Comment on essd-2022-181

Anonymous Referee #1

Referee comment on "Improved global sea surface height and current maps from remote sensing and in situ observations" by Maxime Ballarotta et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-181-RC1, 2022

General Comments

This paper begins by presenting a new gridding method for producing maps of currents and sea surface height by combining data from altimeters and measurements from drifting buoys.

The method was already proposed in a previous work published by one of the authors of this paper and tested using an Observing System Simulation Experiment (OSSE) and Observing System Experiment (OSE). Here the method is applied for the first time to real data and the results appear to be quite interesting.

In its current form, the article also includes a very long description of the mapping method that has already been published, which, at the same time, is also too short for readers unfamiliar with the mathematical details of the discussion. My suggestion is to move section 2.2 (methods) to an appendix leaving in the main text only a qualitative introduction to the two gridding methods. This will also give the opportunity to add some missing information, such as, for example, justify the choice of covariance function or the limit to 1000 observations, which I assume is the result of several trials.

The major merit of this paper is to propose the combined use of al the useful and available data (altimeters and drifters) to obtain an improved product for the global ocean circulation also in view of the future missions based on large swath technologies. Even if the actual improvement of the currents and seal level is not very impressive, I am convinced that the method and the strategy of using data form very different platforms is more than promising. In this sense, I would also be curious to know how far this new interpolation method is from being used in an operational context such as CMEMS.

Overall, I would say that it is a good paper that deserves to be published doing some revisions as suggested in this review

Recommendation: minor to major revisions

Specific Comments

Section 2.1, table 1: date interval in the table "20160115-20200630" please put a space or any other kind of separator between year month and day (this applies for all the dates in the paper). In the same table also add "degrees" in the spatial coverage line. And also define AOML.

Section 2.1.1 line 79-80: Add a reference.

Section 2.1.1 figure 1: How many altimeters are included in these 7 days period?

Section 2.1.2, line 89: The reference to Prandi et al. is not in the references section. This is not the only missed reference, please check the reference section.

Section 2.1.2: probably some of the readers might be interested in understanding how the altimeter can measure sea level in ice-covered areas. Can you add few words about this?

Section 2.1.3: Really a lot of model-based corrections! How much better is this geostrophic estimate than using geostrophic currents directly derived from models from their sea level elevation estimates (when produced)?

Section 2.1.3, figure 3: no drifters in the Mediterranean Sea in 2019?

Section 2.2.1, line 143: Duced et al 2000 in not in the reference section

Section 2.2.1, it would be interesting to see the covariance function. Also, Arhan and Colin de Verdière (1985) in not in the reference list. Definitely the reference section needs to be carefully reviewed!

Section 2.2.1, line 176: "(in this study N=3". The second parathesis is missing.

Section 2.2.1, lines 221-222: "the result strongly relies on the choice of covariance models". Once again, if this choice is so important, I suggest to show your choice.

Section 3.1, line 290: "geostrophic current anomaly data from AOML drifter database"
How "geostrophic currents" are computed from drifter (by the way lagrangian) velocities?

Section 3.1, line 293-294: what is the criterion used to select the 20% to be excluded?

Section 3.2, line 315: the mentioned "geostrophic velocity errors" refers to the intensity or to a specific component?

Section 3.2, lines 333-334: The criterion used to determine the effective resolution is not justified. If not an explanation at least a reference is needed. Moreover, can the slope of the PSD contribute to determine the effective resolution?

Figure 6: Why not show the two variance maps as well?

Table 4: Perhaps you need more digits to appreciate differences of less than 1%? Is that reasonable? Why can you say 0.0% for the Arctic and -0.8 for the equatorial belt when you read the same numbers in columns 2 and 3? Of course, this question applies also for the other tables.

Section 4.2.1, Geostrophic current quality: "Overall, MIOST surface velocities are slightly closer to drifter velocities than the DUACS surface velocities." can it be said that MIOST is closer to the drifters also because it applies a kind of assimilation of them?

Table 6: It would probably be interesting to show the error for velocity intensity as well.

Section 5: Ubelmann et al (2020, 2021): 2020 or 2016? Once again control the reference section.