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Interactive comment on "Impact of HF radar currents gap-filling methodologies on the Lagrangian assessment of coastal dynamics" *by* Ismael Hernández-Carrasco et al.

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Reviewer:

The paper entitled: "Impact of HF radar currents gap-filling methodologies on the Lagrangian assessment of coastal dynamics", presents the results obtained through the application of three different gap-filling procedures used to regularise the hf radar current fields. The paper deals with an argument that, although very technical, is very important for the hf radar community. The method performances are evaluated in terms of both standard metrics (absolute relative error, mean bias and root mean square error) and lagrangian metrics (FSLE, LCS and residence time). The paper is well written, and

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the arguments are addressed with a good methodological strictness. Thus, the paper can be accepted for publication in ocean science even though I have some comments the authors should consider.

Response:

We acknowledge the Reviewer for their positive and constructive comments throughout the revision which have helped us to improve the manuscript.

MAIN COMMENTS:

Reviewer:

1) I don't like very much the introduction. I don't find that it addresses the actual topic of the paper. It is too much unspecific and I found useless the part where the lagrangian and eulerian approach are described. I think that the paper would benefit a lot by a more specific introduction about the problem of gaps in hf radar fields.

Response:

We thank Reviewer for this comment that we have addressed adding a further description of the gap-filling problems in HF Radar velocity fields and improving some paragraphs in the "Introduction" section. We would like to clarify that in this paper we do not focus on the origin of the HFR system failures that produce gaps in the velocity fields but on the effect of the gap-filling methodologies on the Lagrangian assessment of the coastal dynamics using HFR observations. For this reason we consider that it could be beneficial for some readers to keep a paragraph in the manuscript showing the importance and the relative growing use of some Lagrangian metrics like FSLE, LCS, divergence structures. This paragraph can also be used to introduce the fact that to obtain such diagnostics is necessary to fill the gaps in the HFR fields, and therefore the need of checking whether the reconstruction methodologies are introducing artifacts in the final Lagrangian diagnostics rather than in the trajectories computations.

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2) The evaluation of the methods is done comparing standard statistic and lagrangian metrics. I found an excellent idea to catalog the possible causes of data acquisition failures and then for each of the them evaluate which gap filling procedure is the best to apply. On the other end I didn't find any possible explanation of why an approach is better than the other. In general, you concluded that SOM and DINEOF are better than OMA, conclusion based on the statistic comparisons, but the reason why one method behaves better than the other is not explained or at least it is not clear to me. So, the authors should do an additional effort to try to address this point. One of the point the authors should also point out is how and if their results can be extended to other system with different resolution, frequency, etc. . .

Response:

The main goal of the paper is to analyze how the gaps in the HFR velocity field affect in the Lagrangian diagnosis. To address this question we have applied three methodologies (two well established techniques, i.e., OMA and DINEOF, and a new one specifically developed in this study, SOM) to reconstruct the velocity fields necessary for the Lagrangian computations. Then we have addressed an statistical comparison of the three gap-filling methods based on their performance on Lagrangian diagnosis. We agree with the reviewer that a further analysis to unravel the origin of why one method behaves better than the other should be performed. We consider that such evaluation of individual error sources and the combination of different methodologies is relevant, however this is out of scope of this manuscript and should be addressed in a separated further analysis. Indeed this is an ongoing work that we are exploring together with an evaluation of the combination of reconstruction methodologies.

As suggested by the reviewer we have improved the explanation of the possible sources of errors for each gap-filling methodology in the manuscript. A further discussion of the reasons why one method behaves better than the other and if their results can be extended to other systems have been added in the "Discussion and conclusion" section including the following discussion.

“DINEOF and SOM reconstruction methods are based on the patterns of the velocity extracted from statistical concepts. These methods strongly depends on the number of modes/neurons used in the reconstruction. The larger the number of modes/neurons, the less smoothed the pattern will be. However even when using a large size of the neural network or number of modes, these methods are prone to filter the velocity field removing some small scale dynamical processes, and in some cases the resulting patterns are a smoothed representation of the real dynamics. DINEOF and SOM also depend on the choice of the modes or patterns. In the SOM methodology the fact that each neuron is composed of velocity fields at three different times could increase the probability of using a more suitable pattern for the reconstruction, in particular, in the cases where there is a large number of missing points, i.e., experiment C (failure of antenna). This could explains the many points out of the main cloud in the scatter plot shown in Figure 5 for DINEOF for the experiment C. On the other hand OMA is a geometrical approach that also depends on the choice of the modes and the number of modes (combination of irrotational and incompressible field configurations) used to project the velocity field. In general since only a finite number of modes are computed, we are introduced an arbitrariness in the selection of the modes, that is, the real velocity is projected in a subspace of modes that are either tangent to the coastline or in the open boundary. Using a small number of modes could affect the results of the reconstruction obtaining a velocity field far away from the HF radar data but with very simple features. In the situations where the flow patterns are simple and quasi-permanents the use of a few modes is able to capture this feature. In contrast, if a large number of modes is used, the reconstructed field matches the HF radar data, but the data are not sufficiently filtered and smoothed, introducing some dynamical features or even noise which could not be presented in the real flow. In our case, the coastline in the area of the HFR coverage is relatively large originating an increase in the number of modes tangent to the coast to the detriment of the OMA open boundary modes, therefore, conditioning the resulting inferred velocity field. As explained in section 3.1. the constraints applied in the OMA can limit the representation of localized small-scale features as well as flow structures near open boundaries. Besides, as discussed in Kaplan and Lekien, 2007, difficulties may arise when dealing with gappy data, especially when the spatial gap size is larger than the minimal resolved length scale (that explains why the results are worst when there is just data from one antenna). We think that the analysis of the individual sources of errors in the gap-filling methods is not trivial and that it depends on the coherence and persistence of the dynamical features as well as on the full knowledge of the dynamical scales involved in the tracers motion. Further analyses are needed to answer these questions, however this is out of scope of this manuscript and should be addressed in future studies.

All the results reported in this study can not be extended to HFR systems working at different frequencies and resolutions. HFR working at different frequencies capture dynamical features at different scales that could be altered during the reconstruction process. Further studies using data from different HFR systems and regions characterized with different dynamical conditions should be performed to address these questions. For instance as said above some SOM and DINEOF modes used in the reconstruction are smoothed representations of the real dynamics and they could remove some small scale dynamical processes."

We have also added a sentence in the "Discussion and conclusion" section addressing this last point.

Reviewer:

3) In the result section, pag. 13, you concluded that:" . . . .the separation distances after 24 hours of simulation between the reference fields and the gaps fields are lower than this observed between HFR and real drifters", then the performance of the three methods is very good. This conclusion is questionable in my opinion. First: I think you are referring to the drifters used in Solibarrieta et al., 2016. If it is correct, you are using SVP drifters that has the drogue (in standard configuration) centered at 15 m. So, as also pointed out in Solibarrieta et al., 2016:" Since the nominal depth of the available

drifter trajectories ranges from 10 to 20m and most of the trajectories are obtained during stratified conditions, part of the differences in the drift between real and simulated trajectories can be related to the vertical shear of the current", drifters trajectories can be influenced by a different dynamic. In fact, drifters capture subgrid motions that HF radars are not able to measure. Your radar spatial resolution is 5 km, so everything occurs in a square of 25 km2 is filtered out. I have well in my mind a spaghetti diagram showing a very chaotic surface current field whereas hf radar, at 1 km of resolution, was showing a much smoother current field (results unfortunately still unpublished). I think this fact can influence the separation distance evaluated using drifter data and this make the comparison between virtual and real drifters not so immediate and then any direct conclusion can be made. Which is author's opinion on this?

Response:

Our intention was not to compare both experiments (real and virtual HFR drifters) but to discuss the differences in the observed-reconstructed trajectories comparing our results with other studies using HFR trajectories. We agree with the reviewer that drifters and the HFR used in Solabarrieta et al., (2016) capture different scales of motions and that the HFR measure averaged currents over $\sim$1m depth while SVP drifters with a drogue centered at 15m depth can measure different dynamics depending on the stratification. So we have removed the comparison with Solabarrieta et al., 2016 and we have added a discussion comparing our results with other studies using CODE type drifters which measure velocities at 1 m depth.

We have included this discussion in the second paragraph of section 5.2 ("Comparison of trajectories") "The values of D obtained in our computations after 24 hours of simulation are smaller than those observed by Molcard et al., 2009, Bellomo et al. 2015 and Kalampokis et al., 2016, for separation distances between real and virtual drifters trajectories. It is important to keep in mind that in these experiments the trajectories are computed from different source data (HFR and drifters) and sampling (spatial resolution) as well as different regions (Mediterranean Sea). However, the fact that we obtain smaller values of D is an indication that the performance of the three methods used here is very high, with better results observed for the SOM method as compared with OMA and DINEOF. “

Reviewer:

4) The data set you used consists in data collected in the same month (April) but in different year (2012, 2013 and 2014). Do you think that the results you obtained can be extended to the rest of the year? The gaps in your data are (more or less) constant during a year or there are periods when they are much more than the percentage you used in the paper? These questions are in the direction of ncluding in the text also a part where you speculate about your expectation about the use of the gap-filling procedures when you have larger gaps in your data set distributed during the year.

Response:

April has been selected for this analysis because this is a transition period between the winter months (where the persistent Iberian Poleward Current dominates) and summer months where low energy and stable conditions are observed (Solabarrieta et al. 2014). Applying the methodologies during the transition periods, characterized by high dynamical variability, allowed to avoid specific circulation patterns and ensure the results of this study are more easily generalized to other areas. However as pointed out in the Discussion-Conclusion section, further analysis are required to corroborate this affirmation.

Regarding the temporal gaps, a well-functioning HFR system should be working more than 80% of the time, with an optimum functioning and implementation higher than 90%. In this study, we have introduced gaps in the 50% of the time which is much higher than the 10-20 % of the estimated percent of time gaps in a well-functioning system. Nevertheless, this work is more focused in the spatial gaps and deeper analysis should be done in the future to analyze the effect of long time failures affection to the gap-filling methodologies analyzed in our work.

We have clarified these points adding the following paragraphs in the Discussion and conclusion section.

“The time period used here represents the dynamical conditions given in spring time (April). This is a transition period between the winter months (where the persistent Iberian Poleward Current dominates) and summer months where low energy and stable conditions are observed (Solabarrieta et al. 2014). Applying the methodologies during the transition periods, characterized by high dynamical variability, allowed to avoid specific circulation patterns and ensure the results of this study are more easily generalized to other areas. However further analysis are required to corroborate this affirmation.”

“In this study, we have introduced gaps in the 50% of the time period which is much higher than the 10-20% of the accepted time failures in a well-functioning system. Nevertheless, this work is more focused in the spatial gaps and further analysis should be done in the future to analyze the limit of applicability of each method regarding the temporal horizon.”

MINOR COMMENTS:

Reviewer:

1) Pag 4., the 3.1 paragraph starts with several methods, I think they are: some methods

Response:

We have replaced “several” with “some”.

Reviewer:

2) In the description of the gap-filling procedure, you indicated for all the methods, but not for DINEOF, which program/software/routines you used to apply the method. How did you run DINEOF?

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Response:

As noted by the reviewer DINEOF methodology was not sufficiently detailed. The following additional description about the source of DINEOF, and the technical details of how was applied have been added at the end of section 3.1:

The DINEOF Fortran code (http://modb.oce.ulg.ac.be/mediawiki/index.php/DINEOF) was applied to the combination of radial currents from the two antennas. The Matlab/Octave complementary sources provided with the Fortran codes are also used to produce cross-validation masks internally required by DINEOF, following the procedure proposed by Alvera-Azcárate et al. (2005). Like in the case of the above mentioned DINEOF applications, spatial locations with more than a 95% of missing data are not included in the DINEOF reconstruction procedure to prevent to negatively impact the quality of the overall reconstruction. The covariance filtering option proposed in Alvera-Azcárate et al. (2009) was also applied using the standard filter options proposed by these authors and setting the number of iterations of the filter to 12. The number of retained EOFs was high for all experiments (a minimum of 75), independently of the considered gap scenario (see Section 4)."

Reviewer:

3) In the section 3.2 you introduced the parameter Rn before the indication of what it represents. Please correct.

Response:

It has been corrected.

Reviewer:

4) Section 4 pag.10: Random gaps, you used 30% of the domain with random gaps. How did you choice this percentage?

Response:

This value of the number of gaps randomly distributed in the HFR coverage is chosen from the gap scenarios obtained in the K-means analysis (see Figure 2 and Figure S1 in the Supplementary Material). Looking at the K-means results it can be deduced that the pattern with a percentage of gaps larger than the 30% is associated with a radial distribution of gaps which is considered as failure of the antenna or even of the whole HFR system.

We have included the following sentence in section 4 at the end of the paragraph in page 10.

"(D) Random gaps: spatial gaps were randomly distributed in the 30% of the spatial HFR domain and in time. K-means analysis shows that the situation associated to the patterns with a percentage of gaps larger than a 30%, for this system, is likely due to a failure of one of the two antennas than to other causes. This last scenario represents a situation where data in selected locations have been removed (for instance after a quality control procedure based on a velocity or variance threshold, as recommended by the QARTOD manual, IOOS 2016)."

Reviewer:

5) Pag. 11: typo, correct quantity with quantify.

Response:

Thank you, it has been corrected.

Reviewer:

6) Pag 12: typo, correct BIASS with BIAS

Response:

Corrected

Reviewer:

7) Figure 5: DINEOF Exp C, the scatter plot shows many observations below the main cloud. This is the only case in which such behaviour is observed. Why do you think it happens? It could be useful to add this comment in the text.

Response:

The following discussion of why DINEOF produces such a result in experiment C has been added to the manuscript in section 5.1:

“ The scatter plot of original vs. reconstructed zonal velocities from DINEOF in the case of the Experiment C (Figure 5 second column third row) shows many points out of the main cloud. It suggests that the gap scenario C, where a very small number of observations is available (failure of one antenna), is the most aggressive situation for DINEOF methodology. Typically a 5% minimum threshold of available data is used in most DINEOF applications reported in the literature to accept a candidate for a potential reconstruction (for instance, a gappy satellite image, an hourly HF Radar current field, etc.). As described in Sec 3, such a threshold is also applied here as a preprocessing step before the technique is applied. The spatial field of the missing antenna is being reconstructed using the available data from the other antenna, as well as with data from previous and posterior time steps. Although part of the field might be acceptably reconstructed, it seems that there are some parts of the HFR image that are not being properly reconstructed. This result suggests that DINEOF should not be considered in situations represented in the scenario C, specially when the antenna failure is persistent in time.”

Reviewer:

8) Figure 7: I understand that for a best data representation, you used different y scales but please specify this in the text and in the figure caption as well. Also, please add in the figure caption a more complete description of the figure, for instance the error bars are not described but maybe a legend could help to quantify the variations as time increases.

Response:

We have clarified in the caption of Figure 7 why we use different limits in the y-axis and the error-bars.

Reviewer:

9) Pag. 17, typo, correct regimen with regime.

Response:

We have replaced regimen with regimen throughout the manuscript.

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