Atmos. Chem. Phys. Discuss., 1, S166–S169, 2001
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Field measurements of hygroscopic properties and state of mixing of nucleation mode particles" *by* M. Väkevä et al.

Anonymous Referee #1

Received and published: 2 January 2002

This paper summarizes results from field measurements of hygroscopicity of nucleation-mode aerosol particles carried out at four different locations, of which some have been published previously. One of the remarkable findings out of the comparison of the data sets is that freshly nucleated 10-nm particles were hygroscopic at a forest site while 10-nm particles during nucleation events with a clean marine air mass at a coastal site were hydrophobic, which indicates different chemical species and mechanisms were involved in particle formation at the two locations. The paper contains such intriguing contrasts and similarities among the four field studies that should be published. The Helsinki data are very interesting too. However, the authors could take more advantage of integrating the four-site data sets, and in particular the summary could be written better by putting more emphasis on similarities and differences among the data sets. In addition, there are several points that require revision by the authors,

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which are listed below.

specific comments:

1) There is a statement at Line 2, Page 388 that "The observed hygroscopic properties varied with time, but no obvious diurnal cycle was visible" for May 98 10-nm Helsinki data. On the other hand, at Line 6, Page 391, there is a sentence that states "In all the above mentioned sites the diurnal variations were significant and a relatively wide range of growth factors were observed daily" and it seems "all the above" includes the 1998 Helsinki data. These statements are clearly contradictory. The former statement, i.e. there was no apparent diurnal pattern in the Helsinki data, seems to be correct and hence the latter must be modified. In addition, the Luukki data, which are also included in "all the above", do not show a diurnal pattern very clearly. (The data on Feb 19 could be an exception, though.)

2) Line 25, Page 389: About "A few days resembled Helsinki May 1998 measurements", the authors should explain on which days the Luukki data resembled those of Helsinki in May 98. The authors also should address what aspects are similar between the two data sets on those days.

3) Line 26, Page 389: About "During the measurement period, new particle formation events similar to ...", the days when the nucleation events were observed should be given so readers do not have to read Väkevä (2000) to find them. In addition, about "the growth factors and their temporal variations during these events resembled the ones detected in Hyytiälä", the authors should explain more details quantitatively, such as what was observed in Hyytiälä about the growth factor and its temporal variation during nucleation and how they compare to the Luukki data.

4) "3.1.4 Coastal Site" in Page 390: There are no data plots provided for this section. It is necessary for readers to see time-series plots for the growth factor similar to Figs. 1-6, together with information about air mass origins and periods when nucleation bursts were observed, in order to understand the relationship between particle formation and

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hygroscopicity, the effect of polluted air mass on the growth factor, and the comparison with the forest site data which are discussed in the text.

5) Line 18, Page 392, " During the nucleation event days...": the meaning of "the less hygroscopic mode is pronounced as a function of size (Table 3)" is not clear. Probably my question is the use of "the less hygroscopic mode". To my understanding, there was not significant external mixing taking place during spring-time nucleation days for 10-nm particles, and thus "the less hygroscopic mode" is not used to denote one of externally mixed modes. Then, what is this "mode"? This sentence needs to be modified for better clarification, and requires additional explanation about how to read Table 3 in order to understand "... the condensation of less hygroscopic organic compounds during the formation and growth events" in the following sentence.

6) Line 6, Page 393, "Based on the observations...": There is no discussion prior to this sentence about the four hygroscopicity classes and their connection to the data presented in the paper. I do not see how the growth factor ranges were determined. The authors should describe this process. In addition, in Abstract the authors claim "The data can be classified in four hygroscopic growth classes: ...". This sentence should be dropped if the classification is not attempted with the presented data in the text.

7) It is not clearly explained what condition was used as criteria to determine the presence of external mixing of aerosols. For example, I would imagine that there were cases in which the concentration distribution after TDMA data inversion did not have multiple peaks, but was apparently fatter and indicated co-presence of particles of slightly different hygroscopicity. Whether counting this as an observation of an externally mixed aerosol or not may differ from people to people, and this may induce ambiguity when the results in this paper are compared with those by other people in the future. The authors should describe quantitatively what criteria were used in their analysis to sort out observations of external mixing from no external mixing. Such information will set guidelines for future TDMA data analyses.

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technical corrections:

8) Figure 6 caption: "February 1999" should be "February 1997"

9) Line 17, Page 393: "... slightly larger that unity ..."; "that" should be replaced with "than"

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