Journal: ACP
Title: GOMOS O3, NO2, NO3 observations in 2002-2008
Author(s): E. Kyrölä et al.
MS No.: acp-2009-839
Special Issue: GOMOS (Global Ozone Monitoring by Occultation of Stars): data processing, geophysical validation and scientific results

Authors' response to referees

We thank both the referees for very insightful questions and comments. They have helped to improve the quality of the paper. In the following we detail answers to the individual questions posed by the referees. In addition to the changes originated from the referee comments, we have updated the references to the GOMOS special issue and made small text corrections.

Anonymous Referee #1

p 2174 l 15-18: The authors should provide a rationale (or reference) for their choice of median value error.

Author response: The special feature of stellar occultation data is that the targets (stars) have very different intensities and this leads to large variation of signal-to-noise ratios. The random noise in retrieval products vary therefore widely. A reasonable way to calculate statistical quantities from noisy data is by weighting measurements with the inverse of estimated variances. However, in the GOMOS processing version that have been used in the current work error estimates are not always reliable. Therefore, we have not used this approach but the median and interquartile range instead. In addition, there is a question of outliers. Stars are faint sources of light and especially with weakest stars low signal-to-noise ratios make GOMOS retrieval difficult. In this situation outliers are frequently observed in the retrieved constituent profiles. The median and interquartile range are robust estimators against outliers. A discussion and comparison between different estimators for GOMOS data can be found in *Kyrölä, et al., 2006* (cited in the article). We have added some more text on this issue to the present paper. We have also added the definition for the error of the median.

p 2178 l9-12: The authors state in the text that Fig. 7 shows the standard deviation of the zonal mean time series, while in the corresponding figure caption the interquartile range relative to the median value is mentioned. In the next sentence it is stated that the variability of the estimated retrieval errors has been subtracted. Do you mean the standard deviation of the retrieval errors (i.e. the average random retrieval error)? Do you subtract this quantity in order to "isolate" the natural variability? Since natural variability and random retrieval errors add quadratically, this subtraction might be misleading. Another possibility would be to highlight areas in the plot showing a variability greater than the random retrieval errors as areas with significant natural variability (without subtracting them).

Author response: We have meant the interquartile range multiplied with the factors mentioned in page 2174, lines 14-18 and the text is corrected.

The observed variability comes from natural variability as we collect data from a 'box' of time, latitude, longitude. An additional source of variability is uncertainty in GOMOS retrievals. In a monthly 20-degree latitude belt the observed variability for ozone exceeds the variability from the retrieval errors by a factor of 10 (see also discussion after Fig. 7). If we measure variability by the std and if we assume that the two processes (natural and retrieval) are statistically independent we can get the total variability as a square root of the quadratic sum of the variabilities of the two processes. We have assumed that the same quadratic law can also be applied when we estimate variability using the interquartile range. This estimate of variability is robust against outliers and it can be considered to measure the variability of outlier free distribution. We have added some more text about these issues.

p 2184 l 20-23 Can you provide a (speculative) explanation for the positive solar NO2 response at northern latitudes in the upper stratosphere? Might this be related to NOx polar winter descent?

Author response: The reference points to NO3 section but we assume that NO2 is meant. In *Seppälä et al. (2007), Geophys. Res. Lett.* (referenced in the article) it has been shown that NO2 in the upper polar stratosphere correlates strongly with geomagnetic Ap-index. In the present work the positive response concerns mid-latitudes. We could speculate that the solar cycle dependence can result from the transport of polar air mass rich in NO2 to mid-latitudes induced by planetary wave activity and breaking of polar vortex occurring in winter and spring. But can this process affect the upper stratosphere? The NO2 amount is also depended on many photochemical reactions but how the latitudinal asymmetry could arise? A detailed investigation using modelling is needed to pinpoint the reasons. An earlier work by *Vitt and Jackmann in JGR, 101, p6729 (1996)* could be a good starting point for such a study.

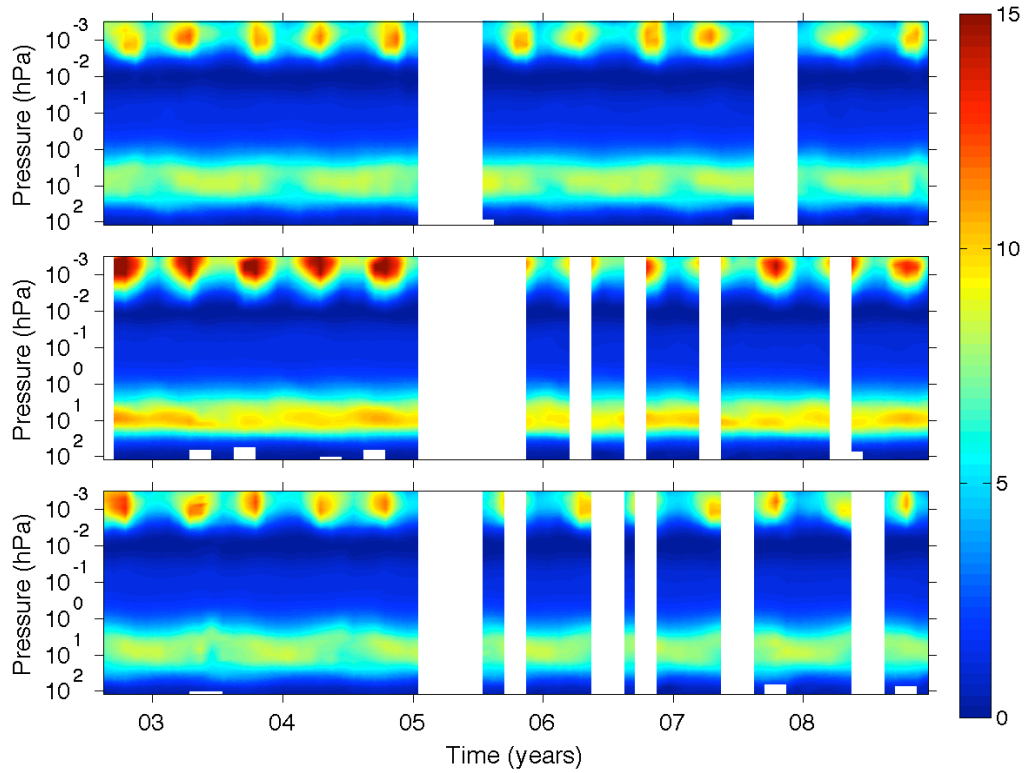
Anonymous Referee #2

Specific comments

1. The climatologies are presented in number densities as a function of altitude and latitude which is the natural first choice of quantities for GOMOS observations. Sometimes, however, other quantities such as VMR on a pressure (or potential temperature) vertical grid make more sense and helps interpreting the underlaying dynamical and chemical processes. Additionally, these quantities are usually needed for comparisons to outputs from atmospheric models. Since conversions from one quantity to another is non-linear, they have to be done for individual profiles rather than on climatological data which means that they are out of reach for the end-data user. Hence, I strongly recommend the authors to provide the GOMOS climatologies with other quantities as well. The paper is a bit figure-heavy so there is no room for additional figures (unless they are particularly interesting) but the features of the climatologies for the other quantities should at least be commented and the data put on the GOMOS website.

Author response: The main reason to use geometric altitude and number densities is that these are natural outcomes from the GOMOS retrieval. We can present results also, for example, in mixing ratio on pressure surfaces but this introduces two quantities, pressure and neutral density, that GOMOS does not measure. As suggested by the referee, we will post the monthly time series

figures (but not the analysis figures) on the FMI's GOMOS/OSIRIS website. The ozone figure is shown below.



2. The general features of the climatologies and their components are well described in the paper but there is a lack of discussion of the deviating data. What could, for example, be the reason for the large residuals of the fitted O3 time series (Fig 9) in some years and what are the sporadic high values of NO2 in the equator belt at around 20 km (Fig 15). Is it only observational noise/bias or something real? Deviating results may be the door to new scientific findings and should not be ignored.

Author response:. Deviations from the regular variations could indeed open doors to new scientific findings. The investigation of deviations is, however, quite difficult because of considerable amount of noise in GOMOS data. We have already seen that ozone data from weak and cool stars lead often to values that cannot be physically right. Therefore, a careful analysis of the fit residuals will be an important task in future studies of GOMOS time series. We think that we should also understand the behavior of the different time series components starting from physical and chemical processes. We hope that this can be achieved by comparing these time series with middle atmosphere models. This is a large task even if most of the observed phenomena are probably already analysed in literature

3. The diurnal influence is studied through a few simulations with a chemical box-model for the three latitude belts. I agree that, WITHIN THE BELTS, the diurnal influence on the GOMOS measurements are small (a few percent at the most) which does not justify any correction. But when you compare DIFFERENT LATITUDE (BELTS), the diurnal effect could be significant. What is approximately the maximum diurnal influence difference between latitude 0 and 50? If this is large, figure 13, 21 and 27 would be hard to interpret (since latitudinal variation is mixed up with diurnal

variation) and you should consider scaling the climatology data to the same local time using the chemical box model.

Author response: In our data analysis we have tried to avoid including anything else than GOMOS measurements. Therefore, no mixing ratios, no pressure altitude grid or diurnal corrections by models. This is because we try to avoid the situation where the results of this paper get out of date only because some model results turn out to be inaccurate. But the concern expressed by the referee is valid and we have overlooked the effect of diurnal variation for latitudinal distributions (for example, in Fig. 13, 21 and 27). In order to estimate the variation between different latitude belts we have convoluted the normalised local hour distributions of Fig. 2 and the diurnal variation curves of Fig. 8 and Fig. 16. If the local hour distribution and the constituent diurnal variation curve were flat this correction factor would have the value 1. In our case, the correction factors for ozone at different altitudes and for NO2 at 30 km and NO3 at 40 km are given in the table below. As anticipated by the referee, these modifications are important (especially for NO2 and NO3) to keep in mind when interpreting the latitudinal figures. These results and discussion have been added to the appropriate places in the article but the figures themselves have not been modified.

Constituent	Correction 5S-5N	Correction 40N-50N
Ozone at 90 km	0.92	0.97
Ozone 70 km	1	0.97
Ozone at 50 km	1	0.98
NO2 at 30 km	0.76	0.92
NO3 at 40 km	0.71	0.97

4. To avoid a large effect from outlayers, the authors have chosen the median operator. This is probably a wise decision but the actual distributions should be looked at to be able to say this? Are there outlayers and do the data follow a normal distribution? For low concentrations data are probably more likely to be log-normally distributed. If so, are the median operator valid? Please comment on this in the text. Also the "variance" of the median operator (sections 2, line 15 and onwards) is not common knowledge. Explain the terms in more depth or give a reference.

Author response: By a complete simulation of the ground segment we could establish the statistical distribution of data from retrieval. This has been done several times for GOMOS at various levels of sophistication and the outcome has been always close to 'normal'. In real life measurements it is impossible (using GOMOS data only) to deduce the distribution of retrievals as natural variation is always involved. If we had a perfect validator instrument and very well co-located measurements (in space and time) we could get a characterisation of the retrieval distribution. The discussion and comparison between different estimators for GOMOS data can be found in *Kyrölä, et al., 2006* (referenced in the article)

We are not constraining data to be positive and therefore log-normal is not a relevant distribution.

We have tried to improve our presentation of statistical estimators in the appropriate places. We have also added the definition for the error of the median.

5. What additional filtering have been done to the data other than the selection of stars for O3? What does flagged data points (P2174,L4) mean? Also, is there a lower limit for the number of profiles within an altitude/latitude/time box for when a statistical analysis is not computed? Figure 1 says something about a two measurements limit for plotting, does this hold for monthly/daily medians as well? This is crucial information for being able to reproduce the study and must be provided.

Author response: For all profiles, data marked by GOMOS retrieval flags have been removed and the interpolation has been done only using valid data points. The lower limit of cases for statistical studies is mentioned in p2174, lines18-. Figure 1 limit is different because we aim to show here the overall coverage of measurements. This figure has been corrected using 1 as the lower limit instead of 2.

6. The difference between time series, monthly means and daily means is a bit unclear. Please be more consistent with the terminology.

Author response: We have clarified the usage of these terms.

7. In section 2, the results from previous validation studies of O3, NO2 and NO3 are presented very differently. For O3, results from the studies are given but neither for NO2 nor O3. Please give the same type of information for all the species. Maybe present the results in a table?

Author response: As mentioned in the text, we hoped to refer to a special GOMOS validation article in this special issue. But a separate validation paper never materialised.

So far the validation of GOMOS measurements have concerned only with nighttime measurements. There are only a few validating instruments working during nighttime. Daytime validating measurements can also be used if the diurnal variation of the target constituent is known to be small. NO2 and NO3 have strong diurnal variation but ozone shows diurnal variation only above 50 km. Therefore, the majority of GOMOS validation is targeted to the stratospheric ozone. For NO2 we have intercomparisons to other satellite instruments. For GOMOS-MIPAS comparison, see *Verronen et al. (2009)* (referenced in the article). For GOMOS- HALOE see *Hauchecorne et al. (2005)* (referenced in the article). For validations as it is customarily understood (ground based, aircraft and balloon measurements) we have only very limited comparisons of NO2 and NO3 in *Renard et al. (2008)* (referenced in the article). We are not ready to issue any definite numbers for NO2 and NO3 validation.

8. It is common to include an autocorrelation term in time series fits since the concentration at one month (day) is, to some extent, a function of the previous month/day. The authors should declare why they don't have this term in equation 1. In addition, all the terms in equation need to be clearly defined in the text just after the expression. Right now it is not even clear that z means altitude...

Author response: The inclusion of autocorrelation affects mainly the error analysis. This extra analysis is warranted for sophisticated long-time trend studies where the error of trend is really important. In our case we have checked autocorrelation values to be nearly as from a normal

distribution (this has been mentioned in page 2177, line 5). We think this approximation is not important for our main focus of the paper, which is to reveal the various components in GOMOS time series.

We have been more careful with the variable definitions.

9. The paper is a bit figure-heavy! Figure 19 and 25 are redundant since that information can be deduced from Fig 18 and 24. I suggest to remove these. Also, why is there not a figure of the O3 constant factor like for NO2 (Fig 18) and NO3 (Fig 24)?

Author response: There are indeed many figures in this article. But let us look the efficiency of our collection of figures. There are 28 figures that summarise results from about 280 000 carefully performed and selected satellite measurements. So, on average, one figure's burden is to summarise 10 000 measurements. We think the compression of information is good enough to warrant all the figures selected.

As the referee points out, there are still some redundant figures because the constant term figures for NO2 and NO3 are shown by a color surface plot and line plot. We have the experience that it is difficult to read accurately a vertical structure from a color plot and to read latitudinal variation from a collection of profiles. In the case of ozone, we dropped the color plate by a bad judgement. We will add this figure to the article. The figure draft is below.

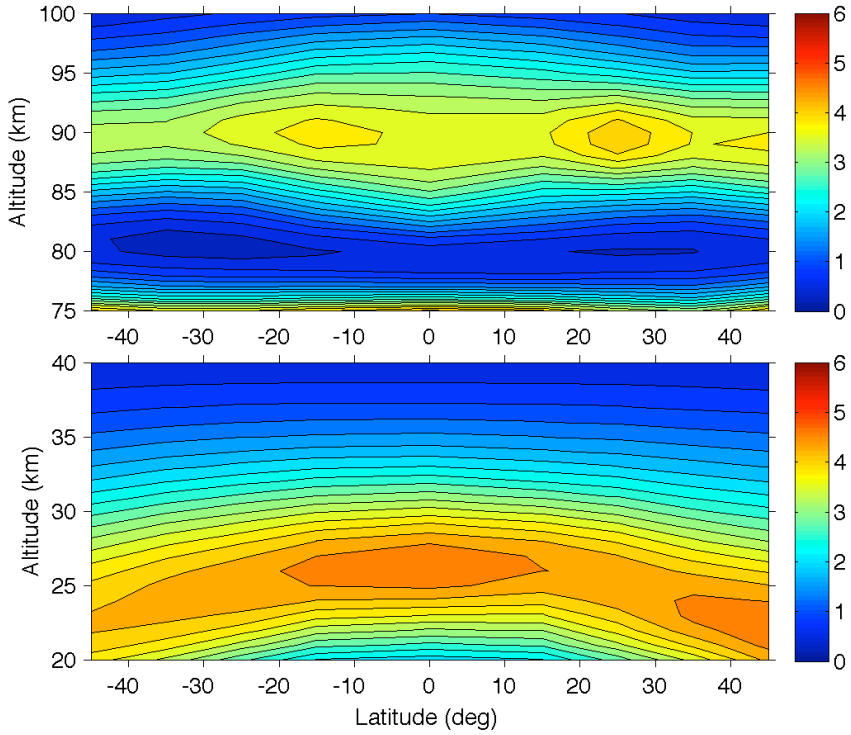


Fig. X: The constant factor c(z) in the O3 time series as a function of latitude and altitude. Upper panel: MLT with the scale 10^{8}\,cm$^{-3}. Lower panel: Stratosphere with the scale 10^{12}\,cm$^{-3}

10. There is a confusion with the word error which appears multiple times in the paper (e.g P2175, L15). I would prefer the word uncertainty or even better precision, when referring to the statistical repeatability of a measurement/operator since error is not well defined (could mean noise (random) , bias (systematic) or even malfunction).

Author response: We will change the text to be in line with the usage in *Tamminen et al., GOMOS data characterization and error estimation in the GOMOS special issue (tp://www.atmos-chem-phys-discuss.net/10/6755/2010/acpd-10-6755-2010.pdf)*. We have difficulty to accept the use of precision because we think that it can be really calculated only in computer simulations or in validating studies where the validator is assumed to be more accurate than GOMOS.

11. On page 2180 and line 26-27 you conclude that the QBO signal is almost zero in the SH for the second ozone maximum but 17-18% for NH and mid-latitudes. This seems to be an interesting result but is not investigated or discussed further in the paper. If this is a new finding, you should probably highlight it in the Abstract and/or Conclusions ?

Author response: We do not show QBO in MLT so this must be solar term in the MLT. The solar term result has already been highlighted in the abstract and conclusions. Without modelling work it is difficult to have any more extensive discussion.

12. In the conclusion the authors refer to a website where plots from GOMOS yearly climatologies can be found. Nice user-friendly interface, but why are the climatologies not presented in the monthly-type way like in the paper? Also the VMR plots of NO2 and NO3 look really noisy which needs a comment.

Author response: We will add similar plots to our web-site. The NO2 and NO3 noisiness: Perhaps you are referring to the white space that is used to mark negative or missing data. We will improve our info text on the web-site.

13. The statement in Abstract and elsewhere: NO3 distribution is controlled by temperature is a bit strong. To me, it is not clear that you have shown this by the results from GOMOS in this paper. In any case, NO3 must at least also be said to be controlled by temperature AND OZONE. You have also found that the relation breaks down at higher altitudes. I recommend a vaguer statement about this or to remove it from Abstract and Conclusions.

Author response: We have changed these statements to vaguer side.

Technical corrections:

- (P2170, L18) Missing word: ...early winter ozone MAXIMA at...
- (P2170, L19) English: ...which results IN...
- (P2171, L15) What is meant by ...UV-visible spectral range, detector 1+2?
- (P2171, L26) English: ...in THE data
- (P2172, L17) Logic: negative bias is not well defined. What data set is higher/lower?
- (P2174,L7) Wording: It does not feel right to say that zonal variations are ignored. Please reword.
- (P2179,L17) Word: I have never seen the unit Dobsons with a genitive 's' before. I think it should be Dobson UNITS?
- (P2183,L5) English: ..symmetric with RESPECT TO the equator.
- (P2183,L22) Error: I read the solar term to a maximum of around 30% not 20%...

• (P2184,L6) It is not clear what this asymmetry refers to.
• (P2184,L12) What does this 3.5% variability concern? You are a bit too short here...
• (P2184,L22) Word: iand should be AND
• (P2185,EQ3) Please explain what ρ, b12 and b9 means in the text below the equation.
• (P2185,L11) Missing word: ...are nearly IN equilibrium
• (P2187,L3) Missing word: .. there exist A recent climatology...

Author response: All these technical corrections have been done.

• (P2184,L14) Why suddenly southern mid-latitudes when the other species are for northern latitudes?
Author response: The annual/semiannual cycle is stronger in south. And we would like to be as much as possible north-south neutral.

Figures:

• (Fig 1) Missing x-axis unit. Should read: month of year. Also the latitude unit is missing on the y-axis, should be (deg)
• (Fig 2) How can the local hour be above 24?!? Limit the x-axis to 24 for clarity
• (Fig 4) The caption indicates that the x-axis unit is in MONTHS but the tics of the axis is in YEARS (03, 04, 05 and so on) which is confusing. I suggest simply to change the x-label to Time (years) and say in the the caption that the figure shows monthly median values. The figure will also look better if the space between the subplots are reduced. The same comments are valid for figures: 5, 6, 7, 15 and 22 .
• (Fig 6) Missing unit of the y-axis. (deg)
• (Fig 10) Missing unit of the x-axis. (cm−3)
• (Fig 11) Double the in the caption
• (Fig 17) Double scaled by in the caption
• (Fig 19) Redundant, can be removed. See comment above
• (Fig 25) Redundant, can be removed. See comment above
• (Fig 27) Missing unit of the y-axis. (deg)

Author response: All the figure correction requests have been done except the ones listed below:
-Fig 1. The x-axis has been explained in caption because the figure is already rather crowded.
-Figs 19 and 25: Figures not removed. See discussion in Specific comments.