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School dental screening programmes for oral health (Review)

Arora A, Khattri S, Ismail NM, Kumbargere Nagraj S, Prashanti E

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[Intervention Review]

School dental screening programmes for oral health

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ABSTRACT

Background

School dental screening refers to visual inspection of children's oral cavity in a school setting followed by making parents aware of their child's current oral health status and treatment needs. Screening at school intends to identify children at an earlier stage than symptomatic disease presentation, hence prompting preventive and therapeutic oral health care for the children. This review evaluates the effectiveness of school dental screening in improving oral health status.

Objectives

To assess the effectiveness of school dental screening programmes on overall oral health status and use of dental services.

Search methods

Cochrane Oral Health's Information Specialist searched the following databases: Cochrane Oral Health's Trials Register (to 15 March 2017), the Cochrane Central Register of Controlled Trials (CENTRAL, the Cochrane Register of Studies, to 15 March 2017), MEDLINE Ovid (1946 to 15 March 2017), and Embase Ovid (15 September 2016 to 15 March 2017). The US National Institutes of Health Trials Registry (ClinicalTrials.gov) and the World Health Organization International Clinical Trials Registry Platform were searched for ongoing trials. No restrictions were placed on language or publication status when searching the electronic databases; however, the search of Embase was restricted to the last six months due to the Cochrane Centralised Search Project to identify all clinical trials and add them to CENTRAL.

Selection criteria

We included randomised controlled trials (RCTs) (cluster or parallel) that evaluated school dental screening compared with no intervention or with one type of screening compared with another.

Data collection and analysis

We used standard methodological procedures expected by Cochrane.

Main results

We included six trials (four were cluster-RCTs) with 19,498 children who were 4 to 15 years of age. Four trials were conducted in the UK and two were based in India. We assessed two trials to be at low risk of bias, one trial to be at high risk of bias and three trials to be at unclear risk of bias.

None of the six trials reported the proportion of children with untreated caries or other oral diseases.

Four trials evaluated traditional screening versus no screening. We performed a meta-analysis for the outcome 'dental attendance' and found an inconclusive result with high heterogeneity. The heterogeneity was found it to be, in part, due to study design (three cluster-RCTs and one individual-level RCT). Due to the inconsistency, we downgraded the evidence to 'very low certainty' and are unable to draw conclusions about this comparison.

Two cluster-RCTs (both four-arm trials) evaluated criteria-based screening versus no screening and showed a pooled effect estimate of RR 1.07 (95% CI 0.99 to 1.16), suggesting a possible benefit for screening (low-certainty evidence). There was no evidence of a difference when criteria-based screening was compared to traditional screening (RR 1.01, 95% CI 0.94 to 1.08) (very low-certainty evidence).

In one trial, a specific (personalised) referral letter was compared to a non-specific one. Results favoured the specific referral letter with an effect estimate of RR 1.39 (95% CI 1.09 to 1.77) for attendance at general dentist services and effect estimate of RR 1.90 (95% CI 1.18 to 3.06) for attendance at specialist orthodontist services (low-certainty evidence).

One trial compared screening supplemented with motivation to screening alone. Dental attendance was more likely after screening supplemented with motivation, with an effect estimate of RR 3.08 (95% CI 2.57 to 3.71) (low-certainty evidence).

None of the trials had long-term follow-up to ascertain the lasting effects of school dental screening.

None of the trials reported cost-effectiveness and adverse events.

Authors' conclusions

The trials included in this review evaluated short-term effects of screening, assessing follow-up periods of three to eight months. We found very low certainty evidence that was insufficient to allow us to draw conclusions about whether there is a role for traditional school dental screening in improving dental attendance. For criteria-based screening, we found low-certainty evidence that it may improve dental attendance when compared to no screening. However, when compared to traditional screening there was no evidence of a difference in dental attendance (very low-certainty evidence).

We found low-certainty evidence to conclude that personalised or specific referral letters improve dental attendance when compared to non-specific counterparts. We also found low-certainty evidence that screening supplemented with motivation (oral health education and offer of free treatment) improves dental attendance in comparison to screening alone.

We did not find any trials addressing cost-effectiveness and adverse effects of school dental screening.

PLAIN LANGUAGE SUMMARY

School dental screening programmes for improving oral health of children

What was the aim of this review?

The aim of this Cochrane Review was to find out if school dental screening improves oral health of children; and if it does, which is the best screening method. We found six relevant studies to answer this question.

Key messages

There is insufficient evidence to draw conclusions about whether there is a role for traditional school dental screening in improving dental attendance. School dental screening programmes with personalised referral letters or additional motivation elements probably have the ability to improve dental attendance over the short term (follow-up of three months up to two years). Screening based on specific criteria may possibly be better than no screening. However, it is not clear if improvement in dental attendance leads to better oral health of children. We still need high-quality studies that measure the impact of screening on oral health carried out over longer periods of time.

What was studied in this review?

Oral diseases, especially dental caries, affect children worldwide. If unchecked, oral health can deteriorate progressively and adversely impact children's general well-being. It also has a financial bearing at family and community levels.

School dental screening is a public health measure wherein oral examination of children is carried out in the school setting followed by informing parents about the oral condition and treatment needs of their child. It aims to identify oral health concerns at an early stage and prompt parents to seek treatment where required. Whether this actually improves children's oral health is the concern of this review.

What are the main results of this review?

We found six relevant studies, with 19,498 children included in the analysis. Four studies were conducted in the UK and two were based in India. The children in these studies were 4 to 15 years old. Studies compared children who were screened in school to children who did not undergo screening in terms of their oral health and visits to the dentist. Studies also compared one type of screening to another (for example, variations in clinical examination or referral process).

We are uncertain whether traditional school dental screening improves dental attendance as we assessed the certainty of the evidence as very low.

Screening based on specific criteria (e.g. non-registration with a dentist) seems to be more effective for improving attendance at the dentist than no screening (low-certainty evidence), but there may be no difference between crteria-based and general screening (very low-certainty evidence).

A personalised referral letter to parents seems to improve dental attendance (low-certainty evidence).

Screening when supplemented with motivation in terms of health education and offer of free treatment seems to improve dental attendance (low-certainty evidence).

All the six studies followed up children for three to eight months after they received screening. We therefore do not know if benefits of screening lasted over time.

We did not find trials that addressed the cost-effectiveness of these programmes or any adverse effects.

How up to date is the review?

We searched for published studies up to 15 March 2017.

SUMMARY OF FINDINGS FOR THE MAIN COMPARISON [Explanation]

Traditional screening compared to no screening for increasing dental attendance

Population: increasing dental attendance
Setting: primary and secondary school
Intervention: traditional screening
Comparison: no screening

Outcomes	Relative effect (95% CI)	Anticipated absolute effects* (95% CI)		Number of participants (studies)	Certainty of the evidence (GRADE)	What happens
		Without no screening	With traditional screening Difference			
Dental attendance Follow-up: 3 to 4 months	Data not pooled			6281 (4 RCTs)	⊕◯◯◯ VERY LOW [1,2]	There was substantial heterogeneity, in part due to study design (3 cluster RCTs and 1 individual-level RCT) . Due to the inconsistency, we downgraded the evidence to 'very low certainty' and are unable to draw conclusions about this comparison

*__The risk in the intervention group__ (and its 95% CI) is based on the assumed risk in the comparison group and the __relative effect__ of the intervention (and its 95% CI).
CI: confidence interval; **RR**: risk ratio; **OR**: odds ratio

GRADE Working Group grades of evidence
High certainty: we are very confident that the true effect lies close to that of the estimate of the effect
Moderate certainty: we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low certainty: our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
Very low certainty: we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1] Praveen 2014 and Zarod 1992 trials have unclear selection bias, performance and detection bias. Downgraded by two levels

[2] High heterogeneity (I^2 = 91%). Downgraded by one level

BACKGROUND

Description of the condition

Oral health is essential to general health, well-being and quality of life (WHO 2003). The World Health Organization (WHO) defines it as "a state of being free from mouth and facial pain, oral and throat cancer, oral infection and sores, periodontal (gum) disease, tooth decay, tooth loss, other diseases and disorders that limit an individual's capacity in biting, chewing, smiling, speaking, and psychosocial well being" (WHO 2003).

The oral health of children is a significant public health issue (WHO 2003). Oral diseases, including dental caries, are progressive and cumulative. Availability of services does not always translate to use of services. CDC 2014 reports that less than half of children aged 21 years or less in the USA used dental care in 2009 and only 14.2% used preventive dental services. In 2003, the UK's National Children's Dental Health Survey reported over four out of ten children showed signs of obvious decay experience by the age of five years and half of eight-year-old children had obvious decay experience (United Kingdom National Technical Reports 2003).

Unrecognised disease and postponed care exacerbate oral and dental problems, leading to pain, discomfort and sometimes irreversible damage. Poor oral health significantly affects children's nutritional intake and consequently their general health, growth and development. The psychosocial impacts of poor oral health, like interference with daily activities, sleeping pattern, quality of life and parental output, can be considerable (AAPD 2008). It may impede learning, activity and interactions with peers in school (WHO 2003). Such problems are compounded among children of deprived communities (Tickle 1999a; Newacheck 2000; Edelstein 2002). Some of the oral diseases that affect children worldwide are described below.

Dental caries continues to be a common chronic childhood disease. In the United States over 50% of five- to nine-year-old children experience tooth decay and the figure rises above 90% in some low- and middle-income countries, signalling that dental caries is a present-day public health crisis (Petersen 2003; Bagramian 2009). According to Montana 2016, 14.2% of children screened in the age group of three to five years old had untreated decay.

Several gingival diseases also affect children and adolescents with varying rates and severity. The prevalence of gingivitis has been estimated at 73% among school children between six and 11 years of age in Iran (Ketabi 2006). Similarly, the prevalence of gingivitis reported in sample of adolescents from Greece was 72.8% (Chrysanthakopoulos 2016).

Developmental defects of enamel (DDE) have a significant impact on oral health and aesthetics in both primary and permanent dentition. Most epidemiological studies show that the frequency of appearance of these defects is on the rise in almost all populations (Robles 2013). It is of high clinical significance when dentine or pulp involvement ensues due to significant enamel loss or high susceptibility to caries (Pitts 2015).

Dental and facial trauma of varying intensity affects children. Azami-Aghdash 2015 reported the prevalence of dental trauma in children and adolescents (under 18 years of age) to be 17.5%, with variation among different geographic regions. Timely intervention may alleviate future complications in children with dental trauma. A recent study by Zhou 2016 revealed a high prevalence of malocclusion in children with primary dentition (66.3%). Identification of modifiable factors that can be addressed through preventive and interceptive orthodontics can save elaborate and expensive treatments later (Pruthi 2013).

Oral diseases impose considerable financial, social and personal burdens. According to Listl 2015, the global economic burden of dental diseases for a year amounted to USD 442 billion, including both direct treatment costs and indirect costs in terms of productivity losses owing to absenteeism at school and work. Advanced disease may necessitate more complex and costly treatments such as root canal therapies, extractions or treatment under general anaesthesia (WHO 2003; Australian Institute of Health 2013). FDI 2015 calls for global action on oral diseases, highlighting the substantial burden on individuals and communities as a result of pain and suffering, impairment of function and reduced quality of life.

Description of the intervention

School dental screening (or 'oral health/dental examination', 'dental assessment', 'dental certificates', 'dental check-up') basically refers to brief visual examination of children's oral cavity carried out in a school setting (Tickle 1999b; AAPD 2008; Irish Guideline 2012; Janakiram 2016). This is followed by making parents aware of their child's oral health status and treatment needs. Follow-up methods can be categorised as:

1. conventional methods, that is sending a referral card/ information letter/consent form (Hebbal 2005; Milsom 2006);
2. additional methods; for example, Reiss 1982 provided phone call reminders and incentives, whereas Zarod 1992 provided intensive follow-up by means of personalised letters.

The focus of a school dental screening programme is not merely to identify children with oral health problems, but also to act as a vehicle to bring these children into contact with oral health services (Donaldson 2001; Morgan 2013). It is imperative to follow up screened children to measure the effectiveness of screening in terms of increased uptake of services, for example registration with a dentist and dental attendance (Zarod 1992; NHS 2000; Milsom 2006; Cunningham 2009).

Hence, the objective of screening is twofold.

1. To identify test-positive cases.
2. To ensure these are followed-up for appropriate management.

School dental screening is usually a part of school health services and its model, process and objectives vary depending on the individual healthcare delivery policies of each country (Jenner 1986; Milsom 1995; Tickle 1999b; Donaldson 2001; AAPD 2008; Irish Guideline 2012). Programmes can be broadly classified based on the following criteria.

1. Personnel involved
   i) Dentist (Milsom 2006)
   ii) Dentally-qualified health professionals such as dental hygienists (Locker 2004), dental nurses (Morgan 2013)
   iii) Health professionals qualified in areas other than dentistry, such as doctors (Bader 2004; Rowan-Legg 2013)
2. Methods used
   i) Visual screening (Tantawi 2015)
   ii) Visual screening with tongue blade (Tantawi 2015)
   iii) Using mouth mirror and probe (Tantawi 2015)
3. Criteria-based screening (referral of children on the basis of pre-established check-list of criteria) versus traditional screening (referral based on the screening dentist's opinion) (Milsom 1999; Kearney-Mitchell 2006)
4. Targeted screening (towards identified/high risk population) (Locker 2004; Chong 2011) versus universal screening (applies to all children of a population or subpopulation) (Milsom 2006)
5. Compulsory screening at school entry (AAPD 2008; Irish Guideline 2012) versus optional screening (Hebbal 2005)

The effectiveness of school dental screening depends on adequate follow-up treatment. Treatment services may be:

i) provided within school premises (screening and treatment services operate as single or linked entity) (Irish Guideline 2012); or

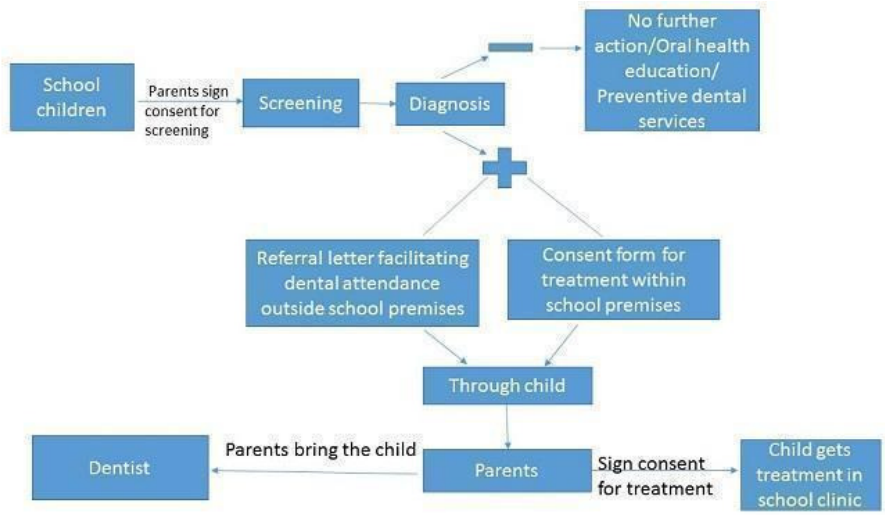
ii) facilitated outside school premises (screening and treatment services function as distinct entities) (Milsom 2006).

The literature suggests that belonging to lower-income groups and the absence of dental insurance reduces the likelihood of children attending the dentist (California Healthcare foundation 2008). Hence, school dental screening outcomes may also rely on the cost of treatment (Milsom 2006) being:

i) charged to parents;
ii) subsidised; or
iii) free.

Figure 1 illustrates a schema of referral process/treatment process post screening.

Figure 1. A schema of school dental screening process



How the intervention might work

Regular oral health assessment is imperative for protecting, improving and promoting children's oral health. Early diagnosis of oral conditions is crucial for avoiding short-term complications and long-term effects of advanced disease. School dental screening aims to detect and intercept disease at a stage earlier than that at which the child would normally present for treatment by making children and parents aware of the condition and its future complications.

A school dental check-up programme in Australia demonstrated screening as a less costly and more effective intervention than the standard of care (Nguyen 2017). It reinforces that reductions in morbidity achieved through screening imply potential cost benefits, both in terms of reduced treatment costs and productivity losses (Listl 2015).

The school provides an ideal setting for oral health screening in children. The benefits of early diagnosis and intervention can be reaped for sustainable oral health all through these years and into adulthood. WHO 2003 endorsed school dental screening as an efficient and effective way to reach over one billion children worldwide; and through them, families and communities.

Why it is important to do this review

School dental screening is one of the most debated aspects of healthcare systems, public health practices and health policy discussions (Janakiram 2016). The literature presents contrasting and contradictory results.

Zarod 1992, Donaldson 2001 and Hebbal 2005 suggest screening to be an effective public health measure. A trial by Burden 1994 reported that the personalised referral letter stimulated greater dental attendance than the non-specific referral letter. The Praveen 2014 study suggests that there is some evidence that vigorous follow-up of children does lead to improved dental attendance rates; however, the acceptability to parents and the cost-effectiveness of putting significant resources into elaborate follow-up procedures would need to be scientifically assured.

In contrast, large cluster-randomised trials by Milsom 2006 did not demonstrate that school dental screening was effective at reducing untreated dental caries in the UK. Additional analyses on data from Milsom 2006 suggested that screening also failed to produce worthwhile benefits for the screened-positive population, as less than half of screened-positive children attended the dentist; and of those who did attend, less than a quarter received appropriate treatment. Similarly, Cunningham 2009 showed that school dental screening did not increase registration at the dentist in a group of 12- to 13-year-old children in Scotland. Milsom 2008 considers school dental screening to be more of a politically-inclined public health practice than a scientifically-based one as, despite the strong emphasis of policy makers and heavy expenditure in terms of finances and manpower resources, there is a lack of clear evidence to demonstrate that this process is effective in improving the oral health of the population (Tickle 1999b; Threlfall 2006; Rodgers 2007; Milsom 2008).

This review synthesises the evidence regarding the effectiveness of school dental screening programmes for improving oral health. Analysis of existing literature may help explore factors that might influence successful provision of school dental screening. This review will aid government policy makers, programme planners at various levels, and administrators in health and education sectors to tailor appropriate school dental health programmes, benefiting the community without unnecessarily burdening fiscal sectors. The review will also be relevant to general dental practitioners, paediatric dentists and oral health promotion teams. It will also be of interest to parents, teachers and all those involved in working with children.

OBJECTIVES

To assess the effectiveness of school dental screening programmes on overall oral health status and use of dental services.

METHODS

Criteria for considering studies for this review

Types of studies

We included randomised controlled trials (RCTs) where randomisation occurs at the level of the group (clustered by school or class, or both) or individual children.

Types of participants

Children and adolescents aged three to 19 years attending a school in any country. Participants can have deciduous, permanent or mixed dentition.

We made inclusion independent of dental disease or dental caries' level at the start of the study, current dental treatment, dentist attendance levels and nationality. We included studies regardless of whether dentists/dental nurses/dental hygienists were involved in the visual inspection of the child's oral cavity. We excluded studies not predominantly done in a school setting, due to the focus of the current review.

Types of interventions

1. School dental screening versus placebo or no screening.
2. School dental screening A versus B (where A and B refer to different types of screening based on the classification presented above).

Exclusion criteria

1. Screening without a specified follow-up plan or less than three months' follow-up.
2. Oral health assessment programmes for children attending special schools.
3. Dental examination performed by personnel other than those licensed or trained in the process as per state laws, for example school teachers, medical practitioners.

Types of outcome measures

We assessed all primary and secondary outcome measures based on duration as follows.

1. Short-term effects (minimum follow-up of three months up to two years) (NICE guideline 2004).
2. Long-term effects (follow-up of more than two years) (Irish Guideline 2012).

Primary outcomes

1. Proportion of children with untreated caries.
2. Proportion of children with other untreated oral health need (e.g. malocclusion, trauma).
3. Dental attendance (registration and follow-up dental appointments).

Secondary outcomes

1. Caries, measured by any validated index (in primary and permanent teeth separately).
2. Gingivitis measured by any validated index.
3. Developmental defects of enamel measured by any validated index.
4. Malocclusion or orthodontic treatment needs measured by any validated index.
5. Trauma to teeth measured by any validated index.
6. Cost effectiveness.
7. Adverse events.

Search methods for identification of studies

Cochrane Oral Health's Information Specialist conducted the systematic searches for RCTs and controlled clinical trials. Due to the Cochrane Centralised Search Project to identify all clinical trials on the database and add them to CENTRAL, only recent months of the Embase database were searched.There were no other restrictions on the language or date of publication when searching the electronic databases.

Electronic searches

Cochrane Oral Health's Information Specialist searched the following electronic databases.

- Cochrane Oral Health's Trials Register (searched 15 March 2017) (Appendix 1).
- Cochrane Central Register of Controlled Trials (CENTRAL) in the Cochrane Register of Studies (searched 15 March 2017) (Appendix 2).
- MEDLINE Ovid (1946 to 15 March 2017) (Appendix 3).
- Embase Ovid (15 September 2016 to 15 March 2017) (Appendix 4).

The subject strategies for databases were modelled on the search strategy designed for MEDLINE Ovid in Appendix 3. Where appropriate, this was combined with subject strategy adaptations of the Highly Sensitive Search Strategy designed by Cochrane for identifying randomised controlled trials and controlled clinical trials (as described in the *Cochrane Handbook for Systematic Reviews of Interventions*, Box 6.4.c. (Lefebvre 2011)).

Searching other resources

The following trial registries were searched.

- US National Institutes of Health Ongoing Trials Register ClinicalTrials.gov (clinicaltrials.gov, searched 15 March 2017) (Appendix 5).
- World Health Organization International Clinical Trials Registry Platform (apps.who.int/trialsearch, searched 15 March 2017) (Appendix 6).

We searched the reference lists of included studies and relevant systematic reviews for further studies.

Data collection and analysis

Selection of studies

Two review authors - Ankita Arora (AA) and Noorliza Mastura Ismail (NMI) - independently and in duplicate screened the titles and abstracts from the electronic searches to identify potentially eligible studies that required further evaluation to determine whether they met the inclusion criteria for this review. We obtained the full-text copies of all eligible and potentially eligible studies and these were further evaluated by Shivi Khattri (SK) and Sumanth Kumbargere Nagraj (SKN) to identify those studies that met all the inclusion criteria. We resolved any disagreement by discussion; or, if necessary, consulted a third review author, Eachampati Prashanti (EP), in order to reach consensus. We recorded those studies which were evaluated in full text but did not meet the inclusion criteria in the Characteristics of excluded studies table, noting the reason for exclusion. We assessed articles in languages other than English by their abstracts, where possible;

and if they appeared to be potentially eligible, we translated the full text of the article.

Data extraction and management

Two review authors (AA and SK) independently and in duplicate extracted the data. The review authors were not blinded to the authors of the included studies. We resolved any disagreement by discussion or by consulting a third review author (EP) in order to reach consensus. We extracted the data using a customised data extraction form, which we designed following the guidance in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011a). We entered the study details in the 'Characteristics of included studies' table in Review Manager 5 (RevMan 5) software (Review Manager 2014).

We recorded the following details for each included trial.

1. Publication details such as year of publication, language.
2. Country of origin.
3. Details of participants including demographic characteristics.
4. Type of trial (sample size; method of randomisation; allocation concealment; blinding; method of assessing the outcomes; and dropouts, if any).
5. Type of intervention and comparison.
6. Details of the outcomes reported.
7. Duration of follow-up.
8. Location and costs of follow-up appointments.
9. Results of the intervention.
10. Funding details.

We contacted the authors of included studies when we needed clarification of details or any additional data, via e-mail whenever possible.

Assessment of risk of bias in included studies

Two review authors (SKN and AA) independently assessed the risk of bias in the included trials in the following domains.

1. Random sequence generation (selection bias).
2. Allocation concealment (selection bias).
3. Blinding of participants and personnel (performance bias).
4. Blinding (outcome assessment) (detection bias).
5. Incomplete outcome data (attrition bias).
6. Selective outcome reporting (reporting bias).
7. Risk of bias specific to cluster-randomised trials.
8. Other biases.

For each of these domains, we assigned a judgement regarding the risk of bias of 'high', 'low' or 'unclear', based on guidance in Higgins 2011b. We contacted the trial authors if details were missing from the publications or were unclear. We resolved disagreements through consensus. We recorded our judgements and justifications in 'Risk of bias' tables for each included study and generated a 'Risk of bias' summary graph and figure. We used these judgements while grading the overall certainty of evidence for each comparison and outcome in the 'Summary of findings' tables. We summarised the risk of bias according to Higgins 2011b as follows.

Risk of bias	Interpretation	Within study	Across studies
Low risk of bias	Plausible bias unlikely to seriously alter the results	Low risk of bias for all key domains	Most information is from studies at low risk of bias
Unclear risk of bias	Plausible bias that raises some doubt about the results	Unclear risk of bias for one or more key domains	Most information is from studies at low or unclear risk of bias
High risk of bias	Plausible bias that seriously weakens confidence in the results	High risk of bias for one or more key domains	The proportion of information from studies at high risk of bias is sufficient to affect the interpretation of results

Measures of treatment effect

For dichotomous outcomes, such as proportions of children attending the dentist, we used the number of events per arm and calculated the risk ratios (RRs) with 95% confidence intervals (CIs). For continuous outcomes, we intended to use means and standard deviations (SDs) presented in the studies to calculate mean differences (MDs) and CIs to summarise the continuous data. We intended to use standardised mean difference if studies used different scales to measure the same outcome. If data were expressed on shorter ordinal scales, we intended to explore the possibility of converting them to dichotomous outcomes. If data were expressed on long ordinal scales, we intended to analyse them as continuous

data. If outcomes had been reported both at baseline and at follow-up or at trial endpoints, we would have used end scores as they are the most commonly reported. However we did not find any such data.

Unit of analysis issues

We encountered two types of non-standard study designs in this review.

1. Repeated observations on participants.
2. Cluster-randomised trials.

In cases of repeated observations on participants for our primary outcomes, we followed the method described in section 9.3.4 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Deeks 2011).

In cluster-randomised trials, we handled the data following the method described in section 16.3 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011c). In cluster-randomised trials, the unit of analysis was the cluster.

In trials where adverse effects were described as counts, we intended to follow the method described in section 9.2.5 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Deeks 2011). However we did not find any such data.

Dealing with missing data

We intended to use the methods in section 16.1.2 of the *Cochrane Handbook for Systematic Reviews of Interventions* to estimate missing standard deviations (Higgins 2011c). However we did not find any such trial with missing standard deviations in our review. We contacted trial authors to try to obtain the missing intra-cluster correlation coefficient (ICC).

Assessment of heterogeneity

In meta-analyses, we assessed the heterogeneity using a Chi² test, where a P value less than 0.1 indicates statistically significant heterogeneity. We quantified heterogeneity using the I² statistic (Higgins 2003) as follows.

1. 0% to 40% implies slight heterogeneity.
2. 30% to 60% implies moderate heterogeneity.
3. 50% to 90% implies substantial heterogeneity.
4. 75% to 100% implies very substantial ('considerable') heterogeneity.

If there had been very substantial heterogeneity ($I^2 > 75\%$), which could not be explained by the subgroup analyses, we intended to not conduct meta-analysis. However, we did not encounter such situations.

Assessment of reporting biases

If we had included more than 10 studies in a meta-analysis, we intended to assess the possible presence of reporting bias by testing for asymmetry in a funnel plot. If present, we planned to carry out statistical analysis using the methods described in section 10.4.3.1 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Sterne 2011). However, we had only six trials included in our review and we did not assess the reporting bias.

Data synthesis

We analysed the data using RevMan 5 software (Review Manager 2014). We meta-analysed the data available from the studies that have similar comparisons and outcomes, using a random-effects model. With this approach, the confidence intervals for the average intervention effect were wider than those obtained using a fixed-effect approach, leading to a more conservative interpretation. For dichotomous data, we used risk ratio for data synthesis. For continuous data, we used end scores when available. We reported the results from studies that are not suitable for inclusion in a meta-analysis using additional tables.

Subgroup analysis and investigation of heterogeneity

Had there been significant heterogeneity, we would have explored the reasons by performing the following subgroup analyses.

1. Age group (age 3 to 5 years, 6 to 12 years, 13 to 19 years) (WHO 2013).
2. Targeted or universal screening.
3. Post-screening treatment offered within the school setting or referred for treatment outside the school setting.
4. Treatment charges borne by parents: a) full charge; b) subsidised costs; c) no cost.

However, we did not find enough trials to perform subgroup analysis.

Sensitivity analysis

Had there been sufficient included studies, we would have performed the following sensitivity analyses.

1. Including only studies at low risk of bias.
2. Using the fixed-effect model for meta-analysis.
3. Using different intracluster correlation coefficients (ICC) estimates where these values are missing in studies.

Summarising findings and assessing the certainty of the evidence

We used the GRADE approach to interpret findings (Schünemann 2011). We used GRADE Profiler software (GRADEpro GDT) and imported data from Review Manager 2014 to create 'Summary of findings' tables for each comparison and for main outcomes included in the review (dental caries, dental attendance, gingivitis, developmental defects of enamel, trauma and adverse events). In these tables we have provided information concerning the overall certainty of the evidence from the trials, magnitude of

effect of the interventions examined and sum of available data on the primary and secondary outcomes. The GRADE approach considers 'certainty' to be a judgement of the extent to which we can be confident that the estimates of effect are correct (Schünemann 2011). A body of evidence from RCTs was initially graded as 'high' and downgraded by one, two or three levels depending on five considerations: limitations in the design of the studies; indirectness (or applicability) of the evidence; inconsistency of results; imprecision of the results; and the possibility of publication bias. A certainty level of 'high' reflects confidence that the true effect lies close to that of the estimate of the effect for an outcome. A judgement of 'moderate' certainty indicates that the true effect is likely to be close to the estimate of the effect, but acknowledges the possibility that it could be substantially different. 'Low' and 'very low' certainty evidence limit our confidence in the effect estimate (Balshem 2011).

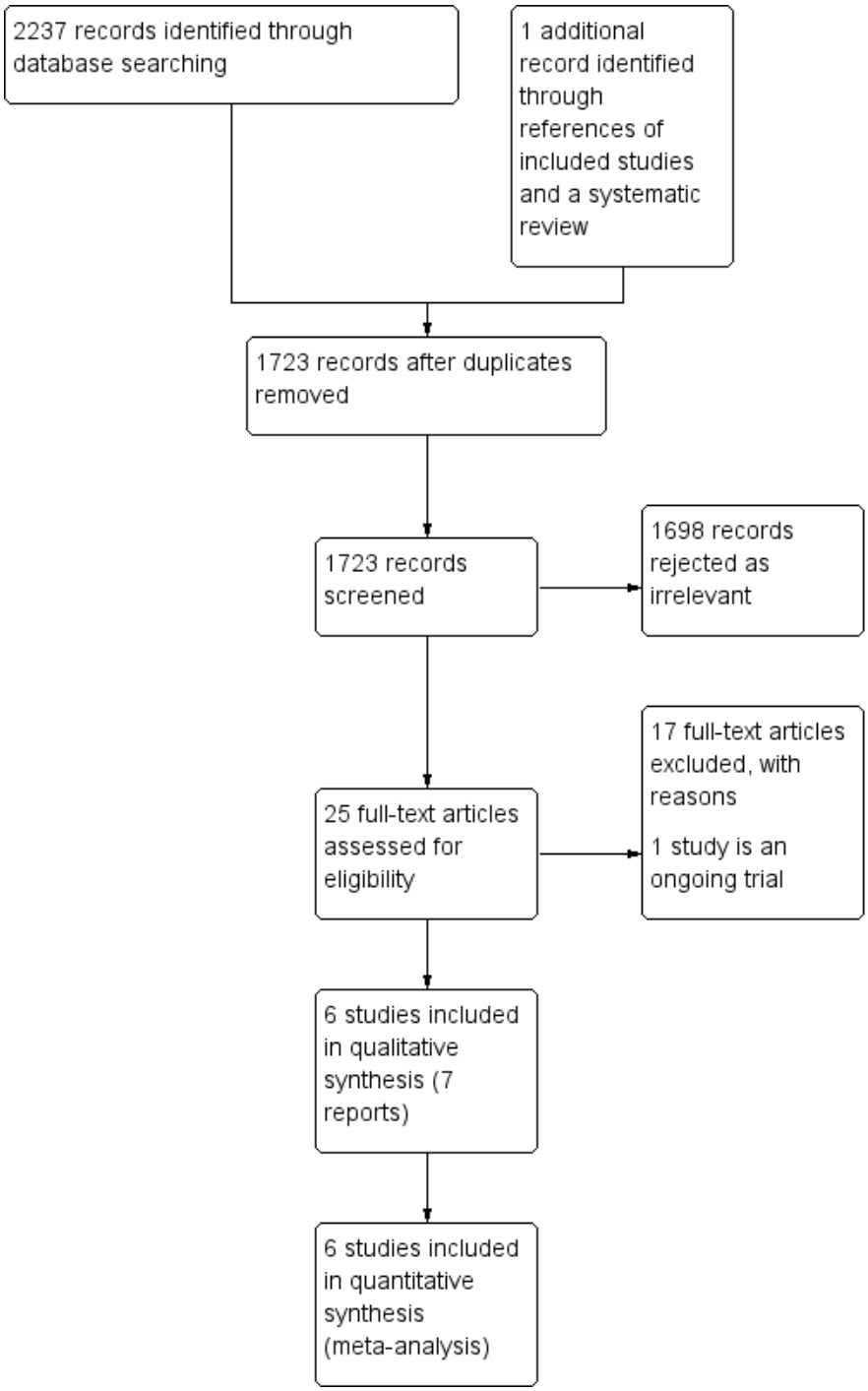
RESULTS

Description of studies

Results of the search

The electronic search strategies identified 2238 records from English and other language databases and cross-references of included trials and other systematic review. At the end of our search, we had 1723 records after duplicates were removed. We discarded 1698 and we requested full-text copies of 25 references. Two review authors (SK, SKN) independently and in duplicate assessed these papers to determine their eligibility. We excluded 17 studies and one study is ongoing. We identified six studies (seven reports) that met the inclusion criteria and included them in this review (Figure 2). For details of the studies we examined and the reasons we included or excluded them, see the Characteristics of included studies and Characteristics of excluded studies tables.

Figure 2. Study flow diagram



We contacted authors of five included trials and we did not receive clarifications on any of the trials (see Characteristics of included studies). We also contacted the authors of an unpublished clinical trial and they refused to share the details of the trial presently (see Characteristics of ongoing studies).

Included studies

See Characteristics of included studies table.

Characteristics of the trial settings and investigators

We included six trials from seven reports in the review. All were in the English language. The countries of origin for the included studies were the UK (Zarod 1992; Burden 1994; Milsom 2006; Cunningham 2009); and India (Hebbal 2005; Praveen 2014).
All trials were conducted in a school setting, with four being cluster-randomised (Hebbal 2005; Milsom 2006; Cunningham 2009; Praveen 2014), and two being individually randomised (Zarod 1992; Burden 1994).
Out of six trials, three provided grant information, two were NHS funded (Milsom 2006; Cunningham 2009), and one had university Royal College funding (Burden 1994).
All the trials used attendance at a dental surgery (general practitioner) as the study outcome. Apart from attendance at a general dental surgery, Burden 1994 measured attendance at a specialist orthodontist. Cunningham 2009 measured registration of unregistered children at the dental surgery.
Milsom 2006 measured change in prevalence of dental caries per child as the primary outcome. Secondary outcomes of the trial measured were sepsis, plaque or calculus and trauma to the permanent incisor teeth. A secondary report further followed up the children who attended the dentist and measured the treatment reception of these children.

Characteristics of the participants

Both male and female school children formed the study population of all six trials.
Zarod 1992 and Milsom 2006 carried out screening trials in primary school children in the age range of four to six years and six to eight years respectively, whereas Burden 1994 and Cunningham 2009 did screening trials on secondary school children in the age range of 11 to 13 years. The age range for trials by Hebbal 2005 and Praveen 2014 was wide, involving both primary and secondary school children of 6 to 15 years. The minimum age included in a study was four years (Zarod 1992); and the maximum age included in a study was 15 years (Hebbal 2005). The minimum sample size was 201 (Burden 1994); and the maximum sample size was 16,684 children in 168 clusters (Milsom 2006).

Characteristics of the interventions

Screening intervention in all the six trials varied considerably in terms of identifying test-positive children and follow-up referral procedures.

Traditional screening compared to no screening

'Traditional screening' refers to a child being given a referral card if, in the opinion of the inspecting dentist, the child needs to attend a dentist.
This comparison was evaluated by two four-arm cluster RCTs (Milsom 2006; Cunningham 2009), and one two-arm cluster RCT (Praveen 2014).
Zarod 1992 did a two-arm trial where participants were individually randomised and also compared traditional screening to screening without any referral. As no intimation was given to parents of control group post screening, this group can be considered comparable to a no-screening control.

Criteria-based screening

This is a screening variant where the dentist has pre-established criteria for referring a child to a dentist. This was compared to traditional and no screening by Cunningham 2009 and Milsom 2006.

Specific versus non-specific referral letters

One parallel-arm RCT compared different referral letters for increasing attendance at a dentist and orthodontist specialist services (Burden 1994). The specific referral letter in the intervention group advised parents to seek advice about treatment to straighten their child's teeth, while the control group was given a referral letter advising parents in a non-specific way to attend a dentist.

Screening versus screening with oral health motivation

Hebbal 2005 compared effects of screening supplemented with oral health motivation or education sessions and an offer of free treatment versus screening and referral alone.

Parents' information leaflets

Milsom 2006 and Cunningham 2009 also had an intervention arm where parents were advised to visit the dentist without dental inspection through an information leaflet. As no oral examination was done in these arms of the aforementioned trials, they did not conform to our definition of screening and so we have not used them in this review.

Characteristics of the outcomes

We proposed to analyse outcome measures based on follow-up duration as short-term (minimum follow-up of three months to two years) and long-term effects (follow-up of more than two years). However, the follow-up period of all included trials was less than two years, hence we report only short-term effects of school dental screening.

Of the three primary outcomes we planned in our protocol, only dental attendance was reported by the included trials (Cunningham 2009 measured registration rather than attendance).

With respect to secondary outcomes of this review, only one study described prevalence of dental caries per child, prevalence of sepsis, presence of gross plaque or calculus and trauma to the permanent incisor teeth as its outcomes (Milsom 2006).

Intracluster correlation coefficient (ICC) and data adjustment to minimise clustering effect

Only one study reported the value of ICC for dental caries (Milsom 2006). Hence, this ICC (0.03) was borrowed for calculating the effect estimate for dental attendance for other cluster-randomised trials in this review.

Data were adjusted to minimise clustering effect as per Adam 2005 (see Table 1).

Excluded studies

We excluded 17 studies and we listed the reasons for exclusion in the Characteristics of excluded studies tables. Six of these excluded trials were not RCTs and five were oral health promotion trials rather than screening trials.

Full text was not available for one trial (Baglee 2000), and another failed to explain if participants were divided randomly into intervention or control group (Binder 1973).

Of the remaining four RCTs, one trial did not send a communication to parents after examination (Rodgers 2007); two trials did not follow participants to the use of services (Locker 2004; Tantawi 2015); and follow-up was less than three months in one trial (Donaldson 2001).

Risk of bias in included studies

We documented the risk of bias for included studies based on the full-text articles. Wherever there was a need for clarification, we tried contacting the authors. Based on the available data, we assessed the risk of bias as low, high or unclear.

We assessed two of the six trials as low risk of bias (Milsom 2006; Cunningham 2009); one trial as high risk of bias (Hebbal 2005); and three trials as unclear risk (Zarod 1992; Burden 1994; Praveen 2014).

See 'Risk of bias' tables within Characteristics of included studies for further details. For a graphical summary, see Figure 3 and Figure 4.

Figure 3. Risk of bias graph: review authors' judgements about each risk of bias item presented as percentages across all included studies

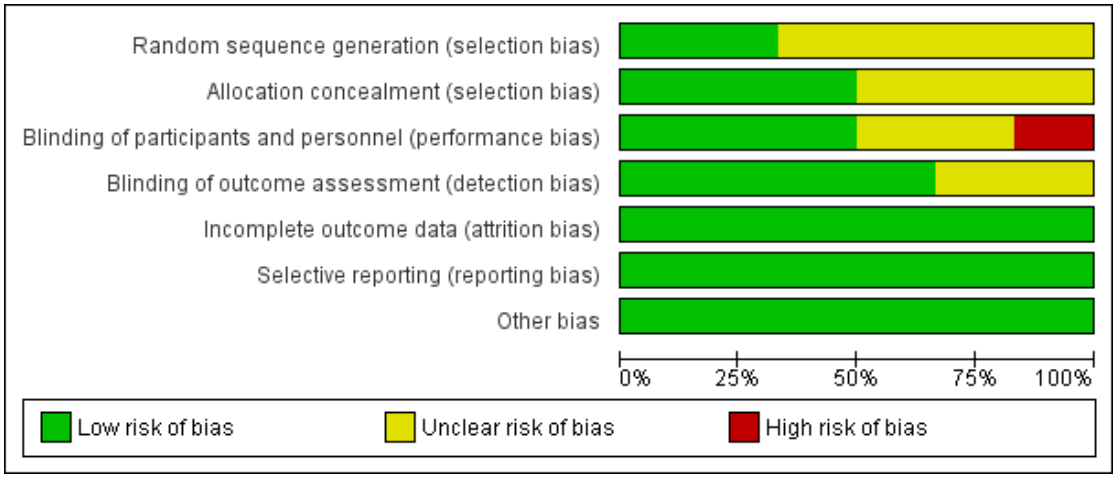
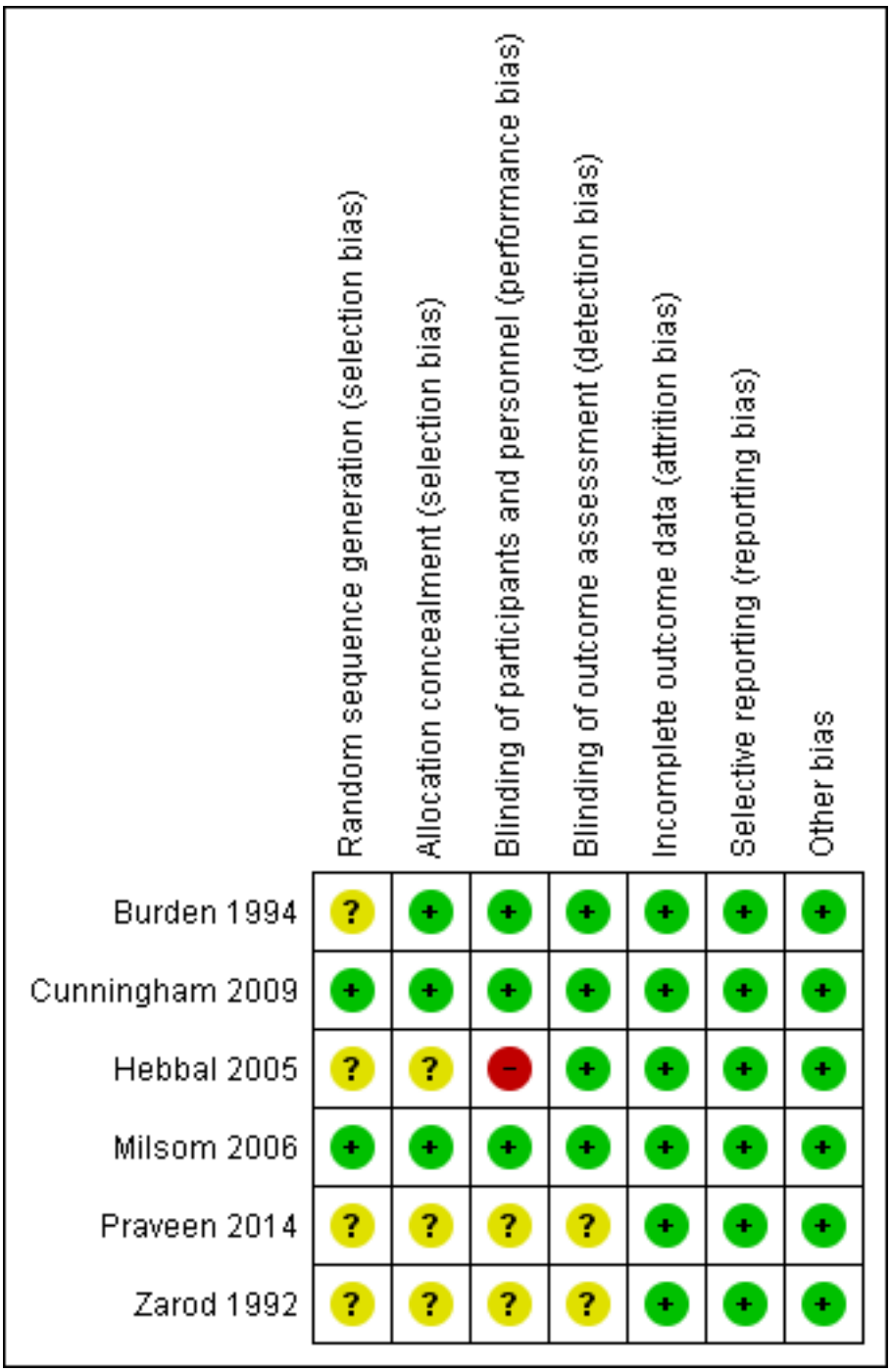


Figure 4. Risk of bias summary: review authors' judgements about each risk of bias item for each included study



Allocation

Only two of the included trials adequately reported the method of sequence generation (Milsom 2006; Cunningham 2009); and three adequately reported concealment of allocation (Burden 1994; Milsom 2006; Cunningham 2009). The studies other than Cunningham 2009 and Milsom 2006 were at unclear risk of selection bias.

Blinding

Out of six included trials, blinding of participants and personnel was not reported in two trials, which we therefore considered to have an unclear risk of performance and detection bias (Praveen 2014; Zarod 1992). Blinding of participants was not done in one trial so we judged it to be at high risk of performance bias, though a computer programme assessed the main outcome 'school attendance' so we considered the study to be at low risk of detection bias (Hebbal 2005). Three trials described blinding of participants and assessors and we assessed them to be at low risk of performance or detection bias (Burden 1994; Milsom 2006; Cunningham 2009).

Incomplete outcome data

In this review, not attending or not registering at the dental surgery is an outcome measure rather than attrition. Hence, we redefined attrition bias for this systematic review as 'parents not receiving call letters from school'. Based on this definition, all six trials were at low risk of attrition bias based on the data presented.

Selective reporting

All the six included trials were at low risk of reporting bias as all pre-stated outcomes in the methods were reported.

Other potential sources of bias

No other potential source of bias was reported.

Effects of interventions

See: **Summary of findings for the main comparison** Traditional screening compared to no screening for increasing dental attendance; **Summary of findings 2** Criteria-based screening compared to no screening for increasing dental attendance; **Summary of findings 3** Criteria-based screening compared to traditional screening for increasing dental attendance; **Summary of findings 4** Criteria-based screening with specific referral compared to criteria-based screening with non-specific referral for increasing dental attendance; **Summary of findings 5** Traditional screening with motivation compared to traditional screening for increasing dental attendance

Proportion of children with untreated dental caries

None of the included trials tested this outcome.

Proportion of children with other untreated oral health needs

None of the included trials tested this outcome.

Dental registration or attendance

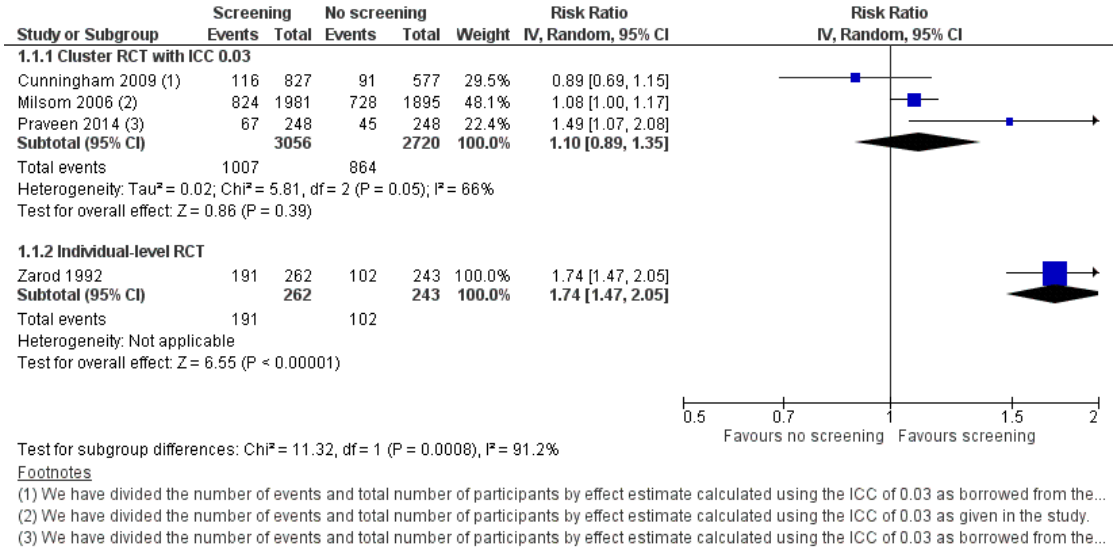
All six trials (19,498 children) included in this review presented dental attendance as an outcome of school dental screening intervention (Table 1). It is an objective outcome and is presented as dichotomous data.

Due to variability of methods of screening within and across trials, we present dental attendance as five distinct comparisons.

Traditional screening versus no screening

Four studies contributed data to compare traditional screening versus no screening. However when pooled, the I^2 measure for heterogeneity was substantial. There were three cluster trials. Cunningham 2009 and Praveen 2014 did not give their ICC values, and we borrowed the ICC value given by the Milsom 2006 trial. This could be one of the reasons for the heterogeneity. Due to the inconsistency, we downgraded the evidence to 'very low certainty' and are unable to draw conclusions about this comparison (Analysis 1.1; Figure 5).

Figure 5. Forest plot of comparison: 1 Traditional screening versus no screening, outcome: 1.1 Dental attendance



Criteria-based screening versus no screening

This comparison was evaluated through two arms of two trials (Milsom 2006; Cunningham 2009). It showed a pooled effect estimate of RR 1.07 (95% CI 0.99 to 1.16; low certainty of evidence), which suggested a possible benefit for screening (Analysis 2.1).

Criteria-based versus traditional screening

Cunningham 2009 and Milsom 2006 also evaluated this comparison and we found an effect estimate of RR 1.01 (95% CI 0.94 to 1.08; very low certainty of evidence), providing no evidence of a difference between these methods for increasing dental attendance or registration (Analysis 3.1).

Specific (personalised) referral letter versus non-specific referral letter

Burden 1994 compared two types of referral letters after screening, results were significantly in favour of specific referral letter for increasing dental attendance at the general dentist clinics with an effect estimate of RR 1.39 (95% CI 1.09 to 1.77), and for attendance at a specialist orthodontist with RR 1.90 (95% CI 1.18 to 3.06) (low certainty of evidence) (Analysis 4.1).

Screening plus motivation versus screening alone

Hebbal 2005 compared screening supplemented with motivation and offer of free treatment to screening alone, and showed attendance favouring motivation activity with an effect estimate of RR 3.08 (CI 95%, 2.57 to 3.71; low certainty of evidence) (Analysis 5.1).

Prevalence of dental caries and other dental diseases

A four-arm cluster-RCT with 16,684 participants described prevalence of dental caries per child as its primary outcome (Milsom 2006). Prevalence of dental caries was originally measured as mean number of teeth with active caries in primary (dt) and permanent teeth (DT). However post-intervention changes of dt and DT were not significantly different from baseline scores, hence data of the study were presented in binary outcomes (yes/no) depicting reduction from baseline and we do not have data to conduct meta-analysis. Similarly, prevalence of other diseases was measured as dichotomous data. Prevalence of sepsis, presence of gross plaque or calculus and trauma to the permanent incisor teeth were other secondary outcomes in this trial. The trial demonstrated no significant difference between prevalence of dental caries per child or other outcomes across the four arms of the study.

Cost effectiveness

None of the trials reported cost effectiveness of school dental screening programmes.

Adverse events

None of the trials reported any adverse events or harms of screening activity.

ADDITIONAL SUMMARY OF FINDINGS *[Explanation]*

Criteria-based screening compared to no screening for increasing dental attendance

Population: school children
Setting: primary and secondary schools
Intervention: criteria-based screening
Comparison: no screening

Outcomes	Relative effect (95% CI)	Anticipated absolute effects* (95% CI)			Number of participants (studies)	Certainty of the evidence (GRADE)	Comment
		With no screening	With criteria-based screening	Difference			
Dental attendance Follow-up: 3 to 4 months	RR 1.07 (0.99 to 1.16)	33.1%	35.5% (32.8 to 38.1)	2.3% more (0.3 fewer to 5 more)	4980 (2 RCTs)	⊕⊕◯◯ LOW [1]	There is 7% relative increase in the dental attendance in criteria-based screening group compared to no screening with 95% CI ranging from 1% decrease to 16% increase

***The risk in the intervention group** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: confidence interval; **RR:** risk ratio; **OR:** odds ratio

GRADE Working Group grades of evidence
High certainty: we are very confident that the true effect lies close to that of the estimate of the effect
Moderate certainty: we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low certainty: our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect

Very low certainty: we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1] Cunningham 2009 trial has wide CI ranging from no effect to favourable effect and ICC is borrowed from Milsom 2006 trial.
Downgraded by 2 levels

Criteria-based screening compared to traditional screening for increasing dental attendance

Population: school children
Setting: primary and secondary schools
Intervention: criteria-based screening
Comparison: traditional screening

Outcomes	Anticipated absolute effects* (95% CI)		Relative effect (95% CI)	Number of participants (studies)	Certainty of the evidence (GRADE)	Comments
	Risk with traditional screening	Risk with criteria-based screening				
Dental attendance follow-up: range 3 months to 4 months	335 per 1000	338 per 1000 (315 to 362)	RR 1.01 (0.94 to 1.08)	5316 (2 RCTs)	⊕◯◯◯ VERY LOW [1,2]	There is 1% relative increase in the dental attendance in criteria-based screening compared to traditional screening with 95% CI ranging from 6% decrease to 8% increase in the attendance

*__The risk in the intervention group__ (and its 95% confidence interval) is based on the assumed risk in the comparison group and the __relative effect__ of the intervention (and its 95% CI).
CI: confidence interval; **RR:** risk ratio; **OR:** odds ratio

GRADE Working Group grades of evidence
High certainty: We are very confident that the true effect lies close to that of the estimate of the effect
Moderate certainty: We are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low certainty: Our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
Very low certainty: We have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1] Results of both the trials are ranging from favouring traditional screening to no effect. Downgraded by two levels.
[2] Wide 95% CI in Cunningham 2009 trial crossing the line of no effect. Downgraded by one level

Criteria-based screening with specific referral compared to criteria-based screening with non-specific referral for increasing dental attendance

Population: school children
Setting: secondary school
Intervention: criteria-based screening with specific referral
Comparison: criteria-based screening with non-specific referral

Outcomes	Relative effect (95% CI)	Anticipated absolute effects* (95% CI)			Number of participants (studies)	Certainty of the evidence (GRADE)	Comment
		With criteria-based screening with non-specific referral	With criteria-based screening with specific referral	Difference			
Dental attendance at general dentist Follow-up: mean 8 months	RR 1.39 (1.09 to 1.77)	49.0%	68.1% (53.4 to 86.7)	19.1% more (4.4 more to 37.7 more)	201 (1 RCT)	⊕⊕◯◯ LOW [1]	There is 39% relative increase in the attendance to general dentist in the specific referral group compared to non-specific group, with 95% CI ranging from 9% to 77% increase in attendance
Dental attendance at orthodontist Follow-up: mean 8 months	RR 1.90 (1.18 to 3.06)	19.4%	36.8% (22.9 to 59.3)	17.4% more (3.5 more to 39.9 more)	201 (1 RCT)	⊕⊕◯◯ LOW [1]	There is 90% relative increase in the attendance to orthodontist in the specific referral group compared to the non-specific group with 95% CI ranging from 18% to 206% increase in attendance

*__The risk in the intervention group__ (and its 95% confidence interval) is based on the assumed risk in the comparison group and the __relative effect__ of the intervention (and its 95% CI).
CI: confidence interval; **RR**: risk ratio; **OR**: odds ratio

GRADE Working Group grades of evidence
High certainty: we are very confident that the true effect lies close to that of the estimate of the effect
Moderate certainty: we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low certainty: our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
Very low certainty: we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1] Burden 1994 is a single study of secondary school children (11 to 12 years) at unclear risk of selection bias. Downgraded by two levels

Traditional screening with motivation compared to traditional screening for increasing dental attendance

Patient or population: school children
Setting: primary and secondary schools
Intervention: traditional screening with motivation
Comparison: traditional screening

Outcomes	Relative effect (95% CI)	Anticipated absolute effects* (95% CI)			Number of participants (studies)	Certainty of the evidence (GRADE)	What happens
		Without traditional screening with motivation	With traditional screening with motivation	Difference			
Dental attendance Follow-up: mean 3 months	RR 3.08 (2.57 to 3.71)	10.0%	30.9% (25.8 to 37.2)	20.9% more (15.7 more to 27.2 more)	2486 (1 RCT)	⊕⊕◯◯ LOW [1]	There is 208% relative increase in the attendance of the motivation group compared to control group with 95% CI ranging from 157% to 271% increase in attendance

***The risk in the intervention group** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: confidence interval; **RR**: risk ratio; **OR**: odds ratio

GRADE Working Group grades of evidence
High certainty: we are very confident that the true effect lies close to that of the estimate of the effect
Moderate certainty: we are moderately confident in the effect estimate: the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low certainty: our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect
Very low certainty: we have very little confidence in the effect estimate: the true effect is likely to be substantially different from the estimate of effect

[1] Hebbal 2005 trial has unclear risk of selection bias and high risk of performance bias. Downgraded by two levels

DISCUSSION

Summary of main results

The main objective of this review was to assess the effectiveness of school dental screening programmes on overall oral health status and use of dental services. We included six RCTs in our review. We assessed two trials as low risk of bias, one trial as high risk of bias, and three trials as unclear risk of bias.

None of the included trials reported on the proportion of children with untreated caries or other untreated oral health conditions.

All six trials reported attendance or registration at the dentist as their main outcome. The methods of screening interventions varied across the trials and only four trials could be grouped in meta-analysis on the basis of common comparison of traditional screening versus no screening with dental attendance as an outcome. We combined data from these studies for the outcome 'dental attendance' and found an inconclusive result with high heterogeneity. The heterogeneity was found to be, in part, due to study design (three cluster-RCTs and one individual-level RCT). Due to the inconsistency, we downgraded the evidence to 'very low certainty' and are unable to draw conclusions about this comparison (see Summary of findings for the main comparison).

Two trials compared criteria-based screening to no screening (Milsom 2006; Cunningham 2009). They found a 7% relative increase in the dental attendance in the criteria-based screening group compared to no screening, with 95% CI ranging from 1% decrease to 16% increase (see Summary of findings 2).

The same two trials compared criteria-based screening to traditional screening (Milsom 2006; Cunningham 2009). They found a 1% relative decrease in dental attendance in criteria-based screening compared to traditional screening, but with 95% CI ranging from 6% decrease to 8% increase in attendance (see Summary of findings 3).

The comparisons of the other two trials are described independently because they used screening interventions of different designs (Burden 1994; Hebbal 2005).

Burden 1994 compared criteria-based screening with specific referral to criteria-based screening with non-specific referral for the dental attendance outcome in general dentists and orthodontists. They found a 39% relative increase in the attendance at a general dentist in the specific referral group compared to the non-specific group, with 95% CI ranging from 9% to 77% increase in attendance (see Summary of findings 4). They found a 90% relative increase in attendance at the orthodontist in the specific referral group compared to the non-specific group with 95% CI ranging from 18% to 206% increase in attendance (see Summary of findings 4). It demonstrated higher attendance at the dentist in the group that was given a personalised referral letter describing orthodontic problems. Furthermore, results demonstrated that significantly more children from this specific referral letter group sought orthodontist specialist services in comparison to the non-specific referral letter group. This trial signals that specific information through a referral letter prompts parents to visit the dentist compared to a non-specific counterpart.

A cluster-randomised trial of 4500 school children supplemented traditional screening with motivation (oral health education, offer of free treatment and motivation to parents through school authorities) and compared it with traditional screening alone (Hebbal 2005). There was a 208% relative increase in the attendance of the motivation group compared to the control group with 95% CI ranging from 157% to 271% increase in attendance (see Summary of findings 5). Even though results of this trial reflect improved response rate in the group that was given oral health motivation along with screening, it cannot be ascribed to the effect of screening per se. Aforementioned oral health motivation activities supplemented with screening activity can be credited for the increased attendance in this group of children rather than the process of screening itself.

Only one trial reported the prevalence of dental caries and other oral diseases (prevalence of sepsis; presence of gross plaque or calculus; and trauma to the permanent incisor teeth) as its outcomes (Milsom 2006). It demonstrated no significant difference between prevalence of dental caries per child or other outcomes across the four arms of the study.

An observational prospective cohort study of Milsom 2006 followed up children from two arms of the trial (traditional screening and criteria-based screening) to describe attendance data on the basis of the socioeconomic quintile. It also presented data on the number of children that went on to receive appropriate treatment amongst those who attended the dentist. This study brings to light that in both the screening arms, children from deprived quintiles constituted higher referral percentages compared to affluent quintiles. Moreover, affluent quintiles when referred were more likely to attend the dentist than children in the most deprived quintiles. It also puts across a noteworthy statistic that amongst children attending the dentists, less than a quarter receive appropriate treatment.

As none of the studies reported cost-effectiveness we could not assess the cost-benefit aspect of school dental screening. It is an important area of concern for governments and administration at various levels as the process requires heavy investment in terms of finances and manpower, with co-ordination of activities from healthcare and education sectors.

None of the included studies reported any data on adverse events.

Overall completeness and applicability of evidence

We systematically searched for trials according to the methodology written in our protocol. We included all RCTs that met the inclusion criteria for our review. The methods of screening and strength of referral varied considerably within and across the trials. We were helped by translators for studies written in languages the review authors do not know.

We proposed to measure short-term and long-term effects of screening with proportion of children with untreated dental caries, proportion of children with untreated other oral diseases and dental attendance as primary outcomes. However, we did not find any trials following up screening activity over a long course. All the included trials measured dental attendance, and none reported proportion of children with untreated dental caries or other oral diseases.

Amongst secondary outcomes proposed in our review, only one trial measured prevalence of dental caries, gross plaque or calculus and trauma to incisor teeth. We did not find any trial describing cost of screening, nor did any trial report adverse effects.

We included in the meta-analysis all those trials whose methodology of screening and referral were comparable. Arms of trials where screening and referral procedure were atypical have been analysed and explained separately. We did not exclude any trial due to missing data. The trial arms where letters or leaflets were sent to parents without oral examination have not been considered in this review, as this is oral health promotion activity rather than screening.

This review has limited evidence on oral health improvement or increase in dental clinic attendance because of school dental screening. However, the review encourages further high-quality RCTs with primary outcomes of proportion of children with active/untreated caries and other diseases, followed up over the long term (more than two years) to derive definitive conclusions and recommendations.

Quality of the evidence

We assessed the body of evidence for a single commonly reported outcome, i.e. dental attendance. This was done using GRADE (version 3.6; GRADEpro GDT), which incorporates study limitations (risk of bias), indirectness of evidence, inconsistency of results, imprecision of the estimates, and risk of publication bias. With respect to traditional screening compared to no screening, there were four RCTs with 6281 participants. We downgraded the quality of evidence by three levels because of inconsistency, imprecision and risk of bias. The certainty of evidence is 'very low' (Summary of findings for the main comparison). The results do not allow us to draw a robust conclusion regarding the improvement in dental attendance.

When we compared pooled data of two trials comparing criteria-based screening to no screening, we downgraded the certainty of evidence by two levels because a trial demonstrated inconsistency and the ICC of this trial was borrowed from another. Hence, the certainty of evidence is 'low' (Summary of findings 2)

We assessed the certainty of evidence as 'very low' for the criteria-based screening versus traditional screening comparison (Summary of findings 3). We downgraded the level of evidence by a total of three levels: two because of inconsistency in both trials and one level due to imprecision.

The certainty of evidence for a comparison between a specific referral letter and a non-specific letter (described by a single study) we determined to be 'low', owing to high risk of bias favouring intervention (Summary of findings 4).

Similarly, we downgraded by two levels the certainty of evidence for a comparison between traditional screening supplemented with motivation and traditional screening alone (described by a single study) owing to high risk of bias. The overall certainty we determined to be 'low' (Summary of findings 5).

Potential biases in the review process

We have taken steps to minimise bias at every stage of the review. We searched the above-mentioned databases, conference proceedings, and trial registries to include all relevant reports. We tried to contact trial authors for missing data through emails. If the reports were very old, we tried to get the contact details of the authors through peer contacts, Google search, Facebook search and university/hospital web sites where they were previously affiliated. Nevertheless there could be unpublished data that we could not trace with the above methods. We tried our best to follow the methodology stated in the protocol.

Agreements and disagreements with other studies or reviews

We found only one other similar systematic review, which described effectiveness of screening on improving oral health in children based on reports from five trials (Joury 2017).

We found 2237 records through our database search results whereas Joury 2017 found 2369. This may be attributed to a difference in the Embase search. We restricted the search of this database to the last six months, due to a Cochrane project to identify all of the trials on this database and add them to CENTRAL. Out of the five trials included by Joury 2017, our review includes four trials (Hebbal 2005; Milsom 2006; Cunningham 2009; Praveen 2014). Our review included Burden 1994 and Zarod 1992 in addition to the these four trials, but we excluded Donaldson 2001 as it had less than three months' follow-up.

Results of our meta-analysis are in agreement with Joury 2017, which stated there was no evidence of improvement in dental attendance or reduction in dental caries or other diseases between 'screening' and 'no screening' groups.

AUTHORS' CONCLUSIONS

Implications for practice

For a long time, school dental screening has held a confident and important place in public health practice as it seems of obvious

value. However, in this systematic review we found very low certainty to low-certainty evidence, which is insufficient to draw conclusions about the role of school dental screening for improvement in dental attendance. There is an absence of evidence to comment on the efficacy of school dental screening to improve oral health.

In this systematic review, we found some evidence that screening as a dual process of clinical examination and informing parents of their child's oral health status might bring enhanced clinical effects if the process of information were strengthened with specific or personalised referrals or periodic reminders. Oral health education and reminders to parents through school may increase motivation in parents to bring their child to a dentist.

Implications for research

All of the trials except one measured only dental attendance as the primary outcome. Even though dental attendance post screening is a desirable outcome, it does not guarantee further follow-up to completion of treatment and is not a measure of improved oral health.

The studies in this review were followed up for an average period of three to four months. None of the studies reported long-term effects of screening. Research that assesses long-term effects of screening with cost-benefit analysis of screening activities would help establish whether or not screening activities are more effective than standard care.

We recommend standardisation of definitions of school dental screening programmes so that future research can be based on it. We encountered different terminology for various screening and referral procedures, for example 'criteria-based' screening, and screening with 'specific' referral letters. We cannot comment on universal application of these terms and it makes drawing comparisons difficult. Hence we also recommend standardising various forms of screening processes.

We recommend the conduct and reporting of clinical trials be improved by following the CONSORT group guidelines.

Population: Clinical trials should be conducted in middle- and low-income countries to provide local evidence for policy making in these nations. School children should be stratified based on their economic background to study the influence of socioeconomic status on dental attendance.

Intervention: In future, high-quality trials related to school dental screening should include incremental dental care (periodic dental care in a step-wise manner treated in tandem with prompt diagnosis) so that dental needs do not accumulate over time. We need trials with incentives in order to motivate parents to seek dental treatment for their children.

Comparison: Further trials should include various forms of screening and motivational factors (examples: oral health education, parental education, reinforcement by school authorities and personalised or specific referral letters), supplemented with screening.

Outcome: Future trials should assess the proportion of children with dental caries and other diseases, measured over a longer duration, to observe treatments provided and completed in children attending the dentist. This will present a better picture of effectiveness of screening than merely a snapshot of attendance measured with follow-up of three to four months. We also recommend that trials include cost-effectiveness as one of their outcomes.

Time stamp: 15 March 2017. Date of recommendation: 5 December 2017

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* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Burden 1994

Methods	RCT Period of study: not given
Participants	**Participants**: **201** Children aged 11 to 12 years attending state-maintained secondary school **Inclusion criteria**: 1) score of grade 4 or grade 5 in the Dental Health Component or grade 8, 9 or 10 in the Aesthetic Component of Index of Treatment Need (IOTN) (in need of orthodontic treatment) 2) not wearing a brace 3) not planned for brace **Exclusion criteria**: 1) caries or periodontal disease clearly needing treatment
Interventions	**Comparison**: screening programme based on IOTN, followed by referral letters to parents: 'personalised' referral letter versus 'non-specific' referral letter in increasing registration and improving access to general and orthodontist specialist services **Intervention**: screening followed by 'specific letter' advising the parent to seek treatment to straighten their child's teeth (n = 103) **Control**: screening followed by 'non-specific letter' advising parents to seek dental advice for their child (n = 98)
Outcomes	**Outcomes used in this review**: 1) number of children attending dentist **Outcomes reported not used in quantitative synthesis in this review**: 2) number of children accessing orthodontic specialist services **Method of outcome measure**: questionnaire to parents and confirmed from dental records **Duration of follow-up**: 8 months
Notes	**Language**: English **Funded by**: "This study was supported in part by the T. C. White Fund. Royal College of Physicians and Surgeons of Glasgow" **Costs of follow-up**: not reported **Conducted in**: Manchester, UK **Unit of randomisation**: individual, within pairs matched for sex, ethnic background, dental disease and scores on index of orthodontic treatment needs **Author contact information**: not reported in the publication; tried contacting authors through university address but email-ids not procured

Risk of bias

Bias	Authors' judgement	Support for judgement

Burden 1994 (*Continued*)

Random sequence generation (selection bias)	Unclear risk	Quote: "Each child within a matched pair was allocated at random by a toss of a coin to either test or control group. The few children who could not be paired were evenly allocated at random to either the test or control group."
Allocation concealment (selection bias)	Low risk	"Once the children had been allocated, sealed letters of referral addressed to the parent or guardian were delivered to the school"
Blinding of participants and personnel (performance bias) All outcomes	Low risk	Participants: sealed referral letters were sent home, Personnel: although it is unclear that authors were blinded to type of letter sent home, test and control groups were matched for sex, socioeconomic status, dental disease and aesthetic impairment, which is unlikely to introduce bias
Blinding of outcome assessment (detection bias) All outcomes	Low risk	The only outcome reported was attendance, which is an objective outcome
Incomplete outcome data (attrition bias) All outcomes	Low risk	No loss to follow-up in the study; all the participants were accounted for in analysis
Selective reporting (reporting bias)	Low risk	Nature of outcome measure unlikely to introduce bias.
Other bias	Low risk	No other source of bias.

Cunningham 2009

Methods	Four-arm, assessor-blinded, cluster-RCT
Participants	First-year students in age group of 12 to 13 years old, attending all 65 state secondary schools in Lothian and Fife (n = 12,765). The study excluded those children who registered with GDS and CDS from analysis and included only unregistered children which makes the total n = 3923 **Inclusion criteria:** children aged 12 to 13 years during the academic year 2003/04 **Exclusion criteria:** children registered for treatment in any facility
Interventions	**Comparison:** the effectiveness of a 'personalised' referral letter' combined with screening versus 'traditional' referral letter **Interventions:** 1: Unregistered children were inspected followed by sending 'personalised' letter to attend dentist (n = 1175) 2: No inspection was done, unregistered children were sent letters. (n = 971) - (data not considered for meta-analysis) 3: All children were examined based on standard criteria and sent letters (n = 958)

	Control: 4**:** The children were neither inspected nor sent a letter (n = 819)
Outcomes	**Outcomes used in quantitative synthesis**: changes in registration status of unregistered children (dental attendance) **Outcomes reported but not used in quantitative synthesis**: a further analysis was included to investigate for differences in children who had never been listed as registered with an NHS GDP and those who had been at one time registered (lapsed more than 9 months) **Duration of follow-up**: 3 months
Notes	**Language:** English **Funded by:** grant OOB/3/19/F29 from the Primary Care Research Fund of the Chief Scientist Office, Scottish Executive **Costs of follow-up**: not reported **Conducted in:** Scotland, UK Data from Groups 1, 3 and 4 contribute to the data in this review, Group 2 (being a group where oral inspection was not carried out) does not fall under definition of 'school dental screening' as per protocol *This study was targeted only at unregistered children, who are further classified into 'never registered' or 'registration lapsed' by the study report **Unit of randomisation:** cluster **Author contact information:** chris.cunningham@nhslothian.scot.nhs.uk Contacted author for information on ICC but did not receive a reply. We mailed the author a query on clarification on the data as there was mismatch in data written in the table and study flow chart. However, we did not receive reply and used data presented in the study table

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Quote: "the schools were randomly allocated using a computer- generated sequence to one of four groups by the study statistician blinded to the interventions that each would receive which was not revealed until completion of analysis."
Allocation concealment (selection bias)	Low risk	Quote: "the schools were randomly allocated using a computer-generated sequence to one of four groups by the study statistician blinded to the interventions that each would receive which was not revealed until completion of analysis."
Blinding of participants and personnel (performance bias) All outcomes	Low risk	Although this was an unblinded study, the cluster design and the nature of the intervention makes performance bias unlikely

Cunningham 2009 (*Continued*)

Blinding of outcome assessment (detection bias) All outcomes	Low risk	Allocation was concealed. Outcome is available from electronic records
Incomplete outcome data (attrition bias) All outcomes	Low risk	No loss to follow-up in the study; all participants were accounted for in the analysis
Selective reporting (reporting bias)	Low risk	All pre-stated outcomes in the methods reported
Other bias	Low risk	No other source of bias detected

Hebbal 2005

Methods	Cluster RCT
Participants	**Inclusion criteria**: school-going children of Davangere city who were between 6 and 15 years old **Exclusion criteria**: not reported
Interventions	**Comparison**: the effectiveness of screening programme with motivation in increasing dental registration compared to screening alone **Interventions**: 1) screening and referral card supplemented with oral health education to children and motivation to parents from school authorities (no. of clusters = 7, n = 2100) 2) received screening and referral cards (no. of clusters =7, n =2400)
Outcomes	**Primary outcome**: response rate (dental attendance) **Duration of follow-up**: 3 months
Notes	**Language**: English **Funded by**: not mentioned **Costs of follow-up**: not reported **Conducted in**: Davangere, India **Unit of randomisation**: cluster **Author contact information**: drmamatahebbal@yahoo.co.in We tried contacting authors for information on results, ICC values and risk of bias assessment queries, but did not receive a reply

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	"These twenty schools were then subjected to a two-stage simple random sampling technique for selection of the schools. In the first stage, fourteen schools were selected out of twenty by lottery method, and in the second stage these schools were assigned

Hebbal 2005 (*Continued*)

		randomly either to the study or control group (seven schools in each group)" Comment: no mention of how schools were allocated to study or control group
Allocation concealment (selection bias)	Unclear risk	No details given
Blinding of participants and personnel (performance bias) All outcomes	High risk	"The response rate was calculated during the three-month period from the date of initiation of the school screening program. During this period, the students who visited the dental college from the control group were examined, and dental findings were recorded. In order to obtain data regarding the number of children requiring treatment in the control group, a separate screening program was conducted after the waiting period of three months." Comment: children from study group had additional interventions like free treatment and also special attention from school authorities apart from standard intervention planned
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Primary outcome is attendance. Quote, "were investigated by using a computer program validated to be approximately 95% accurate" Comment: risk of detection bias is low.
Incomplete outcome data (attrition bias) All outcomes	Low risk	No loss to follow-up in the study; all participants were accounted for in the analysis
Selective reporting (reporting bias)	Low risk	All pre-stated outcomes in the methods were reported.
Other bias	Low risk	No other source of bias

Milsom 2006

Methods	Cluster RCT (four arm)
Participants	**Participants**: 17,098 **Inclusion criteria**: all children aged between six and eight years old in state maintained schools **Exclusion criteria**: 1) children attending special schools 2) children whose parents declined invitation 3) children who refused to be examined on the day of examination 4) children who were present at the time of outcome examination, but not at the baseline
Interventions	**Comparison**: tested three models of screening against a control **Interventions**: 1) 'criteria-based' screening: screening was based on a set of clinical criteria prepared as per consensus view from clinicians that would prompt a referral following a screening

	examination. Referral letter was posted to parents. (n = 4087) 2) 'traditional model' - according to the principle that a child is referred if, in the opinion of the screening dentist, dental care is required. Referral was posted to parents. (n = 4418) 3) 'dental information leaflet' distributed via the schools, which encouraged parents to examine their child's mouth and to take their child to a dentist if any problems were noted. (n = 4133) (data not considered for meta-analysis) **Control:** no intervention during the study period (n = 4226)
Outcomes	**Duration of follow-up**: 4 months after baseline **Outcomes reported and used in quantitative synthesis for the review**: 1) dental attendance **Outcomes reported and not used in quantitative synthesis for the review**: **Primary outcome**: 1) prevalence (DT > 0) and mean number of teeth with active caries (DT) in the permanent dentition and prevalence (dt > 0) and mean number of teeth with active caries (dt) in the primary dentition **Secondary outcome**: prevalence of oral sepsis, gross plaque or calculus and dental trauma to incisor teeth
Notes	**Language:** English **Funded by:** project grant from the NHS Executive North West R&D Directorate **Costs of follow-up**: not reported **Conducted in:** UK **Unit of randomisation**: cluster Data from Group 1, 2 and 4 contribute to the data in this review, Group 3 'Dental information leaflet', being a group where oral inspection was not carried out, do not fall under definition of 'school dental screening' as per protocol **Author contact information:** martin.tickle@manchester.ac.uk.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Quote: "The schools within each stratum were randomly allocated to four intervention arms by reference to a random number table."
Allocation concealment (selection bias)	Low risk	Quote: "The study statistician carried out the stratified randomisation and concealed the randomisation codes from the field workers and co-investigators until analysis was complete."
Blinding of participants and personnel (performance bias) All outcomes	Low risk	Participants: no blinding of schools or participants due to nature of intervention but cluster randomisation at the level of school is deemed by the authors to account for this Personnel: quote: "The leaflet was distributed to the children by school staff."

Milsom 2006 (*Continued*)

Blinding of outcome assessment (detection bias) All outcomes	Low risk	Quote: "Outcome epidemiological examinations were undertaken in the schools, after a four-month period, by trained and calibrated dental examiners who were blinded to the study arm to which each school had been allocated."
Incomplete outcome data (attrition bias) All outcomes	Low risk	Quote: "These analyses were performed on all children included in the randomisation (N=16,864)"
Selective reporting (reporting bias)	Low risk	Although we did not find trial registration in a trial registry, all pre-stated outcomes in the methods were reported
Other bias	Low risk	No other source of bias

Praveen 2014

Methods	RCT
Participants	**Inclusion criteria**: school children aged 6 to 13 years **Exclusion criteria**: 1) Children whose parents declined the invitation to participate 2) Children who refused to be screened on the day 3) Children who were present at the time of outcome measurement but not at the baseline examination
Interventions	**Comparison**: compared the effectiveness of screening versus no screening in increasing dental attendance **Intervention**: screening followed by referral card sent to parent (n = 300) **Control:** no screening and no referral card (n = 300)
Outcomes	**Duration of follow-up**: 3 months **Primary outcome**: dental attendance rate
Notes	**Language**: English **Funded by**: not mentioned **Costs of follow-up**: not reported **Conducted in**: Vikarabad, India **Unit of randomisation**: cluster **Author contact information**: gaddephd6@gmail.com We contacted author for information on sampling procedure and ICC values but did not receive a reply

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Quote: "These 37 schools were then subjected to a two stage sampling technique for the selection of schools. In the first phase, 16 schools were selected by lottery method and in the second

Praveen 2014 (*Continued*)

		stage these schools were assigned randomly to either study or control group" - no mention of how randomisation was done
Allocation concealment (selection bias)	Unclear risk	Not reported
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	Not reported
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Low risk	No loss to follow-up in the study; all participants were accounted for in the analysis
Selective reporting (reporting bias)	Low risk	All pre-stated outcomes in the methods were reported
Other bias	Low risk	No other source of bias

Zarod 1992

Methods	**Study design:** RCT
Participants	**Participants**: 528 **Inclusion criteria**: school children aged 4 to 6 years attending primary schools in Wallasey **Exclusion criteria**: children requiring immediate treatment like pulp treatment, extractions or have received such treatment in recent past **Age at baseline**: 4 to 6 years old **Gender**: not mentioned **Number of participants randomised**: test group n = 270, control group n = 258 **Number of participants evaluated**: test group n = 262, control group n = 243
Interventions	**Comparison**:'screening followed by referral letter versus without follow-up letter' programme for increasing dental attendance. Secondly, the study compared effectiveness of dental screening in areas of contrasting socioeconomic status **Intervention**: baseline screening followed by referral letter to parents via child (n = 270) A second letter was mailed to child's home address if return slip was not returned within a week of the first letter. If response to either was not received within 21 days, personal telephone was made wherever possible. **Control**: baseline screening without any further communication, (n = 258)
Outcomes	**Duration of follow-up**: 4 months after baseline screening **Primary outcome**: dental attendance in both groups
Notes	**Language**: English **Funded by**: not mentioned

	Costs of follow-up: not reported **Conducted in**: UK **Unit of randomisation**: individual **Author contact information**: m.a.lennon@sheffield.ac.uk No reply received on queries for risk of bias assessment

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	No information
Allocation concealment (selection bias)	Unclear risk	No information
Blinding of participants and personnel (performance bias) All outcomes	Unclear risk	No information
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	No information
Incomplete outcome data (attrition bias) All outcomes	Low risk	Loss of follow-up of 8 children from test group and 15 children from control group was noted in the trial. However, these numbers would not have affected the overall results (based on our intention-to-treat analysis)
Selective reporting (reporting bias)	Low risk	All pre-stated outcomes in the methods were reported.
Other bias	Low risk	No other source of bias

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Adam 2005	Not a RCT; it is a descriptive survey.
Al Johara 2010	Not a RCT; it is a cross-sectional questionnaire study.
Baglee 2000	Full text not available. Neither university nor authors could be contacted
Binder 1973	Text fails to explain if participants were divided randomly into intervention and control group
Cruz 2012	Not a school oral health screening programme. Postcards were sent to parents without oral examination

(*Continued*)

Donaldson 2001	Follow-up period was less than 3 months.
Glenny 2013	Oral examination was not done in any of the group in study.
Haleem 2011	Intervention is oral health education and not dental screening
Harrison 2003	Not an RCT
Holst 1975	Not an RCT
Locker 2004	No follow-up for attendance or reduction of disease
Mbawalla 2013	Intervention is oral health education and not dental screening
Morrant 1995	Not an RCT
Nelson 2012	Not an RCT; it is a cohort study.
Petersen 2004	Intervention is oral health promotion and not dental screening
Rodgers 2007	No communication was sent to parents.
Tantawi 2015	No follow-up on use of services or reduction of disease

Characteristics of ongoing studies *[ordered by study ID]*

Nelson 2015

Trial name or title	Nelson 2015
Methods	A multi-site randomised controlled trial with caregivers of kindergarten to 4th grade children in urban Ohio and rural Washington State to compare five arms
Participants	K-4th grade children of 10 schools in three school districts
Interventions	Screening and referral Five arms compared were: (1) CSM referral letter alone: this referral letter sent to parents was on the basis of a behavioural approach - 'Common sense model of Self-regulation'. It is a model wherein an individual creates a mental representation of illness on the basis of abstract and concrete sources of information. The CSM proposes that people plan actions and/or coping mechanisms on the basis of cognitive and emotional perception of a disease (2) CSM referral letter + DIG (Dental Infomation guide): "DIG presented as a brochure with illustrations which provides myths and facts about dental caries, making appointments and Medicaid access, transportation and dentist availability resources." (3) reduced CSM referral letter alone (4) reduced CSM referral letter + DIG

Nelson 2015 (*Continued*)

	(5) standard (control) referral
Outcomes	Primary: receipt of dental care
Starting date	Summer 2015
Contact information	Suchitra Nelson, Department of Community Dentistry, School of Dental Medicine, Case Western Reserve University, 10900 Euclid Ave., Cleveland, Ohio 44106-4905, USA Email: sxn15@case.edu
Notes	Contacted the author for query regarding results of trial. Authors replied that results are being tabulated and processed, publication is expected in 2018

DATA AND ANALYSES

Comparison 1. Traditional screening versus no screening

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Dental attendance	4		Risk Ratio (IV, Random, 95% CI)	Subtotals only
1.1 Cluster RCT with ICC 0.03	3	5776	Risk Ratio (IV, Random, 95% CI)	1.10 [0.89, 1.35]
1.2 Individual-level RCT	1	505	Risk Ratio (IV, Random, 95% CI)	1.74 [1.47, 2.05]

Comparison 2. Criteria-based screening versus no screening

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Dental attendance	2	4980	Risk Ratio (IV, Random, 95% CI)	1.07 [0.99, 1.16]

Comparison 3. Criteria-based versus traditional screening

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Dental attendance	2	5316	Risk Ratio (IV, Random, 95% CI)	1.01 [0.94, 1.08]

Comparison 4. Criteria-based screening with specific referral versus criteria-based screening with non-specific referral

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Dental attendance	1	402	Risk Ratio (IV, Random, 95% CI)	1.52 [1.15, 2.00]
1.1 Attending general dentist	1	201	Risk Ratio (IV, Random, 95% CI)	1.39 [1.09, 1.77]
1.2 Attending orthodontist	1	201	Risk Ratio (IV, Random, 95% CI)	1.90 [1.18, 3.06]

Comparison 5. Traditional screening with motivation versus traditional screening alone

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Dental attendance	1	2486	Risk Ratio (IV, Random, 95% CI)	3.08 [2.57, 3.71]

Analysis 1.1. Comparison 1 Traditional screening versus no screening, Outcome 1 Dental attendance.

Review: School dental screening programmes for oral health

Comparison: 1 Traditional screening versus no screening

Outcome: 1 Dental attendance

Study or subgroup	Screening n/N	No screening n/N	Risk Ratio IV,Random,95% CI	Weight	Risk Ratio IV,Random,95% CI
1 Cluster RCT with ICC 0.03					
Cunningham 2009 (1)	116/827	91/577		29.5 %	0.89 [0.69, 1.15]
Milsom 2006 (2)	824/1981	728/1895		48.1 %	1.08 [1.00, 1.17]
Praveen 2014 (3)	67/248	45/248		22.4 %	1.49 [1.07, 2.08]
Subtotal (95% CI)	**3056**	**2720**		**100.0 %**	**1.10 [0.89, 1.35]**
Total events: 1007 (Screening), 864 (No screening)					
Heterogeneity: Tau^2 = 0.02; Chi^2 = 5.81, df = 2 (P = 0.05); I^2 =66%					
Test for overall effect: Z = 0.86 (P = 0.39)					
2 Individual-level RCT					
Zarod 1992	191/262	102/243		100.0 %	1.74 [1.47, 2.05]
Subtotal (95% CI)	**262**	**243**		**100.0 %**	**1.74 [1.47, 2.05]**
Total events: 191 (Screening), 102 (No screening)					
Heterogeneity: not applicable					
Test for overall effect: Z = 6.55 (P < 0.00001)					
Test for subgroup differences: Chi^2 = 11.32, df = 1 (P = 0.00), I^2 =91%					

(1) We have divided the number of events and total number of participants by effect estimate calculated using the ICC of 0.03 as borrowed from the Milsom 2006 study.

(2) We have divided the number of events and total number of participants by effect estimate calculated using the ICC of 0.03 as given in the study.

(3) We have divided the number of events and total number of participants by effect estimate calculated using the ICC of 0.03 as borrowed from the Milsom 2006 study.

Analysis 2.1. Comparison 2 Criteria-based screening versus no screening, Outcome 1 Dental attendance.

Review: School dental screening programmes for oral health

Comparison: 2 Criteria-based screening versus no screening

Outcome: 1 Dental attendance

Study or subgroup	Criteria-based n/N	No screening n/N	Risk Ratio IV,Random,95% CI	Weight	Risk Ratio IV,Random,95% CI
Cunningham 2009 (1)	107/675	91/577		8.6 %	1.01 [0.78, 1.30]
Milsom 2006 (2)	760/1833	728/1895		91.4 %	1.08 [1.00, 1.17]
Total (95% CI)	**2508**	**2472**		**100.0 %**	**1.07 [0.99, 1.16]**

Total events: 867 (Criteria-based), 819 (No screening)

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.27$, df = 1 (P = 0.60); $I^2 = 0.0\%$

Test for overall effect: Z = 1.83 (P = 0.068)

Test for subgroup differences: Not applicable

(1) We have divided the number of events and total number of participants by effect estimate calculated using the ICC of 0.03 as borrowed from the Milsom 2006 study.

(2) We have divided the number of events and total number of participants by effect estimate calculated using the ICC of 0.03 as given in the study.

Analysis 3.1. Comparison 3 Criteria-based versus traditional screening, Outcome 1 Dental attendance.

Review: School dental screening programmes for oral health

Comparison: 3 Criteria-based versus traditional screening

Outcome: 1 Dental attendance

Study or subgroup	Favours criteria-based n/N	Favours traditional n/N	Risk Ratio IV,Random,95% CI	Weight	Risk Ratio IV,Random,95% CI
Cunningham 2009 (1)	107/675	116/827		8.8 %	1.13 [0.89, 1.44]
Milsom 2006 (2)	760/1833	824/1981		91.2 %	1.00 [0.92, 1.07]
Total (95% CI)	**2508**	**2808**		**100.0 %**	**1.01 [0.94, 1.08]**

Total events: 867 (Favours criteria-based), 940 (Favours traditional)

Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.94$, df = 1 (P = 0.33); $I^2 = 0.0\%$

Test for overall effect: Z = 0.21 (P = 0.83)

Test for subgroup differences: Not applicable

(1) We have divided the number of events and total number of participants by effect estimate calculated using the ICC of 0.03 as borrowed from the Milsom 2006 study.

(2) We have divided the number of events and total number of participants by effect estimate calculated using the ICC of 0.03 as given in the study.

Analysis 4.1. Comparison 4 Criteria-based screening with specific referral versus criteria-based screening with non-specific referral, Outcome 1 Dental attendance.

Review: School dental screening programmes for oral health

Comparison: 4 Criteria-based screening with specific referral versus criteria-based screening with non-specific referral

Outcome: 1 Dental attendance

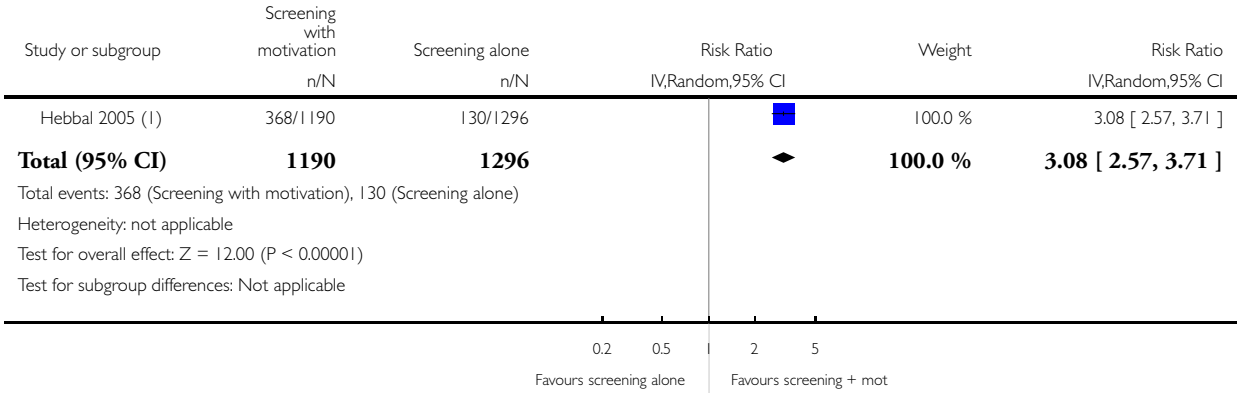
Study or subgroup	Specific referral n/N	Non-specific referral n/N	Risk Ratio IV,Random,95% CI	Weight	Risk Ratio IV,Random,95% CI
1 Attending general dentist					
Burden 1994	70/103	48/98		72.0 %	1.39 [1.09, 1.77]
Subtotal (95% CI)	**103**	**98**		**72.0 %**	**1.39 [1.09, 1.77]**
Total events: 70 (Specific referral), 48 (Non-specific referral)					
Heterogeneity: not applicable					
Test for overall effect: Z = 2.66 (P = 0.0079)					
2 Attending orthodontist					
Burden 1994	38/103	19/98		28.0 %	1.90 [1.18, 3.06]
Subtotal (95% CI)	**103**	**98**		**28.0 %**	**1.90 [1.18, 3.06]**
Total events: 38 (Specific referral), 19 (Non-specific referral)					
Heterogeneity: not applicable					
Test for overall effect: Z = 2.65 (P = 0.0081)					
Total (95% CI)	**206**	**196**		**100.0 %**	**1.52 [1.15, 2.00]**
Total events: 108 (Specific referral), 67 (Non-specific referral)					
Heterogeneity: Tau^2 = 0.01; Chi^2 = 1.34, df = 1 (P = 0.25); I^2 =26%					
Test for overall effect: Z = 2.93 (P = 0.0034)					
Test for subgroup differences: Chi^2 = 1.34, df = 1 (P = 0.25), I^2 =26%					

Analysis 5.1. Comparison 5 Traditional screening with motivation versus traditional screening alone, Outcome 1 Dental attendance.

Review: School dental screening programmes for oral health

Comparison: 5 Traditional screening with motivation versus traditional screening alone

Outcome: 1 Dental attendance



(1) Motivation included oral health education to the children regarding importance of teeth, maintenance of oral hygiene, prevention of oral diseases, school authorities motivating parents 368and free treatment.

ADDITIONAL TABLES

Table 1. Data adjusted to minimise clustering effect

Data values for total number and events to be divided by effect estimate across all studies **Effect estimate:** 1 + (M − 1)ICC M = average cluster size ICC = 0.03 (borrowed from Milsom 2006)	**Total number of participants (original)**	**Total number of participants (adjusted)**	**Events original children attending dental office)**	**Events adjusted (children attending dental office)**
Cunningham 2009 1 + (15 − 1).03 = 1.42 Control arm Traditional arm Criteria-based arm	819 1175 958	577 827 675	129 165 151	91 116 107
Milsom 2006 1 + (42 − 1).03 = 2.23 Control arm Traditional arm	4226 4418 4087	1895 1981 1833	1624 1838 1695	728 824 760

Table 1. Data adjusted to minimise clustering effect (*Continued*)

Criteria-based arm				
Praveen 2014	300	248	80	67
1 + (8 − 1).03 = 1.21	300	248	54	45
Control arm				
Intervention arm				

APPENDICES

Appendix 1. Cochrane Oral Health's Trials Register search strategy

#1 MESH DESCRIPTOR Mass Screening AND INREGISTER
#2 MESH DESCRIPTOR Stomatognathic diseases EXPLODE ALL AND INREGISTER
#3 #1 and #2
#4 ((dental or oral or mouth* or dentist) near5 (screen* or exam* or assess* or certify* or check* or inspect*)) AND INREGISTER
#5 ((caries or carious or (decay near (tooth or teeth)) or (trauma near (tooth or teeth)) or malocclusion or "gum health" or gingivitis or "oral hygiene" near5 (screen* or exam* or assess or certify* or check* or inspect*) AND INREGISTER
#6 MESH DESCRIPTOR Dental health surveys AND INREGISTER
#7 #3 or #4 or #5 or #6
#8 MESH DESCRIPTOR Schools EXPLODE ALL AND INREGISTER
#9 school* AND INREGISTER
#10 #8 or #9
#11 #7 and #10

Appendix 2. Cochrane Central Register of Controlled Clinical Trials (CENTRAL) search strategy

#1 MESH DESCRIPTOR Mass Screening AND TARGET:CENTRAL
#2 MESH DESCRIPTOR Stomatognathic diseases EXPLODE ALL AND TARGET:CENTRAL
#3 #1 and #2
#4 ((dental or oral or mouth* or dentist) near5 (screen* or exam* or assess* or certify* or check* or inspect*)) AND TARGET:CENTRAL
#5 ((caries or carious or (decay near (tooth or teeth)) or (trauma near (tooth or teeth)) or malocclusion or "gum health" or gingivitis or "oral hygiene" near5 (screen* or exam* or assess or certify* or check* or inspect*) AND TARGET:CENTRAL
#6 MESH DESCRIPTOR Dental health surveys AND TARGET:CENTRAL
#7 #3 or #4 or #5 or #6
#8 MESH DESCRIPTOR Schools EXPLODE ALL AND TARGET:CENTRAL
#9 school AND TARGET:CENTRAL
#10 #8 or #9
#11 #7 and #10

Appendix 3. MEDLINE Ovid search strategy

1. Mass screening/
2. exp Stomatognathic diseases/
3. 1 and 2
4. ((dental or oral or mouth or dentist$) adj5 (screen$ or exam$ or assess$ or certif$ or check$ or inspect$)).ti,ab.
5. ((caries or carious or (decay adj (tooth or teeth)) or (trauma$ adj (tooth or teeth)) or malocclusion or "gum health" or gingivitis or "oral hygiene") adj5 (screen$ or exam$ or assess$ or certif$ or check$ or inspect$)).ti,ab.
6. Dental health surveys/
7. 3 or 4 or 5 or 6
8. exp School/
9. school$.ti,ab.
10. 8 or 9
11. 7 and 10

The above search will be linked to the Cochrane Highly Sensitive Search Strategy (CHSSS) for identifying randomised trials in MEDLINE: sensitivity-maximising version (2008 revision) as referenced in Chapter 6.4.11.1 and detailed in box 6.4.c of the*Cochrane Handbook for Systematic Reviews of Interventions* Version 5.1.0 [updated March 2011] (Lefebvre 2011).

1. randomized controlled trial.pt.
2. controlled clinical trial.pt.
3. randomized.ab.
4. placebo.ab.
5. drug therapy.fs.
6. randomly.ab.
7. trial.ab.
8. groups.ab.
9. or/1-8
10. exp animals/ not humans.sh.
11. 9 not 10

Appendix 4. Embase Ovid search strategy

1. Mass screening/
2. exp Mouth disease/
3. 1 and 2
4. ((dental or oral or mouth or dentist$) adj5 (screen$ or exam$ or assess$ or certif$ or check$ or inspect$)).ti,ab.
5. ((caries or carious or (decay adj (tooth or teeth)) or (trauma$ adj (tooth or teeth)) or malocclusion or "gum health" or gingivitis or "oral hygiene") adj5 (screen$ or exam$ or assess$ or certif$ or check$ or inspect$)).ti,ab.
6. or/3-5
7. exp School/
8. school$.ti,ab.
9. 7 or 8
10. 6 and 9

The above subject search was linked to adapted version of the Cochrane Embase Project filter for identifying RCTs in Embase Ovid (see www.cochranelibrary.com/help/central-creation-details.html for information):

1. Randomized controlled tria/
2. Controlled clinical study/
3. Random$.ti,ab.
4. randomization/
5. intermethod comparison/
6. placebo.ti,ab.
7. (compare or compared or comparison).ti.
8. ((evaluated or evaluate or evaluating or assessed or assess) and (compare or compared or comparing or comparison)).ab.
9. (open adj label).ti,ab.

10. ((double or single or doubly or singly) adj (blind or blinded or blindly)).ti,ab.
11. double blind procedure/
12. parallel group$1.ti,ab.
13. (crossover or cross over).ti,ab.
14. ((assign$ or match or matched or allocation) adj5 (alternate or group$1 or intervention$1 or patient$1 or subject$1 or participant$1)).ti,ab.
15. (assigned or allocated).ti,ab.
16. (controlled adj7 (study or design or trial)).ti,ab.
17. (volunteer or volunteers).ti,ab.
18. trial.ti.
19. or/1-18
20. (exp animal/ or animal.hw. or nonhuman/) not (exp human/ or human cell/ or (human or humans).ti.)
21. 19 not 20

Appendix 5. US National Institutes of Health Ongoing Trials Register (ClinicalTrials.gov) search strategy

school AND dental AND screen

Appendix 6. World Health Organization International Clinical Trials Registry Platform search strategy

school* AND dental AND screen*

CONTRIBUTIONS OF AUTHORS

Ankita Arora: drafting the protocol, screening trials, data extraction, assessment of risk of bias and entering data into RevMan 5, selection of trials, drafting the final review and updating the review.

Shivi Khattri: undertaking searches, selecting trials, data extraction and entering data into RevMan 5, drafting the final review and updating the review.

Noorliza Mastura Ismail: screening articles, selecting trials, drafting the final review and updating the review.

Sumanth Kumbargere Nagraj: selecting the trials, data analysis, assessment of risk of bias, drafting the final review and updating the review.

Eachempati Prashanti: drafting the protocol, drafting the final review, updating the review and acting as arbiter.

DECLARATIONS OF INTEREST

Ankita Arora: no interests to declare

Shivi Khattri: no interests to declare

Noorliza Mastura Ismail: no interests to declare

Sumanth Kumbargere Nagraj: no interests to declare

Eachempati Prashanti: no interests to declare

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- Cochrane Oral Health Global Alliance, Other.

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DIFFERENCES BETWEEN PROTOCOL AND REVIEW

We would have assessed reporting bias as planned if there were more than 10 studies included in a meta-analysis.

In the case of dropouts, we intended to use the data as reported by the paper and deal with it in the 'Risk of bias' assessment. However, in the outcome 'Dental attendance', dropout was considered as a part of the outcome (not attending the dentist) and hence we redefined the term 'dropout' in this review.

We planned subgroup analysis on the basis of age group, targeted or universal screening, post-screening treatment set-up and treatment charges. However, we performed subgroup analyses on the basis of cluster versus parallel group study design because of substantial heterogeneity.

INDEX TERMS

Medical Subject Headings (MeSH)

*Oral Health; *School Dentistry [statistics & numerical data]; Dental Care for Children [statistics & numerical data]; Randomized Controlled Trials as Topic; Tooth Diseases [*diagnosis]

MeSH check words

Adolescent; Child; Child, Preschool; Humans