REVIEW 1

Synergy of active and passive airborne observations for heating rates calculation during the AEROCLO-SA field campaign in Namibia

Mégane Ventura et al.

General comments to the Editor:

The submitted manuscript demonstrates the synergy of active LIDAR and passive polarimeter measurements for quantifying the vertical distribution of the heating rates caused by the light-absorbing, lofted layers of biomass burning aerosols over the cloud deck off the coast of Namibia, Africa. The active-passive synergistic method was applied to the airborne measurements of aerosols collected by OSIRIS polarimeter and lidar operated during the AEROCLO-sA campaign in September 2017. The radiative transfer code was used to calculate heating rates in the solar and thermal part of the spectrum. Authors find strong positive heating rate 2-5 K/day caused by aerosols lofted over clouds. The cooling effects of water vapor through infrared radiation is found to generally balance its warming effect from solar radiation. The heating rate calculations were validated against the altitude-resolved irradiance measurements taken during the sounding portion of the flights.

The scientific content presented in the paper effectively demonstrates the value and merit of the active-passive synergistic approach to quantify the aerosol absorption effects over the clouds—a seasonal phenomenon observed over several regions of the world, including the prime hotspot region of southeastern Atlantic Ocean. The methodology, sensors, datasets, results, and their interpretation/evaluation discussed in the paper look sound. The plots showing different sets of results are discussed appropriately in the text. The application of the presented synergistic method to the planned future sensors carrying lidar and polarimeter/image is also discussed towards the end of the paper.

I don't see any major flaw or concern on this paper that can send back the manuscript to a major revision. However, I have prepared a long list of specific suggestions and questions, included in this review, for authors to consider in the revision. I would suggest authors to proofread the revised article in terms of the language and structure for even more effective presentation.

I should be available re-review the revised submission.

Thanks for the review opportunity.

Reply to the reviewer's general comments:

We are grateful to the reviewer for his interest in our study and for his very detailed review of the article. We have done our best to take into account these suggestions for improvement and to answer the questions raised. Below are our responses, presented point by point.

Specific Comments to authors:

Abstract:

Line 12: "We present original results derived from the airborne observations acquired from the AErosol RadiatiOn and CLOud in Southern Africa (AEROCLO-sA) field campaign led in Namibia in August and September 2017".

Thanks, we modified this sentence in the abstract.

Line14: "…an airborne prototype.."

We corrected it.

While abstract mentions about the use of OSIRIS measurements, radiative transfer code, and meteorological parameters, it misses mentioning what aerosol and cloud measurements were actually used in the RT model to calculate the heating rate. This should be added to the abstract.

As you suggested, we modified the following sentence in the abstract:

"To calculate this parameter, we use a radiative transfer code and meteorological parameters provided by dropsondes"

For

"To calculate this parameter, we use a radiative transfer code, meteorological parameters provided by dropsondes and OSIRIS-retrieved aerosol optical thickness, size, and absorption above clouds."

Line 30: "…variable chemical, optical, and microphysical properties."

Thanks. We modified this sentence in the manuscript.

Line 37: "The aerosol radiative forcing estimates provided by climate model over the Southeast Atlantic Ocean show large discrepancies"

We modified this sentence.

Lines 45-48: These sentences can be re-written as, "Biomass burning aerosols are primarily composed of two components: black carbon and organic or brown carbon. The former strongly absorbs radiation across a wide spectral range, whereas the latter has relatively weaker absorption in the visible part of the spectrum but exhibits strong spectral dependence of absorption at the UV wavelengths."

When you write 'organic or brown carbon,' it implies to us that they are the same. However, brown carbon is a subset of organic carbon that exhibits light-absorbing properties, while most organic carbon in aerosols contributes primarily to scattering.

We chose to keep our initial sentence, as we believe it offers more comprehensive information.

Line 59: "Our study focuses…" Also these two sentences can be re-organized as, "This study focuses on quantifying the radiative impact of biomass burning aerosols and aims to estimate the profiles of atmospheric heating (or cooling) rates attributable to these particles. We use airborne measuerements acquired during the AErosols Radiation and CLOuds in southern Africa (AEROCLO-sA) campaign (Formenti et al., 2019) conducted over Namibia between 5 and 12th September 2017 with the French Falcon 20 environmental research aircraft of Safire."

We modified this sentence. Thanks.

Line 65: "..such as ORACLES…"

We modified it.

Line 78: SSA quantitatively describes the ability of aerosols to absorb or scatter the solar radiation.

We clarified our text by including your sentence.

Lines 79-80: I would not call it "To a lesser extent". Given the same state of aerosols (AOT, SSA, asymmetry parameter), the extent of the radiative forcing or heating rates is primarily determined by the brightness of the underlying clouds, which is a function of cloud optical depth. Please reconsider this statement.

We agree. We clarified this point in the manuscript as you suggested.

We changed the following sentence:

"To a lesser extent, the radiative properties of the underlying target (e.g., clouds) and the vertical profiles of the thermodynamical quantities (humidity and temperature) also need to be known."

For

"Given the same properties of aerosols (AOT, SSA and particles size), the extent of the heating rates is primarily determined by the brightness of the underlying clouds, which is a function of cloud optical depth. For accurate heating rate estimate, the vertical profiles of the thermodynamical quantities (humidity and temperature) also need to be known".

Lines 81-85: Author may consider citing Jethva et al. (2024), which uses UV-VIS-NIR satellite measurements from OMI and MODIS combined with AOT measurements from HSRL-2 lidar and 4STAR sunphotometer to derive UV-VIS-NIR spectral SSA over clouds in the SEA region. Satellite retrievals of SSA agree overall with the range measured by in-situ observations, and show relatively strongest (weakest) absorption in August (October).

Jethva, H. T., Torres, O., Ferrare, R. A., Burton, S. P., Cook, A. L., Harper, D. B., Hostetler, C. A., Redemann, J., Kayetha, V., LeBlanc, S., Pistone, K., Mitchell, L., and Flynn, C. J

(2024), Retrieving UV–Vis spectral singlescattering albedo of absorbing aerosols above clouds from synergy of ORACLES airborne and A-train sensors, Atmos. Meas. Tech., 17, 2335–2366, https://doi.org/10.5194/amt-17-2335-2024.

We included this recent study in the manuscript and added it in the list of references.

We included in our manuscript the information regarding the seasonal variability of the aerosol absorption.

We changed in the manuscript the following paragraph:

"Among the different results obtained under the recent international projects, one important common finding is that the absorption by BBA appears to be very high over SEA (Wu et al., 2020; Pistone et al., 2019; Chauvigné et al., 2021) with a column integrated SSA as low as ~0.85 (at 550 nm) or 0.80 (at 865 nm)."

For :

"Among the different results obtained under the recent international projects, one important common finding is that the absorption by BBA appears to be very high over SEA with maximum absorption in August and minimum absorption in October (Wu et al., 2020; Pistone et al., 2019; Chauvigné et al., 2021; Jethva et al., 2024) with a column integrated SSA as low as ~0.85 (at 550 nm) or ~0.80 (at 865 nm) in August".

Line 86-87: Author should consider citing following two papers on aerosol radiative effects over clouds.

Meyer K. G., S. E. Platnick, L. Oreopoulos, et al. 2013. "Estimating the direct radiative effect of absorbing aerosols overlying marine boundary layer clouds in the southeast Atlantic using MODIS and CALIOP." J. Geophys. Res. Atmos. 118 (10): 4801-4815 [10.1002/jgrd.50449] [Journal Article/Letter]

Zhang, Z.*, K. Meyer, H. Yu, S. Platnick, P. Colarco, Z. Liu, and L. Oreopoulos (2016), Shortwave direct radiative effects of above-cloud aerosols over global oceans derived from 8 years of CALIOP and MODIS observations, ACP, 16(5), 2877–2900, doi:10.5194/acpd-15-26357-2015.

Thanks, we added these two references in the introduction section of the manuscript and we updated our list of references.

Line 92: Jethva et al. (2014) should be corrected to Jethva et al. (2018).

We corrected it in the revised version of the manuscript.

Jethva H., O. Torres, C. Ahn and et al. 2018. "A 12-year long global record of optical depth of absorbing aerosols above the clouds derived from the OMI/OMACA algorithm." Atmospheric Measurement Techniques 11 (10): 5837-5864 [10.5194/amt-11-5837-2018] [Journal Article/Letter].

Line 98: CALIOP vertical profiles of extinction, if properly constrained, can provide altitude-dependent heating rates.

We added this sentence in the manuscript:

"If properly constrained, CALIOP vertical extinction profiles can provide altitude-dependent heating rates (Deaconu et al., 2019)."

Line 129: Define OSIRIS here, if it is not defined earlier in the text.

We now explain the acronym OSIRIS in the manuscript.

Line 125: Double dots (..).

Line 135: Some restructuring: "During the campaign, the air mass intercepted (or measured) by the on-board instruments were mainly transported from the in-land biomass burning source areas, emitting substantial amounts of carbonaceous aerosols transported over the southeastern Atlantic Ocean, as far as the Ascension Island. The airmass was then drifted to the southeast towards the Namibian coast due to the anticyclonic circulation located over South Africa".

Thanks, we included your revised sentence in the manuscript.

Low-pressure system rotates in clockwise direction in southern hemisphere. So, make sure that the cyclonic or anticyclonic movement is referenced to the southern hemisphere.

We confirmed that "anticyclonic movement" is the correct term.

Line 149: "…with the aircraft during specific flights". It would be desirable to mention the dates of these flights.

This information is already given in table 1. In table 1's legend:

"Sounding" indicates that an aircraft sounding (i.e., rapid descent of the aircraft) was performed during the flight."

So, aircraft soundings were performed on the 7th, 8th and 12th, as indicated in table 1.

Reviewer 2 also requested a modification of the 'aircraft soundings' term for "spiral descents" for sake of clarity. We have also addressed this feedback by incorporating the term "spiral descent" in the text.

Table 1: Add the wavelength of the AOT measurements (865 nm) in the respective column. Similarly, ABS should be labelled as the imaginary part of the refractive index. Please correct me, if wrong. Also, the wavelength of the ABS should be included in the table and text. AOT values of > 0.4 at 865 nm translate to AOT of > 1 at 500 nm.

We have implemented the changes to Table 1 as you recommended.

Line 78: Define OSIRIS the first time it was referred in the text.

OSIRIS acronym is now defined.

Line 184-185: It is assumed here that the LNG is a downward looking lidar.

Depending on field campaigns and objectives, LNG lidar was used for both upward and downward measurements on the F-20 aircraft.

We added this sentence in this manuscript for clarification:

"In our study, we use lidar LNG to depict the vertical profiles of aerosol extinction below the aircraft".

Line 189: Re-structuring: "Consequently, lidar data acquired for optically thick plumes at these wavelengths become less reliable in accurately determining the base altitude of the aerosol layer. On the other hand, the 1064-nm signal penetrates deeper into the aerosol layer due to significantly reduced attenuation, providing a better view of the depth of the aerosol layer. For this reason, we used LNG lidar data acquired at 1064 nm to accurately depict the aerosol extinction profile."

Thanks, we included this revised paragraph.

Line 194: "the extinction aerosol optical thickness". PLASMA resembles the 4-STAR sun photometer. Are there any major differences in the way both measure AOTs?

To my knowledge, the NASA 4-STAR sun-photometer can measure both AOT (by tracking the sun) and diffuse scattered light (through scan measurements), while PLASMA can currently only measure AOT in a similar manner to the 4-STAR.

Line 210: "600 m above the sea level".

Thanks, we modified it.

Line 213: What is the swath width of the OSIRIS instrument/retrievals?

We added two paragraphs in the manuscript:

-in the section 2.2:

"This instrument provides 20-meter resolution images for visible and near-infrared light (440-940 nm) and 60-meter resolution images for shortwave infrared light (940-2200 nm) at a 10-kilometer altitude."

-in the section 3.3.1:

"The aerosol retrieval is based on solar plane measurements and assumes spatial homogeneity over the entire OSIRIS visible image (an area of approximatively 20x10 km²). This procedure increases the sensitivity to aerosol properties, which are retrieved at this spatial resolution using an optimal estimation algorithm (Chauvigné et al., 2021)."

Line 229: Equation 1: This is simply the scaling of lidar extinction profile to match with the OSIRIS-retrieved AOTs. OSIRIS measured columnar AOT above the clouds, whereas the lidar measurements, according to Figure 7 (a & b), correspond to the atmospheric depth between cloud-top to 6-km. This leads to an assumption that the smoke aerosols are confined within these altitude range. Any amounts of aerosols above 6 km, therefore, are ignored in the analysis. This should be mentioned in the text.

We added this sentence in the manuscript:

"We also assume that the lidar accurately measures the base and top altitudes of the smoke layer, and that smoke aerosols are primarily confined within this range, typically between the cloud top and 6 km (see Figures 7a and 7b)."

Line 249: Which satellite/sensor total column O3 data was used here?

It was OMI data. We added this information in the text.

Line 257: Where Cp stands for…

Ok. We modified it.

Line 264: Optimal estimation method.

Thanks, we modified it.

Line 266-269: This is not understood. Both OSIRIS and GAME calculations ignore particles larger than one micron (radius or diameter?). How did author accounted for the aerosol optical properties to longer wavelengths?

This sentence: '... ignore particles larger than one micron…' was not clear.

We modified:

"The OSIRIS algorithm and GAME calculations assume both a simplified aerosol representation: the real part of the complex refractive index is constant (1.47) across the spectrum, the imaginary part is independent of wavelength, and particles larger than one micron are neglected"

For

"The OSIRIS algorithm and GAME calculations assume both a simplified aerosol representation: the real part of the complex refractive index is constant (1.47) across the spectrum and the imaginary part is independent of wavelength."

"A single lognormal particle size distribution is used for which the mean radius is retrieved whereas the variance is fixed (Chauvigné et al., 2021)".

How did author accounted for the aerosol optical properties to longer wavelengths?

"The previous aerosol model, assuming spectrally flat complex refractive index and a log-normal particles particle size distribution, is used to extrapolate the aerosol optical properties to longer wavelengths using Mie theory."

Line 280: MODIS cloud product of cloud effect radius histogram over the southeastern Atlantic Ocean shows peak around 12 microns, which is closer and near-consistent with the 10 microns assumed in this study. It is imperative to mention here that which cloud model is used here. Most likely, author is assuming water clouds. What droplet size distribution was assumed here? Modified GAMMA? Also, did author compare the Irradiance-based COT retrievals with those derived from OSIRIS algorithm? How well do they compare?

We added this sentence in the manuscript:

"A two-parameter gamma distribution was employed to describe the size distribution of cloud water droplets in the cloud layer situated below the aerosol layer. The effective radius and variance of the droplets were fixed at 10 microns and 0.1, respectively."

Also, did author compare the Irradiance-based COT retrievals with those derived from OSIRIS algorithm? How well do they compare?

A detailed comparison of cloud optical thicknesses retrieved by OSIRIS and measured by flux would require significant additional work, which is outside the scope of this article.

Line 322: "aerosol absorption optical depth of 0.03".

Here, this is not the aerosol absorption optical depth, 0.03 corresponds to the imaginary part of the complex refractive index of the particles.

We changed in the text the following sentence:

"Optical properties used were: Cloud Optical Thickness (COT) 11.69, Aerosol Optical Thickness (AOT) 0.43, and aerosol absorption 0.03"

For

"Optical properties used were: Cloud Optical Thickness (COT) 11.69, Aerosol Optical Thickness (AOT) 0.43, and imaginary part of the aerosol complex refractive index 0.03"

Line 325-327: spectrally neutral aerosol absorption: Does author mean the spectral imaginary part of the refractive index or aerosol absorption optical depth? In either case, assuming spectral neutral behavior might not be an appropriate assumption. Biomass burning aerosols are often rich in organics (brown carbon), which exhibits strong spectral absorption at shorter wavelengths. If it is not too much of work and computation, author is suggested to assume spectrally varying aerosol absorption optical depth assuming the Absorption Angstrom Exponent in the range 2.0-2.5, a typical range for biomass burning aerosols.

Spectrally neutral aerosol absorption refers to the imaginary part of the refractive index that is assumed spectrally neutral.

We clarified this point in the text.

We modified this sentence:

"For particles of the size range shown in table 1, and based on our assumption of spectrally neutral shortwave aerosol absorption (i.e., the aerosol absorption it is assumed to be the same across both the thermal infrared and solar spectrums)"

for

"For particles of the size range shown in table 1, and based on our assumption of spectrally neutral shortwave aerosol absorption (i.e., the aerosol absorption refers to the imaginary part of the complex refractive index and it is assumed to be the same across both the thermal infrared and solar spectrums)"

It looks like the aerosol absorption of 0.03 assumed in the simulation referred to the imaginary part of the refractive index.

You are right. It refers to the imaginary part of the refractive index.

Biomass burning aerosols are often rich in organics (brown carbon), which exhibits strong spectral absorption at shorter wavelengths. If it is not too much of work and computation, author is suggested to assume spectrally varying aerosol absorption optical depth assuming the Absorption Angstrom Exponent in the range 2.0-2.5, a typical range for biomass burning aerosols.

In a previous study (Siméon et al., 2021), we estimated the absorbing fraction of organic aerosols, specifically Brown Carbon (BrOC), within biomass burning plumes over the southeastern Atlantic Ocean to be between 2% and 3%. Although BrOC is not the primary constituent of organics and biomass burning aerosols, we recognize its non-negligible impact on aerosol optical properties. However, our previous work (Siméon et al., 2021) clearly indicates that soot carbon content, aerosol loading, and particle size are the primary factors controlling the aerosol optical properties and consequently their radiative impact over the region of interest. Therefore, accounting for brown carbon could be considered a refinement of our methodology that will be considered in further work. Including brown carbon (BrOC) in our calculation would be associated with a significant amount of work that is out of the scope of our study.

We mention this limitation in the manuscript:

We added in the manuscript:

"This assumption implies that our simulation neglects the contribution of brown carbon. Consequently, we assume here that soot exclusively governs the absorption properties of aerosols generated from biomass burning"

If it is not too much of work and computation, author is suggested to assume spectrally varying aerosol absorption optical depth assuming the Absorption Angstrom Exponent in the range 2.0-2.5, a typical range for biomass burning aerosols.

We already have an aerosol absorption optical depth that varies with the wavelength in our simulation.

Indeed, for a given spectrally flat imaginary refractive index, the aerosol absorption optical thickness spectral dependence is primarily control by particles size. Since we consider fine mode aerosol (mean radius of 0.1 microns) that shows a strong spectral dependence in terms of aerosol optical properties, the aerosol absorption optical depth already strongly depends on wavelength. Including BrOC will modulate the spectral dependence of the aerosol absorption.

Line 425-430: What was the variability in the COT retrievals during spiral descent? Since COT is retrieved at each change in altitude, these numbers should be handy and mentioned here.

We added this information in the manuscript:

"During the spiral descent, the majority of COT retrievals fall within the range of 8 to 14."

Could author remind here what wavelength range was considered to calculate solar and thermal heating rate, respectively?

This information is already given in the manuscript (see the section 3.2):

"The code considers 208 spectral intervals distributed between 0.2 and 3 µm for the solar spectrum. For thermal infrared, 115 intervals are used to cover the wavelength range from 4 microns to 47 microns"

For thermal IR range, fine mode smoke particles do not play a significant role; however, cloud properties (COT and effective radius) drive the heating/cooling rates. Does the blue curve in Figure 5 (f) use the improved cloud modeling (cloud retrievals at each change of altitude)?

We accounted for it but while this procedure may have some influence, our analysis of thermal infrared results indicates no significant impact. COT and cloud effective radius (and cloud top altitude) primarily influence the calculation of heating/cooling rates (and irradiances) within the cloud layer itself. This is primarily because we are examining spectrally integrated fluxes and heating/cooling rates (integrated from 4 to 47 microns). As water vapor absorption significantly impacts the thermal infrared spectrum, this is the knowledge of the water vapor profiles that is of paramount importance for our study.

We already mentioned this point: see "Note that this reasoning applies to measurements acquired in the solar spectrum, with measurements in the thermal infrared appear to unaffected or negligibly affected by this effect."

Line 464: "Despite heterogeneity in COT…"

Thanks, we included your suggestion.

Figure 6: It is interesting that there are seemingly three layers of aerosols identified in the solar heating rate. Could author confirm this with lidar and/or PLASMA AOT profile measurements?

No, there is only main layer of aerosols between 1 and 6 km for this case study Figures 8-e and 8-j further support this observation, demonstrating relatively homogeneous vertical distribution of aerosol radiative properties for the 12th September flight. While Figure 6 exhibits three distinct positive peaks in the solar heating rate profile, these are not attributed to atmospheric variations. Instead, they likely stem from spatial heterogeneity in cloud properties as measured during the spiral descent.

See the discussions in the manuscript in section 3.4 :

"Airborne irradiance measurements made above clouds during spiral descents are associated with different cloud targets. Thus, the heating rates calculated from the irradiance measurements considered in this study are related to variations in the radiative properties of the atmosphere and also to variations in the radiative properties of the underlying cloud layer. These rates cannot be considered as the "intrinsic" heating rates of the aerosol and atmospheric layers that we seek to calculate and quantify.

"However, by integrating target albedo variability into the simulations, the heating rates estimated from the measurements will become comparable to the simulated ones. Even if these latter do not reflect the intrinsic properties of the atmosphere, if our theoretical approach, namely the simulations, is able to reproduce the measured heating rates above cloud targets changing at each altitude level z, a fortiori, it will also be able to predict these rates if the cloud target remains fixed (simpler case). The direct comparison of measured and simulated irradiances acquired during the soundings will therefore be carried out in a validation perspective."

Line 499-500: "which are all found in all rate calculations (>10 K/day)." This is not understood.

OK. We modified.

Figure 7: About cloud formation, as mentioned in the text, at the top of aerosol layer for Sep 8th flight. Did author notice similar cloud layer during Sep 12th flight? Or the cloud layer is filtered out in the heating/cooling rates? There are drastic differences in the 1064-nm backscatter values between these two flights. Mentioning the averaged AOTs during these two flights here, for the high-altitude segments, would aid the reader in interpreting the results.

Did author notice similar cloud layer during Sep 12th flight?

Our observations during the 12th flight did not reveal any cloud formation at the top of the aerosol layer.

There are drastic differences in the 1064-nm backscatter values between these two flights. Mentioning the averaged AOTs during these two flights here, for the high-altitude segments, would aid the reader in interpreting the results.

This difference in the 1064-nm backscatter values can be attributed to variations in aerosol levels. Aerosol concentrations were significant during the 8th flight and moderated during the 12th flight. The AOT was respectively equal to 0.45 and 0.18 on average (at 865 nm) for the 8 and 12th flights respectively.

We added this paragraph in section 4.4.1:

"For 12th September, no cloud formation was observed at the top of the aerosol layer (see Figure 7b). Average AOT at 865 nm was 0.45 and 0.18 respectively on the 8th and 12th September, explaining the lower 1064-nm backscatter for the 12th September flight (see Figures 7a and 7b). Consistent key findings were observed for both flights. The specific results of the 8th September flight are discussed below."

Line 507: "fairly homogeneous values of the order of 4 to 5 K/day…"

Ok, we modified.

Figure 8: It appears that the plots for different days are ordered w.r.t highest to lowest heating rates. Please mention this in the text. Also, label the x-axis as heating rates for the plots shown at top. It is worth to mention the averaged COTs measured on these days in Table 3. It is assumed here that the heating rates are vertically integrated. Please clarify it in the text.

It appears that the plots for different days are ordered w.r.t highest to lowest heating rates. Please mention this in the text

We added in the manuscript:

The plots shown in Figure 8 are ranked by decreasing heating rate.

Also, label the x-axis as heating rates for the plots shown at top.

We improved figure 8 but we did not include this suggestion. This figure is already populated and this information is indicated in the text.

It is worth to mention the averaged COTs measured on these days in Table 3.

The COT values are aalready given in table 1.

It is assumed here that the heating rates are vertically integrated. Please clarify it in the text.

Ok. We added this sentence in the text to emphasize this point :

"The table 3 contains the vertically integrated values of the heating rates"

Line 578: "…observed during the September 2017 AEROCLO-sA airborne campaign"

Ok, thanks. We corrected it.

Line 616-620: This is a good point. The COT should also play somewhat significant role in heating rates, depending upon what range of COT is observed. Also, one needs the true COT below the aerosol layer, after aerosol attenuation correction, in order to accurately assess the role of COT in both radiative effects and heating rates.

Thanks.

Line 621: desert dust above the clouds not on a global scale, but regional scale, i.e., Saharan dust transport over the Atlantic, dust plumes over the Arabian Sea, and Asian desert dust over the clouds along the eastward transport pathways over the Pacific.

Right. We used now the word "regional".

At the end, it is an excellent demonstrative study of how the synergy of active lidar and passive measurements brings new quantitative information on the effects of light-absorbing aerosols above the clouds.

Thank you again for your efforts in helping us improve the paper.

Citation: https://doi.org/10.5194/amt-2024-121-RC1

REVIEW 2

Review of "Synergy of active and passive airborne observations for heating rates calculation during the AEROCLO-SA field campaign in Namibia"

Mégane Ventura et al, AMT 2024

The manuscript presents a synergistic method of determining aerosol heating rates from a Lidar and a multi-angle polarimeter, and demonstrates with airborne data from the AEROCLO-SA campaign in coastal Namibia. This is a novel approach of relevance due, in part, to forthcoming polarimeter and lidar observations. The study is approached in a reasonable manner, and a few issues with description and presentation are resolvable. I believe the manuscript is ready for final publication after minor revisions.

We would like to thank the reviewer for their valuable comments and suggestions for improvement, as well as for their interest in our study. Our point-by-point responses are provided below.

Specific comments

Abstract: the abstract mentions (line 21) how the methodology is validated, but not what that validation indicated. One of the challenges in this approach appeared to be horizontal variability, such that the vertical profiles/spirals have a diameter wide enough to observe significant variability.

We added the following sentence in the abstract:

"Despite the challenges posed by cloud horizontal variability observed during the spiral descents, simulated and measured results generally agree in most cases".

Introduction and Conclusion: There are many examples of strangely formatted paragraphs (e.g line 67, 590, 612, 615). In some cases these seem to be accidents and the text after this point should be part of the previous paragraph. In other cases it is unclear, and makes the logical flow more difficult to follow.

We corrected it. Thanks.

Introduction (Line 105): the term ‘in situ’ here is used to refer to the irradiance measurements, I believe. I generally think of this term to mean non-remote sensing measurements such as particle counters or other instruments that assess a specific parcel of the atmosphere. Perhaps my definition is too specific, but I recommend adding ‘irradiance’ (or some other words) to this to clarify.

You are right. We added the word “irradiance” in the text.

Figures – I feel these are out of order. The order in which they are revealed in the text doesn’t correspond to the numbering order. For example, Figure 7 is mentioned in section 2.3 before figures 2-6. I also find the figures to be overly compact and the text too small compared to that of the manuscript. I spent a lot of zooming and squinting on figures 1b, 2, 3, 4, 5, 6, and especially 8.

We did our best to improve the figures:

-We modified and increase the size of figure 1b

-We modified and improved the figure 2. We have increased the size of the legends and curves.

-We improved figure 8 in a similar way.

The order in which they are revealed in the text doesn’t correspond to the numbering order.

We opted to maintain the current order of the figures, as we believe it facilitates reader comprehension of the paper's logical flow. Figures 7 primarily encompass results, necessitating their placement within the results section. To facilitate the analysis of the results, we have to present the attenuated backscattering coefficient alongside the ***figures displaying our heating/cooling rate calculations****. While we acknowledge mentioning Figures 7-a and 7-b earlier the manuscript, this serves solely to support the description of the atmospheric vertical properties for the case studies.*

Figure 1: the caption doesn’t indicate where/how the cloud optical thickness is determined (or reference section 3.3.2).

In this caption, we added: “The method used to retrieve the COD is described in section 3.3.2.”

Figure 2: It took me a moment to understand why the parameter values were not in order, although it is described in the caption. I think swapping the reference case to be black, with a different symbol would best illustrate the difference/importance of these values to the other data.

“Thank you for your suggestion. As indicated in the caption, the blue curve represents the reference case, which we believe is clearly identified.”

Figure 7: It isn't mentioned anywhere in the text why there seems to be regular gaps in the lidar data for the Sept. 8th case. Was there an instrumental problem? I'm guessing it is due to turns in the 'square spiral' during vertical profiles, but if that is the case why isn't this present in the Sept. 12th dataset?

The gaps in Figures 7 can be attributed to several factors:

- ***Lack of extinction profile data for LNG:*** *This can occur because clouds were regur lay formed in the upper part of the aerosol plume are dense enough to entirely attenuate the lidar signal.*
- ***Missing or unusable OSIRIS aerosol data:*** *The aerosol parameters retrieved by OSIRIS (AOT, absorption, size) may be absent due to various reasons: unusable OSIRIS data, rejected data because of insufficient modeling accuracy (see Chauvigné et al., (2021).*

Although raw lidar data (attenuated backscattering coefficient) are always available, we chose to represent it only when the aerosol extinction profiles were also available.

I'm guessing it is due to turns in the 'square spiral' during vertical profiles

No, we did not show the data acquired during the loop descent in Figures 7.

We added these two sentences in the manuscript to clarify:

In section 2.3

"In Figure 7-a and 7-b, note that the attenuated backscattering coefficient is shown only when lidar-derived aerosol extinction profile is also available. Compared to the flight on 12th September, the increased number of missing lidar retrievals (in white in Figure 7-a) observed on the 8th September flight is attributed to clouds forming at the top of the biomass burning layer."

In section 4.4.1

"For 12th September, no cloud formation was observed at the top of the aerosol layer (see Figure 7b). Missing data in heating rate figures for this second flight (Figures 7d, g, h and j) result from missing available OSIRIS data or retrievals."

Table 1: I eventually figured out that ABS specifically is the imaginary component of the aerosol refractive index. This needs to be more clear. In the table and in parts of the text it is labeled as 'aerosol absorption' but that is unclear, since it could mean an absorption coefficient, co-albedo, absorbing aerosol optical depth, etc. In fact, I would argue that the imaginary component of the aerosol refractive index shouldn't be labeled as 'absorption' at all, because the amount of light absorbed depends not just on that parameter but others that define the aerosol, such as size.

In Table 1, we switched the acronym ABS for the acronym ImRI and we now explicitly state that ImRI represents the imaginary part of the complex refractive index. This sentence was added to the caption of Table 1:

“ImRI stands for the imaginary part of the complex refractive index at 865 nm”.

When necessary, we switch the term “'aerosol absorption' for “imaginary part of the aerosol complex refractive index”.

Table 2: I also dislike the terminology of ‘aircraft sounding’ as that can be confused with dropsondes which you also use. I would stick to the ‘spiral descent’ terminology used elsewhere.

Thanks. We have included your suggestion in the manuscript.

Section 3.1, Line 224: While I understand the necessity of assuming minimal variation of aerosol properties vertically within the aerosol layer, I am not convinced that this is in fact the case. What does the literature from other assessments of aerosol properties from AEROCLO or ORACLES or CLARIFY indicate?

Analysis of in situ data collected within the biomass burning aerosol plumes located in the free troposphere during the CLARIFY campaign (Ascension Island region) demonstrated some vertical variability in aerosol chemical composition and mixing state. While particle size exhibited comparatively minor vertical changes, the observed variations in composition and mixing state led to changes in single scattering albedo (SSA) within the BBA layer. A prominent trend emerged, with SSA generally increasing from the base to the top of the free-tropospheric BLA layer

Extract from Wu et al., 2020:

“In the BB-polluted FT, average SSAs at 405, 550 and 658 nm increased from 0.82, 0.81 and 0.79 in the low FT (around 2 km) to 0.87, 0.86 and 0.85 at an altitude up to 5 km.”

So, we agree that assuming constant aerosol properties (size and imaginary refractive index and consequently SSA) throughout the vertical column is a limitation of our methodology.

We mention this limitation in the conclusion section of the manuscript.

We added this paragraph:

“Furthermore, our current method assumes constant aerosol properties (size, complex refractive index, and single scattering albedo, SSA) with altitude. However, observations suggest that SSA varies with altitude within biomass burning layers (Wu et al., 2020). As our method currently only considers variable above-cloud aerosol concentration as a function of the altitude, incorporating altitude-dependent aerosol properties could enhance its accuracy”

Section 3.3.1. This section would benefit with a table of retrieved parameters from the optimal estimation algorithm – and identification as to which are directly retrieved parameters and indirect (I suspect SSA is this).

We did not include a table but we now mention in the text the parameters that directly retrieved and the ones that are computed from the retrieved parameters, such as SSA.

We added in section 3.3.1:

"The method directly retrieves the AOT, the mean radius and the imaginary part of the refractive index. The SSA is computed from the retrieved particles size distribution and the complex refractive index of the particles."

Section 3.4 Are high order polynomials really the best way to provide a fit to irradiance profiles? Most likely you are overfitting. If the choice of polynomial order leads to variation of 1-1.5K in the heating rate, I consider that a problem when the overall heating rate is on the order of 3-5. Consider alternative approaches – perhaps lower order polynomials, splines, or other approaches from the interpolation literature.

-Simulated irradiances profiles can be accurately model with polynomial fitting. The choice of the order of the polynomial fitting depends on the vertical variability of the aerosol and atmosphere properties. This approach with polynomial fitting of the net irradiances was already used in a previous study to calculate atmospheric heating rates (Mallet et al., 2016).

-As noted earlier, the large diameter of the spirals leads to significant variability in cloud properties within the measurement area, impacting our irradiance measurements. This makes more challenging the determination of the optimal order for polynomial fitting of the net irradiance profiles in our study.

-The provided confidence interval (1-1.5 K) accounts for uncertainties in the intrinsic properties of the atmospheric heating rates and also for uncertainties in the variations in the underlying cloud scene.

-Lower order polynomial fitting leads to lower correlation coefficient and the model does not well reproduce the observed tendency.

Figure 4d. More details on how this reconstruction happened, or pointing to the relevant section in the paper, is necessary in the figure caption description of 4d

You are right, this information is missing. We added this paragraph in section 4.2:

"We have used an instrumental synergy to reconstruct a water vapor profile over the entire atmospheric column (Figure 4-d), which is used in subsequent calculations. The aircraft probe allows the measurement of the water vapor profile between 1 and 8 km (altitudes of the end and beginning of the spiral descent). The dropsonde provides information on the water vapor present in the cloud. A factor is applied to these data to ensure consistency between the aircraft probe measurements and the dropsonde measurements PLASMA measurements are used to check the water vapor content above the aircraft, which is negligible in most cases."

We also now indicate the relevant section in the caption.

Section 4.4.1 is very limited in its description of the Sept 12th case. I would have thought that this would be the focus of the description due to the presence of clouds at the top of the aerosol layer for most of the Sept 8th data.

As suggested by the first reviewer, we added more discussion related to the Sept 12th case in this section.

Section 4.4.1 lines 513-517: is this section referring to figure 6 rather than figure 7?

You are right. Thanks. We wanted to refer to see Figure 6f

We corrected it.

I was confused why figure 8 and table 3 are not presented in chronological order. It took me a moment to realize they were sorted in terms of aerosol heating rate. That's fine, but it should be noted.

The other reviewer also noted this issue. This is now indicated in the text : "The plots shown in Figure 8 are ranked by decreasing heating rate".

Additionally, what is the meaning of the highlight on sept 5th data in table 3?

It was a bug. We corrected it.

Conclusions, line 615: could space based profiles of water vapor also be used?

Profiles of water vapor are operationally provided over cloud-free scenes by sensors such as IASI-NG utilizing thermal infrared measurements. Currently, to our knowledge, water vapor profile retrievals for cloudy scenes remain challenging and are an area of ongoing research.

Conclusions, line 626 – this is the first time AERO-AC products are mentioned (with that name, at least). Best to include a citation, spell out the acronym, etc.

We improved this part.

Conclusions, line 629: ACCP is now called "Atmosphere Observing System (AOS)".

Thanks. We corrected it.

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