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Ocean Science Discussions

Interactive comment on "Tracer distribution in the Pacific Ocean following a release off Japan – what does an oceanic general circulation model tell us?" *by* H. Dietze and I. Kriest

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We thank referee 2 for the very helpful and constructive comments. However, because of the long time elapsed since the accident, we unfortunately felt that we are a bit late for following the suggestion to give model-based advice on where to carry out surveys. This delay was mainly caused by the computational cost of the additional simulations we felt were necessary. On the other side, we have now gained access to data which enables us to to what we initially proposed and we reorganized the paper accordingly: (1) Focus is now on the cross-shelf exchange of tracers - here: 137Cs - in circulation models. (2) Instead of using an artificial tracer, we now simulate 137Cs inputs, as

given by IRSN (2011). (3) For the sake of brevity, we have omitted the discussion of the potential impact of biota on the tracer distribution.

Reviewer: 1. The authors state that, after the Fukushima-Daiichi accident, "a comprehensive set of 137Cs measurements could be a unique opportunity to evaluate and advance general circulation models". They also discuss some of the general uncertainties on scavenging of radioactive material that could also be addressed by an observational programme of the (coastal and offshore) region affected by the accident. Nevertheless, in the paper there are no explicit suggestions for these much needed in situ observations. What are the specific recommendations that follow from the preliminary model analysis performed in this paper? Which is the region that need to be sampled? In which temporal relation with the planktonic bloom? What are the the key physical and biological measurements that will help to solve the uncertainties? What is the minimal spatial and temporal resolution needed? For how long the survey programme should go on? Considering that the main strength of the paper resides in my view in promoting some discussion among the different oceanographic communities, the discussion should be - substantially – enlarged and some recommendations for further studies added. The authors can focus on the questions I suggested above, or on some other issues that they judge more relevant or more at reach.

Authors:

Now that we have gained access to 137Cs measurements and estimates of the 137Cs release to the sea we have been able to actually cary out what we proposed in the original manuscript, that is: Evaluate horizontal mixing and the cross-shelf transport as simulated in an eddy-resolving general ocean circulation model. We hope that the analysis presented in the revised manuscript has lost its preliminary character.

Reviewer: 2. Related to the issue above, it is not completely clear to me how much the results presented in the paper can be compared to future in situ measurements. At p. 1445 the authors write that the results are not directly comparable. How much and in

which sense indirectly? The explanation at L 20-25 is not clear to me, and should be substantially extended.

Authors: Now that more data is available we are able to present actual concentrations rather than admittedly awkward relative concentrations.

Reviewer: Abstract: "evaluate and advance the evaluation": please find a better wording.

Authors: Phrase is omitted.

Reviewer: P1442 L 19-20: citation of N. Fisher : please indicate the timescale that N. Fisher referred to when talking about dilution, otherwise the sentence is meaningless.

Authors: We agree. We omitted the reference to Fisher, also because the focus of the manuscript has changed.

Reviewer: P1444 L24-25: The spinup of the model covers 1993-1998 and the integrations used starts from 1993. Hence at the beginning of the spin up. The authors should analyze the model after the spin up. Or am I missing something?

Authors: The explanation reads now: " After a spinup of 5 years, covering the period 1993 to 1998, the clock assigning the model forcing is reset to 1993 and the model is integrated for another 5 years."

Reviewer: P1445 L6: Fukushim =>Fukushima

Authors: corrected

Reviewer: P1445 L21-24: Note that .. differences: I do not understand this sentence, could you clarify it?

Authors: The revised manuscript does now present 137Cs activities. The relative concentrations in the original manuscript are omitted.

Reviewer: P 1453 L27-30: I do not understand the caveat described there.

Authors: In the first version of our manuscript, our brief review of biotic particle interaction with radiocesium (measured in the laboratory) showed that it is hardly taken up/adsorbs to biogenic particles. In contrast, the analysis of biogenic material by Nyffeler et al. (1984) showed that it seems to adsorb onto (trap-derived) biogenic material at least to some extent. This is what we meant with "caveat". We are sorry for having caused this confusion. However, due to the reorganisation of the manuscript we omitted this part in the revised version.

Reviewer: FIG. 2 and 3: I think it is even more important to compare the TKE and sea surface height variability to the 2011 case as well. This will show how much the 1993 choice and model realism may have affected the dispersion analysis. This is a small modification but an important one, since one of the main conclusions (Fig. 7) depends on the position of the tracer patch in respect to the 2011 (not 1993) Chl bloom, and the position of the patch is directly determined by the TKE and EKE pattern.

Authors: We omitted the discussion of Chlorophyll in the revised manuscript and included a more comprehensive analysis of inter-annual variability.

Interactive comment on Ocean Sci. Discuss., 8, 1441, 2011.