Interactive comment on "Global investigation of the Mg atom and ion layers using SCIAMACHY/Envisat observations between 70 km and 150 km altitude and WACCM-Mg model results" by M. Langowski et al.

Anonymous Referee #2
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In the current manuscript the authors present the global distribution of Mg atom and Mg+ 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 bite 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+ reduction and NLC occurence.

I should acknowledge that there is some discussion of the observed latitudinal structure of Mg+ 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+ 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+ lines.

As an example, one might wonder why the authors show Mg+ 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+ 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+ 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 ben-
efit 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
Minschwaner-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 included 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 refered to explicitely.
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 an 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 wortwhile 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+ 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^+ 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 occuring sporadic events.
p1983, line 16: Please mention the method used by Minschwaner et al.
- a combined NO and Mg+ retrieval from satellite limb spectra.... actually this is mentioned in the text.
p1986: When introducing the Mg/Mg+ chemistry a corresponding schematic might be useful.
- a reference figure is referenced now.
p1986, line 17-19: Please give a geophysical justification for scalilng 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^+.