Author's response to reviews

Title: Predictors of well child care adherence over time in a cohort of urban Medicaid-eligible infants

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Version: 3 Date: 8 April 2011

Author's response to reviews: see over
Dear editors and reviewers:

Thank you very much for the opportunity to revise and resubmit this paper. We very much appreciate the time you have taken in reviewing it. Following is a point-by-point response to the reviewers’ comments.

Dr. Mulvihill’s comments:

“First, I would suggest that in the Methods section the authors display the data collected in a tabular format that would indicate what data including instruments used, the variables that instrument yielded, and the intervals at which each type of data was collected. This would make it much easier to understand what was done and when.”

We agree that clarification of the variables and the timing of their collection is helpful. We have added a new table 1 titled “Data elements, measurement, and source”.

“Second, I think the authors need to clarify the time periods of the required EPSDT visits. The first category says by 6 months (does that include the 6 month visit or up to 6 mo?) then between 6 and 12 months (are 6 month and 12 month visits included in this interval/), etc for the rest of intervals. This needs clarification. The intervals were presented more clearly on p9 in the second paragraph of the results.”

In order to clarify the categorization of our outcome, we added table 2, titled “Well child visits included in each time period”.

“The third item in this category is to tell how many children were excluded because they attended practices out of the network. (second paragraph under the Outcome section)”

Under outcome, now at the top of page 8, we restate the number of children followed for at least one six-month period.

“Lastly, I do not think it is correct to say that “adherence was predicted most strongly by maternal prenatal care adherence”. That was one of the stronger but not the strongest predictor.”

In the discussion we corrected the first sentence of the second paragraph on page 13, by deleting as well as and most strongly.

“In the first paragraph of the background is it not clear if WCC and immunization adherence are meant to be the same thing.”
We have updated the first paragraph of the background section to more precisely reflect the evidence for predictors of well child care adherence, and removed references to immunization adherence, which we hope clarifies the summary.

"Would be nice to know how the SF 36 was scored."

On page 8, we have added a reference to the Stewart and Sherbourne article that describes version 1 of the SF 36. We use this standard RAND scoring system.

“For the Confidence Intervals on Figure 1 would be nice to see the numbers.”

We have added confidence interval labels to figure 1.

“There is a long awkward sentence in p. 13 beginning with "Mitigating this potential .....”

The awkward sentence originally on page 13 has been revised, now on page 15.

Dr. Chung’s comments:

“1. Introduction. The incremental value of well-child care visits with respect to most short-term and long-term outcomes remains unclear, despite the hospitalization study cited by the authors. In general, we believe that more is better, but there is strikingly little evidence to tell us what constitutes enough, and what schedule provides maximum value. So beginning with a presumption that adherence to relatively arbitrary visit guidelines (even as consensus-based as they are) is somehow intrinsically meaningful is up for debate. Nevertheless, much of the literature uses a dichotomous adherence indicator similar to what the authors selected, so the authors can’t necessarily be faulted for doing the same. Neither the introduction nor the discussion, however, acknowledges this elephant in the room. This is unfortunate, because the manuscript hinges on the presumptive validity of a dichotomous adherence indicator.”

The dearth of evidence supporting well-child care as currently structured is clearly problematic for any study of adherence to well child care. Our justification relies on the fact that the majority of visits during the first two years of life coincide with the immunization schedule, for which there is abundant evidence. We have added a sentence reflecting this in the first paragraph: “Although there is ongoing debate about how WCC should be structured and limited evidence of its efficacy, for children under two years old, the majority of visits coincide with recommended immunizations, for which the evidence is robust.”

“2. Methods, par 1. The n’s for Medicaid eligibility and English proficiency are unclear. Is 1432 a subset of 1535, or were the 2 numbers independently determined? How exactly did they get down to 744?”

We described in greater detail the inclusion criteria and added appendix 1, a flowchart indicating numbers of subjects eligible for and retained in the study.

“3. Methods, Outcome, par 1. Going back to #1, how did the authors validate their dichotomous outcome? Why, for instance, should missing 1 out of 3 visits between 0 and 6 months be considered qualitatively equivalent to missing 2 out of 3 visits? Were sensitivity analyses performed using other characterizations of the outcome variable?”
Our initial outcome was defined as complete adherence. In order to validate this choice, we performed the analysis re-categorizing all subjects who had missed up to one visit as adherent, with the exception of the fourth six-month interval, which has only one recommended visit to begin with. We describe this at the bottom of this paragraph, and indicate the results on page 12. In this analysis, we found that income and prenatal care adherence remained significant predictors of well child care adherence. The other previously significant independent variables no longer had a significant relationship, but preserved the direction of their effect.

“4. Methods, Outcome, par 2. The way outcomes were assigned as missing is somewhat troubling, but appears to be insoluble given the data limitations. Additional sensitivity analyses using assumptions about the missing outcomes might be useful, just to reassure the authors about bias.”

In addition to the sensitivity analysis we performed for item 3 above, we tested our model for sensitivity to the assignment of missing status as recommended. We did this by including all intervals with any visit and all intervals following the first visit as non-missing. The description of this appears at the top of page 8, and the results are described on page 12 and in appendix 3. In this analysis, parity and prenatal care remained significant predictors, while marital status and income are no longer significant. Based on these two sensitivity analyses, we’ve modified our conclusions to emphasize parity over marital status as a predictor of adherence. While marital status may indeed play a role, the number of married women in our sample was too small to discriminate in light of these new analyses.

“5. Methods, Predictors, par 2. No justification is given for the dichotomization of predictor variables.”

Now on page 8, we detail which variables were re-categorized and the rationale for dichotomization when it was performed.

“6. Results, par 1. Although it’s understandable that patients who did not complete at least 6 months of follow-up were dropped, the sheer number of them—165—creates a potentially worrisome source of selection bias and concerns about representativeness. These patients could be fundamentally different from the patients who stayed, especially from the ones who stayed the entire 2-year period.”

We concur that the number of children lost to follow-up may introduce bias. On page 10, we describe a demographic comparison between the full cohort and those who dropped out and include the results in appendix 2. There were no significant differences between the two. While this does not rule out selection bias, it does offer some reassurance. In addition, in what is now table 3 (previously table 1), we have added the comparisons between the EMR subset and the remainder of the cohort, which were inadvertently omitted in the previous version.

“7. Results, par 4. The authors write, “Specifically, married (OR 1.71, 95% CI: 1.09-2.69) and primiparous (OR 1.87, 95% CI: 1.36-2.63) mothers had significantly greater odds of adherence than single mothers with more than one child.” Based on the data in Table 3, this is incorrect. The statement would only be true if the authors had tested interactions between marital status and parity. As it stands, the authors can only state that married mothers had greater odds of adherence than single mothers (controlling for parity), and that primiparous mothers had greater odds than mothers with more than one child (controlling for marital status).”
We concur that the original phrasing did not correctly represent our findings, and have modified it accordingly.

“8. Discussion, par 2. Again, the discussion is imprecise with respect to marital status and parity. Moreover, including the interaction term might yield some insight re: the two proffered hypotheses.”

As above, we clarified the wording. We additionally tested for interaction between marital status and parity and did not find a significant effect.

“9. Discussion, par 3. This finding isn’t particularly counterintuitive. It’s consistent with literature suggesting that children with Medicaid may follow EPSDT guidelines as well or better than privately insured children, whose providers and insurance companies may be under no obligation to recommend or cover the same number of visits.”

We deleted ‘counterintuitive’ here. For clarification, the number of children who were not insured by Medicaid in this sample was small (less than 5%), as a consequence of the inclusion criterion of Medicaid eligibility. Our ability to detect any significant associations related to insurance status is therefore significantly limited.

“10. Conclusions. The last sentence of the conclusions, both in the text and the abstract, are not supported by the authors’ analyses. The authors are probably correct, but their analyses are silent about responding to families’ needs.”

In both the conclusion and the abstract, we have deleted the sentence in question.

In addition to changes in response to the reviewers’ comments, we updated the title page to reflect Dr. Pati’s affiliations. Additions to the references have been underlined.

We thank you again for your time.

Yours sincerely,

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