Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-170-RC1, 2019

Ocean Science
Discussions
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Interactive comment on "Assessment of ocean analysis and forecast from an atmosphere-ocean coupled data assimilation operational system" *by* Catherine Guiavarc'h et al.

Anonymous Referee #1

Received and published: 4 March 2019

General comments:

This study presents a new ocean-atmosphere coupled operational system (CPLDA) with weakly coupled data assimilation developed at Met Office. In the first part, the model components are described and compared to previous coupled and uncoupled systems (GloSea and FOAM respectively). The weakly coupled data assimilation method from Lea et al. (2013, 2015) is also detailed. This system is operated and evaluated during one year (2015) with 6-hour analysis and daily 7-day forecast. The system performances are then compared with the operational ocean-only FOAM (Ryan et al. 2015) and Mercator PSY4 systems and with different observational (SST, MLD, 15 m velocities, SLA) datasets. Atmospheric forcings and turbulent fluxes are also analysed. In summary, the CPLDA system performs as well as ocean-only systems despite its increased complexity and constitute a promising first step toward a fully coupled operational system.

The manuscript provides a precise description of the CPLDA system, underlining the benefits and limitations compared to other MetOffice systems and observations. It is globally well written and scientific analyses are seriously presented. However, I recommend the following modifications to improve the manuscript quality and understanding. I have the feeling that some sensitivity tests and analysis are still missing to better cover the system description and validation. This is mainly due to the fact that besides the ocean model similar configurations between CPLDA and FOAM, there a large number of differences between the two systems. Consequently, it is sometimes difficult for the authors to assess precisely the differences between the systems because too much parameters change at the same time. Some additional sensitivity tests allow to better understand those differences (for example, the one-month experiment is a different scheduling for the DA). But others sensitivity tests are missing to clearly understand and to disentangle the numerous modifications between the systems. As a consequence, some analysis are not very convincing because of this lack of sensitivity experiments. Depending on the authors will and capacity to run these additional simulations, some corrections will have to be made to the text to confirm or infirm the hypothesis proposed. If no supplementary experiments can be done, some comments must be added to the text to emphasize the limitations of the study. Because atmospheric forcings and surface fluxes are strongly related to SST, upper temperature and MLD, the section 3.4 should be moved before the section 3.3 about velocities (that I would rename “upper or ML velocities”). It is not always clear if the authors are talking about the analysis, the forecast or both in the whole manuscript. It makes the study understanding less clear to follow and more difficult to understand. I think the manuscript can be easily improved by clearly stating this.

Specific comments:

p.1 - Introduction: The introduction is not really relevant because it is mostly a repeat of the system description in section 2. The introduction should be improved to better describe the context of this study, i.e to describe the main physical and technical arguments in favour of developing a coupled system with weakly coupled assimilation (instead of independent systems and assimilations). A description of what is done in other operational centres (ECMWF, NCEP, . . .) could also help to better understand the framework and the interest of the system presented here.

p.3 l.10-12: it is not clear if VarBC is activated or not in the present study, please specify this in the text.

p.5 l.30: please justify why such changes regarding IAU and observation operator were made in CPLDA compared to FOAM.

p.6 l.20: scheduling of the operational system ?

p.6 l.25: I don't understand why some results should come from the operational version of the system and others from "test" version ? I find this confusing. Perhaps it should be better to talk about the actual operational version only in the conclusion to avoid any confusion.

p.6 l.31: please add Lellouche et. al 2018 as a reference for Mercator-Ocean PSY4 system.

p7 l.9-11: I think these results are important and should be added to the paper as a distinct section or paragraph with a dedicated figure to illustrate the differences in terms of SLA bias and RMSE between FOAM and the 2 versions of CPLDA with the different schedulings. It is also unclear if the results presented here are those from the current operational system or from the version used in the study ("old scheduling").

p.14 section 3.4 should be moved close to the sections 3.1 and 3.2 as it is directly related to changes in SST and MLD. It would improve the readability of the manuscript.

p.7 l.28-30 This hypothesis should be directly tested by doing some additional sensitivity experiments using a 24h window with CPLDA or a 6h window with FOAM to confirm it or not.

p.7 l.30 “in CPLDA” -> “in CPLDA analysis”: it is not clear when the author is talking about the analysis or the forecast. Please specify it everywhere it is necessary in the text.

p.7 l.31: is there a way to measure this “overfitting” ? It is not clear why FOAM SST forecast performs better than CPLDA and how is it related to this overfitting.

p.8 fig.1: the forecast duration is 7 days (168h), please expand the figure axis accordingly

p.8 l.19: it could be interesting to give the effective SST resolution for both system even without showing the figure to get an idea of the scale range resolved.

p.9 section 3.2: please add PSY4 in the analysis and Figure 3 if possible to be more coherent with others sections and to get a more complete comparison between products.

p.9 l.9: please give the reference associated with this product.

p.9 l.11: please give the range of the RMSE increase to be able to compare it to the SST RMSE.

p.10 l.15: the warm bias and associated significant RMSE ($\sim$1°C) located at $\sim$100m is not discussed nor explained in the text. Please add a paragraph about this. If the discussion about King et al. 2018 results explain it, it should appear more explicitly in the text.

p.10 l.17: are you talking about analysis or forecast ? Please detail both aspects regarding MLD.

p.11 l.5: I totally agree with the author: an additional experiment using FOAM with a

6h window or CPLDA with a 12h window is needed to disentangle this effect from other possible factors such as atmospheric forcings, turbulent fluxes schemes, . . . Hence, I strongly suggest to the author to do this simulation if the modifications of the systems are not too heavy and depending on available computing resources. I would greatly improve the manuscript discussion and strengthen the results presented here.

p.11 section 3.3: please add a word if the product is also assimilated or other products related to oceanic currents. Please make a better distinction between analysis and forecast biases and RMSE.

p.11 l.20: the sentence is in contradiction with what is stated above at p.11 l.15-16 (“bias and RMSE stable during forecast”). Please correct this or give more explanations.

p. 12 l.1-2: is it possible to confirm this statement by comparing directly the number of assimilated observations between both systems please ? Would it be possible to conduct the same kind of tests to address other questions or comments in the manuscript related to the difference in term of assimilation time window or observation number ?

p.12 l.6-7: again, an additional test with a different time window would strongly clarify these statements and give a real matter for discussion.

p.12 l.10-11: again, an additional 1-month experiment using the new MDT would clarify this.

p.12 fig. 5:Figure 5 is not described nor employed in the text. Please supress it or comment it.

p.13 l.2-3: on the contrary, it has been shown that coupled models have more EKE damping than ocean-only forced models (see Renault et al. 2016 for example). Consequently, this explanation is incomplete or erroneous.

p.14-15 – section 3.4:

There is no numbers in this section text to quantify the differences between atmospheric forcings heat fluxes and stresses. It will make the comparison easier and more "physical" if you add them.

p.15 l.1-3: the shortwave (SW) bias pattern suggest an SW overestimation on all eastern boundary upwelling systems. This is usually related to an underestimation of the low-level stratiform clouds which atmospheric models have difficulties to represent. A comment about this atmospheric bias should be added if it is also present in NWP.

p.15 l.21: please give a quantification of the contributions of atmospheric state and latent heat to the total net heat flux difference.

p. 15 – wind stress analysis: I have some difficulties to correctly understand this part. First, there is no distinction between atmospheric analysis and forecasts. I suppose analysis are relatively close to MetOp observations as they are probably assimilated, while 7-days forecasts must have quickly increasing surface stress errors. Consequently, it is difficult to guess where the stress differences between CPLDA and MetOp come from. Then, CPLDA wind stress is strongly and globally underestimated (∼-0.1 N/m2) compared to MetOp. This bias is far larger than the difference between CPLDA and FOAM (∼+0.5 N/m2). I don't think this is related to bulk formulae differences (COARE 3.0 is nearly identical to COARE 3.5 except in very strong wind conditions which statistically almost never occur). It can be partially related to the fact that scaterometers usually provide surface stress in atmospheric neutral stability condition, but it cannot explain the global underestimation observed here. This can also be related to the absolute or relative wind/current coupling in CPLDA. But finally, I don't understand why this global stress underestimation is not associated with warm SST and shallow MLD biases in both FOAM and CPLDA as it should be the case. Please explain clearly why it is not the case.

p.16 – conclusion: Conclusion should be updated accordingly to the modifications done in the manuscript.

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Discussion paper

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