Atmos. Chem. Phys. Discuss., 13, C9809–C9811, 2013
www.atmos-chem-phys-discuss.net/13/C9809/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics and sources of gravity waves observed in NLC over Norway" *by* T. D. Demissie et al.

Anonymous Referee #1

Received and published: 8 December 2013

This paper presents results for gravity wave properties at higher latitudes than generally reported by ground observers. Thus it fills in a gap in the latitude coverage of summertime gravity wave properties, and therefore is an original contribution to our knowledge of this important phenomenon. However, the potential of the paper is reduced by a lack of substantial discussion of the origin of the short-period wave structure, omits an important effect in producing this structure, omits a citation for a statement that may not be obvious to the reader, neglects entirely the issue of Doppler-shifted phase velocities, and lacks an error analysis of the uncertainties of the wind model on their results.

Some specific comments: (1) The authors interpret the preponderance of northward-tending gravity wave (GW) propagation as being due to either wave filtering, and/or the geographical distribution of wave sources. However, both the present paper and

the earlier paper by Pautet et al (2011) neglected a third explanation. As described by Jensen and Thomas (1994) the coincidence of the line of sight with a tilted NLC caused by a GW will cause undulations in the brightness of the NLC (see their Figures 2 & 3). The relevant factors are the tilt, determined by the ratio of the horizontal to vertical wavelengths, and the angle between the line of sight and the GW propagation vector. Because NLC are necessarily viewed at small elevation angles, this dynamical-geometrical effect can produce the appearance of substantial changes in wave brightness, even in the absence of any variations of ice density within the cloud itself. The effect is maximum when the GW direction is aligned directly towards or away from the line of sight, but can be important even for less favorable geometries. It seems more than a coincidence that the more prominent waves and billows tend to be perpendicular to the line of sight. It may happen that NLC are not tilted according to the simple formulas, but I see no reason to discount this as a viable explanation. The authors need to discuss this possibility, since if it is important then the other two explanations favored by both papers must be seriously questioned. Furthermore if this effect is indeed the cause, then the projection onto a flat plane (the so-called 'satellite view') is not what a satellite imager would "see", since this effect disappears for more nadir viewing. Also, the authors claim (page 6, lines 11 &12) that the brightness changes are directly proportional to the ice density changes is no longer true, and furthermore their conclusions about the altitude of the source region are no longer valid, since the "density modulations" were the primary reason for deciding on the height of the wave source.

(2) Since neither of the two papers address 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 indentifying 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.

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.

(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.

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

(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.

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29303, 2013.