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Geoscientific Model Development Discussions

Interactive comment on "Reanalysis of the PacIOOS Hawaiian Island Ocean Forecast System, an implementation of the Regional Ocean Modeling System v3.6" *by* Dale Partridge and Brian S. Powell

Anonymous Referee #1

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The paper analyses an implementation of the Incremental Strong Constraint Variational Data Assimilation scheme within the ROMS model system to generate a reanalysis for the Hawaiian Archipelago. In particular, the improvements in the system due to a newer version of the model source code and modifications in the data processing in comparison to a free running simulation are evaluated. However, the modifications to the model source that can be relevant to the results are not discussed and the modifications to the observations processing not clear. While the analysis techniques are appropriate, they are not novel. Although the work is interesting and provides information that can

be useful for researchers developing similar systems for other regions, there are analysis flaws and misconceptions that must be addressed before it is ready for publication. The reanalysis evaluation is made against a free running simulation, and based on the same set of observations assimilated in the reanalysis. Therefore the results should be considered as an evaluation of the assimilation scheme, and not as improvements to the system. A comparison to previous versions of the reanalysis and/or global high resolution products (such as the GLORYS reanalysis) is necessary to evidence any ocean estate representation gain by the present simulation. This is useful to justify the effort of implementing and maintaining a regional operational system. Moreover, there is a lack of physical interpretation of the simulation misfits and the increments to the initial and boundary conditions. I recommend the authors are given the opportunity to improve their manuscript through a MAJOR REVISION before it is accepted for publication in the Geophysical Model Development Journal.

MAIN COMMENTS:

While the paper is partly based on the upgrade to a newer version of ROMS, the latest version is not used. The study uses ROMS 3.6 while the version 3.7 was released over 4 years ago, and the current version is 3.8. The authors must justify why an older version of the model code is used.

Since an improvement is claimed for the current reanalysis, there should be a comparison against the previous version of the Hawai'i reanalysis in order to identify where and why such gain occurs.

Although the comparison against a free running model that constitutes a downscaling of a global reanalysis is interesting, the manuscript would benefit from a comparison against the global reanalysis itself. This can provide an estimation of the improvement in relation to the solution used to force the open boundaries, and justify the effort of implementing a regional assimilation system.

How are the super-observations generated? Is there a projection/average over the model grid? This is especially important for the HFR since it presents higher resolution than the model – and may therefore contain processes unresolved by the simulation.

The case for salinity is curious in Figure 4. There are “negative improvements” even during the period covered by Aquarius mission. Figures 7 and 8 reinforce the idea that there is no benefit in assimilating SSS. Moreover, I wonder why other SSS observations were not explored after the end of the Aquarius mission (SMOS). Since the assimilation of SSS is not a common feature in ocean reanalysis and operational systems, a more detailed analysis of the reasons behind this relatively poor performance would improve the interest on the manuscript.

In sections 3.2 and 3.3 you show the a priori SST errors are overestimated for both the observations and the background model solution. Please elaborate on how this impacts your reanalysis results. Did you make any experiments changing the errors estimation method? The same applies to the HFR (underestimated errors).

Although the comparison against the assimilated observations is interesting, it basically diagnoses the assimilation is working. However, it says nothing about any improvement in the representation of the regional dynamics. For this purpose, the model must be compared against independent observations (not assimilated). In special, observations in under-sampled areas/variables would be interesting. Perturbations generated during the estate estimate process can degrade the solution in regions where no constraint is provided due to the lack of observations. Are there any other observations that can be used for such (WOD, drifters, SMOS, etc)? If no independent observations can be found, the authors should clearly state the limitations of the provided comparisons.

It is interesting to note there is a good agreement between the variability of both RMSA and CCA from the forecast and the analysis for all observations in Figures 7 and 8 (especially the low frequency). This suggests a physical process may be absent from the background solution, represented by the forecast. Although the assimilation reduces the mismatch, it does not introduce the missing physics. Please comment on this.

In the evaluation of the analysis of increments in the initial conditions, I don’t understand why you averaged over the top 100m. By one side most of the data is in the surface (SST), by the other side there is great interest in what happens below the mixed layer. Especially after you show the larger mismatches are at the thermocline. For both sections 6.1 and 6.2, a deeper interpretation of the physical meaning of the EOFs is lacking. This kind of analysis can give you an insight into processes that are not well represented by the background solution, and fuel improvements in the system. The inclusion of the temporal components of the EOFs should help such interpretation.

DETAILED COMMENTS:

Lines 106 – 113: Please comment on the compatibility between the 2 sources for atmospheric forcing used. Are there discontinuities in the fluxes from the different sources? How does the ocean model respond to this change?

Line 122: Please explain in more details how the observations errors (uncertainty) are calculated and the assumptions made. There are several possible methods for doing so, and the outcomes can greatly impact the assimilation results. (you already started this in lines 194-196)

Line 129: Were sensibility tests conducted to estimate the optimum number of inner/outer loops so J is properly minimized? Can the authors present a plot (probably as a supplementary figure) to justify their choice?

Lines 140-141: What is considered as “close to the boundary” and “shallow water”? The values should be given.

Lines 143-160: Please justify the change in the SST data source to a coarser product in April 2008.

Line 205: What do you mean by that the HFR data is less reliable at the range limit. Does it mean there was a different treatment for such data? Or are you referring to a smaller number of observations?

Lines 231-233: You should briefly explain how the breakdown of the cost-function is made, and offer references for further details.

Line 232: Probably you meant model variable by “observation type”.

Line 233: change “is” for “it”

Lines 259-261: It is not clear what the “optimality value” is. If it is the result of equation 1, would this mean that a optimality of 1 refers to the model perfectly representing the observations? Please be more straightforward about it.

Paragraph bellow line 307 (has no line numbers): Are the initial conditions for the analysis and the forecast the same? You specifically say that “the boundary and atmospheric forcing are altered as part of the state estimate solution”. However, the modification of the initial conditions is usually the most important factor impacting the model solution. Please clarify what are the initial conditions used by the forecast (updated or otherwise).

Line 393: The explanation of the skill metrics is confusing. What is the persistence assumption? Please explain it better. Moreover, if the assimilation window is 4 days how do you calculate the analysis skill for 8 days?

Lines 410-412: The fact there is a reduction in skill once per day for the SST is highly dependent on how you define the persistence. What would happen if it was defined from the mean of the first day? Would it be a more balanced option? Please comment.

Lines 420-422: Could you please provide some evidence the perturbations of the boundary conditions don’t influence the model solution? Or at least references of similar systems that provide such evidence.

Lines 451-452: How was the update interval for the perturbations in the forcing chosen? What was the reasoning behind the 6 hours value?

Lines 470-472: Was a flux correction (QCORR) applied to ROMS?

Line 475: Please substitute “vertically” and “horizontally” by “meridionally” and “zonally”.

Section 5: It is surprising how the analysis represents a small improvement in performance when compared to the forecast. Also, it seems the forecast skill is very stable along the 8 days window. Please elaborate on the reasons behind this.

Sections 6.1 and 6.2: Please include the temporal (different cycles) component of the EOFs. These are indispensable for the interpretation of the results.

Figure 3: Do you know the reason for the “sinusoidal“ variation on the number of Aquarius observations?

Figure 11: Interesting how the assimilation can’t reduce the RMSA or improve the CCA bellow 500m for the Argo and HOT data. But the same doesn’t happen for the gliders data. Can you articulate about why this happens?

Figure 12: The standard deviation around the mean SS should be presented. Also, a grid would make the plot easier to read.

Figures 13 and 14: Please include the time component of the EOFs.

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