RESPONSE TO REFEREE 1

Manuscript egusphere-2026-3070

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

Dear Referee 1,

We thank you for the constructive assessment and for recognizing the value of the multi-instrument dataset. We agreed that the manuscript sometimes assigned more certainty to source, age, and radiative implications than the evidence supports. The revision now separates observations, retrievals, model output, and interpretation; narrows the objective to an observational case study; and integrates the relevant qualifications where each result is introduced. Section 4 then brings the argument together in a continuous sequence from demonstrated findings and model-supported interpretation to limitations and implications.

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

1. Meaning of 'river of smoke'

Comment: *The term was qualitative and required an objective definition.*

Response: We removed 'aerosol river' from the title (lines 1-2) and no longer use the expression as an analytical classification. The Introduction defines the earlier 'river of smoke' usage descriptively as a spatially coherent, large-scale smoke plume transported from Southern Africa toward the Indian Ocean (lines 57-60). In the regional analysis, we use 'organized smoke-transport corridors' (line 296) and explicitly state that the maps indicate plausible regional connectivity rather than a threshold-based source classification (lines 301-303).

2. Logical integration of datasets

Comment: *The manuscript read as a sequence of independent datasets rather than a combined chain of evidence.*

Response: We revised the opening of Section 3 to state what each evidence type constrains. Ground-based lidars provide the observed layer structure and optical properties; CAMS provides model-based composition information; FLEXPART and FTIR constrain broad CO air-mass transport; and regional satellite observations provide spatial context (lines 250-255). The first two paragraphs of Section 4 then synthesize the observed/retrieved quantities first (lines 486-495) and the model-supported interpretation second (lines 496-504), making the logic explicit without interrupting the discussion with additional subheadings.

3. Source attribution and FLEXPART uncertainty

Comment: *The layer assignments, especially on September 25th , relied heavily on FLEXPART and should be more cautious.*

Response: We agree. FLEXPART is now introduced as a two-source sensitivity experiment rather than an exhaustive source-apportionment calculation. The Methods incorporate uncertainties in GFAS emissions and injection heights, ERA5 transport and convection, unresolved mixing, omitted sources, and the different lifetimes and removal processes of CO and aerosol (lines 243-248). The September results use 'consistent with' and 'supports' rather than definitive attribution (lines 462-482). The percentages are explicitly conditional on the two simulated regions, and Madagascar and other unsimulated sources remain possible.

4. Interpretation of Ångström exponent

Comment: *Ångström exponent alone cannot determine aerosol age*.

Response: We removed definitive age classifications. The observed Ångström exponents are treated as optical descriptors, with size distribution, mixing state, combustion conditions, humidification, processing history, and retrieval assumptions identified as alternative controls (lines 336-342 and 357-360). Section 4 retains the comparison with published biomass-burning ranges but states that Ångström exponent cannot diagnose aerosol age uniquely (lines 506-509).

5. Lidar-ratio uncertainty

Comment: *Using 75 sr for both days and wavelengths requires sensitivity discussion*.

Response: The transferred lidar ratio is now identified within the retrieval description as the principal systematic assumption affecting absolute extinction and layer AOD. The text specifies that 75 ± 9 sr was retrieved at 355 nm for the upper layer on September 25th and then applied to September 19th and 532 nm; it also explains that layer altitude and the one-versus-two-layer structure rely mainly on observed backscatter and are less sensitive to this choice (lines 161-165). The same qualification is incorporated into the synthesis of limitations (lines 518-520). We therefore addressed the requested sensitivity by narrowing the affected claims rather than adding new retrieval profiles.

6. WVMR uncertainty

Comment: *Large WVMR uncertainty weakens quantitative interpretation of moisture differences*.

Response: We now distinguish temporal variability from retrieval uncertainty and interpret the September 19th sub-layer pattern qualitatively (lines 370-372). For September 25th, the approximately 74% uncertainty of the lower-layer mean is retained, while the conclusion is limited to the robust contrast between the very dry upper layer and the moister lower layer (lines 387-391). The Discussion keeps the numerical values and uncertainty while avoiding a quantitative microphysical inference (lines 491-493).

7. CAMS aerosol composition

Comment: *CAMS OM/BC partitioning is model-dependent*.

Response: We agree. The Methods now state that CAMS assimilates total-AOD observations, while its species partition is controlled by inventories, transport, removal, and optical assumptions; nitrate is not represented among the components used here (lines 213-217). The Results explicitly identify the 50-60% OM contribution as model output and state that the AERONET comparison does not validate speciation (lines 275-276). This distinction is retained in Section 4 (lines 496-498).

8. Radiative-forcing claim

Comment: *No radiative-transfer or radiative-forcing calculation was presented*.

Response: We agree and removed radiative-forcing uncertainty reduction from the objective. The objective now concerns vertical structure, optical properties, moisture environment, and whether transport from selected regions is consistent with the observations (lines 101-107). Radiative forcing is retained only as broader motivation and a future application. The final paragraph states explicitly that the study does not calculate radiative forcing (lines 531-535).

9. Additional radiative-forcing literature

Comment: *The Introduction and Discussion could cite broader recent forcing literature*.

Response: We added a concise paragraph to the Introduction connecting source-emission variability, biomass-burning aerosol absorption, and the need to combine observations with models (lines 48-51),

citing Wang et al. (2025), Samset et al. (2018), Konovalov et al. (2021), and Kahn et al. (2023). The final paragraph of Section 4 now explains, without implying that a forcing calculation was performed here, how absorption, particle size, mixing state, and column loading affect inferred forcing (lines 531-535), citing Tiwari et al. (2023), Guan et al. (2026a, b), and Tiwari et al. (2026). Thus, all of the studies suggested by the referee are now incorporated at the points where they directly support the text.

10. Terminology

Comment: *Observed aerosol type, model species, and inferred source terminology were mixed*.

Response: Terminology is now tied to the evidence source. 'Elevated smoke' and 'polluted continental/smoke' are presented as CALIOP algorithm labels rather than chemical measurements (lines 139-141 and 315-319); OM, BC, sulfate, dust, and sea salt refer to CAMS model species (lines 279-282); and 'BB aerosol' or 'smoke' denotes an inferred source category supported by the combined evidence. FLEXPART CO is consistently described as an air-mass tracer (lines 246-248 and 453-454).

11. Scientific novelty

Comment: *The manuscript needed a clearer statement of what is new relative to earlier campaigns*.

Response: The knowledge gap and objective are now stated narrowly: vertically resolved observations remain sparse at Reunion in the eastern SWIO, several thousand kilometres downstream of the main source regions (lines 81-87). Section 4 compares the results with DeLiAn, SAFARI-2000, ORACLES, AEROCLO-sA, regional transport-corridor studies, and trans-Atlantic observations before identifying the specific advance as the vertically resolved characterization of aerosol layers reaching Reunion, including two simultaneous elevated layers with contrasting optical and moisture environments (lines 511-515). We do not claim novelty for long-range smoke transport itself.

We believe these changes preserve the observational contribution while aligning each conclusion with the level of evidence available and presenting the limitations as part of the scientific reasoning.

RESPONSE TO REFEREE 2

Manuscript egusphere-2026-3070

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

Dear Referee 2,

We thank you for the careful distinction between measurement quality and strength of inference. We adopted that distinction throughout the revision. Observations and retrieved quantities are presented before model-supported interpretation, the objective is limited to an observational case study, and the relevant qualifications are integrated where the results are discussed. Section 4 follows a continuous progression from demonstrated findings to model-supported interpretation, literature comparison, limitations, and implications.

Major comment 1 - Objective not achieved

Comment: *The study claimed to reduce radiative-forcing uncertainty but presented no forcing calculation.*

Response: We agree and removed that claim from the objective. The revised objective is to characterize vertical structure, optical properties, moisture environment, and whether transport from selected source regions is consistent with the observations (lines 101-107). Radiative forcing is mentioned only as motivation and a possible application of future work; the final paragraph explicitly states that no radiative-forcing calculation is performed (lines 531-535).

Major comment 2 - Circular CAMS reasoning

Comment: *Agreement in total AOD cannot validate species-resolved CAMS AOD.*

Response: We agree. The Methods now distinguish assimilated total AOD from model-determined species partition and list the relevant model assumptions (lines 239-259). In the Results, the AERONET-CAMS comparison is used only as qualitative temporal context; the OM contribution is explicitly model-derived and is not validated by the total-AOD comparison (lines 213-217). The same evidence hierarchy is retained in the Discussion (lines 496-498).

Major comment 3 - Observation versus inference

Comment: *Direct observations, retrieved quantities, simulations, and interpretation were interwoven.*

Response: The opening of Section 3 now defines the contribution of the observations/retrievals, CAMS, FLEXPART, and the combined interpretation (lines 250-255). In Section 4, the first paragraph reports the observed and retrieved quantities, including the numerical values and uncertainties (lines 486-495); the second paragraph then presents the CAMS- and FLEXPART-supported interpretation (lines 496-504). We implemented the requested distinction in this explicit paragraph order rather than adding visible subsections, which keeps the discussion readable while preserving the evidential boundary.

Major comment 4 - Model-dependent source attribution

Comment: *Individual-layer source statements should be consistently qualified.*

Response: Source language is now conditional throughout. The Methods define FLEXPART as a two-source experiment and explain that Madagascar and other sources were not simulated separately (lines 243-248). The Results use model-supported language and define the reported percentages relative to the two

selected simulations (lines 462-483). Section 4 states that these percentages describe represented CO air-mass transport, not aerosol-mass fractions or unique source assignments (lines 502-504 and 522-524).

Major comment 5 - Representativeness

Comment: *Two routine lidar nights cannot establish representativeness of the September episode.*

Response: We agree. The two lidar nights are described as illustrative cases selected from the available routine measurements. Section 4 states that they do not establish how frequently these configurations occur during September or over the biomass-burning season (lines 524-525). The implications paragraph identifies longer lidar records, systematic case selection, and aerosol-specific simulations as the work required to quantify representativeness (lines 530-531).

Major comment 6 - Alternative hypotheses

Comment: *Transport timing, hygroscopicity, mixing state, and injection-height uncertainty should be considered.*

Response: Alternative explanations are now incorporated where each inference is made. Ångström-exponent contrasts may reflect size, mixing, combustion conditions, humidification, or processing (lines 336-342 and 357-360); WVMR is used only to describe the moisture environment and not hygroscopicity (lines 370-372); and the lower-layer interpretation includes mixed transport pathways, humidification, combustion, and processing as alternatives (lines 387-391). Section 4 then synthesizes injection-height, meteorological, mixing, retrieval, and omitted-source limitations (lines 517-525).

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.

Abstract and novelty wording

Comment: *The phrase 'for the first time' should be qualified.*

Response: The broad phrase has been removed. The revised Introduction and Section 4 limit the contribution to vertically resolved characterization at Reunion in the eastern SWIO, far downstream of the principal source regions, including one case with two simultaneous elevated layers with contrasting optical and moisture environments (lines 81-87 and 511-515).

Knowledge gap after previous campaigns

Comment: *The residual observational gap was unclear.*

Response: The Introduction now explains that most intensive measurements focused on source-proximal regions, the southeast Atlantic, or the western SWIO, whereas vertically resolved observations remain sparse at Reunion in the eastern SWIO after several thousand kilometers of transport (lines 81-87).

AOD_{500}-to-AOD_{550} conversion

Comment: *The conversion introduces uncertainty.*

Response: The conversion paragraph now identifies the Ångström-law assumption, explains how uncertainty in the measured Ångström exponent propagates to the converted AOD, and treats the converted values as comparison quantities rather than independent measurements at 532 or 550 nm (lines 118-123).

Klett inversion and lidar-ratio sensitivity

Comment: *Dominant assumptions and sensitivity should be discussed.*

Response: The Methods now identify transfer of the 75 ± 9 sr lidar ratio as the main systematic assumption for absolute extinction and layer AOD, while noting that observed backscatter controls the layer boundaries and one-versus-two-layer structure (lines 161-165). The same qualification is incorporated into the synthesis of limitations (lines 518-520). We therefore addressed the referee's 'assess or discuss' alternative through explicit sensitivity discussion and qualified conclusions rather than new retrieval profiles.

FTIR averaging-kernel smoothing

Comment: *The effect of smoothing on comparison with FLEXPART should be explained.*

Response: The revised Methods explain that averaging-kernel smoothing makes FLEXPART and FTIR comparable at the limited FTIR vertical resolution but removes fine model structure; the comparison therefore cannot validate individual aerosol-layer boundaries (lines 240-242). The event comparison repeats this limit when interpreting the broad vertical distribution (lines 444-446).

FLEXPART inputs and uncertainties

Comment: *GFAS, injection height, and ERA5 uncertainties affect attribution.*

Response: These uncertainties are now integrated into the FLEXPART description: GFAS emissions and injection heights, ERA5 transport and convection, unresolved mixing, incomplete source coverage, and the different lifetimes/removal of CO and aerosol are all identified (lines 243-248). Their implications are synthesized in Section 4 (lines 521-524).

CAMS-AERONET metrics

Comment: *RMSE, bias, and sample size were requested in addition to Spearman correlation.*

Response: We removed the model-validation claim on which this request depended and, after re-examining the point, also removed the Spearman statistic. The AERONET-CAMS comparison is now explicitly qualitative and is used only to place the observed episode in the temporal context of the reanalysis; it neither evaluates model performance nor validates CAMS composition (lines 215-217 and 275-276). Because no quantitative performance assessment is retained, adding RMSE, bias, or sample-size metrics would no longer support a manuscript claim.

CAMS OM dominance

Comment: *OM dominance is model-derived.*

Response: This is now stated in the Methods (lines 213-215), Results (lines 285-286), and Discussion (lines 496-498).

Qualitative 'river of smoke'

Comment: *Either define the criteria or identify the term as descriptive.*

Response: We followed the second option. The earlier literature term is defined as descriptive in the Introduction (lines 57-60), removed from the title, and replaced in the Results by 'organized smoke-transport corridors'. The text states that the maps support plausible regional connectivity rather than a threshold-based source classification (lines 301-303).

CALIOP typing

Comment: *Classification uncertainty should be acknowledged.*

Response: CALIOP subtyping is now described as an algorithmic classification based on measured optical properties, location, and assumed aerosol models; mixed or weakly scattering layers may be misclassified (lines 139-141). The Results accordingly treat the CALIOP labels as regional context rather than direct chemical identification (lines 318-319).

Ångström exponent

Comment: *Humidity, mixing state, and combustion conditions also affect Ångström exponent.*

Response: We agree and now list these influences explicitly. The observed values are used as optical descriptors, not unique age diagnostics (lines 336-342 and 357-360).

WVMR uncertainty

Comment: *The moisture interpretation was too strong for the reported uncertainty.*

Response: The revised text distinguishes retrieval uncertainty from temporal scatter and treats the September 19th pattern qualitatively (lines 370-372). For September 25th, it retains the approximately 74% lower-layer uncertainty and restricts the interpretation to the clear dry-upper-layer/moister-lower-layer contrast (lines 387-391).

Discussion and conclusions

Comment: *Demonstrated findings should be distinguished from model-supported interpretations.*

Response: Section 4 now does this through a continuous sequence: observed and retrieved quantitative findings (lines 486-495), CAMS- and FLEXPART-supported interpretation (lines 496-504), comparison with current literature (lines 506-515), limitations (lines 517-525), and implications/future work (lines 527-535). This preserves the requested distinction without introducing short, disconnected subsections.

RESPONSE TO REFEREE 3

Manuscript egusphere-2026-3070

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.

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.