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Comment on os-2021-19

Anonymous Referee #1

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-RC1>, 2021

This paper investigates how M2 internal tides are influenced by changes in background stratification in two important and contrasted settings with large internal tide variability: the western equatorial Atlantic close to the shelf off the Amazon estuary and the bay of Biscay. The approach is to use the same shelf and slope shape with prescribed barotropic forcing, and to investigate how the TUGO model solution (without currents and spatially homogeneous stratification) varies as a function of realistic changes in stratification. These changes are prescribed, based on a cluster analysis of deep ocean profiles (CORA V4.3). This analysis extracts the seasonal variability in the Bay of Biscay, and a varying pycnocline depth in the western equatorial Atlantic, but interestingly no major change in the stratification close to the surface. The clustering approach is interesting, as it provides a robust analysis of the major characteristics of the density profiles in each region, but the result is somewhat obvious: the seasonal cycle in the Bay of Biscay (this could have been set and tested a priori without the analysis), and the large influence of the pycnocline displacements in the western equatorial Atlantic. In that last case, 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 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.

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).

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 to its east. I would thus strongly recommend a title change. Maybe

'... : case studies for the western equatorial Atlantic and the Bay of Biscay'

I will now list some minor comments (quite a few are just editorial, and for that I strongly recommend that the paper be read by an English speaker before being resubmitted).

l. 77: 'in the rest of this section' should be replaced by 'Further discussion of the ITCZ and stratification are found in 1.2 and 1.3'. I think that 1.1 is what is usually called an introduction. 1.2 and 1.3 provide background information (could be a part 2)

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

l. 80: 'horizontal baroclinic pressure gradient' is not an 'instability'. I would write 'This will propagate as ...' The previous sentence also needs to be rewritten.

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).

l. 89: N is not a 'proxy'. Change the sentence: 'Stratification is commonly characterized by N...'

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

l. 104: 'is defined'

l. 108: 'Thus, stratification and depth both...'

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

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...'). For the surface layer, instead of circulation, I would indicate 'water masses'. L. 116: 'The seasonality of this process is weaker in the tropics than at mid or high latitudes'

l. 143: 'Section 2...'

l. 147: 'The results for 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).

l. 157-158: '... from 60°W to 35°W'... 'from 10°W to 0°W)

Figure 2: only show data that are not on the shelves (?) What is the unit of the number of profiles (panel d) and what is the period considered for the profiles.

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

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

l. 193: '... rather than acting as a restoring force'.

l. 197: '... need to have a sufficient vertical resolution.'

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

l. 231: 'but larger for the Spectral method'

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).

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?)

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.

l. 275: 'There is a very noisy seasonality...'

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.

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).

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.

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

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

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

l. 397: 'All simulations...'

l. 401: 'As explained earlier, ... to adjust as in a time stepping calculation.'

l. 436: '... as follows'

l. 448: '... Is first investigated.'

l. 450: '... is influenced by...'

l. 452: '... the further the mode ...'

l. 455: '... while for a deeper pycnocline...'

l. 457: 'The only exception to this tendency happens for the horizontal structure of mode 3 at the surface... , whereas AS-85m'

l. 462: '... with a dominance of...'

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).

l. 463: 'The wavelength for...'. l. 467: '... but creates...'

l. 469: 'Figure 7 presents...'

l. 479: '... for mode 2...'

l. 48: '... the shift associated with mode 2...'

- l. 480: '... if the stratification leads to an...'
- l. 481: 'This rough calculation helps to understand why small changes in wavelength...'
- l. 498: '... elevation is on the order of 6cm.'
- l. 501: 'During the 2015 NEMO model simulation... is on the order of 5cm.'
- l. 503: '... but have the right order of magnitude'
- l. 508: ' of the barotropic tidal energy...'
- l. 518: 'Because of the variability in N, ...'
- l. 522: '... upon the constant stratification maximum at 800m...'
- l. 523: '... Presents the same tendency..., the further the mode...'
- l. 529: 'the trend' should be replaced (in most cases in this section) by 'the change..'
- l. 533: 'This could be due to the stratification...'
- l. 539: 'These results...'
- l. 552: '... and might not include the weaker Its amplitudes...'
- l. 553: '... gives...'
- l. 559: '... are not influenced in the same way...'
- l. 566: 'The amplitudes of the modes'...'
- l. 579: '... the closest extremum...'
- 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 Amzon' ? What is meant there.
- l. 610 'Because the NEMO simulation only is for the area...'
- l. 632: 'be enhanced...'
- l. 634: 'As shown here, this approach is very useful...'
- L. 649: '... is maintained year-round... than the ones at mid-latitudes.'
- l. 662: '... should be preferably used...'
- l. 675: '... the different approaches used, in order to ...'
- l. 678: '... upon request to the authors.'