Author’s response to reviews

Title: The Relationship between Body System-Based Chronic Conditions and Dental Utilization for Medicaid-Enrolled Children: A Retrospective Cohort Study

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Dear BMC Oral Health Editorial Staff,

Thank you for the suggestions regarding MS: 3388818965805310. Revisions in the updated manuscript and responses to each referee are noted in purple. We look forward to working with you as we progress through the review process.

Sincerely,

Donald Chi
Corresponding Author
Responses to Referee 1

Overall evaluation
The purpose of this investigation was to assess utilization rates of dental care services among Medicaid-enrolled children across 10 body system-based chronic condition subgroups. The rationale for the study was well defined and reasonable. The methods were thoroughly described and appropriate for the purpose stated. The source and form of data (claims data from the Iowa Medicaid Program for 2005-2006) was appropriate for the analyses. In general, the authors did a good job responding to my initial critique. The sample selection diagram is a useful improvement. The changes to Tables 1a and 1b are helpful (although additional edits are suggested – see below). The response to the recommendation for using different age categories was understandable but disappointing. I do not believe parents decide to take their child to the dentist based on their child’s dentition. The tables are still difficult to read and suggestions are offered for improvement.

The paper now more explicitly explains that this paper focuses on children with chronic conditions, only, and makes a few references to children without chronic conditions for comparison. Although I agree with the other reviewers that not including children without chronic conditions leaves questions unanswered, I believe that the current report can stand on its own without said comparison. The question remains whether the analyses are ultimately useful without these comparisons, as clinical parameters and public health policies are not likely to be developed for such specific sub-groups of the populations.

Major Compulsory Revisions
No major compulsory revisions are required.

Minor Essential Revisions
1. Tables 1a and 1b have multiple columns representing children who do not have each chronic condition. These column data are unnecessary and clutter the tables. They should be removed.

We present our data in a format that is typical for studies on dental care use. Tables 1a and 1b contain multiple columns (no and yes for each chronic condition) to indicate the relationship between model covariates and each chronic condition. We agree that the Tables contain a lot of data, but believe that this level of detail is needed to provide readers with information on the bivariate relationship between each covariate and the 10 chronic conditions.

2. Tables 1a and 1b contain symbols that correspond to statistical tests however it is not clear what the tests are or what they mean. A key should be included to explain what these tests represent.

A key has been provided as a footnote in Tables 1a and 1b to clarify the statistical test that was used (Pearson chi square) and what the test represents (the bivariate relationship between model covariates and each chronic condition).

3. Table 2 also contains unnecessary “no” columns and rows. What the reader is most interested in is the percentage of children with a chronic condition (e.g., respiratory condition) that utilized dental care services in 2006 (e.g., any dental care). What children without this condition did is unnecessary in the tables.
As stated above, we include a “no” column to indicate the bivariate relationship between each model covariate and outcome measure. This is a standard way of presenting descriptive data on the study population.

4. Again, the statistical tests are not easily understood and a key should be included for Table 2.

A key is provided.

5. Table 3 is probably unnecessary as the chi-square results can be included in the abbreviated Table 2 (see comments above).

Table 3 provides data on the unadjusted relationship between each chronic condition and outcome measure. It addresses the question of whether there are significant differences in dental use across each chronic condition subgroup (and provides actual percentages).

6. Table 4 is extremely difficult to interpret. This table should be greatly shortened – perhaps the no categories can be eliminated and some models can be eliminated.

To improve the readability of Table 4, we removed the p-value column (which is not necessary because we provide 95% confidence intervals) and the “no” categories. The models were left intact because the corresponding relative risk ratios are needed to address the main research question.
Responses to Referee 2

I think the change from odds ratios to relative risk ratios has greatly improved the paper. (I continue to believe that marginal effects would have been better, however. I think if you look at my examples in my previous review you will see that marginal effects provide a particularly meaningful way to consider the magnitude of effects not only relatively, but also absolutely.) However, RRRs are a big improvement over ORs. Most of your RRRs are not that far from one, though.

I continue to believe that previous preventive care use probably should not have been included in the model and don’t think a reference to previous literature is very convincing. But it probably depends on your goal. If you are including some covariates — e.g. whether the child has at least one Medicaid-enrolled sibling — since you think those variables can be targeted — then you should probably exclude the preventive care variable. If you found that children with another Medicaid sibling were significantly and strongly more (or less) likely to receive dental care, then assumedly the policy implication would be that you would particularly want to target children without (with) Medicaid-enrolled children for outreach. But having a Medicaid-enrolled sibling is also likely a predictor of preventive care use and so the preventive care use variable is likely to “take out” some of the effect of the sibling variable. Unless you are arguing that you can target children with more or less preventive care, and you truly just want to know the marginal effect of sibling after controlling for preventive care use? I tend to think for policy purposes (which I assume is the point of the article) that you would be better off without the preventive care variable in.

We tested for a potential interaction between the sibling and previous preventive medical care use and found no evidence of such an interaction. Had there been a significant interaction, there is no reason to believe that such an approach would be the most logical way to approach intervention development. One limitation of using claims data to address out research question is the difficulty of adjusting for all potential confounders, including patient-level behaviors. Based on our conceptual model, previous medical care use is a proxy for patient (or caregiver) preferences for health care services and was left in the final models. Additional research is needed to fine tune the relationship between preventive medical care use and dental care use, but this issue is beyond the scope of the current study. In the revised manuscript, we include additional text on the potential for residual confounding and suggest possible ways to avoid this problem in future studies.
Responses to Referee 3

In this revision, the authors have provided new results and/or text that partly addresses two of my original three concerns. However, the new results, which report probability ratios, as suggested, only reinforce concerns expressed originally in my third comment, namely that the observed differences between most medical subgroups are small and of little public health relevance. For example, the abstract draws attention to dental attendance ratios for three sets of conditions "(RR: 1.03 to 1.13; 1.0 to 1.08; 1.02 to 1.12)". Yet there is no new text stating that these are very small effect sizes.

We agree that the effect sizes associated with the RR in our study are small, but disagree that small effect sizes are of "little public health relevance". On an individual-level, a statistically significant RR of 1.03 may not be important. However, when examining dental care use on the population-level for thousands of Medicaid-enrolled children with chronic conditions, a 3% higher dental use probability could indicate better access to care for hundreds of children. In the revised manuscript, we call attention to the small effect sizes but also highlight the public health significance of these small effect sizes.

My original concern was not merely a technical one of statistical modeling, so I repeat it here: "...a third major shortcoming of this paper, namely its discussion of interpretation and implications. The fact is, all of these groups have levels of dental care that are MUCH lower than desired when considering their medical conditions and low socioeconomic status." In their response to my critique, the authors agree, yet there is no new text in this revision that critically discusses this point. What's needed is a critical discussion of these new results.

We failed to emphasize this important point. The revised manuscript (Abstract and Conclusion) addresses the larger problem of low utilization in our study population while emphasizing our main finding that there is heterogeneity in dental care use within the population of Medicaid-enrolled children with chronic conditions.

One good place to start would be the authors' own study, now cited as Reference 12, where dental attendance odds-ratios were 1.04 to 1.07. In the revised text of this paper, that same study is now referred to in this way: "In regards to dental utilization patterns, children with these conditions appear to be similar to Medicaid-enrolled children without chronic conditions [12]." If odds ratios of 1.04 to 1.07 are indicative of "similar" dental utilization patterns in Ref 12, there seems to be little justification for the claim in this abstract that "Children with respiratory, musculoskeletal, or ear/nose/throat conditions were more likely to use most types of dental care ...".

Thank you for pointing out this confusing sentence. There was a typographical error in the revised manuscript. The quoted sentence has been removed.

In fact, with the exception of catastrophic neurological conditions, experienced by a small number of these Medicaid-enrolled children, the overall impression is that chronic dental attendance probabilities are similar across medical conditions.

Our goal was to identify potential heterogeneity in dental care use across the 10 chronic conditions subgroups. The concern regarding the public health significance of small effects sizes is addressed in the revised manuscript, with a relevant reference from behavioral medicine (Rutledge & Loh 2004).
On a minor note: "relative risk" is a measure of effect used in studies of disease risk. A more appropriate term is "probability ratio", and more specifically, "dental attendance probability ratio".

Given that our outcome measure is not a disease, “dental utilization probability ratio” (DUPR) may be a more appropriate term. However, we believe RR is easier to understand and using DUPR may confuse readers. Therefore, we use the term RR. If *BMC Oral Health* has a particular preference, we would be happy to use the preferred term.