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Comment on amt-2022-234

Anonymous Referee #2

Referee comment on "Airborne coherent wind lidar measurements of the momentum flux profile from orographically induced gravity waves" by Benjamin Witschas et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-234-RC2, 2022

The paper by B. Witschas and co-authors presents a method to measure gravity wave momentum fluxes from line-of-sight wind observations using an airborne 2-micron Doppler wind Lidar (DWL). The technique is applied to measurements gathered over a flight leg of the Falcon aircraft during the GW-LCYCLE II campaign in Scandinavia (2016). Lidar wind and momentum flux retrievals are compared with colocated in situ measurements by the HALO aircraft available for this case study.

While the manuscript is well-written and the topic of interest to AMT readership, a major point of criticism I have is that a significant part of the material presented was already published by two of the authors of the present manuscript in Gisinger et al. (2020, article cited). Some figures are very similar to that previous study. In this context, I would expect the present manuscript to provide a thorough description of the instrument and its performance. On the contrary, although the method is sound and the results very impressive, the technical discussion remains superficial, in particular regarding the advantages of this new measurement mode with respect to other scanning modes and the uncertainties of the technique. I appreciate that earlier papers by the authors may already provide some of this information, but it would be necessary to repeat some of the details here.

For those reasons, the paper should be reconsidered after major revisions.

Major comments :

1) I find that the paper lacks an a priori estimate of wind error and resulting momentum flux noise level. Granted, the agreement with in situ measurements is very good (for wind), but there may be sources of difference other than instrumental errors (e.g., slightly different timing inducing a phase shift). Would it not be possible to estimate the expected error, even roughly, and compare with the empirical estimate? This point should at least be discussed.

Furthermore, the authors do not explain the preliminary steps involved in LOS wind retrievals (e.g., estimating Doppler shift, subtracting the aircraft ground-relative speed). The potential impact of aircraft motions and associated uncertainty could also be discussed in more detail.

2) The comparison between Lidar and in situ wind observations could be more thorough. For instance, what is the power spectral density of the differences in Fig. 6? Are there specific artifacts at given frequencies?

For a better understanding of the differences in momentum flux estimates (Fig. 9), it would be clearer to show the wavelet co-spectra of u_par and w for in situ and Lidar observations.

Other comments :

Please double check Eq. 2. I obtain the same result as Referee 1, different from yours.

Line 148-149 : Could you elaborate a bit on the 'scanner control loop on a 1-second basis'? What is the uncertainty in attitude and how does it translate in LOS wind uncertainty ? Are pilot oscillations of attitude present? If yes, at which frequency? Are they sufficiently resolved at 1 s?

Line 154-155 : If I understand correctly, the 'wind vector mode' already enables

computation of the momentum flux, do you confirm ?

Line 160 : ‘the wind field is constant for the time of intersecting fore and aft laser beam pairs’ : specify the length of that delay in practice

Line 164 ‘ kept for 2 s ’ : Why is it necessary to have 2 s (1000 laser pulses) when 1 s is enough for the vertical wind retrieval ?

Line 234 : A comparison with a priori estimates of error would be valuable here

Figure 8 and associated discussion : I am not certain how to interpret the quantity shown. The main signature in this u’w’ product is that of the high-frequency vertical wind oscillations in a lower frequency horizontal wind (Fig. 7 b), but this contribution cancels out over a period and does not contribute to the momentum flux. I would recommend showing wavelet co-spectra of u and w, as in the Gisinger paper (Fig. 7).

Fig. 7 is very similar to Figure 9 of Gisinger et al. (2020).

Fig. 9 is very similar to Figure 10 of Gisinger et al. (2020).