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Atmospheric Measurement Techniques Discussions

AMTD

8, C2233–C2250, 2015

Interactive Comment

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

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Responses to anonymous referee # 2

We thank the reviewer for providing his/her valuable comments and suggestions on our article "Shortwave surface radiation budget network for observing small-scale cloud inhomogeneity fields" (amt-2014-313). In the process of revision, we have made the following changes in the original manuscript:

1. The title of the article was revised as "Shortwave surface radiation network for

observing small-scale cloud inhomogeneity fields”.

2. Section 3 (Data processing and initial quality control) from the old manuscript was revised to include the uncertainty estimation. The revised section 3 (Data processing, quality control and uncertainty estimation) excludes the statistical procedure completely and the quality control procedure was limited to observational flags. Accordingly, figure 4 and previously defined terminology for interpretation of results were removed. An uncertainty assessment of global horizontal irradiance (G) and corresponding derived global transmittance (T) measurements influenced by various operational sources was undertaken with the available observations during HOPE. These details were included in the revised manuscript as sub-section 3.3 with a more detailed report including figures as a Supplement.

3. Table 1 was updated and revised to include more information about ADC data logger and amplifier characteristics. Table 3 from old manuscript was labeled as table 4 in the revised manuscript. Uncertainty estimates of global horizontal irradiance and corresponding derived transmittance from various sources was included in Table 3 of the revised manuscript.

4. Minor corrections to figures 2 and 3 were done. Figures 5, 6 and 7 from old manuscript are renumbered as figures 4, 5 and 6 in the revised manuscript.

5. Following the revision of quality control procedure, the results and discussion section was revised accordingly retaining the same idea but with improved structuring.

Following are the responses to the referee comments (in *italicized* font):

AMTD

8, C2233–C2250, 2015

1 Specific comments

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.*

- The incoming solar radiation reaching the Earth's surface is modulated by two ways: (a) first order (or primary) interaction with clouds (water droplets and ice), aerosols (mineral dust, soot etc.), water vapor and ozone, and (b) second order (or secondary) interaction with 3D cloud effects (i.e., cloud edge scattering with enhancement in surface reaching radiation) and aerosol-indirect effects. In our statement, the first order interaction associated with clouds was referred to 'direct interaction', while the second order interaction associated with clouds (i.e., 3D cloud effects) was referred to 'indirect multiple interactions'. With a view to avoid further confusion, we have revised the phase as "modulated by direct and multiple interactions with clouds".

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

- This motivating statement was included in the revised manuscript. However, the present study will not include any details regarding area-averaging accuracy estimates required for validation studies. A detailed study focused on the multi-

AMTD
8, C2233–C2250, 2015

Interactive Comment

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Interactive Discussion

Discussion Paper

resolution analysis is ongoing and will include these estimates for different cloud regimes or sky conditions.

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".*

- The broader vision of HOPE was mentioned in the sentence. Apart from the pyranometer network, there were a suite of other instrumentation available at three distinct supersites within the HOPE domain during Jülich campaign. The nearest and farthest distances between two EKO pyranometer stations were 0.095 m and 10.94 km respectively during Jülich campaign. Another extended HOPE campaign in Melpitz, Germany was conducted with 50 pyranometer stations setup in a much higher spatial domain of 2 km × 3 km during September to October 2013 along with other collocated measurements. In this case, the closest and farthest distances between two EKO pyranometer stations were 0.095 m and 2.7 km respectively. So, combining the pyranometer network measurements from Jülich and Melpitz campaigns, we can explore the small-scale variability of shortwave global radiation measurements due to cloud inhomogeneity at different spatial and temporal resolutions that are either comparable or par better than $HD(CP)^2$ or any other existing model resolutions. To avoid further confusion, we have removed “with 1 m spatial and 1-s temporal resolution” and revised the statement as - “... was designed as part of the observation module to measure the sub-grid

AMTD

8, C2233–C2250, 2015

Interactive Comment

scale variability of dynamical, thermodynamical and cloud micro-physical properties".

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 data sheets (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 et al., 2011, Dutton and Long, 2012 or Vuilleumier et al., 2014 have shown how such uncertainties can be estimated.

- A new sub-section 3.3 (along with table 3) providing the details of uncertainty sources and corresponding estimates with different operational conditions was included in the revised manuscript. In addition, a detailed version with figures was also included in the Supplement of the revised manuscript.

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?*

- The quality flags were necessary to get a preliminary idea about whether the measurements from a particular station were influenced by soiling and/or horizontal misalignment (i.e., operational sources of uncertainty) as observed at the time of maintenance. However, it is impossible to ascertain the exact day and time when these sources influenced the respective pyranometer measurements either individually or together. Additionally, occurrence of precipitation in the period of no maintenance leads to unrealistic assignment of flags. So, keeping in view of all these aspects, we have not considered the flag information in screening the data. In the revised manuscript, we have translated the flag information into possible uncertainty estimates. The statistical approach was mainly included to identify the spurious/mal-functioning stations, if any, and is independent of observational quality flags information. Since the statistical approach involves various uncertainties, we have removed the same completely in the revised manuscript.

- A spirit (or bubble) level was available on the leveling plate, which mounts the pyranometer sensor. The screws of the leveling plate were adjusted on the mounting rod such that the bubble in the spirit level is enclosed within the marked circle (i.e., indicator for perfect horizontal alignment). This information was included in the sub-section 2.2 of revised manuscript. Figure S2 (in the Supplement) shows the picture of leveling plate adjusted with screws on the mounting rod with perfect horizontal alignment and other two possible misalignment positions.

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

- Assuming the possibility of close by structures (e.g., trees, buildings, etc.) leading

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

to obstruction of free horizon and increased effect of atmospheric refraction (and curvature effects) at higher solar zenith angles, we have included the statement as one of the motivating reasons for using atmospheric/global transmittance (T). We have removed this statement in the revised manuscript.

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.*

- We agree with the reviewer. Due to lack of information on the solar spectrum used by EKO during calibration process, we have used Gueymard (2004) extraterrestrial solar spectrum. The EKO sensor specific extraterrestrial irradiance was obtained as 619.91 Wm^{-2} when Gueymard (2004) extraterrestrial irradiance at the TOA was 1366.15 Wm^{-2}. However, we obtain the scaled EKO sensor extraterrestrial irradiance as 617.48 Wm^{-2} by considering climate significant total solar irradiance value of 1360 ± 0.5 Wm^{-2} at the TOA (Kopp and Lean, 2011).

AMTD

8, C2233–C2250, 2015

Interactive Comment

The relative deviation between the climate significant total solar irradiance at the TOA and Gueymard (2004) extraterrestrial irradiance values was obtained as 0.43%. Further, the performance of Gueymard (2004) spectral irradiance model was very high with typical differences of 1–2% when compared to other reference models (Gueymard, 2008). So, all together an expanded uncertainty of 2.43% was assumed in the spectral mismatch of extraterrestrial solar irradiance. These details were included in the sub-section 3.3 (along with Table 3) of the revised manuscript. Detailed information on uncertainty assessment was included in the Supplement.

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 measurements. 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).*

- We thank the reviewer for relevant references. In the revised manuscript, we have translated the observable flag information into possible uncertainty estimates (see section 3.3, table 3 and supplement).

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.*

- We agree with the interpretation given by the reviewer. The minimum and maximum values correspond to varying sensors and are obtained from the variable spatial distribution as time goes on. To avoid confusion over the commonly accepted terminology, we have removed the same from the revised manuscript. Instead, we use a more general measure of variability, known as 'coefficient of variation' or 'relative standard deviation' (c_v). This is defined as the ratio of standard deviation to the corresponding mean of spatial field as time goes on. We have revised the interpretations with respect to the relative standard deviation in the results and discussion (section 4).

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*

AMTD
8, C2233–C2250, 2015

or not.

- The 10% maximum error was an approximate estimate based on the manufacturer specifications (table 1). This statement was removed and a more robust analysis on uncertainty from various sources (based on the available measurements from HOPE) was included in the revised manuscript in subsection 3.3 (along with table 3 and Supplement) following the methodology in GUM.

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.*

- Due to the non-relevance of the context, the description concerning Michalsky et al. (2006) was removed in the revised manuscript.

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.

- The reviewer shall note that the measurements obtained from thermopile pyranometers (i.e., FZJ, KIT1 and KIT2) were not corrected for zero offset and are available at different resolutions. The measurements from FZJ were available as 1 minute averages while that of KIT1 and KIT2, they are available at 1 second resolution. We do agree that 5% bias includes the combination of uncertainty due to spectral mismatch involved during calibration process and directional response of EKO pyranometers. However, the zero-offset error (of thermopile pyranometers) and the spatial separation between the two measurement stations on different sky conditions lead to additional uncertainty. In the revised manuscript, we have focused on computing the uncertainty estimates basing on the measurements from our EKO 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.*

- The revised statement is - “As a result of this, the intensity of diffuse irradiance is mainly determined by the presence or absence of cloud patches in the vicinity of the sun and their optical depth”.

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.*

AMTD

8, C2233–C2250, 2015

Interactive Comment

- The first paragraph of sub-section 4.2 was revised as: “Under broken cloud conditions, the pyranometer views a portion of clear sky or even direct sunlight. As a result of this, the intensity of diffuse irradiance is mainly determined by the presence or absence of cloud patches in the vicinity of the sun and their optical depth. If the clouds are not too thick, then the diffuse irradiance from a clear sky is smaller than that of a cloudy sky. Broken clouds vary considerably in their horizontal and vertical extents. A non-homogeneous thicker cloud (or a portion of the cloud that is relatively thick) loses solar energy due to scattering at the cloud edges eventually complicating the determination of flux absorbed in a cloud layer due to horizontal leakage of photons. Consequently, both the reflection and transmission are reduced relative to a plane-parallel cloud of the same cloud thickness and micro-physics. However, under clear and partially cloudy conditions, both reflection and transmission are enhanced by the incoming photons scattered by the neighboring thicker clouds. Subsequently, the uncertainties in the input parameters required for radiative transfer calculations result in errors that are comparable or even larger than the discrepancies between observed and computed cloud absorptions.”

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.*

- We have removed the statements related to correlation and variance between mean and median of the dataset as a function of time in the revised manuscript.

AMTD
8, C2233–C2250, 2015

Further, we have revised the statements in terms of the differences between the mean and the median in the revised manuscript.

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 homogeneous 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.*

- The statement was revised as “If the median is higher than the corresponding mean, then most of the global transmittance values are observed to be higher than this mean value”.

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.

- We have revised the statement as "An overcast sky is characterized by relatively lower global irradiance than that of a clear sky situation at any wavelength". The uncertainty introduced due to the non-uniform spectral response of EKO pyranometer will be significant during clear sky conditions while it will be insignificant during overcast conditions.

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.*

- These statements were removed in the revised manuscript.

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.*

- In the revised manuscript, we have included a new sub-section 3.3 (in section 3) providing the details of uncertainty due to various sources influencing the global-horizontal irradiance and corresponding derived transmittance measurements.

20. *page 2572, lines 13-15: The fact that the thermopile pyranometer measurements*

AMTD

8, C2233–C2250, 2015

Interactive Comment

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.

- Table 4 in the revised manuscript includes the bias and root mean square error resulting from the linear regression of the time series measurements of global transmittance for different days considered in the present study.

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.*

- We thank the reviewer for the suggestion to include the uncertainty estimates. In the revised manuscript, we have included a new sub-section (in section 3) and a supplement providing the details of uncertainty due to various sources influencing the measurements.

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.

- Though the mounting rod is not vertical, the screws of the leveling plate are adjusted such that the bubble in the spirit level is enclosed within the marked circle for perfect horizontal alignment (see Figure S2 in the Supplement). In the revised manuscript, we have included a new sub-section 3.3 (in section 3) providing the details of uncertainty.

2 Corrections

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

- Corrected.

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

- Corrected.

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

- Corrected.

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

- Corrected.

AMTD
8, C2233–C2250, 2015

Please find the revised manuscript with markups to highlight the corrections we have made. The following are the conventions adopted to show the markups in the revised manuscript:

- Original text in the manuscript is retained in **black** color.
- Deleted original text is crossed and highlighted in **red** color.
- Added new text with an underline in **blue** color.

References

Gueymard, C.: The Sun's total and spectral irradiance for solar energy applications and solar radiation models, Sol. Energy, 76, 423–452, 2004.
Gueymard, C. A.: Prediction and validation of cloudless shortwave solar spectra incident on horizontal, tilted, or tracking surfaces, Sol. Energy, 82, 260–271, 10.1016/j.solener.2007.04.007, 2008.
Kopp, G. and Lean, J. L.: A new, lower value of total solar irradiance: evidence and climate significance, Geophys. Res. Lett., 38, L01706, http://dx.doi.org/10.1029/2010GL045777, 2011.

AMTD
8, C2233–C2250, 2015