

2nd Review of the manuscript OS-2017-90 entitled “Rapid transport of FNPP1-derived radiocaesium from subtropical mode water in the western North Pacific Ocean to the Sea of Japan” by Inomata et al.

I thank the authors for their consideration of my previous comments. I acknowledge that they have made some efforts to improve the paper and to take into account some of my comments. However, I think the revised manuscript is not yet acceptable, it still contains mistakes and inconsistencies. I also found that some important results are kind of lost within “vague” discussion. I recommend minor revision; please be careful, extensive, rigorous and to the point this time.

Major points:

- The authors may have improved the English of the revised manuscript but not of the response letter which contains numerous mistakes and is, again, very difficult to read and understand. Some co-authors of this paper have extensive experience in publishing scientific papers in English; I am puzzled by the fact they have co-signed such English text (e.g. did they really read that paper and that response letter at least once?).
- I am convinced by the authors that the Cs137 signal in the SOJ first originates at the subsurface (e.g. 140 and 200 m) and then propagate to the surface, thus supporting their hypothesis of “mode water transport”. However, I am not convinced by the first entrance through the Tsushima strait: indeed, on Fig. 3 c/d and Fig. 5 a/b, one can see that the subsurface peak of Cs137 occurred first (mid 2013) at station 105-11 (within the SOJ) and 1 year later at station 304-01 (ECS, e.g. upstream). Why?
- The “blob” of high Cs137 (8-12 Bq/m³) reported on Fig. 6a, b: could it be related to “exceptional sampling sites”; e.g. sampling within the core of an eddy that transport Cs137? (e.g. Prants et al. 2017 Ocean Sci. Discuss., doi:10.5194/os-2016-103, 2017).
- Please read better section 3.3. “Vertical distribution of Cs-137 and interior pathways” of Rossi et al. 2013 and please summarize their findings better in your introduction. The corrigendum concerns only the concentrations simulated (1 order of magnitude lower than reported), not the validity of the interior (and surface) pathways and subduction process.
- Figures are still of poor quality. For instance, labels have been misplaced in Fig. 7; labels are not readable on Fig. 8; etc...
- Fig. 7: those latitude-time plots are usually called “Hovmoller diagram”.
- Title of section 3.2 (p 7): upstream sites are SJPN and ECS, right?
- Reference incomplete p 10 line 41.
- Abstract line 16-18: a sentence was repeated.