Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-357-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Extreme levels of Canadian wildfire smoke in the stratosphere over central Europe – Part 1: AERONET, MODIS and lidar observations" *by* Albert Ansmann et al.

Anonymous Referee #2

Received and published: 12 May 2018

The paper by Albert Ansmann and coauthors documents a record-breaking observation of smoke aerosols above Germany and Czech Republic. The results reported in the Part 1 are based on active and passive remote sensing of aerosol properties using respectively two EARLINET lidars (Leipzig and Kosetice) and two AERONET photometers (Leibzig and Lindenberg). MODIS space-borne observations are used to illustrate the geography of Canadian fires and to provide support for the ground-based observations. The main outcome of the study is based on 3 days of observations and provides estimates of peak extinction coefficient, aerosol optical depth/thickness (AOT), particle mass concentration and accumulation mode effective radius.

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Discussion paper

The impact of biomass burning and associated pyroconvection on the stratospheric aerosol load is well known. However observational evidence of smoke aerosols in the stratosphere is rare and therefore valuable. The estimates of the smoke plume's optical and microphysical properties reported in the paper are potentially useful for constraining the models, as suggested in the Introduction. The Canadian wildfires during summer 2017 did indeed have an outstanding impact on the stratospheric aerosol and the presented high-quality observations should be documented in the peer-reviewed literature. However, I believe that the study would make a stronger point had it been consolidated with Part 2. Further, the scientific value of this observational study could be much enhanced, if the authors discuss more carefully their observation in the context of other outstanding aerosol events at northern midlatitudes. The comparison of stratospheric impact of the Canadian smoke event with that of Pinatubo tropical eruption is not totally appropriate as explained below. With that, comparisons with midlatitude eruptions and other biomass burning events are totally missing. I suggest that the authors invest an effort towards enhancing the scientific value of this study through consideration of the following remarks.

General remarks.

*It appears that the key statement of this study (as well as of companion paper) is that the observed extinction values were 20 times higher than after Pinatubo. To the casual reader this may suggest that the impact of these fires on stratospheric aerosol is actually much larger than that from the Pinatubo eruption. While this statement is simply misleading, the comparison as such is not correct either. The authors compare peak extinctions of a fresh and compact patch of smoke with that of an older well-mixed volcanic plume spreading over a wide range of altitudes as it was observed by lidars above Europe in 1991-1992. A direct comparison of plumes' optical properties would be justified had a Pinatubo-sized eruption occurred at northern midlatitudes. I believe that before pointing out the superiority of the Canadian smoke peak extinction and AOD over those of Pinatubo, the authors should carefully discuss the aerosol source

locations (upwind at midlatitudes versus tropics) and aerosol transport processes (fast zonal transport within LS jet versus slower meridional exchange and mixing). The temporal extents of the observed stratospheric perturbation due to fires (presented observations cover a few days only) and Pinatubo (several years) should also be discussed. Finally, it would be more pertinent to compare the stratospheric AOD (0.6 vs 0.2-0.3) and not the peak extinctions.

* I am not sure to understand the reasoning for separating this study in two parts. Both parts are centered around lidar soundings and both of them incorporate collocated or nearby AERONET measurements. The distinction seems to be made on the retrieved parameters, e.g. volume particle size distributions in part 1 and mass distribution in Part 2. I think the reader would much appreciate having all the parameters from a time-limited observation in a single article.

* The structural organization of the article should be reconsidered. The introduction appears too lengthy, whereas the discussion section is totally missing. I suggest to introduce the discussion section, which would include some parts of the Introduction and a careful stipulation regarding smoke vs Pinatubo comparison. Section 3 (observations) would be much easier to follow had it been structured by the observation sources.

* Coming back to the extreme extinction and AOD observed by Leipzig lidar. While there is no reason to question the data, I wonder if similar levels of aerosol abundance were observed by the neighboring lidars stations, e.g. Cabauw, Garmish, Hohenpeisenberg, etc. And what is truly puzzling is why the authors do not compare the extinction from two Polly lidars that operated in close vicinity. Is it possible to invert the Kosetice 532 nm data and present the time curtain of extinction rather than attenuated backscatter?

Specific remarks

P.3, The second paragraph should belong to the discussion section

P.4, l.6. Typo in Leipzig longitude

P.5, l.30. “Similar indications . . . were observable. . .” An appropriate reference is missing here.

P.6, l.5. “Figure 3 provides an overview of aerosol layering over Central Europe. . .” To me this is an example rather than an overview.

P.6, l.20-22. Why mention global (general?) circulation models here? HYSPLIT is a Lagrangian trajectory model. Also, how does the pyrocumulonimbus convection influence the long-range transport? I think, the caveats of trajectory analysis for smoke tracking should be explained more carefully.

P.6, l.23. “. . .at tropospheric as well as stratospheric height. . .”. The highest-level trajectory is initialized at 12000 m, which is just around the tropopause on 21 August. The smoke was observed as high as 15-16 km above Kosetice so it would be useful to show trajectories initialized at higher levels.

P.7, l.33-34. Here the authors refer to the period of smoke observation by the instruments involved in this study either as the “smoke period” or “smoke event”. Such a terminology is not quite correct because the actual period when the smoke was observed above Europe and elsewhere in NH spanned several months.

Figure 1. MODIS detected powerful wildfires at different locations across Canada, particularly in British Columbia and Northwest territories. Which cluster of fires has caused the extreme levels of smoke in the stratosphere? Could anything be inferred from trajectory analysis?

Figure 2. The photos serve a nice illustration of the stratospheric smoke. However, in order to place them in a scientific context, one should at least provide the azimuthal direction at which these photos were taken.

Figure 3. The plot could be much improved by adding the tropopause height curve (or perhaps even a drawn line). This would help distinguishing tropospheric and strato-

spheric smoke plumes.

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