Ocean Science
Discussions

Open Access

OSD

Interactive
comment

Interactive comment on "Recent updates on the Copernicus Marine Service global ocean monitoring and forecasting real-time 1/12° high resolution system" *by* Jean-Michel Lellouche et al.

L. Vandenbulcke (Referee)

luc.vandenbulcke@ulg.ac.be

Received and published: 12 April 2018

The paper entitled "Recent updates on the Copernicus Marine Service global ocean monitoring and forecasting real-time 1/12° high resolution system" presents the innovations in the near-real time global PSY4V3 system simulating 2006-present, compared to the previous system. The paper then validates the innovations by looking at essential ocean variables. The discussion of the improvements is generally convincing. The model itself is actually very convincing, and some of the examined metrics are truly impressive with very low errors. The paper is also very well written and clear.

I have a small list of remarks / questions, in no particular order:

- to facilitate the reading of the paper, please make the names of the systems consistent through the whole text and figures (PSY4V3 or HRG-V2V3, etc)

- section 3.2 about correction of precipitation. It seems that figure 6 represents ECMWF (left panel), and ECMWF corrected by PMWC (right panel). In this case, of course in the right panel, the differences compared to PMWC are smaller than in the left panel. Thus Fig 6 is not very relevant and could be removed. On the contrary, Fig 7 shows the impact of the correction, and is very convincing.

- section 3.3 about assimilating climatological profiles at depth, starts by giving a list of possible causes for this drift. Could you expand this a little? One would like to understand why below 2000m, the model would drift and present larger and larger biases over time (such as written page 16 around line 21, and illustrated in Fig 11a); as this is kind of surprising and one wonders if there is something that can be done to the model itself ?

- section 3.4, page 19 line 32, about the filtering of SLA anomalies, and the trapping of small structures, you say "this happens less" when filtering. Can you show this ? It is also surprising that there seems to be no clear advantage of 10 or 300 passes of the filter. Does this tell something about the spatial scales ?

- section 3.5, page 20, lines 5-8: you say the "error increases" when TIW are marked, and this can be explained by cloudy images or by the model shift of TIW. This could be more clear. I don't understand why images would be more clouded when TIW are more marked, is there any relation between these 2 processes (TIW and clouds). I agree with the second reason. In an ideal world, the error on model (resp. observations) would be determined without using the observations (resp. model); but the world is not ideal, and that the "Desrozieres" method is effective and hence should indeed be used to improve the model. Therefor, if the model shifts structures (such as TIW), one indeed may need to modify the error affecting observations. If this is what you meant, maybe somewhere in the section you could write such an introduction.

- for the whole of section 4, or maybe in section 1 or 2 already, you could specify clearly and once and for all if there are differences between the catch up period (2006-2016) and the operational period (2016-ongoing) ? I mean for atmospheric forcings, in situ observations reprocessing mode, etc. For example in section 4, at one point you say that in Jan 2014, you start using NRT observations (if I understood correctly) ? Does that mean that before 2014, you used reprocessed observations ?

- section 4.2 in particular seems to indicate that the system relies a lot on data. But this feeling is present in the whole paper. Actually almost all of the improvements to the PSY4V3 system seem to be data-based, whether they concern data assimilation, error modelisation, atmospheric forcing data (precipitation)... This is not a criticism, just something that I notice. Maybe it becomes extremely difficult to improve the model itself any further, apart from forcing it with more and better data.

- page 25 lines 1-5, you talk about 3.2 mm/year. But I understand from the previous sections that the mean SSH is not allowed to evolve freely, but is forced to increase 2 mm/year. Can you clarify this ?

Very general comments

- Among the errors that you noticed in section 4, is it possible to identify some culprit processes, where the model leads to biases that could potentially be corrected by better algorithms ? Maybe it's worth saying something about that in the paper.

- In the same line of ideas, it seems that the validation was done mostly on 2006-2016 (we don't see much about the NRT system, or maybe this is just a wrong impression I have). In particular, only for SLA do we see something about the lead time (1-7 days, so "3.5 days"). For other error metrics (SST RMS, etc), could it in the future be possible to assess the model as a function of lead time ? For example, when you give the SST rms ($0.1°C$ for example), and we speak about the NRT ("OPER") model, could you (a posteriori) compare observations with the forecast generated at day-1, day-2, ... , day-10, and give 10 values for the SST rms ?

Typos, language errors, and minor remarks:
- in general, the paper mixes direct and indirect styles "we do this ..." , "this was done..."
- page 2 line 24: "an"
- page 21 L28 : missing "The"
- page 22 L 27 : "as" -> "such as"
- page 23 L18 : 'worst" -> "worse"
- page 23 L23 : "after" -> "afterward"
- page 24 L18-20 : phrase is badly formulated
- page 25 : remove L20 (duplicates what's said just above)
- page 25 L 28: "solutionS"
- page 26 L5 : "from" -> "for"
- all figures are too small when printed on paper

_____

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2018-15, 2018.