**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 Fukushima 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 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).

We are grateful to referee #3 for valuable comments. According to the suggestion, we changed the construction of paper to separate between the results from previous paper and this study. We rewritten introduction to indicate our objectives. We moved review of direct release (section 4.2 in the original version) to introduction and presented our objectives in introduction (see below). We deleted section 4.3 and reduced figures from 20 to 12.

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.

We rewritten introduction completely. We added the following description to show our objectives in introduction,

"In this paper, we newly include atmospheric deposition based on the previous study (Tsumune et al., 2012) to estimate the contribution of direct release and atmospheric deposition on behavior of $^{137}Cs$ in regional ocean. In addition, we expanded the model domain and extended the period of simulation for 1 year, until the end of February 2012 to compare with wider and longer measured data and to confirm the estimated direct release rate. Buessler et al. (2011) and Kanda (2013) pointed out direct release may continue. Therefore estimation of direct release for longer period is important to assess the oceanic contamination. Measured data have been increased since the end of May 2011 by TEPCO, MEXT and others. Oceanic numerical simulation is useful for estimating the rates of direct release and for representing and predicting the behavior of radioactive materials. Reconstruction of the history of the activities of radioactive materials by numerical simulations is useful for understanding the history and processes of radioactive contamination of oceanic biota (Tateda et al., 2013)."

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

We removed the description of monitoring data from introduction.

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

We added the following sentences and reference,

"There was a systematic less than 33 % error in the data based on the manual by

MEXT(1992) when the activities were reported because if one sigma of the counting error exceed 33 % the activity should be reported as " below detection limit". When $^{137}Cs$ activity is larger than the order of $10^6$ Bq $m^{-3}$, enough count can be obtained and the one sigma of the counting error might be less than 1 %."

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.

We ran the WRF to acquire the data for CAMx. We cannot get all data from MANAL data for CAMx. We changed the sentence as follows, "The initial and boundary fields were produced from the published operational mesoscale numerical weather analysis data by the Japan Meteorological Agency (MANAL; every 5 km and 3 h). Note that all data for the CAMx simulation is not acquired from the published MANAL data."

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

Yes. We think that effect of reduction of water depth is not so large because we nudged to JCOPE2 data.

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

We added the sentence as follows,
"instead of the Real-time 1/12° Global HYCOM (HYbrid Coordinate Ocean Model) Nowcast/Forecast System results (Chassignet et al., 2006) which was used in previous simulation (Tsumune et al., 2012)."

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.

We added the sentence as follows,
"Horizontal resolution of JCOPE2 is 1/10°. Temperature and salinity were nudged to the JCOPE2 reanalysis results to represent mesoscale eddies during the simulation period

in the ROMS with higher resolution (1km x 1km)."

No tide. Justify!

We added the sentence as follows,

"Previous simulations considered the tidal effect (Tsumune et al., 2012). After that we confirmed that tidal effects were small on the behavior of $^{137}Cs$ in these simulations. Therefore we omitted tidal effect in this study to simplify the model simulation."

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 theses results?

The paper is still in preparation.
We showed a part of their results in Figure 4. This figure shows a good agreement with simulation and observation.

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.

We changed the sentence as follows,
"atmospheric deposition onto the ocean began to occur from 11 March to the early April"

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

We changed and shorten description in 3.2 to separate between previous study (Tsumune et al., 2012) and this study.

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.

We changed and shorten description in 3.2 to separate between previous study (Tsumune et al., 2012) and this study.

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.

We added the following description

"Although we expanded model domain and changed nudging data, we obtained 2.2 x $10^{14}$ Bq $day^{-1}$ as release rate from 26 March to 6 April 2011 (period [1]), which is same release rate obtained by Tsumune et al. (2012). The reason is that coastal current adjacent to the 1FF was not changed by same wind forcing in Tsumune et al. (2012)."

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.

The horizontal resolution of global model is about 60km. Coast line of atmospheric model is different from the one of the regional ocean model. Therefore we choose the regional atmospheric model for this study.
We used the same release scenario for global and regional atmospheric transport model. Deposition process is complex and still unknown especially on the ocean. As a result, total deposition in a regional ocean by regional model is 3 times smaller than the one by global model.

We moved this description of comparison to 3.4 Inflow from boundary sections.
And we discuss about this discrepancy in 4. Discussion as follows,

"We used atmospheric deposition by the regional atmospheric model and inflow by global atmospheric model. Total amount of atmospheric deposition by global atmospheric model was three times larger than the one by regional atmospheric model in this model region, therefore inflow rate by global model was smaller than the one by regional model. It is difficult to set atmospheric deposition in the global scale with higher resolution corresponding to the regional scale in this study. This study just indicated that the amounts of atmospheric deposition and inflow were underestimated. Quantitative analysis for atmospheric deposition both in the regional and global scale is a future work for us because atmospheric deposition rate on the ocean has a great uncertainty due to a lack of measured data."

Page 6272 line 8 Pacific (not pacific)

We changed to "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.

Previous observation by current meters showed the characteristics of coastal current (Nakamura, 1991). Alongshore (North south component) currents were dominant. The direction of the currents changed roughly every 3–4 days because of changes in the synoptic scale wind fields. Peak current speed in each 3-4 days period was 0.1 – 0.5 m s-1. Figure 2 shows the temporal changes of simulated current vectors adjacent to the 1F NPP. The characteristics of the simulated results were consistent with previously observed results.

We added pages information in reference.

"Nakamura, Y.: Studies on the Fishing Ground Formation of Sakhalin Surf Clam and the Hydraulic Environment in Coastal Region, Fukushima suisan shikenjo research report, 1-118, 1991, (in Japanese), http://www.pref.fukushima.jp/suisan-shiken/houkoku/kenpou/index.htm"

Report is in Japanese. Figure of current velocity can be assessable by website.

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.

According to the suggestion, we compressed a single figure from Figure 8, 9 and 10, and Figure 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?

Mechanism changing the curvature is still unknown. We added the following description

"From 7 to 26 April 2011 (term [2]), the release rate decreased exponentially in a manner similar to the $^{137}Cs$ activity. Kanda (2013) pointed out that the exchange rate of water in the main harbour area was estimated by the decrease of radioactivity immediately after the intense release of highly radioactive water by 6 April 2011. This exponential decrease of direct release from the main harbor to the ocean was caused by the release of high contaminated water in the main harbor by water exchange between inside and outside of main harbor. Tsumune et al. (2012) estimated the release rate to be constant after 27 April 2011. In this study, direct release rate continued to be exponentially decreased after 27 April 2011. This means flow rate and/or activity of radioactive water might be exponentially decreased. The mechanism of exponential decrease of direct release rate was still unknown."

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

We deleted the original sentences and discussed about underestimation of activity and inventory in 4. Discussion as follows,

"Measured $^{137}Cs$ activities attributable to atmospheric deposition were $1.0 \times 10^5$–$1.0 \times$

$10^6$ Bq $m^{-3}$ adjacent to the 1F NPP before 26 March 2011, $1.0 \times 10^4$–$1.0 \times 10^5$ Bq $m^{-3}$ adjacent to the 2F NPP and in Iwasawa coastal waters after 27 March 2011, and 1000–30,000 Bq $m^{-3}$ 30 km offshore before 8 April 2011. Simulated activities were one or two orders of magnitudes lower than measured activities at the 1F NPP, at the 2F NPP, in Iwasawa coastal waters, and 30 km offshore. To estimate the underestimated inventory, affected area was roughly estimated to be 10km x 2 km, 50km x 4km and 100km x 30km for 1F NPP, 2F NPP and Iwasawa coast, and 30km offshore, respectively. And mixed layer depth was set to be 10m. The underestimated inventory was estimated by multiplying underestimated $^{137}Cs$ activity by affected volume. We estimated that the underestimated inventories were $2.0 \times 10^{14}$ Bq, $2.0 \times 10^{14}$ Bq and $3.0 \times 10^{14}$ Bq for 1F NPP, 2F NPP and Iwasawa coast, and 30km offshore, respectively. This underestimated inventories were smaller than the total amount of radioactivity deposited from the atmospheric in the simulated area, which was 1.14 PBq."

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.

We added the following description,
"Averaged current was southward at 0.06 m $s^{-1}$ from 26 March to 20 April 2011 and changed to be northward at 0.06 m $s^{-1}$ from 21 April to 27 May 2011. When the averaged current direction was northward, $^{137}Cs$ activities at 2F NPP and off the Iwasawa coast was not attributable to direct release. We, therefore, estimated that the simulated $^{137}Cs$ activities underestimated significantly during from 21 April to 27 May 2011."

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.

We added the following description,
"Previous model simulations nudged with HYCOM reanalysis (Tsumune et al., 2012) underestimated offshore transport because the simulated mixed layer depth was deeper

than the mixed layer depth simulated by JCOPE2. Estournal et al. (2012) calculated the mixed layer depth by the observed data by JAMSTEC, which is about 10m at 13 April 2011, 30km offshore. Simulated mixed layer in April 2011 was 50m by HYCOM and 10m by JCOPE2 (S-Fig. 7)."

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.

We added the following description,
"Therefore, measured points are shown by the same symbol, gray shading indicates the range of simulated activities at eight sites, and red line shows the averaged simulated activities at eight sites."
Yes, we described as follows,
"The differences of $^{137}Cs$ activities were attributable to atmospheric deposition. Increasing the simulated $^{137}Cs$ activity attributable to atmospheric deposition by about one or two orders of magnitude, would diminish the magnitude of the underestimation before 9 April and after 1 May."

Is it possible to propose an estimation about the amount of atmospheric input that is missing?

We added the following sentences.
"To estimate the underestimated inventory, affected area was roughly estimated to be 10km x 2 km, 50km x 4km and 100km x 30km for 1F NPP, 2F NPP and Iwasawa coast, and 30km offshore, respectively. And mixed layer depth was set to be 10m. The underestimated inventory was estimated by multiplying underestimated $^{137}Cs$ activity by affected volume. We estimated that the underestimated inventories were $2.0 \times 10^{14}$ Bq, $2.0 \times 10^{14}$ Bq and $3.0 \times 10^{14}$ Bq for 1F NPP, 2F NPP and Iwasawa coast, and 30km offshore, respectively. This underestimated inventories were smaller than the total amount of radioactivity deposited from the atmospheric in the simulated area, which was 1.14 PBq."

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).

We mentioned the mesoscale eddy in 3.1.

Here we added the following sentence

"It is difficult to compare observations and simulations at each point because the vertical profiles were significantly affected by the position and movement of the mesoscale eddy that are still difficult to simulate precisely in the model.

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.

We deleted 4.2 and moved review in introduction.

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.

Yes. We deleted 4.2 and moved review in introduction.