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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.

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We thank both reviewers for their detailed and constructive comments on our manuscript. We have revised the manuscript attempting to take into account all the comments raised by both reviewers. We apologize for the delay due to the time required to perform the requested additional analysis.

General Comments of Reviewer 1 :

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. The introduction has been modified accordingly to the reviewer comment. Some part of the introduction has been removed while we added more references related to the EUCAARI project and to the goal of the study by itself.

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 their interpretation of the cloud chemistry is speculative [pg 3326, line 18]. Why not de-emphasize the cloudthing 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)'. The main purpose of this paper is to emphasize the impact of the cloud onto the aerosol properties. To achieve that, we showed aerosol properties in the vicinity of the cloud, above the cloud, below the cloud as well as inside the cloud. The comparison of these measurements allowed us to interpret the effect of the cloud processing on the particles. Thus the authors change the title into 'Airborne investigation of the aerosols-cloud interactions in the vicinity and within a marine stratocumulus over the North Sea during EUCAARI (2008).'

3) The discussion on pg 33238 on conservative variables is faulty. The variable _e is not a measure of stability. Stability is determined by the virtual potential temperature: $\theta$v = $\theta$ (1 + 0.609qv – ql), where _ 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 $\theta$e = $\theta$ + (L/cp) qv. This definition clearly shows that for evaluating differences in air masses a variation in $\theta$e 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 equiva-



ACPD
11, C16596–C16602, 2012

lent 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 quantities $\theta$e, $\theta$l [=$\theta$ (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 $\theta$e, qT, and possibly $\theta$l. so that we can see the evidence of their claim of different air masses. The reviewer is right and indeed two different air masses can theorically have the same equivalent potential temperatures ($\theta$e). However, it has been shown that $\theta$e can be a good parameter used to characterize and distinguish air masses with different origin (Colette et al., 2005; Sturman and McGowan, 1995). The reviewer is right again in pointing out that the use of more conservative parameters like total water content together with liquid water potential temperature is far more accurate. However, $\theta$e could be sufficient in distinguishing qualitatively different air mass origin and history in the present study. The discussion has been revised accordingly. We also added $\theta$v values together with $\theta$e values previously shown in the table 1 and the tables 2. In the boundary layer, the averaged $\theta$v is about 284.2 +/- 0.9 K and 283.9 +/-0.4 K in the morning and in the afternoon respectively. While in the free troposphere, the averaged $\theta$v is about 292.0 +/- 1.2 K and 291.7 +/-1.1 K in the morning and in the afternoon respectively. Thus, the results show a strong stability in each layer and a strong difference between both layers.

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. The word concentration is commonly used to describe the number concentration. This is why we precised when we are not using a number but a mass concentration. The mass concentration is used to refer to the quantity 'particle mass within a volume of air'.



ACPD
11, C16596–C16602, 2012





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. The authors claim that this part is a really important part of the study. Indeed, this is the first observations of the chemical composition (AMS) of cloud residual particles (CVI). This part allowed us to estimate the cloud activation efficiency and the in cloud production of the different compounds. As this is a first time observations, it's also of great interest for the modeling community.

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 referee is right in saying that the North Sea is a region surrounded by numerous industrialized nations and that the aerosol sources are manifold (cities, oil refineries, sea vessels, etc...). As shown by the synoptical conditions (Hamburger et al., 2010), an anticyclonic blocking event dominated the weather over the North Sea. Furthermore, the air mass trajectories calculated by FLEXPART as well as the wind observations show that the air masses are coming from the North East, except for one segment during which the wind direction observed was inverted. This segment was not used for this study because of the potential influence of anthropogenic emissions. In the answer to reviewer 2, time series of the AMS mass concentrations are shown. The first 20 minutes of the morning flight and the last 20 minutes of the afternoon flight are representative of the pollution observed over the continent. One can see that the nitrate and organic mass concentrations reach rather high values of 15$\mu$gm-3 over the Rotterdam area while these concentrations decrease dramatically to values lower than 1-5$\mu$gm-3 (respectively for nitrate and organics) over the North Sea. Thus the measurements performed in air masses exposed to strong pollution are strongly different from those made over the North Sea and we can thus assume that the air masses sampled over the North Sea were not impacted by the pollution coming from Rotterdam and surrounding areas. Part of this discussion has been added to the manuscript.



ACPD

11, C16596–C16602, 2012

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. A list of acronyms has been inserted.

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.!] The V in the abstract has been removed and we took care to use more frequently the terms V, C and I.

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. The authors would like to apologize for this mistake. Indeed, the cloud top temperature data are delivered in the MODIS file with an offset. The offset had not been added in the previous version but this has been corrected in the current version.

10) I miss an explanation of PCASP, pg 33236, line 29. The PCASP is an acronym for Passive Cavity Aerosol Spectrometer Probe. This instrument sizes particle in the range 100 <Dp < 3000 nm on-board of the ATR-42.

11) cTOFs and TOF's : whats the difference [pg 33236, line 15] The cTof stands for "compact Time of Flight" and is a type specification of the manufacturer Aerodyne Inc.. The expression is used to correctly indicate which type of AMS was used. In order to stay consistent throughout the manuscript we added a 'c' everytime.

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? The sentence has been replaced by :'The model revealed that air masses coming from the North West were sampled with the ATR-42 on 15 May 2008 during the morning



ACPD

11, C16596–C16602, 2012

(RF51) and also the afternoon flight (RF52).'

13) Pg 33240, line 9: Principalis?? Principalis has been replaced by principally

14) Pg 33240, line 18: scanvenging?? scanvenging has been replaced by scavenging

15) Pg 33242: I do not understand the word ' tendencies' in this context. Do you mean gradients?? The word 'tendencies' has been replaced by 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. The numbers are located in the middle of the plume (pink/purple color) of each picture but may be easily overseen.

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. Each wind rose represents the wind speed and direction measured during each segment. The authors didn't want to make any distinction between the different layers as the wind parameters are similar. As the reviewer suggests, we added in the figure caption and in the text that the wind roses correspond to measurements in both layers.

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? The colors and labels of the figure 5 have been modified as requested by the reviewer. The nucleation mode which is observed above the cloud in the morning and below the cloud in the afternoon are interesting, but unfortunately we do not have enough measurements in the gas phase and also in the lower diameter range to get something interesting out of it. It may be due to the depletion of the clouds and/or evaporation of the precipitation but a detailed model study is necessary to analyze these cases.

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



ACPD

11, C16596–C16602, 2012



concentration of nitrate (NO3) is similar in both layers, but the concentration of nitrate is larger in the PBL. The nitrate may have different origins like continental, lightning, bacteria, etc. . .In this case, the influence of industrialized areas surrounding the North sea are the main sources of nitrate.

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



ACPD

11, C16596–C16602, 2012

Interactive Comment