

Interactive comment on “Copernicus (CMEMS) operational model intercomparison in the western Mediterranean Sea: Insights from an eddy tracker” by E. Mason et al.

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Received and published: 1 May 2019

We thank the reviewer for their helpful input. We also wish to apologise to the reviewer regarding the incomplete state of our initial submission. This we discovered was because we submitted the wrong pdf, and neither we nor OSD picked up on that mistake. The fault is entirely ours. We appreciate that this made reviewing the submission rather challenging for both the reviewers.

We mark our replies below with —

Anonymous Referee #2 Received and published: 3 April 2019 General Comments: In

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this manuscript, the authors applied an eddy tracker and an eddy compositing technique, to three forecasts models: (i) the Mediterranean Forecasting System (MFS), (ii) the Global Mercator model (GLO), and (iii) the Iberia-Biscay-Ireland system (IBI), provided by the Copernicus Marine Service (CMEMS) website. They used the py-eddy tracker, an eddy detection and tracking method similar to the procedure proposed by Chelton et al. (2011). This method is widely used by the scientific community. The eddy compositing analysis is applied to analyse the 3-D structures of three gyres of the Alboran Sea (WAG, EAG, CRT), an interesting area of the Western Mediterranean, characterized by strong density gradients and high mesoscale activity. The authors compared the models results to evaluate their performance and conclude the paper with some technical suggestions in order to improve these CMEMS products. This paper is well structured and the methodological approach is correct for the purpose of the journal. Nevertheless a more detailed description of the methods and additional results are needed to support the interpretation and conclusion. Therefore, at this stage, a major revision is required before acceptance. Please see below for more details:

Major remarks: 1) I found the Section 2.4 (Eddy compositing) hard to follow. The methodology used for the 3-D eddy composites and for the computation of the anomalies of T and S is not adequately explained. The authors refer to the article of Mason et al., 2017 (see pg. 8 line 15) but it is applied in a different context and in a different area. ————A full description is now included, which includes reference to figure 2.

2) The eddy properties, detected by py-eddy tracker applied to the three models and to the ALT data, are not adequately compared (section 3.1). In Section 3.1.1, the results on the numbers of eddies detected and tracked in relation to their lifetimes are not quantified. In Section 3.1.2, the authors have identified similarities “between the patterns” of eddy radius, amplitude and intensity (pg. 10, l.16, pg. 11, l.1 and l.11). These may be due to the strong signal of the Atlantic Water flowing eastward, which generates the large and energetic Anticyclonic Eddies. This flow causes differences in terms of eddy properties between the northern and the southern part of the basin.

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Pessini et. al, 2018 identified these N-S differences as function of the area of formation and lifetime. Escudier et al., 2016 compared the eddies distribution per radius and lifetime, of altimetry and model data. I suggest the authors to follow the last approach since, in my opinion, more detailed comparison between the models and ALT data are needed. ————We have added several lines to the Introduction about the reasons for the N-S differences in the WMED.

3) The authors did not describe how they calculated the mean and median coordinates/radii of the eddies, listed in Table 2. There are various methods to calculate them for example: (i) selecting all the eddies detected by the eddy tracker independently for lifetime, (ii) discriminating for lifetime (i.e. avoiding the shortest) and (iii) selecting only the eddies identified simultaneously by the three models in the same sub-area. The 3-D eddy composite analysis should depend on these variables (pg.3, l. 10 to l. 22) and therefore should be method-dependent. Some results (pg. 18, l. 11 to l.12) and conclusions (pg. 24, l. 13 to l. 21) are based on vertical structure of T'. The authors actually cannot demonstrate “how closely these eddy composite results may correspond to reality” (pg. 26, l.10-11). For these reasons, a check to verify if the 3-D subregion eddy composite results are method-dependent should be done. Alternatively, I suggest the third method because is the most appropriate to compare the model results. ————These statistics were calculated using the second method suggested by the reviewer, where the minimum eddy lifetime (5 days) is set as an input parameter to the eddy tracker. The third method is not practical because IBI does not have data assimilation so there is no meaningful correspondence at all with altimetric eddies. Lastly, we are working on providing a reference for the composite T and S profiles using Argo floats.

4) In Table 2, please provide the mean and median coordinates and the properties of the eddy for the altimetry data (section 3.2). This will allow a comparison among the models and the altimetry data. ————The altimetry information has been added to the table.

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5) Table 3 is not acknowledge through the text. The variables Rmin, Rmax, Rmean, Rmedian, Rmad are not defined. Some values are missing and other seem to have no sense. Specifically, in “Product: MFS” at line “CRT” are listed the values: 111, 222,333, 444, 555 which seem to be out of scale. In Section 2 (Data and methods) the effective and the speed-based (inner) radii are indicated respectively with the symbols L_e and L while in Table 3 are labelled as R. ———Table 3 has been removed as it was redundant.

6) In Figure 10, the seasons are not mentioned. In winter and autumn, the deepening in the mixed layer depth (MLD) in the center of the anticyclonic eddy increase from GLO to IBI (fig. 10). This seems coherent with the zonal and meridional relative vorticity of figures 7 and 8, having more intense anomaly for higher resolution models. I assumed that these differences in the MLD inside the anticyclonic eddy are due to rotational speeds, better simulated in these higher resolution models. Anyway the authors assert that “the incoming Atlantic Jet in IBI is suspected to be too strong such that these ζ values may be an overestimate (pg. 16, l. 2-3)”. The last sentence should be motivated and this part must be clarified also because these considerations could be used to evaluate the performance of the models (pg. 24, l. 22) and to provide a more consistent conclusion. ———The text in the MLD section (3.2.6) has been modified. Labels for the seasons in figure 10 have been added. Concerning the statements about the Atlantic Jet in IBI, we added a reference to the CMEMS MedSub project report which contains more information about this anomaly.

Minor remarks: 7) In Section 2.1 (pg. 5, l. 10-11), the authors state that the variables, for the period 1 January 2013 to 30 June 2016, are downloaded by ftp from the Copernicus CMEMS portal from: GLOBAL_ANALYSIS_FORECAST_PHY_001_024 (section 2.1.1), MEDSEA_ANALYSIS... (section 2.1.2), IBI_ANALYSIS... (section 2.1.2). However, the dataset mentioned above provides values just from 2016. Therefore, I supposed the authors used the dataset: GLOBAL_REANALYSIS_PHY_001_030, MEDSEA_REANALYSIS_...

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IBI_REANALYSIS. . . . In the last case, please check the sentence "(based on a 3DVAR scheme)" in Section 2.1.1. For MED- SEA_REANALYSIS_..., it should be substitute with "(based on a OceanVAR scheme)". ————No, we did not use the suggested *REANALYSIS* products. The products we used were the ones that were available at the start of the MedSub project in March 2016. The GLOBAL*_001_024 data are still available for the period 2006-2016 by using the motu_client software, rather than using the subsetting option on the CMEMS portal. ————We changed the text to "based on an OceanVAR scheme" as suggested.

8) In the Table 1, please check : i) Column 'MFS' , Line 'Resolution': 1/16â (âij7 km) or 1/16â (âij5-6 km); ii) Column 'GLO', Line 'Topography': should be "GEBCO8>200 m, ETOPO1<300 m" instead of "GEBCO8<200 m, ETOPO1>300 m"; iii) Column 'IBI', Line 'Topography': "GEBCO, ETOPO, ??? "???. Please replace the ??? with actual Values. ————We believe what we have is correct regarding the topography; the information was taken from the CMEMS GLO website. We will confirm this in the new manuscript.

9) In the text the variable ζ/f is sometimes indicated as the normalized relative vorticity (pg. 8, l. 21) and sometimes as normalized relative vorticity anomaly (pg. 15, l. 11). In the latter case it should be labelled as ζ'/f . ————'Anomaly' has been removed in all cases.

10) Pg 3, l. 11: Please add more recent references (Escudier et al., 2016; Pessini et al, 2018) because they deal with eddies properties in the Algerian basin detected by eddy detection and tracking algorithms. ————These references have been added.

11) Pg. 3, l. 26 Please check the sentence: "(Results from other sub-regions in the WMED are included in Supp. 2.5)" because in the Supplementary materials I did not find the Supp. 2.5 and in general the "Results from other sub-regions". ————These will appear in the new manuscript.

12) Pg 13, l. 14-15: Please provide a reference for the sentence: "This sea is the

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most energetic region of the western Mediterranean". ———We added a reference to Pascual et al 2007 and Capó et al JPO 2019.

13) Pg 13, l. 27: I did not understand what the authors mean with "In the WAG the eddy positions are in the deepest waters in the center of the gyre". ———We changed this sentence to: "In the WAG, the median eddy positions for each model are located in the center of the gyre, which corresponds to the deepest water."

14) Pg 3, l. 5 and l. 13: Please substitute)) with) ———Done.

15) Pg 6, l 4: The subject is missing. The MFS is produced by the Mediterranean Forecasting System (Italy). ———This has been corrected.

16) Pg 8, l 10: Some variables, for example nonlinearity and eddy intensity, are mentioned before being declared. Therefore, I suggest to substitute the sentences from line 9 to 14, at pg. 8, with "these eddy properties are nonlinearity ($N=u/c$, where c is . . .) and eddy intensity ($EI=A/L$). N provides a measure of . . . EI is a potential proxy for the presence of elevated vertical motions (e.g., Frenger et al., 2015; Mason et al., 2017)". ———Done as suggested.

17) Pg 10, l 5: Please substitute Sec. 2.6 with Sec. 3.2.6 ———Done.

18) Pg 20, from line 5 to line 9: Please delete the sentence "Both anomalies T' and S' in the EAG are slightly weaker than those of the WAG. . . . 50 and 150 m". This is the repetition of the sentence at pg. 20, line 1 to line 4. ———Corrected as suggested.

19) The text appears to be incomplete. I found many question marks through the text. Please replace them with actual text. Pg 9, l. 9: Supp. ?? Pg 13, l. 13: Supp?? Pg 14, fig. 5: nonlinearity in i through p are shown in Supp. ??. Pg 16, l. 26: See Sec. 2.5 and Supp. ?? Pg 22, l. 2: see Figs. ?? and S?? in Supp. ??. Pg 24, l. 24: Supp ?? Pg 24, l. 25: Figs ?? ?? ———The text was indeed incomplete owing to our error with the pdf. These issue will all be corrected in the new manuscript.

References

Capó, E., A. Orfila, E. Mason, and S. Ruiz, 2019: Energy Conversion Routes in the Western Mediterranean Sea Estimated from Eddy–Mean Flow Interactions. *J. Phys. Oceanogr.*, 49, 247–267, <https://doi.org/10.1175/JPO-D-18-0036.1>

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Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-169>, 2019.

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