Yoga as an Intervention for Psychological Symptoms Following Trauma: A Systematic Review and Quantitative Synthesis

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Despite evidence of the physiologic impact of trauma, treatments are only beginning to focus on the impact of trauma on the body. Yoga may be a promising treatment for trauma sequelae, given research that supports yoga for general distress. The present study aims to systematically assess and quantitatively synthesize the effectiveness of yoga interventions for psychological symptoms (posttraumatic stress disorder [PTSD], depression, anxiety symptoms) following potentially traumatic life events. The following electronic databases were systematically searched: PsycINFO, Ovid Medline/PubMed, Cumulative Index to Nursing and Allied Health Literature, and Embase/Embase Classic. Google Scholar, Mendeley, Open Research and Contributor Identification, and Fig Share were hand searched post hoc. The review focused on studies with a comparison group that measured psychological symptoms before and after intervention. After screening and reviewing, 12 articles ($N = 791$) were included, with interventions ranging from 2 days to 16 weeks. If a study contained multiple conditions, between-groups differences were only examined between the yoga and inactive control group. Though overall between-groups (yoga vs. comparison) effect sizes ranged from ds = 0.40–1.06, the systematic review and quantitative synthesis did not find strong evidence for the effectiveness of yoga as an intervention for PTSD, depression, and anxiety symptoms following traumatic life experiences due to low quality and high risk of bias of studies. As yoga has promise for managing psychological symptoms among trauma survivors, this review calls for more rigorous design of future studies to allow definitive conclusions regarding the use of yoga in mental health treatment of trauma survivors.

Keywords: yoga, trauma, posttraumatic stress disorder, systematic review, meta-analysis

Trauma impacts many, as the majority of the population experiences some type of traumatic event in their lifetime (e.g., Frazier et al., 2009). Experiencing traumatic events is associated with increased rates of PTSD, depression, and anxiety in certain individuals (e.g., Frazier et al., 2009; Kilpatrick et al., 2003). These psychological disorders may be debilitating and functionally impair those with the disorders. There is growing evidence of the effectiveness of psychological treatments for PTSD (Cusack et al., 2016), but even among effective treatments, significant percentages of treated patients remain with debilitating symptom levels (Schottenbauer, Glass, Arnkoff, Tendick, & Gray, 2008; Steenkamp, Litz, Hoge, & Marmar, 2015). Treatment ineffectiveness might stem, in part, from the fact that none of these treatments focus on the impact of trauma on the body despite evidence of physiologic impact (e.g., Afari et al., 2014; van der Kolk, 1994;

van der Kolk, 2006). Yoga, which draws on connections between the body and mind, may thus be a promising intervention for the aftermath of trauma.

Yoga has been a traditional practice of uniting mind, body, and spirit through various techniques, including physical postures, breath work, meditation, and chanting (for a review, see, e.g., Cramer, Lauche, Langhorst, & Dobos, 2013). Physical postures, or asanas, refer to the various body positions in yogic exercise that practitioners may maintain for a certain number of breaths. These physical postures may also be linked with breath work, in which practitioners move from posture to posture on an inhalation or exhalation. Breath work involves various techniques for practitioners to notice or manipulate their breath, for example, decreasing or increasing the rate or rhythm of respiration. Meditation involves a reflection or observation of thoughts, which may be accompanied by verbal chants or with the physical postures as part of a moving meditation. Chanting may also be practiced without meditation, often to open and close a practice. Recently, the practice of yoga has been adapted to the specific needs of trauma survivors (Emerson, Sharma, Chaudhry, & Turner, 2009), making it a feasible therapeutic option for this particular population.

Yoga may be a promising treatment for trauma sequelae, given research that supports yoga for general distress, specifically in decreasing physical symptoms and emotional distress and increasing quality of life (for a review, see, e.g., Emerson et al., 2009).

Because the experience of trauma is physically impactful both during (e.g., in the midst of domestic violence, sexual abuse, combat, natural disasters) and after (e.g., alterations in physiological stress responses) the trauma, mind and body connections may be particularly healing. A systematic review demonstrated that those who have experienced a trauma were 2.7 times more likely to have a functional somatic syndrome (e.g., chronic pain, temporomandiular disorder, chronic fatigue syndrome) than those who did not report experiencing a trauma (Afari et al., 2014); these results are similar in studies examining specific types of trauma, for example, sexual abuse (Finestone et al., 2000). Thus, as Afari et al. (2014) express, effects of the experience of trauma that alter one's cognitive and behavioral responses may also result in the expression of somatic changes. From evidence suggesting how traumatic stress has lasting impacts on the body, van der Kolk (2006) suggests that the most effective treatments involve (a) increasing one's tolerance of the physical sensations in one's body, (b) regulating arousal, and (c) learning effective actions in the body, which are particularly important after the experience of peritraumatic physical helplessness. Interoceptive, body-oriented therapies, which allow for more physical and mental self-awareness and mindfulness, may be promising interventions for trauma survivors. Yet, the current state of evidence-based treatments for posttraumatic stress lack these components. For instance, due to high noncompleter rates and residual symptoms, the National Academies Health and Medicine Division (formerly the Institute of Medicine [IOM]) deems that PTSD treatment outcomes do not have sufficient certainty in their effectiveness (Institute of Medicine Committee on Treatment of Posttraumatic Stress Disorder, 2008).

Yoga contains these components that allow for increased physical self-experience and self-awareness, which are particularly important for these posttraumatic symptoms. However, though systematic reviews have shown promise for yoga as an intervention for certain conditions (e.g., cancer; Smith & Pukall, 2009), there is no systematic review and meta-analysis to the authors' knowledge that assesses the effectiveness of yoga specifically in the aftermath of trauma. Though Macy, Jones, Graham, and Roach (2015) conducted a metareview of yoga for trauma and related mental health problems, only two of the included reviews focus specifically on populations of trauma survivors (Longacre, Silver-Highfield, Lama, & Grodin, 2012; Telles, Singh, & Balkrishna, 2012), of which only the latter examines specifically yoga (vs. general complementary and alternative medicine practices in Longacre et al., 2012). The Telles et al. (2012) review is also limited in that it includes studies that are methodologically weak (e.g., single-arm studies) and events (i.e., incarceration) that do not necessarily meet the definition of trauma given by Criterion A of PTSD in the *Diagnostic and Statistical Manual of Mental Disorders* (5th ed.; *DSM–5*; American Psychiatric Association [APA], 2013), which is similar to the fourth edition of the *DSM* (*DSM–IV–TR*; APA, 2000) except that unexpected death of a family or close friend due to natural causes is no longer included.

In summary, the current literature is limited in assessing the impact of yoga on psychological symptoms. Though studies demonstrate the effectiveness of yoga as an intervention, these studies do not account for the context of trauma despite growing evidence for impact of trauma on the body (e.g., van der Kolk, 2006). Thus, the present study aims to add to the literature by being the first to systematically assess and quantitatively synthesize the effectiveness of yoga interventions for psychological symptoms (e.g., PTSD, depression, anxiety symptoms) following potentially traumatic life experiences (e.g., natural disasters, interpersonal violence, military combat).

Method

The present review was planned and conduced in accordance with the recommendations of the Cochrane Collaboration (Higgins & Green, 2008) and reported according to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (Moher, Liberati, Tetzladd, Altman, & the PRISMA Group, 2009).

Eligibility Criteria

Types of participants. Participants were individuals identified as having survived a trauma as defined by PTSD Criterion A of the *DSM–5* (APA, 2013). This includes exposure to actual or threatened death, serious injury, or sexual violence that is experienced either directly or indirectly. Though this differs from the fourth edition of the *DSM* (*DSM–IV–TR*; APA, 2000) in that unexpected death of a family or close friend due to natural causes is no longer included, diagnostic criteria between the two editions are similar and result in minimal changes in the prevalence of PTSD diagnoses (Kilpatrick et al., 2013). Studies that recruited participants who were experiencing a physiological health condition (e.g., stroke, cancer) were excluded because it is unclear whether the participants felt exposure to actual or threatened death and whether the participants' physiological health condition resulted from a serious injury. Studies with participants of any age group were eligible for review.

Types of interventions. Studies that explicitly used yoga as an intervention, either primary or adjunctive as a cointervention, were eligible for review. This includes both movement-based and breath-based yoga. Breath-based yoga interventions were included, as yoga does not necessarily entail a physical exercise component (for a review, see Cramer et al., 2013) and because these practices differ in intention and scope from other breathing practices or techniques. For instance, breath-based yoga focuses on breathing from the belly with training in retention and mindfulness of movements in the body (see, e.g., Jerath, Edry, Barnes, & Jerath, 2006); on the contrary, other breathing techniques (e.g., Pilates breathing) may focus on diaphragmatic breath (e.g., Cancelliero-Gaiad, Ike, Pantoni, Borghi-Silva, & Costa, 2014) or not incorporate setting an intention (e.g., purpose, awareness), which is important in yoga (see, e.g., Harris, Jennings, Katz, Abenavoli, & Greenberg, 2016). Studies examining interventions in which yoga is a minor part and not the explicit name of the intervention (e.g., mindfulness-based stress reduction; Kabat-Zinn, 1982) were excluded. There were no restrictions based on yoga tradition, length, frequency, or duration of the intervention.

Types of comparators. Studies were eligible for review if there was a comparison group to the yoga intervention, including but not limited to treatment as usual, an inactive control group, or an active control group. Randomization of participants was not required. Separate meta-analyses were conducted for randomized controlled trials (RCTs) and non-RCTs. Single-arm studies were excluded from review.

Types of outcome measures. In the present review, PTSD symptoms were considered the primary outcome of interest. Depression and anxiety symptoms were considered secondary outcomes of interest. Clinical diagnoses or cutoffs were not necessary, as measures of symptoms related to the aforementioned constructs (PTSD, depression, anxiety) were sufficient for inclusion. That is, the present review intended to measure whether yoga is effective in decreasing overall baseline levels of PTSD, depression, and anxiety, regardless of whether a clinical diagnosis of the aforementioned disorders was met. In this review, PTSD, depression, and anxiety were all considered psychological symptoms, of which these symptoms were measured after a potentially traumatic life event (for a description of trauma types, see the Types of Participants section above). Studies were included if they quantitatively measured at least one of the aforementioned three outcome measures. Studies that contained qualitative data only were excluded. There were no restrictions based on the measurement scale used or the manner in which the data were gathered (e.g., self-report, clinician interview).

Types of timing. Studies were included if outcome measures were completed at least twice, once preintervention and at least once postintervention. There were no restrictions based on timing of postintervention assessment or length of follow up.

Types of settings. There were no restrictions based on the setting of the yoga intervention. Studies were included regardless of geographic location. Further, studies were included regardless of where the intervention was delivered: for example, a clinic or community setting.

Search Methods

The following electronic databases were searched from their inception through February 25, 2016: PsycINFO, Ovid Medline/PubMed, Cumulative Index to Nursing and Allied Health Literature (CINAHL), and Embase/Embase Classic. For PsycINFO, the following search strategy was used: (*yoga AND (trauma* OR violence OR assault OR accident OR combat OR war OR military OR disaster)).mp.* [mp = title, abstract, heading word, table of contents, key concepts, original title, tests and measures], following similar constructions for the other three databases. As permissible by the search algorithms of each database, journal articles were then limited to works that were peer-reviewed English publications. A post hoc hand search of the following databases and publicly available paper repositories were also conducted using the aforementioned terms: Google Scholar, Mendeley, Open Research and Contributor Identification, and Fig Share.

Two review authors (the first and second authors of the present study) independently screened the title and abstracts identified through the systematic literature search. The two review authors read the potentially eligible articles in full to determine eligibility based on the criteria that were developed a priori. Reference lists of the identified articles were searched manually for additional eligible works. During this process and the processes detailed below, the two review authors discussed disagreements between themselves or with a third review author (the third author of the present study) until a consensus was reached.

Data Extraction and Management

The first and second authors independently extracted data on participants (e.g., trauma type, age, gender/sex), intervention (e.g., type of yoga, primary or adjunct status, frequency, duration), comparators, outcomes (e.g., measures used), timing (e.g., length of follow-up), and setting (e.g., location of intervention) into a data extraction form that was developed a priori.

Risk of Bias

The first and second authors independently assessed risk of bias using the Cochrane Back Review Group risk of bias tool (Higgins & Green, 2008). This tool assesses risk of bias in the following six domains: sequence generation, allocation concealment, blinding of outcome assessors, incomplete outcome data, selective outcome reporting, and other sources of bias. Blinding of participants and personnel was not considered in the present study due to the infeasibility of blinding participants, given the nature of the intervention. Allocation concealment was considered in the context of concealing allocation when study design incorporated an active control group. For each domain, risk of bias was assessed as (a) low risk of bias, (b) unclear, or (c) high risk of bias. Low risk of bias was assigned for a domain if the study met all appropriate judging criteria given by the Cochrane Back Review Group risk of bias tool. Overall, low risk of bias was assigned if there was low risk of bias in all domains with the exception of allocation concealment, given the aforementioned study design constraints. High risk of bias was assigned overall if there was a high risk of bias in any additional domain outside of allocation concealment. Otherwise, studies were assigned an unclear risk of bias.

Strength of Evidence

The first and third authors independently assessed strength of evidence for each outcome using the Grading of Recommendations Assessment, Development and Evaluation (GRADE) guidelines (Balshem et al., 2011). According to these guidelines, the initial quality of a body of evidence is based on risk of bias in study design (e.g., randomized trial has a high strength of evidence; observational studies have a low strength of evidence). This initial strength of evidence is then lowered if there are serious or very serious infractions in each of the following four domains: inconsistency, indirectness, imprecision, or reporting bias. The strength of evidence is then increased based on criteria in the following three domains: large effect, dose response (i.e., evidence of a gradient), or all plausible residual confounding factors were considered. Overall quality ratings were assessed for each study and overall per outcome as high, moderate, low, or very low strength of evidence.

Data Analysis

Effects of the yoga intervention compared with different comparison interventions were analyzed separately. If a study contained multiple intervention conditions (e.g., yoga as a cointervention or multiple comparison conditions), then only differences between the primary yoga intervention and an inactive control group were included in the meta-analysis. Because the active arms were not appropriate comparators and were inconsistent across

studies (e.g., aerobic dance, experimental testimony), the decision to use only the inactive control groups was a consequence of the literature review. Simple meta-analyses were conducted if at least two studies assessing the specific outcome were available. Because all outcomes were measured on a continuous scale, standardized mean differences were calculated as within-group and between-Group Cohen's ds, the difference in means between groups divided by the pooled standard deviation. When no standard deviations were available, they were calculated from the standard deviations given by the change scores. This method followed when unweighted means were not available. A positive within-group d was defined as increased scores on the outcome measures from pre- to postintervention: for example, more PTSD symptoms. Between-groups ds indicated the difference in within-group ds between the yoga intervention and the comparison condition, in which a positive between-groups d was defined as the yoga intervention having more change than the comparison condition. Scores were adjusted as necessary to allow for all ds to be interpreted in the same direction. Cohen's categories (Cohen, 1988) were used to evaluate the magnitude of effect sizes as either small ($0.20 \leq d < 0.50$), medium ($0.50 \leq d < 0.80$), or large ($d \geq 0.80$). Confidence intervals around the standardized mean differences were not calculated. Due to the wide variability of studies, this quantitative synthesis is intended to be a rough estimate of effect for the body of evidence.

Results

Literature Search

The literature search on PsycINFO generated 52 articles, of which 12 were retained after title and abstract screening. Ovid Medline generated 59 articles, of which 12 were retained after title and abstract screening. CINAHL generated 40 articles, nine of which were retained after title and abstract screening. Lastly, Embase generated 249 articles, 10 of which were retained after title and abstract screening. Articles excluded during this stage were removed if they clearly failed to meet any of the eligibility criteria (see the Eligibility Criteria subsection above), as evident by specific statements in the article's title or abstract, for example, participants had not experienced a potentially traumatic event, lack of a yoga intervention, the study was described as single-arm only, none of the psychological symptoms of interest were measured, data were only qualitative, and there were no pre/post assessments. After removal of duplicates and screening, 17 articles were retained for full-text review. Of the 17 articles, eight were excluded for the following reasons: data appeared to be secondary analysis of another study ($n = 3$; Dick, Niles, Street, DiMartino, & Mitchell, 2014; Martin, Dick, Scioli-Salter, & Mitchell, 2015; Reddy, Dick, Gerber, & Mitchell, 2014), no comparison group was used ($n = 3$; Pradhan, Gray, Parikh, Akkireddi, & Pumariega, 2015; Staples, Hamilton, & Uddo, 2013; Telles, Naveen, & Dash, 2007), data were only qualitative ($n = 1$; Jindani & Khalsa, 2015), or article did not involve data collection ($n = 1$; Fiore, Nelson, & Tosti, 2014). No additional articles were retrieved from the reference lists of identified articles. Three additional articles (Carter et al., 2013; Jindani, Turner, & Khalsa, 2015; Quiñones, Maquet, Vélez, & López, 2015) were retrieved from the hand search of nonsystematically searchable databases and publicly available paper repositories. Thus, 12 articles in total ($N = 791$) were included in the qualitative synthesis. A flowchart of the literature search is displayed in Figure 1.

Study Characteristics

Of the 12 included articles, 10 were RCTs ($n = 542$; Carter et al., 2013; Culver, Whetten, Boyd, & O'Donnell, 2015; Franzblau, Echevarria, Smith, & Van Cantfort, 2008; Jindani et al., 2015; Mitchell et al., 2014; Quiñones et al., 2015; Seppälä et al., 2014; Stoller, Greuel, Cimini, Fowler, & Koomar, 2012; Telles, Singh, Joshi, & Balkrishna, 2010; van der Kolk et al., 2014) and two were non-RCTs ($n = 249$; Descilo et al., 2010; Thordardottir, Gudmundsdottir, Zoëga, Valdimarsdottir, & Gudmundsdottir, 2014). All but one study (Culver et al., 2015) included adults only. Half of the RCTs ($n = 5$) were conducted outside of the United States: Australia (Carter et al., 2013), Canada (Jindani et al., 2015), India
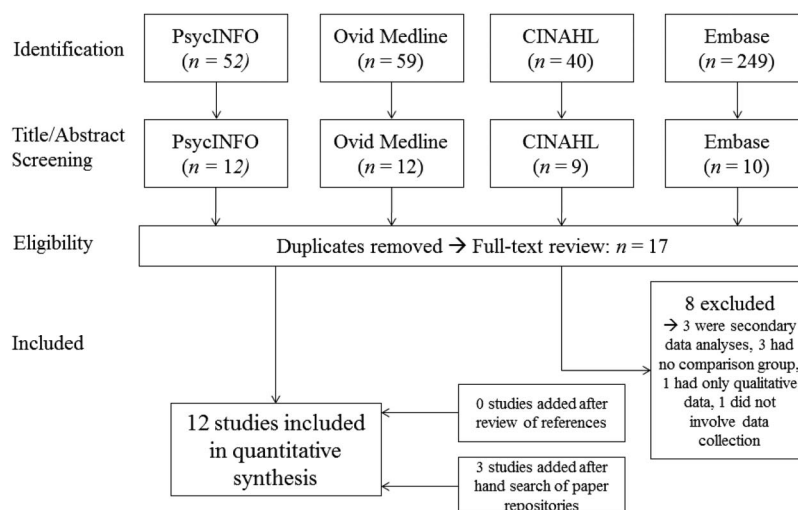

Figure 1. Flowchart of the results of the literature search.

(Telles et al., 2010), Colombia (Quiñones et al., 2015), and Haiti (Culver et al., 2015). Both non-RCTs were conducted outside of the United States, one in India (Descilo et al., 2010) and the other in Iceland (Thordardottir et al., 2014). Trauma type included: broadly defined Criterion A trauma ($n = 4$; Culver et al., 2015; Jindani et al., 2015; Mitchell et al., 2014; Quiñones et al., 2015), natural disaster ($n = 3$; Descilo et al., 2010 [tsunami]; Telles et al., 2010 [flood]; Thordardottir et al., 2014 [earthquake]), military-related trauma ($n = 3$; Carter et al., 2013; Seppälä et al., 2014; Stoller et al., 2012), and interpersonal trauma ($n = 2$; Franzblau et al., 2008 [intimate partner violence]; van der Kolk et al., 2014 [childhood assaults]). Studies primarily included both male and female participants, though three studies included male-identified participants only (Carter et al., 2013; Seppälä et al., 2014; Telles et al., 2010) and three studies included woman-identified participants only (Franzblau et al., 2008; Mitchell et al., 2014; van der Kolk et al., 2014). A summary of study characteristics is included in Table 1.

Intervention Characteristics

Yoga was the primary intervention in all of the studies with the exception of van der Kolk et al. (2014), in which participants were required to have ongoing supportive therapy while participating in the yoga program; however, this ongoing therapy was not assessed or intended to be assessed by van der Kolk et al. Though yoga protocols were sparsely described, all yoga interventions were an integration of movement and breath, with the exception of Franzblau et al. (2008), which was breath only. The type of yoga stemmed from a variety of sources, including Kriya (Carter et al., 2013; Descilo et al., 2010; Seppälä et al., 2014), trauma-sensitive (Mitchell et al., 2014 [Kripalu]; van der Kolk et al., 2014), hatha (Stoller et al., 2012 [sensory enhanced]; Thordardottir et al., 2014 [integrated]), Patanjali (Telles et al., 2010), Kundalini (Jindani et al., 2015), Satyananda (Quiñones et al., 2015), and simply poses with guided meditation and without a formal yoga lineage affiliation (Culver et al., 2015). Six studies (Carter et al., 2013; Descilo et al. (2010); Jindani et al., 2015; Stoller et al., 2012; Thordardottir et al., 2014; van der Kolk et al., 2014) explicitly stated using a certified yoga instructor for the intervention piece (vs. intervention development; Descilo et al., 2010), though the certifying organizations were unclear with the exception of Jindani et al. (International Kundalini Yoga Teachers Association). Excluding one study that had monthly booster sessions (Carter et al., 2013), the number of yoga sessions ranged from 2 to 16 ($M = 10.67$ sessions, $SD = 7.67$ sessions), spanning from 2 days to 16 weeks. The duration of a single yoga session ranged from 45 to 300 min ($M = 89.17$ min, $SD = 51.51$ min). Yoga completion rates were calculated as the percent of participants who completed the yoga intervention, except that Stoller et al. (2012) and Quiñones et al. (2015) only provided the percent of participants who met the minimum class completion requirement and Culver et al. (2015) only provided the percent of overall classes completed. Yoga completion rates ranged from 49% to 100% ($M = 85.46\%$, $SD = 15.45\%$). Only one study used an attention control (van der Kolk et al., 2014). Nine used a waitlist control (Carter et al., 2013; Culver et al., 2015; Descilo et al., 2010; Jindani et al., 2015; Mitchell et al., 2014; Quiñones et al., 2015; Seppälä et al., 2014; Telles et al., 2010; Thordardottir et al., 2014), and two did not provide control group

details (Franzblau et al., 2008; Seppälä et al., 2014). While only data from yoga and inactive comparison conditions were used in the present study, the three multiple comparison studies also included aerobic dance (Culver et al., 2015), traumatic incident reduction therapy (Descilo et al., 2010), and experimental testimonies (Franzblau et al., 2008). With the exception of Franzblau et al. (2008), between-groups ds favored the yoga intervention over these other conditions. A summary of intervention characteristics is included in Table 1.

Outcome Measures

Only three studies included follow-up assessments of outcome measures beyond a postintervention (Mitchell et al., 2014 [1 month]; Seppälä et al., 2014 [1 month, 1 year]; Descilo et al., 2010 [6 weeks, 12 weeks, 24 weeks]); thus, these follow-up data were not included in the quantitative synthesis though results were either maintained, or if symptoms had increased, levels remained below baseline levels. Carter et al. (2013) tapered their yoga intervention from daily, to weekly, to monthly. Because their assessment occurred during the weekly tapering of the yoga intervention, it was considered as a postassessment (rather than a follow up, as described in Carter et al., 2013), particularly considering no postassessment occurred immediately after the daily yoga intervention. A summary of outcome measures is included in Table 1.

PTSD symptoms. Ten of 12 studies included a measure of PTSD symptoms, which was the primary outcome of interest. Nearly every study incorporated a different measure of PTSD: a fear visual analog scale (in Telles et al., 2010), the University of California at Los Angeles PTSD Reaction Index (Steinberg, Brymer, Decker, & Pynoos, 2004; in Culver et al., 2015), the PTSD-Checklist Civilian (Andrykowski, Cordova, Studts, & Miller, 1998; Dobie et al., 2002; in Mitchell et al., 2014; Quiñones et al., 2015), the PTSD-Checklist Military (Blanchard, Jones-Alexander, Buckley, & Forneris, 1996; in Seppälä et al., 2014), the PTSD Check List (Weathers, Litz, Herman, Huska, & Keane, 1993; in Jindani et al., 2015; Descilo et al., 2010), the Clinician-Administered PTSD Scale (Weathers, Ruscio, & Keane, 1999; in Carter et al., 2013; van der Kolk et al., 2014), and the Posttraumatic Stress Diagnostic Scale (Foa, Cashman, Jaycox, & Perry, 1997; in Thordardottir et al., 2014). Carter et al. (2013) included two measures of PTSD, though only their main PTSD symptom measure was included.

Depression symptoms. Eight of 12 studies included a measure of depression symptoms. The Beck Depression Inventory—Second Edition (Beck, Steer, & Brown, 1996) was the modally used measure of depression symptoms ($n = 3$; in Franzblau et al., 2008; van der Kolk et al., 2014; Thordardottir et al., 2014). Other measures of depression symptoms included the Center for Epidemiological Studies—Depression Scale (Radloff, 1977; in Carter et al., 2013; Mitchell et al., 2014), the Beck Depression Inventory (Beck & Steer, 1987; in Descilo et al., 2010), the Depression, Anxiety, and Stress Scale (DASS-21; Lovibond & Lovibond, 1995; in Jindani et al., 2015), and a sadness visual analog scale (in Telles et al., 2010).

Anxiety symptoms. Five of 12 studies included a measure of anxiety symptoms. The State–Trait Anxiety Inventory—State subscale (Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983) was

Table 1
Characteristics of the Included Studies

Authors (year)	Location	Trauma type	N (Sum = 791)	Sample	Yoga type	Yoga duration (completion rate)	Conditions	Follow-up	Measures, language (if not English): (a) PTSD, (b) depression, (c) anxiety symptoms
Randomized controlled trials									
Culver et al. (2015)	Haiti	Criterion A	76	7–17yo males, females	Poses and guided meditation[a]	16 sessions × 45 min, 8 weeks (92%)	(1) Yoga (2) Aerobic dance (3) Waitlist	Post	(a) UCLA PTSD Reaction Index, not listed (potentially Haitian Creole)[e-g]
Jindani et al. (2015)	Canada	Criterion A	80	Adult males, females	Kundalini[c]	8 sessions × 90 min, 8 weeks; 15 min daily homework (49%)	(1) Yoga (2) Waitlist	Post	(a) PCL-17[f] (b) DASS-21, Depression[f] (c) DASS-21, Anxiety[f]
Mitchell et al. (2014)	United States	Criterion A	38	18–65yo women	Trauma-sensitive: Kripalu[b]	12 sessions × 75 min, 6–12 weeks (70%)	(1) Yoga (2) Waitlist	Post 1 month	(a) PCL-Civilian[f] (b) CES-Depression[f] (c) STAI-State[f]
Quiñones et al. (2015)	Colombia	Criterion A	100	Adult males, females	Satyananda[b]	32 sessions × 60 min, 16 weeks (84%)	(1) Yoga (2) Waitlist	Post	(a) PCL-Civilian, Spanish[f]
Telles et al. (2010)	India	Natural disaster: flood	22	Adult males	Patanjali[a]	7 sessions × 60 min, 7 days (100%)	(1) Yoga (2) Waitlist	Post	(a) Fear VAS (b) Sadness VAS (c) Anxiety VAS
Carter et al. (2013)	Australia	Military-related	31	Adult males	Sudarshan Kriya yoga[c]	22 hr over 5 days; then 4 session × 120 min, 4 weeks; then 5 session × 120 min, 5 months (88%)	(1) Yoga (2) Waitlist	Post[d]	(a) CAPS[f] (b) CES-Depression[f]
Seppälä et al. (2014)	United States	Military-related	21	Adult males	Kriya[a]	7 sessions × 180 min, 7 days (91%)	(1) Yoga (2) Waitlist	Post 1 month	(a) PCL-Military[f]
Stoller et al. (2012)	United States	Military-related	70	Adult males, females	Hatha: sensory-enhanced[c]	9+ sessions × 75 min, 3 weeks (71%)	(1) Yoga (2) Control	Post	(c) STAI-State[f]
Franzblau et al. (2008)	United States	Interpersonal: intimate partner violence	40	18–45yo women	Yogic breathing[a]	2 sessions × 45 min, 2 days (100%)	(1) Yoga (2) Testimony (3) 1 and 2 (4) Control	Post	(b) BDI-II[e]
van der Kolk et al. (2014)	United States	Interpersonal: childhood assaults	64	18–58yo women	Trauma-sensitive: adjunct to therapy[b]	10 sessions × 60 min, 10 weeks (97%)	(1) Yoga (2) Control	Post	(a) CAPS[f] (b) BDI-II[e]
Nonrandomized controlled trials									
Descilo et al. (2010)	India	Natural disaster: tsunami	183	18–65yo men, women	Kriya: breath, water, sound[c]	4 sessions × 120 min, 4 days (100%)	(1) Yoga (2) Yoga and exposure therapy (3) Waitlist	Post 6 week 12 week 24 week	(a) PCL-17, Tamil[e-g] (b) BDI-21, Tamil[f,g]
Thordardottir et al. (2014)	Iceland	Natural disaster: earthquake	66	20–67yo men, women	Hatha: integrated[e]	12 sessions × 60 min, 6 weeks (84%)	(1) Yoga (2) Waitlist	Post	(a) PDS, Icelandic[f] (b) BDI-II, Icelandic[f] (c) BAI, Icelandic[f]

Note. yo = years old; min = minutes; PTSD = posttraumatic stress disorder; VAS = Visual Analog Scale; UCLA = University of California at Los Angeles; PCL = PTSD Checklist; DASS = Depression, Anxiety, and Stress Scale; CES = Center for Epidemiological Studies; STAI = State-Trait Anxiety Inventory; BDI = Beck Depression Inventory; CAPS = Clinician-Administered PTSD Scale; PDS = Posttraumatic Stress Diagnostic Scale; BAI = Beck Anxiety Inventory. Included studies have been ordered by trauma type and then alphabetized by author within trauma-type categories.
[a] Unclear who delivered the yoga intervention. [b] Yoga intervention delivered by professional yoga instructor, though certification unclear. [c] Yoga intervention delivered by professional yoga instructor with certification explicitly stated. [d] Assessments occur a month after the intensive five-day yoga sessions, though post the weekly yoga booster sessions. [e] Validity or reliability of measure established elsewhere (e.g., by authors of instrument) and not reported by authors of intervention study. [f] Validity or reliability of measure established, as reported by authors of study. [g] Validity or reliability of measure translated from English is not reported.

the modally used measure of anxiety symptoms ($n = 2$; in Mitchell et al., 2014; Stoller et al., 2012). The other measures of anxiety symptoms were the Beck Anxiety Inventory (Beck & Steer, 1993; in Thordardottir et al., 2014), DASS-21 (Lovibond & Lovibond, 1995; in Jindani et al., 2015), and an anxiety visual analog scale (in Telles et al., 2010).

Risk of Bias

Three RCTs were determined to be unclear in their risk of bias (Franzblau et al., 2008; Seppälä et al., 2014; van der Kolk et al., 2014). The remaining six RCTs (Carter et al., 2013; Culver et al., 2015; Jindani et al., 2015; Mitchell et al., 2014; Quiñones et al., 2015; Stoller et al., 2012) and both non-RCTs (Descilo et al., 2010; Thordardottir et al., 2014) were determined to have high risk of bias. Studies in which there were active comparison groups were deemed to have unclear status for allocation concealment ($n = 3$; Culver et al., 2015; Descilo et al., 2010; Franzblau et al., 2008), as the effect of multiple study arm choices on the potential to influence assignment is unknown. Given the comparative results of the other studies, Telles et al. (2010) was determined to have moderate risk of bias, as it was the only study in which there was only one category deemed as high risk of bias and all risk of bias assessment categories were determinable. Telles et al. (2010) scored high on its risk of bias for allocation concealment regarding its study participants; however, the outcome assessor was blinded. Five studies explicitly stated that outcome assessors were blinded (Carter et al., 2013; Quiñones et al., 2015; Stoller et al., 2012; Telles et al., 2010; van der Kolk et al., 2014;) while four others studies explicitly discussed a lack of blinding (Culver et al., 2015; Descilo et al., 2010; Jindani et al., 2015; Mitchell et al., 2014). Half of the studies ($n = 6$) either had 100% retention (Descilo et al., 2010; Franzblau et al., 2008; Telles et al., 2010) or used statistical models that accounted for missing data (Culver et al., 2015; Seppälä et al., 2014; van der Kolk et al., 2014) while the other six studies did not clearly identify how missing data were addressed (Carter et al., 2013; Jindani et al., 2015; Mitchell et al., 2014; Quiñones et al., 2015; Stoller et al., 2012; Thordardottir et al., 2014). Studies were primarily determined to be unclear in their

selective reporting ($n = 8$) with the exception of three studies that were determined to be at low risk of bias due to reporting of all primary and secondary outcomes (Jindani et al., 2015; Telles et al., 2010; Thordardottir et al., 2014) and one study that was determined to be at high risk of bias due to not having a PTSD measure despite presenting a rationale for it in the study's introduction (Stoller et al., 2012). Studies appeared to lack other biases (e.g., baseline imbalance), particularly due to the interventional study design that did not feasibly permit for an early stopped data collection. As 10 or fewer studies were included in each meta-analysis, funnel plots were not analyzed for possible publication bias. The risk-of-bias assessments are presented in Table 2.

Analyses of Overall Effects

PTSD symptoms. Across studies ($n = 10$), quantitative syntheses demonstrated that the average within-group d was -1.32 for yoga interventions and -0.26 for comparison conditions. Based on Cohen's categories, the effect size for yoga interventions decreasing PTSD symptoms was large while the effect size for the comparison condition was small. The between-groups effect size was large, $d = 1.06$, with a positive d indicating that the yoga intervention had greater changes in pre- and postintervention measures of PTSD symptoms than did the comparison condition. The between-groups effect size was medium-large for RCTs ($d = 0.76$; $n = 8$) and large for non-RCTs ($d = 2.00$; $n = 2$). Table 3 displays a summary of the meta-analyses and all within-group effect sizes for PTSD symptoms and the following outcomes (depression and anxiety symptoms).

Depression symptoms. Across studies ($n = 8$), quantitative syntheses demonstrated that the average within-group d was -0.72 for yoga interventions and -0.21 for comparison conditions. Based on Cohen's categories, the effect size for yoga interventions decreasing depression symptoms was medium-large while the effect size for the comparison condition was small. The between-groups effect size across studies moderately favored yoga interventions, $d = 0.53$. The between-groups effect size was small for RCTs ($d = 0.24$; $n = 6$) and large for non-RCTs ($d = 1.39$; $n = 2$).

Table 2
Risk of Bias Assessment Using the Cochrane Back Review Group Risk of Bias Tool

Authors (year)	Sequence generation	Allocation concealment	Blinding of outcome assessor	Incomplete data addressed	Free of selective reporting	Free of other biases	Risk of bias
Randomized controlled trials							
Culver et al. (2015)	Unclear	Unclear	High	Low	Unclear	Low	High
Jindani et al. (2015)	Low	High	High	High	Low	Low	High
Mitchell et al., 2014	Low	High	High	Unclear	Unclear	Low	High
Quiñones et al. (2015)	Low	High	Low	High	Unclear	Low	High
Telles et al. (2010)	Low	High	Low	Low	Low	Low	Moderate
Carter et al. (2013)	Low	High	Low	High	Unclear	Low	High
Seppälä et al. (2014)	Low	High	Unclear	Low	Unclear	Low	Unclear
Stoller et al. (2012)	Low	High	Low	Unclear	High	Low	High
Franzblau et al. (2008)	Unclear	Unclear	Unclear	Low	Unclear	Low	Unclear
van der Kolk et al. (2014)	Unclear	High	Low	Low	Unclear	Low	Unclear
Nonrandomized controlled trials							
Descilo et al. (2010)	High	Unclear	High	Low	Unclear	Low	High
Thordardottir et al. (2014)	High	High	Unclear	Unclear	Low	Low	High

Note. Low = low risk of bias; Moderate = moderate risk of bias; Unclear = unclear risk of bias; High = high risk of bias.

Table 3
Meta-Analysis of Effect Sizes

Outcomes	Within-group *d* yoga	Within-group *d* comparison	Between-group *d*	No. of studies
Overall				
Posttraumatic stress disorder	−1.32	−.26	1.06	10
Depression	−.72	−.21	.53	8
Anxiety	−.56	−.16	.40	5
N at preintervention	314	300		
Randomized controlled trials				
Posttraumatic stress disorder	−1.05	−.25	.76	8
Depression	−.43	−.19	.24	6
Anxiety	−.61	−.18	.43	4
N at preintervention	229	207		
Nonrandomized controlled trials				
Posttraumatic stress disorder	−2.39	−.39	2.00	2
Depression	−1.60	−.21	1.39	2
Anxiety	−.39	−.09	.30	1
N at preintervention	85	93		

Note. Between-group *d*s are calculated as the comparison condition within-group *d* minus the yoga condition within-group *d*. Positive *d*s indicate greater changes in the yoga intervention condition than in the comparison condition.

Anxiety symptoms. Across studies ($n = 5$), quantitative syntheses demonstrated that the average within-group *d* was −0.56 for yoga interventions and −0.16 for comparison conditions. Based on Cohen's categories, the effect size for yoga interventions decreasing anxiety symptoms was medium while the effect size for the comparison condition was negligible. The between-groups effect size across studies was small in favor of yoga interventions, $d = 0.40$. The between-groups effect size was small for RCTs ($d = 0.43$; $n = 4$) and not computable for non-RCTs due to only one eligible study (between-groups $d = 0.30$; Thordardottir et al., 2014).

Strength of Evidence

The strength of evidence for the PTSD, depression, and anxiety outcomes were all found to be low (Table 4). The studies contributing to the PTSD, depression, and anxiety outcome categories appeared to have a high risk of bias. Strength of evidence was reduced for directness if measures were used that did not directly measure the psychological symptom of interest (i.e., Telles et al., 2010) or involved another experimental question (i.e., Franzblau et al., 2008); in general, studies across psychological symptoms primarily seemed direct. Strength of evidence was reduced for precision if the finding was not adequately powered, had high counts of missing data, or used clinical scales that did not demonstrate validity or reliability in capturing the symptom of interest,

particularly those that had been translated into a language other than English (i.e., Culver et al., 2015; Descilo et al., 2010); this described a majority of the studies, demonstrating that the studies across psychological symptoms showed mostly imprecision. Only three studies that contributed to the body of evidence for all three psychological symptoms (Jindani et al., 2015; Telles et al., 2010; Thordardottir et al., 2014) were deemed likely not to have reporting bias because they included the widest range of relevant psychological symptoms. All studies appropriately related concepts and theoretical rationale but all would have benefited from more robust control of potential confounders and none examined a dose gradient.

Discussion

This systematic review and quantitative synthesis of 12 empirical studies did not find strong evidence quality and was unable to confirm or refute the effectiveness of yoga as an intervention for PTSD, depression, and anxiety symptoms following traumatic life experiences. Further research is warranted. Though overall between-groups (yoga vs. comparison) effect sizes ranged from ds = 0.40–1.06, this conclusion is based on the low quality and high risk of bias of studies and not only the quantitative synthesis of data. Nonetheless, while yoga may seem to be a promising treatment for PTSD, depression, and anxiety among trauma survivors, the rigor of future studies must improve to allow more

Table 4
Strength of Evidence Using Grading of Recommendations Assessment, Development, and Evaluation Guidelines

Outcome	Between-group *d*	Risk of bias	Inconsistency	Indirectness	Imprecision	Reporting bias	Quality of a body of evidence
Posttraumatic stress disorder	1.06	High	Consistent	Direct	Imprecise	Unclear	Low
Depression	.53	High	Consistent	Direct	Imprecise	Unclear	Low
Anxiety	.40	High	Consistent	Direct	Imprecise	Unlikely	Low

Note. Between-group *d*s are calculated as the comparison condition within-group *d* minus the yoga condition within-group *d*. Positive *d*s indicate greater changes in the yoga intervention condition than in the comparison condition.

definitive conclusions regarding the use of yoga in mental health treatment of trauma survivors.

This review is unique in being the first systematic review and quantitative synthesis of yoga interventions for psychological symptoms following trauma and is a first attempt to summarize the existing literature in this growing field. The ability to cull quantitative data for a meta-analysis strengthened the quality of the review. This review improves upon the one prior review of the literature (Telles et al., 2012) that did not include a quantitative synthesis, did not clearly define trauma (e.g., from *DSM–5* PTSD Criterion A in the present study), and included studies that were methodologically weaker than in the present review (e.g., noncontrolled trials).

However, this review is not without limitations. The methodological quality is limited by the number and rigor of studies included in the review, particularly with the non-RCTs. Of main concern is the lack of attention controls for the studies, as only one study used an attention control (van der Kolk et al., 2014). This leaves the other studies unable to separate effects due to yoga versus effects due to attention in the context of a therapeutic relationship. Studies lacked adequate follow up to determine the effectiveness of yoga in the long term. Additionally, the studies in this review included yoga interventions that lasted only from 2 days to 16 weeks. This review was also unable to assess a dose gradient on intervention effectiveness due to the varied yoga treatments in addition to differences in doses. This review's approach was to group all studies reporting an outcome to look for any signal that yoga is effective. Though GRADE also calls for increased strength of evidence for larger effects, this criterion was not used in this review; due to the small sample sizes of the included studies and the differential in effect size between the randomized and nonrandomized studies, the effect sizes may be reduced in more methodologically rigorous studies. Given the nature of these yoga intervention studies, it is difficult to control for all possible confounders, as it is not feasible to blind participants and only seemingly feasible to blind data analysts.

In addition, though traumas were defined under *DSM–5* Criteria A (APA, 2013), there were a variety of different trauma types that were assessed together. Participants of all genders and sexes were also grouped together despite gender- or sex-based differences in yoga outcomes, for example, differences in anatomy, hormonal energy; though six of the 12 studies were coed, only one (Stoller et al., 2012) included data by sex, which did not permit for subgroup analyses in this domain. Comparisons of studies among the genders or sex appear to be comparisons of trauma type rather than gender or sex. Specifically, of the three studies with males only, two of them focused on military-related trauma (Carter et al., 2013; Seppälä et al., 2014); of the three studies with women only, two of them focused on interpersonal traumas, namely, interpersonal violence, childhood assault (Franzblau et al., 2008; van der Kolk et al., 2014). Subgroup analyses were also not conducted by the country in which research originated. Two of the four studies using measures translated in languages other than English (Culver et al., 2015; Descilo et al., 2010) failed to give validity and reliability for their translated instruments, which are important in establishing the precision of the measures; however, because of the limited number of studies conducted in non-English-speaking countries, these subgroup analyses were unable to be conducted. A variety of yoga programs were also assessed together, as the number of studies included in the review was not adequate enough to allow for proper subgroup analyses. Due to the lack of manualized yoga interventions for trauma, a lack of fidelity checks in the included

sample, and a lack of detail regarding the training and certifications of yoga instructors, it was not possible to conduct proper subgroup analyses of the various yoga programs. Some of the yoga interventions were not specific to yoga only (e.g., yoga being offered adjunctive to psychotherapy treatment). This lack of standardization makes it difficult to translate the yoga interventions to a clinical environment. Lastly, the review only includes articles that were written in English. This has the potential to exclude research conducted in other countries that might have met the study criteria and limits the generalizability of results to non-English-speaking countries.

This review has implications for future research and clinical practice to provide more information to understand the effectiveness of yoga interventions on psychological symptoms following trauma. Particularly because yoga interventions may be considered a complex suite incorporating various components (e.g., awareness of physical movements, breath work), future research is needed to support guidelines to replicate such interventions and translate them to clinical practice. The effect sizes from the controlled studies may not translate to real-world settings, nor do the studies necessarily take client preferences into account. Future reviews and studies also need to take into account translatability into the community (e.g., Clark et al., 2014) and perceptions of safety (e.g., Nguyen-Feng, Feng, Babbar, Rankins, & Blando, 2014) in addition to simply outcome-based assessments. Nguyen-Feng et al. (2014) demonstrate how safety perceptions of yoga may be impacted by personal characteristics of participants (e.g., health, previous experiences with yoga); particularly, some individuals are more likely to believe yoga safety information from yoga studio staff rather than health care providers, which points to the importance of how yoga is presented in research studies and who presents that information. Safety perceptions seem particularly important to consider in research with trauma survivors given how safety is impacted in survivors' traumatic experiences. Despite the importance of assessing and monitoring safety, studies in the review seldom did so. Future studies that are more methodologically rigorous (e.g., attention control, testing dose-response gradient, larger sample sizes, blinded data analysts) to address the limitations of current studies are also strongly needed to provide more empirical support for the various forms of yoga as an effective intervention following trauma. Though the popularity of yoga is increasing, the literature on yoga as an intervention for psychological symptoms following trauma appears to be underdeveloped, despite its potential to be significantly beneficial. Rather than simply more research on this topic, the present review calls for higher quality, better designed studies to move this field forward.

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