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Effectiveness of school-based humanistic counselling for psychological distress in young people: Randomised controlled trial with follow-up

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Effectiveness of school-based humanistic counselling for psychological distress in young people: Pilot randomised controlled trial with follow-up in an ethnically diverse sample

Objectives. The aim of this study was to pilot a test of the effectiveness of school-based humanistic counselling (SBHC) in an ethnically diverse group of young people (aged 11-18 years old), with follow up assessments at 6 and 9 months.

Design. Pilot randomised controlled trial, using linear mixed effects modelling and intention to treat analysis to compare changes in levels of psychological distress for participants in SBHC against usual care (UC). Trial registration: ISRCTN44253140.

Methods. In total, 64 young people were randomised to either SBHC or UC. Participants were aged between 11 and 18 ($M = 14.2$, $SD = 1.8$), with 78.1% of a non-white ethnicity. The primary outcome was psychological distress at 6 weeks (mid-therapy), 12 weeks (end of therapy), 6 months and 9 months follow-up. Secondary measures included emotional symptoms, self-esteem, and attainment of personal goals.

Results. Recruitment and retention rates for the study were acceptable. Participants in the SBHC condition, as compared with participants in the UC condition, showed greater reductions in psychological distress and emotional symptoms, and greater improvements in self-esteem, over time. However, at follow-up, only emotional symptoms showed significant differences across groups.

Conclusions. The study adds to the pool of evidence suggesting that SBHC can be tested, and that it brings about short-term reductions in psychological and emotional distress in young people, across ethnicities. However, there is no evidence of longer term effects.

Key words: school counselling; adolescent psychotherapy; client-centred therapy; humanistic psychotherapy; treatment outcomes

Practitioner points:

- School-based humanistic counselling can be an effective means of reducing the psychological distress experienced by young people with emotional symptoms in the short term.
- The short-term effectiveness of school-based humanistic counselling is not limited to young people of a White ethnicity.
- There is no evidence that school-based humanistic counselling has effects beyond end of therapy.

Levels of mental health problems in children and adolescents are increasing (World Health Organisation, 2006). Over fifty per-cent of life-long mental illness begins by 14 years of age, with one in ten young people in the UK suffering from a diagnosable mental health disorder (NHS England & Department of Health, 2016; Kessler et al., 2005). Worldwide, epidemiological studies reveal that up-to twenty per cent of the youth population suffer from mental health difficulties (Belfer, 2008). The long-term implications of mental health problems include low levels of educational attainment, financial hardship, high levels of unemployment, and family instability (Colman et al., 2009).

Schools have been identified as a 'prime choice' for the delivery of interventions to address emotional difficulties (Kavanagh et al., 2009). They have become a key focus of the UK government's mental health strategy for children and young people (NHS England & Department of Health, 2016). As a community-wide resource, schools have the potential to maximize the extent to which mental health interventions are socially inclusive (Kavanagh et al., 2009). Young people are as much as ten times more likely to access a school-based mental health service as compared with a non-school-based one (Catron, Harris, & Weiss, 1998; Kaplan, Calonge, Guernsey, & Hanrahan, 1998).

A range of universal, targeted and specialist mental health provisions have been deployed within the school context (Vostanis, Humphrey, Fitzgerald, Deighton & Wolpert, 2013). For counselling and psychotherapy interventions, meta-analysis indicates an overall effect size (Cohen's *d*) of 0.45 (Baskin, Slaten, Crosby, Pufahl, Schneller, & Ladell, 2010). The studies in this meta-analysis are primarily of a cognitive behaviour nature, and CBT has now been established as an effective intervention for a range of clinical presentations (National Institute for Health and Care Excellence, 2005).

There is a need to develop and test non-CBT forms of therapy. This is for three reasons. First, not all young people improve through CBT (e.g., Brent et al., 1997). Second,

research in the adult psychotherapy field indicates that some clients may prefer -- and benefit from -- a less directive approach (Hatchett, 2015; Sandell, Clinton, Frövenholt, & Bragesjö, 2011; Beutler, Harwood, Michelson, Song and Holman, 2011). Third, many young people referred to school counselling services do not present with clinical disorders for which CBT has been found effective (Cooper, 2009). Rather, they experience distress related to a range of life difficulties.

Within the UK and other regions of the world (Harris, 2013), a widespread, alternative school-based mental health intervention is counselling, based on the principles of humanistic and integrative therapies (Cooper, 2013). In England, studies suggest that between 61% and 86% of secondary schools provide access to a counsellor (Department for Education, 2015; Hanley, Jenkins, Barlow, Humphrey & Wigelsworth, 2012) (although, given response biases, the actual proportion may be somewhat lower). The Department for Education (2015) in the UK has now published advice on counselling in schools, which includes the ‘strong expectation’ that ‘over time all schools should make counselling services available to their pupils’ (p. 11).

Practice-based evidence indicates that school-based counselling, as delivered in the UK, is associated with significant reductions in psychological distress (Cooper, 2009; Hill et al., 2011). However, there is no controlled evidence to assess the efficacy of the intervention, and the heterogeneity of practices makes any attempts at evaluation problematic. To address this, *school-based humanistic counselling* (SBHC) has been developed as a standardised form of humanistic school counselling (Cooper, Rowland, McArthur, Pattison, Cromarty, & Richards, 2010). The intervention is grounded in evidence-based competences for effective humanistic counselling with young people (Hill, Roth & Cooper, 2013; Roth, Hill, & Pilling, 2009).

Three pilot randomised trials have been conducted of SBHC against usual care (UC) for young people experiencing moderate to severe levels of emotional distress (Cooper et al., 2010; McArthur, Cooper, & Berdondini, 2013; Pybis et al., 2014). The UC condition consisted of the schools' pre-existing systems for supporting the emotional health and wellbeing of their pupils. The total numbers of young people participating in these trials ranged from 31 to 42. These studies found that it was feasible to test the effects of SBHC, with acceptable levels of recruitment and retention. In addition, on the primary outcome measure of psychological distress (YP-CORE, Twigg et al., 2009, 2015), meta-analysis gave an estimated effect size for the SBHC intervention of 0.59 (95% CI 0.16-1.01) at 6 weeks from baseline, and 0.82 (95% CI 0.30-1.35) at 12 weeks from baseline/end of therapy (Cooper, 2013). Methodological limitations were that there were no indications of follow-up outcomes beyond the 12 week endpoint, and ethnic backgrounds of the participants tended to be relatively homogenous (70.7-97.7% White European).

The aim of this present study was to extend this line of research by piloting a randomised controlled trial of SBHC in a more ethnically diverse sample, and with follow up measures at 6 and 9 months post-baseline assessment (3 and 6 months from end of intervention), which has not been included in previous studies. In addition, the present study aimed to contribute data to an estimate of the effectiveness of SBHC, as required for a power analysis for a definitive trial. Since pilot studies have small sample sizes, individually each provides only an imprecise estimate of effects (Kraemer, et al., 2006). However, meta-analysis allows effect size estimates to be pooled, leading to more precise estimates to help guide a power analysis.

Method

Design

The study was a pilot individual randomised controlled trial, using linear mixed effects modelling and intention to treat analysis to compare changes in levels of psychological distress for participants in school-based humanistic counselling (SBHC) against usual care (UC). This comparison group was chosen--rather than an alternative active treatment, such as CBT--because this we wanted to examine the effects of SBHC against the standard non-counselling alternative in the UK.

The required sample size for pilot studies is fiercely debated, with rationales including variance estimate precision, one-sided confidence interval width, and often relatively arbitrary rules of thumb (see Cocks and Torgerson, 2013, for a review). We chose a lower bound of 32 participants for the total sample size, following a recommendation by Torgerson and Torgerson (2008, p. 123). This enables a large effect of $d = 1$ to be detected with 80% power (two-tailed test, 5% significance level) which the authors argue is the minimum one should aim for in pilot studies in case an intervention is 'unexpectedly... extremely effective'. This effect size is greater than previous effects found for SBHC (see above), but is within the confidence intervals, and meant that we would be unlikely to miss a particularly large impact for the intervention. Torgerson and Torgerson also argue that anything over 30 allows a reasonably precise estimate of the variance. As one goal of this study was to contribute towards pooled data for meta-analytic estimating of effects for a fully-powered trial, we chose to continue recruiting past this minimum mark, and took into the study all young people referred--and assessed as eligible--during our one term recruitment period. In total, we recruited 64 young people to the study. This gives 80% power to detect an effect size of 0.71.

Participants

Recruitment took place during the spring school term, 2013. Participants came from three urban secondary schools in the UK, which provided state funded day education to

young people aged between 11 and 18, each with a diverse population. Two of the schools were mixed sex, and one was a single sex girls' school. Each of the schools was located in areas of significant deprivation, as indicated by their Income Deprivation Affecting Children Index (IDACI). Here, 15-46% of children aged 0 to 15 in these areas lived in income-deprived families. Inclusion criteria for schools were that they should not already have a pre-existing counselling service. This was to ensure that young people who might otherwise have received counselling would not be allocated to a UC condition.

Inclusion criteria for young people's participation in the study were: (1) aged between 11 and 18 years at baseline assessment; (2) experiencing moderate or high levels of emotional distress, as assessed by a score of 5 or more on the Emotional Symptoms subscale of the self-reported Strengths and Difficulties Questionnaire (SDQ-ES, Goodman, Meltzer, & Bailey, 1998) at baseline assessment; (3) considered capable of giving informed consent for participation in the trial, as assessed by the staff in each school responsible for emotional and behavioural support and by the researcher at baseline assessment; (4) greater than 85% attendance at the school, as assessed by the school. Exclusion criteria were: (1) at serious risk of harm to self or other, as assessed by pastoral care staff and by the researcher at baseline assessment; (2) planning to leave school within the period of the study; (3) involved with other child and young people mental health agencies, as indicated by the pastoral care team and/or the young person at assessment. We also excluded pupils in 'Year 11' (approximate age: 15-16) and 'Year 13' (approximate age: 17-18) as there was a high likelihood that they would be completing their studies that year and would not be available for follow-up assessment. The young people were not given any financial incentive for participating in the trial.

A total of 116 young people were referred by teaching staff for assessment (Figure 1). Of these, 64 participants were considered eligible and randomised into the trial: 34 to the

SBHC condition and 30 to the UC condition. Completion rates for our assessment points ranged from 70.3% (9 months follow-up) to 95.3% (end of therapy).

The mean age of the 64 randomised participants was 14.2 ($SD = 1.8$) years old (Table 1). Nine were male and 55 were female. Most commonly, participants were from a Black ethnic background (including African, Caribbean and Black British, $n = 25$); with 10 from a mixed ethnic background, 10 from a white British ethnic background, three from an Asian ethnic background, and 16 classifying themselves as 'White other' or 'other'.

Measures

The primary outcome measure was the Young Person's CORE (YP-CORE). This is a self-report measure of psychological distress in young people (Twigg et al., 2009; Twigg et al., 2015) and the most commonly used outcome measure in secondary school-based counselling in the UK (Cooper, 2013). Young people are asked to rate their psychological distress on ten items using a five point scale (0-4), giving a total score between 0 and 40, with higher scores indicating greater levels of distress. The YP-CORE measure has been shown to be acceptable to young people, with a good level of internal consistency (Cronbach's $\alpha = .85$; Twigg et al., 2009), test-retest stability (Pearson's $r = .76$, Twigg et al., 2015), and a differentiation between means for clinical and non-clinical samples (19.0 [$SD = 7.5$] and 9.4 [$SD = 7.3$], respectively, Twigg et al., 2015). For the present sample, the internal consistency at 12 weeks was $\alpha = .82$.

The self-report Strengths and Difficulties Questionnaire (SDQ) is a widely used brief behavioural screening instrument for children and young people aged 11 to 16 (Goodman, 2001). The measure consists of 25 items which are grouped into five subscales. Four of these are distress-related: emotional symptoms (SDQ-ES), conduct problems (SDQ-CP), hyperactivity (SDQ-HA) and peer problems (SDQ-PP); and one is strengths-related: pro-social (SDQ-PS). Young people are asked to rate the 25 items according to how they have

been feeling over the past 6 months (at assessment) and past month (at follow-up). The Total Difficulties score of the SDQ (SDQ-TD) is generated by summing the scores on four distress-related scales. Convergent validity for the overall SDQ-TD scale has been demonstrated (e.g., Cooper, Stewart, Sparks & Bunting, 2013) and internal consistency has been found to be acceptable (Cronbach's $\alpha = .82$ and $.78$, Goodman, Meltzer, & Bailey, 1998; McArthur et al., 2013). However, evidence has been found of low reliability and poorly fitting items on some subscales (Hagquist, 2007). For the present sample, the internal consistency at 12 weeks for total difficulties was $.70$; and $.63$ for emotional symptoms, $.53$ for conduct problems, $.70$ for hyperactivity, $.58$ for peer problems, and $.80$ for pro-social.

The Rosenberg (1965) Self Esteem Scale (RSES) comprises ten items that are rated on a 4-point scale from 'Strongly disagree' to 'Strongly agree'. It was originally developed for use with young people and has since been established as a reliable and valid measure of self-esteem (Blascovich & Tomaka, 1993). A cross-cultural evaluation of the RSES in 53 nations revealed the RSES factor structure as consistent across nations (Schmitt and Allik, 2005), with a mean Cronbach's α of $.81$. In the present sample, the RSES had an internal consistency of $.90$ at 12 weeks.

The Goal-Based Outcomes (GBO) tool is a personalized questionnaire developed for use with under-18s (Law, 2009). Young people are asked to identify up to three goals for therapy and, at each of the time points, rate on a 0-10 scale how much progress they feel they have made towards that goal. The GBO tool has shown convergent validity against measures of functioning and psychosocial difficulties, and acceptable levels of internal consistency ($\alpha = .71$, Edbrooke-Childs, Jacob, Law, Deighton & Wolpert, 2015). The internal consistency in the present sample was $.83$ at 12 weeks.

Interventions

School-based humanistic counselling.

Counsellors were asked to offer school-based humanistic counselling (SBHC), delivered in up to twelve weekly sessions of approximately 45 minutes each. This therapy is based on competences for humanistic psychological therapy (Roth et al., 2009) which have been adapted for young people (Hill et al., 2013), and are derived from the person-centred and experiential approach to therapy (Rogers, 1959; Sanders & Hill, 2014). The assumption underlying SBHC is that young people have the capacity to successfully address difficulties in their lives if they have an opportunity to talk through these problems with an empathic, supportive adult. School-based humanistic counselling uses a range of techniques to facilitate this process, including active listening, empathic reflections, and helping clients to reflect on their emotions and behaviours. In contrast to CBT, SBHC is a non-directive intervention, and counsellors are required to ‘clearly and consistently follow the client’s lead’ (Freire, Elliott, & Westwell, 2014, p.3). This means refraining from conveying to their clients interpretations, advice, or psychoeducational input.

Usual care.

Usual care consisted of all supports and mechanisms the schools put into place, as detailed in the Department for Education guidance on supporting pupil’s well-being (Department for Education, 2015; Public Health England, 2015). This is a whole school approach to supporting children’s emotional well-being. For schools in the present study, support was available from school nurses, class teachers, the special education needs coordinator and their team, and (peer) mentors. Schools in this study also had specific *Pastoral Care Teams*. These teams included one or two members of staff who were trained to support pupils with difficulties and to whom pupils could self-refer or be referred to by other staff. Specific counselling services were not provided in any of the study schools. However, pupils could be referred to services outside the school, such as their primary care physician or specialist child and adolescent mental health service (CAMHS).

Those allocated to the UC condition were informed that, nine months from assessment, they could receive a standard program of weekly counselling for up to one school term. Twenty-six of the UC participants (86.7%) chose to do so. However, due to delays in organizing the nine month follow-up interviews not all participants completed the interviews within the 9 months' time-frame. Hence, 13 of these UC participants (43.3% of all UC participants) began counselling before their 9 month follow up assessment. For ethical reasons, we felt it would not have been appropriate to delay the start of their counselling until after this assessment point and, following best practice for intention-to-treat analyses, we continue to report on these 9 month follow-up results. These UC participants who began counselling had a mean of 4.8 sessions of counselling prior to 9 month follow up (median 5, range 4–6 sessions). This meant that participants in the UC condition who completed the 9 month follow up assessment had had a mean of 2.3 sessions of SBHC (mode 0, interquartile range 0–4.25, range 0–6), while those in the SBHC condition had had a mean of 7.5 sessions (mode 10, interquartile range 6–10, range 2–10).

Procedures

Ethical approval was obtained from the relevant university ethics committee.

Contact was made with secondary schools across inner London, and four schools indicated an interest in participating in the trial. The leadership team in each of the four schools were visited by members of the research team, and the details of the study were presented and discussed. Based on the school inclusion criteria, an assessment of the number of referrals needed, and the schools' capacities to participate, three of the four schools were identified for participation in the trial. Consent from stakeholders in each participating school was obtained.

Teachers responsible for pupil well-being at the selected schools were provided with training about the participant selection criteria and asked to identify young people whom they

thought might benefit from counselling. The Pastoral Care Team were also asked to confirm whether the pupils identified would be able to provide consent. In the UK, consent made by a young person under the age of 16 follows the ‘Gillick competence’ principle arising from a legal ruling (Gillick v Norfolk, 1985; Cornock, 2007) that a young person is able to provide consent for treatment based on their intellectual and emotional competence, and their understanding of the proposed treatment and its implications.

Parents or carers of pupils deemed to be potentially eligible--and who were interested in, and willing to, participate in the study, and willing for their parents or carers to be informed--were sent letters with further information about the study. The letters included an opt-out consent form for parents who did not want their young person to participate. If a parent or carer did not opt their young person out of the study, the young person then met with the researcher in a confidential and secure environment in the school. If young people consented to participate in the study and met all inclusion criteria, they were randomised to SBHC or UC using an automated text based system and advised immediately of their allocation. Blocked randomisation was performed, stratified by school to balance the group sizes in each condition at each school. A block size of six and an allocation ratio of 1:1 was specified.

At baseline, mid-therapy (6 weeks from baseline), end of therapy (12 weeks from baseline), 6 months follow-up (from baseline), and 9 months follow-up (from baseline), participants met individually with a researcher to complete the psychological measures. The researchers were blind to the participants’ allocations.

Counsellors.

There were four counsellors employed to work within the trial, all female, with a mean age of 49 years. All had a minimum of three years of person-centred counselling training and the qualification to practice as a counsellor in the UK. They had also completed

additional training in working with young people. The four counsellors were chosen from eight shortlisted applicants following a selection process that included observation of live counselling practice. Counsellors were asked to study a manual of humanistic competences (Roth et al., 2009) and to deliver their counselling accordingly. The counsellors were new to each school and worked on a paid basis.

To assess adherence to these competences, two 10-minute audio segments of sessions were randomly selected for each counsellor and rated for adherence to humanistic competences using the Person-Centred & Experiential Psychotherapy Scale (PCEPS, Freire et al., 2014). In this validated measure, external judges are asked to rate the segments on 15 scales, ranging from 1 to 6, with greater scores indicating greater adherence to competences. There were three judges. Two were members of the research team (the first and second authors) who rated the segments together, and the third was an independent expert in person-centred and experiential therapy. There was high agreement in the ratings between the research team members and the independent expert ($r = .97$). Practice was deemed to be adherent to SBHC if the total score on the PCEPS scale was 60 or above (average rating of 4 or more per item). The average score across all counsellor was 61.7 ($SD = 4.0$). In one instance, practice did not meet the required standard (score = 52.5). This was recorded and discussed in detail with the counsellor to look at ways of facilitating increased adherence. Feedback on practice was provided to all of the counsellors.

Testers.

A team of independent testers with experience of working with young people were recruited to undertake the assessments within the trial. Testers attended one full day's training in which they were familiarised with procedures for assessments, the use of and scoring of the outcome measures, and the eligibility criteria for randomization.

During initial assessments, it became apparent that some participants were not able to complete the measures independently. Our sample, with its diverse inner city profile, had many students with a below-average English reading level. This was often as a consequence of having had disruptions to their education as recent asylum seekers/refugee families, with English as an additional language. On discussion with the schools, and in line with other research where a high proportion of participants have had reading difficulties (e.g., Barr, Hodge, Leeven, Bowen & Knox, 2012; Miranda et al., 2006), it was decided the measures should be administered orally by the testers, with the testers recording participants' responses. For the purpose of consistency, this testing method was then adopted for all participants across all sites. Testers were trained to follow standard administration instructions in order to reduce variation and ensure quality.

Data analysis.

Data were analysed using R version 3.2.3 (R Core Team, 2014). Descriptive statistics were calculated for the continuous variables, and SDQ scores were categorized using a four-level system (see <http://sdqinfo.org>). Here, participants scoring within the 80% lowest difficulties scores of a population are classified as 'close to average', the next 10% 'slightly raised', the next 5% 'high' and the top 5% 'very high'.

Multilevel models were fitted using lmer (Bates, Maechler, Bolker, & Walker, 2014) for each outcome measure, with fixed effects of time (as a category, to make non-linear effects more transparent, with the first time point as the comparison level), group (counselling versus usual care), and school (since there were three schools a fixed rather than random effect was used for school). A random intercept by participant was used. The key tests for the effectiveness of counselling were the interactions between time and group on each measure. These were tested using log-likelihood ratio (LLR) tests (asymptotically χ^2 distributed with 4 degrees of freedom for the interaction) and are reported both with and

without correction for multiple testing using the Bonferroni-Holm procedure (Holm, 1979). The level of statistical significance was set to .05. Estimates of the simple effect sizes are provided as mean differences with 95% confidence intervals as computed from the multilevel model interaction coefficients. Standardised effect sizes were calculated by dividing the multilevel model-estimated mean intervention effect at each time point by the pooled pre-intervention standard deviation. The primary analyses use intention to treat, i.e., participants were analysed in the group they were assigned at randomisation. Additional analyses were also performed excluding nine month assessment data points from participants in UC who had attended one or more counselling sessions before their assessment.

Results

At baseline, the mean scores on the YP-CORE for participants in the SBHC and UC groups were 18.94 ($SD = 7.31$) and 19.07 ($SD = 5.91$) respectively (see Table 2 for baseline characteristics on continuous measures). Based on the SDQ four-level taxonomy, 50% of young people in the SBHC group had very high levels of total difficulties and 24% had high levels, with 60% and 13% respectively for the UC participants (see Table 3).

Following Bonferroni-Holm correction for multiple testing, scores on the YP-CORE, SDQ-ES, and RSES showed a statistically significant interaction between group and time effects (LLR = 15.7, adjusted $p = .028$; LLR = 23.3, adjusted $p < .001$, LLR = 15.2, adjusted $p = .03$; respectively; Table 4). This indicates that participants in the SBHC condition, as compared with participants in the UC condition, showed a greater reduction in psychological distress and emotional symptoms, and a greater improvement in self-esteem, over time. Without correction, the group by time effect for the SDQ-TD was also statistically significant (LLR = 13.6, unadjusted $p = .009$, adjusted $p = .053$). There were no significant group by time effects for conduct problems, hyperactivity, peer problems, pro-social and goal attainment.

The descriptive statistics for all outcome measures, and estimates of the differences between SBHC and UC, are presented in Table 5. Figure 2 shows the model-estimated mean differences for all measures between time and group. On all three measures for which there was a significant group by time interaction, there was a significant difference at end of therapy, with standardized mean differences of 0.86 on the YP-CORE, 1.33 on the SDQ-ES, and 0.75 on the RSES. Additionally, the SDQ-ES subscale showed significant difference at all time points (standardized mean difference = 0.77 at 6 weeks (mid-intervention), 1.59 at 6 months follow-up, and 0.81 at 9 months follow-up). However, on the YP-CORE and RSES, differences were no longer significant at 6 or 9 months follow-up.

The data were reanalysed excluding 9 month follow-up data from UC participants who attended any counselling session before being assessed, leaving 21 SBHC and 11 UC participants (Table 6). The overall pattern of results--i.e., in terms of which confidence intervals included and excluded zero--was identical to the intention-to-treat analysis.

Discussion

In terms of feasibility, our pilot study indicated that we could successfully recruit to a trial of school-based counselling with an ethnically diverse, inner city population. Retention rates were acceptable at mid- and end of therapy; although, by 6 and 9 months follow-up, they were dropping to marginal levels of acceptability. This suggests that, for future trials, it may be important to develop strategies for maximising retention at follow-up. For future studies, it will also be essential to schedule follow-up assessments in such a way that they are completed before UC participants commence counselling.

An important learning from this study was that, within this sample, a significant minority of young people could not independently complete our measures. The strategy that we used to address this--oral administration--has the limitation that our measures had not been validated for use in this way. Although the testers were blind to the participants'

allocation (reducing the risk of interview bias), it remains possible that young people in the two conditions were differentially influenced in their responses. In particular, as a consequence of demand characteristics, those in the counselling arm may have experienced greater pressure to report positive results. Future research, therefore, may need to consider use of measures that have been validated for oral administration, that require a lower reading age, or that are observer-completed.

Using an ethnically diverse sample, the results of this study are consistent with previous outcome evidence at mid- and end of therapy (Cooper et al., 2010; McArthur et al., 2013; Pybis et al., 2014). SBHC brought about significant reductions in psychological and emotional distress, but had no significant impact in areas such as peer problems, conduct problems, and hyperactivity. However, in contrast to previous studies, the present study found that SBHC brought about significant improvements in self-esteem but not personal goal attainment. Overall, the data suggests that the principal effects of SBHC are in the emotional and self-perceptual--rather than behavioural--domains, as would generally be predicted by humanistic theory (Rogers, 1959).

The findings from this study found no evidence that SBHC had enduring effects after the end of therapy. The one exception to this was that participants in the SBHC group, at 6 and 9 months follow-up, showed lower levels of emotional symptoms than those in UC. Our sensitivity analysis suggests that this attenuation of effects was not because some participants in the UC group had had counselling by 9 months follow-up. However, further research is needed here, with larger samples, to test this adequately.

More generally, as a test of the effectiveness of SBHC, the study is limited by its sample size. Although this is larger than in previous pilot studies of this intervention, remains underpowered. Hence, Type II errors may have been made, with the study failing to detect effects that SBHC actually has. The drop-out rates at six and nine months follow-up

also make estimations of effects less exact, as does the poor internal consistency of some SDQ subscales. In addition, the outcome data may have been biased by the verbal administration of measures. Another important limitation is that the study was only conducted in schools without a pre-existing counselling service, which now represents only a minority of schools in the UK. The external validity of the study was also limited by the fact that the ethnicity of our participants, although more mixed than in previous trials of SBHC, was not representative of the national composition (in which there is a greater proportion of people with Asian ethnicities, and fewer with Black ethnicities, Office for National Statistics, 2011). The proportion of females in our study, 90%, was also higher than is typical of school-based counselling clients (approximately 60%, Cooper, 2013). Finally, the comparison against usual care means that we do not know whether nonspecific therapy factors--such as placebo expectations, or time spent with an adult--were responsible for positive outcomes; and data on the nature of the UC intervention were not available.

Summary

The findings from this pilot randomised controlled trial suggest that it is feasible to test the effectiveness of SBHC in an ethnically-diverse sample, using follow-up measures. The findings also add to the pool of data suggesting that SBHC can bring about short term reductions in psychological and emotional distress. However, the study provides no evidence for the long term benefits of this intervention.

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Tables

Table 1. Demographic information for randomised participants

	UC (n = 30)	SBHC (n = 34)	Total (n = 64)
Mean age in years (SD)	14.0 (1.7)	14.3 (1.8)	14.2 (1.8)
Age (n, %)			
11	1 (3.3%)	0 (0.0%)	1 (1.6%)
12	5 (16.7%)	4 (11.8%)	9 (14.1%)
13	7 (23.3%)	12 (35.3%)	19 (29.7%)
14	7 (23.3%)	4 (11.8%)	11 (17.2%)
15	5 (16.7%)	5 (14.7%)	10 (15.6%)
16	0 (0.0%)	4 (11.8%)	4 (6.3%)
17	5 (16.7%)	3 (8.8%)	8 (12.5%)
18	0 (0.0%)	2 (5.9%)	2 (3.1%)
Gender (n, %)			
Male	3 (10.0%)	6 (17.6%)	9 (14.1%)
Female	27 (90.0%)	28 (82.4%)	55 (85.9%)
Ethnic origin (n, %)			
White British	7 (23.3%)	3 (8.8%)	10 (15.6%)
White Other	1 (3.3%)	3 (8.8%)	4 (6.3%)
Black/Black British	9 (30.0%)	16 (47.1%)	25 (39.1%)
Asian/Asian British	2 (6.7%)	1 (2.9%)	3 (4.7%)
Mixed Background	5 (16.6%)	5 (14.7%)	10 (15.6%)
Other	6 (19.9%)	6 (17.5%)	12 (18.8%)

Table 2. Characteristics for continuous measures at baseline.

	<i>Counselling</i>			<i>Usual care</i>			<i>Combined</i>		
	<i>Mean</i>	<i>SD</i>	<i>N</i>	<i>Mean</i>	<i>SD</i>	<i>N</i>	<i>Mean</i>	<i>SD</i>	<i>N</i>
YP-CORE	18.94	7.31	34	19.07	5.91	30	19.00	6.64	64
SDQ Total	20.18	4.06	34	20.47	4.81	30	20.31	4.40	64
SDQ Prosocial	7.65	1.79	34	7.47	2.65	30	7.56	2.22	64
SDQ Emotion	6.68	1.34	34	6.53	1.46	30	6.61	1.39	64
SDQ Conduct	3.53	1.96	34	3.90	1.56	30	3.70	1.78	64
SDQ Hyper	5.97	2.12	34	6.27	2.42	30	6.11	2.25	64
SDQ Peer									
Problems	4.00	2.07	34	3.77	2.40	30	3.89	2.22	64
RSES	16.03	5.20	34	15.72	5.46	29	15.89	5.28	63
Goals (average)	2.67	1.72	31	2.85	1.79	28	2.75	1.74	59

Table 3. Baseline SDQ four-band categorisations (% of participants within group) as close to average and levels above average (or below average, for the SDQ-PS subscale).

	<i>Counselling</i>				<i>Pastoral care</i>			
	Close to average	Slightly raised (/lowered)	High (/low)	Very high (/low)	Close to average	Slightly raised (/lowered)	High (/low)	Very high (/low)
SDQ Total	6	21	24	50	10	17	13	60
SDQ Prosocial	71	21	6	3	70	10	7	13
SDQ Emotion	0	21	29	50	0	33	23	43
SDQ Conduct	56	18	9	18	37	33	17	13
SDQ Hyper	44	18	12	26	33	20	13	33
SDQ Peer Problems	26	21	24	29	30	30	10	30

Table 4. Log-likelihood ratios (LLR) tests of the interaction between intervention group and time. The adjusted p-value uses Bonferroni-Holm adjustment.

<i>Outcome</i>	<i>LLR</i>	<i>p</i>	<i>Adjusted p</i>
YP-CORE	15.7	0.003	0.028
SDQ Total	13.6	0.009	0.053
SDQ Prosocial	3.0	0.564	1.000
SDQ Emotion	23.3	< .001	0.001
SDQ Conduct	3.2	0.524	1.000
SDQ Hyper	4.9	0.298	1.000
SDQ Peer Problems	4.5	0.338	1.000
RSES	15.2	0.004	0.030
Goals	9.0	0.062	0.311

Table 5. Descriptive statistics and mean difference between groups estimated from the multilevel models. Standardized mean differences (SMD) were computed by dividing the mean difference estimate by the pre-intervention SD. Statistically significant effects (95% confidence intervals excluding zero) are in bold.

	<i>Counselling</i>			<i>Usual care</i>			<i>Model-estimated mean difference</i>			
	<i>Mean</i>	<i>SD</i>	<i>N</i>	<i>Mean</i>	<i>SD</i>	<i>N</i>	<i>Estimate</i>	<i>95% CI</i>		<i>SMD</i>
<i>YP-CORE</i>										
6 weeks	16.32	8.89	25	19.08	6.16	26	-3.03	-6.65	0.61	-0.46
12 weeks	12.61	7.57	31	19.30	6.92	30	-5.70	-9.13	-2.28	-0.86
6 months	13.48	7.17	23	16.57	6.69	23	-3.20	-6.96	0.56	-0.48
9 months	16.14	6.84	21	14.92	7.99	24	0.81	-2.98	4.61	0.12
<i>SDQ Total</i>										
6 weeks	17.24	6.92	25	19.27	4.96	26	-2.05	-4.35	0.26	-0.47
12 weeks	14.45	5.38	31	18.80	4.63	30	-3.60	-5.77	-1.42	-0.82
6 months	14.70	5.12	23	17.65	5.75	23	-3.05	-5.45	-0.66	-0.69
9 months	15.38	6.17	21	16.38	6.46	24	-0.68	-3.10	1.74	-0.15
<i>SDQ Prosocial</i>										
6 weeks	7.76	1.67	25	7.50	2.61	26	0.48	-0.42	1.38	0.22
12 weeks	8.29	1.62	31	7.43	2.56	30	0.52	-0.32	1.37	0.24
6 months	7.65	2.04	23	7.96	2.25	23	-0.13	-1.06	0.81	-0.06
9 months	7.67	1.65	21	7.25	2.67	24	0.19	-0.76	1.13	0.08
<i>SDQ Emotion</i>										
6 weeks	4.88	2.26	25	5.58	2.10	26	-1.06	-2.02	-0.10	-0.77
12 weeks	4.00	2.22	31	5.80	2.09	30	-1.85	-2.75	-0.95	-1.33
6 months	3.48	1.88	23	5.52	2.47	23	-2.20	-3.19	-1.20	-1.59
9 months	3.95	2.01	21	4.88	2.94	24	-1.12	-2.13	-0.12	-0.81
<i>SDQ Conduct</i>										
6 weeks	3.52	2.37	25	3.85	1.71	26	-0.29	-1.16	0.58	-0.16
12 weeks	2.74	1.93	31	3.47	1.81	30	-0.23	-1.05	0.59	-0.13

6 months	3.35	1.75	23	3.30	1.87	23	0.06	-0.84	0.96	0.03
9 months	3.24	2.10	21	3.17	1.90	24	0.49	-0.42	1.40	0.28
<i>SDQ Hyper</i>										
6 weeks	5.36	2.87	25	6.04	2.32	26	-0.39	-1.35	0.57	-0.17
12 weeks	5.00	2.39	31	6.13	2.42	30	-0.69	-1.60	0.21	-0.31
6 months	5.09	2.43	23	5.48	2.79	23	-0.15	-1.15	0.85	-0.06
9 months	5.33	2.63	21	5.38	2.86	24	0.37	-0.64	1.38	0.16
<i>SDQ Peer Problems</i>										
6 weeks	3.48	1.90	25	3.81	2.53	26	-0.30	-1.21	0.62	-0.13
12 weeks	2.71	1.90	31	3.40	2.31	30	-0.83	-1.69	0.03	-0.37
6 months	2.78	1.81	23	3.35	1.92	23	-0.78	-1.73	0.17	-0.35
9 months	2.86	1.98	21	2.96	2.29	24	-0.42	-1.38	0.54	-0.19
<i>RSES</i>										
6 weeks	16.80	5.93	25	14.85	4.95	26	1.84	-0.63	4.31	0.35
12 weeks	19.67	5.29	30	14.60	5.40	30	3.98	1.64	6.33	0.75
6 months	17.91	6.11	23	15.17	7.14	23	2.14	-0.42	4.71	0.41
9 months	17.43	4.96	21	17.21	6.47	24	-0.37	-2.95	2.22	-0.07
<i>Goals (average)</i>										
6 weeks	4.55	2.24	20	3.01	1.88	25	1.77	0.36	3.18	1.02
12 weeks	5.56	2.52	26	4.21	2.49	29	1.36	0.04	2.69	0.78
6 months	6.10	2.66	23	4.72	2.43	23	1.69	0.30	3.09	0.97
9 months	6.62	2.33	21	5.25	2.78	24	1.58	0.17	2.99	0.91

Table 6. Reanalysis excluding nine months data from UC participants who had attended any sessions. Mean differences between groups at 9 month follow-up were estimated from the multilevel models. Standardized mean differences (SMD) computed by dividing the mean difference estimate by the baseline SD. Statistically significant effects (95% confidence intervals excluding zero) are in bold.

	<i>Model-estimated mean difference</i>			
	<i>Estimate</i>	<i>95% CI</i>		<i>SMD</i>
YP-CORE	-1.55	-5.96	2.85	-0.23
SDQ Total	-0.10	-2.91	2.70	-0.02
SDQ Prosocial	-0.07	-1.17	1.03	-0.03
SDQ Emotion	-1.46	-2.63	-0.29	-1.05
SDQ Conduct	1.00	-0.05	2.04	0.56
SDQ Hyper	0.71	-0.49	1.90	0.31
SDQ Peer Problems	-0.41	-1.54	0.72	-0.18
SEQ	-0.55	-3.57	2.48	-0.11
Goals (average)	1.73	0.12	3.35	1.00

Figures

Figure 1. Participant flow diagram

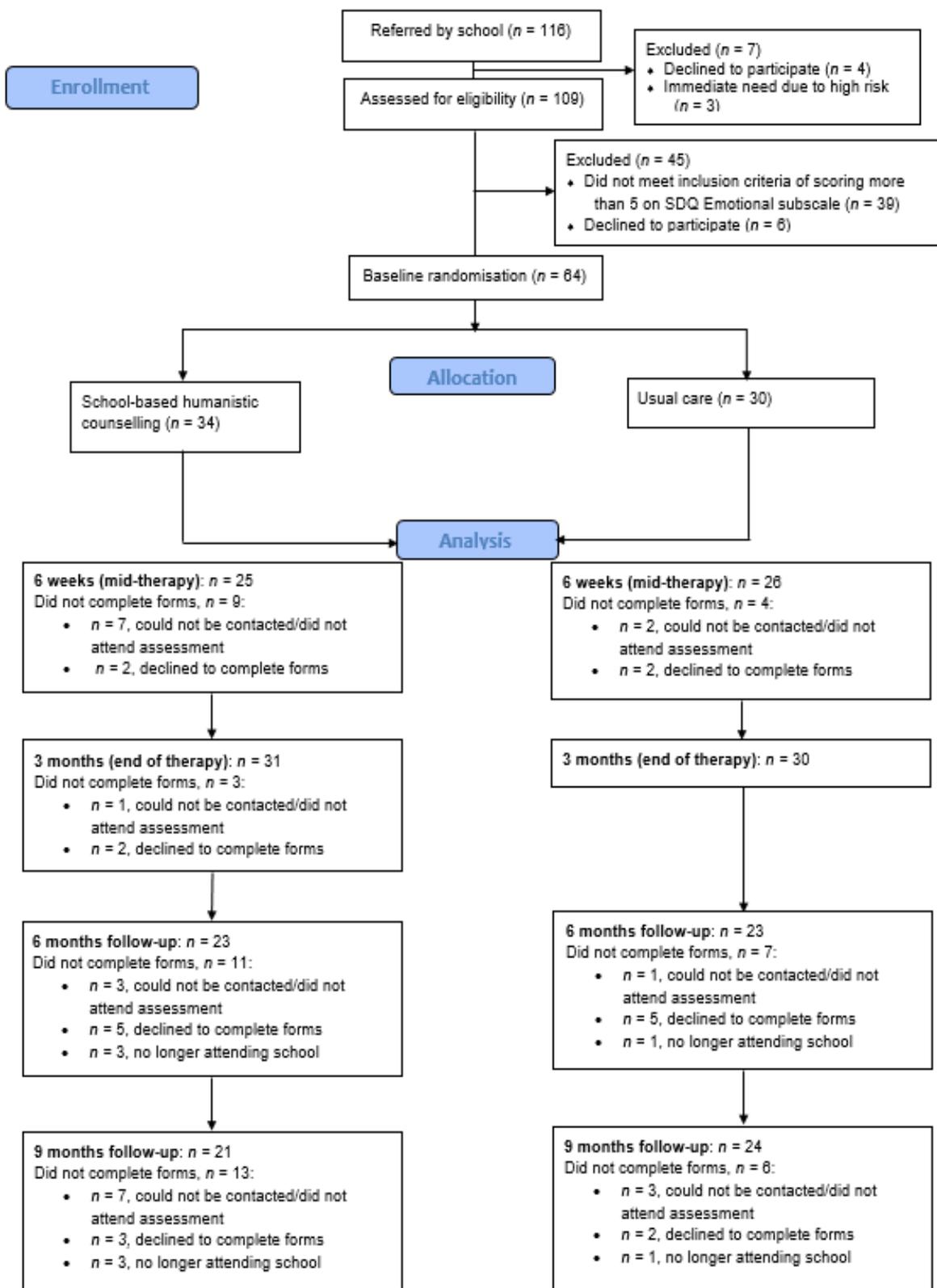


Figure 2. Model-estimated mean difference (and 95% CIs) between counselling and usual care at mid-therapy (6w), end of therapy (12w), 6 month follow-up, and 9 month follow-up.

