**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Charging of ice-vapor interfaces" by J. Nelson and M. Baker

Anonymous Referee #1

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General Comments

The paper consists of two only loosely connected parts. In the first one the authors expand Fletcher's theory of charge distribution in ice crystals to the case of growing or sublimating ice crystals. They find that a moving crystal surface can change the surface charge density by a factor of 10.

In the second part they consider charge transfer during ice–ice collisions. They devise a simple conceptual model that is able to explain the existence of the negative charging region in thunderstorms. Other observations fit also nicely in the predictions of the presented model.

Both topics are certainly interesting for ACP, however there are some points that should be adressed before the paper can be accepted.

The main deficiency of the paper is that the solutions of the numerical model of the

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first part (see Eq. 1) are not presented, so the reader must believe the qualitative arguments the authors give. To my view these solutions *must* be presented in a number of figures before this paper can go to ACP.

I recommend that the paper is split into its two parts (i.e. two papers), and that the model description and the simulations (which are promised without commitment to be given elsewhere) are included in the first paper that then would only deal with the surface charging.

Specific Comments

1.) Eq. 2 states that creation–recombination balance does not occur when $v \neq 0$. Isn't this only true at the surface where $d' \neq 0$? It is conceivable that deep inside the crystal $d' \approx 0$ such that near cancellation of defect production and recombination may occur.

2.) Eq. 3 seems to be simplified too much. The growth speed v cancels out in the present form, even when the dropped $\delta l(x)$ term is included. This does not make sense. I think that it is necessary to retain the term $v^2(\delta d)'$. Would inclusion of this term change the discussion thereafter?
Furthermore I am concerned about the assumption that δd is small. It is said that the defect concentration increases towards the crystal surface by a factor of 10^6 (for D defects). In view of this tremendous effect, how can one be sure that δd should be a small perturbation?

3.) The sentence before Eq. 7 introduces slices of thickness Δx in the model. This is an unnecessary complication, because slices are mentioned neither before nor after this one place. Eq. 7 itself contains the numerical value 1923. It would be easier to read if a symbol (e.g. ρ_i) would be used with an explanation of its meaning in the following text.

4.) In the paragraph before Eq. 8 a symbol σ is introduced but not explained. Furthermore, the derivation of Eq. 8 should be given; from the information given in Fig. 4, the

text, and Eq. 7 it is not possible. The text after Eq. 4 up to the end of the subsection is hard to read because of the wild mixture of text, numbers, formulas, etc.

5.) In section 6.3 the authors speculate on the role of electrostatic attraction and pressure melting for the build–up of snowflakes. Aside from blizzards, I see snow mostly to fall gently through the air. I doubt whether there is enough kinetic energy for pressure melting to occur in this case.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 41, 2003.