Reply to Referee #1:

Authors response (AR): We would like to thank referee #1 for his/her review and for the suggestions to improve the manuscript. We respond below to the referee's comments.

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.

AR: As suggested by the reviewers, we moved the technical description of the DUACS and MIOST mapping approach to an Appendix section, leaving in the main text our motivation for comparing the DUACS and MIOST approaches. Section 2.2 has been rewritten.

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.

AR: We corrected it in the new version of the manuscript

Section 2.1.1 line 79-80: Add a reference.

AR: We added “(see Taburet et al., 2021, for the reference associated to each mission corrections)” in the updated version of the manuscript.

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

AR: We mentioned the number of altimeters in the updated version of the manuscript

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.

AR: This has been corrected in the updated version.

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?

AR: Sentences have been added in the introduction of the section

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)?

AR: The experimental design proposed in this study was mainly motivated by using only observation as input datasets. We are aware that each dataset (from model or observation) has its limitations (residual error in observations or models, constrained scales in models, …). Here, we wanted to keep/constrain the part of the geostrophy signal that is consistent between two sources of observations: the altimeter data and the insitu drifters’ data.

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

AR: There is no drifters in the Med. Sea because the Ekman correction is not available for the basin. Consequently, we didn't include the available drifters in the analysis. We added a sentence about it in the description of the drifter's dataset: "The Ekman component is not available in the Mediterranean basin, so there is not drifter used in this region for the study."

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

AR: Corrected in the updated version

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!

AR: We added the reference and the formulae of the Ahran and Colin de Verdiere covariance model.

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

AR: Parathesis is removed

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.

AR: Examples of covariance models for geostrophy and equatorial waves are provided in section 2.2.2 of the manuscript.

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?

AR: the geostrophic current computation is mentioned in the section 2.1.3 with equations 2 and 3, and it is also described in the section.

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

AR: No investigation was conducted to find specific criteria to justify the choice to exclude 20% of drifters. Drifters were excluded randomly. We needed to exclude enough drifters to be able to account for enough data in the mapping while having enough independent data for validation.

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

AR: It refers to the zonal and meridional component: we reformulated as follows: "The similar statistical analysis can also be performed on the

geostrophic velocity errors Uerror = Umap- Udrifter, for the zonal component, and Verror = Vmap - Vdrifter, for the meridional 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?

AR: We added a reference: " As in Ballarotta et al. (2019), the effective resolution is then given by the wavelength λs where the SNR(λs) is 2 (Equation 25), i.e., the wavelength where the SSHerror is two times lower than the signal SSHalongtrack."

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

AR: We did not show the two variance maps (MIOST and DUACS) because they are relatively close at 10% as shown in the manuscript figure. We propose to update the figure by including the two variance maps and their differences.

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.

AR: It is right, more digits are needed to appreciate the differences less than 1%. We corrected all the tables in using numbers with more digits and using cm unit instead of meter.

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?

AR: This question is difficult to answer. By construction, the MIOST maps will be closer to the 20% drifter's dataset used for the validation because the physical content of the assimilated drifters is somehow "injected" into the MIOST maps whereas the DUACS maps do not assimilate drifter data. On the other hand, the comparison of MIOST allsat-1 and DUACS allsat-1 experiment (provided in the manuscript) which do not assimilate drifters shows that the MIOST maps remain closer to the actual drifter data than the DUACS maps. So even without drifter information the MIOST surface velocities are slightly closer to drifter velocities than the DUACS surface velocities

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

AR: The table on regionally averaged mapping error variance and gain/reduction of error variance for the surface currents between experiment MIOST allsat-1 and MIOST allsat-1 80% drifters + equatorial waves+ L3 arctic is shown in Figure 7

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

AR: References are corrected in the updated version