Reviewer's report

**Title:** Exposure Assessment of Dietary Cadmium: Findings from Shanghainese over 35 years, China

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**Reviewer:** Andrea Amici

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Exposure Assessment of Dietary Cadmium: Findings from Shanghainese over 35 years, China by Ping He et al.

Is the question posed by the authors well defined?

1) The Authors consider the “dietary” cadmium exposure in citizens aged over 35 years in Shanghai, China (also including smoke; is this a dietary source? In the background, they indicate “non-occupational” exposure may be this definition is more appropriate. In the abstract-conclusion is reported “ … as a result of recent total cadmium exposure”. In the discussion, they indicate, “This study provided a detailed assessment of the environmental cadmium exposure especially…. I think that the purpose is confuse!).

2) The main question the AA wish to give an answer is: are adults in Shanghai exposed to dietary/non occupational cadmium above the tolerable intake? As a general consideration, they give an answer to the question but criticism can be made for several details that are listed below.

Are the methods appropriate and well described?

3) The methods are generally/probably adequate but the description is rather confuse and not exhaustive. Just for example:

- they did not explicit formally the model trough with the probabilistic estimation was performed
- the method used to determine the biomarker “creatine” has not been described in the M&M section
- concerning cadmium determination “cadmium (BCd) were tested using the national standard methods of WS/T 32-1996 and WS/T 32-1996” I don’t know this methods, I suppose are adequate but a wide literature is available worldwide that should be used to validate the method.
- the authors did not provide both limit of detection and limit of quantification of the AAS methods they used for the Cd quantification in foodstuffs and urine
- uncensored data treatment has not been described and it is not clear if it was performed (see comment 12 hereafter)

4) In more details the sample population is indicated (209 peoples over 35 years old) but was chosen by “convenience”.
5) The data “on food intake was collected using the food frequency questionnaire (FFQ). The FFQ was quote from National Nutrition Survey and verified to have a good reliability and validity”. More details on the questionnaire should be provided and/or literature references.

6) Concerning the cadmium level in foods etc. “A total of 1680 food items cadmium were detected and combined with the intake of the inhabitants”, this part should be more carefully described and should be reported in methods instead of in results.

7) The Monte Carlo simulation model is adequate (but without references); it was described by Metropolis N. and Ulam S (1949 - The Monte Carlo method. Journal American Statistician Association 44:335–341).

8) This method was used in heavy metals exposure determination by Danieli P.P., Serrani F., Primi R., Ponzetta M. P., Ronchi B., Amici A., (2012. Cadmium, lead and chromium in large game: a local scale exposure assessment for hunters consuming meat and liver of wild boar. Arch. Environ. Con. Tox. 63:612-627), and in other studies. All of them have been ignored by the authors.

9) Concerning the number of peoples included in the study criticism could be raised; are 209 peoples representative of 17-20 million inhabitants of the area? For instance, Danieli et al 2012 (reported above) included in their study 107 questionnaires (250 people) on a total of 4000 hunters. de Winter–Sorkina R, Bakker MI, van Donkersgoed G, van Klaveren JD (2003 - Dietary intake of heavy metals (cadmium, lead and mercury) by the Dutch population. Report no. 2003.016. Rikilt Institute of Food Safety, Bilthoven, Netherlands) included 6250 peoples, on a total of 6 million of inhabitants.

Are the data sound?

10) The data are interesting as they point out on cadmium exposure for people of a specific area. I don’t know if data on that area are available in literature, but the results are not transferable to other areas and nothing new is reported in the paper. The scientific interest is limited.

11) In addition, no data are provided for children and it is well known that cadmium burden is particularly important before 12 years of age. The information concerning children is considered necessary when analysing cadmium exposure. In addition the information on other heavy metals are available by the same experimental schedule only with supplementary analysis, why other heavy metals have not been considered?

Does the manuscript adhere to the relevant standards for reporting and data deposition?

12) In my opinion the authors should refer to international guideline on exposure/risk assessment data reporting (e.g., ATSDR, EFSA, other)

Are the discussion and conclusions well balanced and adequately supported by the data?

13) Although there are several inconsistencies on the aims and the conclusions
the authors present data and discuss the topic reported in the title. However, criticism can be made on the deficiencies reported above. A vast literature exists on this topic and authors seem not to have deeply considered existing knowledge when discussing the results.

Are limitations of the work clearly stated?
14) Some limits are reported, but several limits reported in the revision should be considered.

Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
15) No, many references are omitted mainly concerning methods.

Do the title and abstract accurately convey what has been found?
16) No, The Authors consider dietary cadmium exposure in citizens aged over 35 years in Shanghai, China (also including smoke; is this a dietary source? In the background they indicate “non-occupational” exposure may be this definition is more appropriate. In the abstract-conclusion is reported “there was no greater health risk among adult residents in Shanghai, China as a result of recent total cadmium exposure”. I think that the purpose is confuse!)

Is the writing acceptable?
17) A part some typewriting mistakes (e.g. repeating/repeating) English is quite good but a mother tongue revision or professional editing is advisable.

As a general consideration the paper provide a very little contribute to scientific knowledge and should be rejected.

All the suggestions should be considered as Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal.

Quality of written English: Needs some language corrections before being published.

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.
Andrea Amici