

Interactive comment on “Aerosol measurements with shipborne sun-sky-lunar photometer and collocated multiwavelength Raman polarization lidar over the Atlantic Ocean” by Zhenping Yin et al.

Anonymous Referee #1

Received and published: 20 April 2019

Review of Z Yin et al, “Aerosol measurements with shipborne sun-sky-lunar photometer and collocated multiwavelength Raman polarization lidar over the Atlantic Ocean”

I was excited to have the opportunity to review this manuscript, as I have been anticipating the deployment of an automated ship-borne sun photometer for some time. I commend the authors for their efforts, but am struggling to understand the direction of the paper. Is this intended to be a validation of the sun photometer observations? If so, I find this to be an overly superficial analysis that does not make the case that this instrument is ready for adoption yet. Is this intended to be an examination of the

C1

aerosol state in Atlantic transects? If so, I'm not sure what new information is being presented, nor the implications for our knowledge of the atmosphere.

The conclusion seems to indicate it is the former, so I will proceed as if the purpose of this paper is to demonstrate new observations by the CE318-T. In a general sense, I feel like the following things are not well described and need to be resolved: 1. How different is the shipborne CE318-T from the standard (land based model)? I see that an air pump has been added, along with an anemometer and compass. But how are the compass and GPS data used to help the instrument track the sun? Compensating for ship motion has been an engineering challenge for such instruments for quite some time, so a description of how this has been solved is needed. 2. Does the instrument have a different measurement protocol than the CE318-T? Does it require special data processing? What are the limits of its operation? The anemometer is presumably intended to determine the wind speed and park the instrument when it becomes too rough – at what wind speed does this occur? 3. How accurate do you expect the instrument to be? I would assume calibration and shot noise errors are identical to the land based version, but is there an additional error source due to the difficulties of sun tracking?

These issues would be less relevant if there were some prior literature describing them. As far as I can tell, the only potential description is in the Goloub citation which is a slide presentation for me not appropriate for use as a reference. In any case, that document does not resolve the questions I noted previously. So it falls to this publication to make these things clear.

Regarding your analysis:

Sections 3.1.1 and 3.1.2: ok, these are good. They give an introduction to the comparisons.

Section 3.2.3: an interesting idea to compare this, but I find it very difficult to follow. It is not even clear which data (or times) are associated with day and night – I have to

C2

guess based on the blackout time near solar noon in the lidar data. The conclusions mention issues in this comparison and sensitivity to leveling errors in night time data. But I see no description of these problems in the actual section the conclusion refers to! The conclusion must be supported by previous sections, it is not a place to reveal new information.

Section 3.2: it is unfortunately common in analysis today, but the use of linear regressions and the coefficient of determination to test the hypothesis that two types of measurements agree is statistically unsound. This is especially the case for AOD, which tends to have a log-normal distribution, not a normal distribution that is the basis for linear regression statistics. There is a literature going back decades demonstrating this, including, for example:

Altman, D.G. and Bland, J.M., 1983. Measurement in medicine: the analysis of method comparison studies. *Journal of the Royal Statistical Society: Series D (The Statistician)*, 32(3), pp.307-317.

Bland, J.M. and Altman, D., 1986. Statistical methods for assessing agreement between two methods of clinical measurement. *The Lancet*, 327(8476), pp.307-310. (note this has been cited > 40,000 times and is in the top 100 papers by citation of all time)

Willmott, C.J., 1982. Some comments on the evaluation of model performance. *Bulletin of the American Meteorological Society*, 63(11), pp.1309-1313.

Or more recently:

Seegers, B.N., Stumpf, R.P., Schaeffer, B.A., Loftin, K.A. and Werdell, P.J., 2018. Performance metrics for the assessment of satellite data products: An ocean color case study. *Optics express*, 26(6), pp.7404-7422.

And an example of the “Bland-Altman” analysis and the corresponding “Limits of Agreement” can be found here:

Knobelspiesse, K., Tan, Q., Bruegge, C., Cairns, B., Chowdhary, J., van Dienenhoven, C3

B., Diner, D., Ferrare, R., van Harten, G., Jovanovic, V. and Ottaviani, M., 2019. Intercomparison of airborne multi-angle polarimeter observations from the Polarimeter Definition Experiment. *Applied optics*, 58(3), pp.650-669.

I gather that the CD318-T and Microtops data do agree well, but I have seen examples of high correlation coefficients masking a bias or other problem with the agreement in data.

Also, why just present AOD(500) and Angstrom? I think a comparison of all available bands would be useful.

In any case, I hope that I have not discouraged the authors from continuing with this important paper. I sincerely hope that this can be published in a form that is useful to the community. I think most of my comments can be addressed with more detailed descriptions, with the exception of section 3.2.

Detailed comments follow:

Page 1 Line 26: “cannot be used to resolve diurnal cycle of the boundary layer” -> perhaps too strong of a statement considering new geostationary observations.

Page 1 Line 30: AERONET-MAN was actually preceded by a NASA program called SIMBIOS (Sensor Intercalibration and Merger for Biological and Interdisciplinary Oceanic Studies), and in fact inherited the Microtops instruments from SIMBIOS after it ended in 2004. More details about SIMBIOS sun photometry can be found here:

Fargion, G.S., Barnes, R. and McClain, C., 2001. In Situ Aerosol Optical Thickness Collected by the SIMBIOS Program (1997-2000): Protocols, and Data QC and Analysis. NASA/TM-2001-209982, Rept-2001-01672-0, NAS 1.15:209982 <https://oceancolor.gsfc.nasa.gov/docs/technical/SIMBIOS-AOT-2000.pdf>

Knobelspiesse, K.D., Pietras, C., Fargion, G.S., Wang, M., Frouin, R., Miller, M.A., Subramaniam, A. and Balch, W.M., 2004. Maritime aerosol optical thickness measured by handheld sun photometers. *Remote Sensing of Environment*, 93(1-2), pp.87-106.

Page 3, line 4: "AOD shift of 0.002" it is not clear which (later) analysis this is referring to

Page 3, lines 8-11: this is where I would have liked more of a description between the differences between the ship-based CD318-T, and the land based version.

Page 3, line 14: What is the wind speed / sea state shut off based on the anemometer? Are there other mechanisms that would shut off the instrument other than clouds and rain?

Page 3, line 19: I think you can't expect that all readers would know what level 1.5 means, this should be explained.

Page 3, line 21: "In the framework of MAN" is vague, are you saying that it is a MAN instrument, or another Microtops that is calibrated, etc. the same way? If the latter, how is this done, specifically?

Page 5, line 10: I'm assuming the +/- values are standard deviations, but it wouldn't hurt to say it.

Page 6, line 19: "free of pollution" Do you mean to say free of fine mode aerosols? I would assume there is a lot of smoke here, but some of it might be natural.

Page 7, line 6: I must assume that between 0:00 and 7:00 UTC is nighttime, but this is never mentioned. What is the local UTC offset, and sun rise and sun set? Seems like an important part of this analysis. It should also be shown in Figure 10, probably easy to indicate with vertical lines at sunrise/set Page 7, lines 13-14: You conclude that the MBL was contaminated by dust and smoke... but this section set out to demonstrate how well day and night sun photometer observations agree, right? Why isn't that discussed instead? And why are the issues mentioned in the conclusions mentioned?

Page 7, lines 18-30: This analysis, and figure 13 are fine, I guess, but considering that the range of locations span two hemispheres and weeks of time, I think this can't be used to demonstrate that Microtopes and Cimel agree. Yes, they both capture the

C5

transition from maritime to African smoke/dust plumes and back again, but those are such large features as to not really illustrate the differences between the instruments.

Page 8, conclusions: see my previous comments, these seem unsupported by the previous material.

Page 11, line 8: This isn't really a citation for MAN. At the very least provide the website address. Or cite the Smirnov 2009 paper instead.

Page 20, line 9: You mention the color red for pure marine conditions in fig 4, on my version of the figure this is marked in blue.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-132, 2019.

C6