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Interactive comment on "Long-term study of cloud radiative effect, cloud fraction and cloud type at two stations in Switzerland using hemispherical sky cameras" *by* Christine Aebi et al.

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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

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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:

- 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.

- 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.

Specific Comments, minor suggestions, technical corrections

- 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.

- 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).

- 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.

- 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.

- 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.

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

- 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.

- 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”.

- 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.

- 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.

- 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.

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