

Answers to the Referee reports:

We carefully read the Referee reports and tried to correct every mentioned point. As was the major point of criticism we improved on the discussion and interpretation of the results. See below for the detailed answers (blue) to the reports (black).

When preparing our final manuscript, we found a minor issue in the Retrieval algorithm, as sensitivity of the instrument to polarized radiation was not treated correctly. We added a short section for the proper treatment of this effect and updated the figures.

In the context of the discussed features, the latest correction is rather small and beside the absolute numbers for the number densities, nearly all findings of the previous version are still valid. The only considerable difference is that for the latest version the asymmetries between the observations in the Northern and Southern Hemisphere are reduced.

We included 2 new sections:

Section 3 shortly describes the latest correction of the retrieval algorithm, which is a polarization correction.

Section 8 shortly discusses the connection of the Mg/Mg<sup>+</sup> with NLCs, which is a part of the improvements on the discussion and interpretation of the results, the referees asked for.

Answers to Report 1:

General Comments

1) This paper describes global observations of the Mg and Mg<sup>+</sup> ion from the SCIAMACHY satellite instrument. The observations are discussed in terms of previous measurements and concluded to be in generally good agreement. The SCIAMACHY measurements are compared to 3D model results of the Mg and Mg<sup>+</sup> ion number density. These comparisons indicate better agreement for Mg ions than for Mg<sup>+</sup>, although differences exist. In particular the model indicates a seasonal cycle in Mg that is not present in the observations, and the model does not indicate latitudinal dependence in the Mg ion peak height which the observations show.

2) The results of this paper are relevant to a variety of areas including studies of meteor influx and deposition of meteoric material in the atmosphere, and studies of aerosols and chemistry where interaction with meteoric remnants (smoke) is relevant.

3) The paper is, however, somewhat lacking in scientific interpretation but rather reads as a recitation of results. For example, the discrepancies between model and observation concerning the seasonality in Mg could be discussed in terms of the potential measurement and model shortcomings, ultimately leading to suggestions for future work.

We would like to thank the reviewer for his insights.

To meet the general criticism we have improved the manuscript.

We initially described the data. Our focus was to show the Mg and Mg<sup>+</sup> measurements, make comparisons with model results and discuss similarities and differences.

In the discussion we point where the model and measurements agree and where not.

For the point where the agreement is well, the assumptions put into the model appear to be correct.

We have addressed this issue (see also the answers to others referees comments pointing harsher at the same point) by adding discussion for the possible reasons for differences between the measurements focusing on the most likely explanations.

Specific Comments

1) Many of the Mg<sup>+</sup> observations are presented separately for retrievals using two different wavelengths. The results are different, however, there is no discussion of why they vary for the two wavelengths, or if the authors suspect that one wavelength gives

a better result. This may be discussed elsewhere, but the present study should give a synopsis of any issues. It would be preferable to show only results from the most reliable of the two wavelengths, and then state that the other wavelength indicates similar variability in time and space yet with some bias.

- Agreed, removed figures for the 279.6 nm line. The initial intention was to show that the densities retrieved from both lines show very similar results and to make it directly visible to the reader where the small differences are and to judge which features appear to be significant and which not. The 279,6 nm has the better signal to noise ratio, but also has the stronger self-absorption effect, which is however very small for Mg+. However, the 280.4 nm line is independent of any polarization related issues, as it depolarizes the light and a wrong polarization correction leads to systematic biases. However, with the latest corrections results from both lines are in better agreement.

2) pg. 1980, second paragraph: The discussion of electron distributions comes with no motivation. Please begin with a sentence or two discussing why this is relevant to the study.

- An introductory sentence has been added and the discussion of the transport processes in electromagnetic fields has been moved to the section, where the differences between model and measurements are tried to be reasoned.

3) Section 5: The comparison of SCIAMACHY to previous observations is useful and appreciated. It would be instructive, however, to convey these results in the form of a figure or table if possible.

- It is of course more instructive to show figures for comparison. I added these figures to the manuscript with the recommendation of using the original figures as a reference. I hope this is sufficient.

4) Section 6, Figures 16: It would be useful to add panels that show the VCD vs. time for both model and observation on the same plot, for selected two latitudes. This is suggested because the model - observation comparisons are a major part of the work, and could benefit from some additional illustration.

- Added as part in point 3)

5) Section 6, Figures 17 & 18: The comparisons of SCIAMACHY and WACCM are instructive as shown, but it might be more illuminating with an additional panel that shows VCD vs. latitude for both model and observation as lines on the same plot.

- I understand that showing a 2D(x-y-plot) slice of the presented data might be easier to read, than the 3D(x-y-color-plot). However, it has also been criticized by the other referee, that there are already too many figures in the manuscript, and I think its better to keep the VCD vs. lat. vs. time plot, as much it contains much more information than a slice of it.

Technical Corrections

pg. 1974, line 26: "125 km." remove period

- done

pg. 1975, line 2: "density" should be "number density"

- done

pg. 1975, line 14: "can act" should be stated "are thought to act as"

- done

pg. 1976, line 2: "wavelengths" should be "wavelength"

- done

pg. 1977, lines 19-20: please clarify this sentence.

- tried to clarify the sentence

To correctly explain, what is meant here, several sentences are needed. And this would need to much space compared to the weighting of the statement made.

To find the reference orbit from, e.g. the 15 daily orbits it is not just sufficient to take of this orbits. For technical reasons it should be the orbit with the most measurements, to avoid unnecessary gaps

in latitudinal coverage. Furthermore every 2nd orbit has a slightly shifted scanning pattern, alternating the role of limb and nadir measurements to cover all latitudes with both measurement modes. Therefore, the reference orbit is build from the 2 orbits with the most number of measurements, one for the odd orbit numbers and one for the even orbit numbers and the geolocations of this 2 reference orbits are merged, to form the final reference orbit. The point here is, that not just the spectra are averaged, but there must be also information provided on the geolocation, which are used.

pg. 1978, line 5: try this "These features are not dependent on the orbit phase, but. . ."

- done

pg. 1980, line 24: you introduce undefined nomenclature, "E x B-Drift", please define these terms.

- The ExB is shortly introduced now.

pg. 1980, line 27: within 3 deg. latitude of what?

- of Mg<sup>+</sup>, done

All figures with latitude as the abscissa: The left and right abscissa labels are "-60" and "60", respectively. These should be "-90" and "90"

- This was not well enough explained. Figure 1 shows the latitudinal and local time coverage during an Orbit of SCIAMACHY. The highest latitude is 82°. High latitudes are passed twice, e.g. 70°N is passed at 11 and 21 hours, which are very different local times. To separate this, additional latitude bins are used for the ascending node (when the satellite moves northwards).

- As the first measurements of the sunlit part of the orbit are cut off, beside for Fig 9 (in the ACPD version), it appears to be reasonable to use 82° as the highest latitude, to avoid confusions and just to add a sentence to Fig 9 hinting to Fig 1 and the explanation of the latitude grid choice

- Figures 2, 3, 5, 6, 8, 11, 12, 17 and 18 have been changed

pg. 1983, line23: "(±200m)" should be "±200m"

- done

General: You could introduce the terms Southern Hemisphere (SH) and Northern Hemisphere (NH) early in the paper as they are used extensively.

- done

pg. 1985, line 18: You should briefly define the "Ring effect".

- done

pg. 1988, line 8: by "3.3 km step size" I believe you mean 3.3 km vertical resolution. Please clarify.

- done

-Averaging Kernels for the 1D and linear version of the retrieval algorithm have been calculated, showing nearly diagonal entrys for  $dx/dx_{true}$ . Comparisons with the full 2D and non linear algorithm indicate, that there have not been significant changes in the resolution. And the folding of the model results with the resolution function (approximately a triangle with a base width of 4 km), does not show significant changes in to the unfolded model results, as the metal layer width is wide enough to make this effect negligible, which is also shown in Fig 19 and 20.

Fig 19 actually does not show the global, but equatorial profile. This has been corrected in the text. In figure 17 larger differences in the Mg altitude can be found in the high latitudes region and the corresponding paragraph is changed.

## Answers to Report 2:

In the current manuscript the authors present the global distribution of Mg atom and Mg<sup>+</sup> ion concentrations based on SCIAMACHY limb observations. Data have been gathered over a total period of 4 years during which both species were observed in the 70 - 150 km altitude range every two weeks for 15 consecutive orbits (=1 day). This data base is used to construct the seasonal and latitudinal variation of both species which is subsequently compared to previous measurements as well as WACCM model results.

The distribution of metal species in the mesosphere/lower thermosphere region has recently attracted renewed interest since it is now understood that corresponding studies allow insight into such diverse processes like the meteoric mass flux into the MLT, the neutral wind circulation, ionospheric transport effects as well as interesting chemical cycles of the metal species themselves. The current study presents the so far most comprehensive global data set both regarding geographical and altitudinal coverage as well as total amount of data (and coverage in time). As such this is an interesting manuscript which clearly deserves publication in a general sense.

However, when it

comes to publication in ACP, I am a bit concerned about the scientific content beyond simply describing what the observed features are and how they compare to other data sources (i.e., independent observations and model data). My recommendation is hence that the scientific content of the manuscript should be expanded after which the manuscript should be re-reviewed but then certainly be published.

We thank the reviewer for his comments, We would like to point out that, this manuscript is to be published in the inter journal ACP/AMT special issue on "Limb observations of the middle atmosphere by space- and airborne instruments". Thus the discussion of measurement results is also important. We have expanded our discussion of the observations and the physical interpretation.

My major and minor comments are given below.

Major comments:

1.) My major and most important point of criticism is that the manuscript is very descriptive in its current form and makes little to no attempt to interpret the observations from a geophysical point of view. Obvious questions that should be addressed are for example what the geophysical factors are that cause the observed seasonal and latitudinal variations.

I agree with the reviewer, that we do indeed need to explain the geophysical reason or offer potential reasons. The observed latitudinal and seasonal variation in the model is already broached in the introductory section lines page 1974 line 7 to page 1975 line 22.

We added a part in the discussion, which picks up this discussion and give reasons for the geophysical behavior of the model.

(For the differences between the SCIAMACHY and Model results

please see the answer to the last point of the report), we also included a small chapter discussing coincidences between Mg/Mg<sup>+</sup> reduction and NLC occurrence.

I should acknowledge that there is some discussion of the observed latitudinal structure of Mg<sup>+</sup> in terms of the ionospheric fountain effect on page 1980 but I strongly believe that this is not the only feature of the data set that deserves discussion.

[This section has been moved to the discussion of the differences between model and measurements section now.](#)

2) In the same vein, when it comes to comparison to WACCM data the reader might be wondering what the main purpose of this comparison is.

[Clarifying introductory sentences have been added now.](#)

Is it presented to validate the retrieval results or vice versa? Or is it meant to be used to analyze cause and effect relationships leading to the observed morphologies? This should be clarified and a corresponding discussion should be added. For the time being, it also appears to me that the very short description of the model results does not justify to have the model comparison mentioned in the paper title.

[This paper represents the retrieved SCIAMACHY limb Mg/Mg<sup>+</sup> data products and the WACCM-Mg model simulations.](#)

[All other measurements, which we compare our results to are published somewhere else.](#)

[All 3 tables and roughly 1/4 to 1/2 of the figures show model results or the comparison between the model and the SCIAMACHY measurements.](#)

3) The manuscript contains far too many figures (with many sub panels).

[We thank the reviewer but point out that another reviewer asked for more figures.](#)

[Novel data sets and simulations are presented and discussed, plots have been used to show important aspects of the data and simulations and their comparisons.](#)

[To reduce the number of plots 3D\(x-y-color\) representation are used instead of 2D plots \(On the other hand more 2D plots are demanded?\). Due the printform heritage of journals it is unfortunately not possible to publish the plots with the many subpanels as videos. To reduce the amount of plots we only mostly only show results for one of the Mg<sup>+</sup> lines.](#)

[As an example, one might wonder why the authors show Mg<sup>+</sup> results retrieved from two different wavelengths.](#)

[To give the reader the chance to compare both results and draw his own conclusions on how significant certain details are etc., but we now removed the 279.6 nm lines results.](#)

[I understand that it is a strength of the work that both independently retrieved Mg<sup>+</sup> fields show consistent results, but then then this is certainly a purely technical result which might be a point for an AMT-paper but not so much for ACP.](#)

[Only figures for one species are shown now.](#)

[It is of course OK to mention in the text that both lines were used and that they yield consistent results, but beyond this I do not see the point to show these comparisons in this manuscript which in my view should focus on the geophysics and not on the retrieval.](#)

[There is no focus on the retrieval in this manuscript. All retrieval relevant issues are explained in Langowski et. al., AMT, 2014. In this manuscript we focus on the geophysical results and their explanations.](#)

[If the latter was the main intention of the authors, I think the manuscript should better be submitted to AMT.](#)

[As explained above this manuscript focuses on the observed behaviour of Mg and Mg<sup>+</sup> and our understanding and explanations of this behavior.](#)

4) While observational results are presented in many figures, it is striking that comparison to previous measurements from rockets and other space borne instruments is only done verbally.

[Plots have been added, please also see the comments in referee report #1.](#)

[This part of the manuscript is very difficult to read and would benefit from one or two appropriate plots, e.g., comparing mean rocket profiles \(plus their](#)

standard deviations) with corresponding mean limb observations and the same for the Minschwanner-results.

Error estimations, as part of retrieval details, have been presented in Langowski et. al, AMT, 2014. In the manuscript we pointed out which features in the retrieval results seem to be robust. Most of the rocket profiles now shown in figures also did not include error bars.

I certainly see that this would add even more figures to an already long list, but in this case it would make the manuscript easier to follow.

5) In its current form, the presented data are shown without any detailed information on retrieval errors and significance of presented features.

The sources for errors and the error estimation have been presented in Langowski et. al., AMT, 2014 paper.....(see point 4), and this is referred to explicitly.

Even though the data sets

contains a total of four years of data, it needs to be realized that the actual amount of data is not so large since Mg and Mg+ were only observed every two weeks for 1 day.

Please provide some information on errors and significance (e.g., which of the features in Figs. 2, 4, 6, 7, etc. should be taken as real?).

This actually has been discussed in the manuscript, and is summarized in the text:

I think any more comments and figures on this would only make the manuscript harder to follow and understand and just longer, or could be misleading.

Fig. 2 :

Mg peak altitude at around 90 km is significant, seasonal variation, are much smaller than the day to day or month to month variations, so this may not be captured correctly. FWHM of Mg layer is significant. Densities at the bottom are influence by systematic errors of the Filling in of Fraunhofer lines (Ring effect), depending on how strong this is corrected the lower peak edge might be too steep or not steep enough. For small density region above and below the metal layer, there is a systematic bias to higher than 0 densities, as the statistical error leads to an systematic bias there. Most other systematic issues do not influence the shape of the vertical profile (Fig 4), but could scale the vertical profile by a constant factor.

Fig 5. When carefully adding more measurements at high latitudes (this means checking every single day result by eye for retrieval artifacts) for Mg, we see a summer minimum, which is more pronounced than the strong month to month variations, which we think is mainly caused by errors, and this summer minimum, which is less pronounced than e.g. for our Na results, but can also be seen there and in the WACCM-Mg, Na and Fe.

Fig 6:

For Mg+ the situation is much easier than for Mg. Beside the feature shown in Figure 9 at high northern latitudes, which is also critically discussed, why it might be real or not, all other features of the high density region appear to be real and significant and we did a lot of checks, e.g. whether the latitudinal dependence of the peak altitude can also be seen in the raw data etc..

Errors are larger for the region above and below the maximum density region.

The sometimes high densities at the top of figure (at 150km) are most probably just artifacts of the retrieval (edge effects).

The change at 110 km, that the densities are higher at the equator at the same altitude than at higher latitudes is only shown for the average over all measurements, to reduce the statistical errors as far as possible.

It can be seen for both Mg+ lines but as the errors are larger, where densities are smaller it depends where to set the significance criterion to judge whether this effect is significant or not. However, if it is real, there is the equatorial outflow model which can explain it.

Also, please add error bars to the line graphs in Figures 19 and 20.

- The errorbar estimation from the Langowski AMT 2014 paper is added as text into the figure

description.

Minor comments (in order of appearance in the text):

p1974, line 16: that -> than

- corrected

p1974, line 26: km. -> km

- corrected

p1975, line 9: suggest: insert "of neutral Mg" after "change"

- corrected

p1975, line 15: condensation -> nucleation; add "are thought to" between "clouds" and "play"

- corrected

p1975, line 16: please add reference to review paper on PMC

- reference has been added

p1975, line 18: please add reference to role of meteor smoke/heterogeneous nucleation of PSC; E.g., Voigt et al., ACP 2005 and Curtius et al., ACP 2005.

- reference has been added

p1975, line 26: Please add reference to rocket measurements; e.g., the Grebowsky et al. paper you mention later

- referred to Section 5 later,

p1976, line 9: It might be better to refer to the journal publications of Scharringhausen and co-workers here; also reference should be made to the AMT-paper by Langowski et al. (2014)

- Citations have 2 reasons. The first reason is to acknowledge the work of authors. The second reason is to give the reader hints where to look best for the mentioned detail. In some cases these two references do not match.

Both the AMT-Paper by Langowski, as well as the Scharringhausen papers are acknowledged in the text, so there is no reason to use the reference here, if it is used at a more appropriated position in the manuscript. In the context of the sentence the citation of Scharringhausens Ph. D. work makes the most sense.

p1977, line 12/13: It might be worthwhile pointing out that the total amount of data is 100 days with 15 orbits each; this is still quite an impressive amount of data; but it is significantly less than one might suspect when just reading that four years of observations are used.

- I fully agree with this statement. Actually there are just 84 single days of data, and the reason for using all 4 years of data is to reduce the noise as much as possible, which would not be necessary if there was limb-MLT measurements every day.

I tried to formulate this better.

p1977, line 25 and below: So what? What is the reader to take home from this paragraph? Please explain why you mention this and where it will be important for this work.

- I added the "sunlit part" of the orbit. This is important, as it drastically reduces the coverage of the interesting northern polar latitude region. The one figure showing the results without omitting the straylight contaminated measurements shows very interesting transport features at the northpole, which also has been discussed. The question here is, whether these features are real, or artifacts from the straylight contamination.

p1978, line 20/21: I do not understand the meaning of the last clause; please explain in other words.

- explained differently and referenced figure 1. Figure 1 shows that at high latitudes there are very different local times for the northward (titled nightside) and southward moving part of the satellites orbit. This was separated, which shows this very interesting feature at northern high latitudes, just mentioned above.

p1980: I believe the whole discussion of the ionospheric fountain effect on the Mg<sup>+</sup> distribution should be moved to a discussion section where also other interesting features will be scrutinized; see my general comment above.

- Ok moved this part to the discussion of differences between WACCM and SCIA results.

p1981, line 18-20: Any geophysical explanation/hypothesis for this observed morphology? -> Discussion section.

- Partly already explained in the Introduction. For the discrepancy that VCDs are not highest at summer high latitudes, the Cleft Ion Fountain (Lockwood 1985), which may cause transport of Mg<sup>+</sup> from the dayside pole to the magnetosphere and to the nightside polar regions is discussed in the paper.

p1982, line 25: peak densities and altitudes?

- No actually the peak altitudes do not fit that well. The Rocket measurements typically show the peak altitude at around 95 km, independently from the latitude.

The new added Plots, which show the results of rocket measurements typically only show MgII at higher altitudes, at seldom occurring sporadic events.

p1983, line 16: Please mention the method used by Minschwaner et al.

- a combined NO and Mg<sup>+</sup> retrieval from satellite limb spectra.... actually this is mentioned in the text.

p1986: When introducing the Mg/Mg<sup>+</sup> chemistry a corresponding schematic might be useful.

- a reference figure is referenced now.

p1986, line 17-19: Please give a geophysical justification for scaling the Mg ablation flux by a factor of 1/15 when compared to the Na flux. At least this requires some discussion.

- a discussion has been added.

p1987: General comment on comparison of model and SCIAMACHY results: Shouldn't the averaging kernel of SCIAMACHY be applied to the WACCM-data before comparison? This would make arguments about different resolutions obsolete.

- Resolution and Averaging Kernel is the same quantity.

- See same discussion for referee report #1

- The result is, that the metal layers are wide enough, that applying or not applying the averaging kernels of SCIAMACHY to the WACCM results results nearly in the same profiles, which is shown in the comparison figures.

p1988, line 22-24: Any idea what the geophysical explanation for this discrepancy is?

We note that the lack of a full electro-dynamical treatment in WACCM may be a cause of some of these discrepancies (line 25-27), and state (line 24/25) that there is slightly better agreement with the rocket data - which allows for some uncertainty in the SCIA retrievals of Mg<sup>+</sup>.