Author's response to review #2 of research article AMT-2022-314

The comments of referee #2 were very helpful in advancing and revealing some issues in the manuscript We very much appreciate the time and effort the referee dedicated to reviewing the manuscript.

Answers to specific reviewer comments

Major comments:

- **One major concern I had was in the description and usage of the turbulent kinetic dissipation rate, ε. Although by definition this is a scalar quantity (describing the rate of viscous dissipation of the scalar quantity turbulent kinetic energy), the authors repeatedly refer to it as having different components (ε_U and ε_w) as well as having anisotropy between these components. I believe that this may stem from a misunderstanding of equation 8 in the manuscript, whereby they describe the wavenumber as a scalar property, whereas it is actually a vector quantity. Given that they are applying Taylor's frozen flow hypothesis, it appears that Equation 8 is actually describing the longitudinal wavenumber spectrum, in which case E(k) describes the energy content of the velocity component parallel to the wavenumber, k. This then means that the Kolmogorov constant α for the w component of velocity should actually be closer to 0.65 (from Pope (2000), page 232) leading to a difference in ε estimates of about 30%, when using the frequency spectrum to estimate it, which could explain the difference the authors are seeing in their values of ε_w an ε_u. Alternativley the different noise floor for the U and w components of velocity from the sonic on the BELUGA could also impact their estimates from the second order structure function, if not properly accounted for. Either way, I found the description of ε as having different components to be confusing.**

  We agree that our original description of anisotropy for ε was misleading in the sense that anisotropy does not exist for ε itself, but rather for the one-dimensional spectral formulation $E(k) = \alpha \cdot \varepsilon^{2/3} \cdot k^{-5/3}$. We use the spectral method for the DataHawk ε estimation from fluctuations in the relative wind, hence this is a longitudinal measurement with α =0.5. For BELUGA, we use both the *w* and u components (separately) to estimate ε from the structure functions, where the instrument's *u* is aligned (weathervanes) into the relative wind. We changed some formulations in the manuscript, replacing "components of ε" with ε estimated from the vertical (*w*) /horizontal (*u*) spectrum or structure-function.
  Further, we slightly modified our approach and hope that it is more straightforward now:
    - For DH2, ε is calculated with the longitudinal spectrum
    - For BELUGA, ε is estimated from lateral and longitudinal second-order structure functions with different C for longitudinal and lateral components (we originally had used the same longitudinal C for both). We call the resulting dissipation rates ε_U and ε_w based on the wind component they originate from.

  To evaluate the anisotropy of the flow, we relate ε_w to ε_U (depending on stability expressed by Ri) and apply this ratio to ε from DH2 because the Hanna 1968 method is based on the vertical velocity spectrum. The mean ratio $\varepsilon_w/\varepsilon_U$ at Ri=0 (Fig. 8b) is now closer to 1 as would be expected for isotropic conditions, rather than 4/3 as it was before. For BELUGA, the noise floor is accounted for when calculating ε by accepting only exponents in a certain range when fitting Eq. (9).

- **I am also concerned with their parameterization of anisotropy, as anisotropy is a relic of the boundary conditions at which the turbulence is produced it is unlikely to be readily parameterized, particularly near a surface. Furthermore, limiting to relatively small scales the calculation of σ^2_u and σ^2_v could also be impacting the value of their anisotropy estimates due to the difference in the expected inertial subrange scaling for longitudinal vs lateral wavenumber spectra mentioned above.**

  We agree that anisotropy cannot be entirely parameterized. We added some text about this in the manuscript in Sect. 2.3.5, line 412ff: "… anisotropy depends on many influence factors beyond stability, especially near the surface, and cannot be entirely parameterized. However, finding an empirical correlation between anisotropy and layer stability provides a useful way to predict when anisotropy could be expected." Please see also the comment above about how we aim to use this formulation. Since we derive the anisotropy ratios from BELUGA by including the same scales as for DH2, we do not describe anisotropy of the entire spectrum, but only for the scales of interest for our application.

Minor comments:

1. **[Line 100] It would be valuable for the authors to provide the radius of their spiral ascent/descent, as this impacts the validity of how they apply Taylor's hypothesis in Eq. 8. (Tighter spirals are not likely to provide effective approximations of longitudinal spectra).**

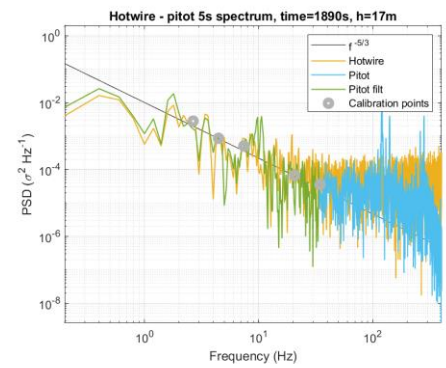
   Diameters of the spiral ascents/descents are 150m to 200m, we've now added this information (line 103).

2. **[Line 117] How is the 2 s used to estimate the spectrum determined? For a 15 m s^{-1} airspeed, this would only correspond to eddies of wavelengths shorter than 30 m. One would expect integral scales to roughly increase with altitude, such that eddies on the order of 100 m or larger could be present for the boundary layers shown in Figs. 9,10 11 and the full energy content not captured in these quantities.**

   That is correct, the original 2s intervals do not capture the full energy content. The main reason we are using short intervals is that we aim to resolve the vertical structure with shallow layers, as they occur typically in the Arctic. We increased the interval to 5s as a compromise. Please also see our response to major comment no. 1 of reviewer # 1.

3. **[Line 118] I am curious about the frequency response for the pitot probe. Typically the pitot tube would experience some attenuation at high frequency due to viscous damping and resonance at certain frequencies due to the transducer cavity. In Hamilton et al (2022) only the hot-wire probe is cited as having the O(1kHz) frequency response so the use of the pitot probe for these calculations requires some justification.**

   The pressure sensor manufacturer does not specify the sensor bandwidth, and as the reviewer notes, the pitot tube and associated tubing to the sensor can result in filtering of the airspeed signal. However, we use a very small pitot tube, and very short tubing to the sensor (< 5 cm) to avoid these issues. The best evidence of this is seen in the power spectra of airspeed fluctuations, which do not show any inherent roll-off in addition to the expected $f^{-5/3}$ inertial range cascade up to the Nyquist frequency (400Hz for 800Hz sampling). In this typical example spectrum (below) of the pitot probe, the high frequencies are influenced by the noise floor and motor vibrations above about 40 Hz, and the roll-off at 300 Hz is due to anti-alias filtering of the pitot signal. These effects do not influence the ε estimates due to the use of spectral averages at lower frequencies (“calibration points”). We added some of this discussion in Sect. 2.1.2, line 127ff.



4. **[Line 143] “and temporary 23 m made ” should perhaps be “and temporarily 23 m were made”.**

   Thanks, done.

5. **[Line 150] There should be a space between 23 and m.**

   Done.

6. **[Line 178] σ_w should be specifically defined to refer to the standard deviation of the vertical component of velocity. Similarly, the quantity ε_w should also be more specifically defined since ε is later defined as the dissipation rate.**

   We changed ε_w to ε in the formulation and defined $\sigma_{w.}$

7. **[Equation 9] What is the quantity U_τ and how does it relate to U used in equation 8?**

   Please see the comment below.

8. **[Line 219] How does the local dissipation rate ε_τ differ from ε?**

   The index τ served only to show that local dissipation rates are estimated in defined time periods τ. As this is the case for other parameters and the spectral method as well, we agree that using the index is confusing and we removed it.

9. **[Line 245] Referring to Fig.2, it would appear that the frequencies above 40 Hz are elevated above the -5/3 slope. How is this impacting the dissipation rate calculation, given that it is calculated using the full 2-400 Hz spectrum?**

   Frequencies that are impacted by peaks in the spectra, caused by motor vibrations, are excluded from the spectral estimations. The 2-400Hz range is the maximum range in which the dissipation rate is calculated, but the frequencies impacted by the peaks and the noise floor are excluded. We formulated that more clearly in Sect. 2.3.1, line 252f.

10. **[Line 249] Given that Doddi (2021) is a thesis, perhaps it would be best to have some of the details of the spectral analysis included in this paper?**

The details of the spectral analysis are included in the text passage above the reference to the Doddi (2021) thesis. We have clarified this in the revised text.

11. **[Line 295] As I understand the procedure being described, the authors are anchoring the fit to the lowest frequency point in the spectrum and using that as an initial reference for discarding points whose least-squares value is too high. However, is this justified given that the lowest wavenumbers are likely to be the least accurate (i.e. it represents the amplitude of only a single wave)?**

    Two things to note here. 1. The lowest frequencies contain the fewest data points (highest frequencies - most data points). The spectra are frequency binned (equal bin width in log space), this equally weights all the frequency bins. The fitting is conducted on the binned spectra. 2. A more thorough analysis (not included in this study) of the effect of windowing on each frequency bin suggests a very small effect on the lowest frequencies which negligibly affects the fit.

12. **[Figure 5] Assuming a wind speed of 5 m s^{-1} and UAS airspeed of 15 m s^{-1}, it might be worthwhile noting that the comparison here shows energy content in scales on the order of 30 m for the UAS, and between 10 and 150 m for the BELUGA. I would argue that this explains the trends observed between the different high-pass filter window lengths, but the best comparison to the UAS would be expected from the the 5 s window (depending on horizontal wind speed).**

    BELUGA and DH2 use different averaging times for variances (DH2 5s intervals, BELUGA 15s intervals, based on established methods from prior work and practical reasons), because of which the lengths scales included in the variance calculations are similarly in the order of 75m. We included in Sect. 2.3.2 in the paragraph about BELUGA variances and comparison to DH2 (line 325ff): “Note that since the airspeed of the DH2 is about 15m/s and the typical wind speed measured by BELUGA is about 5m/s, using a 5s analysis window for DH2 measurements would be equivalent to a 15s analysis window for BELUGA measurements in terms of the wind field scales included in the variance estimates.” However, the included effective length scales depend on the mean wind speed for BELUGA.

13. **[Line 351] It is perhaps not surprising that that behavior of the turbulent Prandtl number is less clear for stable flows, given that its definition presumes the existence of turbulence (implicit in the eddy viscosity/eddy diffusivity definition).**

    We added this remark.

14. **[Line 378] “It remains open that the DH2 provides the horizontal component,” horizontal component of which quantity? This sentence is unclear.**

    We hope that the revised sentence is more clear: “It remains open that the DH2 airspeed measurements represent the near-horizontal vector component of velocity fluctuations (due to the small slant-path angle), whereas the vertical component is needed for the method discussed above.”

15. **[Line 384] The statement that strongly turbulent flows are more isotropic than less turbulent flows is only appropriate for high wavenumbers. At small wavenumbers, strongly turbulent flows can be very anisotropic (e.g. in the neutral turbulent boundary layer).**

We re-formulated the passage, now it reads: “Generally, anisotropy is favored by strong stability (with low turbulence); horizontal modes dominate in anisotropic flows with high Richardson numbers (Mauritsen and Svensson, 2007). Galperin et al. (2007) showed that turbulence in an otherwise stable environment is influenced by anisotropy and internal waves. Anisotropy also depends on the height above the surface: close to the surface, horizontal mixing becomes dominant due to the spatial limitations of vertical eddies.”

16. **[Line 406] The authors refer to A, but previously had defined only A_σ and A_ε.**

    Changed.

17. **[Line 429] q was not previously defined.**

    q is defined above Eq. 3.

18. **[Figure 9] Which value of σ^2 is being plotted, previously had defined σ^2_U and σ^2_w. In the text e.g. Line 503) it is referred to as σ^2_U**

    The new legend entries should explain which component is depicted.

19. **[Line 443] What is meant by this statement? Equations 8 and 9 are only valid in the inertial subrange.**

    Thank you, hopefully this is a better formulation: “… because ε is a measure of energy dissipation at small scales in the inertial subrange which are more isotropic.”

20. **[Line 506] The acronym LLJ was not defined.**

    Definition added.