Anonymous Referee #2Received and published: 18 September 2019

We thank the Reviewer for the careful and constructive comments. The suggestions and corrections have greatly improved the quality of this manuscript.

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.

Thank you for this link. We were not aware of this method. I fact, this is an interesting method, which is based on EOF as our approach is. However, one big difference is, that we do not use the altimetry observations for EOF analysis, since they are not available in fixed locations and with uniform epochs. In contrast, we use external data (a model) in order to fill the data gaps. That's why we decided not to mention DINEOF in the introduction of our manuscript.

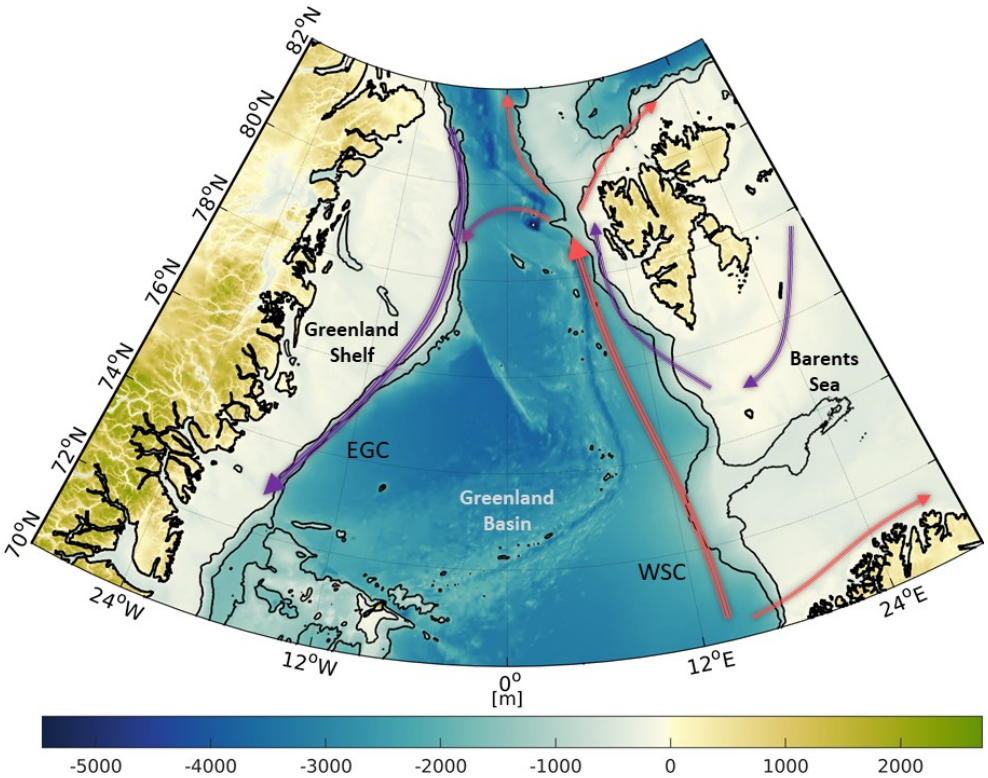
The authors have carefully addressed the preprocessing required to merge the 2 datasets (offsets etc). The dataset itself is easy to download and read (at least for users with experience of netcdf files, which is standard). Given each file is large (~2 GB), users who wish to download the entire dataset would maybe appreciate compressed netcdf files. Most services are switching to netcdf4 compressed files (e.g. CMEMS).

We follow the Reviewer's suggestion and compress the NetCDF files with the highest compression level, which results in a total reduction of about 14GB.

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).

We follow the Reviewer's suggestion and added a map containing major currents and bathymetric features to the introduction (see new Figure 1)



Bathymetry of the study area (norhern Nordic Seas, Fram Strait) based on RTopo2 topography model (Schaffer et al. 2016). Major current systems (West-Spitsbergen Current, WSC; East Greenland Current, EGC) are displayed by arrows in red (inflowing Atlantic water) and blue (returning polar water). Contour lines indicate depths of -450 and -1500 meters.

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

We changed the text, accordingly.

* 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

You are right that the list of individual combination steps is redundant information. The intention is to highlight the key processing steps in order to make it easy for the readers to understand the procedure. We think that the Figure is helpful for other, less experienced readers and decided to keep the figure and explanations.

* 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.

We agree and changed the text, accordingly.

The combined DOT and geostrophic current velocity fields are based on DOT heights from satellite altimetry [...].

* 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.

We agree and added some text to section 3.2. (step 4). Text passages in chapter 4 are now redundant and deleted.

New text chapter 3.2 (P 10 Line 5) :

Furthermore, an outlier detection based on an accuracy determination of the combined principal components is performed to reject erroneous combination estimations.

Deleted text chapter 4: (P 11 Line 15):

Furthermore, an outlier detection based on the accuracy of the computed combined principal components is performed rejecting erroneous combination estimations.

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

We tried quiver plots for displaying the flow direction. However, the unstructured grid is very dense (~1km), resulting in a lot of arrows reducing the readability. Therefore we decided to indicate the flow direction by using a circular phase map ranging from [-180° to 180°].

* 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 preprocessed (e.g. taking into account only the geostrophic part). Also, line 17, how come the RMSE is suddenly large (0.13 m/s) in this case, especially compared to the velocity itself?

We apologize for the confusion. We only use "processed" drifter data, which means only the geostrophic part of the drifter total velocity. We use a 6 hourly interpolated drifter dataset and reduce the individual observations by a-geostrophic (wind, wave) movements. In this second comparison, we directly compare the geostrophic drifter trajectories, which means the single geostrophic drifter observations, with the combination velocities. The numbers are higher than for the gridded comparison, because the individual drifter trajectories are characterized by a high noise budget and a strong variability.

Change text to P13 L16: *When computing the RMSE between the measured geostrophic velocities and the combined velocities based on the individual trajectories for each drifter, a mean of…*

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.

We don't think that a comparison of the new product with the original FESOM model is able to provide new findings. Of course, differences will exist, but the observations will not improve the model (for this purpose a data assimilation or calibration would be required, which keeps the model physics). This was not the aim of the study. We want to improve observation-based velocities, that's why we only include comparisons to observation-only products.

* page 17 line 6 : the comparison between "uncompressed" FESOM geostrophic cur-rents 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.

This sentence has been deleted, since it was wrong. There is no comparison between FESOM and the combined dataset.