

Dear editor and referees,

Thank you very much for this round of reviews; we appreciate the commentary and acknowledgements on the manuscript, and are happy to resubmit our revised manuscript. Please see our responses below, and our revised manuscript (with a LaTeX-diff file, relative to the previous revision/version 2, also provided).

We have addressed each comment below with our responses in black and the reviewers' original comments in *grey italics*. We refer to line numbers in the revised manuscript (L__) where changes were made in response to the comments, and include changes (blue text for reformulations/additions in the revised manuscript, and red text for the relevant text in the original manuscript).

Sincerely,

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On behalf of all coauthors: Jinglan Fu, Gabriel Pereira Freitas, Ian Crawford, Pavla Dagsson-Waldhauserová, Radovan Krejci, Yutaka Tobo, Karl Espen Yttri, and Paul Zieger

Response to RC3

1) I only recommend that the authors fix a few remaining typographical/formatting issues, such as “succesful,” “warm-activing INPs,” missing spacing in “particle’s p_{pol},” “outputs also allows,” “This high of a dust fraction,” and “implicating morphology information.”

We are happy to hear the referee’s thoughts and review, and are thankful for pointing out the typos and language issues. In addition to the typo and language edits made in response to Referee 4 (see *Response to RC4*), we have made edits according to the above fixes, including:

- L234: Fixed typo: “succesful” → “successful”
- L471: Fixed typo: “warm-activing” → “warm-activating”
- L395: Fixed language: “outputs” → “output”
- L499: Changed: “This high of a ...” → “This high a ...”
- L644: Changed word for easier reading: “implicating” → “incorporating”

We have confirmed there is a space in place in “particle’s p_{pol} ” in L392, which may be difficult to distinguish with LaTeX typesetting; because this may change in the final typeset, we have not made any changes.

The code availability statement should also provide a specific repository or archived version rather than only the general GitHub organization page.

Following changes to figures and analyses in the latest revisions, the code has now been finalized for upload and publication via Zenodo, giving them citable DOIs. The exact repositories are now given and cited in the code availability statement (L648-652):

“The code used to reproduce these results and the classifier algorithm can be found at <https://github.com/SU-air>, and the data produced in this study are hosted at the Bolin Centre for Climate Research’s database at <https://bolin.su.se/data/jonsson-2026-aerosol-mbs> for processed data and <https://bolin.su.se/data/jonsson-2026-aerosol-mbs-raw> for raw (Jönsson et al., 2026a, b).”

→ “Code for reproducing the analysis and figures can be found on GitHub (<https://github.com/aidenrobert/MBS-source-characterization>; Jönsson, 2026a) and for the classification algorithm (<https://github.com/aidenrobert/truffle>; Jönsson, 2026b), and the data produced in this study are hosted at the Bolin Centre for Climate Research’s database at <https://bolin.su.se/data/jonsson-2026-aerosol-mbs> for processed data and <https://bolin.su.se/data/jonsson-2026-aerosol-mbs-raw> for raw (Jönsson et al., 2026a and Jönsson et al., 2026b, respectively).”

Response to RC4

1. We highly recommend including a summary detailing how the different particle types (pollen, bacteria, PE plastic, PSL, ship pollution, dust, and sea spray aerosol) distinguish from each other. This could be placed at the end of Section 3.2 or at the beginning of Section 3.3. Such a summary would help readers grasp the key takeaways and improve the readability of Section 3.3.

We agree that a concise summary of the results of the comparison would be useful for guiding the rest of the study (ML and algorithm construction, and application to field data) by reminding the reader of how differently the particles look when seen by the MBS. We feel that this would fit best at the end of Section 3.3; please see the revised manuscript's lines L361-373:

"To summarize the comparisons described here and illustrated in Fig. 4, common coarse-mode aerosol sources can produce particles with distinct, consistent sets of features when measured by the MBS. Among biological particles, bacteria displayed prominent tryptophan-like early channel-heavy fluorescence spectra; while fungal spores shared early channel-skewed fluorescence spectra, they exhibited stronger fluorescence in later channels and variable peak channels. Pollen fragments and pure cellulose had broader fluorescence spectra easily distinguishable from bacteria. The fluorescence spectra of freshly emitted combustion and microplastic particles shared similarities with those of all biological particles measured; although microplastic particles fluoresced at similar intensities to most biological particles, the combustion particles fluoresced more strongly despite their small sizes. Morphologically, bacteria stood out from fungal spores with smaller sizes and weaker scattering signals with stronger peaks. While this can help distinguish between them, combustion particle scattering signals resembled those of bacteria. Dry-nebulized pollen fragments were generally larger and had scattering signals indicative of rougher surfaces and higher asymmetry than those of wet-nebulized pollen fragments. Among non-biological particles, the lab-nebulized dust and SSA particles analyzed here had distinctly different optical morphologies, with SSA particles imparting strong diffraction peaks indicative of crisp, sharp edges and dust particles having stronger, non-peaked scattering signals indicative of rough surfaces."

2. Line 667-681 and reviewer #1 comment Nr. 1: In short, it means the method proposed in this study does not outperform the existing ones and still need to be improved. In addition, our question is: are you comparing with ground truth? Does it valid to evaluate the performance of your new method in comparison with the field study which may not be the ground truth? We think the strength of this study can be seen more from Figure 6 but not Figure 7. This should be highlighted more in your conclusion part.

As stated in our discussion, no tracer is perfect for type-specific aerosol identification due to overlap in e.g. chemical composition between types, instrumental uncertainties, and different temporal resolutions. Other methods like single-particle sample analysis using SEM/TEM, flow cytometry, and microscope evaluation have limitations like low counting statistics and also suffer from limited temporal resolution. It is therefore very difficult to establish a "ground truth" as all methods of observation come with their uncertainties, especially in our case where only chemical tracers and not biological particle number concentrations were measured in parallel using other methods. However, the analysis of complementary evidence supporting the time series of the type of PBAP established in Freitas et al. (2023) was thorough and vetted their PBAP identification methods. For this

reason, we used it as a baseline test of a different classification method that has been useful in prior work, which helped expose certain strengths and weaknesses while opening further questions about PBAP number concentrations (i.e., to what extent did the DT method used in Freitas et al., 2023 exclude PBAPs of other types). We agree that this is a key point of our study that warrants further highlighting. While we have discussed that it is impossible to ascertain what “ground truth” is in our assessment scenario prior to the conclusions, we have revised the manuscript’s Conclusions to reiterate this in the following lines (L632-642):

“It is unlikely that any classification scheme or instrument configuration will fully eliminate the influence of interferents, highlighting the need for parallel observations of e.g., black/brown carbon and source-specific chemical tracers for assessing fPBAP quantifications. Despite site-specific variability in aerobiomes and fluorescence signatures, the framework provides a flexible and expandable basis for estimating PBAP concentrations across environments and reanalyzing existing MBS datasets. However, additional training data are clearly needed to improve both interferent and specific PBAP class identification before it may be suitable for use as a standalone tool.”

→ “It is unlikely that any UV-LIF instrument configuration or classification scheme will fully eliminate non-PBAP interference, highlighting the need for parallel observations of e.g., black/brown carbon and source-specific chemical tracers for assessing fPBAP quantifications. However, it is also unlikely that any one method can establish “ground truth” PBAP tracer and number concentration data to compare with due to e.g., source overlap in chemical composition and limits to counting statistics. Despite site-specific variability in aerobiomes and fluorescence signatures, the framework provides a flexible and expandable basis for estimating PBAP concentrations across environments and reanalyzing existing MBS datasets. While our comparison of laboratory characterization data shows that MBS measurements of different particle types have distinct properties readily learnable by ML methods, additional training data are clearly needed to improve both interferent and specific PBAP class identification before it may be suitable for use as a standalone tool.”

Minor comments:

Line 5: no comma after ‘accurate’. The line number here and hereafter refers to the revised manuscript.

Although the form was originally grammatically correct, we have removed the comma so that “accurate type-specific identification” is maintained as one category (L4 in the revised manuscript).

Line 6-8: a very long phrase, reads awkward

We agree that it was awkward, and have rearranged the sentence for easier reading (L5-8):

“Using laboratory-based single-particle ultraviolet light-induced fluorescence spectroscopy and morphology measurements made with a Multiparameter Bioaerosol Spectrometer (MBS), we provide a new reference dataset for coarse-mode aerosols from common sources, including pollen, dust, bacteria, and microplastics.”

→ “We provide a new reference dataset for the fluorescence spectra and morphology characteristics of coarse-mode aerosols from common sources, including pollen, dust, bacteria, and microplastics, measured in controlled experiments with a Multiparameter Bioaerosol Spectrometer (MBS).”

Line 9: 'Including published datasets' as the sentence subject sounds awkward and implicit. Better rephrase it.

We have now changed "Including" → "Comparisons with" (L8) in the revised manuscript for clarity.

Line 13: what are 'there confounding factors'? Please be specific because it is at the beginning of a paragraph.

We have changed "factors" → "similarities" to connect this beginning with the sentence at the end of the previous paragraph, specifying that these are what we refer to (L12).

Line 32: should be -15°C but not -20°C

Changed as suggested (L26).

Line 38-40: A non-negligible point is that PABPs nucleate ice at warm temperature where secondary ice production is effective and can multiply ice crystal numbers by orders of magnitude above the primary ice levels initiated by ice nucleating particles.

This is a very good point; we thank the reviewer for suggesting this motivation be explicitly stated and have revised the sentence to highlight this (L32-34) with a reference added to provide context on the relevant temperature ranges of SIP processes ([Field et al., 2017](#), [Zhao and Liu, 2021](#)):

"Although PBAPs are far less abundant and highly variable, typically between 10^{-3} - 10^1 L⁻¹ (Després et al., 2012), even low concentrations may substantially affect mixed-phase cloud properties due to highly efficient glaciation feedbacks (Huang et al., 2021)."

→ "Although PBAPs are far less abundant and their number concentrations are highly variable, typically between 10^{-3} - 10^1 L⁻¹ (Després et al., 2012), even low concentrations may substantially affect mixed-phase cloud properties due to highly efficient glaciation feedbacks at temperatures >-15° C like the Hallett-Mossop process (Field et al., 2017; Zhao and Liu, 2021)."

Line 44: what are 'these instruments' ?

"These" refers to the instruments listed in the previous instruments. We have now changed "these" → "UV-LIF" (L56) to specify that the subsequent design detail is often implemented in instruments using UV-LIF spectroscopy.

Line 47: <50% or <<50%? Please be more specific and clearer to support your statement saying 'very low'

The detection efficiencies reported in the reference given are very low compared to single-particle instruments, and are size-dependent. Even at optimal sizes for single-particle AMS (such as the PALMS-NG, ~5 µm), detection efficiencies do not surpass 50% and fall off exponentially with decreasing size (Froyd et al., 2019). This is far below typical particle measurement rate ceilings for UV-LIF (such as the MBS, which is >50% for particles >0.5 µm, and the WIBS is even better at this size range). It is therefore not feasible to use such a technique to detect PBAPs, which typically exist in low fractions of the total aerosol and would thus be missed by instruments not able to analyze a sufficient sample size. In response to this comment we have now reformulated the sentence to remove the relative statement of "very low" and added a further reference on the technique (L38-41):

“Although single-particle mass spectrometers can offer valuable information, they typically have very low ($\ll 50\%$) detection efficiencies (Jacquot et al., 2024) and are costly and difficult to miniaturize, making their employment quite rare (Lai et al., 2025).”

→ “Although single-particle mass spectrometers can offer valuable information about aerosol type, they typically have limited, strongly size-dependent detection efficiencies (Froyd et al., 2019; Jacquot et al., 2024) and are costly and difficult to miniaturize, making their employment quite rare (Lai et al., 2025).”

The statement that the detection efficiencies are “strongly” size-dependent is backed up by and paraphrased from the reference (Froyd et al., 2019).

Line 42-48: Super long sentence and the reading flow is not concise

This comment refers to two sentences in the manuscript at the lines specified in the difference file, which we have not broken up further. However, we have now shortened the first sentence (L36-L38) to help the flow:

“Online aerosol mass spectrometry offers higher temporal resolution for chemical composition; these instruments are designed to determine bulk aerosol composition in only a narrow size range (Nash et al., 2006; Pratt and Prather, 2012) or at single-particle resolution.”

→ “Online aerosol mass spectrometers offer higher temporal resolution for chemical composition, but typically only determine bulk composition in a narrow size range (Nash et al., 2006; Pratt and Prather, 2012) or at single-particle resolution.”

Line 69-70: please check the language

We have determined that the language of this sentence (“This property has been used to detect airborne and marine microplastics in the environment...”) is correct.

Line 72: are all PAHs fluorescent?

PAHs are very often fluorescent due to their defining benzene ring, which is made exclusively of C-C bonds that are a common, strong source of fluorescence. PAHs with non-benzene ring structures exhibit reduced fluorescence when they are made up of non-C-C bonds. Oxidizing PAHs would reduce their fluorescence but remove their status, strictly speaking, as PAHs. We have revised the statement to give the caveat that many are fluorescent rather than drawing a broad generalization (L56):

“Many polycyclic aromatic hydrocarbons (PAHs) are highly fluorescent...”

Line 85: A possessive tone in scientific writing should be avoided. Please also make corrections through the manuscript. There are many.

Although we recognize that possessive tone is avoided in certain strict scientific writing styles, we do not find this to be a universal rule and prefer to keep our tone as-is. On one hand, we understand that possessive and first-person tone can intrude on the objective value of scientific literature, but on the other hand, humans are the ones who perform scientific studies. Recognizing that humans performed the experiments and analyses helps to remind the reader that human flaws are always possible in the work. There are also no clauses regulating this in Copernicus publication guidelines.

Line 95: ‘adjustable fixed’ sounds awkward

We agree and have reformulated the first part of the sentence (L85-86) for clarity:

“The MBS operates at an adjustable fixed instrument flow rate , ... “

→ “The MBS operates at a fixed instrument flow rate set by the user, ... “

Line 96: what is the frequency used in this study? And at which frequency, the instrument will be saturated? This will determine the upper limit of total particle number concentration that the MBS can handle.

The frequency is set by the rate of coincidence in the measurement stage, i.e. the frequency that particles enter while another particle is being measured and are thus not able to be measured. This depends on flow rate, which may vary as it can be set by the user, and the triggering sequence, which is fixed by instrumental design and electronics. With typical settings, this is roughly 160 particles /s, as stated in L102-104 with reference to Freitas et al. (2022) where it is also stated. In laboratory characterization experiments where aerosols are generated, sample particle concentrations may be high enough to cause particle arrival frequencies in excess of this, but it is rarely exceeded in ambient atmospheric observations except in very polluted urban environments.

Line 97: Is 0.165 the fraction value? Then move the number right after the fraction but not keep it after total flow.

The fraction has now been moved as suggested (L87).

Line 100: what is the size range of the MBS? Like the lower limit because of using Mie scattering technique?

The size range of the MBS is approximately 0.5-20 μm , which is indeed limited by Mie theory, since intensities at the lower range increase sharply with diameter below this, yielding a near-vertical/infinite slope. We thank the reviewer for asking for this clarification, and it has now been specified with an added sentence in the revised manuscript at L91:

“With its optical configuration, the effective particle size range for measurement with the MBS is 0.5 to $\sim 20 \mu\text{m}$.”

Line 116: better use ‘Data processing’?

We believe that “Data treatment” is as valid as the suggestion and therefore have chosen not to revise the section title.

Line 214: to train

Logistic functions are fitted to training data; we believe that this is correctly formulated and have therefore not made changes in the revision.

Line 283 & 286: intact pollen grains

Fixed as suggested (L267 and L270).

Line 294-295: we do not agree. The broader spectra of wet generated samples than those of dry generated ones is pronounced for willow but not for all, for instance alder. One distinction we could tell is also that some wet samples show stronger signal intensity than dry counterparts, including ash, pine and willow.

We agree that the fluorescence spectral shift behavior with wetting was not as consistent as the sentence reflected, and that it would benefit the manuscript to be more specific. We have therefore revised the discussion around these findings (L276-283) with more specificity:

“Most dry pollen fragments peak in the C channel (~414 nm), with ash, juniper, and pine having slightly broader spectra. Wet nebulization consistently broadens the emission spectra of all pollen fragments and shifts maxima to channel D (~461 nm). In general, dry pollen fragment fluorescence is dominated by fluorophores on the grain exterior, composed mainly of sporopollenin (Zimmermann, 2010), whereas pollen fragments can include interior fluorophores, such as phenols, carotenoids, azulene, and anthocyanin, all with different emission characteristics (Fennelly et al., 2018). Wet nebulization produced more numerous, smaller particles than dry nebulization, indicating a different emission mode, likely including soluble material re-formed as residue after drying.”

→ “Most dry pollen fragments peak in the C channel (~414 nm), with ash, juniper, and pine having slightly broader spectra (Fig. 1o-u). Wet nebulization consistently broadens the emission spectra of pollen fragments for willow pollen (Fig. 1u) and shifts fluorescence peaks to channel D (~461 nm) for birch, hazel, juniper, pine, and willow pollen (Fig. 1q-u). In general, dry pollen fragment fluorescence is dominated by fluorophores on the grain exterior, composed mainly of sporopollenin (Zimmerman, 2010), whereas pollen fragments can include interior fluorophores, such as phenols, carotenoids, azulene, and anthocyanin, all with different emission characteristics (Fennelly et al., 2018). Wet nebulization produced more numerous, smaller particles (Fig. 1h-n) than dry nebulization (Fig. 1a-g), indicating a different emission mode, likely including soluble material re-formed as residue after drying.”

Line 293-304: Should refer to figure panels when interpreting/discussing the results. Only Figure 1g to 1n were referred to support the discussions.

In the revisions following the previous comment, this paragraph now includes more specific figure references; please see the previous reply and changes.

Line 310-311: refer to relevant panels in Figure 1 about pollen fluorescence spectra

The relevant panels in Figs. 1-2 have now been referred to in the revised sentence (L292-294):

“PE spectra closely resemble those of dry pollen fragments with C channel maxima, and cellulose shows similar pollen-like fluorescence.”

→ “PE spectra (Fig. 2j-k) closely resemble those of dry pollen fragments with C channel maxima (Fig. 1o-u), and cellulose shows similar pollen-like fluorescence (Fig. 2l).”

Line 311-312: refer to relevant publications about the low number concentration of micro-plastics

We agree that references should be given here to substantiate this, and we have revised the sentence to include these, as well as specified that aerial microplastic concentrations are highly environment/location-dependent (L294-297):

“While atmospheric coarse-mode microplastic concentrations are unlikely to be high enough to strongly bias PBAP detection, these results show that synthetics can mimic the fluorescence properties of fPBAPs.”

→ “While atmospheric coarse-mode microplastic concentrations are likely only high

enough to significantly bias fPBAP detection in specific locations with strong anthropogenic activity, such as roads or urban centers (Han et al., 2024; Ahmadi et al., 2026; Evangelou et al., 2026), these results show that synthetics can mimic the fluorescence properties of fPBAPs."

Line 354: please clarify which properties and how these properties support your statement
Line 340: Is BB (PE after UV) abundant in the atmosphere? Doesn't it contradict to the statement in the Introduction? Or is it biomass burning aerosols? Better use BBA to differentiate it from BB in Figure 3. This also applies to the following text, e.g., Line 456.

Any reference to this panel would be referenced to in the text as Fig. 3bb. Because "BB" was defined as biomass burning in L252, we do not believe that this warrants a change in the manuscript. The figure labeling was changed to comply with the Copernicus typesetting guidelines, so that the corresponding panel in Fig. 3 is labeled as "bb", preventing any such confusion.

Line 355: delete 'also'

Fixed (L333).

Line 356-358: Figure 1 shows that wet-generated pollen show smaller sizes which may indicate fragmentations whereas Figure 4c shows that wet-generated pollen is more spherical. Does it mean pollen fragments are more spherical than intact pollen? Unfortunately, it is impossible to visualize dry- and wet-generated pollen as you responded to reviewer #1.

We would like to reiterate that both dry and wet pollen experiments performed in this study produced particles much smaller than the reported intact pollen grain sizes, which mainly lie outside of the MBS's size range (L265-275). It is therefore impossible to say whether these fragments are more or less spherical than intact grains using data from MBS measurements; therefore, we do not make any claims comparing them in the manuscript. Comparing particles of similar size ranges between wet and dry pollen experiments, wetted pollen fragments consistently showed more droplet-like morphology characteristics, such as the low asymmetries (Fig. 4c). This has now been clarified in the discussion on this figure comparing wet and dry pollen fragment morphologies (L338-340):

"Consistent with the discussion in Section 3.1, wet nebulized pollen fragments show more spherical morphologies, indicating that many particles are dried residues of dissolved constituents."

→ "Consistent with the discussion in Section 3.1, wet nebulized pollen fragments have more sphere-like morphologies than dry nebulized pollen (seen in e.g. their asymmetries; Fig. 4c), indicating that many particles are dried residues of dissolved constituents."

Line 393: discussion on Figure 4 not around

Changed as suggested (L387).

Line 391-395 and Figure 5: does it mean you discard particles <2.5 μm and showing fluorescence > 9σ? How will this treatment influence the later comparison with DT method by Freitas et al. (2022)?

This does not mean that particles <2.5 μm are discarded in the PBAP identification branch of the algorithm. The sentence referred to has now been reformulated to avoid any confusion (L385-387; see below). We do not think that Fig. 5 conveys this, as the 2.5 μm filter appears

only in the lower branch of the flow chart. We have therefore not made any changes to the figure.

“In our algorithm, particles are first filtered according to fluorescence; highly fluorescent ($\geq 9\sigma$) particles are passed to the pollution and fPBAP identification branch, while the remaining particles $\geq 2.5 \mu\text{m}$ are passed to the dust/SSA branch.”

→ “In our algorithm, particles are first filtered according to fluorescence; highly fluorescent ($\geq 9\sigma$) particles of all sizes are passed to the pollution and fPBAP identification branch, while the remaining particles with sizes $\geq 2.5 \mu\text{m}$ are passed to the dust/SSA branch.”

Line 379-426: may be better moved to Methodology part.

Although this was considered in the original preparation and previous round of revisions, we decided to keep the algorithm construction in this section because it is informed by results from the characterization experiments and comparisons between particles. For instance, we elected to apply size filters for the non-fluorescent coarse particles, for reasons described in the discussion on Fig. 4. Because these would be difficult to refer to in the methods, we prefer to keep the arrangement as is, and describe the computational tools used for the classification tasks in the Methods section.

Line 461-464: not clear how do you choose the sample periods with sufficient tracer data to label HFPs. Please clarify this in the SI S1 (e.g., Line 64-69).

“Sufficient” tracer data here means overlapping biological and pollution tracer sampling periods; we thank the reviewer for pointing out the potential confusion here. To address this, we have now reformulated and split up this sentence for clarity and specificity while still referring to the SI, but did not make changes to the SI, since this is made clear there and in the revised lines. The changes can be found in L451-455:

“This is accomplished by choosing a number of sample periods with sufficient tracer data and labeling HFPs observed during these periods as either pollution or not based on the soft label, and further training the LRM on these data (the tuning block) with lower weights than those of the initial training data to limit its influence on model fit; details of this step are explained in the SI (Section S1).”

→ “This is accomplished by choosing a selection of sample periods with overlapping tracer data for both pollution and biological aerosols, attributing “soft” (i.e. continuous and non-discrete) pollution labels for HFPs observed during these periods based on tracer concentrations, and further training the LRM on these data (the tuning block) with lower weights than those of the initial training data to limit its influence on model fit. Details of this step are explained in the SI (Section S1).”

Line 675-677: please refer to results in Section 3, e.g., Fig. xx

We believe that Conclusions sections should be as concise as possible in summarizing results, reiterating the main points and not repeating any discussions on or reporting of results. We therefore have elected not to revise the manuscript according to this suggestion.

Figures and Tables:

Figure 1: Overall, it is a very nice figure. We like it. Better use different colours for $dN/d\log D$ and Central wavelength to visualize data. In addition, EGU journals, like AMT and ACP, use lower case to label figure panels. Please correct all labels, if this is the case.

We are happy to hear the recognition, and thank the reviewer for pointing out ways to improve the visualization. We have changed Fig. 1, as well as Fig. 2 where the same color map was used, as suggested. All figure panel labels have now been changed to lowercase.

Figures 5: Font size may need to be adjusted to obey egusphere publication policy. Please check.

We have adjusted the scaling of Fig. 5 in the revisions to improve the readability of the font.

Further changes

In revising the Conclusions section, we have sharpened some sentences (L620-625) by removing redundancies, i.e. references already given in the Discussion section:

“Compared to the previously used fluorescence-only approach, our algorithm yields up to two orders of magnitude higher fluorescent PBAP concentrations during summer, in agreement with previously reported concentrations of PBAPs in Ny-Ålesund during July-August (Johansen and Hafsten, 1988), while reproducing the annual PBAP cycle reported in Freitas et al. (2023a). Although the algorithm identified fungal spores as a major type of PBAP present at Zeppelin Observatory, in line with previous studies (Johansen and Hafsten, 1988), it clearly overestimated pollen fragment concentrations, which may be attributable to similarities shared with plant debris and possibly biomass burning particles.”

→ “Compared to the previously used fluorescence-only approach, our algorithm yields up to two orders of magnitude higher fluorescent PBAP concentrations during summer, in agreement with previously reported concentrations of PBAPs in Ny-Ålesund during July-August, while reproducing the annual PBAP cycle. Although the algorithm identified fungal spores as a major type of PBAP present at Zeppelin Observatory, in line with previous studies, it clearly overestimated pollen fragment concentrations, which may be attributable to similarities shared with plant debris and possibly biomass burning particles.”

The acknowledgments section was changed to attribute the contributions from Pavla Dagsson-Waldhauserová and Yutaka Tobo to specific projects. The additional funding acknowledgments can be found in L664-667 of the revised manuscript:

“Contributions by P.D.-W. were supported by the Icelandic Research Fund (Rannís) grant no. 2612250-051. Contributions by Y.T. were supported by JSPS KAKENHI (JP24H00761), Arctic Challenge for Sustainability 3 (ArCS-3, JPMXD1720251001), and the Environment Research and Technology Development Fund (JPMEERF20232001, JPMEERF20262002) of the Environmental Restoration and Conservation Agency of Japan.”

References (in order of appearance)

Freitas, Gabriel P., et al. "Emission of primary bioaerosol particles from Baltic seawater." *Environmental Science: Atmospheres* 2.5 (2022): 1170-1182. DOI: [10.1039/D2EA00047D](https://doi.org/10.1039/D2EA00047D)

Field, P. R., et al. (2017). Secondary ice production: Current state of the science and recommendations for the future. *Meteorological Monographs*, 58(1), 7-1. DOI: [10.1175/AMSMONOGRAPHS-D-16-0014.1](https://doi.org/10.1175/AMSMONOGRAPHS-D-16-0014.1)

Zhao, X., & Liu, X. (2021). Global importance of secondary ice production. *Geophysical Research Letters*, 48(11), e2021GL092581. DOI: [10.1029/2021GL092581](https://doi.org/10.1029/2021GL092581)