

Earth Syst. Sci. Data Discuss., referee comment RC2
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Comment on **essd-2022-178**

Anonymous Referee #2

Referee comment on "SDUST2020 MSS: A global 1'×1' mean sea surface model determined from multi-satellite altimetry data" by Jiajia Yuan et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2022-178-RC2>, 2022

The manuscript presents the construction of a new global MSS model SDUST2020 with the resolution of 1'x1' from multi-satellite altimetry data, and evaluated its accuracy using several methods. Some of the novel features of the new MSS are longer timeseries and the use of J-3+S3A+HY-2 data.

Upon reading the manuscript I felt that there are serious uncertainties in the manuscript related to the method used to derive the new MSS but also to the evaluation, which needs to be addressed before it can be considered for publication. In many instances the authors Unfortunately, the manuscript suffers from very many sentences that are very difficult to understand which has made the review very difficult. I decided to recommend that the manuscript is rejected due to the following major issues:

Altimeter processing description: how were the altimeter data processed and potentially retracked. Were 1 or 20 hz data used for the derivation?. Which range and geophysical corrections were used?. Where the state-of-the-art tide model FES2014b is used consistently.

Averaging technique. Compared to the CLS15 and DTU18 which has a well-known averaging date of 01.01.2003, the averaging technique outlined from line 110 onwards is problematic. Here the 9 MSS models shifted slightly in time are averaged using the weighted average according to the reciprocal square of the estimated SSH error. In this process, 9 grids that are heavily correlated are averaged. Firstly, the correlations should be taken into account in forming the average. Otherwise, it will not be possible to determine an averaging time for the combined MSS. Without such the MSS is very hard to use. Secondly, there is no indication of what the estimated SSH error is and how it was derived.

The crossover adjustment is also severely problematic. When the author writes that the

crossover adjustment is carried out in two steps using 1) a condition adjustment method and 2) filtering and prediction along the track. I do think that the second step is a crossover adjustment and that the sentence is largely garbled. Also, the central quantity f is not defined in eq 4.

In the crossover adjustment, the authors select regions of 20 x 20 degrees. Dependent on the latitude all wavelength longer than the region will be absorbed (the a_0 term in Eq 4). At high latitude, this can be wavelength down below 1000 km and even lower depending on the number of parameters used in the adjustment. Please explain how the signal longer than say 1000km is preserved in the solution. Especially for the MSS at high latitudes. My gut feeling is that the adjustment must have been made in a remove restore fashion with CLS15MSS and that the SDSU consequently becomes a correction to this MSS. This would also explain the pattern of differences at high latitudes. Figure 5 onwards which shows that CLS15MSS and SDFU20 have the same voids at high latitude. If this is the case CLS15MSS should have been acknowledged.

interesting enough I noticed large discrepancies with other MSS models above 80N. This could fit with the fact that 80N is the northern limit of the 20x20 degree boxes, so data in the few degrees to the north of 80N are not adjusted?

The author process to perform something they call a self-crossover adjustment. I have never heard this word before. If what the authors perform is a moon mission crossover adjustment, this is problematic by several standards. First, the adjustment should have been performed as a multi-mission adjustment with the reference tracks of Jasons. Secondly, what is the usage, and interpretation of this adjustment except for the obvious that the numbers reduces. In principle, this has nothing to do with the MSS derivation unless derived errors are used for the following step. Why does the authors not perform a multi-mission adjustment with the reference tracks.

Section 5.1 present the comparison with CLS15 and DTU18 models. Here the authors present the central table 5 which is used to infer the accuracy of the models from high to low. In my view, it only explains that the authors are doing something wrong in my view. First of all the DTU15 and CLS18 MSS are not different on average by 1.27 cm Many investigations (e.g., Pujol et al. 2019) show much smaller numbers. The differences in Table 5 between the model's present standard deviation of >29 centimeters are clearly not what other authors present. Also, the following figure 6 demonstrates that the standard deviation is far less than 29 cm. I think that the authors have potentially forgotten to apply the confidence mask in the Arctic Ocean and elsewhere.

Consequently, the conclusions drawn in line 275 -280 are not correct because the numbers can be from a specific region (or even from land?).

A little later the authors also present the average and RMS about the formal error (again

garbled sentence) of 1 and 1.5 cm for SDSUT. What does this mean and how does it relate to Table 5.

The comparison with tide gauges is questionable. First of all, have the authors included the formal error on the MSS in this comparison and does the MSS fit within this?. Secondly, have the authors ensured that the same version of the reference ellipsoid (TOPEX vs WGS84/GRS80) has been used and that the version is employing the tide system?. In this section the authors only present numbers but no interpretation of the results. Is it realistic that the differences range up to nearly a meter (with a formal error of 1 cm claimed for the SDSUT).

Finally, we are in 2022. CLS and DTU have both released 2021 versions of their models.

Please note, that throughout I do not disagree with the fact that the SDSUT might compare favorably in the various comparison. This is in my view somewhat expected as longer time series are used in its derivation