Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-1084-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effect of sea breeze circulation on aerosol mixing state and radiative properties in a desert setting" *by* Yevgeny Derimian et al.

Anonymous Referee #3

Received and published: 17 February 2017

Review of 'Effect of sea breeze circulation on aerosol mixing state and radiative properties in a desert setting' by Derimian et al.

The paper studies the modifications in summertime sea breeze conditions of the aerosol compositional, microphysical, optical, and radiative properties at an inland location of the Negev Desert (Israel). It is well written and easy to read. The study is original and the scientific methodology sound. The complementarity of the inversion of remote sensing (sunphotometer) observations and of the direct, and I suppose time-consuming, off-line individual analysis for characterizing the particles is particularly interesting. The authors evidence for the first time the significant influence of the daytime intrusions of marine air on the aerosol characteristics at such a remote place.

Not only the composition, but also to internal structure of the particles greatly differ in the pre- and post- see breeze situations. In particular, the proportion of mineral (desert-dust) particles surrounded by a coating is unexpectedly large in both cases, which contradicts the common assumption that desert dust is hydrophobic. These modifications of the aerosols characteristics need to be taken into account for quantifying their radiative effect. Finally, the numerical simulations made by the authors show that the current remote sensing inversion algorithms need to be modified in order to take the core-shell structure of the particles into account. My opinion is that this paper deserves publication provided the following concerns are addressed: General comment: After reading the paper, one is left with the impression (see for instance lines 21-23, page 2) that the aerosols initially present at Sede Boker are modified by the arrival of the sea breeze. In fact, these pre-existing aerosols are most probably blown further downwind of the experimental site by the breeze and replaced by new freshly advected particles. The authors make an exhaustive and quite interesting comparison of the characteristics of these two sets of particles, but if the particles are not the same, is it possible to conclude that the size increase observed after the arrival of the sea breeze could be due to the water vapor uptake? More generally, the mineral particles observed during the marine intrusions probably have a long history of coexistence with the other species (sea-salt and anthropogenic aerosols and gases), what's more in humid air-masses. Therefore, they are more liable to have formed internal mixtures than the resident aerosol of Sede Boker. Miscellaneous: 1) P. 7: What can the origin of the Ti-rich particles be? 2) P. 8, line 25; on Fig. 4a, the Angström exponent increases with the arrival of the sea breeze on 14 August. Is there a plausible explanation for this exception to the rule? 3) P. 10, line 8: the nephelometer 'dries'. . . 4) P.10, lines 12-13: couldn't the 'abrupt response' also be due to the increase of the aerosols concentrations and to a shift in their size? 5) P.10, line 24: This is Fig. 6 (not 5) 6) P.10, line 33: The unit is μm not mm 7) Fig 6c and d : the blue line corresponds to WV larger than 8) P.11; line 5: As said in the following sentences, the large standard deviation does not allow concluding that there is 'a decrease' of the mean real refractive index. I would

remove the 'Curiously a decrease. . .'. 9) P.11; line 31 and the notations of Fig. 8: Usually, PM1 and PM2.5 correspond to particles with diameters smaller than 1 and 2.5μm, respectively. Here, PM1 corresponds to particles with diameters between 1 and 2.5, and PM2.5 to the range 2.5-10μm. This is confusing. 10) P. 12, lines 15-16: the 'other' particles represent 7% of the coarse fraction but are said to be smaller than 1μm in diameter. Isn't this contradictory? 11) P. 13, line 1: the authors say that the shift towards larger sizes of the marine particles during the sea breeze could be due to hygroscopic growth. Aren't the SEM observations made under a vacuum, i.e. in dry conditions? Moreover, if we go back to my first comment, please consider that the marine particles observed at the inland site before the arrival of the sea breeze might be more aged than the new ones. Consequently, their size-distribution might have been modified by the size-selective dry deposition process. For instance, I cannot help observing on Fig 9 that the very fine and the coarse particles present in the fresh marine air-masses (Fig. 9 b) have disappeared on Fig 9a, and that on the latter figure, only the particles with a diameter corresponding to the smallest deposition velocity (around 1μm) subsist. 12) P. 13, line 24: There is no Mg in the composition of the calcite. 13) Fig. 11: On the upper right-hand panel, this should be Ca (not C). 14) Fig. 12: I cannot see the arrows mentioned on page 14. 15) P. 15, line 19: In the reduction of 5%, what are the respective shares of the 1) aerosol changes and 2) WV increase? 16) P. 15, line 26: this should be Fig 13, not 12. 17) Section 7: please, consider reformulating the whole section. It is much harder to follow than the rest of the paper. For instance, the reader discovers only on page 17 that forward calculations have been made (and with which inputs), then that different scenarios have been considered for inversion simulations. 18) P.19, lines 2-5: could you be more specific regarding implications for the satellite and LIDAR inversions?

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