

Interactive comment on “Geostrophic Currents in the northern Nordic Seas from a Combination of Multi-Mission Satellite Altimetry and Ocean Modeling” by Felix L. Müller et al.

Anonymous Referee #2

Received and published: 18 September 2019

The paper describes a new dataset of altimetry and associated geostrophic currents, obtained by merging satellite observations and model simulations, and based on principal component analysis. This approach is a novel way to combine both sources of information and seems to produce consistent results.

I note a certain similarity between this approach and the DINEOF method (see e.g. http://modb.oce.ulg.ac.be/mediawiki/index.php/DINEOF_references) which fills gaps (clouds) in satellite observations using also a PCA method. However in the current paper, the EOFs are obtained from another source of information (a model) than the observations.

C1

The authors have carefully addressed the pre-processing required to merge the 2 datasets (offsets etc).

The paper is well written, generally clear and generally does not contain typos. I only have a few general comments or requests for clarifications. In the remainder of the review, by "dataset" I mean the new product that readers can download.

* page 2 line 7-11. The mentioned regions and main currents should be indicated on a map (e.g. Fig 2 that could then be moved higher in the text).

* page 9 line 12: write out NN. Only on a later page do we learn it means nearest neighbour (page 10 line 1)

* page 9 line 25 : it is not clear why the the description of the "individual steps" is provided separately. For example what is written on page 9 line 27-28, has already been mentioned explicitly before (page 8 line 15). Also the corresponding figure (5) is useless as it is just a zoom on a previous figure

* page 11 line 9-10: "contain" is misleading. Reading this, I could have the impression that the dataset contains the 2 things (satellite & model). By now, the reader has understood that the dataset is build using the 2 sources, but it (i.e. the netcdf file) does not "contain" them. Please rephrase.

* page 11 line 15: the fact that outliers in the results are rejected, has not been mentioned in the method description. We learn only now about it.

* figure 6, right column. Wouldn't a quiver plot (arrows) be more explicit than the colors for indicating the direction ?

* pages 11,13-15 : the authors compare the dataset with different other sources of DOT and currents: "processed" drifter data, original drifter data, ADT. About the second comparison, page 13 lines 16-19, please specify if the rmse computed directly between the drifter and the dataset, is taking as input the original drifter velocity, or is pre-processed (e.g. taking into account only the geostrophic part). Also, line 17, how come

C2

the RMSE is suddenly large (0.13 m/s) in this case, especially compared to the velocity itself ?

Finally, apart from the comparisons proposed in the paper, would it make sense to compare the dataset with the DOT obtained directly from FESOM ? In a perfect world, the dataset would even be compared with a data-assimilating version of FESOM, but I understand this is a whole new work and out of the scope of the article.

* page 17 line 6 : the comparison between "uncompressed" FESOM geostrophic currents and the dataset : what is the meaning of uncompressed ? Also in the article itself, please indicate clearly where you compared the pure FESOM outputs with the dataset, leading up to this phrase in the conclusion.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2019-102>, 2019.