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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The impact of mineral dust on cloud formation during the Saharan dust event in April 2014 over Europe" *by* Michael Weger et al.

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Given the reluctance of one of the reviewers to submit their comments after agreeing to do so, as the editor of this paper, I will provide the second review so that the manuscript can be revised and submitted in a timely fashion. I have attached an annotated version of the manuscript to this review where most of my comments, suggestions and corrections are posted. The primary questions that I would like addressed addressed are listed below.

This study is primarily a modeling evaluation of how mineral dust impacts the formation and evolution of cirrus clouds. The model results are compared against space-borne,

airborne and ground-based measurements in order to highlight how the parameterization of dust as ice nuclei (IN) impact the microphysics of of cirrus. The specifics of the model are described in detail along with sufficient references to allow the interested reader to learn more about the modeling.

I am not a modeler and hence cannot comment on most of the aspects of the simulation that are detailed. I was, however, struck by the fact that the authors wait until the final paragraphs of the discussion section to reveal that there is what I consider to be a very large discrepancy between the vertical profiles of temperature and humidity measured with the radiosondes and those simulated by the model. Given how every aspect of the modeled microphysics depend on the water vapor mixing ratio, why isn't this uncertainty introduced at the very beginning before any of the discussion of IN? I think that there has to be some type of exercise that shows the reader how sensitive the model is to these differences.

That being said, the authors suggest various reasons why the model differs from the sounding, all of them potentially valid; however, they fail to use the most powerful tool at their disposal, i.e. the aircraft that measures temperature and water vapor mixing ratio to a high degree of accuracy. If the arguments are to be convincing than I argue that the model meteorology needs comparing with that measured on the aircraft.

This brings me to my second point that concerns the dust aerosol that most certainly was being transported to the area of interest but its parameterization is based on a data base that may or may not be relevant to the study at hand. Yes, the AOD is measured with Aeronet but the only vertical profiles of dust are from a single lidar that derives an extinction coefficient from the back-scatter profiles and clearly shows two layers whereas the model only shows one. It gives little quantitative information about the aerosol microphysics. Why are the aircraft measurements not being used? Were there no aerosol spectrometers on the aircraft that could be used to estimate the dust concentrations and size distributions, as well as measuring the interstitial aerosol to see if indeed there were potentially more IN on April 4 than on April 3? The vertical profile of the aerosols would also be provide by the aircraft.

Without these questions being addressed (as well as others in the manuscript), the final conclusions are less than impressive and we are all left wondering if it meteorology or aerosol composition that really matters.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-685/acp-2018-685-RC2-supplement.pdf

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