

## ***Interactive comment on “Retrieval of atmospheric parameters from GOMOS data” by E. Kyrölä et al.***

### **Anonymous Referee #1**

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Review of Manuscript ACP-2010-27

Retrieval of Atmospheric Parameters from GOMOS Data

by Kyrola et al.

This manuscript discusses in detail the algorithms used to process the GOMOS stellar occultation data to retrieve profiles of O<sub>3</sub>, NO<sub>2</sub>, NO<sub>3</sub>, O<sub>2</sub>, H<sub>2</sub>O, and aerosol extinction. The GOMOS data set is unique in that it is the most comprehensive set of stellar occultation measurements available to date, and this paper is of general interest to the atmospheric science community.

The paper is well written and very thorough in its description of the GOMOS algorithms, including discussion of a number of difficulties and problems with the data. I can find no major omissions or errors in the paper – I recommend that it be published essentially

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as is. However, the following observations and comments are provided for the authors to consider in strengthening the paper.

Specific Comments & Questions:

#### Section 4

Regarding the discussion of Figures 8 & 9 – vertical sampling – I don't think the temporal sampling frequency is stated. I may have missed something, but it is important to state this number explicitly. Reference is made later to a 0.5 s exposure – is this the measurement frequency?

Page 10156, line 13 – The dark limb solar zenith angle criteria is confusing as written – suggest you either say it's 107 deg or 108 deg.

#### Section 5.4

If the dark charge in individual pixels can vary significantly on timescales of seconds then it's hard to see how a single dark measurement every orbit can effectively correct for the variation. I assume that this RTS phenomenon must be modeled and included as a large component of the potential random retrieval error.

#### Section 5.6

Since no correction is done for either the internal or external stray light, does this imply that the GOMOS team is confident that these effects are not a significant source of error for the night-time occultations, or simply that you do not know how to effectively correct for them? If data users are told to use the illumination condition flag as a possible stray-light indicator, can you estimate for them the potential magnitude of the error in these products?

The discussion of background removal for bright limb occultations is presumably given for completeness. I think it is stated in the paper that these data have not been deemed to be of sufficient quality to release publicly, is that correct?

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### Section 9.3

There is no real discussion of the estimated accuracy of the high-resolution temperature retrieval from the photometers (except the single comparison plot to ECMWF in Figure 23). Has this retrieval been validated? Presumably, since it's not used in any way in the level 2 processing, the assumption is that it is not as accurate as the interpolated temperature/pressure profiles from the ECMWF analysis.

### Section 11.2

Regarding the decision to fix the neutral density (Rayleigh scattering component) to the ECMWF rather than retrieving it from the data – the fact that the retrieved total density shows a large bias relative to ECMWF (the magnitude of the bias is not quantified, but presumably it's large enough that the retrieved density is not believable) could mean that this component is absorbing a systematic error in the data. Fixing the Rayleigh to ECMWF could translate that error into other components, most likely aerosols since they are spectrally similar in extinction. In other words, retrieving an unrealistic total density might make the other species more stable. Has this been considered, or quantified?

### Section 11.3

This is the only section of the paper I find unsatisfying. The retrieval of O<sub>2</sub> and H<sub>2</sub>O from these measurements is indeed quite complicated, as the authors point out, but the details presented here on how this algorithm works in practice seem simplistic and vague. The effective transmission is not only pressure-dependent (and I think the dependence is more than “slight”, as stated) but temperature-dependent as well. It's hard to believe that you can capture this complicated dependence by using a few simple reference atmospheres. What is the nature of the “background” fit by the last two terms in Eq (44)? At these wavelengths you need to account for ozone, Rayleigh and aerosols in addition to the target gasses. Presumably the ozone is constrained from the UV/visible retrieval, and the ECMWF data is used for Rayleigh, but this should

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be mentioned. So I assume that this background term accounts for aerosols only? Finally, I think it would be useful to the reader to summarize the accuracy of these retrievals at a top level – can GOMOS indeed retrieve O<sub>2</sub> and H<sub>2</sub>O profiles that are scientifically useful?

Minor comments and corrections:

Section 4, sentence 2 – This sentence reads awkwardly. Suggest transposing a few words, e.g., “This is especially important for limb viewing instruments where the accuracy of the vertical geolocation directly affects the accuracy of vertical profiles.”

Section 4, page 10156, line 8 – The text should say “Table 1 provides. . .” rather than “The following table provides. . .” so that the reader knows exactly what table you’re talking about.

Figures 13 – 15 lack a label and units on the ordinate axis.

Section 6, page 10163, line 26 – I am not sure what this sentence means. The word “punctual” seems out of place here. Can you please make it clearer?

Figure 20 – the y-axis should be labeled. Are these absolute or relative (%) differences?

Figure 22 – the units for the red lines (vertical profiles) should be cm<sup>-3</sup>, not cm<sup>-2</sup> as stated.

Figure 23, left panel – the caption should specify the wavelength associated with this aerosol extinction. Also, there is no scale on the x-axis.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 10145, 2010.

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