Referee #2

The authors would like to thank Reviewer 2 for the questions and suggestions made that help us make the paper overall more understandable and accessible for the Reader. Below, each comment (black) is addressed (blue) in detail, indicating the changes we have made on the manuscript.

1) Page 6 starting from Line 15: ": : :in the whole NH >40N mid to end of August: : :" I am not sure I understand Fig. 1B, is there any data shown from mid to end of August 2017?

We agree that this sentence is confusing, because the 'mid-to end- August bin' is actually missing for the SAGEIII data set with no measurements. We have changed the sentence to

"A strongly enhanced aerosol extinction signature in the SAGE III data set is first visible in the beginning of September, in the whole NH >40N (Fig. 1B). Previous studies (Khaykin et al., 2018; Ansmann et al., 2018; Haarig et al., 2018) have shown increased aerosol extinction values associated with the fire plume from mid August."

2) "The aerosol signature descends with 2 mm in altitude per second" Could you please draw a line on Fig.2B to show the decline slope? It is hard to tell by eyeball. "Hence, the descent of the aerosol is due to sedimentation" Well, I believe sedimentation plays an important role. However, what about the longitudinal dilution and cross-latitude transport from higher latitude to lower latitude (not necessarily happen in ASM region)? How do those affect the 5 km/month rate derived in the manuscript?

We added a decline slope (Fig. 1B) as requested.

We have changed the paragraph accordingly:

"To exclude most cloud features and also background aerosol in Fig. 1B, we focus on the aerosol extinction region from ~0.6-0.9 km-1 for our analysis. A strongly enhanced aerosol extinction signature appears in the SAGE III data set, in the whole NH >40_N mid to end of August (Fig. 1B), after the beginning of the major fire event in Canada, confirming the results of multiple previous studies (Khaykin et al., 2018; Ansmann et al., 2018; Haarig et al., 2018). Between November 2017 and ~March 2018, the aerosol signature descends with 0.64 mm in altitude per second based on aerosol extinction values > 0.8 km-1 (5 km in three months, October to Janurary). This is in the order of the rate expected for the downwelling of the BDC (see Abalos et al.,2015). The effect of sedimentation is expected to play an important role. However, the contribution of sedimentation as well as dilution/mixing is not quantified here, microphysical and dynamical sensitivity studies would be necessary. The troposphere and lower stratosphere are filled with enhanced aerosols until mid-April 2018."

To separate and quantify pure microphysical effects and dynamical ones, we would 1st need to compare simulations in one hydrostatic case (no transport) and 2nd include all processes (microphysics and dynamics). This would go beyond the scope of the presented study.

3) Fig.1E: From OMPS, ATAL is mostly in troposphere; while from SAGEIII (Fig.2A,4A), ATAL's peak extinction is above 15km. Please explain why OMPS' ATAL is lower.

The ATAL signal in the OMPS data set is visible up to ~18 km altitude (page 7 line 6, Figure 1E). In the SAGEIII data set (in Figure 2A and 4A) the ATAL signal has a maximum at 16 km. Hence, the agreement between both data sets for the ATAL height is very good.

Change in the manuscript: end of page 7/ beginning of page 8: '…in the wider Asian monsoon area (green box from Fig. 1A). Considering the data coverage of OMPS and SAGEIII, the ATAL height of the two data sets are in a reasonable agreement (up to 18 km for OMPS and peaking at 16 km for SAGEIII).

Looking at the green box… '

See also answer to comment number 6.

4) Any stratospheric adjusting is taken into consideration in RF calculation especially for the 2017 fire plume with absorbing substance?

Consistently with previous RF estimations for this event (e.g. Hu et al., 2019) no stratospheric adjustment is applied to our RF estimations. Nevertheless, this should have, in principle, a much smaller impact than for these previous studies, as we suppose a less absorbing fire plume (SSA=0.90-0.93) due to the longer atmospheric life when arriving to the AMA region than in Europe (see also Comment 2, Referee #1).

5) In terms of equatorward transport of the plume, any other mechanisms/pathway can happen? For example, can the plume be lifted higher in the stratosphere in mid-hight latitudes, and then been transported to the tropics? Is there any way you can quantify/ compare the relative fraction of plume transported to tropics via the two ways respectively?

Quantifying the exact transport of the fire plume of the Canadian wild fires in 2017 into the stratosphere is an interesting subject. Part of the fire plume was directly injected into the stratosphere above the fires, then transported within the jet to the Asian monsoon region. However, it is possible that part of the fire plume was first transported within the troposphere and later uplifted into the UTLS. Some of the coauthors are working on a different study to explore this further.

6) Fig.4 I am a little lost here: a. In 4A, which is ATAL and which is fire?
b. Is CO in 4B and 4C associated with fire at all? From Fig.3, authors suggest that ASM barrier prevents fire smoke mixed in; If CO can be mixed in, why not aerosols?

The peak in Figure 4A at 15-16 km altitude can be associated with the ATAL and the peak at 17-18km with the fire plume.

Enhanced CO values in Figure 4B are associated with the 'general' enhancement of tropospheric tracers inside the AMA and do not originate from the fire plume.

We realize that the corresponding sentences lack some clarifying details (starting line 33 page 8). Therefore, we have changed it to: "A measurement profile with readily identifiable and vertically separated AMA and fire plume signatures is shown in Fig. 4A. At around 370 K, the profile inside the AMA shows no clear evidence of enhanced aerosol from the fire plume (Fig. 4 A at ~16km altitude). This is consistent with the *existence of the generally strongest confinement (transport barrier) at around 380 K (Ploeger et al., 2015). The generally weaker confinement at around 400 K compared to 380 K is reflected by the CO gradient and Montgomery stream function shown in Fig. 4B. The enhanced CO mixing ratios displayed in Figure 4B, indicate the entrainment of tropospheric tracers insider the AMA (e.g. Santee et al 2016, Park et al. 2006). The profile in Fig 4 is selected here, because of its location within the eastern flank of the AMA circulation (Fig. 4B), within the canonical northsouth transport pathway from the extra-tropics to the tropics. Back-trajectories show that air masses from the altitude levels of the fire plume and ATAL peaks pass over partly different regions 9 days prior to the SAGE III measurement profile (Fig. 4C). For this study ultra-violet (UV) aerosol index measurements by OMPS and the position of detected enhanced aerosol extinction values by CALIPSO are displayed in Fig. 4C. While CO mixing ratios are a fire indicator for 'fresh' plumes, enhanced aerosol can be traced over longer time scales. Because of the spatial distance between the fire plume origin and the Asian monsoon region, aerosol extinction values rather than CO measurements are taken as an indication for the fire plume…."*

7) Minor: Fig.3, why there are 2 identical color bars?
We replaced the two color bars by one color bar.