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Interventions to Increase Attendance at Psychotherapy:

A Meta-Analysis of Randomised Controlled Trials

Abstract

Objective: Rates of non-attendance for psychotherapy hinder the effective delivery of evidence-based treatments. Although many strategies have been developed to increase attendance, the effectiveness of these strategies has not been quantified. The aim of the present study was to undertake a meta-analysis of rigorously controlled studies to quantify the effects of interventions to promote psychotherapy attendance.

Method: The inclusion criteria were that studies (1) concerned attendance at individual or group psychotherapy by adults, (2) used a randomised controlled trial design to test an attendance strategy, and (3) used an objective measure of attendance. Computerised literature searches and hand searching resulted in a total of 31 RCTs that involved 33 independent tests of strategies for reducing treatment refusal and premature termination (N = 4,422). Effect sizes from individual studies were meta-analysed and moderator analyses were conducted.

Results: Interventions had a small-to-medium effect on attendance across studies (d_+ = .38). Interventions to reduce treatment refusal and premature termination were similarly effective (d_+ = .37 and .39, respectively). Choice of appointment time or therapist, motivational interventions, preparation for psychotherapy, informational interventions, attendance reminders, and case management were the most effective strategies. Diagnosis also moderated effect sizes; samples with a single diagnosis benefited more from attendance interventions than samples that had a variety of diagnoses.

Conclusions: Interventions to increase attendance at adult psychotherapy are moderately effective. However, relatively few studies met the strict study inclusion criteria. Further methodologically sound and theoretically informed interventions geared at increasing attendance are required.

Interventions to Increase Attendance at Psychotherapy:
A Meta-Analysis of Randomised Controlled Trials

A substantial proportion of clinical time is wasted because of patient non-attendance at scheduled adult psychotherapy appointments (Pekarik, 1985). The financial costs of non-attendance are marked (Hicks & Hickman, 1995; Kleine, Stone, Hicks & Pritchard, 2003), with patients not receiving help (Joshi, Maisami & Coyle, 1986) and therapists losing confidence as a result (Sledge, Moras, Hartley & Levine, 1990). Service efficiency is impaired when non-attendance rates are high (Rusius, 1995). Garfield (1994) noted that some patients fail to attend at assessment and essentially reject treatment. Hampton-Robb, Qualls, and Compton (2003) estimated that such treatment refusal (TR) occurs for 40% of referrals, on average. Premature termination (PT) occurs when patients fail to complete agreed treatment contracts (i.e., they 'drop-out' of therapy). A meta-analysis of 123 studies reported a PT rate of 46.8% (Wierzbicki & Pekarik, 1993) across treatment modalities. High PT rates are troubling in light of evidence that PT is associated with poor clinical outcome (Barrett, Chua, Crits-Christoph, Gibbons & Thompson, 2008; Lambert, 3007). Clearly, successfully starting and finishing a course of psychotherapy is no certainty, with Walitzer et al. (1999) noting that TR and PT rates remain disturbingly high and unchanged over time, context and modality.

Knowledge of the patient factors associated with TR and PT remains piecemeal (Self, Oates, Pinnock-Hamilton & Leach, 2005; Johansen, Lumley & Cano, 2011). Reis and Brown (1999) concluded that only lower socioeconomic status (SES) and membership of an ethnic minority group were consistent predictors of PT. Self et al. (2005) investigated the impact of SES across different stages of patient contact, noting that lower SES was significantly associated with TR and PT during the first four treatment sessions. However, no differences in SES could be identified at the 'opt in' stage or PT after four or more sessions of psychotherapy. This suggests that different stages of the psychotherapy care pathway should be studied separately, as the reasons for patient disengagement may vary significantly according to phase (Barrett et al. 2008).

Frankel, Farrow & West (1989) argued that the strategies used to promote attendance are far more important than patient factors in determining rates of non-attendance.

Narrative Overview of Strategies to Promote Psychotherapy Attendance

Although correlational studies of the predictors of attendance provides valuable information about who should be targeted by interventions, an important concern is what strategy should interventions adopt to promote attendance – what methods should be used to ensure initial engagement and secure retention across the psychotherapy care pathway? A wide variety of strategies have been tested that seek to promote attendance at psychotherapy assessment and treatment (see Table 1). The TR strategies researched include preparation for psychotherapy, reminder letters/telephone calls, providing service/treatment/research information, flexible appointment booking, providing a choice of therapists, priming patients by asking them to imagine successful attendance, and the formation of if-then plans (implementation intentions; Gollwitzer & Sheeran, 2006). PT interventions include preparation, case management and providing feedback on patient progress, whilst some strategies have been applied to both TR and PT (e.g., reminder telephone contact and motivational interviewing).

In relation to TR, the largest proportion of studies involves an educational intervention that prepares patients for individual psychotherapy. Preparation for such psychotherapy typically involves education about assessment, ensuring positive and balanced expectations regarding the duration and aims of therapy, and 'role induction' which involves outlining the rights, expectations, and responsibilities of both patient and therapist in psychotherapy. Preparation information has encompassed information on the dose-effect relationship (Swift & Callahan, 2011), provision of service information (McFall, Malte, Fontana and Rosenheck, 2000), and treatment information (McFall, Malte, Fontana & Rosenheck, 2000). Preparation has been administered variously through didactic educational interviews/talks (Jacobs, Charles, Jacobs, Weinstein & Mann, 1972) and the use of different media including both video (France & Dugo, 1985; Stosney, 1994; Strassle, Borkardt, Handler & Nash, 2011; Wilson, 1985; Zwick &

Attkisson, 1985) and audio materials (Lambert & Lambert, 1984). Interventions to reduce TR by preparing patients for group psychotherapy have involved structured group exercises and specific training on group therapy processes (Piper, Debbane, Bienvenu & Garant, 1982; Piper, Debbane, Garant & Bienvenu, 1979). Although most approaches prepare patients for therapy in a general manner, there are examples of tailoring preparation efforts at particular patient groups. For instance, Stosney (1994) employed a video presentation that specifically targeted perpetrators of domestic violence. Whereas the majority of interventions have targeted patients, Jacobs et al. (1972) prepared therapists to work with specific patient groups, by increasing awareness of potential SES factors preventing effective alliance formation.

Other methods to reduce TR have focussed on appointment letters, patient choice, and getting patients to either plan to or imagine attending at assessment. Appointment reminder letters significantly reduce TR (Rusius, 1995), whereas pre-assessment questionnaires increase TR (Soutter & Garelick, 1999). Patient choice appears to reduce TR. When patients are allowed to choose a therapist whose style appears matched to their perceived needs, TR rates are reduced (Ersner-Herschfield, Abramowitz & Baren, 1979). Similarly, TR is lower when patients can choose the date and time of their appointment via a flexible appointment booking system (Kenwright & Marks, 2003). Although patients may intend to attend for psychotherapy assessment, this does not guarantee that they will actually attend. The formation of an 'if-then' plan (or implementation intention) reduces the gap between intentions and action (Gollwitzer, 1999; Gollwitzer & Sheeran, 2006). Sheeran, Aubrey, and Kellett (2007) developed an implementation intention induction designed to enable patients to regulate negative affects regarding psychotherapy assessment attendance, and found that the intervention group attended at a significantly higher rate than controls (75% vs. 63%). Two studies (Buckner et al., 2009; Sherman & Anderson, 1987) have applied the use of imagination to reduce TR. Both studies asked participants to visualise themselves walking into the therapy centre and talking to their

therapist. Although this strategy significantly reduced TR in the intervention group for Sherman and Anderson (1987), this effect was not replicated by Buckner et al. (2009).

A commonly employed TR and PT strategy in medical settings is telephone contact prior to appointments (see Macheira, Leon, Rowe, Stephenson & Haynes, 1992, for a meta-analysis). The use of telephone reminders prior to scheduled psychotherapy appointments significantly reduces TR (Kluger & Karras, 1983; Macdonald, Brown & Ellis, 2000), but has no effect on PT (Conduit, Byrne, Court & Stefanovic, 2004). Telephone appointment conformation by the treating therapist does not significantly reduce PT, when compared to matched clerical contact or no contact (Hershorn & Rivas, 1993).

'Motivational interviewing' (e.g., Miller & Rollnick, 2002) has been used widely in the substance abuse field to prepare people to change addictive behaviour. This technique has been used to increase attendance for psychotherapy through a set of 3 hour-long sessions prior to assessment to reduce TR (Westra & Dozois, 2006), and throughout treatment to reduce PT (Milton, Crino, Hunt & Prosser, 2002). Zanjani, Bush & Olson (2010) used brief motivational telephone sessions prior to assessment and reduced both TR and PT in a veteran population.

In relation to PT, case management has been used to ensure continued service engagement with patients with severe and enduring mental health problems. For instance, Miranda, Azocar, Organista, Dwyer and Areane (2003) used a mixture of telephone and one-to-one contact (approximating to 10 hours of contact) outside of psychotherapy treatment sessions, to support patients with regards to psychotherapy attendance. Warren and Rice (1972) also showed reduced PT by providing four 30-minute support sessions focused on making use of the therapy on offer. Another set of PT studies (e.g., Hawkins, Lambert, Vermeersch & Tuttle, 2004; Lambert *et al.* 2001) investigated the effect of providing therapists with feedback on patient outcomes during therapy, in order to highlight those patients failing to improve and therefore at risk of PT. Lambert et al. (2001) found that feedback increased the number of sessions completed for those patients who had been shown to be struggling to improve, but decreased the number of

sessions completed when feedback demonstrated improvement. Hawkins et al. (2004) found greater clinical improvement for the feedback group, but no average increase in sessions attended. Johansen et al. (2011), Latour & Cappeliez (1994) and Zwick & Attkisson (1985) all found no effect on PT rates for patients shown induction videos, whilst Lambert & Lambert (1984) found that audio-taped role induction reduced PT for an immigrant population. When written and verbal preparation methods have been compared, they have been shown to be equivalent in terms of PT (Garrison, 1978). Preparation may be helpful in terms of engagement and reducing TR, but its effect appears to wane over time in terms of reducing PT, and factors such as the therapeutic alliance and progress are presumably become more influential.

The Present Review

During the past 10 years, three qualitative reviews have evaluated the evidence that intervention strategies are effective at increasing attendance at psychotherapy (Barrett et al., 2008; Ogrodniczuk et al., 2005; Walitzer et al., 1999). Barrett et al. (2008) and Walitzer et al. (1999) included studies of both child/family and adult psychotherapy services, whereas Ogrodniczuk et al., (2005) focussed on adult psychotherapy. All three reviews discussed the pros and cons of diverse attendance strategies and agreed that interventions to reduce TR and PT did show promise. Each review also noted that definitive conclusions regarding the differential efficacy of interventions could not be reached due to the methodological concerns about many of the extant attendance studies.

Although qualitative reviews offer rich portraits of the attendance literature, they do not enable the quantitative assessment and comparison of the impact of different intervention strategies on attendance (Johansen et al., 2011). The present review therefore sought to address the issues of methodological shortcomings and quantitative assessment of effects, by conducting a meta-analysis solely on rigorously controlled intervention studies. Another weakness noted in previous reviews is that attendance is often measured via self-reports, which may be subject to self-presentational, social desirability or memory biases. Studies were therefore included in the

present review if, and only if, they used (1) a randomised controlled trial (RCT) design, and (2) included an objective measure of attendance. Meta-analysis of RCTs provides succinct information to services wishing to make rational decisions on implementing attendance strategies based on the methodologically sound evidence base (Higgins & Green, 2005).

As well as estimating the overall effect of interventions on attendance, moderator analyses were also undertaken to assess the impact of the type of intervention strategy (e.g., preparation vs. telephone contact vs. feedback), format of the intervention (group vs. individual format), sample characteristics (diagnosis and country of origin), and methodological features (active vs. passive control group, how attendance was measured, study quality) on effect sizes. In sum, the present meta-analysis provides the first quantitative review of rigorously designed studies of strategies to increase attendance at adult psychotherapy.

Method

Selection of Studies

The following methods were used to generate the sample of studies: (a) computerised searches of medical and social scientific data bases (Web of Science, PsycINFO, and MEDLINE) for articles written between January 1970 and September 2011 using the search terms pretherapy or psychotherapy or prepar* or prevent or reduce or role induction or case management or remind* AND dropout or premature termination or dropping out or unilateral termination or attend* or nonattendance or attrition, (b) all studies that cited the identified articles were checked, and (c) reference lists in each article were evaluated for inclusion.

The following inclusion criteria were used: (i) the study sample included adults (18 years or older) at the outset or during a course of psychotherapy (group or individual), (ii) the study involved random allocation of patients to either an attendance intervention group or a comparison group (who received either a control intervention or treatment as usual, TAU) and (iii) an objective measure of attendance was used (e.g., attendance chart review). Literature from the substance abuse field was excluded, due to key differences between substance abuse and

psychotherapy samples (Watkins, Paddock, Zhang & Wells, 2006). Attendance studies for psychiatric out-patient appointments were excluded, as this literature has recently been reviewed elsewhere (Lefforge, Donohue & Strada, 2007).

Figure 1 shows the flow of information through the phases of the present review (Moher, Liberati, Tetzlaff, & the PRISMA Group, 2009). We screened 3,249 articles and retrieved 62 full-text articles, of which 31 were excluded. Most were excluded because the articles did not meet the inclusion criteria (e.g., non-random assignment of participants, non-adult sample); two articles did not provide sufficient information to compute relevant effect sizes. In total, 33 tests of interventions to increase attendance were suitable for analysis from the 31 articles (articles included in the meta-analysis are preceded by an asterisk in the reference list). Table 1 presents the characteristics and effect sizes for each study.

Meta-Analysis Strategy

Attendance data from each study were converted to a common metric, namely, Cohen's d.[1] Computations were undertaken using STATA (Release 11, StataCorp, 2009). A random effects model (STATA command metan, with option random) was used to compute weighted average effect sizes because studies were likely to be "different from one another in ways too complex to capture by a few simple study characteristics" (Cooper, 1986, p. 526). The homogeneity Q statistic (Cochran, 1954) was used to evaluate variability in effect sizes from the primary studies. When Q is statistically significant the effect sizes are heterogeneous. Homogeneity was also assessed via the I^2 statistic which indicates the proportion of inconsistency in the individual studies that cannot be explained by chance.

Table 2 presents the moderator variables that were hypothesised to explain variance in attendance outcomes: (a) type of attendance (TR or PT), (b) attendance intervention strategy, (c) the sample diagnosis, (d) measurement of attendance, (e) whether the attendance intervention was

[1] Additional information concerning the computation of the effect size for each study can be obtained from the authors.

carried out in a group or with individuals, (f) whether studies involved an active or passive control group, (g) the country of origin for the study (to examine healthcare context effects), and (h) study quality. Study quality was ~~(~~assessed via the three rating scales developed by Chalmers et al.~~,~~ (1990)~~.~~, which assess (a) method of treatment assignment (lowest score given to studies where randomization was not mentioned, highest score given to studies where the treatment assignment process was truly randomized), (b) control of selection bias after treatment assignment (lowest score given to studies where results were analyzed only by treatment received, highest score given to studies where results were analyzed by original treatment assignment), and (c) blinding of participants and investigators (lowest score given to studies where double-blinding was possible but was not used~~given to studies which reported using double-blinding~~, highest score given to studies which reported using double-blinding ~~given to studies where double-blinding was possible but was not used~~).

Two procedures were used to assess moderation. First, we used the Q statistic to test whether the variation in the effect sizes obtained for the different levels of the moderator differed significantly from chance. Second, we used meta-regression (STATA command metareg) to examine moderation. For the meta-regressions, β and the associated p value, indicate whether the moderator variable has a significant association with the effect sizes from the primary studies.

Two coders with doctoral degrees in psychology (the second and last authors) independently coded the moderators in each study. Kappa coefficients indicated satisfactory inter-coder reliability (M = .89; range = .72 to 1.0). Disagreements were resolved through discussion.

Results

The effect sizes for the 33 attendance interventions ranged between -0.26 and 1.53, and had a standard deviation of 0.43 (see Figure 2 for a forest plot). The weighted mean effect size was d_+ =.38 with a 95% confidence interval from .26 to .49 (k = 33, N = 4,422). According to Cohen's (1992) power primer, d_+ = .20 is a "small" effect, d_+ = .50 is a "medium" effect, d_+ = .80

is a “large” effect. This suggests that interventions to promote attendance at adult psychotherapy have a small-to-medium effect on attendance behaviour.

Removing outlying values (d = 1.53 or d = -0.26) made little difference to the overall effect size (d_+ = .36 and d = .40, respectively). The funnel plot was ~~generally~~ somewhat asymmetrical, with an absence of small studies reporting negative or zero effect sizes (see Figure 3). Consistent with this interpretation, Egger's regression showed that there was ~~no~~ significant bias in the observed pattern of effect sizes (p = .~~81~~01). However, Duval and Tweedie's (2000) trim and fill method indicated that the overall effect size was still significant (d = .26, p < .001) when these missing effect sizes were imputed. Moreover, the fail-safe N (Rosenthal, 1979) indicated that 1,326 unpublished studies with zero effect sizes would need to exist in order to invalidate the conclusion that the interventions had no significant effect on attendance behavior (p < .05). ~~As t~~This value greatly exceeds Rosenthal's recommended tolerance value of 5n + 10 (where n is the number of effect sizes), corresponding to a fail-safe N of 175 in this instance. ~~,~~ T~~t~~hese analyses suggest that our data are resistant to publication bias.

~~The dataset was heterogeneous (i.e., t~~There was significant variation in the effect sizes derived from the primary studies ~~),~~ (Q = 91.3, p < .001), with a level of heterogeneity across studies~~;~~ (I^2 = 65.0%, 95% CI = 49% to 76%) which is considered to be moderate-to-high (Higgins, Thompson, Deeks, & Altman, 2003). Therefore, moderator analyses were ~~performed~~undertaken in order to determine the sources of this variability in effect size across studies ~~which encouraged the examination of moderators~~ (see Table 3). We first examined the type of attendance. Analyses of studies of TR (i.e., strategies designed to reduce non-attendance for assessment) and PT (i.e., strategies designed to reduce ‘drop-out’ from on-going treatment) showed that interventions were similarly effective in reducing TR and PT (d_+ = .37 and .39, respectively), Q = 0.11, p = 0.74, I^2 = 0.0%. Meta-regression confirmed that studies of PT had comparable effect sizes to those concerned with TR (β = 0.01, p = .95).

Next, we examined the impact of intervention strategy on effect sizes for attendance. There was significant heterogeneity in effectiveness across strategies ($Q = 56.14$, $p < 0.001$; $I^2 = 84.0\%$, 95% CI = 74% to 91%). Pairwise tests using the Q statistic indicated that providing choice of appointment times or therapist, motivational interviewing, preparation for psychotherapy, informational intervention, appointment reminders, and case management were similarly and highly effective. Providing choice, motivational interviewing, and preparation were each more effective than implementation intentions, imagination, therapist feedback, and use of a pre-assessment questionnaire (ps < .05). Informational interventions, reminders, and case management were significantly more effective than therapist feedback and use of a pre-assessment questionnaire. Use of a pre-assessment questionnaire proved less effective than each of the other strategies in pairwise comparisons.

Analyses of the impact of intervention strategy via meta-regression revealed a slightly different pattern of findings. In particular, 8 strategies (providing choice of appointment times or therapist, motivational interviewing, preparation for psychotherapy, informational intervention, appointment reminders, case management, implementation intentions, and imagination) were not significantly associated with the effect sizes from the primary studies ($|\beta s| < .30$, ps > 0.17). That is, these 8 individual strategies were not significantly more effective compared to all of the alternative intervention strategies combined. Therapist feedback ($\beta = -0.41$, $p = .05$) and use of pre-assessment questionnaires ($\beta = -0.66$, $p = .02$), on the other hand, were significantly less effective compared to the alternative intervention strategies. In sum, these findings suggest that there is little difference in effectiveness among the most successful interventions. While each of the six most effective strategies had significantly larger effect sizes as compared to the least effective strategies (the question answered by pairwise Q statistics, in which each intervention is contrasted with each other intervention in turn), no individual strategy stood out as being significantly more effective than the others when compared to the other interventions as a whole

(the question answered by meta-regression, in which one strategy is contrasted with all of the other strategies put together).

The third moderator concerned the psychological problem (i.e., diagnosis) for which participants were being treated with psychotherapy. Although diagnosis was not reported in the majority of studies (17/33, 52%), patients with anxiety, depression, impulse control disorders, or various diagnoses were apparent in other studies. Diagnosis had a significant impact on intervention effects ($Q = 25.37$, $p < 0.001$; $I^2 = 84.2\%$, 95% CI = 65% to 93%). Pairwise comparisons indicated that interventions involving participants with various diagnoses had significantly smaller effects on attendance (ps < .05). This finding was confirmed by meta-regression ($\beta = -0.33$, $p = .02$).

The next moderator concerned how PT was measured. Three measurement approaches were identified, namely, the number of sessions that participants attended, the proportion of participants that attended a set number of sessions, and attrition after the first session. Findings showed that the difference between measurement approaches was not significant ($Q = 4.45$, $p = 0.11$; $I^2 = 55.1\%$, 95% CI = 0% to 81%), and meta-regression confirmed that none of the measurement approaches were associated with significantly larger or smaller effect sizes (βs < 0.21, ps > 0.21). However, it is notable that the effect size for dropout after the first session ($d_+ = .19$, 95% CI = -0.27-0.65) was not statistically reliable (the confidence interval contains zero). Neither the format of the intervention (group vs. individual), nor whether the control group was active vs. passive, moderated effect sizes (Qs = 2.62 and 0.02, ns; βs = 0.24 and 0.04, ps = 0.23 and 0.81, respectively). Effects from samples with different countries of origin were homogeneous ($Q = 4.79$, ns; $I^2 = 37.4\%$, 95% CI = 0% to 78%) and none of the individual countries were associated with significantly larger or smaller intervention effect sizes (βs < 0.23, ps > 0.28).

Study quality was rated using the 0-3 scales developed by Chalmers et al. (1990). Studies were generally of good quality with respect to the method of treatment assignment and control of

selection bias after treatment. The modal rating for treatment assignment was 2 (57.6%) indicating that although random assignment was used, the randomization procedure needed to be described in greater detail or reassurance was needed that the investigators were blind to participant's condition. The modal rating for control of selection bias was 3 (84.8%) indicating that intention-to-treat analysis was used routinely. However, studies generally scored poorly on blinding of participants and investigators. The modal rating was 1 (66.7%), the value assigned "when blinding was impossible or when it was impossible to judge whether or not it had been attempted" (p. 1404). Meta-regression indicated that none of the three ratings of study quality was associated with the effect sizes for attendance interventions (β = .09, .08, and -.03, p = .36, .40, and .66, for treatment assignment, selection bias, and blinding procedures, respectively). Reasons why study quality did not influence effect sizes may be the lack of variability in ratings of study quality or the modest number of effect sizes that could be included in the review.

Discussion

Interventions to reduce TR and PT from adult psychotherapy are effective and have an effect size of small-to-medium magnitude (d_+ = .38) according to Cohen's (1992) guidelines. This effect size is typical of psychological, educational, and behavioral interventions; Lipsey and Wilson (1993) found that the modal effect size for interventions was in the range d_+ = .30 to d_+ = .39 across 302 meta-analyses. The practical significance of interventions of this magnitude can be illustrated using the binomial effect size display (BESD; Rosenthal & Rubin, 1982) and the number needed to treat (NNT; Kraemer & Kupfer, 2006) analyses. The BESD involves converting d to Pearson's r and then using the formulas (.50 + r/2) and (.50 - r/2) to compute the success rate for treatment and control groups, respectively. Thus, interventions that promote attendance for adult psychology where d_+ = .~~37~~ 38 equate to increasing the attendance rate at a first appointment from 41% in the control group to 59% in the intervention group. NNT analysis on ~~an~~the overall effect size of .38 illustrates that services would need to ~~perform~~undertake an indicated attendance intervention on 4.72 referrals in order to have one more patient attend for

psychotherapy. Augmenting attendance by 18% at such a cost-benefit ratio is likely to be considered an efficient and efficacious method of improving access to psychotherapy services ~~a meaningful improvement~~ by ~~psychotherapy service~~ commissioners and managers.

The present meta-analysis was based on 33 independent tests of attendance intervention strategies involving a total of 4,422 adult patients, and offers a different conclusion to the inferences drawn from previous qualitative reviews. In particular, the results are not consistent with previous conclusions that attendance strategies are generally ineffective (Piper & Perrault, 1989), that non-attendance is an intractable problem (Barrett et al. 2008), or that it is impossible to ascertain which is the most effective strategy for reducing non-attendance (Ogrodniczuk et al. 2005). The strength of the present meta-analysis derives from selecting only those intervention studies that used both random allocation and an objective measure of attendance (Higgins & Green, 2005). The implication is that when interventions are tested rigorously, attendance strategies are found to be moderately effective in promoting rates of both initial and sustained attendance.

The intervention strategy that had the largest effect in increasing attendance was providing patient choice with respect to appointment time or choice of therapist. This finding needs to be considered in the light of the small number of relevant studies, but is consistent with a long tradition of research on self-determination theory, which has shown that circumstances that promote autonomy lead to improved motivation and well-being in a wide variety of domains (e.g., health, occupational, educational; review by Deci & Ryan, 2000). Offering choice may foster patients' sense of volition, whereas purely service-determined appointments (i.e., a pre-set location, time, date, and therapist) may be experienced as limiting or controlling, and undermine patients' intrinsic motivation to attend. Interventions in general practice and outpatient clinics (Sharp & Hamilton, 2001) and sexually transmitted disease clinics (Kellock, Bingwa & Carlin, 2007) have also shown that offering patients a choice of appointments can substantially improve attendance rates. Whereas offering choice over times and dates for psychotherapy appears

straightforward, offering choice of modality or therapeutic style is more complicated. Such issues of patient preference need to be based on patients making an informed choice, based on sound and equitably presented evidence. Patients cannot effectively choose a modality or a therapist without such information, and would be forced to rely on guesswork. A patient cannot choose a cognitive-behavioural therapy over an interpersonal-dynamic therapy (or vice-versa) without comparative information that is scrupulously vetted for sources of bias in content and presentation. Patient preference trials capture the importance of patient choice, by randomising all those patients who cannot decide which intervention they would prefer and matching patient preference to intervention for all remaining patients (Howard & Thornicroft, 2006). This process ensures that patients with strong preferences do not refuse to enter research trials (Brewin & Bradley, 1989), which has the potential to skew recruitment and results.

Although offering patient choice was the single most effective strategy, motivational interviewing, preparation for psychotherapy, informational interventions, appointment reminders, and case management were equally effective strategies. Whilst such intervention strategies were equivalent in terms of efficacy, a vast difference in terms of organisational commitment and cost of such interventions is apparent. For example, case management interventions took ten (Miranda, et al, 2003) and two hours (Warren & Rice 1972) of staff time, respectively, to implement. Case management interventions (and preparation for psychotherapy, and motivational interviewing) can therefore can be criticised for being a complex intervention, whose sole purpose is to enable another complex intervention (i.e., psychotherapy) to take place. This criticism is particularly stark when comparing the effect sizes for case management, preparation, and motivational interviewing with those of appointment reminders, which are relatively simple, do not require therapists' time, and are cost effective (Downer, Meara, Da Costa, & Sethuraman, 2006). Relatively new technologies (such as texting and e-mail) potentially represent low-cost ways of increasing attendance (Pilkington, Preston & Healy, 2011), assuming patients agree to be

contacted in this manner (Donaldson & Tayar, 2009). Similarly, podcasts represent a relatively inexpensive delivery format that could be utilised for future patient preparation trials.

Barrett et al. (2008) noted that projected financial costs of implementing and evaluating attendance interventions should to be balanced against the ongoing financial burden of TR and PT, and Bech (2005) criticised the attendance evidence base for neglecting the evaluation of the health economics of attendance interventions. It is also the case that reducing TR and PT may place additional pressure on service efficiency in terms of keeping wait-list times to a minimum, when larger numbers of patients engage with psychotherapy services and fewer patients drop out. Cost effectiveness and clinical efficacy therefore need to have equal standing in the design of future attendance trials.

The present meta-analysis indicates that the majority of attendance research has focused on reducing TR and that fewer studies have evaluated interventions for PT. However, effect sizes were equivalent for interventions to reduce TR and PT. As previously noted, efforts to reduce TR assume little or no previous contact with the patient, whereas efforts to reduce PT are based in the context of on-going therapeutic relationship, in which dissatisfaction with that relationship is likely to be the key driver for 'drop-out.' Future trials could therefore focus on how best to train therapists in recognising ruptures in the therapeutic relationship and engaging in repair sequences (Safran, Muran, Eubanks-Carter, 2011) to facilitate reduced PT.

Findings indicated that diagnosis had a significant impact on intervention effects such that interventions involving patients with various diagnoses had significantly smaller effects on attendance rates compared to interventions involving participants with specific and single diagnoses or when diagnosis was not reported. However, a weakness of the present review was that diagnosis varied greatly both across and within studies, and in most studies (17/33), diagnosis was either unavailable or not reported. A further 7 studies used samples that had a variety of diagnoses, and it was usually impossible to disaggregate the proportion of the sample with different disorders. The fact that diagnosis was a significant moderator of intervention

effectiveness highlights two issues: (1) where patients present with co-morbid psychological problems (i.e., various diagnoses), this is likely to indicate a level of complexity which attendance interventions fail to match, and (2) future trials of attendance strategies need to reliably record the patient groups on which interventions are being tested (Ogrodniczuk et al., 2005). Specific diagnoses permit inferences about well evidenced deficits and problems (i.e., inertia/rumination in depression and avoidance/escape in anxiety), and prompt the development and testing of theoretically driven interventions that target the disorder specific mechanisms creating TR and PT in reliably identified patient groups. It is also of note that the manner in which diagnoses were achieved was not described in sufficient detail in many of the studies. It is therefore highly likely that diagnoses were made by informal clinical opinion, rather than the use of standardized diagnostic interview schedules. Informal clinical assessment is the default 'diagnostic method' in the routine practice settings (Marriott & Kellett, 2009), which is the context in which virtually all attendance trials to date have been conducted.

Of interest is the finding that the nature of the control group did not influence intervention effects. Studies that employed an active control group had a similar overall effect size to those that compared intervention groups with TAU. Studies involving an 'active' control group could be seen as providing a more stringent test of attendance intervention, as it counterbalances the potential effects of extra time spent with participants in the experimental group, rather than no contact at all. De Bruin, Viechtbauer, Hospers, Schaalma and Kok (2009) noted that any wide variations in TAU provided to control groups may have considerable influence on effect sizes, and meta-analyses should control for variability in TAU, by coding the clinical realities of TAU. The description of the content of the control conditions in the trials used in the current review were not sufficient to enable coding of the relevant content, and we acknowledge that this is a weakness of the current study.

Several potential moderator variables failed to explain variation in effects sizes including the nature of the attendance measure, the study's country of origin, the format of the intervention

(group versus individual), and study quality. Although the attendance measure was not a significant moderator, the small and unreliable effect for attrition after the first session was notable; this finding suggests that this measure should not routinely be used to assess attendance in future studies. Although the difference between group versus individual intervention formats was not conventionally reliable ($p = .23$), there were only four studies using the group format. The consideration that group-format interventions had an effect size of $d_+ = .59$ (as compared to $d_+ = .37$ for interventions with individuals) suggests that further tests of this format are desirable.

Limitations

The main limitation of this meta-analysis is the small number of studies that met the inclusion criteria. Although there were 14 tests of the effects of preparation on attendance, the number of tests of the other intervention strategies was always $k \leq 3$, and for 6 out of the 10 intervention strategies there were two tests or fewer. This consideration suggests caution in interpreting the effect sizes for different intervention strategies. More important, this consideration clearly demonstrates the need for more trials of interventions to increase attendance at adult psychotherapy and routine use of objective measures of attendance. It is important not only that further rigorous studies with large samples are conducted, but also that these studies get published even if non-significant or small effects are observed. Only a greater number of studies involving larger samples will afford more definitive conclusions from future meta-analyses about the effectiveness of different attendance strategies.

Conclusion

Bech (2005) recommended that future attendance trials focus on interventions that both fit easily into the everyday running of existing services and require limited use of resources. The present review suggests that providing a choice of appointment times and using reminders are effective intervention strategies that meet these criteria. The use of implementation intentions to reduce TR shows promise, as this strategy only requires a theoretically informed and short questionnaire to be posted to participants prior to psychotherapy appointments (Sheeran et al.,

2007). Docherty (1992) argued that TR and PT rates should be the primary outcome measure for evaluating the effectiveness of psychotherapy services, as without attendance all other patient outcomes are unlikely. This review indicates that attendance is a more tractable problem than previous reviews have suggested. In particular, the present meta-analysis of RCTs shows that it is possible to increase attendance across the adult psychotherapy care pathway. Our findings suggest that future studies should (a) undertake tests in reliably identified patient groups, (b) compare attendance strategies with active control conditions or alternative strategies, (c) pay careful attention to features of study quality (Chalmers et al., 1990), and (d) integrate cost effectiveness analyses in the evaluation of interventions to reduce PT and TR.

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Table 1.

Sample Characteristics and Effect Sizes for Studies Included in the Review

Authors	Sample Characteristics		N_E	N_C	Intervention	Effect size (d)	95% CI
	Mean age	% female/male					
Buckner et al. (2009)	28.05	63/37	80	92	Imagining attending at least 4 sessions	-.05	[-.35, .25]
Ersner-Hershfield et al. (1979)	NR	NR	24	21	Choosing a therapist	.54	[-.06, 1.14]
France & Dugo (1985)	28.3-31.6	60/40	20	20	Preparation	.67	[.03 1.31]
Garrison (1978)	29	52/48	18	9	Preparation	.75	[-.07, 1.57]
Hawkins et al. (2004)	30.8	68/32	70	64	Therapist feedback on patient progress	-.06	[-.40, .28]
Hershorn & Rivias (1993)	38.24	NR	66	33	Telephone reminder	.16	[-.26, .58]
Jacobs et al., (1972)	58% 18-39	72/28	30	30	Preparation	.68	[.16, 1.20]
Johansen et al. (2011)	25.87	77/33	70	35	Preparation	-.04	[-.45, .37]
Kenwright & Marks (2003)[a]	30	46/54	27	30	Fixed versus partial booking system	.88	[.34, 1.42]
Kenwright & Marks (2003)[b]	35	58/42	39	41	Fixed versus partial	.54	[.09, .99]

					booking system		
Kluger & Karras (1983)	32	50/50	66	75	Orientation statement	.41	[.08, .74]
Lambert & Lambert (1984)	Mdn = 28-32	53/47	15	15	Preparation	1.53	[.72, 2.34]
Lambert et al. (2001)	22.2 yrs	70/30	307	302	Therapist feedback on patient progress	.04	[-.12, .20]
Latour & Cappeliez (1994)	Mdn = 69	83/17	14	15	Preparation	.62	[-.13, 1.37]
MacDonald, Brown, & Ellis (2000)	34.48	69/31	190	496	Telephone reminder	.47	[.30, .64]
McFall et al. (2000)	51	NR	189	155	Outreach brochure and telephone call	.43	[.22, .64]
Milton et al. (2002)	37.6	28/72	20	20	Motivational intervention	.63	[.00, 1.26]
Miranda et al. (2003)[a]	49.10	81/19	35	42	Case management	.50	[.04, .96]
Miranda et al. (2003)[b]	49.30	59/41	61	61	Case management	.35	[-.01,.71]
Piper et al. (1982)	34.6	54/46	45	24	Preparation	.67	[.16, 1.18]
Piper et al. (1979)	33.8	55/45	22	16	Preparation	.63	[-.03, 1.29]
Rusius (1995)	NR	NR	67	77	Postal reminder	.37	[.04, .70]
Sheeran, Aubrey & Kellett (2007)	35.59	67/33	199	191	Implementation intention	.26	[.06, .46]
Sherman & Anderson (1987)	NR	NR	22	21	Imagining attending at	.56	[-.05, 1.17]

					least 4 sessions		
Soutter & Garelick (1999)	NR	NR	102	138	Pre-assessment questionnaire	-.26	[-.52, .00]
Stosney (1994)	33.75	0/100	54	52	Preparation	.52	[.13, .91]
Strassle et al. (2011)	30.46	61/39	44	40	Preparation	-.04	[-.47, .39]
Swift & Callaghan (2011)	26.68	62/38	29	31	Preparation	.80	[.27, 1.33]
Warren & Rice (1972)	28.90	44/56	19	36	Preparation	.56	[-.01, 1.13]
Westra & Dozois (2006)	38	70/30	25	30	Motivational intervention	.46	[-.08, 1.00]
Wilson (1985)	26.3	64/36	33	33	Preparation	.70	[.20, 1.20]
Zanjani, Bush & Oslin (2010)	52.8	4/96	57	56	Motivational intervention	.67	[.29, 1.05]
Zwick & Attkisson (1985)	29	60/40	32	30	Preparation	-.22	[-.72, .28]

Note. N_E = number of participants in the experimental group, N_C = number of participants in the control group, NR = not reported

[a]Not including a stamped addressed envelope, [b]Including a stamped addressed envelope, [c]Spanish as first language, [d]English as first language.

Study	Diagnosis	Method for calculating attendance	Format of intervention	Type of control group	Study quality			Sample country of origin
					Assign	Select	Blind	
Buckner et al. (2009)	Mixed diagnoses	Number of sessions	Individual	Active	2	2	3	USA
Ersner-Hershfield et al. (1979)	Not reported	First session	Individual	Passive	1	3	1	USA
France & Dugo (1985)	Mixed diagnoses	Number of sessions	Individual	Passive	1	3	1	USA
Garrison (1978)	Mixed diagnoses	Number of sessions	Individual	Active	1	2	1	USA
Hawkins et al. (2004)	Not reported	Number of sessions	Individual	Passive	1	3	1	USA
Hershorn & Rivias (1993)	Mixed diagnoses	First session	Individual	Passive	2	3	1	USA
Jacobs et al. (1972)	Not reported	Proportion attending a set number of sessions	Individual	Passive	2	3	1	USA
Johansen et al. (2011)	Mixed diagnoses	Attrition after the first session	Individual	Active	2	3	2	USA
Kenwright & Marks (2003)a	Mixed diagnoses	First session	Individual	Passive	3	3	3	UK
Kenwright & Marks (2003)b	Mixed diagnoses	First session	Individual	Passive	3	3	3	UK
Kluger & Karras (1983)	Not reported	First session	Individual	Passive	2	3	1	USA
Lambert & Lambert (1984)	Not reported	Number of sessions	Individual	Active	2	3	1	USA
Lambert et al. (2001)	Mixed diagnoses	Number of sessions	Individual	Passive	2	1	3	USA

Latour & Cappeliez (1994)	Depression	Number of sessions	Group	Active	2	2	2	Canada
MacDonald et al. (2000)	Not reported	First session	Individual	Passive	2	1	1	New Zealand
McFall et al. (2000)	Anxiety	First session	Individual	Passive	2	3	1	USA
Milton et al. (2002)	Impulse control disorder	Proportion attending a set number of sessions	Individual	Passive	2	3	1	Australia
Miranda et al. (2003)[c]	Depression	Proportion attending a set number of sessions	Individual	Passive	2	3	1	USA
Miranda et al. (2003) [d]	Depression	Proportion attending a set number of sessions	Individual	Passive	2	3	1	USA
Piper et al. (1982)	Anxiety	Number of sessions	Group	Passive	2	3	1	Canada
Piper et al. (1979)	Not reported	Number of sessions	Group	Passive	2	3	1	Canada
Rusius (1995)	Not reported	First session	Individual	Passive	1	3	0	UK
Sheeran et al. (2007)	Not reported	First session	Individual	Active	3	3	3	UK
Sherman & Anderson (1987)	Not reported	Proportion attending a set number of sessions	Individual	Active	2	3	2	USA
Soutter & Garelick (1999)	Not reported	First session	Individual	Passive	1	3	1	UK
Stosney (1994)	Impulse control disorder	Proportion attending a set number of sessions	Group	Passive	1	3	1	USA

Strassle et al. (2011)	Mixed diagnoses	Attrition after first session	Individual	Passive	1	3	1	USA
Swift & Callahan (2011)	Not reported	Number of sessions	Individual	Passive	2	3	1	USA
Warren & Rice (1972)	Not reported	Proportion attending a set number of sessions	Individual	Passive	1	3	1	USA
Westra & Dozois (2006)	Anxiety	Proportion attending a set number of sessions	Individual	Passive	1	3	1	Canada
Wilson (1985)	Not reported	Attrition after first session	Individual	Passive	1	3	1	USA
Zanjani, Bush & Oslin (2010)	Depression	First session (d = .82) Number of sessions (d = .51)	Individual	Passive	2	3	2	USA
Zwick & Attkisson (1985)	Not reported	Number of sessions	Individual	Passive	2	3	0	USA

Note. Study quality was assessed using Chambers et al.'s (1990) coding scheme. Assign = coding for method of treatment assignment (where 0 indicates that "randomization was not mentioned explicitly" and 3 indicates that "the treatment assignment process was deemed to have been truly randomized"); Select = coding for control of selection bias after treatment assignment (where 0 indicates that "results were analyzed only by treatment received" and 3 indicates that "results were analyzed … by original treatment assignment"; Blind = coding for blinding of participants and investigators (where 0 indicates that "study could have been conducted as double-blinded, but had not been" and 3 indicates that "study was reported to have been double-blinded" (p. 1404).

[a]Not including a stamped addressed envelope, [b]Including a stamped addressed envelope, [c]Spanish as first language, [d]English as first language.

Moderator	N	k	95% CI	d	Q	I^2 (95% CI)
Type of Attendance					0.11	0.0%
Treatment refusal	2339	11	0.20-0.54	0.37	33.29***	70.0% (44-84)
Premature termination	2083	22	0.23-0.55	0.39	55.54***	62.2% (40-76)
Intervention strategy					56.14***	84.0% (72-91)
Choice of therapist or appointment	182	3	0.34-0.94	0.64	1.05	0.0% (0-90)
Motivational interviewing	208	3	0.33-0.88	0.61	0.40	0.0% (0-90)
Preparation	831	14	0.28-0.72	0.50	30.07**	576.8% (21-76)
Informational intervention	485	2	0.24-0.61	0.42	0.01	0.0%
Telephone/postal reminder	929	3	0.28-0.56	0.42	1.90	0.0% (0-90)
Case management	199	2	0.13-0.69	0.41	0.26	0.0%
Implementation Intention	390	1	0.06-0.46	0.26	-	-
Imagination	215	2	-0.39-0.78	0.20	3.10	687.7%
Feedback	743	2	-0.12-0.16	0.02	0.27	0.0%
Pre-assessment questionnaire	240	1	-0.52-0.00	-0.26	-	-
Diagnosis					25.37***	84.2% (65-93)
Anxiety	468	3	0.26-0.65	0.45	0.46	0.0% (0-90)
Depression	341	4	0.29-0.73	0.51	1.54	0.0% (0-85)
Impulse control disorder	146	2	0.22-0.88	0.55	0.08	0.0%
Various diagnoses	1201	7	-0.09-0.32	0.11	13.74*	56.3% (0-81)
Not reported	2266	17	0.26-0.60	0.43	48.77***	67.2% (46-80)
Measurement of premature termination					4.45	55.1% (0-87)
Number of sessions attended	1270	11	0.13-0.64	0.38	35.97***	72.2% (49-85)
Attendance/non-attendance at set number of sessions	558	8	0.34-0.67	0.51	1.41	0.0% (0-68)
Dropout after first session	255	3	-0.27-0.64	0.19	6.30*	68.3% (0-91)
Group v individual intervention					2.62	6261.9%
Individual	4180	29	0.23-0.48	0.37	86.25***	687.5% (52-78)
Group	242	4	0.32-0.85	0.59	0.21	0%.0% (0-85)
Active v passive control group					0.02	0.0%
Passive	3626	26	0.26-0.51	0.39	71.73***	65.1% (47-77)
Active	796	7	0.07-0.68	0.38	18.64**	67.8% (29-85)
Sample country of origin					4.79	37.4%
U.S.A	2594	22	0.22-0.51	0.36	57.99***	643.8% (43-77)
U.K.	911	5	-0.03-0.65	0.31	21.90***	821.7% (58-92)
Canada	191	4	0.29-0.88	0.59	0.31	0.0% (0-85)
Australia/New Zealand	726	2	0.32-0.64	0.48	0.23	0.0%

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Figure Captions

Figure 1

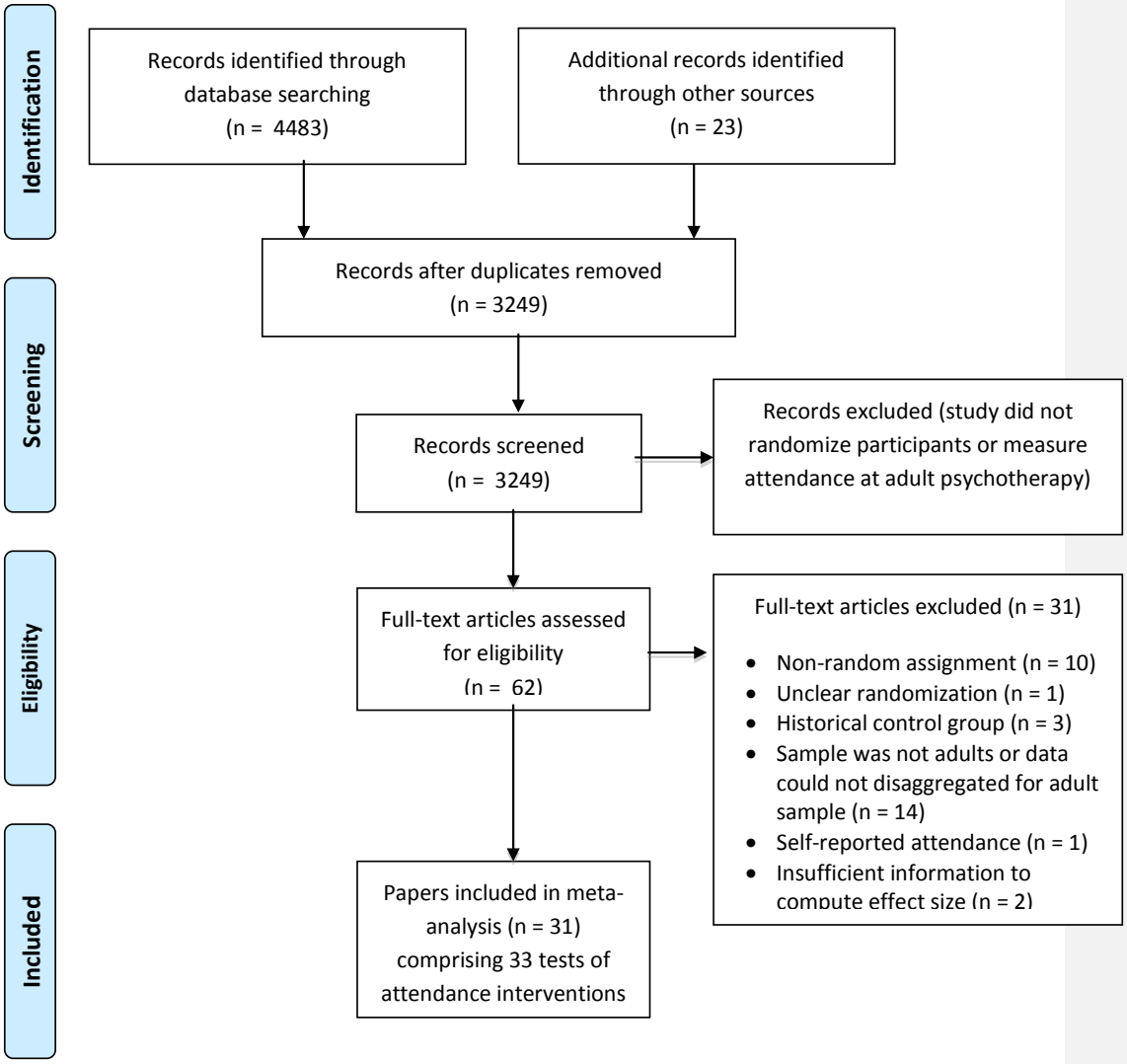
Flow of Information through the Phases of the Review

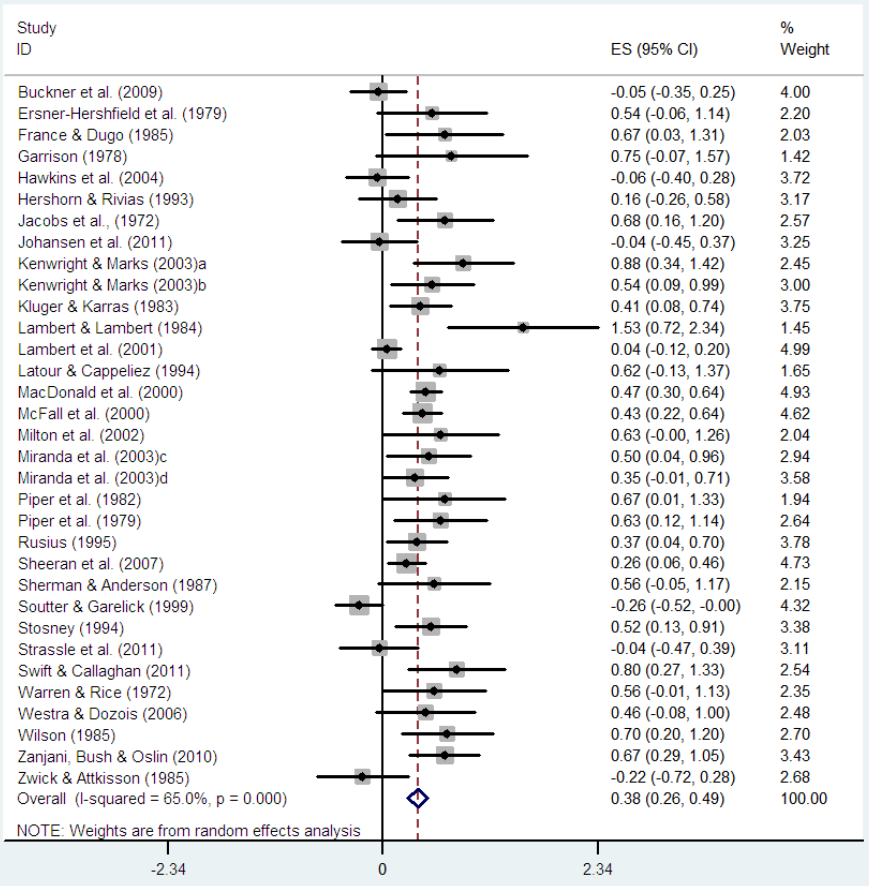
Figure 2

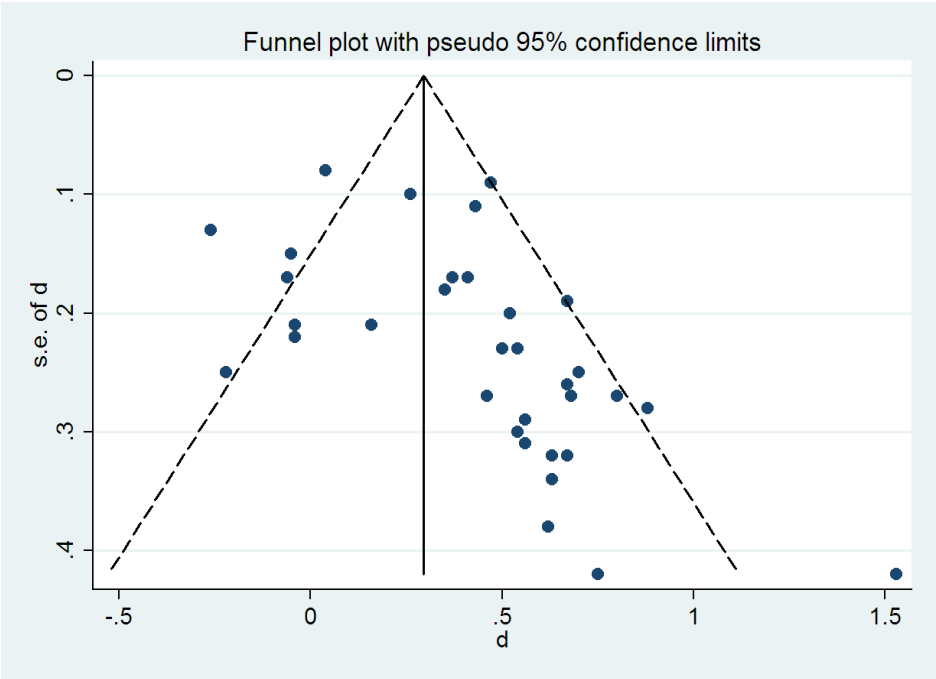
Forest Plot of Effect Sizes (d) for Attendance Interventions

Figure 3

Funnel Plot of Effect Sizes (d) for Attendance Interventions


Identification
Screening
Eligibility
Included
Records identified through database searching (n = 4483)
Additional records identified through other sources (n = 23)
Records after duplicates removed (n = 3249)
Records screened (n = 3249)
Records excluded (study did not randomize participants or measure attendance at adult psychotherapy)
Full-text articles assessed for eligibility (n = 62)
Full-text articles excluded (n = 31)
Non-random assignment (n = 10)
Unclear randomization (n = 1)
Historical control group (n = 3)
Sample was not adults or data could not disaggregated for adult sample (n = 14)
Self-reported attendance (n = 1)
Insufficient information to compute effect size (n = 2)
Papers included in meta-analysis (n = 31) comprising 33 tests of attendance interventions


Study ID
ES (95% CI)
% Weight
Buckner et al. (2009) -0.05 (-0.35, 0.25) 4.00
Ersner-Hershfield et al. (1979) 0.54 (-0.06, 1.14) 2.20
France & Dugo (1985) 0.67 (0.03, 1.31) 2.03
Garrison (1978) 0.75 (-0.07, 1.57) 1.42
Hawkins et al. (2004) -0.06 (-0.40, 0.28) 3.72
Hershorn & Rivias (1993) 0.16 (-0.26, 0.58) 3.17
Jacobs et al., (1972) 0.68 (0.16, 1.20) 2.57
Johansen et al. (2011) -0.04 (-0.45, 0.37) 3.25
Kenwright & Marks (2003)a 0.88 (0.34, 1.42) 2.45
Kenwright & Marks (2003)b 0.54 (0.09, 0.99) 3.00
Kluger & Karras (1983) 0.41 (0.08, 0.74) 3.75
Lambert & Lambert (1984) 1.53 (0.72, 2.34) 1.45
Lambert et al. (2001) 0.04 (-0.12, 0.20) 4.99
Latour & Cappeliez (1994) 0.62 (-0.13, 1.37) 1.65
MacDonald et al. (2000) 0.47 (0.30, 0.64) 4.93
McFall et al. (2000) 0.43 (0.22, 0.64) 4.62
Milton et al. (2002) 0.63 (-0.00, 1.26) 2.04
Miranda et al. (2003)c 0.50 (0.04, 0.96) 2.94
Miranda et al. (2003)d 0.35 (-0.01, 0.71) 3.58
Piper et al. (1982) 0.67 (0.01, 1.33) 1.94
Piper et al. (1979) 0.63 (0.12, 1.14) 2.64
Rusius (1995) 0.37 (0.04, 0.70) 3.78
Sheeran et al. (2007) 0.26 (0.06, 0.46) 4.73
Sherman & Anderson (1987) 0.56 (-0.05, 1.17) 2.15
Soutter & Garelick (1999) -0.26 (-0.52, -0.00) 4.32
Stosney (1994) 0.52 (0.13, 0.91) 3.38
Strassle et al. (2011) -0.04 (-0.47, 0.39) 3.11
Swift & Callaghan (2011) 0.80 (0.27, 1.33) 2.54
Warren & Rice (1972) 0.56 (-0.01, 1.13) 2.35
Westra & Dozois (2006) 0.46 (-0.08, 1.00) 2.48
Wilson (1985) 0.70 (0.20, 1.20) 2.70
Zanjani, Bush & Oslin (2010) 0.67 (0.29, 1.05) 3.43
Zwick & Attkisson (1985) -0.22 (-0.72, 0.28) 2.68
Overall (I-squared = 65.0%, p = 0.000) 0.38 (0.26, 0.49) 100.00
NOTE: Weights are from random effects analysis
-2.34
0
2.34


Funnel plot with pseudo 95% confidence limits
s.e. of d
0
.1
.2
.3
.4
-.5
0
.5
1
1.5
d

PRISMA Checklist

Section/topic	#	Checklist item	Reported on page #
TITLE			
Title	1	Identify the report as a systematic review, meta-analysis, or both.	1
ABSTRACT			
Structured summary	2	Provide a structured summary including, as applicable: background; objectives; data sources; study eligibility criteria, participants, and interventions; study appraisal and synthesis methods; results; limitations; conclusions and implications of key findings; systematic review registration number.	2
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of what is already known.	2, 6-7
Objectives	4	Provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS).	7
METHODS			
Protocol and registration	5	Indicate if a review protocol exists, if and where it can be accessed (e.g., Web address), and, if available, provide registration information including registration number.	NA
Eligibility criteria	6	Specify study characteristics (e.g., PICOS, length of follow-up) and report characteristics (e.g., years considered, language, publication status) used as criteria for eligibility, giving rationale.	7-8, 18-21
Information sources	7	Describe all information sources (e.g., databases with dates of coverage, contact with study authors to identify additional studies) in the search and date last searched.	7
Search	8	Present full electronic search strategy for at least one database, including any limits used, such that it could be repeated.	7
Study selection	9	State the process for selecting studies (i.e., screening, eligibility, included in systematic review, and, if applicable, included in the meta-analysis).	8

Data collection process	10	Describe method of data extraction from reports (e.g., piloted forms, independently, in duplicate) and any processes for obtaining and confirming data from investigators.	8-9
Data items	11	List and define all variables for which data were sought (e.g., PICOS, funding sources) and any assumptions and simplifications made.	9
Risk of bias in individual studies	12	Describe methods used for assessing risk of bias of individual studies (including specification of whether this was done at the study or outcome level), and how this information is to be used in any data synthesis.	NA
Summary measures	13	State the principal summary measures (e.g., risk ratio, difference in means).	9
Synthesis of results	14	Describe the methods of handling data and combining results of studies, if done, including measures of consistency (e.g., I^2) for each meta-analysis.	8-9

Section/topic	#	Checklist item	Reported on page #
Risk of bias across studies	15	Specify any assessment of risk of bias that may affect the cumulative evidence (e.g., publication bias, selective reporting within studies).	9
Additional analyses	16	Describe methods of additional analyses (e.g., sensitivity or subgroup analyses, meta-regression), if done, indicating which were pre-specified.	9-10
RESULTS			
Study selection	17	Give numbers of studies screened, assessed for eligibility, and included in the review, with reasons for exclusions at each stage, ideally with a flow diagram.	Figure 1
Study characteristics	18	For each study, present characteristics for which data were extracted (e.g., study size, PICOS, follow-up period) and provide the citations.	Table 1-2
Risk of bias within studies	19	Present data on risk of bias of each study and, if available, any outcome level assessment (see item 12).	NA
Results of individual studies	20	For all outcomes considered (benefits or harms), present, for each study: (a) simple summary data for each intervention group (b) effect estimates and confidence intervals, ideally with a forest plot.	Table 1-2
Synthesis of results	21	Present results of each meta-analysis done, including confidence intervals and measures of consistency.	9, Table 3

Risk of bias across studies	22	Present results of any assessment of risk of bias across studies (see Item 15).	Figure 2
Additional analysis	23	Give results of additional analyses, if done (e.g., sensitivity or subgroup analyses, meta-regression [see Item 16]).	10, 23
DISCUSSION			
Summary of evidence	24	Summarize the main findings including the strength of evidence for each main outcome; consider their relevance to key groups (e.g., healthcare providers, users, and policy makers).	11-14
Limitations	25	Discuss limitations at study and outcome level (e.g., risk of bias), and at review-level (e.g., incomplete retrieval of identified research, reporting bias).	13
Conclusions	26	Provide a general interpretation of the results in the context of other evidence, and implications for future research.	14
FUNDING			
Funding	27	Describe sources of funding for the systematic review and other support (e.g., supply of data); role of funders for the systematic review.	unfunded

From: Moher D, Liberati A, Tetzlaff J, Altman DG, The PRISMA Group (2009). Preferred Reporting Items for Systematic Reviews and Meta-Analyses: The PRISMA Statement. PLoS Med 6(6): e1000097. doi:10.1371/journal.pmed1000097

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