Response to the comments of Referee 1

Dear Referee 1, thank you for carefully reading our paper and for your helpful and constructive comments and suggestions. We have revised the paper based on those comments and recommendations, and have provided detailed answers to their questions. We would like to extend our appreciation for taking the time and effort necessary to provide such insightful guidance.

1) Regarding the sinusoidal structure of the gravity wave as being the primary cause of the wave structure observed in the NLC:

We agree with the referee that we have interpreted the prevalence of the northward and northeastward propagation of gravity waves mostly due to either wave filtering and/or the geographic distribution of wave sources.

Since any vertical displacement of an air mass would result in compression or rarefaction and a changing density, the picture of a sine wave with uniform particle density would not be physical. These compressions and rarefactions within the wave are essential for propagation, and result in both density and temperature enhancements and depletions, respectively. Rapp et al., 2002, have shown that due to the difference in particle growth and sublimation times, the temperature fluctuations do not cause the observed structure in the NLC display. Rather, it is the enhancement of particles associated with the density perturbation that modulate their intensity (Jensen and Thomas, 1994, referenced in the text). Indeed, accounting for these compressions and rarefactions and their effect on the airglow chemistry is essential for modelling the wave structure in the hydroxyl or Na airglow (E.g. Swenson and Gardner, 1998; Vargas et al., 2006). In addition, the work of Taylor et al. (2007) shows that there are temperature wave structures coincident with the airglow intensity fluctuations, showing that a compression or rarefication is taking place in the wave. Such a temperature perturbation would not be enhanced by any geometric viewing directions. In addition, all-sky images of structure in the airglow intensity do not show any apparent geometric enhancement of the airglow structure (E.g. Taylor et al., 2007), even in displays stretching from horizon to horizon.

However, even though the published literature would indicate that any geometric enhancements appear to be small, it is true that it could artificially enhance the density perturbation. This would mean that the waves propagating upward from 5 km, which have a similar density perturbation to those observed from the NLC, could actually be coming from higher altitudes. However, since the stratopause and tropopause waves differ in amplitude by orders of magnitude, and there is little support for an effect of this size in the literature, we feel that our conclusions are sound.

In terms of the directions of propagation, the viewing enhancement along the phase front would mean that southward moving waves would be more evident near zenith and northward moving waves observed off zenith. Similarly, E-W moving waves would only appear at the edges of the picture. In reviewing the positions and directions of the waves analysed, this pattern does not appear to be the case, indicating that there is minimum enhancement associated with viewing along the phase fronts.

In order to warn the reader of these possible biases, in the discussion (p. 11, line 12-16) we have added the following:
"Viewing along tilted phase fronts may artificially enhance the density perturbation inferred from the images and bias the observations to waves moving towards the north (E.g. Pautet, 2011)."

(2) Since neither of the two papers addresses the fact that they are not viewing the intrinsic phase speed, but the Doppler-shifted phase speed, this should be acknowledged. The winds may be small in this region at times, and identifying this with the approximate intrinsic wave speed may sometimes be justified. Again, this needs to be made clear. Perhaps there are published studies of winds in this region that could be used in a statistical manner to place an uncertainty on the intrinsic phase speeds inferred from their analysis.

For wave sources fixed with respect to the observer, the horizontal phase speed is not Doppler shifted (as the Doppler shift depends only on the relative motion between the source and the observer, not on the motion of the medium). Rather, it is the intrinsic phase speed (that is, intrinsic to the medium or relative to an observer moving with the medium) that is Doppler shifted. If this were not the case, waves would never reach a critical level in the background wind (the point at which the Doppler shifted phase velocity goes to zero). In our case, where most of the sources appear to be of tropospheric origin, the Doppler shifting will be in the realm of tropospheric velocities.

More detailed comments: (1) Page 2, lines 3-6. Please do not use "easterly" and "eastward" particularly in the same sentence! I advise that you use the "_ward" ending consistently through the text.

This has been corrected in the paper.

(2) Page 6, lines 11-12. The off-hand claim that brightness variations are due to density variations for these short period waves requires a reference. The only one I have been able to find is that of Jensen and Thomas (JGR, 99, 3421-3430, 1994), which is actually cited in the manuscript.

We have added the reference at this point in the text based on the referee's recommendation.

(3) Page 6, line 27. Again, this statement must be defended against my argument made above.

We believe that we have defended this statement in the preceding section.

(4) Page 8, line 15. This statement gives the impression that the ray tracing model shows many of the waves with wavelengths greater than 40 km is a new result. Actually there are at least two other papers of observations showing that the peak occurs at much larger wavelengths. I am sure the authors are aware of this, and Pautet et al actually cite these references.

We agree with the referee that there are other papers that have observed wavelength greater than 40 km. However, we reported here to highlight the bias of the ray traced waves towards larger horizontal wavelength while the gravity wave observed in the NLC structures have horizontal wavelength less than 35 km. These larger wavelengths may be the result of the uniform spectrum of waves initiated in the model, which may not reflect the real spectrum. The primary use of the model was to identify those regions where waves may propagate into the FOV of the imager, and to assess the density fluctuations associated with those waves. However, to avoid a misinterpretation of the result by the readers, we have added the following text in the paper (p. 8, line, 4-7):

"The ray tracing results are presented that the histograms represent the propagation of various wave properties into the FOV of the camera, although the actual spectrum of waves may not contain these spectral elements."

Figures: (1) Figure 2 poorly represents the cloud structures. Isn't there a better image than this, and if not, can't the wave structure be enhanced?

The wave structures studied are evident in Figure 1, and are highlighted in figure 2. These images represent the raw data, and while one could contrast stretch or otherwise alter the images to enhance the wave structures, the purpose of the picture was to present the raw data, with the original contrast, to the reader. The primary purpose of figure 2 was not to show the wave structure so much as it was to demonstrate the geographic area covered by the imager, and the typical size of an analysis region used.

Figure 8 shows preference toward N, NE and S, SE propagation, what one would expect of wave tilts. My limited knowledge of the NLC GW literature is that the waves at mesopause heights mainly propagate in the east-west (zonal) direction. This is what is expected from the wave filtering theory and the distribution of summertime winds. The ray tracing model used by the authors shows otherwise. I wonder whether this is a result of the wind model used in the analysis. My impression is that the MSIS wind model is not very reliable, particularly if it has to simulate the winds at a given time and location.

Our results show that the largest number of waves propagate in the ENE or WSW direction. The surveys of mesospheric waves (particularly Kim Neilson's (2009) velocities from Halley, or even the momentum flux observations of Espy et. al., 2006) show significant northward or southward components. The papers by Tang and Swenson (2002) say that the flow is towards the summer hemisphere. Thus, one cannot expect strict E-W propagation without northward or southward components.

References

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