Reviewer's report

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

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Reviewer: Paul J Chung

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This is an interesting manuscript about an important topic. In general, it is well-written and clear. The abstract fairly represents the manuscript, the introduction provides an adequate review of the literature, the methods are straightforward, the results appear reasonably valid, and the discussion largely follows from the results. The findings are modest but may contribute something of value to the literature.

Major Compulsory Revisions

This reviewer would suggest that the authors and editors view some of the following as questions or comments rather than specific revisions.

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.

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?

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?

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

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.

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).

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.

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.

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.

**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.