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Atmospheric Measurement Techniques Discussions

Interactive comment on "An automated cloud detection method based on green channel of total sky visible images" *by* J. Yang et al.

Anonymous Referee #2

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Recommendation: needs major revision before acceptable for publication

General Comments:

The paper described a methodology for inferring fractional sky cover from color hemispheric sky images. The methodology described is work further developing a method presented previously in Yang et al., 2012. The subject of the paper is appropriate for the scope of AMT, and the methodology described seems to afford a useful improvement in sky cover retrieval methodology.

One major weakness of this paper is that the sky images used are significantly tinted blue. This has a significant impact on several of the previous methodologies for inferring fractional sky cover from color sky images that are discussed in a less than compli-

mentary way in this paper. As presented in the paper, a ratio of the red over blue pixel values or similar means has been widely used to process color sky images for cloud amount. But if the entire sky image is blue tinted, clouds included as in this paper, then obviously that negatively impacts using an R/B ratio methodology. To honestly compare the author's methodology to previous methodologies, using a better, more true-color sky camera instead of only these blue-tinted sky images is required. And this study uses only a few sky images, comprised of what appear to be unambiguous cumulus clouds. A more robust analysis needs to include the more problematic cloud types such as thin cirrus and extreme optically thick cumulonimbus (which often are darker than the clear-sky pixel brightness values). This study is flawed in that respect, and better, truer color images of all cloudiness situations are readily available from the US NOAA SURFRAD and US DOE ARM Program archives and web sites. The study needs to include a variety of these more realistic color rendered sky images before the claims of superiority in their technique are justified and demonstrated. Far too many overstated comments about the superiority of the methodology presented here are made that are not justified by the very limited scope, sky conditions, and comparisons presented in the study.

The paper lacks details and descriptions sufficient such that a reader could repeat the work. I have noted a few particular examples in the specific comments below. However, I suggest that the authors also have someone who is not at all familiar with sky imagery, camera technology, cloud amount retrieval methodologies, etc. read the paper and point out where more explanation and/or description is needed.

The use of the English language needs improvement. The phraseology in a significant number of sentences makes it difficult to understand what the authors mean. Perhaps the authors might consider using a professional proof reading and editing service once they have revised the paper.

I have read through the entire text and no where do I find any sort of definition of what the authors mean by a "cloud" as opposed to "clear sky" here. Their Figure 9 purports

to compare theirs with 3 other methodologies, but without defining what they consider “truth” to be. I would recommend the authors at least give some sort of idea what they consider to be a cloud defined in some way such as color level or some such. If the sky images they were using weren’t so blue tinted, they might use “blueness’ as an indication of where they delimit between clear sky and cloud. But give SOME effort at defining what they are attempting to classify here. The authors might want to consult the literature, such as Dupont et al. (2008), where others have defined their clear/cloudy limit in terms of optical depth and the like.

Dupont J.C., M. Haeffelin, and C.N. Long (2008): Evaluation of cloudless-sky periods detected by shortwave and longwave algorithms using lidar measurements, GRL 35(10), doi:10.1029/2008GL033658.

Specific Comments:

1) Abstract: "...GBSAT algorithm is robust for all types of test total sky images and has more complete and accurate retrievals of visual effects than those found through traditional methods." But you only use your blue tinted images in this study, so essentially there is no evidence that this technique would be "robust" or has more accurate retrievals for more normally tinted sky images.

2) Introduction, line 22: "...provide high temporal resolution measurements for clouds over the horizon." I do not understand how a sky image can detect or "see" clouds that are over (below) the horizon? I do not think this is what is meant here, so this needs rewording to be clearer.

3) Page 4583, line 20: “Long et al. (2006) detected clouds in WSC images using 0.6 as a single FT in R/B space.”

While the above is true, the authors neglect to also mention the more sophisticated limit function also described that the HSI and TSI sky imagers employ, thus making it seem that only the simple single limit value is presented in the entire paper.

From Long et al. (2006): “But because the TSI is a commercial instrument intended to be deployed at many locations, a generic baseline clear-sky function has been established using the pixel distance from center, distance from the sun, and solar zenith angle (SZA) as independent variables. The user then uses configurable settings to set the clear/thin and thin/opaque limits as desired, to a first approximation as a percentage offset from the baseline value for that pixel.

Reference: Long CN, JM Sabburg, J Calbo, and D Pages. 2006. “Retrieving Cloud Characteristics from Ground-based Daytime Color All-sky Images.” Journal of Atmospheric and Oceanic Technology 23(5): 633-652.

Not including some mention of the HSI/TSI’s more sophisticated clear envelope formulation unduly downplays that there have indeed been more sophisticated means to shape sky image clear/cloudy sky limits than just a single value for the entire sky image.

4) Page 4585, line 25: “. . .and normalized R/B results. . .” How was the R/B ratio normalized? There is no explanation in the text how this was done.

5) Page 4586, lines 8-11: The authors here are essentially saying that using the separate RGB color level values is better than using any ratio or differencing as plotted in the lower right hand plots of Figures 2 and 3. However, the sky images the authors are using to demonstrate and champion their assertion are not usual or ‘natural color” sky images, but rather have a distinct blue tint to the sky images they are using. This blue tint is obvious in the sample images because one only has to walk outside and look at the sky to see that by far most clouds are white or gray, not blue as depicted in these sample images. Sky images that are a far more realistic color rendering can be seen in many of the papers that the authors cite in their own introduction e.g. look at the Long et al. (2006) paper above, Figures 2 and 4. And actual sky images are readily available from web sites and archives such as for the US NOAA SURFRAD network or the US DOE ARM Program, all of which have much more natural color rendering.

The simple fact is that by using such extremely blue tinted sky images; the ratio of red over blue pixel color value is highly skewed. Were these sample images more realistically colored, the R/B ratios depicted in their Figures 2 and 3 would show much greater differences between the cloud and cloud-free portions of the sky view, as shown in the Long et al. (2006) paper.

It is no wonder that these authors need to develop some other technique to better infer sky cover from these blue tinted images. I'd recommend that they get a better sky imager that does not have such a significant blue tint to the images. And then present an analysis similar to Figures 2 and 3 with more realistically colored sky images.

6) Page 4586, line 19: "Given a reasonable threshold, it can easily distinguish between clear and overcast." The authors are ignoring the optically thick overcast case, such as cumulonimbus over the surface site, where the overcast "brightness" can be less than that for pristine, low aerosol, high altitude clear sky. Not so easy then to distinguish between the two using ONLY brightness criteria, but some sort other criteria must also be used such as a color criteria.

7) Page 4589, lines 7-13: The description of the non-uniformity of the clear-sky sky image in this paragraph is essentially paraphrased from Long et al., 2006. The reference should be acknowledged.

8) Page 4589: The authors might be very familiar with the "BSAT method" and what they mean by "morphology opening operation" but I sure have no idea what the authors are talking about here. So in order to understand their nuanced different approach here, can the authors please give at least a brief explanation of just HOW the initial "BG" is produced? And then how are the values for the alpha, beta, and gamma coefficients determined? Magic? Ideally, a peer reviewed scientific paper should contain enough information such that the reader can repeat the work. The lack of useful information and description here precludes that. More information and description is needed!

9) Page 4591, "Results Comparison" section: I cannot speak for the R-B and BSAT

methodologies, but I can say for certain that the authors use of the R/B methodology here is significantly flawed, thus extremely biasing this comparison in favor of their own method. As mentioned before, one grave problem here is their use of blue tinted images, which negatively impacts the use of the R/B methodology through no fault of the methodology itself. Then, despite their use of blue tinted sky images, they still use the most primitive 0.6 single limit value in the Long et al. (2006) paper. And finally, despite their acknowledgement and much discussion about the negative effects of the sun in the image and circumsolar brightness, they apply the technique despite the Long et al., (2006) paper's explicit description of blocking the direct sun from the camera to mitigate the adverse effects to the retrievals of the "bleed" and CCD/CMOS overload by the direct sun in all the camera systems included in the Long et al. (2006) paper. Then the authors make comments about the failures of the R/B method in this comparison and how well their method does instead.

These results need to be redone properly. The ARM and SURFRAD programs have plenty of available sky images and the corresponding retrievals (including cloud decision images similar to those presented in this paper) using the Long et al. (2006) clear-sky limit methodology that accounts for the near-sun and near-horizon brightness better than a flat "0.6" limit setting. And the sky images themselves have a much more realistic color rendering of the actual sky than these blue-tinted images included here. The authors can then easily apply their technique to those more natural color sky images, and compare to actual retrievals using the variable clear-sky limit function of the TSI processing. That would be a much more honest and useful comparison than the seemingly biased work presented here.

I have no doubt that the Author's methodology when applied to more true-color sky images affords some improvement. And a proper comparison with other methodologies I believe would show that. But this particular comparison presented here is biased and thus flawed.

Page 4592, lines15-16: "However, the imaging theory of cameras shows that the color

images we acquire from cameras are not really true color images. . .” And “human examination” of the blue-tinted sky images used in this study certainly are extreme examples of that!

Page 4592, lines 18-21: “. . .and the use of 2-D red-to-blue bands methods will amplify this noise, resulting in significant detection errors, especially in the transition regions of the sky and clouds. In actuality, the original RGB channels are more suitable for cloud detection.” I cannot agree with these comments, especially given the flawed use of the R/B methodology in this study. Similar methods for determining an adaptive background clear/cloudy limit very well are possible using permutations of color ratios, including using the green pixel values along with the red and blue pixel values. Such a blanket “using the channel values has been shown here to be unequivocally better than any possible use of ratios” type of claim is unjustified. The authors have come up with a useful, and likely improved, methodology but it is has not been shown that no other possible methodology is better. One distinct disadvantage of using the author’s methodology is that they do not (and I’d wonder how they could) distinguish between optically thick and optically thin cloudiness. This is a distinct advantage of (properly) using an R/B ratio methodology, and is a significantly useful distinction when using sky cover in reference to cloud radiative effects.

Page 4593, lines 3-4: “The experimental results show that the GBSAT algorithm is robust for all types of cloudy images and obtain satisfactory visual effects.” This statement has not been shown by this study. This study used a few images taken by only one make and model of sky imager, and the images are significantly blue tinted. How well the methodology would work compared to others using other more natural color-rendered sky images, and in climates ranging from polar to moist tropical, has yet to be demonstrated before such a claim is justified.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 4581, 2015.