ReEditor:

>>>We thank the editor for the time to comment our manuscript. Our replies are behind >>>

Public justification (visible to the public if the article is accepted and published):
Your paper was returned to one of the reviewers. The reviewer would like to see further strengthening and provides helpful suggestions that will make the paper more impactful and easier to read. I would like you to address the reviewer's concerns. Since the two reviewers differed in their view of the merit of the paper, I have reviewed it carefully myself. I share the reviewer's concerns that the key messages of the paper are not always clear, although I do find the work interesting and worthy of publication. I encourage you to critically revise the paper and focus it on the messages that you want the reader to take away. Maybe you can remove some elements? For example, if you never show or use the data from the sites shown in Figure 1, why show them? Selecting the elements of theory or observations that you will use later in the paper will help the reader to see where the paper is going, otherwise some sections feel like a review of internal wave processes.
>>>Thank you for the appraisal. We have carefully considered all comments, reread the manuscript and modified where necessary. The results from data from sites in Fig. 1 are used in the paper, as these were crucial for understanding where internal waves break generating most vigorous turbulence. Data from these sites are mentioned in values of mean turbulence dissipation rates and stratification in (old) Section 1.2. This is better indicated now. More selections have been indicated.

Here also are some more minor corrections that I spotted going through.

L50 most deep sea turbulence
>>>Modified as suggested, thank you.
L163 It is confusing that section 1.1 starts here, so long after section 1. I would suggest making it easier for your readers to follow the structure by calling this section 2 with a subheading 2.1 here.
>>>Alright, in which case we renamed other sub(sub)sections as well.
L197 kilometre for consistency with choice of European English elsewhere
>>>Yes.
L212 I was puzzled by the comment “It is noted that atmosphere dynamics is not driven by the ocean, except indirectly by modification of moisture content)”. In some regions of the world, atmospheric dynamics are indeed driven by the ocean through its sea surface temperature (I’m thinking of the MJO or ENSO]. Maybe rephrase?
>>>This seems like a chicken-egg discussion, considering the thought that (atmospheric) trade-winds lead ocean’s SST variations and thereby ENSO, for example. Anyway, the remark is now removed as it is not really relevant for the manuscript.
L267 because of the rather precise... Or if you prefer, owing to the rather precise...
>>>We prefer ‘owing to’.
L291 dissipation rate amounts to 4....
>>>Corrected.
L298-299 could you give an indication where Mount Josephine is? Just the name of the ocean or sea will do. [This should then be removed from l314]
>>>Modified as suggested.
L338 again we have section 1.2.1 appearing long after section 1.2. Please re-organise heading numbering to make it easy for readers.
>>>Re-organised, 1.2.1→2.3.
L352 I would add period after buoyancy for clarity. At least a buoyancy period, and better an inertial period, ...
>>>Yes, better.
L389 published by Costello
>>>Modified, thank you.
L419 Faroe-Shetland
>>>Modified as suggested.
Figure 2. This figure must be revised not using colormap jet, and to be colour-blind friendly. See further information at https://www.ocean-science.net/submission.html#figurestables
>>>Regarding figure 2 being yes or no colour-blind friendly we would like to remark the following. Colour-blindness comes in multiple combinations of colours and various degrees of severeness. The second author of this manuscript is colour-blind himself. He not only has a weak vision for the more common red and green, but for blue as well. In his experience colour-blindness simulators, such as

the OS-submission page is referring to, often give a too extreme impression of how colour-blind people are supposed to see the environment. The first and second author together have designed figure 2 in such a way that the second author is able to distinguish enough details to understand the message we want to convey by this figure. To be able to convey the message of a figure it is not necessary to use colours that colour-blind people and people without this impairedness see in exactly the same manner (in fact this is even impossible given the many variations in colour-blindness). As long as the reader is able to see enough details to understand the message, the figure performs its task. Making alterations to the figure could make the figure clearer for one group of colour-blind people, but at the same time will also make it more difficult to interpret for another group.
If the editor insists, we offer alternative Fig. 2 below.

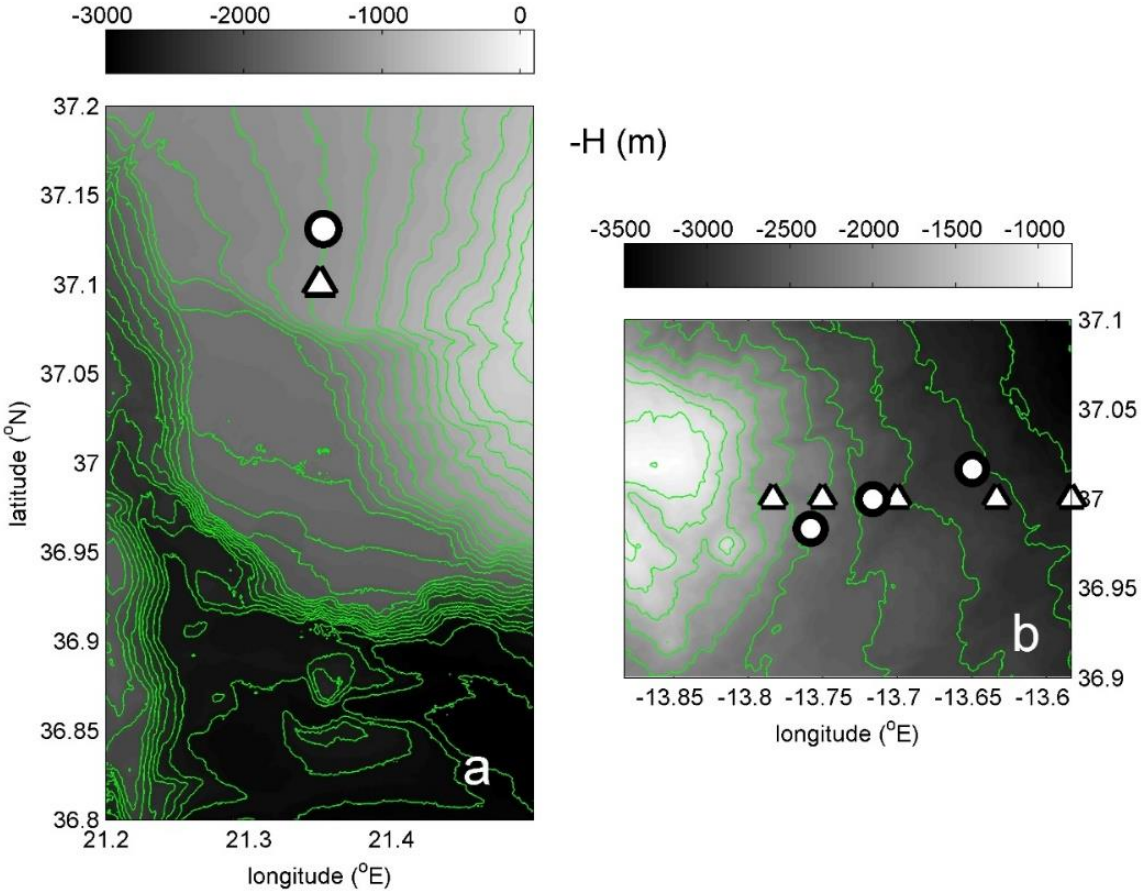

Figure 3. It would be helpful to state explicitly whether this is logarithm base e or base 10.
>>>Done as suggested, lg is the mathematical notation for logarithm base 10.
L566 How and where do internal waves shape the seafloor?
>>>Ok, thank you.
L574 modelling for consistency with choice of European English elsewhere
>>>Modified, thank you.
L601 compared with inertial waves (or than inertial waves)
>>>Used 'with' now.
L629 As this is a new paragraph (signifying a change of topic), (re)state explicitly what "it" is, and where "here" is. Refer to a figure in support of this sentence.
>>>'Here' is removed and 'it' is made more explicit.
L703 magenta lines
>>>OK, modified as suggested.
L833 You didn't give a reference in support of this sentence, so why "recent"? If you are referring to the data sets described in the paper, I would omit recent.
>>>References inserted now.

ReReRef2:

>>>We thank the reviewer for the time to comment our manuscript. Our replies are behind >>>

Second review of "A global summary of seafloor topography influenced by internal-wave induced turbulent water mixing" by Hans van Haren and Henk de Haas

Though some aspects are improved in this new revision, the reasoning behind some of the conclusions presented in this paper remain somewhat opaque. Broadly, the authors discuss two aspects of internal wave topography interactions. First, they note a roll off in variance at scales shorter than internal wave excursion lengths at two sites, linking the roll off to seafloor erosion. Second, they posit that internal wave breaking over slightly super-critical slopes over 5% of the global ocean is sufficient to maintain the current global stratification, then use bathymetric products combined with two full depth stratification profiles to demonstrate that this could be the case. The first point is clearly supported by the data presented in the manuscript, however I find the arguments for the latter point unclear, with several figures and sections that do not obviously address this point.
>>>Thank you for the thoughtful summary. The second point is also supported by our results from some 25 sites of moored and shipborne observations. We have carefully reread the manuscript and clarified where necessary.

The authors focus on two sites, one in the Mediterranean Sea, the other in the North East Atlantic. Most of my criticism is of Section 3.3 and associated Figure 6, though there are several other parts of the paper (detailed below) where I struggle to understand the point being made, and how it supports the conclusions presented. For example:
- l618: Though most of the volume and surface area of the ocean is between 3000-6000m depth, there will be biases depending on slope, with an expectation given the geometry of the ocean basins that steeper slopes will be found shallower than 3000m. It is not sufficient to assume that "most of these percentages" will be between 3000-6000m.
>>>l658. Thank you for pointing out. According to Costello et al. (2010) the mean and std of slopes are not significantly varying between -7500 < z < 1500 m, but have significantly lower and higher values at z > -1500 m and z < -7500 m, respectively.

- Am I correct in saying that the point being made in the paragraph starting l677 could be summarized by saying that 5% of topographic slopes are supercritical at the Mt Jos and East Med regions? A clear statement like this would be helpful here, though I am skeptical of this result as the authors' do not seem to have taken into account the depth distribution of criticality. i.e. at the East Med site, the argument seems to be that 5% of slopes are steeper than 9.6', which is the critical slope evaluated using a stratification near 1100m. However, some of those steeper slopes may be at a different depth than 1100 m, and thus have a different N and different critical slope. To make this argument about 5% of all slopes being super-critical (is this the point?) then depth dependence must be considered. Why not simply calculate the areas in Fig 2 that are super-critical and see if they are 5% of the region, then repeat the calculation on global scales?
>>>Yes indeed, such a clear statement is now inserted. We compared statistics of seafloor slopes with (theoretical) M2 internal wave characteristics and with internal wave slopes from local N. It is impossible to determine for every single seafloor data point the precise internal wave slope to infer its super-criticality, as we lack CTD profiles every 1' and its time mean from at least a day-long yoyo-series.

- Fig 6: What is the purpose of plotting the M2 characteristic slopes vs. N/Nmax? Could the authors at least mark N/Nmax on the right side of the figure (e.g. using plotyy in Matlab, or equivalent), and explain how they interpret this with respect to the ratio slope-occurrences?
>>>Good point, and the purpose is made more explicit now. Nmax scaling was introduced to have values like the ratio of seafloor slopes, for theoretical N. Now we give the scale of N on right side. We compare its distribution with that of slope-occurrences, as indicated.

In general, I do not find that this section supports the arguments presented about the inter-dependence of stratification and topographic slope.
>>>We have improved explanations now.

Some other points are noted below:

- 158: "wide variety of sites"? This paper only considers two sites.
>>>The mean main results from some 25 sites are used in general description, besides those from two studied in detail.

- 490: Include the name of the parameter in the definition, i.e. "ratio slope-occurrence".
>>>Yes better, we replaced 'percentage' by that parameter name.

- 500: Help the reader here and explain the significance of 37'N
>>>There is not a specific reason for 37°N, other than that around this mid-latitude most of our observations were made (cf., Fig. 1) and a comparison between sites with the tide-limited Mediterranean was possible. It is halfway Mediterranean latitudes (between 30 and 45°N) and we wanted to have a tide-dominated open-ocean site at the same latitude, for proper comparison of near-inertial motions. This is better explained now, in (old) Section 2.2 around Fig. 2.

- 527: above -> on
>>>No, we mean the lower 400 m above seafloor of CTD-profiles.

- Section 2.1: There seems to be a missed opportunity here to use this large dataset to support the arguments made in this paper. Instead, the authors discuss only two sites (yet describe all of them in general).
>>>That is not quite true, as main results from other moorings are used as well. This is better referenced now.

- Section 3.1: How does the layering described in this section and shown in Fig 3 relate to the arguments made in the paper about near-bottom turbulence and erosion? I do not understand what point is being made with Fig 3. If it is simply to demonstrate variability in near-bottom N, a more comprehensive approach would plot N vs height above bottom using all of the sites presented in Fig 1.
>>>Several points are made with Fig. 3. The variability from the (rare) yoyo-CTD time series over one inertial period in Fig. 3a, compared with those from different seafloor and depths in Fig. 3b. These are specifically given for comparison between the two detail-study sites. Most of the 25 sites have limited (read one or two) CTD profiles and would not be informative in a single figure.

- The "transition wavelength" is still not explicitly defined.
>>>It follows from spectral observations: indicate where the spectral slopes change to a loss of seafloor variance. This is better indicated now.

- Section 3.3:
>>>Improved now.

- Fig 7: caption line 687, green -> blue
>>>Thank you pointing out, although it is green → black.