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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 1 for his time and effort. His criticism, in particular his statement "You are running an eddy-resolving model for the area, so it's got to be possible to say more than that" guided the complete reorganization of the paper. Further, we agree that it might have been too early to publish. But then, on the other hand, our intention was to trigger a timely discussion.

Reviewer: The authors attempt to do this by presenting results from model experiments with idealised tracers and by a reviewing the existing literature on marine transport of 137Cs. The two parts are somewhat disjointed and it is hard to tell what conclusions

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one ought to draw from the paper. The paper in the very least needs a complete reorganisation, to make clear to which extent the results are relevant to the issue at hand. I also feel it tries to do too many things without going into depth for any of them.

Authors: We reorganized the paper, skipped the review on marine transport of 137Cs and focus now on horizontal mixing as simulated by our model. We compare our model estimate with generic parameterizations in coarse resolution models and observed 137Cs activities on the shelf.

Reviewer: 1) The review of the literature on 137Cs that you add at the end of the manuscript is long and rambling and it is hard to extract the points that are relevant to your study. It would, e.g., make more sense to take the section on biological scavenging of 137Cs into the methods section and introduction, explaining to the reader why you think you might be able to treat 137Cs as a conservative tracer in this case. I realise that you were looking for previous studies on 137Cs in particular, but the Baltic Sea/Chernobyl example makes it difficult to draw conclusions from for the Japan case. If my Eastern European geography hasn't deserted me completely, most of the Chernobyl 137Cs in the Baltic must have gotten there through air-sea fluxes. Also, the Baltic is a shallow shelf sea which, of course, makes sediment processes crucial. But you never discuss the shelf part of your model results. I think it might have been a better idea to look at the literature on the direct release of radioactive material into the ocean from sites like Sellafield or La Hague, even if the material released there was not 137Cs.

Authors: We skipped the review.

Reviewer: 2) You say you are investigating cross-shelf transport of the tracer, but there is little on this in the manuscript. Why does the tracer suddenly decide to leave the shelf after 11 weeks? Is that timescale the same for all your runs? Why does the tracer leave in a narrow filament rather than in several locations? You are running an eddy-

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resolving model for the area, so it's got to be possible to say more than that. Giving model resolution in km rather than degrees in Fig.1. Also, the physical mechanisms are the only thing you can really use your tool to draw any definite conclusions about rather than just speculating. So I would focus on that.

Authors: We have added an analysis showing where and when the tracer leaves the shelf in a set of experiments. Further, the cross-shelf transport is now, that we skipped the review, the focus of our manuscript. Model-grid resolution is now given in km.

Reviewer: 3) Figure 2 which is one of your two model evaluation figures merits one sentence in the text. I think you need to give more quantitative detail and references here to make the reader trust your model.

Authors: We extended the discussion of simulated surface currents and compare it now with an estimate based on Absolute Dynamic Topography.

Reviewer: 4) On p. 1445, line 27 you say you ran an ensemble of tracer releases. I'd probably refer to what you did as a series of sensitivity studies. Anyway, your model run setup seems very arbitrary to me. Why start again in 1993 and risk having a jump in your model physics and not use 1999? How much inter-annual variability is there in the area? Is 1993 an odd year or is it fairly representative?

Authors: The paper discusses now 5 releases starting on 1st April 1993 to 1997 (the old simulations are omitted now that there is some certainty concerning the timing and magnitude of the release), respectively. They are identical as far as the timing of the releases is concerned. Differences are discussed.

Reviewer: 5) A large par t of your motivation comes from the impact on biology, but it is really hard to figure out whether there is one. You seem to think that open ocean tracer concentrations of 1/10000 of that at the release site matter, but since you do

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not have observations of 137Cs concentrations at the release site and your Baltic Sea review doesn't really give an indication of this either, it is unclear if that magnitude of contamination matters. I wonder whether the paper with the aims as they stand actually does need that reference to make it work and that it might just be too early to publish the manuscript as it is.

Authors: We have now observations of 137Cs as well as estimates of 137Cs releases. This enabled us to refocus the manuscript. Our model agrees reasonably with observations although we do not account for biotically mediated transfer of 137Cs from the water column to the sediment. The impact on biota is no longer discussed.

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