Author's response to reviews

Title: The Hall Technique; a randomised control clinical trial of a novel method of managing carious primary molars in general dental practice: outcomes at two years.

Authors:

Nicola P T Innes (n.p.innes@dundee.ac.uk)
Dafydd J P Evans (d.j.p.evans@dundee.ac.uk)
David R Stirrups (d.r.stirrups@dundee.ac.uk)

Version: 4 Date: 18 October 2007

Author's response to reviews: see over
The following are the authors responses (in blue), to the comments made by the referees. In order to fully understand the referees comments, and the authors responses to them, it should be noted that this paper was originally submitted as two papers, which have subsequently been combined.

**Reviewer’s report**

**Title:** The Hall Technique; a randomised control clinical trial of a novel method of managing carious primary molars in general dental practice: outcomes at two years.

**Version:** 3  **Date:** 15 July 2007

**Reviewer:** Paulo Nadanovsky

**Reviewer’s report:**

**General comment**

1- This was a very interesting and highly relevant study. The tested intervention is simple and does appear to be comfortable for the patient and effective for the treatment of dental caries in primary teeth. The study adopted a pragmatic approach, which reflects more accurately the reality of clinicians and patients in everyday real life circumstances, than most traditional clinical trials do.

Response to point 1:
No response required

**Specific comments**

**Title**

2- Instead of “randomised control clinical trial”, may be it is better “randomized controlled clinical trial”. Not only in the title, but also throughout the text, this term should be changed as suggested.

Response to point 2:

a) **Accept:** This is a thorny issue. The word randomised is standard in the English speaking world as the un-Americanised version of randomized. If you search for randomised drug trial in Scopus (which covers PubMed), limit to Health Science and “article”, the following will be picked up:

- 6 results in 2007 with the UK spelling
- 15 with the US spelling and randomi*ed picks up all 21.

There is no easy answer to this problem but it is hoped that most researchers will use the * in their search. Because more trials tend to be picked up with the US spelling, we are happy to change to “randomized”.

b) **Accept:** Both ‘control’ and ‘controlled’ are acceptable, and widely used, versions of this term.

**Introduction**

3- The authors should avoid the discussion about “to fill or nor to fill” primary teeth. Their study did not deal with this question. They compared different
restorative techniques, but did not assess the effectiveness of not restoring the teeth. They focused specifically on the performance of preformed metal crowns, comparing them to conventional restorative techniques. The authors relied too heavily on the study by Pine et al. (2006), to conclude that it is in the best interest of the child to restore their decayed primary teeth than not to restore them. In the study by Pine et al. 2006 (reference 12 in their manuscript), according to their regression model, restorations increased the chances of sepsis (odds ratio 1.15). It is noteworthy that the number of filled teeth in their sample was very small. Relatively few dentists filled primary teeth; possibly, their sample included preferentially less complex cavities and/or fillings placed by especially skilled dentists. Also, Pine et al. (2006) did not include in their regression model, the total caries experience (dmft). It appeared that dmft was an important predictor of sepsis. Pine et al. (2006) did not comment on either of these points. The social class, income and education of the parents were not considered in their analysis (deprivation score of residential area is a contextual variable; contextual socioeconomic variables have consistently shown much lower associations with disease at the individual level than individual socioeconomic variables. Thus, the confounding effect of socioeconomic factors was poorly controlled in Pine et al). These factors are related to increased risks of most diseases and may increase the chances of sepsis, through poorer diets, lower tooth-brushing frequency with fluoridated toothpaste and other social related factors. For these and other reasons the group of children with untreated decay and the group with treated decay were not comparable, in Pine et al (2006). Also, the relatively few children with treated decay in Pine et al (2006) may not allow a wider generalization of the treatment effect. Therefore, Pine et al. (2006) study was not strong enough to allow the authors of the present manuscript to conclude that “The question should not be whether it is necessary to restore carious primary teeth, but instead how best to do so in the Primary Care environment so as to lead to a reduction in pain and dental sepsis.” Until now, there is not enough evidence supporting the view that the pain and psychological distress caused by restorative dental treatment are worthwhile costs to pay, on average, for children with one, a few or several decayed teeth, at every age. A better justification for the present study is that, despite the fact that it is still not known whether it is better for the children to restore (or not to restore) their decayed primary teeth (references 4, 6, 12), many dental practitioners do restore them. In this case, it is important to assess the effectiveness of different techniques available to dentists, when they decide to restore a primary tooth.

Response to point 3:
Accept: These are helpful and informative comments on this matter. The emphasis on this point has been changed to reflect the reviewer’s comments and the focus changed to reflect the paper addressing the question of which restorative approach is the most clinically effective.

Materials and methods
4- First paragraph is unaligned and in bold. Correct this.
   Response to point 4:
   Accept: this change has been made.

5- The authors referred to a paper (reference 17), which is incomplete in the
reference list (lacks the title of the journal, year of publication, volume and pages). Due to this error, I did not manage to have access to that paper. Thus, it was not possible to evaluate crucial aspects of the methods applied in the study.

“Further details of the study methodology, including entry criteria, randomisation, recruitment of patients, recruitment and training of dentists, and data relating to the placement of the restorations have been reported (reference 17)."

Response to point 5:
Accept: This should not have been a problem as this paper was one of two, paired, papers submitted, with the request these papers be reviewed together. However, this point has now been dealt with as the papers have been combined.

6- There is a sentence (“List of figures”), which is out of place (second line above “Power calculation”).

Response to point 6:
Accept: This was one of a series of formatting errors created during the BMC formatting procedure after the paper was sent and had been rectified now.

7- The study population needs to be better defined. Who are the participant children and dentists? Regularly attending patients or any patient with eligible teeth? Etc. This information should be given in the paper.

Response to point 7:
Accept: This information is already included in the other paper submitted alongside this one and now that both papers have been combined, will now be more obvious.

8- When more than two eligible teeth were present, how were the teeth selected?
And what about the remaining teeth, how were they treated? Were both selected teeth treated at the same appointment? If not, what was the time interval between the appointments? This information should be given in the paper.

Response to point 8:
Accept: This information has now been added (page 6 and page 8) “Where more than one pair of matched carious lesions were present in a child’s mouth, the dentist chose which pair should be part of the study. Any carious teeth outwith the study were managed as per the dentists’ normal treatment regime.” “whether the restorations were carried out at the same or separate treatment appointments;”

9- The Hall Technique PMC should be explained in more detail. In the title it is claimed that this is a novel technique. But it appears that this is the usual stainless steel crown, popular in pediatric dentistry at least since the 1980s. May be the innovation is to use it without removing carious tissue and no tooth preparation, but this has to be explained in the methods section. So, it is not clear what is new about the technique and being new or not, the intervention procedure should be clearly explained in the methodology. For example, it should be explained how it is possible to fit a crown without any
prior tooth preparation, and not to create a premature occlusal contact point with the antagonistic tooth, which could make the child feel uncomfortable due to the presence of a high restoration.

Response to point 9:
This information was explained in more detail in the paper that went alongside this as well as being in the original text in the introduction: “This method, the Hall Technique, uses preformed metal crowns (PMCs), which are filled with glass-ionomer cement, and simply pushed onto the tooth with no caries removal, local anaesthesia or tooth preparation. The Hall Technique embraces changing concepts of managing dental caries, moving from the dogma requiring its complete surgical excision, even at the expense of cavity size and pulpal health\(^27\), to the understanding that caries in dentine can be slowed, arrested, and possibly even reversed, within a meticulously sealed environment\(^28-30\).”

However, the explanation should now be more obvious with the data being combined.

10- There was no explanation of how the data were analyzed. For example, what statistical tests would be applied to reject the null hypotheses? Include sub-section about data analysis in the methods.

Response to point 10:
Accept: This has now been included in a section on “Data analysis” in the methodology

Results
11- The reporting of major failures, of episodes of pain and of minor failures; as this was a split mouth design, it is appropriate to use statistical tests for paired data, in order to reject the null hypotheses.

Response to point 11:
The data has now been reanalysed using McNemar’s test

Figures
12- All figures were identified as Figure 1. Correct this.

Response to point 12:
Accept: this has been rectified

General
Comment 1.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
Comments 3, 5, 7, 8, 9, 10, 11.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
Comments 2, 4, 6, 12.

Discretionary Revisions (which the author can choose to ignore)
None.
What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests.
Reviewer's report
Title: The Hall Technique; a randomised control clinical trial of a novel method of managing carious primary molars in general dental practice: outcomes at two years.
Version: 3 Date: 7 August 2007
Reviewer: Trevor Burke
Reviewer's report:
Recommend acceptance with minor changes. Also acceptability paper 3347095191467641 to be 1 and outcomes 4339137131467774 2.

Response to Reviewer's report:
The papers have now been combined so this point is no longer relevant.
Reviewer's report
Title: The Hall Technique; a randomised control clinical trial of a novel method of managing carious primary molars in general dental practice: outcomes at two years.
Version: 3 Date: 8 August 2007
Reviewer: Wil Sanden
Reviewer's report:
General
This study, a RCT, describes an interesting modification of an old technique for treatment of dental caries in deciduous molar teeth in a high caries risk population. The paper is well written and easy to read. The use of the CONSORT diagram gives a good insight in this study. After having read this paper, I could better understand the other paper from the same authors, I received [about patients’, parents and GDPs’ preferences] some weeks ago. To understand this study, presented in two papers, they should be read together, as otherwise some necessary information is missing. Therefore, I would recommend to combine both papers, as this will make the paper more readable, and thus this interesting study more understandable.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached) 1) After reviewing the first study, some weeks ago, I wrote: “I have some doubts on the description of this study/RCT, as e.g. no power calculation [sample size] or CI are given [see usual form for ‘critical appraisal of RCTs’]. More background information is necessary to understand the setting and findings of this study… “. After reading this [second] part of the study, I received all the information I need. To understand this study, both papers should be read together. Therefore, I recommend that the manuscripts should be combined and treated as one paper.

Response to recommendation:
This recommendation has been accepted and the papers combined into one paper.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
1) List of figures [pages 20, 21] is correct, but does not correspond with written # in heading of individual figures, as all figures are listed as ‘figure 1’ [footer shows the right figure#].

Response to point 1:
Accept: this has been rectified

2) Page 8, line 8: were radiographs reassessed independently by the two authors? If so [as I assume], I would add that.

Response to point 2:
Accept: This information is already in the paper at the end of the methodology section and has been retained in the same format for the combined papers (page 10)
“Investigation of inter- and intra- examiner reproducibility and repeatability were carried out using a computer generated randomisation table to select 10% of individual radiographs, which were then reassessed by two of the authors (DE and NI). Kappa analysis values ranged from 0.60 to 0.84 (‘good’ to ‘very good’).”
3) Page 11, lines 7 – 9 [Non-visualisation …debatable]: this belongs to the ‘discussion’ section.

Response to point 3:
Accept. This point has been moved to the “discussion” section.

4) Page 15-16: the message is that some ‘old’ ideas about caries management are not valid anymore; why spend so many words on that? This study represents a sound RCT, and as such the results should be reliable. I recommend to shorten the text.

Response to point 4:
Reject: This study represents one of the very few prospective RCTs which has set out to look at the effect of sealing in caries with no caries removal at all. In fact, the only other study which has attempted to do this is a study by Eva Mertz-Fairhurst (Mertz-Fairhurst EJ, Curtis JW, Jr., Ergle JW, Rueggeberg FA, Adair SM (1998). Ultraconservative and cariostatic sealed restorations: results at year 10. J Am Dent Assoc 129(1):55-66). However, it should be noted the Mertz-Fairhurst study did not actually set out to look at the effect, on the tooth or the patient, of sealing in caries but aimed initially to look at the effect on restorative materials’ performance when they were placed over caries (Mertz-Fairhurst EJ, Call-Smith KM, Shuster GS, Williams JE, Davis QB, Smith CD, Bell RA, Sherrer JD, Myers DR, Morse PK, et al. (1987). Clinical performance of sealed composite restorations placed over caries compared with sealed and unsealed amalgam restorations. J Am Dent Assoc 115(5):689-94). In addition, despite the Mertz-Fairhurst work being published many years ago, the philosophy of incomplete caries removal has not been taken on by the profession, perhaps as a result of incomplete understanding of the pathophysiology behind the results. Our study holds a central position in the research field of cariology (especially when it is considered that caries management and tooth restoration are probably the most common set of procedures carried out in dentistry every day). This is especially so when the findings support a methodology absolutely contrary to the way these procedures are most commonly carried out (complete caries removal). It is true that the results of the trial are sound and reliable, however, for the reasons discussed above, we feel that a full discussion of the possible hypotheses behind the results is important in moving the research field forward and encouraging debate on results that many researchers and clinicians find surprising given the extent of damage to the teeth on entry to the trial.

5) Page 17, line 25 [This was with…] till page 18, end of section: what does this add? I recommend to remove this part.

Response to point 5:
Reject: This is a novel use of PMCs where there is no local anaesthetic used, no tooth substance removed to make space for the PMC and no caries removed. These figures put in context the clinical outcomes using this novel method (The Hall Technique) with the literature’s report of those PMCs placed with conventional preparations using local anaesthetic, tooth substance removal with a high speed bur and where caries is removed.
Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions.

Level of interest: An article of importance in its field.

Quality of written English: Acceptable.

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests.
Reviewer's report

Title: The Hall Technique; a randomised control clinical trial of a novel method of managing carious primary molars in general dental practice: acceptability of the technique.

Version: 5 Date: 23 July 2007
Reviewer: Wil Sanden

Reviewer's report:

General
This study describes an interesting modification of an old technique for treatment of dental caries in deciduous molar teeth. The paper is well written and easy to read. However, there are some shortcomings that need further explanation.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1) I have some doubts on the description of this study/RCT, as e.g. no power calculation [sample size] or CI are given [see usual form for 'critical appraisal of RCTs']. More background information is necessary to understand the setting and findings of this study. Children may like every treatment different from 'drilling', this might have influenced the results. Therefore, an extensive description [with exact in- and exclusion criteria, used forms/formulations for questioning children, etc., etc.] of the study is necessary to be able to judge if this is a reliable RCT or not.

Response to point 1.
Accept: This point has been addressed by combining the papers

2) The GDPs were asked to judge the level of discomfort, as experienced by the child. This might cause some observer bias [as the study deals with Halls PMC versus control], and might weaken the results of this study. The authors already mention this is subjective, but further explanation is necessary, as this is one of the statistically significant results. Did GDPs receive a training in judging children's level of anxiety?

Response to point 2.
Accept: The GDPs did not receive any training and there may have been some observer bias. However, this particular result is reflected in the procedure preferences that the parents, children and dentists expressed which would partly, at least, reflect each groups' judgement of the discomfort of the procedure. The text has been changed to reflect this.

3) From abstract:

Response to point 3.
Not a point to be addressed – number is a typo

4) Results:
Around nine out of 10 children and dentists, and three-quarters of carers, expressed a preference. For 77% of the children, 83% of the carers and 81% of the dentists, this preference was statistically significant in favour of the Hall PMCs (Chi square test, p < 0.0001). There was no significant difference in the time taken to explain and provide either restoration.
Conclusion:
The Hall Technique was more acceptable than conventional restorative techniques for carious primary molar teeth to the majority of children, their carers and GDPs. In addition, children were gauged by their dentist as experiencing less discomfort with the Hall Technique. Conventional restorations and Hall PMCs took similar times to explain and provide.

Response to point 4.
not a point to be addressed – numbering is a typo

5) Further explanation is needed why the Hall technique is more acceptable, as concluded by the authors: the question dealt with ‘patients' preference’, but this is something else. Regarding figure 4, a subjective estimation, for PMC 130, and for control 126 children ‘managed’ the treatment, thus accepting it.

Response to point 5.
Accept in part. It should be noted that although 126 children “accepted” a conventional restoration, for the 29 children where a reason was given for incomplete caries removal, for 17 of these, the reason reported was lack of co-operation or patient becoming distressed, so it cannot be assumed that because a restoration was placed, that the child found the process acceptable. To clarify this point, the term “acceptable” has been replaced with “patient preference” where appropriate.

6) The references are somewhat limited, 10 [from total 32] are from the same journal. This might cause some reader bias.

Response to point 6
Reject:
Now that the two papers submitted have been combined, there are 58 references which comprehensively cover the material in the introduction, the methodology and the discussion. It is not surprising that 10 of these references come from the BDJ as this study is based in the UK and refers to salient work carried out in the same environment which supports the rationale behind this work. This is not felt to be a drawback as they virtually all have different authorships. In addition, it is not possible to change pertinent references just because they happen to appear in the same journal. The references support the paper.

------------------------------------------------------------------------------------------------------------------------
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
1) Abstract: Background: the sentence ‘16% have had teeth extracted’ is not mentioned in the original text, but only in the abstract.

Response to point 1.
Accept: Has been inserted into the text beside the appropriate reference

2) List of figures is correct, but does not correspond with written # in heading of some individual figures, e.g. figure 1 appears three times, and is used for figure 1, 4, and 9, respectively [footer shows the right figure#].

Response to point 2.
Accept: This has now been changed.
3) Figure 3, description false: tooth 75 is mentioned (LRE), this should be LLE, and tooth 85 is mentioned (LLE), this should be LRE.
   Response to point 3.
   Accept: This has now been changed.

4) Fig. 3, further recommendation: number radiographs with ‘a’ and ‘b’, in combination with tooth#, to avoid confusion.
   Response to point 4.
   Accept: This has now been added.

5) Figure 4: recommendation: scale 3: use e.g. ‘mild, not remarkable’ or ‘not important’ instead of ‘not significant’, in order to avoid [statistical] confusion.
   Response to point 5.
   Reject: In retrospect, it may have been more appropriate to have used the term “not important” in the original documentation the GDPs completed. However, this was not so and the exact wording they responded to was “not significant”. As this is in a clinical context rather than statistical, it is unlikely the reader will be confused between the statistical connotation of “significance” and the word’s use here.

6) Figure 7: bitewing radiograph does not agree with description of technique to be used [Rinn technique]; my advise: use another bitewing radiograph [figure 8 shows a good example of a BW radiograph].
   Response to point 6
   Accept; radiograph has been changed to another one from the study

7) Table 1: ‘%’ appears both in heading and in column, only in heading is enough.
   Response to point 7:
   Accept
   Has been changed.

8) Table 6: add ‘min’ in heading of each column
   Response to point 8:
   Accept
   Has been changed.

9) Text [recommendations]
   Response to point 3:
   Not a point to be addressed – number is a typo

10) Page 2: add a reference after ‘amongst the barriers …support this view’.
    Response to point 10:
    The references are in the text already so the paragraph has been altered to make this clear.

11) Page 8: Results section: both under ‘dentists’ and ‘patients’ the 132 patients/children are mentioned. Once should be enough.
    Response to point 11:
    The text has been changed to avoid repetition.
12) Page 8: ‘Patients’ section: seems there is a sentence missing between ‘… for a clinical reason. Therefore, it was…’. At least, this decision should be explained more clearly!
   Response to point 12:
   Accept: Changed to improve clarity. Now reads: “This decision was not based on clinical rationale relating to the treatments, it was, therefore, decided to include these patients.”.

13) Page 10, “Use of separators…”: please explain difference between figure 5 [max is 21 patients per GDP] and the sentence ‘one dentist used them for six out of 25 cases, …’.
   Response to point 13:
   Accept: This is an error and has been altered in the text to read: “them for six out of their eight cases”

14) Page 11, ‘consequences…’ section: make figures more readable [2.39 should be 2.4. etc.], as this exactness is somewhat overdone, here.
   Response to point 14:
   Accept: The figures have been changed accordingly to one decimal place.

15) Discussion [recommendations]
   Response to point 15:
   not a point to be addressed – numbering is a typo

16) Most results are repeated completely, this is not necessary, should be shortened; this will make the text more readable.
   Response to point 16:
   Accept: This point has been addressed.

17) More important points in discussion
   Response to point 17:
   Reject: As the two papers have now been combined and would seem to be very long, it is now considered inappropriate to place more material in the discussion which already comprehensively covers the results section and the implications of this clinical trial.

18) Discussion on ‘socially accepted answers’ given by young children, as this is one of the main results of this study.
   Response to point 18:
   Reject: Please see point 17 above.

19) Discussion on the aesthetics of the crown is necessary, this might have altered the results! The treatment might be more acceptable, but I don’t think children and parents are happy with the view of these metal crowns. Moreover, I assume the normal treatment would be a resin composite filling, thus tooth coloured.
   Response to point 19:
   Accept in part. This study did not specifically look at the patients and parents perception of the aesthetics of the crown, although they were specifically asked which restoration they preferred once both had been placed. Despite 89% of the 120 conventional restorations placed
indeed being tooth coloured, a significant majority of both children and their parents still preferred the Hall crowns. This indicates that the aesthetics of the crowns is perhaps not as problematic as the referee might think. As the findings on patient preference are so clear, an extended discussion on the role of aesthetics has not been included in this paper in the interests of maintaining readability. The issue of the appearance of the crowns will, however, be investigated in a further study.

20) Discussion on the relatively high number of not satisfactory Hall crowns (15%), and what is the clinical meaning of this [re-restoration, removal?].
   Response to point 20:
   Accept: This point had not been addressed in the discussion of the paper as, given the very low failure rate of the PMCs, both for “Major” and “Minor” failures, it would not appear to have clinical relevance. However, this point has now been made in the discussion to guide the reader and reads “Despite there being quite a high level of PMCs judged as being an unsatisfactory fit radiographically (15%), the very low failure rate for the PMCs over the two year period (both for “Major” and “Minor” failures) seems to indicate, with the numbers of patients in this study, this was not of great clinical relevance.”.

21) Further, more explaining, discussion [see page 14] on the dentists’ subjective assessment of the children’s level of discomfort should be given, as this deals with one of the main results
   Response to Point 21:
   Accept. See response to Major Compulsory Revision Point 2; above.

Discretionary Revisions (which the author can choose to ignore)
What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions
Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests
Reviewer's report
Title: The Hall Technique; a randomised control clinical trial of a novel method of managing carious primary molars in general dental practice: acceptability of the technique.
Version: 5 Date: 7 August 2007
Reviewer: Trevor Burke
Reviewer's report:
Page 8
Discomfort Assessment. Was this scale derived from any previous work? Was it validated? How are the authors certain that there was no bias?
Response:
This study was set up in 2001 and we found no validated measure of dentists’ assessing child patients’ discomfort during dental procedures to use as part of the methodology. Although there are now a number of validated measures of children’s anxiety, there is very little attention paid to the perception children have of the treatment carried out. The assessment scale was derived in order to allow a pragmatic measure of what the dentist’s perceived the child felt with regard to the treatment. It is not validated and we cannot be certain there was no bias except that the results are mirrored in the patients’ preferences for each treatment option.

Page 8
3 lines from bottom
Surely one year is too long for the follow up in terms of occlusal contact. Would 2 weeks not have been more appropriate? This should be discussed.
Response:
Accept: It was not possible within the study design to build in an extra visit at two weeks for the patients to be seen by their dentist and trying to construct an ideal trial design within the limits of the general dental practice setting is one of the challenges that faces Primary Care based research. Many of the children would have been seen by their GDP following treatment within the clinical trial, in order to complete other restorative treatment and comments from the dentists (and personal experience) is that the occlusion does tend to adjust within a short time span (days to weeks rather than months). This would be something worth looking at more closely.
The following has been entered into the discussion. “Although it would have been ideal to have the dentists seeing the children two weeks after the crowns were fitted to assess the occlusion, it was not possible within this study design. Trying to construct an ideal trial design within the limits of the general dental practice.”.

Page 12 Was there any assessment of the aesthetic acceptability of the PMCs? This reviewer has anecdotal evidence that some parents object to the metal appearance of PMCs. Any evidence in the present work?
Response:
Patient and parent views on the esthetics of the crowns were not specifically investigated in this study, although the preferences of these groups for either the Hall crowns or conventional restorations were investigated. Despite 89% of the 120 conventional restorations placed indeed being tooth coloured, a significant majority of both children and
their parents still preferred the Hall crowns. This indicates that any aesthetic problems children and parents might have over the appearance of the crowns does not outweigh the advantages as perceived by these groups. As the findings on patient preference are so clear, an extended discussion on the role of aesthetics has not been included in this paper in the interests of maintaining readability. The issue of the appearance of the crowns will, however, be investigated in a further study.

Page 13
Discussion –10 lines from bottom
There are also many examples of successful clinical evaluations in general dental practice. Some of these should be referenced just to balance the argument.

Response:

Page 15
Discussion on time taken.
Perhaps the authors could compare this with another minimal intervention methodology, Carisolv, which patients liked but dentists didn’t because it took too long.

Response:
We accept that this would be an interesting comparison but now that this paper has been combined with the other one, there is limited room to explore wider issues.

Recommend acceptance with minor changes. Also acceptability paper 3347095191467641 to be 1 and outcomes 4339137131467774 to be