Author's response to reviews

Title: A cluster randomised control trial investigating the effectiveness of personalised letters sent subsequent to school dental screening in increasing dental registration rates in unregistered 12-13 year old children in South-East Scotland

Authors:

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Author's response to reviews: see over
**Reviewer’s report**

**Title:** A cluster randomised control trial investigating the effectiveness of personalised letters sent subsequent to school dental screening in increasing dental registration rates in unregistered 12-13 year old children in South-East Scotland

**Version:** 1  
**Date:** 26 August 2008  
**Reviewer:** Robert A Baker

**Reviewer’s report:**

**Major Compulsory Revisions**  
None

**Minor Essential Revisions**

1. Is the question posed by the authors well defined?  
Yes,

2. Are the methods appropriate and well described?  
Yes,

3. Are the data sound?  
There is doubt about the numbers of children actually inspected as opposed to the number potentially available to inspect. No figures are given for the number of children excluded due to refusal, parental refusal or absenteeism. No comment is made as to whether or not letters were sent to children who were not seen e.g. due to absenteeism but known to be unregistered.

The data which the reviewer refers to regarding children who were not examined is not presented in the study as the Education (Scotland) act (1980) requires parents to submit their child for dental inspection at least 3 times in their school career. Refusal rates from either parents or children of this age in Scotland are extremely low; perhaps this is a reflection mostly of using negative consent. Additionally, in randomising to study groups we would anticipate that rates of refusal would be similarly low in both groups.

The abstract suggests that the study took place in 2002/3 [p2] but the Methods part of the study suggests 2004 [p7]. We have changed the date on page 7 as the reviewer indicates.

I have not checked the statistics but they appear correct.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?  
Yes

5. Are the discussion and conclusions well balanced and adequately supported by the data?  
Yes

The authors should mention the Group C letter and its effect upon parental attitude.
The current study was not powered for such sub-analysis and a larger sample would be required however, the randomisation process should ensure that the letter categories (letters A, B and C relating to treatment urgency) were evenly distributed between both groups. The point is however an interesting proposal for any future work.

6. Are limitations of the work clearly stated?
Mainly, however the Authors should mention:
• The three month period of the study – was this too short? Could dental treatment have been completed; and the dental estimate sent to and processed by the Board? This was also shorter than previous studies.

We wanted to use a similar time interval as previous studies had used to allow comparability (previous studies ranged between 2-4 months). We were concerned when we planned the study that this period might be inadequate when utilising the unique electronic registration information available to us and sought advice from ISD, Scotland (managers of the MiDAS database) who confirmed that 3 months would be an adequate interval to capture the registration information in this age group. In actual fact, the period was closer to 4 months than 3 by the time the data was retrieved following inspections. A longer period between the intervention and checking registration information runs the risk of bias from other reasons for registering not related to the dental inspection.

• The time of year as holiday periods could influence the results in such a short period.

As the study was conducted between February and June we are relatively confident that we avoided the major summer holiday period.

• Local waiting times for registration / treatment.
• NHS treatment is free for children and that the registration period is 15 months

We can confirm that at this point in time there were no access problems for children in Lothian or Fife

• Fife and Lothian are separate Boards, having different rates of decay. NDIP reports indicate that 50+% of P1 children are decay free.
• Are these areas urban or rural or a mixture.

Lothian and Fife together have a mixture of rural and urban areas, Lothian is predominantly urban. We took these two points into account through stratifying the sample by NHS board, deprivation category (closely related to caries prevalence) and size of school (rural schools are almost always smaller than those in cities).

• Mention should be made of the differences in the NDIP letters i.e.
Letter A (High Risk) - severe decay and should seek immediate dental care; or
Letter B (Medium Risk) - some decay experience and should seek dental care in the near future; or
Letter C (Low Risk) - no obvious decay but should continue to see the family
dentist on a regular basis
• The proportions of children found in these groups A, B & C should be stated.
• The proportions of these groups who did register? It may have been that all the re-registrants were in group A which would have had value and that all the non-registrants were in group C.
• The C group letter - does it inspire registration or parents to continue present practice?
As we did not intend to analyse the data by letter category (see response to point 5 above) we did not feel that this information was necessary

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
The authors might wish to mention that Topping is also an author of the NDIP report for 2004.
We can confirm that Dr Topping is an author of the annual NDIP reports but were not sure why the reviewer would like to see this acknowledged or how to do this, the work is related to annual epidemiological surveys following BASCD criteria

8. Do the title and abstract accurately convey what has been found?
Yes, although the title is overly long. Perhaps “The effectiveness of personalized letters sent subsequent to school dental screening: a cluster-randomised control trial” would be simpler.
We have shortened and simplified the title as directed by reviewer 2

9. Is the writing acceptable?
Yes, however the punctuation needs to be improved. The authors should use hyphens for: “cluster-randomized” and “12- to 13-year-old children”
Several sentences omit full stops [p4x3, p5, p8, p12, p13x2, p14, p15]. Colons should be used for lists [p2, p10]. Lists should be punctuated with commas and full stops [p2, p6, p10].
P5 the authors mean “school rolls” not “school roles”.
P8 line 2 omit files, line 15 “as 2 years would have passed”.
P11 re-registration.
We have corrected the punctuation as the reviewer has suggested

Discretionary Revisions
It would have been a more interesting study if the study had investigated what percentage of children requiring treatment had registered for treatment as a result of the letters.
We agree this would be an interesting investigation but feel in would need to be addressed in a further study

It would be nice to have totals on the Tables.
To keep the tables simple we have left the totals off as the information is provided for readers to calculate these if required
Level of interest: An article of importance in its field
Quality of written English: Needs some language corrections before being published
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I work for Torbay NHS Care Trust which undertakes school dental inspections.
Reviewer's report

Title: A cluster randomised control trial investigating the effectiveness of personalised letters sent subsequent to school dental screening in increasing dental registration rates in unregistered 12-13 year old children in South-East Scotland

Version: 1 Date: 26 September 2008
Reviewer: Peter Robinson

Reviewer's report:

Cunningham et al

A cluster randomised control trial investigating the effectiveness of personalised letters sent subsequent to school dental screening increasing dental registration rates in unregistered 12-13 year old children in South East Scotland

This is an interesting study and definitely needs to be reported. However, it contains a number of areas that could be clarified before it can be published.

1. The title is too long. I suggest “A randomised control trial of the effectiveness of personalised letters sent after school dental screening in increasing registration of unregistered children”.

The title of the article has been changed as the reviewer indicates.

2. The background to the abstract is overwritten. The first two sentences could be deleted and the second part of the background could be reduced substantially.
We have determined that the reviewer is referring to the Introduction in the main section of the paper as there is no background in the abstract in this draft of the paper. We have taken the reviewer’s advice. The first two sentences have been deleted and the end section from “Jones (2001) demonstrated..” has also been removed

3. More detail is needed about the control group in the abstract.
The description of the control group has been clarified in the list on page 2

4. The results of the abstract do not report the primary outcome of the study.
The whole of the results section in the abstract has been rewritten to focus on the primary outcome as the reviewer indicates

5. I think the aim could be expressed a little bit more clearly. Certainly the aim could be self-standing and not referring to other parts of the background. In addition I found it confusing to hear the Lothian and Fife system was “traditional” when the previous paragraph had said how there were aspects of it that were unusual.
We have removed the description, in the main text, of the Lothian and Fife system as traditional. We have then only used this term in the tables (in inverted commas) as a shorthand for the system in place prior to the introduction of NDIP
6. At the bottom of page 5 refers to three groups of 12-13 year old children. What are those three groups?
We have inserted a reference to figure 1 which details the contents of the three groups

7. My principle concern with the rest of this paper relates to the sub-analysis of children who had never been registered or were lapsed. Why was this done? There’s no hint that it would be done in the background or the aims. In fact this sub-analysis pretty much dominates the results (paragraphs 3 and 4) and by the bottom of page 10 I got the feeling that the authors were trawling the data for a positive finding because their primary outcome had shown no difference.
We have inserted a sentence at the end of the introduction explaining our reasons for carrying out a secondary analysis

Likewise in the discussion quite a lot is made of the difference in outcome between these two groups of children even though it is unrelated to the aim. There is another interpretation of this significant finding by the way. There are three working hypotheses and three groups (the total population, those who had never been registered, and those who were lapsed). Using an alpha of 0.05 this gives a 45% chance of type 1 error. It may be that this difference between groups is just by chance.
We have inserted the second paragraph in the results section thus focusing the main part of the results section on the primary outcome as the reviewer indicates. We then added an extra paragraph at the end of the results section regarding the secondary analysis.
In addition we have largely rewritten the first three paragraphs of the discussion to focus on the primary outcome and then explain why we felt the secondary analysis was important in the context of previous studies.

8. The authors also over-interpret a non-significant finding at the bottom of page 11 referring to the slightly lower re-registration rate amongst the traditional group.
This section has been removed in the re-write

9. Actually the authors have some really interesting data because the system for following the children was unique and as the authors point out could never be used again. The authors ought to play this up as any misclassifications in earlier studies by other authors would mask any differences between the groups.
Two new sentences have been added to the discussion to emphasise the uniqueness of this system as indicated.

10. The profile does not seem to indicate any loss to follow up. Is this correct? It would be interesting to know about loss to follow up and indeed whether any other aspects of the CONSORT statement were omitted.
A new paragraph has been added to the discussion (starting “One of the unique features of this study…..”) which explains how the MIDAS system can trace
children across Scotland and why we have no apparent loss to follow up and also, that under this system it is possible that children could, unbeknownst to us, be lost from the study through migration but additional references are given as to why these numbers are small and unlikely to affect the results of the study.

11. The conclusions should really relate to the primary outcome of the study rather than the strange sub-analysis. 
As stated in response to point 7 this advice has been acted upon by re-writing the discussion

12. I found table 1 slightly confusing. In the traditional group the description says the letter was tailored to the confirm registration status of the child prompting registration when necessary. What this really means is that they only sent a letter to the children who were not registered. 
This is incorrect, we sent letters to all children. The letter varied depending on the registration status of the child and suggested registration only in those seen to be unregistered.

13. In tables 2 and 3 I did not like the phrase “changes in registration levels”. What these tables describe is the proportion in each group who had registered at the end of the study. Table 2 could be simplified as if we know the number in each group and then the proportion who registered we don’t have to be given the absolute number who registered. The extra space gained could be used to describe the characteristics of the group (very briefly!) rather than just referring to the group number. 
We have changed both the title and the layout of these tables as indicated by the reviewer

14. Given what I have said about the sub-analyses, table 3 might be a distraction. 
Having clarified table 3 (see above) and added additional significance data, we feel this table is useful for the secondary analysis

In conclusion this is an interesting and potentially important paper but it needs to be clarified and to focus more on the primary outcome than an exploratory analysis.

Major compulsory revisions: 4, 7, 8, 9, 10, 11,
Minor essential revisions: 1, 2, 3, 5, 6, 12, 13,
Discretionary revisions: 14

Level of interest: An article of outstanding merit and interest in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests