Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-26-RC2, 2018
© Author(s) 2018. This work is distributed under
the Creative Commons Attribution 4.0 License.

Ocean Science Discussions

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

Anonymous Referee #2

Received and published: 14 May 2018

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 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.

Printer-friendly version

Discussion paper

Main comments:

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.

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. . .

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

CC BY

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?

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 including 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.

Minor comments:

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

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?

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

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

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

6) Pag 12: typo, correct BIASS with BIAS

7) Figure 5: DINEOF Exp C, the scatter plot shows many observations below the main

CC BY

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.

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.

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

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2018-26, 2018.

Printer-friendly version

Discussion paper

CC BY