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.
We thank Luc Vandenbulcke (Referee #1) for his careful reading of our manuscript and for his constructive remarks. Following his advices, we tried to make the manuscript clearer. All remarks detailed below by the referee were considered and/or discussed.

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).

Figure 1, Table 1 and Table 2 have been modified and the text has been changed accordingly.

- 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.

We think that Figure 6 allows to locate the areas of low and strong correction of precipitations and therefore the areas that potentially will be the most impacted. For this reason, we would like to keep this figure. The maps of the Figure 6 have also been re-centered on Pacific at it was already the case for Figure 7.

- 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?

Referee #2 has also mentioned this point. The text has been completed and the following reference has been added:
De Lavergne C., Madec, G., Le Sommer, J., Nurser, A. G., and Naveira-Garabato, A. C.: On the consumption of Antarctic Bottom Water in the abyssal ocean, J. Phys. Oceanogr., 46, 635–651, doi:10.1175/JPO-D-14-0201.1, 2016.

- 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?

We agree that the first version of the text was not very clear. Some sentences have been changed to clarify this point.

- 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 "Desroziers" method is effective and hence should indeed be used to improve the model. Therefore, 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.

We agree with all of that. We added in section 2.2 that "only one SST map is assimilated on the fifth day of the 7-day cycle" and that "cloudy regions are filled by the analysis performed in OSTIA". We also added some supplementary explanations in section 3.5 making, we hope, the section clearer.

- 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?

The text has been completed in the introduction and in section 2.2 for all assimilated observations. For in situ temperature and salinity vertical profiles, it was already partially mentioned in the original manuscript page 9 (lines 7-14).

- 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.

You are right. It is difficult to improve the model itself with the used version of NEMO.
We develop currently the next version of the global system, based on the version 3.6 of NEMO. Some new parameterizations present in this version will allow to improve the model itself (see first point in "Very general comments" part).

- 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?

The mean sea level time evolution is the result of an imposed trend for mass inputs (2.2 mm yr^{-1}, see section 2.1) together with a diagnostic steric effect re-computed from model temperature and salinity. Although the distribution between mass and steric diagnosed from the model is not yet fully satisfactory, the trend of the Global Mean Sea Level is consistent with the observations. This is already said in section 2.2 of the original manuscript.
The expected steric effect is about 1 mm yr^{-1}. The trend of the mean sea level corresponds to the sum of this steric effect and the trend of mass of 2.2 mm yr^{-1}.

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.

You are right. We think also that some model biases could potentially be corrected by using better algorithms and more sophisticated parameterizations. Some of them are already available in NEMO 3.6 version. We added in the conclusion of the paper some sentences about the following algorithms/parameterizations we plan to use in the future system version:

- LIM3 multi-category ice model.
- Z* vertical coordinate, which basically consists in changing the total ocean thickness and the sea level accordingly. This leads to the relaxation of the linear free surface assumption approximation and allows for exact global tracers conservation and the removal of unphysical surface salt fluxes.
- Split explicit free surface in place of the actual filtered free surface of Roullet and Madec 2000. Apart from the better representation of external gravity waves, this also provides a substantial CPU gain on massively parallel architectures.
- Vertical mixing (transition from a one equation TKE closure to a two-equations GLS scheme, Reffray et al., 2014).
- A third order horizontal advection scheme (UBS Upstream Bias auto diffusive Scheme – Shchepetkin et al., 2005), replacing the second order vector form differencing for momentum.

Reffray, G., Bourdalle-Badie, R., and Calone, C.: Modelling turbulent vertical mixing sensitivity using a 1-D version of NEMO, Geosci. Model Dev. Discuss., 7, 5249-5293, http://www.geosci-model-dev.net/8/69/2015/gmd-8-69-2015.html, 2014.
Shchepetkin, A.F. and McWilliams, J.C.: The regional oceanic modeling system (ROMS): a split-explicit, free-surface, topography-following-coordinate oceanic model, Ocean Modelling, 9, 347-404, doi:10.1016/j.ocemod.2004.08.002, 2005.

- 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 ?

You are right. We chose in this paper to show the impact of the many updates only on the hindcasts (catch up to real time which corresponds to the 2006-2016 period).

We performed also validation in NRT on forecasts and the performance of the daily 10-day forecasts has been checked. For instance, Figure A represents temperature RMS differences (model minus observation) for best analysis (hindcast) and for 1-day, 3-day, 5-day, 7-day and 9-day forecasts. As expected, the best analysis has the lower RMS and this RMS increases with the forecast length. Similar results are obtained for salinity, SLA and SST.

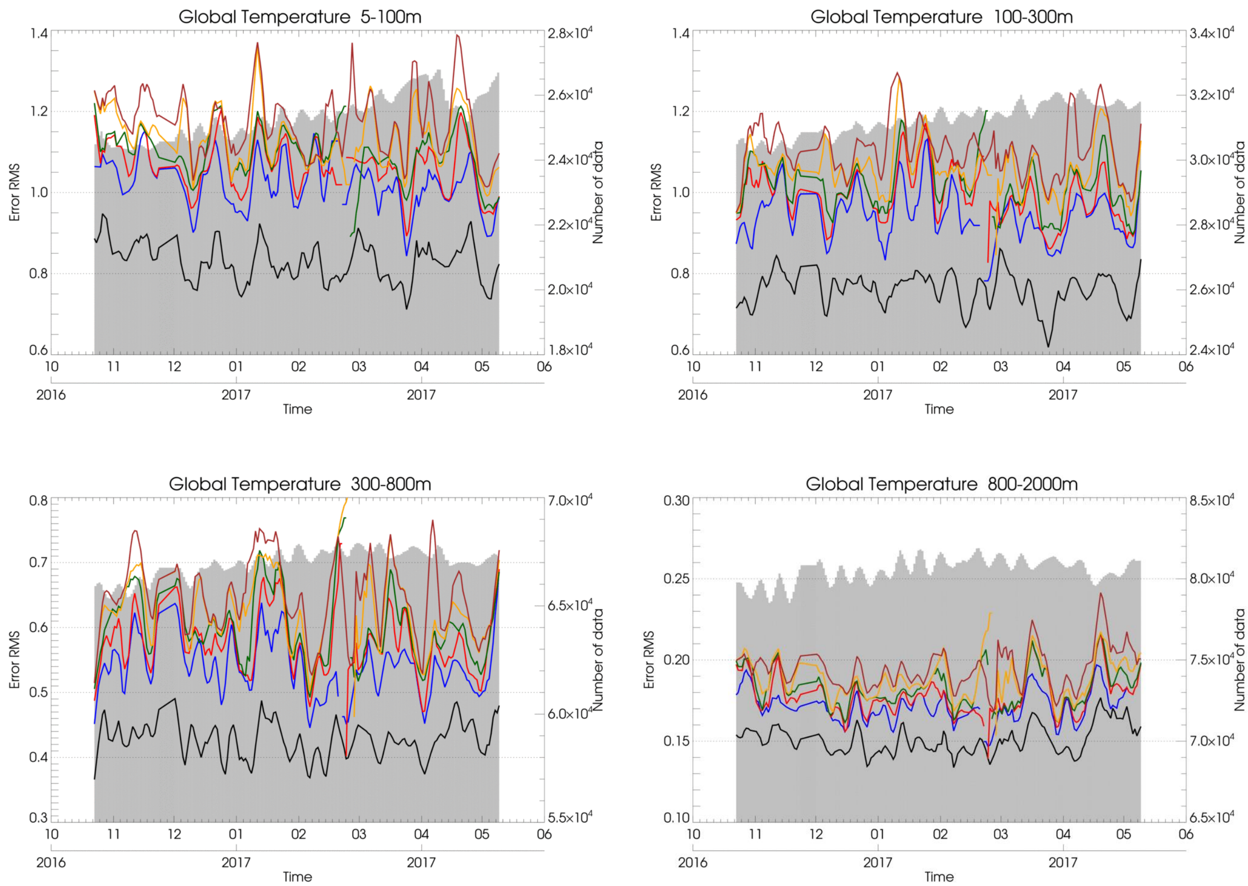

Figure A: Temperature (°C) RMS differences (model minus observation) in the 5-100m, 100-300m, 300-800m and 800-2000m layers. Statistics are displayed for best analysis (black line) and for 1-day (blue line), 3-day (red line), 5-day (green), 7-day (orange line) and 9-day (brown line) forecasts. The number of available observations appears in grey in the background.

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.

All these "typos" errors have been corrected. Some sentences have been reformulated. The size of the figures has also been increased.