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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of meteoric smoke distribution to microphysical properties and atmospheric conditions" *by* L. Megner et al.

Anonymous Referee #2

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This paper is really an update of the seminal study of Hunten et al. (1980), which developed a 1-D model to investigate the likely evolution of meteoric smoke particles in the mesosphere and stratosphere. The present paper also uses a 1-D model, and is able to replicate the earlier study using the same meteoric ablation input function, eddy diffusion coefficient etc. The main part of the paper is then a study of the sensitivity of the predicted particle size distribution profiles to factors such as the meteoric input function, eddy diffusion, vertical wind velocity and particle coagulation rates. The value of the paper is in showing that the particle size distribution is particularly sensitive to the vertical wind velocity and the coagulation rates. A seemingly important conclusion is that during summer at high latitudes, when there are large upward winds, the number density of particles will be less than about 1 cm-3 at the height at which noctilucent

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clouds form. Since the ice particle concentration is several 100 cm-3, is one to conclude that meteoric smoke particles cannot be the main ice nuclei for noctilucent cloud formation? This point needs to be discussed properly in the paper.

The paper serves a useful purpose in identifying which of the (many) unknown factors in smoke particle formation and evolution are likely to be important. However, as the authors point out, the use of a 1-D model is not really appropriate to this problem because the timescales of particle growth and sedimentation are similar to meridional transport times. Hence, this study is a first step. The paper is also very clearly written and appropriately illustrated, and so should certainly be published in ACP. There are, however, five perhaps major points that need to be addressed.

1. Besides expanding the discussion of whether there are sufficient smoke particles to act as ice nuclei (see above), another question is whether particles of radius less than 3 nm are actually much good as nuclei. The authors assume this to be the case (p. 5366, line 6), but recent research on ice nuclei shows that size seems to be a more important criterion than composition of a nucleus in determining its efficiency.

2. The authors assume that coagulation is coalescent (p. 5363, line 24). While this makes the modelling easier, is it realistic given the nature of the smoke constituents?

3. The authors assume that because particles collide with relatively low kinetic energies, the particles will not "bounce off" each other (p. 5361, line 20). But the requirement for two particles to stick together is that the binding energy between the two fragments is large enough to overcome the decrease in entropy involved. At around 200 K, this will be of the order of 0.6 eV. What evidence do the authors have for thinking that this is the case?

4. In the coagulation code, the authors use size bins that increase with a volumetric ratio of 1.6 (p. 5363, line 27). This seems to be quite coarse, and it is not obvious why they use such a large ratio when the particles do not grow bigger than a few nm.

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5. The vertical extent of the model is from 10 to 110 km. Steady-state is apparently achieved above 65 km after 3 (model) months. Presumably approach to steady-state in the stratosphere takes substantially longer. Why then are the particle column densities reported between 10 and 100 km (page 5371)? What about the particles above 100 km (which are not insignificant according for figure 4), and how is the stratospheric population dealt with after only 3 months of integration?

There are also a few typographical errors (very few, in fact!):

p. 5370, line 9. miniscule

p. 5371, line 22. relative to

p. 5373, line 5. particles

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 5357, 2006.