Replies to Referee #1

We would like to sincerely thank the Referee for their careful reading of our manuscript and for their constructive and helpful comments. Their suggestions have helped us to significantly improve the clarity, structure, and scientific depth of the paper. In the revised version, we have addressed all comments point by point. Changes in the text are clearly marked in the manuscript *with red fonts*.
Below, we provide detailed responses (*red fonts*) to each reviewer's comment (*black fonts*) and indicate where the corresponding revisions have been implemented.

General comments:

The manuscript *"Profiling pollen and biomass burning particles over Payerne, Switzerland using laser-induced fluorescence lidar and in situ techniques during the 2023 PERICLES campaign"* by Gidarakou et al. presents vertical profiles of atmospheric particles obtained with a multi-channel elastic-fluorescence lidar combined with ground-based on- and offline instrumentation, adapting Veselovskii's (2021) single-channel approach for distinguishing smoke from pollen to a multi-channel system. The authors also introduce a methodology, based on Richardson (2019), that deconvolutes LIF lidar signals and compares them with reference fluorescence spectra from a pollen database to characterize pollen types. Pollen were observed near the ground up to ~2 km, exhibiting strong fluorescence backscatter coefficients b_F (2×10^{-4} to 8×10^{-4} Mm^{-1} sr^{-1} at 355 nm) and confirmed via *in situ* fluorescence measurements and Hirst trap sampling. Biomass burning particles from Canadian and German wildfires were detected higher in the atmosphere (3–5 km) with weaker bF values. Comparison across European lidar stations revealed a ~50% reduction in b_F for smoke plumes transported in the free troposphere, which the authors suggest may result from photochemical aging and mixing with non-fluorescent particles. Ice-nucleating particle (INP) measurements near the ground showed a correlation between $WIBS_{ABC}$ aerosols and INPs at -14 °C, indicating bioaerosol contributions at warmer temperatures. No such correlation was observed at -20 °C, where the authors suggest coarse dust particles are linked to INPs.

The study presents a valuable dataset and analysis in my view. From a formal perspective, the quality of the manuscript is good; it is well-written, the figures and tables are mostly clear, and the arguments are presented clearly. The manuscript seems in line with the scope of ACP. In terms of content and format, it would profit from some minor improvements as listed below. Overall, I recommend to accept the manuscript for publication in ACP after these points have been addressed.

We sincerely thank the Referee for the constructive review and for the overall very positive assessment of our manuscript. The Referee's feedback, especially regarding the scientific relevance, clarity of the manuscript, and the robustness of the results, is deeply appreciated. We have also addressed all specific and technical comments with detailed revisions and responses, as outlined below.

Specific comments:

Structure

Comment:

The “Discussion and Conclusions” section begins by introducing an analysis of aged smoke particles, which seems inappropriate for this section. The corresponding results and figure should be moved to the “Results” section.

Reply:

We agree with the reviewer. The corresponding analysis and figure have been moved to the *Results* section, and the *Discussion and Conclusions* section has been revised accordingly.

Figures

Comment:

Clarity and accessibility of the figures could be improved. Few points here that caught my attention: (i) The rainbow color scale used in multiple graphs is quite controversial, as it may distort and mislead due to non-uniform changes, and should therefore be avoided. (ii) Fig. 2: Panel b) is rarely recognizable in the printed version, and panel c) offers little scientific relevance, as it only shows a container without technical details (e.g., inlets or instrument placement) and appears more like a group photo. I therefore suggest removing both panels, as they add no substantial information. (iii) Fig. 5 and Fig. 8: The x-axis ticks are not consistently aligned across the subplots, which is confusing and makes it unclear whether the same time stamps are shown. (iv) Fig. 6 and Fig. 9: Subplots d)–i) are slightly misaligned. Introducing uniform distances between the individual subplots would improve overall presentation. Additionally, scaling the y-axes to match those of a), b), and c) would make the graphs easier to compare.

Reply:
(i) We acknowledge this point. Unfortunately, for these specific plots, alternative color maps did not preserve the necessary contrast to distinguish signal variations. Therefore, we maintained the existing color scale but improved the colorbar normalization and annotations for better readability. (ii–iv) All issues regarding alignment, scaling, and figure clarity have been corrected in the revised manuscript. The updated figures can be found in the new version.

WIBS Data

Comment:

No thresholds are specified. Without forced-trigger measurements and appropriate correction, the data is likely overestimated.

Reply:

We have clarified this point in the revised manuscript (L169–171):

“Here, we used the averaged forced trigger signal plus 9σ to subtract background noise for calculating the fluorescent signal of sampled aerosol particles, instead of 3σ used in other studies (Savage et al., 2017), to minimize the influence from interfering particles.”

INP Analysis

Comment:
The manuscript associates –20 °C INPs with coarse-mode dust based on correlation with WIBSCoarse particles, but this could also include pollen, spores, or sea salt. The statement should be toned down, and the INP results should be better integrated into the discussion.

Reply:
We thank the reviewer for this valuable comment. We have revised the relevant text to soften the interpretation and to better integrate the INP analysis into the overall discussion. Specifically, L488–489 was corrected to:

"In addition, the INPs at –20 °C are correlated with $WIBS_{coarse}$ particles (Fig. S4c, d), showing $\rho_{Pearson} = 0.72$ ($p_{Pearson} = 0.01$) and $\rho_{Spearman} = 0.52$ ($p_{Spearman} = 0.10$), suggesting that coarse-sized particles such as soil dust and pollen contribute to the observed INPs at –20 °C."

We also added the following in L189-191: "The freezing ability of aerosol particles determines their removal processes, lifetime, and climate impacts. Therefore, it is important to evaluate the freezing and cloud interaction abilities of the observed aerosol sources."

In addition, relevant aspects of the INP discussion have been expanded in the revised manuscript.

Figure 10

Comment:
It is not clear why the pollen fluorescence spectra from 12/06 and 15/06 differ so strongly, despite both representing 100% *Dactylis* presence.

Reply:
We clarified this point in the revised text (L563): "Note that differences between the in vivo and lidar-derived spectra can result from the contribution of other fluorescing particles or other grass pollen species (*Dactylis* is used as a proxy for the grass pollen family). The absence of a tail at longer wavelengths, however, suggests a dominant contribution from grass pollen."

Technical Comments:

1. Standardise in situ throughout the manuscript.

The term *in situ* has been standardized throughout the text.

2. Rapid-E Instrumentation is mentioned but no data are provided; consider adding data or removing the mention.

References to the Rapid-E instrument have been removed, as no corresponding data are presented.

3. Use non-breaking spaces for units to avoid splitting units at line breaks.

Thank you, corrected.

4. Line 94: Add a space after "Fig. 1(d-e)."

A space was added.

5. Line 120: Clarify if "Swisensat" refers to Swisens Poleno.

Minor typographical corrections have been implemented (spaces after “Fig. 1(d–e)”, clarification that “Swisensat” refers to *Swisens Poleno*

6. Line 124: Correct “Induced light-induced fluorescence”.

Correction of “Induced light-induced fluorescence”, and single definition of LIF).

7. Lines 177–178: Define LIF only once.

Corrected.

8. Line 205ff: Define PF as the fluorescence channel.

PF has been defined as the fluorescence channel.

9. Line 290ff: The description in Fig. 4 is sufficient; consider removing repeated explanation from the main text.

Repetitive text following Fig. 4 has been shortened.

10.Line 339: Clarify “Improper calibration has been documented”; did it occur in this study or do you refer to calibration errors in the literature?

Regarding Line 339 (“Improper calibration has been documented”), this sentence now clarifies that we refer to calibration errors reported in the literature. We also added the sentence:

“Those limitations are currently still balanced by the more precise discrimination capabilities of Hirst measurements compared to the commercially available automatic instruments.” (Line 355-356)

11. Line 404: Replace “approximately” with “relatively” or “fairly”.

“Approximately” was replaced with “relatively” (Line 422).

12 Fig. 10a): Correct the continuous line legend to the dates 13/6–16/6.

The legend in Fig. 10a has been corrected to indicate the continuous line refers to 12–15 June.

13 Line 606: There might be an error in this phrase: “Our novel methodology characterizes pollen types by deconvoluting LIF lidar signals and comparing them with reference fluorescence spectra from an extensive pollen database”.

The sentence at Line 606 (now 623) has been rephrased for clarity.