Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-655-RC3, 2018

Atmospheric Chemistry and Physics Discussions
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Interactive comment on "A study of long-range transported smoke aerosols in the Upper Troposphere/Lower Stratosphere" *by* Qiaoyun Hu et al.

Anonymous Referee #3

Received and published: 17 August 2018

The paper provides unique measurements of an extreme event of smoke advection from Canada to Europe. Smoke could be observed within the Troposphere as well as in the Stratosphere. I consider the measurements and the resulting data as very valuable and of interest for the scientific community and the topic fits well in the scope of ACP.

However, in my opinion the paper tries to cover too much topics at once (optical properties of smoke, microphysical properties, source analysis, change of depolarization with aging time by using Calipso data, radiative transfer calculations, estimating of heating rates, temperature changes in the stratosphere etc.). Thus, the paper is partly confus-

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ing and for some of the topics the proper fundament needed for the conclusion drawn are missing.

I therefore recommend first to focus on your key expertise and present only the unique measurements, which are already shown and exploit as much as possible (optical and inversion results). Of course you should also include the satellite data for source analysis. However please focus on the lidar measurments in France.

Then, in another paper(s), the other topics could be covered (i.e. radiative transfer calculations and heating rates etc.) Especially the discussion of the impact of the smoke plumes need much more detail and evidence, as for example it is not shown nor discussed that temperature increases in the stratosphere at a certain height are not caused by the complicated upper atmospheric circulation including tropopause foldings etc. I therefore recommend for the second paper to include some expertise of the upper atmospheric dynamics and probably some modelling to show and prove the impact of the smoke layer with its possible warming on the overall general condition of the UTLS region above Europe.

I also think that the use of Garrlic/Grasp radiative transfer code to obtain radiative impact must be explained much better. Currently, it is not understandable how the calculations are performed.

From the current version, I also doubt for the evidence of the increase in smoke depolarization with aging time. I think the use of Calipso data for such a study is again a topic of its own. Uncertainties in the Calipso retrieval (see comments in pdf) should be discussed. Even more important, other influencing factors as the relative humidity, the altitude of the smoke with respect to ground level but also tropopause should be investigated. Among others influencing factors (fire source, burning types).

I therefore recommend major revision with the recommendation written above.

Find other comments below and as pdf-comments in the supplement.

Paper structure:

-The abstract is too long. Please shorten.

-The introduction is in my opinion a loose sequence of different paragraphs and therefore not constructive. Please revise and make it more focusing on your topic. E.g.: have such events been reported earlier? Is this the first time? . . .

-Several facts concluded in the observations section are again raised in the discussion. I think you should shape your paper. Either describe your observations only, and then make a discussion in a separate section or make conclusions also in the observations section but then discuss only new issues in the discussion section.

Terminology

-I would recommend not to use “upper troposphere/lower stratosphere UTLS” as a standard the term. This historical term covers all altitudes between 5 and 30 km and thus does not make clear that a significant portion of the smoke you detected is above the local tropopause. Please feel free and state, that there is stratospheric smoke as well as smoke in the free troposphere. In most times you anyhow refer to stratospheric smoke with your statements. . .

Major scientific remarks:

-What about the uncertainties of Calipso. For example for the PLDR, the particle backscatter coeff. is needed which is in turn calculated with a Klett-like approach, i.e. a a-priori lidar ratio. Thus the questions arises, which aerosol type was classified by Caliop and which lidar ratio was used to obtain the PLDR and is this correct for your aerosol type of investigation.

-Depolarization: The molecular depolarization ratio depends on your filters used in the lidar. Are the theoretical values you stated valid for your system? And can you neglect temperature effects? Which molecular depol ratio value did you use for the PLDR calculation? The measured one or the theoretical one?

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-Radiative transfer: The methodology explanation is too short. It is not reproducible how you performed the radiative transfer. Which parameters did you use as input? Which are constrained by the algorithm? and so on... thus, either you introduce a much more detailed description of this method and the paper gets longer or you shift these calculations to a second paper.

-Inversion: Is it useful to perform an inversion when having only 3 elastic signals? I mean lidar ratio is not an independent variable in your case...please discuss this! What justifies using a sphere model when particle size is small.

-Increase in stratospheric temperature. Your explanations are not convincing concerning the temperature increase. Did you consider also all other processes in the UTLS? May this only be normal variability? How long do you observed a temperature increase? So please add more detail or shift to another paper (I think this is a topic alone).

-Depolarization ratio vs. aging time: The provided graphic and literature review does not convince me of the given causality, please also investigate the RH, the height etc. vs depolarization ratio.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-655/acp-2018-655-RC3-supplement.pdf

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