

Interactive comment on “MALTE – Model to predict new aerosol formation in the lower troposphere” by M. Boy et al.

M. Boy et al.

Received and published: 12 July 2006

Dear Referees,

thanks for your constructive comments and your help to improve the value of this manuscript. Below I will give short statements to the different points from both of you. However I have to mention that the manuscript was submitted in the beginning of this year and the model runs were mostly done at the end of last year. During the last 5 or 6 month the model was improved in different parts and so I will prepare new runs for the final version of the paper to include most up today results from MALTE and submit this new version of the manuscript asap to the editor.

Statements to comments from Referee 1:

1.) The major comment was the use of measured sulphuric acid concentrations instead

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of calculated ones. To some extent I agree with the referee about this point and I tried for all days the use of measured concentrations. However, finally there are some reasons why I would still prefer to use the calculated sulphuric acid concentrations instead of measurements. First and most important the new results of the calculated and measured concentration on the selected days agree now much better as before. There are still fluctuations on some parts of the days which comes - as already mentioned in the manuscript - from the measured SO₂ concentrations and there low values (<0.5 ppb) around the detection limit of the instrument. Second, we do not have any information about the vertical structure of sulphuric acid and it would include one more uncertainty in the calculation of the particle number concentration in the mixed layer and so the vertical transport. By assuming any vertical profile for this gas the information from the model for the last part of the manuscript would be worthless. Taking the advantages and disadvantages of measured and calculated sulphuric acid concentrations and the new much better daily pattern of the calculated ones into account I hope that referee # 1 agrees that the value of this paper will be higher with the use of the calculated values.

2.) Concerning Fig. 8: This figure is now different related to the new sulphuric acid concentrations during this day.

3.) Actinic flux: This sentence is a little confusing and I will explain it in more detail in the new version. However, the way it is handled in the model is that we use the global radiation measurements from Hyytiälä to validate the daily maximum of the model with measurements and further we use an average ratio for each wavelength step from the spectral measurements from Hyytiälä to achieve spectral radiation data in the code. In the end we only compare the calculated and measured spectral radiation to insure that the calculated values are in good agreement with the measurements.

4.) Particle above 100 nm from measurements: An idea which would definitely be good if only the particle formation would be of interest. However, in our study we would also like to achieve information about the growth of the particles by sulphuric acid and organic vapours and for this reason it is important to use the whole size distribution

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in the aerosol dynamic. It is right that the horizontal advection of background particle distribution will affect the nucleation and growth processes. However, in this study we tried to compare the principles of the used mechanism and our goal and the goal of a one dimensional model was not to reproduce exact the same daily pattern of aerosols as observed. This is already limited by the fact that although all measured input values are from Hyytiala not all instruments are at the same location, e.g. the spectral radiation data are about 200 m away from the cottage and mast; sulphuric acid was measured about 100 m away.

5.) Appearance of 3 nm particles at midnight in Fig. 3: This is different in the new figure; however on this day we have a strong increase in measured organic vapours in the late evening and up to my opinion most probably also a strong increase in emission of sesquiterpenes which are not measured during this campaign and rarely understood in both - their emission and chemical reactions. In the future we have to include more detailed organic vapours to ensure that the growth by condensation will be better reproduced.

The English will be checked.

Statements to comments from Referee 2:

Concerning the comments on figure 3, 5, 6: These figures will be changed in the next version of the manuscript. The referee is correct that the growth of the particles by organic vapours - already mentioned above - need to be improved. The problem which appears hereby is that specially the concentrations and the emissions of sesquiterpenes are still only to a small extend understood. We know that these organics will contribute at least to the growth of the aerosols (most probably already at a couple of nm) and maybe also to the formation of clusters. However, it will take more experimental and model work to improve our knowledge in this direction.

1.) We used interpolated input values for the monoterpene concentration based on hourly measured data.

2.) If we use measured values as an input from the ground layer and mix this data values by vertical advection we would underestimate the concentration in the mixed layer. In the future or new version of MALTE we use the emission code by Alex Guenther MEGAN and first results show high agreement with measured data. The vertical gradient we used for the monoterpene concentration is based on measurements during the OSOA campaign in Hyytiala and on discussions with Alex Guenther and Jim Greenberg who had many experiences in vertical distribution of organic vapours.

3.) Aerosol yield of 0.55: This was an error by writing the manuscript and the real values are both 0.05, which leads to an aerosol yield for the organics of 0.1 close to the value published by Hoffmann et al. (1977).

4.) Correlation factor K: After improving the model during the last months and also found some major errors concerning the condensation sink we now found the new value for this factor of 1×10^{-12} which is close to the value of 0.6×10^{-12} published by Sihto et al. (ACPD 2006).

In the new model runs and the final version of the manuscript we will also include the comments made by referee # 2 concerning the density and molar mass of the organic vapours.

I will again thank both referees for their contribution and send the new version asap to the editor.

Best regards Michael

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3465, 2006.

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