Comment on essd-2022-111

Anonymous Referee #1

Referee comment on "Sea surface height anomaly and geostrophic current velocity from altimetry measurements over the Arctic Ocean (2011-2020)" by Francesca Doglioni et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-111-RC1, 2022

The authors constructed a monthly mean sea level and dynamic ocean topography dataset using data from CryoSat-2 observations. The main differences from previous studies (but not all) are that the data construction focused on intra-month variability, the handling of data bias in the sea ice region and open water, and the validation using time series data from mooring system observations.

Correlation coefficients and RMSDs are calculated using the CPOM DOT as a comparison, but the differences with the CPOM DOT do not lead to the conclusion that the authors' product is truly better. In particular, the sensors in the mooring system that acquire in-situ data are discrete in the vertical direction, and the steric height based on the linearly interpolated vertical profile is questionable. Also, the baroclinic Rossby radius is shorter than the L=300 km used in the first step of DIVA, which may be too much smoothing. Other comments are presented below.

Major comments:

1) Mooring data1

The authors state that they are generating sea level data for the entire Arctic Ocean, but the only mooring system data used as validation are those installed in the Fram Strait and Laptev Sea. For example, WHOI has deployed BGOS mooring systems with MMP in the Canadian Basin. If time series data are important, then all available mooring system data should be used.

2) Mooring data2

The authors calculate steric height using vertically discrete water temperature and salinity from mooring system observations. In the Arctic Ocean, where tilt pressure structures dominate, it is questionable whether the authors' method can correctly determine Steric height. Do the linearly interpolated vertical profiles of temperature and salinity reproduce the results of CTD observations made at the same time?

3) Reference ellipsoid (line 82 "e.g., WGS84".)

  From Figure 6d, I see that you used WGS84 instead of TP ellipsoid. Please specify somewhere that WGS84 was used in this study, not "e.g."

4) Sea ice concentration (line 107)

Isn't the 15% sea ice concentration a threshold to avoid so-called pseudo sea ice that misinterprets water vapor as sea ice, for example in the Bering Sea in summer, and doesn't it need to be 15% in the Arctic Ocean? Wouldn't the results be the same if this threshold were set to, say, 5%, 10%, or 20%? Isn't it usually the Waveform, for example Pulse Peakiness, that determines if it is sea ice or sea surface?

5) ADCP (line 180)

ADCP velocity data is averaged in the upper 50 m. Shouldn't the velocities within the surface Ekman layer be excluded? Also, there must be a momentum flux due to sea ice movement, so shouldn't the surface still be excluded?

6) High-pss filter(line 205)

Is there evidence that the high-pass filtered data is valid? If no, should it be excluded from the validation data?

7) Specific volume anomay

The reference depth the authors used is 400 dbar. Why? It is the depth of upper Atlantic Water. R. Kwok and other scientists used about 750 dbar (deepest depth of ITP).

8) Figure 2b

The INSET PANEL in Figure 2b is difficult to understand. How about color-coding the grid points by AWI and RADS?

9) FIgure 4 inset panel

The color of the inset panel in Figure 4 indicates the number of crossovers, which basically increases as you go to higher latitudes since these are polar-orbiting satellites.
If this inset panel is independent and the color of each season indicates the difference of η at the crossover point, it is easy to understand where and in which season there is a difference.

10) Spatial resolution

Is the resolution setting just following the CPOM DOT, or if you want to differentiate yourself from CPOM, is there some strategy to change the resolution? At the moment, it is no different from CPOM except in the Siberian Sea.

11) lines 358 & 360

I don't understand it because there is no detailed explanation of where the 4.2 cm and 8.2 cm came from.

12) Local gradients between 7W and 4W (line 462)

The authors used L=300 km, so they just applied too much smoothing. Why not take into account the bathymetry and reduce the value of L if there are steep velocity changes in the horizontal direction, for example?

13) Figure 9a

I guess relatively low correlation is due to incorrect steric height based on linear interpolation.

Minor comments:

Line 82 : e.g, --> e.g.,

Line 129: CPM --> CPOM

Table 3: The table shows FES2014, but the caption describes it as FES2004.