Review of Zhang et al., "Investigating the vertical extent of the 2023 summer Canadian wildfire impacts with satellite observations".

Reviewer: Mike Fromm

Zhang et al. use satellite remote sensing observations to evaluate an attention-getting atmospheric condition in the northern wildfire season of 2023. During that season, fires across the extratropical globe—especially in Canada—were unusually numerous, large, and active. The fire season was punctuated by an unprecedented number of pyrocumulonimbus (pyroCb) storms, again particularly in Canada. A natural science question arises: How did these fires and pyroCbs perturb the upper troposphere and lower stratosphere (UTLS) compared to other years that experienced large stratospheric smoke perturbations? The hypothesis might be an expectation of an unusually large and/or persistent biomass burning plume in the 2023 stratosphere. Zhang et al. tackle that question and hypothesis. This is self evidently an appropriate topic for ACP.

Zhang et al. use ACE-FTS, OMPS-LP, and MLS data in their pursuit. All three are well suited, individually and in combination, to this study. Indeed, they may be optimal as compared to, for instance, models or localized observation systems. The authors are commended for assembling these data in short order after the 2023 season. I would expect that a thorough deployment and analysis of these data could address the above-mentioned science question.

Whereas the authors have made available an interesting set of observations, the current presentation generated—in my assessment—more questions than answers. These concerns are listed in detail below, first the substantial issues, then minor/technical ones. The list is fairly long, which represents to my mind a systemically unconvincing line of reasoning and/or presentation.

Here is a summary of my concerns. The authors report that only a single occultation profile from ACE-FTS embodies indicators of stratospheric biomass burning emissions in the stratosphere in 2023. If true, this would indeed be a notable finding, given the ~800 profiles in play. But I was left doubtful by the way the data were presented. The authors identified MLS as a core data item but presented only a limited analysis rather than a season-long survey. The authors' deployment of OMPS-LP aerosol data was not thoroughly described and the presented results were confusing, perhaps even in error. Air-parcel trajectories were used to connect a single ACE stratospheric observation to a potential pyroCb, but that analysis was unconvincing.

The potential is there for a substantial improvement by addressing the concerns detailed below. The author team is ideally equipped to make the necessary improvements. My hope is that my report is accurate and helps guide the authors to the key areas that need revision before this manuscript merits publication in ACP.

Below I list all the issues in bold text to guide the authors to line numbers, quotes, and/or figures. My input follows in plain text.

**Substantial Concerns**

**Line 80:** The authors give two reasons for invoking MLS:
1. ACE's relatively poor spatial coverage
2. as a validation of ACE CO.
But they only show a single plot in Supplemental Information. As far as I can tell from the literature, ACE needs no validation of its CO retrievals. Moreover, MLS data aren't exploited for its superior coverage. I see little to no value in the MLS component of this manuscript. My recommendation is to more fully analyze MLS data for the 2023 season or drop this part of the work.

**Line 89:** This contention needs more explanation. MLS CO is good down to 215 hPa, which is nearly ideal for this study. MLS 215 hPa CO was used quantitatively in the study of the PNE smoke event (https://doi.org/10.5194/acp-21-16645-2021), Black Saturday and Great Divide (doi:10.5194/acp-11-6285-2011). What is the basis for questioning some "**individual profiles**"? The broader MLS vertical resolution would seem to be well compensated for by its twice daily sampling, near global orbital coverage, and high-resolution sampling along orbit.

**Line 132; Figure 1:** I'm perplexed by the high August tropopause and the explanation. The twenty August ACE occultations are distributed evenly, longitudinally. For an average tropopause to be 16 km, some individual ones would have to be even higher to compensate for those distant from the Asian Monsoon sector (which represents at most about ¼ of the zonal belt even at its widest, south of 40N). I examined the August 2023 ACE occultations north of 40N and the temperature data. Eyeballing the tropopause from the temperature profile, I estimated an average ~14 km. I also used NCEP Reanalysis data, interpolated in space and time to the ACE occultations, to calculate tropopause height based on the dynamical definition. The average for these occultations was 13.1 km using the threshold: pv=2.5 pvu. If my results are accurate, this disparity calls into question tropopause height calculations here and perhaps elsewhere in the paper. Considering the crucial importance of the tropopause height in this study, I would encourage the authors to check their calculation method and results, and update all the analyses dependent thereon.

**Line 132, Figure 1:** The August panel is also puzzling in that the 2017 and ACE-lifetime statistical HCN are larger at 16 km w.r.t. the other months. Even the sigma width is larger. Are the August data from other years all subject to a similar tropopause bulge? Is the entire ACE August 40-70N record that much different than the adjacent months? Because the August pattern in Figure 1 is so strikingly different, it needs to be addressed exhaustively or removed from the analysis.

**Line 138:** This is an argument for using a tropopause-relative reference frame. Given the physical reality of the 2023 plume heights congregating near the tropopause, it is recommended that a tropopause-relative analysis be performed as a replacement or complimentary construct.

**Line 138:** A possible complicating factor comes to mind while contemplating this analysis. The phenomenon of the double tropopause [e.g. Homeyer et al. (2014; https://doi.org/10.1002/2014JD021485)] could well be a factor in the tropopause-height calculation and assessing 2023 plume tropopause-relative height. It might be worth noting this and expressly stating if/how double tropopause situations are handled in the calculation of tropopause height.

**Figure 3:** These statistics show that the authors have done a trop-relative analysis of all ACE data. So, it should be considered elsewhere.

**Figure 4:** It would be helpful to the broad readership to add a plume-free spectrum for comparison, akin to Figure S4 in Boone et al. (2020).

**Line 163, "saturated":** Saturated with what, or because of what? This needs to be fleshed out because 3 of three distinct smoke plumes shown in Boone et al. (2020) all had a prominent C=O stretch feature. So, the disparity of the present example w.r.t. these published examples raises the question of why it is different. IR spectroscopy of smoke as compared to other particulate compositions is not yet a mature and intuitive topic for the general science audience. Help is required for the benefit of those not attuned to the specifics of this niche ACE-FTS data application.

**3.2 Occultation ss107570:** This section is where the authors state that only one occultation (ss107570) had a demonstrable smoke enhancement above the tropopause. I have examined all the 2023 ACE profiles north of 40N, in concert with tropopause heights, and found several that are as convincing as ss107570. E.g. ss107346 and sr108266. Given the above-mentioned concern with the tropopause-height calculation, I encourage the authors to re-examine the occultation record for other examples of extinction enhancements clearly above the tropopause to find other candidates for stratospheric smoke. This would have two crucial benefits, 1. Additional IR spectra to exploit, and 2. Other candidates for matching the ACE observation to a potential pyroCb. Another suggestion to the authors is to extend their analysis to October ACE data, considering the fact that there were two Canada pyroCb events late in September and the likelihood of ACE stratospheric smoke observations post September.

**Section 3.3:** Either this section or section 2.2 need to contain a detailed description of the OMPS analysis method. Much is unknown. How are clouds handled? How is the vertical data ensemble between 11-16 km treated and reduced to single extinction ratio values? How is the molecular extinction calculated? What is the publication background on this method, if any?

**Figure 5:** The color range here seems to indicate an average extinction ratio <<1.0. This plot does not conform to Figure 8 (top). The general average between 40-80N is <<1.0 in Figure 5 while Figure 8 shows a baseline near 1.0. Something appears to be amiss.

**Section 3.4:** Suggestion: Move this section to 3.3 to connect it directly to the individual ACE profile presented in 3.2.

**Figure 6:** I don't understand the rationale for launching 3-day trajectories within a 3.5-day span of launch times. How does this **"compensate for limited ACE-FTS coverage?"** The ACE occultation location and time are known precisely. The back trajectory from ACE time does not nearly extend long enough to implicate any land/fire surface area. Only the trajectories launched ~3 days before ACE pass over a suspect landscape. If one were to launch forward trajectories from these times to ACE time, it is not certain where they'd end up and if/how they would apply to the ACE occultation. Hence, as presented, this analysis is unconvincing. It is suggested that the authors restrict launch times to ACE time and run them back long enough to encounter locations/times of reported pyroCbs.

**Figure S4:** Ditto the concerns expressed above. In this case there is some wiggle room for initiation time, but only on the order of a couple hours. As it stands, the trajectory launched at pyroCb time ends up somewhat close to the ACE location but 3 days before the ACE occultation.

**Line 218, "The maximum VMR and aerosol extinction values are similar between the two events, and Fig. S5 also shows that the aerosol loading near the tropopause is similar between the two years.":** This sentence is confusing. Are the data in Fig. 7 taken as representative of PNE and 2023, such that the comparative gas and aerosol data maxima reflect a similarity between the PNE and 2023 **"events"**? Moreover, the PNE profile's CO max is not similar to the 30 July 2023 CO max. The extinction max for the two are similar, but the value for 2023 is in the troposphere. Substantial elaboration and clarification are called for. This is especially important given that the authors relate their synthesis of Figure 7 to the season-long graphic in Figure S5.

**Line 219-221, "However, the stratospheric impact of the PNE is visibly larger since the entire plume is measured well above the tropopause whereas only part of the Yukon plume is clearly in the stratosphere.":** Again, the authors seem to be taking this singular PNE profile as somehow representative of PNE in general, and in terms of injection height specifically. The PNE pyroCbs did not inject smoke to the tropopause+9 km.

**Figure 8:** The pre-PNE color shows extinction ratio <= 0.0 according to the color bar, but the line on the upper panel indicates values exceeding 0.4. Is there a discrepancy here?

**Figure 8:** The difference in extinction ratio between 2017 and 2023 is striking, from the beginning of May onward. The loading in 2023 is 2 to 3 times larger than 2017. What explains this difference? The earliest Canada pyroCbs were on 4-5 May, so the large 2023 values from onset are difficult to understand. What was the extinction ratio pattern prior to May? Do the authors have an explanation for this apparent puzzle?

**Figure 8:** The 2017 extinction ratio jumps to values ~1.5 after PNE in the lower panel. But the color-scaled top panel manifests no such value, even at its maximum value. Something is apparently amiss.

**Figure S1:** If this is to remain in the paper, as a validation of ACE CO, more information is called for. Was there an attempt to match MLS profiles with the ACE profiles? Were all the MLS data, day and night, included in the averaging? If essentially all the MLS data 40-70N are used, it probably doesn't qualify this as an ACE validation.

**Figure S2:** Recognizing the authors' uncertainty about enhanced HCN w/o extinction, certain data points call out for an explanation. By that I mean the gray enhancements, high above the tropopause, exceeding the red-dot enhancements in May, July, August, and September. As such, the reader might wonder about how robust the red dots are or what to make of these gray HCN enhancements. Please explicitly deal with these perplexing data points.

**Figure S5.** The top two panels are defined as extinction ratios for a single, thin layer between 10.5 and 11.5 km. But the bottom panel is defined as relating to 11.5-16.5 km (like the plots in the main body). Is this the authors' intention? It seems odd.

**Figure S5 (bottom panel):** The 2023 time series shows a pronounced mound of extinction ratio from mid-July to August. This is not apparent in Figure 5, which is this panel's mate, according to the figure caption. It looks much more like the 2023 panel above. Regardless of which panels are matches, the numeric values in the time series do not conform to the color-scaled plots in either figure. Something is apparently amiss.

**Figure S5 (bottom panel):** How is this panel's construction for 2017 different than Figure 8's top panel? Both are described identically in their captions but the time series lines are different.

**Figure S5:** Logically, this figure—once corrected--belongs in the main paper. It is central to the authors' thesis.

**<u>Technical Issues</u>**

**Abstract, Line 29:** Mention MLS here given that ACE-FTS and OMPS-LP are mentioned.

**Line 18:** Replace "**Profile**" with "Profiler."

**Several locations:** use hectares instead of acres.

**Line 51, "It has been reported that at least 135 pyroCbs occurred in Canada between May and August 2023…":** Please cite the "report."

**Line 53, "…perturbations to stratospheric composition (NASA EarthData, https://www.earthdata.nasa.gov/).":** It's not clear what this citation refers to in the context of this sentence. This is a generic data web site. Please provide a more specific citation.

**Line 81:** A citation is needed for the MLS instrument.

**Line 127, "2017…":** Please reword to avoid starting a sentence with a numeric.

**Figure 1 caption, "…tropopause altitudes calculated for 2023 are plotted for reference.":** Please insert **"**as horizontal dashed lines" after "**plotted**".

**Figure 1 caption, "**…**approximate monthly average…":** What is meant by the "**approximate**" qualifier? Please consider dropping this unless there is a purpose for it.

**Line 133, "external processes":** What is meant by "**external**" here? What processes would be internal? Please clarify or reword.

**Line 137 and elsewhere, "concentrations":** Mixing ratio instead? ACE retrieves mixing ratio, which is a cousin to concentration, but they are distinct. Please clarify.

**Figure 2:** Are the horizontal dashed lines tropopause height? These are ~0.5 km different than the July value in Figure 1. Please clarify or correct.

**Figure 3 ordinate titling:** Replace "**above**" with "relative to the".

**Figure 3, lower right panel:** Please consider adding statistical results as shown in the other 3 panels.

**Line 185, "72 hour trajectories…":** Please reword to avoid starting a sentence with a numeric.

**Line 187, "shown in Fig. 6, with possible source fire locations":** How are the source fire locations shown in FIg. 6? I don't see any marks or symbols, or description in the caption.

**Figure 6 caption:** Please correct the ACE longitude.

**Line 195:** Change "**Fig. 5**" to "Fig. 6".

**Line 204:** Delete "mean".

**Line 214, "…~1 km injection height…":** Please correct the number.

**Figure S3 caption, "Atmospheric profile…":** Please replace "**atmospheric"** with "temperature."

**Figure S3**: Please provide an explanation of the data set used for this temperature profile. Since it is stated that the data are at 03 UTC, it is unlikely that the profile is sourced in radiosonde data.