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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical mixing in the lower troposphere by mountain waves over Arctic Scandinavia" *by* M. Mihalikova and S. Kirkwood

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The paper by Mihalikova and Kirkwood studies the vertical mixing by mountain generated gravity waves in the troposphere. The authors analyze ozone sondes data and construct two composites of individual ozone profiles: for days when gravity waves are present or absent. To detect days when the waves are present the authors use data of a VHF wind-profiling radar located on the lee-side of Scandinavian mountains. The mean vertical ozone gradient at altitudes about 2-3.5 km is found to be larger for 'outside wave' conditions than for 'in-wave' conditions. The authors conclude that the difference is attributable to turbulent mixing associated with gravity waves. They estimated the eddy diffusivity coefficient to be of order 5000 m2/s.

The paper formally fits within the scope of topics covered by ACP, it is logically well structured and easy to read. However I do not believe in the estimated diffusivity co-

efficient; I find the calculations too qualitative, and I think that such calculations should be based on data from a more controlled experiment. Therefore I cannot recommend this paper for publication in ACP. My criticism is detailed below:

1. The main problem with the article is that, in my opinion, the difference between composite profiles is not enough proof for claiming there has been a wave-induce mixing. An accurate experiment should include sampling of the same air mass before and after a mixing event. Otherwise, the results are very uncertain and therefore have little value.

2. The usage of Eq. (1) is questionable even if the concept of wave-induced mixing is accepted. First, the mixing within the layer is not complete; there is still a non-negligible vertical ozone gradient in the wave composite. Second, the use of density scale height is not justified for tropospheric ozone. The characteristic scale for tropospheric ozone in the free troposphere is larger than that for air density (the mixing ratio changes in vertical slower than the air density). Instead of Eq. (1) one could apply the diffusion equation: d[O3]/dt=K d2[O3]/dzˆ2 and fit the observed values to the equation as follows (my estimates based on figure 4): d[O3]/dt=5ppb/2.5hours; d2[O3]/dzˆ2=5ppb/(1km)ˆ2. The value of K based on these calculation is about 100 m2/s. In my opinion it is more realistic value although probably is still too large.

3. Another shortcoming of the manuscript is that the authors do not present other evidences of wave breaking apart from the ozone gradient difference. For example, can evidences for wave breaking be found from wind profiles? A related question not addressed by the authors is why the waves break at such low altitudes? Were there favorable conditions for wave breaking? These questions should be addressed in the manuscript in order to gain more credibility for the theory of wave-induced mixing.

4. It is questionable that the gravity wave-induced mixing can make a considerable contributes to the seasonal cycle of surface ozone. The gravity wave mixing events are episodic (according to the authors about 5-10% of time) and limited in space. It

would be more illustrative if the authors provided estimates of the downward ozone flux caused by the gravity wave-induced mixing.

5. It would be useful to show radar measurements of vertical velocity for a wave event.

Technical comment:

6. The vertical axis in Figures 1 and 4 misses units (meters).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31475, 2011.