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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Isoprene oxidation products are a significant atmospheric aerosol component" *by* S. N. Matsunaga et al.

**Anonymous Referee #1**

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General comments:

The manuscript investigates the aerosol production by isoprene oxidation, supported both by field experiments and model results. The work presented here is original and can be useful in global model studies due to the proposed simple parameterization which has a low computational cost. However, the manuscript requires some major improvements prior to its publication.

The main weakness of the presented work is the large uncertainties associated with the results, which can substantially change the atmospheric importance of the aerosols produced from isoprene. When looking at the scattered plots of figure 3, I would expect

to see 3 sets of equations as the main result of this work: one with the maximum predicitions, one with the minimum, and one (the already submitted ones?) that would be the best guess. My feeling is that when using the minimum and maximum equation sets in a global model you will get higher uncertainty than the range of 10-120 Tg/year.

Specific comments:

1) general comment for all equations: you should include the plus/minus values that describe your uncertainty.

2) page 11146, line 23, and figure 1: I wouldn't so easily accept that the model reproduces accurately the measurements based on figure 1, since there are two secondary maxima in the measurements, that are not reproduced by the model.

3) page 11147, line 18: Since temperature is a very important parameter to the partitioning of compounds between the gas and aerosol phase, a comment concerning the temperature variation during the measurements is needed. The temperature effect needs also to be taken into account in the global model calculations, since it will affect considerably the partitioning of the conpounds.

4) page 11148, line 2, and figure 3a-c: It is hard to believe that there is linear relationship. Especially for figure 3b-c, the r2 is $<$0.2. The scatter of the points suggest that there is a parameter missing. This could be either temperature (see comment 3) of the pre-existing aerosol mass, where the gas phase compounds can condense on. Indeed, this factor is not taken into account, though most secondary organic aerosol measurement and model approaches do.

5) page 11148, equations 2a-c: the r-square values and the number of points considered, are needed to indicate the statistical significance of the results.

6) page 11148, line 23: please mention briefly the "other controlling variables".

7) figure 2: In all 3 charts, the model results look very similar when comparing the first with the second day, though in the second day the observed concentrations of



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GA, HA and MG show a local minimum at noon. As a first guess, this could be due to reduced photochemistry (more cloudy?). The authors should mention in the box model description (page 11146, line 23) how they treat photochemistry in the model. Do they constrain it with photolysis rate measurements, or they just have a typical expected light intensity for the site? Additionally, the model values go to almost zero during night, while the measured ones not. Is there a nighttime source missing in the model? It should not be fast removal, since in the case of MG the small maximum that is observed during night (figure 2c), indicate a potential nighttime source.

8) figure 3: As mentioned earlier, figure 3 has a considerable scatter that increases the uncertainty of this work. This is reflected by the low r-square values. It might worth looking at the 4 different cases presented there separately, since the conditions of the experiments might give indications of other important parameters (e.g. temperature, particle acidity).

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 11143, 2005.