Ocean Sci. Discuss.,
doi:10.5194/os-2016-103-RC2, 2017

Ocean Science
Discussions

Interactive comment on "Lagrangian simulation and tracking of the mesoscale eddies contaminated by Fukushima-derived radionuclides" *by* Sergey V. Prants et al.

Anonymous Referee #2

Received and published: 27 February 2017

General comments

The authors used altimetry-derived surface geostrophic currents, to performed a series of Lagrangian experiments in the north-western Pacific Ocean, East of Japan, in the months following the Fukushima Nuclear Power Plant (FNPP) accident. They produced a series of successive maps in which each Lagrangian particle is back-tracked for two years and labelled according to its region of origin. The maps are used to infer the origin of the material associated with various mesososcale eddies identified in the region of study, and to investigate how the presence of such structures affected the dispersion of the Fukushima radioactive material after the accident. The results are compared with already published in-situ observations from three different cruises

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showing good agreement.

Overall I found the paper to be clear and the figures of good quality. Although being too descriptive, the results presented provide good support for the interpretation of the in-situ observations and could be eventually worth publication. However, I do not recommend the paper to be published in its current state.

Major comments

As it is, a large portion of the paper is dedicated to the description of the results from previous ocean campaigns which have been already described in previous publications (Buessler et al. 2012; Prants et al. 2014; Kaeriama et al. 2013). The originality of the manuscript needs to be improved. For instance, the authors should provide more details on both a) the computation of Lagrangian diagnostics; b) the eddy identification and tracking. Regarding the Lagrangian diagnostics: little is said other than that they are derived from AVISO velocity fields. Are the trajectories derived using time varying fields? Are the velocity fields interpolated in both sapce and time? If so, how? At which spatial resolution are the Lagrangian particles used to generate the maps in Figures 2 to 4 deployed? Which type of AVISO product was used (dt or nrt; two or all sat merged)? On lines 95 to 107 the authors list a series of what they call "Lagrangian indicators", however, they are never shown in the following section. My suggestion is to include only the ones that are discussed in the rest of the manuscript. Among those the authors briefly mention the Finite-time Lyapunov exponent (FTLE). Again, if such diagnostic is used for any of the analyses described in the manuscript (i.e. for definign the elliptic and hyperbolic points introduced in section 2?), then more details on the background of the FTLE and the way they are defined and computed should be provided. More details should also be provided on how the elliptic and hyperbolic points are identified. In the manuscript (lines 163-165) they are simply defined as points of zero velocity. How is the circular and diverging/converging motion around those points identified? Regarding eddy detection and tracking, even less is provided. If they have been identified from previous studies, it should be explicitly said. Otherwise, more details on the methodology should be given.

A second major issue is with the interpretation of the results from Figures 2 to 4. If I interpret them correctly, each point in the plot has been advected backward in time for two years and then color coded according to its region of origin. The regions are defined in figure 1a. White points are the ones that do not originate from any of those regions. I do agree with the authors, that such backtracking is quite powerful for understanding the origin of waters trapped within the 3 mesoscale eddies investigated. However, a 2-year backtracking means also that a portion of the “yellow” waters in the figures could have originated from the FNPP area from dates earlier than the accident, and thus not being necessarily contaminated with radioactive material. Wouldn’t a Lagrangian experiment forward in time from the region of the FNPP after the time of the accident provide more suited trajectories/diagnostics to infer the fate of the radioactive material released? Specific of Figure 2a, at 144 E between 36 and 38 N, there is a very sharp zonal boundary between the different water masses. To me it appears similar to an artefact (either from data processing or visualization). Can the authors comments/correct that?

A final issue regards the methodological approach: in several occasion throughout the manuscript the authors refer to their Lagrangian approach as “special” (e.g. line 433). Although I agree with the authors when they claim that this type of diagnostics provide a more condensed, easier to read/interpret information, compared to spaghetti plots of particle trajectories, the approach they propose is not entirely novel, as several studies in recent years (for instance d’Ovidio et al. 2015, Biogeoscience to cite one of the most recent ones) have been based on the analysis of similar Lagrangian diagnostics. I suggest to rephrase some of the sentences describing the approach to make this clearer.

Furthermore, it is not clear to me how the presented approach would improve the limitations and uncertainties of deriving Lagrangian trajectories in a chaotic environment (lines 71 to 74). In the paragraphs from lines 75 to 85, the authors seem to hint to that

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the adopted Lagrangian approach is more robust because it does not require “a precise solution of the Navier-Stokes equation”. However, any Lagrangian diagnostic is based on trajectories which require a velocity field to be derived. Later in the manuscript, the author further remark the robustness of their approach stating that it is based on a “statistically significant number of particles”. However, other than a large number of particles, shouldn’t a statistical Lagrangian approach include also a random-walk term of some sort to simulate sub-grid diffusive-like processes? In my opinion, increasing the number of advected particles, while maintaining the same velocity field and the same equations as a coarser experiment, will provide more detailed results, but not significantly different than a coarser experiment (see for instance Hernández-Carrasco et al., 2011, Ocean Modelling). These two aspects should be clarified by the authors.

Minor comments

- The term “Lagrangian particles” should replace “tracers” in several instances in the manuscript.

- Line 66: if possible I would add some references to the Lagrangian studies which focused on the Horizon accident in the Gulf of Mexico. I am sure that not all of them were only based on the analysis of “spaghetti-like” plots.

- Fig 1a: I would plot SST rather than velocity magnitude (since temperature variation and fronts are repeatedly used in the introduction), and I would remove the indication of elliptic and hyperbolic points since in a climatological velocity field they do not represent mesoscale structures.

- Line 79: Diffusion will always occur. It is the mixing induced by advection that is reduced across transport barriers.

- Line 123: replace “twofold” with “threefold” since the same paragraph contain a “Firstly...”, a “Secondly...” and a “Finally...”

- Line 147-149: AVISO provide the geostrophic component of the real near-surface velocities.

- Line 165-167: The sentence should be moved after line 169, since it refers to elliptic points only only.

- Line 176: Several studies in the last few years have shown indeed that LCS and hyperbolic points can be identified and tracked for several days from in-situ observations: Haza et al. 2010, Ocean Dynamics; Nencioli et al 2011, JGR-Oceans; Olascoaga et al. 2013, GRL. They should be cited here.

- Line 381: were the ARGO floats regular float, or were characterized by specific configurations?

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2016-103, 2017.