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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal variation of aliphatic amines in marine sub-micrometer particles at the Cape Verde islands" *by* C. Müller et al.

Anonymous Referee #1

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General Comment

The paper presents new results on aliphatic amine salts detected in marine aerosol particle at Cape Verde. The hypothesized process of formation is a gas to particle conversion mechanism confirming previous observations carried out in North Atlantic. The variation of amine salts concentrations observed in aerosol samples is related to the oceanic biological activity and an interesting observation is carried out in winter during an anomalous algal bloom associated to high Saharan dust deposition and an intensive ocean layer deepening. The paper merit the publication on ACP after having addressed these major points

1)Title and inside the text: the term amines is not correct for condensed species detected in n aerosol particles the authors should use the term amine salts or alkyl ammonium. 2) Abstract and Summary are somehow in contradiction : abstract is mainly cantered on impactor data while Summary discusses Hi Vol data. I suggest to change the summary in the direction of abstract for the reason explained in the next point. 3) There is a main problem in the data set: the impactor samples were analyzed only for stages 2,3 and 4 excluding the coarse size range based on a previous observation in North Atlantic reporting amines salts distributed in the accumulation range and not detectable concentration in the coarse fraction. The concentration obtained by summing the impactor stages 2,3 and 4 (corresponding to 0.14-3.5 um size rage) is not superimposed with PM10 but a relevant fraction of coarse particle is missing. For this the authors cannot compare impactor data to Hi Vol data which also show higher concentration concluding that these are due or to positive artifacts or to negative artefacts on the impactor foils. The authors first have to show that the impactor coarse fraction not analyzed does not contain amine salts I suggest to analyzed a few impactor samples in the interval 3.5-10 um and to show the comparison with Hi Vol on the same sampling period. From these data the author can conclude that Hi Vol overestimates the concentrations and discuss the reasons. Moreover the author can show with independent measurement the secondary origin of amine salts. Then I suggest to discuss the single component concentration only for impactors as done in the abstract and use the Hi Vol data only for discussing the seasonal trends. For these reasons I think that it is very important to change the summary and making this coherent with the abstract (see pont 2)

3) The analytical technique is not published anywhere else ?The text do not contain citation of previous papers. If not published before a more details description of the analytical methodology is needed including detection limits, errors and details on the calibration procedure based on a microscale derivatization.

4) The author discuss N budget but they did not measured total N and thus organic

nitrogen. The relative contribution of amine salts to what is called "total N" (which is in reality the sum of inorganic plus aminic N) is misleading. I suggest to remove this session and in particular the discussion following the citation of Gibbs et al., (1999) and Fig 8. In alternative the author could measure organic N to discuss the topic properly. 5) Introduction is quite poor : the author should discuss the possible primary origin (from sea spray processes) of amine salts suggested in many papers in literature in the case of organic N of marine aerosolas well as amine and aminoacids. Moreover I also suggest to include in the introduction a brief review of the current knowledge of primary and secondary marine OA before starting the specific discussion on amines. More literature references are needed in general in the text.

Minor Points

1)Introduction line 10;: It is not true that Facchini et al., (2008) found "not negligible" concentration of DMA and DEA: these were relevant representing 11% of the sub micron SOA during high biological activity period. 2)I suggest to eliminate the discussion on Morfoline from "Results and discussion" since this is clearly an artifact product. 3)3.1.4 Lines 1-18 This discussion is too much detailed and fig 6 is very complicated and not necessary. Fig 7 or a table is sufficient to describe the contribution of amines salts to total OC

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14825, 2009.