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Dear Dr Chen

Subject: UNSCEAR Report 2020/21 Volume II Annex B Attachment A-9.

I have read with great interest Attachment A-9, Annex B of UNSCEAR2020/2021 Report, where I found a few questionable statements and some inconsistencies. Having discussed the issues with various friends and colleagues, I could not clear my mind of concerns, and thus I write to your Committee directly.

The matters significantly affect the conclusion of the report, and they will subsequently have significant repercussions on Japanese official policy-making and policy decisions in central government, local government, as well as the judicial sector, and thus will impact the lives of many Japanese citizens. For this reason, I would be grateful if you could kindly provide me with answers to my questions below.

Furthermore, noting that the mandate of UNSCEAR is to assess and report levels and effects of exposure to ionizing radiation, and particularly governments and organizations throughout the world rely on the Committee's estimates as the scientific basis for evaluating radiation risk and for establishing protective measures, I strongly believe that it is important to correct or explain any misunderstanding or concerns.

For reasons of transparency and good faith in scientific and academic exchanges of view, I have posted this letter as an Open Letter to UNSCEAR on the public domain website, UNSCEAR2020/21 report-verification-networks (https://www.unscear2020report-verification.net); I will of course post your reply to the same website, as many people sincerely wish to know your response. When I find similar inconsistencies in any academic journals, I normally write to the editor to verify the facts or write a letter that points out issues and submit it to the journal, but in the case of UNSCEAR's report, I cannot do that, so I ask you directly.

Therefore, I am hoping that you, or other scientific colleagues concerned, can look into the points I raise, and answer my questions.

1. In Paragraph 4, it is written "Specifically, concentrations of different radionuclides in the air in the early stage of the FDNPS accident have been estimated at several monitoring posts in Fukushima Prefecture from pulse height distributions measured with sodium iodide scintillation detectors [Hirayama et al., 2015; Moriizumi et al., 2019; Terasaka et al., 2016]." This description contradicts what is shown in Table A-9.1, "Summary of measurements of concentrations of radionuclides in air over Japan", where it is shown that Moriizumi's paper is for 21 monitoring stations in Ibaraki Prefecture, and Terasaka's Paper for 6 monitoring stations also in Ibaraki Prefecture. Would you please clarify which is the correct description? If the description in Table A-9.1 is correct, it is equivalent to that the description in this paragraph misleads readers by conveying a false impression that new data other than that by Hirayama really exist in Fukushima Prefecture.

2. In Paragraph 11, it is written "Figures A-9.V and A-9.VI show a comparison of the ATDM results for the deposition densities of ^{137}Cs and ^{131}I with the measurements from JAEA EMDB[Saito and Onda, 2015]"; however, the units of the vertical and the horizontal axes of Figure A-9.V are both Bq/m^3. These should be Bq/m^2. Are these typographical errors?

3. Even if these are simple mistakes, there still remains a serious issue. It is shown by the measurements done by MEXT that the deposition density of ^{131}I is 10 to 30 times larger than that of ^{137}Cs in Fukushima. However, the deposition density of ^{131}I in Figure A-9.V (right side) is about one three-hundredth (1/300) of the deposition density of ^{137}Cs in Figure A-9.V (left side). I would appreciate it very much if the Committee could explain the reason why this is so.

4. In Paragraph 17, it is written "Bulk deposition velocities – as shown in Figures A-9.IX and A-9.X – were derived from the results of the ATDM local model by calculating the ratio of deposition density to time-integrated concentration in air." In a paper written by Amano et al. [1] , which is one of the references listed in Appendix A-9, it is written "Deposition velocities during 14-17 March, when there was no precipitation, were around 0.2 – 0.3 cm/s for ^{134}Cs and ^{137}Cs, and 0.1 – 0.2 cm/s for ^{131}I. Deposition velocities during 21-24 March, when there was precipitation (38 mm), were around 1 – 14 cm/s for ^{134}Cs and ^{137}Cs, and 0.4 – 3 cm/s for ^{131}I. These differences between radioactive Cs and ^{131}I were slight but measurable under both wet and dry conditions." (Note: In this quotation I changed m s^{-1} in the paper to cm/s in order to make it easier for readers of my letter to compare the values in the paper with those of Attachment A-9.) Amano paper shows that the bulk deposition velocity of ^{131}I is usually somewhat smaller than that of ^{137}Cs at the same location and at the same time, but it is very unusual if the bulk deposition velocity of ^{131}I is as small as one tenth of the deposition velocity of ^{137}Cs. With respect to this, let me ask the Committee to explain why the UNSCEAR report says that the bulk deposition velocity of ^{131}I shown in Figure A-9.X is extremely small (between 10^{-5} cm/s and several times of 10^{-3} cm/s), compared to that of ^{137}Cs (between a few times 10^{-2} cm/s and 10^2 cm/s) shown in Figure A-9. IX.

5. Also in Paragraph 17, the report states that "The results of the estimation of concentration of radionuclides in air using the deposition scaling approach are illustrated in the following four figures. Concentrations of radionuclides in air – as shown in figures A-9.VII and A-9.VIII for ^{137}Cs and ^{131}I, respectively – were derived from the deposition measurement data from the JAEA EMDB data sets [Saito and Onda, 2015], in particular, the 2,200-location soil deposition data set for ^{137}Cs (see attachments A-6 to A-8)." I read the concentration in air for ^{131}I from Figure A-9.VIII and the deposition velocity of Fukushima-city from Figure A-9.X. They are about 10^{8} Bq・s/m^{3} and 10^{-3} cm/s, respectively. Multiplying these two values, I get 10^{3} Bq/m^{2} as a deposition density, which is a few thousand times smaller than the measured deposition density of the city, about 2 times 10^{6} Bq/m^{2}. I would like to ask the Committee to explain why this seemingly absurd situation has happened. Let me emphasize that it is clearly written in paragraph 15 "The 'bulk deposition velocities' were estimated from the ATDM results for the radionuclides in the plume and deposited on the ground as a function of time and location, using the local model of Terada et al. (with a spatial resolution of 1 × 1 km) within Fukushima Prefecture." Above mentioned absurd situation should not have happened.

In summary, I believe that some of the Figures in the Attachment are inconsistent and misleading. Especially I cannot get any reasonable values of the deposition velocities of ^{131}I at all, even though these values are of vital importance to the estimation of the absorbed doses to thyroid by inhalation. I would request that the intermediate data on which the Committee made these Figures be opened to the public, so that people who have much interest in the issues can verify the facts for themselves.

[1] Amano, H., M. Akiyama, B. Chunlei et al. Radiation measurements in the Chiba Metropolitan Area and radiological aspects of fallout from the Fukushima Dai-ichi Nuclear Power Plants accident. J Environ Radioact 111: 42-52 (2012).

Sincerely yours,

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