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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

Anonymous Referee #2

Received and published: 9 August 2011

The paper presents a study of the dispersion of radioactive materials accidentally released off Japan in the aftermath of the earthquake on 11 March 2011. This is achieved by (i) a model study, in which a passive tracer is advected by an eddy-resolving general circulation model and (ii) a review of the literature on scavenging of radioactive tracers, with which the pattern of the modeled tracer and satellite-based estimation of phytoplanktonic activity are discussed. The main finding is that the tracer dispersion was probably controlled by mixed layer dynamics and in much smaller part to biological activity. The presence of large uncertainties behind these estimations is emphasized.

The analysis presented in the paper is correct although not very surprising nor very

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deep. However, the paper in my opinion provides a very good job to the oceanographic community by identifying a few months only after the nuclear accident some potential uses of circulation models, the most relevant literature on scavenging of radioactive materials, and suggesting some key uncertainties that hinder the possibility of getting to more quantitative assessments. The timeliness of this work together with the importance of responding to the Fukushima-Daiichi accident in my opinion largely offset the somewhat preliminary nature of the results. In my view the paper can be a valid contribution to OSD and has a strong potential to catalyze discussions among modelers, biogeochemists, and physical oceanographers on this important subject, if the issues below can be addressed by the authors.

MAJOR ISSUES

1. The authors state that, after the Fukushima-Daiichi accident, “a comprehensive set of ^{137}Cs 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.
2. Related to the issue above, it is not completely clear to me how much the results



Interactive
Comment

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.

OTHER ISSUES

Abstract: “evaluate and advance the evaluation”: please find a better wording.

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.

P1444 L24-25: The spin up 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?

P1445 L6: Fukushima → Fukushima

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

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

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.

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

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