

Review of "Trends in surface radiation and cloud radiative effect at four Swiss sites for the 1996-2015 period," by Nyeki, Wacker, Aebi, Gröbner, Martucci, and Vuilleumier, submitted for publication in ACP.

This paper describes a study of climatological statistics and trends in various radiative quantities over the period 1996-2015 based primarily on measurements of longwave and shortwave radiative fluxes at four sites in Switzerland. In addition, methods of identifying clear-sky periods and parameterizing clear-sky fluxes are examined. This paper contributes to the growing body of literature about trends in radiative energy budget terms and their causes. The study is generally of good quality. I have no major complaints, but a fairly long list of shorter questions and comments.

Major comments:

The attention the authors give to the details and quality of the radiometric measurements is unusual and much appreciated. The use of multiple techniques to evaluate continuity of the measurement time series and the significance of results is also laudable.

Minor comments:

p. 2, paragraph starting "iv": This paragraph is unclear. Does it mean that the measurements from PMOD are wrong and that those from BSRN are right? Which data requires correction?

p. 3, paragraph starting "v": Are these uncertainties for instantaneous measurements?

p. 3, line 20: Was a sampling threshold also applied to the clear-sky data?

p. 4, line 3: What is the source of these reference values?

p. 4, lines 20-21: Could you at least give the general basis for the derivation of AOD values - sun photometer measurements? satellite measurements?

p. 4, line 30: What is the definition of the "effective atmospheric boundary layer temperature" and how is the value obtained?

p. 5, line 3: Are the values of a and b the same for all times and locations?

p. 5, lines 33-34: Isn't there snow or ice at the other locations?

p. 5, lines 35-40: I'm not convinced that means of 0.7, 0.68, and 0.67 are significantly different. However, the different seasonal pattern at PAY looks interesting. Can you comment on that?

p. 6, lines 10-11: Why should the frequency of occurrence of clear-sky conditions affect the average clear-sky DSR value?

p. 6, line 23: It looks to me like just as many DSR trends are significant at the 90% level as T2m trends (although fewer are significant at the 95% level).

p. 6, lines 38-42: Please give specific results of the homogeneity tests for DSR and DLR at Davos, since text on page 2 describes differences in the instrumentation used for different time periods.

p. 7, first full paragraph: Is it meaningful to compare trends from all these different time periods? It also seems that trends from global means and individual stations are being compared. I wouldn't expect trends from climate models to be particularly accurate. (Why would you look at two RCPs when we know what the CO₂ concentrations were over the time periods of interest?) You might think about presenting results from satellite studies instead/in addition.

p.7, line 30: Do we expect long-term trends in the solar zenith angle?

p. 7, line 35: Why is altitude important to LCE?

p. 8, line 2: How does the reduction in daylight hours during the winter affect CRE at DAV and PAY more than at LOC and JFJ? Aren't all the sites at about the same latitude? Also, does this sentence only pertain to winter?

p. 8, line 18: Is the Sanchez-Lorenzo study relevant to your results? You say it only showed cloud cover trends in the 1970s and 1980s.

p. 8, lines 21-23: Another paper that ties changes in DSR to changes in clouds (rather than aerosols) is Parding et al., 2016 (J. Climate).

p. 8, lines 33-37: By "anomaly," do you still mean the difference between estimated and measured clear-sky DLR? If so, how do the other authors estimate DLR? Do they include aerosols and trace gases, as mentioned next? It's not clear how results with respect to these variables were obtained.

p. 9, lines 17-21: This text needs to be clarified. It sounds like you are trying to evaluate the sky-camera method of estimating cloud cover, but the results are given in W/m². What are you actually doing? And why do you believe that the results are "likely" to improve when more data is available?

p. 10, lines 9-10: It would be useful to compare the magnitudes of the detected trends and measurement accuracy in the text. Otherwise we are left with the impression that the standard deviations given in the tables accurately represent your confidence in the results.

Figure 1: If you have no comments about the trends determined using the Weatherhead method, why are they included?

Table 3: Any idea why the clear-sky DSR decreases at JFJ but not at the other stations? Is there a reason cloud cover trends aren't included in this table?

Figure 3: Did you also check the AOD data for artificial jumps? There looks like there might be a discontinuity in the PAY data around 2011.

Table 4: The text on page 7 lists fairly large biases and RMSEs for the SW clear-sky fluxes, as much as 17% of the means and 3x the standard deviations of SCE shown in this table, respectively. Are these errors important to the SCE estimates?

Clarity of presentation:

At some points, additional detail or improved clarity is needed. Questions about the meaning of certain phrases and suggestions for wording changes are included in the accompanying PDF file.

Note: A comma is required

- before (and after) a phrase starting with "which"
- after (and before, if they're not inside parentheses) "e.g." or "i.e."
- before "etc."