Author comments to reviewer 2

We thank Geraint Vaughan for the time and effort to review our manuscript and for the constructive comments. Following the reviewer's suggestion, we carefully evaluated the former Sect. 5, the analytical modeling approach, and concluded that the model does not provide sufficient value to support the observations, as discussed in detail below. Our responses to individual comments are outlined below, highlighted in blue color.

General

[...] the paper tries to use a model calculation to examine the hypothesis further. I did not find this part of the paper convincing: the model is very crude and I could not understand the presentation of the results (fig 10). I can see the value of trying to compare the observations to the 1-D model, but given the disparities I do not see the value of using the model to examine the hypothesis. For sure, if you reduce the diffusion coefficient such that tracer released into a stable layer cannot reach the surface, advection will move the tracer downstream. But I don't see how this adds value to what can be deduced from the observations themselves, because the LLJ conditions are manifestly not 1-D.
We thank the referee very much for his careful evaluation and clear statement. Owing to the lack of data required for the specification of the initial and boundary conditions, a 3D modeling study could, unfortunately, not be conducted within the framework of the present phenomenological study. Despite the known restrictions associated with the application of a 1D modeling approach to high-Arctic LLJs we decided to add to the present study at least a separate analysis, in which available observational data were merged with qualified empirical a-priori information on ABL turbulence to simulate an LLJ within the framework of Blackadar's inertial-oscillation theory. The underlying assumptions, parameterizations, and steps of data evaluation are described together with first-guess estimates of the TKE-budget terms in a very detailed manner in a separate non-peer-reviewed document (Hellmuth et al., 2023). Our main intention, however, was to set up a conceptual model to demonstrate (and quantify) the impact of an LLJ on the long-range transport of a passive tracer. This was clearly achieved. In any case, it speaks for the referee's meteorological expertise and experience when he does not see "a need" for a model-based proof of LLJ potential for long-range tracer transport. It is very fine with us if this aspect is considered sufficiently substantiated by the presented observations. Thus, in response to the referee, we decided to remove former Section 5 from the paper. At the same time, we hope that the referee accepts the short discussion of the model in the "summary and discussion" section, referring to the above-mentioned non-peer-reviewed study.

Detailed comments:

l. 28. Richardson number (not Richards)
Changed.

l.90, eqn 2. Suggest you write $(\Delta u(z))^2$ since $\Delta u^2(z)$ could be confused with taking the difference in u^2
Changed.

l.94. Please provide appropriate references for equation 3. Siebert et al (2006) makes no mention of structure functions, and Wyngaard's book does not present the t*u formulation used in this paper.
There might be a misunderstanding but the structure-function approach for estimating ε in Siebert

et al. (2006) is explained in detail with equations 10 and 11 therein. These equations also make use of the Taylor transformation (t*u). We add the equation numbers and remove the Wyngaard reference in this context.

Also, it is not clear to me why you use the same averaging period (2s) for the structure function and for the mean velocity.

The dissipation as estimated from the structure function is interpreted as an instantaneous value "valid" for the 2s period and, therefore, the Taylor transformation should be performed over the same integration period as described around Eq. 10/11 in Siebert et al. (2006).

l.101, equation 4. Again, please give a suitable reference – I cannot find it in Wyngaard's book but if it's there please provide the page number. There is on p. 16 the expression $\varepsilon \sim u^3/\ell$ referring to the largest (energy-containing) eddies, so I presume this is what is meant (this is also implied by l. 110). Turbulence is a phenomenon with a cascade of motions from the energy-producing scales to those of viscous dissipation. One could even say that a defining characteristic of turbulence it that it doesn't have a typical scale!

Yes, we refer to the largest (energy-containing) eddies, this is changed in the text now and we added the page number. Although Wyngaard's original definition refers to a mean value for ℓ, the book also mentions epsilon intermittency, and this is what we refer to by our locally defined ℓ.

l.102. You say that ε is derived over 5 s scales but on l.96 you say 2 s. In any case, why are you performing a regression of two quantities evaluated over different scales (σ is smoothed over 30 seconds)?

We agree that using 5 s instead of 2 s for ε (as used otherwise in this paper) was confusing. Earlier studies (e.g. Egerer et al., 2019 and Siebert et al., 2006) showed that the estimation of dissipation rates is insensitive to the choice of the averaging time, but smaller time windows provide a better time resolution and can reflect intermittency. However, as suggested by the reviewer, for this plot we now average ε over 30 s to be consistent with the σ calculation. Now, each dot in Fig.2 represents a 30 s time interval. This adjustment does not change our message about derived length scales and the fit parameters of the regression do not change significantly.

L.105, 108. figure caption says Ri_g, not Ri_b. Which is it?

Please excuse the typo, the figure correctly shows Ri_g, we changed this in the text.

l.106. The only length scale that can be sensibly deduced from fig 2 is 4 m, from the regression line. I presume that by 'local length scale' you mean the ratio of individual $\frac{\sigma^3}{\varepsilon}$ values, but this will be severely affected by stochastic noise.

By "local length scale" we mean the ratio of individual, locally-fluctuating σ^3/ε values, we added this explanation in the text (see also comment l. 101.). We interpret the "scatter" in Fig. 2 not as stochastic noise, but instead as naturally occurring, short-lived fluctuations of observed turbulent length scales. Of course, we are aware that this is not so simple, because although the dissipation rate can be interpreted as a local lognormally distributed quantity according to K62, this interpretation is not directly applicable to σ. However, we think that this discussion may go a little bit beyond the main topic of this manuscript. With Fig. 2, we aim to show the range of these length scales and how they relate to the different turbulent parameters ε, σ and Ri_g.

l.112. analogously

Changed.

fig.3 caption: ascents and descents

Changed.

l.185. Looking at fig. 6 I can only see two maxima, one at the surface and the other at z/zi   1.5, not three as the paper claims. Likewise, there isn't a minimum at z=zi.

We agree that some patterns in the profile were over-interpreted and changed the text to: "In the LLJ period, the average ε structure has a characteristic shape with two local maxima: near the surface and at $z/z_i \approx 1.5$ with reduced values around z_i itself. A local minimum of ε at $z/z_i \approx 0.3$ suggests decoupling of the LLJ from the surface."

All subsequent comments referring to former Sect. 5 - the analytical modeling approach:

Referee's comments on Section 5, especially on L288 (figs. 8 & 9), L306-307, and on fig. 10 are absolutely correct and helpful. A correct explanation of what has been presented in the incriminated figures can be found in the non-peer review study cited in the summary and discussion section. We do not respond to the comments here, since this section is removed in the new version of the manuscript.