

Interactive comment on “Investigation of diurnal patterns in vertical distributions of pollen in the lower troposphere using LIDAR technique” by Y. M. Noh et al.

Anonymous Referee #2

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The paper reports simultaneous PBL lidar and meteorological measurements, and aims at interpreting the lidar measurement in view of pollen release, dispersion and removal, establishing a correlation with in-situ pollen observations. The dataset presented is interesting and surely deserves publication. However, I am afraid the paper can not be published as it is, as a number of points must be clarified. The main problem I see is an improper use of one parameter accessible to lidar sounding, the total depolarization as the ratio of parallel and cross laser pulse returns. This is a parameter that depend both on the average morphology of the scatterers, and on their concentration. Its variability can be attributed to both changes in aerosol type, or in aerosol burden. The authors should compute the aerosol depolarization instead, and use that as an

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intensive parameter not depending on the aerosol concentration, but only on its average morphology. Hence they should discuss separately the extinction measurements as proxy for the aerosol concentration, and aerosol depolarization as a parameter to discriminate among aerosol types. Anyway, I am willing to see the paper published as the case study is interesting. A detailed review follows:

(31188,14-17) This is a strong statement. A correlation “suggests” more than “implies”.

(31188,18) Turbulent transport may be promoted by increasing temperature and wind speed. I do not see immediately the connection with decreasing relative humidity. . . The sentence “Which . . . humidity” can be dropped out of the abstract.

(31190,1) I do not think you can consider pollens as “pollutant”. Some may be harmful and even cause acute respiratory diseases. Nevertheless they should be held distinct from air pollutants as they are naturally occurring, while the substances typically referred to as pollutants are created by human activities.

(31190,9) ollen?

(31190,27) As the authors aim to compare lidar data with ground observations, they should provide some information on the altitude of the laser - telescope FOV overlap region, and how the managed – if they did – to extrapolate their measurement to the ground. Moreover they should report time and altitude resolution.

(31191,2) An elastic backscatter lidar does not allow the simultaneous independent determination of backscattering and extinction coefficients. How the extinction coefficient was retrieved? Was a Raman channel available? Did the authors use the aerosol optical depths available through the sun photometer? Additional details are needed here.

(31192,3) and Figure 1. The authors show and discuss plots of total depolarization ratio. As already stated in the review, this is not the right intensive parameter characteristic of the aerosol morphology, as its value depends also on the amount of aerosol

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present, with a dependence close to linear, if the depolarizing aerosol concentration is low. This is of great importance for all the subsequent discussions on the correlation among depolarization and other extensive quantities. So the authors must present and discuss also the aerosol depolarization ratio (see as instance Cairo et al., Appl. Opt., 1999), as more indicative of the difference in various kind of aerosol eventually present. Moreover, if the extinction come from an independent measurement (i.e. if it is not simply computed from the backscatter coefficient with a fixed a priori lidar ratio) they should also present the aerosol backscatter-to-extinction coefficient, to improve their capability to discriminate among different aerosol types.

(31192,17) The authors should describe in more detail under what respect the patterns are different.

(31192,18) Neither the surface PM10 nor sun/sky radiometer reported data can be used to “identify” non-spherical particles. More properly, the authors are studying correlation between aerosol concentration, optical depths and mean dimensions, with depolarization. Again, use also the aerosol depolarization otherwise the correlation may became trivial.

(31192,26-27) PM10 time resolution is far too coarse in Fig. 2, compared to Fig. 1, to judge anything. The authors should substantiate more their claim, and maybe quote also that the average pollen dimension is beyond the detection limits of the PM10 (If that is the point in stressing the lack of correlation between PM10 and depolarization).

(31192,28) to (31193,7) Here I had some difficulty in following the reasoning; the author should rearrange the discussion as this is the key passage of the paragraph. Basically, the attribution of these aerosol observations to pollen is based on: 1) Lack of correlation between PM10 and depolarization. 2) Unusual (but they should explain what is usual for Asian dust episodes) diurnal pattern of vertical distribution of depolarization and extinction. 3) High values of the Angstrom exponent, compared to what is typical for Asian dust. 4) Distance (geographical – not “geometrical” - location) from the sea?

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Actually, 50 km is not that far. . . although I understand that the authors wish to imply that the “repetitive diurnal and vertical patterns” of these vertical “puffs” of depolarization are suggesting a local surface source, maybe they should comment on the wind regimes on those days, to rule out marine aerosol transport. All the discussion there must be made more clear.

(31193,15) fig. 4 should be renumbered, introduced immediately after fig. 1 and there discussed and confronted as a “background” condition where the evolution of the PBL does not show any significant increase of the depolarization at noon. Here, the figures should be rediscussed in light of pollen measurements.

(31193,20-23) There the authors should explicitly report a brief summary of the main findings they have found in the quoted literature, with respect to pollen release, lifetime, wet and dry removal in conjunction with meteorology, as my understanding of the text that follows is that the author suggests a release of pollen in the morning, vertical transport and mixing at noon and subsequent (dry) removal in the late afternoon. Is it consistent with previous studies?

(31194,23) The interpretation of the measurements the authors are putting forward is that pollen is heavily released in the first, warmest part of the day, mixed, and then removed in the afternoon. They here discuss fig. 6, showing morning sounding of humidity with a gradient in the PBL, disappearing later on (not on 4 May) thus suggesting that efficient mixing went on. The stability and inhibition to mixing should be more properly assessed by looking at potential temperature and Richardson number profiles. Are those humidity measurements valuable in studying the removal processes and how they depend on relative humidity? would be worthwhile to look at that.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31187, 2012.

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