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Discussions

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.

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Response to referee#4 O2 paper amt-2019-54.docx 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 #4 Received and published: 12 July 2019

General Response to general comments. We are very much grateful for the great amount of time spent by Referee #4 in reading carefully our manuscript and pointing out some mistakes that are now corrected. We write in blue our answers. General Comments: I believe that the authors intend to show in the submitted manuscript that

O2(1_g) airglow can be modeled with sufficient accuracy to use the 1.27 μm O2 absorption band to retrieve O2 columns for greenhouse gas studies. answer: No, we did not show that the subtraction of a model would provide sufficient accuracy to retrieve the O2 columns. On the contrary, we wrote at end of Section 8.1: "However, the degree of accuracy that is needed for the determination of Psurf for useful measurements of GHG gases is very large, about $\sim$ 0.1 hPa for the bias and 1 hPa for random error. According to our simulations, and if the airglow is ignored in the inversion but subtracted from a model, this airglow intensity model would have to be accurate to $\sim$1.5% (for a mean radiance with albedo= 0.2) to achieve the 1 hPa random error. Therefore, in most cases it is insufficient to rely entirely on a model to predict the actual airglow intensity to be subtracted from an observation. We need to disentangle in the observed spectrum itself the contribution of the airglow and the contribution of the solar scattered radiation. For this, we will rely on the fact that the spectrum shape of the O2* airglow is different from the O2 absorption spectrum. " Our purpose in this paper was also to fight wrong ideas about this 1.27 μm, like :"it is highly erratic and variable", "we do not fully understand the physical process of the emission", etc. We showed that the spectral shape predicted by our new model coincides with the SCIAMACHY observations, and that the model intensities are lower than SCIAMACHY by about 10-15%, suggesting that ozone is underestimated in our CTM model. But we show that with a good spectral resolving power, the airglow emission may be disentangled from the O2 absorption in nadir viewing.

Length of the paper: We recognize that this paper is long, but we still believe that its overall length is appropriate. At an early stage one reviewer suggested to split the paper in several papers but we have been quite reluctant to continue along this line (split or shorten substantially) for the following reasons. All parts of the paper are relevant to the same subject: is it possible to use the O2 1.27 μm absorption band for CO2 mixing ratio retrieval, in spite of the strong airglow contamination? The team that was assembled for this scientific research had to cover several scientific aspects: our understanding of this airglow, building a model for the intensity, and a model for the

spectral shape, validation with comparisons with SCIAMACHY/ENVISAT data, separation of airglow from absorption. One reader is not obliged to read carefully all sections, he can pick up what he is most interested in. We estimate that if we would split our paper into two papers, the overall total length of the two papers would be longer than the present version, because of unavoidable repetitions (each paper must be self-consistent, including references). It would require also twice more reviewers and Editor work. AMT stands for Atmospheric Measurements Techniques and therefore our paper is perfectly in scope with the profile of the publication. Our paper is long because it is deliberately rather detailed, because we wish to ease the possibility that anybody else to be able to reproduce our results. The spirit of AMT, with public discussions before final publications, is in line with the "open source" philosophy. Cancelling parts of the paper would jeopardize this philosophy. Remember that the results of about 30% of all scientific papers cannot be reproduced by other scientists, and this comes to 50% of papers in biology, a very embarrassing situation. One great advantage of AMT publication is that it does not require paper printing, therefore cancelling a source of CO2 production. Only an interested reader would potentially print it. Therefore, with AMT we may reconcile CO2 economy and detailed description for better reproducibility of results. In its present form, our paper is somewhat "self-consistent" on its subject. It will serve as a reference, not only for the MicroCarb project, but also on other future GHG monitoring space projects that may consider the use of the 1.27 μm band. Finally, we note that the length of the paper did not discourage a fairly large number of scientists to download the paper when discussed in AMTD: The paper has been viewed HTML 175 times and the pdf downloaded 91 times (25 august 2019), about half from the US. If the final version were cut significantly, it would introduce an advantage to those who uploaded the early version versus those seeing only the final version.

It seems that the authors have done a lot of very good work over the past 3 years or so, and a lot of it appears in this manuscript. The paper is very long at 75 pages, and I am not sure that all this detail needs to be in the paper, as some of it is in the published literature (O2 spectroscopy and non-LTE calculations, for example) and

references might suffice. I feel that the paper might be easier to follow and make a stronger case for the conclusion if content in the main body were limited to what is needed to support the conclusion, and use references or supplemental material otherwise. answer: to our knowledge, the detailed shape of the O2* airglow and the principles of its calculation have not be published elsewhere, except perhaps Sun et al. (2018) who have most likely achieved similar results independently, but did not explain all the details in the short format of their GRL paper. 1. The theoretical development (to obtain a theoretical spectrum of the O2* airglow emission) that we present here was done in 2017, and completely independently from the work of Sun et al. (2018). If we were following the suggestion to just quote the equations of Sun et al. (2018) and not present our own analysis, it would give to the reader the false impression that we have followed blindly the developments of Sun et al. 2018, which is not true. The fact that both groups have developed the same kind of theory (form the same theoretical approach based on what can be found in Simeckova et al. (2006)) re-inforce the credibility of this approach, which is very important for "hundreds of millions dollars space projects". 2. Our equations (14) and (15) give an original result: a formula giving the wavenumber variation over the whole band of the ratio ε/SS of airglow emission ε to the line strength SS found in Hitran table. This formula is NOT in the paper of Sun; the referee#1 had to check by some manipulation of equations that such a relation COULD be retrieved form formulas in Sun et al. (2018). Doing this, referee#1 shows that both groups are using consistent descriptions of the physics involved, a satisfactory piece of information for which we must thank him. 3. In fact, in the present study, we have used the formulation of this ratio in order to build very simply a synthetic spectrum of the airglow emission, by using the LBLRTM code computing the local absorption, and multiplying by the function $\varepsilon(\nu)$/SS(ν) (14) and (15). This is a totally original method, and we wish that all the AMT readers to be able to reproduce it and use it. This is why all equations establishing $\varepsilon(\nu)$/SS(ν) must be kept in the present paper.

More specific suggestions follow below. I thought it might also help to include some discussion regarding how the O2 column retrievals using the 1.27 μm band will be

validated, given the very high accuracy (0.01%) that is required. Will they be compared against the O2 A-band retrievals? But if O2 A band retrievals are good enough to be the standard, then what is the benefit of switching to the 1.27 μm band, given the added complication of the airglow correction? How will one know if the new retrievals are better? Answer: we quote from our paper: "Kuang et al. (2002) recognized the virtues of the O2 band at 1.27 μm (nearest to the CO2 bands), but discarded its use because it is contaminated by the intense O2 airglow day side emission". Therefore, the O2 A band at 0.76 μm was taken "by default". The whole idea of this investigation is to revisit the rejection of the O2 band at 1.27 μm, recognized to be much better than the O2 A band in case of aerosols, but only if it can be corrected from the dayglow. The problem of validation is important, and is not addressed here. MicroCarb is using both bands of O2, therefore allowing useful comparisons. One criterium would be the retrieval of Psurf, which is known from meteo fields. Also the difference between the two retrievals of Psurf (from the two bands of O2) could be correlated with the quantity of aerosols. We suspect that this difference will increase with the quantity of aerosols, because in many instances with aerosols the O2 A band (0.76 μm) underestimates Psurf when compared to the meteo field. But this discussion is well beyond the scope of this paper.

Specific Comments: I found the papers by Zarboo et al. (2018), Sun et al. (2018), and Simeckova et al. (2006), all cited by the authors, to be particularly helpful, and I think that there are places where the present manuscript presents conclusions or material that is similar (although clearly independently derived), so there are opportunities to make use of references to shorten the text. Again, my intent in making this comment is to find a way to limit the material in the paper to what is required to support the conclusion. a) Introduction, sections 2.0, 2.1: I think this was about the right length, although I'm not sure that the discussion of observations of Venus and Mars add much to supporting the goal of the paper. b) 2.2,2.3: This section is around 7.5 pages, and includes a lot of standard spectroscopy and line-by-line radiative transfer calculation information, including the use of Einstein A coefficients for non-LTE situations. The

discussion could be shortened considerably by the use of references and limiting the text to what is unique. Answer: As said in the beginning, we are quite reluctant to cut out or reduce some parts of the paper, because we wish to have it self-consistent. I was a little uncomfortable with the way that LBLRTM is being used in section 2.3.7, as it would be cleaner to just start from scratch with fresh code and do it right, but I appreciate that this may not have been practical given the resources available and it seems to have worked. Answer: we agree on this remark and we plan to do a calculation from scratch in the future. However the use of LBLRTM was very convenient; this software is widely used in the community, and maintained properly. If calculations from scratch on some examples will give the same results as LBLRTM, we will select the most convenient way to proceed. c) Section 3, the use of SCIAMACHY data: The authors have done a lot of work here, but I would suggest including only those elements of this section that are directly relevant to section 6. This section is 11.5 pages, and it is not clear to me how the onion peeling retrieval of VER from limb scans is relevant to simultaneous nadir retrievals of O2 column and airglow from MicroCarb. Answer: The onion peeling vertical inversion of SCIAMACHY limb observations (accounting for self-absorption by O2) was crucial to determine the true nadir intensity of the O2* airglow that will be observed in nadir MicroCarb geometry, and this was needed to test the algorithms allowing to disentangle the O2* emission spectrum from the general radiance coming from the ground and the lower atmosphere.

d) Section 4, comparison between REPROBUS airglow model and SCIAMACHY observations: The conclusion seems to be that the model underpredicts ozone and so underpredicts airglow, and so is not suitable for estimating airglow instead of retrieving it. Not sure if this is worth 12.5 pages; perhaps this work could be summarized? Answer: This discrepancy between the REPROBUS CTM model and SCIAMACHY O2* data is a very important scientific result. The comparison of GOMOS ozone data with REPROBUS ozone suggests that the airglow discrepancy is due at least (but may be not only) to a deficit in the ozone predicted by REPROBUS at high altitudes. As a result, co-author Franck Lefèvre will completely renew his REPROBUS code with new

chemistry solving algorithms, a "work in progress" expected to be achieved in 2020, and of course well beyond the scope of this paper.

e) Sections 5 and 6: It seems to me that this is the heart of the paper, and other sections should be adjusted so that they contain just what is needed to support the material in these two sections. Answer: as said above, we are reluctant to cut other parts. We wish to keep all the informations available to the reader, necessary for the reproduction of our results. f) Section 7: This seems to be a literature review, and not directly relevant, except perhaps the comments regarding CO2 airglow and potential impact on MicroCarb retrievals. Answer: this is not a literature review, but a non-exhaustive list of some situations when nadir viewing observations of one particular molecule are contaminated by fluorescence of the same molecule. We agree that the case of CH4 and CO are not directly relevant to MicroCarb, but we wish to take this opportunity to draw the attention of other scientists to this problem that seems to have been mostly ignored in the past. O2 band A and CO2 are relevant to MicroCarb. We suspect that contamination of the O2 band A might be larger than estimated by Sioris, because the emission was not estimated below 50 km. Also, it was not measured below 50 km in the analysis of Zarboo et al. of special SCIAMACHY MLT limb mode. g) Section 8, Conclusion: might need adjusting if the revisions above are considered.

Technical Corrections: Some of these corrections may be OBE if the major changes identified above are considered: 1)page 17, eq. 19: I think that the expression under the sq root in the third line should be $r20 - p21$, NOT $r21 - p20$ Answer: yes, you are absolutely correct! In earlier versions of the paper it was correct; a typo was introduced when we switched to word equation style for these equations. In fact $r21 - p20$ is negative. This error is not in the code. 2) Figure 19: It was very difficult to distinguish the stars and diamonds in the plots. Answer: this figure was redrawn with triangles instead of diamonds. 3) page 40, line 21: 4ARTIC retrieves CO2 and H2O on 19 vertical layers: what is the typical number of degrees of freedom for these retrievals? Is the profile information actually meaningful, or is it really just a column retrieval? Answer:

according to some other studies performed by ACRI, the typical number of of degrees of freedom for these retrievals is about 2. Therefore, you are right when you question the approach of 4ARCTIC with 19 levels. However, many other investigations are also using this 19 levels approach. 4) page 41, line 20: "...Henyey Greenstein function with g currently fixed to 0.8." A reference might be helpful here, for the function and for the choice of g. answer: this value of 0.8 is often selected in the literature to describe preferential forward scattering, but may be adjusted later. This kind of topics will be addressed in future papers, and is not addressed here in a detailed fashion: beyond the scope of the present paper. We have added the following sentence: "...fixed to 0.8, a value used frequently in the literature to describe preferential forward scattering, but could be adapted if necessary."

5) page 44, line 4: drop the extra "(" in Ag((_); done line 13: what reference spectrum is used?; Answer: this reference spectrum may be any O2* computed airglow spectrum. This normalization is done in order that the sum of the two coefficients of a linear combination of the two spectra (warm and cold) are of the order of unity, only for convenience. line 14: drop the "." after "spectrum.,"; done line 27: change "spectru" to " spectrum"; done line 28: So random error only, no calibration error, channel crosstalk, etc? Answer: yes, only random error is considered in this exercise with SCIAMACHY nadir viewing data. The random error is estimated from the fluctuations of the data. 6) page 55, line 14: delete "contaminated" (redundant after "contamination"); line 35: change "ETL" to "LTE"?; done 7) page 59, line 11: delete "inclusion of a" (redundant with previous "inclusion in the") done 8) page 66, line 15: I think that O2(b1S) should be $O2(b1\Sigma)$? done Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-54, 2019.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2019-54/amt-2019-54-AC2-supplement.pdf

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