Review on

# An Overview of the Vertical Structure of the Atmospheric Boundary Layer in the Central Arctic during MOSAiC

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This manuscript describes a statistical analysis for a year-round period of Arctic boundary layer observations based largely on radiosondes during the MOSAiC project accompanied by data observed by the DataHawk2 unmanned vehicle. As a central tool, a "self-organizing map" approach was applied to the data. There is no doubt that such a statistical analysis is extremely helpful in addition to all the case studies that have been and are being evaluated. Therefore, the enormous amount of work is greatly appreciated, and after major improvements to the manuscript, I also support the publication of this analysis. However, the manuscript needs a thorough revision that goes far beyond classical "major revisions". I will try to justify this in detail below.

__My main concerns are:__

The manuscript is extremely difficult to read and understand; this is largely due to the very intensive use of abbreviations. Especially when referring to the different stability regimes of the boundary layer, the abbreviations are not really intuitive and hardly anyone will be able to remember them while reading (see at line 428/29 for example when a sentence almost completely is based on abbreviations). In particular – see Tab 2- the capital “S” is sometimes used for “shallow” and sometimes for “stable” – I have no good suggestion at the moment to improve this, but please consider of a better and simpler way to categorize the different regimes and avoiding abbreviations.

Very often it is concluded in the paper that the analysis yields unsurprising results or that the results are logically and physically explainable - well, I expect that with the correlations but do you want to evaluate the tool or deliver new scientific findings?

It is a bit tiring for the reader to have each figure described in such detail (and the figures contain a large amount of detail...), and you should try to find a slightly better and more compact way of presenting and introducing the figures. I know this comment is quite generic but maybe you find a good way to describe your figures in a more comact way.

When I first read the manuscript and got to Figure 2, I was completely overwhelmed. Why do you need 30 schemes to describe the ABL? With many patterns you only see marginal differences when you look very closely. I think that the manuscript could be made much simpler and more readable if the analysis was limited to a handful of characteristic patterns.

A general note on writing style: please try to avoid repetition to strengthen the manuscript. Furthermore, the sentences are often so complicated and convoluted that a fluent reading - at least for me - was very difficult or impossible. I myself am not a native speaker, but there are enough competent co-authors who can edit the manuscript thoroughly.
General comment about most of the figures (although here I refer explicitly to Fig. 3):

The figure is based on 30 subplots which are by definition quite small but if you try to include even more information in terms of several additional numbers and vertical or horizontal lines, the plots will get really crowded. Even worth in Fig 4 where I am not able to read at all the numbers you included into the subplots – they are simply too small and too many.

I am not convinced that discussing material in the manuscript that has been moved to a supplement is the correct or formal way to do it. If you have to many figures, you should solve this problem differently.

About the analysis of temperature inversions: I am a little bit skeptical about this analysis and I wonder of how much of these results are based on self-correlation because the definition of the stability regimes is also based on temperature gradients. You mentioned this issue briefly but this needs to be discussed in more detail.

Maybe one solution for an improved structure of the entire manuscript would be a stricter separation between explaining the results in one section and discussion and interpretation in another one.

I am not convinced about the meaningful interpretation of parameters averaged over the entire MOSAiC cruise. For example, what can I learn from a statement such as "*The average ABL height during MOSAiC was 150 m, and ABL height increases with decreasing stability.* " (line 703)? You average ABL height over completely different ABL regimes and the second part of the sentence is somewhat trivial and expected – right?

**<u>More specific comments:</u>**

Introduction (line 35): what do you exactly mean with high temporal and spatial resolution – please specify.

Line 39ff: quite generic comment; please provide references - I think it is quite obvious that the Arctic ABL is not necessarily stably stratified in summer - right?

Line 49ff: I assume that - depending on the temperature stratification - the turbulence maybe also increased above the LLJ core because the shear could be similar - right? Furthermore, why should a LLJ weaken stability? The main preconditions for the existence of a LLJ is an almost vanishing turbulent transfer coefficient - typical for stable regimes.

Line 101: although you provide an explanation later on, I think a short introduction of what a
"self-organizing map" is should be included - simply because you mention it and I think a few words about this technique is essential already at this point and not all readers are knowing about this?!

Methods:

Line 116 ff: is this information about RV Polarstern movement is of interest for your work here – why do you mention this?

About Tab 1:

- What are the sources for the uncertainties? The Vaisala manual? Please provide a reference.
- Furthermore, I have serious concerns about the given uncertainty of wind observations: I know that this is the value given in the specifications by Vaisala but there is a lot of discussion about errors in determining the wind velocity - in particular at high latitudes where GPS comes to its end.
- Why do you mention uncertainties above 16 km here?
- I don't understand why a sonic is not enough to estimate the friction velocity?

Line 165ff: How can you expect a slope for higher altitudes just based on the lowermost 10 m? Why not simply compare the highest measurement point of the tower with the lowermost observation level of the radiosonde - I assume 12 m (helideck) and 10 m (top of the mast) should compare quite well? If not, you have a problem with the radiosonde – right? Or did I completely misunderstand your approach? – Simply double check the wording.

Line 168: interpolated => extrapolated? Please check.

Line 172: I thought a low-pass filter removed this pendulum motion? Please comment on this.

Line 185: I am somewhat surprised by the high critical Ri value which is two-times higher compared to the “classical” value. Most values published in literature are below 0.25 – do you have an explanation for this?

Line 186: I think you can shorten this part a little bit by citing your paper only one times

Line 189: just to understand it correctly: the gradient is a mean gradient from 35 m to ABL top - right?

Line 225: What do you exactly mean with "theta anomaly profile"? And in which way is one approach "better" than the other one? - please specify.

Line 230 ff: What details are "better" when using 30 patterns instead of 20 or 35? What did I learn from this detail?

Line 228ff: I understand that you want to explain details about SOM in a specific part of the paper but you mentioned SOM several time before your explanation - maybe you should at least mention at the beginning what SOM stands for and refer to this point here. I feel that many readers have never heard about SOM before and at least a brief introduction at the beginning could help – or did I have overseen this?!

Line 244ff: Maybe at this point a comment about the low-pas filtering of GRUAN data is useful and how it effects your data and evaluation?! Or why using 5 m as a grid spacing when the low-pass filtering is at 75 m or so? (see also the comment by Günther Heinemann)

About Fig 2.: Maybe I missed it but why do I need 30 patterns to describe typical ABL stratifications? For example, what is the difference between pattern 27 and 28? By eye there is no difference. A technical comment on Fig 2: the pattern number and the number of observations is in the same font and partly not well visible - maybe you could provide a color background for the two set of numbers?

Line 259ff: when reading this part, I immediately ask myself if the DH2 observations have a chance to cover all the different patterns because it didn't fly in the Polar night so it should miss the real stable conditions- right?

Line 267ff: Why is a SOM based on anomalies more visual? If you anticipate the result here, I immediately wonder why you used the gradient first and did not start with the analysis of the anomaly right away – I am confused here…

Line 279ff: I partly understand the motivation to define so many different stability regimes, but I fear that the usefulness for most readers is very limited. These 12 regimes are linked in the manuscript with 12 abbreviations that I definitely cannot remember and when these are mentioned and discussed in the text, I as a reader jump back and forth to remember the abbreviations. This disrupts the flow of reading, at least for now, whether I can do much with the information or not.

Line 285: A possible solution for a better reading flow could be to distinguish even more clearly between methods and observations - this is only one possibility but in some places these two aspects blur a bit.

line 286ff: Why Antarctica? I think MOSAiC should really be sufficient and citing a non-published paper from the other side of the world does not really help here…

line 290ff: Why is the gradient in 42.5 m representative for the AGL? I understand that this value might be representative for the surface layer (at least in summer) but the entire AGL - or do I misunderstand? Please clarify.

Line 325ff: Why are you defining possible regimes that were never observed in the data from MOSAiC? Maybe you have some good reasons but just reading this sentence confuses me.

Fig 3.: This figure (Fig. 3) contains a lot of information, and I suspect that most readers will have difficulty understanding all the lines and what they mean. Perhaps there is a way to make the diagrams a little clearer.

The colored frame lines describing the regimes should be somewhat thicker to better distinguish the different regimes

Also, at this point I wonder how the ABL height is defined! For example, in pattern 8 it is quite difficult to estimate an AGL height even by eye. I assume that the inversion and the entrainment layer are not part of the AGL height according to your definition, right? Is then the term "mixed layer" more appropriate compared to AGL height? You should at least define the phrases carefully at a prominent place.

Line 358ff: why "perhaps" - you should have the data to evaluate this "unique processes"!

Line 367: I am not convinced that the Arctic ABL is "always" stably stratified" - in particular in summer this is definitively not the case (see Tjernström et al.) So, I think to sell this as a "new finding" is going too far.

Fig 4: most of the numbers are more or less invisible, at least the numbers in the upper line. Furthermore, black labels on a dark background are quite challenging. I think you should find a much better way to illustrate your point here

Line 377: Again, I am not in favor of discussing material that is not in the manuscript but has been moved to a supplement. Regardless, I don't quite understand the following statement; DH2 couldn't fly in clouds - right? How then can DH2 observations cover all patterns when clouds affect stratification so much - or have I misunderstood something?

Line 383ff: Do I understand correctly that LLJs are to be analyzed based on mean values within one individual pattern? As you have already noticed, this must - at least partially - average out the LLJ's with having their wind maximum at different heights - right? Then I have serious doubts whether this is the right method and whether one can make statements about LLJs in this way. However, it might be a misunderstanding on my side but then please clarify and consider rephrasing. In addition, there are from my point of view quite valid remarks of Günther Heinemann about the quality and (in)accuracy of wind measurements in the low altitude and the low-pass filtering of the wind data. Please take these comments seriously.

Line 388: Physically, I don't understand this point: a LLJ is by definition linked to a more stable ABL because one precondition for a LLJ is the almost vanishing turbulent exchange coefficient - right? So how can you say that "Thus the LLJ is more closely coupled to the ABL in the weak stability cases…" Maybe I don't understand your point here but then you should clarify it.

Fig. 5: Again, too much information and details in the plots.

Line 415 ff: The is probably one of the most prominent places to say: I am lost in all the details and even more I lost track due to the huge number of abbreviations….

line 454: If a parameter x changes over the depth of the ABL it is not the same as the gradient dx/dz – right?

Line 455ff: If you cite a figure within the main text, it should be included in the main text - why has it been shifted to a supplement?

line 463: "...the fact that we see this drastic increase also supports the choice of this threshold...." can you please explain this in a little bit more detail?

line 465ff: I cannot follow this sentence at all - please double check and consider rephrasing.

line 468ff: It is quite unusual to start a sentence with a equation - I would avoid it. Furthermore: "shear" => "wind shear".

So why do you mention Ri with index "b" (for bulk?!) here? If you mention the (local?) gradients then you have the basics for the classical local Ri definition - right?

line 475ff: again, a reference outside the paper is not helpful and I suggest to avoiding this. Furthermore, I do not understand your conclusion about the physically meaningful definition of the regimes - please specify what you mean here.

line 477: what is exactly meant by "dV/dz result" - from my point of view this makes no sense

line 479ff: It is surprising that you start the discussion with the Richardson number and now you move on to the friction velocity - so why? Furthermore, you correctly mentioned that Ri describes more the tendency of turbulence development but it is not a measure of the degree of turbulence or the intensity but you concluded already that the near-surface atmosphere is always turbulent. From my point of view this is going too far. This part also needs some careful reconsiderations and not only rephrasing.

line 487: Well, turbulence itself might describe a flow but probably not the Arctic - this makes no sense. Maybe I missed it but I suggest to define u_star at the place of first occurrence (around line 145 or so). Also, I suggest to use the word „increased" turbulence.

488ff: this not really surprising and I think this statement does not need a supplementary figure - right?

492ff: I am not surprised that u_star and and wind shear do not show a clear dependency because it is a Richardson number problem - but is this a conclusion as you mentioned?

line 495ff: I cannot follow your argumentation, in particular the last part "...when stability aloft is greater." So, is there a connection between stability at higher altitudes and surface layer mixing? Please explain what you mean here.

Fig 7 (and maybe other figures as well): you put the labels of the y-axis on the x-axis which is formally not correct and took me a time to understand the plot - please change this labeling.

line 511ff: Maybe I am wrong and I don't have the details of Banta et al in mind but how can a LLJ exists (or develop) in a well-mixed ABL? Maybe during the transition to a classical Ekman-layer like ABL this makes sense but from a theoretical point of view a LLJ and a well-mixed layer are exclusive; the turbulent exchange coefficient K_m has tend to zero to decouple the LLJ region from the surface - right? In fact, that is the background for your comment in line 513ff but I think this is a precondition for the development of a LLJ....

line 515ff: maybe you mentioned it earlier but is the "LLJ speed" defined as the maximum speed in the LLJ core or the difference to the surrounding?

line 548ff: I think this is physically not really meaningful, maybe it should read as: "...it needs more wind shear in a more stable environment to create mixing ..." or similar. Furthermore, I think the phrase "hypotheses" is going a little bit too far because you never formulated a hypothesis which can now be verified or falsified...

line 555: what do you exactly mean with "excess turbulence"?

Fig 8.: See my comment on Fig 7 about the axis labels

Line 567ff: About your supplement: it just includes many further figures which are partly mentioned within the main manuscript but not explained and deeply discussed. I think this is not the right way to use a supplement because the main manuscript should be readable by itself without reading the supplement. If you have a distinct and interesting topic which might be useful for some readers but distract from the red line of the manuscript a supplement might be the right choice but if you have simply too many figures which you want to mention in the manuscript you cannot just move them into the supplement and refer to them (different to an appendix).

line 577ff: Do you consider TIs as a cause or effect of ABL development?

Line 578ff: About the TI analysis: I am a little bit skeptical about this analysis because I wonder how much of this analysis is based at least partly on self-correlation because the stability regimes are based on the temperature gradients - right? Maybe this should be discussed at least a little bit before interpreting the results.

Line 581 and general: Maybe better distinguish between explaining figures and results, than interpreting them and finally compare with other studies - the structure is often a bit confusing and you jump back and force

line 591ff: I cannot follow your argumentation about the potential for exchange of momentum when TI is well above ABL height - what do you mean? I have the feeling that here is a lot of speculation on play but a careful physical interpretation is missing.

line 603: As mentioned earlier, I have the feeling that this part is based on a big portion of self-correlation which has to be ruled out before the interpretation.

line 621: why using a decimal value for the second cloud base height? Why 6.1 km and not 6 km??

line 633ff: What do you mean with the statement that "… a regime is driven by the radiative signature of clouds..."? and further on in line 634 what are those other mechanisms? This explanation is hardly to follow and needs some careful rephrasing.

The summary will certainly need to be completely revised when the previous analyses have been appropriately re-sorted and revised - so I have refrained from detailed comments on this chapter now.