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Interactive comment

Interactive comment on "Transport of the 2017 Canadian wildfire plume to the tropics and global stratosphere via the Asian monsoon circulation" *by* Corinna Kloss et al.

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I am compelled to offer some questions, comments and suggestions to help improve the research and paper of Kloss et al..

Abstract. In the first sentence ("…reached the tropics, and subsequently the tropical stratosphere…") Kloss et al. seem to suggest that the Canadian smoke plume, upon entry into the area of the AMA, had a discernible tropospheric component. Only subsequently was it lofted into the stratosphere by the BDC according to this claim. This is a fairly provocative claim. However I could not find any evidence given or figures showing upper tropospheric smoke adjacent to and wrapping around the AMA. They attribute all

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the aerosols displayed below the tropopause to the ATAL. The evidence in Figure 1 and Khaykin et al. (2018) shows that by late August the smoke near the AMA was already at stratospheric heights and potential temperatures. If my understanding of the claim set forth in the abstract is correct, to defend it would require two things. 1. an unambiguous discernment of upper tropospheric smoke upstream of the tropical observations, and 2. evidence ruling out quasi-isentropic transport of the observed stratospheric smoke to the tropics. If on the contrary it is acknowledged that the smoke moving into Asia in late August was already spanning the lower stratosphere (as Khaykin et al. (2018) show) then it is hard to defend the abstract's claim convincingly.

On a technical but important note, the Abstract mentions "July" as part of the Canadian smoke event. There is no evidence here or in other papers that July was in play. This wording should be removed.

Introduction, L2-3. The manuscript stipulates that pyroCb activity is the source pathway for this plume. Hence it is critical to accurately establish the pyroconvective source. That is best done by citing Peterson et al. (2018) in this sentence. Peterson et al. give detailed and accurate constraints on both the pyroCb injection in the Pacific Northwest and the 3D footprint of the pyroCb plume on 14 August. Khaykin et al. (2018) points the reader to fires that did not exhibit pyroCb activity. (Sergey and I have had a personal communication on that matter.) Hence that paper is not fitting as a citation here.

On that topic, the choice of initializing CLaMS over three days centered on a box that is neither focused on the Pacific Northwest pyroCbs nor the pyroCb plume on a subsequent day seems destined to introduce many spurious or useless trajectories. The growing realization that there was significant diabatic lofting of the smoke further diminishes the applicability of the CLaMS construct and setup. Consequently little confidence can be gained from a set of these trajectories at a single potential temperature surface (especially since the plume was lower than 380 K in the first days (See Fig. 4 of Khaykin et al. (2018)).

Introduction, L29. Of the 3 papers cited on this line, only one postulates the Nabro troposphere-ASM -convection pathway: Bourassa et al. (2012). Fairlie et al. dispute that claim. Sellito et al. seem to be noncommittal on the pathway. Considering that Kloss et al. are apparently attempting to draw parallels with the Nabro publications and the 2017 AMA/smoke interaction (P2, L30), it is important to accurately portray the literature on the Nabro event.

P3, L22. Why was it decided to use "cloud unfiltered" SAGE 3 data? Thomason and Vernier (ACP, 2013) were compelled to go to great lengths to adopt a rigorous cloud clearing in SAGE II data for the study of tropospheric aerosols (indeed the ATAL). For inadequately constrained data sets such as SAGE and OMPS it is essential to either attempt aerosol-cloud discrimination or acknowledge that the tropospheric information content is uncertain. This is especially true for a regime like the particularly cloudy ASM.

P6, L17. Like one of the reviewers, I do not see evidence of descent. In fact it can be argued from this figure that aerosol is ascending. Indeed Khaykin et al. (2018) show that the extratropical smoke plume height increased dramatically, presumably due to diabatic forcing. What is the indicator of descent?

P6, L20. I don't see any difference in the extinction pattern after mid-April as compared to just prior to mid April. In fact tropospheric extinction appears to be saturated red throughout the timeline. I refer back to my comment above regarding cloud contamination and suggest that it is not possible to argue that the preponderance of the unfiltered tropospheric extinction signal on display is from aerosol.

P6, discussion of Fig. 1C. The value and information content of this figure panel is not obvious. As the authors state, detailed interpretation of smoke layers is hindered by the lack of filtering. In addition, half of the period rendered is the winter season, when there is no anticyclone and confinement. Presumably smoke aerosols would be in evidence in any other longitudinal sector in the winter. Hence some additional explanation of the meaning of Figure 1C is called for.

P6, L29. Like the discussion of descent earlier, it is not evident what feature suggests ascent in Figure 1D. Moreover, there are additional plausible explanations for a sloping aerosol feature in a time series set in a localized domain. For instance, wind shear upwind of the domain box can generate a sloping aerosol feature within the time series; an apparent descending slope for aerosols below the jet max, apparent ascent for above the jet max. Khaykin et al. (2018) actually allude to this as a factor in the transport of the 2017 smoke plume. Considering that the smoke plume was transported from afar to the Asian sector, the role of wind shear in the transport and deformation should be acknowledged and investigated.

As a general matter, it has been shown in published results, of this case and other pyroCb stratospheric smoke plumes, that large meridional excursions of the plume from extratropics to subtropics and tropics is routine and not beholden to the AMA. Khaykin et al. (2018) show that for the 2017 event; their Figure 3 shows Canadian smoke south of 30N over the western Atlantic Ocean. Jost et al. (GRL, 2004) showed Canadian stratospheric smoke at subtropical latitudes. (In a paper under review, Fromm et al. extend the Jost et al. case study and findings to latitudes as low as 14N.) Fromm et al. (JGR, 2008) showed stratospheric pyroCb smoke at a tropical location (Hawaii). The path there did not involve nor require the AMA circulation. Pumphrey et al. (ACP, 2011) showed Australian stratospheric pyroCb CO in the tropical southern hemisphere. Siddaway and Petelina (JGR, 2011) showed the tropical aerosol aspect of the CO plume that Pumphrey et al. presented. Hence the challenge for the present work is to convincingly show that the AMA was of consequence to the exclusion of (or together with) other demonstrable tropical plume excursions (E.g. Khaykin et al.'s Atlantic smoke).

Kloss et al. claim that there is no profile showing fire plume presence inside the AMA black box (Conclusions, P12, L14) but also infer (P6) that there is a SAGE smoke profile on 30 August inside that box. Their claim is at odds with Khaykin et al. (2018) who show (their Figure 3) CALIPSO plume detections well inside the black AMA box on two dates in late August. Back trajectories that I calculated show that these plume segments connect with the synoptic-scale plume from a few days earlier over Europe, as shown in this paper (Figure 4) and Khaykin et al. (2018). This is seemingly at odds with the contention that the smoke plume bypassed the AMA center. Moreover, it is consistent with the general antecedent conditions of a large and expanding smoke plume advected from Canada to Europe to east Asia, including the region of the black box. Hence the big picture, as shown in this paper and Khaykin et al. (2018), is more in line with advective transport equally under the influence of all the flow regimes present throughout the northern hemisphere at that time.

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