RESPONSE TO REFEREE 3

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Vertically stratified long-range transport of biomass-burning aerosol over Reunion Island during September 2017

Dear Referee 3,

We thank you for identifying the need to distinguish an informative intensive case study from a seasonal or regional assessment. We have narrowed the manuscript's scope and conclusions. The revised paper no longer claims to reduce radiative-forcing uncertainty or to provide complete source apportionment, and the two lidar nights are explicitly treated as illustrative. The qualifications are integrated into the relevant methods and results, while Section 4 moves continuously from findings to comparison, limitations, and future work.

Line references below refer to the revised manuscript with tracked changes and continuous line numbering.

1. Novel scientific insight

Comment: *The manuscript did not clearly distinguish its contribution from previous SWIO smoke studies.*

Response: The Introduction now identifies the observational gap at Reunion in the eastern SWIO, several thousand kilometres downstream of the main source regions (lines 81-87) and defines the objective accordingly (lines 101-107). Section 4 retains quantitative comparison with DeLiAn, SAFARI-2000, ORACLES, AEROCLO-sA, regional corridor studies, and trans-Atlantic observations. It then identifies the specific advance as the vertically resolved characterization of layers reaching Reunion, including one event with two simultaneous elevated layers showing contrasting optical and moisture environments (lines 511-515). We do not claim novelty for long-range transport itself.

2. Relation to the broader SWIO and biomass-burning season

Comment: *Observations at one location and two nights cannot represent the broader region or season.*

Response: We agree. The AERONET record, MODIS/CALIOP observations, and FTIR record provide temporal and regional context, but the lidar cases remain illustrative. Section 4 explicitly states that two routine nights cannot establish the frequency of the observed configurations during September or the wider season (lines 524-525). Longer records and systematic case selection are identified as necessary to quantify representativeness (lines 530-531).

3. Radiative-forcing uncertainty

Comment: *The study did not quantify forcing or demonstrate uncertainty reduction.*

Response: We agree and removed that objective rather than adding a new radiative-transfer analysis. The revised objective focuses on vertical structure, optical properties, moisture environment, and model-supported transport histories (lines 101-107). The final paragraph states explicitly that the study does not calculate radiative forcing; instead, the layer-altitude and optical characterization provide context for future aerosol-radiation and aerosol-cloud studies (lines 531-535).

Aerosol and water vapour

Comment: *WVMR does not represent aerosol hygroscopicity, and 'decoupled' needed definition.*

Response: We agree. WVMR is now used only to characterize the moisture environment. For September 19th, 'decoupled' is defined descriptively as altitude ranges in which aerosol extinction and WVMR do not

covary, without a causal or hygroscopicity inference (lines 370-372). For September 25th, the approximately 74% uncertainty of the lower-layer WVMR is retained, and interpretation is limited to the robust contrast between the dry upper layer and moister lower layer (lines 387-391and 491-493).

For the referee's information, during the preparation of this manuscript, the hygroscopic growth factor was estimated using the so-called Hänel parameterization. The only layer that showed simultaneous variations of extinction and relative humidity is the bottom layer of September 25th. A hygroscopic growth factor of 5.2 was found, far beyond literature values that fall in the range 0.10 – 1.84 (Sicard et al., 2022). We concluded that hygroscopic growth was not the driving phenomenon here.

Sicard, M., Fortunato Dos Santos Oliveira, D. C., Muñoz-Porcar, C., Gil-Díaz, C., Comerón, A., Rodríguez-Gómez, A., and Dios Otín, F.: Measurement report: Spectral and statistical analysis of aerosol hygroscopic growth from multi-wavelength lidar measurements in Barcelona, Spain, Atmos. Chem. Phys., 22, 7681–7697, https://doi.org/10.5194/acp-22-7681-2022, 2022.

4. Madagascar and other source regions

Comment: *The two-source FLEXPART experiment omitted Madagascar despite nearby fires.*

Response: This is a valid limitation. Rather than adding a partial third tracer that would still not provide complete aerosol source apportionment, the Methods now define the calculation as a two-source experiment, state that Madagascar and other possible regions were not simulated separately and explain why the reported fractions remain conditional (lines 243-248). The event interpretations use 'consistent with' or 'supports', acknowledge unsimulated contributions, and avoid unique attribution (lines 499-504). The limitation is included in the final synthesis (lines 522-524).

CO versus aerosol transport

Comment: *CO has a longer lifetime than aerosol, and aerosol-specific tracers would be preferable.*

Response: We agree with the physical distinction. CO is now explicitly described as an air-mass tracer, not the aerosol itself (lines 246-248 and 502-504). Section 4 states that the simulated fractions describe represented CO transport and cannot be converted quantitatively into aerosol-mass contributions because CO and aerosol have different lifetimes and removal processes (lines 522-524). Aerosol-specific simulations, including additional source regions, are identified as future work (lines 530-531).

CAMS aerosol bias attribution

Comment: *The sea-salt/spatial-averaging explanation was speculative; regridding, nitrate omission, and assimilated satellite AOD required clarification.*

Response: We removed the causal attribution. The Methods now explain that the 1°-radius average is a sampling choice around Reunion, not a regridding of the native field, and distinguish total-AOD assimilation from model-determined species partition. Nitrate is explicitly noted as absent from the species used here (lines 211-217). The Results list measurement mismatch, spatial sampling, model assumptions, and assimilated satellite observations as possible contributors but state that the analysis cannot isolate the cause of the discrepancies (lines 277-279).

Satellite-ground AOD comparison

Comment: *A satellite/AERONET comparison was suggested if feasible.*

Response: Because this suggestion was conditional and would require a separate spatial/temporal collocation and cloud-screening analysis, we did not add it. Instead, we removed the unsupported diagnosis of the CAMS bias and no longer use total-AOD agreement to validate aerosol composition (lines 215-217 and 275-276). This keeps the claim within the evidence provided by the present case study.

5. Extend the analysis beyond a single event

Comment: *At least a full biomass-burning season or more cases were recommended*.

Response: We respectfully retain the intensive case-study scope. A seasonal assessment would require systematic case selection, additional lidar retrievals, source simulations, and a different statistical objective. The revised Discussion instead states explicitly that the two nights are illustrative and do not establish seasonal frequency or regional representativeness (lines 524-525). It identifies longer records, systematic selection, and aerosol-specific simulations as the appropriate next step (lines 530-531).

The revision therefore addresses the central concern by aligning the scope with the available evidence, while reserving seasonal statistics and aerosol-specific source modelling for a dedicated future study.