

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-74

Anonymous Referee #1

Referee comment on "Fluorescent biological aerosol particles over the central Pacific Ocean: covariation with ocean surface biological activity indicators" by Kaori Kawana et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-74-RC1>, 2021

The manuscript by Kawana et al. studies the relationship between ambient bioaerosols (measured using single-particle autofluorescence) and various chemical and biological parameters of aerosols and sea water. The data was collected through a transect on the Pacific Ocean. The authors find various correlations between the concentration of fluorescent aerosol particles and biogenic sea water proxies and windspeed. From these observations, various parameterizations are developed to predict the concentration of FAPs in the marine atmosphere using biogenic sea water proxies and wind speed.

The manuscript is well written and the analysis appear (at least for main parts) sound and thorough. The study presents new and useful observational data which can be used to develop model parameterizations. My main criticism relates to the statistical significance of the presented findings and the developed parameterizations. Due to the limited number of samples (which is natural due to the comprehensive analysis needed) some of the equations and statements seem to come with a very large uncertainty. However, a thorough error analysis and discussing is currently missing and should be added before being finally published in ACP. This and further detailed comments are given below.

Detailed comments (in arbitrary order):

- Introduction: Many of the references are selective. I would suggest to add "e.g." before them to indicate that the references are just a selection.
- Page 1, line 26: could -> can
- Page 3, line 7: More details on the aerosol inlet are needed. For example, Was a size-cut used? What was the length of the inlet? Was it heated or was the sample flow dried? What was the average RH at the inlet of the WIBS?
- Page 4, line 5: Since the instrument does not only count but also sizes the particles by optical means, I would suggest to replace OPC by OPSS: optical particle size spectrometer.

- Page 5, line 6: Why was a starting (or arrival) altitude of 500 m and not sea level (or inlet height) chosen for the trajectory analysis? I could imagine that it could make a difference for some of the trajectories.
- Figure 1 and 2: To save space you could consider to move those figures to the supplement as well since they are mainly used to justify the two periods and don't provide much information concerning the biological aerosol particles.
- Figure 3 and 4: It would be helpful to also indicate the different periods here.
- Figure 4b: There are two striking peaks for ABC particles (right at the beginning and close to the 11th of March. Is this linked to ship pollution?
- Page 6, last paragraph and Figure 5: The number of samples from the Bioplorer analysis is limited to only 10 samples, correct? Or the Bioplorer data been averaged to match the filter chemical analysis? An overview table with the performed analysis, instruments and their temporal resolution / number of samples would be useful (e.g. within the method part).
- Page 7, line 15ff and Figure S2: The correlations are for certain parts driven by a few data points only. It should be stated that the statistics is still limited due to the low number of samples.
- One major weak point of this study is that no measurement uncertainties or other statistical parameters (like standard deviations or percentiles) are given. The study would significantly improve if uncertainty bars would be added to the key figures or given in tables and discussed within the text. In Sect. 3.4, the presented parameterizations (Eq. 1-3) need more profound statistical analysis. This is important because readers and potential users need to be aware of the potentially large uncertainties involved here. For example, it can be easily seen that the relationship between bioaerosol concentration and the chl-a*ws parameter is driven essentially by one point (Fig. 9a). I would therefore like to ask the authors to add an uncertainty analysis (incl. error bars in the main figures, especially Fig. 9). The parameters of Equations 1-3 should include uncertainties. One approach could be to use a bivariate weighted fit (see e.g. York et al., 2004). Please also check the units of all coefficients carefully (e.g. the intercepts need units).
- I would recommend that the authors include in the presentation and discussion of their results the study by Santander et al (2021), who recently characterized the performance of the WIBS in controlled sea spray experiments.
- Why hasn't the size information of the WIBS not been fully exploited? How did the particle size of the different fluorescent particle classes (e.g., Fig. 4) change with time? The authors could think to add an overview table to the manuscript or the SI that gives averages parameters (and their variation) of the different classes. Figure S1 does show that certain FAP classes have much larger diameters. Could these be more primary bioaerosols? See also paper by Santander et al. (2021).
- The last paragraph of Sect 3.4 could be shortened and combined with the conclusions.
- Data availability: It would be beneficial to the reader and the community if the data behind this study can be found on a public repository to follow the FAIR principles of data sharing (see data policy of ACP, https://www.atmospheric-chemistry-and-physics.net/policies/data_policy.html).
- In general, the time series figures could be increased in size (mainly width). Especially for Fig. 3, 4, 6 and 7 it is hard to discriminate all the features.
- Figure 8: Similar to the comment above, what kind of regression was used? It should be an orthogonal one. Please add this information to the text or figure caption(s).
- Figure S2: As mentioned above, it is also clear from this figure that the correlation coefficients are driven by a few outliers. This should be properly discussed within the text.
- Figure S5: This is an interesting figure (which could be moved to the main manuscript?) since it shows that the contribution of phytoplankton species clearly changed through the cruise. Is there any link between the WIBS particles classes with the phytoplankton species? Or is this not to be expected? The authors could test if they see a relationship of intensive parameters from the WIBS (e.g. ratios of particle classes) to the

phytoplankton species contribution.

References:

York, D., Evensen, N. M., Lopez Martinez, M., and De Basabe Delgado, J.: Unified equations for the slope, intercept, and standard errors of the best straight line, *Am. J. Phys.*, 72(3), 367–375, 2004.

Santander et al.: Tandem Fluorescence Measurements of Organic Matter and Bacteria Released in Sea Spray Aerosols, *Environ. Sci. Technol.*, 2021.
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