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Interactive comment on “Cloud top pressure retrieval with DSCOVR-EPIC oxygen A and B bands observation” *by* Bangsheng Yin et al.

Anonymous Referee #1

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This paper introduces an algorithm for the determination of the cloud top pressure inferred from measurements of oxygen absorption in the NIR by the EPIC sensor onboard the DSCVR platform. The topic is important and appropriate for the journal.

The paper shows some sound science and the authors have structured their manuscript in the correct way. All major sections needed to present a retrieval algorithm are, in my opinion, addressed. However, major improvements are still needed and I will be willing to evaluate a revised version of the paper. I bullet-list improvements and remarks "general comments" section and then I delve in the explanation of specifics later on.

*** General main comments

Printer-friendly version

Discussion paper

- Before any scientific content scrutiny, I suggest to throughly check punctuation, syntax and word C-(c)-apitalization and arrangement prior publication. Copernicus service should definitely help here, but also and foremost checks by the english native co-authors. Uneven sentences or awkward wording are present throughout the manuscript and are too many to be listed by a referee. This will help to showcase the logic of the method and the importance of the results.

- In the introduction state clearly and make explicit the difference with Yang et al. JQSRT, 2013. As both papers share the same goal, data source and co-authors, it is important to highlight the advancement achieved in this paper with respect to previous literature. Some scientific insights about the difference between the A and the B-band are given in Yang et al. but are put to little of any use in this work. One would expect some science advancement and not a mere application or repetition of a method. All my criticism and required improvements naturally follow from this remark.

- Therefore, the treatment of aerosols is overly simplified or neglected together with error analysis as function of cloud optical thickness or cloud cover, since we know that from the remote sensing perspective these two quantities are connected.

- The coefficients A, B, C (P6 L199) must be presented otherwise the reader is not equipped with the knowledge to replicate the results.

- The presentation and analysis of the results is suboptimal. Without proper and customary validation with external independent data sets little knowledge can be won about the applicability ranges of the presented method in real geophysical scenarios, which is one of the stated goals of the paper, otherwise Section 5 would not be presented.

*** Specific comments to individual sections

- Abstract

P1 L28: why "obviously"? It is not a straightforward inference and it is not objective, but subjective instead. Please, remove it from the abstract.

P1 L29-30: could you provide quantitative figures for the comparisons? Something like "Out of N cases, we found an average bias between CTP b- and A-band of xxx hPa $\pm$ xxx hPa_stdv".

P2 L44: "their atmospheric profiles"? You may want to check this, because the atmospheric profile is the same. You are simply converting between quantitities based on the P-T levels.

P2 L46: you may want to cite the Yamamoto-Wark paper as first historical record of CTP retrieval from oxygen absorption.

P2 L49-50: "Many approaches are designed to retrieve clouds' effective top pressures without considering their in-cloud photon penetration, and therefore derive effective top pressures higher than CTP."

I have two remarks for this statement.

1) there are other approaches taking into consideration in-cloud photon penetration. They must be correctly cited. Notably, analytical radiative trasnfer has been implemented by Kokhanovsky and Rozanov, JQSRT 2004 (forward problem) and Rozanov and Kokhanovsky, JGR 2004 (inverse problem) and globally deployed and validated by Lelli et al, AMT, 2012 and Lelli et al. ACP 2014. For the LUT method, the reference is Loyola et al. AMT 2018. So please, cite this literature.

2) The authors assume that the reader already knows the scientific reasoning behind the CTP overestimation / CTH underestimation. Which might not be true. So, please, explain here why the neglection of photon penetration and multiple scattering within the cloud gives rise to this effect.

P2 L 67-68: "the differences between in-band and reference band are negligible". This statement cannot be generalized. So, please add "at nominal EPIC response functions" or similar.

P3 L 86-87: "the ratios of absorption/reference are less impacted by the instrument calibration and other measurement error." I might agree with this statement if the authors can provide at least a reference to some EPIC assessment reports or papers where absolute (nor relative neither ratioed) calibration and degradation of the NIR channels are provided. I tend to believe it is the case but I would like to have this information at hand for sake of consistency.

Still Section -2- does not mention any surface influence. We know that the continuum at 779 nm is impacted by the red edge, whereas the b-band is not. So, I find myself left with the doubt: are the authors aware of this?

P3 Figure 1: Can the authors provide here in the caption or in the text the details of the simulation for these oxygen spectra? Mainly observational geometry, aerosol total load, ozone concentration and surface reflectivity/albedo?

P4 L106-107: "Cloud pressure thickness can be estimated with cloud optical thickness using statistical rules." Which are? Can the authours explain what statistical rules are they referring to and the physical principles behind this statement? References are also welcome along the way (this remark has to be read jointly with the remarks for Section 4.4 below).

P4 L 108-110: "It is worth noting that certain variables will have a non-linear effect on EPIC observations, however, these variations occur smoothly." Well, never poke a bear: could you please explain what are the variables smoothly having a non-linear effect on EPCI observations? First, what observations? Second, are these variables of radiometric or geometric origin? Are they clouds themselves? What kind of non-linear relationship are the authors thinking at? And if it a smooth one, this means it has been already well charachterized. Would you provide some figures or references as well?

P4 L114-116: "In physics, the retrieval accuracy is impacted by two main uncertainty sources: (1) the limited ability of EPIC in identifying cloud thermodynamic phase, which will affect the accuracy of cloud optical thickness retrieval, and 2) the uncertainty in estimating Cloud pressure."

Yes correct. But this is disconnected from the sentence above about the interpolation error and the sentence here reads as a filler. So, I suggest to either expand this paragraph and describe throughly how the total error in CTP splits into random and systematic components, model and retrieval errors, and what originates them or, please, remove this sentence. Also because Section 3.1 is just about the LUT method. Ah, by the way, it would be very insightful to substantiate with numbers or references the LUT interpolation error component. Your choice.

P5 L145-146 and ff: "However, their attenuations from Rayleigh scattering and aerosol extinction are close to each other. Thus ... " I am personally not satisfied by these reoccurring statements in the manuscript. Too general, subjective and overly simplyfing. As such, the inference that photon path length can be derived by ratioing continuum and in-band channels does not follow from that. If you invert the logic, would the converse hold? Saying that molecular and aerosol extinction are not "close to each other" would still CTP retrieval be feasible? I would say it does. So, the issue here is that the authors simply avoid aerosol description for the sake of simplicity, but it is not what one would expect from an algorithm.

P5 L149-151: Please, refrain from wording like "and etc." and try to be rigorous. Assumptions are fine, as long as they are clearly presented and justified by a scale analysis or a scientific reasoning. So, please enumerate all assumptions you make and justify each of them.

P5 and ff: could you please use the standard \tau symbol for optical depth throughout the paper? \t can be misinterpreted as transmission.

P7 L215: missing to introduce the \k_i in the text. Please, correct.

P7 L222 and ff: How does Eq.14 relate to the conversion between CTP and CTH? Please, expand and/or reword this paragraph clearly exposing the practical usage of this relationship w.r.t. cloud parameters to be retrieved. Also, what are the \M_i (i=1...6) model atmospheres? Are you subsetting a yearly cycle in six different model atmospheres? Are you slicing after zonal bands?

P8 Equation 16: please be rigorous and consistent through the paper. Here you use \t as temperature, while \t was optical depth in the previous sections. So, temperature is \T, optical depth is \tau. Also, capital \H is not present in the equation.

For the time being let me assume that the y-axis displays the following quantity: 100*(LBL - DBL_K)/LBL.

Also, without information about aerosol in the simulations, these results indicates that molecular scattering introduces a systematic bias, as can be seen in the continuum outside absorption. For the in-band channels, however, the sign of the residuals reverses. This points to a different treatment of oxygen layered extinction. From the perspective of the CTP retrieval, what counts is the ratio of the channels. Given Fig.2 and the definition of the residuals introduced above, my guess is that you are overestimating molecular scattering and underestimating oxygen absorption.

This translates into a quenched ratio between continuum and in-band channel than it is in reality, so that you will introduce a retrieval bias, because you will assign less oxygen absorption to the EPIC measurements and your CTP_top will be lower (or CTH_top higher).

I admit that after convolution with the instrument response function you might be less prone to this, but then I would appreciate also such values in Table 1, together with the same values for the A-band wavelengths.

In summary:

- please expand Table 1 with results for a Thick Cloud (which optical depth?) - provide also the altitude/pressure of the simulated thin and thick cloud (ensure that you have a representative altitude for the specific cloud: low-level thick cloud and high-level thin cloud) - Specify if the thermodynamic phase of the thin cloud is mixed or ice. Assuming the low-level thick cloud is warm, aka liquid. - Present results for all 4 EPIC channels (680, 688, 764, 779 nm) seperately *AFTER* convolution with the EPIC narrowband functions - It is not clear to me what is the last column about. Is the Difference (+0.08%, -0.02%) the average relative difference across the band or only at 688 nm? As such, these numbers are little informative.

P10 L329: You might be correct about the similar behaviour of the A-band compared to the b-band. However, the presence of the red edge beyond 690 nm would make your results different for Figure 3-d. The authors suggest to have already such results for the A-band as well, so could you please create a separate Figure with only the dependence on surface albedo with the A and b-band together? This is more informative to the reader in general, as there are several instruments not convering the b-band but solely the A-band.

P11 Section 4.4 "Case studies ... "

This section is missing some important information and is disappointing to read because it lacks a clear structure and explanation of the results is not satisfying. I have several remarks.

Beside some corrections listed in the "Minor Comment" section, I wonder why are the authors introducing Eq.(15) about COT while ending the introducing paragraph with considerations about CTP retrieval.

Nevertheless, first, it is not clear where the data for Figure 4 come from. Please add a source repository to enable the replication of your results. It is not clear what L1 data are you processing. So, please give information on the timestamp and the data versioning, reprocessing and so on and guide the reader to the actual source, as not everyone ought to be fluent in EPIC data acquistion and handling.

Second, are the retrievals of Figure 4 for the full EPIC disc? The scatterplots show clustering that must be analysed and understood. So, I invite the authors to subset L1 radiances after underyling surface reflectance and cloud optical thickness, or latitude or cloud system/regime so that you will be able to geophysically explain the scatter-plots. Also, in absence of bias histograms, they must be at least redrawn as heat or occurrence maps with a color coding for the third axis.

Third, Figure 4: you are comparing an "effective" CTP retrieval (the NASA ASDC L2 record) that does not include photon penetration with your "baseline" CTP method, which does not include photon penetration either. And you still have mean biases for low-level clouds of 100 mb and 150 mb for the A-band and b-band respectively. The apparent "banana" shape, bending toward the ground, might also indicate that you are using different P-T atmospheric profiles, which then impact gaseous extinction. Have you ensured that you are using the same atmosphere of the standard L2?

Fourth, I hope that the authors would agree with me that the results of Section 4 are still simply a verification of their algorithm and cannot be considered a real validation of their method. Figure 4 compares tow similar methods (as stated by the authors at P11 L335-336) while Figure 5 is simply an internal check of the methods presented in the paper. These results are already known in the literature bulk of A-band algorithms (e.g. by comparison of SACURA, FRESCO, ROCINN, See the TROPOMI S5P Science Verification Report). So, to gain insight in the validity and limitation of your algorithm and to let the reader decide whether your approch is best suited for a cloud type or another (for instance low-level warm or high-level thin cirrus clouds) independent validation is needed and must be carried out against a different CTP derived from coincident retrievals and alternative methods, being this ground-based or space-borne, your choice. But validation is needed.

Fifth, can the authors provide the reasoning behind the choice of their "statistical approach" to estimate cloud geometrical/pressure thickness? Why are you calling it a statistical approach, I would rather call it assumption. Surely this assumption is based on evidence, likey drawn by references or assessment studies. So, please make the derivation of your assumption about this approach explicit. Moreover, no details on the physics behind are given. Where are all the terms of the expression (i.e. the multiplicative factor 2.5, the additive +26) coming from? Expected limitations and range of applicability of this assumption? Any relationship with/dependence on cloud liquid water content and/or cloud type? One pertinent reference on my own I can come up with is Carbajal Henken et al. AMT, 2015 where CTP is related to pressure thickness and optical depth. But the same result has been obtained also by Rozanov and Kokhanovsky, JGR 2004 and Lelli et al, AMT, 2012 and ACP 2016 (see Appendix). It will be interesting to augment this bulk of literature with the references provided by the authors.

Finally, Figure 5. Fig. 5-a and 5-b extend the results of Fig.4-c and Fig.4-d, correct? You are using the same scenes of the NASA ACDC L2 record and you compare your baseline-CTP with the retrieved-CTP? Could you please elaborate why is the B-band closer to the A-band retrieval when photon penetration in the cloud is allowed? The sentence at P13 L378 ("This indicates, as expected, more photon penetration correction for B-band than A-band") reads a gap filler and sounds like tha authors want to get away with this without further investigation. There is a reason why the B-band is not customarily used for calibration of surface pressure. Some of the co-authors are surely aware of this effect.

Section 4.5 "Retrieval of global observation"

It is not clear if the same filtering (cloud cover = 1, cloud optical thickness $\geq$ 3, surface albedo < 0.25) is applied for the generation of the RGB snapshot of Fig.6-a. Also in view of Fig.6-d, COT: based on the visual inspection of the patterns, the cloud systems are quite different between the two maps, which are in turn also different from the CTP maps. The patterns are, in my opinion, quite different: the Nothern Pacific system is captured neither in the COT (Fig.6-d) nor in the CTP (Figs.6-b,c,e,f), being the B-band overall shallower/fainter than the A-band. This could point to the choice of grounding all filtered NANs (not-a-number) to 1013 mb, making them valid retrievals in the color scale, albeit representing a fake surface pressure. I would then make this point grey or white, in all Figs.6 b to f and leaving Fig.6-a untouched.

It is not clear to my why the authors are using the L2 COT from NASA ASDC and not their own as specified by Eq.(15). If the calculation of COT in this paper differs (or it is the same) from the one in Yang et al.(2013) this must be stated at the beginning of Section 4.4. Otherwise the reader cannot judge in any way the soundness of the sentence in P13-14 L399-402 about the error propagation of COT into CTP.

To conclude, this section lacks some explanation about the patterns we see in the disc. I understand that the Pacific is a favourable geophysical scene to analyze, due to the lack of difficult reflective ground. However, the authors are capturing a wealth of cloud systems: deep convective clouds within the tropical belt, subsidence clouds in the trade wind belts, near-polar clouds at high latitudes, low-level warm cloud decks, even some cirrus clouds may slip through a COT filter of 3 (perhaps). Each of this cloud type can be categorized after its average cloud optical thickness. Please, introduce COT in your error analysis.

And also create difference maps centred on 0 mb with a divergent color palette for Fig.6c-Fig.6f and Fig.6c-NASA_L2_ASDC.

P14 L410 Conclusions.

- There is no Yuekui et al. 2012 in the bibliography. Please check.

- Here the authors need not just to summarize what they have done but also discuss in a compact way the results and highlight limitations of their method and future developments.

*** Minor comments

P1 L15: was -> is

P9 Figure 2: Please, define in the caption how the difference in reflectance is defined.

P10 L299: "sensibility of every variant"? You mean "sensitivity to every variable"?

P10 Figure 3: in the caption please specify that "umu" is cosine of SZA.

P10 L309: "ratio of upward diffuse ... ", missing a word, perhaps radiance or radiation?

P10 L318: please refrain from subjective statements such as "This is easy to understand".

P10 L327: you mean "thick" cloud and not "heavy" cloud?

P11 L338: if the baseline-CTP mothod is adopted, then in-cloud penetration is not "ignorable" but "ignored" instead. "Ignorable" suggests the existence of an option to be chosen, such that the method still enables the calculation of in-cloud penetration but the authors choose otherwise. "Ignored" implies that the method offers no option other than those provided. So, "ignored" is more rigorous and exact.

Section 4.4, Figures 4 and 5: control axis labels. "Pressure" not "Pressue".

P11 L339: "light reached cloud top is assumed". missing "that"

P12 L371: what do you mean here with the word "interaction"?

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