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The association between therapeutic alliance and outcome in internet-based psychological interventions: A meta-analysis

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ABSTRACT

Background: Internet-based psychological interventions have proven effective and yield advantages that make them a viable alternative to face-to-face therapy in many fields. Yet, the role of therapeutic alliance in technologically mediated interventions has been discussed critically. The aim of this meta-analysis is to summarize the association between therapeutic alliance and outcome in therapist-assisted online interventions.
Methods: A systematic search of the databases PubMed, Web of Science, PsycINFO and PubPsych was conducted for articles published before February 2020 that assess the association between therapeutic alliance and outcome in internet-based interventions involving remote therapist contact. Results were systematically screened and information on the alliance-outcome-association was extracted. A multilevel meta-analysis was conducted.
Results: Overall, 51 effect sizes were extracted from 20 included studies. The average weighted effect size is $r = 0.203$ ($p < .0001$). The correlation was larger when alliance was measured near the end of an intervention. There was no impact of therapist contact frequency or mode and availability of self-help content on the effect size.
Conclusions: Therapeutic alliance and outcome are significantly correlated in internet-based therapy. This points to the relevance of a stable alliance in internet-based interventions and suggests that fostering alliance might be beneficial to treatment success.

1. Introduction

The effectiveness of internet-based interventions has been shown by systematic reviews and meta-analyses examining effects of online interventions on mental disorders like depression, anxiety disorders, or post-traumatic stress disorder (PTSD) (Andersson, Cuijpers, Carlbring, Riper, & Hedman, 2014; Brown, Glendenning, Hoon, & John, 2016; Carlbring, Andersson, Cuijpers, Riper, & Hedman-Lagerlöf, 2018; Hedman, Ljótsson, & Lindefors, 2012; Kuester, Niemeyer, & Knaevelsrud, 2016; Spijkerman, Pots, & Bohlmeijer, 2016). Internet-based interventions have been promoted for a variety of advantages, including flexible access, anonymity (Aboujaoude, Salame, & Naim, 2015; Musiat & Tarrier, 2014) and cost-effectiveness (Hedman et al., 2012; Musiat & Tarrier, 2014) which can help to reduce barriers to treatment attendance.

In electronically delivered interventions information is exchanged with a therapist or treatment program remotely. While there are interventions employing exchange via telephone, most programs employ communication via Internet like videoconferencing, chat/texting, or e-mail. A variety of names is used for remote interventions (e.g., e-mental health, internet-based interventions, telehealth). For the current meta-analysis the term internet-based psychological interventions (IPIs) shall be used for all internet-based or remote psychological interventions (including telephone), emphasizing the focus on all forms of electronically delivered therapy as opposed to therapy administered in direct face-to-face contact.

The question has been raised, whether communication in IPIs can foster a stable therapeutic alliance because visual and auditory nonverbal cues are mostly absent and communication is often asynchronous. A widely used conceptualization of the term alliance by Bordin (1979, 1994) focuses on the collaborative nature of therapy and identifies the bond between therapist and patient as well as agreement on goals and tasks as components. Several reviews suggest that a stable therapeutic alliance can be achieved in IPIs (Berger, 2017; Pihlaja et al., 2018; Simpson & Reid, 2014; Sucala et al., 2012). Studies comparing therapeutic alliance ratings in different settings found alliance in remote contact equal to alliance in face-to-face contact in some cases (Anderson et al., 2012; Freeman, Duke, & Harris, 2013; Kiropoulos et al., 2008) and

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even superior in others (Andersson, Paxling et al., 2012; Chong & Moreno, 2012; Preschl, Maercker, & Wagner, 2011). These results suggest that a stable therapeutic alliance can be established in IPIs.

This does, however, not entail the conclusion that therapeutic alliance is of equal importance in IPIs as it is in face-to-face therapy. Several meta-analyses have shown a robust association between therapeutic alliance and outcome in face-to-face therapies, with the most recent studies showing moderate correlation estimates between r = 0.275 and r = 0.294 (Flückiger, Del Re, Wampold, & Horvath, 2018; Flückiger, Del Re, Wampold, Symonds, & Horvath, 2012; Horvath, Re, Flückiger, & Symonds, 2011), while older analyses revealed correlations between r = 0.22 (Martin, Garske, & Davis, 2000) an r = 0.26 (Horvath & Symonds, 1991). The causal direction of the association between therapeutic alliance and outcome has been debated, and it was shown that a good therapeutic alliance can act to enhance therapy outcomes (Falkenström, Granström, & Holmqvist, 2014; Xu & Tracey, 2015). This points to the relevance of fostering a stable alliance in order to promote efficacy of treatment in face-to-face therapies (Falkenström et al., 2014; Flückiger et al., 2018).

To assess whether fostering therapeutic alliance might be an equally useful strategy in IPIs, the association between therapeutic alliance and outcome needs to be examined. A growing number of studies have explored the relationship between therapeutic alliance and outcome in IPIs. Yet this research is still at an early stage. Recent narrative reviews suggested that in IPIs, therapeutic alliance and outcome are also associated (Pihlaja et al., 2018; Sucala et al., 2012). As an adjunct to the most recent meta-analysis on therapeutic alliance in face-to-face therapy, Flückiger et al. (2018) also provided a short analysis of alliance-outcome associations in "e-mental health or Internet-based therapy" (Flückiger et al., 2018, p. 15). We argue that the results of this meta-analysis should be regarded with caution due to several decisions the authors made concerning inclusion criteria: (1) Interventions without therapist contact (unguided self-help) were included. Alliance here refers to the alliance with the program itself and is thus not directly comparable to therapeutic alliance in other treatments that involve contact with a human therapist. (2) Online group therapy was included. In a group therapy setting, complex relational processes take place (e.g., alliance with the group itself and with a group therapist are highly correlated; Robak, Kangos, Chiffriller, & Griffin, 2013) and therefore therapeutic alliance may be associated with outcomes in a fundamentally different way than in individual therapy. (3) The authors included electronically augmented face-to-face treatments, where therapeutic alliance as well as treatment success are the result of a combination of face-to-face and remote contact. Thus alliance-outcome effects specific to remote contact cannot be identified. Those three factors might have influenced the association between therapeutic alliance and outcome found by Flückiger et al. (2018). The aggregated effect size of r = 0.275 reported by Flückiger et al. (2018) does reflect on alliance in interventions that include remote contact or content. There is, however, not enough information to discern the role of the alliance between therapist and patient in exclusively digital one-on-one therapy.

The aim of the current analysis was to learn more about the particularities of therapeutic alliance in IPIs. To this end, we decided to restrict our analysis to IPIs that were purely remotely administered by a human therapist (therapist-assisted IPIs). Thus, results may be particularly useful for understanding the role of therapeutic alliance in IPIs in comparison to conventional face-to-face therapies.

Original research articles produced mixed empirical evidences concerning the association between therapeutic alliance and treatment outcome. Variability in results may be attributable to methodological artifacts (e.g., sample size) or to differences in true effects (e.g., related to intervention topic or modality). A meta-analytic approach allows a weighted summary of existing evidence and exploration of possible sources of variability. To our knowledge, this is the first meta-analysis examining the association between therapeutic alliance and mental health outcomes (i.e., psychopathology) in exclusively remotely administered therapist-supported individual therapy for adult participants.

2. Method

This meta-analysis follows the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) (Moher, Liberati, Tetzlaff, & Altman, 2009). It was registered with PROSPERO (CRD42018086126).

2.1. Search procedure

The electronic databases PubMed/Medline, Web of Science, PsycINFO and PubPsych were systematically searched for papers published until February 2020 for articles that report on the association between therapeutic alliance and outcome in IPIs. The following search string was adapted to the respective databases:

(online* OR web* OR internet* OR computer* OR tele* OR iCBT OR app OR Mobile OR eHealth OR mHealth) AND (therap* OR treat* OR intervent*) AND (alliance OR "therapeutic relationship" OR "therapy relationship" OR "working relationship")

Search results were screened as follows: (1) duplicates were removed by digital object identifier; (2) titles and abstracts were screened for inclusion criteria; and (3) a full-text screening of all articles which were not excluded in the preceding step was carried out by the first author (JK). A randomly selected subset of 30 full-texts was also screened by the second author (FH). Inter-rater reliability was assessed using Cohen's kappa and discrepancies were resolved by discussion with the third author (AK).

Additional publications were acquired by scanning the references of included papers.

2.2. Eligibility criteria

The inclusion criteria for publications were:

(1) Publication of primary quantitative data in a peer-reviewed journal in English (A decision was made to include only English language articles, because English is regarded as the primary publishing language in the field of research.)
(2) treatment consisting of a remotely administered therapist-assisted intervention targeting mental health;
(3) the intervention was applied to adults (18 years or older) in an individual setting (no group interventions);
(4) therapeutic alliance is measured as participant rating during or at the end of treatment and the targeted mental health outcome is assessed post-treatment;
(5) the association between outcome and therapeutic alliance is reported.

2.3. Data extraction

Data from all studies matching the criteria were extracted with the help of a data extraction sheet containing the following variables: source (author, year), characteristics of the sample (N, gender, age), study design, intervention characteristics (including effect size), measures used for therapeutic alliance and mental health outcome and results (alliance-outcome association). When information on methodology was lacking and another paper on the research project was explicitly cited in the included paper, information was extracted from the additional paper.

The Cochrane Collaboration's tool for assessing risk of bias (Higgins & Green, 2012) was used to provide an estimation of study quality (see Appendix B). One criterion has been omitted as it was deemed mostly inapplicable and irrelevant in the context of the current meta-analysis: Blinding was assumed to be mostly not warranted, because the control

condition often was a wait list control group. Yet, we argue that blinding is of little relevance in our analyses because effect sizes were usually only drawn from one (the active) treatment group.

Pearson's product-moment correlation coefficient (r) was used as measure of effect size (ES) indicating the association of therapeutic alliance and outcome. The direction of correlations was standardized so that a positive correlation indicates higher therapeutic alliance to predict more favourable outcomes. Authors of papers that reported data that was not provided as r (e.g., beta-weights) or that reported incomplete data were contacted and, in case of no answer, reminded up to two times. If authors did not provide the r metric for an ES, reported data was transformed to r where possible with the R package "compute.es" (Del Re, 2013) or following the procedure suggested by Peterson and Brown (2005) for beta-weights (see Appendix A).

Change scores were preferred as outcome over plain post-intervention measurements and chosen where possible. Where no change score was available as outcome, models controlling for pre-intervention values were preferred to those without.

Only those outcome variables were chosen that reflect the target of intervention (e.g. depression but not anxiety in an intervention targeted at depressed individuals), because there might be a bias leading towards publishing associations of therapeutic alliance with additional outcomes if those are significant or large and omitting them if they are small and insignificant. This bias would lead to an overestimation of the association.

2.4. Data aggregation

For reporting purposes (but not for statistical analyses) one aggregated ES was computed for each independent sample that reported multiple relevant ESs. When associations were reported for multiple relevant outcome measures and/or for multiple measurement times or dimensions of therapeutic alliance, a composite correlation was computed under consideration of the correlation between outcomes using the R package "MAd" (Del Re & Hoyt, 2014). Where not reported within the included paper, a correlation between outcomes of $r = 0.7$ was assumed, because the chosen outcomes reflect one specific target of intervention and therefore the same or highly related constructs.

The original and converted ESs as well as composite ESs can be found in Appendix A.

Papers that provided data on two or more independent eligible samples were treated as separate data sets, while multiple papers on the same dataset were treated as one study with multiple reported associations.

2.5. Statistical analysis

For statistical analyses multiple ESs were not aggregated within samples. Instead, a three-level random effects analysis was conducted. A random-effects model was chosen to account for the heterogeneity of included studies, e.g., in treatment modality, content and targeted psychosocial outcome. A three-level meta-analysis was conducted to account for dependence of ESs which were nested within samples (Level 1: ESs, Level 2: within-samples model, Level 3: between-samples model) following the rationale by Cheung (2014). Pearson's product-moment correlation coefficients were transformed to fishers z values for computations and back to r values for reporting (Borenstein, 2009; Wasserman, Hedges, & Olkin, 1988). Heterogeneity was assessed via Q statistics and I^2 (Higgins, Thompson, Deeks, & Altman, 2003), which are both suited to describe the model fit of a tree-level model (Cheung, 2014). Publication bias was assessed using the non-parametric trim & fill method (Duval & Tweedie, 2000). In a second step, a moderator analysis was conducted to control for the effects of methodological decisions made in this article (conversion of effect sizes: yes vs. no; inclusion of papers that do not control for pre-intervention values: change score vs. post-measurement only). Properties of the studies of origin (times of measurement, targeted disorder, therapist contact frequency and mode, availability of self-help content) were also tested in moderator analyses. Because seven comparisons were made, Bonferroni correction was applied and resulted in $\alpha = .05/7 = 0.0071$ as criterion for significance.

For all analyses the statistical software "R" (R Development Core Team, 2008) was used with the additional packages compute.es (Del Re, 2013), MAd (Del Re & Hoyt, 2014) and metafor (Viechtbauer, 2010). Codes used for the analyses are provided in the OSF repository (https://osf.io/9p35k).

3. Results

The literature search described above yielded a total of 5859 articles, 1910 of which were removed as duplicates. After titles and abstracts of the remaining 3949 articles were screened, 210 publications were chosen for full-text screening. Of those, 19 met the eligibility criteria. A list of excluded studies is provided in Appendix C. One article was added after screening the references of relevant papers. Ultimately, 20 articles were included in this systematic review. Interrater-reliability between the first two authors in a screening of 30 randomly chosen full-texts was $\kappa = 0.902$. The procedure is summarized in Fig. 1.

3.1. Study characteristics

An overview of the included studies can be found in Table 1. Overall, 25 independent samples (20 articles) were found, which reported a total of 51 ESs.

The studies investigated IPIs with therapist assistance delivered via videoconferencing (Yuen et al., 2013), phone (Anderson et al., 2017; Beckner, Vella, Howard, & Mohr, 2007), chat (Gieselmann & Pietrowsky, 2016) and e-mail (Alfonsson, Olsson, & Hursti, 2016; Andersson et al., 2015; Andersson, Paxling et al., 2012; Beckner et al., 2007; Berger, Boettcher, & Caspar, 2014; Bergman Nordgren, Carlbring, Linna, & Andersson, 2013; Hadjistavropoulos, Pugh, Hesser, & Andersson, 2016, 2016; Herbst et al., 2016; Jasper et al., 2014; Knaevelsrud & Maercker, 2006, 2007; Preschl et al., 2011; Richards, Timulak, & Hevey, 2013; Rüegg, Moritz, & Westermann, 2018; Scherer et al., 2016; Wagner, Brand, Schulz, & Knaevelsrud, 2012) with mostly large effect sizes. Targeted diagnoses or syndromes include depression, anxiety, and posttraumatic stress among others. Intervention duration ranged from 1 to 16 weeks (mean 8.76). Therapist contact occurred mostly once a week and included the provision of therapy itself and/or feedback for therapy tasks. In some studies additional self-help content was available for participants.

All papers were inspected for methodological properties. The majority of studies were randomized controlled trials and two were

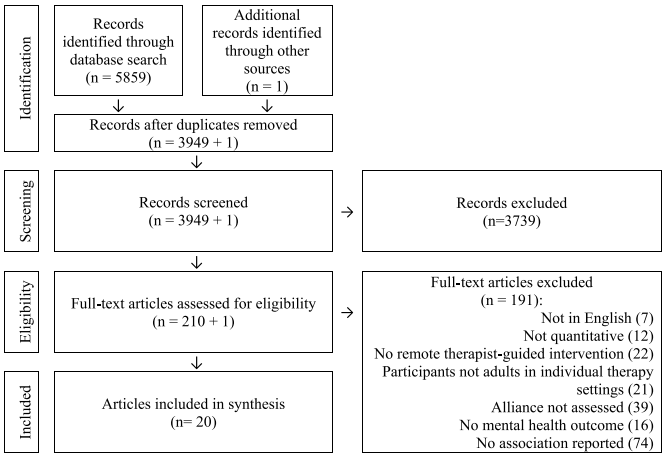

Fig. 1. Flowchart of included and excluded articles.

Table 1
Characteristics and results of included studies.

Study	Sample				Intervention			Results		
	No.	N Age (SD) % female	Disorder	Ana-lysis type	Contact mode (duration, c/w)	Content	Efficacy	Outcome Measure	Alliance Measure	Association
Alfonsson et al. (2015) Additional information from Alfonsson et al. (2016)	1	96 34.5 (13.1) y 73,2% f	Mild to Moderate Stress and Anxiety (no cutoff)	C	E-Mail (4w, 1 vs. 5c/w)	Applied relaxation, release-only and rapid relaxation, positive imagery, everyday training and maintenance	d = 0.51–0.80	Reliable Change on PSS (+-10)	WAI-S, mid-treatment	OR = 1.21 (95% CI 1.09–1.34) converted r = .05
Anderson et al. (2017)	2	56 51.1(9.4) y 33% f	Depression in persons with HIV (PRIME-MD diagnosis)	C	Telephone (9w, 1c/w)	**Interpersonal Therapy:** Psychoeducation, identification and addressing of problematic relationships	d = .53	BDI-II (controlling for pre value)	WAI-S, w 5	r = .14
Andersson et al. (2015)	3	96 34.9 (12.7) y 33.7% f	Obsessive-Compulsive Disorder (SCID diagnosis)	C	E-Mail (10w, 2-3c/w)	Psychoeducation, cognitive restructuring, exposure with response prevention	d = 1.55	Y-BOCS (change score)	WAI-S, w 3	r = .22[c]
Andersson, Paxling et al. (2012) Additional information from Andersson, Carlbring, & Furmark (2012); Paxling et al. (2011); Vernmark et al. (2010)	4	49[a] 38.9 (13.5) y	Depression (SCID diagnosis)	C	E-Mail (8w, 1c/w)	Behavioral activation, cognitive restructuring, sleep management, defining goals (e-mails)	d = 2.18	BDI (RGS)	WAI, w 3	r = .09 (N = 25)
	5	75% f		C		Behavioral activation, cognitive restructuring, sleep management, defining goals (guided self-help)	d = 1.39			r = .20 (N = 24)
	6	35 40.0 (11.2) y 80.6% f	Generalized Anxiety Disorder (PSWQ >5.7)	C	E-Mail (8w, 1c/w)	Psychoeducation, relaxation, cognitive restructuring, problem solving, sleep management	d = 1.17	PSWQ (RGS)	WAI-S, w 3	r = .13
	7	90 37.7 (11.4) y 59.3% f	Social Anxiety (SCID diagnosis)	C	E-Mail (9w, 1c/w)	Cognitive model & restructuring, attention training, social skills, exposure	d = .97	LSAS (RGS)	WAI-S, w 4	r = .10
Beckner et al. (2007)	8	48 48.6 (9.6) y 75.8% f	Depression in persons with multiple sclerosis (BDI-II≥16)	C	Telephone (16w/s, 1c/w)	**T-CBT:** Cognitive strategies, pleasant activities, problem solving, management of interpersonal difficulties	not available	HRSD, BDI-II (controlling for early response)	WAI, w 8	HRSD n.s. [d]; BDI-II *beta* = .39, converted r = .23
	9	49 47.4 (10.1) y 78.5% f		C		**T-SEFT** (supportive emotion focused therapy): Empathetic reflecting and encouragement of emotional expression	not available			HRSD n.s. [d]; BDI-II *beta* = −.15 converted r = −.08
Berger et al. (2014)	10	44 35 (10.9) y 59,1% f	Anxiety Disorders (≥1 SCID diagnosis)	ITT	E-Mail (8w, 1c/w)	**Tailored:** (for all relevant disorders) Motivation, psychoeducation, cognitive restructuring, mindfulness, exposure, problem solving	d = 1.09	BAI (change score)	WAI-SR, w 2, 4, 6	r = −.04 [c]
	11	44 34.4 (11.6) y 54,5% f		ITT		**Standardized:** (for one main disorder) Motivation, psychoeducation, cognitive restructuring, mindfulness, exposure, problem solving	d = 1.12			r = .25 [c]
Bergman Nordgren et al. (2013) Additional Information from Carlbring et al. (2011)	12	27 39.3 (11.2) y 67% f	Anxiety Disorders (≥1 SCID diagnosis)	C	E-Mail (10w, 1c/w)	Cognitive restructuring, symptom-specific modules, behavioral activation, applied relaxation, sleep	d = 1.24	CORE-OM (RGS)	WAI-S, w 3, 10	r = .45
Gieselmann and Pietrowsky (2016)	13	25 24.3 (3.4) y 48% f	Procrastination (elevated values)	C	Chat (1s, 1c/w)	Coaching for procrastination (manualized): punctuality, realistic schedules	not available	APSI (RGS)	WAI-SR, post	r = .49

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Table 1 (*continued*)

Study	Sample				Intervention			Results		
	No.	N Age (SD) % female	Disorder	Ana-lysis type	Contact mode (duration, c/w)	Content	Efficacy	Outcome Measure	Alliance Measure	Association
Hadjistavropoulos et al. (2016)	14	41 40.2 (12.6) y 69.5% f	Depression (PHQ-9>5)	C	E-Mail (12s, 1c/w)	Psychoeducation, cognitive and behavioral strategies, relapse prevention	ITT: d = 1.25	PHQ-9 (controlling for pre value)	TAQ s 6	r = .16
	15	58 40.2 (12.6) y 69.5% f	Generalized Anxiety Disorder (GAD-7>5)	C	E-Mail (12s, 1c/w)	Psychoeducation, cognitive and behavioral strategies (incl. exposure), relapse prevention	ITT: d = 1.07	GAD-7 (controlling for pre value)	TAQ, s 6	r = .10
Hedman et al. (2015) Additional Information from Hedman et al. (2014)	16	79 41.7 (13.6) y 79% f	Severe Health Anxiety (ADIS diagnosis)	ITT	E-Mail (12s, 1c/w)	**ICBT:** Systematic exposure, response prevention, Mindfulness	d = 1.8	HAI (change score)	WAI-S, w 1	*beta* = .21 (multivariate) converted r = .26
		79 41.4 (13.2) y 79% f				**IBSM:** Applied relaxation, stress management strategies	d = 1.2			
Herbst et al. (2016) Additional Information from Herbst et al. (2014)	17	29 35.6 (9.4) y 65% f	Obsessive Compulsive Disorder (DSM-IV diagnosis)	C	E-Mail (14s, 8w, 2c/w)	Psychoeducation, exposure, modification of maintaining factors	ITT: d = 0.82 (Y-BOCS) d = 0.87 (OCI-R)	Y-BOCS, OCI-R (change score)	WAI-SR, post	r = .31
Jasper et al. (2014)	18	38 51.9 (10.6) y 39.5% f	Distress in persons with tinnitus (THI ≥18 or MTQ ≥8)	C	E-Mail (10w/s, 1 c/w)	Focus exercising, cognitive restructuring, relaxation	not available	THI (RGS)	WAI-SR, w 2,5,9	r = .24
Knaevelsrud & Maercker (2006, 2007	19	41[b] 34 y 84% f	Posttraumatic stress (no cutoff)	C	E-Mail (10s, 5w, 2c/w)	**Interapy:** Self-confrontation (exposure in sensu), cognitive restructuring, social sharing	d = 1.4	IES-R ('06: RGS; '07: controlling for pre value)	WAI-S, w 2,5	r = .24
Preschl et al. (2011)	20	25 34.9 (9.5) y 84% f	Depression (BDI-II ≥ 12)	C	E-Mail (16s, 8w, 2c/w)	Behavioral analysis, activity planning, daily structure, cognitive restructuring, social competence, relapse prevention, life review, written disclosure	not available	BDI-II (RGS)	WAI-S, w 4, 8	r = .13
Richards et al. (2013)	21	20,14,19 26.5 (7.5) y 62% f	Depression (BDI-II >14 and < 29)	ITT	E-Mail (8w, 1c/w)	**Beating the Blues:** Cognitive restructuring, activity planning, problem solving	d = 2.3	BDI-II (post value)	WAI-SR, w 2, 4, 6	r = .47
Rüegg et al. (2018)	22	15 44(8.3) y 467.7% f	Schizophrenia (clinical diagnosis)	C	E-Mail (6w, 1c/w)	**Meta-Cognitive Training:** Psychoeducation, cognitive strategires	d = −0.31 - .27	Paranoia Checklist, CAPE (post value)	WAI-SR, mean of w 1 to 6	r = .10 [c]
Scherer et al. (2016)	23	31 32.9 (3.49) y 100% f	Stress and anxiety in preterm labor (no cutoff)	C	E-Mail (6w, 1c/w)	Psychoeducation, relaxation, activity, stress and problem solving protocol	not available	PSS, STAI-S, STAI-T (RGS)	WAI-SR, mean of w 2 to 5	r = .44
Wagner et al. (2012)	24	47 27.2 (7.0) y 81% f	Posttraumatic stress disorder (no cutoff)	C	E-Mail (10s, 5w, 2c/w)	**Interapy:** Self-confrontation (exposure in sensu), cognitive restructuring, social sharing	not available	PDS (change score)	WAI-S, w 2, 5	r = .42
Yuen et al. (2013)	25			ITT					WAI-S, w 2	r = .05

(*continued on next page*)

Table 1 (*continued*)

Study	Sample				Intervention			Results		
	No.	N Age (SD) % female	Disorder	Ana-lysis type	Contact mode (duration, c/w)	Content	Efficacy	Outcome Measure	Alliance Measure	Association
		21 35.0 (10.8) y 25% f	Social anxiety (SCID disorder)		Video-conferencing (12w, 1c/w)	Pychoeducation, in-session exposure and exposure homework, strategies, acceptance of anxiety	d = 1.23–1.91	SPAI, LSAS, Brief-FNE (RGS)		

Abbreviations: y = years; f = female; w = weeks; s = sessions; C = completer analysis; c/w = contacts per week; DSM-IV = Diagnostic and Statistical Manual of Mental Disorders IV; ITT = intention-to-treat analysis; RGS = residual gain score; **Measurement tools:** ADIS = Anxiety Disorders Interview Schedule; APSI = Academic Procrastination State Inventory; BAI = Beck Anxiety Inventory; BDI (II) = Beck Depression Inventory (version II); Brief-FNE = Fear of Negative Evaluation Scale; CAPE = Community Assessment of Psychic Experiences; CORE-OM = Clinical Outcomes in Routine Evaluation - Outcome Measure; GAD-7 = General Anxiety Disorder-7; HAI = Health Anxiety Inventory; HRSD = Hamilton Rating Scale for Depression; IES-R = Impact of Event Scale Revised; LSAS = Liebowitz Social Anciety Scale; MTQ = Mini Tinnitus Questionnaire; OCI-R = Obsessive Compulsive Inventory-Revised; PDS = Posttraumatic Diagnostic Scale; PHQ-9 = Patient Health Questionnaire 9; PRIME-MD = Primary Care Evaluation of Mental Disorders; PSS = Perceived Stress Scale; PSWQ = Penn State Worry Questionnaire; SCID = Structured Clinical Interview for DSM; SPAI = Social Phobia and Anxiety Inventory; STAI-S/STAI-T = State/Trait version of State-Trait Anxiety Inventory; TAQ = Therapeutic Alliance Questionnaire; THI = Tinnitus Handicap Inventory; WAI (S, SR) = Working Alliance Inventory (short, short revised); Y-BOCS = Yale-Brown Obsessive Compulsive Scale.

[a] Demographic data only available for main study sample.

[b] The two papers were integrated because they examine the same sample. Demographic data of the slightly larger '07 sample was chosen for display here. (2006: N = 40)

[c] Data from personal communication with authors.

[d] The correlation of alliance with the HRSD was reported only non-numerically to be nonsignificant. The respective r values were conservatively assumed to be zero.

uncontrolled trials (Rüegg et al., 2018; Yuen et al., 2013). Sample sizes range from 14 to 158 (mean 46.7, sum 1167). Most studies analysed completer samples, i.e., they did not impute missing measurement points, while four employed intention-to-treat analyses (Berger et al., 2014; Hedman, Andersson, Lekander, & Ljotsson, 2015; Richards et al., 2013; Yuen et al., 2013). All studies used validated instruments to assess therapeutic alliance and relevant outcomes. All but four papers (Knaevelsrud & Maercker, 2006, 2007; Scherer et al., 2016; Wagner et al., 2012) reported an explicit cut-off value concerning the targeted symptomatology to serve as inclusion criterion, ensuring that the intervention targeted individuals with issues of clinical relevance. All studies reported exclusion criteria concerning comorbidity or medication use. While in most studies therapeutic alliance was assessed during treatment, in some studies it was not assessed until the end of treatment (Gieselmann & Pietrowsky, 2016; Herbst et al., 2016; Knaevelsrud & Maercker, 2007). All studies except two (Richards et al., 2013; Rüegg et al., 2018) provided an association between therapeutic alliance and outcome that controlled for pre-intervention values of the outcome measure. Only one study reported that therapists were kept unaware of alliance values during the intervention (Anderson et al., 2017). Most papers provided explicit information on intervention content, duration and frequency of therapist contact. Most articles reported data in the form of a correlation coefficient, while six reported other data formats or insufficient information. The respective authors were contacted for data in the form of a correlation coefficient and three of them could provide the requested data (Andersson et al., 2015; Berger et al., 2014; Rüegg et al., 2018). For the remaining three articles, reported data was transformed to r according to the rationale described above (Alfonsson, Olsson, & Hursti, 2015, 2016; Beckner et al., 2007; Hedman et al., 2015).

A summary of risk of bias can be found in Table 2 and a verbose version can be accessed in Appendix B. A formal assessment of risk of bias (Higgins & Green, 2012) revealed that a considerable number of included articles did not report on adequate allocation concealment, and several articles reported incomplete outcome data due to dropouts which may have biased the association of interest in this meta-analysis. Also there was indication of selective outcome reporting in some cases, furthering the risk of bias.

In conclusion, all studies were deemed methodologically sound enough for inclusion in the analysis. Yet, some risk of bias was detected.

3.2. *Therapeutic alliance and outcome*

Reported ESs (k = 51) for the alliance-outcome association range from $r = -0.15$ to $r = 0.59$, while aggregated ESs for each independent sample (k = 25) range from $r = -0.08$ to $r = 0.49$. All samples are summarized in Fig. 2. The overall weighted average ES is $r = 0.203$ (95% CI [0.141, 0.263]; $N = 1167$), which significantly differs from zero ($p < .0001$). This can be deemed a small significant effect. Heterogeneity in ESs was small ($Q_{(50)} = 56.89$; $p = .23$) and correspondingly $I^2 =$ 27.7% indicates that variability between studies that cannot be explained by chance is small (Higgins et al., 2003). Publication bias was assessed with the trim & fill procedure (Duval & Tweedie, 2000) and yielded an estimation of zero missing studies. A funnel plot is shown in Fig. 3.

Converted ES estimates were not significantly different from non-converted ESs ($p = .22$), and ESs based on post-measurements of an outcome did not differ from those based on change scores ($p = .82$).

Alliance-outcome correlations were higher when therapeutic alliance was assessed near the end of treatment (next-to-last session or later: $r = 0.38$, $k = 8$; before next-to-last session: $r = 0.18$, $k = 43$; $QM_{(1)} = 7.43$, $p = .006$). Samples were grouped by targeted disorder (anxiety, depression, PTSD, obsessive-compulsive disorder (OCD) or other), and no differences between groups were found ($p = .56$). The frequency of therapist contact had no significant impact ($p = .30$). There was no difference between interventions in which all content was provided

Table 2
Risk of bias.

Study	No.	N	r	Adequate Sequence generation	Allocation Concealment	Incomplete Outcome Data Addressed	Free of selective reporting	Free of other Bias
Alfonsson et al. (2016)	1	96	0.05	+	?	–	–	+
Anderson et al. (2017)	2	56	0.14	+	?	–	?	–
Andersson et al. (2015)	3	96	0.22	+	+	?	+	+
Andersson, Paxling et al. (2012)	4	25	0.09	+	+	+	+	+
	5	24	0.20	+	+	+	+	+
	6	35	0.13	+	+	+	+	+
	7	90	0.10	+	+	+	+	+
Beckner et al. (2007)	8	48	0.23	+	?	?	–	+
	9	49	−0.08	+	?	?	–	+
Berger et al. (2014)	10	44	−0.04	+	?	–	?	+
	11	44	0.25	+	?	–	?	+
Bergman Nordgren et al. (2013)	12	27	0.45	+	?	–	–	–
Gieselmann and Pietrowsky (2016)	13	25	0.49	+	?	–	?	+
Hadjistavropoulos et al. (2016)	14	41	0.16	+	+	–	–	+
	15	58	0.10	+	+	–	–	+
Hedman et al. (2015)	16	158	0.26	+	?	?	+	+
Herbst et al. (2016)	17	29	0.31	+	?	?	+	–
Jasper et al. (2014)	18	38	0.24	+	?	–	?	+
Knaevelsrud & Maercker (2006, 2007)	19	41	0.24	+	?	+	?	–
Preschl et al. (2011)	20	25	0.13	+	?	–	?	+
Richards et al. (2013)	21	20	0.47	+	?	–	–	–
Rüegg et al. (2018)	22	15	0.09	/	/	–	+	–
Scherer et al. (2016)	23	31	0.44	+	?	–	–	+
Wagner et al. (2012)	24	35	0.47	+	?	–	+	+
Yuen et al. (2013)	25	21	0.07	/	/	+	+	+

+ = positive judgement, - = negative judgement, ? = no judgement possible, / = not applicable.

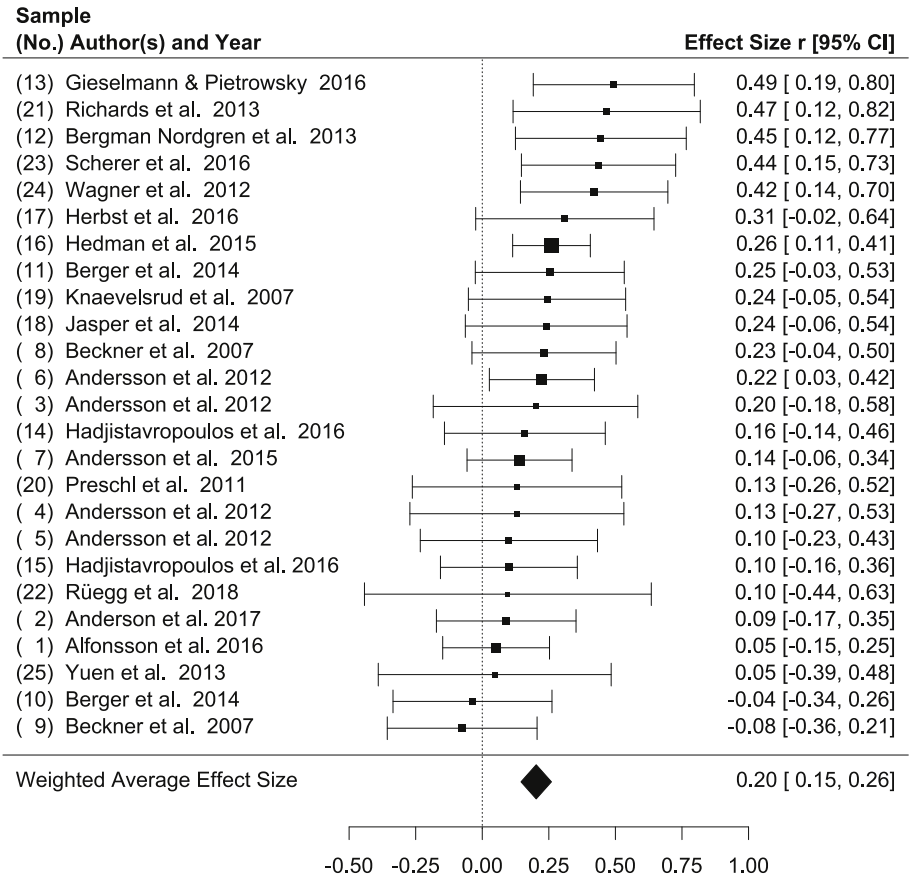

Fig. 2. Forest plot of effect sizes of all included studies and result of a three-level random effects (RE) model.

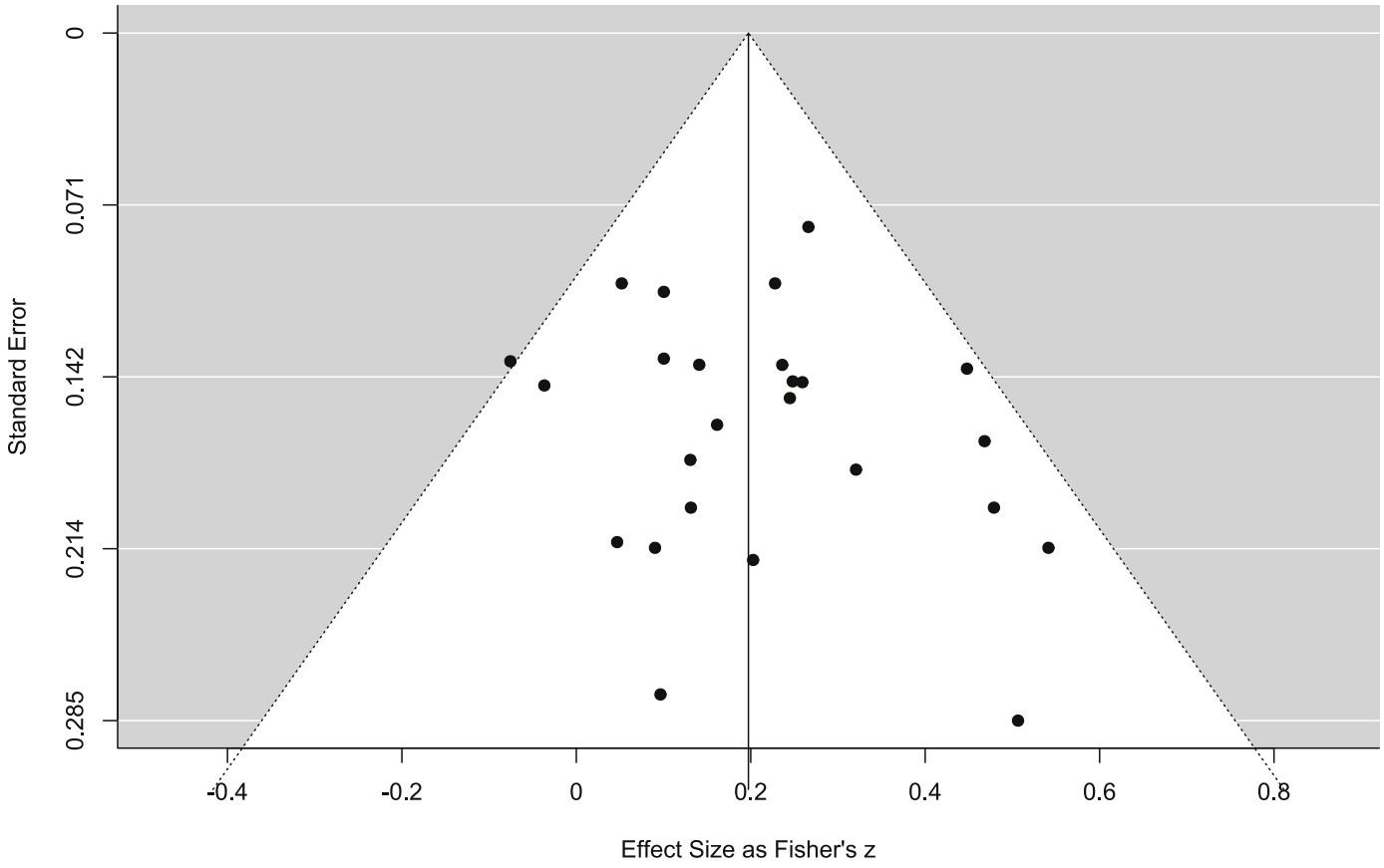

Fig. 3. Funnel plot of all included studies.

through communication with a therapist and those in which some self-help content was available ($p = .64$). No difference emerged between samples that employed written contact (e-mail, chat) versus oral forms (telephone, videoconferencing; $p = .09$).

4. Discussion

To our knowledge, the association between therapeutic alliance and outcome in therapist-assisted IPIs has not previously been examined in a meta-analysis. Several meta-analyses report a moderate association between therapeutic alliance and outcome in face-to-face therapy (Flückiger et al., 2012, 2018; Horvath et al., 2011; Horvath & Symonds, 1991). Interpretation of the results of a first meta-analysis on various forms of e-mental health and hybrids with face-to-face therapy (Flückiger et al., 2018) is limited due to the inclusion of very diverse studies. The current meta-analysis is the first to provide a systematic overview of research results that sheds light on the specifics of IPIs by restricting the focus to individual therapist-assisted standalone remote interventions.

Results from the 20 included papers revealed a small mean weighted ES of $r = 0.203$ (95% CI [0.141, 0.263]) for the association between therapeutic alliance and mental health outcomes in the described interventions. Heterogeneity was small and there was no indication of publication bias. Therefore it can be concluded that alliance is significantly associated to treatment outcome in IPIs, even though therapist-client contact is conducted in a fundamentally different way than in face-to-face therapies. However, there is indication that therapeutic alliance is somewhat less relevant for therapy outcomes in IPIs than in face-to-face therapy, because for face-to-face therapies alliance-outcome associations up to $r = .294$ were reported (Flückiger et al., 2012).

The current analysis provides an overall weighted ES which is also slightly smaller than that reported for e-mental health by Flückiger et al. (2018) ($r = 0.275$). Several factors may have contributed to this finding. In the current analysis, only specific outcomes were included, while Flückiger et al. also included more general outcomes such as quality of life and remarked that those have a stronger association with therapeutic alliance than specific outcomes focusing on symptoms. Also, unguided self-help, group interventions and interventions that used remote communication as an adjunct to face-to-face therapy were excluded in the current analysis, leading to a more homogeneous sample of remotely conducted therapist-assisted individual therapy. This sample may be characterized by a systematically smaller impact of therapeutic alliance on outcome than the mixed forms presented by Flückiger et al. (2018).

There was a trend towards the association between therapeutic alliance and outcome being larger when alliance was measured late in treatment. This is in line with previous findings from face-to-face therapies (Flückiger et al., 2018; Horvath et al., 2011). Alliance measured later in treatment may be confounded with treatment success and may be regarded more as an indicator of treatment success than as a predictor.

Neither targeted diagnosis, nor frequency or mode of therapist contact or availability of self-help content were associated with the strength of the alliance-outcome association, indicating a stable association which is independent of treatment content and design.

4.1. Limitations

The current meta-analysis includes studies with heterogeneous outcomes describing psychopathology. While the results serve to provide an overview of the association between therapeutic alliance and outcome in therapist-assisted IPIs it might be fruitful for future research to examine the association in groups that are more homogeneous with regard to, e.g., psychopathology or intervention modality.

Although a significant association between therapeutic alliance and

outcome was found, no definitive statements can be made regarding causality. Therapeutic alliance as well as symptom severity (outcome) may change gradually over the course of treatment. Therefore, good therapeutic alliance may cause greater desirable change in addressed symptoms, but early change in addressed symptoms may also increase a client's trust in their therapist, resulting in better therapeutic alliance. As suggested (Falkenström et al., 2014; Xu & Tracey, 2015) both constructs may be subject to mutual reinforcement. Therefore further research is needed to determine whether influencing therapeutic alliance may lead to better outcomes in IPIs.

No indication of a publication bias could be found, yet there may be studies that were not discovered because they were not published or not listed in the chosen databases. There may have been a bias towards investigators not publishing alliance-outcome correlations if they were not significant. However the risk of this particular bias is here deemed small because the majority of included studies indeed reported nonsignificant ESs.

Therapies conducted within randomized controlled studies with presumably high therapist adherence and credibility were overrepresented in the current analysis. This may have impacted treatment results as well as therapeutic alliance. Though it has been shown that therapist adherence and perceived competence are not related with outcome in face-to-face therapies (Webb, DeRubeis, & Barber, 2010), this may be different in online therapy.

Quality assessment with the Cochrane Collaboration's tool for assessing risk of bias (Higgins & Green, 2012) revealed that there was some risk of bias in several studies. Especially missing data and selective outcome reporting may have led to an overrepresentation of higher ESs and therefore overestimation of the mean weighted ES.

Many studies assessed multiple outcomes. Because there might be a bias leading towards publishing associations of therapeutic alliance with other outcomes if those are significant or large and omitting them if they are small or insignificant, only those outcomes were chosen for the current analysis that reflect the targeted symptoms. The chosen procedure yielded a conservative measure and can be assumed to counteract bias due to selective outcome reporting to some extent.

Additionally to this decision, a series of smaller methodological decisions were made (see Appendix A) to ensure that the overall weighed ES represents a conservative measure of the true effect. Therefore the reported overall weighed ES may underestimate the true effect, but it can be assumed with sufficient certainty that the true effect was not overestimated.

4.2. Implications for research and practice

The current analysis indicates that in IPIs therapeutic alliance and outcome are associated, irrespective of targeted symptoms, therapist contact frequency or mode (i.e. video, voice or text based communication). This calls for general attention towards therapeutic alliance in the design of IPIs. Specific attention should be paid towards elements of an intervention that may promote therapeutic alliance, such as design of communication modalities, individualized feedback or therapist credibility. Further studies are needed to asses which practices are adequate to promote therapeutic alliance in online interventions and to which extent fostering therapeutic alliance can improve outcomes. Newly developed IPIs may be strengthened by incorporating routine assessment of therapeutic alliance as a relevant therapeutic variable.

Self-help programs without any therapist support have proven effective (Clarke et al., 2002; Eckman et al., 2018; Karyotaki et al., 2017, 2018; Meyer et al., 2015; Ormrod, Kennedy, Scott, & Cavanagh, 2010), which raises the question under which circumstances the availability of a human therapist is necessary or beneficial. Further research could focus on whether a resemblance of therapeutic alliance can be fostered in the absence of a therapist, i.e. with the program itself, and where human contact provides added value. This may further our understanding of the peculiarities of alliance in IPIs, and clarify what makes therapist assistance beneficial.

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Declaration of competing interest

All authors declare that they have no conflicts of interest.

Appendix

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