

Interactive comment on “The use of O₂ 1.27 μm absorption band revisited for GHG monitoring from space and application to MicroCarb” by Jean-Loup Bertaux et al.

Anonymous Referee #1

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The feasibility of using the 1.27 μm, or the “singlet Delta” O₂ band for satellite nadir remote sensing of greenhouse gases has been investigated previously (Sun et al. 2018). However, this manuscript, to my knowledge, firstly presents this observation strategy within the framework of a planned satellite mission (MicroCarb). Since such a satellite mission may cost hundreds of millions of euros/dollars and provide crucial data with global coverage for many years, this strategy may potentially have a far-reaching impact, and this work may become an import reference for future missions. Given the aforementioned significance, this manuscript at this stage may still be substantially improved in terms of both presentation quality and technical rigor. The following lists my general comments concerning the manuscript structure and specific comments con-

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cerning individual sentences, equations, and figures.

1 General comments

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comment

1.1

In general the manuscript has been clearly improved since the initial submission. That said, it still reads a bit fragmental with different sections apparently from different groups of authors. The O_2 molecules in $a^1\Delta_g$ state, for example, was denoted as O_2^* , $O_2(a^1\Delta_g)$, $O_2^*(a^1\Delta)$, $O_2(1\Delta)$, and $O_2(a^1\Delta)$ throughout the manuscript.

As to the paper structure, Section 5 describes the MicroCarb mission and its physics-based retrieval, 4ARTIC. Then it was deviated to a retrieval of SCIAMACHY nadir airglow using a different tool called LATMOS breadboard (sections 6.1–6.2) before 4ARTIC was applied to test different airglow mitigation approaches in section 6.3. The authors may consider moving sections 6.1–6.2 after section 3, because the SCIAMACHY nadir/limb airglow comparison naturally follows the onion-peeling VER retrieval, and combining section 5 with section 6.3. Some parts of section 7 seem to be out of the scope of this study (e.g., the discussion of methane and CO fluorescence), and the remaining parts may be combined with section 2.1.

1.2

Section 2.3 could be clarified and largely removed from the manuscript.

First, the main conclusion from this section, equations 14 and 15 that will be used to convert absorption spectra simulated by LBLRTM to airglow emission spectra, seems to have already been demonstrated by equations 3–5 in Sun et al. (2018). Combining

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equations 4 and 5 in Sun et al. (2018):

$$r_g(\nu) = n(\text{O}_2(a^1\Delta_g)) \sum_{ij} A_{ij} \frac{g' \exp(-c_2(E_i + \nu_{ij} - \nu_{0,0})/T)}{Q(T; \text{airglow})} f(\nu, \nu_{ij}, T, p)$$

Following the terminology in this manuscript, define a similar “emission rate per O₂* molecule” for transition ij , denoted by ε_{ij} :

$$\varepsilon_{ij} = A_{ij} \frac{g' \exp(-c_2(E_i + \nu_{ij} - \nu_{0,0})/T)}{Q(T; \text{airglow})}$$

Line intensity S_{ij} is given by equation 3 in Sun et al. (2018), copied here:

$$S_{ij} = I_a \frac{A_{ij}}{8\pi c \nu_{ij}^2} \frac{g' \exp(-c_2 E_i/T) (1 - \exp(-c_2 \nu_{ij}/T))}{Q(T)},$$

Take the ratio

$$\frac{\varepsilon_{ij}}{S_{ij}} = 8\pi c \frac{Q(T)}{Q(T; \text{airglow})} \nu_{ij}^2 \frac{\exp(-c_2 \nu_{ij}/T) \exp(-c_2(E_i - \nu_{0,0})/T)}{\exp(-c_2 E_i/T) \exp(-c_2 \nu_{ij}/T) (\exp(c_2 \nu_{ij}/T) - 1)}$$

and simplify

$$\frac{\varepsilon_{ij}}{S_{ij}} = 8\pi c \frac{Q(T)}{Q(T; \text{airglow})} \nu_{ij}^2 \frac{\exp(c_2 \nu_{0,0}/T)}{\exp(c_2 \nu_{ij}/T) - 1}$$

The equation above is identical to equations 14–15 in the manuscript. As such, most equations/figures in section 2.3.2–2.3.6 can be replaced by a simple reference to equations 3–5 in Sun et al. (2018).

Second, the symbols are not consistently defined, making section 2.3 challenging to follow. For example, the upper state energy is E_2 in equation 9, E_{2i} in equation 10, and E_i in line 9, page 11. The total partition sums for both absorption transition and airglow emission are denoted as $Q_{tot}(T)$, but separately as $Q_{tot}^{lo}(T)$ and $Q_{tot}^{up}(T)$ in equation

15. The summation index is i in equation 10, but appears to be J' in equation 11. The local emission rate spectrum is given by $E_m(\lambda)$ (line 17, page 15) and $E_{mn}(\lambda)$ (line 18, page 15) in section 2.3.7, but it is unclear what this quantity really is.

Finally, it will be very useful to discuss the improvements over previous studies. I did not find an equation that calculates $Q_{tot}^{up}(T)$ in Sun et al. 2018, which is given by equation 10 in the manuscript. However, I cannot reproduce the results $Q_{tot}(T = 296K) = 147.196$ (line 10, page 11) using equation 10 and HITRAN 2016 data because it is unclear what “index i refers to each value of E_2 ” (line 36, page 10) exactly means.

1.3

One significant missing piece of this study is the fine-scale spatial variation of airglow, potentially due to gravity waves. The REPROBUS CTM ($2 \times 2^\circ$) and SCIAMACHY limb data (400×960 km) used in the study are too coarse to capture those fine-scale variations. The fine-scale airglow features may propagate to the retrieved XCO_2 as artefacts from the disturbed mesospheric temperature (spectral shape of both emission and re-absorption), excited O_2 molecule concentration (emission), and O_2 molecule concentration (re-absorption). The authors may have missed one opportunity as the SCIAMACHY nadir data over ocean have much higher spatial resolution and may reveal some gravity wave features. Larger scatter is seen from SCIA nadir ocean compared to SCIA limb (Figure 30). Is that just retrieval random errors or real spatial variation? How do they look on a map?

2 Specific comments

Line 31, page 1: the statement that the theoretical synthetic spectrum is from “a new approach” is contradictory to the following statement that this approach “is very similar

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(likely identical) to the approach of Sun et al. (2018)” (lines 7–8, page 2). The current work should be more clearly distinguished from previous before claiming it is “new”.

Line 32, page 2: may include OCO-3 and GOSAT-2.

Line 40, page 4: why is pressure broadening proportional to the “square” of air density?

Figure 2 caption: how can “transmittance” be “larger” than emission? Did the authors mean “wider”?

Line 35, page 6: equation 3 looks the same as the second equation in the Appendix A1.1.1. They should be consistent.

Line 32, page 7: Dave Crisp et al. should be just Crisp et al.

Line 30, page 9: quenching is significant below 50 km, and the airglow peak is also below 50 km. Are there any consequences if the quenching is ignored?

Line 15, page 14: $E_0 = 7892.02 \text{ cm}^{-1}$ is inaccurate. Refer to Leshchishina et al. (2010).

Line 40, page 16: ε appears to be spectrally integrated here, but spectrally resolved in equation 13, and back to be spectrally resolved in line 28, page 18 again.

Line 5, page 18, equation 21, and line 4, page 19: a bold face ε is used, but definition is unclear.

Line 24, page 21: why does O_2 absorption have to be neglected? It is important and all parameters should be readily available to include O_2 absorption in the analysis.

Figure 11: this figure may be removed. The SCIAMACHY limb data are binned across track so it is unnecessary to show separate across-track positions. The globe distribution of SCIAMACHY orbits is well represented by Figure 15.

Figure 12b and the left panel in Figure 13: these should be removed, see the following argument.

Line 8, page 23: This is not a “simplified” model; the equation for absorption cross

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section is not simpler than equation for airglow emission. This is an “inaccurate” or “inappropriate” model.

Figure 16: $SZA_{\text{recomputed}}$ appears in the figure but is only explained at the end of page 31.

Lines 5–6, page 27: VER has been defined and should be only defined at first appearance.

Section 4.1.1, page 27: the authors are suggested to better articulate the usefulness of the REPROBUS model. Its low resolution is not sufficient to capture the fine-scale variation; its model top (0.01 hPa) does not fully capture the airglow profile (what is the error from neglecting airglows above 0.01 hPa?); the model is not used in the following MicroCarb retrieval tests.

Lines 13–14, page 29: how much is the error due to neglecting the reabsorption, and what is its relative contribution to total error?

Figure 18: it’s maybe useful to mark the subsolar point in the plot.

Line 23–24, page 31: may change to “...an observer placed above the tangent points...” as there are multiple tangent points that are close to each other.

Line 41, page 31: the SZA is defined at “one of the two points corresponding to the intersection between the line of sight and TOA”. Which one?

Figure 19: removing the lines between markers may improve the figure clarity.

Line 7, page 33: it could probably be delivered in a more rigorous way than asserting: “must have been”.

Figures 20–22: Figure 22 should be enough to present all results so Figures 20–21 can be removed.

Line 2, page 36: modified to “five orders of magnitude of variation of ozone with alti-

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tude”.

Figure 25: what is the difference between the left and right? What are the observation time/SZA?

Line 5, page 39: the claim that “the night side model bias is quite negligible below 60 km” contradicts the main conclusion of this section, i.e., the ozone deficit below 60 km of 10–20% in the REPROBUS model is the main reason of airglow underestimation. Moreover, is it possible to compare with other reanalysis/observation data, especially MERRA-2?

Line 8, page 41: change “maximum height Z_{aero} ” to something like “peak AOD height Z_{aero} ” to avoid confusion.

Line 27, page 41: the 2.04 μm band is not the same as OCO and GOSAT satellites, which use the 2.06 μm band. Why MicroCarb is using a slightly shifted window (lower left panel, Figure 26)?

Lines 8–9, page 42: the water vapour lines are not present in this figure.

Figure 27: what are the vertical axes on the right? In addition, it is hard to see, but there is a red line very close to the horizontal axes. What is that?

Lines 12–19, page 42: those are repetition of lines 36–40, page 4, but not exactly the same. The same argument does not need to be repeated twice in the article.

Figure 28: The “@1.27 μm ” may be removed in the vertical axis label.

Line 18, page 49: The REPROBUS model is coarse and cannot capture small-scale intensity variations, if they exist.

Line 4, page 50: what is the resolution of the “very high resolution” spectrum?

Lines 23–26, page 50: what are the a priori errors, especially for P_{surf} and airglow scaling factors? What is the impact of a priori on posterior error? Does the airglow

impact retrieved XCO_2 ?

Line 29, page 50: what is “Lmoy”?

Lines 10–16, page 51: the cold/warm spectra here differ from what has been defined in equations 29–30 (spectra simulated at 217/270 K). Does this make any difference?

Line 31, page 56: change “dayglow” to airglow.

Line 39, page 56: may remove the exclamation mark after “contaminated”.

Lines 13–15, page 57: the comparison with day-side GOMOS ozone profiles appears to be inconclusive according to section 4.3.2 and may not support the statement here.

Lines 31–32, page 57: the first sentence may be inaccurate because a few factors stated previously in the manuscript. The absorption is still saturated while airglow emission grows linearly, the absorption is much more pressure-broadened, and only CIA is present in absorption. All those factors still hold even the airglow spectral shape were identical to absorption.

Line 5, page 58: the $O_2 a^1\Delta_g$ band is significantly more complicated than just P, Q, and R branches. The same argument applies to discussion of transitions in sections 2.3.3–2.3.5, which is suggested to be removed.

Lines 31–32, page 58: I’ve found CIA being considered in Figure 3 and Eq. S5 in Sun et al. (2018), so it is inaccurate to state that “their whole analysis was done without accounting for the CIA O_2 absorption”.

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