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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

Anonymous Referee #2

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This manuscript presented a case study of vertical mixing in the lower troposphere induced by mountain waves over Arctic Scandinavia. The authors used measurements made by ozonosondes and wind-profiling radar. They separated the observed ozone profiles into two groups depending on whether mountains waves were present (in-wave profiles) or absent (outside-wave profiles). The authors used back-trajectories to select the ozone profiles used in there analysis based on the origin of the air masses. The main result in this manuscript is that the vertical gradient of ozone was found to be smaller and less steep in the presence of mountain waves. The authors then conclude that the small vertical gradient of ozone is due to mixing induced by breaking lee waves. Finally the authors gave an estimation of the eddy diffusivity which was found to be around 5000 $m^2 s^{-1}$. The manuscript is well organized, and the problem addressed is of interest to many ACP readers. The quantification of the eddy diffusivity is an

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Interactive Discussion

Discussion Paper

interesting topic that has important applications in subgrid scale parameterizations. However, I don't feel that the authors presented convincing evidences to support their conclusions.

Overall, I found that this manuscript is speculative in some places; and in my opinion, there are major concerns that need substantial revisions, and further analysis that must be addressed prior to being accepted for publication. Therefore, I cannot recommend the acceptance of this manuscript in its present form. Examples of my concerns are listed below:

a) The conditions listed in Section 2.2 (page 31479, line 10) to distinguish between the presence and the absence of mountain waves are in my opinion incomplete. How do we know that mountain waves were present in the "in-wave profiles" cases, and how do we know that these are lee waves? The authors state that the observed direction of the horizontal wind and the amplitude of vertical velocity are indicative of the presence of mountain waves, and they may be right. But their claim needs some quantification and comparison with theory, as other waves or synoptic motions may contribute to ozone and temperature variations that they observe. So confirmation that these variations are due to mountain waves needs to be supplemented by further evidence and analysis. For example, the authors could look at the relationships between the measured components of velocity, potential temperature and ozone fluctuations, and verify if these relationships satisfy (at least in a linear case) the well-known polarization relations for gravity and mountain waves. These relations are given in many papers (i.e. Fritts and Alexander, 2003, Rev. Geophys: Gravity wave dynamics and effects in the middle atmosphere). The amplitude of these fluctuations could also be used to quantify the activity mountain waves.

b) The presence of favorable conditions for mountain wave generation does not necessarily mean a presence of breaking lee waves or wave-induced turbulence. Mountain waves could propagate vertically depending on the stratification and the mean flow. A value of vertical velocity around 0.25 ms^{-1} (section 2.2, page 31479, line 20) does

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not provide in itself evidence for the presence of turbulence. In fact, the presence of turbulence or mixing means that the profile of potential temperature must be adiabatic (or nearly-adiabatic) at some levels. The potential temperature profiles presented in Fig.1 and Fig. 4, which I assume are plotted as a function of altitude, do not show any evidence of that. In Fig 1, the potential temperature profiles show similar gradients between the two cases, with a constant difference of 4 K (section 2.3, page 31480, line 10). In an other hand, the vertical profiles of ozone show higher gradients when the waves are not present. If there was turbulent mixing, the vertical gradient of potential temperature should also be small. Furthermore, gravity (or mountain waves) may induce local variations in the vertical gradients of ozone and potential temperature even when the wave motion is dominated by reversible dynamics, without any mixing or turbulence. So, evidence for mixing should be provided by showing, for example, individual and representative cases in “in-wave” conditions where potential temperature profiles show mixed or nearly-adiabatic layers. Also, estimation for the Scorer parameter may shed light on whether the waves responsible for ozone and temperature fluctuations are lee waves or propagating mountain waves.

c) The selection of the ozone profiles presented in Fig. 4 is based on the origin of air masses using back-trajectories (Fig. 3). The authors motivated their choice by the fact that the contribution of the difference in the air mass sources to the mean profiles would be minimized as air masses would be similar (section 2.3, page 31480, line 25). However, the authors did not provide any evidence that the air masses have the same properties at the locations where they were originating from. The only similarity that I can see from Fig. 3b is that air masses are originating from the west of Scandinavia. Distributions of ozone at the origin of air masses are needed to show that they have similar properties. Otherwise, the difference in the ozone profiles presented in Figs. 1 and 4 could partly be explained by the difference in the properties of air masses at the origin.

d) The authors state in Section 3 (page 31482, line 5) “The in-wave profile has a more constant gradient. . .this can be a sign of turbulent mixing in the in-wave layer". This is purely speculative. There is no argument in the manuscript to support the presence of wave-induced turbulent mixing.

e) Section 4 (page 31483, line 5): "This similarity, in addition to the potential temperature values, supports the view that the sources are air masses with similar properties". I don't understand this sentence, and it is not clear to me how this statement implies that the sources of air masses have similar properties. While total ozone is similar, the vertical distribution of ozone could be different.

f) The estimation of the eddy diffusivity presented in Section 4 (page 31484, top) is in my opinion very simplistic. There is no justification of the height scale and the characteristic time scale for vertical mixing used in Eq (1). The time scale was derived from the time spent by the air parcels while crossing the Scandinavian mountain range. Air parcels could have just been transported by the wave motion without any vertical mixing.

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

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