A systematic review and subset meta-analysis of published randomised controlled trials of psychological therapies for children and adolescents with chronic pain is reported. A search of four computerised abstracting services recovered 123 papers from which 28 potential trials were identified. Eighteen met the criteria for inclusion in the review. The majority of these papers reported brief behavioural and cognitive behavioural interventions for children with headache and many were conducted in community (i.e. school) settings. Meta-analysis was applicable for 12 headache trials and one trial of recurrent abdominal pain using the Pain Index. The odds-ratio for a 50% reduction in pain was 9.62 and the number needed to treat was 2.32, indicating that the psychological treatments examined are effective in reducing the pain of headache. The quality of the 18 trials retrieved is narratively reviewed and suggestions for the development of trials in this field are made. © 2002 International Association for the Study of Pain. Published by Elsevier Science B.V. All rights reserved.

Keywords: Systematic review; Meta-analysis; Children; Chronic pain; Headache; Psychology

1. Introduction

Children and adolescents frequently experience and report pain (e.g. Goodman and McGrath, 1991; Fearon et al., 1996). A minority become patients who report significant pain and pain-associated distress and disability. This paper reports a systematic review of the randomised controlled trials (RCTs) of psychological therapies for children and adolescents with chronic pain and associated distress and disability.

Chronic pain, pain-related distress, and pain related disability are thought to be commonly under-reported by children and adolescents (Varni et al., 1996; Elliott et al., 1999). However, a number of well-designed studies have found that children and adolescents do report persistent or recurrent chronic pain. For example, in a recent investigation, Perquin et al. (2000, 2001) analysed a large representative sample of school children for all pain experiences and found that 25% of the sample reported chronic or recurrent pain of 3 months or longer. They also found evidence in support of earlier findings that the most common locations of pain are head, limb, and gut; that the report of chronic pain increases with age; and that chronic pain is more common amongst older girls. Of interest to the present study was the finding that 8% of this large school-attending sample reported their experience of pain to be both chronic and severe.

Older children with chronic pain also report chronic disability and emotional distress due to recurrent or persistent pain (Bursch et al., 1998); distress that is often also reported by family members (e.g. Walker and Greene, 1989). Psychological therapies have recently been promoted as potentially effective interventions for the management of severe pain and its disabling consequences (McGrath and Finley, 1999). However, although there is an established systematically reviewed evidence base for the effectiveness of psychological treatments for chronic pain in adults
There is both indirect and direct evidence for the idea that psychological therapies may have a role to play in paediatric chronic pain management. Indirect evidence comes from the field of paediatric psychotherapy. Kazdin and Weisz (1998), in a thoughtful and challenging narrative review of child and adolescent psychotherapy outcome research, report that there is good evidence for the effectiveness of cognitive behavioural therapy in treating childhood anxiety disorders, childhood depression, and oppositional behaviour. Perhaps of most relevance to the current review is their conclusion that the results are promising for the psychological treatments that are aimed at reducing the anxiety and symptom reporting associated with medical and dental procedures (see also Kibby et al., 1998).

Direct evidence for the claim that psychological therapies may have a role to play in paediatric chronic pain management is provided in a number of recent review articles. Holden et al. (1999) reviewed 31 studies of treatments for children with chronic headache and found good evidence for the efficacy of relaxation and self-hypnosis in reducing pain. The authors included non-randomised trials, and did not analyse pooled data. In their review of paediatric migraine Hermann et al. (1995) did employ data-pooling techniques and found biofeedback and muscle relaxation to be more efficacious than placebo treatments and prophylactic drug treatments in controlling headache. Recurrent abdominal pain is a difficult area of study due to the heterogeneity of presentation and the lack of consensus on a standard treatment approach (Walker, 1999). Nevertheless, some attempts have been made at intervention studies. Janicke and Finney (1999) reported on nine studies, of which four employed control groups. The authors included uncontrolled studies and studies of dietary fibre supplements. No data were available for pooling, although these authors judge the cognitive behavioural approach taken by Sanders et al. (1994) to be promising. Walco et al. (1999) undertook the difficult task of reviewing the literature on interventions for disease related pain, including patients with oncologic, rheumatologic and hematologic disorders. No data pooling was performed and no study met standards from which to extract evidence of treatment effectiveness. Usefuly these authors conclude that there is promise in recent trial work from established research groups (e.g. Gil et al., 2001) and in recent theoretical developments of a psychobiological perspective (Zeltzer et al., 1997).

In this paper, we report a systematic review of the published RCTs of psychological therapy of children and adolescents with chronic pain. Systematic review is a specific approach within evidence based medicine that has three related goals: first, it aims to review comprehensively the world literature. Second, where possible, it aims to produce a statement of the efficacy of specific treatments or classes of treatment. If data are combinable meta-analytic techniques are often appropriate. Where data are not combinable narrative review is appropriate. Finally, the most important aim is to describe the quality of the evidence base for all the interventions in a field, paying attention to the omissions in equal measure to the inclusions (Chalmers and Altman, 1995). This last aim is particularly important because the product of such a systematic review is rarely a clear statement of treatment efficacy in all domains of outcome, but more usually a detailed description of the methodological strengths and weakness of trials that can inform trial development.

We chose to review all trials that examine a psychological intervention, and that have pain as an outcome variable, across all treatments, for all chronic pain conditions. Chronic pain in children and adolescents was accepted as a label for pain that persists or recurs for 3 months or longer in people of 21 years or under. The aim of this review is to provide a systematic overview of the current evidence base for psychological interventions in child and adolescent chronic pain, focussing on methodological details of existing trials. An explicit objective is to provide information and guidance that will help shape the next generation of trials in the field.

2. Methods

2.1. Search strategy

A search was conducted for published RCTs of psychological therapies for children and adolescents with chronic pain. A priori decisions were made to search only for papers reported in full, in peer reviewed journals, and to search electronically across databases. In order to maximise the number of papers we adopted a three-stage search strategy, similar to that employed by Morley et al. (1999). First, the Cochrane register of controlled trials, Medline on Ovid from 1966 to 1999, Psychlit on silver platter from 1987 to 1999, Embase from 1980 to 1999, and the Social Science Indices from 1981 to 1999 were all electronically searched. Full search strategies using the available data-management techniques for each abstracting service are available from the authors on request. This search yielded 3715 papers of which 123 were identified as possible trials and eight were identified as relevant review articles. Nineteen of the 123 papers examined were relevant. Second, reference lists of the recovered 19 articles and eight review papers were searched yielding a further ten papers. Finally, a list of possible papers was compiled and sent to the first named author of each paper and review asking them to cross-check these with their own records for missing trials. This produced one more paper. A total of 30 papers, representing 28 studies was reached. All papers were read initially by four of the authors and a consensus on the suitability of the paper for inclusion in the review was reached, based on whether the paper was an RCT of a psychological treatment for child and adolescent chronic pain. Trials were included
if they had a clearly defined psychological treatment even when this treatment was concomitant with other non-psychological treatments given as standard care. Trials without a clearly defined psychological treatment, for instance, made only mention to a psychological component of a multi-component treatment package, were excluded. Ten studies were discarded: one proved to be a treatment of acute pain in a chronic pain population, five were single case studies, three compared two active treatments without a no-treatment control, and one had fewer than five patients in each group. This procedure left a total of 18 studies considered to be RCTs (see Table 1). Of these 18 RCTs only 13 provided data suitable for meta-analysis (see below for criteria).

2.2. Coding

Coding schemes were developed from the scheme used by Morley et al. (1999) and were piloted on 11 papers. The version used in this study comprised several sections to record the following aspects of study design: Verification of study eligibility e.g. randomisation, appropriate treatment and control groups, sample size \( \geq 5 \) per group, quantitative outcomes; Design and method e.g. statement of inclusion and exclusion criteria, sample sizes from recruitment through to endpoint, demographic characteristics of the sample and caregivers, therapist characteristics, manualisation, manipulation, and credibility checks; Interventions e.g. individual or group treatment, hours of therapy, treatment components – relaxation, biofeedback, behavioural and cognitive interventions, homework and maintenance strategies; Outcomes e.g. details of power calculations, attrition and loss in groups, duration of therapy and follow-up, intention-to-treat or endpoint analysis, measures used; Individual outcomes – details for each identified outcome e.g. reliability, origin of data (self-report, observer, assessor), and available summary statistics. (Copies of the coding scheme are available electronically from the corresponding author.) The resultant coding scheme was applied to all 18 papers from the first three authors. The mean inter-rater reliability across all of the separate measurements was \( \kappa = 0.68 \). Fleiss (1981) suggests that as per a rule of thumb a \( \kappa \) between 0.60 and 0.75 is ‘good’ (0.4–0.6 is ‘fair’ and greater than 0.75 is ‘excellent’). Consensus was reached on non-agreements through discussion.

3. Results

3.1. Trial design

Table 1 gives summary data for the 18 studies. These studies involved 11 lead investigators in five countries. A total of 808 patients entered trials, 438 in treatment conditions, and 370 in comparison conditions. The trials were relatively simple: ten trials used three arms and eight trials used two arms. The most common comparison group was a waiting list control (eight trials). The mean number of subjects entered into each condition was 18 (SD = 11.8, range 6–65). Fifteen trials treated patients with headache, two recurrent abdominal pain, and one sickle cell pain.

The treatments examined were described as relaxation (11 trials), relaxation with biofeedback (four trials), cognitive behavioural therapy (nine trials), and cognitive behavioural family intervention (one trial). Twelve trials took place in a clinic setting and six in a community setting. Treatment contact time was relatively brief with a mean duration of 3 h (range 45 min–9 h, 20 min).

3.2. Trial quality

All the 18 trials reported that randomisation had taken place, but in no case was the exact method of randomisation given. Eight trials explicitly reported restrictions on randomisation e.g. to produce equal numbers per group, or restricting allocation within a school class group. In five other trials randomisation took place after stratification or a matching procedure. Finally, there was one study in which cluster randomisation of whole school classes occurred. None of the studies was double blind. This conventional methodological criterion is inapplicable to most psychological interventions. Eight of the trials stated either that the credibility of the treatment or the expectation of therapeutic improvement had been assessed. In no trial was there mention of the therapeutic allegiance of the trial therapist although this may influence the outcome of the trial.

3.3. Treatment delivery

Ten trials reported treatment delivered to individuals or parent–child couples, in one to one contacts with a therapist. In seven trials a group-based intervention was used: included in this were interventions applied to a whole school class or a subset of members of that class as well as groups which were formed de novo for the purposes of the trial. There was one trial in which individuals received a mixture of individual and group treatment. Eleven of the trials used a treatment manual, and where this was not explicitly stated the authors either referred to another authority on which the treatment was based (e.g. Bernstein and Borkovec, 1973), or gave details of the structure and content of treatment within the paper. Studies that tested the effectiveness of self-help strategies used a combination of manual and audio taped instructions. In contrast to this high level of manualisation, only three studies explicitly mentioned checks for therapist adherence to the manual. One study conducted a partial check on initial sessions.

The trials employed a variety of therapists ranging from undergraduate assistants to experienced psychological and medical personnel. The major group used as therapists were graduate trainees in clinical psychology (six trials). Other trials employed non-psychologists specifically trained for the trials (e.g. school nurses and teachers) to deliver structured interventions. The level of therapist training was not
Table 1: Eighteen studies included in further analyses

<table>
<thead>
<tr>
<th>Study</th>
<th>Setting</th>
<th>Sample description: mean age, age range, gender ratio (f:m)</th>
<th>Groups and sizes (n); number and gender labels</th>
<th>Duration of treatment</th>
<th>Outcome measures</th>
<th>Included in meta-analysis</th>
</tr>
</thead>
<tbody>
<tr>
<td>Barry and von Bayer (1997) Clinic</td>
<td>Clinic</td>
<td>M = 9.4; range = 7–12; gender 19:10</td>
<td>Abbreviated cognitive therapy (12); WLC (17)</td>
<td>3 h</td>
<td>Diary; included; mood; school attendance, activity, medication, pain management strategies used</td>
<td>✔</td>
</tr>
<tr>
<td>Fentress et al. (1986) Clinic</td>
<td>Clinic</td>
<td>M = 10.1; range = not given; gender = 11:7</td>
<td>Relaxation (6); relaxation + biofeedback (6); WLC (6)</td>
<td>9 h</td>
<td>Diary</td>
<td>✔</td>
</tr>
<tr>
<td>Gil et al. (1997) Clinic</td>
<td>Clinic</td>
<td>M = 11.9; range = 10–12; gender = 23:26</td>
<td>Cognitive coping skills (25); standard care (24)</td>
<td>One 45 min session</td>
<td>Modified Coping Strategies Questionnaire; Pain sensitivity test</td>
<td>×</td>
</tr>
<tr>
<td>Griffiths and Martin (1996) Clinic</td>
<td>Clinic</td>
<td>M = 11.3; range = 10–12; gender = 21:21</td>
<td>CBT @ clinic (15); CBT @ home (15); WLC (12)</td>
<td>12 h</td>
<td>Diary; mood (anxiety and depression); coping</td>
<td>✔</td>
</tr>
<tr>
<td>Labbé and Williamson (1984) Clinic</td>
<td>Clinic</td>
<td>M = 10.7; range = 7–16; gender = 14:14</td>
<td>Autogenic biofeedback (14); WLC (14)</td>
<td>6.67 h</td>
<td>Diary; medication</td>
<td>✔</td>
</tr>
<tr>
<td>Labbé (1995) Clinic</td>
<td>Clinic</td>
<td>M = 12.0; range = 8–18; gender = 13:17</td>
<td>Temperature biofeedback + autogenic training (10); autogenic training (10); 7.5 h Dairy</td>
<td>7.5 h</td>
<td>Diary</td>
<td>✔</td>
</tr>
<tr>
<td>Larsson and Carlsson (1996) School</td>
<td>School</td>
<td>M = not given; range = 10–15; gender = 25:1</td>
<td>Relaxation (13); NTC (13)</td>
<td>3.3 h</td>
<td>Diary</td>
<td>✔</td>
</tr>
<tr>
<td>Larsson and Melin (1986) School</td>
<td>School</td>
<td>M = not given; range = 16–18; gender = 30:2</td>
<td>Relaxation (11); information- contact (13); NTC (7)</td>
<td>6.75 h</td>
<td>Diary; medication; mood</td>
<td>✔</td>
</tr>
<tr>
<td>Larsson et al. (1990) School</td>
<td>School</td>
<td>M = not given; range = 16–18; gender = 43:5</td>
<td>Self-help relaxation (31); WLC (17)</td>
<td>1.67 h</td>
<td>Diary; medication; somatic complaints; anxiety; depression; daily stress</td>
<td>✔</td>
</tr>
<tr>
<td>Larsson et al. (1987a) School</td>
<td>School</td>
<td>M = not given; range = 16–18; gender = 35:6</td>
<td>Self-help relaxation (16); therapist assisted relaxation (14); self-monitoring control (11)</td>
<td>6.75 h</td>
<td>Diary; medication; school attendance; mood (anxiety–depression)</td>
<td>✔</td>
</tr>
<tr>
<td>Larsson et al. (1987b) School</td>
<td>School</td>
<td>M = not given; range = 16–18; gender = 34:0</td>
<td>Self-help relaxation (12); problem discussion (10); self-monitoring control (12); e</td>
<td>3 and 7 h</td>
<td>Diary; medication; school attendance; mood (anxiety–depression); psychosomatic symptoms; social relationship</td>
<td>✔</td>
</tr>
<tr>
<td>McGrath et al. (1988) Clinic</td>
<td>Clinic</td>
<td>M = 13.1; range = 9–17; gender = 69:30</td>
<td>Relaxation (32); ‘Own best efforts’ (30); placebo: attention control (37)</td>
<td>6 h</td>
<td>Diary</td>
<td>×</td>
</tr>
<tr>
<td>McGrath et al. (1992) Clinic</td>
<td>Clinic</td>
<td>M = not given; + range = 11–18; + gender = 63:24</td>
<td>CBT self-administered (24); CBT therapist administrated (23); attention control (26)</td>
<td>8 h</td>
<td>Diary; depression</td>
<td>✔</td>
</tr>
<tr>
<td>Osterhaus et al. (1997) Clinic</td>
<td>Clinic</td>
<td>M = 15.2; range = 12–22; gender = 29:10</td>
<td>CBT + temperature biofeedback (24); WLC (15)</td>
<td>9.33 h</td>
<td>Diary</td>
<td>✔</td>
</tr>
<tr>
<td>Passchier et al. (1990) School</td>
<td>School</td>
<td>M = 13.7; range = not given; gender = 65:54</td>
<td>Relaxation – teacher presented (65); placebo – physical exercises (54)</td>
<td>2.5 h</td>
<td>Diary; medication; school problems; fear of failure</td>
<td>×</td>
</tr>
<tr>
<td>Richter et al. (1986) Clinic</td>
<td>Clinic</td>
<td>M = 12.9; + range = 9–18; + gender = 34:17</td>
<td>Relaxation (15); cognitive coping (15); attention control placebo (12)</td>
<td>9 h</td>
<td>Diary</td>
<td>×</td>
</tr>
<tr>
<td>Sanders et al. (1989) Clinic</td>
<td>Clinic</td>
<td>M = 9.0; range = 6–12; gender = not given</td>
<td>CBT (8); WLC (8)</td>
<td>Eight sessions</td>
<td>Diary; parent and observer behaviour ratings; Problem Behaviour Checklist; Connors CBCL</td>
<td>✔</td>
</tr>
<tr>
<td>Sanders et al. (1994) Clinic</td>
<td>Clinic</td>
<td>M = 9.3; + range = 7–14; + gender = 28:16</td>
<td>Cognitive behavioural family intervention (22); standard paediatric care (22)</td>
<td>6 h</td>
<td>Pain diary; interference; parent observation of pain behaviour; child behaviour checklist</td>
<td>✔</td>
</tr>
</tbody>
</table>

a WLC, waiting list control; NTC, no-treatment control; CBT, cognitive behaviour therapy. 
b Numbers based on trial completers unless indicated by + where data are available for entrants only. 
c Used as separate control treatment contrasts in first analysis. 
d Used as contrast in second analysis. 
e Groups pooled in second analysis. 

C. Eccleston et al. / Pain 99 (2002) 157–165
stated in six trials. Only three trials explicitly mentioned that therapists received supervision during the trials. This coupled with the general failure to note whether checks on adherence were made must be considered a weakness when judging the overall quality of the trials.

3.4. Measurement domains

A number of measures were employed in a range of domains of chronic pain. Only pain experience was measured in all trials. Recent practice in the conduct of many trials in medical settings is to provide an unequivocal statement of the primary outcome measure. This has not been the tradition in trials of psychological treatments and this was reflected in these studies. We could find only two trials (McGrath et al., 1992; Larsson and Carlsson, 1996) where the authors explicitly stated a ‘primary’ or ‘major’ outcome. However, given the prevalence of the use and analysis of pain diaries it seems reasonable to infer that pain was the primary outcome variable, but we note that there is considerable uncertainty as to the primary variable used by authors. Most report multiple parametric analyses of frequency, intensity, and duration (but did not provide sufficient information for meta-analysis) as well as the compound Pain Index.

In total, 47 distinct measurement instruments were employed; the modal number of instruments used in each study was one (range one to six). The second most common measurement domain represented was mood, reported in eight trials. Only six papers reported medication use, and only four papers reported school attendance as outcomes. Despite the number of measurement instruments employed sufficient data for statistical meta-analysis were available in only 13 trials for the single domain of pain experience. All trials used a version of a daily pain diary derived from Budzynski’s early research on biofeedback for headache and subsequently widely adopted (Budzynski et al., 1973). The diary records the frequency, duration, and intensity characteristic of pain episodes. There was some variation in the diaries e.g. use of 0–4 or 0–5 scale points, frequency of rating (1, 3 or 4 times per day but most authors report using some transformation to capture a function of the total amount of pain experience which we denote as the Pain Index). The most common transformation was a simple summation of intensity ratings over a set period, usually a week. While the precise scaling of the diary data varies, outcomes were expressed as a ratio representing the percentage change from baseline in the Pain Index. In all studies where this metric was reported the authors use a 50% reduction in the Pain Index as a criterion of clinically significant improvement. This value is widely used in many studies of treatment for acute pain (McQuay and Moore, 1998) and enables the outcome to be expressed as the number of participants in each group achieving a clinically significant gain. These data were reported as a dichotomous outcome variable (improved vs. unimproved) for treatment and control groups in 13 studies and are therefore susceptible to meta-analysis using odds ratios (OR) as the outcome statistic. A number of papers reported continuous (mean, SD) outcome data but in most cases the additional data (SD and/or sample size) were not available in a form suitable for analysis, and attempts to retrieve suitable data from authors were not successful.

3.5. Meta-analysis

Our intended analytic strategy was to explore the effects of treatments on a range of outcomes, but we were unable to do this because of limited analysable data. For example, although mood was assessed in a number of trials, and in general the mood scores decreased from pre- to post-treatment assessments for both treatment or control groups, these data are not of a quality to perform statistical meta-analysis, due largely to incomplete reporting, or the use of unreliable measurement instruments. Pain experience data were, however, analysable. We conducted two analyses using a random effects model. The pooled OR was computed using the DerSimonian and Laird (1986) method. Tests for combinability, bias detection (OR against 1/trial weight), and funnel plots were examined for the pooled data. The tests for combinability indicated that the data could be legitimately pooled but both the bias detection and funnel plots suggested that the OR was systematically related to the size of the trials with smaller trials having larger ORs. The first analysis was the comparison between a treatment group and the designated control. For studies with more than one arm the data are confounded because there is a common control group used to compute the effect size. A second analysis pooled the treatment arms within each study and estimated a common treatment effect against a single control group. This procedure assumes that the outcome of the treatment groups is similar irrespective of differences in treatment content. Indeed, there was no evidence within the trials of differences between treatments. The small number of trials precluded an analysis based on variations in the content of control groups, i.e. active treatment controls vs. waiting list and vs. no treatment controls.

The first analysis gave a pooled OR = 9.2 (approximate 95% CI = 5.64–15.00; $\chi^2 = 78.97$, d.f. = 1, $P < 0.0001$). The second analysis produced a similar estimate: pooled OR = 9.62 (approximate 95% CI = 5.17–17.92; $\chi^2 = 50.95$, d.f. = 1, $P < 0.0001$). A partial Cochrane plot for the latter analysis is shown in Fig. 1. Both analyses indicate that psychological treatments are effective when compared with a pooled group of control conditions. As OR are not intuitively easy to interpret we also computed the number needed to treat (NNT) statistic from the pooled data set. The NNT = 2.32 (95% CI 1.96–2.88), which implies that therapists need to treat just over two people for one to benefit who would not have done so in the non-treatment control condition. Inspection of the ORs for studies conducted in the clinic and those conducted in the community showed no
systematic difference according to setting. We also inspected the ORs associated with various methods of recording the pain diary. There appeared to be no systematic variation of OR with method and the subsamples were too small for statistical analysis.

4. Discussion

4.1. Resume

From the 30 papers reviewed, 18 trials met criteria for inclusion in the review and 13 provided data suitable for meta-analysis. The trials were relatively simple in design, with one or two treatment conditions in comparison to a waiting list, standard care or placebo control group. No trial was fully blinded, indeed, this criterion that is generally applied to evaluate pharmacological interventions would seem to be largely misplaced in the trials of psychosocial interventions. Nevertheless, it is important for the design of these trials to consider ways in which the equivalence of the trial arms can be assessed. This requires the careful design of control treatments (Schwartz et al., 1997) and the assessment of critical components. However, in the sample of trials reported in this study credibility of the therapy, therapist allegiance, therapist training, allocation of therapist to treatments, and supervision of therapists were rarely mentioned. The use of treatment manuals was relatively common but adherence to manuals was measured in only three studies. Although 47 separate measurement instruments were used covering the range of chronic pain experience, data from these instruments were rarely reported in full. Domains of measurement that one might have expected to be well reported such as function or school attendance were either not included or not reported. Pain was the most common domain assessed, followed by mood. Twelve papers provided self-report data of pain severity in children and adolescents with headache and one paper reported data in children with recurrent abdominal pain. There was an NNT of 2.32 for psychological therapies producing more than 50% relief in pain. This compares favourably with other published NNTs in chronic pain (McQuay and Moore, 1998). There was only one non-headache trial with meta-analysable data and there were insufficient non-pain outcome data from all trials to subject to meta-analysis.

A striking feature of the set of trials examined was the inclusion of six community trials, five from a Scandinavian research group working in a school setting. There were no differences between studies conducted in the clinic and the community.

We made a number of decisions that should be taken into account when interpreting the findings (Fishbain et al., 2001; Thompson et al., 1995). Hand searching was not undertaken, but multiple electronic databases and author...
contact were used. In line with the primary questions, we chose to include a heterogeneous sample of treatments and patients. Data were independently abstracted and reliability of the data extraction measured. Reliable study quality criteria were employed. Both effect sizes and NNTs were calculated and the heterogeneity between studies was controlled for. Where insufficient data were reported these were simply omitted from the review.

4.2. Current status and future investigations

The provision of a reliable evidence base of paediatric pain treatments is a shared goal of the paediatric pain research community. If one takes account of the complexities involved in undertaking psychological research in paediatric chronic pain, we would argue that the current status of the evidence base for psychological treatments is good. Particularly impressive was the use of simple designs and the use of well-validated pain measures. This systematic review raises a number of implications for the design of trials of psychological therapy for children with chronic pain.

First, the lack of a tradition in psychological research for specifying primary outcomes causes problems for interpretation. In the present studies, no data were analysable from non-pain outcome domains suggesting that pain relief alone was the most important goal of treatment. In some trials pain relief may be the only target. However, children, adolescents, and their families normally present for treatment with more than only pain severity and frequency as primary complaints (McGrath and Hillier, 2001). For many, improvements in pain relief, function, and mood are legitimate outcomes of psychological therapies. The current evidence base does not allow for a judgement of the effectiveness of psychological therapy in improving mood, function, and school attendance, nor in reducing disability.

All studies included and reported standardised pain data. However, there is no similar standardised multidimensional instrument designed to measure the impact of chronic pain on the life of a child or adolescent or family members. Instead, in all of the reviewed studies single domain instruments were employed. The development of a multidimensional outcome tool may provide agreement on the importance of including domains of pain experience other than pain severity and frequency.

Second, a striking result of this study was the finding that therapy contact was typically individual, brief, and well manualised. The relatively frequent mention of manualisation may be due to the brief skill-based type of interventions. For example, the 13 treatments that were labelled relaxation were relatively well defined and referenced. In contrast, the more time consuming 11 treatments that were labelled as having a cognitive component were more poorly described. The active components of psychological treatments labelled cognitive behavioural therapy for children remain somewhat of a mystery. To better specify the content of therapy will strengthen the evidence base.

Third, a relatively small number of patients have been exposed to controlled treatments (n = 438) with an average sample size of an active treatment arm of 15 patients. However, what will be of interest to the development of the field is a further level of analysis to determine exactly which treatment components are effective for which patients on which outcomes. We recognize that this is not a problem specific to chronic pain but is true for many child and adolescent treatment domains (Durlak et al., 1995; Kazdin and Weisz, 1998). It is unlikely that treatment components will match directly to specific outcomes. Trials that assess the addition of a psychological treatment to existing non-psychological treatments are required (e.g. Olness et al., 1987). This second level of analysis will require studies with larger sample sizes. Multicentre trials may be a necessity in this field.

Fourth, there is an urgent need for well-designed, conducted, and reported trials of behavioural and cognitive behavioural therapies for children and adolescents with chronic pain other than headache, in which the management of pain-associated disability and distress is a primary aim of treatment (Walco et al., 1999). The current evidence base is representative of patients mostly with headache that does not lead to chronic limitations on social and family functioning, and that does not lead to prolonged psychological distress. Trials are required for children and adolescents whose complaints are of chronic pain that is more severely limiting and distressing, in which pain relief is not the only, and not always the important outcome. Important to consider will be the design of trials that focus treatment not upon the child or adolescent alone, but on combinations of parents, carers, and children (Malleson et al., 2001).

Finally, the apparent effectiveness of simple psychological interventions for headache applied in school settings raises the possibility of whether such interventions could be applied as a method of secondary prevention to ameliorate the development of chronicity in the population. The simplicity of the intervention and the apparent ease with which non-pain specialists can implement it suggests the possibility that a preventive psychosocial intervention could be developed (Masek, 1999).

4.3. Conclusions

There is insufficient evidence to judge the effectiveness of psychological therapies in improving mood, function, or disability associated with chronic pain in children and adolescents. Well-designed and comprehensively reported RCTs of psychological therapy for non-headache chronic pain in children and adolescents are urgently needed. There is strong evidence that psychological treatments, principally relaxation and cognitive behavioural therapy, are highly effective in reducing the severity and frequency of chronic pain in children and adolescents. There is a strong
References


