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Atmospheric Measurement Techniques
Discussions

# *Interactive comment on* "Impacts of spectroscopic errors on $O_2$ measurement requirements for the ASCENDS mission" *by* S. Crowell et al.

**F.-M. Bréon (Referee)**

breon@lsce.ipsl.fr

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Comment on "Impacts of spectroscopic errors on O2 measurement requirements for the ASCENDS mission" submitted for possible publication to Atmospheric Measurement and Techniques by Sean Crowell et al.

This paper analyses the potential of a differential absorption (DIAL) measurement in an oxygen band, associated to the same in a CO2 band, to normalize the surface pressure impact on the CO2 column estimate.

I found this paper rather difficult to follow. This is because the paper focuses on the mathematical development rather than the physical interpretation of the results. I provide examples below. Although I carefully read the paper 4 times, I still could not un-



derstand several aspects. In the present state, the paper may be accessible to a very narrow community, to which the present reviewer does not belong. I remain convinced that, in addition to my own deficiencies, the paper lacks physical interpretation of the results. The fact that the figures are not discussed in the body of the paper is a clear indication of that. In addition, I think there is an error in the mathematical development (next paragraph). I therefore recommend major revision.

Equation A5 shows the sensitivity of the differential CO2 optical depth $\Delta\tau$CO2 to the surface pressure. It is supposed to be derived from equation 1. Yet, when I derive (1) with respect to p*, I get a result that is very different from that of the authors: (qCO2(p*)$\Delta$xCO2(p*))/(ma g). This is because they make the (invalid I believe) assumption that the CO2 profile qCO2(p) varies with p*. Indeed, this derives from the author assumption of a sigma atmospheric profile. This problem also affects equation A6. How this error impacts the results is unclear to this reviewer.

As discussed in section 2, the DIAL observables are the differential optical depth $\Delta\tau$CO2 and $\Delta\tau$O2, or their ratio $\Delta\tau$CO2/$\Delta\tau$O2. R is the observation error covariance matrix. However, the paper uses the observations independently. R is then a scalar. It is then misleading (and I have been misled) to state (line 141) that R is treated as a diagonal matrix. It may be easier for the reader not to use matrices for scalars.

In section 4.1, a short analysis is given of the surface pressure error in NWP model. This is done through a comparison against surface observations. 1$\sigma$ and 2$\sigma$ statistical values are provided. Is there any reason why the latter is not double the former ? If not why provide both ? As for the results of this analysis, I was surprised to read that the surface pressure statistical error is larger over the US than it is over the global region, in particular since the analysis over the US uses a higher resolution model. I would think that, on average, the atmosphere is better modelled over the US than it is over the world. The result deserves at least some discussion.

In section 4.2 (line 225), the central wavelengths for the CO2 and O2 channels are given. There are several options. Yet, there are no justifications nor argument for the various options.

Line 267 : The layer optical depths are normalized by either the pressure or the vertical thickness. What is the usefulness of a vertical thickness normalization? What choice was made in the paper, in particular for Figure 11 ?

Line 274 : "The weighting functions were created for -10 pico meters offset for the CO2 absorption features. . ." What is the justification for an offset. How was the 10 pico meter chosen ?

Line 276 : Figure 11 (should be 1) is mentioned but absolutely not discussed. No need to show a figure if it does not seem to provide any input to the analysis.

Figure 11 shows the measurement weighting function. There seems to be different choice for the central wavelength of the lidar measurement. These possible choices lead to very different results on the weighting function. These are never discussed in the paper. One choice seems to lead to a weighting function that is proportional to atmospheric pressure. Other choices lead to maxima of the weighting function that is higher up in the atmosphere. I tend to assume that, if the O2 and CO2 channels have very different weighting functions, the results will be significantly different than when the weighting functions are similar.

Line 283 : "This matrix has dimensions (2nlayers)X(2nlayers)." I could not follow

Line 292 : Figure 12 (should be 2) is mentioned but is not discussed. In the legend of Figure 12, it is said that the variance of one is two orders of magnitude larger than the others. This is not even mentioned nor discussed in the body of the paper. I assume this has strong implications

Line 307 : The uncertainties in $\Delta\tau$CO2 are provided in %. I wonder why they are not provided in equivalent ppm, which can be done as the authors make the assumption of a mixing ration of 400 ppm. The input numbers are provided in Table 11 (should be



1). In this table, the values are provided with 4 significant digits, which is ridiculous. In the table, there are many different values depending on the choice of the central wavelength for both the CO2 and the O2 band. Yet, in the text, a single value is provided with no discussion on the variability with the channel choice. I tried to go from the values of Table 11 to the percentage given in the text (line 307) but could not. I made the assumption that both $\Delta\tau$CO2 and $\Delta\tau$O2 are close to 1 because this is optimal for remote sensing. Please correct me if I am wrong.

It may be the result of a deficiency in the reviewer capability, but he could not understand equation 9. Indeed, equation 9 seems to be layer-dependent (the derivation indicates "i") when the potential user of the DIAL product is interested in column integrated quantities. Similarly, the reviewer could not understand the derivation of 10 from 9.

Figure 11 : Why use a logarithmic scale on the Y axis ? The integration is on P, not log(P). As the figure is shown, it gives too much importance to the high level (low pressure) of the atmosphere. On the same figure, the labelling of the X axis is strange.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 6855, 2014.