

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.

Anonymous Referee #2

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General comments:

This manuscript aims to experimentally characterize key physical properties of aerosols for different air masses arriving at Monte Cimone (Italy), and thus offers another dimension to the size-segregated aerosol chemical characterization performed in parallel and reported by Putaud et al. (2004). This paper is well constructed, field strategy (instrumental set-up) is well sounded, and the results presented in this paper merit undoubtedly to be published.

Although the sampling site is ideally located in terms of latitude/longitude to investigate

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various air masses origin, it is not in terms of altitude; only night-time measurements (00:00–07:00) are valuable for the purposes of the manuscript, reducing by more than a factor of 3 the amount of data collected in the field. What a pity! (Especially for chemical characterization)

Also, it seems to the reviewer that the conclusions drawn in the present paper are unfortunately weakened by vague and qualitative descriptions of the physical properties of aerosols. For instance, in the abstract: + “WSOM is slightly-to-moderately hygroscopic...” + “the occurrence of “less” hygroscopic particles has mostly such a low occurrence rate ...” + “The relative contributions of the fractions [...] show a similar composition within the uncertainty of the data...” With such statements, how the author will manage to convince us that their instrumental set-up is absolutely necessary and complementary to more classical chemical characterization of aerosols?

Unfortunately, the authors do not provide in their manuscript a brief literature overview on previous works dealing with volatility / hygroscopicity DMA measurements. Such adding will be very appreciated by the readers who are specialist in this field.

It remains that the results coming out from this manuscript are particularly relevant and that the instrumental set-up proposed here will offer a unique opportunity to link, in a quantitative way, (“fast”) chemical composition with some key physical properties of aerosols. Such comparisons could be done maybe here with WAD-SJAC measurements for the hygroscopic behaviour? In order to demonstrate us such potential, will we get any chance to see a short temporal variation of complementary chemical and physical properties records during the campaign?

Specific comments:

+ Among the main results of this paper, the comparison of hygroscopic behaviours obtained from chemical analysis and TDMA records is of particular interest, showing that WSOM is “slightly-to-moderately” hygroscopic belongs to the authors. Later in the manuscript (page 1090, line 27), the authors conclude that aerosols can be classified in

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4 non-overlapping fractions and tentatively assign OM as “non-soluble+volatile”. Why? Does it mean that in a first approximation OM (including WSOM) can be considered as non-soluble? Would “non-hygroscopic+volatile” fraction be more suitable to describe OM? Also, I would like to go one step farther than the authors and assume that WSOM can be considered, in a first approximation, as non hygroscopic for ambient RH conditions. Indeed, hygroscopic properties definition critically depends on the RH you are working with, and a compound that is “slightly-to-moderately” hygroscopic at 87% RH could be considered as non hygroscopic for lower RH. Such conclusion has been already reached by Malm et al. (J. Geophys. Res., 2003) who took an optical growth factor of 1 for OM to fully reconstruct ambient RH scattering coefficient from chemical analysis.

+ In the experimental section (page 1072, line 15), the “refractory” aerosol refers to non-volatile OC, EC, sea salt and dust. Later in the text (paragraph 4.2.3, page 1086, line 14), refractory core consists here only in soot and dust. Why?

+ Figure 11a. Any chance to calculate/estimate the BC number fraction?

+ Many comparisons are drawn in the text without an idea of the amount of data that were compared (see for instance page 1072, line 4 and 23; page 1076, line 10; Ę)

+ Many expressions/symbols are used in the manuscript. To make the discussion of this paper even more clear/accessible to the readers, a strong care should be taken to use the same expressions/symbols through the whole manuscript. It is not the case. Hence, some of these expressions are not or poorly introduced in the text:

- The authors should choose between the use of “submicrometer” or “sub- μm ” or “sub-micron” in their manuscript

- Correlation coefficient is sometimes written “R2” (page 1076, line 10) sometimes “R” (Figure 7). Any reason? If yes, specify.

- Abstract, page 1068, line 20: WSOM is not properly introduced in the text

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- “NWSOM” should be used instead of “non-soluble OM” (page 1087, line 21)
- Page 1073, line 21: $\sigma(g, \min)$ is not introduced in the text
- Line 18, page 1068: “no closure could be made”. Be more specific; closure of what?
- In the term “ambient and refractory size distributions”, used in the manuscript, it should be mentioned at least once that “ambient” does not refer to ambient conditions but to “ambient temperature (is it true?) and RH below 15°C”
- The use of “EBC” should be consistent through the whole manuscript and figures (this term is sometimes replaced by “BC”, “Equivalent BC”, “black carbon”, “absorption coefficient”...)
- Please specify “LH” and “MH” in Figure 3
- Please specify “DMA priority” in Figure 6
- Please specify the meaning of “Ambient, dry” in Figure 7
- + Expressions such as “e.g.”; “for what concerns”; “of which”, “from which”; “from where”; “in what follows”, are widely used in the beginning of the manuscript. Anything better?
- + Table 2: Any chance to get in a last column, the relative contribution of each air masses origin?
- + Page 1072, line 19: “...re-nucleation”. Why re-condensation could not also occur?
- + Page 1072, line 23: The values of 78(+/-5)% and 79(+/-6)% are obtained from the comparisons of integrated aerosol number and volume? If yes, please specify the term “integrated”
- + Page 1087, line 20: “...e.g. ammonium salts, H₂SO₄ ...”. Even partially neutralized by ammonium (Putaud et al., 2004), there is little chance to have H₂SO₄ in the particulate phase. Maybe this should be replaced by ammonium sulphate salts?

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+ Page 1083, line 21-22 : “Hence, we conclude that a dust-rich ...”. Could such statement be validated with chemical analysis performed by Putaud et al. (2004)?

+ Page 1086, lines 5-6: “Only for the dust episode ...”. Such statement is based on chemical analysis results published by Putaud et al. (2004). In this paper, it is not specified if ultra-pure water extractions for IC analysis were performed by sonication. Sonication is normally such an efficient process that an important fraction of dust particles, which is normally insoluble, can be solubilized in the ultra-pure water and analyzed by IC (eg the case for Ca²⁺ in dust, which is normally not fully water soluble). As a matter of fact, sonication could artificially increase the amount of ions analyzed by IC, and thus could maybe explain why measured hygroscopicity is lower than expected from the chemical composition.

Typing errors: + Page 1071, line 13: “instead” instead of “in stead” + Page 1079, line 10: “Arctic” instead of “Arctid” + Page 1080, line 24: “air masses” instead of “airmasses” + Page 1081, line 21: Please add a comma between “mode 2” and “DgN=88nm” + Figure 7: “300°C” instead of “300°” + Figure 13: “based on measured physical properties” instead of “based on physical properties measured”

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