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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 23 April 2019

In this paper, the authors analyze global-scale cloud detections derived from measurements by the AIRS and IASI instruments, to infer the diurnal cycles of high-level clouds over oceans and several land regions in the tropics.

The paper is well-written and structured. It is easy to follow and makes its arguments convincingly. The methods used to analyse the data appear sound and described with detail. The evidence is well presented and supports the paper's conclusions. The results are useful and relevant to the field. I appreciate the way the authors derived a daily cycle from a couple of measurements made at two local times, and the focus on well-selected land regions. I have no problem recommending the paper for publication,

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Discussion paper

although I have a few very minor comments below.

Minor comments

- The first two paragraphs of the introduction lack a few references.
- 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
- p. 5, l. 19: "the phase shift... was found": by who? Is this part of your results?
- 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
- 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 function selected by the authors*. It does not proves 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.
- 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?

- 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?
- p. 11, l.9: Wyley -> Wylie
- Fig. 6: A more direct legend would be "Same as Fig. 5 for July"
- p. 18, l. 10: "(Fig. 8)" -> Fig. 9? (Fig. 8 shows the land regions)
- p. 19, l.17: Maybe a dumb question, but who wrote the paper?
- p. 19, l.26: Where were the CATS data shown in Fig. 3 obtained?
- 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

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