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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Shortwave surface radiation budget network for observing small-scale cloud inhomogeneity fields" *by* B. L. Madhavan et al.

Anonymous Referee #2

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General comment:

This manuscript describes an experiment and the corresponding dataset for the study of the small-scale variability in time and space of surface global irradiance. This variability is mostly induced by clouds and is especially significant in case of broken cloud situations. This experiment is designated as the HD(CP)2 Observational Prototype Experiment (HOPE) and is conducted in the vicinity of the Jülich Observatory in Germany. The setup includes 99 stations covering an area of 10km by 12km in farmland. The stations are equipped for measuring global horizontal irradiance (GHI, with EKO photodiode pyranometers), air temperature and relative humidity.

The cloud-induced small scale variability of irradiance is an issue that is worth investigating and a better understanding of it would have numerous applications from climate-related research on cloud-aerosol-radiation interactions to radiation transfer representation in various numerical weather prediction models or for solar energy developments. In this sense it is a subject that is worthy of publication in AMT. I have however major concerns that I think preclude publication of this manuscript in AMT unless major revisions are made.

The main issue is the absence of a real assessment of the accuracy that is reached with the measurements performed within HOPE. Since this manuscript presents the experiment and the dataset, it should give the foundation on which building other research with such a dateset. HOPE being designed to explore the variability of ground-based global irradiance, one needs to know the accuracy of the measurements in order to know if a difference between two measurements is significant or if such a difference is within the uncertainty. In several instances the authors seem to ignore uncertainty determination or overlook uncertainty sources (see specific comments 4, 5, 7, 8, 10, 19 and 22 below). In addition the influence of the non-uniform spectral response of the EKO pyranometer is incorrectly considered, especially when computing the effective transmission for global irradiance (see comment 7, 12 and 17).

Specific comments:

The numbering given in the remarks below correspond to the "print-friendly" version of the AMTD manuscript.

1. page 2556, lines 16-17: The authors mention solar radiation being "modulated by direct and indirect multiple interactions with clouds." What is meant by these direct and indirect interactions? Direct and indirect aerosol effects on clouds are commonly accepted concepts, but it does not seem to be what is mentioned here, and the exact meaning is unclear.

2. page 2558, lines 1-10: The authors correctly mention uncertainties in satellite re-

trievals induced by small-scale variability in the cloud fields. They should also mention that understanding such small scale variability will help assess what is the best agreement that can be reached in validation studies when comparing point measurements with satellite area estimates (linked to pixel size), when comparing point measurements to model cell results or when comparing satellite area estimates to model cell results.

3. page 2558, lines 20-23: It is indicated that HOPE "was designed [. . .] to measure the sub-grid scale variability of [. . .] cloud micro-physical properties with 1m spatial and 1s temporal resolution". It is difficult to understand how these properties can be measured at 1m resolution with 99 instruments distributed over an area of 120 km2, roughly 1 instrument every kilometer. There are at least several hundred meters between measurement points, which is more than 2-order of magnitude larger than the spatial resolution sought. On the next page it is further claimed that the focus is on "probing the spatio-temporal variability of cloud induced radiation fields at the surface with a resolution comparable to or even better than HD(CP)2 model".

4. page 2559, line 22 to page 2560, line 27: The information concerning the uncertainty is incomplete in the description given here. A full system including sensors, ADC modules and logger unit is described as a whole. The uncertainty of the whole system is a combination of the uncertainty of its various components. The sensors have uncertainties including intrinsic sensor uncertainties, calibration uncertainties and operational uncertainties. Intrinsic uncertainties seem to be taken from the manufacturer datasheets (Table 1 and maybe point ii), the calibration process is described in Appendix A, but the uncertainty of the process is not estimated. It should consider the uncertainty of the initial reference, the uncertainty of the transfer reference sensor used, and the uncertainty of the process, which is probably dominated by uncertainties in the spectral response. A list of possible source of uncertainty is given at the end of the Appendix A, but apparently ignored (see comment 21). The fact that the uncertainty of the calibration process is difficult to evaluate does not allow ignoring it. If the authors lack information about it, they should assign an uncertainty that reflects their lack of

knowledge. The third sensor uncertainty component are the operational uncertainties, these are typically linked to operational conditions such as soiling, leveling of the sensor, etc. that the author at some point seem to refer to as "observable factors". These conditions are mentioned in the manuscript. They are evaluated on different scales (section 2.2), but the corresponding uncertainty is not determined in a clear manner. In addition to the sensor, the other parts of the observing system also have uncertainties: The ADC/logger system has uncertainties linked to linearity, logger resolution, etc. that are usually found in the technical description. It is also indicated (p. 2561, l. 1-2) that an amplifier is used, and the linearity of the amplifier should be indicated, and if needed taken into account. Finally, all these uncertainties should be combined in the appropriate manner to indicate at what level a spatio-temporal variability (differences between neighboring or successive measurements) is significant, and when it should be considered within the uncertainty. The uncertainty sources considered, the manner they were combined and the corresponding overall uncertainties in different conditions should be described and it should be one of the main goal of this manuscript. For radiation measurements, Reda (2011), Dutton and Long (2012) or Vuilleumier et al. (2014) have shown how such uncertainties can be estimated.

5. page 2561, lines 20-27, and page 2565, line 27 to page 2566, line 2: Although the information about the status of the sensors is my opinion crucial to estimate overall data quality, it seems that these flags were finally not used because "checking of the data quality for each station requires enormous time" (page 2565, line 27 to page 2566, line 2). Either the authors should translate this information into usable operational uncertainty estimates, or they should not mention such flags if they are not used. The authors also indicate the level imbalance of the mounting platform was noted, but they do not indicate how it was evaluated. Did the sensor included bubble level for this? Similarly, was the condition of the thermopile sensors assessed, and was the leveling of these regularly checked with a bubble level?

6. page 2562, lines 19-20: It is indicated that "At higher solar zenith angles [. . .] at-

mospheric refraction leads to an increasing effect on the global irradiance". The atmospheric refraction is responsible for a small difference between the apparent solar zenith angle and the real (astronomic) solar zenith angle. This difference is non-negligible only at high solar zenith angle (>85 degree) with maximal differences less than 1 degree. The effect on GHI is minimal, except maybe at sunrise and sunset if there are no horizon effects. Such an effect for this study is negligible when compared to other sources of uncertainties such as error in sensor leveling or, at high solar zenith angle, the directional uncertainties of the sensor. The reasons for the refraction being mentioned here are unclear.

7. page 2563, Line 3: "Corrected" values for extraterrestrial irradiance and global surface irradiance are used in Eq. 4. However, using corrected or uncorrected values does not make any difference because of how the corrections were made. In this case the value 619.91 Wm-2 appears in the numerator and denominator and cancels. The real issue is that the spectral response of the EKO photodiode sensor is already taken into account in the sensitivity value that is obtained in the calibration process. However, the correction for the non-uniformity of the EKO spectral response is determined with a given solar spectrum for the surface. This already introduces an uncertainty because the surface solar spectrum changes between clear-sky and cloudy situations and also depends on the solar zenith angle. This additional uncertainty is probably the main component of the spectral error of 2-5% mentioned in Table 1. But for computing the corresponding extraterrestrial irradiance, one needs to take into account the significant difference between the surface solar spectrum (typically the one used by EKO in its calibration process) and the extraterrestrial solar spectrum. This is the only manner to correctly compute a global transmittance and correctly assess the corresponding uncertainty.

8. page 2563, lines 20-24: The "observable factors" are sources of uncertainty that cannot be "nullified". Even if the largest errors are removed by ignoring the most obvious erroneous data, uncertainties linked to such factor also affect the other mea-

surements. They should be estimated. Various studies described these factors (e.g., Michalsky et al., 1988 or Geuder and Quaschning, 2006 for soiling; Long et al., 2010 or Vuilleumier et al., 2014 for leveling).

9. page 2565, lines 5-8: The definition of RMSD is unclear: it is difficult to figure out if the RMSD is computed over time and space. My interpretation is that it is computed over a given time interval (which is not indicated), and that it uses the difference at any time between the maximum and the minimum in the spatial field. However, this is not following the commonly accepted meaning for RMSD, which usually is for a difference between a measurement and a reference value, or between two given time series. If my interpretation is correct, the minimum and maximum are not corresponding to given sensors, but correspond to varying sensors as time goes on.

10. page 2566, lines 5-15: Determinations made on a single day or with a single sensor are not representative of the uncertainty for the whole period and dataset with changing conditions. The given indicators do not allow inferring an uncertainty. In addition, the 0.99 linear correlation between one thermopile and one photodiode sensor is not meaningful. If the conditions are favorable, high correlation are always obtained between neighboring radiation sensors, simply because GHI depends strongly on solar zenith angle (GHI is low at high solar zenith angle and inversely). The correlation in the transmittance is much more meaningful, but there are here issues on how the transmittance was computed (see comment 7). At the end of the paragraph, a 10% maximum error is quoted. This may be an uncertainty statement but no indication is given on the way the uncertainty computation is made and no indication is given on the type of uncertainty (combined or not, expanded or not). The GUM (Guide to the Expression of Uncertainty in Measurement by the Joint Committee for Guides in Metrology) indicates how to make a proper uncertainty statement. Furthermore, the authors indicate that "these are not further considered [...] as [the] focus was to study the small-scale spatial and temporal variability of cloud inhomogeneity fields." This statement cannot be justified because it is precisely a correct estimation of the uncertainty that will allow

determining whether a difference between two measurements is meaningful or not.

11. page 2567, lines 15-16: It is noted that neglecting the model to observations biases observed by Michalsky et al. (2006) may lead to substantial errors in clear sky radiative transfer parametrizations. But it should also be emphasized here that Michalsky et al. (2006) used some of the best commercially available technologies, including absolute cavity radiometers for determining separately direct and diffuse irradiance, whose combination allows the most precise determination of GHI. This together with observance of the strict ARM maintenance guidelines allowed reducing measurement uncertainties including those described in this manuscript as "observable". The measurement uncertainty in the case of Michalsky et al. is well understood and well under control and allows determining model-observation biases as low as 1%. In the research described here, it is not the case and biases could be obtained that would not be linked to model error but measurement error. In case these would be interpreted as model error this could lead to erroneous conclusions.

12. page 2568, lines 16-21: The 5% bias found between thermopile and photodiode pyranometer is important, especially when compared to the 1-2% biases mentioned in the previous comment. If these measurements were trusted and compared to a clear-sky model it would lead to unrealistic aerosol parameter needed for matching the model to the measurements. Here, the bias is most likely due to the combination of the difference between surface and extraterrestrial spectra and the spectral response of the EKO pyranometers (comment 7), and possibly also the uncertainty linked to the directional response of the pyranometers.

13. page 2568, lines 26-27: It is indicated that "the diffuse irradiance decreases rapidly as patches of clear sky enter the field of view of the pyranometer". Actually, it is mostly the presence or absence of cloud patches in the vicinity of the sun and their optical depth that determines the intensity of the diffuse radiation as Fig. 6b shows. In this case, the cloud coverage is relatively low, but a cloud patch close to the sun results in a diffuse irradiance most likely high.

14. page 2569, lines 1-7: The authors focus on the flux absorption in the cloud layer. But the scattering is a more important process here, especially in inhomogeneous situations. A large fraction of the radiation is reflected back upward by the cloud as a consequence of scattering.

15. page 2569, lines 22-24 and page 2571, lines 2-4: The study of the correlation and variance between the mean and the median of a dataset as function of time is not really informative. The mean and the median of a distribution will always correlate well, because the variability in the mean and the median is originating from the time evolution of a single dataset. Here it is not the correlation or variance of different measurements that are considered. The only point of interest is checking the difference between the two. If there is a difference between the mean and the median, it is an indication that strong outliers (distribution tails) are not symmetrically distributed.

16. page 2569, lines 24-26. If the mean is lower than the median, this indicates that the distribution is not symmetric with a longer tail toward low values. However it is not straightforward to infer from this that the cloud cover is low. Even with totally overcast sky, one can imagine relatively homogenous sky radiance except for a given relatively small region of clouds with high optical depth that would produce at some location very low value of transmittance. This situation could also produce an asymmetric distribution with similar properties. As another example, during the morning with the sun relatively low on the horizon, one can imagine a situation with a high cloud cover, but with a clear region over the horizon where the sun would be located. In such a case, if a few sensors are shaded, this would also produce a long tail toward low value with the same result.

17. page 2570, lines 10-12: The statement "An overcast sky is characterized by relatively high irradiance towards the shortwave end of the spectrum compared to the corresponding spectrum for a clear sky" is difficult to understand. The global irradiance in overcast situation will be lower than the irradiance of a clear sky situation at any wavelength. Do the authors mean that the spectral distribution of the irradiance for an

overcast sky is different than the one of a clear-sky situation? The spectral distribution of a clear-sky situation depends on the solar zenith angle, and also on which component (direct or diffuse) one is referring to, maybe the authors refer to their combination in the GHI? In addition, it is difficult to understand why the authors mention difference in the irradiance spectrum since they do not measure it. The only reason why it would be important is because of the uncertainty introduced because of the non-uniform spectral response of the EKO pyranometer. But the authors do not attempt any evaluation of this effect.

18. page 2570, lines 12-16: Again the authors focus on absorption within the cloud and do not mention scattering (see comment 14). Furthermore, the author mention that the differences between net fluxes are noisier than the original flux measurements, but it is difficult to understand how this information is pertinent here since the study does not measure net fluxes (one would need to measure upward fluxes for this) and they do not have any measurements above the cloud layer.

19. page 2572, lines 6-9: It is mentioned that a future study will looked into the accuracy of the measurements. As mentioned several times above, this is the crucial point that will allow the dataset to be used with pertinence in various studies, and this will allow determining what conclusions in which domain can be drawn. For instance, in the current situation, the dataset seem to be unfit for modeling studies (see comment 11 and 12). The current manuscript should be revised only when such study will have been performed.

20. page 2572, lines 13-15: The fact that the thermopile pyranometer measurements fell within the limit of spatial variability is not a very stringent test. The bias observed in clear-sky situation is more indicative and is not mentioned in this list.

21. page 2575, lines 5-8: It is difficult to understand how the author justify that known uncertainties are not accounted for. It is a requirement (c.f. GUM) to give an uncertainty estimate when making observations and publishing an analysis of the corresponding

measurements. This is what allows deciding whether an effect shown by the analysis is significant. While it is possible to use measurements for which sources of error reported by various studies are not compensated or corrected, it is necessary to estimate how they contribute to the uncertainty, sum up the contributions of the different uncertainty sources and give an estimate of the overall uncertainty. It is also possible to say that some uncertainty sources are considered as negligible, but one should then to give arguments justifying such assumptions.

22. Figure 1: The picture in Fig. 1 shows some problematic aspects of the experimental setup. The picture shows that the mounting rod is not vertical. It should be assumed thus that the pyranometer sensor is not leveled horizontally. A difference with horizontal leveling of only a couple of degree can lead to a difference on the order of 4% in clear sky situations. In addition, the picture shows that the area is farmed with agricultural engines. This would certainly lead to additional sources of uncertainties (soiling, changes in leveling, etc.). This requires an assessment of uncertainties. The author should collect all the times at which clear sky situations were observed (not only full clear-sky days but all such daily opportunities) and use these for an uncertainty assessment for sources linked to the operation of the setup together with the assessment of "observable" factors that was made during the study.

Corrections:

1. page 2558, line 13: replace "and are often difficult" with "and it is often difficult".

2. page 2559, line 20: replace "for HOPE campaign" with "for the HOPE campaign".

3. page 2559, lines 24-25: replace "caused due to the spectral response of the photodiode" with "due to the spectral response of the photodiode".

4. page 2569, line 20: replace "Occassional decoupling" with "Occasional decoupling".

References:

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