

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 #2

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General Comments:

This paper presents an evaluation of the ocean component of an atmosphere-ocean coupled prediction system that has been running in real-time at the UK Metoffice and builds upon the work presented by Lea et al. (2015). In particular, this system employs a “weakly coupled” data assimilation approach, which affects the analysis and forecast quality in a number of ways. A systematic approach is used to provide an objective evaluation of the different state variables (sea surface temperature, mixed layer depth, currents) against accepted reference datasets, including other operational forecast products. The presentation is generally clear, but remains fairly descriptive,

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limiting the conclusions that can be drawn from the study. Many of the differences between the coupled and operational forecasting systems are due to differences in particular parameter choices and not directly due to the coupled model or “weakly coupled data assimilation” themselves. As a result, the value of the paper is limited to a presentation of the baseline skill of the system. Even this is put somewhat in question, as the results (for currents and sea level) are shown to be sensitive to differences in data assimilated in the two systems resulting in much larger errors in the coupled system. The paper would benefit significantly from having additional experiments to clarify the impact of the different parameter choices and a more in-depth analysis of results. Additionally, the paper would benefit from some reorganization as the differences in fluxes are described near the end of the paper, much of which is needed to understand the differences being presented in previous sections.

I recommend the paper for publication but strongly encourage the authors to deepen the analysis of the results and provide additional experiments to shed-light on the source of differences.

Specific Comments:

Section 1: The introduction is limited to activities at the UK Metoffice and would benefit greatly from some context into why weakly coupled data assimilation is of interest and previous efforts. For example, a study has recently been published that should be referenced in this discussion (Browne et al., 2019). Moreover, several centers are now running coupled forecasting systems operationally, which should be mentioned to provide context into UK Metoffice efforts. Browne, P.A., de Rosnay, P., Zuo, H., Bennett, A. and Dawson, A., 2019. Weakly Coupled Ocean–Atmosphere Data Assimilation in the ECMWF NWP System. *Remote Sensing*, 11(3), p.234.

Pg1, L19: Johns et al. (2012) is a technical report that appears not to be publically available. There are a number of such reports referenced here. If these reports are only available upon request, than greatly detail regarding their content should be included

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in the text. Also for Lea et al. (2013).

Pg2, L14: “To avoid duplication of previous work . . .we did not investigate the impact on the diurnal cycle already covered in Lea et al., (2013)”. This is a fairly important point that is referred to later in terms of errors in temperature and mixed layer depth. Since Lea et al., (2013) is only a technical report these results could be included here (or at least explained more clearly).

Pg. 3, L1: COARE3.0, please add a reference. Also this is not noted in Table 1.

Pg. 3, L9: Lea et al. (2015) note significant difficulties associated with runoff. How is runoff handled here? Are there differences between FOAM and CPLDA?

Pg. 3, L32: “Haney retroaction”. Please provide a reference and more details. I.e. what is the timescale of the relaxation? To what fields?

Pg.4, L19: It says here that CPLDA uses a fixed freezing temperature, which differs from what is indicated in Table1.

Pg. 5, L27-32: It may be useful to note here that there is also an impact on the observation cutoff time that affects the number of SLA observations used.

Pg. 6, Sec2.6: If the CPLDA system has been running operationally since October 2016, why is it necessary to run additional experiments? If the aim here is to benchmark the skill of CPLDA as compared to FOAM why not use the operational runs? If it possible to make additional runs, could a longer run with the SLA observation cutoff corrected be produced? This would make the evaluation much more straightforward.

Pg. 6, L17: “There are a few differences. . .”. What is the impact of these differences? If they are not too significant an analysis of the operational run compared to FOAM for Oct. 2016 onwards could add additional insight to the paper.

Pg. 7, L7: SLA is an important variable providing information regarding the circulation and mesoscale activity. It is also relevant here to provide context to the errors in 15m

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velocity. I would strongly encourage you to include these results, despite the poor results. Ideally, an additional run could be made with the cutoff issue corrected.

Pg. 7, L16: It is argued that sea ice is not evaluated because of a difference in freezing temperature. However, there are differences in many other aspects as well (bulk formulae, SLA observation cutoff, assimilation window). If the aim is to benchmark the performance of the system, than an evaluation of the sea ice component should be included as well.

Pg. 7, L20: Is it the instantaneous SST or a daily average?

Pg. 8, L9: Should this be 0.01K? The global mean difference in bias between CPLDA and PSY4 appears to be less than 0.05K. Also, if OSTIA is cooler than CPLDA and PSY4 assimilates OSTIA, why is PSY4 warmer than CPLDA?

Pg. 8, L19: The Fiedler reference is only “in prep”. I recommend providing a more thorough explanation with a “personal communication” to Fiedler.

Pg. 9, Fig. 2d: What is the global mean value of this? It would be helpful to include this on all four panels. It appears to me that it is positive, which seems in conflict with Fig. 3b that shows CPLDA has a more negative global near-surface temperature increment.

Pg.10, L5-14: This asymmetrical effect of SST increments is quite an important difference between CPLDA and FOAM and warrants further explanation. Perhaps an example figure showing profiles and how the increments deteriorate the profile. However, the argument is based on the fact that large increments in CPLDA then create a large asymmetrical effect on SST. But why are the increments larger in the first place?

Pg. 10, Fig. 3: What is the explanation for the large increments between 50-100m and associated increase in RMSE in CPLDA? Is it related to the asymmetric effect of SST increments? If so, this should be described.

Pg. 10, L16: How is the mixed layer depth calculated? The Fig. 4 caption quotes Kara (2000), but some explanation of the methodology used would be appropriate.

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Pg. 10, L17: “mean error .. is -5.2m”. The text (Pg. 10, line2) notes that CPLDA has a deeper mixed layer than FOAM, shouldn't this value be positive?

Pg. 11, Fig. 4b: Please state whether positive values are deeper or shallower in CPLDA.

Pg. 11, L2-4: It is difficult to follow this reasoning when we haven't yet seen the differences in wind stress. This section would be easier to follow if Sec3.4 was presented first providing context regarding differences surface fluxes.

Pg. 11, L7: “Predictions of ocean current are important for marine activities”. It would be helpful to expand somewhat since this statement is vague. Also, it should be “currents”. Pg. 11, L19: Are differences in tropical Pacific along capable of affecting global statistics this much? Or are there large differences elsewhere as well?

Pg. 11, L21: If the shorter window affects the increments in a direct manner that influences the results to this degree, I think it warrants some further description. Perhaps some examples of increments in this area. This is extremely relevant to the overall effort to produce a “weakly coupled” data assimilation system and should be elaborated upon.

Pg. 11, L21: By how much are the number of observations limited? This too could be expanded upon to clarify the impact of the assimilation window on results.

Pg. 12, L3: If the operational system uses the updated scheduling than why not evaluate it instead of the experiment with the reduced number of observations?

Pg.12, L7: If the unrealistic currents are not always caused by the reduced number of SLA observations, than perhaps the observations are simply covering up an existing issue in the coupled model. This issue warrants further investigation, it its central to many of the results presented.

Pg. 14, L8-9: A “reduced heat loss” should lead to warmer SSTs shouldn't they?

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Pg. 14, L9-10: While the differences in short wave radiation and latent heat clearly contribute to differences in net heat flux difference. These fields both have large scale patterns, whereas there is a notable small scale pattern in the net heat flux difference not explained.

Pg. 17, L25-27: As noted above, the argument about the noisier increments is quite important to the overall conclusions. I would recommend a deeper analysis be made of this issue.

Pg. 19, L8: should be “well-placed”

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