

Interactive comment on “Profiling of aerosol microphysical properties at several EARLINET/AERONET sites during July 2012 ChArMEx/EMEP campaign” by M. J. Granados-Munoz et al.

Anonymous Referee #2

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The manuscript titled "Profiling of aerosol microphysical properties at several EARLINET/AERONET sites during 2012 ChArMEx/EMEP campaign" intends to analyze the microphysical properties of aerosols at five different lidar ground-based stations, and to use the results obtained for the validation of different mineral dust models.

The paper addresses an interesting and sound topic related to the aims of the ChArMEx campaign. The English language and presentation are very clear and up to the standard of an international journal. The figures and tables in the manuscript are also relevant.

The paper is well organised and detailed. I strongly appreciate the effort of the authors to compile different ground-based observations and models. In this sense, the authors present a nice description of the state of the atmosphere during the ChArMEx campaign (9-11 July 2012). This technical work is noticeable; but somehow does not importantly contribute to science. After having read carefully the paper, I am not feeling having learnt a lot, for the following reasons.

Most of the paper is devoted to the description of aerosol optical properties, aerosol mass and layering at different stations. The processes involved in the dynamics of transport of dust to the Mediterranean are widely known (as also stated by the authors in the references included). These processes were largely studied in a number of publications, such as Pey et al., 2013; Salvador et al., 2014; Gkikas et al., 2013 and 2015, Sicard et al., 2015; just to cite some recent papers. The models used (BSC-DREAM8b, NMMB/BSC-Dust, DREAM8-NMME, COSMO-MUSCAT) are not new either, and have been extensively validated in other studies (e.g. Perez et al., 2008; Pérez et al., 2011a, 2011b; Basart et al., 2012; Haustein et al., 2012; Mona et al., 2014, and especially, in Binietoglou et al., 2015, among many others). Also, using GARRLIC and LIRIC for retrieving microphysical properties is not a new contribution either.

Therefore, the authors should clarify which part of the manuscript is innovative and how this paper contributes to an advancement of the scientific knowledge.

Also, the authors have to further improve the discussion on the skills of the models. The authors calculate the relative bias (what the authors call the Relative Differences); but is this figure good enough to characterize the models? The bias may largely compensate with the layers where under- and overestimations are produced. In other words, a "zero" bias can come from very large absolute errors that compensate. The authors may use the US EPA (1991; 2005) indicators of those statistical figures coming from FAIRMODE initiative in order to have an idea of the ability of the models for reproducing dust mass concentration. It would also be desirable to find some information related to the temporal skills: correlation coefficients, variability, etc.

Moreover, the authors do not provide any insight on the differences between the models (for instance, why some models indicate dust and some do not) or their skill. This has to be extended in the manuscript.

Other minor comments:

1. I cannot find the reference Gama et al. (2015) in the literature section.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32831, 2015.

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