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Atmospheric Measurement Techniques Discussions

Interactive comment on "Atomic oxygen retrievals in the MLT region from SCIAMACHY nightglow limb measurements" *by* O. Lednyts'kyy et al.

Anonymous Referee #1

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This paper describes how upper mesospheric-lower thermospheric atomic oxygen densities have been derived from nighttime SCIAMACHY observations of atomic oxygen green line emissions. The retrieval methodology, error analysis, and retrieval results are described and assessed via comparisons with MSIS and SD-WACCM model results as well as with TIMED/SABER retrievals. The paper is well written, has a clear and logical flow, uses sound methodologies, and is relevant to AMT. There are only a few relatively minor concerns (detailed below) that need to be addressed before the paper could be published.

General comments

It is mentioned in the introduction that there are WINDII, OSIRIS, and ISUAL derived

[O] data sets. Has there been any attempt to obtain the [O] research products from these missions? Direct comparisons with OSIRIS and ISUAL would be highly beneficial, and perhaps comparisons of SCIAMACHY and WINDII [O] for periods of similar geomagnetic conditions could be useful as well. SCIAMACHY and WINDII [O] probability density functions could also potentially be compared.

The results section at times lacks specificity, and at times explanations of differences between different methodologies/data sets are not offered. Instead of quantifying results, vague terms such as "close" or "bigger than" are used in qualification. Results need to be explicitly stated in quantifiable terms, and then possible explanations for these results need to be considered. Specific instances are given below.

Specific comments

Abstract: Should highlight more results, i.e. error analysis results, quenching model works best, etc.

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Line 1: "if the data were available" is misleading. Granted, [O] is not a standard level 2 or 3 product for these instruments, but undoubtedly the research products are readily available from the missions' science teams.

Line 19: should maybe be "the Earth's radiation. . ."

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Line 6: "in the vertical"

Line 16: what issues do the calibration flags identify?

Line 20: should add reference for the SABER instrument, I believe that it's Mlynczak et al. 1997.

Line 22: "provides the data"

Line 22: “the Internet” is somewhat vague. Consider “their website.”

Page 10835

Line 4: is a constant mixing ratio valid at 105 km? Do the MSIS and WACCM data suggest that this is the case? What errors are introduced by this assumption (a point aimed more at the error analysis section).

Line 10: “incorporates” would probably be more accurate than “includes”.

Line 12: “to complete them” isn’t necessary.

Line 16: “the National. . .”

Line 17: consider “online” instead of “via the Internet”.

Line 24: What benefits does a nudged GCM have over a standard GCM?

Page 10836

Line 5: It is not clear what is meant by “we exploited the longitudinal variation”. Does this mean that you sampled the model output at the longitude where the local time is 22:00?

Line 18: The SAA data is not the only data in Fig. 2 that is in green (as per the caption). Please make the main text and the caption consistent.

Lines 21-22: I assume the subtraction of the 110-126 km average is because any airglow signal is below the detector threshold, hence giving the detector noise signal. Please explicitly state the reason for this step.

Line 37: delete “like”.

Line 11: “see section 2.1” is unnecessary.

Line 29: “and so they should be considered” is unnecessary.

Page 10837

Lines 9-10 (and afterward): LER has already been defined as limb emission rate. There is no need to continue defining the acronym.

Line 16 (and afterward): VER has already been defined as volume emission rate. There is no need to continue defining the acronym.

Line 23: since the forward model has not yet been introduced, “A linear. . . was used to map volume. . .” may be more appropriate.

Page 10839

Line 5: the vector term b should be more generally defined as the random error of model parameters (even if the only parameter considered is the tangent height registration).

Page 10840

Line 15: Why is it assumed that S_e informs S_a? Shouldn’t a priori data be obtained independently from the measurements? Either way, it is unclear what you are using as S_a, please explicitly state how S_a is obtained/derived.

Line 26: Please specify what is meant by “similar”.

Page 10841

Lines 4-5: The averaging kernel isn’t necessarily expected to be “a simple peak”. In an ideal observing system it would be a delta function, but different observation systems will yield different averaging kernel shapes. This should be rephrased.

Line 25: “coarser” would be more accurate than “bigger”.

Page 10843

Line 5: “parameters means the retrieval is over-regularized and. . .”

Lines 18-19: The first part of this sentence is a bit confusing. Are you trying to say that only the systematic forward model parameter error is represented by S_f? Please

rephrase.

Line 20: the vector term b was previously defined as tangent height registration error, and is now being defined more generally. It might make more sense to define in general terms earlier in the text.

Line 5: the way the smoothing error is defined here (as it is defined in Rodgers) assumes that the true state is known. Can you please explain further exactly how you are determining the smoothing error?

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Line 19: "1S-1D" should be "O(1S-1D)".

Page 10846

Line 8: "approaches of retrieve" should maybe be "approach to retrieving".

Lines 24-25: Why were the quench/SABER profiles chosen? Could the other profiles be shown as well?

Page 10847

Line 17: "(denoted by subscript. . ."

Line 18: The phrasing is a bit confusing. Consider, "avoid assumptions about the parameter distributions, e.g. that they are Gaussian."

Line 20: "unperturbed" would be more accurate than "error-free".

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Line 14: "assessment" is more suitable than "validation", as the profiles aren't actually being validated.

Lines 22 and 24: It's not exactly clear what is meant by "fields". Perhaps climatologies?

Page 10851

Section 7.1: Please explicitly state the magnitude of the errors shown in Figs. 6 and 8. Are these results typical of the entire data set? What are the average errors? How do the errors compare with the overall errors of other satellite derivations of [O]?

Line 24: what specifically is meant by "certain deviations"? And "shows" should be "exhibits". The figure starts at 85 km, but the range being discussed is 82-87 km (or is 82 a typo?).

Page 10852

Lines 1-3: Please specify what is meant by "much higher", "a bit higher ones", and "quite similar".

Line 9: Please specify what is meant by "quite similar".

Lines 9-13: This discussion seems to imply that the SABER measurements are considered to be of better quality than the model results. Please explain why the SABER measurements are preferred.

Lines 16-17: How much SABER data is there for this time period? Is it biased to any season/region?

Line 21: Please specify what is meant by "even bigger".

Page 10853

Line 1: "has to be" should maybe be "is".

Lines 1-4: Can you justify that SABER is the better source for reference profiles? What is the reason for doubting MSIS density/temperature profiles in this region? SD-WACCM profiles should also be reliable. Can you please comment on this?

Line 13: Please quantify what is meant by "very good".

Section 7.4: are you simply multiplying the correlation coefficient by 100 in order to express it as a percent? If so, why is this being done instead of using values between

-1 and 1?

Line 24: even though the correlation coefficient values are significant, a value of $\sim$-0.3 is not at all a strong anti-correlation. Can you really deduce a 180 degree phase shift from such a weak correlation?

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Lines 3-6: Are there any physical reasons that could explain why this is the case?

Lines 13-14: The daily SABER correlation values aren't much higher than 30% (again, I assume this means a correlation coefficient of 0.3). These are quite weak values that would indicate essentially no correlation. Does this not indicate that either one or both data sets (SCIA, SABER) are too noisy on a daily timescale to make these types of comparisons?

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Line 26: Which earlier studies are you referring to?

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Lines 10-17: Please briefly summarize your error analysis results.

Lines 18-25: There is no mention of comparison results with WACCM.

Table 1, last line of caption: delete "in".

Figures: Units should be in parentheses to avoid confusion (especially when plotting scaled densities).

Figure 2: "related to contaminating".

Figure 3: How many scans are included in the averaging? This should also be mentioned in the main text.

Figure 7: Caption should explain the meaning of the shaded/non-shaded regions.

Figure 9: To avoid confusion, could label the SCIAMACHY data as "SCIA [O] (VER+SABER)".

Figures 9-11: The x-axis is expanded far beyond the maximum [O] values, presumably to fit the legend in the top right corner. The plots would be much easier to read if the x-axis was limited to 12$\times$10ˆ11 atoms/cm and the legend was more strategically placed. Also, to avoid confusion, could label the SCIAMACHY data as "SCIA [O] . . .".

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 10829, 2014.