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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of cloud processes on aerosol particle properties: results from two ATR-42 flights in an extended stratocumulus cloud layer during the EUCAARI campaign (2008)" *by* S. Crumeyrolle et al.

Anonymous Referee #1

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General comments:

This paper has potential and in some form will probably finds its way into ACP. But it still needs a fair amount of work to make it acceptable. My main reservations are a) incomplete and sometimes faulty interpretation of the material at hand; b) organisational issues, and c) stylistic difficulties.

I have a number of suggestions for improvement. The authors should particularly address topics 1,2,3 and very much topic 6. The others are important as well in various

degrees, but wouldn't hold up publication as much as these.

Specific comments

1) There are only three lines in the Introduction dedicated to the issues that the authors precisely want to address in this paper [lines 1-3 on page 33234]. The rest of the material there is perfunctory, as if the authors had trouble filling the introduction with material. This makes for difficult reading. They need to be specific what they want to do right up front. 'Analysis' is not enough. Also, a bit more reference to the EUCAARI experiment would be useful as well.

2) I do not believe that 'cloud processes' is the main topic of this paper, even though it is mentioned in the title. It is even admitted that some of there interpretation of the cloud chemistry is speculative [pg 3326, line 18]. Why not de-emphasize the cloud-thing and make another title reflecting more completely the work that was performed: Something like: 'Airborne investigation of the chemical composition of aerosols in the marine atmosphere over the North Sea during EUCAARI (2008)'.

3) The discussion on pg 33238 on conservative variables is faulty. The variable θe is not a measure of stability. Stability is determined by the virtual potential temperature: θv = θ (1 + 0.609qv – ql), where θ is the potential temperature, qv is the water vapor specific humidity and ql is the liquid water specific humidity. To a good approximation the definition of equivalent potential temperature is θe = θ + (L/cp) qv. This definition clearly shows that for evaluating differences in air masses a variation in θe is not enough evidence, because there are two variables involved and both can change independent of each other. For example two air masses can have the same equivalent potential temperature, but different potential temperatures and specific humidities. So, in order to discern differences between air masses you need specific humidity as a variable as well. There is a large amount of literature on the subject, starting with Rossby in the '30's of the last century, and ending with Alan Betts in the '80s and '90s. Particularly Betts' papers should be consulted, on saturation level conservative quan-

tities θe, θl [= θ(1 – (L/cp)ql, the liquid water potential temperature], and qT [=qv + ql, the total water specific humidity]. The plots of these variables are missing in this paper, and I strongly suggest that the authors read up on the matter of conservative variables and include some plots of θe, qT, and possibly θl. so that we can see the evidence of their claim of different air masses.

4) Pg 33242. The use of ' mass concentration ' is confusing to me. Its either mass, of weight perhaps. The word 'concentration' usually refers to the number concentration of particles.

5) Pg 33243. The section 3.2.2 is within the context of this paper pretty much without meaning and I suggest that the authors leave it out altogether.

6) In the section about chemical composition, I like the various plots, as there are pretty useful and interesting. However, I miss a thorough effort to interpret these results in terms of air masses, something the authors started out with but somehow forgot to persist with. The North Sea is a region in between several industrialized nations. Furthermore, sea vessels have quite a large influence on the chemistry of the aerosols present there. We know that they emit large quantities of sulphur containing exhaust. Given the wind direction, there is also the good possibility that large quantities of aerosols were encountered that originate from the oil refineries just west of Rotterdam. This is arguably one of the largest sources of NOx in Europe. Satellite plots of shipping routes reveal that the North Sea sticks out over all of Europe in this regard! I suggest that there must be large gradients in aerosols present in the data as the aircraft traversed the region from extremely polluted to 'relatively clean' near Newcastle. Also missing is discussion on the transition from a continental boundary layer to a marine boundary layer. Under the conditions of the flight, it is quite possible that the air which was encountered just above the marine boundary layer [say 1000 – 1200 high] originated from the Netherlands, or Denmark. So the authors leave the information present in the FLEXPART data largely unexplored. There must be changes in wind direction between the PBL and the free troposphere that they can look at a bit more. There are clear

horizontal gradient in the amount of organics from NW to SE, and they must amount to something. Also have a look at the ACPD paper by Amewu and other from the Juelich that is currently under discussion. Although it deals with ground based observations at Cabauw, the period is the same, and possibly the compositions are similar. I really believe that they should rework this section more thoroughly addressing all these issues.

Technical remarks

7) The paper is awash with acronyms. I think I understand most of them but the list is so large that it would be useful to summarize at the end.

8) The use of V, V1, V2, C1 etc is so casually introduced that I had to read for quite a bit in the paper before I could understand what they were on about. Be much more specific, and upfront about these abbreviations. They start in the Abstract [pg 3321, line 13. [There should be no place for a V in the Abstract at all.!]

9) I do not understand the gray scale on Figure 1. According to the grey scale plot , the brightness temperatures of the Sc deck are around 120 K, or -150C. This seems more than a little low for a Sc deck in the summer.

10) I miss an explanation of PCASP, pg 33236, line 29.

11) cTOFs and TOF's : whats the difference [pg 33236, line 15]

12) I suggest you read the lines 22 – 25 on pg 33237 a couple of times very carefully. These lines convey exactly nothing: 'An air mass approached the sector of measurement during the morning'. Well, yes, but that is what air masses invariably tend to do namely approaching air sectors. What exactly would you like the reader to know here?

13) Pg 33240, line 9: Principalis??

14) Pg 33240, line 18: scanvenging??

15) Pg 33242: I do not understand the word ' tendencies' in this context. Do you mean

gradients??

16) Figure 3: The captions mention ‘ numbering’. I don’t see any numbering on the plot, so do not understand what the authors mean.

17) Figure 4: Show different wind roses for the Sc covered PBL and the FT. Surely, they must be different. If not, at least mention it somewhere.

18) Figure 5: This figure is very hard to read, the colours are very similar, and the broken versus solid line are just about indiscriminate. The authors should redo this figure. And in English it is ‘diameter’, not ‘diametre’ There seems to be a nucleating region in the Sc covered marine PBl in the afternoon, you mention it in the text, but can you make anything of it?

19) There is higher nitrate above the PBL. Is this continental influence?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 33229, 2011.