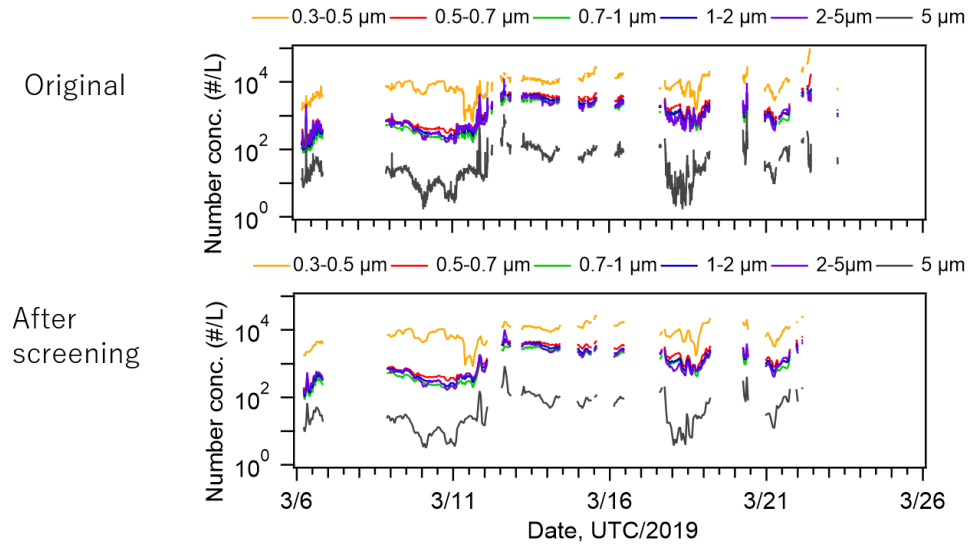
Reviewer #2:

In this manuscript, K. Kawana et al. investigate fluorescent biological aerosol particles over the central Pacific Ocean and propose equations to derive atmospheric bioaerosol number density in the marine atmosphere from a combination of biogenic proxy quantities and wind speed. These equations could help in the parameterization of models, and as such, this manuscript will make a nice addition to the literature. My only relatively major concern is the absence of discussion on analytical or sampling uncertainties, especially for the following parameters: nutrients, Chl-a, bacteria, TEP, and CSP. The rest of my comments (see below) are mostly minor.

We would like to thank the reviewer for providing valuable comments and better perspective on our work. Our responses are given below. All changes are shown in color in the revised manuscript.

Page 4, lines 7-9: "To avoid contamination from ship exhaust, the data points from the online measurements were screened using the same criteria that were applied to the operation of the pump of the high-volume air sampler". Have you checked whether this criteria of ±75° from the bow is stringent enough? A simple test is to check whether you have instances with sudden ozone titration (due to NOx emissions from the ship exhaust).

We used the OPC data to evaluate this criterion. When the same criteria were applied for the OPC data, the sudden increase/decrease events were removed so we concluded the criteria of ±75° from the bow was acceptable basically. The screening criteria for ozone data were developed independently as the location of the inlet was different from that for the aerosol measurements. Thus, the direct comparison of the criteria was not considered straightforward.



Page 4, lines 10-13: surface seawater sampling was carried out with a bucket but you only analyzed 200 mL of water (if I understood correctly). Is this volume of water (200 mL)

representative of what was in the bucket (homogeneous sample)? Did you collect replicate filters? If so, please indicate the results as error bars in the figures. If no replicate samples were collected, could you please at least discuss analytical uncertainties and report them as error bars in the figures? I'm simply wondering if the temporal variation you describe later in Figures 6-7 is significant or if it's just noise.

We added the following sentence describing uncertainties and precision in the revised manuscript:

"For the analysis of TEPs and CSPs, 200 mL of seawater out of ~10 L collected in a bucket was filtered onto a Whatman 0.4 μm Nuclepore hydrophilic polycarbonate membrane filter (Cytiva, Tokyo, Japan) where the particles were retained. By repeating this procedure, sample filters were made in triplicate." (Page 4, Lines 16-18).

We also described the representativeness of the data or related information on the nutrients, Chl-a, as well as TEP and CSP as follows:

"The analytical precisions of the nutrients and Chl-a concentrations were all <1%. For bacteria, ranges are shown for samples where duplicate measurements were made (Fig. 6c). For TEP and CSP, error bars represent one standard deviation from the repetitive analysis (Figs. 6d and 6e). They were all small enough to regard that their natural variations were captured by the observations." (Page 7, Lines 26-29).

Page 5, lines 5-6: There should be a section on back-trajectories in the Methods. In addition: 1) which meteorological data did you use to generate the trajectories? 2) why did you use a starting altitude of 500 meters? 3) please also include the fact that you only generated trajectories twice a day (0600 and 1800 UTC according to Fig. 1 caption).

Although considered, we decided to keep this description on the trajectory methods with the results together in section 3.1 for better readability.

1) As suggested, we added the following sentence.

"The used meteorological field was the Global Data Assimilation System with 1°×1° resolution (GDAS1) by the National Center for Environmental Prediction (NCEP) analyses." (Page 5, Lines 12-13).

2) The starting altitude lower than 500m increased the number of cases where the air parcel hit the sea surface and discontinuity occurred. Calculations were also carried out with a starting altitude of 1000m and there was no essential difference in the results. Therefore, only the results with the starting altitude of 500m were presented. As described in the manuscript, the timing of change of air masses suggested by the trajectories from 500 m was consistent with the timing of the increase in the ozone concentration, and thus the results of the trajectories are considered to be reasonable.

3) As already indicated in the Figure caption, we do not repeat this in the main text.

Page 5, lines 10-13: Are these results in line with literature? See Bourgeois (2020) for example.

We added the following sentence in the revised manuscript.

*"*Bourgeois et al. (2020) studied ozone concentrations over the similar latitudinal range and season and reported that the concentration increased in the north of ~10°N, further north than ~5°N for this study. It is likely that a strong northeasterly wind efficiently carried air masses with relatively high concentrations of ozone down to 5°N during our study period." (Page 5, Lines 23-26).

Page 6, lines 1-5: How about the increase in type B particles at the end of the campaign?

We added the following sentence.

"Type B particles increased at the end of the observation (Period 2), and the response with the wind was different from the behavior of types A and C. Effects from continental air masses during Period 2 (Fig. 1) or changes in the influential marine biota as aerosol sources were suspected. Detailed discussion is given in section 3.4." (Page 6, Lines 17-20).

Page 6, lines 18-19: It's hard to tell with the log scale. Can you please add in the text the mean ± standard deviation before and after March 12 for each size range?

We added the following sentence:

"The number concentrations of particles smaller (and larger) than 1 μm increased from 874±552 (and 913±680) cm^{-3} during 10–12 March to 6903±650 (and 7436±2180) cm^{-3} during 12–14 March." (Page 7, Lines 3-5).

Page 6, lines 30-31: Please make it clear in the caption that the y-axis is different for the two instruments; it took me a while to realize that.

We corrected as suggested in the revised manuscript.

Page 7, line 8: "high again in the south of the KR on March 22". This is not obvious…I'd appreciate error bars on this Figure. Same comment for the rest of this section.

We are unable to add error bars for nutrients as repeated measurements were not made. However, the Y axis in Fig. 6a is displayed as logarithmic for clarity and the values in the North Pacific subtropical region and the south of the Kuroshio Extension were described as follows.

"Nutrient concentrations were high in the EQ region, low in the NP region (especially nitrate was almost depleted throughout this region), and slightly increased again in the south of the KR region on 22 March. In detail, concentrations in the south of the KR region (nitrate: ~0.30, phosphate: ~0.07, ammonium: ~0.06 μmol L^{-1}) were higher than those in the NP region (nitrate: ~0.03, phosphate: ~0.05, ammonium: ~0.03 μmol L^{-1}). " (Page 7, Lines 30-33).

Figure 1: Color on trajectories show air parcel altitude pressure (not altitude).

We corrected as suggested in the revised manuscript.

Figure 2: Very few data points for both ozone and CO before 3/12. Is that due to instrument issues or to pollution from the ship exhaust (wind out of the clean air sector)? If the latter, how did this pollution impact the representativeness of bioaerosol results presented in the manuscript? I'd appreciate a table showing the daily hours of operation of each instrument during the campaign to better appreciate the temporal representativeness of the samples.

The scarce ozone and CO data before 12 Mar were due to a data screening criterion to remove hourly ozone data with standard deviations of 1-min data exceeding 10% of the average. Though NO titration was minor, the low-level ozone data (~10 ppb) were deleted because of natural variability with 1-min data in a range of 1 ppb (corresponding to 10%). We found this criterion too strict and will loosen it slightly in future studies. Thus, the issue was only for O_3 (and CO, where the same criterion was applied) and therefore bioaerosol data during this period were not removed similarly to O3. A new table (Table 1) will include information on temporal representativeness.

Figure 4c: please make it clear in the caption that the y-axis is different for type C. I initially got confused.

We corrected as suggested in the revised manuscript.

Figure 6: please add the different regions on the map (Figure 1).

We corrected as suggested in the revised manuscript.

Figures 6-7: please add error bars!

We corrected as suggested in the revised manuscript.

Table S1: please clarify what "Zone" refers to. This should be added in the caption.

We corrected as suggested in the revised manuscript.

Other changes:

1) We excluded all observation data from 12:23 on 07 March 2019 12:23 to 00:10 on 09 March 2019, due to the Exclusive Economic Zone (EEZ).
2) We changed the colors in the markers in Fig.7, Fig.8, Fig.S2, and Fig.S4 for clarity.

Reference:

Bourgeois, I., Peischl, J., Thompson, C. R., Aikin, K. C., Campos, T., Clark, H., Commane, R., Daube, B., Diskin, G. W., Elkins, J. W., Gao, R.-S., Gaudel, A., Hintsa, E. J., Johnson,

B. J., Kivi, R., McKain, K., Moore, F. L., Parrish, D. D., Querel, R., Ray, E., Sánchez, R., Sweeney, C., Tarasick, D. W., Thompson, A. M., Thouret, V., Witte, J. C., Wofsy, S. C., and Ryerson, T. B.: Global-scale distribution of ozone in the remote troposphere from the ATom and HIPPO airborne field missions, Atmospheric Chem. Phys., 20, 10611–10635, https://doi.org/10.5194/acp-20-10611-2020, 2020.