The manuscript presents a comprehensive, state-of-the-art study on pollen in the atmosphere (emission, vertical layering, regional transport) based on surface observation, lidar remote sensing and advanced modelling. The work includes even a detailed study on uncertainties and error sources. The paper is well written, but a bit long. Short, compact paper attract readers, too long papers often cause the opposite.

REPLY: Thank you very much. We greatly appreciate the reviewer positive feedback. We have made a general effort to shorten the paper. In addition, Table 5 and Fig. 11 were removed. Fig. 16 was removed as well as the subsequent discussion (last paragraph before the conclusions). Appendix A and B were moved to a supplementary material. Overall the paper was reduced from 48 pages (original) to 35 (revision) pages.

Minor revisions may further improve the paper.

Some detailed comments:

P3, l3: Please have also a look into the recent paper of Veselovskii, AMT, 2021 (fluorescence lidra, focus on pollen) and also Saito, Rem. Sens., 2018. Could be included in the introduction.

I miss a bit a discussion on: How did the papers of Bohlmann (ACP 2019) and Shang (ACP 2020) contributed to the field, and even improved the knowledge about pollen and lidar measurement approaches (after the pioneering papers of Noh 2013 and Sicard 2016). And what about Veselovskii, AMT 2021? I miss something like a small review (on progress) in the field of pollen and lidar applications by the expert.

REPLY: Bohlmann et al., 2021; Veselovskii et al., 2020; 2021 and Saito et al., 2018 have been duly added in the introduction. A sentence presenting the method developed by Shang et al. (2020) to retrieve the depolarization ratio of pure pollen (or pure pollen mixture) has been added. The recent findings of Veselovskii et al. (2020; 2021) measuring fluorescence backscattering and fluorescence capacity with broadband interference filters are now mentioned in the revised manuscript.

P3, l12-13: My request was triggered by the final, not very specific sentence of the paragraph.

REPLY: In this last sentence we were referring to the ongoing research made by the authors of this article, and not to the scientific community in general. The last sentence has been reformulated as follows: “The present journal paper is the apogee of the knowledge presented in the latter three conference proceedings and acquired throughout a continuous effort since 2016.”.

P4: Sect. 2.2: Shang mentioned pollen lidar ratios of 65 sr.

Is your approach (methodology) is in full agreement with latest approaches (Bohlmann 2019, Shang 2020)?

Please check also Veselovskii. You will find some hints to pollen lidar ratios as well.

REPLY: Our methodology was also applied by Bohlmann et al. (2019) and Shang et al. (2020). However Shang et al. (2020) went a little further by presenting a new method to retrieve the linear depolarization ratio of pure pollen (or of a mixture of pollen) present with the background aerosol from measurements of particle backscatter coefficient and depolarization ratio and Ångström exponent.

About the pollen lidar ratio at 532 nm (next all values are given at 532 nm): we used the value of 50 sr in our elastic inversions. We think it is an appropriate value for a mixture dominated by *Pinus* and *Platanus* given the large range of values found in the literature and the large variability associated to these values. In the literature, some of the values found are:

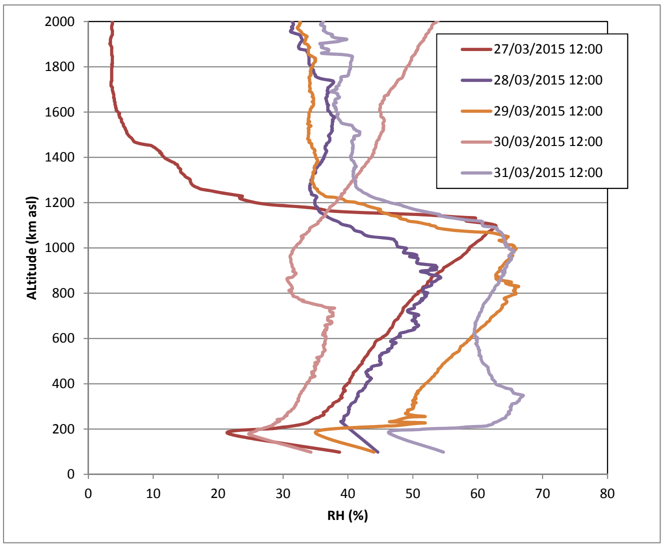
- 50±6 sr (Noh et al., 2013) for *Pinus* and *Quercus*,
- 52±12 sr (Bohlmann et al., 2019) for *Betula*,
- 62±10 sr (Bohlmann et al., 2019) for *Betula* and *Picea*,
- 61±8 sr (Shang et al., 2020) for *Betula*.
- 63±14 sr (Shang et al., 2020) for *Pinus* (scots pine).
- 40 - 60 sr (Veselovskii et al., 2021) for *Betula* and 30-60 sr for *Poaceae*.

Veselovskii et al. (2021) is not conclusive on the lidar ratio of pure pollen for both *Betula* and *Poaceae* events they observed. They say: "in many cases we observed lidar ratios below 40 sr at both wavelengths. However, we also had cases when the lidar ratios at both wavelengths were in the 50–60 sr range. Thus, at the moment we are not able to specify lidar ratios for pure pollen". One sees that within the same taxon, the interval of values, taking into account the error bars, can be quite large. For *Betula* it varies between 40 and 72 sr. For *Pinus* it varies between 44 and 77 sr.

Concerning depolarization ratios: What about masking effects? Dust or dry-marine-related depolarization enhancements? I think, these effects are negligible. But there are several field sites (cities) close to the Mediterranean Sea (and there will be sea breeze effects, advecting sea salt particles across the coastal regions…, sometimes up to 10-20 km into the continent). Please, provide a short comment on this.

REPLY: For sure dust was not present during the five days of measurements presented. This has been checked already in Sicard et al. (2016) with the dust transport models BSC-DREAM8b v2 (Barcelona Supercomputing Center – Dust Regional Atmospheric Model 8 bins) and NMMB/BSC-DUST (Nonhydrostatic Multiscale Meteorological Model on the B grid/Barcelona Supercomputing Center – Dust), as well as HYSPLIT (Hybrid Single Particle Lagrangian Integrated Trajectory) back trajectories.

As far as dry marine aerosols are concerned, Barcelona is a coastal city and sea breeze effects are present. During the event, the relative humidity at ground level during daytime varied between 40 and 70 % (Fig. 3 of Sicard et al., 2016). And the tendency on all days was to increase with increasing height (see figure below from radiosounding measurements performed daily at 12 UTC). This behavior is the opposite of Haarig et al. (2017) observations of dry marine particles. According to Haarig et al. (2017), values of depolarization ratio of ~0.15 can be reached for dry marine particles. This occurs in the top part of the boundary layer when the relative humidity falls to values on the order of 40%. Given the above, it is highly unlikely that dry marine particles have been present in the measurements presented in the paper. For information, the UPC lidar team, which develops and operates aerosol lidars since the late nineties has never experienced the presence of depolarizing, dry marine particles in the PBL.



Haarig, M., Ansmann, A., Gasteiger, J., Kandler, K., Althausen, D., Baars, H., Radenz, M., and Farrell, D. A.: Dry versus wet marine particle optical properties: RH dependence of depolarization ratio, backscatter, and extinction from multiwavelength lidar measurements during SALTRACE, Atmos. Chem. Phys., 17, 14199–14217, https://doi.org/10.5194/acp-17-14199-2017, 2017.

P7, Figure 1 is very nice and rather helpful. If possible, provide a bar, indicating 10, 20 or 50 km distance…, in the right lower corner… In Figure 1, one can see that sea breeze (and land breeze effects in the night) will affect the pollen transport.

REPLY: Thank you for your comment. We've included a length bar in the right lower corner of the image following the reviewer suggestion. We agree with the reviewer comment that the dispersion in the region is significantly dominated by land-sea breeze circulations, especially during summertime. However, the period of study was not characterized by this type of circulation.

P12, Figure 3b is very convincing, shows excellent agreement! I did not expect such an agreement between a point observation (at ground) and a column observation (lidar). This corroborates that dust and dry sea salt effects are probably negligible. Should be mentioned.

REPLY: True. It also validates our hypothesis that the first lidar measurement (225 m) is a good proxy of what it would be at ground level.

P15, Figure 6: Please mention that the solid line (in each plot) shows the coast line (and not a river…, as I was thinking in the first moment).

REPLY: This explanation has been added in the caption of Fig. 6.

P24, Figure 16 is mentioned… To my opinion, the lidar profiles are biased (above 400m) . The profiles are unrealistically smooth. I speculate that the pure Rayleigh depolarization background is varying with time. By using a fixed, but a bit smaller values than the actual (instrument-related) background Rayleigh value, you get such a bias. But in reality, it is the background, and not a pollen depolarization contribution. At least it looks strange.

REPLY: The pure Rayleigh depolarization background may vary with time if the temperature varies. This has been studied in the past by Behrendt et al. (2002). The following figure taken from that paper shows how the Rayleigh depolarization ratio varies with temperature for different bandwidths of the interference filter used at 532 nm. One sees that the absolute values are small (<0.015) and that the variations with temperature are also small. For information the bandwidth of the interference filter of our MPL is smaller than 0.2nm.

Anyhow Fig. 16 and the discussion (last paragraph before the conclusions) have been deleted in the revised manuscript. Referee #1 suggested to avoid to include the nighttime profiles in the calculation of the statistics in Table 8. This allowed us to delete the lengthy discussion about possible nighttime residual layers observed by the lidars and underestimated by the models.

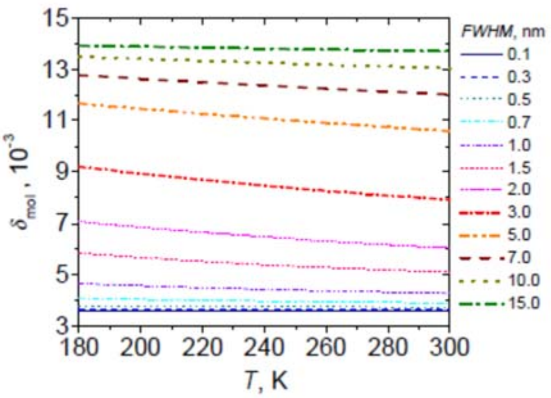


Fig. 4. Molecular volume depolarization ratio δ_{mol} against temperature *T* for different values of the width of the transmission band of the lidar receiver *FWHM* calculated for Gaussian-shape transmission bands centered at a laser wavelength of 532 nm.

Behrendt, A. and Nakamura, T.: Calculation of the calibration constant of polarization lidar and its dependency on atmospheric temperature, Optics Express, 10, 805–817, 2002.

Appendix B: Comparison of meteorological and pollen surface observations with respective model results…… sounds better… However, do we need Appendix B?

REPLY: Appendix A and B have been moved to Supplementary materials.