

# Response to the Referees' Comments

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# Response to Anonymous Referee 1

We thank the reviewer for their careful follow-up and for the helpful minor suggestions, which we have addressed to further improve the manuscript.

## 1

**Line 192.** The DUACS spatio-temporal grid does not represent the formal resolution of the data. Maybe the text should say, “DUACS provides data at daily temporal increments on a 1/8 degree spatial grid.”

The revised manuscript has been modified accordingly.

## 2

**Figure 1.** “Maps of deviation from cyclogeostrophy”. I understand NeurOST to be an ocean surface topography product. What constitutes the truth for this product in order to determine deviation from cyclogeostrophy? Is the minimization-based method assumed to be the truth?

There is no “truth” here, we simply show the evaluation of the cyclogeostrophic imbalance from Eq. 9. In the revised manuscript, the caption now state more clearly: “Maps of cyclogeostrophic imbalance, computed from Eq. 9 ...”

## 3

**Line 338.** “As discussed in Sections 2.2 and 2.3, these differences are likely linked to the distinct mathematical nature of the two approaches.” As written this could be interpreted to say that the mathematical nature of the two approaches is the same for both and distinct from other methods? If the intent is to say that there are mathematical distinctions between the two approaches, then it would be good to say that directly: for example, “As discussed in Sections 2.2 and 2.3, the differences are likely linked to the mathematical distinctions between the two approaches.”

The revised manuscript has been modified accordingly.

## 4

**Figure 5.** The distinctions between panels b and c are difficult to see. I think the authors want readers to notice that there is more blue in panel b and more red in panel c, but that point is not universally consistent. The text at line 398 points to western boundary current regions, but these regions are small and difficult to see. Would an inset panel help to highlight the relevant differences?

Indeed. In the revised manuscript, we have added insets to Fig. 5 and refer to them in the text.

## 5

Line 194. “data has” -> “data have”

Line 347. “results suggests” -> “results suggest”

Line 370. The reference to three previous studies, separated by semi-colons, doesn’t read cleanly. Maybe a better phrasing would be, “Consistent with previous work (e.g. Archer et al, Tchonang et al, Tranchant et al), ...”

Line 401. Missing word. Change to “rather than geostrophy”

The revised manuscript has been modified accordingly.

# Response to Anonymous Referee 2

We thank the reviewer for their positive assessment of the revised manuscript, noting the improved clarity and scientific content. We have carefully considered their minor suggestions and addressed them in the revised version.

## 1

**Line 37:** The paper of Dù et al. [2025] might be worth mentioning here. It is a modeling work that extends the geostrophic balance. Although it does not directly involve SWOT data, it investigates the theory behind the common approximations, including cyclogeostrophic balance. For example, it elucidates the connection between Zhang and Callies [2025] and the cyclogeostrophic balance.

Dù et al. [2025] might indeed be of interest for some readers, and it refer to SWOT even if it is not at all the core of the paper. We added the reference in the revised manuscript.

## 2

**Line 45:** the term “ageostrophic” and “not capture by the balance” is confusing.

We remove from the revised manuscript the “not capture by the balance” part which is indeed a bit confusing.

## 3

**Line 137:** This sentence is confusing. Do you mean it can be written as an L2 norm, without the integral? I do not see how it is used in later “further discussions”.

In the revised manuscript we clarified the sentence and make it explicit in which section of the manuscript the distinction is helpful.

## 4

**Line 236:** Here, a discussion of the error of SWOT SSH is warranted. For the purpose of showcasing the method, it is ok to not add noise into the simulated data. But SWOT observation is definitely not going to be the same as the model data. Error or SWOT is discussed in many papers, including Wang et al. [2019], Peral et al. [2024], Zhang and Callies [2025] for a non-exhaustive list.

We agree that a note on the SWOT data errors is useful. We added one in the revised manuscript, along with references explicitly discussing SWOT errors.

## 5

**(14):** for the model case, the sum should also include over the domain points. So N should be much larger than 31.

As already stated in the text, in Eq. 14 the sum is applied point-wise (spatially) along the time dimension. Therefore,  $N=31$ , the number of days in the month of August.

## 6

**Line 342:** In fact Tchonang et al. [2025]’s results show cyclogeostrophy did not improve over geostrophic balance. One should be careful when citing.

Indeed, it was introduced as at some point during the revision process when discussing the regional differences in performance when applying the cyclogesotrophic balance, and we forgot to remove it when using the reference in the eNATL60 section. It has been removed from the revised manuscript.

## 7

**Line 355:** Standard error made a sneaky comeback. This is perhaps a typo.

Indeed, it is fixed in the revised manuscript, along with another typo.

## 8

**Figure 4:** Is it correct to call these RMSE? What mean?

The mean is performed along the time dimension (as explained when introducing Eq. 14). In the revised manuscript we reorganised the sentences of Fig. 4 caption to make it more explicit.