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Interactive comment on “One-year, regional-scale simulation of ^{137}Cs radioactivity in the ocean following the Fukushima Daiichi Nuclear Power Plant accident” by D. Tsumune et al.

Anonymous Referee #3

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General comments

I think this paper needs an important revision before being published. The first problem is that a major objective other than to carry out an annual simulation is missing. I read the first paper of the main author on the Fuushima accident, Tsumune et al., 2012, and I appreciated it a lot. But now, I feel disappointed as this second paper is not really new. Many things are taken from the previous paper as well as elements of articles by other authors (literature review for the direct inputs for example).

My first impression after reading a large part of the paper was that it focused on the fact that atmospheric deposition inferred from atmospheric models was too low and

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consequently I was waiting for a new assessment. Finally, this idea failed at the end of the paper with the two last subsections -4.2 devoted to direct inputs but mainly illustrated by a literature review, and 4.3 on mesoscale eddy- that do not bring anything new. At the end of the reading, it seems that the authors have sought to achieve a synthesis, but the paper is not presented in that way.

The second problem is the design of the paper. This is true "at different scales." First in the general structure as explained above, but also within the paragraphs where ideas are difficult to follow because of poorly connected sentences. This makes the reading of the paper difficult. Finally, there are too many figures (about 20 without supplementary information), especially if we consider the low number of new results. Several figures are used to show almost the same result.

To conclude, I think this paper requires a thorough review before being published. I suggest that the authors choose a main objective and develop it without wanting to talk about all the mechanisms of supply and dispersion in all the details. Conventionally, preliminary results should be presented in the first part and the new results in the following section. If the main objective is to understand the origin of the radionuclides throughout the first year after the accident, authors should find a way to present these results which does not require dozens of figures (probably a total of 10 figures should be sufficient).

Specific comments

Introduction is not well structured. This concerns especially page 6262 starting from line 17 until the end. The objective of the paper is not, or very briefly, discussed.

Section 2.1 Section Monitoring data is almost a repetition of what is said in the introduction

page 6263 line 18: a 33% error on the data is mentioned. This is a new information that deserves to be explained.

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Page 6264: explain what is the benefit of using WRF at 5 km resolution as it is forced by the Japan operational model itself at 5 km.

page 6265 line 31 the ocean depth is probably much more than 1500m.

Page 6266 Why the Hycom model is mentioned here? Was it used in the previous paper of the authors?

The nudging time is very short (1day). In such conditions, it is difficult to understand what is the benefit of using the ROMS model compared to JCOPE2. The limit of the bathymetry at 1000 m probably does not allow to represent correctly some oceanic processes. These two points make difficult to understand why the authors do not use JCOPE2.

No tide. Justify!

page 6266: Two important references are not available for the large scale forcing (Aoyama et al., 2012c is a presentation and 2013b is in preparation. Is it possible to give indications about the quality of these results?

Page 6268 line 1: Atmospheric deposition onto the ocean began to occur by early April. Is it not contradictory with previous results? Atmospheric deposition near the FDNPP occurred in the days following the explosions in March.

Section 3.1: the use of "we" alterns with the use of Tsumune et al., 2012. This can be confusing.

Page 6268: I understand that the use of a release rate of 1 Bq/s is not important as this result is then multiplied by a scaling factor being a ratio of activities. This is not clear at the beginning of the explanation (line 25). I think the authors should find a smart way to explain that without repeating twice this value of 1Bq/s which does not represent anything. Another reason to shorten this section is that it is a repetition of what is explained in Tsumune et al., 2012.

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Page 6270: I understand that the calculated release rate did not change between Tsumune et al., 2012 and the present paper. I am a little bit surprised because the authors changed the forcing oceanic model (1.Hycom; 2. JCOPE2). The choice of the forcing model should be important especially as the authors use a very short nudging time. In my opinion, the release rates obtained by all the models which ran to represent the dispersion are different because of the current fields responsible of the dispersion are also different as they depend on the large scale circulation and atmospheric forcing. I would like the authors comment this point.

Page 6271 "global model estimated the total amount of radioactivity deposited in this area to be 3.04 PBq (MASINGAR mk2, Yukimoto et al., 2011), a value that we used as a boundary condition in this simulation (Aoyama et al., in preparation)" The total deposition is not the value prescribed for the boundary condition.

The use of two sets of atmospheric deposition (one at regional scale and another at large scale interacting with the first one at the boundaries) is a weakness of the paper especially if we consider that deposition over a common area is 3 times lower in the regional model than in the global model. I think that this discrepancy is a problem to be published in a high standard journal. I suggest that the authors use the result of the global model at both scales.

Page 6272 line 8 Pacific (not pacific)

Fig.6: very difficult to see the currents "The characteristics of the simulated results were consistent with previously observed results". You should tell more about this consistency. Especially because the reference is not a paper but a report in Japanese.

Figures 8, 9 and 10 give almost the same information. I suggest to compress the information into a single figure. In addition, it appears that the agreement between model and observation at the grid point adjacent to the NPP is logical. Indeed, the measured concentrations were used to calculate the release rate. Same problem for Figures 11 and 12.

page 6273: the curvature of the exponential curves changed on 26 April and 30 June. Is it possible to give a comment for the necessity to change the release rates trends at these periods?

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Page 6274 line 9: Total deposition was two times smaller than the measured inventories in the North Pacific. Near the NPP, it seems that the underestimation is much more pronounced (one or two orders of magnitude). This could be another good reason to use the deposit from Aoyama which are three times higher than the values used here.

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Simplify the paragraph between lines 14 and 20. The ideas are simple (after several readings) but difficult to follow.

Page 6275. The two simulations with and without atmospheric deposition are very close at 2F and Iwasawa. They underestimate significantly the activity at these two points from the middle of April. The authors explain that this underestimation is due to an underestimation of atmospheric deposition in March. In my opinion, it is difficult to prove that. I think that another explanation could be that the patterns of currents responsible of the dispersion of direct releases could be deficient. This is an important issue of the paper that should be discussed in details.

Page 6276: Mixed layer depth more realist in JCOPE2. The authors should give more details. A figure with mixed layer depth measured at 30 km is presented in Estournel et al., 2012. The authors could refer to this figure and give numbers for both simulations.

Explain better the red curve in the shaded zone of Fig. 13 and 14. The comparison at 30 km once more suggests a strong underestimation of the atmospheric inputs. Is it possible to propose an estimation about the amount of atmospheric input that is missing?

Page 6278. "because the mesoscale eddy effects cause the vertical profiles to be complex" Please check that the mesoscale eddy was discussed before (I think it was only cited in the beginning of the paper).

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Section 4.2 seems inappropriate here. I don't understand why the authors chose to make this bibliographic synthesis on direct release here. In the preceding sections, the novelty concerns the suspicion of underestimation of atmospheric inputs. It is expected that the paper further develops this idea and proposes a new estimate. Section 2 could be justified if providing something really new on the direct release (I think that it is not the case). As I said in the general comments, my opinion is that the paper should focus on the new aspects. This should make it easier to read. This section on direct release should be put at the beginning of the paper in a section of context.

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