Reviewer’s report

Title: Health, poverty and cognitive function in pre-school children: a cohort study in a middle income urban context

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Reviewer: Margaret Caughy

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BMC Public Health Manuscript
“Health, poverty and cognitive function in pre-school children: A cohort study in a middle income urban context”.

Reviewer comments

Before I provide a critique of this paper, I’d like to provide a caveat. I am not familiar with this journal or the audience it serves. Most of the journals I read are developmental journals, and as such, there may be different standards for manuscript format, depth, and analytic approach. However, as the research question is fundamentally a developmental one, I feel it is appropriate to evaluate this paper from such a perspective.

Overall, I felt the paper was interesting with some intriguing results. I believe it has potential to contribute to the literature. However, I feel that additional work is needed before the paper is appropriate for publication. I will organize my comments according to the sections of the paper.

Introduction: I found the introduction to be unusually short. Again, perhaps my perspective is different, but introductory sections are usually 4-5 times as long as the introduction for this paper. Because of the brevity of the introduction, I did not feel I had a very good idea of the primary goal of the analysis. It appears the authors may be focused on contrasting the importance of poor early health with the importance of early stimulation as predictors of cognitive competence in young children, but this is not explicit. I am not familiar with such studies of determinants of cognitive competence for children in countries such as Brazil, and it would be useful to have a fuller explication of that literature as a foundation for the current investigation.

With regards to the conceptual model for this study, although I appreciate the contextual perspective, I’m not sure I agree with the presentation in the figure. Specifically, all of the more distal factors are seen as being mediated by psychosocial stimulation. I could see arguments for a number of the factors that are not mediated in such a way. For example, I would suggest that the impact of child health status (at least as measured by the factors in this study) on cognitive competence is not mediated by psychosocial stimulation as measured by the HOME score or preschool attendance. In order for that to be the case, one would have to argue that compromised child health status results in significant disruption of the quality of the home environment and/or preschool attendance. Although it’s not outside the realm of possibility, there are likely other, more direct paths of influence.

Methods: Although the figure delineating the study sample is helpful, I’d like additional information regarding those who refused and/or lost to follow up. Were they different or similar to those who are included in the analysis in terms of demographics, child characteristics, etc?

More detail should be provided regarding the details of data collection. Specifically, although the children were under age 42 months before 1999, what was their age range at the time when the Bayley was administered? If that range is very wide, it might be problematic. Where did data collection occur and by whom? Was the data collection in the home or in a clinic setting? I assume that because the HOME was administered, at least part of the data collection occurred in the child’s home. Were the medical data collected via self report or from the child’s medical record?

I’d like to see more rationale for how the infectious illness variables were operationalized. As detailed at the bottom of page 8, there are four different measures of infectious illness – 3 related to specific organisms and one related to experience of diarrhea. For the 3 measures specific to organisms, 2 are measured on a four point intensity scale, and one is a yes/no variable. The diarrhea variable is dichotomous (0-6 vs. 6+ days/year). In terms of rationale, I’d like more justification for these variables specifically in terms of how
they are seen to be related to child cognitive competence. Perhaps if the literature review had been more extensive, this formulation of variables would be apparent to the reader. However, speaking as a developmentalist, it is my assumption that these variables are supposed to provide an index of the burden of ill health experienced during the child’s early years. If so, I don’t understand the need for separate variables as opposed to some sort of combination which more accurately captures the overall burden of illness.

Likewise, I’d like to see further explication of the breastfeeding variables. The authors decided to create three groups – 0-7 days, 8-60 days, and 61+ days of breastfeeding. I’m not sure why these cut points were chosen, that is, whether these cut points are seen as important determinants of cognitive functioning. I suppose that depends on whether the authors see breastfeeding as having a direct effect or an indirect via reductions in infectious illness. Also, it would be helpful to have more information about how breastfeeding was determined. Was it any breastfeeding or more breastfeeding that formula? For example, a mother could have breastfed her infant for 9 months but only partially, with half or more of the infant’s nutrition coming from formula. Again, if the introduction were more extensive, then it might be apparent why the authors are including breastfeeding in a model predicting cognitive functioning. This rationale would, in turn, inform the construction of the breastfeeding variable.

Statistical analysis: The approach to the analysis was unusual in some regards, and I think there are more effective ways of presenting the information. Specifically, in the first column of Table 2, the authors present univariable analyses in the form of R2. A more informative analysis would be to present an intercorrelation matrix of the WPPSI-R scores and all of the covariates. Such a matrix would provide the identical information as what is presented in column 1 plus the added information of the intercorrelations of the predictors. The intercorrelations of the covariates themselves are important in trying to disentangle the different domains of factors and their influence on child cognitive functioning.

The additional information that could be provided by such an intercorrelation table would help inform model building that I believe would more effectively address the study questions. Although there is nothing technically wrong with the multivariate regression analyses presented in Step 3, I felt as though I was left with more questions than answers. If the purpose of the analysis was to disentangle these factors in terms of confounding, mediation, etc, then a more systematic approach to model building would have been more illuminating as opposed to presenting all covariates in a single multivariate model.

Although I think the message that the early psychosocial environment appears to be a more potent predictor of cognitive functioning than burden of illness, I believe there is, potentially, a significant limitation in the paper’s findings as currently presented. In the multivariate analysis presented in Table 3, early cognitive performance as measured by the Bayley is included as a covariate. This presents a problem for concluding that poor early health doesn’t compromise cognitive functioning at age 5. If early cognitive functioning mediates the association between burden of illness and later cognitive functioning, including the Bayley in the multivariate regression model would mask the association between illness and later cognitive functioning. Such would argue for a more systematic approach to model building.

A minor point: I think that some of the rows of coefficients in the Step 3 regression are shifted up. They are not lined up with the variables I think they are supposed to be for.