Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC2
https://doi.org/10.5194/bg-2021-10-RC2, 2021

Comment on bg-2021-10

Anonymous Referee #2

Referee comment on "Tritium activity concentration and behaviour in coastal regions of Fukushima in 2014" by Michio Aoyama et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-10-RC2, 2021

Review of Aoyoma et al., "Tritium activity concentration and behaviour in coastal regions of Fukushima in 2014."

This was a difficult paper to read/review due to the lack of flow and logical sequencing in the paper. Moreover, the grammar and writing style made it difficult to interpret the precise intent of the authors. The paper does not appear to have been proof-read carefully – there are duplicate redundant sentences (e.g., lines 130-132, 138-139) and figure labels do not always match what is in the figure caption (e.g., Figure 4).

The data, 3H and 137Cs data collected over 2014-2018 from the coastal waters of eastern Japan near the Fukushima Nuclear Power Plant and two sites further south, seem to be looking for a home. Although *Biogeosciences* hosted a special issue dedicated to the Fukushima event, it is unclear to me if *Biogeosciences* is the 'best' journal for these newer monitoring style data compared to a journal more specific to radionuclides and radiochemistry (e.g., *Journal of Environmental Radioactivity, Journal of Radioanalytical and Nuclear Chemistry*).

The authors expend a good deal of writing for background on atmospheric weapons testing derived tritium (^{3}H). I do not believe this was a good use of space in the manuscript. Of the ^{3}H produced by weapons testing, and if we use 1963 as the initial time zero, less than ~6% of weapons testing ^{3}H is still in the environment (atmosphere, terrestrial, ocean reservoirs). The authors could significantly shorten and tighten the introduction to simply state the background ^{3}H (and ^{137}Cs) in the western subtropical Pacific, that controlled releases from FNPP elevated coastal water ^{3}H prior to the earthquake/tsunami induced cataclysm, and then go straight into line 120: "In this paper, we present…"

The authors could also state the purpose or what they were looking to

explore/understand. Were they looking to better understand/constrain the relative influences of FNPP impacted submarine groundwater discharge versus surface (river) input of ^{3}H and ^{137}Cs on coastal water concentrations?

Does the different physical chemistry of cesium and tritium lead to different input functions in the coastal waters (eg., cesium will desorb off particles when it gets to higher salinity)? This is particularly relevant with regards to submarine groundwater discharge which is a significant source of 137Cs (e.g., Sanial et al., 2017 www.pnas.org/cgi/doi/10.1073/pnas.1708659114) post direct discharge (eg., Buesseler et al, 2012).

Key takeaways:

From the TEPCO 56N canal data, it is pretty clear that FNPP is (still) a source of 3H, regardless of the sensitivity of their methods being limited to > 1650Bq-m-3.

The Aoyoma et al., additional data capture the input of 3H and 137Cs into coastal waters.

One of the most intriguing aspects of the data is the 3H/137Cs ratio that has varied post direct discharge in 2011 to the newer data. The authors do not provide a credible discussion/interpretation of this observation.

What are the uncertainties on the flux (input) estimates? Are there any 'real' differences in the estimates provided in eg., table 3?