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Interactive comment on "Physical aerosol properties and their relation to air mass origin at Monte Cimone (Italy) during the first MINATROC campaign" *by* R. Van Dingenen et al.

R. Van Dingenen et al.

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First of all we would like to thank both referees for their constructive comments to the manuscript. We have listed below our response, and have decided to integrate most of them into a revised version of the manuscript.

Comments are addressed in the order they have been listed by the referee.

We have specified in the text that the particle size indeed refers to 'diameter'.

We did actually not intend to use the good agreement of the overlapping sizes in DMA and OPC as a verification of the OPC humidity. We actually expected a shift because

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particles in the OPC were not dried. We suggested the dissipated heat inside the instrument as a possible explanation for the good agreement. However, as the referee suggested, a perfect overlap is not to be expected for instruments based on different measuring techniques, hence we decided to leave out the statement on dissipated heat.

We have now specified that 300˚C is the setting of the furnace, leading to 270˚C in the air stream. We have consequently always used "270˚C" in the remainder of the text, as this is the actual temperature to which the aerosol is exposed.

The laboratory tests performed cover indeed the aerosol loadings observed during the experiments (up till 12μm^3/cm^3 total aerosol volume, with a sulfate-to-NaCl ratio of 9:1; now mentioned in the text).

We now mention in the text that there is effectively no size dependency in the difference between ambient and volatility DMA (hence the similar values for number and volume). The reason is not clear, but the fact that the losses are independent of particle size suggests that the sheath-to-excess air flow ratio may be different than calculated from the presumed mechanical properties of the instrument.

The hygroscopicity of the refractory aerosol was only tested on a few occasions, and as knowledge of the air masses was not available at the time of the experiment, this does indeed not cover all sectors. However, as we have added in the text, referring to the accompanying paper by Putaud et al. 2004, the chemical measurements show a negligible Na^+ content throughout the experiment (< 1% sea-salt), except during the dust event, when, assuming that Na originates solely from sea-salt, an upper limit of 1.6% was found.

The RH could not be held precisely at 90% during the campaign, but all growth factors were recalculated to that value, as extensively explained in the text. We have changed in the abstract 'at 90%' to 'near 90%'.

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Kinetic effects are indeed probably the cause: passing the inlet sample over a water surface at room temperature is sufficient to shift the GF to the correct value. As we have not further investigated the effect, we prefer not speculate about the cause.

We have defined the different 'air mass classes' as 'sectors'. The class 'Arctic' has been renamed 'N-EUR'.

Regarding the discussion on in-situ nucleation, the point we wanted to make is that new particle formation is clearly happening in the air mass reaching the site, and that this is likely to be a consequence of mixing between two air masses (boundary layer – free troposphere). We agree with the referee that the nucleation process may well have started upwind the measurement station, but there are several arguments that nucleation did not happen already inside the boundary layer and is just exported along with other BL tracers:
- Due to the large scavenging surface area of the accumulation mode aerosol, in-situ nucleation is quite uncommon in the N-Italian hazy polluted boundary layer during summer months, as supported by observations at our home institute (Rodriguez et al, 2005, manuscript submitted to Atm. Env.) In fact during summer months of 2000, only 2 days of nucleation per month have been observed in the boundary layer, and only under specific meteorological conditions leading to a very clean atmosphere.
- The volatility of the small particles shows that they are not primary emitted (soot) particles
- their (maximum) number concentration is not correlated to the black carbon mass, unlike the number concentration in the accumulation mode
- mixing of air masses (in this case: polluted boundary layer air with clean free tropospheric air) favours nucleation (Easter and Peters, 1994; Nilsson and Kulmala, 1998)

The question why the refractory fraction is so high in the N-EUR sector can not be answered straight away; the air mass may be influenced by uplifted biomass burning plumes in Northern Europe or Canada, but we have not found direct evidence for this.

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The value of 62ng m^{-3} was wrong and has been corrected to correspond with the value in the table. The uncertainty is the 95% confidence level on the mean. We do not claim that this value is so high because of aircraft; the value of 2 ng/m^3 by Pueschel is given as a comparison. We have clarified this better in the text.

References:

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