Dear Editor,
below you will find our responses to the comments from reviewer 2. We have also added a further reference to the use of ECMWF data and corrected the multiple references chronologically.
Yours sincerely,

Vanessa Cardin

Title: Tipping of the double-diffusive regime in the Southern Adriatic pit in 2017 in connection with record high salinity values
Author(s): Felipe L. L. Amorim, Julien Le Meur, Achim Wirth, and Vanessa Cardin
MS No.: egusphere-2023-2481
MS type: Research article Iteration:
Revised submission
Special issue: Extremes in the marine environment: analysis of multi-temporal and multi-scale dynamics using observations, models, and machine learning techniques

We thank the reviewer 2 again for his corrections. Please find our answers in blue and the corrections performed in **red**. A revised version with our corrections in **color** is also provided.

Major comments :
The manuscript has been corrected according to the comments received for the initial version. Especially, more care is given to the question of salt-fingering (SF) probability of occurrence. However, I still have a few comments as you will see below.

Abstract:
The abstract concludes with "Consequently, we observe an alteration of vertical stratification throughout the water column". This adverb, "consequently", arises just after two sentences describing the increased predisposition to SF. Thus, the reader gets the impression that the alteration of vertical stratification **throughout** the water column may be due to SF while it is clearly due to convection after winter heat loss at the surface. This sounds misleading.

We removed:
~~**Consequently**~~

l. 23: parentheses should be removed.

Thanks for pointing this out, the brackets have been removed.

l. 55: correct me if I am wrong but the bulk diffusivity coefficient of 5x10-4 m2 s-1 based on two types of data from Cardin at al. (2020) encompasses all dynamical processes acting for years and not only double diffusion activity (for example inertial waves, courant-topography induced mixing, ...). Because of previous sentences and the way it is stated, the reader gets the idea that double diffusion is responsible for this eddy value. More care is needed in the formulation if I am right, and more details should be given when referring to Cardin at al. (2020). Moreover, Cardin at al. (2020) focused on regions deeper than 750 dbar while this manuscript often focuses on the regions above.

We now added:
in the deep SAP (below the sill depth of 750 m)

It was and is written in the paper that:
“can enhance vertical mixing through double diffusion”

and further down (l. 59)
“it can affect larger spatial extents by mixing water mass properties”

in l. 62:
“can enhance the mixing”

in l. 63:
“their potentially important contribution to vertical mixing”

We do not feel that due to our formulation “the reader gets the idea that double diffusion is the only responsible for this eddy value”

l. 58: it is true that the “bulk” (i.e. calculated over large vertical scales of several hundreds of meters) eddy diffusivity coefficient of tracers (temperature, salinity) are around 2-6 x10-4 m2 s-1. Those values come out when the large-scale tracer gradient are taken for the computation. However, this large-scale gradient encompasses steps/interfaces (strongly stratified with double diffusive activity) and layers (almost unstratified). As Bryden et al. (2014) stated, double diffusive processes operate on the thin steps (meter-scale). There, $k_S = 3.7 \times 10^{-5}$ m2 s−1 and $k_T = 2.0 \times 10^{-5}$ m2 s−1, that is 15 times weaker than the bulk estimates. Those eddy coefficients are really those associated with the double- diffusive activity and were also measured with microstructure data (Schmitt et al., 2005, doi:10.1126/science.1108678; Ferron et al. 2021, doi: 10.3389/fmars.2021.664509) and in Radko and Smith (2012, doi:10.1017/jfm.2011.343)’s model for instance. Those eddy diffusivities remain small, but are associated with interfaces (“strong” vertical property gradients, which is the other important parameter when computing turbulent property fluxes) and, more importantly, concerns most of the time large spatial areas (as assumed at least in the Med, but also in some regions of the Atlantic for example). Thus, the overall contribution of double diffusive processes is thought to have an impact at the scale of those large areas, especially when they act for a long time (as assumed for the Tyrrhenian Sea). Maybe that a bit more detail and caution is needed when giving the eddy diffusivity values.

The reviewer is right, the problem of how gradients and turbulent motion and different scales interact to cause mixing, and how this can be cast into a simple diffusion equation with a scale dependent diffusivity, is the coarse-graining problem. The coarse-graining problem has so far only been solved for very simplified extended systems (spin classes) using renormalization group methods. These methods are gradually being applied to diffusion problems in homogeneous isotropic turbulence, but are still far from providing insights into real data. In the SAP, the situation is inhomogeneous, non-isotropic and a large number of processes interact non-linearly. We do not want to mention this topic in this paper, as it only causes confusion. In Cardin et al. (2020) and in the present paper, there is no confusion because the estimates are based on data separated by O(100 m) in the vertical. Thus, we only can and only speak of large-scale (bulk) coefficients. We have now added:

large-scale (bulk)

l. 309: ‘that’ missing ? ‘… due to the arrival of high saline waters **that** favored a convective event penetrating’

We added **that**.

l. 313: ‘possible’ missing: ‘ Salt fingering is shown to be a consequence and a **possible** driver of the density structure…’

We added **possible**.

l. 317: ‘How the competition between sporadic-extreme and continuous processes changes the structure of the water column and, in the future, how it acts on the thermohaline circulation of the Mediterranean is not decided.’
→ to gain insight on this competition for the SAP, you could apply a rapid calculation of salt and heat fluxes using bulk eddy coefficient typically encountered when SF is supposed to be active, and compare them to those due to the fast convection events. Given various assumptions regarding the duration and the vertical extension of assumed SF activity, you would get a rough idea of the competition between sporadic-extreme convection and more or less ‘continuous’ processes associated with SF processes.

This was done in Cardin et al. (2020), where we constructed a 1D forced diffusion model and estimated the diffusion parameter, which was still unexplained at that time. We also determined a characteristic time scale for the diffusion process in the dSAP of about 7 years. In the present work, we evaluate SF as an important factor. The forcing due to gravity currents is the subject of current research in the ongoing PhD thesis of J. Le Meur (co-author). However, the aim of this work is to use mainly the E2M3A high-frequency time series.

The text has been changed to:

How the competition between sporadic-extreme and continuous processes changes the structure of the water column and how it acts on the thermohaline circulation of the Mediterranean will be a subject of future studies.

l. 325: ‘The high vertical diffusivity coefficient of 5x10-4 m2 s-1 found in Cardin et al. (2020) is explained by the dominant SF double-diffusion regime below 750 dbar, and the probability of SF occurring in the whole water column increased after the winter of 2016/17.’

Several points:

1- There is an ambiguity:
When I read this, I wonder whether the explanation comes from Cardin et al. (2020) or from your study and conclusions. From my (too) fast reading of Cardin et al. (2020), I would say this is your assumption.

One of the main goals of this work is to explain the high vertical diffusivity coefficient found in Cardin et al. (2020). We believe that this has been achieved with our analysis, in which it is mainly explained by the dominant SF double diffusion regime below 750 dbar, and that the probability of SF occurrence in the entire water column has increased after the winter of 2016/17.

The text has been changed to:

The high vertical diffusivity coefficient of 5x10-4 m2 s-1 found in Cardin et al. (2020) is here explained by the dominant SF double-diffusion regime below 750 dbar, and the probability of SF occurring in the whole water column increased after the winter of 2016/17.

2- Again, if my reading of Cardin et al. (2020) was not too rapid, their reported eddy diffusivity only concerns waters deeper than 750 m. You should then be more specific. Given Fig. 5, it appears safe enough to assume that double diffusion (SF) may be the main contributor of this diffusion in the depth range (750-900). However, above 750 m, Turner angles are rarely very favorable to SF and your assumption is then quite weak and it sounds more like a speculation.

Indeed, Cardin et al. (2020) base their analysis on two types of data, namely 13-year time series of observational data (2006–2019) of temperature from the E2M3A Observatory and 55 vertical profiles (1985–2019) below 750 m depth in the dSAP. The aim of extending this work to the layers above this depth was to observe the effects of deep convection and salt finger conditions. The citation of the reviewer includes "below 750 dbar" so we explicitly state that it is about the deep SAP.

3- on the opposite, to support your SF assumption, you could use the two Argo profiles and calculate the Turner angle across the interfaces in regions where staircases were observed. Indeed, in the first version of your manuscript, the Turner were only moderately favorable (50-70°) to salt-fingering, but not that close to very favorable (close to 90°). I was wondering if there was any sensitivity in the way you calculated those Turner angles with the Argo profiles (which were smoothed over 20 m or so instead of calculating them across the identified interfaces in the staircase structures?). If, you could obtain Turner angles closer to 90°, this would bring some support to SF activity and contribution, at least in the regions where staircases were observed. And then you have an argument to say that the very limited vertical resolution of the mooring, that shows weak to moderate favorable angles to salt- fingering in the upper layers, may hide some much more favorable angles to SF (if you had more resolution to compute them).

The strongest evidence for SF is staircases. Therefore, the Argo data was used to show that staircases occur where they are predicted by our coarse-resolution TU analysis. Thus, we make clear that vertically coarse resolution data can be used to indicate the possible occurrence of SF. A detailed investigation of TU variations across staircases is not possible with our mooring observations and is not the subject of this paper (although it is undoubtedly an interesting topic).