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Interactive comment on “Particle characterization at the Cape Verde atmospheric observatory during the 2007 RHaMBLe intensive” by K. Müller et al.

Anonymous Referee #2

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This manuscript presents and discusses results from filter and size-segregated atmospheric aerosol characterisations during a one-month campaign (i.e., from 14 May to 14 June 2007) at an observatory in the Cape Verde Islands. The analytical data seem to be of good quality and their interpretation is overall fairly OK, but the manuscript does not really present much new interesting information. It is unclear whether the air mass transport during the campaign was like one could expect. From 8 to 13 May, thus before the actual start of the campaign, the air masses came from the African continent, but during the campaign itself, they did not really touch the continent, although they came generally from the NNE (Fig. 1). The authors state on page 22751 that May and June 2007 were (as expected) low-dust summer months. Thus, after all, the air mass transport during the campaign may have been fairly normal. This should be indicated when discussing the back trajectories in Section 3.1 and perhaps already in

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the Introduction.

The manuscript leaves me somehow on my hunger. The Introduction talks about soluble iron and about the importance of long-term measurements and makes you expect more from this manuscript than there is in it. I recommend that the authors rewrite their Introduction somewhat.

With regard to style and language, the definite article is to my feeling used on some occasions where it should not. I indicated below (under Specific comments) where this is the case.

Specific comments:

1. P. 22740, l. 18: Replace “particles size” by “particle size”.
2. P. 22743, l. 14 and l. 17: The use of the term “annealed” in this context seems strange to me. It is more common to use “pre-fired” or “pre-baked”.
3. P. 22743, l. 18, and also later on, with regard to the Nuclepore foils: First, I presume that these were Nuclepore polycarbonate foils. The actual material of the foils should be specified, not only the trade name. Secondly, it is unclear whether the Nuclepore foils were also weighed in order to obtain the PM mass. Thirdly, as I understand it, both Nuclepore and aluminum foils were used for each BI stage. It is unclear what fraction of the ring was Nuclepore and which fraction was aluminum. This should be specified. Fourth, on p. 22750, l. 12, it is indicated that the PM spots for stage 5 were not visible. It is unclear why this formed a problem in the analysis (e.g., if one half of the ring consisted of a Nuclepore foil). Some explanation on this is needed.
4. P. 22743, l. 1: It says here that a fraction of the aluminum foils was used for metal analysis, whereas it was stated earlier that the Nuclepore foils were used for metal analysis. Clarification is needed.
5. P. 22745, l. 1: “on the polished” should be replaced by “on polished”.

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6. P. 22745, l. 4: “concentrations” should be replaced by “amounts”.
7. P. 22746, l. 19: Replace “anthropogenical” by “anthropogenic”.
8. P. 22747, l. 6: Replace “within” by “with”.
9. P. 22747, l. 10: “Sect. 3” should be replaced by “Sect. 2.2”.
10. P. 22747, l. 22: A reference is needed for the 4% iron in Saharan dust. Later on (i.e., on p. 22751, l. 2) two references are provided. Presumably, the same two references can already be used here.
11. P. 22748, l. 11: It is not really clear what the authors mean by “water content”. Do they mean the water content of the aerosol, as calculated from the ionic composition and explained on p. 22751, l. 10-12? Clarification is needed.
12. P. 22748, l. 24: “range the differences” should be replaced by “range differences”.
13. P. 22749, l. 23, continuing through p. 22750, l. 7: The discussion on the dicarboxylic acids is meager. Malonate and succinate are associated with coarser particles than oxalate. Is there any explanation for this behaviour, which looks strange to me.
14. P. 22750, l. 14: It is totally unclear to me why the TXRF needs a piece of sample smaller than 9 mm in diameter.
15. P. 22750, l. 27, and p. 22751, l. 1: It is unclear to me why the authors can state here that the back trajectories on 14 and 15 May indicate a preferential direction from Western Africa and the Canary Islands. None of the trajectories shown in the right part of Fig. 1 touch the African continent.
16. P. 22751, l. 2: “was about” should be replaced by “is about”.
17. P. 22751, l. 11: “r.H.” should be replaced by either “r.h.” or “R.H.”.
18. P. 22751, l. 15 “error” should be replaced by “uncertainty” or perhaps by “standard deviation”. One should try to avoid making errors.

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19. P. 2252, l. 2: I presume that the authors refer about the study of Chiapelli et al. (1995) when talking about these two years. This should be made more clear.

20. P. 2252, p. 20: Comparing the MSA data from this study with those above the North Yellow Sea is a little farfetched. There are plenty of other MSA data sets that would be more suitable to compare to. Also referring to Antarctic data for the absence of correlation between, on the one hand MSA and nss-sulphate, and, on the other hand, DMS may not be that appropriate. Actually, the authors should better talk about the absence of this correlation in their own data set.

21. P. 22752, l. 25: I do not understand that there are problems with estimating the water content for the HV filter samples when this calculation could be done for the BI samples. Are the authors perhaps referring to water uptake by the filter itself, which is mentioned on p. 22753, l. 22-23? If so, they should already mention this water uptake here.

22. P. 22753, l. 20: “The differences” should be replaced by “Differences”.

23. P. 22754, l. 10-15: I suggest that some information is provided on the filter material, sampling location, and height above sea level for the TSP sampler of Pszenny and Keene. This could provide some clues on the difference for Br between the NAA data and the IC data of the current study. Incidentally, how did the NAA and IC data compare for Na? The authors attribute the difference to the presence of non-ionic Br. Could this be gaseous Br that was adsorbed by the filter material in the samplings of Pszenny and Keene?

24. P. 22755, l. 17: “B. Keene” should be replaced by “W.C. Keene”.

25. P. 22755, l. 18: “their help” should be replaced by “her help”.

26. P. 22757, l. 18-20: No reference is made to Lim et al. (2003) within the text.

27. P. 22758, l. 8-9: No reference is made to Prospero et al. (1981) within the text.

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28. P. 22766, Fig. 5: The units for the Ca data should be given.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22739, 2009.

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