

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.

S. Crumeyrolle et al.

suzanne.crumeyrolle@gmail.com

Received and published: 11 May 2012

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 2 :

1) A clear and concise discussion of the conceptual model that the authors are basing

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their analysis on is needed. After some thought, I think I understand what the authors are assuming, but I also do not agree with all aspects of what I believe is their model. The authors have added a cartoon to explain the cloud conceptual model that we have used in this paper (Figure 1). We added few sentences to explain this model :

To help the reader, Figure 1 summarizes the state of the lower troposphere during cloud free and during the presence of a precipitating stratocumulus. During cloud free conditions the boundary layer appears to be a well-mixed layer while the layer below the stratocumulus is decoupled from the surface induced by drizzle evaporation under the cloud base (Feingold et al., 1998). Above the cloud, the entrainment is leading to exchanges between the free troposphere and the cloud layer.

2) More detail in the presentation of the observations is needed as well as a better technical discussion of the limitations of the measurements (e.g. AMS and CVI; see detailed comments). It is important to see the time series of the AMS measurements rather than just a few averages. You need to show how the AMS and SMPS measurements compare across the time series. You suggest that organics increase due to cloud, but your results are neither statistically significant nor properly consider the uncertainties in the measurements. All the details to answer this question have been given in our response to the specific comments relevant to this question.

3) An improved discussion of the processes and impacts is needed. I assume that there were measurements of cloud microphysics made, in which case these need to be included. How do the below-cloud measurements in a particular location compare with the in-cloud measurements? Vertical profiles from below-cloud to above cloud are fundamental to understanding clouds. Cloud microphysical measurements are important if you are to properly discuss the cloud processes and relate them to the measurements downstream of the CVI. Other questions: where is the input aerosol to the cloud; what is the activation point; what is the cycling time of the aerosol through the cloud; how does number change in addition to volume; what are the chemical precursors that may contribute to changes in the size distribution; is collision-coalescence important to

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the change in the distribution? Cloud-top entrainment is thrown out to explain reductions in aerosol above cloud, but none of the observations address this issue and the attributions that I gather seem extreme. Unfortunately, measurements could only be performed along the aircraft trajectory. In addition, aerosol properties have not been measured simultaneously in cloud and non cloud phase. These measurements have been performed subsequently (alternation), thus, relating below and in cloud features that have been measured at considerable distances, horizontally. Furthermore, thermodynamics and dynamics may more or less differ along the horizontal extension of the cloud layer. We agree with the reviewer that open questions remain regarding activation processes and cloud-drop closure. Unfortunately, supersaturation measurements for activation calculations have not been performed on the ATR. Nevertheless the gerber probe was measuring the liquid water concentration simultaneously with the CVI (Figure 2). The LWC measured with the CVI and with the Gerber probe are similar. The CVI measurements are overestimating the LWC by a factor of 0.1 which is in the range of the uncertainty in both measurements. According to this results, the authors believe that the mass concentration measured downstream the CVI are representative to the cloud residuals.

Moreover, the authors agree that the assumption of cloud top entrainment reducing the aerosol loadings above the cloud has not been proved. Indeed we assume that cloud top entrainment is occurring during these flights (assumed from the important variability of thermodynamical parameters as shown in Figure 2) in a 200m layer where the aerosol concentration has been found minimum. As we didn't prove that the entrainment is responsible for that loading decrease but we suspect it, we nuanced then this position in the manuscript.

4) As presented, there is no concept of close vicinity or in the vicinity and hence no relevance. There is no discussion to tie those measurements to the measurements below, in and above the cloud field. The distance scale of the measurements is large (about 500 km) as well as the time scale (2 hours). Your trajectories indicate an increas-

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ing continental influence as you move from NW (Newcastle) to SE (Rotterdam). Your "Sc vicinity" data are collected towards Rotterdam where the continental influence was strongest. This seems to be the main reason for the strong concentration differences between the 'Sc vicinity' and the cloud region. A transition from continental to marine influence will explain the cloud, the differences in boundary layer depth and the higher concentrations in the cloud-free area. More detail is needed to substantiate your assumption. The relevance of the vicinity periods has been discussed in the specific comment 15 based on the time series of the mass concentrations measured by the AMS. The Sc vicinity periods (called now V segments to be consistent through the paper) were measured right before (36km and 62km respectively in the morning and afternoon flight) the ATR-42 has been entering the cloud. Moreover, all these V segments were made over the North Sea at a distance of more than 180km from the Rotterdam area and 125km from the UK coast. In addition, back trajectories confirm that corresponding air masses originated from the northeast. Thus the authors believe that the influence of the Rotterdam area is not 'strong' which is supported by the mass concentration differences observed in the early morning and in the late afternoon (Figures 5 and 6).

See response 6 for Reviewer #1 (aerosol chemistry and back trajectories)

SPECIFIC COMMENTS: 5) Page 33231, lines 14-17 – This sentence did not make sense to me until after I had gone through the paper. But then, what is the importance of this? The authors removed this sentence from the new version of the manuscript

6) Page 33231, lines 22-28 – What do you mean by saying that the nitrate and organics "affect" the sulphate and ammonium compounds? In this case, the authors tried to say that the decrease of relative concentration of nitrate and organics correlate with an increase in the relative concentrations of sulfate and ammonium. This has been clarified in the corrected version of this manuscript.

7) Page 33232, line 21 to page 33233, line 6 – You need to be more specific here

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to help the reader understand your paper. There is a lot known about aerosol-cloud interactions, but you do not convey that. For example: a. It has been long identified that sulphate is produced from the oxidation of S(IV) in clouds. Global models suggest that 50-90% of the atmospheric sulphate is produced in the aqueous-phase. Nitrate is often found in higher concentrations in cloudwater than in the atmospheric aerosol due to the scavenging of HNO₃ or N₂O₅ from the gas phase. Mechanisms for an increase in the organic material (OM) by the aqueous phase have been discussed, but a significant increase in OM in the aqueous-phase relative to the dry particle phase has not been observed in the atmosphere. b. Briefly discuss then how the chemical changes may affect the post-cloud size distribution. c. Then discuss some of the physical scavenging processes, including collision-coalescence. d. Finally, it has been well established that the concentration of cloud-base aerosol particles exerts a significant control on the number concentration of cloud droplets. That process is believed to currently offset climate forcing by greenhouse gases by 10-30% (IPCC, 2007). However, considerable uncertainty remains in how other microphysical and dynamical cloud processes may buffer the net impact (Stevens and Feingold, 2009). e. What interaction(s) depends on the type of cloud?

We thank the reviewer for the above comments. We have added in the introduction section further material on the state of art of the aerosol and cloud interactions to revise and modify the entire introduction in order to help the reading of this manuscript.

8) Page 33234, line 19 – The brightness temperature, from MODIS, is also shown in Figure 1. The sentence is now : ‘The brightness temperature (Figure 1), from MODIS satellite documents, indicates the presence of the stratocumulus field that was studied in the northern part of the domain.’

9) Page 33234, line 27 - I don't understand “Nevertheless” in the context presented. We removed ‘Nevertheless’ in this sentence

10) Page 33235, lines 6-10 – The cut size and the enhancement factor of the CVI

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are critical to your estimation of mass concentrations in the cloud. Mountain-top CVIs do not necessarily have the same cut size as airborne CVIs. Our experience is that the Mtn-top CVI has a much lower cut size, and that the airborne CVI cut size estimated from comparison with measurements is higher than the theoretical value. I see no suitable justification here to assume that your cut size was 5 μ m, unless you present comparisons of the residual number concentrations with the details of the cloud droplet size distributions. But you also need to confirm your CVI enhancement factor. Your enhancement factor should be confirmed with CPC measurements from the CVI compared with CDNC from a cloud droplet probe. Were those measurements made?

The authors have quite some experience in calculating and calibrating CVI cut-sizes. In previous work it is clearly shown that theoretical cut sizes are well calculated when comparing to glass bead calibrations (Schwarzenboeck, 1999; Schwarzenboeck & Heintzenberg 2000). Cut size is not a question of ground based or airborne instrument. It's a question of geometry, design, impaction velocity, counterflow to supply flow ratio. The enhancement factor has been calculated with a theoretical equation taking into account the speed of hydrometeors impinging upon the CVI and return flow speed in the probe. Droplet break up studies and previous droplet to residual particle analysis suggest a pretty good correlation between drop and CVI extracted particle numbers (Schwarzenboeck, 1999).

11) Page 33236, lines 7-22 – The discussion of the PCI could be removed, since you indicate that you have no valid PTOF measurements. More importantly, you talk about the efficiency of the AMS without making the distinction between the transmission efficiency (TE) and the collection efficiency (CE). What you discuss is the TE that is mostly inlet dependent. However, it is also well know that more crystalline particles bounce off the oven before they can evaporate, which is often referred to as the CE (e.g. see paper[s] by Ann Middlebrook at NOAA for a discussion of this issue; pay particular note to the effects of RH). Ammonium sulphate in a relatively dry environment (such as in the CVI sample stream) has one of the lowest values of CE (pure AS in the lab will

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give a CE of about 25%). Since sulphate is the dominate component of your aerosol throughout this case study, it is important to take into consideration this issue. You can not say that the AMS “quantitatively measures mass loadings” without some other corroborating evidence. We fully agree with the reviewer, the CE of our cToF-AMS must have changed when measuring downstream of a CVI due to extensively drying the sampled aerosol. The recent study by Middlebrook et al., 2012 analysed the CE coefficient for aerosol types comparing the AMS measurements with PILS and SP2 measurements for different field (airborne and ground based) campaigns. Unfortunately during the EUCAARI airborne campaign, such comprehensive instrumentation equipment was missing on board the ATR-42. Moreover the particle sizing PToF mode of our ToF-AMS was not properly working due to a not further identified instrumental malfunction. Thus the same comparison than Middlebrook et al. (2012) can't be done to test and validate the CE determination algorithm proposed by these authors. What has been done as the only option to provide an instrumental comparison is to compare cToF-AMS measurements with those of other particle counters such as SMPS and PCASP. In a companion paper (Weigel et al., to be submitted), the cToF-AMS measured aerosol mass concentrations and according mass concentration, determined (density is assumed to be 1.8g/cm³) by measurements with the particle sizers (SMPS and PCASP), are compared. The results are shown in the Weigel paper for one complete EUCAARI flight when all instruments were operated downstream of the conventional aerosol inlet (i.e. not sampling via CVI) under cloud-free conditions, yielding a good agreement of these mass concentrations. However, as the reviewer argues, the drying within a CVI the cToF-AMS CE decreases - for sulfate dominated species down to 0.25. As a consequence, assuming a lower CE, increases the results of measured mass concentration of respective chemical species by a factor of up to 2 (for CE = 0.25) relative to our results. Thus, our results of aerosol mass concentrations, assuming a CE of 0.5, provide minimum values and an uncertainty has to be considered that multiplies these values by a maximum factor of 2. Without further characterization of the CVI performance, precise measurements of the RH within the aerosol line and com-

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prehensive equipment for proper instrumental comparison, however, a distinct CE can truly not be determined.

As per the reviewer's comment, we removed the discussion of the PCI.

12) Page 33237-33238 and Figure 2 – Comments: a. Vertical profiles are critical to understanding clouds and aerosols, and appropriately you conducted many of them (e.g. Figure 2). However, we see no profile data only averages over apparently large times and spaces. Such averages neither allow any resolution of the processes nor do they offer particular value in discussing a net result of the processes because of problems with instrumentation and the lack of a conceptual model. b. Figure 2 is difficult to interpret because of the many colours and some of the explanations. I suggest that you only colour the period when the aircraft was in cloud. c. Your cloud layer was up to about 1km deep, assuming that the “blue” represents cloud (you also indicate about 800 m mean thickness in the text). On the basis of such large cloud depths, the definition of these clouds as SCu does not seem to fit, at least during the morning flt. It looks more like deep stratus or Cu that may have become SCu as the day progressed. A better discussion is needed. The referee is right to say that profiles are of main importance to understand aerosol and cloud interactions. In this paper, the averages have been made over 2-3 minutes periods (see Table 1) which correspond to a maximum of 15km. According to the authors, this presentation is useful to compute a large set of data and to help the reader to see the aerosol property modifications between the different periods. In accordance to a recent study, Brenguier et al. (2011), published within the EUCAARI special issue of ACP, the clouds fields observed during May 15th 2008 have been classified as stratocumulus clouds. A persistent offset has been found on the parameter used to highlight the cloud presence (i.e. the liquid water content measured by the gerber probe) in figure 2 (Burnet personal communication). Thus, the cloud depth presented on the figure 2 was largely overestimated by 500m. The figure 2 in the manuscript has been modified accordingly.

13) Page 33239, Lines 2-9 – Here you state the good things that you are going to as-

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sess, but in the end you do not adequately address these issues. We rephrased the paragraph to : 'The most important measurements for this study were those of particle number size distributions and aerosol chemical composition. Subsequently aerosol particle size spectra and chemical composition were analysed at different locations, with respect to the cloud layer. The V segments were made more than 130km away from the coasts (English and Netherlands), thus away from the immediate continental sources, and will serve as a reference (i.e. non cloud processed) aerosol. To estimate the effect of cloud processing and aerosol activation, aerosol properties will be compared in the sc-vicinity within the boundary layer and inside clouds. The influence of precipitation scavenging of drizzle clouds on aerosol particles as well as cloud processing is assessed by comparing aerosol properties in the sc-vicinity in the sc-vicinity within the boundary layer and below the cloud layer at comparable altitude.'

14) Page 33239, lines 11-13 – Better in the discussion of instrumentation where there is also a reference. We moved this sentence to a more appropriate in the instrument section.

15) Page 33240, lines 18-24 – You are telling the reader things that are already known, but not presenting your data in a manner to say anything significant about these processes. The suggestion that entrainment is responsible for the reduced concentrations above cloud relative to the "vicinity" concentrations provides some indication of your overall conceptual model. That is, I assume you are saying the higher concentration aerosol, represented by the 'vicinity' aerosol was reduced by the cloud as the aerosol and cloud moved from the NE to the SW. To achieve this, the aerosol in the boundary layer was removed by drizzle and the aerosol was fed from above by entrainment down into the BL via the cloud where it continued to be removed. Perhaps you are right, but you would need measurements upwind of your flight track by a hundred kilometres or more to truly say anything about this. For that to have happened you would have needed substantial entrainment at cloud top. Profiles through the cloud will indicate the depth of your transition layer and should help address whether this is reasonable.

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However, the real problem is that it is unclear whether your in-vicinity measurements have any relationship to the cloud. It is highly unlikely that the concentrations a few hundred kilometres upwind over the North Sea were 8-10 ug/m³, but it would help your case if you can somehow show that is reasonable.

To clarify the reading, we (1) show a detailed picture of the conceptual model we are referring to (2) show the vertical profiles of the potential temperature and of the vapour mixing ratio measured on-board the ATR-42 (Figure 3 and Figure 4), and (3) we rephrased the sentences into : 'Figure 3 and Figure 4 shows the vertical profile of the potential temperature and the vapour mixing ratio measured onboard the ATR-42 during the RF51 and RF52 flights. These thermodynamic parameters showed a considerable variability in the layer 200m above the stratiform cloud due to the mixing of moist and cold air from the cloud layer with dry and warm air from the free troposphere.'

16) Page 33242, lines 14-18 – It is not just about what happened between the morning and afternoon flights. It is about what was going on several hundred kilometres upwind of the flight path on both flights. The analysis of the virtual potential temperature (i.e. suggestion of the reviewer 1 to replace the equivalent potential temperature) show the strong relation between air masses observed in the morning and in the afternoon. It also shows the decoupling of the boundary layer with the free troposphere. Thus, we can assume that air masses are the same in the morning and in the afternoon. Nevertheless, if the aerosol natural source strength varies no corresponding signal would be seen on the virtual potential temperature. Thus, we changed the statement to : ' The differences in below-cloud mass concentration decreases (25% and 58% of aerosol mass during the RF51 and RF52, respectively) can then be explained by the duration of precipitation when comparing morning and afternoon flights or a different contribution of aerosol sources.'

17) Page 33242, lines 25-27 – the term "decreasing", which you use a lot here, implies a steady reduction across some interval. What you show is a step change. However, you have AMS measurements every 23 seconds and you could quite easily demon-

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strate whether the aerosol is decreasing or simply drops with a time series from the AMS across each flight path. I would not use the measurements from each 23 seconds as this could be noisy due to mass counting statistics. Each 23 second point is a 10 second average, or about 10-15 cm³, and the sampled mass is further reduced due to oven bounce (CE). Using your SMPS distributions, you need to estimate what minimum sampling average of the AMS data is needed to keep the mass sampling noise below about 10%; you need to know your AMS detection limit for each species. Then show a time series of the sulphate, nitrate and organic mass concentrations for each of the two flights. Probably 2-3 minutes averages will suffice. In this study, the averages are done on a short flight segment which corresponds to a small period (~ 2-3 minutes see tables 1 and 2). Thus, the average the reviewer is recommending is almost the same we have been done in the manuscript. As the referee recommended, we are presenting (just in this document) the times series of the organic, sulphate and nitrate mass concentration as well as the Liquid Water Content (LWC)(Figure 5 and Figure 6). These time series highlight few different periods : 1. Over the continent before 7:00 for the RF51 and after 14:30 for the RF52 : During these periods the mass concentration of nitrate and organics are reaching 15 μ gm⁻³ while the sulfate concentrations are about 5 μ gm⁻³ in the morning and in the afternoon. The air masses sampled during these periods are under the influence of industrialised areas close to Rotterdam and Amsterdam. 2. Over the North Sea and before the cloud presence (7:00-7:24 for RF51 and 14:17-14:26 for RF52): The total concentration reaches 10 μ gm⁻³. The sulfate and the organics are prevalent and are in average about 5 μ gm⁻³. The composition as well as the mass concentrations are strongly different to the one observed over the continent corroborating the air mass trajectory results that the air masses observed over the north sea are not impacted by industrialized aerosol sources. These results have also been discussed into the answer to reviewer 1 and thus have been introduced briefly into the manuscript. 3. Over the North Sea during the cloud presence (above/below/in-cloud). In this case the time series are not a relevant way to present the results. Indeed it's hard to distinguish the different periods especially the periods when the aircraft was

C16613

above and below the cloud. The successive peaks in the sulfate are corresponding to the measurements below the cloud base, while the minimum in between peaks are corresponding to the measurements above cloud.

For purposes of discussion, we show the figures here, but do not include these figures in the manuscript.

18) Page 33243, lines 18-22 – You have to work pretty hard to lose 100-300 nm particles in an inlet line, and I doubt that was the problem; here it would help to have a comparison of CVI-CPC and cloud droplet probe measurements. I suspect two main reasons for your high estimated activation diameters, relative to what you expect based on the minimum in your distribution: 1) in the CVI you dry the particles substantially and, for a sulphate dominated aerosol, oven bounce may significantly reduce the CE of the AMS; 2) your enhancement factor used in the CVI is incorrect (there is no discussion of this in the instrumental section, but there needs to be).

The drying in the CVI is only related to the dry carrier air where droplets evaporate, there is no heating of the sample tube or other parts of the CVI. Evaporation temperature in the CVI thus is not higher than cabin temperature. The continuously calculated enhancement factor is derived according to a one dimensional calculation which is based on the velocity ratio between hydrometeor velocity outside the probe tip and return flow velocity of the carrier gas (dry air). All CVI probes have to deal with that enhancement since the sampling is per definition non-isokinetic (despite the shroud that helps aligning the trajectories, the velocities of air mass outside the CVI and return flow are usually not identical. In order to proof that one dimensional continuous calculation of the enrichment factor is justified, correlation studies of droplet numbers and aerosol particle numbers have been studied in a wind tunnel (Schwarzenboeck 1999).

19) Page 33244, lines 10-18 – A casual reference to entrainment that is another example of the need for a description of your conceptual model. You refer here to sulphate as being produced in cloud and this is recognized as a significant chemical process

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in the atmosphere, but your results do not indicate an increase in sulphate in cloud. Possible reasons need to be discussed.

20) Page 33245, lines 1-3 – Here again, the absence of a properly explained conceptual model presented early on hurts this paper. Why can the observations not come about without drizzle? The conceptual model has been introduced into the paper (Comment1). But, the reviewer's comment is not clear to us and thus could not have been treated.

21) Page 33245, lines 11-12 – what are you referring to by the “total N-R PM”? Total non refractive particle matter means the mass concentration measured by the AMS. The definition is found in section [XX] of the manuscript when we introduce the AMS measurements.

22) Page 33245, lines 15-16 – If these clouds were truly SCu, then the aerosol would have been cycling between below- and in-cloud with a time constant of perhaps 20-30 minutes (see work by Feingold and Kreidenweis, early 2000) along the wind path. In addition, there would be some limited exchange of aerosol at cloud top and some removal by precipitation. It would be more relevant to compare below-cloud with incloud concentrations. The comparison of the in cloud residuals and the particles below cloud would lead to an interpretation of the cloud processing biased by the non-activated particles which not be easily interpretable. While, the direct comparison with aerosol in the boundary layer (over the evaporation level) and before the cloud presence (aerosol reference) to the in-cloud residuals allowed us to estimate the activation efficiency and the comparison of aerosol population not influenced by the cloud (aerosol reference) with the aerosol population below the cloud highlighting the impact of the cloud processing.

23) Page 33245 – The “sulfate EAP is low”. Again, losses in the CVI line are unlikely to explain this (unless there is something untold). The apparent deficiency in your EAP is more likely based on AMS problems and CVI characterization. A factor of CE=0.5 has been used to correct the AMS data set, this is a guess only, we cannot proof it. In case

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we would assume a CE of between 0.25 and 0.5 - which means that you put a further factor of 2 on the sulfate aerosol, then we could state that the data as they are provide the lower end and a further factor of 2 (meaning a CE of 0.25) as an uncertainty is thinkable. Its true this factor/uncertainty of two is possible due to the drying by the CVI. But this value cannot be assumed to be true in general.

24) Page 33245 – For many years ago, nitrate has often been found in cloudwater at levels comparable to sulphate, in continental situations, and it is more likely to result from the scavenging of HNO₃ (e.g. Leaitch et al., JGR, 1986) or N₂O₅ (e.g. Leaitch et al., JGR, 1988). See also Hayden et al. (JGR, 2008) for results of airborne CVI-AMS measurements of nitrate in cloud.

25) Page 33246, lines 9-12 – You have many uncertainties in the measurements (your “error” bars are not true uncertainties, just standard deviations) as well as the conceptual model, but you ascribe a 0.12 ug/m³ difference in the organic mass to in-cloud production. Even if I accepted the uncertainty as only described by your SD, the difference between 0.938 and 0.825 is not significant at any reasonable confidence level. The ‘error bar’ used in this section are accounting only for the standard deviations (1σ) of the measurements. The authors also corrected a mistake in the units of the concentration which are not in μg cm⁻³, but in μg m⁻³ which may explain the concerns of the reviewer for the AMS detection limit. The detection limit for each compound has been quantified by De Carlo et al. (2006) :

Table 1 : Measured Detection Limits for the C-ToF-AMS Detection limit (μg m⁻³) organics 0.019 sulfate 0.0022 nitrate 0.0012 ammonium 0.016 chloride 0.004

The difference in the mass concentration observed for the organics, nitrate and sulphate are an order of magnitude over the detection limit. Thus, the use of these differences should be relevant. The authors agree with the referee that the standard deviations are high in comparison to the actual measurement values; however, we are comparing the in-cloud concentrations (0.938 and 0.825 +/- 0.4 μg m⁻³, respectively in

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the morning and the afternoon) with the mass concentrations measured in the vicinity of the cloud (3.355 and 1.82 +/- 0.4 $\mu\text{g m}^{-3}$, respectively in the morning and the afternoon). Our goal, here, is to provide an estimation of the efficiency of activation and in-cloud production and the authors show that the differences between these concentrations are large enough to be significant.

26) Page 33247, line 8 – “framework”. We replaced the word ‘frame’ by ‘framework’

27) Page 33248, lines 4-7 – the bimodal shape of the below-cloud distribution does not need to rely on drizzle. If these clouds were truly SCu, then the in-cloud and belowcloud air will be mixed to some degree. The below cloud base aerosol population has been modified by the cloud (i.e. activation, scavenging and reinjection). The bimodal distributions have long been shown to be a result of cloud processing (Hoppel et al. 1994, Saxena 1996, Clarke et al., 1996, Weber and McMurry 1996). We have modified the sentence into the manuscript.

Clarke, A. D., Z. Li, and M. Litchy (1996), Aerosol dynamics in the equatorial Pacific marine boundary layer: Microphysics, diurnal cycles and entrainment, *Geophys. Res. Lett.*, 23(7), 733–736, doi:10.1029/96GL00778.

Saxena, V. K. (1996) Bursts of cloud condensation nuclei (CCN) by dissipating clouds at Palmer Station, Antarctica. *Geophysical Research Letters.*, 23, 69-72.

Schwarzenboeck A., and Heintzenberg J. (2000). Cut size minimization and cloud element break-up in a ground-based CVI. *J. Aerosol Sci.*, 31(4), 477-489.

Schwarzenboeck, A. Development and application of complementary hydrometeor and interstitial aerosol samplers, 150 pp., VWF Berlin, ISBN: 3-89700-093-8, 1999. <http://www.vwf.de/autoren/3-89700-093-8.php3>

Weber, R. J., and P. H. McMurry (1996), Fine particle size distributions at the Mauna Loa Observatory, Hawaii, *J. Geophys. Res.*, 101(D9), 14,767–14,775, doi:10.1029/95JD02271.

C16617

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C16618

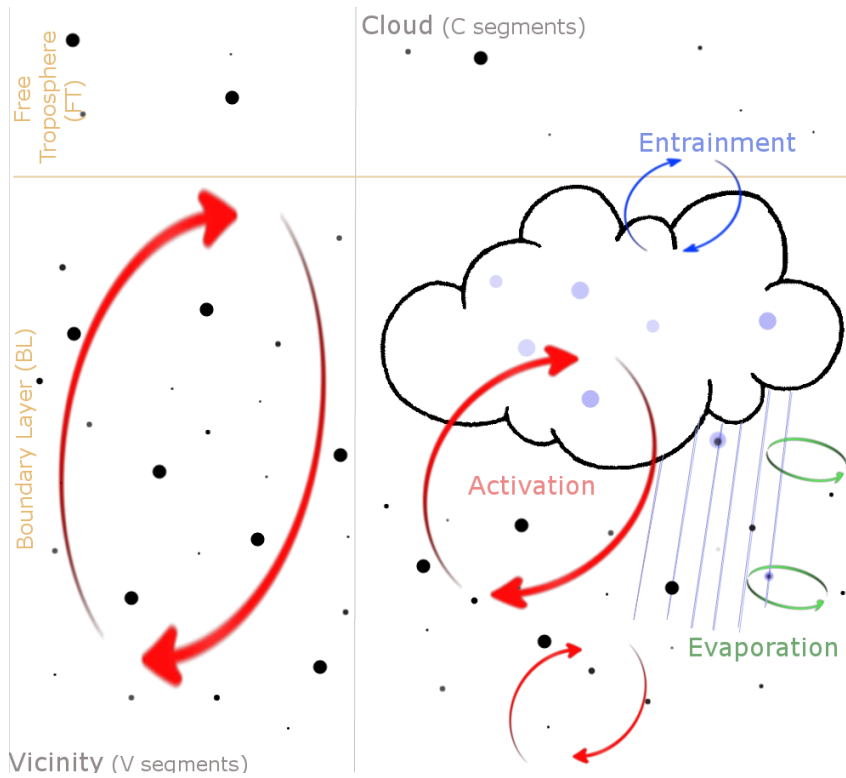


Fig. 1. Sketch of the mean thermodynamics structure of the lower troposphere in cloud free condition (Vicinity) and during the presence of a precipitating stratocumulus (Cloud).

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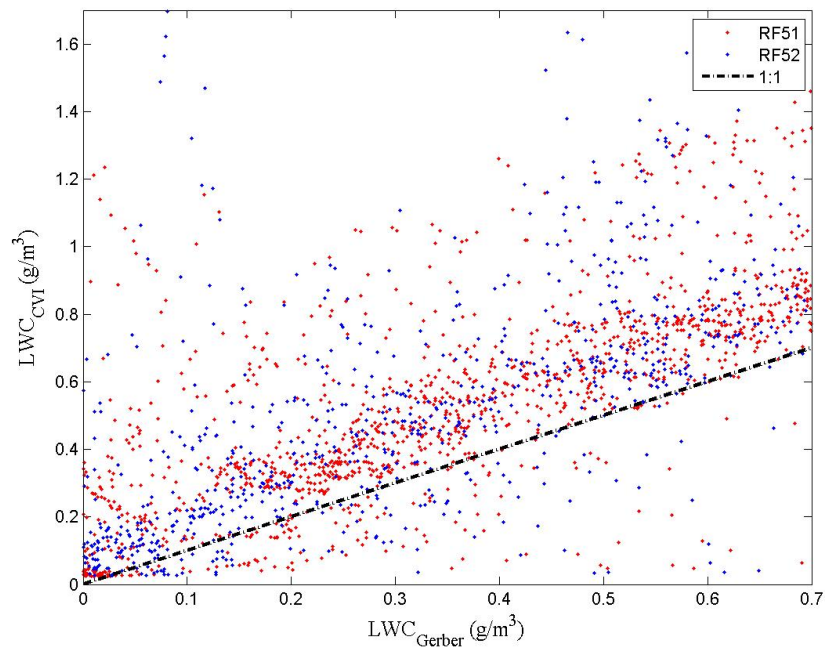


Fig. 2. Comparison of the Liquid water content (LWC) measured by the CVI and by the Gerber probe for the research flights (RF) 51 and 52. The dashed line represents the slope of 1 line.

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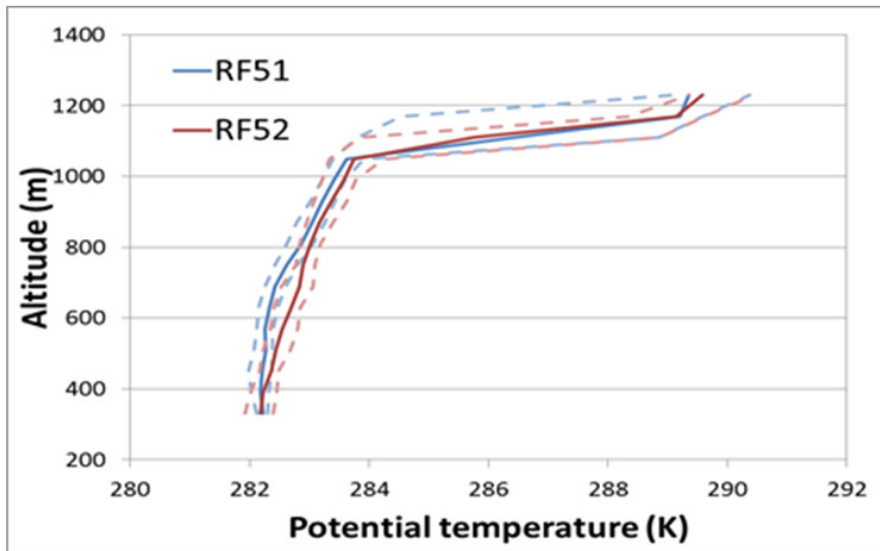


Fig. 3. Vertical profiles of the potential temperature measured by a rosemount sensor on board the ATR-42 during the RF51 (blue lines) and RF52 (red lines)

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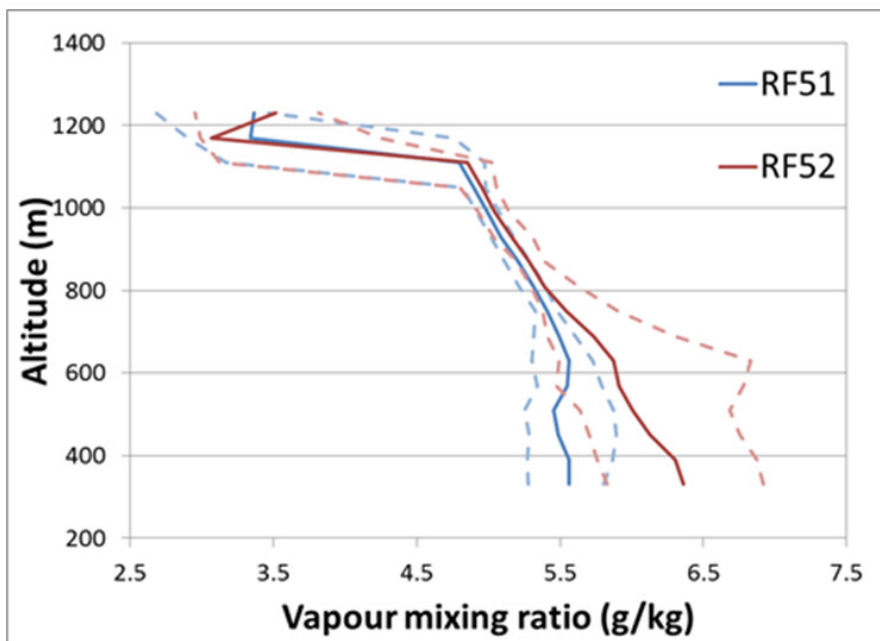


Fig. 4. Vertical profiles of the vapour mixing ratio measured by a dew point hygrometer on board the ATR-42 during the RF51 (blue lines) and RF52 (red lines)

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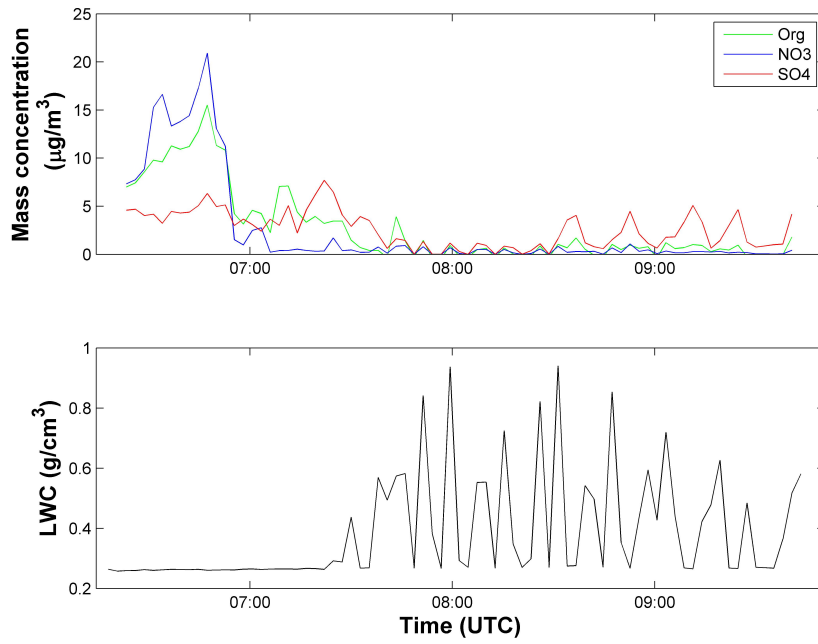


Fig. 5. Mass concentrations of the organics (Org), nitrate (NO₃), and sulfate (SO₄) measured by the AMS as a function of time (top) and liquid water content as a function of time (bottom) for RF51

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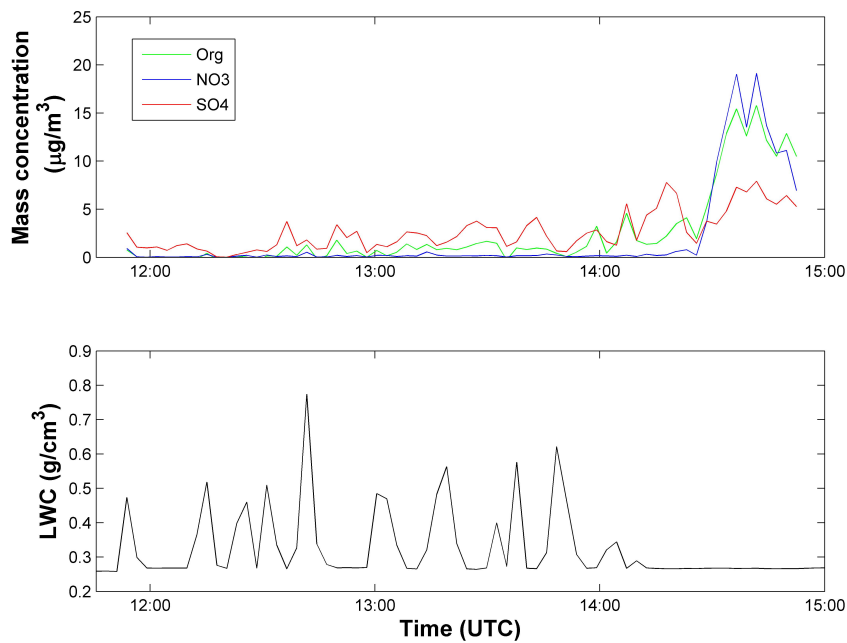


Fig. 6. Mass concentrations of the organics (Org), nitrate (NO₃), and sulfate (SO₄) measured by the AMS as a function of time (top) and liquid water content as a function of time (bottom) for RF52

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