Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., author comment AC1
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Reply on RC1

Gina Jozef et al.

Author comment on "Testing the efficacy of atmospheric boundary layer height detection algorithms using uncrewed aircraft system data from MOSAiC" by Gina Jozef et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-383-AC1, 2022

The authors thank anonymous referee 1 for taking the time to review our manuscript and for their helpful comments, which have improved 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.

A novel dataset of detailed atmospheric profiles gathered by a UAS in the Arctic region is being explored to determine how to derive the height of the atmospheric boundary layer using automatic methods. "Subjective", visual height detection is used as a reference standard to evaluate a number of "objective" methods available in the literature. While the presented work is promising, highlighting the difficulties of accurate layer height detection in the Arctic region, the manuscript requires major revisions to better demonstrate the research results.

Overall, the text is very descriptive and could be made more concise in many sections. The actual research findings need to be pointed out more clearly and should be placed into context to the literature. It is important to highlight how the manuscript provides novel insights and methodological advances. This could be partly improved by removing the severe imbalance between the methods description (Section 2 has 390 lines + 8 figures) compared to the results section (section3 has 150 lines + 2 figures). The focus of the presented figures should be placed on the research findings rather than the introduction of the methods that are in most parts explained in the literature, i.e. the current manuscript does not present a novel method.

Thank you for your comments. The authors recognize and agree with your comment that there was an imbalance between the length of the methods and results sections. To improve this, we now better highlight how the manuscript provides novel insights and methodological advances, while spending less space on discussing aspects of previously published methods that were unchanged. When describing the subjective methods, we now include the details in a table (Table 3 of the revised manuscript beginning on line 291), which condenses the information and makes it more readily available. When describing the objective methods, we now provide just a brief description of each (lines 315-366) and leave it to the reader to review the original citation for more details about

these methods, such as the guiding equations. We also moved Figures 3-6 from the original manuscript, which provided examples of the application of each of the previously published method, to the Supplementary Figures document (they are now Supplementary Figures S1-S4). We have also revised Figure 7 from the original manuscript (which is now Figure 3 of the revised manuscript, on line 403) to show two examples (a SBL and NBL) which demonstrate the ABL heights determined by all of the subjective and objective methods used in this manuscript.

The changes described above have greatly reduced the length of the methods section and thus better balance the methods to results section lengths. We have also added some material to the results section, most notable that we now briefly discuss the results of the scatter and bar plots in the context of each stability regime, and provide similar plots with stable and neutral cases separated out in the Supplementary Figures. We also discuss the sensitivity of the efficacy of the objective methods to the vertical resolution of the data. We hope that these changes address your concerns.

Minor comments:

At times, a more precise wording could help make the text less descriptive. E.g. use established terms such as "vertical gradient" instead of "change with height".

The authors have changed the text throughout the manuscript to use the term "vertical gradient" when referring to change with altitude (for example, line 229-230, 238, 260, etc.). Other changes made along these lines include making sure to use active rather than passive voice, changing 'ABL height' to 'Z_{ABL}' (introduced on line 81), changing 'Rib method with critical value of 0.5' to 'Rib(0.5)' (introduced on line 365), changing phrases like 'ABL height of an SBL' to 'SBL height' (for example, line 327), and specifying local maximum and minimum when these terms are relevant (for example, line 351). If there is additional wording you were intending for us to change, please specify.

Section2: Maybe the criteria for the subjective height detection could be summarised in a table? What is the expected uncertainty in these methods based on visual assessment of at times very subtle signatures in the profiles?

We now summarize the subjective ABL height detection criteria in a table (beginning on line 291), rather than writing it out in paragraph form. The primary uncertainty is not due to the exact height of the kinks, as the uncertainty here is subject only to the vertical averaging procedure and response time of the sensors, and thus on the order of ~1 m. Instead, primary uncertainty is due to whether or not the feature we are using to define ABL height is representing the top of the ABL. To address this, we comment in lines 298-304 that 90% of cases had a fairly clear ABL depth, and only 10% had ambiguous depths (for example if the height of the theta_v and humidity kinks that could both be interpreted as ABL height are at a different altitude). In these cases, we state that depending on which kink was chosen, ABL height could vary by 10-30 m.

Line 54: Please provide a short explanation on the concept of "radiative mixing forced by cloud cover".

We have changed the wording to "turbulent mixing below cloud base due to cloud top radiative cooling," which can be found beginning on line 55. We hope this is a sufficient explanation to eliminate confusion.

Line 64: maybe reword. The literature on ABL height detection is obviously very extensive so it would be good to clearly state that Table 1 lists a few examples of relevant publications and atmospheric variable.

The authors now clarify that we list only some of the atmospheric variables used for ABL height identification (line 86), and only some of the publications which reference them (line 87). We also clarify that we list only thermodynamic and kinematic processes, as these are the focus of the paper, and are what is available from the DH2 data.

Line 71: which humidity variable is analysed here?

The authors now clarify in parentheses that we use relative and absolute humidity (line 98).

Line 73: You state the entrainment zone is located "above" the ABL. Maybe a few words on the relation between ABL height and entrainment zone characteristics would be useful.

The authors add a few words on lines 69-70 describing that the entrainment zone is a stable layer between the ABL and free atmosphere, but we were unsure what relationship between ABL height and entrainment zone characteristics you are referring to, so we were unable to fully address this comment. However, the DH2 data would be a great resource to conduct such a study in the future, so we mention this when we discuss future work in the conclusion (line 660-661).

Line 132: what is meant by "assess the ice alongside the Polarstern"

The wording in the manuscript is "*access* the ice alongside the Polarstern," rather than "*assess*." We assume the confusion comes from misreading this sentence. This text refers to the ability of scientists onboard the Polarstern to be able to exit the ship and go onto the sea ice.

Line 259: the term "mixed layer" has not yet been mentioned before. Explain why you are using it now for NBL?

To avoid confusion with using different terms, the authors change "mixed layer" to "well-mixed ABL" (line 341) which is characteristic of an NBL or CBL.

Line 262-267: These sentences are very descriptive. Please condense the key information and try to generalise.

The authors have removed this paragraph, as we now present this information in a table (line 291) rather than paragraphs, based on your earlier comment. Thus, the information is now presented in a more generalized manor, with only key information provided.

Line 270: again, try to be less descriptive. E.g. the term "increase with altitude" could be replaced by "vertical gradient"

While this specific sentence no longer exists due to the presentation of this information in table form, throughout the text we now say "vertical gradient" instead of "increase with altitude." This is exemplified in Table 3 of the revised manuscript beginning on line 291.

Line 278: "… extends from the surface to …".

Due to presentation of the subjective methods in table form, we removed this sentence, and this comment is no longer applicable.

Line 284: replace by "change in vertical gradient"

Due to presentation of the subjective methods in table form, we removed this sentence.

Throughout the table, we still use the word 'kink' to describe a change in vertical gradient for the sake of conciseness, but in the paragraph before Table 3, we specify that when we say 'kink' we refer to dramatic shift in slope, i.e., drastic change in vertical gradient (line 288).

Line 340: what about the methods mentioned e.g. by Collaud Coen et al. (2014)?

Are you referring to the sentence beginning on line 341 of the original manuscript: "There is no recognized equation to determine SBL top height accurately without observations supporting the derivation of turbulent kinetic energy profiles"? We have moved discussion of the TKE method to the paragraph after Table 1 (line 106-108), where we mention that though TKE is perhaps the most valuable profile for ABL height identification, these data are not available to aid in the current study. We also now include methods used in Collaud Coen et al. (2014) in Table 1. However, the methods for SBL height identification discussed in Collaud Coen et al. (2014), including identifying ABL at the top of the temperature inversion or where dtheta/dz = 0, don't perform well for an Arctic atmosphere where a weak temperature or theta_v inversion can often extend deep into the profile, well above the ABL. We have added a sentence to the paragraph below Table 1 (lines 104-106) where we discuss this as an explanation as to why we do not use these methods in the current study.

Line 452: Explain how the data acquisition platform (radiosonde vs UAS) or the geographic location (mid-latitude vs arctic) are expected to influence the performance of the detection methods and hence warrant the outlined adaptations.

We have added some text on lines 372-375 to discuss these concerns. For the data acquisition platform, we explain that the difference is that the DH2 samples at a higher resolution. For the geographic location, we explain how the Arctic ABL structure is often quite different than that observed in the mid-latitudes, primarily due to the lack of daytime convection or a diurnal cycle most of the time in the Arctic, which means there are no deep unstable layers, or residual nighttime layers above the SBL in the Arctic. In addition, the Arctic ABL is also often much shallower than in the mid-latitudes and this required some adjustment of fixed height criteria in previously published objective methods. This information has also been added to the introduction in lines 68-73 where we discuss the inherent difference between the Arctic ABL and that in mid-latitudes.

Line 462: what causes this warm bias in the lowest levels?

This is due to the *Polarstern* acting as a heat source. A explanation of this has been added to the text on line 389.

Line 502-511: Shorten introduction for interpretation of linear regression. It can be assumed that the reader of this scientific publication is familiar with this common approach.

This explanation has been shortened. Now, we simply say "The R^2 value demonstrates how much of the variation in objective Z_{ABL} can be explained by the difference in subjective Z_{ABL}" (line 442-443).

Figure 9: list number of samples.

The number of samples for each scatter plot has been added to the caption of Figure 9 from the original manuscript (Figure 5 of the revised manuscript). This can be found in lines 484-490.

Line 550: Careful with such statements. Very few samples with DH2 results above 150m

The authors have added a statement to suggest that this increased variation for shallower ABL height may be due to the greater number of samples of low ABL (lines 480-481).

Lines 512-552: So what is the interpretation of these results? How do they compare to the expectations in context of the literature?

The overall interpretation of these results is discussed after Figure 10 of the original manuscript (Figure 6 of the revised manuscript), but we have provided some additional context to this, including how the results compare to the literature. Primarily, we point out that previous literature also favors the Rib method when mechanically-produced turbulence dominates, as is true in the central Arctic (lines 539-541).

Lines 599: discuss relation of LLJ and ABL in the study area. How is it assessed when the LLJ is located above the ABL? How does this relate to the expectations and literature?

We now clarify that the altitude of the LLJ core is determined through the application of the SBL Liu-Liang method, which is usually located above the subjective ABL top (line 567-568). We also add a comment that this agrees with what has been found in previous literature and include some citations. Several of these citations specifically state that the LLJ core is typically at or above the ABL top (line 568), and one of these citations states that using LLJ core height to define SBL top produces results inconsistent with those from other methods (line 568-570). Thus, our results are in agreement with previous literature that using the LLJ core height to determine ABL top is often inappropriate.