Title: Effect of double diffusion processes in the deep ocean on the distribution and dynamics of particulate and dissolved matter: a case study in Tyrrhenian Sea
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My main comment on the second revised version concerned the lack of clear evidence of retention-aggregation process. To address this point, the authors have now included two new plots which make use of a larger number of CTD profiles to statistically show the influence of staircases on the Junge index and the AOU. In my view, this is a useful addition that provides clearer evidence of the retention-aggregation process. While the differences between the two groups of profiles (with and without staircases) in the decreasing slopes of the mean Junge index and AOU with depth are weak, they exist. This new figure provides more convincing arguments than the former versions of the manuscript, and I thank the authors for this improvement. Statistically speaking, more could probably be done, but as it is now, the manuscript has already gained in strength, and the arguments are more convincing to readers.

I don't have any further major comments regarding the core of the manuscript.

However, this new version includes a point about the Richardson number (Ri) in response to a query from one of the other two reviewers. Using this Ri causes confusion and does not provide any useful information:

My first comment relates to the way Ri is calculated, which makes use of buoyancy frequency (from CTD) and shear (from LADCP) profiles, both smoothed over a large scale (48 m, l. 268). At this scale, it is unlikely that shear-driven favourable regions will be detected. This is because, at these scales, the shear is usually far too weak and that open-ocean instabilities far from boundaries occur at meter-scale. The only reason Ri is sometimes below the theoretical value of ¼ in this dataset is due to the very weakly stratified layers. Shear-driven turbulence is driven by shear, not by weak buoyancy!

From my experience, even with a 10-m scale LADCP shear and N^2, it is difficult to associate Ri (say lower than ¼, or lower than some number such as 1 to account for the low vertical resolution of the LADCP shear profiles) with, for instance, enhanced levels of turbulent kinetic energy dissipation rates (i.e. measured turbulence levels) unless you sample very active, shear-driven turbulent regions. Thus, using 48-m scale resolution in an open-ocean staircase environment, a worse configuration, will only exhibit weakly stratified regions, which could be achieved using N^2 only. Attempting to link $Ri < 1/4$ and turbulent mixing is meaningless with this configuration (l. 487 - 497).

My second comment relates to the interpretation of Ri in the context of staircases. As mentioned (l. 532-546) in the text and by previous studies, vertical shear (low Ri) tends to prevent the onset of salt-finger instability. At l. 495 – 497, because staircases have almost homogeneous layers that consequently drop Ri below ¼, the authors state that turbulent mixing (= shear-driven here) is "concentrated in the homogeneous layers of the staircases". The fact that homogeneous layers exist is not linked with a shear-driven process but with a convective salt-fingering process. Using a 48-m scale Ri cannot be used to infer the existence of enhanced shear-driven turbulence in those homogeneous layers.

l. 566-57: "Below 1000 m, reduced stratification and shear create low Ri conditions, which appear

to be important for both the formation and maintenance of thermohaline staircases, as evidenced by both float measurements and cruise data in the basin's central region”
So you mean that low Ri (= shear-driven turbulence, if you can ever diagnose it with this Ri) is helpful for the formation and maintenance of thermohaline staircases ?! This contradicts what you wrote about previous studies at l. 540-546. Some clear explanations of the processes are needed.