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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

Received and published: 22 April 2014

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.

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[Interactive Discussion](#)

[Discussion Paper](#)

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.

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 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.

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. 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

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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.

3) The manuscript contains far too many figures (with many sub panels). As an example, one might wonder why the authors show Mg+ results retrieved from two different wavelengths. 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. 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. If the latter was the main intention of the authors, I think the manuscript should better be submitted to AMT.

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. 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 Mischwaner-results. 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. 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?). Also, please add error bars to the line graphs in Figures 19 and 20.

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Interactive Discussion

Discussion Paper

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

p1974, line 16: that -> than

p1974, line 26: km. -> km

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

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

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

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.

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

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.

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.

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

p1980: I believe the whole discussion of the ionospheric fountain effect on the Mg⁺ dis-

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tribution should be moved to a discussion section where also other interesting features will be scrutinized; see my general comment above.

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

p1982, line 25: peak densities and altitudes?

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

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

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.

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.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 1971, 2014.

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