Clinical Psychology Review 74 (2019) 101786

Contents lists available at ScienceDirect

Clinical Psychology Review

journal homepage: www.elsevier.com/locate/clinpsychrev

Review

The effectiveness of initial therapy contact: A systematic review

Katie Aafjes-van Doorn[a,*], Kristen Sweeney[b]

[a] Yeshiva University, New York, NY, USA
[b] Adelphi University, Garden City, NY, USA

HIGHLIGHTS

- Few empirical studies have examined the direct effect of initial therapy contacts.
- Initial therapy contact is more effective than no treatment or a waiting list.
- Initial therapy contact might be as effective as 6-session treatment.
- Several effective initial therapy formats and approaches reduce patients' symptoms.

ARTICLE INFO

Keywords:
Initial sessions
Single-session
Effect
Systematic
Review

ABSTRACT

'Initial therapy contacts', defined as (the first) 3 h or less of face-to-face psychological treatment, encompassing both the early phase of a longer therapy and one-off single session therapies, are seen as a critical phase of treatment. However, little is known about the direct effect of initial therapy contacts on change in common symptoms typically presented by patients in psychological therapy services. Our systematic literature search resulted in 35 identified empirical studies on the effect of initial therapy contacts. These studies were analyzed in three stages: 1) A systematic comparison of study characteristics using the preferred reporting items for systematic reviews and meta-analyses; 2) A domain-based evaluation of methodological rigor of the studies, in line with Cochrane's guidelines on assessing risk of bias; 3) A narrative synthesis of reported findings.

The considerable variability in therapy format (a stand-alone single session, 2 + 1 format, or initial session of multisession therapy) and study design (post/pre-post measurement, with/without control) limited comparability of studies. The quality assessment indicated that the majority of studies had relatively weak methodologies overall. Qualitative synthesis of the effectiveness results suggests that a significant proportion of patients reported benefits, including symptom change. This positive effect is especially clear when compared to no-treatment controls, and appears to be maintained at follow-up. The findings suggest that a broad range of initial therapy formats, could in itself be beneficial to patients in primary care treatment settings, and that further research is warranted.

The initial hours of therapy are viewed as a critical phase in treatment. The importance of "initial therapy contacts", here defined as the face-to-face therapeutic interaction between the therapist and the patient in (the first) 3 h or less of psychological treatment, is reflected in various strands of research. Firstly, original enthusiasm for single-session therapy stems from the days of Freud, and was more recently ignited when Talmon and his colleagues found that at 3 to 12-month follow-ups, 59% of patients reported that a single session had been sufficient and led to improvement in the presenting problem (Hoyt & Talmon, 1990; Talmon, 1990). Secondly, in the majority of practice-based settings, most patients only attend a few sessions, less than five on average (Olfson & Pincus, 1994), and the modal number of appointments attended by outpatients is only one session (Hoyt & Talmon, 1990). Thirdly, exploration of the dose-effect relationship (Howard, Kopta, Krause, & Orlinsky, 1986; Lambert, Hansen, & Finch, 2001) in session-by-session outcome research has consistently shown that therapeutic changes are not spread evenly across all sessions, but tend to be concentrated early in therapy (Lambert et al., 2001). This rebutted the assumption that patients who cease to attend after the initial sessions are unmotivated drop-outs; instead, it is possible that not returning for more therapy is a decision made in light of patient's perceived improvements following the initial sessions (Hymmen, Stalker, & Cait, 2013). Moreover, for those patients who complete the full treatment, change early in therapy has been shown to be a good

* Corresponding author at: Ferkauf Graduate School of Psychology, Yeshiva University, Rousso Building, 1165 Morris Park Avenue, Bronx, NY 10461, USA.
E-mail address: katie.aafjes@yu.edu (K. Aafjes-van Doorn).

https://doi.org/10.1016/j.cpr.2019.101786
Received 13 May 2018; Received in revised form 17 July 2019; Accepted 25 September 2019
Available online 09 November 2019

predictor of later treatment outcome (e.g., Fennell & Teasdale, 1987; Haas, Hill, Lambert, & Morrell, 2002; Szegedi et al., 2009).

1. Literature reviews on the effectiveness of initial therapy contact

A number of researchers have attempted to quantify the therapeutic benefits of single-session, or very brief treatments, in both narrative and systematic reviews. Five narrative reviews have been conducted on the effect of single-session therapy for a variety of disorders (Bloom, 2001; Campbell, 2012; Hurn, 2005; Rockwell & Pinkerton, 1982) and walk-in psychotherapy (Cameron, 2007). These reviews concluded that, in general, patients, are satisfied with the single-session, and many benefit from a reduction of intra-psychic and interpersonal problems, to the extent that they require no further psychological therapy. Bloom (2001) also suggested that additional controlled outcome studies were urgently needed to evaluate the conditions under which the single-session intervention is most appropriate. Although these narrative reviews are helpful in illustrating this clinical area of single-session treatments, these were not intended as formal overview of the literature. The validity of their conclusions is unknown, because the methodological adequacy of the reviewed studies was not considered and their 'ad hoc' selection of outcome studies possibly reflects a selection bias.

Moreover, several systematic reviews have been conducted on single-session debriefing following trauma, which a number of studies suggest is unhelpful (e.g., Bisson, 2010; Rose, Bisson, & Wessely, 2003; van Emmerik, Kamphuis, Hulsbosch, & Emmelkamp, 2002). Three further systematic reviews have reported on the positive effects of single-session treatments in specific contexts. For example, reviews have shown that single-session exposure therapy for phobia (Zlomke & Davis III, 2008) and single-sessions of motivational interviewing for people with addictions (Lundahl & Burke, 2009) were more effective than no treatment. Also, Hymmen et al. (2013) conducted the most recent systematic review of single-session interventions conducted within family counselling services, and identified 18 studies that examined the effect of pre-planned walk in sessions for children and families. Their findings suggest that single-session family interventions can result in improvement in specific problems, such as depression, anxiety, general distress level, parenting skills and possibly self-harm. In line with the other reviews, they emphasized the need for more standardized measures and control groups in these studies. In addition, a different type of single-session treatment was reviewed by Poston and Hanson (2010), who conducted a meta-analysis of the therapeutic impact of psychological assessments. They reported that psychological assessment procedures, consisting of formal diagnostic tests and feedback on the results, positively affected treatment processes and outcomes, significantly more so than diagnostic assessments that were not fed back to patients. Although relevant, this focus on diagnostic test results in initial therapy contact is very specific to therapy services in the United States of America (USA), where insurance companies request a diagnosis to enable funding for treatment. This difference in focus makes it difficult to generalize these therapeutic assessment findings to psychological services where formal psychological assessment procedures and diagnoses are not routinely undertaken (e.g. United Kingdom, see Llewelyn & Aafjes-van Doorn, 2017). Overall, the very narrow scope of these reviews on interventions, specific to particular presentations and treatment settings, limits the clinical relevance of these findings as they do not tell us about the role of initial therapy contacts in adult psychological services more generally.

2. Aims of the current review

This systematic review is designed to complement the existing literature, by providing an empirical understanding of the breath of initial therapy contacts relevant to general psychological therapy services, and its effect on common mental health problems. In contrast to the session-by-session outcome literature that provides an alternative and complementary perspective to our research question (Lambert et al., 2001), this review focuses on studies that explicitly report on the assessment of the direct effect of initial therapy contacts. More specifically, we looked to explore the following two research questions: a) Are stand-alone single sessions effective? b) Are initial sessions in multisession therapy effective?

The studies identified in the systematic literature search are analyzed in three stages; 1) A systematic comparison of the study characteristics using the preferred reporting items for systematic reviews and meta-analyses (PRISMA); 2) A domain-based quality assessment to evaluate the methodological rigor of the identified studies, in line with Cochrane's guidelines on assessing risk of bias (after Thomas, Ciliska, Dobbins, & Micucci, 2004); 3) A narrative synthesis of the reported findings.

Given the suggestions of aforementioned previous reviewers, a particular focus in this review is on methodological issues and limitations. More rigorous studies are more likely to yield results that are closer to the truth, and differences in risks of bias may help explain heterogeneity of results. For clarity of presentation, the studies' characteristics, methodological quality, and findings are described in the results-section and the resulting limitations are described in the discussion-section of this paper.

3. Method

3.1. Systematic search

Given the complexities of searching a large, disparate literature base, a number of steps were taken to ensure the search was systematic. Firstly, the review was informed by published guidance for systematic reviews of evaluations of health care interventions (Liberati et al., 2009), including the five 'PICOS' components (population, intervention, comparators, outcome and study design) identified as Preferred Reporting Items for Systematic reviews and Meta-Analyses (PRISMA). Secondly, operational definitions were used to identify and clarify constructs of interest. 'Initial *therapy contact*' was defined broadly as the face-to-face interaction between a therapist and a patient, involving a verbal exploration of the patient's current problems, problem development and goals, based on a psychological framework. These therapeutic interactions were classified as '*initial* therapy contact' if it involved 3 h of individual contact or less, which could include more than one appointment. As defined here, 'initial therapy contact' refers to a variety of therapy models and encompasses both the early phase of a longer therapy and one-off single-session therapies. Both efficacy and effectiveness studies were included in the review.

The literature review was conducted using the following digital databases: AMED, BNI, CINAHL, EMBASE, MEDLINE, PsychInfo, Health Business Elite and Scopus. Search terms included variations on the terms for (a) initial therapy contact (single-session, initial session, first session, initial assessment, therapeutic assessment, one-session, two-plus-one, very brief therapy, ultra brief therapy, ultra brief psychotherapy, very brief psychotherapy, therapeutic consultation, initial appointment, initial contact, trial therapy, intake interview or pre-therapy), (b) treatment outcome (efficacy, effect or outcome) and (c) psychological therapy (therapy, psychotherapy, counsel*, treatment or intervention). The variations of the three terms, resulted in 255 ($17 \times 3 \times 5$) separate search term combinations.

Five inclusion criteria were used: (i) the study was reported in the English language and published in a peer-reviewed journal before March 2018; (ii) the study reported on one or more adult patients of working age (18–65), and not exclusively on children or adolescents (e.g., Ollendick et al., 2009; Schleider & Weisz, 2017) or older adults (e.g. Nowlan, Wuthrich, Rapee, Kinsella, & Barker, 2016) in line with our focus on general adult mental health services, and the majority of existing therapy research (Talley, 1992); (iii) the study reported on

individual therapy interventions, rather than family treatments or team consultations, (e.g. Hymmen et al., 2013; Miller & Slive, 2004); (iv) the initial hours of therapy addressed either particular mental health disorders (identified by, for example, a Diagnostic and Statistical Manual of Mental Disorders diagnosis (DSM) or validated symptom measures, or commonly associated symptomatology e.g., self-harm and insomnia. We thus excluded studies on parenting stress (e.g. Sommers-Flanagan, 2007) and developmental, neurological or physical disorders (such as chronic obstructive pulmonary disease examined by Kunik et al., 2001); (v) therapeutic benefit of the initial therapy contact was measured in terms of symptom relief, regardless of whether further psychological treatments were offered afterwards. This therapeutic benefit could be assessed through patient-, therapist- or observer-rated measures, including (un)standardized measures, verbal feedback, and therapist observation, and (vi) the focus of the intervention did not overlap with the narrowly focused single-session treatments that were reported in recently published reviews (e.g., trauma debriefing by Bisson, 2010; single-session exposure therapy for phobia by Zlomke & Davis III, 2008; or motivational interviewing by Lundahl & Burke, 2009). If the study did not fulfil all inclusion criteria, it was excluded from the review.

This literature search was conducted twice by the first author, and all steps of the systematic search were repeated by the second author to double check the accuracy of the search findings. These three systematic searches identified the same set of 35 empirical studies to be included in this review.

Fig. 1 shows a PRISMA diagram of the flow of sources through the literature search.

3.2. Quality assessment

Methodological quality of studies was assessed in accordance with the domain-based evaluations for assessing risk of bias in systematic reviews, described in the Cochrane Collaboration handbook (Higgins & Green, 2008), and followed the specific quality assessment guidelines developed for the Effective Public Health Practice Project (EPHPP; Thomas et al., 2004). In line with Cochrane guidelines (Higgins & Green, 2008), we tested our quality assessments of risks of bias on a pilot sample of six articles to ensure that criteria were applied consistently, and that consensus could be reached between the raters.

Various domains (selection bias, study design, confounders, blinding, data collection methods, withdrawals and dropouts) were rated as 'strong', 'moderate', or 'weak'. In terms of "Selection bias", studies received a strong rating when they included over 80% of approached participants and used a representative patient/therapist sample, a moderate rating when they included 60–79% of approached participants, and a weak rating for lower percentages or when information on sample selection was not stated. Studies received a strong rating for "Study design" when they employed a randomized controlled trial (RCT) or controlled clinical trial, a moderate rating for pre-post cohort studies, case control studies, or interrupted time series and a weak rating for all other designs. The category "Confounders" was rated as strong when authors controlled for more than one confounder, moderate when they controlled for one confounder, and weak when they did not control for any confounders. The next category 'Blinding' was deemed strong if both outcome assessor and study participants were blind to intervention status and/or research question. If outcome assessor or study participants were blinded, it was rated as moderate. If neither one was blinded, or no blinding procedures were reported, this category was rated as weak. For "Data collection", studies received a strong rating when they reported good overall validity and reliability of their used measures or when the psychometric properties were widely accepted (e.g. BDI), a moderate rating when it was reported that validity/reliability testing was currently in progress, and a weak rating when no evidence of validity and reliability was provided or published. Studies received a strong rating for the category "Withdrawals and dropouts" when they included over 80% of participants in follow-up, medium when this was 60–80% and weak when they included < 60% of participants in follow-up or did not report attrition. When a study was cross-sectional (i.e. one-off measurement), did not involve follow-up measurement, or reported on a single case, this category was rated as not applicable. The overall quality rating for each study was determined by assessing the six domain ratings (Thomas et al., 2004). Studies with at least four strong ratings and no weak ratings were considered "strong". Those with less than four strong ratings and one weak rating were considered of "moderate" quality, and studies with two or more weak ratings were considered weak.

4. Results

4.1. Overall study characteristics

4.1.1. Study designs

The 35 studies used different study designs, including controlled outcome studies with pre-post measurement (n = 17), controlled outcome studies with post measurements only (n = 4), uncontrolled pre-post outcome studies (n = 9), and outcome studies without baseline measurement (n = 5), and included four case studies. The study designs applied by Barkham, Shapiro, Hardy, and Rees (1999), Barkham, Rees, Stiles, Hardy, and Shapiro (2002); Lessard et al. (2012) and McManus, Van Doorn, and Yiend (2011) were most comprehensive in that they included two active treatments (single-session interventions) and compared this with pre-post change in no-treatment/waiting list conditions. Twenty-one studies included follow-up measurements.[1]

Of all reviewed studies, 32 were effectiveness studies and only three studies (Gawrysiak, Nicholas, & Hopko, 2009; Gellis, Arigo, & Elliott, 2013; Goerling et al., 2014) fulfilled the essential features of an efficacy design, i.e. inclusion of a control group, randomization and no concurrent (e.g. medication) treatments (Peleikis & Dahl, 2005). Given the fact that study designs that use randomization of patients are more robust, and limit the possibility of selection bias (Deeks et al., 2003), more weight may be given to the (positive) results of the 12 RCTs. However, the 9 non-randomized controlled studies reported no significant differences in patient characteristics between the intervention and the control group, suggesting that failure to use a randomization procedure may not have had much impact.

4.1.2. Outcome measures

The reviewed studies also differed substantially on the type of outcome measures they used. For example, some studies used in-session data based on therapist observation (Barkham & Hobson, 1989), patient verbal feedback during the session that implied symptom change (n = 2; Freeman & Jackson, 1996; Gangdev, 1998), or in-session physiological measures (Goerling et al., 2014). In other studies, researchers conducted phone interviews to evaluate the progress made since the initial therapy contact, using a self-designed satisfaction interview that included a question on symptom change (Silverman & Beech, 1984). Some studies used behavioral reports of suicide frequency as a measure of effectiveness (Lamprecht et al., 2007). Other studies implied symptom change based on patients' answers on self-designed outcome scales that suggested they were better at handling problems (Johnson, Whitaker, & Porter, 1980) or did not require further therapy (Hersch & Lathan, 1985). The majority of studies used a variety of standardized general symptom measures and specific anxiety or depression measures. Seven studies included patients with a particular score on the Beck Depression Inventory (BDI) (Beck, Ward, Mendelson, Mock, &

[1] Some studies used the term "follow-up" even when they did not report on post-measurements, whereas others only used the term "follow-up" for measurements that followed post-measurements. In our review, the reported follow-up and post-measurements reflect the terms used by the authors of each respective paper.

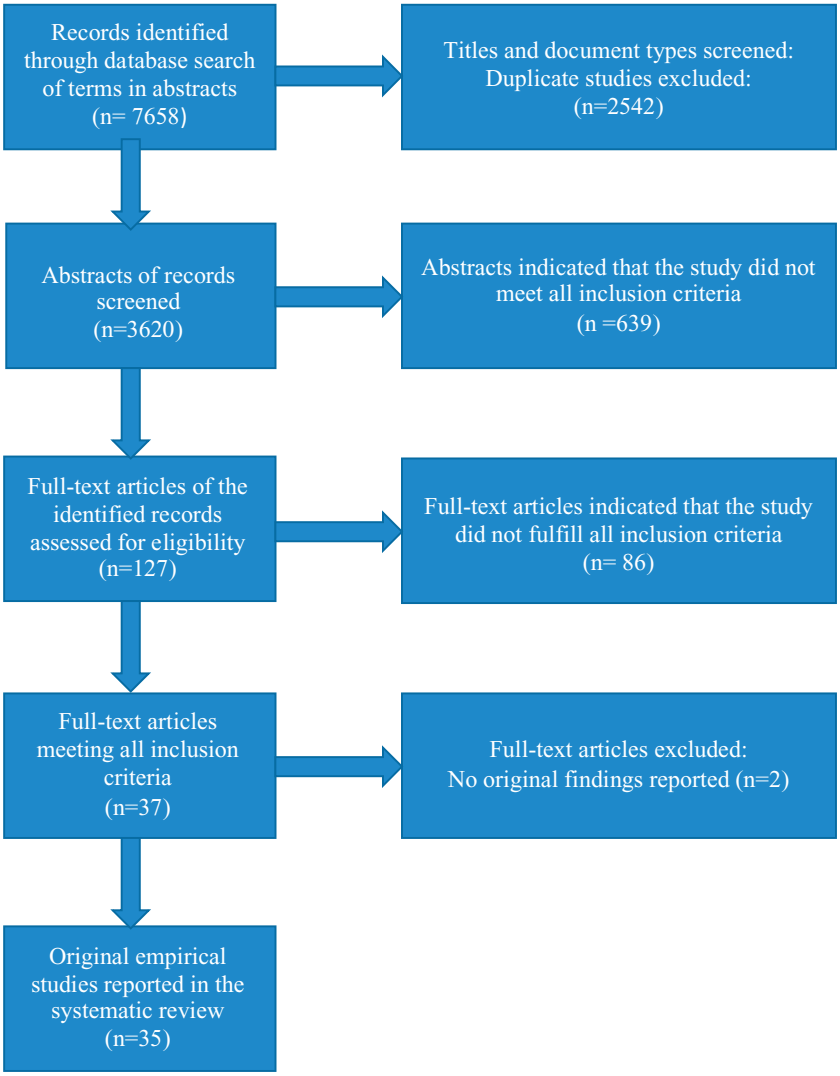

Fig. 1. Search strategy for the review.

Erbaugh, 1961) but used different clinical cut-off points and, thus, could not be easily compared.

4.1.3. Patients

Eleven of the 35 studies included patients with a particular DSM diagnosis; most studies either reported no particular inclusion criteria (n = 16) or included patients with mild symptoms (n = 8) without a DSM diagnosis, or used non-clinical samples (e.g., Danitz & Orsillo, 2014; Danitz, Suvak, & Orsillo, 2016; Goerling et al., 2014; Kashdan, Adams, Read, & Hawk, 2012; Stalker et al., 2015). The patients in the studies experienced a variety of mental health problems, such as depression, anxiety and eating disorders, common in outpatient settings. Six studies explicitly reported that they included patients who had been in previous therapy, five studies reportedly excluded patients who had previously been in therapy, whereas the majority of the studies did not report on previous therapy experiences (n = 14).

4.1.4. Therapists

In the majority of the studies the number of therapists was unreported (n = 9) or only one therapist conducted the initial sessions (n = 13), meaning that the effect of the initial therapy contact may have been caused by the particular therapist rather than the intervention under investigation. Thirteen studies (e.g. Barkham, Moorey, & Davis, 1992; Busch, Kanter, Landes, & Kohlenberg, 2006; Coppock, Owen, Zagarskas, & Schmidt, 2010) controlled for therapist effects by using multiple therapists, ranging from two to twenty therapists.

4.1.5. Interventions

The identified empirical studies reported on a number of different formats of initial therapy contact that might be beneficial. These included a stand-alone single-session (n = 29), two sessions (n = 1) (Barkham et al., 2002) and two-plus-one interventions (n = 5), with a range of duration from 20 min (Hersch & Lathan, 1985) to 3 h (Abbass, Joffres, & Ogrodniczuk, 2008). The length of the initial therapy contact was not specifically addressed in any of the studies. The studies that included therapy contact longer than 1 h (e.g. the 2 + 1 therapy in the studies by Barkham and colleagues) was not explicitly compared to briefer interventions.

Most studies reported on the effect of the initial therapy contact based on a CBT model, either delivered as a combined cognitive behavioral intervention (n = 12), a solely cognitive intervention (n = 3), or solely behavioral activation (n = 2). Others were based on interpersonal psychotherapy (n = 2), different models of psychodynamic therapy (n = 3) (Aafjes-van Doorn, Macdonald, Stein, Cooper, & Tucker, 2014; Abbass et al., 2008; Barkham & Hobson, 1989), or did not explicitly identify the psychological approach (n = 6).

Table 1
Quality assessment of the 35 reviewed studies.

First author, year of publication	Selection bias	Design	Confounders	Blinding	Data collection methods	Withdrawals and dropouts
Aafjes-van Doorn, 2014	W	M	S	W	S	S
Abbass, 2008	W	M	W	W	S	W
Armento, 2012	M	S	S	W	S	S
Askevold, 1983	M	M	M	W	W	S
Barkham, 1989	W	W	W	W	S	S
Barkham & Hobson, 1989	W	M	N/A	W	W	N/A
Barkham, 1992	M	S	W	W	S	M
Barkham, 2002	W	S	W	W	S	M
Barkham, 1999	M	S	M	W	S	S
Beckham, 1989	W	M	W	W	S	M
Busch, 2006	W	M	W	W	S	S
Coppock, 2010	M	M	W	W	S	S
Day, 1993	W	S	M	W	M	S
Danitz, 2014	W	S	M	W	S	W
Danitz, 2016	W	S	M	W	S	M
Dunn, 2006	W	S	M	W	S	M
Eustis, 2017	W	M	S	W	S	W
Freeman, 1996	W	W	N/A	W	W	N/A
Gangdev, 1998	W	W	N/A	W	W	N/A
Gawrysiak, 2009	M	S	M	W	S	S
Gellis, 2013	W	S	M	W	S	S
Goerling, 2014	W	S	M	W	S	S
Hersch, 1985	W	W	W	W	W	N/A
Hutchinson, 1988	W	M	M	M	S	N/A
Johnson, 1980	W	W	W	W	W	N/A
Kashdan, 2012	W	S	S	W	M	S
Lamprecht, 2007	M	M	W	W	W	M
Lessard, 2012	W	S	S	W	W	S
McManus, 2011	M	S	S	W	S	S
Muris, 1995	W	M	W	W	W	S
Nuthall, 2007	M	S	W	W	S	S
Silverman, 1984	M	M	S	M	W	S
Stalker, 2015	W	S	S	W	S	S
Tolchard, 2006	W	M	N/A	W	S	N/A
Turner, 2013	W	S	S	M	S	S

Note: W = weak; S = strong; M = moderate; N/A = not applicable.

4.1.6. Concurrent treatments

Of the 35 studies, only five (Busch et al., 2006; Gawrysiak et al., 2009; Gellis et al., 2013; Goerling et al., 2014; Nuthall & Townend, 2007) ensured that there was no concurrent treatment by excluding people who were on medication, whereas twenty studies did not report medication usage. The other ten studies included patients who used medication in their sample, which means that one cannot screen out the effect of the initial therapy contact from that of the medication, especially as some form of interaction may have occurred. Several authors (e.g. Armento, McNulty, & Hopko, 2012) argued it was unlikely that medication affected the results of their studies because their response to the medication was required to be stable for at least five to six weeks.

4.2. Quality of study methodology

See Table 1 for an overview of the quality of methodologies of the 35 studies. In terms of "Selection bias", none of the studies were rated as strong, and ten studies were rated as moderate. Twenty-five studies were rated as weak, because they did not report how many people were approached to participate, had a participation rate of < 60%, or used a non-representative convenience sample. Most studies received a strong rating on "Study design" (n = 17), whereas the quality of the design was deemed moderate in thirteen studies and weak in five studies. The category "Confounders" was rated as strong in nine studies, moderate in ten studies and weak in twelve studies and was deemed not applicable to the four case studies. Almost all studies received a weak rating for "Blinding" (n = 31), whereas four studies received a moderate rating. Twenty-three studies received a strong rating for "Data collection", two studies received a moderate rating, and nine studies received a weak rating, because no evidence of validity and reliability was provided or published. For "Withdrawals and dropouts", nineteen studies received a strong rating, six studies received a moderate rating and three studies a weak rating. For the other seven studies, this category was not applicable, because they used a cross-sectional design, single case or did not include follow-up data. Calculation of overall quality ratings, resulted in seven studies of moderate methodological quality and twenty-eight studies with weak overall methodology ratings.

4.3. Study findings

To reflect the disparity in quality of study designs and the degree to which conclusions can be drawn, the 35 reviewed studies are organized into four sections (see Tables 2–5). The results of the outcome studies that did not include a control condition or baseline measurement (Table 2) will be described first, because only minimal conclusions can be drawn from these relatively weaker designs (Thomas et al., 2004). This will be followed by the results of slightly more robust study designs, which also did not include a control condition, but offered a comparison of symptom scores before and after the initial therapy contact (Table 3). Then, the studies with an arguably stronger design, which included a control condition but again no baseline measurement, will be described (Table 4). Lastly, the most robust study designs, which included both a control condition and measurements before and after the intervention (Table 5) will be described. As robustness of design increases, the direct effects of initial therapy contacts become clearer.

4.3.1. Uncontrolled outcome studies with post-measurement only

Of the fourteen descriptive studies, five studies only measured the effect of the initial therapy contact afterwards and did not compare these outcomes with baseline scores (Table 2). All five studies reported

Table 2
Uncontrolled outcome studies using post-measurement only.

Study	Therapy intervention					Study design		Patient population				Outcome		
First author, year of publication	Initial therapy contact	Start of longer therapy	Model	Length in min.	*N*	Post: x weeks post session	Follow-up: x months post session	Therapists	Treated problem	Inclusion criterion	Meds	Previous therapy	Measures	Post	Follow-up
Barkham & Hobson, 1989	2 + 1	No	BET	.	1	0	.	1	Ph	.	.	.	Observation	+	.
Freeman, 1996	Single session	No	CBT	45	1	0	.	1	PD/A/D	.		Yes	Verbal feedback	+	.
Gangdev, 1998	Single session	Yes	CT	.	1	1,2	4	1	OCD	DSM	.	.	Verbal feedback	+	+
Hersch, 1985	Walk-in	No	.	20	100	0	.	.	EP/IP/D	.	Yes	.	Unstandardized measure	+	.
Johnson, 1980	Walk-in session	Yes	.	35	.	0	.	< 20	D/A/IP/Id	.	.	.	Unstandardized measure	+	.

Note: = No information available.
Therapy Intervention: BET = Brief Exploratory Therapy; CBT = Cognitive Behavioural Therapy; CT = Cognitive therapy.
Patient Population: A = Anxiety; D = Depression; DSM = Diagnostic and Statistical Manual of Mental Disorders diagnosis; EP = Emotional Problems; Id = Identity problems; IP = Interpersonal Problems; OCD = Obsessive Compulsive Disorder; PD = Personality Disorder; Ph = Phobia of thunder.
Outcome: + = Positive effect on at least one of the measures.

positive effects of the initial therapy contact. Two of the three case studies reported on patients' positive verbal feedback, that was later interpreted as symptom improvement by the therapist (Freeman & Jackson, 1996; Gangdev, 1998). Freeman's patient, for example, stated that "it was helpful in countering the negative thoughts", which according to Freeman "led to a lifting of the concomitant depression and a diminution in the self-injurious behavior." (Freeman & Jackson, 1996, pp. 207).

The other three studies reported on improvements on unstandardized measures ($n = 2$) or satisfactory observations by the therapist (Barkham & Hobson, 1989). In the two studies that used an unstandardized questionnaire to obtain written feedback, a reduction of symptoms was implied if patients thought that they were better at handling the problem (Johnson et al., 1980) or if they said that they did not require further therapy to deal with the problem (Hersch & Lathan, 1985). All but one study (Gangdev, 1998) used sub-clinical samples with low scores on symptom measures. Gangdev (1998) reported a positive effect at follow-up, the other three studies did not include follow-up measurement. Moreover, three of the five studies were single-case studies. Data on the therapy model (Hersch & Lathan, 1985; Johnson et al., 1980), length of intervention (Barkham & Hobson, 1989; Gangdev, 1998) and sample size (Johnson et al., 1980) were unreported in several of the studies.

4.3.2. Uncontrolled outcome studies with pre- and post-measurements

Nine descriptive studies compared symptom severity after the initial therapy contact with a baseline measurement prior to the intervention (Table 2). In comparison to the brief interventions examined in the other five descriptive studies (20–45 min), these pre-post outcome studies all reported on longer initial therapy contact (ranging from 84 to 150 min). All studies used standardized outcome measures and reported a reduction in symptoms following the initial therapy contact. For example, according to Barkham and Hobson (1989), two out of three patients showed a 30% reduction in symptoms after the initial therapy contact. Abbass et al. (2008) reported that patients improved to such an extent that 30% of patients did not need further therapy to deal with their problems. All four studies that included follow-up measurements, reported that these gains were maintained over time, from 4 weeks (Eustis et al., 2017) to 6 months after the intervention (Barkham & Hobson, 1989). Although, Eustis et al. (2017) initially included a relatively large sample size of 78 pre-intervention, their follow-up sample was significantly smaller ($n = 29$). Tolchard, Thomas, and Battersby (2006) reported on a single-case study.

4.3.3. Controlled outcome studies with post-measurement only

Four of the controlled outcome studies evaluated treatment outcome by asking patients about their symptoms post-intervention (Table 3) and three of these studies reported positive feedback after the initial therapy contact. However, rather than a comparison of pre- and post-intervention data, comparison was made in relation to a control group. Askevold (1983) conducted phone interviews to ask patients about the symptom change/recovery following a psychological interview and compared this with phone interview data on the effect of two longer-term therapies. Silverman and Beech (1984) also conducted phone interviews and compared the reported level of solved mental health problems in a single-session with a longer-term therapy. In contrast, Hutchinson, Krippner, and Hutchinson (1988) used standardized symptom measures to compare the effect of an intake interview with that of no treatment. Lamprecht et al. (2007) measured self-harm frequency at a hospital's emergency department for a year following a single-session, compared with treatment as usual. Similar to Barkham and Hobson (1989), Askevold (1983) reported that 60% of people had said they had 'recovered totally' after their initial interview.

When the reported symptoms of patients following the initial therapy contacts were compared with the reported symptoms of a control group, only one study found the initial therapy contacts to be

Table 3
Uncontrolled outcome studies incorporating pre- and post-measurements.

Study	Therapy intervention					Study design			Patient population				Outcome		
First author, year of publication	Initial therapy contact	Start of longer therapy	Model	Length in min.	*n*	Post: x weeks post session	Follow-up: x months post session	Therapists	Treated problem	Inclusion criterion	Meds	Previous therapy	Measure	Pre- post change	Pre- follow-up change
Aafjes-van Doorn, 2014	Single session	No	EDT/STDP	150	31	2	.	2	GD	DSM	Yes	Yes	CORE-TAF/OM, BSI-18, GAF, IIP-32, SCQ-SF, ATOS	+*	.
Abbass, 2008	Trial therapy	Yes	ISTDP	84	30	5	.	1	D/A/Ad/PD/S	DSM	Yes	.	BSI/IIP	+*	.
Barkham, 1989	2 + 1	No	CBT	180	3	2	6	1	D	BDI	.	.	SCL-90	+	+
Beckham, 1989	Initial session	Yes	CBT	.	23	0	.	.	D	DSM	.	.	SCS/VAS/CTI/DACT	+	.
Busch, 2006	Initial session	Yes	CT	.	38	0	.	4	D	DSM	No	.	BDI	+	.
Coppock, 2010	Initial session	Yes	Div.	.	43	0	.	10	D/A/IP	.	.	.	OQ-45/ SHS	+*	.
Eustis, 2017	Single session	No	ABBT	90	29	0/1	1	1	D/A	.	Yes	Yes	DASS-21, BFNE, SDS	+*	+
Muris, 1995	Single session	No	ET	150	36	.	24	1	P	.	.	.	M BSS, SPQ, BAT	.	+*
Tolchard, 2006	Single session	No	ET	.	1	.	6	.	G	DSM	.	.	BDI, SCL-90-R, SOGS, GSCL	.	+

Note: = No information available.
Therapy Intervention: ABBT = Acceptance-Based Behavioral Therapy; CBT = Cognitive Behavioral Therapy; CT = Cognitive therapy; DBT = Dialectical Behavior Therapy; Div. = Ddiverse therapies depending on the therapist; EDT = Experiential Dynamic Therapy; ET = Exposure Therapy; ISTDP = Intensive Short Term Dynamic Psychotherapy. POI = Psycho-Oncological Intervention; STDP = Short-Term Dynamic Psychotherapies.
Patient Population: A = Anxiety; Ad = Adjustment Disorder; BDI = Beck Depression Inventory score; BFNE = Brief Fear of Negative Evaluation Scale; D = Depression; DSM = Diagnostic and Statistical Manual of Mental Disorders diagnosis; G = Gambling; GD = General Distress; GSCL = Gambling Severity Checklist; IP = Interpersonal Problems; P = Phobia; PD = Personality Disorder; S = Somatoform Disorder; SI = Suicidal Ideation.
Outcome: ATOS = Achievement of Therapeutic Objectives Scale; BAT = Behavioral Approach Test; BDI = Beck Depression Inventory; BSI = Brief Symptom Inventory; CORE-OM = Clinical Outcomes in Routine Evaluation - Outcome Measure; CORE-TAF = Clinical Outcomes in Routine Evaluation -Therapy Assessment Form; CTI = Cognitive Triad Inventory; DACT = Depression Adjective Checklist; GAF = Global Assessment and Functioning; GSCL = Gambling Severity Checklist; DASS-21 = Depression Anxiety and Stress Scales–21 Item Version; HADS = Hospital Anxiety and Depression Scale; IIP = inventory of Interpersonal Problems; MBSS = Miller Behavioral Style Scale; OQ-45 = Outcome Questionnaire; PSQ = Perceived Stress Questionnaire; SCL-90 = Symptom Checklist SCL-90-R = Symptom Checklist-90-Revised; SCS = Self Control Scale; SDS = Sheehan Disability Scale; SCQ-SF = Self-compassion Questionnaire - Short Form; SHS = State Hope Scale; SOGS = South Oaks Gambling Screen; SPQ = Spider Phobia Questionnaire; VAS = Visual Analogue Scale.
+ = Positive effect on at least one of the measures.
* $p < .05$ on at least one of the measures.

Table 4
Controlled outcome studies using post-measurement only.

Study	Therapy intervention				Comparator			Study design			Patient population				Outcome			
First author, year of publication	Initial therapy contact	Start of longer therapy	Model	Length in min.	*N*	Type of control	Randomized	*n* control	Post: x weeks	Follow-up: x months	Therapists	Treated problem	Inclusion criterion	Meds	Previous therapy	Measure	Post	Follow-up	Between group[a]
Askevold, 1983	Psych. Interview	No	.	90	23	Al/Al	No	16/12	.	108	.	ED	DSM	.	.	Phone interview	.	+	−/−
Hutchinson, 1988	Intake interview	No	Cou.	50	65	N	Yes	61	0	.	7	D/A/IP/Gr	.	.	.	BDI/STAI	.	.	−
Lamprecht, 2007	Single session	No	SFBT	90	40	N	No	302	.	12	.	SH	.	.	No	Self-harm frequency	.	+	+
Silverman, 1984	Single session	Yes	CMHT	60	47	Al	No	54	.	12	.	IP/EP	.	.	.	Phone interview	.	+	−

Note:. = No information available.
Therapy Intervention: CMHT = Community Mental Health Team intervention; Cou. = Counselling; SFBT = Solution Focused Behavioral Therapy.
Comparator: Al = active control group (longer-term therapy); N = no treatment.
N control: n / n = size of two types of longer-term therapy control groups in the study.
Patient Population: A = Anxiety; D = Depression; DSM = Diagnostic and Statistical Manual of Mental Disorders diagnosis; ED = Eating Disorder; EP = Emotional Problems; Gr = Grief; IP = Interpersonal Problems; SH = Self-Harm.
Outcome: BDI = Beck Depression Inventory; STAI = State Trait Anxiety Inventory.
+ = Positive effect on at least one of the measures; − = The intervention is similarly effective or less effective than the control condition.
[a] Difference between the intervention and control condition.

more effective than the control condition. Lamprecht et al. (2007) found that a single therapy session was more effective than no treatment in reducing self-harm within the next year. In contrast, Hutchinson et al. (1988) found no difference in symptoms of anxiety and depression between people who attended a single-session and those who did not receive any treatment. Similarly, the two studies that compared initial sessions with longer-term therapy found that the initial therapy contact was less effective in reducing symptoms than longer-term therapy (3–10 or > 10 sessions; Askevold, 1983; > 5 sessions; Silverman & Beech, 1984). Altogether, three of the four studies indicated that patients found the initial therapy contact helpful, although less helpful than longer-term therapy.

4.3.4. Controlled outcome studies with pre- and post-measurements

The most robust study designs measured change pre- and post-intervention and included a control condition (Table 4). In line with the results of the uncontrolled studies, all of these seventeen controlled studies reported an improvement in symptoms following the initial therapy contact. Ten of the seventeen studies reported a statistically significant improvement on at least one of the outcome measures and eight of the thirteen studies that reported on follow-up measurements showed that significant improvement maintained over time. Five studies reported a clinically and reliably symptom reduction for between 29% and 93% of the patients (Barkham et al., 1992; Barkham et al., 1999; Barkham et al., 2002; Gawrysiak et al., 2009; Gellis et al., 2013), whereas other studies reported on significant symptom changes in patients (e.g., Lessard et al., 2012; Turner, Hambridge, Baker, Bowman, & McElduff, 2013) or non-clinical samples (e.g., Danitz & Orsillo, 2014; Danitz et al., 2016; Goerling et al., 2014; Kashan, Adams, Read & Hawk, et al., 2012; Stalker et al., 2015).

In relation to the control conditions, the results of these seventeen studies were less straightforward. When the effect of the initial therapy contact was compared to a waiting list or no-treatment control ($n = 8$), the initial therapy contact was clearly more effective in reducing symptoms than the control group, as one might expect (Stalker et al., 2015). Although some initial therapy contact was more effective than treatment as usual (Danitz et al., 2016; Stalker et al., 2015), this was not the case in other studies (Barkham et al., 1992; Nuthall & Townend, 2007). Of the four studies that compared the initial therapy contact with another active, brief intervention of the same length (e.g. single-session or 2 + 1) but a different therapy model, two studies (Barkham et al., 1999; McManus et al., 2011) found no difference between the two very brief interventions, whereas Armento et al. (2012) reported a more beneficial effect of their single-session behavioral activation compared to a single-session supportive control. Moreover, Goerling et al. (2014) found a significant decrease in anxiety for both the single-session psycho-oncological therapy and relaxation intervention but depression significantly decreased in the single psycho-oncological therapy group only. When comparing the initial therapy contacts with longer treatments, Barkham et al. (2002) reported that two initial sessions were less effective than a control condition of either 6 or 12 sessions, and this was in line with the findings of Askevold (1983) and Silverman and Beech (1984). However, Lessard et al. (2012) and Turner et al. (2013) did not find a significant difference between a single CBT session and a 6 or 7-session CBT treatment. The twelve studies that used a randomized allocation of patients reported that the initial therapy contact was more effective than a control group. However, four of the five pre-post studies in which the allocation of patients was not described as randomized reported the initial therapy contact as either equally effective or less effective, irrespective of the type of control group.

5. Discussion

Thirty-five empirical studies on the effectiveness of initial therapy contacts were identified. In line with previous reviews (e.g. Hymmen et al., 2013), our quality assessment indicated that the majority of

Table 5
Controlled outcome studies incorporating pre- and post-measurements.

No	Therapy Intervention					Comparator			Study Design			Patient Population				Outcome			
First author, year of publication	Initial therapy contact	Start of longer therapy	Model	Length in min.	*n*	Type of control	Rando-mized	*n* control	Post: x weeks	Follow-up: x months	Therapists	Treated problem	Inclusion criterion	Meds	Previous therapy	Measures	Pre- post change	Pre-follow-up change	Between group[a]
Armento, 2012	Single session	No	BARB	60	25	A	Yes	25	2	1.5	1	D	BDI	Yes	.	BDI/EROS/ BAI/STAI/ QOLI	+*	+*	+*
Barkham, 1992	2 + 1	No	CBT	180	21	TAU	No	6	0	12	10	D	BDI	.	.	BDI/SCL-90	+*	+*	−/.
Barkham, 1999	2 + 1	No	CBT/IPT	180	29/28	W	Yes	59	0	3,12	3	D	BDI	Yes	No	BDI/SCL-90/ IIP	+	+	+/+*
Barkham, 2002	Two sessions	No	CBT/ IPT	180	39	Al/Al	No	34/32	0	2–3	3	D	BDI	Yes	No	BDI-SCL-90/ IIP	+	+	−
Day, 1993	2 + 1	No	CBT	180	40	W	Yes	29	0	1	3	Alc/D/ Ang	.	.	.	SADDQ/ SAV/SADQ/ LCB	+	+*	+*
Danitz, 2014	Single Session	No	CBT	90	49	W	Yes	49	.	3	2	D/A	.	.	.	DASS-21, PHL-MS, VLQ	.	+*	−/+*
Danitz, 2016	Single session	No	ABBT	75	119	TAU	No	94	.	3	2	D	.	.	.	DASS-21, VLQ, PHL-MS	.	+	+*
Dunn, 2006	MET	Yes	MI	60	45	N	Yes	45	0	4	1	ED	DSM	.	.	EDDS/ EDEQ/ URICA	+*	+*	+*
Gawrysiak, 2009	Single session	No	BATD	90	14	N	Yes	16	2	.	2	D	BDI	No	No	BDI/EROS/ BAI/ MSPSS	+*	.	.
Gellis, 2013	One session	No	CRT-I	30	27	A	Yes	24	4	.	1	I	DSM	No	.	ISI, PSAS, PHQ-9, GAD-7	+*	.	+/+*
Goerling, 2014	Single session	No	POI	30	17	A	Yes	18	0	.	.	D/A/S	.	No	.	PSQ, HADS	+*/−		+/+
Kashdan, 2012	Single session	No	ET	60	16	W	Yes	16	0	.	1	P	.	.	.	FSQ, ADIS-IV	+*	.	+*/.
Lessard, 2012	Single session	No	CBT	120	24	Al/N	Yes	19/15	0	3,6	1	PD	DSM	Yes	Yes	ADIS/IV/ BSQ/ACQ/ PAS/ASI/ CAQ	+*	+*	−/+*
McManus, 2011	Single session	No	CT/BT	30	30/31	N	Yes	30	0	1	2	OCD	BR	.	Yes	OBQ-44/IBI /BR	+*	+	−/+
Nuthall, 2007	Single session	No	CBT	45	12	TAU	No	9	0	1,3	.	PA	DSM	No	No	PDSS-SR	+*	+	−
Stalker, 2015	Single-Session	No		90	229	TAU	No	142	.	1,2	.	.	.	Yes	Yes	GHQ - 12	.	+*	+/+

(*continued on next page*)

Table 5 (*continued*)

No	Therapy Intervention					Comparator			Study Design			Patient Population				Outcome			
First author, year of publication	Initial therapy contact	Start of longer therapy	Model	Length in min.	*n*	Type of control	Rando-mized	*n* control	Post: x weeks	Follow-up: x months	Therapists	Treated problem	Inclusion criterion	Meds	Previous therapy	Measures	Pre- post change	Pre-follow-up change	Between group[a]
Turner, 2013	Single session	No	CBT, EDMR, PD CBT	60	25	AI	Yes	32	.	2,6,12	1	D/A	BDI-II	Yes	.	BDI-II, HADS, HADS-A, SCID, OTI, CTS	.	.+*	.+/−

Note:. = No information available.

Therapy Intervention*:* ABBT = Acceptance Based Behavioral Therapy; BARB = Behavioral Activation of Religious Behavior; BATD: Behavioral Activation Treatment for Depression; BT = Behavioral Therapy; CBT = Cognitive Behavioral Therapy; CRT-I = Cognitive Refocusing Treatment for Insomnia; CT: Cognitive Therapy; EDMR = Eye Movement Desensitization and Reprocessing; ET = Exposure Therapy; IPT = Interpersonal Psychotherapy; MI = Motivational Interviewing; PD = Psychodynamic Therapy; POI = Psycho-oncological Therapy.

n & *n* = Size of two different single-session interventions.

Comparator*:* A = Active control group (similar short intervention); Al = Active control group (longer-term therapy); N = No treatment; TAU = Treatment as usual; W = Waiting list; ../.. = Two different control conditions.

n/n = Size of two types of longer-term therapy control groups in the study.

Patient Population: Alc = Alcohol problems; Ang = Anger; D = Depression; ED = Eating Disorder; I = Insomnia; INT = Intrusive Thoughts; OCD = Obsessive Compulsive Disorder; PA = Panic Attack; PD = Panic Disorder.

Outcome: ACQ = Agoraphobic Cognitions Questionnaire; ADIS-IV = Anxiety Disorder Interview Schedule for DSM-IV; ASI = Anxiety Sensitivity Index; BAI: Beck Anxiety Inventory; BDI = Beck Depression Inventory; BDI-II = Beck depression inventory, second industry; BR = Belief Rating; BSQ = Body Sensations Questionnaire; CAQ = Cardiac Anxiety Questionnaire; CTS = Cognitive Therapy Scale; DASS-21 = Depression Anxiety and Stress Scales–21 Item Version; DSM = Diagnostic and Statistical Manual of Mental Disorders diagnosis; EDDS = Eating Disorder Diagnostic Scale; EDE-Q = Eating Disorder Examination Questionnaire; EROS = Environmental Reward Observation Scale; FSQ = Fear of Spiders Questionnaire; GAD-7 = Generalized Anxiety Disorder Scale; GHQ-12 = General Health Questionnaire; HADS = Hospital Anxiety and Depression Scale; HADS-A = Hospital.

Anxiety and Depression Scale-Anxiety; IBI = Irrational Belief Inventory; IES = Impact of Event Scale; IIP = Inventory of Interpersonal Problems; IMI = Intrusive Memory Interview; ISI = Insomnia Severity Index; LCB = Locus of Control of Behavior Sale; MSPSS = Multidimensional Scale of Perceived Social Support; OBQ-44 = Obsessional Beliefs Questionnaire-44; OTI = Opiate Treatment Index; PANAS = Positive and Negative Affective Scale; PAS = Panic and Agoraphobia Scale; PDSS-SR = Panic Disorder Severity Scale; PHL-MS = Philadelphia Mindfulness Scale; PHQ-9 = Patient Health Questionnaire-9; PSAS = Pre-Sleep Arousal Scale; PSQ = Perceived Stress Questionnaire; SADDQ = Short Alcohol Dependency Data Questionnaire; SADQ = States of Anxiety and Depression Questionnaire; SAV = Survey of Attitudes to Violence; SCID-I = The Structured Clinical Interview for the DSM-IV; SCL-90 = Symptom Checklist; SCID = Structured Interview for DSM-IV; STAI = State Trait Anxiety Inventory; QOLI = Quality of Life Inventory; URICA = University of Rhode Island Change Assessment Scale; VLQ = Valued Living Questionnaire;.

\+ = Positive effect on at least one of the measures; − = The intervention is similarly effective or less effective than the control condition.

[a] Difference between the intervention and control condition.

* $p < .05$ on at least one of the measures.

reviewed studies had relatively weak overall methodologies. Studies widely differed in the rigor of their research design and, for example, included four uncontrolled single-case designs, as well as twenty-one controlled studies, of which twelve RCTs and three efficacy studies. Findings of the present review support the conclusion that (the first) 3 h or less of therapy can possibly be an effective intervention in itself for adults with mild to moderate mental health problems. Importantly, reported effects appeared to last over time. All studies that included a follow-up measurement in their design reported that the positive effect of the initial therapy contact had been maintained several months or even years after the intervention, even if no further therapy occurred after the initial therapy contact. Some studies stated that a proportion of patients (e.g. 30% in Abbass et al., 2008) derived sufficient benefit that they did not require further treatment. Although this was not specifically addressed in any of the studies, there appeared to be no difference in outcomes between the effects of initial therapy contact when it consisted of stand-alone sessions ($n = 27$) or when it consisted of the start of longer therapy ($n = 8$). For example, a 3-hour stand-alone therapy spread over different sessions (e.g. 2 + 1) appeared to have comparable outcomes to a 3-hour session at the start of a longer treatment (Abbass et al., 2008). This suggests that these very brief interventions may be effective in a variety of different formats.

5.1. Stand-alone session(s)

The majority of studies in this review reported on stand-alone session(s). Two specific types of effective stand-alone therapies were identified. First, a "single session therapy", referred to a planned single-session intervention. The single session may be previously scheduled or provided in a "walk-in counselling clinic" (e.g. Hymmen et al., 2013). Another type of effective stand-alone initial therapy contact has been developed by Barkham and colleagues (Barkham et al., 1999). Their "two-plus-one model" (2 + 1) reflects a very brief three session intervention, comprising of two 1-hour sessions one week apart, followed by a third 1-hour session three months later. The results of this review are in line with other reviews of single-session therapies (e.g. Bloom, 2001; Cameron, 2007; Rockwell & Pinkerton, 1982) and stand-alone therapeutic assessments (Poston & Hanson, 2010), which showed that stand-alone single session(s) in a variety of therapies are effective in reducing symptoms.

5.2. Initial session(s) in multisession therapy

In contrast to stand-alone sessions, "initial sessions" refer to the first session(s) of several, or rather, the start to longer therapy. Of the 35 reviewed studies, only 8 studies examined the effectiveness of initial session(s). As mentioned previously, these studies showed comparable outcomes to stand-alone single session(s) intervention. Notably, only one of these eight studies was conducted as RCT (Dunn, Neighbors, & Larimer, 2006), the others used relatively weaker study designs. This means more research comparing the effect of an initial session to overall pre- post treatment change is needed to identify any "first session gains" (Busch et al., 2006) that set the course of the therapy as a whole (Lambert & Ogles, 2004).

There are several hypotheses as to why initial sessions of multisession therapy might be particularly effective. Frank's (1974) theory of remoralization suggests that the first hours of therapy are likely to lead to a decrease in symptoms because they help to clarify a patient's problems, inspire hope and provide experiences of success (see also Howard's phase model of change; Howard, Lueger, Maling, & Martinovich, 1993; Stulz & Lutz, 2007). In addition to remoralization, others have drawn on goal-setting theory (Locke & Latham, 2006) and the Theory of Planned Behavior (Ajzen, 1988) and emphasized the importance of early changes in the patient's expectations of therapy (e.g., anticipatory beliefs about what will happen during or because of therapy) (Constantino, 2012; DeFife & Hilsenroth, 2011). According to Ajzen's (1988) Theory of Planned Behavior, beliefs about expected outcome, self-efficacy concerning necessary 'patient role' behaviors and motivation to achieve improvement determine intention to engage and, therefore, the success of therapy. Locke and Latham's (2006) goal setting theory similarly suggests that an individual's expectations of therapy may be linked to how much the individual is motivated to engage in working toward their goals and, therefore, achieve symptom change.

It has to be noted, however, that these theories of remoralization, goals, and planned behavior not necessarily explain therapeutic benefits of planned single-session therapy or very brief interventions as stand-alone treatment. Moreover, these theories do not explain why initial therapy contact seemed to be effective irrespective of treatment modality that was used, with vast theoretical and technical differences. Whilst it should be stressed that no statistical comparison between the different models has been attempted, the findings appear to be congruent with the 'equivalence paradox' or 'Dodo-bird verdict' (Luborsky et al., 2002). These common factors may have an effect on initial therapy contact irrespective of further therapy offered and thus seem to contradict the expectations hypothesis (Constantino, 2012; Greenberg, Constantino, & Bruce, 2006). Similarly, the fact that longer-term therapies were not always more effective than the initial therapy contact is inconsistent with the dose-effect literature which identified a relatively larger effect early in therapy, and continuing but diminishing levels of improvement over time.

The extent to which conclusions can be drawn from the reviewed studies was limited by their respective internal validity (i.e. whether the study results can be attributed to the effect of the initial therapy contact, or whether they might be a result of other factors, such as concurrent treatments, type of control group or therapist effects unrelated to the model of treatment) and external validity (i.e. whether the findings can be legitimately generalized to other people and situations in clinical practice). First, it is possible that we have underrated the overall quality of the reviewed studies in our methodological assessment. For example, two well-designed efficacy studies (Gellis et al., 2013; Goerling et al., 2014), received an overall weak quality rating, due to their reported low participation rates. Also, attempts to assess risk of bias are often hampered by incomplete reporting of what happened during the conduct of the study (Higgins & Green, 2008). It is therefore possible that the reviewed studies were of higher internal validity than indicated in our systematic review, because important details of the applied study methodologies were left out in the final publications of the studies.

Second, publication bias may have impacted the results of this systematic review by alluding to a treatment efficacy when non-published studies failed to replicate such findings (Liberati et al., 2009). Whilst a publication bias should be considered within any systematic review, the existence of negative reporting (e.g. Hutchinson et al., 1988) suggests that it is unlikely a publication bias would have significantly impacted the findings of the current review. Notably, the majority of the more robust study designs were reported in the most recent publications, possibly indicating the current focus in psychotherapy research on evidence-based practice and increased standards of peer-reviewed journals.

Third, some of the results of the descriptive studies might have been affected by a response bias, when patients' inclination to give positive feedback when contacted by the service (Battaglia, Shapiro, & Zell, 1996), which could explain why the patients in both the longer-term therapy control groups and the initial therapy contact reported symptom improvement/ recovery in the phone-interviews. Alternatively, in the case of the study by Askevold (1983), the results might also have been affected by patients' memory of the content, as there was an extraordinary nine years between intervention and follow-up.

Besides these aspects of internal validity, evidence for the external validity of the findings also appears to be mixed. The populations in the studies appear representative of mainstream adult mental health

services with respect to age and gender balance (Barkham et al., 2001). However, many relevant patient and therapy characteristics were not reported. Future research would, for example, benefit from detailed descriptions of treatment setting (e.g., outpatient, crisis clinic), level of therapist training, therapist orientation and location of services (e.g., rural/urban).

Moreover, the majority of the studies involved patients with mild to moderate mental health problems, often subclinical, which means that the conclusions are confined to treatments for patients with relatively mild problems, that is, consistent with the severity of problems of patients seen in primary care mental health services (Barkham et al., 2001; Haaga, 2000). However, this also resulted in a small scope for reduction in scores on standardized measures, and might thus understate the potential efficacy of initial therapy contacts. The impact of initial therapy contacts on interpersonal or personality problems was not addressed in the majority of studies reviewed and, therefore, the current studies cannot challenge the existing notion that these difficulties take longer to change (Hardy et al., 1995; Lambert & Ogles, 2004; Merbaum & Butcher, 1982). It may be useful, in future research, to explore the impact of initial therapy contacts for more severe presentations, such as those seen in, for example, secondary and tertiary care services. To know beforehand for whom ultra-brief therapy is enough (e.g. patient factor) would be extremely important knowledge for the cost-effectiveness and efficiency of mental health services.

In order to develop the evidence-base of initial therapy contact further, it is crucial to determine not only the patient and treatment moderators but also the mediators of improvement. An important next step could be to examine the relative benefits of key process elements, such as alliance (Hilsenroth & Cromer, 2007), expectations and hope (Constantino, Arnkoff, Glass, Ametrano, & Smith, 2011) that form early in therapy and are assumed to mediate the effectiveness of initial sessions (Messer & Wampold, 2002). Given that the reviewed empirical studies did not report on moderation or mediation analyses, future researchers should address this gap in the psychotherapy literature.

As next step in future research, the effectiveness as well as relevant moderators and mediators of initial therapy contact should be examined in a meta-analysis. In this review, meta-analytic strategies could not be applied because only a small number of studies ($n = 13$) (e.g. Abbass et al., 2008; Armento et al., 2012) reported significance levels, and the vast majority of studies did not provide enough information to enable calculation of effect sizes. When researchers ensure that the intervention and treatment effect (or effect size) is reported consistently from one study to the next, meta-analysis can be used to numerically pool the results of the studies and arrive at a summary estimate to identify this effect of initial therapy contact. Subgroup analyses and meta regression can then be conducted to test if there are subsets of research that capture the summary effects. This next step will then help test hypotheses around the relative importance of the length, format or therapy model of the initial therapy contact in determining symptom change.

Further research might illuminate whether patients who substantially improve during the initial sessions of longer therapy would have done so even if treatments were planned to be only ultra-brief (3 h or less). Building on our findings, future researchers may clarify if initial therapy contacts with varying total treatment dose are equally effective (see dose-effect model; Howard et al., 1986), or if rate of change is related to total dose of therapy (see the good-enough level model of change; Baldwin, Berkeljon, Atkins, Olsen, & Nielsen, 2009).

5.3. Clinical implications

The reported risks of bias in the reviewed studies precludes drawing any strong conclusions about the effectiveness of each specific intervention. However, for clinicians it will be important to know that, in contrast to the review of single-session debriefing after trauma (Bisson, 2010; Rose et al., 2003), there were no indications of any significant harm or distress following the initial therapy contacts applied in general outpatient services. In other words, initial therapy contacts (as a stand-alone intervention or as part of longer treatment) may constitute a beneficial therapeutic intervention in itself. The self-reported patient outcomes suggest that there might be a number of initial therapy formats (single-session, 2-1, & initial session), which could potentially benefit patients in primary care treatment settings, more than no treatment or being on a waiting list.

Understanding the direct effect of these initial therapy contacts is thus of great significance to clinicians who, in this managed-care era, are under pressure to provide effective relief in the shortest time possible. Clinicians will want to know who gets better and what it is that leads to that improvement, to stratify selection or alter therapy content accordingly. Moreover, by identifying the most important factors associated with change in initial therapy contacts, we may aid the development of therapist training to maximize the beneficial impact of this critical stage in therapy.

Ultimately, effective initial treatment sessions might not only benefit patients, but might also aid service providers, when these early outcomes translate into lower non-attendance or drop-out rates in clinical services. Similarly, the very brief 2 + 1 therapies are likely to harness the effects of elapsed time within the service context and may thus be both cost effective and clinically effective. Service administrators may view single session therapy as a less risky and more long term cost-effective alternative to lengthy waitlists and may argue that one session of therapy may be all that many patients need (Boyhan, 1996; Talmon, 1990). Therefore, the application of these very brief interventions in clinical services also raises ethical concerns that decisions to limit the number of sessions available to patients may be based on budgetary constraints rather than clinical judgements (Campbell, 2012; Hurn, 2005).

Role of funding sources

No funding for this study was provided. No funding sources influenced the study design, collection, analysis or interpretation of the data, writing the manuscript, or the decision to submit the paper for publication.

Contributors

The first author designed the study, conducted the initial systematic search, and wrote the initial draft of the manuscript. The second author conducted a second literature search to check and update the previous review findings and updated the document formatting and referencing. Both authors contributed to and have approved the final manuscript.

Declaration of Competing Interest

None.

Acknowledgements

We are grateful to Dr. James Macdonald for his contributions to several earlier drafts of this manuscript and to Kathy Portier for contributing to the quality assessment ratings.

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

References

*Aafjes-van Doorn, K., Macdonald, J., Stein, M., Cooper, A. M., & Tucker, S. (2014). Experiential dynamic therapy: A preliminary investigation into the effectiveness and process of the extended initial session. *Journal of Clinical Psychology, 70*, 914–923. https://doi.org/10.1002/jclp.22094.

*Abbass, A. A., Joffres, M. R., & Ogrodniczuk, J. S. (2008). A naturalistic study of intensive short-term dynamic psychotherapy trial therapy. *Brief Treatment and Crisis*

Intervention, 8, 164–170. https://doi.org/10.1093/brief-treatment/mhn001.
Ajzen, I. (1988). Attitudes and personality traits. In T. Manstead (Ed.). *Attitudes, personality and behavior* (pp. 1–24). Maidenhead, UK: Open University Press.
*Armento, M. E. A., McNulty, J. K., & Hopko, D. R. (2012). Behavioral activation of religious behaviors (BARB): Randomized trial with depressed college students. *Psychology of Religion and Spirituality, 4*, 206–222. https://doi.org/10.1037/a0026405.
*Askevold, F. (1983). What are the helpful factors in psychotherapy for anorexia nervosa? *International Journal of Eating Disorders, 2*, 193–197. https://doi.org/10.1002/1098-108X(198322)2:4<193::AID-EAT2260020428>3.0.CO;2-Y.
Baldwin, S. A., Berkeljon, A., Atkins, D. C., Olsen, J. A., & Nielsen, S. L. (2009). Rates of change in naturalistic psychotherapy: Contrasting dose–effect and good-enough level models of change. *Journal of Consulting and Clinical Psychology, 77*, 203–211. https://doi.org/10.1037/a0015235.
*Barkham, M. (1989). Brief prescriptive therapy in two-plus-one sessions: Initial cases from the clinic. *Behavioral Psychotherapy, 17*, 161–175. https://doi.org/10.1017/S014134730001613X.
*Barkham, M., & Hobson, R. F. (1989). Exploratory therapy in two-plus-one sessions II-A single case study. *British Journal of Psychotherapy, 6*, 89–100. https://doi.org/10.1111/j.1752-0118.1989.tb01265.x.
Barkham, M., Margison, F., Leach, C., Lucock, M., Mellor-Clark, J., Evans, C., & McGrath, G. (2001). Service profiling and outcomes benchmarking using the CORE-OM: Toward practice-based evidence in the psychological therapies. *Journal of Consulting and Clinical Psychology, 69*, 184–196. https://doi.org/10.1037/0022-006X.69.2.184.
*Barkham, M., Moorey, J., & Davis, G. (1992). Cognitive-behavioral therapy in two-plus-one sessions: A pilot field trial. *Behavioral Psychotherapy, 20*, 147–154. https://doi.org/10.1017/S014134730001692X.
*Barkham, M., Rees, A., Stiles, W. B., Hardy, G. E., & Shapiro, D. A. (2002). Dose-effect relations for psychotherapy of mild depression: A quasi-experimental comparison of effects of 2, 8, and 16 sessions. *Psychotherapy Research, 12*, 463–474. https://doi.org/10.1093/ptr/12.4.463.
*Barkham, M., Shapiro, D. A., Hardy, G. E., & Rees, A. (1999). Psychotherapy in two-plus-one sessions: Outcomes of a randomized controlled trial of cognitive–behavioral and psychodynamic–interpersonal therapy for subsyndromal depression. *Journal of Consulting and Clinical Psychology, 67*, 201–211. https://doi.org/10.1037/0022-006X.67.2.201.
Battaglia, M. P., Shapiro, G., & Zell, E. R. (1996). Substantial response bias may remain when records are used in a telephone survey. *1996 Proceedings of the section on survey research methods* (pp. 452–455). Alexandria (VA): American Statistical Association. Retrieved from http://www.cdc.gov/nchs/data/nis/data_collection/battaglia1996.pdf.
Beck, A. T., Ward, C. H., Mendelson, M. L., Mock, J. E., & Erbaugh, J. K. (1961). An inventory for measuring depression. *Archives of General Psychiatry, 4*, 561–571. https://doi.org/10.1001/archpsyc.1961.01710120031004.
*Beckham, E. E. (1989). Improvement after evaluation in psychotherapy of depression: Evidence of a placebo effect? *Journal of Clinical Psychology, 45*(6), 945–950.
Bisson, J. (2010). Post-traumatic stress disorder. *BMJ Clinical Evidence Available online: http://www. clinicalevidence.com*.
Bloom, B. L. (2001). Focused single-session psychotherapy: A review of the clinical and research literature. *Brief Treatment and Crisis Intervention, 1*, 75–86. https://doi.org/10.1093/brief-treatment/1.1.75.
Boyhan, P. A. (1996). Clients' perceptions of single session consultations as an option to waiting for family therapy. *Australian and New Zealand Journal of Family Therapy, 17*, 85–96. https://doi.org/10.1002/j.1467-8438.1996.tb01078.x.
*Busch, A. M., Kanter, J. W., Landes, S. J., & Kohlenberg, R. J. (2006). Sudden gains and outcome: A broader temporal analysis of cognitive therapy for depression. *Behavior Therapy, 37*, 61–68. https://doi.org/10.1016/j.beth.2005.04.002.
Cameron, C. L. (2007). Single session and walk-in psychotherapy: A descriptive account of the literature. *Counselling and Psychotherapy Research, 7*, 245–249. https://doi.org/10.1080/14733140701728403.
Campbell, A. (2012). Single-session approaches to therapy: Time to review. *Australian and New Zealand Journal of Family Therapy, 33*(1), 15–26. https://doi.org/10.1017/aft.2012.3.
Constantino, M. J. (2012). Believing is seeing: An evolving research program on patients' psychotherapy expectations. *Psychotherapy Research, 22*, 127–138. https://doi.org/10.1080/10503307.2012.663512.
Constantino, M. J., Arnkoff, D. B., Glass, C. R., Ametrano, R. M., & Smith, J. A. Z. (2011). Expectations. *Journal of Clinical Psychology, 67*, 184–192. https://doi.org/10.1002/jclp.20754.
*Coppock, T. E., Owen, J. J., Zagarskas, E., & Schmidt, M. (2010). The relationship between therapist and patient hope with therapy outcomes. *Psychotherapy Research, 20*, 619–626. https://doi.org/10.1080/10503307.2010.497508.
*Danitz, S. B., & Orsillo, S. M. (2014). The mindful way through the semester: An investigation of the effectiveness of an acceptance-based behavioral therapy program on psychological wellness in first-year students. *Behavior Modification, 38*, 549–566. https://doi.org/10.1177/0145445513520218.
*Danitz, S. B., Suvak, M. K., & Orsillo, S. M. (2016). The mindful way through the semester: Evaluating the impact of integrating an acceptance-based behavioral program into a first-year experience course for undergraduates. *Behavior Therapy, 47*, 487–499. https://doi.org/10.1016/j.beth.2016.03.002.
Deeks, J. J., Dinnes, J., D'amico, R., Sowden, A. J., Sakarovitch, C., Song, F., & Altman, D. J. (2003). Evaluating non-randomized intervention studies. *Health Technology Assessment, 7*(27), 1–179. https://doi.org/10.3310/hta7270.
DeFife, J. A., & Hilsenroth, M. J. (2011). Starting off on the right foot: Common factor elements in early psychotherapy process. *Journal of Psychotherapy Integration, 21*, 172–191. https://doi.org/10.1037/a0023889.
*Dunn, E. C., Neighbors, C., & Larimer, M. E. (2006). Motivational enhancement therapy and self-help treatment for binge eaters. *Psychology of Addictive Behaviors, 20*, 44–52. https://doi.org/10.1037/0893-164X.20.1.44.
*Eustis, E. H., Williston, S. K., Morgan, L. P., Graham, J. R., Hayes-Skelton, S. A., & Roemer, L. (2017). Development, acceptability, and effectiveness of an acceptance-based behavioral stress/anxiety management workshop for university students. *Cognitive and Behavioral Practice, 24*, 174–186. https://doi.org/10.1016/j.cbpra.
Fennell, M. J. V., & Teasdale, J. D. (1987). Cognitive therapy for depression: Individual differences and the process of change. *Cognitive Therapy and Research, 11*, 253–271. https://doi.org/10.1007/BF01183269.
Frank, J. D. (1974). Psychotherapy: The restoration of morale. *American Journal of Psychiatry, 131*, 271–274. https://doi.org/10.1176/ajp.131.3.271.
*Freeman, A., & Jackson, J. (1996). Single session treatment of a borderline personality disorder. *Cognitive and Behavioral Practice, 3*, 183–208. https://doi.org/10.1016/S1077-7229(96)80037-X.
*Gangdev, P. S. (1998). Faith-assisted cognitive therapy of obsessive-compulsive disorder. *Australian and New Zealand Journal of Psychiatry, 32*, 575–578. https://doi.org/10.3109/00048679809068333.
*Gawrysiak, M., Nicholas, C., & Hopko, D. R. (2009). Behavioral activation for moderately depressed university students: Randomized controlled trial. *Journal of Counseling Psychology, 56*, 468–475. https://doi.org/10.1037/a0016383.
*Gellis, L. A., Arigo, D., & Elliott, J. C. (2013). Cognitive refocusing treatment for insomnia: A randomized controlled trial in university students. *Behavior Therapy, 44*, 100–110. https://doi.org/10.1016/j.beth.2012.07.004.
*Goerling, U., Jaeger, C., Walz, A., Stickel, A., Mangler, M., & Meer, E. V. (2014). The efficacy of short-term psycho-oncological interventions for women with gynaecological cancer: A randomized study. *Oncology, 87*, 114–124. https://doi.org/10.1159/000362818.
Greenberg, R. P., Constantino, M. J., & Bruce, N. (2006). Are patient expectations still relevant for psychotherapy process and outcome? *Clinical Psychology Review, 26*, 657–678. https://doi.org/10.1016/j.cpr.2005.03.002.
Haaga, D. A. F. (2000). Introduction to the special section on stepped care models in psychotherapy. *Journal of Consulting and Clinical Psychology, 68*, 547–548. https://doi.org/10.1037/0022-006X.68.4.547.
Haas, E., Hill, R. D., Lambert, M. J., & Morrell, B. (2002). Do early responders to psychotherapy maintain treatment gains? *Journal of Clinical Psychology, 58*, 1157–1172. https://doi.org/10.1002/jclp.10044.
Hardy, G. E., Barkham, M., Shapiro, D. A., Stiles, W. B., Rees, A., & Reynolds, S. (1995). Impact of cluster C personality disorders on outcomes of contrasting brief psychotherapies for depression. *Journal of Consulting and Clinical Psychology, 63*, 997–1004. https://doi.org/10.1037/0022-006X.63.6.997.
*Hersch, J. B., & Lathan, C. (1985). The mental health walk-in clinic: The University of Massachusetts experience. *Journal of American College Health, 34*, 15–17. https://doi.org/10.1080/07448481.1985.9939612.
Higgins, J. P., & Green, S. (2008). *Cochrane handbook for systematic reviews of interventions.* Chichester, UK: John Wiley & Sons.
Hilsenroth, M. J., & Cromer, T. D. (2007). Clinician interventions related to alliance during the initial interview and psychological assessment. *Psychotherapy: Theory, Research, Practice, Training, 44*, 205–218. https://doi.org/10.1037/0033-3204.44.2.205.
Howard, K. I., Kopta, S. M., Krause, M. S., & Orlinsky, D. E. (1986). The dose-effect relationship in psychotherapy. *American Psychologist, 41*, 159–164. https://doi.org/10.1037/0003-066X.41.2.159.
Howard, K. I., Lueger, R. J., Maling, M. S., & Martinovich, Z. (1993). A phase model of psychotherapy outcome: Causal mediation of change. *Journal of Consulting and Clinical Psychology, 61*, 678–685. https://doi.org/10.1037/0022-006X.61.4.678.
Hoyt, M. F., & Talmon, M. (1990). Single-session therapy in action: A case example. In M. Talmon (Ed.). *Single-session therapy: Maximizing the effect of the first (and often only) therapeutic encounter* (pp. 78–96). San Francisco, CA: Jossey-Bass.
Hurn, R. (2005). Single-session therapy: Planned success or unplanned failure? *Counselling Psychology Review British Psychological Society, 20*(4), 33–40.
*Hutchinson, R. L., Krippner, K. M., & Hutchinson, E. P. (1988). Effects of an intake interview on students' anxiety and depression. *Journal of College Student Psychotherapy, 3*, 59–71. https://doi.org/10.1300/J035v03n01_06.
Hymmen, P., Stalker, C. A., & Cait, C. A. (2013). The case for single-session therapy: Does the empirical evidence support the increased prevalence of this service delivery model? *Journal of Mental Health, 22*, 60–71. https://doi.org/10.3109/09638237.2012.670880.
*Johnson, N., Whitaker, L., & Porter, G. (1980). The development and efficacy of a university mental health service walk-in clinic. *Journal of the American College Health Association, 28*, 269–271.
*Kashdan, T. B., Adams, L., Read, J., & Hawk, L. J. (2012). Can a one-hour session of exposure treatment modulate startle response and reduce spider fears? *Psychiatry Research, 196*(1), 79–82. https://doi.org/10.1016/j.psychres.2011.12.002.
Kunik, M. E., Braun, U., Stanley, M. A., Wristers, K., Molinari, V., Stoebner, D., & Orengo, C. A. (2001). One session cognitive behavioural therapy for elderly patients with chronic obstructive pulmonary disease. *Psychological Medicine, 31*(4), 717–723. https://doi.org/10.1017/S0033291701003890.
Lambert, M. J., Hansen, N. B., & Finch, A. E. (2001). Patient-focused research: Using patient outcome data to enhance treatment effects. *Journal of Consulting and Clinical Psychology, 69*, 159–172. https://doi.org/10.1037/0022-006X.69.2.159.
Lambert, M. J., & Ogles, B. (2004). The efficacy and effectiveness of psycho-therapy. In M. J. Lambert (Ed.). *Bergin and Garfield's handbook of psychotherapy and behavior change* (pp. 139–193). New York, USA: John Wiley and Sons.
*Lamprecht, H., Laydon, C., McQuillan, C., Wiseman, S., Williams, L., Gash, A., & Reilly, J. (2007). Single-session solution-focused brief therapy and self-harm: a pilot study.

Journal of Psychiatric and Mental Health Nursing, 14, 601–602. https://doi.org/10.1111/j.1365-2850.2007.01105.x.

*Lessard, M. J., Marchand, A., Pelland, M., Belleville, G., Vadeboncoeur, A., Chauny, J. M., & Lavoie, K. L. (2012). Comparing two brief psychological interventions to usual care in panic disorder patients presenting to the emergency department with chest pain. *Behavioral and Cognitive Psychotherapy, 40*, 129–147. https://doi.org/10.1017/S1352465811000506.

Liberati, A., Altman, D. G., Tetzlaff, J., Mulrow, C., Gotzsche, P. C., Ioannidis, J., & Moher, D. (2009). The PRISMA statement for reporting systematic reviews and meta-analyses of studies that evaluate health care interventions: explanation and elaboration. *Annals of Internal Medicine, 151*(4), 1–18. https://doi.org/10.1016/j.jclinepi.2009.06.006.

Llewelyn, S., & Aafjes-van Doorn, K. (2017). *Clinical psychology: A very short introduction.* Oxford, UK: Oxford University Press.

Locke, E. A., & Latham, G. P. (2006). New directions in goal-setting theory. *Current Directions in Psychological Science, 15*, 265–268. https://doi.org/10.1111/j.1467-8721.2006.00449.x.

Luborsky, L., Rosenthal, R., Diguer, L., Andrusyna, T. P., Berman, J. S., Levitt, J. T., & Krause, E. D. (2002). The dodo bird verdict is alive and well - mostly. *Clinical Psychology: Science and Practice, 9*, 2–12. https://doi.org/10.1093/clipsy/9.1.2.

Lundahl, B., & Burke, B. L. (2009). The effectiveness and applicability of motivational interviewing: A practice-friendly review of four meta-analyses. *Journal of Clinical Psychology, 65*, 1232–1245. https://doi.org/10.1002/jclp.20638.

*McManus, F., Van Doorn, K., & Yiend, J. (2011). Examining the effects of thought records and behavioral experiments in instigating belief change. *Journal of Behavior Therapy and Experimental Psychiatry, 43*, 540–547. https://doi.org/10.1016/j.jbtep.2011.07.003.

Merbaum, M., & Butcher, J. N. (1982). Therapists' liking of their psychotherapy patients: Some issues related to severity of disorder and treatability. *Psychotherapy: Theory, Research & Practice, 19*, 69–76. https://doi.org/10.1037/h0088419.

Messer, S. B., & Wampold, B. E. (2002). Let's face facts: Common factors are more potent than specific therapy ingredients. *Clinical Psychology: Science and Practice, 9*, 21–25. https://doi.org/10.1093/clipsy.9.1.21.

Miller, J. K., & Slive, A. (2004). Breaking down the barriers to clinical service delivery: Walk-in family therapy. *Journal of Marital and Family Therapy, 30*(1), 95–103. https://doi.org/10.1111/j.1752-0606.2004.tb01225.x.

Nowlan, J. S., Wuthrich, V. M., Rapee, R. M., Kinsella, J. M., & Barker, G. (2016). A comparison of single-session positive reappraisal, cognitive restructuring and supportive counselling for older adults with type 2 diabetes. *Cognitive Therapy and Research, 40*(2), 216–229.

*Nuthall, A., & Townend, M. (2007). CBT-based early intervention to prevent panic disorder: A pilot study. *Behavioral and Cognitive Psychotherapy, 35*, 15–30. https://doi.org/10.1017/S1352465806003031.

Olfson, M., & Pincus, H. A. (1994). Outpatient psychotherapy in the United States: II. Patterns of utilization. *American Journal of Psychiatry, 151*, 1289–1294. https://doi.org/10.1176/ajp.151.9.1289.

Ollendick, T. H., Öst, L. G., Reuterskiöld, L., Costa, N., Cederlund, R., Sirbu, C., & Jarrett, M. A. (2009). One-session treatment of specific phobias in youth: a randomized clinical trial in the United States and Sweden. *Journal of Consulting and Clinical Psychology, 77*, 504–516. https://doi.org/10.1037/a0015158.

Peleikis, D. E., & Dahl, A. A. (2005). A systematic review of empirical studies of psychotherapy with women who were sexually abused as children. *Psychotherapy Research, 15*, 304–315. https://doi.org/10.1080/10503300500091835.

Poston, J. M., & Hanson, W. E. (2010). Meta-analysis of psychological assessment as a therapeutic intervention. *Psychological Assessment, 22*, 203–212. https://doi.org/10.1037/a0018679.

Rockwell, W. K., & Pinkerton, R. S. (1982). Single-session psychotherapy. *American Journal of Psychotherapy, 36*, 32–40. https://doi.org/10.1176/appi.psychotherapy.1982.36.1.32.

Rose, S., Bisson, J., & Wessely, S. (2003). A systematic review of single-session psychological interventions ('debriefing') following trauma. *Psychotherapy and Psychosomatics, 72*, 176–184. https://doi.org/10.1159/000070781.

Schleider, J. L., & Weisz, J. R. (2017). Little treatments, promising effects? Meta-analysis of single-session interventions for youth psychiatric problems. *Journal of the American Academy of Child & Adolescent Psychiatry, 56*(2), 107–115.

*Silverman, W. H., & Beech, R. P. (1984). Length of intervention and patient assessed outcome. *Journal of Clinical Psychology, 40*, 475–480. https://doi.org/10.1002/1097-4679(198403)40:2<475::AID-JCLP2270400215>3.0.CO;2-N.

Sommers-Flanagan, J. (2007). Single-session consultations for parents: A preliminary investigation. *The Family Journal, 15*(1), 24–29. https://doi.org/10.1177/1066480706294045.

*Stalker, C. A., Riemer, M., Cait, C. A., Horton, S., Booton, J., Josling, L., & Zaczek, M. (2015). A comparison of walk-in counselling and the wait list model for delivering counselling services. *Journal of Mental Health.* https://doi.org/10.3109/09638237.2015.1101417.

Stulz, N., & Lutz, W. (2007). Multidimensional patterns of change in outpatient psychotherapy: The phase model revisited. *Journal of Clinical Psychology, 63*, 817–833. https://doi.org/10.1002/jclp.20397.

Szegedi, A., Jansen, W. T., Van Willigenburg, A. P., van der Meulen, E., Stassen, H. H., & Thase, M. E. (2009). Early improvement in the first 2 weeks as a predictor of treatment outcome in patients with major depressive disorder: A meta-analysis including 6562 patients. *Journal of Clinical Psychiatry, 70*, 344–353. https://doi.org/10.4088/JCP.07m03780.

Talley, J. E. (1992). *The predictors of successful very brief psychotherapy: A study of differences by gender, age, and treatment variables.* Springfield, IL: Charles C Thomas.

Talmon, M. (1990). *Single-session therapy: Maximizing the effect of the first (and often only) therapeutic encounter.* San Francisco, CA: Jossey-Bass.

Thomas, B. H., Ciliska, D., Dobbins, M., & Micucci, S. (2004). A process for systematically reviewing the literature: Providing the research evidence for public health nursing interventions. *Worldviews on Evidence-Based Nursing, 1*(3), 176–184. https://doi.org/10.1111/j.1524-475X.2004.04006.x.

*Tolchard, B., Thomas, L., & Battersby, M. (2006). Single-session exposure therapy for problem gambling: A single-case experimental design. *Behaviour Change, 23*, 148–155.

*Turner, A., Hambridge, J., Baker, A., Bowman, J., & McElduff, P. (2013). Randomised controlled trial of group cognitive behaviour therapy versus brief intervention for depression in cardiac patients. *The Australian and New Zealand Journal of Psychiatry, 47*(3), 235–243. https://doi.org/10.1177/0004867412460592.

Zlomke, K., & Davis, T. E., III (2008). One-session treatment of specific phobias: A detailed description and review of treatment efficacy. *Behavior Therapy, 39*, 207–223. https://doi.org/10.1016/j.beth.200.

Katie Aafjes-van Doorn is Assistant Professor of Clinical Psychology at the Clinical Psychology Program of the Ferkauf Graduate School of Psychology. She received a MSc in Clinical Psychology from the Vrije Universiteit in Amsterdam, as well as a MSc in Psychological Research and a doctorate in Clinical Psychology from University of Oxford. Over the years, she has worked clinically in different settings within the National Health Service, in the UK and in outpatient clinics in the USA. Dr. Aafjes-van Doorn completed a one-year postdoctoral research fellowship at the Derner Institute for Psychological Services, Adelphi University. Her research focuses on the evidence-base of (psychodynamic) psychotherapy, as well as (non) verbal (psychodynamic) processes, including the coding and analyses of affect experiencing, defenses, reflective functioning, learning styles and other potential moderators and mediators of change. She has co-authored an introductory book on clinical psychology, chapters on process-outcome research and research in clinical psychology and has written about psychotherapy training, in-session processes and video-recording in psychotherapy.