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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Diurnal variation of high-level clouds from the synergy of AIRS and IASI space-borne infrared sounders" *by* Artem G. Feofilov and Claudia J. Stubenrauch

Artem G. Feofilov and Claudia J. Stubenrauch

artem.feofilov@lmd.polytechnique.fr

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Response to Reviewer 1

We thank the Reviewer 1 for his/her positive assessment of the manuscript and for the corrections. Below, we provide point-by-point answers to each of the comments.

We mark the reviewers' comments/questions and the authors comments/responses by "**RC:**" and "**AC:**", respectively.

Where it was appropriate, we modified the text of the manuscript in accordance with the recommendations. We provide a separate file to track the changes.

General comments

RC: The first two paragraphs of the introduction lack a few references.
AC: We have rewritten the introduction.

RC: p. 3, l. 21: CIRS has already been described and Stubenrauch et al., 2017 already cited higher on the same page (l. 1 and 2, respectively), please fix
AC: Perhaps, the reviewer was misled by the same name, but CIRS cloud climatology described in the beginning of the page is not the same as the CIRS cloud property retrieval package discussed in this line and (Stubenrauch et al., 2017) refers to both of them, so the reference should remain.

RC: p. 5, l. 19: "the phase shift... was found": by who? Is this part of your results?
AC: Yes, we parameterized the curves of (Cairns, 1995). We changed the text to “By analysing the plots of (Cairns 1995) using least-square fitting of Eq. 1 we found that the phase shift is equal to -2 h for the low- and mid level clouds and 0 h for high-level clouds”

RC: p. 5, l. 24: the "moving profile" approach is quite smart, did the authors invent it? If so please state it, otherwise provide a reference to previous use.
AC: We did not perform a search in literature for this approach and we cannot state that this is our “invention”. We added a reference to (Goldberg et al., 2013) where a similar approach was used by one of the authors to find the period and phase of the interhemispheric coupling.

RC: p. 7, l. 25: "This justifies using Eq. (1) for the analysis." The experiment described between l. 20 and 25 justifies using Eq. (1) for the analysis over the 24-h harmonic

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function selected by the authors. It does not prove that Eq. (1) is the best function to use for the analysis. For instance, it would be possible to conjure many additional functions which might prove a worse fit than Eq. (1), but it would still not justify Eq. (1) as the best choice for the analysis. I understand the authors explain that searching for a better function is beyond the scope of the paper in the following sentence, and I'm fine with that, but the statement above is still incorrect. Unless I have misunderstood, please revisit the reasoning of this paragraph and make it more robust.

AC: We'd like to refer the Reviewer 1 to the first figure we provided in the answers to the questions of Reviewer 2, which illustrates the aforementioned experiment performed using *real data*, and not for the *function selected by us* (the latter would give a perfect fit with all correlation coefficients equal to 1). As one can see, if the clouds exist (right-hand side of the plot), the "natural noise" related to uncertainties of the ancillary data and cloud retrieval methodology as well as to errors of parameterization given by Eq.1 leads to cases, for which the correlation coefficient is lower than 1. Still, the "semidiurnal fit" which corresponds to Eq. 1 gives higher correlation coefficients than a simple harmonic fit. The smoothness of the "tail" of the histogram and the general considerations regarding noise tell us that any other fitting function will not give a perfect fit, either and we are already close to good fitting.

To avoid the confusion, we rewrote the introductory sentence: "...we have performed the following numerical experiment using real data: one year of cloud data retrieved from AIRS and IASI with the help of CIRS has been processed..."

RC: section 3 : High clouds are identified unambiguously in CATS data by the altitude from which the lidar signal is backscattered to the instrument. This is not the case for the cloud detections documented in the AIRS/IASI dataset. Could you comment on how the uncertainties in cloud altitude in the AIRS/IASI dataset might affect the retrieved diurnal cycle of high clouds in one way or another, and if these effects are consistent with the differences with CATS results?

AC: This is a good question and it requires several references to be answered. In Fig.

2 of (Feofilov and Stubenrauch, 2017) we provide the basics for the cloud pressure error estimate in chi2-retrievals. As one can see, for high clouds, due to larger contrast between the radiation of the cloud and that of clear sky scene, the chi2-curve is steep and the pressure error is small. Correspondingly, the emissivity error is small, too (emissivity curve is estimated in $P \pm \Delta P$ points). In Fig. S3 of (Stubenrauch et al., 2017), the cloud heights retrieved from AIRS are compared with those from CALIOP and they show quite a good agreement and stability over the whole range of cloud emissivity. Finally, in the figures attached to this answer we show the emissivity error and pressure error distributions for high clouds. As one can see, both errors are small and therefore they should not affect high cloud determination accuracy to a significant degree.

RC: section 3, figure 3: the comparison with CATS is interesting, but how do the AIRS/IASI cycles compare with the ISCCP daily cycles described by Rossow and Schiffer 1999? Their results are presented as part of the introduction, why not compare them to the AIRS/IASI results in addition to CATS in Fig. 3?
AC: We followed the advice and added the curves of (Rossow and Schiffer, 1999) to Fig. 3 and modified the text correspondingly. The behavior of all three curves over land is quite consistent whereas the ocean areas with their weak amplitudes show moderate agreement. Please, also see the discussion around Fig.3.

RC: p. 11, l.9: Wyley -> Wylie
AC: Fixed, thanks.

RC: Fig. 6: A more direct legend would be "Same as Fig. 5 for July"
AC: We have changed the legend following the advice.

RC: p. 18, l. 10: "(Fig. 8)" -> Fig. 9? (Fig. 8 shows the land regions)
AC: Actually, we meant Fig. 7, relative humidity, but anyway thanks for pointing out this inconsistency.

RC: p. 19, l.17: Maybe a dumb question, but who wrote the paper?
AC: Originally, the draft of the paper was written by the first author, but then the paper converged to its final form in the course of several iterations, so now it is difficult to assign this or that part of the text to this or that author.

RC: p. 19, l.26: Where were the CATS data shown in Fig. 3 obtained?
AC: We took the data directly from the plots of (Noel et al., 2018) by digitizing them with the help of freely distributed Tracer 2.01 software by Marcus Karolewski (https://sites.google.com/site/kalypsosimulation/Home/data-analysis-software-1). In manual mode, the accuracy of digitizing is one screen pixel that is about 0.1

RC: p. 24, l.30: I tried getting the Wylie and Woolf paper by following the doi link but it does not work. Please fix it.
AC: The official doi copy-pasted from the journal's Web-site looks as follows: https://doi.org/10.1175/1520-0493(2002)130<0171:TDCOUT>2.0.CO;2 and if clicked it opens the page with the article (checked on the 31/08/19). Perhaps, a space or some other symbol spoiling the hyperlink was introduced on the author's or ACPD' side while formatting the file for online publishing. We believe, this will be checked and fixed by the production team when it comes to publication, but anyway, we've updated the link in the text.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-166/acp-2019-166-AC1-supplement.pdf

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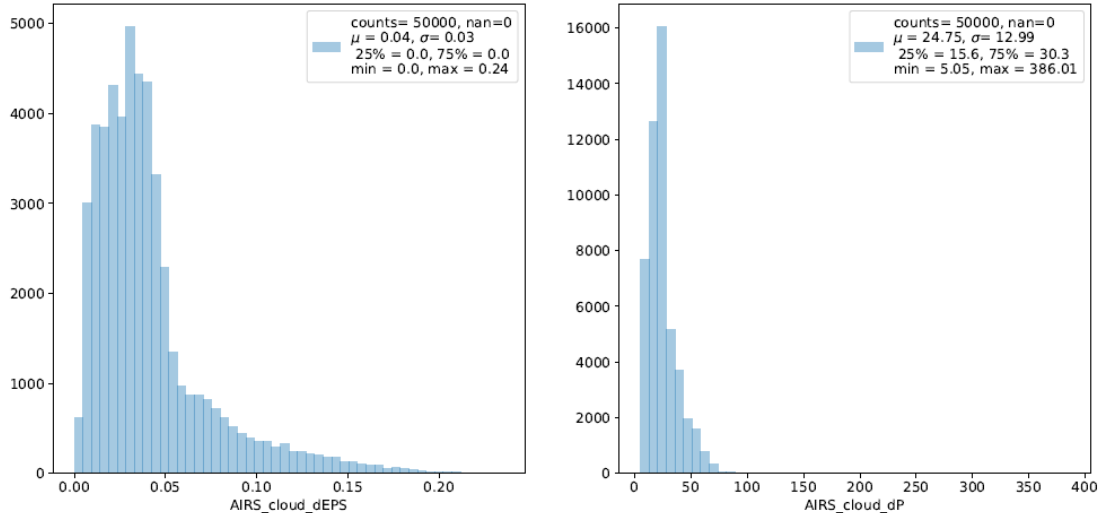

Fig. 1. Left: Emissivity error histogram for high clouds; right: Pressure error histogram for high clouds

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