Psychological therapies for people with intellectual disabilities:
A systematic review and meta-analysis

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Abstract

The aim of this study was to evaluate the efficacy of psychological therapies for people with Received 15 July 2013 intellectual disabilities (IDs) through a systematic review and meta-analysis of the current literature. A comprehensive literature search identified 143 intervention studies. Twenty-two trials were eligible for review, and 14 of these were subsequently included in the meta-analysis. Many studies did not include adequate information about their participants, especially the nature of their IDs; information about masked assessment, and therapy fidelity was also lacking. The meta-analysis yielded an overall moderate between-group effect size, g = .682, while group-based interventions had a moderate but smaller treatment Psychological therapy effect than individual-based interventions. Cognitive-behaviour therapy (CBT) was Cognitive behavioural therapy efficacious for both anger and depression, while interventions aimed at improving Learning disabilities interpersonal functioning were not effectual. When CBT was excluded, there was insufficient evidence regarding the efficacy of other psychological therapies, or psychological therapies intended to treat mental health problems in children and young people with IDs. Adults with IDs and concurrent mental health problems appear to benefit from psychological therapies. However, clinical trials need to make use of improved reporting standards and larger samples.

KEYWORDS intellectual disability, systematic review, meta-analysis, psychological therapy, cognitive behavioural therapy, learning disabilities, neurodevelopmental disorders

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meta-analysis

Disadvantageous life events and genetic vulnerability are believed to increase the risk

of mental health problems amongst people with intellectual disabilities (IDs; Clarke, 2003;

Emerson & Hatton, 2007; Hulbert-Williams & Hastings, 2008). The prevalence of psychiatric

disorders amongst this population is difficult to determine because epidemiological studies

have made use of different diagnostic criteria and assessment methods, but are also based on

different subpopulations. As a consequence, prevalence estimates range between 10% and

39% (Borthwick-Duffy, 1994; Deb, Thomas, & Bright, 2001; Emerson & Hatton, 2007).

Available mental health treatment for people with IDs generally constitutes

pharmacological and behavioural approaches, with a recent trend towards providing more

psychotherapeutic interventions. However, the increasing demand for psychotherapy for

people with IDs has been met with both practical and theoretical concerns. These include the

perceived lack of appropriate training amongst mental health practitioners (Royal College of

Psychiatrists, 2004) and perceived ''therapeutic disdain'' towards this population (Bender,

1993). Theoretical concerns regarding whether people with IDs actually suffer from mental

illnesses, as well as assumptions that IDs is associated with cognitive problems that prevent

engagement in therapy, further constrain the provision of psychotherapy to this population

(Adams & Boyd, 2010; Butz, Bowling, & Bliss, 2000).

The assumption that cognitive problems render therapy ineffective with this

population has been successfully challenged, and there has been an increase in therapy

research with people who have IDs. There is a case study literature demonstrating that

psychotherapy for various types of psychopathology including mood disorders (Fernandez,

Tom, Stadler, Cain, & Knudsen, 2005), anxiety disorders (Arntzen & Almas, 1997; Chiodo &

Maddux, 1985; Hurley, 2004), symptoms of obsessive-compulsive disorder (Klein-Tasman & Albano, 2007), post-traumatic stress disorder (PTSD; Fernando & Medlicott, 2009; Lemmon & Mizes, 2002; Mevissen, Lievegoed, & de Jongh, 2011; Stenfert-Kroese & Thomas, 2006), psychosis (Barrowcliff, 2008; Haddock, Lobban, Hatton, & Carson, 2004), and anorexia nervosa (Cottrell & Crisp, 1984) may be effective. There is also emerging evidence from single group studies as well as controlled clinical trials.

However, considering the literature within this area, there are issues associated with the methodological quality of studies; many studies have a small number of participants and lack comparison groups or randomised allocation (Bhaumik, Gangadharan, Hiremath, & Russell, 2011). Sampling bias is likely due to recruitment through gatekeepers, such as family members, carers, service providers or disability groups. Some gatekeepers may actively try to prevent people with IDs from taking part in research, in an attempt to ''protect'' them, because of unfounded fears and concerns that researchers may not be acting in the best interests of people with IDs. Therefore, recruitment strategies such as these may not adequately capture those not receiving formal services or people without supportive gatekeepers (Becker, Roberts, Morrison, & Silver, 2004). Furthermore, the capacity of people with IDs to give or withhold informed consent is highly relevant within clinical trials. Arscott, Dagnan, and Stenfert-Kroese (1998, 1999) assessed the ability of participants to consent to different treatment options. Their findings indicated that participants had a sufficient understanding of treatment procedures, but found it more difficult to consider the potential risks and benefits of treatment. Similar difficulties were reported regarding the right to withdraw from a study and the understanding of randomisation. The appropriateness of the inclusion of a treatment-asusual (TAU) control group for people who seek help for mental health needs has also been questioned (Oliver et al., 2002).

The scarcity of controlled outcome studies within this area can partially be explained

by a combination of recruitment strategies, concerns about treatment effectiveness, and ethical concerns regarding consent and randomisation. However, considering the marked prevalence of psychiatric disorders amongst people with IDs, there is a clear need for further clinical trials within this area. Others have attempted to undertake both narrative and meta-analytic reviews in order to examine the effectiveness of psychological therapy with people who have IDs who have mental health problems, including forensic mental health problems. For example, in a survey of reviews, Gustafsson et al. (2009) identified 55 reviews of therapy for people with IDs and concurrent mental health problems, and the evaluation of theoretical aspects of psychological therapy provision was sparse. Primary research was considered to lag behind due to the lack of randomised control trials (RCTs); however, it could be argued that evidence from non-RCTs should not be disregarded due to the ethical challenges associated with undertaking psychological research within this area.

Several other reviews have been undertaken in this area. First, Prout and Nowak-Drabik (2003) conducted a comprehensive literature review identifying ninety-two intervention studies of which thirty-five comprised a control group. Half of these controlled trials were published dissertations. Nevertheless, the diversity in psychotherapeutic techniques, and approaches under review, which included accounts of relaxation and social skills training, led to the conclusion that there was a moderate treatment effect. Also, approximately one third of the included studies omitted details regarding the underlying psychotherapeutic theory.

Second, evidence for the treatment of post-traumatic stress disorder (PTSD) in people with IDs was evaluated by Mevissen and De Jongh (2010). They found prevalence rates of PTSD difficult to estimate due to the absence of valid and reliable diagnostic measures, and a symptomatology differing from that in the general population. Only case studies reporting on the successful treatment of PTSD could be identified and it was concluded that currently no

empirically validated treatment is available.

Third, and turning to forensic mental health problems, there have been several attempts to review interventions for people with IDs who have forensic mental health problems, such as criminal offending or anger problems. For example, a systematic review by Ashman and Duggan (2009) aimed to evaluate the efficacy of interventions for sex offenders with IDs but failed to identify published randomised controlled trials. An update of their Cochrane review in 2009 still yielded no results.

Behavioural and cognitive-behavioural interventions to reduce aggressive behaviours were evaluated by Hassiotis and Hall (2008) and found to have some temporary effect. Outcome data were available for three studies but the considerable between-study heterogeneity in the population and outcomes prevented estimations of treatment effect across studies. The inclusion of behavioural modification interventions in this review may be a confounding factor and therefore the results do not provide sufficient support for the efficacy of traditional psychological therapies.

Another review of cognitive-behavioural interventions for anger, by Hamelin, Travis, and Sturmey (2013), presented a large between-group effect size for randomised controlled trials of approximately 1.5. This estimate was based on the analysis of two trials, and the inclusion of studies that were not fully randomised reduced the effect size to 0.9. The lower limit of the 95% confidence interval then dropped from 1.49 to 0.16. However, double counting of evidence occurred in this second analysis, as both the individual and group therapy arms of Rose, O'Brien, and Rose (2009) were included.

The final and most recent review was conducted by Nicoll, Beail, and Saxon (2013). They completed a meta-analysis of cognitive-behavioural interventions for anger yielding large treatment effects for individual and group therapy. Estimates of treatment efficacy were based on uncontrolled effect sizes as studies with uncontrolled designs were included in the

analysis. Their rationale was that the variety of comparison groups across studies would result in increased and potentially problematic heterogeneity in the analyses. Taking the small sample sizes into account the estimated treatment effect is likely to be more conservative.

Some authors have criticised the evaluation of cognitive therapy with people with IDs as being biased due to confounding with behavioural interventions that frequently constitute treatment packages (Sturmey, 2004, 2005). Beail (2005) argued that contrasting the efficacy of behavioural and cognitive-behavioural interventions would be problematic because they have been evaluated with different groups of people with IDs. For example, most ''pure'' behavioural interventions have been evaluated for challenging behaviour (CB) in people with severe to profound IDs, whereas CBT evaluation has focused on people with ''mild-to-moderate'' IDs and mental health problems living in the community. Hurley (2005) and Taylor (2005) further contend that relaxation and assertiveness training require cognitive skills, such as self-monitoring, in addition to the use of behavioural techniques; many other common techniques within CBT are grounded within learning theory (e.g. graded exposure).

The efficacy of behavioural interventions for CB has been well-documented, but predominantly behavioural approaches may not be sufficient to address the mental health problems of people with IDs (King, 2005). The Royal College of Psychiatrists (2004) noted that psychological therapies, whilst employing disorder- or theory-specific psychotherapeutic interventions, should also aim to address the emotional needs of people with IDs. Self-reports of emotional regulation have proven to be a valuable predictor of emotional adjustment, whilst dysfunctional adjustment to a situation may cause behavioural problems (Berking, Orth, Wupperman, Meier, & Caspar, 2008). The prevention and treatment of mental health problems will hence have to address emotion regulation processes, a component of many psychotherapeutic interventions.

As a consequence, considering the problems with some of the previous reviews in this

area, it was considered timely to undertake a meta-analysis of the current literature in order to examine whether or not structured psychological therapy, such as cognitive behaviour therapy, is efficacious when used with people who have IDs. This review therefore has three aims: (a) identify and evaluate controlled outcome studies of psychological therapies with people with IDs, excluding approaches such as applied behavioural analysis, (b) conduct a meta-analyses to determine overall efficacy of treatment, as well as the efficacy of various psychotherapies for different mental health problems where possible, and (c) identify areas with limited available evidence to suggest directions for future research.

METHODS

Study Eligibility Criteria

A systematic search of the literature was conducted to identify all independent group trials examining the efficacy of psychological therapy for people with IDs. Eligible studies were published in English and in a peer-reviewed journal.

The psychotherapeutic approach adopted by studies had to encompass the systematic application of interventions based on well-established psychological principles and techniques aimed at the prevention or treatment of emotional, behavioural or mental health problems (Norcross, 1990, pp. 218–220). Studies which aimed to evaluate treatments targeting behavioural problems and interventions using applied behavioural analysis were excluded. Primary outcomes of interest were measures of intensity and/or frequency of emotional and mental health problems.

Participants within studies should have a diagnosed intellectual disability in accordance to the DSM-IV criteria (American Psychiatric Association, 1994), or ICD-10 (World Health Organisation, 1992). These criteria include an intelligence quotient (IQ)-score below 70 and impairments in social and adaptive functioning, whereby age of onset is before

the age of 18. The full inclusion and exclusion criteria are found in Table 1.

Search Strategy

Studies were identified through systematic searches of PsychINFO, MedLine and CINAHLplus databases in July 2012. The search strategy combined population search terms for IDs with intervention search terms for psychological therapy, as illustrated in Table 1. References of key articles were examined and the ancestry method was used with key journals to identify additional studies. If the authors identified studies that were in press, these were appraised for inclusion

Data Collection

Data collection and extraction was performed by the first author. Studies were reviewed regarding methodology, study quality and reported outcome measures. The data were entered in a database and prepared for meta-analysis.

Quality Appraisal

The use of quality assessment scales in systematic reviews has been both recommended and discouraged. The lack of objectivity in scoring methods makes it difficult to interpret the extent of bias in each study, as well as across the sample of studies. Furthermore, nearly half of the published systematic reviews fail to incorporate the findings of their critical appraisal of methodological quality in the overall interpretation and discussion of intervention effects (Moja et al., 2005). The current review will therefore identify, but not score, the potential sources of bias in each study in the table of study characteristics, hence facilitating the interpretation of the evidence in light of the critical appraisal. Study quality will be reviewed for potential bias in the selection and allocation of participants, blinding during assessment, the process for dealing with incomplete outcome data, attrition of participants and selective reporting. Hence, both study quality and reporting quality were addressed.

Meta-analysis

Studies comparing at least one intervention arm to a control, waiting-list control, or no-treatment control arm were included in the meta-analysis. Studies for which the results are included in a later study were excluded from the metaanalysis to avoid double counting of the evidence (Senn, 2009). For the same reason, data from various intervention arms was pooled when only one control arm was available.

A random-effects meta-analysis was conducted for standardised mean differences of independent groups for outcomes assessed immediately post intervention. The primary outcomes entered in the analysis are printed in bold in Table 2. The random-effects model was preferred because variations in treatment effect are likely to be associated not only with the 'common factors' in therapy, but to differences in study designs and clinical populations. Therefore, the resulting heterogeneity cannot be accounted for by sampling error alone and a fixed-effects model would be unsatisfactory.

Effect sizes for each study were corrected using correction factor J, resulting in Hedges' g (Hedges, 1981) as the estimate of effect size; hence, taking into account the likeliness of small study samples. Study weight was calculated using inverse variance methods to assign greater value to more precise studies with large samples or small variances. The treatment effect was estimated using DerSimonian and Kacker's (2007) two-step approach based on the random-effects model estimate for t2 (DerSimonian & Laird, 1986). This adjusted model is believed to provide a more accurate and conservative estimate of between-study heterogeneity and overall treatment effect.

Subgroup meta-analysis was conducted provided at least two studies fulfilled the requirements for meta-analysis. Planned analyses included random-effects meta-analyses of randomised and non-randomised trials, individual and group therapy, and different clinical characteristics. Positive estimated effects should indicate improved mental health or

reduction of mental health symptomatology. Therefore, the direction of computed effect-sizes of individual studies will be reversed where appropriate. Intention-to-treat analysis was not possible because the majority of studies did not provide sufficient data. The reported analysis is therefore based on participants who completed outcome assessments.

RESULTS

The search strategy identified 259 studies requiring full text review, of which twenty-two met all review eligibility criteria. The review process is illustrated in Fig. 1. The main reasons for excluding studies were because they were singlearmed studies or they lacked intervention outcome data. Table 2 provides an overview of the characteristics of included studies. The data from Willner et al. (2013) and Hassiotis et al. (in press) were obtained from the authors following the publication of their respective study protocols (Hassiotis et al., 2011; Willner et al., 2011).

[Figure 1 about here]

The search results in Fig. 1 illustrate the large quantity of research in this area. However, the majority of these were excluded because they concerned interventions for challenging behaviour or life skills training programmes. Nearly half of the relevant published work concerning psychological therapy with people with IDs comprised descriptive studies, narrative reviews and expert opinion. Single case studies made up nearly 60% of intervention studies, whereas only 15% employed an independent groups design.

Cognitive-behavioural interventions, and group CBT in particular, make up the vast majority of studies. Rose et al. (2009) compared the efficacy of individual and group CBT for anger and therefore this study was classified as making use of both individual and group

therapy (Fig. 1).

Methodological Issues

There was marked variation within the included studies, which varied according to participant information, treatment length, delivery mode, and outcome measures.

Participants. The reported demographic information varied widely between studies. McGaw, Ball, and Clark (2002) and Silvestri (1977) reported an average IQ in the borderline IDs range for their intervention groups, whilst Dowling, Hubert, White, and Hollins (2006) included people with ''severe'' IDs. However, the majority of studies included samples of people with ''mild'' IDs. Only eight studies reported measures of intelligence for the treatment and control group, and one study (Lawrence, 2004) omitted any information regarding level of intellectual functioning.

Recruitment of research participants was mainly based on people being referred for psychotherapeutic interventions, rather than active recruitment by the researchers. These clinical referrals may be associated with the relatively small sample sizes found in most studies, ranging from as little as 14 (Willner, Jones, Tams, & Green, 2002) to 162 (Willner et al., 2013).

Study design. Ethical concerns in psychological therapy research for people with IDs encourage the use of TAU control groups as opposed to a no-treatment control group, whilst some studies opted to deliver two or more independent treatment packages, without a wait-list control group. For example, Benson, Rice, and Miranti (1986) compared the effects of four types of self-control training: relaxation, self-instruction, problem solving or a combined anger management package. The effects of reality therapy group counselling on self-determination were examined by Lawrence (2004), who employed a mutual support group as

the independent comparison group. Finally, Dowling et al. (2006) delivered either an integrated bereavement intervention or traditional counselling to bereaved adults with IDs.

The eligibility criteria for this review required studies to employ an independent groups design. However, within the study by McCabe, McGillivray, and Newton (2006) the groups do not appear to be entirely independent as those allocated to the waiting-list control arm received the intervention six weeks after those allocated to the intervention arm. Hence, it appears that participants who were allocated to the waiting-list control arm, also appeared in the intervention arm, meaning that the data may not be entirely independent. This apparent semi-independence should be aken into account when interpreting results from the meta-analysis. Similarly, Rose, Loftus, Flint, and Carey (2005) reported that some of the participants within the waiting-list control arm may have been included within the intervention arm.

Allocation to the treatment or control groups was mostly randomised based on setting, sex, date of referral, intensity of the mental health problem, or geographic location, to create balanced study arms. However, allocation procedures in Rose, Dodd, and Rose (2008) and Rose et al. (2009) were based on the availability of a group treatment starting within two months upon referral, or the availability of a therapist for individual therapy; when this was not possible, participants were allocated to a waiting-list control group. Similarly, McGaw et al. (2002) did not randomise participants, but rather allocated them to the intervention arm on a first-come first-serve basis.

There were issues associated with the lack of blinding across studies, with only five studies reporting that they attempted to blind the researchers who were responsible for measuring outcome (Benson et al., 1986; Hassiotis et al., in press; Lindsay et al., 2004; Matson & Senatore, 1981; Willner et al., 2013). Six studies reported the use of independent

raters where masked assessment could not be guaranteed. Nearly half of the studies either did not employ blinding procedures or did not provide details regarding masked assessment.

Treatment mode. The majority of studies evaluated group-based interventions, and the majority of individually delivered treatments were conducted by the same authors (e.g. Rose et al., 2008, 2009; Taylor, Novaco, Gillmer, Robertson, & Thorne, 2005; Taylor, Novaco, Gillmer, & Thorne, 2002; Taylor, Novaco, Guinan, & Street, 2004). Treatment was delivered by clinical psychologists, or by staff who were given training to deliver the treatment. Staff and carers who served as lay therapists generally received a two day training and were supervised by a clinical psychologist (Dowling et al., 2006; McGillivray, McCabe, & Kershaw, 2008; Willner et al., 2013; Willner, Brace, & Phillips, 2005).

Substantial variations were found in treatment length and time of follow-up. Fourteen studies conducted follow-up measurements within three to six months post treatment, whereas four studies did not collect any follow-up data. Lindsay et al. (2004) conducted the longest intervention with approximately forty group sessions of anger treatment over nine months which included up to thirty months of follow-up data for some participants.

Treatment integrity was likely to be best in anger management trials because of the use of treatment manuals and associated methods for monitoring treatment delivery. Treatment fidelity was assessed by Willner et al. (2013) and Hassiotis et al. (In Press) only and indicated that both lay-therapists and practicing therapists showed moderate to high levels of adherence to the respective treatment manuals.

Treatment outcomes. Outcome measures of anger treatments typically included the Novaco Anger Scale, Anger Inventory, and the Provocation Index. All trials providing psychological therapy for depression used either the Beck Depression Inventory-II or the Beck Depression Inventory-Youth to assess clinical symptoms of depression. In addition to outcome scales, studies employed idiographic measures such as participant behaviour in role-

plays and direct behavioural observations by therapist or staff, which at times made comparisons across studies problematic. Reasons for attrition were not consistently reported, whilst one study by Rose et al. (2008) recorded that no participants dropped out. The majority of studies reviewed did not undertake an analysis of intervention data based on intention-to-treat.

Meta-Analyses

An initial meta-analysis was conducted for controlled trials with either a no-treatment or a waiting-list control group, employing cluster, matched or full randomisation procedures. Additional eligibility criteria were applied to exclude studies if data were included in a later study (Rose, West, & Clifford, 2000; Taylor et al., 2002), or if insufficient data were reported to perform the meta-analysis (Matson, 1981; Willner et al., 2005). Finally, the study by Silvestri (1977) was excluded because twenty-three out of thirty items of its primary outcome measure had been excluded from the original data-analysis. The selective reporting of outcomes in this study, if included, would have led to confounding results.

The inclusion of Rose et al. (2009) is based on a comparison of the combined interventions arms, individual and group therapy, versus the control group. This approach is recommended to avoid double counting the evidence of the comparison group, and is preferred over selecting a single intervention arm for data-analysis as this might result in a loss of information or biased data-selection (Senn, 2009). The combination of data within the two intervention groups followed the recommendations of the Cochrane Handbook (Higgins, Deeks, & Altman, 2011).

The Beck Depression Inventory-Youth data was included as the outcome data from the study by Hassiotis et al. (in press). This study concerned the treatment of both depression and anxiety with one manualised intervention. However, it could be argued that anxiety and depression may have different clinical formulations, and as a consequence, require different

interventions. The data pertaining to outcomes for depression were included as this increased the data available to evaluate the cognitive-behavioural treatment of depression, from two to three trials.

Randomised versus non-randomised studies. The meta-analysis of randomised trials yielded an average treatment effect of, $g = .555$, 95% CI [.178, .932], N = 388, which is regarded as a moderate treatment effect (Cohen, 1988). The analysis highlighted a substantial amount of heterogeneity with 62%, $p < .05$, of the variability in estimated treatment effect not explained by sampling error alone. The meta-analysis of non-randomised studies revealed an average large treatment effect, $g = .846$, 95% CI [.355, 1.337], N = 275, while the heterogeneity increased to 69%, $p < .01$. Combining randomised and non-randomised trials revealed a moderate treatment effect, $g = .682$, 95% CI [.379, .985], N = 663, and the heterogeneity was 67%, $p < .001$. The forest plots in Fig. 2 included studies with their standardised mean differences and corresponding confidence intervals, as well as the estimated treatment effect and corresponding confidence interval for both the subgroup analysis and the overall meta-analysis. When adopting the two-step DerSimonian and Laird method (DerSimonian & Kacker, 2007) across all studies the treatment effect increased to $g = .700$, 95% CI [.386, 1.015], N = 663. The adjusted $t2$ measure of heterogeneity also increased from $t2 = .207$ to $t2 = .249$.

Leave-one-out analysis for the eight randomised studies highlighted the impact of the McCabe et al. (2006) depression trial. Exclusion of this study resulted in a small estimated treatment effect of, $g = .386$, 95% CI [.116, .656], N = 339. However, the estimated average effect increased to $g = .647$, 95% CI [.262, 1.031], N = 367, and to $g = .636$, 95% CI [.228, 1.044], N = 358, when excluding the study on interpersonal functioning by Matson and Senatore (1981) and the small-scale RCT by Hassiotis et al. (in press), respectively.

[Figure 2 about here]

Individual versus group-based psychological therapy. Subgroup meta-analysis of

combined randomised and non-randomised trials indicated individually delivered therapy, g =

.778, 95% CI [.110, 1.445], N = 124, was more effective than group-based therapy, g = .558,

95% CI [.212, .903], N = 477, as illustrated in the forest plot in Fig. 3. It should be noted,

however, that there were fewer trials involving individual therapy than group therapy

available for the analysis. Furthermore, the large variability in the effectiveness of individual

therapy is likely to be associated with differences in clinical diagnosis and primary outcome

measures, as well as the large within-study variance of Taylor et al. (2004). Rose et al. (2009)

was not included in the analysis to avoid double counting of the control group. The shared

control arm for both intervention arms in this study would have led to correlated multiple

comparisons that cannot be accounted for in the meta-analysis (Higgins et al., 2011).

Studies making use of individual therapy had a moderate to large effect size, while

group-based therapy, regardless of clinical disorder, had a moderate effect. Within the group-

based studies the average treatment effect and heterogeneity are negatively affected by

McGaw et al. (2002), Matson and Senatore (1981) and Willner et al. (2013). It should be

noted, however, that McGaw et al. (2002) provided group interventions to support parents

with IDs in the forming and maintaining of relationships, and to improve their self-concept.

Likewise, Matson and Senatore (1981) delivered group therapy to improve interpersonal

functioning. The latter two studies are therefore quite distinct from the other group

interventions which aimed to treat mental health problems.

[Figure 3 about here]

Clinical presentation. The variability in study samples supported a subgroup meta-analysis based on clinical presentation. These were completed for anger, interpersonal functioning and depression. The forest plots in Fig. 4 show that the average treatment effect ranges from a null-effect for interpersonal functioning to a large treatment effect for people with IDs suffering from depression.

CBT for anger and aggression had an average estimated effect size of $g = .827$, 95% CI [.508, 1.146], N = 494. The inclusion of some studies with relatively large samples resulted in a narrow confidence interval, although there is considerable betweenstudy variance, and individual and group therapy were combined (Fig. 4).

Psychological therapy for interpersonal functioning was not supported by the analysis of data from Matson and Senatore (1981) and McGaw et al. (2002). Results are inconsistent from these studies and hence do not provide sufficient evidence that treatment is efficacious, as evidenced by the negative effect of $g = 0.342$, 95% CI [.946, .262], N = 43. However, participants in the intervention arm of both studies did show improvements from pre-test to follow-up and from post-test to follow-up, indicating that treatment effects might take longer to establish for these therapies.

Turning to depression, studies evaluating CBT generated a moderate to large effect size, $g = .742$, 95% CI [.116, 1.599, N = 126. The between-study variance is high, as only three studies with distinct study designs were identified. The feasibility study of Hassiotis et al. (in press) caused methodological concerns due to its use of a single therapy for two separate clinical disorders. McGillivray et al. (2008) employed a staff-administered treatment programme, but there were no attempts to investigate whether or not the inclusion of staff within such an intervention increased efficacy.

[Figure 4 about here]

DISCUSSION

The results of the meta-analysis indicated that psychological therapy with people who

have IDs is efficacious with a moderate effect size of g = .682 when calculated using all the

studies included within the current review. However, this effect size varied depending on

whether the studies made use of randomisation, individual or group based interventions, and

also varied according to the type of problem being treated. Randomised studies were

associated with a lower, but moderate effect size, g = .555, compared to non-randomised

studies which had a large effect size, g = .846. Individual therapy, g = .778, appeared superior

to group-based interventions, g = .558; treatment for depression, g = .742, and anger, g =

.827, was associated with moderate and large effect sizes, while there was no evidence that

therapy had an effect on interpersonal functioning, g = .342.

There are some similarities and differences between the current analysis and some of

the previous systematic reviews that have also attempted to synthesise the evidence for the

efficacy of psychological therapies for people with IDs. Nontraditional psychotherapeutic

interventions, such as relaxation or social skills training, were included in the analysis by

Prout and Nowak-Drabik (2003), but were excluded from the current study. By contrast,

staff-delivered treatments, excluded in that review, were included in the current analysis

because staff had received training and acted as lay therapists. All but one study in the

present meta-analysis had been published in the last decade, whilst the previous meta-

analysis conducted by Prout and Nowak-Drabik (2003) mainly comprised research published

in the 1980s.

The subgroup meta-analysis for anger problems is comparable to the anger-specific

reviews of Hamelin et al. (2013), Nicoll et al. (2013) and Hassiotis and Hall (2008). The

estimated treatment effect of g = 0.827, presented in Fig. 4, is slightly lower than the

estimated 0.88 reported by Nicoll et al. (2013), and the un-weighted estimate of 0.89

presented by Hamelin et al. (2013), whereas Hassiotis and Hall (2004) did not perform such

an analysis. However, there were some differences in how the effect size was calculated

across these different studies; Hamelin et al. (2013) calculated standardised mean differences

of pre-post changes in each arms to estimate the treatment effect, while Nicoll et al. (2013)

relied on a fixedeffects analysis of uncontrolled mean differences calculated within the

intervention arm. Nevertheless, the findings of these two reviews and the current study are

remarkably comparable, despite the different inclusion criteria and methodology.

It is also worth noting that the methodological quality of psychological therapy

research for children and adolescents with IDs is much lower than that involving adults, as no

independent groups designs or RCTs were identified, bearing in mind that there are some

RCTs involving children with autistic spectrum disorders (Sofronoff, Attwood, & Hinton,

2005; Wood et al., 2009). Ethical concerns in the recruitment of young people with IDs for

intervention studies may partially explain the current lack of research, but should not be seen

as justification for the lack of controlled outcome studies. It is unclear whether evidence from

psychotherapy research with adults with IDs, or young people without IDs, can be adequately

generalised to this young population. Related to this, the search results indicated the

proportion of single case studies involving adults has steadily increased over time and

provide evidence for the trend towards more controlled psychotherapy research for adults

with IDs.

There were no studies of psychodynamic therapy identified that fulfilled the eligibility

criteria for inclusion within the current study. Expanding the inclusion criteria to single-

armed pre–post studies revealed few psychodynamic intervention studies. Their analysis falls

outside the scope of this review, but the apparent lack of well-conducted primary research in

this area does not seem to support psychodynamic therapy as an empirically supported

treatment for people with IDs.

Following the guidelines for empirically validated treatments, at least two well-conducted group design studies, conducted by different research groups, should be identified (Chambless et al., 1998). Although the search results did not yield two studies with a sufficiently large sample size, which also reported both detailed participant characteristics, as well as information regarding the treatment protocol, the criteria for probably efficacious treatments appear to be met for cognitive-behavioural interventions for both anger and depression. As a consequence, both individual and group psychotherapy are likely to be efficacious treatments for mental health problems in adults with IDs, but further studies are still needed.

However, there is variability in the methodological quality of the studies included within this review. For example, the inclusion of participants with varying levels of intellectual functioning, ranging from borderline to severe, and the absence of reported measures of general intellectual functioning make it difficult to compare study samples and lead to an increase in heterogeneity. General intellectual functioning varied widely between studies, but more than half of the studies reported including samples with a mean IQ above 65, indicating that people with ''borderline'' IDs were frequently included. Varying levels of intellectual functioning may affect the outcomes from cognitive therapy, at least theoretically, but the existing literature does not provide sufficient data to include general intellectual functioning as a covariate in the analysis. Similarly, therapy setting and treatment intensity are important factors that were not controlled in this review or any previous reviews. When providing psychological therapy to people with IDs, many may attempt to make changes to the intervention in an attempt to improve efficacy, and it remains unclear whether or not these changes are genuinely associated with improvements in treatment outcome. As a consequence, the results presented within this study do not account for differences in assessment or treatment, but rather yield a general indication of psychological therapy

efficacy including both its common and specific factors (Kazdin, 1985).

These problems can be translated into four recommendations which should be considered by researchers undertaking clinical trials of psychological therapies with people who have IDs. First, researchers should measure and report the general level of intellectual functioning of their participants within publications. This will allow for a greater understanding of the participant sample and help to reduce heterogeneity across studies. Second, researchers need to describe their methods and their interventions thoroughly, and third, changes to psychological therapies, which are made in order to improve engagement, understanding, and outcomes for this population should be described. There is a literature that has attempted to elucidate some of these issues (Bruce, Collins, Langdon, Powlitch, & Reynolds, 2010; Dagnan & Chadwick, 1997; Dagnan, Chadwick, & Proudlove, 2000; Hatton, 2002; Joyce, Globe, & Moody, 2006; Sams, Collins, & Reynolds, 2006; Stenfert-Kroese, Dagnan, & Loumidis, 1997), but further research is needed, not only to generate further evidence for the effectiveness of psychological therapies, but for the effectiveness of any adaptations and changes that are made to psychological therapies for people with IDs. However, although it may be possible to broadly examine the utility of adaptations to CBT for people with IDs, psychological therapies, including CBT, are formulation-driven. Considering the heterogeneity within the population of people with IDs, individually tailored formulations will reflect this heterogeneity in presentation and ability, and as a consequence, any adaptations should be tailored to this formulation in order to meet individual need. As a consequence, it would be appropriate to consider these issues within any future trial involving participants with IDs. Fourth and finally, it is clear that further robust and well-designed clinical trials are needed which involve a range of mental health problems, involving not only adults with IDs, but also children and adolescents with IDs.

CONCLUSIONS

The current meta-analysis evaluated the available evidence and indicated that psychological therapy has a moderate effect in treating symptoms of mental health problems amongst people with IDs. This effect is biased by studies where allocation was not randomised. The results further suggest CBT to be at least moderately effective in the treatment of anger and depression. Individual therapy may be more effective than group psychotherapy, but this conclusion must remain tentative until further research is completed. As more controlled psychotherapy research continues with people with IDs, it is expected that sufficient evidence will be available in the future to determine whether various psychological therapies can actually be regarded as empirically validated treatments.

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Table 1.

Systematic Review Search Strategy and Eligibility Criteria.

Search Terms in Title and Abstract
mental* N2 (handicap* OR retard* OR disab* OR impair*)
(learning OR intellect* OR develop*) N2 (difficult* OR disab* OR impair*)
imbecile OR subnormal
psycho* N2 (therap* OR treatment* OR intervention*)
training OR management OR counsel*
psychotherap*
(1 OR 2 OR 3) AND (4 OR 5 OR 6)

Inclusion criteria
Intellectual disability: IQ < 70
Age > 5 years of age (targeting 'talking' therapies and interventions)
Psychotherapy: the systematic application of interventions based on well-established psychological principles and techniques aimed at the prevention or treatment of emotional, behavioural or mental health problems (Norcross, 1990, p. 218-220), excluding interventions primarily using applied behavioural analysis
Intervention studies: 2 or more independent groups
Published in English in peer-reviewed journals

Exclusion criteria
'Strict' behavioural interventions, unless embedded in wider psychotherapeutic treatment. For example: applied behavioural analysis, behaviour modification, behavioural relaxation only, restraint, differential reinforcement of other behaviour, and token economy.
Problem behaviours and challenging behaviour. For example: drooling, sleeping problems, and self-injurious behaviour.
Non-traditional and other psychotherapeutic interventions. For example: life skills training, vocational rehabilitation, electro-convulsion therapy, biofeedback training, occupational therapy, play therapy, milieu therapy, pharmacotherapy, community management.
Intervention based on well-established psychological principles aimed at teaching or improving behavioural patterns. For example: social skills training and assertiveness training.

Table 2.

Independent Group Trials Evaluating Psychological Therapies for People with IDs.

First author (Year)	Study design	Participants	Intervention	Outcome	Follow-up	Quality Appraisal - Sources of Bias
Benson (1986)[‡]	4 independent groups EG1: combined anger-management (EG2 + EG3 + EG4) EG2: problem solving group EG3: self-instruction group EG4: relaxation training	N = 54 Mean age = 32 37 M / 17 F IDs: only data for receptive vocabulary Anger control difficulties Country: USA	All EGs: 12 weekly 90-min sessions EG1: EG2 + EG3 + EG4 EG2: four-step plan to anger solving, role-plays EG3: discriminating coping and trouble statements, role-plays EG4: relaxation based on Jacobson tension release Group size: 5 – 9 Setting: vocational centre	All EGs: reduced aggressive gestures, reduced length of responses, and more appropriate responding as assessed by Self-report anger inventory (AI), Conflict Situations Test, videotaped role-play and supervisor ratings. No significant between-group differences.	4-5 weeks Effects maintained, except for aggressive gestures.	Selection: groups balanced on verbal ability, anger inventory score, gender, race and vocational training centre. Performance & detection: independent and masked raters. Attrition*:* 68 % of approached participants consented. Other: treatment fidelity not assessed; no control arm.
Dowling (2006)[‡]	2 independent groups EG1: integrated interventions by carers EG2: traditional counselling by bereavement counsellors	EG1: N = 11 EG2: N = 23 Age = +18 IDs: mild – moderate – severe Bereaved adults Country: UK	EG1: Integrated support by family carer and day centre staff using bereavement-oriented activities, and discouraging continued grief at day centre. EG2: approximately 15 weekly or fortnightly 1-hour sessions with volunteer. Setting: at home or day centre.	EG2 improved more than EG1regarding display of aberrant behaviour (Aberrant Behaviour Checklist – Community, and Health of Nation Outcome Scales for People with Learning Disabilities), as recorded by staff informants.	No follow-up conducted	Selection: cluster and individual randomisation, allocation sequence human generated and concealed. Performance & detection: no blinding. Attrition: 8% and 63% completion rate for consenting participants in EG1 and EG2 respectively, analysis on 'intention-to-treat' Other: 2-day training and supervision available to lay therapists; high withdrawal rate by carers in high-demand EG1; treatment fidelity not assessed; no control arm.
Hagiliassis et al (2005)	2 independent groups EG: cognitive-behavioural anger management CG: waiting-list, treatment as usual	EG: N = 14, mean age 45 CG: N = 15, mean age 44 IDs: none or borderline (8), mild (2), moderate (8), severe (11) Anger control difficulties	EG: 12-weekly 2-hour individual anger management training sessions, including physiological and cognitive components, based on Novaco's theory of anger (1975).	**Novaco Anger Scale**: significant group x time interaction, anger control improved for EG only. Outcome Rating Scale: no main or interaction effects, but slightly better outcomes for EG.	4 months Improved anger control for EG maintained, no change for CG.	Selection: randomisation stratified by region and gender, concealed allocation. Performance & detection: no blinding, but assessment by independent researcher. Attrition: 85% of referred participants were offered and completed treatment.

			Country: Australia			Other: treatment manual referenced, treatment fidelity not assessed.
Hassiotis et al (In Press)	2 independent groups EG: individual cognitive-behavioural treatment for depression and anxiety + treatment as usual CG: treatment as usual	EG: N = 16, mean age 34, 5 M / 11F CG: N = 16, mean age 38, 7 M / 9 F IDs: mild (30), moderate (2) Anxiety and/or depression Country: UK	EG: 16 weekly 1-hour sessions of manualised individual cognitive-behavioural therapy for anxiety and depression Setting: IDs service	**Beck Depression Inventory – Youth**, Beck Anxiety Inventory – Youth: no treatment effect, slight non-significant improvement for CG, not for EG. EG showed positive change only for participants with depression, but without anxiety.	6 months EG and CG improved slightly, yet non-significant, and CG fared better than EG.	Selection: permuted block randomisation, concealed allocation. Performance & detection: assessment by masked researchers. Attrition: 48 referrals, 32 entered of which 27 completed. Data-analysis based on N = 15 in EG and CG. Other: secondary outcome to assess quality of life inadequate for use with people with IDs; study protocol published, treatment manual available; treatment fidelity recorded as high.
Lawrence (2004)[‡]	2 independent groups EG1: reality therapy group EG2: mutual support group	EG1: N = 16, mean age 40 EG2: N = 14, mean age 46 M/F: equal between groups IDs: no data presented Country: USA	Six-weekly 1-hour group sessions Group size: max. 8 Setting: vocational service	Arc's Self-Determination Scale: Improved self-determination self-regulation, and self-realisation for EG1 compared to EG2, but no effects on autonomy and psychological empowerment.	No follow-up conducted	Selection: randomised allocation, but not concealed. Performance & detection: no blinding. Attrition: 6% drop-out rate. Other: clear description of treatment plan and session contents; treatment fidelity not assessed; no control arm.
Lindsay (2004)	2 independent groups EG: Group CB anger management CG: 6-month waiting-list	EG: N = 33, mean age 28, 75 % M CG: N = 14, mean age 24, 57 % M IDs: EG mean IQ: 65 CG mean IQ: 66 Anger control difficulties Country: UK	EG: 40 group sessions, 40-60 minutes. Includes behavioural relaxation, stress inoculation, group discussions about anger responses, and role-plays. Group size:	**Dundee Provocation Inventory**: reduced anger response for EG, but not for CG Anger provoking role-plays: reduction in anger responses (only data reported for EG, N = 21) Daily reports of anger: reduced feelings of anger in self-reports of EG, but not CG	3 months, sometimes also at 9, 15, 21 or 30 months. Further reduction on all outcome measures at 3 months. Then stabilizes at post-test or 3 month follow-up level.	Selection: referrals-based randomisation. Recruitment/referral over +10 year period. Performance & detection: masked raters for role-plays. Attrition: attrition acknowledged but rates not reported, anger-provoking role-plays and anger reports missing for some participants. Reporting: no information on group size. Other: no baseline scores CG for anger-provoking role plays as considered inappropriate by authors; treatment fidelity not assessed.

Matson (1981)[#]	2 independent groups EG: group intervention of participant modelling for fear CG: no-treatment, waiting-list control	N= 24 Age: not reported 50 % M IDs: mild to moderate Phobia Country: USA	EG: 3-weekly 1-hr group sessions over 3 months. Training based on behaviour modelling and in vivo sessions. Group size: 5 Setting: mental health service, sheltered workshop	Fear, as measured by approach behaviour, substantially decreased and number of adaptive verbal and non-verbal shopping skills performed improved for EG. Less phobic avoidance registered by staff for EG.	4 months No follow-up specific data reported.	Selection: matched pairs: degree of fear, sex. Performance & detection: 2 independent but not masked raters. Attrition: not reported. Reporting: only results of ANCOVAs presented, no group means and standard deviations. No data on age, or level of ID. Other: Raters received training. 96% inter-rater agreement. Treatment plan detailed, but treatment fidelity not assessed.
Matson et al (1981)	2 independent groups EG1: traditional group psychotherapy EG2: group social skills training CG: no treatment	EG1: N = 11, EG2: N = 11, CG: N = 10 Age: mean 34, range 28-49 21 M / 11 F IDs: mild to moderate Socially inadequate behaviour Country: USA	EG1: twice weekly 1-hr sessions discussing empathy, respect, concreteness and genuineness. EG2: twice weekly 1-hr sessions with direct teaching of 3 target behaviours, role-play and modelling. Group size: 3-5 Setting: workshop	Behaviour in role-plays and during group meetings: significant improvements for EG2, only role-plays improved for EG1. **Nurses' Observation Scale for Inpatient Evaluation – 30**: significant improvements for EG2. No changes on Social Performance Survey Schedule.	3 months. Behavioural improvements for EG2, although lower than posttest. EG1: only improved for role-plays	Selection: randomising triads matched on pretest skills. Performance & detection: masked raters. Attrition: 35 consented; insufficient outcome data for one person in each group (reasons specified). Other: Raters received training to reach 90% inter-rater agreement. Group attendance rates reported; treatment fidelity not assessed.
McCabe (2006)	2 quasi-independent groups EG: cognitive-behavioural group intervention CG: no-treatment control	EG: N = 19, mean age 34, 10 M / 9 F CG: N = 15, mean age 40, 6 M / 9 F IDs: mild to moderate Depression Country: Australia	EG: 5 weekly 2-hr sessions. Session contents cover social support, activity setting, core beliefs, negative thoughts, problem solving and setting future goals. Group size: 3-5 Setting: workplace	**Beck Depression Inventory – II**, Social Comparison Scale, and Automatic Thoughts Questionnaire: significant improvements for EG on all measures (for N=34). Rosenberg Self-esteem Scale: no significant change noted.	3 months (for N=18) Gains maintained at follow-up but no further improvement	Selection: participants randomised Performance & detection: no blinding. Attrition: 1 person lost to follow-up, reason not specified. Other: data of 15 CG participants who completed intervention after 3-month follow-up included in analysis. For N=15 these participants acted as own control. Session outlines reported, but treatment fidelity not assessed
McGaw (2002)	2 independent groups	EG: N = 12, mean age 29, 3 M / 9 F CG: N = 10, mean age 30,	EG: 14 weekly 2-hr sessions. home-based teaching program + group	**Judson rating scale (self-concept subscale).**	13 weeks	Selection: Not randomised, allocation on first-come, first-serve basis.

	EG: cognitive-behavioural group intervention CG: control parent group	4 M / 6 F IDs: borderline or mild. EG mean IQ 73 CG mean IQ 72 Parents with IDs Country: UK	intervention to improve relationships and self-concept of parents with ID. CG: home-based teaching intervention Group size:	Behaviour problem index, Malaise Inventory. No improvement for EG or CG on parental relationships and parental self-concept.	Improved self-concept from posttest to follow-up for EG. Social Changes Questionnaire showed increased parental support for EG.	Performance & detection: no reports of blinding procedures. Attrition: not reported. Other: no information on session content or treatment fidelity.
McGillivray (2008)	2 independent groups EG: staff-administered group CBT CG: waiting-list	EG: N = 20, mean age 38, 13 M / 7 F CG: N = 27, mean age 31, 19 M / 8 F IDs: mild, IQ range 50-70 Depression Country: Australia	EG: 12 weekly 2-hour sessions. Programme based on 'Think happy, feel happy, be happy'. Group size: 5-6	**Beck depression inventory-II**; Automatic thoughts questionnaire – Revised; Social readjustment rating scale; Social comparison scale. Decrease in depressive symptoms and automatic thoughts for EG	3 months Gains maintained at follow-up.	Selection: cluster randomisation of 2 vocational agencies. Performance & detection: staff naïve to design, but not masked during delivery; assessment by independent research assistant. Attrition: 2 people removed from analysis due to illness. no follow-up data for further 2 people from CG who continued to receive treatment. Other: 2-day training for staff to act as lay-therapists. Session content outlined, but treatment fidelity not assessed.
Rose (2000) [#]	Data included in Rose 2005.					
Rose (2005)	2 quasi-independent groups EG: group cognitive-behavioural interventions for anger CG: waiting-list, treatment as usual	EG: N = 50, mean age 39, 40 M / 10 F CG: N = 36, mean age 35, 31 M / 5 F IDs: only data for receptive vocabulary Anger control difficulties Country: UK	EG: 16 weekly 2-hour group sessions to reduce aggressive behaviour. Group size: not reported	**Anger inventory**: lower expressed anger for EG, and increased expressed anger for CG. Post-hoc: presence of staff and receptive vocabulary associated with better treatment outcomes.	3 – 6 months Gains of EG maintained. No follow-up data for CG.	Selection: no randomisation or concealed allocation, allocation based on availability of treatment. Performance & detection: no reports of blinding procedures. Attrition: 11 out of 61 recruited participants dropped-out of EG1. Other: includes data from Rose (1999) and Rose (2000); data for CG N = 11 included in data-analysis for EG; assessed clinical relevance of outcomes; treatment content referenced and reported, but treatment fidelity not assessed.

Study	Design	Participants	Intervention	Outcome measures/results	Follow-up	Methodological quality
Rose (2008)	2 independent groups EG: Individual cognitive-behavioural intervention for anger CG: waiting-list	EG: N = 20, mean age 37 13 M / 7 F CG: N = 21, mean age 37 16 M / 5 F IDs: mild – borderline Aggressive behaviour UK study	EG: 14-18 individual sessions of 30-60 minutes of cognitive behavioural interventions to reduce aggressive behaviour.	Adapted **Anger Inventory**: EG showed significant decrease in self-reports of anger intensity. Decrease more pronounced for people with higher anger intensity at baseline.	3 – 6 months. Decreased anger intensity maintained for EG.	Selection: no randomisation or concealed allocation, allocation based on availability of treatment. Performance & detection: no reports of blinding procedures. Attrition: no drop-outs occurred. Other: assessed clinical relevance of outcomes; brief outline of sessions presented, but treatment fidelity not assessed.
Rose (2009)	3 independent groups EG1: individual cognitive-behavioural intervention for anger EG2: group cognitive-behavioural intervention for anger CG: waiting-list, treatment as usual	EG1: N = 18, 12 M / 6 F EG2: N = 23, 14 M / 9 F CG: N = 21, 16 M / 5 F IDs: only data for receptive vocabulary Aggressive behaviour Country: UK	EG1: 14-18 individual sessions of 30-60 minutes of cognitive behavioural interventions to reduce aggressive behaviour EG2: 16 weekly 2-hour sessions of cognitive behavioural interventions to reduce aggressive behaviour. 3 groups. Group size:	**Anger Provocation Inventory**: EG1 & EG2 showed significant reductions; no difference in efficacy of EG1 and EG2.	No follow up conducted	Selection: not randomised, allocation based on availability of treatment. Performance & detection: no reports of blinding procedures. Attrition: EG2 had 2 drop-outs. Other: assessed clinical relevance of outcomes; treatment content referenced (Rose, 2000, 2008), but treatment fidelity not assessed.
Silvestri (1977) [#]	3 independent groups EG1: implosive therapy EG2: pseudo-treatment oriented discussions CG: no-treatment control	EG1, EG2, CG: N = 8 average 5 M / 3 F Mean age 21 IDs: mild to borderline, mean IQ 70.3 Anxiety Country: USA	EG1: 10 45-minute sessions of individual implosive therapy over 3 weeks. Therapy includes imagery exposure and role-plays to reduce anxiety. EG2: 10 45-minute sessions where people discussed dreams and fantasies.	Brief Psychiatric Rating Scale; Nurses' Observational Scale for Inpatient Evaluation (NOSIE-30); Modified version of Adjective Checklist; Occupational Rating Scale. EG1 improved on all outcomes compared to EG2 and CG.	6 weeks Gains of EG1 on NOSIE-30 not maintained at follow-up, deterioration occurred but not below pretest level. CG improved significantly from pretest and posttest to follow-up.	Selection: randomised allocation based on age, sex, race and IQ. Performance & detection: no blinding. Attrition: follow-up data for 2 people from EG2 not available due to drop-out. Reporting: NOSIE-30 data based on 7/30 items. Other: treatment procedures referenced, but treatment fidelity not assessed.

Taylor (2002) #	Data included in Taylor (2005)					
Taylor (2004)	2 independent groups EG: individual cognitive-behavioural treatment for anger CG: waiting-list, routine care	EG: N = 9, mean age 29 CG: N = 8, mean age 29 100 % M IDs: EG mean IQ 69.3 CG mean IQ 66.4 Anger control difficulties Country: UK	EG: 18 individual cognitive-behavioural sessions over 3 months, including stress inoculation training Setting: in-patient forensic service	**Imaginal Provocation Test**: EG showed significant improvement on anger reaction, behavioural reaction, and **anger composite subscales**. EG also improved anger regulation, not significant but large effect.	No follow-up conducted	Selection: no randomised allocation, allocation procedures not specified Performance & detection: assessment by independent but not masked research assistant. Attrition: 1 person in each arm did not complete study, attrition rate 2/19; data not included in analysis. Other: therapists supervised by developer of treatment, treatment content referenced, but treatment fidelity not assessed.
Taylor (2005)	2 independent groups EG: individual cognitive-behavioural treatment for anger CG: waiting-list, routine care	EG: N = 16, mean age 29 CG: N = 20, mean age 30 100 % M IDs: EG mean IQ 67.1 CG mean IQ 70.7 Anger control difficulties Country: UK	EG: 18 individual cognitive-behavioural sessions over 3 months, including stress inoculation training Setting: in-patient forensic service	Novaco Anger Scale (NAS), **Provocation Inventory (PI)**, Anger Expression Scale, Ward Anger Rating Scale. Significant treatment x time interaction for Novaco Anger Scale. No significant differences between trends of EG and CG on NAS or PI. EG trend appears positive.	4 months EG1 improvements maintained	Selection: randomised concealed allocation based on date of referral; EG significantly lower IQ than CG. Performance & detection: assessment by independent but not masked research assistant. Attrition: data of 2 drop-outs in EG and 2 people in EG lost to follow-up are not included in analysis; demographic data of these 4 people is reported. Other: therapists supervised; treatment content referenced; random reviews of treatment files to check treatment fidelity.
Willner (2002)	2 independent groups EG: cognitive-behavioural anger management group CG: waiting-list control	EG: N = 7, mean age 31, 4 M / 3 F CG: N = 7, mean age 30, 5 M / 2 F IDs: EG mean IQ 63.9, CG mean IQ 65.3 Anger control difficulties Country: UK	EG: 9 weekly 2-hour group sessions of cognitive-behavioural anger management; minimal attendance 5/9 sessions. Group size: 5 – 7.	Carer and client ratings on Anger Inventory, **Provocation Index** All anger ratings decreased significantly for EG and increased (non-significantly) for CG. Improved anger ratings highly correlated with verbal IQ and full-scale IQ. Improvements greater for	3 months Treatment gains maintained and further improved at follow-up for EG. No follow-up conducted for CG.	Selection: randomised allocation based on alternate referrals Performance & detection: client & carer ratings not masked; some carer-ratings at baseline and post-treatment not by same staff. Attrition: 16 out of 21 referrals were allocated to study arms, 2 further participants swapped groups but later dropped-out.

				participants accompanied by carers		Other: treatment content referenced, but fidelity not assessed.
Willner (2005) [#]	2 independent groups EG: staff-delivered cognitive-behavioural anger-management group CG: no-treatment control	N = 17 EG: N = 9, mean age 45, 7 M / 2 F CG: N = 8, mean age 32, 5 M / 3 F IDs: only data for receptive vocabulary Anger control difficulties Country: UK	EG: 12 weekly 2-hour group sessions; intervention delivered by 2 staff; minimal attendance 8/12 sessions. Group size: 8 – 9	EG significantly lower scores than CG for both participant and carer ratings on Provocation Index and significantly better anger coping skills.	6 months EG maintained gains for carer ratings and increased gains for client ratings of Provocation Index. Anger coping skills maintained for EG.	Selection: allocation not randomised, but based on preference of participants and staff Performance & detection: no blinding, some staff involved in both delivering intervention and assessment of outcomes. Attrition: no drop-outs reported; missing data at baseline and follow-up replaced with post-treatment data for 2 participants Other: EG significantly lower PACS scores at baseline; staff lay-therapists trained and supervised by clinical psychologist; treatment content referenced.
Willner (2013)	2 independent groups EG: cognitive-behavioural group anger management CG: treatment as usual	EG: N = 91, median age 37, 71% M CG: N = 90, median age 39, 70% M IDs: EG median IQ 59.0 CG median IQ 55.0 Anger control difficulties Country: UK	EG: 12 weekly 2-hour psycho-educational cognitive-behavioural group sessions on anger-management delivered by lay-therapists. Group size: 5 + 2 lay therapists	**Client ratings on Provocation Index** EG showed small, but non-significant improvement for client ratings on Provocation Index. Key-workers' ratings showed significant improvements in anger management. Home carers' ratings showed less improvement.	6 months Treatment gains maintained for all ratings, except home carers' ratings.	Selection: cluster randomisation, clusters balanced on anger scores. Performance & detection: assessments by independent and masked researchers. Attrition: 179 participants randomised, 143 completed; intention-to-treat analysis. Other: Study protocol published; treatment content referenced; treatment fidelity 68.8 % (range: 19 – 86 %)

Note. Studies and outcome measures printed in bold were included in the meta-analysis.

[†] Excluded from meta-analysis because no control, waiting-list control, or no-treatment control arm was included.

[#] Excluded from meta-analysis because study did not provide sufficient data to calculate between-group effect sizes from post-treatment scores.

[##] Excluded from meta-analysis because data included in later study.

EG, experimental group; CG, comparison group; N, number of participants included in the study's data-analysis; M/F, male-female ratio; IDs, level of intellectual disabilities.

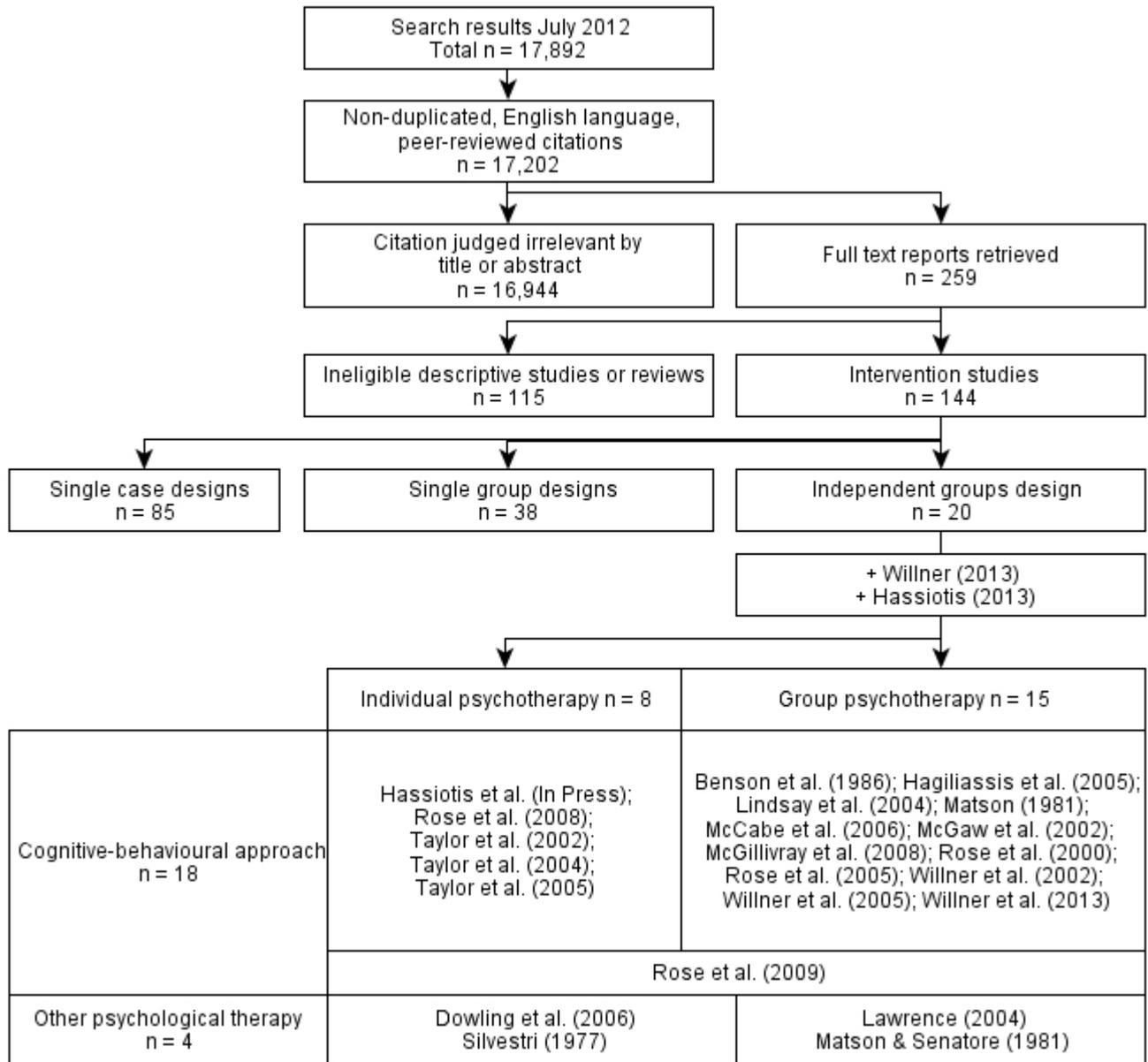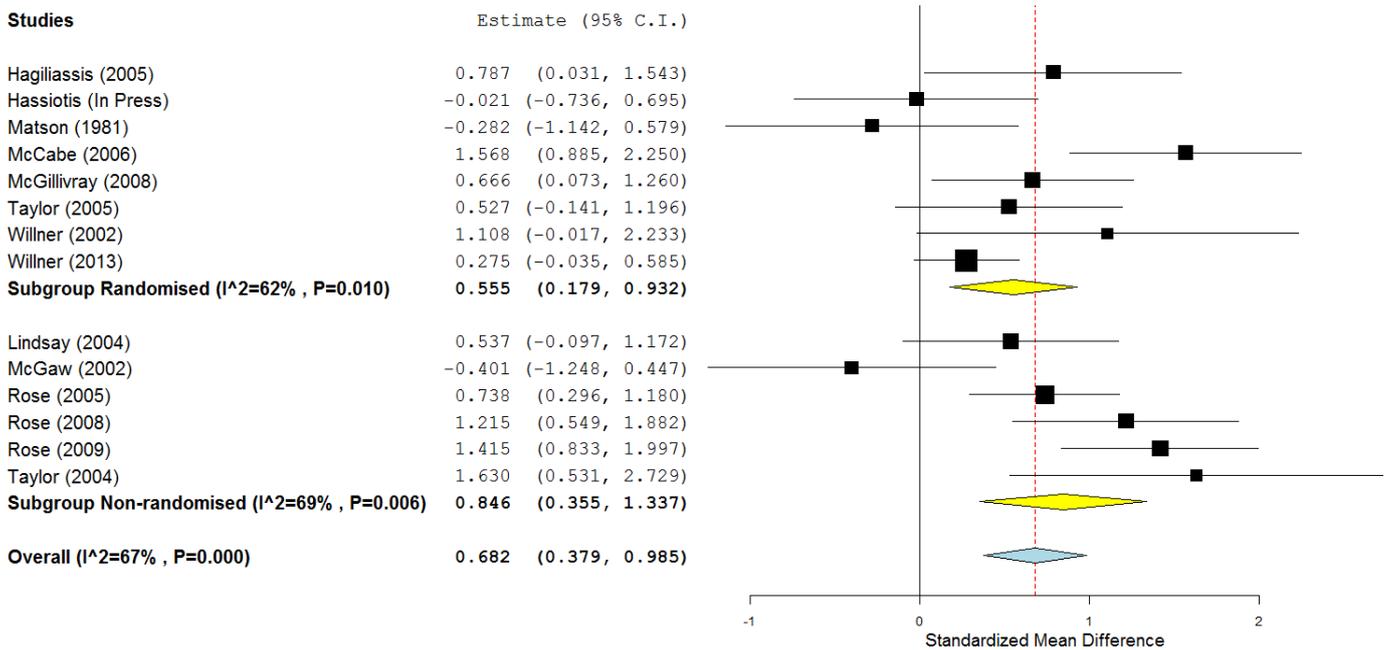*Figure 1*. Flowchart of study selection for systematic review.

Figure 2. Forest plot of estimated treatment effect of psychological therapy for people with IDs.

Horizontal lines represent the confidence interval for the standardised mean difference (black squares) of each study. The size of the black square is indicative of the study's sample size. The centre of the diamonds indicates the effect size for that subgroup analysis, while the width of the diamond covers the 95% CI. The vertical dashed line and bottom diamond indicate the overall size and its corresponding 95% CI.

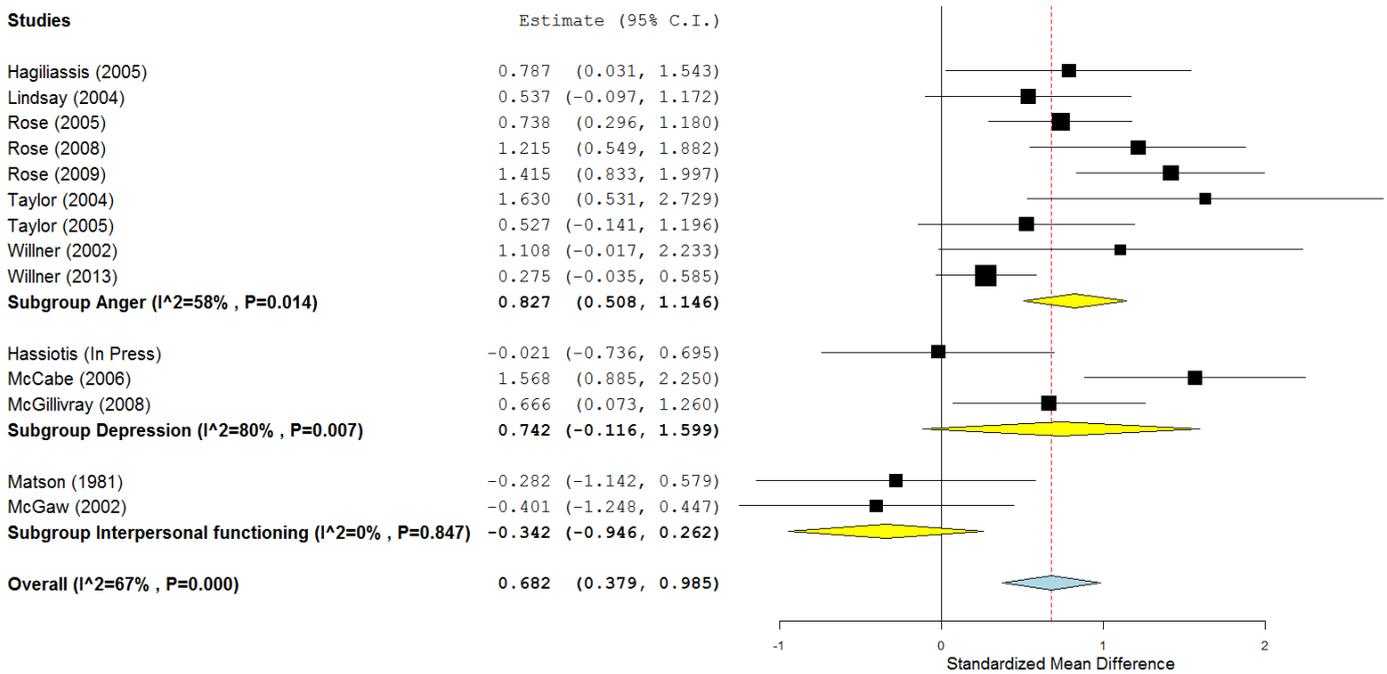*Figure 3*. Forest plot of subgroup meta-analysis for group-based and individual psychological therapy.
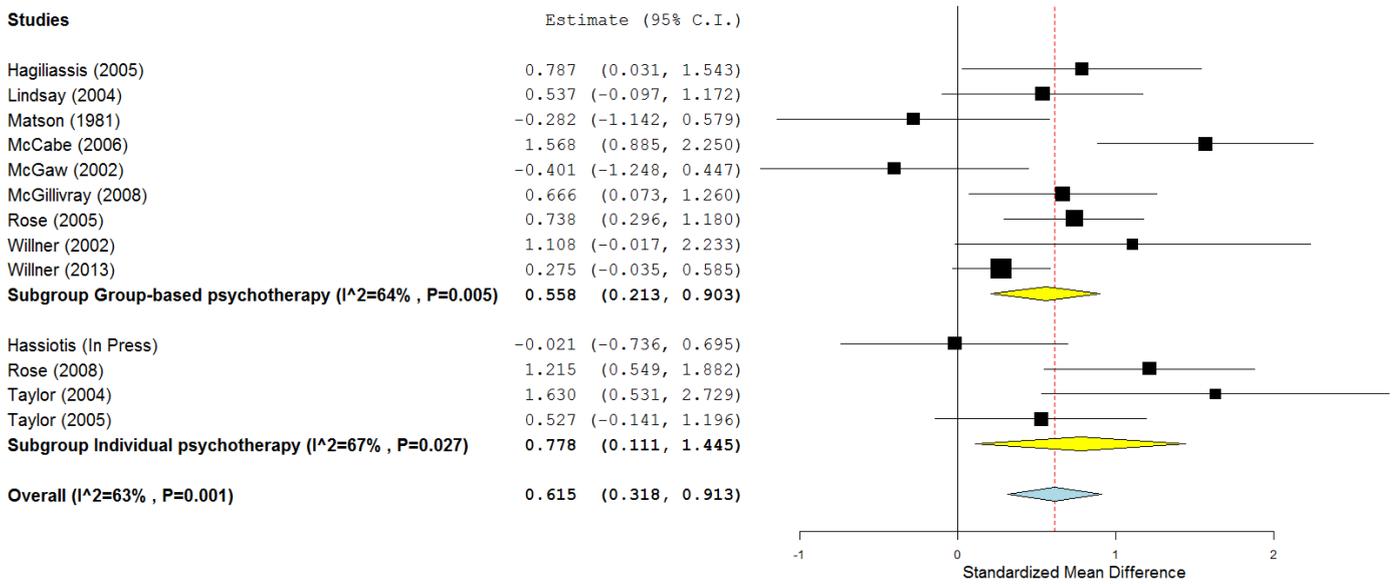
Horizontal lines represent the confidence interval for the standardised mean difference (black squares) of each study. The size of the black square is indicative of the study's sample size. The centre of the diamonds indicates the effect size for that subgroup analysis, while the width of the diamond covers the 95% CI. The vertical dashed line and bottom diamond indicate the overall size and its corresponding 95% CI.

Figure 4. Forest plot of subgroup meta-analysis based on clinical presentation.

Studies	Estimate (95% C.I.)
Hagiliassis (2005)	0.787 (0.031, 1.543)
Lindsay (2004)	0.537 (-0.097, 1.172)
Rose (2005)	0.738 (0.296, 1.180)
Rose (2008)	1.215 (0.549, 1.882)
Rose (2009)	1.415 (0.833, 1.997)
Taylor (2004)	1.630 (0.531, 2.729)
Taylor (2005)	0.527 (-0.141, 1.196)
Willner (2002)	1.108 (-0.017, 2.233)
Willner (2013)	0.275 (-0.035, 0.585)
Subgroup Anger (I^2=58% , P=0.014)	**0.827 (0.508, 1.146)**
Hassiotis (In Press)	-0.021 (-0.736, 0.695)
McCabe (2006)	1.568 (0.885, 2.250)
McGillivray (2008)	0.666 (0.073, 1.260)
Subgroup Depression (I^2=80% , P=0.007)	**0.742 (-0.116, 1.599)**
Matson (1981)	-0.282 (-1.142, 0.579)
McGaw (2002)	-0.401 (-1.248, 0.447)
Subgroup Interpersonal functioning (I^2=0% , P=0.847)	**-0.342 (-0.946, 0.262)**
Overall (I^2=67% , P=0.000)	**0.682 (0.379, 0.985)**

Horizontal lines represent the confidence interval for the standardised mean difference (black squares) of each study. The size of the black square is indicative of the study's sample size. The centre of the diamonds indicates the effect size for that subgroup analysis, while the width of the diamond covers the 95% CI. The vertical dashed line and bottom diamond indicate the overall size and its corresponding 95% CI.