Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2022-234-RC1, 2022

Comment on amt-2022-234

Anonymous Referee #1

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-RC1, 2022

The article reports on moutain-wave momentum-flux (MF) measurements performed during the GW-LCYCLE II airborne campaign above Scandinavia. A novel scanning pattern technique, which has been specifically designed to retrieve gravity-wave momentum fluxes, was used with the 2µm lidar flown onboard the Falcon aircraft during one leg flight across the Scandinavian mountains on January 28, 2016. This technique is based on classical radar MF measurements, where the beam is pointed obliquely in two opposite directions, which allows to measure both the wind vertical component and its horizontal component along the line of sight. During that leg, the HALO aircraft performed a coordinated flight, flying 2 km below the Falcon aircraft, therefore allowing a direct comparison first between lidar and in-situ winds, and then between gravity-wave momentum fluxes. While the former comparison is excellent, the later is somewhat disappointing.

The article is well written and presents a very promising technique for measuring gravity-wave MF from an airborne platform. The detailed 2D perspective that it provides on MF is notably really impressive. I therefore support the publication of the article in AMT. Yet, I have several concerns on the paper, notably on how the gravity-wave observations are (or are not) interpreted, and ultimately on the significance of the momentum flux comparison. I think that these concerns need to be addressed before publication.

Major concerns

- My main major concern is associated with Figure 8a, where gravity waves MF derived from the 2µm wind lidar are shown for different altitudes. One striking feature of these fluxes is that they present oscillations around 0. This feature is simply ignored in the paragraph devoted to the figure (lines 269-287), while it has profound implications on the type of waves that are observed. Indeed, one can imagine two situations associated

with MFs that oscillate around 0: it may either be associated with freely-propagating gravity-wave packets with systematical horizontal direction of propagation almost perpendicular to the aircraft leg (which seems rather unlikely in this mountain-wave case), or it may be trapped waves for which u' measured along the wave propagation direction and w' disturbances are in phase quadrature. This latter situation seems to me very plausible in the considered case: it is for instance
consistent with the (almost) vertical structure of the short-scale wave disturbances displayed on Figure 7c) and d). I also note that the authors very briefly proposed the same interpretation on lines 312-313 (while commenting Fig. 9) and in the conclusions (lines 342-343).
Now, if most of the waves oberved during the aircraft leg were trapped waves, one would expect that the associated leg-averaged MF should nearly vanish. I therefore wonder whether leg-averaged momentum fluxes shown in Figure 9 are not simply residuals from the almost zero-mean timeseries displayed in Figure 8a), which might explain the observed discrepancy between the lidar and in-situ MF estimates. In other words, despite the excellent agreement between both wind measurements, this leg might not be the best case to compare gravity-wave momentum fluxes.

- Somehow related to the previous comment, I have another concern associated with the filtering that is chosen to extract the wave disturbances from the raw observations. On line 252, it is stated that a "5th order polynomial" is used to determine the background wind. On a 700-km long leg, this will typically filter out wavelengths longer than 150 km. On the other hand, the authors note on line 257, while commenting Figure 7a), that the u_par wind varies with wavelength of about 400 km, as is also obvious in Figure 6 (upper right panel). What was the reason to filter out this wavelength? It might actually be that this wavelength is not associated with a trapped wave and might therefore be a better testbed for lidar and in-situ MF comparisons. This can be achieved by choosing a different filter to extract the fluctuations from the background, e.g. a simple straight line between end points of the raw observations.
  On the other hand, I do not agree that this 400-km wavelength is also appearing in Figure 7b), as stated in l 258. It should have been filtered out from the disturbances!

Other concerns

- The article discusses the same measurements than those studied in Gisinger et al. (2020, ref. cited), and shares a number of very similar figures (e.g. Fig 5, 7 and 9 in this paper, compared to Fig. 8, 9 and 10 in Gisinger et al.(2020)). This may be acknowledged since the Introduction, where the focus of this paper with respect to Gisinger et al. might be stressed.
- l. 37: This sentence is slightly confusing: only the projection of gravity-wave MFs *on the flight direction* can be estimated. In other words, the "par" direction is that of the flight, not that of the different wave packets. This probably needs to be reminded to the reader more explicitely. Of course, in the mountain-wave case presented here, the leg direction has been chosen to be along the expected direction of propagation of mountain wave packets (which might also be more explicitely highlighted).
- I had difficulties in understanding the reasoning behind the "wind mode" and the "vertical wind speed" modes of the lidar, since 3D winds are already measured with the first mode (as far as I have understood). My understanding is that the horizontal resolution and the accuracy/precision of the retrieved vertical wind speed is different in both modes, but this is not explicited at first place in the paper. I would therefore recommend to provide further details on the two modes as soon as line 95-97, when lidar modes are first mentioned: e.g. wind mode means 3D wind vector with lower

horizontal resolution than in the vertical mode.
Writting this, I am yet not fully sure if the "Wind vector" data product reported in Table 1 is a 3D or 2D (horizontal) vector.

- l. 154: Related to the previous comment, this sentence is also confusing. "Simultaneous measurements of the horizontal and vertical wind speed" are already achieved in the "wind mode" if a 3D wind vector is retrieved. The advantage of the new scan pattern seems to me more associated with the horizontal resolution and the precision of the measurements rather than in their simultaneity.
- l. 160-161: "flying along wind direction": I guess you had in mind the special case of mountain wave (in an homogeneous wind field). Either extend why it is important to fly along the wind direction here, or simply remove this since the MF scan technique does not request to fly along the *wind* direction. (see also my comment for l 37.)
- Equations 2 just look wrong to me. Starting from Eq. (1), I obtain different formulas:
u_par = csc(theta_f - theta_b) * (v_f cos(theta_b) - v_b cos(theta_f))
w = csc(theta_f - theta_b) * (-v_f sin(theta_b) + v_b sin(theta_f))
- Figure 5: Why are you using a different x-axis in this figure (4 to 17°E) and in the following ones (3 to 16°E)?
- Figure 8: I would recommend to reverse the rows in the figure in order to ease comparisons with Figure 7 for instance: i.e., put the top/bottom altitudes in the top/bottom panel.
- Figure 8b: Since MF is a quadratic quantity, it varies horizontally with a wavelength that is half that of the wave-packet u' and w' disturbances. The interpretation of Figure 8b) in terms of "wavelengths" of the wave packet is therefore a bit confusing.

Typos and minor concerns

- l 28: Did GW-LCYCLE II campaign occurred in 2014? or in 2016 (see e.g. caption of Figure 1)??
- l. 178: the data *are* linear*ly* interpolated.
- l 226-228: Please refer to Figure 6 here.