Response to Anonymous Referee 1 Comments

We would like to sincerely thank Anonymous Referee 1 for taking the time to read our manuscript and provide their helpful comments. These comments have helped to significantly improve the manuscript. Each referee comment is given below in ***bold italics*** followed by our response to the comment. The line numbers provided in our responses refer to line numbers in the revised manuscript.

General Comments
This paper presents a comprehensive overview of the distributions of atmospheric boundary layer (ABL) stability during the MOSAiC field campaign by classifying observed radiosonde vertical profiles into 1 of 12 stability regimes. The authors also thoroughly detail physical (thermodynamic and kinematic) explanations for these observed distributions, both in a bulk annual sense and by accunting for seasonal variations in the Arctic. Overall, this paper is organized very well, the discussions are scientifically sound, and the writing style is clear and concise. I especially appreciate the situational awareness that was demonstrated when it came to instrument placement during fog events and how a synthesis of observations can show a more complete picture. These results are certainly pertinent to future studies of the lower atmosphere during the MOSAiC campaign, making this paper a suitable fit for the journal ACP. I am pleased to recommend this paper for publication after the authors address a handful of minor comments that are outlined below.

Thank you for your positive review of our paper. Below we address each of your comments, and explain how and where changes have been made to the manuscript.

Minor and Technical Comments
1. Line 53: The phrase "negative longwave balance" is somewhat contradictory, perhaps change to read "net negative longwave radiation at the surface."

This change has been made, and the sentence now reads:

"… there is negative net longwave radiation at the surface…" (line 54)

2. Line 123: Perhaps I missed it, but please define the acronym "ARM" somewhere in the text before using it here.

We have added a sentence to introduce ARM (Atmospheric Radiation Measurement mobile facility) before discussing ARM instrumentation and products:

"Several additional measurements come from instrumentation deployed as part of the Atmospheric Radiation Measurement (ARM) mobile facility (Shupe et al., 2021)." (line 122).

3. Line 184: The sentence seems to awkwardly break with the phrase "..., within the ABL, ..." Since you already describe the criteria for dθv/dz to be near the surface, I think the qualifier "within the ABL" can be omitted here.

The authors agree that this sentence was phrased poorly. We have restructured the sentence to now read:

"Since the stability criteria in part depend on stability within the ABL and some observations have an ABL height as low as 50 m, we first include a measurement of $d\theta_v/dz$ at 42.5 m (this determines the near-surface stability), calculated across a 15 m interval between 35 m (lowest point of the profile) and 50 m." (line 207)

4. Section 2.3: I think this paper would strongly benefit from the inclusion of example profiles from some or all of the stability regimes outlined in this section and in Table 2. These could be either synthetic data with linear profiles in each altitude range considered, or they could also be an example profile from real data that exemplify the criteria for each regime. Because there are not too many figures already, please consider including an additional figure to go along with this section, as I think this will help readers more firmly grasp the physical arguments discussed throughout. The example profiles can also be color-coded to match the same color scheme used throughout this paper for consistency.

Thank you for this very nice suggestion. The authors now include a figure in Sect. 2.3 (Fig. 2 of the revised manuscript) which shows an example profile for each stability regime from the radiosonde data, aside from NN, as there are no purely NN cases in the observations. The example profiles show both the $d\theta_v/dz$ and θ_v anomaly profiles for each example (line 264). We also have added some text introducing the new figure:

"All of the resulting options for stability regime are listed in Table 2, and an example case for each regime (except NN) is shown in Fig. 2. The color-coding in Table 2 will be used to discern each regime henceforth. While we list NN as a stability regime option, a purely NN case without enhanced stability aloft was never observed in a MOSAiC radiosonde profile, and as such no NN example is given in Fig. 2." (line 251).

5. Section 2.3: In general, I think it would be useful to contextualize the stability regime criteria with others in the literature based on parameters such as the Richardson number or a layer-specific lapse rate (see, for example, Sorbjan, 2010; Sorbjan and Grachev, 2010; Pithan et al., 2014). Additionally, I think it would be interesting to consider the joint distributions of surface net radiation and bulk ABL lapse rates for quasi-direct comparison with those by Pithan et al. (2014) using data from the SHEBA campaign.

We have added some text in the beginning of Sect. 2.3 to explain how the definition of stability regime in the current manuscript expands upon that used in prior literature:

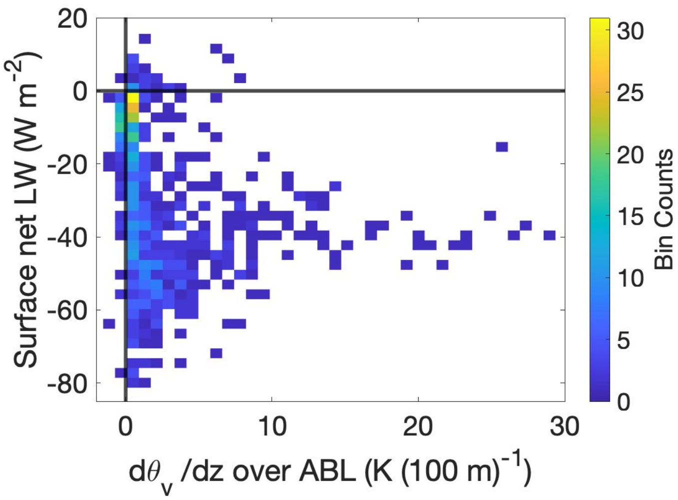
"By defining twelve distinct stability regimes, we expand upon the traditional categorization of stability into one of three categories: stable, neutral, and unstable (Stull, 1988; Liu and Liang, 2010). While some prior studies have separated the stable regime into a few subcategories for the Arctic (weakly stable, very stable, and extremely stable; Sorbjan, 2010; Sorbjan and Grachev, 2010), our analysis expands upon this through the inclusion of additional subcategories for stability above the ABL." (line 192)

We also added some text toward the end of Sect. 2.3 which mentions the methods of stability regime identification used in previous literature (including Sorbjan (2010;), Sorbjan and Grachev (2010), and Pithan et al. (2014)), to contrast with the methods of the current paper, and in the end explain why the current methods were chosen:

"While other studies define stability in the Arctic based on Ri_b and local Obukhov length (Sorbjan, 2010; Sorbjan and Grachev, 2010), or based on temperature lapse rate (Pithan et al., 2014), we found the above methods for defining stability regime based on $d\theta_v/dz$ and ABL height to yield reliable results while providing the best potential for repeatability in future work (e.g., Dice et al., submitted), as the methods rely only on standard radiosonde observations (and do not require additional measurements). This also allows us to apply the same methods to both the near-surface and aloft stabilities. Additionally, as the focus of this study is to analyze the relationships between turbulent forcing mechanisms and stability, metrics for stability regime identification that include these forcing mechanisms in their definition (e.g., Obukhov length and Ri_b include wind speed in their calculations) were avoided. Comparison of the stability regimes determined using the methods described in this section to bulk friction velocity from the met tower (Jozef et al., 2023b) shows that the current methods discern meaningful differences in turbulence between the various stability regimes." (line 240)

Lastly, we have created a figure comparable to the bivariate pdf comparing low level atmospheric lapse rate to surface net LW radiation shown in Fig. 10 in Pithan et al. (2014). See the figure below, which is a binned scatter plot of $d\theta_v/dz$ over the depth of the ABL vs. surface net LW during MOSAiC. A similar bimodal distribution is seen in the MOSAiC data as in Pithan et al. (2014), in which there is a cluster of cases with surface net LW less than -25 W/m2 (the clear sky state) and surface net LW greater than -25 W/m2 (the cloudy state), where stability decreases (indicated by a decreasing $d\theta_v/dz$) with greater longwave cooling (lower surface net LW values). This is the same result as was presented in Pithan et al. (2014). It was a nice suggestion to add this analysis. Thus, we have added some discussion in the text to indicate that the MOSAiC data shows the same results as Fig. 10 in Pithan et al. (2014).

"Comparison of surface net longwave radiation to ABL stability reveals that there is a bimodal distribution with weaker stability more often occurring in the cloudy sky mode (surface let longwave greater than -25 W m2) and stronger stability more often occurring in the clear sky mode (surface let longwave less than -25 W m2). Further, within the clear sky mode, stronger stability corresponds to weaker longwave cooling. These results agree with Pithan et al. (2014) which revealed these conclusions using data from the SHEBA project." (line 437)



We additionally include another statement where the results of the current paper agree with the results from Pithan et al. (2014):

"This again agrees with the results of Pithan et al. (2014) which showed that stability is stronger in the Arctic clear sky state." (line 510).

6. Table 2: In the first column header, it seems Dθv/dz should rather read as dθv/dz for notation consistency.

Thank you for pointing this out. The authors had missed this auto capitalization of the table heading and only noticed after the manuscript had been submitted. It has now been corrected to $d\theta_v/dz$ (line 256).

7. Table 2 and throughout: I appreciate the consistent use of the color scheme throughout the paper for classifying each stability regime. However, please consider using a colorblind-friendly alternative to the red/green/orange base palette utilized throughout the paper.

We have changed the color scheme to be more color-blind friendly. While it is impossible to find 12 colors that look distinct for all possible colorblindness options, we do at least now separate out the colors that look similar to colorblind people so that they don't appear next to each other. Additionally, in order to accommodate for colorblindness, we always include a legend that lists the stability regimes in the same order as they appear on the figure, so one could always determine which stability regime they are looking at based on its location among all stability regime options.

8. Lines 316–338: The logic in using pressure as a proxy for synoptic setup seems reasonable to me, but would pressure tendency ∂p/∂t be a more useful proxy for the onset of storm systems in this case? The sign may also indicate whether a storm is approaching or receding, so if this is too granular for the purposes of this study, maybe even just the magnitude of the pressure tendency could be useful. Please discuss.

We have added a panel to the figure showing 2 m wind speed and pressure depending on stability regime and season (Figure 5 in the updated manuscript, line 401) which shows the absolute pressure tendency (dp/dt) calculated as the change in hPa over the 3 hours preceding each observation, as well as corresponding significance testing in the supplementary figures (Supplementary Figure S2 in the updated manuscript). We found similar trends in dp/dt as were found in the 2 m pressure, in that dp/dt is greater for the weaker stability regimes, further supporting the theory that synoptic scale storm systems contribute to the higher wind speed events that correspond to the weaker stability regimes. We have added some discussion on the dp/dt results throughout Sect. 3.3:

"Figure 5f-j shows the range of 2 m pressure and Fig. 5k-o shows the range of absolute 2 m pressure tendency (dp/dt) corresponding to each radiosonde launch for each stability regime and season (refer to Supplementary Fig.

S2 for corresponding significance testing), where the annual mean of 2 m pressure and dp/dt throughout MOSAiC were 1010.8 hPa and 0.77 hPa (3 hr)$^{-1}$ respectively. Annually, the pressure results mimic what was seen with 2 m wind speed, in that lower pressure and greater dp/dt (suggestive of a stormy setting with faster wind speeds) is correlated with weaker stability, with the most drastic reduction in pressure and increase in dp/dt values being between SS, MS and the VSM regimes (pressure largely above average and dp/dt largely below average) and the WS and NN regimes (pressure largely below average and dp/dt largely above average; difference in means of 6.6 hPa and 0.31 hPa (3 hr)$^{-1}$ respectively). This is supported by Supplementary Fig. S2a which shows a high level of significance when comparing 2 m pressure and dp/dt between different stability regimes." (line 375).

"Differences in 2 m dp/dt between stability regimes in winter are not as great as annually or in fall or spring (Fig. 5m and S2c) suggesting more slowly evolving low and high pressure systems in winter than in other seasons. The smallest differences in 2 m pressure and dp/dt between stability regimes occurred in summer (Fig. 5j and S2e), again echoing the results from the 2 m wind speed, and further supporting the statement that the presence of storms, and resulting wind shear, are not the most important drivers of ABL stability in summer." (line 394).

The authors did not want to replace the 2 m pressure results with the dp/dt results, as dp/dt only tells us about the onset or diminishment of a storm, but can miss periods during a storm when the low pressure persists. Thus, by including both 2 m pressure and dp/dt, we get the full story, and our argument is strengthened.

9. Line 364: When stating that the "...interquartile ranges of net radiation for ... regimes exceeded zero," to my understanding this means the 75th percentiles exceed zero. Am I correct in this reasoning? Please clarify.

You are correct in this reasoning. To clarify this, we now state:
"… the 75th percentiles (upper limit of the interquartile range) of net radiation… exceed zero…" (line 427)

10. Line 385: Please remove the "a" so the first full sentence reads "This all suggests that longwave radiation is more coupled to ABL stability..."

Thank you for pointing out this error. The "a" has been removed so the sentence now reads:

"This all suggests that longwave radiation is more coupled to ABL stability throughout the span of the year than shortwave radiation." (line 454)

11. Lines 552–556: As discussed previously in the paper, the causal relationship between surface net longwave radiation and stability within and above the ABL is difficult to determine in a bulk statistical sense such as that presented here. With the given dataset, is it possible to determine a distribution of, e.g., $\partial\theta v/\partial z$ at cloud base height as a function of stability class or surface net longwave radiation? This may provide additional context in the role that clouds play in destabilizing the lower atmosphere. This analysis is not critical to include, but at the very least I think an additional discussion similar to that provided at lines 281–286 is warranted here in the summary and conclusions section.

We have added some text in the summary and conclusion section stating that:

"While we discuss the results of this analysis with the assumption that stability occurs as a response to wind and radiation features, we recognize the possibility that wind and radiation features can also occur as a response to stability, and further work is needed to fully understand the complex relationships between stability and the turbulent processes addressed in this paper." (line 649)

Analysis of $d\theta_v/dz$ at cloud base height as a function of stability class or surface net longwave radiation is an interesting idea, but (as you also mentioned) the authors believe this to be outside of the scope of the current paper, as one could spend a whole paper exploring in more detail the interactions between clouds, radiation, and stability, whereas this paper is intended to give a more broad perspective on the subject. It would be useful for a future study to isolate only cloudy cases and perform the analysis you have suggested. We have added some text to the summary and conclusions section stating that the results of the current study only scratch the surface of the relationship between clouds and stability, and have suggested some areas for future research:

"While this study provides a high-level perspective on the interaction between clouds and stability, further research is needed to fully understand the complexities of the relationship. For example, future work could repeat the current study for cloudy versus clear sky conditions, examine the effects of multiple cloud layers, or analyze potential temperature gradients at cloud base height and within a cloud layer as a function of stability or surface net longwave radiation." (line 632).

References

- ***Pithan, F., B. Medeiros, and T. Mauritsen, 2014: Mixed-phase clouds cause climate model biases in Arctic wintertime temperature inversions. Climate Dynamics, 43 (1), 289–303, https://doi.org/10.1007/s00382-013-1964-9.***
- ***Sorbjan, Z., 2010: Gradient-based scales and similarity laws in the stable boundary layer. Quarterly Journal of the Royal Meteorological Society, 136 (650), 1243–1254, https://doi.org/10.1002/qj.638.***
- ***Sorbjan, Z., and A. A. Grachev, 2010: An Evaluation of the Flux–Gradient Relationship in the Stable Boundary Layer. Boundary-Layer Meteorology, 135 (3), 385–405, https://doi.org/10.1007/s10546-010-9482-3.***