Reply to comments by J. Calbó (Referee #1)

on the manuscript " Long-term study of cloud radiative effect, cloud fraction and cloud type at two stations in Switzerland using hemispherical sky cameras " by Aebi et al., submitted to *Atmospheric Measurement Techniques*.

We thank the referee for the constructive comments that we have tried to accommodate in the text. Detailed answers to the comments are given below (bold: referee comment, regular font: authors response).

This paper presents a summary of radiation measurements performed at two sites in Switzerland, in combination with estimations of cloud cover and type based upon an automatic method performed on whole sky images. Specifically, radiation measurements are presented as cloud radiative effect, as the corresponding modelled irradiances for a cloudless sky are subtracted from measurements. Although the literature on cloud radiative effect is pretty large, there is still room for more studies that add insight on this matter, specially for observational studies from ground-based measurements. So this study is worth to be published, although a number of issues should be addressed before publication. I must say that in general I enjoyed reading the manuscript and that all my comments below are provided with the intention of further improving this study.

General comments:

(1) **Cloud radiative effects are computed by subtracting model estimations of cloudless sky shortwave and longwave irradiances from the corresponding measurements. Therefore, the performance of the "cloudless" models is critical to get suitable values of CRE. The authors give the mean bias of models for both sites, but I suggest that more detail about the performance of cloudless sky models is shown in the paper. It should be quite easy, just by showing the CRE computed for cases corresponding to 0 octas. This could be shown as function of SZA (for shortwave) or as function of month or temperature (for longwave). If the models were correct, the CRE for these cases should be 0 (or at least, centered at 0). If there is a systematic bias at either of the sites, for some SZA, etc., this could be used to further discuss the results. You should also clarify if the "clear sky cases" that you use to assess the models are the same that are later defined as "cloud-free" cases.**

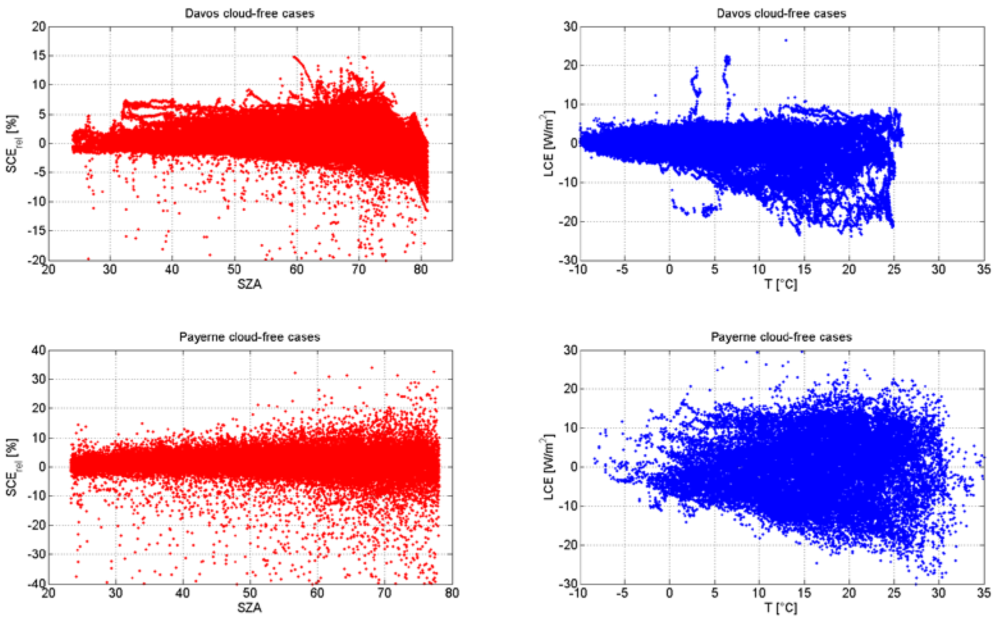
Thanks for this comment. The Figure here show in different panels the mean SCE_rel depending on SZA (left) and the mean LCE depending on the screen-level temperature (right) for the cloud-free cases in Davos (top row) and Payerne (bottom row). For the shortwave we see that there is a slight increase in the uncertainty with higher SZA.

We decided to not include this Figure in the paper. However for the shortwave we calculated the mean and the standard deviation separately for SZA < 50° and SZA > 50° and added these values in the text:

p. 4, l. 31 and p.5, l. 1-4:

The difference between SW measurement and the cloud-free model depends on the SZA. The bigger the SZA, the higher the mean difference. In Davos, the mean difference changes from 7.2 ±20.7 Wm^{-2} (0.9 ±2.6 %) for data with SZA < 50° to 5.7 ±14.7Wm^{-2} (1.1 ±3.8 %) for data with SZA > 50°. In Payerne, the mean difference is 7.3 ±41.7 Wm^{-2} (1.0 ±5.2 %) for data with SZA < 50°. The mean difference is with 3.3 ±34.1Wm^{-2} (0.6 ±8.9 %) slightly larger for data with SZA from 50 to 78°.

Thanks also for the comment about the clear-sky and cloud-free cases. We changed now everything to cloud-free.



(2) Some deeper discussion of results is needed. In particular, there are some important differences between the two sites, and some strange behavior of CRE that should be highlighted and commented. The authors already make some comments, but additional insight would be appreciated. For example, regarding cloud type (figure 2), there are almost no Cu (and very few St-As) at Payerne, while there are almost no Cb-Ns at Davos. Or, Table 2 shows, for St-As class, that while in Davos enhancements (i.e. CRE > 0) are found for cc < 5, in Payerne CRE reaches very low values (CRE < -35%). It is particularly strange the value for cc = 1, as the median is equal to the first and third quartile (-70%). This also affects results in Table 3, where the behavior at Davos and Payerne is strangely different, in particular for cc < 5 and for most cloud type classes. I wonder if this might be the result of a bias in the cloudless irradiance estimation at one of the two sites (see my previous point) or also a consequence of a very limited number of cases for some particular conditions of cc and cloud type. I mean, for statistics to be somewhat representative, a minimum number of instances should be included; moreover, a number of instances corresponding to different seasons, years, etc., would be convenient.

We added some more paragraphs to discuss the different behavior in the data following your suggestions:

p. 7, l. 6ff.:

In Davos, as determined by our algorithm, from October to May St-As is 5 present in at least 40 % of the cases per month. This fraction of St-As is rather too high and might be due to a limitation of the cloud type algorithm. The limitation is, that the algorithm applied for Davos is trained with images from Payerne. Therefore it might be more difficult to distinguish between low-level cloud classes (e.g. St-As and Sc) in Davos. This limitation might also be responsible for the rather infrequent determination of Cu in Davos.

p. 12, l. 5ff.:

For the calculation of the values in Table 2 different numbers of cases have been taken into account (see Table A1 and A2). Analysing e.g. the images that belong to the group St-As and 2 oktas in more detail, leads to the result that at all the images for this specific group in Payerne the sun is covered by a cloud, whereas in Davos, of the 58 images only in around 20 % of the cases the sun is occulted and in the remaining 80 % the sun is visible. As further discussed in Section 3.3.2, this fact of visible or occulted sun can lead to a large difference in SCE_{rel} values. These larger differences in SCE_{rel} values between the two stations mainly occur when only a limited number of images is available. Therefore, some of the SCE_{rel} values have to be taken with caution.

Additionally we added in the appendix two tables (A1 and A2) which show the number of cases that have been taken into account for the calculation of Table 1, Table 2 and Table 3.

Specific comments:

(1) **Title. I don't think the word "long-term" reflects the content of the study, which is performed on 3-4 years of observations. In fact, no further attention is put on the length of the time series, so simply removing "long-term" from the title would be adequate.**

We changed the title to:

Cloud radiative effect, cloud fraction and cloud type at two stations in Switzerland using hemispherical sky cameras.

(2) **Abstract. OK in general. You could add that CBH is from ceilometer and IWV from GPS measurements. You could simplify the writing when referring to occulted (measured direct radiation less than 120 Wm^{-2}) or visible Sun (direct radiation greater than 120 Wm^{-2}).**

The following sentence has been added in the abstract:

p. 1, l. 3-4:

The cloud base height (CBH) information are retrieved from a ceilometer and integrated water vapour (IWV) data from GPS measurements.

As suggested, the two terms occulted and visible sun have been added and are used in the following:

p. 1, l. 9-11:

In cases where the measured direct radiation value is below the threshold of 120 Wm^{-2} (occulted sun) the SCE_{rel} decreases substantially, while cases where the measured direct radiation value is larger than 120 Wm^{-2} (visible sun) lead to a SCE_{rel} of around 0 %.

(3) **2.2 SCE_CSM is not a radiative effect, as you correctly state when defining this symbol. Therefore, I wouldn't use SCE_CSM, but something as SW_CSM, to avoid possible confusion.**

We changed the symbol SCE_CSM to DSR_cfm in Equation 2 and thereafter also in the text:

p. 4, l. 11:

$SCE_{rel} = SCE/DSR_{cfm} * 100\%$

(4) **2.3. Clear sky models. If aerosol conditions are used in the SW model, the source of aerosol measurements/data should be explained in section 2.1.**

In Section 2.1, p. 3, l. 20-21, we already have the sentence:
Aerosol optical depth data are retrieved from precision filter radiometers (PFR, manufactured by PMOD/WRC).

However, to be more clear, we added:

p. 3, l. 20-21:

Aerosol optical depth (AOD) data, used for the shortwave cloud-free model, are retrieved from precision filter radiometers (PFR, manufactured by PMOD/WRC).

(5) **2.4. Cloud fraction and cloud type. If I understand correctly, LCE is also part of the algorithm for cloud type recognition. Although this may be good for obtaining good classification results, it is quite strange in the frame of the present study, as in this way, the "dependent" variable to be studied (LCE) takes also part in the definition of one of the "independent" variables (cloud type). In other words, some "circularity" is introduced by using LCE as a feature for cloud type discrimination. This could partly explain why dispersion of LCE values depending on cloud type/cover is much lower than dispersion of SCE values.**

LCE as a feature in the cloud type detection algorithm has been added in order to better distinguish between low and high level clouds in cases the sky is fully covered. To add the LCE in the algorithm is an advantage, because in fully covered sky images, the textural features do not give enough specific information to distinguish between different cloud levels and therefore cloud types. Thus it would be too difficult to distinguish between different cloud types which would result in many misclassified images. Since LCE values are quite distinct for low and high level clouds, it helps to distinguish the different cloud levels.

(6) **3.1.1. LW cloud effect. Could you at least speculate a reason for the non-linearity shown in Fig. 3?**

We added the sentences:

p. 8f, l. 11ff:

Clouds at different zenith angles in the sky have a stronger or weaker impact on the downward longwave radiation measured at the surface. In case the zenith angles of the clouds are not equally distributed in our analysed time period, this might be a reason for this nonlinearity in LCE. However, we have not analysed it in more detail yet and is subject of a future study.

(7) **3.1.2. SW cloud effect. The first sentence could be set between parentheses within the current second sentence. I would recall some times that "higher" means "less negative". In fact, in the third paragraph, where you say "For Payerne, a clearly lower…" I think it should say "higher". In general, the use of relative values is "risky", as for large SZA the SW irradiance may take very low values, so (given the unavoidable uncertainties in both measurements and cloudless estimations) the relative SCE_rel may tend to very large values. I would suggest using a maximum SZA (SZA < 80 deg?) for the cases included in the analyses. Maybe the horizon characteristics of the two sites already limit the range of SZA, but this should be explicitly commented in the text.**

We tried to make it more clear that for the SCE_rel higher means less negative.

We decreased the maximum SZA for Davos and we specified the SZA ranges taken in section 2.1, p. 3, l. 25ff.:

Data have only been taken into account for daytime measurements when the sun is located minimum five degrees above the horizon and the mountains. For Payerne, the study of CRE includes data from January 1, 2013 to April 30, 2017 with a time resolution of five minutes. Data considered are during daytime with a solar zenith angle (SZA) of maximum 78°.

(8) **3.2.1, Figure 5. I wonder if it is necessary to show results for Cu and Sc, as these results are almost undistinguishable. In addition, it doesn't make sense to put a CBH of 5 km for a low cloud; maybe results for 0.5 km up to 2.5 km for only one cloud type would be more adequate. In any case, the similar behavior between Cu and Sc might be the result of similar microphysical characteristics, not similar "shape".**

With Figure 6 (we guess that you meant this one), we want to show the influence of IWV on the LCE in general and not for a specific case or station. Thus, we think that this graph shows nicely this influence.

We changed the sentence as suggested (p.16, l. 10-11):

Cu and Sc show a similar behaviour in the model which might be explained by similar microphysical characteristics of the two cloud types.

(9) **3.2.1, Figure 6. I think that the black line corresponds to 10 km and above, not to above 12 km as written in the text. It would be nice adding another panel, where the LCE is shown, for**

the 8 octas cases (i.e., for cc > 0.95), against CBH, and also distinguishing by ranges of IWV. I would say that this could be a quite interesting plot that would complement current Fig. 5 and 6.

We decided to remove the current Figure 7 and put a new one which shows the dependence of LCE on cloud base height for Payerne and linear regression lines of the following measured IWV ranges.

(10) Figure 7 is very interesting, but it is showing that the median value of SCE_rel for a given cloud clover might not very representative of what is happening, since in fact there are two very different effects (reduction and enhancement) depending on whether the Sun is occulted or not. Although you comment these two effects, it should be mentioned that median values in tables 2 and 3 are to be taken with caution.

We mentioned now that the median values in Table 2 have to be taken with caution (p. 12, l. 10-11). The reason why we still think that it makes sense to calculate the median with all data (reduction and enhancement) is, that e.g. in weather prediction models the input about clouds is an average over a certain time period where also enhancements and reductions occur.

(11) Conclusions. As a general comment, I would suggest shortening a little bit this section, by removing some repetitive statements and non-essential results. In fact, most general statements correspond to well-known facts (e.g., "…cloud base height and fractional cloud coverage have an influence on the range of the LCE…"). When writing this kind of well-known results, it should be stated that the current study is confirming them. In other words, it should be made clearer what it is really a finding of the current study, and what are expected results and known facts that the study is confirming.

We have shortened and changed a large fraction of the conclusion.