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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric data over a solar cycle: no connection between galactic cosmic rays and new particle formation" *by* M. Kulmala et al.

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We thank Reviewer 2 for his/her constructive comments. Our detailed answers to his/her comments are as follows:

The authors show convincingly that they don't observe any significant correlation between CRII and the occurence of particle formation events at the SMEAR II site over a period of 12 years. The authors then generalise this finding to the whole tropospheric column. This extension may be misleading since formation of lowlevel liquid phase clouds and formation of upper tropospheric ice clouds are very different processes. While a particle's ability to act as liquid-phase CCN is largely a function of its size

(Dusek et al., 2006), the nature of ice nuclei (IN) is a matter of ongoing investigation. Only a small fraction of the particles available in the upper troposphere will act as IN (e.g. Twohy and Poellot, 2005), and the formation of ice-clouds is often limited by the availability of IN (Spichtinger et al., 2002). While particles formed from CRII induced nucleation may be a small fraction of the total particle burden in the upper troposhere, it may actually be charged particles that serve as IN.

We agree with the reviewer. However, we would like to point out that we do not comment on the possible effect of CRII on cloud formation in any other way except for making the point that cosmic rays do not seem to be responsible for the fraction of CCN formed via nucleation events. We draw no conclusion regarding the other CCN or IN sources.

The lack of correlation between CRII and particle formation events in the boundary layer, measured over a whole solar cycle, is a very convincing argument for the point the authos's want to make. The conclusion for the tropospheric column however is based only on one aircraft field campaign. In order to make a similar convincing point for the tropospheric column, another aircraft campaign in the opposite phase of the solar cycle would be helpful.

We agree with the reviewer. We have softened the conclusion related to the generalization of the result to the whole tropospheric column.

The conclusion for the tropospheric column is based on aircraft-based NAIS measurements, which is a rather new technique. Neither in this article nor in the article quoted for the airborne NAIS are possible aircraft inlet effects on the cluster charge distribution discussed. An aircraft inlet, decelerating the sample by roughly two orders of magnitude, can be seen as a type of nozzle. It is a well known experimental fact that a nozzle may change the charge distribution of particles in a sample. It is not certain that such an effect is relevant for airborne NAIS measurements, but the author's should certainly investigate this possibility.

ACPD

9, C10384–C10387, 2010

We agree with the reviewer that this is indeed a point that is worth further investigation – even on top of the work that has been conducted by Mirme et al. (2010). Since the observations on the aircraft are only a minor part of this work – the main point being the lack of correlation between CRII and boundary layer nucleation – we think that a detailed investigation of the inlet on the charge distribution is out of the scope of this paper and leave it for future studies.

Section 2.1.1: The full size range covered by the DMPS at Hyytiälä should be mentioned.

We have added the measured size range to the revised MS.

p. 21537, ll. 20ff.: The article should be in general understandable without consulting the quoted literature. The particle nucleation rates J should therefore be properly introduced and explained. The reference to Figure 7 should probably a reference to Figure 8.

We have added an equation describing the neutral and ion-induced contributions to observed particle formation rates to the revised MS. The reference to Figure 7 has been replaced by a reference to Figure 8.

p. 21538, ll. 4 - 9: Before drawing conclusions from Figure 9, the general features visible in the figure should be described.

We have added a sentence summarising the contents of the figure to the revised MS.

Table 2: The particle growth rate GR mentioned in this table should be introduced and defined somewhere in the article.

The reviewer is correct. We have added a sentence referring to this variable (the particle growth rate) to the revised MS.

References:

Mirme, S., Mirme, A., Minikin, A., Petzold, A., Hõrrak, U., Kerminen, V. -M., and Kul-

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9, C10384–C10387, 2010

mala, M.: Atmospheric sub-3 nm particles at high altitudes, Atmos. Chem. Phys., 10, 437-451, 2010.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21525, 2009.

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9, C10384–C10387, 2010