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Reply on RC2

Natalie Kaifler et al.

Author comment on "Signatures of gravity wave-induced instabilities in balloon lidar soundings of polar mesospheric clouds" by Natalie Kaifler et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-572-AC2, 2022

We thank Referee #1 for his suggestions and questions and provide our answers below.

Line 36, "By means of…" I think the author is trying to say that GW or other small-scale dynamics changes the altitudes of ice particles and their surrounding temperatures. Please consider to revise.

We changed the sentence to "The altitude and temperature variations induced by gravity wave or other small-scale dynamics can result in the rapid sublimation of ice particles."

Line 60, How can the lidar measure the PMC brightness? The lidar measures the strength of the lidar echoes, so it is easy to understand the lidar can measure the "density" and thickness of the layer. But does the brightness also depend upon the size of the ice particle, which the lidar cannot measure? Please clarify?

The volume backscatter coefficient, often termed "PMC brightness", that the lidar measures depends on both the ice particle density and the ice particle's size (Mie scattering). In l. 79 we wrote "The relevant physical quantity measured by the lidar is the volume backscatter coefficient beta, a measure for PMC brightness influenced by both the size and number density of ice particles within the observed volume." Few large particles will result in the same volume backscatter coefficient as many smaller particles, and it is impossible to distinguish using a single laser wavelength. This is the same as with naked-eye observations, which is probably why the term "brightness" is also applied to lidar measurements. More details on the volume backscatter coefficient is found in Kaifler et al., ESSD, 2022. On the scales considered in this study, the changes in volume backscatter coefficient, however, are mainly due to changes in number density, because changes in size occur on longer timescales. We have added: "Lidar measurements of the c hanges of volume backscatter coefficient, also termed ''PMC brightness'', are at these scal es therefore predominantly due to changes in density, and not in particle's size." And "volume backscatter coefficient or PMC brightness" in line 60.

Line 88, it would be helpful to describe how this b is defined based on the lidar measurement. I know it is articulated in the other papers, but I think it can make the paper more reader friendly.

This was partly described in 75-79 and we have completed this explanation for the correction of background and range and normalization to a standard density profile. This

removes the Rayleigh part and results in the volume backscatter coefficient.

Line 104, "Notable is the bright …", this sentence reads a bit strange. Also, I think the "brightness" above is referring to the lidar's echo strength.

We changed the sentence to "The lower boundary, that descends by 1 km from 82.2 km to 81.2 km, is of both very large β and very large $d/dz \beta$."

Line 127-128, this sentence seems to be incomplete. Please consider to revise.

Changed to "We illustrate the derivation of PGW and Pbeta in Fig. 2 using a PMC measurement on 11 July 2018 including the part around 3:30 UT we ..

Line 134, 135, the nv and np parameters are not defined. What are they?

They were just to abbreviate the number of PMC detections (brightness values) and the number of PMC profiles, to later estimate the percentage of how many of them are associated with gradients in the tails of the distribution. As they were only used in three instances (l. 134, Table 2, and l. 180), we have replaced them with text.

Figure 2. The differences between the reconstructed results and the observations are quite noticeable, may need some explanations.

We had on purpose selected a range of scales (excluding the very short and very large scales) to construct the proxy representing specific gravity waves. Both curves were printed into Fig. 2b because they show the same quantity, not because we expect a very good agreement (which we don't). The text was modified to:
"The mean PMC altitude **z_c** (Fig. 2b, black curve) is a good proxy for layer altitude and the filtered time series effectively captures the part of the motion in the desired range, excluding both the few-hours- and the minute-scale perturbations, such that it is a good representation for the gravity wave activity we seek to characterize (Fig. 2b, blue curve)."

In addition, the distinction in Figure 2c may need more discussion.

We are not sure what is meant by "the distinction". The two quantities were merely plotted into one subplot to save space. As the dimensions are different, we agree it is better to make two subplots c) and d), as both quantities have different meanings.

Line 181-182, it may be a good idea to address how these results of category reflect Fritts' assessment of GW activities and forcing.

We added: "The assessment by Fritts et al., 2019 of weak, moderate or strong dynamics or forcing is reflected in our result for category, in the way that mostly weak dynamics or forcing relates to category A or C, and stronger dynamics and forcing to category B."

Line 202-204, what is "high definition of the layer edges"? In addition, "Second, due to…" this sentence reads funny. Please consider to revise.

Rephrased to: "This type of PMC layers have sharp boundaries, often at the lower edge, a nd thus vertical gradients are large where the layers emerge from or fade into the backgro und. This is seen e.g. in **$\partial_z\beta$** on 10 July 2018 at 19:20 -- 20:10~UT (Fig. 5a). Because of the oscillatory motions of these layers, also large temporal gradients are induced, e.g. on 13 July 2018 10:25--11:05~UT (Fig. 5c).

Line 207, it may be helpful to show some PMC images that are associated with the

discussion here to provide the direct evidence of your argument.

We have added the reference to Fritts et al., 2019, where high-resolution images from these times are shown and interpreted. A more detailed study of single cases will be done during future work.

Line 218, I am not sure the feature can indicate "instability dynamics", without the in situ temperature and horizontal wind information, just physical form of the lidar echoes may implicate some instabilities, but not enough to draw this conclusion. The same can be said for the conclusion, the first sentence in the Conclusion. These results may be the indirect evidences of the atmospheric instability.

l. 218 was changed to "likely caused by.." We had mentioned in l. 219 that additional information is necessary. The first line in the conclusions was changed to "small-scale signatures that are likely caused by.."