Atmos. Meas. Tech. Discuss., 6, C3094–C3098, 2013
www.atmos-meas-tech-discuss.net/6/C3094/2013/

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Ground-based all-sky mid-infrared and visible imagery for purposes of characterizing cloud properties" *by* D. I. Klebe et al.

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Received and published: 28 October 2013

This paper describes a new instrument being developed to infer fractional sky cover and other cloud properties, particularly using the atmospheric infrared window spectral region. This affords retrieval of sky cover 24-hours a day, a boon to study of the diurnal cycle of cloudiness. But the paper falls short on concrete evidence that at least some of the other variables the authors attempt to retrieve are really achievable to a useful degree of accuracy. Nevertheless, cloud amount is the "zeroth" order piece of information needed about clouds, so 24-hour fractional sky cover retrievals alone are well worth it. I have several comments that I believe would improve the presentation of this

material.

1) The terms "cloud fraction" and "fractional sky cover" have been previously defined and used in many, many atmospheric science papers. To wit, "cloud fraction" refers to the nadir-projected cloud amount, i.e. if the outlines of the clouds were projected straight down to the ground, what percentage of the total ground would the cloud outlines cover? And "fractional sky cover" (often then shortened to just "sky cover" thereafter) is the angular measure of the amount of the hemispheric view that contains cloud elements. I see no reason to reinvent the latter term for this paper, i.e. coining the phrase "hemispherical cloud fraction" with the associated acronym (HCF). I recommend the authors just use the already well defined and widely used "fractional sky cover" here.

2) Pg 7986, line 23: "HCF, however, has only been directly determined at the ARM sites during daytime hours utilizing the Total Sky Imager (TSI) (Long et al., 2001)." This is not true. The ARM Program originally used the Scripps Whole Sky Imager as their sky cover instrument, since it was the only one available at the start of the Program. These were later phased out in favor of the TSI.

3) Pg 7986, line 25: "Other indirect cloud fraction data products can be derived from surface radiometers..." Some references for these techniques are:

Long, C. N., T. P. Ackerman, K. L. Gaustad, and J. N. S. Cole, (2006): Estimation of fractional sky cover from broadband shortwave radiometer measurements, JGR, 111, D11204, doi:10.1029/2005JD006475.

Durr B. and R. Philipona, (2004): Automatic cloud amount detection by surface longwave downward radiation measurements, JGR, 109, D05201, doi:10.1029/2003JD004182.

4) Pg 7992, line 15: "...is fit to a 2nd order polynomial equation (gold line)..." Yellow (or gold) on white background is not a good combination as it is hard to distinguish. I sug-

gest that all occurrences of yellow on white be modified to use a color more contrasting to white.

5) Pg 7992, line 21: "This image forms the basis of cloud fraction determination." No, to be precise, it forms the basis of fractional sky cover determination, or in the Author's vernacular the "hemispherical cloud fraction." The phrase "cloud fraction" by itself is most associated in the literature with nadir projected cloud fraction, and not the hemispheric sky view.

6) Pg 7993, line 11: "Using the thresholds (in normalized radiance units) of 0.03 < thin cloud < 0.05 and opaque cloud 0.05, the HCF comparison plots derived for 21 July 2009 are shown in Fig. 9." I do not understand this. The TSI visible sky image technique essentially uses color to determine whether a pixel is clear sky (blue) or opaque cloud (white or gray). Then somewhere between blue and white/gray, a limit is set as to how "whitish" a blue will be called a "cloud", and then how "whitish" again before it is classified as opaque cloud. But how that same concept translates to emitted radiance I do not see. It seems to me that an optically thick (in the IR) cloud that is high and colder would produce the same radiance amounts as lower, warmer cloud that is optically thin enough to not be saturated in the IR window (thus the observed radiance is a combination of colder sky background and optically thin emission from the warm cloud). How do the authors distinguish between the two to know that the "cloud" element in question is one or the other, and thus equivalent or not with the TSI "thin" cloud classification?

7) Figures 7, 8, 10, 12, 15, 16, 17: it would be very helpful to include a "normal" ASIVA Visible or TSI visible sky image so readers can understandably see what sky these various massaged retrievals are from.

8) Pg 7993, line 28: "It may be that these thin clouds are indeed present but that the TSI is insensitive to thin clouds in highly overcast conditions." Not likely, given the explanation of how the TSI thin and opaque classifications are determined. This sounds

to me like what I was referring to above. Here the emission is related to the radiating temperature of the cloud, as well as its optical thickness. Given these cirrus clouds are also cold clouds, it makes perfect sense that the radiance limit threshold needs to be lowered in order to classify them as "thin cloud" or even cloud at all. The TSI "thin" and "opaque" classifications are directly related to the visible optical depth. But the IR radiance being used here includes both the IR optical depth and the cloud radiating temperature, as noted above. The two scenarios are just not equivalent. Either the ASIVA developers need to include some other factors to deconvolve the two influences on the radiances, or I'd say just classify the IR image pixels as "cloudy or not" and leave it at that, i.e. no opaque and thin subclasses. Those can be incorporated in the ASIVA Visible image processing during daylight hours just like the TSI processing, but I do not see how this simple technique being described can work for the IR radiances.

9) Pg 7995, line 6: "Development of ASIVA's HCF data product is nearly complete and is currently being applied to the entire field campaign dataset." It would be good to have some statistics from a comparison using this longer data set. Frankly, while the current paper presenting just two examples is illustrative, it cannot say much about the veracity of the ASIVA retrievals overall. Especially given the admitted and noted need for "tweaking" between the two example days with different sky conditions in order to get good agreement with the comparison TSI retrievals. The paper would be much stronger if it included comparison results from a larger and statistically significant number of comparison data pairings.

10) Pg 7998, line 10: "This is presumably due to the clouds being much cooler than where the lapse rate would place them. In addition, the ASIVA instrument measures the mean temperature of the cloud to one optical depth. This will always be located at a higher altitude then the cloud base." This sounds like hand waving, and not very good waving at that. The ceilometer is a tried and tested instrument of known accuracy and dependability. So if the ceilometer puts the cloud bases at 500 - 1000m on 7/21, then I'd be willing to bet that's very close to where they were. From 14-16 GMT the ASIVA

retrievals vary from 2000 - 4000m with a very few excursions down to close to zero. To say that these clouds were "much cooler than where the lapse rate would put them" is saying that the clouds were colder than the air they were imbedded in at 500-1000m? Given a standard lapse rate of 10 degree C per km, that would mean the clouds were 10-20 degrees C colder than the air at 500-1000m? The ARM Program puts out an interpolated soundings product called the Merged Sonde VAP. I'd challenge the authors to use that and show us the temperature profile as it evolved this day to prove their claim here. And while the authors are correct that the "radiating temperature" is effectively from an imaginary "radiating surface" about 1 optical depth into the cloud, the analysis presented prior (such as the opaque cloud amounts in Figure 9 for this time period of 14-16 GMT) strongly suggest that these were not optically thin clouds. And generally low clouds (500-1000m bases) that are classified as "opaque" clouds by the TSI are optically thick such that the 1 optical level is certainly not 2 km in from the cloud base. Simply, there's a lot more work to be done to get to an accurate cloud base height using IR radiances. It has been tried before to use some determination of cloud radiating temperature (whether brightness temperature or color temperature) and corresponding sonde temperature profiles, with very limited success. I suggest the authors drop this section from the paper until such time as they can show useful results and not speculate about the failures with arguments that make no physical sense.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 7985, 2013.