Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-169-RC1, 2019

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

Anonymous Referee #1

Received and published: 30 March 2019

General Comments:

In this study, the authors present an eddy tracking tool to investigate the eddy properties derived from three different MFC reanalysis products. A gridded altimetry dataset is used as reference. The main argument for the added value of this study, is that such a tool is useful for model intercomparison, with the objective to evaluate model performance.

The manuscript shows some interesting results regarding the eddy properties for the three different MFC reanalysis products, focusing on the western Mediterranean between Gibraltar and Sardinia, an area governed by intense eddy activity. In my view,

Printer-friendly version

Discussion paper

this model inercomparison is valid with such a tool and quite interesting. However, without references to make a proper comparison it is impossible to evaluate model performance between the MFCs, let alone to propose remedies for their implementation. This is a major constrain in the way the motivation of this study is posed. I would advise the authors to reshape their motivation, but not necessarily limit their analysis only in the model intercomparison. For instance, I would like to see a more comprehensive analysis for the differences between products, based on attribution to physics and on the knowledge of the region's dynamics, in order to bypass the lack of a reference dataset. They only thing that the authors should be careful is to clearly stress that they make fair assumptions on how they foresee the future improvements for those MFCs, based on the findings of the eddy tracker. After all, some of the co-authors are leading the developments in those MFCs and have a good knowledge of the systems they operate. In the same line of thinking, the draft is too descriptive focusing on the eddy properties and the model intercomparison, without the authors attempting to discuss in details differences in physics between the MFCs products.

Finally, one additional problem with the manuscript is that it is sloppily written, as if it was copied/pasted in a rush manner from another draft or report. The figures are not properly introduced in the text and I had to guess throughout the whole review in which figure the authors are referring to. Overall, I find the manuscript worthy of publication in Ocean Science, after a major revision. Please find below a list of comments following the flow of the text, that the authors should address in the revised manuscript.

Specific comments:

1) In this study, an eddy-tracker modelling tool is used to evaluate other models. How confident the authors are that their approach is valid and what are the limits of the method? In the Introduction section I felt that there are not enough references to link this approach with previous studies.

2) In the Abstract, the authors stress the fact that "This information can be informative

for the ongoing development". I agree and I was eager to see such an analysis in the Discussion and Conclusion section, but in my view the authors failed to do so. I think that such an analysis is important and the authors should provide a more comprehensive and detailed discussion.

3) Page 2, line 16: What is "STA (2017)"?

4) Page 3, line 7 and elsewhere in the text for all the figures: "Fig. 1.1". This is not the proper format to mention the figures and should be changed everywhere in the text. In addition, I 've noticed that some figures should have been mentioned earlier in the text with respect to other figures, or other figures are not mentioned at all (e.g. Fig. 2 seems not to be mentioned in the text, unless it is Fig. 2.1, but then should be mentioned earlier). In addition, all figures in the supplement material are referred with question marks, and it was impossible to understand which is which to the point that it become annoying.

5) Page 3, line 21: "that extend a horizontal distance of 8x the eddy radius". Why? It seems to me that an explanation or a reference is missing here.

6) Page 5, lines 12-13: "Delivered products are bilinearly interpolated onto a regular lon/lat 1/36 grid". This is a major issue for the analysis that follows. Why to interpolate coarser MFS and GLO products into the higher resolution IBI and not leave it as is or make a coarsening of the IBI and MFS to GLO resolution? I guess this is also linked to the tuning of the eddy tracker, but an explanation is missing here and this fact compromises the findings of this work. The authors should elaborate us regarding this pre-processing interpolation. Perhaps, as a supplement material the authors can provide one example for the opposite technique to coarsen IBI and MFS into 1/12 GLO resolution and perform the eddy tracker (with same/different tuning?).

7) Page 5, lines 13-17: "The stated general...ocean dynamics". This is a very general statement that is redundant in this section. Remove or include it in the Introduction.

8) Table 1 IBI ETOPO definition is presented with question marks and is missing.

9) Page 8, lines 1-3: "The same... will differ". How valid is the fact that the authors use the same tuning for different resolution products (this is also related to the pre-processing interpolation at 1/36; see my previous comment 6).

10) Page 8 in section 2.3: the definitions Le, A and EI should all be declared one line earlier I think.

11) Page 8 section 2.4: Is the composite grid at 1/36? Calculations of dynamical properties (e.g. vorticity) are first calculated on the native MFC regular grids as delivered through the CMEMS portal and then on 1/36? Clarify in the text.

12) Page 10, line 5: "...and their range variability is shown in Sec. 2.6". We are in Sec. 2.6, what I have missed here?

13) Page 13, line 25: "...property anomalies of the...". Anomalies with respect to what? Clarify in the text. The term "anomalies" is also used elsewhere in the text and a clarification is needed there as well.

14) Pages 15-16 "The model increase can be explained by the increasing model resolution". Bad phrasing. Rephrase in the text.

15) Page 16, lines 2-3: "in IBI is suspected...may be an overestimate". Why do you suspect that, is there any reference or an explanation?

16) Page 16, line 26: "...at each vertical level". Is there some sort of vertical re-gridding in the post-processing for the different products pertaining to the fact that the MFCs have different vertical grids? Clarify in the text.

17) Page 18, line 2 (and Table 2): "...are around 9% smaller than the means...". The radii median is always found smaller than the radii mean. Does this means something for the eddies properties, shape and distribution?

18) Page 22, lines 19-20: "As shortwave solar radiation increases from spring to sum-

Printer-friendly version

Discussion paper

mer (Ruiz et al., 2008),...". Do we really need a reference for this statement?

19) Page 24, lines 15-17: "The most likely hypothesis...acts to cool the surface mixed layer". Why not Data Assimilation, how can you tell? How do we know that IBI has the best performance in terms of upper ocean temperature due to tides, when the other MFCs include SST DA? I guess you should rephrase this part of the text and include DA in your fair assumptions. In addition, in the continuation of this paragraph the authors attempt to make some recommendations to MFCs, based on tides and bulk heat fluxes. I am OK with that, but I think there are also other factors that should be mentioned here, such as vertical parametrizations, turbulent closer schemes, vertical stretching and number of levels (especially when the MFS has increased by a lot the number of vertical levels in the new version, hinting perhaps to better performance?) etc. Finally, suggesting that the GLO MFC should have tides is a logical but not straightforward suggestion, because of the complexity in doing so, and because the area under investigation in this study is very small to derive any strong conclusion for the global configuration.

20) Regarding the authors comment for the ARMOR3D product in the Discussion and Conclusion section, I agree that without such products is difficult to assess how close to reality the eddy composites are. However, even if you had this product on its coarse resolution (if not mistaken at 1/4 of a degree?), that would still not be sufficient for your domain of application, so I guess the ARMOR3D product for regional applications should be delivered on a higher resolution grid? Perhaps the authors want to comment on this.

Best regards.

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