Answer to Qian Li, Southern Ocean deep mixing band emerges from a competition between winter buoyancy loss and upper stratification strength

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December 2023

Dear Qian Li,
Thank you for your careful consideration of our manuscript. We believe that based on your comments, we were able to improve the clarity of our manuscript.
Please find our answers to the reviews in this document. We used `this monospace font` to cite the original comments, and we provided point-by-point responses.
Best regards,
Romain Caneill and co-authors

Major comments

1. `Lines 305-307`

   (a) `What is the definition of ‘‘`**`narrowness`**`’’ of DMB here? What is the horizontal resolution of observational data and ECCO outputs used in this study? Compared with the MLD simulated from an eddying (0.1°) ocean model (e.g., Fig. 2 in Li & Lee, 2017), the meridional extent of wintertime MLD (Fig. 1b) looks still quite broad to me. Note that the MLD from de Boyer Montégut et al. (2023) is at 1° × 1° spatial resolution. More discussion/clarification is needed here.`

   Thanks for the question. Narrow can indeed have different meanings depending on the context. Here it should be understood as "having a belt shape, i.e. being more elongated than wide, and representing only a small region (in latitude) of the Southern Ocean". We replaced "narrowness" by "limited latitudinal extent" to clarify.

   The observation data have 1 degree of resolution for the fluxes, and MIMOC (used for columnar buoyancy) is at $0.5° \times 0.5°$ resolution. We added a sentence in methods (Sect. 2) providing the resolution.

   Eye inspection of Fig. 2 in Li & Lee, (2017) gives a latitudinal extent of 5 to 10 degrees of the 250 m contour, which is consistent with the white contour defining the DMB (Fig. 1 (a) of our manuscript). We use a different colormap saturating at 350 m, instead of 550 m in Li & Lee (2017). We belive that this is the main reason the DMB appears "narrower" in the latter figure.

   (b) `The authors also need to clarify that ‘‘this balance is sufficient. . . ’’ on the timescale of annual mean or 6-month-mean, which is the time period focused in this study. In fact, I am concerned on comparing the contributions of surface buoyancy fluxes and Ekman buoyancy transport on the timescale of 6-month or longer, as they both can play the dominate role of preconditioning in the deep MLD formation but during different periods. For example, the May net air-sea heat flux and June Ekman heat advection are both critical in the August/September MLD formation (Li & England, 2020).`

   This is true that this study focuses on large spatial scale, annual (or 6-month) state, and climatological state. We added to the sentence that the balance is sufficient to predict the DMB in the climatological state.

   Regarding your question about comparing the contribution of surface buoyancy fluxes and Ekman transport of buoyancy: both are considered on a time-mean basis during the cooling season, which spans 6 months. In our analysis, we account for both of their individual effects, integrated along all the cooling season. We added a sentence in section 3.2 mentioning that the Ekman transport can participate in the interannual variability of the MLD (Cerovečki et al., 2019; Li & England, 2020).

(c) The first-order role of surface buoyancy fluxes is mostly considered on a large-scale or a zonal average (Fig. 5). In southeast Pacific, Ekman buoyancy transport actually dominates the deep MLD formation and Subantarctic Mode Water (SAMW) formation (Cerovečki et al., 2019; Li & England, 2020). More discussion is needed here.

Thanks for this comment. The balance done in the study is between the stratification, and the *total* buoyancy fluxes (surface + Ekman). Ekman transport is thus taken into account. We realised that it was not entirely clear in the manuscript, as $\mathcal{B}^{CS}$ was defined in Sect. 2.1.1, before we introduce the Ekman buoyancy transport. We clarified the manuscript by moving the definition of $\mathcal{B}^{CS}$, and explicitly writing that it is the sum of the surface fluxes and Ekman transport. Moreover, we did not aim to compute a full MLD budget, but only compare the balance between two important quantities (stratification and buoyancy loss). Computing the balance between the stratification and the *surface* buoyancy loss during the cooling season (i.e. not taking the Ekman transport into account) predicts the DMB with a quite good accuracy (Fig. 1 of this document). We thus do not find that Ekman transport is of primary importance in setting the DMB. It still acts as a secondary process that make the DMB slightly more larger.

(d) In the MLD budget, there are some other terms, such as vertical Ekman pumping and vertical mixing, that could be potentially important. More discussion is needed here.

We already mentioned in the conclusions that mixing contributes to deepening the mixed layer, we added a sentence to mention the Ekman pumping. As the balance between the buoyancy loss and the stratification is sufficient to predict the deep mixing band, other processes will likely modify slightly the existing deep mixing band, but do not think that they may dominate as their spatial distribution does not match the DMB.

2. Section 2.2: I do not follow why the authors use a **deep mixed layer threshold of 250 m** in the estimation of stratification intensity. The wintertime MLD, that forms north of the Subantarctic Front (SAF), can be much deep over 500 m. The summertime MLD at southern high-latitudes can be less than 100 m. Thus, it is unjustified to apply this threshold across the entire Southern Ocean. I suggest to use the actual MLD in the calculations (Eq. 11). Then, this equation can be written as follows:

$$B_{\mathrm{MLD}} = \frac{g}{\Delta t}\left(\int_{-\mathrm{MLD}}^{0} \alpha(z)\frac{\partial \theta}{\partial z} z dz - \int_{-\mathrm{MLD}}^{0} \beta(z)\frac{\partial S}{\partial z} z dz\right)$$

We think that this comment comes from a confusion between the mixed layer depth and its threshold used to define the deep mixing band (250 m), and the depth at which we compute the columnar buoyancy. As the focus of this study in on the deep mixing band, we must use the same depth everywhere for the columnar buoyancy, otherwise the balance between B_{MLD} and $\mathcal{B}^{CS}$ would not predict the DMB. It could be a useful quantity to verify globally where the mixed layer is produced by buoyancy loss, and where other processes are playing a dominant role. We leave this question for another study.

We clarified the text regarding this, and we added a figure in the manuscript's appendix (reproduced in Fig. 2 of this document) to show that our result do not depend on the exact value of this depth, as long as the depth is the same to define the deep mixing band and for computing the columnar buoyancy.

Minor comments

- Figure 8: The authors examine three different transects in the Atlantic, Indian, and Pacific sectors of the Southern Ocean. However, these transects cover a large domain, in which many different processes may mix together. I recommend changing each domain to that more localized in the deep MLD formation region. For example, the SAMW formation regions analyzed in Li et al. (2021) and Cerovečki et al. (2013).

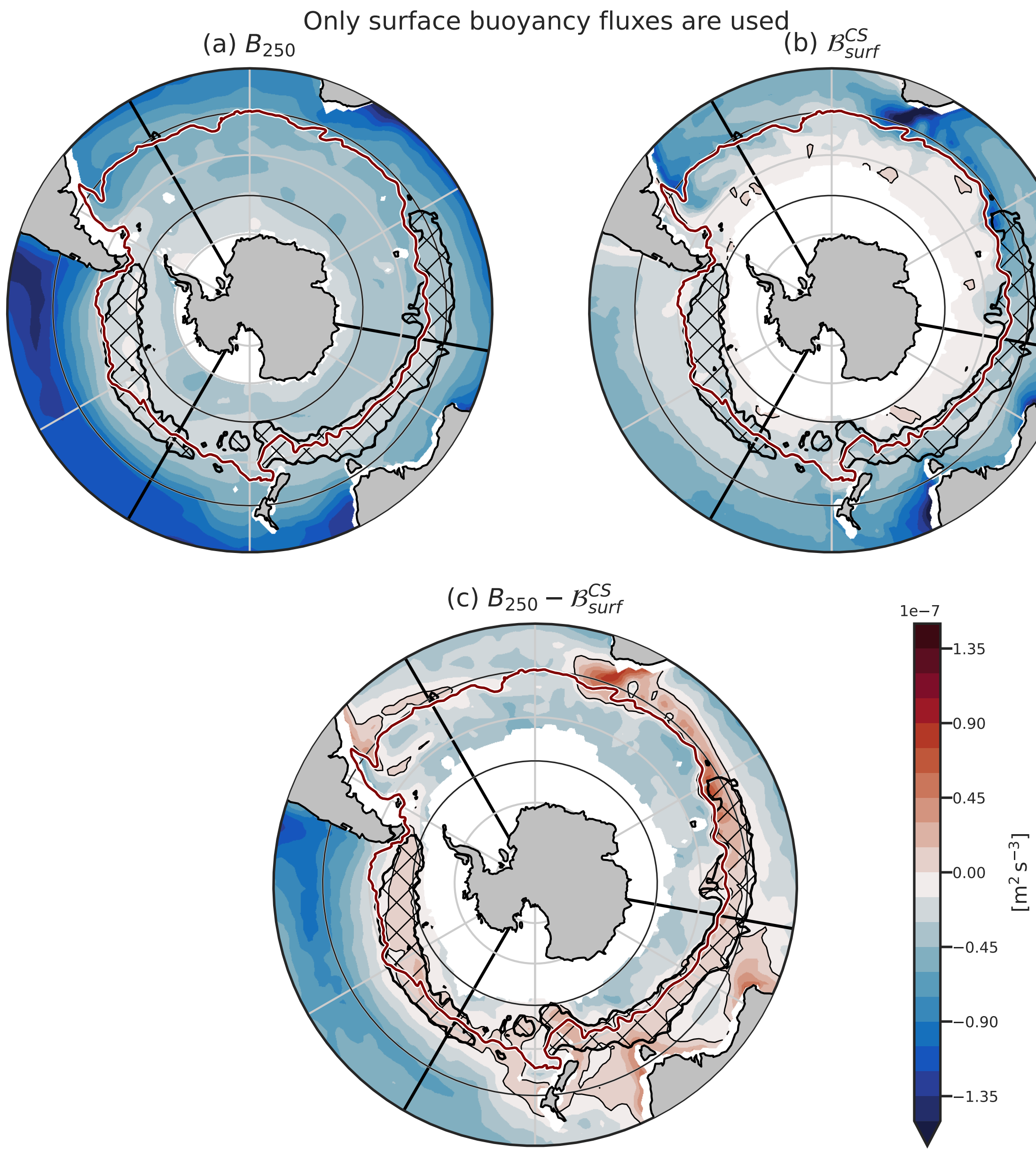

Figure 1: Same as Fig. 7 of the manuscript, but using only the surface component of the buoyancy fluxes.

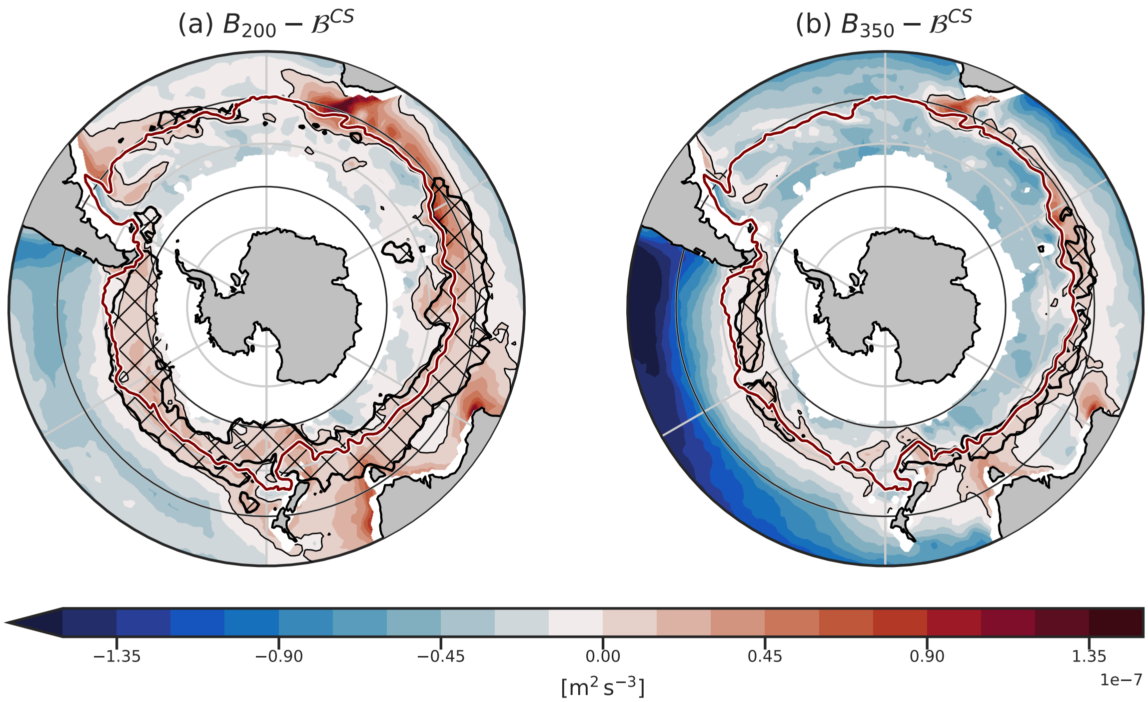

Figure 2: Comparison between different thresholds for the definition of the deep mixing band, and for computing the columnar buoyancy (added in Appendix of the manuscript).

In Cerovečki et al. (2013), they define the sectors with longitude bounds "150°E–70°W for the Pacific sector, 70°W–20°E for the Atlantic sector, and 20°–150°E for the Indian sector". Our 3 transects are taken in the middle of the three sectors (transects in the middle of the Atlantic sector at 330°E, in the middle of the Indian sector at 100°E, and in the middle of the Pacific sector at 210°E). The three transects we provide are thus already localised (they are not means for each sector).

- `Equation (3): Define the` τ^x `and` τ^y.

  Thanks for seeing that the definition was missing. We added it.

- `Equation (4)-(7): Define the` θ `and` S.

  Thanks for seeing that the definition was missing. We added it.

- `Equation (8): Define the` Z `and` z `right after this equation.`

  We added the definitions.

- `Figure 6: Add ''annual'' to the figure caption.`

  We added to the caption that the stratification is computed in April (it is not an annual stratification).

- `Line 331: Change Fig. 3 (f) to Fig. 4 (f).`

  Thanks for catching this typo, we corrected.

- `There are too many acronyms in the paper, and I suggest to reduce the use of them if possible. For example, I may suggest to spell out the ''Southern Ocean (SO)'', ''cooling season (CS)'', ''warming season (WS)'', etc.`

  We believe that most of the acronyms we use are commonly used (e.g. SO, the name of the fronts, the ACC). We acknowledge that CS and WS are new to this study (to our knowledge), without bringing much new information. Except in equations, we replaced "WS" with "warming season". Except in equations, we replaced "CS" by "during the cooling season" or an equivalent formulation.