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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 #1

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General Comments:

In this manuscript the authors investigate the sensitivity that the modeled meteoric smoke distribution shows with respect to initial parameters such as meteoric input, height of maximum ablation, atmospheric state, vertical eddy diffusion, coagulation, material density and vertical wind. Some of this variables are under investigation and the authors show that the result predicted smoke distribution seems to be mostly affected by changes in the input vertical wind and coagulation efficiency without showing severe effects by changes in other parameters. In general the paper is well thought and appropriate for publication on ACP. The manuscript requires some editing. I found several misspelling along the text. However, before considering for publication I list a series of points I would like the authors to address.

1) The authors state on the abstract that vertical winds provides global and temporal variation in meteoric smoke. What about seasonal variability on meteoric input. For example, Janches et al. (2004) and Singer et al. (2004) have recently shown that at high latitudes the seasonal differences in meteor rates are very large. Would this variability affect at any level the smoke distribution? Or this model does not have yet any temporal information?

2) In section 4.2 the authors state: "The studies giving the lowest estimates of the total influx (Mathews et al., 2001) has omitted part of this mass range...." It is true that this estimates are the lowest so far... but the "omitted part of" the mass influx is not a proven fact. I suspect the authors are somehow influenced by the work presented in von Zahn (2005) which is just a series of speculations based on personal opinions and where a large fraction of references, representing more than 5 years of recent work, have been left out. These works are strong arguments against the claims presented in that paper. Unfortunately, von Zahn (2005) is a proceedings from a conference which probably did not undergo a through review nor give the chance for a response. The authors could, for example, refer to ReVelle (2004) to see how the Arecibo derived flux does agree quiet well with newly optical and satellite derived fluxes. I could develop in detail arguments against the claims presented in von Zahn (2005) if the authors are interested, however I think such discussion would escape the scope of the manuscript since the model doesn't even seem to be too sensitive to changes in the meteoric flux. I suggest the authors remove the "has omitted part of this mass range" portion of the sentence and perhaps change it to something like "The total meteoric mass flux is a quantity under current investigation which estimates varies almost an order of magnitude. The lowest given by Mathews et al (2001), while the largest.....".

3) Throughout the paper and specifically in section 4.3 the authors use as the meteoric ablation altitude the 80-100 km altitude range based mostly on the Hunten et al. (1980) paper. Although that paper is a milestone for this type of work, there has been a number of most recent publication with data and theory that indicates that micron-size

ACPD
6, S1620–S1623, 2006

Interactive Comment

EGU

extraterrestrial objects seem to ablate at much higher altitudes (100-120 km; peaking at 105 km). These papers are mostly using high power and large aperture radars and examples of the measured distribution can be found in Janches et al. (2003 and reference therein). Also excellent agreement between observations and ablation theory can be found in Janches and ReVelle (2005). How does the meteoric smoke modeling would change with these altitude changes. Note that most of the work the authors refer to (Hunten et al. 1980 and Koloshnikova et al. 2000) is theoretical with no validation through observation.

4) Section 4.1 at the end of the second paragraph the authors wrote “4% of percent of the particles”... It is either “4%” or “four percent of the particles...”

5) First paragraph of section 2 the authors state: “At which altitude this happens depends in the speed, size and composition of the meteoroid...” Actually it also depends strongly on entry angle for particles of this size. Meteoroids of these size don’t come from random directions but from fairly well known radiant directions. It is also known now that if these angles are large, they may ablate even at higher altitude than those discussed earlier (see Janches and Chau, 2005 and Janches et al. 2006). May be this question is way too far from the scope of this manuscript, but would different entry angle distributions alter the model results?

6) First sentence of section 4.5 the authors state: “The majority of the meteorites found in the ground...” This is a redundant sentence... if it is found in the ground is a meteorite... I suggest remove “found in the ground” from the sentence.

7) My final question/comment of this review is about the relevance of this work. 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?

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ACPD

6, S1620–S1623, 2006

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