

Interactive comment on “Towards a first classification of aerosol shrinkage events” by E. Alonso-Blanco et al.

Anonymous Referee #3

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This manuscript presents a classification scheme for atmospheric particle shrinkage events based on a 3.5-year field measurement dataset in an urban background site in Spain. As particle shrinkage events have been sporadically reported in only a handful of studies, this long-term dataset provides a unique opportunity to thoroughly study the types and possible reasons of particle shrinkage. As such, the study subject falls within the scope of ACP and would be of interests to its readers. Although the manuscript is well written and the adopted scientific methods are fair, there are notable rooms for improvements; specifically, the inference made in the manuscript is rather descriptive and lacks quantitative information from “all” shrinkage events. Specific comments are given below: 1. In the Methodology, p.25237 lines 5-9, it is recommended to briefly provide what have verified during the intercomparison campaigns of the REDMAAS. In addi-

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tion, have the authors synchronized all the collected data with varying time-average (i.e., 4.5, 7, 10 min, etc.) to a unified time resolution (e.g., 60 min)? 2. In the Methodology, p.25239 lines 1-9, please briefly provide the difference between 1a and 1b for benefits of the readers. 3. In the Methodology, p.25239 lines 15-19, please justify the use of “10% difference” as an indicator of atmospheric dilution. In the event of NPF, a substantial increase (much greater than 10%) of nucleation mode particles is common and expected, of which obviously may not be necessarily related to dilution. On the other hand, a 10 % difference of accumulation mode particles would be relatively difficult, of which is more likely related to dilution. I therefore suggest the “10% difference” criterion should be particle size-specific. 4. In the Results and Discussion, p.25242 lines 3-17, the authors attributed the seasonality of shrinkage events to increase production (photochemistry and biogenic VOC) and increased residence time for aerosol processes. The latter reason in specific is questionable and lack strong evidence. NPF events are commonly considered as “regional” phenomena that oftentimes span over a spatial scale of 10s- 100s km, and that the winds in the study area were actually stronger during spring and summer. See the following comments. 5. In the Results and Discussion, p.25242 lines 18-24, the inference on the preferable meteorological conditions would be more reasonable to be based on “non-event” vs “event” days as the occurrence of particle shrinkage appears to be case-specific. In addition, it is important to describe and discuss the prevailing winds in order to relate the observed particle shrinkage to possible source regions (e.g., biogenic or anthropogenic). 6. In the Case study analysis, the authors attempted to generalize and attribute a specific type of particle shrinkage event to certain favorable conditions, prior to each case report. Although I find some inferences are reasonable, I am not fully convinced because of the lack of quantitative information from all the “event” days and that from the “non-event” days. It is strongly recommended to summarize those quantitative (statistical) information into a table to support the arguments.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25231, 2015.

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