

Interactive comment on “First national intercomparison of solar ultraviolet radiometers in Italy” by H. Diémoz et al.

Anonymous Referee #1

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The manuscript describes a measurement campaign of UV radiometers organised in Italy in June 2010. As stated by the authors, this is the first of a planned series of intercomparisons organised to assess the quality of the UV measurements performed in Italy. The participating institutions and the locations of their instruments come from different areas in Italy, and thus can serve as a preliminary core group for the eventual establishment of an Italian UV network. In that sense, the presented activity is an important first step towards achieving this objective.

While the activity is of high importance for the participants to this campaign, and on a larger scale to the Italian Institutions planning to become active in the monitoring of solar UV radiation, it is not obvious that the general readership of AMT is targeted by this manuscript. Indeed, no substantial conclusions are drawn from the intercompari-

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son but only results from individual instruments are shown without a critical appraisal of the observed deviations to the reference. Obviously, this document presents a very well written report and serves as an excellent internal report to the participants but the outreach to external readers is very limited.

I think it is therefore essential that the authors considerably expand the manuscript to demonstrate the scientific significance of this work to the AMT readership.

Furthermore, The analysis procedure used in this manuscript is different for particular instruments (Sections 4.1 and Sections 4.2) which renders the comparison between these subsets a questionable task. I recommend the authors to use a common analysis approach for all instruments in the campaign, even if the analysis of particular radiometers could be improved (e.g. Section 4.1).

I have some additional remarks on the manuscript which are discussed in the following comments:

- The analysis procedure described in Section 4.1 for a subset of the UV Radiometers of this comparison is very elaborate and reference is made to a procedure used in a previous campaign (COST726). A significant difference between the COST726 and the present campaign is that the former was also used to calibrate the radiometers while the one described here was limited to comparing the results from the UV radiometers applying the calibration from the home institutes. Thus I expect the analysis approach to be fundamentally different. In that sense I believe that Section 4.1 contains unnecessary relict information from the COST726 intercomparison and should be considerably simplified. Lines 12-21, including equations 2-4 are unnecessary for the analysis of the present campaign and should be omitted.

- The main benefit of the elaborate analysis procedure in Section 4.1 is related to broken cloud conditions when the solar spectrum measured by the spectroradiometer is significantly affected. In that case however, also the broadband measurements are difficult to estimate due to the questionable way of correcting for angular response

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deviations (cosine corrections). As stated in 5.4, only a clear sky cosine correction was applied which is simplistic and in marked contrast to the elaborate comparison methodology. It would be interesting to determine the effect from that simplification on the performance of the radiometers during broken cloud conditions.

- line 1, page 7 I do not understand this sentence (How can the integrated clear irradiance I0BB (please define it in the text) NOT change during a spectral scan? What is meant by appreciably? Please quantify?

- Section 4.2. The use of cubic splines is very handy, but it can produce nasty surprises when applied automatically. Furthermore, how do you know that the radiation changes like a cubic spline in between the missing measurements? I would recommend the use of a simple linear interpolation.

- Section 5.1, lines 11,12: Please provide a description of how the radiometers were calibrated relative to the Bentham, or a reference where that method is described. How does this "exercise" provide information on the reliability of the angular correction?

- Page 8, line 24. Was that statement verified by measuring the angular response of these radiometers? As far as I know this information is not supplied by the manufacturers.

- page 9, line 16. What does mean "the full range of ratios increases" ? Could you try to rewrite this sentence?

- The acronyms used in this manuscript are not consistently used throughout the manuscript and should be spelled out at first use: Examples are : ARPA, IBIMET, APPA, PMOD (identical to PMOD-WRC?), QASUME.

The list of references are extensive (maybe too much for the type of manuscript). I think some redundant references could be omitted if the original citation is kept. Reference on page 11, line 30 seems incomplete. Is there a web-link?

Table 1 gives a good overview of the participants and their instruments. I would use
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that table to resolve the issue on the acronyms mentioned previously.

I am uncomfortable with the content of the reference scale column, as I wonder how a specific instrument can represent a reference scale. I would recommend stating the Institute or laboratory to which the measurements are traceable to and modify the title from "reference scale" to "Traceability"

tables 2 and 3 should state clearly the use of expanded or simple uncertainties (coverage interval etc...).

Was the linear drift of the responsivity of the reference spectroradiometer (see Figure 1) of 1% taken into account in that uncertainty estimate?

Table 3: Intuitively, I would expect the wavelength uncertainty to increase with increasing SZA. This does not seem to be the case for the range 310-400 (last line of the table). Can the authors confirm these values?

table 4: The radiative transfer model is used not only for the analysis, but also as an independent radiometer. Thus the use of particular parameters is worthwhile to be discussed. Indeed, can the authors comment on the following aspects:

a) Please state the reason for reducing the default SSA by 10% . Is there an independent reason for that change from the default values?

b) beta is defined at 1000 nm, while the aod from the Brewer is obtained at 320 nm (Section 2.2). How was it converted?

c) There is an inconsistency in the use of the total ozone from the brewer and its use in the RT model because the Brewer retrieves the ozone using the Paur & Bass x-sections while the model uses the Molina&Molina X-sections. It would be interesting to see how much the model output changes if PB x-sections are used.

Figure 2: There are substantial obstructions compared to a clear horizon. While this will not significantly affect the instruments since they all have more or less the same

angular response (did you check this assumption?) in contrast the RT Model computes its irradiance for an unobstructed horizon. Did you apply corrections to the RT model to take the true horizon into account?

Figure 3: I would not call the periods before sunrise and after sunset as missing data. If really necessary, a possible quantification could be the total number of possible measurements versus the actual measurements).

Figures 5,6,9, 10 would benefit from thicker lines. In the caption of figure 5, there is only one line inside the box (no plural).

Figure 9: Typo, the id should 14, not 04.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 2789, 2011.