Dear Editor!

Thank you for your comments to further clarify several critical points. We worked on this and added the requested information in the revised version of the article.

Our answers in the step-by-step reply in BLUE.

There is clearly a disagreement between the authors and Mike Fromm on the validity of the inferences in this paper. To resolve these I require a number of points to be clarified.

We provide answers to all of the points and improved the manuscript accordingly!

The first point that I'd like clarified is the assertion very early in the paper (p.2, l.13) that the AOT of the Raikoke volcanic aerosol cloud should have been of order 0.005-0.01 at 500 nm in the winter half year over the Arctic. For the given references, Kloss et al showed 0.01 – 0.02 at high latitude for 675 nm, my observations at 355 nm and 52°N showed AOD above 12 km of 0.01-0.02, Cameron et al do not show AOT at 500 nm and Gorkaiyvi et al show the same measurements as Kloss. A minor point maybe but where do the authors get the 0.005-0.1 range? Is this based on previous eruptions?

We think that a compact Introduction (of a paper with focus on wildfire smoke over the North Pole region) is not the right place for the necessary and then quite detailed discussion of this aspect. So, we just say in Section 1 that a detail discussion is given in Sect. 3.

In Sect. 3, all the details regarding the potential Raikoke impact (max AOT, and decrease of the impact with time) to the stratospheric aerosol burden over the North Pole region are given within three paragraphs.

Here, we also introduce the reasonable hypothesis that the smoke dominated in the height range from 6 to 12-13 km height, and the volcanic sulfate aerosol may have dominated above 13-15 km height up to 17-19 km height.

Afterwards, we mention this potential smoke-sulfate layering several times, on pages 11, 14, and 15.

More important in this context is the definition of AOT. A meaningful comparison between different AOT values requires them to be over the same height range. According to figs 11 and 12, you calculate AOT from 6 to 15 km, with the maximum at 9 km. There is nothing scientifically wrong with this of course, but much if not most of this aerosol is in the troposphere and comparison with stratospheric AOT measurements is not meaningful. Kloss for example presents sAOD, the integrated AOT above the tropopause.

We avoid any comparison of UTLS AOT and pure stratospheric AOT. We also avoid to state that the Siberian smoke formed a stratospheric layer. We mention, throughout the paper, that the smoke contributed to an UTLS aerosol layer.

Indeed it would seem to me that much of the dispute between the authors and Mike Fromm comes from the altitude at which the measurements are made. The clear evidence of smoke particles comes from the measured lidar ratios, but in figs 11 and 12 these are only shown between 7 and 12 km. Is there evidence that the layer above 12 km was actually smoke?

No! By using the smoothing lengths of 2000 m, lidar values at 12 km (maximum height shown) are based on signals measured up to 13 km height. So, there is no evidence for smoke occurrence at heights above about 13 km height.

The smoothness of the profiles could arise from the considerable smoothing used in the data processing, and hide the possibility that the aerosol layer was not homogenous. Could the Raikoke aerosol be above the smoke layer? (That would help with the conceptual difficulty many readers will have of the smoke self-lofting to 17 km which is above the region where it was geographically confined).

Yes! We agree with this argumentation and state that now at several places (page 11, 14, and 15).

The authors comment on fig. 4 that the blue colours between
13 and 17 km from 45 to 60 N 'may indicate Raikoke – related volcanic aerosol'. Like Mike Fromm, I would contend that it's 'very likely', not 'may'.

We changed that accordingly, in the figure caption as well as in the text. On page 8, we now state that CALIPSO observations indicate smoke up to the tropopause. We removed any impression that the smoke was already in the stratosphere above the Siberian fire areas.

Your estimate on p.13 of the Raikoke volcanic aerosol contribution does not consider the possibility that there were two or more aerosol layers (e.g. smoke below the tropopause, smoke + some Raikoke aerosol 1-2 km above the tropopause and volcanic aerosol above). The 10-15% contribution to the extinction coefficient at 532 nm from volcanic aerosol applies (as I understand it) to the entire layer from 6 km upwards, so it is not at all surprising that it is dominated by the smoke.

You are right! Now we present two different approaches or ways to estimate the Raikoke sulfate contribution of 10-15%, on page 14. We think that the revised version of the manuscript meets properly all the points Mike Fromm criticized. The Raikoke sulfate aerosol is no longer ignored.

I don't understand how the self-lofting argument gets smoke into the stratosphere, if the response in the troposphere is to produce plumes. But there is continual exchange of air between the troposphere and the bottom couple of km of the stratosphere so over a couple of months it is entirely possible that some smoke originally in the upper troposphere would be found in the lowest layers of the stratosphere. However, such mixing would not lift the smoke to 17 km, which is well above the extratropical tropopause.

On page 9, we mention that smoke reaching the upper troposphere can easily enter the stratosphere over the High Arctic: When these lifted smoke plumes are afterwards advected northward they can easily enter the lower stratosphere over the High Arctic because the tropopause is no longer a barrier for airmass exchange between the upper troposphere and lower stratosphere.

Such a statement should be sufficient as an argument that we found smoke up to 12 or 13 km height with the tropopause at 8-10 km height.

The smoke had time in August and September 2019 to get further lifted up to 12-13 km height, slowly, but steadily. We observed smoke up to 14 km height at Leipzig on 14 August 2019. A probably similar ascent (slowly and steadily) was observed after the Canadian fires in 2017 where the smoke, injected by pyroCb to 11-13km height, was finally found up to 23 km height.

On page 10 we state: (1) The self-lifting hypothesis is of key importance in our attempt to relate the MOSAiC UTLS smoke observations from October 2019 to May 2020 with the strong fires in Siberia in July-August 2019. We leave open here in which way the smoke was further lifted into the lower stratosphere. Khaykin et al. (2018) observed smoke layers, injected by pyroCb activity into the tropopause region over Canada in August 2017, which further ascended by 2-3 km per day during the first days after injection. After a few weeks, theses layers were found up to 23~km height and distributed over large parts of the northern hemisphere (Baars et al., 2019, Torres et al., 2020).

Fig 4 – please give date and time of Calipso overpass in the caption

We improved this.

Besides all these improvements, we added the following points:

- **On page 8, we had to mention, that pyroCb activity was absent over Siberia in July and August 2019. Peterson et al. (2021) stated that pyorCb activity was responsible for the Siberian smoke in the UTLS height range. This is wrong. And this was the main reason to introduce the self-lifting concept.**
- **In Section 3, and especially in Section 3.1, we introduce our new paper, Ansmann et al. (2021b), on the 'misclassification of Siberian smoke as Raikoke sulfate aerosol in 2019 by the CALIPSO aerosol typing scheme'. This paper was submitted to Frontiers Earth Science end of August, and is now under review. The paper is a contribution to a wildfire-smoke-related Special Issue. In this paper, we present and discuss the lidar observations of smoke layers at Leipzig performed on 14 August 2019 in detail.**
- **Finally, on page 16, in Sect. 4.2, we compare the Siberian fire smoke amount with the ones of the Canadian fire event in 2017 and the Australian smoke event in 2019-2020. We write:….. and yield 0.2 Mt of smoke as a guess for the mean value of the smoke aerosol load over the Arctic during the winter half year 2019-2020. The overall Siberian smoke particle mass injected into the UTLS height range of the northern hemisphere may have been a factor of 2 higher. For comparison, the initial particle mass injection of the record-breaking Canadian pyroCb smoke event into heights of 11-13 km in August 2017 was about 0.3 Mt (Yu et al., 2019) and the rather strong Australian bushfires in December 2019 and January 2020 caused a stratospheric smoke particle mass of the order of 0.5-1 Mt (Peterson et al., 2021).**