Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1240-RC3, 2021


Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Pyrocumulonimbus Events over British Columbia in 2017: The Long-term Transport and Radiative Impacts of Smoke Aerosols in the Stratosphere" *by* Sampa Das et al.

**Anonymous Referee #3**

Received and published: 2 March 2021

The quiescent state of stratospheric aerosols formed from precursors welling up from the troposphere is known to be often perturbed by injections from volcanic emissions. In the early years of this century, a completely different source of stratospheric aerosols was discovered in the form of fire-driven thunderstorms or pyrocumulonimbus (pyroCb) events. These events have occurred at mid latitudes in both hemispheres mostly from Canadian and Australian fires and there is a strong focus on these events at this time. This paper is a modelling study of the intense pyroCb events that took place in August 2017 over British Columbia, Canada. There have been a number of modelling



studies of this particular event in recent years attempting to simulate the stratospheric perturbations. Several space borne sensors as well as ground based instruments have provided measurements on this event. The authors calibrated several of the input parameters for their simulations using data from these measurements but more may be necessary for improved modelling. The paper is well within the scope of ACP and I recommend publication after revision.

Major comment:

1. The authors have optimized their model for aerosol size by using the Angstrom Exponent (AE) derived from SAGE III retrievals, but I didn't find any explicit discussion of the possible impact of morphology or shape of the aerosol particles. In particular, one of the most intriguing observations from the Canadian pyroCb events in August 2017 as well as from the Australian events of January 2020 is the high depolarization ratio in the plumes implying significantly non-spherical particles. As shown by Christian et al. (2020), the depolarization ratio for the Canadian pyroCb kept increasing for several weeks after injection. Several explanations have been put forward in the recent papers but the issue is not completely settled, in my opinion. In any case, I believe realistic modelling efforts should address this aspect and its impact on the evolution of the plume. Yu et al. (2019) had specifically addressed this by using fractal aggregates of black carbon. They found that these fractal aggregates produce higher absorption in the mid-visible. I am wondering if inclusion of non-spherical particles will help solve the lower amount of lifting produced by the model in the current study as compared to the observations. I believe the paper would improve significantly by addressing this issue.

Minor comments:

1. Lines 212-213: CALIOP measures both the parallel and perpendicular component of the backscattering signal at 532 nm but only the total backscatter at 1064 nm.

2. Please use the same scale on the x axis for Figures 2a and 2b. In this figure the comparison between model and SAGE III extinction profiles shows a peak at ∼14 km

on September 3. Is this from another episode of pyroCb, since going by Figure 4, the plumes should be near 22 km by September 3? It is not very clear to me if the simulated AE shown here was actually used or is it just to show the methodology used as an example, as the authors state, page 7, line 236? Would it not be better to present this comparison for some cases in August? Why not show only the SAGE III extinction profile for the wavelength being compared at?

3. Since the simulations run for several months for the aging smoke, I wonder if a time dependent AE and SSA were considered for these simulations.

4. The authors present a very brief comparison of the CALIPSO data and simulation in Figure 5. Firstly, why is the color bar for the CALIPSO backscatter placed upside down? It can be a little confusing. Also, why is there an apparent discontinuity near 8 km in both the panels? The scene shown in Figure 5a was also shown in Torres et al. (2020) but using the directly available CALIPSO browse image and this discontinuity was not seen in that figure. I suspect it is a plotting artifact and related to the vertical resolution of the downlinked data changing around this altitude. This should be clarified in the text for the benefit of readers. This plume observed by CALIPSO actually has interesting constraints for modelling. As mentioned in Torres et al. (2020), this plume (Figure 5a) is a mixture of ice and smoke, with rather high depolarization ratio of 0.2-0.5. While ice is not likely to survive in the stratosphere for too long, this is an ice-smoke mixture and it will be interesting to study if this mixture impacts the evolution of the plume in the first few days when the plume is rising steeply. In fact for the Australian pyroCb events of January 2020, Khaykin et al. (2020) have presented evidence of ice up to 22 km from MLS ice-water content retrievals. Also, I notice that the model missed out those parts of the plume which are more aerosol rich, i.e. the extended plume between 6-10 km between 60N-65N. This would have been clear if the authors had presented the attenuated color ratio image from CALIPSO.

5. I wonder why the authors did not use the latest version 2.0 OMPS data. Perhaps some of the differences between the model and OMPS data noted by the authors could



be alleviated by the new data.

6. Section 3.3 on the link to the ASMA is not particularly convincing. For instance, referring to Figure 6a, I am not sure if I understand the statement on line 347-348. The enhanced values of extinction at 16 km and even at 18 km over south Asia seem to be simply the ATAL feature, which the authors don't seem to mention. Also in addition to these lat-lon plots, a height latitude plot of the extinction might be useful. The legend for Figure 6a may need re-wording, since no smoke is seen in either model or OMPS data in this figure. Similarly the statements in lines 355-360 are not clear or convincing.

7. Figure 9—I am curious as to why there is a bump in AOD around day 180 after the pyroCb injection with the model showing a significant underestimate particularly at 16 km.

8. In Figure 11a, why is there a sharp drop from the peak value to about day 10 and then a slow rise to about day 30 before the steady decline?

9. Lines 454-455—since the 2 estimates differ by such a lot (factor of 20-25), is there any way to evaluate this?

---

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1240, 2020.