

Interactive comment on “An analysis of the September 2015 severe dust event in the Eastern Mediterranean” by Philipp Gasch et al.

Anonymous Referee #1

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The Manuscript entitled “An analysis of the September 2015 severe dust event in the Eastern Mediterranean” describes in great detail the mechanisms of the dust storm episode using the ICON model coupled with a desert dust module (ART). The paper is very well written and I would recommend it to be published on ACP after the following comments have been addressed:

1) Abstract, Line 3: The authors state that “...state-of-the-art dust transport models were unable to forecast the event...”. I don't think this is accurate. For example the publication of Solomos et al., 2017 in ACP describes the same episode using another model. I think that the two manuscripts were submitted very close to each other and the authors were not aware of this, even though they give reference to this work when it was still in ACPD. Please correct this statement accordingly. This is present in other areas of the manuscript as well and should also be corrected.

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- 2) Page 3, Line 20: The 3rd research question posed by the authors is “What are the meteorological drivers responsible for pick-up and long-range transport of mineral dust?”. It is not clear if they mean in general or in cases such as the event described in the manuscript, because the dust-cycle mechanisms in general are well known and documented. Please rephrase
- 3) Page 4, Line 15: You state that “. . .the seamless modelling capabilities of ICON are of crucial importance because inconsistencies in tracer transport and tracer physics at the nest boundaries can be avoided. . .”. Please remove the word “seamless”. Also I cannot understand how these inconsistencies are avoided. Please expand.
- 4) Page 4, Line 22: What do you mean by sedimentation? Does it refer to the sand-blasting mechanism for production or the deposition of particles?
- 5) Page 5, Line 9: It is important to see how the model defines the dust sources between the nests. Are they defined separately for each domain? If so how do you assure there are no continuity problems in the fields? Is this what you mean in Page 4, Line 15 (see comment 3)?
- 6) Page 5, Line 17: You state the timestep for calling RRTM is 288 seconds. Please provide the timestep of the simulation as well.
- 7) Page 5, Line 18: “. . .ART modifies the radiative transfer parameters of the climatological dust distribution. . .”. What do you mean by “climatological”? Do you mean the dust distribution as described by the dust module? Please expand.
- 8) The same confusion in Line 20: “The parameters returned by ART are the combined values from the local ART dust concentration plus the Tegen climatology”. During the simulation dust concentration is calculated using both prognostic dust and climatological values?
- 9) Page 6, Line 14: “Therefore, the median diameter of each mode is expected to decrease during transport”. You mean that during the simulation the size of the particles

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changes? Please expand a little as this is very interesting.

10) Page 7 Line 29: "...which represents the soil moisture conditions in the region more realistically...". Do you have a reference or actual data to support that this method provides more realistic values? This is essential as soil moisture dictates dust production. How can you be sure that the underestimation of the dust concentration by the model (as described in later chapters) is not attributed to false soil moisture?

11) Page 9, Figure 3: I would like to see clearer plots, especially the national borders as to know exactly where they refer to. Maybe resizing them?

12) Section 3: Since this is a very detailed description of the event it would be very interesting to see a vertical cross-section of dust concentration and precipitation in the same plot, for different forecast hours. Like what you have in Figures A1-A4, but vertically.

13) Figures 6 and A1-A4. Please add labelbars to the plots (where applicable of course).

14) Section 3.4.1: I would like to see a comparison for Dust Optical Depth from more stations in the computational domain. Is this possible? Maybe using AERONET data? If there is no additional data available then add a sentence in the text stating that.

15) The authors show that the ICON-ART underestimated dust concentrations and give a very thorough explanation as to why this happens. However how can you be sure that this substantial difference is not caused by something simpler like wrong description of the strength of the dust source areas (maybe in reality the areas are more active than described in the model) or, as I stated above, bad definition of the soil moisture? Have you tried some sensitivity runs based on these?

16) Page 28, Line 21: "...this study presents the first successful simulation of the September 2015 severe dust event...". See point (1) in my review.

17) Page 29, Line 10: "...ICON-ART results are one order of magnitude better than

those from other models...". This statement is rather odd since you do not present the capabilities of other models in the manuscript to support this. Of course this does not reduce the very good performance of your model in any way.

18) Finally, as far as I can tell, the model does not support the indirect effect of dust particles (cloud and precipitation). Does this affect the performance in this particular case? If the model was able to simulate dust acting as CCN would the results be any better? Just add a small paragraph expanding on this.

In conclusion I believe that the manuscript deserves to be published at the Atmospheric Chemistry and Physics Journal after the issues above have been addressed.

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