Summary

While this is an interesting data set in my opinion the authors have made significant errors in their analysis that makes it unacceptable in its present form. I do think though that the data set is potentially valuable, and I would urge the authors to rework their text, based on the comment I have provided, as a study of the characteristics of instabilities.

The major problems are as follows. 1) it is unlikely that any significant portion (or possibly none) of the features they are observing, at wavelengths below 5 km, are gravity waves (GWs). It is most likely they are instability features commonly seen in airglow images. 2) the presentation of the K and epsilon data is not clear in its present form. This has to be more convincing and warrants a larger discussion as the values are much higher than expected, or found in other studies. They also need to read the literature they cite.

Are the seeing any gravity waves (GWs)?

This study uses a high-spatial and temporal resolution imager that has 24 m pixels at the airglow layer. Unfortunately, to achieve the spatial resolution they restrict the FOV to just under 10 km. The data from this instrument have been analyzed, using 2D FFTs, to indicate the presence of wavelike features with horizontal wavelengths from 48 m to 4.5 km. They claim these are GWs although they do allow that some could be instabilities. They note that they could be advected by the wind as secondary gravity waves. The following are issues I have.

1. If they read the literature they cited, notably the Nakamura (1999) study and the series of Hecht papers (say the Rev of Geophysics review paper and the 2012 paper) it would immediately become obvious that current thinking is that features with scale sizes below 10 km are probably instability features caused either by the breakdown of an existing GW or instability features, such as KHIs, that are routinely formed in the airglow due to the formation of large wind shears. Now, while the authors do seem to imply that features with periods below the BV period are not GWs, the authors seem to argue that features that have periods above the BV period are GWs. But a 4 km wavelength instability feature moving with the wind at 5 m/s would have an observed period of 800 s, well above the BV period. So, all the features could easily be instabilities despite having long apparent periods.
2. Instability features are blown by the wind. GWs in general do not travel in the wind direction. But when they do two effects occur that make them less likely to be observed in the airglow layer. They are both related to the dispersion relation that shows that as the intrinsic velocity (the velocity with respect to the background wind) approaches the wave velocity the vertical wavelength decreases because the intrinsic frequency becomes small. This causes two effects.
    a. Unless the intrinsic frequency is very close to the BV frequency the vertical wavelength will be less than the horizontal wavelength. Now for the waves that are presented in this paper the vertical wavelength will be 4.5 km or much

smaller. GWs that have wavelengths thinner than the airglow layer (8-10 km) will suffer phase cancellation and will have vastly reduced amplitudes and likely will be difficult to see (Swenson and Liu,1998).

b. As the vertical wavelength decreases the waves undergoes viscous dissipation and instability formation. This is discussed somewhat in Hecht et al., 2000 as well in Hecht et al., 2018. For the former and assuming the very large viscosity implied by the current work GW, lifetimes could seconds to a few minutes for the GWs in this study.

Related to b is that if the features are really blown by the wind they are at a critical level and probably do not survive. I should note that waves in this study are travelling at a very low speed so it is very likely that extremely common wind variations would exceed the wave speed and the critical level interaction (viscous dissipation or instability formation) would occur. Hence, it seems very unlikely these are GWs.

3. The characteristics of these waves if they are GWs, as currently presented, seem strange. Their phase speeds are quite low-below 20 m/s. GW climatology's typically show phase speeds of up to 50 m/s with the histogram of speeds centered closer to 40 m/s.
4. I was somewhat curious on how the monochromatic wavelengths were derived. They state they use a 2D FFT. Now FFTs assume the wave is present over the whole field of view and are often a little misleading with respect to monochromatic waves for airglow images because waves may be present over only a small fraction of the field. In the Hannawald reference they give a very nice image showing waves and I believe the FFT approach should be appropriate for date like that. But to date, while small scale instabilities have been identified with horizontal wavelength of a few to ~10 km there have been no reports of GWs with horizontal wavelengths of 5 km to 0.05 km. I would like to see images with their respective FFTs for images where the wavelengths are ~ 4 ,1,0.5,0.1 and 0.05 km. I am wondering if most of those images show features that resemble OH images (shown in the Hecht references) with instability features and their associated secondary instabilities and the resulting turbulences. I am really curious about GWs (or even instabilities/wave trains) with wavelengths at or much below ~500 m. These have not been reported before.

Are the derived K and epsilon values realistic?

This paper argues that they are seeing rotating cylinders of turbulence. Using a formulism developed by Prolss they proceed to analyze their data for the eddy diffusion, K (from Prolss) and energy dissipation rate, epsilon (using a Weinstock formula). There are two issues. The first is whether they are using the right formulae and measurables. The second is whether the rotating cylinder is the correct geometry.

Deriving K and epsilon from airglow images

In this paper they use the following formula to derive K as ~0.1Lv where L=2R, R is the radius of a rotating cylindrical tube and v is the rotation velocity. This is based on a Prolss 1961 analysis that I have not read. The energy dissipation rate epsilon is given by KN^2 where N is the BV frequency. Based on these relations they assert the following:

"The derived values of eddy diffusion coefficients are in the range around $10^3 - 10^4$ m²/s and agree mostly with earlier results from rocket and lidar measurements and simulations. Considering the respective values of the BV frequency as calculated by Wüst et al. (2020) we retrieve energy dissipation rates between 0.63 W/kg and 14.21 W/kg, that cause estimated heatings by 0.2 - 6.3 K per turbulence event. These have the same order of magnitude as the daily chemical heating rates as reported by Marsh (2011)."

There are a number of issues with this statement.

1. Recently there have been several attempts to derive the energy dissipation rate, epsilon, based on techniques and formulae that have been applied to radar images (Chau et al. 2020), TMA releases (Mesquita et al. 2020) and airglow imaging (Hecht et al, 2021). The Chau reference provides a formula for epsilon=v^3/L. Here v is the root mean square horizontal velocity and L is horizontal scale size. In the Chau paper they derive an epsilon of about 1 W/kg and they note that this is quite high compared to rocket measurements, essentially contradicting the statement in this paper (that has no references). Hecht et al. 2021 also derive an epsilon of ~ 1W/kg using airglow data.
2. With respect to #1 Hocking(1999) provides a good discussion (as does Chau) of a generalized approach to the dependence of K and epsilon on the measured parameters, v and L. There is no particular need to assume a rotating model as used in this paper. See eqns 14-15 in Hocking and also Weinstock for the relations between K and epsilon, and between N and L and v (N~6.8v/L), and see Chu for the constant (~1) in eqn 14 (epsilon= v^3/L). One important point is that N is not a constant that one can take from a climatology such as Wust. N varies due to the temperature gradient that can be quite steep in either direction. N thus could be significantly larger or smaller than climatology. It is 0 when the lapse rate is the adiabatic lapse rate. At other times, say for shear instabilities, often N is larger than the background. Fortunately, one doesn't need the T profile to calculate epsilon, only L and v both of which can be obtained from airglow images. That is the approach followed in Hecht et al., 2021. However, while L is relatively easy to see from the airglow images v is more difficult as the background wind velocity must be subtracted, and some estimate of a root mean square velocity must be made. Hecht et al., 2021 provide one approach to this problem and suggest some uncertainties.
3. The statement that the K and epsilon values derived in this work, which I think are not accurate, are consistent with the literature is misleading. The current values I believe are

too high. (see Hocking 1999 Figure 8 for another plot of measured epsilon where values well above 1 W/kg are not there). Also, several additional studies suggest background atmosphere K values below 100 m^2/s (see Hecht et al., 2018, 2021, and Guo et al., 2017).

Are the turbulent motions best represented by 3D rotating cylinders?

Looking at the video quickly it is easy to imagine a 3D rotating cylinder as indicated in Figure 3. However, looking more carefully many of the features seem to swirl around and grow and fade in brightness, in 2D, all which may lead the brain to interpret the motion as rotating in 3D. What complicates this interpretation is that the image plane is at an angle so we could be seeing motion predominantly in 2D as opposed to 3D. While the rotating cylinder model might apply to some features it is unlikely this is valid for most of the turbulence in their images.

I think the best approach would be to follow the approach of Chau and assume that the mean velocity in any direction scales as the feature size in that direction. Then they would need to measure the scale size of the feature and the mean velocity associated with that feature. That will not be easy if they don't have a way of measuring the mean wind. If they have images with instability features, they can used to track the mean wind and then they could try to follow the approach of Hecht et al (2021) to retrieve v and L. However, even that approach can lead to uncertainties especially in epsilon since that goes as v^3.

I also suspect that some of their images are just showing the result of (larger scale) gravity wave breakdown where just turbulence features, but no distinct wavelike features, are observed. In that case, they could try to estimate the mean wind by the mean motion of all the turbulence, and then calculate v from the parcel velocity deviations from the mean wind for a particular eddy parcel.

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