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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