Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-52

Anonymous Referee #1

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-RC1, 2022

In this paper, a new retrieval algorithm for temperature and O_2 VER is introduced for the $O_2(^1Delta)$ and $O_2(^1Sigma)$ bands measured by Sciamachy. O_2 VER and temperatures have been derived from these observations before; what is new here is that both are derived simultaneously, and self-absorption is considered in a consistent. The retrieval is applied to one year of data (2010), and temperature data are compared to ACE-FTS and Mipas. The Mipas comparison is particularly useful as Mipas was on the same satellite as Sciamachy, therefore providing close coincidences. The O_2 airglow is highly relevant both for the accuracy of greenhouse gas remote sensing products, and for the energy budget of the mesosphere / lower thermosphere, and the data from this new algorithm provide a large step forward compared to previous publications. The paper is also generally very well written. However, I have some questions e.g., regarding the derivation of the prior error and the comparison to Mipas MA/UA data, as well as a few minor points listed below.

Line 249-250: Doesn't this imply an altitude dependent differently strong weighting, as the self-absorption affects the lower levels exponentially stronger?

Line 251-252: The statement that a prior error of 100 times the prior value leads to a weak to negligible prior constraint seems not correct in the lower altitudes affected by self-absorption: as there the prior profile is too low, and might be orders-of-magnitude too low, so is the prior error actually quite low. A climatology might be a better estimate of the prior values here, if available.

Line 355: These missing points … are these related to high solar zenith angles? As during daytime the dominating formation mechanism is O_3 absorption, the O_2 airglow varies strongly from daytime to nighttime, and observations with high SZAs would provide very different (lower) values, the signal-to-noise is also low. This should be discussed somewhere, as you don't separate daytime and nighttime observations at high latitudes, and it should also be stressed in discussing your climatology of O_2 airglow: it is a climatology covering a whole year of observations, but at a very specific time of day, about 10:00 local solar time.

Line 437: can you provide some idea why the A band has such a stable cold bias compared to the ^{1}Delta?

Line 468: "Mipas temperature retrieval in 2010 is only available below ~80 km": This statement is factually not correct. A) there are the middle atmosphere / upper atmosphere limb modes of MIPAS which scan up to 120 km respectively 170 km every ten days since 2007. These were coordinated with the Sciamachy MLT mode in such a way that corresponding observations are available every 30 days – about once per months. Observations in the MA/UA modes were carried out also in 2010, and temperatures were retrieved from these modes up to at least 120 km, see e.g., Fig 4 in Sinnhuber et al, JGR, 2022 for an example. Data are available on the MIPAS data server at IMK (https://www.imk-asf.kit.edu/english/308.php), and I am sure the Mipas team (e.g. Bernd Funke or Thomas von Clarmann for the MA/UA modes) would be happy to help in accessing and applying the data. If there are coincidence data between Sciamachy and Mipas for 2010 (and there should be at least 12 days) please do the comparison. B) Just as a caution, the nominal limb mode of Mipas scans up to 68 km, so values above ~70 km are probably dominated by the prior profile.

Figure 16: Here Mipas temperatures are used up to nearly 100 km – if they are from the nominal mode as you imply, the large differences above 80 km are to be expected, as the nominal mode scans up to 68 km only. It's rather surprising that the region 70-80 km seems to agree fairly well in most month.

Minor points:

Abstract: I know they are commonly used, but nevertheless I found the use of the abbreviations (1Δ and A) for the bands slightly irritating. Could you use the full names ($O_2(a^1\Delta_g)$, $O_2(b^1\Sigma_g^+)$) at least in the abstract?

Line 5: as the nominal mode only scans up to 93 km in 2010, how do you derive $O_2(^1$Delta) in 93-100 km?

Lines 9 – 11: please add altitude range where temperatures can be retrieved (~40 – 95 km for nominal mode, 65 – 105 km for the MLT mode?).

Line 62: Yang et al, SPECTROSCOPY AND SPECTRAL ANALYSIS, 2021 also used the O_2 airglow to derive temperatures

Line 84: in the nominal limb mode, Sciamachy scanned up to 93 km in 2010. It was slightly higher at the beginning of the Sciamachys operations, but unfortunately this was changed to 93 km already in late 2003.

Line 166: where is the number 1.4387760 cm K coming from?

Line 192: … will also be N. Actually if you formulate it like that, the number should be N-1. It is N in your retrieval because you add an upper bound layer at the top. Can you clarify this?

Line 300-302: "only limited limb views with deeper tangent heights could observe those layers" I am not quite sure I understand this statement. Does it mean only some of the nominal limb scans (which all go down to the surface) provide a good signal-to-noise ratio in these altitudes? This is how I understood this sentence, however I don't understand how it applies to the discussion of a single limb profile as given here. Please clarify.

Line 303: as supported by comparison to results of the MLT mode retrieval

Lines 311-312: This is by design … due to the self-absorption

Lines 317-318: a) the lowest tangent altitude of the MLT mode is around 51 km; b) why is the upper limit set to below 120 km?

Line 324: erase the would. They do.

Line 371: the maximum abundance