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A randomized controlled effectiveness trial of training strategies in cognitive processing therapy for posttraumatic stress disorder: Impact on patient outcomes

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ABSTRACT

This randomized controlled hybrid implementation/effectiveness trial aimed to compare the impact of three different models of training and consultation by examining the PTSD treatment outcomes achieved by therapists who were learning a front-line recommended psychotherapy for posttraumatic stress disorder (PTSD), Cognitive Processing Therapy (CPT; Resick, Monson, & Chard, 2017). Therapists ($N = 134$) were randomized into one of three conditions after attending a standard CPT training workshop: No Consultation with delayed feedback on CPT fidelity, Standard Consultation involving discussion and conceptualization of cases without session audio review, and Consultation Including Audio Review, which included a review of segments of audiorecorded CPT sessions. Across all training conditions, the patients treated by these therapists ($N = 188$) evidenced statistically significant reductions in PTSD symptoms, ($d = -0.95$ to -1.78), comorbid symptoms and functioning ($d = -0.27$ to -0.51). However, patients of therapists in the Standard Consultation condition (ΔPTSD = -19.64, $d = -1.78$) experienced significantly greater improvement than those in the No Consultation condition (ΔPTSD = - 10.54, $d = -0.95$, ΔDEV = 6.30, ΔParms = 2, $p = .043$). This study demonstrates that patients who receive evidence-based psychotherapy for PTSD in routine care settings can experience significant symptom improvement. Our findings also suggest that to maximize patient benefit, therapist training should include consultation, but that audio review of sessions during consultation may not be necessary, at least for structured protocols. Implications for implementation, including the reduction of burden and cost of post-workshop support, are discussed.

1. Introduction

Posttraumatic stress disorder (PTSD) is one of the most common and debilitating mental health conditions, with substantial morbidity and mortality (Kessler, 2000). Trauma-focused psychotherapies are front-line recommended treatments, with meta-analyses showing larger effects for psychotherapies compared with pharmacotherapies in controlled studies (Watts et al., 2013). However, concerns have been expressed about the potential for decreased effectiveness (Chambers, Glasgow, & Stange, 2013), or even PTSD symptom exacerbation, when trauma-focused treatments are implemented in non-research settings (e.g., van Minnen, Hendriks, & Olff, 2010). To increase treatment

Abbreviations: CPT, Cognitive Processing Therapy; IS, Implementation Strategy; CQI, Continuous Quality Improvement; VA, Veterans Affairs; OSI, Operational Stress Injury clinic; MHS, Mental Health System; EBP, Evidence Based Psychotherapy; US, United States; PTSD, Posttraumatic Stress Disorder; DOD, Department of Defense; PE, Prolonged Exposure; VAC, Veteran Affiliated Clinic; TX, Texas; PCL, PTSD Checklist; OQ-45, Outcomes Questionnaire-45

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effectiveness and access, it is essential to understand how best to prepare providers to deliver these treatments effectively in clinical settings. Thus, this study tested the effects of three different post-workshop learning conditions on patient treatment outcomes.

Previous research indicates that the use of manuals, web-based trainings, or workshops alone are insufficient to achieve the level of treatment fidelity (i.e., adherence to, and competence in delivering, essential elements) found in randomized control trials (RCTs; Herschell, Kolko, Baumann, & Davis, 2010). Systematic reviews have concluded that, to achieve acceptable fidelity in clinical settings, some form of follow-up consultation or support is necessary (Herschell et al., 2010). The emphasis on feedback in supervision and consultation models (Edmunds, Beidas, & Kendall, 2013; James, Milne, Marie-Blackburn, & Armstrong, 2007), combined with findings that clinicians are not always able to accurately assess their level of skill or adherence to a psychotherapy protocol (Brosan, Reynolds, & Moore, 2008; Tracey, Wampold, Lichtenberg, & Goodyear, 2014), suggest that observation and feedback may be necessary when training clinicians. Some previous comparisons of training strategies support this possibility, with therapists who received consultation that included feedback on session recordings demonstrating higher levels of competence and other key process variables (e.g., eliciting change talk) at the end of their training (Miller, Yahne, Moyers, Martinez, & Pirritano, 2004; Sholomskas et al., 2005). In fact, researchers have considered observation of full sessions and feedback to be a "gold standard" for psychotherapy training (Lewis, Scott, & Hendricks, 2014), and an essential element of training and monitoring psychotherapists in clinical trials (Perepletchikova, Treat, & Kazdin, 2007).

Although the larger literature on consultation and training suggests the importance of observation and feedback, it is unclear whether this intensive and costly level of review is necessary to achieve good outcomes. To date, studies have typically compared workshops or web-based trainings alone to workshops or web-based trainings with consultation (Beidas, Edmunds, Marcus, & Kendall, 2012; Miller et al., 2004; Ruzek et al., 2014; Sholomskas et al., 2005). Only one study, a study of Motivational Interviewing, included a condition that provided any form of observation and feedback based on session recordings (Miller et al., 2004). However, feedback was written, limited to two clinician-selected 20-min recordings, and fewer than half of the participants provided recordings in the study.

A critical barrier to the widespread use of individual feedback on therapy provision is its feasibility (Rakovshik & McManus, 2010). Direct observation or audio review of full therapy sessions, which is the standard for clinical trials and training in academic contexts, is impractical in lower-resourced, routine care settings (Ruzek & Rosen, 2009). In addition, it is possible that full session review may not be necessary to produce good outcomes. In a recent non-randomized comparison of individual consultation with full session review context of group consultation that included review of segments of sessions, participants in group consultation were able to achieve a level of competence that was non-inferior to those for whom full session review was provided (Stirman et al., 2017). A potential advantage to this model is that providers are exposed to a broader sample of case material and peer examples than they would be in individual consultation, with more specific and accurate feedback than they could receive in a form of consultation that does not include any form of observation.

Meanwhile, research on training in evidence-based psychotherapies (EBPs) has largely focused on outcomes at the therapist level (e.g., fidelity to the treatment protocol) rather than on patient-level outcomes (Carpenter et al., 2012; Creed, 2016; Godley, Garner, Smith, Meyers, & Godley, 2011; Herschell et al., 2010; Rakovshik, McManus, Vazquez-Montes, Muse, & Ougrin, 2016), likely due to the logistical challenges associated with collecting patient-level data in studies of this nature. For example, Sholomskas et al. (2005), and Miller et al. (2004), conducted RCTs to compare different training strategies for substance abuse interventions and examined fidelity, as measured by independent ratings of competence in structured role plays with trained actors. Notably, recent research has indicated that fidelity measured via role play does not correspond with therapist competence in sessions with their patients (Decker, Carroll, Nich, Canning-Ball, & Martino, 2013). A later study established that Internet-based training plus Internet-based supervision resulted in greater competence (as measured by independent ratings of therapists' psychotherapy session recordings) than delayed training or an Internet-based training with a consultation worksheet (Rakovshik et al., 2016). Although uncontrolled patient-level program evaluation data from implementation programs has been reported (Eftekhari et al., 2013; Ehlers et al., 2013; Karlin et al., 2012), to our knowledge, there are no RCTs evaluating patient-level outcomes for different training strategies when implemented in routine care settings.

In light of numerous state and national-level policy initiatives to train therapists to deliver EBPs (Clark, 2011; Eftekhari et al., 2013; McHugh & Barlow, 2010), guidance on scalable strategies that yield optimal patient-level outcomes is overdue. Thus, this RCT employed a hybrid type III implementation-effectiveness design (Curran, Bauer, Mittman, Pyne, & Stetler, 2012) to compare the effects of three different approaches to training clinicians to deliver Cognitive Processing Therapy (CPT; Resick, Monson, & Chard, 2017) for patients with PTSD in routine clinical practice. In this trial, a national sample of therapists were randomized to receive one of three post-workshop support strategies: 1) standard expert-led group consultation without review of session audio (Standard Consultation); 2) expert-led group consultation including review of session audio (Consultation Including Audio Review); or 3) No Consultation. We hypothesized that reviewing segments of audiorecorded therapy sessions in the context of group consultation would yield better patient outcomes than consultation without audio review or no consultation.

2. Method

2.1. Procedures

2.1.1. Recruitment and enrollment

Mental health therapists from Veterans Affairs Canada Operational Stress Injury Clinics, Canadian Forces mental health services, and the broader Canadian community were eligible to participate in the study if they: attended a standardized CPT workshop provided by the first author; were licensed mental health therapists with psychotherapy in their scope of practice; were currently providing psychotherapy to individuals who were therapist-assessed to have PTSD; consented to be randomized to one of the study conditions; and were willing to provide audiorecordings of therapy sessions, and measures of PTSD symptoms and psychosocial functioning from consenting patients. Therapists were invited to participate prior to, or at, the standardized workshops teaching the CPT protocol (Resick, Monson, & Chard, 2017). After completing the workshop, therapists who provided informed consent were randomized to one of the three post-workshop consultation conditions. Full participation in the study required therapists to provide recordings of all CPT sessions with at least two patients for use in consultation or for fidelity ratings, and to collect patient outcome measures. To incentivize participation, therapists who uploaded the required number of session recordings and patient symptom measures were eligible to become "Quality-rated" CPT Providers and placed on a public roster of CPT providers if they met a minimum threshold for competence (www.cptforptsd.com). After the 6-month post-workshop phase was completed and all data were collected, all therapists (including those assigned to No Consultation) received written feedback on their fidelity to CPT.

Consistent with other studies on EBP training (Miller et al., 2004;

Eftakhari et al., 2013), participating therapists facilitated recruitment by identifying eligible patients, who provided informed consent for all study procedures before beginning CPT. To participate, patients had to be diagnosed with PTSD by their therapist according to the Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition (DSM-IV; American Psychiatric Association, 2000) criteria, and have a score of ≥ 50 on the Posttraumatic Stress Disorder Checklist - Fourth Edition (PCL-IV; Weathers, Litz, Herman, Huska, & Keane, 1993). Patients also consented to participate in CPT, and have their sessions audiorecorded and reviewed by independent fidelity raters, as well as potentially other therapists during consultation. Consistent with the evidence base for CPT, patients were ineligible if they had current uncontrolled psychosis or bipolar disorder, substance dependence (abuse was permitted), imminent suicidality or homicidality, or significant cognitive impairment (mild to moderate traumatic brain injury was permitted).

All participants provided voluntary informed consent after a clear description of the Research Ethics Board (REB)-approved study procedures. Approval for this study was obtained from the parent REB at Ryerson University. Eleven REBs from sites across Canada with which therapists were affiliated also provided approval for the study.

2.1.2. Post-workshop support strategies

Therapists in all conditions attended a standard 2-day CPT workshop provided by the first author, received the CPT manual and related materials, and had access to resources available through the free CPT-web online training (https://cpt.musc.edu/index). Participants in the two consultation conditions received 6 months of weekly 1-h group consultation with a CPT expert via a web-based program that allowed therapist participants to access consultation meetings through the Internet or to use their phones to dial in. Both consultation conditions included 4–6 therapists per meeting. Consultation meetings included discussion about provision of the CPT protocol, challenging cases, treatment obstacles, and specific issues raised by participants within each group. In the Consultation Including Audio Review condition, one or two therapists per meeting presented segments of their audio recordings (typically 5–10 min) from a recent session and received feedback from other group members, as well as from the expert consultants. Participants were asked to play segments that reflected their use of a specific CPT intervention from that session or to play a segment in which they struggled to deliver CPT. In later sessions, consultants were encouraged to ask therapists to play random segments of their session if they did not identify specific parts that were challenging for them. Therapists who did not present session content in a given week had the opportunity to provide a brief check-in about the progress of their current cases, and received input and feedback. All therapists who submitted audio, including those in the No Consultation condition, received feedback on their fidelity to two sessions after they had completed the six-month post-workshop phase and completed all required study measures.

Experienced CPT consultants, originally trained to provide CPT training and consultation by the treatment developers, led consultation meetings across both consultation conditions to control for any consultant-related effects. They were trained in study consultation procedures for each condition by study team members prior to beginning consultation meetings. The prescribed and proscribed activities for each consultation condition are described in manuals (Chard, 2009; Stirman & Monson, 2011). Study investigators reviewed self-reported consultation checklists, and also reviewed recordings of consultant meetings on a monthly basis to ensure that consultant fidelity to the consultation condition was maintained. Consultants received oversight and feedback on their fidelity to the consultation condition from the principal investigators every 4–6 weeks, and as needed throughout the study. Study team members reviewed 15% of the consultation session recordings and rated the adherence checklists (Consultant-Observer agreement $\kappa = 0.94$, study team observers' agreement $\kappa = 0.74$). Adherence to the procedures specified for the consultation conditions was high and differentiation between conditions was possible: No audio was played in the Standard Consultation meetings, only 5% of the Consultation Including Audio Review sessions did not include audio review due to technical difficulties.

2.2. Measures

2.2.1. Therapist demographic characteristics and experience

A pre-workshop questionnaire was administered to assess relevant therapist demographic information (i.e., age, sex, education), therapist experience [i.e., years of licensed practice, hours of formal training in cognitive-behavioral therapy (CBT), hours of supervised post-graduate CBT training, prior CPT training experience (workshop hours and supervision hours), experience treating patients with PTSD (number of patients), and caseload size].

2.2.2. Fidelity ratings from audio-recorded CPT sessions

To assess the therapists' fidelity to the CPT protocol, audiorecorded therapy sessions were randomly selected at four timepoints across the 6-month consultation period for each clinician. Trained independent raters evaluated audio recordings with a modified version of the CPT fidelity measure that has been used in previous clinical trials (Resick et al., 2008). The CPT fidelity measure examines therapists' adherence to specific CPT interventions (4-point Likert-type scale, with 0 = incomplete, 1 = slightly complete, 2 = mostly complete, and 3 = fully complete) as prescribed in each session, and their competence or skill in delivering them (7-point Likert-type scale, from 0 = not competent to 6 = outstanding competence). A mean score of all unique and essential items per session was calculated to determine adherence and competence scores. Two studies have assessed the reliability of this measure and found 97% agreement between two raters across all items for adherence in one study (agreement was not reported for competence scores; Resick, Nishith, Weaver, Astin, & Feuer, 2002), and 100% agreement for adherence and competence in another (Resick et al., 2008).

2.2.3. Patient characteristics

Patient information, including gender, age, race, ethnicity, diagnostic information, military status, and education level, were reported by clinicians upon the patient's enrollment in the study.

2.2.4. Patient outcome measures

Posttraumatic Stress Disorder Checklist (PCL-IV Weathers et al., 1993) is a well-validated, 17-item, self-report questionnaire of the severity of distress related to PTSD symptoms. The items are consistent with the DSM-IV (American Psychiatric Association, 2000) criteria for PTSD, and are measured on a 5-point Likert scale. A cut-score of ≥50 indicates likely PTSD (Weathers et al., 1993). The PCL was completed at pre-treatment and prior to every CPT session. The internal consistency of the PCL was high in the current study (Cronbach's alpha = 0.94).

The Outcomes Questionnaire-45 (OQ-45) is a 45-item, self-report measure of functioning, symptoms, interpersonal problems, social role functioning, and quality of life. A total cut-off score of 63 has been found to correspond to difficulties functioning (Lambert et al., 1996a). The OQ-45 has demonstrated strong psychometric properties (Lambert et al., 1996b) and was administered at baseline, mid-treatment, and post-treatment.

2.3. Statistical analyses

2.3.1. Power calculation

To determine an adequate sample size that accounted for 30%

assessment attrition, we computed a design effect for a repeated-measures analysis based on the within-patient and within-therapist correlation for the sample. Based on a two-sided test, the required sample size for at least 80% power to detect at least a medium effect [0.50 or above, per Cohen's classifications (Cohen, 1992)] between groups was a sample size of 59 patients per condition, or a total sample size of 176.

2.3.2. Analyses of patient engagement and outcomes

We examined treatment engagement by computing the mean number of sessions attended, as well as the average number of days between baseline and sessions attended. We conducted one-way ANOVAs to assess for differences in engagement between conditions. To evaluate patient-level outcomes, and by condition, multilevel growth curve modeling (full maximum likelihood estimation) was conducted using the Linear and Nonlinear Modeling software program (HLM7; Raudenbush, Bryk, Cheong, Congdon, & Du Toit, 2011). To test competing models, we adopted a model building approach using the change in the −2 log-likelihood value (often referred to as change in deviance, ΔDEV). This approach follows a chi-squared distribution with *df* equal to the difference in the number of parameters (ΔParms) across models. A preliminary variance decomposition revealed that the vast majority of the variance in PTSD symptoms (96.9%) was due to variance at Level-1 (within patients, 37.2%) and Level-2 (between patients, 59.7%) with only 3.1% of the variance due to variance at Level-3 (within therapists, 0.89%) and Level-4 (within clinics, 2.24%). Because most of the variance in PTSD was at Levels 1 and 2, and 4-level models are cumbersome and can lead to convergence problems, we adopted a 3-Level model with repeated PTSD assessments (Level-1) nested within patients (Level-2) nested within therapists (Level-3).

As shown in Table 3, which includes descriptive statistics for the number of days since baseline assessment for treatment sessions one through twelve, there was considerable variability in the timing of treatment sessions across patients (the number of days between treatment and number of days from session 1 to session 12). Therefore, time was modeled as the number of days since baseline assessment, rather than as session number or week, in the multilevel regression growth curves. We initially evaluated several alternative 3-Level unconditional change models. The best fit for the data was a quadratic change model (with time modeled by including number of days since baseline and this variable squared) specifying change as a fixed effect at Level 3 but a random effect at Level 2, which was significantly better than a linear model ($\Delta DEV = 117.75$, $\Delta Parms = 7$, $p < .001$).

Effect sizes (*d*) for change in outcomes from baseline to session 12 and effect sizes for differences in change between the consultation conditions was computed by the procedures described by Feingold (2009), which produces effect size estimates from growth curve analyses that are comparable to those derived from more traditional repeated measures designs (e.g., repeated measures ANOVA).

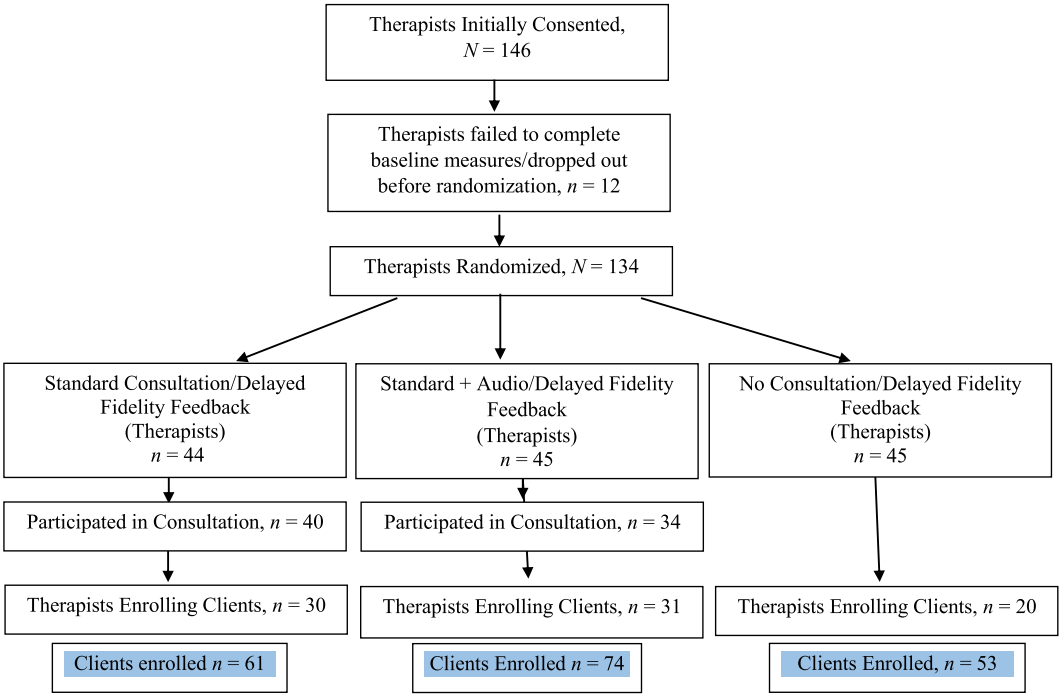
3. Results

3.1. Sample characteristics

Fig. 1 contains a CONSORT chart detailing therapist and patient enrollment in the study. Therapist demographic and background characteristics are found in Table 1. Patient demographic and baseline clinical characteristics are presented in Table 2. There were no significant differences in therapist or patient variables by study condition.

3.2. CPT protocol fidelity

Intra-class correlations using random effects were calculated to assess inter-rater reliability on the fidelity scales, with 19% of the total number of session recordings ($N = 1531$) coded. Of these, 25% were coded by all raters to assess agreement. Results indicated very high rater agreement on the CPT adherence and competence scale, with



Note. One therapist in Standard including Audio condition turned in recordings but no patient outcome data.

Fig. 1. CONSORT diagram.

ICC = 0.87 for adherence, and ICC = 0.79 for competence. Adherence to the protocol was reasonably high across all sessions that were rated (M = 2.26; SD = 0.49), indicating that on average, essential session elements were mostly completed. Competence was lower than that found in clinical trials (M = 2.82, SD = 0.80). No between-condition differences emerged in aggregated fidelity ratings (Adherence $F(2, 295) = 0.812$, $p = .445$; Competence $F(2,295) = 0.793$, $p = .453$).

3.3. Treatment engagement

As shown in Table 3, the average number of sessions attended was 8.37 (SD = 4.32), 8.85 (SD = 4.31), and 9.49 (SD = 3.87) for the No Consultation, Consultation Including Audio Review, and Standard Consultation conditions, respectively. A one-way ANOVA indicated that the three consultation conditions did not significantly differ on average number of sessions attended, $F(2, 185) = 1.03$, $p = .359$. One-way ANOVAs also indicated no consultation condition differences in the average days from baseline for all sessions attended, $F(2, 185) = 1.03$, $p = .359$, and the average days from baseline for the last session attended, $F(2, 185) = 1.03$, $p = .359$.

3.4. Treatment outcomes

Aone-way ANOVA indicated that average PCL score at pre-treatment did not differ across groups, $F(2, 146) = 0.13$, $p = .882$. In the unconditional growth curve analyses without condition included in the model, the regression intercept indicated that the average pre-treatment patient-rated PCL was 61.23, which is in the severe range (Weathers et al., 1993). Average reduction in PCL scores from pre-to post-treatment was 14.28 points, a large pre-to post-treatment effect ($d = -1.29$; Cohen, 1977).

When consultation condition was added to the model, a significant time × condition interaction emerged (see Table 4 and Fig. 1). Mean post-treatment PCL scores were 49.06 (SD = 21.05) for the No Consultation group, 49.45 (SD = 18.45) for the Consultation Including Audio Review group, and 42.33 (SD = 13.37) for the Standard Consultation group. The modeled decreases in PCL scores for the three conditions across treatment (and related within-treatment effect sizes) for each condition were: No Consultation (−10.54 points, $d = -0.95$), Consultation Including Audio Review (−12.09 points; $d = -1.09$), and Standard Consultation (−19.74; $d = -1.78$). Pairwise comparisons of the consultation conditions revealed a significant difference between the No Consultation and Standard Consultation conditions (ΔDEV = 6.30, ΔParms = 2, $p = .043$). Patients' scores on the PCL decreased 9.20 points more in the Standard Consultation condition compared with the No Consultation condition, representing a $d = 0.83$ effect size difference. The comparison between the Consultation Including Audio Review and the Standard Consultation conditions did

Table 1
Therapist demographic and background characteristics (n = 81).

Variable	Total (n = 80)	No Consultation (n = 20)	Consultation Including Audio Review (n = 30)	Standard (n = 30)	Difference (*ANOVA or $\chi 2$*)
Age (M, SD)	47.63, 9.73	46.42, 10.38	47.21, 10.19	48.86, 10.19	$F(2,79) = .067$, $p = .935$
Gender					$\chi 2(2, n = 81) = .554$, $p = .758$
Male	26%	26%	30%	21%	
Female	74%	74%	70%	79%	
Education					$\chi 2(10, n = 81) = 14.67$, $p = .145$
PhD/PsyD	41%	65%	28%	35%	
Master's (counseling, social work)	41%	30%	56%	35%	
Bachelor's	7%	0%	7%	11%	
MD	5%	4%	7%	5%	
Years of Practice					$\chi 2(6, n = 81) = 6.82$, $p = .338$
5 or fewer	11%	22%	7%	7%	
6–10	21%	17%	17%	29%	
11–20	38%	35%	50%	29%	
20 or more	28%	26%	27%	36%	
CBT experience					$\chi 2 (6, n = 81) = 3.307$, $p = .769$
No prior training or experience	1%	0%	0%	4%	
Basic knowledge, no formal training	7%	4%	7%	11%	
Coursework or training, no supervision	38%	35%	43%	36%	
Coursework or training with supervision	53%	61%	50%	50%	
Caseload Size (M, SD)	26.10, 15.68	27.68, 18.19	22.00, 13.65	28.96,15.24	$F(2, n = 66) = 1.33$, $p = .272$
PTSD Caseload (% of patients)					$F(8, n = 81) = 1.49$, $p = .993$
< 25%	53%	53%	47%	57%	
25–50%	25%	22%	30%	21%	
50–75%	6%	4%	7%	7%	
> 75%	16%	17%	17%	14%	
Practice Setting					$F(10, n = 81) = 5.92$, $p = .822$
Provincial Health Agency	26%	30%	27%	21%	
Private Practice	30%	30%	30%	29%	
Operational Stress Injury Clinic	20%	22%	20%	18%	
Canadian Forces or other Federal Health Service	17%	13%	18%	17%	
Other	7%	4%	3%	14%	

Note. Sample characteristics are provided for the therapists who provided client data. Means (M) and standard deviations (SD) reported for continuous variables. F-values and corresponding p-values of one-way analysis of variance (ANOVA) to test group differences were also reported for all continuous variables. Sample sizes (n for total, I per condition; and n for a portion of the sample) and corresponding percentages (%) are reported for categorical variables. χ^2-values and corresponding p-values of Chi-squared tests of group differences were also reported for categorical variables.

Table 2
Patient demographics and baseline clinical characteristics (n = 188).

Variable	Total (n = 188)	No Consultation (n = 53)	Consultation Including Audio Review (n = 74)	Standard (n = 61)		
Category	M (SD)	M (SD)	M (SD)	M (SD)	ANOVA	p
	n (%)	n (%)	n (%)	n ($)	$\chi 2$	p
Gender					$\chi 2(2, n = 175) = 5.65$	.059
Male	84 (48.0%)	17 (34.0%)	37 (55.2%)	30 (51.7%)		
Female	91 (52.0%)	33 (66.0%)	30 (44.8%)	28 (48.3%)		
Race/Ethnicity					$\chi 2(10, n = 175) = 14.59$	.148
White	154 (88.0%)	40 (80.0%)	62 (92.5%)	52 (89.7%)		
Black	3 (1.7%)	1 (2.0%)	2 (3.0%)	0 (0.0%)		
Hispanic	1 (0.6%)	0 (0.0%)	0 (0.0%)	1 (1.7%)		
Asian/Pacific Islander	5 (2.9%)	3 (1.7%)	1 (1.5%)	1 (1.7%)		
Indigenous	7 (4.0%)	5 (10.0%)	1 (1.5%)	1 (1.7%)		
Other	5 (2.9%)	1 (2.0%)	1 (1.5%)	3 (5.2%)		
Relationship Status					$\chi 2(2, n = 173) = 2.33$	.313
Committed Relationship	102 (59.0%)	26 (52.0%)	44 (65.7%)	32 (57.1%)		
Single/Widowed/Divorced	71 (41.0%)	24 (48.0%)	23 (34.3%)	24 (42.9%)		
Military Status					$\chi 2(2, n = 173) = 6.61$	.158
Veteran	40 (23.3%)	15 (30.0%)	12 (18.5%)	13 (22.8%)		
Active Duty	33 (19.2%)	5 (10.0%)	18 (27.7%)	10 (17.5%)		
Non-Military	99 (57.6%)	30 (60.0%)	35 (53.8%)	34 (59.6%)		
Age (Years)	39.39 (11.27)	37.31 (9.64)	41.94 (11.85)	38.00 (11.27)	$F(2, 165) = 2.97$	.054
Education (Years)	12.84 (2.58)	12.61 (3.31)	13.28 (2.33)	12.52 (2.03)	$F(2, 165) = 1.44$	.211
Pre-Treatment PTSD (PCL)	61.15 (11.07)	60.58 (11.07)	61.68 (10.89)	61.04 (11.49)	$F(2, 146) = 0.13$	.882
Pre-Treatment OQ-Total	96.80 (26.90)	98.11 (21.70)	100.54 (28.93)	90.66 (28.96)	$F(2, 131) = 1.51$	.208
Pre-Treatment OQ-SR	15.80 (6.29)	14.83 (5.70)	17.30 (6.17)	15.02 (6.83)	$F(2, 135) = 2.35$	.100
Pre-Treatment OQ-SD	59.74 (16.30)	61.12 (13.69)	61.21(17.87)	56.56 (16.69)	$F(2, 139) = 1.22$	.300
Pre-Treatment OQ-IR	21.45 (7.58)	22.48 (6.78)	22.15 (7.09)	19.49 (8.71)	$F(2, 137) = 2.08$	.129
Number of Sessions	9.13 (4.21)	8.62 (4.20)	9.09 (4.40)	9.61 (4.01)	$F(2, 185) = 0.78$	.462
Number of Comorbid Axis-I Diagnoses	1.16 (0.97)	1.13 (0.70)	1.23 (1.11)	1.11 (1.01)	$F(2, 151) = 0.25$	.767
Axis-II Comorbidity					$\chi 2(2, n = 154) = 1.10$	.578
Absent	119 (77.3%)	36 (75.0%)	49 (81.7%)	34 (73.9%)		
Present	35 (22.7%)	12 (25.0%)	11 (18.3%)	12 (26.1%)		

Note. Means (*M*) and standard deviations (*SD*) reported for continuous variables. F-values and corresponding *p*-values of one-way analysis of variance (ANOVA) to test group differences were also reported for all continuous variables. Sample sizes (*n* for total, I per condition) and corresponding percentages (%) are reported for categorical variables. χ^2-values and corresponding *p*-values of Chi-squared tests of group differences were also reported for categorical variables.

not reach conventional levels of statistical significance (ΔDEV = 5.45, ΔParms = 2, $p = .066$). However, there was a 7.65-point greater decrease in the PCL for the Standard Consultation condition relative to the Consultation Including Audio Review condition, representing a $d = 0.69$ effect size difference between these conditions, a clinically significant difference (Monson et al., 2008). The difference between the Consultation Including Audio Review and the No Consultation condition was not statistically significant (ΔDEV = 0.26, ΔParms = 2, $p = .876$), and the effect size difference was $d = 0.14$ (See Fig. 2).

We evaluated similar growth models with the OQ-45 total and subscale scores (Table 4). There were significant improvements in these measures (all ΔDEVs > 287.45, all ΔParms = 7, all $ps < .001$), with

Table 3
Descriptive Statistics for Number of Days Since Pre-Treatment Assessment and PCL, and OQ scores.

Session	*n*	Number of Days Since Baseline		PCL		OQ-Total		OQ-SR		OQ-SD		OQ-IR	
		M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
0	149	0.00	0.00	61.15	11.07	96.80	26.90	15.80	6.29	59.74	16.30	21.45	7.58
1	164	13.66	15.94	59.63	11.84	96.42	26.58	15.86	6.08	59.09	15.94	21.87	7.61
2	168	22.68	17.75	57.58	13.21	93.99	26.75	15.37	6.12	57.65	16.60	21.38	7.53
3	154	35.31	37.30	57.24	12.62	95.09	25.81	15.37	5.79	58.41	16.51	21.73	7.16
4	141	45.92	42.72	56.94	13.84	92.51	28.83	14.94	6.39	57.48	18.40	20.73	7.84
5	127	54.78	42.87	55.56	14.76	90.06	28.50	14.37	6.19	56.29	18.45	20.94	7.29
6	127	64.56	45.48	53.17	15.67	88.06	28.62	14.58	5.97	54.99	18.35	20.19	7.86
7	121	77.13	49.71	50.94	14.74	87.51	27.03	14.58	5.74	54.06	17.88	20.49	7.54
8	120	86.50	51.05	50.15	15.92	85.53	28.39	14.02	5.97	52.35	18.39	19.77	7.60
9	116	95.97	53.16	48.97	16.31	83.99	29.63	13.89	6.07	51.89	19.56	19.91	7.76
10	113	109.7	59.58	47.59	15.44	82.92	29.42	13.92	5.62	51.15	19.34	19.15	7.66
11	108	118.2	61.19	47.28	15.72	79.41	28.91	12.97	5.25	49.04	19.47	18.56	7.73
12	108	122.5	48.32	44.31	16.26	77.84	31.17	12.32	5.55	47.40	20.80	18.41	7.95

Note. PCL = Posttraumatic Checklist; OQ = Outcome Questionnaire; SR = Social Roles; SD = Symptom Distress; IR = Interpersonal Relationships; *M* = Mean; *SD* = Standard Deviation.

Table 4
Growth curve estimates for each post-workshop support condition and baseline to session 12 changes for patient outcomes.

Parameter	Condition	b	95% CI	p
Posttraumatic Stress Disorder Checklist				
Intercept/Baseline				
	No Consultation	59.61	(56.25, 62.97)	< .001
	Standard Consultation	61.76	(59.45, 64.06)	< .001
	Consultation Including Audio Review	62.06	(58.40, 65.72)	< .001
Linear Time (Days)				
	No Consultation	−0.12	(−0.23, −0.01)	.038
	Standard Consultation	−0.12	(−0.20, −0.05)	.002
	Consultation Including Audio Review	−0.22	(−0.30, −0.13)	< .001
Quadratic Time (Days Squared)				
	No Consultation	0.0003	(−0.0004, 0.0009)	.391
	Standard Consultation	0.0002	(−0.0003, 0.0006)	.394
	Consultation Including Audio Review	0.0004	(−0.0003, 0.0012)	.221

Baseline - S12 Change		Est	*d*
	No Consultation	−10.63	−0.96
	Standard Consultation	−19.62	−1.77
	Consultation Including Audio Review	−12.31	−1.11

Outcomes Questionnaire-Total				
Intercept/Baseline		90.21	(82.28, 98.15)	< .001
	No Consultation	91.9	(83.25, 100.56)	< .001
	Standard Consultation	85.37	(78.71, 92.03)	< .001
	Consultation Including Audio Review			
Linear Time (Days)		−0.09	(−0.18, 0)	.053
	No Consultation	−0.12	(−0.17, −0.07)	< .001
	Standard Consultation	−0.1	(−0.15, −0.05)	< .001
	Consultation Including Audio Review			
Quadratic Time (Days Squared)		0.00	(0.00, 0.00)	.532
	No Consultation	0.00	(0.00, 0.00)	.520
	Standard Consultation	0.00	(0.00, 0.00)	.258
	Consultation Including Audio Review			

Baseline - S12 Change		Est	*d*
	No Consultation	−10.92	−0.41
	Standard Consultation	−12.45	−0.46
	Consultation Including Audio Review	−15.07	−0.56

Table 4 (*continued*)

Parameter	Condition	b	95% CI	p
OQ-Symptom Distress				
Intercept/Baseline				
	No Consultation	55.90	(50.72, 61.08)	< .001
	Standard Consultation	55.48	(50.32, 60.65)	< .001
	Consultation Including Audio Review	52.70	(48.41, 57)	< .001
Linear Time (Days)				
	No Consultation	−0.06	(−0.11, 0)	.058
	Standard Consultation	−0.08	(−0.11, −0.05)	< .001
	Consultation Including Audio Review	−0.08	(−0.11, −0.04)	< .001
Quadratic Time (Days Squared)				
	No Consultation	0.00	(0.00, 0.00)	.753
	Standard Consultation	0.00	(0.00, 0.00)	.576
	Consultation Including Audio Review	0.00	(0.00, 0.00)	.321

Baseline - S12 Change		Est	*d*
	No Consultation	−6.75	−0.41
	Standard Consultation	−9.11	−0.56
	Consultation Including Audio Review	−9.22	−0.57

OQ-Interpersonal Relationships				
Intercept/Baseline		20.58	(18.54, 22.63)	< .001
	No Consultation	21.59	(19.33, 23.84)	< .001
	Standard Consultation	19.12	(17.09, 21.14)	< .001
	Consultation Including Audio Review			
Linear Time (Days)				
	No Consultation	−0.02	(−0.04, 0)	.092
	Standard Consultation	−0.02	(−0.03, −0.01)	< .001
	Consultation Including Audio Review	−0.01	(−0.03, 0)	.074
Quadratic Time (Days Squared)				
	No Consultation	0.00	(0.00, 0.00)	.405
	Standard Consultation	0.00	(0.00, 0.00)	.983
	Consultation Including Audio Review	0.00	(0.00, 0.00)	.056

Baseline - S12 Change		Est	*d*
	No Consultation	−1.80	−0.29
	Standard Consultation	−2.64	−0.42
	Consultation Including Audio Review	−2.36	−0.38

Note. *b* = unstandardized regression coefficient; *95% CI* = 95 percent confidence interval; *p* = *p*-value; Est = estimate, *d* = effect size estimate (.20 small, 0.50 medium, 0.80 large; Cohen, 1977), S12 = session 12. None of the individual change parameters differed significantly across groups. The consultation condition x time overall interaction was due to differences in the combined effects of both change parameters (linear, quadratic) as a function of group.

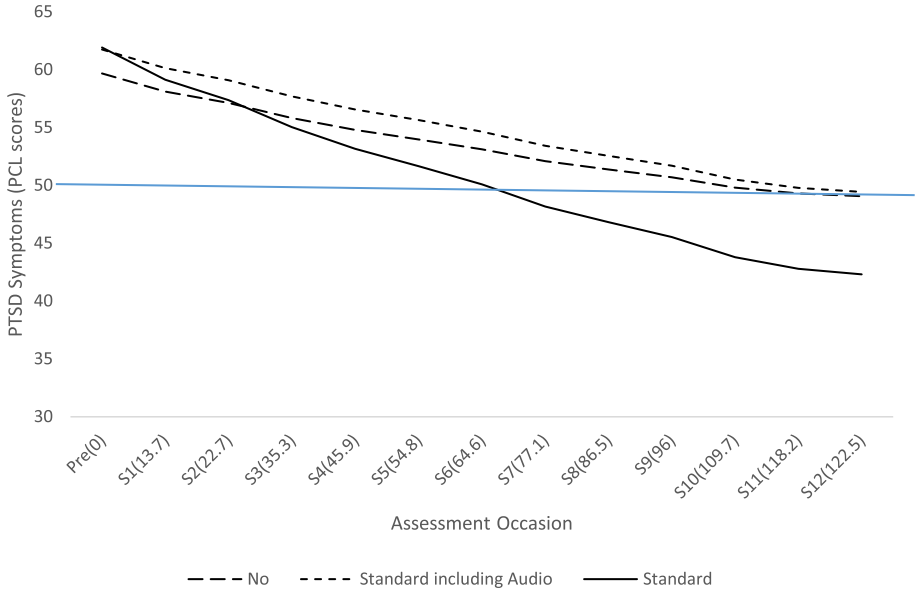


Fig. 2. Change in PTSD. Note: The lines indicate change as a function of consultation condition. Pre-pretreatment PCL score, S1−S12 indicates scores on PCL administered prior to treatment sessions. PTSD = Posttraumatic Stress Disorder, PCL = Posttraumatic Checklist. Horizontal access is labeled with Session Number and average number of days from baseline in parentheses. No=No Consultation condition, delayed fidelity assessment; Standard Including Audio = Consultation Including Audio Review condition, standard consultation including a review of segments of session audio; Standard = Standard Consultation condition. A score of 50 on the PCL indicates probable PTSD diagnosis, the horizontal line indicates this cutoff.

across treatment effect size improvements ranging from $d = -0.27$ to -0.51. There were no significant condition × time interactions (all ΔDEVs < 1.73, all ΔParms = 4, all *ps* $> .786$).

4. Discussion

Although PTSD is one of the most common and debilitating mental health conditions, efficacy trials indicate potent treatment effects for some trauma-focused psychotherapies. Studies investigating methods to effectively train clinicians in these methods are sorely needed. Toward this end, we examined patient outcomes for a sample of clinicians learning to deliver CPT, and conducted the first RCT investigating the relative effectiveness of different post-workshop follow-up strategies on patient outcomes.

Results indicated that, despite being treated by therapists who were minimally experienced in delivering CPT, patients in all conditions experienced significant improvements in their self-reported PTSD symptoms and psychosocial functioning. Patients in the Standard Consultation condition reported improvements in their PTSD symptoms that were on par with effect sizes found in RCTs examining the efficacy of CPT (Watts et al., 2013). Furthermore, in the Standard Consultation condition, patients' average post-treatment self-reported levels of PTSD symptoms were below the cutoff for probable PTSD (Weathers et al., 1993). These findings challenge concerns that CPT (and other EBPs for PTSD) are only efficacious in research settings, or highly controlled clinical contexts, such as when treatments are delivered by the developers or highly trained experts in the therapy.

Contrary to hypothesis, the patients whose therapists received Standard Consultation without audio review experienced the largest symptom reductions, with medium-to-large effect size differences compared with the patients in the other conditions. There are several potential explanations for the unexpected advantage of Standard Consultation. First, playing segments of audiorecorded sessions for some therapists necessarily reduced the amount of time that each therapist who did not play audio could spend discussing his or her own cases during each group consultation meeting. When learning a new treatment, periodic feedback on brief portions of a session may be less important for learning than routine allocation of ample time to discuss each session, next steps in treatment, case conceptualization, and difficulties with other aspects of delivering the protocol. Additionally, technical difficulties associated with playing audiorecordings in web-based consultation, and the time required to identify session segments, may have led to less efficient consultation sessions for the Consultation Including Audio Review group.

It is important to consider the possibility that Standard Consultation is well-suited to CPT, a highly-structured treatment in which specific skills are prescribed for each session. For CPT, consultation that is focused on identifying key beliefs maintaining PTSD, refining therapists' skill in using Socratic dialogue, and learning how to address avoidance and other common challenges in delivering CPT may be most beneficial to clinicians learning the treatment. However, some EBPs afford clinicians greater latitude in selecting which treatment strategies to employ in a given session. Audio review may be a strategy that is comparatively more helpful when teaching clinicians to deliver therapies that require them to make more decisions about which problems to target and which interventions to deliver on a session-by-session basis. It is also possible that consultant review of full sessions may have yielded improved fidelity or better clinical outcomes than were found in this study, although this strategy would likely be impractical in most settings (Ruzek & Rosen, 2009). It is interesting that the No Consultation group achieved patient outcomes that were not different from the Consultation Including Audio Review group; however, the clinicians assigned to No Consultation were still aware that they were submitting audiorecordings for fidelity rating and would be receiving feedback at the end of their training. It is also noteworthy that approximately half of the therapists in our sample had previous training in cognitive behavioral therapies. In light of other studies that have identified differential changes in therapist skill based on therapist characteristics and training strategies (Bearman et al., 2016; Carpenter et al., 2012) further exploration of potential interactions between therapist experience, attitudes or prior training, and consultation condition is warranted, We recommend replication of our finding with other treatments before concluding that audio review is unnecessary for training.

This study makes several contributions to the implementation literature focused on training in EBPs. Although the patient-level outcomes that we examined are not typically considered implementation outcomes (Proctor et al., 2009), they are important “bottom line” outcomes for policymakers, who invest in training and implementation with the assumption that doing so will improve patient-level outcomes (Stirman et al., 2015). At the aggregate level, CPT treatment fidelity did not differ between conditions. It is interesting to note that, while on average therapists' adherence levels were relatively high, competence levels were lower than those found in efficacy trials (e.g., Resick et al., 2008). These lower rates of fidelity make sense, as they are an average of therapists' first attempts to deliver CPT after a training workshop through the end of a 6-month consultation phase. Thus, in contrast to

previous training studies, which often report pre- and post-training consultation scores (Creed et al., 2016; Sholomskas et al., 2004), or clinical trials that require therapists to maintain a minimum level of competency, our competence levels represent an average of competence levels across the training periods. Importantly, Standard Consultation still achieved outcomes in the range of those found in CPT clinical trials. This finding is encouraging, as it suggests that expert-level competence in CPT may not be necessary to achieve good clinical outcomes, and that clinicians in a variety of treatment settings can be trained to deliver effective treatments. However, it is possible that more intensive consultation, and more than two training cases, are necessary for therapists to achieve consistent levels of fidelity that are on par with those found in clinical trials. A thorough examination of the relationship between fidelity and clinical outcomes, and whether patterns of fidelity across the protocol (Boswell et al., 2013) somehow differed in the Standard Condition is beyond the scope of the current paper, given the potentially complex relationship between fidelity and symptom change (Barber, Triffleman, & Marmar, 2007; Hogue et al., 2008; Lorenzo-Luaces, German, & DeRubeis, 2014). However, this is an important topic for future research.

Building on research that suggests the importance of follow-up consultation and support after initial training (Herschell et al., 2010), our findings provide important guidance for those tasked with training and implementation programs. Fewer therapists in the No Consultation condition enrolled patients, suggesting that motivation and ability to identify appropriate patients may have been lower in this group, or that their confidence to deliver CPT without additional support was lower. Furthermore, although patients whose therapists were trained in the No Consultation condition experienced significant improvements in symptoms, therapist participation in expert consultation appeared to enhance patients' outcomes substantially, suggesting that consultation is a strategy that may be protective against degradation of clinical outcomes when EBPs are delivered in routine care settings. The potential for reduced clinical benefits when EBPs are delivered in routine clinical care has been a source of concern, and there have been recent calls to identify ways to minimize or eliminate the "implementation cliff" (Chambers et al., 2013; Weisz, Ng, & Bearman, 2014). Given the majority of our clinician participants were employed in provincial or federal agencies that were implementing EBPs for PTSD, our findings may be more generalizable to implementation programs than previous studies that have included highly motivated participants who sought out training (Miller et al., 2004). Finally, our finding that, at least for a structured manualized treatment, audio review may not be necessary during consultation is highly informative to training programs that require scalable, lower-burden procedures for training therapists. However, although several public mental health systems have developed training programs involving workshops and expert consultation (Creed et al., 2016; Lau et al., 2016; Karlin et al., 2010; Stirman et al., 2010), access to expert consultants, and quality assurance among those consultants, still requires resources that may be out of reach in some mental health settings, which may be one reason that interest in cascade, or train-the-trainer models has grown in recent years (Herschell et al., 2010).

Given the challenges in conducting research on treatment effectiveness outside of controlled clinical settings Stirman, Gutner, Langdon, & Graham, (2016), we note some limitations to this research. First, obtaining structured diagnostic interviews of patients across a wide geographic region was impractical. Therefore, we included patients in the study through a combination of therapist diagnosis and a self-report measure with a cutoff score indicating probable PTSD. We also assessed changes in self-reported symptoms using a validated and widely used measure of PTSD as our primary patient outcome measure. This strategy, along with our strategy of having therapists identify patient participants from their caseloads, rather than the study team independently recruiting patient participants, is consistent with recommendations for hybrid research methodology that focus primarily on evaluating the impact of implementation strategies in routine care (Curran et al., 2012). Changes in psychosocial functioning were more modest in this study, with patients, on average, above the clinical cut-score on the OQ-45 at post-treatment. This finding is consistent with prior efficacy studies and the call for treatment innovations that better improve psychosocial outcomes in addition to symptomatology (Monson et al., 2012). Although we did not collect data on dropout or reasons for early termination among patient participants, the average number of sessions attended was at, or above, the number of sessions determined to be minimally adequate in treatment guidelines (Department of Veterans Affairs, 2003), and should be considered in light of findings that not all patients require 12 sessions to experience substantial improvement (Galovski, Blain, Mott, Elwood, & Houle, 2012; Szafranski, Smith, Gros, & Resick, 2017).

The patient-level outcomes that we examined are important "bottom line" outcomes for policymakers, who invest in training and implementation with the assumption that doing so will improve patient-level outcomes (Stirman et al., 2016). Additionally, by building on research that suggests the importance of follow-up consultation and support after initial training (Herschell et al., 2010) to examine the impact of different forms of observation and feedback, this study provides important guidance for those tasked with training and implementation programs. Future research should examine patterns of dropout and symptom change among patients, along with other key implementation outcomes, such as changes in fidelity, satisfaction, and attitudes toward CPT and other evidence-based treatments. Such information can ultimately enhance our understanding of the strategies that are most likely to improve access to EBPs, and yield the most benefit for patients who receive these treatments in routine clinical practice.

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Conflicts of interest

The authors declare that they have no conflicts of interest.

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