Geoscientific
Model Development
Discussions

Interactive comment on "Verification of the mixed layer depth in the OceanMAPS operational forecast model" *by* Daniel Boettger et al.

Anonymous Referee #2

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general comments

The presented study proposes an evaluation, during the autumn (April-July 2016), around Australia, of the forecast Mixed Layer Depth (MLD) simulated both by the new and old version of the operational forecast systems of Australian Bureau of Meteorology. To validate, the authors use data developed in the framework of the GODAE Ocean View program. They propose a very precise description of different oceanic stratification cases that can induce changes in the calculation of MLD. Then, both systems are evaluated on their ability to predict the MLD. To finish, a discussion is proposed around the impact of the model changes and around definition of the MLD and its sensitivity to the thresholds. I have found this paper very interesting with very good point as the choice of the data to validate (a shared data base). This give the possibil-

ity to others groups reimplementing the proposed diagnostics. The particular attention to describe the problematic of the MLD determination (the different cases depending on the temperature and salinity variations) is convincing and very well explained. The quality of the graphs is good. My major comments are i) on the very short period used to perform the study compare to some affirmative sentences (and title of the paper) ii) for most of the time, the attribution of the observed changes to the vertical mixing scheme without clear proofs. (A.1) About the first point, I understand this prototypes are huge and running long tests is quite impossible. But, I think in these cases, it is not correct speaking of verification without mentioning the study period. This study occurs in austral autumn and stratification/destratification events will be completely different compare to other seasons. The authors should moderate some affirmations, which could change during other periods, and precise more often the study period as in conclusion, title, . . . (A.2) About the second point, I aslo agree that the vertical mixing scheme should play a major role but regarding the list of changes between both version of systems (assimilation method, cycles of assimilation, mixing scheme, bathymetry, horizontal grid, version of the code ?), I think it's quite impossible giving strict affirmations on its major or minor impact. All the listed changes mention in table 1 are major. The authors should use the words of "we suppose", "we suspect",. . . On this attribution of the changes, the authors give some justification but not always convincing. See the specific comments in the following part for more details (B.1).

specific comments

(B.1) Some discussion to attribute the major part of observed change to the vertical mixing are not enough convincing. The authors should moderate some affirmations. For example, page 6 line 5-10, the paragraph that explains the new grid has no impact, is not so clear for me. The authors base their argumentation on MAE and not on normalized values. On the figure 2, we observe that the observed MLD is zonal. For this reason, I think this is normal to have the same zonal pattern on MAE even if the grid changes. But a MAE of 20m is not the same for an MLD of 200m than for an

MLD of 50m. A map with, percentage of error should be more convincing. Or page 7 line 10-14, I think the affirmation is too strong. The Figure 6 present the skill of both systems. In each bands, the new system performs better than the old one at T+0h. I think, it demonstrates the importance of the change of the cycles and data assimilation scheme. Therefore, the profiles presented in figure 8 or figure 9 could be a consequence of the "assimilation work" and not of the vertical mixing scheme ?

(B.2) For model description, the authors report to (Oke et al., 2013). Despite I think a short description of the vertical grid could be useful because it is a model characteristic of prime importance for the representation of the MLD.

(B.3) In (Oke et al., 2013), the presented simulation is performed with (Chen et al. 1994) algorithm. I wonder why it was decided to change the vertical scheme in the operational system and why you have chosen k-epsilon formulation. Have you performed some studies with idealized, 1D or with lower resolution model as for example in Reffray et al. 2015 ?

(B4) The vertical mixing scheme is briefly described. Could you add some information as: i) Have you some modifications/parameterizations of GOTM tacking into account the effect of waves (change of roughness depending on wind field, mixing induced by breaking waves)? A such parameterization could have a strong impact on vertical mixing, especially in ACC. ii) On the possible impact if stability functions are included (see for example Burchard and Bolding, 2001)

(B5) In this paper, a few words about the possible impact of a Sea Ice model would have been interesting. Somme comments about this point would be interesting, especially the impact during the austral winter in the south of the domain ?

(B.6) Have the authors evaluated the changes induced by the modification of the bathymetry data base and/or the grid? The circulation in the north of the study domain is strongly driven by the straits and their representations in the model. If there are changes in the circulation, it could explain differences particularly in the North-West of

the domain (outflow coming from Indonesia).

(B.7) The affirmation page 5 line 10 (RMSE between 0.2-0.7) needs a reference.

technical corrections

(C.1) in legend Figure 5 there is a report to a "section 0" ?

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