Atmos. Chem. Phys. Discuss., 3, S1065–S1066, 2003
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative effects of tropospheric ionisation" *by* K. L. Aplin

Anonymous Referee #3

Received and published: 22 July 2003

General Comments: This manuscript raises an interesting possibility for an influence of hydrated atmospheric ions on the radiative properties of the atmosphere. However, there are some weaknesses in the manuscript that detract from its overall impact. The weak sections should be revised or removed before it could be suitable for publication.

Specific Comments: In section 3, last paragraph, several values of concentrations, in units of cmˆ-3 are given for laboratory and atmospheric situations, and are related to the amount of IR absorption expected. But it is not concentration, but column content, measured in units of cmˆ-2, that determines absorption. So some atmospheric path length must be specified if such a discussion of absorption is to be applied to the real atmosphere. There is a discussion of the atmospheric path in the later section 5, but no reference to it in section 3. It is not at all clear what the author is trying to say in this part of Section 3. In section 4, pgh. 3, it is stated that the CR count rates smoothed and used for constructing figure 2 were obtained from the Sodankyla Observatory, that

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measures neutrons from a broader spectral range of CR energies than is present in the tropics. This is true; it means that the highly variable low energy components (below about 8 GeV/nucleon), that contribute most to the Sodankyla CR variations are not present in the Niger atmosphere. The CR anomaly in the tropics is always several times smaller, as a percentage of the mean, than at high latitudes. It may be that the percentage variation in the Niger CR flux is considerably smaller than the percentage variation in the Lw-down radiation, in which case there would be a problem for the theory. At any rate, the actual percentage variations of the Sodankyla CR flux and the Lw-down radiation should be given, if figure 2 is to be retained. However, there is another problem with this figure. The quoted statistical significance of P = 97.6% taking into account persistence appears to be incorrect. Both the CR flux and the Lw-down flux were smoothed with one day moving averages, and most of the calculated correlation coefficient seems to have been generated from the one excursion in each trace during one day (228-229). So the persistence was actually about one day. With a sample size of one event the statistical significance is very low. Also, while there seems to be a good (anti)correlation of the two traces for that day, this is not so for the variations on day 230-231. The alternative explanation of a chance agreement of an increase in low energy CR at Sodankyla and a meteorological event in Niger on day 228-229 cannot be ignored. Unless a more convincing case can be made for a statistically significant correlation, and with the percentage variation of CR flux, scaled to the latitude of Niger, being comparable to or greater than the percentage variation of Lw-down flux, Figure 2 and the associated section should be deleted.

Technical Correction: The reference to Gash et al 1977 has a different title and an omitted author compared to that in the paper published in J. Hydrol.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3205, 2003.

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Interactive Comment