however, prompted a necessary and rapid shift to telehealth delivery of mental health care, and emerging research has demonstrated that online formats can achieve outcomes comparable to traditional face-to-face PTSD treatments (Swint et al., 2024; Scott et al., 2022).

Importantly, our study contributes to the still limited body of evidence examining fully online PTSD interventions implemented during an active global health crisis. Unlike pre-pandemic trials conducted under more controlled conditions, our participants were exposed to ongoing stressors and uncertainties related to COVID-19. These contextual factors may have influenced both treatment responsiveness and retention and are essential to consider when interpreting our findings.

The absence of consistent between-group differences in outcomes aligns with recent meta-analyses suggesting that the specific therapeutic modality may play a smaller role than common therapeutic factors such as alliance quality, patient engagement, and session completion (Flückiger et al., 2018; Saxler et al., 2024). Supporting this view, our results showed no significant differences in therapeutic alliance scores (CALPAS-P) across treatment groups, reinforcing the idea that non-specific elements may be central to treatment success in digital formats.

Despite the study's limitations, several important clinical implications emerge. First, the demonstrated efficacy of all three online therapeutic approaches suggests that clinicians have multiple viable options for delivering PTSD treatment via telehealth. This flexibility is particularly valuable when in-person interventions are not feasible, such as during public health emergencies, in geographically remote areas, or for individuals with mobility limitations.

However, the elevated dropout rates, particularly among younger participants, point to critical challenges in implementing online PTSD care. Retention rates varied across groups (PP: 52.2%, TBCT: 53.8%, MBHP: 28.6%), suggesting that both intervention structure and treatment fit may influence adherence. The lower retention rate in the MBHP group may be related to the daily mindfulness practice requirement and the introspective nature of the intervention, which can be particularly challenging for individuals with PTSD experiencing high stress. Additionally, participants reported difficulties maintaining practice outside of sessions, especially in the context of hypervigilance and emotional overload during the pandemic, which may have negatively impacted adherence. Tailored engagement strategies for younger adults may include more frequent therapist contact, modular or shorter sessions, and hybrid delivery formats that integrate in-person and remote elements. These findings support a personalized approach to PTSD treatment that considers individual characteristics and preferred mechanisms of change. For healthcare professionals, the data highlights the need for tailored adaptations, including (1) flexible scheduling to accommodate shift work, (2) trauma-informed modifications addressing moral injury, and (3) additional psychoeducation about occupational re-traumatization during treatment.

In terms of treatment intensity, dropout patterns suggest that more intensive delivery formats, such as 2–3 sessions per week over 5–7 weeks, may enhance retention, particularly for younger individuals. This structure may help maintain therapeutic momentum while minimizing long-term commitment. Future protocols should consider offering both standard and intensive tracks based on patient preference and demographic factors.

Finally, technological enhancements could further improve engagement. Mobile app components for between-session practice, gamification features for younger users, and automated reminders may help strengthen adherence and continuity in virtual PTSD care.

The predominance of female participants (94.7%) in our study reflects patterns commonly observed in PTSD research and clinical practice in Brazil. This gender distribution is consistent with epidemiological studies showing higher PTSD prevalence among women (Ribeiro et al., 2023), increased help-seeking behavior among females (Douglas & Shafer, 2015), and cultural factors in Latin American contexts that may influence mental health service utilization (Nuñez et al., 2016). Brazilian studies of trauma and PTSD consistently report similar gender distributions, with women comprising 75—90% of participants in clinical samples (Zambaldi et al., 2011; Olff, 2017). This pattern reflects both biological vulnerabilities and sociocultural factors affecting trauma exposure and recovery in Brazilian populations.

Our recruitment difficulties provide important insights into future pandemic-era mental health research. The 10% conversion rate from initial screening to study completion was lower than anticipated and reflects numerous factors. Firstly, the discrepancy between screening and diagnostic

interviews suggests that pandemic-related distress may not always meet full PTSD criteria, highlighting the importance of rigorous diagnostic procedures. This distinction is further supported by the development of specific instruments to capture the unique aspects of pandemic stress, such as the COVID-19 Stress Scale (CSS) validated in an Iranian non-clinical population (Nooripour et al., 2022). Secondly, the pandemic context created unique barriers to research participation, including technological challenges, privacy concerns in shared living spaces, and competing demands from health and economic stressors. Finally, the 14-session commitment may have been prohibitive for individuals experiencing acute pandemic-related stressors, suggesting that shorter, more flexible interventions might improve recruitment and retention. Future studies should consider hybrid recruitment strategies, more flexible intervention designs that accommodate varying participant circumstances, enhanced technological support and training, and preliminary engagement phases to assess treatment readiness before randomization.

Several research priorities emerge from our findings. First, adequately powered comparative effectiveness studies are needed to definitively establish the relative efficacy of different online PTSD interventions. These studies should incorporate longer follow-up periods to assess the maintenance of treatment gains and should stratify analyses by relevant participant characteristics, such as age, trauma type, and comfort with technology.

Additionally, comparative effectiveness research examining intensive versus standard scheduling is warranted to optimize treatment delivery. Randomized controlled trials comparing weekly sessions to massed formats (e.g., two or three sessions per week) could inform evidence-based scheduling recommendations and potentially improve retention and outcomes.

Second, profession-specific interventions merit focused investigation. Although this study identified specific challenges among healthcare workers, dedicated surveys are needed to fully understand their needs.

Third, exploring hybrid treatment models that combine virtual sessions with brief in-person components or peer support groups could offer a promising avenue to enhance engagement and retention while preserving the accessibility advantages of online therapy.

Fourth, implementation of scientific research is crucial to support the scalable adoption of online PTSD interventions globally. Studies examining how these interventions can be effectively integrated into existing healthcare systems, including evaluation of cost-effectiveness, quality assurance procedures, and clinician training requirements, are essential for real-world deployment.

Furthermore, the mechanisms of change observed in our mediation analyses require replication and extension. Future research should investigate the temporal sequencing between mediating variables and clinical outcomes to better elucidate how and when therapeutic change occurs in online formats.

Finally, the use of predictive modeling to identify patients most likely to benefit from specific virtual interventions could enable precision mental health care. Such approaches would facilitate personalized treatment matching, potentially improving both clinical outcomes and retention rates.

In summary, this randomized controlled trial provides preliminary evidence that multiple online psychotherapeutic approaches are viable for reducing PTSD symptoms in the context of the COVID-19 pandemic. All three interventions, TBCT, MBHP, and PP, led to significant within-group improvements, with no between-group differences reaching statistical significance after correction for multiple comparisons. These findings suggest that treatment selection may be better guided by patient preferences, clinician expertise, and contextual factors rather than assumptions of comparative efficacy.

Recruitment and retention challenges observed in this study offer valuable insights for conducting trauma-focused research during crises. High dropout rates among younger participants and healthcare professionals indicate that treatment delivery models must be tailored to the specific needs of these groups. Adaptations may include adjusting session frequency and duration, modifying content to address moral injury, and accommodating demanding work schedules, particularly among frontline healthcare workers.

The scalability, accessibility, and reduced stigma associated with virtual interventions position them as essential tools for addressing the global PTSD treatment gap, especially in post-pandemic settings where demand for mental health services continues to rise. Nevertheless, the absence of statistically robust evidence for comparative effectiveness underscores the need for adequately powered trials with extended follow-up periods. Future research should incorporate adaptive designs capable of accounting for ongoing external stressors and variability in participant engagement. As digital platforms become increasingly integrated into mental health care, the findings from this trial offer meaningful support for the implementation of scalable, accessible, and evidence-based virtual PTSD treatments. Equally important, the operational challenges encountered throughout this study provide critical methodological insights. Together, these clinical and procedural contributions can inform future efforts to advance trauma-focused research and improve the delivery of psychological care in dynamic and high-stakes global contexts.

Limitations and Future Directions

Although this randomized controlled trial demonstrated that TBCT, MBHP, and PP were each associated

with significant within-group improvements in PTSD symptoms, anxiety, depression, and well-being, with no statistically robust between-group differences after correction for multiple comparisons, these findings must be interpreted cautiously. The apparent equivalence across interventions, while clinically encouraging and consistent with literature emphasizing common therapeutic factors, is substantially constrained by methodological limitations, particularly reduced statistical power, high attrition rates, and the contextual challenges of conducting fully online psychotherapy during an active global health crisis. Consequently, the absence of differential effects should not be interpreted as definitive evidence of equal efficacy, but rather as preliminary evidence requiring confirmation in adequately powered and methodologically optimized trials.

In addition to the fundamental limitation of inadequate statistical power, the following constraints should be acknowledged. First, our final sample size ($n = 57$) was smaller than the target ($n = 135$), primarily due to recruitment challenges associated with the COVID-19 pandemic. This reduction limits both the statistical power to detect between-group differences and the generalizability of the findings, particularly for subgroup analyses.

Second, the study experienced high dropout rates, especially among younger participants (41.2% dropout vs. 22.1% in older adults). Feedback obtained during exit interviews suggested that the 14-to-17-week protocol may not have been optimal for all demographics, particularly younger adults, who expressed a preference for shorter, more intensive treatment formats. This highlights the need to explore adaptive treatment schedules that account for demographic-specific engagement patterns.

Third, the single-blind design prevented participant blinding, introducing the possibility of expectancy effects that could have influenced self-reported outcomes. While blinding evaluators helped mitigate assessment bias, participants' awareness of their treatment allocation remains a methodological limitation common to most psychotherapy trials.

Fourth, the heterogeneity of trauma experiences within the sample, including healthcare-related trauma, domestic violence, bereavement, and severe illness, added interpretive complexity. Because randomization was not stratified by trauma type, it is possible that differences in trauma profiles influenced both engagement and treatment outcomes, limiting the precision of our findings.

Fifth, while the exclusive use of online delivery was appropriate given pandemic-related constraints, it may have disadvantaged participants with limited technological proficiency or inadequate internet access. This introduced a selection bias toward more technologically capable individuals, which may limit the generalizability of our results to broader clinical populations. Additionally, healthcare workers in our sample encountered unique challenges, such as work-related re-traumatization during treatment and scheduling conflicts due to irregular shifts. These barriers underscore the need for profession-specific adaptations in both research design and clinical protocols.

Sixth, our follow-up was limited to immediate post-treatment assessments, preventing evaluation of longer-term treatment effects. Considering that PTSD often requires sustained treatment and that maintenance of therapeutic gains can vary over time, the absence of longitudinal follow-up is a significant limitation regarding the clinical applicability of our findings.

Finally, the study's cultural context must be considered. The Brazilian, Portuguese-speaking sample, although geographically diverse, may limit cross-cultural generalizability. Variations in trauma conceptualization, help-seeking behaviors, and therapeutic relationship dynamics across cultures suggest that findings may not be fully transferable to other populations without further validation.

An important limitation of this study was the small final sample size ($n = 24$ with post-treatment data), which prevented mediation analyses with adequate statistical power. Mediation analyses generally require substantially larger samples ($N > 100$) to produce stable estimates and reliable confidence intervals. Consequently, it was not possible to examine the mechanisms through which the interventions may have produced their effects. Future studies with larger samples are needed to investigate the underlying change processes of the three therapeutic approaches.

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Flávia Leite Moris: Conceptualization, Methodology, Investigation—Original draft preparation.

Leonardo Machado: Conceptualization, Methodology, Investigation—Original draft preparation, Supervision.

Irismar Reis de Oliveira: Conceptualization, Methodology, Investigation, Writing—Original draft preparation, Supervision.

Marcelo Demarzo: Conceptualization, Methodology, Investigation—Original draft preparation, Supervision, Funding acquisition.

Jesus Montero-Marin: Formal analysis, Writing – Review & Editing.

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Data Availability Due to ethical and privacy restrictions associated with clinical data, the datasets generated and analyzed during this study are not publicly available. Anonymized data may be made available by the corresponding author upon request.

Declarations

Ethics Approval The study was conducted in accordance with the Declaration of Helsinki and approved by the Ethics Committee of the Climério de Oliveira Maternity, Federal University of Bahia (CAAE: 30769420.0.0000.5543) on April 6, 2020.

Informed Consent Informed consent was obtained from all subjects involved in the study via an online form. Participants explicitly authorized the audio recording of therapy sessions for supervision and fidelity assessment purposes.

Conflict of interest I.R.d.O. is the developer of Trial-Based Cognitive Therapy (TBCT). Aside from this, the authors declare that they have no commercial or financial relationships that could be construed as a potential conflict of interest.

Use of Artificial Intelligence Statement Artificial intelligence tools were used to assist with English language editing (grammar, clarity, and style). All AI-generated outputs were rigorously reviewed and verified by the authors, who take full responsibility for the accuracy and integrity of the data and content.

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