Dear Reviewer!

We thank You for reading the manuscript and for your comments and remarks! We agree with your criticism concerning our tropospheric self-lofting part (observations vs simulations). The discussion was not convincing and straight forward.

As a consequence, we re-wrote the entire section on tropospheric self-lofting of smoke (now section 5, section 4.4 in the submitted version). We skipped the simulation part in Sect. 5. CALIOP observations (and one Leipzig lidar observation) are presented only in this revised section. We re-analyzed the entire CALIOP data set over central eastern Siberia and the Arctic from 22 June 2019 (at that day the Raikoke volcano erupted) until October 2019 (to have an overlap with the MOSAiC Polarstern lidar observations that started in the end of September 2019). Together with new literature on stratospheric smoke (Knepp et al., 2022, Xian et al. 2022), corroborating our hypothetical approach, we believe that we have now several indications that self-lofting contributed to the upward transport of smoke within the troposphere. However, we agree, it remains a hypothesis! And that is emphasized several times in the revised manuscript.

However, the topic is a very new aspect in atmospheric science that clearly deserves publication. Even if we are in trouble to demonstrate that self-lofting played a major role in vertical smoke transport processes in the troposphere, our simulations, the impact study and uncertainty analysis, and the applications to Canadian and Australian fire events are worth enough to be published. No doubt!

The first part of the article (Sections 1-3), and also the discussion of comparisons of observations vs simulations for stratospheric ascending Canadian and Australian wildfire smoke layers (in Section 4) remained widely unchanged with respect to the submitted version.

Now to the item by item response:

Our answers are in BLUE!

Significantly changed text or added text is given in BOLD in the revised version of the manuscript.

Before we start, please note that the title of the article has been shortened and we added a co-author (Fabian Senf), he is an expert for atmospheric dynamics and modeling.

The manuscript by Kevin Ohneiser and coauthors addresses the solar-driven lofting of wildfire smoke plumes in the troposphere and stratosphere using ECMWF radiation transfer scheme with different parameterizations and satellite observations using CALIOP and MODIS instruments. The ascent rates of smoke plumes produced by Canadian, Australian and Siberian wildfires derived from CALIOP observations are compared with the calculated ascent rates from radiative transfer simulations. The main goal of the study, as stated in the abstract, is to demonstrate that the radiative heating of intense smoke plumes is capable of lofting them from the free troposphere up to the tropopause and into the stratosphere without the need of PyroCb injections.

We changed a bit the main goal. The main focus is now on tropospheric self-lofting only, i.e., from the injection heights of 2-6 km to the tropopause. Lofting of smoke from the tropopause to greater heights within the stratosphere is already well documented and well described in the literature (as described in the introduction).

After a detailed description of the modeling setup, sensitivity tests and uncertainty discussion, the authors demonstrate in Fig. 11 that a 2.5 km- thick smoke plume with a realistic BC fraction of 2.5% and a very large AOT above 2 should rise from 3 km altitude into the lower stratosphere in two weeks. However, the analysis of CALIOP observations of tropospheric smoke from Siberian wildfires in the following section does not provide any support for the cross-tropopause transport of aerosol plumes rendering the main goal of the study unachieved and casting doubt on the usefulness of the simulation results.

Yes, this is true! Therefore, we changed the strategy of our study. As mentioned above, the study is now based on the CALIOP data set (day by day, scene by scene) from the Raikoke eruption on 21-22 June 2019 until the end of October 2019 with focus on smoke layers (pyroCb-related, self-lofting related) but also on features produced by Raikoke sulfate aerosol. We concentrated on the high northern latitudes >65°N. We found a number of fingerprints and arguments for self-lofting in the upper troposphere in line with our simulations. But we do not show any simulation here. Our 1D simulation model cannot be used to simulate 3D-air motions in the troposphere. We mention that in Section 3.4. However, even after listening a number of convincing arguments in Section 5, it remains a hypothesis that self-lofting really occurred. This is emphasized frequently!

More specifically, there are several major issues as follows.

The description of the satellite instruments, data versioning, measurement uncertainties and the approach to data treatment is totally missing in the manuscript.

We agree! However, we exactly provide similar information on CALIOP and MODIS and other instruments, data use, etc. as in our foregoing papers on Arctic smoke (Ohneiser et al., ACP, 2021) and Australian smoke (Ohneiser et al, 2022). And that was acceptable. CALIOP and MODIS are so well known that such a detailed introduction of these instruments (including, data handling, quality levels, uncertainty analysis, etc) is no longer needed to our opinion. And all necessary information, needed in this self-lofting paper, is given in Section 4.1, i.e., how we got the layer center heights (in the case of the presented Canadian and Australian smoke layers) and how we got the AOT values from CALIOP observations and (now also in more detail, from MODIS data).

As far as I understood, the authors used CALIOP quicklooks to derive the layer thickness and mean attenuated backscatter, from which the AOT is calculated using an arbitrarily chosen factor of 1.5, which should account for the light attenuation. While the derivation of layer thickness from the quicklooks may be deemed sufficiently accurate (although for compact stratospheric plumes only), it is unclear how the authors derived the layer mean backscatter from the images. Was it done by reading the colors of each individual pixels and using the color bar to retrieve the values? If so, the uncertainty of such estimates might be unacceptably high and I wonder how such estimates would compare with those by Kablick et al. provided in Fig. 16.

The factor of 1.5 accounts for the probably too low lidar ratio of 65 sr (the smoke lidar ratio in the CALIOP data base), because we observed lidar ratios of 90-100 sr for the Australian smoke (Ohneiser et al., 2022). This is now correctly mentioned in the revised version. This was not well described in the submitted version.
The use of quicklooks of attenuated backscatter (color plots) or the use of downloaded particle backscatter values doesn't matter much. At the end, the obtained AOT values may differ by about 5-10%. That is our experience. Kablick also used backscatter coefficients, multiplied by the smoke lidar ratio, and then integrated the obtained extinction values from layer base to top.

Constrained by the CALIOP AOT from Kablick et al., the simulated ascent is nowhere near the observed one and the authors opt to constrain the simulation with MODIS total AOT data (ignoring the tropospheric aerosols), which is substantially higher than both the CALIOP-derived AOT and, what is particularly puzzling, much higher than the estimates by Ohneiser et al. (2020), their Fig. 5b, reporting the lidar-derived AOT@532 of 0.1 – 0.3 for the Australian smoke plume in late January 2020 (which would be consistent with Kablick et al. data in Fig. 16). The authors thus seem to deliberately ignore their own observations for the sake of reproducing the observed lofting in the simulation.

The observations in Ohneiser et al. (2020) were done at the edge of the smoke-filled vortex and the AOT values were therefore smaller. The MODIS data overestimate the UTLS AOTs before 17 January (this is shown now in the new, more compact Figure 13) and after 17 January the values became reasonably small (after subtracting a typical marine AOT value). Then, the CALIOP AOTs of Kablick et al. and the MODIS AOTs compared reasonably well.
In the revised version, we mention that a tropospheric marine background AOT of 0.05 was subtracted from the observed total smoke MODIS AOT. The tropospheric AOT contribution was cleatly underestimated until 16 January when using a tropospheric AOT of 0.05.
As a consequence, the simulations based on MODIS AOTs start on 17 January 2020.

Section 4.4 and Fig. 17. The Siberian tropospheric smoke plumes show rather complex vertical structures, whereas the determination of the aerosol layer vertical boundaries (critically influencing the AOT estimate) appear to be somewhat too arbitrary. Personally, I do not see any significant lofting for the both cases shown in Fig. 17. It rather appears that the smoke was found in the UT from the very beginning, which would point to the PyroCb-driven vertical transport.

Section 4.4 including Figure 17 is removed now, and substituted by the new section 5.

Discussing the Siberian smoke plumes, the authors state that “fractions of this plume must have reached the tropopause and later on the lower stratosphere” without providing any supporting observations, and the only reason I can possibly think of is the absence of such observational evidence. Moreover, the simulations based on an assumption of the persistent Gaussian vertical shape of the layer (which is obviously not the case here) show even weaker lofting than what is inferred from CALIOP quicklooks. I also wonder why the simulation was not extended further in time (using, e.g. 15% daily AOT decrease) to provide at least the modeling support for the potential lofting up to the tropopause level.

Again, Section 4.4 is removed, and substituted by the new section 5. It makes no sense to compare simulations with observations in the convective and turbulent troposphere. We explain why our simple ECRAD model cannot be used to simulate 3D-air motions in the troposphere in Section 3.4. The assumption of the real shape structures or the assumption of a Gaussian shape profile almost lead to the same results.

All in all, we think that the manuscript is in a good shape now. We also believe that we could convincingly show that the 2019-2020 aerosol conditions as observed with CALIOP and the MOSAIC Polarstern lidar in the UTLS height range can only be reasonably explained by considering smoke self lofting as a significant process to transport smoke into the tropopause region. To summarize again the main 3 arguments that point to a strong role of smoke self-lofting are:

(a) The observed UTLS AOT (caused by smoke and sulfate) was much higher (0.1-0.2 at 532 nm) than the expected sulfate AOT (<0.025) originating from the Raikoke eruption. However, the observed low depolarization ratios pointed to a minor impact of pyroCb-related smoke lofting. Consequently, another process is needed to transport smoke upward, towards the tropopause.

(b) By carefully expecting all CALIOP observations at high northern latitudes from June to October 2019, we detected a near-tropopause aerosol layer, clearly visible from mid July to mid of August (in the period of strongest fires over Siberia). The occurrence of this layer is in line with one of the main simulation results. The lofting rate decreases with height in the upper troposphere, and shows a minimum at the tropopause. In case of a steady upward transport of smoke this leads to smoke accumulation at the tropopause.

(c) The found lidar ratio characteristics (355 nm lidar ratio is much smaller than the 532 nm lidar ratio, lidar ratio at 532 is typically larger than 70 sr indicating strongly absorbing aerosol particles) unambiguously points to smoke as the dominating aerosol type in the UTLS aerosol layer. Such a unique spectral slope of the lidar ratio has never been observed by the lidar community for any other aerosol type.

Finally, we should add that we presently have to review a JGR manuscript on global aspects of ascending BC-containing aerosol layers and these authors find similar results concerning self lofting in the troposphere and stratosphere as we discuss in our manuscript. One highlight is that the used Earth System Model indicates a strong increase in UTLS smoke mass concentrations (by 50%, annual mean increase) in the 8-22 km height range of the northern part of the Northern Hemisphere when BC absorption of solar radiation is considered in the simulations. As these authors pointed out in the introduction, their JGR article was motivated by our self-lofting studies presented in Ohneiser et al. (2021) on MOSAiC observations and by this ACPD manuscript (Ohneiser et al., 2022), and by other recent papers dealing with self lofting processes.