

The newer version of this manuscript, in my opinion, flows much better. The scientific content is also much improved. I recommend its publication.

Here are some minor suggestions:

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.
2. Line 45: the term “ageostrophic” and “not capture by the *balance*” is confusing.
3. Line 137: This sentence is confusing. Do you mean it can be written as an L^2 norm, without the integral? I do not see how it is used in later “further discussions”.
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.
5. (14): for the model case, the sum should also include over the domain points. So N should be much larger than 31.
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.
7. Line 355: Standard error made a sneaky comeback. This is perhaps a typo.
8. Figure 4: Is it correct to call these RMSE? What mean?

References

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