Dear Pr. Fer,

We do thank you for your careful reading of our manuscript and relevant comments. A strong revision of the manuscript have been made to shorten it, clarify the objectives and highlight the main results. Additional results about the ITs energy flux have been produced following the advises of RC#3 and yours. Additional validation of the simulation have been made following the advises of RC#2. The results from Tchilibou *et al.* (to be submitted) about the seasonal study of ITs in the NEMO simulation are provided in advance thanks to the authors and will be submitted to Ocean Science journal by September 2021. The appendix about the sensitivity of the clustering methods have been moved to supplementary material as the methods are not new and the description quite long.

Below your comments are listed (after #) and followed by our answers (after >>). The modifications are provided when they are not too long (between "..."). Changes made in the manuscript are highlighted in red and blue in the revised version of the manuscript. We took the liberty to reorganize the document by gathering together the comments tackling the same issue, in order to make our answers clearer. We hope this will ease the your reading.

#I strongly support reviewer #3's suggestion on a discussion of baroclinic energy fluxes.
>> A subplot is added on the figure 6 and 8 (previously fig.7 and 9) to show the energy flux of each simulation for the five first modes. To meet more realistic energy fluxes, the barotropic tidal current have been reduced to 5 cm/s. This modification does not change the conclusions highlighted in the study and only affect the amplitude of the values. The discussion about the comparison between the simulation amplitudes and the ITs altimetry atlases have been updated consequently.
Energy flux value have also been used to validate the T-UGOm simulations (WEA) comparing them with NEMO simulation (data from Tchilibou et al., to be submitted).

#There are repetitions and basic textbook information which can be reduced substantially. I made some suggestions and give examples below, but these are not exhaustive. Please attempt similar cuts where needed. Line numbers refer to manuscript-version3.pdf. I stop my comments at Section 4.2 and expect you to make similar improvements and clarifications throughout.
#Abstract is too long, and in parts not clear (e.g. last sentence can be rewritten). Some other suggestions are below.
>> Many parts have been removed and/or reformulated to improve the reading.

#Li3, not clear to the reader why the repetitivity of the orbit implies what you claim.
#Li1-3 could be shortened, e.g. «The forthcoming SWOT altimetric missions aim to resolve the mesoscale with an unprecedented spatial resolution and swath, but high frequency processes, such as internal tides (ITs) are undersampled in time and aliased onto lower frequencies."
>> The SWOT mention have been shorten as suggested.

#Li 9-12, can be shortened: "Here we present a method to quantify the impacts of background stratification using a clustering method for classification of a broad range of stratification and idealized modelling of the ITs in frequency domain." (avoid T-UGO which needs introduction, and there's no room in the abstract)
>> Done

#Li 15-16, for example: "…increases the total ITs' amplitude, transfers energy from mode 2 to mode1, and increases the wavelengths of both modes. In the …"
>>Rephrased: "For the western equatorial Atlantic, a single pycnocline is observed and only the two first vertical modes of ITs have significant amplitudes. The depth of this pycnocline linearly impacts on the amplitudes and wavelengths these two modes. An increase of the pycnocline depth increases the total ITs' amplitude but also transfers the energy from mode 2 to mode 1."

#Li 17-18: "…of modes 2 and 3 and the surface elevation of ITs. On the other hand, the wavelengths…."
>>The results about the wavelength have been removed as they are not the main point of the article (in agreements to the comments of reviewer #2)

#Avoid use of «significant» when not related to statistical analysis (e.g., li6, li18, where «substantial» could be an alternative)
#Throughout: care with use of “e.g.” (see lines 37-38, all e.i.’s must be e.g.)
>>Done

#Li59- coarse temporal resolution
>> Done

#Li 53: is T/P introduced?
>>Corrected

#Li 76, reword,”after a brief introduction to Its and stratification”…(but I recommend you heavily reduce this brief introduction).
#Li 122-130: the entire set of two paragraphs can be cut out.
>>The section « Background knowledge » have been removed as well as the figure 1. The introduction have also been reworked and shorted to clarify the objectives and the approach.

#Eq.1 : This equation is not correct and is definitely not what is implemented in TEOS-10. In li173 you say you are using TEOS-10 for calculation of potential density profile. Are you also using it to calculate Nsquared? If so, give the TEOS-10 equation (and also with potential density approximation) for Nsquared (see their manual). Your rho0 appears to be pot density referenced to surface pressure. It is very important to note that (when working with water depths of several km) the potential density used in the gradient must be referenced to local pressure when calculating Nsquared.
#Fig 2 caption: What do you mean “in situ” density profile when you prefer potential density anomaly? Must also mention the reference pressure. How is N2 calculated? Using the mean density profile?
#Li170-175. Readers of Ocean Science know what “potential density” is. Cut out and simplify this to: “Potential density and N are calculated using the TEOS-10 ……”
>>The figure 1 (previously fig 2) now shows the buoyancy frequency as calculated in TEOS-10, the equation have been removed and the text have been reworked.

#Li 140: plural extramum is extrama
>>Also corrected everywhere else.

#Fig 2. There’s a sharp change in N2 at 1500 m depth for both sites. I am sure this is not natural and is an artefact of your smoothing filters.
#Li 245-250: all this filtering appears confusing, not well justified (especially see the profile change at 1500 m). A simple sorting of the median profile, and them vertical smoothing over 25 m or so would have been much simpler (or perhaps a spline fit to selected data points of the profile).
>> Yes, the smoothing method have been modified, thank you for pointing that out.

#Li 193-[statically] stable density
>>Done

#Li 196-202: need substantial cut and simplification. Example: “All profiles with more than 5 measurements over upper 100 m, where the stratification is most variable, are linearly interpolated to a uniform 1 m vertical resolution, and processed using the principal…..”
>> The section about numerical processing of the profiles have been reworked and shortened.

#Li 239: …profiles must be stable and defined…
>>Done

#Li 243: of the deepest 4 measurements
>>Done

#Li 263: This reads like you “reserved” the 6th group to suspicious profiles, but this is not the case for BoB. Please reword.

>>Rephrased: "As these profiles are less numerous, they will form the last cluster
of the classification (WEA-6). Form now on, the cluster WEA-6 is neglected and only the realistic profiles contained in the other clusters are considered."

#Li 283: plum[e]
>>Also corrected everywhere else.

#Fig 3a, PC1 vs PC2. There are outliers, all WEA-6 and a few WEA-1. Is it an idea to use the scatter plot to exclude such profiles, without claiming one (the 6th) group separates the suspicious profiles?
>>The scatter plot helps to understand how the clustering method gathers neighbour profiles/group of profiles. The outliers of WEA-6 are easily identified and are numerous enough to form a cluster. At the opposite, the outliners of WEA-1 are not numerous enough to form a cluster on their own.

#Fig 3 caption. Isobath contours are gray (not black)
>>Done

#Li 278-309: this part can be reduced substantially. Also some comments on the stratification made earlier in #li 177-185 can be omitted (from li 1777-185), since they are repeated here.
>>Previous mention of NBC removed and section shortened.

#Li 317: highlight that
>>Done

#Fig 4. a) again there're some outliers (in BB-5). It could be an idea to exclude from analysis? Caption: delete the entire text and use: "Same as Figure 3 but for the Bay of Biscay shelf."
>>The outliers in the Bay of Biscay profiles are way closer to the main groups than the one in the western equatorial Atlantic profiles. In addition, there is only 2 profiles concerned so, there are not numerous enough to form a cluster. Exclude them won't change the classification.

#3.4. Discussions- some of the descriptions from previous sections (on stratification variability and its causes) are repeated here. It can be better organized here (and please avoid repetitions)
>>Some sentences have been removed and reworked.

#Li 350-352: reduce to "V4.3 seasonal profiles, likely due to the spatial and vertical smoothing used in the climatology."
>>Done

#Li 411-423: delete the sentence "As explained earlier…"
>>Done

#Li 436-443: The entire paragraph can be reduced to " In the vertical, 80 sigma-layers (which follow the bathymetry) are used, with increased resolution in the upper ocean, set by a cosine function between 0 and pi/2" (please correct and improve my interpretation)
>>Rephrased: "On the vertical, 80 sigma-layers (which follows the bathymetry) are distributed using a cosine function between 0 and pi/2. This vertical distribution enable to better represent surface pycnocline and the associated ITs than a uniform distribution."

Dear Reviewer #1

We do thank you for your careful reading of our manuscript and relevant comments. A strong revision of the manuscript have been made to shorten it, clarify the objectives and highlight the main results. Additional results about the ITs energy flux have been produced following the advises of RC#3, supported by the editor. Additional validation of the simulation have been made following the advises of RC#2. The results from Tchilibou *et al.* (to be submitted) about the seasonal study of ITs in the NEMO simulation are provided in advance thanks to the authors and will be submitted to Ocean Science journal by September 2021. The appendix about the sensitivity of the clustering methods have been moved to supplementary material as the methods are not new and the description quite long.

Below your comments are listed (after #) and followed by our answers (after >>). The modifications are provided when they are not too long (between "..."). Changes made in the manuscript are highlighted in red and blue in the revised version of the manuscript. We took the liberty to reorganize the document by gathering together the comments tackling the same issue, in order to make our answers clearer. Some of your comments are no longer relevant because the sentences have been removed, all these comments are gathered at the end of the document. We hope this will ease your reading.

#l. 56: 'since the tidal currents are not in geostrophic balance.'
#l. 130: Sentence 'Such stratification…' repeated twice (second time with italics)
>> Corrections included in the reworked introduction

#l. 77: 'Section 2 addresses the usage…' what do you mean by that?
>>Rephrased:"Section 2 details the clustering method and compares its results to the classical three-month mean. Section 3 details the modeling of the ITs based on the typical profiles obtained from the clusters."

#l. 138: '… is to consider either seasonal means (the mean of three months…), or…'
#l. 176: replace 'both' by 'the two'
#l. 235: '… that enables us to discuss…'
>> Corrections included in the reworked methodology of the clustering.

#l. 170: 'The potential density is calculated for all data…' Then, 'The potential density is …, which is equivalent to …'
#l. 172 and 173, replace 'convention' by 'definition' or something else more appropriate.
>>Rephased: "The potential density and bouyancy frequencies are calculated from TEOS-10 definitions…"

#l. 204: 'only two principal components are used…' : what does it mean: there are many depths…
then line 209: '… by the surface depths'?
Maybe further explanations are required on the PCA method used, as this is not the most standard one, when it would be an analysis of T(P) for all the individual profiles.
>> The PCA is only a method that enable to calculate the distance matrix, needed to perform the cluster analysis. Further explanation are given in the supplementary material about the clustering methods sensitivity: "The clustering methods can classified the different profiles in several clusters using a matrix of the distances from every profiles to the other ones. Performing a principal component analysis (PCA) with two principal components on the profiles enable us to transform a system of (NxD) (with N the number of profiles and D the number of depths) to a system of (Nx2). In such system, named the PCA manifold, the distance matrix is easily calculable (Fig. A1)."

#l. 214: 'directly affected by the value of the density r0'… This is a minor effect, not a 0-order one, and I would not mention it. There is virtually no information lost by applying a normalization, contrary to what is stated here.
>> The normalization by the mean of the profile affect the profiles with an offset like the one in WEA-6 and inhibit the clustering method to consider them as outliers. These sentences have been removed in the shortened version of the methods.

#l. 257: '… and were measured during the same period…'
>> Done

#l. 260: '… for cycles 150-152 and for cycles 166-180 (except cycle 176)
Note that these should not have passed the standard quality control, based on min-max applied on the CORA data set (but you use a fairly old version; note that even for recent versions, the individual 'validated/qualified' profiles are available, and not just the gridded product, contrary to what is stated at the beginning)
>> Done. Thank you for the details, a more recent version of CORA will be used for other analysis.

#l. 272: '… defined during a specific season, but rather during the whole year'
>> Done

#l. 304: '… the difference in pycnocline depth…'
>> Done

#l. 326: 'to build a surface stratification without…'
>> Done

#3. 4 'Discussion'
>> Done

#l. 335: 'The stratification in the …'
>> Done

#l. 337: '… are limited-extent processes, whereas…'
>> Rephrased: "The stratification variability due to the circulation is spatially bound, whereas the one due to radiative forcing affects the area homogeneously."

#l. 343-344-346-347: 'is compared with…'
>> Also corrected everywhere else.

#l. 348: '… are averaged in the same areas…'
>>Done

#l. 354: '… show…'
>>Done

#l. 360: '…, as well as the cluster classification do.'
>>Done

#l. 361: '… is not effective.'
>>Done

#l. 365: '… profiles have.'
>> Corrected: "profile has..."

#l. 445: maybe '… in order to fit the bottom velocity boundary condition'.
>> Rephrased: "Using a frequency domain calculation, there is no spin-up of the simulation that could lead to a stable value of the bottom friction."

#Figure 6 and Fig 8 captions, line 3: '… show the extrema of the modes.'
>>Done

#l. 462: 'We will first discuss the vertical structure of the modes for…'
>>Done

#l. 555: ‘…, while mode 2 is controlled…’
>>Done

#l. 559: ‘…, but have the right magnitude. The difference could originate from differences in…’
>>Done

#l. 585: ‘… seems to …’
>>Done

#4.3 ‘Discussion’
>> Renamed: “Validation and discussion”

#l. 604: ‘The Its simulations…’
>>Done

#l. 608: ‘… have been equally set in the two configurations.’
>>Done

#l. 615: I would not call the western equatorial Atlantic’ ‘the amazonian one’
>> Yes, it was a missing correction from the previous reviews.

#l. 638: ‘This almost perfectly fits the wavelength of HRET and NEMO.’
>>Done

#l. 654: ‘… is an improvement compared with the mean…’
>>Done

#l. 19: sentence should not start by Whereas…
#l. 26: ‘… enhance vertical mixing in the ocean.’
#l. 31: ‘Their method…’
#l. 36: replace ‘as well as …’ by ‘as well as ocean currents’
following lines: ‘for a realistic approach’ ‘for an idealized approach’
#l. 64: ‘The SWOT measurements will face two issues: …’
#l. 142: ‘To maintain the realism of the typical profiles with only a few of them…’
to be replaced by ‘To extract a limited set of profiles that are representative of the whole set of profiles, we will use clustering methods.’
#l. 169: ‘… is also built…’
#l. 193: senetence ‘The measurements…’ unclear. What is meant by ‘unstable water masses’?
#l. 200: remove ‘Now that the profiles are properly selected and interpolated,’
#l. 206 ‘Both axes…’
#l. 353: ‘… the fall profiles show… than the spring ones’
#l. 283: ‘In addition to the water masses…’
#l. 403: ‘The concerns affect the…’ I am not sure that I understand the sentence
#l. 410: ‘As earlier explained, …’
#l. 437: ‘… also needs to be investigated.’
>> Removed sentences

Dear Reviewer #2

We do thank you for your few but relevant comments. A strong revision of the manuscript has been made to shorten it, clarify the objectives and highlight the main results. Additional results about the ITs energy flux have been produced following the advises of RC#3, supported by the editor. Additional validation of the simulation has been made following your advises. The results from Tchilibou *et al.* (to be submitted) about the seasonal study of ITs in the NEMO simulation are provided in advance thanks to the authors and will be submitted to Ocean Science journal by September 2021. The appendix about the sensitivity of the clustering methods have been moved to supplementary material as the methods are not new and the description quite long. We hope this new version will meet your expectations.

Below your comments are listed (after #) and followed by our answers (after >>). The modifications are provided when they are not too long (between "..."). Changes made in the manuscript are highlighted in red and blue in the revised version of the manuscript. We hope this will ease the your reading.

#1. In response to reviews, the authors state: "The quantification of the ITs non-stationarity requires a dedicated study that is in preparation..." and yet in this manuscript "The present study aims at contributing to the understanding of the ITs' non-stationarity through the investigation of the ITs variability." These two statements are at odds. I suggest the authors state that this manuscript concerns seasonal variability in IT and do not discuss non-stationarity.
>> They are not. The non-stationarity of the ITs in the altimetry is caused by temporal stratification variability as well as the interaction between the internal waves and the oceanic circulation. This study only focuses on the part of the ITs non-stationarity due to the temporal variability of the stratification. The sentences have been reworked.

#2. "The wavelengths in HRET and NEMO are coherent with both modes 1 and 2 wavelengths calculated from the clusters." In the case that you are concerned about wavelengths, there is no reason to run TUGO. Wavelengths are simply arrived at from solutions to a linear eigenvalue problem. Without seasonally-varying amplitude comparisons in HRET and NEMO to the TUGO solutions, and demonstrating that the TUGO solutions are sensible, I do not see the point of running the TUGO simulations. Particularly with a model implementation that has not been peer-reviewed.
>> This comparison is impossible to do with HRET or any other ITs atlases built from altimetry observation as there is no seasonal processing of the altimetry that have been developed so far. In NEMO simulation, the ITs properties have been investigated for two seasons by Tchilibou et al. (to be submitted). We used the elevation amplitude and the integrated energy flux of the ITs of these two seasons to compare them with corresponding T-UGOm simulations (Tab. 3).

Dear Reviewer #3

We do thank you for your careful reading of our manuscript and relevant comments within such short notice. A strong revision of the manuscript have been made to shorten it, clarify the objectives and highlight the main results. Additional results about the ITs energy flux have been produced following your advises, supported by the editor. Additional validation of the simulation have been made following the advises of RC#2. The results from Tchilibou *et al.* (to be submitted) about the seasonal study of ITs in the NEMO simulation are provided in advance thanks to the authors and will be submitted to Ocean Science journal by September 2021. The appendix about the sensitivity of the clustering methods have been moved to supplementary material as the methods are not new and the description quite long.

Below your comments are listed (after #) and followed by our answers (after >>). The modifications are provided when they are not too long (between "..."). Changes made in the manuscript are highlighted in red and blue in the revised version of the manuscript. We took the liberty to reorganize the document by gathering together the comments tackling the same issue, in order to make our answers clearer. Some of your comments are no longer reverent because the sentences have been removed, all these comments are gathered at the end of the document. We hope this will ease the your reading.

#What is the difference in (horizontal) IT energy flux between the different cases? I assume that is an easy think to compute from the model and it would be interesting to see a figure of that. After all, the ITs themselves are not what matters for the Earth system, it is the energy they redistribute that matters. Adding this would add value to the study and make the title more valid.
>> A subplot is added on the figure 6 and 8 (previously fig.7 and 9) to show the energy flux of each simulation for the five first modes. To meet more realistic energy fluxes, the barotropic tidal current have been reduced to 5 cm/s. This modification does not change the conclusions highlighted in the study and only affects the amplitude of the values. The discussion about the comparison between the simulated amplitudes and the ITs altimetry atlases have been updated consequently.
Energy flux value have also been used to validate the T-UGOm simulation comparing them with NEMO simulation (data from Tchilibou et al., to be submitted).

#Typos need to be fixed.
L39: e.i. should be e.g. (for example).
>>Done

#L130: freshwater is another controller of stratification, completely dominant in some locations and should be mentioned. This is especially key for western Atlantic region being discussed (cf. L364 where the amazon plume suddenly appears).
>> Correction included in the reworked introduction.

#L508: “was expected weaker”; rewrite or clarify. Do you mean as expected?
>> Competed: “whereas WEA-88 m was expected weaker to be sorted between WEA-70 m and WEA-108 m like for mode 1 and 2.”

#Figure 7 and its discussion in the text: clarify that you mean horizontal wavelength.
>> Done