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Reply on RC1

Simon Barbot et al.

Author 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-AC1>, 2021

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 retroflexion, 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 of 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 proceeding 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 of 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 $5.0 \cdot 10^{-7}$ 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 than 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 retroflexion 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 retroflexion (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 ($2,3-2,5 \cdot 10^{-2} \text{s}^{-1}$). 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.