Point-by-point responses for review #3

In general:

All authors appreciate greatly for the encouragements and comments from reviewer 3. Coordinating with addressing the comments of reviewers 1 & 2, all points that reviewer 3 concerns have been fully addressed in the revision. The paper is renewed as the reviewer's suggestions. New tables have been added in the paper. We also exchanged the location of section 2.2 and 2.3. What follows is a point-by-point reply for reviewer 3:

General comment:

This paper investigates how the model parameter estimation works in an EnKF for an atmosphere-ocean coupled system. This study performs a series of parameter estimation experiments using a low-order, toy system based on the famous Lorenz-63 three-variable model but with an extension of additional near-surface and deep ocean components. The results are somewhat interesting that the fast atmospheric component's state estimation plays a key role in the parameter estimation problem both for the ocean-atmosphere coupling coefficient c2 and the internal dynamical parameter a2 for the second atmospheric variable x2. I find the topic of parameter estimation stability jointly with state estimation stability is very interesting, and this paper is a useful contribution in the field, although could be done better. I find the value of publishing this article, but I found some issues that need to be addressed before final publication as below:

RE: Thanks for your encouragement. All issues are replied point-by-point as below. We hope the whole manuscript has been essentially improved.

1. There are a number of grammatical errors, which need to be corrected.

RE: A few rounds of reading/editing from native English speaker were conducted. The grammatical errors were fixed. Thanks.

2. {"Signal-to-noise" of the ensemble-based error covariance between the states and parameters appears repeatedly, but there is no direct investigation about it. Since this study performs idealized toy-model experiments, I would assume that the authors may find a better way of investigating and presenting the signal-to-noise more explicitly.}

RE: Thanks for the suggestion. The signal-to-noise ratio of the ensemble-based error covariance between the states and parameters is better to be diagnosed in SE only experiments. In these runs, there are no PE processes to fix the biased parameter spread so that parameter perturbations can be fully transferred to the model states. Then the state-parameter covariance can be checked without any disturbance from a PE correction. Following the previous work of Zhang et al. (2012), we defined a new index (called r_{s2n}) to measure the signal-to-noise ratio of the ensemble-based error covariance between the states and parameters. The best (worst) representation of the signal-to-noise ratio is characterized by a r_{s2n} value of 1(0). A new Table 3 is added in the paper. It shows all r_{s2n} and related values in the 8 SE only (no PE) experiments. Description of this index and related discuss is added in P9L16~27.

3. {P.7, L.7-9, "Here our results suggest that in a coupled system, to determine oceanic coefficients, it is more important to get more atmospheric measurements to constrain the atmospheric states than to get more oceanic measurements to directly apply to oceanic PE." This is an interesting hypothesis inspired by the simple toy model results, but this statement seems to be an overgeneralization. The real coupled atmosphere ocean system is much more complicated than the two-time-scale toy system with only 3 atmospheric and 2 oceanic variables. This statement should be a hypothesis or speculation at this point.}

RE: The sentences are rewritten as in P8L10~12. Thanks.

4. { 4. P.7, L.21-22, "reducing x2 uncertainty is critical", I do not find this statement well supported or proven by the experimental results. This statement seems to be a hypothesis or speculation.}

RE: The statement was changed to "Instead, reducing x2 uncertainty (enhancing the estimation accuracy of the atmospheric states) is more relevant to the solution of the problem." (P8L24~25)

Minor comments:

1. {Eq. (2) does not contain observation error statistics, and I am curious how to interpret this equation intuitively. I understand that this equation gives analysis increments for the ith ensemble member. The analysis increments should balance between the observation error and background error. This equation has only the background error variance in the observation space as the denominator, but does not contain the observation error variance which usually appears in the data assimilation equations as an R matrix.}

RE: The observation error variance is calculated before this projection process. The observation are firstly compared to their simulated values, the difference between them are manipulated to produce the observational increments. The production of the observational increments considers the observation error variance and its PDF. The observation error is set as a constant number in our simulation. The standard deviation of "observational" errors are 2 for the atmospheric variables $x_{1,2,3}$ and 0.2 for the oceanic variable w. New introduction of the EAKF method is added in the section 2.2, P4L1~10.

2. P.6, L.30, eta-to-c6 PE suddenly appears here, without any description about observations for eta (deep ocean state variable). Section 2.2 described only x2 and w observations, and the readers would assume the experiments use only x2 and w observations.

RE: Thanks for the reviewer's suggestion. Description about observations for eta is added in the new η-to-c_6 section (P7L31~34). It directly points out that the experiments uses η only for the PE and uses all state variables for the SE.