Author’s response to reviews

Title: Maternal Fish Consumption and Infant Birth Size and Gestation: New York State Angler Cohort Study

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PDF covering letter
Thank you for your review of our paper entitled “Maternal Fish Consumption and Infant Birth Size and Gestation: New York State Angler Cohort Study”. We have carefully reviewed the comments forwarded to us on April 21, 2003 and have revised our paper accordingly. Listed below is our detailed response to your comments. Please note that all authors have approved of the changes to the manuscript.

- *A priori* power calculations were considered to ensure that our fixed sample size would have statistical power for being able to detect a 100-200 gram difference in birth weight by fish consumption. Our calculations revealed nearly complete power (99-100%) to detect a 100-200 gram difference. Dr. Goldman’s comment is asking about the potential for a Type II error. To address this, we have performed *post hoc* calculations to determine the least detectable difference in birth weight between fish eaters and noneaters (assuming alpha 0.05, power 80%, our observed mean birth weights, and using the observed standard deviation of 550 grams). We are sufficiently powered to detect a 61-gram difference. Thus, we are confident that we did not miss a difference in birth weight as reported by earlier authors (160-190 grams).

- In response to your comments…
  1. We apologize for the confusion between the terminology sport fish and fish. Although most fish consumption from Lake Ontario and its tributaries pertains to consumption of sport fish (and we query participants about frequency of sport fish consumption in 1991), we cannot be sure that our duration (years eating fish) captured only sport fish. The wording of the question did not specify the species of sport fish consumed, as did the frequency of consumption variable for 1991. As a result, we have edited our text to say fish from Lake Ontario and its tributaries reflecting the manner in which the duration variable was ascertained.
  2. SI abbreviations have been used throughout the text.
  3. The Ponderal index can be calculated in kg/m or in g/cm³. We chose to do the latter as measurements of infants birth size are typically measured and recorded in grams and centimeters (not kilograms and meters as for adults). A second reference in addition to the one we cite in the paper supports our

4. The text of the abstract (originally, last 8 words) has been revised.

5. We have qualified our language to state that only some studies have reported a negative relation between contaminated fish consumption and birth size. See page 4, line 5. However, we prefer not to introduce the negative studies as yet because the first paragraph of the introduction is intended to present the positive experimental findings at one end and the controversial observational findings at the other end.

6. Yet again we have reviewed the Taylor paper and conclude from the authors’ text that the birth weight effect disappeared after controlling for gestation. We have edited this sentence to report this finding while trying not to emphasize it. See page 4, lines 9-11.

7. We have deleted text comparing fish consumption to other routes of exposure.

8. Cigarette smoking in this dataset was ascertained from medical records via maternal self-report. Thus, like Olsen et al., misclassification remains a possibility. We indeed considered smoking in our multivariate model rather than assessing it via stratification.

9. The text on contamination of fish oil supplements has been removed (former reference #25).

10. The sentence has been edited as suggested. See page 5, lines 6-7.

11. Fetal well-being has been changed to fetal development.

12. The text pertaining to IRB procedures has been reduced. See page 6, lines 17-18.

13. We have clarified our text to denote fish from Lake Ontario and its tributaries throughout the manuscript.

14. The suggested editorial change has been made. We note the mixtures of contaminants fish pose including its limitation on page 14, lines 18-19.

15. We have clarified which covariates came from which data source – questionnaire or hospital records. See pages 7-8. We had two questions on medical record abstraction forms – use of cigarettes or alcohol use during pregnancy (yes/no) and the amount (average number daily cigarettes or average number of monthly drinks during pregnancy). Only smoking significantly differed by fish consumption and was entered into the final multivariate models. We believe the amount of smoking is more informative than a simple dichotomy (yes/no) and modeled accordingly.

16. We have restricted the final analysis to infants without defects (excluded 467 infants with confirmed defects). We have included these new results in Table 4. However, our findings do not change – fish consumption is not associated with birth size. Moreover, the absence of a change in risk estimates for the highest category of the main exposure (fish consumption) is less than 10% suggesting the absence of effect modification or confounding.
As described to you in an earlier email in response to the reviewer’s suggestion to exclude children with birth defects, we believe the proper model for our study’s purpose is with birth defects treated as a covariate.

17. Our strategy for including/excluding covariates from the model is not unusual but consistent with an established approach as described by Hosmer & Lemeshow. See new reference 37 and page 10, lines 11-12. In addition, one needs to consider the magnitude of the effect only to be assured about covariate retention even if varying cut-points for selecting or retaining variables.

18. While we disagree about what constitutes new results versus available supporting evidence to aid in the interpretation of results, we have deleted the sentence as requested.

19. Technically, unmeasured confounders may not be the same as residual confounding in that the former could reflect a complete absence of control for a potential confounder while the latter could refer to a poorly measured confounder. We have edited the text per your request.

20. We have deleted much of this text (additional “results”) previously requested from a reviewer. We have simply said that recent eaters in 1991 were more likely to have reported lifetime duration than noneaters. The intent is to provide some support for the fact that eaters tended to remain eaters and that noneaters did not just start eating in 1991. See page 13, 9-11.

21. We have refrained from using the term “biologic” determinants from this paper. Rather, we refer to known determinants based upon the literature.

22. We have edited the description of the NYSACS’ reliability study as requested. However, we do not feel it is appropriate to describe this study as a sub-study within this paper as it pertains to the overall NYSACS sample. Our study focuses on a very select group of moms – giving birth in the 5 years before enrollment into the study. We simply do not know if we would obtain the same findings if we attempted to assess reliability of self reported fish consumption among this group of mothers. We prefer to entirely delete the text if it doesn’t fit with this paper rather than to describe a study for the full NYSACS. [The results of this study are currently being prepared as a separate paper focusing on measurement of fish consumption and are beyond the scope of the present study.]

23. This sentence has been deleted to avoid sending a confusing public health message.

24. The request text has been added as requested. All authors have read and approved the content of this revised paper. See page 16, line 15.

25. The reference in question has been verified. It is correct as noted.

26. The requested changes have been made to Table 1.

27. The same font has been used for Table 2.

28. The word “of” has been added as suggested to Tables 3 (and new Table 4).