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Atmospheric Chemistry and Physics Discussions

Interactive comment on “First space-borne measurements of the altitude distribution of mesospheric magnesium species” *by* M. Scharringhausen et al.

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Interactive comment on “First space-borne measurements of the altitude distribution of mesospheric magnesium species” by M. Scharringhausen et al. Anonymous Referee #5 Received and published: 19 December 2007

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Responses to referee’s comments are marked with ’###>’

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The manuscript starts with a review of the Mg/Mg+ chemistry as it can be found in literature. The following and largest part of the paper focuses in great detail on the retrieval algorithm developed for the SCIAMACHY instrument on board the EVNISAT satellite. Similar algorithms have been published in the past but the comprehensive overview in the manuscript gives the unfamiliar reader many valuable insides about the limitation and difficulties involved in the data analysis. In the last part these algorithms are used for the retrieval of MG/Mg+ from two weak resonance lines at 280 & 285 nm.

Section 3.1.2: ...It is assumed that the atmosphere is homogenous horizontal as well as vertical within a layer of thickness h...

From ground based observations of other metals, like Na, K, Ca, Fe it is well known that metal layers are in general not horizontal and vertical homogeneous which is an important and very basic assumption of the analysis. Metal layers are strongly influenced by gravity waves/tides and phenomena like sporadic layers above ˜90 km are a common case. The measurement of Mg+ in Figure 9 (last panel) show also such structures with a maximum in the density at ˜100-105 km and a second ’sporadic’ event with similar density at 115-120 km altitude which is very different from the average profile derived by models (also shown in Figure 9). It is not clear how small scale structures influences the analysis.

########> Two follow-up papers of mine are cited therefore.

Section 3.1.4 De-excitation then leads to isotropic and unpolarized radiation of the same wavelength.

Resonance scattering is not isotropic as often believed. The earth magnetic field leads to non spherical scattering due to the Hanle effect (Hanle, W., Z. Phys., 30, 93-105, 1924). The Handle effect has been studied in the past for the observation geometry of lidars (e.g. Na-lidar: Fricke and von Zahn, J. Atm. Terr. Physics, 47, 499-512, 1985). Depending on polarisation and magnetic field vector and viewing geometry the scattered intensity can change significant. To my knowledge the Hanle effeect has not been calculated for Mg/Mg+ but the example of Ca/Ca+ at ˜393 nm and 423 nm show that the Hanle factor can be as large as 1.5/1.25 (Values from Table1 of:

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Alpers et al., Geophys. Res. Letters, 23, 5, 567-570, 1996). It is unclear how such an asymmetric scattering influences the retrieval and how important it is for satellite observations.

#########> Two follow-up papers of mine are cited therefore. See those for clarification and extension of the algorithm.

Section 3.4.5 Improvement of S/N One can improve S/N by dividing the spectrum by the solar spectrum. However the assumption here is that the solar spectrum is known precisely enough and represents the true disturbing ’background (-spectrum)’. Otherwise the derived spectrum may be systematically biased. Please comment how good the solar spectrum is known and represents the ’background’. For very weak features like the Mg/Mg+ lines a good knowledge of the ’background’ is crucial.

#########> Done.

4.2.2. The mesospheric column densities agree well with lidar observations of the total column done over Wallops island 1.7*10ˆ10 cm-2 and Sardinia 2.1*10ˆ9 cm-2. The retrieved value of 3.22*10ˆ9 is in this range but the two values from lidar observations show already that the column density varies strongly. Two other in situ measurements by rocket born mass spectrometer have shown 1.9*10ˆ9 (Zbinden et al., Planet. Space Sci., 23, 1621-1642, 1975) and 4*10ˆ7 - 2*10ˆ9 (Steinweg et al., J. Atmos. Terr. Phys. 50, 93-104, 1992). The observed value varies therefore by ˜3 orders of magnitude...

#########> True. The majority of models/column densities yields alues of 10ˆ9 – 10ˆ10, though. See my follow-up papers for more references.

4.2. Mg/Mg+ My main critic results from the misleading title of the manuscript. The largest part of the paper focuses on the retrieval method including a sensitivity study. In contrast the title of the manuscript implies that the main point is the ’Alti-

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tude Distribution of Mesospheric Magnesium Species’. However, only Figure 25 (Mg) and Figure 27 (Mg+) show a single Mg/Mg+ profile. Moreover, in both Figures only two points at the topmost altitudes are ’statistical significant’ in the sense of the diagonal elements of the covariance matrix (see description of the Error analysis section 3.2.4) which represents for a Gaussian distribution a confidence interval with a 1-sigma probability of roughly 66%. From statistical arguments alone and on average 1/3 of all points of a measurement with 1-sigma uncertainties should deviate ’significant’ from the true value. In this particular case (and of course always only on average) roughly 2 points out of 6 should deviate ’significant’ from zero from statistical arguments alone if all the assumption like a Gaussian distribution etc are fulfilled. Taking further into account the relative low resolution of the instrument at theses altitudes of ˜5 km (derived from the averaging kernel by the authors) the existence of Mg/Mg+ is hardly demonstrated here since the two points may be even not statistical independent. The significant of the Mg/Mg+ profiles are derived and discussed in the paper by the complex mathematical treatment of a single measured profile only. On top of this systematic biases which can easily contaminate the retrieved Mg/Mg+ profile are not included in the discussion even though many are shown and discussed throughout the paper before. An example of such a systematic bias is the increase of the neutral density above 85 km altitude in Figure 22 caused by stray light as stated by the authors. One could ask e.g. if a similar effect may have occurred at the topmost altitudes of the Mg/Mg+ profiles of Figure 25/27?. Such effects and the weak signal from Mg/Mg+ with the resulting large errors in the analysis require in my mind a proof of the results beyond mathematical arguments alone and on a much larger data base. In particular the claim that the ’Altitude Distribution’ has been obtained is certainly overdrawn. Model calculations and the few observations (figure 9) show, that the largest part of Mg/Mg+ is presumably above 90 km and therefore even above the measurement capability of SCIAMACHY. The author already discuss many critical points like the topmost altitude of 92 km altitude at the tangent point (which is below the maximum of at least the Mg+ layer (see Figure 9))

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or a possible influence of the chosen TOA in the model. Even so many limitations are considered or modelled it is not sure if Figure 25/27 are biased. Beyond the mathematical treatment of a single profile only one could demonstrate the reliableness of the method by applying the analysis on a larger data set and/or another already well known metal and compare it with model results or ground based measurements. Well known metal layers like the widely studied Na layer show an annual cycle with a winter maximum and summer minimum which for example should show up in the measurements of SCIAMACHY. The analysis of a larger data set would also give additional insides in the variability/reliability/significant of the measurements and would serve as an independent proof of the analysis method. The comparison of a single measurement with the average result of a model is of limited use since the observed and may be strongly disturbed profile can deviate largely from the average state. Nevertheless the author conclude in 4.2.2 ’The Mg profile shows a pronounced peak around 85 km. These values are significant in terms of the retrieval/measurements error as well as in terms of the information content that can be read from the averaging kernels. This result is consistent with model calculations...’ Even by assuimg that the two points are signifcant one can argue here that the pronounced peak of Fig. 25 may be nothing else than the lower edge of the Mg layer given by these two ’significant points’. These retrieval contains no information about the Mg density at altitudes above˜90 km and the agreement with the model may simply reflect the fact that no observations are available above ˜90 km and the measurement are not significant at all below ˜82 km. The peak must be therefore in this particular case between 82 and 90 km. The (not observed) real peak of the Mg layer in this particular case may have been well above 90 km and can be at any time very much different from the average profiles simulated by models as discussed before due to the well known strong natural variability of metal layers. From the observation point of view many observations by SCIAMACHY are available. It would be very important to see if the features of Figure 25/27 repeat from retrieval to retrieval and show systematic difference with latitude etc. Even though the authors have done a great job in devel-

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oping and describing a state of the art data analysis procedure the conclusion that the altitude distribution of the Mg/Mg+ layer has been retrieved is overdrawn in my mind. The spectrum shown in figure 4 is a clear indication for the presence of Mg/Mg+ in the data but a real proof if altitude resolved profiles like Figure 25 & 27 can be retrieved requires further data analysis on a much larger data set to ensure that such profiles are not just statistical events or artefacts. I suggest either to shift the focus of the paper (in particular the title and the claim of altitude resolved measurements) to the retrieval method or at least to extend the last section about Mg/Mg+ by applying the analysis on a much larger data set as discussed above.

 The title, abstract and conclusions have been changed accordingly. The profiles are termed “preliminary” now. I admit that the resolution is not optimal. The best SCIA resolution is not better than 3.3 km, though. Processing of a larger data base is not possible as the algorithm is not operable anymore. This is due to the fact that the concept itself is obsolete. It could be shown in [Scharringhausen, 2007, University of Bremen, PhD thesis] that a pure limb algorithm cannot deliver more than estimates of the Mg/Mg+ distribution.

Minor corrections:

Section 2. ...covers the wavelength range from 280-2380 nm... Is the 2380 nm correct?

##########> Yes.

Figure 4: Is the second peak on the right side of the ˜280 nm Mg+ line the second Mg+ line or something else? Please comment (and label in the panel) on the relative broad feature of Mg+ at ˜280 nm with its broad ’wings’.

##########> Fixed.

Figure 9: Check labels a,b,c,d in text and panels. Panels are given as a,b,c only. Panels are much too small. Labels and axis are unreadable.

#########> Panel labels are corrected. The small labels are due to the two-column

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format. I suggest using an electronic source to have the option to zoom.

Figure 12: Please label the individual lines in the right panel and check in particular the wavelengths given in the legend. Mg I is given as 280.213 nm but should be 285.165 nm.

##########> Fixed.

Figure 16, 17, 18... and many others figures. In my print out the differences between dashed lines and solid lines is often nearly undistinguishable and the legends in the panels therefore not helpful. Not all figures are needed and e.g. Figure 16 and 17 could be rejected and discussed in the text only.

##########> See above.

4.2.2 ...Wallops Island... 1.7*10-10 cm-2 is wrong

##########> Fixed.

Figure 25/ 27 It would be helpful to include an average model profile for comparison. Figure 9 has complete different scales and a comparison is difficult due to the numerous curves etc in the panels.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4597, 2007.