

## Reply to RC1

### General comments

We have grouped by topic the reviewer's comments (reported in italic). Our answers follow each topic. A copy of the revised paper was not requested by the editor at this stage of the reviewing process. Therefore we report within square brackets the page and line numbers [PxLy] where the AMTD paper will be modified. The modified text is then reported in red.

*This paper presents a reasonable strategy for retrieving the vertical distribution of CO<sub>2</sub> in the stratosphere. An alternative strategy would be to use solar occultation instead of thermal emission for these same bands. This could improve the shot noise and the vertical resolution since:*

*1) The sun will always be a stronger source of radiation than the Earth's atmosphere at any wavelength.*

*2) For solar occultation, the vertical resolution is not tied to the vertical structure of the temperature (see Fig. 7 bottom left of Carlotti et al.), and therefore would not worsen severely in the tropopause region as is the case for thermal emission as expected.*

The purpose of this paper is not to compare the performance of emission and occultation experiments with respect to the measurement of atmospheric CO<sub>2</sub>. Our purpose is to show the potentiality of a new strategy that, to the best of our knowledge, was never investigated before.

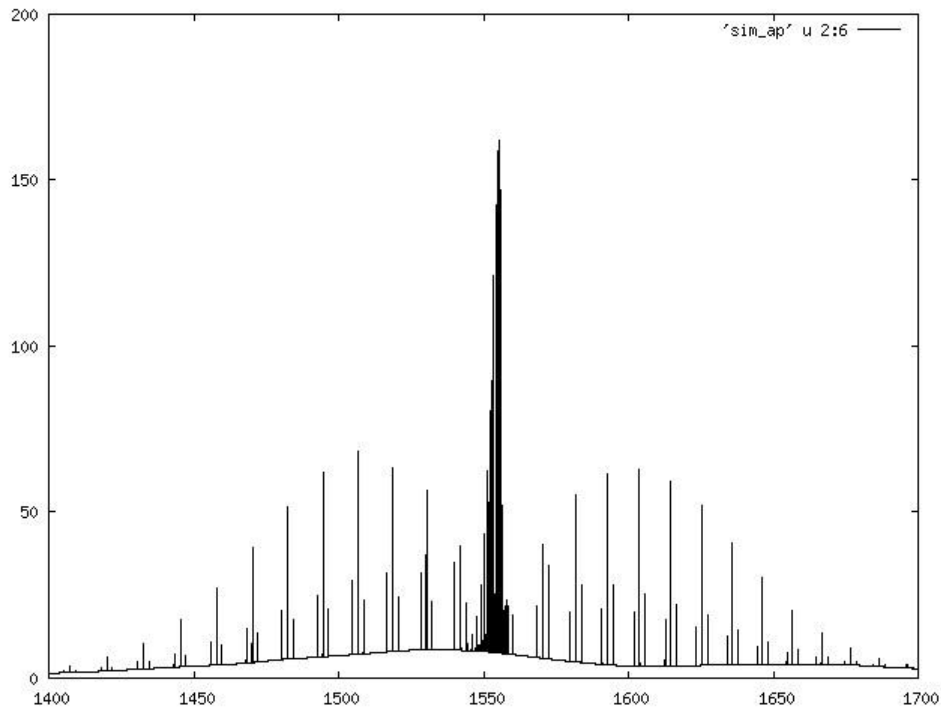
*For CO<sub>2</sub>, given the main interest of the authors to observe the long-term slight increases in VMR in the stratosphere, frequent measurements are not required and thus space-based solar occultation could be applicable.*

We never mention our “main interest to observe the long-term slight increases in VMR in the stratosphere”. The “long-term slight increases” could be a constraint to knowledge imposed by the lack of information about stratospheric CO<sub>2</sub>.

*The authors could consider the O<sub>2</sub> lines in the TIR as well, which are useful in the stratosphere up to ~38 km, based on ACE-FTS O<sub>2</sub> retrieval accuracy. This could alleviate the need for two detectors regardless of whether thermal emission or solar occultation is used.*

We did not consider the use of the O<sub>2</sub> band around 1500 cm<sup>-1</sup> because, as we point out in sect. 2.1, our concern was to avoid as much as possible the effect of interfering transitions and we expect the spectrum to be more crowded in the region of

rovibrational transitions than in the one of pure rotational transitions. Moreover, the intensity of the  $1500\text{ cm}^{-1}$  transitions is 15-20 times lower than that of the FIR transitions. This can be appreciated by comparing the lower panel of Fig. 1 with the figure below that reports a simulation of the  $1500\text{ cm}^{-1}$  band for the same observation conditions.



*There are three main problems with this paper:*

*1) The error budget is incomplete, specifically with regard to sources of systematic uncertainty.*

*the authors present their method as one that has small systematic uncertainties and an accuracy of 1 ppm (P2L30 & P12L3), yet ignore many significant systematic sources of uncertainty and consider only minor ones. The major revisions involve accounting for more sources of systematic error. Furthermore, the statement that CO<sub>2</sub> VMR is retrievable to 1 ppmv between 10 and 50 km is grossly misleading in my opinion. There is no point to getting the community excited about an instrument that can supposedly measure CO<sub>2</sub> profiles to ~1 ppm, when it hinges on line intensities of CO<sub>2</sub> to be measured to 0.25%..... The combined biases in CO<sub>2</sub> and O<sub>2</sub> spectroscopy could either cancel or lead to a 2% bias in the worst case.*

*Some bias correction can be applied by first validating against CO<sub>2</sub> measured using techniques accurate to <1 ppmv but the authors would need to discuss this, especially since they insist on using the term 'accuracy' instead of 'precision' in a couple of spots (see above) in the paper.*

Following these reiterated criticisms, we decided to thoroughly review the error sources that were considered in the MWs selection process. The outcome is that, for

an academic study, the choice was to assess the performance of an ideal instrument so that no instrumental errors were considered. About the spectroscopic data uncertainties, they were also neglected on the basis of the assumption that they can be measured with the desired accuracy. In order to account for the above considerations Sect. 2.2 will be modified at [P4L27] by adding after “Dudhia et al., (2002)”:

For the purpose a set of error sources must be defined and quantified in order to evaluate the uncertainty associated to each spectral point. Here we have considered errors deriving from the VMR uncertainty of all of the atmospheric constituents, and the error deriving from the Non Local Thermal Equilibrium (NLTE) conditions when they are not modeled in the retrieval system. Instrumental and spectroscopic errors have been omitted in this academic study by assuming that, in the case of operational implementation, they will have to be assessed on the basis of the existing technology. According to these statements we will introduce the following changes:

In the Abstract “accuracy” will be modified into **precision** at [P1L17], and the period starting at [P1L21] will be:

The error budget, estimated for the test-case of an ideal instrument and neglecting the spectroscopy errors, indicates that, in the 10-50 km altitude range, the total error of the CO<sub>2</sub> fields is set by the random component. This is also the case at higher altitudes provided....

In the Introduction section at [P2L30] “target accuracy” will be “**target precision**”.

In Sect. 4.4 the period starting at [P10L17] becomes:

This budget indicates that, among the considered error sources (see Sect. 2.2), the dominant components.....

In the “Conclusions” section the paragraph starting at [P11L29] will be:

The assessment of the systematic errors considered in this study (VMR of the atmospheric constituents and NLTE conditions) shows that below 50 km their contribution to the total error.....

The sentence starting at [P12L2] will be omitted in the revised paper.

*The claim that dedicated spectroscopic measurements will be made in the future is not acceptable to me for the present manuscript. I would assume the spectroscopists previously involved in measuring line intensities were dedicated to achieving the best accuracies possible. See table 3 of Tashkun et al. (2015). Searching through the systematic uncertainty column (2nd last column) of this table, I see values as low as 2% (e.g. by Delière et al., 2012) for the lines in a region overlapping the spectral region proposed by the authors. The latter study was dedicated to a specific band and is recent (2012). I take this to be a reasonable or even favourable estimate of the expected uncertainty in CO<sub>2</sub> spectroscopic line parameters in the OXYCO<sub>2</sub> experiment.*

To date, CO<sub>2</sub> line intensities (as well as other spectroscopic parameters) can be determined with uncertainties that are better than 0.3 % (see e.g. Oleg L. Polyansky et al., Phys. Rev. Lett. 114, 243001 (2015), G. Casa et al., J. Chem. Phys. 130, 184306 (2009)). Apart from this consideration, our paper should not be read as a proposal to a space agency. As stated above we investigate the potentiality of a new strategy that, if ever considered for operational implementation, will have to take into account the state of the art of the existing technology for both instrumental errors (see previous answer) and the laboratory measurements. We will remark this point in the “Introduction” section by adding after the period at [P3L6]:

**This academic study is directed to assess the intrinsic capability of the proposed approach irrespective of some technological aspects that need to be evaluated when an operational experiment is considered.**

On the other hand, the reviewer seems to neglect that, as specified at [P8L9-11], “the dominant information about T comes from the shape of the Planck function rather than from the dependence of the line strengths from T” (see Carlotti et al., 2013 where this statement is better quantified).

*Also, it hinges on O<sub>2</sub> line intensities to be measured extremely accurately: a 1% bias in O<sub>2</sub> line intensities will lead to a 70 m bias in TH (or a 1% bias in pressure). This will translate to a ~1% bias in CO<sub>2</sub>, much larger than the sources of systematic error that the authors have selected.*

We retrieve pressure (P) together with temperature (T) and the VMR targets. Since we do not assume hydrostatic equilibrium, any altitude bias translates into a bias on the retrieved P profiles. As reported in Sect. 4.1 the errors on P have a negligible impact on the CO<sub>2</sub> VMRs. This consideration already appears in Sect. 4.4.

*O<sub>2</sub> does not appear to be adding much p T information outside of the 20-35 km range, raising the question about strong correlations between CO<sub>2</sub> VMR and T. The authors correctly state (P2L13) that strong correlations exist between retrieved T and retrieved CO<sub>2</sub> when retrieving T from CO<sub>2</sub> lines and this correlation “prevents” the retrieval of CO<sub>2</sub> from these same lines. I believe the authors have the same issue over a large portion of their retrieval range since Figs. 5 and 6 show that the CO<sub>2</sub> VMR precision is not changed much if the O<sub>2</sub> lines are used or not outside of ~20-35 km.*

The reviewer refers to [P2L13] in the “Introduction” section where we pose the problem. We carried out the test with and without O<sub>2</sub> transitions just “to assess whether and to what extent the FIR observations are necessary and contribute to the precision obtained in our retrieval tests” [P9L27-28]). In our opinion this was a

question to answer and Figs. 5 and 6 provide the answer. These figures are self-explaining and we don't think further comments are necessary in the revised text.

*I consider only the region between 20-35 km to be appropriate for retrieval and I believe the authors should “prevent” themselves from retrieving outside of this range, given their retrieval setup.*

Part of this study is also the assessment of the altitude range where CO<sub>2</sub> can be retrieved. In order to do this, the vertical region of the retrieval had to be as wide as possible. The reviewer writes the above statement only after having inspected Figs. 5 and 6 that are results of our study. Nevertheless, these figures show that the altitudes outside the 20-35 km range could be appropriate for the retrieval of CO<sub>2</sub> VMRs even without the FIR contribution.

*The 20-35 km corresponds to the region where the information load (IL) is large, whereas outside this altitude range, both above and below, there is a sharp decrease in IL. I believe that the pT information is coming predominantly from CO<sub>2</sub> lines (whose IL tends to be larger in the upper stratosphere) and that strong correlations will result.*

This is a reasonable interpretation. On the other hand the IL analysis provides indications about the distribution of information with respect to a single retrieval target. Therefore we prefer not to include this conjecture in the text because, as we notice at [P10L10-11]; “the complex interdependence between the many variables of the MTR inversion makes difficult the interpretation” of our results in terms of IL.

*The major revisions involve accounting switching back to sequential retrievals. The authors also admit (P8L2) that when they tried the sequential estimation, they could not retrieve CO<sub>2</sub> VMR precisions that approach the target value because of problems retrieving T exclusively from the O<sub>2</sub> lines. I believe the authors should demonstrate that they can retrieve CO<sub>2</sub> in a sequential setup over this ‘sweet-spot’ range. By going to simultaneous retrievals of CO<sub>2</sub> VMR and pT, the authors could be confusing themselves in terms of the benefit of the O<sub>2</sub> lines.*

In our opinion the simultaneous retrieval of CO<sub>2</sub> VMR and pT (and also H<sub>2</sub>O VMR) is not a source of confusion but a powerful tool widely used in the analysis of remote sensing measurements. In our study we have verified that the sequential estimation is not suitable to get the required precision (even in the sweet-spot) because of the insufficient precision of T provided by only the O<sub>2</sub> lines. We have demonstrated that the problem can be overcome by exploiting at best the available retrieval techniques such as the 2-D approach (that merges information from adjacent limb-scans and is not applicable to sun occultation measurements since they do not observe along the

orbit plane) and the MTR approach that merges information about pT from the spectral features of different atmospheric constituents (O<sub>2</sub>, CO<sub>2</sub> and H<sub>2</sub>O in our case). Moreover, the benefit of using the O<sub>2</sub> within a Multi-Target, instead of a sequential Retrieval, is not only the independence of its VMR from the CO<sub>2</sub> VMR but also (as specified at the end of Sect. 4.1) the advantage obtained by joining FIR and TIR observations that makes the sampling of the Plank function more extensive and nails down the temperature more efficiently than using TIR or FIR alone. Therefore we don't see any reason to switch to a strategy that we know to be unsuited to our objective.

*Figures 5, 6, and 8 are of low quality (and I am not very picky).*

We have generated a new version of these figures (of better quality) for the revised manuscript.

*Figure 1 does not serve the intended purpose. It shows me that the O<sub>2</sub> lines are not prominent, which contradicts the claim by the authors (P1L26).*

The reviewer is right (we assume he refers to P2L26). The lower panel of Fig. 1 is meant to show an overall picture of the atmospheric pure rotational spectrum of O<sub>2</sub> that we could not find in the literature. The statement about prominence of O<sub>2</sub> transitions is taken from the reference “Carli and Carlotti, 1992” that we will add in the revised manuscript after the period at [P2L26] and in the “References” section. In the upper panel of the figure we do not see the prominent lines of O<sub>2</sub> because of both the insufficient frequency scale expansion and the low TH. After the period at [P2L29] we will add in the revised paper:

**The compressed scale of Fig. 1 prevents the identification of O<sub>2</sub> lines in the upper panel. However the comparison of the two panels shows that, below 170 cm<sup>-1</sup>, the intensity of the O<sub>2</sub> lines matches the maximum emission of the atmospheric spectrum. On the basis of these considerations.....**

The upper panel of Fig.1 is also meant to show the “steep growth of the Planck function” that we mention at [P3L1].

*A nice addition to Figure 2 would be the IL for TIR + FIR.*

We considered this further panel but we decided not to include it in Fig. 2 because, due to the higher values of the IL in the TIR, the quadratic summation combination-law (see the matrix algebra in appendix A) and the resolution of the color palette makes the TIR+FIR map indistinguishable from the upper right panel. After the period at [P5L30] in the revised manuscript we will add:

(The IL with respect to the T of combined set of 15 MWs is not shown because, due to the different magnitudes, the quadratic-summation combination law makes this map quite similar to the upper right panel).

## Specific scientific comments

*P1L21 ..... Something should be said about the impact of thin clouds, particularly on the TIR radiances, since the authors talk about retrieving in the troposphere many times.....*

The issue of clouds is not considered as it is a known problem for TIR radiances in general. No specific effect is expected on OXYCO<sub>2</sub>.

The answers and the modifications proposed within the above “general comments” cover the remainder of this reviewer’s comment (not reported).

*P2L15 The authors could refer to Emmert et al. in Nature Geosci. for the mesospheric ACE-FTS CO<sub>2</sub> measurements.*

The suggested reference will be added at [P2L15] and in the “References” section.

*P2L19 There is no such discussion in Bernath et al. (2005).*

True. We will correct the reference that is to Foucher et al., 2011.

*P4L8 1.5 cm<sup>2</sup> sr is a very large throughput. I’m wondering if this is a typo. Could the authors specify the solid angle subtended by the field of view?*

This was a typo. The right number is 0.015 cm<sup>2</sup> sr. The whole period will be reassembled after the period at [P4L7] as:

**The NESR requirement assumed for this study can be obtained with a detector-noise limited spectrometer with an optical layout, similar to the one used in SAFIRE, with an optical throughput of 0.015 cm<sup>2</sup> sr, and using 4.2 K cooled detectors.**

This modification also accounts for the reviewer’s request that we report below within the “minor comments”.

*P7L1 State clearly whether A and phi are variable or constant along the OC. I assume they are variable from this line.*

The required statement will be introduced by modifying the [P7L9] as:

For each perturbation profile along the orbit a random value of  $A \leq B$  and a random value of  $\phi$  between 0 and  $2\pi$  is assigned.

*P9L17 Are these the B values used in Figure 2?*

Figure 2 shows IL maps that, as specified at P5L20-21, have been calculated using the reference climatological atmosphere. There is no reason to perturb the reference atmosphere for the IL calculations.

*P9L19 values -> absolute values*

Done

*P24 As mentioned above, there does not appear to be much relaxing of the strong correlation (P2L23) outside of 20-35 km since the red and green lines are not very different. The authors need to show that the strong correlation of T and CO<sub>2</sub> VMR is not a problem outside this vertical range.*

See the answers about this point provided within the “general comments”.

### **Minor comments**

*P1L15 operational limb sounders -> an operational limb sounder. (I see MIPAS as the only operational limb sounder).*

MIPAS is the only operational space-borne TIR limb sounder. Other operational limb sounders exist in different spectral regions (e.g. MLS) and on different platforms (balloons or aircrafts). In our paper we exploit the heritage of SAFIRE which is a FIR aircraft-borne limb sounder.

*P1L27 biosphere -> atmosphere*

CO<sub>2</sub> affects the radiative budget of the entire “zone of life on Earth” (the biosphere).

*P4L9 Can the authors be clearer that this is one component of the overall noise, related to the detector? Also, I understand the units, although they don't appear to be power units and this may confuse some readers.*



For this feasibility study we have considered a detector-noise limited spectrometer (which is consistent with the FTS choice) having the main requirement of 5nW for the spectroscopic measurement. We will modify the sentence starting after the period at [P4L7] as:

The NESR requirement assumed for this study can be obtained with a detector-noise limited spectrometer with an optical layout, similar to the one used in SAFIRE, with an optical throughput of  $0.015 \text{ cm}^2 \text{ sr}$ , and using 4.2 K cooled detectors.

All of the minor comments that we don't mention will be implemented in the revised text as suggested by the reviewer.