

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.: egosphere-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

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.

l. 23: parentheses should be removed.

l. 55: correct me if I am wrong but the bulk diffusivity coefficient of  $5 \times 10^{-4} \text{ m}^2 \text{ s}^{-1}$  based on two types of data from Cardin et al. (2020) encompasses all dynamical processes acting for years and not only double diffusion activity (for example inertial waves, current-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 et al. (2020). Moreover, Cardin et al. (2020) focused on regions deeper than 750 dbar while this manuscript often focuses on the regions above.

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  $2-6 \times 10^{-4} \text{ m}^2 \text{ s}^{-1}$ . 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,  $k_S = 3.7 \times 10^{-5} \text{ m}^2 \text{ s}^{-1}$  and  $k_T = 2.0 \times 10^{-5} \text{ m}^2 \text{ s}^{-1}$ , 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.

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

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

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.

l. 325: 'The high vertical diffusivity coefficient of  $5 \times 10^{-4} \text{ m}^2 \text{ s}^{-1}$  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.

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.

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).