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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Overview of aerosol properties associated with air masses sampled by the ATR-42 during the EUCAARI campaign (2008)" *by* S. Crumeyrolle et al.

Anonymous Referee #1

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Review of the ACP manuscript acpd-12-9451-2012 "Overview of aerosol properties associated with air masses sampled by the ATR-42 during the EUCAARI campaign (2008)" by S. Crumeyrolle et al.

General Comments:

The manuscript "Overview of aerosol properties associated with air masses sampled by the ATR-42 during the EUCAARI campaign (2008)" by S. Crumeyrolle et al. deals with aerosol measurements carried out with the ATR-42 aircraft platform during the EUCAARI intensive observation period. As aircraft-borne aerosol measurements are scarce such measurements and the presentation of respective data are always help-

ful. However, in the current case I miss some important points in order to make it a valuable scientific publication, e.g., the reference to previous/similar studies, a more detailed description of flight details and meteorological conditions, some minimum statistical requirements concerning the data, or some "substantial conclusions". For me the scientific value of the manuscript in its current version is not clear, what is new, what do we learn? Moreover, there are many "typos"; the manuscript seems not to be proof read by the co-authors. These typos made me tired while reading. In summary, I recommend to accept the manuscript only under major revisions. Below I try to give a few hints what should be improved.

Specific Comments:

- p. 9452 l. 16, The nucleation mode is sometimes defined as all particles with particle diameters of 1-10 nm, sometimes even up to 20 nm, but I never heard of nucleation mode particles with 25-28 nm particle diameter, please correct this statement.

- p. 9453 l. 9, the indirect effect is not only the modification of the "cloud distribution", it is also the modification of cloud properties.

- p. 9453 l. 14, I do not fully agree that the aerosol concentrations over "Europe are still not well quantified". There are many publications on this topic and the authors of the present manuscript should refer to these studies, here in the introduction, but also in the results section. This is a major criticism! Here are, far from being complete, a few potential references missing in the manuscript:

Coen, M. C. et al., Aerosol climatology and planetary boundary influence at the Jungfraujoch analyzed by synoptic weather types, Atmos. Chem. Phys. 11, 5931-5944, DOI: 10.5194/acp-11-5931-2011, 2011.

Birmili, W. et al., Atmospheric particle number size distribution in central Europe: Statistical relations to air masses and meteorology, J. Geophys. Res., 106(D23), 32,005–32,018, doi:10.1029/2000JD000220, 2001.

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Tunved, P., E. D. Nilsson, H.-C. Hansson, J. Ström, M. Kulmala, P. Aalto, and Y. Viisanen, Aerosol characteristics of air masses in northern Europe: Influences of location, transport, sinks, and sources, J. Geophys. Res., 110, D07201, doi:10.1029/2004JD005085, 2005.

Petzold, A., Fiebig, M., Flentje, H., Keil, A., Leiterer, U., Schröder, F., Stifter, A., Wendisch, M., and Wendling, P., Vertical variability of aerosol properties observed at a continental site during the Lindenberg Aerosol Characterization Experiment (LACE98), J. Geophys. Res. 107(21), 8128, doi: 10.1029/2001JD001043, 2002.

- p. 9454 l. 15, the aerosol-cloud-climate scale does not stop in the “millimeter” size range, please clarify what you mean or correct the range

- p. 9454 l. 19, the description of the measurement flights (and also of the measurement instrumentation below) should be detailed enough to understand the results of this manuscript without having to look into several other publications. It is not sufficient to provide abstract flight numbers. Moreover it is not clear at which altitude it was flown. Hence, please provide a description of both in THIS manuscript. This is, again, a major criticism.

- p. 9455 l. 2, “isokinetic” includes isoaxial, because it means that one has the same velocity vectors (magnitude AND direction) in the free stream and the inlet tip. So, please remove the bracket term.

- p. 9455 l. 8, depending on the, for the reader unknown, flight altitude the dp50 of CPCs shifts with operating pressure up to several nanometers. Was this the case for your flights?

- p. 9455 l. 17, please provide a reference for the thermo-desorption column or described it more in detail, in particular the particle residence time in the column. Is it large enough that the volatile mass fraction had enough time to evaporate?

- p. 9456 l. 1, a residence time of 612 s or more than 10 min would correspond to

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a spatial resolution of about 61 km. How representative is this? By the way, 10 min seems to me unrealistic long, is this correct?

- p. 9456 l. 24, is ”Squirrel” commercial software? Please provide a reference.

- p. 9457 l. 14, please specify what is meant with “small segments”. Were the retro-plumes only initialized when the aircraft changed altitude?

- p. 9457 l. 25, the choice of the geographical sectors seems to me arbitrary. Please provide arguments why you did it in the way you did. And again, you are not the first on doing this kind of analysis, what does the literature tell which number and size of wind sectors makes sense? After explaining the choice of the sectors more in detail, please explain also more in detail how the air masses were attributed to the sectors (mathematical formalism?).

- Tab 1., the potential temperatures provided here are totally wrong, -159°C down to -216°C !!!

- p. 9458 l. 16, please provide two weather map figures displaying the differences in the meteorological conditions more clearly. You could and should use these maps also to show the major air mass pathways you distinguish. How representative are the two weather conditions (and hence the aerosol data) for the spring season over Europe?

- p. 9458 l. 26 the free troposphere reaches from the top of the boundary layer up to the tropopause. Were your measurements performed in this whole altitude range? Or are your data rather representative for the lower free troposphere? If yes please make this clear in the text and in the figures as well.

- p. 9459 l. 5, again, the given potential temperature cannot be correct.

- p. 9459 l. 11, where does this statement come from? You did not provide any proofs that the air mass was loaded with particles in these regions.

- p. 9460 l. 2, it is hard to follow the discussion, because there are so many param-

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eter names. You would help the reader if you would indicate the weather situation (LP/HP) and measurement altitude (BL/FT) directly in the graphs of Fig. 2 (also Fig 3, ...). Moreover, if you use LP as synonym for the cyclonic conditions, please be so consequent to use it everywhere, also in the figure captions, e.g., Fig. 2.

- p. 9460 l. 6, the E-EUR sector is relatively small, hence the variability of the aerosol in this sector should be "small", compared to other sectors. But it isn't and this is probably due to the statistics. Your measurements cover more and different meteorological situations, hence there is a larger variability in both the meteorological parameters and the aerosol. And consequently, how representative are the measurements for the NE-EUR sector with only 4% of measurement time in the FT under HP? You should set and name statistical requirements, e.g., at least ten flight hours during three different days, in order to get at least a minimum statistical significance. Otherwise the comparison of different sectors makes little sense. By the way, did the sectors change their size from flight to flight? This is at least what you suggest when talking about the "variabilities ... of the sector the air mass is originating from".

- p. 9460 l. 27, I do not see "generally trimodel" size distributions in Fig. 3 d

- p. 9462 l. 17, I might have missed it, but why is there no bar for the measurements with polar origin and cyclonic weather conditions (as there are data e.g. in Fig. 2)?

- p. 9463 l. 12, again, how do your measurements compare to previous aircraft studies, e.g., Morgan et al., ACP, 2009 or Pratt and Prather, JGR, 2010?

- p. 9465 l. 7, 50 nm as activation diameter for the rather clean FT might be OK, but in the BL, where are many more and larger particles this value seems to be low, 100 nm seems to me more appropriate. Would 100 nm as minimum activation diameter change your results?

- p. 9465 l. 15, I wonder about the rather constant CCN/N50 ratio in the BL. The particle size distributions (Fig. 3) are different, hence I would assume that the CCN/N50 ratios

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also differ. And it is unlikely that the effect of the size distribution is balanced by the chemical composition plus dynamics to result in the same ratio for so many cases.

- p. 9468 l. 1, sorry, I didn't get the message of this paragraph about the Angström exponent and the asymmetry parameter, I'm too tired . . .

- p. 9468 l. 21, the "Conclusions" are only descriptive, what and which values were measured. What is missing is if there is something new in the data, something unusual, a new observation, or a new conclusion. Please invest some more time into the data analysis.

Technical Comments:

- p. 9453 l. 6, should be "long-range"

- p. 9453 l. 11, add "and " before "mixing"

- p. 9453 l. 25, the last sentence is not correct (the verbs), please rephrase

- p. 9454 l. 1, "measurements of" is missing

- p. 9454 l. 19, please use "were grouped" instead of "have been grouped". More generally, please use past tense for actions which are finished in the whole manuscript.

- p. 9454 l. 24, the usual way of setting the brackets when citing references is used wrongly here and in several other places too, please check the whole manuscript.

- p. 9454 l. 22, should be "of" instead of "over"

- p. 9456 l. 25, please replace "currently" by a date

- p. 9457 l. 2, the PCASP should be part of the instrument list in the preceding sentence and not a sentence fragment

- p. 9457 l. 14, insert space between "altitude" and "-"

- p. 9459 l. 11, "indicate" without "s"

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- p. 9459 l. 19, please exchange “difference” with “variability”

- p. 9460 l. 20, please replace “numerous ultra-fine particles” with “by the growth of nucleation mode particles”, because you suggest new particle formation in clean air masses to be the reason

- p. 9461 l. 7, how do the measured size distributions and fit parameter compare to the ones presented by e.g., Petzold et al., 2002? What is similar, what is different and if so, why?

- p. 9462 l. 2, you (hopefully) did not “heat” the “SMPS”, please correct

- p. 9463 l. 2, sentence incomplete

- p. 9463 l. 5, delete sentence fragment

- p. 9463 l. 12, sentence incomplete

- p. 9463 l. 17, city name missing

- p. 9465 l. 9, delete “The evolution of”

- p. 9465 l. 21, sentence incomplete

- p. 9467 l. 10, “dependant” should be “dependent”

- p. 9468 l. 3, “Coming soon ref from JC Roger” ???

- p. 9468 l. 10, two times the same sentence ???

- p. 9480 Tab 2., the “0” in the N500 particle concentration in the free troposphere should be 0.1 or 0.01 or whatever, but there are at least some particles. And please use a space between the particle diameter and the respective unit, e.g., “10 nm”.

- p. 9481 Tab 3., but also other tables: please provide the numbers only with significant digits, e.g., “22.7” should be “23”, your fits are probably not better.

- p. 9481 Tab 3., please indicate more clearly, which parameters are for BL and which

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are for FT measurements, in the current layout of the table it is not clear.

- Fig. 2, 5, and 7: the provision of 0% and 100% percentiles might be misleading because how sure can you be that there are no outliers in your data? Wouldn't it be better to provide the 5% or 10% (95% or 90%) percentiles?

- Fig. 3: the numbers at both axes are hard too small and hard to read, please enlarge. Same for Fig. 4, . . .

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