Author comments to reviewer 1

We appreciate anonymous reviewer #1's feedback on our manuscript and are grateful for the time and effort for the review. Addressing the helpful review comments remarkably improved the quality of the manuscript. We respond to the individual comments below in blue color.

General Comments

Overall, the paper is very well-written and the analysis is very relevant for improving our understanding of the relationship between LLJs and the ABL in the Arctic, which is crucial for the improvement of global climate models and the enhancement of knowledge regarding the interaction between the surface and overlying atmosphere in the central Arctic.
Thank you, we really appreciate this feedback.

To improve this paper, I suggest the authors add some more description of a few aspects of the methods, some chosen terminology, and some figures, as discussed in the minor comments below.
We address the individual comments on the observational part as discussed below. However, leveraging the feedback from the other reviewer, we decided to remove the model part (former Sect. 5) from the paper entirely. Taking into account the critical points, we see that the simple model study does not add sufficient value to the observations. For a detailed explanation, please see our response to the other reviewer's comments.

Minor Comments

L13: It should be noted that often the LLJ is above the ABL, rather than always being within it. This can be particularly evident when the LLJ is decoupled from the surface in the case of a stable LLJ, and the formation mechanism is inertial oscillations.
We agree with this clarification and modified the sentence accordingly: "An LLJ , especially in conjunction with stably stratified boundary layers, is usually found in the upper region of the ABL or even partly just inside or above the inversion."

L57: Please include some uncertainties for the two sensor packages described in (i) and (ii).
We agree that a statement about accuracy and resolution of our measurements is missing in the manuscript and include the following sentences into the manuscript: "The absolute accuracy of the wind speed measurement with the Pitot-static tube is determined by the accuracy of the differential pressure sensor and is additionally inversely proportional to the wind speed itself. For typical wind speeds of 5 m/s, the absolute accuracy is only in the range of 0.5 m/s which is essentially noticeable by a zero offset and can therefore be partially corrected; however, the relative resolution is higher by a factor of 10 for the Pitot-static tube and the spectrally determined resolution of the hot-wire anemometer is in the range of 0.2 cm/s (Egerer et al., 2019)."

L60: It is mentioned that the ABL transitioned to being a "more classic stable ABL." You should include a characterization of the ABL before the transition, so it is well-understood how the ABL changed (aside from just the presence to dissipation of the LLJ).
We agree with the suggestion to describe in more detail what we understand by a classical boundary layer structure and therefore extend the sentence as follows: " ... more classically stable ABL which is characterized by the wind speed increasing mostly logarithmically from the ground with height. The turbulence is generated by the wind shear and is therefore strongest near the ground and decreases continuously with height, depending on the damping effect of the temperature inversion."

Figure 1: I am a little confused by panel (a). Please specify what StdMeteo and Hotwire refer to – are these the two sensor packages described in L52-57? You should clarify the abbreviations in the figure legend. Also, it appears the BELUGA doesn't always fly with both sensor at the same time? Please also clarify why this is and how it was determined in the field that certain profiles only had one or the other sensor in this case study.
Yes, the abbreviations refer to the instrument packages, we added that information in the Figure caption. Also, we added the following explanation in the text: "The instrument packages were deployed in a rotating sequence aboard BELUGA to ensure adherence to the payload limit; a flight overview table with deployed instruments is provided in Egerer et al. (2019b)."

L75-78: Your criteria for defining LLJs differ some from the standard literature (e.g., the requirement of the LLJ core being below 250 m, and the LLJ strength being relative to the higher value of the wind minimum above and below the core). Please add some explanation about why you divert from the more standard criteria for an LLJ. For example, include mention of any testing you might have done to determine that these methods provide valid results.
A widely used LLJ definition is the one by Andreas et al. (2000) as follows: "If the wind speed profile shows a local maximum that is 2 m/s higher than speeds both above and below it, we call the feature a jet. Notice, with this definition, the jet must be elevated and cannot occur at the surface. This definition is similar to Stull's (1988, p. 521), except he does not require the jets to be elevated." We modified the Andreas et al. (2000) criteria to exclude profiles that objectively do not resemble an LLJ; two examples are shown in Fig. 1 of this document. Our criteria are tailored to our specific case and we don't argue that they should be used universally. Reasons for modifications are:

- wind speed difference above *and* below as in Andreas et al. (2000)

- wind speed difference of 2.5 m/s instead of 2 m/s because our profiles are at a high resolution and small-scale fluctuations contribute to the wind speed difference (right example in Fig. 1).

- height below 250 m because we want to continuously study a specific, consistent LLJ at the lowest level.

We add in the text: "We tailor some of the criteria used in the literature for defining LLJs to our specific observations and the unique characteristics of this particular LLJ."

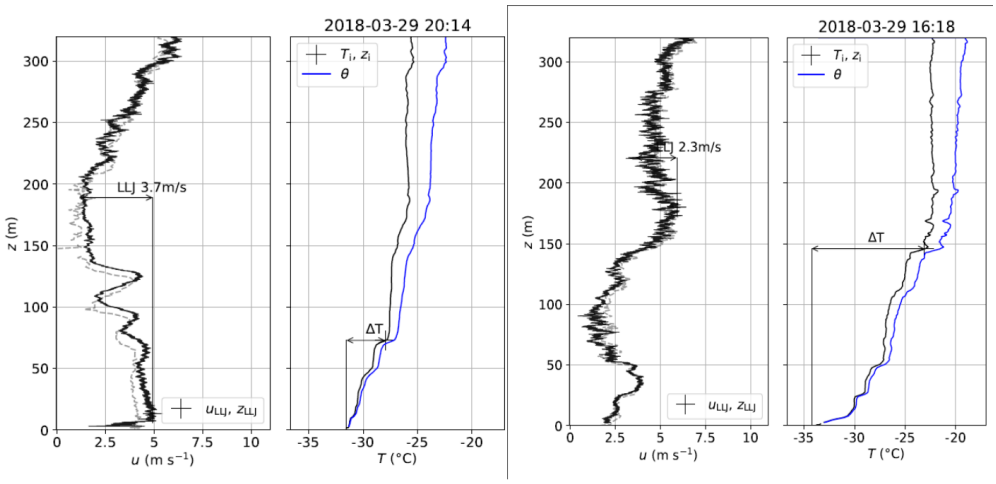

Figure 1: Example LLJ profiles that we objectively do not consider as LLJs.

L102: Add a space between 5 and s in “5s-long”
Done.

Figure 3: Rather than “the ascends and descends are from the BELUGA” perhaps say “The time-height profiles are from the BELUGA.”
Done.
Also, for consistency, it would be better to include all temperature measurements in the same units, either temperature or potential temperature, rather than a mix of both.
We argue that it makes sense to display the vertical profiles and constant-height time series in different temperature representations. For vertical profiles, the potential temperature provides the benefit that thermodynamic stratification is directly visible based on the gradient. In contrast, it makes no sense to transform temperature at a constant height to potential temperature, even if the difference is negligible at the low altitudes shown. We therefore kindly ask the reviewer to accept our explanation and leave the temperature presentation as it is.

L122: Somewhere (maybe in the Intro?) you should define what you mean when you say “standard stable ABL” for northern Greenland, as this could be different depending on location, and it shouldn’t be assumed that a reader would know what you mean. Perhaps use wording such as “<description of standard ABL>, which will hereafter be referred to as a “standard stable ABL.”
Agreed that we did not define what we mean by standard stable ABL. We added: ”By a ”standard stable ABL” we refer to a cloud-free, shallow stable ABL in which terrestrial radiation causes a surface-based temperature inversion, comparable to a nocturnal boundary layer at mid-latitudes.”

L130: By “strong surface temperature gradient” do you mean strong surface temperature inversion? Please specify.
Yes, we mean strong surface temperature inversion. We changed this in the text.

Referee’s comments to L297, L312, L314, and Fig. 10
These comments refer to Section 5 of the originally submitted manuscript, which addresses the LLJ modeling approach. Referee 2 did not find the modeling part of the paper convincing because the model is very crude, does not add value to what can be deduced from the observations themselves, and is not able to describe the LLJ conditions which are manifestly not 1-D. As the data availability does not allow the setup of a 3D model study we decided to remove this section from the revised manuscript. Nonetheless, the comments of referee 1 on this part were found absolutely correct and helpful. We are grateful!

L326: In some central Arctic locations, an LLJ is almost ubiquitous, so it is rather the more “normal” case that a stable ABL co-occurs with an LLJ. Please add some discussion (probably in the Intro) of literature which leads to the conclusion that in northeast Greenland, it is more common to not have an LLJ with a stable ABL.
We agree that for northeast Greenland, it is hard or even impossible to define a ”normal” ABL. By ”normal”, we mean the classical textbook nocturnal ABL as mentioned in comment l. 122. We change the wording in l. 326 to ”classically stable nocturnal ABL”. Additionally, we add some more LLJ characterization for the study area in the introduction.