Review of the ACP manuscript acp-2025-1346

"Transport of Biomass Burning Aerosol into the Extratropical Tropopause Region over Europe via Warm Conveyor Belt Uplift"

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The above manuscript describes aircraft-borne aerosol particle measurements conducted in the upper troposphere and lowermost stratosphere over Europa. The analysis focuses on a short time period of one of the TPEx campaign flights, when the outflow region of a warm conveyor belt (WCB) was probed and influence from biomass burning was found. Overall, the manuscript is well written, the set-up with towed sensor shuttle is relative unique, and the supporting meteorological data are very useful. In contrast to previous studies, the detected biomass signature stems from smaller wildfire events which reach the tropopause region due to the WCB and not by their own dynamics (i.e. as pyroconvection). As this is somehow new and could be relevant to the UT/LS chemistry or UT/LS influence on the radiation budget, the manuscript should be published. However, there are a few questions concerning the data processing and the interpretation of the results. Moreover, the prove of the relevance of such kind of events is missing, which diminishes the value of the results (are they relevant or not?).

Specific remarks:

p. 1, l. 9: I can see the "800 particles per cm^3" in Fig. 3, but for me the background is about 200 particles per cm^3, hence at least a factor of four lower, not two.

p. 2, l. 24: the sulfate aerosol and the BC you refer to, which one is it? The average total atmospheric amount? Please specify.

p. 2, l. 35: Please cite not only references from your group, there are other publications which have shown which are related to your topic, for instance Brioude et al, Atmos. Chem. Phys., 7, 4229–4235, 2007 or Zahn et al., J. Geophys. Res. 105, 1527-1535, 2000.

p. 3, l. 57: The sentence on the BC lifetime, this hold true for other particles as well. And after modification, please move it somewhere else (or delete it), as it does not fit to the rest of the paragraph.

p. 3, l. 74: You focus on a dedicated, short section of one flight. But looking at the flight pattern and having in mind that there was a second flight on the same day, did you check your data for the chance of having probed the airmass a second time, later (or earlier) in the campaign? On page 15 you suggest that partly probing the same airmass again have been the case.

p. 3, l. 78: The statement of the "highly variable tropopause altitude", well, it looks variable, but not highly variable, e.g. there is no tropopause fold. Please modify the statement or justify why it is valid.

p. 5, l. 97: Even if the particle size range which is most interesting for you is less prone to particle losses in your inlet system and sampling line, you must at least provide an estimate or an upper limit on the respective particle losses.

p. 5, l. 105: Related to the point above, how about an in-flight intercomparison between the TOSS and the Learjet instruments? There must be a flight condition, where this should have been possible and this would highly increase confidence in the data quality. Similar to the radiation calculations later on, it is not sufficient to refer to a potential future paper.

p. 6, l. 108: Again, the statement on the upper inlet cut, which analysis is it based on?

p. 6, l. 115: You state that you operated 3 CPCs, but you provide only two lower threshold diameters. Why?

p. 6, l. 120: If I´m not totally wrong, 5 km in 30 s would result in a TAS of 167 m/s, which seems to be very slow for a jet aircraft in the upper troposphere. Are you sure the number is correct?

p. 7, l. 154: I did not find the time resolution of the filter sampling, please provide this information.

p. 7, last paragraph: I´m not an expert in this, but can you exclude particle changes during the storage times? Either way, could you please write a sentence, if these can be excluded (and if yes, why) or if they are of minor importance or …

p. 9, l. 214: Same statement as in the first comment, I do not see this factor two, it is at least a factor of four.

p. 9, l. 225: If I´m not totally wrong a H_2O mixing ratio of 100 ppmv is rather typical for the midlatitude summertime UT and not an indicator for stratospheric air, or?

p. 11, l. 250: Why do you provide the information that the flight pattern was flown after the TOSS deployment? Do you want to say, that the TOSS was not applied during that flight pattern? If so, please state it in that way. Same in the figure caption of Fig. 4.

p. 11, Fig. 5.: This figure puzzles me a lot. First of all, why is the first a delay in the AMS data and in the next peak the AMS is ahead? Secondly the strange looking UT/LS background volume size distribution, why are there jumps of 50% in relative narrow size bins? Is there an issue with the assumed refractive index of the particles? And the error bars are misleading, the measurement period is short, hence it would be much better to indicate the measurement uncertainty here, which should be some ten percent, I guess. Volume size distributions derived from OPC measurements are highly uncertain! The data behind this figure need a deeper analysis. The different colors in Fig. 5b are not explained in the legend.

p. 12, last paragraph: You argue here and in the following that you have at least up to the LMS a non-negligible amount of soot in your particles (see also Fig. 7). And in the troposphere it seems to be (for me) unrealistically high. This will definitely affect your optical particle measurements, was this considered in your data processing? Otherwise you cannot trust the distributions in Fig. 5b.

p. 16, Fig. 8: I understand the “normalized by sample volume” on the y-axis (please remove the empty parenthesis), but I do not understand the “normalized to the flight blank and sampling time” in the figure caption. And these are two different statements, right?

p. 16, l. 319: The interpretation of the particulate BB tracers, what do the results indicate? That there have always been BB influence during the sampling period?

p. 16, l. 323: The short lifetime of levoglucosan is how long? Please provide this number in the text.

p. 19, l. 374: The whole paragraph. Either you show results of the radiative impact here or you give an estimate on how frequent such events might occur. Otherwise the value of your results is hard to estimate, i.e. are they relevant.

p. 20, Fig. 11: Again I doubt the BC fraction of 40% at the lowest flight level. I assume that there must have been other particle material like dust.

p. 28, Fig. D1: The first too low AMS mass concentrations, could these be caused by the AMS warm-up time?

Technical corrections:

p. 3, l. 76: Please change to "The TOSS and the aircraft were equipped …"

p. 3, l. 81: Please specify what is meant with "stratospheric intrusions which increase during the flight", are they reaching deeper into the troposphere or are they covering a larger area or do they occur more frequently?

p. 4, Fig. 1: The flight path is displayed in red, not in black, as stated in the figure caption.

p. 6, l. 132: Please provide a reference publication for the instrument and the uncertainties.

p. 7, l. 149: The first two sentences of this paragraph provide partly the same information. Please remove this doubling.

p. 8, l. 208: The information that you use N_2O for defining the chemical tropopause is already given in line 203. Please remove one of the two sentences.

p. 9, l. 211: This subsection is quite long, do you see any chance to split it? This would make it easier for the reader to follow your line of arguments.

p. 9, l. 235: The "recent particle formation event" could it be an aircraft plume encounter? Did you check for instance flightradar24 for such a possibility?

p. 10, l. 237: "the chemical stratosphere" is probably not the correct term (what would this be?), you mean "chemically stratospheric air".

p. 11, Fig. 4: Please provide the particle size range information in the legend of Fig. 4a.

p. 13, l. 285: You refer to Fig. D1 and discuss it here in the main text. Consequently, the figure should be shown here. Same for figure F2 later on.

p. 17, l. 346: The half sentence "and the biomass burning pollution trajectories" does not fit here and seems to me remnant from a former test version.

p. 18, l. 359: Please change "aerosol number concentration" to "aerosol particle number concentrations".

p. 21, l. 407: "the chemical … stratosphere" is wrong again, you mean "in chemically stratospheric air".

p. 21, l. 429: Please exchange "into" with "on", otherwise the sentence does not make sense.

p. 28, l. 477: there is a space missing in-between "gcm" and also in ".5min" in the next line.

p. 31, Fig. F1: the red line indicating the back trajectories are hard to see over the orange/brown background. Please use a different color, e.g. bright green.