Response to Referee #1

We thank reviewer for his energy to review our paper. We greatly acknowledge his comments and suggestions that helped us to improve the quality of our paper. Below, we present the detailed replies to each comments. Note: original comments of the referee are given in bold.

Substantive comments:

The first is that the authors seem unduly wedded to discussing and showing their results in the form of a slant column density. While this may be appropriate when comparing to their correlative measurements, it is a significant barrier to the comprehension and use of these results by the wider atmospheric chemistry community. Given that they clearly have the capability, I would strongly encourage them to recast much of their discussion in terms of either concentration in molecules per cm^3 or, better still, as a volume mixing ratio. The latter would allow direct comparison to ClOx/ClOy measurements from other sensors (and hence to studies of chlorine partitioning etc.) and far easier comparisons to models. Granted, a vmr calculation requires knowledge of temperature and pressure, but estimates of these from meteorological analyses are readily obtainable, and uncertainties in these, while non-negligible, would probably not significantly impact the resulting vmr estimate. Specifically, I recommend the discussion in section 5 be recast along these lines, and that figures 5,7,8,9 and 10 are converted to these quantities

Response: The question of the use of slant column densities instead of vertical profile of concentration is quite relevant. You are right to say that the use of OClO concentrations profiles would increase the number of potential users. However, we have decided to keep the SCD in the revised version of our manuscript for several reasons that we detail hereafter. First of all, even though it was originally intended to retrieve OClO using single GOMOS measurements, it turned out impossible to do it because of the low signal-to-noise ratio of a single GOMOS measurements. Thus, OClO is not retrieved with the operational processor and has never been included in the official distribution of GOMOS level 2 data. Our OClO product is an off-line product and, as such, to deliver them in the form of SCD with "reasonable" error bars appears to us to be the best thing to do. Secondly, the retrieval of the OClO product is based on a statistical analysis of several co-located GOMOS measurements. We have to keep this in mind. The retrieval is not based on a single measurement, a preliminary step is required to build a "virtual" measurement that we will use to retrieve OClO. This additional step is a first source of uncertainty. The second step is the spectral inversion (DOAS) used to retrieve the SCD of the different species involved in the attenuation of the radiations. Here again, this step implies some uncertainties. Thereafter, it is always possible and easy in the case of GOMOS to perform a spatial inversion to retrieve vertical profile of concentration but the error bars become too large. It is difficult to be confident with a product with very big error bar. We are at the limits of the possibilities of the GOMOS instrument. Only the SCD provides error bars that make the product usable for seasonal studies, latitudinal studies,… The SCD relative errors extend from

about 5% to 70% at some levels. A second panel inserted to Figure 5, shows the vertical profile of the SCD relative errors. On the other hand, the relative errors concerning the vertical profiles of OClO concentrations are generally greater than 60% and can reach about 180%. These values are too important to use scientifically the OClO concentrations. Nevertheless, we have used vertical profiles of concentration to perform the comparison with the balloon observations (Fig. 6) because we have only the concentration profiles derived from these balloon measurements. A warning has been added in the text explaining the limitations of this comparisons which is in fact a simple verification. Moreover, Figure 9 has been removed because it shows the anti-correlation between NO_2 and OClO in terms of concentration. It is a nonsense to keep this figure in the text and to affirm that OClO concentration is not a "good" product.

A small paragraph has been added at the end of section 3 to explain why we do not use the vertical profile of concentrations. We have also decided to change the title of our manuscript: "OClO slant column densities derived from GOMOS averaged transmittance measurements".

My second major comment centers on the discussion on page 3524, starting around line 22 ("Overall, the conclusion..."). Given the poor comparison with SALOMON, I think you need to say more. Even though not all the comparisons are encouraging, there is still presumably information in the GOMOS temporal and geographical variability – the main results of your paper. Can you do some kind of bottom-up estimate of how reliable this information is? A harsh reviewer might try to discount any features in your results smaller than 0.5e8/cm3 (the peak size of the GOMOS/SALOMON bias). You would need to make such "stability" arguments to counter him/her, and would presumably win those arguments, so why not make them here anyway.

Response: Agree. We have tempered our conclusions. This section is just a simple verification and it has to be considered as such. It is not a complete validation (that will be done later). Thus, we have added a sentence at the beginning of this section explaining this :"… as a result, the following discussion should be considered as a simple verification and not as a validation of the GOMOS OClO product."
The conclusion of this comparison section has been modified: "the conclusion is that our OClO product is of sufficient quality for some scientific uses such as seasonal or latitudinal studies and needs a more thorough validation for some others (comparisons with models for example)."

Finally, I did not see figures 9 or 10 discussed in the text. Either talk about them or delete them. Any discussion of them would probably need further review, so it's probably easiest to delete them.

Response: Figure 9 is about the anti-correlation between OClO and NO_2 concentrations. It has been deleted for consistency with our choice not to perform vertical inversion. Figure 10 (figure 9 in the revised article) is well discussed in the text (section 5.2).

Minor comments:

- Abstract

Line 17: Be more quantitative than "generally satisfying" (this is the abstract after all)
Done. We have added the values in brackets.
Line 21: Similarly, quantify the "strong concentrations"
Done in brackets also.

- Page 3513
Line 3: "physico-chemical" -> "physical and chemical": Done
Line 4: "It appears that the halogen..." -> "Halogen" : Done
Line 10: "toward" -> "into" : done

- Page 3514
Line 1: Insert "wintertime" before "permanent" : done
Line 4: Consider defining the polar vortex a bit for the less familiar reader.
We have defined it in the text: "a large-scale region of air that is contained by a strong jet stream that circles the polar region"
Line 17: This discussion is a little disjointed. You begin by saying there haven't been many measurements, but then your "for example" list gives what feels like a large number of cases, with no comparable list for NO2 for comparison. I suggest rephrasing by turning it the other way round (something like: "Previous OClO measurements have included.... This represents a very small and disjoint collection of observations compared to the large array of [space / airborne / ground / etc., delete as appropriate]observations for species such as NO2.""
Done, we have used bullets to list more clearly the previous OClO measurements, taking into account your suggestions
Line 24: "In this study" -> "In our study": done
Lines 27 - first 3 lines of next page. This discussion is awkwardly worded. You introduce OSIRIS and SCIAMACHY but then don't really say why you're not discussing them. The SAGE discussion is clearer, but again the English is clumsy.
This discussion has been reformulated in the bullet list. We have added a sentence to explain why we are not using OSIRIS and SCIAMACHY: "These measurements will not be used in our comparison study because GOMOS perform nighttime measurements. The comparison between these measurements performed under different illumination conditions requires the use of photochemical box--model and will be done later."

- Page 3516
Line 17: "scintillation is right" -> "scintillation, while correct ": done
then Line 17: add comma after "plane)" and replace "but" with "is" so: "plane), is": done
Line 26: "is in fact made up of" -> "comprises": done

- Page 3517
Line 6: "algorithm" -> "algorithms": *done*
Line 19: "written previously" -> "discussed above": done

- Page 3518

I think the "bullets" discussion would be easier to write (and thus easier to read) if you fused #2 (temporal) and #3 (latitudinal) together into one discussion.: done
Line 13: Add "dark" or "low" before "straylight"? according to GOMOS scientific advisory group recommendations, we have decided to keep the usual denomination of the illumination conditions.
Line 15: "These measurements are supposed" -> "Such measurements can"? : done

- Page 3519
Line 8: "should be" -> "are"? : done
Discussion around line 15: Why not use some coordinate like PV or equivalent latitude to get round this problem completely? It would also enable far more useful comparisons with models etc.
You are right about the use of dynamic coordinates (PV or equivalent latitude and also potential temperature for the altitude grid). Nevertheless GOMOS products are delivered with geolocation data corresponding to geographical latitude and vertical grid in kilometer. Considering the huge amount of GOMOS measurements, we have decided to not compute PV, equivalent latitude and potential temperature. Instead, we have used geographical latitude and altitude in km and analyzed the data set as explained in the text to ensure the homogeneity of the binned data sets.
Also, what happens to the signals when you look across the vortex edge? Is that a "third class" of observations?
This is a particular case of observation and it is difficult to detect it by looking at the transmittance distributions. In such a case, extinctions along the line of sight are due to vortex air (poor in NO_2) and non vortex air (rich in NO_2) and the effects of these are merged. Nonetheless, the statistical analysis (specifically the outliers detection/rejection) allow to overcome somewhat this problem. We have added in the text a sentence explaining this point.

Line 26: Presumably this weighting is by noise? It should be stated.
Done: “we combine transmittances weighted with respect to their estimated measurement errors”.
Also, please state why the median was chosen rather than the mean?
Done: “the weighted median transmittance is calculated instead of the mean because the median is known to be more robust against the presence of residual outliers”

- Page 3520
Line 1: "values" -> "value": done

- Page 3521
Line 16: "differences" -> "difference" (or "is" -> "are" above) : done (singular expressions chosen)

- Page 3522
Line 14: add "+" before the second "10" for symmetry (or say within +/-10%): done.

- Page 3524

Line 1: Perhaps give a rough percentage for "above". *:* We are sorry, we do not understand this comment because we have not written the word “above” neither in the first line of page 3524 nor In the previous or next lines.

Line 13: "Moderate" is a rather unclear word in this context. Would "less encouraging" be better? : done

Line 22: See substantive comments above .: see my answer above.

- Line 3525

Line 4/5: More discussion needed - why a Lorentzian (capital L needed by the way). What is the physical basis for that choice of function? Also, what is meant by "retrieval errors" here? (precision? accuracy? observed biases?)

We have chosen a bell-shape function because the latitudinal variation of OClO SCDs shows maximum and minimum. Thus, there are no physical reasons to choose a Lorentzian function rather than any other bell shaped function (like Gaussian or Pearson function). Our choice is based on numerical reasons. The fact is that based on a few dozen of examples, the best fit was achieved using Lorentzian functions. We have therefore implemented a latitudinal fit with Lorentzian functions. This have been specified in the text.

Line 20/21: Perhaps give a rough numeric range here. Numerical values are given lines 21/22/23.

- Page 3526

Line 1: Does MLS say anything about the variability of this region that could help interpret your observed variability?

No, we have read several articles about ClO in the stratosphere using MLS and nothing about the variability of ClO in this region. The presence of ClO in the stratosphere is well detected by MLS (we have plotted maps of ClO using MLS data and it appears clearly as you can see it in our previous paper, tetard et al. (2009). In order to interpret the observed variability, we will use modelisation.

- Page 3527

Line 2: "remove" -> "removed": this paragraph about the correlation between OClO and NO_2 concentrations has been removed for consistency with our choice not to discuss the OClO concentrations.

Discussion around line 10: Do Polar Stratospheric Clouds have any impact on GOMOS observations. If so, describe the impact and your methods for alleviating it.

There is an impact of PSC on the GOMOS transmittance measurement but this impact has a smooth spectral behavior and is therefore characterized by the second order polynomial used in the DOAS process. PSC effects has been added in the short list of the phenomena with a smooth spectral behavior in the third section.

- Figures

Figures 1 to 3 and their captions have become confused. I will try to be as clear as possible when discussing them.

We are sorry about that and in the revised paper, the captions correspond to the right figure.

The latitudinal coverage figure (above caption labeled Figure 1): Consider a smaller (filled?) symbol size for clarity? Also, in its caption (labeled "Figure 2"), and in the text it, should be made clear that (or if?) this is just the OClO observations or all GOMOS observations. I suspect it's the former.

Done. We have added in the text and in the caption that this is just the OClO observations.

Figure 4: The x-axis on the bottom left plot is unclear as there is only one number. Why use a log scale anyway, this is a mean residual, so presumably negative numbers are allowed? Do we expect it to be logarithmic?

Yes, values can be negative but it turns out to be positive here. We have added some values on the x-axis for clarity.

Figure 5: Change to density or vmr (or have one SCD one density/vmr panel).

We have decided not to change SCD in density/vmr for the reason specified previously.

Figure 6 - last line of caption: How are these errors defined? Precision? Accuracy?

We have computed the retrieval random error using the hessian matrix of the chi-square and propagated these errors in the spatial inversion process. We thank the referees because thanks to their comments about this figure we have found a small error in our computation of the error. It has been corrected and the new error bars appear in the revised version of our manuscript.

Figure 7: A lot of wasted white space. Drop x axis for all but bottom row, y axis for all but leftmost column, make plots larger and closer together. Perhaps label x-axis "latitude" rather than "\phi"

done

Figure 8: Last line of caption: "differs" -> "differ"

done

Figure 9: Are all the points in the right hand panel included in the fit?

My answer is yes but this figure has been removed.

References:

Press, W.H., Teukolsky, S.A., Vetterling, W.T., and Flannery, B.P.: Numerical recipes, the art of scientific computing, third edition, Cambridge University Press, 2007, ISBN-10: 0521880688

Tetard, C.T., Fussen, D;, Bingen, C., Capouillez, N., Dekemper, E., Loodts, N., Mateshvili, N., Vanhellemont, F., Kyrola, E., Tamminen, J., Sofieva, V., Hauchecorne, A., Dalaudier, F., Bertaux, J.L., d'Andon, O.F., Barrot, G., Guirlet, M., Fehr, T., and Saavedra, L.: simultaneous measurements of OClO, NO2 and O3 in the Arctic polar vortex by the GOMOS instrument, Atmos. Chem. Phys., 9, 7857-7866, doi:10.5194/acp-9-7857-2009, 2009.