Author's response to reviews

Title: Disparity of anaemia prevalence and associated factors among rural to urban migrant and the local children under two years old: a population based cross-sectional study in Pinghu, China

Authors:

shiyun hu (jiejoukeneng2000@gmail.com)
hui tan (htan@fudan.edu.cn)
xu qian (xqian@fudan.edu.cn)
hong jiang (h_jiang@fudan.edu.cn)
aiping peng (aipingpeng@163.com)
jianmei wu (1419390732@QQ.COM)
sufang guo (sguo@unicef.org)

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Author's response to reviews: see over
To BMC Editor and the reviewers

Dear reviewers,

Thank you very much for the comments of the reviewers and the editor. We have attempted to address all these comments and give a point-by-point response below. We copy the comments below and response using **bold font**.

Reviews' Comments to Author/s:

**Reviewer: 1 (Zumin Shi)**

This is an interesting and important study. It described the disparity of anemia between migrant and non-migrant children in an economically booming area in China. The paper is well written.

We thank the reviewer for the careful review of our manuscript. We valued the useful and relevant comments and response as below:

**Major**

1. In history, the Yangtze delta had the highest prevalence of anemia among adults in China (results from Chinese National Nutrition Surveys). However, the cause of anemia has not been well studied. Simply assuming iron deficiency anemia (due to low iron intake) is harmful.

We agreed the comments from the reviewer, and will avoid “simply assuming iron deficiency anemia (due to iron intake) was the only type of anemia”. Our study conducted in the Yangtze delta used to have the highest prevalence of anemia among adults in China, as reviewer commented. The prevalence of anemia among local and migrant children are significant different probably due to different determinants. Even the cause of anemia has not been well studied, local children originally from Yangtze delta shared same social, cultural and physical context with adults. Most migrants we investigated were from middle or western part of China. Two groups possibly have different of key determinants of anemia. With comparison of the anemia prevalence among migrant and local children and potential risk determinants, our study might help to set the foundation for further study.

We updated the interpretation of our findings and tried to address the questions from the reviewer in the manuscript.
a. Can the author comment more on the fact that no association between iron rich food consumption and anemia among local children, while a strong inverse association was found among migrant children?

Thanks for the suggestion. This was a very important finding in our study which was highlighted by the reviewer. Migrant and local children probably had different risk factors. To add comments on this fact, we had included the contents in our discussion section. “In our study we find the migrant child who was breastfed at aged 6 months and lack of iron supplements was significant associated with anemia prevalence. These two associated factors are not significant in local children suggesting that breastfeeding and lack of iron supplements are not necessarily increasing the risk of childhood anemia unless over reliance on it for nutrition after aged 6 months.”; “For local children, iron supplement probably not played important role as it to migrant children. Instead, other micronutrient factors, such as Vitamin B2 intake [38] probably were more important for local children.”

b. Is iron-fortified supplement the best way of preventing anemia in this region especially for the local children? Using data from Jiangsu province, China, we found that among adults when riboflavin intake is adequate, iron intake does not make a difference regarding the risk of anemia (Shi Z, Zhen S, Wittert GA, et al. Inadequate riboflavin intake and anemia risk in a Chinese population: five-year follow up of the Jiangsu nutrition study. PLoS One. 2014;9:e88862.).

Thanks for the question. Again, this is a very important point. As we mentioned before, we will avoid “simply assuming iron deficiency anemia (due to iron intake) was the only type of anemia”. Of course, we agreed that iron-fortified supplement is not the best way of preventing anemia in this region especially for the local children. As the research subjects include migrant and local children, the recommendation should cover both groups. Based on our study, we conclude the better recommendation could be to improve diversity of food and frequency of meals of children.

2. Among the migrant children, is there any difference of the prevalence of anemia regarding their birth place (or where their parents came from)?

Thanks for the question. There is a difference of the anemia prevalence among the migrant children regarding where their parents came from. Migrant children whose parents are from type VI areas (which indicated less developed provinces of China) had higher prevalence of anemia. We tried to focus our analysis on the comparison between the migrant and local children on potential factors. As this is not an influencing factor of local children, we didn’t report this finding in this paper.
It is pitiful more detailed dietary information is not available. In our study, the standardized methodology developed and recommended by WHO were used. This simplified dietary survey is specially designed for breastfeeding and feeding for children at their early life (aged 6-23 months). Only information on the types of food and frequency of meal were collected. We don’t know quantity of each kind of foods children had in the previous day which makes impossible to present iron and micronutrients intake.

Minor
1. Please check reference 20 (page 4). It is not correct.

Thanks for the reminding. We had corrected the mistake.

2. Definition of MMF, MDD (Table 1) should be described in the text (method section). The definition of MMF is difficult to understand.

Thank you for the suggestion. We moved all the definition of indicators from Table 1 to measurement part in our method section. At the same time, all the definition and coding of the variable were rewritten in details. To make it easy to be understood, we had added the description in definition of MMF. “MMF is proportion of breastfed and non-breastfed children 6–23 months of age who receive solid, semi-solid, or soft foods (but also including milk feeds for non-breastfed children) the minimum number of times or more. For breastfed children the minimum number of times varies with age (two times if aged 6-8 months and three times if aged 9-23 months). For non-breastfed children, the minimum number of times does not vary by age (four times for all children aged 6-23 months)”. (WHO. Indicators for assessing infant and young child feeding practices: Part 1 Definitions. World Health Organization 2008. Available at: http://whqlibdoc.who.int/publications/2010/9789241599290_eng.pdf [Accessed 25 October 2011]).

3. What food can be classified as iron rich food in the study? Please give some examples. If the intake of iron is presented, this information is not necessary.

Thank you for the question. In our study, iron-rich foods was defined as recommended by WHO. (WHO. Indicators for assessing infant and young child feeding practices: Part 1 Definitions. World Health Organization 2008. Available at: http://whqlibdoc.who.int/publications/2010/9789241599290_eng.pdf [Accessed 25 October 2011]). It was define as flesh foods, including any organ meats such as liver, kidney, heart, or others; any meat, such as beef, pork, lamb, goat, chicken, or duck; fresh or dried fish, shellfish or seafood. Iron-fortified foods include
commercially fortified foods specially designed for infants and young children which contain iron, or foods fortified in the home with a micronutrient powder containing iron or a lipid-based nutrient supplement containing iron. This classification is based on WHO recommendation. The investigated items were adapted in research conducted in western rural area of China (Feng Yao et al. Study of the infant and young child feeding practice in some areas of China and a cross-country comparison.). We had added this detailed explanation in the method parts as suggested by the reviewer.

The quantity of foods children eaten in the previous day are not collected. So the intake of iron or any other micronutrients are unavailable. Based on the survey, only the binary variable (consume iron-rich and/or iron fortified foods) could be calculated.

4. Page 7. I do not think that references 17-18, 20 are researches based on clinical resource data.

Yes. The reviewer is right. Those researches were not based on clinical resource data. We had deleted the description “Previous studies on anaemia prevalence research among migrant children in China were mostly from clinical resource data [17-18, 20]” in our discussion section.

**Reviewer: 2** (Marly A Cardoso)

We thank the reviewer for the valuable comments and response for the points as below:

Major Compulsory Revision

The main objective of the study is "To understand the difference of anemia status and its associated factors among rural to urban migrant and the local child under 2 years old in a small city in China". Using a community-based, cross-sectional survey, the authors described comparisons of associated factors for anaemia between rural and urban migrant. The manuscript in this version needs improvements in writing and data analysis.

1) Writing

Thanks for the comments. To take the reviewer’s advice, we had invited one English native speaker help the review and edit of our manuscript. We will use professional editing company to help on our writing if further edit are still needed.
2) Data analysis

Thanks the reviewers for the comments on our data analysis. We had made changes as suggested as below.

a. The use of "risk factors" for a cross-sectional study is not adequate.

The reviewer is right. We will take your advice and consistently use “associated factors” instead of “risk factors” in our paper.

b. Data analysis must be revised to avoid residual confounding and improve the interpretation of findings.

Thanks for the comments. To avoid residual confounding, first, stratification strategy had been used when we designed the study. We had generated two samples in the same area, one for migrant children and one for local children (two distinct sample frames were used in our study) considering the possible difference between migrants and local people on social economic status, demographic characters, feeding practices and other patterns. Second, multivariate regression models were constructed separately using database of migrant or local children. It could avoid confounding factors due to unbalanced characters between two groups. We do agree to the reviewer that we need improve our data analysis. To avoid the possible effect of colinearity between variable of breastfeed, iron consuming and of other feeding practices (MDD and MMF), we constructed model 2 and model 3 for migrant and local respectively. We also improved the interpretation of our findings. As suggested by reviewer, we emphasized the discussion of the possible reasons which contributed to the difference of anemia between migrant and local children.

c. Epidemiological procedures for selection of variables to be retained in multiple models should be revised and described in the methods.

Thanks for the suggestions. The selection of variables to be retained in multiple models is based on possible risk factors listed in the literatures. We described the procedures in the Statistical analysis section of methods as “Associations between potential risk factors and outcomes were first evaluated by univariate logistic regression. Multivariate regression model were then developed. Either significant variables in univariate logistic regression (Odds Ratio>1) or variable was identified as risk factors of childhood anemia in the literature [28-30] were retained. Three multiple regression analyses were then developed and performed respectively among migrant and local children”.

d. As the migrant children differ from the local ones by sample size, age and
economic variables, any comparison among them should take into account those differences.

Thanks for the suggestion. The comparison among two groups included two parts in our study. One is difference of anemia prevalence among migrant and local children. We used weighted age of child to report the prevalence. The Second we compared associated factors of anemia among two groups. Multivariate regressions were used to identify associated factors for migrant children and for local children respectively.

e. The use of prevalence rates for this comparison would be more appropriate for many reasons (high prevalence rates and cross-sectional design).

Thanks for the suggestion. We had used the term “prevalence rate of anemia” instead of “proportion of anemia” in univariate analysis (table 4).

3) The authors should be careful to conclude that breastfeeding over 6 mo and not using fortified foods are "risk factors" for anemia since these variables could be considered a proxy of poor complementary feeding practices.

Thanks the reviewer for the advice. We agreed that it’s not proper to conclude “breastfeeding over 6 mo and not using fortified foods are ‘risk factors’ for anemia”. We had changed the statement in our discussion: first we will use “associated factors” instead of “risk factors”; second, in the discussion sections, we made the statement as “in our study we find the migrant child who was breastfed at aged 6 months and lack of iron supplements is significant associated with anemia prevalence. These two associated factors are not significant in local children suggesting that breastfeeding and lack of iron supplements are not necessarily increasing the risk of childhood anemia unless over reliance on it for nutrition after aged 6 months. Higher proportion of breastfeeding and lower of complementary diversity hint there might be a later introduction and lower quality of complementary foods in migrant children. For local children, iron supplement probably not played important role as it to migrant children. Instead, other micronutrient factors, such as Vitamin B2 intake [38] probably were more important for local children”. In the conclusion, we delete the statement “that breastfeeding over 6 mo and not using fortified foods are "risk factors" for anemia”. Instead, we conclude “disadvantageous socio-demographic characteristics and inappropriate feeding practices were highly associated with anemia in migrant and local children aged of 6 to 23 months”.

Thus, the title, data analysis, discussion and conclusions of this version need to be revised.
Thanks for the comments. We followed the advice from reviewer and had changed title of our paper from “Disparity of anaemia prevalence and risk factors among rural to urban migrant and the local children under two years old: a population based cross-sectional study in Pinghu, China” to “Disparity of anemia prevalence and associated factors among rural to urban migrant and the local children under two years old: a population based cross-sectional study in Pinghu, China”. At the same time, we changed title, data analysis, discussion and conclusions to make all description to be consistent as response of the questions from reviewer as above.

Reviewer: 3(Johari Surin)

We thank the reviewer for comments on our writing and we response as below:

1. Minor Essential Revisions.
   1) While the paper merits a significant finding, the text has to be written in a manner that it should be free from typographical errors.

   Thank you for the advice. We carefully reviewed our manuscript and corrected all identified typographical and grammar errors. As you may notice in our revised version, we had made lots of correction in our manuscript.

   2) In some parts of the discussion, the sentences are poorly constructed.

   Thank you. To take your advice, as reprocessed our data analysis as suggested by other reviewer, we re-wrote and updated our discussion session. The details include: we had re-organized those poorly constructed sentences; we had deleted the sessions which is improper; one outside reviewer was invited to help the editing of the manuscript. As to make our expression more clear, we also tried to shorten the sentences.

   Again, all those review points are very helpful and we are happy to response your further comments accordingly when we receive your feedbacks.

   Yours sincerely,

   Shiyun Hu, on behalf of the co-authors