

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2022-52

Anonymous Referee #2

Referee comment on "An optimal estimation-based retrieval of upper atmospheric oxygen airglow and temperature from SCIAMACHY limb observations" by Kang Sun et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-52-RC2>, 2022

This was a straightforward, well written paper that I enjoyed reading. For the most part, the methodology and results were clearly presented, although clarification is needed in a few spots (as listed below). After properly addressing the following (mostly minor) issues, I'd recommend the paper for publication.

" 1Δ " should be " ${}^1\Delta$ " throughout the manuscript

line 8 – I'm not sure I've ever heard the term "loading" before in the context of airglow. If this is a common term and I just haven't been paying attention, then it's fine, but otherwise I would suggest the word "density" or "concentration" instead.

line 35 – Please briefly explain in the text what is meant by "confounds the retrieval algorithm"

36 – "As an alternative" confused me, as you've already talked about the ${}^1\Delta$ band. I'd suggest you don't need an introductory remark for this sentence

40-41 – Please be specific about what there is a "lack of assessment" of

42 – I think "chemically" should be "photochemically"

53 – Please specify what is meant by “subject to errors”

70 – Based on your discussion of the results I don’t think “degrees of freedom of signal” is the correct term. As per Rodgers, DOFS is $\text{trace}(A)$, which is a single value. It looks to me like what you’re calling DOFS is what is more commonly referred to as the retrieval response, i.e. the sum of the rows of A . Please clear this up.

71 – please specify what is meant by “the formula”

84 – “tangentially” is not needed

87 – file format details are unnecessary

99-104 – I would assume that a proper background correction would be critical in altitude regions far from the emission profile peak, so I think this requires a bit more discussion. It would also be interesting to see a plot of typical or averaged background signals. Whether or not you include a plot of the background signals, I would appreciate at least a brief discussion on the shape of the background signals, and the assumptions/limitations that go in to using a scaled thermospheric signal.

Section 2.2 – MSIS v2 is now the most recent version, <https://doi.org/10.1029/2020EA001321>. I don’t necessarily think you need to update to this version (although it would definitely strengthen the paper), but, if you don’t, you at least need to discuss the limitations of MSIS-E-00 (especially in polar MLT regions) as is done in some of your references.

118 – “into two high and low altitude regimes” should just be “into two altitude regimes”

Section 2.3 – temperatures from ACE-FTS have been used in multiple comparison studies (easily found at <https://ace.scisat.ca/publications/>). Please briefly discuss the results of these studies so readers have an idea of the quality of the ACE-FTS temperatures in the upper atmosphere.

Section 2.4 – same as ACE-FTS, please briefly discuss the quality of MIPAS temperature retrievals

132 – You use the line parameters from HITRAN to calculate absorption/emission spectra. Also, please indicate here what version(s) of HITRAN you're using

142 – should be "coefficients" as they are different for the two bands

142 and after – the "n" in "n[x]" is not necessary as the square brackets already (typically) indicate number density

140-150 – It seems odd that you're discussing this in terms of density of "emitting" O₂ instead of excited O₂ in the specific state. The math is fairly straight forward, and all you need to add to the equation is a branching ratio, e.g. the Franck-Condon factor for the A-band.

151 and after – please use the more standard variables λ and $\bar{\nu}$ (nu with a bar over it) to represent wavelength and wavenumber

Figure 1 – could use a dashed vertical line in the middle to indicate the center of the line-of-sight/location of tangent height. Also, the description indicates that the tangent height is in the middle of a layer, whereas here it looks like it is at the bottom of a layer. Please make it consistent.

Section 3.3 – In optimal estimation, the measurement vector is typically represented by y (the retrieval function is typically R), so it's a bit odd having the measurement vector as r

238 – I would suggest using something like "retrieval system" instead of "forward model" in order to be more encompassing

250 – I would assume that results of the linear inversion would be prone to large, unrealistic oscillations that could lead to convergence issues. Is that the case? And if so, could some type of heavy regularization (smoothing) be applied to the result to get the profiles closer to a realistic estimate?

258 – was it mentioned earlier that this is performed in log space? If not, please explicitly state this prior to here and discuss the trade off of retrieving log values.

265 – why does the x_{i+1} variable have a "d" in front of it?

280 – The “airglow retrieval” is not attempted

338 – What is meant by “over and above the mesopause region”?

343 – I get what you’re saying about the profile being “W” shaped, but it’s not exactly intuitive what that means. If you want to describe it in that way, I’d say it’s more “ε” shaped, but I’d suggest simply describing it as an inversion layer or a possible double mesopause.

382-383 – The secondary ozone peak has been studied for multiple decades now, so the Li et al. 2020 reference is not appropriate on its own.

393 – a value of 0.5 seems relatively low, as I usually expect ~0.7-0.8 as a cut-off point. Can you please discuss the distribution of “DOFS” values (what I would call “response” values) for the the retrievals (e.g., what percentage of retrievals are rejected if you use 0.5, 0.75, etc., like what is done later in section 5)

409 – Please explain what you mean by “due to horizontal heterogeneity at large SZA”. Are you saying at larger SZAs there is more diurnal variation along the line-of-sight? If so, wouldn’t it be for SZAs closer to 90° (i.e. sunset or sunrise), not necessarily larger?

Figure 13 – please include the 1:1 lines

436 – What is meant by “very consistent”? because that’s not how I would necessarily describe those results.

440 – coincidence criteria

Figure 14 – a color bar is needed to indicate altitude.

478 – please quantify the findings of previous MIPAS temperature comparisons

Figure 16 – Please use either a dashed line or maybe shading to indicate areas where the MIPAS data is from climatology

500 – It's unclear what is meant by "due to horizontal heterogeneity of airglow"

Math is not my thing, so I did not check the equations in the appendices. In my opinion, the appendices aren't necessary, but I'm not opposed to them.