

**TRAUMA-FOCUSED COGNITIVE BEHAVIORAL THERAPY FOR CHILDREN AND
ADOLESCENTS AND THERAPISTS' ATTITUDES TOWARD EVIDENCE-BASED
PRACTICE**

Effectiveness, Long-Term Outcomes and Validation of the German Evidence-Based Practice
Attitude Scale-36

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submitted by

Jonathan Felix Benjamin Thielemann from Hamburg

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Examiner: Prof. Dr. Rita Rosner

Co-examiner: Prof. Dr. Antonia Barke

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LIST OF ABBREVIATIONS

AT/TAU	Active Treatment and Treatment as Usual
CAPS-CA	Clinician-Administered PTSD Scale for Children and Adolescents
CBITS	Cognitive Behavioral Intervention for Trauma in Schools
CBT	Cognitive Behavioral Therapy
CPT	Cognitive Processing Therapy
CDI	Children's Depression Inventory
CFA	Confirmatory Factor Analysis
CPSS	Child PTSD Symptom Scale
EBP	Evidence-Based Practice
EBPAS(-15; -36; -50; D)	Evidence-Based Practice Attitudes Scale (15-item version; 36-item version; 50-item version; German version)
EFA	Exploratory Factor Analysis
EGI-TG	Extended Grief Inventory-Traumatic Grief Subscale
EMDR	Eye Movement Desensitization and Reprocessing
ES	Effect Size
ICG	Inventory of Complicated Grief
ICS	Implementation Climate Scale
ISP	Intention Scale for Providers
KIDNET	Narrative Exposure Therapy for children and adolescents
K-SADS	Schedule for Affective Disorders and Schizophrenia for School-Age Children
PE	Prolonged Exposure
PTSD	Posttraumatic Stress Disorder
PTSS	Posttraumatic Stress Symptoms
RCTs	Randomized Controlled Trials
SCARED	Screen for Child Anxiety Related Emotional Disorders
(S)MFQ	Mood and Feelings Questionnaire (Short Version)
STAIC-T	State-Trait Anxiety Inventory for Children, Trait subscale
Specific TF-CBT	Trauma-Focused Cognitive Behavioral Therapy according to Cohen, Debliner and Mannarino (2006; 2017)
TF-CBT	Trauma-Focused Cognitive Behavioral Therapy
UCLA	UCLA Posttraumatic Stress Disorder-Reaction Index

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**PART 1. TRAUMA-FOCUSED COGNITIVE BEHAVIORAL THERAPY FOR
CHILDREN AND ADOLESCENTS AND THERAPISTS' ATTITUDES TOWARD
EVIDENCE-BASED PRACTICE**

1. Abstract

In children exposed to any traumatic experience, a conditional prevalence rate of 15.9% was estimated for posttraumatic stress disorder (PTSD; Alisic et al., 2014). In order to achieve best treatment outcomes, selecting evidence-based treatments is necessary (Durlak & DuPre, 2008). For pediatric PTSD, international guidelines recommend the use of manualized trauma-focused cognitive behavioral therapy (TF-CBT; Forbes, Bisson, Monson, & Berliner, 2020; National Institute for Health and Care Excellence, 2018; Phoenix Australia Centre for Posttraumatic Mental Health, 2013) and many effectiveness studies and initiatives (e.g. Goldbeck, Muche, Sachser, Tutus, & Rosner, 2016; Jensen et al., 2014; National Child Traumatic Stress Network, 2012) disseminated the TF-CBT manual of Cohen, Mannarino and Deblinger (2006, 2017; specific TF-CBT). However, the uptake of evidence-based treatments such as specific TF-CBT into practice is strongly affected by service providers' attitudes toward evidence-based practice (EBP; Aarons, 2004; Aarons, Cafri, Lugo, & Sawitzky, 2012; Aarons, Sommerfeld, & Walrath-Greene, 2009; Moullin, Dickson, Stadnick, Rabin, & Aarons, 2019). These attitudes can be measured with the Evidence-Based Practice Attitudes Scale-36 (EBPAS-36; Rye, Torres, Friberg, Skre, & Aarons, 2017). In Germany, several effectiveness trials are underway further disseminating specific TF-CBT (Rosner, Barke, et al., 2020; Rosner, Sachser, et al., 2020) but a reliable and validated German version of the EBPAS-36 is missing. In addition, in terms of pooled effect sizes (ES), only one review evaluated specific TF-CBT 10 years ago (Cary & McMillen, 2012). Furthermore, little is known about the stability of its treatment effects, the concordance between children and adolescents' and their caregivers' ratings regarding the youths' symptoms in this context as well as the influence of the heterogeneity of instruments used by individual studies.

Therefore, the aims of this dissertation were fourfold: Publication 1 pooled ES for specific TF-CBT regarding posttraumatic stress symptoms (PTSS) and secondary outcomes to provide an update of the literature. Large improvements were found across all outcomes and results were favorable for specific TF-CBT compared to all control conditions at post-treatment. Publication 2 pooled ES for specific TF-CBT at 12-month follow-up for the same outcomes to evaluate the stability of treatment effects and investigated concordance between participants and caregiver ratings. Again, large improvements were evident across all outcomes and TF-CBT was superior to active treatments and treatment as usual at 12-month follow-up. No significant differences were detected between participant and caregiver ratings with high reliability across almost all outcomes and assessment points. Publication 3 produced a validated German translation of the EBPAS-36 (EBPAS-36D) to facilitate EBP implementation research in Germany. Good psychometric properties and validity of the EBPAS-36D were confirmed. As additional content, this dissertation provides an unpublished report on the pooled ES of publication 1 differentiated by instruments used to assess the outcomes. ES did not differ by the instrument used to assess the respective outcome.

The conclusions that can be drawn from the studies as well as their limitations are discussed in the final section. While the EBPAS-36D was successfully validated and specific TF-CBT was strongly supported, several issues were identified that need further research attention.

2. Background

Among minors, traumatic experiences are common with one US study showing exposure to at least one type of traumatic experience in the past year in 57.7% of their sample (Finkelhor, Turner, Shattuck, & Hamby, 2013). Subsequent to any traumatic event, a conditional prevalence rate of 15.9% for PTSD was estimated in children (Alisic et al., 2014). Additionally, many children and adolescents with PTSS also present symptoms of depression and anxiety (American Psychiatric Association, 2013) with nearly one in two children and adolescents with PTSD also fulfilling the criteria of comorbid diagnoses (Kar & Bastia, 2006). If the underlying traumatic experience involves the death of a loved one, unresolved grief symptoms may further complicate this symptomatology (Cohen, Mannarino, & Staron, 2006). In recent years, numerous crises involving mass casualties have taken place (e.g. Covid-19, wars around the world, natural disasters, and terrorist attacks) leaving many young people in need of treatment. Consequently, there is a great need for effective treatments that can be quickly delivered on a large scale.

2.1. Emergence of trauma-focused cognitive behavioral therapy for children and adolescents.

With the development of the DSM-III (American Psychiatric Association, 1980), PTSD was clearly operationalized for the first time (Saigh, Green, & Korol, 1996). Researchers soon realized that many reports describing the mental health consequences of sexually abused children fit the diagnostic criteria of PTSD (Deblinger, McLeer, Atkins, Ralphe, & Foa, 1989; Krener, 1985; McLeer, Deblinger, Atkins, Foa, & Ralphe, 1988). However, at the time, research on treatment was mainly focused on the adult domain. For the most part, the literature on pediatric treatments consisted of descriptions of suggested interventions, anecdotes and case

studies (Deblinger, McLeer, & Henry, 1990). First efforts were made to evaluate treatment programs in sexually abused children (Giaretto, 1978; Sgroi, 1982) but they were subject to many methodological weaknesses and standard outcome measures were missing.

In 1990, a first uncontrolled study of specific TF-CBT specifically designed for sexually abused children and their parents was conducted by Deblinger et al. (1990) which later resulted in an early version of their manual (Deblinger & Heflin, 1996). Around the same time, another research group developed a very similar treatment approach (Cohen & Mannarino, 1993) resulting in the two manuals being merged in 1997 (Cary & McMillen, 2012). After being digitally available for several years, the official manual was published in 2006 (Cohen, Mannarino, & Deblinger, 2006) and revised in 2017 (Cohen et al., 2017) to address advances made in research, application to complex trauma and group settings, as well as updates for DSM-V (American Psychiatric Association, 2013). Over the years, the TF-CBT manual of Cohen, Mannarino, and Deblinger (2006, 2017) has been applied to many different pediatric populations (e.g. adjudicated youth, trafficked children, refugees, complex PTSD cases, inpatients, young adults) with various as well as multiple traumatic experiences (Cabrera, Moffitt, Jairam, & Barton, 2020; Cohen et al., 2016; Hébert & Amédée, 2020; Peters et al., 2021; Unterhitzberger, Wintersohl, Lang, König, & Rosner, 2019; Wang et al., 2016). As a result, several implementation manuals exist for application in residential treatment facilities (Cohen & Mannarino, 2013), military families (Cohen, Mannarino, & Cozza, 2014), foster care (Deblinger, Mannarino, Runyon, Pollio, & Cohen, 2016), as well as LGBTQ youth (Cohen, Mannarino, Wilson, & Zinny, 2018). Furthermore, international guidelines recommend the use of manualized TF-CBT for pediatric PTSD (Forbes et al., 2020; National Institute for Health and Care Excellence, 2018; Phoenix Australia Centre for Posttraumatic

Mental Health, 2013).

Although specific TF-CBT is widely used and the most extensively studied pediatric treatment for PTSD, other treatments originally developed for adults were also successfully applied to children and adolescents (Foa, McLean, Capaldi, & Rosenfield, 2013; Moreno-Alcázar et al., 2017; Neuner et al., 2008). Moreover, several other trauma-focused CBT treatments are available to treat pediatric PTSD, sharing many treatment components with the specific TF-CBT manual (Berger, Pat-Horenczyk, & Gelkopf, 2007; Celano, Hazzard, Webb, & McCall, 1996; Smith et al., 2007; Stein et al., 2003). Unfortunately, in the pediatric literature, this resulted in the term TF-CBT being used not only for the specific TF-CBT manual but also as a generic term for all trauma-focused CBT treatments. Therefore, the TF-CBT manual of Cohen, Mannarino, and Deblinger (2006, 2017) and its earlier versions are referred to as 'specific TF-CBT' throughout this dissertation.

2.2. Specific TF-CBT in its essence

Specific TF-CBT can be grouped in three treatment phases: (1) stabilization and skill building, (2) trauma narration and processing and (3) consolidation and closure (Cohen et al., 2017, 2006). The first phase educates about the impact and treatment of traumas as well as teaches skills to cope with trauma reminders and stress. The second phase addresses the traumatic memories by gradually evoking the details of the experiences and discussing them, usually through a written trauma narrative. The third phase focuses on embedding and cementing the acquired knowledge and skills in real life and concentrates on the parent-child communication and the development of safety skills. These three treatment phases can be broken down further to a sequence of nine components that produce the acronym PRACTICE. The stabilization and skill building phase contains the components (P)

psychoeducation and parenting skills, (R) relaxation, (A) affective modulation and (C) cognitive coping; the trauma narration and processing phase comprises only the component of the same name, (T) trauma narration and processing; the consolidation and closure phase includes the components (I) in vivo exposure, (C) conjoint parent-child sessions and (E) enhancing safety and development (Cohen et al., 2017, 2006). According to the authors, at least eight sessions are necessary to cover all PRACTICE components.

Next to the session-by-session component-based approach, specific TF-CBT is characterized by integrating caregivers into the treatment. Caregivers fulfill a central role in specific TF-CBT as half the time of the sessions is allotted to them. Normally, the therapist works on parallel components with the children and adolescents and their caregivers. Additionally, at a later stage of treatment, conjoint sessions are used to share the trauma narrative, integrate skills and enhance the caregiver-child communication. In this way, caregivers can also learn skills to cope with their own trauma-related stress and depressive symptoms as well as model and practice the acquired skills with the children and adolescents between the sessions (Cohen et al., 2017, 2006).

Another characteristic of specific TF-CBT is its adaptiveness and flexibility. This includes the integration of additional elements in the existing treatment protocol such as culture-specific aspects (e.g. metaphors, songs, jargon, religious beliefs and games; McMullen, O'Callaghan, Shannon, Black, & Eakin, 2013; Murray et al., 2013; Murray et al., 2015; O'Callaghan, McMullen, Shannon, & Rafferty, 2015; O'Callaghan, McMullen, Shannon, Rafferty, & Black, 2013; Wang et al., 2016). Additionally, treatment application can be varied to fit the setting (e.g. group formats, inclusion of interpreters, delivery in schools, inpatient units or via video connection; Cabrera et al., 2020; Cohen et al., 2017, 2006; Schottelkorb,

Doumas, & Garcia, 2012; Stewart et al., 2020; Unterhitzberger et al., 2019) and treatment length, session length and session frequency can be adjusted accordingly. Moreover, when the traumatic event involved the death of a loved one, the manual offers four additional grief components for the treatment of traumatic grief that can be used once the PRACTICE components are completed to foster transition to a healthier grief response.

2.3. The evidence base of specific TF-CBT and the current state of research

In the pediatric literature, many authors refer to the systematic review by Cary and McMillen (2012) when detailing the evidence for specific TF-CBT. Cary and McMillen (2012) analyzed specific TF-CBT and studies that did not include all but at least 4-5 of the most relevant treatment components, separately. With regard to specific TF-CBT, small to medium ES were found in its favor compared to active non-CBT control conditions for PTSS and depression at post-treatment. At 12-month follow-up, this effect was maintained for PTSS but not for depression. However, the results should be interpreted with caution as only three RCTs were included in the analyses. This was due to the exclusion of CBT control conditions and limited literature available at the time. A few years later, Gutermann et al. (2016) analyzed 18 studies on specific TF-CBT, reporting large uncontrolled ES for PTSS from pre- to post-treatment. However, specific TF-CBT was not their main focus of analysis. Thus, no between-group effects or other outcomes were explored. Nevertheless, it was the first meta-analysis on specific TF-CBT that was not limited to RCTs only. A more recent meta-analysis of Bastien, Jongma, Kabadayi, and Billings (2020) compared specific TF-CBT to any available control condition at post-treatment. Regarding PTSS, they found a medium ES for TF-CBT. Notably, a shorter inclusion period from 2011-2019 was defined and some studies were missed that would have met the inclusion criteria. Thus, the analysis was not fully representative of the

literature. Yet another general issue is the use of a multitude of instruments to assess the same constructs, which could potentially influence the meta-analytic results, particularly in the context of adapted construct definitions over time (Higgins et al., 2022; Murad, Wang, Chu, & Lin, 2019; Puhan, Soesilo, Guyatt, & Schünemann, 2006). Additionally, some older categorical instruments may have disguised true ES.

Apart from these analyses on specific TF-CBT, several meta-analyses exist that also included other similar trauma-focused CBT treatments such as EMDR (Shapiro, 2018), CBITS (Jaycox, 2018), KIDNET (Neuner et al., 2008), PE (Foa, Hembree, Rothbaum, & Rauch, 2019) and CPT (Resick, Monson, & Chard, 2017) among several others (Hoogsteder, Thije, Schippers, & Stams, 2021; Lenz & Hollenbaugh, 2015; Mavranouzouli et al., 2020; Morina, Koerssen, & Pollet, 2016). In sum, they showed large ES for PTSS compared to wait-list conditions and small to medium ES against active treatments. With regard to depression, effects were small to medium compared to wait-list and small against active treatments. It should be mentioned that some of these analyses have to be treated with caution due to limitations such as the exclusion of intention-to-treat analyses (Lenz & Hollenbaugh, 2015) and collapsed outcomes (Hoogsteder et al., 2021).

Regarding long-term effects on PTSS, results of these meta-analyses were inconclusive. One of the meta-analyses showed that the large post-treatment effect on PTSS compared to wait-list increased at 1-4 month follow-up (Mavranouzouli et al., 2020), while another found that the small post-treatment effect on PTSS against active treatments disappeared at 3-24-month follow-up (Morina et al., 2016). However, a direct comparison is difficult to make, due to the different time frames and control conditions. Long-term effects on depression were only analyzed by Morina et al. (2016) and trauma-focused CBT treatments could only be

compared to active control conditions (i.e. not active treatments) including psychoeducation, supportive counselling etc. The medium ES for these control condition found at post-treatment was sustained at 3-24-month follow-up.

Neither meta-analyses that focused solely on specific TF-CBT nor meta-analyses including other trauma-focused CBT treatment considered concordance between children's self-reported and caregiver-reported outcomes. To this date, only two meta-analyses have attempted to investigate concordance between these reports, generally (Achenbach, McConaughy, & Howell, 1987; Los Reyes et al., 2015). Both analyses point to low to moderate concordance between reports. Interestingly, several individual studies using samples with physical illnesses (e.g. cancer) detected significant moderate to high correlations for self-reported and caregiver-reported symptoms as well as no mean difference between youth and their caregivers (Clawson, Jurbergs, Lindwall, & Phipps, 2013; Erickson, Krapf, & Gerstle, 2017; Phipps, Long, Hudson, & Rai, 2005; Stevanovic, Jancic, Topalovic, & Tadic, 2012). This may be due to a greater awareness and involvement of caregivers in their youth's health and treatment. In specific TF-CBT, this effect may as well be present as caregiver sessions are an integral treatment component, facilitating awareness and caregiver involvement.

Next to the favorable meta-analytic results for specific TF-CBT discussed above, several RCTs on specific TF-CBT also consider group settings and other clinical outcomes such as anxiety and grief as well as caregiver-reported outcomes that have not been investigated by meta-analyses yet. Moreover, the amount of effectiveness studies is increasing with many trials currently under way, warranting a meta-analytic examination of how well specific TF-CBT performs under 'real-life' conditions (e.g. Espeleta, Peer, Are, & Hanson, 2021; Rosner, Barke, et al., 2020; Rosner, Sachser, et al., 2020).

2.4. Dissemination and implementation

International guidelines usually orientate on the evidence-base provided by research to decide which treatments are the most effective and will likely achieve the best patient outcomes (Spring, 2007). However, effectiveness trials often struggle to implement these treatments in ‘real-world’ settings and several factors that can facilitate or hinder the transfer of evidence-based treatments into practice have been identified (Shafran et al., 2009). At the level of practitioners’ characteristics, one such factor that has gained recognition is their attitudes toward evidence-based practice (EBP; Aarons et al., 2009; Moullin et al., 2019). These comprise positive attitudes (e.g. EBP appeal) as well as conflicting attitudes (e.g. balance between science and clinical skill) toward EBP (Aarons, 2004; Aarons et al., 2009; Rye et al., 2017). Not only can these attitudes influence whether treatments are actually initiated but also how they are implemented and whether they are used beyond the first implementation period. Thus, these attitudes are important to consider when transferring evidence-based treatments into practice.

In reaction to the need of effective trauma-focused treatments for children and adolescents, several effectiveness studies and initiatives disseminated and implemented specific TF-CBT in different countries (e.g. Amaya-Jackson et al., 2018; Dorsey et al., 2020; Goldbeck et al., 2016; Jensen et al., 2014). In the US, comprehensive implementation approaches were developed to enhance implementation on a large scale targeting multiple agency levels (e.g. therapists, supervisors, senior leaders) as well as professionals from key service organizations (e.g. schools, child welfare; Hanson et al., 2019). Despite these enormous efforts, difficulties in consistent delivery of some components of specific TF-CBT persist (Ascienzo, Sprang, & Eslinger, 2020). In addition, some studies reported difficulties with

treatment initiation, treatment completion as well as therapist dropout (Amaya-Jackson et al., 2018; Cohen et al., 2016), all factors that are likely to be influenced by attitudes toward EBP. Therefore, evaluating therapists' attitudes toward EBP and how these may change over the course of training and treatment delivery is an important aspect that is usually considered in large scale effectiveness trials of specific TF-CBT. While several instruments to measure attitudes toward EBP in practitioners exist, currently running effectiveness trials in Germany are missing a validated translation of such an instrument (Kien, Schultes, Szlag, Schoberberger, & Gartlehner, 2018).

2.5. The EBPAS-36

The Evidence-Based Practice Attitude Scale (EBPAS) is a psychometrically robust instrument to assess mental health care providers' positive as well as conflicting attitudes toward EBP (Aarons, 2004; Aarons et al., 2010). It has been developed in a joint effort of practitioners and researchers (Aarons, 2004; Aarons et al., 2012) and in accordance with mental health dissemination and implementation theories (Glisson, 2002; Schoenwald & Hoagwood, 2001; Simpson, 2002). Originally, this resulted in a psychometrically strong 15-item version (EBPAS-15) covering four domains. Next to the initial US study, the EBPAS-15 also demonstrated high validity in different settings and samples from several European countries (Aarons et al., 2010; Egeland, Ruud, Ogden, Lindstrøm, & Heiervang, 2016; Melas, Zampetakis, Dimopoulou, & Moustakis, 2012; van Sonsbeek et al., 2015). However, to include other important dimensions, the EPBAS-15 was first broadened to include 50 items and 8 additional dimensions (EBPAS-50; Aarons et al., 2012) but then again shortened to 36 items to reduce lengthiness while maintaining all dimensions (EBPAS-36; Rye et al., 2017). The 12 dimensions refer to positive as well as conflicting attitudes toward EBP and can be grouped

accordingly. Namely, the domains associated with positive attitudes are (1) the intuitive *Appeal* of EBP, (2) the likelihood to adopt EBP given the *Requirement*, (3) providers' *Openness* to new practices, (4) the *Fit* of EBP with values and needs of client and clinician, (5) the *Job Security* linked to competence in EBP (6) perceived *Organizational Support* and (7) perception of *Feedback*. In contrast, the domains linked to conflicting attitudes relate to (8) the perceived *Divergence* from routine practices, (9) *Limitations* of EBPs in addressing client needs, (10) negative perceptions of *Monitoring*, (11) the *Balance* between clinical skill and science, and (12) the *Burden* of learning EBP (Aarons et al., 2012; Rye et al., 2017). While the EBPAS-36 also demonstrated good psychometric properties in US and Norwegian samples, a validated German version is missing still.

3. Description of the dissertation plan

3.1. Relevance and derivation of the research questions

The previous chapters gave a brief overview of the emergence of specific TF-CBT, its content and evidence-base, related dissemination and implementation efforts as well as a brief description of the EBPAS-36, while also pointing out some aspects that need further research attention. Ten years have passed since the last review on specific TF-CBT and many RCTs and uncontrolled studies on specific TF-CBT have been published since then, including aspects that have not yet been fully summarized by reviews and meta-analyses. Moreover, challenges in implementation related to practitioners' attitudes toward EBP persist but a validated German instrument to measure these attitudes is missing. This dissertation addresses several of these aspects focusing on a fourfold objective.

The first objective of this dissertation is to compile and quantify all the available evidence on specific TF-CBT. To this end, treatment effects on PTSS and secondary outcomes of depression, anxiety, and grief are considered from pre- to post-treatment as well as in comparison to control conditions. Subsequently, additional attention will be directed to the different designs, comparators, and settings. In this way, the full extent of the current specific TF-CBT literature will be summarized and a clearer picture will be drawn regarding its treatment effects, taking individual and group settings as well as in efficacy and effectiveness studies into account. Moreover, we will differentiate between wait-list and active treatment and treatment as usual (AT/TAU) controls to investigate how well specific TF-CBT performs compared to them, respectively.

Building on this work, the second objective is to investigate long-term effects on the same outcomes also considering the same designs, comparators, and settings. In addition,

caregiver-reported effects and their concordance with self-reports will be investigated. Thus, in light of the scarce and inconclusive meta-analytic results, this approach will delineate long-term effects by looking at 3-month follow-up, 6-month follow-up and 12-month follow-up separately with the latter as the primary end-point. Additionally, it will shed some light on the caregiver-reported treatment effects in specific TF-CBT and how well these caregiver reports concur with the self-reported symptoms of their children and adolescents.

The third objective is to develop and validate a German translation of the EBPAS-36 measuring practitioners' attitudes toward EBP. There is a great need for such an instrument as there are currently considerable efforts made to disseminate and implement specific TF-CBT and other evidence-based treatments in Germany, using private practitioners to deliver treatments. Therefore, a validated translation will enable evaluation of their attitudes toward EBP as well as how these may change over the course of training and treatment delivery. This may also help to explain difficulties with the transfer of specific TF-CBT and other evidence-based treatments into practice.

At last, the fourth objective is to address the multitude of instruments being used in specific TF-CBT studies to assess PTSS, depression, anxiety and grief by analyzing the measures separately. Particularly, some criticism exist regarding the use of older (categorical) instruments that may have disguised ES in earlier specific TF-CBT studies, namely the Schedule for Affective Disorders and Schizophrenia for School-Age Children (K-SADS; Kaufman et al., 1997; Orvaschel, Puig-Antich, Chambers, Tabrizi, & Johnson, 1982) and the Screen for Child Anxiety Related Emotional Disorders (SCARED, Birmaher et al., 1997; Birmaher et al., 1999). In order to provide answers to the first objective, the analyses will comprise many different studies using different instruments to assess PTSS and secondary

outcomes. Therefore, sub-group analyses will be used to investigate ES for all instruments separately. This should give some insight to which extent ES differ by instrument and whether older measures are indeed disguising ES.

3.2. Research questions of the dissertation

In view of the objectives described above, the following research questions can be derived:

- 1) Does specific TF-CBT significantly improve PTSS, depression, anxiety and grief in children and adolescents? How strong are these effects and are they different for individual and group settings and efficacy and effectiveness studies? How well does specific TF-CBT perform against wait-list and AT/TAU? These research questions were addressed by a systematic review and meta-analysis (publication 1).
- 2) Does specific TF-CBT significantly improve PTSS, depression, anxiety and grief in children and adolescents also at 12-month follow-up? How strong are these effects and are they different for individual and group settings and efficacy and effectiveness studies? How well does specific TF-CBT perform against wait-list and AT/TAU at 12-month follow-up? Does specific TF-CBT also significantly improve caregiver-reported PTSS and secondary outcomes at post-treatment and at 12-month follow-up? How well do caregiver-reports concur with self-reports? These research questions were addressed by a second meta-analysis (publication 2).
- 3) Does the EBPAS-36D have sound psychometric properties? Do the individual items perform satisfactory? Is the translation internally consistent? Does the scale show convergent validity? Does it capture the intended construct? These research questions were addressed by a validation study among licensed private practitioners and practitioners in training to become licensed (publication 3).

4) Are the pooled ES of publication 1 influenced by the instruments used to assess PTSS, depression, anxiety and grief? Is the criticism that the K-SADS and SCARED disguise true ES in specific TF-CBT studies justified? These research questions will be addressed by additional content provided in the following chapters.

4. Synopsis of the publications and additional content

4.1. Publication 1: A systematic review and meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents

Thielemann, J.F.B., Kasparik, B., König, J., Unterhitzenberger, J., & Rosner, R. (2022). A systematic review and meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents. *Child Abuse & Neglect*, 134. doi: <https://doi.org/10.1016/j.chiabu.2022.105899>

Background: Children and adolescents are often exposed to traumatic experiences and PTSS are a typical consequence. Subsequently, some of them also go on to develop full-blown PTSD, symptoms of depression, anxiety and grief as well as related comorbid diagnoses. Many of these children and adolescent require trauma-focused treatment. A previous systematic review positively evaluated specific TF-CBT but it has been published 10 years ago. Since then, the literature has grown substantially and an update is required.

Methods: The study was pre-registered with PROSPERO and conducted in line with the PRISMA guidelines. Pre-defined combinations of search terms were entered in seven databases (PsychInfo, MEDLINE, Cochrane Library, PTSDPubs, PubMed, Web of Science, and OpenGrey) to search for studies published between 1990-2021. All titles and abstracts were screened by the first and second author and conflicting ratings were resolved in discussion. Final eligibility was assessed through full-text reading by the first author.

Ambiguous cases were resolved by contacting corresponding authors and in discussion with the co-authors. Risk of bias was assessed by the first and second author.

Results: In total, k = 28 RCTs and k = 33 uncontrolled studies comprising N = 4523 participants were included. For specific TF-CBT, large ES from pre-treatment to post-treatment were detected for PTSS and grief (PTSS: g = 1.14, 95% CI 0.97 - 1.30; grief: g = 1.35,

95% CI 1.21 - 1.48) as well as medium ES for depression and anxiety (depression: $g = 0.63$, 95% CI 0.51 - 0.76; anxiety $g = 0.56$, 95% CI 0.43 - 0.69). Results were also favorable for specific TF-CBT compared to any control condition (PTSS: $g = 0.52$, 95% CI 0.31 - 0.73; depression: $g = 0.40$, 95% CI 0.27 - 0.52; anxiety: $g = 0.26$, 95% CI 0.13 - 0.29). Effects were greater compared to wait-list than AT/TAU but effects remained significant across outcomes. Moreover, effects on PTSS were more pronounced for group settings and effectiveness trials. ES are adjusted for risk of bias and publication bias, initially limiting the data quality.

Discussion: Specific TF-CBT can effectively address pediatric PTSS as well as symptoms of depression, anxiety and grief. It is superior to wait-list as well as AT/TAU, supporting its recommendation as a first-line treatment by international guidelines. The counterintuitive greater ES in effectiveness trials can best be explained by its heterogenous composition of control groups including more often wait-list comparators and group settings for which the effects were greater. In contrast, efficacy trials included exclusively AT/TAU conditions as comparator and one group RCT only. The greater effect in group studies may be partly explained by its application mostly to underserved population, often in low and middle income countries with higher baseline symptomatology. Nevertheless, the stronger ES in group settings can not fully be attributed to this phenomenon and a specific group factor might be at play supporting trauma-focused work and healing in young people.

4.2. Publication 2: Stability of treatment effects and caregiver-reported outcomes: A meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents

Thielemann, J.F.B., Kasparik, B., König, J., Unterhitzberger, J., & Rosner, R. (2022). Stability of treatment effects and caregiver-reported outcomes: A meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents. *Child Maltreatment*. doi: <https://doi.org/10.1177/10775595231167383>

Background: The effectiveness of specific TF-CBT has been previously confirmed and recently corroborated (publication 1). However, little is known about the long-term treatment effects as well as the concordance between self- and caregiver-reports in specific TF-CBT. While effective treatment is important, positive effects need to be sustained over time to achieve the best outcome for patients as well as the healthcare system. In addition, the concordance of caregiver-reports with self-reports needs to be investigated to evaluate whether caregivers arrive at the same conclusions and to assess how adequately caregiver-reports can capture the symptoms of their children and adolescents.

Methods: The dataset was derived from publication 1 but an additional eligibility criterion was added. Namely, studies had to include at least one follow-up assessment of PTSS, depression, anxiety or grief or had to include a caregiver-reported outcome measure for PTSS, depression, anxiety or grief assessed at least pre and post-treatment.

Results: For specific TF-CBT, large ES were found across all outcomes from pre-treatment to 12-month follow-up (PTSS: $g = 1.71$, 95% CI 1.27 - 2.15; depression: $g = 0.99$, 95% CI 0.70 - 1.29; anxiety: $g = 0.95$, 95% CI 0.55 - 1.35; grief: $g = 1.44$, 95% CI 0.94 - 1.94). Intermediate follow-up assessment points were mostly consistent with this result with further gains observed between 6 and 12-month follow-up for PTSS. Results were also in favor of specific TF-CBT against AT/TAU at 12 month follow-up for PTSS ($g = 0.35$, 95% CI 0.13 - 0.56) but no significant difference were found for secondary outcomes. Again, greater effects on PTSS were found in group settings and effectiveness trials. Furthermore, no significant differences were found between self- and caregiver-reports with high reliability across almost all assessment points and outcomes, increasing further over time. In accordance with publication 1, the pooled estimates are adjusted for risk of bias and publication bias.

Discussion: One year after treatment, positive uncontrolled effects of specific TF-CBT on pediatric PTSS and secondary outcomes of depression, anxiety and grief could not only be sustained but additional gains were observed across outcomes. For PTSS, the effect increased further between 6 and 12-month follow-up, supporting the argument that patients continue to improve after treatment has ended. Specific TF-CBT also showed superior effects on PTSS compared to AT/TAU at 12-month follow-up. However, concerning secondary outcomes, post-treatment advantages over AT/TAU disappeared at 12-month follow-up. While the studies were drawn from the same study pool as publication 1, the number of studies with follow-up assessments of secondary outcomes and a control group was substantially lower, making it more difficult to find significant effects. Yet another explanation could be that in the long run, other treatments may be equally effective in addressing secondary outcomes as specific TF-CBT mainly addresses PTSS. Thus, smaller effects can be expected. Again, the greater effects on PTSS in group settings and effectiveness trials can be best explained by the heterogenous composition of control groups in effectiveness trials and the greater baseline symptomatology found in underserved populations that group settings have mainly been applied to (see publication 1). However, a specific group factor supporting trauma-focused work and healing in young people may also positively influence specific TF-CBT in group settings. Caregiver-reports paralleled longitudinal and cross-sectional analyses of self-reports for all outcomes. Contrary to earlier meta-analyses, no significant differences between assessors were found and reliability was high across assessment points and outcomes. This could be due to greater caregiver awareness, greater caregiver distress and problem attribution or symptom exacerbation to emphasize treatment needs. However, as reports converge over time, this suggests that caregiver ratings become more accurate. Through their participation

in treatment, caregivers may gain awareness and understanding of PTSS.

4.3. Publication 3: Cross-cultural adaption and psychometric investigation of the German version of the Evidence Based Practice Attitude Scale (EBPAS-36D)

Szota, K., **Thielemann, J.F.B.**, Christiansen, H., Rye, M., Aarons, G.A., & Barke, A. (2021).
Cross-cultural adaption and psychometric investigation of the German version of the
Evidence Based Practice Attitude Scale (EBPAS-36D). *Health Research Policy and Systems*
19(1), 90. doi: <https://doi.org/10.1186/s12961-021-00736-8>

Background: EBP implementation in mental health care has many benefits to patients but the transfer of EBP to ‘real-world’ settings is often difficult. Therefore, research is needed to investigate factors that can facilitate successful implementation. One such factor that has been identified is service providers’ attitudes toward EBP. The EBPAS-36 is a measure with good psychometric properties that assesses positive as well as conflicting attitudes toward EBP. However, a German version is missing still.

Methods: The EBPAS-36D was developed in accordance with WHO recommendations. German psychotherapists completed the EBPAS-36D, the Implementation Climate Scale (ICS), the Intention Scale for Providers (ISP) and a global assessment of interest in EBP via an online survey. Next to standard item and reliability analyses, an exploratory (EFA) as well as a confirmatory factor analysis (CFA) was conducted in two subsamples (random split) to assess construct validity. For the EBPAS-36D, a high positive correlation with the direct and indirect scores of attitudes from the ISP as well as with the interest in EBP score from the global assessment was predicted to evaluate convergent validity. Additionally, the ability of the EBPAS-36D to predict the intention to use EBP was investigated.

Results: A total of $N = 599$ psychotherapists were included in the study. The mean item difficulty was $p_i = .64$, the mean inter-item correlation $r = .18$, and the mean item-total correlation $r_{itc} = .40$. Internal consistency was high for the total scale ($\alpha = .89$) and adequate to very good for the subscales ($\alpha = .65-.89$). An acceptable model fit was found for the original 12-factor structure of the EBPAS-36 (RMSEA = .064 (90% CI = .059-.068); SRMR = .0922; AIC = 1400.77). However, the EFA revealed a second-order factor structure with an even better model fit (RMSEA = .057 (90% CI = .052-.062); SRMR = .0822; AIC = 1274.56). When included in a hierarchical regression model, the EBPAS-36D predicted intention to use EBP (Change in $R^2 = .28$, $p < .001$) subsequent to gender, age and participants' report of ever having worked in science.

Discussion: The original factor structure of the EBPAS-36 was confirmed in the German version and the scale showed good item characteristics, internal consistency and convergent validity. The use of the EBPAS-36D in implementation research in German-speaking countries might advise strategies to increase providers' readiness to implement EBP. In clinical practice, the EBPAS-36D could help to indicate relevant areas that need further attention in training and supervision. The suggested second-order model may stimulate further research on the construct of attitudes toward EBP.

4.4. Additional content: Investigation of instrument-related effect sizes in trauma-focused cognitive behavioral therapy for children and adolescents

Thielemann, J.F.B. (2022). Meta-analytic investigation of instrument-related effect sizes in trauma-focused cognitive behavioral therapy for children and adolescents. *Unpublished*.

Background: Meta-analyses often rely on a multitude of instruments to assess the same constructs due to the variety of measures being used by individual studies. This may have an influence on ES, especially when construct definitions change over time. In new editions and

revisions of the ICD and DSM, changes are made to diagnostic criteria and instruments have to be newly developed or updated, accordingly. In specific TF-CBT, the degree to which ES are influenced by outcome measures has not yet been investigated by meta-analysis.

Methods: The complete dataset from publication 1 was reanalyzed but additional coding by outcome measure was done. Outcome measures were analyzed separately when at least three independent ES were available for meta-analysis. Accordingly, four instruments could be coded for PTSS, three for depression, two for anxiety and two for grief.

Results: Within-group pre-post ES analyzed by instruments were highly similar across outcomes (PTSS: $g = 1.17-1.45$; depression: $g = 0.53-0.79$; anxiety: $g = 0.49-0.59$; grief: $g = 1.25-1.42$). Between-group analyses at post-treatment revealed a greater ES variety by instrument in PTSS, ranging from a small non-significant effect (CAPS-CA: $g = 0.20$) to a large significant effect (UCLA: $g = 1.12$) but not in secondary outcomes (anxiety: $g = 0.28-0.35$). For depression and grief, instrument-specific between-group analyses were only possible for one instrument, respectively. However, the ES were close to the overall ES of the respective outcomes. The K-SADS and the SCARED showed ES in the mid-range close to the overall ES in both, the pre-post analysis (K-SADS: $g = 1.28$; SCARED: $g = 0.59$) as well as the between-group analysis at post-treatment (K-SADS: $g = 0.56$; SCARED: $g = 0.35$). In accordance with publication 1 and 2, the pooled estimates are adjusted for risk of bias and publication bias.

Table 1*Pre-post within-group effect sizes for PTSS, depression, anxiety and grief by instrument.*

Outcome	Instrument	n	g	95% CI	SE	z	Q	I ²	Duval and Tweedie
PTSS	All instruments	42	(1.28) 1.17 ^a	(1.10-1.44) 1.00-1.34	(.09)	(14.65 ^{***})	341.00 ^{***}	87.98	5
	CAPS-CA	4	(1.46) 1.28 ^a	(0.96-1.97) 0.78-1.78	(.26)	(5.69 ^{***})	11.34 [*]	73.54	1
	CPSS	10	1.45	1.14-1.77	.16	9.14 ^{***}	47.87 ^{***}	81.20	0
	K-SADS	8	(1.37) 1.28 ^a	(0.88-1.85) 0.82-1.73	(.25)	(5.54 ^{***})	122.71 ^{***}	94.39	1
	UCLA	18	1.18	0.97-1.39	.11	10.96 ^{***}	72.86 ^{***}	76.67	0
Depression	All instruments	28	0.63	0.51-0.76	.06	9.94 ^{***}	87.97 ^{***}	69.31	0
	CDI	15	0.53 ^b	0.45-0.62	.06	9.28 ^{***}	22.72	38.38	0
	MFQ	4	0.79	0.26-1.33	.27	2.90 ^{**}	21.00 ^{***}	85.72	0
	SMFQ	4	0.72	0.26-1.18	.23	3.08 ^{**}	16.98 ^{**}	82.33	0
Anxiety	All instruments	18	0.56	0.43-0.69	.07	8.17 ^{****}	32.92 [*]	48.35	0
	SCARED	10	0.59 ^b	0.48-0.71	.06	9.94 ^{***}	10.63	15.31	0
	STAIC	4	0.49	0.19-0.79	.15	3.18 ^{**}	8.15 [*]	63.17	0
Grief	All instruments	8	(1.37) ^b 1.35 ^{a,b}	(1.23 - 1.51) 1.21- 1.48	(.07)	(19.21 ^{***})	13.05	46.34	1
	EGI-TG	3	1.25	0.54-1.96	.36	3.44 ^{**}	6.79 [*]	70.52	0
	ICG	4	1.42 ^b	1.25-1.58	.09	16.38 ^{***}	2.18	0	0

Note. High risk of bias studies are excluded; n = number of included TF-CBT conditions; PTSS = Posttraumatic Stress Symptoms; CAPS-

CA = Clinician-Administered PTSD Scale for Children and Adolescents; CPSS = Child PTSD Symptom Scale; K-SADS = K-SADS =

Schedule for Affective Disorders and Schizophrenia for School-Age Children; UCLA = UCLA Posttraumatic Stress Disorder-Reaction

Index; CDI = Children's Depression Inventory; (S)MFQ = Mood and Feelings Questionnaire, (Short Version); SCARED = Screen for Child Anxiety Related Emotional Disorders; STAIC-T = State-Trait Anxiety Inventory for Children, Trait subscale; EGI-TG = Extended Grief Inventory-Traumatic Grief Subscale; ICG = Inventory of Complicated Grief; * $p < .05$; ** $p < .01$; *** $p < .001$.

^aRecalculated with imputed studies (trim and fill method according to Duval and Tweedie).

^bFixed model assumed due to non-significant Q-value.

Table 2*Post-treatment between-group effect sizes for PTSS, depression, anxiety and grief by instrument.*

Outcome	Instrument	n	g	95% CI	SE	z	Q	I ²	Duval and Tweedie
PTSS	All instruments	22	0.52	0.31-0.73	.11	4.83***	98.46***	78.67	0
	CAPS-CA	4	0.20 ^a	-0.02-0.42	.11	1.81	4.37	31.42	0
	CPSS	7	0.39	0.09-0.69	.15	2.57**	24.47***	75.48	0
	K-SADS	4	0.56 ^a	0.34-0.78	.11	4.94***	0.37	0	0
	UCLA	6	1.12	0.39-1.85	.37	2.99**	43.90***	88.61	0
Depression	All instruments	14	0.40 ^a	0.27-0.52	.06	6.14***	18.80	30.83	0
	CDI	9	0.37 ^a	0.22-0.52	.08	4.93***	6.79	0	0
Anxiety	All instruments	12	0.26 ^a	0.13-0.39	.07	3.82***	14.28	22.95	0
	SCARED	4	0.35 ^a	0.15-0.55	.10	3.48**	2.88	0	0
	STAIC-T	4	0.28 ^a	0.07-0.49	.11	2.63**	0.84	0	0
Grief	All instruments	5	0.30	-0.06-0.67	.19	1.63	19.58**	79.57	0
	ICG	4	0.33	-0.07-0.73	.20	1.63	19.25***	84.41	0

Note. High risk of bias studies are excluded; n = number of included comparisons; PTSS = Posttraumatic Stress Symptoms; CAPS-CA

= Clinician-Administered PTSD Scale for Children and Adolescents; CPSS = Child PTSD Symptom Scale; K-SADS = K-SADS =

Schedule for Affective Disorders and Schizophrenia for School-Age Children; UCLA = UCLA Posttraumatic Stress Disorder-Reaction

Index; CDI = Children's Depression Inventory; STAIC-T = State-Trait Anxiety Inventory for Children, Trait subscale; ICG =

Inventory of Complicated Grief; * $p < .05$; ** $p < .01$; *** $p < .001$.

^a Fixed model assumed due to non-significant Q-value.

Discussion: In specific TF-CBT, pre-post ES for PTSS, depression, anxiety and grief seem unaffected by individual outcome measures. However, in between-group analyses, PTSS ES ranged from small to large according to outcome measure. This difference can best be explained by the different control conditions. While three of four ES of the CAPS-CA were stemming from studies that compared specific TF-CBT against AT/TAU, five of six ES of the UCLA were derived from studies that used wait-list comparisons. Thus, the great difference in ES is not surprising considering that AT/TAU conditions can include powerful interventions that sometimes have effects similar to the intervention in question. Interestingly, three of four studies using the CAPS-CA were conducted in an individual setting for which ES appeared to be smaller in publication 1 while five of six studies using the UCLA were conducted in a group setting for which greater ES were found in publication 1. Thus, theoretically speaking, this could also suggest that the greater ES in groups could be explained by the instrument being used to assess PTSS. However, this seems unlikely, given that the uncontrolled ES for the CAPS-CA was very similar to the ES for the UCLA. Therefore, the post-treatment difference can best be attributed to the different control groups used by the studies. With regard to the K-SADS and the SCARED, ES did appear in the mid-range of ES found by other instruments. Thus, it is unlikely that they disguised ES in meta-analyses. However, newer instruments that assess symptom clusters in full accordance with current diagnostic manuals should be used in future studies to assure there is no divergence from the construct being assessed.

5. Discussion and conclusion

5.1 General discussion

This dissertation aimed at giving an update on specific TF-CBT as well as addressing the need of a validated German version of the EBPAS-36. Four main research questions were explored through three publications and one unpublished report.

The first research question (publication 1) was directed at the evaluation of meta-analytic effects of specific TF-CBT on PTSS, depression, anxiety and grief from pre- to post-treatment as well as in comparison to control conditions at post-treatment. More specifically, pooled ES were investigated for individual and group settings, for efficacy and effectiveness studies as well as for wait-list and AT/TAU, separately. This extended focus offered new insights into these areas of specific TF-CBT that have not yet been investigated by meta-analyses. While large overall uncontrolled effects and medium controlled effects on PTSS were in line with previous analyses, the most striking result was that these effects were greater in effectiveness studies and group settings, respectively.

Usually, the transfer of treatment effects found in efficacy studies into practice is assumed to be difficult (Singal, Higgins, & Waljee, 2014). The most likely explanation for the larger controlled effects in effectiveness studies rather than efficacy studies is the use of different comparators. Naturally, effects on PTSS were greater compared to wait-list conditions than against AT/TAU conditions, as the latter often contains powerful interventions (Frost, Laska, & Wampold, 2014). Efficacy studies more often use AT/TAU control conditions and are usually conducted in individual settings. In contrast, effectiveness studies vary more often with regard to comparator and setting. Consequently, all efficacy trials in publication 1 included AT/TAU conditions only and solely one RCT was conducted in a

group setting. On the contrary, effectiveness trials included a mixture of both types of control conditions and settings. However, within group settings, the greater controlled effect on PTSS could not be attributed to the control conditions as wait-list and AT/TAU controls were evenly distributed and this effect was also apparent in the uncontrolled analysis. Thus, this larger effect in group settings as well as the larger effects found in comparison to wait-list conditions may explain the greater effects on PTSS found in effectiveness studies.

Concerning the greater uncontrolled and controlled effects on PTSS in group settings rather than individual settings, they can be partly explained by the different target populations and the specific context. Most of the specific TF-CBT group studies included in publication 1 were conducted in low and middle income countries. Considering the lack of mental health care and the high baseline symptomatology that is often inherent to this setting, greater effects can be expected when treatment is offered. Moreover, necessary infrastructures were often missing, requiring pragmatic solutions that do not concur with the requirements of efficacy trials. As a consequence, all but one group RCT were identified as effectiveness studies. However, the context can only partially explain the greater effects on PTSS in group settings as a significant proportion of group studies was conducted in high income countries (36% uncontrolled ES, 43% controlled ES). This result contrasts earlier studies that found smaller effects for group interventions in children, adolescents and adults (Gutermann et al., 2016; Lewis, Roberts, Andrew, Starling, & Bisson, 2020). Thus in specific TF-CBT groups, a specific group factor such as a sense of community may be present that facilitates trauma-focused work and consequent healing in children and adolescents.

Regarding secondary outcomes of depression and anxiety, medium uncontrolled and small controlled effects were found across analyses. These were in line with earlier findings

suggesting specific TF-CBT can successfully alleviate these symptoms although it does not target them directly (Cohen, Deblinger, Mannarino, & Steer, 2004; Deblinger, Mannarino, Cohen, & Steer, 2006). In contrast to PTSS, no differences concerning design, setting or comparators were apparent for these outcomes. However, large uncontrolled effects on grief indicated that specific TF-CBT can also address these symptoms effectively. Nonetheless, it should be mentioned that all studies that assessed grief symptoms were either undertaken in a group setting in low and middle income countries or by the working group of the treatment developers. In addition, a minimum time-criterion was missing in some studies. Thus, these results may be partly due to natural remission over time as participants with normal grief reactions may have been included. In line with that, only a small non-significant effect on grief was found in comparisons to AT/TAU, the only available comparator. Moreover, in one of the RCTs, the baseline symptomatology was not clinically significant leaving almost no room for improvement in either condition. Yet another explanation could be that other treatments were just equally effective in addressing these symptoms. Unfortunately, the interpretation is further complicated by the instruments used to assess grief. So far, no reliable instrument evaluating grief symptoms in children and adolescents has been established. Thus, the presented evidence should be treated with caution as the ES may have been clouded and the number of studies was limited to two RCTs and three uncontrolled studies.

In sum, specific TF-CBT is an effective treatment for pediatric PTSS and secondary outcomes. At post-treatment, it is also superior to control conditions with regard to PTSS and secondary outcomes of depression and anxiety, advocating for its extensive use all over the world and international guidelines recommending it as a first-line treatment. The presented findings encourage further dissemination with group settings offering a feasible cost-effective

and timesaving option in settings with limited resources. This is particularly beneficial in the aftermath of mass casualties affecting large groups of children and adolescents such as wars, natural disasters, terrorist attacks and Covid-19. Specific TF-CBT may also be applied to treat pediatric traumatic grief since the preliminary results are encouraging and the evidence in favor of other grief-related treatments is scarce.

The second research question (publication 2) targeted long-term effects of specific TF-CBT on PTSS, depression, anxiety and grief in children and adolescents from pre-treatment to 3-month, 6-month and 12-month follow-up and in comparison to control conditions at the respective follow-up assessment point. In line with publication 1, pooled ES were also computed for individual and group settings, for efficacy and effectiveness studies as well as for wait-list and AT/TAU at 12-month follow-up, separately. In addition, caregiver-reported effects as well as their concordance with self-reported symptoms were investigated. While some previous meta-analyses explored long-term controlled effects on PTSS, they were mostly limited to a few studies or had to collapse assessment points due to insufficient amounts of data (Cary & McMillen, 2012; Mavranouzouli et al., 2020; Morina et al., 2016). With one exception (Cary & McMillen, 2012), they were also not specific to specific TF-CBT, including other trauma-focused CBT treatments. In addition, pooled ES for caregiver-reported outcomes in specific TF-CBT were completely absent in the literature and also their concordance with self-reported outcomes has not been investigated in the context of specific TF-CBT by meta-analyses yet. Thus, publication 2 closed a gap in the literature by providing more comprehensive meta-analytic results.

The stability of treatment effects of specific TF-CBT were not only confirmed but the uncontrolled effects increased even further with analyses showing large uncontrolled effects

for all outcomes at 12-month follow-up. For PTSS, further gains were observed between 6 and 12-month follow-up. This supports the argument that participants continue to become better after treatment has ended (Tutus, Pfeiffer, Rosner, Sachser, & Goldbeck, 2017). Only one exception was found for anxiety with the uncontrolled effect decreasing at 3-month follow-up. However, this analysis relied on the minimum amount of three ES and included a high risk of bias study to enable analysis. All other assessment points were consistent, providing strong support for the long-term effects of specific TF-CBT.

In terms of controlled effects, a small effect on PTSS was found in favor of specific TF-CBT compared to AT/TAU conditions at 12-month follow-up which is in line with the only earlier meta-analysis specific to specific TF-CBT including follow-up assessment points (Cary & McMillen, 2012). In contrast to that, the only other meta-analysis on pediatric trauma-focused CBT interventions covering this assessment period found no significant difference to active treatments (Morina et al., 2016). In line with that, no significant difference was found at the intermediate 6-month follow-up assessment point. However, this assessment point included fewer comparisons and with one exception, included studies were conducted in individual settings, which also showed smaller effects at 12-month follow-up and in publication 1. Thus, the 6-month follow-up assessment was less robust and more homogenous in terms of treatment setting than the 12-month follow-up assessment. In addition, Morina et al. (2016) collapsed assessment points from 3-24 month and included other trauma-focused CBT treatments. Therefore, drawing a direct comparison is difficult. Considering the uncontrolled effect on PTSS, an explanation could be that at 6-month follow-up, specific TF-CBT may not have fully taken effect yet with TAU/AT conditions temporarily catching up.

Regarding the subgroup analyses at 12-month follow-up, the same pattern of results as in publication 1 was found. Effectiveness studies as well as studies conducted in group settings showed a larger effect on PTSS than efficacy and individual studies, respectively. As described before, this result can partly be explained by the characteristics of the included studies. However, they cannot fully account for the stronger effects found in group settings and a specific group factor facilitating trauma-focused work in specific TF-CBT might be at play. That this result was also found at 12-month follow-up underscores that specific TF-CBT can easily be transferred into practice and that group settings are as a cost-effective and timesaving option, especially when the available resources are limited.

Regarding controlled effects of secondary outcomes, all pooled ES of specific TF-CBT at 12-month follow-up and earlier follow-up assessment points were small and non-significant when compared to AT/TAU, the only available controls. Again, this result was in accordance with the only earlier meta-analysis specific to specific TF-CBT which also found a small non-significant controlled effect on depression at 12-month follow-up (Cary & McMillen, 2012) but in contrast to Morina et al. (2016) who found a medium effect on depression at 3-24 month follow-up in favor of trauma-focused CBT interventions. However, as previously mentioned, it is difficult to make a direct comparison between analyses as the follow-up periods varied greatly and other trauma-focused interventions than specific TF-CBT were included. Moreover, the comparison was made to active control conditions (i.e. psychoeducation, supportive counseling etc.), excluding active treatments and thus, greater ES can be expected. Nonetheless, the loss of all significant controlled effects for all secondary outcomes at all follow-up assessment points in publication 2 is surprising, considering the large uncontrolled effects and the medium controlled effects favoring specific TF-CBT at post-

treatment in publication 1. A possible reason could be that the study sample in publication 2 was different, although it was drawn from the same pool of studies as publication 1. The number of studies that was used for the controlled follow-up analyses was considerably lower due to the lesser amount of available data. Therefore, detecting significant effects was more difficult. Another possible explanation could be that the interventions in the AT/TAU conditions had a long-term effect on secondary outcomes similar to specific TF-CBT. Since specific TF-CBT mainly targets PTSS, smaller effects on secondary outcomes can be expected. However, an interesting observation was that the controlled effect on grief at 12-month follow-up was approaching significance, necessitating further evaluation as the data was limited to two studies only and a reliable measure of pediatric grief has not been developed yet.

Caregiver-reports paralleled the self-reported uncontrolled effects from pre- to post-treatment and from pre-treatment to 12-month follow-up as well as the controlled effects at post-treatment for all outcomes. Controlled follow-up effects could not be calculated due to insufficient data. Accordingly, no significant differences were found between raters and intraclass correlations indicated high reliability across outcomes and almost all assessment points. These findings are contrary to earlier meta-analyses that showed only low to moderate concordance between caregivers and their youth (Achenbach et al., 1987; Los Reyes et al., 2015) as well as individual studies on PTSS that showed limited agreement with caregivers detecting less symptoms (Exenberger, Riedl, Rangaramanujam, Amirtharaj, & Juen, 2019; Meiser-Stedman, Smith, Glucksman, Yule, & Dalgleish, 2007; Scheeringa, Wright, Hunt, & Zeanah, 2006; Schreier, Ladakakos, Morabito, Chapman, & Knudson, 2005; Shemesh et al., 2005; Stover, Hahn, Im, & Berkowitz, 2010). However, several individual studies using

samples with physical illnesses found similar results (Clawson et al., 2013; Erickson et al., 2017; Phipps et al., 2005; Stevanovic et al., 2012). It may be that caregivers who seek treatment for their youth are more aware of their youth's mental health problems. Yet another explanation could be that caregivers attribute problems more strongly to their children and adolescents or even exacerbate symptomatology to highlight treatment needs (Asbrand et al., 2021). Some studies also indicate that greater caregiver distress and trauma symptoms are associated with greater symptom reporting for their children (Exenberger et al., 2019; Schreier et al., 2005; Shemesh et al., 2005). However, the incremental reliability suggests that the concordance between raters may increase through the caregivers' treatment participation. That is, caregivers may gain additional awareness and understanding of PTSS through the parallel and conjoint sessions, which may help them to recognize and accommodate their children's behavior. Over the course of treatment, they may also learn techniques that help them to manage their own symptoms.

Taken together, results from publication 2 confirmed not only sustained but also increased treatment effects of specific TF-CBT on PTSS and depression, anxiety and grief at 12-month follow-up. Moreover, it was superior to AT/TAU controls with regard to PTSS at 12-month follow-up but this advantage disappeared for secondary outcomes. In line with publication 1, long-term effects on PTSS were larger in effectiveness studies and group settings, affirming that it can be easily transferred into practice with group settings offering a viable cost-effective and timesaving option. This adds further evidence supporting the widespread use of specific TF-CBT and international guidelines (Forbes et al., 2020; National Institute for Health and Care Excellence, 2018; Phoenix Australia Centre for Posttraumatic Mental Health, 2013). Apart from that, caregiver-reported effects paralleled those of self-

reports and concordance between raters was high. No difference between raters was detected for any outcome. Thus, either informant is sufficient to assess symptom severity. However, when assessing symptom clusters and diagnoses, both perspectives should be considered as they may offer unique information and diagnostic agreement could not be investigated.

The third research question aimed at the development and evaluation of the EBPAS-36D. In Germany, there are increasing research effort to disseminate and implement evidence-based treatments using private practitioners to deliver them. Although this should benefit patients, transfer of evidence-based treatments into practice does not always work well (Morris, Wooding, & Grant, 2011; Shafran et al., 2009; Singal et al., 2014) and attitudes toward EBP have been identified as a critical factor influencing the initial uptake and sustained application by practitioners (Aarons et al., 2009; Moullin et al., 2019). The development and evaluation of the EBPAS-36D by publication 3 therefore enables the assessment of these attitudes and may help to explain transfer difficulties in German-speaking countries.

Most item difficulties were in the medium range, indicating excellent difficulty to discriminate between psychotherapists with varying attitudes. Item removal would not have increased internal consistency and could have reduced content validity. Items and their respective subscales showed correlations at least in the medium range, while the item-whole correlations were more variable. In line with that, correlations of some subscales (Fit, Monitoring, Burden, Feedback) with the total scale were only moderate and only some inter-correlations were high among subscales (Appeal and Fit, Divergence and Limitations, Openness and Divergence, Appeal and Openness, Job Security and Organizational Support). This corresponds well with earlier evaluations of the EBPAS-36 in US and Norwegian samples that showed high inter-correlation only among the subscales Appeal and Organizational

Support (US), Divergence and Limitations, as well as Job Security and Organizational Support (Norway; Rye et al., 2017). Internal consistency of the total scale was good and also in accordance with the US and Norwegian versions. In terms of subscales, internal consistency ranged from acceptable to good. In line with the Norwegian sample, the lowest internal consistency was found for the subscales Appeal, Divergence, Fit and Balance and thus, interpretations should be treated with caution. While the original factor structure was confirmed by the CFA, the EFA revealed a second order factor structure with an even better fit. The four second order constructs were Positive alignment with EBP, Reservations toward EBP, Institutional Endorsement and Constraints by the institution. Moreover, the factor structure suggested by Rye, Friberg, and Skre (2019) also performed better than the original one. Thus, these second-order models might capture the underlying construct even better than the original model. High correlation were found between the EBPAS-36D and the direct and indirect attitude scales of the ISP as well as with global assessment of participant's interest in EBP, confirming convergent validity. Additionally, the Behavioral Intention Scale of the ISP measuring the intention to use EBP was predicted by the EBPAS-36D subsequent to gender, age and participants' report of ever having worked in science.

Interestingly, in the German sample of psychotherapists, older and licensed psychotherapists had more negative attitudes toward EBP, while younger and psychotherapists in training had more positive attitudes. The evidence from earlier studies is inconsistent with some showing similar effects (Egeland et al., 2016; Gray, Elhai, & Schmidt, 2007; Melas et al., 2012; Rye et al., 2019) while others found more positive attitudes with increasing age (Aarons et al., 2010; Aarons & Sawitzky, 2006). However, the German psychotherapy training was changed substantially over the last 30 years and thus, a cohort

effect may be present confounding age effects. In addition, gender differences were found with women showing more favorable attitudes toward EBP. While this confirms earlier research (Aarons, 2006; Aarons et al., 2010; Egeland et al., 2016; Rye et al., 2019; van Sonsbeek et al., 2015), this effect has not been found consistently across studies (Aarons, 2004; Gray et al., 2007; Melas et al., 2012) and thus future research should consider potential moderators.

En masse, the EBPAS-36D showed good item properties, internal consistency and convergent validity in a sample of psychotherapists. In addition, the original factor structure was confirmed although a second-order factor in the EFA found an even better model fit. Moreover, the EBPAS-36D predicted the intention to use EBP. Therefore, the validated EBPAS-36D can be used to evaluate practitioners' attitudes toward EBP and may help to explain transfer difficulties as well as to inform implementation strategies for evidence-based treatments in Germany. It is a reliable instrument that can be useful not only to researchers but also training directors and supervisors. In clinical practice, the EBPAS-36D may help to indicate targets in training, treatment delivery as well as supervision. Moreover, the second-order factor structure may stimulate further research on the construct of attitudes toward EBP.

The fourth research question aimed at the evaluation of instrument-related ES in specific TF-CBT studies for PTSS, depression, anxiety and grief using meta-analysis. This investigation is a crucial aspect in the evaluation of the meta-analytic findings of publication 1 as a great variety of measures were used to assess the outcomes. The different instruments used to assess the respective outcomes could have potentially influenced the results, especially in the context of adapted construct definitions (Higgins et al., 2022; Murad et al., 2019; Puhan et al., 2006). That is, older instruments have been developed in accordance with earlier

editions of the ICD and DSM and have been used in earlier studies, accordingly. In addition, some older measures have been criticized for using a categorical rather than a dimensional approach potentially clouding true ES. In specific TF-CBT, the degree to which ES are influenced by outcome measures has not yet been investigated by meta-analysis and thus, the unpublished report described before covers an important aspect of specific TF-CBT research.

The evaluation of ES by instrument showed that the uncontrolled effects were highly similar across instruments for the respective outcome. Therefore, it seems unlikely that the variety of instruments used by different studies influenced true ES for PTSS, depression, anxiety or grief in specific TF-CBT. Although a greater variety was found for the controlled ES for PTSS, this is most likely due to the different comparators (wait-list vs. AT/TAU) being used. Although it could possibly be that the greater effects in groups and effectiveness studies are caused by the respective instruments used, this seems unlikely given the similar uncontrolled ES of the instruments. The older instruments that were considered most likely to show diverging ES, namely the K-SADS and SCARED, fit well into the range of ES found by newer instruments. Therefore, they did not impact the results of publication 1. Nevertheless, future studies should of course always use updated or newer instruments that are in line with current diagnostic manuals to assure that they measure the intended construct.

5.2 Limitations

Next to the many strengths of the publications and the report discussed above, some limitations need to be discussed. Concerning meta-analyses, some limitations are always unique to individual studies that have been included (e.g. specific methodological weaknesses). All presented meta-analyses tried to account for this variance by rigorously assessing risk of bias in each study and only including those with sufficient evidence for

largely unbiased results. Moreover, when sensitivity analyses detected evidence for publication bias, results were adjusted accordingly. While this was the closest approximation of true ES possible, the true influence of unpublished report remains unknown.

On the level of the meta-analyses themselves rather than the individual studies, some further aspects should be mentioned. While the search terms were carefully defined, it is still possible that they missed some studies that would have fit our inclusion criteria. Additionally, some studies that did fit our inclusion criteria eventually had to be excluded because the data could not be recovered. In terms of methodological limitations, the differences between subgroups were not confirmed by formal analyses since the statistical program did not allow for this computation. In addition, inclusion criteria did not define a minimum symptom criterion as too many different instruments were included leading to great variance in baseline symptoms. Moreover, no reliable instrument was available to assess pediatric grief and some analyses were limited to very few studies. Thus, some of the results should be interpreted with caution. Regarding the long-term effects of specific TF-CBT, results were limited to 12-month follow-up as there were not enough data for later assessment points. Also, at 12-month follow-up, only AT/TAU conditions were available for controlled ES, since all wait-list conditions had naturally expired.

In terms of the validation of the EBPAS-36D some disadvantages should also be considered. All data stemmed from a cross-sectional online survey using self-report methods. In addition, many respondents answered that they worked in science at some point and had a CBT approach. Thus, in this convenience sample, a selection bias toward interest in EBP may have been present and the sample is not fully representative of the population of all mental

health practitioners in Germany. While the dropout rate was not unusual, it cannot be ruled out that only participants interested in the topic completed the survey.

5.3 Implications and future directions

Although specific TF-CBT has a long research history and is largely supported, some specific applications require further research and some gaps remain to be closed. A very promising avenue that needs further evaluation is its application in group settings as effects seem to be larger than in individual settings. Moreover, group applications can be used to provide treatment to large groups of trauma-affected young people in a timely and cost-effective manner. Furthermore, specific TF-CBT studies should more frequently use follow-up assessment, including longer periods to understand its effects beyond 12-month follow-up. Self-reports or caregiver-reports can be used to assess symptom severity in the context of specific TF-CBT when either informant is missing as high concordance can be assumed. However, on a diagnostic level, both perspectives should be considered as they may offer unique information. Future studies should report symptom clusters and probable diagnoses for caregiver-reports more often to enable meta-analysis on a diagnostic level. Additionally, the effects on traumatic grief need to be further evaluated with the development of a reliable instrument still outstanding. Another interesting field that only recently received some more research attention is specific TF-CBT's application in telehealth to reach patients during Covid-19 and in rural areas with little mental health infrastructure available. In addition, first results of applications in inpatient settings as well as with young adults seem promising, warranting further investigation. Although results from effectiveness trials were promising in the presented meta-analyses, these effects can partly be explained by the frequent use of group settings and wait-list comparators. Speaking from my own research experience with private

practitioners in Germany, uptake of specific TF-CBT is rather slow and the use of the EBPAS-36D may help to inform about new implementation strategies and targets for training, treatment delivery and supervision. Of course, the EBPAS-36D is not limited to specific TF-CBT and should also more frequently be used in other implementation studies to increase the knowledge about successful implementation strategies and pitfalls for evidence-based treatments in German-speaking countries. While the EBPAS-36D validation sample was not fully representative, this does not influence the evaluation of the psychometric properties. Nevertheless, results should be confirmed by studies using a more representative sample. In addition, other psychometrically evaluated instruments with fewer items than the ISP should be considered in future studies.

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PART 2. PUBLICATIONS

Publication 1. A systematic review and meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents

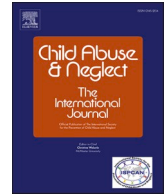
Title	A systematic review and meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents
Authors	Jonathan Felix Benjamin Thielemann ¹ Barbara Kasparik ¹ Julia König ¹ Johanna Unterhitzberger ^{1,2} Rita Rosner ¹
Affiliations	¹ Catholic University Eichstätt-Ingolstadt ² Centre for Children and Adolescents Inn-Salzach e.V.
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Invited Review

A systematic review and meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents

J.F.B. Thielemann^{a,*}, B. Kasparik^a, J. König^a, J. Unterhitzberger^{a,b}, R. Rosner^a^a Department of Psychology, Catholic University of Eichstätt-Ingolstadt, Levelingstr. 7, 85049 Ingolstadt, Germany^b Centre for Children and Adolescents Inn-Salzach e.V., Department of Child and Adolescent Psychiatry, Vinzenz-von-Paul-Straße 14, 84503 Altoetting, Germany

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ABSTRACT

Background: Among minors, posttraumatic stress symptoms (PTSS) are a common consequence of traumatic events requiring trauma-focused treatment.

Objective: This meta-analysis quantified treatment effects of trauma-focused cognitive behavioral therapy (TF-CBT) with PTSS as primary outcome and symptoms of depression, anxiety, and grief as secondary outcomes.

Participants and setting: Inclusion criteria for individual settings: (1) patients aged between 3 and 21, (2) at least one traumatic event, (3) minimum 8 sessions of (4) TF-CBT according to Cohen, Mannarino and Deblinger (2006, 2017), (5) a quantitative PTSS measure at pre- and post-treatment, (6) original research only. Inclusion criteria for group settings: had to involve (1) psychoeducation, (2) coping strategies, (3) exposure, (4) cognitive processing/restructuring, (5) contain some reference to the manual and no minimum session number was required.

Methods: Searched databases were PsychInfo, MEDLINE, Cochrane Library, PTSDpubs, PubMed, Web of Science, and OpenGrey.

Results: 4523 participants from 28 RCTs and 33 uncontrolled studies were included. TF-CBT showed large improvements across all outcomes from pre- to post-treatment (PTSS: $g = 1.14$, CI 0.97–1.30) and favorable results compared to any control condition including wait-list, treatment as usual, and active treatment at post-treatment (PTSS: $g = 0.52$, CI 0.31–0.73). Effects were more pronounced for group settings. We give pooled estimates adjusted for risk of bias and publication bias, which initially limited the quality of the analyzed data.

Conclusions: TF-CBT is an effective treatment for pediatric PTSS as well as for depressive, anxiety, and grief symptoms. It is superior to control conditions, supporting international guidelines recommending it as a first-line treatment.

1. Introduction

Traumatic experiences are common among minors with 57.7 % reporting exposure to at least one type of traumatic event in the past year (Finkelhor, Turner, Shattuck, & Hamby, 2013). For posttraumatic stress disorder (PTSD), an estimated conditional prevalence rate of 15.9 % was found for children exposed to any traumatic experience (Alisic et al., 2014). Co-occurring symptoms of depression

* Corresponding author at: Department of Clinical and Biological Psychology, Catholic University of Eichstätt-Ingolstadt, Levelingstr. 7, 85049 Ingolstadt, Germany.

E-mail address: jonathan.thielemann@ku.de (J.F.B. Thielemann).

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and anxiety are common (American Psychiatric Association, 2013) with evidence of comorbid diagnoses in almost one in two children and adolescents with PTSD (Kar & Bastia, 2006). In addition, comorbidity may be further complicated by unresolved grief symptoms when the trauma involves the death of a loved one (Cohen, Mannarino, & Staron, 2006). International guidelines recommend the use of manualized trauma-focused cognitive behavioral therapy (TF-CBT)¹ with caregiver participation for the treatment of pediatric PTSD (Forbes, Bisson, Monson, & Berliner, 2020; National Institute for Health and Care Excellence, 2018; Phoenix Australia Centre for Posttraumatic Mental Health, 2013).

TF-CBT according to the manual of Cohen, Mannarino, and Deblinger (2006, 2017) and its earlier versions (Cohen & Mannarino, 1993; Deblinger & Heflin, 1996) is the single most extensively studied intervention manual for pediatric PTSD. It is a widespread, easy-to-learn, and cost-effective treatment employing standard cognitive behavioral therapy (CBT) techniques, and has been used in several countries worldwide. The official manual was published in 2006 (Cohen, Mannarino, & Deblinger, 2006) and revised in 2017 (Cohen, Mannarino, & Deblinger, 2017) to reflect advances made in research, application to complex trauma and group settings, as well as updates for DSM-5. TF-CBT according to Cohen et al. contains a sequence of nine components that form the acronym PRACTICE including psychoeducation and parenting skills (P), relaxation (R), affective modulation (A), cognitive coping (C), trauma narrative (T), in vivo exposure (I) conjoint parent-child sessions (C) and enhancing safety and development (E). The authors suggest a minimum of eight sessions to cover all the relevant components. Additionally, caregiver participation is seen as an integral part of TF-CBT according to Cohen et al. Typically, a session lasts 90 min with 45 min dedicated to the child and caregiver, respectively.

TF-CBT according to Cohen et al. has been well-evaluated and found to be effective in reducing posttraumatic stress symptoms (PTSS) as well as symptoms of depression, anxiety, and grief in children and adolescents after exposure to various types of trauma in different populations and settings (Cohen, Deblinger, Mannarino, & Steer, 2004; Deblinger et al., 2006; Dorsey et al., 2020). As a result, it has been extensively disseminated in the US also using the National Child Traumatic Stress Network Initiative (National Child Traumatic Stress Network, 2012). Furthermore, it has been applied to new patient groups such as refugees (Unterhitzberger, Wintersohl, Lang, König, & Rosner, 2019) and trafficked children (Wang et al., 2016), as well as complex PTSD cases (Hébert & Amédée, 2020). In addition, several implementation manuals for specific settings have been developed including residential treatment facilities (Cohen & Mannarino, 2013), military families (Cohen, Mannarino, & Cozza, 2014), foster care (Deblinger, Mannarino, Runyon, Pollio, & Cohen, 2016), and LGBTQ youth (Cohen, Mannarino, Wilson, & Zinny, 2018). Although TF-CBT according to Cohen et al. was originally developed in the US as an individual therapy, it has also been used in low and middle income countries and in a group setting with promising results (Dorsey et al., 2020). Only recently, the intervention was offered to inpatients (Cabrera, Moffitt, Jairam, & Barton, 2020) and young adults up to the age of 25 (Peters et al., 2021). Given its widespread use and the new treatment applications, it is of the utmost importance not only to summarize the well-conducted efficacy RCTs but also to evaluate how well these effects carry over into practice, and determine whether they also apply to these specific patient populations. For the latter, we also need to consider uncontrolled studies to gain an overview of the patient groups to whom TF-CBT according to Cohen et al. can be applied. As a consequence of recent crises all over the world, there is a great need for effective trauma treatment for minors.

Most systematic reviews and meta-analyses that have been published on interventions for traumatized children used TF-CBT as a generic term to refer to both TF-CBT according to Cohen et al. and general CBT with trauma-focused work. In addition, the researchers who pooled effect sizes (ES) for PTSS mainly analyzed TF-CBT in subgroup or moderator analyses only (Bastien, Jongasma, Kabadayi, & Billings, 2020; Gutermann et al., 2016; Hoogsteder, ten Thije, Schippers, and Stams, 2021; Mavranezouli et al., 2020; Morina, Koerssen, & Pollet, 2016). Several systematic reviews are available on TF-CBT according to Cohen et al. (de Arellano, Lyman, Jobe-Shields, George, Dougherty, Daniels, and Delphin-Rittmon, 2014) and more recently, there have been reviews of specific aspects, such as the role of the caregiver (Martin, Everett, Skowron, & Zalewski, 2019), its effectiveness in low and middle income countries (Thomas, Puente-Duran, Mutschler, & Monson, 2020), in refugees (Chipalo, 2021), and in children of preschool age (McGuire, Steele, & Singh, 2021). However, we only know of one review that set out to evaluate the TF-CBT according to Cohen et al. and calculated pooled ES for PTSS but it was published 10 years ago (Cary & McMillen, 2012).

1.1. Summary of previous analyses

The systematic review by Cary and McMillen (2012) is commonly referred to when describing the evidence base of TF-CBT. The authors distinguished between TF-CBT according to Cohen et al. and studies that did not comprise all but at least 4–5 of the most relevant treatment components. In comparison to active non-CBT control conditions, they found small to medium ES in favor of TF-CBT according to Cohen et al. for PTSS and depression. This effect was sustained at the 12-month follow-up for PTSS but not for depression. However, the analyses were limited to three RCTs as studies with CBT control groups were excluded, and many of the frequently cited RCTs were published later (e.g. Dorsey et al., 2014; Goldbeck et al., 2016; Jensen et al., 2014; Murray et al., 2015).

In their meta-analysis, Gutermann et al. (2016) performed a sub-group analysis of 18 studies on TF-CBT according to Cohen et al., reporting a large pre-post ES for PTSS. Most notably, this was the only meta-analysis on TF-CBT that considered pre-post ES and studies other than RCTs. However, TF-CBT was not the authors' main focus. Consequently, they did not explore between-group effects or any outcomes other than PTSS.

Another recent meta-analysis (Bastien et al., 2020) included a comparison of TF-CBT according to Cohen et al. with any control condition. For PTSS, a medium ES in favor of TF-CBT according to Cohen et al. was found. However, it should be noted that the study

¹ ¹ In the literature, TF-CBT is used as a generic term for CBT with trauma-focused work as well as for Cohen et al.'s TF-CBT manual. 'TF-CBT according to Cohen et al.' will be used throughout the manuscript when referring to their manual or its earlier versions.

defined a narrower period from 2011 to 2019 and unfortunately missed some studies that would have fit their inclusion criteria, resulting in only seven RCTs being included. Thus, this analysis only partly represented the existing literature.

Apart from that, several meta-analyses are available of interventions for traumatized children and adolescents that include other TF-CBT therapies (Hoogsteder, ten Thije, Schippers, & Stams, 2021; Lenz & Hollenbaugh, 2015; Mavranzouli et al., 2020; Morina et al., 2016). In sum, for PTSS these analyses found large ES in favor of TF-CBT when compared to wait-list conditions and small to medium ES when compared to active treatments. For depression, these effects were less pronounced with small to medium ES compared to wait-list conditions and small effects compared to active treatments. However, in some of these analyses, limitations were obvious such as the exclusion of intention-to-treat analyses (Lenz & Hollenbaugh, 2015) and collapsed outcomes (Hoogsteder et al., 2021). Interestingly, one of the meta-analyses (Mavranzouli et al., 2020) included separate analyses for TF-CBT group settings for the first time. Unfortunately, the TF-CBT group analyses mostly included studies with no reference to TF-CBT according to Cohen et al.

1.2. Current study

In the light of this state of the literature, an update on TF-CBT according to Cohen et al. is warranted. In addition to examining gold standard efficacy RCTs, we extended the focus by looking at the intervention's effectiveness, and also considered uncontrolled studies and group settings. Effectiveness studies can offer insights into how treatment can be implemented in mental health care settings with varying stakeholders and resources. They can likewise estimate the degree to which the effects of efficacy RCTs translate into the field. In addition, uncontrolled studies may provide valuable information on implementation in differing contexts as well as on applications that warrant further investigation in RCTs. In an attempt to provide a more complete picture, this systematic review and meta-analysis quantified the treatment effects of TF-CBT according to Cohen et al. from pre- to post-treatment as well as in comparison to control conditions at post-treatment with due consideration of designs, comparators, and settings for PTSS and secondary outcomes of depression, anxiety, and grief.

2. Methods

2.1. Search and screening of studies

The meta-analysis was conducted in line with the PRISMA guidelines (Liberati et al., 2009; Page et al., 2021; for the PRISMA checklist, see supplementary material S1), and pre-registered with PROSPERO (CRD42020139403). We searched the databases PsychInfo, MEDLINE, Cochrane Library, PTSDpubs, PubMed, Web of Science and OpenGrey for studies published between January 1, 1990, and August 19, 2021. A pre-defined combination of search terms was used for the title and abstract searches (see Table 1). After removing any duplicates, the database results and a manual search of reference sections of relevant works coupled with expert suggestions identified 1262 publications (see Fig. 1). There were no limitations regarding language. All titles and abstracts were screened by two independent raters using Covidence (Veritas Health Innovation, 2014). Any conflicts between raters were resolved by reviewing the abstracts. For the remaining studies, the first author read all full texts and assessed the inclusion and exclusion criteria. Ambiguous cases were resolved by contacting the authors of the publications in question and discussing them with the co-authors.

2.2. Inclusion and exclusion criteria

For individual treatment settings, (1) the patients had to be aged between 3 and 21, (2) had to have experienced at least one traumatic event, (3) had to have participated in at least 8 sessions (4) of TF-CBT according to the Cohen et al. manual (Cohen, Mannarino, and Deblinger, 2006, 2017) or one of its earlier versions (Cohen & Mannarino, 1993; Deblinger & Heflin, 1996). In addition, (5) results based on a quantitative PTSS measure applied before and after treatment had to be reported via clinical interview or self-report. (6) Only original research was included, excluding reviews, meta-analyses, and case reports. The only exclusion criterion was if the recipients of treatment were not children or adolescents themselves (e.g. their parents only). If we were unable to extract PTSS pre-post ES (c.f. criterion 5), we contacted authors and included the study if they provided supplementary data. For group settings, there were some differences in the inclusion criteria: no minimum number of sessions was required as many TF-CBT groups

Table 1

Pre-defined search terms.

Search categories	Search terms
Diagnosis	Trauma* or Posttrauma* or Post-trauma* or PTSD or PTSS or grief or griev*
Trauma-related	Abuse* or Assault* or Abduct* or Accident* or Kidnapp* or Life-threat* or Maltreat* or Mistreat* or Neglect* or Refugee or Shooting or Terroris* or Victim* or Violence or War or Hurricane or Tsunami or Earthquake or Flood or "Natural disaster" or bereave* or loss
Youth	Adolescen* or Child* or Youth or Kid or Juvenile or Infant or Minor or Teenager or Young*
TF-CBT	"Trauma focused cognitive behavioral treatment" or "Trauma-focused cognitive behavioral treatment" or "Trauma focused cognitive behavioral therapy" or "Trauma-focused cognitive behavioral therapy" or "Trauma focused cognitive behavior*" or "Trauma-focused cognitive behavior*" or "Trauma focused cog*" or "Trauma-focused cog*" or "Trauma focused" or Trauma-focused or TF-CBT or grief-focused or "grief focused"

Note. Combination: (Diagnosis or Trauma-related) and Youth and TF-CBT.

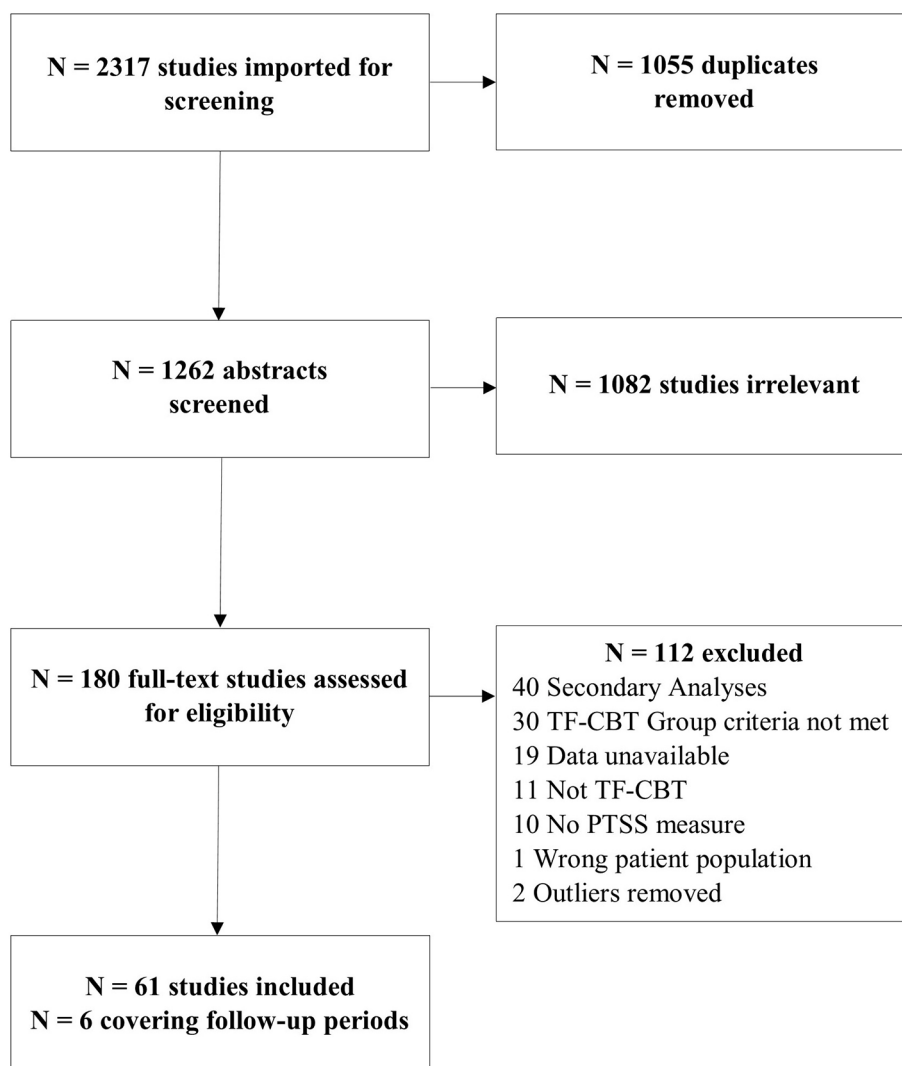


Fig. 1. Flowchart study selection.

Note. The two outliers refer to the same study covering the follow-up period.

are conceptualized with fewer sessions. Consequently, they also did not need to cover all components of the manual as some of them are usually left out. Instead, group settings had to involve (1) psychoeducation, (2) coping strategies (i.e. relaxation, affective modulation, cognitive coping), (3) exposure, (4) cognitive processing/restructuring of trauma-related thoughts and beliefs, and (5) some reference to the manual or one of its earlier versions. For the latter, a clear statement was sufficient. However, in case of ambiguity, the decision was made in discussion with the co-authors.

2.3. Treatment and control groups

For the control groups, randomized wait list (WL), treatment as usual (TAU), and active treatment (AT) conditions were included. As the latter two contained comparable treatments, they were merged for analysis. If two TAU/AT control groups were available, the higher dose of treatment was used. If studies included two TF-CBT conditions, these were included separately in the pre-post analysis unless only merged results were reported. If a control group was available, it was compared to both TF-CBT conditions, separately. In one case, there was a combination of four TF-CBT conditions and four control conditions that were comparable with regard to treatment dose (Dorsey et al., 2020). These control conditions were matched to their respective TF-CBT condition as data were collected in separate countries and regions.

2.4. Efficacy and effectiveness

While efficacy refers to the intervention operating under perfectly controlled conditions to maximize internal validity, effectiveness is characterized by its application to ‘real-world’ settings (Singal, Higgins, & Waljee, 2014). In contrast to the preregistration, we decided to distinguish between efficacy and effectiveness RCTs rather than RCTs and dissemination and implementation (D&I) trials as some D&I trials had randomized control groups whereas others did not. This decision was made to keep our analyses more parsimonious. The criteria for classifications were adapted from Gartlehner, Hansen, Nissman, Lohr, and Carey (2006), and can be found in the supplementary material (S2). These assessments were carried out by two independent evaluators (JT and BK) and disagreements were resolved in discussions. It should be noted that efficacy and effectiveness are on a continuum and a clear-cut distinction is hardly possible. Thus, studies referred to as efficacy or effectiveness RCTs may contain some features of the other type of trial.

2.5. Risk of bias assessment

Following recommendations from the Cochrane Handbook (Higgins et al., 2022), two independent evaluators (JT and BK) carried out risk of bias assessments for all RCTs with the Risk of Bias assessment tool (Rob 2.0) using Excel (Sterne et al., 2019). This tool assesses five domains that potentially pose a risk of bias. Namely, these are the randomization process (D1), deviations from the intended intervention (D2), missing outcome data (D3), measurement of the outcome (D4), and selection of the reported results (D5). Accordingly, studies were attributed the ratings ‘low risk’, ‘some concerns’ or ‘high risk’.

For uncontrolled studies, the Risk Of Bias In Non-randomized Studies – of Interventions (ROBINS-I) assessment tool was used (Sterne et al., 2016) following the same procedure. While D2-D5 were the same, D1 was replaced by three additional domains referring to ‘confounding’, ‘selection bias’, and ‘bias in classification of intervention’. However, since TF-CBT and valid pre-post assessments were defined by the inclusion criteria and we were not interested in including non-randomized control groups, these domains were not applied. Additionally, ROBINS-I used slightly different risk ratings including ‘low’, ‘moderate’, ‘serious’ and ‘critical’ as well as ‘no information’. To ensure comparability, we converted the ratings to the RoB 2.0 categories with ‘serious’ and ‘critical’ being subsumed as ‘high risk’. ‘No information’ was viewed separately and did not affect the overall ratings (see Results section). Five RCTs and 22 uncontrolled studies were identified as posing a high risk of bias. All analyses were rerun excluding ‘high risk’ studies.

2.6. Outcomes and data extraction

All data were extracted by JT and BK to protect against errors, and inconsistencies were resolved in discussion. We extracted outcome data on PTSS, depression, anxiety, and grief. Clinical interviews were the first choice. If these were not available, we used self-report. If only subscales of PTSD clusters were reported, we merged them using the formula presented in the Cochrane Handbook for Systematic Reviews of Interventions (Higgins et al., 2022). Authors were contacted for missing information and studies were excluded if the PTSS data could not be obtained for pre- and post-treatment. Missing data on other variables merely resulted in exclusion of studies from analyses of the respective variable.

2.7. Statistical analyses

We included all eligible studies in the within-group and between-group analyses generating ES (Hedges' g and 95 % CIs) for PTSS, depression, anxiety, and grief. We used Comprehensive Meta-Analysis, version 3 (Borenstein, Hedges, Higgins, & Rothstein, 2013). If available, we used intention-to-treat data. If the pre-post correlation necessary for the calculation of pre-post ES was unavailable, it was imputed based on the overall mean of included studies with available correlations for the respective outcome. The pooled ES were based on a random effects model as the samples, modality of treatment, and methods were very heterogeneous (Hedges & Vevea, 1998). Q statistic was computed to confirm the model and I^2 to assess heterogeneity (Higgins, Thompson, Deeks, & Altman, 2003). If Q was non-significant, we assumed a fixed model, and reported ES accordingly. We employed funnel plots with Hedges' $g > 4$ being deemed to be indicative of outliers (see supplementary material, S3). We conducted additional subgroup analyses of within-group ES for RCTs only, individual setting, and group setting. Within the RCT group, further sub-group analyses (within-group and between-group) were performed for individual and group settings, efficacy and effectiveness trials as well as WL and TAU/AT conditions. Sub-group analyses were performed when at least three TF-CBT conditions or three post-treatment comparisons were available. Sensitivity analyses indicated the presence of publication bias in some analyses. To address these biases, we used Duval and Tweedie's (2000) trim and fill method to impute missing studies to the left of the mean in order to make the funnel plots symmetrical.

3. Results

Fig. 1 shows the study selection procedure. After removing duplicates, 1262 remained of which we ultimately selected 61. Of these, $k = 28$ studies were RCTs and $k = 33$ were uncontrolled studies. The majority ($k = 50$, 21 of them RCTs) were conducted in an individual setting, and $k = 11$ (seven of them RCTs) in a group setting. Within the RCTs, there were $k = 14$ efficacy trials and $k = 14$ effectiveness trials. Seven RCTs included WL control conditions whereas $k = 15$ contained TAU/AT control conditions. The control conditions of the remaining six RCTs were either a second TF-CBT condition and thus included as an additional treatment condition or were excluded for other reasons.

3.1. Study characteristics

An overview of all included studies and study characteristics can be found in the supplementary material S4. Even though the majority of studies were from the US (60 %), we were able to include studies from 14 other countries mostly from Africa (13 %), Europe (11 %), Asia (5 %), and Australia (5 %). The weighted means and pooled standard deviations presented in this section rely on the analyzed sample.

3.1.1. Participants and caregivers

We were able to include 4523 out of the original 6370 participants in the analyses (TF-CBT $n = 3490$, control groups $n = 1033$, range 11–640 participants per study). The mean age was 11.99 years ($SD_p = 2.47$, range 3–21, available for $k = 46$, $n = 3805$). The overall sample included slightly more female participants (59.46 %, $k = 56$, $n = 4322$). Participants experienced a mean of 4.93 traumatic events ($SD_p = 3.96$, $k = 27$, $n = 2594$). Diagnostic status for PTSD derived via structured diagnostic interviews (i.e. excluding diagnoses derived from self-report instruments) within the TF-CBT conditions at baseline and post-treatment was available for 334 participants with 269 meeting the diagnostic criteria at baseline (80.54 %) and 81 at post-treatment (24.25 %, $k = 12$). In control conditions, this information was available for 198 participants with 161 meeting the criteria at baseline (81.31 %) and 85 at post-treatment (42.93 %, $k = 5$). The mean number of treatment sessions was 16.66 ($SD_p = 7.97$, $k = 16$, $n = 1099$). In total, 1750 participants dropped out of studies ($k = 51$ studies), 988 of these after randomization or during treatment and 348 were lost to follow-up. For the remaining 414 participants, the time of dropout could not be coded. However, most studies adopted an intention-to-treat approach, including participants with premature treatment termination in their analyses.

Nearly all studies ($n = 59$) reported caregiver involvement including biological parents, foster parents, adoptive parents, step-parents, other relatives, legal guardians, professional caregivers (e.g. social workers) or a combination thereof. In total, 3255 caregivers were recorded ($k = 34$), most of whom were female (87.89 %, $k = 17$, $n = 1952$ caregivers). However, the degree of involvement varied markedly across studies ranging from almost no involvement or involvement only for some children to the full TF-CBT protocol with parallel sessions in place or professional caregivers delivering the intervention.

3.1.2. Therapists

In total, 881 therapists were recorded (range: 1–133 therapists per study, $k = 41$), most of them female (85.05 %, $k = 13$, $n = 516$ therapists). Education ranged from no education in mental health prior to the study over bachelor's and master's degrees in psychology and social work to doctorates. However, most studies used therapists with master's degrees or did not specify their education level simply referring to them as 'therapists' or 'psychologists'. They had mean experience of 6.8 years ($SD_p = 19.24$, range 0–10.77 years, $k = 8$, $n = 232$). Most of the therapists were trained by treatment developers or certified TF-CBT trainers. Training usually involved online training (tfcbt2.musc.edu), a 1–2 day in-person workshop, and reading the TF-CBT manual. In some instances, less (e.g. reading the manual only) or more intensive training (e.g. a 10-day in-person workshop) was provided. Typically, therapists received weekly supervision, again ranging from less frequent (once per month) to more frequent (three times per week). Overall, we calculated a mean of 0.70 supervision sessions a week ($k = 25$, $n = 621$).

3.2. Treatment applications

Some studies reported changes to standard TF-CBT according to Cohen et al. These included adding 4–8 sessions specific to grief, additional activities from other manuals, and culture-specific elements (e.g. metaphors, jargon, songs, games, and religious beliefs). Moreover, some studies varied the mode of treatment delivery by making use of group formats, tablet-supported therapy, telehealth, animal-assisted therapy, school setting, inpatient setting, and interpreters. Additionally, some adjusted treatment length, session length, or session frequency. In addition, patient groups (e.g. refugees, orphans, foster children etc.), and trauma types (e.g. sexual, war-related, mixed etc.) varied strongly across studies. In fact, the studies were so heterogeneous regarding patients and traumas that we did not extract these variables as coding turned out to be impossible. However, none of the studies reported major difficulties in integrating these applications into the existing protocol.

3.3. Risk of bias assessment

All risk of bias assessments of the individual studies can be found in the supplementary material S5. Five out of the 28 included RCTs, were assigned an overall 'low' risk of bias rating, 18 studies were classified as presenting 'some concerns', and five trials were identified as having a 'high' risk of bias according to RoB 2.0's algorithm.

Problems with the randomization process (D1) appeared in only one study. We did not consider differences in the characteristics of participants' parents, as they were not subject to randomization. There were few deviations from the interventions (D2) with most studies providing acceptable to excellent adherence ratings. However, two studies were attributed 'high risk' ratings as some problematic aspects were identified. One study reported deviations caused by the trial context (providers made fewer referrals after disappointment over assignment to control condition). Additionally, the study reported doubtful treatment adherence (treatment adherence was described as 'variable'). Another study provided no information on any measure of treatment fidelity and prompted several reasons for questioning adherence (e.g. no information on training and supervision, one therapist only, additional activities from another manual, etc.). Although many studies reported missing outcome data (D3), it was either confirmed that missingness did not depend on the true value of the outcomes or at least it could not be assumed (i.e. data were missing at random). Regarding the

outcome measurement (D4), there were several studies that presented at least ‘some concerns’. In most cases, the outcome assessors were (partly) not blinded to treatment conditions. Given the likelihood that this knowledge may have influenced the outcomes, this resulted in either ‘some concerns’ or ‘high risk’ ratings. Consequently, two studies received a ‘high risk’ rating because of non-blinded assessors. The reason for the last ‘high risk’ rating was unequal assessment points across groups. The criterion ‘selection of reported results’ (D5) led to most ‘some concerns’ ratings, as many authors of RCTs did not specify their analyses adequately beforehand. Although many trials were registered, the available information was, in many cases, not sufficient.

With regard to the 33 included uncontrolled studies, none received a ‘low’ risk of bias rating, $k = 11$ were rated ‘moderate’ (i.e. ‘some concerns’), and $k = 22$ studies were identified as ‘serious’ or ‘critical’ (i.e. ‘high risk’). Deviations from the interventions (D2) were difficult to assess as information on adherence was frequently missing ($k = 10$). However, we felt it was inappropriate to rate all these studies as posing a high risk of bias in this domain. They were often well conducted in terms of training and supervision, and did not give any reason to doubt treatment adherence. Thus, we decided not to judge these studies in this domain (i.e. ‘no information’), leaving the overall rating unaffected. Nevertheless, one study received a ‘critical’ rating as too many other interventions were provided at the same time, making it impossible to judge the treatment effect. Almost half ($k = 13$) of these studies received ‘serious’ to ‘critical’ ratings with respect to missing outcome data (D3), as substantial proportions of the original sample were not included. Outcome assessors were always assumed to have knowledge of the intervention received, resulting in at least a ‘moderate’ risk in the outcome measurement category (D4). However, we made a distinction between assessors. Participants (self-report) were regarded as posing a ‘moderate’ risk since they may have different hypotheses regarding treatment effects. In contrast, therapists or researchers (interviews) were regarded as posing a ‘serious’ risk since they normally have hypotheses in favor of their intervention. This resulted in $k = 16$ ‘serious’ risk ratings. Although we only detected problems in five instances in the selection of the reported results (D5), this domain always resulted in at least ‘moderate’ risk ratings, as none of the uncontrolled studies was preregistered. ‘Serious’ risk ratings were attributed for including more than one outcome measure without reporting all results, reporting incomplete data for only some assessment points, and reporting data on a subset only.

3.4. Effect sizes

Within-group and between-group pooled ES are shown in [Tables 2 and 3](#), respectively. If not stated otherwise, all reported ES below refer to the analyses excluding studies with a high risk of bias rating as ES did not differ substantially (see supplementary material S6). Within-group analyses for ‘all eligible studies’ refers to RCTs and uncontrolled studies in individual as well as group settings. Accordingly, subgroup analyses for ‘RCTs only’ include individual and group settings and subgroup analyses for ‘individual’ as well as ‘group’ includes RCTs and uncontrolled studies. For between-group analyses, ‘any control’ refers to WL and TAU/AT comparators in individual and group settings including efficacy as well as effectiveness trials. Consequently, subgroup analyses for ‘WL’ and ‘TAU/AT’

Table 2
Pre-post within-group effect sizes for PTSS, depression, anxiety and grief.

Outcome	Sample	n	g	95 % CI	SE	z	p	Q	I ²	Duval and Tweedie
PTSS	All eligible studies	42	(1.27) 1.14 ^a	(1.10–1.44) 0.97–1.30	(0.09)	(14.68)	(.000)	336.13***	87.80	6
	RCTs only	31	(1.34) 1.26 ^a	(1.08–1.59) 1.01–1.51	(0.13)	(10.23)	(.000)	294.33***	89.81	2
	Individual	29	(1.09) 0.94 ^a	(0.91–0.1.27) 0.77–1.11	(0.09)	(11.88)	(.000)	198.56***	85.90	6
	Group	13	(1.66) 1.53 ^a	(1.35–1.97) 1.22–1.85	(0.16)	(10.40)	(.000)	65.04***	81.55	2
Depression	All eligible studies	28	0.63	0.51–0.76	0.06	9.94	.000	87.97***	69.31	0
	RCTs only	20	0.59	0.43–0.75	0.08	7.16	.000	56.99***	66.66	0
	Individual	23	0.63	0.49–0.77	0.07	8.78	.000	75.37***	70.81	0
	Group	5	0.65	0.35–0.95	0.15	4.27	.000	12.29*	67.45	0
Anxiety	All eligible studies	18	0.56	0.43–0.69	0.07	8.17	.000	32.92*	48.35	0
	RCTs only	16	0.52	0.38–0.66	0.07	7.34	.000	27.88*	46.20	0
	Individual	14	0.59	0.42–0.77	0.09	6.79	.000	30.56**	57.43	0
	Group	4	0.49 ^b	0.29–0.68	0.10	4.92	0.000	2.23	0	0
Grief	All eligible studies	8	(1.37) ^b 1.35 ^{a,b}	(1.23–1.51) 1.21–1.48	(0.07)	(19.21)	(0.000)	13.05	46.34	1
	RCTs only	5	1.40 ^b	1.23–1.56	0.09	16.46	.000	3.56	0	0
	Individual	3	1.25	0.54–0.1.96	0.13	3.44	<.01	6.79*	70.52	0
	Group	5	(1.44) ^b 1.41 ^{a,b}	(1.28–1.59) 1.26–1.55	(0.08)	(18.08)	(.000)	2.57	0	1

Note. High risk of bias studies are excluded; n = number of included TF-CBT conditions; Individual = TF-CBT conducted in an individual treatment setting; Group = TF-CBT conducted in a group setting.

^a Recalculated with imputed studies (trim and fill method according to Duval and Tweedie).

^b Fixed model assumed due to non-significant Q-value.

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$.

contained studies in individual and group settings as well as efficacy and effectiveness trials. Likewise, subgroup analyses for ‘individual’ and ‘group’ contained WL and TAU/AT comparators as well as efficacy and effectiveness trials. Subsequently, ‘efficacy’ and ‘effectiveness’ analyses included WL and TAU/AT comparators as well as individual and group settings. For the ES of individual studies and the funnel plots showing observed and imputed studies, see supplementary material S7.

3.4.1. Pre-post within-group effects

Across all included TF-CBT conditions, the mean pre-post within-group ES was large for PTSS ($g = 1.14$, CI 0.97–1.30) and grief ($g = 1.35$, CI 1.21–1.48) and medium for depression ($g = 0.63$, CI 0.51–0.76) and anxiety ($g = 0.56$; CI 0.43–0.69). Similar results were found for RCTs (PTSS: $g = 1.26$, CI 1.01–1.51; grief: $g = 1.40$, CI 1.23–1.56; depression: $g = 0.59$, CI 0.43–0.75; anxiety: $g = 0.52$, CI 0.38–0.66) and individual settings only (PTSS: $g = 0.94$, CI 0.77–1.11; grief: $g = 1.25$, CI 0.54–1.96; depression: $g = 0.63$, CI 0.49–0.77; anxiety: $g = 0.59$, CI 0.42–0.77). Looking at studies conducted in a group setting only, the mean ES for PTSS ($g = 1.53$, CI 1.22–1.85) was even larger, but, again, similar for grief ($g = 1.41$, CI 1.26–1.55), depression ($g = 0.65$, CI 0.35–0.95), and anxiety ($g = 0.49$, CI 0.29–0.68). All sub-group analyses within RCTs paralleled these patterns (see Table 4).

3.4.2. Post-treatment between-group effects

Compared to any control group, the post-treatment between-group ES for TF-CBT conditions was medium for PTSS ($g = 0.52$, CI 0.31–0.73) and small for depression ($g = 0.40$, CI 0.27–0.52), anxiety ($g = 0.26$, CI 0.13–0.39), and grief ($g = 0.30$, CI -0.06–0.67). Compared to waitlist conditions, the ES were large for PTSS (1.18, CI 0.55–1.82) and again small for depression ($g = 0.47$, CI 0.27–0.68) and anxiety ($g = 0.32$, CI 0.13–0.51). Compared to TAU/AT conditions, the ES were small across all outcomes (PTSS: $g = 0.32$, CI 0.15–0.50; depression: $g = 0.34$, CI 0.19–0.50; anxiety: $g = 0.20$, CI 0.03–0.36; grief: $g = 0.30$ CI -0.06–0.67). The pattern was similar in individual therapy settings (PTSS: $g = 0.37$, CI 0.23–0.50; depression: $g = 0.40$, CI 0.26–0.53; anxiety: $g = 0.25$, CI 0.10–0.39) and in efficacy trials (PTSS: $g = 0.33$, CI 0.17–0.50; depression: $g = 0.34$, CI 0.17–0.52; anxiety: $g = 0.17$, CI -0.02–0.36). Regarding effectiveness trials, the ES was medium for PTSS ($g = 0.70$, CI 0.38–1.01) and small for secondary outcomes (depression: $g = 0.45$, CI 0.27–0.63; anxiety: $g = 0.35$, CI 0.16–0.54; grief $g = 0.33$, CI -0.07–0.73). The same pattern was found for group settings (PTSS: $g = 0.79$, CI 0.38–1.20; grief: $g = 0.33$, CI -0.07–0.73). Some results must be interpreted with caution due to the limited number of available comparisons. For the WL comparison regarding depression, it was only possible to compute an ES that included high risk of bias studies as excluding them resulted in fewer than three comparisons. The analysis for anxiety regarding efficacy trials as well as all grief-related analyses did not yield statistically significant results.

Table 3
Post-treatment between-group effect sizes for PTSS, depression, anxiety and grief.

Outcome	Sample	n	g	95 % CI	SE	z	p	Q	I ²	Duval and Tweedie
PTSS	Any control	22	0.52	0.31–0.73	0.11	4.83	.000	98.46***	78.67	0
	TAU/AT	16	0.32	0.15–0.50	0.09	3.69	.000	36.74**	59.18	0
	WL	6	1.18	0.55–1.82	0.32	3.65	.000	41.25***	87.88	0
	Effectiveness	13	0.70	0.38–1.01	0.16	4.35	.000	84.56***	85.81	0
	Efficacy	9	0.33 ^a	0.17–0.50	0.08	3.98	.000	10.27	22.13	0
	Individual	12	0.37 ^a	0.23–0.50	0.07	5.42	.000	10.74	0	0
Depression	Group	10	0.79	0.38–1.20	0.21	3.74	.000	83.89***	89.27	0
	Any control	14	0.40 ^a	0.27–0.52	0.06	6.14	.000	18.80	30.83	0
	TAU/AT	10	0.34 ^a	0.19–0.50	0.08	4.41	.000	16.33	44.89	0
	WL	6	0.47 ^{a,b}	0.27–0.68	0.11	4.50	.000	1.56	0	0
	Effectiveness	6	0.45 ^a	0.27–0.63	0.09	4.89	.000	5.71	12.48	0
	Efficacy	8	0.34 ^a	0.17–0.52	0.09	3.82	.000	12.38	43.43	0
Anxiety	Individual	12	0.40 ^a	0.26–0.53	0.07	5.75	.000	18.45	40.38	0
	Any control	12	0.26 ^a	0.13–0.39	0.07	3.82	.000	14.28	22.954	0
	TAU/AT	8	0.20 ^a	0.03–0.36	0.09	2.30	<.05	9.93	29.512	0
	WL	6	(0.38) ^a	(0.17–0.58)	(0.11)	(3.57)	(.000)	2.84	0	1
		6	0.32 ^{a,c}	0.13–0.51						
	Effectiveness	5	0.35 ^a	0.16–0.54	0.10	3.65	.000	2.90	0	0
Grief	Efficacy	7	0.17 ^a	-0.02–0.36	0.10	1.77	n.s.	9.56	37.24	0
	Individual	10	0.25 ^a	0.10–0.39	0.07	3.36	<.01	11.362	20.79	0
	Any control	5	0.30	-0.06–0.67	0.08	1.63	n.s.	19.58**	79.57	0
	TAU/AT	5	0.30	-0.06–0.67	0.08	1.63	n.s.	19.58**	79.57	0
	Effectiveness	4	0.33	-0.07–0.73	0.20	1.63	n.s.	19.25***	84.41	0
	Group	4	0.33	-0.07–0.73	0.20	1.63	n.s.	19.25***	84.41	0

Note. High risk of bias studies are excluded; n = number of included comparisons; PTSS = Posttraumatic Stress Symptoms; TAU/AT = Treatment as usual/active treatment control conditions; WL = Wait-list control conditions; Effectiveness = Effectiveness RCTs only; Efficacy = Efficacy RCTs only; Individual = RCTs conducted in an individual treatment setting; Group = RCTs conducted in a group setting; n.s. = non-significant.

^a Fixed model assumed due to non-significant Q-value.

^b Analysis includes high risk of bias studies (calculation otherwise not possible due to low number of studies).

^c Recalculated with imputed studies (trim and fill method according to Duval and Tweedie).

** $p < .01$.

*** $p < .001$.

Table 4

Pre-post within-group effect sizes for PTSS, depression and anxiety for subgroups within RCTs.

Outcome	Sample	n	g	95 % CI	SE	z	p	Q	I ²	Duval and Tweedie
PTSS	Effectiveness	16	1.45	1.19–1.72	0.14	10.73	.000	78.22***	80.82	0
	Efficacy	15	(1.17)	(0.80–1.54)	(0.19)	(6.21)	(.000)	114.04***	87.72	1
Depression			1.11 ^a	0.76–1.47						
	Individual	19	1.10	0.82–1.38	0.15	7.59	.000	127.72***	85.91	0
	Group	12	1.66	1.32–2.01	0.18	9.50	.000	64.36***	82.91	0
	Effectiveness	9	0.66	0.42–0.90	0.12	5.37	.000	27.49**	70.90	0
	Efficacy	11	0.53	0.31–0.75	0.11	4.69	.000	25.47**	60.75	0
	Individual	16	0.62	0.41–0.82	0.10	5.95	.000	54.93***	72.69	0
Anxiety	Group	4	0.52 ^b	0.32–0.71	0.10	5.23	.000	2.00	0	0
	Effectiveness	7	0.57 ^b	0.43–0.70	0.07	8.34	.000	5.94	0	0
	Efficacy	9	0.50	0.27–0.74	0.12	4.18	.000	18.80*	57.46	0
	Individual	12	0.55	0.37–0.74	0.10	5.81	.000	25.65**	57.12	0
	Group	4	0.49 ^b	0.29–0.68	0.10	4.92	.000	2.23	0	0
	Effectiveness	4	1.42 ^b	1.25–1.58	0.09	16.38	.000	2.18	0	0
Grief	Group	4	1.42 ^b	1.25–1.58	0.09	16.38	.000	2.18	0	0

Note. High risk of bias studies are excluded; only analyses with a sufficient number of TF-CBT conditions are reported; n = number of included TF-CBT conditions; Effectiveness = Effectiveness RCTs only; Efficacy = Efficacy RCTs only; Individual = RCTs conducted in an individual treatment setting; Group = RCTs conducted in a group setting.

^a Recalculated with imputed studies (trim and fill method according to Duval and Tweedie).

^b Fixed model assumed due to non-significant Q-value.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

4. Discussion

This systematic review and meta-analysis assessed the treatment effects of TF-CBT according to Cohen et al. for pediatric PTSS and secondary outcomes of depression, anxiety, and grief. Our results that are derived from 61 studies that met the inclusion criteria, provided strong support for TF-CBT according to Cohen et al. Effects for PTSS were greater than for secondary outcomes, as well as more pronounced in group settings and effectiveness studies than in individual settings and efficacy studies. Additionally TF-CBT according to Cohen et al. was found to be superior to control conditions, with greater effects when compared to WL conditions than TAU/AT. These findings are in line with previous analyses confirming TF-CBT as an effective treatment for pediatric PTSS and secondary outcomes as well as its superiority over no treatment and other treatment approaches (Cary & McMillen, 2012; Morina et al., 2016). Furthermore, this was the first meta-analysis to confirm the feasibility of implementing TF-CBT according to Cohen et al. in 'real-world' settings and its application in group settings.

4.1. PTSS

Regarding the uncontrolled large ES for TF-CBT according to Cohen et al., our findings for PTSS closely matched those of Gutermann et al. (2016). Moreover, the controlled medium ES compared to any control group is in line with previous analyses on TF-CBT according to Cohen et al. (Bastien et al., 2020; Cary & McMillen, 2012). The results also fit the broader TF-CBT literature with small effects when comparing treatment to TAU/AT conditions (Lenz & Hollenbaugh, 2015; Morina et al., 2016) and large effects when compared to WL conditions (Lenz & Hollenbaugh, 2015; Mavranouzouli et al., 2020; Morina et al., 2016). In the context of general pediatric PTSS treatments, the effects tend to follow the same pattern but the large effects versus WL conditions were less pronounced for other treatments (Gutermann et al., 2016; Morina et al., 2016). Psychotherapy research suggests that achieving large treatment effects compared to TAU/AT conditions is generally hard to accomplish since these conditions may themselves contain powerful interventions (Frost, Laska, & Wampold, 2014). Consequently, the small ES in favor of TF-CBT according to Cohen et al. in comparison even to TAU/AT underlined its treatment capabilities and superiority over other treatments.

This might help to interpret the counterintuitive finding that effectiveness trials had a greater PTSS ES than efficacy trials when compared to control conditions, which contrasts with the common notion that effects from efficacy RCTs do not translate well into practice (Singal et al., 2014). Efficacy trials tend to include TAU/AT conditions as comparators rather than WL conditions and are conducted in individual settings most of the time. In contrast, effectiveness trials are more heterogeneous in terms of comparator and setting. Accordingly, all studies identified as efficacy trials exclusively contained TAU/AT conditions and only one group RCT while effectiveness trials included a mixture. Within group settings, WL and TAU/AT controls were evenly distributed across the studies. However, slightly more ES were extracted for TAU/AT conditions due to one study yielding multiple ES. Since greater ES were found in group settings and in comparison to WL rather than TAU/AT conditions, this probably contributed to the difference between efficacy and effectiveness trials.

Regarding group settings, the small number of efficacy trials and the large ES for PTSS may be further explained by the fact that most of the TF-CBT according to Cohen et al. group studies targeted underserved populations, often in low and middle income countries. In this context, these very strong effects are not surprising considering the general lack of access to mental health care and

the high baseline symptomatology found in these samples. Additionally, a strictly controlled design was not feasible in many circumstances due to missing infrastructure, which prompted the need for rather pragmatic solutions. Subsequently, all but one group RCT were identified as effectiveness trials. Nevertheless, the strong effect on PTSS found in group settings was not entirely limited to underserved populations with limited access to mental health care as 36 % of studies contributing to the uncontrolled ES were conducted in high income countries (43 % for controlled ES). This is a very interesting finding especially since smaller effects for group interventions were found in an earlier analysis (Gutermann et al., 2016) and in adults (Lewis, Roberts, Andrew, Starling, & Bisson, 2020). Thus, a specific group factor might be at play such as a sense of community that supports trauma-focused work and subsequent healing in children and adolescents.

4.2. Secondary outcomes

Regarding secondary outcomes, the large uncontrolled effect of TF-CBT according to Cohen et al. on grief suggests that it can effectively address these symptoms. However, all relevant studies were either conducted in a group setting in low and middle income countries or undertaken by the working group of the treatment developers (Brown, Goodman, Cohen, Mannarino, & Chaplin, 2020; Cohen, Mannarino, & Knudsen, 2004; Cohen, Mannarino, & Staron, 2006; Dorsey et al., 2020; O'Donnell et al., 2014). Additionally, some studies did not define a minimum time criterion and possibly included participants with both normal and pathological grief reactions. Thus, this result may be partly explained by naturally occurring improvements over time and is difficult to generalize. Furthermore, compared to control conditions, only a small non-significant effect was found. There may be a threefold explanation for this result. First, all available comparators were TAU/AT conditions, and it may be that other treatments were equally effective in this domain. Second, grief baseline symptomatology was not clinically significant in one of the RCTs leaving almost no room for improvements in either condition. Third, no reliable instrument assessing grief in children and adolescents has been established up to now. So far, the evidence is still preliminary as only two RCTs and three uncontrolled studies have evaluated grief symptoms.

TF-CBT according to Cohen et al. showed reliable uncontrolled effects on depression and anxiety in the medium range from pre- to posttreatment and small effects compared to control conditions across all analyses. This confirmed earlier findings on the ability of TF-CBT according to Cohen et al. to alleviate these secondary symptoms, even though they were not targeted directly (Cohen, Deblinger, et al., 2004; Deblinger et al., 2006). In contrast to PTSS, we found no differences with regard to design, setting or comparators for these outcomes.

The analyzed sample confirmed that TF-CBT according to Cohen et al. could be effectively delivered to children and adolescents with single as well as multiple traumatic experiences. It effectively reduced pediatric PTSS in a relatively short number of sessions (Hansen, Lambert, & Forman, 2002) in settings with varying degrees of caregiver involvement, therapist training, and prior treatment experience of therapists. Thus, it is a perfect candidate for cost-effective D&I in settings with variable resources available. Furthermore, the included studies varied in terms of treatment duration, session length, and session frequency as well as treatment applications such as culture, telehealth, setting and additional elements. These are important factors for the treatment of affected minors with a wide range of traumas and specific care settings, underlining the flexibility of TF-CBT according to Cohen et al.

4.3. Limitations

We encountered several limitations whilst conducting our analyses. Most importantly, the indication of publication bias suggested that some studies with smaller effects may not have been published. We used appropriate methods to address this problem, but the true results of unpublished reports can, of course, never be known. It should also be mentioned that we may have missed studies that were not captured by our search terms. Additionally, some studies that would have matched our inclusion criteria ultimately had to be excluded due to unavailable data. Another major limitation was the inclusion of categorical rather than dimensional instruments in some of the older studies. This may have disguised ES and contrasting them with newer measures should be addressed in future studies. In addition, we did not run a formal analysis to confirm the large difference between ES in the subgroup analyses statistically. Moreover, due to the multitude of instruments used to assess the outcomes, no minimal symptom criterion was defined, leading to great variance in baseline symptomatology. Additionally, an instrument to assess pediatric grief reliably was not available. Furthermore, some analyses were limited to very few studies revealing some specific areas that warrant further attention.

5. Conclusion

TF-CBT according to Cohen et al. is an efficacious and effective treatment for pediatric PTSS with promising results on secondary outcomes of depression, anxiety, and grief. With regard to PTSS, it is superior to control conditions including TAU/AT, justifying its widespread use and recommendation in international guidelines as a first-line treatment (Forbes et al., 2020; National Institute for Health and Care Excellence, 2018; Phoenix Australia Centre for Posttraumatic Mental Health, 2013). Results also support further D&I in countries other than the US with group settings constituting a viable cost-effective and timesaving option when resources are limited. This is especially useful in the case of mass casualties involving large groups of young people such as Covid-19, wars around the world, the increasing frequency of natural disasters due to climate change, and terrorist attacks. TF-CBT according to Cohen et al. groups can be easily implemented and delivered via schools, child and youth welfare system facilities or other institutions. In practice, it may also be used to treat child and adolescent traumatic grief since the initial results are very promising, and the evidence for other grief-specific interventions is still relatively sparse (Boelen & Smid, 2017; Bui, 2018; Currier, Holland, & Neimeyer, 2007; Rosner, Kruse, & Hagl, 2010).

Future TF-CBT research should focus on group settings with a view to evaluating whether they do indeed produce stronger effects, and to providing treatment for large groups of people in a timely and cost-effective manner. In addition, we are in need of a reliable instrument to assess pediatric grief symptoms. RCTs in individual settings and western countries with different comparators are required to substantiate treatment effects on grief and to evaluate TF-CBT according to Cohen et al. in comparison with other grief-related treatments. Promising avenues may also be its application in telehealth to provide treatment during the pandemic and in rural areas where less mental health care is available. Moreover, offering TF-CBT according to Cohen et al. to inpatients may constitute a way of addressing the high rates of PTSD among this population. Additionally, providing treatment to young adults could expand the range of effective treatments available to this group.

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Declaration of competing interest

None.

Data availability

Data will be made available on request.

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Publication 2. Stability of treatment effects and caregiver-reported outcomes: A meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents

Title	Stability of treatment effects and caregiver-reported outcomes: A meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents
Authors	Jonathan Felix Benjamin Thielemann ^{1,2} Barbara Kasparik ¹ Julia König ¹ Johanna Unterhitzberger ^{1,3} Rita Rosner ¹
Affiliations	¹ Catholic University Eichstätt-Ingolstadt ² Alexianer Krefeld GmbH ³ Centre for Children and Adolescents Inn-Salzach e.V.
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Jonathan Felix Benjamin Thielemann^{1,2} , Barbara Kasparik¹, Julia König¹,
Johanna Unterhitzberger^{1,3}, and Rita Rosner¹

Abstract

The efficacy of trauma-focused treatments for children and adolescents is well researched. However, less is known about the long-term and caregiver-reported effects. Searched databases were PsychInfo, MEDLINE, Cochrane Library, PTSDpubs, PubMed, Web of Science, and OpenGrey. Treatment effects of trauma-focused cognitive behavioral therapy (TF-CBT) were computed at 12-month follow-up with posttraumatic stress symptoms (PTSS) as primary outcome and symptoms of depression, anxiety, and grief as secondary outcomes. Concordance between participant and caregiver ratings were investigated. TF-CBT showed large improvements across all outcomes from pre-treatment to 12-month follow-up (PTSS: $g = 1.71$, CI 1.27–2.15) and favorable results compared to active treatments and treatment as usual at 12-month follow-up (PTSS: $g = .35$, CI .13–.56). More pronounced effects were found in group settings. No significant differences were detected between participant and caregiver ratings with high reliability across almost all outcomes and assessment points. TF-CBT is a reliable treatment for pediatric PTSS and secondary symptoms with stable results at 12-month follow-up.

Keywords

adolescents, caregivers, child, meta-analysis, traumatic stress, longterm effects

Introduction

Rates of traumatic experiences are high among children and adolescents with one US study finding that half their sample had experienced a traumatic event in the last year (Finkelhor et al., 2013). In children exposed to traumatic events, a conditional prevalence rate of 15.9% is estimated for post-traumatic stress disorder (PTSD; Alisic et al., 2014). Symptoms of depression and anxiety are common concomitants (American Psychiatric Association, 2013) with research suggesting comorbid diagnoses in half of the pediatric PTSD cases (Kar & Bastia, 2006). According to international guidelines, trauma-focused cognitive behavioral therapy (TF-CBT) is the treatment of choice for pediatric PTSD with caregiver participation being an important treatment factor (Forbes et al., 2020; National Institute for Health and Care Excellence, 2018; Phoenix Australia Centre for Posttraumatic Mental Health, 2013).

In the pediatric TF-CBT literature, TF-CBT is used as a generic term for CBT with trauma-focused work as well as specifically for the manual of Cohen et al. (2006, 2017) in

order to make a clear distinction, the latter will be referred to as ‘specific TF-CBT’. In the context of pediatric TF-CBT treatments, specific TF-CBT is the most widely evaluated treatment protocol. It includes standard cognitive behavioral therapy (CBT) techniques, forming the acronym PRACTICE: psychoeducation and parenting skills (P), relaxation (R), affective modulation (A), cognitive coping (C), trauma narrative (T), in vivo exposure (I) conjoint parent-child sessions (C) and enhancing safety and development (E). According to the

¹Department of Psychology, Catholic University of Eichstätt-Ingolstadt, Ingolstadt, Germany

²Alexianer Krefeld GmbH, Hospital Maria-Hilf, Clinic for Mental Health, Department of General Psychiatry, Krefeld, Germany

³Centre for Children and Adolescents Inn-Salzach e.V., Department of Child and Adolescent Psychiatry, Altoetting, Germany

Corresponding Author:

Jonathan Felix Benjamin Thielemann, Department of Clinical and Biological Psychology Catholic, University of Eichstätt-Ingolstadt Levelingstr. 7, 85049 Ingolstadt 85049, Germany.

Email: jonathan.thielemann@ku.de

developers, at least eight sessions are necessary to cover all components with 45 minutes each assigned to the child and caregiver. Hence, caregiver involvement is essential in specific TF-CBT. For a more extensive overview, the reader is referred to our previous review and meta-analysis (Thielemann et al., 2022).

Although specific TF-CBT has been previously confirmed as an effective treatment (Cary & McMillen, 2012; Thielemann et al., 2022), little is known about the stability of treatment effects and the agreement between children and adolescents (self-report) and their caregivers (caregiver-report) regarding youths' symptoms in the context of specific TF-CBT. While the effectiveness of interventions is an important outcome, we also have to consider whether these effects can be sustained over time to choose treatments that achieve the best long-term outcomes for patients as well as the healthcare system. Furthermore, if we continue to use caregiver-reports as an outcome for pediatric posttraumatic stress symptoms (PTSS) and secondary symptoms in TF-CBT studies, we need to assess whether they reflect children and adolescents' experience or provide different information. Should the assessment not correspond, we also need to investigate the degree and direction of disagreement to understand their relation and their individual value.

Summary of Previous Analyses

As mentioned above, in the pediatric TF-CBT literature, TF-CBT is used as a generic term as well as specifically for the specific TF-CBT manual. Accordingly, next to the specific TF-CBT manual, most reviews and meta-analyses on TF-CBT included other trauma-focused CBT manuals such as EMDR (Shapiro, 2018), CBITS (Jaycox, 2018), KIDNET (Neuner et al., 2008), PE (Foa et al., 2019) and CPT (Resick et al., 2017) among several others (Hoogsteder, Thije, Schippers, & Stams, 2021; Lenz & Hollenbaugh, 2015; Lewey et al., 2018; Mavranezouli et al., 2020; Morina et al., 2016). Additionally, most of them did not include follow-up assessments (Hoogsteder et al., 2021; Lenz & Hollenbaugh, 2015; Lewey et al., 2018) or merged all follow-up assessment points covering different periods (Mavranezouli et al., 2020; Morina et al., 2016) and none considered concordance between self-reported and caregiver-reported outcomes.

The first systematic review on specific TF-CBT found positive small to medium effect sizes compared to active non-CBT control conditions for PTSS and depression at post-treatment (Cary & McMillen, 2012). At 12-month follow-up, the effect for PTSS was maintained but depression only yielded a small non-significant effect. The authors found the same pattern for variants of TF-CBT that did not strictly adhere to the manual. Unfortunately, studies with CBT control groups were excluded from analyses, limiting the analyses on specific TF-CBT to three randomized controlled trials (RCTs) at the time.

Apart from that, only two other meta-analyses on TF-CBT addressed follow-up assessments (Mavranezouli et al., 2020;

Morina et al., 2016). However, they did not conduct analyses for specific TF-CBT but included other manuals in their analyses (e.g. CPT, KIDNET, PE, CBITS). Mavranezouli et al. (2020) found a large positive post-treatment effect on PTSS compared to wait-list conditions. At 1–4-month follow-up, the effect was not only sustained but the large effect size increased further. In contrast, Morina et al. (2016) found a small positive post-treatment effect on PTSS compared to active treatments that disappeared at 3–24-month follow-up. However, these findings are difficult to compare due to the different follow-up periods and comparators used. Additionally, Morina et al. (2016) analyzed depression at 3–24-month follow-up. However, effect sizes could neither be calculated for wait-list nor active treatments but only for active control conditions containing psychoeducation, supportive counselling and treatment as usual. Compared to these control conditions, TF-CBT maintained a medium effect on depression.

To the best of our knowledge, only two meta-analyses have attempted to investigate concordance between self-reported and caregiver-reported symptoms in children and adolescents (Achenbach et al., 1987; Los Reyes et al., 2015). Both of them found low to moderate concordance between raters with somewhat greater agreement for externalizing than internalizing symptoms. This is mostly likely due to the subjective experience of internalizing symptoms that is directly only accessible by the individual (Asbrand et al., 2021; Los Reyes et al., 2015). Thus, for caregivers, they are more difficult to observe than externalizing symptoms. Additionally, children and adolescents with more internalizing symptoms might have been more withdrawn and interacted less with their caregivers, providing fewer situation for caregivers to recognize their problems (Bass et al., 2014). Concerning PTSS, most studies that have investigated concordance between children and adolescents and their caregivers also showed limited concordance with caregivers reporting a lower symptom load (Exenberger et al., 2019; Meiser-Stedman et al., 2007; Scheeringa et al., 2006; Schreier et al., 2005; Shemesh et al., 2005; Stover et al., 2010). However, some studies indicate that the reports tend to converge over time (Meiser-Stedman et al., 2007; Schreier et al., 2005). While discrepancies between reporters were often discussed in terms of measurement error, they can also be considered as the unique perspectives of different observers and the context-specific symptom occurrence (Los Reyes et al., 2015). That is, some behaviors may only be observable in a specific context (e.g. with peers) or only recognized by children and adolescents or their caregivers. In this sense, discrepancies are different yet valid information that can greatly assist diagnosis and treatment decisions. Interestingly, some studies from populations with physical illnesses such as cancer (Clawson et al., 2013; Erickson et al., 2017; Phipps et al., 2005) and epilepsy (Stevanovic et al., 2012) found significant moderate to high correlations between self-reported and caregiver-reported PTSS, depression and anxiety as well as no mean difference between raters. This contrary finding may be explained by a

greater awareness and involvement of parents in their children's health and treatment in this population. As parallel sessions with caregivers are an important component of specific TF-CBT, this effect may also be present. However, concordance between children and adolescents and their caregivers regarding youths' symptoms in the context of specific TF-CBT has not been investigated by meta-analysis yet.

Current Study

Our previous analysis confirmed the ability of specific TF-CBT to reduce PTSS and comorbid symptoms, as well as its superiority to other treatment approaches (Thielemann et al., 2022). These results were in line with earlier findings on specific TF-CBT (Cary & McMillen, 2012; Gutermann et al., 2016) and the broader TF-CBT literature (Lenz & Hollenbaugh, 2015; Mavranouzouli et al., 2020; Morina et al., 2016). In contrast to previous analyses (Gutermann et al., 2016; Singal et al., 2014), we also found greater effects on PTSS in group settings and effectiveness trials. However, we only assessed symptoms post-treatment and restricted outcomes to self-report and clinical interviews. Thus, it is still unclear how stable these effects are and how well the reported symptoms concur with the experience of the caregivers in specific TF-CBT who interact with the children and adolescents on a day-to-day basis and are an integral part of the treatment. In light of these results and the very few meta-analyses on follow-up periods as well as the absence of meta-analyses of concordance between self-reported and caregiver-reported pediatric PTSS and secondary outcomes in the context of specific TF-CBT, an investigation into these issues is warranted. This meta-analysis quantifies the treatment effects of specific TF-CBT from pretreatment to 3-month follow-up (FU I), 6-month follow-up (FU II) and 12-month follow-up (FU III), from FU II to FU III as well as in comparison to control conditions at the follow-up assessment points for PTSS and secondary outcomes of depression, anxiety and grief. FU III was defined as the primary endpoint as the other follow-ups were assessed relatively early after treatment and we expected fewer studies to be included in these analyses. Nevertheless, we included the earlier follow-up periods to cover as many studies as possible and to have a closer look at the course of symptoms over time. RCTs, individual and group settings as well as effectiveness and efficacy trials will be considered separately for the primary endpoint.

Methods

Search and Screening of Studies

To ensure comparability, this meta-analysis used the same search terms as in our previous analysis (Thielemann et al., 2022; PROSPERO: CRD42020139403). Databases included PsycInfo, MEDLINE, Cochrane Library, PTSDpubs, PubMed and

Web of Science as well as OpenGrey and were searched with a pre-defined combination of search terms for articles published between Jan 1st, 1990 to Aug 19th, 2021 (see Table 1). In addition, we conducted a manual search of reference sections of relevant works and sought expert suggestions, resulting in 1262 publications without duplicates (see Figure 1). No language limitations were applied. Covidence (Veritas Health Innovation, 2014) was used for title and abstract screening with two independent raters (JT and BK) assessing the articles. Conflicting assessments were solved in discussion by reviewing the abstracts. Full-text readings and assessments of inclusion and exclusion criteria were conducted by the first author. If assessments were inconclusive, authors were contacted and articles were discussed with the co-authors to resolve the issues.

Inclusion and Exclusion Criteria

The following inclusion criteria were originally applied to TF-CBT studies conducted in individual settings: (1) age range of 3–21 years, (2) participants had been exposed to at least one traumatic experience, and (3) had to complete at least 8 sessions (4) of specific TF-CBT (Cohen et al., 2006; 2017) or earlier versions of the same manual (Cohen & Mannarino, 1993; Deblinger & Heflin, 1996). Additionally, (5) results on PTSS were assessed pre and post-treatment with a quantitative self-report measure or clinical interview. (6) Moreover, only original research was considered, excluding reviews, meta-analyses, and case reports. For the present meta-analysis, we added an additional criterion: Either (7a) studies had to include at least one follow-up assessment of PTSS, depression, anxiety or grief reported via self-report or clinical interview, or (7b) include caregiver-reported outcome measure for PTSS, depression, anxiety or grief assessed at least pre and post-treatment. The only exclusion criterion was if children did not receive treatment (e.g. only parents were treated). If PTSS pre-post effect sizes could not be extracted (c.f. criterion 5), we approached the authors and included the article if supplementary data was supplied.

The inclusion criteria for TF-CBT studies conducted in group settings differed only slightly: no minimum number of sessions was required, and treatment did not need to include all PRACTICE components. Instead, it needed to contain (1) psychoeducation, (2) coping strategies (i.e. relaxation, affective modulation, cognitive coping), (3) exposure, (4) cognitive processing/restructuring of trauma-related thoughts and beliefs and (5) some reference to the manual or its earlier versions.

Treatment and Control Groups

Control groups consisted of randomized wait list, treatment as usual (TAU) and active treatment (AT) conditions. TAU and AT were merged as interventions were comparable. If two TAU/AT control groups were reported, we used the higher treatment dose. If two TF-CBT conditions were reported, we included

Table I. Pre-Defined Search Terms.

Search Categories	Search Terms
Diagnosis	Trauma* or posttrauma* or post-trauma* or PTSD or PTSS or grief or griev*
Trauma-related	Abuse* or assault* or abduct* or accident* or kidnapp* or life-threat* or maltreat* or mistreat* or neglect* or refugee or shooting or terroris* or victim* or violence or war or hurricane or tsunami or earthquake or flood or "natural disaster" or bereave* or loss
Youth	Adolescen* or child* or youth or kid or juvenile or infant or minor or teenager or young*
TF-CBT	"Trauma focused cognitive behavioral treatment" or "trauma-focused cognitive behavioral treatment" or "trauma focused cognitive behavioral therapy" or "trauma-focused cognitive behavioral therapy" or "trauma focused cognitive behavior*" or "trauma-focused cognitive behavior*" or "trauma focused cog*" or "trauma-focused cog*" or "trauma focused" or trauma-focused or TF-CBT or grief-focused or "grief focused"

Note. Combination: (Diagnosis or Trauma-related) and Youth and TF-CBT.

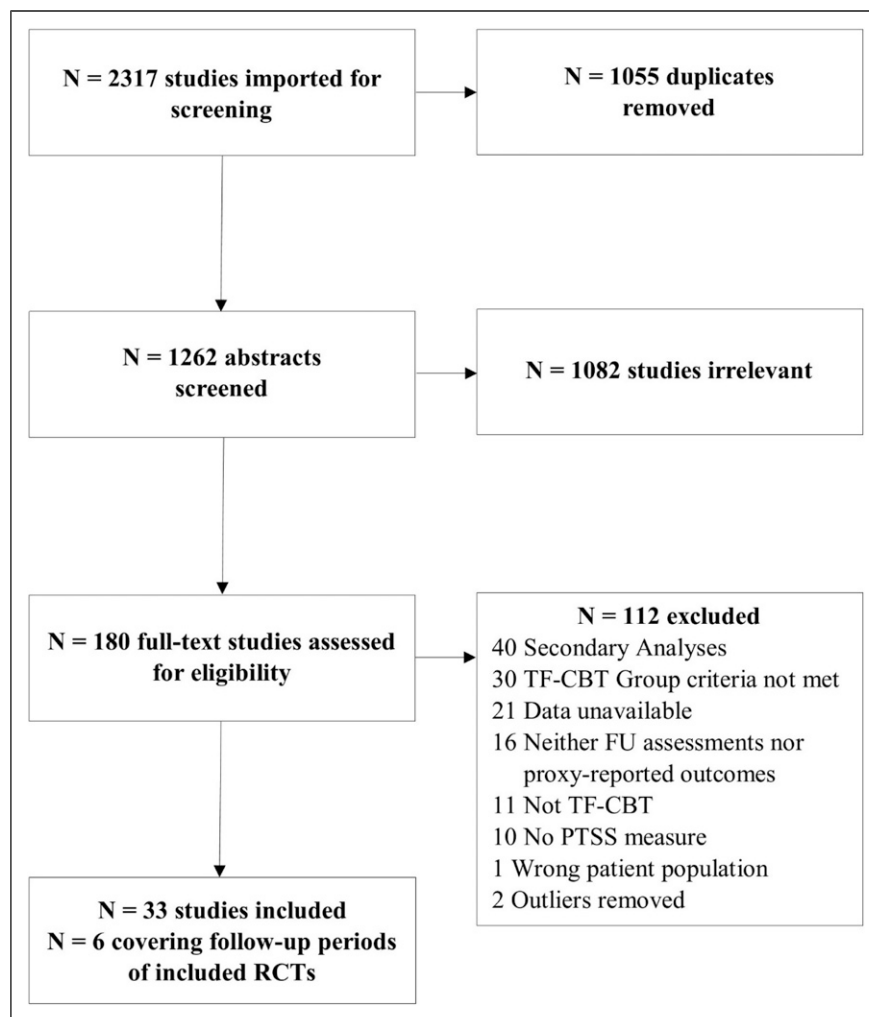


Figure 1. Flowchart study selection. Note. The two outliers refer to the same study covering the follow-up period.

them both in the pre-post analysis unless results were merged in the original report. If a control group was reported as well, it was compared to both TF-CBT conditions, separately. One study used a combination of four TF-CBT conditions and four

control groups that were comparable regarding the treatment dose (Dorsey et al., 2020). These control groups were paired with the corresponding TF-CBT condition as data collection was conducted in different countries and regions.

Efficacy and Effectiveness

Efficacy trials take place in highly controlled settings to increase internal validity while in effectiveness trials, treatments are implemented directly in the health care system (Singal et al., 2014). We distinguished the two by adapting the classification criteria from Gartlehner et al. (2006) considering study title, settings, inclusion criteria, analyses, adaptations, therapists, caregiver participation, as well as control conditions. The detailed criteria can be found in S6. The first and second authors (JT and BK) independently assessed all studies and solved inconsistent ratings in discussion. However, an unequivocal distinction is sometimes difficult as efficacy and effectiveness trials function on a continuum and may include some characteristics of the other trial type. Decisions were made depending on which criteria preponderated.

Risk of Bias Assessment

The first and second author (JT and BK) assessed risk of bias with the Risk of Bias assessment tool (RoB 2.0; Sterne et al., 2019) and the Risk Of Bias In Non-randomized Studies – of Interventions (ROBINS-I) assessment tool (Sterne et al., 2016), resulting in ‘low risk’, ‘some concerns’ or ‘high risk’ ratings. The tools present five (Rob 2.0) to seven (ROBINS-I) domains to assess risk of bias. For the latter, only four domains were used since the concerns of the other domains were covered by the inclusion criteria and we did not include non-randomized control groups. Four RCTs and seven uncontrolled studies were identified as ‘high risk’ studies and excluded from analyses. For a more detailed overview, the reader is referred to our previous meta-analysis (Thielemann et al., 2022).

Outcomes and Data Extraction

All data were extracted by the first and second authors including outcome data on PTSS, depression, anxiety, and grief as well as caregiver-reported outcomes on PTSS, depression, anxiety and grief. Clinical interviews were the first choice of participants’ data. If unavailable, we used self-report instruments instead. When appropriate, we merged subscales using the Cochrane formula (Higgins et al., 2022). We contacted authors for missing information and excluded studies from the originally selected sample (Thielemann et al., 2022) if no outcome data could be acquired for follow-up or pre- to post-treatment caregiver-reported outcomes.

Statistical Analyses. All eligible studies were included in respective analyses generating effect sizes (Hedges’ g and 95% CIs) for PTSS, depression, anxiety and grief. We used Comprehensive Meta-Analysis, version 3 (Borenstein et al., 2013) for meta-analyses and IBM SPSS Statistics (Version 25) for computing two-way mixed method absolute agreement intra class coefficients and t-tests ($\alpha = .05$). If completer data and intention-to-treat data were reported, the

latter were our first choice. If necessary, we imputed correlations for pre-post effect sizes based on the overall mean of available correlations for the respective outcome from our first meta-analysis and assumed the same for later assessment points. As we were able to include fewer studies than in our original meta-analysis, we also calculated pre-post (within-group) and post (between-group) effect sizes to ensure effects were comparable to our previous results. We assumed a random-effects model as there was a lot of heterogeneity in the samples (Hedges & Vevea, 1998) and confirmed it with Q statistic (Higgins et al., 2003). When Q was non-significant, we accepted a fixed model, and reported effect sizes accordingly. I^2 was used to estimate heterogeneity. For the detection of outliers (Hedges’ $g > 4$), we used funnel plots. We performed additional subgroup analyses when at least three TF-CBT conditions or three post-treatment comparisons were available and the respective counterpart (individual and group; efficacy and effectiveness) could be calculated as well. We detected publication bias in some analyses and addressed it with Duval and Tweedie’s (2000) trim and fill method. We computed intraclass correlations between self-reported and caregiver-reported outcomes at the different assessment points and compared ratings using t-tests.

Results

Study selection is shown in Figure 1. An overview of all included studies can be found in the Supplementary Material S1. We ultimately selected 33 studies of which 21 were RCTs and 12 were uncontrolled studies. Most studies used an individual treatment setting ($k = 26$, 15 of them RCTs) and some were conducted in a group setting ($k = 7$, six of them RCTs). Of the RCTs, 11 were efficacy trials and 10 were effectiveness trials. While 20 studies ($k = 16$ RCTs) were considered for follow-up analyses of self-reported outcomes, 21 studies ($k = 9$ RCTs) were included in the concordance analyses of self-reported and caregiver-reported outcomes. Wait list control conditions were included in six RCTs and TAU/AT control conditions in 11 RCTs. In three RCTs, the control condition was a second TF-CBT condition, which we used as an additional treatment condition. In the remaining RCT, the control condition was excluded as some participants received TF-CBT while others did not. Thus, it represented neither a viable control groups nor a clear-cut TF-CBT condition. The within-group and between-group effect sizes for the primary endpoint (FU III) can be found in Table 2 and Table 3, respectively. High risk of bias studies were excluded from all analyses if not otherwise noted. Table 4 shows the effect sizes for caregiver-reported outcomes. Means, SDs, intraclass correlations and t-statistics for self-reported and caregiver-reported outcomes can be found in Table 5. For the effect sizes of individual studies and the funnel plots showing observed and imputed studies, see supplementary material S2. Uncontrolled effect sizes for intermediate follow-up

Table 2. Pre-FU III and FU II-FU III Within-Group Effect Sizes for PTSS, Depression, Anxiety and Grief.

Assessment Points	Outcome	Sample	n	g	95% CI	Se	z	Q	I ²	Duval and Tweedie	
Pre-FU III	PTSS	All studies	13	(1.98) 1.71 ^a	(1.53–2.43)	1.27–2.15	(.23)	(8.66***)	117.65***	89.80	3
		Individual	8	1.63	1.07–2.19		.29	5.67***	61.76***	88.67	0
		Group	5	2.47 ^b	2.25–2.69		.11	21.81***	6.87	41.79	0
	RCTs only		12	(1.93) 1.71 ^a	(1.46–2.39)	1.24–2.18	(.24)	(8.08***)	109.86***	89.99	2
		Efficacy	6	1.69	.80–2.57		.46	3.74***	60.95***	91.80	0
		Effectiveness	6	(2.16) 2.00 ^a	(1.69–2.62)	1.53–2.47	(.24)	(9.10***)	31.86***	84.31	1
		Individual	8	1.63	1.07–2.19		.29	5.67***	61.76***	88.67	0
		Group	4	2.44 ^b	2.20–2.68		.12	19.91***	6.41	53.18	0
		Depression	All studies	9	.99	.70–1.29		.15	6.55***	37.48***	78.65
	RCTs only	8	.90	.63–1.17		.14	6.46***	23.86**	70.66	0	
	Anxiety	All studies	6	.95	.55–1.35		.20	4.69***	29.84***	83.25	0
		RCTs only	6	.95	.55–1.35		.20	4.69***	29.84***	83.25	0
	Grief	All studies	6	(1.73) 1.44 ^a	(1.29–2.18)	.94–1.94	(.23)	(7.62***)	28.67***	82.56	2
RCTs only		5	(1.61) 1.44 ^a	(1.12–2.10)	.91–1.97	(.25)	(6.47***)	24.24***	83.50	1	
FU II-FU III	PTSS	All studies	7	.20 ^b	.07–.33		.07	3.05**	1.68	0	0
	Depression	All studies	7	.09 ^b	–.04–.21		.06	1.37	3.25	0	0
	Anxiety	All studies	5	.12	–.02–.26		.07	1.70	3.65	0	0

Note. High risk of bias studies are excluded; n = number of included TF-CBT conditions. *p < .05; **p < .01; ***p < .001.

^aRecalculated with imputed studies (trim and fill method according to Duval and Tweedie).

^bFixed model assumed due to non-significant Q-value.

Table 3. FU I-III Between-Group Effect Sizes for PTSS, Depression, Anxiety and Grief.

Assessment Point	Outcome	Sample	n	g	95% CI	Se	z	Q	I ²	Duval and Tweedie	
FU I	PTSS	Any control	4	(.39 ^{a,b}) .28 ^{a,b,c}	(.04–.73)	(–.03 - .60)	(.18)	(2.20*)	2.59	0	1
	Depression	Any control	4	.20 ^{a,b}	–.14–.54		.17	1.14	.62	0	0
FU II	PTSS	TAU/AT	7	.05	–.30–.41		.18	.30	16.70**	64.07	0
	Depression	TAU/AT	6	.17 ^a	–.04 –.37		.10	1.63	5.39	7.17	0
	Anxiety	TAU/AT	4	.10 ^a	–.14–.33		.12	.81	4.47	32.82	0
FU III	PTSS	TAU/AT	11	.35	.13–.56		.11	3.11**	28.90**	65.40	0
		Efficacy	6	.32 ^a	.11–.52		.11	3.01**	3.37	0	0
		Effectiveness	5	.39	.02–.76		.19	2.08*	25.15***	84.10	0
		Individual	7	.29 ^a	.10–.47		.09	3.05**	3.79	0	0
	Group	4	.45	.00–.89		.23	1.96*	23.66***	87.32	0	
	Depression	TAU/AT	7	.14 ^a	–.05–.32		.09	1.47	1.76	0	0
	Anxiety	TAU/AT	5	.18 ^a	–.02–.38		.10	1.76	5.11	21.77	0
	Grief	TAU/AT	5	.33	–.02–.67		.18	1.85	16.60**	75.91	0

Note. High risk of bias studies are excluded; n = number of included comparisons; PTSS = Posttraumatic Stress Symptoms; TAU/AT = Treatment as usual/active treatment control conditions; Effectiveness = Effectiveness RCTs only; Efficacy = Efficacy RCTs only; Individual = RCTs conducted in an individual treatment setting; Group = RCTs conducted in a group setting. *p < .05; **p < .01; ***p < .001.

^aFixed model assumed due to non-significant Q-value.

^bAnalysis includes high risk of bias studies (calculation otherwise not possible due to low number of studies).

^cRecalculated with imputed studies (trim and fill method according to Duval and Tweedie).

analyses (pre to FU I and FU II) as well as uncontrolled pre-post and controlled post-treatment effect sizes for the respective studies involved in the follow-up analyses can be found in S3. Risk of bias assessments and analyses including high risk studies can be found in S4 and S5, respectively.

Participant-reported Outcomes

PTSS within-Group Effects. Within TF-CBT, uncontrolled effect sizes for PTSS were large from pre to all follow-up assessment points (FU I: g = 1.63, CI 1.22–2.04; FU II: g = 1.65, CI 1.07–2.24; FU III: g = 1.71, CI 1.27–2.15). In comparison to the pre-

Table 4. Within-Group and Between-Group Effect Sizes for Caregiver-Reported PTSS, Depression and Anxiety.

Analysis	Assessment Point	Outcome	n	g	95% CI	Se	z	Q	I ²	Duval and Tweedie
Within-group	Pre-post	PTS	18	1.15	.86–1.44	.15	7.68***	177.87***	90.44	0
		Depression	7	(.57) .50 ^a	(.33–.82) .23–.76	(.13)	(4.58***)	14.79*	59.44	1
		Anxiety	3	.52 ^{b,c}	.34–.71	.09	5.55***	.08	0	0
	Pre-FU I	PTSS	4	(.89) .71 ^a	(.45–1.33) .22–1.21	(.23)	(3.95***)	12.69**	76.35	1
		Depression	3	.57 ^b	.36–.78	.11	5.31***	5.10	60.76	0
	Pre-FU II	PTSS	3	.88 ^c	.30–1.46	.09	2.96**	12.86**	84.45	0
Pre-FU III	PTSS	6	2.02	1.34–2.69	.34	5.86***	77.37***	93.54	0	
Between-group	Post	PTSS	7	.59	.19–.98	.20	2.89**	50.62***	88.15	0
		Depression	3	.31 ^b	–.02–.64	.17	1.87	3.58	44.14	0

Note. High risk of bias studies are excluded; n = number of included comparisons; PTSS = Posttraumatic Stress Symptoms.

*p < .05; **p < .01; ***p < .001.

^aRecalculated with imputed studies (trim and fill method according to Duval and Tweedie).

^bFixed model assumed due to non-significant Q-value.

^cAnalysis includes high risk of bias studies (calculation otherwise not possible due to low number of studies).

Table 5. Descriptive Statistics, Intraclass Correlations and t-Statistics for Self-Reported and Caregiver-Reported Outcomes.

Outcome	Assessment Points	Assessor	n	M	Sd	r	t	p
PTSS	Pre	Participants	28	33.01	8.67	.74	1.10	.28
		Caregivers		30.10	11.02			
	Post	Participants	28	19.65	8.98	.94	.42	.68
		Caregivers		18.55	10.76			
	FU I	Participants	4	13.78	6.56	.95	.07	.95
		Caregivers		13.45	7.84			
FU III	Participants	6	11.08	7.45	.99	.37	.72	
	Caregivers		9.47	7.66				
Depression	Pre	Participants	10	10.46	1.58	.38	1.91	.07
		Caregivers		8.81	2.24			
	Post	Participants	10	6.43	2.62	.91	.90	.38
		Caregivers		5.43	2.36			
	FU I	Participants	3	5.52	4.23	.99	.05	.96
		Caregivers		5.35	3.95			
Anxiety	Pre	Participants	3	23.91	15.72	.89	.55	.61
		Caregivers		17.89	10.68			
	Post	Participants	3	14.63	8.87	.97	.36	.74
		Caregivers		12.23	7.39			

Note. n = number of included self-reported and caregiver-reported means; r = intraclass correlation between self-reported and caregiver-reported outcome at the respective assessment point.

post effect sizes of the respective studies involved in the analyses, effect sizes were stable and even increased at FU II and FU III. In individual settings at the primary endpoint (FU III), the large effect size (g = 1.63, CI 1.07–2.19) was only slightly smaller than the overall effect size as all but two studies were conducted in individual settings. Accordingly, effect sizes for group settings came from two studies only, showing a large effect (g = 2.47, CI 2.25–2.69) that is considerably greater than the overall effect size and the effect size in individual studies. The results for RCTs were, again, almost identical (g = 1.71, CI 1.24–2.18) to the overall and individual settings results as only two of the included studies were uncontrolled and all but one RCT were conducted in an individual setting. Effectiveness (g = 2.00, CI 1.53–2.47) and

efficacy (g = 1.69, CI .80–2.57) trials both showed large effect sizes with effectiveness studies showing a somewhat greater effect size. In addition, a direct comparison between follow-up assessment points was possible from FU II to FU III showing a small significant increase in the effect size (g = .20, CI .07–.33). For this analysis, all eligible studies were RCTs. The overlap of studies included in FU I and later assessment points was limited to one and two studies, respectively and thus effect sizes could not be calculated.

Secondary Outcomes within-Group Effects. Across all follow-up assessment points within TF-CBT, effect sizes were medium to large for depression (FU I: g = .89, CI .54–1.24; FU II: g = .60, CI .48–.73; FU III: g = .99, CI .70–1.29) and small to large for

anxiety (FU I: $g = .38$, CI .17–.59; FU II: $g = .82$, CI .52–1.11; FU III: $g = .95$, CI .55–1.35). However, the small effect size for anxiety at FU I relied on three studies only and needed to include studies with a high risk of bias in order to achieve a sufficient number of comparisons for analysis. We were able to calculate grief only at FU III and found a large effect size ($g = 1.44$, CI .94–1.94). Compared to pre-post effect sizes of the studies involved in the analyses, effect sizes were stable and even increased at FU I (depression), FU II (depression and anxiety) and FU III (depression and anxiety). From FU II to FU III, no significant change was observable for secondary outcomes.

PTSS Between-Groups Effects. In comparison to control groups, effect sizes for PTSS were in favor of TF-CBT and small across follow-up assessment points (FU I: $g = .28$, CI -.03 - .60; FU II: $g = .05$, CI -.30 - .41; FU III: $g = .35$, CI .13–.56). However, the difference was not significant at FU II. It should also be noted that at FU I, only two studies contributed effect sizes of which one was a high risk of bias study using a wait list condition as comparator. It was included in the analysis to reach a sufficient number of comparisons. All other analyses used TAU/AT control groups only. Compared to the post-treatment effect size of the respective studies included in the analyses, the effect size decreased at FU I and FU II but was stable at FU III. At FU III, the small effects were paralleled in individual settings ($g = .29$, CI .10–.47), group settings ($g = .45$, CI .00–.89), efficacy ($g = .32$, CI .11–.52) and effectiveness ($g = .39$, CI .02–.76) trials.

Secondary Outcomes Between-Groups Effects. In comparison to control groups across all follow-up assessment points, effect sizes were small and non-significant for depression (FU I-III), anxiety (FU II-III) and grief (FU III). Interestingly, grief was approaching significance ($p = .06$). However, for the studies involved in the follow-up analyses, non-significant differences were already observable at post-treatment in some instances. In terms of effect sizes, follow-up effects were comparable to post-treatment, except for depression showing somewhat smaller effect sizes at FU II and FU III.

Caregiver-Reported Outcomes

Within TF-CBT, effect sizes for PTSS were large at post-treatment ($g = 1.15$, CI .86–1.44) and at the primary endpoint (FU III: $g = 2.02$, CI 1.34–2.69). At FU I (FU I: $g = .71$, CI .22–1.21) and FU II ($g = .88$, CI .30–1.46), within-group effect sizes were somewhat smaller but relied on fewer studies. For FU II, analysis had to include high risk of bias studies to achieve a sufficient number of comparisons. Considering the pre-post effect sizes of the respective studies involved in the follow-up analyses, caregiver-reported effects were stable at FU I and FU II and increased at the primary endpoint. For the secondary outcomes, within-group effect sizes were medium across the available assessment points and outcomes (depression post: $g = .50$, CI .23–.76; FU I: $g = .57$, CI .36–.78;

anxiety post: $g = .52$, CI .34–.71). The within-group effect size for depression at FU I was stable when compared to the pre-post effect size of studies involved in the analysis. For grief, no caregiver-reports were reported.

For caregiver-reported outcomes, between-group effect sizes could only be computed at post-treatment. In comparison to control conditions, a medium effect size was found for PTSS supporting TF-CBT. For depression, a small effect size was found in favor of TF-CBT. However, it was only approaching significance ($p = .06$).

Concordance of Self-Reported and Caregiver-Reported Outcomes. Looking at the agreement between participants and their caregivers, t-tests showed no significant differences between self-reported and caregiver-reported outcomes across assessment points. However, depression was approaching a significant difference at baseline ($p = .07$) and a trend was visible with caregivers consistently producing scores somewhat lower than participants across outcomes. This gap between raters narrowed over time as participants improved. In terms of intraclass correlations, reliability was excellent for most assessment points but tended to be weaker at baseline (PTSS: $r = .74$; depression: $r = .38$; anxiety: $r = .89$) and greater at post-treatment (PTSS: $r = .94$; depression: $r = .91$; anxiety: $r = .97$), FU I (PTSS: $r = .95$; depression: $r = .99$) and FU III (PTSS: $r = .99$). Notably, reliability was poor for depression at baseline. The follow-up analyses and anxiety analyses should be interpreted with caution due to the substantially lower number of included studies. Furthermore, we included high risk of bias studies since we believed risk of bias to be equal for participant and caregiver outcomes and thus not affecting their relationship.

Discussion

With this meta-analysis, we evaluated the stability of treatment effects of specific TF-CBT from pre to 12-month follow-up and assessed caregiver-reported outcomes as well as their concordance with self-reports. This closes two important gaps in the literature, as it is the only recent meta-analysis for specific TF-CBT on long-term outcomes and the first on caregiver-reports in this context. Additionally, intermediate follow-up assessment points were considered and sub-group analyses were performed for RCTs, individual and group settings as well as effectiveness and efficacy trials at 12-month follow-up. Results showed firm support for specific TF-CBT and indicated high concordance between self-reported and caregiver-reported outcomes. For PTSS, effects were stronger than for secondary outcomes and in comparison to efficacy trials and individual settings, effects were more pronounced in effectiveness trials and group settings. Furthermore, specific TF-CBT outperformed treatment as usual and active treatments with regard to PTSS but not secondary outcomes. Results for RCTs only were highly similar to the overall results.

PTSS

To the best of our knowledge, no other meta-analysis has investigated uncontrolled effect sizes for specific TF-CBT or similar variants at follow-up. The large uncontrolled effect sizes found from pre to all follow-up assessment points confirmed the stability of treatment effects. Moreover, considering the pre-post effect sizes of the studies involved in the respective analyses, we observed further gains at later assessment points and even from FU II to FU III, supporting the notion that some treatment effects may unfold over time (Tutus et al., 2017).

At the primary endpoint, we found a small controlled effect size favoring TF-CBT compared to TAU/AT conditions. This fits the previous analysis of specific TF-CBT (Cary & McMillen, 2012) but not that of TF-CBT therapies (Morina et al., 2016) which found no significant differences to active treatments. In line with the latter analysis, we also did not observe a significant difference at the FU II intermediate assessment point. However, FU II included fewer comparisons and all but one came from studies using individual treatment settings, which also showed smaller effects at the primary endpoint and in our previous analysis (Thielemann et al., 2022). Thus, the FU II intermediate assessment point was less robust and more homogenous with regard to treatment setting than the primary endpoint. In addition, the findings reported by Morina et al. (2016) are not directly comparable to our results since their analysis combined follow-up periods between 3 and 24 month. In light of the results for the uncontrolled effect sizes, a possible explanation could be that at the earlier follow-up assessment points, TF-CBT may not have fully taken effect yet with TAU/AT conditions temporarily catching up. Unfortunately, the small controlled effect size at FU I could not be compared to earlier findings since none included a mixture of wait list and TAU/AT conditions. We also could not analyze wait list conditions at any follow-up assessment point, since most of them naturally expired. Besides, we are aware of only two studies assessing even longer follow-up periods than our primary endpoint but unfortunately, data was insufficient for meta-analysis (Deblinger et al., 1999; Jensen et al., 2017). In sum, the small effect size at FU III supporting specific TF-CBT over AT/TAU conditions emphasizes its advantage over other treatments as these control groups may themselves be very effective (Frost et al., 2014).

In contrast to the assumption that results from efficacy trials are difficult to transfer into practice (Singal et al., 2014), we found a somewhat greater effect size for effectiveness trials. Additionally, contrary to earlier results (Gutermann et al., 2016), we observed a greater effect size in group settings than individual settings. We discussed this phenomenon in detail in our previous meta-analysis (see Thielemann et al., 2022). In short, specific TF-CBT efficacy trials have so far mostly included studies conducted in an individual setting while effectiveness trials more often included studies conducted in

group settings. Group settings showed stronger effects that may explain the counterintuitive difference in favor of effectiveness trials. The stronger effects for group settings can be partly explained by higher baseline symptomatology in underserved populations. However, this cannot fully account for these greater effects and we hypothesized that a specific group factor might be at play that may favor trauma-focused work and consequent recovery in young people. That this pattern of results is also observable at FU III underlines that specific TF-CBT in group settings is a cost-effective time-saving option, especially if resources are limited (Dorsey et al., 2020). It may be used to address mass casualty events affecting many children and adolescents such as pandemics, wars, natural disasters and terrorist attacks.

Secondary Outcomes

Again, this meta-analysis is unique in investigating uncontrolled effect sizes for specific TF-CBT at follow-up for secondary outcomes of depression, anxiety and grief. Stable treatment effects were confirmed by the small to large uncontrolled effect sizes found from pre to all follow-up assessment points. Regarding the effect sizes from pre to the primary endpoint only, effect sizes were large for all secondary outcomes. Considering the pre-post effect sizes of the studies involved in the respective analyses, further gains were observed at later assessment except from FU II to FU III. This finding further supports the argument that participants continue to improve after treatment, also for secondary outcomes (Cohen et al., 2005; Tutus et al., 2017).

Concerning controlled effect sizes of secondary outcomes, only depression was investigated by previous meta-analyses. Our finding of a small non-significant controlled effect sizes compared to TAU/AT conditions at the primary endpoint is in line with the results of Cary and McMillen (2012) who also found a small non-significant effect for depression at 12-month follow-up compared to active non-CBT conditions. In contrast, Morina et al. (2016) found a medium effect size at 3–24-month follow-up in favor of TF-CBT in comparison to active control conditions (i.e. not active treatments). As stated above, it is difficult to draw a direct comparison between studies due to the differing follow-up periods and the other manuals that were included in their analysis. In addition, the greater effect size can be explained because active treatments were not part of the control condition.

Nevertheless, the disappearance of all significant controlled effect sizes for all secondary outcomes at all follow-up assessment points in our analyses was rather surprising considering the strong pre to FU III effects as well as the post-treatment effects in favor of specific TF-CBT in our earlier analysis (Thielemann et al., 2022). One possible explanation is that the study sample was different in the present analysis. While we drew them from the same pool as our previous analysis, fewer studies included follow-up assessment points and control groups, making it more difficult to detect

significant effects. Supporting this hypothesis, the studies with follow-up assessments, while reporting effect sizes comparable to our previous analyses, typically had fewer significant effects. However, another explanation could be that other treatments are simply equally effective in the long run with regard to secondary symptoms. In addition, TF-CBT mainly targets PTSS and thus smaller effects can be expected for secondary outcomes. Interestingly, grief was approaching significance at the primary endpoint warranting further investigation as the available evidence came from two studies only and no reliable pediatric grief instrument was available.

Caregiver-Reported Outcomes

In terms of effect sizes, caregiver-reports paralleled the effects found in the pre-post and pre-FU III analyses of participants for all outcomes and were also concordant with participant outcomes in our previous analysis (Thielemann et al., 2022). The controlled post-treatment effect sizes against any control groups were also comparable to our previous results. In line with that, we found no significant differences between raters and their reliability was high across outcomes and assessment points. This finding is contrary to earlier meta-analyses that only found low to moderate agreements between caregivers and their children and adolescents (Achenbach et al., 1987; Los Reyes et al., 2015). Nevertheless, some individual studies in children and adolescents with physical illnesses found similar results (Clawson et al., 2013; Erickson et al., 2017; Phipps et al., 2005; Stevanovic et al., 2012). One explanation could be that caregivers are more aware of their children's and adolescents' mental health concerns and actively seek treatment for them. However, it could also be that caregivers strongly attribute problems to their children and adolescents or even report greater symptoms to emphasize treatment needs (Asbrand et al., 2021). Some literature also suggests that greater parental distress and trauma symptoms also result in more symptoms being reported for their children (Exenberger et al., 2019; Schreier et al., 2005; Shemesh et al., 2005). However, since we found that reports converged over time with the treatment being completed and symptoms decreasing, this suggests that the greater concordance between raters in specific TF-CBT may be facilitated by caregivers being directly engaged in treatment. Thereby, they may gain awareness for their youth's symptoms and possibly learn techniques that also help them to deal with their own symptomatology. Furthermore, as a consequence of treatment participation, caregivers' greater understanding of PTSS may help them accommodate their children's behaviors given their condition.

In line with that, there was one exception to high rater agreement with depression showing poor reliability at baseline and excellent reliability at later assessment points. At baseline, the difference between raters was close to significance, suggesting that caregivers might not be suitable to rate their children's and adolescents' depression before treatment. In other words, caregivers were not fully aware of the extent of

depressive symptoms the young people experienced prior to treatment initiation. This is in line with the literature suggesting that caregivers have more difficulties to assess internalizing symptoms as they are often more difficult to observe (Achenbach et al., 1987; Los Reyes et al., 2015).

Although concordance between self-reports and caregiver-reports was high, we based our analysis on total scores. Thus, on a diagnostic level, we did not evaluate concordance with regard to symptom clusters and diagnoses as these information were mostly unavailable. Earlier studies found that diagnostic agreement was often limited (Choudhury et al., 2003; Grills & Ollendick, 2003; Kassam-Adams et al., 2006; Meiser-Stedman et al., 2007) also in studies with moderate to high total score concordance (Clawson et al., 2013; Erickson et al., 2017). Thus, while our findings suggest that either informant is sufficient to assess symptom severity, when arriving at a diagnosis and treatment decisions are made accordingly, it seems advisable to consider both perspectives as they may offer unique information that can assist diagnosis and treatment decisions.

Limitations

First of all, the presence of publication bias indicated that some smaller effect studies did not get published. We did address this problem in our analyses but unfortunately, the true value of unpublished (grey) literature remains unknown. Furthermore, we might have missed studies that were not recorded by the search terms. Besides that, some studies and assessment points were ultimately excluded, as data could not be obtained. Subsequently, some intermediate analyses were limited to very few studies, individual settings and TAU/AT control conditions. Moreover, follow-up assessments were limited to 12-month post-treatment as data for later assessment points was insufficient. Furthermore, we could assess concordance between caregiver and participant ratings only in terms of total scores but not diagnoses. Another shortcoming was the inclusion of some older studies that sometimes used categorical instruments instead of dimensional ones. In addition, we used many different instruments for the same outcome and thus no minimal symptom criterion was defined. Consequently, baseline assessments varied strongly across studies. Moreover, a reliable instrument for pediatric grief is still lacking.

Conclusion and Clinical Implications

This meta-analysis confirmed specific TF-CBT as an effective treatment for pediatric PTSS and secondary outcomes of depression, anxiety and grief at 12-month follow-up. In terms of PTSS, specific TF-CBT showed advantages over TAU/AT control conditions at this primary endpoint. However, regarding secondary outcomes, advantages over other treatments disappeared at follow-up. Effectiveness trials also showed favorable results for specific TF-CBT at the 12-month follow-up, confirming that an easy translation into practice is possible with group settings as a feasible timesaving and cost-

effective alternative. This provides further support for TF-CBT's large-scale use and endorsement by international guidelines (Forbes et al., 2020; National Institute for Health and Care Excellence, 2018; Phoenix Australia Centre for Posttraumatic Mental Health, 2013). Besides, caregiver-reports mirrored the findings of our previous analysis (Thielemann et al., 2022) and showed high concordance with self-reported outcomes. No significant difference between raters was detected for any outcome.

In sum, specific TF-CBT should be the first choice of treatment for pediatric PTSS. Group settings may be used to address high treatment demands with limited resources. In the context of specific TF-CBT, self-reports or caregiver-reports can serve to assess symptom severity in children and adolescents when either informant is unavailable. However, caregiver assessments of internalizing symptoms should be treated with caution prior to treatment initiation. Additionally, both perspectives should be considered when diagnoses are derived and treatment arrangements are made as they may each provide unique information that can assist decision-making.

Future TF-CBT studies should more frequently consider follow-up assessments and longer follow-up periods to fully understand its long-term effects and to investigate whether further treatment gains occur. In addition, effects in group settings warrant further investigation also in western countries and populations with good health care available and should be used to provide treatments to large numbers of individuals in a timely manner. If screening instruments assess self-report as well as caregiver-report and derive presumptive diagnoses, they should report them for both raters to further evaluate the diagnostic concordance. Furthermore, a reliable instrument for pediatric grief symptoms needs to be developed and the categorical instrument of older studies should be contrasted with newer dimensional ones.


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ORCID iD

Jonathan Felix Benjamin Thielemann  <https://orcid.org/0000-0002-8719-5167>

Supplemental Material

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Publication 3. Cross-cultural adaption and psychometric investigation of the German version of the Evidence Based Practice Attitude Scale (EBPAS-36D)


Title	Cross-cultural adaption and psychometric investigation of the German version of the Evidence Based Practice Attitude Scale (EBPAS-36D)
Authors	Katharina Szota ¹ Jonathan Felix Benjamin Thielemann ² Hanna Christiansen ¹ Marte Rye ³ Gregory Aarons ⁴ Antonia Barke ²
Affiliations	¹ Philipps-University of Marburg ² Catholic University Eichstätt-Ingolstadt ³ UiT The Arctic University of Norway ⁴ University of California
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RESEARCH

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Cross-cultural adaption and psychometric investigation of the German version of the Evidence Based Practice Attitude Scale (EBPAS-36D)

Katharina Szota^{1*} , Jonathan F. B. Thielemann², Hanna Christiansen¹, Marte Rye³, Gregory A. Aarons⁴ and Antonia Barke²

Abstract

Background: The implementation of evidence-based practice (EBP) in mental health care confers many benefits to patients, and research into factors facilitating the implementation of EBP is needed. As an important factor affecting the implementation of EBP, service providers' attitudes toward EBP emerged. The Evidence-Based Practice Attitude Scale (EBPAS-36) is an instrument with good psychometric characteristics that measures positive and ambivalent attitudes toward EBP. However, a German version is missing. The present study therefore aims to provide a validated German translation of the EBPAS-36.

Methods: The scale was translated and back-translated as recommended by standard procedures. German psychotherapists were recruited to participate in an online survey. They provided demographic and professional information, completed the EBPAS-36, the Implementation Climate Scale (ICS) and the Intention Scale for Providers (ISP). Standard item and reliability analyses were conducted. Construct validity was evaluated with exploratory (EFA) and confirmatory factor analyses (CFA) in two subsamples (random split). Convergent validity was tested by predicting a high positive correlation of the EBPAS-36D with two scores of attitudes of the ISP and an interest in EBP score. It was tested whether the EBPAS-36D predicts the intention to use EBP.

Results: $N = 599$ psychotherapists participated in the study. The item analyses showed a mean item difficulty of $p_i = 0.64$, a mean inter-item correlation of $r = 0.18$, and a mean item-total correlation of $r_{itc} = 0.40$. The internal consistency was very good for the total scale ($\alpha = 0.89$) and ranged from adequate to very good for the subscales (0.65–0.89), indicating high reliability. The original factor structure showed an acceptable model fit (RMSEA = 0.064 (90% CI = 0.059–0.068); SRMR = 0.0922; AIC = 1400.77), confirming the 12-factor structure of the EBPAS-36. However, a second-order factor structure derived by the EFA had an even better model fit (RMSEA = 0.057 (90% CI = 0.052–0.062); SRMR = 0.0822; AIC = 1274.56). When the EBPAS-36D was entered in a hierarchical regression model with the criterion Intention to use EBP, the EBPAS-36D contributed significantly to the prediction (Change in $R^2 = 0.28$, $p < 0.001$) over and above gender, age and participants' report of ever having worked in a university context.

*Correspondence: szota@staff.uni-marburg.de

¹ Department of Psychology, Philipps-University of Marburg, Gutenbergstr. 18, 35032 Marburg, Germany

Full list of author information is available at the end of the article



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Conclusions: The present study confirms good psychometric properties and validity of a German version of the EBPAS-36 in a sample of psychotherapists.

Keywords: Evidence-based practice, Evidence-based treatments, Interventions, Implementation, Attitudes, Therapists, Mental health

Contributions to the literature

- The article provides a rigorously conducted German translation of the Evidence-Based Practice Attitudes Scale (EBPAS-36).
- The EBPAS-36D was tested in a large sample of German psychotherapists and demonstrated good item characteristics and internal consistency.
- The EBPAS-36D predicted the intention to use evidence-based practices.
- The examination of the factor structure of the EBPAS-36D advances theoretical considerations regarding the underlying constructs and cross-cultural consistency of provider's attitudes toward EBP.

Background

The Institute of Medicine defines evidence-based practice (EBP) as “the integration of best research evidence with clinical expertise and patient values” ([1, 2], p. 147). In order to maintain and improve the effectiveness of health care, the implementation of EBP in routine care is a major objective [3]. If successful, EBP implementation may result in several advantages, including participation in informed health care decisions [4], and better outcomes for patients [5], guidance for the development of treatment plans for practitioners [6], and increased cost-effectiveness of interventions for the health care system [7–9]. Although facilitating the uptake of EBP is in the interest of all stakeholders (including government agencies and insurance companies), a substantial gap between research and practice is evident [10]. This gap results in a large proportion of patients who receive interventions that are not justified in terms of safety, efficacy or cost-effectiveness [11–13]. Accordingly, in the past 20 years, enormous efforts have been made to disseminate and implement EBP in mental health care [14–16]. This gave rise to a rapidly growing research interest in implementation of EBP in health care [17].

Previous research efforts have identified several determinants of successful implementation of EBP. The Consolidated Framework for Implementation Research (CFIR) [18] provides a typology for the complex and interacting constructs associated with successful implementation. The major domains comprise (a) intervention

characteristics, (b) outer setting, (c) inner setting, (d) characteristics of individuals and (e) the implementation process. Regarding the organizational factors of implementation success (b and c), the implementation climate of the organization in which EBP should be established is an important determinant, which can be measured with the Implementation Climate Scale [19]. Perceived barriers to adopt EBP in psychotherapy were found [20] and more positive attitudes toward EBP were linked to higher organizational support [21, 22].

The Exploration, Preparation, Implementation, Sustainment (EPIS) framework identifies provider characteristics and attitudes as important in the uptake of EBPs [23, 24]. These characteristics are most relevant in the inner context of organizations where mental health services are provided [23–25]. Specifically, attitudes can influence the initial decision to consider EBP, how it is implemented and whether it is sustained beyond the implementation phase [24, 26–28]. Demographic factors seem inconsistently related to attitudes toward EBP among mental health care providers [21, 29–33]: Higher age was associated with more positive attitudes in [34–37], with less positive ones in [38–41]; women were reported to show more positive attitudes in [34, 36–38], whereas other studies found no sex differences [26, 39, 40].

An important part of the implementation research agenda is the development of pragmatic measures capturing potentially important implementation determinants, mechanisms, and outcomes [42, 43] that promote or obstruct dissemination and implementation [44]. Martinez, Lewis and Weiner [45] identify several challenges for such instruments including the use of frameworks and theoretical models including consistent construct definitions and appropriate assessments of psychometric properties [45].

The Evidence-Based Practice Attitude Scale (EBPAS) [26] is an instrument that has been identified as a psychometrically strong measure assessing positive as well as ambivalent attitudes toward EBP [26, 34, 44]. It was developed specifically for the target group of mental health care providers, but has since been employed in broader contexts [42]. In line with suggestions put forward in the literature [44, 46], it is based on mental health dissemination and implementation theories [47–49] and has been developed in collaboration with service providers and researchers [26, 27, 50]. The original

15-item version showed strong psychometric properties including high validity in various settings and samples from the US, Norway, Greece and the Netherlands [34, 37, 38, 40]. In an effort to incorporate additional relevant dimensions, the 15-item EBPAS was expanded to 50 items and 12 dimensions through formative work, which included researcher input, focus groups with program managers and clinicians, and subsequent data reduction [27]. In the interest of rendering the measure more brief and pragmatic [46], the 50-item version was reduced to 36-items while retaining 12 dimensions [52]. On the one hand, the domains assess positive attitudes toward EBP: the intuitive Appeal of EBP, the willingness to adopt EBP given the Requirements to do so, providers' Openness to new practices and manualized interventions, the Fit of EBP with values and needs of providers and clients, and providers' perceptions of an increased Job Security provided by learning EBP, of the Organizational Support for learning EBP and of receiving Feedback [27]. On the other hand, the following domains assess ambivalent attitudes toward EBP: the Divergence between research-based interventions and current practice, the Limitations of EBP due to not addressing client needs, negative perceptions of Monitoring by supervisors, the perceived Balance of clinical skills and science in therapy, and the Burden of learning EBP [27]. The EBPAS-36 has shown good psychometric properties and cross-cultural validity in US and Norwegian samples [51].

As argued by Kien et al. [52], German instruments assessing implementation science constructs are scarce and psychometric properties rarely reported. This stands in contrast with the growing relevance and increasing efforts of implementation research in German-speaking countries [53–58]. Over the last decades, psychotherapy in these countries has experienced a significant professionalization and focus on EBP. These countries may benefit from reliable and valid instruments in implementation science. In Germany, a law ('Psychotherapeutengesetz') regulates the practice of psychotherapy since 1999, stating that only state approved practitioners may offer treatment [59–61]. In September 2019, the German parliament approved an adapted law that aims to further align the postgraduate training for psychotherapy to the structure of medical education [59]. Learning about German psychotherapists' attitudes toward EBP may help to inform the psychotherapy training. To the best of our knowledge, two independent German translations of EBPAS-15 exist [62, 63], but no translation of the EBPAS-36 is available. Therefore, the present study aims to present a German translation of the EBPAS-36 and evaluate its psychometric properties.

Methods

Ethics

The cross-sectional online survey study was approved by the Internal Review Board of the University of Marburg (approval number: 2019-58 k). Participants received study information and provided informed consent before they were able to access the survey. Data were collected anonymously. All raw data were stored securely at the Department of Clinical Child and Adolescent Psychology at Philipps University in Marburg, Germany.

Participants

Eligible participants were licensed psychotherapists and psychiatrists for adults, children and adolescents as well as psychotherapists and psychiatrists enrolled in post-graduate training to obtain such a license. No exclusion criteria were applied.

Procedure

Translation. The translation was carried out in accordance with the WHO recommendations (www.who.int/substance_abuse/research_tools/translation/en/), including the following steps: (1) Forward translation, (2) Expert panel back-translation, (3) Pre-testing and cognitive interviewing, and (4) Final version. The EBPAS-36 [51] was translated into German by the second author (JT) (step 1) and back-translated by the bilingual English-speaking senior author (AB). The back-translation was reviewed by the original authors of the scale (MR, GAA) who provided feedback to assure the items represented the meaning and original constructs (step 2). The original and back-translated versions were then reviewed in a consensus meeting of the translating authors. Relevant items of previously available German translations of the EBPAS-15 were compared and the translations showed a good match. The consensus version was then reviewed by a group of German clinical psychotherapists (in training) and researchers ($n=26$) as well as a graduate linguist for comprehensibility and wording (step 3). Their revisions were discussed and considered by the translating authors in a second consensus meeting, resulting in a final German version of the scale (step 4) (see Additional file 1, Additional file 2).

Recruitment and data collection. Data were collected (14/11/2019–27/04/2020) via an openly accessible online survey, using the scientific survey platform SoSci Survey (www.socisurvey.de). The link was widely distributed via e-mail lists of professional psychotherapy organizations that all licensed psychotherapists are members of, universities, training institutes, and psychiatric in- and outpatient institutions as well as Facebook groups of psychotherapists and psychiatrists. On the first page of the survey, potential participants received study information

and were required to provide informed consent before they were able to proceed with the survey. Additional information on the survey is found in the 'Checklist for Reporting Results of Internet E-Surveys' (CHERRIES) [64] in the Additional file 3.

Measures

Demographics and information on training and profession. Participants gave standard demographic information and professional information (university degree, license status, therapy orientation, and current occupation).

Evidence Based Practice Attitudes Scale (EBPAS-36D). The EBPAS-36D is an instrument to assess mental health providers' attitudes toward adopting EBP [51]. The 36 items of the EBPAS-36 load on 12 subscales of three items each: Requirements, Appeal, Openness, Divergence, Limitations, Fit, Monitoring, Balance, Burden, Job security, Organizational support, and Feedback. Respondents are asked to rate their agreement with statements on a 5-point Likert scale ranging from 0 ('not at all') to 4 ('to a very great extent'). Most items are worded in such a way that a higher total score indicates a more positive attitude toward the adoption of EBP; 15 items are scored reversely. A mean of the subscales can be computed to create a total scale. The German instrument can be found in the Additional file 1, Additional file 2.

Implementation Climate Scale (ICS). The ICS is an 18-item instrument measuring the implementation climate in organizations and work groups [19]. The original English version was translated into German by the first author (KS) and back-translated by the bilingual English-speaking senior author (AB). In order to adapt the scale for psychotherapists in private practice, a parallel version was constructed that captures the implementation climate in the health system. Respondents are asked to rate their agreement with statements describing how the respondents perceive the climate in the institution they work at with regard to the implementation of evidence-based interventions. A 5-point Likert scale ranging from 0 ('not at all') to 4 ('to a very great extent') was used. Six subscales can be calculated: Focus on EBP, Educational Support for EBP, Recognition for EBP, Rewards for EBP, Selection for EBP, and Selection for Openness toward EBP. Means of the subscales are computed to create a total scale. In the present study, the internal consistency for the ICS regarding organizations was Cronbach's $\alpha=0.91$ for the total scale and between $\alpha=0.77$ (Selection for Openness) and $\alpha=0.92$ (Focus on EBP) for the subscales. For the ICS with respect to the health system, Cronbach's α was $\alpha=0.90$ for the total scale and between $\alpha=0.77$ (Rewards for EBP) and $\alpha=0.88$ (Educational

Support for EBP) for the subscales. (A separate manuscript for this measure is in preparation.)

Intention Scale for Providers (ISP). The ISP is a 70-item instrument assessing individual behavioral intentions for EBP use [65] based on the theory of planned behavior [66]. The original English version was translated into German by the first author (KS) and back-translated by the bilingual English-speaking senior author (AB). Responses are given on 7-point and 4-point rating scales. Seven subscales can be calculated. Direct measurement scales of attitudes (A-D, 5 items, $\alpha=0.69$), subjective norms (SN-D, 3 items, $\alpha=0.85$), perceived behavioral control (PBC-D, 4 items, $\alpha=0.69$) and behavioral intention (BI-D, 4 items, $\alpha=0.89$) are generated by calculating the average subscale scores. Indirect measurement scales of attitudes (A-ID, 22 items, $\alpha=0.80$), subjective norms (SN-ID, 18 items, $\alpha=0.89$) and perceived behavioral control (PBC-ID, 14 items, $\alpha=0.89$) are created by multiplying and summing up the belief and influence items (e.g., Normative Beliefs and Motivation to Comply).

Global assessments. As a subjective self-assessment measure, participants were asked to rate their interest in EBP on visual analogue scales for nine questions, e.g. "How great is your interest in evidence-based treatment methods?" (see all items in Additional file 4). A total score was computed ($\alpha=0.84$). At the end of the survey, participants were asked to rate the honesty of their responses ('How honestly did you answer the questions of this study?') and their self-reported tendency toward social desirability when answering the survey ('Did social desirability play a role in the survey?') on visual analogue scales.

Before answering the EBPAS-36D, ICS and ISP, participants were provided the following definition of evidence-based methods: "Evidence-based methods are treatment or intervention methods (in psychotherapy, e.g., certain therapy manuals; in physical medicine, e.g., medications or surgical procedures) whose effectiveness has been empirically demonstrated in various scientific studies. This can be done, for example, by demonstrating the efficacy of a psychotherapy over that of a waiting list condition or an alternative treatment."

Statistical analysis

All statistical analyses were performed using IBM SPSS 26 for Windows (Chicago, IL, USA). For the confirmatory factor analysis (CFA), SPSS AMOS version 26.0.0 was used. P values <0.05 were set as thresholds for statistical significance in all analyses. For the EBPAS-36D, means were computed if there was a maximum of one missing item per scale. Otherwise, respondents were excluded from analyses. For item analyses, item difficulties, corrected item-whole correlations and Cronbach's alpha if

item is deleted were calculated. To obtain internal reliability coefficients of the scales and subscales, Cronbach's alpha was calculated. Values above 0.70 are regarded as acceptable, higher than 0.80 as good, higher than 0.90 as excellent. In order to assess construct validity, the factorial structure of EBPAS-36D was investigated by dividing the total sample randomly into two samples: With the first subsample, we conducted an exploratory factor analysis (EFA), followed by a confirmatory one (CFA) with the other subsample. Differences between both samples regarding age, gender distribution and the EBPAS-36D total scale and subscales were examined with independent *t*-tests. The suitability of data for EFA was assessed with the Kaiser-Meyer-Okin (KMO) sample adequacy measure [67, 68] and Bartlett's test [69]. To determine the number of components for the EFA, Horn's parallel analysis and Velicer's MAP test were conducted using the SPSS programs available online (<https://people.ok.ubc.ca/briocconn/nfactors/nfactors.html>) and the results compared [70]. Since parallel analysis of principal factor analysis tends to over-extract factors [71], parallel analysis of principal component analysis was conducted with raw data permutation and 1000 datasets. The EFA was conducted using principal axis factoring analysis with promax correlated factors rotation method. Subsequently, a CFA was conducted to test and compare the original 12-factor structure of EBPAS-36 against a second-order factor structure derived by the EFA, merging the EFA components 4 (Constraints by the institution), 5 (Monitoring) and 6 (Burden) into one second-order factor, and another second-order factor solution that was proposed by Rye et al. [41]. Maximum likelihood estimations were used. Since the Mardia-test for multivariate normal distribution is significant ($z=19.16$) and all variables exceed either the limits for skewness or for excess as postulated by West et al. ([72], skewness < 2, excess < 7), an increased χ^2 value was expected and the Bollen-Stine bootstrap procedure (1000 samples) was performed. The chi-square test statistic the χ^2/df ratio, the root mean square error of approximation (RMSEA), the standardized root mean squared residual (SRMR), the comparative fit index (CFI) and the parsimony-adjusted comparative fit index (PCFI) were reported as fit indices. To assess the convergent validity of EBPAS-36D, the following hypotheses were tested by calculating Pearson correlation coefficients: The EBPAS-36D total scale shows a high positive correlation with the ISP direct and indirect scale scores of attitudes (A-D, A-ID), as well as the interest in EBP score. According to Cohen [73], $r=0.50$ indicates high correlations. To test whether the EBPAS-36D total scale is an incremental predictor of the direct scale of behavioral intention to use EBP of the ISP (BI-D), a hierarchical linear regression analysis with the method ENTER

was conducted. Gender and age (block 1), having ever worked in science (block 2), and the EBPAS-36D total scale (block 3) were successively included in the regression model to assess incremental improvements of model fit. Mean differences across gender and professional groups on the EBPAS-36D were assessed with independent *t*-tests. Pearson coefficients were calculated to assess correlations between age as well as demographic/professional variables and the EBPAS-36D. Lastly, Pearson correlations between ICS and EBPAS-36D were assessed. The findings are reported following the STrengthening the Reporting of OBServational studies in Epidemiology (STROBE) guideline [74] and informed by the Consensus-based Standards for the selection of health Measurement INstruments (COSMIN) taxonomy [75].

Results

Participants

The link to the online survey was clicked 2.417 times. Overall, 913 participants continued after informed consent. Of these, 863 met the inclusion criteria (i.e., profession). A total of 261 participants were excluded due to drop-out before completion of the EBPAS-36D, two due to implausible answers (for example being 99 years old), one due to conspicuous response patterns in EBPAS-36D (e.g., straight-lining despite reverse coded items). Of the remaining 599 participants, 502 were female (83.8%) and their age ranged from 23 to 82 years ($M=36.62$, $SD=11.26$). Roughly half of the sample (56.26%) stated being in postgraduate training to become psychotherapists or psychiatrists. 42.7% of the German psychotherapists reported ever having worked in science. Further information on profession is presented in Table 1.

Item analysis

Detailed information on valid *n* and missing values for the EBPAS-36D items are found in Table 2. Item difficulties of EBPAS-36D ranged between $p_i=0.21$ (item 28) and $p_i=0.93$ (item 13) with a mean difficulty of $p_i=0.64$. The mean inter-item correlation was $r=0.18$. The item-total correlations of the individual items with the total scale ranged from $r_{itc}=0.07$ (item 26) to $r_{itc}=0.62$ (item 2) with a mean item-total correlation of $r_{itc}=0.40$. Eight items showed item-total correlations under 0.30 (see Table 2). Considering the subscales, the correlations of the individual items with their subscales ranged from $r_{itc}=0.44$ (item 5) to $r_{itc}=0.87$ (item 9 and 29).

Reliability

The internal consistency of the EBPAS-36D total scale was $\alpha=0.89$ and would not have benefitted from

Table 1 Demographics and information on profession

Therapy orientation	%	Professional group	%	Current occupation	%
Cognitive Behavior therapy (CBT)	74.5	Psychotherapist in training	40.7	Outpatient practice	64.1
Psychodynamic psychotherapy (PDT)	14.9	Licensed psychotherapist	26.9	Psychiatric hospital	15.1
PDT and psychoanalytic therapy	4.3	Child and adolescent psychotherapist	16.2	Clinic for psychosomatic medicine	6.5
CBT and systemic therapy	2.2	Child and adolescent psychotherapist in training	15.4	Rehabilitation clinic/center	5.0
Other	4.1	Other	0.8	Psychiatric day-clinic	4.5
				University	2.2
				Other	2.6

removing any item. Internal consistencies of the EBPAS-36D subscales were Requirements $\alpha=0.89$; Appeal $\alpha=0.69$; Openness $\alpha=0.75$; Divergence $\alpha=0.65$; Limitations $\alpha=0.82$; Fit $\alpha=0.68$; Monitoring $\alpha=0.77$; Balance $\alpha=0.65$; Burden $\alpha=0.81$; Job security $\alpha=0.89$; Organizational support $\alpha=0.85$; Feedback $\alpha=0.76$.

Subscale correlations

The correlation coefficients between the EBPAS-36D total scale and the 12 subscales are presented in Table 3. The highest correlation was between the total scale and the Openness subscale ($r=0.689$). On subscale level, high correlations were between the Appeal and Fit subscales ($r=0.609$), the Divergence and Limitations subscales ($r=0.550$), the Openness and Divergence subscales ($r=-0.531$), the Appeal and Openness subscales ($r=0.514$) and the Job Security and Organization Support subscales ($r=0.547$).

Validity

For analyses regarding factorial validity, 25 (4.2%) of the participants were excluded as they had more than one missing item in at least one subscale of the EBPAS-36D, so that no means could be calculated. Of those 25 excluded participants, 80.0% dropped out of the survey during the EBPAS-36D after item 15. Of those included in analyses ($n=574$), six participants (1.0%) had missing information on only one item of EBPAS-36D, none had more than two items missing. More than 98.9% of the included participants answered all items. Split samples for EFA and CFA showed no group differences on mean age, gender, and EBPAS-36D total scale or subscales (see Additional file 5).

EFA. The sample adequacy measure ($KMO=0.844$) and significant Bartlett's test ($\chi^2(630)=5616.83$, $p<0.001$) indicated suitability of data ($n=296$) for analysis. Parallel analysis and MAP test both recommended the extraction of six factors. The six extracted factors accounted for 57.56% of variance. The rotated factor matrix is found in the Additional file 6. Eleven items

loaded on factor 1 and explained 24.33% of the variance (factor loadings from 0.357 to 0.844). Ten items loaded on factor 2 and explained 9.41% of the variance (0.410 to 0.773). Factor 3 comprised six items that explained 7.92% of the variance (0.343 to 0.969). Three items loaded on factor 4 and explained 5.93% of the variance (0.735 to 0.946). Three items loaded on factor 5 and explained 5.27% of the variance (0.549 to 0.815). Factor 6 comprises three items that explained 4.71% of variance (0.693 to 0.841). With the exception of item 2 of the Openness subscale, items of the original subscales loaded on the extracted factors together. Item 3 of the Openness subscale showed nearly equally high factor loadings on factor 1 (0.357) and factor 2 (-0.335).

CFA. The path diagrams of the original 12-factor structure (model A), a second-order 4-factor structure derived by the EFA (model B) and the second-order 3-factor model established by Rye et al. [41] (model C) are shown in Figs. 1, 2 and 3, respectively. Although the model fit of the original factor structure was adequate, both second-order models showed even better model fits (see Table 4). For all three models, all regression weights were significant.

Correlation analyses. The EBPAS-36D total scale correlated with the direct scale of attitudes (A-D, $r=0.663$, $p<0.001$, $n=574$) and the indirect scale of attitudes (A-ID, $r=0.531$, $p<0.001$, $n=126$) of the ISP. The global 9-item assessment of participant's interest in EBP correlated with the EBPAS-36D total scale ($r=0.529$, $p<0.001$, $n=566$).

Regression analysis. The EBPAS-36D total scale was included in a hierarchical regression model to predict the Behavioral Intention Scale of the ISP as a third block, subsequent to the predictors gender and age (block 1) and ever having worked in science (block 2). The inclusion improved the model fit (Change in $R^2=0.28$, $F=267.32$, $p<0.001$) and the significant regression coefficient of the EBPAS-36D total scale ($\beta=2.13$; $t=16.35$; $p<0.001$) indicated incremental

Table 2 Item analyses of EBPAS-36D

Item	Short description	M (SD)	p_i	r_{itc} total	α_{total} if deleted	r_{itc} subscale	$\alpha_{subscale}$ if deleted	Valid n	Missing values (%)
1	Like to use new therapy/interventions	2.85 (0.80)	0.71	0.358	0.89	0.516	0.73	599	0 (0.00)
2	Will follow a treatment manual	2.75 (1.05)	0.69	0.617	0.88	0.621	0.62	599	0 (0.00)
3	Will try therapy/interventions developed by researchers	2.99 (0.82)	0.75	0.572	0.88	0.615	0.62	599	0 (0.00)
4 (r)	Research based treatments/interventions not useful	3.22 (0.93)	0.81	0.404	0.89	0.473	0.55	599	0 (0.00)
5 (r)	Clinical experience more important	1.65 (1.06)	0.41	0.479	0.88	0.441	0.59	599	0 (0.00)
6 (r)	Would not use manualized therapy/interventions	3.29 (1.03)	0.82	0.551	0.88	0.479	0.53	599	0 (0.00)
7	Makes sense	3.58 (0.63)	0.90	0.374	0.89	0.449	0.68	599	0 (0.00)
8	Supervisor required	2.14 (1.10)	0.54	0.520	0.88	0.841	0.79	599	4 (0.67)
9	Agency required	2.17 (1.09)	0.54	0.501	0.88	0.870	0.76	599	3 (0.50)
10	State required	2.26 (0.96)	0.56	0.421	0.89	0.655	0.94	599	1 (0.17)
11	Colleagues happy with therapy	2.92 (0.89)	0.73	0.429	0.89	0.517	0.59	599	2 (0.33)
12	Enough training	3.26 (0.86)	0.82	0.532	0.88	0.583	0.49	599	1 (0.17)
13	Right for your clients	3.71 (0.62)	0.93	0.329	0.89	0.506	0.58	599	1 (0.17)
14	Had a say in how to use the evidence-based practice	3.22 (0.90)	0.81	0.288	0.89	0.478	0.63	599	1 (0.17)
15	Fit with your clinical approach	3.51 (0.74)	0.88	0.272	0.89	0.522	0.54	599	1 (0.17)
16 (r)	Clients with multiple problems	2.48 (1.07)	0.62	0.410	0.89	0.588	0.83	581	2 (0.34)
17 (r)	Not individualized	2.35 (1.11)	0.59	0.474	0.88	0.714	0.71	581	2 (0.34)
18 (r)	Too narrowly focused	1.99 (1.10)	0.50	0.514	0.88	0.718	0.70	581	2 (0.34)
19 (r)	Work without oversight	2.06 (1.22)	0.52	0.363	0.89	0.688	0.60	581	2 (0.34)
20 (r)	Looking over my shoulder	2.18 (1.23)	0.55	0.292	0.89	0.626	0.67	581	2 (0.34)
21 (r)	My work does not need to be monitored	1.90 (1.22)	0.48	0.318	0.89	0.513	0.27	581	2 (0.34)
22 (r)	Positive outcome is an art	2.73 (0.99)	0.68	0.489	0.88	0.524	0.46	581	2 (0.34)
23 (r)	Therapy is an art and a science	1.24 (1.14)	0.31	0.175	0.89	0.432	0.59	581	2 (0.34)
24 (r)	Overall competence is more important	1.38 (1.02)	0.35	0.437	0.89	0.422	0.59	581	2 (0.34)
25 (r)	Don't have time to learn anything new	2.90 (1.06)	0.73	0.133	0.89	0.628	0.76	581	2 (0.34)
26 (r)	Can't meet other obligations	3.02 (1.01)	0.76	.067	0.89	0.726	0.67	581	5 (0.86)
27 (r)	How to fit evidence-based practice in	2.87 (1.14)	0.72	0.199	0.89	0.618	0.78	581	2 (0.34)
28	Help me keep my job	0.83 (1.11)	0.21	0.431	0.89	0.653	0.95	581	5 (0.86)
29	Help me get a new job	1.52 (1.32)	0.38	0.538	0.88	0.870	0.76	581	3 (0.52)
30	Make it easier to find work	1.50 (1.32)	0.38	0.522	0.88	0.850	0.78	581	4 (0.69)
31	Continuing education credits provided	2.25 (1.33)	0.56	0.470	0.88	0.642	0.88	581	4 (0.69)
32	Training provided	2.54 (1.20)	0.64	0.587	0.88	0.806	0.71	581	3 (0.52)
33	Ongoing support provided	2.41 (1.17)	0.60	0.549	0.88	0.728	0.79	581	3 (0.52)
34	Enjoy feedback on performance	2.92 (0.87)	0.73	0.312	0.89	0.574	0.71	581	4 (0.69)
35	Feedback helps me to be better	3.28 (0.82)	0.82	0.380	0.89	0.709	0.53	581	3 (0.52)
36	Supervision helps me to be better	3.59 (0.69)	0.90	0.214	0.89	0.511	0.76	581	3 (0.52)

p_i : item difficulty, r_{itc} corrected item-whole correlation. (r): Item to be reverse scored

prediction beyond the previous predictors (see Tables 5 and 6).

Group differences and correlations

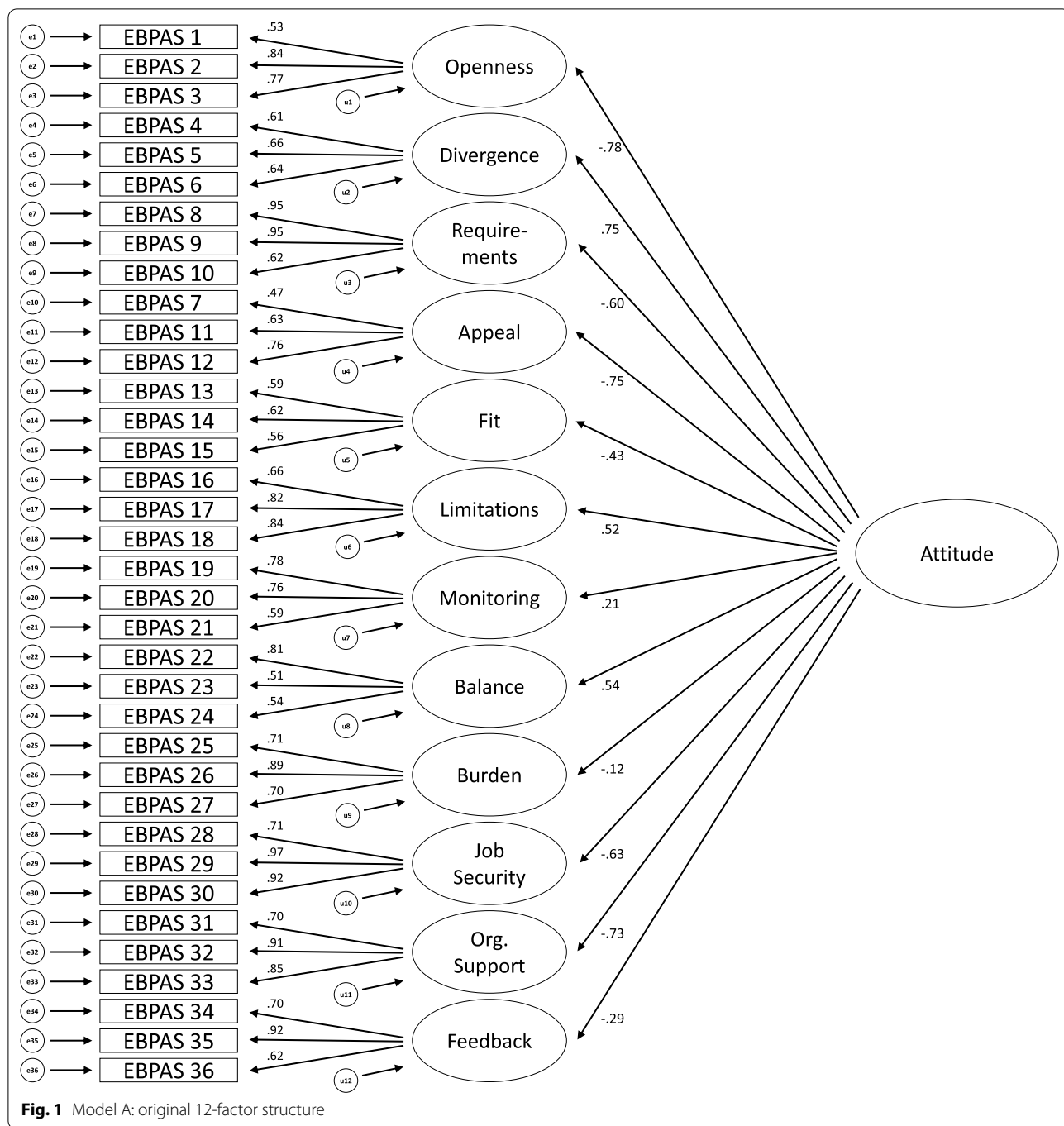
Compared to women, men scored lower on the EBPAS-36D total scale ($t(570)=2.59$; $p=0.010$) and the subscales Requirements ($t(592)=2.91$; $p=0.004$), Appeal ($t(595)=3.73$; $p<0.001$), Fit ($t(595)=2.91$; $p=0.004$)

and Organizational Support ($t(579)=2.66$; $p=0.008$). Age was correlated with the EBPAS-36D total scale and all subscales (see Table 3), indicating more negative attitudes toward EBP with increasing age. Consistent with this, licensed psychotherapists differed from psychotherapists in training on all subscales and the total scale of the EBPAS-36D ($t(445.82)=-7.581$; $p<0.001$; $d=-0.72$), indicating more positive attitudes of psychotherapists in

Table 3 Pearson correlations of EBPAS-36D total scale, EBPAS-36D subscales and age

	2	3	4	5	6	7	8	9	10	11	12	13	Age
1 EBPAS-36D Total scale	0.586**	0.618**	0.689**	-0.677**	-0.602**	0.427**	-0.471**	-0.536**	-0.232**	0.613**	0.665**	0.425**	-0.441**
2 EBPAS-36D Requirements	-	0.465**	0.359**	-0.281**	-0.177**	0.261**	-0.135**	-0.199**	0.016	0.411**	0.376**	0.151**	-0.410**
3 EBPAS-36D Appeal	-	-	0.514**	-0.376**	-0.208**	0.609**	-0.091*	-0.127**	-0.002	0.289**	0.436**	0.286**	-0.376**
4 EBPAS-36D Openness	-	-	-	-0.531**	-0.359**	0.340**	-0.113**	-0.281**	-0.082*	0.391**	0.468**	0.261**	-0.424**
5 EBPAS-36D Divergence	-	-	-	-	0.550**	-0.214**	0.203**	0.452**	0.119**	-0.258**	-0.393**	-0.096*	0.230**
6 EBPAS-36D Limitations	-	-	-	-	-	-0.081	0.256**	0.445**	0.185**	-0.251**	-0.267**	-0.059	0.160**
7 EBPAS-36D Fit	-	-	-	-	-	-	-0.034	-0.032	-0.088*	0.107**	0.252**	0.272**	-0.176**
8 EBPAS-36D Monitoring	-	-	-	-	-	-	-	0.350**	0.182**	-0.151**	-0.120**	-0.264**	0.137**
9 EBPAS-36D Balance	-	-	-	-	-	-	-	-	0.126**	-0.179**	-0.220**	-0.018	0.182**
10 EBPAS-36D Burden	-	-	-	-	-	-	-	-	-	0.113**	0.168**	-0.063	-0.172**
11 EBPAS-36D Job security	-	-	-	-	-	-	-	-	-	-	0.547**	0.223**	-0.371**
12 EBPAS-36D Org. support	-	-	-	-	-	-	-	-	-	-	-	0.291**	-0.388**
13 EBPAS-36D Feedback	-	-	-	-	-	-	-	-	-	-	-	-	-0.232**

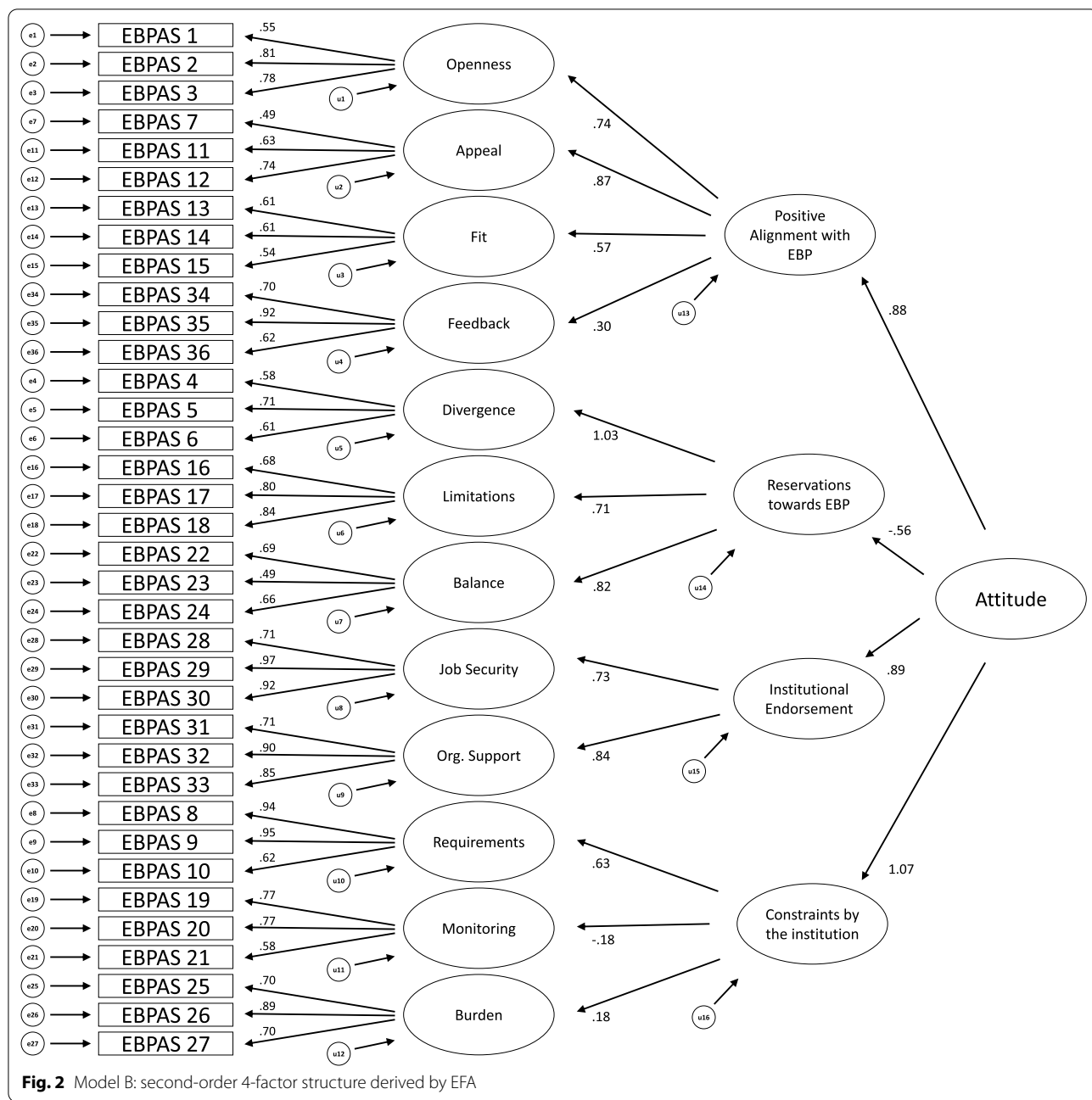
n = 574–599. EBPAS-36D: German version of the Evidence-Based Practice Attitudes Scale *p < 0.05 **p < 0.001



training, which is consistent with prior findings regarding professional development level and attitudes toward EBP [26].

The EBPAS-36D total scale was associated with the total score of the ICS on organizations and work groups ($r=0.432, p<0.001, n=408$), but not with the total scale of the ICS on the health system ($r=0.138,$

$p=0.080$) that was exclusively completed by licensed psychotherapists and psychiatrists working in private practices ($n=161$). The self-rated honesty when answering the survey showed a small correlation with the EBPAS-36D total scale ($r=0.146, p=0.001, n=556$).



Discussion

The present study is the first to present a German version of the EBPAS-36 and investigate its validity and psychometric properties. In a sample of psychotherapists and psychiatrists, the original factor structure was confirmed and the EBPAS-36D demonstrated good item properties, internal consistency and convergent validity.

Rising implementation research efforts in German-speaking countries necessitate the development and psychometric examination of German instruments assessing

implementation research constructs [52]. Regarding characteristics of individuals, additional well-suited instruments have been translated and validated during the course of our study, for example the Evidence-based Practice Inventory (EBPI) questionnaire [76]. The EBPI assesses health care providers' adherence to EBP as well as barriers and facilitators for the use of EBP. A total of 26 items load on five domains: attitude, subjective norm, perceived behavioral control, decision making and intention and behavior. A German version was

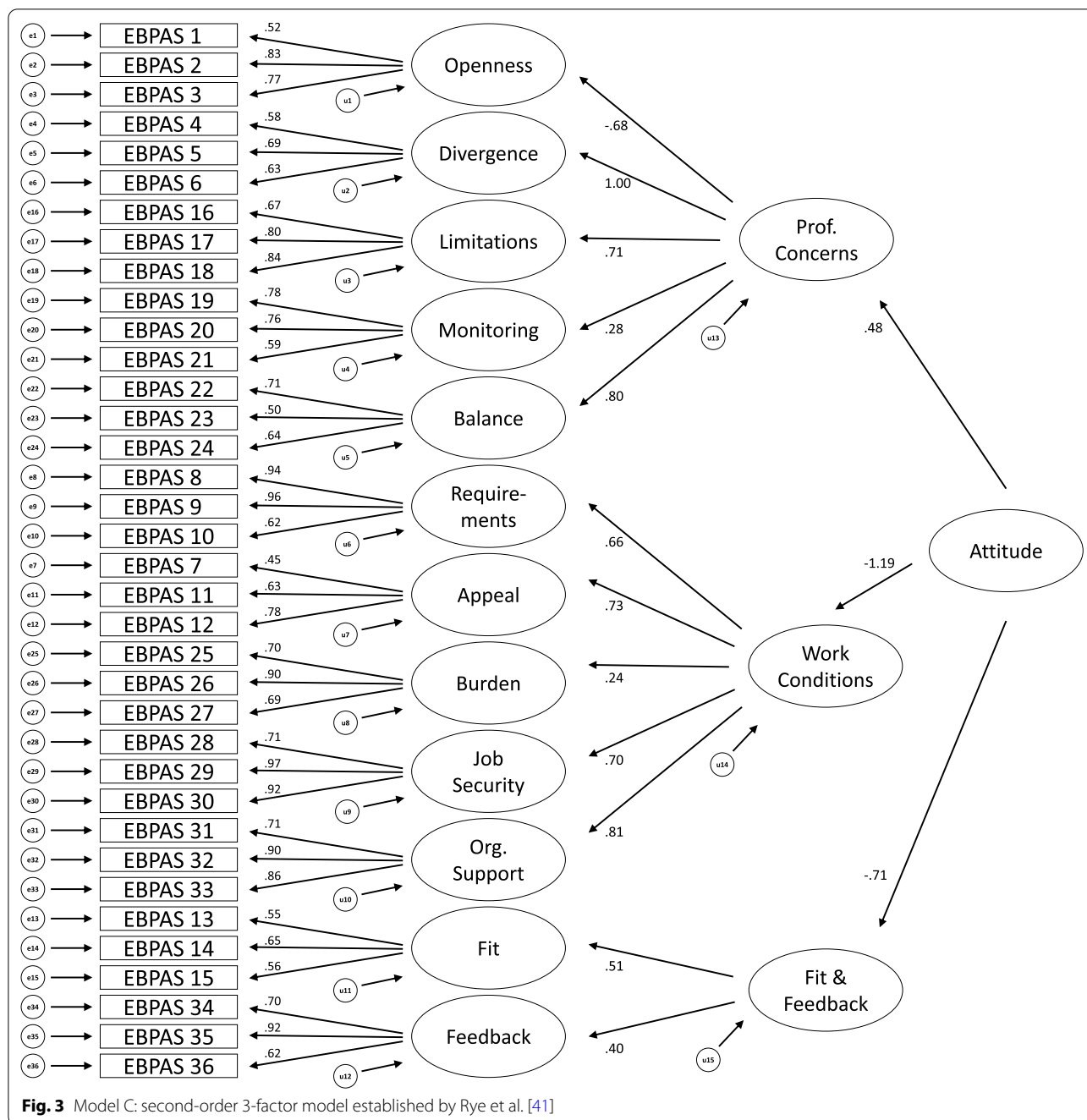


Table 4 CFA: model fit indices

Model	χ^2	df	p	χ^2/df	RMSEA [90% CI]	SRMR	CFI	PCFI	AIC
A	1232.77	582	0.001 ^a	2.12	0.064 [0.059; 0.068]	0.0922	0.856	0.791	1400.77
B	1098.56	578	0.001 ^a	1.90	0.057 [0.052; 0.062]	0.0822	0.885	0.812	1274.56
C	1121.06	579	0.001 ^a	1.94	0.058 [0.053; 0.063]	0.0857	0.880	0.809	1295.06

n = 278. Model A: Original 12-factor model. Model B: Second-order model derived by EFA. Model C: Second-order model by Rye et al. a: Bollen-Stine-corrected. RMSEA root mean square error of approximation, SRMR standardized root mean residual, CFI comparative fit index, PCFI parsimony-adjusted CFI, AIC Akaike information criterion

Table 5 Model summary

Model	R ²	R ² _{corr}	SE	F	df	p	Change in R ²	F	p
1	0.098	0.095	1.54	29.30	2; 540	< 0.001			
2	0.156	0.151	1.49	33.17	3; 539	<0.001	0.058	37.01	<0.001
3	0.436	0.432	1.22	103.99	4; 538	<0.001	0.280	267.32	<0.001

n = 543. Dependent variable: Behavioral Intention Scale of Intention Scale for Providers. Model 1: Gender, Age. Model 2: Gender, Age, Work in Science. Model 3: Gender, Age, Work in Science, EBPAS-36D total scale

Table 6 Regression coefficients

Model		B	95% CI of B	SE	t	p
1	(Constant)	7.55	6.98; 8.13	0.29	25.78	< 0.001
	Gender	0.12	- 0.24; 0.47	0.18	0.65	0.519
	Age	- 0.05	- 0.06; - 0.04	0.01	- 7.64	< 0.001
2	(Constant)	8.81	8.12; 9.50	0.35	25.11	< 0.001
	Gender	0.03	- 0.31; 0.37	0.17	0.16	0.873
	Age	- 0.05	- 0.06; - 0.03	0.01	- 7.47	< 0.001
	Work in Science	- 0.79	- 1.05; - 0.54	0.13	- 6.08	< 0.001
3	(Constant)	1.22	0.14; 2.29	0.55	2.23	0.026
	Gender	0.17	- 0.11; 0.45	0.14	1.18	0.237
	Age	- 0.01	- 0.02; 0.00	0.01	- 2.02	0.044
	Work in Science	- 0.35	- 0.56; - 0.13	0.11	- 3.27	0.002
	EBPAS-36D total scale	2.13	1.88; 2.39	0.13	16.35	< 0.001

n = 543. Dependent variable: Behavioral Intention Scale of Intention Scale for Providers

adopted and its reliability was examined in a nationwide online survey [76]. The EBPI and EBPAS-36D could thus complement each other well in future implementation research studies, with the latter focusing on attitudes and capturing diverse aspects of positive and ambivalent attitudes toward EBP. The evaluation of providers' attitudes toward EBP with help of both instruments might inform about successful strategies in implementation efforts in research in German-speaking countries as well as potential targets for improvement in clinical training and practice. For example, The Leadership and Organizational Change for Implementation (LOCI) strategy can be used to improve workplace climate for EBP, which should then influence provider attitudes toward, and use of, EBP with fidelity [77, 78]. Thus, attitudes could be considered a mechanism by which an implementation strategy has its effects on clinical practice. This is consistent with an implementation science approach where it is recommended to identify and integrate the use of implementation frameworks and strategies to address implementation determinants, mechanisms, and outcomes [79, 80].

Regarding the psychometric properties of the individual items, most item difficulties were in the medium range. In the context of attitude measurements, a high item difficulty translates into low endorsement of the

item. Medium difficulty is desirable as it is optimal to differentiate between respondents with different attitudes. Items of the subscales Fit, Feedback and Appeal received high approval (subscale means > 3.2), whereas items of the subscales Burden, Job Security and Divergence were less strongly endorsed (subscale means < 1.3). Removing any item would not have improved the internal consistency of the total scale and could result in poor content validity. While correlations between items and their subscales were at least in the medium range, item-whole correlations demonstrated considerable variability. Consistent with this, the subscales Fit, Monitoring, Burden and Feedback showed only moderate correlations with the EBPAS-36D total scale and only few subscales showed high inter-correlations, namely the subscales Appeal and Fit, Divergence and Limitations, Openness and Divergence, Appeal and Openness, and Job Security and Organizational Support. This accords well with previous results in US and Norwegian examinations of the EBPAS-36 that demonstrated high inter-correlations only between the Appeal and Organizational Support subscales (US) and between the Limitations and Divergence, and the Job Security and Organizational Support subscales (Norway) [51].

The internal consistency of the EBPAS-36D total scale obtained is good and comparable to those found for the

US and Norwegian versions. The internal consistencies of the subscales ranged from acceptable to good, with the subscales Appeal, Divergence, Fit and Balance demonstrating the lowest internal consistencies, as seen in the Norwegian sample [51]. Due to their limited reliability, these subscales should be interpreted with caution. In consideration of the extreme brevity of the subscales (3 items), the overall reliability of the EBPAS-36D subscales can be considered high.

The CFA confirmed the 12-factor structure of the original EBPAS-36 by demonstrating adequate model fit. Nonetheless, two second-order factor structures, one derived by EFA in the present sample and one that was proposed by Rye et al. [41], showed even better model fits. Therefore, second-order models might map the actual underlying construct of attitudes toward EBP even better than the original factor structure. The four second-order constructs we found are: Positive alignment with EBP, consisting of the Openness, Appeal, Fit and Feedback subscales, Reservations toward EBP, consisting of the Divergence, Limitations and Balance subscales, Institutional Endorsement, consisting of the Job Security and Organizational Support subscales and Constraints by the institution, consisting of the Requirements, Monitoring and Burden subscales.

As expected, the EBPAS-36D showed high correlations with two other scales assessing attitudes toward the use of EBP, the direct and indirect measurement scales of attitudes of the ISP. This confirms the convergent validity of the scale. Accordingly, a high correlation was found between the EBPAS-36D and a global assessment of participant's interest in EBP, a nine-item subjective self-rating of one's interest in research on psychotherapy and clinical psychology, clinical guidelines, EBP and treatment manuals with high face validity. Moreover, the EBPAS-36D allowed incremental prediction of the intention to use EBP as assessed with the Behavioral Intention Scale of the ISP beyond gender, age and working in science.

Responders of the 50-item version of the scale commented on being annoyed and fatigued by answering the items [51]. This might result in response biases and missing answers, limiting the validity of the scale. The acceptability of the shorter EBPAS-36D appears to be high as indicated by a low amount of missing answers in the current study. Further, the EBPAS-36D is 28% shorter than the prior 50-item version and is consistent with calls for brief and pragmatic measures in implementation research [46]. With its 36 items and 12 first order subscales, the EBPAS-36D is a complex instrument and the reader may wonder about its feasibility. However, evidence-based practice itself is a complex construct and the attitudes of health professionals may vary on a number

of dimensions. In our view, it is helpful to represent these dimensions in a detailed fashion on a measurement instrument. The most likely contexts of measurement will be evaluations of existing services and its stakeholders prior to the implementation of changes. In these contexts, a fine-grained assessment of the attitudes and views of the professionals may offer information about problem areas that might need attention in transformation processes (i.e. providing training, identifying obstacles). Given that the target group who will complete the questionnaire are health-professionals who are generally acquainted with such instruments, we feel confident that the length and complexity should not present any barriers.

In the present sample of German psychotherapists, higher age was associated with less favorable attitudes toward EBP. Consistent with this, licensed psychotherapists reported more negative attitudes compared to psychotherapists in training. While this result is in contrast to two previous studies reporting higher scores on the Requirements and Openness subscales of the EBPAS with increasing age [34, 35], it is in accordance with other studies [38–41]. As assumed by van Sonsbeek et al. [37], professionals may rate their own clinical experience higher than EBP with increasing age and experience. However, it should be noted that as yet, age effects may be confounded by cohort effects, since psychotherapy training underwent considerable changes in Germany over the last 30 years. Concerning sex differences, women reported more positive attitudes toward EBP in the present study. Sex differences were apparent for the total scale and the subscales Requirements, Appeal, Fit and Organizational Support. This result adds to other research demonstrating more positive attitudes toward EBP in women [34, 36–38, 41]. Still, these sex differences were not found consistently [26, 39, 40], which is why possible moderators should be investigated in future research.

As demonstrated in previous studies [27, 35], provider's attitudes toward EBP were associated with organizational climate for the implementation of EBP. Since psychotherapists working in private practices were unable to rate the implementation climate of any organization or work group, those participants were asked to rate a parallel version of the ICS, capturing the implementation climate in the German health system. Interestingly, individuals' attitudes toward EBP were not associated with their evaluation of the implementation climate of the health system. One reason for this might be that the German health system does not provide support for EBP to a similar extent as some organizations do, thus not leading to more positive attitudes toward EBP [28]. Another reason might be that psychotherapists with a positive attitude toward

EBP are able to choose organizations with a better implementation climate for EBP or affect the implementation climate of the organization they work at, while they are probably unable to choose a health system according to its implementation climate or affect the implementation climate of the health system.

When interpreting the results of the present study, some limitations must be borne in mind. All data are based on self-reports in a cross-sectional online survey. A large proportion of the sample reported ever having worked in science and having a cognitive behavioral therapy approach. The findings refer to a convenience sample that is most likely self-selected for interest in EBP and not representative [81] of the population of mental health providers. This does not affect the evaluation of the psychometric properties of the EPBAS-36D; still future studies with representative samples should seek to confirm the results. Although the dropout rate in the present study can be considered as average for online surveys [82], a selection bias cannot be precluded with only particularly interested participants completing the survey [83]. The total number of items of the survey would have been significantly reduced if we had the EBPI available when we planned our study. It would have been fruitful to use it instead of the ISP to investigate the convergent validity of the EBPAS-36D not only due to its smaller item number but because of the availability of a German version instrument that has been psychometrically examined [76]. In future studies, this well-suited questionnaire might be used to further validate the EBPAS-36D. Finally, although a definition of EBP preceded the questionnaire, some comments indicated that individual participants may have been uncertain about the exact meaning of EBP. Future research should assess the comprehensibility of the definition to ensure that all participants rate the same construct.

Conclusions

The consideration of providers' attitudes toward EBP in implementation research might inform about successful strategies to address their readiness to implement EBP, while in clinical practice this could point to important targets for addressing in training and supervision. Therefore, reliable instruments assessing attitudes toward EBP could be useful for researchers, training directors, and supervisors. Although further validating research is required, the present study confirms good psychometric properties and validity of a German version of the EBPAS-36 in a sample of psychotherapists. The proposed second-order model of attitudes toward EBP may initiate further research on the construct of attitudes toward EBP.

Abbreviations

AIC: Akaike information criterion; CBT: Cognitive behavioral therapy; CFA: Confirmatory factor analysis; CFI: Comparative Fit Index; CFIR: Consolidated Framework for Implementation Research; CHERRIES: Checklist for Reporting Results of Internet E-Surveys; COSMIN: COnsensus-based Standards for the selection of health Measurement INstruments; EBP: Evidence-based practices; EBPAS: Evidence-based Practice Attitudes Scale; EBPI: Evidence-based practice inventory; EFA: Exploratory factor analysis; EPIS: Exploration, preparation, implementation, sustainment framework; ICS: Implementation Climate Scale; ISP: Intention Scale for Providers; PCFI: Parsimony-adjusted Comparative Fit Index; RMSEA: Root Mean Square Error of Approximation; SRMR: Standardized Root Mean Squared Residual; STROBE: STrengthening the Reporting of OBServational studies in Epidemiology; US: United States.

Supplementary Information

The online version contains supplementary material available at <https://doi.org/10.1186/s12961-021-00736-8>.

Additional file 1: EBPAS-36D (German).

Additional file 2: EBPAS-36D Scoring Instructions (German).

Additional file 3: CHERRIES Checklist.

Additional file 4: Global assessments.

Additional file 5: Split sample t-tests.

Additional file 6: Rotated factor matrix.

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None.

Authors' contributions

KS, AB and HC designed the study. KS, JT and AB translated the instruments. MR and GAA provided feedback on the translations. KS collected the data. KS and AB analyzed the data and drafted the manuscript. JT provided a first draft of the introduction. HC, MR and GAA commented on the initial draft and critically revised it. All authors have read and approved the final manuscript.

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Availability of data and materials

The datasets analyzed in the current study are available from the corresponding author on reasonable request.

Declarations

Ethics approval and consent to participate

The study was approved by the Internal Review Board of the University of Marburg (approval number: 2019-58 k). Participants received study information and provided informed consent. Data were collected anonymously.

Consent for publication

Not applicable.

Competing interests

The authors declare that they have no competing interests.

Author details

¹Department of Psychology, Philipps-University of Marburg, Gutenbergstr. 18, 35032 Marburg, Germany. ²Department of Clinical and Biological Psychology, Catholic University of Eichstätt-Ingolstadt, Levelingstr. 7, 85049 Ingolstadt, Germany. ³Faculty of Health Sciences, UiT The Arctic University of Norway, Regional Centre for Child and Youth Mental Health and Child Welfare, 9037 Tromsø, Norway. ⁴Department of Psychiatry, University of California, 9500 Gilman Drive (0812), La Jolla, San Diego, CA 92093-0812, USA.

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CURRICULUM VITAE

Personal Information

Name Jonathan Felix Benjamin Thielemann
Date of birth December 05th, 1993 in Hamburg, Germany
Current address Mülhauser Str. 9, 50739 Köln

Education

Since 04/2019 **Accredited training in behavioral therapy**
Institut for behavioral therapy, behavioral medicine and sexology,
Fürth

Since 02/2019 **Ph.D. program**
Catholic University Eichstätt-Ingolstadt, department of clinical and
biological psychology

09/2017 – 08/2018 **Clinical Psychology, MSc. (dutch grade: 8.0)**
University of Groningen, thesis: “I see what you are saying”:
Intrusive images from listening to a traumatic verbal report’ (dutch
grade: 8.5)

09/2014 – 07/2017 **Psychology, BSc. (dutch grade: 7.9)**
University of Groningen, thesis: ‘The Impact of Acute Stress on
Cognitive Flexibility and the Moderating roles of Gender and
Chronic Stress’ (dutch grade: 7.0)

10/2013 – 03/2014 **Bioscience, BSc. (no final examination)**
University of Potsdam

08/2004 – 06/2012 **Baccalaureate (grade: 2.0)**
Gymnasium Othmarschen, Hamburg

Professional experience

Since 03/2023 **Practical training 1 (1200h) in behavioral therapy**
Alexianer Krefeld GmbH, Hospital Maria-Hilf, Clinic for Mental
Health, general psychiatry

02/2019-02/2023; **Research assistant**
Since 04/2023 BMBF-funded project “Better Care for Unaccompanied Young
Refugees: Comparing a Stepped Care Model with Usual Care”,
Catholic University Eichstätt-Ingolstadt

06/2019 – 06/2021 **Practical training 2 (600h) in behavioral therapy**
Outpatient clinic of the Catholic University Eichstätt-Ingolstadt for
psychotherapy

2016 – 2018 **Refugee Project ‘Schöner Tag’**
St. Martins Church Uplengen-Remels

04/2016 – 06/2016 **Research internship**
University of Groningen

2015 – 2016 **Student assistant**
Psychological Practice B. Wusowski, Hamburg

2012 – 2014 **Logistics, fair construction, catering**
Fideles Eventmanagement, Hamburg

2010 – 2012 **Pharmacy deliveries**
Reventlow Pharmacy, Hamburg

2009 – 2015

IT-consulting

Self-employed, Hamburg

LIST OF PUBLICATIONS

Original research

(* part of the dissertation)

*1. **Thielemann, J.F.B.**, Kasparik, B., König, J., Unterhitzberger, J., & Rosner, R. (2023). Stability of treatment effects and caregiver-reported outcomes: A meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents. *Child Maltreatment*. doi: 10.1177/10775595231167383

*2. **Thielemann, J.F.B.**, Kasparik, B., König, J., Unterhitzberger, J., & Rosner, R. (2022). A systematic review and meta-analysis of trauma-focused cognitive behavioral therapy for children and adolescents. *Child Abuse & Neglect*, 134. doi: 10.1016/j.chiabu.2022.105899

*3. Szota, K., **Thielemann, J.F.B.**, Christiansen, H., Rye, M., Aarons, G.A., & Barke, A. (2021). Cross-cultural adaption and psychometric investigation of the German version of the Evidence Based Practice Attitude Scale (EBPAS-36D). *Health Research Policy and Systems* 19(1), 90. doi: 10.1186/s12961-021-00736-8

4. Rosner, R., Sachser, C., Hornfeck, F., Kilian, R., Kindler, H., Muche, R., Müller, L.R.F., **Thielemann, J.F.B.**, Waldmann, T., Ziegenhain, U., Unterhitzberger, J., & Pfeiffer, E. (2020). Improving mental health care for unaccompanied young refugees through a stepped-care approach versus usual care+: study protocol of a cluster randomized controlled hybrid effectiveness implementation trial. *Trials*, 21(1013). doi: 10.1186/s13063-020-04922-x

Conferences

1. **Thielemann, J.F.B.** & Daniels, J.K. (2020). Can Listening to a Traumatic Report Induce Intrusions? – A Replication Study. Submitted poster for the 38th Symposium of the Division of Clinical Psychology and Psychotherapy of the DGPs, University of Mannheim, May 20-23, 2020, canceled due to Covid-19.

2. **Thielemann, J.F.B.**, Unterhitzberger, J., & Rosner, R. (2019). Implementierung der traumafokussierten kognitiven Verhaltenstherapie für junge Geflüchtete im Rahmen des gestuften Versorgungsansatzes BETTER CARE (Teilprojekt 3) [Implementation of trauma-focussed cognitive behavioral therapy with young refugees through the stepped-care approach BETTER CARE (Subproject 3)]. Poster presented at the annual meeting of the research consortium for mental health of refugees, University of Bochum, October 9-11, 2019.

EIDESSTATTLICHE ERKLÄRUNG [STATUTORY DECLARATION]

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