Interventions for prevention and treatment of childhood obesity typically target increases in physical activity and, more recently, reductions in physical inactivity (sedentary behaviour such as television viewing). However, the evidence base for such strategies is extremely limited. The main aim of the present review was to update the systematic review and critical appraisal of evidence in the light of the recent rapid expansion of research in this area. Randomised controlled trials (RCT) that targeted activity or inactivity, that followed up children or adolescents for at least 1 year and that included an objective weight-related outcome measure were included. Trials were appraised using previously published criteria (Harbour & Miller, 2001), and literature search strategies described previously (Reilly et al. 2002) were updated to May 2002. A total of four new RCT, two new systematic reviews and one meta-analysis were identified. The evidence base has increased markedly since the completion of earlier reviews, although high-quality evidence is still lacking. The evidence on childhood obesity prevention is not encouraging, although promising targets for prevention are now clear, notably reduction in sedentary behaviour. There is stronger evidence that targeting activity and/or inactivity might be effective in paediatric obesity treatment, but doubts as to the generalisability of existing interventions, and the clinical relevance of the interventions is unclear. Further research in settings outside the USA is urgently needed, and two ongoing RCT in Scotland are summarised.

Obesity prevention: Obesity treatment: Systematic review: Childhood: Adolescence

Impact of childhood obesity

The developed world has experienced an epidemic of childhood obesity in recent years (Reilly & Dorosty, 1999; Strauss & Pollack, 2001), and rapid increases in obesity prevalence have also been observed in some developing countries (Martorell et al. 2000). The epidemic is of great concern because of its likely clinical and public health impact (Reilly et al. 2003), and there is increasing recognition of the problem; at least eighteen editorials or position statements have been published in paediatric, nutrition and general medical journals since 1998 (Dorosty, 2001).

Role of physical activity and inactivity in the aetiology of childhood obesity

It is widely believed that reduced physical activity and/or increasing sedentary behaviour, such as television viewing, is implicated in the aetiology of childhood obesity (Troiano & Flegal, 1998; Reilly & Dorosty, 1999). Definitive evidence of a role for reduced physical activity and energy expenditure in the causation of obesity is lacking, but the ‘circumstantial’ evidence for an important role is now substantial. First, in most developed countries increases in obesity prevalence have occurred in parallel with declines...
in children’s energy intakes (Gregory et al. 1995; Troiano & Flegal, 1998). Second, in both cross-sectional and longitudinal studies exposure to sedentary behaviour (television viewing) has consistently been associated with increased paediatric obesity risk (for example, see Gortmaker et al. 1996), and sedentary behaviour has probably increased substantially in recent years in children and adolescents. Direct evidence of reduced activity or total energy expenditure (TEE) is lacking, because the necessary cross-sectional surveys have not been carried out and most published studies have used small highly-selected samples. Our own recent work in Scottish children measuring TEE by the doubly-labelled water method and physical activity and inactivity by accelerometry has focused on larger, representative, samples. These studies indicate that levels of TEE in young children are very low (Montgomery et al. 2002) and levels of sedentary behaviour exceptionally high (Jackson et al. 2002). Third, in certain paediatric groups characterised by increased obesity risk, such as the survivors of some cancers, TEE and energy expended on activity are abnormally low and predict risk of excess weight gain prospectively (for example, see Reilly et al. 1998). Finally, interventions that alter exposure to activity, or inactivity, support the hypothesis that increased sedentary behaviour is implicated in the aetiology of childhood obesity (Robinson, 1999a). Interventions that change activity, or inactivity, also add considerably to the effects that can be achieved by dietary treatment alone (Epstein et al. 1998, 2000).

Role of intervention studies

Intervention studies can be used, therefore, to test hypotheses in relation to the aetiology of obesity, but they have important and more obvious roles in testing strategies for prevention and treatment of obesity. However, the evidence base for treatment and prevention strategies is extremely limited at present (Campbell et al. 2001a; Reilly et al. 2002; Summerbell et al. 2002), and the effectiveness of all available strategies must be considered questionable. Our recent review concluded that there are no successful generalisable evidence-based intervention strategies at present (Reilly et al. 2002). Evidence on modifiable risk factors for obesity has also provided few evidence-based targets for prevention and treatment of childhood obesity (Reilly et al. 2002). Evidence appraisal used methodology described elsewhere (Harbour & Miller, 2001), which is widely available via the internet (Scottish Intercollegiate Guidelines Network, 2002). In summary, evidence that was relevant to the prevention and treatment of paediatric obesity was identified using inclusion or exclusion criteria given later. Two reviewers then appraised each study independently and agreed on a rating of methodological quality. This rating (Harbour & Miller, 2001) defines RCT as providing ‘level 1’ evidence, the highest grade of evidence available (hierarchy of evidence in descending order to level 4, expert opinion). Each published study was rated as: ++, all or most methodological criteria met, low risk of bias; +, some criteria not met or inadequately described, low risk of bias; −, few or no criteria met, high risk of bias; rejected (Harbour & Miller, 2001).

Many published studies have been given a negative quality rating (high risk of bias) in previous critical appraisal exercises in this area. For example, in our previous systematic review (Reilly et al. 2002) sixteen RCT of obesity treatment were identified but thirteen had major methodological flaws. This assessment may seem excessively critical, but older RCT, carried out before the emergence of the Cochrane Collaboration and the CONSORT statements (Moher et al. 2001) on the design, conduct and reporting of RCT, were very weak and prone to bias. This appraisal can be illustrated by listing the methodological quality criteria used by the present and the earlier review (Reilly et al. 2002;
were analysed in the groups to which they were randomly allocated? How high was dropout rate? Were relevant outcomes measured in valid and reliable ways? Apart from the treatment being investigated, were groups treated equally? Were subjects and/or investigators kept ‘blind’ to treatment allocation? Was allocation concealed? Does the study address a clear question or aim? Were subjects and/or investigators kept ‘blind’ to treatment allocation? Was allocation concealed?

Table 1. Criteria for evaluation of randomised controlled trials* (adapted from Scottish Intercollegiate Guidelines Network, 2002)

<table>
<thead>
<tr>
<th>Criteria</th>
<th>Rating</th>
</tr>
</thead>
<tbody>
<tr>
<td>Does the study address a clear question or aim?</td>
<td>1</td>
</tr>
<tr>
<td>Were subjects allocated randomly to treatment groups?</td>
<td></td>
</tr>
<tr>
<td>Was allocation concealed?</td>
<td></td>
</tr>
<tr>
<td>Were subjects and/or investigators kept ‘blind’ to treatment allocation?</td>
<td></td>
</tr>
<tr>
<td>Were treatment and control groups similar at the start of the trial?</td>
<td></td>
</tr>
<tr>
<td>Apart from the treatment being investigated, were groups treated equally?</td>
<td></td>
</tr>
<tr>
<td>Were relevant outcomes measured in valid and reliable ways?</td>
<td></td>
</tr>
<tr>
<td>How high was dropout rate?</td>
<td></td>
</tr>
<tr>
<td>Were subjects analysed in the groups to which they were randomly allocated?</td>
<td></td>
</tr>
<tr>
<td>Was a power calculation carried out?</td>
<td></td>
</tr>
<tr>
<td>Was sample size adequate?</td>
<td></td>
</tr>
<tr>
<td>What is the likely direction of study bias?</td>
<td></td>
</tr>
</tbody>
</table>

Studies rated as 1—meet few or any of these criteria adequately.

Inclusion and exclusion criteria in present review

Only the RCT that followed up children or adolescents for at least 12 months from the start of the intervention were included. This condition is a common and important inclusion criterion (Campbell et al. 2001b; Reilly et al. 2002; Summerbell et al. 2002). The rationale is that almost all interventions in this area are grounded in lifestyle changes and the behavioural change literature shows consistently that short-term lifestyle changes can be made relatively easily, but are difficult to sustain. Short-term studies in this area are therefore prone to bias. Indeed, a number of high-quality studies with 1–2 year follow-up periods have concluded that their studies were limited in this respect, and that even longer follow up would have been desirable (for example, see Luepker et al. 1996; Gortmaker et al. 1999; Epstein et al. 2000; Sahota et al. 2001). Short-term studies can usefully test hypotheses in relation to the role of activity or inactivity in the causation of obesity, and can be of high methodological quality (for example, see Robinson, 1999a), but do not provide definitive evidence on the long-term efficacy of intervention strategies. For inclusion, studies on prevention had to have included non-clinical groups of subjects (recruited from nursery, school or community). Studies on obesity treatment had to have children defined as obese using objective criteria. Both studies of prevention and treatment had to have included objective outcome measures of body weight, BMI or body composition. All papers that met our entry criteria were appraised, but studies that were given a negative quality rating in the present review have not been summarised.

Table 1. Criteria for evaluation of randomised controlled trials* (adapted from Scottish Intercollegiate Guidelines Network, 2002)

| Does the study address a clear question or aim? | 1 |
| Were subjects allocated randomly to treatment groups? | |
| Was allocation concealed? | |
| Were subjects and/or investigators kept ‘blind’ to treatment allocation? | |
| Were treatment and control groups similar at the start of the trial? | |
| Apart from the treatment being investigated, were groups treated equally? | |
| Were relevant outcomes measured in valid and reliable ways? | |
| How high was dropout rate? | |
| Were subjects analysed in the groups to which they were randomly allocated? | |
| Was a power calculation carried out? | |
| Was sample size adequate? | |
| What is the likely direction of study bias? | |

Studies rated as 1—meet few or any of these criteria adequately.
school environment, although one arm of CATCH tested the impact of a home-based intervention. Results of these large-scale long-term interventions are not encouraging. CATCH reported no marked change in BMI at follow up, although data were not presented in terms of changes in BMI SD scores, or changes in overweight or obesity prevalence (Luepker et al. 1996). There was some evidence of increases in intensity of physical education classes in the intervention schools relative to control schools and the process evaluation of CATCH indicated that implementation of the intervention was good, and was sustained over three school years. Despite these positive changes, the impact of the intervention on primary and secondary outcomes was limited. The authors concluded that this result could reflect a combination of factors, including inadequate power, insufficient duration of follow up and the difficulty of making effective changes to the environment. The Planet Health Study (Gortmaker et al. 1999) reported significant declines in obesity prevalence in girls in the intervention groups (adjusted odds ratio 0·47, 95 % CI 0·24, 0·93, P<0·05) and significantly greater remission of pre-existing obesity in girls (adjusted odds ratio 2·16, 95 % CI 1·07, 4·35, P<0·05). There was some evidence that the intervention effects were mediated by reductions in television viewing targeted by the intervention. The obesity intervention effects were mediated by reductions in television viewing, although data were not presented in terms of changes in BMI SD scores, or changes in overweight or obesity prevalence (Luepker et al. 1996). There was some evidence of increases in intensity of physical education classes in the intervention schools relative to control schools and the process evaluation of CATCH indicated that implementation of the intervention was good, and was sustained over three school years. Despite these positive changes, the impact of the intervention on primary and secondary outcomes was limited. The authors concluded that this result could reflect a combination of factors, including inadequate power, insufficient duration of follow up and the difficulty of making effective changes to the environment. The Planet Health Study (Gortmaker et al. 1999) reported significant declines in obesity prevalence in girls in the intervention groups (adjusted odds ratio 0·47, 95 % CI 0·24, 0·93, P<0·05) and significantly greater remission of pre-existing obesity in girls (adjusted odds ratio 2·16, 95 % CI 1·07, 4·35, P<0·05). There was some evidence that the intervention effects were mediated by reductions in television viewing targeted by the intervention. The obesity outcomes did not change significantly in boys, although sample size or power may have been a limiting factor. Process evaluation indicated that the Planet Health intervention was generally implemented well and was sustained by schools.

Evidence appraisal and summary: obesity treatment

The literature review for the present study identified two papers that met our inclusion criteria: one new RCT (Warschburger et al. 2001) and one meta-analysis on the impact of exercise as a treatment (Le Mura & Maziekas, 2002). The recent Cochrane review was also noted (Summerbell et al. 2002).

The study by Warschburger et al. (2001) provided no information on the randomisation process, and the issues of blinding and concealment were not addressed. Subject drop-out rate was not reported, and there was no evidence of an ‘intention to treat’ analysis. Subjects varied widely in age at trial entry and it is unclear whether there were differences in treatment (other than the intervention) between groups. There was some evidence of differences between groups at baseline. The study was also dependent on some outcome measures that are not readily measurable.

The meta-analysis (Le Mura & Maziekas, 2002) also received a negative quality rating, largely on the grounds (Scottish Intercollegiate Guidelines Network, 2002) that the search strategy was incomplete, and there was no attempt to formally appraise the quality of studies included in it. The meta-analysis was heavily dependent on short-term studies, and a number of study designs were entered, including non-randomised trials and uncontrolled trials. Biases in this meta-analysis are likely to inflate the apparent efficacy of treatment, and its conclusions may have been more positive about the effect of exercise in obesity treatment than is warranted by current evidence.

A brief summary of the high-quality obesity treatment RCT identified in our previous review (Reilly et al. 2002) is appropriate, in view of the absence of new high-quality evidence. The RCT employed complex interventions (Table 3) that were fairly intensive in terms of their resource utilisation (clinic and therapist time, access to specialist groups of health professionals), and were from the same research group in the USA. Epstein et al. (1995) compared the ‘traffic light’ diet with one of three randomly-allocated treatments: (1) targeted reductions in sedentary behaviour; (2) targeted increases in aerobic activity; (3) combination of treatments 1 and 2. There were significant differences in outcomes between the three groups (P<0·05), with the greatest change in percentage overweight at +4 and +12 months in the group targeted for reductions in sedentary behaviour (e.g. mean –20 % change in percentage overweight at +12 months v. –12 % in the group targeted for increases in activity). The clinical relevance of these changes is unclear, but this study highlights the potential value of targeting sedentary behaviour in the treatment of childhood obesity. It may also be a more practical and realistic target than aerobic activity for health professionals to focus on. A more recent high-quality RCT by the same research group (Epstein et al. 2000), which involved twenty treatment sessions over a 6-month period, provided further support for the treatment effects of controlling sedentary behaviour (Table 3).
Table 3. Summary of obesity treatment trials rated 1+*

<table>
<thead>
<tr>
<th>Trial</th>
<th>Subject characteristics at baseline</th>
<th>Nature of intervention or target</th>
<th>Drop-out rate (%)</th>
<th>Primary outcome(s)</th>
<th>Duration of follow up (months)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Epstein et al.</td>
<td>Age (years) 8–12, n 61, Location USA</td>
<td>Traffic light diet plus ↓ sedentary behaviour or ↑ physical activity, or both</td>
<td>10</td>
<td>Percentage overweight, aerobic capacity, dietary intake, activity (self reported)</td>
<td>12</td>
</tr>
<tr>
<td>(1995)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Epstein et al.</td>
<td>Age (years) 10–5, n 90, Location USA</td>
<td>Traffic light diet plus: ↓ Sedentary: Low dose (&lt;10 h/week) High dose (&lt;20 h/week) ↑ Activity: Low dose (16 km/week) High dose (32 km/week)</td>
<td>16</td>
<td></td>
<td>24</td>
</tr>
<tr>
<td>(2000)</td>
<td></td>
<td></td>
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↓, Reduce; ↑, increase.
*These studies were rated as 1+ and met all or most of the criteria listed in Table 1 (Scottish Intercollegiate Guidelines Network, 2002).

Discussion

Major implications of present review

The present study represents the most up to date systematic review in this area. Increasing awareness of the problem of paediatric obesity, combined with the associated increase in funding opportunities for research, has led to a rapid increase in the amount of evidence available in recent years. However, the increase in evidence over the past 2 years has consisted largely of studies that did not meet our entry criteria (particularly short-term studies, which remain common). Of those that were eligible for inclusion, most were prone to bias as a result of limitations in their design, conduct and/or reporting (Moher et al. 2001). Systematic reviews must be updated, and this process is particularly important in the rapidly-developing field of obesity prevention and treatment (Campbell et al. 2001b; Summerbell et al. 2002). An important strength of the present review was the formal critical appraisal of published evidence. Published evidence should be formally appraised and graded if it is to inform clinical and public health approaches adequately (Harbour & Miller, 2001).

There remains serious doubt as to the long-term efficacy, clinical relevance, and generalisability of published interventions in this area. The clinical relevance of treatment interventions is difficult to address and perhaps, as a result, rarely addressed. Most of the published interventions of high quality are from the USA, and most of these (at least for obesity treatment) are from the same research group. There is an urgent need to test the efficacy of treatment and prevention strategies in other settings; can the principles of successful treatment be adapted for use in other circumstances? In the UK, for example, the amount of health professional time available for treatment of paediatric obesity is limited, and access to other professional groups that have been important in published studies (e.g. clinical psychologists) is even more limited. There are also training needs; most health professionals involved in treatment of childhood obesity in the UK are unaware of recent evidence in this field, and there is confusion over basic issues such as diagnosis of obesity (Reilly, 2002b).

Potential for adverse effects of interventions

Concern is often expressed that paediatric obesity prevention or treatment may produce adverse effects (particularly on psychological health), but studies that have included adverse outcome measures, such as measures of psychological health or growth retardation, have generally failed to find evidence of adverse effects (Mellin et al. 1987; Epstein et al. 2001b; Muller et al. 2001; Sahota et al. 2001). If handled sensitively, interventions may not exacerbate the existing stigmatisation of obese children (Reilly et al. 2003), and may not increase health risks for non-obese children. However, all interventions should consider the possibility of causing harm and should quantify this factor if possible (Scottish Intercollegiate Guidelines Network, 2002).

Suggestions for further research: prevention

As noted earlier, high-quality ‘long-term’ RCT on paediatric obesity prevention have produced promising targets for intervention (notably control of sedentary behaviour), although doubt remains as to the long-term efficacy of current strategies. One promising approach is to focus the intervention wholly on inactivity. At present this approach has been limited to short-term studies (for example, see Robinson et al. 1999a), but it does appear to produce benefits. One attraction of the approach is that the intervention ‘effort’, which is usually limited by time and/or financial considerations, can be concentrated at a single target. Interventions that target a wider range of behaviours may experience an extent of dissipation of intervention effort in each of the behaviours and so reduce the overall impact of the intervention (for example, see Sahota et al. 2001). In addition, a number of published RCT in obesity prevention, and increasingly in obesity treatment, have targeted behaviours that are peripheral to the aetiology or management of obesity, such as consumption of fruit and vegetables (for example, see Epstein et al. 2001a; Sahota et al. 2001). Focusing on sedentary behaviour is more likely to target the intervention effort on the factor that is causally linked to obesity development and maintenance (reduced physical activity). This approach would also bring...
the benefit of focusing trial outcomes on more measurable variables such as activity or inactivity. Most dietary outcome measures are not measurable with any confidence, and the practice of comparing one dietary assessment method of unknown validity against another of unknown or doubtful validity to provide a ‘validation’ remains common. An additional consideration is that the burden that dietary assessment places on subjects tends to increase drop out. Targeting sedentary behaviour may also have effects on a range of behaviours; there is some evidence that it can reduce energy intake, by changing eating habits, in both obesity prevention and treatment (Robinson, 1999a; Epstein et al. 2000). Evidence-based targets of prevention efforts are limited at present (Dietz, 2001), and it seems appropriate to focus interventions on those modifiable variables that are evidence based and most closely associated with the development of positive energy balance. Finally, it should be noted that inactivity is best considered as a distinct construct from activity. Determinants of activity and inactivity are likely to be different, and the more traditional model of targeting physical activity is not essential for paediatric obesity prevention (Dietz, 1996; Robinson, 1999a).

**Ongoing research: obesity prevention**

A major British Heart Foundation-funded obesity prevention RCT is now underway in Scotland, the Movement and Activity Glasgow Intervention in Children (MAGIC) Study. The intervention consists of a 24-week programme of structured physical activity that aims to both increase activity levels and improve basic motor skills. The intervention is delivered three times per week (90 min/week) in 3–4 year olds who attend nursery schools. The nursery-based intervention is supplemented with a home-based health education element that targets the reduction of sedentary behaviour. As noted earlier, our previous studies of TEE and physical activity (and inactivity) in representative samples of Scottish preschool children have shown that TEE is extremely low (Montgomery et al. 2002) and engagement in inactivity high (Jackson et al. 2002). These behavioural risk factors for obesity seem appropriate, therefore, for modification, particularly with the short- and long-term benefits that school-based interventions may bring (Story, 1999). Approximately 220 children have been randomised to the intervention and 220 to the control group, using a cluster randomisation design. The primary outcome is change in BMI SD score measured at baseline, +6 months and +12 month follow up. Secondary outcomes include the measurement of physical activity (Fairweather et al. 1999) and inactivity (Reilly et al. 2001a) by accelerometry, body fat distribution, body composition, blood pressure and motor skills. Researchers blinded to trial allocation will measure all outcomes. A pilot study for MAGIC, conducted over 12 weeks in sixty children, indicated that the intervention was associated with a significant improvement in both total activity (40 % increase in accelerometry output, as an index of total physical activity, on days on which the intervention was delivered, and 29 % increase on days on which it was not, \( P < 0.001 \) in both cases; Fig. 1). The intervention was also associated with a significant \( (P < 0.01) \) improvement in motor skills (Fig. 1). Furthermore a process evaluation was encouraging: the intervention was easily implemented in the nursery school, well attended, enjoyed by nursery staff, and available at low cost. First results on the efficacy of the intervention from the MAGIC study should be available in 2005. The MAGIC study was intended to avoid the design problems that have affected older RCT in obesity prevention, and the aim was to test the efficacy of a simple low-cost intervention that could be implemented.
more widely with minimal logistical problems. The secondary outcomes included in the design were intended to address the problems of outcome measurement noted in previous Cochrane reviews (Campbell et al. 2001b; Summerbell et al. 2002), to provide information on compliance with the intervention and the mechanisms underlying any effects. Major methodological difficulties have hampered research in this area and these issues are discussed later.

Suggestions for further research: treatment

Many of the comments made earlier in relation to interventions aimed at preventing childhood obesity apply equally to interventions aimed at treatment. Targeting sedentary behaviour appears to provide a treatment benefit that is greater than targeting lifestyle activity, and both strategies add considerably to the benefits of dietary treatment (Epstein et al. 1995, 1998, 2000; Robinson, 1999b). In addition to the major study design problems in older RCT in this area, there is a serious methodological problem; practical valid outcomes for many of the variables of interest are lacking, or need further methodological evaluation (Summerbell et al. 2002). Measurement of dietary energy intake in children is generally less prone to bias than in adults (Hill & Davies, 2001), but a wealth of evidence has shown that obesity produces similar reporting bias to that in adults, and this conclusion is true even in relatively young children (for example, see Maffei et al. 1994). In addition, even the best available dietary assessment methods are time consuming, yet fairly imprecise, and accurate only at the group level (Reilly et al. 2001b). Further research on dietary assessment methodology may be of limited value if the attainable limits of precision and accuracy have been reached (Reilly et al. 2001b), and dietary intake data from obese children must be regarded as misleading. Other outcomes are also problematic; there is good evidence that body composition is more informative for intervention studies than proxies for body composition (such as body weight or BMI) in paediatric research (Reilly, 1998). However, methodology available is of limited accuracy (Wells et al. 1999), and can present practical problems (Reilly, 1998). At present, no simple method has been validated against a multicomponent reference (Reilly, 1998; Wells et al. 1999).

Methodological research on the measurement of physical activity and inactivity is more promising. Limitations in older self-report methodology have seriously weakened research in this area (Robinson, 1999a; Summerbell et al. 2002). The advent of objective methods such as accelerometry provides the possibility of valid and precise measurement of both activity (Fairweather et al. 1999) and inactivity (Reilly et al. 2001a), which is practical even in young children and in large-scale interventions.

Ongoing research: obesity treatment

A major Scottish Executive Health Department-funded obesity treatment RCT, the Scottish Childhood Obesity Treatment Trial, is underway in Scotland. The trial is intended to test the hypothesis that the behavioural change strategies that have been shown to be successful in high-quality studies in the USA can be successfully adapted to the typical clinical setting in the Scottish Health Service, and can be delivered by a single health professional (a dietitian). The intervention consists of a 24-week programme that targets sedentary behaviour and employs a modified version of the ‘Traffic Light’ diet (Epstein et al. 2000), using the principles of behavioural change outlined in previous studies (for example, see Mellin et al. 1987; Epstein et al. 2000). The primary outcome of the study is change in BMI SD score, measured at baseline, +6 and +12 months. Secondary outcomes include measurement of physical activity and inactivity (by accelerometry), blood pressure, body fat distribution, body fatness and adverse effects (on growth and psychological health). Researchers who are blinded to trial allocation will measure all outcomes. The trial aims to enter approximately 110 6–10-year-old children to treatment or (typical care) control groups. Typical care consists of the low-intensity very general dietetic advice on healthy diet and increasing physical activity. First results from the Scottish Childhood Obesity Treatment Trial should be available in 2005.

Conclusions

The evidence base for interventions in childhood activity, with the aim of prevention or treatment of obesity, remains limited. Simple, effective and generalisable interventions are lacking. This position reflects a combination of factors: the recent emergence of the childhood obesity epidemic, which was not obvious in the UK until 1999 (Reilly & Dorosty, 1999); limitations in trial design before the advent of the Cochrane and CONSORT processes; the major difficulties (methodological, financial, practical, ethical) presented by carrying out long-term research in this area. A wealth of new research is likely to improve our ability to address the childhood obesity epidemic, but this research should attempt to build on previous evidence and employ the most rigorous trial design available.

A number of previous studies have noted the possibility that the large-scale societal changes that have driven the obesity epidemic may be beyond the reach of interventions aimed only at the family or school environment (Luepker et al. 1996; Gortmaker et al. 1999; Sahota et al. 2001). Large-scale policy or strategic initiatives, employing macroenvironmental or ‘ecological’ approaches, may be essential if the public health impact of the childhood obesity epidemic is to be addressed (Koplan & Dietz, 1999; Swinburn et al. 1999). However, any such initiatives should also be evidence based and should be evaluated rigorously.

Acknowledgements

The pilot study for our RCT of obesity prevention and our research on the aetiology of childhood obesity was funded by Sport Aiding Medical Research for Kids (SPARKS). Ongoing RCT are funded by the British Heart Foundation and the Scottish Executive Health Department. We thank the Scottish Intercollegiate Guidelines Network for literature searching and Dr Carolyn Summerbell for sharing and discussing the results of Cochrane reviews.
References


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