Review1
>>>I thank the reviewer for the time to provide comments that helped improving my manuscript. My replies are behind>>>

Despite its title, this paper has no real connection with the AMOC--the use of that label appears as simply a wish to be noticed and is quite misleading.
>>>An impression of misleading and simply a wish to be noticed was not my intention as 'e.g.' was included before 'Atlantic Ocean meridional overturning circulation'. I hope by modifying the title to: 'A note on small-scale potential feed-back mechanisms of large-scale ocean circulations' such impression no longer exists.

The paper is really a recapitulation of the author's work on the northwest Mediterranean. Apart from both that area and the northern North Atlantic being regions where deep convection occurs, perhaps periodically, the physical state of the two regions differs radically. Amongst numerous other differences, the Atlantic is the site of western boundary currents, both surface and deep, of an intense wind-curl generated circulation, and a radically different bottom and sidewall topography. The NW Med. is, as the author has shown in the cited papers a region, in places, of extremely weak stratification (very small N/f), but if that regime exists anywhere in the N. Atlantic, the reader is never told.
>>>It is not only northwest Mediterranean work, but in general about turbulent mixing in the deep-sea. The paper brings new insights from re(newed)analysis of historic, 40-year old but also very recent, data. The regions have more in common than (periodic) deep convection. Compared with the North Atlantic, the NW Med. also is characterized by a vigorous boundary current, which becomes unstable and sheds off eddies of multiple sizes (Crepon et al, JGR1982; Albérola et al 1995; Millot,1999). Sidewalls are equally rugged and incised with deep canyons. Stronger stratification $N > 10f$ occurs on large (100-m) vertical scales in upper layers in the NW Med, and very weak stratification of $N \sim f$ occurs in the North Atlantic on 100-m scales in deep basins such as, e.g., Bay of Biscay, Canary Basin, below 4000 m, and on small (8-m) vertical scales, e.g., in the Irminger Sea (van Haren, 2007). This will be better indicated now.

A review of the physical oceanography of the NW Med. is perhaps justified---with no need to claim direct relevance to AMOC. As it stands, the paper leaves the reader with a long list of questions. For example, are these brief records typical of all times or all years? Don't the topographic gradients play a role in the larger scale circulation? A major literature now exists on the boundary layers on topographic slopes of rotating stratified fluids. (Ferrari, McDougall, Garrett, Holloway, etc.). The Bergen group and others have written much about internal wave interactions with mesoscale eddies. None of this is mentioned.
>>>The 'brief' 100-day records are typical for winter/convection and summer/stratified seasons, as has been verified with other data sets and as was also indicated by Saint-Guily and the Medoc-group in the 1970's. I am reasonably aware of and acknowledge the importance of sloping topography for deep-ocean mixing and the impact on larger scale circulation. I have worked for several decades on observations of internal wave turbulence above seafloor slopes, starting with work described in (van Haren, Oakey, Garrett, JMR1994), via observational programs above a wide variety of ocean topography including more recently in a deep canyon (van Haren et al., 2024; Wynne-Cattanach et al., 2024), a program that was initiated following the modeling works by, e.g., Ferrari

and McDougall. Now, I have somewhat more clearly indicated this in the manuscript, but one cannot mention everything (in a note). As the oceanographic literature is vast, it would have helped if the reviewer specified some of suggested works of the 'Bergen Group'. As far as I am aware, few works exist on spectral coupling from mesoscales through internal band into turbulence ranges. Suggestions are welcome.

Is the discussion by Scott and Marotzke of convection in numerical models irrelevant? (They conclude that convective regions are numerically just regions of strong diffusion, with vertical velocities being important elsewhere. Applies to the Med.?)
>>>Thank you for the suggestion, the effect of convection in numerical models is mentioned now. I also include now the numerical modelling findings by Scott & Marotzke indicating that boundary mixing is important for AMOC, although that is not the main topic in this manuscript (which in fact states that such items are ignored in pure mathematical modelling of the ocean, unlike the works by Scott & Marotzke2002 who include physical processes like boundary mixing although in a different way than recent works by Ferrari and McDougall, a.o.). This will be better indicated now.

Paragraph starting on line 106. Is the description applicable everywhere? Or just in the Med.?
>>>Everywhere, and references are from various ocean regions, as better indicated now.

Line 125. What is "sufficient" mixing?
>>>sufficient for maintenance of deep-sea stratification (Munk and Wunsch, 1998), as indicated now

The theory of open ocean inertial waves treats them as wave caustics (Airy functions) of the background internal wave field. Is that local physics unimportant here? Maybe the Med. can be treated as a constant f ocean? But surely not the N. Atlantic.
>>>The latitudinal variation in the Med allows variation in f by up to 15%. More likely, (sub-)mesoscale eddies induce larger local effects in 'effective' inertial frequency by varying relative vorticity. Such local physics will have effects, besides other effects like local boundaries.

Line 175 parenthesis missing?
>>>OK, modified to (3+/-2)f

Line 215. "frequency" is missing
>>>Yes, thank you, added now.

Line 239 what region was San-Guily discussing?
>>>Saint-Guily's theoretical work was inspired by western Mediterranean observations. This is indicated now.

Line 341. Is "re-analysis" meant in the sense that meteorologists use the term?
>>>No, more generally analyzing something again, re(newed)analysis.

Line 351. "Complexing factors" is not standard English

>>>Complicating factors was meant, thank you.

Review2
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Review of manuscript by Hans van Haren

Technical Note: A note on stabilization mechanisms of, e.g., Atlantic Ocean meridional overturning circulation

This is a very interesting manuscript considering the AMOC as a complex system depending on many processes in the ocean.

The author shows that account for such physical processes as internal waves and internal tides influences the AMOC. Accurate account for these processes in the model would stabilize the circulation preventing its collapse.

The paper deserves publishing.

>>>Thank you for the appreciation.

Minor remarks

(1) I think it is not correct to write abbreviation e.g. in the title

>>>The title has been modified now to: 'A note on small-scale potential feed-back mechanisms of large-scale ocean circulations'.

(2) The author writes on pages 53-54 that Schematically, the Atlantic(-Ocean) Meridional Overturning Circulation (AMOC) transports heat from the equator to the poles near the surface.

Then on page 70 he cites Wunsch and Ferrari and writes that the ocean is not a heat machine.

>>>Correct, but perhaps this is confusing. In l.53 I now modified 'AMOC) transports' → 'AMOC) is depicted to transport'. In l.70 I modified 'not a heat engine (Wunsch' → 'an ineffective heat engine (Munk and Wunsch, 1998; Wunsch', noting that my previous formulation literally appears in Wunsch and Ferrari (2004), and adding 'despite its heat transportation' after '2004)'.

To my opinion on page 73 it should be written that in addition to the heat machine that warms in the tropics and cooled at high latitudes it is also wind driven and tide driven. Fomation of Antarctic Bottom Water that spreads to northern mid-latitude occurs only to cooling in the Weddell Sea and ice formation. Then tides and internal waves cause mixing.

>>>*According to Wunsch and Ferrari (2004), following Munk and Wunsch (1998), if we consider the main drivers of the overturning circulation, wind and tidal (turbulent mixing) are the main drivers of the heat transport, while the buoyancy driven heat engine is the minor driver. That is better explained now (in (old) l.73 and the preceding sentence).*

Page 79 Without turbulent mixing, the AMOC would be confined to a 100-m thick near-surface layer and the deep-ocean would be a stagnant pool of cold water…

To my opinion this idea was first put forward by Mink and Wunsch (Abyssal receipts, DSR, 1998)

>>>*Yes indeed, this reference is now added, thank you.*

Review3

>>>*I thank the reviewer for the time to provide comments that helped improving my manuscript. My replies are behind*>>>

I find this article interesting, but too speculative for publication, especially in an area as topical and important as the AMOC. The key argument, I believe, is that internal wave driven mixing is an important driver of the AMOC and its variation under climate change needs to be included in models of the changes in AMOC. There are also appropriate words about caution in predicting the evolution of complex systems. Both of these things have been said before and not much is added here.

>>>*Thank you for the interest. The key argument is that several 'small-scale' physics processes are complex factors in driving the large-scale ocean circulation. As an example, one of these processes is internal wave driven mixing. I agree, as may be inferred from the cited references, that this has been said before but apparently not incorporated in some recent modeling works. In my manuscript I add information from re(newed)analysis from deep-sea observations. I must have been unclear, but very few observations have been presented of convection turbulence in the deep ocean.*

The bulk of the paper describes a few observations in the deep Mediterranean, from which it is concluded that internal waves and submesoscale motions may be dynamically linked. There is a significant body of theoretical and modeling work which supports this idea, but the arguments here at add little. In particular, the attempt to link subinertial and superinertial dynamics based on comparisons of frequency spectrum slopes is entirely unconvincing. There are lots of difference ocean processes with slopes near -2, so drawing strong conclusions from these alone is difficult. Speculation that scalars do not have a -5/3 wavenumber spectrum in idealized high-Reynolds number turbulence is also disturbing, since it is well established that they do. I don't know how any of this has anything to do with decadal changes in the MOC.

>>>Not only deep Mediterranean observations are shown, also from the North-Atlantic. It would be good if the 'significant body of …work' was substantiated by naming a few references. I do not recall having seen clear presentations of spectral observations that demonstrate a possible coupling between internal waves and submesoscale motions, so I disagree that these new presentations add little. The observations allow distinction between spectral slopes different from -2 at a statistically significant level, and several existing ocean processes with slopes near -2 are named. I would appreciate if the reviewer could elaborate on the qualification 'entirely unconvincing'. I do not speculate that scalars do not have a -5/3 frequency (not wavenumber) spectrum, but I present observations that show a large buoyancy subrange and which, apparently, do not resolve the inertial subrange. The observations merely demonstrate significant deviations from a -5/3 spectrum in the (sub-)mesoscale –internal wave-large turbulence frequency range. This is better indicated now. The moored records were too short to resolve decadal variations, unfortunately.