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Atmospheric Chemistry and Physics Discussions

Interactive comment on "n-Aldehydes (C_6–C_{10}) in snow samples collected at the high alpine research station Jungfraujoch during CLACE 5" *by* K. Sieg et al.

Anonymous Referee #2

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This paper reports concentrations of higher aldehydes in fresh snow sampled at Jungfraujoch and attempts to interpret them. The measurements are interesting, appear to be conducted well, and add to the small body of data available on higher aldehydes in snow. The discussion can be divided in 2 parts: first the physical mechanism of incorporation of theses aldehydes in snow, then their possible chemical mechanism of formation in the atmosphere.

While the data are interesting, I was disappointed by the interpretation and the discussion. The authors are studying a complex reactive multiphase system consisting of 3 or 4 phases: air, aerosols, snow and most likely super cooled water droplets. They

perform just one measurement of melted snow and analyze one family of chemical species. In my opinion, it is close to impossible to interpret the data under those conditions. As a result, their conclusion that higher aldehydes are in aerosols scavenged by snow, although reasonable, cannot be substantiated by the data and are only speculative. Likewise, their proposed formation mechanisms of aldehydes, in the absence of data on co-products or on the postulated initial reactants, although again plausible, are only speculative. In an attempt to make constructive comments, I very strongly recommend that their data be analyzed in the light of the aerosol measurements that were also performed within CLACE 5. There appears to be some data sharing issues in the consortium, but these need to be resolved in the interest of science. Furthermore, I am surprised to see that the physical state of the snow was not observed. In particular, what was the degree of riming ? How can chemical data of soluble compounds be interpreted if the fraction of rimed ice onto snow crystals is not estimated ? The authors themselves seem to be aware of this, as they cite Kalina and Puxbaum (1994) and Poulida et al. (1998) and it is therefore extremely surprising that the adequate observations were not made. They can also read Mitchell and Lamb (1989), JGR 94, 14831. In summary on my general impression, I suggest that future campaigns be organized in such a way that more phases and chemical species are analyzed simultaneously, and that an adequate data sharing protocol be agreed upon, to make the best possible use of the data. Unfortunately, at present, given that too many variables are left unconstrained, I do not see how the quality of this paper can be improved to meet my idea of ACP standards.

Minor comments include :

1- Adsorption is ruled out as an incorporation mechanism (p. 8080, l. 17). However, testing this requires strict sampling and analysis procedures, given the reversibility of the process, as stressed by the authors. Species can desorb, but can also adsorb from ambient air. Not enough experimental details are given to tell whether adsorption can indeed be tested.

2- P. 8081, l. 5. A variable degree of riming could also explain the large variations in the concentrations of aldehydes in snow. Riming acts on gases solubility and also on aerosol scavenging. In one precipitation event, riming can vary on the timescale of minutes, so that it is essential to observe the degree of riming of the sample taken.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8071, 2009.