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

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).
- 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?
- 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).

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.

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.

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.

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.