Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Marine organic matter in the remote environment of the Cape Verde Islands – An introduction and overview to the MarParCloud campaign" *by* Manuela van Pinxteren et al.

Anonymous Referee #2

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The manuscript presents an overview of the MarParCloud campaign at Cape Verde Islands in Sept – Oct 2017. Several interesting new scientific findings are reported in brief or just mentioned, but the main scope here is to present a synopsis of all oceanographic and atmospheric observations that have been carried out during the field campaign. If the Authors' intention was to give a flavour of a very multidisciplinary experiment, I think this emerges quite clearly from the paper. I have only two major remarks about the science (see my major comments below). Besides, since most of the results are object of specific papers in preparation, my remarks are mainly on the

quality of the presentation.

Major comments:

1. The Abstract is rather descriptive but it ends up with a strong statement: "from a perspective of particle number concentrations, marine contributions to both CCN and INP are rather limited". I think that the Authors here should make an effort to a) be more quantitative (or refer to other papers either published or in preparation) and b) make sure that there is enough information in the Abstract as well as in the main body of the manuscript to support such conclusion. I have not found any sections in the paper dealing with CCN, just a brief mention at line 560 focusing on the proportion of CCN at low supersaturations accounted for by coarse particles, but what about the estimated contribution by primary marine aerosols?

2. There is instead a section about INP (5.7.4) providing a short summary of the study of Gong et al. (2019b) and concluding that primary marine INP should be four orders of magnitude more abundant to account for the ambient INP concentrations measured in cloud and aerosol samples. If this is the basis for the final statement included in the Abstract, I suggest to report it along with the main hypothesis made by the Authors who assume "INP not enriched or altered during the production of sea spray" from the SML (lines 966 – 967), which is a strong assumption, in my opinion. Otherwise, more supporting information can be extracted from Gong et al..

Specific comments:

1. Three of the seven research questions (page 5) should be retuned:

a. Question #1: specify what are the metrics of interest (number, mass, CCN, etc.).

b. Question #2: do "OM groups" mean source contributions or chemically-defined classes?

c. Question #3: this reads like a rhetorical question; it should be restructured into something like "What are the main biological and physical factors responsible for the

occurrence and accumulation of OM etc."

2. Line 225: Hg cannot be considered a good "example for trace metals", as it exhibits unique chemical properties.

3. Line 226: "pigments [. . .] were captured..", I do not think the verb is appropriate.

4. Line 227: ". . .(DMS), VOCs.."; as DMS is a VOC, this should be better put as ".. (DMS), other VOCs..".

5. Line 439: "These issues will be further analysed", does it mean elsewhere in the paper or in a future publication? Please specify.

6. Section 4.1.4: What is the relevance/representativeness of the five cloud scenes?

7. Lines 478 − 480: "A synergic combination with ground-based in-situ and remote sensing measurements would be highly beneficial for future investigations". The sense of this sentence is rather obscure ("beneficial" for what? For which of the seven research questions listed in the introduction??), please clarify.

8. Section 4.2. I suggest to report here only the concentration levels of the main indexes of biological productivity (chlorophyll concentrations) and their spatial distribution (sub-section 4.2.2 is ok), while I would postpone the discussion about pigment distribution to section 5.4.

9. Line 535: satellite fluorescence measurements. Which satellite?

10. Section 5.1.1: see major comments.

11. Lines 602-603: "..suggested an ocean influence on cloud water". Please, be more precise here. The data show a cloud water composition dominated by seasalt with little quantities of other solutes: this is simply the effect of the larger scavenging efficiency of coarse particles with respect to submicron ones, but it is worth reminding that this picture is "mass-based" (all cloud drops coalesce into one single sample inside the CASCC) while in terms of number concentrations (how many cloud drops originated

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from marine sources), it is difficult to tell solely on the basis of the data shown in Fig. 11.

12. Line 644: ".. by southern Hemisphere": it is not clearly shown in Figure S1.

13. Section 5.3.2: The results about HONO look so preliminary that I am not sure they deserve a dedicated section. They can be reduced to a short paragraph at the end of the previous section about trace gases.

14. Line 850: Enrichment factors for DOM are reported. Please specify the concentration unit used for DOM (organic carbon, organic nitrogen or UV absorption?).

15. Lines 987 − 988: ".. a daily variation of the number of particles formed was observed (but from a very limited set of samples, n = 3) probably related to the daily sampling conditions. To explain these observations, two different hypothesis can be postulated. . ." Actually, it is not clear at all what observations the Authors are referring to, because no data are shown but simply a "daily variation" is observed and only for three samples. Similarly to the HONO case, the impression is that the state of the analysis of the Go:PAM dataset is just too preliminary to be discussed in a dedicated section.

16. Section 5.9.1. Please provide a short description of COSMO.

17. Section 5.9.2. Please provide a short description of MUSCAT.

18. The graphical quality of the figures must be improved.

19. Figure S4: there is something wrong in this figure.

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