I enjoyed reading the paper and believe it should be accepted for publication after the consideration of a few comments.

The presented manuscript is a nice comprehensive overview modelling study of the dispersion and radiative impact of the British Columbia stratospheric fire plume from 2017. GEOS simulations represent the observed transport (vertically and horizontally) including the self-rising feature of the plume and aerosol properties in form of aerosol extinction well, which is of big asset for the investigation of fire plume transport mechanisms.

Confusions especially about the Asian summer monsoon dynamics should be revised for the new version of the manuscript. Have a look at my comments below. A lot of them are only ideas and suggestions. At some places I would have wished for more clarifications and comparisons with other studies (especially your radiative forcing results).

Comments:

line 344 to the end of the page does not seem correct/logical at this point. You want to justify why OMPS and GEOS see enhanced aerosol extinction values at those 'high' altitudes, which is not so much related to the BDC. This (or similar) is what you could write: During the Asian summer monsoon tropospheric trace gases and aerosols are convectively lifted into the UTLS, where they remain largely confined within the transport barriers of the Asian monsoon anticyclone during the monsoon season (June-September) (references). During August 2017 the center of the ASMA was between 15-45N and 40-110E as defined in Kloss et al., 2019. This explains…

For the references here: I would not use Randel et al., 2010: They quickly mention the tracer enhancement within the ASMA, but the focus of this paper is about what happens after the break down of the ASMA (September/October), i.e. where do those 'isolated' air masses go. And even this mechanism has been extensively discussed and revised since then. Only a very small fraction will end up in the ascending branch of the BDC, which would not show in your data. I would use Park et al., 2008 (as an early paper) https://acp.copernicus.org/articles/8/757/2008/ or/and Santee et al., 2016 (a comprehensive more recent study) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016JD026408 Both those studies focus on the trace gas enhancement within the ASMA.

Rather than citing Vernier 2015, I would take the original Vernier 2011 paper (where the ATAL: aerosol enhancement within the ASMA was first discovered): https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2010GL046614

AND for your specific case in 2017 have a look at Kloss et al., 2019. They show the ATAL signal in 2017 in mid-August up to 18 km altitude with SAGE III data.

Sentence starting in line 354: No, this is not the ascending branch of the BDC, but rather a result of the circulation around/above the ASMA. Please have a look and refer to Wu et al., 2010: https://acp.copernicus.org/articles/17/13439/2017/

And Tissier and Legras, 2016: https://acp.copernicus.org/articles/16/3383/2016/

This feature is also seen in Vernier et al., 2011 (have a look at their Figure 3 e.g.)

And again have a look at and refer to Kloss et al., 2019. There you even see for your specific example that the fire plume is around 2-3 km higher than the ATAL (their Figure 2). Also, mean SAGE III profiles at roughly the same time/region seem 1-2 km lower in altitude than what you observe with OMPS. The SAGE III profile matches the model results better. Maybe this might be worth mentioning?

Sentence starting at 373: This is a feature often seen after volcanic eruptions as well. You could cite Haywood et al. 2010 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2010JD014447) for OSIRIS-HadGEM2 comparison after the Sarychev eruption (their Fig. 5) and Kloss et al., 2021 (https://acp.copernicus.org/articles/21/535/2021/) showing the same feature with OMPS data (compared to WACCM) for the Raikoke eruption (2019).

Section 3.6: Please compare your radiative forcing values with the ones from Kloss et al., 2019 (UV Spec simulation based on SAGE III data in the ASMA region), they seem to match well. Furthermore (see also line 480), instead of or additionally to the Pinatubo comparison it might be worth bringing the radiative forcing estimations of your study from the BC fires in context to the most recent (much stronger) Australian fires. Please have a look at Khaykin et al., 2020 (https://www.nature.com/articles/s43247-020-00022-5). If you'd like to compare your radiative forcing results to all stratospheric aerosol events of the past few years, also have a look at Kloss et al., 2021 (Figure 8 and 9 for radiative forcing estimations of the Raikoke plume).

Section 3.7 and general: The model from Yu et al., 2019 was not nudged after 12 August 2017 (see their supplements), which limits the trustworthiness of the early distribution. Might this be worth considering when comparing your models and to explain some differences?

Figure 4: Could you clarify if those lines are averages over the whole NH (as for the background aerosol extinction)? But if so: Even though you only consider data above the cloud top height for OMPS, would the result not be biased by different tropopause altitudes (tropics vs. higher latitudes)? How can you distinguish between 'plume clouds' and other clouds for the OMPS observations?

Minor comments/suggestions:

Line 1 Erasing the term 'Pyrocumulonimbus events' would make the title more accessible and the pyroCb event itself is also not really the topic of your manuscript. All together your paper is a modeling study with satellite comparisons, I would add the term 'model study' or 'GEOS' in the title. Just a suggestion..

Line 19-21: 'The model simulated… are in close agreement' ?

Line 50: You could include and generally also have a look at Lestrelin et al., in ACPD : https://acp.copernicus.org/preprints/acp-2020-1201/

Around line 51: You might want to add ground based observations as well, e.g. Ansmann et al., 2018: https://acp.copernicus.org/articles/18/11831/2018/ Baars et al., 2019, Khaykin et al., 2018..

A lot of the literature within the paper is not found in the reference list: e.g. Peterson et al., 2016; De Laat et al., 2012; Chen et al., 2016; Chen et al. 2020 …

Line 91/93-100: Don't you think the model description is a bit too detailed for the introduction? You might consider shifting it to the methods section.

Line 161: The ‘baseline experiment includes the injection’….?

Section 2.2: The newest OMPS version is 2.0, giving aerosol extinction observations on multiple wavelengths. I am wondering, why you didn’t use the latest version.

Section 2.3: Why did you choose to bring SAGE III and CALIOP in one methods section?

Section 2.3, SAGE III: For OMPS you explain how you treat clouds, how about SAGE III? I understand that you only show one profile and the AE shows very obviously that there were no clouds at that point, but you also write that this is only an example.

Throughout the manuscript you change between ‘SAGE III’, ‘SAGE-III’ and ‘SAGE-III/ISS’. I think the last version is the best..

Line 239: 42°N seems too north to be called ‘sub-tropical’

Line 245: Maybe you could add a ‘(not shown here)’

Line 323-328: Nice comparison. Maybe you could state the motivation of showing this Figure. In my eyes, this paragraph seems a bit lost at the beginning of 3.3.

Line 338: nor -> neither?

Line 391: ‘It is clear from the comparison,…, (that?) the magnitudes…’

Line 397: You mean ‘down to’ I assume..

Line 412: compared

Line 442: I thought you wrote earlier (section 3.3 / Figure 4) that the lofting around the ASMA is not well represented in the model.

Line 466: August 2017_over..

Line 477: that-> than

In Data Availability: You might include the data version for SAGE III and CALIOP

Figure 1: Could you replace the titles by more meaningful ones?

Figure 6a: km-1