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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.

Anonymous Referee #2

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General comments This Discussion paper discusses the main updates of the Mercator Ocean operational forecasting systems at 1/12 resolution, which is the highest resolution deterministic forecast product released by CMEMS. The manuscript is certainly interesting and deserves publication because documents the main changes and quality increase achievements of a state-of-the-art oceanographic analysis system. It can be useful for both developers and users. However, in my opinion there are many scientific issues that are only empirically formulated, lack scientific justification and require a deeper explanation. In general, it is also not clear why some updates are discussed in details in section 3 and some others only mentioned in section 2: this seems quite arbitrary. I therefore recommend revising the manuscript to address the specific points

below to help the readership to understand the motivation and justification of such changes, which will really help the usefulness of this work in the oceanographic community and the robustness of the paper. I found the article well-written; it is a bit long, and I suggest the authors to consider removing some figures (29 figures are really too many in my opinion).

Specific comments

Abstract L23: forecast error → background error

Introduction P1L16: I believe the fact that Mercator is entrusted by EC is not relevant: here it is relevant that Mercator Ocean is in charge of the global analysis and forecast system

P1L27: "four many areas": I count 6 areas from the manuscript, moreover this number is subjective

P3L5-10: seems a repetition and suggest to merge in P2L14-26

P3L27: "three twin ..." the number three appear evident only later in the paper, suggest dropping it

P5L4-6: The point here is not that parameterizations in the version 3.1 of NEMO are still in the version 3.6, but how many new parameterizations and improvements of NEMO are you missing using version 3.1? In my opinion it should be discussed this way: although I perfectly understand that upgrading version is not easy for an operational system, and this is a justification for me, there are many years of ocean model developments not exploited here, which should be honestly mentioned.

P6L8: maybe is good to say what are the problems coming from the use of z-coordinate you are referring to? Would it be better then to use sigma-coordinates? Or you mean something else?

P6L22: it would be interesting to know what you found for 0, 50 and 100% of relative wind and which was the criteria to choose 50%

P7L1: negative gridded anomalies : maybe better to say negative variations of water masses estimated from GRACE (if I interpret correctly)

P7L11: "...known..." suggest adding a reference

P7L32: I assume covariances are static (seasonal) but do not vary inter-annually in the real-time system. It is better to state it explicitly

P9 paragraphs starting at L15 and L19 seem in contradiction: if the obs errors are adaptive, why do you need a retuning?

P9L33: this requires a clarification on how you changed the formulation: from the anomaly dataset how do you define the SSH in the old and new system? Wind effect is also barotropic, i.e. is not clear what you actually changed

P10L12: suggest adding that the new approach is more consistent with what you actually do (using not a free run but a bias-corrected free run, which better mimics the operational system)

Section 2.3.1 & 2.3.2 It is not clear if these criteria are completely empirical or have some theoretical justification. If empirical as I guess, please discuss the criteria you used to obtain the values for the thresholds

P12L11 it seems weird that there are more suspicious obs in 2012 and 2013. Any idea why?

P13L1: how do you define the adjustment, achieved in 3 months?

Figure 5 and discussion. It looks like the new initialization bears more subsurface biases, so that it is not really convincing that it is better than the old one. I think it deserves a better discussion

Section 3.2: the discussion on the result (Figure 7) will benefit from a quantitative

assessment (RMSE and bias reduction of the model vs salinity obs at global scale will be sufficient)

Section 3.3 A reason for drift might be also inadequate background-error covariances that contain spurious correlations. This should be mentioned at the beginning. Again, the thresholds seem to be empirical and suggest writing the criteria for their adoption.

P18 Fig 12: It is weird that without the SEEK you have more variability than the observational product: I wonder whether the two datasets are really comparable, given that the 1/12 model may have a signal at higher resolution than the gridded altimeter product.

Section 3.4.2 Suggest putting it more in the context. I assume that the filtering is applied to the anomaly from the BIAS experiment before covariance computation, and then these differently filtered covariances are used in single-track experiment. However, it is a deduction and recommend to begin the section explaining this.

Section 3.5 This is the section that I found very hard to justify. I don't see a reasoning why observation errors are flow-dependent and should change so much with time, except the representativeness error component that might slightly change with season and/or particular events (eg presence of fronts, etc.). But this is less crucial than the background-errors that are certainly modulated by observation availability, large- and small scale processes, forcing, etc. This seems particularly true when looking at an observational dataset with nearly constant sampling (SST, Fig 18/19). I think the results improve not because observational errors really change with time, but because you are changing the ratio between background and observation errors, provided that background errors are of course flow-dependent as mentioned before. Moreover, the Desroziers method implies simultaneous tuning of background and observation errors. If the authors are able to provide a similar complementary retuning of background errors (I mean not with experiments but with diagnostics), it will really improve the robustness of the section. Otherwise a better discussion is needed, probably mentioning that what is actually done is to change the relative weight between background and observation errors, rather than changing observation error themselves.

Figure 21: suggest better putting the figure and related discussion in context: the figure shows scores for assimilated vs non-assimilated (NOAA) datasets, so it is not clear the goal of the figure

Section 4.1.2 Title and text: as the SST source is similar between OSTIA and CAT-SAT (night time measurements from infrared sensors), I don't think the latter is really independent. I would define it "external" or similar Section 4.2 L21: 2005-2012 is not a decade; moreover, suggest trying the entire (inter-decadal) climatology to get rid of the weird increase of RMSE after 2012, probably due to the fact that the decadal mean is much too affected by the inter-annual variability therein.

Section 4.3.1 Please clarify how you estimate 2 and 4 cm for instrumental and MDT errors; the MDT one seems in particular arbitrary; also, in the computation of the statistics, do you use any threshold to filter out certain misfits, in order to obtain that global value of RMS, or you use all observations?

Section 4.3.2. Please provide a reference for BADOMAR and GLOSS/CLIVAR. Also in section 4.4.1 and 4.5 there are products that are referred to only through links: is there any better way to refer to them?

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