Review of Ylivinkka et al. “Clouds over Hyytiälä, Finland: an algorithm to classify clouds based on solar radiation and cloud base height measurements” by Anonymous Referee #2

We thank Referee 2 for the valuable comments and improvements to our manuscript. We have revised our manuscript, and provide below a point-by-point answers to the comments, which are repeated in italic.

This paper revisits the relatively old topic of guessing cloud characteristics from solar radiation measurements. Specifically in this case, the main novelty is the simultaneoususe 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.

We are grateful for the positive viewing of our manuscript and helpful comments to improve it.

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

We agree, and thank Referee for the valuable comment. We changed the order of the sections, and now Sect. 2 is followed by the section describing the derivation of the algorithm.

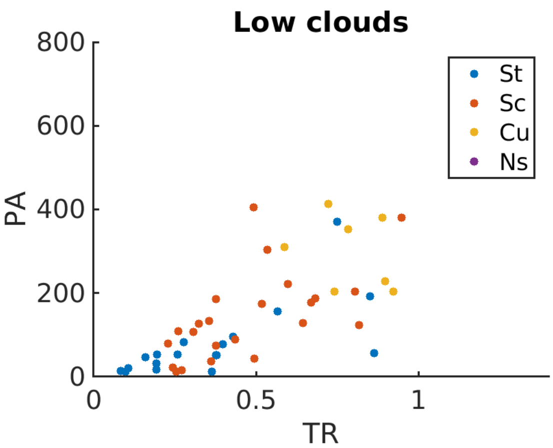
2. To my understanding, the fundaments used to determine occurrence (lines 177-78, “the ratio between the measured global radiation and modeled radiation at the top of the atmosphere (I)”) is almost the same as the “brightness parameter” (lines 340-341, “relation between the measured global radiation 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.

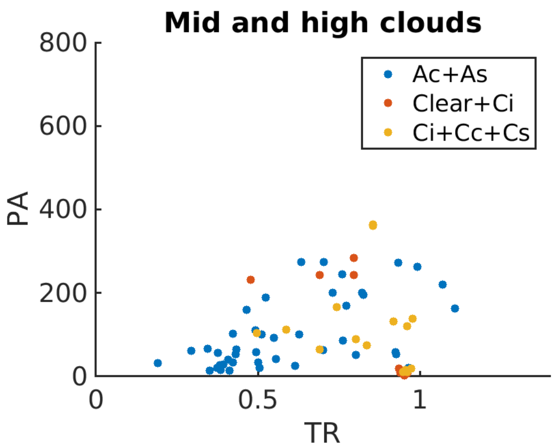
Referee is correct. We unified the terminology and theory related to cloud occurrence calculations from pyranometer measurements, and changed the term ‘brightness parameter’ to ‘clearness index’. Now, in Sect. 2.3 we introduce the clearness index, and in Sect. 3.4.1 we discuss its implementation in previous publications and ecosystem-atmosphere interactions related studies.

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.

We have now further tested the springtime performance of the algorithm by taking additional 124 TSIs from March and April 2014 including also middle and high clouds (minimum CBH>2000 m). The method was similar as described in the Sect. 3.1: we visually classified the TSIs, and placed the transmittance

and patchiness values of the cases in the plane of parameters (TR,PA). The results are shown below separately for low clouds, and middle and high clouds. We can see that the parameters mainly fall well to the parameter ranges displayed in Fig. 2 and Table 1. The results of the analysis are shown in the table.





		Algorithm								
		Cu	St	Sc	Ns	Ac+As	Ci+Cc+Cs	Clear+Ci	Other	Agreement (%)
Visual inspection	Cu	6	0	1	0	0	0	0	0	86
	St	1	13	1	0	0	0	0	2	76
	Sc	2	8	9	0	0	0	0	3	41
	Ns	0	0	0	0	0	0	0	0	NaN
	Ac+As	0	0	0	2	41	2	0	1	91
	Ci+Cc+Cs	0	0	0	0	0	4	4	7	27
	Clear+Ci	1	0	1	0	1	0	14	1	78
	Total									70

The discrepancies between St and Sc are mainly caused by cases when either both of them were present simultaneously, and it was hard to tell which one was more representative, or there was Ac or As layer on top of St/Sc clouds which affected the patchiness and transmittance readings. Similarly, the discrepancies between cirriform and clear sky cases from the introduced parameter ranges were mainly caused by partly cloudy conditions.

After this additional analysis, we are confident that the performance of the algorithm is good also during springtime, and therefore we removed the above-mentioned sentence considering the overrepresentation of the summertime cases. Furthermore, as the data availability of AERONET

data was low during March and April of 2016 and 2017, we produced median values of the data that we had, separately for spring (March+April) and September as explained in L. 135-139, we could define classes for many of the cases that had been in the “Base, no class” cloud class.
After careful consideration, we decided not to increase the SZA threshold from 70 deg to 80 deg as we could see that there was discrepancies between what the upward pointing ceilometer and the pyranometer measured at high SZAs. Cases falling into “No base, no class” cloud class 50 % had SZA > 60 deg, and in 25 % of the cases had SZA > 65 deg. Moreover, the further application of this algorithm is related to the studies investigating ecosystem-atmosphere interactions. SZA < 70 deg includes daytime data from March to September i.e. the whole growing season in southern Finland.

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”

We kept the original wording as we specifically were pointing to the possibility of clouds to affect aerosol formation as discussed in Dada et al. (2017), instead of more general concept of aerosol load/concentration in the presence of clouds.

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.

Referee is correct. We moved the paragraph as suggested.

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.

We agree with Referee. We reduced the number of citations to Duchon and O’Malley (L. 84-90): “Our automatic cloud classification algorithm is based on global radiation and CBH measurements. It is an adaptation of the work by Duchon and O’Malley (1999). Their so called “pyranometer method”, using only pyranometer data, was developed to classify clouds in places where no human observations were available. Even though the pyranometer method is simple and effective, its cloud type classes are rather broad (stratus, cumulus, cumulus+cirrus, cirrus, clear sky, precipitation+fog, and other), and the classification was found to be in agreement with human observations only 45 % of the time. Our improved cloud type classification algorithm uses additionally CBH data. Hence, the number of cloud type classes can be increased compared to Duchon and O’Malley (1999) because the clouds at different levels can be distinguished.”

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.

Referee is correct. We removed the information of the age of the forest and added the information of canopy height as well as that the radiation measurements are conducted above the canopy level. The text now reads (L. 108):

“The station was established in 1995, and it is surrounded by Scots pine (Pinus sylvestris) dominated forest with canopy height of ca. 18 m (Hari and Kulmala, 2005).
The main data set in this work includes data from a pyranometer and a ceilometer. The pyranometer (Middleton solar SK08 pyranometer) measures global radiation at wavelengths of 0.3–4.8 μm. The measurements were conducted above the canopy level at SMEAR II.”

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.

This is a good notation, and we agree that the need of AOD and precipitable water for the clear sky model may complicate the transferability of the algorithm. It is possible, however, to change the clear sky model to some other that is better suitable for the environment or does not need extra variables. We added this information to the text (L. 118):
“We used Solis model because it explicitly takes into account the aerosol load in the atmosphere. However, in case AOD and precipitable water data are not available when applying this algorithm in other environments, other clear sky models may be employed, though we recommend to use as accurate model as possible.”

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.

We agree with Referee, and changed the wording as follows (L. 127):
“In the development and validation process of the algorithm, we employed cloud classification made by human observer based on total sky images taken during the BAECC campaign between 01 May and 31 July 2014 (Fig. 1).”

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

Referee is correct. We changed the wording. The text now reads (L. 146):
“However, for the cloud occurrence and CBH analysis using the ceilometer measurements, we used data independent of the time of day and season, because unlike the pyranometer, the ceilometer is not sensitive to SZA.”

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

We agree with Referee that leaving the discussion of the scaling for later complicated the readability unnecessary. The text was changed accordingly (L. 180).

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

Please, see answer to item j.

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).

After revising the algorithm, it was clear that TR_max was a redundant parameter, and hence we removed it completely, and thereby also the section discussing its use.

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

We changed the wording as suggested. The text now reads (L. 199):
"It is determined as a relation between the measured global irradiance and modeled irradiance on a horizontal surface at the top of the atmosphere:"

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

It was not as the cloud occurrence from the ceilometer is simply the number of cases when the ceilometer detected a cloud base, but we now added clarifying sentence (L. 197):
"From the ceilometer data, the cloud occurrence is simply the number of cases when the ceilometer detected a cloud base."

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

Referee is correct. We removed the sentence describing the variation in cloud occurrence from the pyranometer data.

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

We have now decreased the upper limit of middle clouds from 7000 m to 5000 m to avoid problems with distinguishing middle and high clouds. This had negligible effects on our results.

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

We modified the sentence to make it easier to understand. The text now reads (L. 233):
"We took a sample of 665 total sky image–measurement data pairs randomly yet timewise uniformly, i.e. making sure that we utilized the whole measurement period, from among a set of total sky images taken between 01 May and 31 July 2014 in Hyytiälä."

p. L. 244. "We first VISUALLY classified..."

We changed the wording as suggested.

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

We changed the caption to be more detailed:

"Illustration of the transmittance and patchiness ranges used as classification criteria for different cloud types. Markers display the locations of the maximum data point density of each cloud type, and whiskers extend to the lower and upper limits of the permitted parameter ranges, listed also in Table 1. Color shows the average CBH of each cloud type."

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

Referee is correct. We removed the paragraph.

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

We thank Referee for pointing out the discrepancy in the text. The averaging was done over 21 min moving time window. We clarified the text and followed the 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.

The reason for discussing about ecosystem interactions comes from the idea of future application of the algorithm. However, we agree with Referee that the paragraph fits better to Sect. 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).

The selection of the suitable threshold is indeed depending on the application in question. Yet, the information of cloud variability is important as there can be situations when transmittance and clearness index are high e.g. in the presence of cumulus and cirriform clouds. We modified the text a bit to be more precise (L. 504):
"We found that cumulus, altocumulus, altostratus and cirriform clouds were present when clearness index was above 0.7 threshold that has been used as a limit for clear sky when studying aerosol–cloud interactions. Thus, the studies defining clear sky cases based on clearness index, may be biased. High clearness index threshold is deficient criterion as in the presence of patchy clouds, the clearness index may still be high periodically. Hence, the criterion should concern conditions with high transmittance and low patchiness."

Besides the comments by Referee, we changed term "transparency" to more generally used "transmittance". We changed the upper limit of transmittance for stratus clouds from 0.4 to 0.6. This was done because we could see that many St clouds fell into this area but were previously not classified. This, along with the applied median AOD and precipitable water values (see item 3), decreased the number of cases falling into the class "Base, no class". Now also the frequency of occurrence of stratus clouds is better in accordance with observations in *Climatic Atlas of Clouds Over Land and Ocean* (available online at https://atmos.uw.edu/CloudMap/, last access: 10 January 2020). We additionally changed the upper transmittance limit of Ns from 0.4 to 0.3 and lower transmittance limit of Ac+As clouds from 0.4 to 0.3. This was done because we could see that especially in springtime Ac+As clouds were previously falsely classified as Ns. The overall occurrence of Ns clouds decreased (from 1.4 % to 0.6 %) and occurrence of Ac+As increased (from 9.0 % to 10.3 %) but otherwise the change did not affect our results.

References

Dada, L., Paasonen, P., Nieminen, T., Buenrostro Mazon, S., Kontkanen, J., Peräkylä, O., Lehtipalo, K., Hussein, T., Petäjä, T., Kerminen, V.-M., Bäck, J., and Kulmala, M.: Long-term analysis of clear-sky new particle formation events and nonevents in Hyytiälä, Atmos. Chem. Phys., 17, 6227–6241, https://doi.org/10.5194/acp-17-6227-2017, 2017.

Duchon, C. E. and O’Malley, M. S.: Estimating cloud type from pyranometer observations, Journal of Applied Meteorology, 38, 132–141, 1999.

Hari, P. and Kulmala, M.: Station for Measuring Ecosystem–Atmosphere Relations (SMEAR II), Boreal Environment Research, 10, 2005.