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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.

L. Megner et al.

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We would like to thank the anonymous reviewers for their helpful comments and suggestions. We here reply to the comments in a point to point manner.

Anonymous referee 1

1) Seasonal variability on meteoric input

The model does not have any temporal information, it is run to a steady state using a constant input. The effect of varying meteoric input, due to seasonal variations or simply due to uncertainties in the meteoric influx, is studied in section 4.2

2) Uncertainty of meteoric influx

We have included the suggested changes in the manuscript.

3) Height of meteoric ablation

We have investigated the effects of an even higher ablation height (peaking around 105 km) and found that this is has little influence on the distribution. We have also studied the effects of different ablation heights using a 2-dimensional model and found negligible effect of the ablation height even up to 110 km. This work is still in progress. We have included a statement about the effect of higher ablation altitudes in the manuscript.

4) Percent

Of course, thank you.

5) Dependence on entry angle of meteoroid

Indeed, this dependence should certainly be mentioned in the text. A different entry angle would simply shift the height at which the ablation takes place, the angular dependence is therfore implicitly included in the study of the sensitivity of ablation height.

6) Meteorites are by definition found on the ground

Indeed, this will be clarified.

7) "Is there any previous work that have made any conclusions which are now questioned by the sensitivity issues of the model to input parameters presented in this manuscript?"

Previous work has been using only what we call the reference profile, without taking the uncertainties or variability of this profile into account. The relevance of this work is thus to point out that this profile is by no means a given fact, and at the same time determine which of the unknown factors are important for the smoke distribution, so that further studies can concentrate in these factors.

Anonymous ref 2

1) Ice nucleation

A discussion about lack of nanometer-sized particles at the summer mesopause and their role as nucleation kernels for NLC has been added to the manuscript. With the comment about the minimum particle size required to act as ice nuclei, we believe that the referee is referring to a recent publication (Dusek et al.,2006). In this paper, the authors study cloud particle nucleation on aerosol particles in the troposphere and conclude that the aerosol particle size is more important than the chemical properties of the aerosol. Unfortunately, it is not clear at all, whether these results can anyhow be transferred to the problem of ice particle nucleation in the upper mesosphere where temperatures are much lower (i.e., down to less than 130K) and equilibrium vapour pressures are many orders of magnitude smaller than in the troposphere. The problem of ice nucleation in the upper mesosphere was recently reviewed in Rapp and Thomas (2006) who concluded that our current knowledge on the ice nucleation process under the thermodynamic conditions of the mesopause region is poor and that new laboratory investigations in the appropriate temperature and vapour pressure range are mandatory for an improved understanding. In light of this problem, the only feasible approach to handle nucleation of ice particles in the mesopause region has been to use classical droplet-theory and extrapolate the involved constants to the thermodynamic conditions of that altitude range. This results in a minimum radius of approximately 1 nm for a particle to act as a nucleation kernel. We have added a brief summary of the above discussion to the manuscript.

2) Coalescence

Yes, assuming that coagulation is coalescent is a simplification in order to make the modelling easier. However, since the nature of the coagulation is unknown, it cannot be modelled in detail. Until the mechanism of the coagulation is better understood, we therefore have to model it in generalised way. The most general way to describe coagulation is stochastic growth, in which the particles develop towards spheres. Hence, coalescence, which also results in spherical particles, is not a bad assumption of this

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process. Particles resulting from coalescence are dense whereas particles resulting form stochastic growth may be more porous. The implication of this is studied in section 4.5. A short discussion about this has been added to the manuscript.

3) Sticking efficiency of smoke particles

Yes, there is today an uncertainty in our knowledge of the sticking of nanometre-sized dust particles. This is implicitly taken care of in section 4.5 where the effects of a lower coagulation efficiency is discussed. We have pointed this fact out in the up-dated version of the manuscript.

4) Size bin ratio

We have studied the effect of varying the size bins and found negligible change in the resulting smoke distribution using volumetric ratio between 1.4 and 2.0. This can also be seen in figure 2 where we reproduce the result by Hunten et al. (1980), since that model uses a volumetric size bin ratio of 2.0.

5) Column density

Yes, to reach a steady state in the stratosphere would take much longer than 3 months. This is too long for a steady state ever to be reached in the real atmosphere, hence it would not be meaningful to run the simulation that long. However, below 65 km the particle number density drops sharply, so that the particles here do not contribute significantly to the column number density. The column number density is calculated using all the model domain (10 -110 km) and not only up to 100 km as reported in the article. This is a typographical mistake which has now been corrected.

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