

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 #2

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Review of Yin et al, 2019 “Aerosol measurements with shipborne sun-sky-lunar photometer and collocated multiwavelength Raman polarization lidar over the Atlantic Ocean”

General comment

The manuscript by Yin et al, 2019 “Aerosol measurements with shipborne sun-sky-lunar photometer and collocated multiwavelength Raman polarization lidar over the Atlantic Ocean” presents sunphotometry and lidar measurements acquired on board

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the ship Polarstern during two transects of the Atlantic Ocean in 2018. The authors show the AOD time series for the two cruises and discuss in more detail three case studies. Unfortunately, I don't think the manuscript is publishable in its current state and needs to be substantially reworked.

My main critique of the manuscript is that the authors do not do enough to show the significance of their contribution. The instruments that the manuscript is based on, namely Cimel318-T, MICROTOPS II and PollyXT lidar, are not new and have been thoroughly discussed in the literature. The data processing techniques are also standard and contain no new developments. The night-time AOD measurements are a bit more novel, but there has been significant development in that area in the past decade with star- and moonphotometers (Perez-Ramirez et al, 2008, 2011, Barreto et al, 2012, Baibakov et al, 2015). Furthermore, the discussion of the three events (background marine aerosols, dust and dust+smoke) while not insignificant, does not lead to any new findings or conclusions. If the emphasis is on the first use of the Cimel318-T on board a ship, then the authors should say so and perhaps include a separate section on how this was accomplished from the engineering point of view. Even so, this by itself might not merit a separate publication in AMT. If the emphasis is on the night-time AOD measurements of marine aerosols, then the authors should spend more time discussing day/night variability, expected variations and the importance of night-time AODs. Finally, if the emphasis is on the actual AOD time series across the Atlantic, including the observed events, the authors should exert more effort to differentiate the dataset from other ship campaigns and coastal area AERONET measurements and otherwise show its significance.

Specific comments

- The literature review of the AOD measurements of marine aerosols and their significance is insufficient – the authors should try to provide a better picture of what has been already done and how their work contributes to the pool of knowledge.

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- Since comparing CE318T and PolyXT AOD is a significant part of the analysis, it should be clearly shown how AOD estimated from sunphotometry relates to PolyXT measurements through extinction (I could not find a single formula in the entire paper!) Also, I found practically no discussion of error analysis of the derived quantities.

- There should be a more detailed discussion of the cloud screening techniques and their influence on the results.

- The manuscript should be proofread for multiple English errors scattered throughout the document – I'm only pointing out some of them. Phrases like “slightly enhanced”, “quite low” or even simply “large” or “small” need to be quantified. The word “probably” is used too many times and by itself is not adequate when discussing scientific findings.

Technical corrections and minor comments

P1L27: “ARE very challenging”

P2L1: “transport” rather than “transportation”?

P2L10: why only almucantar measurements are discussed? Is your Cimel not capable of Principal Plane measurements?

P2L29: “The instruments . . . are scheduled” is awkward phrasing

P3L6: I feel that calling it a “prototype CE318-T” is misleading since no changes were made to the instrument itself but rather to the platform on which it operates. It would be interesting to see a picture or an engineering diagram of this modification

P3L25: This section should be reworked. The section discusses backscatter and extinction coefficients (presumably derived directly from Raman technique) next to the lidar ratio and (further in the article) Fernald methodology. It should be stated which quantities are measured directly and which are estimated. “Far-range” and “near-range” should be quantified. “Total and cross” should be defined.

P4L7. Not sure what is being meant here by “These intensive parameters are sensi-

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tive to particle size, shape and chemistry properties”. Which parameters? Angstrom exponent? Particle size and shape are themselves intensive parameters (i.e. are per-particle rather than bulk properties).

P5L4. Why do the authors present the “attenuated backscatter coefficient” rather than the extinction coefficient more relevant to AOD comparisons?

P5L14. The Fernald/Klett method and the choice of the lidar ratio should first be discussed in the methodology section.

P5L18. The last two sentences of the section should use references

P6L4 Which “reference values”? What does it mean “tuned to achieve the best agreement of AOD”? How does it affect your results/comparisons?

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