

Interactive comment on “Biomass burning aerosols in the southern hemispheric midlatitudes as observed with a multiwavelength polarization Raman lidar” by Athena Augusta Floutsi et al.

Anonymous Referee #2

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This work presents the description with lidar technique of two case studies of lofted aerosol layers over an interesting location in the South Hemisphere, for its importance as observation point for southern mid-latitudes. The layers have been identified according to transport path and possible sources and described in terms of aerosol optical properties and potential for cloud condensation and ice nuclei. A short overview of the lofted aerosol layers observed for 1 year is also included. Although the relevance of the addressed topic can be guessed, the work does not present enough scientific quality to be included in ACP. My main criticisms are the following ones (not going into detail of many particular examples throughout the manuscript):

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Scientific significance I find that the approach and results do not represent a substantial contribution in the scope of the journal. The measurement and study of single cases can be of interest in the case that they are exceptional because of their nature, intensity, etc., or if a really deep, detailed description and analysis is given (including proper literature comparisons, etc.). The cases presented here do not fulfill those criteria, and considering that the authors have a longer database of the site, with 16 cases already identified, it would have been more appropriate if a full statistical study of the period had been performed, for example.

Scientific quality- “Introduction” The introduction is poor in several aspects. The justification of the importance of studying biomass burning aerosols, which variables are we interested and why, the relevance of calculating CCN and INP concentrations, etc., is completely missing. There is also no mention to the specific and clear objective of the work and the approach, and which is its impact and novelty. Also, the state of the art should be more complete with references on aerosol (maybe not only lidar) studies at those latitudes, or of the same aerosol type, etc. Some references from the existing networks (only mentioned in the following section without references) should also be discussed.

Scientific quality- “Experiment and Instrumentation” First, the title should also include “methodology”. The description of the lidar products does not include any mention to the uncertainties. For the analyzed cases, this is of crucial importance, because of the weakness of the detected layers. There is no explicit mention to several quantities that are later discussed in the manuscript, as AE and lidar ratio. The quality assurance is mentioned but only in a sentence that has no link with the rest of the paragraph. The part about CCN and INP calculation is barely explained, with several symbols not defined. There is only one sentence about sun-photometer, without mention to AERONET or why the level 1.5 is chosen. When writing about “TRACE”, the authors should have given some information on how the graphs 4 and 8 are created. Are these accumulated times referring of all the 27 trajectories of the ensemble? In this section,

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the target classification scheme is completely missing, as it is also some mention to the radiosondes or the fire detection algorithms as ancillary information. The method for calculating geometrical thickness of the layers is also not specified.

Scientific quality- Results There are several sentences that are too strong with so weak explanations and discussions. For example, "The higher values of volume linear depolarization ratio, with respect to the previous day, indicate the presence of non-spherical particles". Figures 3 and 4 are not clear enough for the unclear description given in the text (maybe, if there was different colour for the trajectory section that is over the land you could really distinguish different periods). The description of optical properties is unclear and sometimes not correct in the view of the graph, and there are several statements claimed to be true or evident without any explanation, discussion or reference. In general, the analysis is not discussed and contrasted with enough literature. Therefore, there are some crucial issues that are very poorly mentioned (e.g., the lidar ratio values are strongly dependent on the vegetation, combustion types, ageing, etc. It is stated in the manuscript, but no examples of literature are given, and no connection with the presented case). There are some sentences in this section that should be in the methodology (e.g. line 180). There are missing references and discussion also in the values and interpretation of INP. In the second study case, the 355 nm profile is missing with no explanation. This case has also the same weaknesses as for the previous one. The long-term analysis is very vague and does not give enough information. This section should have been the main part of the manuscript, with a complete analysis of all the cases and some statistics. There is no explanation on how the layers were identified, etc.

Scientific quality- Conclusions There are several discussions and references to literature that should not appear (explicitly) here, and that actually were not included in the results section. There are also statements in the conclusions that have not been shown in the results (e.g. lines 305-306). Presentation quality The general quality of the text is not good enough. There are many mixed tenses (future tense should not be

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used, and sometimes past and present are mixed in the same sentence referring to the same thing). The information is in general not well structured. Some of the paragraphs should have been divided into 2-3 (e.g. lines 66-82). The ideas are often in wrong order, there are many sentences out of place (e.g. lines 150-151 or 163).

In conclusion, I find that a deeper analysis should be done to present this work. Moreover, the manuscript must be re-thought and re-written to avoid the major previously mentioned problems.

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