

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 #2

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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 authors mention in the introduction and the summary that the system presented

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

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

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

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

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

Lines 140-141: How close and how shallow?

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

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

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

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

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

Figure 3: Hard to read the legend

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

Line 246: and have higher errors ...?

Line 243-245: This statement is very vague.

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

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) ?

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?

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.

Line 371-372: This sentence is unclear.

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

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

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salinity below 500m?

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

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?

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

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

Line 446: increasing or decreasing (you don't show the temporal expansion function which could have both negative and positive sign)

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

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).

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