Ship borne rotating shadow band radiometer observations for the determination of multi spectral irradiance components and direct sun products for aerosol

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Dear Editor and Reviewers,

We thank the editor and the five reviewers for the detailed reviews and thoughtful suggestions. We largely agree with the comments and have considered their concerns in the current paper revision. In the following we address the reviewer's comments point-by-point. Also the latexdiff file indicates the changes between old and the current version of the manuscript.

In order to separate the reviewer's comments and the author's response we printed the comments in black and the response in blue.
We highly appreciate the detailed comments and suggestions which helped to improve the manuscript.

Sincerely, on behalf of all authors

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Response to the comments of Referee #1:

- *The authors make the assumption that the solar spectral irradiance at the top of the atmosphere is perfectly known. This is not true and this source of error needs to be taken into account in their analysis as was the case in [Miller et al., 2004]*

- Thank you for raising this important point. In general, a priori knowledge on the TOA solar spectrum is only required using laboratory calibrations, so this also relates to your next point. We have now implemented the uncertainty estimate of the NewGuey2003 spectra in our processing. This had a large effect on the AOD uncertainty estimate. To section 4.1.2 we have added the following text:

> The optical depth calculated from Eq. (1) can only be as certain as the TOA irradiance I_0 is known. In our processing the extraterrestrial spectrum "NewGuey2003" [Gueymard, 2004] is used. The uncertainty estimate from [Gueymard, 2004] range from 3.5% in the 280-400 nm band to 0.8% in the 700-1000 nm band. The uncertainty related to each channel of the GUVis is propagated through our processing causing a mean uncertainty of AOD of 0.008 for the 510 nm channel. Absolute mean values for this uncertainty for all channels are presented in table 2.

- *Furthermore this new device needs a Langley calibration*

- We strongly agree, a Langley calibration done at a high-altitude site is clearly beneficial to further reduce the calibration uncertainty of the instrument. We therefore plan to carry out such a calibration in future work. After the second laboratory calibration by the manufacturer, a Langley calibration was performed to verify the findings of the lamp-calibration. The agreement between both calibrations was within ±5%, but the Langley calibration was performed on sea level which causes some uncertainties due to the expected changes in aerosol properties over the course of a day. The good agreement convinces us to trust the laboratory measurements for this work. We added the following text in section 4.1.2:

  > Additionally, a Langley calibration was performed on clear days at sea level in San Diego after the recalibration to verify the calibration from the laboratory. Solar measurements for Langley calibrations from sea level causes uncertainties due to fast changing conditions in the boundary layer, also the extraterrestrial spectrum is not known to be better than 3.5% for wavelengths below 400 nm and 0.8% above [Gueymard, 2004]. For channels with hard-coat filters and wavelengths of up to 875 nm, differences between lamp-based and Langley calibrations differed between 0 and 5%. For channels with wavelengths between 1020 and 1640 nm the difference was 5 to 6%. Considering that the Langley calibration was performed at sea level under far from ideal conditions, the agreement can be considered good. Therefore, a Langley calibration on a high-altitude site for this instrument is mandatory and will be done in future. This will decrease the calibration uncertainties to about 1% [Schmid and Wehrli, 1995]. The drift of the spectral filters will be investigated with ongoing laboratory calibrations in future.

- *I strongly disliked figure 12. I recommend keeping the land based measurements in one part of the figure and the marine observations in another. I would also include designated regions for different aerosol types as was done. In section A of the figure plot the Cimel and GUVis data and in section B plot the Microtops and GUVis data.*

- We agree with the referee that figure 12 is confusing and not clear to a reader. We have modified the figure to separately show land-side and shipborne observations in two panels allowing a direct comparison as suggested by the referee. We have also removed the colouring of data according to date to further increase the clarity of the figure further.

- *I found a discrepancy between the text and figure for figure 11.* "Figure 11 shows the daily mean values of AOD obtained from the Microtops and GUVis measurements during the whole cruise. Shown also is the uncertainty estimate as described in Sect.4.1." *I don't believe the uncertainty is shown. I would like to see the figure redrawn. I think it would be better without the lines and with points with errorbars for the GUVis data that can be clearly seen. A separate portion should be used to show the difference between Microtops and GUVis mean observations.*

- We agree with the referee, and have modified the figure to increase its clarity. We have removed the lines and have shifted the Microtops observations to the right. We also removed two channels from the daily mean AOD comparison. All matching channels are shown in the new second panel, which shows the difference of Microtops and GUVis daily mean AOD. The uncertainty is now shown with errorbars.

Response to the comments of Referee #2:

- *In Page 18, Line 6 you said that* ".. causing negligible calibration uncertainty" *and in the page 15, line 24 you said that* "... the calibration uncertainty is the dominating contribution ...". *So the dominating type of uncertainty is negligible? Is the accuracy indeed so perfect?*

\- In Page 18 Line 17 we wanted to state that the calibration uncertainty cancels out for the direct/diffuse ratio of irradiances. We have now hopefully clarified this point. In contrast to that, Page 15 line 24 refers to the contribution of the calibration to the estimated uncertainty of the observed irradiance, which can't be neglected, as it is unfortunately not negligible.

- *You also compared to AERONET and said* "that both random and systematic uncertainties are lower for the Cimel observations .." *than for GUVis-3511. AERONET AOD has a typical uncertainty of 0.01–0.02 [Eck et al., 1999] and you give an estimate of 0.0032 at 510nm. I understand that you give a relative uncertainty and AERONET uncertainty is an absolute estimate, still it seemed that there was a some sort of inconsistency or contradiction here and your uncertainty estimate "too good to be true"?*

\- The presentation of the AOD uncertainty in the original paper is confusing. This uncertainty was calculated only considering the estimation of AOD from a known total atmospheric optical depth, thus due to uncertainties in the estimation of gas-OD (H20,O3...) and Rayleigh scattering. The propagation of the calibration uncertainty and other uncertainties of the observed irradiance was neglected. This fact was not clearly highlighted in the manuscript, and we have now chosen to present the total AOD uncertainty. We have updated the values for the uncertainties in table 1 and the text accordingly.

- *The discussion about uncertainties should be more thorough and clear.*

\- We hope to have clarified the discussion on uncertainties in our revised manuscript.

- *You did not explicitly discuss the fraction of aureole irradiance that gets blocked as well or is it so that your 2.5cm band does not introduce a significant blocking angle?*

\- Thank you for rasing this important and interesting aspect. The shadow band is actually relatively broad compared to the MFRSR or FRSR. It blocks an solid angle of 15° in zenith position, where it is closest to the sensor. We have done some radiative transfer simulations where we have modelled the observations during a sweep with our algorithm, and have compared the results to modelled values of diffuse and direct irradiance. These simulations show that the uncertainty is lower than 1% for most cases and a sun higher than 30° elevation for standard aerosol types. We thus have decided to keep this value in our study. It is however well known that the aureole/circumsolar radiation is important for strong forwardscattering/large aerosol particles or ice crystals [Nakajima et al., 1983]. We mentioned the blocking angle in the instrument description:

> Due to its geometry, the shadowband occults a solid angle of 15° of the sky from the sensor in zenith position. The width of the BioSHADE shadowband is broader compared to the MFRSR (3.3°,[Harrison et al., 1994]) and the TCRSR (2° and 5°, [Bartholomew et al., 2011]) and therefore not feasible to measure the shape of the solar aureole for thin-cloud retrievals [Min and Duan, 2005]. The uncertainty arising from the shadowband width on the calculation of the direct horizontal irradiance is discussed in Sect.3.2 and Sect.6.

We have added a brief discussion on this point in section 3.2.:

> With lower sun and increased AOD load, the sweep minimum becomes less pronounced and it is more challenging to identify the shadow of the band on the sensor. Also the uncertainty of the occulted diffuse irradiance calculated by extrapolation (blue line in Fig. 2) depends on the shape of the solar aureole and varies with aerosol type [Grassl, 1971]. The accuracy of extrapolations for different aerosol types and low sun has to be investigated in further work. So far first radiative transfer calculations for different aerosol conditions and variable solar zenith angles show, that the uncertainty from this extrapolation is around 1% for most conditions with the sun elevated more than 30° above the horizon. This uncertainty may increase when the aerosol has strong forward scattering (eg. desert dust). Nevertheless, a uncertainty of 1% agrees with the estimation of the "edge-shadow voltage uncertainty" for less variable sweeps observed by [Miller et al., 2004]. At this stage, we do not use observations with the sun close to the horizon (solar zenith angle $> 70°$).

And section 6.:

> The accuracy of the calculation of the direct irradiance from the sweep data using extrapolation to estimate the blocked diffuse irradiance by the shadowband (see Sect. 3.2) is still an open question. The extrapolation is done with a linear regression in the current processing algorithm and the uncertainty is assumed to be about 1% for data measured when the sun is higher than 30° elevation. Since the blocked diffuse irradiance contains the aureole of the sun the uncertainty of this linear regression depends on the shape of the circum solar radiation which in turn depends on aerosol type [Grassl, 1971]. Therefore we expect the uncertainty to be higher for strongly forward scattering aerosol like desert dust, especial because we are using a broad shadowband which occults up to 15° of the sky. Also the occulting time of the sensor changes slightly with relative azimuth position of the sun to the radiometer. This may also affect the extrapolation of the blocked diffuse irradiance. In the future we going to investigate the uncertainty of the estimated blocked diffuse irradiance in more detail, especially determining the effect of different aerosol types and azimuth dependence.

- *Page 2, line 18:* "... measurements of aerosol optical properties and radiative fluxes ..." *Confusing sentence, when the instrument measures radiative fluxes only and aerosol optical properties are derived.*

\- We agree with the referee, and have changed the sentence

> "*The simultaneous measurements with the shadow band radiometer of aerosol optical properties and radiative fluxes avoids inconsistencies in calibration which are unavoidable if multiple detectors are used.*"

to follows:

> "*The simultaneous measurement of spectral irradiance components with a single radiometer avoids inconsistencies in calibration which are unavoidable if multiple radiometers are used.*"

- *Page 5, line 16: Acronym OD, optical depth, was never introduced.*

- done

- *This is likely not to be included in the scientific paper, but for my own curiosity: what is the price of this instrument?*

- It is significantly more exensive than a standard MFRSR. The pricing also depends on the configuration of the instrument, specifically the choice of filters for the channels. While a set of standard channels using hard-coated filters is available, using hard-coated filters for custom channel choices strongly increase the price, or soft-coated filters have to be used. Please contact Biospherical Instruments Inc. for a quote about the pricing of the instrument and individual setup. `http://www.biospherical.com/index.php?option=com_flexicontact&Itemid=100`

Response to the comments of Referee #3:

- *a. In general, the paper is (in parts) well readable, and the algorithm steps and errors are described in detail. However, the structure is often confusing and not logical. Different topics are mixed in one section. Reordering of sections and figures is needed. The overall presentation should be clearly improved. See specific comments below.*

- Thank you for your comment. We have now reordered some sections, redrawn some of the figures and revised some parts of the text to improve the overall presentation. Replies to your specific suggestions are given below.

  1. *The title should include the terms "algorithm" and "error analysis". Please replace the vague term "direct sun products" by AOD.*

     - We rephrase the title of the paper, due to your suggestions to.:

       Algorithms and uncertainties for the determination of multispectral irradiance components and aerosol optical depth from a shipborne rotating shadowband radiometer

  2. *Abstract: define CI. This acronym is used throughout the paper without any explanation. Please replace where possible by a clear term.*

     - done

  3. *Throughout the paper errors are given as percentages with 2 decimals. This suggests an accuracy that is not attainable, as is shown by the results. For example, the abstract mentions 4.24 % total uncertainty. See also Table 1. Please reduce the number of decimals to 1 or 0.*

     - done

  4. *How to calibrate in the field the fast degrading 750 nm and 1550 nm channels? Could you use a fixed relation between stable and unstable channels for specific scenes?*

     - For the 750nm channel, we can use the Angstroem exponent to derive the AOD at this wavelength, and compare this value to the observed AOD. For the 1550nm channel, this is more challenging (but could maybe still be done considering the 1.2 and 1.6 micron channels), as the Angstroem behaviour is expected to be robust only for shorter wavelengths, and relative uncertainties are higher due to the overall smaller absolute values of AOD. We also think that frequent laboratory calibrations before/after field campaigns can help to quantify the degradation, and plan to carry out Langley calibrations at a mountain site at longer intervals. We have added a new section (5.3) about the spectral consistency of AOD observations.

5. *Sect. 3 contains three topics: the correction steps, the AOD retrieval algorithm, and the error analysis. Please separate these three topics in in three sections.*

  - done

6. *Equation 1: define τ. In general: define all symbols directly when they are used.*

  - done

7. *Equation 1: it is strange that you define R_E as a ratio of distances and not as a distance. Please use a more appropriate symbol.*

  - R_E is indeed a distance given in astronomical units.

8. *Add directly below Equation 1 in an equation that you assume m = 1/mu0.*

  - done

9. *P. 5, l. 21:* "sun below a zenith angle of 70 deg" *: this is unclear, please rephrase (occurs more often)*

  - We agree, it leads to confusion if we write lower sun while solar zenith angles increase. We rephrase this sentence to: "*...with the sun close to the horizon (solar zenith angle* $> 70°$*)*"

10. *P. 5, l. 28: first step: how are the steps numbered?*

  - The steps of processing are not numbered in this sense. We changed this term to "*...before the actual processing.*".

11. *Equations 3-5: How are these factors C1, C2, C3 used in your algorithm? What is the correction formula?*

  - C1 is the correction factor assuming only direct irradiance, C2 and C3 improve C1 to account also for diffuse irradiance. C2 (with isotropic diffuse irradiance) is shown only to demonstrate difference to C3 (with Rayleigh scattering) in Fig.3. C3 is the most realistic case and therefore used for correction in our processing. We make small changes in the text and add the correction equation to clarify on that in Sect. 3.1.

12. *P. 6, l. 18, l. 24: The deviation of what from what?*

  - done

13. *P. 6, l. 18: lower wavelengths > smaller wavelengths*

  - done

14. *P. 7, l. 12-13: repetition of text. Refer to the above subsection.*

  - done

15. *P. 7, l. 21: Please give the resulting error that follows from Fig. 4.*

  - We have added to Sect.4.1.1.:

    > At the recent *Polarstern* cruise PS83 the swell conditions were calm for the most time (see Fig. 8), which is defined as misalignment of the ship smaller than 5°. The mean uncertainty contribution of the motion correction to the irradiance measurements from this cruise was about 0.3%

16. *P. 8, l. 28: ... is calculated as the difference between the global and the direct irradiance.*

  - done

17. *P. 9, l. 20: please start a new subsection here on H2O channel calibration.*

- done

18. *Equation 10: please remove the superfluent term X*
    - done
19. *P. 9, l. 27: transmittance from Rayleigh ... > extinction by Rayleigh ...*
    - done
20. *P. 10, l. 1-14: this part is very unclear. Please remove if possible, since the use of this channel is so debatable. Basically: what is the use of the GUVIs 940 nm channel which drifting so much that you need an alongside Aeronet measurement?*
    - We agree that at this stage, the 940 nm channel has large uncertainties for estimating water vapor OD due to the temporal instability of the filter. Nevertheless, we think it is important to be able to determine the water vapor OD/column to correct for water vapor absorption at the longer wavelength channels (1.0, 1.2, 1.5 and 1.6 micron), and have therefore chosen to keep and clarify this section. We also think that the channels do not change fast enough that collocated Aeronet measurements are always required. Specifically, operating the instrument before and after ship cruises at our institute/Aeronet station in Leipzig seems sufficient to account for the degradation. In addition, the degradation likely does not alter the spectral response of this channel, so the change in filter transmission could be determined during a lab calibration.
21. *P. 10, l. 15 – l. 27: please move this part up, above the H2O discussion, since Rayleigh and ozone-NO2 correction is much more important than H2O correction.*
    - The method and uncertainty estimation have been separated in the revised manuscript.
22. *P. 10. L. 16: uncertainty > absolute uncertainty* Δ
    - done
23. *P. 10., l. 20: variate > varying*
    - done
24. *P. 10, l. 25: please give recent references on the accuracy of the current OMI product versions.*
    - Additional to the OMI theoretical basis documents we cite [McPeters et al., 2008, Bucsela et al., 2013].
25. *P. 11, l. 4:* $\tau_w > \Delta\tau_w$
    - done
26. *P. 10: Please summarize all OD errors from Sect. 3 in a Table.*
    - We have added table 2.
27. *Sect. 4 contains four different topics in one section: a theoretical uncertainty estimate in Sect. 4.1, two field experiments - one on land and one on ocean - and a discussion. Please detach these parts. Sect. 4.1 clearly belongs to the last part of Sec. 3, the theoretical error estimate. The two fields experiments, showing the real errors, are different in content, plots etc., and could be separated. The ship based measurements show the realistic capability of the instrument. The discussion in Sect. 4.4 deserves a separate discussion section in which the theoretical errors should be confronted with the real errors.*
    - The sections have now been reordered. Also the uncertainty estimation has been moved to a separate section.

28. *Please show also the differences in AOD between GUVis and Microtops in Fig. 11.*
  - done
29. *P. 12, sect. 4.1: Number the equations on l. 6 and l. 19.*
  - done
30. *P. 12, l. 6: What is DNI_{noi} ? Same as DNI_{hf}?*
  - Yes, now both terms are renamed with *af* for amplifier noise.
31. *P. 12, sect. 4.1: Please do not use acronyms in equations but symbols. So please use I instead of DNI in the equation on l. 6 and eq. (15). Please use τ instead of AOD and OD in Eq. on l. 19.*
  - done
32. *P. 13, l. 4: What are T_G and T_C?*
  - Sorry that I forgot to insert brackets. T_G and T_C are defined in this sentence as the observed spectral direct beam transmittance of the GUVis and Cimel instruments, respectively.
33. *P.13, l. 4-9: this paragraph is unclear, please rephrase.*
  - We have rephrased this paragraph to:

    A comparison of GUVis and Cimel observed spectral direct beam transmittance (T) and AOD is shown in Fig. 6 for three matching channels of both instruments. This comparison was extended for all matching channels and the corresponding regression parameters for T are listed in Table 3. We have decided to compare the transmittance rather than AOD in Table 3, because this quantity is more directly related to the instrumental measurements. Specifically, the non-linearity introduced by the Beer-Lambert law and processing uncertainties in Rayleigh scattering and gas absorption are avoided.
34. *P. 13, l. 33: only small...: the difference for ozone is very large.*
  - Yes, you are right. The difference of the OD for ozone in the 340nm channel is very large. This difference can not be explained only with a difference in the ozone columnar number concentration. For this comparison, all optical depths are calculated for the Cimel centroid wavelengths, which is 341.5nm for the 340nm channel. The Figure 1 show the calculated ozone optical depth for a columnar number concentrations of 360 DU (Cimel) and 370 DU (GUVis), observed during Melpitz-Campaign. The green line in this figure is smoothed with the response function of the 340nm channel, like it is done for the ozone OD calculation in the GUVis processing. Therefore, the large difference of ozone OD in the 340nm channel is caused by the high variability of the spectral ozone OD around 340nm, like it is shown by the difference of the green and the blue line in figure 1 at 341.5nm. We have replaced the very general "only small" description and add a brief description of this issue.
35. *P. 14, l. 4: what is E_T? it is not used in any equation.*
  - It is ΔT. Sorry, this used to be an artefact of a former draft version.
36. *P. 14, Eqs. 17 and 18: please give these equations earlier, in sect. 4.1, as part of (new) sect. 3 error analysis.*
  - done
37. *P. 15, l. 17-21: please give a quantitative result of the real GUVis AOD error on the ship, from comparison with the Microtops.*

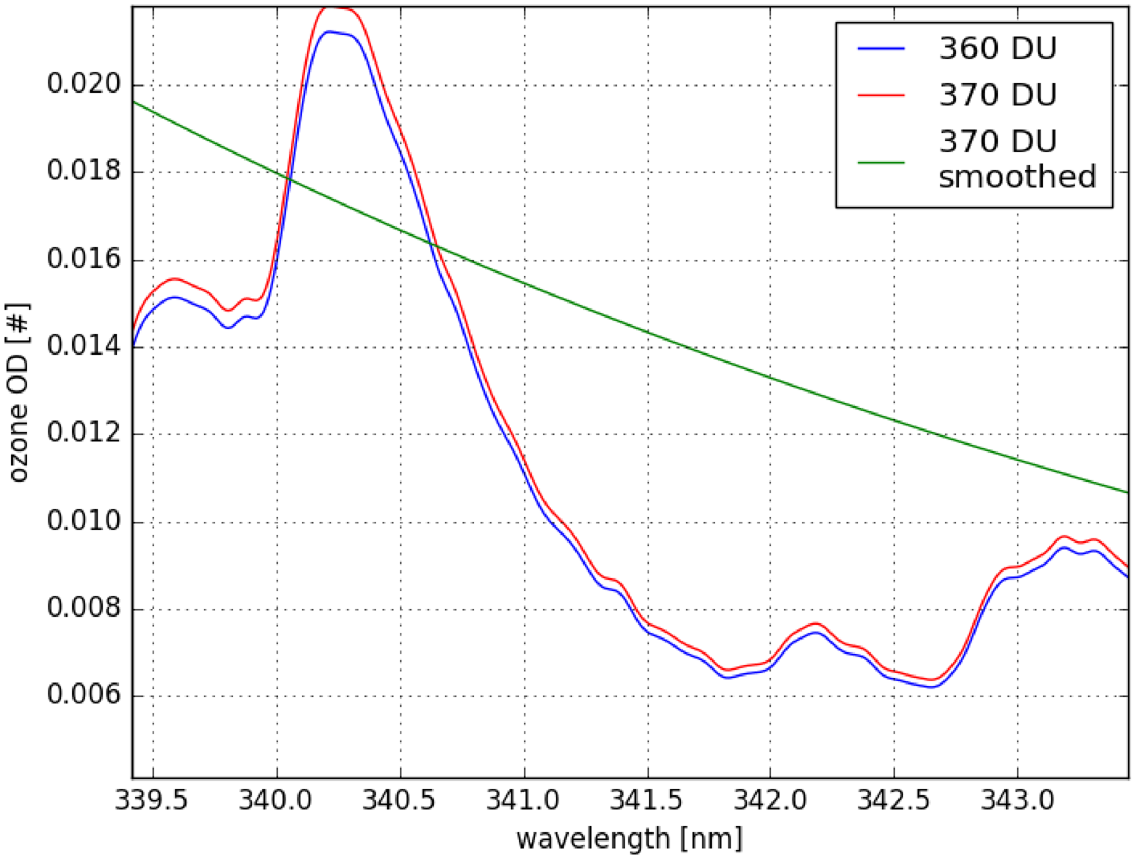

Figure 1: O3-OD for different Ozone columnar number concentrations. The green line is smoothed with the response function of the 340nm channel of the GUVis.

- The deviation of the AOD observations of both instruments is now also shown in Figure 10. We add some sentences about it in the text.

38. *P. 17, l. 11 ff: Does the percentage error mentioned here relate to AOD? Percentage errors are not very useful for AOD, since the AOD is very variable. Only absolute errors are useful, which can also be seen from Eq. 1, which is the relationship between AOD and transmittance. This point holds for the entire AOD error discussion.*

    - Now all values for AOD uncertainties are given in absolute values.

39. *P. 18, l. 4: remove: and radiative effects (since this is not shown).*

    - done

40. *P. 18, l. 13: if only > but only*

    - done

41. *P. 18, l. 21-25: For these applications of this instrument, it should be demonstrated that the other wavelengths of the GUVis, for which no results were shown in this paper, are indeed functioning as required.*

    - We plan to implement algorithms for these applications in the future. At this stage, however, it seems difficult to know what accuracies are required for them, so we strongly believe this is out of scope for the present paper. Relying on the Angstrom relation however, we demonstrate in the revised manuscript that also the channels not covered by the Aeronet photometers work reliably. Please have a look in the new section 5.3.

42. *Table 1: Please clarify caption and header. Caption: please always indicate the number of the column. Header: Deviation of what? Uncertainty in what? What do slope, σ and R mean? Comparison to Cimel = Land? Comparison to Microtops = Ocean? Aerosol > AOD ?*

- done

43. *Please add a table (or a column in Table 1) with the spectral bandwidth and central wavelength of each channel. For which wavelength was the OD calculated?*

- We have added table 1 which shows the centroid wavelength and bandwidth of each channel.

44. *What is the shape of the spectral response functions of the 19 channels?*

- Except for the unfiltered channel, the response function is almost rectangular. Figure 2 show the spectral response of all channels.

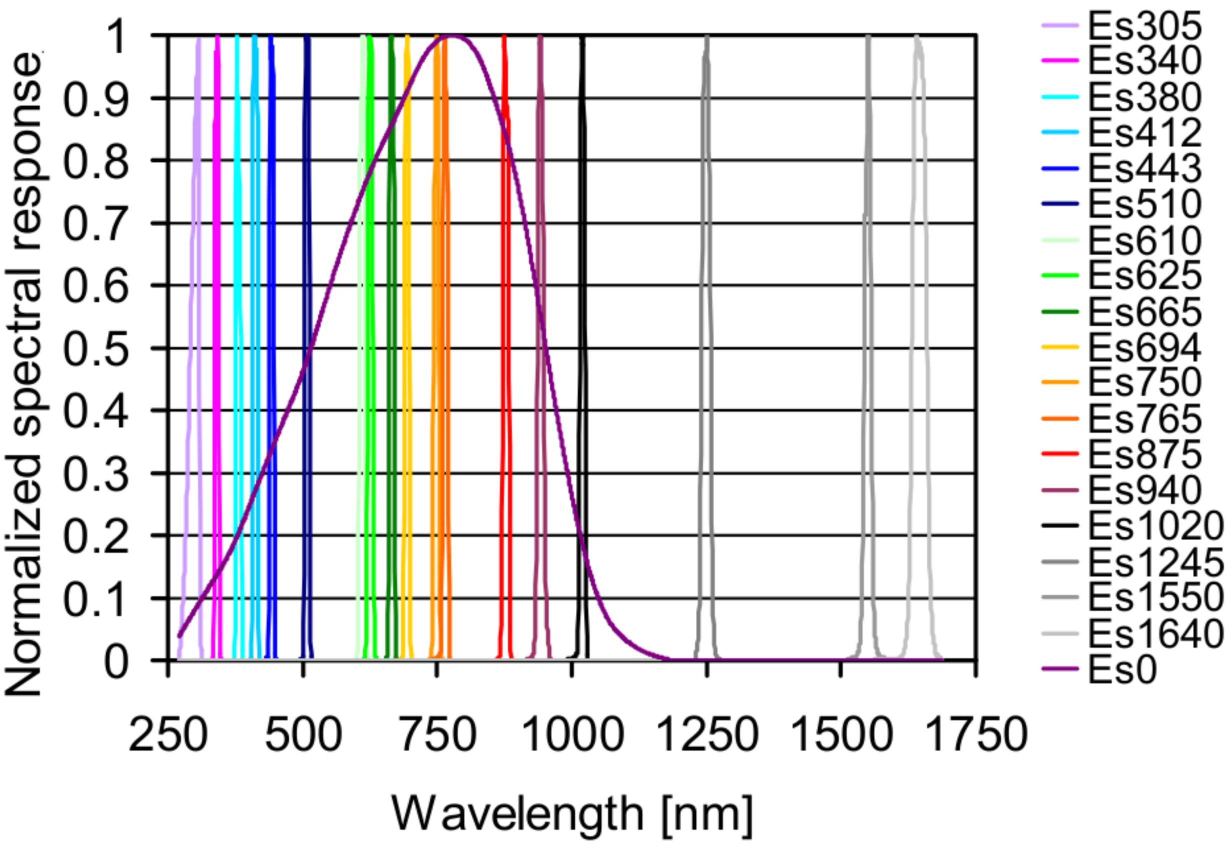

Figure 2: Response functions of all filters of the GUVis radiometer.

45. *Figure 1: please explain what is what, e.g. with arrows. It would be helpful to have sketch of the GUVis, or a top view.*

- done

46. *Figure 2: Please number the steps of the data processing algorithm. Calibration: radiometric or spectral? Surrounding pressure> surface pressure. Concentration > column density.*

- done

47. *Figure 3: explain the two y-axes. The last lines of the caption are a repetition from the main text. Explain in the main text how C_i are used.*
    - We added some text in section 3.1 and update the figure and caption.
48. *Figure 4: Caption: Error due to aerosols ...*
    - done
49. *Figure 5: This figure should be shown earlier, because it nicely shows the principle of the shadowband measurement.*
    - The sections are reorganized. Figure 5 becomes Figure 2.
50. *Figure 6: this figure can be removed, since its content can be well described in a few words in the main text.*
    - You are right. We have removed the figure.
51. *Figure 7: please explain the symbols of the legend in the caption and identify the two equations.*
    - done
52. *Figure 8: Caption: Deviation > Difference ... Mention the Melpitz campaign and the time period.*
    - done
53. *Figure 9: Explain y axis: Difference in OD ... Please zoom-in by removing the single outlier (mention specifically) and rescaling the y-axis.*
    - done
54. *Fig. 11: Label the GUVis and Microtops points. Add error bars. Show also the AOD differences GUVis – Microtops.*
    - done
55. *Figure 12: This is a very difficult and confusing plot. There is too much information. Land and ocean data are mixed? Are there also two color codings mixed? Please make separate figures. It anyway requires more explanation in the caption. Give also the year.*
    - We agree with the reviewer. We reorganize this figure to separate land and shipborne observations and close comparison of the GUVis and sun photometer observations. The observations are no longer color coded with respect to the date to increase the clarity of the figure.
56. *Fig. 13: This figure should be should before Figure 11, of course. Please give the dates and location in the caption.*
    - done
57. *Why is the slope of the transmittances in Fig. 13 closer to 1 than the AOD? OD correction differences?*
    - Yes, the OD correction is one source which influence the slope of the regression. Also note, that the OD is calculated logarithmicly from the transmittance, so variations in low transmittance values cause a large impact on the OD values and therefore also the regression.

- *b. The theoretical AOD error estimate of Sect. 3 should be confronted with the real AOD errors of Sect. 4, preferably in a separate discussion section for land and ocean. Now the Melpitz field campaign over land gets much more attention than the Polarstern field experiment, whereas the latter is the real innovative application.*

- The discussion section is now a stand alone section.

- *c. The paper is missing a description of the advantage/purpose of measuring at 19 wavelengths. Only results for a few wavelengths are shown. Please add more spectral results to show the capability of the instrument for future applications.*

- We mention some additional planned future applications of the instrument in the introduction and outlook. We have also added an investigation of the performance of the additional channels relying on the Angstrom behavior of the AOD in the new section 5.3.

- *Technical corrections*

  – *Explain all acronyms the first time they appear: TROPOS, OCEANET, BSI, OD, . . .*

  - done

  – *Explain all symbols the first time they appear. E.g. T_G, T_C, E_T, ... are not explained.*

  - done

  – *Write much used scientific and technical terms not as separate words, but as connected words: shipborne, shadowband, multispectral, subproject, airplane, etc.*

  - done

  – *All symbols, either in text or in equations, should be in italics. For example, T and w in equation 9.*

  - done

  – *All Acronyms should be in upright font. All units and molecular formulae should be in upright font. For example, CO2 and CH4 on p. 11, l. 8-9.*

  - done

  – *Please number all subsections (with a boldface title). Now it is confusing that some are numbered and others are not.*

  - done

  – *Please remove the historical references to Beer on the extinction law and to Junge on the power law size distribution. This is now all standard textbook material.*

  - done

  – *Eq. 7 - 8: remove the unnecessary brackets around the gases in the subscripts, and remove the A, since A stands for aerosol.*

  - done

  – *Please check the plurals: This values, etc.*

  - done

Response to the comments of Referee #4:

- *There is one issue with shipborne measurements (shadowband type) that I've never heard discussed, and that is how confident one can be in identifying the precise sun-obscured moment from each shadowband pass? For clear sky conditions the exorcise is straightforward. For very overcast conditions one might follow the authors' example and only provide global data. What about everything in between? It's not hard for me to imagine sky conditions that obfuscate the actual sun-obscured moment, and lead the algorithm to an* *incorrect determination. I'll admit to never having worked with shipborne measurements, but it seems to me it would be important to develop an algorithm that compares each measurement with the preceding and subsequent data point(s) as a way to gain confidence in the exact timing of the moment when the sun is completely obscured. Such a test could be developed using clear sky data with the goal being to produce a confidence level for the timing of when the sun is blocked during each measurement set (or sweep). If this issue has been dealt with adequately in a prior paper, then please provide some text along with a reference.*

- At this stage, we deal with this challenge in the following way: One sweep of the shadow band takes about 40 seconds. The irradiance is sampled with 15Hz during the sweep. The shadow band is designed that at least five samples are taken while the sun is completely blocked. As you said, for clear sky conditions and overcast situations the processing is straight forward. If there is any cloud obscuring the sun multiple times during one sweep, the data will show multiple minima, an extended minimum or many fluctuations. In case of strong fluctuation or multiple minima, the sweep is not processed and only the global irradiance is reported. Still it is possible that processed sweeps are affected by cloud influences, especial in the presence of thin clouds. In this case, the AOD will vary more strongly compared to the clear sky case, and in turn will be filtered out by the cloud mask at the end of the processing. We have added this aspect to the "separation of irradiance" section. As we also know the geometry of the instrument, ship and the sun, one could also determine the expected position of a minimum, and use this knowledge to further improve the processing. We will likely try to implement this as a future improvement of our processing algorithms. We have added this explanation also to section 3.2

- *A shadowband instrument is presented, along with land-based data, yet there are no Langley calibrations presented. A long enough time series of Langley cals might show a temperature dependency that could be used to further improve data. (I do understand the instrument is temperature stabilized).*

- We agree that a Langley calibration on a high-altitude site is clearly needed to determine the calibration constants with high accuracy, and will be done in future work. We do hope however that dependencies on variations in the instrument and ambient temperature are small. We also want to mention that after the instrument was calibrated in the laboraty by the manufacturer, a Langley calibration was performed to verify the findings of the lamp-calibration. The agreement of both calibrations was within $\pm 5\%$, but the Langley calibration was performed on sea level which causes some uncertainties due to temporal changes of aerosol properties. The good agreement found between both calibrations does however convince us to trust the laboratory calibration. We have added some sentences about this in section 4.1.2. We have not yet investigated a possible temperature dependency and plan to do this in future work. Please see also the response to Referee#1 comments 1 and 2.

- *Uncertainties are given to two decimal places throughout the paper. One decimal at most for this work.*

- done

- *In the introduction the instrument is described as having "a constantly moving shadow band" (P3 L1). From the instrument picture (Figure 1), which, BTW, is an exceedingly poor picture, it's obvious the shadowband (one word) cannot move continuously. Later in the manuscript the shadowband motion is described as "sweeping" which sounds more* *accurate. Are measurements made in each direction, or does the shadowband always return home after a measurement set? How often are measurements taken? Is the frequency fixed or user configurable?*

- We have rephrased "a constantly moving shadowband" with ".. a sweep with constant speed". Also Figure one has been changed. The data of the sweeps are taken in both directions. One sweep lasts around 40 seconds, and is performed once per minute. In the remaining time, the global irradiance is observed. The frequency of the radiometer samples are user configurable, and can go up to 15Hz. We have also tried to clarify these aspects in the revised manuscript.

- *So in an effort to learn more about how this instrument operates I looked to the [Seckmeyer et al., 2010] reference (P3 L16) as the manuscript strongly implies it to be a description of the instrument. It's not. Is there a peer-reviewed reference that describes this instrument in detail? Preferably with the BioSHADE accessory.*

- Seckmeyer is a general paper introducing multi channel filter radiometers, a detailed description of the GUVis-3511 radiometer can be found in the brochure of the instrument (`http://www.biospherical.com/images/pdf/guvis-3511.pdf`). The BioSHADE accessory is described in [Morrow et al., 2010]

- *P3 L2 Should be channels, and "...includes all AEORNET and MFRSR channels." I would say rather it includes five channels that are very close to standard MFRSR channels and one that matches exactly (940). I cannot say if similar wording changes should made in respect to CIMELs.*

- Yes you are right, the channel centroid wavelengths do not match exactly with the channels of both Cimel and MFRSR instruments. We have thus rephrased this sentence to:

> The radiometer offers 18 narrow spectral channels ranging from 305 nm to 1640 nm and one broadband channel with a sensitive range from 400 to 1000 nm. It includes channels with a centroid wavelength close to those of the AERONET Cimel and MFRSR instruments, as well as a number of additional wavelength bands.

- *P3 L22. At this location in the manuscript are the authors asserting the 18 channels are measured simultaneously?*

- Yes, each channel has its own microradiometer and they can be sampled synchronously at rates up to 15 Hz. See the description of the instrument brochure: `http://www.biospherical.com/images/pdf/guvis-3511.pdf`

- *P4 L8. black anodized not "anodized black."*

- done

- *P4 L9. "when the band is moving" to "during a measurement sequence." This goes back to the earlier statement that the band "is constantly moving." Also "rotates". To me rotation implies 360deg. I like the use of "sweep" better as is done later in the manuscript. It better describes the movement of the shadowband.*

- done

- *P4 L10. Band can't be "stowed" if it's constantly moving.*

- Yes, the shadowband is not moving constantly in terms of a constant rotation. The sweeps are performed with a constant speed moving the band smoothly. We rephrased some parts of the text to make this clear.

- *If there isn't an authoritative article on the GUVis-3511 then this section needs significant improvement. Also, there is no mention of why the band width is 2.5 cm and the diameter 26.7 cm. I'm hopeful there is a better picture of the instrument, preferable taken from slightly above the sensor.*

- We have now changed the picture. An article describing the GUVis-3511 and the BioSHADE can be found in the instrument brochure (`http://www.biospherical.com/images/pdf/guvis-3511.pdf`) and [Morrow et al., 2010]. The width of the shadowband is determined by the diameter of the GUVis diffusers, which is slightly smaller than 2.5 cm. The band obviously needs to be wider to cast a shadow on all parts of the diffuser. If it were wider, a larger part of the sky would be blocked, which increases the uncertainty.

- *P4 L24. extent not extend*

- done

- *P4 L31. "To improve stability..." sentence is poorly worded.*

- We rephrased the sentence.

- *P5 L4. Is it possible to load one's own calibrations?*

- For the user, it is not possible to store the calibration constants in the firmware of the instrument. But the uLogger software allows to record the uncalibrated raw data and thus to apply the calibration afterwards.

- *P6 L3. airplane is one word.*

- done

- *P6 L10. These factors...*

- done

- *P7 L2-4. Are the internal measurements of "pitch and roll" applied internally or during post-processing? Are these data part of the datastream? Consider using x-axis and y-axis for land-based situations as pitch and roll are ship/aircraft terms.*

- The values of the angles from the internal accelerometer are simultaneously measured with the irradiance data. The correction of the irradiances described here is however only carried out during post-processing, and is in fact not using these internally measured values but ones obtained from the more accurate sensors of Polarstern. We prefer to keep the terms pitch, roll and yaw , because they are also commonly used describing the angles observed by accelerometers and other inertial measurement units.

- *P7 L19. Figure 4 demonstrates... I read L19 - L21 many times. I now think I understand what is being conveyed, but the passage is confusing.*

- We have now rephrased this sentence to:

Figure 5 shows the deviation of C_3 calculated with and without aerosol influence for the 305 nm and the 510 nm channels for $\Theta_A = \Theta - 6°$ (e.g., high swell). For a smaller difference between Θ and Θ_A (e.g., lower swell), the error will be reduced and turn negative when $\Theta_A > \Theta$.

- *P7 L22. From these calculations...*

\- done

- *P7 L27. In this section detecting the minimum when skies are clear, or at least the sun is not obscured, and what to do when direct irradiance is very small are both discussed. There are many situations in between these extremes that are not addressed at all. I see this as a major deficiency. If the paper were only on AOD and direct beam that's one thing, but the opening sentence of the abstract promises us "shipborne (one word) measurements of the direct, diffuse and global spectral irradiance components..."*

\- To address this issue, we have revised section 3.2. Please see also the response to your first comment, and the fourth comment of Referee#2.

- *P10 L25. uncertainties are...*

\- done

- *P11 L1. From these values...*

\- done

- *P11 L22. ...reaches up to 0.5. The ending of that sentence leaves me hanging.*

\- We have rephrased this sentence to:

For some situations, the cloud cover reaches up to 0.5, while the data is not flagged as cloudy.

- *P10 L12. ...therefore been excluded*

\- done

- *P12 L22. ..with an uncertainty*

\- done

- *P14 L15. Why should I believe the Microtops II is an instrument worthy of making a claim the GUVis compares well with? The first referenced article in this section [Macke et al., 2010] only briefly mentions the Microtops, focusing mostly on its operation. The second referenced article [Smirnov et al., 2002] doesn't reference the Microtops II in the text at all. There is nothing here to give the reader confidence the Microtops II is anything more than an instrument that provides the operator a general idea of AOD. And actually, I don't understand the Smirnov reference in the context of the text at all.*

\- We have add the reference to [Smirnov et al., 2009] for a description of MAN and the Microtops II instrument. We have left in the reference to [Smirnov et al., 2002], giving a description of the processing of the sunphotometer data.

- *P16 L24. ...the fact that the...*

\- done

- *Fig 1. How about... GUVis mounted on research vessel Polarstern during cruise PS83. A total sky imager is to the left.*

- We have replaced this figure by another one in the revised manuscript.

- *Fig 2. ...are in yellow... ... are in green...*

- done

- *Fig 3. How about... Figure shows factors for motion correction measurements of 305 nm and 510 nm GUVis channels. Existing caption is unnecessarily wordy. Why say "Additionally" when the opening sentence states this figure shows correction factors? "By adapting...into account" is superfluous.*

- We have rephrased the caption. Thank you for your suggestions.

- *Fig 6. Relationship of precipitable water obtained from CIMEL sun photometer and GUVis shadowband radiometer during Melpitz-Column experiment.*

- We have removed this figure completely since all the informations is given in the text and the suggestion of referee#3.

- *Figs 7-13. Often more text than necessary.*

- We have made some changes in the captions of the figures.

- *Fig 11. This figure doesn't present the data clearly. Consider a different approach. Maybe plotting the differences?*

- Yes, we have now modified this figure also based on the feedback of the other reviewers.

- *Fig 12. I'm not sure what is being presented?*

- Yes, this figure is confusing. We have modified this figure.

Response to the comments of Referee #5:

- *The manuscript has been carelessly prepared and that severely detracts from what should be the message of the paper. Rather than provide a detailed review, at this point, I would prefer to point out three specific examples that form the basis of my opinion that the paper needs significant revision before it should even be considered for publication.*

- We have significantly revised the original manuscript, taking into account these 3 points and the recommendations of the other reviewers. We hope that these revisions address the concerns of Referee#5, and clarified the overall message of the paper.

  - *Starting with what should be a straightforward description of the instrument, the GUVis instrument is described in the abstract as* "The 19 channel rotating shadow band radiometer..." *while the instrument description on p.3 (line 18, section 2 Instrumentation) states that* "GUVis radiometer is a multi channel filter instrument...with 18 spectral channels"

  - Yes, the radiometer indeed has 19 channels. However, narrowband spectral filters are only used for 18 out of these, while one channel is unfiltered, yielding a broadband shortwave irradiance (with the response of its silicon detector). The data from the unfiltered channel is not used in this paper. We have clarified this point in the revised manuscript.

– *In the Introduction to the paper on p.2 line 20, the authors state* "In addition, it provides direct information about radiative fluxes..." *this statement is not true. The instrument does not measure fluxes.*

- We have replaced the expression radiative fluxes with irradiance components, also for consistency in our terminology.

– *Similarly, the statement (line 25 of the Introduction p. 2)* The simultaneous measurements with the shadow band radiometer of aerosol optical properties and radiative fluxes avoids inconsistencies in calibration which are unavoidable if multiple detectors are used. Aerosol size distributions can be obtained from the spectral dependence of AOD..." *is also untrue. First, I have already objected to the term "flux" to describe the measurement. Second, while I agree that calibration would be more of an issue with multiple detectors the instrument contains multiple filters that are more of a problem for spectral calibration than multiple detectors would be. Third, the instrument uses multiple detectors: namely silicon photodiodes are used for wavelengths up to 1020 nm while indium gallium arsenide detectors are used at longer wavelengths.*

- This statement was meant to refer to fact that inconsistencies have to be taken into account if different instruments are used to measure the different irradiance components (direct, diffuse, global). We have clarified/rephrased this sentence to:

> The simultaneous measurement of spectral irradiance components with a single radiometer avoids inconsistencies in calibration which are unavoidable if multiple radiometers are used. Also the calibration uncertainty can be neglected for direct to diffuse irradiance ratio products, because both components are measured with the same sensor.

• *While revising the manuscript, the authors should consider expanding their discussion of the cosine correction. On p. 7 (lines 6-10) they state that they are using the measurements provided by the manufacturer. They should specifically examine their data for errors in this cosine characterization which should be filter dependent and therefore introduce a spectrally-dependent source of error/uncertainty which would show up in an examination of daily time series of retrieved aerosol properties and add some discussion to Section 4.4 (Discussion of the Uncertainty). Doing this would allow the authors to separate the spectral uncertainties due to errors (uncertainties) in the characterization of the filter response function and errors (uncertainties) in the characterization of the cosine response of the filters.*

- The cosine response characterization/correction is indeed done separately for all channels, and has some (likely channel-dependent) uncertainty, which will affect the diurnal cycle of retrieved aerosol properties. The remaining cosine response error is considered negligible in this paper, because only data with the sun higher than 30° elevation is used. The section 3.1 about the cosine correction is now expanded to address this issue.

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