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

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Dear Justin Small,

We appreciate your very relevant comments on our manuscript, and we believe that your suggestions will improve its clarity and scientific value.

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

- `Lines 17-20, Fig. 1. The text mentions Orsi et al. While the figure quotes Park et al. for identifying fronts. Which do you use?`

  We use the Park et al. climatology of the fronts (as written in the caption of Fig. 1). We added the Park et al. citation in the text.

- `Lines 33. I was surprised that the following important papers on SAMW were not referenced, as they are relevant to this paper: Cerovečki et al (2013), and Cerovečki and Mazloff (2016). These papers give an alternative viewpoint on where the DMB and SAMW forms from Watermass Transformation theory.`

  Thank you for pointing to these two references. They are indeed important, we added them.

- `Line 36. I wonder if the following references are more related to the DMB: Frölicher et al. (2015) and Li et al. (2023)`

  Thanks for these two references. It is true that the two references we cite at Line 36 are more general, but still relevant. We added your suggestion to the manuscript.

- `Lines 96-109. As an aside, CORE (Large and Yeager 2009) and JRA55-do (Tsujino et al. 2018) follow a similar approach of globally-adjusting surface fluxes. You may also want to view Fig. 17 of Small et al. (2021) which compares 4 products for surface heat flux. There the OAFLUX product was 0.25deg., personally provided by Lisan Yu, which will hopefully be freely available soon (https://oaflux.whoi.edu/data-1-4o/).`

  It is true that closing the heat budget is a hard task. . . . The new 0.25deg OAFlux product seems to be a major improvement in closing the budget, but it is not available yet.

- `Lines 150-152. The stratification measure shown in Eq. 9 was used by Lee et al. 2011 and Small et al. 2021 who should be referenced.`

  Thanks for pointing it out, we added the references.

- `Line 185. There is also large buoyancy loss off eastern Australia, an eastern boundary current!`

  You are right, the Leeuwin Current flows southward and induces large heat loss to the atmosphere. We added it to the sentence.

- Re discussion of Fig. 4: the temperature and salinity parts of B^{Ek} strongly compensate (i.e. SST gradients at fronts are compensated by SSS gradients) so that their net effect (Fig. 4c) is small compared to surface heat.

  Thanks for pointing that. It was not explicitly mentioned in the text, we added it.

- Fig. 5. State that these are zonal means (presumably).

  The solid lines are the zonal medians. We added this in the caption.

- Line 225-226. The sign conventions are confusing. The temperature component of stratification is negative but B250 is positive (in near-polar regions). Maybe just use one metric, B250, and be clear of the meaning of the sign.

  The confusion may have arisen from the swap in the caption of Fig. 7. B250 is negative everywhere, even in the polar region (Fig. 7a). We explicitly added that B_{250}, B_{250}^{Θ}, and B_{250}^{S} are negative under stabilising conditions.

- Line 235. I would remove the Small et al. citation from this line, replace with DuVivier et al. 2018, also Yeager and Large 2007: https://doi.org/10.1175/2007JPO3629.1

  Thanks for pointing it out, we changed the references.

- Line 236. Begin this paragraph with a statement like ‘‘We now consider the total stratification, B_S_250+B_theta_250. . . ’’

  Thanks for the clarification, we updated the text.

- Fig. 7 caption. Panel a) and b) titles are swapped.

  It is the caption that had the swap. We corrected.

- Fig. 7a, line 239. You could relate this to Small et al. 2021 their Fig. 3 and Supp. Fig. S4, which confirms that the DMB has weak stratification year-round -- i.e. preconditioning.

  Thanks for pointing to this figure of the Small et al. 2021 paper, which shows similar pattern. We added the reference and the remark on preconditioning.

- Fig. 8 is a nice figure but is not sufficiently described in the text.

  Thanks for your appreciation. We added a few sentences to describe better Fig. 8.

- Lines 243-251. This discussion of Fig.5 can be moved earlier, before the discussion of Figs. 6,7.

  We moved the discussion of Fig. 5 to the beginning of section 3.3.

- Section 4. The effect of non-constant TEC is interesting, but I do not think it is a first order factor for the DMB. The DMB will have overall similar characteristics with constant TEC (Fig. 11c) { although it is likely to be deeper and wider in the Pacific, and the Indian Ocean will have high latitude deep mixed layers. You say this in lines 298-304. Perhaps you can slightly de-emphasize the importance of TEC.

  We agree that the TEC variations do cause the formation of the DMB, however, they impact its location and *narrowness*. It is true that in the Indian Ocean, with a constant TEC, the predicted DMB has a similar shape as the observed one. This is not true in the Pacific Ocean, where the predicted DMB extends to the sea-ice edge. In this sense, we respectfully disagree with the reviewer and consider that variations in the TEC are first-order factor controlling the DMB.

  We slightly modified the last sentence of the paragraph to emphasise that the southern boundary of the DMB is strongly constrained by the TEC in the Pacific Ocean, while in the Indian Ocean the TEC variations prevent the formation of a second deep ML region south of the DMB.

- Lines 324-328 seem to discuss a small effect. Perhaps delete this paragraph.

  We shortened and moved this paragraph into the methods (Section 2.1.2).

- `Line 345. Finish the first sentence of this paragraph with ‘‘using constant TEC.’’`

  Thanks for clarifying this point, as only using a variable TEC can lead to a correct comparison between heat and freshwater fluxes. We corrected the sentence.

- `Lines 331-33 and 353-355. I think an interesting follow-on question is why the stratification in SE Pacific is so weak year-round? (see also Fig. 3a of Small et al. 2021). Sloyan et al. 2010 is a good reference which I was not aware of before. Perhaps it is relevant here that in the South-East Pacific the atmosphere storm track, and associated waves, are present year-round? You could also look at summertime air-sea buoyancy flux, and whether it receives much buoyancy gain in summer.`

  Thanks for this interesting question. We can see on Fig. 4 (f) that the SE Pacific is a region of negative annual buoyancy fluxes. It is also a region with only small buoyancy loss during the cooling season (Fig. 7 (b)). The summer buoyancy fluxes are positive, but quite small in this region. Precisely looking at the processes leading to the only small summer buoyancy gain would be an interesting follow-up study.

- `Finally, useful relevant papers are Qiu and Chen (2006) and Yu et al. (2020). These papers look at interannual variability of mixed layer depth, and how it depends on pre- winter stratification vs surface fluxes.`

  Thanks for these two interesting papers that we were not aware of. They could be the basis of a future work assessing variations in the position and properties of the DMB.