**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.

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Comments from Breon ============================== Comment on "Impacts of spectroscopic errors on O2 measurement requirements for the ASCENDS mission" submitted for possible publication to Atmospheric Measure- ment 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. Al- though I carefully read the paper 4 times, I still could not understand several aspects.

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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 fig- ures 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.

Specific Comments:

Equation A5 shows the sensitivity of the differential CO2 optical depth $\triangle\tau$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*)$\triangle$xCO2(p*))/(ma g). This is because they make the (invalid I believe) as- sumption that the CO2 profile qCO2(p) varies with p*. Indeed, this derives from the author assumption of a sigma atmospheric profile. This prob- lem also affects equation A6. How this error impacts the results is unclear to this reviewer.

RESPONSE: Equation (A5) is the derivative of equation (A1), not equation (1). A1 represents the transformation from the model CO2 to the measurement. It is true that this is a consequence of discretizing the atmospheric column. The difference between deriving (1) and (A1) is necessary since we are looking for the information in the measurement on the model profile of CO2, since transport inversions are done in the the discretized atmosphere. We will add text and equation references to clarify this.

As discussed in section 2, the DIAL observables are the differential optical depth $\triangle\tau$CO2 and $\triangle\tau$O2, or their ratio $\triangle\tau$CO2/$\triangle\tau$O2. R is the observation error covari- ance 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.

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RESPONSE: This is fair, and the sentence in question will be made clearer in the revised draft.

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. 1σ and 2σ statistical values are provided. Is there any reason why the latter is not double the former ? If not why provide both ?

RESPONSE: The error statistics are not necessarily normal. We have not examined the histogram of errors carefully to check for normality, but we shall for the revised manuscript.

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.

RESPONSE: The following text highlights the discussion we will include in the revised manuscript: 1) The CONUS model view consists not only of the USA but Canada, Mexico and much of Central America, Cuba and the the Caribbean. 2) The fact that this region is well instrumented has it pluses and minuses: a) not only are populated costal and flat lands are well represented by surface measurements, but also those with very diverse terrain and height differentials. This makes the statistics more terrain neutral unlike much of the rest of the globe (excluding the CONUS and Europe) where observation are biased towards population centers and not necessarily what is best for the NWP community. b) the extent to which all observation are incorporated into anyone model is at the discretion of the modeler making them more or less independent measurements. So even though there are more potential inputs to the NAM, they may not be used to as tightly constrain the model allowing the model values to depart from the observations in a more substantial way c) In densely sample areas, the model

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representation for any given grid box maybe a compromise of multiple observations given an representative box height.

Given all these and other factors that are model implementation specific, the provided numbers were meant to provide a bound on the uncertainties in surface pressure not specific regional statistics and are used as such in this work.

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.

RESPONSE: The selection of the central wavelengths is done by the instrument design teams at JPL, NASA Goddard and NASA Langley. References to the papers published by the ASCENDS team members that discuss the different wavelengths' strengths and weaknesses will be included in the revised draft.

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 ?

RESPONSE: The weighting function for the lidar measurement is defined as the derivative of co2 mixing ratio normalized differential optical depth with respect to pressure or height. The usefulness of the vertical thickness normalization is the ability to see the sensitivity as a function of the geometric height above the surface, which is often compressed in a sigma coordinate.

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 ?

RESPONSE: This again is a design choice of the ASCENDS instrument teams at the NASA centers. The fact that the details about these wavelengths are interesting to the reviewers indicates a more thorough review of the ASCENDS instrument concepts

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would clarify a lot of the discussion.

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.

RESPONSE: This figure is an example weighting function for the various instrument concepts. We will discuss this in greater detail in the revised manuscript.

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.

RESPONSE: More discussion of the weighting function choices and their shapes will be present in the revised draft.

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

RESPONSE: We will change "this matrix" to "The matrix $R_{\Delta\xi}$" in the revised draft. The matrix has dimensions that correspond to the number of model layers, and the dimensions are twice that value because there is a variation due to temperature and another due to water vapor.

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

RESPONSE: This figure will be revised and discussed in greater detail in the next draft. It actually does not have strong implications, because the impact of the variations in

T/Q on the weighting functions is much smaller than the surface pressure effects

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 be1). In this table, the values are provided with 4 significant digits, which is ridiculous.

RESPONSE: We are using the results of a RT computation from a model, not a measurement of OD here, so our precision is limited to machine accuracy.

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.

RESPONSE: All of the numbers across the 1mb row in Table 1 round to 0.09 with two significant figures, which is nearly 0.1

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.

RESPONSE: The tabulated values of Delta tau are different for each gas and wavelength, and these numbers will be discussed in more detail in the revised draft

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 inte- grated quantities. Similarly, the reviewer could not understand the derivation of 10 from 9.

RESPONSE: Equation 9 is the component form of Equation 4 with the explicit derivatives rather than the Jacobian H. Indeed the column is the thing that is measured, but

we state the inequality layer-wise, because it is more general. From this form, the fact that the information content for the CO2 only measurement is smaller than the ratio measurement follows by squaring and summing. With regard to Equation 10, this comes from breaking the observation error variance into the three component forms discussed in previous sections and moving all of the terms to the right side except for the precision component for the O2 measurement. Clearly more discussion of these derivations is needed, and will be provided in the revised draft.

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.

RESPONSE: The reviewer's point is well made. We will adjust scales for the revised draft.

Comments to Ref 2 The retrieval of atmospheric CO2 using lidar observations depends not only on the lidar differential absorption measurement itself, but also on the assumed state of the atmosphere in the vicinity of the measurement. The surface pressure, temperature profile, and water vapor profile are the key atmospheric parameters that influence the retrieval. An O2 lidar, co-aligned with the CO2 lidar, can be employed to retrieve the surface pressure. The authors address the question "How accurate must a lidar determination of the surface pressure be in order to improve the estimate when compared with using surface pressure data derived from NMP models?" The authors use an information-content based methodology for assessing the O2 lidar measurement requirements. Their results are an important finding.

Specific Comments: **This manuscript appears to be excerpted from a larger, more in-depth investigation. Curiously, the first table and the first figure are denoted Table 11 and Figure 11, respectively.**

RESPONSE: The misnumbering of the Tables and Figures was due to a LaTeX pro-

cessing issue, and was resolved in the professionally typeset version of the document.

The reader would be more informed with the inclusion of additional information to subsection 4.2 and section 5, where the "environmental contribution" to the observation uncertainty is described and quantified, and the impact on the O2 measurement requirement is quantified.

First, it is important to clarify specifically what lidar on-line/off-line frequency pairs were used in this analysis. For the 1.571 μm CO2 lidar, we understand that on-line wavelengths are either 3 picometers (pm) or 10 pm displaced from line center. The on- line frequencies are provided for the other three lidars in section 4.2. What are the off- line frequencies for the 1.571 μm and 2.051 μm CO2 lidars? Ditto for the 0.765 μm and 1.263 μm O2 lidars. The sensitivities to water vapor uncertainties in particular can depend on the specific off-line laser frequencies as well as the on-line laser frequencies, as pointed out in Caron and Durand [Applied Optics 48, 5413-5422, 2009].

RESPONSE: We were not precise in stating the online/offline pairs, nor the motivation for the pairs used for the statistics plotted in the figures. We will correct this in the revised manuscript.

Figure 12 ("Spectroscopic Error Variance") provides results vs. altitude for each of the four lidar cases. I would like more insight into the relative contributions due to temperature uncertainties and water vapor uncertainties. What governs the altitude dependences of the 2.05 μm and 1.26 μm cases?

RESPONSE: The altitude dependency is governed by combination of the weighting functions shown in the upper panel of figure 11 and the uncertainties in temperature and moisture as a function of vertical height.

Why the dramatic increases near the surface? Is this due to water vapor uncertainty? Can you separately show results for the continental U.S. vs. the global

basis?

RESPONSE: The increase in differential optical depth errors at surface is due to both increase in T and RH errors near the surface, the weighting functions and temperature sensitivity and a number of atmospheric related effects which vary on a line-by-line basis that include pressure and temperature broadening, shifting of the line due to changes in temperature, as well as the impact of a complex set of strong/weak water vapor lines that also vary within the selected band.The analysis was done without taking geographic location into account because we were specifically interested in a generic result over the globe. We would expect the size of these errors to be much smaller in the US since the observational network that drives the global NWP models is so much more dense over CONUS that the rest of the globe.

The near-surface values for the 2.05 μm case, relative to the values for the 1.57 μm case, do not appear to agree with the relative sensitivities to temperature and water vapor that are in Caron and Durand [2009].

RESPONSE: The work by Caron and Durand, which provides an estimate of the relative impact for a number of error terms on the observed optical depths and the resulting XCO2 values, constructs separate relative error terms for both T and WV by adding a constant value to temperature as function of height and multiplying the WV by 1 plus some epsilon value. While this approach provides reasonable results for a single idealized atmospheric state vector it is hard in general to separate the two for an ensemble set of profiles that are only physically consistent on a profile-by-profile basis. One could to attempt for instance to mix the observed T profiles with the modeled water vapor and compare the results to based on observed T plus observed WV. While this is mechanically possible, it does not mean that the resulting observed T plus modeled WV state vectors are physically realizable, and or their results provide meaningful results. For this reason, the authors of this work have chosen to treat the atmospheric state as single entity not as separable parts.

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