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Faith-adapted psychological therapies for depression and anxiety: systematic review and meta-analysis

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Abstract

Background

Incorporating faith (religious or spiritual) perspectives into psychological treatments has attracted significant interest in recent years. However, previous suggestion that good psychiatric care should include spiritual components has provoked controversy. To try to address ongoing uncertainty in this field we present a systematic review and meta-analysis to assess the efficacy of faith-based adaptations of *bona fide* psychological therapies for depression or anxiety.

Methods

A systematic review and meta-analysis of randomised controlled trials was performed.

Results

The literature search yielded 2,274 citations of which 16 studies were eligible for inclusion. All studies used cognitive or cognitive behavioural models as the basis for their faith-adapted treatment (F-CBT). We identified statistically significant benefits of using F-CBT. However, quality assessment using the Cochrane Risk of Bias tool revealed methodological limitations that reduce the apparent strength of these findings.

Limitations

Whilst the effect sizes identified here were statistically significant, there were relatively few relevant RCTs available, and those included were typically small and susceptible to significant biases. Biases associated with researcher or therapist allegiance were identified as a particular concern.

Conclusions

Despite some suggestion that faith-adapted CBT may out-perform both standard CBT and control conditions (waiting list or 'treatment as usual'), the effect sizes identified in this meta-analysis must be considered in the light of the substantial methodological limitations that affect the primary research data. Before firm recommendations about the value of faith-adapted treatments can be made, further large-scale, rigorously performed trials are required.

Keywords

Psychological therapy, Cognitive behavioural therapy, Religion, Spirituality, Depression, Anxiety

Abbreviations

CBT Cognitive behavioural therapy

F-CBT Faith-adapted therapy (based on cognitive or cognitive behavioural models)

Introduction

The Pew Research Center estimate that 84% of the global population self-identify with a religious group, whilst only 16% report no religious affiliation (Hackett and Grim, 2012). Religious involvement has been associated with better mental health (Koenig, 2008b; Koenig et al., 2001; Moreira-Almeida et al., 2006). Further, religious service attendance is believed to be a protective factor against completed suicide (Kleiman and Liu, 2014).

Many studies have reported that people with mental health problems are more likely to be religious. A US study reported that over 80% of people with persistent mental illness use religious beliefs or activities to help them cope (Tepper et al., 2001), and other studies report that clients are both willing and want to discuss religious concerns in psychotherapy (Baetz et al., 2004; Rose et al., 2008).

Psychiatrists and psychologists, however, are less likely to be religious than the general population (Baetz et al., 2004; Koenig, 2009; Pargament, 2011). Since the time of Freud, there have been widespread negative views about religion within the psychiatric community, perhaps linked to the pathological expression of religious delusions (Koenig, 2008b; Moreira-Almeida et al., 2006). Previous suggestion that good psychiatric care should include spiritual components (Koenig, 2008b) provoked controversy (Cook, 2011). Despite these barriers, a wealth of literature explores the inclusion of religion in counselling and psychotherapy. Pastors and psychologists alike have published case studies to explain or promote 'cognitive-theological' approaches (see for example, Bingaman (2007) or Cole (2008), and in the past decade a number of randomised controlled trials have evaluated faith-adapted psychological treatments.

Previous reviews (Hook et al., 2010; McCullough, 1999; Smith et al., 2007; Worthington et al., 2011b) have produced somewhat contradictory conclusions about the value of incorporating religion into psychological treatment. A meta-analysis (Smith et al., 2007) of mixed study designs identified 'moderately strong' evidence that faith-adapted treatments outperform standard psychological treatments (effect size 0.56), but more recent reviews of randomised controlled trials (Hook et al., 2010; Worthington et al., 2011b) found insufficient evidence to identify differential outcomes. One review (Hook et al., 2010) identified that "methodological problems... make it difficult to make strong conclusions about the specificity of therapies." However, neither this, nor subsequent reviews have formally analysed the quality of individual studies nor the impact of biases on review findings. Further, previous meta-analyses have typically combined results of interventions for different mental health problems and different faith groups, resulting in meta-analysis of highly heterogeneous studies.

Culture and religion are highly interlinked, and the National Institute for Health and Care Excellence (NICE) now recommend that clinicians should be competent to provide a "culturally sensitive" assessment and treatment (National Institute for Health and Clinical Excellence, 2010), including "seeking the advice and support of an appropriate religious or community leader to support the therapeutic process" (National Institute for Health and Clinical Excellence, 2006) where necessary (for example, in treating obsessions with religious content).

Definitions

There has been some controversy over the definition of the terms 'religion' and 'spirituality' (Koenig, 2008a). In line with other researchers (Koenig, 2009; Moreira-Almeida et al., 2006) we use 'religious' to refer to an allegiance to the beliefs, teachings or traditions of an *organised* religion, arising from a group of people with common beliefs and practises concerning the sacred. By contrast, we use the term 'spiritual' to refer to a more *personal* relationship with or belief in the transcendent, which may or may not coincide with the recognised beliefs of an organised religion. We use the term 'faith' to encompass both religion and spirituality. Note that, using these definitions, faith is categorically contrasted with secular (e.g. atheist) perspectives.

Whilst religiosity is relatively straight-forward to measure, spirituality is more difficult. Clients may self-categorise as 'spiritual' but care must be taken to avoid inadvertently conflating measures of spirituality with features of positive mental health such as feelings of well-being, harmony or peacefulness (Koenig, 2008a). For the purpose of this

review measurement of a participants' degree of spirituality was not necessary: self-categorisation as religious or spiritual was sufficient for inclusion in studies, and randomisation to treatment groups was independent of the degree of a participant's spirituality (if this was measured).

The relationship between religion and culture is complex and, for some, they are inextricably linked. For practical reasons we distinguish faith-based adaptations as those with explicit reference to faith (including but not limited to: reference to religious teachings or scriptures, discussion of a person's faith or beliefs, or use of faith-based practices such as prayer within the therapy).

Aim

Owing to the expansion of research in recent years and the limitations of previous reviews, the aim of the current study was to establish whether there is now sufficiently robust evidence to recommend clinicians provide faith-adapted treatments for their religious or spiritual clients. We systematically review the clinical efficacy of faith-based adaptations of *bona fide* psychological treatments for common mental health problems (depression and anxiety) relative to standard psychological interventions or control conditions, using data from randomised controlled trials.

Methods

This review has been prepared in accordance with PRISMA guidelines (Moher et al., 2009).

Eligibility criteria

Our review protocol, which specified methods of analysis and eligibility criteria, is reproduced in the online supplement to this article.

Types of studies: Studies were restricted to randomised controlled trials (RCTs). No language, publication date, or publication status restrictions were imposed. Two Persian full-text articles and a number of French, German, Dutch and Arabic abstracts were translated.

Types of participants: Adult participants (aged 16 and over) with depression or anxiety in any setting were considered. Diagnosis could use a gold standard (e.g. DSM-IV), attainment of a threshold on a psychological measure (e.g. a cut-off on the Beck Depression Inventory) or clinician diagnosis. During the screening stage we added one further exclusion criteria that was not identified in our original protocol: a significant number of studies focused on populations with particular physical conditions, for example, breast cancer or HIV. We hypothesised that such populations might respond differently to faith-adapted interventions, compared with the typical populations that present to psychiatric services with depression or anxiety. For this reason we excluded any study in which the study population was defined by a physical illness, and instead refer readers to recent Cochrane (Candy et al., 2012) and Department of Health (Holloway et al., 2011) reviews of spiritual interventions for patients with terminal conditions.

Types of intervention: We considered trials that compared faith-adapted psychological interventions with the same type of standard (secular) psychological therapy or control conditions (waiting list, placebo or treatment as usual). Faith-adapted psychological interventions were limited to those developed from a *bona fide* psychological treatment, that is, an intervention based on recognised psychological theory and intended as therapeutic (Wampold et al., 1997).

Studies examining culturally adapted treatments that had minimal faith-based adaptations were also excluded. However, if an intervention was described as a cultural adaption, but the description of the intervention indicated that the adaptations wholly or substantially concerned religion or faith, the study was included.

There have been a number of recent systematic reviews assessing the efficacy of Mindfulness-based meditation and yoga in mental health settings (Chiesa and Serretti, 2011; Cramer et al., 2013; Klainin-Yobas et al., 2012; Meyer et al., 2012; Piet and Hougaard, 2011). Whilst these interventions are derived from Buddhism and Hinduism respectively, they are not generally evaluated or delivered as a faith-adapted approach. We have therefore excluded them from this review.

Types of outcome measure: Studies had to report mental health outcomes using a recognised psychological severity scale or diagnostic tool (whether dichotomous or continuous).

Search strategy

An evaluation of databases for use in Mental Health and religion reviews informed our selection of databases (Wright et al., 2014). Our sources included a range of health, social science and grey literature databases, and all were searched from inception: MEDLINE, MEDLINE In-Process, PsycINFO, EMBASE, Global Health, HMIC, CINAHL, Cochrane Central Register of Controlled Trials, Cochrane Database of Abstracts of Reviews of Effect, Cochrane Database of Systematic Reviews, ProQuest Conference Papers Index, Clinicaltrials.gov, ProQuest Dissertations & Theses, Sociological Abstracts, ArabPsyNet, FRANCIS, Controlledtrials.com. Full details are available in the online supplement.

All searches were run in January 2014 by a dedicated information specialist (JW). We also examined the reference lists and conducted reverse-citation searches of all included studies, and used the reference lists of previous systematic reviews to identify additional studies (Bonelli and Koenig, 2013; Hook et al., 2010; McCullough, 1999; Smith et al., 2007; Worthington et al., 2011a). If publications (e.g. conference abstracts) did not provide sufficient detail to include them, we made additional searches for subsequent publications by the same author(s) or contacted the study authors to request unpublished results.

We used three search concepts: mental health or psychotherapy, faith, and randomised controlled trials. The Cochrane Highly Sensitive Search Strategy for identifying randomised trials in MEDLINE: sensitivity- and precision-maximizing version was used to identify RCTs in MEDLINE. A final search strategy combining all three search concepts was developed in PsycInfo and agreed with the project team before translating into the other databases. Full details of all search strategies are available in the online supplement.

One reviewer (NA) assessed all titles and abstracts to identify potentially eligible studies using pre-specified sifting criteria (available in the online supplement to this publication). The reviewer then assessed full text articles of all the potentially eligible studies to identify studies for inclusion. At each stage queries were discussed with a second researcher and disagreement resolved by consensus and where necessary, consultation with a third reviewer.

Data extraction

One reviewer extracted data to a standardised data extraction sheet. The data extraction sheet was pilot-tested with the first four studies and modified accordingly (to include therapist allegiance as a descriptive characteristic). The modified data extraction sheet covered descriptive characteristics of the studies, quality assessment, and outcome data.

For each study we recorded the setting (including faith group, mental health problem and summary inclusion criteria), population demographics (age, gender and location), intervention (name of intervention, psychological theory from which it was developed, mode of delivery, number and duration of sessions, and number of participants), comparator, and therapist characteristics (including level of qualification, number of therapists, therapist allocation, therapist faith, and whether therapists were study researchers)

The risk of bias in individual studies was assessed by one reviewer using the Cochrane risk of bias tool. Queries were discussed with a second researcher and disagreement resolved by consensus. In line with recent Cochrane reviews

of psychological interventions, e.g. Shinohara (Shinohara et al., 2013), the questions from this tool were supplemented by additional ones to assess other sources of potential bias that are unique to or particularly problematic for trials of psychological interventions, and which are not covered by the Cochrane tool. These included questions to assess researcher allegiance (whether the researchers developed the faith-adapted treatment themselves), therapist adherence to protocol and therapist competence in the delivery of the psychological intervention. Publication bias was assessed using funnel plots.

Analysis

As the studies reported outcomes using a variety of continuous severity scales, we calculated standardised mean differences with 95% confidence intervals. If an individual study presented data for different subgroups but all subgroups would have been eligible for inclusion in this review (for example, studies that stratified results by client or therapist religiosity), pooled summary measures were extracted.

As studies typically reported results immediately post-treatment, at 4-6 weeks post-treatment, or 12 weeks post-treatment (with no studies reporting longer term follow-up), we altered the temporal groupings proposed in the original protocol (post-treatment, 6 months and 12 months post-treatment) to reflect the available data.

Meta-analyses were performed if studies were sufficiently homogenous ($I^2 < 50\%$) and clinically similar for comparison to be meaningful. In cases where $I^2 \geq 50\%$ (pre-specified as 'significant heterogeneity'), the effect of outliers was investigated: if removal of a single outlier reduced the degree of heterogeneity to $I^2 < 50\%$ the outlier was excluded from the meta-analysis. For clinical similarity we pre-specified that studies should treat the same mental health condition (depression versus anxiety), be adapted for the same faith group, use the same type of comparator (standard psychological treatment versus control conditions) and the same type of therapy. Using the classifications proposed by a recent Cochrane review (Shinohara et al., 2013) all adapted treatments in the present review were broadly classified as cognitive behavioural, thus this additional pre-specified grouping was redundant, and faith-adapted treatments are henceforth referred to as faith-adapted cognitive behavioural therapy (F-CBT).

We calculated effect sizes using Hedge's g (i.e. the standardised mean difference, with correction for biases due to small sample sizes (Hedges and Olkin, 1985)), and used a random-effects model in all meta-analyses to reflect the variation between F-CBT protocols, comparators and study populations. All analyses were performed in Review Manager 5.1

There was insufficient documentation of therapists' beliefs in the primary literature, so this pre-planned subgroup analysis was not feasible. As predicted in the protocol, it was not feasible to perform meaningful sensitivity analyses or meta-regressions based on quality assessment data. Instead, the potential impact of biases across studies will be discussed narratively.

Results

Study selection

As shown in the PRISMA flow-diagram (figure 1), our searches yielded 2,276 citations, which left 1,435 after de-duplication, and 85 after screening of titles and abstracts. Of these, 69 were discarded after examination of the full-text articles and 16 met all the inclusion criteria for final inclusion in this review.

One article (Razali et al., 1998) reported results for clients with either depression or anxiety (with results recorded by diagnosis), and a number of studies reported three-arm trials, in which F-CBT was compared with both standard CBT and control conditions. Thus, some articles documented more than one comparison. In total, 16 articles (21 comparisons) were obtained, of which one was translated from Persian and two were unpublished doctoral theses.

The individual reasons for excluding full-text articles are provided in the table A1 (in the online supplement). Overall 69 articles were rejected: 12 were not randomised controlled trials, 2 were excluded on the basis of ineligible population, 42 were excluded on the basis of intervention, 2 did not report a psychological outcome using a recognised psychological severity scale or diagnostic tool, and 11 did not report sufficient detail to compute the standardised mean difference. Of the 42 studies excluded on the basis of intervention, 6 used add-on therapies which were not integrated into the standard psychological therapy, 19 interventions were predominantly cultural rather than faith-based, and 17 did not adapt a bona fide psychological therapy. Of the 11 articles that gave insufficient detail to be included, 6 were conference abstracts or thesis that were published elsewhere (i.e. duplicates), 3 were conference abstracts that were not published elsewhere (and attempts to contact authors was unsuccessful), and 2 related to ongoing studies for which results are not yet available.

Study characteristics

Characteristics for all included studies are provided in table A2 (in the online supplement), and summarised in table 1. Of the studies comparing F-CBT with control conditions, 7 studied depression (4 Christian-CBT, 3 Muslim-CBT), and 6 studied anxiety (1 Christian-CBT, 3 Muslim-CBT, 1 Taoist-CBT, 1 Jewish-CBT). Of the studies comparing F-CBT with standard CBT, 7 studied depression (6 Christian-CBT, 1 Muslim-CBT), and one studied anxiety (Muslim-CBT).

Methods of incorporating faith into the faith-adapted treatment (see table 1) were generally limited to various combinations of the following:

- Discussion of religious teachings or scriptures as supportive evidence to counter irrational thoughts or to support cognitive or behavioural change;
- Use of positive religious or spiritual coping techniques (for example, applying scriptural or spiritual solutions to the psychological problems of fear, anger, guilt, shame or despair);
- Promotion of helpful belief or value systems, or use of shared value systems to strengthen therapeutic relationships;
- Incorporation of religious practises such as prayer.

On average, courses of Muslim-CBT lasted longer (17 weeks; range 8 to 26 weeks) than Christian-CBT (9 weeks; range 4 to 18 weeks) or spiritual-CBT (3.5 weeks; range 2 to 5 weeks). This difference was significant at the 95% confidence level ($F=4.49$, $df=4$, $p=0.02$). The two studies (Armento et al., 2012; Gibbel, 2011) that investigated spiritual-CBT accepted participants of any faith, but their samples were predominantly Christian (94% and 77% respectively).

Two studies (Gibbel, 2011; Rosmarin, 2010) used computer-delivered therapy, four (Azhar and Varma, 1995; Azhar et al., 1994; Barron, 2007; Bowland et al., 2012) used face-to-face group sessions and ten (Akuchekian et al., 2011; Armento et al., 2012; Ebrahimi et al., 2013; Johnson and Ridley, 1992; Johnson et al., 1994; Pecheur and Edwards, 1984; Propst et al., 1992; Razali et al., 2002; Razali et al., 1998; Zhang et al., 2002) provided individual face-to-face sessions. There was no significant difference between the average length (in weeks) or duration (in minutes) of F-CBT provided in group settings, compared with individual sessions.

Table 1: Description of ways that faith was incorporated into the faith-adapted treatment

Description of faith adaptation		Depression	Anxiety	F-CBT v	
				Control	CBT
<u>Christian-CBT</u>					
• Barron (2007)	Used religious beliefs and practices to support religious coping skills.	✓			✓
• Bowland (2012)	Discussed spiritual histories and spiritual 'gifts' (eg forgiveness, trust); formulated a 'spiritual recovery action plan'.		✓	✓	
• Johnson (1992, 1994)	Used biblical teachings to dispute irrational beliefs; emphasised prayer and Christian content within therapy.	✓			✓
• Pecheur (1984)	Used religious and biblical teachings to challenge negative cognitions.	✓		✓	✓
• Propst (1992)	Applied Christian religious rationales for CBT procedures (eg to counter irrational thoughts).	✓		✓	✓
<u>Spiritual-CBT</u>					
• Armento (2012)	Used value assessments based on religious / spiritual views; encouraged participation in religious activities; reframed situations using religious ideas of acceptance.	✓		✓	
• Gibbel (2010)	Used spiritual strategies to try to cope with and overcome negative cognitions.	✓		✓	✓
<u>Muslim-CBT</u>					
• Akuchekian (2011)	Incorporated Muslim teachings into therapy; provided joint care from a psychiatrist and a religious teacher.		✓		✓
• Azhar (1994, 1995)	Discussion of religious issues with reference to Koran; included prayer.	✓	✓	✓	
• Ebrahimi (2013)	Used religious beliefs to suggest goals and questions use within therapy.	✓		✓	✓
• Razali (1998, 2002)	Challenged negative thoughts using teachings from the Koran and Hadith; discussed religious issues related to illness.	✓	✓	✓	✓
<u>Jewish-CBT</u>					
• Rosmarin (2010)	Used Jewish scriptures and stories to inspire change; used spiritual exercises and prayer.		✓	✓	
<u>Taoist-CBT</u>					
• Zhang (2002)	Incorporated Taoist values into therapy.		✓	✓	

Risk of bias within studies

A summary of the quality assessment of included studies is shown in figure 2.

Randomisation

Only one article (Rosmarin, 2010) described the method of sequence generation in order to randomise participants, and no studies reported methods for concealing allocation.

Blinding

In line with other authors (Shinohara et al., 2013) we do not consider effective blinding of participants and therapists in psychotherapy trials to be possible. For this reason all studies were judged to be at high risk of performance bias.

In terms of blinding of outcome assessment, six articles reported adequate strategies for blinding, four made it clear that they did not blind outcome assessment, and five gave insufficient detail to make judgement.

Attrition

6 studies reported sufficiently low levels of attrition ($\leq 10\%$) for us to rate the risk of attrition bias as low. The two studies reporting use of computer-delivered therapy (c-CBT) had very high levels of attrition: one (Gibbel, 2011) reported over three times the levels of attrition from the standard c-CBT group (46%) compared with faith-adapted c-CBT (13%), and the other (Rosmarin, 2010) recorded that 66% of participants dropped out of the study before

completion (although this study did use 'intention to treat' analysis and recorded similar drop-out rates across groups).

The remaining studies all had attrition rates of 10-20%. Even in the studies that demonstrated no statistically significant difference at baseline between participants that dropped out and those that completed, we were not able to confidently judge the effect of the drop-outs on the reported outcomes or study power, and therefore rated the risk of attrition bias in these studies as 'unclear'.

Reporting bias

With one exception (Bowland et al., 2012), studies were judged to be at low risk of reporting bias. Although none actually referred to study protocols in detail, we judged that the 'low risk' studies appeared to report all expected outcomes regardless of effect size or direction.

Therapist factors

As defined by Waltz *et al* (Waltz et al., 1993), therapist *adherence* refers to "the extent to which a therapist used interventions and approaches prescribed by the treatment manual and avoided the use of intervention procedures proscribed by the manual." By contrast, *competence* refers to "the level of skill shown by the therapist in delivering the treatment. By skill, we mean the extent to which the therapist conducting the intervention took the relevant aspects of the therapeutic context into account and responded to these contextual variables appropriately." They note that "competence pre-supposes adherence, but adherence does not necessarily imply competence" (Waltz et al., 1993). In the case of faith-adapted treatments, competency might also be influenced by a number of therapist factors, for example, the therapist's faith beliefs, their confidence in working with clients of a different- or same-faith background as a central part of the intervention, and their ability to manage therapeutic impasses related to these beliefs. The question of therapist-client matching is also important: a previous meta-analysis (Cabral and Smith, 2011) that studied the effect of matching clients and therapists by race or ethnicity found some evidence to suggest that such matching improves therapeutic outcomes. Similar effects might be expected when therapists and clients are matched by religion.

Based on the above definitions, we looked for evidence that researchers used techniques (e.g. scrutinising tape-recordings of sessions) to assess adherence and competence. Two studies (Bowland et al., 2012; Johnson et al., 1994) were rated 'low risk' with respect to therapist adherence. No other studies attempted to quantify adherence. Similarly, no studies adequately analysed therapist competence. Both these therapist factors were rated 'not applicable' if the therapy was delivered electronically.

Only one study (Propst et al., 1992) used separate religious and non-religious therapists to deliver both CBT and F-CBT: each individual therapist only provided one type of therapy (F-CBT or CBT), and participants were randomised to one of eight therapists. Significant differences in psychological outcomes were identified, based on the faith of the therapist as well as the faith basis of the therapy. Five of the remaining seven studies comparing F-CBT with CBT (Akuchekian et al., 2011; Barron, 2007; Johnson and Ridley, 1992; Johnson et al., 1994; Pecheur and Edwards, 1984) used the same therapist(s) to deliver both treatments, of which only two (Johnson and Ridley, 1992; Pecheur and Edwards, 1984) recorded the faith of the therapists. One study (Gibbel, 2011) was computer delivered, and the final one (Ebrahimi et al., 2013) gave no information about the therapist(s).

Researcher allegiance

If a researcher has an allegiance with one particular treatment, this may bias the treatment outcomes (Luborsky et al., 1999; Munder et al., 2013). In the case of faith-adapted treatments various forms of allegiance could potentially occur. On the basis of the information available from the included studies, we rated studies as being at 'high risk' of researcher allegiance bias if the faith-adapted treatment protocol was designed by one of the study authors. Only one study (Bowland et al., 2012) adopted an existing treatment protocol; all other studies were rated as 'high risk'.

Problems of researcher allegiance may be compounded if the researcher acts as the therapist. Eight studies (Akuchekian et al., 2011; Armento et al., 2012; Barron, 2007; Bowland et al., 2012; Johnson and Ridley, 1992; Pecheur and Edwards, 1984; Propst et al., 1992; Zhang et al., 2002) reported that the primary researcher took an active part in delivering one or both therapies, and only one study (Johnson et al., 1994) made it clear that the primary researcher did not deliver any therapy. In the remaining five studies (Azhar and Varma, 1995; Azhar et al., 1994; Ebrahimi et al., 2013; Razali et al., 2002; Razali et al., 1998) it was unclear whether the researchers acted as therapists.

Faith adapted treatment versus control conditions

Depression studies

F-CBT is compared with control conditions (waiting list, placebo or treatment as usual) in figure 3. The seven studies comparing the use of F-CBT for depression with control conditions included two Christian-adapted, two spiritually adapted and three Muslim-adapted interventions. All seven studies of depression indicated beneficial effects of F-CBT, although not all results were statistically significant (i.e. the 95% confidence interval included zero). Effect sizes are quoted with their 95% confidence intervals [95% CI].

One Muslim-adapted intervention (Ebrahimi et al., 2013) was excluded from the meta-analysis as an outlier, which reduced the heterogeneity amongst the Muslim-adapted studies from $I^2=91\%$ to $I^2=0\%$. The meta-analyses of the remaining three pairs of studies are shown in figure 4 and summarised in table 2. The heterogeneity within these pairings was minimal ($I^2=0\%$), and the pooled effect sizes were -1.47 [-2.09, -0.70] for Christian-adapted interventions (n=44), -0.48 [-0.88, -0.07] for spiritually adapted interventions (n=96), and -0.30 [-0.60, 0.01] for Muslim-adapted interventions (n=164). These results indicate that participants receiving F-CBT for depression improved more than those in control groups. Note that the excluded Muslim-CBT study indicated a greater benefit of F-CBT than was seen in the included Muslim-CBT studies.

Table 2: Comparison of F-CBT with control conditions or standard CBT in the treatment of depression

Depression studies	F-CBT versus control conditions				F-CBT versus standard CBT			
	Effect size	n	I^2	studies	Effect size	n	I^2	studies
Christian-CBT	-1.47 [-2.09, -0.70]	44	0%	2	-0.59 [-0.95, -0.23]	124	0%	4
Spiritual-CBT	-0.48 [-0.88, -0.07]	96	0%	2	-0.55 [-1.17, 0.06]	43	-	1
Muslim-CBT	-0.30 [-0.60, 0.01]	164	0%	2	-0.31 [-1.01, 0.39]	32	-	1

At the 95% confidence level, benefits of F-CBT were statistically significant for Christian- and spiritually adapted interventions, and border-line significant for Muslim-adapted interventions. Visually, the benefit of adapting CBT for Christian clients appears to be greater than that for Muslim clients. However, there were numerous other differences between the studies (e.g. different treatment duration, different study settings, different study eligibility criteria, different study sizes) so formal comparison of the different faith-adapted treatments cannot be made from the available data.

The two spiritually adapted studies (Armento et al., 2012; Gibbel, 2011) reported outcomes at one month follow-up (that is, one month after completion of the treatment period), as shown in figure 5. For these two studies (n=94, $I^2=0\%$) the pooled effect size was -0.30 [-0.71, 0.10], indicating no significant benefit of spiritual-CBT compared with control conditions at follow-up. Additionally, one Muslim-CBT study (Ebrahimi et al., 2013) reported outcomes at 12 week follow-up. However, as this study's comparison of F-CBT with control conditions was previously identified as an outlier, it is difficult to draw meaningful conclusions from the statistically significant benefit they recorded at follow-up.

Anxiety studies

Six studies compared F-CBT for anxiety with control conditions (see figure 3). Of these, one used was Christian-CBT, three used Muslim-CBT, one used Jewish-CBT and one used Taoist-CBT. The three Muslim-adapted studies demonstrated significant statistical heterogeneity ($I^2=86\%$) with no clear outlier, and the other studies were clinically different (i.e. adapted for different faiths). Thus our pre-defined conditions for performing meta-analysis were not met.

Instead we note that from the individual studies identified, there were statistically significant benefits of F-CBT compared with control conditions for three studies (one Muslim-CBT ($n=62$, $p<0.001$), one Christian-CBT ($n=43$, $p=0.01$) and one Taoist-CBT ($n=87$, $p<0.001$) study). There were borderline significant benefits of one Jewish-CBT ($n=83$, $p=0.08$) and one Muslim-CBT ($n=103$, $p=0.12$) study.

One Muslim-CBT study ($n=165$, $p=0.36$) indicated that F-CBT may be marginally inferior to control conditions, but this particular study (Razali et al., 2002) provided separate data for 'religious' and 'non-religious' Muslim clients. They identified statistically significant benefits of F-CBT in the religious subgroup, but not in the non-religious subgroup. Our protocol stated "samples will typically consist of people who define themselves as religious, spiritual, or having a faith, [but] this will not be an inclusion criterion." Therefore the data that we included in our meta-analysis from this study represented the pooled outcome from the religious and non-religious subgroups.

Note, for demonstrative purposes, that if the anxiety studies comparing F-CBT with control conditions had been combined across faiths, excluding this single Muslim-CBT study (Razali et al., 2002) as an outlier, the pooled effect size ($n=378$, $I^2=45\%$) would be -0.64 [-0.93 , -0.36], suggesting that there may be some advantage of F-CBT in treating anxiety compared with control conditions. However, the clinical heterogeneity in pooling these studies mean that great caution is required interpreting such a result.

The Jewish-CBT study (Rosmarin, 2010) recorded outcomes six weeks after completion of treatment (see figure 5): they identified a statistically significant benefit of F-CBT (effect size -0.99 [-1.50 , -0.48] from $n=73$), suggesting that benefits continued at follow-up. However, the Muslim-adapted study (Azhar et al., 1994) that recorded outcomes at 12 week follow-up did not find any statistically significant benefit of F-CBT at this stage.

Faith adapted treatment versus standard CBT

Depression studies

As shown in figure 6, all seven studies comparing F-CBT with standard (secular) CBT for the treatment of depression indicated a benefit of F-CBT, although no study found the benefit to be statistically significant. Five studies compared Christian-CBT with standard CBT. Pooling all five studies produced a non-significant effect size of -0.31 [-0.80 , 0.17]. However, there was significant heterogeneity between the studies ($I^2=50\%$). As shown in figure 7, removing one study (Johnson et al., 1994) as an outlier reduced the heterogeneity to $I^2=0\%$, giving a revised pooled effect size from the remaining four studies ($n=124$) of -0.59 [-0.95 , -0.23]. This revised estimate indicates a statistically significant benefit of Christian-CBT compared with standard CBT. As would be expected, the magnitude of this effect is somewhat smaller than that comparing F-CBT with control conditions.

Five of the studies (Ebrahimi et al., 2013; Gibbel, 2011; Johnson et al., 1994; Pecheur and Edwards, 1984; Propst et al., 1992) compared F-CBT with standard CBT at follow-up (figure 8), but none of the results were statistically significant. Two studies (Johnson et al., 1994; Propst et al., 1992) were clinically similar, recording 12 week follow-up results after receiving Christian-CBT for depression, but they demonstrated significant statistical heterogeneity ($I^2=71\%$). Thus, clinical and statistical heterogeneity precluded pooling.

Anxiety studies

Only one study (Akuchekian et al., 2011) compared F-CBT with standard CBT in the treatment of anxiety (see figure 6). This single study ($n=46$) indicated a statistically significant advantage of F-CBT over standard CBT ($p<0.001$), with

an effect size of -1.58 [-2.25, -0.91]. This result is of such an extreme magnitude (compared with the effect sizes for F-CBT versus control conditions) that we suspect it would eventually be excluded as an outlier. The cause of this extreme result is unclear, but we note that this particular study used a faith-adapted therapy to treat obsessive compulsive disorder *with religious content*. It is possible that the benefits of using a faith-adapted treatment in this setting are significantly different to those of F-CBT in other anxiety contexts (typically generalised anxiety disorder or post-traumatic stress disorder). No follow-up results were available from this study.

Publication bias

Assessment of publication bias is limited by the very small numbers of studies within each clinical group (for example, Christian-CBT for depression versus control conditions). Nevertheless, we felt that it was informative to group the depression studies across faiths in order to visually inspect the distribution of studies.

The funnel plot in figure 9 provides a possible indication that, despite inclusion of unpublished studies (Barron, 2007; Gibbel, 2011), our estimate of effect size between F-CBT and control conditions may over-estimate the true benefit of F-CBT. However, as the Christian-CBT studies were smaller than the spiritual-CBT studies, which, in turn, were smaller than the included Muslim-CBT studies, it is possible that the differential efficacy of F-CBT across the studies instead reflects the different faith-groups that the adaptations catered for, or other differences (e.g. intervention duration) between the trials.

Depression studies comparing F-CBT with standard CBT are shown in the funnel plot in figure 10. The Christian-CBT studies were all closely grouped around the pooled effect size, with no clear indication of publication bias. Further, the individual spiritual- and Muslim-CBT studies fit closely around the same effect size estimate. This would appear to contradict the above suggestion that the efficacy of F-CBT might vary according to the faith of the target population.

Discussion

Main findings

This meta-analysis found some suggestion that faith-adapted CBT may outperform both control conditions and standard CBT in the treatment of depression. Statistically, effect sizes were of moderate magnitude; however as all studies were at high risk of various biases (in particular, researcher allegiance), considerable caution is required in interpreting this result.

Results from anxiety studies were broadly similar, but there were insufficient numbers of clinically similar and statistically homogenous studies to perform a meta-analysis. Five of the six studies identified indicated a possible benefit of F-CBT compared with control conditions (three of which were statistically significant), and the one study comparing F-CBT with standard CBT also suggested that F-CBT may be superior for treating anxiety.

Our findings are in line with previous meta-analyses of faith-adapted psychological therapies (Hook et al., 2010; McCullough, 1999; Worthington et al., 2011b) regarding the superiority of F-CBT compared with control conditions. However, to our knowledge, this is the first review to identify a benefit of F-CBT compared with standard (secular) CBT: previous meta-analyses have identified equivalence of F-CBT and CBT, but have had insufficient power to differentiate between treatments.

Given that previous comparative meta-analyses found that “the efficacies of *bona fide* [psychological] treatments are roughly equivalent” (Wampold et al., 1997), it is important to consider in detail the limitations of the primary data sources, which may provide an alternative explanation for the observed differences, particularly for the comparison of F-CBT with standard CBT.

Limitations

Whilst the effect sizes identified here were statistically significant, there were relatively few relevant RCTs available, and those included were typically small and susceptible to significant biases. Only one of the included studies adequately described their method of randomisation, and none of the studies described strategies to ensure allocation was adequately concealed. Further, although it is not possible to blind participants or therapists to their treatment group, less than half of the studies used blinded assessors to record outcome scores.

Of greatest concern, fifteen of the sixteen studies included described the use of a faith-adapted treatment that had been devised by the study authors. Researcher allegiance is well recognised as an important form of bias in psychological studies (Gaffan et al., 1995; Leykin and DeRubeis, 2009; Munder et al., 2013), and previous discussions of the evidence basis for psychotherapy have identified authorship of the adapted treatment protocol as particularly 'worrysome' (Leykin and DeRubeis, 2009). Note that in eight of the studies, the primary researchers delivered the therapy, which may further compound the problems associated with researcher allegiance. All of these potential problems would serve to artificially inflate the effect size. It is possible, if not likely, that the observed effect size in these studies overstates the actual effectiveness of faith-adapted treatments.

As studies did not publish summary statistics for the *improvement* in psychological outcome scores, this meta-analysis was limited to the use of post-treatment scores to compare treatments. Comparing the post-treatment mean scores with the recognised diagnostic cut-offs for studies comparing F-CBT with standard CBT, in all seven depression studies both the experimental and the control groups had a post-treatment mean that fell into the 'minimal' symptom range (e.g. BDI<10 in both groups). Thus, both treatments would appear efficacious for treating depression. However, as the post-treatment sample variances were typically small, these small differences in post-treatment outcome scores may generate significant effect sizes despite the negligible difference clinically. Thus, it is very difficult to make a meaningful assessment of the clinical benefit that would result from the statistical benefit identified in this meta-analysis.

Sources of heterogeneity

Statistical heterogeneity was identified between F-CBT trials for clients of different faiths, and whilst there were too few similar studies to perform a meta-regression (which typically requires at least ten similar trials), several factors may have contributed to the apparent differences. Important clinical sources of heterogeneity include the duration of treatment (course length), the mode of delivering F-CBT (individual, group or internet sessions), the study inclusion criteria (e.g. DSM-IV based diagnosis compared with a cut-off on a psychological severity measure; mild versus severe depression), the scales used to record outcomes (e.g. self-reported measures compared with clinician rated scores), and the relative study sizes (ranging from n=10 to n=165).

We note that the Muslim-CBT studies were typically much larger (n=32 to 165, mean n=82) than the Christian-CBT studies (n=10 to 62, mean n=33), which could lead to inflated estimates of effect size amongst the Christian-CBT studies secondary to publication bias. In addition, there was significant variation in the treatment course-length, with Muslim-CBT lasting longer (typically 3 to 6 months) than Christian-CBT (typically 6 to 12 weeks). Thus, stratifying studies according to the faith of the target population (Muslim, Christian, or spiritual) also effectively grouped the studies by size and treatment duration.

Faith-adapted treatments may enable religious clients to tap into pre-existing resources or beliefs that complement or augment the methods and strategies used in CBT. For example, F-CBT might use the biblical passage "God demonstrated His own love towards us, in that while we were yet sinners, Christ died for us" (Romans 5:8) as evidence that "God loves, accepts, and values us just as we are" in order to challenge and help the client change the person's negative cognitions about him or herself (Pecher and Edwards, 1984). This might be easier for a religious client to accept and work with than the corresponding secular approaches to the problem. Thus, using their existing religious beliefs and skills in order to bring about change may be quicker than teaching a client a whole new set of

skills to modify their depressive symptoms, resulting in a quicker recovery. However, with time, participants in the control groups (whether standard CBT or waiting list) may gradually develop similar skills - and thus, after a longer treatment comparison (e.g. the six month Muslim-CBT protocols), results of F-CBT are relatively less beneficial than those measured after brief treatment courses. The primary study authors that compared treatments across different time frames (Azhar and Varma, 1995; Ebrahimi et al., 2013; Razali et al., 2002; Razali et al., 1998) all documented this effect.

Thus, as noted in the results section, the number of confounding factors makes it difficult to use the results of this meta-analysis to directly compare different F-CBT treatments with one another. Whilst it is possible that the ideologies of western style CBT are better aligned with native western religions (eg Christianity), study sizes, and perhaps the study durations, are equally (or perhaps more) plausible explanations of the observed heterogeneity.

The identified sources of heterogeneity also help explain differences between studies included and excluded in the meta-analyses. For example, the excluded study (Ebrahimi et al., 2013) comparing Muslim-CBT for depression with control conditions was of a much shorter duration than the other similar studies. Similarly, the unexpectedly large effect size from the anxiety study (Akuchekian et al., 2011) comparing Muslim-CBT with standard CBT may reflect a particular receptiveness to F-CBT amongst their specific study population (clients with OCD *with religious content*).

Implications

Based on the findings of this meta-analysis, there is some indication that faith-adapted CBT appears to be effective (i.e. superior to control conditions), and there is some possible suggestion that F-CBT may be superior to standard CBT in the treatment of depression and anxiety. However, the effect sizes identified in this meta-analysis must be considered in the light of the substantial methodological limitations that affect the primary research data. Using the GRADE approach described in the Cochrane handbook (Higgins and Green, 2011), the overall quality of the body of available evidence is rated 'low' (double-downgraded randomised trials, downgraded on the basis of indirectness (no UK studies) and limitations in primary research design methodology). In order for primary research in this area to inform UK clinical practice, rigorously performed British trials are urgently required.

In addition to the general guidance provided by publications like the CONSORT statement (Schulz et al., 2010), future studies should use the strategies implemented in other fields of psychological research to guard against biases. For example, as advocated by Leykin & DeRubeis (2009), we recommend explicit collaboration between investigators "who possess complementary areas of expertise, and correspondingly opposite allegiances." Thus, members of research teams should come from a range of faith allegiances and backgrounds (including atheist). Further, we would suggest that lead authors avoid delivering therapies themselves, and that they select therapists to represent a range of areas of expertise and faith allegiances. In addition, researchers should monitor and record measures of adherence to treatment protocols and therapist competence. Finally, as blinding of participants and therapists is not possible, we would suggest that outcomes be measured by blinded assessors, perhaps including a combination of self-report scales and clinician assessed scores (to enable future researchers to assess the impact of detection bias on trial outcomes).

In the absence of stronger evidence on the effectiveness of faith-adapted psychological treatments, it is difficult to make firm recommendations about the value of such interventions. Although there are considerable methodological limitations with the current evidence base, it would, however, be inappropriate to conclude that such treatments should not be offered. Previous research (Wynaden et al., 2005) suggest that religious patients may be unwilling to accept standard psychological therapies, and that incorporating their faith into the treatment may make those therapies more acceptable. Assessment of the acceptability of faith-adapted treatments was beyond the scope of this review, but remains an important question for commissioners.

In line with NICE guidance, provision of F-CBT might be a good way for mental health services to reduce barriers and improve access for this disadvantaged (National Institute for Health and Clinical Excellence, 2010) group of patients.

There is, however, considerable work to be done to establish the clinical effectiveness of these interventions. Further, before such interventions could be offered universally, there are a number of outstanding questions. For example, how are faith-adapted treatments best provided in areas with significant religious diversity, and can such treatments be provided effectively (and with integrity) by therapists of a different or no faith.

Role of funding source

This work was commissioned and funded by Bradford District Care Trust.

Conflict of interests

Our review team was composed of people of a range of different faiths (two Christians, one Muslim, two atheists and one agnostic). We believe that we addressed the question of faith-adapted treatments from a position of neutrality: no member of the review team is currently involved in providing faith-adapted treatments, but neither were we averse to the potential benefits of making faith-based adaptations for religious or spiritual clients.

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Figure 1: PRISMA flow-chart of the literature search

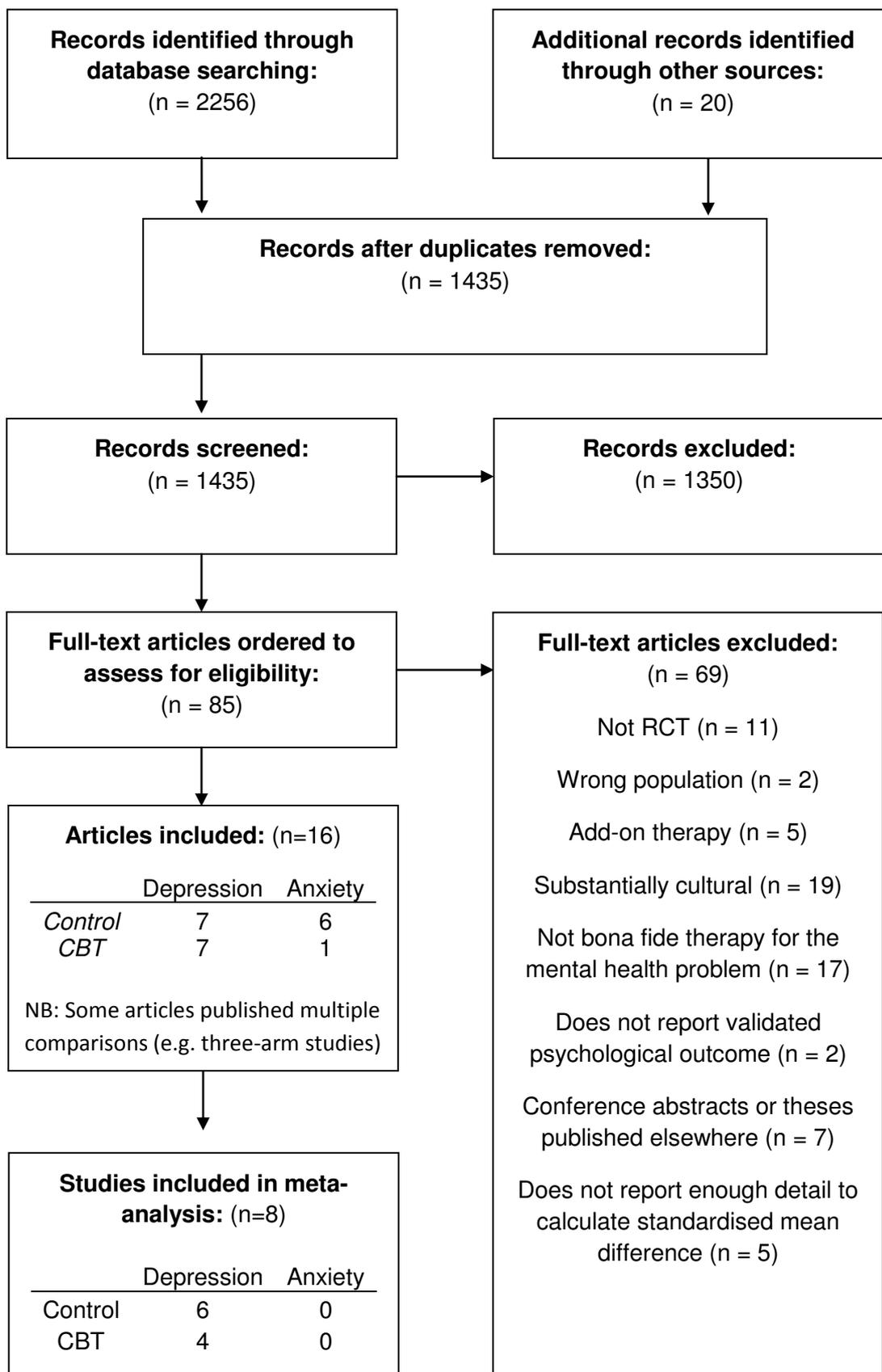


Figure 2: Risk of bias summary

	KEY: ● High risk of bias ? Unclear + Low risk of bias															
	Akuchekian 2011	Armento 2012	Azhar 1994	Azhar 1995	Barron 2007	Bowland 2012	Ebrahimi 2013	Gibbel 2010	Johnson 1992	Johnson 1994	Pecheur 1984	Propst 1992	Razali 1998	Razali 2002	Rosmarin 2010	Zhang 2002
Random sequence generation	?	?	?	?	?	?	?	?	?	?	?	?	?	?	+	?
Allocation concealment (selection bias)	?	?	?	?	?	?	?	?	?	?	?	?	?	?	?	?
Blinding of participants and personnel	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-	-
Blinding of outcome assessment	-	-	+	+	-	+	+	?	-	?	+	+	?	?	+	?
Incomplete outcome data (attrition bias)	+	+	?	+	?	?	+	-	+	?	+	?	?	?	-	?
Selective reporting (reporting bias)	+	+	+	+	+	-	+	+	+	+	+	+	+	+	+	+
Therapist adherence	?	?	?	?	?	+	?		?	+	?	?	?	?		?
Therapist competence	?	?	?	?	?	?	?		?	?	?	?	?	?		?
Researcher allegiance	-	-	-	-	-	+	-	-	-	-	-	-	-	-	-	-

Note: Therapist adherence and competence biases are not applicable to computer-delivered therapies.

Figure 3: Comparison of F-CBT with control conditions

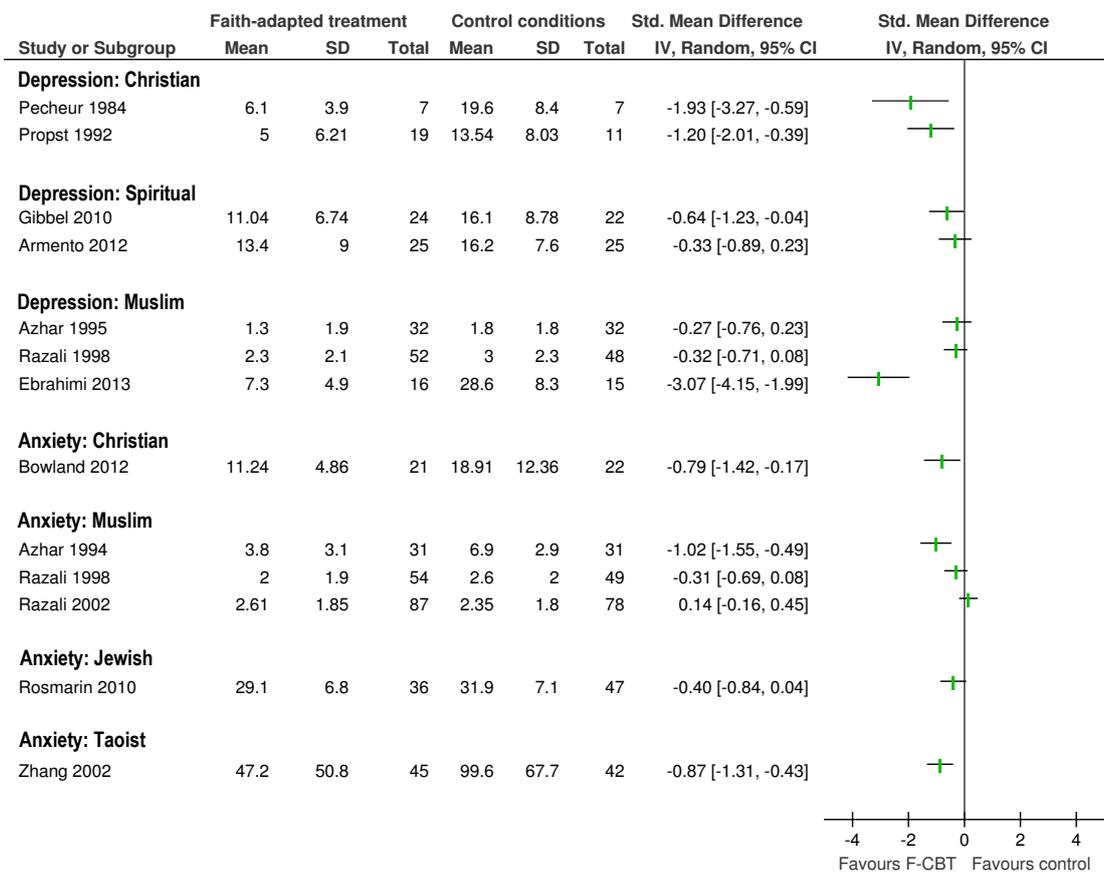


Figure 4: Meta-analysis of depression studies comparing F-CBT with control conditions

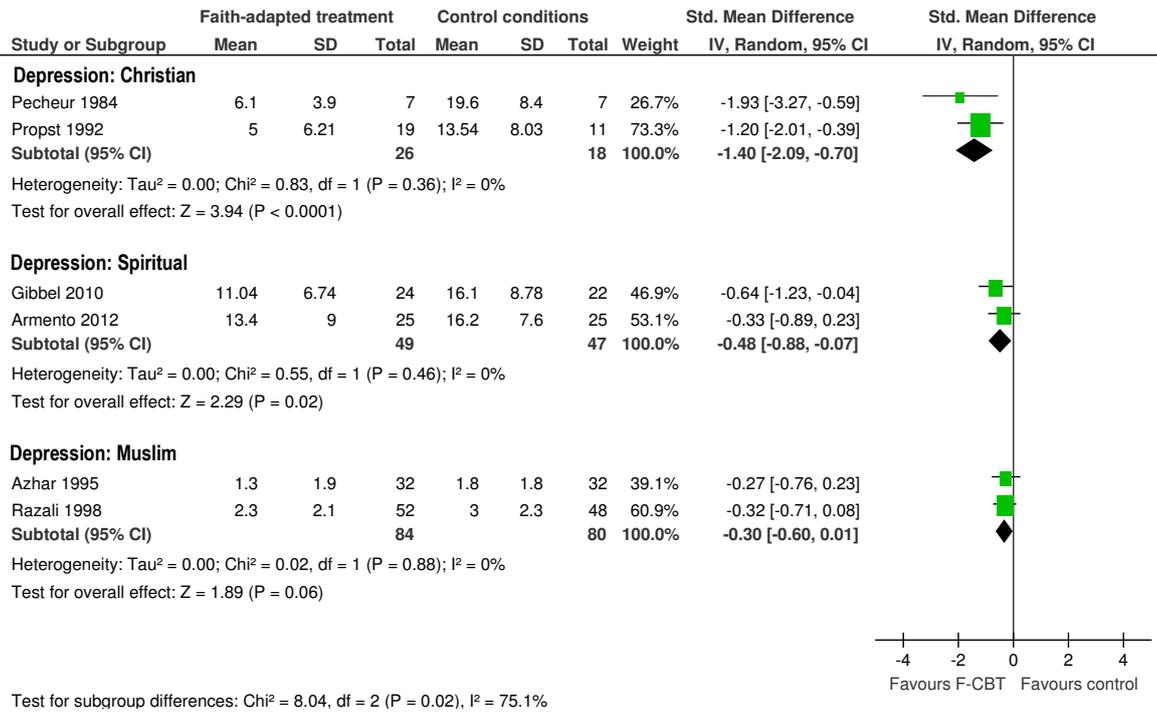


Figure 5: Follow up results comparing F-CBT with control conditions

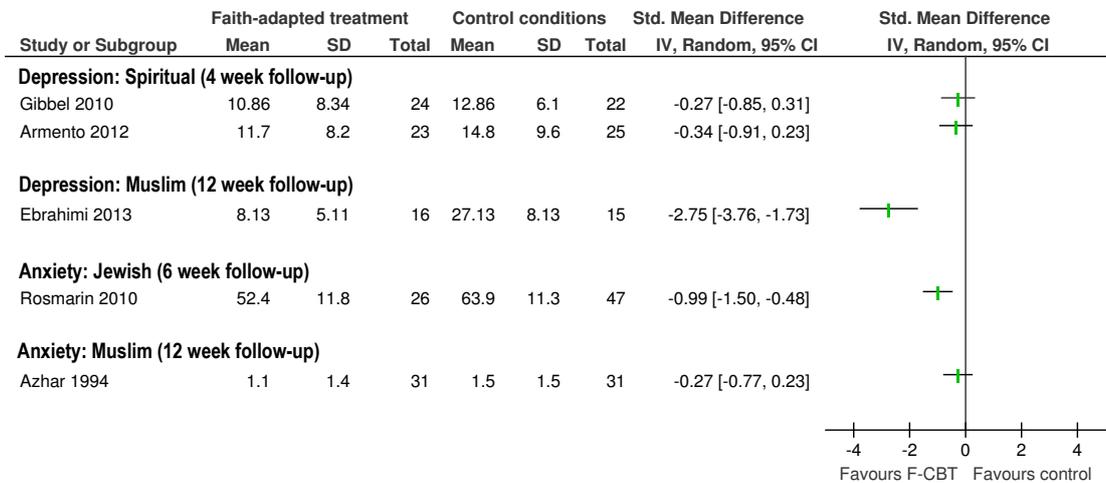


Figure 6: Comparison of F-CBT with standard CBT

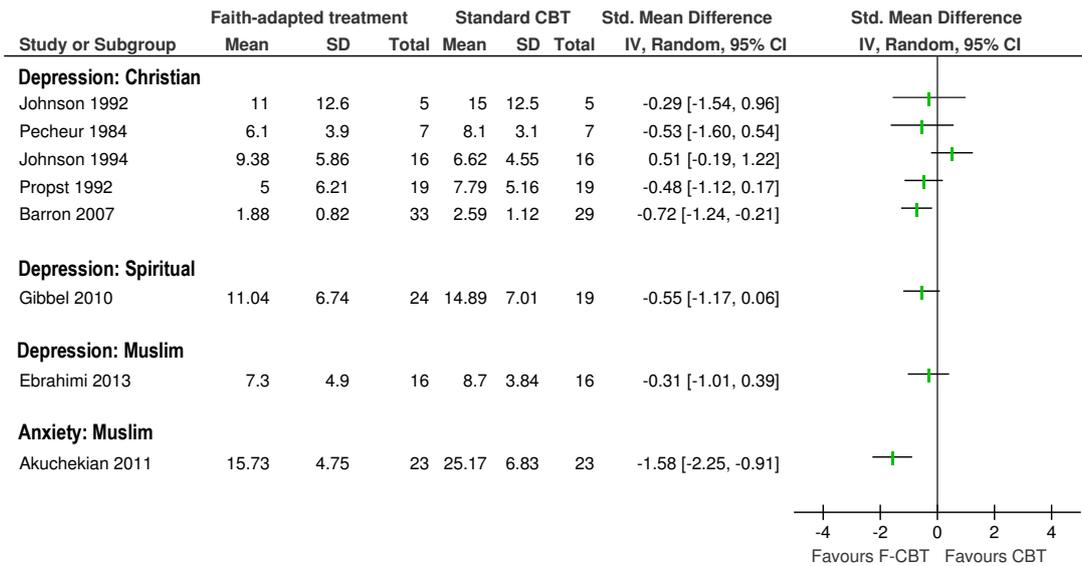


Figure 7: Meta-analysis of depression studies comparing F-CBT with standard CBT

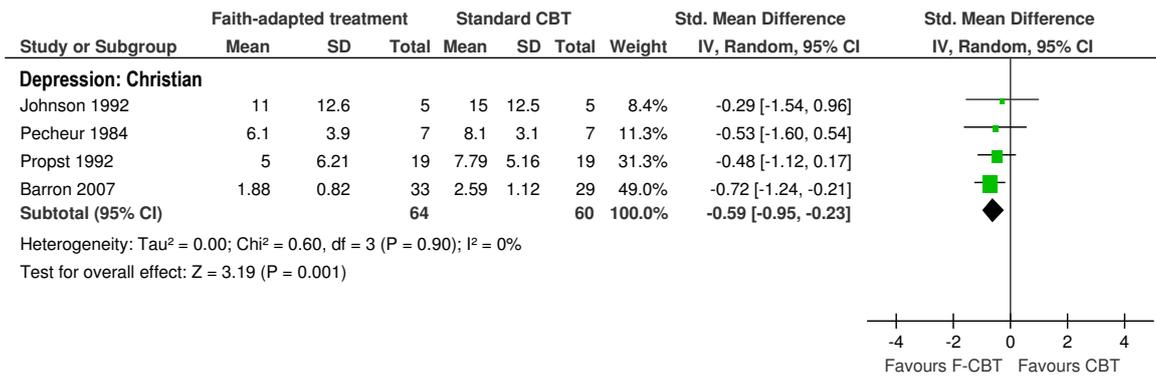
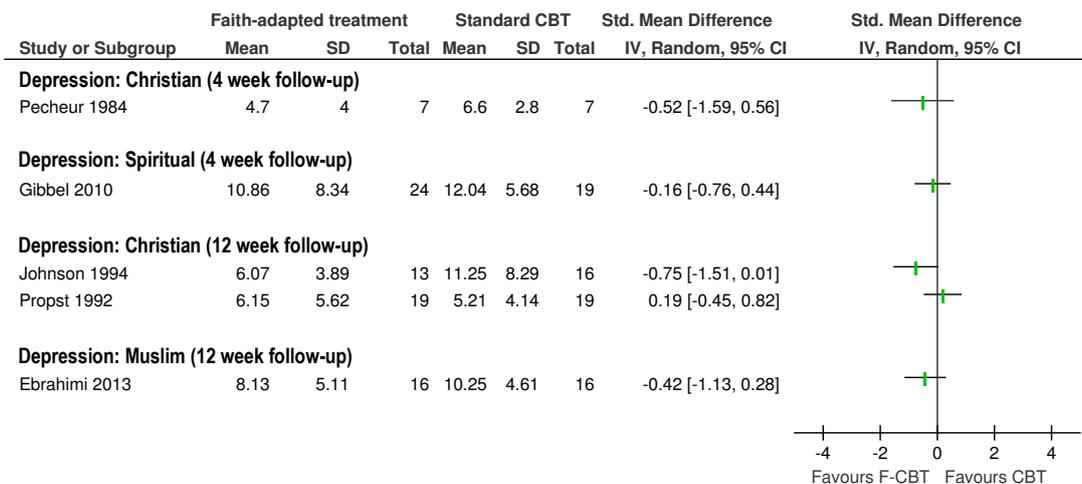


Figure 8: Follow up results comparing F-CBT with standard CBT



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