Referee #1 (Dr. Yue Ying)

Thank the authors for careful revision of the manuscript and addressing the issues. The replies have been satisfactory and I would suggest the manuscript be accepted. Some very minor wording issues can be fixed, which I list below.

We thank the reviewer for carefully checking the manuscript. We have modified the manuscript following your comments.

#1) Line 105: ...because SSH depends on..., I suggest starting a new sentence here, and "might be" sounds uncertain, I suggest you say it more definitively like "SSH tends to be overcorrected if...".

We have divided the one sentence into two sentences, and have replaced “might” with “tends to”.

#2) Eq. 1: try to use [()] when parentheses are nesting (it is clearer).

We have modified the outer parentheses in Eq. (1).

#3) Line 152: surface horizontal velocity can be approximately represented..., why "approximately", shouldn't it be "exactly" geostrophic + ageostrophic velocities? Or you define total velocity = geostrophic + ageostrophic + residual? Be precise here.

Given the quasi-geostrophic velocity equation, it is obvious that the velocity should be decomposed into geostrophic, ageostrophic, and residual. In the first sentence in subsection 2.4.1, we have removed “approximately” indicating the assumption of the geostrophic approximation.

#4) Line 153: Instead of saying "ageostrophic velocity is assumed to be caused by...", you shall just state that in this study you "define" ageostrophic velocity as... (then I guess you have residual velocity to close the equation?)

We have replaced “assumed” with “defined” in the second sentence in subsection 2.4.1. In the classical Ekman theory, the ageostrophic velocity is derived on the assumption that the geostrophic vertical shear is neglected. Cronin and Tozuka (2016) proposed the frontal Ekman theory where the geostrophic vertical shear has substantial impacts on the

ageostrophic velocity especially around the frontal regions.

#5) Line 155: "atmospheric field is not analyzed": Just mention that the surface wind stress (atmospheric field) is from the lateral boundary condition that is not part of the ocean model state vector (in the DA analysis).

We have modified the third sentence in subsection 2.4.1 to indicate that the atmospheric field is not included in the model state vector.

#6) Line 175: Be precise in statements: If analysis increment satifies the geostrophic balance (or the NBE actually?), there will be "zero" Delta_NBE and "no" shock.

As indicating the reply to the third comment, there might be residual velocity even if the geostrophic balance is completely satisfied in the analysis field. Therefore, we have maintained the last sentence in subsection 2.4.1 to give an accurate explanation.

#7) Line 185: Suggest rephrasing: "Significance of the improvement/degradation to the dynamical balance and analysis accuracy is tested in a bootstrap approach. We resample 10,000 cycles from the assimilation experiments and IRs at a 99% confidence level are considered significant"

We have modified the last sentence in subsection 2.4.2 referring to the reviewer's comment.

#8) Line 309: "might result in a better", delete "might"

We have removed "might" in the first paragraph in subsection 4.2.1.

Referee #3

Thank the authors for their efforts to address previous comments. In my opinion, the manuscript is significantly improved, especially with the forecast results.

We thank the reviewer for your helpful comments. We have added the forecast RMSDs of the SSH and SSHA relative to the AVISO to Fig.7 and confirmed that the results of the forecast RMSDs are qualitatively almost consistent with those of the analysis RMSDs.

1. I want to confirm that experiments with IAU (e.g., NO INFL + IAU, RTPP+IAU, RTPS+IAU) do not update SSH? At l105, "because SSH depends on density and might be overcorrected if the temperature, salinity, and SSH increments are used at the same time". I am curious that is there an IAU experiment tried with SSH increment? SSH could be more sensitive to imbalance than the other variables, and thus IAU might have stronger impact on SSH than the other variables. Since the sensitivity experiments without IAU updating SSH but sensitivity experiments with IAU not, there is a gap for explaining the results. Are the differences among the experiments with and without IAU from IAU only or from IAU and no SSH update? It would be helpful if the authors can shed insights on this gap between the two groups of sensitivity experiments.

As indicated by the original manuscript and our reply to the previous comment, the SSH increments are not used in all IAU experiments since the SSH increments tend to cause initial shocks. In the IAU experiment, the SSH is modified properly in response to the temperature and salinity increments. Table 2 of Martin et al. (2015) shows that no SSH update in the IAU is adopted in four out of six existing ocean DA systems, and therefore, it is considered as a common approach to ocean DA. We have added the related description to the end of subsection 2.1.

2. Thank the authors to run additional forecasts. Figure 7 shows the forecast errors at 11-d lead times for surface winds. How about the forecast errors for SSH and SSHA? It would also be helpful to show the errors at different forecast lead times.

Following the comments, we have added forecast RMSDs of the SSH and SSHA relative to the AVISO to Fig. 7 in the revised manuscript. The results shown in Fig. 7 generally agree with the results of the analysis RMSDs in Fig. 3, except for the RTPP09+IAU and RTPS09+IAU experiments showing improved forecast SSHA accuracy relative to the NO INFL+IAU experiment. We have added the description to the last paragraph in subsection

4.2.1. We have also confirmed that the results are qualitatively the same if the forecast period is changed.