Reply to comments by J. Calbó (Referee #2)

on the manuscript "Cloud fraction determined by thermal infrared and visible all-sky cameras" by Aebi et al., submitted to Atmospheric Measurement Techniques.

We would like to thank the referee for the constructive comments that contributed to the improvement of the manuscript. Detailed answers to the comments are given below (bold: referee comment, regular font: author's response, italic: changes in the manuscript).

This paper introduces a new sky camera, specifically an infrared camera which can take sky images both in daylight and nighttime conditions. The paper explains the algorithm that is applied to derive cloud cover from the images of this camera. Moreover, a thorough validation-comparison effort is performed between cloud cover derived from these images, from images of other two (visible, that is, only during daylight hours) commercial sky cameras, and from the APCADA algorithm (based on cloud effect on downward longwave radiation measured with a pyrgeometer).
In some way this paper is a follow-on of a previous paper by the same research group (Aebi et al, 2017, AMT) where they presented an analysis of a long series of diurnal cloud cover obtained with a sky camera. The present paper, however, has several added values: the introduction of a new concept of an infrared sky camera (looking downwards to a convex mirror), the suggestion of the method for image processing, and the comparison with other estimations of cloud cover. Therefore, the paper is worth of being published in AMT. It seems to me that a few changes could be considered to make it more complete and to get higher impact in the scientific community, but even in the present version, the paper may be good enough to merit publication.

Suggested general change:

1. **In order to make more significant the comparison among all estimations of cloud cover, authors could consider applying exactly the same horizon mask to all images. For example, they could use a mask for the part of the image that is below 70 deg. SZA (20 deg. over the "flat" horizon). In fact, even APCADA algorithm is unsensitive to clouds that are in the horizon, so using this mask for all images would make the comparison more homogeneous.**

Thanks for this comment. The authors are aware that the comparison of the cameras and APCADA are problematic when different horizon masks and different field of views are considered. However, the focus of the paper is mainly to present a new camera system (IRCCAM) and to show the possibilities of this new camera system. Thus we decided to not decrease the field of view of the IRCCAM due to the fact that one of the camera software is not able to detect clouds below 70°.

Minor changes and technical details to be corrected:

2. **The word "significant" is used several times in the manuscript. I have my doubts about this use, as no statistical tests are applied (at least, they are not mentioned). So I would suggest use "significant" with caution, as it has a meaning related to statistical tests. If possible, try to use another word. In page 11, line 25, it is said that a difference of 0.02 is statistically not significant, but with no reference to what statistical test is applied.**

We exchanged the word significant throughout the manuscript.

3. In lines 13-24 tow different approaches for cloudiness estimation are summarized. But in my opinion they are not clearly differentiated. Calbó et al 2001 suggests a method based on pyranometer measurements (i.e., hemispheric measurement of solar irrandiance), which is very different to Nephelo or Nubiscope, which are measuring in the infrared and in a narrow field of view. Please consider slightly modifying the writing of this paragraph.

We changed the writing of this paragraph (p. 2, l. 20ff.):

.... Depending on the wavelength range, the presence of clouds alters the radiation measured at ground level (e.g. Calbo et al., 2001; Mateos Villàn et al., 2010). Calbo et al. (2001) and Dürr and Philipona (2004) both present different methodologies to determine cloud conditions from broadband radiometers. Other groups describe methodologies using instruments with a smaller spectral range. Such instruments are for example the infrared pyrometer CIR-7 (Nephelo) (Tapakis and Charalambides, 2013) or Nubiscope (Boers et al., 2010; Feister et al., 2010; Brede et al., 2017), ...

4. I wouldn’t say that WSI is among the most common all-sky cameras (as it is indeed the TSI). The WSI is one of the pioneering cameras, and presents very interesting characteristics and developments, but, to my knowledge, is no usually commercialized and therefore, is not quite common.

We changed the sentences discussing the TSI and the WSI (p. 3, l. 8f.):

The most common all-sky camera is the commercially available total sky imager (TSI) (Long et al., 2006). Another pioneering hemispherical cloud detection instrument is the whole sky imager (WSI) (Shields et al., 2013).

5. Eq (3). Could you explain why the zenith angle is divided by 65?

Equation 3 is a normalized function to fit the sky brightness temperature. Since we are taking the sky brightness temperature at 65° (T_{65}), we also divide by 65. Smith and Toumi, 2008 present the example to normalize at 90°, but in our case 90° is not representing the sky, but the mountains.

6. It is interesting to note that different thresholds are used in the processing of the visible images. This could partly explain some of the difference found in this paper. In fact, selection of the threshold is critical to distinguish between a cloudy pixel, and a clear (but sometimes, containing aerosol) pixel. Some discussion on this matter may be found in Calbó et al 2017 (and other studies cited therein). [Calbó, J., C. N. Long, J. González, J. Augustine, and A. Mccomiskey, 2017: The thin border between cloud and aerosol: Sensitivity of several ground based observation techniques. Atmos. Res., 196, 248–260, doi:10.1016/j.atmosres.2017.06.010.]

We added the reference in the conclusion (p. 14, l. 10ff.):

Differences in the cloud fraction estimates can be due to different thresholds for the camera systems (as discussed in Calbo et al. (2017)) as well as some other issues addressed throughout the current study.

7. The authors recognize that IRCCAM fails at both extremes of the cloud cover distribution. This (unexpected) result should merit more attention, with a deeper discussion if possible.

We added a short discussion about a possible reason for this distribution (p. 10, l. 4ff.):

It is noteworthy that the IRCCAM clearly underestimates the occurrence of 0 oktas in comparison to the cameras measuring in the visible spectrum (by up to 13 %). On the other hand, the relative frequency of the IRCCAM of 1 okta is clearly larger (by up to 10 %) compared to the visible cameras. This can be explained by higher brightness temperatures measured in the vicinity of the horizon above Davos. These higher measured brightness temperatures are falsely determined as cloudy pixels (up to 0.16 cloud fraction). Since these situations with larger brightness temperatures occur quite frequently, the IRCCAM algorithm detects more often cloud coverages of 1 okta instead of 0 okta.

8. The first lines of section 3.2.3 (lines 21-29) do not address seasonal analyses, so I suggest moving them to another section.

We moved these lines to section 3.2.

9. Somewhere in the Results or Conclusion sections, I would appreciate a short discussion of the present results in comparison with performance of other IR whole sky cameras (if you can find any) or other sky cameras that take night images. If no previous work can be found with an estimation of the performance of such night images, this should be highlighted in the paper. Suggested references: [Shields, J. E., M. E. Karr, R. W. Johnson, and A. R. Burden, 2013: Day/night whole sky imagers for 24-h cloud and sky assessment: history and overview. Appl. Opt., 52, 1605–1616, doi:10.1364/AO.52.001605; Gacal, G.F.B. Antioquia, C., and N. Lagrosas, 2016: Ground-based detection of nighttime clouds using a digital camera. Appl. Opt., 55, 6040–6045, doi:10.1364/AO.55.006040.]

The authors could not find any comparisons to other IR whole sky camera systems.

10. Figure 4, caption and related text. There is some mistake in the definition of oktas from cloud fraction. According with the caption, 0 oktas is for cloud fraction between 0 and 0.05 (which looks correct to me) but 8 oktas is between 0.875 and 1.0, that is a much larger interval, which seems wrong (at least, it is not symmetrical). And, for example, 4 oktas should be 0.4375-0.5625 (that is, a bin centered in 0.5 with a width of 0.125). If you correct this, some differences among the methods may change.

Thanks for this comment. The description of the okta ranges in the caption of Figure 5 (before Figure 4) was indeed wrong and we corrected it. However, the analysis was done with the correct ranges.

11. Table 1 and table 2 could be put together in a single matrix-like table (like the authors do in Table 5). In each cell (only in one triangle of the matrix) both the median and the percentiles may be written (for example, as 0.01 [-0.24,0.21].

Since Table 1 and Table 2 are placed in the corresponding sections where they are discussed, we decided to not merge them.