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Interactive comment on "Improvements of the OMI O_2-O_2 Operational Cloud Algorithm and Comparisons with Ground-Based Radar-Lidar Observations" *by* J. Pepijn Veefkind et al.

Anonymous Referee #2

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General Comments

The paper describes several modifications to an Ozone Monitoring Instrument (OMI) cloud pressure and cloud fraction retrieval algorithm which derives information from the hyperspectral reflectance measured at the oxygen dimer absorption feature near 477 nm. The improvements to the algorithm are diverse, and two in particular, the use of climatological temperature profiles, and a new look-up table scheme, produce significant, systematic effects in the retrieved cloud pressures at small cloud fractions. Other changes have a smaller general impact but are appropriate. The authors highlight the utility of their cloud pressure data for trace gas applications, particularly where the cloud fraction is low. These new satellite cloud pressure data agree well with ground

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based measurements of cloud height at mid-latitudes when cloud conditions are most favorable for comparisons. A significant amount of work has clearly gone into the updates to the algorithm which are presented here, and the focus on these low cloud fraction data is very relevant and important to the satellite trace gas community, so this work is a relevant and useful contribution to the literature.

Specific Comments

Though the authors carefully and extensively documented and investigated the changes they made in their revised algorithm, the motivation for these changes is presented less rigorously. The manuscript states that the OMCLDO2 product is "driven by what [trace gas] algorithms need for cloud information" (33), but in the introduction there is no explanation of precisely why the cloud data are needed and how they are used. The authors are asked to address this in some fashion, as it will provide context for the improvements made to the algorithm, and give greater significance to the changes and therefore this paper.

The term effective cloud fraction is used several times in the manuscript, but it is not clearly defined. A mathematical definition is easy to give in the introduction and should not be left for the reader to find in cited references. Further into the paper, the authors stress that the changes, and uncertainties, in retrieved cloud pressures are highly dependent on cloud fraction, so a definition of effective cloud fraction presented early will assist critical understanding of the reasons behind the statements. In the sentence beginning at (559), please explain what is being weighted by the effective cloud fraction.

Why are tropospheric trace gas retrievals, like those of NO2, so sensitive to pressures in particular, such that “biases will have a significant impact on trace gas retrievals, commonly limited to scenes with small cloud fractions.” Is it possible to evaluate the quality of cloud pressure data retrieved at low cloud fractions in any way, given their greater associated uncertainties, and the importance the authors place on these data? The use of the scene pressure and albedo data for trace gas retrievals appears rela-

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tively limited. How are scene pressure and albedo data reported in the new product intended to be used?

The authors contrast in fig. 1 and its caption that there is a difference between the primary cloud model used to support trace gas retrievals, the Independent Pixel Approximation (IPA), and another, the Lambertian Equivalent Reflector (LER) model. With this parallel exposition it is not clear whether the authors suggest that the IPA model is, by definition, composed of Lambertian opaque reflectors. If so, this is incorrect. In fact, Zuidema and Evans (1998) which the authors cite, used a Heyney-Greenstein approximation for Mie scattering in their IPA plane parallel cloud simulations.

More likely, the authors here are aware that the IPA approach simply assumes that neighboring pixels (or satellite sub-pixels in the present case), can be described as horizontally independent with regard to radiative transfer. However the text and figure captions (parenthetical as they may be) are likely to lead some readers to think otherwise; that the IPA approach necessarily uses opaque Lambertian surfaces, and it would be unfortunate to encourage that strict association. Could the authors please clarify part of the discussion as they see fit.

The comment at (345-346), that it is common for scene pressures over the cloudless ocean to be greater than surface sea-level pressure, is puzzling. The question arises whether something, physical or numerical, similarly biases the clear sky calculations in the IPA cloud pressure estimates the authors provide?

In fig. 2 it is not clear why in the lower (cloud pressure) figure some of the circle symbols representing the table calculations near reflectance of 0.1 have a different colors than the surrounding interpolated colormap beneath. Could this be a plotting error? Also, the interpolation in the lower left of this same figure (dark red) looks somewhat strange. How far away from the table nodes is the extrapolation permitted?

Is temperature dependence of the cross-section an issue or not (179-180)?

Can the statement at (235) be further explained in terms of the physics or of the model used for the cloud pressure retrievals?

Please indicate in table 3 whether the differences reported are old minus new or the opposite. It seems to be the former, consistent with maps shown in fig. 4. In table 3, the effect of new look-up tables is reported as -0.01 while at (225) this difference has a positive sign. The sign and magnitude obviously matter at low cloud fractions where a small change in cloud fraction can have a large impact on air mass factor. Please clarify.

Suggest mentioning OMI is on a satellite in the abstract near (8).

Is the amount of information in the OMI spectral range truly limited, as stated at (38)?

Replace “middle of” to “scattering within the” at (55).

Are the authors referring to differences in the algorithm, or in the results, at (72)?

Use of “cloud pressures” at (73) is unspecific. Please indicate these pressures are satellite-derived.

The correction discussed at (344) may not be needed for the OMCLDO2, but saying this about all OMI retrievals seems unwarranted.

Finally, the authors use the term “a priori” to refer to a wide range of input data used by retrieval algorithms, including theirs. For example they describe their absorption cross-section measurements and new DEM data as a priori information. From a philosophical perspective this is not entirely incorrect, but I feel the term a priori should be reserved for data that describes assumptions, often implicit, about the character of the solution to an ill-constrained inverse problem as discussed by Jackson (1979). Neither the cross-section or DEM data fall squarely within this category. The assumed shape of the vertical O2-O2 profile would however, because that information is used to constrain the inversion of O2-O2 slant column to estimate cloud pressure.

Technical Corrections - changes and additions below are in [square brackets]

(1) The readability of the title would improve if the authors changed "Improvements" to "Improvement," or replace "of" with "to."

(32) Suggest using "field-of-view" instead of "ground pixel."

(76) "DOAS (Differential [Optical] Absorption [Spectroscopy])."

(77) "fit is performed on [radiances in] the spectral region."

(83) "of the [irradiance measurement] Doppler shift."

(85) "are [matched] on the same spectral grid."

(96) "diagnostics of the fit [are] obtained."

(99) "Although [all] the information."

(99) "some [bad pixels] may remain."

(115) Remove "respectively."

(152) The phrase "even if" is ambiguous. Consider a revision.

(160) Specify n is a function of z, i.e. n(z) in eq. 2.

(175) The O2-O2 cross-section in eq. 4 is shown twice.

(349) "Comparison with ground-based [R]adar [-Lidar]."

(354) remove "As discussed below" since this is unnecessary here.

(364) Cloudnet is mentionned here for the first time here without explanation.

(437) remove the word "of."

References

Jackson, D.D., The use of a priori data to resolve non-uniqueness in linear inversion, Geophys. J. R. Astron. Soc., 57, 137–157, 1979.

Zuidema, P. and Evans, K.F., On the validity of the independent pixel approximation for boundary layer clouds observed during ASTEX, J. Geophys. Res. Atmospheres, 103(D6), 6059–6074, 1998.

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