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Atmospheric Measurement Techniques Discussions



# *Interactive comment on* "Clouds over Hyytiälä, Finland: an algorithm to classify clouds based on solar radiation and cloud base height measurements" *by* Ilona Ylivinkka et al.

**Anonymous Referee #2**

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This paper revisits the relatively old topic of guessing cloud characteristics from solar radiation measurements. Specifically in this case, the main novelty is the simultaneous use of ceilometer (cloud base height) data so in principle a better estimation of cloud type can be made. The paper is in general correct, but with some effort it could be quite significantly improved.

1. In my opinion, mixing the presentation and validation of the algorithm with "climatic" style (but for only 3 years) analysis of observations is somewhat confusing. So section 3.1 and then 3.3 and so, are kind of distracting the attention. I would focus on the new algorithm, so after sections 1 and 2 I would jump to current section 3.2. Then, you



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could add a whole new section regarding results of applying the “occurrence” criteria and the new algorithm.

2. To my understanding, the fundaments used to determine occurrence (lines 177-78, “the ratio between the measured global radiation and modeled radiation at the top of the atmosphere (I)”) is almost the same as the “brightness parameter” (lines 340-341, “relation between the measured global radiation and the radiation at the top of the atmosphere”). So, I would suggest defining this once, and then using for occurrence (setting a threshold) and after that using it also to further explore cloud type characteristics. Moreover, brightness parameter is usually known as “clearness index” in this context of cloud determination from solar radiation measurements. This would simplify the whole manuscript.

3. The dataset used to develop (and test) the algorithm is quite limited. Actually, the authors already recognize this (lines 312-13 “This may be caused by the fact that the used total sky images were taken between 1 May and 31 July, leading to overrepresentation of summertime clouds. Thus, the number of undefined cases could increase in spring and autumn”). In addition, the authors decided not to use observations with SZA > 70 deg. I understand their concerns, but this threshold is usually set at 80 deg. In summary, admitting observations up to 80 deg SZA, and using more whole sky images, would allow a largest number of cases to be used in the algorithm development and validation.

Besides these three general comments, that should be addressed as comprehensively as possible, I do have a number of minor comments:

a. L. 14. “aerosol formation” or simply “aerosol load”

b. L. 53-57. This paragraph breaks the introduction. In my opinion, it could appear later, along with the content of the paragraph starting in L. 84.

c. L. 84-89. In these 6 lines, the reference Duchon and O’Malley appears 4 times (!). I

understand that this reference is important in the present study, but this is made clear by saying that the new algorithm is based on that previous work. You don't need to repeat it 4 times.

d. L. 107, in the context of an applied Meteorology paper, the age of the pines is not relevant. Actually, it would be more relevant the height of trees and possible "shadows" on the instruments.

e. L. 118-119. You mention here that the Solis model is fed with AOD and precipitable water from Aeronet. This is ok, but then, this means that the method is not as easily "transferable" to other sites: they need to have not only pyranometer and ceilometer, but an Aeronet (Cimel sunphotometer) equipment too.

f. L. 124-125. Authors introduce here, too early, the idea of "parameter ranges". I think is not needed yet, before presenting the method for cloud classification.

g. L. 141-142. I would say that the ceilometer is not at all (or hardly, if any) sensitive to SZA.

h. L. 156. Why not presenting and discussing here the need for "scaling" radiation measurements?

i. L. 157. I would start a new paragraph regarding TR_max. Otherwise, it might be confused with the CBH from ceilometer.

j. L. 159. Well, before applying the classification you don't know if there are cumulus clouds. You should mention any type of varying cloudiness (obscuring and not obscuring the sun).

k. L. 178. You could better use "irradiance on a horizontal surface" instead of the too generic "radiation".

l. Section 2. Is cloud occurrence from ceilometer data described in this methods section?

m. L. 200-201. Cloud occurrence from radiation data cannot show seasonality as 5 moths are missing.

n. L. 224. How are middle and high clouds distinguished if there is a range (5000-7000 m) where both are cloud types are included?

o. L. 241. What do you mean by “uniformly and randomly”?

p. L. 244. “We first VISUALLY classified. . .”

q. Figure 4, caption. Explain the meaning of whiskers.

r. L. 289. With the current form of Fig. 4, the U shape is difficult to catch.

s. L. 341. “averaged over half an hour”, but later you talk about 21 minutes. Please clarify (and consider my previous general comment #2)

t. Section 3.4, last paragraph. This is a somewhat confusing paragraph. If this paper is about cloud classification, why a discussion about ecosystem interactions? And, in this section the algorithm is not used, but the brightness parameter. Regarding the use of 0,7 as threshold, it is clearly too low to garantee a clear sky. In any case, if this is a discussion about results, it would fit better in section 3.5.

u. L. 464-465. Well, this is a matter of which threshold you use. The “clearness index” in different versions is used to detect clear skies, by applying (if I’m right) a higher threshold (about 0,9).

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