Reviewer comments for: Thermodynamic and Kinematic Drivers of Atmospheric Boundary Layer Stability in the Central Arctic during MOSAiC

Gina C. Jozef et al.

Atmospheric Chemistry and Physics

Recommendation: Minor Revisions

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.

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."

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

3. Line 184: The sentence seems to awkwardly break with the phrase "..., within the ABL, ..." Since you already describe the criteria for $\mathrm{d}\theta_v/\mathrm{d}z$ to be near the surface, I think the qualifier "within the ABL" can be omitted here.

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.

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.

6. Table 2: In the first column header, it seems $\mathrm{D}\theta_v/\mathrm{d}z$ should rather read as $\mathrm{d}\theta_v/\mathrm{d}z$ for notation consistency.

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.

8. Lines 316–338: The logic in using pressure as a proxy for synoptic setup seems reasonable to me, but would pressure tendency $\partial p/\partial t$ 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.

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

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..."

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.

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.