Accept, after minor revision

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 b_F 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.

Specific comments:

Structure: 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, in my view.

Figures: 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.

WIBS data: No thresholds are specified. Without forced-trigger measurements and appropriate correction, the data is likely overestimated. If a threshold was applied, it should be specified (e.g., 3σ) to ensure comparability.

INP-Analysis: The manuscript strongly associates -20 °C INPs with coarse-mode dust based on correlation with $WIBS_{Coarse}$ particles. However, no direct evidence for dust is presented; $WIBS_{Coarse}$

could also include pollen, spores, or sea salt. This statement should be toned down accordingly. In addition, the observation is currently mentioned only in the abstract and results, but not in the discussion, where it should be addressed in detail. In general, the reasoning for the INP measurements and the conclusions drawn from them could and should be more thoroughly integrated into the overall argumentation, as the INP analysis otherwise appears somewhat arbitrary.

Fig. 10: It is not clear why the pollen fluorescence spectra from 12/06 and 15/06 differ so strongly, despite both representing 100% *Dactylis* presence.

Technical comments:

- Standardise *in situ* throughout the manuscript.
- Rapid-E Instrumentation is mentioned but no data are provided; consider adding data or removing the mention.
- Use non-breaking spaces for units to avoid splitting units at line breaks.
- Line 94: Add a space after “Fig. 1(d-e).”
- Line 120: Clarify if “Swisensat” refers to Swisens Poleno.
- Line 124: Correct “Induced light-induced fluorescence”.
- Lines 177–178: Define LIF only once.
- Line 205ff: Define P_F as the fluorescence channel.
- Line 290ff: The description in Fig. 4 is sufficient; consider removing repeated explanation from the main text.
- Line 339: Clarify “Improper calibration has been documented”; did it occur in this study or do you refer to calibration errors in the literature?
- Line 404: Replace “approximately” with “relatively” or “fairly”.
- Fig. 10a): Correct the continuous line legend to the dates 13/6–16/6.
- 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”.