Review of "Forcing For Varying Boundary Layer Stability Across Antarctica", by Dice et al. (egusphere-2023-2062)

General

This paper follows up findings presented in an earlier study (egusphere-2023-1673) in which the climatology of boundary-layer and lower atmosphere stability was examined at five Antarctic stations. In that paper, a system was developed to classify stability regimes based on potential temperature gradients within and just above the boundary layer. In this follow-up paper, the variations of two possible forcing factors – downwelling longwave radiation and near-surface wind speed – across the stability regimes are examined at the five stations and conclusions are drawn about the roles of these two factors in driving the regimes.

The paper is a logical follow-on from egusphere-2023-1673 and does provide some useful insight into the controls on near-surface atmospheric stability across Antarctica. It is generally well-written, although with 20 different stability regimes being examined at five stations during four seasons it can get quite hard to follow at times. The figures are informative but may not all be clearly readable at publication size – the legibility of some text is marginal even in the preprint version.

I do have some concerns about the methodology and approach used in the study, which I have set out below. Adequately addressing these points may require fairly major revision, but once this has been done the paper should be suitable for publication in *WCD*.

Major points

1. The paper examines variations in stability associated with two forcing factors – near-surface wind speed and downwelling longwave radiation. The reasoning behind this is that, within the boundary layer, the potential temperature gradient results from an interplay between the surface heat flux and mixing by turbulence. Over the Antarctic ice sheets there is a reasonably close relationship between the surface heat flux and the net radiative flux at the surface. Hence, outside the summer season, downwelling longwave should be a reasonable proxy for surface heat flux. During summer, shortwave radiation is a significant contributor to the surface energy balance and needs to be taken into account. This is mentioned (lines 660-665), but hasn't been followed up – surely shortwave data are available for all stations? Windspeed is probably a reasonable proxy for mixing in the near neutral to moderately stable regimes, but things get more complicated at higher stabilities as buoyancy forces increasingly suppress the mechanical production of turbulence. The relationship between wind speed and mixing is, therefore, not straightforward in this high stability range (see Vignon et al, 2017, referenced in the manuscript). This may explain some of the counterintuitive behaviour seen in, e.g., fig. 13.

2. In much of your analysis I think that you are making an implicit assumption that the boundary layer is in equilibrium with its forcing by radiative cooling and wind-driven mixing. This may not always be the case, particularly at the coastal sites where conditions can change rapidly as a result of the movement of synoptic-scale cyclones and other weather systems. Your VSML regime, for example, could be the result of a sudden increase in wind speed following an extended period of surface cooling resulting in the erosion of the surface inversion from below by wind-driven mixing. Your data don't have sufficient temporal resolution to study this in detail but I do think that you should mention it as a possible limitation to your analysis.

3. In your analysis, you study variations in stability both within the boundary layer and in the layer between the top of the boundary layer and the upper limit of your study (500m above surface level). By your definition, there is no (or very limited) turbulent mixing within this upper layer and it is largely decoupled from the surface (and hence from the surface energy balance). I would thus argue that it is not appropriate to try to explain variations in stability in this upper layer in terms of wind speed and downwelling longwave radiation as these parameters are strongly associated with the structure of the turbulent boundary layer but will not directly influence stability in the free atmosphere above. Stability in this upper layer will be influenced by things such as advection, subsidence, moist diabatic processes and radiative flux divergence. The thermal structure of this layer could also be a "relic" of a deep, stable boundary layer that has been modified at low levels as a result of increased wind-driven mixing (see also my point 1 above) or reduced surface heat flux due to an increase in downwelling longwave radiation. You could discuss these points further, e.g. in the paragraph starting at line 734.

Minor points

1. L180-181: The sentence starting "Above 20m…" is a bit confusing and needs rephrasing, e.g. "The 20 m lower limit was chosen because radiosonde measurements below this level are often biased warm". You have already mentioned (L. 156) that you do not use data below 20m because the sonde may not be in equilibrium.

2. Figure 2: Are all of these profiles from the same station? Are they single profiles or means for the class? What is the temperature anomaly with respect to?

3. L 508-509: Sentence starting "In the winter…" is incomplete – it does not have a verb.

4. L665: Delete "during the summer months" to aboid repetition.

5. L685: "lower", not "less".

6. Figure 13: Presentational point – the horizontal axis does not show a continuous variable so, strictly, you should not join the points with a line.

7. L 712: "…is almost always lower…". Is the difference statistically significant?