Review of "Southern Ocean deep mixing band emerges from a competition between winter buoyancy loss and upper stratification strength" by R. Caneill, F. Roquet and J. Nycander, submitted to EGUsphere.

Overview: This paper examines the deep mixing band (DMB) in the SubAntarctic Zone of the Southern Ocean. It presents a useful (& novel in this context) diagnostic of the DMB based on wintertime surface buoyancy loss and pre-winter ocean stratification. To the best of my knowledge this is possibly the first such diagnostic to identify the geographical location of the DMB.

It then proceeds to analyze the impact of having temperature dependence of the thermal expansion coefficients. I think this part is a bit over-stated, but interesting.

Overall I think this is a well-written paper with robust findings, of interest to EGU journals such as Annales Geophysicae, Ocean Science etc.

I recommend a minor revision with comments below.

Signed, Justin Small, NCAR

Minor comments:

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

Lines 33. I was surprised that the following important papers on SAMW were not referenced, as they are relevant to this paper:
Cerovečki I, Talley L D, Mazloff M R, Maze G (2013) Subantarctic mode water formation, destruction and export in the eddy-permitting Southern Ocean state estimate. J Phys Oceanogr 43: 1485-1511
Cerovečki, I., and M. R. Mazloff, 2016. The spatiotemporal structure of processes governing the evolution of Subantarctic mode water in the Southern Ocean. J. Phys. Oceanogr., 46, 683-710.
These papers give an alternative viewpoint on where the DMB and SAMW forms from Watermass Transformation theory.

Line 36. I wonder if the following references are more related to the DMB:
Frölicher TL, Sarmiento JL, Paynter DJ, Dunne JP, Krasting JP, Winton M (2015) Dominance of the Southern Ocean in Anthropogenic Carbon and heat uptake in CMIP5 models. J Clim 28:862–886
Li et al. 2023 https://www.nature.com/articles/s41467-023-42468-z

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/).

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.
Lee M-M, Nurser AJG, Stevens I, Sallée J-B (2011) Subduction over the Southern Indian Ocean in a high-resolution atmosphere-ocean coupled model. J Clim 24:3830–3849

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

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.

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

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.

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

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

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

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.

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

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

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.

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

Line 345. Finish the first sentence of this paragraph with "using constant TEC."

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.

Finally, useful relevant papers are Qiu and Chen 2006 and Yu et al. 2020
Qiu, B., and S. Chen, 2006: Decadal variability in the formation of the North Pacific subtropical mode water: Oceanic versus atmospheric control. *J. Phys. Oceanogr.*, 36, 1365–1380, https://doi.org/10.1175/JPO2918.1.
Yu et al. 2020 https://doi.org/10.1175/JCLI-D-20-0119.1

These papers look at interannual variability of mixed layer depth, and how it depends on pre-winter stratification vs surface fluxes.

For future work (this is not a review request, just a comment), it could be interesting to look at interannual variability in high-resolution models, including HighResMIP, also Small et al. (2014) https://www.earthsystemgrid.org/dataset/ucar.cgd.asd.output.html
And Chang et al. 2020 https://doi.org/10.1029/2020MS002298