

The manuscript “Data assimilation of Argos profiles in North-west Pacific Model” by Wang et al uses a 3DVAR with a novel specification of the background error covariance to assimilate Argos profiles in the North-west Pacific and validates the performance. Overall the manuscript is not well-written, with many grammatical errors, the figure caption confusion and awkward wording throughout the text. It is my recommendation that this manuscript should be significantly revised before it is considered for publication.

### **Major comments**

The background error covariance matrix  $B$  is one of important factors which affect the performance of the assimilation. A good  $B$  should reflect the flow-dependence of the background error covariance. In this manuscript, the authors mentioned a novel method used to estimate the  $B$ . More details should be given. E.g. How to calculate the VEOFs ? The VEOFs are associated with the univariate or multivariate? What is the structure of the  $B$ ? What is the difference from the conventional  $B$ ? What are the advantages of the new  $B$  in describing the structure of the background error covariance? Some figures are necessary.

To assess the performance of an assimilation method, the comparisons with the independent observations are necessary. The comparisons with the observations used in the assimilation only confirm the assimilation code, and make no significant sense. The independent observations are obtained by withdrawing some observations from the assimilation experiment. In the manuscript, the EN4 and MGDSST are used for comparisons. But, these are not independent observations (EN4 include Argo, MGDSST is the relaxation data of the model). The comparisons to the independent observations should be added. The authors may validate the performance via many ways. E.g. The assimilation system is aimed for the operational forecasting. The authors may carry out some forecast or hindcast experiments with the initial conditions from the assimilation or no assimilation, and evaluate the forecast skill. The sense is very significant. Or the improvement on some physical phenomena or mechanisms are also convincing evidences.

Page 6, Section 3.1, the description throughout the paragraph 2 is confused. E.g.

“The model SST (Fig. 3.1(b)) is consistent with that derived from MGDSST (Fig. 3.1(a)).” Firstly, in Bohai sea and Japan sea, the difference reaches  $2.5^{\circ}$ , is it consistent? Should point out the regions where the model is consistent with observations. Secondly, according to this statement, Fig. 3.1(a) should be MGDSST, but the figure caption indicates AF. In the statement “In subtropical basins, temperature is generally high near the western boundary. While in sub-polar basins, the zonal temperature gradient reverses sign, with low temperature in the western basin.”, according to the definition of the subtropic (north of  $23.5^{\circ}\text{N}$ ) in the manuscript, fig3.1 does not show high temperature near the western boundary in the subtropic basins, and the zonal temperature gradient reversed sign with low temperature in the western basin in the sub-polar basins. In the statement “Simulated SST is higher in summer and lower in winter, compared to MGDSST (Fig. 3.1(d)).”, where is figure 3.1(d)? Moreover, according to figure 3.1(c), the simulated SST is lower than MGDSST in summer(July) and higher in winter(January).

#### **Minor comments.**

1. Firstly, what is Argos? Is it the same as Argo? If yes, “Argo”, instead of “Argos”, should be used conventionally. If it is an abbreviation, please give full name.
2. How long is the assimilation window, the frequency of the assimilation?
3. MGDSST should not be considered as independent observations, because it is the relaxation data. Many SST observations are available. Why to choose it?
4. Figure 3.1 should be SST, but the caption is at 150m. The depth 150m should not be as the sea surface. Please confirm it.
5. Why the SST after assimilation is higher than MGDSST while the SST without assimilation is lower in the Bohai Sea and Japan sea in July? Why is it not the case in other seasons?
6. The experiment without assimilation should be added to fig3.2
7. It is difficult to identify the difference between AF and EN4 from Fig 3.3 and 3.4 due to such a deep depth. The maximum of the observation depth is about 2000m,

and the impact of the assimilation on the upper ocean is greatest. To highlight the difference, the depth of figure 3.3-3.4 should be limited to 1000m.

8. The figure captions are not clear. E.g. Fig3.5-3.6 should give the specific date. Fig 3.7-3.8 are related to the whole column or sst alone?