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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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At first we would like to thank all reviewers for their efforts in reviewing our article and valuable comments. We will present our answers to the individual points raised .

Review 1)

1) We are aware of the shortcomings of the analysed data as it was not gathered in a campaign targeting exactly this research problem. However, setting up a coordinated experiment on both sides of Scandinavian mountain range, in places where no regular ozone sonde measurements are made, is a complicated and rather expensive matter.

This should not be done without prior investigation of the situation and justification of the worthiness of such an experiment. As stated in the closure of discussion part of the article, we see this study as indication that further investigation is worthwhile, i.e. we say :
For better understanding and for better determination of the influence of turbulence in mountain wave conditions on the concentrations of ozone in lower troposphere and ground level ozone, more measurements are needed of ozone profiles in targeted conditions, preferably including observations in air masses crossing the Scandinavian mountain range from both sides of the range.
Maybe to make this text clearer we should have used the term *for more exact determination* instead of *for better determination* and *including coordinated observations* with explicit use of the word *coordinated* which we have understood as given but might not be understood as such by other people. With this we hoped to present this study as incitement of further, specifically targeted experiments. While aware of this big shortcoming of the data at hand, we made the effort to analyze the data and include only such measured data that had similar origin to minimise the difference between the arriving air-masses as much as we could, without having the measurements prior to the air masses crossing the Scandinavian range, with the remaining main difference considered being the time air masses spend in the mountain waves region after crossing the mountain range. We think that the value of the results does lie in the exhibited behaviour of the ozone profiles which suggests that there may indeed be substantial mixing taking place, which is worth further study.

2) In the second point, a quick estimation of the value of K based on the diffusion equation was made. As the whole reasoning behind the reviewers comment was not given, we were not completely able to follow reviewer's thought process. From his estimation, for us, several questions arise. The time change of ozone in the lower level was around 5ppb/2.5hours. However, why or with what justification should we take a

vertical layer of 1 km for the estimation of K? By choosing another layer we would arrive to value of K different from 100m^2/s. So what shall the value of K actually represent?

We would like to point out that the equation we have used, Eq.(1) , comes directly from the diffusion equation. We have considered the diffusion equation in the following form:

$$\frac{\partial \rho m_i}{\partial t} - \frac{\partial}{\partial z}\rho K \frac{\partial m_i}{\partial z} \approx 0$$

Right side of this equation is 0 as we have considered no production or destruction of ozone in the free troposphere.(Index i denoting the property of species i in our case ozone.) Further we considered following dependences of density and mixing ratio on height and time:

$$\rho = \rho_0\ exp(-\frac{z}{H}), \text{ thus } \frac{\partial \rho}{\partial z} = \frac{\rho}{H} \text{ and } \frac{\partial \rho}{\partial t} = 0$$
$$m_i = \frac{\rho_i}{\rho}, \text{ where } \rho_i = \rho_i(t) exp(\frac{-z}{H_i(t)})$$

After substitution into the diffusion equation and assuming

$$m_{i0}(t) = m_{i0}(0) exp(-\frac{t}{\tau})$$

we gain following equation at t=0 and z=0:

$$K = \frac{H^2}{\tau}\frac{H_{i0}}{(H_{i0}-H)} \text{ (A)}$$

where H_{i0} is the scale height of the species at the time t=0. When analyzing Eq.(A) we are presented with several posibilities for the value of H_{i0} either $H_{i0} >> H$ or $H_{i0} << H$ or $H_{i0} \approx H$. In the case of $H_{i0} \approx H$ the equation becomes singular but in this case the mixing ratio is constant with height and will not change whatever the value of K. For our situation, $H_{i0} > H$. In the limit, $H_{i0} >> H$, Eq. (A) becomes Eq. (1) used in our paper :

$$\tau \sim \frac{H^2}{K}$$

As the ozone scale height H_{i0} is larger than H (mixing ratio increases with height) we have used the approximation as shown above.

As pointed out by the reviewer the mixing within the layer is not complete and there is still a vertical ozone gradient in the wave composite. Since we expect (possibly complete) mixing to occur only within thin layers, at different heights/locations as the air travels over the mountains (as found in Kirkwood et al., 2010) , there is no reason why the mixing over the whole height interval should be complete.

3) and 5) These comments concern the characteristics of mixing in the waves. As all these questions were addressed in a previous paper written by Kirkwood et al., 2010 (cited in our paper) we did not consider necessary to do the same here. By citing the paper in the introduction section and while estimating the path and time spend by air-masses in mountain-waves we hoped to direct interested reader to this study for further evidence of wave breaking in this area and heights. We acknowledge, that we should probably have written this more explicitly, so there would be no misunderstanding about why answers to these questions were not included in this paper.

4) The paper did not make the claim that gravity wave induced mixing can make a considerable contributes to the seasonal cycle of surface ozone. Seasonal variation in surface ozone is mentioned in the *Introduction* part of the article while explaining our first motivations for the start of this study. Even there, we do not state that the gravity wave induced mixing can make the contributes on its own, but we state that it might help to explain this seasonal variation in the connection with major stratosphere-troposphere exchange (STE) processes like seasonal winter maximum in occurrence of tropopause folds. In other words, it might help to mix the stratospheric air brought to lower altitudes by tropopause folds to the lower tropospheric environment and with that increase the effectiveness of such transport. In the *Discussion* part we make a

following statement:
The difference of 4ppb (7.9μg.m^{-3} at measured mean temperature and pressure) between in-wave and outside-wave conditions which we find in the lower levels of troposphere is of the same order as typical diurnal variations of ground ozone. The mean amplitude of variations of ozone within each day in February and March 1997 is, according to ground ozone measurements performed in Esrange, 9.8μg.m^{-3} (based on hourly means of ozone concentrations, http://www.ivl.se/tjanster/datavardskap/luftkvalitet.html). From this we can conclude that in individual cases the changes in ground ozone concentrations can be highly influenced by turbulent down-mixing of ozone from higher altitudes. As has been previously shown (Terao et al., 2008) the seasonal ozone changes in the middle troposphere can be linked and are correlated with the changes of ozone in lower stratosphere. This stratosphere - troposphere exchange is driven by synoptic scale processes eg. tropopause folds. In the presence of mountain waves during these events, ozone is more efficiently down-mixed and can influence levels of ground ozone.
As we have just noticed now a paragraph devision before the *As has been previously shown...* is missing to make this text probably easier understandable. We made our conclusion only about individual cases of the diurnal change and not about the contribution to seasonal variation of ground ozone as we do believe that for this kind of conclusion more research, not only on gravity wave induced mixing but also on processes of STE like tropopause folds in polar regions is needed. But in the cases with present mountain wave turbulence it might be more efficiently down-mixed and could influence the ozone concentrations in individual cases even at low tropospheric levels significantly. In the second part of the text we just want to show that through the link of turbulent down-mixing these changes in concentrations can be linked to the STE processes. There are however other processes which are not completely understood yet that contribute to the uncertainty of the absolute contributions as discussed further in the *Discussion* section of the paper.

6) Thank you for pointing the missing units on the axes of figures 1 and 4. We have missed this mistake in the editing process.

Review 2)

a), b) and d) Similarly to points 3) and 5) of the first review, we based our study on previous work that was done concerning the mountain wave induced turbulence in the area of northern Scandinavia as seen by the ESRAD radar by Kirkwood et al., 2010 (cited in our paper) which refers also to other studies such as eg. Rechou et al.,1999. We did not consider necessary to present same studies for this article as these were already published. As mentioned before, we acknowledge that we could have, instead of just citing the latter article few times, referred to it more explicitly.

c) We agree that there is a risk that there could be differences in source air masses (before crossing the mountains) in our in-wave and outside-wave cases. While we have done what we can to minimise this risk, with the data available, it can only be eliminated completely by coordinated ozonesonde launches on both sides of the mountains. As explained in 1) above, we see this study as a first attempt to see if there are indications of significant mixing so that it would be worthwhile to mount such a campaign of measurements.

e) When we say that the air masses have 'similar' properties, this is of course a qualitative statement. We could perhaps have said 'not totally dissimilar'.

f) See section 2) above for the discussion of height scale. The time scale for passing

the mountains and the expectation of turbulent mixing along the path, is based on the previously published study by Kirkwood et al., 2010.

Review 3)

As Esrange, where the ESRAD radar is located is not a site with regular ozonesonde measurements available data is very limited. In this paper we have analyzed series of ozonesondes launched during ILAS validation balloon campaign. As these kind of campaigns are not regular, there is no more ozonesonde data available. Also as in previous reviews we acknowledge that we should refer to previous work done on turbulence in mountain waves over northern Scandinavia as seen by ESRAD radar (Kirkwood et al.,2010) more explicitly as this presents further evidence about turbulence and mixing and explanations about how these are seen by ESRAD.

Minor points:

Page 31478, line 6 and Line 8: we are happy to include the linguistic changes suggested.

Page 31478, lines 7-11: We have used the paper by Kreher et al. to refer to this instrumental part of our paper because this analysis is based on the same set of sondes. We have checked the vertical resolution of the sondes which is indeed between 30-60m, data has temporal resolution of 10-15 seconds and average ascent rates of 3.3-5m/s.

Page 31479, lines 12-14: The method is based on the typical characteristics of mountain waves when they are observed at this particular radar site, as documented in Kirkwood et al., 2010 and references therein. The waves are generally quasi stationary, with a long vertical wavelength, at least equal to the height range of the

radar measurements, so that fluctuations over time or height are not particularly useful indicators of the waves presence.

Page 31483, lines 15-16: At first all heights until 6000m were inspected and described in the result section. Discussion was made only for the heights under identified tropopause folds ($\approx$3600m).

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