Review of the manuscript entitled: "Revealing the meteorological drivers of the September 2015 severe dust event in the Eastern Mediterranean" by Gasch et al.

This review (second round) has two parts. I (Albert Ansmann) will focus on the response of the authors to my request to provide a proper Introduction (Sect.1) into the field of research regarding this extreme (record breaking) dust event, i.e. to provide a proper overview on the published work concerning the September 2015 dust event. I asked my colleague, the modeling expert Stavros Solomos from the National Observatory Athens to critically review the revised version. His comments are given in Part 2.

Part 1 (Albert Ansmann):

I appreciate very much that the authors did a lot to improve the first version of the manuscript. The paper will become an excellent contribution to the atmospheric science literature. Nevertheless, I am not satisfied with the Introduction. The authors did not fully follow my suggestions (Q2 in their reply letter). The introduction must therefore be further improved to my opinion.

The main focus of the Gasch et al. paper is modeling of the September 2015 super dust event. The title of the paper clearly indicates that. An overview of the published literature regarding observations and modeling efforts of this super dust event and the main findings described in these papers must be given. In the case of observations this is done. Only one peer reviewed paper is available here (Mamouri et al., 2016). The rest is more or less grey literature. Alpert et al. (2016) is an EGU abstract.

In the case of modeling efforts, the requested review is not given. Thus, the Introduction must be improved here significantly. There is the paper of Solomos et al. (2017) on this super dust storm, which goes into deep details. The findings of this paper must be summarized in the Introduction and afterwards the motivation for the Gasch et al. paper should be outlined in view of the already known facts and analysis results of Solomos et al. (2017).

The main (exiting) questions regarding this dust storm were: Why did the forecasts more or less failed to predict this dust storm? What were the reasons for the development of the enormous dust storm? And these answers can already be found in the Solomos et al. paper. The Gasch et al. paper is a follow-up paper of the Solomos paper. This is very clear! But this not a 'draw back'. By saying what is already known it becomes very easy to say what will be the new contributions of Gasch et al.? What is missing, what are the new points. And there are many, as the paper nicely shows. This is the well-accepted and logical way of Introductions. However, the authors failed to follow my opinion. The Introduction is not acceptable.

Because of the low resonance to my recommendation (Q2) I have the feeling I should expand my own ideas what the authors should present:

Let me start with the following (defintions): There is a nice paper (Gkikas et al., ACP, 16, 2016, should show up in the references) presenting an extended climatological overview on dust storms in the Mediterranean and they defined: a) STRONG dust storm… when the 500 nm AOD exceeds the climatological mean AOD for given site by 2 STD and is between 2-4 STD, b) EXTREME dust outbreak .., when the AOD exceeds 4 STD. And this September 2015 dust storm exceeded the STD at Limassol by a factor of more than 25 (as written in Mamouri et al., 2016) !!! This expresses best what happened in September 2015 in the Middle East.

Now to the most surprising question (motivating the Solomos paper and the Gasch paper): Why did the dust forecast models fail… in this specific case of this super dust storm?

Solomos et al. (2017) answered this question already in a number of points. They provide a list of reasons for the development of the dust event, based on complex modeling and in-depth comparison with spaceborne remote sensing.

Solomos et al. already showed why all these dust forecast models failed because ‘convection permitting resolution’ is required. Solomos et al. already discussed the reason(s) for the enormous dust emissions (development of MCS, of cold pool outflows, density currents occurrence, humid air transport from the south, even changes in the surface characteristics, probably a consequence of the long-lasting political crises in the Middle East).

In the conclusions of Soloms et al. the reasons for the dust storm were summarized:

1. the formation of a strong thermal low and of convective outflows over Syria that lifted dust up to 4 km,

2. the intrusion of moist and unstable air masses from the Arabian Sea and the Red Sea that triggered convective activity over the Iraq, Iran, and Syria (Turkey border),

3. the generated outflow boundaries that led to dust deflation and formed a westward-propagating haboob that merged with the previously elevated dust over Syria,

and 4. the increased efficiency of Middle East dust sources in the aftermath of war and the related changes in land use.

I was trying in my first review, to convince you to present exactly this. What is available in case of this super storm (observations, modeling)? What are the facts…?

And based on this information, then your paper comes into play. What are the new points presented in your paper? What is still open. What do you want to demonstrate? And then you can introduce ICON-ART and the incredible potential of this modeling infrastructure, linked to this fantastic and unique ART module. So, your paper will become a rather valuable addition to the scientifc literature, no doubt. And this high level paper should have a high-level Introduction as well.

Let me say at the end (because I had this impression when reading the current version of the Introduction)… Science is not an Olympic racing competition (who is the best?). Science is a careful, sensitive, steady and slow accumulation of new knowledge, step by step, paper by paper, …, and a good Introduction reflects that, provides a review of these papers before coming up with the own new idea and contribution.

Part 2 (Stavros Solomos and Albert Ansmann)

The submitted manuscript presents an ICON-ART simulation of the record breaking dust event of September 2015 over Middle East and the Eastern Mediterranean. The authors use an extended convection-resolving domain to analyze this episode and in our opinion their major findings (i.e. transport of moisture from the Red Sea, formation of a strong thermal low in Syria, three consecutive and eventually merging cold pool outflows) support the results presented in previous studies of the same event by Mamouri et al., 2016 and Solomos et al., 2017.

Their analysis is valid and provides for the first time the ability to examine convective processes over such an extended domain. However we feel that the authors do not fully exploit their model results and that they should provide more insight and quantification of the convective cloud processes, the vertical structure of the developing storms and the dusty outflows. In general we recommend that this manuscript should be accepted for publication in ACP with major revisions. Specific comments follow below:

Specific Comments

P2. Line 35 – P3. Lines 1-3: "Solomos et al. (2017) model the event at convection permitting resolution, however the spatial extent of their convection permitting domain does not cover the full MCS region. Consequently, their model fails to reproduce the observed CPO outflow structures and connected dust plumes realistically (see discussion in Sec. 3.2 and 3.3)."

As stated in Solomos et al., 2017:"The key to forecasting these events in atmospheric models is the use of cloud-resolving grid space. However, such high-resolution grid space can only be applied over limited areas due to restrictions in computational power. Forthcoming studies using an extended cloud-resolving grid over the entire Middle East (e.g. Gasch et al., 2017) could provide more detail on the individual atmospheric processes during this episode".

However, the analysis and the general findings presented here are very similar to the ones shown in Solomos et al, 2017. It is somehow contradicting for the authors to say that this previous work lacks crucial information since their major findings are basically the same. ICON-ART may be performing better at some places but by no means RAMS "failed" to reproduce the event. Furthermore ICON-ART also clearly misses several event processes with deviations of up to one order of magnitude. This paragraph should be rephrased.

P3. Lines 20-21: "It is capable of local grid refinements, in this study the finest nest has a convection permitting grid spacing of 2.5 km."

P8. Lines 13-15: "A grid spacing of 2.5 km is generally assumed to be sufficient to permit the development of convection in a non-hydrostatic model. Therefore the convection parametrization, including the parametrization for shallow convection, is switched off for the finest grid."

Whether a 2.5×2.5 km grid space is adequate for resolving convection is somehow questionable. Especially during the first crucial stages of convective development even this resolution may not be sufficient for accurately representing the initial cloud dynamics and microphysics. The authors should support this argument with literature references.

P9. Lines 9-10: "The aerosol - cloud microphysics interaction is not included in this study as it creates a new set of research questions and the focus in this study is on the mineral dust radiation interaction."

Based on previous studies, the effects of dust-cloud microphysics interactions in the formation of similar systems is probably very limited compared to the dynamics of the system (see for example Solomos et al., 2012).

P9. Lines 16-18: "The IFS initialization data for soil moisture was modified in a region along the Syrian-Iraqi border which showed high soil moisture values and spatial inhomogeneities without preceding rain or changes in soil properties."

Is there any observational evidence to support this change in initial model soil moisture? What would be the difference in modeled dust fields if the authors used the original IFS fields?

P12. Lines 19-20: "We focus on the results from the convection permitting domain, as it yields remarkable improvements compared to the global domain."

How do the intermediate domain results compare to the fine-grid results? The benefits of using a convection permitting grid space (though reasonable) are not properly justified. The authors should compare their high resolution runs with ICON-ART results using for example only 3 nested domains (e.g. 40-20-10-5 km) and discuss the possible improvements.

P13. Lines 8-11: "Our analysis contrasts the simulation results by Solomos et al. (2017, their Fig.4c), who model AOD values above 20 already before the onset of strong downward mixing of momentum. Furthermore, their modelled bimodal maximum dust distribution was not observed by satellites and no closed cyclonic flow around the heat flow appears to have existed."

In Figure A1 (a) and also in SEVIRI images it looks that dust flow is actually following a cyclonic circulation over Syria. The extreme AOD values in RAMS simulations shown in Solomos et al., 2017 appear over a few grid points mostly due to the overlapping of multiple dust layers. These may indeed be unrealistic but the event itself was extreme and furthermore there is no reliable satellite retrieval at such high AODs. Atmospheric models are far from being perfect and as shown in the current manuscript even ICON-ART fails to reproduce the transport of dust at certain areas or presents an underestimation of one order of magnitude compared to dust measurements.

P13. Lines 18-19: "The reinitialization of ICON-ART with IFS at 12 UTC impairs the CPO2 development due to the termination of convective structures."

The authors should justify why it is needed to reinitialize ICON-ART after 18 hours run. What would be the model results if the authors let this simulation continue? Initialization with the IFS at this stage actually means that ICON-ART meteorological fields are overwritten by the IFS analysis that already assimilates all available observations of the MSC organization.

P14. Lines 5-8: "The above findings again contradict those of Solomos et al. (2017, their Fig. 7b), who in their model results find a northward travel direction of a small cold-pool structure. Based on the good agreement between ICON-ART and satellite observations this result is implausible. Furthermore, the intensity and spatial extent of their modelled CPO is much too small.

This is not true and the authors should remove this comment. As explained in Solomos et al., 2017, Figure 7 shows a primary convective cell that travels towards the north and triggers the generation of the larger cold pools which are shown in Figure 8 and indeed travel westward / south-westward. This activity is also evident in SEVIRI images.

P14. Lines16-27: "Past midnight on 07 September and explosive intensification……. and a greater dust plume depth which subsequently extends throughout the full CPO"

In our opinion, this section (accompanied by the A4, A5 cross-section plots) shows a clear picture of the developing situation and moreover exhibits the benefits of using such an extended convection resolving domain. However it is not clear what is shown in each of these plots, the captions do not include information about the plotted quantities and the quality of the images should be improved (if possible adding also an indication of the cross-section locations over a SEVIRI horizontal plot). The horizontal evolution of the episode is more or less evident from SEVIRI but the vertical analysis can only be obtained through modeling simulations as shown here. Improving this information and also adding these plots in the main manuscript will improve the analysis and the overall understanding of the event.

P15. Lines 1-2: "Downstream the line-shaped rainfall distribution near surface wind speeds increase strongly as the CPO3 reaches the surface (Fig. 5b). Wind speeds above 12 m s^{-1} are modelled inside the CPO3 region."

The formation of a gusty density current in front of the rain curtain must be further analyzed with a vertical plot of the microphysical and dynamical structure of the storm evolution at this stage. This is the crucial stage for the generation of the haboob and more information must be provided on the contribution of the various cloud properties to the strength of the generated CPO.

P15, Line8: "The maximum DOD value is 4.15, it is reached in an area close to the leading edge of CPO3 which shows the highest values of DOD."

The authors should extend their contour scale up to 5 in Figure 5. The same applies to Figure 6 and other AOD images in the manuscript where the maximum values are clearly above 2.

P15, Line 30: "Unfortunately, no measurements by MODIS are available over the northern Mediterranean Sea"

May be "northeastern parts of Mediterranean Sea" is more appropriate.

P15, Line 32: "For the eastern part of CPO3 the MODIS AOD measurements seem doubtful when comparing to the MODIS visible satellite image."

In our opinion the MODIS AOD retrievals do not fully represent the severity of the event. The extreme PM measurements in Cyprus and Israel (away from the sources) and the total attenuation of CALIPSO backscatter imply much larger values of AOD close to the areas of intense dust activity.

P18, Lines 8-9: "The vertical structure of the dust plume can be investigated at this point in time with the help of a CALIPSO overpass which occurred at 10:35 UTC."

The qualitative comparison presented here does not provide valuable information on the unique characteristics of this event. We can offer to the authors our quantitative CALIPSO analysis from Solomos et al., 2017 so as to compare ICON-ART results and conclude on the validity of their simulation.

P18, Lines 15-22: The satellite passes the region where the merged one and two day old sea-breezes ….. CALIOP reports high values of attenuated backscatter in this region whereas ICON-ART simulates a minimum due to the clean air characteristics of the sea breeze"

The explanation given by the authors on the discrepancy between ICON-ART and CALIPSO profiles suggest the mobilization of dust particles by Mediterranean sea-breeze winds penetrating somewhat 800 km inland the Arabian Peninsula. Are these winds strong enough to mobilize dust? This is something hard to believe by the reader and the authors must further support their argument with data.

P19, Line 3-6: "Sec. 3.3 When comparing our results to those of Solomos et al. (2017, their Fig. 10),again large differences become apparent, both in spatial dust plume structure as well as vertical dust distribution. The driving MCS and related CPOs and their clearly marked borders discussed above are not identifiable in their model results and they do not provide a detailed analysis of the flow structures."

Yes the two models differ. Regarding the vertical dust structure the authors should try to compare the same quantities in Figure 7 so they can reach to some quantitative results for their simulations. We can offer our CALIPSO analysis so as to compare ICON-ART results and conclude on the validity of their simulation.

P.20, Lines 4-6: "The high surface wind speeds and turbulent mixing inside the CPO3s result in enormous dust emissions during daytime, consequently the dust is transported within the full boundary layer height up to 5 km (Fig. A4, A5).

How enormous? The authors should give a range of the modeled dust concentrations inside the CPOs.

P.20, Lines 14-15: "ICON-ART DOD values are one order of magnitude higher and show better spatial agreement than other global dust forecast simulation results in the northern part of the EM (see Sec. 1)."

This is not exactly true. If you take a look for 8 September 2015 12:00 UTC in the SDS-WAS portal (https://sds-was.aemet.es/forecast-products/dust-forecasts/forecast-comparison) several models indicate AOD values up to 1.2 over the area of interest.

P.20, Lines 15-21: "Taking into account the 2° longitudinal offset in ICON-ART, the vertical structure of the dust plume arrival represents observations by Mamouri et al. (2016) (Fig. A7) ….. During 08 September, dust concentrations increase up to 3500 g /m3 and the plume thickness grows further, extending from 0.5 km up to 3 km height in the model."

Comparing a Cyprus station with a model grid point 200 km towards the Mediterranean Sea does not make a lot of sense for aerosol studies. On 8 September the lidar was not operating so there is no point to include the comparison shown in the right plot of A7 Figure. It is not clear what the authors mean by : "A first, elevated plume extending from 2-4 km with concentrations up to 1000 μg / m3 is noticeable during 07 September in both lidar measurements and model results" and how this is supported by Figure A7. Also the authors state that: "During 08 September, dust concentrations increase up to 3500 μg /m3 and the plume thickness grows further, extending from 0.5 km up to 3 km height in the model". But the near-surface concentrations in Figure A7 are below 250 μg/m3 on this day while the surface stations in Cyprus recorded concentrations up to 10000 μg/m3 and AOD values greater than 5 indicating a model underestimation of several orders of magnitude. In general we recommend that the authors should remove this entire paragraph since it does not contribute to the explanation of the episode.

P21. Lines 18-20: "The maximum modelled DOD for Sede Boker is 1.0 on 08 September, compared to 4.1 measured by AERONET. The AERONET values appear

realistic, as they are in good agreement with MODIS AOD measurements in the region. Thus, ICON-ART shows an underestimation of DOD by a factor of four."

If the measured AOD is 4.1 in Israel, then the authors should reconsider their much lower DOD values at the areas of intense haboob activity. Also, nobody ever validated ground-based data with satellite observations. The authors should rephrase or remove this sentence.

P21. Lines 21-22: "When comparing PM10 measurements, a larger difference between model results and observations on the order of one magnitude becomes apparent."

To our understanding, this large underestimation implies that near the sources the event was much stronger than simulated by ICON-ART and even MODIS AOD retrievals are not able to capture its severity in terms of optical thickness. It looks like some important meteorological or surface mechanism is still missing in the proposed analysis.

P23. Line 2: "The underestimation by a factor of four between model and measurement is consistent with the AERONET measurements in Sede Boker."

This sentence does not make sence.

P23. Line 29: "leading to a compressions"

Leading to compression

Figure 9, Caption.

The authors should number properly the panel plots as a,b,c,d and explain what is shown there.

Figure 11.

The authors should provide statistical metrics for their comparisons.

P. 27, Lines 19-20, 33-35 "As a result, possible mineral dust emissions due to the super-critical flow cannot be captured by ICON-ART."

"In combination with already underestimated dust emissions due to the recent land cover changes and soil degradation in the Mesopotamia region (see Sec. 2 and 3.3), this provides an explanation why dust transport into the southern EM is underestimated by an order of magnitude. Nevertheless, ICON-ART provides a detailed understanding of previously unknown processes contributing to the historic dust event which makes these findings worthwhile to report."

We agree with the authors that the detailed description of this flow feature is valid and adds to the overall description of this record episode. The hydraulic jump shown in

Figure 10 occurs due to the steep topographic gradient between Golan Heights and the Sea of Galilee. However as the authors also mention, the modeled dust values were already too low before the system reached this area and in our opinion the extreme PM10 measurements in Israel cannot be explained by local production.

P28. Lines 4-5:"The validation of the mineral dust distribution and transport characteristics with satellite and station measurements show overall good agreement between ICON-ART and the observations, especially during the early stages of the event."

Quantitative comparisons in previous sections indicate a severe underestimation of modeled dust and obviously the quantification of dust concentrations is crucial for radiative transfer calculations. Nevertheless we believe that this section (3.5) is still useful but we recommend that the authors should examine the vertical CPO structure at various stages of development (stratification of dust layers, distribution of TKE and speed inside the haboobs) with and without the interactive dust radiative transfer. Investigation of such vertical cross-sections could really provide some insight on the impact of radiative active dust in CPO dynamics.

P.32. Line 11:" This study presents a successful simulation of the September 2015 severe dust event at convection permitting resolution."

The simulation clearly underestimates the dust concentrations and there is no quantitative comparison for the vertical dust concentrations. There is really no need to claim a simulation as "successful". Instead we would propose that the authors rephrase this sentence to something like: "This study presents a simulation of the September 2015 severe dust event at convection permitting resolution that allows the reproduction of the main atmospheric processes during in this event."

P.32. Lines 13-15: "the inland penetrating Eastern Mediterranean sea-breeze and the widespread occurrence of super-critical flow conditions and subsequent hydraulic jumps are suggested as important drivers for dust emission."

This is not supported by the presented analysis. Even if these processes exist their effect is minimum compared to the huge convective outflows.

P32. Lines 21-22: "(1) Is the forecast of the dust event improved by running convection permitting simulations? The convection permitting simulation of the dust event with ICON-ART improves the forecast quality decisively."

This is not supported by the presented analysis. In order to prove this argument the authors should compare their results with ICON-ART results at lower resolution. For example it is not justified if a 4 grid configuration with parameterized convection at 40-

20-10-5 km cannot reproduce the event. A comparison between these two runs could be easy to do and will show how "decisively" is the forecast quality improved.

P30. Lines 24-25: "As the energy reaches the surface nevertheless, reductions in surface temperature due to mineral dust are smaller than those found by two other studies."

Which other studies? Are these studies comparable to an extreme haboob event?

P32. Lines 27-28:"A comparison with lidar observations in Cyprus (Mamouri et al., 2016) shows very good agreement in vertical dust distribution."

This is not supported by the presented analysis and a comparison of a Cyprus station with a model grid point 2° east has no meaning for aerosol studies. Also on 8 September the lidar was not operating so the right plot in Figure A7 is not needed.

P32. Line 30: "and without data assimilation"

Actually when the authors reinitialize their model with IFS at 12 UTC on 6 September, they practically assimilate all available observations of the already evolving event in their simulation. From an operational forecasting point of view, the real value would be to show the results of ICON-ART from the previous day run.

P33. Lines 1-2, 5: "For the transport to the southern EM, a hydraulic jump is demonstrated to be of importance for dust emission in addition to the advection of the dense dust plumes into the region"

"Modelled DODs are in the range of 0.5-1.5 over Israel and PM10 concentrations reach up to 600 µg/ m3 in Jerusalem."

The dust concentrations are already underestimated before the plumes approach Israel. An increase from 600 to 6000 µg/m3 cannot be explained by local emissions.

References:

Mamouri, R.-E., Ansmann, A., Nisantzi, A., Solomos, S., Kallos, G., and Hadjimitsis, D. G.: Extreme dust storm over the eastern Mediterranean in September 2015: satellite, lidar, and surface observations in the Cyprus region, Atmos. Chem. Phys., 16, 13711-13724, https://doi.org/10.5194/acp-16-13711-2016, 2016.

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