Title: Tipping of the double-diffusive regime in the Southern Adriatic pit in 2017 in connection with record high salinity values
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Major comments :
The manuscript presents an original 5-year-long time series from the southern Adriatic that characterizes the evolution of the thermohaline content from the surface down to 1200 m. Despite the relative coarse vertical resolution of the measurements, the large scale variations of the salinity and temperature contents capture the winter deepening of the mixed layer, which depends on the intensity of surface heat losses and the salinity stratification. The analysis focuses on the potential role of the salt fingering regime on the evolution of the stratification of the region.

The pieces of information provided by this study are new, interesting for the region and complement previous studies. Therefore I would recommend the study to be published after some revisions.

My main comment is that, it's not because the stratification is prone to salt-fingering dynamics that salt fingers will indeed develop. Many times in the text, the opposite is mentioned, that is, authors observe a favorable regime for salt fingering, thus they conclude that there is mixing associated with that process. More care should be given in the use of the Turner angle.

At other places, the term "mixing" is used (good examples can be found in the start of the discussion). "Mixing" is a generic term that covers many processes. The process(es) should be mentioned whenever the term mixing is used so that the reader knows what it is all about (winter convection/deepening of the mixed layer, salt-fingering, …).

In the same vein, there are few descriptions of the figures done in the text that lack of precision and/or do not seem correct. Some examples are given in the detailed comments below.

Detailed comments:

l. 54: in the Med Sea there are several estimates of the diffusivities associated with "double-diffusion" that could be referenced on purpose (e.g. *Bryden, Harry L. et al. "Thermohaline staircases in the western Mediterranean Sea." Journal of Marine Research 72 (2014): 1-18)*

l. 120: How were the geostrophic velocities estimated at 1150 dbar ?

l. 146: I guess there is a connection between "the intrusion of high salinity into intermediate layers" and "the strong convection event", but the two are presented as independent observations…. Thus, I'm not sure that I have correctly understood if one is the consequence of the other, or if I do not focus on the right observations on Fig. 2…

l. 148: More care has to be given while referring to figures. As far as I can see on Fig. 3a, the heat loss do not exceed about 600 W m^{-2} (and not 700) in January and 100 or 200 W m^{-2} in March (and not 500). In Tab. 1, the max. heat loss of winter 2016-2017 is 623 W m^{-2}.

l. 178: Are you referring to isohalines (38.76 to 38.80) on Fig. 3b and Fig.2ab (38.78) ?

l. 181-182: “There is no convection during winter 2019…” ?!? If winter 2019 is winter 2018-2019, given Fig 2ab and Fig. 3ab , I would disagree. The deepening of the mixed layer reaches 400-500 m. It is not as strong as the previous two winters but it is not very far behind.

l. 195-196: “...and the departure of the less saline… above the LIW (second).” This departure is not really visible on Fig. 3. I was wondering if referring to Fig. 2b would be better ? On Fig. 2b, we easily identify the salinity maximum associated with the LIW and the salinity minimum above in 2015 and 2016. After Winter 2016-2017, there is no more clear salinity maximum (LIW) and minimum (above).

l. 197: At a scale of a gyre or a meso-scale, cyclonic conditions can favor MLD deepening. Here there is a cyclonic vorticity observed. I was wondering what was the horizontal extent of the cyclonic circulation ? (and this also relates to my previous question on how was determined the vorticity, from which geostrophic current estimates ? On what scales ? Fig. A3 does not really helps in that matter).

l. 199: that “cyclonic preconditioning of the stratification + strong heat loss” favors a low N^2 due to deeper than usual convection, I agree. But I’m not sure that the same sequence implies “observed salt fingering”. So far this is not observed and not described and this comment is confusing at this point of the manuscript. Later in section 3.3, you observe conditions that are somehow favorable to a salt fingering regime, though over most of the water column, the Turner angle is not strongly favorable (close to 90°). It remains weakly or moderately favorable, except in the 700-900 m layer where it is strongly favorable during year 2018-2019.

l. 254: “… the uppermost layer returns to strong SF”: I guess this comment refers to the very short time peak in January (-February ?) 2017 that brings the turner angle close to 90° (since later on the turner angle is “only” weakly SF favorable). On the other hand, the vector length is very weak apart from a peak at the very beginning of January (from what I can guess looking at Fig. 5 and 6). If the vector length is weak, does the SF favorable peak in Turner angle matters ?

l. 226: “… leading to SF”: How can you be sure that salt fingering is really actively occurring ? The sole observation of the Turner angle is insufficient.

l. 241: the paragraph starts with the two deepest layers? Then, second line we move to the top 3 layers (350-00). Third line, a prominent peak in Tu is mentioned, but is it that of layer 1 (350-550) occurring early 2017, or that of the lowest layer (1000-1200) in spring ? I’m a bit lost here.

l. 248: “… and a decrease in the two lowest layers”: it is not clear for the deepest one. There is a increase in spring followed by some oscillations. I’m not sure about any decrease if I compare with years 2015-2016.

l. 249: wording… “The Tu shows significant destabilization….”: What is significant ? What is “destabilization” ? Destabilization would mean instability. Again the Turner angle is not a proof that salt fingering is active. You could re-word in terms of “more or less favorable SF regime”..

l. 278: “we observe an increase of SF development”…. ??? Is there a figure showing that staircases increased ? You observe an increase of favorable conditions of the SF regime. You do not really observe the mixing associated with SF except for the example of staircases given with the Argo float, with the assumption that SF is at the origin of steps and layers.

l. 286: “reduced the mixing…” : Do you refer to winter convective mixing or salt fingering mixing here ?

l. 287: “… further reducing the vertical mixing”: this would be right if SF favorable mixing conditions (as diagnosed with Tu angle) => active mixing. This is not the case. You can just say that it reduces the possibility of having active salt fingering.

l. 292 – 298: a schematic showing the different contributors acting on the stratification in the SAP would be nice to summarized these ideas. To avoid adding a supplementary Figure, I would suggest to replace Fig. 7 c-d-g-h-k-l by this schematics. Fig. 7 c-d-g-h-k-l (Turner angles) could be grouped with Fig. 7 a-b-e-f-i-j using a color scaling that depend on the Tu angle, or different markers depending on the Tu regimes.

l. 301: the vector length is a novelty, ok, but it is not that much used in this manuscript.

Minor comments:

- e.g. l. 51, 54, … end elsewhere: Some care to the formatting of units and numbers is needed (...x10-4 m2/s => … $\times 10^{-4}$ m^2 s^{-1})(another example among others, caption of Fig. 3 “W*m-2”...)

l. 67: Could you add the seafloor depth at the mooring position ?

Fig. 3A, what are the red parts of the blue line ? This should be described in the caption.

l. 258: Fig. 7K instead of 8K ?

l. 289: suggestion “… due to the arrival of high saline surface waters that favored a convective…” (since its a part of the story, preconditioning by salinity, the other part being the enhanced heat loss compared with the two previous winters).

l. 409: Radko (not Ratko)

Figs A5-A8: what is the meaning of the color of the points ? (the only color-scale is depth).