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Discussions

Interactive comment on "Water masses and mixing processes in the Southern Caribbean upwelling system off Colombia" *by* Marco Correa-Ramirez et al.

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Anonymous Referee #1 This manuscript presents a study of the Caribbean upwelling system based on four cruises as well as the analysis of outputs from numerical simulations performed with the Mercator model. There are two main objectives: the first one is to determine the origin of the upwelled waters and the second one is to characterize mixing processes that may influence biological productivity. The upwelled waters, that are mainly constituted of Subtropical Water Mass (SUW), are characterized by a local salinity maximum. This salinity maximum presents a strong seasonal variability and is significantly smaller than that of the SUW, as inferred from in-situ data. The pathway

of these upwelled waters is inferred from the Mercator model outputs: they originate from the Western Caribbean Sea and are transported by the intense Caribbean coastal undercurrent (CaCU). Mixing processes are estimated and shown to be significant with vertical diffusivities of 10-4m2s-1 for double diffusive processes and 10-3m2s-1 for mechanical mixing. This mixing impacts the salinity distribution of these coastal upwelled waters but the impact on the nutrient content in the upwelling region is to be determined.

The manuscript is generally well written and presented with clear figures. The topic is interesting and the questions adressed relevant though I don't know to which extent the results are new, not being a specialist of the watermasses and circulation in the area. Also I find a lack of convincing results with respect to the points addressed. My major concern is on mixing processes, with estimates of diffusion by salt fingering (SF), active below the subsurface salinity maximum, and by turbulence (T). The SF diffusvity is derived from a formula (eq. (3)) with out any reference. I guess that this formulation is derived from Schmitt (1981), in any case it should be mentionned. R:// In fact the Salt finger difusivities are derived from Schmitt approximation. The The missing reference was incorporated in line 169.

I wonder about the background constant value, which is large, as well as the maximum Ksf value, how were they prescribed? R:// Part of the Ksf background values correspond to the Kinf = 3x10-5, which is a constant value that account for the diapycnal diffusion due to processes unrelated to double diffusion, like the internal wave breaking (Schmitt, 1981). This value should be considered when estimating the total diffusivity of the salt, since Kinf difusivities are no related with salt fingers neither the shear instabilities generating the mechanical mixing. To ensure a better comparison between Ksf and Kt, we decided to follow the suggestion of the reviewer and not add this constant value in the Ksf estimates of the revised manuscript, which are shown in the new figures 2 and 4. This change are now explained in L171-172 "This coefficient was not considered in the Ksf estimates to guarantee a direct comparison only between the double diffusion process and mechanical diffusion due to shear instabilities."

The vertical eddy diffusivity, KT, is inferred from density overturns. The method is described with details except for the background value when no density overturns. R:// The explanation of the eddy diffusivity estimation (KT) thought Thorpe scale was expanded in the revised manuscript (L174-196). The profile sections where no overturns were detected do not necessarily mean there is no vertical mixing, because small overturns lower than the vertical CTD sampling intervals could exist but not be detected. Considering the smallest detectable overturn, a conservative value of 1x10-6 m2 s-1 was set in this regions, as suggested by Zhu and Zhang (2018). This is now explained in L190-194.

Its application to in-situ data is not detailed. It would be interesting to know how the KT vertical sections shown in Figure 4 were obtained: indeed according to the N2 sections, the stratification is always stable, so one may wonder whether density overturns were resolved or not, how the computation for each individual vertical profile was performed (with or without a background value) and how the interpolation was performed for KT. R:// We includes a new figure (figure 2) showing an example of how the Kt computation was done. The N2 profiles showed are calculated from smoothed density profiles with a 10 m mobile mean, to avoid that the artifacts caused by density spikes biasing the Kt estimates, as now explained in L190. Because that, small stratification changes due overturns are not evident in N2 profiles. Besides, the diffusivities below the 300 m depth where the respective N2 values were lower than 3x10-5 s-2 are excluded since low N2 values could produce high erroneous diffusivities.

For all sections showed in the manuscript figures we use DIVA interpolation method, as explained in L35-39 of Methodology.

It is also confusing to discuss the relative part of the salt fingering and turbulent mixing contribution to diffusive salt fluxes with a background Ksf taking into account other mixing processes (i.e. mechanically driven mixing, for instance by internal wave breaking).

This analysis is not convincing: this may result both from the lack of details provided and mostly from the inadequacy of the dataset to this aim. R:// As explained in the above responses, we decide do not consider Kinf in the recalculated Ksf estimates of the revised manuscript, following the reviewer suggestion. With this change is now possible a direct comparison between Ksf and Kt diffusivities.

In conclusion my advice would be to remove the part on mixing processes as it is not convincing(see details above). Also the objective is too ambitious owing to the data available, without current measurements and microstructure measurements. Regarding the water mass part and upwelled waters pathways, it should be strenghtened with further analysis. For instance a lagrangian analysis based on retro-trajectories may be helpful to this purpose for tracing water masses pathways and provide more convincing results. R:// We improved Figure 8 (Figure 9 in the revised version) with the inclusion of streamlines to observe the flow path of subsurface waters in the Caribbean. We also show in the new version of Figure 6 that the modeled data available does not accurately reproduce the salt and the temperature at depth. Because of that, the use of this data to track water masses, as the reviewer suggests, would not produce a precise result. We believe that adequate tracking of water masses should be carried out through the use of tracer experiments in further studies.

My recommandation is to submit a new manuscript focused on the circulation and water masses excluding mixing processes. R:// Diffusivity and salt flux estimates have not be done previously in the region. We considered important report this estimations as a valuable piece of information needed to contributes of a better understanding of this upwelling system. Besides, is an important finding establish the depth where the different mixing processes domain the proprieties transport in the water column of this upwelling system.

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