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Atmospheric Chemistry and Physics Discussions

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 #1

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This paper presents two aerosol transport events over Punta Arenas observed by a 3-wavelength lidar. Aerosol transport in the middle and upper troposphere is a hot topic to improve our understanding of the climate system. Nevertheless, the study carried out in this article is rather sporadic and does not reflect the title chosen, which suggests scientific work over a large geographical area, important for studies of aerosol-cloud interactions. One should be clearer and write in the title that it is above Punta Arenas. The scientific information content of this article seems to me too light for publication at ACP. It should be given more relevance and better explain certain delicate points of the approach.

To give more content to this article, using the existing lidar dataset, it would be better to contextualize the study and to use complementary observations. These observations could be those of spaceborne measurements and one can think primarily to the CALIPSO and MODIS missions. Thus, several years of observations would be available, which would make it possible to have a more statistical study on a larger scale effectively covering the southern hemispheric midlatitudes.

Different emission, injection and transport conditions have been described in the scientific literature with the help of tools such as lidar instrumentation associated with the study of air mass back trajectories. It would have been interesting to have a more representative synthesis in the introduction of this article which could be based on the major international field campaigns. These different campaigns are also particularly good examples of more global approaches.

The algorithmic approach used requires a sufficient amount of aerosols in the atmospheric column. In particular, a sufficiently high aerosol extinction coefficient is required. For the values presented in this paper, the uncertainties are extremely high and make it difficult to make the data representative for conclusions. A simple error calculation already shows that the Ångström exponent are associated with a very high uncertainty which reduces their power of discrimination of aerosol types. It is essential to discuss all sources of uncertainty when analysing extremely low aerosol signature on lidar profiles. This discussion is totally absent from the paper, which significantly reduces its scientific value. There are no error bar on the profiles presented and they should be added in parallel with the discussion of uncertainties.

Points of more or less importance in the text (this list is not exhaustive as the article must undergo major revisions before it can be reviewed again).

Abstract

Define CCN and INP.

Section 2.1

Why present all the instruments when only PollyXT is used?

What justifies the size of the sliding windows? A sensitivity study should be presented. The cut-off frequencies of the low-pass filters used should be known. This type of filters also have bounces that can lead to signal distortion in low signal conditions.

Formulas (1) and (2) are not clearly explained. Each variable needs to be defined and the numerical values chosen must be better justified. The sentence " To reduce the uncertainties, a specific parameterization for smoke is under development." doesn't have much relevance if it's not made for the paper.

Section 2.2

In level 1.5 data, clouds may be present, especially high clouds of type Ci. This may be a difficulty, especially for the second case study.

The way in which back trajectory portions are selected, based on a single planetary boundary layer criterion may not be sufficient, especially for biomass fires where the injection heights can be well above the top of the planetary boundary layer.

Section 3

In Table 1, the altitude of the aerosol layers should be indicated.

Give the uncertainties on extremely low AOD. The very small aerosol extinction coefficients limit the interest of calculating an Ångström coefficient, it must be better justified using error calculations.

For the identification of the fires that should be shown, the fire product of MODIS could help.

The references given for the aerosol extinction coefficient of biomass burning aerosols to justify the low values are not sufficient. There are much higher values in the biomass

burning aerosol layers.

Explain why this is evident in the sentence " Below 2.4 km the spectral dependency of the light absorbing smoke particles is evident.".

How is supersaturation identified?

How is the 6% attributed to soot particles justified?

The role of soot as INPs is difficult to demonstrate in this study. If soot aerosols are young and aspheric, they may well serve as icy nuclei. The microphysical structure must be close to that of ice. In the presence of a coating, that doesn't work. Here, the aerosols are old and therefore have a coating.

For the second case study, what proves that there is no contamination by fine cirrus?

What is the purpose of the sentence " Thus in this specific case, we have no evidence that the aerosol did influence cloud formation, but nevertheless the particles may have the potential to do so if atmospheric conditions change.", knowing that the identified aerosols are at a much higher altitude than the clouds at 2 km AMSL?

In this second case study, it would also be good to show the fires.

In this case, the Ångström exponent seems calculated on the aerosol backscatter coefficient, one must be aware that it is often different from the one calculated on the aerosol extinction coefficient.

Section 4 It needs to be supplemented using interannual satellite observations.

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