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Atmospheric Measurement Techniques Discussions

Interactive comment on "HelioFTH: combining cloud index principles and aggregated rating for cloud masking using infrared observations from geostationary satellites" *by* B. Dürr et al.

Anonymous Referee #2

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1 General comments and recommendation

This paper describes a cloud masking algorithm for geostationary imagers, named HelioFTH. It is mostly a porting of a previous algorithm to new platforms. The methodology also allows an estimate of cloud-top pressure. It is applied to a month of MVIRI data, and compared with CM SAF SEVIRI and ISCCP data products, as well as some ground-based data.

The algorithm is fairly straightforward, and appears to perform adequately. The

manuscript is also reasonably straightforward. I see no fatal flaws in it, and the topic is suitable for AMT. However some revisions are needed; textually, it is awkward in places because of heavy use of acronyms which are either not defined at first use, or not explained. The discussion is also quite limited in terms of physical reasons and meanings for some of the observed differences between the datasets. The algorithm is not hugely innovative or theoretically advanced, so the paper would benefit from more discussion about the motivation for its development, otherwise it is not clear why the journal readership should be interested in it. For these reasons I recommend minor revisions and then acceptance for AMT if these general comments, and the specific comments and corrections listed below, are addressed adequately.

2 Specific comments

P 1861, L 5-6: I believe there was a recent/will be soon a GEWEX cloud assessment in BAMS, too.

P1862, first paragraph: You state that using counts has an advantage because you can create a self-calibrating LCI scheme across multiple platforms. But surely this is also possible with radiance or brightness temperature, too? I would have thought radiance/BT would be preferable as these would be more consistent between different sensors (for notionally similar spectral response functions) than counts (which are pretty much sensor-dependent); basically, radiance is a physical quantity while counts are not. Am I missing something here? Why are counts better than radiance? Note that I am not saying that counts should not be used for this, I just don't see why they are better than using radiance. After all, counts-to-radiance is a simple coordinate transform. This should be explained, and if the explanation is not satisfactory, I suggest

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removing the statement that processing raw counts is an advantage.

P1864, section 2.1.2 and 2.1.3. You should talk a little more about these datasets, since you are using them for intercomparison. A brief discussion of the data and any known issues with them (e.g. for ISCCP I know there are issues in some aspects) would be welcome here.

P1873, L115: Do you have a link to this report? The more common reference I have seen for the US standard atmospheres is McClatchey, R. A., R. W. Fenn, J. E. A. Selby, F. E. Volz, and J. S. Garing (1971), Optical properties of the atmosphere (revised), Tech. Rep. AFCRLTR-71-0279, 354 pp., Air Force Cambridge Res. Lab., Cambridge, Mass; is this the same set of atmospheres, or a different one? As it stands, the reference given on P1873 is very clumsy.

P1874, Equation 26/L4: I suggest changing the name of category 2 ('cloud-contaminated'). This term is used in many fields to refer to either cloudy pixels, or else pixels which are cloudy but mislabelled as clear. So this could be semantically confusing to readers. I recommend the term 'partially cloudy' or 'uncertain cloudiness' or similar, to remove ambiguity, as I think this is what you are referring to (from e.g. Equation 27 and text). Also, references later in the text to 'cloud-contaminated' should similarly be modified.

Section 4: This is basically fine, but there should be some discussion of the differences in the context of known limitations of the ISCCP/CM SAF datasets, as well as from potential limitations of this algorithm. There is not much exploration of the meaning of/reasons for the results in this section, rather just a straight comparison.

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Section 1 or section 5: What I felt was missing from this study was more about motivations and future plans. Presumably you are doing this because the existing datasets were not satisfactory. I think you should talk about how HelioFTH will be used, what you would do about identified problem areas (mentioned as desrt/snow/open sea), and possible implications for the ISCCP/CM SAF products.

Table 6-7: Why not add a map of the whole disk, showing the various regions you use in this table, and maybe overplot the sites from Table 2?

Figures 1-3: Can you indicate in some way (error bars or shading?) how variable the data are? You show mean diurnal cycles, but how constant are they from day to day? Do the satellite datasets dampen or exaggerate the day-to-day variability?

Figures 4-5: These are difficult to read because the colours are strong and the continental boundaries are thin and weak. Perhaps the continental outlines could be thickened (no need for the individual country border, though). Also the colours are not very intuitive, I'd consider changing them.

Figures 2-5: The text on these figures is very small. This is ok when the figures are in large AMTD format, but I think these will become unreadable in AMT format when printed out or viewed at standard size on-screen. The font should be increased several points in all of these figures.

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3 Technical corrections

P 1861, L7: Acronyms ISCCP and DX should be defined at first use. The Abstract does not count, they should be defined in body text too.
P 1861, L18: Acronym SPARC should be defined at first use.
P 1861, L21: Acronyms MVIRI, SEVIRI should be defined at first use.
P 1863, L1: Acronym CF SAF should be defined at first use.
P 1864, L13: Acronym ASRB should be defined at first use.
P1864, L18: Acronym BSRN should be defined at first use.
P 1864, L19: Acronyms SIS, SDL should be defined at first use.
P1864, L20: Acronym APCADA should be defined at first use.
P1865, L8: Subscript AC is given in lower case here but in upper case in Equation 3 and the preceding text. Please be consistent. Also, these subscripts A and AC are not explicitly given a meaning; these should be stated.
P1865, L17: Acronym WMO should be defined at first use.
Table 1: This is not referenced in the text, only in the appendix. Therefore I think this should be Table A1, and Tables 2-7 should become Tables 1-6 in the revised manuscript.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 1859, 2013.