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Ocean Science Discussions

Interactive comment on "A new Lagrandian based short term prediction methodology for HF radar currents" *by* Lohitzune Solabarrieta et al.

Anonymous Referee #2

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In the paper by Solabarrieta et al. a new short-term prediction method for surface marine transport is presented. The method is based on Lagrangian "analogues" calculated using velocity data from high-frequency coastal radars located in two different regions: the Bay of Biscay and the Red Sea. New-method errors and predictions are compared with those based on persistence. The performance is comparable to other methods reported in previous literature (e.g. Solabarrieta et al, 2016) as mean separation distances are shown to be similar. The new method can be more easily implemented operationally than the others due to its computational cost, which is allegedly low.

A process of major revisions is suggested to address the following concerns:

1) L123: "well demonstrated results". Please explain why OMA was chosen and quan-

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tify the OMA skills providing values and the advantages wrt to other methodologies like DINEOF or SOM.

2) L138-146: not clear paragraph here and the concept may be missed. Are the authors trying to justify the choice of a Lagrangian vs Eulerian approach for the analogues? If so, wouldn't be enough to say that Lagrangian trajectories are direct measurements of transport of substances at sea? And also that they are more dependent on resolution as they are more keen on accumulating errors being integrals of the velocity fields?

3) L151: uniqueness and originality of the work. Authors should clearly state whether or not this is the first application of the method of analogues in the ocean.

4) L156: numbers expressing a quantification of the computational costs for the different methods should be provided here. How long does it take to run this new method wrt the one in Solabarrieta et al (2016)? What about wrt other methods?

5) L162-177: how do resolutions in the two regions compare with the Rossby radii? Are spatial resolutions of the HF radars fine enough to capture the marked seasonal variability of the mesoscale features in the whole year for both regions? Please provide number and quantify.

6) L209: a conceptual question that should be addressed. It is my understanding that the OMA method is based on finding the best combination of geometrical modes in a specific region able to maximize the fit with the observations at a specific time. In a way, isn't the combination and gap-filling technique already based on "analogues" modes? Isn't this procedure already creating analogue situations from a dynamical perspective, introducing a bias when epsilon_ANL is calculated? I guess that the other way to pose the same question is: how sensitive are results to the use of OMA? How much do they change if a simple linear interpolation technique is used instead of OMA?

7) L213: clearly say here that the "most similar" concept will be defined later in the paper.

8) L212-218 and L220-226: more concepts are repeated in both paragraphs. Please combine them and shorten accordingly

9) L228-230: where is this shown? I have the impression that a section has been completely cut off from the paper. This is also related to point 23 below

10) L237: is conceptually correct to use the whole period as a test period and a Lagrangian catalogue at the same time for the Red Sea? How do results change if the first year is used as catalogue and the second year as test period?

11) L244: I would suggest swapping Fig.2 and Fig.3 positions as this latter is introduced in the text before.

12) L269: please remove not needed.

13) L326-330 and Fig.4: contradictions and big confusion here. Not easy to understand whether or not black dots show periods when epsilon_STP is either larger or smaller than epsilon_PRS. My guess is that dots are when errors in the predictions are larger than in the persistence. Please double-check and rephrase the whole paragraph

14) L331: what is the time-scale of the persistence of these currents during winter months?

15) L343: indicatES

16) L349-357, Fig.6 and throughout the manuscript: please use the already introduced notation for the mean separation distance like, for example, \deltaˆSTP_6h (\deltaˆPRS_6h) and not STP_dist (PRS_dist).

17) L356: not sure what "especially after 12 hours mean"? Maximum values are at 36h. Do the authors want to say that larger values are reached and remain almost constant after 24h? Please rephrase.

18) L357: it should be also mentioned that at t=6h PRS is always better that STP (Fig.6). However we have a problem here: at t=6h Rˆ2 for PRS is is lower than for STP

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(0.07 vs 0.19). How is this possible?

19) L364: isn't this choice unfair wrt persistence? Shouldn't we consider all of them for a fair comparison?

20) L371: correct, it should be indeed added that persistence during the first hours is actually slightly better

21) L380-381: why does the mean drift follow more the persistence curve in the Red Sea case?

22) L390: the advantage is not clear as this is the difference between the two, does not necessarily mean that one is better than the other. Please modify Figs.9 and 10 as suggested in point 37 below

23) L404-407: what does this mean? Only Lagrangian analogues are shown in the manuscript. Has a section been cut off from the paper? This is also related to point 9 above.

24) L417: contradiction with L327-328

25) L423: "first and only the first". Not really but please quantify as it looks that for BoB is at least during the first 6h and for the Red Sea at least for the first 15h!

26) L429: not sure about this value as it was reported 853 km2 before (e.g. at L342 and L364)

27) L441: Fig.7 not Fig.4, correct?

28) L447-453: these lines belong more to the introduction. They are also qualitative while differences and comparisons between methods should really be quantified.

29) L463-472 and in general for the whole section: discussion is poor. Why aren't HF radars able to capture currents if they are persistent? I would expect radars not to be able to resolve highly-variable small-scale structures, not persistent features! Not

getting (or buying) the idea that something persistent cannot be seen by analogues. A better dynamical insight is needed and expected in the discussion of the results.

30) Fig.1: can we have GDOP maps in the two regions? Can they help discussion? Asking for more reasons: a) obtained ranges look large compared to the radar system positions and distances between them; b) it would be important to visualize in which areas OMA operations are more to be carried out; c) it would be nice to compare/discuss GDOP maps wrt to the error distributions of the new Figs.9 and 10 (see point 37 below)

31) Fig.2: resolution is really terrible, please increase it. Line should be thicker as in Figs. 7 and 8. Why are there gaps in the blue line? Really confused by the fact that caption is reporting Nov 17 2015 instead of April 13-15, 2015 as in Fig.3.

32) Fig.3: why is this time chosen? Is this a good or bad example?

33) Fig.4: resolution is really terrible, please increase it. Lines should be thicker as in Figs. 7 and 8. I would suggest to put them in three different panels as they mostly overlap. Double-check figure and text for black dots meaning.

34) Figs.5 and 6: resolution is really terrible, please increase it. Lines should be thicker as in Figs. 7 and 8.

35) Figs.7 and 8: rearrange x-axis labels to have 6-h intervals ending at 48h.

36) Fig.8 caption: remove (UP)

37) Figs.9 and 10: both figures need improvements to show the errors and not only their differences. Suggestion is to have a total of 12 panels in each region and show for each time three panels, one with \deltaˆSTP, the second with \deltaˆPRS and the third one with their difference.

38) Figs.9 and 10: put labels indicating times either on top of each panel or in the right bottom corners, on land

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