**Atmospheric
Measurement
Techniques
Discussions**

Interactive comment on "Comparison of SMILES ClO profiles with other satellite and balloon-based measurements" *by* H. Sagawa et al.

Anonymous Referee #2

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General Comments:

This paper presents a stratospheric/mesospheric profile data set for ClO from the JEM-SMILES instrument on ISS. This six-month data set provides measurements at a range of local solar times which will be of interest to the experimental and modelling communities. The paper describes the data set from the NICT version 2.1.5 processor and compares it with results from the JAXA SMILES processor and other satellite and balloon borne instruments (e.g. Aura MLS, Odin SMR, Envisat MIPAS, MIPAS-B and TELIS). Also, it includes an extension of previous error analysis (by Sato et al., 2012) to cover different local times and geographic locations.

This work is appropriate for publication in AMT. However, for this paper to be published, significant revisions will be needed to address the comments and questions below.

This paper provides a description of this new data set and its quality which is a useful contribution to the scientific literature.

Specific Comments:

Sec. 2.3 - Error analysis:

Page 622, Lines 4-5: The reference to the JPL spectroscopic database (Pickett et al., 1998) does not appear to include the details on the measurement of the line position for this ClO doublet. An additional reference should be added to provide this information.

Sec. 3 - Methodology:

Page 624, Line 2: I am not sure about the term "a median of the zonally accumulated dataset". Is this a climatological comparison? It seems that the term used to describe the comparison differs through the paper and this should be made more consistent. Also, are there references that can provide additional insight into using this method?

Page 624, Lines 8-10: Could you provide a reference or further explanation about this "difference in sensitivity" issue that excluded the ground-based comparisons?

Page 624, Lines 13-16: Are there some general comments that could be made about the coincidence criteria at this point? The justification for the choices was not always clear in the discussion in Sec. 5. On Page 624, Line 15, what is meant by "representative values"? Is this the difference of the median profiles for each instrument or the median of the individual profile differences? The calculation methods seems to be specified for the average relative difference calculation (Lines 19-22) but is not as clear for the average absolute difference.

Use of Averaging Kernels and Vertical Resolution:

Page 624, Lines 25-26 and further: The term vertical resolution is used here and further on in the paper. However, it is not defined. Is this the FWHM of each averaging kernel? Is the same definition used for each instrument or are these values (and the profiles

shown in Fig. 5) obtained using different metrics? A consistent definition should be used.

Page 625-626, Lines 25-2: For the smoothing calculation, it is not clear if a representative averaging kernel is used or if individual ones are used for each coincident pair. This should be clarified.

Page 626, Lines 11-14: It was not clear why the triangular functions were used for smoothing for the SMILES/MIPAS zonal median distribution comparisons. Were these applied before or after the zonal median calculation? Were the averaging kernels not available or were only the zonal median distributions provided? It would be good for the authors to clarify the reason for this approach and the impact of using versus not using the smoothing for this comparison.

Sec. 4 - Comparisons between SMILES versions:

Page 627, Lines 19-20: Could you be more specific about the differences in the a priori temperature and pressure profiles used for the two retrievals? Are these "first guess" for a retrieval of pT or a fixed input for the ClO retrieval?

Page 628, Lines 8-9: Is there a reason why the JAXA vertical resolution is higher?

Page 629, Lines 1-16: It would be helpful to clarify which spectroscopic parameters were changed in this experiment and how they were changed. Was it all of those listed in item 3 on Page 627? Were they changed one at a time or all together? Further information would be helpful to provide justification for the comment on Lines 15-16.

Page 629, Lines 22-25: Have any tests been done to confirm this suggestion about the limited spectral bandwidth? It would be helpful to be able to describe a preliminary result if it is available.

Page 630, Lines 9-12: I am not sure I follow why the latitudinal shift in daytime stratospheric ClO creates a difference in retrievals made from the same set of observations. Could you clarify this point?

Sec 4.2.2: In the earlier discussion, it was noted that the JAXA retrievals were smoothed by the NICT averaging kernels for this. Is there any discussion of the impact of this? It would be useful to quantify the differences.

Page 630, Lines 15-17 and 21-23: It is not clear what is meant by "MLS-derived meteorological products"? Were they derived for MLS rather than SMILES observations? Also, how was location of the SMILES profile relative to vortex determined? Did it use data at one altitude, at the ClO peak or throughout the stratosphere? How did the change in SZA criterion for daytime profiles (between lower latitude and polar observations) affect the comparisons? What was the change in the number of profiles available and the median difference? It would also be instructive to examine the impact of this change in SZA criterion using the lower latitude profiles.

Sec. 5 - Comparisons with Other Instruments

Sec. 5: For each of the instruments, slightly different sets of parameters are given. To provide the appropriate background for the reader, it would be best to include consistent information (e.g. antenna dimensions, spectrometer resolution, spectral range or bandwidth, number of measurements per day, measurement duration/integration time, scan altitude range). Also, no description is given (in this section or in the SMILES data descriptions) of the vertical coordinate used for each of the retrievals and the method used to change all of the observations to a common grid. For example, in Fig. 11, the change in vertical coordinates is obvious between the three panels but it is not discussed in any detail in the text.

Page 634, Lines 1-3: It is unclear if the time criterion is also changed in these comparisons (versus what was used for MLS).

Page 634, Lines 3-7: If the SMR measurements cluster near local sunrise and sunset, why were the comparisons not split this way as the day and night ones were for MLS? Does this make a difference in the comparisons? Did the coincidence criteria also ensure that the SMILES and SMR measurements were taken on the same "side" of

the sunrise or sunset (e.g. both before or both after)?

Page 634, Lines 14-21: Were the SMR averaging kernels used to smooth the SMILES result for the mesospheric comparisons? An experiment to look at the effect of smoothing should be considered in revising the comment made on Lines 20-21.

Page 635, discussion of MIPAS data versions and errors: It is not clear which version (high-resolution or reduced-resolution) data is being described and used. In particular, the "offline-reprocessing high-spectral-resolution version" description (Line 21) does not seem to match with the time period and description of when MIPAS measured with reduced spectral resolution (Lines 12-14). This should be clarified further.

Sec. 5.3 methodology: There are some details on this zonal median distribution comparison that should be clarified. The pixel size (latitude by pressure levels or altitude bins) needs to be defined (here or Page 262, Line 5). Why were the two data sets not sampled over the same/more similar time period to remove this issue in the comparison? Could the data have been filtered into vortex and extra-vortex measurements to reduce complexity for the Arctic? This technique would allow the comparisons at higher and lower latitudes to be made independently and could help with some of the more challenging issues. I think that these comparisons could be useful but need to have a number of uncertainties and inconsistencies reduced to better understand the differences.

Balloon Comparisons

Sec. 5.4 measurement details and discussion: There is a lot to take in from Fig. 10 and it would help to provide a bit more information on the scan range, rates and other measurement characteristics of TELIS and MIPAS-B. The measurement location for each instrument is not provided at the same altitude and none seem to match the level used for the PV field. Could some comment be made on the "mismatch" in the comparison arising from this?

Page 639-640, Lines 27-5: It is not clear which specific SMILES and MLS profiles are being considered as having high gradients and contributing to difficult comparisons. I think that this section would benefit from the use of output from a chemical transport model to assist with the interpretation, as mentioned by the authors.

Conclusions:

Some care should be taken to revisit these after doing further work to ensure that they are not over stated in terms of "good agreement". It is better to quantify what is meant. The last statement sort of downplays the usefulness of the current dataset (issues that are "not fatal"). If it will get better in the next version, what is the appropriate use for this version?

Tables & Figures

Table 1: This needs a bit more work with the text to explain where the values came from. Did all of these values come from Sato et al., 2012? There need to be some more references. Also, some listings do not describe an assumed uncertainty they describe the two configurations that are contrasted. These should be made more consistent.

Table 2: Consistency in the capitalization of headers needs to be fixed. Also, is precision the term used mostly in the text or is is random error. One term should be used consistently.

Fig. 2: Which ozone isotopologue appears in this spectral window? It would be helpful to explain that all of these analyses came from one scan (date, time, location). Are the averaging kernels for altitudes or pressure levels?

Generally, the figures need a bit of work to make sure that the captions provide sufficient information and that they do not contradict the text.

Technical Corrections and Suggestions for Improvements:

Page 615, Line 11: All acronyms except for JAXA are defined in the abstract. This

should be fixed.

Page 615, Lines 13-16: Based on this sentence, it is not clear which retrieval processor uses the limited spectral region. This should be clarified in the abstract as is done in the text.

Page 616, Line 6: UARS is not defined in the text.

Page 616, Lines 8-10: Is it possible to rearrange the sentence to make it clear which reference is for the satellite and which is for the balloon MIPAS?

Page 616, Line 25: Should be "...for daytime mid-latitude conditions."

Page 617, Line 7: Should be "instrument modelling" or "instrument function".

Page 618, Line 27: Should be "...sampling density of about 1639 points per day..."

Page 618, Lines 24-25: Does the direction of scan influence the results or is this not necessary for this discussion?

Page 619, Line 17: rms is not defined in text.

Page 619, Line 25, Page 623, Line 24: These data are referred to as calibrated spectra, level-1B product or calibrated radiance or a combination of these terms. I suggest that a consistent term is used for this and for the level-2 data.

Page 621, Line 25: It is not clear in the latitude range that was used for the nighttime assessment. This should be clarified.

Page 625, Lines 9-10: Is the reduced FOV for TELIS due to being in the atmosphere or really because the instrument is closer to the tangent altitude being measured?

Page 627, Line 6: "...instead of mean values to calculate..."

Page 626, Line 8: The abbreviation MADs should be used here.

Page 627, Lines 1-2: Suggest that the bandwith of Band-C be mentioned here for

comparison.

Page 627, Lines 6-8: Is the offset parameter retrieved in this process?

Page 628, Line 13: Please clarify if these comparisons from two months (February and April) or for three months (February - April). There is a discrepancy between the text and the information in Fig. 6.

Page 628, Lines 21-22: Is there a better way to say this? Perhaps it could be described as "the region around the peak in ClO concentration"?

Page 629, Lines 19-20: Is it more appropriate to discuss this here once the JAXA comparisons have been introduced?

Page 633, Line 17: The full name of Chalmers should be used here.

Page 634, Line 17: Do you mean -0.05 ppbv rather than -0.05 hPa?

Page 634, Line 28: Which is the ascending and descending equator crossing time for Envisat?

Page 639, Lines 15-20: It might be nice to show the difference plots as well as the individual profile comparisons.

Page 641, Lines 4-5: Which altitude region does this refer to or is it all?

Page 642, Line 15: I think that "DRL" should be "DLR".

References: I think that some of the abbreviations are not quite right or fully abbreviated. Please check Journal of Quantitative Spectroscopy and Radiative Transfer, Applied Optics and the IEEE journals and transactions.

Page 646, Line 1: Subscripts missing on N_2 and O_2.

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