Reply to comments by Anonymous Referee #2

on the manuscript "Quality assessment of solar UV irradiance measured with array spectroradiometers" by Egli et al., submitted to *Atmospheric Measurement Techniques*.

Thank you very much for your beneficial assessment of our manuscript and detailed and helpful comments. We have carefully considered each of your general and specific comments. The original comments are copied below (in italic font), followed by our specific responses (in regular font). The changed new text is written between quotation marks and reference is given to respective changes in the revised manuscript. We are convinced that thanks to the reviews the manuscript is improved and we believe that it is acceptable for publication in *Atmospheric Measurement Techniques*.

Thanks for your efforts, with Best Regards,

Luca Egli and Co-Authors

1) In this paper, an extensive evaluation of a several spectroradiometers has been carried out. Intercomparisons of this type are very useful in describing the relative and overall performance of such instruments if data are handled in a 'blind' fashion and over a wide variety of atmospheric conditions. Using the data taken at the intercomparison, it is clear that the several instrument performed adequately with respect to the reference. Nevertheless, poor stray light rejection, combined with poor sensitivity and potential variable offsets make the accurate assessment of the UV data of many of the instruments very difficult. With the increasing drive for measurement devices capable of rapid time responses in atmospheric chemistry, the use of spectroradiometers like the single monochromator instruments described here is likely to become much more widespread. The time resolution that these instruments offer is not usually found in scanning double monochromator instrument. Thus several of the instruments describe here could be an obvious choice for the measurement of actinic flux and/or UV measurement in the field. However, in order for the data to be useful and accurate with respect to measurement of flux and UV, the data presented in this paper supports other studies that show a full and extensive error correction regime must be employed to overcome the shortcomings of such instruments.

Having said that, the paper describes several important findings about the nature of these spetoradiometers and describes how to employ necessary instrument corrections. Therefore, it is worth of publication in AMTD with a necessary corrections and clarifications.

We acknowledge this favourable summary of our work.

2) I was a little puzzled by the authors desire to report the agreement between the instruments as plots of the total UV index, rather than as actinic flux action spectra. Although the goal of the paper is to ascertain the performance of the instruments with respect to UV index, the overall UV index between instruments can agree but this can mask fundamental differences in the way instruments characterize the spectra. Two instruments can seem to agree over the total UV index but one my overestimate another by 50% in one region of the actinic flux spectra and underestimate by 50% in the other. The authors should explain this potential drawback in more detail.

We acknowledge the Referee's suggestion to investigate the performance of the array spectroradiometer with respect to the actinic flux spectra. However, this suggestion addresses more the atmospheric chemistry community rather than the solar UV community focussing its work on

human health protection and UV monitoring (stated several times in the manuscript, e.g. page 13612 lines 1-25). For the UV community, UV irradiance is the measurement parameter and the UV Index is the standard parameter for the dissemination of UV exposure to the public.

For further clarification we have added an additional statement in the revised manuscript: page 16 lines 4-6 and a new citation:

"The standard parameter for disseminating UV exposure on human health is the UV index (WMO, 1998) . Therefore and as product of the spectral measurements…"

We agree that due to the potential of acquiring solar UV spectra with temporarily high resolution, these instruments may also suitable to investigate photochemical processes. We have acknowledged this potential application with references to research addressing actinic flux measurements. (e.g. Hofzumanhaus et al. 1999, Edwards and Monks 2003) and stated this in the revised manuscript (page 3, lines 4-7):

"Measurements of UV spectra in the aforementioned spectral range can also be converted to actinic flux spectra (e.g. Kazadzis et al .2002) to investigate photochemical processes in the atmosphere in terms of photolysis frequencies (e.g. Hofzumanhaus et al 1999, Edwards and Monks, 2003)."

However, to measure the actinic flux, different entrance optics is needed. The comparison with the reference instrument QASUME was designed to investigate the performance of measuring irradiance instead of actinic flux. Comparing the actinic flux would require a conversion from irradiance to actinic flux, which is not trivial and would certainly exceed the aim of the publication.

As stated in the manuscript we also agree with the Referees conclusion that the UV indices may agree but the spectral analysis shows over- or underestimates in parts of the spectrum. Therefore, we have stated on page 13632 line 13-15 that "….the main quality criteria for ASRMs is the good agreement of spectral irradiance with the reference. Then any weighted quantity will be in good agreement too, even with different weighting functions as erythema or Vitamin D formation."

Specific comments:

1) Page 13612, Line 10: "The knowledge of the entire UV spectrum from 290 to 400nm is essential to weight the measured spectrum with different action spectra" Knowledge of the actinic flux spectra gives info on atmospheric photolysis frequencies or J values. These are vital for quantifying atmospheric photochemistry and the atmospheric radical budget. Reference to this application should be included in the text.

We acknowledge again your suggestion of this further application. We revised the manuscript accordingly (page 3 lines 4-7):

"Measurements of UV spectra in the aforementioned spectral range can also be converted to actinic flux spectra (e.g. Kazadzis et al. 2002) to investigate photochemical processes in the atmosphere in terms of photolysis frequencies (e.g. Hofzumanhaus et al 1999, Edwards and Monks, 2003)" .

2) Page 13612, Line 12: "solar UV radiation at the earth surface for human health protection, accurate instruments for measuring global solar spectral UV irradiance are required." This is also needed to test radiative transfer models and the authors should acknowledge such.

We have acknowledged and referenced the atmospheric radiation transfer models in the revised manuscript (page 3 lines 9-10).
"….and to test radiative atmospheric transfer models (e.g. libRadtran, Mayer and Kylling, 2005)."

3) Page 13612, Line 15: "measuring the solar irradiance spectrum sequentially, which requires several minutes of scanning time" This statement is not true of all instruments as several authors have described instruments with scan times <1 minute over the 280-420nm range.

The sentence about the several minutes of scanning time was referred to scanning instruments (pp. 13612 line 15). The Referee might have assumed that the statement refer to array spectroradiometer.

4) Page 13613: Line 6: "caused by the sharp cut off of the solar UV spectrum as a consequence of ozone absorption". The word "stratospheric" should appear here to clarify the sentence.

For clarification we have added "stratospheric" in the revised manuscript (page 3, line 32).

5) Page 13614: Line 9: "The double monochromator setup ensures stray light reduction to cover 6 orders of magnitude of irradiance levels, while the photomultiplier is able to detect irradiance levels as low as 10-6." Was this measured experimentally? If so, results of these experiments this should be included or referenced.

During the regular characterization and calibration of the QASUME reference instrument in the Word Radiation Center laboratory, the slit function is also measured with a HeCd Laser showing that the stray light effects occur between 10^{-5} to 10^{-6} orders of magnitude. Since double monochromators are generally able to suppress straylight to this extent we do not plan to include a figure of the measured slit function and refer to Gröbner et al. 2005 (see reference list).

6) Page 13614: Line 15-19. A more detailed description of the topography of the field site would help understand the legitimacy of the data, especially at large solar zenith angles. Did the site offer a full 2pi field of view? Were the instruments mounted in an East-West plane of orientation to allow maximum capture of the solar diurnal cycle?

We have included a respective note in the revised manuscript (page 5, lines 9-12):

"The PMOD/WRC building is located in the valley bottom of Davos-Dorf surrounded by mountain ridges, causing a mean shading of diffuse radiation of 5.4%. The setup of the entrance optics on the roof allowed capturing direct radiation for solar zenith angles at least smaller than 70°."

7) Page 13621, Line 11: "No stray light correction was applied to EKB" As the authors themselves indicate, together with a wealth of literature data describing ASTM type instruments, stray light (SL) evaluation and correction is critical in assessing the accuracy of any spectroradiometers. If this instrument was not corrected for SL then it seems obvious that this instrument will not achieve the accuracy of measurement sought in this intercomparison. Aside from the desire to further the large body of evidence as to the need to correct for SL, I do not see much point in considering the data produced by this instrument as part of the intercomparison.

We agree with the Referee's comment that the EKB instrument, without any stray-light correction applied should perform less than other instruments. However, we accepted all instruments as prepared by the end-users to assess their performance even if no stray light correction was applied. Since also other instruments participated without a stray light correction, we would not like to exclude EKB in particular.

8) Page 13621, Line 14: The authors mention the blind nature of the data acquisition. This is a crucial feature of any true intercomparison exercise and this point should be made earlier, preferably in the introduction and/or abstract.

We have stated the blind nature of the data acquisition and comparison in the abstract and in the introduction of the revised manuscript (page 2, line 12 & page 4, line 19-21).

9) Page 13623, Line 14: "overcast or cloudy skies are less critical than for a spectral analysis" I would disagree with this statement as the effect of clouds and aerosol's in their ability scatter different wavelength's by different amounts can reveal important bias owing to wavelength dependent effects that may not well characterized by some instruments.

We clarify that clear sky conditions are important for analysis of potential cosine errors of the entrance optics. We have seen on a clear sky day that the effect of the cosine response is minor compared to the overall performance. We agree that overcast sky impact also the performance of the instruments. Therefore, we have included also overcast days for the intercomparison. With respect to the comment of Referee #1, we have changed the text in the revised manuscript (page 12, lines 24-29):

"For the analysis of the cosine response of the entrance optics clear skies from sunrise to sunset as for day 197 are needed. The analysis of day 197 regarding cosine response showed that the effect of the cosine response is minor compared to the overall quality of the measurements. Therefore, and also to assess the performance of the instruments including effects of scattering of clouds and to analyze as much data as possible, also overcast days are included in the study."

10) Page 13626, Line 7: "This instrument shows an average ratio close to 1, indicating a good absolute calibration." I would be careful with this statement as, indeed, one would hope that this sentence to be true but in fact, good agreement, does not necessarily mean good calibration in all cases.

Most likely the good agreement is based on a good calibration, but we agree that this has not necessarily to be true. To be careful with such statements we have left this sentence out of the revised manuscript.

11) Page 13626, Line 23: "Surprisingly the ratio decreases between 310 and 320nm with a rapid increase below 310 nm" I would not say that these data were surprising, I would say the data were originally quiet poor in terms of agreement, but got closer, (probably due to SL counts) so, not really better, just closer due to this artifact.

Indeed "surprisingly" is a bit disproportionate, since most likely the increase below 310 nm is caused by stray-light contribution. We have removed the word "surprisingly" in the revised manuscript.

12) Page 13629, Line 23: "single spectra deviate more than 5% from the mean value at large SZA (Fig. 5)." A clarification to the end of the sentence "where SL is more of a factor" should be added.

We have added the sentence for clarification in the revised manuscript (page 17, lines 31- page 18 line 1).
"…, where stray light contribution plays a major role in the wavelength region below 310 nm."

13) Page 13630, Line 15: "This indicates that the overestimation in the UV-B part compensates the under estimation of the UV-A part resulting in a reasonable agreement for UV index" This is the crux of the problem with using total UV and not actinic flux spectra in such intercomparisons as described in my overall comments above.

We have addressed this issue in general comment number 2.

14) Page 1360, Line 19, "However, a large deviation from the mean value (Fig. 5) may be attributed to the calibration which cannot account for the difference in atmospheric conditions". A light source based calibration round robin would at a later date be useful in similar exercise in the future.

Many thanks for this suggestion. We additionally have clarified this sentence according to the comment of Referee #1 in the revised manuscript (page 18, lines 26-27):

"However, a large deviation of the single UV index estimates from the mean value (Figure 5) is found for this instrument. This may indicate that the compensation effect may not account for the different atmospheric conditions."

15) Page 1360, Line 29: "Therefore, we may conclude that a stray light reduction using filters improves the measurements of ASRM, at least, for the determination of the UV index" Not really remarkable, just shows excellent SL rejection, a crucial factor that this paper and other authors have suggested (e.g. Edwards et al. 2003) must be very well quantified.

This part should state that straylight can be rejected using filters as well as using correction methods. We have revised the manuscript (page 19, lines 7-9) in this manner and also acknowledged the work done by Edwards et al. 2003.

"…improves the measurements of ASRM as well as stray light correction, at least, for the determination of the UV index. This finding highlights again the importance of stray-light rejection as stated in earlier publications (e.g. Edwards et al. 2003)."

16) Page 1361, Line 19: "Therefore it may be necessary to add another layer of post processing where the cut-off wavelength in the UV-B range" This is the key take home message here. This should probably be done as a routine for all single mono instruments and the authors should make this clear in the discussion.

Indeed, the post processing of the data is crucial to improve products such as the UV index (or potentially actinic flux) derived from the spectral data. The post processing may be optimized for specific applications maybe also including radiative transfer models simulating the ozone absorption effect etc. However, the aim of the study is to focus on the quality assessment of the measurements (in particular the spectral analysis) rather than improving the derived products or applications.
We have added a respective sentence in the revised manuscript (page 20, line 1 to 8).

"It should be highlighted, that sophisticated post processing methods, e.g. including radiative transfer models simulating the noisy spectrum at lower wavelength may significantly improve products derived from the spectrum, such as e.g. the UV-Index. However the aim of the study is to investigate the quality of the raw spectral measurements rather than improving the applications."

17) Page 1362, Line 14. Comma missing after "Therefore"

The comma was inserted in the revised manuscript.

18) References: A large intercomparison of several spectroradiometers was done in the late 1990s and is described in Calvert et al. 2003. Many of the conclusions of that study mirror those if this paper and hence, these previous findings should be extensively referenced.

Thanks for this suggestion. However, we have not found this reference in literature. We have cited the publication from the SUSPEN intercomparison of ultraviolet spectroradiometers (Bais et al. 2001).