

Interactive comment on “Composition of ice particle residuals in mixed phase clouds at Jungfrauoch (Switzerland): Enrichment and depletion of particle groups relative to total aerosol” by Stine Eriksen Hammer et al.

Anonymous Referee #1

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Hammer et al. consider the properties of particles within ice crystals, aka ice particle residuals (IPRs), at the Jungfrauoch, a high altitude mountain site in Europe. For this work they use a published ice selective inlet aka ‘Ice-CVI’, and compare this to the total aerosol via a simultaneous heated inlet sample to provide all, aka ‘total’, particles. In total, they consider $\sim 4\text{k}$ ICRs over 7 days and in the ~ -10 to -20 deg C range (i.e., mixed phase clouds). These particles were deposited on substrates for off-line analysis with electron microscopy (EM).

There are several papers over the last decade, many by the groups represented here,

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describing such measurements at the Jungfrauoch so this is not altogether novel. It does represent important data in an area of atmospheric science that is currently in need of more information. My major concern, however, is that the way ice residuals are described seems very simplistic and not of use to the broader atmospheric science community. For example, what is a C-rich particle? How would someone using AMS or a modeler compare this to their understanding of the atmosphere? Since studies of this type have been published, again, many by this group, I believe they here have a responsibility to make the results more comparable, and therefore more useful, to the broader community.

I therefore suggest the authors consider the following suggestions and, pending another review, that this paper could be published in ACP.

1. The literature seems to predominantly use the term 'ice residual' (IR) as opposed to IPR. Is there a reason the authors have suggested a new term? Is it different than IR? If so a comprehensive description – and difference from IR - needs to be made. As it reads it seems they are the same so, if there is no difference, could you please remain consistent with the literature term IR so as not to confuse the reader.
2. Page 3 Line 8 : The authors seem to suggest water vapour homogeneously nucleates ice at -38 deg C and 140% RH; but that is not correct. One must first have droplets. Please remove 'vapour or' from this sentence.
3. Page 5 Line 5&6 : From the intro, the authors 'assume' the IPRs are INPs, not 'consider'. This is a very important distinction. The authors discuss artifacts; thus they themselves show they can not make the direct association implied by 'consider'.
4. Table 1 : Most aerosol composition measurements show the presence of sulfate and SOA particles as a dominant, if not the dominant, aerosol, at least by number. Is the 'complex secondary particles' this class? This seems to be implied later in the Discussion but is never clearly stated. If so, even if it is an assumption, this needs to be stated for clarity and comparability to the literature. (note: this seems to be suggested on Page 10 but warrants more than 1 line of text).
5. Similarly, biomass burning particles are often noted as being of abundance just

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below sulfates and SOA. Is this the C-rich class here? If so please state this. I stress here: while the authors are reporting what they observe with their EM technique, they also need to make it clear their classes relate to common particles types if they wish to publish in a journal such as ACP. Please make these comparisons. 6. Similarly, what are Ca-rich particles? The discussion seems to suggest they are mineral dust? How are these associated with any common aerosol type? 7. The authors discuss observations of biological IRs in the introduction. These don't appear on Table 1 or Figures 4 or 5. Are they not observed or is there an instrumental reason they can't be detected? Are they a subset of the C-rich category? This needs to be stated clearly in the paper, especially in Figure 5, as it goes directly to comparison to the literature on ice residual composition and in the location where Table 1 is described. 8. Page 7, Line 6. Please remove 'Unfortunately, only'. Data are what was collected. 9. Sample S-2b looks rather like a local combustion event. This seems to be implied later in the discussion. If it is please state at the location of the Figure for clarity to the reader. 10. Given this, could you please present figure S2 at this location – move it into the main paper from the supplement - for the samples to give the origin some context? It is mentioned in the text in the discussion but really needs to be given at this location. 11. Sample S-5b looks rather like a mixed mineral and perhaps combustion event. Is this so? It again does not look like a clean troposphere. Is this so? If it is please state at the location of the Figure for clarity to the reader. Please also see last comment re: back-trajectories. 12. I am confused with Section 4.1, 'Methodological Problems'. I believe this all needs to do in the Experimental section (2), not held until after the data is presented. First, Figure S5 seems to indicate that most of the collected particles are artefact. Is this the case? If so please state the percentage in the Artefact section. Second the reader goes through the results but only after they are presented learns there are issues with the inlet and perhaps the EM data which appear to outnumber the real data by several factors. This is not a logical order. This all needs to be clearly stated and placed in Section 2, not held to Section 4.

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