Biogeosciences Discussions

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Comment on bg-2020-491

Anonymous Referee #1

Referee comment on "Impact of typhoons on particulate and dissolved ^{137}Cs activities in seawater off the Fukushima Prefecture: results from the SOSO 5 Rivers cruise (October 2014)" by Michio Aoyama et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-491-RC1, 2021

This work presents radiocesium in water and particles collected from the areas off the mouths of five rivers north of Fukushima Dai-ichi Nuclear Power Plant (FDNPP1) shortly after the passage of two typhoons. As the decrease of local atmospheric deposition and the direct discharge of radiocesium from the reactor, rive runoff input of radiocesium into the Pacific Ocean might become an important contribution. However, data presented in work did not give a clear picture about the impact of typhoon on particulate and dissolved 137Cs activities in seawater off the Fukushima Prefecture. The quality of writing, data presentation and results discussion do not meet the criteria to be accepted as a research article on Biogeosciences.

General comments:

- The quality of figures in this work are very poor. Some of the figure are hardly to see the values, and the trends as the authors described in the context, especially on paper print. Some of the figures can be plotted in a different way to make them more illustrative.
- Novelty and Objective. It is not very clear in the introduction what is the scientific question in this work? The novelty is not highlighted either, what is new in this study? Why the authors carried out the investigation to exam the radiocesium in water and particles? The sentence in line 86-87 is very confusing. The authors mentioned 'to understand riverine fluxes in the coastal region' but in the discussion on the 'river fluxes' and its connection to precipitation is very weak. It is still not clear how significant these 'river fluxes' contribute the total inventory of radiocesium in the study area.
- The definition of organic particle. The authors used concentrated nitric acid and hydrogen peroxide to process the samples and claim the obtained radiocesium was associated to organic form. I disagree to term this fraction as 'organic particle', because 1) the experiment details were not clear. What was the ratio between sample and reagents, what temperature was applied? 2) There was no data/literature to support the obtained radiocesium was only associated to organic matter. As concentrated nitric acid is a strong acid, it can easily dissolve inorganic radiocesium absorbed on the

particle surface and can also extract radiocesium incorporated to the mineral lattices. Even though the authors reported the results of radioacesium for this 'organic particle' fraction, the interpretation and discussion on the results is not in depth. Why the authors analyzed radiocesium in 'organic particles'? Why 137Cs activities concentration in 'organic particles' were one or two order lower than dissolved 137Cs (line 173), while 137Cs in all particles were higher than dissolved 137Cs (line 169)? How these results are connected to typhoon?

- 137Cs/34Cs ratio in particles. The authors in section 4.2 discuss the source term of 137Cs based on the 137Cs/134Cs in seawater, but why the authors did not discuss the obtained results for 137Cs/134Cs in particles? The authors claim the 137Cs/134Cs ratios in particle do not changes with distances but what are these values obtained in this work? Do they agree with the estimated ratios for FDNPP? Again, how typhoon impact the distribution of 137Cs/134Cs ratios?
- API calculation. I am not convinced by the API calculation approach. As the definition of K is not clear, what is the difference between K1, K2, …Kn, Why the authors set K to 1? Besides, from Fig. 17, the maximum value for R is about 0.5, meaning the R2=0.25. Therefore, none of these correlations is significant. I do not think it make much sense to perform such correlation analysis. Direct use of the metrological data (e.g., rainfall) would be sufficient to support the conclusion that high 137Cs activity concentrations in September-October were connected to the typhoon events.
- The language shall be improved thoroughly, as many descriptions are not concise. There are also repeated contexts.

Specific comments:

- Line 37: please give uncertainties for these 137Cs/134Cs ratios.
- Line 40: why the measurements were inaccurate? Please explain.
- Line 43-49: these are earlier findings about the different isotopic signatures on the particles affected by different units of FDNPP. The summary should be shortened to focus on the key point for this work.
- Line 62-74. These are also very tedious literature review. It is better to extract the most important findings which are relevant to this work.
- Line 157-159: these values are not reflected anywhere in the figures or table. These sentences should also be more concise.
- Line 162-165. These are somehow repeated sentences.
- Line 187-189: it is a very long sentence, please modify. Besides, it is not clear whether the findings about organic particles in this work agree with Naulier et al.
- The conclusion should be re-written. The conclusion shall not compile all the detailed results obtained from the work. It shall be more informative to let the reader understand the main points of the work and provides the reader with a sense of closure on the topic.
- 1. It is better to combine two figures, with one larger scale map indicating geographical positon and the current circulation pattern of the study area, and another details the sampling stations. It better to remove some of the annotations to the figure caption or use smaller fonts so it does not look so squeezed, and to use different symbols to mark different rivers so the reader can easily follow.6
- 2 and Fig. 9, they are very unclear. Why the authors do not use Ocean Data View to present the distribution of radio cesium on the surface water?
- Fig 3-7, the symbols are too small to be distinguished. The authors pull data for all stations in these figures, very hard to extract useful information. Why the authors do not use different symbols to present different rivers, so the reader can visualize the

variations.

- 12-16 and Fig.19, I do not see the meaning for presenting these figures. All the figure do not show any statistical analysis data, based on the visual inspection, I do not see any of them have significant correlation. I would suggest remove them.
- 18. It is very hard to see the color change, especially after 2014. I suggest either delete the color bar data before 2013, or add another zoom-in figure for the data after 2014.

Technical corrections.

- Line 41: 'small but significant', this is a contradictory expression. Please modify.
- Lien 55: 'primarily' should be 'primary'.
- Line 113: 'disillusion' should be 'dissolution'.
- Line 127-138. There is an overlap with the description in 'Data availability'.
- Line 150 and 155: please delete repeated data information 'xxxdoi:xxxxx', only keep the reference is sufficient.