Atmos. Meas. Tech. Discuss., 8, C2376–C2380, 2015
www.atmos-meas-tech-discuss.net/8/C2376/2015/

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 #1

Received and published: 3 August 2015

General comments:

This paper describes a new technique to analyze all-sky images taken by ground-based sky cameras. The new technique is an updated version of a previous method presented by the same authors (Yang et al. 2012), the use of the green channel only being the main difference.

The scientific question addressed in this paper is within the scope of AMT. This paper presents and develops a quite new idea, which is the use of the green channel in all sky color images to distinguish between cloudy and cloudless areas in the image. Conclusions are substantial enough, in the sense that the method presented may be useful for image processing. In general, the method presentation is clear and complete,

but it could be improved at some points (see my comments below). Although the method is applied to a limited number of sample images, it seems to me that results are sufficient to support conclusions. The authors reference most of previous papers published about the subject. However, the description of previous works that use the green channel, if any, is too superficial and should be more detailed to be able to clearly distinguish which is the original contribution of the present work. The title correctly reflects the content of the paper, and the abstract is also a good summary. The paper is well written and structured. My overall assessment is that this paper is acceptable to be published in AMT, provided that the authors address adequately some issues.

Specific comments

1) The authors provide a comprehensive review of sky cameras and algorithms for analyzing sky images in the second and third paragraph of the Introduction. This overview is good. However, some more attention should be given to previously published methods that use all channels (that is, not only the red and blue channels). For example, the work by Kazantzidis et al (2012), which is referenced elsewhere in the manuscript, is not even mentioned in the introduction.

2) Similarly, the last paragraph of section 2 should be improved. The authors affirm that the original RGB channels are more suitable for cloud detection. It seems like the methods presented so far do not use the original channels. But in fact, all methods previously presented use these original channels: more specifically, a combination of them. Moreover, with this sentence the authors seem to intend to finish with more than ten years of research that focused on the use of the combination of red and blue signals for analyzing sky images. I think that more justification or discussion is needed here. While the selection of the green channel (combined with the methodology presented by the authors) might be a good option, this does not mean that the use of the other channels must be forgotten. In fact, it seems to me that the use of the other channels (according to previously published methods) is still useful to distinguish completely cloudless and completely overcast skies. So the authors should insist on the idea

that the new method is particularly useful for partly cloudy (broken, scattered) skies. This restriction of the study should be clearly stated both in the abstract and in the conclusions section.

3) A very important part of the method is the cloud detection process, which is performed by a “morphology opening operation”. This terminology is probably usual in the context of digital image processing, but not in the atmospheric literature. So the authors should provide some a short summary about this technique, or at least some references. I understand that the technique is presented in their previous publication (Yang et al 2012) with the name of BSAT, but this reference (and some other original paper/book about the morphology opening operation) should be mentioned again in section 3.3.

Technical corrections:

- Abstract

l. 5 Add “here” or “in this study” in the sentence “. . .has been selected to replace. . .” l. 14 “visual effects”?

- Introduction

First sentence. Clouds may influence weather phenomena, but the main relationship is the other way around (weather phenomena cause different cloud types). Some satellites present spatial and temporal resolution which is not “low”.

Second paragraph. Not all sky cameras are based on a fish-eye lense. The TSI device is built upon a parabolic mirror.

p. 4584, l. 3 “Souza-Echer”

- Imaging principle and band selection

p. 4585, l. 25. What do you mean with “normalized R/B”?

Please recall that you only present two examples of images. While I do not criticize this, you should clearly state that this is a limitation of your conclusion here.

p. 4586, l. 3. I would say that the brighter pixels are in positions 300-600, not 200-500.

- Cloud detection algorithm for total sky images

p. 4586, l. 15 Histograms of what variable?

l.21 You say that this paper focuses only on “cloudy pixels”. I understand that you mean on “[partly] cloudy images”, don’t you?

p. 4587, l. 8 “The position of the sun in the sky is relevant with. . .”, do you mean that the position of the sun "depends on. . ."?

p. 4588, eq. (3). S and L are the area and perimeter of what? Of CSP or HBP pixels?

Section 3.2. Give some example (typical) values of circularity for occluded and visible sun. Give also some value for the threshold used to define HBP.

Section 3.3. I have already commented on the need to shortly describe the morphology opening operation.

- Results

p. 4592, l. 9-12. Indeed the proposed GBSAT method seems to give better results than the previous ones. Among the latter, BSAT also outperforms methods based on R to B ratio or difference. However, you should discuss the effect of using the suggestions given by Long (2010) regarding the circumsolar and horizon areas in the R/B method. In addition, you should make it very clear that your results come after the analysis of only a limited number (5) of images. Moreover, these images are not so different, unlike you mention in p. 4591, l. 14. In fact, all these images present cumulus clouds of different sizes. So, there are not cirrus, altocumulus, or other type of clouds.

- Figures

Figures 2 and 3, bottom row, right panel. You say you show R/B, R-B and (R-B)/(R+B). However, these ratios and differences should be less than 1 and less than 0 respectively (according with the values shown for each channel in the left panel). So I don't understand the vertical axis scale (going from 0 to 250) and the label "digital number"

Figure 4. If I understand correctly the method, when you adjust the simulated background, you use information about whether the sun is blocked by clouds or not. This should come from the solar mask that you have previously determined. So an arrow should connect the "solar mask" rectangle with the "morphology opening" rectangle. In addition, the use of an adaptative threshold approach for the cloud mask should be made apparent in the flow chart.

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