Reviewer’s report

Title: Estimated postnatal p,p’-DDT and p,p’-DDE levels and body mass index at 42 months of age in a longitudinal study of Japanese children

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Reviewer: German Cano-Sancho

Reviewer's report:

Dear Editor, authors, I have read with attention the manuscript entitled 'Estimated postnatal p,p’-DDT and p,p’-DDE levels and body mass index at 42 months of age in a longitudinal study of Japanese children' and I sincerely believe the document provides high quality and valuable information. The objectives are clear and the methodology is adequate, the results are interesting, in the line of previous findings but adding evidence on the associations with postnatal exposures, scarcely explored.

I am providing some comments that may help to clarify some minor issues and help the readers to follow the narrative and results.

Introduction

Line 61, maybe some specific reference to the current exposure of Japanese mothers to DDT if available?

Page 6. Line 94 'Pan et al'

Materials and methods

Page 8 Line 141 The sentence "Mothers' breast milk was collected one month after delivery" seems not belonging to 'Data collection' sub-section. Maybe removed because this is repeated in 'Chemical analysis'?

Page 9 Line 153 - Indicate the freezing temperature

Page 10. Line 183. It's not clear how the children's milk consumption during breastfeeding was calculated/estimated? Nothing is said in 'Data collection' on this regard.

Page 11. DDT and DDE were detected in all samples? Management of left-censorship was required?

Page 11 Line 198. What software was used to run the simulations?
Page 12 Line 213. The DAG with the underlying causal structure assumed by the authors could be included in a supplemental figure.

Page 12 Line 215. Not clear why the authors have not considered some common confounding variables such as maternal age, birth weight and/or gestational age, maternal smoking. Not clear why the infant's sex was considered a confounder and not an effect modifier or the rationale behind including fish intake (potential antagonistic effects from PUFAs?, if so include some reference; if this is because it is considered a DDT driver, it should not be considered a confounder). Have the authors some information about the metabolic status of mothers beyond gestational diabetes?

In my opinion, energy and/or breastmilk macronutrient intake becomes a major (conceptual) confounder on child growth models. I wonder if the authors have considered using the concentration of breastmilk fat (proxy of energy content) as a potential confounder variable.

Maybe worth testing for non-linear associations (quantile regression, quadratic term or cubic splines)?

Page 12 Line 218-220. It seems that using WHO growth charts leads some conflicts when applied to Japanese population (PMID 29267170), maybe this should be commented in the light of missing national growth charts.

Page 12 Line 226-229. As written it's hard to understand the difference between Model 1 and 2.

Discussion

Some sentence could discuss the levels of DDTs found in breast milk in comparison with the levels currently found in Japan and previous studies on impact on child growth.

The present study further supports the potential of using of toxicokinetic models to extend the hypothesis-making through single-point biomarkers in epidemiology by integration of anthropometric and other individual data. The authors could add some sentence in the discussion on this regard.

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