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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

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article

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Response to Reviewer #2

We thank the reviewer for the thoughtful and thorough review of the manuscript. We have made a major revision to the manuscript to address all of the reviewer's comments, and we believe that we have addressed the concerns raised. We accept all of the reviewer's comments.

General comments: This paper presents a 10-year reanalysis of the Hawaiian Islands region, using IS4DVar and assimilating a variety of observations. The authors show that the state-estimates provide an improved representation of the (assimilated) observations compared to the forecasts, which is to be expected. It is unfortunate that they do not present comparison to any independent observations.

The purpose of the I4D-Var method is to represent the observations by exploiting the linearized model dynamics. Therefor, all available observations are used to constrain this representation. The authors refrained from reserving data sets for validation as this is an arbitrary procedure that would result in less constrained state estimate. This is now clearly mentioned in the manuscript.

The authors mention in the introduction and the summary that the system presented uses an updated model and data assimilation scheme, but do not elaborate as to how the model has been updated nor do they quantify the improvements. Mention of this "updated version" should be removed unless they are going to provide some quantitative comparison along with a description of the updates.

The original text was rather vague in reference to "improvement", rather the suggestion is that we are conducting a decadal reanalysis of the Hawaiian region using the configuration as detailed in Souza et al. (2015). The test period used in Souza et al. (2015) was only an 18 month test case. This paper uses all available data over 10 years. The manuscript has been changed accordingly

The authors present EOFs of the increment adjustment to the initial conditions and surface forcing, however physical interpretation of the increments is lacking.

We added the principal components for the EOFs shown in the manuscript. We have revised the discussion of the EOFs. It must be kept in mind,however, that we are dealing with EOFs of residuals with non-physical variance. In the revised manuscript we discuss the spatial patterns of EOFs but clearly state that no physical modes came out of our EOF analysis.

In general, the model and data assimilation methodology presented is sound, but the manuscript lacks insight that the authors can gain from their work. How do the results help to understand and improve their model and assimilation system? I recommend that the manuscript be returned to the authors for MINOR revision before acceptance

for publication in the Geoscientific Model Development Journal.

Specific comments: Line 13: remove the first HFR

Corrected.

Line 52: The HLCC is perhaps a weak current, but it is not a small current (in its spatial extent).

Corrected.

Figure 1: I wonder if it would be useful to label the islands, as you refer to them quite a bit in the text.

Done.

Lines 127-128: The inner loops are performed before the NL model is updated in the outer loop.

This paragraph was re-written and the error was corrected.

Lines 140-141: How close and how shallow?

The text in the manuscript has been updated. It was specified that observations within on Rossby radius ($\sim$ 80 km) of the boundaries are ignored. The term "shallow" has been removed.

Section 2.3: The author does not explain if super-observations are used and how the observation error variances are specified.

For most datasets, we do not use "super-observations". In a least-squares minimization, finding the minimum variance of co-located values is the same as averaging them and assigning a larger variance. That said, for HFR, we did combine some due to the large number of observations; however, oftentimes, we have only one in a single grid cell. Regions where there are more than one HF radial in a single grid cell are those nearer to the HFR source. As such, the angles are very close, and we tested averaging the angle and radial value versus projecting them. Some of the details are found in Souza et al. (2015).

Line 201: Call the Big Island Hawai'I instead for consistency throughout the paper.

Corrected.

Line 206: . . . around Hilo Bay (on the northeast of the island) . . .

Corrected.

Line 207: And what happens if it's $< 80\%$, is it still used?

We have updated the manuscript explaining the HFR measurements from any return location that it missing more than 20 percent of its data over the 4-day assimilation period are ignored.

Line 208 on: Do you grid in space to get super-observations?

Please see response above. HFR observations are averaged in space if necessary.

Figure 3: Hard to read the legend

The quality of all figures including labels and legends has been improved.

Figure 4, 6: Legends and text are too small to read

The quality of all figures including labels and legends has been improved.

Line 246: and have higher errors . . .?

That's correct for SSS fields. Here the assumed noise level is 0.2 ppt. Furthermore, due to the loss of two SeaGliders in 2014, the SeaGlider data stopped in mid-2014 and only very few salt measurements were available thereafter (sporadic Argo data). We have updated the manuscript to better explain the small impact of salinity on the cost function reduction.

Line 243-245: This statement is very vague.

The shape of the cost function depends on the type and number of observations. When the observations change, also the cost function will change. This has now been *clarified in the revised manuscript.*

Line 305: . . . better forecasts than other methods that perturb the state at single times. These methods may better reduce. . ... , but can add . . .. . .

The sentence has been rephrased.

Section 4: Comparisons are only made against observations that are assimilated. This limitation needs to be made clear in the section introduction. Are there not any other observations of the region that could provide comparison to independent observations (e.g. ship-based CTD observations) ?

Please see response above. The purpose of the I4D-Var method is to represent the observations by exploiting the linearized model dynamics. Therefor, all available observations are used to constrain this representation. This is now clearly explained in the manuscript

Figure 9 and line 344: Do you mean the atmospheric model has inaccuracies in the representation of the heat fluxes? Or something else? Is this evident in the surface forcing increments discussed later?

This mismatch is driven by a combination of three things: The lee side of Hawai'i is characterized by low cloud cover resulting in a relatively strong diurnal cycle of surface heat fluxes and hence SST. This diurnal cycle of surface heat fluxes is not fully represented in the atmospheric model. The surface layer of the ocean model, on the other hand, is too thick to simulate the impact of larger heat fluxes on SST to its full extent.

Line 356 on: Did you look at how these adjustments extended beyond the radar coverage regions? Some snapshot examples might be nice to elucidate how the currents deviated in the forecast and how they were improved in the state-estimation (as well as how it looked beyond the coverage region). This may help add some physical context to this analysis.

Figure 14 of the revised manuscript shows the EOF1 of adjustments applied to the near-surface currents. It should be emphasized that increments are made due to all variables considered by the I4D-Var scheme. The individual contribution by e.g. HFR to velocity adjustments remains elusive.

Line 371-372: This sentence is unclear.

We rephrased the sentence.

Line 380: It's not much worse, I would say "of the same magnitude".

Corrected.

Line 385-387: Is this because the model background errors are low and the observation errors are relatively high below 500m, so the state-estimate makes little adjustment to salinity below 500m?

Exactly. The background error covariance is low below 500 m, so it requires strong

observational evidence to overcome the penalty in increments there. The sparse Argo provides an occasional profile; whereas, the SeaGliders (when available) provided many profiles per day in the same region.

Figure 12D: Interesting that the forecast skill degrades within the first 12 hours for the radials.

The reason behind this decrease is given by the fact that the radials are dominated by the semi-diurnal both baroclinic and barotropic tides. Hence a reduction of the skill after 12 hours can be expected. This explanation has been added to the manuscript.

Lines 431-433. By 'background values' do you mean the background standard deviations? It is surprising that the SSH is adjusted so little. Does the relatively large adjustments to the velocities suggest the system may be over fitting to the HF radar observations? Can you comment on the relative increments for the different variables as a percentage of the background standard deviations?

The change is given as percentage of the initial conditions. This has been clarified in the manuscript. The larges adjustments to the initial conditions of the *velocity field are made far away from the HFR in the areas dominated by the shear between the NEC and the HLCC. This is not indicative of an overfitting to the HFR data.*

Line 435: Positive everywhere, so this suggests a bias that is being corrected. Can you comment on bias?

The text has been clarified. The EOF1 has the same sign across the entire model domain. This is indicative of a bias correction that varies on the time scale of days. The total adjustment, however, is given by the product of the EOF1 and the PC1.

Line 437: Do you mean horizontal temperature gradients? Can you explain how this dynamical characteristic relates to the higher increments in this region.

The discussion of the EOFs has been revised.

Line 446: increasing or decreasing (you don't show the temporal expansion function

which could have both negative and positive sign)

The PCs are now shown in Figures 13-15.

Figure 14 a) mode 1 is all negative. Again, can you talk about bias.

The fact that the EOF1 has the same sign across the domain is discussed in the revised version of the manuscript. We interpret this feature as a bias correction.

Lines 491-493: it is not clear if the improvement you are referring to is relative to the 'older' model version, or relative to the forecasts (which is the comparison that has been made throughout the paper).

The paragraph in the summary has been revised.

References

J. M. A. C. Souza, B. S. Powell, A. C. Castillo-Trujillo, and P. Flament. The vorticity balance of the ocean surface in Hawaii from a regional reanalysis. *Journal of Physical Oceanography*, 45(2):424–440, 2015. doi: 10.1175/JPO-D-14-0074.1. URL http://dx.doi.org/10.1175/JPO-D-14-0074.1.