We do thank Referee#1 for his/her careful reading of our manuscript and relevant comments. Below his/her comments are listed (after #, in italic) and followed by our answers (after >>) as well as the modification in the manuscript (between "…"). Changes made in the manuscript are highlighted in red and blue in the revised version of the manuscript. As some remarks of the General comments are also addressed in more details in the Specific comments, we took the liberty to reorganize the document by gathering together the comments tackling the same issue, in order to make our answers clearer. Each subject of the general comments is gathered under a dedicated title. We hope this will ease the reviewer's reading.

Overall comment #1:

In that last case (western equatorial Atlantic), I would also emphasize that all modes are not necessarily present in the same areas (for example in one season mode 1 has a strong dominance in northern NBC retroflection, and thus not present near the shelf break closer to the equator, and modes 2 and 3 on the other hand seem to be more present near the equator. Then, between 2 and 5°N, there is a bit o AS-1 and AS-4, AS-5. I mention that, as I think that the whole range of solution explored (for example in figure 6, right panels) might not necessarily be present at the same place (at different times). Thus, maybe proceding in that way might locally overestimate the range of variability in ITs characteristics that is possible due to changes in stratification.

>>The spatial distribution of the clusters in the western equatorial Atlantic is exposed with Fig.3c and mention l.303. In order to clarify the discussion, one general sentence in the results (l.297) and a dedicated paragraph at the beginning of the discussion (l.364) are added:

l.297: "In addition, the spatial distribution of the cluster is not homogeneous within the area highlighting spatially-bound processes responsible for some specific stratification."

l.364: "The stratification of the two areas of interest are driven by very different forcing: the Amazon plume and the circulation for the western equatorial Atlantic and the radiative forcing for the Bay of Biscay. The Amazon water recirculation at the North of the domain and the NBC rings along the shelf break are limited-extent processes whereas the radiative forcing affects the Bay of Biscay homogeneously. Thus, the spatial variability is stronger in the western equatorial Atlantic. In such area, the stratification variability presented by all the clusters does not happen in every parts of the domain but this method enables us to distinguish the specificity of each sub-region at once."

Overall comment #2:

#In the presentation, I also wondered about the choice of presenting together the clustering in the two regions, and then the TUGO solutions in the two regions. I wonder whether it would not make more sense to have one chapter for the western equatorial Atlantic (clusters, then simulations), followed by one chapter for the Bay of Biscay (cluster, then simulations).

>>This choice is made in order to emphasize the variety of stratification variability that the clustering is able to handle with a common setup. Addition at the end of the introduction:

l.86: "This organization enables to better compare how the presented methodology can handle the two areas that have two different stratification variabilities."

Overall comment #3:

#I also found the paper's title misleading. There is no discussion in it of internal tides on the Amazon shelf. The AMazon shelf is not really considered, the profiles analyzed are not on the shelf (where there is often a large freshwater cap), and the model solutions discussed are for the deep ocean ot its east. I would thus strongly recommend a title change. Maybe: '... : case studies for the western equatorial Atlantic and the Bay of Biscay'

Related comments:

#l. 115-119, this is not a very good introduction for the Amazon shelf… (where freshwater inputs are major sources of stratification variability) (I realized after that most of the emphasis and investigation is not on the Amazon shelf, but on the nearby deep tropical Ocean; for that, the title should not be 'a case study on the Amazon shelf…).

#l; 153: from what I know, the latest versions (>5.0) also include the individual profiles. I would use one of these versions which also includes non-Argo data, and thus CTD data from cruises on the shelves (that are clearly mostly absent of the data set used for the analysis presented) (later I read (page 10) that the period investigated was 1984-2015; this period includes a large number of research cruises and surveys on both shelves).

#Figure 2: only show data that are not on the shelves (?)

#l. 175: 'For the Amazon shelf area…' misleading. The figure clearly shows that the Amazon shelf is almost ignored (almost no data). Similar in the Bay of Biscay for the shelf areas. These are regions with lots of CTD cruises (in particular Bay of Biscay, but even on the Amazon shelf, there is a large number of CTD (and Ocean stations) available, as can be seen by a quick search with the NCEI tools, but I am sure that the same would be true for recent versions of CORA (version number > 5.0)

#l. 188: does I understand correctly that profiles not extending to at least 600m (Amazon shelf) and 300m (Bay of Biscay) are not considered (thus removing all shelf areas, as well as part of the slope areas).

#l. 356: do not refer to the data set as 'Amazon shelf'.

>>Thank you for pointing that out.. We used the term "Amazon shelf" because we cannot find a proper name for this part of the ocean, but we realize that it can be confusing as the focus is not made on the shelf itself. Indeed, the term you propose "western equatorial Atlantic should be less confusing. It has been modified everywhere in the article. "AS-#" clusters are renamed "WEA-#". A new title is proposed:

"Background stratification impacts on internal tides generation and abyssal propagation in the Western Equatorial Atlantic and the Bay of Biscay "

CORA V4.3 does provide the cruises data and some of them are used in the selected data we used.

Important comments :

#l. 79: 'stratification is not a force'. I think that the first sentence should be reworded. Maybe 'stratification is what 'controls' the internal waves'.

>>Reformulated:

"The stratification controls the buoyancy of the water masses that is the restoring force of the internal waves."

#l. 95: notice that the dispersion relation is 'local', and not as the sentence starts with 'For a given N profile'

>>Thanks you for pointing that out. "For a given N is removed".

#Figure 2: What is the unit of the number of profiles (panel d) and what is the period considered for the profiles.

>>The number of profile has no units: " Number of profiles [#]". Period used: 1984-2015. Figure edited.

#l. 190-192: these lines of comments should be placed elsewhere, probably in the introduction.

>>Moved to the section describing the model configuration (l.436-439).

#l. 205: what are these two PCA axes? Why two?

>>Addition of few sentences to clarify this point:

l.220: "As the profiles are only described by the density versus depth, only two principal components are used. Thus, the shape of each profile is evaluated according to two orthogonal axes. The PCA manifold is the plan defined by these two new axes and where each profile is characterized by a point. The both axes explain a different part of the standard deviation of the profiles. For example, if the profiles are mainly controlled by the pycnocline depth but also by the surface density, the first axis (PC1) will be controlled by the different depths of the pycnocline whereas the second axis (PC2) will be controlled by the surface density."

#l. 115-119, For the surface layer, instead of circulation, I would indicate 'water masses'.

>>The water masses are already mentioned: l.131 "The second one is the circulation of the ocean and the induced mixing of the water masses." Maybe it is better this way:

"The second one is the mixing of water masses induced by the circulation."

#l. 236: I don't understand the sentence 'For the Bay of Biscay, 10 clusters are not enough…(why does it imply that 6 clusters is a good compromise, as mentioned in the next sentence).

>>The section 2.2.2 (now 3.2.2) have been rewritten on the advice of Referee #2 and further details are presented about the clustering methods tests in the appendix A. This specific sentence is reformulated:

l.249: "For the Bay of Biscay, the variability of the density profiles is way more complex and N profiles are very different even for 10 clusters. But a high number of clusters leads to have some clusters with few profiles. Thus, for both areas, a classification of 6 clusters is a good compromise that enables us to detail the evolution of the density profiles while keeping well represented clusters (more than 100 profiles)."

#l. 244: I don't understand what is done. The previous analysis done to 600m (300m) for the two regions. Is it that the vertical profiles are then considered below those depth ranges, and a median average is estimated for the profiles included in each cluster?

#l. 247: 'as such a deep depth'? Which deep depth is considered here? (is it 600m or 4000m? anything in between?)

>>The model configuration is defined from 0 to 4000 m depth so the density profiles used for the modeling need to be extended. This paragraph and the one before are reformulated:

"Once the classification is done, the typical profile from each cluster is calculated in order to use it as a forcing in the simulations (see section 3.1). The density profiles need to be strictly stable and defined from 0 to 4000 m depth.

Because most of the profiles used for the classification are not defined that deep, the completion process is detailed here. The median of the profiles within the clusters is used as deep as possible. The standard deviation below 1000 m is very weak (Fig. 2a,f) so the profile can be completed with the median of the profiles from the other clusters. If the profile does not reach 4000 m, then the bottom of the profile is extrapolated using the density gradient of the latest 4 measurements. The density gradient used for the extrapolation needs to be weaker than 5.0*10^{-7} kg.m^{-4} that is a common gradient at such a deep depth.

The median profiles have been smoothed …"

#l. 289: similarity between AS-2 and AS-3. This is interesting, but why did the analysis project the profiles into the same category. I am also confused in this paragraph, which starts that the NBC is strongly influenced by large anticyclonic eddies, and ending by the clusters identify… the steady state of the NBC.

>>Mention added that profiles in WEA-3 have a shallower pycnocline that the ones in WEA-2. This paragraph is reformulated:

"WEA-2 and WEA-3 are similar: they have the same seasonality, the same spatial coverage and gather the profiles with a pycnocline depth from 80m (WEA-3) to 110m (WEA-2). The North Brazil Current (NBC) is a strong geostrophic current flowing along the Amazon shelf break all year-round. The seasonality of the NBC is mainly influenced by wind-driven eddies from August to November that enhance the retroflection of the NBC water masses into the North Equatorial Counter Current (NECC, Johns et al. 1998). The seasonality of WEA-2 and WEA-3 as the large spatial distribution of the clusters clearly point out that they identify the steady state of the NBC, without the eddies."

 # I guess that the separation between clusters 2, 3, 4, and 5 is a rather continuous transition in pycnocline depth. I was wondering whether maximum N^2 also changes between the cluster, but at first glance this does seem to be the case on figure 3. Another puzzler is that surface density of AS-1 does not seem particularly less than for the other cluster (if I read well figure 3). I would have expected lower surface density as it includes the Amazon plume in NBC retroflection (but also other profiles in the other seasons south of 5°N).

>>The table 1 does highlight that the N^2 value is the same for all the clusters ($2{,}3\text{-}2{,}5*10^{-2}s^{-1}$). The 90% interval of the cluster 1 highlights more low surface density profile than for the other cluster, but yes, the median of the cluster 1 is the same as the other clusters. Maybe it can be explained because the profiles are quite far from the shelf break so the Amazon waters have already mixed down to 50 m.

#l. 346: I am not sure that the differences between the two ranges of years included in ISAS13 and CORA V4.3 explains the difference. I checked in recent years, and the CORA stratification remains. If anything, using a shorter period sharpens it? I suspect that spatial (and even more) vertical smoothing could influence the weaker vertical gradients in ISAS13.

>>The sentence is reworded and the following comment added:

"ISAS13 climatology is only based on the 2004-2014 period but this different period cannot explain all the differences either. The spatial and vertical smoothing applied to construct the climatology might have been stronger in this area compared to the filtering we used here."

#l. 376: what is meant by 'There, the cluster classification is more concise'?

>>Reformulated:

"The 6 clusters classification gathers the same amount of information about the seasonality as the 12 groups of the monthly classification. Thus, the cluster classification is a more condensed approach."

#l. 529: 'the trend' should be replaced (in most cases in this section) by 'the change..'

>>Is the term "pattern" better than "change" ?

#l.601: 'the slope of the shelf'. I am wondering whether it is shelf or slope region that is considered (I believe the 'latter'). At the end of the line 'than in the Amazon' ? What is meant there.

>>Reformulated:

"The slope of the shelf break is a bit steeper in the Bay of Biscay than in the Amazonian region"

#l. 634: 'As shown here, this approach is very useful…'

>>Can the conclusion start this way ? Reformulation:

"The classification of density profiles through clustering methods is very useful to describe both spatial and temporal variability of the stratification."

Minor Comments:

#L. 84: 'Those wavenumbers project on different vertical modes.' (I am not sure I would mention the first mode being the barotropic mode, as the vertical wavenumber mentioned will not project on the barotropic mode).

>>Right, the bracket is removed.

#l. 113: 'Before investigating these impacts, we will discuss the range of stratification variability that needs to be investigated'

>>Sentence replaced.

#l. 170: I would remove the sentence 'The ITs induce pressure oscillations…' (not just the Its, but any adiabatic vertical motion)

>>Sentence removed.

#l. 263-264: erroneous Argo float profiles are found by this approach. This means that there was an error in the flagging. This is quite possible in version 4.3, but should not have happened in more recent versions, where a test of 'possible min-max range is applied at each depth of each profile.

>>Duly noted.

#l. 394 – 396: these sentences can be removed. Maybe to be mentioned in discussion or conclusion.

>>Removed.

#Fig. 6: I don't fully understand what is represented on the right panels. Is it surface elevation? I don't see contributions of modes 3 to 5. Is it that they are negligible, as suggested by Fig. 7 (in which case, no need to put them in caption).

>>Yes it is the surface elevation. The modes 3 to 5 are negligible so they are removed from the figure.

All other minor comments are accepted as formulated by the Referee #1.

We do thank Referee#2 for his/her careful reading of our manuscript and relevant comments. Below his/her comments are listed (after #, in italic) and followed by our answers (after >>) as well as the modification in the manuscript (between "..."). Changes made in the manuscript are highlighted in red ad blue in the revised version of the manuscript. As some remarks of the General comments are also addressed in more details in the Specific comments, we took the liberty to reorganize the document by gathering together the comments tackling the same issue, in order to make our answers clearer. Each subject of the general and specific comments is gathered under a dedicated title. We hope this will ease the reviewer's reading.

General comment #1

#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.

>>The model used in this study does not include any background currents and only resolves the tidal currents, pressure and free surface for the ITs generation and propagation. Some modifications have been made in the introduction and the model description sections to emphasize this. See the answers to the related comments below.

#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.

>>Reformulation:

"In addition of the stratification, the currents also affect the ITs propagation and complexify the ITs signal. The investigation of such dynamical impacts over ITs is beyond the scope of this study. Here, the stratification is investigated without background current in order to only quantify the ITs signal response to the stratification alone. Such stratification is further named background stratification."

#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?

>>The effect of currents on stratification is retained because the realistic stratification profiles are the sum of every processes taking place and the currents are one of them. The current-driven stratifications are used to investigate the response of ITs without any other complexification in order to properly quantify it. The simulations enable to prove that the waters inside the core of the NBC rings (deeper pycnocline) implies a longer wavelength of the ITs than outside the NBC rings (shallower pycnocline). We choose to not deepen the interpretation because the ITs also dynamically interact with the currents and the simulations are not designed for that as there is no background currents.

#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.

>>This generic bathymetry enables to simulate really clean ITs and to only focus on the stratification impacts only. A new figure about the bathymetry comparision is added in the appendix. Following sentence is added in the model description (l.456):

"This bathymetry is similar to an averaged continental slope, a comparison to realistic bathymerty of the two areas of interest is shown on Figure C1."

#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.

>>Such approach could provide the wavelength but not the amplitude of the ITs. As shown on the figure 7 and 9, the amplitudes of the modes are also highly affected by the stratification.

#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?

>>Actually, there is no other references that describe 3D T-UGOm. For short, it solves for the quasi-linearized, frequency domain 3D equations, formulated in the 3D, vertically lagrangian, equivalent of the well-known 2D wave equation. Level displacements and density anomalies (due to advection), governing pressure anomalies, are the primary unknowns. The discretized equations form a linear, complex-valued system which solution is obtained through a single inversion. Once solved, horizontal velocities are obtained by the use of the horizontal momentum equations. However, non-linearities (such as bottom friction/vertical momentum diffusion) are solved by iterating the tidal solver with non-linear terms set from the previous inversion. Boundary conditions are formulated to prescribe the barotropic tidal component and a buffer zone is implemented to avoid internal tide reflection at open boundaries. A complete article is in preparation, based on Nugroho (2017) and the additionnal developpements that have been made since (Lyard et al.). The mention to such article is added to l.429:

"Initially developed to resolve the two dimensional tidal equations (Piton et al., 2020 ; Lyard et al. 2021), this model has been extended to resolve the three dimensional tidal equations in the frequency domain (Nugroho, 2017; Lyard et al., in prep). "

General comment #2

#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.

>>The article will be read by an english-native in order to improve the language.

Specific Comment #1

#My first comments concern the motivation of the work. 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?

>>HRET V8.1 only represents the stationary part of ITs and the NEMO simulations does represent both stationary and non-stationary parts. But the non-stationary part is extremely complex and important. The methods to extract it are very sensitive to the frequency windows used. The quantification of the ITs non-stationarity requires a dedicated study that is in preparation using NEMO outputs (Tchilibou et al.). Here, the motivation is not to quantify the ITs non-stationarity but to improve the comprehension of the ITs variability, to help the community to precise their interpretations of ITs non-stationarity. For this purpose, we focus on the impacts of only one parameter, the stratification, and quantify its impacts on both ITs amplitudes and wavelengths. Reformulation of the goal in the introduction (l. 70):

"The present study aims at contributing to the understanding of the ITs' non-stationarity through the investigation of the ITs variability".

#In Section 3.3, I believe Figure 10 is not what you want to show?

>>Indeed, the figure 10 was a duplicate of figure 9, this has been corrected, sorry for the embarrassment. The figure and the text are updated to consider all the 5 clusters (not only AS-108m) for the comparison (l.670):

"The wavelengths in HRET and NEMO are coherent with both modes 1 and 2 wavelengths calculated from the clusters, but models' wavelengths are slightly longer than the averaged cluster wavelengths. As explained in the introduction…"

Specific Comment #2

#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?

>>Addition of few sentences to clarify this point:

"As the profiles are only described by the density versus depth, only two principal components are used. Thus, the shape of each profile is evaluated according to two orthogonal axes. The PCA manifold is the plan defined by these two new axes and where each profile is characterized by a point. The both axes explain a different part of the standard deviation of the profiles. For example, if the profiles are mainly controlled by the pycnocline depth but also by the surface density, the first axis (PC1) will be controlled by the different depths of the pycnocline whereas the second axis (PC2) will be controlled by the surface depths."

#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?

>>Thank you for this advice, most of the section 2.2.2 (now section 3.2.2) have been moved to the appendix A where 3 additional figures illustrate the discussions. The section has been rewritten as follow:

"Three methods of clustering are compared: Ward, Average and Spectral. Those methods have been selected because they can better classify similar PCA manifold that we have. For each method, the sensitivity of two parameters needs to be investigated: the number of final clusters and the number of neighbors used in the calculation of the distance between profiles. The number of neighbors is important to properly manage the profiles that are isolated outside the PCA manifold. If the number of neighbors is weaker than the number of outsider profiles, then they all would be grouped in a dedicated cluster. Otherwise, they would be included in the cluster of the nearest profiles. This latter case can lead to groups of profiles that do not have the same shape inside the same cluster. The number of neighbors also affects the profiles located at the boundary between two clusters: depending on the number of neighbors, they would be included in one cluster or another.

A wide range of sensitivity tests have been made to choose the best method and the best parameters. The number of neighbors is tested from 4 to 16 and the number of clusters is tested from 2 to 10. These results can be found in the appendix A. The Ward method is used in the rest of the study because it offers a wider range of stratification cases and it is less sensitive to the number of neighbors. The classifications using the 16 nearest neighbors are distributed more equally between the clusters so this parameter is chosen. The number of clusters is set more arbitrarily. For the western equatorial Atlantic, the variability of the density profiles is controlled by the pycnocline depth with almost no modification of the N profile. Thus, few clusters are needed to characterize such variability. For the Bay of Biscay, the variability of the density profiles is way more complex and N profiles are very different even for 10 clusters. But a high number of clusters leads to have some clusters with few profiles. Thus, for both areas, a classification of 6 clusters is a good compromise that enables us to detail the evolution of the density profiles while keeping well represented clusters (more than 100 profiles)."

Specific Comment #3

#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 m in the generation zone and down to 5000m further north" How do the authors know that internal tides are generated at these depths?

>>The term "generation zone" is misleading. The sentence is corrected as follows:

"The bathymetry of the T-UGOm simulations is set capped to 4000 m whereas the real bathymetry in the area can extend down to 4500 m close to the continental slope and down to 5000 m further north."

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. "

>>Reformulation :

"The clusters are not strictly defined during a specific period of the year but rather during all along the year (Fig. 3b,e). In addition, the spatial distribution of the clusters is not homogeneous within the area highlighting spatially-bound ocean processes responsible for some specific stratification. As the pycnocline depth is highly controlled by the circulation, the complex spatio-temporal variability of the clusters refers to the complex spatio-temporal variability of the circulation in this region. The clusters classification enables to focus on a simple parameter (the pycnocline depth) that would be smoothed with a classical seasonal average classification."

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

>> Corrected.

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

>>Reformulation :

"There, the 6 clusters classification gathers the same amount of information about the seasonality than the 12 groups of the monthly classification. Thus, the cluster classification is a more condensed approach."

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

>>Reformulation :

"As the cluster analysis does not preferably consider time dependent or space dependent classification, this method is very effective to investigate circulation-driven stratification variability, such as in the tropics."

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

>> Addition of the following sentences:

"In a classical seasonal or monthly averaged classification, such long-term variability would have smoothed the stratification profiles. Here the clusters mean density profiles are based on similar instantaneous profiles, insuring more realistic profiles."

#Line 390: "Grid"

>> Corrected.

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

>>Reformulation :

" This enables us to compare the simulations with ITs measurements and realistic simulations."

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?

>>Yes but the clustering methods can detect non-uniform horizontal stratification (like in the western equatorial Atlantic). Then, this spatially-bound stratification could be used to create spatially-bound ITs wavelengths maps over abyssal plains and ITs amplitudes maps over generation areas.

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?

>>Reformulation :

"The efforts to find a formulation to link the ITs amplitude and wavelengths to the stratification need to be pursued for the mid-latitudes. is to obtain a parametrization that could unify the different regions of the global ocean.

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."

>>Reformulation on both Figure 6 and 8 :

"…the colored patches represent the modal contribution to the complex sum: if the patch of mode *n* is located on top of the sum line, then mode *n* works against mode *n-1*."