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Reply to the comment on "A new model of charge transfer during ice–ice collisions" [C. R. Physique 4 (2003) 721–722] ☆,☆☆

Réponse au commentaire sur « Un nouveau modèle décrivant le transfert de charge lors d'une collision entre particules de glace »

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The comment by Dash and Wettlaufer [1] (hereafter, DW) gives us the opportunity to clarify some points in our theory of charge transfer via defect advection with pressure melting (DAPM) [2], and to elucidate the differences between our model and theirs (Dash, Mason and Wettlaufer [3]; hereafter DMW).

DW criticize the following three aspects of our theory: (a) vapor growth produces well-facetted crystals "under the conditions of the experiments"; (b) collisions cause pressure-melting; (c) in collisions between a sharp point and a flat surface, pressure-melted liquid is driven from the former to the latter. Here we discuss each of these criticisms in turn.

(a) It is not clear what 'conditions' DW refer to, or why they thought the facetting was relevant. In the actual atmospheric application, as well as the cloud-chamber experiments, the ice crystals involved are freely growing in air; such crystals are clearly facetted. However, the DAPM theory does not require perfect facets; the only requirement is that the collisions involve relatively sharp points striking relatively flat surfaces. In the absence of some unknown (and unlikely) force that causes broadside collisions or point-point collisions, our requirement is a safe assumption.

(b) DW state that elasticity theory is not consistent with the mass transfer in our theory. We have no argument with their statements here. However, the criticisms do not apply to our mechanism because we do not use elasticity theory to predict the melt flow. DW perhaps did not understand this point, which, we admit, was not explained as clearly as possible in [2].

In a realistic atmospheric situation, the collision likely involves a crystal corner impacting a relatively flat region of surface; in such a case, in our collision hypothesis: (1) a very thin region first melts, which tends to cushion the impact; and (2) the melt (which is under high pressure) must be pushed aside before more crystal can melt. We agree with DW that the melting should cause some local cooling; however, the flow of fluid away from the contact point can generate considerable viscous heating. Indeed, in our calculations, most of the kinetic energy that is lost in the collision is used to push the melt aside.

(c) Even without the phase change and heat flow, the collision-deformation-mass exchange problem is complex [4]. Thus, in both DMW and DAPM, assumptions had to be made about the direction of mass flow. Our assumed direction of mass transfer is plausible given the local environment of the collision site in which fluid travels horizontally at high speed. The assumption is further justified by the fact that it can explain (whereas DMW would not) why the observed charging is largely independent of the temperature and impurity content of the flatter surface [5–7].

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DAPM, like DMW, is built on ideas from the earlier literature, with modifications. This is the way science progresses. We summarize below the ideas that are common to both theories, with their origins, and some of the important differences between them. (Items marked 'a' are similarities of DAMP and DMW; those marked 'b' are differences.)

1a. In both DAPM and DMW surface charging occurs via differential motion of charge carriers near the ice surface. This idea appears to have originated with Reynolds [8], and was further elaborated by Latham and Mason [9], Jaccard [10], Fletcher [11], Petrenko and Ryzhkin [12] and others.

1b. In DAPM, as in Jaccard, the dominant charge carriers are the L and D defects. In DMW they are positive ions.

2a. In both DAPM and DMW growth enhances the surface concentration of charge carriers.

2b. In DAPM the surface concentration is increased by a sweeping effect of growth/sublimation. DMW invoke an additional mechanism.

3a. In DAPM and DMW charge transfer between particles is accomplished by transfer of melted charged mass. This idea probably originated with Turner and Stow [13] and was elaborated on in a paper coauthored by GD and MBB [14].

3b In DAPM the melting occurs via classical pressure melting (discovered over a century ago) rather than a new and undocumented process, as in DMW.

4a. The details of the collisions in both DAPM and DMW depend necessarily on untested hypotheses because relevant data are nonexistent.

4b. In DAPM, the melting is assumed to occur in stages. The DMW theory implies that all the melt is formed simultaneously. By not including the work to push the melt aside, they predict a melt mass about 12 times greater than ours.

5a. Both theories were tested on laboratory observations.

5b. Our work is focussed on, and (with the stated assumptions) gives good agreement with, data on collisional charge transfers in the atmospherically relevant case; vapor grown ice crystals colliding with graupel. The DMW theory was focused on rebounding collisions between vapor grown ice surfaces at different temperatures, a phenomenon of little relevance to atmospheric clouds.

DW assert that we have only modified their model in detail, and presented it as new. As we have indicated above, and indeed as their own criticisms of our model emphasize, the differences between the two models are quite important. Given these differences, and our long involvement with the ideas in DAPM, DW's aggressive assertion is simply incomprehensible.

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