Reply to RC1

Dear authors,
Thank you for addressing the reviewers' comments and taking out the FLARE-GMM description. Please take more care in preparing the documents:
1. Track changes version and revised version are not identically. Probably, not the latest version was used to produce the track changes document. Please provide an appropriate track changes document. (e.g., references in L 38 or indices in eq 4)

Thank you very much for your comment, indeed some problems occurred with the track changes version, they should be corrected now.
2. Your references aren't in a good shape:
• Granados-Muñoz et al., 2015, Ansmann et al., 1992, Rao et al., 2002 are not given in the reference list. Probably there are more references not given. How can it happen? Please check carefully, that all references in the text appear in the reference list.

These issues should be addressed now with the latest version.
• Still sometimes Guzman and not Navas-Guzmán. e.g., L 119

Corrected in the new version of the article.
• References: Hu, Qiaoyun. 2018. "Advanced aerosol characterization using sun/sky photometer and multi-wavelength Mie-Raman lidar measurements." -> probably the PhD thesis, but not conclusive

Yes indeed it is the PhD thesis, more precision has been brought to this reference.
• L278 Petters and Kreideweis 2007 – A 2-author paper is not cited with et al.

Correction made.
• This paper is cited twice, once as discussion and once as published paper:
Veselovskii, Igor, Qiaoyun Hu, Philippe Goloub, Thierry Podvin, Boris Barchunov, and Mikhail Korenskii. 2022a. "Combining of Mie-Raman and Fluorescence Observations: A Step Forward in Aerosol Classification with Lidar Technology." Preprint. Aerosols/Remote Sensing/Data Processing and Information Retrieval. https://doi.org/10.5194/amt-2022-81.

Mistake corrected, it is cited only once now.

Further points:
• Still in the caption of the figures only a time and not a time interval is given. Please provide always the time interval.

Time intervals are now given in the caption of the figures.
• You might introduce subsections in section 3, one for each case study to facilitate reading the paper.

Yes this is a good idea, subsections have been added to section 3.

Review paper Miri et al.

The following are the minor corrections to the revised article

1) The next sentence is confusing. I assume that you want to say that you use this channel instead of the other wavelengths because the signal-to-noise ratio for the other wavelengths was low. However, what you have indicated is totally the opposite. Or perhaps I am missing something. Please clarify this point.

Lines 76-78:

"For this study, the aerosol elastic backscatter coeJicients (β) and the particulate linear depolarization ratio (PLDR) were computed at 532 nm from Mie-Raman observation (Ansmann et al., 1992) due to the low signal to noise ratio at this wavelength in comparison with the two others."

Thank you very much for your comment. Indeed in this passage, I made the confusion between low and high signal to noise ratio. We use the signal at 532nm because it has the highest signal to noise ratio. It has been corrected in the article.

2) The next sentence is not formulated correctly, as an increase in backscatter can occur due to changes in aerosol concentration, rather than solely due to hygroscopic processes related to relative humidity. Please clarify this point.

Lines 113-114: "In such cases, the elastic backscatter coefficient evolution can be attributed only to hygroscopic growth."

In general in the literature, the main hypothesis is that we try to identify homogeneous aerosol layers, the aerosol concentration is then not expected to change, and the change of elastic backscatter can be attributed only to hygroscopic growth.

3) In Equation 3, you have already included the normalization using the fluorescence backscatter, but you moved from equation 2 and 3 without mentioning this point. Please, correct this aspect.

More precision and one more equation have been added to introduce better the normalization.

4) I agree that the RH range of observation in which the hygroscopic parameter is obtained can have an impact on its values, however the results that are indicated are extremely high. So this point need to be clarify. How did you do this simulation. Here in this discussion of even in the manuscript should be shown how this calculation was done. I agree that the range of relative humidity (RH) observed when

obtaining the hygroscopic parameter can impact its values; however, the reported results appear to be extremely high. Therefore, this point needs clarification. How was this simulation conducted? The discussion, either here or in the manuscript, should detail how this calculation was performed.

Lines 247-248: "The influence of a shift in RH on γ has also been examined. For the case of 9 March 2021, when RH is decreased by 0.1, the corresponding γ estimation becomes 0.82, while an increase of 0.1 in RH results in a γ value of 0.23."

Here what we do is just take the case of 9 March 2021 and introduce manually a bias of minus 0.1 on RH (if RH = 0.7 at 1300m now RH = 0.6 instead) we then make the fit once again to find the new value of gamma, which is 0.82. We do the same for a positive bias of 0.1 and find a gamma value of 0.23. The idea was to evaluate the dependency of the gamma estimation with the uncertainties on RH which are expected to be in this range. Therefore, with this setup it is complicated to accurately estimate gamma. However this do not change the conclusions of this article since the determination coefficients of the fit are almost not affected by the changes of RH.

5) My comment regarding the revision of the references was not addressed. I can see that the citation of "Navas-Guzmán et al., 2019" still appears as "Guzmán et al." in most of the citations. Please, verify that all your references are cited correctly.

I understood this comment the other way around, the correction is done now.