Review: Vertical distribution, optical characterization and automated classification of airborne pollen from in-situ measurements

To the Authors

Summary: The manuscript entitled "Vertical distribution, optical characterization and automated classification of airborne pollen from in-situ measurements" presents an interesting and valuable investigation of airborne pollen using lightweight optical sensors installed at multiple heights, together with Hirst-type measurements, a Cloud Droplet Analyzer, SILAM simulations and machine-learning classification. The measurements extending from 4 to 272 m a.g.l. are useful and provide an opportunity to examine the vertical distribution of airborne pollen. It is evident that a substantial amount of experimental, analytical and methodological work has been invested in this study. The manuscript is generally well organized, the objectives are scientifically relevant and the results have the potential to make a meaningful contribution to the field of aerobiology and pollen monitoring. However, it could benefit from revision and further clarification of several methodological and interpretative aspects, which are described in detail in the comments below.

General Comments:

1) In Lines 36-52, please clarify the relationship between sampling height and transport distance. Long-range transported pollen can be mixed downward and detected at heights of 10-30 m or at canopy level. However, measurements at these heights alone cannot show whether the pollen originates from local, regional or distant sources. Rojo et al. (2019) mainly examined the effect of sampling height, while Damialis et al. (2017) used aircraft measurements over a wider altitude range. Therefore, I suggest revising the final sentence so that these studies are not presented as direct evidence of medium and long-range transport. In addition, in Line 38, Sofiev et al. (2013) is a valid reference and discusses long-range transport, but it is mainly a description of a birch-pollen emission module. It may not directly support the precise statement that transport over 100-1000 km becomes the dominant contribution before local flowering begins. Please provide more direct references for this statement or modify that sentence.
2) The abstract states that the optical pollen sensors showed good agreement with the CDA measurements ($R^2 \geq 0.91$). However, the reported R^2 values appear to describe the comparison among the three PS2 sensors rather than a PS2-CDA regression. Could the authors please clarify this point? If a PS2-CDA regression was performed, it would be helpful to report the corresponding statistics. Otherwise, please revise abstract and conclusions accordingly.
3) The correction of $PS2_2$ and $PS2_3$ requires clarification in Lines 264-268. The original slopes are reported as 0.93 and 0.76, while the manuscript states that the data were corrected by multiplying them by the respective slope factors. If the dependent sensor has a slope below one relative to $PS2_1$, multiplication by that slope would normally increase the disagreement rather than correct it. Please provide the exact correction equations and clarify which variable was treated as dependent.

4) Figure 3 shows that birch and pine pollen have different optical signatures and may therefore provide a basis for identifying pollen at higher levels. However, the procedure used in the vertical profile analysis is not fully described. Could the authors please clarify whether the PS2 measurements at 115 and 272 m were identified as birch by comparing their Ppol and DoP values with the laboratory clusters shown in Figure 3 or whether they were considered birch mainly because the Burkard measurements at 4 m showed a birch dominated period? If the laboratory values were used, please describe the classification criteria or exact boundaries applied.
5) In Line 220, please explain the basis for the threshold of 142 particles m^{-3} used to filter the PS2 measurements.
6) The Random Forest approach appears suitable for this classification task. However, the validation method may require some clarification. The stratified shuffle-split procedure preserves the proportions of the classes, but the 2 h observations are still divided randomly. As a result, neighboring observations from the same pollen episode may be included in both the training and testing datasets, which could lead to an optimistic estimate of model performance. Could the authors please clarify whether the temporal structure of the data was considered during validation? A temporally separated validation, for example using different years for training and testing, could provide a more independent assessment of the model.
7) Some references may not directly support the statements with which they are associated. For example, Schneider et al. (2015) is a diagnostic study of patients with suspected asthma and does not appear to support the reported prevalence of allergic diseases in industrialized areas. Kuparinen et al. (2009) focuses on the influence of temperature and atmospheric conditions on wind driven plant dispersal, rather than on changes in the water content of individual pollen grains. In addition, the cited Oteros et al. (2019) paper concerns the spatial interpolation of pollen concentrations and may not be the intended reference for the description of the BAA500 instrument. Cariñanos et al. (2000) compares two methods for counting pollen on Hirst-trap slides and does not appear to support the statement regarding the complementary use of the PS2 and Hirst sampler. Finally, Sofiev (2019) discusses pollen data assimilation and the adjustment of seasonal emissions, but it does not directly demonstrate a general tendency of SILAM to overestimate pollen concentrations. Could the authors please check these references or/and either provide more appropriate citations?

Specific Comments:

1) “Most PBAP cause allergic diseases” is too general in Line 18. Many primary biological aerosol particles are not allergenic. Please refer specifically to allergenic pollen and fungal spores.
2) Please explain how the PS2 thresholds Ppol>1.3 and -0.1<DoP<0.5 were determined and report their sensitivity (Lines 152-153).
3) Figure 5 and the caption use inconsistent periods: 1 May-1 July versus 1 May-31 July.
4) Please correct “3d” to “3-D” (Line 179), “versions 5.9” to “version 5.9” (Line 185) and the typographical phrase “analysis of the birch and IPPs” (Lines 277-279).

5) Throughout the manuscript figures, the terminology should be revised to “pollen grains/m^3” rather than “m^3”, when referring to units based on the paper of Galán et al. (2017) [1].

Bibliography:

1. Galán, C.; Ariatti, A.; Bonini, M.; Clot, B.; Crouzy, B.; Dahl, A.; Fernandez-González, D.; Frenguelli, G.; Gehrig, R.; Isard, S.; et al. Recommended Terminology for Aerobiological Studies. *Aerobiologia (Bologna).* **2017**, *33*, 293–295, doi:10.1007/S10453-017-9496-0/METRICS.