Oral health educational interventions for nursing home staff and residents (Protocol)

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Oral health educational interventions for nursing home staff and residents

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ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

To assess the effects of oral health educational interventions for nursing home staff and residents to maintain or improve the oral health of nursing home residents.

To describe the components of the complex interventions used in the included studies.
BACKGROUND

Description of the condition

The demographic shift in developed countries has important implications for healthcare services as there will be more (frail) elderly people with morbidity and care dependency (Branca 2009), and an increasing number of nursing home residents. Whereas until recently most nursing home residents were provided with dentures, nowadays increasingly more residents retain a considerable number of natural teeth (Hopcraft 2012; Samson 2008). Declining rates of edentulism have important implications for oral health and management of these residents because a greater proportion will have more teeth at risk of dental caries and periodontal disease. Additionally, high-quality and complex dental prostheses have to be maintained (i.e. implants, bridges, and removable dental prostheses). Nursing home residents are often unable to carry out adequate oral care such as optimal removal of dental plaque. Therefore, continuous preventive and curative oral health care provided by trained staff seems increasingly important. In nursing homes, nursing staff and other carers play a crucial role in the provision of daily oral health care. This also applies to the organisation of routine prophylaxis and treatment provided by dental professionals (Nitschke 2010).

In the past, it has been reported that nursing staff frequently would not acknowledge the importance of oral health or would lack knowledge of how to achieve it (Glassman 1994). The attitude has changed, but knowledge about oral hygiene is still inadequate among nursing staff (Jablonski 2009; Wardh 2012). There is a large discrepancy between the proposed need for assistance with daily oral hygiene among nursing home residents and the actual assistance provided by nursing staff (Forsell 2009). Elderly who need assistance with oral care most are most likely to resist helping behaviours (Jablonski 2011). This so-called ‘care-resistant behaviour’ is a common phenomenon encountered by nursing staff during the provision of oral care (Frenkel 1999; Jablonski 2011). Measures of poor oral health (e.g. gingivitis, caries, and chewing difficulties), along with discomfort and pain, have been reported for nursing home residents in different countries (De Visschere 2006; Gluhak 2010; Hopcraft 2012; Simunkovic 2005; Wyatt 2002). Dental plaque generally leads to an increased risk for dental caries, gingivitis, and periodontitis as well as other infections in the oral cavity. Also, the accumulation of microorganisms on teeth and denture surfaces, caused by lack of oral hygiene, may influence residents’ general health by causing pneumonia, arteriosclerosis and infection-related disorders (Azarpazhooh 2006; Desvarieux 2003; Scannapieco 2003; Shay 2002). Evidence from randomised controlled trials (RCTs) suggests positive preventive effects of oral care on respiratory tract infection and pneumonia in nursing home residents (Sjögren 2008). In addition, a Cochrane review protocol on this topic is under preparation (Shi 2013). Furthermore, involuntary weight loss has been associated with poor oral health (Mojon 1999; Sheiham 2001). Institutionalised elderly people who suffer from mouth discomfort, problems with chewing or swallowing, compromised dentition, or poorly fitted dentures have a higher incidence of nutritional deficiency (Saunders 2007).

In addition, poor oral hygiene and poor dental status among elderly people is associated with reduced quality of life (Walker 2007). Elderly people in adult day health centres have shown considerably higher levels of oral health problems compared to community-dwelling elderly people (Walker 2007). The reasons are difficulties in maintaining a sufficient level of oral hygiene and difficulties in accessing professional dental care (Forsell 2009). Two-thirds of elderly people living in residential facilities use dental services only in case of dental problems and on demand (De Baat 1993; Isaksson 2007). In a British study, 70% of residents had not seen a dentist for more than 5 years (Frenkel 2000). Consequently, interventions to improve oral health seem warranted as they possibly yield a number of positive effects including improved nutritional status and health-related quality of life (Naito 2010).

Description of the intervention

Oral health-related educational interventions are programmes that facilitate knowledge and skills acquisition in nursing home staff and residents to maintain or improve oral health, using a variety of formats. They may target nursing home residents or staff and they may be provided to individuals or groups. A number of RCTs investigating educational interventions aiming to improve oral health in nursing home residents have been published (Budtz-Jørgensen 2000; Frenkel 2001; Frenkel 2002; MacEntee 2007; Quagliarello 2009).

These interventions are usually designed as complex interventions, consisting of different components. Three components commonly included are:

1. educational sessions for staff or residents or both, aiming at changing knowledge of oral health and oral health care;
2. staff training on how to examine and clean the mouth/dentures of residents; and
3. oral hygiene skills training for residents.

How the intervention might work

The goal of the educational interventions is to improve oral and dental health of residents by increasing knowledge and skill levels of nursing home staff and nursing home residents. Interventions targeting nursing home staff aim to strengthen the staff expertise in oral care for residents. Increased knowledge about oral health, association between oral health and general health, and oral health care may lead to changes in attitudes, oral health-related behaviour and improved health outcomes for residents.
Interventions targeting nursing home residents, especially oral hygiene skills training, may foster the ability to self-perform dental care and thus decrease the need for assistance. There may also be direct effects of education on residents’ attitudes as well as quality of life.

**Why it is important to do this review**

Despite a growing number of RCTs, so far there has been no systematic review of interventions for improvement of oral health in nursing home residents. Considering the different complex interventions addressing various outcome measures, it seems highly warranted to describe the components of interventions and to identify effective intervention strategies. A systematic review will impact the implementation of different approaches and trigger the development of new interventions on the basis of current best evidence. A systematic review on this topic is also urgently needed since interventions of questionable effectiveness and unclear consequences might be in use.

**OBJECTIVES**

To assess the effects of oral health educational interventions for nursing home staff and residents to maintain or improve the oral health of nursing home residents.

To describe the components of the complex interventions used in the included studies.

**METHODS**

**Criteria for considering studies for this review**

**Types of studies**

We will include all individual randomised controlled trials (RCTs) or cluster-RCTs including groups of nursing home staff or residents or both, allocated either:

- to a programme aiming to maintain/improve oral health of nursing home residents by one or more oral health educational/oral hygiene promotion interventions (the intervention group), or
- to (optimised) regular dental care or any other oral health care intervention (the control group).

We will not apply any language restrictions. We will exclude trials with a follow-up period of less than 2 weeks.

**Types of participants**

Participants will be:

- male or female residents living in facilities which provide supervision or (nursing) care for the elderly (e.g. nursing homes or long-term care facilities). We will include studies if the majority of residents were over the age of 64 years or the mean age was at least 65 years;
- nursing staff working in those facilities; or
- a combination of the above.

We aim to include all participants included in the primary studies (i.e. irrespective of residents’ oral health status or staff profession).

**Types of interventions**

Any intervention or group of interventions where subjects or clusters are allocated to receive an oral health education programme versus (optimised) usual oral health care, or any other oral health-care intervention.

Oral health education programmes include either direct-to-staff programmes, direct-to-resident-programmes (e.g. oral hygiene promotion or skills training) or any combination of both. We expect a range of different approaches varying for example in terms of frequency, length and content. Contents are likely to include all or some of the following: oral health, oral diseases and impact on general health, diet, oral hygiene measures, best oral care practices for elderly people with natural dentition and dentures, oral hygiene promotion, and skills training.

Following the ‘framework for design and evaluation of complex interventions’, it will not be possible to extract the effective or ineffective components of educational programmes (Campbell 2000; Craig 2008), but components of included programmes will be collected and described in detail as suggested by Lenz 2007.

We will exclude interventions that do not include educational or oral hygiene promotion components from the intervention group, but we will consider including them as controls. Also, sole organisational interventions aiming to change organisational policies, for example through introduction of practice guidelines, oral health co-ordinators, regular visits by a dentist or dental hygienist for professional oral care and examination, or regular visits of residents in dental surgeries, will only be considered as controls. The same applies to exclusive oral hygiene interventions (e.g. the provision of chemical topical interventions and mechanical auxiliaries).

**Types of outcome measures**

**Primary outcomes**

1. Oral health-related quality of life: measured by instruments such as Oral Health Impact Profile (OHIP) (Slade 1994) or (OHIP-14) (Slade 1997), Geriatric/General Oral Health Assessment Index (GOHAI) (Atchison 1990), Dental Impact Profile (DIP) (Strauss 1993).
2. Oral health: measured by instruments such as Brief Oral Health Status Examination (BOHSE) or Oral Health Assessment (OHAT) (Chalmers 2005; Kayser-Jones 1995).

3. Dental health measures such as:
   - caries, incidence of new caries;
   - dental or denture plaque or both: measured by plaque scores and denture cleanliness scores (scales) such as Plaque Index (Silness 1964), Mucosal-Plaque Index (Henriksen 1999), or Denture Plaque Index (Wefers 1999); or
   - gingivitis: measured by instruments such as Gingival Bleeding Index (Ainamo 1975), Gingival-Index (Löe 1967), or Community Periodontal Index (Benigeri 2000).

Secondary outcomes
- Nutritional status (e.g. body weight, Body Mass Index - BMI).
- Incidence of respiratory diseases and pneumonia.
- Adverse effects of the interventions.

Intermediate Outcomes
- Oral health-related knowledge of staff or residents or both: measured by any instruments used in the included studies (e.g. questionnaire or interview).
- Oral health-related attitude and behaviour of staff or residents or both: measured by any instruments used in the included studies (e.g. questionnaire or interview).

As the oral health-related outcomes listed above might only be achieved along with considerable impediment of residents’ autonomy (e.g. by applying measures against residents’ wills), we will put special consideration on potentially interdependent outcomes (e.g. residents’ quality of life and oral health status). We expect these data to be only rarely available which then will be stated.

Search methods for identification of studies
For identification of studies to be included or considered for this review, detailed search strategies will be developed for each database searched. These will be based on the search strategy developed for MEDLINE (Appendix 1) but revised appropriately for each database to take account of differences in controlled vocabulary and syntax rules.

The search strategy will combine the subject search with the Cochrane Highly Sensitive Search Strategy for identifying reports of randomised controlled trials (2008 revision) (as published in box 6.4.c in the Cochrane Handbook for Systematic Reviews of Interventions version 5.1.0, updated March 2011) (Higgins 2011).

Electronic searches
We will search the following databases.
- The Cochrane Oral Health Group Trials Register (to present).
- The Cochrane Central Register of Controlled Trials (CENTRAL) (The Cochrane Library, current issue).
- MEDLINE via OVID (1946 to present) (see Appendix 1).
- EMBASE via OVID (1980 to present).
- CINAHL via EBSCO (1980 to present).

We will attempt to identify all relevant studies irrespective of language. Non-English language papers will be translated.

Searching other resources
We will search the National Institutes of Health Trials Register for ongoing trials (ClinicalTrials.gov). We will check reference lists of published reviews and retrieved articles for additional trials, and forward citation tracking will be performed by using Google Scholar, Web of Science and Scopus.

We will contact experts in the field to identify unpublished or ongoing studies.

Data collection and analysis

Selection of studies
Two review authors will independently screen titles and abstracts of identified studies, rejecting any which are obviously irrelevant. Full text copies of all eligible and potentially eligible studies will be obtained and evaluated in detail by two review authors to identify those studies which actually meet all inclusion criteria. From this group, those studies which do not meet the inclusion criteria will be recorded in the excluded studies section of the review and the reason for exclusion will be noted in the ‘Characteristics of excluded studies’ table. We will resolve any disagreement by discussion or, if necessary, by consulting a third review author.

Data extraction and management
Two review authors will independently extract data using a standardised data collection sheet and data will be entered in the current version of RevMan (RevMan 2012). The review authors will not be blinded to the authors of the included studies. Disagreement will be resolved by discussion between the two review authors or, if necessary, a third review author will be consulted in order to reach consensus. Data will be sought per participant or randomised cluster (nursing home) on all of the outcome measures of interest from all assessment times (including baseline). We will extract data for: characteristics of participants, baseline data, interventions, duration of intervention, length of follow-up, outcome
For cluster-RCTs, we will extract estimates of the intraclass correlation coefficient if possible. We will retrieve data on process evaluation of the complex interventions on the basis of a criteria list for reporting the development and evaluation of complex interventions in health care (CR-EDEC) (Möhler 2012). The process evaluation comprises developmental details like the description and intensity of the components, feasibility and piloting, as well as evaluation of the complex intervention. In addition, data on the fidelity of the intervention implementation will be extracted. If, as is frequently the case, information about the intervention and the development process have not been reported sufficiently in the publication, we will try to acquire detailed information about the interventions used by contacting authors of the primary study (e.g. by asking about related publications). Also, following earlier suggestions (Lenz 2007), we will perform extra searches for publications related to included studies.

Required data for each trial and each outcome for continuous data are the mean change from baseline, the standard error of the mean change, and the number of residents for each cluster at each assessment. Where changes from baseline were not reported, we will use the mean standard deviation and the number of residents in each cluster at each time point, if available.

### Assessment of risk of bias in included studies

Assessment of risk of bias will follow the Cochrane Handbook for Systematic Reviews of Interventions (Higgins 2011). Two review authors will independently assess studies using a two-part tool, addressing six domains (sequence generation, allocation concealment, blinding, incomplete outcome data, selective outcome reporting, and other issues). The first part of the tool describes what has been reported in the study. In the second part, a judgement concerning the related risk of bias is assigned for each domain, either 'Low risk', 'High risk' or where insufficient information is available on which to make a judgement, the risk of bias will be assessed as 'Unclear risk'. The domains of sequence generation, allocation concealment (selection bias) and selective outcome reporting (selection bias) and selective outcome reporting (reporting bias) will be addressed in the tool by a single entry for each study. Blinding of participants, staff and outcome assessors (performance bias and detection bias) will be considered separately for objective outcomes and subjective outcomes. Incomplete outcome data (attrition bias) will be considered separately for different lengths of follow-up (shorter and longer follow-up). We will summarise the risk of bias as follows.

<table>
<thead>
<tr>
<th>Risk of bias</th>
<th>Interpretation</th>
<th>In outcome</th>
<th>In included studies</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low risk of bias</td>
<td>Plausible bias unlikely to seriously</td>
<td>Low risk of bias for all key domains</td>
<td>Most information is from studies at low risk of bias</td>
</tr>
<tr>
<td></td>
<td>alter the results</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclear risk of bias</td>
<td>Plausible bias that raises some doubt</td>
<td>Unclear risk of bias for one or more</td>
<td>Most information is from studies at low or unclear risk of bias</td>
</tr>
<tr>
<td></td>
<td>about the results</td>
<td>key domains</td>
<td></td>
</tr>
<tr>
<td>High risk of bias</td>
<td>Plausible bias that seriously weakens</td>
<td>High risk of bias for one or more</td>
<td>The proportion of information from studies at high risk of bias is sufficient to affect the interpretation of results</td>
</tr>
<tr>
<td></td>
<td>confidence in the results</td>
<td>key domains</td>
<td></td>
</tr>
</tbody>
</table>

A risk of bias table will be completed for each included study and the results will also be presented graphically.

### Measures of treatment effect

For dichotomous data (e.g. incidence of respiratory disease) the effect measure will be risk ratio (RR). The absolute numbers in each group and the numbers experiencing the outcome of interest will be sought and recorded. For continuous data (e.g. plaque indices) the effect measure will be the mean difference if the same instrument is used, or the standardised mean difference (SMD) if different instruments are used for the same outcome measure. The mean change from baseline, the standard deviation of the mean change, and the number of patients for each treatment group at each assessment will be extracted. Where changes from baseline are not reported, we will extract the mean standard deviation and the number of participants for each group at each time point, if available.

### Unit of analysis issues

We will consider for each study whether groups of individuals were randomised in clusters or individually, whether individuals underwent more than one intervention, or whether there were multiple observation times for the same outcome. As results from
Dealing with missing data

We will contact trial authors to retrieve missing data where necessary. We will describe the amounts and types of missing data related to participant withdrawal in the ‘Characteristics of included studies’ table. We will discuss the impact of these missing data. Their potential impact on the results will depend on the extent of missing data, the pooled estimate of the treatment effect, and the variability of the outcomes. Variation between studies in the amount of missing data may also be considered as a potential source of heterogeneity. Where possible, intention-to-treat (ITT) analyses will be performed. We will seek data on whether or not clusters were subsequently deemed ineligible, or otherwise excluded from treatment or follow-up.

We will only use imputation to calculate missing standard deviations as described in the Cochrane Handbook for Systematic Reviews of Interventions (section 16.1.3.) (Higgins 2011). Recognising that statistical analyses cannot reliably compensate for missing data (Unnebrink 2001), we will assess the impact of any assumption by trying more than one method for a sensitivity analysis. For example, for dichotomous data, it will first be assumed that all missing participants in the first group incurred the event and those in the second group did not, after which the opposite will be assumed. If ITT data are not available in the publications, ‘on-treatment’ data or the data of those who completed the trial will be sought and indicated as such. Data from non-randomised follow-on periods will not be used.

Assessment of heterogeneity

We will only undertake meta-analyses when studies are sufficiently homogeneous in terms of participants, interventions and outcomes. We will consider both clinical heterogeneity and statistical heterogeneity. We will investigate statistical heterogeneity between trials included in each analysis using the I² statistic. A rough guide to interpreting the I² statistic is given in section 9.5.2 of the Cochrane Handbook for Systematic Reviews of Interventions (Higgins 2011): 0% to 40% might not be important; 30% to 60% may represent moderate heterogeneity; 50% to 90% may represent substantial heterogeneity; and 75% to 100% may represent considerable heterogeneity.

Assessment of reporting biases

In order to minimise the risk of publication bias, we will undertake a comprehensive search in multiple databases, including searching for unpublished studies in trials registries. Providing there are a minimum of 10 included studies, we will construct a funnel plot to assess the likelihood of publication bias.

Data synthesis

It has been discussed that there are specific difficulties in defining, developing, documenting, and reproducing complex interventions (Craig 2008; Lenz 2007). These difficulties may lead to important methodological challenges when aiming to synthesise results from the studies (e.g. in meta-analyses requiring new approaches for synthesis) (Pawson 2005; Shepperd 2009). They may even make meta-analyses inappropriate (Lenz 2007). In this context, it has also been claimed that education programmes should not be allocated into categories referring to interdependent components (Lenz 2007), and decisions concerning what should and should not be combined are inevitably subjective, and are not amenable to statistical solutions but require discussion and clinical judgement (Higgins 2011). Therefore, meta-analysis will only be conducted for the same intervention (main trial and replication trials). Two review authors will carry out categorisation of replication trials. We will contact the authors of the included studies to ask whether they feel that their intervention has been categorised appropriately.

We will only conduct meta-analysis for studies with comparable analyses reporting similar outcome measures. We will combine risk ratios for dichotomous data (e.g. number of people in each arm with gingivitis), and mean differences for continuous data (e.g. mean plaque scores in each trial arm), using random-effects models, assuming that the identified studies allow this procedure. Where there are few studies, or the studies are small, it may be impossible to estimate between-study variance with any precision. In that case a random-effects analysis would provide poor estimates to assess the likelihood of publication bias.

In order to minimise the risk of publication bias, we will undertake a comprehensive search in multiple databases, including searching for unpublished studies in trials registries. Providing there are a minimum of 10 included studies, we will construct a funnel plot to assess the likelihood of publication bias.

Subgroup analysis and investigation of heterogeneity

We will conduct subgroup analyses for relevant and clinically meaningful subgroups if sufficient data are available.
Possible subgroup analyses will be carried out for:
- study design (cluster randomised versus individual randomised participants);
- target group of interventions (educational interventions delivered to nursing home staff, to residents, or to both); and
- type of control intervention (usual care versus active control).

**Sensitivity analysis**

Providing that there are sufficient included studies, we will undertake sensitivity analysis based on risk of bias (Higgins 2011).

**Presentation of main results**

A summary of findings table will be developed for the primary outcomes, using GRADEPro software, following the methods outlined in the Cochrane Handbook for Systematic Reviews of Interventions (section 11.5) (Higgins 2011). The quality of evidence will be assessed with reference to the overall risk of bias of the included studies, the directness of the evidence, the consistency of the results, the precision of the estimates, the risk of publication bias, the magnitude of the effect, and whether or not there is evidence of a dose response. The quality of evidence for each of the primary outcomes will be categorised as high, moderate, low, or very low.

**ACKNOWLEDGEMENTS**

We wish to acknowledge the contribution of consumer reviewer Johannes van Dijk (gerontopsychiatric nurse and trainer in dementia care mapping, Hamburg, Germany). We would also like to thank the following peer referees for their helpful comments on the protocol: Ina Nitschke, Ivor G Chestnutt, Aubrey Sheiham, Toru Naito, and Rita A Jablonski.

**REFERENCES**

Additional references

- Ainamo 1975

- Atchison 1990

- Azarpazhooh 2006

- Benigeri 2000

- Branca 2009

- Budtz-Jørgensen 2000

- Campbell 2000

- Chalmers 2005

- Craig 2008

- De Baat 1993

- De Visschere 2006

- Desvarieux 2003

- Forsell 2009
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Frenkel 1999

Frenkel 2000

Frenkel 2001

Frenkel 2002

Glassman 1994

Gluhak 2010

Henriksen 1999

Higgins 2011

Hopcraft 2012

Isaksson 2007

Jablonski 2009

Jablonski 2011

Kayser-Jones 1995

Lenz 2007

Löe 1967

MacEntee 2007

Mojon 1999

Möller 2012

Naito 2010

Nitschke 2010

Pawson 2005

Quagliarello 2009
Samson 2008

Saunders 2007

Scannapieco 2003

Shay 2002

Sheiham 2001

Shepperd 2009

Shi 2013

Silness 1964

Simunković 2005

Sjögren 2008

Slade 1994

Slade 1997

Strauss 1993

Unnebrink 2001

Walker 2007
Walker RJ, Kiyak HA. The impact of providing dental services to frail older adults: perceptions of elders in adult day health centers. *Special Care in Dentistry* 2007;27(4):139–43.

Wårdh 2012

Wefers 1999

Wyatt 2002

* Indicates the major publication for the study
Appendix 1. MEDLINE (OVID) Search Strategy

1. exp Nursing home/
2. Homes for the aged/
3. (home$ adj5 (nurs$ or elder$ or old$ or care or “assisted living” or convalescen$ or rest$ or retir$ or resident$ or “long stay” or longstay or “long term”)).mp.
4. (facilit$ adj5 (nurs$ or elder$ or old$ or care or “assisted living” or convalescen$ or rest$ or retir$ or resident$ or “long stay” or longstay or “long term”)).mp.
5. (institut$ adj5 (nurs$ or elder$ or old$ or care or “assisted living” or convalescen$ or rest$ or retir$ or resident$ or “long stay” or longstay or “long term”)).mp.
6. (residenc$ adj5 (nurs$ or elder$ or old$ or care or “assisted living” or convalescen$ or rest$ or retir$ or “long stay” or longstay or “long term”)).mp.
7. or/1-6
8. Oral health/
9. exp Stomatognathic diseases/
10. Halitosis/
11. ((dental or tooth or teeth or enamel or root$) and (decay$ or caries or curious or “white spot$” or plaque or reminerali$ or deminerali$ or erosion$ or abrasion$ or wear$)).mp.
12. (denture$ and (clean$ or clens$)).mp.
13. (periodont$ or gingiv$ or gingiva$).mp.
14. (stomatitis or “mouth ulcer$” or “oral ulcer$” or (oral adj5 candidi$) or (mouth$ adj5 candidi$) or “aphthous ulcer$” or (aphthae adj5 ulcer$) or (mucositis adj5 mouth$) or (mucositis adj5 oral) or xerostomi$ or “dry mouth$”).mp.
15. ((oral adj5 health$) or (mouth adj5 health$) or (dental adj5 health$)).mp.
16. (halitosis or “mouth odour$” or “mouth odor$” or “mouth malodour$” or “mouth malodor$” or “oral malodour$” or “oral malodor$” or (breath adj5 malodour$) or (breath adj5 malodor$) or (breath adj5 odor$) or (breath adj5 odour$)).mp.
17. exp Mouth neoplasms/
18. (“oral cancer$” or (gingiv$or mouth or lip or lips or tongue$ or “salivary gland$” or palat$ or parotid$ or sublingual or sub- mandibular)) and (cancer$ or carcinoma$ or neoplasm$ or tumour$ or tumor$ or lesion$ or malignan$)).mp.
19. leukoplak$.mp.
21. exp Oral hygiene/
22. exp Mouthwashes/
23. exp Dentifrices/
24. (“oral hygiene” or (mouth$ adj3 care) or (dental adj3 care) or (care adj3 teeth) or (mouth$ adj3 hygiene) or (plaque adj3 control$) or (plaque adj3 remov$)).mp.
25. (toothbrush$ or tooth-brush$ or toothpaste$ or dentifrice$ or mouthwash$ or mouth-wash$ or mouthrinse$ or mouth-rinse$ or fluoride$).mp.
26. (floss$ or “interdental brush$” or “inter-dental brush$” or (tooth adj5 clean$) or (teeth adj5 clean$) or (denture$ adj5 hygien$) or (denture$ adj5 clean$) or (tongue$ adj5 scrap$) or (tongue$ adj5 brush$) or (chewing adj5 stick$) or (chewing adj5 gum$)).mp.
27. ((oral adj3 care) or (oral adj3 “self care”)).mp.
28. or/8-27
29. Health education, dental/
30. exp Health promotion/
31. (instruct$ or advice$ or advise or educat$ or promot$ or teach$ or train$).mp.
32. ((demonstrat$ adj5 toothbrush$) or (demonstrat$ adj5 “tooth brush$”) or (demonstrat$ adj5 tooth-brush$) or (demonstrat$ adj5 floss$) or (demonstrat$ adj5 “interdental brush$”) or (demonstrat$ adj5 “inter-dental brush$”) or (demonstrat$ adj5 “interdental clean$”) or (demonstrat$ adj5 “inter-dental clean$”) or (demonstrat$ adj5 wood-stick$) or (demonstrat$ adj5 wood-stick$) or (demonstrat$ adj5 “wood stick$”)).mp.
33. (demonstrat$ adj5 (denture$ adj3 (clean$ or clens$))).mp.
34. (supervis$ adj5 (denture$ adj3 (clean$ or clens$))).mp.

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The search will be linked with 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 (Higgins 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

CONTRIBUTIONS OF AUTHORS

For the protocol:

- Martina Albrecht (MA) initially planned the review and has written the protocol with important contribution from Sascha Köpke (SK).
- Ramona Kupfer (RK), Daniel Reissmann (DR), Burkhard Haastert (BH), Ingrid Mühlhauser (IM) and SK commented on and approved the protocol.

For the review:

- MA and RK will contribute to all aspects.
- DR will provide specialist dental expertise.
- BH will provide statistical expertise.
- SK will provide methodological and specialist nursing expertise.
- IM will provide methodological expertise.
DECLARATIONS OF INTEREST

None known.

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Internal sources

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