>>>Thank you for your well-balanced report. My replies are behind>>>

Editor report

Dear Dr. van Haren,

One reviewer rates your revised manuscript as good in terms of scientific significance and quality, but fair in presentation quality. The other reviewer rates it as fair in all three aspects and recommends rejection. Our review criteria require a manuscript to be rated excellent or good in all principal criteria and specific aspects.

I find the spectral slope analysis and insights from deep observations interesting and worthy of publication in Ocean Science, subject to some clarifications (see below). However, the links to larger-scale circulation are either far-fetched or not sufficiently supported in the current analysis. I agree that AMOC predictions using models may lack important feedback related to deep ocean mixing and thus be inaccurate. However, this is not the main question you address in detail, and it could better serve as a discussion and speculation point in your manuscript.

I invite you to prepare a revised technical note, focusing on spectral slopes in the deep, weakly-stratified ocean and the importance of coupling between sub-mesoscale motions, low-frequency internal waves, and convection turbulence. The links to AMOC and responses to inaccurate claims can be included as part of the discussion. A suggested title could be: “Spectral slopes in deep, weakly-stratified ocean and coupling between sub-mesoscale motions and small-scale mechanisms”
>>>Thank you for the appreciation. I have carefully reread the manuscript and modified accordingly, using the suggestions including the proposed title.

Additional major comments:
The short summary must be entirely revised to remove the previous focus on AMOC collapse.
>>>This has been revised now removing AMOC collapse predictions (and the lack thereof).

The abstract should be revised to emphasize the spectral observations and findings, with reduced mention of AMOC links.
>>>The abstract has been thoroughly revised as suggested.

The question posed in lines 125-127 is not satisfactorily addressed or answered in the present paper and should not be raised as the main question for this technical note. The closing part of the introduction must be rewritten.
>>>Both are rewritten, emphasizing the little studied deep-sea convection turbulence and sub-mesoscale part of spectra.

Fig 1 and the discussion of data. Why do we expect the buoyancy / convective turbulence 1150 m above the seafloor?
>>> Following observations presented in van Haren and Millot (OA2003): in winter, stratification becomes near near-homogeneous down to about 300-500 m, as observed in limited CTD-profiling, while occasionally the few moored current meter temperature records showed significant inversions, with values highest at

1100 demonstrating strong instability. Also in winter, sub-mesoscale variability is larger due to intensified boundary flows. Current speeds are up to 3 times larger than in summer and polarization of motions is more irregular and rectilinear than in summer, suggesting gyroscopic waves in the internal wave band.

Fig 2 and the discussion of data: This time series is from thermistors at the seafloor (assumed 1 cm above seabed) because the thermistor string fell off the mooring. 1) why is this a good record to study turbulence or related spectral slope? It is far too close to a boundary. Turbulent eddies will be squeezed and affected by the presence of the seafloor. Is near-inertial subrange expected in such conditions? Is convection turbulence scaling valid in such conditions?
>>> The data from T-sensor at 0.01 m is presented because of high precision/low noise and because they show convection-turbulence over an extended frequency range partially due to geothermal heating, indeed also because shear is expected to be limited. For reference, I have now added spectrum from data of an older-type, noisier T-sensor that was attached to a drag-parachute stuck at 140 m ab.

Li 340- You claim that the observations at the seafloor suggest direct coupling between sub-mesoscale motions, IGW, and convection turbulence. I am not convinced that this coupling can be observed 1 cm above the seafloor. This must be clarified and discussed in more detail. At a minimum, please include a caveat.
>>>See above, now a record is included from 140 m ab.

Li 352-355: I don't understand why you bring this up. It seems to be an attempt to link to the broader question of AMOC and the representativeness of the surface data. Even if your 18-day seafloor temperature dataset does not show dense-water formation and convection at this time and location, many other datasets elsewhere could. You certainly do not claim that deep convection does not exist in the world's oceans. This closing statement is misleading and must be reworded to reflect this particular dataset and location.
>>>Indeed, I do not mean to link to the AMOC. Reworded now.

Minor comments and technical edits:
Li 65: please avoid use of brackets like this: "(sub-)mesoscale gyre (eddy) transport", and reword as needed. Also e.g. line 209: "a(n active) scalar>
>>>Manuscript reread and modified where necessary.

Li 88-91: the missing mixing and the link to boundaries is outdated (e.g. https://eur01.safelinks.protection.outlook.com/?url=https%3A%2F%2Fdoi.org%2F10.1175%2FJPO-D-16-0082.1&data=05%7C02%7C%7C10ae098f79d04a44719b08dd1d09bb75%7C9a1651bf58af435b86a83e9334b4b732%7C0%7C0%7C638698649849783121%7CUnknown%7CTWFpbGZsb3d8eyJFbXB0eU1hcGkiOnRydWUsIlYiOiIwLjAuMDAwMCIsIlAiOiJXaW4zMiIsIkFOIjoiTWFpbCIsIldUIjoyfQ%3D%3D%7C0%7C%7C%7C&sdata=Ra%2BwCbTjUperH80BpITmnFQ2%2FbgB6fh3oArDMF2FLk0%3D&reserved=0 and the following up literature). I see you refer to Ferrari et al 2016 in line 124. This can be integrated here too.
>>>I somewhat disagree with this remark, as internal wave breaking is still needed and explaining the mixing of heat downward, to maintain the ocean stratification. What Ferrari et al 2016 (was also cited in old l.76) and McDougall & Ferrari 2017 (reference now included in old l.77) showed was a need for upwelling in a, probably thin, layer above the seafloor, given the observationally

demonstrated increase of turbulence with depth towards steeply sloping seafloors. The requirement for the upwelling in this thin boundary layer, if existent, reads, following computations by McDougall&Ferrari (2017), in terms of eddy diffusivity: Kz =1.5x10^-3 m^2s^-1 for a transport of 100 Sv. This Kz-value has been observed using high-resolution moored instrumentation, e.g., van Haren&Gostiaux JMR2012 found double this value due to internal tide breaking over slope of Great Meteor Seamount. This is better indicated now, near the beginning of Section 2.

Li 172: please consider rewording sub-sub-mesoscale, which I do not recognize as a common term
>>>Reworded to 'smaller than sub-mesoscale'.

li 208: what are you referring to with "buoyancy range"? Please clarify
>>>Apologies, it should have been 'buoyancy subrange' (of turbulence).

li 230: please show how the omega_min value is obtained
>>>OK, definition given now as eq. (1), with that of omega_max.

li 250: typo in "flow field"
>>>Thanks.

li 251: I see that the spectrum is from 1150 m above seafloor, but could the spectral bump be attributed to slope critical for near-f frequencies? Or is such physics not allowed for such very weak stratification? Is the stratification near the current meter same in winter and summer periods? Why do we not have a hint of the spectral bump in the winter spectrum? It is difficult to claim the lack of bump to higher energy level in winter.
>>>Data from the central part of the Ligurian Sea show similar results as in Fig. 1, so slope criticality is unlikely, also because inertial peaks are generally common throughout the ocean. Local stratification is more or less the same between seasons, see old l.198, but upper-sea stratification not. This could imply that atmospheric-generated near-inertial waves have smaller amplitudes in winter than in summer. Besides, the sub-mesoscale activity is much larger in winter, with, as suggested, more spin-off to turbulence range motions. These definitely affect the polarization which is not circular at f in winter (van Haren&Millot, 2003).

Li 284: reword "which bounds are close to"
>>>Reworded now.

Li 296: so omega_min is a hypothetical limit? (see comment on li 230)
>>>No, it depends on N, latitude and wave direction, see definition given now as eq. (1) near old l.230.

Li 297: one would require N = 0.21f-please clarify required for what
>>>For omega_min. Now, an explanatory sentence is inserted around the first mentioning of omega_min, omega_max (old l.240): 'The omega_min and omega_max are functions of N, latitude and wave propagation direction.'

Li 315-316: please reword: " no (cooler) dense-water convection was formed" (e.g., cold, dense water production through convection was not observed). Please avoid excessive use of brackets, which is generally confusing for a

reader.
>>>Reworded as suggested, thank you.

Li 323 and also fig 2 caption: this is one spectrum stitched together using two spectra with different smoothing, DOF before and after 5 cpd. Please clarify / reword.
>>>Reworded as suggested.

Li 379-381 / li 425-426 - please avoid excessive use of brackets that can confuse the reader.
>>>Brackets removed from these sentences, thank you.

Li 402 - "and which speed is of the same order" doesn't read well.
>>>'speed' removed now.

Li 411-412: these citations must be in text.
>>>'such as in' removed now.

Li 434: grammar doesn't look right. Perhaps "...mixing induced by, e.g....coupling, cannot be"
>>>OK, 'is' removed now.

Fig 1. Are the winter and summer spectra offset from each other? (It could be clarifying to mention that they are not offset, if this is the case.) Reading the text, I understand that winter is more energetic (hence not offset), but I doubt the noise level would be different between the records.
>>>The spectra are not deliberately offset from each other, as has been indicated now. I understand your doubt, but the 3600-s sampled data apparently contain not only instrumental noise but also other physics signals near the Nyquist frequency.

Fig 2. Please clarify that this is one spectrum stitched together using two spectra with different smoothing. Please bring the actual spectrum above the other lines. Showing the spectrum in color can help some readers (can also consider in other figures). You can consider showing the 95% confidence intervals for the two different smoothing.
>>>Improved as suggested. The error bar for the most important heavily smoothed spectrum was shown in upper right corner, which is moved now for better visibility.

Fig 3- please bring the spectrum above other lines (and use color if you don't object). Placement of the slope numbers should be improved. I think _I can see a small bar showing the confidence interval. Please define it in the caption.
>>>OK, improved as suggested.

Referee #1
>>>I thank the reviewer for the time to comment my manuscript. My replies are behind >>>

This new version of the ms. has a much more balanced invocation of the AMOC, and it can be sensibly published. One might indeed question the reliance on

apparent power laws, but the approach is defensible and possibly useful. There are a few small remaining issues, mainly of English usage:
>>>Thank you for the appreciation.

Perhaps cite Chunchuzov et al.Tellus 2021 On eddy internal wave generation. (Or one of several other papers.)
>>>Cited now.

Line 71. Presumably "ineffective" should be "effective"
>>>Yes, of course, thank you.

Line 72+ It would be useful to cite the recent paper of Liu et al. Wind-steered pathway of Atlantic MOC. NatureGeoscience 2024 .. on wind control of AMOC
>>>Cited now.

104. "secondly" should be "secondary"
>>>Ok, modified.

132 end of the line, is missing "an order"
>>> Yes, thank you.

157 "associate" should be "associated"
>>> Like the sentences above and below, the sentence is in present tense, as they indicate more general than particular findings.

215 The sentence says the lab experiments are inconclusive. But then goes on to quote their results and I'm left confused.
>>>The following sentence now includes 'On the one hand....on the other hand'.

250 "filed" should be "field"
>>>Yes, thank you.

268 "if existent" should be "if it exists"
>>> Ok, modified.

276+ I don't understand this sentence "without reflexion", but comes back?
>>>As it was meant that inertial waves can propagate from well stratified to near-homogeneous and to well stratified layer, now modified: 'back' → 'vice versa' . Perhaps 'without attenuation' is better wording than 'without reflection', as modified now.

297 "not" should be "non-"
>>> OK.

331 "steeper" should be "more steeply"
>>> Yes, thank you.

346 "due to difference" should be "due to the difference"
>>> Yes.

382 Internal waves exist in the range f<freq <N or N<freq<f . But what is the meaning of internal waves when N=f? (the frequency vanishes from the controlling parameter).

>>>Good point. When N = O(f), including N = f, the traditional approximation is no longer valid and one has to consider the full equations of motion, including the horizontal Coriolis parameter. This results in an inertio-gravity wave band, not only internal gravity waves but also gyroscopic waves (e.g., LeBlond&Mysak, 1978). This band has wider ranges omega_min =<f omega_max>=2Omega, as provided now (around old l.240), and internal waves at N=f can propagate freely, with one component in horizontal direction only (e.g., Gerkema et al., 2008).

422 "required to evidence" should be something like "required to demonstrate"
>>>OK, modified as suggested.

430 I don't think anyone has ever suggested that the AMOC was the main driver of the ocean circulation.
>>>'rather than the AMOC' removed now.

Referee #3

>>>I thank the reviewer for the time to comment my manuscript. My replies are behind >>>

The revised version of the paper is clearer and easier to understand. However, it combines two very different components that are nearly incompatible:
>>>Thank you for appreciation of improvements.

The central part of the paper is about small-scale mixing dynamics. It uses a few deep moored temperature and velocity records from the northwest Mediterranean to argue that the dynamics is a form of convectively driven stratified turbulence, i.e. B-O dynamics. The large increase in spectral level and lack of an inertial peak in winter compared to summer is particularly interesting. Since the mooring is on the edge of the deep convection region, one might expect interesting and unique dynamics here. However, the argument is weak and incomplete:
- What is B-O dynamics? I had never heard of it, so I expect many other readers will also be mystified. It seems to be controversial in the fluids community (Liot doi:10.1017/jfm.2016.190655).
>>>I would rather say BO-scaling instead of BO-dynamics, as it, like KO-scaling, proposes a model for energy transfer of basically turbulent and intermittent motions. While the KO-scaling is most known, describing the inertial subrange for a passive scalar, the BO-scaling describes deviations from KO-scaling, notably the 'buoyancy subrange' for an active scalar. Its description exists since 1959. I would not say it is controversial. As outlined in the manuscript various model conditions yield various scalings and, as of to date, the energy cascade governing BO-scaling has not been well-defined, in contrast with that of KO-scaling. No problem to add a few extra citations on this, like Liot et al 2016 who describe KO-scaling for RTi, but also indicate that it may have to do with their Lagrangian data, as proper transfer brings their data closer to BO-scaling. Others, like Boffetta et al. PRE2009 but especially also Poujade PRL2006 and Celani et al. PRL2006 demonstrate clear BO-scaling for RTi modelling. I agree that thus far BO-scaling has seldom been observed in the ocean, presumably because it is difficult to directly observe convection, and also because standard oceanographic instrumentation and sampling is not precise enough to properly resolve turbulence. This is better indicated now.

- What is the density profile of the moored record? What does velocity show? Was there deep convection at that time? Did it extend to the location of the mooring?
>>>I wonder what the referee is implying with a density profile of a moored record? What data is referred to? Assuming the remark is on Fig. 1: Following observations presented in

van Haren&Millot (OA2003): in winter, stratification becomes near-homogeneous down to about 300-500 m, as observed in limited CTD-profiling, while occasionally moored current meter temperature records showed significant inversions, with values highest at 1100 demonstrating strong instability. Also in winter, sub-mesoscale variability is larger due to intensified boundary flows. Current speeds are up to 3 times larger than in summer and polarization of motions is more irregular and rectilinear than in summer, suggesting gyroscopic waves in the internal wave band.

- There are other oceanographic papers that should be cited (Huang https://doi.org/10.1017/jfm.2024.1030). The author has also argued that B-O scaling occurs in the ocean in other locations (Van Haren et al. 2024 https://doi.org/10.1016/j.dsr.2024.104277).

>>>Yes, it has also been observed in various other locations, including O(100) m above the seafloor in the deep Mediterranean (van Haren OD2023). The Huang et al. paper, quite recently published online on 25 November 2024, is from close to the seafloor in a shelf sea, where (shear) flows may be relatively large. I would not mind citing that paper, if it not contained strange errors in the spectral extent, with the 16-Hz sampling rate (they probably mean a sampling rate of 16 times per second) transferred to 2pi wrong rad s^-1. Also, their lowest frequency, when shifted by 2pi, would imply almost raw (unsmoothed) spectra, resulting in large error bars, which are not given. It seems they mixed up Hz, s^-1 and rad s^-1, unfortunately. Sampling rate in their Fig.4 is not 16 Hz as indicated in the caption, but once per 60 s. Citation is not always correct (e.g., Polzin et al. (2021) do not reveal BO-like scaling).

There may well be a need for an overview paper bringing all these ocean observations together with theoretical and modeling results, to argue the case for B-O scaling in the ocean under certain conditions. This paper is too incomplete to be publishable as such.

>>>I disagree, more oceanographic observations were not available at the time of writing, several extra modelling references are given now.

This small scale argument is concluded by arguing that this data “suggests a direct coupling between sub-mesoscale motions, IGW motions, comprising internal gravity and gyroscopic waves, and convection turbulence.” Without quantification, this is certainly true. None of these processes exist in isolation. All have energy cascades, which means that they modify and are modified by other things. It would be much shorter to just say this in a paragraph with a few references and omit the detailed discussion of B-O scaling.

>>>Initially I did so as a matter of fact, but elaboration was suggested. So, extended work has been done to demonstrate a possibility reflecting such interaction, and BO-scaling is an example, but indeed it is by no means the only one, and several other examples are indicated.

All of the above is used to argue that predictions of the future evolution of the AMOC, based on simple physics with a few parameters, are unlikely to be accurate. It appears to be a response to claims in two other papers. Ditlevsen and Ditlevsen (2024) claim “We estimate a collapse of the AMOC to occur around mid-century under the current scenario of future emissions” and Van Westen et al. claim “Reanalysis products indicate that the present-day AMOC is on route to tipping.” This paper makes a sensible response to such predictions, by saying that they may not include all of the relevant feedbacks and thus be inaccurate. This response could be strengthened by omitting the small-scale discussion of B-O scaling, and instead citing selective papers about the many processes that the above two articles omit.

>>>Thank you, much appreciated. See also response above. Such citations of the many relevant processes was given to a certain level, and somewhat extended now.

In summary, this article makes a weak argument that an obscure type of ocean physics

sometimes occurs and therefore models of the AMOC might be wrong. Harmless, but not of the highest quality. Copernicus describes itself as publishing “highly reputable peer-reviewed” articles. By that criteria the paper should be rejected.

>>>I disagree, as it is a, perhaps overlooked, example of how such interaction between small- and mesoscales can exist, proving the complexity of ocean dynamics.