

Review of the revised 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 revised manuscript improved, but there is still a small number of issues which are not well addressed. One of them is major and requests some bigger change.

Specific remarks:

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.

You state that you added the Brioude et al. reference, but it does not show up in your list of additional references. As I could not find the revised manuscript, I could not check if it is listed there.

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

I fully understand your answer and your reasons, my request is to let the reader know your arguments (add it to the text), otherwise the reader will puzzle as I did.

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 ...

Again, your answer is good, but please provide this information to the reader and change your text accordingly. (As I said, I did not have the revised manuscript, so sorry, if you already did this)

p. 11, Fig. 5.: This figure puzzles me a lot. First of all, why is there 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.

I do not understand your averaging argument concerning the shift. Right averaging does not lead to a shift. Moreover, it would not explain why one peak is delayed and one is ahead, because it would affect both peaks equally.

Concerning Fig. 5b my comment was probably not specific enough. Of course there are jumps when you merge bins, what I meant is that there is no physical reason, why there should be such very narrow particle modes in your particle volume size distribution. I have seen this before in OPC data and they were caused by the wrong refractive index/particle shape assumptions. Hence I do not trust

the shape of your volume size distributions, but admit that the relative amplitudes for the different regions could be meaningful (limitation: see below). As I assume that you do not like to carry out a more detailed optical analysis of your data, my suggestion would be to state this in the text. The shape of the size distribution (the ups and downs) are probably errors of the analysis procedure (wrong refractive index and/or shape), but the amplitudes show (somehow, see below) the relative differences between the different regions.

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.

The problem with the very high potential BC content was also raised by the second reviewer. I also/still believe that even the statement of an “rough estimate” in your reply is wrong, these are unrealistically high fractions and if they are not caused by the uncertainties in the two methods there was likely another aerosol material there.

But this was not the focus of my question. What does not get together is that you state on the one hand a very high BC content in your particles, but on the other hand work with OPC response functions for non-absorbing particles only (as far as I could see in the Bozem et al, 2025 manuscript). Moreover, this BC contents changes with the region under consideration, hence even the relative ratios of the magnitudes in Fig. 5b are questionable.

Sorry for saying this, but this is a big, for me unacceptable inconsistency in your data and makes the absolute values questionable. This issue must be addressed somehow.