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Interactive comment

Interactive comment on "The influence of surface charge on the coalescence of ice and dust particles in the mesosphere" *by* Joshua Baptiste et al.

Anonymous Referee #2

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This paper discusses the coagulation of small particles in the earth's upper mesosphere. It explores in detail the coagulation of particles which carry the same sign charge, which is a process that is not currently included in atmospheric particle charging models (to my knowledge), because it was assumed to be negligible. However, the authors show that coagulation will occur if the particles collide within a window of velocities, so long as the charges are not localized on the particles and roughly facing each other during the collision. The effect is shown to be particularly important where one of the particles is a relatively large ice particle. This is potentially of geophysical interest. The work of Hervig and co-workers has shown that mesospheric ice particles are heavily "contaminated" with meteoric smoke particles (MSPs) by up to 2% by

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volume, which is quite difficult to explain. The work here indicates that once a few ice particles form, they could quickly "mop up" the smoke particles in their vicinity. This means that instead of H2O condensing on all the MSPs to produce a large numbers of small ice particles which are sub-visual, a smaller number of larger ice particles will form. This is a topic that the authors should explore further (not necessarily in a revised manuscript, but perhaps in future work).

I therefore think the paper introduces a potentially important idea, and should probably be published in ACP. I enjoyed the tutorial nature of the description of how the coagulation of like-charged particles can occur. The paper is well-written and appropriately illustrated.

However, it first needs fairly major revision to put it into an atmospheric context. The authors do not discuss the nature of the dusty plasma in the 80 – 85 km region. What makes it challenging to model is that the concentration of plasma and dust particles is roughly the same. Were one or the other in a large excess, life would be a lot easier! The particles are present in a NO+/O2+ and electron plasma, which is almost exclusively produced by photo-ionization of N2 and O2. Cosmic ray-induced ionization is only important below 70 km, and ionization caused by energetic particle precipitation only very occasionally produces enhanced ionization below 85 km. Because electrons are much more mobile than ions, particles of all sizes down to r > 0.5 nm are mostly negatively charged. However, dusty plasma models show that only around 6% of MSPs are charged (consistent with the concentration of charged MSPs measured by rocket-borne dust detectors). This means that the probability of a charged MSP colliding with another charged MSP, rather than a neutral particle, is around 1/16. So the ion-ion coagulation rate would only be important if such collisions resulted in a much higher probability of the particles coagulating, compared with ion-neutral or neutral-neutral collisions. This context of the work is not discussed in the paper.

Another important point is that the authors do not produce a quantitative comparison of the coagulation rate of like-charged particles with charged-neutral or neutral-neutral

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rates. This leaves the reader not knowing whether this process could be significant, or is of negligible importance! What would be extremely useful is to produce rate coefficients for like-particle coagulation that could be applied in dusty plasma models. These models tend to use the parameterized rate coefficients published by G.L. Natanson in 1960 (On the theory of the charging of microscopic aerosol particles as a result of capture of gas ions, Sov. Phys. Techn. Phys. 5(5),538–551). This venerable work covers ion/electron association with particles of opposite or no charge. Would it be possible in future work to produce rate coefficients which would estimate the coagulation rates for two particles of specified radius and charge, at a given temperature? It would be worth at least commenting in the concluding section of the paper about whether this might be possible.

Minor points to address: line 11: "are heated to evaporation temperatures" sounds rather vague. I suggest replacing with "are heated to temperatures above 1800 K, at which point the particles melt and rapidly evaporate". You could cite here one of the recent papers from Plane's group in Leeds e.g. Carrillo-Sánchez, J. D., J. C. Gómez-Martín, D. L. Bones, D. Nesvorny, P. Pokorny, M. Benna, G. J. Flynn, and J. M. C. Plane (2020), Cosmic dust fluxes in the atmospheres of Earth, Mars, and Venus, Icarus, 335, art. no.: 113395.

line 12: not all meteor-ablated species will ionize (this occurs either through hyperthermal collisions with air molecules, or subsequently through charge transfer reactions, so is dependent of speed of entry and height at which ablation occurs)

line 24: these references are quite old. The most recent work on MSP coagulation and atmospheric transport is (I think): Brooke, J. S. A.; Feng, W.; Carrillo-Sánchez, J. D.; Mann, G. W.; James, A. D., Bardeen, C. G.; Plane, J. M. C. (2017), Meteoric Smoke Deposition in the Polar Regions: A Comparison of Measurements With Global Atmospheric Models, Journal of Geophysical Research – Atmospheres, 122, 11,112–11,130.

line 25: Note that the “influence of gravitational force” is negligible on MSPs. They are transported by the residual atmospheric circulation. Ice particles do sediment, but one assumes you are referring to MSPs in this paragraph.

line 43: Should include Hervig’s latest work on this topic: Hervig, M. E.; Brooke, J. S. A; Feng, W.; Bardeen, C. G.; Plane, J. M. C. (2017), Constraints on Meteoric Smoke Composition and Meteoric Influx Using SOFIE Observations With Models, Journal of Geophysical Research – Atmospheres, 122, 13,495–13,505.

line 65: the pressure at 80 km is about 0.01 mbar, which is the region that you are dealing with! So why do you say the pressure “is far below” this?

Table 1: “Common particulates found in the MLT region which are considered in this study.” Firstly, no-one has successfully retrieved MSPs from the MLT and shown that they consist of FeO and MgO. Secondly, FeO and MgO are rapidly oxidized by O3 and O2, and recombine with H2O and CO2. So it would be extremely surprising if particles made of pure FeO or MgO exist in the MLT. I suggest that you indicate here that these are examples you have chosen to illustrate the sensitivity to dielectric constant, but that actual MSPs are likely to be Mg-Fe silicates or mixed oxides (citing the Hervig paper above).

line 86: the temperature of the MLT ranges from $\sim$110 to 240 K. I think you mean the typical temperature at high latitudes during summer.

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