Ocean Science Discussions

Ocean Sci. Discuss., referee comment RC2
https://doi.org/10.5194/os-2021-19-RC2, 2021

Comment on os-2021-19

Anonymous Referee #2

Referee comment on "Background stratification impacts on internal tide generation and abyssal propagation in the western equatorial Atlantic and the Bay of Biscay" by Simon Barbot et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-19-RC2, 2021

General comments

The manuscript, "The dependency of internal tides on background stratification variability: a case study on the Amazon shelf and the Bay of Biscay" by Barbot et al., reports on two types of analyses. First, a clustering algorithm is applied to vertical density profiles to characterize stratification variability in two locations. The second analysis uses an idealized model of internal tide generation to estimate variability of wavelength and vertical mode content. These analyses are motivated by the upcoming SWOT altimeter mission and are potentially useful additions to the scientific literature. In particular, I think the clustering methods, with a few improvements and perhaps additional locations, can be a nice contribution. I have a few concerns about the idealized internal tide modeling. In particular, I am not sure of the utility of the chosen model, especially because its current implementation is not well-explained in the current manuscript. Although overall the analysis and figures are engaging, I found much of the language used to be awkward and at times misleading. I strongly recommend additional editing of the language before resubmission.

Specific Comments

My first comments concern the motivation of the work. In Section 3.3, I believe Figure 10 is not what you want to show? If one of the motivations are to quantify internal tide non-stationarity, this is not done. Stratification profiles are examined, and non-stationarity is inferred, but don't the realistic models, HRET V8.1 and NEMO (line 605) include non-stationary tides? Why not use the 2D FFT method to actually address the non-stationarity effects?

The second motivation difficulty is that the distinction between two regions is attributed to differences in factors controlling stratification. Namely, the authors suggest the

significance of solar radiation compared to geostrophic currents controlling stratification. But then in all of the simulations, the stratification is horizontally uniform. So, how are the effects of currents on stratification actually retained in these results?

On Line120, the authors state: "Because the ocean circulation affects the ITs propagation, the complexity of its impacts on the ITs is beyond the scope of this study. Even though the stratification will be derived from the circulation, the stratification will be investigated as stationary in order to prevent further interaction with the circulation." These are very confusing sentences.

My second comment is on the profile clustering methodology. I have a few suggestions that can clarify:

Line 205 - 235: The description here should be improved. For example, why are there only 2 coordinates in the PCA? Section 2.2.2. discusses some optional parameters in the clustering, but what is the effect? In particular, I am not sure how the authors determined "the best results (line 235)" and why the Ward method would have a stability criteria? Perhaps this sensitivity analysis can be moved to an appendix that includes a few of the examples written in words here, but instead portrayed graphically so that the reader can follow?

My final general comment concerns the use of the tidal model in this study. I think there needs to be much more care in this section:

The choice of bathymetry (Equation 4) seems very generic, while the two basins shown in figure 2 are very different. How relevant is this choice of idealized bathymetry to either region? Perhaps comparing a transect of bathymetry from figure 2 to equation 4 would be useful.

If the overall point that the authors want to make concerns the variability in IT wavelength at multiple vertical modes, a linear eigenvalue solution to the stratification profiles would give that result without needing this idealized model.

I do not recommend the authors use the Nugroho, 2017, reference as a primary citation for 3D T-UGO model configuration or for the modal decomposition as this work has not been through peer-review. Instead, if these modelling results are to be used, many more details can be provided: What are the equations solved, boundary conditions, and solution procedure?

Line 620: "The bathymetry of the T-UGOm simulations is set capped to 4000m whereas

the real bathymetry in the area can extend down to 4500 min the generation zone and down to 5000m further north" How do the authors know that internal tides are generated at these depths?

Technical corrections

Please rewrite the sentences on line 275. They are very confusing and grammatically incorrect: "The temporal variability of the clusters (Fig. 3b,e) shows that every cluster happen all the year. There is a seasonality very noisy due to the complexity of the circulation, its spatial distribution and its seasonality (explained below). The cluster classification enable to focus on a simple parameter (the pycnocline depth) rather than being blurred by the noise of a classical seasonal average classification. "

Line 375: Missing a period "… uniform horizontally There, the cluster…"

Line 375: I don't understand the use of the word "concise" in this sentence.

Line 380: I don't understand what the relevance of these statements. Can you reframe?

Line 385: What is the relevance of observing long-term variability here?

Line 390: "Grid"

Line 400: "This enables us to compare the simulations with realistic cases." What cases are you referring to?

Line 630: I am not sure that what the authors propose here would work. Wouldn't the addition of a mesoscale create non-uniform horizontal stratification? How would a cluster analysis help in that situation?

Line 670: "The definition of a good parameter controlling the ITs amplitude and wavelengths need to be pursued in mid-latitude to unify the processing of the different regions of the global ocean." I do not understand this sentence. Can you rewrite?

Figure 8 caption: I don’t understand this, please reword: “…the colored patches represent the part of each mode in the sum: the modes on top of the sum line refer to destructive interaction between the modes.”