Review of Das et al. "Pyrocumulonimbus Events over British Columbia in 2017: The Long-term Transport and Radiative Impacts of Smoke Aerosols in the Stratosphere".

The manuscript by Sampa Das and Coauthors addresses a topical issue of the physical properties and behavior of PyroCb smoke plumes in the stratosphere by simulating their spatiotemporal evolution using GEOS general circulation model with prognostic aerosol module. Compared to previous modeling studies of BC smoke in the stratosphere, Das et al. perform a careful estimation of the smoke optical properties via constraining the simulations of smoke evolution by available observations. In view of the emerging realization of the effects of extreme wildfires on the stratosphere, the accurate information on the radiative properties of stratospheric smoke, in particular the SSA, is certainly valuable. The paper is well organized and fluently written, the conclusions are substantiated by the presented material, which altogether renders the paper suitable for ACP. There are however several issues that require important revisions.

My major concern is that the authors take no account for the underlying clouds that could significantly enhance the heating of stratospheric smoke (e.g. Boers et al., 2010). This issue is not even mentioned in the paper, which is surprising given the effort the authors invest to accurately constrain the smoke optics. This shortcoming should lead to underestimation of the diabatic ascent rate, which actually seems to be the case judging from Fig. 4, where the observed plume top appears 2 km higher than the simulation after T+20 days. Obviously, a global-scale simulation including the clouds would substantially complicate the modeling experiment. However, it would be of great value for this study if the authors estimate how much a convective cloud extending up to the tropopause layer (which were widely encountered in the ASMA region during that time) would affect the heating of the stratospheric smoke plume.

Specific remarks

Figure 2. It would be interesting to see the simulated and observed profiles in the same plot

Figure 3a. Would it be possible to display the peak values from Leipzig observation?

Figure 6. The amount of information conveyed by this figure does not justify 40 panels. I believe most of them could be moved to supplementary material. I also have a concern on how the OMPS-LP data are presented in this figure: there appears to be a strong boxcar smoothing in the zonal direction, which was applied to mask the gaps between the orbits. It might be a better solution to aggregate OMPS-LP data over 3 days to avoid smoothing across these gaps.

Figure 7. The enhanced SAOD in the Arctic after day 150 is obviously due to PSCs. This should be mentioned in the text and/or figure caption. Alternatively, the PSCs could be removed using the same approach as in OMPS-LP V2.0 retrieval, which would enable a better comparison with the simulation.

p.10, l. 340. "… the model overestimates the background aerosol extinctions…". First, it is not totally obvious from Fig 6a that the model actually shows significantly higher extinctions. Secondly, the strongly enhanced extinction in the tropics is most certainly due to TTL cirrus and not due to aerosols.

p.10, l. 344 – 348. This sentence contains several statements that are either overly general or not entirely correct. The references provided are not quite relevant too. I would suggest to omit this sentence.

P.11, l. 355 – 365. I disagree with the interpretation provided. The BC smoke plumes were mostly contained in 3 bubbles confined by smoke-charged vortices (SCV), of which only one (Vortex A in Lesterlin et al., 2020 https://acp.copernicus.org/preprints/acp-2020-1201/acp-2020-1201.pdf) ascended to 23-24 km, whereas the two others (B1 and B2) ones did not rise as high. The vortex A was already at 19 km while overpassing Europe (Khaykin et al., GRL , 2018) and by the time it arrived to the Asian region, it was already at 21 km, i.e. well above the Asian anticyclone (Lesterlin et al., 2020; Bourassa et al., JRG, 2019). Hence, the AMA could not have played much role in terms of the upward transport. Likewise, the cloud scavenging is of no relevance here as the A bubble is well above the TTL clouds. What could be the actual reason why the model falls short reproducing the diabatic rise is the absence of clouds in the simulation.

p.11, l. 375 – 377. Given the large extent of the initial cloud, it is unlikely that it was located in between the 3-slit swaths. I believe, a more important reason why OMPS-LP doesn't get the early cloud is the saturation at extreme extinctions.

p.11, l. 378. Same issue here. The injected smoke cloud is already above the tropopause on 14 August (Fig, 5b) therefore since the cloud screening was applied for clouds below the tropopause (Sect. 2.2), it should not have discarded this plume.

p. 11, l. 381 and p.12, l.392. Total AOD would imply the entire column. Do you mean stratospheric AOD here?

p.12, l.387. rather be said "…there were no strong volcanic eruptions or PyroCb events…"

p.12, l.404. The sentence needs revision, cf. previous remarks.

p.13, l.423. It would be very useful to provide the value of global-equivalent monthly-mean RF to be compared with that of Australian bushfires (Khaykin et al., 2020).

p.13, l.425 – 427. The gaseous composition of the stratospheric PyroCb plumes is vastly different from the background values (strongly enhanced H2O and CO, depleted O3), which may potentially have an important impact on the plume heating. This should be discussed in a more careful way, i.e. how the alteration of these gases could affect the plume rise.

Technical remarks

p.4, l. 114, In August

p.6, l.196. The reference to Chen et al. is missing in the bibliography.