The authors sincerely thank both reviewers for their comments on our paper. Below are our responses to each concern.

Reviewer #1

General comments

Unfortunately, the article has one flaw, which is the use of year 2000 meteorology in the model, instead of the years of each campaign. I would thus strongly suggest torerun the model with the appropriate meteorologies in order to strengthen the results.
However, in case this isn't feasible, I would still recommend this paper for publication, as the data and insight gained through their new measurement methodology is still highly valuable and worth publishing. Fortunately, even with the "wrong" model year, the overlap of in situ measurements with the model results is highly remarkable.

We appreciate this comment, which has been made by both reviewers. We agree that simulations that match the meteorological year of the measurements would be the ideal. Per this comment we explored the inclusion of this data and rerun of the simulations. We regret that it became evident that the workload involved in such simulations was beyond the current capabilities of our groups. Given that the major focus of the work was the comparison of the experimental records with a comparison to simulations we do not believe it is strictly required (per the reviewer comments). We have therefore added the following text at the end of Section 2.5:"Here we focus on the model's ability to capture the vertical profile and the average concentration, features which we do not expect to exhibit substantial interannual variability." and on page 7, "A more detailed investigation matching the meteorological year and the size range measured could help provide quantitative insights into the deficiencies in the model description of bioaerosol emissions, removal and vertical transport by convection, and we suggest this is as possible future work."

Because of the value of this data for the community, I would also suggest to the authors to publish their data openly as a supplement, as it could be re-used to improve climate models that work with bioaerosols.

After the publication of the paper data will be shared on Harvard Dataverse, similar to our previous paper, Zawadowicz et al. (2017). Reviewer #2 has also noted that our Data Availability statement was missing from the paper. This has now been added, as follows, "Data used to generate the figures are included in a Harvard Dataverse dataset with the same name as this paper (*citation to follow*)."

Specific comments

p. 2 line 29: Citation for WIBS instrument lacking. Add link to http://www.dropletmeasurement.com/wideband-integrated-bioaerosol-sensor-wibs-neo, not just to articles using it in their research. Alternatively, you can add the link to page 3, line 30, where you mention which company is producing WIBS.

Link to DMT has been added to page 3, line 30.

p. 3 line 12: Maybe add a reference to the 2015 mountaintop study you're mentioning.

As of now, the FIN-03 overview paper is still in preparation.

p. 6 line 16: It is a pity, that the simulations run with GLOMAP were performed for the year 2000 and haven't used initial data from the years the sampling took place. Is there a reason why you did this? If so, please explain. Using the wrong year might lead to uncertainties and weaken the strength of your result. If possible, I would strongly advise for rerunning the model to match the specific year of each campaign.

Please see the comments above.

p. 6 line 25: Please explain in a bit more detail how the seasons affect the mixing ratios of bioaerosol with silicates. Especially in the MACPEX campaign the ratio is rather high. Why?

Because data from several different campaigns were used, sampling locations as well as seasons have effect on bioaerosol variability and mixing state. In this case, the strong influence of silicates in MACPEX is probably driven by sampling location. Those flights were carried out over Texas and were aimed at dust effects on ice nucleation. We revised this to say, "Depending on the campaign, 30% - 82% of all bioaerosols also had silicate features (Table 1). The MACPEX campaign had a very high proportion of particles with silicate features (82%), likely owing to flights over Texas."

p. 7 lines 9 and 10: It is very unfortunate, that the model is driven by year 2000 meteorology and wasn't rerun for the years of each campaign. Convective systems are one of key factors for transporting bioaerosol into higher atmosphere levels where they can act as ice nuclei and thus influence precipitation patterns.

Please see the comments above. We included this statement in our discussion, "A more detailed investigation matching the meteorological year and the size range measured could help provide quantitative insights into the deficiencies in the model description of bioaerosol emissions, removal and vertical transport by convection, and we suggest this is as possible future work."

Style and orthography comments

Overall, the manuscript would benefit from another read-through in order to improve the flow of language and correct typographical errors, like missing punctuation. This is however not a deal-breaker, as the science is sound and the findings important.

We thank the reviewer for catching our errors. Overall language editing has now been done. Please refer to the "Track changes" version for the exact nature of the corrections.

p. 2 line 5: replace "it is" with "they are"

Fixed.

p. 2 line 8: there is a full stop missing between "et al., 2012)" and "In order to evaluate. . .".

Fixed.

p. 2 line 8: instead of "spatially, temporally" write "spatial, temporal"

Fixed.

p. 2 line 12: Bioaerosol are usually treated as a plural noun. Therefore, one should write: “Bioaerosol originate from. . .” instead of “. . .originates from. . .”.

Fixed.

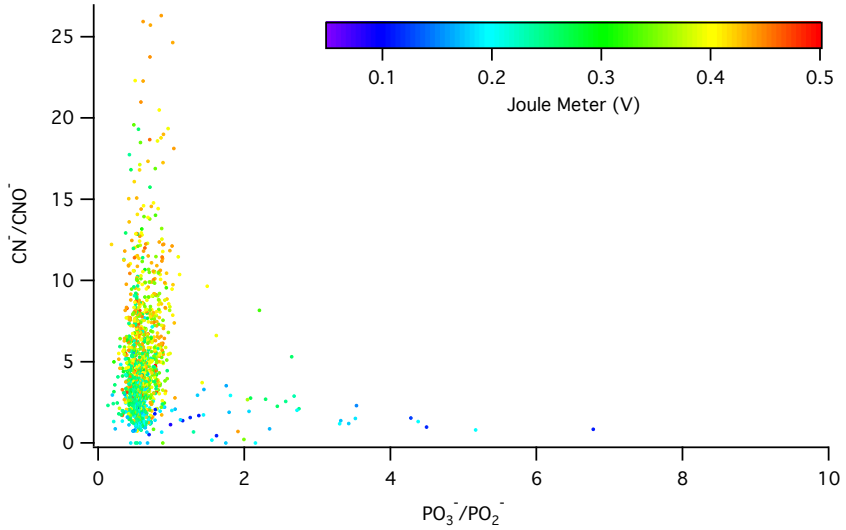
p. 15 Table 1: space between “

Spaces were inserted in the Table column headings, as appropriate.

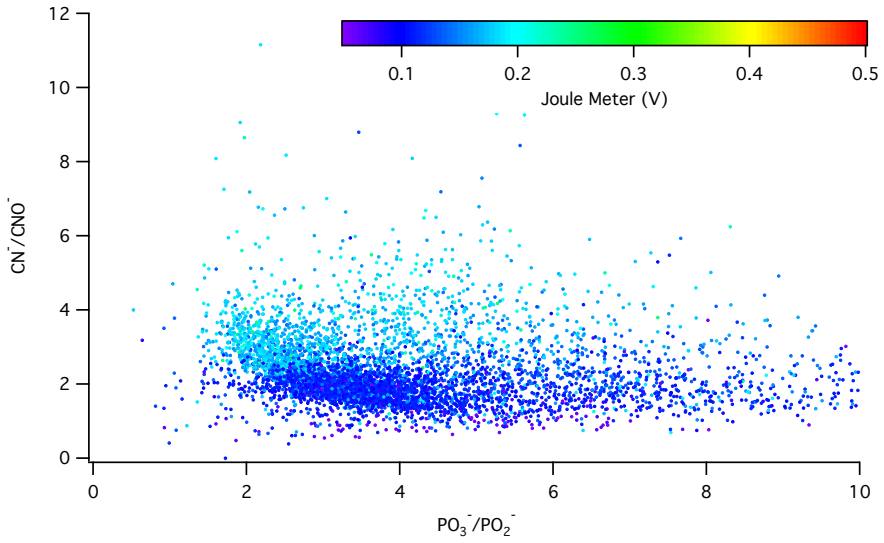
Reviewer #2

In many instances of applying classification routines to historical data, the ’drift’ or variable instrument performance can significantly affect performance. Can the authors comment on effective mitigation strategies for this or is this implicitly accounted for in fitting the SVM?

One of the reasons for using mass spectral peak ratios, instead the signal of spectral peaks in the bioaerosol identification techqniue described in our previous paper, Zawadowicz et al. (2017), is better insulation against instrumental drifts. Single particle mass spectra tend to be affected by drifts in excimer laser power in particular. Variations in laser power don’t shift distributions of CN^-/CNO^- and PO_3^-/PO_2^- ratios enough to make them indistinguishable using the SVM techqniue, as shown below, using previously unpublished data.



Apatite particles ionized using different laser powers.



Bioaerosol (*Aspergillus* cellulase) particles ionized using different laser powers.

While these two particle populations were sampled using different laser power ranges, we hope this demonstates that SVM is valid for a range of laser powers.

We added the following note the the Methods section, “Those spectra were plotted in a CN^-/CNO^- vs. PO_3^-/PO_2^- space, according to relative abundances of phosphate and organic nitrate ions. Peak ratios are used instead of absolute peak signals to better account for any instrumental drifts.”

Why is the year 2000 simulated in GLOMAP? Is there much variability in vertical profiles in the model. If not, is this realistic? I find this to be an important feature that needs expanding on in the paper before publication given the title. I would request the model is re-run.

Please see our comments to Reviewer #1.

Im not sure how a variable baseline fluorescence in the WIBS might change results?

The following explanation was added to the Methods section, “In analysis of WIBS data, it is necessary to define the threshold above which a particle is considered fluorescent in a given channel. Here the instrument was run in "Forced trigger" mode (lamps flash in the absence of particles) once daily and a particle is considered to be fluorescent in a given channel if the resulting signal is more than three standard deviations above the mean of the corresponding forced trigger signal. Day to day variations in the calculated fluorescent threshold were minimal.”

Data and code availability. The authors need to add a section on data and model avail- ability following the copernicus guidelines before publicatio: https://www.atmospheric- chemistry-and-physics.net/about/data_policy.html

Please see our comments to Reviewer #1