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Atmospheric Measurement Techniques Discussions

Interactive comment on "Laboratory-generated primary marine aerosol via bubble-bursting and atomization" *by* E. Fuentes et al.

Anonymous Referee #2

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The manuscript is a worthy though not very substantial contribution to the field. However, some of the data, hygroscopicity and CCN measurements in particular, are interesting. It is a comprehensive summary of already available knowledge of laboratory generated primary marine aerosol via bubble bursting and data are presented quite clearly. Some of the important details are missing and some of the conclusions are overstated, which need adjustment. The manuscript can be accepted with minor revisions, but those revisions are nevertheless significant.

My two major comments are: The statement that the best generating technique was found to be impingement of water jets is not unique. Previous paper by Sellegri et al. clearly demonstrated it back in 2006. Therefore, I would rather say that this paper confirms previous findings with some additional data which should be clearly stated.

The findings of this paper in general are indeed confirming many previous findings, but in a more comprehensive, detailed manner.

Large overview of references presented in the introduction may have a common underlying feature, which should be adequately addressed throughout the paper, i.e. bubble path. Many laboratory generating systems use small tanks, therefore, short bubble path and lifetime. Bigger tanks like the ones used by Keene et al. (2007) or Facchini et al. (2008) would ensure longer bubble path and life time. A possibility that the bubble path could explain differences between the results of different studies should be seriously considered.

Specific comments:

The abstract stated that laboratory measurements were compared with ocean measurements. I did not see any ocean measurements performed in this study and the title is solely about laboratory experiments. If it were literature data (looks like of Sellegri et al.) that should be clearly stated.

Page 2283 O'Dowd et al. (1997) has been misinterpreted in that wind speed above 4m/s produces droplets up to 10um. It is, indeed, very good review paper, which demonstrated that sea salt particles produced by wind stress can be as large as 50um.

Page 2284 lines 6-15 Authors claim that data from field experiments (please name them) provided evidence for the presence of significant concentrations of biogenic organics, which is interpreted as primary. While such evidence is compelling it is important to state that organic matter in oceanic aerosols certainly is not all primary, but a significant fraction of it is secondary. Turekian et al. (2003, JGR), Ceburnis et al. (2008, GRL) and Facchini et al. (2008, GRL and ES&T) are just a few late papers demonstrating that. MSA, depending whether it is considered as organics or a separate intermediate compound nevertheless is secondary marine species as well.

Page 2286, line 16 refers to natural seawater. What the authors meant by natural?

The reader founds only artificial seawater made of inorganic salts and artificial seawater + synthesized biogenic organic material which was explicitly in the form of DOC. Natural seawater contains both DOC and POC with some intermediate colloidal matter (Facchini et al. 2008).

Page 2287 There is inconsistency with tank dimensions. Tank capacity looks like 12 l according to dimensions. It was filled with 6 l of water, which corresponded to 13cm water level, which should have been half of the tank, but then the height of the tank would be 26cm (it is 21cm in dimensions). I guess it was typo error somewhere.

Line 19. 8 water jets were mentioned, but no total flow was given. Figure 3 mentions flow rates of 1.0-5.0 lpm, which dividing into 8 jets would yield as little as 0.125 lpm – surprisingly low plunging water flow rate. What was the velocity of water jets?

Page 2289 Again the term "natural seawater" is used which was rather artificial seawater + biogenically synthesized organics.

Thalassiosira Rotula should be written with rotula in small cap. No information is given in terms of viability of the culture at the time of separating DOC nor the length of growth cycle.

DOC and TN analyses mentioned, but not clear to what purpose. In Page 2298 DOC concentration of 512 uM was mentioned. Was there any particular reason to choose it? In any case that info should be moved to Experimental section.

Page 2292. There is a long consideration about the shape of particles, ranging from cubic to spherical. How relevant is that without giving details at which RH the aerosol size distribution measurements were performed (except HTDMA at 10%)? Having a mixture of inorganic salts, namely magnesium salts, even at lower than 40% RH there is more than monolayer of water, which would change a cubic shape closer to sphere.

Page 2295 What was the criterion to decide on "optimum" number of lognormal modes? Was it an error minimization between composite and measured size spectrum, correla-

tion between the two or just common sense? I guess the latter would not be sufficient.

Page 2303, lines 19-21 I don't quite understand a conclusion that comparison of plunging-water jet system produced aerosol size distributions with that of Sellegri et al. real world aerosol size distributions indicates the superiority of water jet system. Was it not exactly that demonstrated by Sellegri et al.? In any event I am not sure if one can unambiguously argue that laboratory generated primary aerosol spectrum should agree with the ambient spectrum which is clearly a combination of primary and secondary processes (both may have biogenic origin). Certainly, same issue applies to Sellegri et al. results.

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