General

The paper contains smoke observations over Finland. But this aspect alone is, to my opinion, not sufficient to justify publication. Meanwhile there are so many smoke observations with lidar in the literature (see review of Adam et al., 2020) and even over the North Pole (Ohneiser et al., 2021). Therefore, the goals of the paper need to better emphasized: lidar-ceilometer observations and comparison with model results is probably one goal. Another goal is the careful analysis (some kind of a feasibility study) to what extent Vaisala ceilometers (and these huge ceilometer networks) can contribute to tropospheric smoke monitoring (even in terms of mass concentration profiling). The paper is worthwhile to be published, however only after significant improvement. Furthermore, the paper contains many speculative and questionable aspects. Their own AERONET approach to derive smoke conversion factors is unacceptable. So, there are many parts that need to be significantly improved.

Major revisions are required.

Details:

Abstract:

It should be clearly stated in the beginning: What is the main goal of the paper, what is new in this paper (in view of the numerous smoke observations with lidar in the literature, see review of Adam et al., 2020). First lidar smoke observations over Finland …. is not a convincing argument (or goal). Recently, TROPOS people even measured smoke over the North Pole (Engelmann et al., 2020, Ohneiser et al., 2021) … with lidar aboard an ice breaker.

To my opinion, to combine lidar and ceilometer observations (and even to include modelling) is an attractive approach. And especially, if the main goal is: … to demonstrate the usefulness of a Vaisala ceilometer to monitor smoke in the troposphere!

However, feel free to define your specific goals! This is just a suggestion. In this context, you can then easily present all your nice smoke results on changing depolarization ratios, on this unique smoke feature with larger lidar ratios at 532 than at 355 nm, and the comparison with model results for smoke.

Introduction:

P 2-3: The Introduction should be improved. You mention the Mueller1999 paper, then I would add the Mueller2005 paper as well because that paper is directly related to smoke observations (and lidar inversion application). Furthermore, you need to mention this Adam 2020 review paper!

In the next step, you may want to continue with network activities (before you introduce the ceilometer network aspect), and maybe, also CALIPSO observations. Again, with clear focus on smoke. There are these Baars2019 and Khaykin2018 papers as examples for network and space lidar activities. This would show the added value towards regional to global scale smoke characterization when using networks. This motivates, to my opinion, then the next ste0: …. to analyse to what extent the existing and exciting (European) ceilometer infrastructure could do in case of smoke monitoring… and so on…. All this would corroborate the importance of the paper. Are there some smoke observations with ceilometers in the literature (I am not sure)? If yes, should ber cited. If not, that would be new point to be mentioned! One may also indicate similar approaches such as the ceilometer observations of volcanic aerosols (Eyjafjalla volcanic aerosol, Emeis and Flentje papers in 2010/2011?) to indicate the usefulness of modern ceilometers to detect aerosols (and not only clouds).

Feel free to define your own specific goals of the paper. It is not very clear to me at the moment what the goals are.

Now some more detailed remarks:

P6, line 176: I do not believe that you can get the backscatter coefficient at 910 or 1064 nm with an uncertainty of less than 10%. The uncertainty in the reference value is too large. And a proper Rayleigh fit at these long wavelengths almost impossible. The uncertainty is certainly always in the range of 20-30% at 910 or 1064 nm for the backscatter coefficient. And the conversion (backscatter to extinction) will introduce another 20-40% uncertainty in the case of smoke layers. The lidar ratio for smoke was found to be 50, 60, 70, 80, even 110 sr in smoke observation (see Adam et al, including the ACPD version and supplementary tables). So, using, e.g., 75 sr as smoke lidar ratio at 532 nm, and the range is from 50 to 100 sr, than the error is 33%. The uncertainties are probably similar for 910 nm.

P7, lines 201 – 214: I speculate that there was an air mass transport from central Europe to Finland at heights below 3 km height (not presented), when I see the backward trajectory figure for the arrival height of 4 km. And this aged European haze widely determined the observed AOD over the field sites. You mention 500 nm AODs of 0.24-0.42 (as written on page 6). And for the smoke layer the 532 nm AOD was found to be 0.02 to 0.13 (page 7, Sect 3.1.). So the smoke impact was at least not dominating. This means that the AERONET observations cannot be used to derive smoke conversion factors. This point will be further discussed below.

To continue: Surface (in situ) observation cannot be used when discussing lofted layers. And the in situ measured aerosol values are most probably enhanced because of the advected central European haze. So, the final paragraph in Sect. 3 (before Sect. 3.1) makes no sense, and should be skipped.

P8, line 234: Sedimentation of large particles is not a good argument here. Smoke particles always show a pronounced accumulation mode, so difference in falling speed is low, when coarse mode particles are absent. Particle aging is more likely. Smoke aging process mainly occur in the first 36-48 hours after emission, and afterwards aging is slow. At the end of this aging process, the particles are usually spherical or almost spherical in shape. Particles show an almost perfect core-shell structure (coating, OC material) and the shell is often liquid at lower heights. And the probability that smoke particle are glassy (not perfectly round) increase with decreasing temperature. That could also be a reason that you saw a decreasing trend in the depolarization values with decreasing height.

P9, line 256: It makes no sense to me at all to use the actual AERONET data to derive smoke conversion factors. As mentioned, the AOD was obviously dominated by European pollution, so that the conversion factors reflect European fine mode haze properties. All the efforts to get proper conversion factors from AERONET (dust, smoke, marine, etc.) were done in regions with pure dust or marine or smoke conditions, etc. One should therefore use the conversion factors presented in this Ansmann 2020 paper, or you try to use the Polly multiwavelength information (inversion) to obtain the smoke volume concentration in the smoke layer together with the backscatter and extinction coefficients in these layers, and in this way the required smoke conversion parameters. Your conversion factor of 0.21 perfectly describes the conversion factor for urban haze. The smoke conversion factors are in the range from 0.12-0.15, and thus considerably lower.

P10, lines 288-289, please re-calculate the uncertainties by assuming 20% (BSC), 30% (LR), 20% (conversion factor from literature) and 20% (particle density), probably the uncertainty is 40-50%.

P10, lines 295-300, this is a 'pure' speculation about dust (only fine-mode dust, no coarse mode dust), is my feeling. On the other hand, the enhanced depolarization ratio can easily be explained by non-spherical smoke. Already small deviations from the ideal spherical shape causes depolarization as Gialitaki etal., ACP, 2020 shows.

P10, lines 301-318, These paragraphs do not make any sense. I would remove this part. It is pure speculation. Sure, you may have fine dust, but without the presence of any coarse dust? Is that possible? And again, non-spherical smoke is a convincing argument for the enhanced depol values.

Figure 6: If you include a 40 or 50% uncertainty bar to the mass concentration values, you do not need to speculate about any dust contribution!

In general, I miss uncertainty bars in Figures 6 and 7. Not many, but at least one or two per lidar and ceilometer profile!

P11, L319-334. All this should be removed, just speculation, simply not convincing! Impossible, to accept that as a reviewer!

Now we need conclusions: One conclusion should deal with the question: What is now the value of the ceilometer? The ceilometer is able to detect smoke layers even in the middle to upper troposphere? With what overall uncertainty? What about mass retrieval from ceilometer observations? Possible? Yes or no? Conversion factors for 910 nm are not available. How to proceed? ..with 910/532 nm smoke backscatter color ratios? ..to convert 910 backscatter into 532 nm backscatter for which smoke conversion factors are available.