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Title:

Diurnal variation of high-level clouds from the synergy of AIRS and IASI space-borne infrared sounders

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Overview:

In this article, Feofilov and Stubenrauch present a nice analysis of the diurnal cycle of high clouds as derived from AIRS and IASI space-borne infrared sounders. While I think the description of the analysis technique could be clearer and the article would benefit from more concrete discussion on how the results are similar (or not) to previous studies, my only serious concern is that the article does not have sufficient discussion of uncertainties due to assumptions made in the retrieval approach or uncertainties do to sampling limitations.

While I hesitate to call such a “major revision” (as it may well be possible to fix these shortcomings with relatively minor changes to the text), I nonetheless think it is important that these uncertainties be addressed in a quantitative way (much as the 20% uncertainty in the diurnal amplitude due to random error is established).

Recommendation: Major Revisions

General Comments:

1) Description of fitting technique / section 2.2.

At first reading, I found the material in section 2.2 difficult to follow. In particular, it was not initially clear to me that you were fixing BOTH the ratio  $A_{12}/A_{24}$  and  $\Delta\phi$ . At least that is what I think you are doing.

Question 1A) Is  $\Delta\phi$  for all your high cloud analysis fixed to 0?

Question 1B) Is this value of 0 also based on Cairns 2005? (If yes please make this clear in the text, and otherwise explain how this value is determined).

Assuming the answer to 1A above is yes, I suggest you modify the text (starting somewhere around page 5 line 20) to read:

“With the  $A_{12}/A_{24}$  set to 0.28 and  $\Delta\phi$  set to 0, the diurnal “shape” of Eq. (1) is fixed (see the gray line in Fig. 2a) and the problem reduces to one of determining the amplitude  $A_{24}$  and  $\phi_{24}$ . There are four total measurement (measurements at four times of the day) and the problem is therefore overdetermined. Thus we

determine the best fit amplitude and phase using a “sliding profile approach”, as depicted in Fig 2 and described below.

As regards the description of Figure 2 and the fitting process:

Page 7, line 13-15. I don't follow this description. Please rephrase. Why should a cloud fraction value of 0.2 yield a zero over zero? Does this mean the first red point in Fig 2c (near 1 UTC) is not used in the amplitude calculation?

It is in no sense obvious to me that your sliding profile approach is any better than doing a least squares fit or any other minimization approach. Not that I think that it needs to be better, per se, but if you have a specific rationale for developing this approach rather than using established techniques for over determined problems it would be good to explain such.

\*\*2) Uncertainty in diurnal amplitude and phase due to ASSUME A12/A24 ratio and delta\_phi.

You assess the uncertainty in your results due to the impact of a 20% random uncertainty in the cloud fraction, the result of which is a 20% uncertainty in the amplitude (which is not surprising) and 1.5 hours uncertainty in the phase. This is a GOOD result! But what about the assumed values for A12/A24 and delta\_phi? I can easily imagine that even a small change in either of these assumed values might have a large impact. In particular, I expect these values might have spatially coherent regional variations that might impact your later analysis.

\*\*3) Uncertainty due to sampling variability.

I am very surprised to see that you do not discuss the sampling variability. Is the sampling variability small relative to the diurnal variations? For example, if you plotted the same result using any randomly chosen 5 or 8 years of data (rather than all 10 or 15 years) would all the results be so close together that I couldn't see any differences in any of the plots or values presented in Tables 1 and 2?

4) What is new here ?

Do the results of the diurnal analysis differ from results of prior observational studies or extend our understanding of the diurnal cycle in some way? While reproducibility of results is an important aspect of science and having some “new result” is not strictly necessary, I think the article would be more interesting if it does. In general, more concrete discussion on how the results are similar (or not) to other studies would make the analysis more valuable.

Minor Comments:

Introduction, first paragraph: I know this is well known material, but if you are going to start off a publication with such, you ought to support the material with appropriate references. Frankly, I think you might just drop this first paragraph and start the introduction with the second paragraph "Due to the importance of clouds ... " (line 3, page 2).

Page 2, line 5. Starting phrase is awkward, I suggest change to read simply, "The diurnal variation modulates ...".

Page 2, line 8. Perhaps change to "Many early global observational analyses ... ". I thought Rossow had a few papers looking at diurnal cycles before 1995 (1983 perhaps ???) and I think there were some earlier studies yet (pre ISCCP). My point is there is no need to declare "first", and in general this might depend on how one defines "global".

Page 2. Line 10. Perhaps change "are significant" to "were found to be".

Page 2. Line 20-23. I think these three bullet points are also intended to apply to cloud over land in the tropics – and don't for example apply to mid-latitude in the winter. Perhaps move the phrase "over land in the tropics" from the first bullet into line 18, or otherwise clarify.

Page 4, line 1. What does "transformed" mean here?

Section 2.1. The spatial resolution of AIRS and IASI is coarse compared with most IR imagers. Perhaps it would be good to include some comments on how resolution impacts the cloud classes: high opaque ( $\epsilon_{\text{cld}} > 0.95$ ), cirrus ( $0.95 > \epsilon_{\text{cld}} > 0.5$ ), and thin cirrus ( $0.5 > \epsilon_{\text{cld}} > 0.1$ ).

Page 4, line 1. Also what does the correction to SST entail? If it is truly unimportant to the study results, then perhaps leave this line out. If it does matter, then you should probably explain a bit more.

Page 4, line 5. Perhaps use "pixels" or "fields of view" rather than "scenes". To me "scene" implies a collection of pixels on some scale, for example a 60 km scene. As such a scene might have high-cloud even if only a fraction of the pixels contain clouds.

Page 5, line 6. The word "admixture" seems a bit archaic to me and in general the idea of the diurnal cycle being a mixture of semi-diurnal cycles seems odd. Perhaps change to "... since variations in cloud amounts are known to include variations on both diurnal and semi-diurnal time scales."

Page 7, line 1. What do you mean by "hit rate"? How does this translate into a 20% random noise?

Page 7, line 3. Perhaps change “build the function” to “solve for A23 and phi\_24”. Also as far as I can see, you are only looking at high clouds in this study, so delta\_phi is simply 0.

Page 7, line 4. Perhaps change to “Having solved for phi\_24, ...”

Page 7, line 13-15. I don't follow this description. Please rephrase. Why should a cloud fraction value of 0.2 yield a zero over zero?

Page 8, line 3. Change “... separately of high opaque ...” to “... for high opaque ...”.

Figure 3. What is being "averaged" in Figure 3? Are the data being spatially averaged (if so what was the starting spatial scale)? Or temporally averaged (what time period)? For example do you first calculate the diurnal amplitude and peak time for each 1x1 latitude region (for each cloud type) using all years of data and then average spatially, or using 1 year of data and average in space and time?

Page 8, line 14. Early studies. Please provide specific references.

Page 8, line 20. I presume you mean with the estimate 1.5 hours uncertainty. Perhaps clarify. (As a very minor point I note that 0 h to 22 h is a 2 hour difference, so perhaps just say, they agree to within 2 hours).

\*\* Top of Page 9. What does “In situ Freezing” mean?

\*\* Page 8 / figure 4 Discussion.

A) What about mid-latitudes? Don't they deserve some discussion? In particular, (A1) Over land, why are cirrus and thin cirrus cloud in phase and LEAD opaque cloud? This is very different than tropical/subtropical land. (A2) Over mid-latitude land, diurnal variation of thin cirrus is larger than cirrus while the opposite is true for subtropics and tropics (where cirrus larger than thin cirrus). This is worth a comment, I think. (C3) Over mid-latitude ocean, are the relatively small variations real (larger than uncertainty)? If yes, does this mean that cirrus form more prominently overnight and thin during the day Or might this be a retrieval artifact or ??

B) Subtropics ocean is similar to tropical ocean and subtropical land is similar to tropical land for boreal summer. This doesn't seem too surprising. What about boreal winter? Are subtropics similar to tropics in boreal winter? Similar to mid-latitudes? Or differ from both? (Perhaps put NH winter and SH winter plots in supplementary material).

C) In general, is there anything here that is new or different from previous studies?

Page 11, line 2. 0.85 seems an arbitrary choice. I presume your results are NOT sensitive to this choice, meaning if you choose 0.7 it has little impact? Please comment.

Page 11, line 10. Are the differences with Hong ... Random? Systematic in some regions? Earlier or later?

Page 11, line 22. On stability. I agree that there are many fewer points with a retrieved diurnal cycle in the winter hemisphere but there are some signals that perhaps deserve some comments. In Figure 5a, is the feature in the (A) Atlantic off the US East Coast, (B) the Central Pacific, and (C) that Himalayas real? (Do other climatologies show these features? Any idea what is going on here?). Likewise, I wonder if the diurnal cycle observed of the Southern Ocean is real.

Page 11, line 26. Perhaps change to “In July, there is a large contrast between continental opaque clouds and nearby oceanic regions. Over the continents, the peak typically often occurs in the evening around 20h as compared with oceanic areas near the continents with peaks closer to noon. Nonetheless, in some other ocean locations opaque clouds also peak in the evening or overnight (e.g. tropical longitudes -115 to -135).”

Page 11, line 27. As regards Indonesia, I think the Maritime continent has a complex land/sea breeze interactions that I don't think the 1 degree data can capture well. I suggest it might be better to simply note this (and perhaps cite appropriate references) rather than trying to make any conclusions for this region.

Figure 8. I suggest changing the numbering scheme used in Figure 8 and the discussion to match the order of the regions shown in Figure 9 and tables #1 and #2 from top to bottom. This will make the text easier to follow.

Section 3.3. What guided the selection of regions analyzed here? In particular, why are no oceanic regions selected for analysis? Also, the North America region that is highlighted includes the west coast and Rockies but misses most of the central U.S. and Eastern U.S. Was the intent to avoid Mesoscale Convective Systems (MCSs) that strongly influence the diurnal phase and amplitude in the Central/Southern US? I note that Figure 6a shows the Central and Eastern US is notably different from the western US.

Page 15, line 8. I agree that multiple peaks could be explained by orographic effects, however, I also think that MCSs likely play some role here. In general, what leads you to make this statement? Is the intent that this statement is a conclusion Or are you speculating?

Figure 9. Suggest change column header “Conv.” To “Opq” in keeping with the figure caption, and to be consistent with the point that Opaque is not perfectly synonymous with convection.

Page 17, line 10. As with earlier comment, I do not know what “in situ freezing” means.

Page 18, line 3. I am not clear on what “in the same limits” means here. Please rephrase.

Page 19, line 4-6. You write, “It is interesting to note that the local time of the minimum moves from the summer hemisphere midlatitudes towards the tropics from 6h to noon and the one of the maximum from 17h to 1h.” I am confused, what is moving, the total amount of upper level cloud? What does “one of the maximum” mean? Perhaps expand and/or rephrase this remark.

Page 19, line 10. What other analyses? Suggest references. Is there anything in the present work which adds to OR departs from the “other analysis?”